Comment on "Accounting for Factorless Income" (Karabarbounis and Neiman, NBER Macro Annual 2018)

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1 Overview

This paper provides the most careful and clearheaded study to date of the factor distribution of income in the United States. Its most important contribution is the introduction of a new concept, *factorless income*, which is the residual after assigning aggregate income to labor and capital. Unlike many other studies, which simply assume that factorless income corresponds to either economic profit, unmeasured capital, or a return premium, this paper is agnostic and entertains all three possibilities. It turns out that none of the three is a perfect match for the data, but that economic profit is a particularly ill-fitting explanation. This calls into question some recent work on rising markups in the US, and I suspect that Karabarbounis and Neiman's critique will quickly become central to the literature.

The following comment has two parts. First, I will provide my own brief tour of factor income trends in the US, covering much of the same territory as Karabarbounis and Neiman but in a cursory and simplified way. Second, I will discuss the paper's key contributions, especially its rejection of "case Π", the interpretation of factorless income as economic profit. I conclude that the paper is quite successful in making its case, and that future work should build upon it by combining the paper's three cases, with a special emphasis on "case R", the discrepancy between interest rates and the rate of return on capital.

2 Factor income in the US: a whirlwind tour

2.1 What income shares should we even be using?

In principle, the question is simple: what is labor's share of income? Sadly, this is a minefield for the casual observer. For the US, the most commonly cited source is the BLS's Labor Productivity & Costs, if only because this is the only official release of a "labor share" series. The BLS nonfarm business sector labor share series, however, suffers from several key weaknesses. The most severe is the imputation for the labor share of mixed income, which has bizarre features documented by Elsby, Hobijn and Şahin (2013) that exaggerate the decline in the labor share. Another limitation is that this is only a gross series, with no allowance for depreciation.

What are the other options? For casual discussion, another popular option is to look at corporate profits, normalized either by corporate value added or GDP. This is intended to be the complement of the labor share, and at face value, it addresses the main weaknesses of the BLS series: it excludes proprietors' income altogether, and it is net of depreciation. But NIPA profits bring their own weaknesses. One is that the most common series includes profits from foreign investment, but we have no corresponding figures for labor compensation or value added.

Another much more serious—and almost entirely unknown—problem is the treatment of inflation. Profits are net of nominal interest payments, and ignore the real capital gain from inflated-away debt. Traditionally, this downward bias in profits is offset by an upward bias from using nominal historical costs to measure depreciation, but this offset is not present in NIPA, which uses current costs instead. The consequence is a large downward bias in profits whenever there is substantial inflation—and in the US time series, this means an exaggerated decline centered around the 1970s and 80s.[1]

2.2 The best measure: net shares of domestic corporate factor income

In short, the labor share should be as simple concept, but without an in-depth understanding of the national accounts, we can quickly be led astray. Amid the complexity, is there a standard measure of the labor share that we can use as our first pass at the question? Yes. The single best measure is the *net labor share of domestic corporate factor income*. This measure divides labor compensation by the sum of labor compensation and net operating surplus for the domestic corporate sector. This excludes proprietors' income, excludes depreciation, and is unaffected by the split between capital income accruing to debt vs. equity. It also excludes income from foreign investments, which has no labor income counterpart and is conceptually not part of the domestic production function.

Two potentially serious biases remain, but we can take some consolation from the fact that they point in opposite directions. First, restricting to the corporate sector no longer means that we avoid all mixed income: as Smith, Yagan, Zidar and Zwick (2017) show, the remarkable rise of S corporations, which have a tax incentive to shift labor income to profits, has likely biased downward the trend in corporate labor share. On the other hand, in a world of tax havens and profit shifting, excluding all foreign profits is not really the right choice, and will miss some capital income that actually corresponds to domestic production.[2] An important ongoing task for research is to quantify the magnitudes of each bias.

When we look at this measure of the labor share, what do we see? Figure 1 shows its complement, the non-labor share, which will be more natural once we begin to do accounting. Surprisingly, the postwar trend is ambiguous: non-labor income is high now, but it was also high in the 50s and 60s. Moreover, its rise has been recent and sharp, having occured entirely since 2000.

