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The Economic Consequences of Major Tax Cuts for the Rich

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Abstract

This paper uses data from 18 OECD countries over the last five decades to estimate the causal effect of major tax cuts for the rich on income inequality, economic growth, and unemployment. First, we use a new encompassing measure of taxes on the rich to identify instances of major reductions in tax progressivity. Then, we look at the causal effect of these episodes on economic outcomes by applying a nonparametric generalization of the difference-in-differences indicator that implements Mahalanobis matching in panel data analysis. We find that major reforms reducing taxes on the rich lead to higher income inequality as measured by the top 1% share of pre-tax national income. The effect remains stable in the medium term. In contrast, such reforms do not have any significant effect on economic growth and unemployment.

Keywords: Tax cuts for the rich, income inequality, growth, unemployment, difference-in-differences, Mahalanobis matching

1. Introduction

Recent years have seen a resurgence in academic research on income inequality, driven by the influential body of work by Piketty and co-authors charting the evolution of top incomes in the advanced economies over the course of the 20th century (Alvaredo et al., 2013; Atkinson et al., 2011; Piketty, 2014). A central finding from that literature is that while top incomes fell for several decades after the Second World War, they turned a corner and began rising, most dramatically in the Anglo–Saxon economies, from the 1980s onwards (Alvaredo et al., 2013; Atkinson and Piketty, 2007). Correlational evidence from cross-country panel studies has found that lower taxes on the rich, especially top marginal income tax rates, are strongly associated with rising top incomes over this period (Huber et al., 2019; Piketty et al., 2014; Roine et al., 2009; Volscho and Kelly, 2012). Although, studies exploring the effects of individual tax reforms paint a less clear picture, with some finding persistent effects on income inequality (Rubolino and Waldenström, 2020) and others only short-term effects (Saez, 2017).

Proponents of tax cuts for the rich often argue for their beneficial effects on economic performance. In fact, this line of reasoning, focusing on efficiency gains and the reduction of behavioural distortions, was central to the arguments made for major tax reforms in the US (Auerbach and Slemrod, 1997; Bartels, 2005; Gale and Samwick, 2017). There are few empirical studies exploring the relationship between taxes on the rich and economic performance, however, and the evidence we do have is mixed. While some cross-country empirical studies find higher top marginal income tax rates and tax progressivity adversely affect economic growth (Gemmell et al., 2014; Padovano and Galli, 2002), a number of other studies find no significant association (Angelopoulos et al., 2007; Lee and Gordon, 2005; Piketty et al., 2014).

Given the lack of consensus in existing studies and the difficulties of drawing causal conclusions from macro-level panel data analyses, it remains an open empirical question how cutting taxes on the rich affects economic outcomes. In this paper, we use data from 18 OECD countries covering the last fifty years to investigate the effects of major tax cuts for the rich on income inequality, economic growth, and unemployment. We contribute to the existing empirical literature in two ways: first, we use a newly constructed, comprehensive measure of taxes on the rich to identify years in which major tax cuts occurred across a wide range of advanced economies; and second, we move beyond correlational evidence on the economic effects of taxing the rich by applying a novel matching method that allows for the estimation of causal effects from time-series cross-sectional data.

Our approach to identify major tax cuts utilises a newly constructed indicator of taxes on the rich (Hope and Limberg, 2020). The indicator was developed using Bayesian latent variable analyses covering a wide range of taxes and indicators, which allows for the comparison of taxes on the rich across countries and over time. We code major tax cuts as years in which the index fell by over 2 standard deviations. Across our sample of 18 OECD countries from 1965 to 2015, this provides us with 30 country-year observations where taxes on the rich were significantly reduced.

Our empirical identification strategy relies on our binary variable for major tax cuts for the rich, which permits us to apply a nonparametric generalization of the difference-in-differences indicator for panel data analysis (Imai et al., 2020). The technique uses Mahalanobis matching to compare treated and untreated units that have similar treatment histories and pre-treatment covariate trajectories. Furthermore, the methodology allows for explicit checks of covariate balance. We use this approach to estimate the causal effect of major reductions in taxes on the rich on income inequality, economic growth and unemployment. We obtain estimates of the cumulative effects for up to half a decade after the reform, so can also assess the extent to which effects persist over time.

Our results show that, for both matching methods, major tax cuts for the rich increase the top 1% share of pre-tax national income 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 0.8 percentage points. The results also show that economic performance, as measured by real GDP per capita and the unemployment rate, is not significantly 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 align closely with the existing correlational evidence showing that tax cuts for the rich are associated with rising top income shares (Atkinson and Leigh, 2013; Huber et al., 2019; Piketty et al., 2014; Roine et al., 2009; Volscho and Kelly, 2012). We make an important contribution to this literature, however, as our empirical strategy allows for the estimation of causal effects. This is particularly pertinent in this case, as there is a large political science literature on the power of rich voters and organised business interests to shape public policies (incl. tax policies) in their favour (Bartels, 2009; Emmenegger and Marx, 2019; Gilens, 2005; Hacker and Pierson, 2010; Svallfors, 2016), which suggests reverse causality could be a major issue in empirical studies lacking a clear identification strategy.

Existing causal evidence is limited to one study. Rubolino and Waldenström (2020) utilise 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. 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 advanced economies from 1965 to 2015, which allows us to draw stronger and more generalizable conclusions.

Our findings on the effects of growth and unemployment provide evidence against supply-side theories that suggest lower taxes on the rich will induce labour supply responses from high-income individuals (more hours of work, more effort etc.) that boost economic activity (see standard models of optimal labour income taxation in Piketty and Saez, 2013 and Saez, 2001). They are, in fact, more in line with recent empirical research showing that income tax holidays and windfall gains do not lead individuals to significantly alter the amount they work (Akee et al., 2010; Jones and Marinescu, 2018; Martinez et al., 2018).

Overall, our analysis finds strong evidence that cutting taxes on the rich increases income inequality but has no effect on growth or unemployment. We do not directly test mechanisms in our analysis, but using a measure of top 1% share of pre-tax national income that includes *both* labour and capital income makes it less likely that tax shifting and avoidance are driving the results. Our results are in line with those in Piketty et al. (2014), which 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 paper proceeds as follows. Section 2 sets out our approach for identifying major tax cuts for the rich and presents a visualisation of the resulting binary variable. Section 3 presents the data, empirical strategy, and results for our analysis of the effects of major tax cuts for the rich on income inequality. We turn our attention to the effects on growth and unemployment in Section 4, before carrying out a series of robustness tests in Section 5. Finally, Section 6 provides some concluding remarks.

