Tax progressivity and top incomes: Evidence from tax reforms*

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Abstract

We study the link between tax progressivity and top income shares. Using variation from large-scale Western tax reforms in the 1980s and 1990s and the novel synthetic control method, we find large and lasting boosting impacts on top income shares from the progressivity reductions. Effects are largest in the very top groups while earners in the bottom half of the top decile were almost unaffected by the reforms. Cuts in top marginal tax rates account for most of this outcome whereas reduced overall progressivity contributed less. Searching for mechanisms, real income responses as measured by growth in aggregate GDP per capita, registered patents and tax revenues were unaffected by the reforms. By contrast, tax avoidance behavior related to the management of capital incomes in the very income top appears to lie behind the observed effects.

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1 Introduction

How does changing tax progressivity affect the distribution of pre-tax income? This question has interested researchers and policymakers alike in the wake of a decline over several decades in income tax progressivity around the Western world. Several studies have examined the issue, looking at either cross-country evidence or within-country variation over the income distribution (see, e.g., Feenberg and Poterba, 1993; Feldstein, 1995, 1999; Slemrod, 1996; Slemrod and Bakija, 2000; Brewer, Saez and Shephard, 2010; Bach, Corneo and Steiner, 2013; Förster, Llena-Nozal and Nafilyan, 2014; Piketty, Saez and Stantcheva, 2014; Duncan and Sabirianova Peter, 2016; Frey and Schaltegger, 2016 and Saez, 2017), but the complex interdependence between income taxation and income inequality poses powerful hurdles to identify this relationship and it is fair to say that consensus has not been reach over how it looks.

In this paper, we approach the question of how tax progressivity affects income inequality from a new angle: studying the effect of tax reforms on the income shares earned by the top of the income distribution. Tax reforms are particularly tractable study objects for our purposes. First, they offer a distinct and usually large-scale source of variation. Second, they account for much of the recorded decline in Western tax progressivity over recent decades. Third, tax reforms have not been studied extensively before in the context of explaining income inequality change.

Our analysis covers all personal income tax reforms carried out in Western countries since the 1970s, but for identification purposes we focus on three cases where progressivity decreased extraordinarily much: Australia in 1987, New Zealand in 1989 and Norway in 1992. Analyzing single events puts specific requirements on the statistical methods used, and in our baseline analysis the identification strategy relies on the newly created synthetic control method (SCM) of Abadie, Diamond and Heinmueller (2010). The idea behind the SCM is to construct a control group that captures what would have happened in the absence of treatment. Rather than choosing one or more countries to use as a comparison group (as in Difference-in-Differences estimation), we create a synthetic control country in the form of a weighted average of non-treated countries selected based on how similar they are to the treated country in terms of levels and trends in top income shares, structure of the tax system and other relevant background characteristics. We perform several robustness tests to examine the validity and sensitivity of the assumptions underlying the SCM.

In addition to the SCM-analysis, we run standard panel regressions. While these partly serve to complement the main SCM analysis, they also allow us to examine mechanisms in the progressivity effects such as the relative role of reduced tax rate progression over the distribution versus cuts in top marginal tax rates. In addition, they allow us to investigate more subtle dimensions of tax reforms, such as how changes in the number of tax brackets, broadening of the tax base or variations in the amount of tax deductions (Kopczuk, 2005; Doerrenberg, Peichl and Siegloch, 2017) affect the income distribution. Moreover, our main results are still robust when we account for other political and economic reforms carried out over the period of interest.

In a final analysis, we study the impact of tax reforms on economic efficiency. As Saez (2004) noted, the intellectual weight behind many of the dramatic cuts in top tax rates during the 1980s adhered to supply-side economics and a broad notion that lower tax rates fuel economic activity. We evaluate this hypothesis by running SCM estimations replacing top income shares with three indicators of real activity: GDP per capita, number of registered patents per capita, and total tax revenues over GDP. While these are admittedly coarse indicators of efficiency, they capture policy-relevant dimensions of real economic activity and are therefore interesting to study in contrast to inequality outcomes.

Our findings show that the reductions in tax progressivity coming with the studied tax reforms had a strong boosting impact on top income shares. The income share of the top percentile increased by between 20 and 50 percent in the reformed countries relative to their synthetic controls. The size of this impact was highest in the very top: income shares of the top 0.1 percentile rose by between 50 and 100 percent, whereas they hardly changed at all for the lower half of the top decile. We cannot find any significant impact by tax reforms on economic output or other efficiency indicators, suggesting that the effect on top income shares was rather due to a redistribution of exisiting resources than to new resources being generated by the income elite. Instead, the patterns are in line with tax planning and increased capital incomes among top income earners.

The paper contributes to two literatures. First, most relevantly to the above-cited tax policy studies, particularly those dealing with tax progressivity effects on income distribution. Furthermore, we add to the broader top incomes literature where there are still few studies that have established the determinants of the trends in top income shares. While some attempts are made to study the association between top income taxation and top income shares (e.g., Roine, Vlachos and Waldenström, 2009; Atkinson and Leigh, 2013; Piketty et al., 2014), they primarily offer correlational evidence and do not make attempts to clarify causal mechanisms.

2 Tax reforms of the 1980s

The evolution of Western personal income taxation since the 1970s exemplifies how tax reforms are important drivers of tax progressivity change. In particular, the 1980s was an era when several countries restructured their income tax systems. According to a survey by Brys et al. (2011), these reforms may have differed in scale and scope but they shared an overall ambition to reduce top marginal tax rates, broaden tax bases and decrease the number of income tax brackets.

We wish to study tax reforms that *significantly* diminished the progressivity of personal income taxation since this facilitates identifying any effect on the income distribution. As a consequence, we will disregard all the minor and more gradual reforms that took place during our studied period.

2.1 Identifying significant tax reforms

There are several ways to define and measure tax progressivity; we follow Musgrave and Thin (1948) and choose the *average rate progression* (ARP) as a measure of structural progressivity. Denote Y_0 and Y_1 two income levels in the income distribution, with $Y_0 < Y_1$, and T_0 and T_1 their respective tax liabilities. The average tax rate, ATR_i, is thus $ATR_i = T_i/Y_i$ for $i = 0, 1$ and this allows us to write the ARP as follows:

$$ARP = \frac{ATR_1 - ATR_0}{(Y_1/Y_0) - 1}. \tag{1}$$

Empirically, we compute the ARP using data on reported incomes and average tax rates in different countries over time (see further below in the data section). For country i at time t, we assign to Y_1 and ATR_1 the reported income and the average tax rate, respectively, of a taxpayer in the top percentile of the income distribution, while for Y_0 and ATR_0 we use income and tax rate of an average-income taxpayer. For country *i* at time *t*, our measure of tax progressivity then becomes:

$$ARP_{i,t} = \frac{ATR_{i,t}^{top} - ATR_{i,t}^{avg}}{(Y_{i,t}^{top}/Y_{i,t}^{avg}) - 1}. \tag{2}$$

Finally, our key statistic for evaluating and classifying changes in tax progressivity in countries at different points in time is defined as the change in this empirical ARP:

$$\Omega_{i,t} = ARP_{i,t} - ARP_{i,t-1}. \tag{3}$$

Negative values of $\Omega_{i,t}$ reflect progressivity-reducing reforms and the larger the $\Omega_{i,t}$ the more intense the progressivity change. We compute $\Omega_{i,t}$ since 1981 (the earliest year for which we have detailed fractile-specific tax data, see data section below) for 18 OECD countries and can thereby observe which are the tax reforms that led to the largest negative change in structural progressivity.[1]

Figure 1 plots tax reforms according to their impact on progressivity. Most of them lowered progressivity, but three of the reforms stand out: New Zealand in 1989 ($\Omega = -0.047$), Norway in 1992 ($\Omega = -0.030$) and Australia in 1987 ($\Omega = -0.026$). The reforms in the US and the UK during the 1980s also lowered progressivity, but not as significantly and not as distinctly as in the other three countries.[2] Furthermore, the UK and US reforms occurred in several steps over

[1] In Appendix A4, we provide a brief description of the tax reforms for which $\Omega_{i,t}$ has been computed and the corresponding numerical value. Note that $\Omega_{i,t}$ relies on the computation of the ATRs. Data limitations (see further in the data section below) imply that our empirical ATR could differ from the effective tax rate since some tax components are not available. It follows that if additional components of overall taxation are considered, it may be the case that other reforms have produced a larger erosion in structural progressivity.

[2] Our selection of the reforms of interest is basically unchanged even using the marginal rate progression (MRP) instead of the ARP (see Figure A1 in Appendix A4). The nature of our data, however, does not allow to derive

a sequence of years (see Adam et al., 2010, and Brewer et al., 2010, for the UK and Piketty and Saez, 2007, for the US). For this reason, in the subsequent analysis we will consider only the tax reforms carried out in Australia, New Zealand, and Norway.

[Figure 1 about here]

2.2 The Australian Tax Reform

The Australian tax reform was announced on September 19, 1985, but it received the royal assent only in June 1987.[3] The highest marginal income tax rate was reduced from 60 to 49 % and the tax base was broadened. Taxation of realized capital gains and fringe benefits was also changed, and full dividend imputation was introduced. As discussed in Burkhauser et al. (2015), under the new dividend tax system, these taxes effectively became withholding taxes since their payment could be used to offset personal income tax on dividends or any other taxes. Hence, this was a major reduction in the effective tax rate on dividends, with the greatest reduction in tax rates going to those with the lowest marginal income tax rate.[4] The company tax rate was aligned to the top personal tax rate (at 49 %). However, this alignment lasted only two years. Such a high company tax rate proved unsustainable, and the rate was reduced to 39 % in the 1988 business tax reform, without a same change in the income tax rate. All these changes radically transformed the Australian tax system, thus providing a well-defined switch in the fiscal system, where the starting point can be identified by the change in the treatment of capital gains implemented in June 1986, i.e., the 1985-1986 fiscal year.[5] Therefore, effects on top incomes are likely to begin before 1987, the year we use to refer for the reform.

Figure 2 displays the historical evolution of the income share held by the top percentile, the top marginal tax rate on personal income, and the average rate progression. Atkinson and Leigh (2007) and Burkhauser et al. (2015) discuss the long-term trends and the potential driving forces of income inequality, emphasizing the role of both income and dividends top tax cut among other things.[6]

other commonly used indicator of tax progressivity such as the Kakwani or Suits index.

[3] In his opening statement, Labor M.P. and Government Treasurer Paul Keating introduced a program for fundamental tax reform announcing: "Today we are addressing a crisis in our national taxation system that has been left by a succession of Governments to compound year upon year. There was a time when Australia had a reasonably sane and credible taxation system. But that time is long gone. The system has been broken and beaten by an avalanche of avoidance, evasion, and minimization. It is the deterioration and decay that occurred in the late 1970s and early 1980s that has now made substantial reform so essential."(Keating, 1985, p.2).

[4] One reason for the passage of this legislation was to treat company profits in the same way of how profits by trusts were treated (Burkhauser et al., 2015). The Australian government (Australian Government, 1985) identified the shifting from companies and partnership to public and private trusts as one of the major threat to the tax system since it was steadily becoming a relatively low cost legal vehicle for doing business in the 1970s and 1980s that successfully circumvent the classical taxation system. According to tax record data, trusts grew from 117,616 (aggregate net income of 0.3 billion dollars) in 1972 to 258,846 (aggregate net income of 2.7 billion dollars) in 1982. The government declared that "the phenomenal growth in recent years in private business and trading trusts has reflected a desire to avoid the two-tier taxation of company income which can feature under the private company structure." (p.55).

[5] Note that Australia and New Zealand have fiscal year starting on 1 July and ending on 30 June.

[6] Burkhauser et al. (2015) show how top income shares substantially decrease as dividend imputation credits are

2.3 The New Zealand Tax Reform

The tax reform in New Zealand in 1989 gave rise to the largest measured progressivity reduction of all the reforms we study. The reform took place during an era of profound economic change, when many economic institutions were liberalized and deregulated (see Evans et al., 1996, and Atkinson and Leigh, 2008, for a survey of those reforms). The tax reform broadened the personal income tax base, halved from 66 to 33 % the top marginal tax rate and reduced the number of tax brackets from five to two. As in the Australian reform, the legislator introduced an imputation system for the company tax, which made dividend payments more attractive.[7] Other important changes were the introduction of a fringe benefit tax at an initial rate of 45 % and a substantial simplification in the tax system of sale and other indirect taxes.

The effect of the tax reform on top income shares is clearly shown in Figure 2.[8] Atkinson and Leigh (2008) identify some potential factors driving the surge in top incomes during this period. They state that "progressive taxation may have contributed to the fall in top income share over the 1930s and 1940s, with the top marginal tax rate rising from 25 percent in 1930 to 65 percent in 1940, peaking at 77 percent from 1942 to 1945. Likewise, top tax rates may have been a factor in the growth in top income shares during the late 1980s." (p.162). In addition, they provide evidence of a negative relationship between the top tax rate and the income share held by the top percentile (Table 2, panel b, column 5).

2.4 The Norwegian Tax Reform

The Norwegian tax reform in 1992 was mainly inspired by the US tax reform of 1986 and its ambitions to reduce tax-induced distortions by lowering statutory tax rates and broadening tax bases (Aarbu and Thoresen, 1997). Similar to the Australian and New Zealand reforms, the reform in Norway substantially lowered the top marginal tax rate, but only made small reductions of taxes on lower incomes. Before 1992, the tax system has been more or less unchanged. Both the pre- and the post-reform income tax system consist of two tax bases: net and gross income. The reform affected taxation of wage earners, self-employed and corporations. Aarbu and Thoresen (1997; 2001) describe the main changes in the personal tax structure. The most important change was that the progressive national tax was removed. The tax rate on net income was reduced from a maximum of 40.5 % to a flat rate at 28 % and the marginal tax rate

excluded from the series. In a separate analysis, we will replicate our findings using their data.

[7]Note that the introduction of an imputation system, under which part of any company tax paid is treated as a pre-payment of PIT, may weaken the comparability of series around the tax reform. Atkinson and Leigh (2008) also discussed this issue, while Easton (2013) attempts to account for it. However, Easton's series are not highly comparable with other international data available from WID (i.e., the dataset we use). Indeed, he notes that in his series "calibration difficulties make international comparisons difficult, so we must be cautious about ranking New Zealand's top income inequality with economies elsewhere" (p. 144). For this main reason, we prefer to carry on with the WID series.

[8]Note that the increase in top income shares decrease somewhat in the 2000s, after a substantial spike in 1999. One tax-related explanation is linked to the announcement in 1998 made by the opposition Labour Party that, if elected, would have raised top tax rate from 33 to 39 % (Atkinson and Leigh, 2008). After this announcement, many taxpayers decided to realize business earnings, boosting up the top income shares in 1999.

on capital income was reduced likewise.

Figure 2 shows the evolution of the top percentile's income share.[9] The most striking feature is clearly the turning point in 1992, the year of the tax reform. While the early part of the post-war period was characterized by a highly progressive income tax schedule, the 1992 tax reform clearly represents a structural break. As a consequence of this tax rate reduction, dividends became increasingly more important as a source of income (Aarbu and Thoresen, 1997).

[Figure 2 about here]

3 Analytical framework

Our purpose is to estimate the effect of tax progressivity on top incomes, and to do this we need to account for all the effects that a tax reform may induce to reported income. We build upon the large existing literature on estimating behavioral responses to taxation (see Saez et al., 2012, for a survey of this literature). This uses an extension of the static labor supply model where individuals maximize their utility that depends positively on disposable income, which they can consume, and negatively on reported income, on which they have to pay taxes. A crucial empirical parameter estimated by the literature is the elasticity of taxable income, which captures all the taxpayers' behavioral responses to tax variations. It provides the percentage variation in taxable income as the net-of-tax rate (i.e., 1 - MTR) changes by one percent. Focusing on top income shares and letting t_0 and t_1 the pre- and post-reform period respectively, the elasticity ϵ^s can be estimated as follows:

$$\epsilon^s = \frac{log(y^s_{t_1}) - log(y^s_{t_0}) - \Delta log(y^s)}{log(1 - MTR^s_{t_1}) - log(1 - MTR^s_{t_0})}, \tag{4}$$

where y denotes the share of income held by the top income share s and $1-MTR^s$ the net-of-marginal tax rate. $\Delta log(y^s)$ aims to capture how top income shares would have changed absent the tax reform. In other words, this term accounts for how non-tax related factors influence the income distribution. Properly account for this term represents one of the main empirical challenge to provide an unbiased estimate of ϵ^s.

Note, also, that in a cross-country study as this, it is crucial to allow ϵ_s to vary with the specific characteristic of the tax system, such as the availability of deductions or tax avoidance opportunities, and on other things such as how broadly defined the tax base is (Kopczuk, 2005; Doerrenberg et al., 2017).

It is well-known that top incomes may respond differently to income taxation than the rest of the taxpaying population. In a recent paper, Piketty et al. (2014) propose an extended variant of the optimal taxation model of top incomes in which top incomes respond to marginal tax rates

[9] We use the series from the WID, showing reported taxable incomes. As Alstadsaeter, Jacob, Kopczuk and Telle (2016) recently showed, these shares are much below the actual top income shares after a dividend-tax reform in 2005 when the reporting of retained earnings was changed. Fortunately, because this reform comes after our estimation window its impact on top income data does not influence our results.

through three main channels: standard labor supply, tax avoidance, and compensation bargaining, all summing to ϵ in equation (1). The first top income elasticity is the standard labor supply elasticity, reflecting real economic responses to the net-of-tax rate (i.e., more hours of work, more intense effect per hour worked, occupational choice etc.). The second elasticity reflects tax planning and tax avoidance behavior of top-income minimizing their taxes paid. Several studies provide compelling evidence that top incomes may respond to tax changes through tax avoidance and income sheltering (e.g., Auerbach, 1988; Slemrod, 1996; Saez, 2017). The third top income elasticity captures the incentive to bargain more aggressively for pay increases in response to lower marginal tax rates since that gives the top earners a larger fraction of the remuneration.

We use these models of top income taxation as basis for identifying the effect of tax progressivity on top income shares. The baseline estimation will be close in spirit to the empirical panel regression model specified in Piketty et al. (2014): $y_{it} = \epsilon \times log(1 - MTR_{it}) + \beta X_{it} + u_{it}$, where y_{it} denotes a top income share in country i and time t, MTR is the marginal tax rate and X are control variables such as time trend or country fixed effects. This regression gives the average expected link between the net-of-tax rate and the top income share. A related estimation approach is that of Saez (2017) where medium-term tax-reform responses of top incomes are computed by subtracting a counterfactual top-share change from the observed top-share change. While rudimentary, this medium-term response is actually close in spirit to the causal estimation of progressivity-reducing tax reforms that we attempt to do using a different, and more comprehensive, identification strategy.

4 Empirical strategy and data

Estimating the causal impact of tax progressivity change on top income shares is associated with several difficulties. The key empirical challenge is to properly control for non-tax related factors. Indeed, progressivity-reducing tax reforms mostly happened during the 1980s, a period when income inequality was increasing for several non-tax related factors. Hence, using standard OLS regressions may confuse tax responses with any underlying factor affecting income inequality. Achieving identification with a Difference-in-Differences (DiD) approach is hampered by country-level variations in top incomes, tax progressivity and institutional factors, which make a-priori difficult to find a proper control group. Likewise, instrumental variable methods can certainly mitigate problems of endogeneity, but there remains doubt if they are sufficient (Doerrenberg and Peichl, 2014).

We attempt to solve these challenges by using the novel synthetic control method (SCM), which was developed in the seminal paper of Abadie et al. (2010). This approach, described in detail below, estimates counterfactual outcomes by combining information from many control variables and potential control units, and it is particularly useful when studying rare outcomes in single countries, such as tax reforms.

As complement to the SCM, we also present cross-country panel regressions where we estimate the relationship using DiD methods with country and time fixed effects, time-varying controls and country-specific time trends. Although this approach is more problematic, it gives a broad robustness check and also allows us to examine the role of certain mechanisms. Reassuringly, the SCM and the DiD generate similar results in both sign and magnitude.

