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Preface

The last decades have been marked by an increase in inequality across many dimensions. On the one hand, interpersonal inequality has increased significantly in the developed world (Atkinson, Piketty, and Saez, 2011). On the other hand, there has been a growing divide between prosperous and lagging regions (Ehrlich and Overman, 2020). This development is not only worrisome from an economic perspective, but has also led to political political backlash (Rodriguez-Pose, 2018). This thesis will investigate both potential causes of and remedies for the increase in inequality. It consists of four chapters, each of which can be read independently. The first chapter investigates both the efficiency and equity effects of a place-based policy, a common tool used by governments to reduce regional inequality. The second chapter analyzes the influence of political partisanship on tax competition and labor mobility, two important factors contributing to the rise in income inequality. The third chapter evaluates the effect of fiscal rules on distributional policies such as progressive income taxation. The fourth chapter studies the effect of disclosure laws on politicians' outside incomes, which have been discussed controversially in debates on earnings inequality. In the following, I will summarize and discuss each of the four chapters in more detail.

Chapter 1, which is joint work with Tobias Etzel and Sebastian Siegloch, evaluates a potential tool to combat the increase in regional disparities. We analyze the efficiency and equity effects of a prominent German place-based policy. After re-unification, the policy, which was mainly targeted at manufacturing firms in East Germany, subsidized up to 50% of the costs of investment projects. Exploiting quasi-experimental variation in regional subsidy rates and administrative employer-employee data, we estimate the causal direct policy effects, finding that a one-percentage-point decrease in the subsidy rate leads to a 1% decrease in manufacturing employment in the long run. Second, we test for various important spillover effects. While we do not find significant regional spillovers within commuting zones, we demonstrate significant local demand effects, as the untreated construction and retail sectors are also negatively affected by subsidy cuts. Furthermore, far-away regions that are connected with the treated regions via trade also experience a negative effect on manufacturing employment. We also show that local policy makers react to subsidy cuts by increasing the local business and property tax. Last, we derive the efficiency and equity implications of the policy. We find that the place-based policy is slightly more efficient than place-blind policies such as unemployment insurance or cash transfers by calculating the marginal value of public funds. In addition, it is significantly more effective in reducing regional inequality. We show that the efficiency and equity effects of the policy are less beneficial when ignoring spillover effects.

One potential policy tool to reduce interpersonal inequality has traditionally been progressive income taxation. Due to the rise of globalization, the mobility of high-income individuals across borders has increased substantially (Kleven et al., 2020). This puts pressure on governments to lower the progressivity of their income tax schedules. A central tenet of classical optimal taxation models is that mobile individuals react to tax differentials through migration, and in turn immobile individuals vote for lower taxes (Simula and Trannoy, 2010). In Chapter 2, which is joint work with Dirk Engelmann, Eckhard Janeba and Lydia Mechtenberg, we investigate to which extent this argument is complete. In particular, political ideology may influence voting on taxes. We vary mobility and foreign taxes in a survey experiment within the German Internet Panel. We find that while the treatment effects qualitatively confirm model predictions how voters take mobility of high-income earners into account when choosing domestic taxes, ideology matters: left-leaning high-income individuals choose higher taxes and emigrate less frequently than right-leaning ones. These findings are in line with the comparative-static predictions of a simple model of inequality aversion when the aversion parameters vary with ideology.

Besides tax competition, austerity policies have increasingly been pointed out as a reason for lower levels of income tax progressivity. Especially in the aftermath of the financial crisis, the potential harms of austerity for the poor have been hotly debated. While the efficiency aspects of austerity have been widely investigated in the literature, there is very little casual evidence on its distributional impact (Alesina, Favero, and Giavazzi, 2018). In Chapter 3, which is joint work with Matteo Alpino, Zareh Asatryan and Sebastian Blesse, we use the autonomy of Italian municipalities in setting non-linear income tax rates and the exogenous introduction of a fiscal rule to analyze the effect of austerity on distributional policies. We show that the mandated austerity actually increases local income tax progressivity. Consistent with this municipal evidence, we find in a panel of countries that austerity correlates with higher marginal tax rates on top earners, but not on average earners. We neither find evidence for reductions in total spending nor in redistributive spending. The increase in progressivity in Italian municipalities is driven by mayors having a college-degree or working in high-skill occupations, while less-educated or lower-skill mayors raise taxes uniformly. In the first post-reform election, mayors that increased progressivity have higher reelection odds. This suggests that politicians trying to ease the potential distributional implications of austerity are rewarded at the ballot box.

When economic disparities increase, the public can become increasingly sensitive to potential conflicts of interest of politicians. If voters perceive a politician to act in their own interest and not in the interest of the public, they have the option to vote the politician out of office. This democratic mechanism is similar to the political incentives discussed in Chapter 3, but a necessary precondition for its functioning is the availability of high-quality information to voters. Only an informed public can provide incentives for politicians to act in their best interest. Chapter 4 of this thesis, which is joint work with Carina Neisser, explores the consequences of providing more information about the outside activities and incomes of politicians to voters and the reaction of politicians to these changed incentives. In Germany, the outside earnings of members of parliament were not observable to voters up to 2007. Starting in July 2007, German federal members of parliament were forced by law to publish their earnings in a bracket system top-coded at 7,000€. In 2013, more brackets were introduced, such that only earnings above 250,000€ were censored, which greatly increased the information available to voters. We exploit both reforms to identify the causal effects of public disclosure rules on politicians' outside earnings using administrative tax return data. This allows us to observe politician's pre-reform income as well as enables us to use unaffected members of state parliaments as a control group in a difference-in-difference setup. Our results indicate that the top-censored nature of the reporting scheme of the first reform had the consequence of raising outside earnings, while the second reform provides evidence that a higher degree of public disclosure leads to a decrease in outside earnings. To identify potential mechanisms behind our findings, we collected the published information on earnings and activities along with political and electoral variables. We provide evidence that electoral accountability mediates the effect of public disclosure.

In summary, this thesis underlines that one has to be careful when trying to understand the causes and remedies of inequality. Place-based policies have long been dismissed as being second-best when compared to people-based policies (Glaeser and Gottlieb, 2008; Glaeser and Gottlieb, 2009). Chapter 1 shows that, when accounting for spillover effects, place-based policies can be at least as efficient as people-based policies. This makes them a viable policy option if one is concerned with the rise in regional inequality. The mobility of high-income individuals has been understood to put pressure on the progressivity of income taxation (Mirrlees, 1971; Simula and Trannoy, 2010). In Chapter 2, we show that this argument is not complete when one ignores the political preferences of voters. If a sufficient number of voters has inequality-averse preferences, we observe less migration of the rich and a higher level of taxation than predicted by the textbook argument. Austerity has largely been seen as hurting the poor disproportionately, especially in the public debate (Blyth, 2013; Mendoza, 2014; Varoufakis, 2016). In Chapter 3, we demonstrate that this is not necessarily the case using the example of a fiscal rule for Italian municipalities. The combination of a multitude of policy options, including progressive income taxation, and strong reelection motives for local politicians lead to more redistributive policies. Last, the introduction of public disclosure rules for politicians was intended to provide voters with the necessary information to potentially pressure politicians to change their behavior. Chapter 4 shows that the effects of income disclosure laws crucially depend on their exact implementation. If the disclosed information lacks precision, such that voters cannot identify top earners, public income disclosure can increase outside earnings and thereby might increase the risk of conflicts of interest. Only when the disclosed information is sufficiently precise, we observe a decline in outside earnings.

Each chapter of this thesis shows that the question asked is more complicated than one expects at first glance. It is of first-order importance to consider unintended consequences, spillover effects and political economy considerations and neglecting these issues can lead to mistaken policy conclusions.

Chapter 1

Direct, Spillover and Welfare Effects of Regional Firm Subsidies

Joint with Tobias Etzel and Sebastian Siegloch.

1.1 Introduction

In many countries and federations, place-based policies are a means to support regions that are economically lagging behind (Glaeser and Gottlieb, 2008). The European Union spent more than €350 billion – about a third of its budget – on regional policies during the funding period from 2014 to 2020 (Ehrlich and Overman, 2020). The U.S. currently devotes about $60 billion to place-based policies (Bartik, 2020; Slattery and Zidar, 2020) – mostly through business tax incentives. A recent wave of papers has demonstrated that place-based policies unfold positive economic effects on targeted regions (see Duranton and Venables, 2018; Kline and Moretti, 2014b; Neumark and Simpson, 2015, for further summaries of the literature). However, it is well-known that the overall welfare effects of place-based policies also depend on the indirect policy effects that go beyond direct effects on treated workers and firms in subsidized regions (Austin, Glaeser, and Summers, 2018). We refer to these indirect effects as spillovers throughout the chapter. Spillovers may take various forms and signs. Positive agglomeration effects in subsidized places might induce relocation effects and negative agglomeration in unsubsidized places (Kline and Moretti, 2014a). There might also be relocation of factors between treated and untreated sectors in subsidized places, shaping local multiplier effects (Moretti, 2010). An increasing concentration of educated workers may unfold positive human capital spillovers (Diamond, 2016; Glaeser and Gottlieb, 2008). Local subsidies might be capitalized into housing prices leaving real wages unchanged (Austin, Glaeser, and Summers, 2018; Busso, Gregory, and Kline, 2013). Local subsidies might also have intra-regional spillovers via trade flows (Blouri and Ehrlich, 2020). Local policymakers might respond to the (foregone) subsidies by adjusting local policy instruments (Ehrlich and Seidel, 2018). Finally, a successful local subsidy should have fiscal externalities on federal-level tax bases and social insurance systems (Austin, Glaeser, and Summers, 2018). Most of the literature has discussed the welfare effects of place-based policies using structural spatial equilibrium models and putting a special emphasis on agglomeration spillovers (Fajgelbaum and Gaubert, 2020; Gaubert, 2018; Gaubert, Kline, and Yagan, 2021; Kline, 2010; Kline and Moretti, 2014a).

In this chapter, we take a different perspective on spillover effects of place-based policies and their welfare implications. We provide cleanly identified reduced-form estimates of the direct and indirect effects of a prominent German place-based policy subsidizing investments of firms in distressed East German regions post reunification. In particular, we investigate a host of potential spillovers on other neighboring and far-away regions, untreated sectors, local housing markets and local policy instruments. We then use the reduced-form evidence and calculate the efficiency costs of the policy using the recently proposed measure of the marginal value of public funds (MVPF) (Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020), which provides an intuitive, yet comprehensive way to translate reduced-form behavioral effects into a welfare metric that accounts for additional fiscal spillovers on other sources of government budget such as the personal income tax or unemployment insurance. The MVPF framework benchmarks the welfare effects of a policy by comparing to other policies, which can be taken from the literature, such as place-blind cash transfers, or the same place-based policy ignoring spillovers. In a last step, we simulate the effects of the place-based policy on regional inequality for given efficiency cost and compare the distributional effects to related place-blind policies.

We study the case of the most prominent German place-based policy called GRW.[1] The GRW constitutes Germany's main regional policy scheme for underdeveloped regions (Deutscher Bundestag, 1997). While not exclusively targeted at East Germany, the overwhelming share of the subsidies went to the formerly socialist part of the country after reunification and it was the main regional subsidy to revitalize the East German economy after reunification. The GRW's main instrument are investment subsidies for manufacturing firms in eligible regions. These subsidies can be used for purchasing new machines or building new production sites. The explicit goal of the policy – and a criterion to qualify – is to boost investment, and thereby creating new jobs and stimulating regional growth.

We combine official data on the universe of subsidy cases with administrative social security data on firms and workers to estimate the reduce-form effects of the policy, differentiating between the direct policy effects and various spillover effects across regions, sectors and to other local policies. Our identifying variation comes from multiple reforms of the maximum subsidy rate of investment cost between 1997 and 2014. These reforms changed subsidy rates differentially across East German counties based on pre-determined economic performance. For each new policy regime, the measure of economic development is based on past performance measures, which are determined

[1] The German name of the policy is *Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur* – throughout the chapter, we will refer to it using the official German acronym GRW.

at a higher regional level. Hence, the measure is difficult to manipulate for counties and we provide evidence that selection into treatment does not seem to be a concern. Explicitly, we compare counties that are below the threshold yielding a higher subsidy rate to counties that are above. In other words, we zoom in on counties that are relatively similar in terms of income, employment dynamics and infrastructure amenities prior to treatment. Eligibility thresholds change across budgeting periods and these changes are partly triggered by EU legislation, which is exogenous to economic developments in East Germany.

We make use of the Establishment History Panel, an administrative plant-level data set, provided by the Institute for Employment Research (IAB) of the German Federal Employment Agency. For the years 1996-2017, we have access to a fifty percent random sample of plants in East Germany. The data cover the annual number of employees at the plant level as well as the county in which it is located. In addition, we rely on administrative data on individual wages included in the Sample of Integrated Labour Market Biographies (SIAB) – a representative two percent sample of German employees subject to social security contributions from 1996 to 2014. Official subsidy data from 1996 to 2016 is provided by the Federal Ministry for Economic Affairs. We have obtained the universe of GRW subsidy cases, including the county, investment volume and amount in subsidies paid. In addition, we gathered regional data to replicate the indicators determining treatment status across all budgeting periods. The main outcome of interest in our study is the effect of GRW subsidies on regional employment. Econometrically, we make use of event study designs to pin down the policy effects. However, we do not restrict our analysis to the overall effect of the policy on the treated regions, but also study the underlying mechanisms and spillovers. Explicitly, we analyze (i) intra-county sectoral spillovers by looking at non-treated industries in treated regions, (ii) cross-county regional spillovers by studying the effect on untreated counties within the same local labor market, (iii) trade spillover by looking at counties with significant trade exposure to the treatment counties, and (iv) policy spillovers by looking at the policy effect on local tax rates. We then use our reduced form estimates to infer the welfare effects of the policy. More specifically, we make use of the novel framework proposed by Hendren and Sprung-Keyser (2020) and Finkelstein and Hendren (2020) to calculate the marginal value of public fund (MVPF) – that is the "bang for the buck" – of the policy. The MVPF explicitly takes into account fiscal spillovers on other tax bases and social insurance programs (Austin, Glaeser, and Summers, 2018). Last, we simulate the policy's capacity to affect regional inequality and compare it to other place-blind transfers for given MVPF.

We derive the following three direct results for a one-percentage-point decrease of the subsidy rate in the treated manufacturing sector. First, subsidized investment decreases by 14.6% and total (i.e. subsidized plus unsubsidized) investment decreases by 6.7%. Second, in the long-run manufacturing employment decreases by 1%. We do not find asymmetric effects of subsidy cuts and increases. Third, wages are largely unaffected.

In terms of spillover effects, we derive the following results. First, a one percentage-point-decrease of the subsidy rate for the manufacturing sector leads to a 0.26% and

0.47% employment reduction in the untreated retail and construction sector, respectively. Second, there is no evidence for positive or negative spillovers of a county-level shock within the local labor market. Third, we find evidence for negative manufacturing employment responses of counties that have a higher trade exposure to treated counties. Fourth, we demonstrate important negative policy spillovers: a decrease in the subsidy leads to a long-run increase in local business and property tax rates, which can be rationalized with a fixed expenditure requirements of municipalities and a decreasing tax base.

Last, in terms of welfare implications, we derive the following three results. First, we calculate a marginal value of public funds of 0.96, which is higher than the estimates of the MVPF of unemployment insurance and cash transfers, which target a similar set of beneficiaries (Hendren and Sprung-Keyser, 2020). Second, a simple back-of-the-envelope calculation shows that the cost per job were about 24,000€. Importantly, we show that both the cost per job and the marginal value of public funds are substantially downward-biased if one does not account for spillovers. Third, given the similarity in terms of the MVPF, we show in a simulation exercise that place-based policies are more effective in reducing regional inequality for given efficiency costs compared to cash transfers since place-based policies as more regionally targeted.

We contribute to the existing and recently growing literature on place-based policies in several ways. First, we provide novel evidence on the direct, reduced-form effects of an important place-based policy. Our findings reinforce recent findings that place-based policies work – in the sense that they have a positive and long-lasting effect on the local economy. Kline and Moretti (2014a) show that the Tennessee Valley Authority, the most prominent regional subsidy program in U.S. history had a positive effect on manufacturing employment that lasted even beyond the program end due to agglomeration forces. Looking at Chinese cities, Alder, Shao, and Zilibotti (2016) show that special employment zones have a strong positive effect on GDP mainly driven by an increase in capital accumulation. A series of papers investigating the effects of the EU Structural Funds (ESF), a regional subsidy paid by the European Union, show that the ESF increase GDP in the subsidized regions, but had no clear effect on employment (Becker, Egger, and Ehrlich, 2010, 2012, 2013). Criscuolo et al. (2019) analyze an industrial policy in the UK, which is similar to the GRW, and find employment effects that are quite comparable to our effects qualitatively and quantitatively. For Germany, Ehrlich and Seidel (2018) investigate a different place-based subsidy paid to West German regions close the Iron Curtain from the 1970s to until reunification and find positive treatment effects. In terms of the GRW, Brachert, Dettmann, and Titze (2019) find no significant treatment effects, looking at West (instead of East) German regions, using different data and a different identification strategy. Our findings of a positive (negative) employment effect of a subsidy increase (decrease) is in line with descriptive, more policy-oriented papers in Germany (Bade, 2012; Bade and Alm, 2010). Analyzing the direct effect of local subsidies is naturally related to work looking at the effect of state and local taxes on workers and firms (see, e.g. Fajgenbaum et al., 2019; Fuest, Peichl, and Siegloch, 2018; Slattery and Zidar, 2020; Suárez Serrato and Zidar, 2016).

Second, we systematically investigate indirect spillover effects of place-based policies by providing cleanly identified reduced-form estimates. While various empirical studies have looked at single spill-overs, this is – to the best of our knowledge – the first comprehensive analysis looking at various relevant spillovers discussed in the theoretical literature. In line with Criscuolo et al. (2019), we find no evidence of positive or negative regional spillovers on neighboring counties. However, we demonstrate important local demand effects as the untreated retail sector and construction sector are negatively affected by decreases in regional subsidies to manufacturing firms. We also point to trade spillover which have not been investigated in the literature before. In contrast to Moretti (2010), our local multiplier effects are somewhat smaller, which might be explained by the fact that subsidies are not targeted at high-tech, high-skill firms, but rather classic manufacturing firms. Last, we find important policy spillovers that have not received much attention so far. We show that local tax rates increase as a result of decreasing subsidies, which adds an additional burden on local firms. This finding is in line with a result by Ehrlich and Seidel (2018) who look at a different German place-based policy and show that the regional subsidy leads to higher local public investment levels. Our results suggest that a decrease in the GRW erodes firm profits and thus the local tax base, yielding higher local tax rates to finance the largely pre-committed local expenditures.

Third, we add to the current debate on the welfare effects of (place-based) policies. We make use of the novel framework recently put forward by Hendren and Sprung-Keyser (2020) and Finkelstein and Hendren (2020) that enables us to transform our reduced-from quasi-experimental estimates into a welfare statement. We evaluate one of the first policies targeted at firms within this framework. Our approach is an alternative to important structural approaches that have seen a recent surge in the literature (Fajgelbaum and Gaubert, 2020; Gaubert, 2018; Gaubert, Kline, and Yagan, 2021; Rossi-Hansberg, Sarte, and Schwartzman, 2019). Clearly and as established in other contexts, both approaches have their advantages and disadvantages. While the structural approach allows to estimate policy counterfactuals and is capable to capture general equilibrium effects of non-marginal policy changes, the MVPF framework – similar to the sufficient statistics approach – allows for a more immediate mapping between clean quasi-experimental evidence and welfare implications (see Chetty, 2009; Kleven, 2021).

The remainder of this chapter is organized as follows. We explain the institutional setting in Section 1.2, followed by Section 1.3 on the research design. Section 1.4 presents the data. Our empirical results are presented in Section 1.5. In Section 1.6, we discuss the welfare and inequality implications of the policy. Section 1.7 concludes.

1.2 The GRW Policy

In this chapter, we study the main German regional economic policy, called "Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur" (GRW). The GRW is jointly coordinated and financed by the federal government and the individual states.

The explicit goal of the policy is to equalize standards of living across German regions by stimulating local business activity. Equivalent living standards across space is an important principle and policy goal in Germany, which is explicitly mentioned in the constitution. The GRW is the main federal program to achieve this goal.

The policy was implemented in 1969 and subsidized West German underdeveloped regions throughout the 1970s and 1980s. In this study, we focus on the post-reunification effect of the GRW until 2017. After reunification, the majority of GRW funds were directed to East German regions, which were considerably less industrialized than their West German counterparts. As such, the GRW was seen as one of the main instruments aiming at re-industrializing East Germany and bringing it to Western levels.[2]

While the GRW consists of a bundle of different instruments, we focus on investment subsidies paid out to plants – the central instrument accounting for 74% of the total GRW budget in our sample period.[3] These subsidies covered up to 50% of the costs of a specific investment project filed by a plant. The subsidy rate varied across counties depending on the regional economic development, making the GRW a place-based policies (see Section 1.2.2 for more details). From 1991 to 2016, on average €1.8 billion of subsidies (in 2010 €) were paid out annually to East German firms.[4]

1.2.1 Eligibility

In order to receive the subsidy, plants need to file an application for approval with their respective state government. In the application, they need to clearly define the investments project to be subsidized. Typical projects comprise the acquisition of machinery, the construction or modernization of buildings, but also licenses and patents. Labor costs can only be subsidized if employees can be directly linked to the corresponding investment project.

Eligibility of a project is determined by three criteria. First, the project has to be relatively large. Either annual investment costs have to exceed the average amount of the plant's capital consumption (economic depreciation) in the preceding three years by at least 50% (criterion 1a), or the project has to increase the number of regular employees

[2] Other policy measures targeted at plants in Eastern Germany included a capital investment bonus program (Investitionszulage), a non-discretionary capital subsidy targeted at entire Eastern Germany, and loans provided by KfW and the European Recovery Program. Our empirical strategy outlined below makes sure that we isolate the effect of the GRW. Another class of programs directed funds to municipalities rather than to plants. We check that the reforms exploited for identification did not affect funds paid to municipalities.

[3] The other important instrument are infrastructure subsidies to municipalities, which were granted independently of the investment subsidies. Importantly, the maximum infrastructure subsidy rates do not exhibit variation across space.

[4] These numbers include co-payments by the European Union via the European Regional Development Fund (ERDF). Whether subsidies were paid for by the ERDF or GRW is irrelevant for the purpose of our analysis since in Germany, ERDF funds simply increase states' subsidy budgets. Restrictions on subsidy usage, such as sectoral restrictions and maximum assistance rates are thus identical for ERDF and GRW funds.

by at least 15% (criterion 1b). New plant opening qualify under criterion 1b. Second, the project has to be limited in time. The maximum duration of the project is three years (criterion 2). Third, the subsidies are intended for exporting firms. At least half of the plants' revenues have to be made outside of the county (criterion 3). The rationale behind criterion 3 as revealed by the policy discussion in the 1960s is that export-oriented firms are supposed to generate additional income within a county, which, in turn, is supposed to stimulate local demand. Due to criterion 3, 74% of the GRW funds go to manufacturing firms. In Appendix Table 1.B.6, we shows an official list of sectors that automatically qualified for the subsidy according to Criterion 3 without the need to provide further evidence. Notice that certain industries were excluded from the subsidies. These include the construction and retail sector which we will investigate for potential spillover effects.

States have an annual budget for projects to be subsidized under the GRW program. In more than 90% of cases, states did not exhaust their annual budgets, which suggests that there was usually no rationing of the funds and no rivalry between projects. Nevertheless, not all projects were granted. While official data on rejected projects is unfortunately not available, survey data for the state of Thueringa from 2011 to 2016 suggests that roughly 39% of applications were denied (IWH, 2018). However, these rejections were almost entirely due to formal reasons. The two main reasons for rejection, accounting for 96% of rejections, were (i) missing documents and (ii) not meeting the eligibility criteria. Hence, there is no reason to believe that the selection of projects was based on their assessed quality.

1.2.2 Subsidy Rates

Upon successful application, plants receive subsidies to cover a certain share of the investment cost stated in the application.[5] There is a binding maximum subsidy rate imposed by federal law, which varies by plant type, year and – importantly – plant location, the latter source of variation making the GRW a place-based policy.

Below, we exploit the variation in maximum subsidy rates to estimate the causal effects of the policy. In the following, we describe this variation in detail. As a general principle, the policy accounted for differences in the economic performance *within* East Germany and assigned higher subsidy rates to relatively less productive counties. Importantly, differentiation was conducted on the *national* level by the Federal government based on *past* economic performance – both the national decision and the past economic behavior being important features for our identification strategy. More precisely, local productivity was measured by a performance indicator at the level of the commuting zone (*Arbeitsmarktregion*) with counties being nested in commuting zones. There were 76 counties in East Germany and 53 commuting zones in the boundaries of 2014.[6]

[5] It takes on average about 8 months for an application to be approved (IWH, 2018).

[6] Over the years, some counties in East German merged. In a robustness check, we make sure that mergers do not affect our results by excluding all counties that were partially treated. We exclude the county of Berlin from all of our analyses because of its status as a federal state.

In the following, we give an example of the performance indicator and how it affected subsidy rates for the year 1997. The performance indicator for commuting zone r is the weighted geometric mean of three sub-indicators and described by the following formula.

$$indicator_r^{1997} = (infr_r^{1995})^{0.1} \times (wage_r^{1995})^{0.4} \times (unemp_r^{1995})^{0.5},$$

where $infr$ measures the quality of a county's infrastructure in 1995, $wage$ represents per-capita earnings in 1995 and $unemp$ measures the unemployment rate in 1995.[7] All counties were ranked according to this indicator as depicted in Appendix Figure 1.A.1. Counties with an index-value below 100 were classified as high funding priority, counties with a value above the threshold as low funding priority. Counties with a high funding priority receive a higher subsidy rate.[8]

Importantly, indicators, cut-off values and subsidy rates are valid for specific regimes that last between 3 and 7 years. At the end of a regime, indicator function, priority statuses and subsidy rates change, which leads to substantial variation in maximum subsidy rates from the perspective of the individual county. In the last part of the subsection, we document the evolution of regimes and the resulting policy variation.

Table 1.1 gives an overview of the policy variation. In the early 1990s, all East German counties were treated equally, with the maximum subsidy rate for small and medium-sized plants being 50% and 35% for large plants. As of 1997, policy makers started to differentiate funding priorities spatially. Based on the performance indicator described above, 27 out of 76 counties were assigned to low funding priority and consequently experienced a cut in the maximum subsidy rates by 7 percentage points across all three plant size groups (see Table 1.1, regimes 1 vs. 2). In 2000, a new ranking of the counties was generated based on updated measures of past economic performances and slight changes in the indicator function (see Appendix 1.B.2). As a consequence, additional counties switched from high to low priority status.

In 2007, the ranking of counties was renewed. This time, all German counties (East and West) were jointly assessed and ranked – in contrast to previous years, where East

[7] The infrastructure sub-indicator is based on measures of accessibility of airports and larger cities by car or train, of the travelling time for trucks to the next trans-shipment center, the share of employees in applied research institutes, the share of apprenticeship training position, the share of employees in technical occupations, the share of high school graduates, capacity of inter-company training centers and population density.

[8] The rule is almost perfectly deterministic such that all counties above the threshold receive lower funding probability. However, there is some noise in the assignment as revealed by Appendix Table 1.B.3. We see that few counties below the cut-off were assigned low priority. This is mainly due to county mergers that occurred after the reform, i.e. a county above the threshold was merged with a county that was below the threshold. As mentioned above, we exclude partially treated counties in a robustness check. In addition, the Federal government (jointly with state governments) reserves the right to deviate from the ranking in rare exceptions (two counties in 1997). This is mostly due one county biasing the commuting zone average upwards. For example, the relatively poorer county of Gifhorn is located in the same commuting zone as the county of Wolfsburg, which contains the head quarters of Volkswagen. Therefore, policy makers decided to assign Gifhorn to a higher priority even though the commuting zone index was too high.

Table 1.1: Subsidy regimes for East German counties since 1990

	Regime 1 1990-1996		Regime 2 1997-1999		Regime 3 2000-2006		Regime 4 2007-2010		Regime 5 2011-2013		Regime 6 2014-2017			Regime 7 2018-	
priority	high	low	high	low	high	low	high	low	high	low	high	medium	low	high	low
small plants	50%	n/a	50%	43%	50%	43%	50%	n/a	50%	40%	40%	35%	30%	40%	30%
medium plants	50%	n/a	50%	43%	50%	43%	40%	n/a	40%	30%	30%	25%	20%	30%	20%
large plants	35%	n/a	35%	28%	35%	28%	30%	n/a	30%	20%	20%	15%	10%	20%	10%
# counties	76	n/a	49	27	41	35	76	n/a	58	18	9	64	3	9	67

Sources: Deutscher Bundestag (1996), Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007), Deutscher Bundestag (2016) *Notes:* Plant size is defined by the number of employees. Small plants have less than 51 employees, medium-sized plants 51 to 250, and large ones above 250.

Germany regions were assessed separately. As West German regions were still richer than their East German counterparts, all East German counties received high priority status. As a consequence, 35 counties saw an increase in their (employment-weighted) subsidy rate.[9] This particular reform is interesting for various reasons. First, the re-ranking was completely exogenous to the economic performance of East German counties. Second, the reform enables us to test whether effects are symmetric.

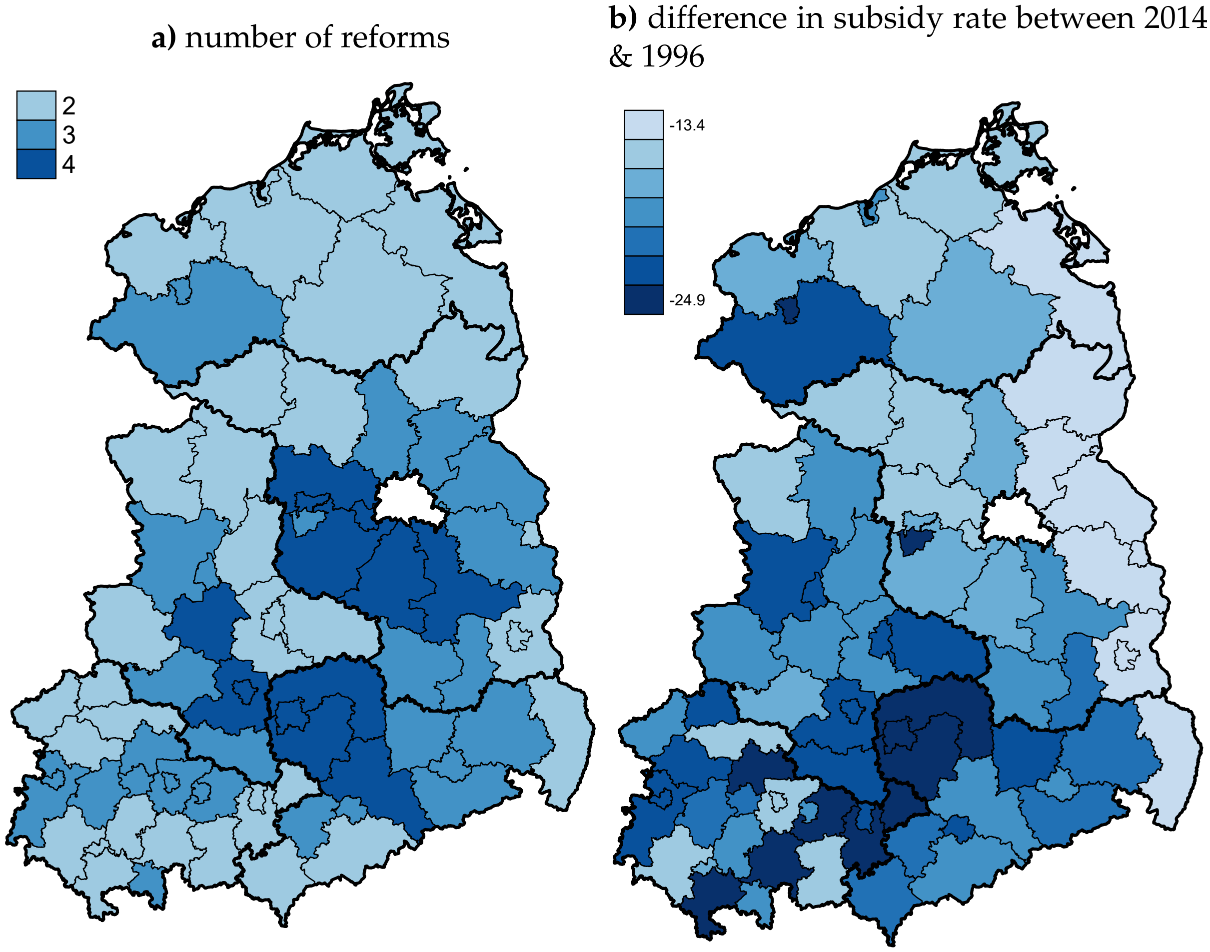
The next reassessment occurred in 2011, when 18 counties were downgraded in their priority status. The reason for this change was the EU's enlargement from 15 to 25 member states resulting in a decline of the average regional GDP per capita in the EU. According to EU regulations, regions above the 75th percentile of GDP per capita lose eligibility for the highest maximum rates. In 2014, Germany was required by the EU to again lower their maximum subsidy rates in two steps. Until 2017, subsidy rates were lowered a maximum of 35% for small plants (25% and 15% for medium and large plants) and in 2018, there was another cut of 5 percentage points.[10] An exception was made for counties that were located directly at the border with Poland since the difference in the subsidy rate between them and the Polish regions would be higher than EU regulations allow. Therefore, these 9 counties were allowed higher subsidy rates throughout the whole period. Note that, even though we do not exploit the 2018 reform directly since our data ends in 2017, we still account for these future reforms in our event study setup.

Overall, the various reforms generate substantial variation in maximum subsidy rates across East German counties, which we exploit in our empirical research design presented in Section 1.3. Figure 1.1a illustrates that all counties experience at least two changes in the subsidy rate, while more than 50% experience three or four changes. The change in the (employment-weighted) maximum subsidy rate varies from a reduction of 13.4 to 24.9 percentage points (see Figure 1.1b). The right panel also shows some interesting regional clustering, e.g. the counties bordering Poland experienced the smallest cuts in rates, while the area around Leipzig, saw the largest. Note that our identification

[9] The subsidy rate for small and large plants, which account for two thirds of manufacturing employment on average, rose by 7 and 2 percentage points, respectively, while the rate for medium plants decreased by 3 percentage points.

[10] Three well-performing counties were directly downgraded to the 2018 level.

Figure 1.1: Map of reforms from 1996 to 2014



Sources: Deutscher Bundestag (1996), Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007), Deutscher Bundestag (2016) *Notes*: Berlin is excluded from the analysis.

strategy only exploits changes within federal states – indicated by the thicker line – for identification.

1.3 Research Design

We estimate the causal effect of the subsidy implementing different variants of event study designs. Given that the policy variation described in Section 1.2 is quite complex, we develop our preferred empirical model step-by-step.

1.3.1 Empirical Model

As described in Section 1.2, the vast majority of subsidy rate changes were decreases. In the simplest form the event study model regresses an outcome y (such as employment or investment) of plant i in county c and year t, $y_{i,t}$ on dummy variables indicating a subsidy cut in county c at time t as follows.

$$\ln y_{i,t} = \sum_{k=-4}^{10} \beta^k D_{c,t}^k + \xi X_{c,t} + \delta_i + \gamma_c + \psi_{s,t} + \varepsilon_{i,t}. \tag{1.1}$$

where $D_{c,t}^k$ is the mentioned set of event indicators indicating whether a change in the maximum subsidy rate occurred for the county $k \in [-4, ..., 10]$ periods ago. We refer to $D_{c,t}^k$ as *binned* event indicators as the indicators at the endpoints of the effect windows, $k = -4$ and $k = 10$, take into account all observable past (future) events going beyond the effect window (McCrary, 2007; Schmidheiny and Siegloch, 2019). Let $d_{c,t-k} = 1$ if county c experienced a subsidy cut in year $t-k$, $d_{c,t-k} = -1$ in case of a subsidy increase and $d_{c,t-k} = 0$ otherwise, then the binned event indicators $D_{c,t}^k$ are formally defined as

$$D_{c,t}^k = \begin{cases} \sum_{s=-\infty}^{-4} d_{c,t-s} & \text{if} \quad k = -4 \\ d_{c,t-j} & \text{if} \quad -4 < k < 10 \\ \sum_{s=10}^{\infty} d_{c,t-s} & \text{if} \quad k = 10. \end{cases} \tag{1.2}$$

The event study design enables us to test for flat pre-trends ($k \leq -1$) and informs about the adjustment paths of the post-treatment effect ($k \geq 0$). All other estimates are to be interpreted relative to the pre-treatment period $k = -1$, whose coefficient is normalized to zero. In some specifications, we additionally include time-varying control variables at the county-level $X_{c,t}$. Our specifications always include plant and county fixed effects γ_c and δ_i as well as state-by-year fixed effects $\psi_{s,t}$ to absorb state-specific shocks. This is important because state governments play a role in granting the subsidy and we see regional clustering of the intensity of subsidy rate cuts (see Section 1.2). Standard errors are clustered at both the county and plant level throughout.

Table 1.1 showed that there is variation in the subsidy rate cuts over time and across counties and plant types as the reforms differentially affected maximum subsidy rates for different plant sizes. To exploit this variation, we define treatment intensity $I_{c,t}^k$ of county c, year t and lead/lag $k \in [-4, 10]$ as

$$I_{c,t}^k = \Delta s_{c,t-k}^{small} \omega_c^{small} + \Delta s_{c,t-k}^{med} \omega_c^{med} + \Delta s_{c,t-k}^{large} \omega_c^{large}. \tag{1.3}$$

The intensity measure is a weighted average of the (absolute) change in maximum subsidy rate $\Delta s^p_{c,t-k} = |s^p_{c,t-k} - s^p_{c,t-k-1}|$ across plant types, $p \in [small, med, large]$. Respective weights are denoted by ω^p_c and defined as the manufacturing employment share of plants of size p in county c

$$\omega^p_c = \frac{E^p_{c,1995}}{E^{small}_{c,1995} + E^{med}_{c,1995} + E^{large}_{c,1995}} \quad \forall f \in [small, med, large].$$

$E^f_{c,t}$ denotes the number of workers in manufacturing plants of size f in county c at time t. Weights ω^p_c are time-invariant and calculated in the data year 1995, hence prior to the first reform.[11]

Based on these definitions, the generalized event study design that accounts for the different treatment intensities is given by:

$$\ln y_{i,t} = \sum_{k=-4}^{10} \beta^k \left[D^k_{c,t} \cdot I^k_{c,t}\right] + \xi X_{c,t} + \delta_i + \gamma_c + \psi_{st} + \varepsilon_{i,t} \tag{1.4}$$

Compared to the basic model given in equation (1.1), this variant of the event study replaces the dummy treatment indicator with an indicator that is specific to the event. As shown in Schmidheiny and Siegloch (2019), event studies – just as the numerically equivalent distributed lag models – can be easily generalized to account for multiple changes of different intensities if treatment effects are homogeneous over time.

1.3.2 Identification and Sensitivity

The classical identification check in event study designs is to assess whether pre-treatment effects are statistically different from zero. Nevertheless, even flat pre-trends might not be sufficient to interpret the estimates causally. The key remaining threat to identification is omitted variable biases concurrent with treatment timing. While plant and county fixed effects control for time invariant confounders at the respective levels, state-by-year fixed effects flexibly account for any confounding shock occurring at the state-level. However, if the concurrent and confounding shock is at the county-level, estimates would still be biased.

The prime suspect in our context is local economic performance as subsidy rates are a function of past regional economic performance (see Section 1.2.2). The better the county performed economically in the past, the higher the probability of a subsidy rate cut. Note that such differences in past economic development should, however, show up in the pre-treatment effects and we would expect pre-treatment effects increasing from below zero. If we expect that a cut in the subsidy rate hurts the local economy, this relationship would bias our estimates towards zero.

[11] We drop year 1995 from the data after calculating the shares.

Improving comparability. Given our institutional setup, we can further improve the comparability of treatment and control group. Using the cut-off between high- and low-priority counties and the resulting discontinuity in subsidy rates, we can restrict the sample to counties close to the cut-off. Denote $\mathbb{T}^{M,R}$ ($\mathbb{C}^{M,R}$) the set of the M counties closest to the performance cut-off from below (above) following the indicator for regime R. Let $\mathbb{S}^{M,R} = \mathbb{T}^{M,R} \cup \mathbb{C}^{M,R}$ be the set of $2M$ counties around the cut-off during regime R. As we look at multiple regimes and counties might move toward and away from the regime-specific thresholds, we define the set $\mathbb{S}^{M}$ that includes all counties that are at least once within the set of counties close to the threshold: $\mathbb{S}^{M} = \bigcap_{R} \mathbb{S}^{M,R}$. We can then refine our empirical model in equation (1.4) by restricting the underlying estimation sample to counties in $\mathbb{S}^{M}$:

$$\ln y_{i,t \mid \mathbb{S}^M} = \sum_{k=-4}^{10} \beta^k \left[D_{c,t}^k \cdot I_{c,t}^k \right] + \xi X_{c,t} + \delta_i + \gamma_c + \psi_{s,t} + \varepsilon_{i,t}. \tag{1.5}$$

In our preferred baseline model, we choose $M = 30$. We also vary M by reducing it or increasing to capture the full sample and find that results (pre and post-treatment effects) do not change in a meaningful way lending credibility to our identification strategy.

Heterogeneous treatment effects. With homogeneous treatment effects, applying an event study with multiple treatments of different intensities produces unbiased estimates of the treatment effect (Schmidheiny and Siegloch, 2019). However, there has been a recent important literature emphasizing that (static and dynamic) difference-in-difference designs with differential treatment timing estimated with a two-way fixed effect model can be severely biased in the presence of heterogeneous treatment effects (Borusyak, Jaravel, and Spiess, 2021; Callaway and Sant'Anna, 2020; Chaisemartin and D'Haultfoeuille, 2020a,b; Sun and Abraham, 2020). Several new estimators have been proposed to get unbiased estimates when treatment effects are not homogeneous. However, all these estimators are not valid for environments with multiple events for the same unit. In order to test for potential biases due to heterogeneous treatment effects, we cut our sample in 2006 and focus on the first three regimes since the reform in 2007 treats all counties (see Table 1.1). This yields a sample where every unit is treated at-most once and we retain a group of never-treated units. We apply the estimators developed in Chaisemartin and D'Haultfoeuille (2020a) and Sun and Abraham (2020) to our basic dummy variable specification described in equation (1.1).[12] Notice that the two estimators use different control groups since Sun and Abraham (2020) only allow comparisons to never-treated units, whereas Chaisemartin and D'Haultfoeuille (2020a) are also using not-yet treated units as controls. We find that our estimates are unlikely to be driven by heterogeneous treatment effects.

[12] Note that in our setup without covariates and with never-treated units, the estimators from Sun and Abraham (2020) and Callaway and Sant'Anna (2020) coincide.

Controlling for observables. As another test, we include county-level controls that control for local business cycle effects. We control for log GDP per capita and the unemployment rate lagged by one year. This specification tries to account for remaining differences in past economic performance and thereby purifies our β^k estimates from potential bias. Estimates are hardly affected and as expected, if anything, slightly more negative. In another check, we include the contemporaneous values of the business cycle covariates – ignoring even more the obvious bad control problem. Effects are again very similar. Last, we also use the business cycle variables as outcomes and test whether we find significant pre-treatment effects pointing to an identification concern. We find flat pre-trends.

Other subsidies. As discussed in Section 1.2, we test whether changes in the GRW subsidy rate have triggered changes in other regional subsidy programs, which could in turn bias our estimates. We test for this possibility by looking at the effect of GRW subsidy cuts on the sum of other subsidies received and find no spillovers.

Symmetry. We estimate a model that explicitly differentiates between subsidy cuts and increases to test for symmetry. Note that we are mostly observing subsidy cuts, but the peculiar reform of 2007 enables us to separately study subsidy increases.

Sensitivity. Apart from these identification tests, we run several sensitivity checks to make sure that modelling choices are not driving our results. First, we implement the basic dummy variable event study specification of equation (1.1) which ignores the size of the subsidy changes. Second, we drop the few counties that – for various reasons discussed in Section 1.2 – were only partially treated. Fourth, we vary the event window between nine, ten and elven lags. Last, we estimate our model in first differences instead of with fixed effects. In none of these checks, results change in a meaningful way.

1.3.3 Extensions to Test for Spillovers

One contribution of this chapter is to systematically look at spillovers. Depending on the context, we have to adjust our baseline model, given in equation (1.5) to assess the role of the spillover.

Testing for regional spillovers. A cut in subsidies might have spillover effects that go beyond county borders and affect neighboring counties. Theoretically, these spillovers can be positive in case local demand or agglomeration effects radiate beyond county lines. They may also be negative if economic activities are relocated from control to treatment counties. We test for those kinds of spillovers by moving the analysis to a higher level of aggregation. Explicitly, we follow Criscuolo et al. (2019) and aggregate equation (1.5) to the level of the local labor market. The difference between the estimate at the county level and the estimate at the local labor market level gives an indication of regional spillovers. Note that there is some variation in subsidy rates across counties within local labor markets. First, counties have different plant size distributions. Second, there were county-level mergers beyond commuting local labor market borders. Third, there were some exceptions in the assignment rules discussed in Section 1.2, for instance, the special treatment of counties bordering Poland in the late 2010s or due to extreme outlier counties in terms of economic performance within local labor markets.

Testing for trade spillovers. Given that manufacturing firms in East German counties are part of a larger value chain, we also test for trade spillover. In particular, we test whether manufacturing plants in other counties that have significant trade exposure to the treatment counties also respond to the subsidy cuts. First, we take the imports (measured in tons per year) of county c coming from treatment county g with $c \neq g$ and divide them by the total imports of county c. Equivalently, we calculate the share of exports that are exported from county c to treatment county g. Then, let the trade exposure of county c in year t to a reform that happened l years ago be defined as:

$$\text{trade exposure}^l_{c,t} = \sum_{g \neq c} \frac{\text{imports}_{cg}}{\text{total imports}_c} \left[D^l_{g,t} \cdot I^l_{g,t}\right] + \sum_{g \neq c} \frac{\text{exports}_{cg}}{\text{total exports}_c} \left[D^l_{g,t} \cdot I^l_{g,t}\right] \quad (1.6)$$

where $D^l_{g,t}$ and $I^l_{g,t}$ are defined as above. To test for trade spillovers, we include the trade exposure measure in our model.

$$\ln y_{i,t} = \sum_{k=-4}^{10} \beta^k \left[D^k_{c,t} \cdot I^k_{c,t}\right] + \sum_{l=-4}^{10} \beta^l_{trade} \text{trade exposure}^l_{c,t} + \delta_i + \gamma_c + \psi_{s,t} + \varepsilon_{i,t} \quad (1.7)$$

where β^l_{trade} represents the effect on plants with trade exposure to a one-percentage-point subsidy cut l years ago.

1.4 Data

In this section, we present the data that we use in our analysis. Detailed information on variable definitions and sources can be found in Appendix Table 1.B.1 and summary statistics are presented in Appendix Table 1.B.2.

1.4.1 Subsidy Data

We make use of administrative subsidy data provided by the Federal Ministry for Economic Affairs. For the years 1996-2016, we obtained the universe of GRW subsidy cases in East Germany including investment volume, subsidy amount and the receiving plant's county. Matching these data to plants is prohibited due to data protection laws, hence we are unable to identify which plants did in fact receive subsidies and which did not. We follow standard practice and estimate the intent-to-treat effect, investigating the employment response of plants in a treated area (Criscuolo et al., 2019). As mentioned above, 74% of all subsidies were paid to manufacturing firms. Appendix Table 1.B.2 shows that the average yearly subsidy payments received by a county amount to €18 million, supporting investment projects worth €82 million.

1.4.2 Employment and Wage Data

We measure employment using the Establishment History Panel (BHP), which is based on social security records and provided by the Institute of Employment Research in Nuremberg (Schmucker et al., 2016). We have access to a fifty percent random sample of plants in Germany for the period of 1996-2017. The dataset includes the annual number of employees by skill at a plant as well as the county in which it is located and its industry classification.

To measure wages, we additionally make use of the IAB's Sample of Integrated Labour Market Biographies (SIAB) from 1996 to 2014. The dataset is a 2% sample of individual earnings biographies and includes individual characteristics as well as employer information from the BHP.[13] We drop all apprentices, social service workers, working students and interns and convert wages to 2010 €. Then, we calculate the median wage at the county level for manufacturing workers, non-manufacturing workers and all workers. As one can see in Appendix Table 1.B.2, workers in the manufacturing sector have a higher median wage than workers in other sectors. We also calculate wages by education level within the manufacturing sector. As expected, high-skill workers earn substantially higher wages than their low-skilled peers.

1.4.3 Investment Data

Moreover, we obtain investment data at the plant level from the AFiD Establishment-Panel provided by the Federal Statistical Office of Germany. The data cover the universe of German manufacturing and mining plants with 20 or more employees for the period from 1996 to 2016. Importantly, we can observe total investment on the plant level which we deflate to 2010 €. Additionally, the AFiD data provide industry codes and information on the plant location at the municipal level. We use that information to restrict our sample to manufacturing plants and locate plants within in the current county borders.

1.4.4 Trade Flow Data

We use trade flow data from the Federal Ministry of Transport and Digital Infrastructure to calculate the trade exposure of German counties as described in Section 1.3.3. The

[13] Earnings histories are in general recorded for persons who have appeared at least once in the social security system, either as an employee or as being unemployed, since 1975.

data include a complete matrix of trade flows between all German counties as well as foreign countries for the year 2010. Trade flows are measured in tons per year and we can observe the direction of trade, i.e. we can differentiate between imports and exports between two counties.

1.4.5 Other Regional Variables

Last, we make use of further regional variables either as outcomes or as control variables. We obtain administrative data on the local business cycle (GDP per capita and local unemployment) as well as labor force and population numbers, provided by the statistical offices of the German states. In order to assess policy spillovers, we additionally obtain data on the municipal local business and property tax rate. While the tax base of these taxes is set at the national level, municipalities can freely set their own tax rates (see Fuest, Peichl, and Siegloch, 2018; Löffler and Siegloch, 2021, for a detailed description). Furthermore, we obtain tax revenues from business and property taxation and use it to calculate the respective tax base.

In addition, we gather data on municipality-level grants and subsidies in order to make sure that other transfers are not confounding our GRW effect. To keep the level of analysis consistent, we aggregate the municipal-level data to the county level using pre-form population shares as weights if necessary. Moreover, we collect data on the net commuting flows normalized by the number of employees from the Federal Office for Building and Regional Planning.

Last, we add on housing prices, to assess whether the GRW subsidies are capitalized into housing prices. In order to populate our long panel starting in the 1990s, we use house price data from the German real estate association IVD. These data cover the largest city within a county.[14]

1.5 Empirical Results

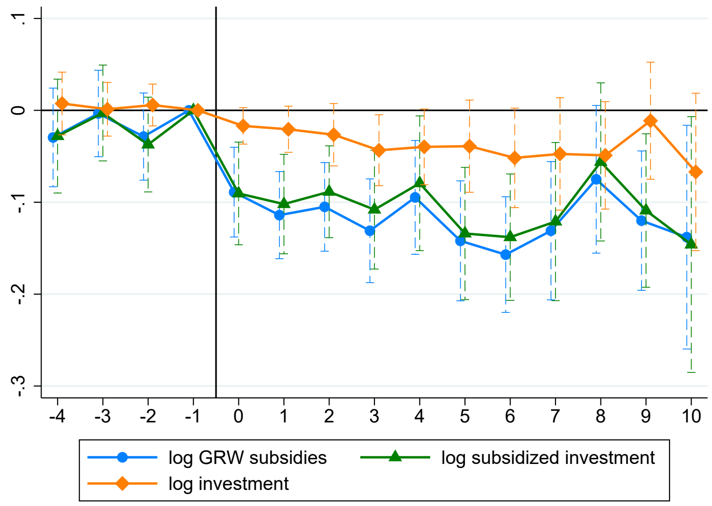
In this section, we present the reduced form effects of the place-based policy. Subsection 1.5.1 focuses on direct policy effect of a subsidy cut for manufacturing plants in treated counties. In subsection 1.5.2, we address various identification challenges and demonstrate that our main effects are robust. Subsection 1.5.3 sheds light on the various spillover effects of the GRW.

1.5.1 Direct Policy Effects

Investment effects. In a first step, we assess whether cuts of the maximum subsidy rates affect the subsidies paid out, that is, we test our first stage. Figure 1.2 shows the

[14] For some county-year pairs, no data is available. We interpolate occasionally missing data points linearly. More comprehensive micro data, e.g. from the online platform ImmobilienScout24 (the German Zillow), only start much later in the mid-2000s.

Figure 1.2: Event study estimates: subsides and investment



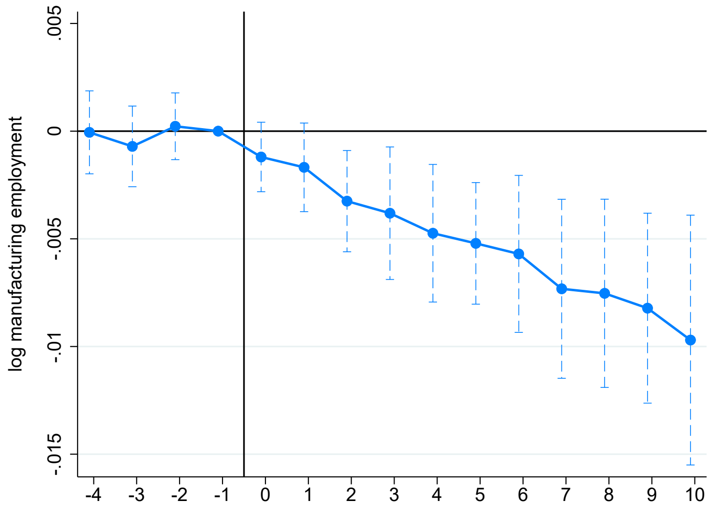
Source: Federal Ministry for Economic Affairs, AFiD *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log subsidies paid to counties, log subsidized investment and log investment on leads and lags of a change in the maximum assistance rate as in equation (1.5). The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level.

effect of a one-percentage-point decrease in the maximum subsidy rate on GRW subsidies at the county level. We find that a subsidy rate cut in treated counties decreases subsidy amounts by 13.8% after ten years which corresponds to a decrease of €2.5 million for the average county. In line with this finding, we also see that log subsidized investment decreases in a very similar manner. The total investment volume subsidized by the GRW decreases by 14.6% ten years after the reform. Reassuringly, treatment and control groups exhibit a very similar development before a reform for both variables as revealed by the pre-treatment trends.

Last and importantly, we are interested in the effects of subsidy rate cuts on total investments by plants. Using the AFiD data, we show that overall investment decreases by roughly 6.7% after ten years. The investment response is almost exclusively driven by investment in equipment, which makes up about 85% of all investment (see Appendix Figure 1.C.1). Note that it is difficult to make a statement about possible crowding-out of private investment because of two reasons. First, the AFiD data only contain plants with 20 or more employees.[15] Second, there might be positive or negative spillovers on untreated manufacturing firms, which are reflected in the AFiD estimates, but not in the effect on subsidized investments. Unfortunately, we cannot disentangle these effects without strong assumptions.

[15] The AFiD is the largest and only administrative microdata set on plant-level investment in Germany.

Figure 1.3: Event study estimates: plant-level manufacturing employment



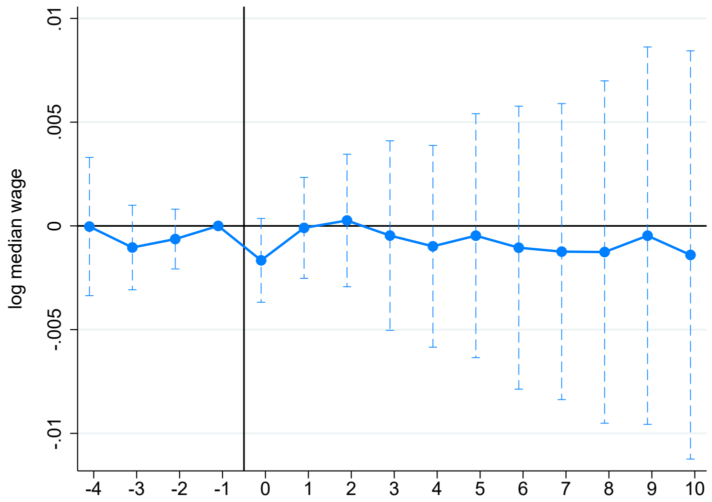
Source: BHP. *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate as in equation (1.5). The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level.

Employment effects. We now move to our main outcome, the employment effect of the GRW policy. Consistent with the finding of a decrease in investments, Figure 1.3 shows that cuts in the subsidy rate significantly reduce plant-level manufacturing employment. While pre-trends are flat, our estimates imply that a one-percentage-point decrease in the maximum subsidy rate leads to a decrease in manufacturing employment of 1% after ten years for our baseline sample.[16] These estimates are quantitatively similar to the main finding of Criscuolo et al. (2019). We find that the decrease in employment is mostly driven by medium-skilled workers, which make up 80% of all manufacturing workers, whereas employment of low- and high-skill workers decreases to a lesser extent (see Appendix Figure 1.A.2). Thus, these results do not speak in favor of human capital spillover playing a major role in the context of the GRW subsidy, which targets mainly German manufacturing firms (Diamond, 2016; Glaeser and Gottlieb, 2008).

Since the negative effect on manufacturing employment at the plant-level only reflects adjustments at the intensive margin, we also look at the number of manufacturing establishments on the county level. Appendix Figure 1.A.3 shows that there is little evidence for any effects on the extensive margin. Accordingly, the negative effect on total

[16] For each regime, we pick the 30 counties which are closest to the cut-off from below and the 30 counties that are closest from above. Aggregating over regimes, we end up with 55 counties that are at least once close to the cut-off. In some years, less than 30 counties are above the threshold, which is why the number of counties is below 60.

Figure 1.4: Event study estimates: median manufacturing wages



Source: SIAB. *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log median manufacturing wages on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level.

manufacturing employment at the county level in Figure 1.A.3 is quantitatively very similar to the plant-level effect in Figure 1.3.

As discussed in Section 1.2, the rationale of the GRW policy was to stimulate the export-oriented manufacturing sector and thereby push the entire local economy. Figure 1.3 shows that the manufacturing sector, which accounts for 18% of total employment, is responding as intended. In terms of total employment, however, we find little evidence that the aggregate effects on non-manufacturing plants are particularly strong. As a result, total employment goes down by only 0.2% (Appendix Figure 1.A.4). Nevertheless, the aggregate effect on non-manufacturing employment masks interesting spillovers on certain industries, which we discuss in Section 1.5.3 below.

In line with the effect on employment, we detect that the number of unemployed increases by about 0.5%, however estimates are imprecise (see Appendix Figure 1.A.5). This suggests that the laid-off workers mostly transitioned to unemployment. Consistent with that, there is no effect on the size of the labor force (see Appendix Figure 1.A.5). GDP per capita at the county-level also drops, but the effect is not significant (see Appendix Figure 1.A.6).

Wage effects. Last, the decrease in labor demand could lead to decreasing wages in the manufacturing sector. Using the SIAB data, we calculate the median wage of workers in the manufacturing sector at the county level. We use the median as around 13% of wages are top-coded in the SIAB data.

As Figure 1.4 shows, wages are virtually unaffected by subsidy cuts. Also, when differentiating by skill, wages for all skill groups are largely unaffected (see Appendix Figure 1.C.2). Wages in non-treated sectors and overall wages do not respond significantly to the subsidy cut either (see Appendix Figure 1.C.3a). Results are similar when using average wages instead of median wages (see Appendix Figure 1.C.3b).

1.5.2 Identification and Sensitivity Checks

In the following, we present various tests demonstrating the robustness of our main results. The rationale behind the different checks is discussed in Section 1.3.2.

Improving comparability. First, our baseline specification improves the comparability of treatment and control group counties by focusing on the jurisdictions that are close to the eligibility cut-off that determines treatment status. Our preferred specification uses 55 counties around the cut-off per regime. This is clearly an arbitrary choice trading off comparability and statistical power. Appendix Figure 1.C.4a presents results for different cut-off samples including the full sample. The magnitude of the employment effect is hardly affected as we vary the number of counties around the cutoff.

Controlling for observables. Next, we add control variables that pick up local business cycle fluctuations (and consequently affected treatment status via the eligibility indicator). Reassuringly, the inclusion has little effects on the results, as demonstrated in Appendix Figure 1.C.4b. Importantly, we do not find significant pre-trends when using log GDP per capita or unemployment as an outcome (see Appendix Figures 1.A.5 and 1.A.6).

Other subsidies. We also test the effect of the GRW reforms on other subsidies received by municipalities. Figure 1.C.5 shows that the reforms did not have a significant effect on other subsidies received by municipalities.

Symmetry. The majority of subsidy rate changes are decreases. However, the reform in 2007 in which all East German counties were assigned high priority status (see Section 1.2.2), led to an increase in subsidy rates for roughly half of the East German counties. Therefore, we can estimate a model that allows for different effects of subsidy increases and decreases. Appendix Figure 1.A.7 shows a symmetric pattern. We can neither reject the null hypothesis that any individual post-treatment effect is asymmetric nor the joint test of asymmetry (p-value = 0.213).

Heterogeneous treatment effects. To test whether heterogeneous treatment effects are biasing our results, we apply the estimators by Chaisemartin and D'Haultfoeuille (2020a) and Sun and Abraham (2020) to our baseline dummy variable model described in equation (1.1). We stop our sample in the year 2006 to have a setup with a maximum of one treatment per county and retain a group of never-treated units. To ensure a comparability across specifications, we also estimate equation (1.1) as a standard event study on the same sample. We plot the resulting estimates and their standard errors in Appendix Figure 1.A.8. The effects are very close both in size and pattern to our baseline event study estimates. We conclude that heterogeneous treatment effects are unlikely to drive our results.

Sensitivity with regard to modelling choices. Last, we provide a set of checks that assess the sensitivity of our findings with regard to modelling choices we make when setting up our baseline. First, we test whether implementing a standard event study design using a discrete treatment indicator following equation (1.1) yields similar results. As Appendix Figure 1.C.6a shows, results are very similar when comparing our baseline model and the dummy-variable specification scaled by the average cut.

Second, recall that due to changes in county border definitions, in some counties only a subset of municipalities receives a decrease in the maximum rate, effectively reducing treatment intensity. Dropping these few partially treated counties yields larger effects, suggesting that our baseline estimate is conservative (see Appendix Figure 1.C.6b).

Third, we vary the number of lags of our event window between nine and eleven years. As Appendix Figure 1.C.7a shows, the effects tend to level off after ten years. Last, Appendix Figure 1.C.7b shows our baseline results estimated both in a fixed effect and first difference model. Size and pattern are again very similar.

