UNIVERSITY OF ESSEX

INSTITUTE FOR SOCIAL AND ECONOMIC RESEARCH

DOCTORAL THESIS

Essays on Public Economics

Enrico RUBOLINO

Supervisors:

Emilia DEL BONO

Andreas MUELLER

Examiners:

Emmanuel SAEZ

Michel SERAFINELLI

October 2020

Acknowledgment

I am very grateful to my advisors Emilia Del Bono and Andreas Mueller for their invaluable guidance and support.

I would like to express my gratitude to Emmanuel Saez who gave me the golden opportunity to visit UC Berkeley and for his encouragements and insightful comments.

I benefited from very helpful comments and stimulating discussions with James Alm, David Agrawal, Alan Auerbach, Mike Brewer, David Card, Marco Francesconi, Ben Meiselman, Alari Paulus, Sauro Mocetti, Michel Serafinelli, Daniel Waldenstrom, Danny Yagan and Gabriel Zucman. Many thanks to Eike Eser and Caterina Pavese who read parts of this dissertation and gave me very useful suggestions.

Contents

List of Figures

List of Tables

Summary (300 words)

This dissertation gathers three essays on public economics. The first chapter studies the effects of local income taxation on tax base and individual mobility since the early 2000s in Italy. I combine novel fine-grained data on the universe of tax residence's transfers with 89,860 local income tax changes and income bracket-by-municipality-level panel data on the tax base. I propose different empirical strategies, resting on tax rate variations both over time and across individuals within locations. I find that taxation significantly affects the location of the tax base. The mobility response mostly reflects tax residence relocation and involves a separation between residence and workplace. Yet, my estimates imply that efficiency losses due to tax-induced mobility are relatively small, thus making local redistribution feasible at least in the medium-run.

The second chapter studies the effects of stricter tax enforcement on tax collections, public goods provision and local tax rates. I study these links in the context of the Ghost Buildings program: an anti-tax evasion policy that detected buildings not reported on land registry in Italy. Using cross-municipality variation in scope for enforcing buildings' registration, I show that tax collections account for around three-fourth of the projected revenue increase. I find complementarity between enforcement and local tax rates on property and income, which ultimately led to larger investments in schools.

The third chapter studies the effect of regulation on intergenerational transmission of occupations. Focusing on the case of Italy since the early 2000s, we exploit the impact of two major reforms in the regulation of professional services. Leveraging the differential effect of regulation among occupations and over time, we show that the progressive liberalization of professional services has affected the allocation of individuals across occupations, leading to a substantial decrease in the propensity to follow the parents' career.

Introduction

This dissertation gathers three essays on public economics. Namely, I provide empirical evidence on how taxpayers and workers responded to different public policy reforms in Italy. At the turn of the 21st century, Italy carried out several large-scale reforms aiming to promote fiscal justice, curb tax evasion and spur social mobility. In this dissertation, I focus on three main public policy changes. First, I study how tax decentralization affects the spatial allocation of taxpayers within a country. Second, I evaluate the local public finance effects of a large scale anti-tax evasion policy, which used an innovative monitoring technique to detect unregistered (taxable) buildings. Third, I focus on the deregulation of professional services to test whether reducing entry barriers to (high-income) occupations leads to a more efficient and equitable allocation of talents.

The first chapter analyzes the effects of tax decentralization on tax base and individual mobility. Since the seminal contribution of Tiebout (1956), there is a controversial debate about the degree of tax autonomy that should be granted to local governments. This debate is not limited to the academic world, but attracts much attention in public and policy debate. At the center of the debate lies the trade-off between the benefits of diversity in fiscal policies and the costs of loosing tax bases when individuals "vote with their feet." The threat of tax base mobility puts hurdles on the ability of governments to redistribute income and finance spending through progressive taxation (Mirrlees 1982; Piketty and Saez 2013) and might trigger socially inefficient tax competition

(Lehmann et al. 2014).

Despite the recent economics literature has seen an increase in research on migration, a recent survey of the literature (Kleven et al. 2020) has emphasized that "direct empirical evidence on the responsiveness of individual locations to taxes has been remarkably scant". Two empirical challenges are likely to explain the paucity of empirical evidence. First, information on migration patterns across locations is difficult to retrieve. Second, tax variations need to be orthogonal to all the other factors influencing location choices, such as local labor market conditions, public goods provision and amenities.

The chapter offers novel evidence on tax-induced mobility since the early 2000s in Italy. Over this period of tax decentralization, regions and municipalities have been granted greater power to set different tax rates across income brackets. I combine novel fine-grained data on the universe of tax residence's transfers with 89,860 local income tax changes and municipality-level panel data on the tax base. I propose different empirical strategies, resting on tax rate variations both over time and across individuals within locations. I find that taxation significantly affects the location of the tax base. The mobility response mostly reflects tax residence relocation and involves a separation between residence and workplace. Responses strongly vary by gender, education, civil status and occupations.

Yet, my estimates imply that efficiency losses due to tax-induced mobility are relatively small, thus making local redistribution feasible at least in the medium-run. Thus, the results are consistent with Epple and Romer (1991) and Agrawal and Foremny (2019), who show that local redistribution is feasible with migration even if it generates a relocation response, but in contradiction to Feldstein and Wrobel (1998), who show that local redistribution involves large efficiency costs. A possible caveat is that mobility could rise in the long run given demographic shifts and technological innovations, which may impose additional constraints on redistributive policy.

The second chapter studies the local public finance effects of curbing tax evasion. Economists and policy makers often advocate tackling tax evasion as a key policy for the development of fiscal capacity (Besley and Persson 2009; Besley and Persson 2013), to finance worthy government projects (Myles 2000; Lindert 2004), and to set tax instruments more efficiently (Saez et al. 2012; Keen and Slemrod 2017). The interest in fighting tax evasion ramps up routinely during economic downturns, when governments face challenges in raising revenue and financing public spending. Despite technological development has enhanced the ability of governments to retrieve reliable information and monitor tax payments, relatively little is known on the economic returns from anti-tax evasion policies.

Whether curbing tax evasion is a successful strategy for improving the fiscal budget and global welfare is not obvious (Slemrod 2007; Slemrod 2019). First, the effectiveness of anti-tax evasion policies can be limited when tax authorities face constraints in enforcing tax payments. Enforcement may in fact be difficult and costly, in particular in weak institutional environments (Carrillo et al. 2017) and when the decision to punish evaders overlaps with political considerations (Casaburi and Troiano 2016). Second, even if stricter tax enforcement would eventually raise revenue, overall welfare depends on how revenue are spent and whether tax rates complement or substitute stricter enforcement (Keen and Slemrod 2017). Additional revenue might indeed not improve welfare if they are diverted in political rents (see, e.g., Brollo et al. 2013; Caselli and Michaels 2013).

In the chapter, I ask the following questions: How successful are anti-tax evasion policies when tax authorities face enforcement constraints? Is curbing tax evasion an effective strategy for financing public goods provision? Does the threat of tax evasion deter the desired degree of tax progressivity? I study these questions in the context of the Ghost Buildings program: an anti-tax evasion policy that detected buildings not reported on land registry in Italy. Using cross-municipality variation in scope for enforcing buildings registration, I show that tax collections account for around three-fourth of the projected revenue increase. I find complementarity between enforcement and local tax rates on property and income, which ultimately led to larger investments in schools. Exploiting a discontinuity in incentives for complying with the program, I show that constraining local administrators' discretion in enforcing tax collection was a crucial mechanism.

The third chapter, written with Sauro Mocetti and Giacomo Roma from the Bank of Italy, studies the effect of regulation on intergenerational transmission of occupations. Distinguishing career following that is motivated by an intergenerational transfer of occupation-specific human capital (through either nature or nurture) from that caused by regulation and positional rents is empirically challenging. To address this issue, we exploit two reforms relating to the regulation of professional services that have been implemented in Italy since the 2000s: the "Bersani decree" in 2006 and the "Monti reform" in 2011.

Although the liberalization of Italian professional services was remarkable in some respects, initial conditions differed substantially across occupations, while the pace and extent of regulatory reform also varied substantially. To measure the strictness of regulation, we build an index for 14 occupations and for three different cohorts (i.e., before and after each reform). The propensity of children to follow their parents' career is measured using data from the Labor Force Survey, matching the degree program on which they are enrolled with the occupation of their parents. Namely, we proxy occupational persistence with an indicator that is equal to 1 if children pursue a course of study that naturally leads to the same occupation as their parents.

We find that the progress toward a more market-friendly regulatory environment leads to a substantial decrease in the propensity for career following. These results suggest that intergenerational persistence in certain occupations depends to a large extent on the existence of positional rents generated by lack of competition. In other words, our findings suggest that regulation significantly biases the allocation of individuals across occupations based on the parental occupation. The impact is stronger for professions in social sciences and in areas where the demand for professional services is higher. Moreover, the impact of regulation on occupational persistence is stronger for less able children, thus confirming allocative inefficiencies in the distribution of talents across occupations.

Chapter 1

Tax-induced Transfer of Residence: Evidence from Tax Decentralization in Italy

1.1 Introduction

Since the seminal contribution of Tiebout (1956), there is a controversial debate about the degree of tax autonomy that should be granted to local governments. At the center of the debate lies the trade-off between the benefits of diversity in fiscal policies and the costs of loosing tax bases when individuals "vote with their feet." This debate is not limited to the academic world, but attracts much attention in the public debate.[1] The threat of tax base mobility puts hurdles on the ability of governments to redistribute income and finance spending through progressive taxation (Mirrlees 1982; Piketty and Saez 2013) and might trigger socially inefficient tax competition (Lehmann et al. 2014).

Despite the recent economics literature has seen an increase in research on migration, a recent survey of the literature (Kleven et al., 2020) has emphasized that "direct empirical evidence on the responsiveness of individual locations to taxes has been remarkably scant". In particular, most of the existing literature has focused on specific segments of the population (e.g., football players, highly paid foreigners, inventors or star scientists) that might be substantially sensitive to taxes, both because they tend to be less tied to specific firms and because their skills are less likely to be location-specific. Two empirical challenges are likely to explain the paucity of empirical evidence. First, information on migration patterns across locations is difficult to retrieve. Second, tax variations need to be orthogonal to all the other factors influencing location choices, such as local labor market conditions, public goods provision and amenities.

