The efficiency and distributive effects of local taxes: Evidence from Italian municipalities*

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Abstract

Different levels of tax progressivity between fiscal jurisdictions might trigger a host of taxpayers' behavioral responses intended to minimize the tax burden. This paper uses panel data from municipality-level tax returns to study the effect of local income tax policy on taxable income and its distribution. Exploiting a reform that granted municipalities the authority to switch from a flat to a progressive local tax scheme on personal income and implementing a difference-in-differences analysis, I find a significant negative effect of tax progressivity on both taxable income and inequality. I estimate an elasticity of taxable income with respect to the net-of-local tax rate of 0.55; the response is mostly concentrated among taxpayers in the top percentile of the pre-tax income distribution. Combining data on transfers of residence with the differential in tax rates across fiscal jurisdictions, I show that the main findings are consistent with the notion of internal mobility and cross-municipality income shifting within local labor markets to minimize the tax liability.

Keywords: Local income tax; Tax progressivity; Taxable income elasticity; Income inequality; Tax-induced migration.

JEL Classification: D31, H21, H24, H30, H71

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"Policies to adjust the distribution of income among individuals must be conducted on a nationwide basis [...] Regional measures are self-defeating, as the rich will leave and the poor will move to the more egalitarian-minded jurisdictions. Progressive income taxation at the upper as well as transfers at the lower end of the scale — if substantial in scope — must be uniform within the entire area over which there is a high degree of capital and labor mobility, which means they have to be a function of the national government." (Musgrave, 1971, p.7).

1 Introduction

How do local taxes affect the taxpayers' income reporting behavior? Public finance literature (Musgrave, 1959; Oates, 1972; Stiglitz, 1988; Feldstein and Wrobel, 1998) provides a clear answer to this question: If individuals can migrate across jurisdictions, more progressive local taxes would result in efficiency losses without achieving any net redistribution of income in the long-run. As early emphasized by Musgrave (1971), local redistributive policies are constrained in a system characterized by a high degree of decentralization of the public sector. Consider, for example, a community that decides to increase the progressivity of the income tax to achieve a more egalitarian distribution of income. Such a program would create incentives for the rich to move out and for the poor to migrate into the community. A more equal pre-tax distribution of income might be achieved, but it would be caused largely by an outflow of the rich and an influx of the poor, with a consequent reduction in the level of per-capita income in the community considered.

The scope for redistributive programs is thus limited by the taxpayers' behavioral response, which tends to be greater the smaller the fiscal jurisdiction under consideration (Kanbur and Keen, 1993). It follows that the extent to which taxpayers respond to variations in local tax policies has crucial implications for policy-makers, encompassing the resulting changes in expected tax revenue and the level and location of local economic activity.

This paper studies the effect of local income tax policy on taxable income and its distribution using panel data from municipality-level tax returns over the 2001-2015 period. As strikingly shown by Figure 1, Italy has experienced a massive process of devolution of functions to local governments over the last decades and thus provides an ideal laboratory to study the effects of local tax changes. Apart from central rates, surtaxes on personal income are levied by regions and municipalities, giving rise to substantial differences in the local tax rate over time and place.

[Figure 1 about here]

A notable change in the normative setting of the municipality surtax on personal income originated with the 2007 and 2011 tax reforms, which granted municipalities the authority to switch from the existing flat tax schedule to a system of multiple rates and tax exemption for low incomes. After these reforms, almost one-third of the municipalities switched to a progressive local tax schedule. These reforms thus arguably provide treatment and control groups for testing how differences in tax progressivity over time and across municipalities affect the behavioral response of taxpayers.

Implementing a difference-in-differences analysis, I find that the introduction of a progressive local tax scheme reduced the municipal aggregate taxable income by nearly 7%. In addition, the reforms reduced inequality; specifically, the pre-tax income share held by the top percentile of the income distribution decreased by around 3%. This finding goes well in-line with the existing cross-country evidence of responsiveness of the rich to tax progressivity and top marginal tax rate (Saez et al., 2012; Kleven et al., 2013; Piketty et al., 2014; Akcigit et al., 2016; Moretti and Wilson, 2017; Rubolino and Waldenström, 2017a; Rubolino and Waldenström, 2017b). However, I do not find any significant effect on the Gini index and in the income share held by other slices of the pre-tax income distribution, thus suggesting that the observed reduction in the income share held by the rich was not compensated by an increase in pre-tax incomes of the poor.

Exploiting within-municipality variations in tax rates and taxable income and implementing an instrumental variable approach, I estimate the elasticity of municipal taxable income with respect to the net-of-local marginal income tax rate (i.e., one minus the tax rate). This parameter determines the extent to which a tax cut will be self-financed through an increase in the tax base and is thus crucial for forecasting changes in tax revenues induced by tax reforms. I estimate an elasticity of 0.55, which lies in the range of other international estimates carried out at the state- or county-level.[1] The behavioral response, however, is mostly concentrated among rich taxpayers: a 10% increase in the net-of-local marginal tax rate raises the income share held by the top percentile by around 5%.

Taxable income elasticity estimate significantly varies according to specific municipality characteristics. First, taxpayers located in municipalities with larger population size are relatively less responsive to taxes, in-line with predictions from theoretical literature (Bucovetsky, 1991; Kanbur and Keen, 1993). Second, the elasticity is significantly higher in places with larger share of rental income, suggesting that rentiers presumably face lower mobility costs and, hence, are relatively more responsive to taxes. Third, tax responsiveness is significantly lower in municipalities characterized by higher cooperative gathering, implying that there is complementarity between norms of cooperation and behavioral responses to tax changes (Andreoni et al., 1998). Finally, the elasticity is relatively larger over regional borders, that is where the spatial tax differential is higher.

In setting optimal local tax policy, particularly regarding the optimal degree of tax progres-

[1] For the US, studies on state tax base elasticity provide contrasting findings. Long (1999) uses cross-sectional state income tax changes and estimates a taxable income elasticity in the range of 0.2-0.8, while Spreen (2018) exploits time-variation in local individual income tax rates and estimates an elasticity of 0.72 in the state of Illinois. On the other hand, Bruce et al. (2010) and Yang and Heim (2017) do not find a significant effect of state income taxes on the tax base. The external validity of these studies, however, is limited: Long (1999) only focuses on income reported in 1999, while Bruce et al. (2010), Yang and Heim (2017) and Spreen (2018) rely on a small sample, which is not representative for the overall US population. In contrast, Milligan and Smart (2018) focus on local tax rates in Canadian provinces and estimate an elasticity of the income share held by the top percentile with respect to one minus the tax rate of 1.2, which mostly reflects tax avoidance behaviors.

sivity, the extent to which people flee local jurisdictions that attempt to impose relatively larger tax burdens on them is a crucial consideration. When local fiscal jurisdictions operate progressive taxes, the optimal degree of progressivity is a declining function of the degree of mobility in response to taxes (Mirrlees, 1982; Lehmann et al., 2014). Previous empirical literature (Day and Winer, 2006; Liebig et al., 2007; Young and Varner, 2011; Young et al., 2016) find overall little evidence of fiscally-induced internal migration. This can be motivated by the fact that individuals do not want to give up their jobs or commute (Winkelmann and Winkelmann, 1998; Kahneman et al., 2004), neither they want to change their local networks and neighborhoods (Dahl and Sorenson, 2010). Thus, a general aversion to migrate seems to counterbalance the fiscal incentives to change location. However, when escaping a jurisdiction's tax net does not necessarily entail any physical movement and can be accomplished by the "mere stroke of a pen" (Slemrod, 2010), the real location can be different from the *tax* location and the general motives behind aversion to migrate weaken. In other words, the application of residence-based taxation - which Italy applies - as opposed to employment-based taxation allows taxpayers to shift their tax burden without shifting their economic activities. Consistently with this argument, the literature on countries applying the residence-based taxation (see, for instance, Agrawal and Foremny, 2018, for Spain; Schmidheiny, 2006, Martinez, 2017, and Schmidheiny and Slotwinski, 2018, for Switzerland) finds significant evidence of tax-induced internal migration.

To investigate whether taxpayers actively move their residence across borders to minimize their tax liability, I compute bilateral migration flows for each province-pair and for every year using administrative data on transfers of residence. Then, I relate bilateral migration flows with the differential in the net-of-personal and property tax rates between destination and origin province in each year, controlling for province-pair fixed effects, time fixed effects and differences in public good provisions. I find that the tax differential has a significant positive effect on the probability of migrating: on average, a 1% increase in the net-of-tax differential between destination and origin provinces rises the stock of movers by around 2.4%.

The nature of the data, however, does not allow to disentangle a real from a fraudulent move, where a taxpayer changes the tax residence to a second property without physically moving. Distinguishing between real responses (e.g. labor supply) and cross-municipality income-shifting is crucial in terms of welfare conclusions and policy recommendations (Chetty, 2009; Piketty et al., 2014). If income, totally or partially, is shifted toward other (less) taxed base, then the deadweight loss would be smaller than in the case effects reflect real responses. I investigate cross-municipality income-shifting responses by testing whether the own tax base is sensitive to variations in the tax differential with respect to competitor municipalities, i.e. bordering municipalities or those located in the same local labor market. While the own tax base does not significantly respond to the tax rate set by bordering municipalities, income-shifting within local labor markets seems to explain part of the changes in the tax base. This would suggest income (and, possibly, labor force) relocation within local labor markets, thus implying that the deadweight loss of taxation born by local governments does not necessarily translate into a similar overall welfare loss under a national perspective.

To probe the validity of these results, I run a vast range of robustness checks. The results are robust to the introduction of leads and lags of the tax reform, other contemporaneous policy events, additional specifications and alternative definitions of the tax rate or tax progressivity.

This paper innovates with the previous literature over several dimensions. First, to my knowledge, there are no studies estimating the effect of local personal income tax on municipal taxable income and its pre-tax distribution in Italy.[2] Second, a voluminous and growing literature is interested in estimating the elasticity of taxable income with respect to net-of-marginal tax rate (see Saez et al., 2012, for a critical survey of this literature). The elasticity of taxable income is a key parameter in public finance, because it captures, under certain assumptions, the full behavioral responses of taxpayers to changes in marginal tax. This paper connects with this literature by estimating the responsiveness of aggregate municipal taxable income to changes in the (net of) local marginal tax rate. Finally, this study also relates to other papers that exploit municipality-level variations in tax rates to identify behavioral responses.[3]

2 Institutional background

Italy is composed of three different sub-national tiers of government: there are 20 regions (*Regioni*), 110 provinces (*Province*), and around 8000 municipalities (*Comuni*). Appendix Table A1 reports information on the number of provinces and municipalies for each Italian region along with some demographic and economic data.

Municipal administration operates in the following sectors: personal and community services (e.g. social and welfare services, sport and culture, school, waste collection and disposal), asset and land use (e.g. urban plans, traffic), and economic development (e.g. tourism promotion, regulation of business hours). To finance these activities, some of the municipal revenue comes from regional or central government transfers, a part is the result of own taxation and the rest consists of revenue from transfers of capital, property alienations or loans.

As an autonomous source of revenue, municipalities have legislative power to levy taxes, duties or fees on properties, income, advertisement, landfill, visitors and public areas.[4] The property tax and the surtax on personal income constitute the bulk of municipal tax revenue, while the remaining taxes, duties and fees represent a negligible share.[5] Revenue from the sur-

[2]However, some studies focused on the effects of local tax rates in a specific town or region. For instance, Baldini et al. (2005) evaluate the distributive impact of local taxes on households living in the town of Modena, while Fedeli (2010) analyzes the determinants of local tax setting of the region Emilia-Romagna from 1998 to 2004.

[3]For instance, Matikka (2018) identifies the ETI in Finland using the variation in municipal personal income tax rates as an instrument for overall changes in marginal tax rates, while Fuest et al. (2018) exploit municipality-level corporate tax changes to estimate the effect on wages in Germany.

[4]The municipal property tax (*Imposta comunale sugli immobili*) was introduced by the 504/1992 decree and then replaced with a new tax (*Imposta municipale propria*) by the 214/2011 decree. The decree 360/1998 disciplines the surtax on personal income (*Addizionale comunale IRPEF*). The decree 507/1993 has introduced taxes, duties and fees on advertisement (*Imposta comunale sulla pubblicità*), landfill (*Tarsu*, replaced by *Tares* and *Tari* by the 147/2013 decree) and public areas (*Tassa per l'occupazione degli spazi e delle aree pubbliche*).

[5]Municipal balance sheets show that property and income taxes formed, on average, nearly the 75% of municipal

tax on personal income have spectacularly increased over the recent decades (from around €1 billion in 2001 to nearly €4.5 billion in 2015), compensating, at least in part, the substantial reduction in central government transfers and underlining the ongoing process of fiscal decentralization.

The decree 360/1998 has introduced the municipal surtax on personal income, establishing that each municipality can choose to levy a surtax on personal income on top of those implemented by the central and regional governments with a cap on 0.8%.[6] The tax rate is decided annually by the mayor and approved by the municipal council at the end of each year. It applies to the taxable income as determined by the National law and has the same tax base of the national personal income tax. The rate levies to where a taxpayer has his or her fiscal residence in that year, regardless where he or she works. The number of municipalities which have set their own surtax on personal income is increasing: while there were about 4,800 (out of 7,960) municipalities with a non-zero tax rate in 2001, in 2015 almost 90% of the municipalities have set their own tax rate. The same rules apply to the regional surtax on personal income, which allows each region to levy a (flat or progressive) surtax on personal income that may vary within a range fixed by the National law.

Since 2007, the legislator has allowed municipalities to introduce an exemption threshold conditional on specific income requirements (decree 296/2006). Another change in the normative setting of the municipal tax originated with the 138/2011 decree, which has granted municipalities the opportunity to switch from the existing flat tax schedule to a system of multiple tax rates.[7] The rates must be structured according to the same income brackets defined by the national personal income tax and be increasing with income. Since 2011, the number of municipalities with a progressive tax schedule is risen; in 2015 more than one-third of the municipalities had a progressive tax.

The 2007 and 2011 tax law changes thus arguably provide a compelling variation in how the tax burden differs according to taxpayers' fiscal residence and income level. This discontinuity is shown by Figure 2, which compares trends in tax progressivity (including both regional and municipal tax rates) between municipalities with a progressive tax schedule ("treated") and those with a flat tax rate ("untreated"). Progressivity is measured as the ratio between the average local tax rate faced by the richest taxpayers (i.e., those whose income is larger than 6 times the national average income) and the poorest (i.e., those whose income is equal to one-third of the average national income).[8] The two groups of municipalities followed a similar pattern until 2007, when the legislator allowed for the introduction of a tax exemption for low income taxpayers. From that year on, the difference in progressivity level started to increase and substantially accelerated after the 2011 reform. While over the 2001-2006 period the local

tax revenue, which is more than one-fifth of the municipal total revenue in 2015.

[6]The only exception is Rome, which is allowed to set a tax rate of 0.9%.

[7]This reform was sudden and unanticipated as it was part of a larger reform approved to face a sovereign debt crisis, with the aim of increasing local revenues and promote fiscal equity.

