Reply to Piketty, Saez, and Zucman (2026): Income Inequality in the United States

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Abstract

National income inequality estimates using tax data must allocate income not reported on tax returns. Starting with estimates from Auten and Splinter (2024), a comment by Piketty, Saez, and Zucman (2026) adjusts the allocation of this missing income. This reply shows that their adjustments rely on reported-income distributions poorly matched to missing income. Whereas the comment scales up reported or overall income even when evidence implies a different distribution, we use source-specific evidence to allocate missing income. For example, our approach is more consistent with the empirical distributions of partnership and S-corporation depreciation and the distribution of underreported income in IRS audit studies. With these data-based estimates and sensitivity ranges, the recent top 1% income share changes by only –0.4 to +0.3 percentage points relative to our baseline.

Keywords: Income inequality, wealth inequality, tax data, income underreporting, tax audits
JEL: D3, E01, H2, H5, J3

This paper updates Auten and Splinter (2025a). *Auten*: This research was conducted while the author is an employee at the U.S. Department of the Treasury. The findings, interpretations, and conclusions expressed in this paper are entirely those of the author and do not necessarily reflect the views or official positions of the U.S. Department of the Treasury. *Splinter*: This paper embodies work undertaken for the staff of the Joint Committee on Taxation, but as members of both parties and both houses of Congress comprise the Joint Committee on Taxation, this work should not be construed to represent the position of any member of the Committee.

The distribution of national income does not correspond neatly to the distribution of income reported on individual tax returns. Roughly 40% of national income is not reported on tax returns, and several national-income components differ from their tax-return counterparts. Taxes and government spending must also be allocated to individuals. Auten and Splinter (2024a, AS) and Piketty, Saez, and Zucman (2018, PSZ18) both allocate these missing components but differ substantially in how some are distributed. Different conclusions about income inequality largely reflect these allocation and incidence assumptions. In their most recent comment on our estimates, Piketty, Saez, and Zucman (2026, PSZ) discuss differences that they argue explain most of the gap between the two studies. This paper responds to that comment.[1]

Our view is that methodological differences should be resolved, wherever possible, using evidence closely related to the income, deductions, taxes, or spending being allocated. Compared to PSZ's scaling up of reported income to allocate missing income, more direct information for these missing components is available from administrative data, audit studies, and incidence evidence. While these sources do not perfectly measure every missing component, they are more informative about its distribution.

The comment repeatedly describes proportional allocations to a reported-income base as "neutral," but proportionality is itself a distributional assumption. For example, PSZ describes as "neutral" allocating consumption taxes proportionally to pre-tax income, certain government spending proportionally to after-tax income, and deficits through a similar proportional rescaling of after-tax income. The same issue arises more generally when missing business income is allocated by reported business income: the distribution of a specific missing component, such as excess depreciation or underreported income, need not follow the distribution of reported net income. Proportional scaling can reconcile income totals but preserves the reported-income distribution, adding little new information about missing income. Rather than treating proportional allocations as neutral, we use source-specific empirical evidence whenever possible.

We consider each of the proposed adjustments and conclude that they do not provide evidence for substantial revisions. In several cases, the comment allocates missing income according to reported income even though the missing component itself lowers reported income, causing reported income to systematically understate true income for its recipients. Accounting for this effect assigns more missing income to lower reported-income groups than an allocation that treats reported income as true income. This is the case for excess depreciation and underreported income, which together account for roughly

[1] This is the seventh critique by PSZ. We incorporated earlier suggestions into our published estimates, see AS supplementary appendix. Prior responses are Auten and Splinter (2019, 2020, 2021, 2025a) and Splinter (2020a, 2023).

half of the comment's total adjustment. Applying more direct evidence produces much smaller effects. PSZ attributes more than five percentage points of the recent after-tax top-share gap to their adjustments, while our alternatives imply a combined effect of less than half a percentage point in either direction.

Excess depreciation provides the clearest example. *Excess depreciation* is the difference between tax and economic depreciation, arising largely from tax provisions such as accelerated depreciation and expensing. Because these tax provisions reduce reported partnership income, that income is a poor proxy for the ownership of excess depreciation. We instead use population tax data that trace partnership depreciation to ultimate owners. The resulting distribution implies the top 1% income-share change of only one-eighth the proposed adjustment. Similar linked S-corporation data imply a modest top-share decrease.

The same issue arises for underreported income and other components. For underreported income, the comment relies primarily on reported business income even though the national-account adjustment itself is based on IRS audit studies that provide direct distributional evidence. Those audit data show a substantially smaller concentration of underreported income at the top than that implied by an allocation based on reported business income.

PSZ argues that we fail to remove consumption taxes, but we explicitly deduct them in computing after-tax income. Moreover, PSZ allocates these taxes proportionally to income, whereas we allocate them to consumers based on standard tax incidence findings. Similarly, for government deficits, the relevant incidence question is who bears the fiscal burden associated with additional debt. Historical and recent evidence that larger debt is ultimately associated with higher tax burdens supports our tax-based allocation rather than a proportional-income allocation.

Table 1 compares PSZ's proposed adjustments to 2019 top shares with our preferred estimates or sensitivity alternatives. For the first three components, linked business-owner data and IRS special audit studies provide direct distributional evidence and imply effects close to our baseline. For the remaining components, available incidence evidence and sensitivity tests likewise imply relatively small effects, with government consumption producing the widest range. For deficits, the historical and recent evidence supports our tax-based allocation. Overall, the PSZ adjustments increase the 2019 after-tax top 1% share by 5.2 percentage points, while our evidence-based alternatives imply an approximate combined effect of only –0.4 to +0.3 percentage points. Together, these alternatives provide little basis for substantial revisions to our estimates.[2]

[2] Figure 4 and AS Table 5 show other sensitivity tests, e.g., full economies of scale would decrease our 2019 top share by 0.9 percentage point. For a benchmark of AS–PSZ differences, see AS Table 4, which also includes re-ranking effects using microdata, controls for sensitivity to the ordering, and considers other differences.

