Referee Report on ``The Distribution of Capital Gains in the United States"

This paper examines the distribution of capital gains, and their contributions to income inequality and tax progressivity in the U.S. The authors leverage the IRS tax data and implement a method to impute total capital gains by linking individuals to their portfolio holdings, capitalizing income to estimate wealth, and estimating capital appreciation by multiplying wealth by asset specific returns. The authors find that capital gains are highly unevenly distributed, leading to an increase in wealth inequality and a reduction in tax progressivity due to low realization rates.

There are many things to like about this paper. The authors use rich, confidential tax returns data, and implement a careful imputation method to measure total capital gains and derive relevant empirical facts that policymakers should care about. While these findings are not surprising *qualitatively*, many of the quantitative estimates are indeed useful to think about the degree of wealth inequality and progressivity of the tax system in the United States.

My only reservation is that I don't think this paper is a good fit for publication at a top field journal such as the Review of Economics and Statistics, which aims to target more general-interest readers in economics. This paper mainly provides descriptive, accounting exercises that, while valuable, do not offer new economic insights or theoretical framework to think about. Even though I find it still useful to reaffirm many of the qualitatively facts that I knew before quantitatively, these quantitative aspects did not provide me with new economic intuition. Some of the descriptive works this paper cites, including Piketty, Saez, and Zucman (2018) and Smith, Zidar, and Zwick (2023), are published in general-interest journals, but they had the first-mover advantage of showing these descriptive facts with very thorough accounting exercises and detailed data. I did not get a sense that this paper adds too much to the existing papers in terms of the methodological contribution. Therefore, I think this paper is a better fit for a more specialized journal such as the National Tax Journal. I only have a couple of comments that may help improve this paper, but overall, my assessment of this paper is purely based on whether this paper is a good fit for the ReSTAT after evaluating its marginal contributions relative to the prior studies.

<u>Comments</u>

(1) Highlight your marginal contributions relative to SZZ (2023) more explicitly
The authors acknowledge that their work is closely related to Smith, Zidar, and Zwick (2023) who also link individual owners to private firms using tax data to estimate their wealth including capital gains. The authors also follow the method of SZZ (2023) closely, with three exceptions: (1) value businesses using valuation multiples from private businesses rather than public firms, (2) link partnership networks to their ultimate owners, and (3) leave the estimates of private business wealth unadjusted. As a consequence, the authors find the higher estimates of business wealth and greater wealth concentration. However, the authors do not offer detailed explanations or justifications as to why their method is better and more accurate. For example, valuing businesses using two datasets from private businesses seems more accurate than just

using valuation multiples from public firms, but it depends on the reliability of the data on private business sales (from Business Valuation Resources). Furthermore, ownership structures in partnership are very complicated, so it would be helpful to offer more detailed explanations for how their method of linking partnerships to ultimate owners leads to a more accurate measure of wealth in partnerships. Perhaps, these details are buried in the appendix, but even if they are, they deserve to appear in the main text so that readers without deep accounting background can get a better sense of how their method adds an important contribution relative to the existing work like SZZ (2023). Finally, it would be helpful to know which of these three methodological exceptions described above contributes to a large difference between their estimates and the estimates from SZZ (2023). The bottom line is that readers would benefit from a more explicit explanation that highlights their marginal contributions relative to the existing studies.

(2) Possible to do some simulations assuming heterogenous returns on equity?
When estimating capital gains, the authors assume individuals across the income distribution have the homogenous expected return on their assets, which it's likely not true in reality. So, this is one of the key limitations of the authors' findings: they lack data on heterogenous returns across all asset classes. While the authors acknowledge that their limitation would understate the degree of capital gains inequality (because richer individuals likely have higher returns on equity compared to poorer individuals), I was wondering whether the authors can do some simulation exercises where they estimate capital gains across different income distribution, where they assume heterogenous returns on equity. For example, they can use a reasonable parameter people have estimated in the other literature, and assign a higher expected return for those at the upper quantile of the income distribution. I would be curious to know how much the distribution of capital gains would change from this simulation exercise relative to their benchmark where they assume a homogenous rate of return across all individuals.