4.1 Synthetic Control Method

The SCM compares a certain outcome in a single treated country with the same outcome in a constructed counterfactual country – a synthetic control – consisting of a weighted combination of those non-treated countries that are the most similar to the treated country in terms of levels and trends of both the outcome variable in question and various relevant background characteristics.

Consider a total of J countries, indexed by j, where the first country ($j = 1$) is the treated country and countries 2 to J ($j = 2, \ldots, J$) compose the donor pool from which the synthetic control group will be generated. The pre-treatment period t_0 and post-treatment period t_1 sum up to the full sample length. The aim of the analysis is to measure the effect of the tax reforms on top income shares throughout t_1, selecting a synthetic control group that best resembles the pre-intervention characteristics of the treated country during t_0. The synthetic control is defined as a weighted average of the countries in the donor pool that can be represented as a $(J-1) \times 1$ vector of weights $W = (w_2, \ldots, w_J)$', with $0 \leq w_j \leq 1$ for $j = 2, \ldots, J$ and $w_2 + \ldots + w_J = 1$. Let $Y_{j,t}$ be the top income shares of unit j at time t. Moreover, let Y_1 and Y_0 be a $t_1 \times 1$ vector and a $t_1 \times (J-1)$ matrix collecting the post-treatment values of top income shares in the treated country and in the synthetic control respectively. For a post-treatment period t_1, the difference in top income shares between the treated country and the synthetic control can be defined as:

$$\alpha_{1,t_1} = Y_{1,t_1} - \sum_{j=2}^{J} w^* Y_{j,t_1}, \tag{5}$$

where the counterfactual outcome can be modeled as:

$$Y_{j,t_1} = \gamma_{t_1} + \theta_{t_1} Z_{j,t_1} + \lambda_{t_1} \mu_j + u_{j,t_1}, \tag{6}$$

where γ_{t_1} is a time fixed effect, Z_{j,t_1} is a vector of time-varying control variables, μ_j is a vector of unobserved factors affecting top income shares (but that does not cause the tax reform analyzed), and $u_{j,t}$ is an idiosyncratic shock. Selecting a weighted average of the untreated countries, the model in (6) becomes:

$$\sum_{j=2}^{J} w_j Y_{j,t_1} = \gamma_{t_1} + \theta_{t_1} \sum_{j=2}^{J} w_j Z_{j,t_1} + \lambda_{t_1} \sum_{j=2}^{J} w_j \mu_j + \sum_{j=2}^{J} w_j u_{j,t_1}. \tag{7}$$

It means that if the model in (7) holds, top income shares are affected by some variables, both observed and unobserved. By constructing (7), it is artificially created a counterfactual which is affected by the same variables (both known and unknown) that have effect on the treated country. Abadie et al. (2010) prove that choosing the vector W^* so that for each year *t* over the pre-reform period t_0, we have matching in both pre-treatment values of the controls, i.e.:

$$\sum_{j=2}^{J} w_j^* Z_{j,t_0} = Z_{1,t_0} \tag{8}$$

and top income shares:

$$\sum_{j=2}^{J} w_j^* Y_{j,1} = Y_{1,1}; \sum_{j=2}^{J} w_j^* Y_{j,2} = Y_{1,2}; \quad ... \quad ; \sum_{j=2}^{J} w_j^* Y_{j,N} = Y_{1,N} \quad \forall t = 1, 2, ..., N \in t_0. \tag{9}$$

Then, even when μ_j is unobserved, the weighted average of the donor pool is an unbiased estimator of the potential outcome, and $\hat{\alpha_{1,t_1}} = Y_{1,t_1} - \sum_{j=2}^{J} w^* Y_{j,t_1}$ represents an estimator of the true treatment effect.

Compared to regression-based methodologies, the SCM offers significant advantages over the standard DiD method when it comes to estimate effects of treatments in single countries. However, the SCM relies on some assumptions that need to be scrutinized critically and we do this in a series of sensitivity checks reported below. Specifically, we run "in-time" and "in-space" placebo tests, vary the control variables used to construct synthetic control groups and examine post-treatment trends of control variables in treated and synthetic controls.

One tractable aspect of the SCM estimation is that its effect evolves over time, offering a dynamic interpretation. However, as already mentioned, this feature also means that potential influences from tax reforms occuring later in time in the control group, potentially in response to the studied treatment (e.g., in a tax competition context), could compromise the identification if there are dynamic feed-back effects. Identifying an "average treatment effect on the treated" would not be trivial (see the dynamic regression discontinuity setting of Cellini, Ferreira and Rothstein (2010) for an example). We therefore focus on the intent to treat long-term effect, but also examine the occurrence of tax reforms in the synthetic control as presented in the robustness analysis.

4.2 Data

Our dataset contains annual observations of income, tax and control variables collected for 18 countries over the period 1960-2010. Details about these series are provided in Appendix A.

Income data: Top income shares and average incomes for different top income fractiles come from the World Wealth and Income Database (WID). As extensively discussed by Leigh (2009), Atkinson, Piketty and Saez (2011), and Roine and Waldenström (2015), these series stem from administrative tax sources and have been compiled using a common methodology for all countries, offering a high degree of comparability over time and space. Among the problems with these tax data is that they do not allow for a complete separation of income sources (earnings, capital income) to scrutinize income-shifting and other avoidance behaviors among top earners.[10]

Tax data: We have data on marginal tax rates and average tax rates. Two different kinds of marginal tax rates are used. MTR^{top} denotes the statutory top marginal income tax rate in the tax schedule, retrieved from Piketty et al. (2014) for 18 countries over the period 1960-2010. While this has been one of the most commonly used measures in the previous literature, it is quite problematic since its coverage of how many that actually pays it greatly varies over time and space (between virtually nobody and over one third of the population pays it in our sample). Therefore, we also use a more accurate measure, MTR^{s}, which shows the marginal tax rate calculated to match the income level of each top-income fractile s for each country and year (see Appendix B in Rubolino and Waldenström, 2017, for details about their construction). We are able to calculate this tax rate annually for 15 countries since 1981 based on tax schedules in the OECD Tax Database (Tables I.1 to I.3) and average top fractile incomes in the WID. An average tax rate, ATR^{s}, also specific for each fractile is calculated in a similar manner since 1981.

We use tax information from the OECD dataset, which provides both central and sub-central government personal income tax information.[11] To derive our tax measures, we account for standard deductions, tax credits, basic personal allowances, major national surtaxes, and other provisions in addition to statutory rates and thresholds at both central and sub-central government levels. However, even though we deem our calculations to be improvements with respect to the previous literature, they are not perfect. We are unable to use income source-specific effective tax rates, which could differ for some countries and some periods, because that information is not available for most countries in the WID. Furthermore, there are smaller taxes and contributions for specific countries and time periods that could not be included in the general formula for reasons of comparability, but we deem that they are so few and small enough not to have any bearing on the main analysis.[12]

[10]Moreover, we have gaps in income and tax series for some countries which prevent us from covering some potentially important tax reforms. A leading example is the Netherlands 2000 tax reform, which lowered the top marginal tax rate from 60 to 52 % and may potentially be important as well. However, since income data for this country are available until 1999, we prefer to do not refer to other external sources and we exclude this reform. Anyway, our main results are robust even if we drop out the Netherlands from our dataset.

[11]Information on this tax data may be found in the OECD Explanatory Annex: http://www.oecd.org/ctp/tax-policy/Personal-Income-Tax-rates-Explanatory-Annex-May-2016.pdf.

[12]First, data limitation on both the tax and income sides does not allow to include social security contributions in the ATRs computations. Second, any income tax that might be due on non-wage income is not taken into account. Despite income from interests, dividends and rents represents a minority of total personal income, capital income is, however, significant, particularly in the top of the income distribution. Third, deductions, allowances, and credits that vary by individuals characteristics are not included in the calculations. Fourth, the focus is on annual incomes, which are not a perfect measure of income over the course of a lifetime. Recent studies (for example, Bengtsson, Holmlund and Waldenström, 2016) show that individual income taxes seem to be less progressive from a lifetime perspective than from an annual perspective, because of year-to-year transitory fluctuations in income.

Control variables: Some control variables come from the literature on the determinants of inequality (Atkinson and Piketty, 2007; Roine, Vlachos and Waldenström, 2009; Doerrenberg and Peichl, 2014): GDP per capita, financial development (sum of bank deposits and stock market capitalization as share of GDP), trade openness (trade share in GDP), trade union density as a percent of employees, the share of working age population and technological progress (growth rate of the number of patents).

Another particularly important group of control variables are those that address the endogeneity of tax reforms. The eventuality that a tax reform is caused by variables driving inequality change (or even by the inequality change itself) is to some extent handled by the synthetic control methodology as the counterfactual should possess the same probability of an unexpected increase in the top income shares as well as of causing a tax reform. However, we also include additional variables that could be drivers of a tax reform (see Brys et al., 2011 for a detailed discussion). In particular, fiscal imbalances could be key and we therefore include three related variables: gross central government debt as a share of GDP (from Reinhart and Rogoff, 2011), total tax revenues as a share of GDP (OECD, 2016b) and and central government spending as a share of GDP (Roine et al., 2009). In Appendix Table A4, we provide detailed information on all these variables.

5 Main results

We now present the main results of the SCM analysis. The effect of progressivity-reducing tax reforms on inequality is measured as the difference in top income shares between the treated country and its synthetic control during the post-reform period. We estimate a synthetic control group for each top income share by including the following variables in the vector of controls: GDP per-capita, globalization, annual hours worked, human capital, financial development, trade union density, top marginal tax rate, average rate progression, and debt growth rate, in addiction to some past values of the outcome variable. For the sake of space, we report in Appendix Tables B1-B3 information regarding the synthetic control composition (i.e., the vector of weights and goodness of fit).

5.1 Australia

Figure 3 reports income shares in Australia (solid line) and its synthetic control groups (dashed line) before and after the tax reform was announced.[13] The test results suggest that top income shares increased substantially relative to the synthetic control group as a consequence of the tax reform. Immediately after the reform, income shares increased drastically: a spike in the top percentile group's income share of 1987 implies an increase by 60 % relative to the synthetic control. Over the first five years, however, the estimated effect of the reform on the top per-

[13]We allocate the treatment (represented by the solid vertical line) to the 31 December of the year before the comprehensive reform legislation began. That is 1985 for Australia, 1988 for New Zealand and 1991 for Norway.

centile group was a 20-25 % increase in its income share relative to the level of the synthetic control.

Looking at different groups within the top income decile (bottom graphs), there are stark differences in responses. The absolute top responded strongest: the 1987 spike in the top 0.1 percentile share represents a 140 % increase, and in the first five post-reform years the effect was about 60 %. However, lower down the top decile the treatment effects are much smaller. The income share of the top 5-1 percent group is only marginally (in the 0-10 % range). To put these figures in perspective, these effects translate into a short-term (3 years) elasticity of top percentile income share with respect to the net-of-top marginal tax rate around 0.89, which reduces to 0.17 in the medium-long term (between 5 and 10 years later).[14]

How should we think about the longevity of the treatment effect in the very top? As the figure shows, the increase in income shares of the very top groups did not die out; in 2010 - more than twenty years after the reform - top income shares were still around one quarter higher than their synthetic counterfactual. However, as the years go by, there are many other things that may influence the income distribution, potentially threatening the crucial SCM assumptions. We investigate this issue in one of our robustness checks (reported in Appendix Figure C4), where we compare post-treatment trends in the controls between treated and synthetic control countries. This test does not provide strong evidence of a different trend for almost all the variables of interest, thus suggesting that the lasting differentials do in fact reflect long-lasting reform-related effects.

[Figure 3 about here]

Accounting for the larger reform effects in the highest income groups, a possible explanation is the relative importance of capital income in total incomes since it offers more leeway to manage income streams over time in order to minimize taxes. Realized capital gains is perhaps the most obvious example of such an income, but dividend incomes may also be smoothed or held back for tax purposes. The previous top income literature offers an ample evidence that capital income is indeed much more important for the highest top incomes than for lower top incomes in most Western economies. We use the WID data to calculate how the capital income share varies across the income distribution and find a consistent increase in the very top. The average capital income share was around 25 % during the 1980s and 1990s in the bottom half of the top decile, but around 70 % in the top 0.1 percentile. To test whether tax avoidance behaviors related to the management of capital incomes in the very top is a good candidate to explain the main findings, we regress the (log of) share of wage income held by the top 0.1 percentile on the (log of) net-of-top marginal tax rate on wage and on capital, separately. With no avoidance behavior, we would expect that wage income share responds solely to its own tax rate, while a cross-rate negative elasticity would suggest avoidance behavior in the form of

[14]This elasticity is computed using the top income shares held by the synthetic control as the $\Delta log(y^s)$ term in the numerator of equation (1). Namely, for the short-term elasticity (i.e., $t = t+3$), the equation becomes: $\epsilon^{top1} = [log(y^{top1}_{AUS,t+3}) - log(y^{top1}_{synt,t+3}]/[log(1 - MTR^{top}_{AUS,t+3}) - log(1 - MTR^{top}_{AUS,t-1})]$.

tax base shifting. Our test result is in in-line with the latter hypothesis: we estimate a positive own-rate elasticity and a statistically significant negative cross-rate elasticity.[15]

5.2 New Zealand

Figure 4 displays the effects of the 1989 tax reform on top income shares in New Zealand. The results indicate fairly large short-term increases in top income shares, varying from 17 % (top 10 percent) to 50 % (top 0.5 percent), though not as large in Australia. The intermediate shares (bottom graphs) show that practically all of this increase happened in the upper half of the top decile, where the income share increased approximately 15 % in the top 1-0.5 percentile group and between 5 and 10 % in the top 5-1 percentile group. By contrast, there was absolute no effect on the income share held by the bottom half of the top decile. This results translate into a short-term top 1 elasticity of 0.32, lower than in the case of the Australian reform.

Differently from the Australian case, the effect do not seem to be long-lasting. Even in the very top groups, there is no difference between New Zealand and its synthetic controls ten years after the reform.

[Figure 4 about here]

When looking at potential drivers for the results, we must again consider the role of the differential impacts within the top decile. Unfortunately, we cannot study the role of income composition in detail because of a lack of such compositional data across top income groups in New Zealand. We know, however, that the tax changes created strong incentives to boost capital income after the reform of the 1980s and before the reform of 2000. For example, the introduction of an imputation system made more attractive to pay dividends; Atkinson and Leigh (2008) argue that this is clearly reflected in the higher 1989 income shares. The increase in the top marginal tax rate from 33 to 39 %, announced in late 1998 and implemented in 2000, encouraged top earners to realize capital gains. In other words, similar to the Australian case there seems as if capital incomes played a role also in the tax-driven rise of top income shares in New Zealand.

5.3 Norway

The SCM-estimated impact of progressivity reductions on Norwegian top income shares during the tax reform in 1992 is presented in Figure 5. The results display similar patterns as in Australia and New Zealand, with top income shares increasing as a consequence of the reform, but from a lower initial level. The top percentile share increased from 6 to 7-8 %, which is by roughly one fifth, whereas the top 0.1 percentile increased from around 2 to around 3 %, i.e., by roughly half. The persistence of this effect was around 15 years, similar to what was found for

[15]For clarity, we run a regression of the following form: $log(wageshare)_t = e_1 log(1 - MTR_t^{wage}) + e_2 log(1 - MTR_t^{capital}) + t + u_t$. We estimate e_1 = 0.550 (0.375) and e_2 = -1.058 (0.544).

New Zealand, but shorter than in Australia. We estimate a short-term top 1 elasticity of around 0.9, similar to the Australian short-term estimate.

Lower down in the Norwegian top income decile, we are not able to find any significant reform effect at all. In fact, post-treatment trends in income shares are higher in the synthetic control than in the lower half of the top decile. However, even pre-treatment trends differ markedly and suggest a relatively bad goodness-of-fit for estimated synthetic control group.

[Figure 5 about here]

When accounting for the differential results within the Norwegian top income decile, we focus again on the importance of income composition. Capital incomes seem to have been important also in this case. Aaberge and Atkinson (2010) examine several potential drivers of the top income share change in the 1990s and show a sharp increase in dividend income and in realized capital gains among high-earning households. Fjærli and Aaberge (2000) point out that the surge is partly explained by income shifting, evidencing as dividend receipts and capital gains received by top decile increased just after the reform. The panel data analysis made by Aarbu and Thoresen (1997) also highlights that post-reform ”winners” are characterized by large increases in capital income.

5.4 Statistical significance of the results

The most common way to check the statistical validity of SCM estimations are by way of “in-space” placebo tests. These tests simulate the treatment to the other units of the donor pool; if any similar or larger effect is found for the countries not directly exposed to the intervention, then the internal validity of the SCM results would be strongly undermined. Abadie et al. (2015) propose that, as in traditional statistical inference, a quantitative comparison between the distribution of placebo effects and the synthetic control estimate can be performed through the use of pseudo p-values. In this context, a p-value can be constructed by simulating the treatment for each unit in the donor pool and, then, calculating the fraction of such effects greater than or equal to the effect estimated for the real treated unit. Following this procedure, we compute these pseudo p-values for each test performed.

The shares reported in Table 1 provides the probability of getting a more extreme positive deviation from the synthetic control group than the deviation for the true treatment country in each post-reform year (in Appendix Figure C1, we report the corresponding graphs). Overall, the placebo results indicate a high level of significance of the estimated effect of tax reform on the highest top income shares, meaning those in the upper half of the top decile or, more clearly, in the top percentile group. For Australia and Norway the outcome is robust both in the short- and medium-run, whereas effects in New Zealand are primarily significant in the short-term.

[Table 1 about here]

6 Robustness and extensions

In this section, we report robustness tests of the main SCM analysis, DiD regression outputs and, finally, estimations of the tax-reform effect on efficiency-related economic outcomes.

6.1 Different control variables and placebo tests

Two robustness checks of the main SCM results are presented here.[16] The first re-runs the SCM analysis using 35 new combinations of controls when constructing synthetic control groups. There are many potential determinants of top income shares and of tax reforms in addition to those used in the baseline model and we here examine if other control variables would give rise to substantially different results than our baseline. Panel a of Figure 6 shows that using different variables to construct the synthetic control does not invalidate our main findings. There are some alternative specifications, especially for New Zealand, that produce a synthetic control with a lower outcome, inflating the effect of the treatment. On the other hand, Norway is fitted very well. This may be due to the fact that the donor pool is composed of few English-speaking countries from which the synthetic control can be picked (only Canada and Ireland), whereas Norway has more potentially similar countries (Denmark, Finland, Netherlands, and Sweden).

The second robustness check is to make "in-time" placebo tests, where we impose falsely timed tax reforms in the three studied countries (5 years before and after the actual reform). Panel b shows that the "in-time" falsification exercise also reinforces the robustness of results. There are no significant effect that can be compared with the actual reform effect in all the cases.[17]

[Figure 6 about here]

6.2 Panel regression estimates

Panel regressions are run to complement the baseline SCM analysis and to allow us to examine how much of the progressivity effects is due to changes in tax rate progression over the income distribution versus cuts in top marginal tax rates. We run DiD regressions on variants of the following basic log-linear equation:

$$y_{it}^s = \epsilon_1^s \tau_{it}^s + \epsilon_2^s \Pi_{it} + \epsilon_3^s Reform_{it} + \beta^s Z_{it} + \gamma_t + \mu_i + t \times \mu_i + u_{it}^s, \tag{10}$$

where i and t represents a country and a point in time respectively. y_{it}^s is the log share of

[16]Appendix C presents extended variants of these tests along with a third sensitivity test: the scrutiny of post-treatment trends of the control variables in treated and synthetic control group countries in order to detect possible confounding effects of the post-treatment outcomes.

[17]In Figure C2 we show that there is one case, Norway 13 years before the reform (i.e., in 1978), where we encounter an evident tax-reform effect. However, when the placebo is handed out in 1981 and 1984 (respectively 10 and 8 years before the actual reform year), the effect vanishes and we choose to interpret it as a mass significance effect.

total income held by income fractile s and we use either τ_{it}^{s}, the log of the net-of-tax rate (i.e., 1 - MTR), or Π_{it}, the tax progressivity indicator, to account for any of the two influences on top shares. $Reform_{it}$ is a dummy equal to one for each post-reform year. We focus on the three studied reforms above but also run tests on the other recorded progressivity-reducing tax reforms in our sample.[18] $Z_{i,t}$ is a vector of the same controls as in the baseline SCM estimations, γ_t and μ_i are time and country fixed effect, respectively, and $t \times \mu_i$ are country-specific linear time trends. u_{it}^{s} is a country-transitory shock that has mean zero at each time *t* and it is correlated over time. Since our dataset only contains a small number of groups on which clustered (i.e., 18 countries), we produce statistical inference based on the Cameron, Gelbach, and Miller (2008) wild-bootstrap approach.