Table 1: Top 1% income-share effects relative to AS baseline, 2019 (pp)

Component	PSZ adjustment to AS	Evidence used in this response	Preferred estimate or sensitivity range
Partnership excess depreciation	1.2	Linked partnership depreciation to owners; microdata re-ranking	0.16
S-corporation excess depreciation	0.5 (0.3 updated data from BEA)	Linked S-corp. depreciation to owners; microdata re-ranking	–0.11
Underreported income	0.8	IRS random-audit distributions; consistent undetected adjustment	0.0
Retirement-account income	0.3	Funded Financial Accounts assets; Form 5498 IRA wealth	0.0 to +0.1
Corporate retained earnings	0.2	Dividend/capital-gains ownership proxies; sensitivity range	–0.05 to 0.0
Consumption taxes	0.8	Consumer incidence; same incidence to add and subtract; standard market-price accounting	0.0
Government consumption (after-tax)	1.5	Benefit-incidence evidence: schooling and sensitivity range	–0.40 to +0.15
Government deficit (after-tax)	0.1	Federal-tax incidence; historical debt financing	0.0
Total after-tax	**5.2**		**–0.4 to 0.3**

Notes: Totals are approximations because estimates are sensitive to ordering. The range is a sensitivity exercise, not a confidence interval. Corp. retained earnings: lower value applies PSZ's 50% dividend weight in AS data and Forbes adjustment. Consumption taxes: consistent incidence when adding and deducting taxes leaves after-tax shares largely unchanged, but PSZ adds the tax proportionally to income and deducts it according to consumption. Government consumption: range corresponds to 75% and 40% per-capita allocations; the 40% lower sensitivity value is motivated by education alone accounting for about 30% of this spending. *Source*: Authors' calculations from PSZ, AS, and Auten and Splinter (2025a).

Beyond these specific allocations of missing income, we address two broader claims. First, contrary to the comment's claim, the evolution of our capital-income concentration is not contradicted by available wealth evidence. Their claim uses capital income for groups ranked by income and wealth for groups ranked by wealth. When the wealth data are instead ranked by income, our top 1% capital-income share and the top 1% wealth share show similar long-run increases. More generally, differences in saving, returns, demographics, and asset valuations (e.g., due to the long-run decline in interest rates) imply that wealth and current capital income need not move one-for-one. Second, PSZ describes their novel measure as the top 1% "share" of a missing income component,

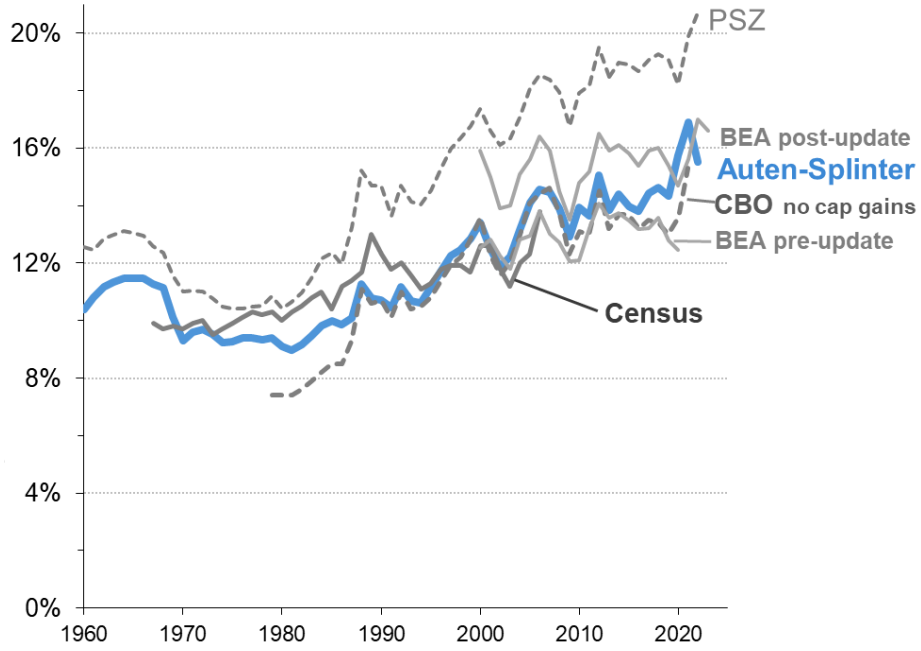
despite the actual share received by the final top 1% often being much larger. This distinction is quantitatively important for excess depreciation and underreported income, for which our final top 1% allocation shares are roughly double the PSZ "share."

More broadly, several of these disagreements reflect different views of how the distribution of income missing from tax returns has evolved. PSZ (p. 20) argues that "to offset the rise in the top 1% [reported] income share, it must be that unobservable income has become much more equally distributed." They suggest this is implausible, but this is exactly what the evidence suggests. The composition of income missing from individual tax returns has changed substantially: missing capital-income shares received by the top 1% *decreased* while missing labor-income shares received by the bottom 90% *increased*. Specifically, between 1962 and 2019, missing capital income of the top group fell from 4% to 2% of national income, largely because corporate retained earnings declined, while missing labor income of the bottom group rose from 4% to 12%, reflecting the growth of tax-excluded items such as employer-provided health insurance and retirement contributions (AS, p. 2188). More comprehensive income measures can therefore show a smaller increase in inequality than income reported on individual tax returns precisely because the forms of income that have grown outside individual tax returns are disproportionately received outside the top 1%.

The compositional change in missing income is also consistent with other studies that move beyond income reported on individual tax returns. As shown in Figure 1, our series is broadly similar to other comprehensive-income estimates, which generally show lower levels and smaller long-run increases than PSZ. It is also incorrect for PSZ (p. 20) to claim that we "make a large number of assumptions that all go in the same direction." In fact, half of our adjustments for pre-tax estimates *increase* recent top shares, as shown in AS Table 1. For example, the comment claims that our size adjustment of income for ranking "mathematically depresses" the top 1% share, but that table shows that it increases both the top share's level and long-run rise. We also reported sensitivity analyses using alternative assumptions that move top shares in both directions. Subsequent estimates consider additional assumptions designed to raise or lower recent top shares; those produce a relatively narrow range and place our baseline estimates toward the top of that range.

Section I examines excess depreciation and underreported income, and Section II considers the other disputed allocations. Section III addresses wealth comparisons, and Section IV explains why PSZ's top-share statistic differs from the actual allocation shares. Section V reports joint sensitivity tests that place our baseline in the upper end of the range.

Figure 1: Pre-tax top 1% income shares across broad-income estimates



Notes: All estimates exclude capital gains realizations. *Sources*: Auten and Splinter (2025b); PSZ with updated methods, Zucman's website March 14, 2023, values for 2020–2022 from realtimeinequality.org Feb. 25, 2025; Burkhauser et al. (2012, Census), BEA (2024b and pre-update accessed Sept. 2024); CBO (2025).