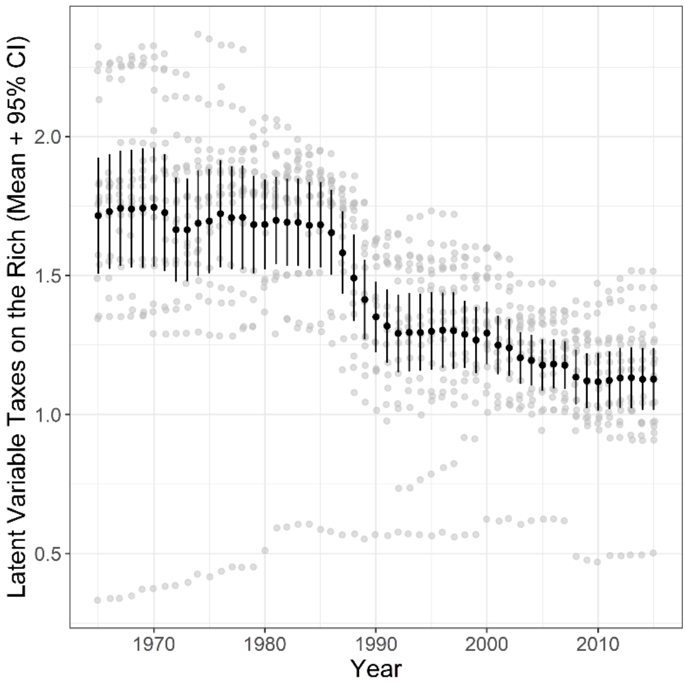
2. Identifying major tax cuts for the rich

Many empirical studies look at single tax policy indicators to identify tax cuts for the rich. However, there is some disagreement on measuring taxes on the rich in the literature. First, there is no consensus on which taxes to look at. Whilst some authors look at taxes on personal income (Egger et al., 2019; Rubolino and Waldenström, 2020), others focus on corporate taxation (Devereux et al., 2002) or inheritance taxation (Piketty and Saez, 2013b). Second, economists have used different tax policy indicators. Some look at top marginal income tax rates (Piketty et al., 2014), while others look at effective tax rates (Egger et al., 2019) or revenue generation (Baunsgaard and Keen, 2010). We propose an encompassing approach that utilises Bayesian latent variable analysis on a range of different taxes and indicators to overcome these problems. This allows us to detect shared variance across 7 indicators that are commonly used proxies for taxes on the rich (see Table A1 in the Appendix). In total, the data cover 18 OECD democracies over 5 decades (1965-2015). We estimate the latent variable using a Bayesian Markov-Chain Monte Carlo (MCMC) approach with diffuse normal priors, three MCMC chains and 1000 burnin iterations (for more information on the estimation of the latent variable, see Hope and Limberg, 2020).

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 (Egger et al., 2019; 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 of the indicator steadily declined from the late 1960s. This indicates that tax policies on the rich have converged among OECD countries over time.

[1] See Figure A1 in the Appendix for country-specific time series.

Figure 1. Latent variable for taxes on the rich, 18 OECD countries, 1965-2015



Source: Authors' calculations; Hope and Limberg (2020).

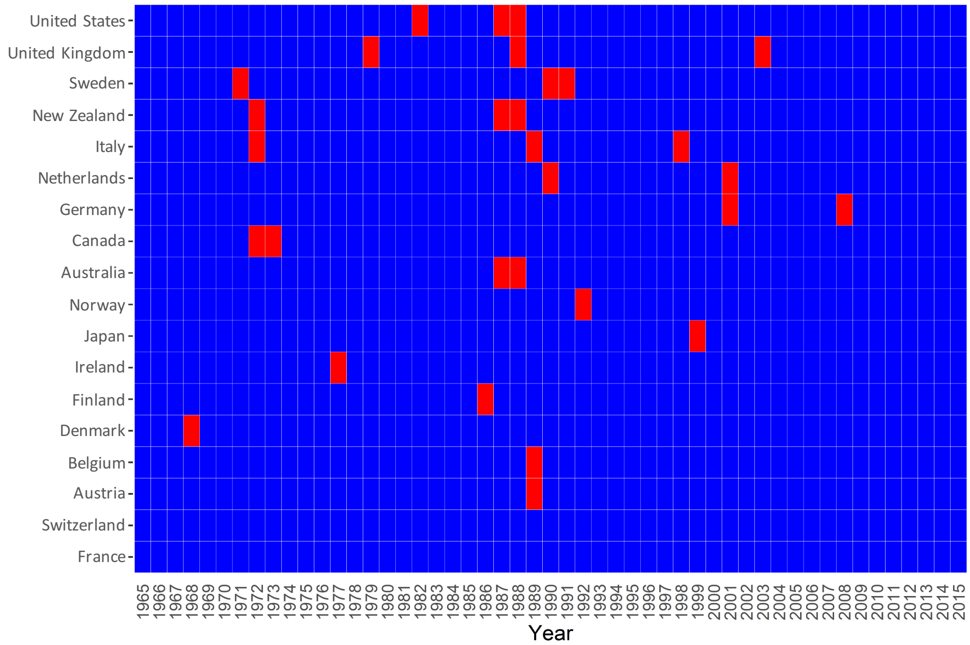
In a second step, we use the latent variable to detect major tax cuts for the rich. We calculate country-specific first-differences of the indicator and define major tax cuts as years in which the indicator drops by at least 2 standard deviations. Since we are interested in the effects of *major* tax cuts for the rich, this high threshold is in line with our theoretical focus. Furthermore, two standard deviation shocks are often employed in the empirical literature in macroeconomics (Dell' Erba et al., 2015; 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 (Alesina and Ardagna, 2010; Blanchard and Perotti, 2002).

Figure 2 visualises the resulting binary variable that picks out years in which taxes on the rich were reduced substantially.[2] In total, we identify 30 country-year observations where taxes on the rich were significantly reduced. Governments enacted major tax reforms in all countries in our sample and across the whole observation period. Many countries implemented major tax cuts for the rich in the late 1980s. Furthermore, the identification of

[2] Figure A2 in the Appendix shows how changes in the latent variable translate into the binary variable of major tax cuts based on the 2 standard deviations threshold.

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 single specific countries (Saez and Zucman, 2019). For instance, echoing these authors' findings, we find two major reforms that reduced taxes on the rich in the US: 1982 (First Reagan Tax Cut) and 1986/1987 (Second Reagan Tax Cut).

Figure 2. Distribution of major tax cuts for the rich, 1965-2015



Source: Authors' calculations.

