

Testing Piketty's Hypothesis on the Drivers of Income Inequality: Evidence from Panel VARs with Heterogeneous Dynamics

March 24, 2024

1 Response to Referee 1

1. I would strongly encourage the author to add a paragraph (probably in Section 7 and/or the intro) acknowledging the ongoing challenges and uncertainties around Piketty's data. [...] To this end, "Income concentration mismeasurement" should be added to the list of potential explanations for why it may be difficult to establish a relationship between Piketty's theory and Piketty's data. It's very plausible that Piketty's data is simply less precise and of lower quality than his book makes it out to be.

Response: Thank you for your comment, I followed your suggestion and discussed some of the data issues:

Another potential source of explanation regarding why it might be difficult to find in the data empirical evidence for the causal links set forth by Piketty is *income concentration mismeasurement*. I relied on the preferred set of estimates by Piketty and his coauthors to test his hypothesis, those from *World Inequality Database*. However, an emergent literature shows that the measurement of top income can be highly uncertain. A large share of income flows is unobserved and its estimation depends on assumptions regarding its distribution and valuation. For instance, Auten, Splinter and Furchtgott-Roth (2021) present alternative series for top income shares, using different assumptions, and conclude that the after-tax income share for top 1% of incomes in the United States stayed flat in the last half-century.

2. "on Piketty's "second law of capitalism" another data quality issue should be acknowledged. Piketty's estimation of capital-to-national income ratios (both at country/region level and worldwide) provides another way for him to validate $r-g$ (by eyeballed correlation) in his book"

Response: This is true and it is why I added a version of the empirical model directly with capital income:

I then turn to **Model 2**, which checks the relationship between $r - g$ and the capital share. This specification can be thought of a direct test of Piketty's model, since his extrapolations to inequality stem from a growth in the capital share.

3. The author notes a previous criticism from Piketty on p. 6 re. its approximations of capital returns via interest rates, and discusses how he/she addressed this. A small note could be added that Piketty's preferred measure - using the Forbes list to estimate wealth growth from reported net worth - is somewhat notorious for its own inaccuracies. See Raub, Johnson, and Newcomb (2010), which compared Forbes listings for US billionaires to probate records of deceased billionaires on the list and found that Forbes tended to severely overstate the value of their assets. If Piketty is relying on Forbes to estimate growth/return on billionaire capital ownership, he's likely picking up some of the same issues that taint the Forbes list. This would be an added argument against his preferred approach and in favor of the authors' resolution of this debate.

Response: Once again, thank you for the great suggestion. I added two segments in the data section that incorporate this:

It should be noted that there is high uncertainty regarding the quality of Forbes estimates. For instance, Raub, Johnson and Newcomb (2010) found that wealth reported in tax data only corresponds to about half of Forbes' list. With different assumptions, either number could be closer to true wealth stocks.

[...]

Data on heterogeneous returns on capital are scarce. Some years ago, the editors of Credit Suisse's *Global Wealth Databook*, who have been publishing estimates of wealth concentration across countries for several years highlight that "study of global household wealth is still in its infancy." (Shorrocks, Davies and Lluberas, 2016). More recently, the *World Inequality Report* recalled that "global wealth data remain opaque," and many of the estimates they provide come from extrapolations from income inequality data, and/or limited population coverage.

4. in figure 1 and related discussion, I notice you use the CBO estimate of growth in wealth at the 95th percentile. Have you looked at the more recent Distributional Financial Accounts series from the Fed? It has quarterly estimates based on the SCF and a few other measures, and might give more fine-grained data.

Response: Another comment that substantially improved the paper. The new paragraphs now read as follows:

More importantly, this argument does not really address whether or not interest rates are a good proxy for returns on capital. Level differences notwithstanding, in so far as returns on capital can be reasonably expressed as a linear function of returns on government bonds, the latter are a good proxy for the former.

[...]

These caveats notwithstanding, the Federal Reserve has recently published its Distribution Financial Accounts. They provide estimates of asset stocks for different ownership brackets going back to 1989. The database uses underlying microdata from the Survey of Consumer Finances. Regressing the 4-quarter net return on assets¹ of the Top 1% on 3-mo treasuries yields the following relationship: $\hat{R}_{q,t}^{1\%} = \frac{1.81}{(0.010)} + \frac{1.60}{(0.000)} R_{q,t}^{3mo}$. With 10-year bonds, the relationship with 10-year bonds is $\hat{R}_{q,t}^{1\%} = \frac{2.23}{(0.93)} + \frac{1.30}{(0.003)} R_{q,t}^{10yr}$.