In this paper, we study whether local income taxation distorts the location choice of broader segments of workers. We focus on Italy, which offers suitable variation in local income taxation across the nearly 8,000 municipalities and 20 regions along with novel fine-grained data on tax base and individual mobility. To identify the effect of taxation

[1] Within-country transfers of tax residence have recently received much attention in the public debate, following episodes of wealthy taxpayers moving their tax residence for tax purposes (see, e.g., New York Times, "Trump, Lifelong New Yorker, Declares Himself a Resident of Florida", October 2019).

on mobility, we exploit a series of recent tax decentralization reforms that gave rise to variation both over time and across income brackets within locations.

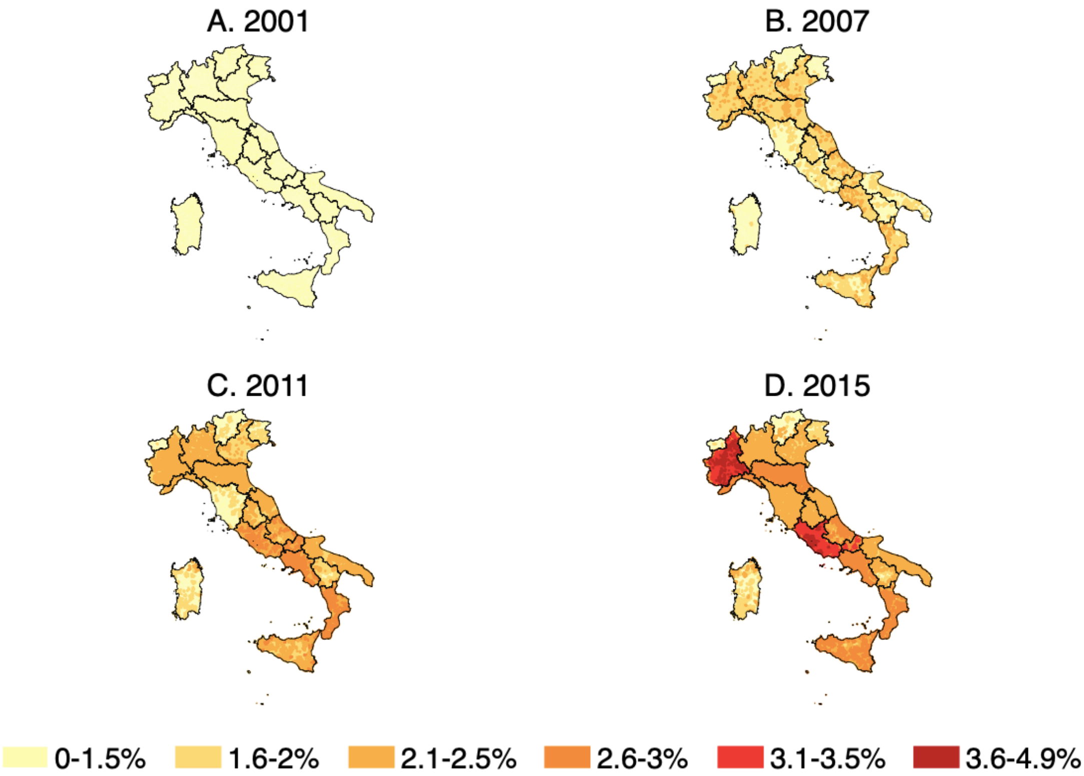
As shown by Figure 1.1, while the income tax rate set by local governments was substantially similar across places in the early 2000s, significant variation began to emerge after 2007 and 2011, when the national government granted municipalities and regions greater power to set different tax rates across income brackets. The local average tax rate on personal income faced by top incomes (summing up the tax rate set by regions and municipalities) varies *across locations for a given income level* by as much as 4.9 percent. The tax rate presents variation not only across locations over time, but also *across income brackets for a given location* (see Figure 1.2). These reforms thus give us a unique opportunity to study how taxation affects location choices in a country where income taxes are purely residence-based and several local public goods (e.g., education, public healthcare, voting) are exclusively provided to their residents.[2]

Our analysis focuses on two outcomes: the location of the tax base and the probability of changing tax residence across locations. To study the effect of local taxes on the tax base, we use taxable income data from tax returns grouped at municipality-income bracket-year cell and a newly compiled dataset on tax rates and tax exemption cutoffs set by municipalities and regions. Our main empirical approach relates changes in the tax base with *within-municipality cross-income bracket* tax rate changes generated by the adoption of a progressive local tax scheme. This strategy allows to flexibly control for municipality-specific time-varying amenities or economic shocks, and to account for secular trends in inequality and cost of living across places and/or income groups. In our final dataset, we exploit 89,860 local tax rate changes implemented since the early 2000s.

To estimate the effect of taxation on the probability of moving, we make use of administrative data that record the origin and destination municipality of almost 12

[2]Income taxes are residence-based in several countries, including Canada, Spain, Switzerland, and some states in the US.

Figure 1.1: Evolution in the local average tax rate on personal income



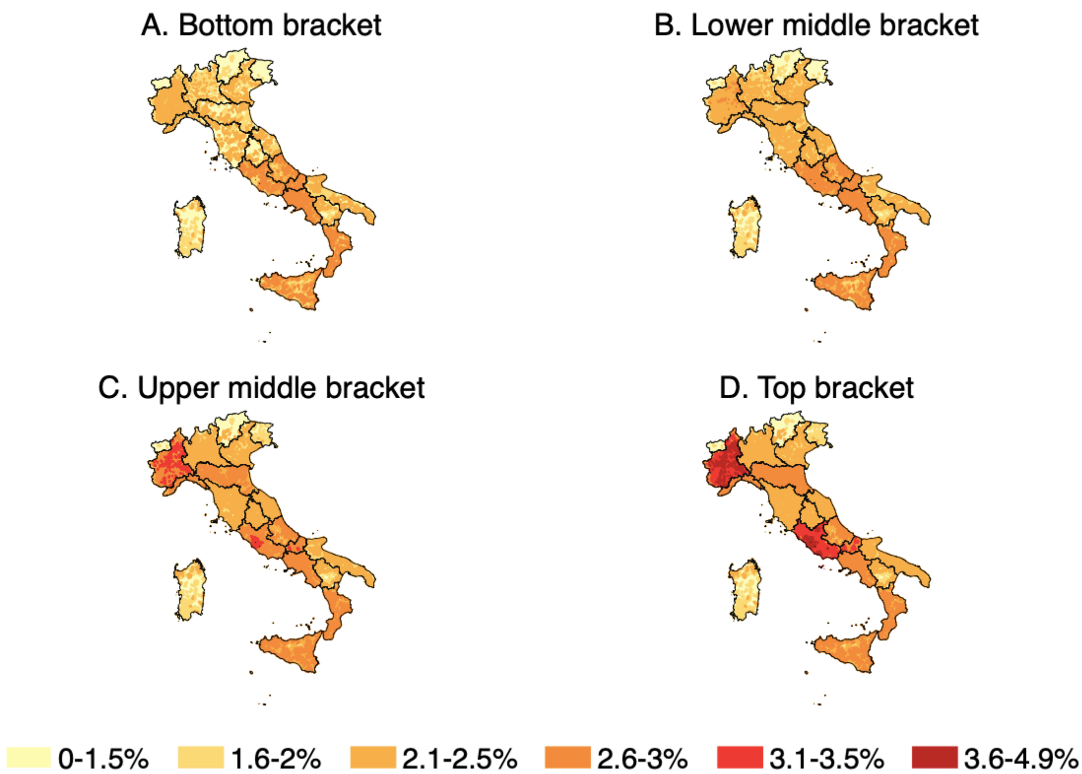
Note: The figure depicts the evolution in the local average tax rate on personal income defined at the 99th income percentile (%). The black line indicates regional boundaries.

millions of tax residence's transfers registered over the last decade. We use this information to compute the outmigration odds-ratio, that is the probability of moving from an origin to a destination location relative to the probability of not moving at all. Then, we relate changes in the outmigration odds-ratio with changes in the net-of-tax rate differential across location pairs to estimate the mobility elasticity. By focusing on changes over time, within a given location pair, our model absorbs all time-invariant factors that can shift the demand and supply of individuals across locations, as well as any origin location- or destination location-specific shock.[3]

For a simple illustration of our methodology, consider a wealthy taxpayer working and living in Rome. Suppose she owns two homes: a main residence in Rome and a second home in Costa Smeralda (Sardinia), an attractive venue for the rich where

[3]This strategy is similar to the approach implemented in Moretti and Wilson (2017) to study migration of star scientists across states in the US.

Figure 1.2: Local average tax rate by bracket in 2015



Note: This graph depicts the local average tax rate on personal income (%) in 2015 for taxpayers in bottom, lower middle, upper middle and top income bracket. The black line indicates regional boundaries.

income is taxed at a lower rate. After the decentralization reforms, moving the tax residence from Rome to Costa Smeralda would reduce the tax burden by 9,783 euros per year.[4] Following a tax increase in Rome, we can compare post-reform change in transfers of residence towards Costa Smeralda, that suddenly became a more attractive location for tax purposes, with transfers of residence towards locations with a similar tax rate change, where tax-related reasons for moving the residence remain constant. Since a tax change in Rome affects the net-of-tax rate differential with respect to all the other potential destinations, we can identify the effect of taxes by accounting for any political economy or business cycle factors that may have coincided with or led to a tax rate change in Rome (or in a destination location).

We find that taxation affects the location of the tax base. On average, switching

[4]This figure is based on the 2015 local top tax rate differential between the two places and the average income reported in Rome's top income group.

from a flat to a progressive local income tax schedule reduces the municipal tax base by about 1.2 percent. In an event study exploiting the staggered introduction of a progressive tax across municipalities, we show that both the tax base and the stock of taxpayers in the top bracket and in the bracket just below were on very similar trends before the local tax scheme switch. Following the local tax scheme change, taxable income and number of taxpayers in the top bracket began to gradually fall. We estimate a tax base elasticity of around 1.2, which mostly reflects an effect taking place over the extensive (mobility) margin.[5] The impact is significantly dampened when higher taxes translate in improved municipality amenities, but it is more intense in places with a higher concentration of capital owners. This result is qualitatively similar when we implement a border discontinuity approach, which exploits *regional* income tax differentials across municipalities located close to the regional border. Delving into a specific case study, we find results in accordance with our main identification strategy by exploiting an unique episode of a tax cut imposed by the national government to a region after an "illegal" tax increase.