3. The effect on income inequality of major tax cuts for the rich

Our empirical design leverages variation in tax reform timings to estimate effects of cutting taxes on the rich on income inequality. Let us consider classical approaches of estimating causal effects for time-series cross-sectional data with N countries and T years. Up to date, most analysis of panel data rely on linear regression techniques with two-way fixed effects and control variables. Such models typically take the following form:

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

for $i = 1, \ldots, N$ and $t = 1, \ldots, T$ and where Y_{it} denotes our main outcome variable and in country i and year t and 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. $\sum_{k=1}^{K}(\beta_k X_{kit})$ denotes a set of K time-varying covariates and ε_{it} is the error term. Using such an approach to estimate the causal impact of major tax reforms that cut taxes on the rich on income inequality creates three methodological challenges. First, the effect of reforms might 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 reform effect $(t + 0)$. Second, related to this, the standard approach does not account for previous reform trajectories. Put differently, if $\beta_0 \neq 0$ for $X_{i,t-n}$, where $n \in \mathbb{N}$, estimating the effect of tax reforms might run danger of being biased due to previous reform trajectories (Rubolino and Waldenström, 2020). Thus, we need to compare cases with similar reform trajectories. Third, reforms do not come at random. Instead, political and economic factors might make reforms more likely, and these factors can also affect subsequent income inequality dynamics. Furthermore, the practice of adding potential confounders as covariates like in Equation 1 does not allow for the assessment of covariate balance.

To deal with these challenges to causal identification, we use a new econometric approach that implements a nonparametric generalisation of the difference-in-differences indicator for panel data analysis (Imai et al., 2020). This technique compares units with a major tax reform in a respective year (treated units) with units that have a similar pre-treatment trajectory of tax reforms but have not enacted a tax cut in the same year (control units). Furthermore, the method allows us to estimate how the treatment effects evolves over time. Most importantly, Imai et al. (2020) introduce F, which denotes the number of years after a tax reform, and L, which denotes the amount of lags 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. As a consequence, the average treatment effect on the treated (ATT) takes the following form,

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

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

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

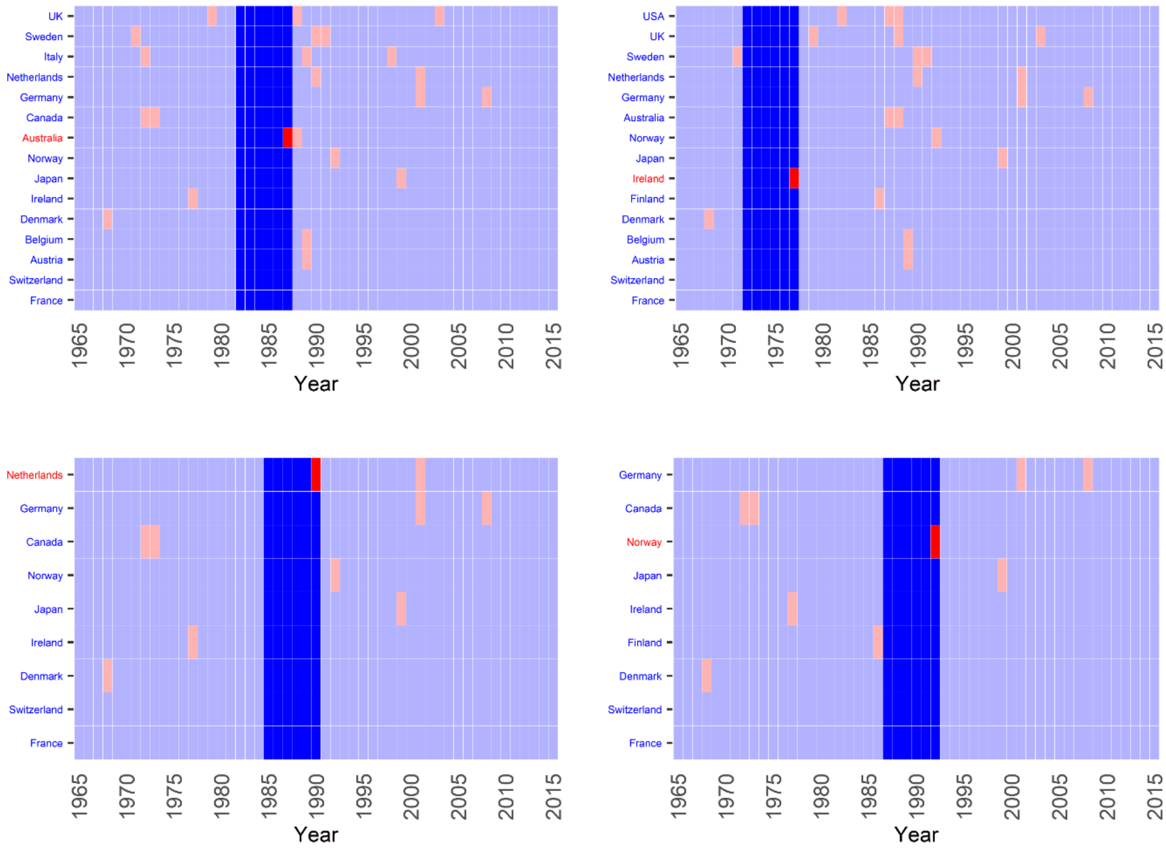
However, tax cuts are not random. In particular, observed confounders, $\sum_{k=1}^{K}(X_{kit})$, as well as unobserved confounders can lead to biased results. Thus, we use a difference-in-differences estimator as well as nonparametric matching techniques for additional time-varying covariates (Imai et al., 2020 p. 14). Matching is an intuitive and powerful tool to deal with selection into treatment (Diamond and Sekhon, 2012; Ho et al., 2007). In contrast to adding confounders as covariates like in Equation 1, it is less prone to modelling decisions and allows for the assessment of covariate balance. Furthermore, the difference-in-differences estimator relaxes the unconfoundedness assumption, but crucially assumes a 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. (2020) 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., 2020, p. 20).

Our main treatment variable is therefore the presence of a major tax cut for the rich. The first dependent variable we look at is income inequality, as measured by the top 1% share of pre-tax national income.[3] This measure includes both labour and capital income. Data come from the World Inequality Database (Alvaredo et al., 2018) and are calculated from administrative tax sources using a common methodology, so allow for comparison over time and across countries (Atkinson et al., 2011; Roine and Waldenström, 2015). Top income shares are also used in many existing studies looking at the relationship between taxes on the rich and income inequality (Huber et al., 2019; Piketty et al., 2014; Roine et al., 2009; Rubolino and Waldenström, 2020; Volscho and Kelly, 2012). We start by estimating the causal effect of major tax cuts for the rich on inequality while solely matching on the treatment trajectory. We match units based on a 5-year pre-treatment window (i.e. $L = 5$). Only 1 observation could not be matched to control units with a similar treatment trajectory (see also, Figure A3 in the Appendix). Figure 3 visualises the matching approach by looking at four exemplary cases of major tax cuts and the matched-on control groups.