Note that several econometric issues arise here. First, including both a measure of tax progressivity or top tax rates and a tax-reform dummy is potentially problematic due to the collinearity between these variables. Therefore, we use them separately in almost all regression models, except when we explicitly test whether there is a possible additional reform effect over and above the level of progressivity or top taxation. Second, the error term might be correlated with measures of tax rate and progressivity due to reverse causality, leading to a biased towards zero parameter. Third, each non-tax related factor affecting top incomes that is not captured by the vector of controls and the country and time fixed effects may give rise to an omitted variable bias, biasing the estimated coefficients of interest. It is because of these problems that we view the panel regressions merely as complementary to the main SCM analysis.

Table 2 and 3 present the main regression results (additional regressions are presented in Appendix D). We analyse the effect of progressivity-reducing tax reforms by running DiD regressions on three outcomes: the income share held by the bottom 90 percent of the top decile (Top 10-1), the income share held by the bottom 90 percent of the top 0.1 (Top 1-0.1), and the income share held by the top 0.1 of the income distribution. In Table 2, we first focus on all the registered tax reforms' episodes (column 1), then we use the dummy only for the three "significant" reforms (column 2) and specifically for each reform of interest (columns 3-5). In-line with the baseline SCM results, the coefficients indicate a positive association between tax reforms and top income shares. The association is also stronger higher up in the income distribution, especially in the groups above the top percentile, and especially when we restrict the analysis to the reforms in the three *treated* countries. On the other hand, we find modest or insignificant effects for the groups below the top percentile, as in the SCM estimations. Focusing on all the registered progressivity-reducing tax reforms, we find that they increased the top 0.1 percentile by 20 % (panel c, column 1), while the magnitude of the effect more than double as we focus on the three significant reforms (panel c, column 2).[19]

[18] These reforms are: Canada 1982, Finland 1991, France 1986, Germany 2005, Ireland 1992, Italy 1989, Netherlands 1990, Spain 2007, Sweden 1991, UK 1988, and US 1987. The selection of these reforms is motivated by their Ω values, displayed in Appendix Table A5.

[19] In Appendix Table D2, we account for the different intensity of the tax reforms by weighting them by Ω values; the results are fairly in-line with those presented in Table 2, showing that the effects are larger as we give more weight to the reforms that reduced tax progressivity more heavily.

[Table 2 about here]

In Table 3, we investigate whether the reform effects were mostly driven by tax progressivity or tax rate variation by running regressions of top income shares on rate progression and/or net-of-tax rate, both separately and jointly with the reform dummies. We find that top income shares respond more to variations in marginal tax rates than in progressivity level.[20] We estimate an elasticity of top 0.1 percentile with respect of net-of-marginal tax rate of 0.852 (panel c, column 6); the elasticity reduces of nearly 30 percent when we add the reform dummy (columns 7-8). The coefficients in columns 3 and 4 show that a percentage reduction in tax progressivity decreases the income share held by the top 0.1 by between 24 and 30 %. Alternative regressions (see Appendix Table D1) show that the association appears to grow when we use the marginal tax rate-based measures than when we use average tax rate-based measures.

[Table 3 about here]

6.3 Other reform effects

In this section, we perform two tests. First, we investigate other subtle dimensions of tax reforms, such as variations in the number of tax brackets and in the tax base broadness. Second, we test whether our main findings significantly change as we control for other political and economic reforms carried out during the period of interest.

6.3.1 Broader base and fewer brackets

Tax reforms often bring many changes to the tax system over and above new tax rate schedules adjusting the level and progression of tax rates over taxable income. In particular, most of the the 1980s and early 1990s tax reforms shared an overall ambition to offset the reduction in top marginal tax rates by broadening the tax base and decreasing the number of income tax brackets. We complement the study of these reforms focusing specifically on the effects of variations in tax brackets and tax base on top incomes shares. Accounting for these additional dimensions might be important since they are likely to have a direct influence on top income earners and confound some of the measured effects in the main analysis (Kopczuk, 2005; Doerrenberg et al., 2017).

We combine income data from the WID with tax information from the OECD Tax Database to construct our measure of tax base broadness and account for variation in the number of legally defined tax brackets on personal income for each country in our dataset over the 1981-2010 period. Namely, we compute a time- and country-varying measure of the tax base as the

[20] Note, however, that it is not possible to perfectly disentangle the two forces since they are connected, both in theory - as they reflect the overall direction of tax policy - and in their construction since the progressivity measures are based on information provided in tax schedules. Moreover, we interpret cautiously the coefficients when the dummy reform and the tax rate or the progressivity measure are used in the same regression because of the collinearity between these variables.

ratio between broad and taxable income defined at the average country by year income level. To compute this indicator, we start from the broad measure of income and then we subtract tax deductions, allowances and credits available at both central and sub-central levels.[21]

In Table 4, we show the effect of tax base and tax brackets on top income shares first separately and then jointly with the net-of-marginal tax rate. We find that the number of tax brackets has a negative effect on the income share held by taxpayers in the groups above the top percentile; as long as more tax brackets underline a higher level of tax progressivity, this result is in-line with our main finding of a negative effect of progressivity on inequality. Panel b shows that a broader tax base has a negative effect on top income shares, but the coefficients are not statistically significant. As we introduce the net-of-tax rate, the tax brackets' coefficient loses its statistical significance, even if the sign remains unchanged. This test further shows that either controlling for variations in the number of tax brackets and in the tax base broadness, the elasticity of top 0.1 with respect to the net-of-marginal tax rate remains statistically significant.

[Table 4 about here]

6.3.2 The role of other political and economic reforms

Governments typically launched into a sequence of political and economic reforms over just a few years. These additional reforms may ideally influence top incomes, confounding the main effect of the tax reform. To account for this potential bias, we retrieve information on reforms in the financial, capital and banking sectors, products markets, and trade for all our countries over the period 1960-2004 from Giuliano et al. (2013). We test whether the effects we attribute to the progressivity-reducing tax reform significantly change as we control for these other reforms. We run regressions with reform dummies separately for all the registered tax reforms and the significant reforms on the top percentile. In table 5, we report the result of this exercise. In column 1, we report the baseline reform effect on the top percentile. We first separately control for each of the reforms of interest (column 2-6) and then we use all the reforms as covariates in the same regression (column 7) in addiction to the baseline controls. Reassuringly, the coefficient of interest is fairly stable either controlling for one reform at time or controlling for all the reforms. In the model with the full set of reforms included among the covariates, the effect of the significant tax reforms on the top percentile decreased by about 20 %, but it remains strongly significant.

[Table 5 about here]

[21] Such reliefs are universally available and are unrelated to expenditures incurred by taxpayers, and typically available as fixed amounts or some fixed percentage of income. When relief is provided in the form of a tapered tax credit, the maximum amount is applied. Note that, however, this measure of the tax base is not perfect, since many reliefs and other dimensions cannot be considered (for instance, tax avoidance and tax evasion, shifting from different tax bases, taxpayer-specific tax reliefs, etc.).

6.4 Efficiency effects: Do tax reforms increase the size of the cake?

Arguments about enhanced economic activity and efficiency were important behind the tax reforms of the 1980s (Auerbach and Slemrod, 1997; Gale and Samswick, 2014). This raises the questions whether our estimated results above reflect a redistribution of incomes from the bottom and middle to the top or if they are the result of new resources being generated by top income earners.

We examine this issue by running SCM estimations on three outcome variables that reflect real economic activity: GDP per capita, registered patents per-capita and total tax revenues as share of GDP. Country sample and control variables are the same as in the main analysis. These "efficiency variables" are, of course, not perfect. They are aggregate for the whole economy and not, as in the case of top income shares, able to distinguish between groups in the top of the income distribution. Furthermore, they capture both efficiency-related and other, not necessarily efficiency-related, processes. The tests should therefore be interpreted with caution.

Figure 7 shows the results: none of the three efficiency variables is significantly affected by the tax reform treatments in any of the three studied countries. In the case of GDP per capita, there is a small difference in the post-treatment period levels, but a closer look indicates that this wedge actually occurred some year before or after the tax reforms were implemented. Moreover, the synthetic New Zealand has a markedly higher post-treatment growth, but that also appears to have started before the reform date.

[Figure 7 about here]

As a further check, we also run panel regressions on the same outcomes as above and using the reform dummy as the right-hand variable of interest. The results - shown in Table 6 - do not provide any evidence between progressivity-reducing tax reforms and efficiency-related outcomes. The only statistically significant effect we find is between the Norwegian 1992 tax reform and the number of registered patents (panel b, column 5). This result seems in-line with the existing evidence of a significant responsiveness of star scientists and inventors to both cross-state (Moretti and Wilson, 2017) and cross-country (Akcigit et al., 2016) tax variations.

[Table 6 about here]

Taken together, we can reject strong real income responses to the progressivity reductions. At the same time, we found the strongest income share response among the absolute top groups for which capital incomes are the most predominant income source. This suggests that activities of tax planning and income shifting of capital income are a more plausible explanation for the documented significant increase in income inequality. This would go well in-line with previous findings in other analyses of tax reforms, e.g., in Auerbach (1988), Slemrod (1996), Piketty et al. (2014) and Saez (2017).

7 Conclusions

The question we set out to answer was how tax progressivity change affects the income distribution. While this question has been studied before, the unique contribution of our study is that we exploit tax reforms as source of progressivity variation and the synthetic control method for identifying the effects on top income shares. To our knowledge we are the first to approach the progressivity-inequality nexus in this way.

Our findings suggest that the reduced progressivity recorded during the tax reforms of the 1980s and early 1990s had strong and positive effects on the income shares of the very top of the distribution in all three countries studied. The effects appear to have lasted for a long time, at least ten years and perhaps as long as 25 years in the case of Australia. In contrast, we are unable to trace any significant income responses to the progressivity reforms in the lower parts of the top income decile. In fact, effects are relatively marginal in all top income groups below the highest income percentile.

The mechanisms behind this result are yet to be identified, but we make some tests allowing us to draw some tentative conclusions. First, we find that reduced top marginal tax rates can account for most of the recorded boost in top percentile income shares, while the reduction in tax rate progression matters less. Second, changes in capital income among the very top groups appear to be an important part of the story behind the top share response. This indicates that tax avoidance behavior could matter, which confirms earlier case studies of tax reform responses. Third, and related, we cannot find any evidence on any real economic responses, e.g., increased labor supply or higher efficiency in general, to the progressivity reductions. An important disclaimer is that our mechanism analyses rest on less precise data and therefore needs to be interpreted with caution. This motivates future research to decompose the different components of the behavioral response to tax reform, improving our understanding of the relationship between taxation and inequality.

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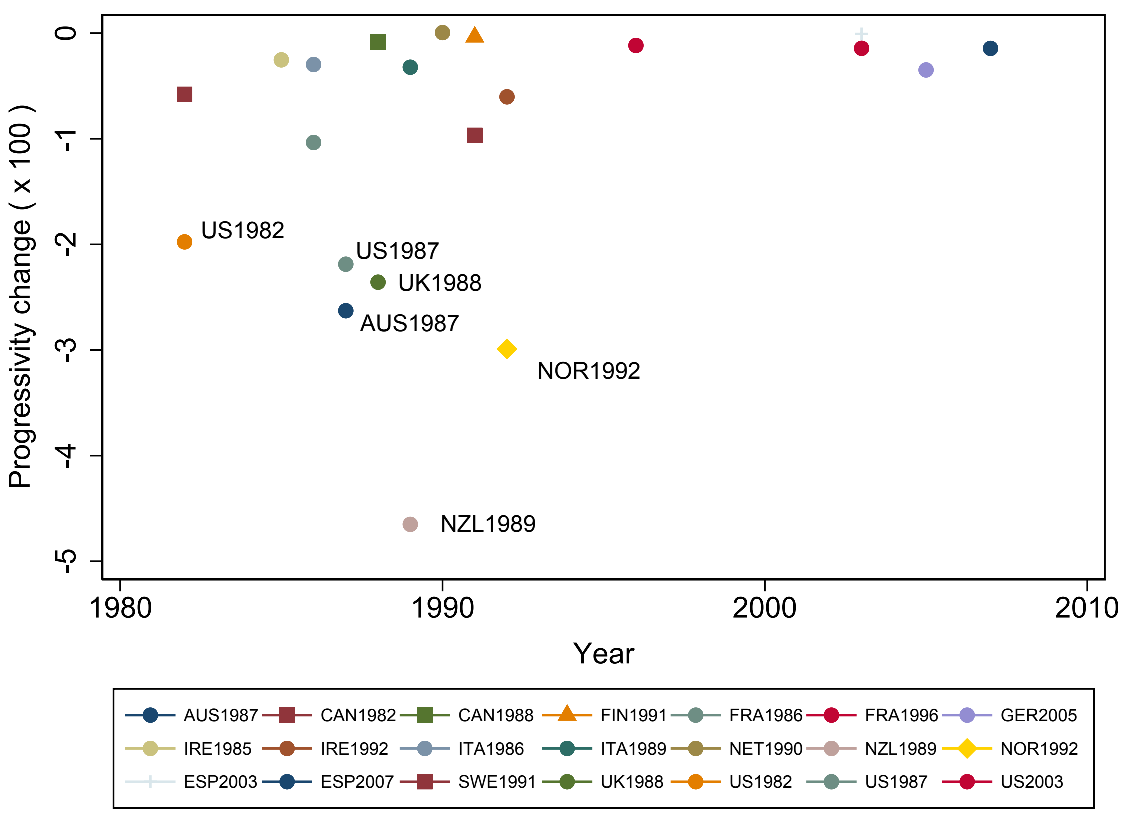
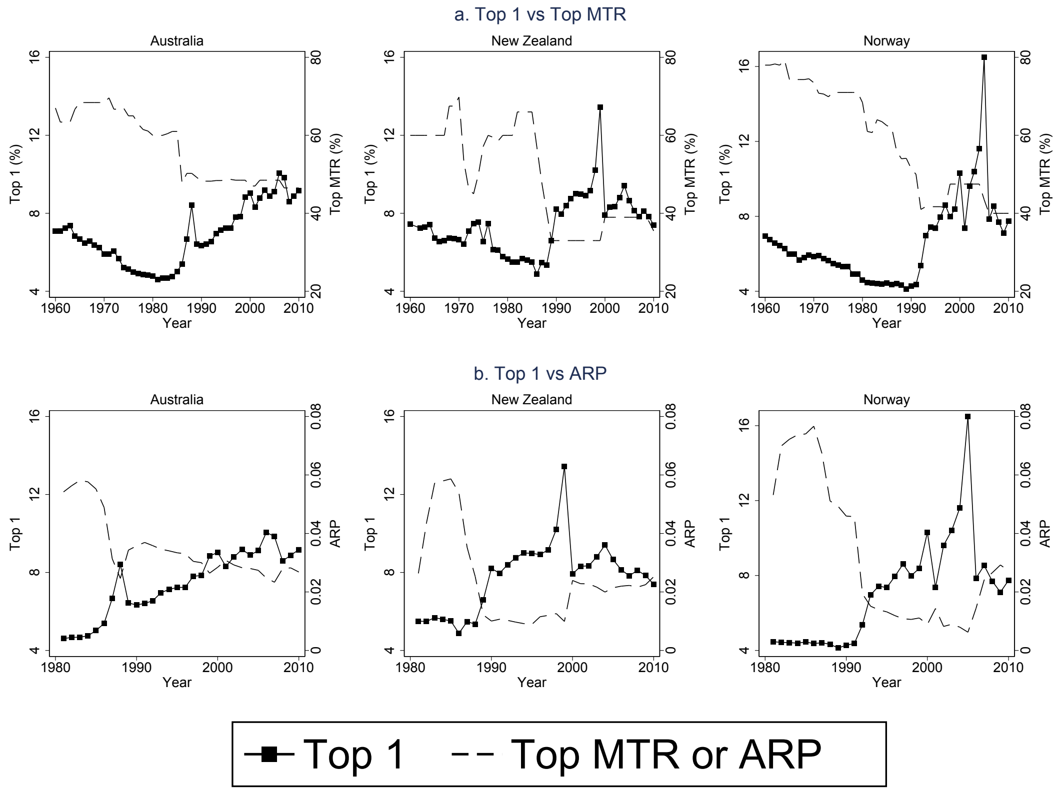
Figure 1: Progressivity-reducing tax reforms since 1981



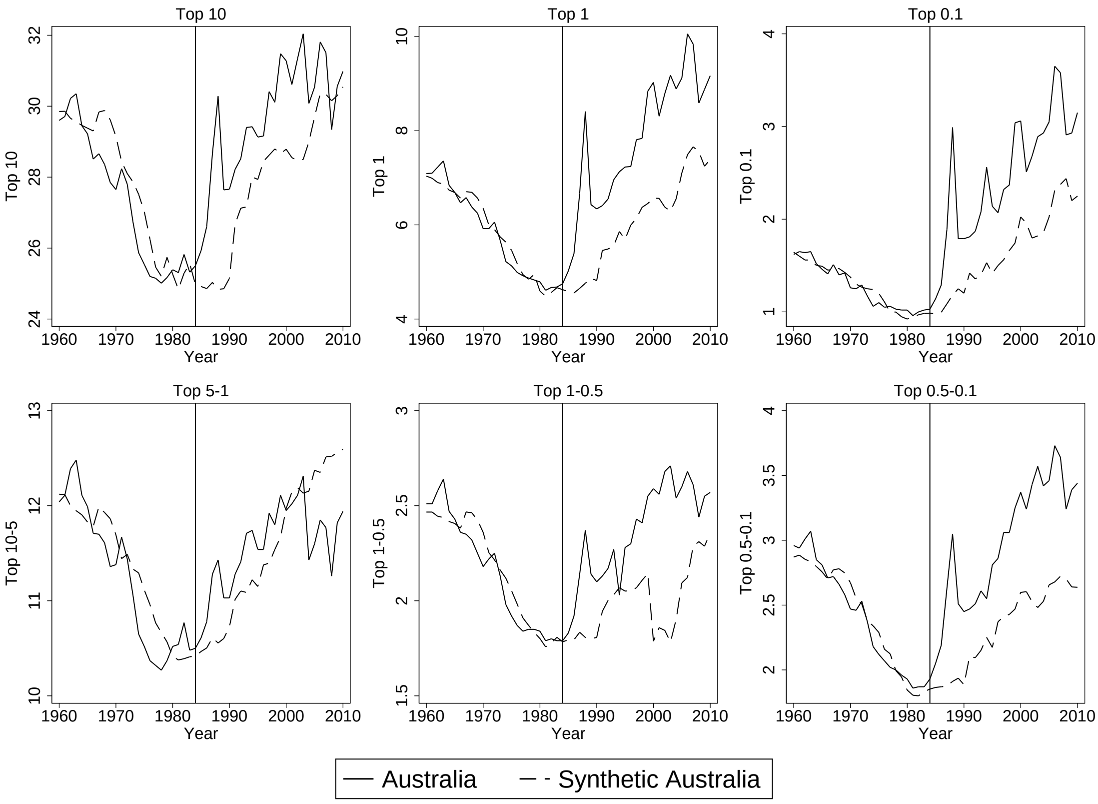
Note: Each point in this figure represents the negative variation in the average rate progression (ARP) over the middle-top income interval after a tax reform. This variation is calculated as the difference in the ARP between post- and pre-reform period (numerical values are reported in Table A5). In countries marked with * (i.e., Finland, Germany and Sweden), the ARP is derived computing the average tax rate (ATR) faced by an earner whose income is equal to 4 time the GDP per-capita and an earner whose income is equal to the GDP per-capita, instead of the baseline differential between taxpayers in the top percentile and those with average income level. ATRs are calculated from OECD Tax Database schedules. Top income series are from the World Wealth and Income Database (WID). GDP per-capita is from The Maddison-Project.