We then provide evidence that our municipality-level results can be explained by taxpayers actively moving their tax residence to minimize their tax liability. We find that the probability of moving from an origin location o to a destination location d significantly increases when the net-of-tax rate in d increases with respect to o. On average, a 1 percent increase in the net-of-tax rate differential raises transfers of tax residence by around 2.2 percent (from a baseline of around 49 individuals moving within a location pair).

To get a sense of the magnitude of our estimated mobility elasticity, consider the effect of the increasing tax differential within the Milan (origin)-Rome (destination) province pair. Rome and Milan had the same local tax rate (1.4 percent) in 2007, but

[5] The fact that this elasticity estimate is larger than those based on country-level variations (see Saez et al. 2012) reflects the mechanical relationship between elasticities and jurisdiction size (Kanbur and Keen 1993).

in 2015 the top marginal tax rate differed by 1.7 percentage points (Rome tax rate was 4.23 percent, while Milan had a tax rate of 2.53 percent). According to our estimate, the number of individuals moving from Milan to Rome would decrease by around 47.9 individuals every year. Assuming that these individuals were located in the Milan's top income bracket, these "missing" movers would reduce the projected increase in the Rome's tax base (revenue from local income taxes) by 13,400 (560) thousands of euros.

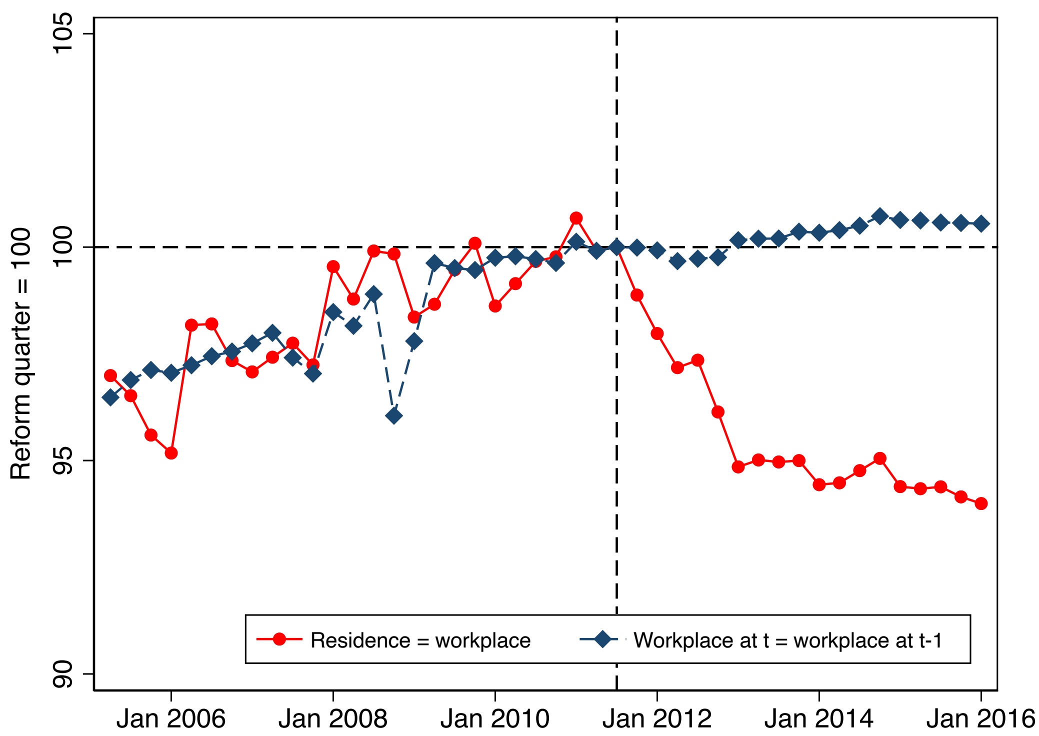
We cannot completely rule out the possibility that our estimates are biased by the presence of unobserved local shocks, but the credibility of our estimates is enhanced by the weight of available evidence. First, we do not find any pre-trend: tax base and mobility patterns were not systematically correlated with the tax rate change before any tax reform. Second, the baseline effects are qualitatively similar when we use the national government decentralization reform as instrument for local tax rate changes. Third, estimated elasticities are qualitatively similar when we control nonparametrically for time-varying shocks to origin or destination places.

The fact that the local business cycle appears to be a trivial issue would suggest that mobility purely reflects a change in tax residence instead of any labor market shocks. Transfers of tax residence data, however, do not allow us to disentangle a real from a fraudulent move, where a taxpayer changes the tax residence to a second home without physically moving. Distinguishing between a labor market-driven response from a simple change in home address does not matter under a municipality-specific tax revenue perspective, but it is crucial in terms of welfare conclusions and policy recommendations (Chetty 2009).

We shed light on the nature of the migration response by comparing workplace changes vis-à-vis with the place of residence using labor force survey data. We find limited, if any, evidence of job-related changes. In contrast, we find a change in the place of residence, in line with our main findings from transfers of tax residence data. This fact can be visually detected by looking at the raw data presented in Figure 1.3: the

2011 tax decentralization reform generated a sharp change in the probability of transferring the residence, but not in the workplace location. One simple interpretation for these findings is that taxpayers changed their tax residence by "the mere stroke of a pen" (Slemrod 2010). This tax-induced separation between workplace and residence location is substantially larger for chief executive officers, high-skill men, and unmarried individuals.

Figure 1.3: Comparing tax residence and workplace



Note: The figure compares the evolution in the share of individuals living in the same municipality where the workplace is located (red solid line) and the share of individuals changing the workplace with respect to the previous quarter of year (blue dashed line). The dashed vertical line refers to the decentralization reform, which gave rise to larger spatial differences in the local income tax rate. The two series are normalized to 100 in the reform quarter.

In the last part of the paper, we study the efficiency costs of local income taxation and discuss the implications of our results for tax revenue and the revenue-maximizing local income (top) tax rate. Although migration is an often-cited justification in proposals to avoid tax progressivity at local level, we find that the benefit of additional revenue from adopting a progressive tax greatly exceeds the cost of foregone revenue due to relocation. Our results, at least over the medium run, are consistent with Epple and

Romer (1991) and Agrawal and Foremny (2019), who show that local redistribution is feasible with migration, but in contrast to Feldstein and Wrobel (1998), who show that local redistribution involves large efficiency costs. Building on the elasticity estimate, we find that the optimal income tax-revenue maximizing rate would be larger than any existing ones set by local governments in Italy.

Our paper contributes to three strands of the literature. First, we contribute to the literature studying the effect of taxation on mobility (see Kleven et al. (2020) for a recent review). A series of recent papers has provided suggestive evidence on the impact of taxation on mobility of football players (Kleven et al. 2013), highly paid foreigners (Kleven et al. 2014; Schmidheiny and Slotwinski 2018), inventors (Akcigit et al. 2016), star scientists (Moretti and Wilson 2017), elderly taxpayers (Bakija and Slemrod 2004) and the rich (Schmidheiny 2006; Young et al. 2016; Martinez 2017; Agrawal and Foremny 2019). However, these studies do not allow us to provide a conclusive comprehensive answer on the effect of taxation on migration, given that they target specific segments of the population that might be substantially more mobile. In fact, as pointed out by Kleven et al. (2020), a key question that has not been addressed by the literature is whether income tax rates distort the location choice of broader segments of workers and, if they do, how large are the responses and what are the implications for policy. We attempt to fill this gap by using administrative data covering the *whole* population.

Second, we contribute to the literature studying the responsiveness of the tax base to income taxation (see Saez et al. 2012 for a review). To the best of our knowledge, however, no quantitative evidence exists for Italy, a high-tax evasion and relatively high-tax burden country.[6] Moreover, this paper relates with a growing literature exploiting state or local taxes for identifying the impact of tax policies on several economic outcomes, including corporate tax incidence (Fuest et al. 2018), reallocation of business activity (Giroud and Rauh 2019), welfare of workers, firms and landowners (Suárez Serrato and

[6]One exception is Rubolino and Waldenström (2019), which focus on rich taxpayers and estimate an elasticity of 0.19 by exploiting historical variation in top marginal tax rate and top income shares.

Zidar 2016), incidence on welfare of heterogeneous residents (Brülhart et al. 2020), job creation (Slattery and Zidar 2020), misallocation costs (Fajgelbaum et al. 2019), cross-border income shifting (Milligan and Smart 2019), foreign direct investments (Hines 1996), and innovation (Moretti and Wilson 2014; Akcigit et al. 2018).

Finally, our findings have implications for the debate on costs and benefits of fiscal decentralization. The economic implications of decentralization have been discussed by economists since the pioneering contribution of Tiebout (1956). Tiebout's core idea emphasizes the benefits of diversity in public good provisions and taxation across local jurisdictions. Yet, the threat of tax base mobility is likely to undermine attempts of local governments to perform redistributive policies (Musgrave 1959; Oates 1972; Feldstein and Wrobel 1998). Our paper shows that efficiency looses from local income taxes are limited, thus making local redistribution feasible at least in the medium-run. This finding can be important for several countries with a federal tax structure (see Glaeser (2013) and Brülhart et al. (2015) for a review on city-level taxation across countries).[7]

The rest of the paper is organized as follows. Section 1.2 provides background information on local income taxation in Italy. In section 1.3, we describe our data sources. Section 1.4 presents the effect of local income taxation on the location of the tax base. In Section 1.5, we analyze and discuss the effect of local income taxation on the probability of changing residence. Section 1.6 studies the implications of our main findings for tax revenue. Section 1.7 concludes.

