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The economic consequences of major tax cuts for the rich

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Abstract

The last 50 years has seen a dramatic decline in taxes on the rich across the advanced democracies. There is still fervent debate in both political and academic circles, however, about the economic consequences of this sweeping change in tax policy. This article contributes to this debate by utilizing a newly constructed indicator of taxes on the rich to identify all instances of major tax reductions on the rich in 18 Organisation for Economic Co-operation and Development (OECD) countries between 1965 and 2015. We then estimate the average effects of these major tax reforms on key macroeconomic aggregates. We find tax cuts for the rich lead to higher income inequality in both the short- and medium-term. In contrast, such reforms do not have any significant effect on economic growth or unemployment. Our results therefore provide strong evidence against the influential political–economic idea that tax cuts for the rich 'trickle down' to boost the wider economy.

Key words: taxation, inequality, income distribution, economic growth, unemployment

JEL classification: D31 personal income, wealth, and their distributions, E62 fiscal policy, O47 empirical studies of economic growth, aggregate productivity, cross-country output convergence

1. Introduction

The past half century has been a period of substantial change in tax policy in the advanced democracies (Steinmo, 2003; Kiser and Karceski, 2017). A particularly prominent part of this transformation has been the dramatic fall in taxes on the rich across the Organisation for Economic Co-operation and Development (OECD) countries (Ganghof, 2006; Hope and Limberg, 2021). While this sweeping policy change has been well documented, its consequences for the economy are less well understood.

Proponents of the tax cuts for the rich often argue for their beneficial effects on economic performance. This line of reasoning, focusing on efficiency gains and the removal of behavioral distortions, has been central to the arguments made for several major tax reforms in the USA

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(Auerbach and Slemrod, 1997; Bartels, 2005; Gale and Samwick, 2017). There are few macro-level empirical studies exploring the relationship between taxes on the rich and economic performance, however, and the evidence we do have is mixed. While some studies find higher top marginal income tax rates and tax progressivity adversely affect economic growth (Padovano and Galli, 2002; Gemmell et al., 2014), a number of other studies find no significant association (Lee and Gordon, 2005; Angelopoulos et al., 2007; Piketty et al., 2014).

On the other side of the debate, many opponents of tax cuts for the rich argue that they simply further concentrate income in the hands of the affluent. The pioneering work of Piketty and co-authors charting the evolution of top incomes over the course of the 20th century has shown that reductions in tax progressivity in recent decades have gone hand-in-hand with soaring income inequality, especially in the Anglo-Saxon countries (Atkinson and Piketty, 2007; Alvaredo et al., 2013; Piketty, 2014). This is supported by evidence from cross-country panel studies that have found that lower taxes on the rich, especially top marginal income tax rates, are strongly associated with rising top income shares (Roine et al., 2009; Volscho and Kelly, 2012; Piketty et al., 2014; Huber et al., 2019).

Given the lack of consensus in existing empirical analyzes and the difficulties of making causal inferences from macro-level panel data analyzes, it remains an open empirical question how cutting taxes on the rich affects economic outcomes. We believe the question is best answered by looking at the effects of major tax cuts packages, as the story of taxing the rich in the advanced democracies over the past 50 years is one of discrete and stark changes in policy. For example, Ronald Reagan implemented two major packages of tax cuts for the rich in his time in the White House, one in 1981 and another in 1986. A similar pattern of large, infrequent tax cuts characterized Thatcher's tax reforms in the UK, as well as reform trajectories in many other advanced democracies (see Section 3).

Focusing on the effects of individual reforms also allows us to apply a new statistical approach for causal inference in observational studies that applies a novel matching method to pooled time series data. This is particularly pertinent in this case, as there is a large literature on the power of rich voters and organized business interests to shape public policies (including tax policies) in their favor (Gilens, 2005; Bartels, 2009; Hacker and Pierson, 2010; Svallfors, 2016; Emmenegger and Marx, 2019), which suggests reverse causality could be a major issue in empirical studies lacking a clear identification strategy.

There are only a handful of existing macro-level studies exploring the economic consequences of specific tax cuts for the rich and their external validity is constrained by focusing on a small number of tax cuts (Saez, 2017; Rubolino and Waldenström, 2020) or on tax reforms in a single country (Zidar, 2019). This article takes a wider lens, looking at *all* major reductions in taxes on the rich across 18 OECD countries from 1965 to 2015. We also draw on a more comprehensive indicator of taxes on the rich, which takes into account changes across an array of taxes on top incomes, assets, and capital. This approach allows us to draw more generalizable conclusions. It also provides researchers with a new dataset of major tax cuts for the rich that can be utilized for future empirical analyzes.

Our results show that major tax cuts for the rich increase income inequality in the years following the reform ($t+1$ to $t+5$). The magnitude of the effect is sizeable; on average, each major reform leads to a rise in top 1% share of pre-tax national income of over 0.7 percentage points. The results also show that economic performance, as measured by real Gross Domestic Product (GDP) per capita and the unemployment rate is not significantly

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affected by major tax cuts for the rich. The estimated effects for these variables are statistically indistinguishable from zero, and this finding holds in both the short and medium run.

Our findings on the effects of growth and unemployment provide evidence against supply side theories that suggest lower taxes on the rich will induce labor supply responses from high-income individuals (more hours of work, more effort, etc.) that boost economic activity (see standard models of optimal labor income taxation in Saez, 2001 and Piketty and Saez, 2013). Relatedly, they also show little support for the influential political–economic idea that tax cuts for the rich 'trickle down' to boost wider economic performance (Sowell, 2012). They are, in fact, more in line with recent empirical research showing that income tax holidays, windfall gains and tax cuts targeted at the top decile of the income distribution do not lead individuals to significantly alter the amount they work (Akee et al., 2010; Jones and Marinescu, 2018; Martínez et al., 2021; Zidar, 2019).

Overall, our analysis finds strong evidence that cutting taxes on the rich increases income inequality but has no effect on growth or unemployment. We employ a measure of top 1% share of pre-tax national income that includes both labor and capital income, which makes it less likely that tax shifting and avoidance are driving the results. In fact, our results are more in line with Piketty et al. (2014), who suggest that lower taxes on the rich encourage high earners to bargain more forcefully to increase their own compensation, at the direct expense of those lower down the income distribution.

The remainder of the article is structured as follows. Section 2 explores the existing literature on the economic effects of cutting taxes on the rich. Section 3 sets out our data and empirical strategy. We present our headline results in Section 4, before carrying out a variety of robustness tests in Section 5. Lastly, Section 6 concludes.