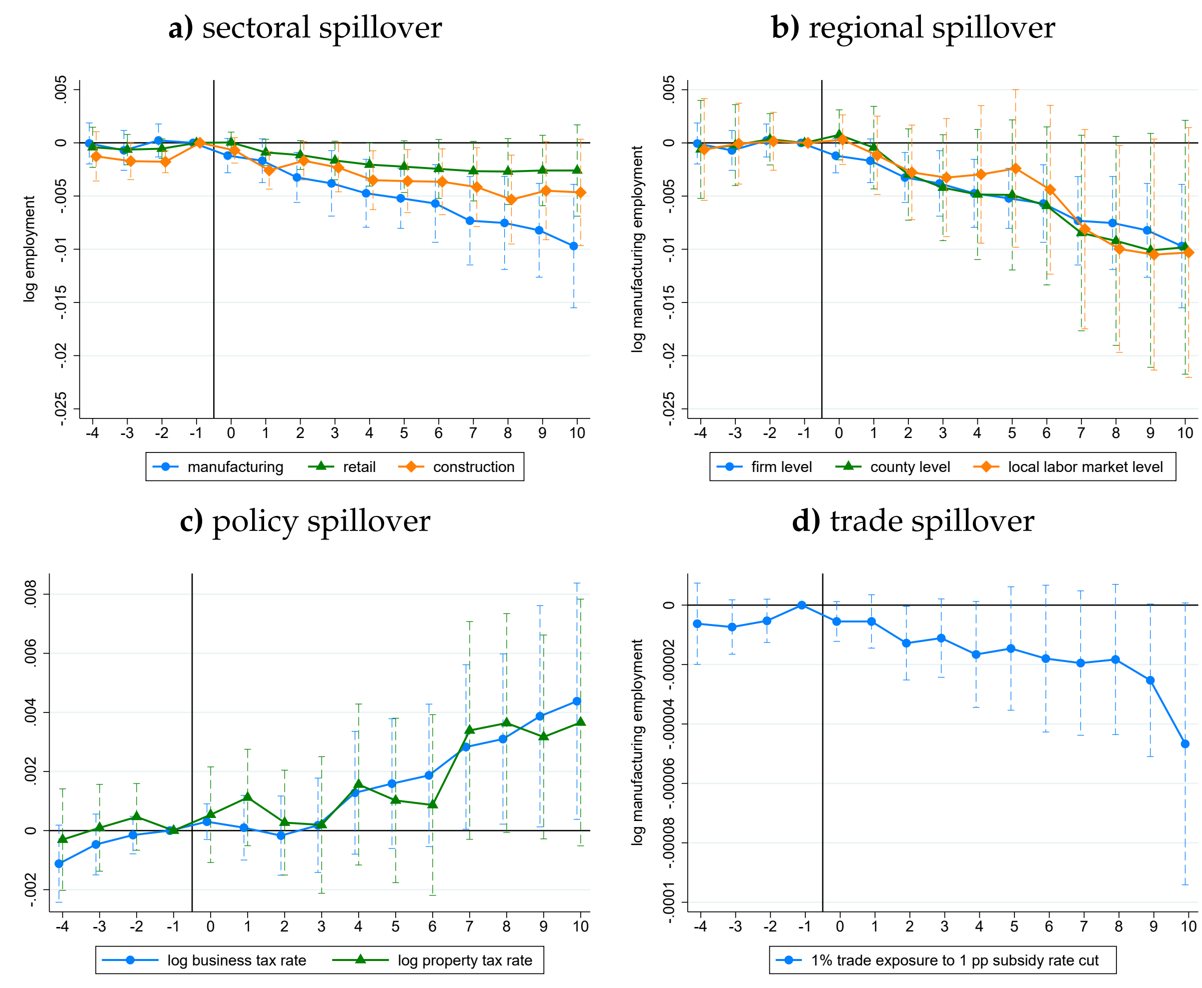
1.5.3 Spillover Effects

While we have established a clean and robust direct policy effect, we investigate various potential spillover effects in the following subsection.

Intra-county spillovers. First, we check whether the place-based policy had an effect on untreated industries in treated counties. Above, we have shown that non-manufacturing employment only responds marginally to the cut in subsidy rates, resulting in a small and imprecise aggregate employment effect. However, when we decompose non-manufacturing employment into finer industries, we do find some evidence of intra-county sectoral spillovers. More specifically, we look at the retail and construction sector which were de jure excluded from receiving GRW subsidies allowing us to pinpoint spillover effects (see Appendix Table 1.B.6). Figure 1.5a shows (positive) spillover effects for the untreated retail and construction sector.[17] A cut in subsidy rates leads to an immediate decreases in employment in the construction sector, which seems intuitive as we have seen that subsidy cuts trigger an immediate decrease in investment projects like building new or extending production facilities. Likewise, we

[17] We define a positive spillover as going into the same direction as the direct policy effect.

Figure 1.5: Event study estimates: spillover effects



Notes: Panel (a) plots coefficients along with 95% confidence intervals of a regression of log industry employment at the plant level on leads and lags of a change in the maximum assistance rate as in equation (1.5). The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level. Panel (b) plots coefficients along with 95% confidence intervals of a regression of changes in log manufacturing employment on leads and lags of a change in the maximum assistance rate at the county and local labor market level. When aggregating to the local labor market level, treatment intensities of counties are weighted by the number of manufacturing employees. The sample includes the counties or local labor markets that contain the 55 counties closest to cutoffs (M = 30). Clustering of standard errors is at the county or local labor market level. Panel (c) plots coefficients along with 95% confidence intervals of a regression of changes in the log local business and property tax rates on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level. Panel (d) plots coefficients along with 95% confidence intervals of a regression as in equation (1.7) using log manufacturing employment at the plant level as the outcome. The sample includes all German counties. Clustering of standard errors is at the county and plant level.

detect a (smaller) negative effect on retail employment, which could be explained by a decrease in local demand. In total, one job lost in the manufacturing sector leads to 0.64 [0.16,1.87] additional jobs lost in retail and construction sectors. This is a somewhat lower estimate of local spillover than Moretti (2010) finds for US cities. A likely reason for this divergence is that the GRW subsidy is paid to traditional manufacturing firms rather than firms that rely heavily on highly-skilled workers.

We also test whether subsidy rate changes are capitalized in house prices. If an increase in the subsidy rate would lead to increased house prices, the distributional impact of the policy would change with (pre-existing) home owners being main beneficiaries. As Appendix Figure 1.A.9 shows, we do not find any effect on house prices.

Regional spillovers. Next, we test whether negative manufacturing employment effects in treated counties spread across county borders within the local labor market. We aggregate county-level manufacturing employment to the local labor market level and use the weighted average of counties' treatment intensities to re-estimate equation (1.4) on the baseline sample. Figure 1.5b shows that the treatment effect on manufacturing employment at the labor market level is very similar to our baseline at the plant and county level implying that there was little reallocation of workers across counties within local labor markets. This is consistent with the null effects on the net commuting flow per employee and population we find (see Appendix Figure 1.A.10).

Policy spillovers. We also test for the possibility of policy spillovers. Since a subsidy cut negatively impacts local employment, municipalities finances are also affected. The effect is theoretically ambiguous. If local politicians want (or are forced) to balance their budget, they might need to increase local tax rates to counteract the loss of tax revenue. On the other hand, local politicians being aware of tax competition might try to compensate firms for the decrease in subsidies by lowering tax rates. Figure 1.5c shows that both local business and property tax rates are raised in response to a cut in the maximum subsidy rate. This finding is not surprising in the context of German municipalities, which are not very flexible in adjusting their expenditures (Löffler and Siegloch, 2021).

Overall, the results on policy spillovers imply that businesses in treatment counties do not only receive a subsidy cut, but also face higher business and property tax rates. Local tax revenues from property taxation increase slightly, whereas business tax revenues decrease (see Appendix Figure 1.C.8a). The latter effect implies a shrinking business tax base. As we do not see any effects on the number of plants, the most plausible answer is that firm profits decrease (see Appendix Figure 1.C.8b).

Trade spillovers. Last, we assess whether cuts in the GRW affected untreated counties that were connected to treated counties via trade flows using the empirical model specified in equation (1.7). We differentiate between import and export exposure to treatment counties. Figure 1.5d shows that a 1% trade exposure to a 1 percentage-point-decrease in the subsidy rate reduces manufacturing employment by 0.005%. This is consistent with the effect of the subsidies propagating through the value chain and thereby also affecting untreated counties with higher levels of trade exposure to treated counties.

1.6 Discussion: Efficiency and Inequality Effects

In this section, we provide a welfare analysis of the GRW policy by assessing its efficiency and redistributive implications.

1.6.1 Efficiency Assessment

To asses the efficiency of the GRW, we calculate the marginal value of public funds (Hendren and Sprung-Keyser, 2020). The measure relates marginal benefits of the policy to its marginal costs by taking the ratio of the willingness to pay (WTP) of all beneficiaries of an incremental change in a government policy to the net costs of the policy change:

$$\text{MVPF} = \frac{\text{Willingness to pay}}{\text{Net government costs}} \tag{1.8}$$

The willingness to pay aggregates the real economic effects of the policy, including the WTP of direct beneficiaries (workers in directly treated plant) as well as additional beneficiaries due to spillovers. Net government costs comprise both the direct program spending and the fiscal externalities caused by the policy, for instance changes in income tax revenues triggered by changes in wages and/or employment.

Since this approach does not rely on assumptions about welfare weights, knowing the MVPF alone is in most cases not informative about the question of whether or not the policy should be implemented.[18] Instead, the MVPF unfolds its potential when comparing across policies that target similar groups of recipients. In this case, the MVPF is informative of which policy can achieve the same goal at a lower cost (Finkelstein and Hendren, 2020). Hence, we compare the MVPF of the GRW with MVPFs for other policies that target a similar group of recipients such as welfare cash transfer or unemployment benefits. Moreover, we can make an internal comparison comparing the the GRW MVPFs with and without accounting for spillover effects.

We consider the policy experiment of increasing the subsidy rate by one percentage point and use our reduced form estimates to calculate the resulting effects. We consider this experiment for two reasons. First, we have a direct mapping between our reduced-form estimates and the MVPF formula. Second, calculating the willingness to pay for marginal policy changes is more straightforward than for large reforms (Finkelstein and Hendren, 2020; Kleven, 2021).

[18] If net government costs are negative, the policy pays for itself and the policy should always be implemented as long as it the WTP is greater than zero.

For the willingness to pay, we consider the following effects: (i) the increase in net earnings due to newly created manufacturing jobs, (ii) the increase in net earnings due to positive spillover effects to the retail and construction sector, (iii) the increase in net earnings due to newly created manufacturing jobs via trade spillover, and (iv) the decrease in unemployment benefits payments for newly hired workers.[19]

Since we do not find evidence for regional spillover, we use our plant-level estimates to calculate the number of jobs created. We multiply the baseline estimates of a 1% increase with the average number of manufacturing jobs in East Germany in our sample period to get the number of manufacturing jobs created. We then multiply that number with the average manufacturing wage in East Germany obtained from the SIAB in our sample period to get the increase in gross earnings. We iterate this procedure for all estimates from year 0 after the reform to year 10 after the reform assuming a discount rate of 3%.[20] We subtract additional income taxes by applying the average income tax rate to obtain net labor earnings. We conduct this procedure for manufacturing, retail and construction jobs. Next, for the trade spillover, we adjust the coefficients for import exposure by the average import exposure in our sample and compute the number of additional jobs by multiplying the adjusted coefficient with the average number of manufacturing jobs in Germany as a whole. The number of jobs is then multiplied with average manufacturing wage in Germany and income taxes are subtracted. Last, we calculate the number of unemployed using our unemployment estimate and multiply it by the average unemployment benefits in our sample period. This yields an estimate of the unemployment benefits that individuals forgo by being employed.

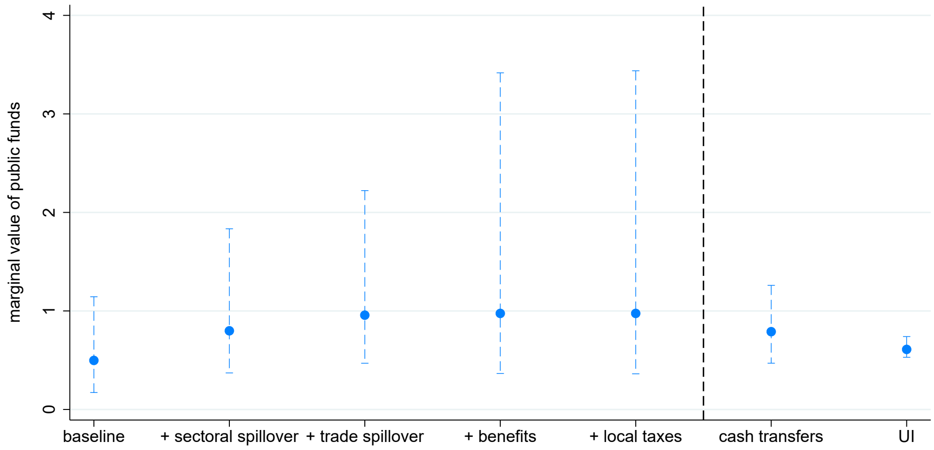
For net government costs, we consider the following items: (i) direct program spending, i.e. the costs of the GRW subsidies, (ii) an increase in income taxes paid due to the increase in net earnings, (iii) a decrease in unemployment benefits paid due to the decrease in unemployment, which also reduces the net costs, and (iv) the changes in local business and property tax revenues.

For direct program costs, we make use of our estimate on the subsidies paid out in response to a one-percentage-point change in the subsidy rate multiplied by the average subsidies paid out. We then subtract the increase in income taxes of manufacturing, construction and retail workers as well as the reduced spending on unemployment benefits. We calculate 95% confidence intervals using the bootstrapping algorithm suggested by Hendren and Sprung-Keyser (2020).

We start by focusing on the direct policy effect, i.e. the policy-induced changes in manufacturing jobs. The estimated behavioral responses yield an aggregate willingness to pay of €0.784 billion and net government costs of €1.594 billion. As Figure 1.6 shows, the resulting marginal value of public funds is 0.50 [0.17,1.14]. Next, we add spillover effects on other sectors, which increases the willingness to pay to €1.214 billion and reduces the net government costs to €1.520 billion. Hence, sectoral spillovers increase

[19] The effect on manufacturing wages is very small and noisy. It does not affect the MVPF estimate but inflates standard errors (see Appendix Figure 1.C.9a). Therefore, we do not include it in our baseline.
[20] Our estimates are virtually unchanged when we vary the discount rate (see Appendix Figure 1.C.9b).

Figure 1.6: Marginal value of public funds



Source: own calculations, Hendren and Sprung-Keyser (2020) *Notes:* Confidence intervals are based on 9,999 bootstrap draws.

the marginal value of public funds to a value of 0.80 [0.37,1.83]. Next, we add the effect of trade spillovers, which increases the willingness to pay to €1.141 billion and reduces the net costs to €1.484 billion. This increases the marginal value of public funds to a value of 0.96 [0.47,2.22]. This shows in turn that disregarding spillover effects of place-based policies can lead to substantially biased welfare conclusions. Last, we add the effect on unemployment benefits and local tax revenues. These additions hardly change the marginal value of public funds (see Figure 1.6). However, it decreases precision since the unemployment and local tax revenue effects are rather noisy.[21]

After having demonstrated the importance of accounting for spillovers, we compare the MVPF of the place-based policy to the MVPF of other policies targeting a similar group of recipients. The average East German worker in the manufacturing sector is 40 years old. We select unemployment insurance and cash transfers as they target individuals of similar ages (30-40 years old) (Hendren and Sprung-Keyser, 2020). Place-based policies aim at saving and creating jobs and thereby stabilizing incomes. Unemployment insurance and cash transfers (welfare benefits) come in when jobs have been lost. We take the estimates for unemployment insurance and cash transfers from the original contribution by Hendren and Sprung-Keyser (2020). Unemployment insurance and cash transfers have an average marginal value of public funds of 0.61 and 0.79, respectively, which are similar, but smaller than the GRW – in particular when accounting for the GRW's positive spillover effects.

[21] As discussed in Section 1.2, the European Regional Development Fund (ERDF) covers some of the direct policy costs. Adopting a purely national perspective and ignoring these direct costs would increase the total MVPF, including spillovers, to 1.16.

Cost per Job. As an alternative to the MVPF, we compute the cost per job created, another standard measure of the cost effectiveness of a policy. This metric has the drawback that it neglects other effects, such as workers forgoing unemployment benefits. Nevertheless, the measure is easy to interpret and allows a comparison with estimates from the previous literature. To calculate the cost per job, we take the estimate of the number of jobs created in manufacturing and other sectors as well as the direct government costs from our marginal value of public funds exercise and take the quotient. Appendix Figure 1.A.11 shows the results for three scenarios. The cost per job is relatively high at €44,412 [€17,446,€114,028] if one neglects all spillover effects. Including sectoral spillover effects substantially reduces the costs per job to €27,113 [€10,250,€50,490] since both the number of jobs increase as well as the net government costs decrease. Accounting for trade spillover causes the estimate to decrease even further to €24,194 [€9,680,€50,981].

1.6.2 Implications for Regional Inequality

In a last step, we investigate the effectiveness of the GRW to achieve its politically stated goal, i.e. to reduce regional inequality. In a first step, we calculate the coefficient of variation of the county-level labor income a measure of regional inequality as suggested in Ehrlich and Overman (2020). We calculate the labor income per capita in county c and year t from the BHP and SIAB data as follows.

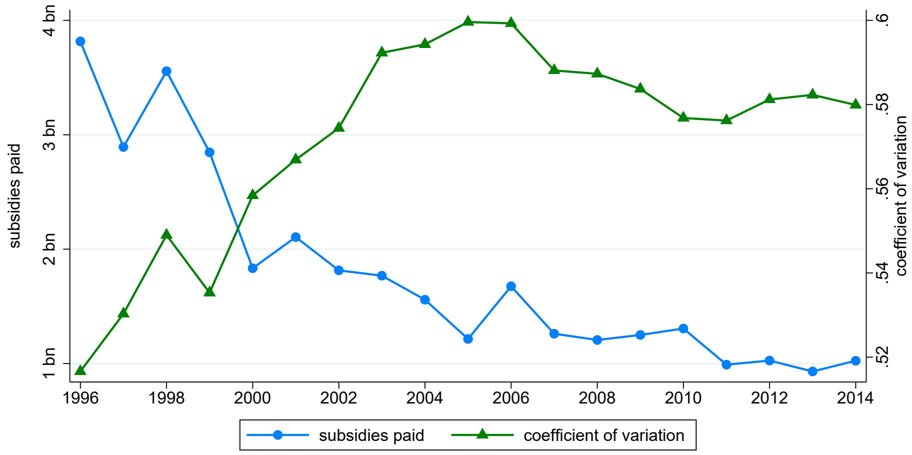
$$\text{labor income per capita}_{ct} = \frac{1}{N_{ct}} \sum_{s \in S} jobs_{cst} \cdot wage_{cst} \tag{1.9}$$

where N_{ct} denotes population in county c and year t and s stands for sector from set $S = \{\text{manufacturing, retail, construction, other}\}$. Moreover, $jobs_{cst}$ and $wage_{cst}$ are the number of jobs and the average wage in county c, sector s and year t, respectively. Figure 1.7 shows that regional inequality has increased from the mid-1990s until recent years. At the same time, the generosity of the GRW as measured by annual spending has been decreasing over time (see Figure 1.7).

We are going to investigate how much of the increase in regional inequality could potentially be reversed by increasing the subsidy rate for low-income counties. Clearly, we cannot causally link the decrease in the generosity of the GRW to changes in inequality, but we can approximate its potential to mitigate the increase by extrapolating from our causal reduced-form evidence derived in Section 1.5 and using regional distributions of jobs and wages per sector in 2014. We simulate the GRW effect on regional inequality under various counterfactuals. In our baseline counterfactual, we increase the subsidy rate of the bottom 20% counties (15 in total) in the East German labor income distribution to their 1996 level.[22] This corresponds to a 21 percentage point increase of the

[22] We also repeat the same exercise for the bottom 15% and 25% of the labor income distribution (see Appendix Figures 1.C.10a and 1.C.10b). The resulting patterns are very similar to our baseline scenario.

Figure 1.7: GRW Subsidies and regional inequality over time in Germany

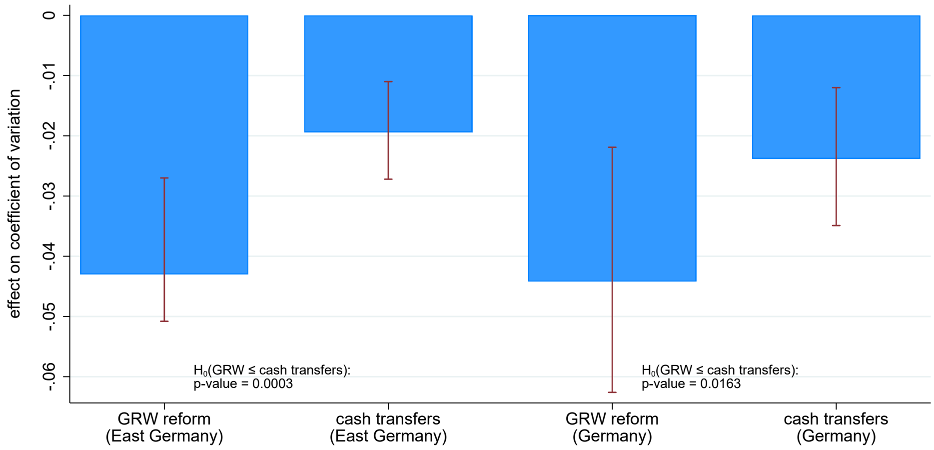


Source: SIAB, BHP, Federal Ministry for Economic Affairs *Notes:* The coefficient of variation is computed with respect to the labor income per capita as calculated in equation (1.9).

subsidy rate for these 15 counties on average. We apply the same methodology as in the previous sections to calculate the additional manufacturing, retail and construction jobs from year 0 to year 10. We also calculate the number of jobs created through trade linkages in all East German counties by multiplying their import exposure to the treatment counties with our trade spillover estimate. In order to calculate the counterfactual regional dispersion under this regime, we modify equation (1.9) to account for these additional jobs in the treated counties. Comparing this counterfactual regional dispersion to the actually observed regional inequality in 2014 indicates by how much an increase in the GRW could curb regional inequality. We calculate 95% confidence intervals by bootstrapping the procedure. As Figure 1.8 shows, expanding the GRW in this way reduces the coefficient of variation in 2014 by 0.0430 [0.0270,0.0508], or about 7%.

Next, we simulate the counterfactual of a revenue-neutral, place-blind policy and calculate its impact on regional inequality. To that end, we uniformly increase welfare payments to every welfare recipient, independent of location. First, we calculate the net cost of the GRW policy by dividing the willingness to pay for the policy by the marginal value of public funds that we calculated in Section 1.6.1, yielding total costs of €0.681 billion. Taking into account that increased payments to the unemployed could have indirect costs on the government budget, for example by reducing labor supply, we need to adjust the amount spend. In our baseline, we are conservative and assume that the efficiency cost of the transfers are equal to the place-based policy even though the

Figure 1.8: Counterfactual regional inequality



Source: BHP, SIAB *Notes:* The first bar displays the effect of an increase in the GRW subsidy back to 1996 levels for counties in the bottom 20% of the labor income distribution on regional inequality within East Germany. The second bar displays the effect a revenue-neutral policy that pays cash transfers to all unemployed within East Germany. The third and forth bar show the effects of the two policies if they were applied to Germany as a whole. East Germany excludes Berlin. Confidence intervals are based on 9,999 bootstrap draws. The p-values refer to one-sided tests whether the effect of the GRW policy is larger than the effect of cash transfers.

MVPF of cash transfers provided by Hendren and Sprung-Keyser (2020) is somewhat lower.[23] This yields efficiency-adjusted costs of €0.668 billion.

Next, we divide the adjusted costs by the number of unemployed in East Germany and assign each unemployed person this amount as a cash transfer. In this policy counterfactual, cash transfers have a presented discounted value of €1,076. We simulate that in this counterfactual the coefficient of variation decreases by 0.0194 [0.0110,0.0272], which is substantially lower than the effect of the GRW policy (see Figure 1.8). This is due to the place-blind nature of the cash transfer policy which captures spatial inequality only in so far that the number of unemployed is higher in poorer areas. Since the counterfactual policy also increases the income of recipients in relatively rich regions, the effect on regional inequality is much smaller compared to the place-specific policy.

So far both counterfactual have been targeted at East Germany. In a last step, we also simulate how the two simulated policies would affect Germany as whole. As before, we increase the GRW subsidy by the same amount for the bottom 20% of the overall German labor income distribution in 2014. This corresponds to a 21 percentage points increase for 80 counties. The total cost of such a policy would be €4.579 billion, which we again adjust by the marginal value of public funds of our GRW policy to €4.487 billion. Dividing by the total number of unemployed in Germany in 2014 yields a presented discounted cash transfer of €1,548 per unemployed. As Figure 1.8 shows, the pattern is

[23] In Appendix Figure 1.C.11a, we apply the MVPF of 0.79 that Hendren and Sprung-Keyser (2020) find, which further strengthens our results.

very similar when we extend the two policy counterfactuals to all German counties. The GRW policy would reduce spatial the coefficient of variation by 0.0442 [0.0219,0.0626] which equals roughly two thirds of the increase in regional inequality we observe from 1996 to 2014. We find a very similar pattern when we use the Gini coefficient as an alternative measure of inequality (see Appendix Figure 1.C.11b). We also investigate the role of spillover on the effect of the policy on regional inequality. Appendix Figure 1.A.12 shows that without accounting for any spillover effects the effect on regional inequality is substantially lower. Adding sectoral spillover further reduces regional inequality as it accounts for additional jobs created in poor regions. The effect of trade spillover is ex ante ambiguous. On the one hand, if poorer regions generally have a higher trade exposure to other poorer regions, it would further reduce regional inequality. On the other hand, if poorer regions are disproportionally connected to richer regions, trade spillovers dampen the reduction in regional inequality. We find that for East Germany the first case applies, while for all of Germany the latter applies. In general, the overall impact of trade spillover is rather modest in size. Last, we also compare the effect of both policies on the gap in labor earnings between East and West Germany as it was a stated goal of the GRW policy to equalize living conditions between the two. According to the BHP and SIAB data in 2014, East Germans have a 33.29% lower labor income per capita than West Germans. The GRW policy would reduce the gap by 4.97 [2.73,7.15] percentage points, whereas the reform policy would reduce the gap only by 2.99 [1.50,4.43] percentage points.

1.7 Conclusions

We investigate the direct, spillover and welfare effects of regional firm subsidies. Investigating the case of investment subsidies predominantly paid to manufacturing firms in East Germany after reunification, we exploit substantial variation in maximum subsidy rates for identification. First, we find that the place-based policy has a strong local effect: a cut in the subsidy rate has a sizable and robust negative effect on local manufacturing employment. A one-percentage-point decrease in the maximum subsidy rate leads to a decrease in manufacturing employment of 1% ten years after the reform. While wages remain unaffected, local unemployment increases. We provide evidence that policy effects are symmetric, such that subsidy rate increases lead to higher levels of manufacturing employment.

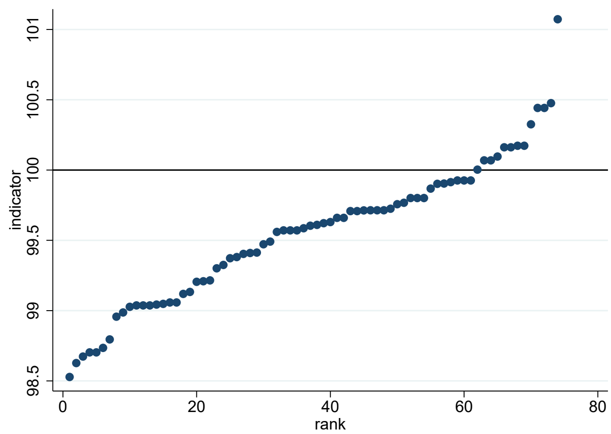
In a second step, we go beyond the effect on treated firms in treated counties and investigate various spillover effects. We find evidence for local multiplier effects in the untreated construction and retail sectors, in which employment also drops as a consequence of the reduction in the subsidy. Our estimates suggest that one lost manufacturing job implies 0.64 jobs lost in the retail and construction sectors. Counties with a high trade exposure to the treated counties also experience a slight decline in manufacturing employment. In terms of regional spillovers, we do not find any evidence for reallocation of labor within the local labor market. Last, we find that local policy makers increase the business and property tax in response to subsidy cuts. We use the framework by Hendren and Sprung-Keyser (2020) to show that the efficiency of the place-based policy is similar to unemployment insurance of welfare cash transfers when ignoring these spillover effects. Accounting for spillovers makes the regional subsidy slightly more cost-effective. Moreover, we show that the place-based policy is favorable in reducing regional inequality compared to place-blind cash transfers.

In the light of the increase in regional inequality observed in many developed countries, place-based firm subsidies could play a role to mitigate regions further drifting apart. In this respect, this chapter adds to a recent set of papers demonstrating the positive welfare effects of place-based policies. For instance, Austin, Glaeser, and Summers (2018) argues that place-based policies are more targeted. Fajgelbaum and Gaubert (2020) demonstrate that place-based policies can increase spatial efficiency because sorting off high-skilled workers is inefficient. Finally, Gaubert, Kline, and Yagan (2021) show that place-based redistribution is favorable compared to place-blind policies like income taxes when society favors spatial equity. This chapter provides further evidence for this case. For Germany, a country where the goal of spatial equity is referred to in the constitution, we show that targeted place-based policies have important spillovers that go beyond traditional agglomeration forces.

Appendix

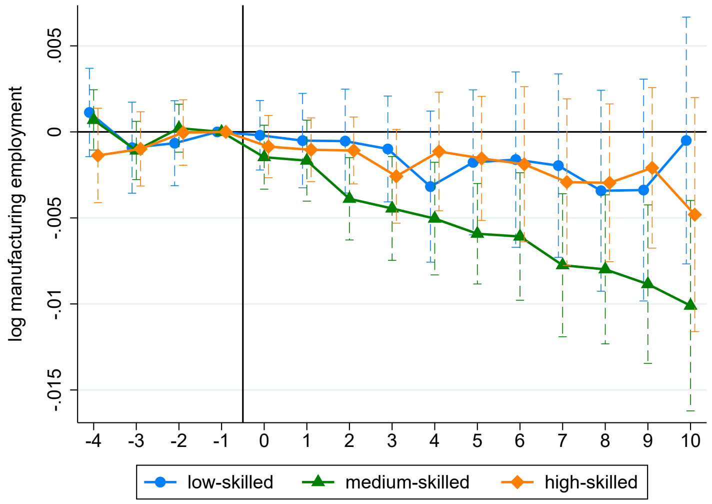
1.A Appendix

Figure 1.A.1: Ranking of counties based on the indicator (year 1997)



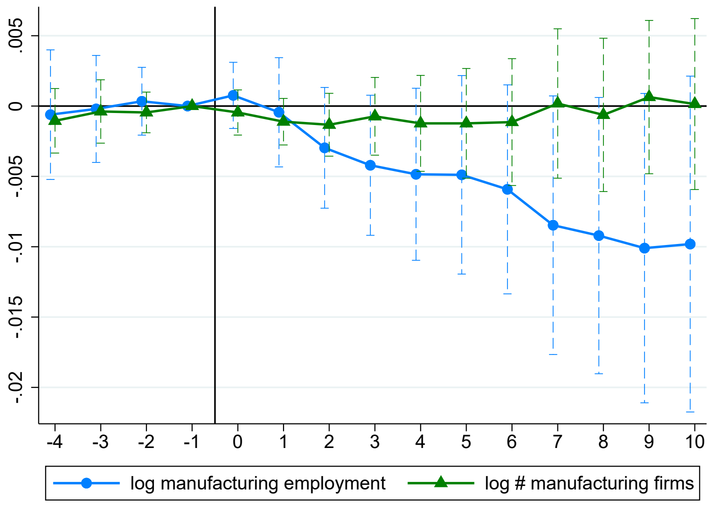
Source: Federal Ministry for Economic Affairs. *Notes*: This figure plots indicator values and the ranks of counties in the year 1997. The cutoff was formally at indicator value 100.

Figure 1.A.2: Event study estimates: manufacturing employment by skill



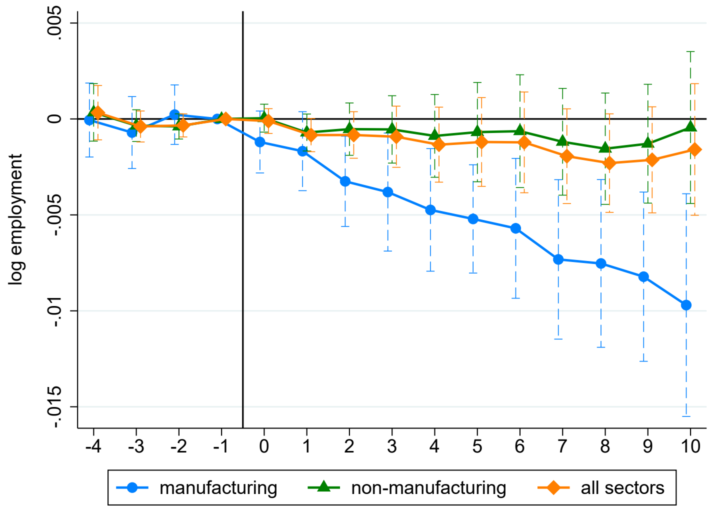
Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment by skill on leads and lags of a change in the maximum assistance rate as in equation (1.5). The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level.

Figure 1.A.3: Event study estimates: number of manufacturing establishments and county-level manufacturing employment



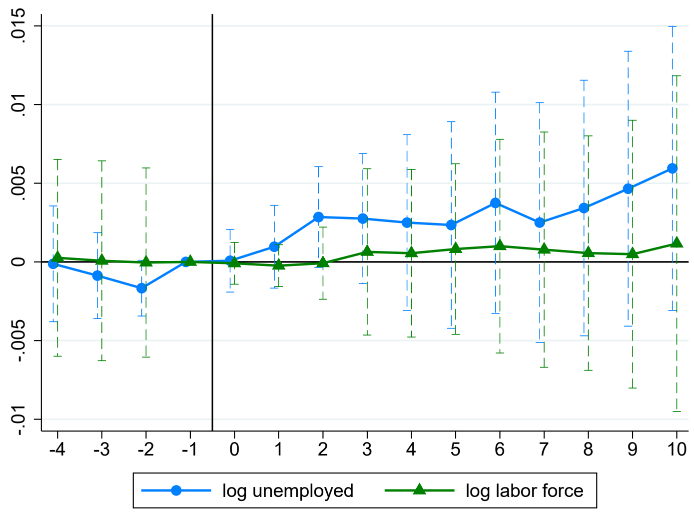
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log number of manufacturing establishments and log manufacturing employment at the county level on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level.

Figure 1.A.4: Event study estimates: total and non-manufacturing employment



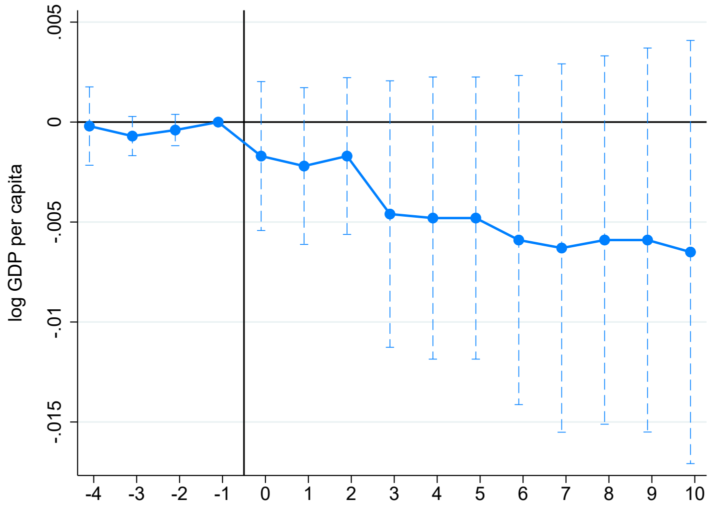
Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log industry employment on leads and lags of a change in the maximum assistance rate as in equation (1.5). The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level.

Figure 1.A.5: Event study estimates: unemployed and labor force



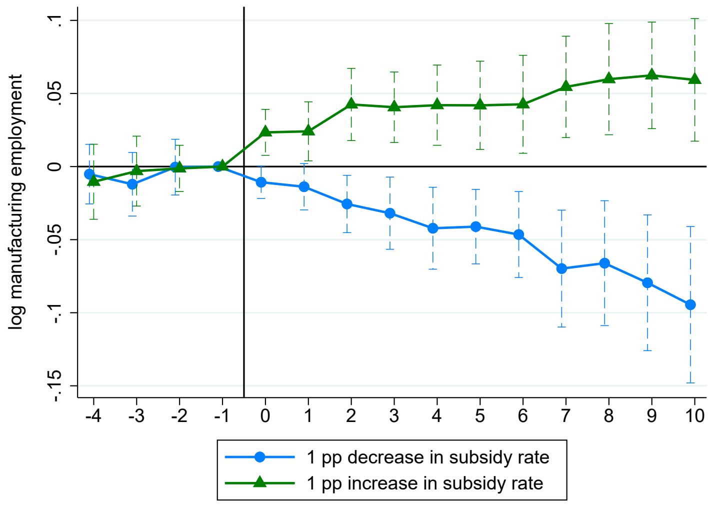
Source: Statistical Offices of German States *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log unemployed and log labor force on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level.

Figure 1.A.6: Event study estimates: GDP per capita



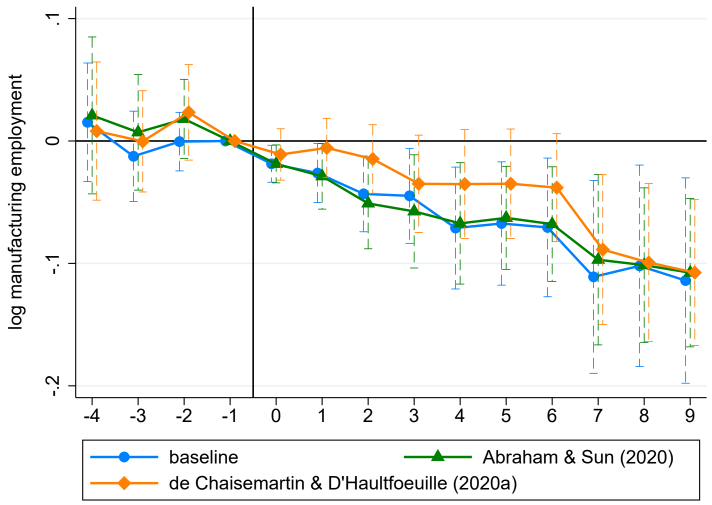
Source: Statistical Offices of German States *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log GDP per capita on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level.

Figure 1.A.7: Event study estimates: manufacturing employment (increases & decreases)



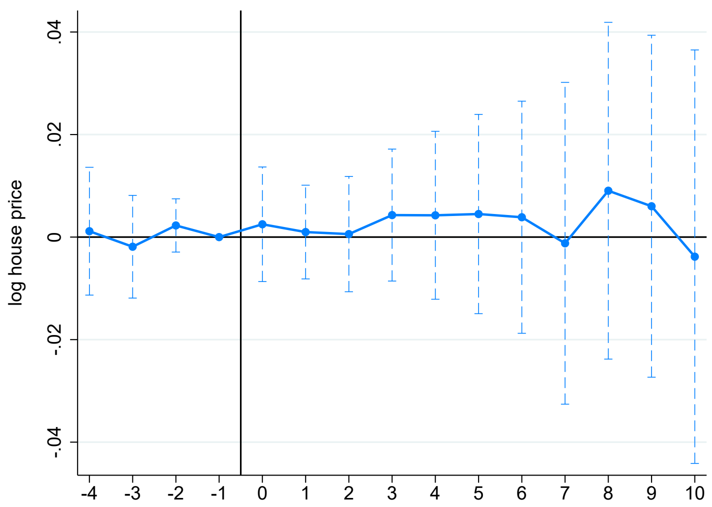
Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate. Treatment is discrete as in equation (1.1) and we include separate dummies for increases and decreases in the subsidy rate. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level.

Figure 1.A.8: Event study estimates: heterogeneous treatment effects



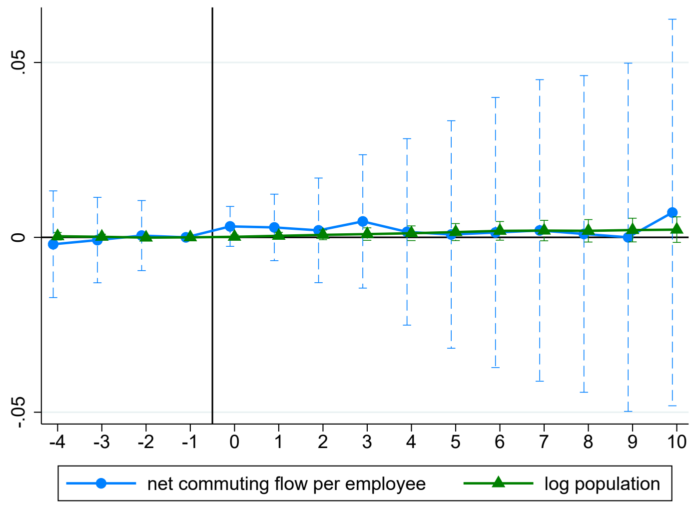
Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of the methods developed in Chaisemartin and D'Haultfoeuille (2020a) and Sun and Abraham (2020) used on equation (1.1) with manufacturing employment as the outcome. We cut the sample in 2006 for all three estimators to only have one treatment per unit and retain never-treated units. We implement the estimator from Chaisemartin and D'Haultfoeuille (2020a) using the Stata command *did_multipleGT* and obtain standard errors through 999 bootstrap iterations. The sample includes the 55 counties closest to cutoffs (M=30). Standard errors are clustered at the county level.

Figure 1.A.9: Event study estimates: housing prices



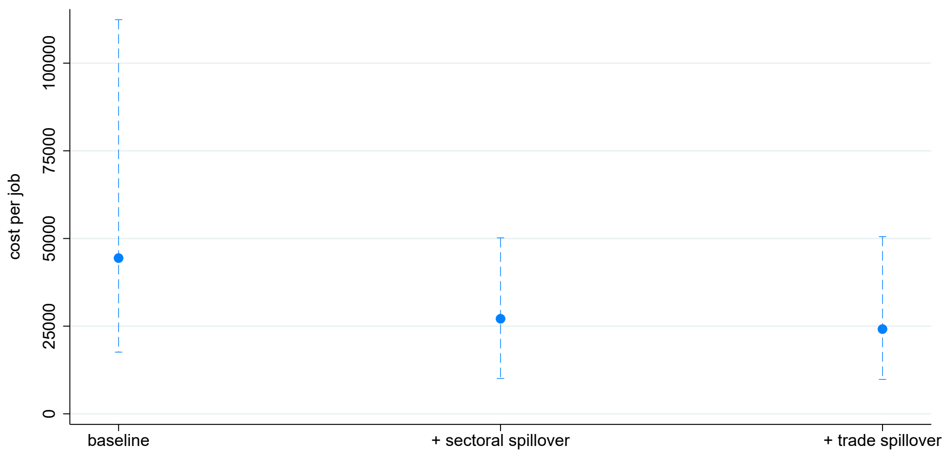
Source: IVD *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log housing price on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level.

Figure 1.A.10: Event study estimates: population and commuting flows



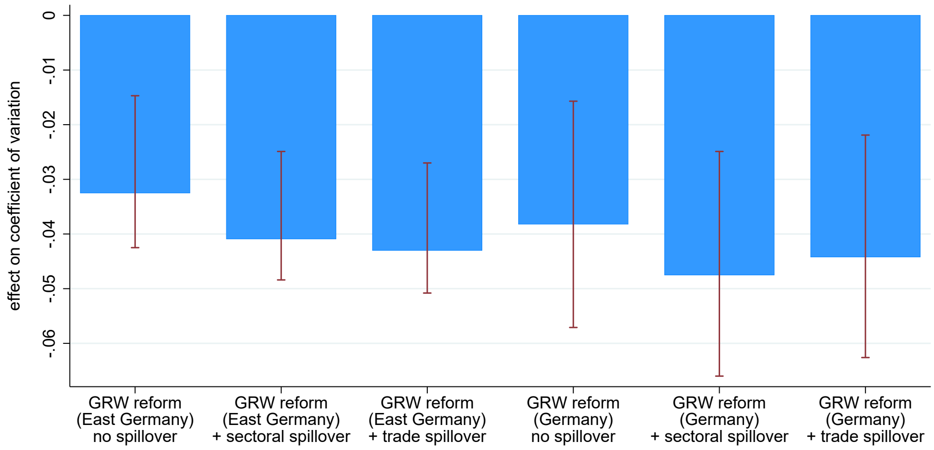
Source: Statistical Offices of German States, Federal Office for Building and Regional Planning *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log population and the inverse hyperbolic sine of the net commuting flow per employee on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level.

Figure 1.A.11: Cost per job



Source: own calculations *Notes:* Confidence intervals are based on 9,999 bootstrap draws.

Figure 1.A.12: Counterfactual regional inequality: the role of spillover



Source: BHP, SIAB *Notes:* The first bar displays the effect of an increase in the GRW subsidy back to 1996 levels for counties in the bottom 20% of the labor income distribution on regional inequality within East Germany without accounting for any spillover. The second and third bar add trade and sectoral spillover, respectively. The forth, fifth and sixth bar show the effects of these scenarios if they were applied to Germany as a whole. East Germany excludes Berlin. Confidence intervals are based on 9,999 bootstrap draws.

1.B Data and Institutions

1.B.1 Data

Table 1.B.1: Definition of variables and data sources

	year	description	source
plant level			
total investment	1996 - 2016	Total investment normalized to 2010 € on the plant-level for manufacturing plants with 20 or more employees.	AFiD
equipment investment	1996 - 2016	Equipment investment normalized to 2010 € on the plant-level for manufacturing plants with 20 or more employees.	AFiD
employees: manufacturing	1996 - 2017	Total number of manufacturing employees at the plant level.	BHP
employees: low-skill manufacturing	1996 - 2017	Number of manufacturing employees with a lower secondary, intermediate secondary or upper secondary school leaving certificate, but no vocational qualifications at the plant level.	BHP
employees: medium-skill manufacturing	1996 - 2017	Number of manufacturing employees with a lower secondary, intermediate secondary or upper secondary school leaving certificate and a vocational qualification at the plant level.	BHP
employees: high-skill manufacturing	1996 - 2017	Number of manufacturing employees with a degree from a university of applied sciences or a university at the plant level.	BHP
employees: retail	1996 - 2017	Total number of retail employees at the plant level.	BHP
employees: construction	1996 - 2017	Total number of construction employees in at the plant level.	BHP
employees: non-manufacturing	1996 - 2017	Total number of non-manufacturing employees at the plant level.	BHP
employees: all	1996 - 2017	Total number of employees at the plant level.	BHP
county level			
employees: manufacturing	1996 - 2017	Total number of manufacturing employees at the county level	BHP
plants: manufacturing	1996 - 2017	Total number of manufacturing plants at the county level.	BHP
GRW subsidies	1996 - 2016	Total subsidies paid out normalized to 2010 € at the county level.	Federal Ministry for Economic Affairs
subsidised investment	1996 - 2016	Total amount of investment that is subsidised by GRW funds normalized to 2010 € at the county level.	Federal Ministry for Economic Affairs
median manufacturing wage	1996 - 2014	Median wage in 2010 € of manufacturing workers at the county level. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students and interns.	SIAB

continued

Table 1.B.1 continued

	year	description	source
mean manufacturing wage	1996 - 2014	Mean wage in 2010 € of manufacturing workers at the county level. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students and interns.	SIAB
median manufacturing wage: low-skill	1996 - 2014	Median wage in 2010 € of manufacturing workers with a lower secondary, intermediate secondary or upper secondary school leaving certificate, but no vocational qualifications at the county level. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students and interns.	SIAB
median manufacturing wage: medium-skill	1996 - 2014	Median wage in 2010 € of manufacturing workers with a lower secondary, intermediate secondary or upper secondary school leaving certificate and a vocational qualification. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students and interns.	SIAB
median manufacturing wage: high-skill	1996 - 2014	Median wage in 2010 € of manufacturing workers with a degree from a university of applied sciences or a university. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students and interns.	SIAB
median non-manufacturing wage	1996 - 2014	Median wage in 2010 € of non-manufacturing workers at the county level. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students and interns.	SIAB
median wage	1996 - 2014	Median wage in 2010 € of workers at the county level. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students and interns.	SIAB
unemployed	1997 - 2014	Number of unemployed at the county level.	Statistical Offices of the German States
population	1997 - 2017	Total population at the county level.	Statistical Offices of the German States
labor force	1997 - 2017	Sum of unemployed and employed at the county level.	Statistical Offices of the German States
GDP per capita	1997 - 2017	GDP per capita normalized to 2010 € at the county level.	Statistical Offices of the German States
other investment subsidies	1997 - 2017	Sum of all other investment subsidies received by municipalities aggregated to the county level.	Statistical Offices of the German States
local business tax: multiplier	1997 - 2017	Average local business tax multiplier weighted with the 1995 population at the county level.	Statistical Offices of the German States
local property tax: multiplier	1997 - 2017	Average local property tax multiplier weighted with the 1995 population at the county level.	Statistical Offices of the German States
local business tax: revenues	1997 - 2017	Local business tax revenues aggregated to the county level and normalized to 2010 €.	Statistical Offices of the German States
local property tax: revenues	1997 - 2017	Local property tax revenues aggregated to the county level and normalized to 2010 €.	Statistical Offices of the German States
local business tax: base	1997 - 2017	Local business tax base normalized to 2010 € and obtained by dividing the local business tax revenues by the product of the local business tax multiplier and the federal business tax rate (*Steuermesszahl*).	Statistical Offices of the German States
local property tax: base	1997 - 2017	Local property tax base normalized to 2010 € and obtained by dividing the local property tax revenues by the product of the local property tax multiplier and the federal property tax rate (*Steuermesszahl*).	Statistical Offices of the German States
net commuting flow per employee	1997 - 2017	Net number of commuters normalized with the number of employees at the county level.	Federal Office for Building and Regional Planning
house price	1996 - 2012	House price index of the largest city within a county. We linearly impute occasionally missing data points. For some county-year pairs no data is available.	Immobilienverband Deutschland
trade flows	2010	Import and export flows between all German counties as well as foreign countries measured in tons per year.	Federal Ministry of Transport and Digital Infrastructure
local labor market level			
employees: manufacturing	1996 - 2017	Total number of manufacturing employees at the local labor market-level.	BHP

Notes: This table provides details on the definition and sources for all variables used.

Table 1.B.2: Descriptive statistics

variable	mean	sd	N	years
plant level				
total investment (in thousand €)	931.16	7176.99	124754	1996 - 2016
equipment investment (in thousand €)	795.32	6618.93	124754	1996 - 2016
employees: manufacturing	21.82	87.53	407694	1996 - 2017
employees: low-skill manufacturing	1.52	8.59	407694	1996 - 2017
employees: medium-skill manufacturing	17.42	68.81	407694	1996 - 2017
employees: high-skill manufacturing	2.67	17.65	407694	1996 - 2017
employees: retail	7.82	21.87	897327	1996 - 2017
employees: construction	8.78	23.20	560518	1996 - 2017
employees: non-manufacturing	10.68	56.39	4055878	1996 - 2017
employees: all	11.70	60.00	4463572	1996 - 2017
county level				
employees: manufacturing	5319.71	3850.82	1672	1996 - 2017
plants: manufacturing	243.84	159.58	1672	1996 - 2017
GRW subsidies (in million €)	18.39	27.54	1596	1996 - 2016
subsidised investment (in million €)	83.90	140.60	1596	1996 - 2016
median manufacturing wage	1894.95	299.55	1444	1996 - 2014
median manufacturing wage: low-skill	1480.08	573.24	1424	1996 - 2014
median manufacturing wage: medium-skill	1925.31	272.36	1444	1996 - 2014
median manufacturing wage: high-skill	3420.31	635.79	1444	1996 - 2014
median non-manufacturing wage	1647.77	163.58	1444	1996 - 2014
median wage	1700.38	145.27	1444	1996 - 2014
population	173891.30	96067.54	1672	1996 - 2017
local business tax: multiplier	357.06	45.30	1672	1996 - 2017
local property tax: multiplier	375.26	61.06	1672	1996 - 2017
local business tax: revenues (in million €)	10.80	9.03	1672	1996 - 2017
local property tax: revenues (in million €)	4.29	2.52	1672	1996 - 2017
local business tax: base (in million €)	72.90	63.50	1672	1996 - 2017
local property tax: base (in million €)	32.65	17.31	1672	1996 - 2017
net commuting flow per employee	-0.13	0.21	1596	1997 - 2017
unemployed	13833.10	8588.68	1444	1996 - 2014
labor force	87131.02	52498.05	1672	1996 - 2017
GDP per capita	16901.04	2259.06	1672	1996 - 2017
other investment subsidies (in million €)	63.43	38.68	988	1996 - 2009
house price (in 1,000 €)	146.87	42.20	797	1996 - 2012
local labor market level				
employees: manufacturing	7628.27	5457.74	1166	1996 - 2017

Notes: There are 76 counties in East Germany (excluding Berlin) according to 2014 county definitions. All monetary variables are expressed in 2010 €. For sources and definitions

1.B.2 Institutions

Indicator formulas The following formulas describe the indicator used to evaluate the economic performance of commuting zone r across regimes

$$
\begin{aligned}
indicator_r^{1997} &= (infr_r^{1995})^{0.1} \times (wage_r^{1995})^{0.4} \times (unemp_r^{1995})^{0.5} \\
indicator_r^{2000} &= (infr_r^{1999})^{0.1} \times (wage_r^{1997})^{0.4} \times (unemp_r^{1996-1998})^{0.4} \times (empforecast_r)^{0.1} \\
indicator_r^{2007} &= (infr_r^{2005})^{0.05} \times (wage_r^{2003})^{0.4} \times (unemp_r^{2002-2005})^{0.5} \times (empforecast_r)^{0.05}
\end{aligned}
$$

where $infr_r^t$ measures the quality of a region r's infrastructure assessed at time t, $wage$ represents per-capita earnings, $unemp$ the average unemployment rate, and $empforecast$ is an employment rate projection.

Construction of cutoff samples Tables 1.B.3, 1.B.4 and 1.B.5 illustrate the indicator rankings and cutoffs for the years 1997, 2000 and 2011. We do not use the rankings of the 2007 reform since all East German counties were treated. When counties merge, we take the average of the individual counties' indicators.

Table 1.B.3: Counties around the cutoff (year 1997)

county	indicator	priority group
...		
Mittelsachsen	99.725	high
Gotha	99.757	low
Zwickau	99.767	high
Magdeburg	99.801	high
Jerichower Land	99.801	high
Boerde	99.801	high
Ludwigslust-Parchim	99.868	low
Salzlandkreis	99.902	low
Rostock	99.904	high
Chemnitz	99.914	high
Spree-Neiße	99.926	high
KS Cottbus	99.926	high
Dahme-Spreewald	99.926	low
Halle (Saale)	100.003	low
Landkreis Leipzig	100.069	low
Nordsachsen	100.069	low
Schwerin	100.096	low
Weimarer Land	100.162	low
Weimar	100.162	low
Sömmerda	100.173	low
Erfurt	100.173	low
Meissen	100.326	low
Saale-Holzland-Kreis	100.442	low
Jena	100.442	low
Leipzig	100.476	low
Dresden	101.073	low

Source: Federal Ministry for Economic Affairs.

Table 1.B.4: Counties around the cutoff (year 2000)

county	indicator	priority group
...		
Hildburghausen	99.724	high
Suhl	99.724	high
Eichsfeld	99.728	high
Gotha	99.742	low
Vogtlandkreis	99.752	high
Jerichower Land	99.765	high
Cottbus	99.774	high
Spree-Neiße	99.774	high
Dahme-Spreewald	99.774	low
Bautzen	99.813	low
Saale-Orla-Kreis	99.854	high
Teltow-Fläming	99.856	low
Zwickau	99.884	low
Rostock	99.902	high
Nordwestmecklenburg	99.951	high
Chemnitz	100.008	low
Ludwigslust-Parchim	100.034	low
Boerde	100.070	low
Magdeburg	100.070	low
Nordsachsen	100.083	low
Weimar	100.144	low
Weimarer Land	100.144	low
Wartburgkreis	100.151	low
Eisenach	100.151	low
Halle (Saale)	100.169	low
Saechsische Schweiz-Osterzgebirge	100.177	low
Sonneberg	100.181	low
Erfurt	100.246	low
Sömmerda	100.246	low
Jena	100.256	low
Saale-Holzland-Kreis	100.256	low
Landkreis Leipzig	100.377	low
Schwerin	100.388	low
Meissen	100.444	low
Potsdam-Mittelmark	100.496	low
Leipzig	100.563	low
Dresden	101.117	low

Source: Federal Ministry for Economic Affairs.

Table 1.B.5: Counties around the cutoff (year 2011)

county	NUTSII region	priority group	GDP per capita
...			
Magdeburg, Stadt	Magdeburg	high	20,822€
Jerichower Land	Magdeburg	high	20,822€
Altmarkkreis Salzwedel	Magdeburg	high	20,822€
Boerde	Magdeburg	high	20,822€
Harz	Magdeburg	high	20,822€
Salzlandkreis	Magdeburg	high	20,822€
Stendal	Magdeburg	high	20,822€
Vogtlandkreis	Chemnitz	high	20,914€
Chemnitz, Stadt	Chemnitz	high	20,914€
Zwickau	Chemnitz	high	20,914€
Mittelsachsen	Chemnitz	high	20,914€
Erzgebirgskreis	Chemnitz	high	20,914€
Mansfeld-Suedharz	Halle	low	21,228€
Burgenlandkreis	Halle	low	21,228€
Halle (Saale), Stadt	Halle	low	21,228€
Saalekreis	Halle	low	21,228€
Elbe-Elster	Brandenburg-Suedwest	low	22,572€
Cottbus	Brandenburg-Suedwest	low	22,572€
Teltow-Flaeming	Brandenburg-Suedwest	low	22,572€
Dahme-Spreewald	Brandenburg-Suedwest	low	22,572€
Havelland	Brandenburg-Suedwest	low	22,572€
Brandenburg an der Havel, Stadt	Brandenburg-Suedwest	low	22,572€
Potsdam-Mittelmark	Brandenburg-Suedwest	low	22,572€
Oberspreewald-Lausitz	Brandenburg-Suedwest	low	22,572€
Spree-Neisse	Brandenburg-Suedwest	low	22,572€
Potsdam	Brandenburg-Suedwest	low	22,572€
...			

Source: Statistical Offices of German States , Deutscher Bundestag (2007).

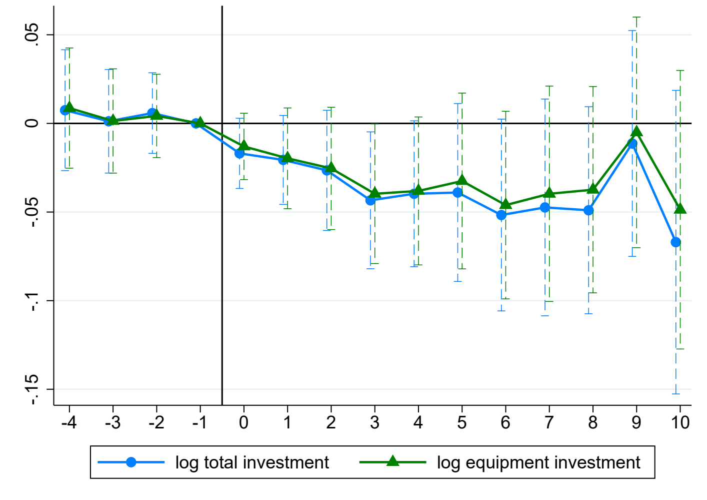
Table 1.B.6: Automatically eligible and non-eligible industries for GRW subsidies

Industries that are excluded from GRW subsidies
Agriculture, forestry and fishing
Mining
Energy and water supply
Construction
Retail except for mail order
Transportation and warehousing
Hospitals
Industries that are automatically eligible for GRW subsidies
Manufacture of chemical products
Manufacture of plastic products
Manufacture of rubber products
Manufacture of ceramic products
Manufacture of concrete products
Manufacture of concrete products
Manufacture of cement products
Manufacture of glass products
Manufacture of signs
Manufacture of iron and steel products
Manufacture of non-ferrous metals
Casting of steel and iron
Casting of non-ferrous metals
Manufacture of machinery and technical devices
Manufacture of office machines and data processing equipment
Manufacture of vehicles
Manufacture of boats
Manufacture of electronics and electric technology
Manufacture of precision engineered, optical and surgical products
Manufacture of clocks
Manufacture of sheet metal products
Manufacture of toys, jewellery, musical instruments and sports equipment
Manufacture of timber products
Manufacture of forms, tools and models
Manufacture of pulp, groundwood, paper cardboard
Manufacture of print products
Manufacture of leather products
Manufacture of shoes
Manufacture of textiles
Manufacture of clothing
Manufacture of upholstery
Production of food for sale outside of the county
Production of animal feed
Mail order
Import and export wholesale
Data processing
Administration of industry firms or supra-regional service firms
Organizing congresses
Publishers
Research and experimental development for industry firms
Legal, accounting, book-keeping and auditing activities
Market research and public opinion polling
Business and management consultancy
Laboratory services for industry firms
Logistics
Tourism

Source: Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007) *Notes*: Industries which are neither automatically eligible nor excluded from the subsidies have to show that the conditions mentioned in Section 1.2 are met.

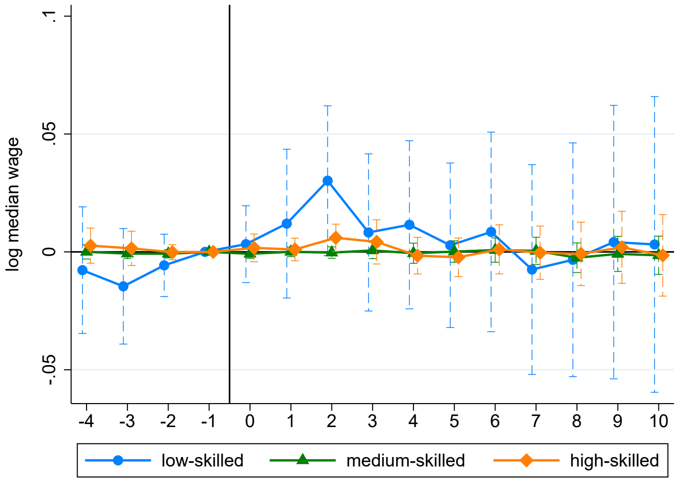
1.C Additional Results

Figure 1.C.1: Event study estimates: total & equipment investment



Source: AFiD *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log total and equipment investment on leads and lags of a change in the maximum assistance rate as in equation (1.5). The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level.

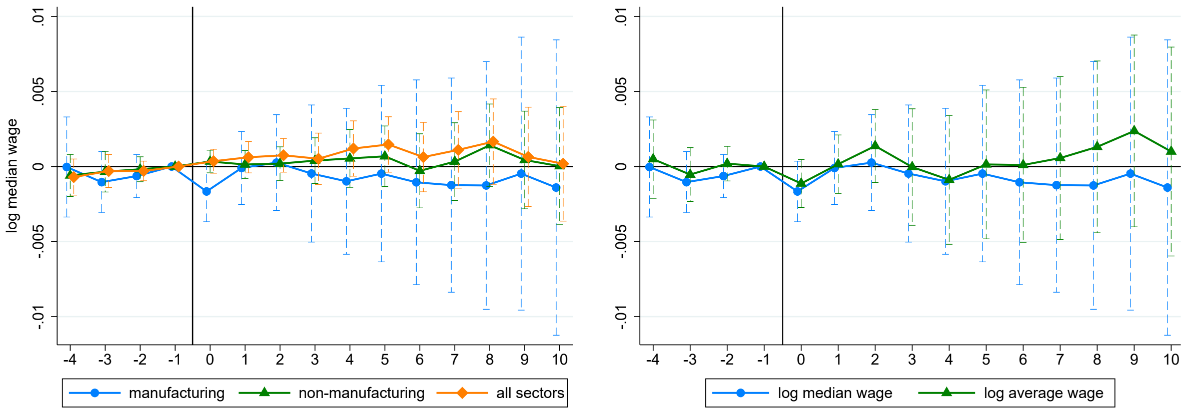
Figure 1.C.2: Event study estimates: wages by skill



Source: SIAB *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log median wages by skill level on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level.