1.2 Local income taxation in Italy

Italy is composed of three different sub-national tiers of government: there are 20 regions (*Regioni*), 107 provinces (*Province*), and 7,918 municipalities (*Comuni*). The 1998 tax reform granted regions and municipalities the possibility to levy a surtax on per-

[7]Despite many countries allow cities to raise taxes, there is very limited empirical evidence on the responsiveness of the tax base to *city*-level taxes. One notable exception is Haughwout et al. (2004), which estimate a very large elasticity focusing on four US cities.

sonal income on top of the income tax rates set by the national government.[8] However, spatial tax rate differences were limited, as i) the government capped the regional and municipal tax rates to 1 and 0.5 percentage points, respectively; ii) any tax rate increase could not be larger than 0.2 percentage points; iii) the tax rate could not differ across income groups. Geographical dispersion has begun to emerge since 2007, when regions and municipalities have been allowed to raise the tax rate to a maximum of 1.4 and 0.8 percent, respectively, and to introduce an exemption threshold. The final step was the 2011 reform, which granted regions and municipalities the possibility to set different tax rates across income brackets and lifted the top regional marginal tax rate cap to 4 percentage points.[9]

Revenue from local income taxes contribute to finance local public spending and accounts for nearly 16 percent of total personal income tax revenue raised in Italy (around 28,300 millions of euros). To simplify the tax collection process, the national government set restrictions on the definition of the tax base and on the structure of tax rates and tax brackets. First, the tax base is uniformly defined and composed of taxable income (i.e., gross income minus deductions), which includes positive incomes from all sources. Second, in the case regions or municipalities implement a graduated tax scheme, the rates are required to i) be structured according to the same income brackets defined by the national personal income tax; ii) diversified and increasing with income. The fact that regions and municipalities share the same tax base and tax brackets guarantees comparability both across places and over time. Moreover, local income tax rates cannot be deducted from income taxes paid to the central government. The municipal and regional tax rates are salient to taxpayers: when filing their tax forms, employees find information on both the central, regional and municipal income tax rate paid.

[8] See article 50 of law 446/1997 for the regional income tax and article 1 of law 360/1998 for the municipal income tax. Appendix subsection A1 offers a detailed description of the fiscal decentralization process.

[9] 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 revenue and promote fiscal equity.

Italy has a residence-based tax system: the tax rate applies to the taxpayer's tax residence at the beginning of the year. While the tax residence is unambiguously determined when a taxpayer spends the entire year in a single municipality, uncertainty arises when she lives in different locations over the year. According to the Italian Income Tax Code, the relevant criterion to define the tax residence is the physical location in a municipality for at least 183 days (see Article 2 of *Testo Unico Imposta sui Redditi*). In practice, this might be cumbersome to verify for some categories of workers (e.g., managers or chief executive officers working for several firms located in different places) or if taxpayers engage in *ad hoc* manipulation of the (self-reported) tax residence (e.g., by moving the tax residence to second homes). The home address of each individual is recorded in the population registry, which is managed by towns and updated daily. Individuals disclose this information for accessing to public goods exclusively provided to residents, such as voting in local elections, public school enrollment and public healthcare.

Any adult individual can transfer her residence by communicating the new address to the registry office of the previous municipality of residence. Applications are submitted online at zero cost. Local police will then inspect the veracity of the transfer within 45 days.[10] While changing residence can generate a change in tax liability, changing *domicile* does not matter.[11] In short, what matters for local taxation is the tax residence: workplace and domicile location have not any (direct) effect on the income tax burden.

[10] If the change of residence is not accepted by the origin municipality's local administrators or verified by the destination municipality's police, local administrators cancel the registration and restore the previous registry position. False declarations of residence entail the payment of a fine and up to two years' imprisonment (articles 75 and 76 of D.P.R. 445/2000; article 485 of criminal law).

[11] The law defines the residence as the "place of usual residence" and the domicile as the "place of business and interests" (see article 43 of civil code). As a response to larger tax differentials induced by the 2011 reform, we observe a substantial spike in the number of google search for "difference domicile and residence." (see Figure A9).

1.3 Data

Our empirical analysis rests on three main sources of data. First, we collect income group $\times$ municipality-level panel data on the tax base, stock of taxpayers and local tax rates. Second, we use administrative data on the universe of tax residence's transfers. Third, we compare residence and workplace location using labor force survey data.

1.3.1 Taxable income, stock of taxpayers and tax rates

The Italian Ministry of Economy and Finance (*Ministero Economia e Finanza*) provides data on income and stock of taxpayers for seven income intervals and for each municipality as reported in tax returns over the 2001-2015 period. Income intervals are constant both over time and across municipalities to guarantee comparability. Nominal income data are then converted to real income using the consumer price index and 2015 as the base year. The tax unit is the individual and the definition of income is taxable income (i.e., gross income minus deductions) as defined by the national government. Taxable income consists of all sources of income, such as labor (including pensions), business and capital.[12]

Table 1.1 reports information on the aggregate average tax base and the stock of taxpayers in each income group. In the first four columns, we display information on the average tax base (expressed in millions of 2015 euros); the last four columns provide information on the average population in each bracket. Considering that the median (average) population size of a municipality is of 2,436 (7,418) individuals, the detailed nature of the data allows to study even very tiny groups of taxpayers exposed to different tax rates over time and across places under a uniformly defined tax system. For instance, the median (average) number of taxpayers observed in the top income

[12] In the case a municipality has introduced a tax exemption cutoff, we are still able to observe incomes reported by those below the exemption cutoff as they still have to fill tax returns to pay the national income tax.

group of a municipality in a given year is only 13 (60).

Table 1.1: Tax base and stock of taxpayers data

Income group	Tax base (1,000 of 2015 euros)				Stock of taxpayers			
	mean (1)	sd (2)	min (3)	max (4)	mean (5)	sd (6)	min (7)	max (8)
1-10,000	8,604	39,950	17	2,834,531	1,753	8,616	4	638,911
10,001-15,000	10,304	48,514	44	3,478,410	821	3,859	6	276,612
15,001-28,000	30,371	164,778	68	1.09e+07	1528	8192	3	545,725
28,001-55,000	27,536	224,610	108	1.84e+07	802	6,353	2	513087
55,001-75,000	6,921	66,598	223	4,944,214	109	1,044	9	77,506
75,001-120,000	8,056	80,449	0	5,743,228	88	873	0	62,438
120,001-	12,983	157,921	0	8,518,113	60	629	0	32,888

Note: For each income group, this table reports the tax base (columns 1-4) and the stock of taxpayers (columns 5-8) as reported in tax returns. The tax base is converted in 2015 thousand of euros. Tax returns data covering 7,918 municipalities averaged over the 2001-2015 period.

We then match these data with a rich comprehensive panel dataset that we compiled on local income tax rates for each income bracket and year set by regions and municipalities. In computing the bracket-specific tax rate, we calculate the marginal and average bracket-specific tax rate by also taking into account the tax exemption cutoff set by municipalities or regions. As tax brackets thresholds are fixed in nominal terms over time and cannot be modified by municipalities or regions, we are able to match the bracket-specific tax rate to its tax base very precisely.[13]

The fact that the local income tax schedule is fixed in nominal terms generates a *bracket creep*: inflation leads taxpayers to "creep" to a higher bracket and, thus, to face a higher marginal tax rate (Saez 2003). In fact, we observe an upward (downward) trend in the share of taxpayers located in top (bottom) brackets (see Appendix Figure A2). One concern is that this source of real change in tax rate schedules might systematically differ across places.[14] If this is the case, then taxpayers located in places with higher

[13]Measurement errors might arise when we account for the municipality-specific tax exemption cutoff. As municipalities do not face any constrain in setting the cutoff below which income is untaxed, we have cases where the exemption cutoff differs from the tax bracket cutoffs we observe in tax data. In this case, the marginal tax rate in an income bracket is a linear combination of zero and the marginal tax rate applied to the bracket above the tax exemption cutoff. This measurement error affects around 1.2 percent of observations.

[14]Boeri et al. (2019) show relatively small dispersion in nominal wages across Italian provinces, but substantial heterogeneity in real wages (mostly driven by differences in housing costs).

inflation rate would be more likely to "creep" to a higher bracket and thus experience a rise in the marginal tax rate than those located in places with a lower inflation rate. Our main empirical approach will account for this issue by interacting year dummies with dummies for each income bracket and dummies for each province, thus accounting for any change in cost of living that differ across provinces and/or income groups.[15]

Out of the potential $7 \times 7,918 \times 15 = 831,390$ income bracket-municipality-year cells, we observe the total tax base, population stock and the tax rate of 704,609 cells.[16] For a given income bracket-municipality-year cell, we observe 89,920 tax rate changes. As shown in the Appendix Figure A3, tax rate changes were mostly implemented in 2007 and 2012, the years after the tax decentralization reforms. Obviously, tax rate changes became more common in the post-2012 period, when local governments were allowed to set different tax rates across brackets. The modal (average) number of tax rate changes observed in a given income bracket-municipality group over the 2001-2015 period is 2 (1.63). In around one-fifth of income bracket-municipality cells, we do not observe any tax rate change (see Appendix Figure A4).

Table 1.2 displays the statutory local tax rate (summing up both the regional and municipal rate) on personal income set in the low, middle and top income bracket for the 20 largest Italian cities. We present here the tax rate in years 2001, 2011 and 2015 (for a graphical representation of the local tax rate over time, see Appendix Figure A5). Two remarks emerge from this table. First, the local tax rate varies *across locations for a given income level*. For instance, in 2015 a rich taxpayer in Rome can reduce her marginal tax rate by 2.3 percentage points by moving the tax residence to Florence. This transfer of residence would allow to save around 5,277 euros of taxes per-year.

Second, the tax rate presents variation *across income groups for a given location over*

[15]Furthermore, when we alternatively deflate the tax base by municipality-specific house prices, which should absorb part of the change in cost of living, we find very similar results.

[16]Taxable income data are not subject to censoring: 94.7 percent of missing values can be attributed to a "real" missing value, i.e., we do not observe any taxpayer in an income bracket-municipality-year cell. In the remaining cases, we were unable to match tax data with income data because of changes in the municipality identifiers across census waves or by unions or divisions between municipalities.