2. The economic effects of cutting taxes on the rich

The 20th century was one of immense change in the tax systems of advanced democracies. Highly progressive income taxes arose in the wake of the two World Wars, with average top marginal income tax rates still standing at around 60% in the early 1980s. That decade proved to be a major turning point, however, and average rates have since fallen to under 40% (Scheve and Stasavage, 2016; Kiser and Karceski, 2017). This trend was mirrored in other taxes on the wealthy and corporations, which also dropped sharply over the past half century (Hope and Limberg, 2021).

A large body of work that spans economics, sociology and political science has sought to explore the causes and consequences of this widespread and significant reduction in tax progressivity. Scheve and Stasavage's (2010, 2012, 2016) pioneering historical research argues that progressive systems emerged due to mass conscription for war, but that the strength of these compensatory demands for fiscal fairness have weakened over time, leading to falling progressivity. Other scholars point to the role of major structural changes in the advanced democracies, such as capital mobility and trade (Swank and Steinmo, 2002), international tax competition (Genschel and Schwarz, 2011), and the rise of the knowledge economy (Hope and Limberg, 2021), in undermining the highly progressive tax systems of the post-war era. Lastly, Blyth (2002) and Swank (2006, 2016) find evidence that the diffusion of neoliberal economic ideas from the USA was crucial to driving the major tax reductions seen elsewhere.

There is already a substantial theoretical literature on the economic effects of cutting taxes on the rich. There are a number of lines of reasoning in that literature that predict

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positive effects of cutting taxes on the rich on economic performance. Standard models of optimal labor income taxation (see, e.g. the textbook models in Saez, 2001 and Piketty and Saez, 2013) predict a positive labor supply response from high-income individuals to lower top tax rates (e.g. working more hours) that boosts overall economic activity. On a more macro-level, theoretical models of economic growth typically predict that more progressive tax systems dampen economic performance by stifling investment in physical and human capital (Gemmell *et al.*, 2014). Recent work has also drawn a link between taxes on the rich and productivity. In so far as taxes on the rich approximate for taxes on entrepreneurs, lower taxes on the rich may stimulate growth and employment by encouraging risk-taking, innovation and entrepreneurship (Lee and Gordon, 2005; Arnold *et al.*, 2011).

On the other side of the coin, there are theories that predict adverse economic effects from reducing taxes on the rich. The most prominent theory relates to the bargaining power of CEOs and other top executives. When taxes on top incomes are lower, high earners have more to gain from aggressively bargaining to increase their own compensation. This rise in unproductive, rent-seeking behavior pushes up incomes at the top but at the expense of employment and growth in the wider economy (Alvaredo *et al.*, 2013; Piketty *et al.*, 2014). Another important strand of the literature focuses on the relationship between corporate income taxes and corporate savings. General equilibrium models with capital and product market imperfections predict that corporate income tax cuts will increase corporate savings (Chen *et al.*, 2017). Related empirical work shows that corporate income tax cuts have indeed contributed to the stark accumulation of savings among non-financial firms in advanced economies in recent decades, which have been largely stashed in financial markets instead of being reinvested in ways that stimulate growth and employment (Redeker, 2021).

While there is clear ambiguity in the theoretical literature on the predicted effects of cutting taxes on the rich on growth and employment, there is more consensus when looking at the predicted effects on income inequality. There are three main arguments in the literature for why we would expect lower taxes on the rich to be associated with higher income inequality, as measured by the pre-tax income share of the top 1% (Huber *et al.*, 2019). The first is that lower taxes on the rich improve the work incentives of high earners, leading them to accrue more earned income, as well as raising their incentives to invest, boosting capital incomes (see the discussion of the relevant literature in Volscho and Kelly, 2012). The second argument relates to tax evasion and avoidance. When taxes on the rich are lower, this may reduce the incentives for shifting taxable incomes into other time periods or bases to minimize tax liabilities (Piketty *et al.*, 2014; Rubolino and Waldenström, 2020). Third, as already outlined previously, lower taxes on the rich may increase the incentives of top executives to bargain forcefully for higher compensation (Alvaredo *et al.*, 2013; Piketty *et al.*, 2014).

Turning to the growing empirical literature on the economic effects of cutting taxes on the rich, we also see more ambiguity when looking at the effects on economic activity. Several prominent cross-country panel data analyzes have found that tax progressivity is not significantly associated with economic growth (Lee and Gordon, 2005; Angelopoulos *et al.*, 2007; Piketty *et al.*, 2014), although there are some exceptions, which find adverse effects of more progressive tax systems on economic performance (Padovano and Galli, 2002; Gemmell *et al.*, 2014). Studies using similar methodologies that explore the relationship between taxing the rich and income inequality tend to find a strong negative association between top marginal income tax rates and top income shares. In other words, they find that falling taxes on the rich since the 1980s have coincided with rapidly rising income

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inequality, especially at the top of the distribution (Roine *et al*., 2009; Volscho and Kelly, 2012; Piketty *et al*., 2014; Huber *et al*., 2019).

A pitfall of a large portion of the empirical literature on the consequences of falling tax progressivity is that it does not take into account the typical pattern of tax reform within countries. Tax cuts for the rich in the advanced democracies have mostly been stark and irregular, as shown in Section 3. It is therefore important to look at the consequences of individual reform packages if we hope to understand the economic effects of cutting taxes on the rich.

There are few existing studies that estimate the economic effects of instances of major tax reform on the rich. Saez (2017) analyzes the 2013 tax increase on the rich in the USA and finds it has only short-term effects on income inequality. In another analysis focused on the USA, Zidar (2019) exploits regional variation to look at the growth and employment effects of cutting taxes on different part of the income distribution. He finds that the effect of cutting taxes on the top 10 percent on employment growth is small. The closest study to ours, however, is Rubolino and Waldenström (2020). They utilize the synthetic control method and find that three major reductions in top marginal income tax rates in Australia, New Zealand and Norway had lasting and large positive effects on top income shares, but no significant effects on economic growth. We build upon this study by identifying major reductions in tax progressivity using a more comprehensive measure of taxes on the rich that goes beyond income tax progressivity. We also look at all major reductions in taxes on the rich across 18 OECD countries from 1965 to 2015, which strengthens the generalizability of our results.