Figure 1.C.3: Event study estimates: wages by sector and mean wages

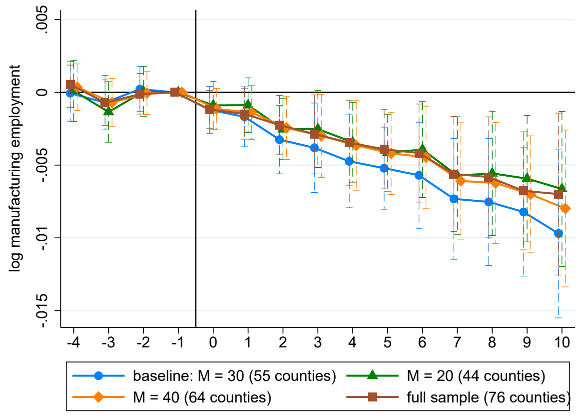
a) wages by sector **b)** average & median wage



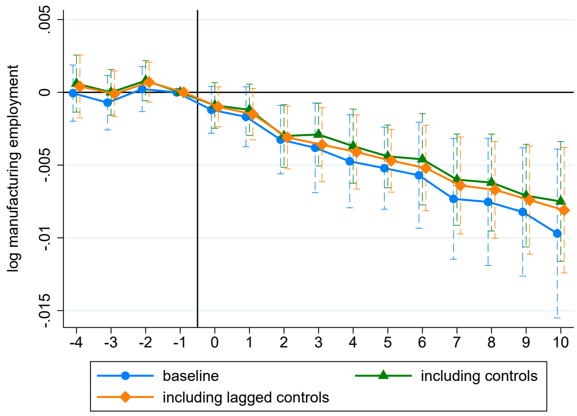
Source: SIAB *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log manufacturing wages by sector (Panel a) and log average wages (Panel b) on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level.

Figure 1.C.4: Event study estimates: manufacturing employment by cutoff sample and with controls

a) manufacturing employment by cutoff sample

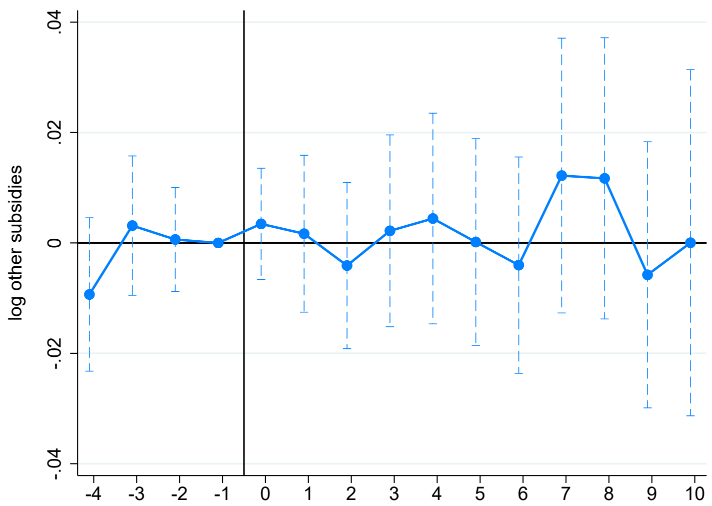


b) manufacturing employment with controls



Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate using different samples (Panel a) and including control variables (Panel b) as in equation (1.5). Clustering of standard errors is at the county and plant level.

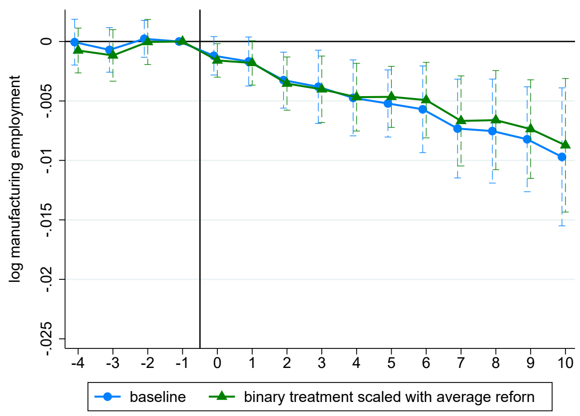
Figure 1.C.5: Event study estimates: other subsidies received



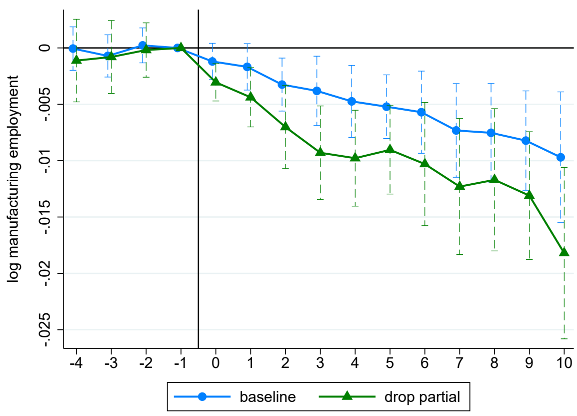
Source: Statistical Offices of German States *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log other subsidies on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level.

Figure 1.C.6: Event study estimates: manufacturing employment with binary treatment and without partially treated

a) with binary treatment



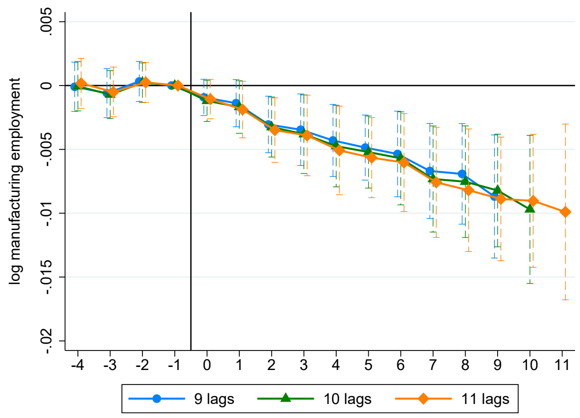
b) without partially treated counties



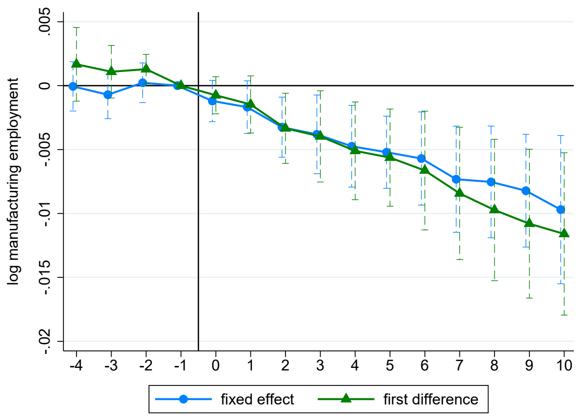
Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate with a binary treatment definition as in equation (1.1) (Panel a) and without the partially treated counties (Panel b) as in equation (1.5). Clustering of standard errors is at the county and plant level.

Figure 1.C.7: Event study estimates: manufacturing employment by number of lags and in first differences

a) by number of lags



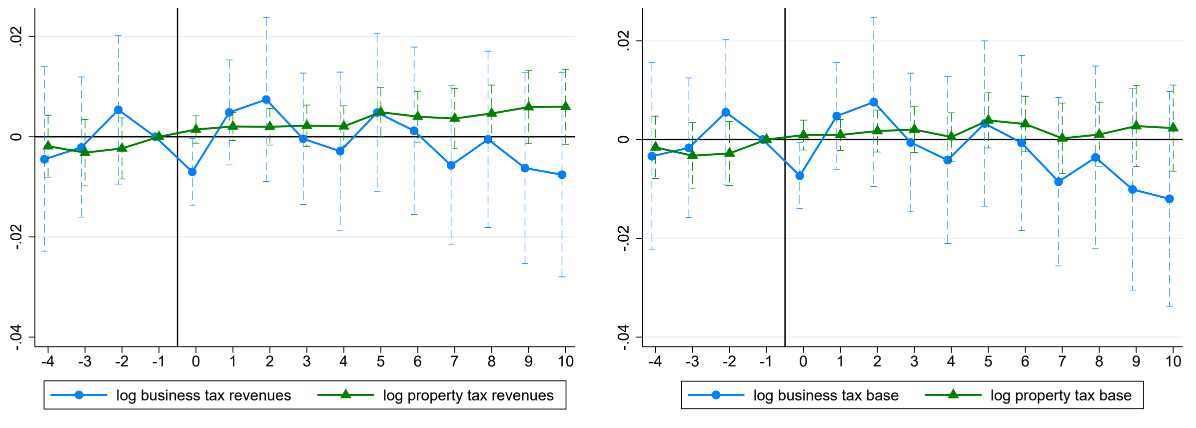
b) fixed effect & first differences



Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate with different lag windows (Panel a) and in first differences (Panel b) as in equation (1.5). Clustering of standard errors is at the county and plant level.

Figure 1.C.8: Event study estimates: local tax revenues and tax bases

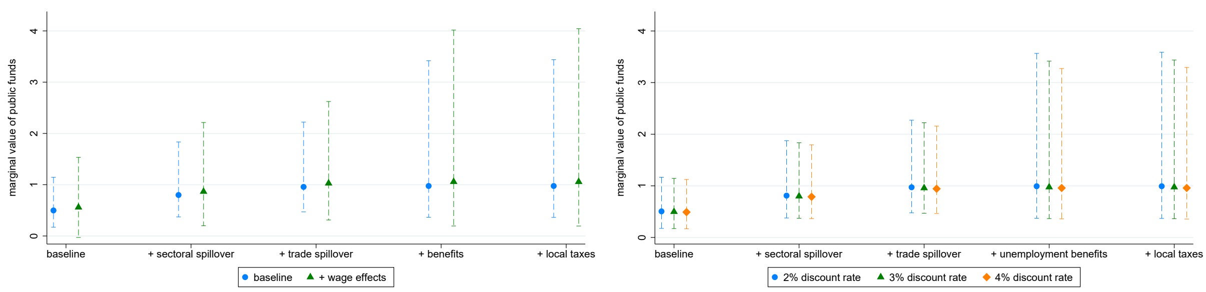
a) local tax revenues per capita **b)** local tax base per capita



Source: Statistical Offices of German States *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log local business and property tax revenues (Panel a) and the log local business per capita and property tax base per capita (Panel b) on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level.

Figure 1.C.9: Marginal value of public funds including wage effects and by discount rate

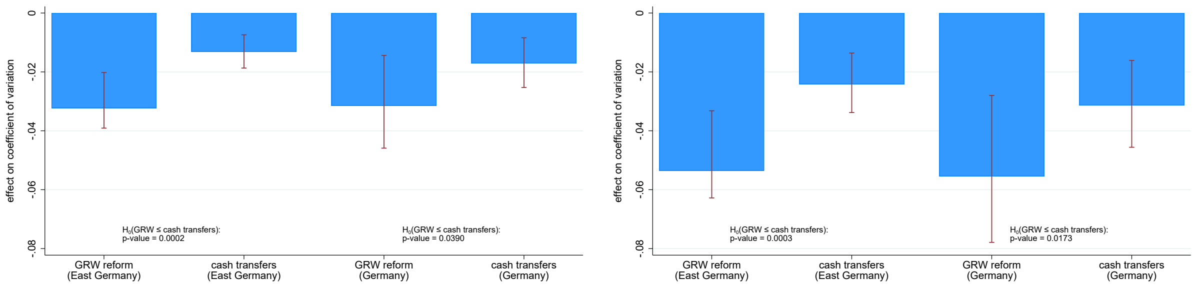
a) including wage effects **b)** by discount rate



Source: own calculations *Notes:* Confidence intervals are based on 9,999 bootstrap draws.

Figure 1.C.10: Counterfactual regional inequality: bottom 15% and bottom 25%

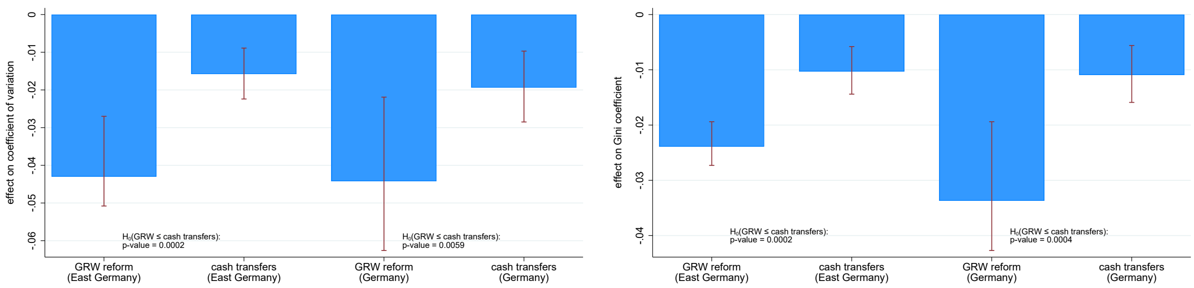
a) bottom 15% **b)** bottom 25%



Source: BHP, SIAB *Notes:* The first bar displays the effect of an increase in the GRW subsidy back to 1996 levels for counties in the bottom 15% (Panel a) and bottom 25% (Panel b) of the labor income distribution on regional inequality within East Germany. The second bar displays the effect a revenue-neutral policy that pays cash transfers to all unemployed within East Germany. The third and forth bar show the effects of the two policies if they were applied to Germany as a whole. East Germany excludes Berlin. Confidence intervals are based on 9,999 bootstrap draws. The p-values refer to one-sided tests whether the effect of the GRW policy is larger than the effect of cash transfers.

Figure 1.C.11: Counterfactual regional inequality: imposing different MVPFs and Gini coefficients

a) imposing different MVPFs **b)** Gini coefficient



Source: BHP, SIAB *Notes:* The first bar displays the effect of an increase in the GRW subsidy back to 1996 levels for counties in the bottom 20% of the labor income distribution on regional inequality within East Germany. Panel (a) does not impose that the MVPF of the GRW and cash transfer policy have to be the same and Panel (b) uses the Gini coefficient as an alternative measure of regional inequality. The second bar displays the effect a revenue-neutral policy that pays cash transfers to all unemployed within East Germany. The third and forth bar show the effects of the two policies if they were applied to Germany as a whole. East Germany excludes Berlin. Confidence intervals are based on 9,999 bootstrap draws. The p-values refer to one-sided tests whether the effect of the GRW policy is larger than the effect of cash transfers.

Chapter 2

Preferences over Taxation of High-Income Individuals: Evidence from a Survey Experiment

Joint with Dirk Engelmann, Eckhard Janeba and Lydia Mechtenberg.

2.1 Introduction

In the year 2012, president Hollande announced a 75% tax on high incomes in France. Despite being a national figure, famous French actor Gérard Dépardieu reacted by exchanging his French for the Russian citizenship. While often seen as confirming the textbook argument that globalization entails tax competition for high-income earners, this well-covered move hardly constitutes more than anecdotal evidence. Indeed, more systematic evidence is surprisingly rare and hard to generate. To be more specific, two immediate questions arise: First, are the rich really as ready to act upon their advantage and migrate for tax reasons as predicted by the textbook argument, or are Dépardieu-like migration decisions rare exceptions? The well-established concept of home bias suggests that not all would migrate when it pays in monetary terms (see, e.g., Feldstein and Horioka, 1980; Ogura, 2006). Second, do net beneficiaries of the tax-transfer system anticipate the degree of mobility of the rich and choose tax rates accordingly? The answer to the second question is not obvious since evidence in experimental public economics suggests that voters are less than rational when choosing taxes (see, e.g., Sausgruber and Tyran, 2011). Moreover, it is neither evident that the rich will migrate nor that the voters will vote as predicted, because a consensus has emerged in the literature on behavioral public economics that redistribution is driven by subjective beliefs about givers and receivers, as well as by social preferences.[1]

[1] For the role of beliefs, see, e.g., Alesina and Angeletos (2005) and Benabou and Tirole (2006). For the role of preferences, see Engelmann and Strobel (2004, 2007) and Höchtl, Sausgruber, and Tyran (2012).

Importantly, both such beliefs and preferences are heterogeneous. It is hence plausible that political ideology - as a composite of redistribution-related beliefs and preferences - may play a major role when it comes to taxing the rich and the migration choices of the rich. Almås, Cappelen, and Tungodden (2020) provide support that ideology matters for redistribution preferences. They compare in an experiment Norwegian and US citizens (two populations who arguably differ in their political ideology) when they, in the role of an unaffected third party, make distributional choices. They find that Norwegians are much more in favor of redistribution than US citizens when inequality is based on luck, but both samples do not differ in the degree to which merit makes them more inequality accepting. In a survey experiment in Sweden, Karadja, Mollerstrom, and Seim (2017) find that informing participants that they rank higher in the income distribution than they thought makes them prefer less redistribution and a more conservative party. Interestingly, this effect is entirely driven by participants who are right-of-center to begin with. Furthermore, in a class-room experiment with students as well as in surveys of politicians, Janeba (2014) finds that party preferences correlate with tax choices of students while party affiliation correlates with beliefs of politicians about tax-induced mobility of firms.

We therefore underpin our above two reasearch questions by a related triple: Do those on the left and those on the right of the political spectrum differ in their expectations regarding the mobility of the rich and therefore in their views on how progressive taxes should be? Do they even hold on to purely ideological views about appropriate taxation that are independent of their beliefs about migration of the rich? Finally, do the left-leaning and the right-leaning rich differ in their tendency to make Départdieu-like migration decisions? Answering these questions is important for evaluating the race-to-the-bottom argument that the increasing mobility of labor (and capital) leads to suboptimally low taxes and public spending (Keen and Konrad, 2013). If in practice, however, on average households migrate less and stick to more progressive taxes than what would be expected in a situation without the influence of political biases, the race to the bottom and the link between globalization and inequality are attenuated.

We address the above questions in a large incentivized survey experiment with a representative sample of subjects. It implements a stylized setting in which rich and poor voters collectively choose the tax rate to impose on the rich, conditional on which tax rate prevails in a (ficticious) neighboring country into which the rich may migrate. Embedding the experiment in the 18th wave of the German Internet Panel (GIP), a large online panel representative on observable variables of the German population aged 16 to 75, we elicit experimental tax choices, migration beliefs, and migration choices of more than 3,000 individuals.

We randomly assign participants to the roles of rich and poor by providing them with high and low endowments, respectively.[2] While the poor are always immobile, mobility of the rich varies across treatments. For them, incentives to migrate when

[2] Hereafter, we will refer to subjects in the role of the poor as "the poor" and to subjects in the role of the rich as "the rich".

mobile can be positive or negative. These migration incentives depend on the domestic tax rate that the rich and the poor collectively choose by vote for their fictitious home country and the exogenous tax rate in a fictitious neighboring country (the potential destination of rich migrants). The latter tax rate varies across treatments. After the domestic tax is chosen, the rich may migrate into the foreign country, then bearing some migration costs. Apart from voting and migration decisions, we also elicit beliefs about the choices of subjects in the other role, i.e., what the poor believe about the choices of the rich, and vice versa.

We match our experimental data with response data obtained in earlier waves of the German Internet Panel, in particular self-declared attitudes on redistribution, party adherence, and demographic variables like age, gender, and education. Using regressions, we analyze the determinants of tax and migration choices to isolate treatment effects and effects of political ideology. Eliciting beliefs of the poor about migration choices of the rich allows us to investigate whether beliefs and, once we control for these, tax choices vary with political ideology. Hence, we can separate whether possible differences in tax choices between more left-leaning and more right-leaning subjects are driven by differences in beliefs about migration choices or by differences in preferences.

Reassuringly, the roles of being poor or rich determine experimental choices while the actual positions in the income distribution do not. Hence, our experiment seems internally valid. With regard to our research questions regarding the role of political ideology, we find that it matters, although more clearly for the rich than for the poor: the left-leaning rich are less willing to migrate than the right-leaning rich when it pays in material terms. The left-leaning rich also vote for higher taxes than the right-leaning rich. These effects do not depend on how we measure ideology, as attitudes toward redistribution or as self-declared support for left-of-center or right-of-center political parties. The poor, in their turn, choose higher taxes when generally in favor of redistribution, although their party preference has no significant effect on tax choices. The effect of a pro-redistribution attitude, however, holds even when conditioning on the poor's migration beliefs. We also find that migration beliefs are not systematically related to political attitudes. Hence, we conclude that political ideology relates to preferences rather than beliefs. These findings are in line with the comparative-static predictions of a simple model of inequality aversion when the aversion parameters vary with ideology.

The comparative-statics predictions of our model are supported by the observed treatment effects. They reveal that the poor understand the migration incentives of the rich and react to these incentives when choosing domestic taxes. The rich, in their turn, also react to migration incentives but much less so than expected by the poor. By contrast, and as predicted by our model, they are willing to forego material gains to benefit their poor fellow-citizens, both when choosing domestic taxes and when deciding whether to migrate or stay. Tax choices are also more benevolent than expected for selfish agents since both poor and rich tend to vote for medium taxes. In fact, the poor tend to choose medium taxes even in the absence of migration incentives for the rich, and although they seem to understand these incentives well. Hence, the poor refrain from exploiting the rich even when exploitation is possible, and although being poor or rich in our experiment is entirely due to luck. This generosity of the poor toward the rich exceeds what can be rationalized by our model.

Finally, our work relates to a large theoretical and empirical literature on taxation and migration. As Mirrlees (1971, p. 176) famously noted, "the threat of migration is a major influence on the degree of progression in actual tax systems". Spelling out this idea in more detail, the theoretical literature robustly finds that if labor is mobile, particularly at the top of the income distribution, then tax competition between governments reduces redistribution from high-income earners to lower segments of the income distribution.[3] However, this literature largely ignores behavioral factors such as social preferences or biased beliefs. If, for instance, low-income voters are inequality-averse with respect to the inhabitants of their own country but do not take other countries into account, they may tend to vote for highly progressive taxes even if such taxes drive the top earners out of the country. Alternatively, if high-income earners are sufficiently averse towards advantageous inequality, they may refrain from migrating despite high taxes in their domestic country. Moreover, beliefs about the willingness to migrate might be biased among voters. Hence, it is important to turn to empirically testing the standard predictions about tax competition in an open political economy.

In fact, the question how taxation affects migration choices of high-income individuals has already been the subject of empirical research. For example, Kleven, Landais, and Saez (2013) analyze the role of taxes on the incentives of professional football players to play abroad. They find that the elasticity of the number of foreign players with respect to their net-of-tax rate is around one, and substantially higher for younger and top players. Qualitatively, the result is in line with Kleven et al. (2014) who estimate high elasticities of migration of foreign high-income individuals with respect to the net-of-tax rate of around 1.5 to 2 in a study of preferential income taxation in Denmark. Similarly, Muñoz (2019) finds an elasticity of above 1 in Europe for the responsiveness of the number of foreign top earners with respect to the net of tax rate. A (stock) elasticity around 1 is reported by Akcigit, Baslandze, and Stantcheva (2016) for foreign superstar inventors, while much lower elasticities prevail for domestic inventors. Agrawal and Foremny (2019) conclude that the tax-induced mobility is large within Spain, in particular in certain industries like the health sector, but with only moderate tax revenue consequences. Overall, empirical studies suggest that mobility of top earners is substantial (see Kleven et al., 2020, for an overview).

[3] The standard approach to optimal income taxation by Mirrlees (1971) applied to a closed economy situation. While also early contributions like Wilson (1980) considered the problem of optimal linear income taxation when workers are mobile, later work has advanced Mirrlees' approach. Simula and Trannoy (2010) analyze the optimal nonlinear income tax schedule when workers are mobile at a cost, while holding tax policy in the outside country fixed. Other authors have analyzed income taxation when governments of several countries compete, which is modeled as a Nash game (Bierbrauer, Brett, and Weymark, 2013; Blumkin, Sadka, and Shem-Tov, 2015; Lehmann, Simula, and Trannoy, 2014; Morelli, Yang, and Ye, 2012; Piaser, 2007).

It is difficult, however, to construct counterfactuals regarding mobility conditions with field data only; and it is difficult to correctly estimate the tendency to migrate without the construction of counterfactuals. Here, experimental work can complement standard empirical research. In an experiment, it is possible to construct counterfactuals and pin down causality for individual decisions, such as migration and voting decisions, by treatment comparisons. Hence, we experimentally test a simple model of optimal taxation and migration in the presence of inequality aversion to investigate to which extent behavioral factors such as social preferences or biased beliefs shall become part of the story.

In doing so, we contribute to a growing experimental literature on tax choices through voting (see Lorenz, Rauhut, and Kittel, 2015, p. 2, for a review of this literature). Sausgruber and Tyran (2011) ascertain that biased beliefs on price effects of taxes distort collective tax choice. Höchtl, Sausgruber, and Tyran (2012) find that inequality aversion affects democratic redistribution if and only if high-income earners are in the majority, while a poor majority does not expropriate the rich. This latter finding is in line with the (standard) model of Meltzer and Richard (1981) and resonates with recent experiments and surveys, including our own. For instance, Weinzierl (2017), in a survey among 2,500 U.S. citizen, reports that between 50 and 95 percent of respondents believe that full equalization of endowments that are due to luck would be unjust. Instead, they advocate the idea that post-tax incomes should depend on pre-tax endowments and that there is an entitlement to one's own endowments even in the absence of effort. Relatedly, Charité, Fisman, and Kuziemko (2015) report results from two experiments suggesting that subjects redistribute less when knowing pre-tax endowments of the better-off or when reference points are more deeply engrained.[4]

We contribute to this literature in two ways. First, we experimentally investigate an open economy in which the top earners can avoid excessive taxation if they migrate. Second, we relate individual tax and migration choices in our survey experiment to survey data about political attitudes and beliefs.

Our work is complementary to the literature on preferences for redistribution in the presence of immigration (see, for example, Alesina, Miano, and Stantcheva, 2018; Alesina and Stantcheva, 2020; Dahlberg, Edmark, and Lundqvist, 2012; Hainmueller and Hiscox, 2010; Martinangeli and Windsteiger, 2019). In this literature, immigration is typically taken as given and thus not endogenous to government policy in the host country. Furthermore, preferences for redistribution are elicited through unincentivized survey questions, in contrast to our approach, where participants make choices that have material consequences, albeit with a small probability. For example, using evidence from a large-scale survey in six countries, Alesina, Miano, and Stantcheva (2018) document that native respondents overestimate the number of immigrants in

[4] Casal, Grimm, and Schächtele (2019) experimentally analyze the role of preferential tax treatment of high-income earners, motivated by mobility, for the tax compliance decisions and equity perceptions of low income earners. They find that exogenously given motivations for tax preferences for the rich have negative effects on tax compliance and equity perceptions of the poor.

their country. Simply reminding respondents of immigration decreases support for redistribution. Behavioral biases thus appear to play a role in the context of immigration.

The rest of the chapter is organized as follows. In the next section, we present the simple model of redistributive taxation that mirrors the experimental implementation. In Section 2.3 we first explain in detail the setup of the survey experiment within the German Internet Panel and then discuss the hypotheses based on the comparative-static predictions of our model. We present the main results and discuss deviations from the point predictions of our model in Section 2.4. Section 2.5 concludes.

2.2 Redistributive Taxation with Inequality-Averse Agents

A country (home) is populated by two types of individuals $\{p, r\}$, called poor and rich, with exogenously given gross incomes $y_p = 20$ and $y_r = 90$. In the base case (closed economy: no mobility) the country has two poor and one rich inhabitant. We model a purely redistributive tax-transfer system. The set of feasible tax rates is limited to three - low, medium, high - with the following values: $t_L = 10$, $t_M = 20$, $t_H = 40$. A rich individual pays the tax $t \in \{t_L, t_M, t_H\}$, which is then divided among the two poor. Under a balanced government budget without other spending, the transfer to each poor individual becomes $\frac{t}{2}$. Net income as a function of the tax rate is therefore $z_p = 20 + \frac{t}{2}$ for a poor and $z_r = 90 - t$ for a rich individual. Notice that even under the highest tax the ranking of pre-tax incomes is preserved post tax and transfer, that is, $z_p < z_r$ holds under any tax.

Next, consider an open economy. Here, we allow for migration of the rich to a second country, called foreign, in which the rich earns the same gross income $y_r = 90$. The tax rate in the foreign country is exogenously given and from the same set of feasible tax rates: $t^* \in \{10, 20, 40\}$. Migration is costly for the rich, however, involving a loss of $m = 15$. If the rich emigrates to the other country, there is no tax revenue generated at home. Then, the net income of a poor in his domestic country equals his gross income: $z_p = y_p$.

As an abundant literature on social preferences has shown, experimental participants in the laboratory and the field often exhibit social preferences, i.e., they tend to care about what others earn, too. Given that our experiment involves simple redistribution choices, it makes sense to model participants' social preferences as inequality aversion, following Fehr and Schmidt (1999). The standard Fehr-Schmidt utility function for person i is given by

$$U_i(x) = x_i - \frac{\alpha_i}{n-1} \sum_{j \neq i} \max(x_j - x_i, 0) - \frac{\beta_i}{n-1} \sum_{j \neq i} \max(x_i - x_j, 0) \tag{2.1}$$

where x is the payoff vector and n the number of players in the game. Note that, by design, in our game the rich always earns more than the poor do. Hence, the poor's inequality aversion can only pertain to disadvantageous inequality (α), while the reverse holds true for the rich. The resulting utility function takes the following form (with U_p denoting the utility of the poor and U_r the utility of the rich):

$$U_p = z_p - \frac{\alpha}{2}(z_r - z_p) \tag{2.2}$$

and

$$U_r = z_r - \beta(z_r - z_p). \tag{2.3}$$

We assume that inequality aversion relates to the net income of the rich and poor and consider the migration cost m as monetary so that it enters net income $z_r = y_r - t^* - m$ if the rich migrates.

In the experiment, the tax rate is chosen by a random dictator mechanism. Hence, beliefs about the other players' tax choices are irrelevant for one's optimal choice because whenever one's own choice matters, those of the others do not. We now derive the preferred tax rate for each type in both the no-mobility and the mobility regime.

2.2.1 No-Mobility Setting

The utility level of a poor player in absence of mobility (nm) is

$$U_p^{nm} = 20 + \frac{t}{2} - \frac{\alpha}{2}\left(90 - t - \left(20 + \frac{t}{2}\right)\right) = 20 - 35\alpha + \frac{t}{2}\left(\frac{3}{2}\alpha + 1\right). \tag{2.4}$$

Utility of the poor is strictly increasing in the tax rate as long as the net income of the rich still exceeds the net income of the poor, which holds for our admissible tax rates. Hence, the preferred tax rate of a poor is $t_p = 40$. By contrast, the utility of a rich is given by

$$U_r^{nm} = 90 - t - \beta\left(90 - t - \left(20 + \frac{t}{2}\right)\right) = 90 - 70\beta + t\left(\frac{3}{2}\beta - 1\right). \tag{2.5}$$

Utility of the rich is a linear function of t, and the sign of the derivative of equation 2.5 with respect to the tax rate depends on the level of inequality aversion. For $\beta < 2/3$, the preferred tax rate is the lowest, as inequality concerns are relatively unimportant. When β is larger than 2/3, the rich favors the highest possible tax rate. For $\beta = 2/3$, the rich is indifferent between all admissible tax rates.

We summarize preferred tax rates for each type:

$$t_p^{nm} = 40 \tag{2.6}$$

and

$$t_r^{nm} = \begin{cases} 10 & \beta < 2/3 \\ \{10, 20, 40\} \text{ if} & \beta = 2/3 \\ 40 & \beta > 2/3. \end{cases} \tag{2.7}$$

2.2.2 Mobility Setting

In the mobility setting, we first analyze under which condition the rich migrates. Assuming that the domestic tax rate is t and the foreign tax rate is t^*, the rich migrates if the utility at home, U_r^h, is smaller than the utility in the foreign country, U_r^f.[5]

$$U_r^h = 90 - t - \beta\left(90 - t - \left(20 + \frac{t}{2}\right)\right) < 90 - t^* - m - \beta(90 - t^* - m - 20) = U_r^f$$
$$\iff m < \frac{t - t^* + \beta(t^* - 3t/2)}{1 - \beta} \tag{2.8}$$

The interpretation is straightforward: migration takes place when the migration costs (weighted by $1 - \beta$) are smaller than the differential between the domestic and foreign tax, adjusted by a term reflecting the difference in realized inequality between the two situations. We assume that inequality aversion is experienced with respect to the citizens of the home country, not the foreign country, even if a rich player migrates.[6] Following the standard assumption of Fehr and Schmidt (1999) that $\beta < 1$ (i.e., aversion towards advantageous inequality does not dominate concerns for own payoffs), the sign of the right-hand side of equation 2.8 depends only on the numerator. Hence, implausible migration outcomes are avoided where the rich would migrate because paying the migration costs reduces inequality. Under this assumption, it is now easy to show how migration depends on the degree of inequality aversion.

Lemma 1: Migration is weakly decreasing in the parameter for inequality aversion β of the rich:

- For $\beta \in [0, 1/5)$, the rich will migrate if and only if $t = 40$ and $t^* \in \{10, 20\}$.
- For $\beta \in [1/5, 3/7)$, the rich will migrate if and only if $t = 40$ and $t^* = 10$.
- For $\beta \in [3/7, 1)$, the rich will not migrate.

We now analyze the preferred tax rate of a rich and a poor, when all anticipate migration behavior. The rich knows their own type β, which then directly yields the migration decision in equation 2.8. A poor, by contrast, needs to form a belief about the rich type's inequality aversion. We denote this belief by $E_p[\beta]$, where E is the expectation. We note that by the same argument as for the no-mobility case, the utility of the poor is increasing in the tax rate as long as the rich does not migrate. Migration leads to lower payoffs for the poor. A poor player could hence only prefer the rich to migrate if he is so

[5] We assume that a rich person who is indifferent between migrating or not will stay in the domestic country. Under the reasonable assumption on the continuous distribution of the inequality aversion parameters, indifference is a zero probability event, so that the assumption on how the rich break indifference is immaterial.

[6] In our experimental setting, where one can influence the payoffs of the participants in the home country, but there are not actual participants in the foreign country, this is arguably plausible.

inequality-averse that the reduction of inequality that is caused by the migration costs exceeds the loss in material payoff.[7]

Preferred tax of the poor

The preferred tax rate of an extremely inequality-averse poor player ($\alpha > 4$) is 40 if the foreign tax rate is medium or high. For poor players with $\alpha \leq 4$ the preferred tax rate is 40 if either $t^* = 40$ or $t^* = 20$ and additionally $E_p[\beta] \geq 1/5$, because in both situations no migration is expected for any domestic tax rate. If $t^* = 20$ and $E_p[\beta] < 1/5$, the migration threat is credible and the highest possible tax rate subject to no migration is chosen by the poor, which is $t = 20$. Finally, when the foreign tax rate is low, the highest tax rate is preferred if the poor believes the rich to be sufficiently inequality averse, that is $E_p[\beta] \geq 3/7$. To summarize:

$$t_p^m(t^* = 40) = 40$$

$$t_p^m(t^* = 20) = \begin{cases} 40 & \text{if} \quad E_p[\beta] \geq 1/5 \;\text{ or }\; \alpha > 4 \\ 20 & \phantom{\text{if}} \quad E_p[\beta] < 1/5 \;\text{ and }\; \alpha \leq 4 \end{cases} \tag{2.9}$$

$$t_p^m(t^* = 10) = \begin{cases} 40 & \text{if} \quad E_p[\beta] \geq 3/7 \\ 20 & \phantom{\text{if}} \quad E_p[\beta] < 3/7 \end{cases}$$

Fehr and Schmidt (1999) estimate that extreme aversion towards disadvantageous inequality ($\alpha \geq 4$) pertains only to about 10% of typical experimental subject pools, which is also confirmed in a direct test by Blanco, Engelmann, and Normann (2011). We therefore expect the inequality aversion of the poor to be of minor importance, such that the poor will typically prefer tax rates that will prevent the rich from migrating.

Preferred tax of the rich

For the rich, the preferred tax rate can in principle also depend on their own migration choices and hence on the foreign tax rate. Note, however, that choosing a tax rate that would drive oneself out of the country is never optimal. Paying tax t^* abroad leads to additional costs m compared to paying $t = t^*$ at home and the additional reduction in inequality is less than m. Hence, since $\beta < 1$, paying $t = t^*$ at home is preferred over paying t^* abroad for any $\beta \in [0, 1)$. Therefore, the preferred tax rate for the rich is the

[7] This can only be the case if $t^* = 20$. If $t^* = 10$, the payoff of a rich who migrates will only be lower by 5 compared to staying at home and paying $t = 20$, but the poor would each lose 10 and hence inequality increases. Therefore, a poor player would never want a rich to migrate if the foreign tax is low. In case of the medium foreign tax, a poor player would prefer the high over the medium domestic tax, such that the rich migrates and pays migration costs if and only if $U_p(\text{rich leaves}) > U_p(\text{rich stays}) \iff 20 - \frac{\alpha}{2}(70 - 15 - 20) > 30 - \frac{\alpha}{2}(70 - 30) \iff \alpha > 4$.

same as without mobility and depends only on the rich's inequality aversion but not the foreign tax rate. To summarize, we obtain the following preferred tax:

$$t_r^m = \begin{cases} 10 & \beta < 2/3 \\ \{10, 20, 40\} \text{ if} & \beta = 2/3 \\ 40 & \beta > 2/3. \end{cases} \tag{2.10}$$

2.2.3 Comparative Statics

Based on the above insights, we can establish the following comparative statics with respect to our treatments and the inequality aversion parameters α and β.

1. **The preferred tax rate of a rich** is weakly increasing in their inequality aversion β (follows from equations 2.7 and 2.10).
2. a) **The preferred tax rate of a poor** is weakly lower when the rich is mobile and the foreign tax is low or medium compared to when the rich is not mobile or the foreign tax is high (follows from equations 2.6 and 2.9).
   b) The preferred tax rate of a poor under mobility is weakly increasing in the expected inequality aversion β of the rich when the foreign tax is medium or low (follows from equation 2.9).
   c) The poor's preferred tax rate is independent of their inequality aversion α if she is not extremely inequality-averse (follows from equations 2.6 and 2.9).
3. a) When the domestic tax is high and the foreign tax is low or medium, **a rich's propensity to migrate** is weakly decreasing in their inequality aversion β (follows from Lemma 1).
   b) In all other cases, inequality aversion β of the rich should not matter for migration decisions (follows from Lemma 1).

It is also useful to make explicit predictions for the benchmark specification of our model when subjects are selfish ($\alpha = \beta = 0$). It is easy to see that a selfish rich will always prefer the low tax and will migrate if and only if the domestic tax is high and the foreign tax is not. A selfish poor, expecting the rich to be selfish, too, will prefer the medium tax if the rich is mobile and the foreign tax is not high. Otherwise, the selfish poor will prefer the high tax rate.

2.3 Survey Experiment

2.3.1 German Internet Panel (GIP)

We implement the above-described model of optimal redistributive taxation in an experimental setting using the German Internet Panel (GIP), a probability-based longitudinal panel survey conducted by the Collaborative Research Center "Political Economy of Reforms" (SFB 884) at the University of Mannheim. Although the GIP is online-based, it is representative for the general population in Germany aged from 16 to 75 living in private households. This is achieved by providing households without internet connection with the necessary devices to participate in the panel, as well as clear technical instructions on their usage (Blom, Gathmann, and Krieger, 2015). The selection of the panel is based on a stratified random sample of both the online and offline population. In comparison to other population statistics, the GIP shows high congruence with regard to personal characteristics like age, unemployment, urbanity, and regionality (Blom et al., 2016; Blom, Gathmann, and Krieger, 2015).

All participants of the GIP are first recruited in face-to-face interviews and then take part in bimonthly surveys of around 20 minutes resulting in a panel data set. The GIP started in September 2012 and has a special focus on opinions and preferences with regard to political reforms. The surveys are accompanied by quality-assurance measures such as extensive plausibility tests conducted by an expert team of the GIP, as well as a pre-test concerning the technical implementation. These provisions are in place to ensure the comprehensibility of questions about complex issues for the general population. In order to maintain the GIP's high retention rates (73% - 80%) there is an incentive scheme in place (Blom, Gathmann, and Krieger, 2015). Participants are getting 4€ for every survey that they take part in and on top of that there is a bonus for those who participated in every survey of the year (10€) and those who only missed one survey (5€), respectively.

2.3.2 Design

In the experiment, every participant of the panel is randomly assigned to a treatment. One quarter of the panel is acting as the control group by playing under the *no-mobility* treatment condition, which is referring to the model without migration option, while the rest plays under the *mobility* treatment condition. Within both the mobile and immobile partition of the panel, two-thirds are assigned to be poor and one third to be rich.[8] The *mobility* types are also exogenously assigned to a foreign tax rate. 40% are facing a low foreign tax rate, 40% are facing a medium foreign tax rate and 20% are facing a high foreign tax rate (see Table 2.3.1 for an overview).[9] Respondents are told that they are part of a hypothetical country, which they share with two other participants such that each country consists of one rich and two poor respondents. Because of the nature of an online survey, the respondents cannot interact directly and are matched only ex post to their respective country by a random mechanism. Therefore, questions that condition on others' choices are asked using the strategy method. The questions are described in more detail later in this section.

[8] Note that we do not use the value-laden term "poor" in the instructions. Instead, we use the more neutral term of a low-income earner.

[9] We assigned fewer participants to the high foreign tax rate, because in this case mobility does not affect the optimal domestic tax rate and is therefore less interesting to study.

Table 2.3.1: Treatment and role assignment

mobility foreign tax	no –		yes low		yes medium		yes high		total	
	N	in %	N	in %	N	in %	N	in %	N	in %
poor	513	16.99	614	20.33	609	20.17	302	10.00	2,038	67.48
rich	257	8.50	293	9.70	285	9.44	147	4.87	982	32.52
total	770	25.50	907	30.33	894	29.60	449	14.87	3,020	100.00

Notes: Slight deviations from the expected share of treated individuals occur because of individuals not completing the whole survey. 40 out of 3,060 participants did not complete the whole survey.

All participants of the panel are required to go through a detailed explanation of the experiment specifically tailored to their type and treatment. This includes detailed step-by-step descriptions and multiple examples of possible outcomes written in easy language as well as simple graphics illustrating the timing of events and the voting system. Furthermore, tables visualizing all potential outcomes of the model are not only presented during the explanation, but also depicted when individuals have to make their decisions. Participants took an average time of about eleven minutes to complete the survey.

After reading the description of the available choices and consequences and before making their tax-rate and migration choices, the participants are made aware that there is an extra incentive scheme on top of the general GIP scheme described above. After the experiment, 20 (out of 1,020) experimental countries are randomly drawn and the participants who were part of these countries are getting their hypothetical income from the game as a bonus payment. This translates into 60 out of 3,060 participants receiving an average bonus payment of 41.33€. Depending on their type, treatment, and their own decisions, this payment can range between 20€ (poor type when the rich migrates) and 80€ (rich type if she does not migrate and the low tax is chosen).

Finally, all participants are asked the following questions: (1) What tax do you vote for? (2) Which tax do you think will the respective other type vote for? Participants in the *mobility* treatment who are of the rich type are additionally asked whether they would migrate conditional on every single possible tax rate in their domestic country (low, medium and high). Analogously, participants of the *mobility* treatment who are of the poor type are asked whether they believe that the rich in their country will migrate, again conditional on every possible domestic tax rate. The full questionnaire can be found in Appendix 2.C.

To sum up, we collect data not only on tax and migration decisions, but also on the beliefs about the behavior of other participants. The random assignment of treatments and roles allows us to identify the treatment effects of mobility, type, and foreign tax rate on the tax and migration choices, as well as tax and migration beliefs by (ordered) logistic regressions.[10] Using our rich data set, we can link these variables to various

[10] Results are very similar when using (ordered) probit regressions. See Appendix 2.B for details.

questions about political opinions and party preferences as well as personal characteristics such as gender, age, and education level (see Table 2.A.1 in Appendix 2.A for summary statistics).[11] With regard to political ideology, we can observe participants' stated party preference and distributive preference. In order to study the effect of party preferences in a more systematic way, we follow the sorting of parties by the Comparative Manifesto Project[12] on an economic left-right scheme. The center-left Social Democrats SPD, the environmentalist Greens, the Pirate party and the most far-left party The Left ("Die Linke") are coded as left-wing, while all other parties[13] are coded as right-wing. In order to infer an individual's preference for redistribution, respondents are asked directly: "Should the government employ policies to lower income inequality?" We group those who stated to be "in favor" or "strongly in favor" as in favor of redistribution, those who answered "against" or "strongly against" as against redistribution and those who chose "neither in favor nor against" as indifferent towards redistribution. We find that redistribution and political preferences are correlated in the expected direction. Left-wing participants are 25.6 percentage points ($p < 0.01$) more likely to be in favor of redistribution than right-wing participants. It is important to differentiate these variables from our treatment variables since they are not randomly assigned. Their effect should therefore be interpreted as (conditional) correlations, not causal effects.

2.3.3 Hypotheses

The comparative statics of our model, combined with the assumption that participants in favor of redistribution as well as left-leaning participants are more inequality-averse, yield testable hypotheses with respect to both tax and migration choices. This assumption is supported by Kerschbamer and Müller (2020) who find implementing the equality equivalence test of Kerschbamer (2015) in the GIP that participants classified as selfish are more likely right-leaning, whereas preference types that are benevolent towards players with lower payoffs are more likely left-leaning. For the ease of exposition, we will call both left-wing participants and those in favor of redistribution left-leaning and other participants right-leaning. The tax choices of the poor players depend on their beliefs about the migration choices of the rich, whereas the tax and migration choices of the rich are independent of their beliefs about the choices of the poor. Therefore, we begin with the tax and migration choices of the rich, then move to the beliefs of the poor about the migration choices of the rich and conclude with the tax choices of the poor. Our first hypothesis follows directly from comparative statics 1.

[11] Given the context of our experiment, one might want to control for actual migration experiences of participants. The closest proxy for this variable is information on citizenship. However, only 5% of participants have a non-German passport, and of those almost half have dual citizenship, making a systematic analysis difficult.

[12] Data and information at `https://manifesto-project.wzb.eu/`

[13] These include the center-right CDU/CSU, the liberal (free market-oriented) democrats FDP, the right-wing populist AfD, and the far-right NPD.

Hypothesis 1 (tax choices and political preferences of the rich)**.** *Left-leaning rich players vote for a higher tax rate than right-leaning rich players.*

Consider now migration choices of the rich. Again, we derive predictions from our model in a straightforward way, as summarized in our next hypothesis below. First, Lemma 1 implies that for any level of inequality aversion, the rich should only migrate if the domestic tax is high and the foreign tax is not because under this condition migration pays materially whereas otherwise migration does not pay materially and also increases inequality. Assuming that not all our participants are strongly inequality averse, this implies Hypothesis 2a. Second, Lemma 1 further implies that when migration pays materially, sufficiently inequality-averse rich players may still decide not to migrate (see also comparative statics 3a). Assuming that the left-leaning participants are more inequality averse than the right-leaning participants, we thus derive Hypothesis 2b. By contrast, if migration does not pay materially, migration choice is not affected by a rich player's inequality aversion (see comparative statics 3b). Under the assumption that political preferences do not affect migration choices in this situation other than through differences in inequality aversion, this yields Hypothesis 2c.

Hypothesis 2 (migration choices and political preferences of the rich)**.**

a) Rich players more frequently migrate when the domestic tax is high and the foreign tax is not high than in the remaining constellations.

b) When the domestic tax is high and the foreign tax is not high, the left-leaning rich are less likely to migrate than the right-leaning rich.

c) If the domestic tax is not high or the foreign tax is high, migration choices of the rich do not vary with their political preferences.

Given that the domestic and foreign tax rates affect the incentives for the rich to migrate, they should also affect the beliefs of the poor about the migration choices of the rich. This in turn affects the optimal tax level from the perspective of the poor. We first address the beliefs of the poor. If the poor understand the material incentives of the game and expect at least some of the rich to be affected by these material incentives, the poor should expect the rich to be more likely to migrate when domestic taxes are high and foreign taxes are not than otherwise (Hypothesis 3a). In our model, the beliefs of the poor about the migration choices of the rich are independent of the poor players' own political ideology. However, a more leftist political perspective might also make participants more optimistic that the rich will not migrate even when it pays materially, for example because the left-leaning expect others to be more inequality averse.[14] This implies Hypothesis 3b.

[14] If the left-leaning are more averse towards advantageous inequality than the right-leaning participants, it is also plausible that they expect others to be more averse towards advantageous inequality due to the (false) consensus effect, (Engelmann and Strobel, 2000), i.e., a correlation of a participant's expectation about other participants with their own type.

Hypothesis 3 (migration beliefs and political preferences of the poor)**.**

a) *The expectation of the poor that the rich will migrate is higher when the domestic tax is high and the foreign tax is not high than in the remaining constellations.*

b) *When migration pays (high domestic tax and low or medium foreign tax), left-leaning poor players tend to be less convinced than right-leaning ones that the rich will migrate.*

The material migration incentives of the rich and the beliefs of the poor whether the rich will act on these incentives are expected to affect the tax choices of the poor. Specifically, if the poor expect the rich to be more likely to migrate when it pays materially as predicted in Hypothesis 3a, the poor should vote for lower taxes (or more precisely, be less likely to vote for high taxes and more likely to vote for medium taxes) if the rich are mobile and foreign taxes are low or medium than if the rich are immobile or foreign taxes are high, implying Hypothesis 4a (see also comparative statics 2a). The poor may, however, also expect the rich not to migrate even when it pays, or they may also expect the rich to migrate when both domestic and foreign taxes are high. Assuming that the poor maximize their expected earnings, then for any level of foreign taxes, if they believe the rich will migrate when domestic taxes are high, the poor should be less likely to choose high taxes, implying 4b (see also comparative statics 2b).

Now consider the impact of political ideology on tax choices. If the left-leaning poor are indeed more optimistic than the right-leaning poor that the rich will not migrate even if this pays materially (Hypothesis 3b) and if in their tax choices the poor take their beliefs about the migration choices of the rich into account (Hypothesis 4b), then the left-leaning poor should choose higher taxes than the right-leaning poor, leading to Hypothesis 4c.[15] High tax choices by the left-leaning poor compared to the right-leaning poor that are not explained by differences in beliefs (i.e. support for Hypothesis 4c, even though 3b is violated) could be interpreted that the left-leaning are more likely than the

[15] As pointed out above, it is also plausible that the belief data will be subject to the (false) consensus effect. There is substantial evidence for the presence of the consensus effect in the experimental literature and the consensus effect can be responsible for patterns in the choice data that may be misinterpreted as evidence of certain preferences patterns. For example, Blanco et al. (2014) show that a correlation between cooperation in the role of first mover and second mover in a sequential prisoner's dilemma is explained to a large degree by cooperative second movers expecting others to cooperate as well rather than by a general preference for cooperation. Similarly, in our setting, the consensus effect would allow for a different underlying channel for support of Hypothesis 4c. It is possible that some strongly inequality-averse poor might vote for high taxes due to their inequality aversion and expect the rich to be strongly inequality averse (and hence not to migrate) due to the consensus effect. Then a correlation between tax choice and migration beliefs would result, even though the beliefs are not causal for the tax choices. Note, however, that this channel requires that the poor are strongly averse towards disadvantageous inequality but expect the rich to be strongly averse towards advantageous inequality. Blanco, Engelmann, and Normann (2011) observe essentially no correlation between estimated parameters for aversion towards advantageous and disadvantageous inequality, making it implausible that due to the consensus effect the poor who are averse towards disadvantageous inequality expect the rich to be averse towards advantageous inequality. Hence this alternative channel is unlikely to play a major role in our data.

right-leaning to be extremely inequality averse ($\alpha > 4$ in our model, see comparative statics 2c).

Hypothesis 4 (tax choices and political preferences of the poor)**.**

a) *The poor vote for lower tax rates in the mobility treatment with the foreign tax being low or medium compared to no mobility or the foreign tax being high.*

b) *The poor are less likely to vote for high taxes if they believe that the rich will migrate when domestic taxes are high.*

c) *Left-leaning poor participants vote for higher taxes than right-leaning participants.*

2.4 Results

As a first step, in order to make sure that the randomization worked properly, we regress treatment dummies on observable characteristics and reassuringly, we do not find any significant effects (see Table 2.A.2 in the Appendix). Following standard conventions, we understand statistical significance being at the 5% level and will note weak significance at the 10% level explicitly. We now move on to testing our hypotheses.

2.4.1 Testing the Comparative-Static Predictions

Following the order of our hypotheses, we begin with the analysis of the tax choices of the rich and continue with their migration choices. We then address whether the comparative statics of the migration choices of the rich are reflected in the beliefs of the poor and whether these beliefs are affected by the political ideology of the poor. Finally, we investigate whether these migration beliefs and the poor's ideology affect their tax choices.

We test Hypothesis 1 by examining the relationship between tax choices of the rich and their political party preferences and redistribution preferences. In line with the hypothesis, the rich who support left-of-center parties are 6.8 percentage points less likely to choose the low tax rate and 4.6 percentage points more likely to choose the high tax rate than rich who support right-of-center parties (see columns 1 to 3 in Panel A of Table 2.4.1).[16] Similarly, those participants who state to be in favor of redistribution are significantly less likely to choose the low tax rate and more likely to choose the medium or high tax rate than those opposed to redistribution, with participants who state to be indifferent in between (see columns 1 to 3 in Panel B of Table 2.4.1). We summarize these findings in

[16] The lower number of observations in Panel A compared to Panel B is because of a higher number of participants not stating their party preference. Results for the individual political parties can be found in Table 2.B.1 in Appendix 2.B.

Table 2.4.1: Tax choices and ideology: by role

	only rich players			only poor players		
	(1)	(2)	(3)	(4)	(5)	(6)
	low	medium	high	low	medium	high
Panel A *ideology* reference category: right-wing						
left-wing	-0.068***	0.022**	0.046***	-0.022	0.001	0.020
	(0.025)	(0.010)	(0.017)	(0.015)	(0.002)	(0.014)
N	686	686	686	1,429	1,429	1,429
Panel B *redistribution preference* reference category: against redistribution						
indifferent	-0.062**	0.020*	0.042**	-0.024	0.001	0.023
	(0.030)	(0.011)	(0.021)	(0.019)	(0.002)	(0.018)
pro redistribution	-0.084***	0.027**	0.058***	-0.062***	0.002	0.060***
	(0.027)	(0.011)	(0.019)	(0.016)	(0.004)	(0.016)
N	887	887	887	1,825	1,825	1,825
controls	yes	yes	yes	yes	yes	yes

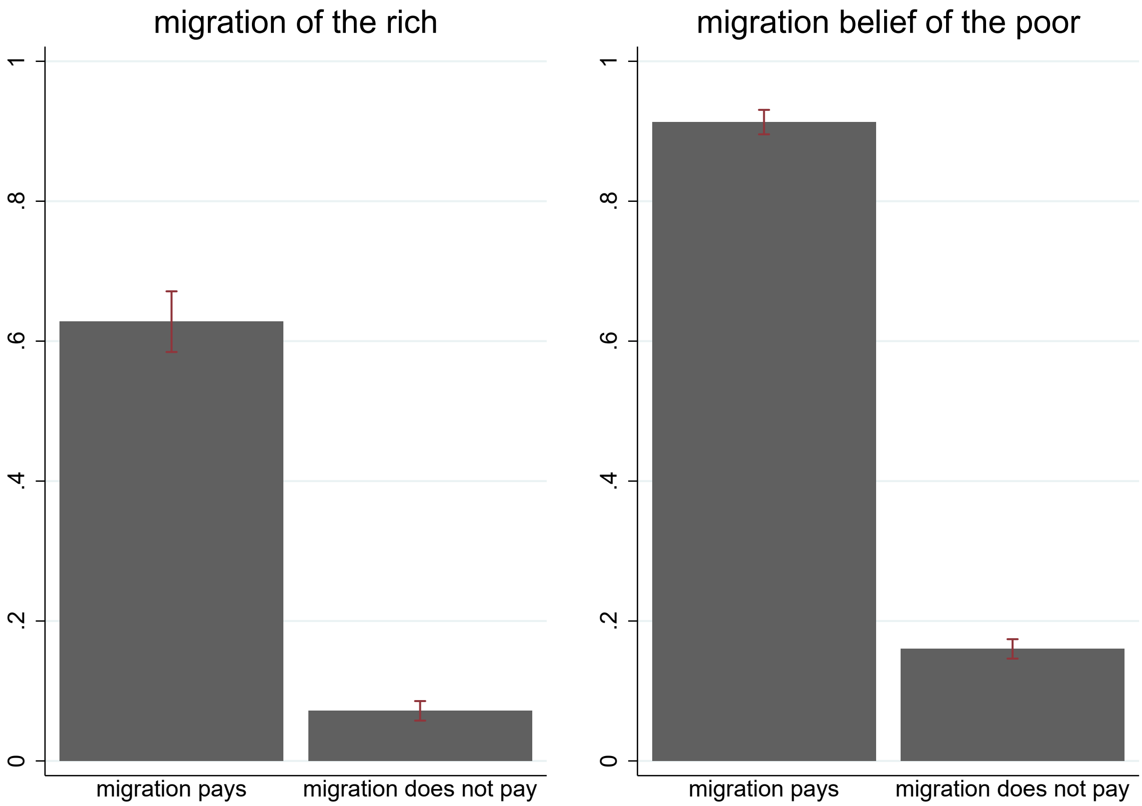
Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of ordered logistic regressions. Robust standard errors in parentheses. Each horizontal line indicates a new regression. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household members).

Result 1 (tax choices and political attitudes of the rich)**.** *In the role of the rich player, left-leaning participants vote for higher tax rates than right-leaning participants, as predicted in Hypothesis 1.*

Next, we turn to Hypothesis 2a, i.e., we test whether the rich understand and act on the migration incentives given by different domestic and foreign tax rates. We first plot migration rates of the rich both for the setting in which migration pays in material terms (high domestic tax and low or medium foreign tax) and for the setting in which it does not pay. As Figure 2.4.1 shows, 62.6% of rich players migrate if it pays. By contrast, if migration does not pay, only 9.8% do migrate. The regression analysis in column 1 of Table 2.4.2 confirms these findings and reveals their significance. Compared to the constellation where migration does not pay materially, i.e., where foreign taxes are high or domestic taxes are not, the propensity to migrate increases significantly when migration pays in material terms. To summarize, the rich seem to understand and react to migration incentives, confirming Hypothesis 2a.

While the rich migrate much more often when it pays in material terms than when it does not, more than a third of them still do not migrate even when it pays. In line with our model, this can be driven by inequality aversion, which we expect to be reflected in their political ideology. We hence compare migration choices of the left- and

Figure 2.4.1: Migration choices by migration incentives



the right-leaning rich, both when migration pays and when it does not pay. As one can see in columns 1 and 2 of Table 2.4.3, participants supporting left-of-center parties or being in favor of redistribution are 12 to 14 percentage points less likely to migrate than right-wing participants when migration pays, supporting Hypothesis 2b.[17] When migration does not pay, however, migration choices are unrelated to the political party preferences (see columns 3 and 4 of Table 2.4.3) or the redistribution preferences, as predicted in Hypothesis 2c. We summarize our results on the impact of political ideology on migration choices in

Result 2 (migration choices and political attitudes of the rich)**.**

[17] When looking at individual political parties, the effect is mainly driven by supporters of the right-wing populist party AfD. Ironically, thus, supporters of an anti-immigration party are most likely to be economic migrants in our study (see Table 2.B.2 in Appendix 2.B). The rich made migration choices for each possible tax rate. Columns 1 and 2 of Table 2.4.3 include for each individual in the treatments with low or medium foreign tax the migration choice for high domestic tax. Columns 3 and 4 include three data points for the individuals in the treatment with high foreign tax (one for each domestic tax) and two data points for the individuals in the treatments with low or medium foreign tax (one for low domestic tax and one for medium domestic tax).

Table 2.4.2: Migration choices and beliefs

	(1) migration choice	(2) migration belief
migration incentives reference category: migration does not pay		
migration does pay	0.348***	0.492***
	(0.007)	(0.007)
N	2,175	4,442

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of logistic regressions. Robust standard errors in parentheses are clustered at the individual level.

(i) The rich react to material migration incentives as predicted by Hypothesis 2a.

(ii) The reaction to incentives is mitigated by their political ideology with the left-leaning rich being less likely to migrate than the right-leaning rich when migration pays materially, confirming Hypothesis 2b.

(iii) There is no difference in the migration choices with respect to political ideology when migration does not pay, confirming Hypothesis 2c.

We have seen that the rich react to the incentives to migrate. We have also seen, though, that the degree to which they do so depend on their ideology. In order to understand whether the migration incentives of the rich affect the tax choices by the poor, we analyze whether the beliefs of the poor are affected by the incentives to migrate for the rich. Further, in order to understand if and how the tax choices by the poor are affected by their own political ideology, we need to understand whether it influences their beliefs that the rich would migrate. Hence, we move on to test Hypothesis 3.

As a first step, we plot migration beliefs both for the setting in which migration pays in material terms (high domestic tax and low or medium foreign tax) and for the setting in which migration does not pay. As Figure 2.4.1 shows, the migration beliefs of the poor closely align with our model with 90.2% of the poor expecting the rich player to migrate when it pays materially, compared to only 18.1% when it does not pay. The difference is shown to be highly significant by the regression analysis presented in column 2 of Table 2.4.2. Hence, the migration threat seems to be perceived as credible by most of the poor players, supporting Hypothesis 3a. In fact, the beliefs of the poor even vary more with the migration incentives for the rich than the actual migration choices of the rich themselves. This suggests that a substantial part of the rich in our sample are driven by inequality aversion, given that only 62.6% of them migrate when it pays materially, but that this is not anticipated by the poor, 90.2% of whom expect the rich to migrate.[18]

[18] One could argue, of course, that the beliefs of the poor are well calibrated because we only elicited a binary belief, which makes it optimal to guess the modal choice of the rich, which in our data is indeed to migrate.

Table 2.4.3: Migration and ideology

	migration pays		migration does not pay	
	(1)	(2)	(3)	(4)
	migration choice	migration choice	migration choice	migration choice
Panel A *ideology* reference category: right-wing				
left-wing	-0.137***	-0.124***	0.012	0.013
	(0.046)	(0.048)	(0.018)	(0.018)
N	405	401	1,146	1,123
Panel B *redistribution preference* reference category: against redistribution				
indifferent	-0.063	-0.054	-0.012	-0.036
	(0.064)	(0.063)	(0.025)	(0.024)
pro redistribution	-0.137**	-0.129**	-0.009	-0.034
	(0.055)	(0.055)	(0.022)	(0.021)
N	536	529	1,477	1,448
controls	no	yes	no	yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of logistic regressions. Standard errors in parentheses are clustered at the individual level. Each horizontal line indicates a new regression. We asked the rich for each possible tax rate whether they would migrate. Columns (1) and (2) show results when the domestic tax is high and the foreign tax is low or medium, while columns (3) and (4) cover all other combinations of the domestic and foreign tax rate. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household members).

That the migration threat is perceived to be real by the majority of the poor raises the question whether this perception is affected by their ideology. As Table 2.4.4 shows, there is no difference in migration beliefs by political party preference or redistribution preferences when we restrict the analysis to the constellations in which migration pays (high domestic tax and low or medium foreign tax).[19] When migration does not pay, there is also no difference by party preference, but those who are indifferent towards redistribution are significantly more likely to state that they expect the rich to migrate than those who oppose redistribution. Given that there is no good reason to migrate in this setting, this difference is likely just due to random variation Therefore, we reject Hypothesis 3b. We can hence rule out differential migration beliefs as a possible reason for potential differences in tax choices between the left- and the right-leaning poor. We summarize the results on the poor players' beliefs about the rich players' migration choices in

Result 3 (beliefs and political attitudes of the poor)**.**

[19] Results for the individual parties can be found in Table 2.B.3 in Appendix 2.B.

Table 2.4.4: Migration beliefs and ideology

	migration pays		migration does not pay	
	(1)	(2)	(3)	(4)
	migration belief	migration belief	migration belief	migration belief
Panel A *Ideology* reference category: right-wing				
left-wing	0.003	0.002	0.025	0.023
	(0.020)	(0.020)	(0.016)	(0.016)
N	872	855	2348	2300
Panel B *Redistribution preference* reference category: against redistribution				
indifferent	-0.028	-0.025	0.057***	0.045**
	(0.026)	(0.026)	(0.021)	(0.021)
pro redistribution	-0.004	-0.004	0.033*	0.026
	(0.023)	(0.023)	(0.019)	(0.019)
N	1,113	1,086	2,990	2,921
controls	no	yes	no	yes

Notes: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The presented coefficients are average marginal effects of logistic regressions. Standard errors in parentheses are clustered at the individual level. Each horizontal line indicates a new regression. We asked the poor for each possible tax rate whether they believe the rich will migrate. Columns (1) and (2) show results when the domestic tax is high and the foreign tax is low or medium, while columns (3) and (4) cover all other combinations of the domestic and foreign tax rate. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, >60), and two dummies for household size (2 and 3 or more household member).