[3] There are some missing data points for top income shares (less than 10% of cases). In these cases, we have used an exponentially weighted 5-year moving averages interpolation procedure.

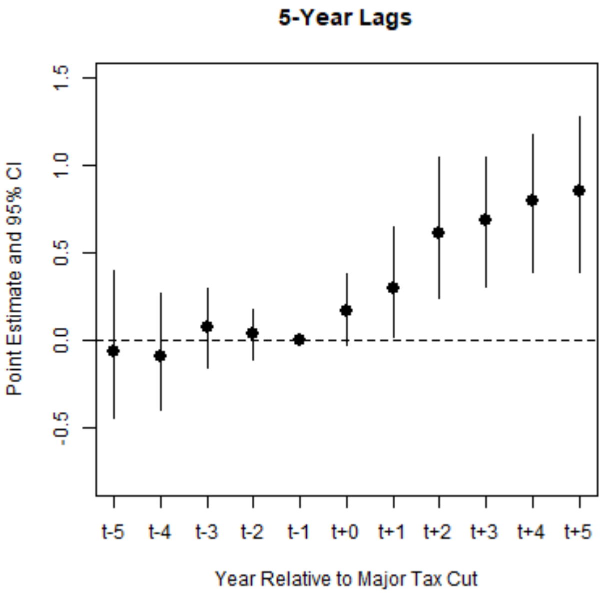
Figure 3. Visualisation of the matching approach, using four major tax cuts as examples



Source: Authors' calculations.

Figure 4 shows the treatment effect after matching on the previous treatment trajectory. In order 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 results show that major tax cuts lead to a significant increase in inequality and that this effect becomes stronger with time. Three years after the reform, the top 1% income share increases by almost 0.6 percentage points in countries with a major tax cut. Over five years, tax reforms increase the top 1% share of pre-tax national income by more than 0.8 percentage points. This effect is highly statistically significant, with P<0.0001. Furthermore, the graph also shows a placebo test by estimating the effect of tax cuts in the years before the reform. These placebo models test whether there have been significant different trajectories of inequality development in the countries with and without a major tax cut prior to the reform.[4] The point estimates of the placebo tests are close to zero and statistically insignificant. In other words, the findings show strong support for the parallel trends assumption that underlies the difference-in-differences estimator.

Figure 4. Effect of major tax cuts for the rich on top 1% income shares after matching on treatment trajectory



Source: Authors' calculations.

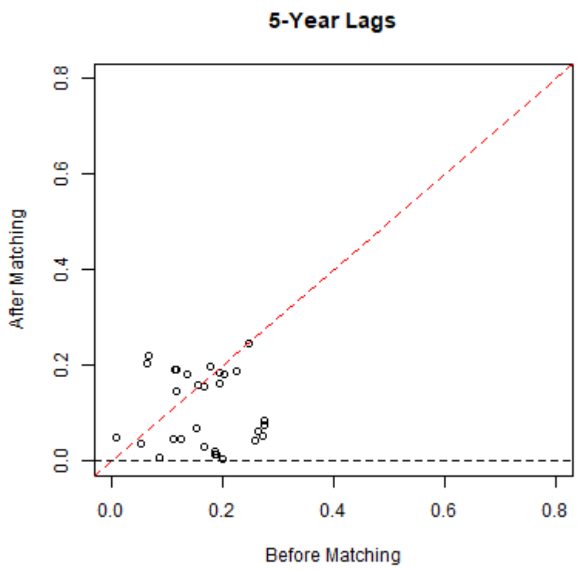
In a second step, we match units with a similar treatment history and with similar covariate trajectories. We match on a battery of time-varying covariates, covering economic factors, such as real GDP per capita (in 2011 US dollars), capital openness via the Chinn-Ito Index (Chinn and Ito, 2006), and trade openness (imports and exports as % of GDP), as well as political factors like the vote share of leftist parties and government expenditure (as % of GDP). Table A2 in the Appendix shows the different covariates and their data sources. We choose a matching approach that is based on the Mahalanobis distance as opposed to propensity score matching, as recent studies have found that the latter can be inefficient and create model dependency (King and Nielsen, 2019). Similar to the previous analysis, we

[4] Since the model calculates the first differences in relation to the year before the tax reform (i.e. t-1), the effect is 0 for this year.

match upon 5-year pre-treatment windows. Importantly, matching on pre-treatment values has the advantage that we do not have a problem with post-treatment bias.

Unlike multivariate regression analysis, the matching approach allows us to assess improvements in covariate balance. Figure 5 visualises the covariate balance by comparing standardised mean differences of the covariates before and after matching on the Mahalanobis distance. Before matching, several variables showed significant imbalance with standardised mean differences beyond the commonly accepted threshold of 0.25 (Rosenbaum and Rubin, 1985). After matching, the standardised mean differences no longer exceed this threshold.

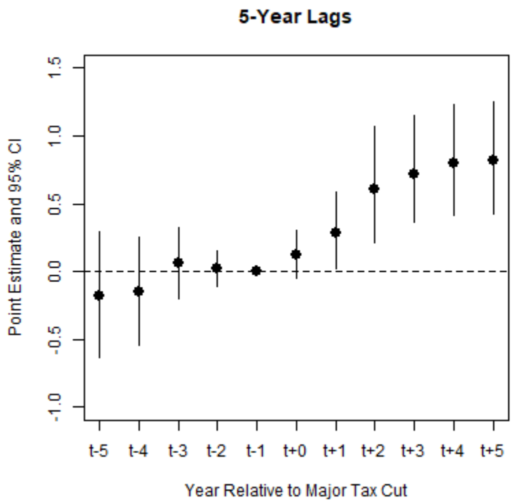
Figure 5. Standardised mean difference of covariates before and after matching



Source: Authors' calculations.

When rerunning the difference-in-differences model after matching on both pre-treatment treatment trajectories and covariate history, our results hold (Figure 6). Reforms that reduce taxes on the rich have a substantial short- and medium-term effect on the top 1% share of pre-tax national income. On average, such reforms increase the top 1% income share by more than 0.8 percentage points after 5 years. We run placebo tests by estimating the effect of major tax reforms on pre-treatment changes in inequality. In line with the previous findings, the parallel trend assumption holds. The point estimate for the time periods 4 and 5 years prior to the treatment are negative (around 0.2 percentage points), but fail to reach conventional levels of statistical significance.