The specific criticism Piketty made regarding the use of sovereign bond yields thus appears to be largely unfounded. However, in recognizing the challenge to find good proxies for the return on capital, I present results with two different proxies for r in the robustness section: short-term interest rates, and implied returns from the national accounts. I also present results ignoring tax rates.

5. That said, I do have some concerns about whether this paper is specifically suited for Public Choice. To fit the themes and readership of the journal, the author(s) would need to do more to tie it in to the public choice subfield. I believe this could be done in the conclusion, and by developing the theme noted in the outset of the paper (specifically: if the Piketty r - g relationship doesn't hold, that substantially alters the policy remediations that can be done over inequality). Right now there's very little overt connection to public choice theory though. [...] One possible way to address this that I'll propose: in addition to positing a causal mechanism theoretical mechanism linking capital stock growth & higher inequality, Piketty also asserts a strong causal link between tax rates and top income concentration. High tax rates are essentially the lever he proposes using to disperse income concentration from the top, and he claims that this approach worked historically. The author(s)' paper already accounts for some tax rate data in its empirical testing. To link to the public choice literature though, they should consider bringing in the public choice angle as an interpretive framework. Buchanan and Brennan's *The Power To Tax* is the classic theoretical examination of how tax regimes develop. See also supplemental work on public choice and taxation by Dwight Lee, and a couple of papers exploring the taxation-inequality link by Robert Tollison and Bill Shughart. A brief lit review, and interpretive tie-ins would help to connect this important paper's findings to the journal's focus and readership.

Response: Thank you for your comment. Admittedly, I am no expert on public choice theory. I chose to submit this paper to Public Choice because it has important policy implications for public finance and political economy, fields that I know readers of this journal are more familiar with. Furthermore, I would argue that the empirical test of Piketty's theory is relevant regardless of whether one chooses to model optimal policy through the lens of a benevolent social planner, rather than adopting a constitutional political economy perspective. Following your advice, I included in the paper a literature review of alternative mechanisms coming from the political economy literature. Additionally, I included some empirical papers that suggest that unintended consequences of the primary policy choices by Piketty and his co-authors (capital and wealth taxations) might be large. Hoping that this satisfy your concerns regarding fit, this is how the new conclusion reads:

¹If $A_{q,t}^{1\%}$ is the asset stock of the Top 1% at quarter q of year t , the 4-quarter net return on assets is: $R_{q,t}^{1\%} = \frac{\sum_{p=0}^3 A_{q-p,t}^{1\%}}{\sum_{p=0}^3 A_{q-p,t-1}^{1\%}} - 1$.

The policy implications of high levels of inequality can be quite important. There are different mechanisms, not evaluated in this paper, that suggest that inequality can be harmful to social welfare. For instance, Alesina and Rodrik (1994) set forth an endogenous growth model in which capital taxes increase returns to labor, inducing a conflict between the optimal tax rate in the presence of inequality in capital ownership. Banerjee and Newman (1993) develop a model with capital market imperfections in which long-run prosperity depends on the initial wealth distribution. Easterly (2007) provides evidence that initial land endowment predicts future inequality and future inequality predicts development. Campante and Ferreira (2007) draw up a theory and Shughart II et al. (2003) provide suggestive evidence that heterogeneity in the capabilities of interest groups can reinforce income inequality.

Without knowing the underlying causes of the recent trends in increased income inequality, it is impossible to design policy actions to counter them. The solution advanced by Piketty and some of his co-authors is increased capital income or wealth taxation (see, for instance, the policy proposals in the book by Saez and Zucman, 2019). However, if the links between his theory and inequality are found wanting, these policies might not lead to the desired outcomes.