(i) *The poor believe the rich to be more likely to migrate when it pays in material terms than when it does not, confirming Hypothesis 3a.*

(ii) *Migration beliefs do not differ between the left- and the right-leaning poor, contrary to Hypothesis 3b.*

Finally, we turn to Hypothesis 4 and investigate how migration incentives for the rich, migration beliefs of the poor and political attitudes of the poor affect the tax choices of the poor. Given the credible threat of the rich to leave the country, the poor have an incentive to vote for a lower tax rate (Hypothesis 4a). We test this hypothesis by comparing the tax choices of the poor in the mobility treatment with low or medium foreign taxes to the tax choices of the poor pooled from the no-mobility treatment and from the mobility treatment with a high foreign tax. Figure 2.4.2a shows that the threat of migration indeed induces the poor to be less likely to vote for the high tax. As panel A of Table 2.4.5 shows, this difference amounts to 5.9 percentage points and is statistically significant at the 1% level. This is in line with Hypothesis 4a, but the effect is quantitatively small.

Table 2.4.5: Tax choices, mobility and migration beliefs

	(1) low	(2) medium	(3) high
Panel A *mobility* reference category: immobile or foreign tax high			
foreign tax low or medium	0.062***	-0.004	-0.059***
	(0.013)	(0.004)	(0.013)
N	2,038	2,038	2,038
Panel B *migration belief* reference category: rich does not migrate if domestic tax is high			
rich migrates if domestic tax is high	0.099***	-0.018**	-0.081***
	(0.024)	(0.008)	(0.021)
N	1,416	1,416	1,416

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of ordered logistic regressions. Robust standard errors in parentheses.

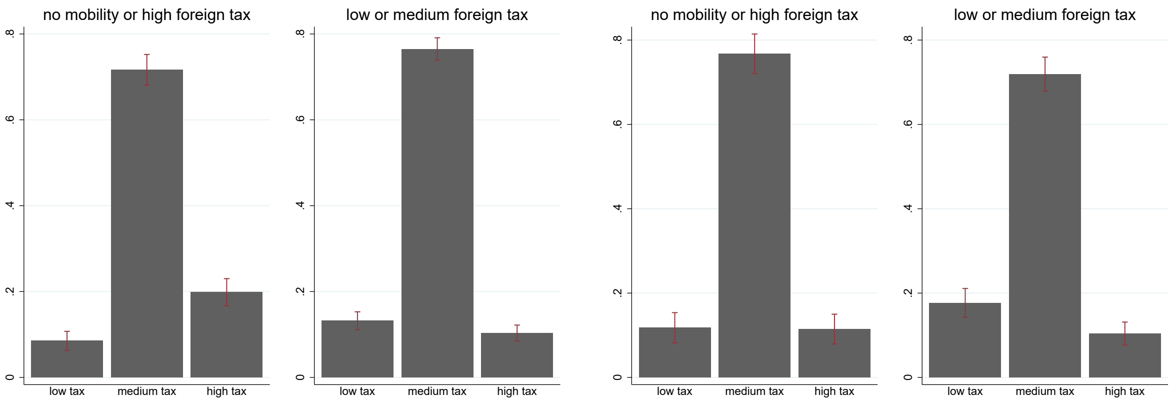
In line with Hypothesis 4b, we also find that the poor are significantly less likely to vote for the high tax if they believe that the rich will migrate when taxes are high (see Panel B of Table 2.4.5), but again the effect is quantitatively small.[20]

Regarding the impact of political ideology on the tax choices of the poor, we considered two possible channels how the political ideology of the poor could affect their tax choices. First, the left-leaning poor could be more optimistic than the right-leaning poor about the migration choices of the rich. Second, the left-leaning poor could be more likely to be extremely inequality averse. Since we do not find an impact of the political ideology of the poor on their beliefs about the migration choices of the rich (see Result 3(ii)), there is no evidence for the belief difference underlying the first channel. Hence, support for 4c would point to the second channel. We find mixed support for this hypothesis. While there is no difference between the poor who support left-of-center parties and right-of-center parties (see columns 4 to 6 of Panel A in Table 2.4.1), we do find that the poor who are in favor of redistribution are significantly more likely to vote for the high tax than those opposed to redistribution (see Panel B of Table 2.4.1). This could indicate that some participants have extreme inequality aversion and that this affects both stated redistribution preferences as well as tax choices in our experiment. The incidence of extreme inequality aversion, however, does not appear to be systematically related to party preferences. We summarize our results on political ideology and tax choices of the poor in

Result 4 (tax choices and political attitudes of the poor)**.**

[20] We note that the optimal tax rate is the medium one when the rich migrate if and only if it pays, while we observe an increase in choosing the low tax rate in both panels of Table 2.4.5 and even a decrease in the frequency of choosing the medium tax rates in Panel B. This may be caused by calculating the optimal tax incorrectly or by an expectation that the rich might even be spiteful and also migrate for medium domestic taxes.

Figure 2.4.2: Tax choices by migration incentives



a) Tax choices of the poor **b)** Tax choices of the rich

(i) The tax choices of the poor react to the possibility of the rich migrating as predicted by Hypothesis 4a, but the effect is quantitatively small.

(ii) The poor are less likely to choose high taxes if they believe that the rich will migrate when domestic taxes are high, supporting Hypothesis 4b, but the effect is quantitatively small.

(iii) The poor players' political party preference is not significantly related to their tax choices, but those with a high preference for redistribution vote for a higher tax rate. Hence, we find mixed evidence with respect to Hypothesis 4c.

As the main insight regarding the role of political ideology in tax choices, we find that it matters when our participants are in the role of the rich but it does not when they are in the role of the poor. This is in line with our model as far as inequality aversion relates to political ideology because for the rich already moderate levels of inequality aversion can affect their choices, while only extreme levels do so for the poor. We do find some weak evidence of extreme aversion towards disadvantageous inequality, which does not seem to be systematically related to political party orientation, though. An alternative channel that predicts political preferences to impact tax choice of the poor via their beliefs does not find support, either. Our results therefore suggest that political attitudes can moderate the effects of mobility on tax competition and a race to the bottom. The channel is not, however, over-optimistic beliefs or ideology-driven taxation choices of the (left-leaning) poor but rather benevolence of a sizable part of the (left-leaning) rich.

Overall, our findings confirm the hypotheses derived from the comparative statics of our model. However, one finding is notable. Although the poor, when choosing taxes, react to migration incentives of the rich as predicted, the effect is quantitatively small (Result 4(i)). This is puzzling in light of the fact that migration beliefs of the poor react strongly to migration incentives (Result 3(i)). In the next section, we discuss this result in more detail.

2.4.2 Levels of Tax Choices

Considering levels of tax choices in our experiment, we find a strong concentration on the medium tax and a relatively weak difference between player types and treatments (see Figure 2.4.2). Why is this a puzzle? The choice of the rich, choosing medium instead of low taxes, is in principle consistent with our model, because for an intermediate level of inequality aversion they could be indifferent between all tax rates (see comparative statics 1). However, it appears surprising that such a high share of the rich is concentrated on the intermediate level of inequality aversion that just makes them indifferent and makes them break indifference in favor of the medium tax.[21] More importantly, the poor also primarily choose medium taxes even when the rich are not mobile or foreign taxes are high; and this can no longer be rationalized within our model.[22] Hence, we find a notable and unpredicted pattern: A large part of the rich and the poor choose medium taxes, with only a small reaction to treatment conditions (see Result 4(i)).

A possible reason for the concentration of tax choices on the medium tax rate that comes to mind is a potential misunderstanding of the experimental task. This is arguably more likely in a survey experiment than in a laboratory experiment: First, participants in the former cannot ask clarifying questions. Second, a sample with a larger variety in terms of education and age than a typical student sample may on average have more problems understanding the task (Snowberg and Yariv, 2021).

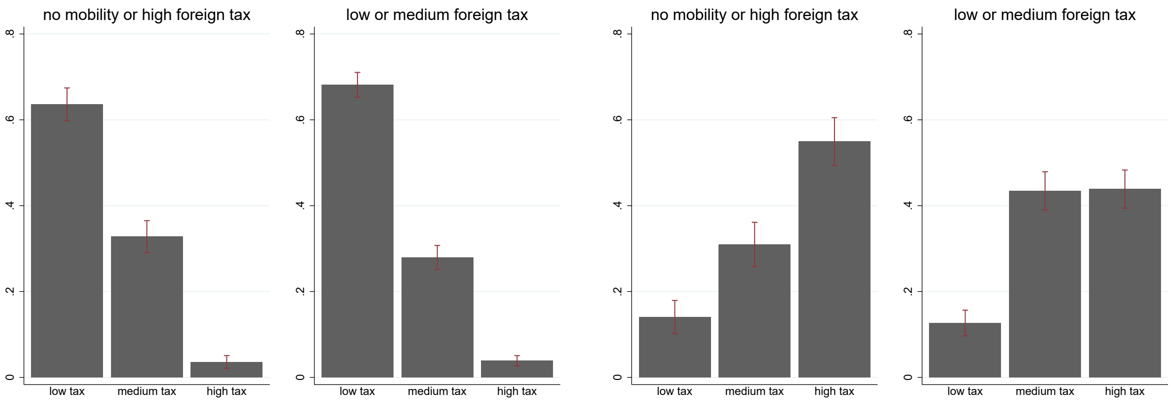
However, for two reasons, we do not believe that misunderstanding is the dominant factor behind our relatively weak support for the point predictions of our model. To see this, consider the benchmark equilibrium of our model with zero inequality aversion, i.e., the equilibrium in which monetary incentives fully determine decisions. First, note that beliefs are much closer to the point predictions of this benchmark equilibrium than choices are (see Figure 2.4.3). This suggests that most participants do understand the monetary incentives in the tax game. At least they appear to understand the incentives for the other type, which makes it implausible that they would not understand them for their own type.

Second, we run robustness checks for our tests, excluding participants who appear most likely to be confused, namely those who are either the fastest or the slowest in completing the experiment. Very fast participants are likely to not have carefully read the instructions and to not have thought deeply about their decisions. Very slow par-

[21] The inability to explain intermediate choices is a problem that is often encountered in applications of the Fehr-Schmidt model due to its linearity. Assuming instead that the utility loss is convex in the inequality allows for explanations of intermediate choices by intermediate levels of inequality aversion.

[22] As we argued in our model section, the inequality aversion of the poor is irrelevant in these cases, because even with high taxes, the rich still have higher payoffs than the poor. Choosing high taxes therefore increases a poor's own payoff while also reducing inequality. Hence, the inequality-averse poor should choose high taxes when the rich are not mobile or foreign taxes are high.

Figure 2.4.3: Tax beliefs by migration incentives



a) Tax beliefs of the poor **b)** Tax beliefs of the rich

ticipants are likely to have thought long because they have had trouble understanding. Excluding both the fastest 10% and the slowest 10% does not overall affect our test results much.[23] Furthermore, we create a dummy for deviating from the benchmark equilibrium. We regress this dummy on the respondents' education level, the time they took to complete the survey, and a dummy equaling one if the respondent interrupted the survey at some point. As one can see in Table 2.B.4 in Appendix 2.B, none of these factors can explain deviations from the benchmark equilibrium both in tax choices and beliefs. Hence, none of these factors points towards a possible misunderstanding of incentives, either.

Nonetheless, for many of our participants the experiment was probably an unusual situation. Hence, they may have been unsure about the appropriate action. Choosing the "middle" may then have appeared to them a good compromise that made them look or feel neither too greedy nor like a fool for forgoing too much. Choices that avoid extremes and favor compromises between various motives have frequently been observed in other contexts, notably marketing research (for a discussion of extremeness aversion and compromise effects, see, e.g., Simonson and Tversky (1992) and Simonson (1989).) We therefore check whether answers to questions in the GIP that are not related to our experiment have a tendency to center in the middle, too. We find that other questions with three or five item response options show a tendency towards the center, though much weaker. Across all questions with three items in waves 17 and 18 of the GIP, the distribution across left, middle, and right is 37%, 41%, and 23%, respectively. Participants shy away more from extremes in questions with five items, where the distribution of answers across the five options from left-most to right-most is (11%/36%/27%/26%/2%). These questions were typically less complicated than ours

[23] See Appendix 2.B for details. Results are also robust to excluding the slowest or fastest 20% (available upon request).

and hence participants may have felt less unsure and thus may have shown a weaker tendency towards the center.

One further possibility is that participants take real tax rates as sensible benchmarks into account. For instance, they may refer to their personal tax rate as a guidance of what is appropriate. If that was true, we would expect that tax-rate choices in the experiment correlate with actual household income, because that correlates with the personal tax rate. However, we do not find such an effect (see Table 2.B.5 in Appendix 2.B for detailed results).[24]

The most plausible explanation for the concentration of tax choices on the medium level seems to be that people confronted with unusual decisions exhibit a tendency toward a compromise between conflicting motives. This leaves the question, however, why beliefs about tax choices of the other type are much closer to the predictions for rational selfish individuals than actual choices are. Possibly, while our participants trade-off the motives of maximizing earnings with a desire not to appear too greedy (by only sharing the minimum if they are rich or asking for the maximum if they are poor), they may underestimate how much others also want to avoid to appear too greedy. When thinking about what other people do, it is easy to understand their material incentives, but much more difficult to predict which further, more complex, motives affect choices. People often have complex motivations, but may underestimate how complex others' motivations are as well.

2.5 Conclusion

We study voting on taxation in the presence of mobility of high-income earners in a simple game that we implemented in an online survey experiment, based on a fairly large, representative sample of the German population. Tax-induced mobility has been at the center of recent theoretical and empirical research, as it is a key component in understanding and quantifying the link between international economic integration on the one hand and the government's ability to provide public goods and to shape the degree of inequality on the other hand. We contribute to this literature by analyzing the role of political attitudes that may correlate with the preferred choice of tax rates, migration decisions, and beliefs of participants in our experiment. Controlling for demographic characteristics such as age, income, and education, we find that behavior does correlate with political attitudes, and in a predictable way. In the role of the rich, left-leaning participants tend to be more likely than right-leaning ones to vote for higher taxes despite hurting themselves. Compared to the right-leaning, the left-leaning also

[24] Alternatively, one might argue that $20/90 = 22.2\%$ is a reasonable approximation for the average tax rate of the median-income person in Germany, while $40/90 = 44.4\%$ is much higher. However, people have a tendency to confuse average and marginal tax rates (De Bartolome, 1995). Coupled with the fact that we explicitly talk about taxing the "rich" in this experiment and that Germany has a "rich tax" with a marginal tax rate of 45% (for incomes above 250,000€ for unmarried individuals and 500,000€ for married couples), the 44.4% tax rate actually appears rather appropriate.

migrate less when migration pays, but exhibit no difference when it does not. Beliefs of the poor about the migration choices of the rich, however, are not systematically related to political attitudes. Our finding that the right-leaning are less prone to redistribution when it hurts them is in line with the result by Karadja, Mollerstrom, and Seim (2017) that only those right of center become more averse to redistribution when they learn that they rank higher in the income distribution than they thought.[25]

There are three interesting implications of our results. First, our findings attenuate the race-to-the-bottom argument that the increasing mobility of labor (and capital) leads to sub-optimally low taxes and public spending (for a survey of this literature, see Keen and Konrad, 2013). A sizable share of participants migrate less and stick to higher tax rates than would be expected in the absence of social preferences. Second, political attitudes do not simply reflect easily measurable demographic characteristics. Third, the possible impact of political attitudes on behavior does not seem to be the result of politically biased expectations about others' behavior but of a correlation between political attitudes and social preferences.[26] We note that in our experiment, the income distribution depends purely on luck and not on effort or skill. In the study by Almås, Cappelen, and Tungodden (2020) Norwegians and US-citizens differ in their redistribution preferences when inequality is based on luck but not in the degree to which they acknowledge merit. Similarly, left-leaning participants might be less inequality-accepting than the right-leaning when inequality is based on luck. We do not know, however, whether they would also be less inequality-accepting when it was driven by merit.

This chapter also provides a methodological contribution to the analysis of the role of political attitudes. Stated preferences in surveys lack incentive compatibility. A standard alternative to surveys are therefore laboratory experiments. Our study, however, reveals advantages of a survey experiment over a laboratory experiment. Prior to conducting our survey experiment, we had studied the impact of migration on tax choices and their relation to political attitudes in a laboratory experiment. In this laboratory experiment, choices are overall much closer to the selfish equilibrium prediction than in our survey experiment.[27] Furthermore, we find little impact of political preferences on the choices in the laboratory experiment. This is apparently due to the fact that most of our participants support one of three main parties (CDU, SPD, Greens) who tend to have moderate views on economic issues. There is some indication in our data that supporters of smaller parties make different choices, but they are too rare in our laboratory sample for a meaningful statistical analysis.[28]

[25] In an earlier study, Cruces, Perez-Truglia, and Tetaz (2013) found that informing survey participants that they are poorer than they thought increases their support for redistribution but they did not assess the relationship to political ideology.

[26] Klimm (2019) finds in a laboratory experiment with a treatment variation whether outcomes can be affected by cheating that left-leaning participants redistribute more when cheating is possible, whereas right-leaning participants do not react to the treatment. In line with our results, he finds that this difference is not driven by differences in beliefs.

[27] See Appendix 2.D for detailed results and a discussion of possible reasons for the differences.

[28] Our results are overall in line with those of a study by Esarey, Salmon, and Barrilleaux (2012). They also find that the behavior of participants in a laboratory experiment on voting about redistribution with

More important than the difference in the overall pattern of choices between the survey experiment and the laboratory experiment is thus the insight that the laboratory sample is not suitable to study the relation of political ideology, tax, and migration choices. Our survey experiment suggests that while the effects of political ideology are relevant, they can only be detected if two conditions are met. First, sample sizes have to be sufficiently large in order to have a sizable share of supporters also for smaller parties, which are more likely to hold strong views. This holds in particular in Germany and similar countries where the majority of people support economically moderate parties. Second, political ideologies need to be sufficiently firmly established. The first condition is hard to meet in a laboratory experiment due to constraints on the budget and subject-pool size.[29] The second condition is arguably also harder to satisfy with a student pool. In contrast to a typical laboratory experiment, our survey experiment is based on a large representative sample of the adult German population, enhancing the external validity of our results. Therefore, for studying the impact of ideology, integrating the experiment into online surveys is a more fruitful approach. It is a general insight that surveys, field experiments, and laboratory experiments are complementary. Survey experiments may be a good compromise for research questions such as ours. They provide exogenous variation, can be incentivized and have the necessary sample size as well as variation in political ideology to permit a thorough investigation of the effects of political attitudes.

earned income is well explained by selfishness and that participants that are more pro-redistribution according to a set of questions from a questionnaire do not vote for higher taxes in general. Interestingly, though, they find that those participants more opposed to redistribution react more to their selfish incentives, which is broadly in line with the more selfish voting and migration behavior of the right-wing participants in our survey experiment.

[29] Selective recruiting based on party preferences does not appear to be a viable way either.

Appendix

2.A Summary Statistics and Randomization Check

Table 2.A.1: Summary statistics

variable	mean	sd	min	max	N	GIP wave
tax choice	1.97	0.54	1	3	3,020	18
tax belief	1.68	0.76	1	3	3,015	18
migration	0.24	0.43	0	1	2,175	18
migration belief	0.38	0.48	0	1	4,442	18
female	0.49	0.50	0	1	3,019	18
age: 16 - 29	0.17	0.40	0	1	3,018	18
age: 30 - 39	0.15	0.42	0	1	3,018	18
age: 40 - 49	0.20	0.43	0	1	3,018	18
age: 50 - 59	0.24	0.49	0	1	3,018	18
age: > 60	0.24	0.50	0	1	3,018	18
married	0.60	0.49	0	1	3,019	18
higher education	0.48	0.50	0	1	2,955	18
household size: 1	0.16	0.37	0	1	3,014	18
household size: 2	0.43	0.50	0	1	3,014	18
household size: 3 or more	0.41	0.49	0	1	3,014	18
left-wing	0.51	0.50	0	1	2,160	16
NPD	0.01	0.09	0	1	2,349	16
AfD	0.10	0.30	0	1	2,349	16
FDP	0.06	0.23	0	1	2,349	16
CDU/CSU	0.28	0.45	0	1	2,349	16
SPD	0.22	0.41	0	1	2,349	16
Green Party	0.16	0.36	0	1	2,349	16
Pirate Party	0.02	0.12	0	1	2,349	16
The Left	0.09	0.29	0	1	2,349	16
non voter	0.07	0.25	0	1	2,349	16
redistribution: in favor	0.54	0.50	0	1	2,776	16
redistribution: indifferent	0.25	0.44	0	1	2,776	16

Table 2.A.2: Randomization check

	(1) mobile	(2) rich	(3) foreign tax low	(4) foreign tax medium	(5) foreign tax high
gender reference category: male					
female	-0.024	0.030*	-0.001	-0.006	0.006
	(0.016)	(0.017)	(0.021)	(0.021)	(0.017)
N	3,019	3,019	2,250	2,250	2,250
age reference category: < 30					
30 to 39	-0.021	-0.003	0.006	-0.017	0.011
	(0.028)	(0.030)	(0.036)	(0.037)	(0.030)
40 to 49	-0.007	-0.001	0.035	-0.030	-0.005
	(0.026)	(0.028)	(0.034)	(0.034)	(0.027)
50 to 59	-0.025	-0.042	-0.029	-0.012	0.041
	(0.025)	(0.027)	(0.033)	(0.033)	(0.027)
> 60	0.004	-0.037	0.023	-0.020	-0.003
	(0.025)	(0.027)	(0.032)	(0.032)	(0.026)
N	3,018	3,018	2,249	2,249	2,249
marital status reference category: not married					
married	-0.013	-0.032*	0.012	-0.008	-0.004
	(0.016)	(0.017)	(0.021)	(0.021)	(0.017)
N	3,019	3,019	2,249	2,249	2,249
educational status reference category: lower education					
higher education	0.006	0.009	0.022	-0.032	0.009
	(0.016)	(0.017)	(0.021)	(0.021)	(0.017)
N	2,955	2,955	2,205	2,205	2,205
household size reference category: 1					
2	0.020	-0.006	-0.034	0.005	0.029
	(0.024)	(0.025)	(0.031)	(0.030)	(0.024)
3 or more	0.023	-0.011	-0.048	0.027	0.021
	(0.024)	(0.025)	(0.031)	(0.031)	(0.024)
N	3,014	3,014	2,249	2,249	2,249
ideology reference category: right-wing					
left-wing	0.009	-0.000	-0.007	0.003	0.004
	(0.019)	(0.020)	(0.024)	(0.024)	(0.020)
N	2,160	2,160	1,624	1,624	1,624
redistribution preferences reference category: against redistribution					
indifferent	-0.027	0.027	0.007	-0.008	0.001
	(0.024)	(0.026)	(0.032)	(0.032)	(0.025)
pro redistribution	-0.001	0.029	-0.002	-0.037	0.039*
	(0.021)	(0.023)	(0.028)	(0.028)	(0.022)
N	2,776	2,776	2,079	2,079	2,079

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented results are based on a linear regression of the form $Y_i = \beta_0 + \beta covariate_i + \epsilon_i$, where Y_i is a dummy for the respective treatment variable (mobile, rich, foreign tax low, foreign tax medium, foreign tax high) and $covariate_i$ is the respective covariate. Robust standard errors in parentheses. Each horizontal line indicates a new regression.

Table 2.B.1: Tax choices and party preference: by role

	only rich players			only poor players		
	(1)	(2)	(3)	(4)	(5)	(6)
	low	medium	high	low	medium	high
Panel A *party preference* reference category: far-left (The Left)						
right-wing populist (AfD)	0.112*	-0.036	-0.076*	0.080**	-0.004	-0.076**
	(0.064)	(0.023)	(0.044)	(0.033)	(0.006)	(0.031)
liberal (free market) democrats (FDP)	0.159**	-0.051**	-0.108**	0.112***	-0.006	-0.106***
	(0.062)	(0.023)	(0.045)	(0.036)	(0.008)	(0.034)
christian democrats (CDU/CSU)	0.124**	-0.040**	-0.084**	0.081***	-0.004	-0.077***
	(0.054)	(0.020)	(0.038)	(0.026)	(0.006)	(0.025)
social democrats (SPD)	0.097*	-0.031*	-0.066*	0.106***	-0.005	-0.100***
	(0.053)	(0.018)	(0.038)	(0.027)	(0.008)	(0.026)
environmentalist (Green Party)	0.039	-0.012	-0.026	0.040	-0.002	-0.038
	(0.056)	(0.018)	(0.038)	(0.028)	(0.003)	(0.027)
N	740	740	740	1,560	1,560	1,560
controls	yes	yes	yes	yes	yes	yes

Notes: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The presented coefficients are average marginal effects of ordered logistic regressions. Robust standard errors in parentheses. Each horizontal line indicates a new regression. Results for very small parties (NPD and Pirate Party) as well as non-voters are not presented. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, $>$ 60), and two dummies for household size (2 and 3 or more household members).

2.B Robustness Checks

Table 2.B.2: Migration and party preference

	migration pays		migration does not pay	
	(1)	(2)	(3)	(4)
	migration choice	migration choice	migration choice	migration choice
Panel A *party preference* reference category: far-left (The Left)				
AFD	0.239**	0.237**	0.010	0.008
	(0.113)	(0.113)	(0.036)	(0.034)
FDP	0.181	0.181	0.010	0.014
	(0.127)	(0.121)	(0.051)	(0.047)
CDU/CSU	0.053	0.034	-0.049	-0.059**
	(0.085)	(0.083)	(0.031)	(0.030)
SPD	-0.007	-0.012	-0.009	-0.019
	(0.087)	(0.086)	(0.030)	(0.028)
The Greens	-0.062	-0.070	-0.038	-0.031
	(0.092)	(0.091)	(0.035)	(0.033)
N	443	439	1,231	1,208
controls	no	yes	no	yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of ordered logistic regressions. Standard errors in parentheses are clustered at the individual level. Each horizontal line indicates a new regression. We asked the rich for each possible tax rate whether they would migrate. Columns (1) and (2) show results when domestic taxes are high and foreign taxes are low or medium, while columns (3) and (4) cover all other combinations of domestic and foreign taxes. Results for very small parties (NPD and Pirate Party) as well as non-voters are not presented. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household members).

Table 2.B.3: Migration beliefs and party preference

	migration pays		migration does not pay	
	(1)	(2)	(3)	(4)
	migration belief	migration belief	migration belief	migration belief
Panel A Panel A *party preference* reference category: far-left (The Left)				
AFD	0.027	0.026	0.012	0.014
	(0.053)	(0.053)	(0.034)	(0.034)
FDP	-0.028	-0.026	-0.060	-0.040
	(0.053)	(0.053)	(0.042)	(0.042)
CDU/CSU	-0.043	-0.043	-0.044	-0.035
	(0.039)	(0.039)	(0.028)	(0.028)
SPD	-0.012	-0.014	-0.002	-0.002
	(0.042)	(0.042)	(0.028)	(0.029)
The Greens	-0.052	-0.052	-0.001	0.019
	(0.041)	(0.042)	(0.030)	(0.030)
N	939	920	2,548	2,498
controls	no	yes	no	yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of ordered logistic regressions. Standard errors in parentheses are clustered at the individual level. Each horizontal line indicates a new regression. We asked the poor for each possible tax rate whether they believe the rich will migrate. Columns (1) and (2) show results when the domestic tax is high and the foreign tax is low or medium, while columns (3) and (4) cover all other combinations of the domestic and foreign tax rate. Results for very small parties (NPD and Pirate Party) as well as non-voters are not presented. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household member).

Table 2.B.4: Deviation from the selfish equilibrium

Deviation from EQ	(1) tax choice	(2) tax choice	(3) tax belief	(4) tax belief
educational status reference category: lower education				
higher education	-0.0327*	-0.0282	-0.0356	-0.0219
	(0.0187)	(0.0217)	(0.0188)	(0.0219)
interruption reference category: did not interrupt the survey				
interrupt	0.0022	0.0058	0.0165	0.0208
	(0.0317)	(0.0345)	(0.0316)	(0.0343)
minutes spend on the survey	0.0001	0.0001	-0.0000	0.0000
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
N	2,754	2,299	2,750	2,296
controls	no	yes	no	yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of logistic regressions. Robust standard errors in parentheses. The mean time spend on the survey is 11 minutes. Controls include dummies for gender, marital status, four age dummies (30-39, 40-49, 50-59, > 60), two dummies for household size (2 and 3 or more household members) and party dummies.

Table 2.B.5: Tax choice and income

	low	low	medium	medium	high	high
Income reference category: < 2000€						
2000€ - 4000€	-0.011	-0.005	0.002	0.001	0.009	0.005
	(0.013)	(0.014)	(0.002)	(0.002)	(0.010)	(0.012)
> 4000€	0.015	0.034	-0.002	-0.005	-0.012	-0.029
	(0.024)	(0.026)	(0.004)	(0.004)	(0.021)	(0.022)
N	2,331	2,285	2,331	2,285	2,331	2,285
controls	no	yes	no	yes	no	yes

Notes: * p<0.1, ** p<0.05, *** p<0.01 Robust standard errors in parentheses. Each horizontal line indicates a new regression. The presented coefficients are average marginal effects.

Table 2.B.6: Tax choices and ideology: by role (drop 10% slowest and fastest participants)

	only rich players			only poor players		
	(1)	(2)	(3)	(4)	(5)	(6)
	low	medium	high	low	medium	high
Panel A *ideology* reference category: right-wing						
left-wing	-0.066**	0.021*	0.045**	-0.019	-0.001	0.020
	(0.027)	(0.011)	(0.019)	(0.015)	(0.002)	(0.016)
N	550	550	550	1,134	1,134	1,134
Panel B *redistribution preference* reference category: against redistribution						
indifferent	-0.034	0.010	0.024	-0.034*	-0.002	0.035*
	(0.032)	(0.010)	(0.023)	(0.020)	(0.003)	(0.021)
pro redistribution	-0.088***	0.025**	0.063***	-0.066***	-0.003	0.069***
	(0.029)	(0.012)	(0.022)	(0.018)	(0.006)	(0.018)
N	706	706	706	1,447	1,447	1,447
controls	yes	yes	yes	yes	yes	yes

Notes: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The presented coefficients are average marginal effects of ordered logistic regressions. Robust standard errors in parentheses. Each horizontal line indicates a new regression. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household members). The 10% slowest and fastest participants are dropped from the sample.

Table 2.B.7: Tax choices, mobility and migration beliefs (drop 10% slowest and fastest participants)

	(1) low	(2) medium	(3) high
Panel A *mobility* reference category: immobile or foreign tax high			
foreign tax low or medium	0.072***	0.001	-0.073***
	(0.014)	(0.006)	(0.014)
N	1,602	1,602	1,602
Panel B *migration belief* reference category: rich does not migrate if domestic tax is high			
rich migrates if domestic tax is high	0.117***	-0.017	-0.100***
	(0.027)	(0.011)	(0.023)
N	1,109	1,109	1,109

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of ordered logistic regressions. Robust standard errors in parentheses. The 10% slowest and fastest participants are dropped from the sample.

Table 2.B.8: Migration choices and beliefs (drop 10% slowest and fastest participants)

	(1) migration choice	(2) migration belief
migration incentives reference category: migration does not pay		
migration does pay	0.343***	0.479***
	(0.006)	(0.009)
N	1,794	3,476

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of logistic regressions. Robust standard errors in parentheses are clustered at the individual level. The 10% slowest and fastest participants are dropped from the sample.

Table 2.B.9: Migration and ideology (drop 10% slowest and fastest participants)

	migration pays		migration does not pay	
	(1)	(2)	(3)	(4)
	migration choice	migration choice	migration choice	migration choice
Panel A *ideology* reference category: right-wing				
left-wing	-0.171***	-0.150***	0.005	0.002
	(0.051)	(0.053)	(0.020)	(0.020)
N	322	319	902	893
Panel B *redistribution preference* reference category: against redistribution				
indifferent	-0.036	-0.022	-0.001	-0.024
	(0.068)	(0.069)	(0.027)	(0.027)
pro redistribution	-0.118**	-0.108*	0.001	-0.018
	(0.059)	(0.060)	(0.024)	(0.023)
N	420	414	1,155	1,140
controls	no	yes	no	yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of logistic regressions. Standard errors in parentheses are clustered at the individual level. Each horizontal line indicates a new regression. We asked the rich for each possible tax rate whether they would migrate. Columns (1) and (2) show results when the domestic tax is high and the foreign tax is low or medium, while columns (3) and (4) cover all other combinations of the domestic and foreign tax rate. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household members). The 10% slowest and fastest participants are dropped from the sample.

Table 2.B.10: Migration beliefs and ideology (drop 10% slowest and fastest participants)

	migration pays		migration does not pay	
	(1)	(2)	(3)	(4)
	migration belief	migration belief	migration belief	migration belief
Panel A *Ideology* reference category: right-wing				
left-wing	-0.021	-0.020	0.041**	0.040**
	(0.021)	(0.022)	(0.018)	(0.018)
N	681	670	1,843	1,813
Panel B *Redistribution preference* reference category: against redistribution				
indifferent	-0.004	0.003	0.043*	0.032
	(0.029)	(0.029)	(0.023)	(0.023)
pro redistribution	-0.003	-0.005	0.030	0.020
	(0.025)	(0.025)	(0.019)	(0.019)
N	875	858	2,356	2,313
controls	no	yes	no	yes

Notes: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The presented coefficients are average marginal effects of logistic regressions. Standard errors in parentheses are clustered at the individual level. Each horizontal line indicates a new regression. We asked the poor for each possible tax rate whether they believe the rich will migrate. Columns (1) and (2) show results when the domestic tax is high and the foreign tax is low or medium, while columns (3) and (4) cover all other combinations of the domestic and foreign tax rate. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household member). The 10% slowest and fastest participants are dropped from the sample.

Table 2.B.11: Tax choices and ideology: by role (probit)

	only rich players			only poor players		
	(1)	(2)	(3)	(4)	(5)	(6)
	low	medium	high	low	medium	high
Panel A *ideology* reference category: right-wing						
left-wing	-0.065***	0.018**	0.047***	-0.022	0.001	0.021
	(0.024)	(0.008)	(0.017)	(0.014)	(0.002)	(0.014)
N	686	686	686	1,429	1,429	1,429
Panel B *redistribution preference* reference category: against redistribution						
indifferent	-0.062**	0.017*	0.045**	-0.025	0.001	0.024
	(0.030)	(0.009)	(0.022)	(0.019)	(0.002)	(0.018)
pro redistribution	-0.083***	0.022**	0.060***	-0.062***	0.002	0.060***
	(0.027)	(0.009)	(0.020)	(0.016)	(0.004)	(0.016)
N	887	887	887	1,825	1,825	1,825
controls	yes	yes	yes	yes	yes	yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of ordered probit regressions. Robust standard errors in parentheses. Each horizontal line indicates a new regression. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household members).

Table 2.B.12: Tax choices, mobility and migration beliefs (probit)

	(1)	(2)	(3)
	low	medium	high
Panel A *mobility* reference category: immobile or foreign tax high			
foreign tax low or medium	0.062***	-0.003	-0.058***
	(0.013)	(0.003)	(0.012)
N	2,038	2,038	2,038
Panel B *migration belief* reference category: rich does not migrate if domestic tax is high			
rich migrates if domestic tax is high	0.094***	-0.015**	-0.079***
	(0.023)	(0.007)	(0.020)
N	1,416	1,416	1,416

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of ordered probit regressions. Robust standard errors in parentheses.

Table 2.B.13: Migration choices and beliefs (probit)

	(1) migration choice	(2) migration belief
migration incentives reference category: migration does not pay		
migration does pay	0.368***	0.526***
	(0.009)	(0.006)
N	2,175	4,442

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of probit regressions. Robust standard errors in parentheses are clustered at the individual level.

Table 2.B.14: Migration and ideology (probit)

	migration pays		migration does not pay	
	(1)	(2)	(3)	(4)
	migration choice	migration choice	migration choice	migration choice
Panel A *ideology* reference category: right-wing				
left-wing	-0.138***	-0.125***	0.012	0.013
	(0.046)	(0.048)	(0.018)	(0.018)
N	405	401	1,146	1,123
Panel B *redistribution preference* reference category: against redistribution				
indifferent	-0.062	-0.054	-0.012	-0.041*
	(0.063)	(0.062)	(0.025)	(0.024)
pro redistribution	-0.136**	-0.128**	-0.009	-0.037*
	(0.054)	(0.055)	(0.022)	(0.021)
N	536	529	1,477	1,448
controls	no	yes	no	yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are average marginal effects of a probit regression. Standard errors in parentheses are clustered at the individual level. Each horizontal line indicates a new regression. We asked the rich for each possible tax rate whether they would migrate. Columns (1) and (2) show results when the domestic tax is high and the foreign tax is low or medium, while columns (3) and (4) cover all other combinations of the domestic and foreign tax rate. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household members).

Table 2.B.15: Migration beliefs and ideology (probit)

	migration pays		migration does not pay	
	(1)	(2)	(3)	(4)
	migration belief	migration belief	migration belief	migration belief
Panel A *Ideology* reference category: right-wing				
left-wing	0.003	0.000	0.025	0.022
	(0.020)	(0.020)	(0.016)	(0.016)
N	872	855	2,348	2,300
Panel B *Redistribution preference* reference category: against redistribution				
indifferent	-0.028	-0.025	0.057***	0.043**
	(0.026)	(0.026)	(0.021)	(0.021)
pro redistribution	-0.004	-0.004	0.033*	0.024
	(0.023)	(0.023)	(0.019)	(0.019)
N	1,113	1,086	2,990	2,921
controls	no	yes	no	yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. The presented coefficients are estimated by a linear probability model. Standard errors in parentheses are clustered at the individual level. Each horizontal line indicates a new regression. We asked the poor for each possible tax rate whether they believe the rich will migrate. Columns (1) and (2) show results when the domestic tax is high and the foreign tax is low or medium, while columns (3) and (4) cover all other combinations of the domestic and foreign tax rate. Controls include dummies for gender, marital status, higher education, four age dummies (30-39, 40-49, 50-59, > 60), and two dummies for household size (2 and 3 or more household member).

2.C Questionnaire

The following survey differs from the usual format. You are participating in an experiment about taxation and redistribution in a fictitious scenario.

The experiment works as follows: You are assigned a role, make decisions and get rewards depending on your own decision as well as the decisions of other participants.

You do not need any prior knowledge to answer any of the questions. You get the usual payment of 4€ for participating in the GIP survey. On top of that you can win up to 80€ based on the decisions that you make. You cannot lose any money under any circumstances.

Imagine that you are living a small country (henceforth called home country) consisting of three people: One person with a high income and two people with low income.

Your role, poor or rich, will be assigned to you randomly. The rich has to pay a tax that is redistributed by the government to the two poor. The rich has an income of 90 and the poor both have an income of 20. The tax on the rich can take three values: low, the rich pays 5 to each of the poor (10 in total), medium, the rich pays 10 to each of the poor (20 in total) and high, the rich pays 20 to each of the poor (40 in total).

[Only for mobility treatment] There is a second foreign country. The rich can migrate to the foreign country. If she does so, she does not have to pay taxes in the home country. Instead, she has to pay taxes in the foreign country. The tax rate in the foreign country is (low/medium/high). Furthermore, she has to pay migration costs of 15.

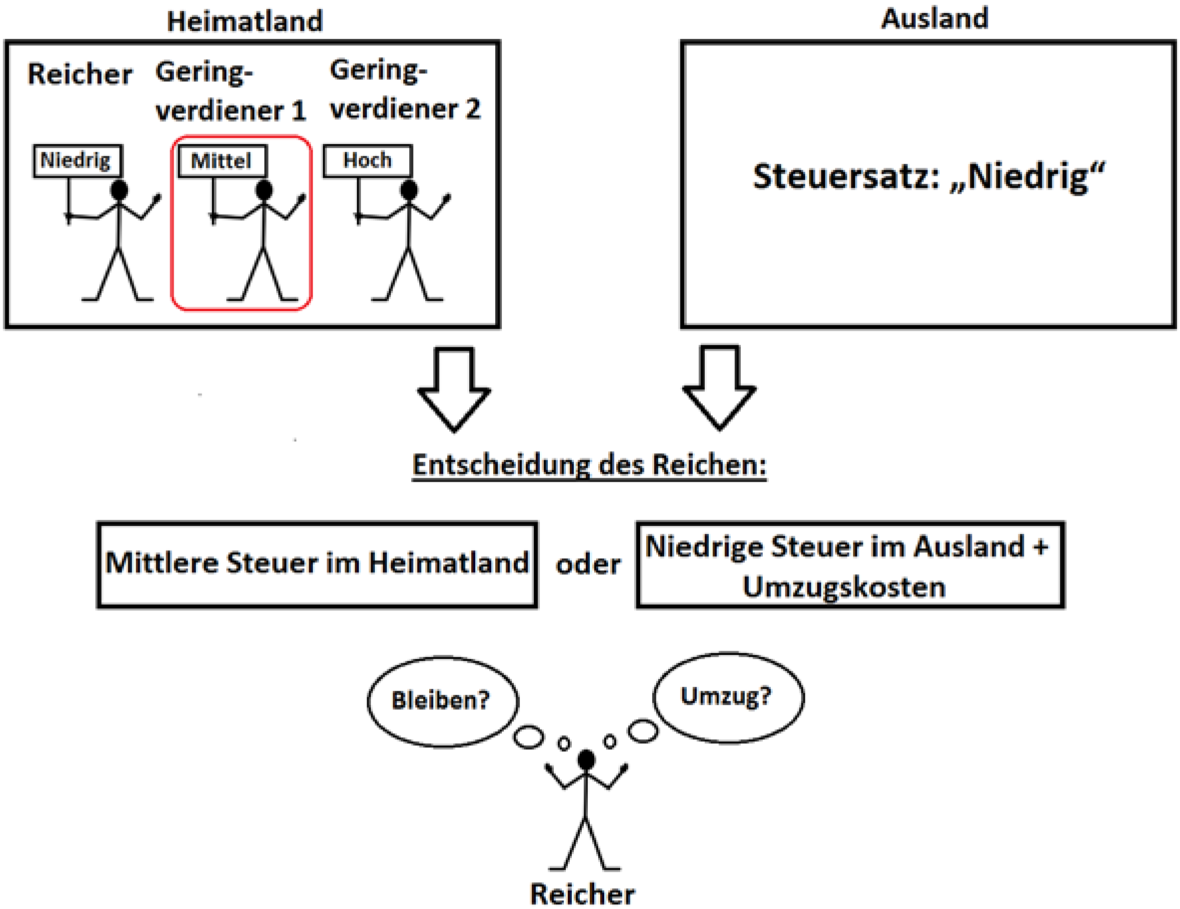
We will ask you to vote on the tax rate in the home country. The tax rate is then determined by a randomly choosing the preferred tax rate of one of the three people in the home country.

[Only for mobility treatment] The rich can decide to migrate after observing the chosen tax rate.

You are in the role of the (poor/rich). We will form countries by randomly choosing groups of three participants (one rich, two poor) after all participants have finished the survey. You will get notice of the tax rate chosen in your country via email. Furthermore, we will randomly choose 20 countries, in which all participants will be paid according to their after-tax income.

What tax rate are voting for in your home country? (low/medium/high)

Figure 2.C.1: Instruction example



What tax rate do you think will the (rich/two poor) vote for? (low/medium/high)

[Only for mobility treatment and role of the poor] **Do you think the rich will migrate?**

The rich will migrate if the home tax rate is high. (yes/no)

The rich will migrate if the home tax rate is medium. (yes/no)

The rich will migrate if the home tax rate is low. (yes/no)

[Only for mobility treatment and role of the rich] **Will you migrate to the foreign country?**

I will migrate if the home tax rate is high. (yes/no)

I will migrate if the home tax rate is medium. (yes/no)

I will migrate if the home tax rate is low. (yes/no)

2.D Laboratory Experiment

2.D.1 Experimental Design

Prior to the survey experiment we ran a more conventional laboratory experiment, in which 108 individuals (mostly students) participated. The general setup parallels the one described above in Section 2. In particular, we kept the composition of a country with one rich and two poor individuals, the distribution of gross incomes, the set of tax rates, the cost of mobility, and the random-dictator voting mechanism. There were, however, also important differences conceptually and in the implementation.

At the conceptional level, there are a number of important differences. One concerns the nature of strategic interaction. In the survey experiment we paired subjects ex post to determine payoffs and used the strategy method to see how subjects make choices conditional on assumed behavior elsewhere. By contrast, in the lab experiment we paired subjects into countries during the experiment and thereby created full strategic interaction. This also allowed us to match countries into pairs where tax choices were endogenously chosen in both rather than being matched with a foreign country with exogenously given tax rate. As a result, countries could not only lose a rich player, but also attract one from another country. A further consequence is that tax payments of a rich player who migrates are not lost because they are paid in another country consisting of experimental participants. As a further difference, the laboratory setting with strategic interaction allowed us to study behavioural dynamics as we repeated rounds of tax and migration choices. After each round subjects are informed about tax rate and migration choices in both countries. Role assignments and the matching of participants into countries and countries into pairs remained fixed during the course of the experiment.

In addition, the assignment of roles differed. In the lab experiment the roles of rich and poor were based on the outcome of a simple, five-minute calculating exercise. The best performing third of subjects in adding four two-digit numbers were awarded the role of a rich person, who has much higher gross income. Finally, in the survey experiment we have a representative sample of the German adult population, while subjects in the lab experiment are mostly students.

In the lab experiment we considered two treatments. In the *ImmobMob* treatment subjects repeated the *no mobility* setup with no migration option for the rich 15 rounds, followed by another 15 rounds of the *mobility* setup with potential migration of the rich. In the second treatment *MobMob*, subjects interacted 30 rounds in the same setup with the migration option. In both treatments subjects were informed about the nature of the interaction in the second phase only after phase 1. At the beginning of the experiment, however, subjects were told that the experiment lasts for 30 periods and new instructions are provided after 15 rounds of play. The two treatments allow us to compare the role of mobility both across subject pools (periods 1-15 in the two treatments) as well as within the same subject pool (periods 16-30 versus periods 1-15).

Subjects were paid on the basis of one randomly chosen period of each phase. Four points in the experiment translated into one Euro payout. No show-up fee was paid. The experiment was conducted in the computerized mLab at the University of Mannheim, using z-Tree (Fischbacher, 2007) and ORSEE (Greiner, 2015).

2.D.2 Results

Table 2.D.1 displays the distribution of tax votes by treatment, phase, and type. A large majority of tax votes is in line with the equilibrium prediction assuming selfishness. Particularly noteworthy are the 92% of poor voting for high tax rates in the absence of mobility, as well as the almost fully selfish play by rich subjects who choose almost always low tax rates, although there are some votes for medium tax rates in the absence of mobility. Interestingly there are also some votes for high taxes among the poor when the rich are mobile. Note that phase-2 behavior is nearly identical across treatments.

Table 2.D.1: Tax choices in the lab

			Poor			Rich	
Treatment	Phase	Low	Medium	High	Low	Medium	High
ImmobMob	no mobility	2.4%	5.6%	92.0%	88.5%	11.1%	0.4%
	mobility	7.0%	73.0%	20.0%	97.8%	2.2%	0.0%
MobMob	mobility	5.4%	57.2%	37.4%	99.6%	0.4%	0.0%
	mobility	5.2%	69.3%	25.6%	97.4%	2.6%	0.0%

We test for treatment differences using linear and ordered probit regressions with standard errors clustered at individual level and linear regression with individual-level random effects. For poor subjects we find that they vote for higher taxes without mobility (as expected) and vote for lower taxes in the second than in the first phase of treatment 2 (*MobMob*). As for rich subjects, they vote for higher taxes without mobility (contrary to expectation), and there are no differences across phases, as well as no treatment difference in the second phase.

We also take a closer look at the role of party preference for the tax vote. We use linear regressions with individual-level random effects in which the omitted category are those without stated party preference. In the role of poor players, voters of the Green Party choose higher taxes ($p < 0.05$), while FDP ($p < 0.05$) and Left Party ($p < 0.1$) voters choose lower taxes. Among rich players Pirate-party supporters choose higher taxes ($p < 0.05$). There are no other significant differences. In general, these results are derived from a low number of observations and thus have little statistical power

(40 out of 108 subjects in the lab experiment did not answer the question about party preference). An exception are the results for supporters of the Green Party.

As substitute for party preference, we use survey questions on political attitudes among participants in the lab experiment. We ask to rate various statements such as (1) socialism/capitalism is a good idea, (2) the rich should show solidarity with the poor, and (3) society is largely fair. Essentially none of them has a significant impact once we control for dependence of observations. Only the attitude toward socialism and a dummy for the role of luck for economic outcomes are significant for the tax choice of the rich if we include treatment and other controls. However, many coefficients do not even have the expected sign. An example is that the belief that luck determines income is related to lower tax choices of the rich.

We finally turn to an analysis of the migration behavior by rich subjects. Rich players almost always switch when they should: When the tax rate in the own country is high, while low or medium in the other country, and thus the condition for profitable migration is met, the switch rates are between 82% and 100%. Rich players very rarely switch when they should not. Exceptions are the following: Migration rates are 19% from a medium tax country to a low tax country, and 12.5% between high tax countries. These choices could be attempts to try to force poor participants in one's own country to vote for lower taxes even if these migrations choices are costly in the short term. Overall, out of 131 migration choices made by rich players only 19 are not in line with the equilibrium prediction assuming selfishness.

More systematic analysis using probit regression with individual-level random effects shows that the probability to migrate increases in the tax rate in own country and decreases in the tax rate in other country, as one would expect. Supporters of the center-left Social Democrats (SPD) are more and supporters of center-right Christian Democrats are less likely ($p < 0.05$) to migrate than those without stated party preference.

2.D.3 Discussion

There are several potential reasons related to differences in design and implementation why we see fewer deviations from the selfish equilibrium predictions in the laboratory than in the survey experiment. First, it is (primarily) a student sample. However, there are also a number of students among the participants of our survey experiment and they do not show substantially different behavior than the non-student participants. Second, the game in the laboratory is repeated. However, play in the first period is not substantially different than in later periods. Third, roles are earned. Possibly, this may have created a feeling of entitlement for those players in the "rich" role, in line with earlier findings for dictator games by Cherry, Frykblom, and Shogren (2002), whereas those in the "poor" role do not agree that the "rich" deserve their better position. This may be driven by a self-serving interpretation whether effort and skill or luck determine the outcome of the real-effort task. Fourth, participants in the laboratory experiment are experienced and self-selected into taking part in the experiment and might therefore be more in a mode to earn money while the participants in the online survey might consider it more appropriate to answer what is “right”. This is consistent with the finding of Snowberg and Yariv (2021) that generosity in student populations reflects a lower bound of generosity in the overall population.

Chapter 3

Austerity and Distributional Policy

Joint with Matteo Alpino, Zareh Asatryan and Sebastian Blesse.

3.1 Introduction

In the aftermath of the financial crisis, increasing debt levels around the world have put austerity measures on the agenda of many policy-makers and academics. While efficiency aspects have been widely debated (see, among others, Alesina, Favero, and Giavazzi, 2019), much less is known about the distributional effects of austerity. At the same time, distributional concerns played a large role in the public debate, with most people arguing that austerity hurts the poor disproportionately (Blyth, 2013; Mendoza, 2014; Varoufakis, 2016).

The aim of this chapter is to provide the first quasi-experimental evidence on the effect of fiscal austerity on distributional policy.[1] To do so, we study a large exogenous reduction of the fiscal space of Italian municipalities caused by the imposition of a fiscal rule, the Domestic Stability Pact (DSP), by the national government. More specifically, our quasi-experiment relies on a reform in 2013 that extended the budget surplus requirement to previously exempted municipalities based on a population cutoff, giving rise to a difference-in-discontinuity design. Italy is well-suited to study our research question due to the substantial autonomy that Italian municipalities have over local non-linear income taxes as well as redistributive spending.

We find that local governments respond to the introduction of the fiscal rule by increasing tax rates in a progressive way: the increase in tax rates increases in income and only becomes significant for taxpayers located above the median taxable income. This finding is consistent with the median voter predictions of Meltzer and Richard (1981)

[1] Previous quantitative work has mostly appeared in response to the global financial crisis, and it usually finds that periods of fiscal austerity are associated with an increase in income inequality (Ball et al., 2013; Heimberger, 2018; Woo et al., 2013). The microsimulations of Avram et al. (2013) and Paulus, Figari, and Sutherland (2016) on several European countries present a more nuanced picture on the distributive effects of austerity that depend on country contexts and measures of austerity.

and Bierbrauer, Boyer, and Peichl (2021). The relative effects are sizeable, with tax rates on earners in the top decile of the municipal income distribution increasing by 13% compared to the sample mean, and by about 3.5 times more compared to the lowest decile. Part of this effect is driven by municipalities switching to a progressive schedule in tax rates, and part of it is due to increases in the level of the exemption threshold.

Since local income tax rates are small in absolute magnitude in Italy, these reform-induced tax rate changes imply only small increases in realized tax revenues. Whereas annual income tax revenues increase on average by about 5€ per capita, revenues from the top bracket increase by an order of magnitude more, amounting to about 73€ per capita on average. Contextual evidence supports the conjecture that, unlike its small absolute size, the local income tax is politically a very salient tax tool of redistribution at the local level.[2] We do not find evidence that the reform affects other local taxes or non-tax revenues raised by municipalities, including the property tax.[3] We also do not find evidence for adjustments in overall or redistributive spending, suggesting that a reduction of public goods provision is unlikely to offset the progressive effects of the local income tax.

Next, we study whether mayors, the crucial decision makers at the local level, respond to austerity in a heterogeneous manner. Our analysis is motivated by the theoretical work of Bierbrauer and Boyer (2013), who introduce vote-share maximizing politicians with ex-ante valence differences in a Mirrleesian model of income taxation. They show that the high-valence candidate is able to capitalize on her advantage and target the majority consisting of relatively poorer voters by proposing a progressive tax schedule, whereas the low-valence politician is left to lobby for the votes of the rich. Consistent with these results, we find that the increase in tax progressivity is driven by mayors with a college degree or working in a high-skill occupation, while other observable characteristics, such as age, gender, party affiliation, do not play a meaningful role. On the contrary, mayors without a college degree or those working in low-skilled occupations rely on flat increases in the local income tax to comply with the reform. To address the issue of selection of mayors, we compare the outcomes of the two types of politicians elected in close races and find similar results.

Finally, we test whether the introduction of the DSP had electoral consequences for the incumbents. While we do not find such evidence for the average mayor, we show that differences in adjustment strategies between high- and low-skilled mayors caused large differences in electoral outcomes. In the first election following the imposition of the fiscal rule, low-skilled incumbents were on average 30 to 37 percentage points less likely to be reelected conditional on running for office again, whereas high-skilled mayors did not experience a significant decline in their reelection prospects. Crucially, these differences in reelection odds only manifest after the reform, and not before. These

[2] For example, the deputy mayor of Corciano said in 2019: "For those who like me earn 1,250€ net per month, the increase is equal to 19.32€ per year" and concluded that "by giving up one pizza a year we help 5,349 citizens who earn less than us".

[3] Potentially the property tax has distributive implications, but since 2013 there was limited scope to increase revenues using this instrument. See Appendix 3.A.4 for more details.

findings suggest that politicians implement progressive tax reforms in order to stay in office, and that high-skilled mayors are more able or more willing to use such a strategy than low-skilled mayors.

We interpret our findings as the impact of austerity on distributional policy. Considering the introduction of the DSP as a case of austerity is natural because it necessarily required a fiscal adjustment in municipalities where the rule bound. Consistent with this interpretation, previous evidence shows that the DSP induces substantial fiscal consolidation (Chiades and Mengotto, 2015; Coviello et al., 2019; Grembi, Nannicini, and Troiano, 2016). Contextual details of the Italian economic situation of the time further reinforce our interpretation: the reform took place in the midst of a severe recession caused by the sovereign crisis, with Italian real GDP shrinking by 3% in 2012 and by 1.8% in 2013, while the central government cut transfers to municipalities several times between 2009 and 2015 (see Figure 3.B.1). The DSP, vertically imposed by the national administration upon municipalities, became a symbol of austerity in the eyes of local administrators and was very unpopular among mayors across the political spectrum.[4]

While it is very challenging to identify reduced-form causal effects of austerity measures in a cross-country setting, an important question is whether our estimates can be generalized beyond the municipal context. To probe the external validity of our municipal findings, we show that a similar relationship holds in a cross-country regression using the cyclically adjusted primary balance as a measure of austerity (Alesina et al., 1998). As Figure 3.A.1 and Table 3.A.1 show, conditional on country and year fixed effects a 1% percentage point increase in the cyclically adjusted primary balance is significantly associated with a 0.6 percentage point increase in the marginal income tax rate at the top, while we observe no correlation between austerity and marginal income tax rates at mean levels of income.

This chapter contributes to a large and important strand of literature studying the political economy of taxation (Acemoglu et al., 2015; Persson and Tabellini, 2002) by providing an empirical evidence to a largely theoretical literature. Past research is based on models of voting over tax schedules with competition between parties (Downs, 1957) and candidates (Besley and Coate, 1997; Osborne and Slivinski, 1996; Panunzi, Pavoniz, and Tabellini, 2020). While most of this literature, such as Meltzer and Richard (1981), analyze the political economy of linear income taxes, our contribution is to study non-linear taxes, which are much more prevalent in practice. In particular, our baseline result that upon an exogenous shock governments increase marginal tax rates for taxpayers located above the median income is consistent with Bierbrauer, Boyer, and Peichl (2021) who characterize the conditions of politically feasible non-linear tax reforms. In addition to this theoretical work, our evidence is in line with historical explanations for the occurrence and rise of progressive taxation. This literature emphasizes the role of compensatory arguments as the main mechanism behind the popular support and

[4] For example, at a rally against the DSP in November 2012 hundreds of mayors from all major parties rallied behind a banner saying "Let us set our municipalities free from the stupidity pact." According to news accounts, the extension of the DSP to municipalities below 5,000 inhabitants in 2013 sparked similar outrage among mayors of these towns.

ultimately the implementation of progressive taxes (Scheve and Stasavage, 2012). The idea is that high taxes on the rich allow politicians to compensate the majority of relatively poor voters for some fundamental unfairness induced by the state. Given that in our sample period Italy endured a double dip recession and that the DSP was very unpopular, this line of argument is also consistent with our results.

Our findings also relate to a rather polarized literature interested in understanding the political costs of fiscal austerity. One strand of this literature finds that incumbent politicians do not face electoral costs when implementing fiscal consolidations at the national level (Alesina, Carloni, and Lecce, 2012; Arias and Stasavage, 2019; Brender and Drazen, 2008).[5] On the other hand, a number of papers show that fiscal austerity has negative effects on voter support for the incumbent (Hübscher, Sattler, and Wagner, 2018; Talving, 2017) as well as on broader socio-political outcomes such as increasing support for right-wing populism (Dal Bo et al., 2018; Fetzer, 2019), or increasing social unrest (Ponticelli and Voth, 2019). We contribute by showing that austerity can indeed carry significant electoral costs, but that these costs depend on the consolidation strategy. In particular, we show that electoral costs can be mitigated by mainly increasing taxes on high-income earners.

Last, we contribute to the literature on the effects of fiscal rules. Grembi, Nannicini, and Troiano (2016) find that an earlier reform of the DSP significantly reduced municipal deficits. Asatryan, Castellon, and Stratmann (2018) show constraining effects of balanced budget rules on debt, but only for a class of rules that are enshrined in national constitutions, while Eliason and Lutz (2018) show that a comprehensive state-level rule in Colorado does not affect public finances, which is partly due to non-compliance with the rule. A meta-study by Heinemann, Moessinger, and Yeter (2018) finds that numerical fiscal rules constrain fiscal policy, but this result weakens if only refined identification strategies are considered. Fiscal rules have also been shown to curb corruption (Daniele, Giommoni, and Orlando, 2021), to alleviate political budget cycles (Repetto, 2018) and to worsen the selection of politicians (Gamalerio, 2019).

The remainder of this chapter is structured as follows. Section 3.2 discusses the institutional framework of Italian municipalities and the reform of the fiscal rule that we are exploiting. We outline our identification strategy and the data in Sections 3.3 and 3.4, respectively. Section 3.5 presents the main empirical results and their robustness tests, while Section 3.6 studies their political economy implications. We conclude with Section 3.7.

[5] Possible explanations are that voters are fiscally conservative (Peltzman, 1992), that leaders implement fiscal austerity in times and as part of policy packages that allow them to electorally survive these reforms (Bansak, Bechtel, and Margalit, 2020), or that the divergent framing of the same issue provided by partisan media mitigates voter responses (Barnes and Hicks, 2018).

3.2 Institutional Setup

3.2.1 Municipal Fiscal Rule

Since 1999, Italian municipalities have been subject to a fiscal rule, the Domestic Stability Pact (*Patto di stabilita' dei comuni*), introduced by the national government. Originally, all municipalities were subject to the fiscal rule, but in 2001 those below 5,000 inhabitants were excluded. In 2013, the threshold was lowered to 1,000, which is the reform that we exploit. Finally, in 2016, the Domestic Stability Pact was abolished and a balanced budget rule for all municipalities was introduced.

In our period of analysis, the Domestic Stability Pact's target object has always been the *Saldo Finanziario*, which is defined as the difference between expenditures and revenues, net of repayment of outstanding debt and of lending. Some budget items were always or occasionally excluded from the *Saldo Finanziario* (e.g. spending for natural disaster relief, EU structural funds). The formula to calculate the numerical target varied over the years, but it was usually defined as a function of budget items in previous years (see Table 3.B.1).

Monitoring of compliance by the central government was tightened in 2008 with the introduction of a compulsory reporting system, and of severe punishment for non-compliers by the central government (Coviello et al., 2019). For instance, punishments include bans on hiring, cuts of transfers from the central government (proportional to the deviation from the rule), salary cuts to mayors and city councilors, a growth cap on current spending at zero percent as well as a ban on new municipal debt. Qualitative evidence from the Ministry of the Interior suggests that the central government implemented the reform quite thoroughly.[6] Dovis and Kirpalani (2020) show that the fiscal behavior of local governments will crucially depend on central government's reputation, and the strict regulations and enforcement practices of the Italian context suggest that it is very unlikely that Italian local governments tried not to comply with the DSP.

3.2.2 Municipal Governance

Municipal governments are composed of a city council, an executive committee, and the mayor. In municipalities with less than 15,000 inhabitants, each candidate for the mayoral office has to be supported by a list of candidates for the city council. Voters cast a single vote for a mayoral candidate, and can express one preference vote for one council candidate within the same list. The mayoral candidate who gets the most votes is elected as mayor. The seats in the city council are split as follows: 2/3 to the list of the mayor, and 1/3 split across the other lists in proportion to their votes shares. The mayor appoints the members of the executive committee, and can also remove them from office at any time. The mayoral term is five year long, and the mayor cannot serve

[6] More than one hundred municipalities faced legal procedures according to ministerial decrees available on the website of the Ministry of the Interior.

for more than two consecutive terms.[7] These institutional details make the mayor the most important player in municipal politics, while the city council's influence is more limited. The list supporting a mayoral candidate is sometimes backed by national-level parties or coalitions, but is often independent (so-called civic lists), especially in small municipalities. Also, since being a politician in a small town is not a full-time job, most mayors work in their normal job while being in office.

3.2.3 Municipal Fiscal Policy

The municipal budget is financed with transfers from higher levels of government and international institutions, and by municipal resources such as local taxes and fees connected to the use of public services. Local taxation plays an important role in municipal revenues, averaging about 21% of total revenues in our sample period (see Figure 3.B.1). The three largest tax instruments in terms of revenues are the property tax, the local income tax and the waste tax, accounting for 8.7%, 4.4% and 7.9% of total revenues in 2015 respectively. In this paper, we focus on the local income tax surcharge, as it allows different degrees of progressivity and its distributional impact is straightforward. The property tax and the waste tax potentially also have distributional consequences, but those are more complicated to detect and to analyze.[8] Furthermore, the upper bound on the main local property tax rate was significantly decreased by the national government in 2013 and 2014, leaving limited scope to increase revenues from this instrument in response to the introduction of the DSP.[9]

In 1999, the local income tax was introduced as a municipal surcharge on the national income tax to grant municipalities more tax autonomy. In our sample period, the income brackets of the national income tax were split at 15,000€, 28,000€, 55,000€, and 75,000€, with their respective marginal tax rates being 21%, 27%, 38%, 41% and 43%.[10] In general, the tax base is composed of wage income, pension income, self-employed income, capital income, rents, and other sources of income. However, income from several sources can be subject to alternative and more favorable taxation (e.g. rents from real estate, investment in government bonds, self-employed income below a certain threshold), so the bulk of the taxable income consists of wage and pension income.[11]

The revenues from the municipal surcharge are based on the residency principle and flow completely to the municipal budget. Starting in 1999, the law allowed municipalities to apply uniform tax rates of up to 0.5% of taxable income on top of the national

[7] This was extended to three terms in 2014 for municipalities below 3,000 inhabitants.