I. Excess depreciation and underreported income

This section discusses shortcomings in the comment's adjustments to excess depreciation and underreported income. Our baseline allocates noncorporate capital consumption adjustments, a measure of excess depreciation, by expensing and depreciation reported on individual tax returns. We allocate underreported income using distributions from IRS special audit studies. PSZ instead allocates these components primarily using reported business income. In their 2024 comment, PSZ said excess depreciation should ideally be allocated using linked owner data; Auten and Splinter (2025a) subsequently did so and found that the linked depreciation distributions have only minor effects on our estimates. The current comment again calls for linked data but does not discuss this evidence.

Note that excess depreciation in 2019 is unusually large, as it coincides with the beginning of expanded tax expensing. But excess depreciation is not permanently tax-exempt income. It just temporarily shifts deductions forward, causing initial adjustments that are reversed in later years. For example, corporate capital consumption adjustments were $95 billion in 2019 but turned negative thereafter, reaching –$271 billion in 2023 and –$368 billion in 2024 (BEA NIPA Table 1.12). Excess depreciation represents only a deferral for tax purposes rather than permanently exempted income, a distinction obscured by the comment's characterization.

1. Partnership excess depreciation

Excess depreciation is tax depreciation in excess of economic depreciation in the national accounts, arising from accelerated depreciation, bonus depreciation, and expensing. Because depreciation reduces reported net income, an otherwise identical business with more excess depreciation reports lower income. The AS online appendix (p. 21) explained that "when allocating by expensing, capital consumption adjustments tend to go lower in the distribution than net business income." We account for these effects using population data that trace partnership depreciation through ownership chains to ultimate owners, following the comment's (p. 7) suggestion that "partnership excess depreciation should be allocated to the ultimate individual owners of the corresponding partnerships." Allocating by expensing on individual returns and then by the linked-owner distribution increases the top 1% income share by 0.16 percentage point, about one-eighth the proposed adjustment.

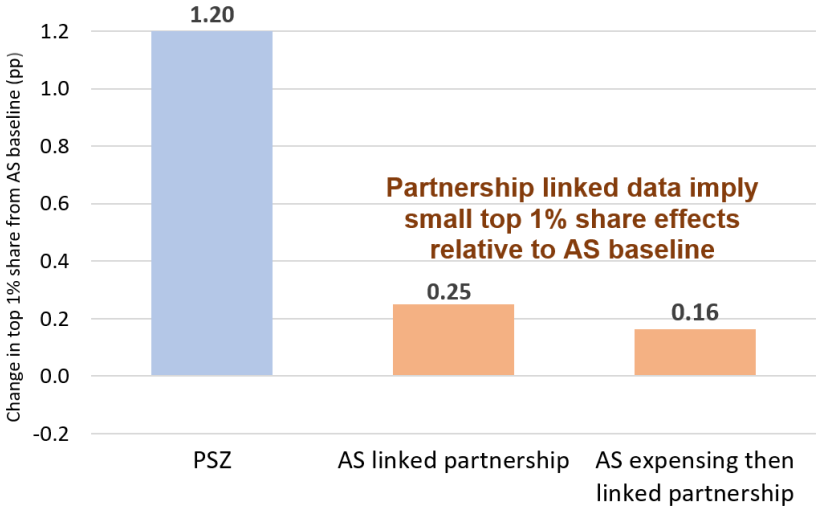
To estimate the top share of partnership excess depreciation, Auten and Splinter (2025a) allocate estimated partnership-level excess depreciation to individual owners using Form K-1 ownership shares.[3] Because excess depreciation tends to lower one's income more than total depreciation, as it concentrates amounts that would have been spread over many years into one year, we apply an assumed 15% excess depreciation re-ranking adjustment, which is validated below. We allocate partnership depreciation to tax microdata by the empirically estimated shares to each adjusted gross income group, and within each group by absolute net partnership income. Before re-ranking, the top 1% of tax returns ranked by reported income receive 33% of this depreciation. After adding excess depreciation and re-ranking, the final top 1% receives 59%.[4] Thus, the microdata top share closely matches the linked data both before and after re-ranking. Note that these are allocation shares and should not be confused with the s_s statistic introduced in the comment, which nets out baseline income displaced through re-ranking.

Figure 2 shows that applying the linked partnership excess depreciation distribution in microdata increases the top 1% income share by 0.25 percentage point. But this allocates too much to partnerships because it ignores bonus depreciation reported on individual tax returns. Our preferred specification first uses directly observed expensing and applies the linked distribution to the remainder, yielding a top 1% share effect of 0.16 percentage point.

[3] Form 1065 line 16c depreciation is traced through up to ten partnership tiers using K-1 ownership shares with data from Love (2026). Love traces nearly all partnership income to owner types and finds that roughly 10% is associated with tax-exempt owners, such as nonprofits and retirement funds, providing another reason not to scale missing partnership amounts by concentrated partnership income reported on individual returns.

[4] To check our excess depreciation top share of 59% after re-ranking, we follow Gabriel Zucman's suggestion to add seven-tenths of excess depreciation to adjusted gross income, yielding a nearly identical top share of 60% after re-ranking.

Figure 2: Top 1% share changes relative to AS from partnership excess depreciation, 2019 (pp)



Notes: AS estimates use tax microdata and include re-ranking. *Sources*: PSZ, Auten and Splinter (2025a).

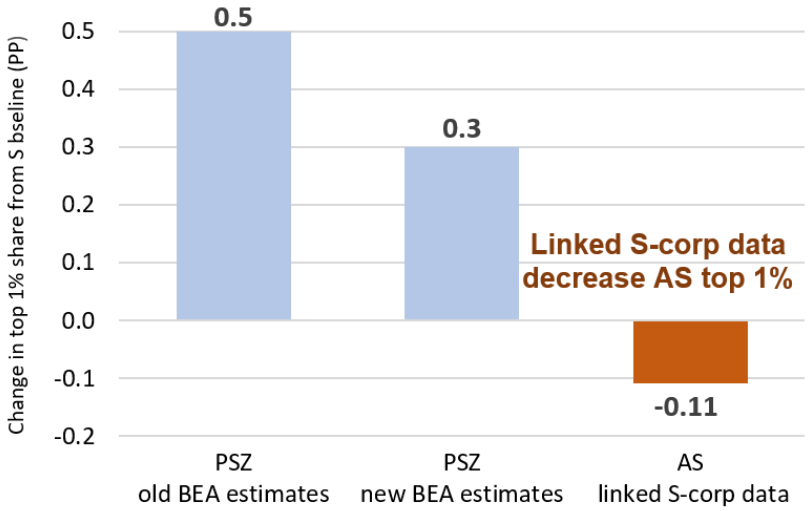
2. S-corporation excess depreciation

The official national income and product accounts do not report corporate excess depreciation separately for S corporations and C corporations. Therefore, we allocate corporate income gaps (i.e., excess depreciation and retained earnings after underreporting) by estimated C corporation ownership shares. Corporate investment is disproportionately undertaken by C corporations, suggesting this allocation was a reasonable approach. In 2011, for example, C-corporation depreciation was seven times larger than S-corporation depreciation (IRS corporate return tables, pp. 17, 129).

