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Prof. Jacob Robbins
University of Illinois at Chicago

RE: AER-2025-0706: The Distribution of Capital Gains in the United States

Dear Cole, Jacob, and Sam,

Thank you for submitting your paper "The Distribution of Capital Gains in the United States" to the *American Economic Review*.

I am sorry to report that I have decided not to publish your paper in the *American Economic Review*. Because I have not sent your paper out for review, I have instructed the editorial office to refund half of your submission fee.

I read your paper with great interest. As you may have guessed, I believe that your paper's topic – capital gains – is very important. When I started reading, in particular the title and first paragraph of the introduction, I was very excited. A careful empirical documentation of the distribution of capital gains in the United States and how capital gains correlate with various income sources and demographics would be very useful and important.

However, I found that this is not what your paper aims to do. Instead, as you write on p.8 "our goal is to construct a dataset of individual level Haig-Simons income and wealth" and you then go on to referring to capital gains as an "income flow" (p.3 and p.5), to document very low "income" tax rates by including unrealized capital gains in the denominator (Haig-Simons income), and to refer to taxation at realization as a loophole (p.37). Unfortunately, my view is that these are misleading exercises and statements as is the Haig-Simons income concept: unrealized capital gains are simply not income and adding, say, labor income and unrealized capital gains is like "adding apples and oranges."

As you may have guessed, my view on this was formed through my work on the topic: the paper you cite but also my recent paper on capital gains taxation with Aguiar and Scheuer. (For what it's worth, when I first started working on this topic about 8 years ago, I started with the prior that unrealized capital gains *were* a form of income but, after thinking this through, I changed my mind.)

I did see that you briefly engage with these issues on p.7 ("some recent theoretical work raises ...."). However, it seems to me that you have not fully appreciated how far-reaching the implications of this argument are.

Specifically, this argument is not only about capital gains that are "driven purely by changes in interest rates." Instead, it's the opposite: the argument applies whenever an individual's capital gains are *not* purely driven cashflow changes (an extreme knife-edge case that we should never expect to apply in reality). The point is that there are lots of other factors that drive fluctuations in the price of an asset besides the cashflows of the particular asset. Just think about the huge observed stock market fluctuations, animal spirits, and so on. This goes back to the Nobel-prize winning work of Shiller (1981). The finance literature calls all these other factors "discount rates". The point is that discount rates have nothing to do with "interest rates." Instead it's a much more general idea: "discount rates" are just a residual that summarizes all drivers of asset prices besides cashflows.

You write that "three drivers of capital gains [are] unrelated to changes in interest rates, each with broad support in the literature and strongly backed by empirical evidence..." I completely agree with this assessment. However, this misses the point. With regard to the third driver, you write "hetergeneous returns on real estate by their very nature cannot be due to aggregate discount rate changes." But why only aggregate discount rate changes? The point is that *anything* that means that real estate capital gains on a particular property do not only reflect the rent on that property cause a problem for Haig-Simons income, i.e. discount rate variation can of course be heterogeneous. With regard to the first two drivers, your argument unfortunately again misses the point: for the criticism not to apply you would need to show that cashflow changes like markups are *the only* factors driving asset prices.

Another way of thinking about this that you may find intuitive is discussed briefly in my paper with Aguiar and Scheuer: *Wealth taxes are sometimes likened to taxes on "presumptive income" (Zucman, 2024, or the Dutch "box 3" wealth tax): for example, a 2% wealth tax is equivalent to a 40% tax on presumed capital income from a constant asset return of 5%. When asset values increase and the increase is entirely due to higher cashflows (the second special case), the asset return remains constant and therefore the increase in presumptive income exactly matches the increase in actual income. But in all other cases, the return falls and therefore actual income rises by less than presumptive income calculated as a constant return to the increased market value of wealth. Thus "presumptive income" is overestimated and hence wealth taxes redistribute suboptimally whenever asset valuations are not exclusively driven by cash flows.*

Finally note that related arguments have been around for a long time. See in particular Paish (1940) "Capital Value and Income," Kaldor (1955) "An Expenditure Tax" (https://benjaminmoll.com/kaldor/) and Whalley (1979)

"Capital Gains Taxation And Interest Rate Changes: An Extension Of Paish's Argument" (although some of these admittedly also make it sound as if this is only about interest rates).

Given all of these considerations, as I already said, I think it's quite misleading to refer to unrealized capital gains as income, compute tax rates on Haig-Simons income, and so on. For what it's worth, my view of Yagan (2023) and Saez and Zucman (2019) is exactly the same.

In light of these concerns, I have decided not to send your paper out for review. Sending the paper out to reviewers (at least those with a good theoretical understanding of capital gains whom I would have picked) would, according to my best judgment and experience, almost surely lead to the same ultimate outcome. However, it would prevent you from submitting the paper elsewhere in the meantime and thus delay the paper's eventual publication.

To be clear, I do think a paper documenting key facts about the distribution of capital gains would be valuable. I would therefore reconsider a drastically revised version of your paper in which you do not refer to unrealized capital gains as income and whose main goal is not to compute the distribution of Haig-Simons income and the implied tax rates. However, I suspect that this is very different from your own vision for your paper which I respect.

I'm sorry I wasn't able to give you positive news and I hope I can do so on another occasion. Please also keep in mind that the AER can only accept about 5 percent of the submissions for publication and that, as a result, many quality papers do get rejected.

Thank you for considering the *American Economic Review* as an outlet for your research. Sorry for the bad news and good luck with the project.

Sincerely,

Benjamin Moll
Coeditor
American Economic Review