[8]Note that the nature of the data does not allow to compute other common measures of tax progressivity such as the Kakwani or Suits index.

average tax rate faced by the poorest taxpayers was almost the same as the one faced by the richest regardless of where income was reported, from 2015 the rich paid 1.7 times more than the poor in treated municipalities and around 1.2 in untreated municipalities.[9]

[Figure 2 about here]

Figures 3 depicts the geographical distribution of the local progressive tax and local top marginal personal income tax rate in 2015. Left-hand side graph shows that there are several clusters of bordering municipalities which have or do not have the progressive tax schedule; this might suggest yardstick competition or tax mimicking across neighbouring municipalities (Bordignon et al., 2003), but it might also arise because spatially proximate jurisdictions have similar preferences on tax policy. The local top marginal tax rate reaches its highest value in North-West (Piedmont), Center-West (Rome metropolitan area) and in some Southern municipalities located around the coast, the lowest across the country border and in Sardinia. The spatial and time variation in tax rates is also illustrated by Appendix Table B1, which compares the local (i.e., regional plus population-weighted average municipal rate) top marginal tax rate set by each region over different time periods. In 2015, the rate varied from 1.282% in Trentino Alto-Adige to 4.146% in Lazio.

[Figure 3 about here]

3 Data

This paper uses panel data from municipality-level tax returns provided by the Ministry of Economy and Finance over the 2001-2015 period, matched to data on municipal and regional surtaxes on personal income and other municipality- and province-level characteristics. Summary statistics are illustrated by Table B2.

3.1 Income and tax data

The Ministry of Economy and Finance provides grouped data on income and total number of taxpayers specific for seven income intervals and for each municipality (see Table B1 for an example). Income intervals are constant both over time and across municipalities to guarantee comparability. Nominal income data are converted to real income using the consumer price index and 2015 as the base year. The definition of income is taxable income (e.g. gross income minus deductions), which includes positive incomes from all sources (labor, including pensions, business, capital). The tax unit is the individual since the Italian personal income tax system is individually based.

[9] The ratio is not equal to 1 for untreated municipalities since I also include the (progressive) regional tax rate on income.

Left-hand side of Figure 4 provides a geographical illustration of municipal taxable income per-capita in 2015. Unsurprisingly, richest municipalities are common in the Northern area, while the poorest are mostly concentrated in the South, underlining a well-known historical Italian phenomenon (Daniele and Malanima, 2007).

[Figure 4 about here]

The regional and municipal personal income surtaxes apply to the taxable income as determined by the National law and have the same tax base of the national personal income tax. The Ministry of Economy and Finance provides data on the tax rate (or the multiple rates in the case of a graduated tax scheme) and, if implemented, the exemption threshold set by each municipality. Using these information, I construct the dummy variable $Reform_{i,t}$, which is equal to 1 in each "treated" municipality *i* for each year *t* under the progressive tax scheme and 0 otherwise. Each municipality with either multiple tax rates or a tax exemption is considered under the progressive tax scheme and, hence, as "treated".[10]

In addition to local personal income tax rates, I retrieve data on the property tax rates set by each municipality over the period of interest. These series are provided by the Italian Institute of Finance and Local Economy (*Fondazione IFEL*) and contain information on the basic property tax rate, the rate which applies to the main dwelling and the amount of tax allowance granted to the main dwelling.

Income and tax information are available over the period of interest for 7,682 out of the total 7,960 Italian municipalities (i.e., about 97%). I drop from the sample the municipalities which have experienced modifications to their administrative borders (i.e., aggregations or divisions) and those without data over the full time period.

3.2 Inequality indexes

To estimate the impact of local taxes on pre-tax income distribution, I compute municipality-specific Gini index and top income shares for each year over the 2001-2015 period. For the sake of space, detailed information on how I move from the limited information published by the Ministry of Economy and Finance to the broad distributional statements in which I am interested are reported in Appendix A. I use the city of Rome as example to show how I have computed these inequality measures.

The Gini index is the most common used measure since it gives an intuitive and graphical representation of inequality. Right-hand side of Figure 4 illustrates the inequality level of the Italian municipalities, according to the value of the Gini index in 2015. Within-municipality inequality seems to be larger in the poorer South, especially in Sicily and Campania.

Since previous literature has provided convincing evidence that high-income taxpayers are more sensitive to tax progressivity and marginal tax rates than the rest of the population, I also

[10] 84% of the treated municipalities in 2015 implemented both multiple rates and a tax exemption, 15% had multiple rates without a tax exemption and 1% provided a tax exemption with a local flat rate.

compute the income share held by groups of taxpayers in the top tail of the municipal income distribution. The standard practice to compute top income shares from grouped data is to assume that the top tail of the income distribution can be approximated by a Pareto distribution. The Pareto law for top incomes gives a distribution function for top incomes of the following form:

$$1 - F(y) = (k/y)^a, \tag{1}$$

where $k > 0$ is a constant and $a > 1$ is the (municipality-specific) Pareto parameter of the distribution. The corresponding density function is $f(y) = ak^a/y^{(1+a)}$. The main property of Pareto distribution is that the ratio between the average income $y^*(y)$ of taxpayers with income above y and y does not depend on the income threshold y. Formally:

$$y^*(y) = [\int_{z>y} zf(z)dz]/[\int_{z>y} f(z)dz] = [\int_{z>y} dz/z^a]/[\int_{z>y} dz/z^{1+a}] = ay/(a-1). \tag{2}$$

A simpler way to express the last equation is as $b = y^*(y)/y$, with $b = a/(a-1)$. Then, b represents how much times the income of taxpayers with income above y is larger than the income threshold y. These structural parameters allow to compute top fractile thresholds and average income. I use these municipality-specific and time-varying estimates to infer the income share held by the top decile (P90-100) and the top percentile (P99-100). Then, from these estimations, I derive the income share held by the bottom 90 percent of the income distribution (P0-90) and the bottom 90 percent of the top decile (P90-99).

3.3 Control variables

Policy measures are themselves responsive to economic and/or political conditions and therefore usually endogenous (Besley and Case, 2000). In order to overcome any resulting empirical problem, I collect several time-varying demographic, economic and political variables that may affect both the tax variables and the outcome of interest for each municipality over the 2001-2015 period.

I retrieve municipality-level data on total population, the proportion of population above the age of 65 and those under the age of 15, and the share of foreign residents for each year over the 2001-2015 period from the National Institute of Statistics database. These variables allow to account for demographic changes over the period of interest.

To account for idiosyncratic shocks in labor demand and supply, which may influence both taxable income, the degree of inequality and the decision to change the local tax code, I collect province-level data on the unemployment share from the National Institute of Statistics database.

Political budget cycles and individual characteristics of the mayor and the other members of the town council might affect growth, tax rate decision and public spending (Alesina et al., 2015; Alesina and Paradisi, 2017). To account for strategic choices of fiscal variables in relation to elections, and for mayor- and town council-specific preferences for fiscal redistribution, I retrieve individual-level data from the Ministry of Interior on gender, educational attainment and age of the mayor and each other member of the town council for each year over the period of interest. Moreover, election-year fixed effects are added to the main empirical specification to control for municipality-specific political budget cycles.

Periods of budget deficits might stimulate local policy-makers to raise more distortionary taxes to increase revenue. I exploit information from *The Gazzetta Ufficiale della Repubblica Italiana* - the official journal of record of the Italian government - on municipalities reporting fiscal difficulties in a given year. Then, I create the dummy variable $Crisis_{i,t}$, which assumes value 1 if the municipality *i* has reported fiscal difficulties in year *t*, and 0 otherwise. In addition, I exploit information on fiscal imbalance from municipal balance sheets to create the dummy variable $Deficit_{i,t}$, which is equal to 1 if total spending in year *t* is larger than total revenue in the same year for municipality *i*, 0 otherwise. These two dummies aim to control for any spurious relation between tax rates and taxable income or income inequality coming from fiscal budget issues.

Finally, I exploit data from the Ministry of Interior on municipal balance sheets to derive a municipality-specific and time-varying measure of expenditure shares. Namely, I account for the share of municipal expenditure in administration, development, law and order, education, and social welfare. These shares allow to control for any change in the composition of public service provision, which may affect taxable income, migration decisions and tax rates.

4 Empirical strategy

This section illustrates the empirical strategy implemented to identify the effects of local taxes on taxable income and inequality.

4.1 Net-of-tax rate elasticities

Following the existing literature aiming to estimate the elasticity of taxable income with respect to marginal tax rates (critically reviewed in Saez et al., 2012), I identify behavioral responses to local tax rate changes by regressing the (log of) municipal taxable income on the (log of) net-of-local marginal tax rate, and controlling for municipality and year fixed effects, province-specific time trends and time-varying municipality characteristics. In the same way, I estimate the distributive effects of local taxes by regressing the (log of) inequality measures on the same variables as above. In this model, the impact of local tax rates on taxable income and inequality is identified by within-municipality variations in tax rates and taxable income or inequality. Formally, I run regressions of the following type:

$$log(y_{i,t}) = \epsilon \times log(1 - \tau_{i,t}) + \beta X_{i,t} + \gamma_i + \delta_t + t \times \mu_{p(i)} + u_{i,t}, \quad (3)$$

where *i* represents a municipality and *t* a year. $y_{i,t}$ denotes taxable income or the inequality index. $\tau_{i,t}$ is the local (municipal plus regional) top marginal tax rate on personal income, so that $(1 - \tau_{i,t})$ represents the net-of-tax rate. The parameter of interest is ϵ, which is interpreted as the approximate percentage change in $y_{i,t}$ when $(1 - \tau_{i,t})$ increases by 1%. Municipality fixed effects, γ_i, control for permanent differences that affect taxable income or inequality; if the systematic determinants of $y_{i,t}$ are additive, time-invariant municipality characteristics, then these factors are controlled for by the municipality fixed effects. The year dummies, δ_t, capture the influence of common aggregate changes, such as variations in central taxes or any other national reform which has equally affected all the municipalities regardless of their characteristics or geography. $X_{i,t}$ contains time-varying municipality-level controls (see section 3.3). The inclusion of a set of linear time trends specific for each province p in which municipality *i* is located, $t \times \mu_{p(i)}$, is motivated by the concern that the choice of changing the tax rate is related to differential pre-trends in the outcome variables and the possibility that the observed effects are due to these pre-trends rather than the tax rate change itself. Finally, $u_{i,t}$ is a municipality-transitory shocks that has mean zero at each time t and it is correlated over time. To account for serial correlation in the error term, standard errors are clustered at the municipality-level, allowing for an arbitrary covariance structure over time within each municipality (Bertrand et al., 2004).[11]

One potential concern is that municipality might set tax rates with behavioral responses in mind. A municipality that expects a large response might be reluctant to change the tax rate than a municipality that expects a little response. In other words, the decision to change the tax rate (or the tax scheme) might be not random with respect to the disturbance term, i.e. $E(log(1 - \tau_{i,t}), u_{i,t}) \neq 0$. The standard way to deal with this issue would be to find a suitable instrument for the tax rate. This suggests that a plausible model of tax rate setting can be approximated by:

$$log(1 - \tau_{i,t}) = \alpha Z_{i,t} + \beta X_{i,t} + \gamma_i + \delta_t + t \times \mu_{p(i)} + v_{i,t}, \tag{4}$$

where the instrument $Z_{i,t}$ is required to be informative, i.e. $\alpha \neq 0$, and valid, that is $E(Z_{i,t}, u_{i,t}) = 0$. My approach is to use the average net-of-local tax rate set by municipalities similar in terms of population size, income level and geography as instrument for the own net-of-local tax rate.[12] Under the idea to purge correlation between the error term and the endogenous variable by assigning municipalities to groups and to use group-specific means as instrument, this approach is similar in spirit to the Wald-type grouping instrumental variables estimator used in the labor supply literature (Heckmann and Robb, 1985; Angrist, 1991; Blun-

[11]Note that since the inequality measures used in this study are estimates, it is possible that the error term contains measurement errors. Even in presence of measurement errors in the dependent variable, OLS can still consistently estimate ϵ, but estimates will be less precise than with perfect data.

[12]I investigate several other potential instruments (i.e., party political affiliation, mayor gender, grants from higher tier of government and error in municipal revenue forecasting) but all explain little of the variation in the endogenous variables and, hence, neglected since it can lead to large inconsistencies in the estimates (Bound et al., 1995)

dell et al., 1998).[13] The correlation in the tax rates is remarkably high in the data (correlation of 0.6), thus satisfying the relevance condition. This instrument exploits the variation in the net-of-tax rate driven by changes in the net-of-tax rate of similar municipalities and it is plausibly exogenous to any individual municipality.[14]

4.2 Progressive tax reform

To analyze the impact of tax progressivity, I exploit the change in the normative setting of the municipal tax originated with the 2007 and 2011 tax reforms to implement a difference-in-differences (DiD) empirical strategy. These reforms provide treated and control groups for testing taxable income and inequality responses to different levels of tax progressivity. The municipalities which have switched from the flat to the progressive tax scheme are considered as treated, while those which carry on to apply a flat tax rate are considered a plausible control group. The effect of the reform is then estimated as the difference in the outcomes for these two groups. Formally, defining $Reform_{i,t}$ as a dummy variable equal to 1 in each treated municipality *i* for each year *t* following the tax reform, I estimate the parameter ϵ from a regression of the following form:

$$log(y_{i,t}) = \epsilon \times Reform_{i,t} + \beta X_{i,t} + \gamma_i + \delta_t + t \times \mu_{p(i)} + u_{i,t} \tag{5}$$

where the notation is the same as in (3).

The identifying assumption to estimate ϵ is that treated and untreated municipalities are similar except for the local tax schedule and that, if the two groups of municipalities had the same tax system, then their post-reform taxable income or inequality level would evolve similarly. If this assumption holds, then the exogenous variation in tax progressivity is what permits a well-identified estimate of the tax reform effect on taxable income and inequality. In other words, the decision to implement the progressive tax should be considered *as good as random*. Public policies, however, are hardly random choices (Besley and Case, 2000) and the previous assumption could be violated if treated and untreated municipalities have different characteristics. Table 1 reports summary statistics separately for treated and control municipalities over the pre-treatment period. There are striking differences in many observable characteristics between the two groups; in particular, treated municipalities are richer, more populated, with larger share

[13] Specifically, I group municipalities by region-population group-income deciles. Population thresholds are selected according to the national rule that determines the size of the city council, the electoral rule, the size of the executive committee, and whether a municipality can have additional elective bodies at the neighborhood level. I use the eleven legislative thresholds set in 2001: below 1,000; 1,000-3,000; 3,000-5,000; 5,000-10,000; 10,000-15,000; 15,000-30,000; 30,000-50,000; 50,000-100,000; 100,000-250,000; 250,000-500,000; above 500,000. Most of the thresholds apply on the policies above have been fixed over the period of interest and date back to 1960. For the few cases in which a ”similar” municipality does not exist, I use the average net-of-tax rate set by neighboring municipalities as instrument.

[14] A similar approach has been recently implemented by Breuillé et al. (2018), which group municipalities according to population density, share of individuals aged under 15 and the level of taxable income per-capita. Similarly, Fajgelbaum et al. (2018) exploit only geographical characteristics and instrument state tax rate with a weighted average of tax rate in other states, where weights are proportional to distance.

of foreign residents, lower unemployment rate and larger share of public spending for education and social activities than control municipalities.

[Table 1 about here]

In a situation where the distribution of the pre-treatment variables differ across treatment and control groups, a widely-used approach to reduce the bias is through the use of matching (Heckman et al., 1998). Matching procedures pair treated and untreated municipalities on baseline characteristics to create a control group that is as similar to the treatment group in observables as they would be under randomization. Letting $i = 1$ denote treated municipalities and $i = 0$ those untreated and letting $D = 0, 1$ treatment status, then matching allows to estimate treatment effect by assuming that, conditional on some observable characteristics *X*, outcomes in treated and untreated municipalities (y_1, y_0) and *D* are independent:

$$(y_1, y_0) \perp D|X, \tag{A1}$$

If (A1) holds, then $F(y_0|X, D = 1) = F(y_0|X, D = 0)$, so, conditional on *X*, outcome in untreated municipalities has the same distribution that treated municipalities would have experienced if they had not switched to the progressive local tax schedule. It follows that: $E(y_0|X, D = 1) = E(y_0|X, D = 0) = E(y_0|X)$.