Our research is also closely related to a nascent strand of experimental research in political economy that looks at how citizens' preferences for taxing the rich are shaped by their views about how the economy works (Barnes, 2021) and by their beliefs in the extent to which the benefits of tax cuts for the rich 'trickle down' to those lower down the income distribution (Stantcheva, 2021).

3. Data and empirical strategy

Estimating the effect of tax cuts for the rich faces two major empirical problems: measuring taxes on the rich and isolating the effect of tax reforms. First, governments do not levy solely one single tax on the richest members of society. Instead, they have a broad toolkit of different tax policy instruments. They can lay taxes on high personal incomes, capital and assets such as inheritances, immovable property, and net wealth. All of these different taxes are commonly seen as highly progressive as they target the richest members of society (Messere *et al*., 2003). Most studies solely focus on one single type of tax. While some authors look at taxes on personal income (Ganghof, 2006; Cansunar, 2020), others focus on corporate taxation (Genschel *et al*., 2011) or taxes on assets like inheritance taxes (Graetz and Shapiro, 2005; Scheve and Stasavage, 2012; Lierse, 2021; Limberg and Seelkopf, 2021) and net wealth taxes (Lierse, 2021; Limberg and Seelkopf, 2021). Although such a zoomed-in focus allows researchers to trace policy-making in one particular field of progressive taxation, it overlooks the fact that all of these taxes can serve as substitutes for one another. For instance, a government might choose to raise taxes on the rich by increasing taxes on inheritances while keeping income taxes the same (or vice versa). Thus, solely focusing on one of these taxes fails to adequately capture overall taxes on the richest members of society as it overlooks the variety of tax policy instruments at hand (Capano and Lippi, 2017; Durazzi, 2020). Furthermore, empirical studies often have to make decisions about how to

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operationalize tax policy-making (Genschel, 2002; Swank and Steinmo, 2002). For instance, some scholars argue that statutory tax rates are more capable of capturing actual policy changes (Lierse and Seelkopf, 2016; Limberg, 2019), while others prefer measures such as effective tax rates or revenue that take the definition of the tax base into account (Osterloh and Debus, 2012). In sum, there is no clear and comprehensive approach to measuring taxes on the rich, as scholars have looked at different taxes as well as different indicators.

Second, estimating the effect of macro-level tax policy change from a comparative perspective faces endogeneity problems. Most studies have analyzed long-term developments of tax indicators and economic outcomes. For instance, Huber *et al.* (2019) find that the top personal income tax rate is negatively correlated with top 1% income shares. Furthermore, some studies find negative correlations between progressive taxation and economic growth (Gemmell *et al.*, 2014), while others find no significant association (Lee and Gordon, 2005; Piketty *et al.*, 2014). These existing studies are important as they detect broad correlations between macro-level dynamics over the long run of history. However, they are only able to make limited claims about the causal effects of specific tax cuts. For instance, if countries that experience lower economic growth rates are more likely to cut taxes on the rich, results will be biased. Hence, we have to compare countries with similar socio-economic trajectories. In more technical terms, this means that we need an approach that allows us to check whether trends of our outcome variables are parallel prior to treatment. Furthermore, we have to ensure that countries with and without a tax cut do not differ with regards to other important variables.

In order to tackle these two problems, we proceed in two steps. First, we use a new, comprehensive measure to identify major tax cuts for the rich. Second, we employ a new approach for panel data analysis that combines matching methods with a difference-in-differences estimation (Imai *et al.*, 2021). We explain each step in turn.

3.1 Identifying major tax cuts for the rich

In order to build a more complete picture of how taxes on the rich have evolved over time in the advanced economies, we use a newly constructed, comprehensive measure of taxes on the rich (Hope and Limberg, 2021). The measure utilizes Bayesian latent variable analysis (Lee, 2007) and covers three types of taxes that fall predominantly on the rich: taxes on top incomes, capital and assets. Table 1 shows the seven indicators that feed into the comprehensive measure, as well as their coverage and sources. For each of the three tax types, there is a measure of both top statutory tax rates and effective tax rates or tax revenues (as a percentage of GDP).

Modeling taxes on the rich as a latent variable that relies solely on the shared variance of commonly used indicators allows for the creation of a measure that is comparable across countries and over time. This approach has three advantages. First, it looks beyond a single tax policy measure. Instead, identifying common variation across a range of different taxes and indicators that are typically used to measure taxes on the rich. Solely relying on statutory tax rates would crucially, overlook reforms that alter the definition of the tax base. Hence, our comprehensive indicator incorporates changes in the definition of the tax base, as well as changes statutory tax rates. Second, the approach avoids the need for fine-grained micro-data and is less sensitive to aggregation rules (Saez and Zucman, 2019). Finally, compared to classic factor analysis based on frequentist models, Bayesian latent variable analysis is particularly robust to missing values, which allows the measure to cover a longer time span. In total, the measure covers 18 OECD economies over five decades (1965–2015). The latent variable is estimated using a Bayesian Markov-Chain Monte Carlo (MCMC) approach with a single

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Table 1 Indicators and data sources for Bayesian latent variable analysis

Tax type	Indicator	Time span	Source
Income	Top personal income tax rate	1965–2015	Scheve and Stasavage (2016), expanded by the authors for the years 2011–2015.
Income	Effective tax rate on top 1% wage earners	1980–2007	Egger *et al.* (2019)
Income/Capital	Top tax rate dividend income	1981–1999; 2000–2015	OECD (2020a)
Capital	Corporate income tax rate	1965–2015	Lierse and Seelkopf (2016), expanded by the authors for the years 1965–1980 and 2011–2015.
Capital	Effective tax rate on capital	1965–2015	McDaniel (2007)
Assets	Top inheritance tax rate	1965–2015	Scheve and Stasavage (2016), expanded by the authors for the years 2011–2015.
Assets	Revenue from taxes on assets (inheritance, net wealth, and property taxes, % of GDP)	1965–2015	OECD (2020a)

dimension, diffuse normal priors, three MCMC chains and 1000 burnin iterations (Lee, 2007; Merkle and Rosseel, 2018; Hanson and Sigman, 2021; Hope and Limberg, 2021).

Figure 1 shows the development of the taxing the rich indicator in the sample.[1] In line with other empirical studies that have found substantially declining taxes on the rich in the last decades (Genschel and Schwarz, 2011; Genschel *et al.*, 2011; Scheve and Stasavage, 2016), the indicator decreases substantially from the mid-1980s onwards. From the late 1960s to the end of the 1990s, the average value of the latent variable for taxes on the rich across the sample dropped by more than 30%. Furthermore, the cross-sectional standard deviation (SD) of the indicator steadily declined from the late 1960s. This indicates that tax policies on the rich have converged among OECD countries over time (Kemmerling, 2010).