PSZ points to prototype estimates in Krakower et al. (2021) to motivate a second adjustment to our estimates. However, those estimates cover only six years, compared with the six decades in our series, and have not been incorporated into the official NIPAs as they remain preliminary. We have already noted this issue in AS, where we estimated a much smaller potential effect than the comment's back-of-the-envelope adjustment.

Moreover, the BEA estimates were substantially revised in BEA (2024a), yet PSZ's benchmark still uses the older estimates. In the updated estimates, the S-corporation capital consumption adjustment, intellectual-property expensing, and mining expenditures sum to $148 billion of depreciation-related adjustments, less than half the $311 billion used in PSZ's benchmark calculation. Rather than using the updated estimates, their comment instead infers the 2019 amount indirectly by holding the S-corporation share of aggregate corporate profits fixed, thereby nearly doubling the 2017 prototype amount. They note that the updated BEA estimates reduce their estimated top-share effect from about 0.5 to 0.3 percentage point but retain the extrapolation based on older estimates for their benchmark.

Figure 3: Change in top 1% share relative to AS from excess S corp. depreciation, 2019



Notes: AS uses new BEA estimates. *Sources*: PSZ, Auten and Splinter (2025a).

A separate problem is distributional. PSZ assumes that S-corporation excess depreciation should be distributed by reported S-corporation income, allocating 56% to the top 1%. This overstates the effect on top-income shares because excess depreciation itself lowers reported income. Linked S-corporation depreciation and owner returns provide more direct evidence on the ownership of the tax depreciation underlying this adjustment. The linked distribution is about half as concentrated at the initial top 1% as the PSZ allocation and implies a small *decrease* in our top shares.

To estimate the distribution of S-corporation excess depreciation, Auten and Splinter (2025a) create a ten percent random sample of the population of S corporations in 2019 and allocate S-corporation entity-level depreciation on Form 4562 to individual tax returns in proportion to ownership shares reported on Forms K-1. Because excess depreciation (which is not broken out in the tax data) tends to lower one's income more than overall depreciation, we apply the same 15% excess depreciation re-ranking adjustment as for partnerships. Using this distribution to allocate S-corporation excess depreciation assigns 33% to the initially ranked top 1%. This top share of S-corporation linked depreciation is identical to that for partnerships, providing a useful cross-check on both estimates.

Incorporating this distribution into the microdata results in a 0.11 percentage-point *decrease* in our top 1% income share (Figure 3). To test whether excess depreciation is disproportionately associated with loss-generating businesses, in an alternative approach we allocated part of S-corporation excess depreciation by S-corporation losses and the remainder by the S-corporation excess depreciation distribution. As a result, the top 1% income share decreases by 0.10 percentage point, virtually the same amount. Following the PSZ suggestion to allocate corporate excess depreciation to S-corporation owners therefore slightly lowers our top share when using a more appropriate distribution.

3. Underreported income and special audit studies

In constructing the 2019 national accounts, BEA adds nearly $800 billion of estimated underreported income to tax-reported income. This adjustment represents nearly 5% of national income and is almost entirely based on IRS special audit studies, which also provide direct evidence for its distribution. Underreporting allocations were the largest difference between our estimates and PSZ18, but those comparisons combined excess depreciation and underreporting. The PSZ comment separates these and argues that tax "evasion" (or underreporting) explains little of the trend difference.[5] Yet underreporting still explains roughly one-fifth of the gap between top 1% shares, so it remains meaningful.

PSZ18 allocated underreported business income only to positive reported business income. About 40% of detected business underreporting, however, is on returns with negative reported *total* income, not business-type income (Auten and Langetieg 2023). The original PSZ18 method also pooled underreporting across business types, despite most being associated with sole proprietors, whose income is less concentrated. This allocated some sole-proprietor underreporting by the more concentrated distribution of partnership income. Both choices had the effect of shifting income up the distribution.

PSZ revised their allocation method, although their resulting top-share effect changes relatively little. They now allocate business underreporting separately across business types, first to offset reported business losses and then by positive reported income. Three issues remain. First, allocating underreporting to business-type losses does not address our point that a large share of underreporting occurs on returns with *total-income* losses. Underreporting itself lowers reported income and pushes recipients down the reported-income distribution. Consistent with this point, audit-study data show much lower underreporting rates at the top and a substantially less concentrated distribution than is implied by the PSZ allocation by reported business income (Auten and Splinter 2021). Second, the revised method still shifts sole-proprietor underreporting to partnership income, which is substantially more concentrated in the proposed allocation. Third, because they allocate excess depreciation and underreporting jointly against partnership losses, the losses available to absorb underreporting depend on the excess-depreciation allocation. If excess depreciation offsets most recent partnership losses, relatively little underreporting is ultimately assigned to losses.

IRS audit studies do not perfectly measure underreporting, particularly for complex business structures, but they provide the best direct evidence on the distribution of underreporting and are the primary empirical basis for national-account adjustments. To allocate underreporting, AS uses distributions from IRS special audit studies. These studies are based on stratified random samples that oversample high-income returns and measure

[5] "Evasion" refers only to intentional underreporting for the IRS. The audit data underlying national-income adjustments include all net income misreporting—whether due to errors, disputed tax treatment, or intentional underreporting (Hemel, Holtzblatt, and Rosenthal 2022). *Underreporting* here refers to gross underreporting less overreporting.

detected underreporting based on auditor adjustments (i.e., before any later reductions due to appeals). In recent years, detected underreporting lowers top 1% fiscal income shares by 0.4 to 0.5 percentage point (Gorman, McGuire, and Splinter 2026; Guyton et al. 2021). We use detailed audit-study estimates since 1988 to approximate this distribution.

To account for *undetected* underreporting, detected underreporting is scaled up two to three times, depending on the year, using detection controlled estimation (DCE). Earlier IRS estimates used four simple multipliers that tended to exaggerate inequality and were subsequently abandoned.[6] We instead use distributionally consistent gradient multipliers that depend on the ratio of detected underreporting to reported income. Higher ratios receive smaller multipliers and lower ratios receive larger multipliers, consistent with the DCE logic that less-effective audits require larger adjustments (Auten and Splinter 2021, 2024b). Directly applying these multipliers in the audit data approximates the new IRS (2024) underreporting distribution, though with slightly *less* allocated to the top 1% (Gorman, McGuire, and Splinter 2026).