If, in addition, it is assumed that:

$$0 < Pr(D = 1|X) < 1, \tag{A2}$$

then the reform effect can be defined for all values of *X*. Under assumptions (A1) and (A2), it becomes possible to account for the nonrandom selection by aligning the distribution of observed characteristics in both treated and untreated municipalities, so to randomize the decision to switch to the progressive local tax schedule.

Rosenbaum and Rubin (1985) show that matching can be performed on the propensity score.[15] Therefore, I complement my empirical analysis by estimating a propensity-score weighting version of equation (5) following Hirano and Imbens (2001).[16] This approach attenuates potential bias due to covariates unbalance by re-weighting the control group observations by a function of their estimated probability to switch to the progressive tax scheme. In practice, this is done by estimating a probit model of the propensity that municipality switches to the progressive tax scheme as a function of the set of pre-treatment controls,[17] obtaining the predicted propensity $p(w)$, and then estimating regression (5) with weights equal to unity for the treated and $p(w)/(1 - p(w))$ for the controls.[18] Column 5 of Table 1 clearly shows that the weighting

[15] Let $Pr(D = 1|X) = P(X)$, they show that (A1) and (A2) together imply $(y_1, y_0) \perp D|P(X)$ and, thus, $y_0 \perp D|P(X)$.

[16] A recent application of this methodology in public finance literature is Gadenne (2017).

[17] Apart from the baseline control variables, I also add pre-treatment mean of taxable income and Gini index to the set of covariates used to predict the decision to implement a progressive tax scheme.

[18] Hirano et al. (2003) prove that this estimator is efficient, while Wooldridge (2007) shows that ignoring the first-

procedure leads to a reasonable balance in pretreatment characteristics.

Even after re-weighting the comparison group to create a valid counterfactual for the treatment group, there might be remaining differences between treated and control group. Indeed, the same arguments related to the endogeneity in the tax rate setting can be advanced in the context of the decision to change the local tax scheme. To deal with this problem, I use the same approach implemented to estimate net-of-tax elasticities and I instrument $Reform_{i,t}$ with the $Reform_{i,t}$ group mean of municipalities similar in terms of population size, income level and geography.

The DiD estimator is based on the critical assumption that, in absence of the reforms, the average in the outcomes of interest for treated and untreated municipalities would have followed parallel trends over time. A transparent way to test the validity of the common trend assumption would be to use lags and leads of the reforms:

$$log(y_{i,t}) = \sum_{j=-m}^{q} \epsilon_j \times Reform_{i,t}(t = k + j) + \beta X_{i,t} + \gamma_i + \delta_t + t \times \mu_{p(i)} + u_{i,t}. \quad (6)$$

In this model, instead of a single treatment effect, I have now also included m ”leads” and q ”lags” of the reform.[19] ϵ_j is the coefficient on the jth lead or lag. The parallel trend assumption holds if $\epsilon_j = 0 \; \forall j < 0$, i.e. the coefficients on all leads of $Reform_{i,t}$ are zero.

Finally, an additional critical assumption to estimate ϵ is that there no other forces affecting the treatment and control municipalities differentially pre- and post-reform. While the impact of common aggregate changes are captured by the year dummies, if other contemporaneous policy events occurred at the municipality-level during the same time interval as the reforms under study, then ϵ would be biased. In the robustness section below, I will show that the main results are robust to other contemporaneous policy events and to controlling for province (region)-time fixed effects in the main specification.

5 Results

This section shows and discusses the main findings of this paper.

stage estimation of the selection probabilities when performing inference gives conservative standard errors. All the results will present standard errors non-adjusted for first-stage estimation, since there is little efficiency lost in implementing bootstrapping procedures. The distribution of the estimated propensity score for the treated and control groups shows overlapping (see Figure B1), suggesting that for each treated municipality there is a control with equal characteristics, so that it is possible to obtain a valid inference (Wooldridge, 2010).

[19]Namely, I include dummies that span the whole period but omit 2001, which is absorbed in the constant and hence serves as a reference year.

5.1 Baseline results

The baseline results are illustrated in Table 2. This table is divided in three panels: panel a shows the estimated elasticity of municipal taxable income or inequality with respect to the net-of-local top marginal tax rate, while panel b and c display the effects of switching from a flat to a graduated local tax schedule by using all the untreated municipalities as comparison group (panel b) or re-weighting the control group by a function of their estimated probability to switch to the progressive tax scheme (panel c). Overall, the results consistently show that local taxes have a negative and significant effect on both taxable income and the pre-tax income distribution.

[Table 2 about here]

The dependent variable in columns 1 and 2 is the log of the municipal taxable income. Two-stage least squares (2SLS) regression estimates an elasticity of taxable income with respect to the net-of-local (municipal plus regional) top marginal tax rate of 0.555. This suggests that, on average, a 10% increase in the net-of-local tax rate would rise taxable income by 5.5%. OLS estimate is slightly upward bias, providing an elasticity of 0.643. OLS estimated reform effect shows weak negative effects on taxable income, which is not statistically significant when the matched control group is used. In contrast, 2SLS regression estimates a non-negligible negative effect of tax progressivity on taxable income: municipalities under a progressive tax scheme have experienced a reduction of 6.8% in taxable income with respect to those with a flat tax scheme (panel b). The effect further increases to 7.7% using the re-weighted control group (panel c).

The distributive effect is illustrated in columns 3-10. The results consistently show that tax progressivity has a negative and significant effect on the pre-tax municipal income distribution. The effect, however, is mainly driven by rich taxpayers (i.e., those in the top percentile of the income distribution). Namely, on average, a 10% percent rise in the net-of-local top marginal tax rate increases the income share held by the top percentile by nearly 5% (panel a, column 10). The effect of tax rates on the other fractiles of the income distribution are, if any, negligible. This heterogeneity over the pre-tax distribution leads to a Gini elasticity estimate of 0.075. In-line with net-of-tax elasticity estimates, panel b and c show that the progressive tax reform significantly reduces the pre-tax income distribution at the top by around 2.2-2.9%, while no significant effect is found in other slices of the income distribution.

Taken together, these results confirm the predictions of the theoretical literature on the efficiency and distributive effects of local tax progressivity. More progressive local tax rates created incentives for the rich to move out. A more equal pre-tax distribution of income was achieved, but it appears to be largely driven by variations in the income share held by those in the top percentile of the income distribution, with a consequent reduction in municipal taxable income.[20]

[20] In the online Appendix (see Table B3), I report results using alternative definitions of the local tax rate and the

5.2 Heterogeneity analysis

This section investigates heterogeneous responses by interacting the net-of-tax rate or the reform dummy with indicators reflecting specific municipality characteristics.

First, models of tax competition (Bucovetsky, 1991; Kanbur and Keen, 1993) emphasize asymmetries among jurisdictions according to their population base, concluding that tax rates are higher in more populous regions. If such an asymmetry is linked with a lower responsiveness of larger municipalities, then those municipalities may actually have a higher optimal tax rate. For this end, I create a dummy variable for municipalities whose population size is lower than 1,000 inhabitants (labelled $Small_i$) or larger than 50,000 inhabitants ($Large_i$).

Second, I consider the possibility that municipalities with larger share of rental income might be more responsive to changes in tax rates, since property owners presumably face lower mobility costs. To test this hypothesis, I interact the tax variables with the dummy $Rentier_i$, which takes value 1 in each municipality *i* having a share of rental income larger than the median value.

Third, previous literature (see, for instance, Andreoni et al., 1998) argues that standard crime models applied to tax evasion may be biased when citizens have social norms that affect the way through which they respond to government policy. Taxpayers may be less likely to engaging in further avoidance or evasion behaviors as a response to increasing tax rates in places characterized by cooperative norms. I investigate whether the response is significantly different in municipalities characterized by higher cooperative gathering - as measured by the share of non-profit organizations - by interacting the tax variables with the dummy $Coop_i$, which assumes value 1 in each municipality *i* having a share of non-profit organizations larger than the median value.

Finally, I test whether estimates are significantly larger across regional borders, that is where the spatial tax differential is higher. I create the dummy variable $Border_i$, which takes value 1 for those municipalities sharing a border with a municipality located in a different region.

The results are reported in Table 3.[21] The negative coefficient resulting from the interaction between the net-of-tax rate and the dummy for larger municipalities confirms the theoretical predictions that the losses associated with the erosion of the tax base are relatively lower for such municipalities, since a larger population size potentially means a higher demand and thus provides an advantage to attract and retain businesses.

”Rentiers” municipalities are strikingly more sensitive to variations in both the top local marginal tax rate and tax progressivity; this is especially remarkable in the share of income held by the top percentile. This might be motivated by the fact that rentiers presumably face lower, if

progressivity measure. Net-of-tax rate elasticities are qualitatively similar using the combined local income and property tax rate, but estimates are slightly larger: taxable income elasticity is 0.71, while the elasticity of the income share held by the top percentile is 0.66. Likewise, reform effects appear similar using the average rate progression instead of the baseline reform dummy.

[21] For the sake of the space, I only report results for the taxable income and the share of income held by the top percentile. Moreover, note that the following results are qualitatively the same using population size, rental income and number of non-profit organizations instead of the dummy variables.

any, costs for changing tax residence, thus the corresponding overall elasticity becomes larger for the municipalities with a larger share of rental income.[22]

I find that taxpayers are less responsive in places with more cooperative associations, suggesting that there is complementarity between norms of cooperation and behavioral responses to tax changes.

Finally, taxable income elasticity is significantly larger across regional borders, while no effect is found for the top percentile and for the progressive tax reform.

To summarize, the revenue leakage due to behavioral responses to local taxes is relatively larger in places with more concentration of rental income and across regional borders, it is lower in municipalities with larger population size and a higher value of civic capital.

[Table 3 about here]

In the online Appendix (see Table B4), I report geographical heterogeneities. First, I show that the estimates are qualitatively similar even dropping municipalities belonging to regions with special autonomy, which have more autonomy to set their own fiscal rules. Second, I estimate coefficients from sub-samples of municipalities located in the Northern, Center and Southern Italy. Taxable income elasticity is larger in the richer North (ϵ = 0.669), significantly lower in the Center (ϵ = 0.152) and not statistically significant in the poorer South, while effects on the pre-tax income distribution are significantly larger in the more unequal *Mezzogiorno* (ϵ = 2.155 for the Top 1 and 0.459 for the Gini index).

5.3 Robustness checks

Motivated by the identification issues discussed in section 4, this section attempts to reinforce the causal validity of the main results.

5.3.1 Leads and lags of the tax reforms

A crucial assumption for the validity of the DiD empirical strategy is that trends in the outcome variables prior to the implementation of the progressive tax schedule were the same in both treated and control municipalities. A transparent way to test this assumption is by using lags and leads of the reforms of interest. To this end, I run regressions as in equation (6) and restrict the sample to the treated municipalities that adopted the reform in the first round and the re-weighted control group. This restriction allows to disentangle the effects of the introduction of a tax exemption for poor individuals (2007 Reform) from those due to the adoption of the graduated tax scheme (2011 Reform).

Figures 5 and 6 display the progressive tax reform coefficients, their leads and lags, and confidence intervals.[23] Taxable income trends were not significantly different between treated and

[22]Evidence of a positive correlation between rentiers and attitude towards tax evasion is documented in Marino and Zizza (2012): they estimate that rentiers evade, on average, 80% of their income, while the average population value is 13.5%.

[23]Numerical coefficients are reported in Appendix Table B6.

control municipalities during the pre-reform period, thus satisfying the parallel trend assumption. Taxable income appears to slightly reduce over the period 2007-2010 in municipalities providing a tax exemption for poor, but effects are not statistically significant. The introduction of a progressive tax reform further decreased taxable income in treated municipalities; effects are statistically significant for the post-reform year 2012, 2014 and 2015.

Inequality effects and parallel pre-reform trends are illustrated in Figure 6. Consistently with the baseline estimates, I do not find any significant effect for the Gini index (panel a), the income share held by the bottom 90 percent (panel b) and the bottom 90 percent of the top decile (panel c), while tax reform effects are negative and statistically significant in the very top of the income distribution (panel d). The figure shows a significant negative reduction in the income share held by the top percentile in treated municipalities both in the two years after the implementation of a tax exemption for low incomes and in the period following the introduction of a graduated tax scheme.

[Figures 5 and 6 about here]

5.3.2 Other contemporaneous policy events

In addition to the parallel trends between treated and control group, another critical assumption of the DiD estimator is that there are no other contemporaneous policy changed.

First, municipalities might compensate any variation in personal income taxes with changes in property tax rates. Property taxes were subject to several reforms over the recent years which, as long as they are common to all the municipalities, are controlled by the time dummies.[24] Apart from the property tax rate applied on the main dwelling, municipalities set a basic property tax rate on the other properties. As taxpayers choose their actual (or fiscal) residence between different municipalities, they are likely to compare the marginal cost indicated by the combined tax rates of the property and income tax rates with the marginal benefit of public services. Hence, to obtain an unbiased estimate of the taxable income elasticity, it is required to control for the property tax. For this end, I retrieve municipality-level data on the property tax rate applied to the main dwelling, the amount of tax allowance provided and the basic rate for each year over the period of interest. These variables are then added to list of the control variables used in the baseline specification.

Second, in 2007, the Italian central government carried out a nationwide anti tax evasion policy that used innovative monitoring technologies to target buildings hidden from tax authorities. The program led to a substantial increase in local tax revenue, which substantially varies in its intensity across municipalities and might a direct effect on taxable income and local taxes (Rubolino, 2018). I retrieve administrative data from the Ministry of Revenue on the share of ”ghosts” buildings - share of properties not included in the land registry as a share of total

[24]The most salient were the 2008 and 2012 reforms (decree 93/2008 and 276/2011), which first removed and then restored the municipal property tax on owner-occupied dwellings. However, the rate continued to apply on luxury properties (i.e., those belonging to cadastral unit A/1, A/8 and A/9).

buildings - for each municipality. Then, I add to the baseline model the interaction of this proxy for tax evasion intensity with a dummy equal to 1 for all the years after the anti tax evasion program.

Table 4 compares the baseline reform effects and net-of-tax rate elasticities with those estimated controlling for property taxes and the evasion proxy. To keep a common sample across models, I drop municipalities for which data on "ghosts" buildings is missing. Controlling for these additional variables slightly vary the elasticity estimates: taxable income elasticity increases from 0.556 to 0.575, while the top 1 elasticity decreases from 0.486 to 0.468. Similarly, progressive tax reform effect on taxable income reduces from 8 to 7.6%, and from 3.2 to 3% on Top 1. Therefore, we can safely assume that these potential confounders do not have any significant effect on the baseline estimates.

[Table 4 about here]

Besides from these contemporaneous policy events, it is still possible that there are other changes in local economic policies other than taxes which are correlated with taxes or with taxable income or inequality. For example, a business enhancing regional (or provincial) reform could both cut taxes and relax regional (provincial) level regulations on labor, thus leading to a biased estimate of the true tax effect. To account for this possibility, in the online appendix (see Table B5), I report estimates from a specification which includes region (province) $\times$ year effects. Reform effect on taxable income reduces of 0.4 (0.6) percentage points once I control for region- (province-) specific time fixed effects, while I do not find any variation in the Gini index. This translates into a more conservative estimate of the reform effect on taxable income of 7.1%, instead of the baseline 7.7%. Baseline estimates on income shares are, instead, conservatives: effects significantly increase for the bottom decile of the income distribution of around 0.2 and 1.1 percentage points, while they are between 1.4 and 2 percentage points larger for the top percentile. Similarly, the alternative specifications provide a more conservative estimate for the taxable income elasticity, while baseline estimates on inequality measures are conservative.[25]

5.4 Mechanisms

In this section, I test whether the main results can be rationalized by the presence of tax-induced internal migration and cross-municipality income shifting.