Additionally, one should take into account the potential unintended consequences of these policies. Well identified empirical studies show that workers may bear the burden of as much as 50-75% of the total corporate tax incidence (Fuest, Peichl and Siegloch, 2018; Malgouyres, Mayer and Mazet-Sonilhac, 2023; Suárez Serrato and Zidar, 2016). As the model posited by Bethencourt and Kunze (2015) rationalizes, increased inequality might lead to a preference for higher tax rates as well as a deterioration of the tax base through tax evasion. Empirically, Guyton et al. (2021) find that tax evasion is very large among top income earners in the U.S., with underreported income accounting for as much as 50% of total income. Hence, even if one abstracts away from the question of how to model optimal policy through the lens of a “benevolent social planner” or a “revenue-maximizing Leviathan” (Brennan and Buchanan, 1980), investigating the implications of this causal link between $r - g$ and inequality would be relevant for outlining optimal choices.

Knowing if increases $r - g$ lead to inequality is very important, not only for economics as a science of human action but also for the policy repercussions of such conclusions. Inequality is a complex phenomenon and its trends are very sluggish. It is certainly possible that the long-term relationships Piketty proposes exist and are simply not captured by the 30 years of data for the 19 advanced economies included in this sample. However, for a large range of countries, the evidence provided in this paper suggests that if one is looking to potential solutions to increasing income inequality, one should not focus on $r - g$, but elsewhere.

2 Response to Referee 2

1. Possible error in the computation of the confidence bands. Appendix B describes the simulation algorithm that is used to compute the standard errors that are used for the construction of the impulse response confidence bands reported in the paper. From what I am able to deduce from the description of the algorithm, it appears not to take into account the cross sectional dependence structure of the panel [...]

Response: Thank you for catching this. This is great refereeing work and the suggestion improved the paper. While it did not ultimately change the results that much, now I have an updated algorithm. See the changes in its description below:

- (a) I re-sample the structural residuals accounting for cross-sectional dependence. First, for each i , I use the loadings Λ_i to recover idiosyncratic shocks $\{\tilde{e}_{i,t}\}_{t=0}^T$, using the equations in (8) to extract $\tilde{e}_{i,t} = u_{i,t} - \Lambda_i \bar{e}_t$, where \bar{e}_t denote the common shocks.
- (b) Let $\dot{e}_{i,t}$ denote re-sampled structural residuals $e_{i,t}$. I resample common shocks $\dot{\bar{e}}_t$ and idiosyncratic shocks $\dot{\tilde{e}}_{i,t}$ and use these variables to construct resample structural residuals that incorporate the cross-sectional dependence structure of the panel as:

$$\dot{e}_{i,t} = \Lambda_i \dot{\bar{e}}_t + \dot{\tilde{e}}_{i,t} \quad (1)$$

2. Concern regarding claimed stationarity of the data. The claim is made on page 12 that "the variables are (trend) stationary." But there is no test offered to support this, and off-hand, there is good reason to expect that variables such as the income share of the top 1 percent of households are likely to be nonstationary particularly when a central premise is that inequality has been rising over time. The issue is important for two reasons. First, it points to whether the VAR estimation is best done in log levels or in log differences with the resulting impulse responses accumulated in the latter case. The second reason, which is potentially even more important, is that bootstrap simulations as described in appendix B need to be done from the stationary representation of the data

Response: Thank you for your comment. Since it was confusing, I removed the remark of trend stationarity and I clarified what I meant by the stability of the model in the text (below). I mean that the inverse roots of the characteristic polynomial associated with each one of the country-specific VARs are within the unit circle. Testing for unit roots in data with $T=30$ is tricky. Unit root tests are known to over-reject the null. If the variables were nonstationary, their characteristic polynomials suggest they would be cointegrated, which makes the approach taken in the paper valid ("the common practice of attempting to transform models to stationary form by difference or cointegration operators whenever it appears likely that the data are integrated is in many cases unnecessary" Sims, Stock and Watson, 1990). More importantly, there are strong theoretical reasons to reject the possibility of nonstationarity: either bounded variables or the requirement of finiteness for the existence of an economic equilibrium in $r-g$. I report that reasoning below, hoping that it is convincing. I incorporated the following language in the manuscript:

Despite country-specific heterogeneity, the estimated VARs are stable. We observe that by noting that the inverse roots of the characteristic polynomial related to each country-specific matrix of VAR coefficients are within the unit circle. Several of the variables are shares and have bounded support, which rules out the possibility of a unit-root in the traditional sense². Others, such as $r-g$ must have some finite mean for an economic equilibrium to exist.

This means that one-off shocks should be interpreted as temporary and, following any shock, variables are expected to converge back to their means or deterministic trends over the long-run. In line with the outlined theory, one should interpret these as deviations around the long-run BGP.