The national-accounts adjustment uses estimates that have already scaled up detected underreporting for sole proprietors by roughly twofold and for partnerships nearly fourfold (IRS 2022).[7] Thus, our baseline already includes substantial undetected partnership underreporting and at a much higher rate than for sole proprietors. Any additional assumed underreporting is beyond the national-accounts adjustment and outside the scope of national income distributions.[8] The remaining disagreement is therefore not whether undetected partnership underreporting exists, but if one should use audit-study distributions and if underreporting should be shifted from sole proprietors to partnerships.

Other evidence also provides little support for assuming that undetected underreporting is highly concentrated at the top. The IRS roughly doubled audits of returns with income over $10 million but concluded that they were unproductive, and large-partnership audits had a negative average income adjustment, indicating *over*reporting of income (Treasury Inspector General for Tax Administration 2024; GAO 2023). A special S-corporation study similarly concluded that the audit studies capture sufficient partnership and S-corporation underreporting (AS supplementary appendix). With the audit-study data, Gorman, McGuire, and Splinter (2026) find tax noncompliance rates about four times higher for the bottom quintile than for the top 1%, with the gap increasing over time. Finally, Weisbach and Coleman (2026) note that high-income people who underreport appear lower in the reported-income distribution, so correcting for underreporting does not necessarily increase measured inequality.

[6] See DeBacker et al. (2020), Johns and Slemrod (2010); Hemel, Holtzblatt, and Rosenthal (2022); and GAO (2024).

[7] Source-specific underreporting rates also decline with income: self-employed underreporting rates are 70–90% at incomes up to $50,000 but below 20% above $3.3 million, while average S-corporation and partnership rates are only about 12–18% (IRS 2022; Gorman, McGuire, and Splinter 2026).

[8] AS estimated modest effects from adding undetected offshore underreporting. See Section V for other missing items.

II. Other allocations

1. Corporate retained earnings

The U.S. has no registry of stock ownership that links C-corporation equity to individual stockholders, so researchers rely on ownership proxies to allocate retained earnings. Our approach is to allocate retained earnings using a fixed weighting of dividends and realized capital gains observed on individual income tax returns. PSZ18 also used these sources, but generally placed less weight on dividends than our fixed 75% weight, while relying more on realized capital gains. This creates two concerns: lumpy gains realizations distort individual rankings because gains accrued over many years may be realized in a single year, temporarily moving them up the distribution and overstating top corporate ownership, and realizations can respond temporarily to policy changes (Auten, Splinter, and Nelson 2016).

The comment's updated approach partly addresses these issues by adopting fixed weights and smoothing realized capital gains over several years, but places only a 50% weight on dividends. Our estimates use 75%, consistent with the Smith, Zidar, and Zwick (2023, p. 549) finding that tax data "strongly support placing substantially more weight on dividends when capitalizing flows to estimate C-corporation wealth." The comment responds that this evidence concerns corporate-equity ownership rather than retained earnings. But we use dividends to proxy who owns corporate equity, not as an assumption that high-dividend firms retain more earnings. In practice, this choice matters little quantitatively: our sensitivity tests using dividend weights of 50% and 100% barely change the long-run trend, moving recent top shares by at most about 0.2 percentage point. Moreover, applying the PSZ approach in our microdata slightly decreases recent top shares.

The updated approach in the comment also uses Forbes 400 data to adjust corporate ownership. It notes that dividend proxies can understate ownership for individuals with large stakes in corporations that pay few dividends and provides evidence for select individuals. But this is most relevant at the extreme top and need not generalize to the full top 1%, where firms with lower and higher dividend payouts can offset one another.[9] There is also a potential overlap: a 50% capital-gains weight already raises imputed ownership for many shareholders poorly captured by dividends, while the additional wealth adjustment targets some of the same owners. The comment's evidence for five individuals in one year does not establish that a 50% dividend weight (plus a wealth adjustment) is preferable to a 75% dividend weight.

[9] For example, ten of the 20 largest S&P 500 companies had above-average dividend yields, averaging 2.8% compared with 1.9% for the index. At least nine heirs or founders of these companies appeared in the Forbes top 100.

2. Pension plan accrued income

For pension income, the comment mischaracterizes our allocation. We use the Federal Reserve Financial Accounts to determine the share of corporate ownership held through retirement accounts and then allocate the *funded* defined-benefit share using earned income and taxable retirement distributions, thereby accounting for both current workers and retirees. PSZ (p. 13) says this income is allocated "proportionally to funded and unfunded pension wealth." But the actual allocation uses Financial Accounts measures of funded assets only.[10]

This claim appears to conflate this allocation with an external-validation exercise, where we compare our pension distribution with that of the Federal Reserve's Distributional Financial Accounts (DFA). But the DFA is not used to allocate pension income. The comparison is also less precise than the comment implies. The DFA and tax-data income rankings differ because their income concepts, units of observation, and reporting differ (Bricker et al. 2016; Bhandari et al. 2020). Moreover, pension wealth is not directly observed in the survey data underlying the DFA. Instead, it is imputed using current and expected pension benefits, wages, and other characteristics, together with assumptions about discounting and mortality (Sabelhaus and Henriques Volz 2019). Variation in assumed discount rates alone can produce substantial changes in estimated wealth distributions (Catherine, Miller, and Sarin 2025). The DFA distribution is therefore a guidepost for validating our estimates, not a directly observed or exactly comparable distribution of pension assets.

More importantly, the comment's adjustment reflects differences beyond pension funding. It is based on all untaxed retirement account income, from both defined-benefit pensions and defined-contribution accounts, which account for roughly equal shares of retirement assets. PSZ highlights an estimated 8.3% top share of funded pension wealth in the DFA but then applies a 10.3% top-share statistic s_s from their broader retirement-account allocation—60% based on taxable pension distributions, 30% on certain wages, and 10% on nontaxable pension distributions. But as shown in AS, nontaxable distributions are a poor proxy for retirement-account income because they are largely rollovers, which transfer stocks of wealth between retirement accounts rather than represent flows of income earned by those assets. Thus, the resulting 0.3 percentage-point difference cannot be attributed specifically to defined-benefit pension funding.