5.4.1 Tax-induced internal migration

If taxpayers are mobile across borders, spatial differences in the local tax rate can affect the geographical allocation of taxpayers across the country. As early discussed, personal income

[25] As for the net-of-tax elasticities, in the alternative specification I estimate net-of-tax elasticities considering only the municipal tax rate and let variations in the regional tax rate be captured by the region (province) $\times$ year fixed effects. In this case, the baseline taxable income elasticity is estimated around 5 and it reduces to 4 (3.5) as region(province)-time fixed effects are added.

surtaxes apply on the taxpayer *tax* residence, which might be different from their *physical* location. The application of a residence-based PIT system combined with differences over time and place in the local tax rate might thus induce taxpayers to intentionally misstate the location of the tax base by shifting their fiscal residence to a less taxed place. Hence, the migration responsiveness might be large, being the sum of both real (i.e, physical transfer) and artificial (i.e., only fiscal transfer) responses.

To test whether individuals actively move as a reaction to local tax rate differentials across borders, I compute bilateral migration flows for each pair of province (110 $\times$ 110) for every year over the 2007-2015 period by using administrative data on transfers of residence from the National Institute of Statistics.[26] Following the previous literature (see, in particular, Moretti and Wilson, 2017), I estimate the elasticity of migration with respect to the net-of-tax rate by relating the changes in the population share that moves from a province to another to variations in tax differentials between the two provinces in each year. Formally, I run regressions as the following:

$$log(P_{o,d,t}/P_{o,o,t}) = \epsilon \times log[(1-\tau_{d,t})/(1-\tau_{o,t})] + \mu_d + \mu_o + \mu_{o,d} + u_{o,d,t}, \tag{7}$$

where $P_{o,d,t}/P_{o,o,t}$ is the population share that moves from one province to another, $P_{o,d,t}$, relative to the population share that does not move, $P_{o,o,t}$. $log[(1-\tau_{d,t})/(1-\tau_{o,t})]$ captures the differential in the net-of-average tax rate between destination $(1-\tau_{d,t})$ and origin $(1-\tau_{o,t})$ province.[27] μ_o (μ_d) is a vector of province fixed effects that captures both consumption and production amenities in the province of origin (destination). $\mu_{o,d}$ is a vector of province-pair fixed effects that captures the cost of moving for each province pair. $u_{o,d,t}$ is the error term. In estimating standard errors, I account for the possibility that they might be correlated over time within the panel dimension (Bertrand et al., 2004) and across pair $\times$ year observations within a given year sharing the same origin or destination province. Therefore, I allow for three-way clustering by origin $\times$ year, destination $\times$ year, and origin-destination pair.[28]

By focusing on changes over time, within a given province pair, this model absorbs all the time-invariant determinants that can shift the demand and supply of individuals across provinces. Thus, the model controls for the permanent heterogeneity in migration flows at the provincial pair level. For example, if individuals tend to move from one origin province lo-

[26]These data are based on administrative forms (called *modello APR.4*) filled out and organized by the Civil Registry (*Anagrafe comunale*) and provide information on when and where individuals transfer their residence. Unfortunately, the data provided by the National Institute of Statistics are not specific for each municipality, but aggregated at the provincial level. Furthermore, due to the change in the provincial administrative setup carried out in 2006, consistent series on migration flows between provincial pairs are available from 2007. In the final dataset, I exclude cells where origin = destination province and where there are no migration flows.

[27]For this analysis, I use the average tax rate - rather than the marginal tax rate - since taxpayers deciding on where to reside should in principle focus on the former. The average tax rate is computed as the sum of regional and (population-weighted average) municipal surtaxes on personal income calculated at the income level equal to the national average for each year.

[28]Formally, I allow for unrestricted serial correlation within the *o-d* pair: $corr(u_{o,d,t}, u_{o,d,t+j})$ can differ from 0, for any j; but I assume that $corr(u_{o,d,t}, u_{p,q,t+j}) = 0$ if $p \neq o$ or $q \neq d$.

cated in the South Italy to a destination province in the North because the latter has historically important clusters of innovation-driven industries, then provincial pair effects will account for these factors as long as they are permanent.

Table 5 reports the estimated ϵ coefficient. The results show that the probability of moving from province o (origin) to province d (destination) increases when the net-of-tax rate in d increases with respect to o. Namely, a one percentage point increase in the net-of-tax rate differential is associated with a 2.1 to 3.7% increase, depending on the specification, in the stock of population that moves (panel a and b) and with a 2.4 to 5.6% increase in the share of population that moves relative to non-movers (panel c and d).

To reinforce the validity of these estimates, I run several robustness exercises. The inclusion of time fixed effects and province (destination or origin)-specific time trends absorbs most of the variation. Stock elasticity significantly reduces by nearly one-third, while share of movers elasticity halves, but they still remain statistically significant.

Second, equation (7) leads to a structural interpretation of the estimated coefficient in the locational equilibrium: ϵ includes both the effect of variations in the tax differential between province-pair and the indirect effects through changes in the provincial wages, i.e. the effect assuming fixed province characteristics (amenities) as given except for tax rates and wages. Given the interpretation of ϵ, the capitalization into wages does not pose a threat, but other unobservable time-varying shocks that are correlated with tax changes would be problematic. To account for this, I control for variations in the differential in unemployment rate and in the share of public expenditure in administration, law and order, education, social welfare and development (column 4). As we control for these factors, stock and share of movers elasticities increase from 2.3 to 3.6 and from 2.7 to 4.

Third, I drop specific provinces. Over the period of interest, the region Emilia-Romagna was hit by a devastating earthquake in 2012 that forced many residents to move out. To account for this event, I drop out from the sample all the destination and origin provinces belonging to Emilia-Romagna. I also remove the provinces belonging to regions with special autonomy (*regioni a statuto speciale*) since they have more autonomy to set their own fiscal rules and might influence more directly the migratory patterns. Columns 5 and 6 show that the stock and share of movers elasticity significantly reduced by 20 and 25%, respectively, once I run regressions on this sub-sample.

Fourth, as individuals choose their actual residence between different local jurisdictions, they are likely to compare the marginal cost indicated by the combined tax rates of the property and income tax rates with the marginal benefit of public services. Hence, to obtain an unbiased estimate of the migration elasticity, it is required to control for the property tax rates. Panel b and d report elasticities estimated with respect to the combined property and personal income tax rate differential: results look very similar to the baseline estimates.

Controlling for province-pair fixed effects and time-varying controls, identification requires that, absent any variation in the tax differential, province-pair stocks are constant over time.

However, tax rates are not set randomly and any province-specific unobservable that is correlated with taxes and migration may threaten the causal interpretation of these results. To deal with this, I follow the same strategy implemented for estimating net-of-tax elasticities (see section 4.1) where I instrument the own net-of-average tax rate with the net-of-average tax rate set by similar municipalities. 2SLS regression elasticity estimates - reported in column (7) - are qualitatively similar to OLS estimates and are my preferred estimates. On average, a 1% increase in the net-of-tax differential between destination and origin province rises the stock of movers by around 2.4%.

Overall, these estimates suggest that the probability of migrating is sensitive to local tax differentials. This result goes in-line with the existing evidence related to countries applying the residence-based tax.[29] The nature of the data, however, does not allow to disentangle a real from a fraudulent move, where a taxpayer changes the tax residence to a second property without physically moving. This distinction is important from a labor supply perspective - real response would matter more than simple misreporting -, but it does not matter from a tax revenue perspective.

[Table 5 about here]

5.4.2 Income shifting

Distinguishing real responses (e.g. labor supply-related) from cross-municipality income shifting is crucial for welfare implications and policy recommendations. Previous literature in sales and property tax (Wilson, 1999; Brueckner and Saavedra, 2001) shows that individuals take advantage of tax wedge created by differing tax burdens across jurisdictions through cross-border shopping, relocation, and other means. If this kind of tax-induced resource flows in which changes in a municipality tax base are due to tax policies in other competitor municipalities accounted for a majority of the overall taxable income elasticity, then the marginal excess burden would significantly decrease. Indeed, if income is shifted toward another (less) taxed base, welfare losses are smaller compared to the case in which elasticity reflected only real responses (Chetty, 2009; Piketty et al., 2014).

To distinguish between real response and cross-municipality income shifting, I follow the previous literature (see Harju and Matikka, 2016, for a theoretical derivation of the following model) and rearrange equation (3) to allow for income-shifting responses:

$$
\begin{aligned}
log(y_{i,t}) = \epsilon_1 \times log(1-\tau_{i,t}) - \epsilon_2 \times log[(1-\sum_{j\neq i} w_{i,j}\tau_{j,t})/(1-\tau_{i,t})] + \\
+ \beta X_{i,t} + \gamma_i + \delta_t + t \times \mu_{p(i)} + u_{i,t},
\end{aligned}
\tag{8}
$$

[29] For instance, Martinez (2017) finds a large elasticity of the inflow of rich taxpayers with respect to the average net-of-tax rate ranging from 3.2 to 6.5 in Switzerland, while Agrawal and Foremny (2018) study tax-induced location choice in Spain and find that a 1% increase in the net-of-tax rate differential raises the probability of moving of 1.7%.

where $\sum_{j \neq i} w_{i,j}\tau_{j,t}$ is the weighted average of the competitor tax rate and $w_{i,j}$ is the weight of municipality j's tax rate in the weighted average. The income shifting elasticity, ϵ_2, measures how the own tax base reacts to changes in net-of-tax rate differences with respect to competitor municipalities, while ϵ_1 is the real elasticity.

I employ two weighting schemes. First, I use weights related to contiguity since the potential cost of shifting tax base across taxing municipalities should be, *ceteris paribus*, inversely related to distance. Under this weighting procedure, each municipality *j* sharing a border with municipality *i* receives the same weight. Second, I assign the same weight to all the municipalities belonging to the same local labor market. This classification is made by the National Institute of Statistics as aggregation of two or more municipalities sharing daily movements of commuters, independently from their administrative definition.

Table 6 compares the baseline taxable income estimates without income-shifting with those estimated from the model which includes the difference in the net-of-tax rates to the full set of covariates. Columns 2 and 5 define municipalities sharing a border as competitors, while columns 3 and 6 those in the same local labor market. Results consistently show that the income-shifting elasticity is negative, but it is statistically significant only across municipalities located in the same local labor markets. This holds both using OLS (columns 1-3) and 2SLS regressions (columns 4-6). Elasticity estimate implies that the own tax base reduces by around 8.3% as the tax-of-tax differential increased by 10%. This would suggest income (and, possibly, labor force) relocation within local labor markets, thus implying that the deadweight loss of taxation born by local governments does not necessarily translate into a similar overall welfare loss under a national perspective.

[Table 6 about here]

6 Discussion and conclusions

Local income tax policy decisions often rely on limited information on how taxpayers actually respond to local tax rates. Responsiveness to tax changes has crucial implications for policy-makers, encompassing the resulting changes in expected tax revenue and the level and location of local economic activity. This paper uses panel data from municipality-level tax returns to study the effect of local income tax policy on taxable income and its distribution.

The results suggest that the introduction of a graduated tax scheme has significantly reduced both taxable income and the pre-tax level of income inequality. On average, a 10% increase in the net-of-local marginal tax rate is associated with a 5% percent increase in taxable income and a nearly equal rise in the income share held by the top percentile of the income distribution. These results confirm the predictions of the theoretical literature (Musgrave, 1959; Oates, 1972; Stiglitz, 1988; Feldstein and Wrobel, 1998) on the efficiency and distributive effect of local tax progressivity. The introduction of a local progressive tax scheme on personal income achieves a more equal pre-tax distribution of income, but is largely driven by variations in the income share held by those in the top percentile of the income distribution, with a consequent reduction in municipal taxable income.

More progressive local tax rates creates incentives for the rich to move towards less-taxed jurisdictions. Tax-induced internal migration and cross-municipality income shifting within local labor markets to minimize the tax liability are found to be the key mechanisms behind the observed effects. On average, a 1% increase in the net-of-tax differential between between destination and origin fiscal jurisdictions rises the stock of movers by around 2.4%. Cross-municipality income shifting estimate suggests that the own tax base reduces by around 8.3% as the tax-of-tax differential increased by 10%.

These findings have implications for optimal tax design. Other things equal, the higher is the elasticity of taxable income to the net-of-tax rate, the lower should be the optimal tax rate, and the higher is the locational responsiveness of a tax base, the lower is the optimal tax rate on that base. Tax-induced mobility is a key element for optimal tax scheme design. From an overall perspective, free mobility improves global welfare by relocating individuals towards more productive places. However, mobility induces tax competition across fiscal jurisdictions to attract tax bases, where *fiscal* location depends on the comparison of net-of-tax returns and might be different from the *physical* location. Even if it may be welfare maximizing for an individual jurisdiction to attract taxpayers by offering a suitable tax scheme, this puts at risk the ability of other jurisdictions to collect taxes. Hence, distortionary taxes combined with mobility may produce an inefficiency when fiscal and physical locational choices are not jointly made.

Another role for these findings is in the determination of the optimal provision of public goods. Arguments supporting local discretion to set tax rates are related to the decentralization theorem of Oates (1972). When local tax rates and public good provisions can align to local preferences and cost conditions, a decentralization gain can be achieved compared to a situation with a global public good provision (i.e. set at the national-level). However, since a higher elasticity of taxable income implies larger marginal costs in public good funding, then the efficiency level of public goods provision becomes smaller than it would have been with nondistortionary taxes.

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Tables

Table 1: Balancing test for pre-reform values

Variable	Group			t-test	
	Treated	Unmatched control	Matched control	Treated vs Unmatched	Treated vs Matched
	(1)	(2)	(3)	(4)	(5)
Taxable income per-capita	21,462	19,223	21,458	0.000	0.959
Gini index	0.395	0.397	0.396	0.022	0.502
P0-90 (%)	66.853	68.310	66.971	0.000	0.249
P90-99 (%)	25.268	25.419	25.215	0.019	0.419
P99-100 (%)	8.117	7.130	8.073	0.000	0.461
Population	12,911	4,141	12,254	0.000	0.674
Share of 65+	20.564	22.884	20.331	0.000	0.092
Share of 15-	13.708	13.065	13.791	0.000	0.196
Share of foreign	5.141	3.935	5.179	0.000	0.672
Mayor age	48.825	48.514	48.665	0.071	0.401
Mayor sex	0.110	0.082	0.117	0.000	0.288
Mayor graduated	0.449	0.398	0.458	0.000	0.430
Average age in town council	44.373	43.622	44.443	0.000	0.403
Proportion of women in town council	0.183	0.172	0.182	0.000	0.624
Proportion of graduated in town council	0.289	0.222	0.289	0.000	0.894
Crisis	0.001	0.001	0.001	0.729	0.991
Unemployment rate (%)	6.629	7.471	6.695	0.000	0.542
Budget deficit	0.103	0.060	0.100	0.000	0.438
Administration expenses (%)	21.933	22.348	21.917	0.008	0.917
Development expenses (%)	0.378	0.283	0.327	0.000	0.003
Justice expenses (%)	0.082	0.045	0.084	0.000	0.724
Education expenses (%)	6.418	4.728	6.391	0.000	0.724
Social expenses (%)	7.643	4.839	7.622	0.000	0.890
Urban	0.696	0.383	0.713	0.000	0.165
Density	0.010	0.026	0.011	0.000	0.463

Note: This table compares the mean value of the variables for treated, unmatched control, and matched control municipalities over the pre-reform period. Columns 4 and 5 show p-values from a t-test where the null hypothesis is of equality of coefficient between treated and unmatched control group (column 4) and treated and matched control group (column 5).

