

The Distribution of Capital Gains in the United States

This paper uses prior estimates (national income distributions from Piketty, Saez, and Zucman 2018) and methods (capitalization) to estimate accrued capital gains and the distribution of an ad hoc income measure including those gains. The paper has three main limitations: (1) the accrued-capital-gains-inclusive income definition double counts income, (2) it is not a Haig-Simons income measure, and (3) the paper's underlying national income data and tax-rate estimates have significant known issues that are unmentioned and unaddressed.

1) Income measure double counts most capital income

Adding capital gains to national income double counts many profits, even after removing corporate retained earnings (as done in this paper). National income is designed to avoid double counting income. The Canberra Group (2021) report on national accounts explained why capital gains represent double counting: "If the value of a share increases because of the increased performance of the company concerned, the increase in the share will be related to the increase in dividends expected in the coming years. To count both as income would be to count the same income flow in two periods."

If one wants to shift from national income to Haig-Simons income including real accrued capital gains, then one needs to address the Canberra critique by removing other capital income besides retained earnings, such as current-year dividends, passthrough business profits, and housing imputed rents. This is because accrued gains essentially bunch profits (losses) from other years into the current year, giving a volatile measure (for individuals and over the business cycle) that is hard to interpret relative to standard measures like fiscal income or national income.

A simple example illustrates how capital income is double counted. Consider a new firm with zero net profit and zero value. If profits increase to \$1 per year and the discount rate is 0.2, the present discounted cash flow valuation increases to \$5, giving an accrued capital gain of \$5. But that gain is due to the subsequent profits of \$1 in all the following years, which are still included in this paper's ad hoc income definition and therefore result in double counting. There is no straightforward way to identify exactly which accrued gains correspond to which changes in profits to correct for this double counting, but the double counting is obvious.

This critique is not unique to real-income valuation changes but also applies to interest-rate changes (a limitation acknowledged but not addressed in the paper). Continuing the example above, assume the interest rate falls from 0.2 to 0.1 and the present discounted value increases to \$10, another accrued capital gain of \$5. The valuation change is from a larger inclusion of future profits in the present. This interest-rate change also represents double counting of capital income. For related discussions see Moll (2020), Cochrane (2020, cited in the paper), Greenwald et al. (2023), and Auclert et al. (2025).

The authors mention these issues but respond by ignoring the double counting or changing the method for fixed-income assets, which yields negative accrued gains. Regarding real-income changes, the paper admits that "If all changes in asset prices were from changes in expected returns, it would not be appropriate to include capital gains as part of the income measure." Regarding interest-rate changes, the paper says: "A rentier owning a consol that yields a real

income stream of \$100 per annum, who does not plan to sell, is neither better nor worse off if a decline in interest rates increases the security's market value. *The income effect of a change in interest rates may be completely independent of the change in asset value for the holders.*" [Italics added]

These examples highlight how changes in estimated wealth (i.e., accrued capital gains) do not give a meaningful measure of annual income. Accrued capital gains can be thought of as "paper" income resulting from timing shifts, not "economic" income like national income. National income combines real economic flows from labor and capital and therefore parallels actual consumption. But this paper creates an accrued-gains-inclusive measure that on average exceeds national income by about 20%, which breaks the link between income and consumption. The extreme volatility of this measure also raises red flags: in 2007 accrued-gains-inclusive income falls 25%, far more than any reasonable measure of changes in living standards using standard approaches.

2) Not measuring Haig-Simons income, which requires removing taxes & adding transfers

The paper says Haig-Simons income "is measured as consumption plus change in wealth, or equivalently (as in this paper) factor income plus [accrued] capital gains." But those two measures are not equivalent. As explained in Larrimore et al (2021), Haig-Simons income is necessarily a post-tax and post-transfer measure if it is consumption plus net changes in wealth. Consider someone with no labor or capital income or wealth in period one or two who consumes from transfers, this person's consumption far exceeds their factor income plus accrued capital gains (which is zero). Now consider someone with no wealth and \$1 million of labor income, half of which is paid in taxes and half of which is consumed: their consumption plus change in wealth is only half of their factor income plus accrued capital gains. These two examples show that this paper is not measuring Haig-Simons income, which requires removing taxes and adding transfers.

3) Issues with baseline Piketty, Saez, and Zucman (2018, PSZ) national income estimates

The introduction provides no indication that this paper is extending the problematic estimates of PSZ. Many limitations of those estimates were highlighted by Auten and Splinter (2024), such as conflating pension flows with pension wealth and allocating misreported income by reported income, which allocates much more income to the top than the audit studies used to estimate misreported income for national accounts (Auten and Langetieg 2023; IRS 2024; Auten and Splinter 2025). These issues tend to exaggerate high-income accrued capital gains in this paper.