Figure 2: Comparison between Top 1, Top MTR and ARP in selected countries



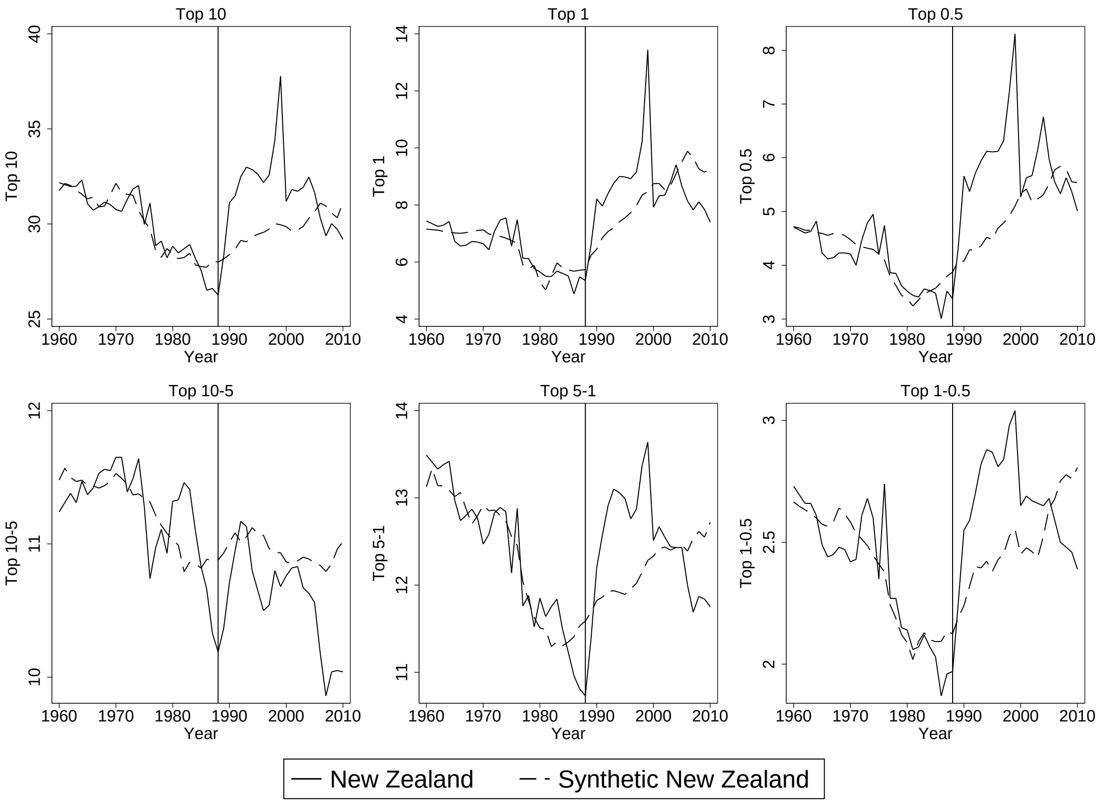
Note: Panel a displays the evolution of top percentile and top marginal tax rate in Australia, New Zealand and Norway. Panel b compares trends in tax progressivity and in the income share held by the top percentile of the income distribution over the 1981-2010 period.

Figure 3: Australian tax reform



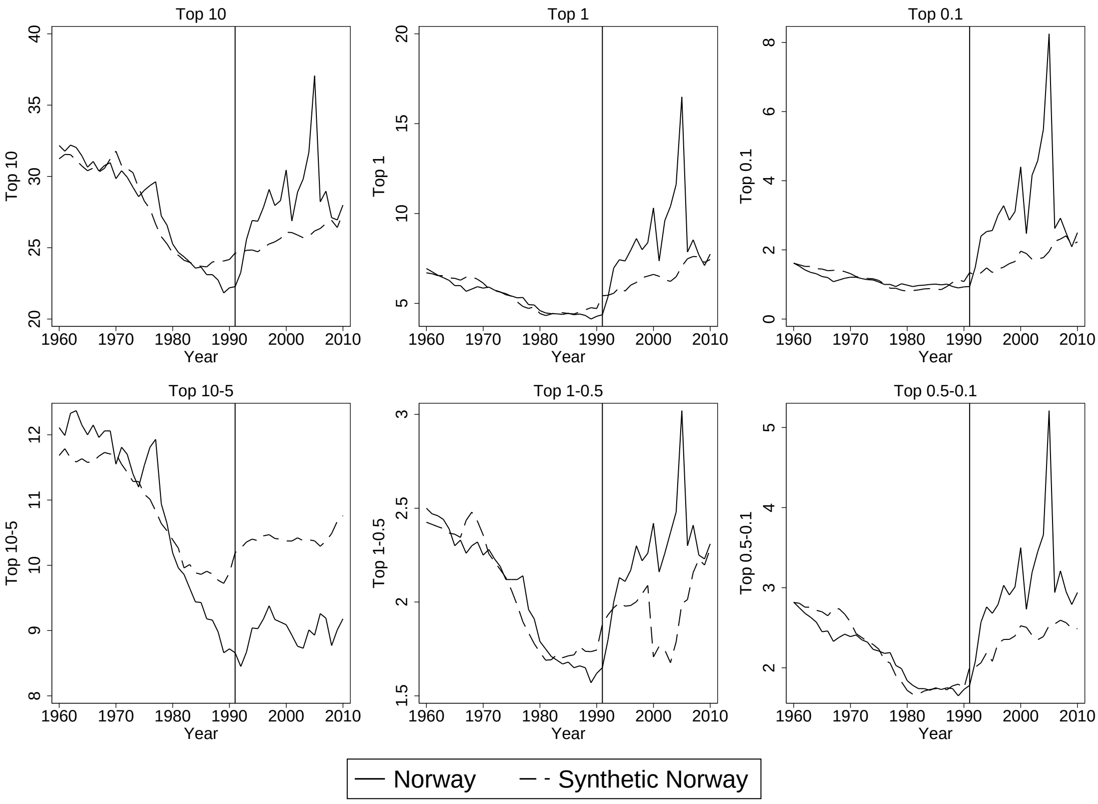
Note: For each top income share, a synthetic control group (dashed line) is calibrated to match the true trend (solid line) before the tax reform. The trend of the synthetic control represents the trend in Australia in absence of the tax reform, and the difference between the two lines is the effect of the tax reform on top income shares. The vertical solid line - which represents the year in which the reform was announced - splits the full time-period (1960-2010) in pre- and post-reform period.

Figure 4: New Zealand's tax reform



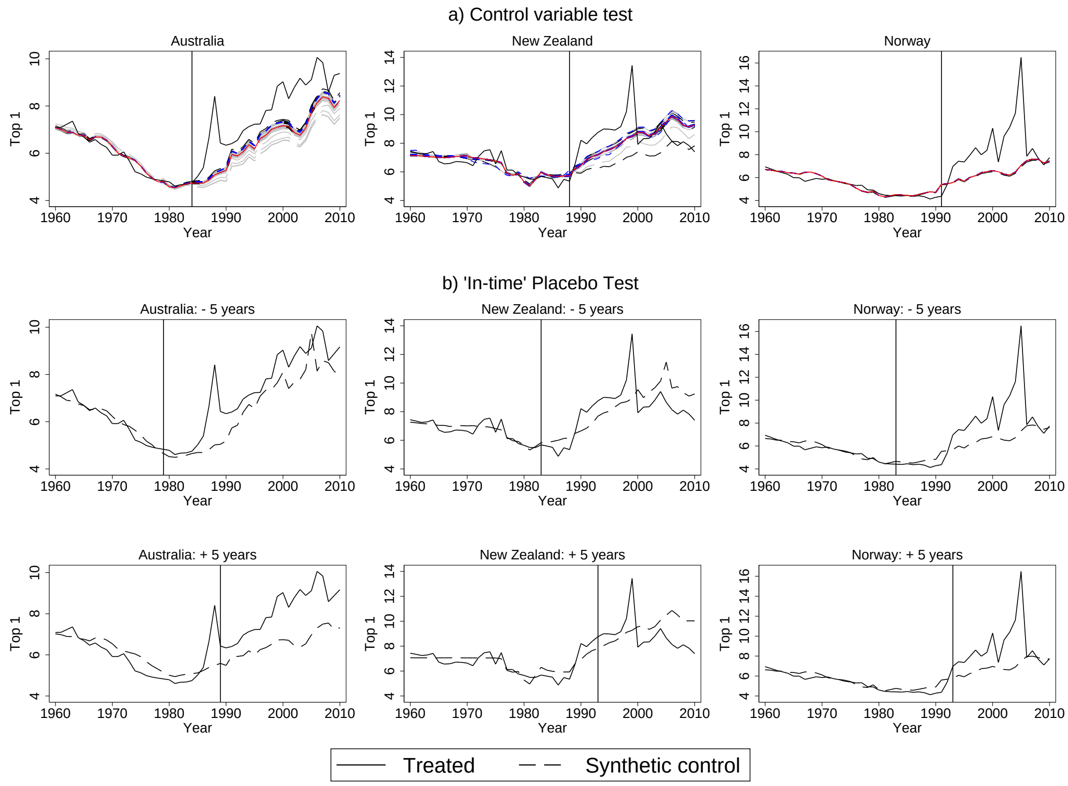
Note: See Figure 3 for details.

Figure 5: Norwegian tax reform



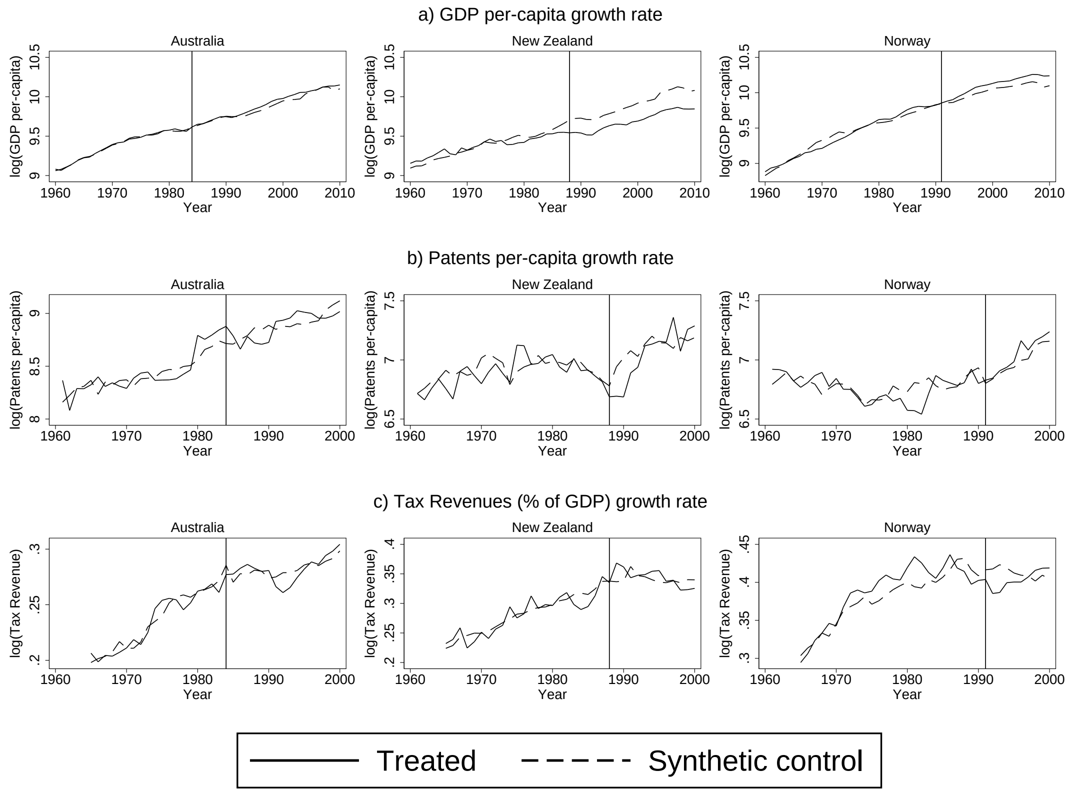
Note: See Figure 3 for details.

Figure 6: Robustness of the main SCM results



Note: Top figures tests whether the outcome is sensitive to the choice of the vector of controls. The black solid line is the true trend of the top percentile in the treated country. The red line is the synthetic control group trend calculated from the baseline set of control variables, and each line represents the synthetic control group obtained with a different set of controls. This robustness test is based on 35 different combinations of variables, combined with three different set of years. Blue and grey lines differ in the set of years used to control for pre-treatment trends in the top percentile. Altogether, there are 105 different combinations and, thus, the same number of potentially different synthetic control for each country. In the figure there are less than 105 lines: this is because the SCM assigns a very small weight to some variable and, thus, the resulting optimization problem is basically identical to the baseline model. Additional notes on this test are reported in Appendix C2. Middle and bottom figures impose falsely timed tax reforms in the three studied countries 5 years before (middle figures) and after (bottom figures) the actual reform year. An extended version of this test - which additionally simulate the treatment in other years - can be found in Appendix C3.

Figure 7: Efficiency effects of tax reforms



Note: This figure reports tax reforms effects on three ”efficiency” outcomes: GDP per-capita, patents per-capita and tax revenue as a share of GDP. Data on patents are from Roine et al. (2009), whereas data on GDP per-capita are from the Maddison Project, version 2013. Data on tax revenue are from OECD.

Table 1: Statistical significance according to "in-space" placebo test

	Australia			New Zealand			Norway		
	Top 5-1	Top 1-0.5	Top 0.1	Top 10-5	Top 1-0.5	Top 0.5	Top 10-5	Top 1-0.5	Top 0.1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1985	0.4	0.27	0.11						
1986	0.3	0.09	0.11						
1987	0.1	0.09	< 0.01						
1988	0.2	< 0.01	< 0.01						
1989	0.3	0.09	< 0.01	0.8	0.2	0.1			
1990	0.3	0.09	< 0.01	0.9	< 0.01	< 0.01			
1991	0.3	0.09	< 0.01	0.5	< 0.01	< 0.01			
1992	0.3	0.18	< 0.01	0.1	0.1	< 0.01	0.91	0.45	< 0.01
1993	0.1	< 0.01	< 0.01	0.4	< 0.01	< 0.01	0.91	0.27	< 0.01
1994	0.3	0.18	< 0.01	0.5	< 0.01	< 0.01	0.91	0.18	< 0.01
1995	0.3	0.09	< 0.01	0.6	< 0.01	< 0.01	0.91	0.18	< 0.01
1996	0.3	0.18	< 0.01	0.7	0.1	< 0.01	0.91	0.18	< 0.01
1997	0.3	< 0.01	< 0.01	0.5	0.1	< 0.01	0.91	0.18	< 0.01
1998	0.3	< 0.01	< 0.01	0.4	0.1	< 0.01	0.91	0.18	< 0.01
1999	0.3	< 0.01	< 0.01	0.6	< 0.01	< 0.01	0.91	0.18	< 0.01
2000	0.3	< 0.01	< 0.01	0.4	0.2	0.3	0.82	< 0.01	< 0.01
2001	0.3	< 0.01	< 0.01	0.3	0.2	0.3	0.82	0.09	< 0.01
2002	0.3	< 0.01	< 0.01	0.3	0.3	0.2	0.82	< 0.01	< 0.01
2003	0.3	< 0.01	< 0.01	0.4	0.3	0.1	0.82	< 0.01	< 0.01
2004	0.4	< 0.01	< 0.01	0.4	0.3	< 0.01	0.82	< 0.01	< 0.01
2005	0.5	< 0.01	< 0.01	0.5	0.3	0.1	0.82	< 0.01	< 0.01
2006	0.5	< 0.01	< 0.01	0.5	0.5	0.2	0.82	0.27	0.22
2007	0.5	0.27	< 0.01	0.6	0.6	0.3	0.82	0.27	< 0.01
2008	0.6	0.36	< 0.01	0.7	0.7	0.2	0.82	0.36	0.22
2009	0.6	0.36	< 0.01	0.7	0.6	0.2	0.82	0.36	0.33
2010	0.6	0.36	< 0.01	0.7	0.7	0.3	0.82	0.36	< 0.01

Note: This table shows the probability of getting a more extreme positive deviation from the synthetic control group than the deviation for the actual treated country for each post-reform year. It relates to the graphs in Figure C1, as the number represents the share of grey lines that are higher than the black line for each post-reform year. The sample consists of the donor pool population for each top income share (all country with 0 or a positive weight in Tables B1-B3). Countries with bad pre-treatment fit are dropped, as suggested by Abadie et al. (2010).

Table 2: DiD estimation of the reform effect

	Reform dummy = 1 for:				
	All reforms	"Significant" Reform	AUS 1987	NZL 1989	NOR 1992
	(1)	(2)	(3)	(4)	(5)
		a. log(Top 10-1)			
Reform	0.026	0.047*	0.095**	-0.011	-0.012
	(0.017)	(0.026)	(0.048)	(0.015)	(0.053)
Obs.	713	713	514	513	514
		b. log(Top 1-0.1)			
Reform	0.080*	0.207***	0.189*	0.158**	0.189**
	(0.043)	(0.072)	(0.069)	(0.093)	(0.098)
Obs.	712	712	612	611	612
		c. log(Top 0.1)			
Reform	0.208**	0.497**	0.420***	0.292***	0.580**
	(0.097)	(0.197)	(0.102)	(0.110)	(0.271)
Obs.	647	647	454	452	454
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Country × time trend	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

Note: This table shows ϵ^s estimate from panel regressions of the following form: $y^s_{i,t} = \epsilon^s Reform_{i,t} + \beta^s Z_{i,t} + \gamma_t + \mu_i + t \times \mu_i + u^s_{i,t}$. The vector of controls is the same as that used in the SCM analysis. In column 1, $Reform$ is a dummy equal to 1 in the post-reform period in the following countries (first year in parenthesis): Australia (1987), Canada (1982), Finland (1991), France (1986), Germany (2005), Ireland (1992), Italy (1989), Netherlands (1990), New Zealand (1989), Norway (1992), Spain (2007), Sweden (1991), UK (1988), and US (1987). In column 2, the dummy is equal to 1 only for the three significant cases (i.e., Australia, New Zealand and Norway). In columns 3-5, we focus specifically on each reform, thus the reform dummy is equal to 1 in the country considered and we drop the other treated countries from the sample. For New Zealand, we replace data on top 1-0.1 and top 0.1 with top 1-0.5 and top 0.5 since data on top 0.1 is missing. The sample is composed of 18 countries over the 1960-2010 period. Wild-bootstrap standard errors clustered at country-level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Tax rate vs progressivity effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				a. log(Top 10-1)				
Reform	0.021			0.027*			0.013	
	(0.014)			(0.016)			(0.016)	
Sig. Reform		0.029*			0.036**			0.042
		(0.015)			(0.015)			(0.026)
log(Rate Prog)			-0.005	0.007	0.009			
			(0.008)	(0.013)	(0.011)			
log$(1-MTR)$						-0.006	-0.017	-0.031
						(0.028)	(0.026)	(0.039)
Obs.	465	465	377	377	377	349	349	349
				b. log(Top 1-0.1)				
Reform	0.082*			0.062			0.044	
	(0.046)			(0.040)			(0.046)	
Sig. Reform		0.205**			0.077			0.181***
		(0.081)			(0.085)			(0.062)
log(Rate Prog)			-0.139***	-0.111***	-0.109*			
			(0.048)	(0.038)	(0.061)			
log$(1-MTR)$						0.207**	0.161**	0.109
						(0.102)	(0.073)	(0.071)
Obs.	464	464	376	376	376	319	319	319
				c. log(Top 0.1)				
Reform	0.212*			0.168			0.143	
	(0.126)			(0.106)			(0.115)	
Sig. Reform		0.613**			0.437*			0.487**
		(0.285)			(0.235)			(0.243)
log(Rate Prog)			-0.304***	-0.235***	-0.118			
			(0.105)	(0.081)	(0.140)			
log$(1-MTR)$						0.852**	0.752**	0.604**
						(0.375)	(0.321)	(0.295)
Obs.	401	401	327	327	327	315	315	315
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES
Country $\times$ t	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Note: This table shows ϵ^s coefficients estimate from panel regressions of the following form: $y^s_{i,t} = \epsilon^s_1\tau^s_{i,t} + \epsilon^s_2\Pi_{i,t} + \epsilon^s_3 Reform_{i,t} + \beta^s Z_{i,t} + \gamma_t + \mu_i + t \times \mu_i + u^s_{i,t}$. The vector of controls is the same as that used in the SCM analysis. For New Zealand, we replace data on top 1-0.1 and top 0.1 with top 1-0.5 and top 0.5 since data on top 0.1 is missing. The sample is composed of 18 countries over the 1981-2010 period. Wild-bootstrap standard errors clustered at country-level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Other dimensions of tax reform

	log(Top 10-1)		log(Top 1-0.1)		log(Top 0.1)	
	(1)	(2)	(3)	(4)	(5)	(6)
a. Tax brackets and top MTR						
log(Tax Brackets)	0.005	0.008	-0.060**	0.008	-0.148***	-0.018
	(0.010)	(0.014)	(0.024)	(0.029)	(0.051)	(0.076)
$\log(1 - MTR^s)$		0.004		0.220		0.814*
		(0.017)		(0.159)		(0.469)
Obs.	430	347	430	317	366	313
b. Tax base and top MTR						
log(Tax Base)	-0.032	-0.041	0.057	0.027	-0.185	-0.214
	(0.059)	(0.077)	(0.126)	(0.052)	(0.292)	(0.408)
$\log(1 - MTR^s)$		-0.004		0.216**		0.870***
		(0.028)		(0.107)		(0.359)
Obs.	376	339	376	309	327	305
Country FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Country $\times$ time trend	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES

Note: This table shows e^s coefficients from regressions of the form $y_{s,i,t} = e_1^s log(1 - MTR_{i,t}^s) + e_2^s Others_{i,t} + \beta^s Z_{i,t} + \gamma_t + \mu_i + t \times \mu_i + u_{i,t}^s$. The vector of controls is the same as that used in the SCM analysis. For New Zealand, we replace data on top 1-0.1 and top 0.1 with top 1-0.5 and top 0.5 since data on top 0.1 is missing. Data on the number of tax brackets on personal income set by the central government are collected for 17 countries over the 1981-2010 period (data is not available for Germany over the whole time period, while tax schedules for Finland and Japan are available only since 2000) from the OECD Tax Database. MTR^s are authors computations from OECD national tax schedules. Tax base data are calculated combining tax information from OECD Tax Database with income data from WID. Wild-bootstrap standard errors clustered at country-level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Controlling for other political and economic reforms

	Dependent variable: log(Top 1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			a. Tax reform of interest: All reforms				
Reform	0.079	0.072	0.078	0.072	0.087	0.069	0.086
	(0.056)	(0.056)	(0.053)	(0.054)	(0.063)	(0.054)	(0.057)
Observations	494	415	415	415	372	415	372
			b. Tax reform of interest: "Significant" reforms				
Reform	0.338***	0.274***	0.266***	0.275***	0.275***	0.279***	0.264***
	(0.102)	(0.103)	(0.098)	(0.105)	(0.105)	(0.106)	(0.102)
Obs.	494	415	415	415	372	415	372
Country FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Country × t	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES
Control for other reforms?	NO	Product market	Trade sector	Capital account	Current account	Financial sector	All

Note: This table replicates the results from Table 2 by adding controls for other political and economic reforms in addition to the baseline set of controls. In the last column, we control for all the reforms simultaneously. See Giuliano et al. (2013, Table 1) for further information on how these proxies for the reforms are computed. Wild-bootstrap standard errors clustered at country-level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: DiD estimation of the efficiency reform effects

	Reform dummy = 1 for:				
	All reforms	"Significant" Reform	AUS 1987	NZL 1989	NOR 1992
	(1)	(2)	(3)	(4)	(5)
		a. log(GDP per-capita)			
Reform	-0.047	-0.008	0.012	-0.052	-0.004
	(0.032)	(0.023)	(0.014)	(0.050)	(0.018)
Obs.	918	918	714	714	714
		b. log(Patents)			
Reform	-0.021	0.076	0.039	-0.222	0.308*
	(0.084)	(0.122)	(0.090)	(0.162)	(0.166)
Obs.	824	824	640	640	640
		c. log(Tax revenue)			
Reform	-0.010	-0.009	-0.024	0.016	-0.027
	(0.032)	(0.019)	(0.020)	(0.027)	(0.045)
Obs.	828	828	644	644	644
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Country $\times$ time trend	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

Note: This table shows ϵ^s coefficient estimate from panel regressions of the following form: $y^s_{i,t} = \epsilon^s Reform_{i,t} + \beta^s Z_{i,t} + \gamma_t + \mu_i + t \times \mu_i + u^s_{i,t}$. The vector of controls is the same as that used in the SCM analysis. In column 1, $Reform$ is a dummy equal to 1 in the post-reform period in the following countries (first year in parenthesis): Australia (1987), Canada (1982), Finland (1991), France (1986), Germany (2005), Ireland (1992), Italy (1989), Netherlands (1990), New Zealand (1989), Norway (1992), Spain (2007), Sweden (1991), UK (1988), and US (1987). In column 2, the dummy is equal to 1 only for the three significant cases (i.e., Australia, New Zealand and Norway). In columns 3-5, we focus specifically on each reform, thus the reform dummy is equal to 1 in the country considered and we drop the other treated countries from the sample. The sample is composed of 18 countries over the 1960-2010 period. Wild-bootstrap standard errors clustered at country-level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Appendix A: Data appendix

A1. Summary statistics

Table A1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Top 10	713	31.635	4.608	18.77	46.35
Top 5	728	20.657	3.759	12.1	33.84
Top 1	763	8.005	2.422	3.49	18.33
Top 0.5	708	5.475	2.002	2.38	14.31
Top 0.1	598	2.399	1.292	0.73	8.25
Top 10-5	677	10.907	1.25	6.28	14.77
Top 10-1	713	23.579	2.799	14.45	32.02
Top 1-0.1	713	6.121	1.57	3.21	13.44
Top 1-0.5	708	2.586	0.481	1.5	4.05
Top 0.5-0.1	582	3.206	0.851	1.63	6.15
MTR^{top}	918	0.557	0.136	0.28	0.963
MTR^{10}	386	0.469	0.107	0.269	0.755
MTR^{1}	386	0.487	0.1	0.269	0.755
$MTR^{0.5}$	385	0.491	0.1	0.269	0.755
$MTR^{0.1}$	340	0.504	0.1	0.269	0.755
MTR^{10-5}	357	0.456	0.114	0.26	0.735
MTR^{10-1}	350	0.451	0.1	0.26	0.755
$MTR^{1-0.1}$	320	0.474	0.095	0.269	0.755
$MTR^{1-0.5}$	384	0.483	0.098	0.269	0.755
ATR^{10}	386	0.379	0.095	0.225	0.637
ATR^{1}	386	0.441	0.09	0.278	0.707
$ATR^{0.5}$	385	0.454	0.091	0.281	0.72
$ATR^{0.1}$	340	0.484	0.094	0.286	0.749
ATR^{10-5}	357	0.348	0.116	0.189	0.65
$ATR^{1-0.5}$	384	0.421	0.098	0.269	0.681
Reform	918	0.303	0.46	0	1
Significant Reform	918	0.071	0.257	0	1
ARP	443	0.08	0.022	0.018	0.144
MRP	443	0.088	0.022	0.028	0.159
GDP per-capita	918	15349	5643	2956	31655
GDP per-capita growth rate	899	0.024	0.027	-0.086	0.119
Party Orientation	600	0.481	0.469	0	1

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International Trade	812	45.71	26.55	6.6	187.36
Globalization	918	69.332	13.495	34.05	92.509
Debt	918	42.302	27.68	3.3	189.1
Debt Growth Rate	900	0.018	0.14	-0.33	1.08
Central Government Spending	830	0.177	0.046	0.072	0.301
Gross Savings	696	24.138	4.983	10.295	41.745
Gross Fixed Capital	831	24.115	3.773	15.914	37.228
Bank Deposits	830	0.574	0.229	0.154	1.416
UK Legal Origin	918	0.333	0.472	0	1
Population Growth Rate	918	0.756	1.037	-0.9	26.4
Working Age Population	918	65.22	3	38.13	70.12
Annual Hours Worked	918	1816	217	1381	2621
Trade Union Density	918	39.184	19.533	7.548	83.863
Stock Market	831	0.475	0.434	0.002	3.034
Financial Development	918	1.11	0.581	.33	4.349
Patents	824	22973	60171	54	384201
Patents Growth	806	0.018	0.112	-0.505	0.815
High Education	162	11.179	6.589	0.83	30.94
Enrollment Ratio	657	42.74	21.072	7.273	97.093
Human Capital	918	2.74	0.455	1.538	3.619
Tax Revenue	828	0.327	0.076	0.133	0.495
Tax Brackets	470	6.202	4.919	1	34
Tax Base	379	0.873	0.106	0.576	0.999
Product Market	812	0.201	0.293	0	1
Trade	773	0.830	0.140	0.166	1
Capital Account	594	0.803	0.302	0	1
Current Account	736	0.785	0.241	0.143	1
Domestic Financial	594	0.701	0.259	0	1

Note: See Table A4 for specific information on these variables and their sources.

A2. Top income data

Table A2: Characteristics of international top income data

Country	Country-specific information	Change to original data
Australia	Income year goes from July 1st year t to June 30 year t+1. Capital gains are included.	No.
Canada	Estimates excluding capital gains. Fractiles defined by total gross income excluding capital gains. Until 1981 the series of every top group are based on tabulated tax data, and relate to adults age 20+. Since 1982, the series are based on LAD (Longitudinal Administrative Database) and relate to taxfilers.	No.
Denmark	Estimates excluding capital gains. Fractiles defined by total gross income excluding capital gains. Until 1968 tax units were individuals aged 15+ minus married females. 1969 was a tax free year. Since 1970, the tax units have become individuals aged 15+ (some individuals below 15 years also file a tax return if they earn a sufficiently high income).	Linear interpolation for year 1969 for each category. Linear interpolation in year 1973 for Top 1 and categories below. Interpolated for years 1967-1970 and 1974-1976 for Top 0.1 and categories below.
Finland	Until 1992 the series are based on tabulated tax data by ranges of income where the unit of analysis is the tax unit, and the income concept is taxable income. Since 1990, the series are based on IDS (Income Distribution Survey), where the unit of analysis is the individual aged 15+ with non-zero incomes, and the income concept is taxable income.	No.
France	Series expressed as percentage of total income excluding capital gains. Fractiles defined by total income excluding capital gains.	No.
Germany	Estimates excluding capital gains. West Germany prior to 1990, with families as tax units. Data available every third year.	Linear interpolation from triannual data since 1961.
Ireland	Estimates excluding capital gains. Up to 1973, estimates based on surtax return. Since 1975, estimates based on income tax returns.	Constant level of Top 10, 1, and 1-0.5 for 1960-1974. For Top 0.5 constant level for 1960-1964, linear interpolation for 1967-1972 and 1974.
Italy	Estimates do not include most capital gains and several components of capital incomes (as interest income).	Constant level for 1960-1973, linear interpolation in 1996 and 1997.
Japan	Estimates excluding capital gains. Fractiles defined by total income excluding capital gains. Tax unit is individual.	No.
Netherlands	Estimates excluding capital gains. Until 1975 estimates based on tabulated data produced by the Central Bureau of Statistics. Since 1977, estimated based on micro-data Income Panel Survey (IPO) using tax and other administrative data. There is a break in the series in 2000/2001, due to changes of definitions and observations that were made to Dutch income statistics by Statistics Netherlands. Data every second or fourth year from 1960 to 1989. Tax unit is family.	Linear interpolation from 1961 to 1989.
New Zealand	Tax unit is individual. Estimates including capital gains when taxable.	No.

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Norway	Tax unit is individual. Estimates including capital gains.	No.
Portugal	Estimates excluding most capital gains.	Constant level for 2006-2010. For Top 10, 1, 0.5, 10-5, and 5-1, 0.5-0.1 constant level for 1960-1975 and interpolations for 1983-1988. For Top 0.1 interpolations for 1979-1988.
Spain	Estimates excluding capital gains. Tax unit is individual.	Constant level for 1960-1980.
Sweden	Estimates excluding capital gains. Tax unit is individual.	No.
Switzerland	Estimates excluding capital gains. Tax unit is family.	Linear interpolation from biannual data.
United Kingdom	Capital gains included when taxable. Until 1974, estimates relate to income net of certain deductions; from 1975, estimates relate to total income. Until 1989 original estimates relate to tax units (married couples and single adults), while, from 1990, original estimates relate to adults.	Linear interpolations in 1961, 1980 and 2008. For Top 0.1 and 0.5-0.1 interpolation for 1987-1992.
United States	Estimates excluding capital gains. Tax unit is family.	No.

Note: See the original source for further details (World Wealth and Income Database, accessed on 04/02/2016: http://www.wid.world/). We use linear interpolations since the synthetic control method relies on a full matrix of pre-treatment observations and missing observations can disturb the computations. When we do not use the synthetic control method, the original data are used.

Table A3: Groups of income share for which country-data are available (1960-2010)

Country	Top 10	Top 1	Top 0.5	Top 0.1	Top 10-5	Top 5-1	Top 1-0.5	Top 0.5-0.1
Australia	x	x	x	x		x	x	x
Canada	x	x	x	x	x	x	x	x
Denmark	x	x	x	x	x	x	x	x
Finland		x						
France	x	x	x	x	x	x	x	x
Germany	x	x	x	x	x	x	x	x
Ireland	x	x	x				x	
Italy	x	x	x	x	x	x	x	x
Japan	x	x	x	x	x	x	x	x
Netherlands	x	x	x		x	x	x	
New Zealand	x	x	x		x	x	x	
Norway	x	x	x	x	x	x	x	x
Portugal	x	x	x	x	x	x	x	x
Spain	x	x	x	x	x	x	x	x
Sweden	x	x	x	x	x	x	x	x
Switzerland	x	x	x	x	x	x	x	x
United Kingdom	x	x	x	x	x	x	x	x
United States	x	x	x	x	x	x	x	x

A3. Control variables description

Table A4: Description of control variables

Variable	Description	Source
MTR^{top}	Top marginal tax rate on personal income.	Piketty et al. (2014).
GDP per-capita	Value of all final goods and services produced within a country in a given year, divided by the average population for the same year.	The Maddison-Project, 2013 version.
ARP and MRP	Average (marginal) rate progression up to an income level equivalent to four times the GDP per-capita. The ATR/MTR used adjusts for allowances/deductions, tax credits, significant local taxes and other main rules of tax code. It does not adjust for deductions, exemptions, and credits that depend on taxpayer specific characteristics.	Authors' computation and Andrew Young School of Policy Studies (2010)
Party Orientation	It classifies into four categories: right (=0), center (=0.5), left (=1), and no information or no executive (=missing value). The classification is made using the following criteria. Right: for parties that are defined as conservative, Christian democratic, or right-wing; left: for parties that are defined as communist, socialist, social democratic, or left-wing; center: for parties that are defined as centrist or when party position can best described as centrist (e.g. party advocates strenghtening private enterprise in a social-liberal context); no information or no executive for all those cases which do not fit into the above-mentioned category or no information.	Keefer (2012)
International Trade	It is the sum of exports and imports of goods and services as a share of GDP.	Roine et al. (2009)
Globalization	2016 KOF Index of Globalization. It accounts for economic (e.g. trade, FDI, tariff rate), social (e.g. telephone traffic, internet users, number of Ikea), and political (e.g. embassies in country, international treaties) variables for defining globalization. See the source for further details.	Dreher (2006) with updated estimations.
Debt Growth Rate	Growth rate of total (domestic plus external) gross central government debt as a share of GDP	Reinhart and Rogoff (2010)
Central Government Spending	Central government spending as a share of GDP.	Roine et al. (2009)
Gross Savings	Gross savings are calculated as gross national income less total consumption, plus net transfers.	World Bank.
Gross Fixed Capital	Gross fixed capital formation (formerly gross domestic fixed investment) includes land improvements (fences, ditches, drains, and so on); plant, machinery, and equipment.	World Bank.
Bank Deposits	Bank deposits as a share of GDP	Roine et al. (2009)
Stock Market	Stock market capitalization as a share of GDP.	Roine et al. (2009)
Financial Development	Equal to the sum of the variables Bank Deposits and Stock Market	Roine et al. (2009)

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UK Legal Origin	Dummy variable equal to 1 whether the country official language is English; 0 otherwise.	Laporta et al. (1997)
Population Growth Rate	Percentage growth in country population	OECD
Working Age Population	Working age population (defined as those aged 15 to 64) as a percentage of population.	OECD
Annual Hours Worked	Average annual hours worked by persons engaged.	Penn World Table (2015)
Trade Union Density	Trade union density as a share of employees.	OECD
Patents Growth	Annual growth rate in the recorded patents.	Roine et al. (2009)
High-Education	Percentage of population 25+ that have completed tertiary schooling (5-years average)	Barro and Lee (2012)
Enrollment Ratio	Share of students enrolled in tertiary education.	UNESCO
Human Capital	Index of human capital per person, based on years of schooling (Barro/Lee, 2012) and returns of education (Psacharopoulos, 1994).	Barro and Lee (2012) and Psacharopoulos (1994)
Tax Revenue	Tax revenue as a share of GDP.	OECD

A4. Omega values for reforms

Estimation of omega values relies on the procedure performed to estimate the ARP, which is computed as:

$$ARP_{i,t} = \frac{ATR_{i,t}^{top} - ATR_{i,t}^{avg}}{(Y_{i,t}^{top}/Y_{i,t}^{avg}) - 1}. \tag{11}$$

It should be noted that changes in ARP can occur for two reasons: tax rate changes or income variations. Changes in the tax rate faced by the top and the average earner will produce a variation in the numerator of the ARP formula. All else equal, if both tax liabilities change by the same percentage, then the difference between ATR^{top} and ATR^{avg} increases will change, thus affecting the estimated ARP. Moreover, changes in income indirectly affect the tax liability. Assuming constant tax rates, it follows that the ARP will decrease (increase) if incomes of both top earner (Y^{top}) and average earner (Y^{avg}) increase (decrease) by the same percentage. Since our aim is to identify changes in progressivity due to variations in tax schedules, it becomes crucial to distinguish whether the variation in the ARP is driven by tax rate or income changes. To account for this issue, we select only the reform in which we find a significant variation in tax rates, and we disregard changes in Ω driven by annual income fluctuations. In the Table A5 below, we provide information on the changes in the ATR and in the number of tax brackets.