Table 1.2: Local statutory marginal tax rate on personal income in Italian cities

City	Year: 2001			Year: 2011			Year: 2015		
	Low	Middle	Top	Low	Middle	Top	Low	Middle	Top
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Rome	0.900	0.900	0.900	1.400	1.400	1.400	1.730	4.230	4.230
Milan	0.900	0.900	0.900	1.200	1.400	1.400	1.230	2.530	2.530
Naples	1.300	1.300	1.300	1.900	1.900	1.900	2.030	2.830	2.830
Turin	1.000	1.000	1.000	0.900	1.900	1.900	1.620	3.550	4.130
Palermo	0.900	0.900	0.900	1.800	1.800	1.800	2.530	2.530	2.530
Genua	1.170	1.170	1.170	1.370	1.870	1.870	1.230	3.110	3.130
Bologna	1.100	1.100	1.100	1.100	2.000	2.100	1.330	2.830	3.130
Florence	1.000	1.000	1.000	1.200	1.200	1.200	1.420	1.880	1.930
Bari	1.300	1.300	1.300	1.400	1.400	1.400	1.330	2.510	2.530
Catania	0.900	0.900	0.900	1.600	1.600	1.600	1.730	2.530	2.530
Verona	1.100	1.100	1.100	1.200	1.700	1.700	1.230	2.030	2.030
Venice	0.900	0.900	0.900	0.900	1.400	1.400	1.230	2.030	2.030
Messina	1.400	1.400	1.400	2.200	2.200	2.200	2.530	2.530	2.530
Padua	1.100	1.100	1.100	0.900	2.000	2.000	1.230	1.930	1.930
Trieste	0.900	0.900	0.900	1.700	1.700	1.700	0.700	2.030	2.030
Brescia	0.900	0.900	0.900	1.200	1.400	1.400	1.230	2.530	2.530
Parma	1.100	1.100	1.100	1.100	1.300	1.400	1.330	2.830	3.130
Taranto	1.300	1.300	1.300	1.700	1.700	1.700	1.330	2.510	2.530
Prato	1.200	1.200	1.200	1.400	1.400	1.400	1.920	2.180	2.230
Modena	0.900	0.900	0.900	1.600	1.800	1.900	1.830	2.550	3.130
Reggio C.	0.900	0.900	0.900	1.400	1.400	1.400	2.530	2.530	2.530
Reggio E.	1.100	1.100	1.100	1.300	1.500	1.600	1.330	2.520	3.130
Perugia	0.900	0.900	0.900	0.900	1.800	1.800	1.230	2.480	2.630
Ravenna	0.900	0.900	0.900	1.700	1.900	2.000	1.880	2.600	3.130
Livorno	1.100	1.100	1.100	1.300	1.300	1.300	2.220	2.480	2.530

Note: This table displays the local marginal tax rate on personal income (summing up the regional and municipal rate) in low, middle and top bracket for the 20 largest Italian cities.

time. This source of tax rate variation has emerged after the possibility of introducing a tax exemption cutoff and to set a graduated tax scheme. For instance, the tax rate in Rome's top bracket increased by 3.3 percentage points over the 2001-2015 period, while the bottom tax rate raised by 0.83 percentage points over the same period.

Focusing solely on the municipal income tax schedule, Figure A6 displays whether a municipality set its own tax rate and, if so, whether it is a single flat rate or a series of different increasing tax rates across brackets. We find that nearly half of the municipalities in our sample taxed income with a flat rate in 2015, despite the share of municipalities with a progressive tax has firmly risen since 2007 (up to around 35 percent). Moreover, the number of municipalities without an income tax has shrunk from

3,341 (42 percent) to 1,223 (15 percent) over the period being analyzed (see Figure A7). We present the evolution in the bracket-specific municipality tax rate in Figure A8. On average, we observe a threefold increase in the difference between the top and the bottom bracket since the early 2000s. Differences across middle brackets have emerged as well, but are relatively less marked.

In addition to local personal income tax rates, we also retrieve data on the property tax rates applied both on the main residence and on second homes. Property tax data are provided by the Italian Institute of Finance and Local Economy (*Fondazione IFEL*) for each municipality over the period of interest.

1.3.2 Transfers of tax residence

To study individual mobility, we use a dataset covering all the transfers of tax residence registered within the country. These data are based on administrative forms (called *modello APR.4*) filled out and organized by the Italian Civil Registry and provided by the Italian Institute of Statistics. Our sample consists of all the 11,932,720 transfers of tax residence observed over the 2007-2015 period.

For each individual moving the tax residence in a year t, the dataset lists the origin address of residence in year $t-1$ and the destination address in year t, allowing us to track the origin and destination municipality of each transfer. We will perform the analysis at the *province*-pair level (107×107), since a municipality-level analysis would be cumbersome both for computational issues (we would need to calculate $7,918 \times 7,918 \times 9$ triplets of origin municipality - destination municipality - year) and because there would be no observations in the vast majority of the municipality pair-year cells.

Using the origin and destination province of each transfer of residence, we compute the number of individuals moving in a year within each origin-destination pair of provinces (including those where origin and destination are the same). We use this number to compute our outcome of interest: the *outmigration odds-ratio*, that is, the probability of an individual moving from an origin province to a given destination province relative to the probability of not moving at all.

Table A1 shows the bilateral average annual outflows among the largest 20 provinces. Most of the transfers happens along the main diagonal, i.e., within the same province. Geography appears to be the main determinant even when we focus on mobility in cells different from the main diagonal: most of transfers still occur across provinces within the same region (e.g., Caserta - Naples, Varese-Milan, Salerno-Naples). Overall, within-region transfers account for around 63 percent of the observed changes of residence. Considering only movements across places located in different regions, the province pair with the most bilateral flows was Naples-Rome, where 2,949 individuals per year transferred their residence from Naples to Rome and 1,040 moved in the opposite direction.

The average number of transfers of residence within a province pair-year cell with positive migration outflow is 127 (which drops to 49 when we exclude within-province mobility). Out of the $107 \times 107 \times 9 = 103,041$ origin $\times$ destination $\times$ year cells, 92,651 have positive migration flows. Not surprisingly, province pairs without migration flows involve sparsely populated and far away provinces. Since our outcome variable - the log odds-ratio - is undefined when the migration flow is 0, 92,651 is the number of observations used in our baseline regressions.[17]

One caveat of the data is that we do not observe income of movers. To account for this issue, we compute the average tax rate by simulating taxes in all years and provinces for a representative taxpayer in the top percentile of the pre-tax national income distribution. This allows to hold fixed variations in income across provinces, thus guaranteeing that the variation in the tax rate is only due to statutory changes and not to local income shocks. We find similar results if we use the top marginal tax rate.

[17] The presence of cells with zero mobility flows might represent a bias if *systematically* associated with tax changes. We regress the probability of a missing cell on net-of-tax rate differentials, conditioning on our set of baseline controls. The estimated coefficients are not distinguishable from 0, indicating that missing cells are not systematically correlated with changes in tax differentials across pairs.

Although migration decisions should theoretically depend on the average tax rate, the marginal tax rate might be more salient and easy to retrieve.

1.3.3 Labor force survey

Our third source of data is the Italian Labor Force Survey (LFS), conducted by the National Institute of Statistics (ISTAT) during every week of a year. The annual sample is composed of over 600,000 individuals and represents the leading source of statistical information for estimating the main aggregates of the Italian labor market at the national and local levels.

One advantage of this dataset is that each individual self-reports past and current municipality of residence and workplace. This information allows us to track the location of their workplace vis-à-vis with her tax residence. Our main outcome variable is the probability of (fiscally) living in the same municipality of the workplace. The data also contain detailed demographic information, including age, civil status, education and occupation.

As in transfers of tax residence data, we need to stress that we do not observe individual income. However, we can impute the income group based on the reported occupation. Following the ISCO classification of occupations, we calculate both the average and marginal tax rate on personal income based on an income level equal to: i. the top income bracket for legislators, senior officials and managers (ISCO group 1); ii. the median income for professionals, associate professionals and technicians (ISCO groups 2 and 3); iii. the bottom income bracket for clerks, service workers and all the other low-skill workers (ISCO groups from 4 to 8).

Respondents in survey might report a different location from that reported in administrative data. Any discrepancy between information reported on tax residence data and labor force survey would suffer from measurement errors if systematically correlated with the local tax rate. In the spirit of the "traces of true income" approach

(Pissarides and Weber 1989) to detect tax evasion, administrative data would be more sensitive to tax changes than information provided in anonymous surveys. If this is the case, then tax-induced transfers of tax residence estimated from labor force survey data would provide a *downward* biased coefficient relatively to that estimated from administrative data.

A well-known criticism is that survey data are hardly reliable when it comes to analyzing richest individuals, given that response rates to surveys are plummeting for them. If the rich are undersampled and their mobility response to taxes is larger, then we would yield *downward* biased estimates. To assess the magnitude of this issue, we will perform a separate analysis on internal mobility within the football labor market. The football data allow us to focus on a segment of the workforce that it is both very wealthy and mobile (see Kleven et al. 2013 for an analysis of tax-induced mobility in the European football market).

1.4 Local taxation and location of the tax base

In this section, we study the effect of local income taxation on tax base mobility. We present different empirical approaches. First, we study tax base responsiveness to the effect of introducing a progressive local tax scheme. Second, we estimate the tax base elasticity by exploiting within-municipality cross-bracket variation in the (net-of-)tax rate generated by the adoption of a graduated tax schedule. Third, we perform a border discontinuity approach, where we relate the discontinuous change in the regional tax rate with variation in tax base across municipalities located close to the regional border. Finally, we leverage a tax cut imposed by the central government to the region of March following an illegal tax increase.

1.4.1 The impact of implementing a progressive tax schedule

We start our empirical analysis by comparing municipalities according to their local tax scheme in a difference-in-differences (DiD) empirical setting. We first split municipalities according to their local tax scheme:

$$i \in \begin{cases} Flat & \text{if } \tau_b = \tau_{b-1} \qquad\qquad \forall \tau_b \in T_i(b_1, ..., b_7) \\ \\ Prog & \text{otherwise,} \end{cases} \tag{1.1}$$

so that municipality i belongs to the progressive tax group if it exists at least a single marginal tax rate in the tax schedule $T_i(b_1, ..., b_7)$ such that $\tau_b > \tau_{b-1}$.