What accounts for this U-shaped pattern? If, as in many macroeconomic models, we interpret the non-labor share of income as accruing to capital, it is natural to start our analysis by looking at the capital intensity of the corporate sector, as measured by the capital-income ratio. Indeed, conditional on a neoclassical production function, if there are no factor-augmenting technology shocks, the capital share should move one-for-one with the capital-income ratio—positively if the elasticity of substitution is greater than one, and negatively if the elasticity is less than one.

Figure 2 displays the ratio of capital to net income for the corporate sector.[3] This is a remarkably stable series: the ratio has been close to 200% for the entire postwar era. There is nothing to match the U-shape

[1] In their memorable work on inflation and equity valuation, Modigliani and Cohn (1979) discussed both these accounting biases, although they were not the main focus.

[2] See, e.g., Tørsløv, Wier and Zucman (2018) for recent estimates of the growth in profit-shifting.

[3] For consistency with other figures, in figure 2 we have the ratio with *net* income, as opposed to the more common ratio with gross income. Since figures 1 and 2 are net, we should interpret them in terms of net production function, which, as Rognlie (2015) emphasizes, always has a lower elasticity of substitution than the gross production function. In this case, however, the analysis is similar if figure 2 is gross: the ratio is even more stable over time, meaning that accumulation of capital cannot explain factor income shifts in either direction.

Figure 1: Non-labor share of net corporate factor income

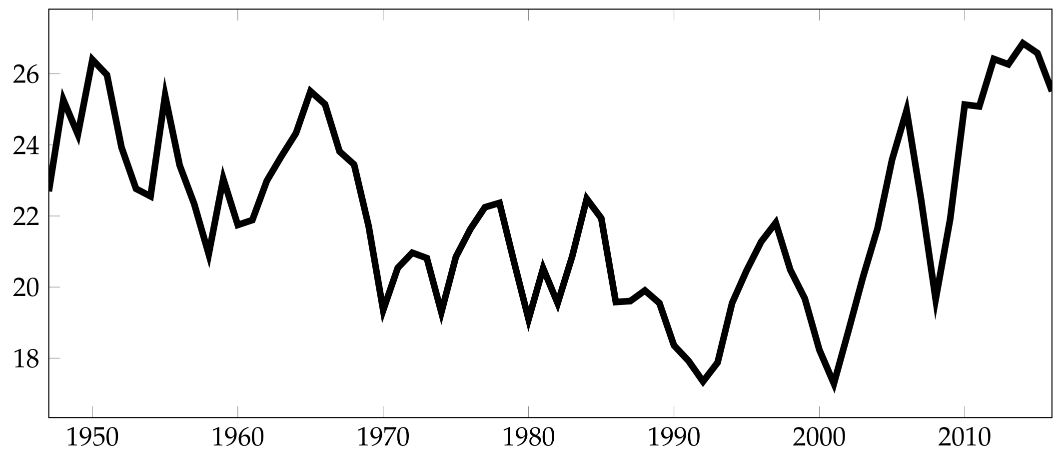
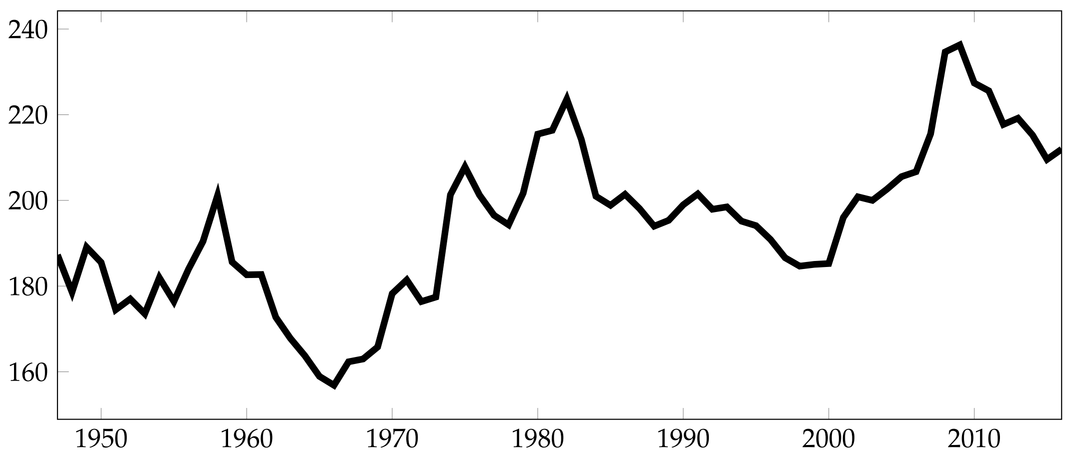


Figure 2: Ratio of fixed assets to net corporate factor income



in the non-labor share, making it difficult to interpret figure 1 directly in terms of a neoclassical production function. Any proposed mechanism for the dynamics of the labor share that works through capital accumulation, like Piketty (2014), is dead on arrival unless the stock of capital unmeasured by figure 2 is large and growing.