[8] For more information on the distributional consequences of these fiscal instruments see Messina and Savegnago (2014) and Messina, Savegnago, and Sechi (2018).

[9] We test the effects on the property tax and report the results in Appendix 3.A.4 together with additional institutional details.

[10] The final tax bill is the gross tax bill net of deductions (*detrazioni*). The gross tax bill is calculated applying the tax rates on taxable income. The taxable income is calculated as total income net of exemptions (*deduzioni*).

[11] Approximately 80% both in terms of taxpayers and of taxable income in 2011.

tax rates. In the period from 2007 to 2011, the cap was raised to 0.8% and municipalities were given the autonomy to set an exemption threshold: tax payers with income below the threshold were fully exempted from the tax, while those above would pay a tax calculated on their total income. Since 2012, municipalities can also set differentiated tax rates in every bracket of the national income tax schedule. In other words, since 2007 municipalities can levy non-linear income taxes. The increase in flexibility of this tax instrument was coupled with technical assistance from the Ministry of Finance: at least since 2011, municipal officials have access to an online calculator that uses individual level data from the tax administration allowing to simulate how revenues and tax base respond to changes in tax rates and in the exemption threshold. In particular, users can vary the following parameters: tax revenues, tax rates, the exemption threshold, and the number of exempted tax payers (broken down by employees, retirees and self-employed). This setting allows us to study the progressivity of income taxation at the local level.

The adoption of differentiated tax rates by municipalities has evolved quickly over time and increased even further with the 2012 reform (Giommoni, 2019). Restricting attention to small municipalities (below 2,500 inhabitants), no municipality operated under a regime with an exemption threshold and a flat tax, 67% implemented a flat tax without exemption, and 33% did not introduce any surcharge in 2007. In 2015, 8% operated a system with five tax rates, with or without exemption, 12% implemented a flat tax with exemption, 56% implemented a flat tax without exemption, and 24% did not have any surcharge. Conditional on having a exemption threshold, the average threshold is about 10,000€ with considerable variation around the mean (see Figure 3.B.2).

Municipalities account for about 10% of total public expenditures (Grembi, Nannicini, and Troiano, 2016). They are responsible for providing a variety of public services, such as administrative services (30% of municipal expenditures in our sample period 2007-2015), waste and water management (24%), public transport and maintenance of municipal roads (15%), social services (8%), education services (7%), culture and recreation (5%), economic development and tourism (3%), and local police and judiciary (2%).

Spending on social and educational programs is of special importance to us, given their potential redistributive nature and Italian municipalities' relatively large discretion over these items. Social spending includes, among others, assistance to poor people, child care, or care for elderly. Education expenditures on the municipal level comprise of spending for pre-school and primary school services, such as refectories and school buses. In our sample period, Italian municipalities are only allowed to take up loans to finance new investment expenditures if the total amount of interest paid was lower than a certain fraction of revenues from taxes, fees and transfers.[12] The main source of borrowing for small municipalities are loans from the Italian Public Investment Bank (*Cassa Depositi e Prestiti*) accounting for almost 80% of debt holdings.

[12] The fraction varied over time, from 15% in 2007 to 10% in 2014.

3.2.4 The Local Income Surcharge in Municipal Politics

Anecdotal evidence from newspapers, social media, electoral platforms and council's minutes suggest that incumbent mayors often refer to the local income surcharge tax in public statements. When raising the exemption threshold as well as the tax rates for high incomes, mayors underline that these reforms increase progressivity, are fair, and help disadvantaged people with little cost for others. For example, the mayor or Brandico wrote in his 2014 electoral platform: "To help disadvantage people, we need to raise the exemption threshold [...] and to introduce progressivity (by raising tax rates more for higher brackets)". The mayor of Milano wrote on Facebook in 2019: "[...] The exemption threshold raises from 21,000 to 23,000 euros, extending the no tax area to 50,000 more citizens. [...] It is the right thing to do to support households and workers." There are also instances when opposition politicians blame incumbents for not exploiting the tax flexibility and implementing a flat tax instead.[13] These anecdotes suggest that the local income surcharge is an important topic in municipal politics, and are consistent with recent empirical evidence that the introduction of differentiated rates generated an election cycle: the surcharge tends to decrease before elections and increase afterwards (Giommoni, 2019). As documented in the same paper with data on google searches and surveys, the municipal income surcharge is a salient fiscal instrument for taxpayers. This is consistent with the fact that the amount paid due to this tax is usually clearly visible on the monthly payslips received by employees and retirees.

3.3 Data

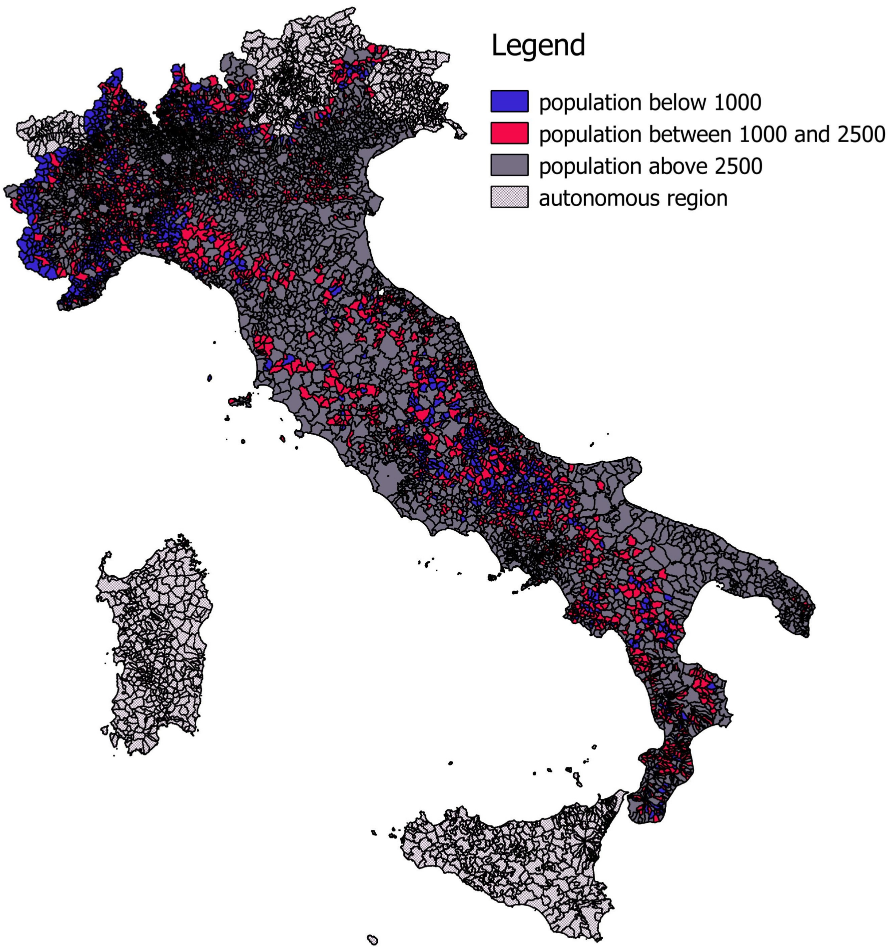
3.3.1 Sample

Our sample starts in 2007, the first year municipalities were allowed to levy non-linear income taxes, and ends in 2015, since all municipalities were subject to a new rule in 2016. We apply a number of restrictions on our sample. First, we drop all observations that are part of a union for inter-municipal cooperation (*Unione dei Comuni*) and at the same time have less than 1,000 inhabitants, since these municipalities are subject to the fiscal rule irrespective of their population (931 municipalities). Second, we drop all municipalities located in one of the five autonomous regions (*Friuli-Venezia Giulia*, *Sardegna*, *Sicilia*, *Trentino-Alto Adige*, and *Valle d'Aosta*), since they are granted a special status by the Italian constitution allowing them to set their own rules (1,392 municipalities). Third, we drop all municipalities that merged in the sample period (79 municipalities). Altogether, our final sample consists of 6,638 municipalities, which represent about 82% of all Italian municipalities.

Our empirical strategy relies on comparing municipalities that are above and below the 1,000 population threshold (see next section). Our estimating sample will always exclude municipalities with more than 2,500 inhabitants, as they are too close to the next

[13] For example, the opposition group *Borgo in Comune* in Borgo San Lorenzo (18,000 inhabitants).

Figure 3.3.1: Map of Italian municipalities



policy-relevant threshold at 3,000 inhabitants.[14] Figure 3.3.1 shows a map of municipalities in our sample, distinguishing between municipalities below 1,000 inhabitants (blue) and those between 1,000 and 2,500 (red). Due to our population restrictions, our sample is composed mainly by municipalities located on the Alps or on the Apennines, the two main Italian mountain ranges. The map suggests also that blue and red municipalities are distributed rather uniformly along these two mountain ranges. Table 3.B.3 in the Appendix shows summary statistics of all variables for the whole sample as well as for municipalities below and above 2,500 inhabitants.

[14] Note that our estimates actually leverage on variation from an even smaller sample of municipalities located around the 1,000 population threshold, and selected using the optimal bandwidth by Grembi, Nannicini, and Troiano (2016). See section 3.4 for more details.

3.3.2 Municipal Tax Rates

We collect annual information on the local income tax from the Italian Ministry of Finance. This includes marginal tax rates for all income brackets and exemption levels at the municipal level. We also obtain the (approximate) municipal-level income tax base distribution from the Italian Fiscal Agency (*Agenzia delle Entrate*). In particular, for every municipality we observe both the number of taxpayers and the tax base in a number of income brackets.[15] We make the simplifying assumption that taxpayers are uniformly distributed within the brackets in order to construct income deciles on the municipality level. This allows us to know the tax rates that apply to each income decile of the respective municipality, e.g., the statutory tax rate that a household earning as much as the 90th percentile of the municipal income distribution has to pay. Using these tax rates as outcome variables allows us to gauge which part of the distribution is affected by changes in tax policy.[16] As discussed above, one can distinguish between three different tax regimes: a uniform tax, an exemption level and a uniform tax, or a fully differentiated tax schedule. We plot the sample mean of the average tax rates for municipalities in the three tax regimes before (Figure 3.3.2a) and after the fiscal rule reform (Figure 3.3.2b). As the blue line indicates, the average uniform tax rate is about 0.48%. For both municipalities with an exemption threshold and those with a fully differentiated tax schedule, the mean tax rate monotonically increases along the municipal income distribution.

To test the distributional effect of the fiscal rule we employ several outcome measures. First, we directly look at the tax rates at the nine income deciles of the municipal income distribution. Second, we study the level of the exemption threshold. Third, we use a binary indicator of whether a given municipality has a progressive tax system or not. Fourth, to obtain a comprehensive measure of progressivity, we use two indicators from the literature: the average and marginal rate progression (Peter, Buttrick, and Duncan, 2010). We construct these variables by running the following regression for each municipality-year pair (i, t) separately:

$$TaxRate_{yit} = \beta_0 + \beta_1 log(y) + \epsilon_{ity} \ \ \forall \ y \in \{1000, 2000, ..., 99000, 100000\} \tag{3.1}$$

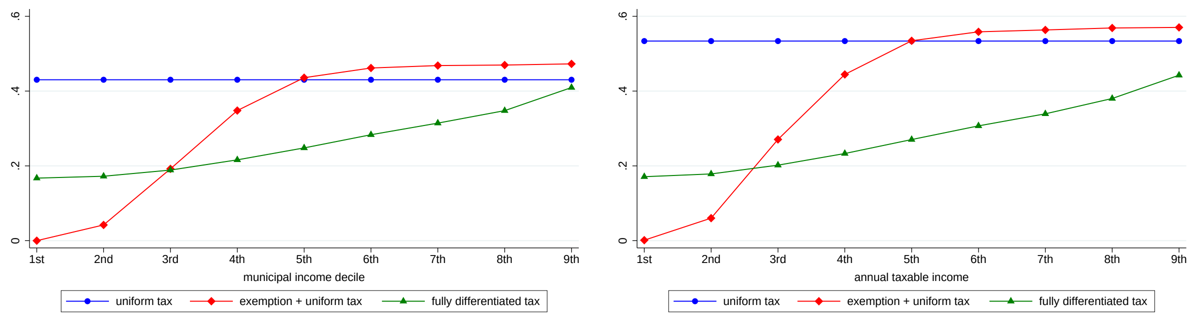
where $TaxRate_{yit}$ is the average (marginal) tax rate at income y in municipality i in year t, and β_1 is an estimate of the average (marginal) rate progression. We normalize the progressivity measures with their sample standard deviations to ease interpretation. The resulting coefficient is by construction negative for regressive, zero for flat, and positive for progressive tax schedules.

Furthermore, we calculate income tax revenues by income brackets. Specifically, we take the product of the total tax base and the average tax rate for each individual bracket. To derive the average tax revenues per taxpayer, we then divide by the number of tax payers in the specific bracket.

[15] See Figure 3.B.3 for a histogram of the number of taxpayers in each bracket.

[16] We also use the average tax rate paid on annual incomes from 5,000 to 75,000€ as alternative outcomes.

Figure 3.3.2: Municipal income tax: average tax rates



a) before reform **b)** after reform

Notes: The figure presents the mean average tax rates in the deciles of the municipal income distribution for three groups of municipalities: those with a uniform tax (blue line), those that have an exemption level and a uniform tax rate that applies to income exceeding the exemption level (red line), and those with a fully differentiated tax schedule (green line). The sample includes only municipalities with less than 2,500 residents. Panel (a) presents data for the period 2007-2012; panel (b) for the period 2013-2015.

3.3.3 Municipal Budget

We complement the data above with municipal budget data from the Italian Ministry of the Interior (*Certificati Consuntivi*). These include detailed accounts of revenues and expenditures. Budgets report figures according to two accounting criteria: cash and accrual bases. We use the latter, since policy changes are reflected in accrual accounts more quickly. We convert all monetary values into 2015 euro and per capita figures using the CPI series and annual population counts from the Italian National Institute of Statistics (ISTAT). Furthermore, we winsorize all budget variables at the first and 99th percentile to account for outliers.

Expenditure figures are split between capital and current spending, and are further disaggregated in broad categories (e.g. education, social). Revenues are available by their source (e.g. local income tax surcharge, transfers from central government). We rely on the officially defined deficit (*disavanzo*) in the accounts, which is the difference between revenues and expenditures plus the difference between revenue carry-overs and expenditure carry-overs from preceding years. Carry-overs are the difference between the figures calculated according to the cash and accrual bases (e.g. credit vis-a-vis taxpayers, or debt vis-a-vis suppliers). Therefore, the official deficit accounts for obligations originated in previous years, which still weight on the public finances. According to this official measure of deficits, 54% of all municipalities in our sample are in surplus.

3.3.4 Politician and Election Data

We collect information on local elections from the historical electoral archive, and information on politicians from the registry of local public office holders. Both datasets are maintained by the Italian Ministry of the Interior. The first database includes the names of all the candidates and of the lists supporting them, and reports information on election results. This allows us to construct both a rerun and reelection dummy for incumbents. The former is equal to one if the incumbent is not term-limited and runs again, and equals zero in case the incumbent is not term-limited but does not run again. The latter variable equals one for incumbents that run again and are reelected, and is zero for those who rerun and fail to be reelected.

The second database has demographic information on all individuals who ever held municipal public office, that is mayors, members of the executive committee, and councilors. Usually runners-up are elected to the city council, so that we also have information on them unless they give up their seat immediately after the election. Therefore, we have information on birthplace, party, (potential) term limit, gender, (former) occupation and education level for both the mayor and the runner-up. Using these variables, we construct our two measures of politician's quality, a dummy for having a college degree and a dummy for being employed in a high-skill profession before becoming a politician.[17] We merge the two databases by matching on name, surname, year and municipality code in order to obtain background information on mayors and runners-up. The matching is successful in 70% of the cases.[18]

3.3.5 Municipal Characteristics

We collect several further (time-invariant) variables on municipal characteristics from the 2011 census: the share of female, college-educated, and inhabitants older than 60 years as well as geographic variables such as altitude, geographic area and a dummy for coastal location. The annual population numbers are retrieved from ISTAT. We calculate the yearly share of income held by the top income earners (more than 55,000€) from the tax base data by the Italian Fiscal Agency.

3.4 Empirical Strategy

3.4.1 Difference-in-Discontinuity Design

Our empirical strategy relies on a natural experiment resulting from the extension of the fiscal rule in the year 2013 to municipalities that were previously exempted. In our sample period of 2007-2015, the Domestic Stability Pact applied to municipalities with

[17] For the latter, we rely on the ISTAT classification of occupations (ISTAT, 2013). We classify occupations in category 1 (legislators, entrepreneurs and managers) and 2 (intellectual, scientific and highly specialized occupations) as high-skill occupations. Among mayors from high-skill occupations, 76% hold a college degree, whereas among those from other occupations only 27% have a college degree.

[18] Non-matches are likely due to second-placed candidates not joining the city council. Table 3.B.4 of the Appendix compares the covariates of matched and non-matched mayors.

5,000 or more inhabitants until 2013, and to municipalities with 1,000 or more inhabitants from 2013 to 2015. One possible strategy could be a comparison of municipalities around the 1,000 threshold using only data for the period 2013-2015 in a classic regression discontinuity design. However, other policies change discontinuously at the 1,000 cutoff (see Table 3.B.2 for details) and thus the standard continuity assumption is violated.

In order to isolate the effects of the fiscal rule, we employ a difference-in-discontinuity design (Asatryan et al., 2017; Grembi, Nannicini, and Troiano, 2016). The intuition behind this empirical strategy is that a confounding policy jump can be netted out if the policy is *time-constant*. This assumption holds in our setup, as all of the confounding policy discontinuities are constant over the whole sample period. This implies that one can estimate the confounding effect at the 1,000 threshold in the years before 2013 and subtract it from the compounded fiscal rule and confounding effect estimated at the 1,000 threshold between 2013 and 2015. In other words, this strategy amounts to a difference-in-differences design evaluated at the 1,000 threshold.[19]

More formally, let Y_{it} be an outcome variable in municipality i at time t (e.g. tax progressivity) and $\tilde{p}_{it} = p_{it} - 1,000$ its normalized population in the previous year. According to the law, the treatment status of a municipality is based on the population of the preceding year.[20] We therefore use $\tilde{p}_{it-1}$ as our forcing variable, where at the cutoff the treatment status jumps sharply from 0 to 1. The difference-in-discontinuity estimator can be written as follows:

$$\hat{\tau}_{diff-in-disc} = \left(\lim_{p \to 0^+} E[Y_{it} | \tilde{p}_{it-1} = p, t \geq 2013] - \lim_{p \to 0^-} E[Y_{it} | \tilde{p}_{it-1} = p, t \geq 2013] \right) - \left(\lim_{p \to 0^+} E[Y_{it} | \tilde{p}_{it-1} = p, t < 2013] - \lim_{p \to 0^-} E[Y_{it} | \tilde{p}_{it-1} = p, t < 2013] \right)$$

where the first row describes the jump in the outcome variable at the threshold between 2013 and 2015 (i.e. the compounded fiscal rule *and* confounding effect), and the second row subtracts the jump in the outcome variable before the reform (i.e. *only* the confounding effect).

We implement this estimator using a local linear regressions as in Grembi, Nannicini, and Troiano (2016) and estimate the following equation:[21]

$$\begin{aligned} Y_{it} = &\beta_0 + \beta_1 \tilde{p}_{it-1} + T_{it}(\beta_2 + \beta_3 \tilde{p}_{it-1}) + Reform_t[(\beta_4 + \\ &\beta_5 \tilde{p}_{it-1}) + T_{it}(\beta_6 + \beta_7 \tilde{p}_{it-1})] + \epsilon_{it} \quad \forall \ (i,t) \ \ s.t. \ |\tilde{p}_{it-1}| < h^\star \end{aligned} \tag{3.2}$$

where T_{it} takes the value of one if municipality i is subject to the fiscal rule in year t, $Reform_t$ is a dummy equaling one from 2013 to 2015, and $h^\star$ is the optimal bandwidth

[19] We do not evaluate the change of the 5,000 inhabitants threshold, since there is a *simultaneous* policy change of gender quotas in local elections in 2013 (see Table 3.B.2).

[20] Consistent with the institutional framework, we are using the yearly population numbers from ISTAT.

[21] We also estimate global polynomial regressions with varying polynomial degrees.

determined by the algorithm suggested by Grembi, Nannicini, and Troiano (2016).[22] Since the results of local linear regressions may be sensitive to the choice of the bandwidth, we also estimate results obtained with different bandwidths. Standard errors are clustered at the municipal level to account for arbitrary serial correlation in the error term. The local average treatment effect (LATE) of the fiscal rule is then identified by the coefficient β_6.

The difference-in-discontinuity estimator identifies the effect of interest if the following identifying assumptions are met. First, as discussed above, other confounding variables can change discontinuously at the threshold, but we must assume that the change is *time-constant*. We test this assumption of local parallel trends by means of placebo reforms. That is, we pretend that the reform was implemented in some earlier year instead of 2013, and then re-do the baseline analysis on the pre-reform sample. Second, in contrast to a classical regression discontinuity design, where there cannot be any manipulation of the running variables, the difference-in-discontinuity estimator allows for time-constant sorting unrelated to the reform. If municipalities were to react to the reform by manipulating their population numbers in order to avoid the fiscal rule, we would have selection bias in the treatment and control assignment. We test this assumption with McCrary density tests both before and after the reform, as well as with a density test of the change in density because of the reform. One important caveat is that, even when our identifying assumptions hold, we are estimating the *local* average treatment effect of the fiscal rule. That means our results only apply to small municipalities and are not representative for all Italian local governments.

3.4.2 Heterogeneous Effects

To examine the mechanisms driving our results, we also test for heterogeneous effects. We put special focus on the mayor's quality measured by having a college education or coming from a high-skill occupation. Following the literature on heterogeneous effects in an RD setup (see Becker, Egger, and Ehrlich, 2013), we interact every term in equation 3.2 with a dummy for being a high-skilled mayor D_{it}:

$$\begin{aligned} Y_{it} =& \beta_0 + \beta_1\tilde{p}_{it-1} + T_{it}(\beta_2 + \beta_3\tilde{p}_{it-1}) + Reform_t[\beta_4 + \beta_5\tilde{p}_{it-1} + T_{it}(\beta_6 + \beta_7\tilde{p}_{it-1})] + \\ & D_{it}[\beta_0^{int} + \beta_1^{int}\tilde{p}_{it-1} + T_{it}(\beta_2^{int} + \beta_3^{int}\tilde{p}_{it-1}) + Reform_t[\beta_4^{int} + \beta_5^{int}\tilde{p}_{it-1} + T_{it}(\beta_6^{int} + \\ & \beta_7^{int}\tilde{p}_{it-1})]] + \gamma_i + X_{it} + \epsilon_{it} \quad \forall\ (i,t)\ \ s.t.\ |\tilde{p}_{it-1}| < h^\star \end{aligned} \tag{3.3}$$

The heterogeneous treatment effect is then measured by β_6^{int}. X_{it} includes dummies indicating whether the mayor is female, has a college degree, is backed by a left-wing, right-wing or centrist party, is term-limited, her age and her win margin in the last

[22] We conduct a standard RD before and after the reform using the STATA command *rdrobust* (see Calonico, Cattaneo, and Titiunik, 2014) and then take the average of the two optimal bandwidths.

election, the number of years to the next election, as well as the top income share and pre-reform deficits of the municipality.[23]

We also include municipality fixed effects γ_i to absorb any time-invariant heterogeneity. Nevertheless, we cannot fully exclude the possibility of unobserved time-varying confounding variables determining both the mayor's quality and our outcome of interest. For example, if municipalities whose population has a higher preference for redistribution tend to elect more skilled mayors, then we would erroneously attribute the estimated increase in progressivity to mayoral quality rather than to the population's preferences.

For this reason, we turn to a more exogenous source of variation in the mayor's quality. We exploit close mixed elections, i.e. races in which the winning candidate and runner-up have a different educational level.[24] First, we restrict our sample to municipalities whose mayors have been elected in a mixed election. Next, we subtract the vote share of the non-college candidate from that of the college-educated candidate to get the vote margin vm_{it}, which acts as our running variable. For positive vm_{it}, the college-educated candidate wins the election, whereas if vm_{it} is negative, the non-college candidate wins. Our identifying variation then stems from close elections, comparing municipalities, in which the college-educated candidate barely won, to those in which she barely lost. More formally, let D_{it} be an indicator that takes the value one if the mayor of municipality i in year t is college-educated. We then estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 vm_{it} + D_{it}(\beta_2 + \beta_3 vm_{it}) + X_{it} + \epsilon_{it} \quad \forall\ (i,t) \ \ s.t.\ |vm_{it}| < h \tag{3.4}$$

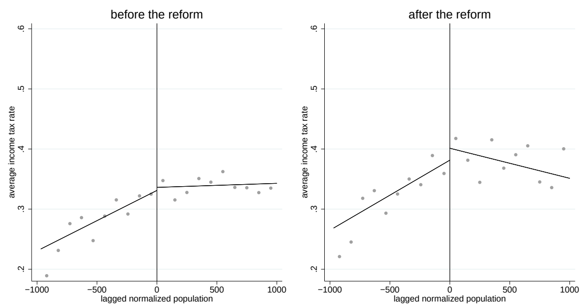
where X_{it} includes all control variables described above, as well as additional characteristics of the second-placed candidate (gender, age and party), and h is the chosen bandwidth. The effect of having a college-educated mayor is then identified by β_2. The most important identifying assumption is that the education level is the *only* characteristic that changes at the threshold. We test this by using other observable characteristics from X_{it} as outcome variables to see whether they also jump at the threshold.

In a last step, we combine equations 3.3 and 3.4 to identify our heterogeneous effects model using only the variation in the quality of the mayor induced by close elections. That is, we interact every term in equation 3.3 with the vote margin between college-educated and non-college-educated candidates and estimate it on the sample of mixed elections. By comparing college-educated and non-college mayors that barely won in a mixed election, we effectively control for unobserved confounders that could possibly drive both the mayor's educational level and tax policy.

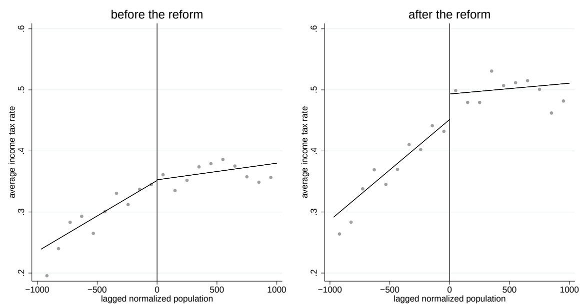
[23] In some specifications, we add additional interaction terms from X_{it} other than D_{it} to test their relative importance in a "horse race".

[24] This strategy has been extensively used in the literature on the effect of female mayors (see, for example, Baskaran and Hessami, 2018). We focus on mixed races between mayors of different education levels since the number of races between mayors from low- and high-skill occupations is considerably smaller.

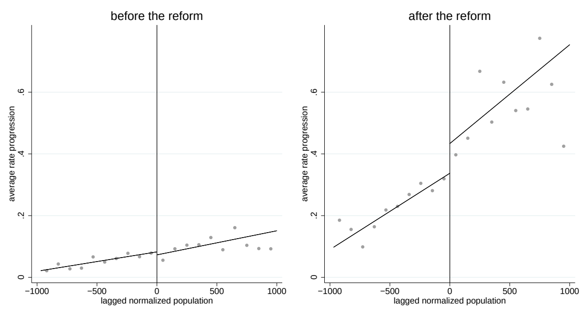
Figure 3.5.1: Regression discontinuity plots: tax progressivity before and after the reform



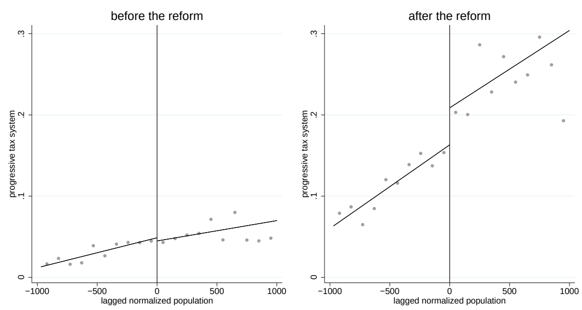
a) income tax rate at the 1st decile



b) income tax rate at the 9th decile



c) average rate progression



d) progressive tax

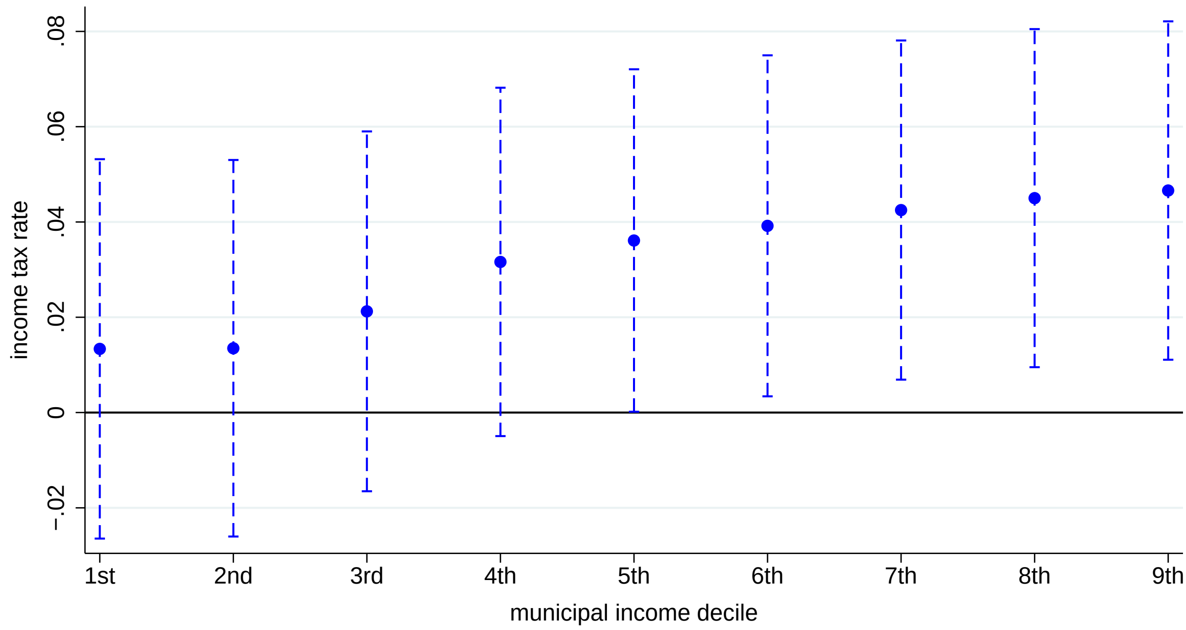
Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Each graph is a regression discontinuity plot for pre-reform years (2007-12, on the left) and post-reform years (2013-15, on the right). The outcome variable is reported underneath each graph. The running variable is lagged normalized population. Plots are obtained with the STATA command *rdplot* (Calonico, Cattaneo, and Titiunik (2015)) organizing the data in 10 bins on each side of the threshold. The lines are linear fits estimated separately on each side of the threshold.

3.5 Results

3.5.1 Baseline Results

We start by presenting some graphical evidence of our results. Figure 3.5.1 shows standard RD graphs estimated separately on the pre-reform (2007-12, on the left) and post-reform (2013-15, on the right) samples for four outcome variables in sub-figures: a) income tax rate at the first decile, b) income tax rate at the ninth decile, c) average rate progression, and d) a dummy for a progressive tax. Each graph plots local means of the outcome variable in ten normalized population bins on each side of the threshold, and a linear fit of the data estimated separately on each of them. Before the reform, the figure does not show a visible jump at the threshold for any of the outcome variables. After the reform, we observe a positive jump in the average tax rate at the first decile, and a more sizable one for the tax rate at the ninth decile. This preliminary graphical evidence suggests that the reform induced a disproportionate increase in the tax for higher incomes.

Figure 3.5.2: Effect of the reform on the income tax rate at different income deciles



Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The figure plots the local average treatment effects also reported in Table 3.5.1 and their 95% confidence bands. The LATEs are from difference-in-discontinuities models estimated with a separate local linear regression for each tax rate and correspond to β_6 in equation 3.2. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). The deciles refer to the income distribution in each municipality.

The finding is confirmed by the fact that both measures of progressivity display a positive jump at the threshold in the post-reform years, but not in pre-reform years (panels c and d).

Next, we turn to the estimates obtained from the difference-in-discontinuity estimation (equation 3.2). Figure 3.5.2 plots the local average treatment effect (LATE) of the reform on the average tax rates at all deciles of the municipal income distribution (estimates are shown in Table 3.5.1). We find that, first, all point estimates are positive. This is consistent with the interpretation that municipalities raise local income taxes to comply with the fiscal rule. Second, the size of the point estimates is monotonically increasing along the municipal income distribution. Third, the estimated effect on the top tax rate translate to about 13% of the sample mean, and is about 3.5 times as large as the estimated tax rate effect on the lowest earners.

To test whether the estimated effects on high-earners are statistically larger than the effects on low-earners, we re-estimate equation 3.2 for all nine tax rates jointly, with seemingly unrelated regressions (SUR).[25] We then implement several one-sided Wald tests with a null hypothesis that the effect on higher incomes is not larger than the effect

[25] We use SUR because the tax rates along the income distribution are jointly determined by the municipal government, and thus can not be considered as independent outcome variables. As such, the confidence

Table 3.5.1: Effect of the reform on the income tax rate at different income deciles

	(1) 1st decile	(2) 2nd decile	(3) 3rd decile	(4) 4th decile	(5) 5th decile	(6) 6th decile	(7) 7th decile	(8) 8th decile	(9) 9th decile
LATE	0.013	0.013	0.021	0.032*	0.036**	0.039**	0.043**	0.045**	0.047**
	(0.020)	(0.020)	(0.019)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
$>$ 1st decile	-	0.488	0.216	0.061	0.035	0.022	0.012	0.008	0.007
$>$ 2nd decile	-	-	0.194	0.049	0.027	0.016	0.009	0.006	0.005
$>$ 3rd decile	-	-	-	0.063	0.034	0.018	0.008	0.005	0.004
$>$ 4th decile	-	-	-	-	0.192	0.091	0.034	0.019	0.018
$>$ 5th decile	-	-	-	-	-	0.049	0.005	0.003	0.008
$>$ 6th decile	-	-	-	-	-	-	0.014	0.007	0.015
$>$ 7th decile	-	-	-	-	-	-	-	0.028	0.050
$>$ 8th decile	-	-	-	-	-	-	-	-	0.157
mean	0.331	0.335	0.347	0.358	0.364	0.366	0.367	0.368	0.370
bandwidth	661	661	661	661	661	661	661	661	661
N	17,609	17,609	17,609	17,609	17,609	17,609	17,609	17,609	17,609

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 3.2. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). The deciles refer to the income distribution in each municipality. The middle panel displays p-values for pairwise one-sided tests (estimated by seemingly unrelated regression) whether the effect is higher than the effect on the tax rate at the first to eighth municipal income decile, respectively. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

on lower incomes. We present the p-values of all these tests in Table 3.5.1. Overall, we can reject the null hypothesis at the 5% or 10% level for almost all comparisons.

Alternatively, we also use the average tax rates at annual incomes from 5,000€ to 75,000€ as outcomes. The effects are again positive for all tax rates and monotonically increasing in income, but only significant at the 95% level for incomes above the national median income (see Figure 3.C.2). As before, one-sided Wald tests reject the hypothesis that there are no differences between the effects on high- and low-earners (see Table 3.C.1).

As a final test for the effect of introducing the fiscal rule on tax progressivity, we estimate the difference-in-discontinuity design (equation 3.2) using our four measures of progressivity: the average rate progression, the marginal rate progression, the exemption level (in €), and a dummy equal to one if the overall income tax schedule is progressive. Table 3.5.2 shows that the reform induces an increase in the average (marginal) rate progression of 0.14 (0.16) standard deviations, corresponding to 80% (86%) of the sample mean. The reform also increases the probability of adopting a progressive tax system by six percentage points (Table 3.5.2, column 2). This large increase in progressivity is partly driven by the effect on the exemption level, which increases by 600€, that is approximately by 67% of the sample mean (Table 3.5.2, column 4).

Exploiting information on the municipal income distribution, we also estimate the effects on tax revenues levied from taxpayers assigned to different brackets both in ag-

intervals plotted in Figure 3.5.2 are not useful for testing whether effects on different tax rates are significantly different from each other.

Table 3.5.2: Effect of the reform on progressivity measures

	(1) average rate progression	(2) progressive tax	(3) marginal rate progression	(4) exemption level
LATE	0.140**	0.056**	0.155**	600*
	(0.062)	(0.027)	(0.066)	(316)
mean	0.175	0.087	0.181	892
bandwidth	668	650	668	635
N	17,775	17,319	17,775	16,955

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 3.2. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). The average and marginal rate progressions are estimates of the slope of the average and marginal income tax schedules. Progressive tax is a dummy for whether the municipality has a tax rate which is not uniform. Exemption level is the amount of income (in €) exempted from the income tax. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: *p¡0.1, **p¡0.05, ***p¡0.01

gregate and in per taxpayer terms (see Section 3.3). In line with the progressive nature of the income tax rate adjustment, our findings suggest that individuals from upper tax brackets contribute more to the extra revenues generated by the reform. The average tax increase for a taxpayer in the top income bracket (above 120,000€) amounts to 73€ (47% relative to the sample mean) which is about an order of magnitude larger than the effect on a taxpayer in the 15,000€ to 26,000€ bracket. In general, the additional tax revenues per taxpayer induced by the reform are strictly increasing in taxable income (see Table 3.5.3)

However, since only few taxpayers have large taxable incomes (on average 15 individuals have taxable incomes above 55,000€), more than half of the extra revenue is levied from tax payers with taxable income between 15,000€ and 55,000€. Our findings also suggest that individuals with taxable income below 10,000€ (on average 38% of the total taxpayers) are the only ones to almost entirely escape the tax rate increase. This result is consistent with our previous findings of an increase in exemption levels.

Furthermore, we also test the effects of the reform on the tax base, and we do not find evidence that taxable income changed (see Table 3.C.2). Taken together with the evidence of a positive effect on revenues, this finding suggest that mayors were able to raise additional income tax revenues without substantially hurting their tax base. Finally, we also compute the upper pareto bounds proposed by Bierbrauer, Boyer, and Peichl (2021) to test whether the pre-reform tax systems were on the left or on the right of the peak of the Laffer curve. We find that, even assuming a large elasticity of taxable income, more than 95% of the municipalities were on the left of the peak, making thus possible for mayors to raise revenues by increasing tax rates (see Appendix 3.A.3 for details.).

Table 3.5.3: Effect of the reform on income tax revenues by bracket

	(1) <10k€	(2) 10k€-15k€	(3) 15k€-28k€	(4) 28k€-55k€	(5) 55k€-75k€	(6) 75k€-120k€	(7) >120k€
tax revenues per taxpayer							
LATE	0.78	4.92**	7.80**	15.88**	22.49*	52.88**	73.05**
	(1.02)	(2.36)	(3.69)	(6.25)	(13.53)	(21.81)	(30.60)
mean	18.14	47.11	76.11	129.62	177.36	195.38	154.85
mean # of taxpayer	308	136	241	109	8	5	2
bandwidth	664	654	665	660	618	479	726
N	17,684	17,444	17,709	17,587	16,544	13,163	19,180
total tax revenues							
LATE	6.24	596.17*	1561.08*	1938.44**	406.45***	486.49***	627.70***
	(307.52)	(317.41)	(919.09)	(760.57)	(149.98)	(182.34)	(239.48)
mean	4,857.04	5,796.83	16,824.49	12,589.17	1,747.59	1,566.40	1,020.69
bandwidth	700	657	628	647	653	608	688
N	18,550	17,508	16,776	17,247	17,408	16,287	18,279

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable. The LATE corresponds to β_6 in equation 3.2. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). The outcome variables are per capita (upper panel) and total (bottom panel) tax revenues in 2015 Euros generated by tax payers with taxable income included in the bracket reported on top of each column. The table reports also the sample mean of the outcome variable, the average number of taxpayers in each bracket, the used bandwidth and the number of observations. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

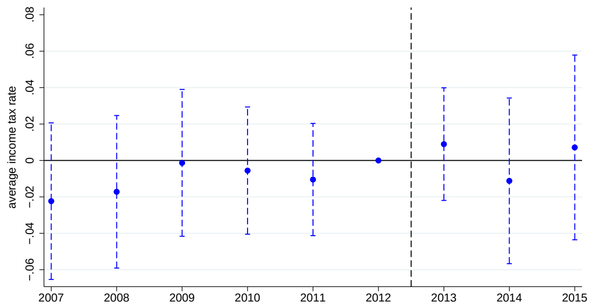
3.5.2 Sensitivity Checks

In this section, we discuss the validity of the two major identifying assumptions as described in Section 3.4. We also perform a number of additional robustness tests with respect to the choice of bandwidth size and polynomial degrees, and a permutation test using placebo thresholds.

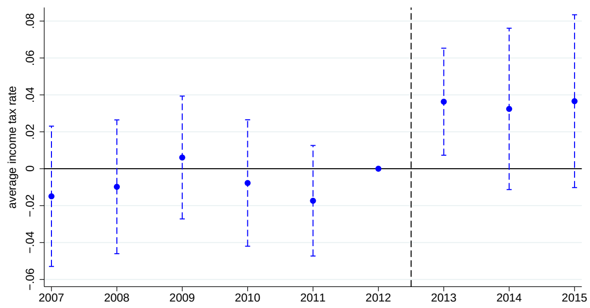
First, the local parallel trends assumption states that any difference at the threshold other than the fiscal rule has to be time-constant. To formally test whether the local common trends assumption holds, we use a dynamic version of equation 3.2, where we replace the $Reform_t$ dummy with year dummies. Normalizing our effects to the pre-reform year of 2012, this allows us to track the local trends before the reform and the dynamic effects after the reform. As Figure 3.5.3 shows, there is no significant pre-treatment trend in the bottom tax rate (Panel a), top tax rate (Panel b), the average rate progression (Panel c), or the probability of a progressive tax system (Panel d).[26] After the reform, there is an immediate significant increase in all variables, but the bottom tax rate. As a further robustness check, we conduct placebo reforms in every pre-reform year of our sample. Specifically, we restrict our sample to the pre-reform period and re-estimate equation 3.2 with the $Reform_t$ dummy taking the value 1 from year t onward with $t \in \{2008, 2009, 2010, 2011, 2012\}$. If any confounding effect was not time-constant, one would expect to pick up a significant effect by at least one of these placebo reforms. Figure 3.D.2 plots the results of the five placebo estimations as well as that of the baseline results. The results show zero effects for every placebo reform and every tax rate. As

[26] This also holds for our other outcomes variables (see Figure 3.D.1).

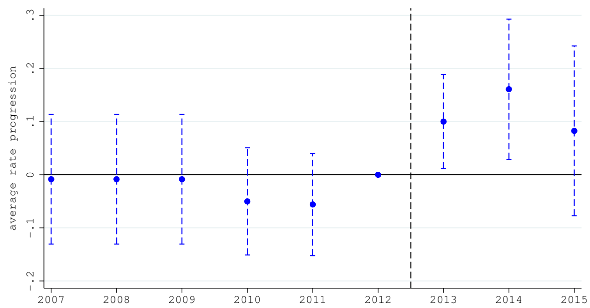
Figure 3.5.3: Dynamic effects of the reform



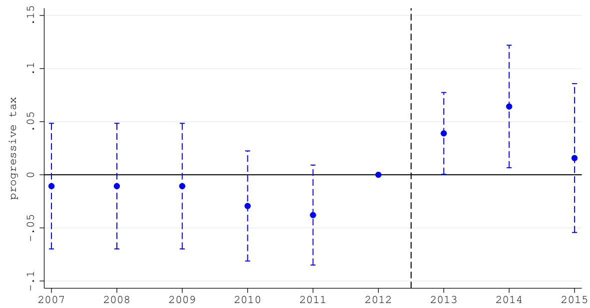
a) income tax rate at the 1st decile



b) income tax rate at the 9th decile



c) average rate progression



d) progressive tax

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Each panel plots estimates from the dynamic model on a different outcome variable, reported underneath each plot. The dynamic model is an extension of the baseline difference-in-discontinuities model that includes year dummies instead of the reform dummy. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). Each dot is the estimate of the deviation of the outcome variable in the year reported on the horizontal axis relative to the pre-reform year 2012. Dotted bars are 95% confidence bands.

expected, the placebo estimates exhibit a constant rather than a monotonically increasing relationship between the estimated tax rate effect and the level of income. Next, we test the continuity assumption by using pre-determined variables as outcomes. Table 3.D.1 shows that none of the 16 variables are significantly influenced by the reform at conventional levels.

Our second identifying assumption is that there is no manipulation of the population numbers in reaction to the reform. In order to test this claim, we present standard McCrary graphs (McCrary (2008)) displaying the density of municipalities around the threshold before and after the reform, as well as a "dynamic" McCrary graph, which shows the difference between the density around the threshold before and after the reform (see Asatryan et al., 2017; Grembi, Nannicini, and Troiano, 2016).[27] We do not find evidence of a significant jump in the density of observations at the 1,000 population

[27] For the "dynamic" McCrary, we first divide normalized log population size in bins of width 0.01. Then we calculate the change in the total number of observations within each bin from the pre- to the post-reform period. Finally, we fit local polynomial plots using a quadratic degree and a triangular kernel.

threshold either before (Figure 3.D.3a) or after (Figure 3.D.3b) the reform. This evidence of *no* manipulation of population numbers in response to the reform is confirmed by the results of the "dynamic" McCrary test presented in Figure 3.D.4.

Our results are also robust to the selection of different bandwidths. Figure 3.D.5 plots the effect on low- and high-earners for bandwidths ranging from 400 to 1,000. As expected, the standard errors somewhat decrease with larger bandwidth, but the point estimates remain stable. Furthermore, Figure 3.D.6 and Table 3.D.2 show that global polynomial regressions yield very similar results to local linear regressions. Additionally, we conduct permutation tests by re-estimating equation 3.2 at placebo thresholds and show that our baseline effect on high incomes is larger than any of the placebo estimates (see Figure 3.D.7). Finally, we show in Appendix 3.A.2 that a difference-in-difference approach yields estimates very similar to our main estimates.

3.6 Mechanisms and Electoral Implications

We have thus far established that local governments increase tax progressivity in response to exogenous consolidation requirements induced by the fiscal rule. This section first explores heterogeneity in the treatment effects estimated in the previous section. In particular, we study whether the type of tax adjustment is different depending on mayor characteristics, with a special emphasis on her skill level. We then study whether introducing the fiscal rule affects reelection chances of mayors.

3.6.1 The Role of High-Skilled Mayors

Following the literature on competence of politicians and its effects on policy outcomes (see Section 3.1), we proxy skill with the politician's education level, specifically if she holds a college degree.[28] As a robustness check, we also use a dummy for being employed in a high-skill profession. About 45% of the mayors in our sample have a college degree and 38% work in a high-skill occupation (see Table 3.B.3).

We first test whether highly-educated mayors are driving our progressivity results as measured by both of our progression measures, the exemption level, and a dummy for progressive rather than flat tax systems. Table 3.6.1 presents estimates of equation 3.3, where the interaction variable D_{it} is a dummy equal to one if the mayor holds a college degree. It turns out that college-educated mayors drive almost all of the increase in progressivity estimated in our baseline model. Columns 1 shows that mayors with a college education increase the average rate progression by 0.30 standard deviations in response to the fiscal rule, whereas non-college-educated mayors do not change the

[28] In this measurement choice we follow the literature that most often approximates the skill of politicians by their level of education (see, for example, Besley and Reynal-Querol, 2011; Gagliarducci and Nannicini, 2013). Other papers measure the skill of politicians by utilizing data on politicians' experience, pre-office market income, quality (rather than only level) of education or the skill level of their occupation (Bertrand et al., 2020; Besley et al., 2017; Fisman et al., 2015).

Table 3.6.1: Differential effect of the reform by mayor's skill

	(1) average rate progression	(2) average rate progression	(3) average rate progression	(4) average rate progression	(5) average rate progression	(6) average rate progression	(7) average rate progression	(8) average rate progression
LATE	0.012	0.013	-0.004	-0.016	0.010	0.015	-0.009	-0.010
	(0.082)	(0.082)	(0.081)	(0.167)	(0.076)	(0.077)	(0.076)	(0.162)
LATE x college degree	0.298**	0.293**	0.231**	0.229**				
	(0.120)	(0.119)	(0.106)	(0.108)				
LATE x high-skill job					0.359***	0.339**	0.294**	0.281**
					(0.133)	(0.133)	(0.120)	(0.119)
LATE x female mayor				0.069				0.076
				(0.163)				(0.168)
LATE x left-wing mayor				0.025				0.020
				(0.154)				(0.155)
LATE x right-wing mayor				-0.360				-0.344
				(0.297)				(0.300)
LATE x centrist mayor				-0.489				-0.605
				(0.342)				(0.388)
LATE x low win margin				0.097				0.088
				(0.114)				(0.117)
LATE x term limit				-0.041				-0.060
				(0.100)				(0.101)
LATE x high pre-reform deficit				0.133				0.129
				(0.132)				(0.132)
LATE x low top income share				-0.177				-0.174
				(0.132)				(0.132)
controls		yes	yes	yes		yes	yes	yes
municipality FE			yes	yes			yes	yes
mean	0.176	0.178	0.178	0.178	0.173	0.177	0.177	0.177
bandwidth	668	668	668	668	668	668	668	668
N	17,378	17,092	17,092	17,092	17,292	16,741	16,741	16,741

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3.3. We measure mayors' skills using two dummies: college degree, which is equal to one in case the mayor holds one; and high-skill job, which is equal to one in case the mayor was employed in a managing position or in an intellectual profession (e.g. lawyer, medical doctor). Details on all covariates are described in Section 3.4. The estimation method is local linear regression. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

progressivity of the income tax at all. This result holds when including municipality fixed effects and several other interactions with potential confounders, such as gender, a proxy for electoral competition, political orientation, binding term limits, pre-reform fiscal position, and income structure (see columns 2 to 4 of Table 3.6.1). Furthermore, results look very similar when using the skill level of the mayor's occupation as an alternative measure (see columns 5 to 8 of Table 3.6.1). Tables 3.C.3, 3.C.4 and 3.C.5 show that this heterogeneous effect also holds for the introduction of progressive tax systems, exemption levels and the marginal rate progression. These results do not mean that low-skilled mayors did not raise local income taxes in response to the reform, but rather that they increased tax rates uniformly (see Figure 3.C.3).

The heterogenous effects estimated so far in this section using equation 3.3 do not have a causal interpretation because mayors' education is not assigned at random to different municipalities. As such, unobserved factors at the mayor or municipal-level might induce omitted variable bias and thus drive the estimated heterogeneity. To tackle this issue, we focus on mixed elections, in which the winner and runner-up have different education levels. Using this sample, we estimate the heterogeneous effect at the election threshold by interacting all variables with the vote margin between the two candidates. In other words, we combine equations 3.3 and 3.4.

Table 3.6.2: Differential effect of the reform by mayor's skill: mixed election RD

	(1) average rate progression	(2) average rate progression	(3) average rate progression	(4) average rate progression	(5) average rate progression
LATE	-0.035	-0.223	-0.391		
	(0.100)	(0.332)	(0.302)		
college degree	-0.048	-0.002	0.167	-0.019	0.145
	(0.032)	(0.094)	(0.264)	(0.042)	(0.100)
LATE x college degree	0.310**	1.033**	0.954**		
	(0.146)	(0.466)	(0.400)		
controls	yes	yes	yes	yes	yes
mixed election RD		yes	yes	yes	yes
municipality FE			yes		yes
pre-reform sample				yes	yes
mean	0.179	0.168	0.168	0.081	0.081
population bandwidth	668	668	668	668	668
close election bandwidth		0.20	0.20	0.20	0.20
N	12,355	2,621	2,621	1,861	1,861

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The sample is restricted to observations for which we were able to match the main dataset with the election data. Columns (1), (2), (3) and (4) report estimates of the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. In rows with "mixed election RD" switched on, the model is augmented with the margin of victory and its interaction with all other terms, and the sample is further restricted to municipality-year observations, in which the incumbent mayor was elected in a race against a runner up with a different education level (college vs. non-college). Population bandwidths are selected following Grembi, Nannicini, and Troiano (2016). Election bandwidths are selected using the using the STATA command *rdrobust*. Columns (5) and (6) report estimates of the college effect from regression discontinuity models where the running variable is the margin of victory, the treatment dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform (β_2) in equation 3.4. Details on all covariates are described in Section 3.4. In the bottom panel, the sample mean of the outcome variable, the used bandwidths and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

This empirical strategy accounts for any municipal level unobserved differences between municipalities with or without a college-educated mayor, such as unobserved preference for redistribution or differences in the income distribution. However, it does not account for mayoral characteristics correlated with education. We thus start by testing whether any characteristic besides the education level of the mayor changes discontinuously at the election threshold. Table 3.D.3 in the Appendix shows that out of 16 variables only the mayor's gender varies significantly between college-educated and non-college mayors. Educated mayors are more likely to be female. Since there was no effect of gender in Table 3.6.1 and we control for gender in all of our previous specifications, we do not regard this imbalance as a serious threat to our empirical strategy. We include all these mayoral characteristics as control variables in the regression models that combine our baseline difference-in-discontinuity with close elections. Furthermore, Figure 3.D.8 shows that there is no discontinuity in the density of the margin of victory.

Estimates from these models are shown in Table 3.6.2 for the average rate progression. Columns 1 and 2 show that the heterogeneous effect estimated on the sample of municipalities, in which the mayor was elected in a mixed election, is very similar to the estimates obtained on the full sample in Table 3.6.1. Next, we present the results of equation 3.3 interacted with the vote margin between the college-educated and non-college-educated candidate in columns 3 and 4. The result confirms our previous findings. The reform-induced increase in progressivity is driven entirely by municipalities ruled by college-educated mayors.This result also holds when using other measures of progressivity (see Tables 3.C.6, 3.C.7 and 3.C.8) or varying the bandwidth of the close election RD (see Table 3.D.9). We also test whether the interaction effect is driven by any specific job category by dropping one job category at the time. As Figure 3.D.11 shows, the effect does not change notably for any job category.

The findings in this section have established that college-educated mayors react to the introduction of the fiscal rule by increasing income taxes progressively, while other mayors increase taxes uniformly. We can rule out that college-educated mayors favor more tax progressivity in general. Using a simple regression discontinuity design based on close elections (equation 3.4) and restricting our attention to years before the introduction of the fiscal rule, we do not find any evidence that municipalities ruled by college-educated mayors have more progressive tax systems (see columns 5 and 6 of Table 3.6.2).

3.6.2 Political Costs of Austerity

We now test whether the introduction of the fiscal rule was associated with a political cost for the incumbent mayor. In particular, we estimate the baseline differences-in-discontinuities model (equation 3.2) with the reelection and rerun dummies as outcome variables (see Section 3.3 for a detailed description of these variables). Note that the mayor's skill level is a predetermined characteristic with respect to the reform in 2013, since we only consider the first election after the reform.

Based on a standard median-voter model, progressive taxation should be less costly than uniform taxation, since only a minority of rich households are taxed at a higher rate (Bierbrauer, Boyer, and Peichl, 2021). Additionally, in our context of austerity, compensatory arguments behind progressive taxes (Scheve and Stasavage, 2016) would suggest to shift the tax increase away from the poorest households if the fiscal austerity imposed by the national government is perceived as unfair towards the poor.

The near zero point estimate in column 1 of Table 3.6.3 suggests that there is no evidence of political costs for the average incumbent. However, this average effect hides interesting heterogeneity. When allowing for heterogeneity in mayoral education in columns 2 and 3, we find that mayors without a college degree experienced a severe drop of 30 to 37 percentage points in reelection probability, while educated mayors do not undergo these costs at all. Both point estimates are significant at conventional levels.[29] The probability of running for office again drops on average, driven by mayors without a college degree, but these effects are not significant at conventional levels (see columns 5 to 7 of Table 3.6.3). This is consistent with non-college mayors also self-selecting out of office, but our results seem to be mainly driven by voter selection. Again, results point in the same direction when using the mayor's occupation as an alternative measure of skill (see Table 3.C.9).

[29] The effect is very stable when varying the bandwidths (see Figure 3.D.10).

Table 3.6.3: Effects of the reform on mayors' reelection odds

	(1) reelection	(2) reelection	(3) reelection	(4) reelection	(5) re-run	(6) re-run	(7) re-run	(8) re-run
LATE	-0.004	-0.297**	-0.370***		-0.090	-0.085	-0.138	
	(0.059)	(0.142)	(0.132)		(0.060)	(0.111)	(0.111)	
college degree		-0.073	-0.036	-0.025		0.209*	0.180	-0.019
		(0.226)	(0.218)	(0.021)		(0.119)	(0.118)	(0.022)
LATE x college degree		0.472**	0.471**			0.105	0.102	
		(0.235)	(0.230)			(0.190)	(0.193)	
municipality FE		yes	yes			yes	yes	
controls			yes	yes			yes	yes
pre-reform sample				yes				yes
mean	0.832	0.832	0.834	0.833	0.607	0.607	0.607	0.594
bandwidth	1059	1059	1059	1059	1088	1088	1088	1088
N	2,833	2,833	2,745	1,410	4,271	4,271	4,135	2,357

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Columns (1) and (5) report estimates of the local average treatment effect (LATE) in the baseline difference-in-discontinuities model. Columns (2), (3), (6) and (7) report estimates of the LATE for mayors without a college degree (LATE) and for mayors with a college degree (LATE x college degree), estimated using the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. Columns (4) and (8) report estimates of the college effect from a regression of the outcome on a dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform. Bandwidths are selected following Grembi, Nannicini, and Troiano (2016). The reelection outcome variable in columns (1) to (4) equals one for incumbents that run again and are reelected, and is zero for those who rerun and fail to be reelected. The rerun outcome variable in columns (5) to (8) equals one for incumbents that are not term-limited and choose to run again, and is zero for those who do not and are not term-limited. Control variables are described in Section 3.4. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Taken together, our findings suggest that more skilled politicians have avoided the political cost of austerity by designing a fiscal adjustment based on progressive taxation. Although we can not provide a direct causal link from increased progressivity to higher reelection odds, we can rule out that skilled politicians have higher re-election odds in general. Using a simple regression discontinuity design based on close elections (equation 3.4) and restricting our attention to years before the introduction of the fiscal rule, we do not find any evidence that college-educated mayors are more likely to be reelected or to run again (see columns 4 and 8 of Table 3.6.3). Any alternative explanation for our findings must thus explain why the introduction of the DSP differentially increased the skilled mayors' re-election odds during our sample period. Existing evidence on the lifting of the DSP in 2001, when implementing local tax progressivity was not yet feasible, actually shows that the fiscal rule decreased the mayors' education level in that occasion (Gamalerio, 2019).[30]

3.6.3 Alternative Channels of Adjustment

As discussed in Section 3.2, the local income tax is not the only policy instrument that Italian municipalities can use to comply with the fiscal rule. To shed more light on the full adjustment behavior of affected municipalities, we estimate the effects of the fiscal rule on all revenue and spending categories using our baseline model (equation 3.2) and municipal account data.

[30] According to the author's interpretation, skilled politicians avoid entering the political arena when their discretion over fiscal policy is constrained. The difference with our results can thus be explained by the additional policy instrument of differentiated local tax rates, which was not available to mayors in 2001. Furthermore, our results seem to be driven mainly by voters' demand rather than purely by the supply of politicians.

In line with our findings on the local income tax rates, revenues from the local income tax increase significantly (see column 1 of Table 3.6.4). We do not find significant increases in any of the other revenue categories: property tax, waste tax, other taxes or fees, sales, loans, and other revenues (see columns 2 to 7 of Table 3.6.4). We go into more detail regarding property taxation in Appendix 3.A.4, where we discuss the institutional setting and show that property tax rates did not change. We also do not find any significant effects of the reform on capital nor current expenditures (see columns 8 to 10 of Table 3.6.4). Placebo tests for both expenditure and revenue categories show that treatment and control municipalities were on parallel trends before the reform (see Table 3.D.4). To test whether the average expenditure effect is masking heterogeneous effects across different categories of expenditures, we estimate the impact of the fiscal rule on each one separately. Looking at various expenditure items rather than just at social transfers only allows us to take into account potential in-kind transfers which have been shown to matter for inequality (Aaberge et al., 2019). Out of the twelve subcategories of municipal expenditures, only tourism spending is reduced significantly with the other point estimates fluctuating around zero (see Figure 3.C.4). Importantly, the two categories perhaps most associated with redistribution, social and education spending, are hardly affected, with the point estimate of social spending even being positive. Still, this null result might hide heterogeneity between high- and low-skilled mayors that could also explain their differential political outcomes. To test this hypothesis, we estimate the heterogeneous treatment effect of high-skilled mayors for all spending categories. Table 3.C.10 shows that there is no significant difference in any of the spending items. We thus conclude that the redistributive effect of more progressive income taxes is unlikely to be offset by adjustments on the expenditure side of local budgets.

Finally, we investigate the effects of the introduction of the fiscal rule on municipal deficits. As one can see in column 11 of Table 3.6.4, we find that the official deficit is reduced by 36€ per capita (significant at the 1% level). Hence, it appears that the fiscal rule was effective in terms of reducing municipal deficits.

Table 3.6.4: Effect of the reform on municipal budget accounts

	(1) income tax revenues	(2) property tax revenues	(3) trash tax revenues	(4) non-tax revenues	(5) transfer revenues	(6) loan revenues
LATE	5.10***	-3.89	6.05	-6.22	-82.26	5.44
	(1.89)	(14.51)	(6.70)	(27.70)	(78.20)	(25.47)
mean	36.19	167.93	109.76	405.36	870.54	161.87
bandwidth	682	574	566	495	562	581
N	17,856	15,243	15,055	13,408	14,960	15,430
	(7) other revenues	(8) total expenditures	(9) capital expenditures	(10) current expenditures	(11) deficit	
LATE	-0.33	-98.21	-25.19	-52.02	-35.73***	
	(20.20)	(84.17)	(29.68)	(65.13)	(8.17)	
mean	114.93	1360.23	824.49	513.61	5.87	
bandwidth	616	515	473	563	666	
N	16,255	13,923	12,929	15,111	17,642	

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 3.2. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). Outcome variables are reported on top of each column. All revenue, expenditure, and deficit variables are expressed in per capita terms and 2015 Euros and are winsorized. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

3.7 Conclusion

This paper provides the first quasi-experimental evidence that governments try to ease the potential distributional implications of austerity by favoring the relatively poor through more progressive income tax policies. Additional evidence suggests that this strategy is used by more competent mayors and is subsequently rewarded in the polls.

These results are consistent with the view that progressive taxation is preferable to uniform taxation for the median voter. We believe that our evidence is particularly relevant for austerity episodes induced by external factors (e.g., resulting from the imposition of budget constraints from a higher layer of government, or being due to interregional spillovers in economic crisis), which can be seen as unfair from the perspective of the local population. Our study suggests that governments can tune their fiscal reform packages to mitigate the distributional consequences of austerity, as recommended by the IMF (2014), and that this adjustment strategy allows them to improve their reelection odds.

Our findings are relevant for policy makers in countries subject to fiscal constraints, but whose public opinion is growing critical of austerity policies. Although our evidence from small Italian towns cannot be immediately extended to other settings, our cross-country evidence on the positive relation between austerity and top income tax rates adds to the external validity of our results.