In a second step, we use the latent variable to detect major tax cuts for the rich. We calculate country-specific first-differences of the indicator and then define major tax cuts as years in which the indicator drops by at least 2 SD.[2] Since we are interested in the effects of major tax cuts for the rich, this high threshold is in line with our theoretical focus. Furthermore, 2 SD shocks are often employed in the empirical literature in macroeconomics (Fernández-Villaverde *et al.*, 2015) and this size threshold is in line with the size of tax and spending changes identified in the literature exploring the effects of large fiscal policy adjustments on economic outcomes (Blanchard and Perotti, 2002; Alesina and Ardagna, 2010).

Figure 2 visualizes the resulting binary variable, which shows years in which taxes on the rich were reduced substantially.[3] In total, we identify 30 country–year observations

1 See Supplementary Appendix Figure A1 for country-specific time series.
2 Standard deviations are calculated based on the variance of the indicator in the whole sample.
3 Supplementary Appendix Figure A2 shows how changes in the latent variable translate into the binary variable of major tax cuts for the rich based on the 2 standard deviations threshold.

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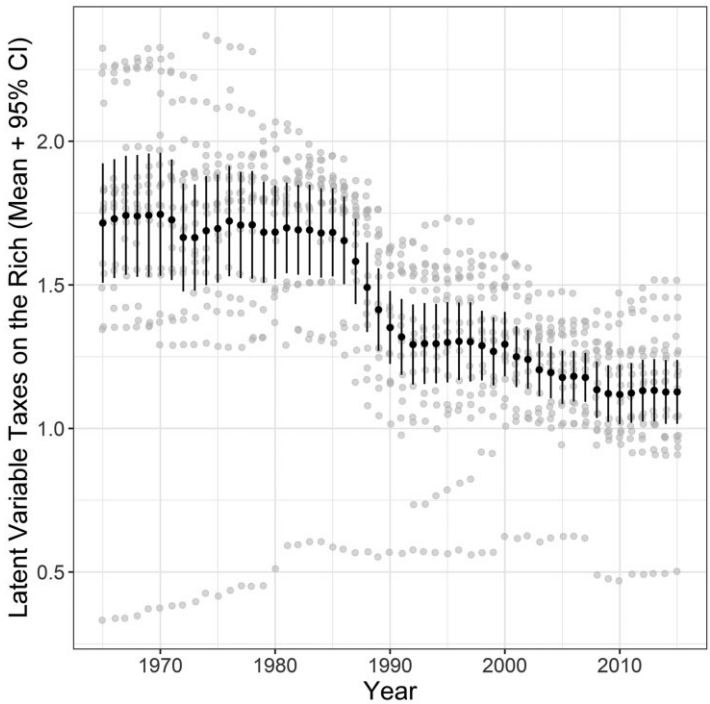


Figure 1. Latent variable for taxes on the rich, 18 OECD countries, 1965–2015. *Note:* Gray dots show country–year observations. Black dots with lines show year-specific means with 95% confidence intervals.

where taxes on the rich were significantly reduced. Governments enacted major tax reforms across the whole observation period and only two countries in the sample (France and Switzerland) did not see any major tax cuts. Many countries implemented major tax cuts for the rich in the late 1980s and early 1990s. Furthermore, the identification of tax cuts is also in line with previous studies that have focused on income tax progressivity (Rubolino and Waldenström, 2020) or on overall tax progressivity within specific countries (Saez and Zucman, 2019). For instance, echoing these authors' findings, we find two major reforms that reduced taxes on the rich significantly in the USA: 1982 (First Reagan Tax Cut) and 1986/1987 (Second Reagan Tax Cut). This approach also identifies other well-known examples of tax reforms that reduced progressivity. Among others, these include the Austrian tax reform in 1989 that cut taxes on top incomes, corporate profits, and capital income; the German tax reform of the red-green coalition, which was legislated in 2000 and became effective from 2001 onwards; Norway's 1992 tax reform that cut top income and corporate tax rates; and the major package of tax cuts in Sweden in 1991. However, we also identify tax cuts for the rich that have received a bit less attention, such as the Canadian reform that repealed the inheritance tax in 1971 and the corporate tax reform in Germany in 2008.

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3.2 Estimating the effect of major tax cuts for the rich

In order to estimate effects of cutting taxes on the rich on economic outcomes, our empirical design leverages variation in tax reform timings. Let us consider classical approaches of estimating economic effects from pooled time series data with N countries and T years. To date, most panel data analyzes on this topic rely on linear regression techniques with two-way fixed effects and control variables. Such models typically take the following form:

$$Y_{it} = \alpha_i + \gamma_t + \beta_0 X_{it} + \sum_{k=1}^{K} (\beta_k X_{kit}) + \varepsilon_{it}, \quad (1)$$

for $i = 1, \ldots, N$ and $t = 1, \ldots, T$, and where Y_{it} denotes our main outcome variable in country i and year t. β_0 is the estimated effect of the binary variable X_{it}, which measures major tax cuts for the rich. α_i is the unobserved time-invariant country-specific effect and γ_t is the unobserved year-specific effect. $\sum_{k=1}^{K} (\beta_k X_{\text{kit}})$ denotes a set of K time-varying covariates and ε_{it} is the error term.

Using such an approach to estimate the effect on economic outcomes of major tax cuts for the rich creates three methodological challenges. First, the effect of tax cuts may vary over time. However, Equation (1) requires the researcher to specify a lag of the treatment registration. For instance, Equation (1) would estimate the contemporaneous effect of tax cuts ($t + 0$). Second, related to this, the standard approach does not account for past tax cuts. Put differently, if $\beta_0 \neq 0$ for $X_{i,t-n}$, where $n \in \mathbb{N}$, estimating the effect of tax cuts might run danger of being biased due to previously implemented tax cuts. Thus, we need to compare cases with similar pre-treatment trajectories of tax cuts. Third, tax cuts do not come at random. Instead, political and economic factors might make major tax cuts on the rich more likely, and these factors can also affect subsequent macroeconomic dynamics. Furthermore, the practice of adding potential confounders as covariates, as in Equation (1), does not allow for the assessment of covariate balance.