Figure 6. Effect of major tax cuts for the rich on top 1% income shares after matching on treatment trajectory and covariates



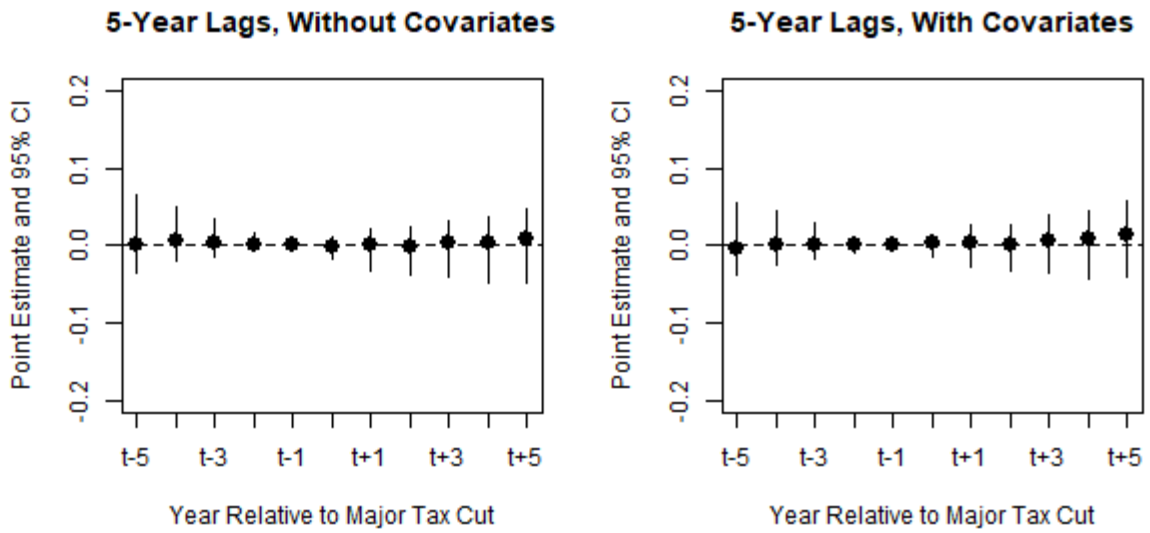
Source: Authors' calculations.

4. The effect on economic growth and unemployment of major tax cuts for the rich

Let us now turn to the effect of major tax cuts for the rich on economic growth and unemployment. First, we analyse whether such reforms 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). Again, we estimate the ATT by using a nonparametric generalisation of the difference-in-differences estimator. Like in the previous section, we match upon the treatment trajectory, as well as on an additional battery of covariates via the Mahalanobis distance. Models are matched upon 5-year pre-treatment windows and we look at the effects of up to 5 years after the tax reform. Again, we use the block-bootstrap procedure designed by Imai et al. (2020) to calculate standard errors for TSCS matching.

Figure 7 presents the results. The left panel shows the model without covariates. 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. The findings are very similar when matching upon pre-treatment covariate trajectories (right panel). 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 on pre-treatment changes. 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 GDP per capita growth (see Figure A5 in the Appendix).

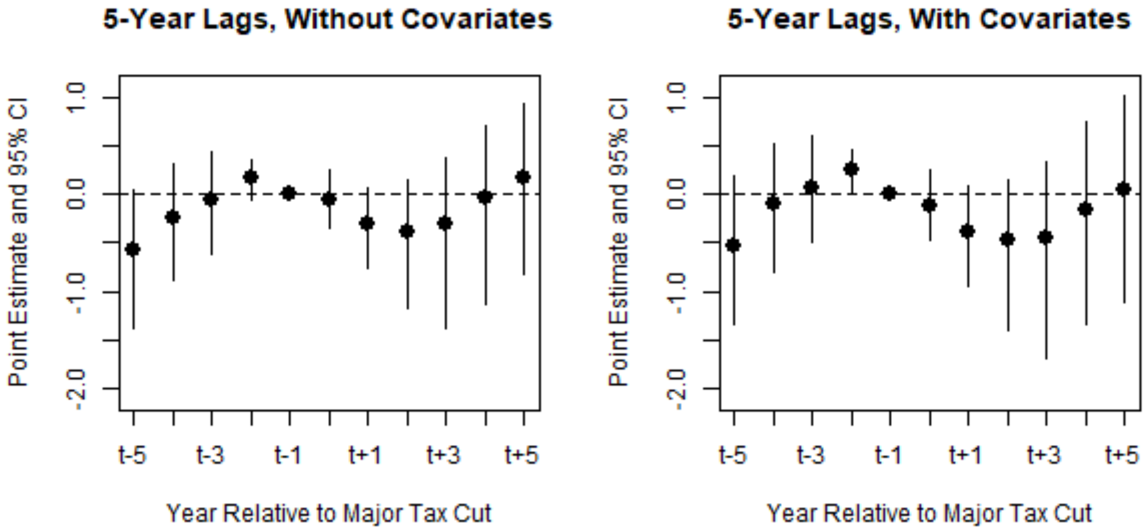
Figure 7. Effect of major tax cuts for the rich on (log) real GDP per capita after matching on treatment trajectory (left panel) and treatment trajectory and covariates (right panel)



Source: Authors' calculations.

Let us now look at the effect of major tax cuts for the rich on unemployment. To ensure cross-national comparability, we use harmonised unemployment rates as provided by the OECD (2020a). Figure 8 shows the results. In general, we see more fluctuation in the estimates. In the years right after the tax reform, the point estimate becomes negative. In the medium term, the estimate becomes close to zero again. However, none of these estimates is significant at the 0.05 level. Furthermore, we can see slight fluctuation in the development of unemployment rates prior to the tax reform. Whilst the placebo estimate for the $t - 5$ time period is negative, yet statistically insignificant, unemployment grew slightly faster in the year prior to the reform. However, these placebo tests do not show indications of a clear pre-treatment trend. In sum, although the results show very 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.

Figure 8. Effect of major tax cuts for the rich on unemployment rates after matching on treatment trajectory (left panel) and treatment trajectory and covariates (right panel)



Source: Authors' calculations.