The comment uses the DFA comparison where it suggests raising our top pension allocation. Yet the same evidence points in the opposite direction for defined-contribution accounts: we assign 10.1% of defined-contribution wealth to the top 1%, compared with 9.7% in the underlying benchmark used in constructing the DFA. Closing that difference

[10] The corporate pension share primarily uses Financial Accounts' corporate equities and mutual-fund shares held as pension-fund assets (FL593064105 and FL593064205), which exclude unfunded portions of promised pensions.

would modestly reduce, rather than increase, our top share.[11] Applying this small downward adjustment and, illustratively, allowing unfunded pension claims to accrue to the top 1% at half the rate of the rest of the distribution reduces PSZ's proposed adjustment to roughly 0.1 percentage point. More generally, these comparisons highlight substantial uncertainty in the pension allocation.

3. Consumption taxes

National income is a pre-tax income concept and therefore includes corporate taxes, employer-paid payroll taxes, and consumption taxes. Because consumers generally bear the incidence of consumption taxes, we allocate these taxes using disposable income less savings, which approximates annual cash flow available for consumption after taxes and transfers. PSZ instead scales incomes proportionately, which differs from a standard incidence-based allocation and from the PSZ18 approach.[12]

A tax should have the same incidence whether it is added to construct pre-tax income or deducted to construct after-tax income. Consumption tax allocations should therefore have little effect on after-tax income shares. As noted by PSZ18 (p. 570): "the incidence of sales taxes make[s] little difference to the level or trend of our income shares." The comment uses different allocations, deducting consumption taxes by consumption (based on standard incidence) but adding consumption taxes proportional to income (ignoring standard incidence). This results in a large increase in top 1% shares.

Instead of acknowledging this difference, PSZ (p. 16) claims that our after-tax income measure "does not remove consumption taxes." But we explicitly deduct them. Consumption taxes (t_c) are first added to fiscal income (y_f) because national income at market prices (y_{NI}) includes taxes on products: $y_{NI} = y_f + t_c$. They are then explicitly deducted in constructing after-tax income: $y_{AT} = y_{NI} - t_y - t_c + g = y_f - t_y + g$, where t_y denotes other taxes and g transfers. PSZ defines consumption (c) at prices exclusive of consumption taxes, so their household budget identity is $c + t_c = y - t_y + g$. Under this convention, our after-tax income equals $c + t_c$: not because the tax was omitted, but because we retain the market-price valuation embedded in national income, whereas PSZ makes an additional conversion to a net-of-product-tax valuation.[13] In contrast, our approach is consistent with national accounts, which value household final consumption expenditures at prices including taxes paid by consumers (OECD 2025).

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[11] The Survey of Consumer Finances is the benchmark data used in constructing the DFA. We capture the increasing concentration of IRA wealth: the top 1% IRA share rises from 7.8% in 1989 to 9.9% in 2018.

[12] PSZ18 (p. 570) allocated sales and excise taxes proportionally to factor income minus saving "so that they are borne by consumers." AS (online appendix p. 34) notes that this proxy "ignores transfers, retirement income, and taxes," understating regressivity. The new PSZ proportional scaling is even less regressive because it no longer subtracts savings. Contrary to PSZ's (p. 16) claim, AS and PSZ do not use the same incidence assumption.

[13] PSZ's terminology has shifted. PSZ (p. 16) says their original estimates "attempted to distribute total national income instead of basic-price national income." PSZ (2024) then defines national income at factor ("basic") prices, excluding $900 billion of taxes in 2019, while the current comment includes these taxes in market-price national income.

PSZ's proportional scaling is not "neutral" and does not avoid an incidence assumption. If T_c is aggregate consumption taxes and Y_{net} is net-of-tax-price national income, scaling y by $(Y_{net} + T_c)/Y_{net}$ is mathematically equivalent to allocating consumption taxes proportionally to income. It therefore allocates consumption taxes proportionally to income for pre-tax income but deducts them by consumption for after-tax income. For example, if two households receive 20% and 80% of income but account for 30% and 70% of consumption, a $100 tax is allocated $20/$80 under proportional scaling but $30/$70 by consumer incidence. The PSZ scaling therefore raises the pre-tax income concentration relative to a standard consumer-incidence assumption.

4. Government consumption

Government consumption includes non-transfer government spending and must be assigned across households if the objective is to distribute all national income, but as noted by AS, its incidence is uncertain. PSZ first says they "leave out" government consumption from their allocation but then clarifies that they allocate it proportionally to after-tax income. However, an income-only allocation ignores "the redistributive and public goods aspects of government consumption captured by our half per capita allocation" (AS, p. 2211). We therefore allocate government consumption half per capita and half by after-tax income as a baseline intended to capture both public-good and income-related benefits. Our sensitivity tests show little impact of alternative assumptions on long-run changes in top 1% after-tax income shares.

Education accounts for about 30% of government consumption and this spending is roughly flat over the income distribution (Riedel and Stichnoth 2024). That provides direct support for a substantial per-capita component. PSZ acknowledges a reason for allocating education spending more equally but argues that no other non-transfer government spending "could in principle" be allocated per capita. But that claim is too strong: a per-capita component captures the public-good and redistributive aspects of other government consumption, as reflected in our half-per-capita allocation.

Our assumption follows the standard recommendation to allocate non-transfer government spending partly by income and partly per capita (Reynolds and Smolensky 1977; CBO 2013). Gethin (2026) similarly argues that allocating government consumption entirely by income, as in PSZ, "dramatically overestimates inequality in public services received," while using only a per-capita allocation underestimates it. Our half-per-capita baseline lies between these alternatives, and varying the per-capita share has only small effects on top shares.

PSZ notes that our allocation has a larger equalizing effect with higher inequality. However, this is just a mechanical implication of a per-capita incidence assumption, whose effect becomes relatively larger as underlying inequality rises. They also note that inequality studies usually use narrower after-tax measures. But conventional measures omit collective consumption precisely because they do not distribute all national income. Once this

government spending is included, it must be allocated—proportional and per-capita allocations are alternative incidence assumptions, but neither is distributionally "neutral."

National defense is a component of government consumption for which incidence is particularly unclear. It could be allocated per capita because it resembles a pure public good, though Simon Kuznets sometimes favored excluding it. Auten and Splinter (2025b) find that excluding defense spending raises our top 1% after-tax share by 0.4 percentage point in 1962 but only 0.1 point in recent years. Because defense spending declines as a share of the economy, excluding it produces a flatter long-run trend in top income shares.