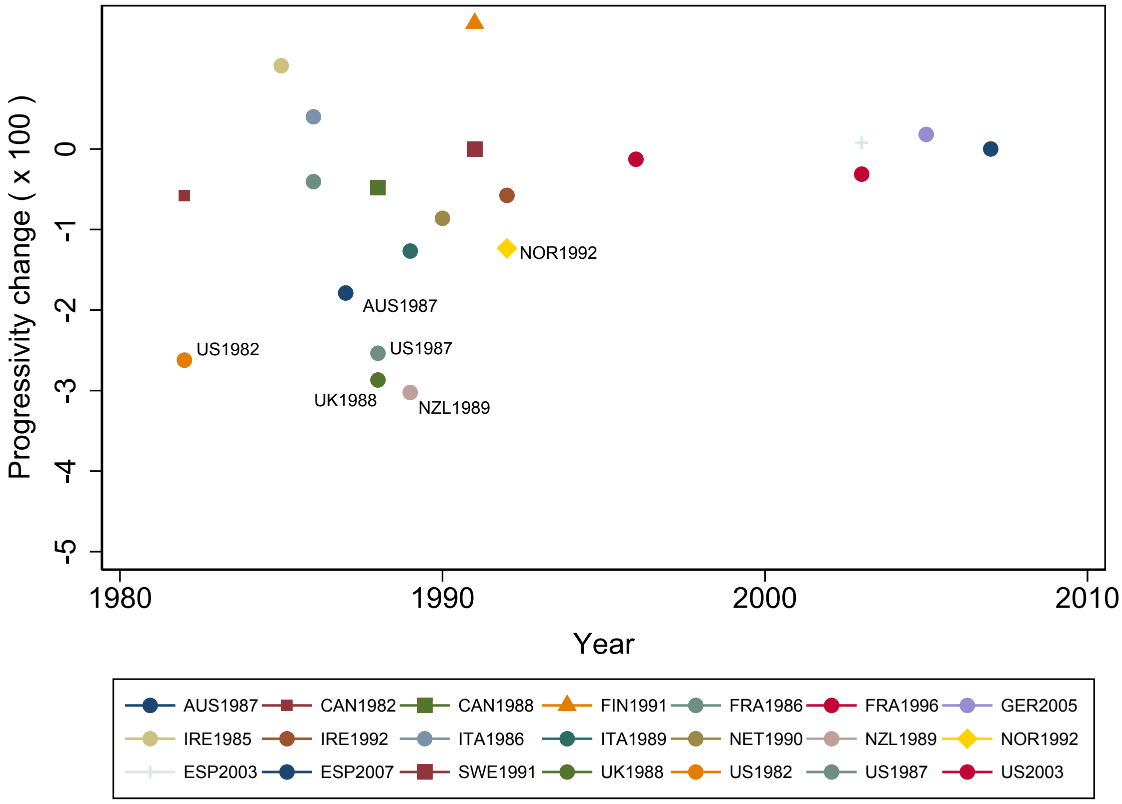
We compute the marginal rate progression (MRP) in a similar manner, where we use marginal rates instead of the average tax rate faced by a top income and an average income taxpayer. Figure A1 shows the values of Ω estimated by using the MRP instead of the baseline ARP. The reforms of interest are still those with the most dramatic reduction in tax progressivity. However, the magnitude seems to be somewhat lower than when we use the ARP. This is in part given by the fact that the MRP does not account for other dimensions directly affected by progressivity-reducing tax reforms, such as variations in the number of brackets and the income thresholds at which the marginal rates applied or amount of tax deductions and tax credits (which are included in the ARP).

Table A5: Omega values

Country	Reform Years	t=1	ΔATR^{top}	ΔATR^{avg}	Δ Brackets	Ω
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Australia	1985-1987	1987	$55.45 \rightarrow 47.03$	$33.26 \rightarrow 29.24$	$7 \rightarrow 5$	-0.02628
Canada (I)	1982	1982	$56.91 \rightarrow 51.50$	$42.16 \rightarrow 39.36$	$13 \rightarrow 10$	-0.00578
Canada (II)	1987-1988	1988	$54.02 \rightarrow 44.09$	$40.87 \rightarrow 29.70$	$10 \rightarrow 3$	-0.00085
Finland*	1991	1991	$37.73 \rightarrow 31.94$	$15.76 \rightarrow 10.08$	n.a.	-0.00036
France (I)	1986-1987	1986	$59.91 \rightarrow 53.15$	$32.73 \rightarrow 30.53$	$15 \rightarrow 13$	-0.01035
France (II)	1996	1996	$51.80 \rightarrow 49.15$	$25.36 \rightarrow 23.36$	$7 \rightarrow 7$	-0.00116
Germany*	2003-2005	2005	$40.07 \rightarrow 31.78$	$21.07 \rightarrow 13.82$	n.a.	-0.00349
Ireland (I)	1985-1986	1985	$59.09 \rightarrow 52.61$	$36.04 \rightarrow 32.33$	$5 \rightarrow 3$	-0.00253
Ireland (II)	1992	1992	$49.33 \rightarrow 45.48$	$29.65 \rightarrow 28.26$	$3 \rightarrow 2$	-0.00603
Italy (I)	1986	1986	$37.85 \rightarrow 34.47$	$17.52 \rightarrow 14.84$	$9 \rightarrow 9$	-0.00297
Italy (II)	1989	1989	$35.88 \rightarrow 33.42$	$14.84 \rightarrow 13.95$	$9 \rightarrow 7$	-0.00322
Netherlands	1990	1990	$61.61 \rightarrow 52.79$	$31.59 \rightarrow 23.63$	$9 \rightarrow 3$	-0.00001
New Zealand	1986-1989	1989	$59.84 \rightarrow 31.78$	$33.38 \rightarrow 24.99$	$5 \rightarrow 3$	-0.04650
Norway	1990-1992	1992	$49.17 \rightarrow 40.06$	$33.77 \rightarrow 31.58$	$3 \rightarrow 1$	-0.02989
Spain (I)	2003-2004	2003	$41.82 \rightarrow 39.89$	$23.38 \rightarrow 21.07$	$6 \rightarrow 5$	-0.00001
Spain (II)	2007	2007	$40.19 \rightarrow 38.25$	$21.45 \rightarrow 20.27$	$5 \rightarrow 4$	-0.00144
Sweden*	1991	1991	$44.30 \rightarrow 36.20$	$15.80 \rightarrow 10.60$	$4 \rightarrow 2$	-0.00967
UK	1988	1988	$54.59 \rightarrow 38.18$	$26.29 \rightarrow 24.32$	$6 \rightarrow 2$	-0.02358
US (I)	1982	1982	$70.71 \rightarrow 53.95$	$45.88 \rightarrow 42.44$	$17 \rightarrow 14$	-0.01976
US (II)	1987-1988	1987	$52.74 \rightarrow 32.82$	$31.14 \rightarrow 27.11$	$16 \rightarrow 2$	-0.02188
US (III)	2003	2003	$42.23 \rightarrow 39.11$	$25.11 \rightarrow 23.76$	$6 \rightarrow 6$	-0.00143

Note: ΔATR^{top} and ΔATR^{avg} display respectively the pre-reform and post-reform ATR faced by an earner whose income is equal to the average reported income by the top percentile and by an earner whose income is equal to the average reported income. Δ Brackets shows the pre-reform and post-reform number of tax brackets on personal income. For countries marked with * (i.e., Finland, Germany and Sweden), the ARP is derived computing the ATR faced by an earner whose income is equal to 4 time GDP per-capita and an earner whose income is equal to GDP per-capita, since we miss data on average income of top percentile. t=1 corresponds to the reform year. In the case a country implemented a single ”significant” reform, the dummy reform corresponds to the t=1 shown in column 3 (i.e., AUS 1987, FIN 1991, GER 2005, NET 1990, NWZ 1989, NOR 1992, SWE 1991, and UK 1988.) In the case of multiple reforms, we select the most relevant in Ω terms: CAN 1982, FRA 1986, ITA 1989, IRE 1992, ESP 2007, and US 1987. We do not find any important negative variation in Ω over the period covered in Denmark (1981-2010), Japan (2000-2010), Portugal (1989-2005), and Switzerland (1981-2010).

Figure A1: Values of $\Omega_{i,t}$ using MRP



Note: Each point in this figure represents the negative variation in the MRP over the middle-top income interval after a tax reform. $\Omega_{i,t} = MRP_{i,t_1} - MRP_{i,t_0}$. It is the difference in the ARP between the post-reform period t_1 and the pre-reform period t_0 for country i. It measures the variation in structural progressivity for each important reform from 1981 to 2010 for 18 countries. A more negative value of $\Omega_{i,t}$ indicates a larger reduction in structural progressivity. For the countries marked with * (Finland, Germany, and Sweden), the MRP is derived computing the MTR faced by an earner whose income is equal to 4 time GDP per-capita and an earner whose income is equal to GDP per-capita. Data for MTRs are calculated from OECD Tax Database schedules. Top income series are from WID source. GDP per-capita is from The Maddison-Project.

Appendix B: Synthetic Control Method estimations

Tables B1-B3 below provide additional information on the SCM estimation. Specifically, they show the weight assigned by the SCM to construct the synthetic control and the pre-treatment trends for the control variable used in the baseline model (i.e., GDP per-capita, globalization, annual hours worked, human capital, financial development, trade union density, MTR^{top}, ARP, and debt growth rate). The tables also show the root mean square prediction error (RMSPE), which evaluates how the synthetic control succeed to be similar to the treated country. The RMSPE computes the lack of fit between the path of top income shares in the treated country and in its synthetic counterpart. It is defined as:

$$RMSPE = [\frac{1}{T_O}\sum_{t=1}^{T_O}(Y_{1,t} - \sum_{j=2}^{J} w_j^* Y_{j,t})^2]^{\frac{1}{2}} \tag{12}$$

where the notation is the same as in Section 4.

Table B1: Australia: synthetic control composition

a. Synthetic control groups composition

Country	Weights for:					
	Top 10	Top 1	Top 0.1	Top 5-1	Top 1-0.5	Top 0.5-0.1
	(1)	(2)	(3)	(4)	(5)	(6)
Canada						
Denmark						
Finland	-		-	-	-	-
France						
Germany				0.379		
Ireland	0.289	0.144	-	-	0.143	-
Italy						
Japan						0.126
Netherlands		0.093	-	0.128	0.097	-
Portugal						
Spain			0.162			
Sweden	0.711	0.763	0.838	0.493	0.76	0.874
Switzerland						

b. Control variables

Variable	AUS	Synthetic AUS in:					
		Top 10	Top 1	Top 0.1	Top 5-1	Top 1-0.5	Top 0.5-0.1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GDP Per-Capita	12007	10866	11669	11635	11977	11672	12197
Globalization	64.85	69.8	70.26	67.08	64.43	70.28	66.52
Financial Development	0.95	0.68	0.68	0.67	0.68	0.68	0.68
Annual Hours Worked	1843	1886	1922	1848	1966	1924	1877
Trade Union Density	47.79	65.83	65.78	62.74	53.15	65.67	67.42
Human Capital	3.02	3	3.04	3.12	2.64	3.04	3.1
Top MTR	0.65	0.75	0.75	0.71	0.67	0.75	0.76
Debt Growth Rate	-0.04	0.03	0.03	0.03	0.03	0.03	0.04
ARP	0.10	0.14	0.14	0.13	0.12	0.14	0.13
RMSPE		0.89	0.23	0.07	0.35	0.1	0.11
Top income share AUS		27.34	5.84	1.27	11.24	2.16	2.42
Top income share Synthetic AUS		27.84	5.89	1.28	11.35	2.18	2.43

Note: Panel a shows the weights assigned to each country in the donor pool to compose the synthetic control. The synthetic control is chosen to best fit each different top income trend and control variables before the reform. Panel b shows the value of the control variables used in the baseline model to find the optimal synthetic control group in the pre-reform period.

Table B2: New Zealand: synthetic control composition

a) Synthetic control groups composition

Country	Weights for:					
	Top 10	Top 1	Top 0.5	Top 10-5	Top 5-1	Top 1-0.5
	(1)	(2)	(3)	(4)	(5)	(6)
Canada	0.048			0.179	0.097	
Denmark	0.52	0.108		0.108	0.369	0.154
Finland	-		-	-	-	-
France	0.124				0.263	
Germany					0.213	0.265
Ireland	0.286	0.344		-	-	0.185
Italy		0.03	0.234	0.062		
Japan						
Netherlands				0.115		
Portugal	0.022	0.389	0.182		0.019	0.152
Spain			0.169	0.335		
Sweden		0.129	0.414	0.201	0.036	0.244
Switzerland					0.004	

b. Control variables

Variable	NZL	Synthetic NZL in:					
		Top 10	Top 1	Top 0.5	Top 10-5	Top 5-1	Top 1-0.5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GDP Per-Capita	11889	11337	8305	10260	11033	12760	10647
Globalization	59.98	69.88	63.17	60.86	65.56	66.02	64.22
Financial Development	0.56	0.58	0.7	0.73	0.84	0.62	0.66
Annual Hours Worked	2062	1893	1872	1842	1899	1877	1889
Trade Union Density	59.04	54.56	57.28	52.11	38.71	44.07	55.22
Human Capital	2.11	2.64	2.61	2.73	2.8	2.58	2.55
Top MTR	0.59	0.63	0.66	0.66	0.61	0.61	0.65
Debt Growth Rate	0.01	0.05	0.02	0.03	0.02	0.04	0.03
ARP	0.09	0.1	0.1	0.09	0.09	0.09	0.11
RMSPE		0.7	0.41	0.32	0.27	0.3	0.13
Top income share NZL		30.11	6.47	4.09	11.27	12.38	2.38
Top income share Synthetic NZL		30.11	6.47	4.11	11.25	12.36	2.39

Note: See Table B1 for details.

Table B3: Norway: synthetic control composition

a. Synthetic control groups composition

Country	Weights for:					
	Top 10	Top 1	Top 0.1	Top 10-5	Top 1-0.5	Top 0.5-0.1
	(1)	(2)	(3)	(4)	(5)	(6)
Canada			0.258			
Denmark	0.606			0.398	0.095	
Finland	-		-	-		-
France						
Germany						
Ireland		0.148	-	-	0.093	-
Italy						
Japan						
Netherlands			-			-
Portugal						
Spain						0.029
Sweden	0.394	0.852	0.742	0.602	0.812	0.971
Switzerland						

b. Control variables

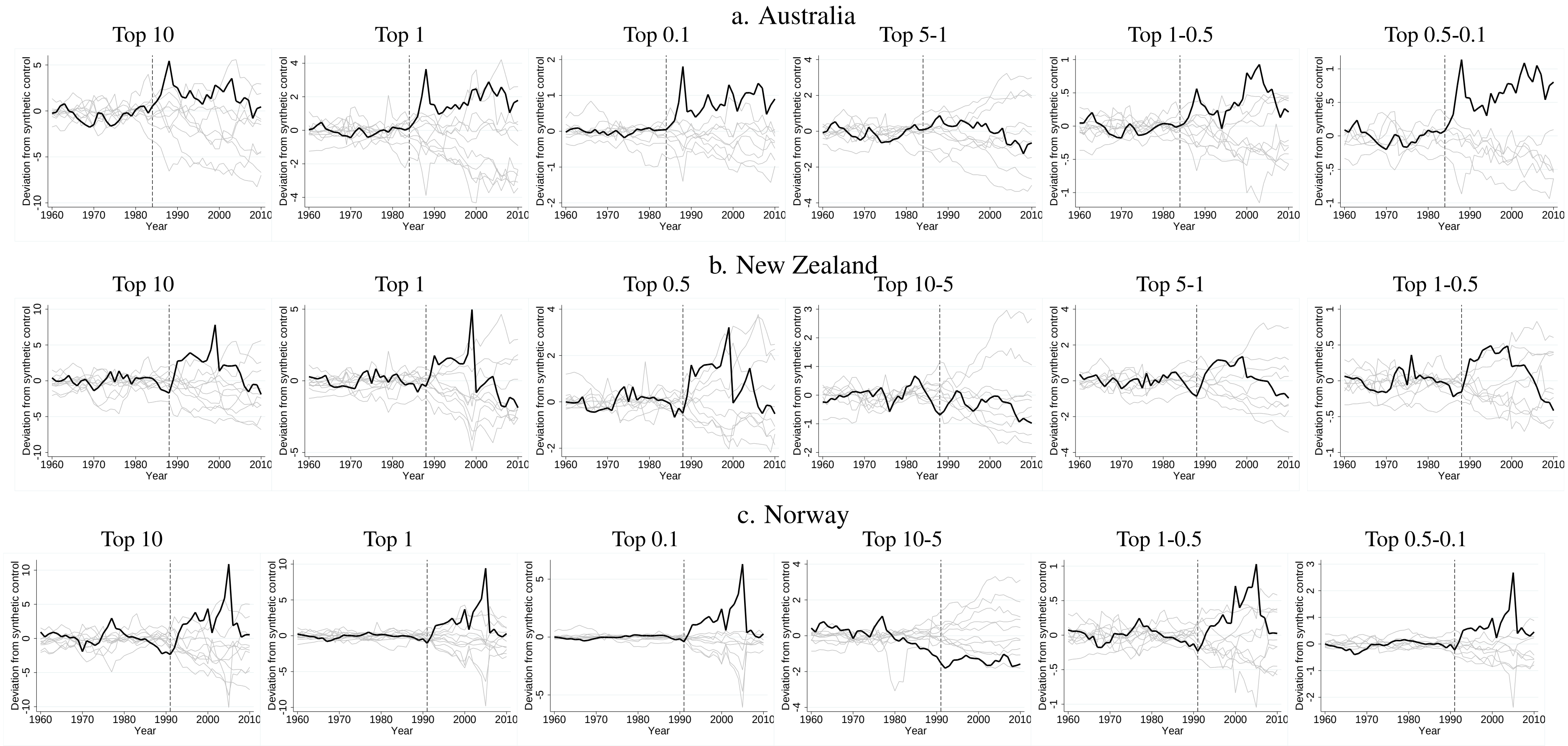
Variable	NOR	Synthetic NOR in:					
		Top 10	Top 1	Top 0.1	Top 10-5	Top 1-0.5	Top 0.5-0.1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GDP Per-Capita	12654	13792	13675	13285	13678	13012	13368
Globalization	70.22	72.54	72.82	72.31	74.12	72.79	72.57
Financial Development	0.67	0.55	0.61	0.69	0.84	0.67	0.69
Annual Hours Worked	2141	1864	1863	1836	1856	1851	1837
Trade Union Density	56.88	70.59	71.43	71.7	63.45	71.47	72.56
Human Capital	2.71	2.95	3.06	3.21	3.13	3.11	3.22
Top MTR	0.69	0.66	0.7	0.75	0.73	0.74	0.76
Debt Growth Rate	0	0.05	0.03	0.02	0.02	0.02	0.02
ARP	0.1	0.11	0.12	0.12	0.11	0.12	0.12
RMSPE		1.09	0.3	0.55	1.09	0.1	0.15
Top income share NOR		27.8	16.94	3.28	10.93	2.05	2.16
Top income share Synthetic NOR		27.75	16.96	3.36	10.89	2.05	2.21

Note: See Table B1 for details.

Appendix C: Robustness analysis of main results

C1. ”In-space” placebo test

Figure C1: ”In-space” placebo test



Note: The black solid line represents the deviation of the treated country from its synthetic control group. Each of the grey line represents a gap between the true outcome and the synthetic control group for every country in the donor pool (for which no deviation is expected). Countries with bad pre-treatment fit are ruled out as suggested by Abadie et al. (2010).