Appendix

3.A Additional Analysis

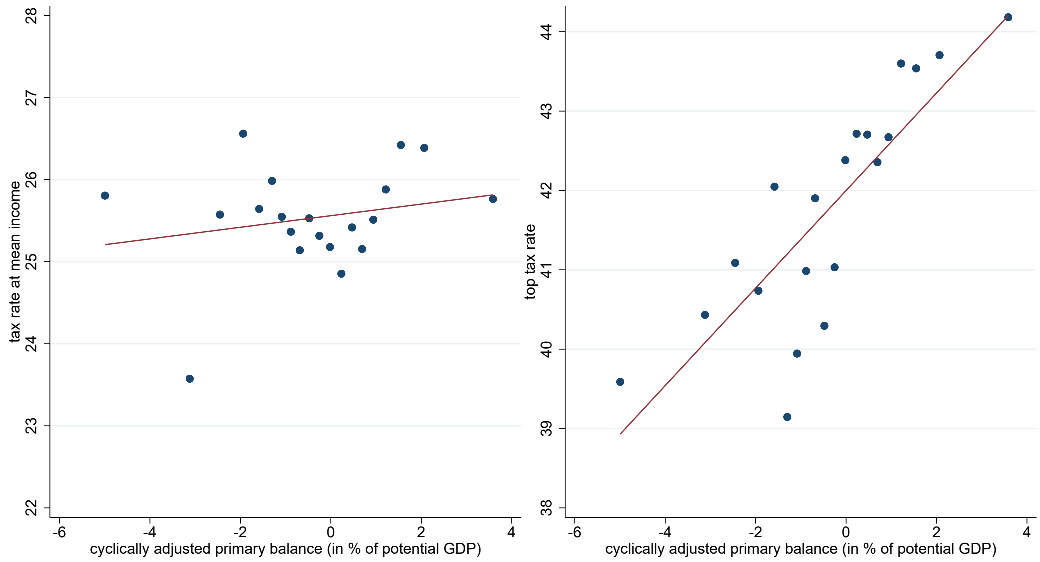
3.A.1 Country-level Analysis

Table 3.A.1: Cyclically adjusted primary balance and tax rates

	(1) tax rate at mean incomes	(2) top tax rate
cyclically adjusted primary balance	0.071	0.615**
(in % of potential GDP)	(0.089)	(0.291)
country FE	yes	yes
year FE	yes	yes
controls	yes	yes
mean	25.533	41.749
N	806	806

Notes: * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors are clustered at the country level. This table shows estimates from the regression $y_{it} = \gamma_i + \lambda_t + \beta capb_{it} + \delta X_{it} + \epsilon_{it}$, where y_{it} is either the tax rate at mean incomes (column 1) or at top incomes (column 2), $capb_{it}$ is the cyclically adjusted primary balance in percent of potential GDP, X_{it} includes log GDP per capita and log population as control variables, γ_i are country fixed effects and λ_t are year fixed effects. Top tax rates are drawn from Rubolino and Waldenström (2019), tax rates at mean incomes are from Peter, Buttrick, and Duncan (2010), supplemented by tax data from the OECD. The budget data comes from IMF DataMapper. Our sample consists of 40 countries (Argentina, Australia, Austria, Belgium, Canada, Chile, China, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, India, Ireland, Israel, Italy, Japan, Latvia, Lithuania, Luxembourg, Malaysia, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Slovakia, Slovenia, South Korea, Spain, Sweden, Switzerland, Turkey, the United Kingdom and the United States) over the period 1990-2017.

Figure 3.A.1: Fiscal austerity and tax rates at mean and top incomes



Notes: This graph shows estimates from the following regression $y_{it} = \gamma_i + \lambda_t + \beta capb_{it} + \delta X_{it} + \epsilon_{it}$, where y_{it} is either the tax rate at mean incomes (left panel) or at top incomes (right panel), $capb_{it}$ is the cyclically adjusted primary balance in percent of potential GDP, X_{it} includes log GDP per capita and log population as control variables, γ_i are country fixed effects and λ_t are year fixed effects. The sample and data are described in Table 3.A.1.

3.A.2 Difference-in-Difference Analysis

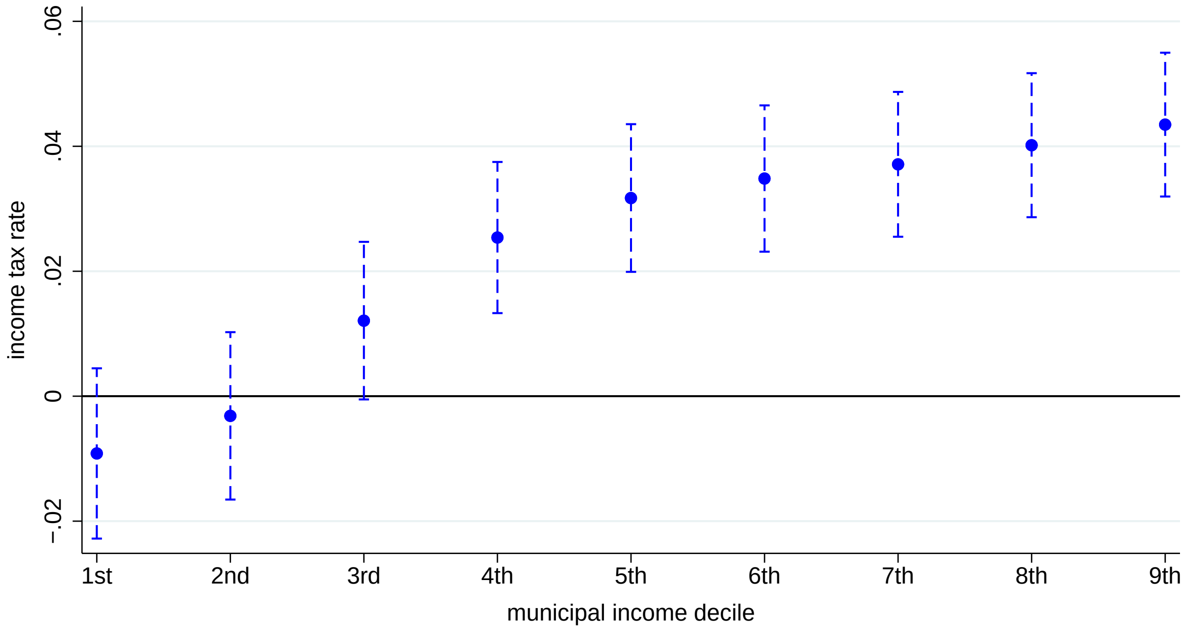
In addition to our main empirical specification, we also run a classical difference-in-difference regression. This allows us to investigate whether our effects can be generalized to broader set of municipalities than just those closely below or above the threshold. To implement this strategy, we define municipalities between 1,000 and 2,000 inhabitants as our treatment group ($T_{it} = 1$) and municipalities with 999 or less inhabitants as our control group ($T_{it} = 0$). The regression equation reads as follows:

$$Y_{it} = \gamma_i + \omega_t + \beta T_{it} Reform_t + \epsilon_{it} \tag{3.5}$$

where $Reform_t$ is a dummy taking the value 1 for the year 2013, 2014 and 2015. γ_i represent municipality fixed effects, whereas ω_t are year fixed effects. Standard error are clustered at the municipality level. The difference-in-difference estimate is then represented by the coefficient β.

Figure 3.A.2 plots the β coefficients for all nine income deciles. The pattern of the estimates is very similar to the pattern of our main difference-in-discontinuity estimates. The tax increase is monotonically increasing in income. The effect size is also close to our main estimates, but standard errors are significantly smaller. In Table 3.A.2 we also present difference-in-difference estimates for our four progressivity measures. The estimates are all positive and statistically significant at the 1% level. In terms of size, the

Figure 3.A.2: Difference-in-difference: income tax rate at different income deciles



Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The figure plots β from equation 3.5 and its 95% confidence bands. The deciles refer to the income distribution in each municipality.

effects are slightly larger than our main estimates. Taken together, these results suggest that our main estimates are not specific to municipalities at the threshold.

Table 3.A.2: Difference-in-difference: progressivity measures

	(1) average rate progression	(2) progressive tax	(3) marginal rate progression	(4) exemption level
LATE	0.257***	0.096***	0.259***	975***
	(0.028)	(0.011)	(0.029)	(132)
mean	0.173	0.084	0.178	870
N	24,081	24,081	24,081	24,081

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports β from equation 3.5 estimated for each outcome variable (reported at the top of each column). The average and marginal rate progressions are estimates of the slope of the average and marginal income tax schedules. Progressive tax is a dummy for whether the municipality has a tax rate which is not uniform. Exemption level is the amount of income (in €) exempted from the income tax. In the bottom panel, the sample mean of the outcome variable and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

3.A.3 Pareto Bounds by Bierbrauer, Boyer, and Peichl (2021)

We construct upper pareto bounds as suggested by Bierbrauer, Boyer, and Peichl (2021), that indicate a level of taxation that is inefficiently high. If tax rates are higher than these bounds, cutting taxes (even for the rich) would lead to a Pareto improvement because revenues would increase. Intuitively, if the upper Pareto bound is violated, the marginal tax rate is on the right side of the Laffer curve. The upper bound is constructed as follows: $D^{upper}(y_0) = \frac{1-F_y(y_0)}{f_y(y_0)y_0}\frac{1}{\epsilon}$, where $F_y(y_0)$ is the cumulative distribution function of taxable income y evaluated at y_0, $f_y(y_0)$ is the density function of taxable income y evaluated at y_0 and ϵ is the elasticity of taxable income (ETI). The bound is violated if $\frac{T'(y_0)}{1-T'(y_0)} > D^{upper}(y_0)$ holds, where $T'(y_0)$ is the marginal tax rate on income y_0. We construct $T'(y_0)$ by adding up the federal, regional and municipality tax rates. Since we only have information on the distribution of taxable income in brackets, we make the simplifying assumption that income uniformly distributed within brackets. For the elasticity of taxable income, we assume values from 0.25 (as reported in a survey of the literature by Saez, Slemrod, and Giertz (2012) and in a meta-analysis by Neisser (2021)) to very high values like 1.25 as found by Rubolino (2019).

As Table 3.A.3 shows, the bounds are never violated for $\epsilon \leq 1$ and only 3% of our sample municipalities violate them if we assume an ETI of 1.25. We additionally split our sample into municipalities with a college-educated mayor and those without one. The two groups of municipalities do not show any difference with respect to the share of violators. Take together, these findings suggest that before 2013 taxation was not inefficiently high, in the sense that after the introduction of the DSP, it was feasible to increase revenues by raising tax rates. Furthermore, the scope for increasing revenues was not different between municipalities with mayors with different skill levels, thus ruling our the possibility that our heterogeneous results are driven by differences in the income distributions or in the pre-reform tax systems. Additionally, we show that the reform itself did not force municipalities to violate their pareto bounds. As Table 3.A.4 shows, we do not find any effect when we use a dummy for the pareto bounds being

Table 3.A.3: Share of municipalities violating the upper pareto bounds

	all municipalities					college-educated mayor					non-college-educated mayor				
ETI	0.25	0.50	0.75	1.00	1.25	0.25	0.50	0.75	1.00	1.25	0.25	0.50	0.75	1.00	1.25
tax on 1st decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 2nd decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 3rd decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 4th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 5th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 6th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 7th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 8th decile	0.00	0.00	0.00	0.00	0.03	0.00	0.00	0.00	0.00	0.03	0.00	0.00	0.00	0.00	0.03
tax on 9th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: The sample is the same as in our main analysis. We additionally restrict the sample to the year 2012 as it was the last year before the reform.

Table 3.A.4: Effect of the reform on violating the pareto bounds

	(1) tax inefficiently high	(2) tax inefficiently high	(3) tax inefficiently high	(4) tax inefficiently high	(5) tax inefficiently high
LATE	-0.009	-0.024	-0.023	0.003	-0.035
	(0.019)	(0.028)	(0.028)	(0.028)	(0.048)
LATE x college degree		0.040	0.038	-0.013	0.006
		(0.035)	(0.033)	(0.033)	(0.033)
LATE x female mayor					0.069
					(0.163)
LATE x left-wing mayor					0.025
					(0.154)
LATE x right-wing mayor					-0.360
					(0.297)
LATE x centrist mayor					-0.489
					(0.342)
LATE x low win margin					0.097
					(0.114)
LATE x term limit					-0.041
					(0.100)
LATE x high pre-reform deficit					0.133
					(0.132)
LATE x low top income share					-0.177
					(0.132)
controls			yes	yes	yes
municipality FE				yes	yes
mean	0.035	0.035	0.035	0.035	0.035
bandwidth	663	663	663	663	663
N	17,433	17,048	17,048	17,048	17,048

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities model and the model extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3.3. We measure mayors' skills using a dummy for college degree, which is equal to one in case the mayor holds one. Details on all covariates are described in Section 3.4. The estimation method is local linear regression. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

violated as our outcomes variable. This holds both for municipalities with a college-educated mayor and for municipalities without one.

3.A.4 Property Taxation

Property taxation was reformed several times during our sample period 2007-2015 (see Messina and Savegnago (2014) for a detailed review in Italian). The main property tax at the start of our sample period was named ICI and its tax base was based on the cadastral value, the cadastral zone and on the size as well as the type of the dwelling. Municipalities had some flexibility in setting tax rates and they could set a different tax rate for main dwellings (where the taxpayer has his regular registered residence) and other dwellings. Municipalities also could set a flat reduction on the tax bill, which was applied only on main dwellings. All revenues would accrue to municipalities. The ICI on main dwellings was abolished in 2008, while it remained in operation on other dwellings. In 2012, a new tax named IMU replaced ICI. In the first year IMU was levied on both main and other dwellings, but already in 2013 a discount on main dwellings was introduced. Since 2014 the IMU on main dwellings was abolished, while it remained in operation for other dwellings. Next, a new tax, TASI, was introduced in 2014 in addition to IMU. The tax base for TASI was the same as for ICI and IMU, but TASI was also (partially) levied on renters. Municipalities could set different TASI tax rates for main and other dwellings, as well as a flat reduction for main dwellings. The range of feasible TASI tax rates and reductions was lower than for IMU. Due to these reforms, there was limited scope for mayors to increase revenues by increasing property taxation in 2013 and later years, due to the abolition of IMU on main dwellings and the introduction of the less remunerative TASI. However, we are going to test whether the introduction of the DSP had any effect on property taxation using data on IMU (tax rates and reductions on main dwellings, and tax rates on other dwellings), and on TASI (only the two tax rates, as the data on reductions is not available). Recall that, in our setting the pre-reform period is 2007-12, while the post-reform period is 2013-15. To test for effects of property taxation on main and other dwellings, we restrict our sample to the year between 2012 and 2015 and add up the IMU and TASI tax rates since they share the same tax base.

Table 3.A.5 contains the difference-in-discontinuity estimates on the property tax rates for both main dwellings and other dwellings as well as the flat reduction on the tax bill. We do not find a significant effect on any of the tax instruments. This is consistent with our result from Table 3.6.4 that property tax revenues did not change because of the introduction of the fiscal rule. Furthermore, we also estimate our interaction model with respect to the mayor's skill. As Table 3.A.6 shows, high-skilled mayors choose (weakly) higher property tax rates on other dwellings, whereas there are no no differential effects with respect to the property tax on main dwellings. While these effects are very small and only weakly significant, they are consistent with our main finding. In fact, people subject to the rate on other dwellings are either residents elsewhere, and therefore not eligible voters in the municipality, and/or owners of more than one dwelling, and so likely wealthier than the median voter. We also do not find any differential effect by the share of non-resident dwellings taken from the census (see Table 3.A.7).

Table 3.A.5: Effect of the reform on property tax rates

	(1) property tax on main dwellings	(2) property tax on other dwellings	(3) deduction amount
LATE	0.127	0.103	1.026
	(0.105)	(0.083)	(1.142)
mean	3.032	8.914	198.770
bandwidth	429	512	578
N	4,898	5,824	6,488

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 3.2. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). Outcome variables are reported on top of each column. The sample includes the years 2012 to 2015. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 3.A.6: Effect of the reform on property tax rates by mayor's skill

	(1) property tax on main dwellings	(2) property tax on other dwellings	(3) deduction amount	(4) property tax on main dwellings	(5) property tax on other dwellings	(6) deduction amount
LATE	0.122	-0.034	1.697	0.084	-0.005	1.847
	(0.145)	(0.114)	(1.472)	(0.135)	(0.102)	(1.654)
LATE x college degree	-0.082	0.376*	-1.766			
	(0.233)	(0.196)	(2.535)			
LATE x high-skill job				-0.054	0.329	-2.222
				(0.258)	(0.209)	(2.121)
mean	3.030	8.913	198.777	3.021	8.911	198.715
bandwidth	429	512	578	429	512	578
N	4,797	5,709	6,362	4,689	5,586	6,214

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3.3. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). Outcome variables are reported on top of each column. The sample includes the years 2012 to 2015. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

The waste tax can be considered as a property tax in disguise, because in most instances the tax bill is a function of the surface of the dwelling and the number of households components (Messina, Savegnago, and Sechi, 2018). Unfortunately, there exists no digitized data on the waste tax. Therefore, we can not investigate effects on waste tax rates. However, in Table 3.6.4 we show that revenues from the waste tax did not change due to the reform. In total, we conclude that the reform had no impact on property taxes.

Table 3.A.7: Effect of the reform on property tax rates by share of non-residents

	(1) property tax on main dwellings	(2) property tax on other dwellings	(3) deduction amount
LATE	0.141	0.153	0.257
	(0.121)	(0.094)	(1.131)
LATE x above-median non-resident dwellings	-0.076	-0.264	4.224
	(0.236)	(0.205)	(3.833)
mean	3.032	8.914	198.770
bandwidth	429	512	578
N	4.898	5.824	6.488

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3.3. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). Outcome variables are reported on top of each column. The sample includes the years 2012 to 2015. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

3.B Institutions and Summary Statistics

Table 3.B.2: Population cutoffs in Italian municipalities before and after 2013

population	mayor's wage		wage of ex. committee		size of city council		signature requirement		gender quota		fiscal rule	
	before	after	before	after	before	after	before	after	before	after	before	after
below 1,000	1,291	1,291	15%	15%	12	12	0	0	no	no	no	no
1,000 - 2,000	1,446	1,446	20%	20%	12	12	30	30	no	no	no	yes
2,000 - 3,000	1,446	1,446	20%	20%	12	12	40	40	no	no	no	yes
3,000 - 5,000	2,169	2,169	20%	20%	16	16	40	40	no	no	no	yes
5,000 - 10,000	2,789	2,789	50%	50%	16	16	80	80	no	yes	yes	yes

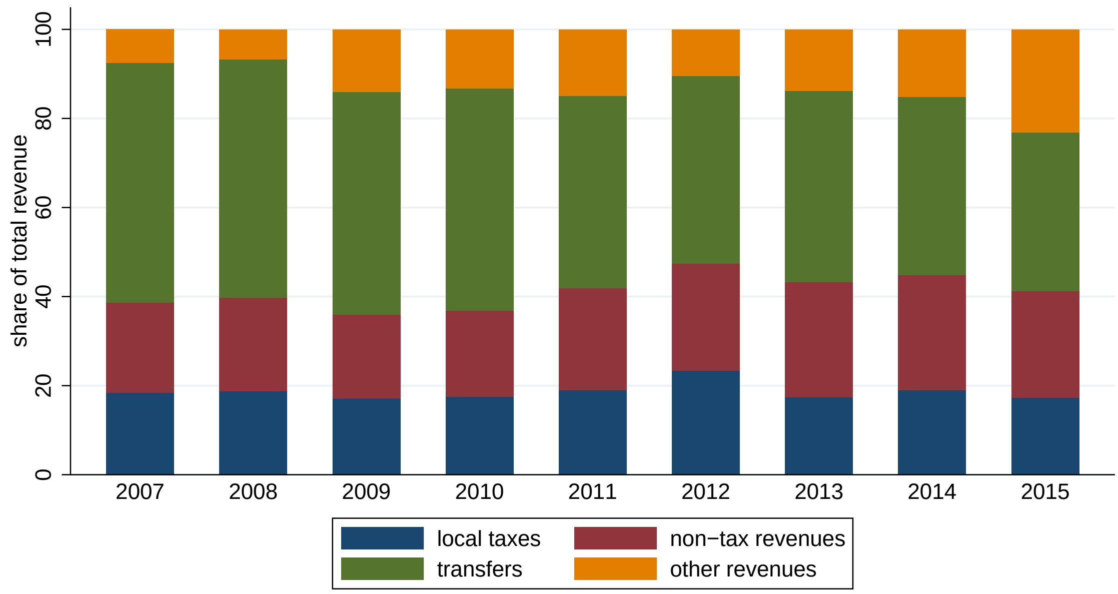
Source: Grembi, Nannicini, and Troiano (2016), Vincent (2017), Baltrunaite et al. (2019). Notes: Policies varying at different legislative thresholds in the period 2007 - 2015. The *before* columns indicate the situation from 2007 to 2012, while the *after* columns refer to period from 2013 to 2015. Discontinuities at thresholds over 5,000 inhabitants are omitted. Population is the number of resident inhabitants. The wage of both the mayor and the executive committee refer to monthly gross wages and the latter is expressed as a percentage of the former. Size of city council is the number of seats in the city council. The signature requirement refers to number of signatures a candidate for mayor requires to be allowed to run, while the gender quota refers to candidate lists and new a system of double preference voting conditional on gender.

Table 3.B.1: Fiscal rule details

year	target	target function of	reference period	pop. threshold
2007	Saldo Finanziario	expenditures	2003-05	3,000
2008	Saldo Finanziario	expenditures	2003-05	3,000
2009	Saldo Finanziario	Saldo Finanziario	2007	5,000
2010	Saldo Finanziario	Saldo Finanziario	2007	5,000
2011	Saldo Finanziario	current expenditures	2006-08	5,000
2012	Saldo Finanziario	current expenditures	2006-08	5,000
2013	Saldo Finanziario	current expenditures	2007-09	1,000
2014	Saldo Finanziario	current expenditures	2009-11	1,000
2015	Saldo Finanziario	current expenditures	2010-12	1,000

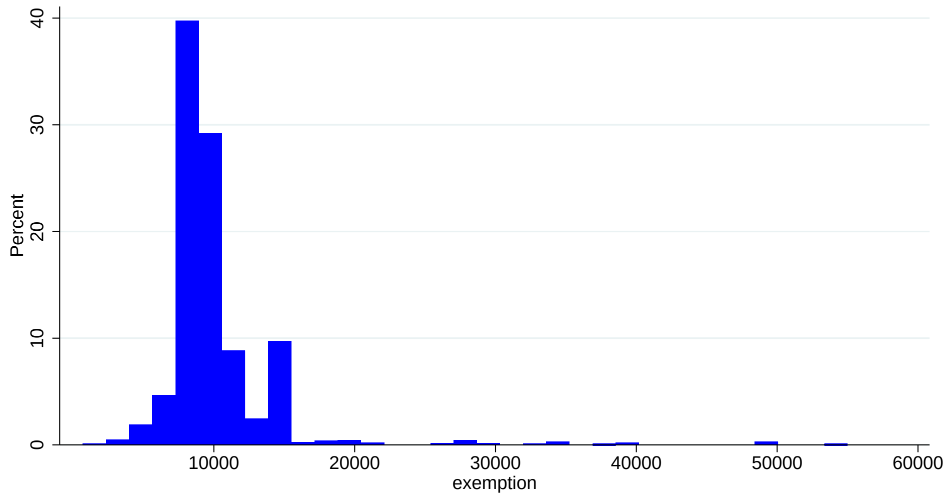
Notes: The table reports details on the target of the fiscal rule for different years. *Saldo Finanziario* is defined as the difference between expenditures and revenues, net of repayment of outstanding debt and of lending. The target *Saldo Finanziario* must be below a target defined as a function of some budget account items measured in a reference period.

Figure 3.B.1: Municipal revenues over time



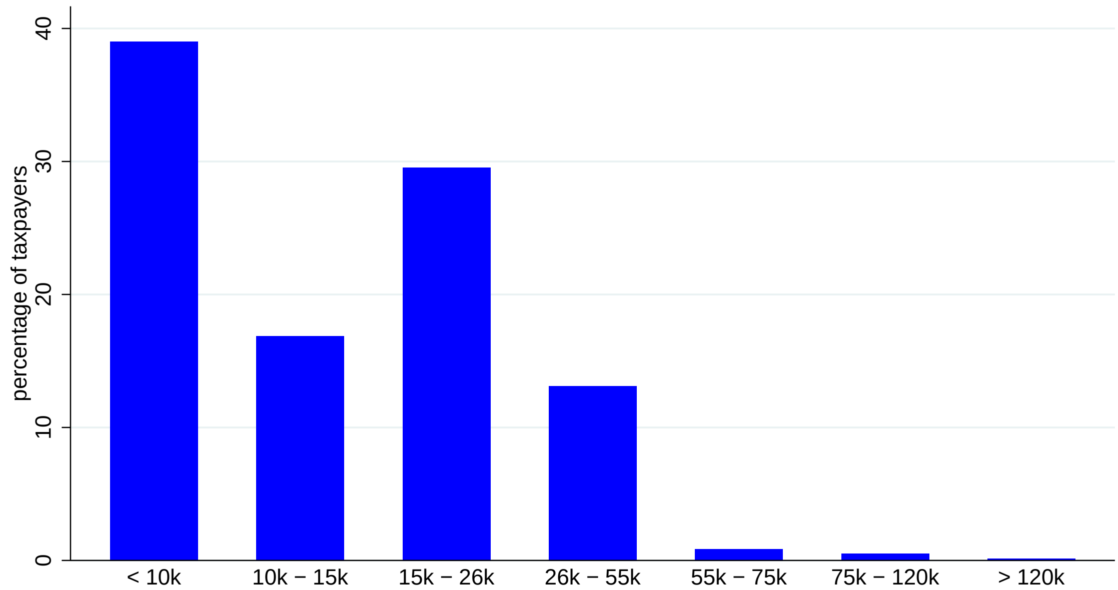
Notes: This figure plots the share of total revenue for different revenue categories of Italian municipalities below 2,500 inhabitants. Transfers also include revenues from the solidarity fund, financed by the property tax. Source: Municipal budget accounts (*Conti consuntivi*, accrual basis, Ministry of the Interior.)

Figure 3.B.2: Distribution of exemption levels



Notes: This figure plots the density of exemption levels for the local personal income tax in Italian municipalities. The sample is restricted to municipalities that have a positive exemption level and less than 2,500 inhabitants.

Figure 3.B.3: Distribution of taxpayers



Notes: This figure plots the percentage of tax payers in each taxable income bracket for municipalities with less than 2,500 inhabitants.

Table 3.B.3: Descriptive statistics

	full sample			population $\leq$ 2500			population $>$ 2500			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	mean	sd	N	mean	sd	N	mean	sd	N	difference
area (in km^2)	34.384	47.298	58,323	23.364	23.063	28,335	44.795	60.209	29,988	-21.431***
coast dummy	0.068	0.252	58,323	0.022	0.147	28,335	0.112	0.315	29,988	-0.090***
altitude (in m)	334.405	275.099	58,323	448.871	292.956	28,335	226.249	204.985	29,988	222.622***
mayor: age	51.912	10.138	57,278	52.018	10.550	28,068	51.811	9.725	29,210	0.207
mayor: female	0.118	0.322	57,278	0.117	0.322	28,068	0.118	0.322	29,210	-0.001
mayor: college degree	0.454	0.498	56,581	0.364	0.481	27,642	0.540	0.498	28,939	-0.176***
mayor: high-skill occupation	0.378	0.485	55,517	0.306	0.461	27,335	0.448	0.497	28,182	-0.142***
mayor: political	0.295	0.456	56,481	0.154	0.361	27,646	0.430	0.495	28,835	-0.275***
mayor: last win margin	0.259	0.256	57,346	0.318	0.306	28,098	0.201	0.179	29,248	0.117***
mayor: term limit	0.302	0.459	57,181	0.287	0.452	28,007	0.317	0.465	29,174	-0.030***
years to next election	1.994	1.410	57,095	1.980	1.404	27,911	2.008	1.415	29,184	-0.028***
share: age $\geq$ 60	0.292	0.064	58,323	0.321	0.068	28,335	0.265	0.045	29,988	0.056***
share: female	0.508	0.015	58,323	0.505	0.019	28,335	0.511	0.010	29,988	-0.005***
share: college degree	0.074	0.027	58,323	0.067	0.024	28,335	0.081	0.028	29,988	-0.014***
top income share	0.106	0.078	58,323	0.075	0.075	28,335	0.135	0.068	29,988	-0.060***
taxable income per capita	12,605.327	3,309.273	57,569	11,962.550	3,047.158	28,148	13,220.292	3,431.061	29,421	-1,257.742***
tax rate at the 1st decile	0.348	0.279	58,062	0.329	0.264	28,148	0.366	0.292	29,914	-0.037***
tax rate at the 2nd decile	0.355	0.278	58,062	0.333	0.264	28,148	0.377	0.290	29,914	-0.044***
tax rate at the 3rd decile	0.388	0.272	58,062	0.346	0.262	28,148	0.426	0.276	29,914	-0.080***
tax rate at the 4th decile	0.412	0.265	58,062	0.358	0.259	28,148	0.464	0.260	29,914	-0.105***
tax rate at the 5th decile	0.425	0.259	58,062	0.365	0.257	28,148	0.482	0.248	29,914	-0.117***
tax rate at the 6th decile	0.430	0.256	58,062	0.367	0.256	28,148	0.490	0.241	29,914	-0.123***
tax rate at the 7th decile	0.433	0.255	58,062	0.369	0.255	28,148	0.494	0.239	29,914	-0.125***
tax rate at the 8th decile	0.436	0.254	58,062	0.370	0.255	28,148	0.498	0.236	29,914	-0.128***
tax rate at the 9th decile	0.440	0.253	58,062	0.372	0.255	28,148	0.504	0.234	29,914	-0.132***
average rate progression	0.428	1.000	58,323	0.198	0.672	28,335	0.646	1.192	29,988	-0.448***
marginal rate progression	0.433	1.000	58,323	0.204	0.690	28,335	0.649	1.183	29,988	-0.444***
exemption level	2,019.226	4,718.198	58,035	997.086	3,348.429	28,135	2,981.029	5,545.364	29,900	-1983.943***
progressive tax	0.179	0.383	58,062	0.094	0.292	28,148	0.259	0.438	29,914	-0.166***
deficit	0.136	202.621	57,400	5.711	268.320	28,061	-5.197	106.800	29,339	10.908***
income tax revenues	45.811	32.760	57,104	36.972	32.142	27,822	54.209	31.091	29,282	-17.237***
property tax revenues	173.672	476.889	57,104	182.533	249.620	27,822	165.252	619.811	29,282	17.281***
trash tax revenues	106.744	84.675	57,104	114.252	88.938	27,822	99.610	79.764	29,282	14.642***
non-tax revenues	370.977	467.983	57,104	443.152	627.985	27,822	302.401	206.758	29,282	140.751***
transfer revenues	682.851	1,296.016	57,104	993.877	1,752.916	27,822	387.333	420.556	29,282	606.544***
loan revenues	143.526	383.864	57,104	173.298	489.218	27,822	115.238	241.489	29,282	58.060***
other revenues	104.349	700.645	57,104	137.844	760.640	27,822	72.525	636.822	29,282	65.319***
total expenditures	1,371.050	1,676.247	57,656	1,784.216	2,254.043	28,237	974.485	555.983	29,419	809.730***
current expenditures	859.789	595.954	57,656	1,003.021	772.813	28,237	722.312	290.224	29,419	280.709***
capital expenditures	511.262	1,360.819	57,656	781.195	1,858.662	28,237	252.174	420.030	29,419	529.022***
exp: administrative	358.101	415.556	57,656	471.887	530.687	28,237	248.887	209.219	29,419	223.000***
exp: culture	25.129	98.424	57,656	27.745	133.727	28,237	22.619	42.526	29,419	5.126***
exp: development	15.072	123.668	57,656	19.856	167.832	28,237	10.481	53.803	29,419	9.374***
exp: education	97.632	123.998	57,656	102.513	155.077	28,237	92.947	83.705	29,419	9.566***
exp: environment	320.293	949.229	57,656	445.122	1,323.818	28,237	200.479	233.465	29,419	244.643***
exp: judiciary	1.132	12.033	57,656	0.591	14.434	28,237	1.651	9.124	29,419	-1.060***
exp: police	32.768	44.306	57,656	33.043	57.428	28,237	32.505	26.107	29,419	0.537
exp: social	97.483	152.768	57,656	93.587	183.620	28,237	101.223	115.537	29,419	-7.635***
exp: sport	32.031	341.246	57,656	43.866	485.028	28,237	20.671	46.479	29,419	23.195***
exp: resources	25.341	461.792	57,656	39.354	649.422	28,237	11.890	113.004	29,419	27.464***
exp: transport	183.282	376.503	57,656	264.642	511.934	28,237	105.191	117.561	29,419	159.451***
exp: tourism	26.458	278.320	57,656	43.964	390.665	28,237	9.655	68.931	29,419	34.309***
re-run	0.594	0.491	13,149	0.599	0.490	6,563	0.400	0.492	6,586	0.010
reelection	0.798	0.401	8,271	0.827	0.378	4,266	0.768	0.422	4,005	0.059***

Notes: * p<0.1, ** p<0.05, *** p<0.01. Column 10 displays the difference between columns 4 and 7. All expenditure, revenue, and deficit variables are expressed in per capita terms and 2015 Euros.

Table 3.B.4: Descriptive statistics: matched sample

	matched sample			non-matched sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	mean	sd	N	mean	sd	N	difference
area (in km^2)	34.855	47.672	44,781	33.000	45.448	12,357	1.855**
coast dummy	0.067	0.250	44,781	0.071	0.257	12,357	-0.004
altitude (in m)	332.851	275.745	44,781	342.933	277.283	12,357	-10.082*
mayor: female	0.115	0.319	44,768	0.127	0.333	12,249	-0.012*
mayor: college degree	0.456	0.498	44,240	0.451	0.498	12,082	0.004
mayor: age	51.807	10.072	44,768	52.261	10.312	12,249	-0.454**
mayor: political	0.296	0.457	44,129	0.293	0.455	12,093	0.003
mayor: term limit	0.295	0.456	44,725	0.330	0.470	12,196	-0.036***
years to next election	2.007	1.433	44,779	1.949	1.319	12,316	0.058***
share: college degree	0.074	0.027	44,781	0.074	0.028	12,357	0.000
share: female	0.508	0.015	44,781	0.507	0.016	12,357	0.001**
share: age $\geq$ 60	0.292	0.064	44,781	0.296	0.064	12,357	-0.004***
top income share	0.105	0.077	44,781	0.104	0.079	12,357	0.001
taxable income per capita	12,557.276	3,307.428	44,549	12,696.957	3,273.648	12,272	-139.681**
average rate progression	0.427	0.999	44,781	0.398	0.966	12,357	0.029*
marginal rate progression	0.431	0.998	44,781	0.403	0.966	12,357	0.028*
exemption level	2,006.282	4,701.750	44,764	1,930.638	4,669.887	12,353	75.644
progressive tax	0.178	0.383	44,781	0.170	0.375	12,357	0.009
runner-up: female	0.147	0.354	44,781				
runner-up: age	51.830	10.951	44,781				
runner-up: college degree	0.443	0.497	43,232				
runner-up: political	0.277	0.447	41,809				
vote margin	-0.033	0.291	44,240				
mixed race	0.439	0.496	42,765				

Notes: The matched sample includes observations for which we were able to match the main dataset with the election data. The non-matched sample includes the remaining observations. * p<0.1, ** p<0.05, *** p<0.01. Column 7 displays the difference between columns 1 and 4.

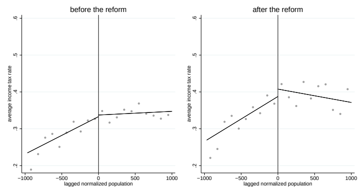
3.C Additional Findings

Table 3.C.1: Effect of the reform on the average income tax rate at different income levels

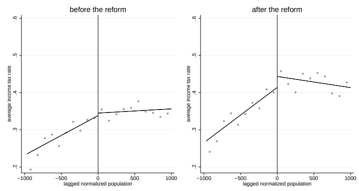
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	5k€	10k€	15k€	20k€	25k€	30k€	35k€	40k€	45k€	50k€	55k€	60k€	65k€	70k€	75k€
LATE	0.014	0.032	0.034*	0.038**	0.040**	0.040**	0.042**	0.043**	0.044**	0.044**	0.044**	0.045**	0.045**	0.045**	0.046**
	(0.020)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
> 5k€	-	0.034	0.049	0.026	0.020	0.020	0.015	0.013	0.013	0.012	0.012	0.012	0.012	0.012	0.012
> 10k€	-	-	0.358	0.195	0.152	0.152	0.116	0.106	0.100	0.096	0.093	0.091	0.089	0.088	0.087
> 15k€	-	-	-	0.109	0.067	0.076	0.050	0.046	0.044	0.043	0.043	0.044	0.044	0.045	0.046
mean	0.331	0.351	0.363	0.367	0.368	0.368	0.369	0.370	0.371	0.371	0.371	0.372	0.372	0.372	0.373
bandwidth	663	663	663	663	663	663	663	663	663	663	663	663	663	663	663
N	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 3.2. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). The middle panel displays p-values for pairwise one-sided tests (estimated by seemingly unrelated regression) whether the effect is higher than the effect on the tax rate at yearly incomes of 5,000€, 10,000€, and 15,000€ respectively. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

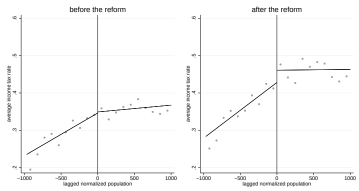
Figure 3.C.1: Regression discontinuity plots: other outcomes



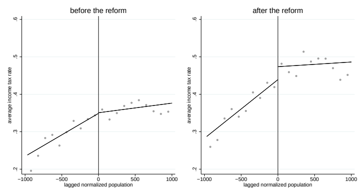
a) 2nd decile



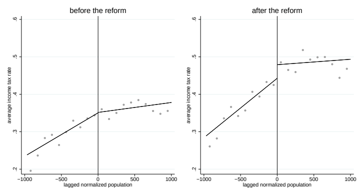
b) 3rd decile



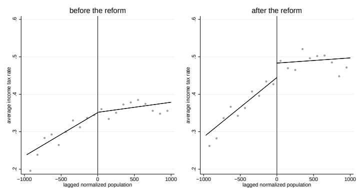
c) 4th decile



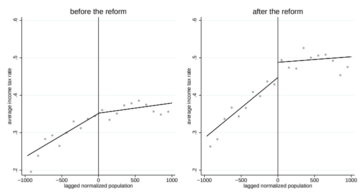
d) 5th decile



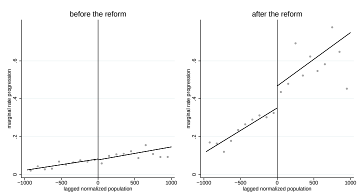
e) 6th decile



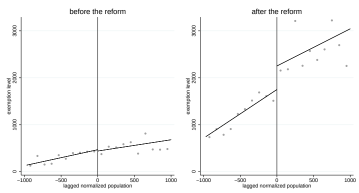
f) 7th decile



g) 8th decile



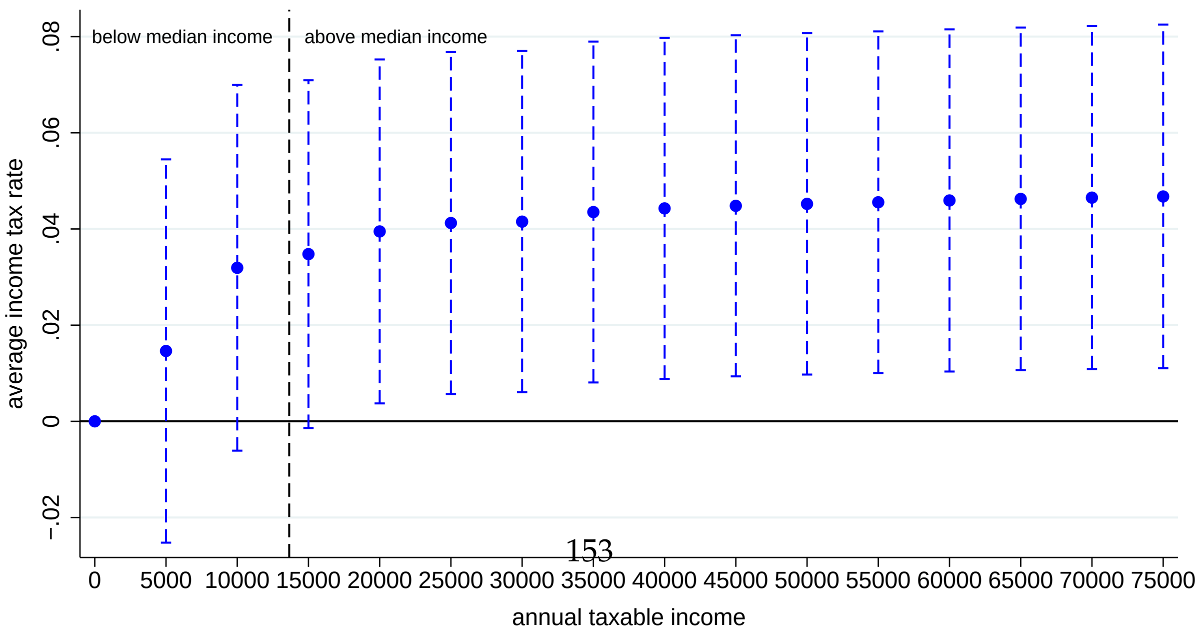
h) marginal rate progression



i) exemption level

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Each graph is a regression discontinuity plot for pre-reform years (2007-12, on the left) and post-reform years (2013-15, on the right). The outcome variable is reported underneath each graph. The running variable is lagged normalized population. Plots are obtained with the STATA command *rdplot* (Calonico, Cattaneo, and Titiunik (2015)) organizing the data in 10 bins on each side of the threshold. The lines are linear fits estimated separately on each side of the threshold.

Figure 3.C.2: Effect of the reform on income tax rates at different income levels



Notes: This figure plots the local average treatment effects reported in Table 3.C.1 and their 95% confidence bands.

Table 3.C.2: Effect of the reform on the income tax base by bracket

	(1) <10k€	(2) 10k€-15k€	(3) 15k€-28k€	(4) 28k€-55k€	(5) 55k€-75k€	(6) 75k€-120k€	(7) >120k€
log taxbase							
LATE	-0.03	0.01	0.03	0.06	0.07*	0.00	-0.06
	(0.03)	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)	(0.07)
mean	14.07	14.17	15.14	14.79	13.24	13.47	14.07
bandwidth	406	394	462	479	935	1058	1515
N	10,974	10,632	12,688	13,163	14,678	12,150	6,436

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable. The LATE corresponds to β_6 in equation 3.2. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). The outcome variables are per capita (upper panel) and total (bottom panel) tax revenues in 2015 Euros generated by tax payers with taxable income included in the bracket reported on top of each column. The table reports also the sample mean of the outcome variable, the average number of taxpayers in each bracket, the used bandwidth and the number of observations. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 3.C.3: Differential effect of the reform by mayor's skill

	(1) progressive tax	(2) progressive tax	(3) progressive tax	(4) progressive tax	(5) progressive tax	(6) progressive tax	(7) progressive tax	(8) progressive tax
LATE	-0.002	-0.002	-0.020	-0.008	-0.000	0.004	-0.009	0.012
	(0.034)	(0.034)	(0.032)	(0.070)	(0.032)	(0.032)	(0.031)	(0.069)
LATE x college degree	0.138***	0.136***	0.121***	0.116**				
	(0.053)	(0.052)	(0.046)	(0.046)				
LATE x high-skill job					0.153***	0.141**	0.113**	0.106**
					(0.059)	(0.059)	(0.051)	(0.050)
LATE x female mayor				0.045				0.045
				(0.070)				(0.072)
LATE x left-wing mayor				0.006				0.006
				(0.064)				(0.065)
LATE x right-wing mayor				-0.190				-0.187
				(0.134)				(0.136)
LATE x centrist mayor				-0.292				-0.327
				(0.232)				(0.253)
LATE x low win margin				0.027				0.024
				(0.047)				(0.048)
LATE x term limit				-0.007				-0.021
				(0.041)				(0.042)
LATE x high pre-reform deficit				0.037				0.029
				(0.054)				(0.054)
LATE x low top income share				-0.076				-0.077
				(0.054)				(0.055)
controls		yes	yes	yes		yes	yes	yes
municipality FE			yes	yes			yes	yes
mean	0.087	0.088	0.088	0.088	0.086	0.088	0.088	0.088
bandwidth	650	650	650	650	650	650	650	650
N	16,932	16,663	16,663	16,663	16,848	16,321	16,321	16,321

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3.3. We measure mayors' skills using two dummies: college degree, which is equal to one in case the mayor holds one; and high-skill job, which is equal to one in case the mayor was employed in a managing position or in an intellectual profession (e.g. lawyer, medical doctor). Details on all covariates are described in Section 3.4. The estimation method is local linear regression. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 3.C.4: Differential effect of the reform by mayor's skill

	(1) exemption level	(2) exemption level	(3) exemption level	(4) exemption level	(5) exemption level	(6) exemption level	(7) exemption level	(8) exemption level
LATE	-45	2	-211	167	-77	10	-158	289
	(406)	(403)	(385)	(861)	(357)	(354)	(355)	(830)
LATE x college degree	1494**	1457**	1363**	1361**				
	(604)	(603)	(553)	(558)				
LATE x high-skill job					1918***	1741**	1629**	1552**
					(700)	(705)	(656)	(647)
LATE x female mayor				466				339
				(819)				(847)
LATE x left-wing mayor				-94				-91
				(743)				(750)
LATE x right-wing mayor				-1988				-1950
				(1612)				(1646)
LATE x centrist mayor				-4601				-4856
				(3388)				(3518)
LATE x low win margin				124				138
				(571)				(583)
LATE x term limit				-168				-260
				(471)				(476)
LATE x high pre-reform deficit				470				366
				(657)				(656)
LATE x low top income share				-1115*				-1068
				(668)				(670)
controls		yes	yes	yes		yes	yes	yes
municipality FE			yes	yes			yes	yes
mean	896	906	906	906	886	904	904	904
bandwidth	635	635	635	635	635	635	635	635
N	16,577	16,319	16,319	16,319	16,493	15,985	15,985	15,985

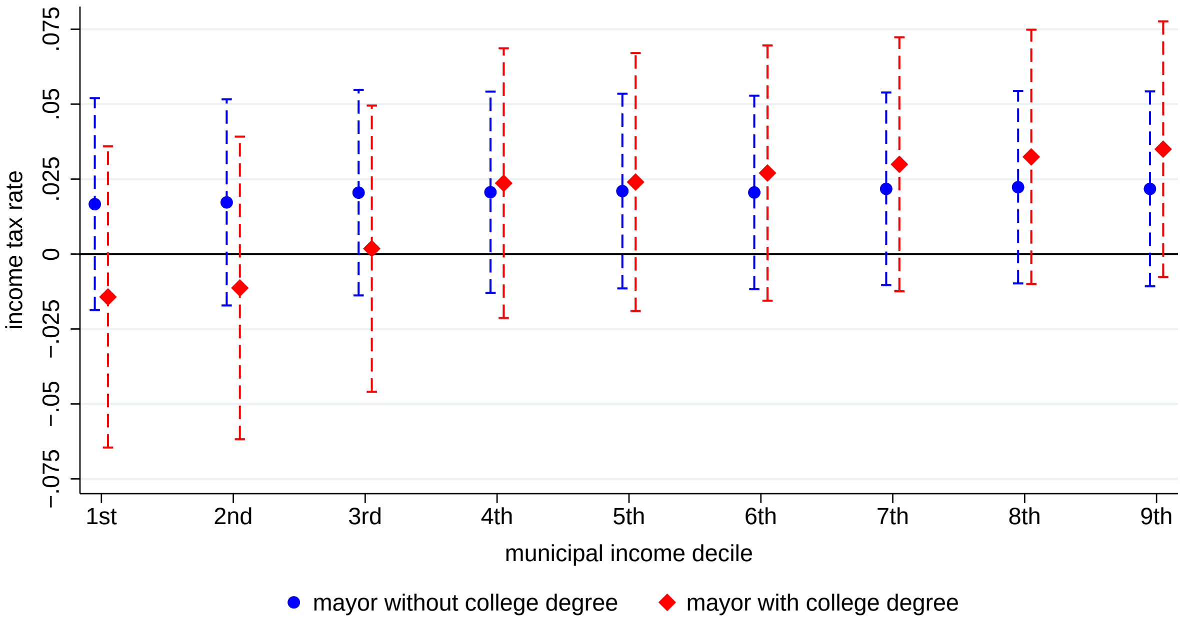
Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3.3. We measure mayors' skills using two dummies: college degree, which is equal to one in case the mayor holds one; and high-skill job, which is equal to one in case the mayor was employed in a managing position or in an intellectual profession (e.g. lawyer, medical doctor). Details on all covariates are described in Section 3.4. The estimation method is local linear regression. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 3.C.5: Differential effect of the reform by mayor's skill

	(1) marginal rate progression	(2) marginal rate progression	(3) marginal rate progression	(4) marginal rate progression	(5) marginal rate progression	(6) marginal rate progression	(7) marginal rate progression	(8) marginal rate progression
LATE	0.005	0.006	-0.009	0.055	0.016	0.024	-0.002	0.080
	(0.086)	(0.086)	(0.084)	(0.180)	(0.081)	(0.082)	(0.082)	(0.176)
LATE x college degree	0.353***	0.346***	0.281**	0.281**				
	(0.127)	(0.126)	(0.115)	(0.117)				
LATE x high-skill job					0.393***	0.364***	0.329***	0.317**
					(0.139)	(0.139)	(0.127)	(0.125)
LATE x female mayor				0.039				0.039
				(0.168)				(0.172)
LATE x left-wing mayor				-0.009				-0.008
				(0.160)				(0.161)
LATE x right-wing mayor				-0.459				-0.444
				(0.330)				(0.334)
LATE x centrist mayor				-0.523				-0.630*
				(0.331)				(0.375)
LATE x low win margin				0.082				0.076
				(0.121)				(0.125)
LATE x term limit				-0.079				-0.105
				(0.105)				(0.107)
LATE x high pre-reform deficit				0.126				0.115
				(0.140)				(0.140)
LATE x low top income share				-0.244*				-0.245*
				(0.143)				(0.144)
controls		yes	yes	yes		yes	yes	yes
municipality FE			yes	yes			yes	yes
mean	0.182	0.184	0.184	0.184	0.179	0.183	0.183	0.183
bandwidth	668	668	668	668	668	668	668	668
N	17,378	17,092	17,092	17,092	17,292	16,741	16,741	16,741

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3.3. We measure mayors' skills using two dummies: college degree, which is equal to one in case the mayor holds one; and high-skill job, which is equal to one in case the mayor was employed in a managing position or in an intellectual profession (e.g. lawyer, medical doctor). Details on all covariates are described in Section 3.4. The estimation method is local linear regression. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Figure 3.C.3: Effect of the reform on income tax rates by mayor's skill level



Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The blue estimates refer to the local average treatment effect for mayors without a college degree (β_6), while the red estimates plot the sum of β_6 and β_6^{int} referring to the effect for mayors with a college degree in equation 3.3.

Table 3.C.6: Differential effect of the reform by mayor's skill: mixed election RD

	(1) progressive tax	(2) progressive tax	(3) progressive tax	(4) progressive tax	(5) progressive tax	(6) progressive tax
LATE	-0.019	-0.024	-0.066	-0.143		
	(0.042)	(0.043)	(0.183)	(0.189)		
college degree	-0.006	-0.007	-0.009	0.235	0.024	0.074
	(0.018)	(0.019)	(0.048)	(0.210)	(0.022)	(0.064)
LATE x college degree	0.140**	0.148**	0.510**	0.339		
	(0.062)	(0.064)	(0.257)	(0.224)		
controls		yes	yes	yes	yes	yes
mixed election RD			yes	yes	yes	yes
municipality FE				yes		yes
pre-reform sample					yes	yes
mean	0.088	0.089	0.088	0.088	0.041	0.041
population bandwidth	650	650	650	650	650	650
close election bandwidth			0.15	0.15	0.15	0.15
N	13,043	12,028	1,949	1,949	1,377	1,377

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The sample is restricted to observations for which we were able to match the main dataset with the election data. Columns (1), (2), (3) and (4) report estimates of the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. In rows with "mixed election RD" switched on, the model is augmented with the margin of victory and its interaction with all other terms, and the sample is further restricted to municipality-year observations, in which the incumbent mayor was elected in a race against a runner up with a different education level (college vs. non-college). Population bandwidths are selected following Grembi, Nannicini, and Troiano (2016). Election bandwidths are selected using the using the STATA command *rdrobust*. Columns (5) and (6) report estimates of the college effect from regression discontinuity models where the running variable is the margin of victory, the treatment dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform (β_2) in equation 3.4. Details on all covariates are described in Section 3.4. In the bottom panel, the sample mean of the outcome variable, the used bandwidths and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

Table 3.C.7: Differential effect of the reform by mayor's skill: mixed election RD

	(1) exemption level	(2) exemption level	(3) exemption level	(4) exemption level	(5) exemption level	(6) exemption level
LATE	-72	-87	-1550	-932		
	(498)	(502)	(2280)	(2368)		
college degree	-118	-116	-182	941	116	386
	(163)	(177)	(539)	(2092)	(209)	(526)
LATE x college degree	1440*	1458*	5694*	3007		
	(735)	(754)	(3130)	(2848)		
controls		yes	yes	yes	yes	yes
mixed election RD			yes	yes	yes	yes
municipality FE				yes		yes
pre-reform sample					yes	yes
mean	910.04	909.19	862.72	862.72	371.17	371.17
population bandwidth	635	635	635	635	635	635
close election bandwidth			0.16	0.16	0.16	0.16
N	12,764	11,770	2,119	2,119	1,509	1,509

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The sample is restricted to observations for which we were able to match the main dataset with the election data. Columns (1), (2), (3) and (4) report estimates of the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. In rows with "mixed election RD" switched on, the model is augmented with the margin of victory and its interaction with all other terms, and the sample is further restricted to municipality-year observations, in which the incumbent mayor was elected in a race against a runner up with a different education level (college vs. non-college). Population bandwidths are selected following Grembi, Nannicini, and Troiano (2016). Election bandwidths are selected using the using the STATA command *rdrobust*. Columns (5) and (6) report estimates of the college effect from regression discontinuity models where the running variable is the margin of victory, the treatment dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform (β_2) in equation 3.4. Details on all covariates are described in Section 3.4. In the bottom panel, the sample mean of the outcome variable, the used bandwidths and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

Table 3.C.8: Differential effect of the reform by mayor's skill: mixed election RD

	(1) marginal rate progression	(2) marginal rate progression	(3) marginal rate progression	(4) marginal rate progression	(5) marginal rate progression	(6) marginal rate progression
LATE	-0.022	-0.034	-0.157	-0.317		
	(0.105)	(0.105)	(0.387)	(0.392)		
college degree	-0.045	-0.049	0.017	0.303	0.022	0.072
	(0.029)	(0.031)	(0.103)	(0.390)	(0.041)	(0.078)
LATE x college degree	0.320**	0.335**	1.077**	0.828*		
	(0.146)	(0.153)	(0.524)	(0.478)		
controls		yes	yes	yes	yes	yes
mixed election RD			yes	yes	yes	yes
municipality FE				yes		yes
pre-reform sample					yes	yes
mean	0.184	0.184	0.175	0.175	0.081	0.081
population bandwidth	668	668	668	668	668	668
close election bandwidth			0.18	0.18	0.18	0.18
N	13,384	12,355	2,418	2,418	1,725	1,725

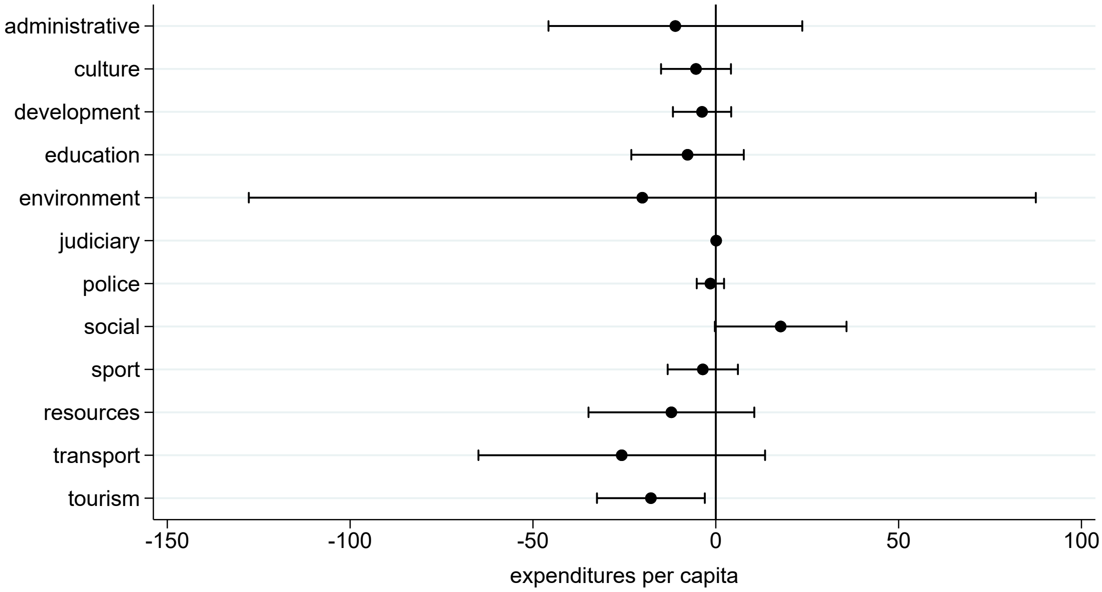
Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The sample is restricted to observations for which we were able to match the main dataset with the election data. Columns (1), (2), (3) and (4) report estimates of the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. In rows with "mixed election RD" switched on, the model is augmented with the margin of victory and its interaction with all other terms, and the sample is further restricted to municipality-year observations, in which the incumbent mayor was elected in a race against a runner up with a different education level (college vs. non-college). Population bandwidths are selected following Grembi, Nannicini, and Troiano (2016). Election bandwidths are selected using the using the STATA command *rdrobust*. Columns (5) and (6) report estimates of the college effect from regression discontinuity models where the running variable is the margin of victory, the treatment dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform (β_2) in equation 3.4. Details on all covariates are described in Section 3.4. In the bottom panel, the sample mean of the outcome variable, the used bandwidths and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

Table 3.C.9: Effects of the reform on mayors' reelection odds

	(1) reelection	(2) reelection	(3) reelection	(4) reelection	(5) re-run	(6) re-run	(7) re-run	(8) re-run
LATE	-0.037	-0.323**	-0.383***		-0.065	-0.069	-0.041	
	(0.060)	(0.144)	(0.129)		(0.061)	(0.112)	(0.116)	
high-skill job		0.400	0.235	-0.019		0.190	0.099	0.010
		(0.295)	(0.278)	(0.025)		(0.134)	(0.137)	(0.027)
LATE x high-skill job		0.644**	0.570**			0.001	-0.183	
		(0.269)	(0.251)			(0.227)	(0.235)	
municipality FE		yes	yes			yes	yes	
controls			yes	yes			yes	yes
pre-reform sample				yes				yes
mean	0.837	0.837	0.839	0.839	0.624	0.624	0.629	0.618
bandwidth	1059	1059	1059	1059	1088	1088	1088	1088
N	2,675	2,675	2,548	1,276	3,935	3,935	3,720	2,059

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Columns (1) and (5) report estimates of the local average treatment effect (LATE) in the baseline difference-in-discontinuities model. Columns (2), (3), (6) and (7) report estimates of the LATE for mayors without a high-skill job (LATE) and for mayors with a high-skill job (LATE x high-skill job), estimated using the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. Columns (4) and (8) report estimates of the high-skill job effect from a regression of the outcome on a dummy is equal to one if the mayors has a high-skill job, and the sample is restricted to years before the reform. Bandwidths are selected following Grembi, Nannicini, and Troiano (2016). The reelection outcome variable in columns (1) to (4) equals one for incumbents that run again and are reelected, and is zero for those who rerun and fail to be reelected. The rerun outcome variable in columns (5) to (8) equals one for incumbents that are not term-limited and choose to run again, and is zero for those who do not and are not term-limited.Control variables are described in Section 3.4. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Figure 3.C.4: Effect of the reform on municipal expenditures by categories



Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The figure plots the LATE corresponding to β_6 in equation 3.2 and its 95% confidence bands. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). Outcome variables are reported on top of each column. All variables are expressed in per capita terms and 2015 Euros and are winsorized.

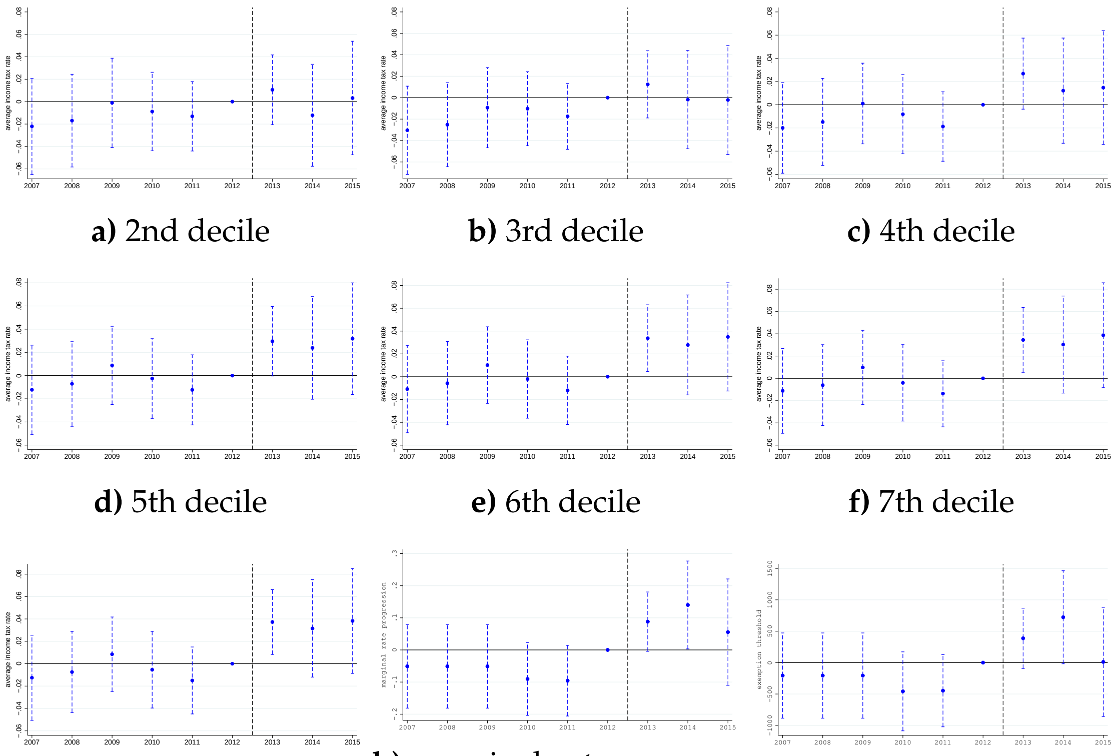
Table 3.C.10: Differential effect of the reform by mayor's skill

	(1) exp: administrative	(2) exp: culture	(3) exp: development	(4) exp: education	(5) exp: environment	(6) exp: judiciary
LATE	-19.07	1.50	-1.95	-6.96	-54.82	-0.03
	(17.02)	(5.11)	(3.30)	(10.22)	(60.10)	(0.08)
LATE x college degree	47.95	-3.91	-5.48	-3.57	93.65	0.15
	(34.15)	(9.03)	(6.96)	(16.26)	(105.79)	(0.12)
municipality FE	yes	yes	yes	yes	yes	yes
controls	yes	yes	yes	yes	yes	yes
mean	418.10	21.28	11.73	104.06	379.13	0.19
bandwidth	514	664	694	583	509	530
N	13,397	16,918	17,618	15,036	13,306	13,738
	(7) exp: police	(8) exp: social	(9) exp: sport	(10) exp: resources	(11) exp: transport	(12) exp: tourism
LATE	0.59	4.31	-6.73	-9.68	-28.19	-12.81*
	(1.77)	(10.44)	(5.41)	(9.63)	(20.84)	(6.85)
LATE x college degree	1.03	1.62	9.58	28.80*	44.90	4.98
	(3.27)	(16.52)	(8.62)	(16.12)	(37.95)	(12.92)
municipality FE	yes	yes	yes	yes	yes	yes
controls	yes	yes	yes	yes	yes	yes
mean	32.38	84.83	29.58	23.14	225.53	25.72
bandwidth	640	549	777	564	591	688
N	16,359	14,225	19,469	14,577	15,216	17,486

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3.3. Details on all covariates are described in Section 3.4. The estimation method is local linear regression. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

3.D Robustness Tests

Figure 3.D.1: Dynamic model: other outcomes



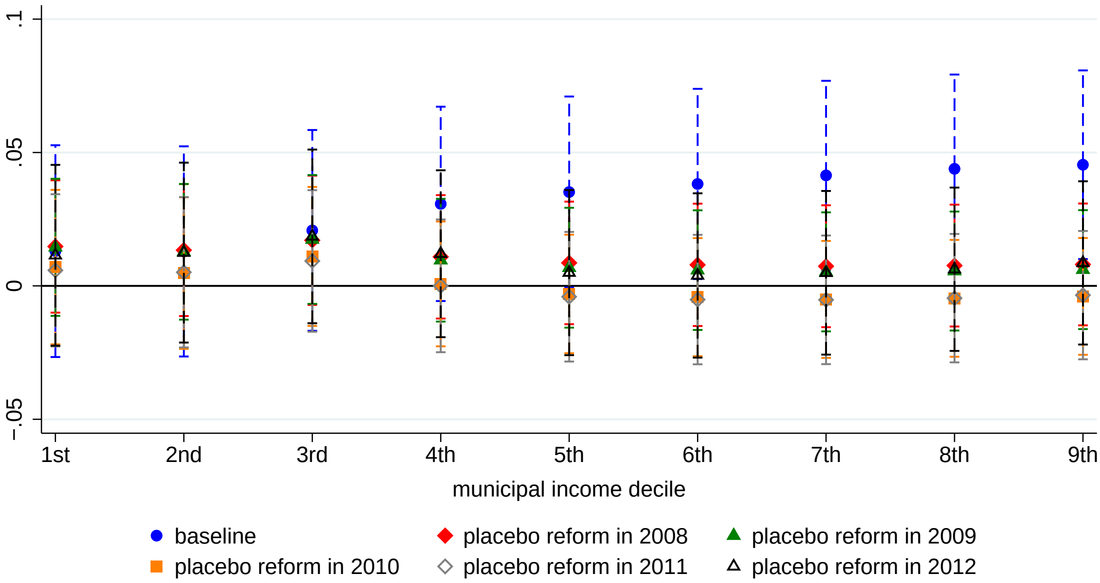
a) 2nd decile **b)** 3rd decile **c)** 4th decile

d) 5th decile **e)** 6th decile **f)** 7th decile

g) 8th decile **h)** marginal rate progression **i)** exemption level

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Each panel plots estimates from the dynamic model on a different outcome variable, reported underneath each plot. The dynamic model is an extension of the baseline difference-in-discontinuities model that includes year dummies instead of the reform dummy. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). Each dot is the estimate of the deviation of the outcome variable in the year reported on the horizontal axis relative to the pre-reform year 2012. Dotted bars are 95% confidence bands.