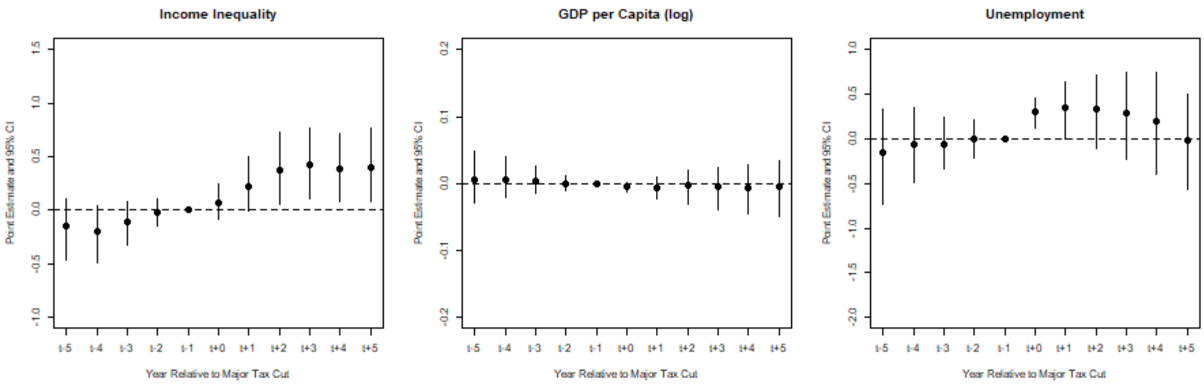
5. Robustness checks

We run several alternative specifications to check whether our results are robust. First, we apply a lower threshold of 1 standard deviation to detect major tax cuts for the rich. Using a 1 standard deviation threshold means that we include tax cuts of smaller magnitude. Hence, it is a more conservative approach of testing the impact of tax cuts on economic outcomes. Figure 9 visualises the effects on inequality, economic development, and unemployment.[5] 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 effect size decreases slightly. The new models using the lower threshold estimate that tax cuts lead to an increase of top 1% income shares by 0.5 percentage points. The smaller effect size is unsurprising given the lower threshold for identifying major reforms.

[5] Again, all models are calculated with 5-year lags and by matching on the Mahalanobis distance.

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 in the short term. However, this effect does not hold over time either. Hence, it supports our previous finding that the effects of tax cuts for the rich on unemployment rates are not robust.

Figure 9. Effect of major tax cuts for the rich, 1 standard deviation threshold for tax cuts

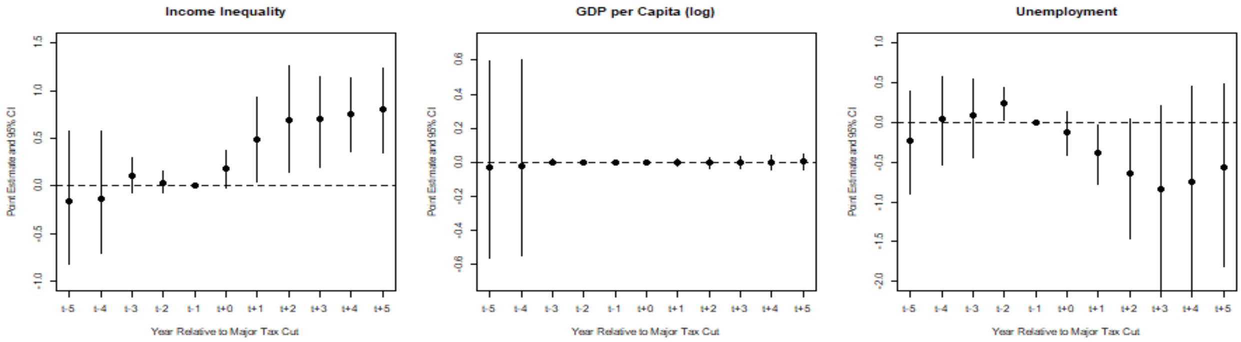


Source: Authors' calculations.

Second, we check the robustness of our findings by changing the length of our time lags. So far, we have calculated all models with 5-year lags. Choosing longer time lags has the advantage that it reduces potential bias from long-term effects of confounders. However, it also makes it harder to find suitable matches. Thus, we check our results by running models that use 1-year lags. This allows for more variance, but the findings are more vulnerable to bias. Figure 10 shows the results. In line with the previous analyses, tax cuts for the rich raise top 1% income shares significantly by around 0.7 percentage points in the medium run. In contrast, tax reforms do not lead to higher economic growth.[6] We find a slightly negative short-term effect on unemployment rates $(t + 1)$. However, this effect disappears in the medium run $(t + 2$ to $t + 5)$.

[6] The fact that this model matches on shorter time windows also leads to larger confidence intervals for the placebo tests.

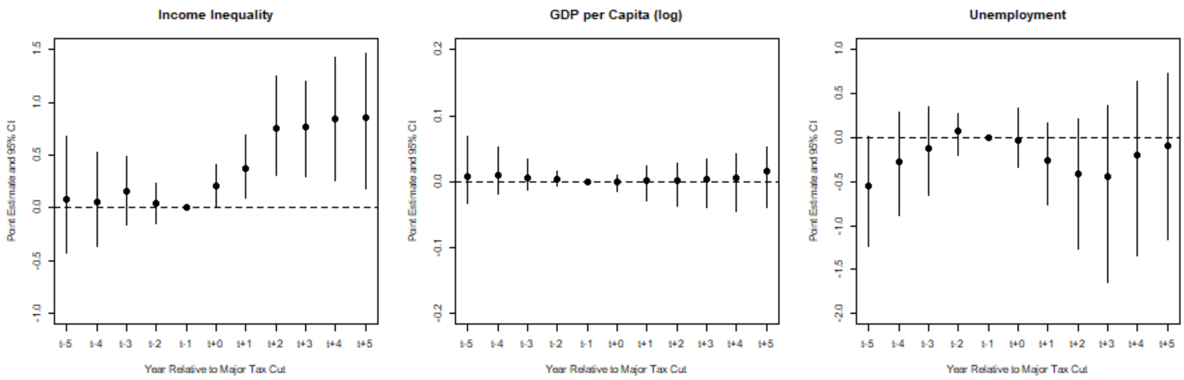
Figure 10. Effect of major tax cuts for the rich, 1-year lag



Source: Authors' calculations.

Third, we use a different matching technique. Instead of matching on the Mahalanobis distance, we match on propensity scores (Rosenbaum and Rubin, 1985). Although some studies have argued that propensity score matching is based on modelling assumptions and might therefore increase bias compared to nonparametric matching procedures (King and Nielsen, 2019), it is an intuitive and widely used matching approach (Caliendo and Kopeinig, 2008). Figure 11 presents the results when matching on propensity scores.[7] The results are very similar to our main approach that matches upon the Mahalanobis distance. Whereas tax cuts for the rich lead to higher income inequality, they do not have a robust effect on either economic growth or unemployment rates.

Figure 11. Effect of major tax cuts for the rich, propensity score matching



Source: Authors' calculations.

[7] In line with the main models, we use 5-year lags again.

6. Conclusion

This paper uses a two-stage process to estimate the causal 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 (greater than 2 standard deviations) in a new, comprehensive indicator of taxes on the rich that covers 18 OECD countries from 1965 to 2015. Second, we apply a nonparametric generalization of the difference-in-differences indicator that implements Mahalanobis matching in panel data analysis to estimate the causal 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 0.8 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 results have important implications for current debates around the economic consequences of taxing the rich, as they provide causal evidence that supports the growing pool of evidence from correlational studies that cutting taxes on the rich increases top income shares, but has little effect on economic performance (Lee and Gordon, 2005; Piketty et al., 2014; Roine et al., 2009). They also align with the causal findings in Rubulino and Waldenstrom (2020), but provide stronger and more generalizable conclusions, as our approach allows us to move beyond looking at tax changes in only handful of selected countries.