Table C1: Statistical significance according to ”in space” placebo test, full version

	Australia, top income shares:						New Zealand, top income shares:						Norway, top income shares:					
	10	1	0.1	5-1	1-0.5	0.5-0.1	10	1	0.5	10-5	5-1	1-0.5	10	1	0.1	10-5	1-0.5	0.5-0.1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
1985	0.27	0.18	0.11	0.4	0.27	0.14												
1986	0.09	0.18	0.11	0.3	0.09	< 0.01												
1987	< 0.01	< 0.01	< 0.01	0.1	0.09	< 0.01												
1988	< 0.01	< 0.01	< 0.01	0.2	< 0.01	< 0.01												
1989	< 0.01	< 0.01	< 0.01	0.3	0.09	< 0.01	0.18	0.27	0.1	0.8	0.67	0.2						
1990	< 0.01	< 0.01	< 0.01	0.3	0.09	< 0.01	< 0.01	< 0.01	< 0.01	0.9	0.22	< 0.01						
1991	< 0.01	< 0.01	< 0.01	0.3	0.09	< 0.01	< 0.01	< 0.01	< 0.01	0.5	< 0.01	< 0.01						
1992	0.09	< 0.01	< 0.01	0.3	0.18	< 0.01	< 0.01	< 0.01	< 0.01	0.1	< 0.01	0.1	0.73	0.33	< 0.01	0.91	0.45	0.11
1993	< 0.01	< 0.01	< 0.01	0.1	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	0.4	< 0.01	< 0.01	0.09	< 0.01	< 0.01	0.91	0.27	< 0.01
1994	0.09	< 0.01	< 0.01	0.3	0.18	< 0.01	< 0.01	< 0.01	< 0.01	0.5	< 0.01	< 0.01	0.09	< 0.01	< 0.01	0.91	0.18	< 0.01
1995	0.18	< 0.01	< 0.01	0.3	0.09	< 0.01	< 0.01	0.09	< 0.01	0.6	< 0.01	< 0.01	0.09	< 0.01	< 0.01	0.91	0.18	< 0.01
1996	0.18	0.09	< 0.01	0.3	0.18	< 0.01	0.09	0.09	< 0.01	0.7	0.22	0.1	0.09	< 0.01	< 0.01	0.91	0.18	< 0.01
1997	0.09	0.09	< 0.01	0.3	< 0.01	< 0.01	< 0.01	0.09	< 0.01	0.5	0.22	0.1	< 0.01	< 0.01	< 0.01	0.91	0.18	< 0.01
1998	0.09	0.18	< 0.01	0.3	< 0.01	< 0.01	< 0.01	0.09	< 0.01	0.4	0.22	0.1	< 0.01	0.17	< 0.01	0.91	0.18	< 0.01
1999	< 0.01	0.09	< 0.01	0.3	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	0.6	0.11	< 0.01	< 0.01	0.17	< 0.01	0.91	0.18	< 0.01
2000	< 0.01	0.09	< 0.01	0.3	< 0.01	< 0.01	0.18	0.18	0.3	0.4	0.27	0.2	< 0.01	0.08	< 0.01	0.82	< 0.01	< 0.01
2001	0.09	0.09	< 0.01	0.3	< 0.01	< 0.01	0.09	0.27	0.3	0.3	0.27	0.2	0.09	0.17	< 0.01	0.82	0.09	0.11
2002	0.09	0.09	< 0.01	0.3	< 0.01	< 0.01	0.09	0.27	0.2	0.3	0.27	0.3	0.09	0.17	< 0.01	0.82	< 0.01	< 0.01
2003	0.09	< 0.01	< 0.01	0.3	< 0.01	< 0.01	0.09	0.27	0.1	0.4	0.36	0.3	0.09	< 0.01	< 0.01	0.82	< 0.01	< 0.01
2004	0.09	0.18	< 0.01	0.4	< 0.01	< 0.01	0.09	0.18	< 0.01	0.4	0.36	0.3	0.09	< 0.01	< 0.01	0.82	< 0.01	< 0.01
2005	0.18	0.09	< 0.01	0.5	< 0.01	< 0.01	0.18	0.36	0.1	0.5	0.36	0.3	< 0.01	< 0.01	< 0.01	0.82	< 0.01	< 0.01
2006	0.09	0.09	< 0.01	0.5	< 0.01	< 0.01	0.27	0.45	0.2	0.5	0.36	0.5	0.09	0.27	0.11	0.82	0.27	0.11
2007	0.27	0.09	< 0.01	0.5	0.27	< 0.01	0.27	0.54	0.3	0.6	0.36	0.6	0.09	0.27	< 0.01	0.82	0.27	< 0.01
2008	0.36	0.09	< 0.01	0.6	0.36	< 0.01	0.27	0.36	0.2	0.7	0.36	0.7	0.18	0.27	0.22	0.82	0.36	0.11
2009	0.27	0.09	< 0.01	0.6	0.36	< 0.01	0.27	0.36	0.2	0.7	0.36	0.6	0.18	0.42	0.33	0.82	0.36	0.11
2010	0.27	0.09	< 0.01	0.6	0.36	< 0.01	0.36	0.54	0.3	0.7	0.36	0.7	0.09	0.33	< 0.01	0.82	0.36	0.11

Note: This table shows the probability of getting a more extreme positive deviation from the synthetic control group than the deviation for the true treatment country for each post-reform year. It relates to the graphs in Figure C1, as the number represents the share of grey lines that are higher than the black line for each post-reform year. The population consists of the donor pool population for each top income share (all country with 0 or a positive weight in Tables B1-B3). Countries with bad pre-treatment fit are ruled out, as suggested by Abadie et al. (2010).

C2. Control variable test

The choice of which control variables are included in vector of controls affects the optimization problem solved by the SCM and the weights assigned to produce the synthetic control. Hence, different combinations of controls might generate different counterfactuals and, in turn, different estimations of the treatment effect. To check whether our results are sensitive to variables' selection, we test how the synthetic control changes as we vary the composition of the vector of controls. Figure 6 displays this robustness test, based on 35 different combinations of controls, arranged with three different set of years used to control for the top income trends. Tables C2 and C3 show all the different combinations of years and controls. Altogether, there are 105 different combinations and, thus, the same number of potentially different synthetic controls for each country. To limit the number of figures, the outcome of the test is shown only for the top percentile.

Overall, our baseline results do not strongly vary. However, some alternative specifications, especially for New Zealand, produce a synthetic control with a lower outcome, inflating the effect of the treatment. On the other hand, Norway is fitted very well. This may be due to the fact that the donor pool is composed of few English-speaking countries from which the synthetic control can be picked (only Canada and Ireland), while Norway has more potentially similar countries (Denmark, Finland, Netherlands, and Sweden). Interpreting this outcome conservatively, our results can be interpreted as a lower bound of the true treatment effect.

Table C2: Variables' combinations included in the vector of controls

Variable	Base	x = the variable is included in the vector of controls																																	
GDP Per-Capita	x					x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x			x			
GDP Growth Rate		x																																x	
MRP													x																				x	x	x
ARP	x	x	x	x	x	x	x	x	x	x	x	x		x	x	x	x	x	x	x	x	x	x	x	x	x	x	x		x	x	x			x
Party Orientation											x																							x	x
International Trade						x																											x	x	
Globalization	x	x	x	x	x		x	x	x	x	x	x	x	x	x	x	x	x	x		x	x	x	x	x	x	x		x		x	x			
Debt Growth Rate	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x						x	x	x	x	x	x	x		x	x	x	x			
Government Spending																x																	x		
Gross Savings					x																														x
Gross Fixed Capital				x																															x
Bank Deposits							x																											x	x
UK Legal Origin												x																					x		x
Population Growth																																		x	
Working Age Pop									x																								x		x
Annual Hours Worked	x	x	x	x	x	x	x	x			x	x	x	x	x	x	x	x	x	x	x	x	x		x	x	x	x	x	x	x	x			
Trade Union Density	x	x	x	x	x	x	x	x	x	x			x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x			
Stock Market								x																									x		
Financial Development	x	x	x	x	x	x			x	x	x	x	x	x	x	x	x	x	x	x	x	x		x	x	x	x	x	x	x					
Patents Growth Rate										x																							x		
High Education											x																							x	
Enrollment Ratio											x																						x		x
Human Capital	x	x	x	x	x	x	x	x	x			x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x			
Tax Revenue																	x		x														x		x

Note: This table shows the different combinations of the vector of controls used to estimate the synthetic control. There are 35 different combinations, arranged with three different set of years (displayed in Table 15), for a total of 105 combinations. Outcomes are shown in Figure 6.

Table C3: Years in Synthetic Control Group choice function

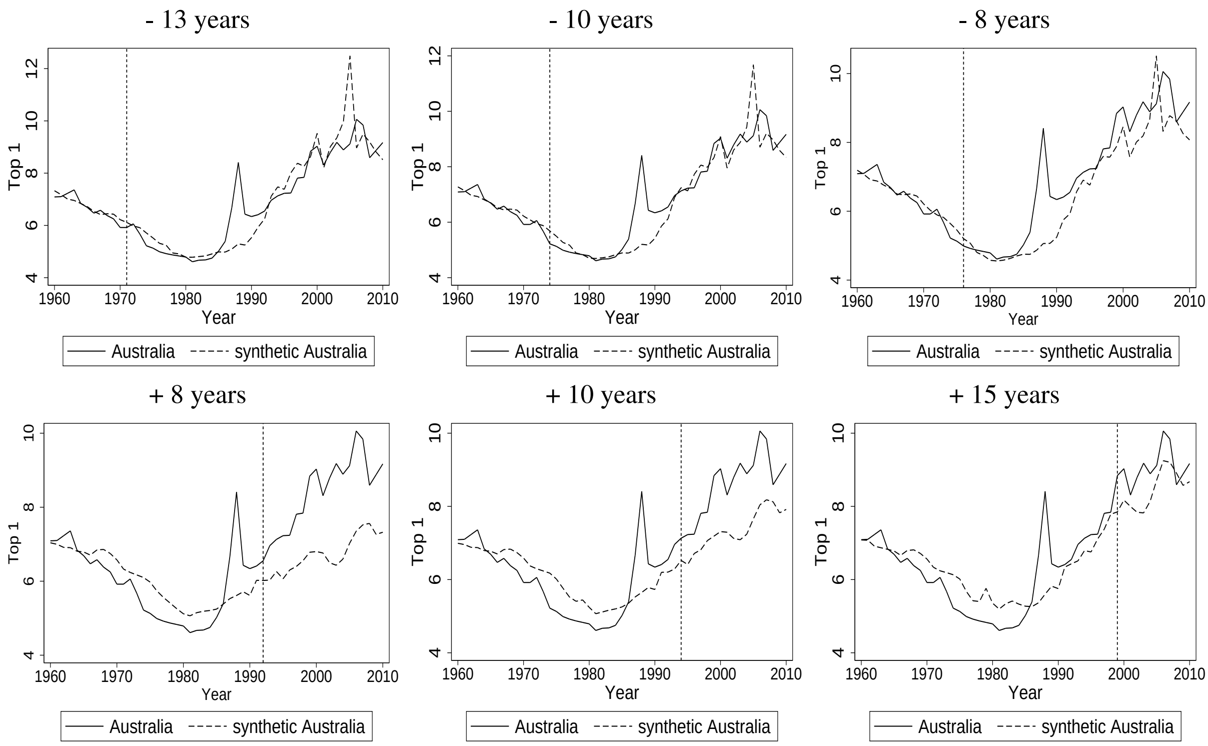
Australia			New Zealand			Norway		
Baseline	Version 2	Version 3	Baseline	Version 2	Version 3	Baseline	Version 2	Version 3
1965	1962	1967	1969	1967	1965	1965	1964	1968
1975	1972	1976	1981	1983	1982	1988	1987	1988
1983	1983	1982	1987	1987	1986	1990	1990	1989

C3. "In-time" placebo test

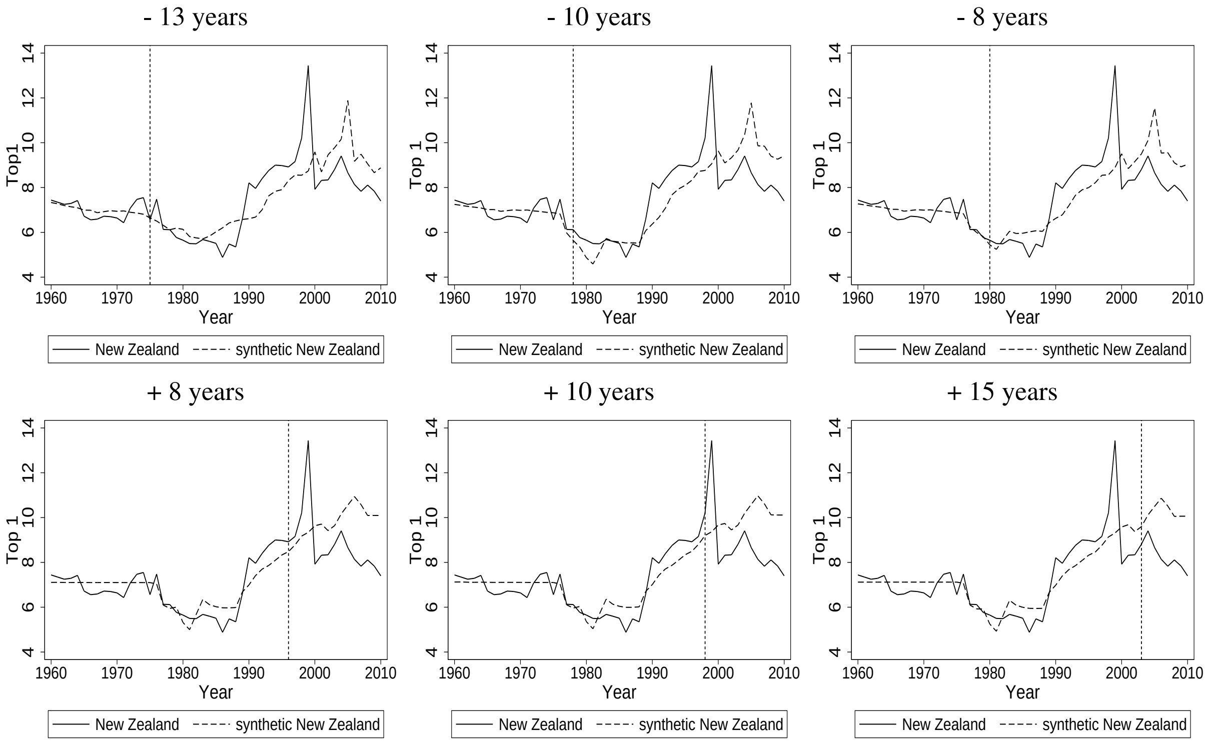
"In-time" falsification exercises are made simulating a placebo treatment some years before (8, 10 and 13 years) and after (8, 10 and 15 years) the real treatment. To limit the number of graphs, those tests are carried out only for the top percentile, since this share can be assumed as representative. The tests reinforce the robustness of results, since the effect appears only in the years after the reform. The only bad new - but however not very sizable - comes from the test carries out in Norway 13 years before the reform (in 1978). Here a negative effect is found. However, when the placebo is handed out in 1981 and 1984 (respectively 10 and 8 years before the real treatment), this troubling effect was absorbed by the synthetic control.

Figure C2: "In-time" placebo test

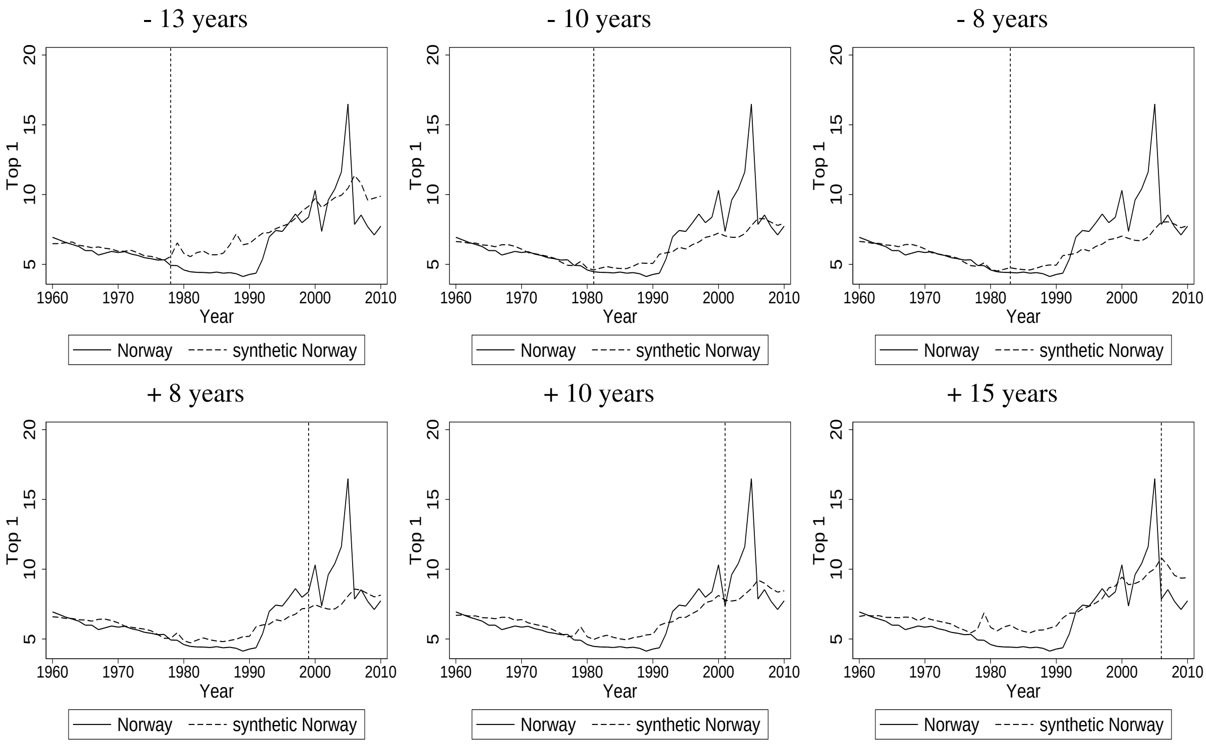
a. Australia



b. New Zealand



c. Norway



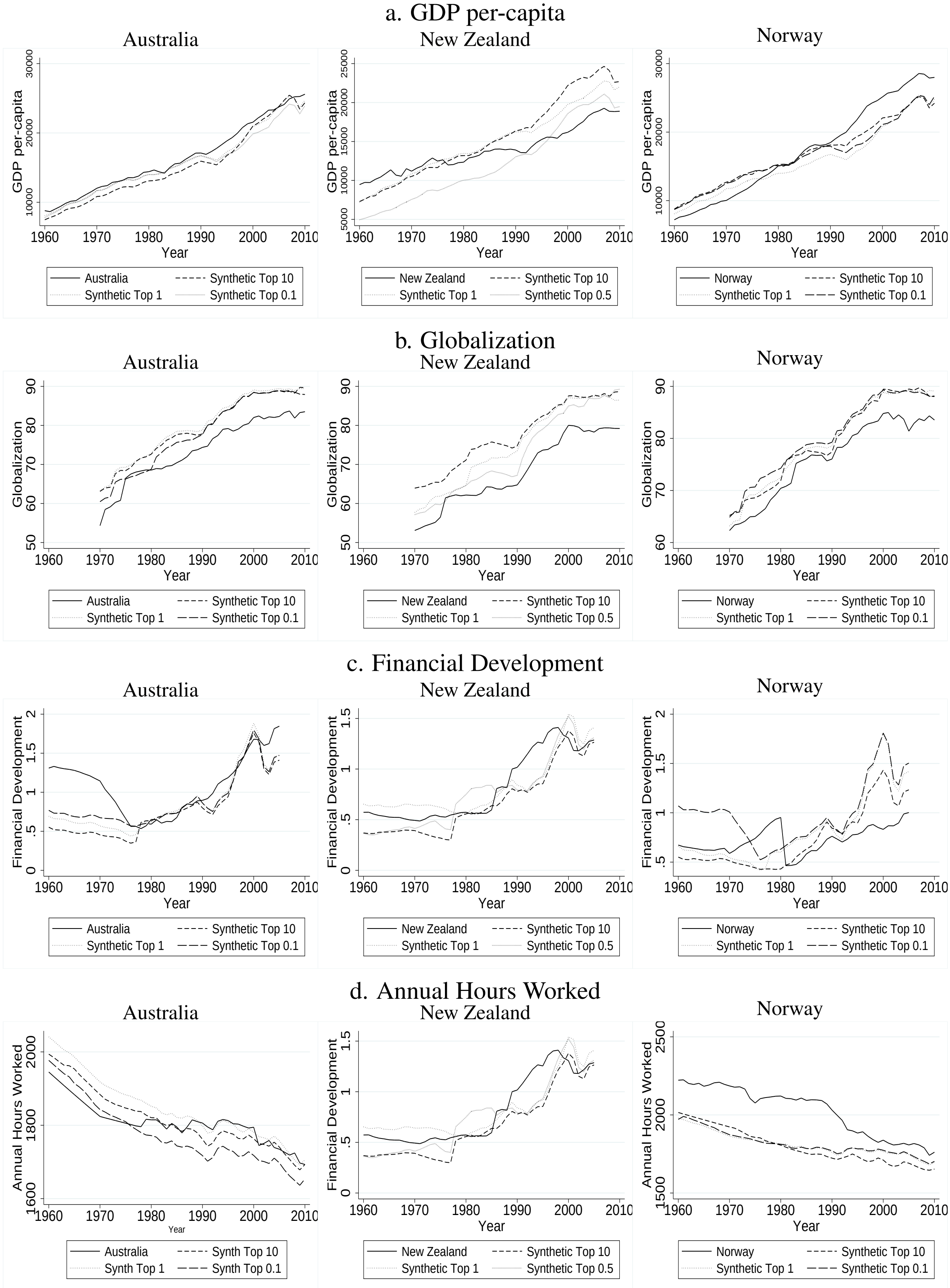
Note: This test imposes falsely timed tax reforms in the three studied countries 8, 10 and 13 years before and 8, 10 and 15 years after the actual reform.