2.3 Accounting for capital's share: an enormous residual

The puzzle worsens if we add information on prices. Abstracting from taxes, capital gains, and risk, the net return on capital should be the real interest rate. Since adequate historical data on inflation expectations are not available, this is difficult to obtain. But we can devise a rough measure, as in figure 3: the 10-year Treasury yield, subtracting the lagged 5-year rate of change of the GDP deflator as a proxy for inflation expectations.

This measure of the real interest rate shows an *inverted-U* shape, peaking in the 1980s.[4] This is exactly

[4]The extremely low real interest rates in the beginning of the sample reflect the sharp postwar inflation—arguably a circumstance where lagged inflation was a poor measure of forward-looking inflation expectations. The same inverted-U shape, however, is present even if we throw out the first few years.

Figure 3: "Real" 10-year Treasury yield (lagged 5-year inflation proxy)

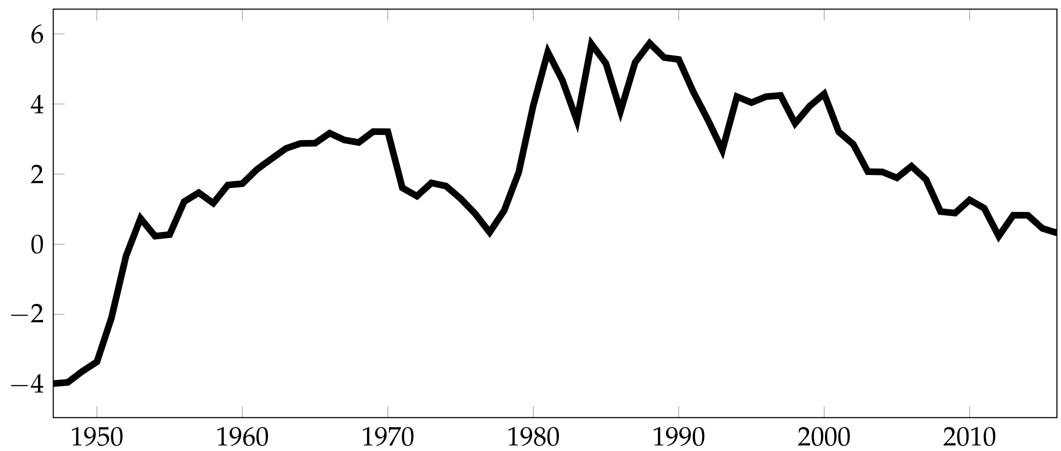
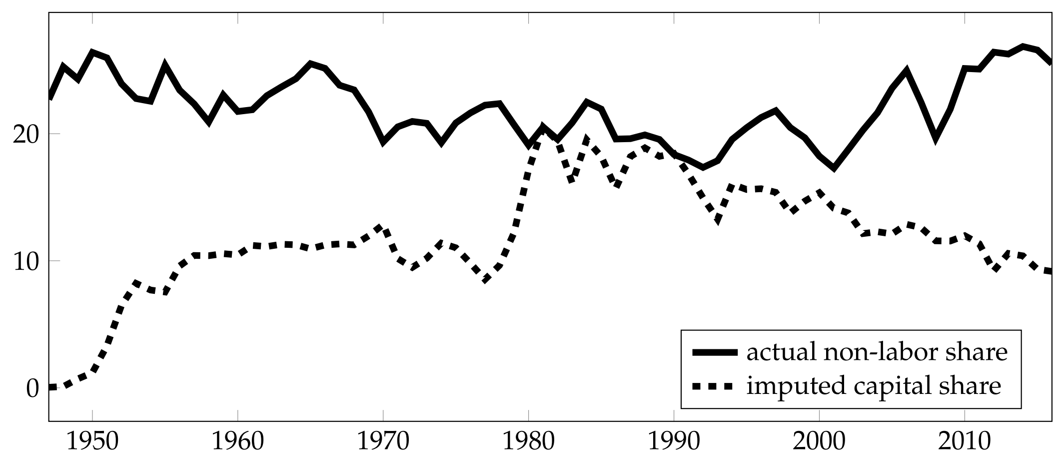