There are several potentially fruitful avenues for future research that come out of our analysis. While our choice of dependent variable (including both capital and labour income) makes it less likely the results are being driven by tax shifting or avoidance, we do not specifically test the mechanisms at work. Follow up research could therefore assess whether the macroeconomic effects we find are being driven by the mechanism outlined in Piketty et al. (2014), which is 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. The analysis could also be extended outside of the OECD to see if the findings hold in countries with lower fiscal capacity. Lastly, from a policy perspective, it would also be important to understand more about the extent to which individuals' attitudes to taxing the rich are influenced (or not) by the provision of new information about its economic consequences.

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Appendices

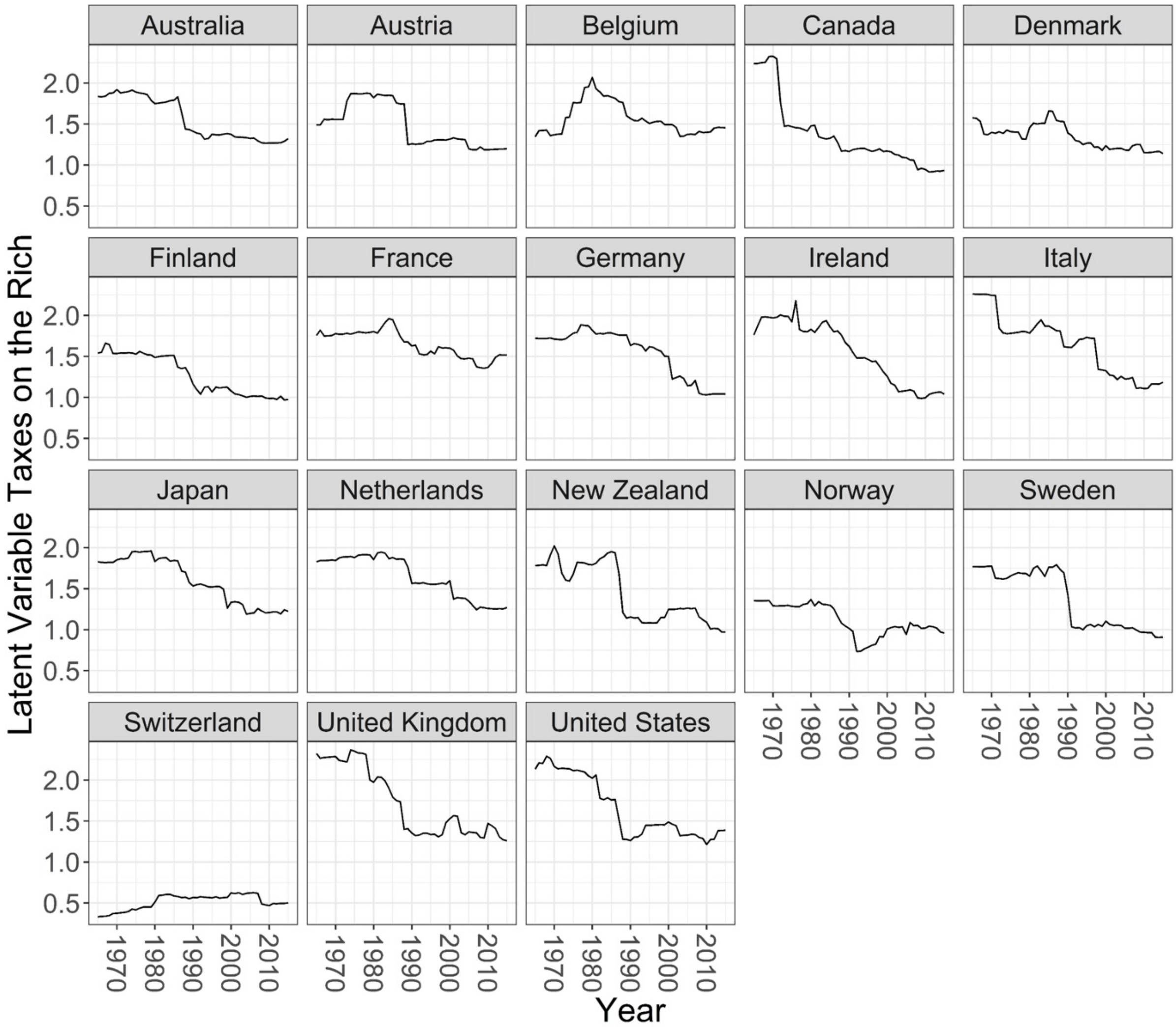
Table A1. Indicators and data sources for the Bayesian latent variable analysis

Tax type	Indicator	Time span	Source
Income	Top personal income tax rate	1965-2015	Scheve & 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 (2020b)
Capital	Corporate income tax rate	1965-2015	Lierse & 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 & 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 (2020b)

Table A2. Covariates and data sources

Indicator	Source
Top 1% share of pre-tax national income	Alvaredo et al. (2018)
Harmonised unemployment rate (%)	OECD (2020a)
Real GDP per capita (in 2011 US dollars)	Penn World Table (Feebstra et al., 2015)
Capital account openness (Chinn–Ito Index)	Chinn & Ito (2006)
Trade openness (imports and exports as % of GDP)	IMF (2020)
Government expenditure (as % of GDP)	OECD (2019)
Left vote share in last election	Brady et al. (2020)