Table 2: Baseline results

	Outcome variable:									
	log(TI)		log(Gini)		log(P0-90)		log(P90-99)		log(P99-100)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
a. Net-of-tax rate elasticities										
$log(1-\tau_{i,t})$	0.643***	0.555***	0.092**	0.075*	-0.089**	-0.100**	0.030	0.005	0.531***	0.494**
	(0.069)	(0.070)	(0.041)	(0.041)	(0.038)	(0.039)	(0.077)	(0.083)	(0.191)	(0.204)
Observations	115,230	115,230	115,230	115,230	114,930	114,930	95,751	95,615	95,751	95,615
b. Progressive tax reform: Unmatched control group										
$Reform_{i,t}$	-0.012***	-0.068***	-0.001	0.002	0.002***	0.013***	-0.002	-0.010**	-0.007**	-0.022**
	(0.001)	(0.004)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.004)	(0.003)	(0.010)
	115,230	115,230	115,230	115,230	114,930	114,930	95,751	95,615	95,751	95,615
c. Progressive tax reform: Matched control group										
$Reform_{i,t}$	-0.001	-0.077***	0.000	-0.000	0.001**	0.005	-0.002**	0.010	-0.003	-0.029*
	(0.001)	(0.013)	(0.001)	(0.004)	(0.001)	(0.003)	(0.001)	(0.008)	(0.003)	(0.016)
Observations	115,230	115,230	115,230	115,230	114,930	114,930	95,751	95,615	95,751	95,615
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Election-year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
t × Province	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mean dependent (1,000€ or %)	93,598	93,598	39.693	39.693	67.658	67.658	25.483	25.483	7.693	7.693

Note: Panel a shows net-of-local tax rate elasticities, while panel b and c show the effect of switching from a flat to a progressive local personal income tax scheme (2007 and 2011 tax reforms). The model controls for municipality fixed effects, year fixed effects, election-year fixed effects, province-specific time trend and the following municipality-specific time-varying variables: town councils characteristics (mayor sex, mayor age, mayor degree, town council average age, proportion of women in town council, share of graduated in town council), expenditure shares (share of spending in administrative services, justice and law services, educational services, development activities, social services), demographic variables (population, share of young, share of old, share of foreign), fiscal imbalances, and province-level unemployment rate. The sample is composed of 7,682 municipalities over the 2001-2015 period. Standard errors clustered at municipality-level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Heterogeneity analysis

	Outcome variable:							
	log(TI)				log(P99-100)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	a. Net-of-tax rate elasticities (2SLS)							
$log(1-\tau_{i,t})$	0.556***	0.211**	0.740***	0.488***	0.494**	-0.012	1.044***	0.545**
	(0.070)	(0.097)	(0.099)	(0.076)	(0.204)	(0.285)	(0.259)	(0.212)
... $\times$ $Large_i$	-0.005***				0.001			
	(0.001)				(0.002)			
... $\times$ $Small_i$	0.000				-0.008**			
	(0.001)				(0.003)			
... $\times$ $Rentier_i$		0.619***				0.841**		
		(0.125)				(0.338)		
... $\times$ $Coop_i$			-0.343***				-1.100***	
			(0.115)				(0.304)	
... $\times$ $Border_i$				0.357**				-0.279
				(0.158)				(0.436)
Observations	115,230	115,230	115,230	115,230	95,615	95,615	95,615	95,615
	b. Progressive tax reform (2SLS - matched control group)							
$Reform_{i,t}$	-0.044***	-0.040***	-0.080***	-0.078***	-0.018	0.012	-0.031*	-0.028*
	(0.012)	(0.012)	(0.013)	(0.014)	(0.018)	(0.021)	(0.016)	(0.017)
... $\times$ $Large_i$	-0.065***				-0.025			
	(0.020)				(0.020)			
... $\times$ $Small_i$	0.065***				0.025			
	(0.015)				(0.056)			
... $\times$ $Rentier_i$		-0.036***				-0.040***		
		(0.006)				(0.013)		
... $\times$ $Coop_i$			0.019***				0.018*	
			(0.005)				(0.010)	
... $\times$ $Border_i$				0.004				-0.003
				(0.006)				(0.012)
Observations	115,230	115,230	115,230	115,230	95,615	95,615	95,615	95,615
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES
Election-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
t $\times$ Province	YES	YES	YES	YES	YES	YES	YES	YES

Note: This table shows the taxable income (columns 1-4) and top percentile (columns 5-8) elasticities from 2SLS regressions and the interaction between the net-of-top marginal tax rate (panel a) or the reform dummy (panel b) with dummy variables for municipalities whose population size is lower than 1,000 inhabitants ($Small$), larger than 50,000 inhabitants ($Large$), those having a share of rental income larger than the median value ($Rentier$), those having a share of non-profit organizations larger than the median value ($Coop$), and those having at least one bordering municipalities located in a different region ($Border$). Standard errors clustered at municipality-level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Controlling for municipality-level variations in tax evasion and property tax rates

	Specification:					
	Baseline	+ Evasion	+ Pr. taxes	Baseline	+ Evasion	+ Pr. taxes
	(1)	(2)	(3)	(4)	(5)	(6)
			a. log(Taxable income)			
$Reform_{i,t}$	-0.080***	-0.080***	-0.076***			
	(0.014)	(0.014)	(0.014)			
$log(1-\tau_{i,t})$				0.556***	0.580***	0.575***
				(0.070)	(0.070)	(0.070)
Observations	110,609	110,609	110,609	110,609	110,609	110,609
			b. log(Gini index)			
$Reform_{i,t}$	-0.001	-0.001	-0.001			
	(0.004)	(0.004)	(0.004)			
$log(1-\tau_{i,t})$				0.075*	0.076*	0.075*
				(0.041)	(0.041)	(0.041)
Observations	110,609	110,609	110,609	110,609	110,609	110,609
			c. log(P0-90)			
$Reform_{i,t}$	0.005	0.005	0.005			
	(0.004)	(0.003)	(0.004)			
$log(1-\tau_{i,t})$				-0.094**	-0.084**	-0.080**
				(0.039)	(0.039)	(0.039)
Observations	110,317	110,317	110,317	110,317	110,317	110,317
			d. log(P90-99)			
$Reform_{i,t}$	0.012	0.012	0.011			
	(0.008)	(0.008)	(0.008)			
$log(1-\tau_{i,t})$				-0.017	-0.028	-0.031
				(0.083)	(0.084)	(0.083)
Observations	91,598	91,598	91,598	91,598	91,598	91,598
			e. log(P99-100)			
$Reform_{i,t}$	-0.032*	-0.032*	-0.030*			
	(0.017)	(0.017)	(0.017)			
$log(1-\tau_{i,t})$				0.486**	0.476**	0.468**
				(0.205)	(0.205)	(0.206)
Observations	91,598	91,598	91,598	91,598	91,598	91,598
Municipality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Election-year FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
t $\times$ Province	YES	YES	YES	YES	YES	YES
Evasion control	NO	YES	YES	NO	YES	YES
Pr. tax (main dwelling)	NO	NO	YES	NO	NO	YES
Pr. tax (basic)	NO	NO	YES	NO	NO	YES
Pr. tax (allowance)	NO	NO	YES	NO	NO	YES

Note: This table compares the baseline coefficients from 2SLS regressions with those obtained controlling for property tax rates and a municipal-specific time-varying measure of tax evasion. Standard errors clustered at municipality-level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Tax-induced migration

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) 2SLS
			a. Stock elasticity: Only income taxes				
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	3.645**	3.443***	2.256**	3.611***	3.733***	2.858*	2.398*
	(1.847)	(1.188)	(1.125)	(1.179)	(1.197)	(1.504)	(1.241)
Observations	91,968	91,957	91,957	91,957	83,724	61,247	61,247
			b. Stock elasticity: Both income and property taxes				
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	3.719**	3.514***	2.101*	3.422***	3.469***	2.868**	2.437**
	(1.767)	(1.172)	(1.105)	(1.152)	(1.169)	(1.458)	(1.190)
Observations	91,968	91,957	91,957	91,957	83,724	61,247	61,247
			c. Share of movers elasticity: Only income taxes				
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	5.637***	5.368***	2.709**	4.045***	4.116***	3.036*	2.449*
	(2.076)	(1.404)	(1.306)	(1.358)	(1.384)	(1.717)	(1.463)
Observations	91,968	91,957	91,957	91,957	83,724	61,247	61,247
			d. Share of movers elasticity: Both income and property taxes				
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	5.596***	5.327***	2.400*	3.708***	3.708***	2.910*	2.349*
	(1.988)	(1.381)	(1.279)	(1.327)	(1.350)	(1.661)	(1.401)
Observations	91,957	91,957	91,957	91,957	83,724	61,247	61,247
Orig. and Dest. province FE	YES	NO	NO	NO	NO	NO	NO
Orig. $\times$ Dest. pair FE	NO	YES	YES	YES	YES	YES	YES
Time FE	NO	NO	YES	YES	YES	YES	YES
t $\times$ Province	NO	NO	YES	YES	YES	YES	YES
Δ in controls	NO	NO	NO	YES	YES	YES	YES
Sample restriction	NO	NO	NO	NO	YES	YES	YES
Mean dependent for a. and b.	0.129	0.129	0.129	0.129	0.136	0.157	0.157
Mean dependent for c. and d.	49.137	49.137	49.137	49.137	49.319	57.316	57.316

Note: This table shows the stock (panel a and b) and share of movers (panel c and d) migration elasticity with respect to the differential in the average tax rate between destination and origin province. The sample is composed of 10,707 origin and destination provincial pair over the 2007-2015 period. Standard errors in parentheses, with three-way clustering by origin-province $\times$ year, destination-province $\times$ year and province-pair. *** p<0.01, ** p<0.05, * p<0.1.

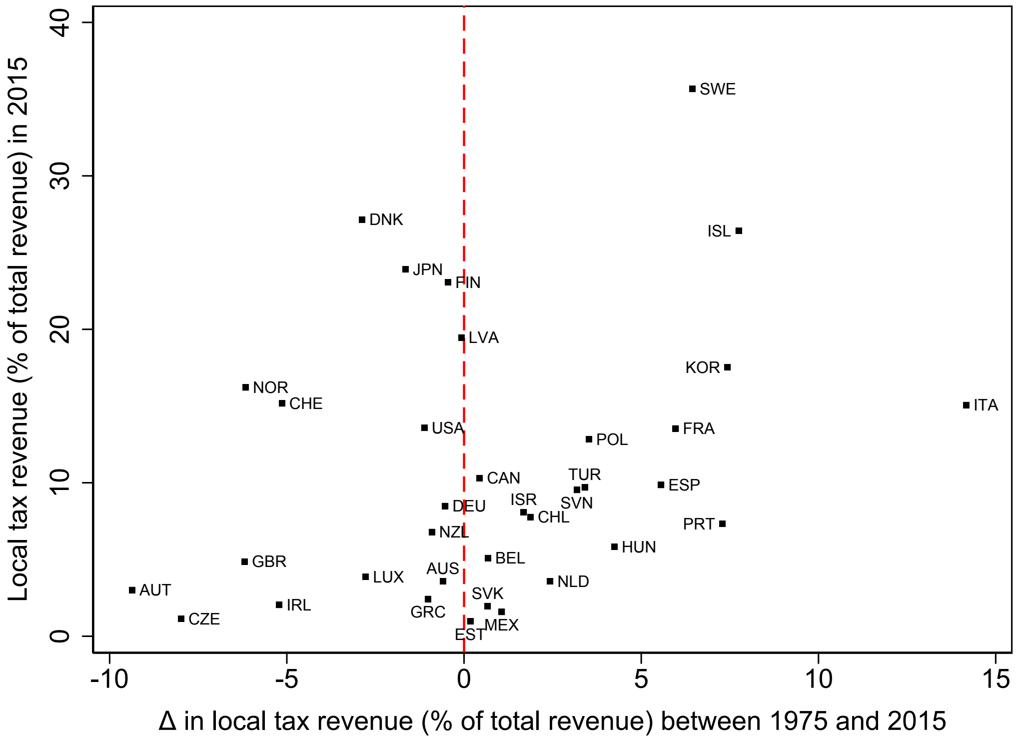
Table 6: Income shifting

	log(Taxable income)					
	OLS	OLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
$log(1-\tau_{i,t})$	0.643***	0.666***	0.252***	0.555***	0.615***	0.296***
	(0.069)	(0.077)	(0.067)	(0.070)	(0.080)	(0.072)
$log[(1-\sum_{j\neq i} w_{i,j}\tau_{j,t})/(1-\tau_{i,t})]$		-0.062	-0.866***		-0.106	-0.830***
		(0.189)	(0.185)		(0.192)	(0.188)
Observations	115,230	106,800	111,929	115,230	106,800	111,929
Municipality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Election-year FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
t × Province	YES	YES	YES	YES	YES	YES
Competitor tax rate	NO	Neigh. local MTR	LLM local MTR	NO	Neigh. local MTR	LLM local MTR

Note: This table compares the baseline taxable income elasticities (columns 1 and 4) with those estimated controlling for the difference between the (log of) net-of-local tax rate set by municipalities sharing a border (columns 2 and 5) or belonging to the same local labor market (columns 3 and 6) and the (log of) net-of-own local tax rate. Standard errors clustered at municipality-level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