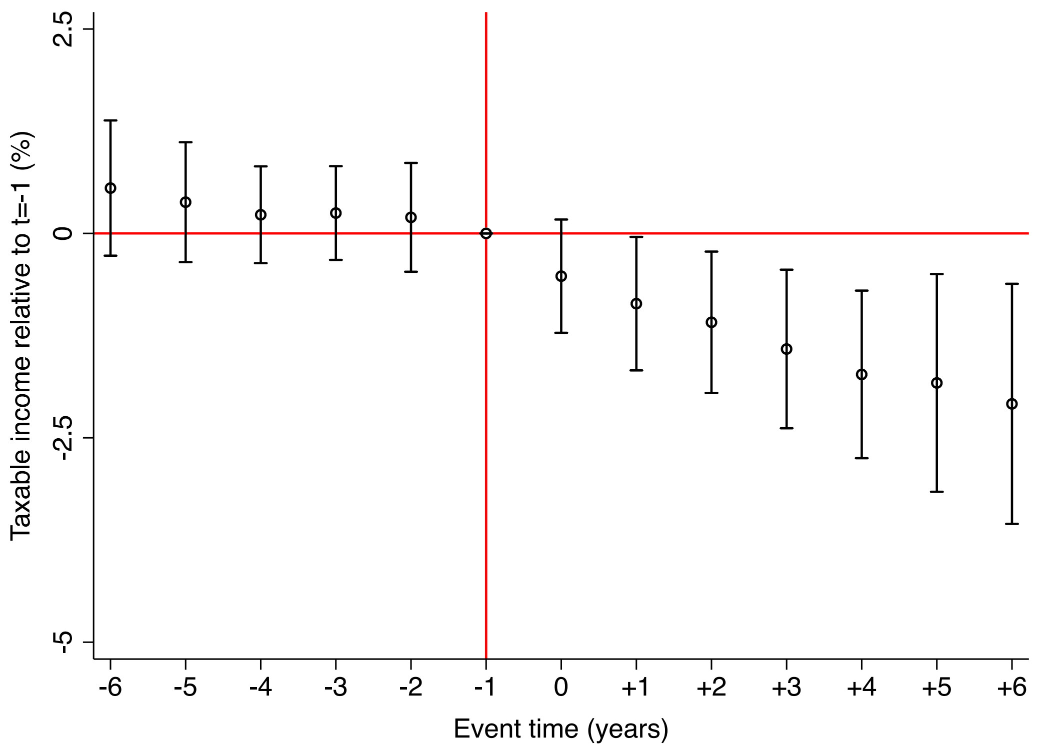
Then, we compare cross-municipality variation in the tax base in the year before and after the local tax scheme switch. This exercise allows us to assess whether the tax base in these two groups of municipalities followed a similar trend before the tax scheme switch, but diverge afterwards. Formally, we run a two-way fixed effects DiD event study specification of the following form:

$$log(y_{i,t}) = \sum_{j \neq -1} \beta_j \cdot 1(i \in Prog) \cdot 1(t = t_j) + \gamma_i + \delta_t + u_{i,t}, \tag{1.2}$$

where $y_{i,t}$ denotes the tax base in municipality i at year t. The interaction between a dummy for municipalities with a progressive tax schedule and years, $1(i \in Prog) \cdot 1(t = t_j)$, omits the year before the local tax scheme switch (denoted by $j = -1$), so that the DiD coefficient β_j can be interpreted as the effect at year t relative to the year before the local tax scheme change. In the absence of differential pre-existing trends, $\beta_j = 0 \; \forall j < -1$. By contrast, for $j > -1$, the coefficients β_j show the dynamic effects of implementing a progressive tax schedule on the tax base. γ_i and δ_t are municipality and year fixed effects, respectively. In some specifications, we also include province or local labor market $\times$ year fixed effects to account for any local shocks or policies. In our baseline approach, we cluster the standard errors at the municipality-level.

Our results are presented in Figure 1.4, which reports β_j estimates and 95 percent confidence intervals for up to 6 years before and after the local tax scheme switch. The graph shows that the difference in taxable income between progressive and flat tax municipalities was not significant when measured during the period before municipalities were allowed to switch to the progressive tax scheme, thus validating the parallel trends assumption. Then, we observe a gradual drop in the tax base, persistent up to 6 years after the implementation of a progressive tax schedule.

Figure 1.4: The impact of implementing a progressive local tax schedule



Note: The figure depicts the impact of switching from a flat to a progressive local income tax schedule. The figure plots coefficient estimates and the 95 percent confidence intervals: each point shows the effect of having implemented a progressive tax schedule for j years (if j >-1) or of starting the policy in j years (if j < -1) relative to the year before the tax scheme switch was implemented. Regressions include municipality fixed effects and year fixed effects. The sample includes 7,918 municipalities over the 2001-2015 period. Standard errors clustered at municipality-level.

Coefficient and standard errors estimates are reported in Table 1.3, where we estimate standard DiD specifications by interacting the dummy for municipalities with a progressive tax with a dummy for the period after the local tax scheme switch. The baseline model with municipality and year fixed effects shows that switching to a progressive tax scheme reduced the mean tax base by 1.2 percent. This translates in an average reduction of around 1,123 thousand euros per-municipality.

We investigate the robustness of this effect in the rest of the table. First, we control for province × year fixed effects (column 3) or local labor market × province × year fixed effects (column 4). The point estimates remain substantially similar. In column (5), we show that the impact is robust to the inclusion of several socio-economic, political and demographic municipality-specific controls.[18]

Table 1.3: Local tax scheme and tax base

	log(taxable income)				
	(1)	(2)	(3)	(4)	(5)
$1(i \in Prog) \cdot 1(t \in Post)$	-0.011***	-0.012***	-0.014***	-0.016***	-0.012***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	118,770	118,770	118,770	118,770	118,770
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
Province × year FE	No	No	Yes	Yes	Yes
LLM × province × year FE	No	No	No	Yes	Yes
Controls	No	No	No	No	Yes
Tax base (€1,000)	93,659	93,659	93,659	93,659	93,659

Note: This table shows the effect of switching from a flat to a progressive income tax. The sample is composed of 7,918 municipalities over the 2001-2015 period. Standard errors clustered at municipality-level in parentheses.

In the appendix A3, we report alternative specifications and robustness checks to test the sensitivity of our baseline results. First, we show that our results are mostly concentrated in municipalities where the slope of the tax rate progression is steeper. Second, we test the sensitivity of our DiD estimate to the presence of negative weights.[19] Following the recommendations of de Chaisemartin and D'Haultfoeuille (2020), we

[18]We include the following control variables: property tax rates on main dwelling and on second homes, share of population above 65, share of population below 15, a dummy for fiscal deficit, election-year fixed effects, gender, education attainment and age of mayor and each member of the town council, municipal public spending share in administration, development, law and order, education, and social welfare. Because these variables are likely endogenous to local income tax rates, we prefer not to include them in our baseline specification, but rather verify that our coefficient estimate is not sensitive to their inclusion.

[19]Negative weights emerge because β is a weighted sum of several DiD, each comparing the evolution of the outcome between consecutive time periods across pairs of municipalities. Given the staggered adoption of the progressive tax scheme, the "control" group in some of the comparisons might be treated at a later period. Then, its treatment effect at a later period gets differenced out by the DiD, thus generating the negative weights.

find that only 0.853 percent of the estimated average treatment effects receives a negative weight, and the ratio between negative weights attached to the regression and the standard deviation of weights is very large. We thus conclude that negative weights are not a strong concern in this setup. Third, we report similar effects when we deflate the tax base by the average housing price in a municipality, that allows to absorb, at least in part, any municipality-specific change in cost of living. Finally, we show that our estimates remain highly significant when we allow for spatial correlation in the error term by clustering the standard errors on a higher level of aggregation (Angrist and Pischke 2009).

Next, we explore heterogeneous responses. First, notice that our baseline estimates should be interpreted as measuring the effect of implementing a progressive tax scheme on tax base mobility after any endogenous change in the provision of public goods or improved amenities. If higher taxes translate in enhanced provision of public goods, our estimates are a lower bound of the effect of local taxation on the tax base. For instance, consider the case when a municipality raises the tax rate for improving schools. If taxpayers value schooling as an important determinant in choosing the residence, the disincentive effect of higher tax progressivity will be in part offset by improved municipality amenities. Therefore, if taxation is internalized in enhanced public goods, the mobility response would be in part dampened. To test this hypothesis, we split our sample according to the median value of municipal school spending (using municipal balance sheets data provided by the Ministry of Interior) to explore whether responses to the introduction of a progressive tax scheme are smaller where school spending is higher. Column (1) in Table 1.4 confirms this hypothesis: we find a positive effect on the interaction between implementing a progressive tax and a dummy for municipalities with larger spending to finance schools.

Second, we study whether tax base mobility is relatively larger in places with a higher share of property owners. Since the mobility response might actually involve

Table 1.4: Heterogeneity effect of implementing a progressive tax schedule

	log(taxable income)			
	(1)	(2)	(3)	(4)
$1(i \in Prog) \cdot 1(t \in Post)$	-0.024***	0.013***	-0.018***	-0.016***
	(0.004)	(0.004)	(0.004)	(0.003)
$\ldots \cdot 1(i \in Good\ schools)$	0.011*			
	(0.006)			
$\ldots \cdot 1(i \in Property\ owners)$		-0.036***		
		(0.005)		
$\ldots \cdot 1(i \in South)$			0.006	
			(0.007)	
$\ldots \cdot 1(i \in Special\ region)$				0.000
				(0.014)
Observations	118,770	118,770	118,770	118,770
Municipality FE	Yes	Yes	Yes	Yes
LLM × province × year FE	Yes	Yes	Yes	Yes
Tax base (€1,000)	93,659	93,659	93,659	93,659

Note: This table shows the effect of switching from a flat to a progressive local income tax on taxable income from a model that interacts the progressive tax post-reform indicator with four dummy variables: i. municipalities where school spending is larger than the median value, $1(i \in Good\ schools)$; ii. municipalities where the share of total income accruing from rental income is larger than the median value, $1(i \in Property\ owners)$; iii. municipalities located in Southern Italy, $1(i \in South)$; iv. municipalities located in a region with special autonomy, $1(i \in Special\ region)$. The sample is composed of 7,918 municipalities over the 2001-2015 period. Standard errors clustered at municipality-level in parenthesis.

just a pure change in tax location rather than a real labor-market driven response, we might expect higher effects in places where residents would easily respond to higher taxes by moving their residence to second homes. To test this hypothesis, we create a dummy equal to 1 for municipalities where the share of rental income (as a share of total taxable income) is larger than the median. In column (2), we show that the erosion in the tax base is relatively larger in municipalities where the share of rental income is larger than the median.[20]

Finally, we interact the progressive tax dummy with a dummy for municipalities located in the Southern part of Italy (column 3) or in regions that are granted more autonomy in managing their public resources (called *Regioni a Statuto Speciale*). In both cases, we do not find any significant difference. In Figure A12, we also test whether

[20] As long as the mobility response reflects a strategy to elude taxes (e.g., relocation to less taxed second homes), this result is in line with Marino and Zizza (2012), which show positive correlation between rentiers and attitude towards tax evasion. They estimate that rentiers evade, on average, 80 percent of their income, while the average population value is 13.5 percent.

the magnitude varies by population size. We find that the impact did not differ with respect to population size.