C4. Post-treatment trends in control variables

An implicit assumption of the SCM is that trends in control variables in both treated and synthetic control are parallel after the reform. However, it is entirely possible that important economic changes happened in the years after the reform in both the synthetic control or the treated country. If this happens, the estimations would be severely biased. For this purpose, we graphically check the validity of this assumption out in the Figure C3 below.

Overall, there are no big changes. Hpwever, one potentially problematic issue is represented the fall in trade union density in New Zealand in the period just after the reform. Other deviations are observed in the variables reflecting the fiscal status of the country (ARP, debt growth rate, MTR). Reductions in such variables are, however, not problematical, since they are directly affected by the tax reform.

Figure C3: Post-treatment trends in control variables



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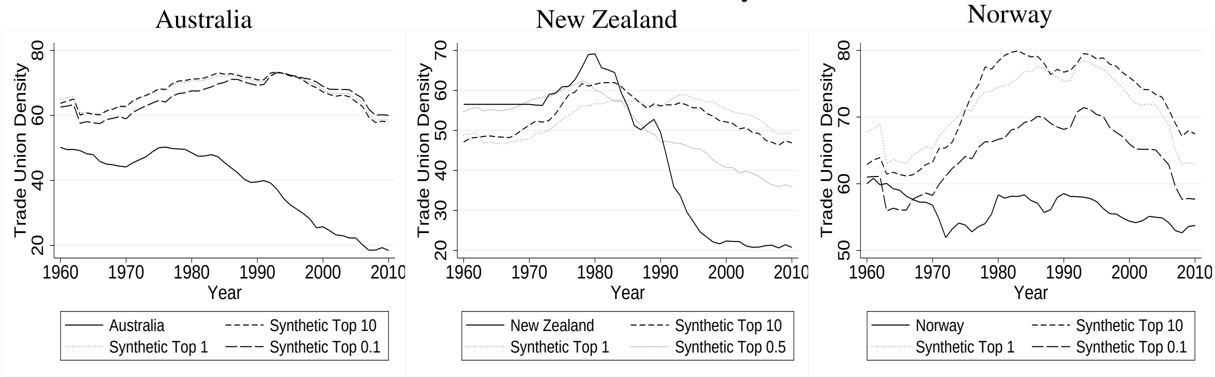
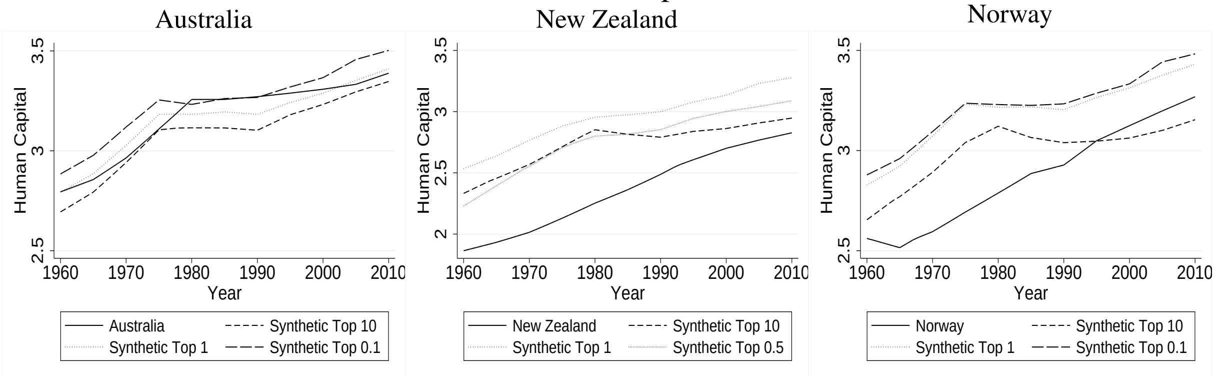
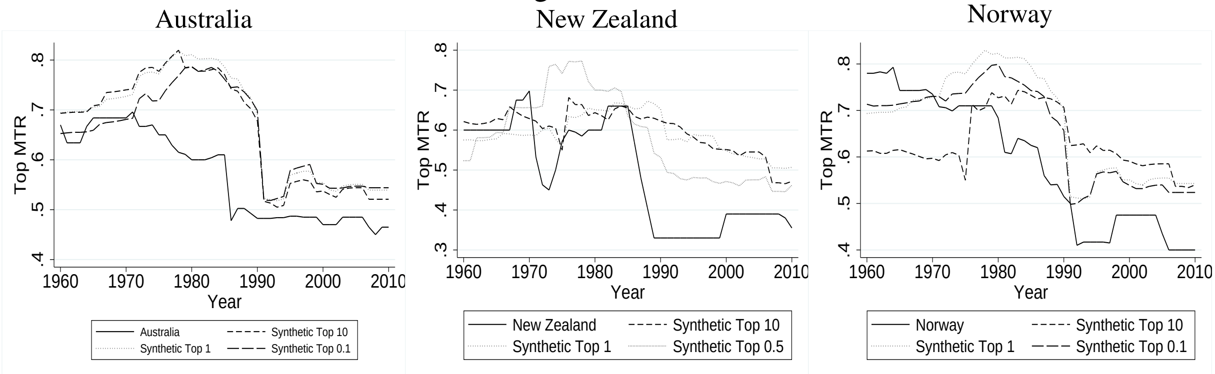
e. Trade Union Density



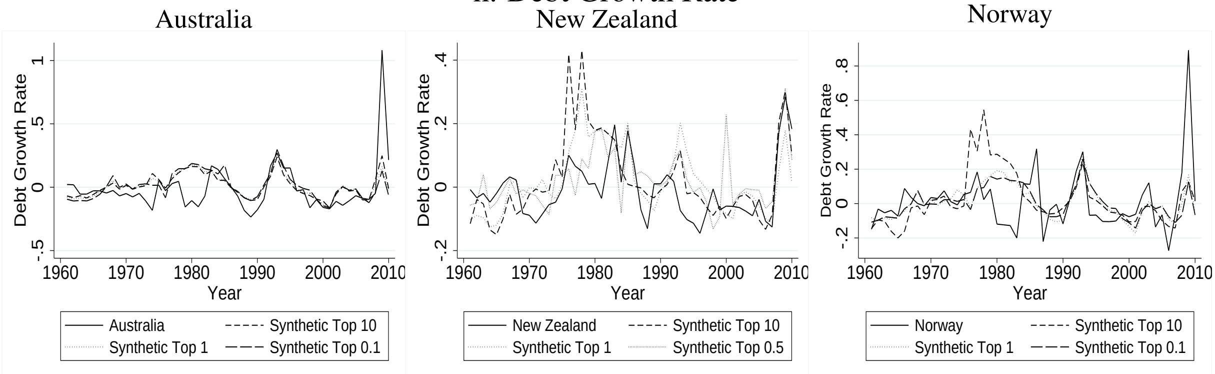
f. Human Capital



g. MTR^{top}



h. Debt Growth Rate



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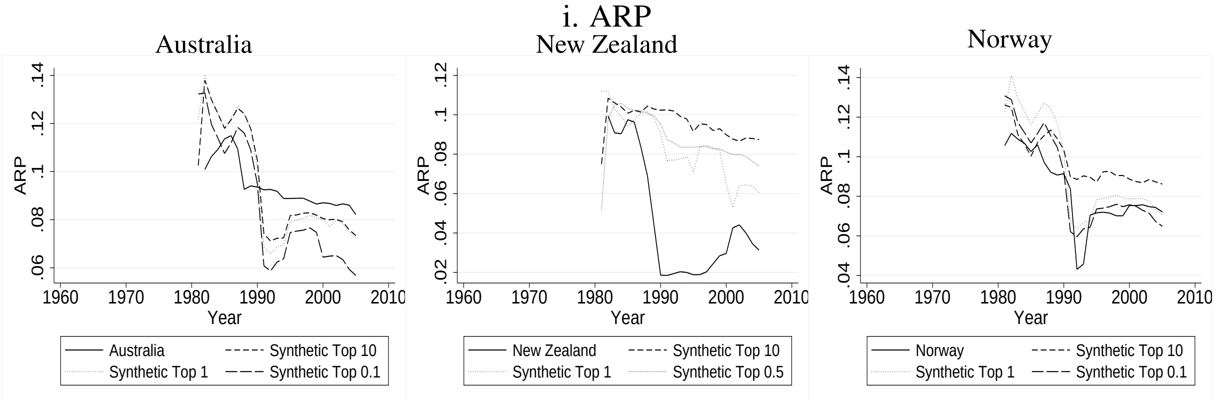
i. ARP



C5. Effect of using a sub-donor pool which includes realized capital gains

Theoretically, capital gains, both realized and unrealized, are undoubtedly a source of income in the classic Haig-Simons definition. However, they represent a highly complicate income component to include in individual's income (Roine and Waldenström, 2015). Only when they are realized, they become observable and, then, appear on tax returns, making difficult to allocate them properly in time. Exclusion and inclusion of capital gains can change the top income series, especially in the very top groups where they are more concentrated.

Table A2 shows that capital gains are not included in each country data. Problems with observing and accurately dating capital gains have led many researchers to exclude the realized capital gains from their top income series. For some countries WID presents both series including and excluding realized capital gains. Separate series, among the countries included in the donor pool, can be found in Canada, Germany, Japan, Spain, and Sweden. The treated countries present only series where capital gains are included in the tax base. To test whether capital gains' treatment in estimation of top income shares affect our results, we restrict the donor pool to the countries whose series included capital gains.

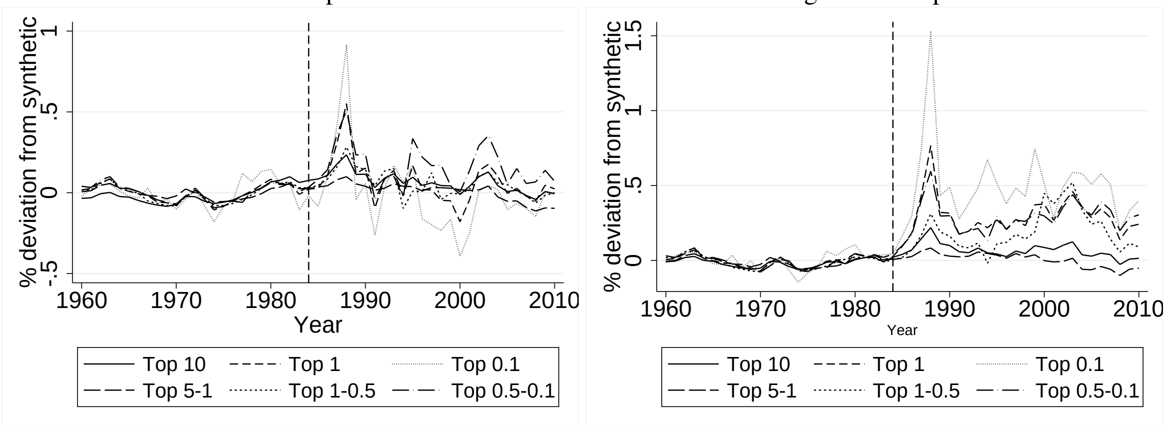
Figure C4 compares the baseline reform effects (right-hand graph) with those estimated using a counterfactual generated from the restricted donor pool. The results show that top income shares are lower in the latter case, suggesting that realized capital gains are an important driver of the spike observed after the reform and that using the original donor pool might upward bias increases the actual reform effect. However, it should be noted that the sub-donor pool is composed of only 5 countries; this leads to generate a worse and less efficient synthetic control. Indeed, the pre-treatment trends are better reproduced by the original donor pool.

Relate to the quality of top incomes' series, we take in consideration the fact that big tax reforms - as those under examination - make several changes on how the tax base is computed and, thus, might make taxable income not entirely comparable over time. Burkhauser et al. (2015) discuss this possibility for Australia, providing adjusted estimations of the top percentile from 1970 to 2010, which accounts for the change on the treatment of company profits and dividends. Figure C5 displays the effect of the reform on top percentile and the SCM result using Burkhauser et al. (2015)'s data. Using these corrected data, the surge in top income shares is still notable, even if weaker than the original estimation made using WID top income series. Taken together, Figure C4 and C5 suggest that the original estimations may somewhat inflate the reform effect, but the sign and the significance of our results remain still valid.

Figure C4: The effect of realized capital gains

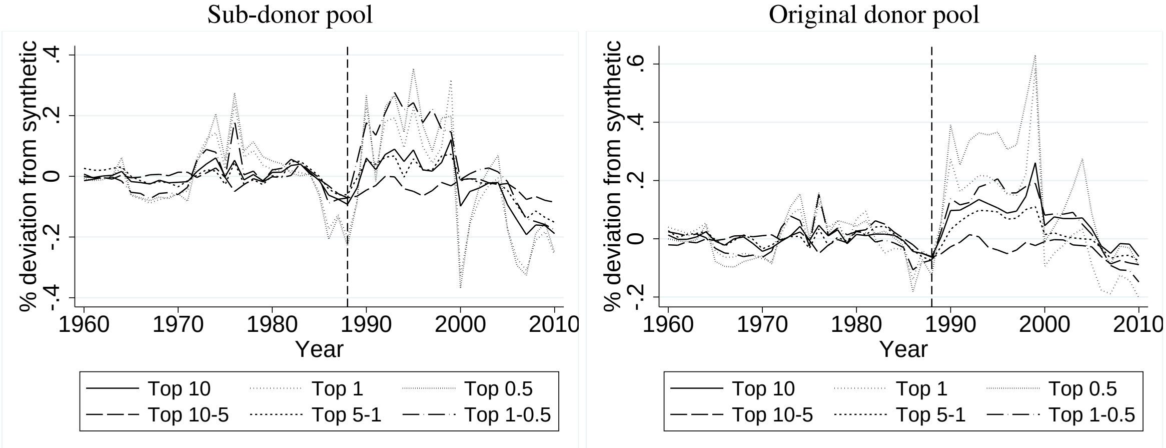
a. Australia

Sub-donor pool Original donor pool



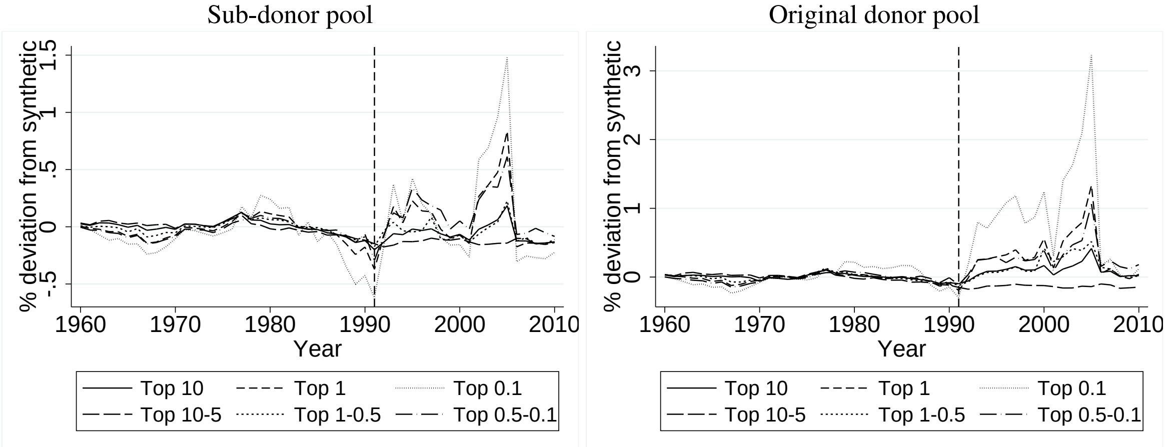
b. New Zealand

Sub-donor pool Original donor pool



c. Norway

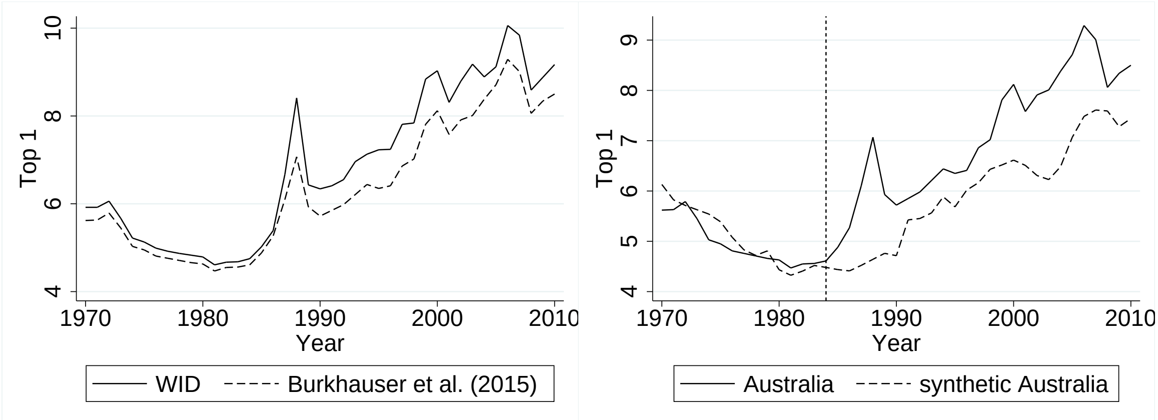
Sub-donor pool Original donor pool



Note: This figure shows the percentage deviations from the synthetic control group for each top income share. Right-hand graphs show the baseline estimation, while left-hand figures provide estimates using a synthetic control generated from a restricted donor pool containing only the countries having separate top income series including capital gains (Canada, Germany, Japan, Spain, and Sweden).

Figure C5: Results using Burkhauser at al. (2015) corrected series for Australia

a. Australia, comparison between series   b. Australia, alternative estimation



Note: Left-hand graph compares top income series from WID and those from Burkhauser et al. (2015). Right-hand figure re-run the SCM estimations using data on top percentile from Burkhauser et al. (2015) data. Burkhauser et al. (2015, Appendix Table A3) data exclude dividend imputation credits from top income share.

Appendix D: Additional regressions

The baseline model, shown in Table 3, uses marginal measures of tax rate and tax progressivity. In Table D1, we re-run panel regressions replacing MTR and MRP with ATR and ARP and then we use both in the same regression to evaluate which effect predominates. As standard economic theory predicts, marginal effects dominates average effects. This is confirmed both using tax rate and progressivity measures.

Since the intensity in the degree of tax progressivity reduction significantly varies among the reform of interest, in Table D2, we re-run the DiD regressions shown in Table2 replacing the dummy reform with the interaction between the latter and the estimated Ω value.[22] The positive coefficient suggests that a more intense reductions in progressivity lead to larger increase in top income shares, confirmed the main argument of our analysis.

[22]For estimations, we take the absolute value of Ω.

Table D1: Marginal vs average effects

	log(Top 10-1)		log(Top 1-0.1)		log(Top 0.1)	
			a. Average vs marginal tax rate			
$\log(1 - ATR^s)$	-0.038	-0.122	0.303***	-0.044	0.848**	-0.738**
	(0.041)	(0.207)	(0.105)	(0.212)	(0.384)	(0.357)
$\log(1 - MTR^s)$		0.048		0.232*		1.403***
		(0.094)		(0.106)		(0.521)
Obs.	356	320	383	319	315	315
			b. Average vs marginal rate progression			
$\log(ARP)$	0.592	1.515*	-1.384**	0.643	-3.947***	-0.207
	(0.388)	(0.815)	(0.671)	(1.252)	(1.359)	(1.578)
$\log(MRP)$		-0.970**		-2.131**		-3.935*
		(0.415)		(0.843)		(2.050)
Obs.	377	377	376	376	327	327
Country FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Country $\times$ time trend	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES

Table D2: Intensity of the treatment

	log(Top 10-1)		log(Top 1-0.1)		log(Top 0.1)	
Reform dummy	0.026		0.080*		0.208**	
	(0.017)		(0.043)		(0.097)	
Reform dummy $\times\ \Omega$		0.040*		0.159**		0.342*
		(0.022)		(0.066)		(0.179)
Obs.	713	713	712	712	647	647
Country FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Country $\times$ time trend	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES

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