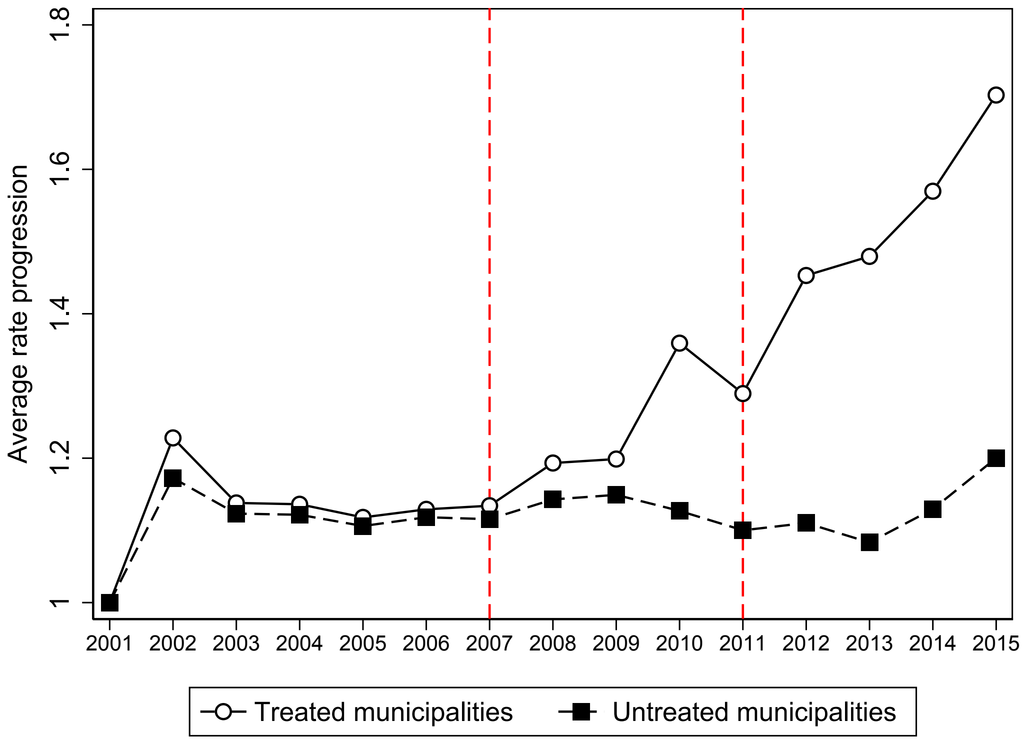
Figures

Figure 1: Fiscal decentralization in developed countries



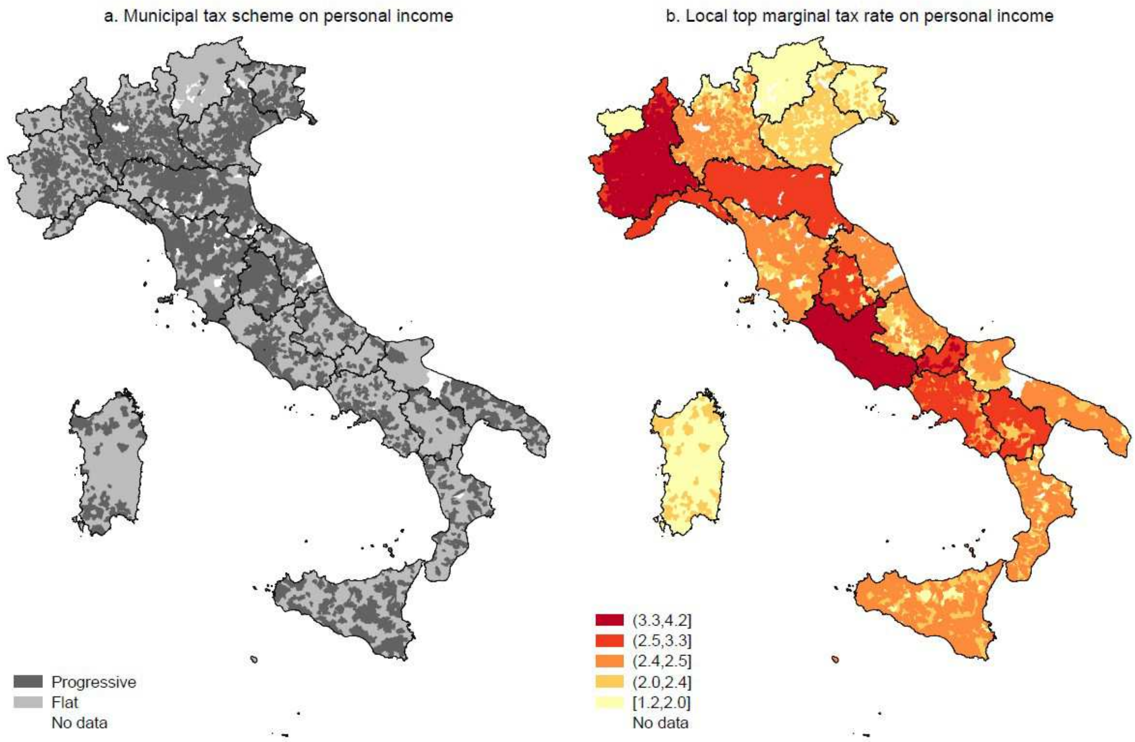
Note: This figure shows the share of tax revenue levies from sub-central tiers of national government in 2015 (left-side) and the variation in the share of sub-central tax revenue as a share of total revenues over the 1975-2015 period (or the closest years if missing) in a panel of developed countries. Revenue statistics from OECD tables.

Figure 2: Tax progressivity trends



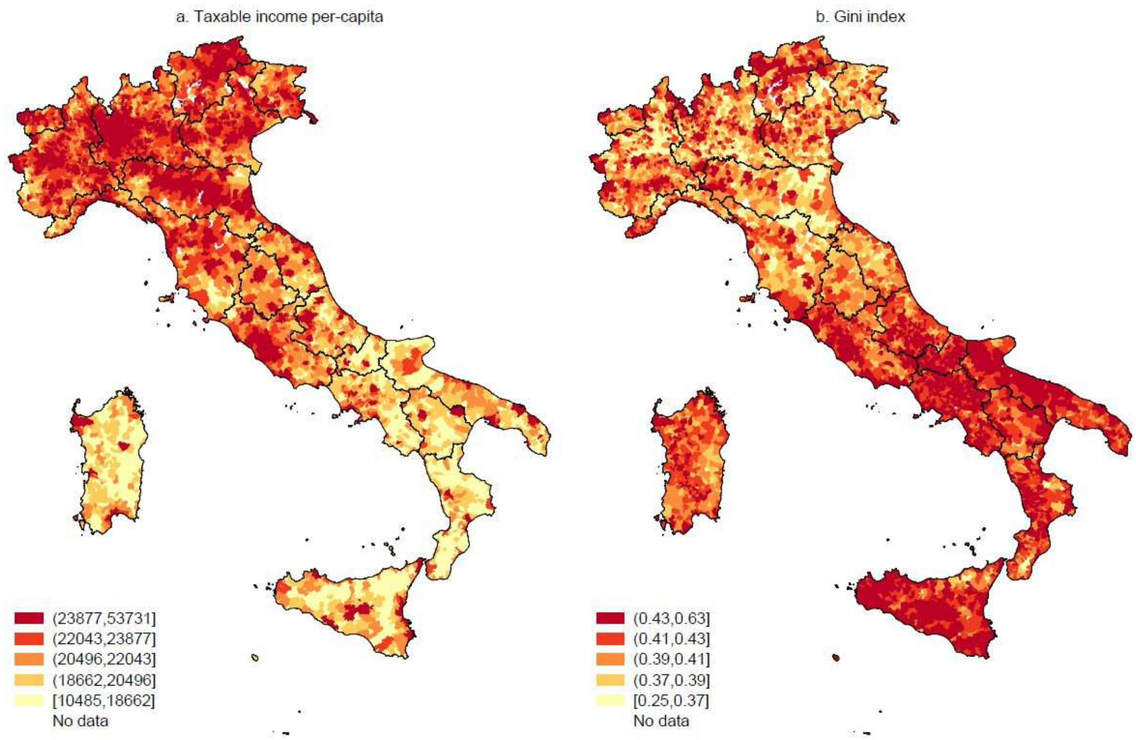
Note: This figure shows the evolution in the structural progressivity of local (municipal plus regional) personal income tax between municipalities which implemented the progressive tax schedule (treated) and those which did not (untreated). The average rate progression is measured as the ratio between the average local tax rate faced by richest taxpayers (i.e., those whose income is larger than 6 times the average national income) and poorest (i.e., those whose income is equal to one-third of the average national income). The vertical lines refer to the year in which it became possible to provide a tax exemption (2007) and to switch from a flat to a graduated tax scheme (2011). Author's elaboration on data from the Italian Ministry of Economy and Finance.

Figure 3: Geographical distribution of local tax scheme and local top tax rate



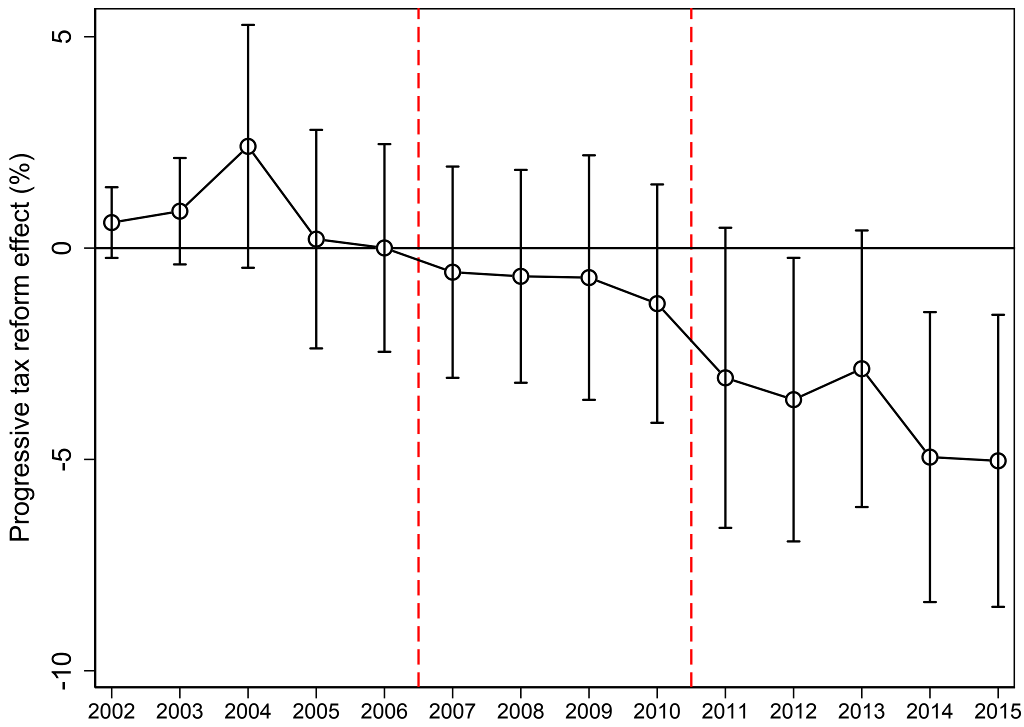
Note: Left figure shows the tax scheme - progressive or flat - on the surtax on personal income adopted by each municipality. Right figure displays the combined regional and municipal top marginal tax rate (%) on personal income in 2015. Author's elaboration on data from the Italian Ministry of Economy and Finance.

Figure 4: A map of taxable income per-capita and its distribution



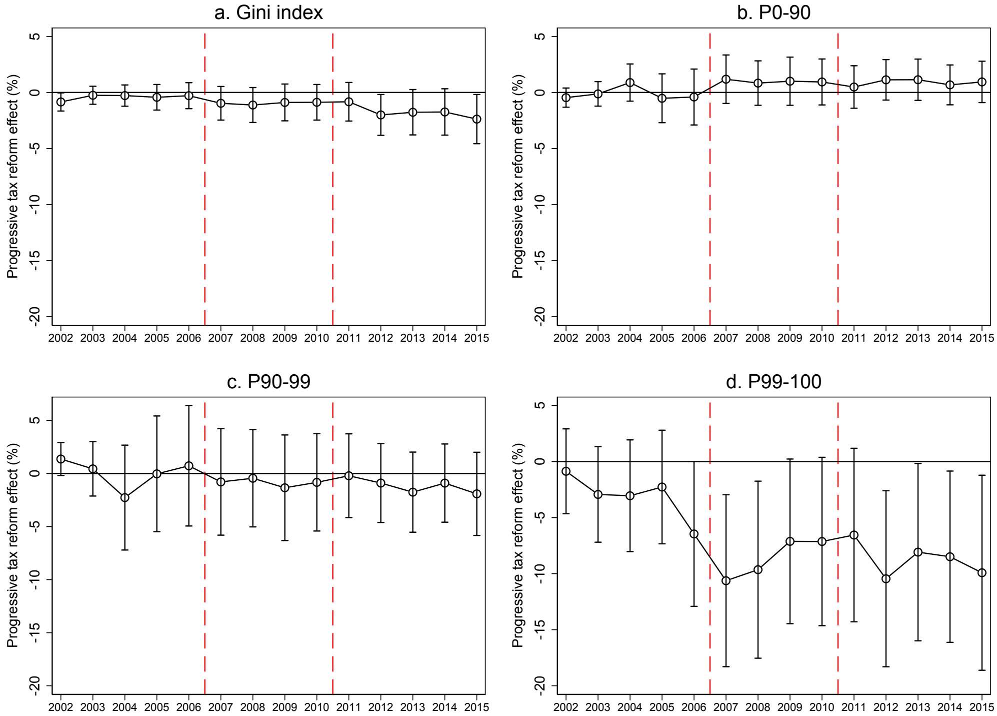
Note: This figure shows taxable income per-capita and Gini coefficient for each municipality in 2015. Author's elaboration on data from the Italian Ministry of Economy and Finance.

Figure 5: Leads and lags effect on taxable income



Note: This figure displays estimated coefficients and confidence intervals of the 2007 and 2011 tax reforms and their leads and lags. The vertical lines refer to the year in which it became possible to provide a tax exemption (2007) and to switch from the flat to the graduated tax scheme (2011). Numerical coefficients are reported in Appendix Table B6.

Figure 6: Leads and lags effect on inequality indexes



Note: This figure displays estimated coefficients and confidence intervals of the 2007 and 2011 tax reforms and their leads and lags. The vertical lines refer to the year in which it became possible to provide a tax exemption (2007) and to switch from the flat to the graduated tax scheme (2011). Numerical coefficients are reported in Appendix Table B6.

Appendix

Appendix A: Inequality measures

The basic income data on which I draw inequality measures are in form of grouped tabulations, as in Table A1. This table illustrates the number of taxpayers and the total income assessed for each income class for Rome in the 2015 fiscal year. A typical issue with this kind of data is that the income intervals do not coincide with the percentage groups of the population of interest. Therefore, I need to interpolate in order to derive inequality statistics such as Gini index and top income shares.

A1. Gini index

The basic assumption to derive the Gini index from data as in Table A1 is that in each income class all the units receive the same income (i.e., the average income of that income class). Several methods have been proposed for calculating the Gini index. The natural approach consists in approximating the Lorenz curve by a number of linear segments, and then estimate the Gini coefficients as the areas (or, weighted areas, as discussed later) between the linear segments and the 45 degrees line. The computation can be performed both parametrically (i.e., assuming a known income distribution), or non-parametrically (i.e., without any assumption on the underlying income distribution).[30] Milanovic (1994) proposes a simple and accurate non-parametric measure designed for ungrouped data. Abounoori and McCloughan (2003) arrange the Milanovic's (1994) formula for grouped data. They show that the original Milanovic (1994)'s formula can be expressed as:

$$G = C[n(1 - y_1/\bar{y}) + (n-1)(1 - y_2/\bar{y}) + ... + (n - (n-1))(1 - y_n/\bar{y})], \quad (9)$$

where $C = 2/n(n+1)$. After reversing the terms of the series in brackets, it becomes:

$$G = C\sum_{j=1}^{n} j(1 - y_{n-j+1}/\bar{y}). \quad (10)$$

With grouped data as in Table A1, the *n* taxpayers are arranged into K=7 mutually exclusive and exhaustive income classes with n_k taxpayers in group *k*, with $k = 1, 2, ...7$. To apply the last equation for grouped data, Abounoori and McCloughan (2003) suggest to calculate the weights corresponding to each group *k* (i.e. the analogue of *j* in the ungrouped version). According to Abounoori and McCloughan (2003), the general formula for calculating weight of group *k* is:

$$w_k = 1/2[\sum_{k=k}^{K} n_k(\sum_{k=k}^{K} n_k + 1) - \sum_{k=k+1}^{K} n_k(\sum_{k=k+1}^{K} n_k + 1)]. \quad (11)$$

[30]Note, however, that they both give a downward biased estimator.

Finally, the original Milanovic (1994) formula becomes:

$$G = C \sum_{k=1}^{K} w_k (1 - \bar{y_k}/\bar{y}). \tag{12}$$

Since Gastwirth (1972), previous literature has extensively discussed the pitfalls deriving from estimation of Gini index from grouped data.[31] The main concern of using grouped data to compute Gini index is that it imparts a nonnegligible downward bias, which is further increasing with the level of inequality and more severe when the number of groups is small. This bias would complicate comparisons of Gini coefficient both across municipalities and within-municipality over time. Van Ourti and Clarke (2011) propose to use a correction term for the Gini coefficient to address the bias due to grouping. They provide an exact expression for the difference between the Gini based on grouped data and the one that would be obtained from ungrouped data by drawing a parallel with the econometric literature on measurement error models. They suggest to weight the estimated Gini coefficient by a term equal to $K^2/(K^2-1)$, where *K* is the number of income classes. This correction term is a "grouped data" adjustment of the variance of the fractional rank, which turns out to be equal to the attenuation bias in the classical measurement error model (see Van Ourti and Clarke, 2011, for details).