To deal with these threats to identification, we use a new econometric approach that implements matching methods together with a difference-in-differences estimator for panel data analysis (Imai *et al.*, 2021). This technique compares units with a major tax cut for the rich in a respective year (treated units) with units that have a similar pre-treatment trajectory but did not enact a tax cut in the same year (control units). Furthermore, the method allows us to estimate how the treatment effects evolve over time. Most importantly, Imai *et al.* (2021) introduce F, which denotes the number of years after a major tax cut for the rich, and L, which denotes the number of periods prior to the treatment. Specifying F allows the researcher to estimate varying treatment effects over time. For instance, setting $F = 5$ measures the cumulative treatment effect for 5 years after a major tax cut for the rich. In contrast, L allows the researcher to adjust for treatment histories, e.g. $L = 5$ adjusts for the treatment history up 5 years prior to the treatment. Thus, the average treatment effect on the treated takes the following form,

$$\delta(F, L) = \mathbb{E}\left\{ \begin{array}{c} Y_{i,t+F}\left(X_{it} = 1,\ X_{i,t-1} = 0, \sum_{\ell=2}^{L} X_{i,t-\ell}\right) \\ -Y_{i,t+F}\left(X_{it} = 0,\ X_{i,t-1} = 0, \sum_{\ell=2}^{L} X_{i,t-\ell}\right) \mid X_{it} = 1,\ X_{i,t-1} = 0 \end{array} \right\}, \quad (2)$$

where countries that experience a major tax cut in year t are the treated unit, hence $X_{it} = 1$ as well as $X_{i,t-1} = 0$. Hence, $Y_{i,t+F}\left(X_{it} = 1,\ X_{i,t-1} = 0, \sum_{\ell=2}^{L} X_{i,t-\ell}\right)$ is the potential outcome for countries that have enacted a major tax cut and

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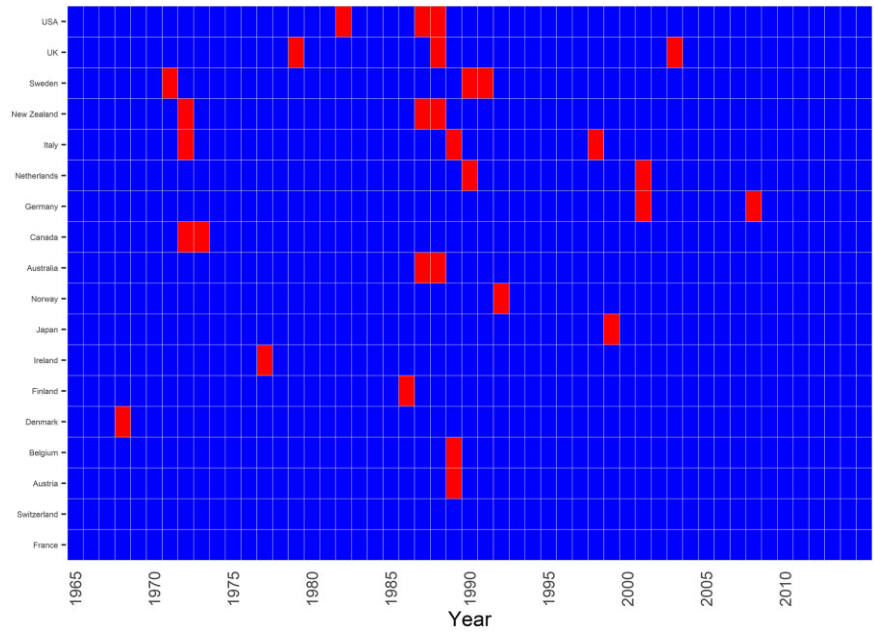


Figure 2. Distribution of major tax cuts for the rich, 1 965–2015. *Note:* Each square shows a country–year observation. Red squares show observations with a major tax cut for the rich, and blue squares show those without.

$Y_{i,t+F}\left(X_{it}=0,\ X_{i,t-1}=0, \sum_{\ell=2}^{L} X_{i,t-\ell}\right)$ is the counterfactual potential outcome. We are interested in the cumulative effect up to F years after a tax reform and adjust for treatment histories up to L years prior to a tax reform.

Unfortunately, the counterfactual outcome for treated countries, i.e. $Y_{i,t+F}\left(X_{it}=0,\ X_{i,t-1}=0, \{X_{i,t-\ell}\}_{\ell=2}^{L}\right) | \ X_{it}=1,\ X_{i,t-1}=0$, cannot directly be observed. Thus, we have to take the potential outcome for countries without a major tax cut for the rich instead:

$$\delta(F,L) = \mathbb{E}\left\{ \begin{array}{l} Y_{i,t+F}\left(X_{it}=1,\ X_{i,t-1}=0,\ \sum_{\ell=2}^{L} X_{i,t-\ell}\right) | \ X_{it}=1,\ X_{i,t-1}=0 \\ -Y_{i,t+F}\left(X_{it}=0,\ X_{i,t-1}=0, \sum_{\ell=2}^{L} X_{i,t-\ell}\right) | \ X_{it}=0,\ X_{i,t-1}=0 \end{array} \right\}. \quad (3)$$

However, tax cuts are not random. In particular, observed confounders, $\sum_{k=1}^{K}(X_{\text{kit}})$, as well as unobserved confounders can lead to biased results. Therefore, we use a difference-in-differences estimator as well as non-parametric matching techniques for additional time-varying covariates (Imai *et al.*, 2021). Matching is an intuitive and powerful tool to deal with selection into treatment (Ho *et al.*, 2007; Diamond and Sekhon, 2013). In contrast to adding confounders as covariates like in Equation (1), it is less prone to modeling decisions and allows for the assessment of covariate balance. Furthermore, the difference-in-differences estimator relaxes the unconfoundedness assumption, but crucially assumes a

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parallel trend in the outcome variable after adjusting via matching on the previous treatment history, $\sum_{\ell=2}^{L} X_{i,t-\ell}$, as well as on the covariate trajectory, $\sum_{\ell=0}^{L}\sum_{k=1}^{K}(X_{ki,t-\ell})$. Thus, we need to explicitly check whether the parallel trend assumption holds.

We use the block-bootstrap procedure proposed by Imai *et al.* (2021) to calculate standard errors. Following Otsu and Rai (2017) and Imbens and Rubin (2015), this approach circumvents the inference problems caused by standard bootstrapping procedures for matching by calculating the weight that each observation gets in the matching procedure. This weight-variable is used as a conditioning factor and is not recomputed in the bootstrapping procedure (Imai *et al.*, 2021, p. 12).