Figure 3.D.2: Income tax rates by municipal income deciles: placebo reforms



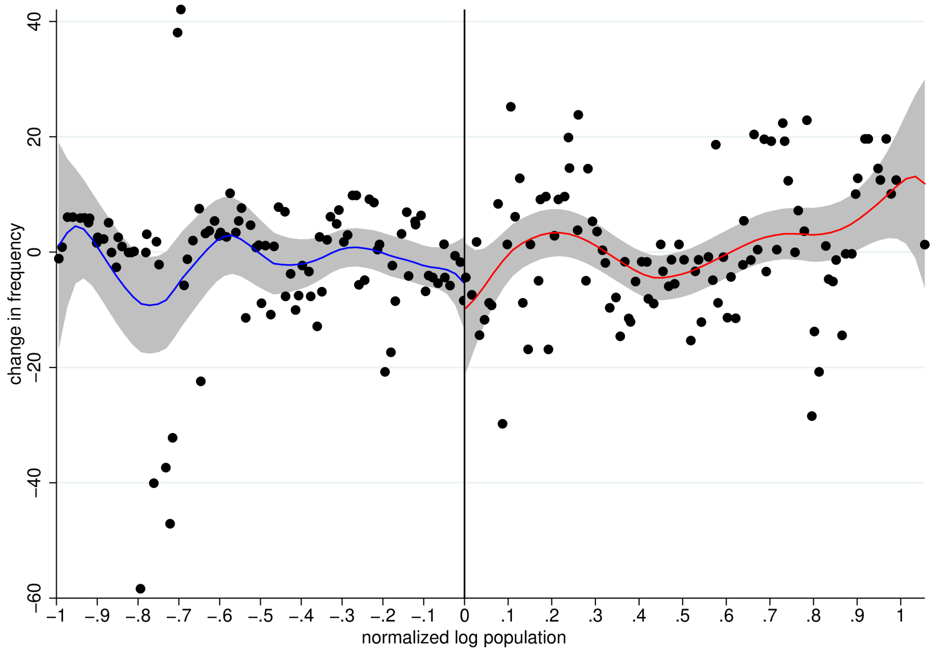
Notes: The blue line plots the local average treatment effect (β_6) and its 95% confidence bands from Table 3.5.1. All other lines plot placebo estimates. These are obtained by restricting the sample to pre-reform years, assigning the reform to a different year from 2008 to 2012 and finally re-estimating equation 3.2.

Table 3.D.1: Continuity tests

	(1) mayor: female	(2) mayor: age	(3) mayor: college degree	(4) mayor: high-skill occupation	(5) mayor: right-wing	(6) mayor: left-wing	(7) mayor: center	(8) mayor: term limit
LATE	-0.012	0.492	-0.007	-0.008	-0.003	0.001	0.000	-0.021
	(0.026)	(0.876)	(0.037)	(0.043)	(0.011)	(0.028)	(0.007)	(0.029)
mean	0.118	52.029	0.361	0.312	0.023	0.108	0.011	0.287
bandwidth	658	625	662	530	597	619	668	563
N	17,404	16,565	17,238	13,946	15,917	16,464	17,670	15,046
	(9) share: age $\geq$ 60	(10) share: female	(11) log area	(12) coast dummy	(13) altitude (in m)	(14) years to next election	(15) top income share	(16) log taxable income per capita
LATE	0.005	-0.002*	0.039	0.012*	-23.308	-0.066	0.006	0.020
	(0.005)	(0.001)	(0.052)	(0.007)	(21.546)	(0.077)	(0.005)	(0.014)
mean	0.321	0.506	2.766	0.021	445.792	1.971	0.075	9.998
bandwidth	586	621	587	581	526	511	614	658
N	15,771	16,604	15,796	15,650	14,244	13,835	16,437	17,537

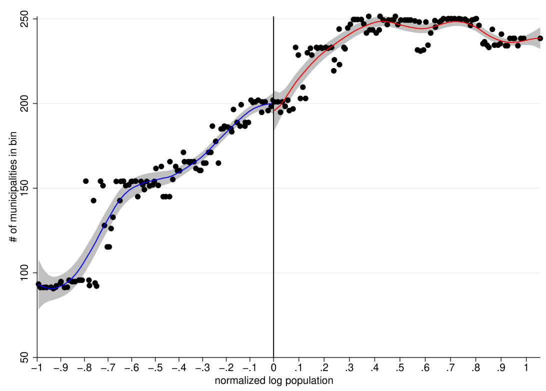
Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 3.2. The bandwidth is selected following Grembi, Nannicini, and Troiano (2016). Outcome variables are reported on top of each column. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * p<0.1, ** p<0.05, *** p<0.01

Figure 3.D.4: Dynamic McCrary test

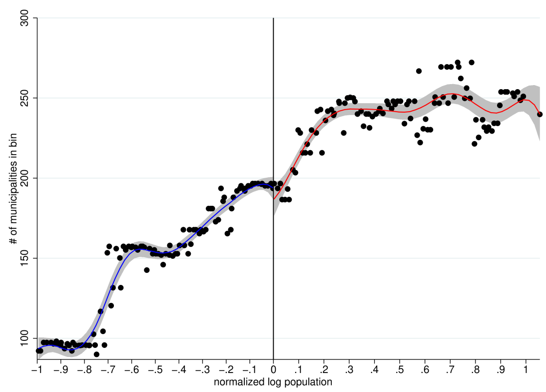


Notes: The figure presents the density plot for the difference-in-discontinuities design in the spirit of (McCrary (2008)). Each dot is the local average of the change in the total number of observations between the the pre- and post-reform periods within each bin of normalized log population. Each bin has width equal to 0.01. The lines are fit of local polynomial using a quadratic degree and a triangular kernel. Grey bans are the corresponding 95% confidence bands.

Figure 3.D.3: McCrary test before and after the reform



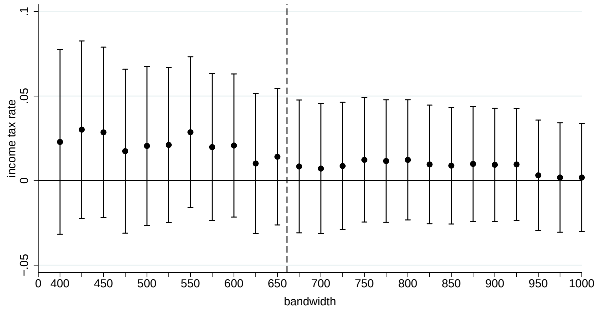
a) pre-reform



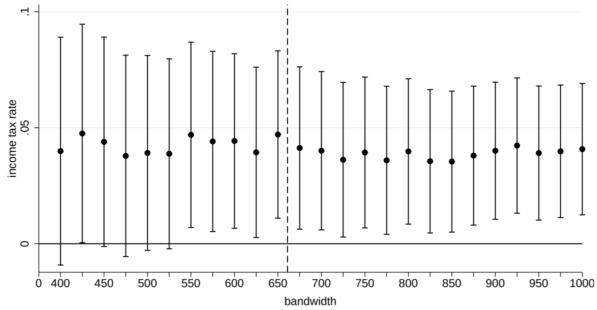
b) post-reform

Notes: This figure presents McCrary density plots (McCrary, 2008). The left panel shows a pooled graph for all pre-reform years, while the right panel shows pool graph for all post-reform years.

Figure 3.D.5: Estimates by bandwidth



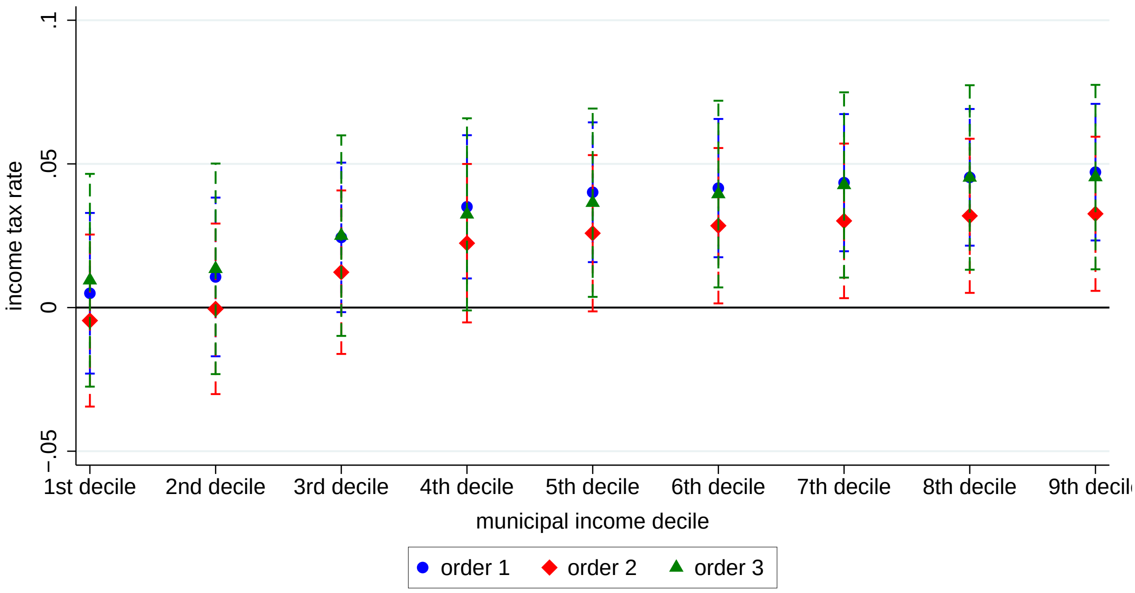
a) Income tax rate at the first decile



b) Income tax rate at the 9th decile

Notes: This figure plots the local average treatment effect (β_6) and its 95% confidence bands reported in Table 3.5.1 for different bandwidths. The dashed vertical line refers to the optimal bandwidth.

Figure 3.D.6: Income tax rates: global polynomial regressions



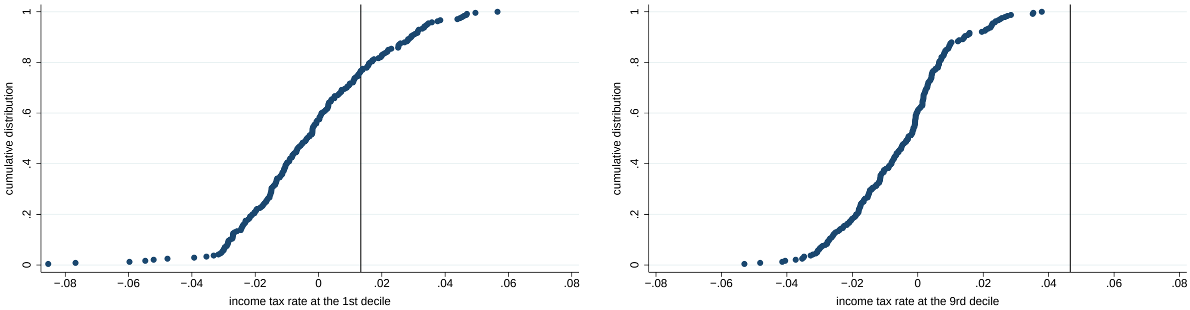
Notes: This figure plots the local average treatment effects reported in Table 3.D.2 and their 95% confidence bands.

Table 3.D.2: Income tax rates: global polynomial regressions

tax rate at	(1) 1st decile	(2) 2nd decile	(3) 3rd decile	(4) 4th decile	(5) 5th decile	(6) 6th decile	(7) 7th decile	(8) 8th decile	(9) 9th decile
polynomial of order 1									
LATE	0.005	0.011	0.024*	0.035***	0.040***	0.042***	0.043***	0.045***	0.047***
	(0.014)	(0.014)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
mean	0.329	0.332	0.346	0.358	0.364	0.367	0.368	0.369	0.371
N	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128
> 1st decile	-	0.055	0.006	0.001	0.000	0.000	0.000	0.000	0.000
> 2nd decile	-	-	0.024	0.002	0.001	0.001	0.000	0.000	0.000
> 3rd decile	-	-	-	0.029	0.012	0.010	0.006	0.004	0.003
> 4th decile	-	-	-	-	0.125	0.097	0.056	0.036	0.026
> 5th decile	-	-	-	-	-	0.252	0.100	0.056	0.040
> 6th decile	-	-	-	-	-	-	0.066	0.042	0.030
> 7th decile	-	-	-	-	-	-	-	0.102	0.051
> 8th decile	-	-	-	-	-	-	-	-	0.075
polynomial of order 2									
LATE	-0.005	-0.000	0.012	0.022	0.026*	0.028**	0.030**	0.032**	0.033**
	(0.015)	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
mean	0.329	0.332	0.346	0.358	0.364	0.367	0.368	0.369	0.371
N	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128
> 1st decile	-	0.101	0.015	0.002	0.001	0.001	0.000	0.000	0.000
> 2nd decile	-	-	0.035	0.004	0.003	0.001	0.001	0.001	0.001
> 3rd decile	-	-	-	0.03	0.022	0.011	0.007	0.004	0.005
> 4th decile	-	-	-	-	0.214	0.103	0.060	0.038	0.042
> 5th decile	-	-	-	-	-	0.055	0.016	0.011	0.026
> 6th decile	-	-	-	-	-	-	0.082	0.034	0.063
> 7th decile	-	-	-	-	-	-	-	0.053	0.107
> 8th decile	-	-	-	-	-	-	-	-	0.285
polynomial of order 3									
LATE	0.010	0.013	0.025	0.032*	0.036**	0.039**	0.043***	0.045***	0.045***
	(0.019)	(0.019)	(0.018)	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)
mean	0.329	0.332	0.346	0.358	0.364	0.367	0.368	0.369	0.371
N	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128
> 1st decile	-	0.161	0.055	0.023	0.014	0.009	0.005	0.003	0.004
> 2nd decile	-	-	0.095	0.040	0.024	0.015	0.008	0.005	0.006
> 3rd decile	-	-	-	0.139	0.082	0.050	0.025	0.016	0.021
> 4th decile	-	-	-	-	0.215	0.120	0.053	0.030	0.042
> 5th decile	-	-	-	-	-	0.140	0.030	0.014	0.036
> 6th decile	-	-	-	-	-	-	0.024	0.011	0.050
> 7th decile	-	-	-	-	-	-	-	0.039	0.147
> 8th decile	-	-	-	-	-	-	-	-	0.462

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate global regression for each outcome variable (reported at the top of each column) on the sample of all municipalities below 2,500 inhabitants with different polynomial orders n. The deciles refer to the income distribution in each municipality. The middle panel displays p-values for pairwise one-sided tests (estimated by seemingly unrelated regression) whether the effect is higher than the effect on the tax rate at the first to eighth municipal income decile, respectively. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Figure 3.D.7: Placebo thresholds



a) tax rate at the 1st decile **b)** tax rate at the 9th decile

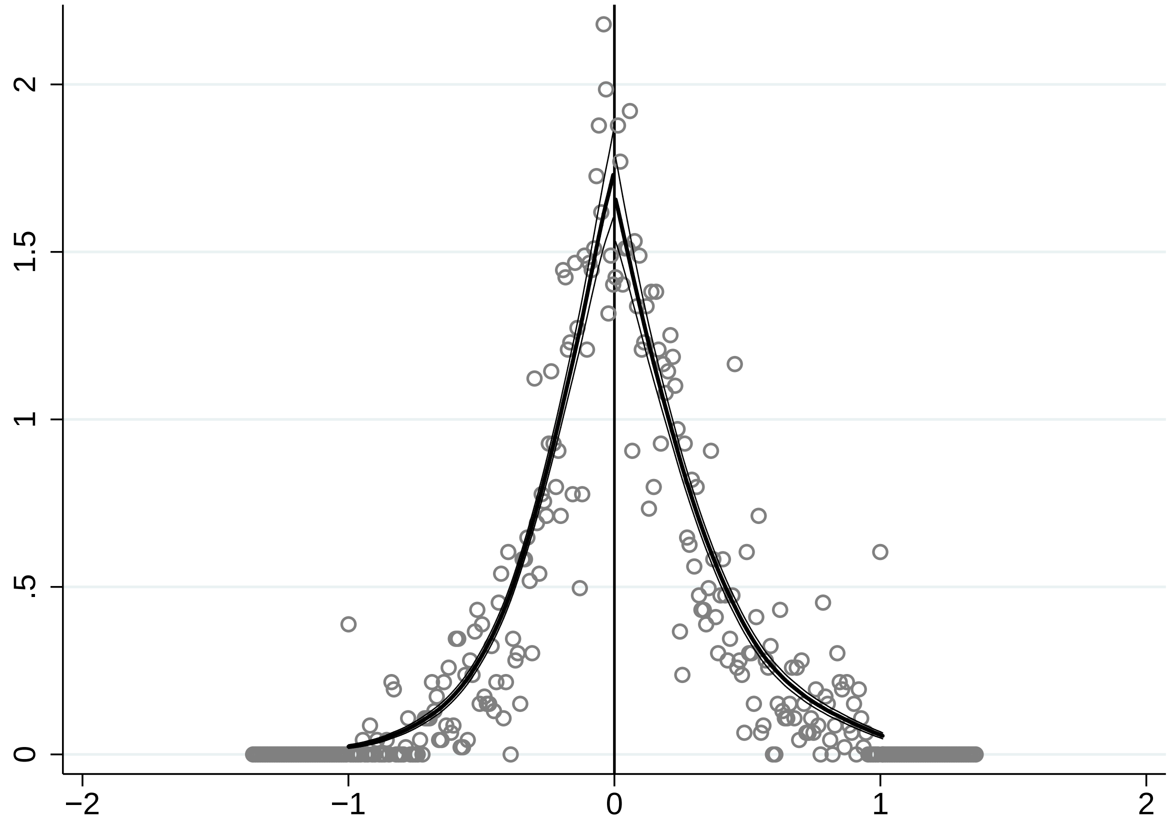
Notes: This figure plots the cumulative distribution of placebo estimates for the income tax rate at the first (panel a) and nineth decile (panel b). The placebo estimates are obtained by estimating equation 3.2 at false thresholds between 400 and 900 as well as 1,100 and 1,600. The effect at the true threshold is indicated by the vertical line.

Table 3.D.3: Mixed election disconuinity: covariate balancing

	(1) mayor: female	(2) mayor: age	(3) mayor: political	(4) mayor: term limit	(5) runner-up: female	(6) runner-up: age	(7) runner-up: political	(8) years to election
RD estimate	0.187***	-2.602	-0.001	-0.071	-0.024	3.120	0.036	0.020
	(0.066)	(1.872)	(0.058)	(0.060)	(0.075)	(2.669)	(0.070)	(0.053)
mean	0.131	51.227	0.130	0.155	0.128	52.016	0.182	1.977
bandwidth	0.15	0.13	0.14	0.13	0.10	0.11	0.15	0.15
N	2,223	2,001	2,010	2,001	1,668	1,740	2,063	2,229
	(9) share: college	(10) share: female	(11) share: age ≥ 60	(12) log area	(13) coast dummy	(14) altitude (in m)	(15) top income share	(16) log taxable income per capita
RD estimate	0.003	-0.002	-0.007	0.043	0.011	-15.828	0.013	-0.008
	(0.004)	(0.003)	(0.013)	(0.158)	(0.037)	(49.379)	(0.014)	(0.047)
mean	0.069	0.507	0.321	2.886	0.018	478.622	0.069	9.306
bandwidth	0.12	0.11	0.13	0.12	0.12	0.16	0.14	0.14
N	1,892	1,750	2,001	1,834	1,852	2,375	2,027	2,108

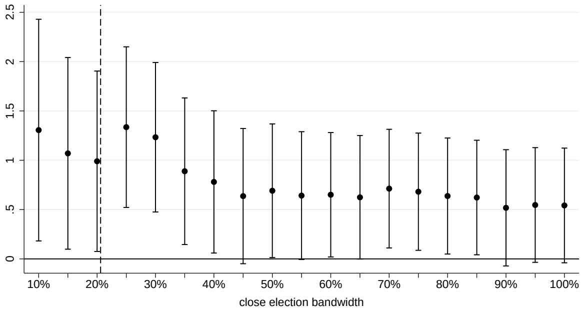
Notes: * p<0.1, ** p<0.05, *** p<0.01. This table displays regression discontinuity estimates using the STATA command *rdrobust* in a mixed election regression discontinuity design for the whole sample period (Calonico et al., 2017).

Figure 3.D.8: McCrary test for mixed elections between college- and non-college-educated candidates

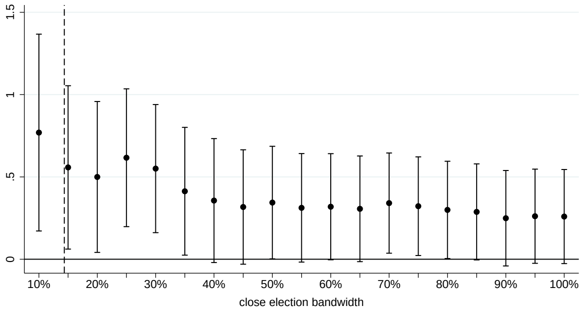


Notes: This figure presents the McCrary density plot for close elections between college- and non-college-educated candidates.

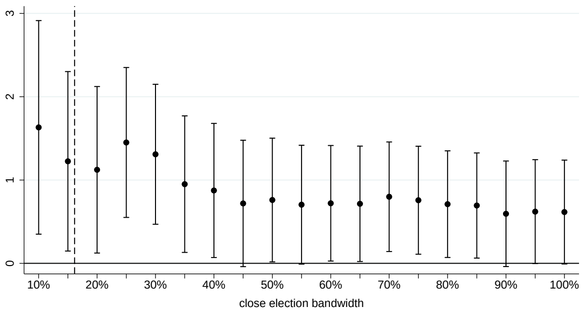
Figure 3.D.9: Close election RD: estimates by bandwidth



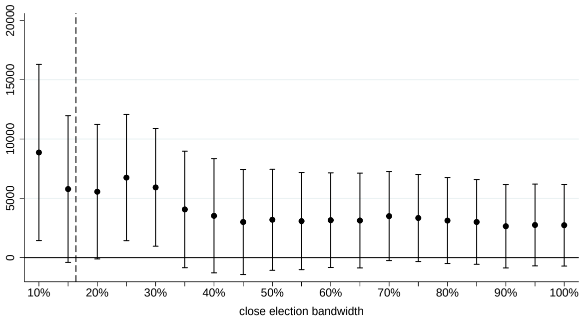
a) average rate progression



b) progressive tax system



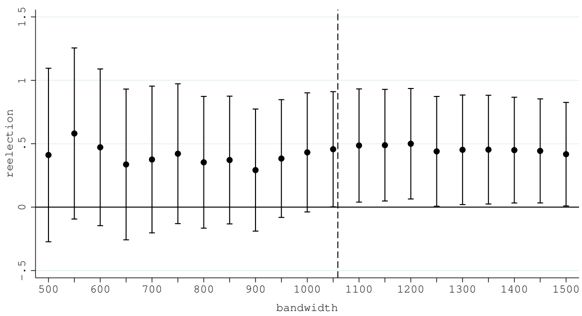
c) marginal rate progression



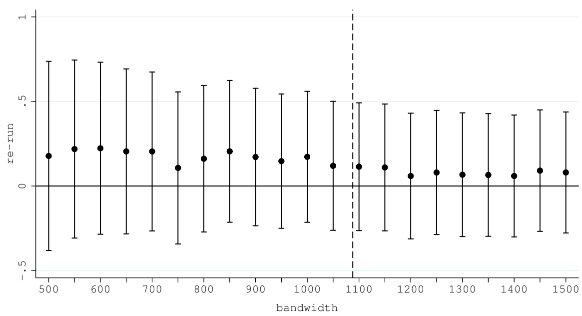
d) exemption level

Notes: This figure plots the local average treatment interaction effect for the mayor having a college degree (LATE x college degree) and its 95% confidence bands reported in Tables 3.6.2 (Panel a), 3.C.6 (Panel b), 3.C.8 (Panel c) and 3.C.7 (Panel d) for different bandwidths. The dashed vertical line refers to the optimal bandwidth.

Figure 3.D.10: Estimates by bandwidth: political outcomes



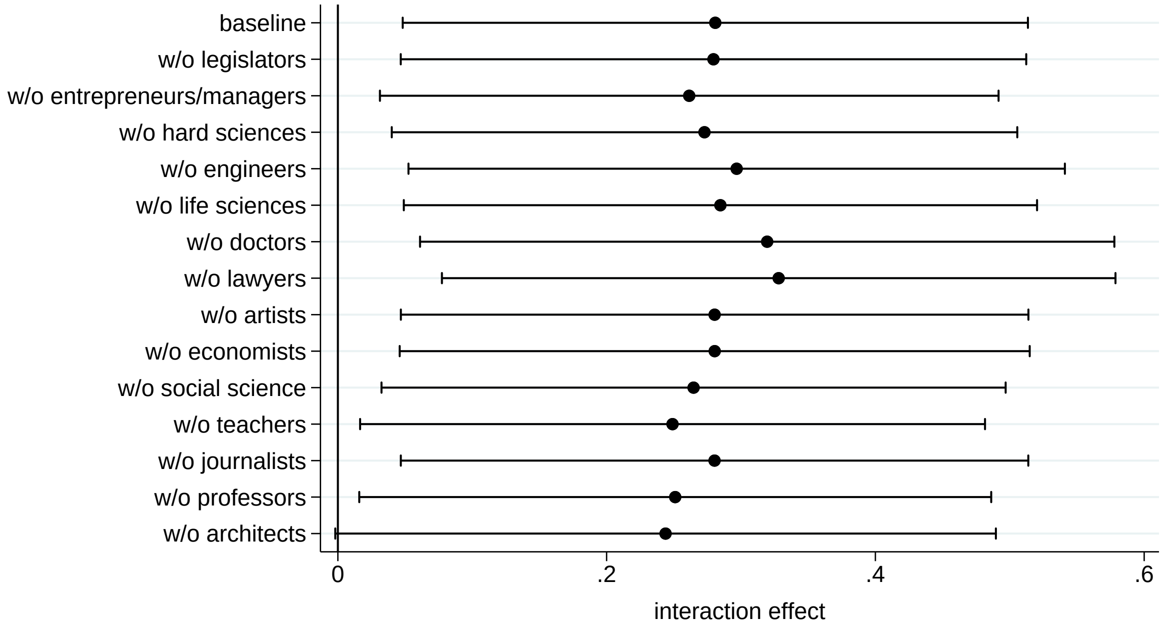
a) reelection



b) re-run

Notes: This figure plots the local average treatment interaction effect for the mayor having a college degree (LATE x college degree) and its 95% confidence bands reported in Table 3.6.3 for different bandwidths. The dashed vertical line refers to the optimal bandwidth.

Figure 3.D.11: Average progression rate interaction effect: dropping job categories



Notes: This figure plots the heterogeneous average treatment effects for college-educated mayors dropping one job category at a time.

Table 3.D.4: Municipal budget accounts: placebo regressions

	(1) income tax revenues	(2) property tax revenues	(3) trash tax revenues	(4) non-tax revenues	(5) transfer revenues	(6) loan revenues
placebo in 2008	0.49	5.42	-3.01	-20.09	-4.23	7.35
	(1.57)	(9.45)	(3.93)	(19.04)	(80.50)	(26.08)
placebo in 2009	1.20	-1.54	-6.04	-24.38	37.52	6.43
	(1.36)	(8.26)	(3.79)	(20.60)	(66.85)	(25.17)
placebo in 2010	-0.34	-0.56	-7.93*	-28.42	45.86	9.97
	(1.25)	(7.77)	(4.05)	(20.92)	(67.41)	(23.42)
placebo in 2011	0.53	3.37	-8.56**	-19.42	-39.05	-10.07
	(1.28)	(8.09)	(4.31)	(21.88)	(66.91)	(23.06)
placebo in 2012	2.51	12.62	-3.93	-14.18	-32.32	-17.42
	(1.57)	(9.54)	(4.09)	(24.05)	(76.90)	(26.41)
mean	32.64	182.74	112.06	383.67	887.94	159.74
bandwidth	682	574	566	495	562	581
N	12,440	10,633	10,503	9,319	10,440	10,757
	(7) other revenues	(8) total expenditures	(9) capital expenditures	(10) current expenditures	(11) deficit	
placebo in 2008	-5.00	68.86	15.99	50.35	-13.81	
	(14.26)	(68.45)	(46.74)	(29.51)	(10.67)	
placebo in 2009	0.80	4.05	-11.83	15.61	-8.07	
	(13.18)	(66.38)	(49.27)	(25.06)	(7.50)	
placebo in 2010	7.22	53.77	17.43	23.39	-4.04	
	(11.87)	(70.00)	(51.36)	(25.62)	(7.02)	
placebo in 2011	24.51*	-32.43	-57.67	19.81	-2.69	
	(13.95)	(73.29)	(56.43)	(23.72)	(7.66)	
placebo in 2012	4.18	-22.16	-63.45	34.29	3.16	
	(13.59)	(78.65)	(62.00)	(23.45)	(10.11)	
mean	75.21	1307.39	501.62	784.42	19.49	
bandwidth	616	515	563	473	666	
N	11,349	12,258	12,258	12,258	12,339	

Notes: * p<0.1, ** p<0.05, *** p<0.01. This table displays placebo effects using equation 3.2. These are obtained by restricting the sample to pre-reform years, assigning the reform to a different year and finally re-estimating equation 3.2. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. All revenue, expenditure, and deficit variables are expressed in per capita terms and 2015 Euros.

Chapter 4

The Effects of Public Disclosure by Politicians

Joint with Carina Neisser.

4.1 Introduction

As in many other countries, German politicians are legally permitted to carry out outside activities in addition to their political work. Politicians engaging in activities other than their work in parliament remains a very controversial topic. On the one hand, there is doubt on whether elected representatives devote all their energy to their political duties and it also raises concerns of potential conflicts of interests (Akcigit, Baslandze, and Lotti, 2018). On the other hand, banning politicians from engaging in outside activities might negatively influence the selection of politicians (Fisman, Schulz, and Vig, 2019; Gagliarducci, Nannicini, and Naticchioni, 2010). A central concern of democratic countries is the degree to which voters can hold members of parliament (MPs) accountable (Djankov et al., 2010). Accountability heavily relies on availability of information about both parliamentary and non-parliamentary actions. One potential policy to inform voters on politician's outside activities are public disclosure laws.[1] If voters observe undesirable behavior, they can vote them out of office. This political pressure could cause politicians to change their behaviour. Despite being widely used, there is little causal evidence on the effects of public disclosure laws on outside activities and earnings. This is due to several reasons. First, it is hard to obtain high-quality data, especially before the introduction of disclosure rules, as politicians outside earnings are unobservable before the implementation of disclosure laws. Second, even the published (and thereby disclosed) data is often coarse and might be misreported. Finally, one has to find a suitable control group to establish a counterfactual scenario.

[1] According to Djankov et al. (2010), 109 countries around the world have some form of a disclosure law, roughly half of those make disclosed information public. They find suggestive evidence that *public* disclosure is associated with better government and perceived corruption.

This chapter aims to fill this gap and identify the causal effect of public disclosure of outside activities and their associated earnings on politicians' outside earnings. We overcome the existing problems by exploiting (i) two policy changes with respect to public disclosure laws in Germany and (ii) high-quality administrative tax return data giving rise to a difference-in-differences setup with German federal MPs as our treatment and state MPs as our control group. We exploit two reforms that differ in the degree of disclosure intensity. First, we use the introduction of a public disclosure law for federal MPs in Germany as a source of exogenous variation. In 2005, a law was passed that requires MPs to publish their outside activities and levels of outside earnings on the website of the German federal parliament (*Bundestag*) that are freely accessible to voters. Initially, disclosure was only private because a group of MPs filed a law suit against such public disclosure rules. In July 2007, the German constitutional court narrowly rejected the law suit, such that disclosure became public. Each activity is assigned an income bracket such that outside earnings were reported in a bracket system top-coded at 7,000€. The fact that information was top-coded was heavily debated in media and parliament and it raised concerns that voters were not adequately informed.[2] In 2013, our second reform under study, more brackets were introduced such that only earnings above 250,000€ were censored. This greatly increased disclosure obligations for MPs and the information available to voters.

We use administrative tax return data for 2001 to 2014 allowing us to observe politicians' outside earnings at a very precise level.[3] Our main outcome is the total amount of outside earnings. Another important feature of the tax data is that it allows us to use state MPs as a control group. Since state MPs were not subject to any disclosure rules during our sample period, we can use them to estimate a difference-in-difference model. German state and federal MPs are highly comparable. Both groups are full-time politicians and they are elected in a similar way. This comparability is underlined by the absence of any differential trend between treatment and control group prior to the reform.

To examine who responded to disclosure of outside activities and earnings, we use (i) different income categories as outcome variables and (ii) run quantile regressions to check for heterogeneous responses along the earnings distribution. On the one hand, voters perceive sources of outside earnings differently (Campbell and Cowley, 2015). On the other hand, the literature on behavioral responses towards taxation shows that the self-employed can more easily adjust their labor supply and also the reporting of their income (Saez, Slemrod, and Giertz, 2012). Therefore, we check for different effects between income from wages and salaries and income from self-employment and busi-

[2] During the campaign in the run-up of the 2013 federal elections, politician's outside activities were a much discussed issue because of large outside earnings of the candidate for the chancellorship, Peer Steinbrück.

[3] In general, tax data has very little amount of socio-demographic information and researchers face strict confidentiality rules. Importantly, we do not observe any names and we are not allowed to link any external data set to the tax data. Therefore, we cannot make statements about variables like party affiliation when using our tax data.

nesses. In addition, we use income from renting, an unaffected income category, as a placebo outcome. Given the differences in the bracket structure across both reforms, we expect heterogeneous responses across the earnings distribution. Since voters cannot distinguish between a moderate and a high-earning MP, the first reform might induce MPs to concentrate their earnings in the highest reporting level 3, which corresponds to income of 7,000€ or more, while the second reform and the associated changes in the bracket structure might discourage MPs to report activities with high levels of outside earnings.

On average, 89% of all federal MPs report an activity and 38% disclose positive outside earnings. The most common disclosed remunerated activities are working as a lawyer (10%), working in management and consulting (10%) or giving speeches (8%). Around 40% of all MPs hold a function in enterprises, either as being a member of the advisory or supervisory board. Using tax return data, we observe that the distribution of outside earnings is highly unequal following a pareto distribution.

Our results show that the introduction of public disclosure *increased* total outside earnings by 15.3%. The amount of MPs having positive outside earnings also increased by 4.5 percentage points. Quantile regressions show that the effect is mainly driven by the upper end of the earnings distribution. This points to the problem of the conservative top-coding of earnings at 7,000€. We show that the increase is mostly driven by income from self-employment and business income, which would be consistent with increased tax compliance as these incomes are self-reported and the public visibility of their incomes might have increased incentives to report income truthfully. However, the timing of the effect suggests that this mechanism is unlikely. We do not see any increase in earnings in two years of private disclosure even though MPs should have anticipated that there a significant chance of their disclosed activities becoming public retroactively. Other possible explanations for the increase include, for example, changing social norms regarding outside incomes, i.e. making outside incomes more normal and therefore, more acceptable.

The tightening of the disclosure law reform provides evidence that disclosure rules lead to a *lowering* of outside earnings. The introduction of seven new brackets allows to distinguish between medium and high-earning MPs. This leads to a reduction in outside incomes of 9.6%. This decrease is mainly driven by reductions in income from wages and salaries consistent with MPs working less for firms other than their own. Quantile regressions show that this decrease is particularly pronounced at the top of the distribution. This is consistent with top-earners being treated most intensely since the new brackets affected them the most.

We also make use of self-collected data on published earnings from the website of the German Bundestag which we combine with rich data on demographic and political variables. First, we examine the relationship between tighter disclosure rules and electoral accountability. Directly elected MPs had significantly lower outside earnings when compared to the runner-up in their election district, who joined via the party list, after, but not before the second reform. Similarly, MPs with an unsafe rank on the party list had lower outside earnings than MPs with a very safe rank after the second reform, while we could not find a difference before.

We contribute to several strands of the literature. To the best of our knowledge, we are the first examining public disclosure rules for politicians with administrative tax return data for a western democracy. More specifically, we test if individuals change their earnings and thereby the amount of outside activities in response to a mandatory disclosure of these activities along with the respective earnings. Most related, Slemrod, Rehman, and Waseem (2020) and Malik (2020) exploit an unexpected release of tax records of Pakistani politicians. In contrast to our study, their focus lies on tax evasion in a developing country. While Malik (2020) consider only MPs and provides strong evidence that the pressure to decrease tax evasion was highest for competitively and directly elected legislators, Slemrod, Rehman, and Waseem (2020) focus on the universe of tax filers and find a 9% increase in the tax paid by individuals that are exposed to public disclosure.

Second, our study contributes to a broader question of how a change in third party information requirements affects income reporting behavior and how public disclosure of income affects the (reported) income itself (Kleven, Kreiner, and Saez, 2016). The effects of income disclosure have been studied among others for the general population (Bo, Slemrod, and Thoresen, 2015; Slemrod, Rehman, and Waseem, 2020), CEOs (Mas, 2016), and public employees (Mas, 2017). Both Slemrod, Rehman, and Waseem (2020) and Bo, Slemrod, and Thoresen (2015) find that income disclosure leads to higher levels of tax compliance driven by shifting social norms and concern for reputation. Dwenger and Treber (2018) explicitly study whether public shaming increases tax compliance through social pressure. They exploit the introduction of a naming-and-shaming policy in Slovenia to show that taxpayers reduce their tax debt to avoid shaming. Perez Truglia and Troiano (2018) run a field experiment to study shaming by sending different letters to tax delinquents in the US. They find that increasing the visibility of the delinquency status increases compliance by individuals who owe less than 2,500$, while the effect on individuals with larger debt is negligible.[4]

Lastly, we contribute to the moonlighting literature, which investigates the relationship between politicians' outside earnings and parliamentary activity, quality and corruption. This literature shows that allowing moonlighting has ambiguous effects. On the one hand, it might attract more competent politicians, on the other hand these politicians are also more likely to shirk in office (Gagliarducci, Nannicini, and Naticchioni, 2010). Furthermore, politicians connected to private firms might hinder the process of creative destruction and thereby lower productivity (Akcigit, Baslandze, and Lotti, 2018). There are also two studies investigating the moonlighting of German MPs. Arnold, Kauder, and Potrafke (2014) show descriptively that (reported) outside earnings are not correlated with absence rates and speeches, but negatively correlated with oral contributions and group activities. Becker, Peichl, and Rincke (2009) find that politicians report less outside income if they face stronger political competition. However, no

[4] See Bursztyn and Jensen (2017) for a survey of the literature on social pressure and shaming effects.

existing study examines the effect of disclosure rules in a casual manner. Furthermore, we are the first who use administrative tax records to evaluate public disclosure rules affecting politicians.

The remainder of this chapter is structured as follows. In Section 4.2, we describe the institutional context and provide more details about the introduction of disclosure rules in 2007, the tightening of these rules in 2013 and briefly describe the German voting system. We describe our different data sources and provide descriptive statistics in Section 4.3. Section 4.4 outlines our empirical strategy for both reforms. In Section 4.5, we present our results both for the introduction and the tightening of the disclosure rules. Last, Section 4.6 concludes.

4.2 Institutional Context

4.2.1 Introduction of Disclosure Rules

Historical background In Germany, both federal and state member of parliament are legally permitted to carry out outside activities besides their political mandate, e.g. lawyers might continue to work within their profession. However, it is clearly stated in §44a of the Members of the Bundestag Act (*Abgeordnetengesetz*) that "the exercise of the mandate of a Member of the Bundestag shall be central to his or her activity". In late 2004, payments to federal MPs by large companies such as Siemens of Volkswagen became the focus of public attention. Subsequently, the German federal parliament passed a law in August 2005 that obliged MPs of the German Bundestag to publicly disclose their outside activities and associated earnings. The purpose of the disclosed information was to "indicate combinations of interests with implications for the exercise of the said mandate". The law was controversial and some MPs filed a lawsuit against it arguing that it would violate their privacy rights and the obligation to public disclosure makes it less attractive to run for office for citizens from certain occupations such as for example entrepreneurs.

Private and public disclosure Until the final decision of the Federal Supreme Court, the president of the German Bundestag (*Bundestagspräsident*) decided that outside activities and earnings would have to be privately disclosed to the administration of the Bundestag, but would not be publicly disclosed. In July 2007, the lawsuit was narrowly defeated by a tied court and MPs were forced to publish their sources and levels of outside earnings on the website of the German Bundestag retroactively. To conclude, starting in 2005 federal MPs privately disclose their information and from 2007 on all information was publicly disclosed.

Outside activities and associated earnings are published on the website of the German Bundestag. Table 4.2.1 summarizes the disclosure rules.[5] Disclosure obligations involve publication of (i) each outside activity, (ii) corresponding outside earnings per

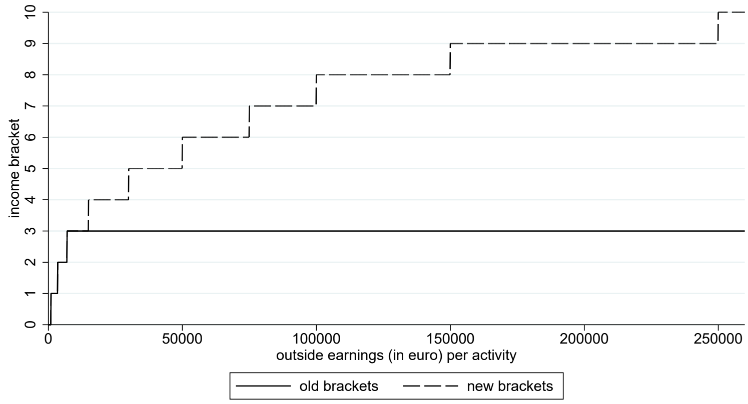
[5] The interested reader can find an English version of the Code of Conduct for Members of the German Bundestag online (Bundestag, 2013).

Table 4.2.1: General disclosure requirements

(A) Outside Activities	
remunerated activity during the term of the mandate	e.g. speech
functions in enterprises	e.g. supervisory board
functions in public corporations and institutions	e.g. board of trustees
functions in clubs, associations and foundations	e.g. development aid agency or foundations
shareholdings in private corporations or partnerships	e.g. law firm
(B1) Outside Earnings (EP 16 and 17)	
level 0	income up to 1,000€
level 1	income between 1,000€ and 3,500€
level 2	income between 3,500€ and 7,000€
level 3	income over 7,000€
(B2) Outside Earnings (EP 18)	
level 0	income up to 1,000€
level 1	income between 1,000€ and 3,500€
level 2	income between 3,500€ and 7,000€
level 3	income between 7,000€ and 15,000€
level 4	income between 15,000€ and 30,000€
level 5	income between 30,000€ and 50,000€
level 6	income between 50,000€ and 75,000€
level 7	income between 75,000€ and 100,000€
level 8	income between 100,000€ and 150,000€
level 9	income between 150,000€ and 250,000€
level 10	income over 250,000€
(C) Frequency and Time Frame	
once, monthly or yearly	starting and ending date
(D) Source	
company's name and location	

Notes: We ignore the information on donations. The name of lawyer's clients are not revealed due to existence of lawyer-client-confidentiality. Shareholdings in private corporations only need to be reported if a MP holds more than 25% and no information about received outside earnings needs to be provided (no information about level, frequency and time frame of the activity). For more details we refer to 'Code of Conduct for Members of the German Bundestag'. Reported earnings and activities are published on the website of the German Bundestag and in *Amtliches Handbuch*.

Figure 4.2.1: Visualization of both reforms and the underlying bracket structure



Notes: This figures visualizes the bracket structure of both reforms. The solid line refers to the first reform, where every activity that is remunerated with more than 7,000€ is is categorized as level 3. The dashed graph shows the bracket structure under the second reform and thereby the increase in disclosure of outside earnings to voters.

activity, (iii) its frequency and (iv) its source. Disclosed earnings are determined by the gross amounts paid, including expenses, compensations and the value of benefits in kind, while deductions are not included. Not all kinds of outside earnings need to be disclosed, for example stock options or shareholdings in private corporations, if they are lower than 25%, are exempt. In addition, activities with associated earnings of less than 1,000€ also need not be reported.

The amount of outside earnings are published in income levels. Earnings below 1,000€ are classified as level 0, those between 1,000€ and 3,500€ were referred to as level 1, outside earnings between 3,500€ and 7,000€ were called level 2, while level 3 described outside earnings of above 7,000€. In addition, the law required MPs to assign the respective source to each outside activity. Appendix Figure 4.A.1 shows a screenshot of the website of an MP. Top-coding at 7,000€ was criticized since MPs might cover their well-paid activities and declare it as level 3. Nevertheless, various watchdog organizations and the media made extensive use of the published data in subsequent years.

The enforcement of the law works as follows. Every MP has to submit all outside activities and associated income levels, time frame and frequency, and its source to the president of the German Bundestag within three months. These data are then published on the individual websites of the respective MP that are administered by the German Bundestag. If a MP misreports or does not report at all, the violation will be made public and a fine has to be paid. Sanctions include cuts in their enumeration of up to 50%. In addition, considerable cost of reputation is added to the monetary fine, since these cases are widely discussed in the media.[6]

4.2.2 Tightening of Disclosure Rules

Historical background In 2012, the former Minister of Finance Peer Steinbrück was nominated as candidate for chancellor in the upcoming federal election. Subsequently, it was pointed out by the media that he was the highest-earning member of parliament by giving a large number of highly-paid speeches.[7] Since most of his outside activities were top-censored, i.e. above 7,000€, his outside earnings were not appropriately reflected in the reporting scheme. This created a prolonged public debate about possible reforms of the reporting requirements throughout 2012 with Google searches spiking (see Figure 4.2.2). Using a digitized database of all parliamentary speeches, we also show that the use of the phrase "outside earnings" in speeches by federal MPs spikes in 2012 (see Figure 4.2.2). Following this debate, the federal parliament passed a stricter version of the disclosure law in March and came into force in September 2013. As MPs could already anticipate the tightening of disclosure law, we treat 2012 as the reform year for the second reform.

Tightening of disclosure rules The new law aimed to provide more detailed information on high-earning MPs. More specifically, seven new income categories were added to the reporting scheme, so that top-censoring occurred at 250,000€ instead of 7,000€. This makes it possible to distinguish between a MP earning moderate amounts and top-earners. Figure 4.2.1 visualizes the bracket structure of both reforms. The solid line refers to the first reform, where every activity that is remunerated with more than 7,000€ is is categorized as level 3. The dashed line shows the bracket structure under the new regime and thereby the increase in disclosure of outside earnings to voters. As a reference, federal MPs receive around 90,000€ as a yearly salary for their work as a politician across our period under study.

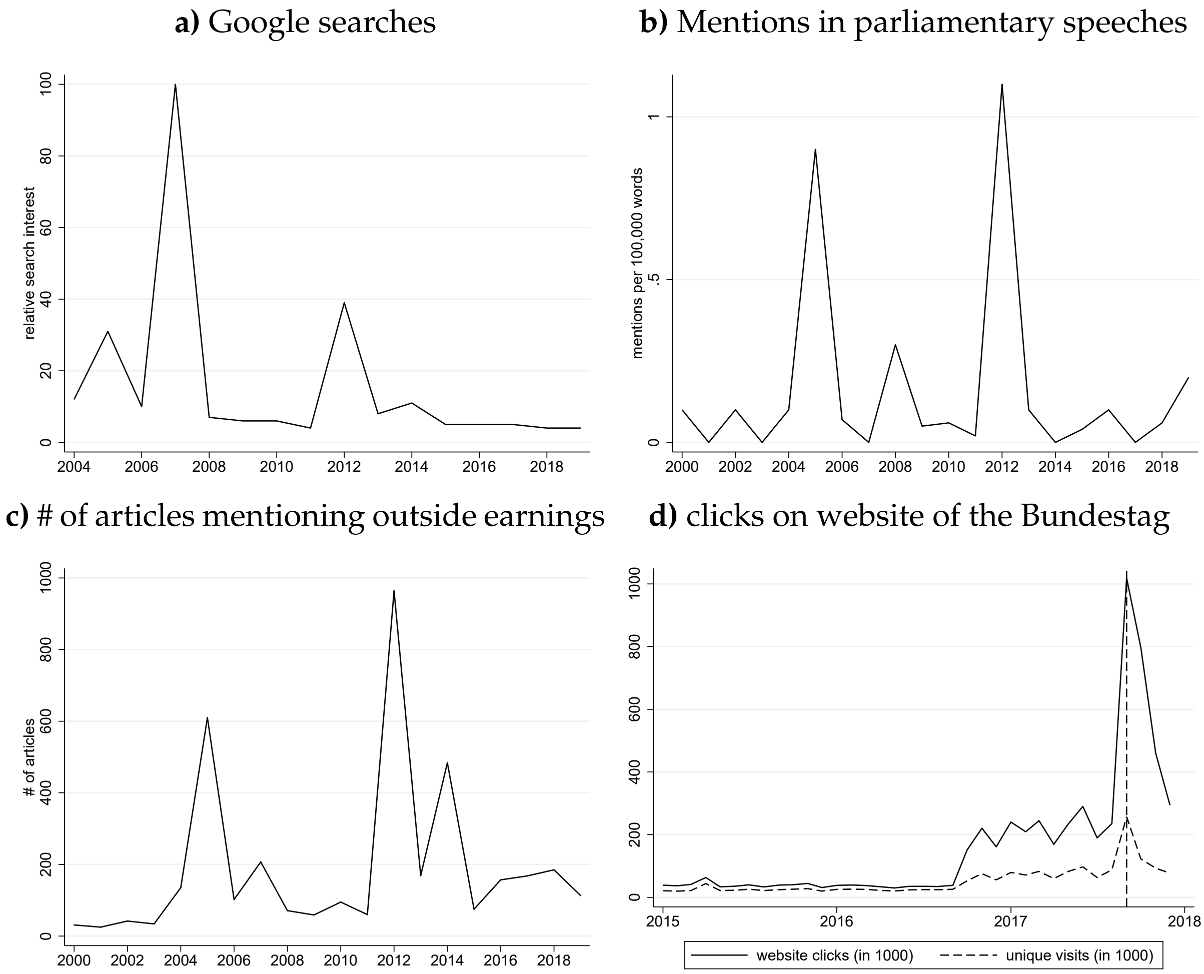
For the disclosure rules to be effective, there has to be sufficient attention paid to the reported earnings. This can either be archived through the media, which made extensive use of the reported earnings, or by citizens themselves. To test the first channel, we plot the number of articles mentioning politicians outside earnings found in the newspaper archive GENIOS from 2000 to 2019 in Figure 4.2.2c. One can clearly see the spikes in articles in 2005 and 2012 when the two big scandals happened. More generally, the number of articles clearly increased after MPs had to disclose their earnings. To test whether citizens themselves look up their MPs earnings, we obtain data on unique visitors and clicks on the website of the Bundestag where the earnings are reported.[8] As one can see in Figure 4.2.2d, the number of clicks and unique visitors increases one

[6] This has already happened twice, most notably to the former minister of the interior, Otto Schily, in 2008. As an attorney, he argued that the rule would violate his client's privacy rights. In the end, he had to pay a 22,000€ fine.

[7] There were even cases of him missing votes in parliament when giving a paid speech (Spiegel, 2010).

[8] Unfortunately, the data are only available from January 2015 to January 2018.

Figure 4.2.2: Interest in outside activities and earnings



Notes: Panel (a) plots the search interest relative to the highest point in the chart for the selected region in the specified time period. The value 100 stands for the highest popularity of this search term. Source: Google Trends; search term: 'Nebeneinkünfte' (engl: outside earnings); Search Period: 01.01.2004-31.12.2019 in Germany. Panel (b) plots the number of times outside earnings were mentioned in speeches held in parliament per 100,000 words. Source: Die Zeit. Panel (c) plots the number of articles mentioning outside earnings of politicians from the newspaper archive *GENIOS*. Panel (d) plots website clicks and unique visitors (in 1000) on the website of the German Bundestag from January 2015 to January 2018 on a monthly basis. The solid line indicate the federal election in September 2017. Source: Deutscher Bundestag (own freedom of information request of 18.11.2019).

year before the federal election in September 2017. There were 61.7 million eligible voters and 47.0 million voters, implying a turnout of 76.2%. In the month of the election, clicks spike at roughly 1,000,000 clicks and 200,000 unique visitors. Together with the large amount of newspaper articles documenting the existence of outside earnings and activities, we argue that sufficient attention was and still is paid to politicians' outside income.

4.2.3 Voting System in Germany

The German Bundestag is the federal parliament of the Federal Republic of Germany, while state parliaments (*Landtäge*) are the legislative bodies of the 16 individual German states. The competence of legislation is split between the state parliaments and the federal Parliament. Elections for the German Bundestag as well as for the state parliaments are based on a "personalized" proportional representation system. Its goal is to combine the advantages of both proportional representation and majority voting system. Each citizen has two votes. The first vote is directly attributed to a candidate representing her electoral district. As there are 299 federal electoral districts, the same number of mandates in the Bundestag are distributed to the candidates winning the plurality of first votes in their districts (directly elected candidates). The second vote supports a political party at the national level. Based on their share of the second vote, political parties send their candidates from predefined electoral lists into the federal parliament. The electoral lists are determined by the parties at the state level. This way 299 additional mandates are distributed to the parties who have received at least 5 percent of the valid second votes.[9] The Bundestag is elected for four years, while state-level elections are held every five years.

In our analysis, we will distinguish between MPs that are directly elected and those who entered parliament through the party list. In particular, directly elected MPs should face a higher level of electoral accountability since voters have the possibility to punish (or reward) them directly given their published information on outside earnings and activities. Furthermore, we will compare MPs with a safe ranking on the electoral list to those with a more insecure ranking. Again, the less secure the rank is, the higher the degree of electoral accountability should be.

4.3 Data

We employ the German Taxpayer Panel for the years 2001 to 2014 (henceforth called *TPP*), which comprises the universe of German tax returns. In addition, we collect publicly disclosed outside activities and earnings for the years 2005 to 2017 as well as pub-

[9] If a party receives more mandates via the first vote than the second vote, all directly elected candidates gain additional seats in the Bundestag (*Überhangmandate*). To keep proportional representation intact, parties whose share of candidates lies below their share of second votes are also given additional seats (*Ausgleichsmandate*).

licly available information on demographics, committee membership and voting statistics (henceforth called *reported data*). The two data sets have distinct advantages and drawbacks. The TPP allows us to precisely measure outside income before *and* after the reforms both for federal *and* state MPs. This allows us to causally evaluate the reforms in a difference-in-difference setting. The main drawback of the TPP is the low number of demographic and political variables. Given the strict data protection rules when working with tax return records, we cannot identify individuals' names or party affiliations. In contrast to the tax return data, our reported data offers a rich set of demographic and political variables, but the publicly disclosed information on earnings are imprecisely measured. Given the nature of the reported data, we can only observe federal MPs after the reform and state MPs are not covered at all. We use the reported data to provide some suggestive evidence on the characteristics of outside activities and demographics, but also to support potential mechanisms. Importantly, we are not allowed to combine these two data sets and both will be evaluated separately.

4.3.1 German Taxpayer Panel

The German Taxpayer Panel (TPP) covers all tax units for the period 2001 – 2014. It is an administrative data set collected by German tax authorities, provided and administered by the German Federal Statistical Office. The unit of observation is a tax unit, i.e., either a single individual or a couple filing jointly. It contains all information necessary to calculate a taxpayer's annual income tax, including basic socio-demographic characteristics such as age, gender, state of residence, marital status, as well as detailed information on income sources and tax base parameters such as work related expenses and (claimed and realized) deductions on a yearly level. Hence, the advantage of tax return data lies in its precise measurement of pre- as well as post-reform income related variables. However, it does not contain of information about the specific type of outside activity (e.g. speech or ongoing work as a lawyer) or personal information (e.g. party affiliation).[10]

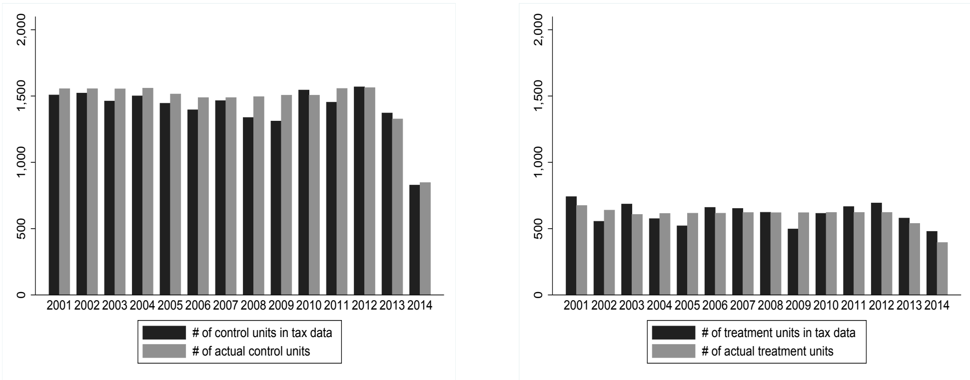
Treatment and control group Our empirical strategy compares federal MPs (treatment group) to state MPs (control group). Now, we outline how we determine the two groups in the TPP. First, we identify all members of federal, state, and EU parliament by having positive income from parliamentary activities. Next, we gather data on the remuneration and election dates of all 16 state parliaments as well as well the federal and European parliament from 2001 to 2014.[11] Since state MPs earn less than federal MPs, we can discriminate between the two groups within state-year cells. Until 2009, members of the European parliament received the same amount of remuneration as federal MPs. To identify those units we exploit an increase in their compensation

[10] Data access is subject to very strict data security rules and we only work with these data via remote-access. Every single request requires a confidentiality check. Moreover, it is impossible to combine these data with any other information.

[11] Appendix Figure 4.A.2 plots the average remuneration for the federal, EU and all state parliaments over our sample period.

Figure 4.3.1: Comparison between tax data and actual numbers

a) control units **b)** treatment units



Notes: Panel (a) shows the number of state MPs that are identified in the tax data (in black) and the actual or expected number of MPs (in grey). Panel (b) shows the number of federal MPs that are identified in the tax data (in black) and the actual number of MPs (in grey). We exclude parliamentarians from Berlin, Hamburg, and Bremen for both groups. We further exclude those units that newly enter parliament and those who leave parliament in a given year. Hence, in our baseline estimations, we only consider 'full year' units such that our results do not get contaminated by e.g. individuals directly entering employment right after leaving parliament. Source: German tax return data, 2001-2014 (Taxpayer Panel, TPP)

in 2009 due to a EU-wide harmonization of their salaries. Hence, we drop observations whose income from parliamentary activities discontinuously jumps in 2009 by the reform-induced amount.[12] Further, we drop households, in which both the head and the spouse are MPs since they could be part of both the treatment and the control group.[13] Next, we exploit the panel structure of our data to exclude individuals who just entered parliament for a given year, since we would wrongly classify their pre-politician earnings as outside earnings. MPs leaving parliament receive a transitional payment (*Übergangsgeld*). We make use of the fact that (i) most MPs leave parliament after elections, and (ii) the transitional payment is lower than the regular salary. This allows us to pinpoint MPs whose income from parliamentary activities drops right after a state or federal election. We classify these MPs as dropouts.[14] As a robustness check, we will report results both with and without dropouts. Finally, we drop all MPs from the three German city-states (Berlin, Hamburg and Bremen) since being an MP is only a part-time job in their state parliaments (so-called *Feierabendparlamente*).

[12] We can identify about two thirds of the 99 EU parliamentarians since one third newly enters the European parliament and is therefore indistinguishably from newly entering federal MPs. Note, that this induces a bias towards zero since a (small) part of the treatment group is not actually treated. Over our sample period there were no changes with respect to income disclosure for members of the European parliament.

[13] This involves only a very small number of couples in our sample period. Including them does not change our results.

[14] Federal MPs receive one additional month of transitional payments for each year they spend in parliament. The transitional payments are capped at 18 months. Starting with the second month after leaving parliament, transitional payments are reduced one to one by all other income a former MP receives.

In 2013, Bavaria was the first state that introduced a public disclosure law for its state MPs. One year later, five further states introduced similar laws (see Table 4.A.7). Therefore, we exclude observations from these states when disclosure laws where in effect to avoid a contamination of our control group. In Figure 4.3.1, we verify the accuracy of our allocation mechanism and compare the amount of units identified in the tax data with the actual number of units that are present in parliament. We match the number of state and federal parliamentarians quite closely.

Outcome variables We capture disclosed outside earnings as closely as possible. We take advantage of the fact that earnings from different sources are separately reported in the German income tax system. Our main outcome is the total income from sources that MPs have to disclose. This amounts to all income from (i) salaries and wages (ii) (non-corporate) businesses and self-employment (iii) agriculture and forestry, as well as other sources. We will also evaluate the effect on each of the categories (i) to (iii) separately. Furthermore, we use rental income as a placebo outcome since such income does not need to be disclosed.[15]

4.3.2 Reported Data

Table 4.3.1: Number of MPs with at least one activity and positive outside earnings

	EP16		EP17		EP18		Total	
	N	in %	N	in %	N	in %	N	in %
MPs who report at least one activity	573	89.81	581	89.11	582	88.45	1736	89.12
MPs with positive outside earnings	241	37.77	250	38.34	252	38.30	743	38.14

Notes: This table provides an overview about federal MPs who report outside activities and who report outside earnings for the election periods 16-18 and the average across all three election periods. All percentages refer to the total amount of MPs for a given election period. Source: Reported Data, own calculations.