Panel a in Figure A1 plots the estimated Gini index for Rome over the 2001-2015 period. Table A2 shows an application of the last equation for Rome in 2015 using the information illustrated in Table A1 as input data.

A2. Top income shares

The standard practice to compute top income shares from grouped data is to assume that the top tail can be approximated by a Pareto distribution. As discussed in section 4 of the paper, the key property of Pareto distribution is that the average income above a given threshold y is always exactly proportional to y by a coefficient equal to $b = a/(a-1)$. To estimate the top decile (P90-100) and the top percentile of the income distribution (P99-100), I use the standard method of Pareto interpolations used by Kuznets (1953) and more recently by Feenberg and Poterba (1993) and the studies following Piketty (2003).[32]

The first step consists in computing the income thresholds corresponding to the percentiles P90 and P99. For these two percentiles, I look first for the published income bracket [r,s] containing the percentile p. Then, I estimate the parameters k and α as the solutions of the

[31] Lerman and Yitzhaki (1989) show that the bias using grouped data is about 2.5% and 7% of the Gini as computed from microdata from Israel and the US. Similar results were found by Davies and Shorrocks (1989) using Canadian data. They show that the bias is significantly reduced as the number of groups increases. In their estimation, just five groups are needed to generate a Gini coefficient equal to 95% of the true value, and only twelve classes are required to converge within 1% of the actual value (see Figure 1 in Davies and Shorrocks, 1989 for a graphical representation of these numbers). However, Cowell and Mehta (1982), which arbitrary compress the nineteen official classes for Swedish income data into five groups, show that it is possible to get good estimates of the Gini index even when the number of groups is small.

[32] The technical appendix in Alvaredo and Saez (2010) provides a detailed overview on this procedure.

equations: $k = rp^{1-a}$ and $k = sf^{1/a}$, where p is the fraction of tax returns above r and f is the fraction of tax returns above s.

Then, I estimate the amount of income reported above the income threshold y_p. Using the estimated Pareto density with parameters α and k, I compute the income reported between the income threshold y_p and s, i.e. the upper bound of the brackets [r,s] containing y_p. This amount is then added to the remaining amounts of income, if exist, reported in all the brackets above s.

The mean income above percentile p is then computed by dividing the total income above y_p by the number of individuals above p. Finally, the income share owned by the individuals above the percentile p is obtained by dividing the total amount of income above y_p by the total income (which is corrected for income of non-filers, see below).

Table A3 shows the income thresholds (columns 1 and 5), the average income (columns 2 and 6), the number of tax units (columns 3 and 7) and the income share (columns 4 and 8) for the top decile and the top percentile in Rome over the 2001-2015 period. Panels b-d in Figure A1 illustrate the time trend in the bottom 90 percent (panel b), in the bottom half of the top decile (panel c) and in the top percentile (panel d) of the pre-tax income distribution.

When computing top income shares, two methodological problems should be addressed and I will follow the suggestions offered by the previous literature, in particular by Atkinson (2007). First, since individual with income lower than a certain threshold do not fill the tax returns, we need to relate the number of taxpayers with the total population. I take the "adult" population for each municipality, defined as those aged 15, using data from the National Institute of Statistics. This definition removes from the denominator those aged under 15 who receive income and may be included in the income tax statistics. It could be argued that the age cut-off is too low since people enter the labour force later. However, this is the only age cut-off for which it is possible to retrieve information for all the municipalities over the period covered. To investigate how much difference the population cut-off is likely to make, let us compare this cut-off with another larger by amount $1 + e$. Assuming that the top tail can be approximated by a Pareto distribution, then the effect of taking a control for population larger by $(1 + e)$ is that we have to go further down the income distribution to compute the income share, and, according to equation (3), the level of income reduced by a factor $(1 + e)^{1/a}$. From equation (3) it follows that this raises the estimated share by a factor $(1+e)^{1-1/a}$. To put this number in perspective, let us compute the effect of using the age cut-off at 15 years with another at 20 years. According to National Institute of Statistics data, in 2015 the population in Rome aged over 15 and over was about 5 % larger than aged 20 and over. Since the estimated α for Rome in 2015 was 2.096, with e = 5% this yields an adjustment of about 2.5%. It means that if the top percentile were to be 10% with an assumed cut-off age of 20, then it would be 10.25% with the cut-off of 15, a difference that can interpreted as negligible.

Second, taxable income differs from actual total income because we do not observe the income of non-filers and incomes not included in the tax base. Computation of top income shares at country-level typically relates the amounts recorded in the tax data to those derived from the national accounts. Then, the income of non-filers would appear as a residual. Unfortunately, such kind of data is not available at municipal-level. Hence, a different approach should be followed to estimate the total income that would have been reported if everybody had been required to file a tax return. As Piketty and Saez (2003) did for the US, I impute to non-filers a fixed fraction equal to 20 percent of filers' average income.

Appendix B: Additional data and results

This section shows and discusses additional data and results.

Table B1 provides economic and demographic information on Italian regions.

Table B2 illustrates summary statistics of the data used.

Table B3 replicates the main specifications by including region or province $\times$ year effects to account for any other non-tax related regional or provincial annual change. Reassuringly, the effects appear to be very similar even including these time interactions.

Table B4 replicates the main specifications by using the average rate progression (as defined in Section 2) instead of the baseline reform dummy as a measure of structural PIT progressivity and the combined income and property local tax rates instead of the baseline regional and municipal top marginal surtaxes. Effects are qualitatively similar even using these alternative tax rate and progressivity measures.

Table B5 tests for geographical heterogeneities by comparing baseline coefficients with those estimated from four sub-samples: i. I drop municipalities belonging to a Statuto Speciale region (Aosta Valley, Friuli-Venezia Giulia, Sardinia, Sicily and Trentino-Alto Adige), which regions with special autonomy that are allowed to set their own fiscal rules for municipal governments; ii. Northern municipalities (Aosta Valley, Emilia-Romagna, Friuli-Venezia Giulia, Liguria, Lombardy, Piedmont, Trentino-Alto Adige, Veneto); iii. Center municipalities (Lazio, Marche, Tuscany, Umbria); iv. Southern municipalities (Abruzzo, Apulia, Basilicata, Calabria, Campania, Molise, Sardinia, Sicily).

Table B6 reports numerical coefficients of Figures 5 and 6.

Figure B1 presents the distribution of the estimated propensity score between treated and control municipalities. For the matching procedure I use the "nearest neighbour" approach (see section 4.3).

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Appendix tables and figures

Table A1: Income tax data, Rome in 2015

Income class (€)	Total taxpayers	Income assessed (€)
1 - 10,000	547,176	2,353,967,201
10,001 - 15,000	207,374	2,353,967,201
15,001 - 26,000	465,392	9,523,343,768
26,001 - 55,000	513,087	18,438,065,283
55,001 - 75,000	77,506	4,944,213,442
75,001 - 120,000	62,438	5,743,228,043
> 120,000	32,888	7,545,279,203

Note: Income data from Italian Ministry of Economy and Finance on reported taxable personal income for the year 2015 in Rome.

Table A2: Application of grouped Milanovic formula to Rome in 2015

k	Frequency	$\bar{y_k}$	$1-\bar{y_k}/\bar{y}$	Reverse cdf	x = Col. 5*(Col.5+1)/2	$w_k = x_k - x_{k-1}$	$Cw_k(1-\bar{y_k}/\bar{y_k})$	Sum of Col. 8 = Gini
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1	547,176	4,302	0.840	1,905,861	1.82e+12	8.93e+11	0.413	
2	207,374	12,469	0.535	1,358,685	9.23e+11	2.60e+11	0.077	
3	465,392	20,463	0.237	1,151,311	6.63e+11	4.28e+11	0.056	
4	513,087	35,936	-0.339	685,919	2.35e+11	2.20e+11	-0.041	0.489
5	77,506	63,791	-1.378	172,832	1.49e+10	1.04e+10	-0.008	
6	62,438	91,983	-2.428	95,326	4.54e+09	4.00e+09	-0.005	
7	32,888	229,424	-7.551	32,888	5.41e+08	5.41e+08	-0.002	

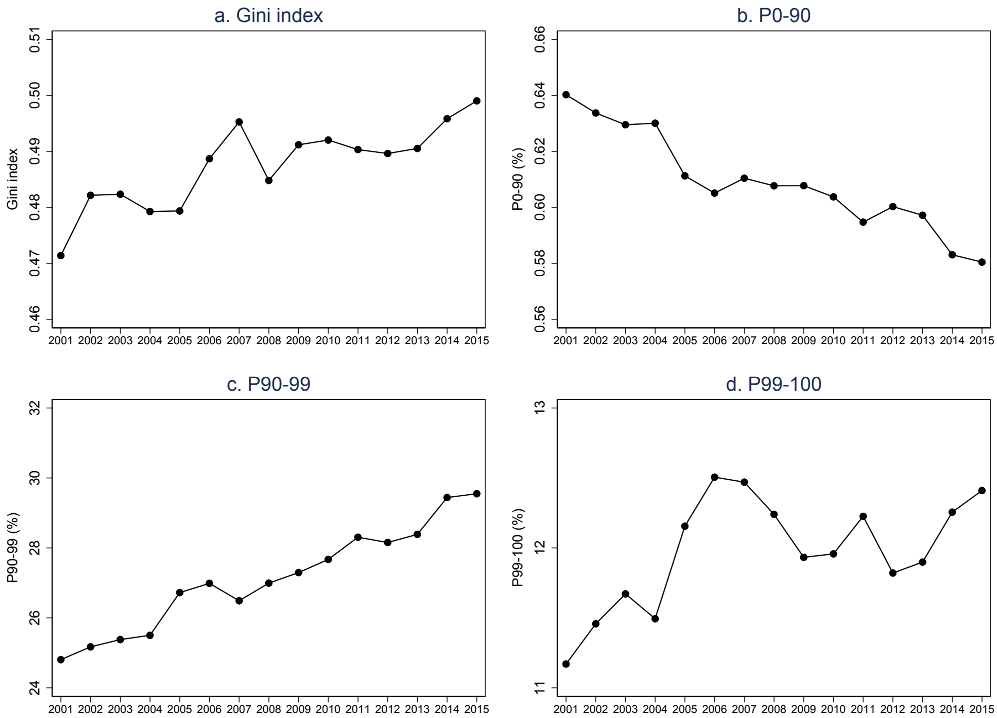
Note: $C =$5.51e-13 and $\bar{y} = 26,829$.

Table A3: Thresholds and average incomes in top income groups in Rome

Year	Top 10 (P90-100)				Top 1 (P99-100)			
	Threshold	Tax units	$\bar{y}$	Share	Threshold	Tax units	$\bar{y}$	Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2001	€32,626	221,917	€77,700	35.98%	€96,818	22,192	€241,248	11.17%
2002	€34,488	221,155	€80,773	36.63%	€102,700	22,116	€252,659	11.46%
2003	€36,384	220,729	€84,063	37.05%	€110,970	22,073	€264,805	11.67%
2004	€37,803	221,512	€85,357	37.00%	€114,952	22,151	€265,193	11.49%
2005	€39,496	220,813	€88,500	38.88%	€120,373	22,081	€276,706	12.16%
2006	€41,333	221,334	€92,314	39.49%	€128,543	22,133	€292,318	12.51%
2007	€42,717	222,675	€94,951	38.96%	€134,399	22,268	€303,907	12.47%
2008	€44,032	222,763	€93,963	39.23%	€136,510	22,276	€293,140	12.24%
2009	€44,763	223,964	€93,876	39.23%	€138,489	22,396	€285,565	11.93%
2010	€45,614	225,645	€94,165	39.63%	€140.688	22,565	€284,132	11.96%
2011	€46,316	226,547	€93,772	40.53%	€142.422	22,654	€282,857	12.23%
2012	€46,107	228,298	€89,834	39.98%	€139,163	22,830	€265,648	11.82%
2013	€46,596	228,298	€89,756	40.29%	€139,814	22,830	€265,101	11.90%
2014	€45,858	247,691	€87,662	41.70%	€136,215	24,769	€257,664	12.26%
2015	€46,241	248,323	€88,694	41.96%	€137.208	24,832	€262.323	12.41%

Note: Author's elaboration from tax returns data.

Figure A1: Inequality indexes in Rome, 2001-2015



Note: This figure shows time trend in Gini index (panel a), the bottom 90 percent (panel b), the bottom half of the top decile (panel c) and the top percentile (panel d) of the pre-tax income distribution for Rome over the 2001-2015 period. See the text for information on how these measures are derived.

Table B1: Demographic and economic information on Italian regions

Region	Capital city (1)	Prov. (N) (2)	Mun. (N) (3)	Pop. (Mill.) (4)	Income pc (Thous. €) (5)	Local top PIT rate (%) in year: 2001 (6)	2005 (7)	2010 (8)	2015 (9)
Abruzzo	L'Aquila	4	305	1.299	20.660	1.153	1.218	1.922	2.433
Aosta Valley	Aosta	0	74	0.125	23.954	0.900	0.900	0.985	1.354
Apulia	Bari	6	258	3.654	19.962	1.192	1.247	1.432	2.461
Basilicata	Potenza	2	131	0.585	19.215	1.171	1.212	1.418	2.972
Calabria	Catanzaro	5	409	1.977	19.072	1.097	1.711	2.200	2.439
Campania	Naples	5	551	5.741	21.006	1.168	1.229	2.229	2.757
Emilia-Romagna	Bologna	9	348	4.156	24.371	1.011	1.128	1.871	3.056
Friuli-Venezia Giulia	Trieste	4	218	1.200	23.532	0.940	1.025	1.228	1.761
Lazio	Rome	5	378	5.414	26.168	1.043	1.167	2.236	4.146
Liguria	Genoa	4	235	1.574	24.129	1.123	1.241	1.936	3.036
Lombardy	Milan	8	1,544	8.616	26.442	1.050	1.594	1.696	2.415
Marche	Ancona	5	239	1.323	21.761	1.225	1.805	2.049	2.512
Molise	Campobasso	2	136	0.317	19.781	1.156	1.496	2.135	3.210
Piedmont	Turin	8	1,206	4.321	24.029	1.124	1.718	1.854	4.048
Sardinia	Cagliari	8	377	1.170	21.213	1.105	1.132	1.218	1.751
Sicily	Palermo	9	390	4.988	20.528	1.074	1.138	1.839	2.468
Trentino-Alto Adige	Trento	2	333	0.939	24.616	0.914	0.947	0.951	1.282
Tuscany	Florence	10	287	3.557	23.304	1.128	1.248	1.365	2.412
Umbria	Perugia	2	92	0.867	21.762	1.131	1.394	1.622	2.579
Veneto	Venice	7	581	4.753	23.525	1.143	1.704	1.343	1.958

Note: This table provides information on each Italian region. Columns 2-5 show the number of provinces, municipalities, total population (averaged over the 2001-2015 period) and taxable income per-capita (municipal population-weighted average over the 2001-2015 period). Columns 6-9 report the local top marginal surtax on personal income, computed as the sum of the regional and the population-weighted average municipal rate.