5. Government deficits

PSZ allocates government deficits proportionally to after-tax income, but proportionality is itself an incidence assumption. We instead allocate deficits by federal taxes because almost all deficits are federal and historical fiscal adjustments have frequently operated through taxes rather than reductions in transfers (Auten and Splinter 2020; Ferriere and Navarro 2025). Recent research provides additional support for this approach. Chatzouz (2026, p. 14) treats public debt as a deferred tax and, using long-run cross-country evidence, finds that higher debt in advanced economies is associated with higher tax burdens and inequality effects "transmitted primarily through the tax burden." Consistent with a tax-based allocation, Smets and Wouters (2024) estimate that most U.S. fiscal shocks are backed by future fiscal adjustments, and Bianchi et al. (2026) find U.S. debt-financed redistribution since 1980 has been backed by future progressive taxes borne by high-income households. Although this evidence does not imply that every dollar of debt is financed in exact proportion to current federal taxes, it supports a tax-based allocation as a reasonable baseline.

III. Capital income and wealth inequality

PSZ argues that our capital income estimates are inconsistent with wealth inequality trends, but they do not make an apples-to-apples comparison. Our capital-income series ranks individuals by income, whereas the wealth series ranks by wealth. Between 1989 and 2019, our top 1% capital-income share increased by 5 percentage points, while the *income-ranked* top 1% DFA wealth share increased by 8 percentage points (Splinter 2023). Thus, when the ranking is more comparable, the two series show broadly similar increases. The composition of capital income missing from tax returns also changed over this period, shifting away from relatively concentrated retained earnings toward more broadly held retirement income and imputed rents.[14]

Capital-income and wealth concentrations need not move together. Wealth reflects accumulated saving and returns, not simply current capital income (Fagereng et al. 2016; Moll 2020). Consistent with this distinction, Kuhn, Schularick, and Steins (2020) show that

[14] We do not report wealth distributions because capital-income flows cannot simply be capitalized to recover wealth distributions, as capitalization depends on heterogeneous rates of return (Smith, Zidar, and Zwick 2023).

differential movements in housing and equity prices can decouple the U.S. income and wealth distributions over extended periods. Kaymak, Leung, and Poschke (2026) likewise find that top wealth concentration largely reflects earnings inequality and heterogeneous returns. Long-run declines in interest rates can increase wealth inequality relative to capital-income inequality (Greenwald et al. 2023). Between 1989 and 2019, for example, the federal funds rate fell from about 9% to 2%, increasing the wealth value associated with a given flow of interest income. Gomez and Gouin-Bonenfant (2024, p. 201) estimate that "the secular decline in interest rates…can account for about 40% of the rise in Pareto [wealth] inequality."

Several mechanisms can cause wealth concentration to rise faster than capital-income concentration in recent decades. The DFA unit of observation is the family, whereas we use percentiles with equal numbers of individuals. Because marriage rates have declined more among lower-income families, family-based rankings can generate different trends even when the underlying individual distributions change similarly. Heterogeneity in saving, returns, and longevity can generate different trends in wealth and current income (Fernández-Villaverde and Levintal 2024). About half the increase in U.S. wealth inequality since 1970 is associated with increased longevity (Makarski, Tyrowicz, and Żoch 2025). PSZ therefore does not establish an inconsistency between our capital-income estimates and the wealth data.

IV. Top-income effects are not top allocation shares

PSZ introduces a statistic they call the top 1% "share" of a missing income source. But this statistic is not the share actually received by the top 1%. It instead measures the net change in top-group income after adding the component and re-ranking, which subtracts the baseline top incomes displaced when new individuals rise into the top group. We therefore refer to s_s as the *net top-1%-income effect*. It lies between the share of the component received by the initial top 1% and the larger share received by the final top 1% after re-ranking from the allocation.

PSZ's statistic s_s summarizes the effect of adding a missing income component s to an initial income measure y. If S is the aggregate amount of s, then $s_s S$ is the increase in aggregate income of the top 1% when moving from y to $y+s$, allowing individuals to be re-ranked. Letting T_y denote the initial top 1% and T_{y+s} the top 1% after adding s,

$$s_s = \underbrace{\frac{\sum_{i \in T_{y+s}} s_i}{S}}_{\text{share of } s \text{ received by final top 1\%}} - \underbrace{\frac{\sum_{i \in T_y} y_i - \sum_{i \in T_{y+s}} y_i}{S}}_{\text{baseline income lost through re-ranking}} .$$

Thus, s_s is the share of the component actually received by the final top group minus the baseline income displaced when new individuals enter the top 1%. It should not be interpreted as an allocation share, as PSZ repeatedly describes it.[15]

[15] PSZ acknowledges that when re-ranking occurs, s_s lies between the share of s received by the original top 1% ranked by y and the generally larger share received by the final top 1% ranked by $y+s$.

This distinction can be large. For example, suppose S = 100, the initial top individual has income of 100 and receives none of s, while an individual with initial income of 70 receives 50 of s and moves into the top group with total income of 120. The final top group receives 50% of s, but top-group income rises by only 20, so PSZ's statistic is s_s = 20%. The 30-point difference reflects displaced baseline income, not where the added income was allocated. Thus, a PSZ measure of only 20% can be much smaller than the actual top allocation share of 50%.

This is quantitatively important in our data. For partnership excess depreciation, our linked-data estimates show that 33% is received by the top 1% when tax returns are ranked by reported fiscal income. After adding excess depreciation and re-ranking, the top group's share rises to 59%. Underreported income provides another example. In 2010, 6% of underreported income is received by the top 1% of returns ranked by reported income (per audit-study data), but the share rises to 16% when ranking by income including underreporting (Auten and Splinter 2024b). Because the s_s statistic subtracts the baseline income displaced through re-ranking, it is only about 11% in nearby years. A small s_s reported by PSZ therefore does not imply that we allocate a small share of the underlying income to high-income individuals. It can instead reflect baseline income displaced as recipients of the missing component enter the top 1%.

Finally, the PSZ decomposition has limitations. Its estimates use aggregate totals, whereas our decomposition applies alternative assumptions to the underlying microdata. In addition, each income component can change the ranking used when the next component is added, so the component-specific s_s estimates depend on ordering. Although the weighted component effects sum to the same overall income change, the attribution of that change to individual components is not unique. Each component-specific s_s is conditional on the components previously added and therefore is not an order-independent marginal effect or an underlying allocation share. We instead calculate the decomposition under two alternative orderings and average the resulting effects.