Our main treatment variable is the presence of a major tax cut for the rich (calculated as outlined in Section 3.1). The first dependent variable we look at is income inequality. The top 1% income share is the most commonly used measure in existing empirical studies looking at the relationship between taxes on the rich and income inequality (Roine *et al.*, 2009; Volscho and Kelly, 2012; Piketty *et al.*, 2014; Scheve and Stasavage, 2016; Huber *et al.*, 2019; Rubolino and Waldenström, 2020). To fit with the previous literature and because our theoretical focus is on the rich (see Section 2), we use the top 1% share of *pre-tax* national income from the World Inequality Database (Alvaredo *et al.*, 2018) as our measure of income inequality.[4] The measure includes both labor and capital income, and is calculated from administrative tax sources using a common methodology, so allows for comparison over time and across countries (Atkinson *et al.*, 2011). It is particularly important to note that this is a measure of market income inequality—i.e. it is *before* taking into account the operation of the tax/transfer system. There is therefore no mechanical feed through of changes in taxes on the rich on this measure of income inequality (Rubolino and Waldenström, 2020). Rather, any effects on income inequality from tax cuts for the rich will be due to behavioral responses (as per the mechanisms outlined in Section 2).

Second, we analyze whether tax cuts for the rich boost growth by looking at the effect on real GDP per capita. In line with other studies, we look at logged real GDP per capita (Piketty *et al.*, 2014; Rubolino and Waldenström, 2020). The data is from the Penn World table 9.1 (Feenstra et al., 2015) and is in 2011 US dollars. Finally, we analyze the effects on the labor market using harmonized unemployment rates from the OECD (2020).

In our models that include covariate matching, we include a battery of additional time-varying covariates, covering economic and political determinants of economic outcomes. We include capital account openness (Chinn–Ito Index; Chinn and Ito, 2006), trade openness (as a percentage of GDP; IMF, 2020), government expenditure (as a percentage of GDP; OECD, 2019), government debt (as a percentage of GDP; IMF 2020), left vote share in the last election (Brady *et al.*, 2020).[5]

4 We also re-run our analysis with two other measures of market income inequality—the market income Gini coefficient and the top 10% share of pre-tax national income (see Supplementary Appendix Figure A6). We choose to present these results as a robustness test of our main results (in Section 5), as these alternative measures of income inequality do not fit as closely with the theoretical focus of our analysis as the top 1% share of pre-tax national income (see Section 2).

5 See Supplementary Appendix Table A1 for the sources and summary statistics of the dataset. There are a small number of missing data points for top income shares (<10% of cases) and some of the additional covariates. In these cases, we have used an exponentially weighted 5-year moving averages interpolation procedure.

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4. Results

Figure 3 presents the results. Countries are matched upon their treatment and covariate histories up to 5 years prior to a tax cut (i.e. $L = 5$).[6] To differentiate between short- and medium-term effects of tax cuts for the rich, we look at the effects for up to 5 years after the reform (i.e. $F = 5$). For each year, the graph displays the cumulative treatment effect and 95% confidence intervals.

The upper panel shows that major tax cuts lead to a significant increase in inequality and that this effect becomes stronger with time. Three years after the major tax cut, the top 1% income share increases by 0.6 percentage points. Over 5 years, the top 1% share of pre-tax national income increases by more than 0.7 percentage points. This effect is highly statistically significant, with $P < 0.0001$. Furthermore, the top panel of Figure 3 also shows a placebo test by estimating the effect of tax cuts in the years before the reform. These placebo models test whether trajectories of inequality are significantly different in countries with and without a major tax cut for the rich prior to the reform actually taking place. The point estimates of the placebo tests are statistically indistinguishable from zero. Thus, we find no evidence that countries that cut taxes for the rich experience a stronger (or weaker) growth in inequality prior to tax reforms.[7] In other words, the findings show strong support for the parallel trends assumption that underlies the difference-in-differences estimator.

The middle panel of Figure 3 repeats the analysis but looks at the effect of major tax cuts for the rich on real GDP per capita. The results suggest that tax reforms do not lead to higher economic growth. The effect size of major tax cuts for the rich on real GDP per capita is close to zero and statistically insignificant. Major tax cuts for the rich do not lead to higher growth in either the short or medium run. Furthermore, we do not find any effect of tax cuts in the placebo tests. Countries with and without major tax cuts for the rich experience similar economic growth trajectories prior to reforms. Thus, the parallel trend assumption holds. We also calculated the same model by replacing (log) real GDP per capita with the real GDP per capita growth rate. Again, we find no significant effect of tax reforms on changes in real GDP per capita growth (see Supplementary Appendix Figure A4).[8]

Finally, we look at the effect of major tax cuts for the rich on unemployment. The lower panel of Figure 3 shows the results. In general, we see more fluctuation in the estimates. In the years immediately following the tax reform, the point estimates are negative. In the medium term, the estimates return to close to zero. However, none of these estimates are statistically significant at any conventional level. Furthermore, we see small fluctuations in unemployment rate trajectories prior to the tax reform. While the placebo estimate for period $t - 5$ is negative, unemployment grew slightly faster in the year directly before the reform. However, the point estimates in the placebo tests are all statistically insignificant and there is no clear trend in unemployment rates prior to tax cuts. In sum, although the results

6 Since we match upon covariate histories prior to tax cuts, controls should not be subject to post-treatment bias.

7 Since the model calculates the first differences in relation to the year before the tax reform (i.e. $t - 1$), the effect is always zero (by design) for this year.

8 Before matching, several variables showed significant imbalance with a standardized mean differences beyond the commonly accepted threshold of 0.25 (Rosenbaum and Rubin, 1985). After matching, the standardized mean differences no longer exceed this threshold. Supplementary Appendix Figure A3 shows changes in the standardized mean difference for different specifications.