Figure 4: Actual non-labor share vs. imputed $(r + 5\%)$ capital income share

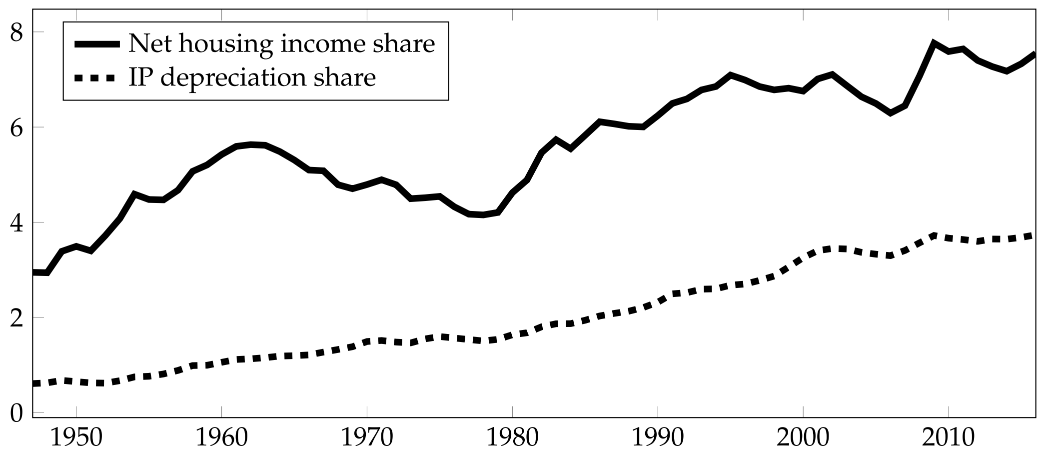


the wrong pattern for explaining figure 1: real interest rates are high when the non-labor share is at its lowest!

We can make this mismatch even clearer. If r is the net return on capital, and $K/(Y - \delta K)$ is the ratio of capital to net income, then their product $rK/(Y - \delta K)$ should be capital's share of income. In figure 4, we compare $rK/(Y - \delta K)$—setting r equal to the rate in figure 3 plus 5% as an ad-hoc equity premium—to the non-labor share from figure 1. With so little movement in the capital-income ratio, movement in r dominates this series, and we obtain an inverted-U for imputed capital income, bearing no resemblance to the actual time series for the non-labor share. This result is robust across many formulations of the equity premium: although we can tweak the average levels, it is impossible to reconcile the dynamics.

It is difficult to overstate the puzzle that figure 4 poses for the usual macro analysis of the labor share. We are accustomed to writing models where capital is either the main or the only factor earning income other than labor. But when we try a simple accounting exercise to reconcile the observed non-labor share with capital's share, we find that the two are completely disconnected. Rather than helping us distinguish between alternative neoclassical stories for the evolving labor share—say, savings versus technological change versus investment prices—figure 4 imperils the common assumptions on which *all* these stories rely. In-

Figure 5: Two unambiguous trends: housing and IP depreciation as shares of aggregate income



deed, the fall and rise of the actual non-labor share, though a worthy puzzle in its own right, pales next to the discrepancy with the imputed capital share.

2.4 Responding to an empirical dilemma

In the face of this seemingly intractable clash between model and data, the literature has moved in two directions.

First, there has been emphasis on other, less ambiguous trends in the factor income distribution. Figure 5 displays perhaps the two most striking: the rise in housing income and the rise in depreciation of intellectual property products. Both these are omitted by construction in figure 1, since net figures exclude depreciation and the corporate sector excludes virtually all housing. But both come up in many other calculations of the labor share—for instance, as Rognlie (2015) documents, housing income is central to the postwar trends in Piketty (2014); and as Koh, Santaeulàlia-Llopis and Zheng (2016) show, intellectual property has played a central role in the evolution of many gross labor share measurements.