V. Sensitivity to alternative assumptions

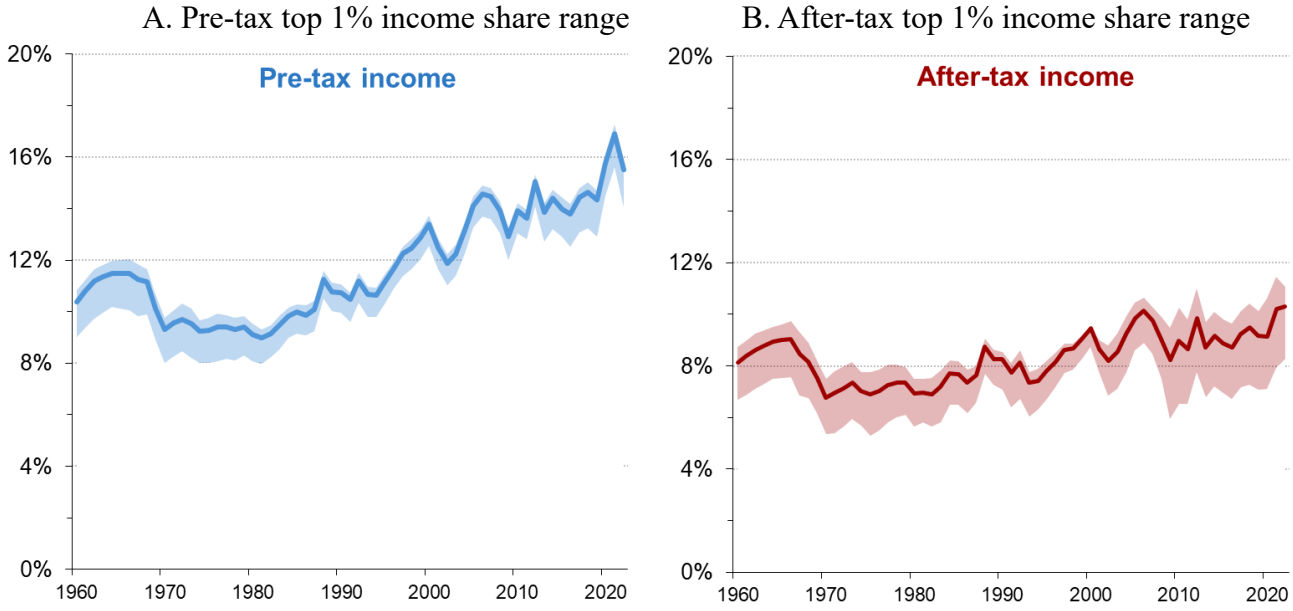
Our published paper included numerous sensitivity tests. Following Clarke and Kopczuk's (2025) view of income-share estimates as ranges, we extended our sensitivity analysis in subsequent work. These show that our baseline estimates are relatively robust when multiple assumptions all increase or decrease recent top shares.

Auten and Splinter (2025b) consider alternative size adjustments and allocations of government deficits, government consumption, corporate excess depreciation, and corporate retained earnings. These are sensitivity ranges rather than confidence intervals, asking how far estimates move when multiple assumptions are changed simultaneously in a direction

that increases or decreases recent top shares. For pre-tax top 1% income shares in Figure 4A, the average lower-share scenario is 1.0 percentage point below the baseline, more than twice the 0.4 percentage-point difference from the higher-share scenario. For after-tax top 1% income shares in Figure 4B, the average lower-share scenario is 1.5 percentage points below our baseline, compared with only 0.7 percentage points for the higher-share scenario. This asymmetry places our baseline estimates toward the upper end of the range generated by these alternative assumptions.

Additional adjustments could raise top shares in earlier years and therefore further flatten the long-run trend. For example, evidence of greater high-income underreporting in the 1960s based on Troiano (2017) implies an increase of about 0.7 percentage point. Omitted personal consumption through generous business expense accounts could add about 0.5 point. Excluding defense spending would raise the early 1960s top share by another 0.4 point.[16] Taken together, these adjustments could increase early top shares and flatten the long-run trend in top 1% income shares. Given their uncertainty, they are not included in our baseline, which relies on more data-informed assumptions.

Figure 4: Range of top 1% income shares under alternative assumptions



Notes: The high- and low-share scenarios vary up to five assumptions that increase or decrease top shares in 2022: (1) corporate excess depreciation (high/base: C-corporation ownership; low: linked S-corporation depreciation); (2) corporate retained earnings (high: 100% dividends; base: 75% dividends/25% capital gains; low: 50%/50%); (3) income-size ranking adjustment (high: none; base: square-root of tax-unit individuals; low: tax-unit indivs.); (4) government deficits (high: exclude; base: federal taxes; low: federal income taxes); and (5) government consumption (high: 40% per capita; base: 50%; low: 75%, rest by after-tax income). Ranges are sensitivity exercises, not confidence intervals. Sources: Auten and Splinter (2025b).

[16] As discussed in AS, offshore income suggests that top shares may be understated by as much as 0.3 percentage point, though about half as much since 2014 due to increased compliance (Gorman, McGuire, and Splinter 2026).

VI. Concluding remarks

In their recent comment, PSZ begins with the largest methodological differences, replaces selected baseline assumptions with their alternatives, and reports the resulting changes in top income shares. But reversing an assumption does not mean the alternative is preferable. Each adjustment requires a substantive rationale, relevant distributional evidence, and a valid calculation linking that evidence to the proposed allocation. As shown in this response, the proposed adjustments fall short on one or more of these criteria. When empirical data provide evidence on the distribution of a particular missing component, that evidence should be used, rather than merely scaling up reported income. For example, linked partnership and S-corporation data suggest little change in top 1% shares from reallocating excess depreciation, as compared with large increases using the PSZ reported-income approach. Similarly, audit studies imply far less concentration of underreporting at the top than the alternative allocation based largely on rescaling reported business income. These three differences alone explain roughly half of the AS–PSZ gap.

The changing composition of missing income has also made comprehensive income more equal than fiscal income reported on individual tax returns. This is because of the growth in unreported employer-paid health insurance, employer payroll taxes, and retirement contributions, which are all disproportionately received by the bottom 99%. Together with the long-run increase in tax progressivity and transfers (Splinter 2020b, Coleman and Weisbach 2025), these changes help explain why pre-tax and after-tax national income show substantially smaller top-share increases than fiscal income.

More broadly, the disagreements discussed here illustrate a central problem in distributional national accounts: matching aggregate national income is only the first step. Distributional estimates also depend on who is assigned missing income and on the incidence of taxes and government spending. The comment repeatedly characterizes proportional allocations as "neutral," but proportionality is itself a distributional assumption. This is the crux of the difference between the AS and PSZ approaches. When direct administrative or incidence evidence is available, it provides a much stronger basis for allocating individual components than proportional scaling of reported income.

We incorporated several helpful suggestions from PSZ into the published AS estimates. We also appreciate that they have updated some of their earlier methods, such as reducing the impact of rollovers in allocating retirement income. However, the evidence reviewed here does not support their comment's proposed adjustments, which are inconsistent with the available evidence.

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