1.4.2 Cross-bracket analysis

Even in the presence of flat pre-trends, the research design presented in equation (1.2) would be invalid if local shocks systematically affected tax rates and tax base. Indeed, if other policies or shocks specifically hit a municipality at the same time it changed taxes, our estimates would be biased. We attempt to overcome this issue by focusing on tax rate variation *across income brackets within a municipality* generated by the adoption of a progressive tax scheme. The main advantage of using income bracket-level data is that it allows to directly control for any municipality-specific time-varying shock.

We start by focusing on top incomes to compare the evolution in the tax base and stock of taxpayers between the top income group and the income group just below. Although implementing a progressive tax is likely to affect taxpayers in both groups, we expect taxpayers in the top bracket to be relatively more affected than those in the bracket just below.[21]

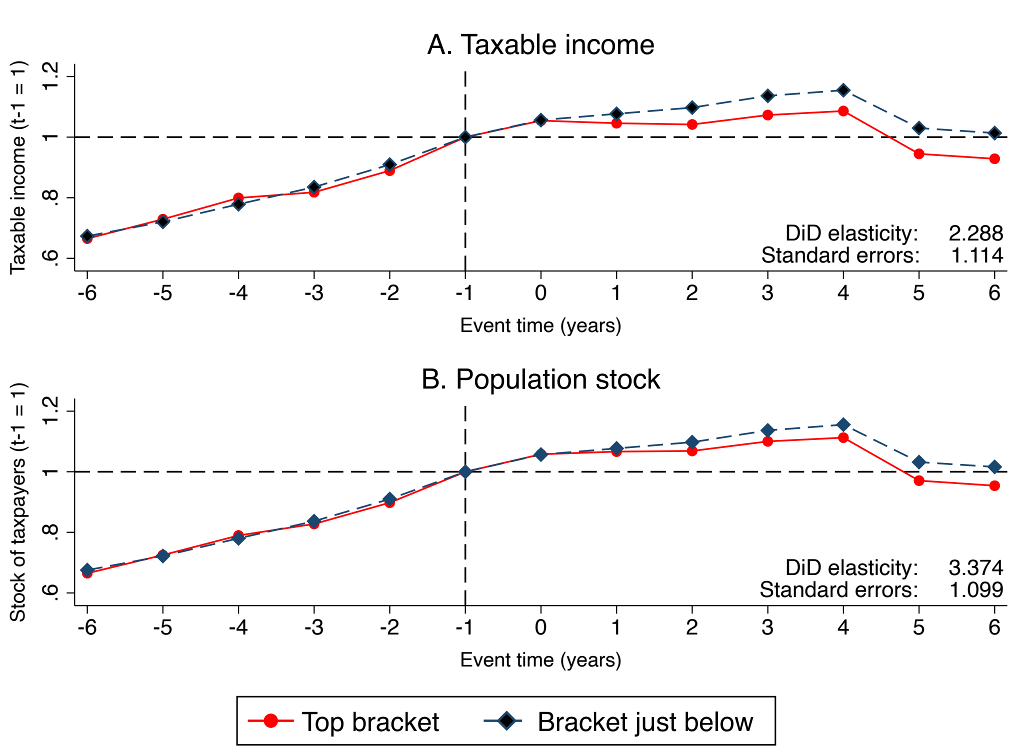
Figure 1.5 shows tax base and population stock trends in the two groups before and after the year when a municipality switched from a flat to a progressive tax. The two series are normalized to match in the pre-reform year. The upward-sloping pattern in these graphs is driven by the aforementioned "bracket creep": inflation leads taxpayers to "creep" to higher income brackets. The figure also reports DiD estimates of the elasticity of taxable income or population stock with respect to the net-of-marginal tax rate.[22] Three key insights emerge from this figure. First, the two groups were on a

[21]This strategy is similar in spirit to the studies exploiting the differential exposure of tax rate changes across income brackets or income groups, such as using lower income brackets as control group (Eissa 1995; Kleven and Schultz 2014; Akcigit et al. 2016) or across households with different family size or different income composition (Eissa and Hoynes 2006).

[22]The difference-in-differences elasticity estimates are based on regressions of log of taxable income or population stock on year dummies, dummies for income group-municipality interactions, and the log net-of-tax rate. Elasticity estimate is similar when using the net-of-*average* tax rate.

parallel trend over the six years leading up to the reform, whereas they started diverging immediately after the implementation of a progressive tax scheme. Second, tax base and population stock differences between the two groups are gradually increasing over time, which is consistent with the results relying on cross-municipality DiD. The effect of implementing a progressive tax schedule is larger both in absolute terms and in elasticity terms, as shown by the DiD elasticity estimates. Third, the fact that the evolution in the tax base mirrors the population stock trend suggests that most of the response takes place over the extensive (mobility) margin.[23]

Figure 1.5: Tax base and population response in top brackets



Note: The figure compares the evolution in taxable income (top graph) and number of taxpayers (bottom graph) in the top income bracket (red solid line) and in the bracket just below (blue dashed line). The dashed vertical line refers to the year before a municipality switched from a flat to a progressive local income tax, that raised the tax rate relatively more on the top bracket with respect to the bracket just below. We display DiD estimates of the elasticities of taxable income (population stock) based on regressions of log of taxable income (population stock) on year dummies, dummies for income group-municipality interactions, and the log net-of-tax rate.

In Appendix A3, we extend this graphical analysis by adding a wide set of fixed effects. Our most conservative estimate suggests that, on average, implementing a

[23]We also perform the same exercise by comparing the very bottom income bracket and the bracket just above. Figure A13 shows limited responses to the implementation of a progressive tax scheme.

progressive tax scheme would induce at most 3.904 top taxpayers to outmigrate from a municipality (that is, around 3.2 percent of the stock of taxpayers in the top bracket). This response amounts to a drop in the tax base by 854 thousand of euros. A rough comparison with the cross-municipality DiD estimate suggests that nearly fourth-fifth of the erosion in the tax base is driven by the migration response in the top bracket.

We then extend this analysis to each income group by exploiting all the 89,860 tax rate changes to estimate the net-of-tax rate elasticity of tax base and stock of taxpayers. This measure is informative about the excess burden of taxation and in predicting the revenue impact of tax changes. Formally, we run regressions as the following:

$$log(y_{b,i,t}) = \beta \cdot log(1 - \tau_{b,i,t}) + \gamma_{b,i} + \delta_{i,t} + \eta_{b,p(i),t} + u_{b,i,t}, \tag{1.3}$$

where $y_{b,i,t}$ is the tax base or population stock in the income bracket b in municipality i at time t. We include income bracket $\times$ municipality fixed effects, $\gamma_{b,i}$, to filter out permanent unobserved heterogeneity across municipalities within a given income bracket and across income brackets within given municipality. These fixed effects account for the fact that the rich in metropolitan cities might be more responsive to tax rate changes than the rich in a rural area, as well as for the fact that, within a specific city, preferences (or possibility) for tax residence relocation are not equally distributed along the income distribution. Municipality $\times$ year fixed effects, $\delta_{i,t}$, account for municipality-specific time-varying amenities or economic shocks. If a municipality becomes more attractive after tax rate changes because of policy changes correlated with the change in taxes (e.g., improvement in public amenities or increase in public spending), then these fixed effects would absorb such difference. Income bracket $\times$ province $\times$ year fixed effects, $\eta_{b,p(i),t}$, control for any different reasons (including regional policies or cost of living) for why tax base or population stock at different points in the income distribution and/or in different local labor markets might experience different income or population growth rates, aside from tax changes. Moreover, the inclusion of $\eta_{b,p(i),t}$

absorbs any region-specific change in income taxes, thereby leading our estimate to rely exclusively on *municipality*-level variation in the net-of-tax rate.

The tax base (or population stock) elasticity, β, yields the approximate percent change in taxable income (population stock) in a tax bracket, $y_{b,i,t}$, when $1 - \tau_{b,i,t}$ changes by 1 percent. In interpreting the elasticity estimate, we need to make four considerations. First, our empirical model captures the long-run mobility effect of tax changes. Since people need time for moving, the long-run effects are likely to be larger than the short-run effects. Second, the elasticity estimate needs to be interpreted net of any endogenous variation in local amenities and public services. As previously stressed, if higher taxes translate into improved amenities, then our elasticity estimate is a lower bound. Third, the size of the elasticity depends on the size of the jurisdiction (Kanbur and Keen 1993). As municipalities are very small open economies located next to each other, relocation costs are likely to be negligible and elasticity estimate larger than those estimated from larger jurisdictions, such as countries or states.[24] Fourth, the mobility response likely depends on whether tax rate changes are perceived as permanent or temporary. Temporary tax changes are likely to have smaller effects than permanent changes.

In estimating standard errors, we need to account for two issues. First, the error term might be correlated over time within the panel dimension, that is the income bracket $\times$ municipality (Bertrand et al. 2004). Second, the error term might be correlated, within a given income group, across municipalities because of any policy or shock directly affecting a specific income group. Likewise, the error term might be correlated, within a given year, across income groups sharing the same municipality because of any common municipality-specific shocks and for the fact that the tax rate did not vary across income brackets over the period before the implementation

[24] In the extreme case of very small jurisdictions, the elasticity becomes infinite. By contrast, very large jurisdictions present lower elasticities as it is costly to relocate (and in the extreme case of the full world, the migration elasticity is naturally zero).

of a progressive tax scheme. This source of bias gives rise to the classical clustering concern discussed in Moulton (1990). To account for these issues, we present standard errors that are robust to heteroskedasticity and allow for three-way clustering by municipality-income bracket, income bracket-year, and municipality-year (using the estimator proposed by Cameron et al. 2011).[25]

Elasticity estimates are presented in Table 1.5, which shows the elasticity of the tax base (top panel) and of the population stock in an income bracket (bottom panel) with respect to the marginal net-of-tax rate (we find similar results using the average net-of-tax rate). Columns (1) displays the elasticity estimate obtained from the full sample, while columns (2), (3) and (4) report the elasticity for the bottom bracket (or those below the tax exemption cutoff), middle bracket and top bracket, respectively. Our baseline elasticity is 1.188 for the tax base and 1.210 for the population stock, thus implying that the behavioral response to local tax rate change is over the mobility margin. We also find heterogeneous responses over the income distribution: the tax base elasticity is significantly larger at the top of the income distribution, while it is zero at the bottom. This trend is quite similar for the population stock elasticity, although the elasticity is less precisely estimated for the top group (where the average stock of taxpayers is 60).