Figure A1. Latent variable for taxes on the rich, 18 OECD countries, 1965-2015

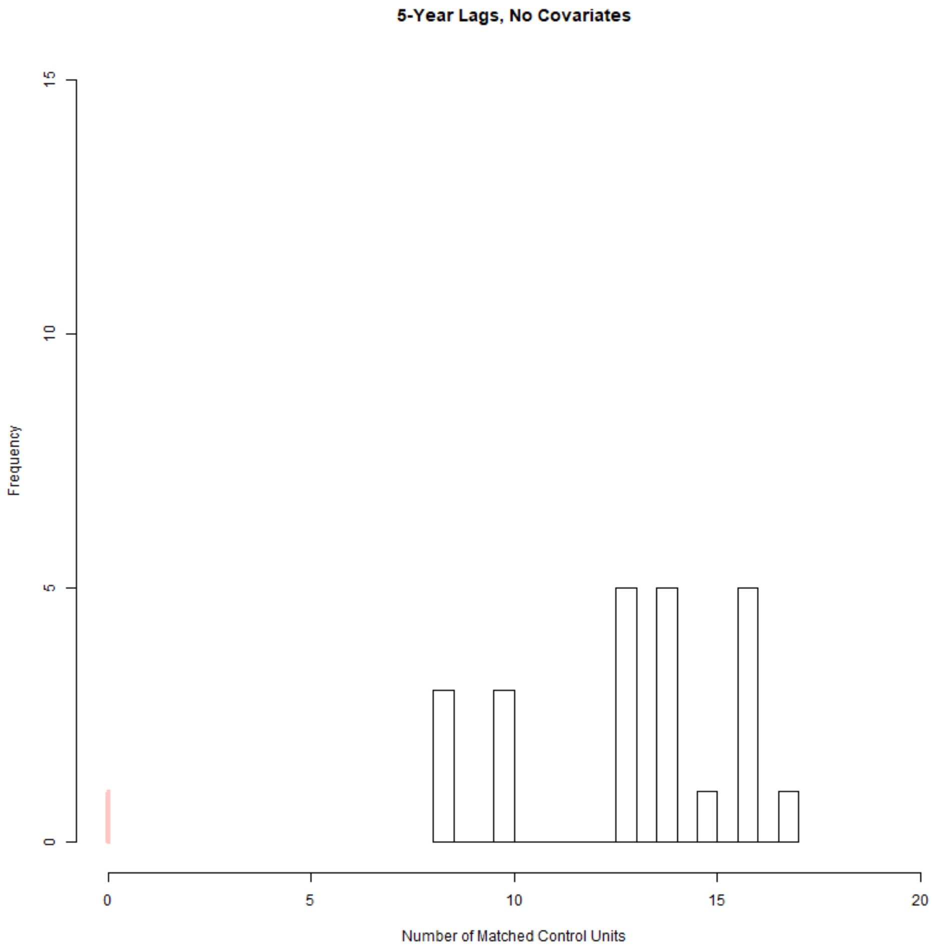


Source: Authors' calculations; Hope and Limberg (2020).

Figure A2. Changes in the latent variable for taxes on the rich and major tax cuts

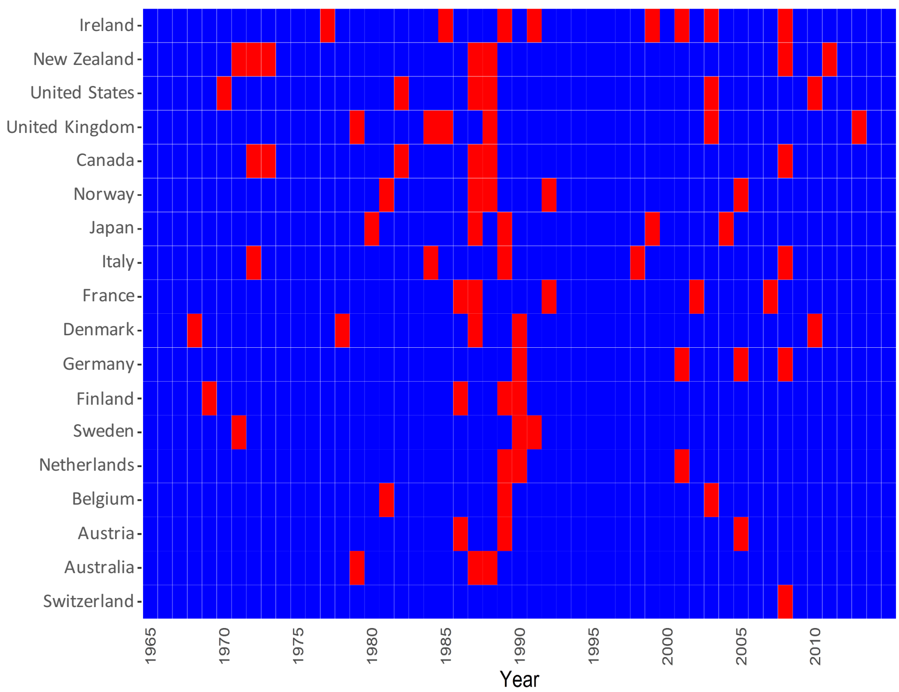
Note: Country-year observations in red show tax cuts for the rich that exceed the 2 standard deviation threshold.
Source: Authors' calculations.

Figure A3. Distribution of number of matched control units



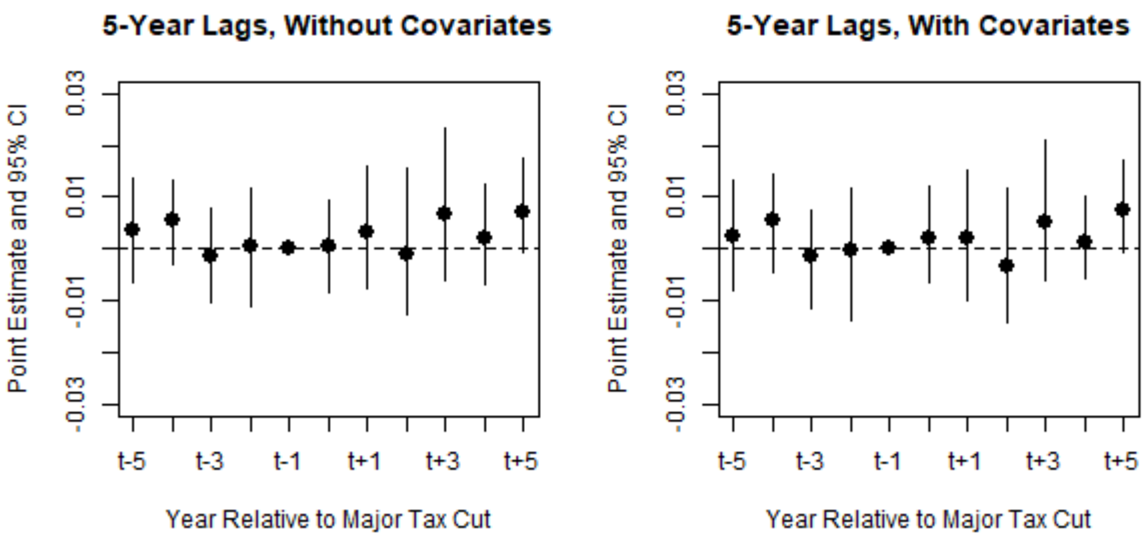
Source: Authors' calculations.

Figure A4. Distribution of major tax cuts for the rich, 1 standard deviation threshold, 1965-2015



Source: Authors' calculations.

Figure A5. Effect of major tax cuts for the rich on real GDP per capita growth rates after matching on treatment trajectory (left panel) and treatment trajectory and covariates (right panel)



Source: Authors' calculations.