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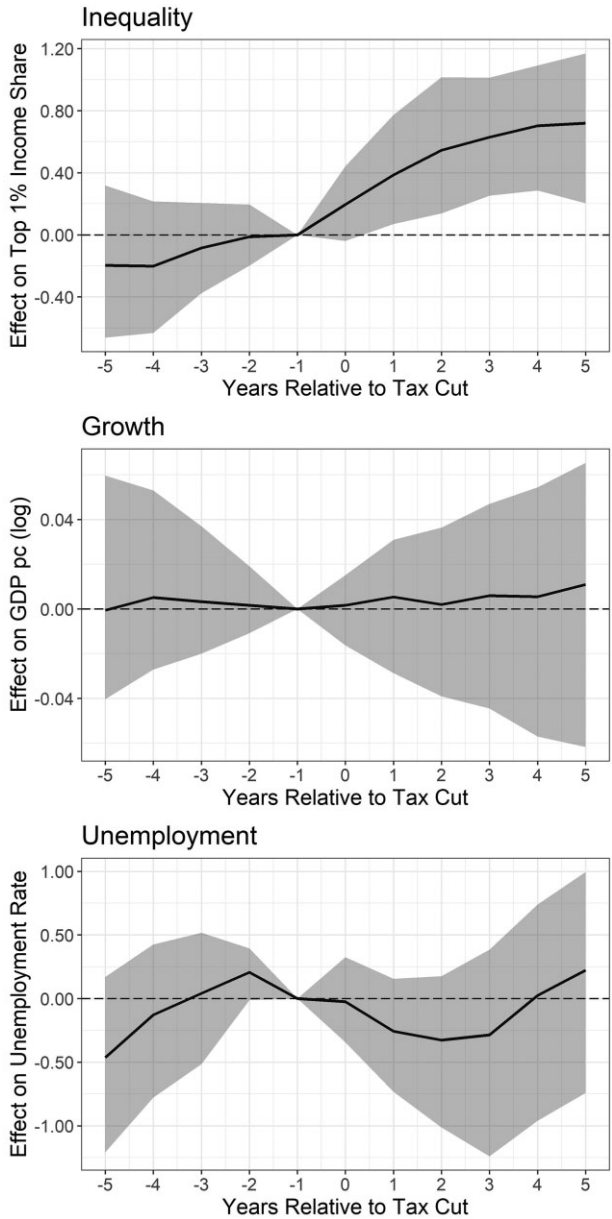


Figure 3. Effects of major tax cuts for the rich on inequality, growth, and unemployment, 1965–2015. *Note:* Solid black line shows point estimates. Gray shaded areas show 95% confidence intervals based on 5,000 bootstrap iterations.

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show slight indications of a flash in the pan effect of tax cuts for the rich on unemployment, these findings are neither statistically significant nor robust.

5. Robustness tests

We run several alternative specifications to check the robustness of our findings. Figure 4 visualizes the results for the different models. First, we repeat our main analyzes but solely match upon previous treatment trajectories. Not matching on covariates increases transparency, as it ensures that our results are not driven by covariate choices (Gelbach, 2016). Our results for the effect of major tax cuts for the rich on inequality, growth and unemployment hold.

Second, we match upon the propensity score instead of the Mahalanobis distance (Rosenbaum and Rubin, 1985). Although some studies have argued that propensity score matching is based on modeling assumptions and might therefore increase bias compared to non-parametric matching procedures (King and Nielsen, 2019), it is an intuitive and widely applied matching approach (Caliendo and Kopeinig, 2008). We apply two types of propensity score matching: one that solely matches upon the propensity score, and one that uses covariate balancing propensity scores (CBPS), which were designed to overcome common problems of propensity scores (Imai and Ratkovic, 2014). Our findings are robust to these alternative specifications.

Third, we use a weighting instead of a matching approach (Hirano *et al.*, 2003). Weights are calculated by using propensity scores as well as CBPS. Again, our main findings hold: major tax cuts for the rich lead to a higher top 1% income share. In fact, the effect size is even slightly larger than in the original model. In contrast, the coefficients for growth and unemployment remain statistically indistinguishable from zero.

Fourth, we apply a lower threshold of 1 SD to detect major tax cuts for the rich. Using a 1 SD threshold means that we include tax cuts of smaller magnitude. Hence, it is a more conservative approach of estimating the effects of tax cuts for the rich on economic outcomes. At the same time, this approach covers more tax cuts, which increases statistical power and ensures that our results are not driven by specific outliers. The findings hold when using this alternative threshold. Cutting taxes for the rich increases the top 1% share of pre-tax national income significantly and this effect persists over time. The new models using the lower threshold estimate that tax cuts for the rich lead to an increase in top 1% income shares of 0.6 percentage points. The smaller effect size is unsurprising, given the lower threshold for identifying major reforms. Furthermore, we find no effect of tax reforms on real GDP per capita. When looking at the effect on unemployment rates, the estimates show a slightly different pattern. Here, tax cuts for the rich lead to slightly higher unemployment rates immediately after the reform. However, this effect does not hold over time either. Hence, it supports our previous finding that tax cuts for the rich do not have a robust effect on unemployment.

In addition to the models shown in Figure 4, we have calculated a range of further robustness checks, which are presented in the Supplementary Appendix. We use a range of other thresholds for major tax cuts (Supplementary Figure A5), look at alternative measures of income inequality (top 10% income shares and market income Gini coefficients; Supplementary Appendix Figure A6), and analyze the effect of tax cuts on domestic and foreign direct investment (Supplementary Appendix Figure A7). Our central results hold across this wide range of additional robustness tests.

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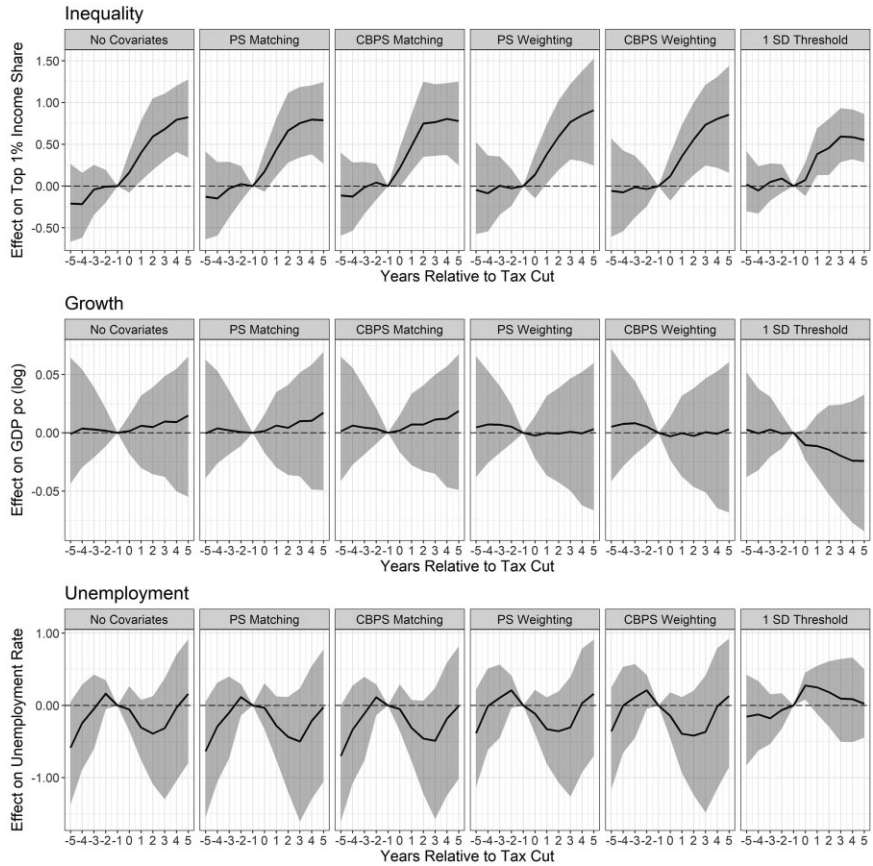