4) Effective tax rate estimates deviate from standard measures

The effective tax rate estimates in this paper do not apply the standard approach and are instead biased in ways that tend to flatten tax rates over the income distribution. Standard tax rates include transfers in the income denominator and deduct refundable tax credits from the tax numerator. All standard estimates of average tax rates—CBO, JCT, Tax Policy Center, Treasury, Larrimore et al. (2021), and Auten and Splinter (2024)—use measures of expanded pre-tax income plus transfers to better capture the potential tax base than that in the PSZ approach, which ignores all transfers (and misclassifies refundable tax credits). Splinter (2020) showed that standard measures give similar results, but that the PSZ approach used in this paper results in flat tax rates across income groups that contrast with all these standard estimates. In that PSZ

approach, bottom 10% tax rates are extremely high because they included sales taxes in the numerator but none of the transfer income used for those purchases in the income denominator, which is why Saez and Zucman (2019) dropped the bottom 10% (as showing them would raise concerns about the method). This paper hides those exaggerated low-income tax rates by only showing the bottom 50% as one aggregated group.

5) Fixed-income assets and removing inflation

While this paper uses real rates of return to convert certain capitalized wealth into income, it does not fully remove inflation from the income measure. For fixed-income assets (e.g., interest-bearing bonds), instead of capitalizing and then applying real rates of return, the paper deviates from the standard approach and retains nominal interest income. The paper notes that the standard approach, applying real rates of return to capitalized wealth, leads to negative real accrued capital gains for fixed-income assets. But that is no excuse for selectively deviating from the main approach, these are real losses compensated for by lower risk.¹ Alternatively, the income measures should remove the inflationary component of nominal interest income in the baseline data. Steuerle (1985), which is cited by this paper, makes this same point and was used by Auten and Splinter (2024) to motivate the removal of inflation from nominal interest income. The deviation for fixed-income assets is especially surprising given the paper's focus on heterogeneous returns, and that fixed-income were the original income source for which heterogeneous returns were highlighted (by SZZ and Fed researchers).

The paper's choice of inflation index (the Net National Product price index) is also not clearly the appropriate measure of inflation for indexing accrued capital gains. Because the gains are from asset-price changes, those changes should be reflected in the measure of inflation, but the index used in this paper ignores them. Ignoring asset-price changes for the inflation adjustment is clearly problematic for housing. If my house price doubles and I want to move to another house nearby, but all those house prices also doubled, then the effective inflation rate is 100%. That is, my real wealth would be unchanged and real accrued capital gains would be zero.

6) Unclear contribution

It is not clear how this paper advances the contributions of prior work using tax data. First, this paper suggests its main contribution is estimating deviations of capitalization factors from the assumption of equal-returns. But this general idea was already addressed in Smith, Zidar, and Zwick (2023, SZZ), which is not cited in this paper until page 13 and then only in a footnote. Moreover, this paper discards the heterogeneous returns for interest-bearing assets emphasized by SZZ by deviating from its approach only for fixed-income assets. Larrimore et al. (2021) already introduced heterogeneous tax rates for owner-occupied housing (this paper essentially copies their method but does not acknowledge that) and implicitly introduced heterogeneous returns for passthrough businesses with a match to the Survey of Consumer Finances. Second, Larrimore et al. (2021) already estimated the distribution of Haig-Simons income and it is unclear how this paper improves on that work.

¹ Real pre-tax losses can also result from lower nominal returns for the tax-exempt interest from certain government debt. As higher-income groups disproportionately hold debt yielding tax-exempt interest and these tend to have lower nominal returns, heterogeneous returns for fixed-income assets means inflation adjustments are a larger share of interest for higher income groups.

The paper misses straightforward points already made in Larrimore et al. (2021) about the issue of within-jurisdiction variation in property assessment ratios. This paper only mentions that “richer individuals face lower assessed value-to-market value ratios within taxing jurisdictions.” But Larrimore et al. (2021) provide a more nuanced discussion, raising issues of tiered property tax rates, e.g. fixed dollar exemptions from property values that benefit lower-wealth individuals and lead to richer individual facing higher (not lower) ratios. They also discuss tenure-based horizontal inequities in assessments, such as from California’s proposition 13.

7) Exaggerated high-income passthrough accrued capital gains

For passthrough businesses, this paper estimates wealth-to-income ratios (capitalization factors) using tax-return-reported incomes. But then for its main measures, it applies these factors to PSZ estimated national accounts business income, which is much larger than tax-reported-incomes, due to the addition of accelerated tax depreciation and misreported income. This can cause large exaggerations of high-income accrued capital gains.

8) No clear explanation of reasons for differences with Larrimore et al. (2021)

The current baseline measures of Haig-Simons income using tax data are from Larrimore et al. (2021), which estimates top 1% Haig-Simons income shares of about 15% in the 2000s with a spike to 22% in 2008, but this paper has top 1% Haig-Simons income shares of about 18% in the 2000s with a spike to about 23% in 2005, three years earlier. The paper hints at heterogeneous passthrough business returns being the reason for differences but never gives a more complete comparison with Larrimore et al. (2021), a paper estimating the same thing with the same methods and same data.

9) Do the selected years exaggerate results?

Robbins (2018) estimated accrued capital gains were only be 8% of national income, but this paper estimates they are about 20% of national income. What explains the large increase when switching from 1980-2015 to 2002-2021? In 2021, there was also a one-year surge in capital gains realizations. This temporary spike increased the top one percent income share by 1 percentage point for just one year and affects the underlying PSZ data, which uses capital gains realizations to allocate many forms of capital income. The inclusion of 2021 could introduce bias, which is spread across a few years of estimates due to the 5-year smoothing.

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