These are important clarifications to our understanding. We want to know whether capital income is going to capitalists or to landlords and homeowners; we also want to know if it merely echoing the capitalization of software in the national accounts.

But these clarifications may have, perversely, increased the general level of confusion among macroeconomists. The reason is that amid widespread discussion of a falling labor share, few observers are aware of the pattern in figure 1: that there is *no* postwar trend in the net corporate labor share. If we deviate from figure 1 along one dimension—say, by adding housing income, or depreciation—then we will inevitably find that this deviation "explains" the entire trend in the labor share. Hence we have a proliferation of papers showing that some force or another accounts for a declining labor share: housing in Rognlie (2015), depreciation in Bridgman (2017), or intellectual property capitalization in Koh et al. (2016). There is no contradiction between these findings, all of which are accurate given their respective starting points. But to some extent they all bury the lede, which is that there have been two very sharp, but offsetting, postwar swings in the labor share.[5]

[5]For instance, Rognlie (2015) starts with the accounting in Piketty (2014), which uses a measure of aggregate net income that includes housing; at this point, removing housing eliminates the postwar trend in the labor share, but only because the analysis already excluded depreciation and used a sensible imputation of mixed income. Housing is still an important issue, but not the only

The other recent direction in the literature is to interpret the gap in figure 4 as a measure of economic profit, or markups. Most prominent in this vein is Barkai (2016), who starts in 1984 and takes the trends in figure 1 literally, arguing that the profit share has risen substantially while the capital share has actually fallen. Barkai attributes these trends to a decline in competition, joining a growing literature that ascribes changes in income distribution to market power in some form.

This interpretation, natural enough at first glance, becomes awkward if we look prior to 1984: in figure 4, the gap widens as we look back a few years into the 1970s, and grows vastly larger (even beyond current levels) as we extend the series to the 1950s and 60s. If we interpret the *entire* series using markups, then these markups were enormous in the early postwar era, before contracting and then plummeting in the 1980s, then inflating back to their earlier levels in the 2000s and 10s. This is not an inconceivable sequence, but it does defy the structural explanations (e.g. market concentration) usually put forward for thinking about markups, none of which should have induced such sharp reversals.

3 This paper's contribution

In the face of an increasingly jumbled literature, this new paper from Karabarbounis and Neiman is a beacon of clarity.

The basic idea of the paper is to document the part of aggregate income that we cannot assign to either labor or capital: *factorless income*, using the authors' very clever term. Factorless income is a residual, a much more sophisticated version of the gap in figure 4 between the non-labor share and an imputed capital share. In studying factorless income, the authors must confront the central dilemma of the literature: why is there such a gap between what we observe and what we can explain?

The brilliance of the term "factorless income" is that it is agnostic: it does not presuppose any particular source or explanation. To see why this is so important, one need only look at the existing literature, which has interpreted the same residual in several mutually exclusive ways: as economic profit, as rent on unmeasured capital, and as return premium. Rather than following the usual custom and writing a model in which only one of these three mechanisms is present, Karabarbounis and Neiman start with all three on equal footing, as cases Π (economic profit), K (unmeasured capital) and R (return premium). This is a practice that should be widely emulated.

3.1 A much-needed critique of case Π

The paper's single most important contribution is its critique of "case Π", the interpretation of factorless income as economic profit.

This is a multi-pronged critique. First, Karabarbounis and Neiman show that factorless income has moved substantially in both directions, and that when the exercise of Barkai (2016) is extended backward, it implies very large markups in prior decades.[6] Furthermore, factorless income has moved very closely with the real interest rate. Although it is not strictly impossible that economic profits are responsible for all this, the authors point out that it is very difficult to think of a mechanism that would cause profits to replicate both the long-term fall and rise in factorless income *and* also this tight connection with real interest rates.

one; the author attempted to clarify this by putting the non-labor share's "fall and rise" front-and-center in the paper's title!

[6] To my knowledge, Karabarbounis and Neiman in this paper were the first to publicly make this observation. In a subsequent paper, Barkai and Benzell (2018) also extended Barkai (2016)'s original series backward and obtained similar results.