Table B2: Summary statistics

	Obs (1)	Mean (2)	Std. Dev. (3)	Min (4)	Max (5)
a. Outcome variables					
Taxable income per-capita	115,230	20,389	3,898	6,523	96,101
Gini index	115,230	0.397	0.038	0.240	0.765
Bottom 90 (%)	114,930	67.692	4.472	28.274	92.174
Top 10-1 (%)	95,751	25.483	2.847	6.564	51.940
Top 1 (%)	95,751	7.693	2.265	1.262	61.480
b. Tax variables					
Municipal (top) MTR (%)	115,230	0.340	0.267	0	0.900
Regional (top) MTR (%)	115,230	1.404	0.465	0.900	4
Municipal ATR (%)	115,230	0.326	0.258	0	0.900
Regional ATR (%)	115,230	1.183	0.314	0	2.051
Property tax (main dwelling)	115,230	0.492	0.091	0	0.860
Property tax (basic)	115,230	0.672	0.153	0.300	1.110
Property tax allowance (€)	115,230	139.061	55.533	0	2,500
Municipal (top) MTR (%) - neigh. competitor	115,035	0.344	0.203	0	0.850
Municipal (top) MTR (%) - SLL competitor	111,929	0.349	0.190	0	0.800
Reform	115,230	0.099	0.298	0	1
c. Demographic characteristics					
Population	115,230	7,365	40,878	30	2,872,021
Share of 65+	115,230	22.409	6.131	4.363	66.379
Share of 15-	115,230	13.198	2.859	0	26.459
Share of foreign	115,230	5.017	4.047	0	38.961
d. Political and economic variables					
Mayor age	115,230	49.094	9.702	19	94
Mayor sex	115,230	0.102	0.299	0	1
Mayor graduated	115,230	0.423	0.489	0	1
Average age in town council	115,230	44.186	4.139	26	77
Proportion of women in town council	115,230	0.196	0.119	0	1
Proportion of graduated in town council	115,230	0.255	0.163	0	1
Crisis	115,230	0.001	0.031	0	1
Unemployment rate (%)	115,230	8.357	4.953	1.600	31.456
Budget deficit	115,230	0.115	0.318	0	1
Administration expenditure (%)	115,230	22.602	9.102	0	85.335
Development expenditure (%)	115,230	0.304	0.760	0	32.738
Education expenditure (%)	115,230	5.469	3.497	0	31.468
Law and order expenditure (%)	115,230	0.057	0.303	0	42.510
Social welfare expenditure (%)	115,230	6.139	5.794	0	73.620

Note: The sample covers 7,682 municipalities over the 2001-2015 period.

Table B3: Alternative progressivity and tax rate definitions

	Progressivity definition:		Local tax rate on:	
	Reform dummy	ARP	Income	Inc. + prop.
	(1)	(2)	(3)	(4)
a. log(Taxable income)				
$Progressivity_{i,t}$	-0.077***	-0.045***		
	(0.013)	(0.004)		
$log(1-\tau_{i,t})$			0.562***	0.708***
			(0.070)	(0.073)
Observations	115,230	115,230	115,230	115,230
b. log(Gini index)				
$Progressivity_{i,t}$	-0.002	0.002		
	(0.004)	(0.004)		
$log(1-\tau_{i,t})$			0.075*	0.113***
			(0.041)	(0.042)
Observations	115,230	115,230	115,230	115,230
c. log(P0-90)				
$Progressivity_{i,t}$	0.005	-0.004*		
	(0.003)	(0.002)		
$log(1-\tau_{i,t})$			-0.100***	-0.130***
			(0.039)	(0.039)
Observations	114,930	114,930	114,930	114,930
d. log(P90-99)				
$Progressivity_{i,t}$	-0.010	-0.018***		
	(0.008)	(0.004)		
$log(1-\tau_{i,t})$			0.004	-0.018
			(0.083)	(0.085)
Observations	95,751	95,751	95,751	95,751
e. log(P99-100)				
$Progressivity_{i,t}$	-0.029*	-0.008		
	(0.016)	(0.008)		
$log(1-\tau_{i,t})$			0.495**	0.655***
			(0.204)	(0.206)
Observations	95,751	95,751	95,751	95,751
Municipality FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Election-year FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
t × Province	YES	YES	YES	YES
Progressivity or tax measure	Reform dummy	ARP	Income	Inc. + prop.

Note: This table compares baseline coefficients (column 1 and 3) with those estimated using the average rate progression instead of the baseline reform dummy (column 2) and the combined local income and property tax rate instead of the baseline income rate (column 4). Standard errors clustered at municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B4: Sub-sample analysis

	Baseline	Subsample:				Baseline	Subsample:			
		St. ord.	North	Center	South		St. ord.	North	Center	South
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
a. log(Taxable income)										
$Reform_{i,t}$	-0.077***	-0.102***	-0.097***	-0.023	-0.046**					
	(0.013)	(0.014)	(0.021)	(0.014)	(0.019)					
$log(1-\tau_{i,t})$						0.555***	0.559***	0.669***	0.152**	0.185
						(0.070)	(0.072)	(0.126)	(0.074)	(0.250)
Observations	115,230	97,275	64,935	14,010	36,285	115,230	97,275	64,935	14,010	36,285
b. log(Gini index)										
$Reform_{i,t}$	-0.000	-0.000	0.003	-0.002	-0.006					
	(0.004)	(0.004)	(0.006)	(0.008)	(0.006)					
$log(1-\tau_{i,t})$						0.075*	0.068	0.135	0.050	0.192
						(0.041)	(0.041)	(0.090)	(0.049)	(0.132)
Observations	115,230	97,275	64,935	14,010	36,285	115,230	97,275	64,935	14,010	36,285
c. log(P0-90)										
$Reform_{i,t}$	0.005	0.000	0.004	0.016**	0.009**					
	(0.003)	(0.004)	(0.006)	(0.008)	(0.004)					
$log(1-\tau_{i,t})$						-0.100**	-0.071*	-0.035	-0.032	-0.206
						(0.039)	(0.040)	(0.081)	(0.048)	(0.130)
Observations	114,930	97,031	64,658	14,008	36,264	114,930	97,031	64,658	14,008	36,264
d. log(P90-99)										
$Reform_{i,t}$	0.010	0.026***	0.024*	-0.039**	-0.002					
	(0.008)	(0.010)	(0.014)	(0.018)	(0.008)					
$log(1-\tau_{i,t})$						0.005	-0.035	-0.555***	0.010	0.030
						(0.083)	(0.085)	(0.166)	(0.098)	(0.293)
Observations	95,751	81,266	55,132	12,276	28,343	95,751	81,266	55,132	12,276	28,343
e. log(P99-100)										
$Reform_{i,t}$	-0.029*	-0.022	-0.027	0.007	-0.067***					
	(0.016)	(0.019)	(0.027)	(0.040)	(0.021)					
$log(1-\tau_{i,t})$						0.494**	0.459**	0.881**	0.287	2.155***
						(0.204)	(0.208)	(0.419)	(0.295)	(0.535)
Observations	95,751	81,266	55,132	12,276	28,343	95,751	81,266	55,132	12,276	28,343
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Election-year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
t × Province	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sub-sample	NO	St. ord.	North	Center	South	NO	St. ord.	North	Center	South

Note: This table compares coefficients from full sample (columns 1 and 6) with those estimated from four sub-samples: i. dropping Statuto Speciale regions (columns 2 and 7); ii. Northern Italy (columns 3 and 8); iii. Center Italy (columns 4 and 9); iv. Southern Italy (columns 5 and 10). Standard errors clustered at municipal level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B5: Alternative specifications

	Baseline (1)	+ Region × Time FE (2)	+ Province × Time FE (3)	Baseline (4)	+ Region × Time FE (5)	+ Province × Time FE (6)
			a. log(Taxable income)			
$Reform_{i,t}$	-0.077***	-0.073***	-0.071***			
	(0.013)	(0.013)	(0.012)			
$log(1-\tau_{i,t})$				5.169***	3.997***	3.479**
				(1.213)	(1.441)	(1.499)
Observations	115,230	115,230	115,230	115,230	115,230	115,230
			b. log(Gini index)			
$Reform_{i,t}$	-0.000	-0.000	-0.003			
	(0.004)	(0.004)	(0.004)			
$log(1-\tau_{i,t})$				2.797***	3.690***	4.246***
				(0.735)	(0.894)	(0.944)
Observations	115,230	115,230	115,230	115,230	115,230	115,230
			c. log(P0-90)			
$Reform_{i,t}$	0.005	0.007**	0.016***			
	(0.003)	(0.004)	(0.004)			
$log(1-\tau_{i,t})$				-2.270***	-3.135***	-4.123***
				(0.638)	(0.764)	(0.808)
Observations	114,930	114,930	114,930	114,930	114,930	114,930
			d. log(P90-99)			
$Reform_{i,t}$	0.010	0.007	-0.015*			
	(0.008)	(0.009)	(0.008)			
$log(1-\tau_{i,t})$				-0.342	0.744	2.986*
				(1.254)	(1.523)	(1.585)
Observations	95,751	95,751	95,751	95,751	95,751	95,751
			e. log(P99-100)			
$Reform_{i,t}$	-0.029*	-0.043**	-0.049***			
	(0.016)	(0.018)	(0.019)			
$log(1-\tau_{i,t})$				8.181***	12.238***	14.433***
				(2.898)	(3.628)	(3.856)
Observations	95,751	95,751	95,751	95,751	95,751	95,751
Municipality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Election-year FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
t × Province	YES	YES	YES	YES	YES	YES
Time FE × Region	NO	YES	NO	NO	YES	NO
Time FE × Province	NO	NO	YES	NO	NO	YES

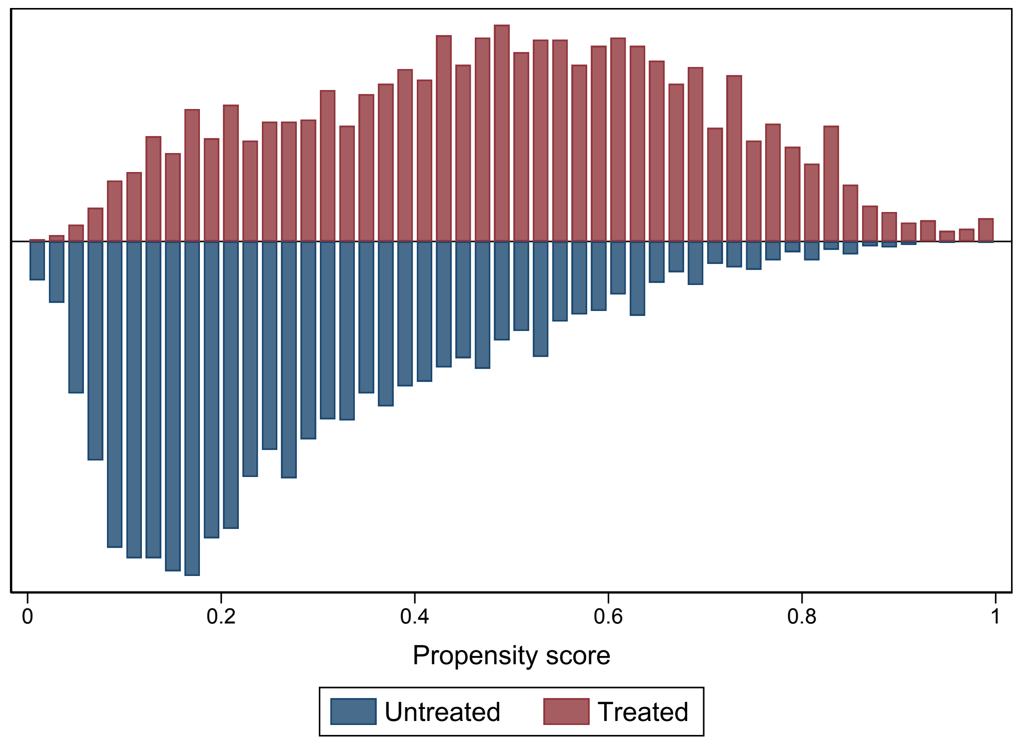
Note: This table compares 2SLS baseline coefficients with those obtained including regional (columns 2 and 5) or provincial-specific (columns 3 and 6) time fixed effects. Note that net-of-tax elasticity estimates refer to the *municipal* tax rate, while in baseline estimations I use the sum of regional and municipal rates. Standard errors clustered at municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B6: Leads and lags of the tax reforms

	Outcome variable:				
	log(TI)	log(Gini)	log(P0-90)	log(P90-99)	log(P99-100)
	(1)	(2)	(3)	(4)	(5)
2007 $Reform_{i,t+5}$	0.006	-0.008*	-0.005	0.014	-0.009
	(0.005)	(0.005)	(0.005)	(0.009)	(0.023)
2007 $Reform_{i,t+4}$	0.009	-0.002	-0.001	0.004	-0.029
	(0.008)	(0.005)	(0.007)	(0.016)	(0.026)
2007 $Reform_{i,t+3}$	0.024	-0.003	0.009	-0.023	-0.030
	(0.017)	(0.006)	(0.010)	(0.030)	(0.030)
2007 $Reform_{i,t+2}$	0.002	-0.004	-0.005	-0.000	-0.023
	(0.016)	(0.007)	(0.013)	(0.033)	(0.031)
2007 $Reform_{i,t+1}$	-0.000	-0.003	-0.004	0.007	-0.065
	(0.015)	(0.007)	(0.015)	(0.035)	(0.039)
2007 $Reform_{i,t}$	-0.006	-0.010	0.012	-0.008	-0.106**
	(0.015)	(0.009)	(0.013)	(0.030)	(0.047)
2007 $Reform_{i,t-1}$	-0.007	-0.011	0.008	-0.004	-0.096**
	(0.015)	(0.009)	(0.012)	(0.028)	(0.048)
2007 $Reform_{i,t-2}$	-0.007	-0.009	0.010	-0.013	-0.071
	(0.018)	(0.010)	(0.013)	(0.030)	(0.045)
2007 $Reform_{i,t-3}$	-0.013	-0.009	0.009	-0.008	-0.071
	(0.017)	(0.010)	(0.012)	(0.028)	(0.046)
2011 $Reform_{i,t}$	-0.031	-0.008	0.005	-0.002	-0.066
	(0.022)	(0.010)	(0.012)	(0.024)	(0.047)
2011 $Reform_{i,t-1}$	-0.036*	-0.020*	0.011	-0.009	-0.104**
	(0.020)	(0.011)	(0.011)	(0.023)	(0.048)
2011 $Reform_{i,t-2}$	-0.029	-0.018	0.011	-0.018	-0.081*
	(0.020)	(0.012)	(0.011)	(0.023)	(0.048)
2011 $Reform_{i,t-3}$	-0.050**	-0.017	0.007	-0.009	-0.085*
	(0.021)	(0.013)	(0.011)	(0.022)	(0.046)
2011 $Reform_{i,t-4}$	-0.050**	-0.024*	0.009	-0.019	-0.099*
	(0.021)	(0.013)	(0.011)	(0.024)	(0.053)
Observations	88,725	88,725	88,433	70,515	70,515
Municipality FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Election-year FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
t $\times$ Province	YES	YES	YES	YES	YES
F-test: 2001-2006 = 0	0.234	0.475	0.297	0.339	0.513

Note: This table shows coefficients of the 2007 and 2011 tax reforms and their leads and lags. In the last row, I report p-values from a F-test of equality of coefficients over the pre-treatment period. The sample is composed of the municipalities that adopted the reform in the first round and the re-weighted control group. Standard errors clustered at municipality-level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure B1: Propensity score distribution



Note: This figure presents the distribution of the estimated propensity score between treated and untreated municipalities. For the matching procedure I use the "nearest neighbour" approach, as discussed in section 4.2.