²Granger (2010) expanded the idea of nonstationarity to bounded variables, terming a stochastic process that behaves as if a $I(1)$ but has at least one bound a limited integration or $LI(1)$ process. Note that for many of the variables I use in the estimation, there are upper and lower bounds, such as capital share or the savings rate, both bounded within $[0, 1]$. In that case, by assumption, those variables cannot be integrated in the traditional sense, but could behave like one away from the bounds.

3. Suggestion regarding notation to avoid confusion. As noted in the paper, section 4 follows Pedroni (2013). The choice of notation is confusing however because it exactly reverses the notation used in Pedroni (2013). [...]

Response: Very pertinent comment, I did not realize the inversion. I have updated the notation.

4. The language that is used in the discussion of shocks as temporary or permanent seems a bit confused.

Response: I added an explanation of how to interpret shock. In the theoretical model, we should interpret them as deviations around the BGP. In the VARs, they are temporary deviations around the long-run average or trend.

5. Minor comments

Response: I corrected spell errors.

References

- Alesina, Alberto, and Dani Rodrik.** 1994. “Distributive Politics and Economic Growth.” *The Quarterly Journal of Economics*, 109(2): 465–490.
- Auten, Gerald, David Splinter, and D Furchtgott-Roth.** 2021. “Top income shares and the difficulties of using tax data.” *United States Income, Wealth, Consumption, and Inequality*, 125–152.
- Banerjee, Abhijit V., and Andrew F. Newman.** 1993. “Occupational Choice and the Process of Development.” *Journal of Political Economy*, 101(2): 274–298.
- Bethencourt, Carlos, and Lars Kunze.** 2015. “The political economics of redistribution, inequality and tax avoidance.” *Public Choice*, 163: 267–287.
- Brennan, Geoffrey, and James M Buchanan.** 1980. *The power to tax: Analytic foundations of a fiscal constitution*. Cambridge University Press.
- Campante, Filipe R, and Francisco HG Ferreira.** 2007. “Inefficient lobbying, populism and oligarchy.” *Journal of Public Economics*, 91(5-6): 993–1021.
- Easterly, William.** 2007. “Inequality does cause underdevelopment: Insights from a new instrument.” *Journal of Development Economics*, 84(2): 755–776.
- Fuest, Clemens, Andreas Peichl, and Sebastian Siegloch.** 2018. “Do higher corporate taxes reduce wages? Micro evidence from Germany.” *American Economic Review*, 108(2): 393–418.
- Granger, Clive WJ.** 2010. “Some thoughts on the development of cointegration.” *Journal of econometrics*, 158(1): 3–6.
- Guyton, John, Patrick Langetieg, Daniel Reck, Max Risch, and Gabriel Zucman.** 2021. “Tax evasion at the top of the income distribution: Theory and evidence.” National Bureau of Economic Research.
- Malgouyres, Clément, Thierry Mayer, and Clément Mazet-Sonilhac.** 2023. “Who Benefits from State Corporate Tax Cuts? A Local Labor Markets Approach with Heterogeneous Firms: Comment.” *American Economic Review*, 113(8): 2270–2286.
- Raub, Brian, Barry Johnson, and Joseph Newcomb.** 2010. “A comparison of wealth estimates for america’s wealthiest decedents using tax data and data from the forbes 400.” Vol. 103, 128–135, JSTOR.
- Saez, Emmanuel, and Gabriel Zucman.** 2019. *The triumph of injustice: How the rich dodge taxes and how to make them pay*. WW Norton & Company.
- Shorrocks, Anthony, James B. Davies, and Rodrigo Lluberas.** 2016. “Credit Suisse Global Wealth Databook 2016.” Credit Suisse Report.
- Shughart II, William F, Robert D Tollison, Zhipeng Yan, et al.** 2003. “Rent seeking into the income distribution.” *Kyklos*, 56(4): 441–456.
- Sims, Christopher A, James H Stock, and Mark W Watson.** 1990. “Inference in linear time series models with some unit roots.” *Econometrica: Journal of the Econometric Society*, 113–144.
- Suárez Serrato, Juan Carlos, and Owen Zidar.** 2016. “Who benefits from state corporate tax cuts? A local labor markets approach with heterogeneous firms.” *American Economic Review*, 106(9): 2582–2624.