Second, they highlight the difficulty by calculating a similar "profit" share for housing following Vollrath (2017), and obtaining a similar but even more volatile series. It is unclear what such large and volatile profits can even mean for the housing rental sector, which has little market power relative to the rest of the economy.[7]

Third, they document important caveats to other recent work showing rising markups. They find that it is difficult to replicate De Loecker and Eeckhout (2017), with deviations appearing in the step for handling measurement error. And as Traina (2018) has pointed out, De Loecker and Eeckhout's estimates of a rapid increase in markups are very sensitive to the assumption that cost of goods sold (COGS) in Compustat represents variable cost, and the trend mostly disappears when we use an alternative measure of variable costs that includes sales, general and administrative costs (SG&A). Also important is Edmond, Midrigan and Xu (2018)'s observation that when sales weights are replaced by the more theoretically appropriate cost weights, there is a much smaller rise in markups.

A subtle but crucial point is that De Loecker and Eeckhout are *not* consistent with case Π in any case: their early markup estimates from the 1960s-70s do not match the decline we see in factorless income, while their later markup estimates show a much larger rise than we see in factorless income.

Fourth, and perhaps most innovatively of all, Karabarbounis and Neiman calculate the implied series for labor and capital-augmenting technological change, and show that case Π leads to enormous fluctuations. In part, these numbers look extreme because the elasticity of substitution is close to one.[8] But in any case, they are large and erratic enough that it seems very unlikely that technology could really be responsible for them.

The key economic insight here is that markups primarily drive a wedge between income and costs, and under reasonable parameters have relatively little effect on the *split* of costs between labor and capital. Although markups do reduce real wages relative to rental rates, this means that markups actually increase the capital share relative to total costs when the elasticity of substitution is less than one. Even if the elasticity of substitution is greater than one, the decrease in the capital share from higher markups is nowhere near what we see in the data, where there is a tight negative relationship between the capital share and factorless income.[9] Without much impact from markups, we are forced to infer very large technological fluctuations to match the data.

Altogether, this amounts to a timely and convincing argument against case Π. It might seem natural to interpret the gap between income and imputed costs as a "markup", but this is not a story that stands up to scrutiny on other dimensions.

3.2 The inevitability of case R

In the end, Karabarbounis and Neiman conclude that their case *R*—a deviation of the rental rate of capital from the usual user cost formula based on bond returns—is the most promising. Their approach is careful and methodical, and the critique of case Π is much-needed, but to many of us (including, I suspect, the

[7] On the other hand, long-lived assets like housing are especially sensitive to expectations of capital gains, which might be difficult to impute properly—so there is reason to expect that this series would be especially erratic.

[8] In the limit, as we approach an elasticity of substitution of exactly one, these blow up to infinity, since shares become independent of labor or capital-augmenting technology under Cobb-Douglas. Shocks to the production function could still produce changes in shares, but the shocks would have to affect the share parameters directly, which the authors do not consider.

[9] Holding the rental rate of capital constant, one can show that the elasticity of the capital share of income with respect to markups μ is $-\sigma$, where σ is the local elasticity of substitution. The elasticity of the cost share of income with respect to μ is -1. If rising markups primarily cause a decline in the capital rather than the labor share, it must be that σ is close to α^{-1}, the inverse of the capital share α of costs. Since α is a small fraction, this requires σ to be far above 1, contradicting almost all empirical evidence.

authors themselves) this is not a very surprising endpoint. The financial world is a *lot* more volatile than the macro world: bond yields and asset prices bounce around much more than the labor share or the capital stock. When we mix financial series with macro ones, we're bound to find big residuals, and these residuals will be too large and variable to admit any consistent macro interpretation.

Some form of case R, therefore, seems inevitable, and should really be the starting point for future work. This is not to dismiss the authors' contribution, which is a necessary step given how case R has been set aside in so much recent work in favor of case Π. But it does make the analysis of case K somewhat less useful. It is clearly impossible to explain the factorless income series entirely using unmeasured capital—but at the same time, it's quite possible that unmeasured capital has made a large long-term contribution.

Indeed, as the authors acknowledge, it may well be that some combination of these three forces—fluctuations in markups, unmeasured capital, and the gap between user costs and bond returns—is needed to really understand factorless income.

3.3 Understanding the reasons for case R

Since case R is the most compelling, it's also an important object of study. Why exactly is the non-labor share so disconnected from bond yields, or other finance-based measures of the cost of capital like equity valuations? While this disconnect is no surprise to anyone who has looked at the data, it's still not something that macroeconomists have fully modeled and understood. I suspect that capital adjustment costs, as usually calibrated, cannot come close to explaining it.