Our second data set consists of several publicly available sources. The most important part of this data are the reported outside earnings and activities from the website of the German federal parliament. We enrich this data with further demographic and political variables. Our reported data covers every MP who was at least present in one of the following three legislative periods of the German Bundestag: 16th legislative period (2005-2009), 17th legislative period (2009-2013) and 18th legislative period (2013-2017).[16] In the following, we describe the different data sources in greater detail.

Demographic variables Using the handbook of German MPs, we extract a number of demographic variables. We observe a politician's name, gender, age, marital status, and number of children. Additionally, we know whether a politician has a PhD degree

[15] We do not consider capital income in our analysis, since MPs were not required to disclose such earnings and investment income is only observable until 2009 in the tax data.

[16] Table 4.A.2 provides an overview about these three election periods under study as well as the composition of MPs in federal parliament by party.

and their resident state. We classify a politician's (former) occupation into ten groups. Importantly, as opposed to the tax data, we know the party membership of each MP. For our sample period about half of MPs are part of a center-right party (CDU/CSU and FDP), while the other half is a member of one of the left-wing parties (SPD, Greens and The Left). Moreover, we group MPs by their political experience into three categories: newcomers (first term), those serving for two to three terms, and MPs with four or more terms in parliament. Lastly, we construct dummies for MPs that leave (or join) parliament in the middle of an election period since they have less time to accumulate outside earnings. Summary statistics of all these variables can be found in Appendix Table 4.A.3.

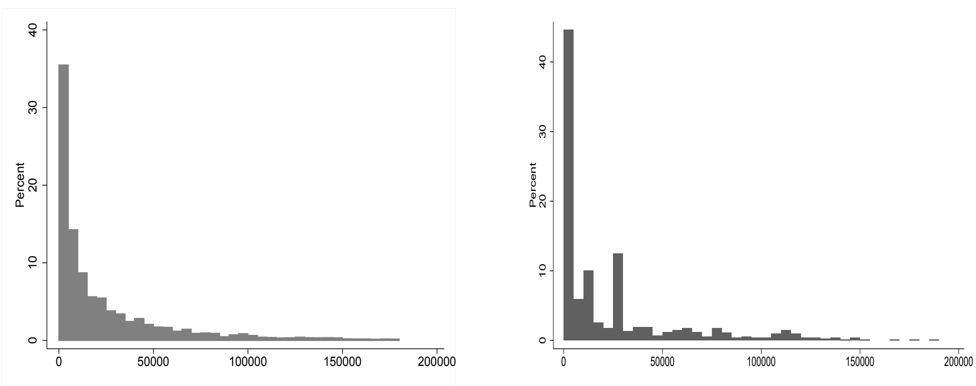
Political and electoral variables A MP can be voted into the *Bundestag* either via party list or direct ballot (see Section 4.2.3). To capture this distinction, we construct a dummy for being elected directly. We also create a dummy for MPs who entered through a safe rank on the party list (above-median ranking) as opposed to those that where placed on a less safe rank (below-median ranking). Furthermore, when a MP ran for direct ballot in one of the 299 electoral districts, we obtain her own as well as her party's vote share in that district. Then, we calculate the vote margin of each MP as the difference to the second-placed candidate for winning candidate and the difference to the first placed candidate for all other candidates. To account for political offices and to capture a politician's policy expertise and interest more accurately, we construct dummies for membership in one each of the committees of the German federal parliament. In addition, to capture the rank and status of the MP, we create dummies for being part of party leadership and for being a committee chair, respectively. Summary statistics are again displayed in Appendix Table 4.A.4.

Published data on outside earnings We collect every disclosed activity, its income level (0 to 3 for election period 16 & 17 and 0 to 10 for election period 18), its starting and end date as well as frequency (monthly, yearly, once), and the respective employer. Table 4.3.1 provides information about the number of MPs with at least one activity and positive outside earnings. 89.12% of all MPs report an activity and 38.14% report positive outside earnings. This is due to the fact that many activities are voluntary work and thus not remunerated. In Appendix Table 4.A.6, we display the distribution of each activity's bracket and frequency. 18% of all activities are assigned level three of higher across all election periods. 94% of all activities are carried out once and only 2% and 4% of all activities happen on a yearly or monthly basis.

To determine a value of outside earnings, we assign the mean value of each bracket to every activity (e.g. an activity with level 0 is measured with 500€). The value assigned to the last bracket is determined by polynomial extrapolation, i.e. an activity with level 3 is assigned 9,500€ (see Appendix Figure 4.A.3). Since the addition of seven new levels in election period 18 mechanically increases this measure, we code every activity of level 4 or higher as a level 3 activity. More precisely, an activity with level 0 is assigned a value of 500€ , level 1 2,250€ , level 2 5,250€ and level 3 and above

Figure 4.3.2: Distribution of outside earnings

a) tax data **b)** reported data: baseline



Notes: Panel (a) displays the distribution of (positive) outside earnings from federal parliamentarians excluding the top 2% for privacy reasons based on the tax return data. Panel (b) shows the corresponding distribution for the baseline measure of outside earnings based on the reported data. Source: German tax return data, 2001-2014 (Taxpayer Panel, TPP) (Panel (a)); Reported Data EP 16 - 18 (Panel (b))

9,500€ .[17] This is likely to underestimate the true level of outside earnings, but ensures comparability over time. In a last step, we calculate the total amount of reported outside earnings of every federal MP for a given election period and divide it by four to ensure comparability to the yearly tax data.

Published data on outside activities The composition of the main activities that MPs undertake are displayed in Appendix Table 4.A.5. 32% pursue a remunerated activity, 40% hold functions in enterprises and 59% hold functions in public corporations. The most popular remunerated activities are classified as law (10% of all MPs report at least one law activity), 10% of all MPs have at least one management and consulting activity and 9% were giving at least one speech. Typical functions in enterprises are member of advisory board (*Mitglied des Beirates*) or member of supervisory board (*Mitglied des Aufsichtsrates*). 11% of all MPs report shareholdings in private corporations with a share larger than 25%, but we cannot observe their income from these shareholdings.

4.3.3 Descriptive Analysis: Outside Earnings

The reported data consists of 1,952 MP-election period observations and covers election period 16-18 of the German Bundestag. We observe 1,108 individual MPs, 264 of which are present throughout all election periods.[18]

[17] As a robustness check, we also use a lower bound measure, where we we assign the lower threshold of 7,000€ to level 3 (and above) activities.

[18] We provide details of the composition of the German Bundestag for the election periods under study in the Appendix.

Table 4.3.2: Descriptive statistics: outside earnings (reported data & tax data)

	mean	sd	min	max	N
tax data					
all MPs					
outside earnings	29,358	146,151			27,974
wages & salaries	14,633	136,463			27,974
business & self-employment	11,762	113,943			27,974
renting	-986	17,880			27,974
other sources	2,963	15,770			27,974
federal MPs					
outside earnings	21,546	75,968			8,537
wages & salaries	8,230	42,613			8,537
business & self-employment	10,390	59,358			8,537
renting	-1,830	14,363			8,537
other sources	2,926	16,702			8,537
state MPs					
outside earnings	32,789	167,837			19,437
wages & salaries	17,445	161,184			19,437
business & self-employment	12,364	130,909			19,437
renting	-616	19,212			19,437
other sources	2,980	15,344			19,437
reported data					
federal MPs					
outside earnings: baseline	9,677	26,957	0	251,875	1,952
outside earnings: lower bound	8,478	23,205	0	227,562	1,952

Notes: Both panels refer to yearly values. The upper panel reports earnings based on the German tax return data, 2001-2014 (Taxpayer Panel, TPP). Outside earnings amounts to all income from (i) salaries and wages, (ii) business and self-employment income and (iii) other sources (except for income from parliamentary activities). Income from renting is our placebo outcome. Due to privacy reasons minimum and maximum values are omitted in the tax return data. In out reported data, outside earnings are calculated as follows: *baseline*: an activity with level 0 is assigned a value of 500€ , level 1 2,250€ , level 2 5,250€ and level 3 and above 9,500€ . In our *lower bound* definition, we assign a value of 7,000€ for each activity with level 3 and above. Source: Outside earnings are based on reported data for the election periods 16, 17 and 18 (lower panel);

Outside earnings Figure 4.3.2 plots the distribution of federal MPs outside earnings both from the reported data as well as from the tax data. Outside earnings is extremely unequally distributed in both data sets. The outside earnings from the tax data closely traces a pareto distribution, while the reported distribution exhibits bunching at different points. Between these bunching points, one can see the missing mass that is caused by the bracket reporting system. In our tax data, half of those MPs who do have positive earnings, have less than 10,000€ and around 30% have more than 30,000€ across the

Table 4.3.3: Outside earnings: correlations

	(1) outside earnings	(2) outside earnings	(3) outside earnings	(4) outside earnings	(5) outside earnings	(6) outside earnings
left-wing	-7,408***					-3,624***
	(1,488)					(1,402)
female		-7,267***				-3,815***
		(1,429)				(1,302)
East Germany			-5,987***			-6,755***
			(1,375)			(1,486)
age between 50 and 60				1,307		-122
				(1,380)		(1,438)
age 60 above				3,191*		1,188
				(1,919)		(1,988)
terms: 2 - 3					1,270	84
					(1,272)	(1,429)
terms: >3					2,069	-1,882
					(1,703)	(2,127)
controls						Yes
N	1,952	1,952	1,952	1,952	1,952	1,952
# politicians	1,108	1,108	1,108	1,108	1,108	1,108

Notes: The outcome variable is outside earnings as described in Section 4.3.2. SPD, Greens and The Left are coded as left-wing (parties). Controls include all variables in Tables 4.A.3 and 4.A.4 for which we have full observations. Robust standard errors clustered at the individual level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Source: Reported data for EP 16 - 18 (2005–2017).

period under study. Next, we compare the outside earnings that were publicly disclosed with the actual outside earnings that we can observe in the tax data.

Federal MPs have on average 21,000€ of outside earnings, while state MPs earn on average 32,000€ (see Table 4.3.2). This difference might be surprising since the focus of the political debate is usually on federal MPs. Possible explanations might be the lower public attention placed on state MPs or simply because they still have a closer relationship to their hometown and thereby their initial occupation. The major income source is business and self-employment income for federal MPs, while state MPs earn (on average) the most from wages and salaries. The mean in the reported data is around 10,000€. The values reported in the tax data are almost twice as high as our baseline measure from the reported data. This confirms one frequent criticism of the public disclosure law. The bracket system, and in particular the highest bracket, mask the real extent of moonlighting that politicians engage in.

Correlations We classify SPD, Greens and the Left Party as left-wing parties and show that they earn less compared to members of other parties, a result often found in the existing literature (Becker, Peichl, and Rincke, 2009; Eggers and Hainmueller, 2009). Table 4.3.3 shows that the unconditional difference amounts to about 7,400€ per year. This difference shrinks to 3,600€ when including all control variables, such as for example their former occupation, but is still statistically significant and of a economically meaningful size. Furthermore, in our sample both female and East German MPs earn significantly less outside earnings. Meanwhile, there is no significant difference by age and experience once we control for all other variables.[19]

[19] Appendix Figure 4.A.4 shows that there is also substantial variation in outside earnings by committee membership. MPs in the economics, agriculture and exterior committee earn on average over 13,000€, while members of the environmental and digital committee earn 3,000€ and less.

4.4 Empirical Strategy

In this section, we outline our empirical strategy. First, we describe both our simple difference-in-differences setting and our identification strategy. Furthermore, we extend our model to a dynamic difference-in-difference strategy. Second, to analyze who particularly responded to disclosure of outside earnings and activities, we run a quantile regression approach and we use different income categories as outcome variables. Last, we explore the mechanisms behind our results using the reported data by comparing MPs with different levels of electoral accountability.

4.4.1 Difference-in-Differences Strategy

Since 2005, federal MPs were obliged to privately disclose their outside activities and earnings. From 2007 onwards, the information was publicly disclosed, including the prior privately disclosed information. We exploit the fact that members of the federal parliament (*Bundestag*) were affected by disclosure rules, while members of state parliaments (*Landtag*) did not face such legal requirements. Thus, members of the federal parliament are our treatment units and members of state parliaments form our control group. This setup gives rise to a difference-in-difference design by comparing federal to state MPs before and after the reform. This identification strategy will uncover the casual effect of the public disclosure law if the assumption of parallel trends between the treatment and control group holds. We implicitly validate this assumption using a dynamic difference-in-difference approach.

Our baseline estimation is structured as follows. Let Y_{ist} be an outcome of politician i resident in state s in year t. We then estimate

$$Y_{it} = \beta Treat_i Reform_t + \gamma_i + \lambda_{st} + \epsilon_{it} \tag{4.1}$$

where $Treat_i$ is a dummy taking the value one if i is a federal MP and $Reform_t$ is an indicator equal to 1 from 2007 onwards. We also include individual fixed effects γ_i to control for potentially unobserved and time-constant features of MPs. The state-year fixed effects λ_{st} absorb aggregate movements as well as state-specific shocks such as local economic conditions. Finally, we cluster our standard errors at the individual level to allow for serial correlation. The coefficient of interest is β, which identifies the casual effect of the public disclosure law. Our sample period runs from 2001 to 2009 for the first reform. Note that, since this is classical 2x2 difference-in-difference setup, we do not have to assume homogeneous treatment effects for our estimator to be consistent (Goodman-Bacon, 2021).

We evaluate the tightening of the public disclosure law in much the same manner as its introduction with one exception. We drop observations in which state MPs were also subject to disclosure rules (see Section 4.3.1). Next, we estimate equation 4.1 on the sample from 2010 to 2014 with the reform dummy being one for $t \geq 2012$. Standard errors are again clustered on the individual level.

Dynamic difference-in-difference As mentioned above, we also estimate a more dynamic version of equation 4.1 both to test for pre-trends and to allow for dynamic post-treatment effects. To do so, we define a set of dummy variables $\mathbb{1}_{k=t}$, which takes the value one if k equals t and zero otherwise. To estimate the effects of the introduction of public disclosure rules, we run the following equation.

$$Y_{it} = \sum_{k=2001}^{2005} \beta_k Treat_i \mathbb{1}_{k=t} + \sum_{l=2007}^{2009} \beta_l Treat_i \mathbb{1}_{l=t} + \gamma_i + \lambda_{st} + \epsilon_{it} \tag{4.2}$$

where we omit the interaction of the 2006 dummy to normalize our estimates to the pre-reform year. Therefore, $\beta_k \ \forall k \in \{2001, ..., 2005\}$ refer to differences in trends between the treatment and control group before the reform, while $\beta_l \ \forall l \in \{2007, ..., 2009\}$ represent the dynamic treatment effects.

Analogous to equation 4.2, we adjust the dynamic difference-in-difference equation such that we check for pre- and post-treatment effects for the second reform:

$$Y_{it} = \sum_{k=2010}^{2010} \beta_k Treat_i \mathbb{1}_{k=t} + \sum_{l=2012}^{2014} \beta_l Treat_i \mathbb{1}_{l=t} + \gamma_i + \lambda_{st} + \epsilon_{it} \tag{4.3}$$

where we omit the interaction of the 2011 dummy to normalize our estimates to the pre-reform year. Again, β_{2010} refers to differences in trends between the treatment and control group before the reform, while $\beta_l \ \forall l \in \{2012, ..., 2014\}$ represent the dynamic treatment effects.

4.4.2 Who responds to the Disclosure of Outside Earnings and why?

Increased transparency makes politicians more accountable. In which way politicians adjust their earnings depend on the preferences of voters. If voters perceive outside income negatively, increased transparency could make politicians more accountable such that they reduce outside activities. We discuss direct ways to test for the effect of electoral accountability in the reported data in Section 4.4.3.

Income components Income disclosure by politicians might have counteracting effects on different categories of outside income. On the one hand, the effect depends on the preferences of voters on incomes from different sources. For example, Campbell and Cowley (2015) show via a survey experiment that voters do not penalize business owners or the self-employed for continuing their business. On the other hand, the literature on behavioral responses towards taxation shows that the self-employed can more easily adjust their labor supply and also the reporting of their income (Saez, Slemrod, and Giertz, 2012). Another possible behavioral effect can occur if income disclosure affects tax compliance. By increasing the possibility to detect evasion behaviour, income disclosure laws incentives tax payers to declare their true income (Bo, Slemrod, and Thoresen, 2015; Slemrod, Rehman, and Waseem, 2020). Given strict third-party reporting standards in Germany, we expect this possible effect only to be present for income from business operations and self-employment, since these income categories are self-declared by the tax payer. Both of these effects should (at least partially) materialize already in 2005 when private disclosure was applied and politicians had to assume that there is a decent chance for public disclosure to be applied retroactively. In contrast, if the effect is only observed from 2007, it is more likely that it is connected to the information that was publicly released.

Social norms towards having outside work might have changed after the introduction of the public disclosure law. Initially, the very conservative top-coding at 7,000€, has prevented voters to distinguish between a high- and moderate-earning MP and might have lead voters to underestimate the true extent of outside earnings. Therefore, from a voter's point of view it might have become more acceptable to have a second job as a politician. The second reform, which introduced more brackets and thereby increased the amount of information available to voters, however, could have had the opposite effect. In response, politicians might then reduce the amount of outside income. Public disclosure could also have changed a previous social norm of not pursuing outside activities among MPs to a market transaction by putting a price on it (Gneezy and Rustichini, 2000).[20] Given that MPs are paying a price, which is the reporting requirement itself, they might engage in more outside work. Moreover, politicians might have misperceived social norms and learned from the behavior of their peers, which causes them to update their beliefs about the acceptability of outside earnings (Bursztyn, Gonzalez, and Yanagizawa Drott, 2020). Last, the reported income could also be used as a signal of skill to (certain) voters. This could be potentially heterogeneous with some MPs wanting to highlight the importance of their mandate by having no outside jobs, while others explicitly start to have outside jobs to signal competence.

Quantile regression As already seen in Figure 4.3.2, outside earnings of politicians are highly unequally distributed. To shed light into the full distribution of outside earnings, we use (unconditional) quantile regressions. Whereas ordinary least squares regressions allow us to estimate the effect of a given variable at the mean, quantile regressions tell us about the effect of a policy change on the entire distribution of outside earnings.

We apply the estimator suggested by Firpo, Fortin, and Lemieux (2009) to estimate the effect of the reform on all nine deciles of the outside earnings distribution. We apply this estimator to both data periods: 2001 – 2009 (first reform) and 2010 – 2014 (second reform). The results are particular interesting for the second reform, since it has changed only the bracket structure. More precisely, until 2012 every activity that was remunerated with more than 7,000€ was top-coded and appeared as level 3 on the web pages of the German Bundestag. After the tightening of the rules, activities that are remunerated with more than 250,000€ are top-coded. Therefore, we expect most of the effect to be concentrated at the top of the distribution.

[20] This is also connected to the concept of moral licensing, where an individual, after doing something perceived as morally good, i.e. a politician being transparent about their outside earnings, it gives herself license to do something that is perceived to be morally bad, i.e. increasing her outside earnings (Merritt, Effron, and Monin, 2010).

4.4.3 Mechanisms: Electoral Accountability

To further investigate the mechanism of the reform, we look at variation in electoral accountability. As explained in Section 4.2.3, we exploit the fact that there are two ways to become a federal MP in Germany: direct ballot election and party lists. Since it is impossible to differentiate between the two groups of MPs in the tax data, we will test this hypothesis using the reported data. As we do not have a control group in this data set, all evidence has to be considered suggestive.

Election via direct ballot or party list Politicians, who enter parliament by direct ballot election, are arguably more accountable to voters. In case for any perceived misbehaviour, voters have the opportunity to directly vote specific politicians out of office. In contrast, voters cannot (directly) vote out specific politicians that enter trough the party list. Therefore, directly elected MPs are more electorally accountable and should react more strongly to the reform if electoral accountability matters. We test the prediction by looking at the subset of electoral districts, from which the second-placed candidate also entered parliament (through the party list). This allows us to compare directly elected MPs to their runner-ups in the following way:

$$Y_{ie} = \beta_e D_{ie}^{direct} + \delta X_{ie} + \gamma_d + \epsilon_{ide} \quad \forall\ e \in \{16, 17, 18\} \tag{4.4}$$

where Y_{ie} are outside earnings for MP i in election period e. D_{ie}^{direct} is a dummy for being directly elected, and γ_d are electoral district fixed effects ensuring that we only compare first-placed candidates to their runner-ups. We estimate this equation both for the two election periods before the second reform and for the election period after the second reform.

We expect β_e to be negative for all election periods, since they are subject to a higher level of electoral accountability. If the tightening of the disclosure rules, which went into effect, in election period 18, increased electoral accountability, directly elected MPs should reduce their outside income relative to MPs entering parliament through the party list. That is, we expect β_e to be even more negative in election period 18.

Safe and unsafe ranking on party list In contrast, MPs entering parliament via party list are only at risk to be voted out of office if they are close to the marginal rank, meaning the last rank which gets into parliament. Therefore, we also compare MPs with a safe list rank to those with an unsafe rank. Given the higher risk of being voted out of office for MPs with an unsafe rank, we argue that they are subject to a higher level of electoral accountability. Since party lists are organized at the state-party level, we construct a dummy $D_{ie}^{unsaferank}$ that takes the value one if a politician has an above median rank. For example, 22 politicians entered through the list of the Bavarian Social Democrats in election period 18. According to our classification, those ranked 1 to 11 had safe list ranks, whereas ranks 12 to 22 were unsafe. We then estimate the following equation:

$$Y_{ie} = \beta_e D_{ie}^{unsaferank} + \delta X_{ie} + \gamma_{sp} + \epsilon_{ie} \quad \forall\ e \in \{16, 17, 18\} \tag{4.5}$$

where Y_{ie} are outside earnings for MP i in election period e. γ_{sp} are state-party fixed effects controlling for the (potentially) different assignment procedures of the state-level party associations. Similar to above, β_e should generally be negative and become even more negative in election period 18 if electoral accountability plays a mediating role.

4.5 Results

4.5.1 Introduction of the Public Disclosure Law

Baseline results We first present the results from our baseline difference-in-difference approach. Table 4.5.1 shows the causal effects of the introduction of disclosure laws. Outside earnings did actually *increase* by about 15%. Also, the probability of having positive outside income increased by 4.5 percentage points. Both of these effect are statistically significant at conventional levels. One potential concern is that we include politicians who just dropped out of parliament in our sample conflating outside earnings with their regular income. To test this possibility, we exclude these MPs from our sample (see column (2) and (4) in Table 4.5.1). This leaves our estimates almost unchanged.

Figure 4.5.1 visualizes the estimates of our dynamic difference in differences approach (see equation 4.2). The effect only emerges after the introduction of public disclosure in 2007. Importantly, there is no evidence for any significant differential trend between the treatment and control group before the reform. This is reinforcing the parallel trends assumption underlying our research design. In addition, we do not observe any differential trend in the time period of private disclosure from 2005 to 2006. Politicians are only reacting to *public*, but not to *private* disclosure. The effect in 2007 is positive, but insignificant. In the following years, the effect becomes stronger and significant at conventional levels.

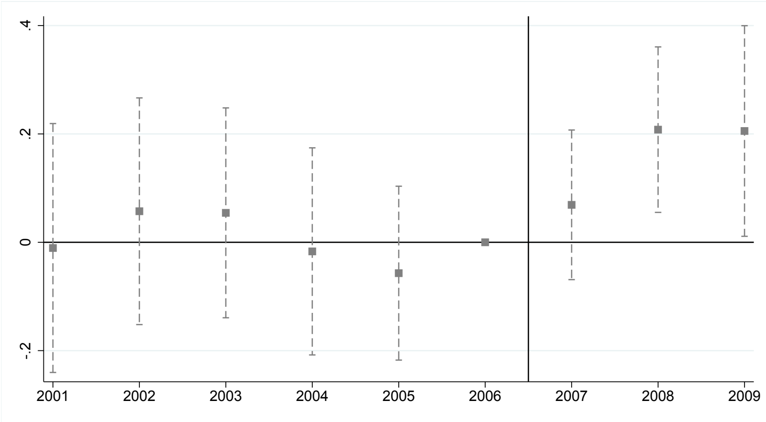
Income components To disentangle the total effect of an increase in outside earnings, we apply our baseline difference-in-difference setup to different income categories. Table 4.5.2 shows the results for wages & salaries (columns 1 and 2), business & self-employment (columns 3 and 4), other sources (columns 5 and 6) and last, renting as our

Table 4.5.1: Introduction of the disclosure law: extensive and intensive margin

	(1) log outside income	(2) log outside income	(3) outside income > 0	(4) outside income > 0
treatment x reform	0.155**	0.153**	0.049***	0.045***
	(0.064)	(0.066)	(0.017)	(0.017)
politician FE	Yes	Yes	Yes	Yes
state-year FE	Yes	Yes	Yes	Yes
w/o dropouts		Yes		Yes
N	14,135	12,955	19,993	18,412
# politicians	3,189	3,013	3,652	3,546

Notes: This tables displays estimates from equation 4.1 using log outside earnings (columns 1 & 2) and a dummy for positive outside earnings (columns 3 & 4) as outcome variables. Robust standard errors are clustered at the individual level. * p<0.1, ** p<0.05, *** p<0.01. Source: German tax return data, 2001-2009 (Taxpayer Panel, TPP)

Figure 4.5.1: Introduction of the disclosure law: dynamic difference-in-difference



Notes: This graphs displays the coefficients $\beta_t \ \forall \ t \in \{2001, ..., 2009\}$ and the corresponding 95% confidence intervals estimated by equation 4.2 using outside earnings as the outcome variable. Robust standard errors are clustered at the individual level.
Source: German tax return data, 2001-2009 (Taxpayer Panel, TPP)

placebo outcome (columns 7 and 8). The results show that the increase is solely driven by income from business and self-employment, which increased by 19.3% at the intensive margin and 3.7 percentage points at the extensive margin. All other coefficients are insignificant. Lastly, rental income, which was not affected by the disclosure law, does also not react to the reform. This increased credibility of that the measured effect is solely driven by the introduction of the disclosure law and not by some other shock occurring at the same time.

Next, we discuss the possibility that an increase in tax compliance might be the explanation for the positive effect on outside earnings. We argue that the timing of the effect is not consistent with this explanation. If politicians were concerned about being caught evading taxes, they should have already reacted in 2005, when private disclosure was introduced. Since it was known that the privately disclosed income would become public retroactively, MPs should have anticipated the possibility of public disclosure and, at least partially, increased their tax compliance starting in 2005. Moreover, tax evasion is a criminal offence and MPs not only would loose their mandate, but also face severe penalties.

Instead, the increase in 2007 is consistent with a change in social norms towards outside activities and earnings. These social norms could only have changed when outside earnings became *public*, not when they were privately disclosed. As the reported amounts were kept artificially low by top-coding at 7,000€, this could have induced voters (and subsequently politicians) to view outside earnings less negatively. This mechanism is also consistent with the increase being driven by income from self-employment as this income category has been shown to be acceptable by voters (Campbell and Cowley, 2015).

Table 4.5.2: Introduction of the disclosure law: income categories

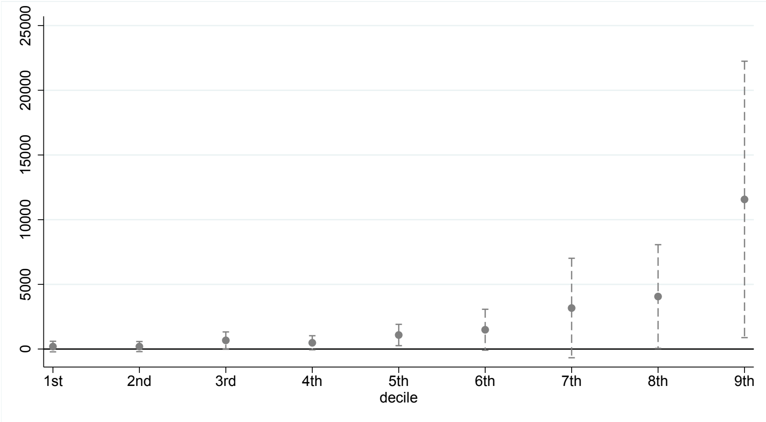
income category	wages & salaries		business & self-employment		other sources		renting (placebo)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log income	income > 0	log income	income > 0	log income	income > 0	log income	income > 0
treatment x reform	0.089	0.001	0.193**	0.037**	0.060	0.009	0.095	0.018
	(0.089)	(0.011)	(0.089)	(0.018)	(0.111)	(0.014)	(0.179)	(0.014)
politician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
w/o dropouts	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5,608	18,412	9,046	18,412	4,463	18,412	3,799	18,412
# politicians	1,518	3,546	2,319	3,546	1,229	3,546	1,550	3,546

Notes: This tables displays estimates from equation 4.1 using log outside earnings and a dummy for positive earnings from wages and salaries (columns 1 & 2), business operations and self-employment (columns 3 & 4), forest and agriculture and other sources (columns 5 & 6), and renting (columns 7 & 8) as outcome variables. Robust standard errors are clustered at the individual level. * p<0.1, ** p<0.05, *** p<0.01. Source: German tax return data, 2001-2009 (Taxpayer Panel, TPP)

Social norms might also change when previously intrinsically motivated is replaced by extrinsically motivated behavior. Gneezy and Rustichini (2000) show in a field experiment that the introduction of a fine for parents who pick up their children late from a day-care center actually increased late-coming. Before the fine, it was simply a social norm to be on time and afterwards it was perceived as a market transaction. Applying this finding to our setting, it might be that it was a social norm not to have little (or no) outside earnings. After the policy change, the price an MP pays for earning outside income is the duty to report it. Therefore, since politicians pay the price, earning outside income becomes more acceptable simply because they report it. Another explanation might be that social norms were initially misperceived. Bursztyn, Gonzalez, and Yanagizawa Drott (2020) define the term “pluralistic ignorance”, referring to a situation where most people privately hold an opinion, but they incorrectly believe that most other people hold the contrary opinion, and end up acting against their own view. When politicians believe having outside jobs are stigmatized, they might be reluctant to reveal their private views to others out of fear of social sanctions. In our setting, MPs might have misperceived the norms regarding outside activities since they were not public knowledge. Although the private view of an MP might have been that earning outside earnings is not necessarily a bad thing, they might have been reluctant to act on that belief, as they thought that others disapprove such behavior. When outside income became public and was seen to be widespread, they engaged more in that behavior.

Quantile regressions We test whether the effect is driven by different parts of the outside income distribution by conducting (unconditional) quantile regressions on the deciles of the outside earnings distribution. That is, we estimate not the average effect, but the effect on all nine deciles (Firpo, Fortin, and Lemieux, 2009). The results are plotted in Figure 4.5.2. The treatment effect is very small for the lower and middle part of the distribution, whereas the effect on the eighth and ninth decile is considerably larger. This implies that most of the treatment effect is driven by high-income MPs that are likely to be top-censored.

Figure 4.5.2: Introduction of the disclosure law: quantile regression



Notes: This graphs displays the coefficient β on log of outside earnings and the corresponding 95% confidence interval when estimating equation 4.1 using unconditional quantile regression for the first to ninth decile. Robust standard errors are clustered at the individual level. Source: German tax return data, 2001-2009 (Taxpayer Panel, TPP)

4.5.2 Tightening of the Public Disclosure Law

Baseline result Recall, that the second reform introduced seven new brackets such that it shifted top-coded incomes from 7,000€ to 250,000€. Therefore, voters were now enabled to differentiate between medium- and high-earning MPs. Our baseline difference-in-difference estimates using equation 4.1 are presented in Table 4.5.3. The tightening of disclosure law significantly decreased total outside income by 9.6%, while leaving the extensive margin unchanged. This result is line with the institutional details of the new rules, since the introduction of new brackets did not change the reporting requirements at the extensive margin. As one can see in Figure 4.5.3, the effect emerges in 2012 with parallel trends between the treatment and control group in the year before. Importantly, the effect occurs before the federal election in 2013 and can therefore not be driven by a changed composition of the federal parliament.

Income categories When we decompose the total effect into the different income categories, we find that the negative intensive margin effect is driven by a reduction of 15.8% of income from wages and salaries (see column 1 of Table 4.5.4). We do not find any significant negative effect on self-employment or business income. This is consistent with the tightening of the rules inducing a sizeable transparency effect as this income category is viewed more favourably among voters (Campbell and Cowley, 2015).

We do not find consistent evidence for a change in the other income categories. Similarly to the introduction of the law, we do not find any effect on rental income, which acts as our placebo outcome.

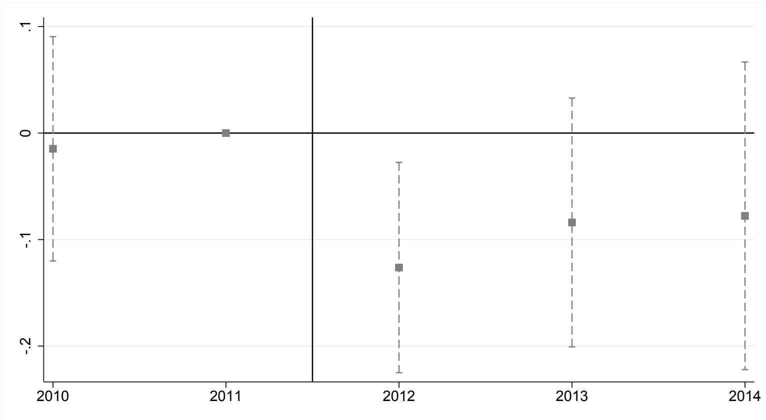
Quantile regression Given that the introduction of the new income brackets mainly affected top-earning MPs, we expect the treatment effect to be concentrated at the top

Table 4.5.3: Tightening of the disclosure law: extensive and intensive margin

	(1) log outside income	(2) log outside income	(3) outside income > 0	(4) outside income > 0
treatment x reform	-0.092*	-0.096**	0.011	0.008
	(0.047)	(0.048)	(0.013)	(0.013)
politician FE	Yes	Yes	Yes	Yes
state-year FE	Yes	Yes	Yes	Yes
w/o dropouts		Yes		Yes
N	8,622	8,299	11,223	10,849
# politicians	2,716	2,600	3,212	3,096

Notes: This tables displays estimates from equation 4.1 using log outside earnings (columns 1 & 2) and a dummy for positive outside earnings (columns 3 & 4) as outcome variables. Robust standard errors are clustered at the individual level. * p<0.1, ** p<0.05, *** p<0.01. Source: German tax return data, 2010-2014 (Taxpayer Panel, TPP)

Figure 4.5.3: Tightening of the disclosure law: dynamic difference-in-difference



Notes: This graphs displays the coefficients $\beta_t \; \forall \; t \in \{2010, ..., 2014\}$ and the corresponding 95% confidence intervals estimated by equation 4.3 using outside earnings as the outcome variable. Robust standard errors are clustered at the individual level. Source: German tax return data, 2010-2014 (Taxpayer Panel, TPP)

of the distribution. We test this hypothesis by estimating quantile regressions for every decile of the distribution. As one can see in Figure 4.5.4, the effect is very small and insignificant for the first deciles and then becomes larger the further one goes along the distribution.

Electoral accountability Next, we explore potential mechanisms of the decrease in outside earnings following the tightening of the disclosure rules.[21] As we argued before, we expect the effect to be stronger the more accountable politicians are to their voters. Since we cannot test this hypothesis in the tax data, we make use of the reported data. In a first step, we compare MPs elected by direct ballot and their runner-up peers, who entered via party list. We additionally add electoral district fixed effects to only compare the winner of a direct election and the second-placed candidate. Panel A in 4.5.5 shows

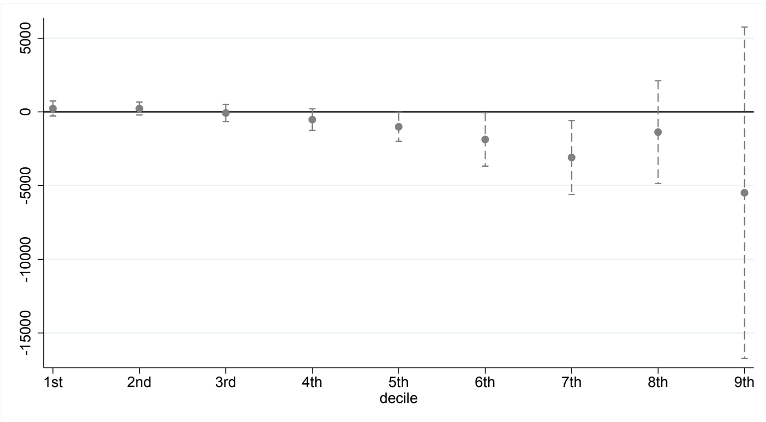
[21] We cannot use the reported data for the first reform since we cannot observe reported outside income before the reform.

Table 4.5.4: Tightening of the disclosure law: income categories

income category	wages & salaries		business & self-employment		other sources		renting (placebo)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log income	income > 0	log income	income > 0	log income	income > 0	log income	income > 0
treatment x reform	-0.158***	-0.000	-0.035	0.034**	-0.116	-0.027**	0.003	-0.017
	(0.052)	(0.009)	(0.064)	(0.015)	(0.073)	(0.011)	(0.095)	(0.012)
politician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
w/o dropouts	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3,580	10,849	5,808	10,849	3,163	10,849	2,554	10,849
# politicians	1,256	3,096	1,978	3,096	1,064	3,096	964	3,096

Notes: This tables displays estimates from equation 4.1 using log outside earnings and a dummy for positive earnings from wages and salaries (column 1 & 2), business operations and self-employment (column 3 & 4), forest and agriculture and other sources (column 5 & 6), and renting (column 7 & 8) as outcome variables. Robust standard errors are clustered at the individual level. * p<0.1, ** p<0.05, *** p<0.01. Source: German tax return data, 2010-2014 (Taxpayer Panel, TPP)

Figure 4.5.4: Tightening of the disclosure law: quantile regression



Notes: This graphs displays the coefficient β on log of outside earnings and the corresponding 95% confidence interval when estimating equation 4.1 using unconditional quantile regression for the first to ninth decile. Robust standard errors are clustered at the individual level. Source: German tax return data, 2010-2014 (Taxpayer Panel, TPP)

that there was no significant difference between MPs elected by direct ballot and MPs joining via the party list before election period 18.[22] In election period 18, when the new rules became effective, the difference increases to roughly 14,000€ and becomes significant at the 1% level (see column (3) of Table 4.5.5). This suggests that directly elected MPs reduced their outside earnings more dramatically because of electoral concerns. We observe a similar pattern for MPs inhabiting more and less safe party list ranks. Before election period 18, there is no significant difference between those, who just made it in, and MPs, who were relatively safe (see columns (4) and (5) of Table 4.5.5). After the reform, we observe a significant difference of about 6,000€. Both results are robust to the lower bound measure of outside earnings (see Appendix Table 4.A.8). Taken together, these estimates provide support for the mechanism of electoral accountability.

[22] The negative, but insignificant coefficients are consistent with the introduction of the law causing minor electoral pressure.

Table 4.5.5: Electoral accountability

	(1) EP 16 outside earnings	(2) EP 17 outside earnings	(3) EP 18 outside earnings
Panel A: directly elected			
D^{direct}	-8,501	-6,112	-13,997***
	(5,653)	(10,725)	(5,282)
electoral district FE	Yes	Yes	Yes
controls	Yes	Yes	Yes
N	318	238	404
# politicians	318	238	404
Panel B: unsafe rank			
$D^{unsaferank}$	-2,790	-605	-5,907**
	(2,471)	(3,968)	(2,360)
party-state FE	Yes	Yes	Yes
controls	Yes	Yes	Yes
N	562	578	593
# politicians	562	578	593

Notes: The outcome variable is outside earnings as described in Section 4.3.2. In Panel A, the sample contains only MPs from districts, where both the first- and second-placed candidate entered parliament to estimate equation 4.4. In Panel B, we use only MPs that were ranked on a party list to estimate equation 4.5. Controls refer to all variables in Tables 4.A.3 and 4.A.4. Robust standard errors. * p<0.1, ** p<0.05, *** p<0.01 Source: reported data EP 16 - 18

4.6 Conclusion

This chapter evaluates the effects of public disclosure rules on politicians' outside earnings. Since 2005, members of the German federal parliament are obliged to publish their outside activities and associated earnings in a (top-coded) bracket-based reporting scheme on the website of the German Bundestag. First, we exploit the introduction of this policy as exogenous variation. We can observe both federal and state MPs in administrative tax records before and after the policy change. Thereby, we use unaffected state MPs as a control group in a difference-in-difference design. Second, we can differentiate between private and public disclosure. Since 2005, information on outside activities and earnings was initially privately disclosed to the administration of the Bundestag. In 2007, the Federal Constitutional Court decided that the information must be publicly disclosed including a retroactive disclosure of the information for the years 2005 and 2006. Third, we evaluate a second reform that tightened existing rules by introducing seven new income brackets in the reporting scheme causing reported outside income to be top-coded at 250,000€ instead of 7,000€. Last, given the sparse number of demographic variables in the tax return data and the inability to merge this data with any other data set, we collect various other data sets to uncover potential mechanisms behind our findings.

We show that the introduction of public disclosure of outside activities and earnings lead to an increase of 15% in outside earnings. This effect is mainly present at the top end of the distribution and is largely driven by income from self-employment and businesses. Importantly, the effect only emerges when disclosure is public, not when it is private. Therefore, it is unlikely that it is driven by increased tax compliance, since MPs should have anticipated that there is a significant chance that their privately disclosed income would become public retroactively. A more likely explanation is a change in social norms regarding outside income that made the practice more acceptable. Next, we find that the tightening of the disclosure decrease outside income, in particular, income from salaries and wages drop by 15.8%, while other income categories are largely unaffected. Using the reported data on outside income, we provide evidence that electoral accountability might explain the decrease in outside income. More specifically, we show that outside income of directly elected MPs drops relative to MPs joining via party list after the reform. Similarly, MPs with an unsafe rank on the party list decrease their outside income relative to MPs with a safe rank. Taken together, our results suggest that the effect of income disclosure laws crucially depends on their exact implementation. If the disclosed information is very limited and lacks precision such that voters cannot identify top-earners, public income disclosure can increase outside activities and earnings and thereby, might increase the risk of conflicts of interest.

This chapter faces various limitations. Earnings in the tax data does not necessarily reflect the time an MP has invested into his or her outside work. Activities differ in the type of activity, the time invested, and the degree of interdependence with third parties, all of which we cannot observe in the tax data. Therefore, we cannot make statements about the impact on the quality of parliamentary work or direct conflicts of interest.

Appendix

4.A Additional Graphs and Tables

Figure 4.A.1: Example of outside earnings public disclosure on the website of the German federal parliament

Entgeltliche Tätigkeiten neben dem Mandat

Compamedia GmbH, Überlingen,
Vortrag, 2015, Stufe 3 (Deutscher Mittelstands-Summit)

CSA Celebrity Speakers GmbH, Düsseldorf,
Vortrag, 2015, Stufe 4 (AGRAVIS-Vortragsveranstaltung, AGRAVIS Raiffeisen AG, Münster)

Econ Referenten-Agentur, München,
Vortrag, 2014, Stufe 4 (Haspa-Branchen Treff „Wirtschaftsfaktor Russland", Hamburger Sparkasse AG, Hamburg)
Vortrag, 2016, Stufe 4 (Optimum Asset Management-Event 2016, Optimum Asset Management SA, Berlin)

Forum Executive AG, Zürich, Schweiz,
Vortrag, 2016, Stufe 4 (Funds Expert Forum)

GUILLOT Referenten-Kommunikation-Speakers Bureau, Ralingen,
Podiumsdiskussion, 2014, Stufe 4 (Das Freihandelsabkommen TTIP - Chance oder Schreckensvision für Europa, Deutscher Zigarettenverband e.V., Berlin)
Vortrag, 2015, Stufe 4 (Immobilien Investment Forum 2015, Savills Investment Management, Frankfurt/Main)
Vortrag, 2015, Stufe 4 (Tacheles 2015 - Das Investmentgespräch, Drescher & Cie Gesellschaft für Wirtschafts- und Finanzinformation mbH, St. Augustin)

Hoffmann & Campe Verlag GmbH, Hamburg,
Publizistische Tätigkeit, 2014, Stufe 8; 2015, Stufe 8
Vortrag, 2015, Stufe 3 (Lesereise)
Vortrag, 2016, Stufe 2 (Lesereise)

iGZ - Interessenverband Deutscher Zeitarbeitsunternehmen e.V., Berlin,
Vortrag, 2015, Stufe 4 (iGZ-Bundeskongress)

Internationales Steuerseminar Schweiz (IStS), Zürich, Schweiz,
Vortrag, 2016, Stufe 3 (Internationales Steuerseminar 2016)

marcus evans Germany Ltd., Berlin,
Vortrag, 2015, Stufe 4 (9. CMO-Gipfel)
Vortrag, 2016, Stufe 4 (9. CEO-Gipfel)

MMM-Club (Moderne Markt-Methoden) e.V., Wettenberg,
Vortrag, 2016, Stufe 4 (54. MMM-Kongress)

pilot München GmbH, München,
Podiumsdiskussion, 2016, Stufe 4 (pilot Business-Lounge: „Zukunft gestalten")

Schweizerisches Institut für Auslandforschung (SIAF), Zürich,
Vortrag, 2014, Stufe 3 (Veranstaltungsreihe „Die Zukunft der Demokratie")

The London Speaker Bureau Germany, Karlsruhe,
Vortrag, 2015, Stufe 4 (UniCredit Wirtschaftsgespräch)
Vortrag, 2015, Stufe 4 (beim Industriebeirat der Triton Beratungsgesellschaft GmbH, Frankfurt/Main)

Vodafone Institute for Society and Communications GmbH, Berlin,
Vortrag, 2016, Stufe 2 (Veranstaltungsreihe „AusZeit")

WBMG - Unternehmensberatung GmbH, Landshut,
Beratung, 2014, Stufe 5; 2015, Stufe 7; 2016, Stufe 6

Zeitverlag Gerd Bucerius GmbH & Co. KG, Hamburg,
Publizistische Tätigkeit, 2014, Stufe 1

Funktionen in Unternehmen

Borussia Dortmund GmbH & Co. KGaA, Dortmund,
Mitglied des Aufsichtsrates, 2015, Stufe 4

ThyssenKrupp AG, Essen,
Mitglied des Aufsichtsrates (bis 31.12.2012), 2014, Stufe 3 (für 2012)

Funktionen in Vereinen, Verbänden und Stiftungen

Deutsche Nationalstiftung, Hamburg,
Mitglied des Senats

Helmut und Loki Schmidt-Stiftung, Hamburg,
Mitglied des Kuratoriums

Stiftung Berliner Schloss - Humboldtforum, Berlin,
Mitglied des Kuratoriums

ZEIT-Stiftung Ebelin und Gerd Bucerius, Hamburg,
Mitglied des Kuratoriums, jährlich, Stufe 3

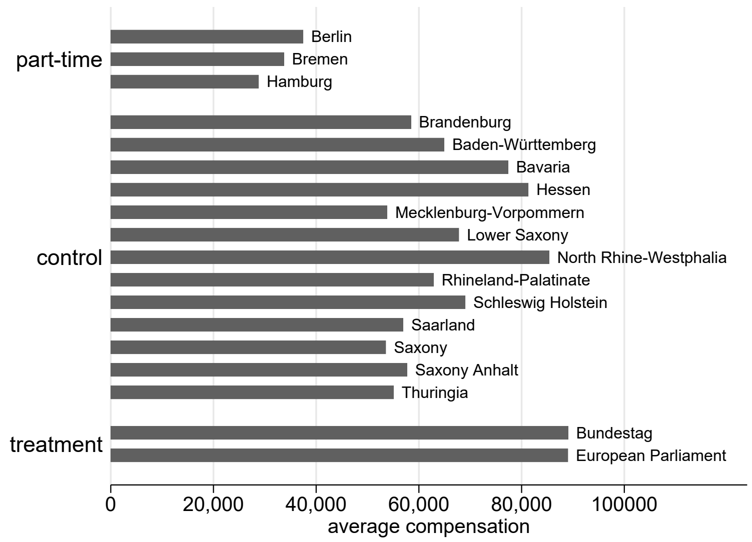
Notes: This figure is a screen shot of Peer Steinbrück's published outside earnings in election period 18. Source: Website of the Bundestag `https://www.bundestag.de/abgeordnete/biografien18/S/steinbrueck_peer/259022`

Table 4.A.1: Public disclosure rules and measures of reported outside earnings

	election period 16 & 17				election period 18			
level	from	to	baseline	lower bound	from	to	baseline	lower bound
0	0	1,000	500	500	0	1,000	500	500
1	1,000	3,500	2,250	2,250	1,000	3,500	2,250	2,250
2	3,500	7,000	5,250	5,250	3,500	7,000	5,250	5,250
3	7,000		9,500	7,000	7,000	15,000	9,500	7,000
4					15,000	30,000	9,500	7,000
5					30,000	50,000	9,500	7,000
6					50,000	75,000	9,500	7,000
7					75,000	100,000	9,500	7,000
8					100,000	150,000	9,500	7,000
9					150,000	250,000	9,500	7,000
10					250,000		9,500	7,000

Notes: All values are in Euros. Public disclosure rules for election period 16, 17 and 18 as well as our two different measures that are used in the reported data. See Section 4.3.2 for details of the construction of the baseline and lower bound measures.

Figure 4.A.2: Average compensation of MPs in each parliament



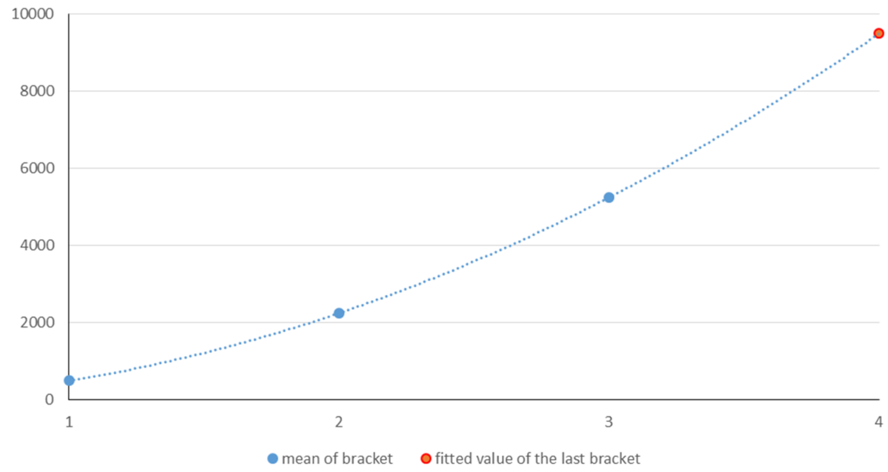
Notes: This figure plots the distribution of average compensation for a MP in each parliament (federal, state or EU). These values refer to the average for the years 2001 to 2014.

Table 4.A.2: Details of election periods in federal parliament

	Election Period 16	Election Period 17	Election Period 18
Election Details			
election date	18.09.2005	27.09.2009	22.09.2013
duration	18.10.2005 - 27.10.2009	27.10.2009 - 22.10.2013	22.10.2013 - 24.10.2017
seats	614	622	631
Party			
CDU/CSU	226	239	311
SPD	222	146	193
FDP	61	93	0
The Left	54	76	64
Greens	51	68	63

Notes: This table consists of information of each election period in federal parliament under study.

Figure 4.A.3: Reporting brackets



Notes: This figures visualizes the imputed values for each bracket. The blue dots are the average value for the respective bracket through which we fit a polynomial (blue dotted line). We then extrapolate the polynomial function to the highest bracket and impute the predicted value (orange dots).

Table 4.A.3: Descriptive statistics: demographics (reported data)

variable	**mean**	**sd**	**N**
female	0.34	0.47	1952
age below 50	0.39	0.49	1952
age between 50 and 60	0.37	0.48	1952
age 60 and above	0.24	0.43	1952
East Germany	0.17	0.37	1952
married	0.72	0.45	1952
# children	1.60	1.37	1952
title: doctor	0.19	0.39	1952
title: professor	0.01	0.09	1952
occupation: other	0.32	0.47	1952
occupation: lawyer	0.19	0.39	1952
occupation: economist/MBA	0.16	0.36	1952
occupation: farmer	0.03	0.16	1952
occupation: teacher	0.09	0.28	1952
occupation: civil servant	0.02	0.15	1952
occupation: doctor	0.02	0.12	1952
occupation: journalist	0.03	0.16	1952
occupation: academic	0.08	0.28	1952
occupation: self-employed	0.07	0.26	1952
party: left-wing	0.50	0.50	1952
party: CDU/CSU	0.41	0.49	1952
party: SPD	0.30	0.46	1952
party: Greens	0.10	0.30	1952
party: The Left	0.10	0.30	1952
party: FDP	0.08	0.28	1952
terms: newcomer	0.31	0.46	1952
terms: 2 - 3	0.38	0.49	1952
terms: > 3	0.30	0.46	1952
early dropout	0.03	0.18	1952
late entry	0.04	0.20	1952

Source: Reported data for election periods 16, 17 and 18.

Table 4.A.4: Descriptive statistics: political and electoral variables (reported data)

variable	**mean**	**sd**	**N**
entry: direct ballot	0.46	0.50	1952
entry: list ranking	10.55	12.31	1733
vote margin: candidate	6.78	16.51	1866
vote margin: party	12.09	10.11	1866
leadership	0.11	0.32	1952
committee chair	0.07	0.25	1952
committee: interior	0.06	0.23	1952
committee: digital	0.01	0.09	1952
committee: social	0.06	0.23	1952
committee: family	0.05	0.22	1952
committee: health	0.05	0.22	1952
committee: culture	0.03	0.18	1952
committee: human rights	0.03	0.16	1952
committee: justice	0.05	0.23	1952
committee: environment	0.05	0.22	1952
committee: election	0.03	0.16	1952
committee: development	0.03	0.18	1952
committee: exterior	0.06	0.23	1952
committee: budget	0.10	0.29	1952
committee: petition	0.04	0.19	1952
committee: accounting	0.02	0.13	1952
committee: sports	0.03	0.16	1952
committee: agriculture	0.05	0.22	1952
committee: tourism	0.03	0.16	1952
committee: traffic	0.06	0.24	1952
committee: defense	0.05	0.22	1952
committee: economics	0.06	0.24	1952
committee: science	0.05	0.22	1952
committee: EU	0.05	0.22	1952

Source: Reported data for election periods 16, 17 and 18.

Table 4.A.5: Composition of outside activities per MP (reported data)

	EP 16		EP 17		EP 18		**Total**	
	N	in %	N	in %	N	in %	N	in %
remunerated activity	195	31.76	219	35.21	195	30.90	**609**	**32.62**
type of activity								
law	61	9.93	65	10.45	58	9.19	184	9.86
speech	52	8.57	55	8.84	45	7.13	152	8.14
management and consulting	61	9.93	70	11.25	57	9.03	188	10.07
other	37	6.03	42	6.75	38	6.02	117	6.27
functions in enterprises	240	39.09	223	35.85	291	46.12	**754**	**40.39**
type of function								
public office	0	0	4	0.64	11	1.74	15	0.80
consult	94	15.31	76	12.22	87	13.79	257	13.77
control	144	23.45	144	23.15	197	31.22	197	25.98
lead	25	4.07	24	3.86	32	5.07	81	4.34
type of membership								
regular member	216	35.18	201	32.32	269	42.63	686	36.74
chairman	33	5.37	34	5.47	42	6.66	109	5.84
functions in public corporations	359	58.47	357	57.40	385	61.01	**1,001**	**58.97**
type of function								
public office	226	36.81	247	39.71	264	41.84	737	39.48
consult	95	15.47	90	14.47	89	14.10	274	14.68
control	70	11.40	71	11.41	104	16.48	245	13.12
lead	35	5.70	39	6.27	31	4.91	105	5.62
type of membership								
regular member	339	55.21	341	54.82	372	58.95	372	56.35
chairman	37	6.03	37	5.95	35	5.55	109	5.84
functions in clubs	437	71.17	469	75.40	446	70.68	**1,352**	**72.42**
shareholdings in private corporations	69	11.24	76	12.22	67	10.62	**212**	**11.36**
Total # MPs	**614**		**622**		**631**			

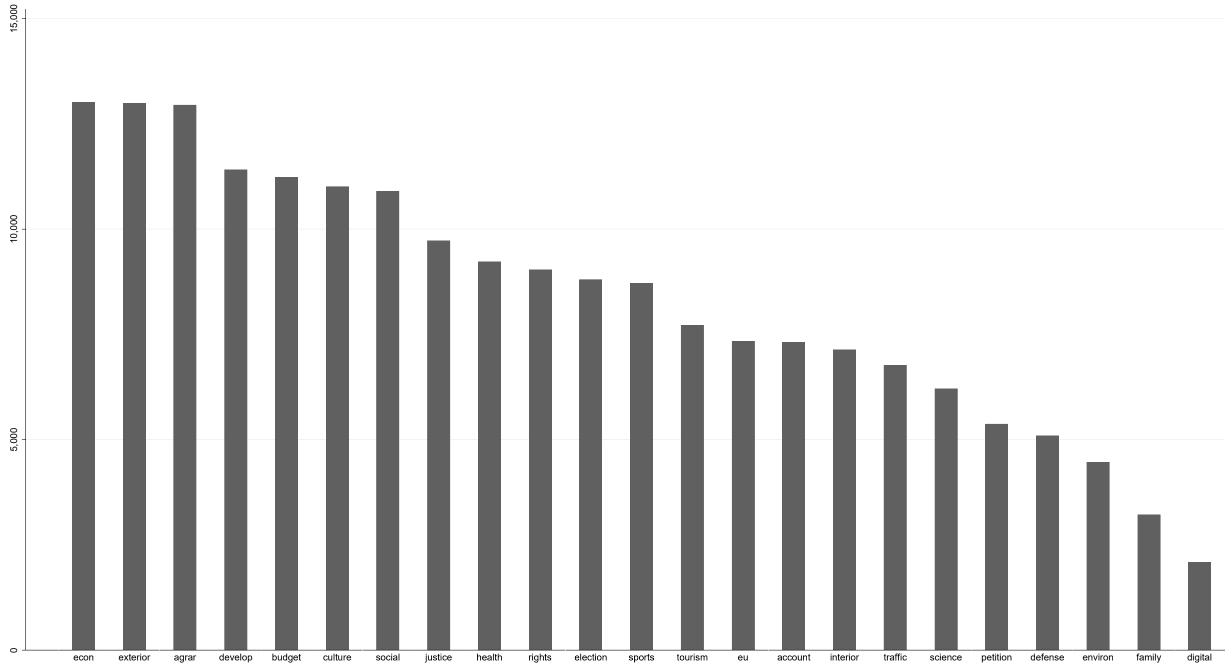
Notes: This table provides an overview about the composition of outside activities per MP, meaning how many MPs pursue a certain activity. The percentages define the share of MPs who persue a certain activity. For example, 32.62% of all MPs report a remunerated activity, while 58.97% of all MPs hold a function in a club. Activities are reported such that they belong to one of the following categories: remunerated activity, functions in enterprises, functions in public corporations, functions in clubs or shareholdings in private corporations. We broadly categorize remunerated activities into (a) law (e.g. lawyer, judge), (b) speech (e.g. speech, publishing books), (c) management and consulting (e.g. business consultant, notary, manager) and (d) other (e.g. farmer, doctor). We classify the type of function into (a) public office (e.g. position in local politics/ church), (b) consult (e.g. advisory board), (c) control (e.g. supervisory board) and (d) lead (e.g. committee, management board, board of trustees). For 1.44% of all remunerated activities and for 0.32% of all functions in enterprises, no information about the type of activity is available. Functions in clubs are often voluntary work. The information 'voluntary' is optional and added in more than 85% of all functions in clubs. In some cases, the name of clients are not revealed due to existence of lawyer-client-confidentiality. We ignore the information of occupational activities pre-dating membership (e.g. lawyer). Shareholdings in private corporations need to be reported if a MP holds more than 25%.

Table 4.A.6: Distribution of levels and frequency by activity (reported data)

	EP 16		EP 17		EP 18		Total	
	N	**in %**	**N**	**in %**	**N**	**in %**	**N**	**in %**
Level								
0	1314	48	1395	48	1745	54	4454	50
1	696	26	780	27	721	22	2197	25
2	206	8	218	8	226	7	650	7
3 and higher	**497**	**18**	**512**	**18**	**519**	**18**	**1528**	**18**
3	497	18	512	18	235	7	1244	14
4					115	4	115	1
5					52	2	52	1
6					31	1	31	0
7					18	1	18	0
8					23	1	23	0
9					21	1	21	0
10					24	1	24	0
Frequency								
once	2559	94	2721	94	3032	94	8312	94
yearly	67	2	59	2	53	2	179	2
monthly	86	3	126	4	129	4	341	4

Notes: Levels and frequencies are reported for the following categories of activities: remunerated activities, functions in enterprises, functions in public corporations and functions in clubs. For functions in clubs, MPs can optionally indicate whether is is voluntary work or not. Source: Reported Data, own calculations.

Figure 4.A.4: Outside earnings by committee membership



Notes: This graphs displays the average outside earnings as defined in Section 4.3.2 for each committee in the German federal parliament. Source: Reported Data EP 16 - 18

Table 4.A.7: Average number of MPs in federal and state parliaments

	number of MPs	election years
Treatment Group	**722**	
Federal Parliament	623	2002, 2005, 2009, 2013
Control Group	**776**	
Baden Württemberg	134	2001, 2006, 2011
Mecklenburg-Vorpommern	71	2002, 2006, 2011
North Rhine Westphalia	210	2005, 2010, 2012
Rhineland-Palatinate	101	2001, 2006, 2011
Schleswig-Holstein	83	2005, 2009, 2012
Saarland	51	2004, 2009, 2012
Saxony	126	2004, 2009, 2014
Control Group ***(excluded in 2013 & 2014)***	**187**	
Bavaria	187	2003, 2008, 2013
Control Group ***(excluded in 2014)***	**557**	
Hessia	112	2003, 2008, 2013
Lower Saxony	163	2003, 2009, 2013
Brandenburg	88	2004, 2009, 2014
Saxony-Anhalt	106	2002, 2006, 2011
Thuringia	88	2004, 2009, 2014
Part-time parliament ***(excluded in all years)***	**352**	
Berlin	146	2001, 2006, 2011
Bremen	85	2003, 2007, 2011
Hamburg	121	2001, 2004, 2008, 2011

Notes: This table consists of information of each parliament under study. The number denotes the average number of MPs in each parliament for the years 2001 to 2014. Germany consists of 16 states (*Länder*). We entirely exclude Berlin, Bremen and Hamburg from our analysis (part-time Parliament (*Feierabendparliament*)). Bavaria, Hessen, Lower Saxony, Brandenburg, Saxony-Anhalt, and Thuringia introduced public disclosure rules in 2013/2014 and are excluded from our sample for these years.

Table 4.A.8: Tightening of the disclosure law: channels (lower bound)

	(1) EP 16 outside earnings	(2) EP 17 outside earnings	(3) EP 18 outside earnings
Panel A: directly elected			
D^{direct}	-7,870*	-5,108	-12,328***
	(4,697)	(8,512)	(4,488)
electoral district FE	Yes	Yes	Yes
controls	Yes	Yes	Yes
N	318	238	404
# politicians	318	238	404
Panel B: unsafe party rank			
$D^{unsaferank}$	-2,466	-417	-4,996**
	(2,130)	(3,473)	(2,044)
party-state FE	Yes	Yes	Yes
controls	Yes	Yes	Yes
N	562	578	593
# politicians	562	578	593

Notes: The outcome variable is outside earnings as described in Section 4.3.2. In Panel A, the sample contains only MPs from districts, where both the first- and second-placed candidate entered parliament to estimate equation 4.4. In Panel B, we use only MPs that were ranked on a party list to estimate equation 4.5. Controls refer to all variables in Tables 4.A.3 and 4.A.4. Robust standard errors. * p<0.1, ** p<0.05, *** p<0.01 Source: reported data EP 16 - 18

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