Figure 4. Robustness checks for the effects of major tax cuts for the rich on inequality, growth and unemployment, 1965–2015.

Note: Solid black line shows point estimates. Gray shaded areas show 95% confidence intervals based on 5,000 bootstrap iterations.

Another important point is that our newly constructed indicator of taxes on the rich takes both statutory tax rates and effective tax rates into account. Crucially, the measurement of effective tax rates partly relies on the underlying macroeconomic tax bases (e.g. corporate income). These tax bases, in turn, might be affected themselves by our outcome variables of interest. This should not pose a significant problem for our estimation for two reasons. First, both the numerator and the denominator of effective tax rates would be affected by economic dynamics. For example, GDP growth is unlikely to drive effective tax rates, as it is connected to both a larger tax base and, consequently, a higher tax burden. Second, we identify tax cuts prior to changes in our outcome variables. This temporal dimension rules out the possibility that our measurement of tax cuts is endogenous to changes in our outcome variables. However, our results also stay similar when running additional analyzes that identify tax cuts by relying on more conventional tax policy indicators (i.e. top

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personal income tax rates and the corporate income tax rate) instead of using our newly constructed indicator of taxes on the rich (Supplementary Appendix Figure A8).

6. Discussion and conclusion

Taxes on the rich have fallen substantially across the OECD in recent decades, but the economic consequences of this dramatic shift in tax policy are still not fully understood. This article aims to close this gap in our knowledge by utilizing a two-stage process to estimate the effects of major tax cuts for the rich on economic outcomes. First, we identify instances of major reductions in tax progressivity by looking at substantial falls in a new, comprehensive indicator of taxes on the rich that covers 18 OECD countries from 1965 to 2015. Second, we utilize a new statistical approach that applies matching methods to pooled time series data to estimate the effect of major tax cuts for the rich on income inequality, economic growth, and unemployment.

We find that major tax cuts for the rich push up income inequality, as measured by the top 1% share of pre-tax national income. The size of the effect is substantial: on average, each major tax cut results in a rise of over 0.7 percentage points in top 1% share of pre-tax national income. The effect holds in both the short and medium term. Turning our attention to economic performance, we find no significant effects of major tax cuts for the rich. More specifically, the trajectories of real GDP per capita and the unemployment rate are unaffected by significant reductions in taxes on the rich in both the short- and medium-term.

Our analysis provides some indicative evidence about the mechanisms underpinning the results. On the income inequality side, the results do not closely align with the theory that the rich have greater incentives to work and invest when their taxes are cut, given that we do not find any statistically significant effects on growth, unemployment or investment from cutting taxes on the rich. Given our measure of income inequality includes both realized capital gains and labor income, it is also unlikely the results are being driven by tax avoidance, because a significant part of avoidance takes the form of shifting income into capital (Piketty *et al.*, 2014). Rather, our results are most consistent with Piketty *et al.*'s (2014) argument that lower taxes on top incomes induce the rich to bargain more aggressively to increase their own rewards, to the direct detriment of those lower down the income distribution. Turning to our (null) results on economic growth and unemployment, it is more difficult to disentangle the mechanisms at work. It could be that cutting taxes on the rich does not affect economic activity, but it could equally be that there are positive and negative effects that are counteracting one another. The latter explanation would fit with the existing (ambiguous) theoretical literature on the relationship between taxes on the rich and economic performance (as outlined in Section 2).

Our results have important implications for current debates around the economic consequences of taxing the rich, as they provide strong evidence that cutting taxes on the rich increases top income shares, but has little effect on economic performance. These findings are in line with a growing pool of macro-level panel studies on the economic consequences of cutting top marginal rates of income taxation (Angelopoulos *et al.*, 2007; Piketty *et al.*, 2014), as well as Rubolino and Waldenström's (2020) synthetic control analysis of major tax cuts for the rich in Australia, New Zealand and Norway.

One open question is why our findings differ from the small number of studies that have found that higher top marginal income tax rates and tax progressivity adversely affect economic growth (Padovano and Galli, 2002; Gemmell et al., 2014)? One reason could be our

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more comprehensive approach that takes different taxes into account when measuring tax cuts for the rich. We have argued that this approach has several advantages over relying on single tax rates and indicators. Our results also hold, however, when using statutory tax rates as the main independent variable (as in Gemmell *et al*., 2014). Hence, the most likely reason for the difference in our findings is that we utilize new methods for causal inference with pooled time series data (Imai *et al*., 2021). In contrast to the methods used in previous work, this approach combines matching methods with a difference-in-differences design. This enables us to (a) identify the effect of tax cuts in the potential outcomes framework, (b) estimate varying treatment effects over time, and (c) check the covariate balance, previous treatment trajectories and the parallel trends assumption. This approach therefore allows us, unlike previous research, to estimate generalizable and dynamic causal effects of tax cuts.

In sum, this study finds that major tax cuts for the rich push up income inequality, but do not boost economic performance. It therefore provides strong evidence against the influential political–economic idea that tax cuts for the rich 'trickle down' to benefit the wider economy. The study also points to a number potentially fruitful avenues for future research. It remains puzzling why 'trickle down' ideas have been so powerful and persistent in tax policy-making in the advanced democracies despite the lack of macroeconomic benefits from cutting taxes on the rich. Further research is also needed to more rigorously test the specific mechanisms driving our results. Lastly, future studies could investigate the extent to which the results generalize to developing and emerging economies, as well as non-democratic regimes.

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Supplementary material

Supplementary material is available at *SOCECO Journal* online.

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