One possibility for this gap is an equity risk premium. As recent work by Farhi and Gourio (2018) shows, this improves our ability to account for capital income over the medium run, but I am skeptical that it is enough to explain case R entirely. Equity valuations are quite volatile, and have less impact on high-frequency investment decisions than theory would suggest. Expected equity returns should have been quite high in the late 1970s and early 80s, given low valuations, but this is precisely when the capital share was also low. Another challenge for this view—at least from a modeling perspective—is that when the price of investment goods is pinned down by technology, it is hard to explain why there should be a risk premium on investment at all, unless adjustment costs or other frictions allow for large temporary declines of capital prices below replacement cost.

An important test for this hypothesis is coming up. If equity valuations continue to rise while bond prices fall, the implied ex-ante equity premium will shrink. It will be very interesting to see whether this coincides with a decline in factorless income.

My guess is that there is a deeper lack of transmission between financial-market rates of return and the real rates used to make investment decisions. To some extent, this is already known: it is well-established that corporate hurdle rates are usually far above stock or bond returns. Could this gap expand or contract over time, leading to variation in factorless income? Perhaps the rise of shareholder payouts and corporate "capital discipline" over the last few decades has propped up the required return on capital, even in the face of declining financial market returns. This hypothesis sounds similar to markups, but there is an important distinction: markups distort overall production, while high hurdle rates distort capital as an input to production.

3.4 Alternative measures of capital-labor inequality

Fianlly, although the primary contribution of this paper is its study of factorless income, it also innovates in its measurement of capital-labor inequality. Usually, we talk about inequality in terms of income shares: some fraction goes to "labor", and some fraction goes to "capital". This is ambiguous on its face—for instance, it is not clear whether we should be measuring gross or net capital income.

If we care about consumption inequality or wealth accumulation, net income is a more natural measure, since capitalists can neither eat nor save depreciation. But this is still imperfect. Imagine two worlds: one in which "capitalists" earn net income at a rate of 6% on assets worth 200% of GDP, and the trend growth rate is 0%, and another in which they earn the same amount but the growth rate is 3%. Capitalists can sustain consumption equal to 12% of GDP in the first world, but only 6% of GDP in the second, where they are investing half their income so that the capital stock keeps up with economic growth.[10] Despite identical income shares, consumption inequality is much higher in the first case.

At a deeper level, the capital income share is a strange measure of inequality: capital is just an intermediate good that contributes to production with a time lag. Why we should we give such special treatment to income from this part of the production process? In their counterfactual exercises in section 4.2, Karabarbounis and Neiman try what I think is the best alternative: looking directly at the impact on consumption instead.

Unfortunately, though this is an important step, the authors are limited by the assumptions needed for the tractability of their overall analysis: perfect foresight, infinitely-lived capitalists and a small open economy. With these assumptions, the slope of capitalists' consumption is pinned down by the tax-adjusted exogenous real interest rate. This slope is unaffected by shocks to the income distribution at any date, which are anticipated under perfect foresight and smoothed across all periods. It is impossible, in this framework, to trace the connection between the time path of income shares and the time path of relative consumption.

To carry out this exercise, we probably need to modify the model on several dimensions: replace perfect foresight with some other model of expectations; replace capitalists with a household side that features richer heterogeneity and less-than-infinitely-elastic long-run savings; and drop the small open economy assumption in order to endogenize the real interest rate. This is likely to be messy, and far beyond the scope of the current paper, but it will build upon the excellent ideas here.

4 Conclusion

Karabarbounis and Neiman have produced *the* state of the art paper on factor income shares, and should be applauded. Unlike many papers in this genre, they do not go all-in arguing for one conclusive mechanism; the message is more subtle, steering us away from poorly-fitting stories like case Π and toward more promising avenues like case R. This might not be as viscerally satisfying as a paper that promises a decisive answer, but it has the great virtue of being correct, and it will prove an indispensable foundation for the coming literature.

[10] Or, perhaps, in the second world saving is done less by a capitalist dynasty, and more by wage-earners that are constantly replenishing the stock of assets. In this case, we would probably also be less concerned about the capital-labor split.

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