1.4.3 Border discontinuity approach

One challenge with estimating the effect of local tax rates on tax base mobility is that tax rates might reflect local economic conditions. The basic idea of the border discontinuity (BD) approach is to get around this issue by focusing on municipalities close to the regional border, where there are sharp discontinuities in the *regional* tax rate,

[25]This three-way clustering strategy would allow us to deal with the first issue by allowing for unrestricted autocorrelation within each income bracket-municipality observations, which is the cross-sectional unit in our dataset; it accounts for the second issue by allowing for income bracket-year and municipality-year clusters. We are unable instead to use Driscoll and Kraay (1998) estimator of standard errors since it is based on large T asymptotics.

Table 1.5: Tax base and population stock elasticity

	Sample of taxpayers:			
	All	Bottom	Middle	Top
	(1)	(2)	(3)	(4)
	A. *Outcome*: log(taxable income)			
$log(1-\tau_{b,i,t})$	1.188**	0.019	1.106**	2.602*
	(0.552)	(0.359)	(0.454)	(1.445)
Mean dependent (1,000€)	15,740	9,454	30,371	12,983
	B. *Outcome*: log(population stock)			
$log(1-\tau_{b,i,t})$	1.210**	0.059	1.108**	0.923
	(0.556)	(0.357)	(0.443)	(1.087)
Mean dependent (#)	849	1,287	1,528	60
Observations	704,609	235,799	117,912	56,335
Income bracket × municipality FE	Yes	Yes	Yes	Yes
Municipality × year FE	Yes	No	No	No
Income bracket × province × year FE	Yes	Yes	Yes	Yes

Note: This table shows the elasticity of taxable income (top panel) and population stock (bottom panel) in an income bracket with respect to the marginal net-of-local tax rate on income. Columns (1) is composed of 7,918 municipalities × 7 income brackets over the 2001-2015 period. Column (2) reports elasticity estimate for the bottom bracket (or below the exemption cutoff); column (3) for the middle bracket; column (4) for the top bracket. Standard errors in parenthesis, with three-way clustering by municipality × income bracket, income bracket × year and municipality × year.

but social and economic differences are at their minimums and there are no barriers to crossing the border. Border regions usually cover short commuting distances, allowing continuity of family, social, and business ties.[26]

We test whether there is a discontinuous change in the outcome variable as one crosses the regional border by running the following regression:

$$y_i = \beta \cdot 1(i \in LowTaxSide) + \gamma \cdot Distance_i + \delta \cdot Distance_i \cdot 1(i \in LowTaxSide) + u_i, \tag{1.4}$$

where y_i is tax base or population stock in the top income bracket in municipality

[26] Previous studies have used a similar approach to study how state minimum wage rates affect employment (Dube et al. 2010), the effect of anti-union right-to-work laws on the location of manufacturing employment (Holmes 1998), and the effect of income taxes on US millionaires (Young et al. 2016).

i, and $1(i \in LowTaxSide)$ is a dummy variable that it is equal to 1 if municipality i is located in a region where the (top) tax rate is lower than its neighboring region. $Distance_i$ is the algebraic distance (in driving time) of municipality i from the regional border. By explicitly controlling for driving distance, we approximate more closely the transaction costs associated with moving the tax base across locations.[27] We cluster the standard errors by municipality and regional border pairs (Dube et al. 2010).

The key identifying assumption of this approach is that any unobserved factors do not change discontinuously at the border, i.e., the conditional distribution of u_i, given $Distance_i$, is continuous at $Distance_i = 0$:

$$lim_{\epsilon \uparrow 0} E[u_i | 1(i \in LowTaxSide) = \epsilon] = lim_{\epsilon \downarrow 0} E[u_i | 1(i \in LowTaxSide) = \epsilon]. \tag{1.5}$$

The sensitivity of this assumption can be assessed by using information before spatial differences in the regional tax rate began to emerge.[28]

The coefficient of interest, β, computes the impact of local taxes on tax base mobility as the discontinuous change in the tax base (or population stock) as one crosses from $LowTaxSide_i = \epsilon$ to $LowTaxSide_i = -\epsilon$, where ϵ is some small number. Therefore, β yields the "local average treatment effect" (LATE) that it is only relevant for municipalities near the regional border. The main difference with respect to the cross-bracket analysis is that the BD approach allows us to compare top incomes located in neighboring municipalities and subject to a different regional tax rate, instead of relating top incomes with those in the bracket just below to exploit municipal tax rate differ-

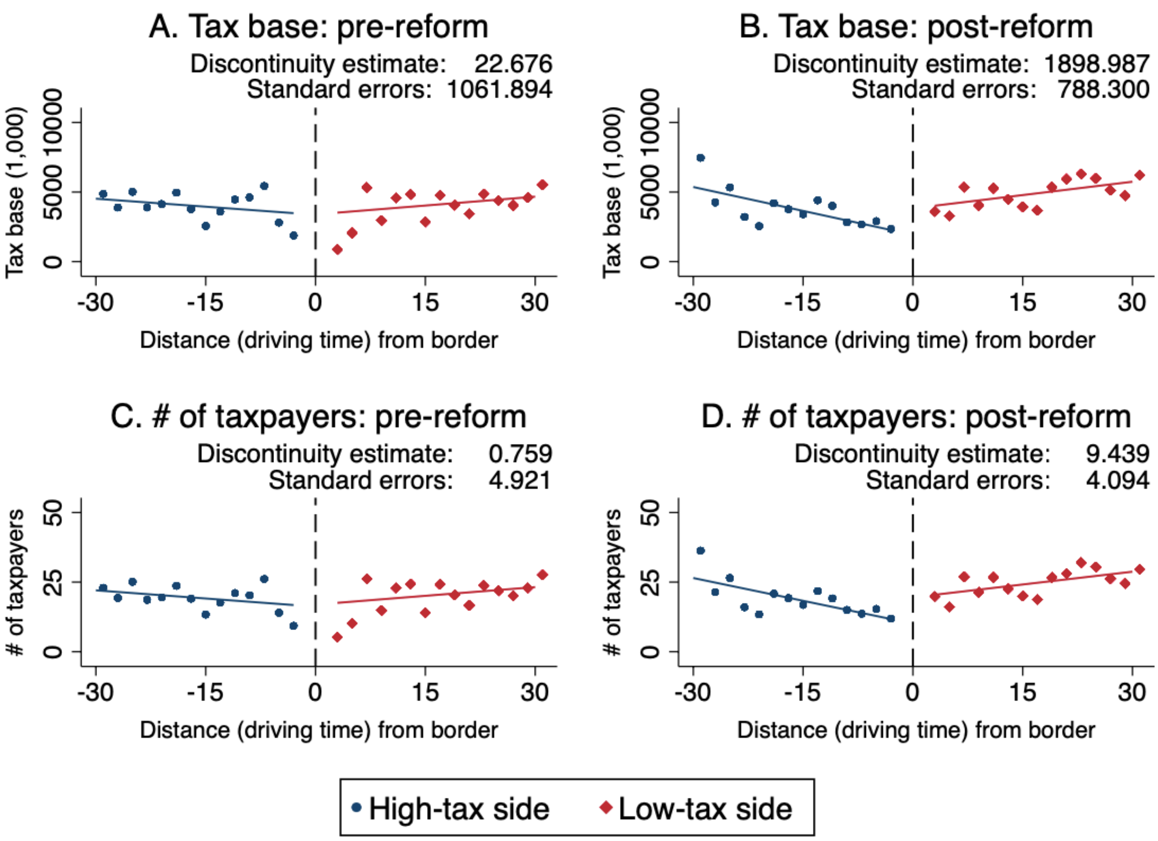
[27] Driving distance would better approximate mobility costs with respect to air distance because it accounts for any geographical barriers, such as mountains or rivers, between municipalities that look adjacent on a map. The empirical analysis restricts the sample to municipalities located no farther than 30 minutes car drive from the border.

[28] Accounting for pre-existing spatial differences in the outcome variable leads our empirical approach to a *difference-in-border discontinuity* design. An additional assumption is that the *municipal* income tax rate does not offset the regional tax rate differential. We validate this assumption in Figure A16, showing a discontinuity estimate of -.009 (.080).

ences. As long as the cost of migrating is a positive function of the distance, we would estimate a relatively larger β compared to that estimated from equation (1.3).

Following the recommendations of Imbens and Lemieux (2008) and Gelman and Imbens (2019), we estimate β by running local linear regressions and restricting the sample to municipalities at a distance ϵ_d from the border, that is $|Distance_i| < \epsilon_d$. The optimal distance is computed using the standard bandwidth selection criterion proposed by Calonico et al. (2014).

Figure 1.6: Relocation of the tax base at regional border



Note: The figure shows the relationship between the differential in the regional (top) tax rate on personal income and mobility. We implement a border discontinuity approach on the sample of municipalities located close to the regional border for two periods: i. the pre-reform (left-hand side graphs), where the tax rate differential was zero or negligible; ii. the post-reform period (right-hand side graphs), where spatial differences began to emerge. The vertical axis in top graphs is the total tax base reported in the top bracket (in 2015 euros); bottom graphs show the stock of taxpayers in the top bracket. The horizontal axis is the algebraic distance (in driving time) of a municipality from the regional border. Scatter points are sample average over intervals of 2-driving time minutes bins. Optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014). We report discontinuity estimate and standard errors with two-way clustering by municipality and regional border pairs standard errors.

Figure 1.6 plots tax base (top graphs) and stock of taxpayers in the top bracket (bottom graphs) in 2-driving minute bins against the municipality's distance from the regional border. Positive distance (red circles) denotes the outcome in municipalities located in the low-tax side of the border. We analyze the existence of any discontinuity at the regional border specifically over the period before the 2011 tax decentralization reform (when tax rate differentials across regions were, if any, limited) and the following years (when spatial tax variation started to emerge).

Left-hand side graphs show that there is no clear evidence of any discontinuity at the border in the pre-decentralization period, thus validating the continuity assumption stated in equation (1.5). This suggests that the regional tax rate change was not endogenous to any pre-existing difference in the distribution of rich taxpayers at the border. By contrast, a clear discontinuity emerged over the post-reform period. In line with previous evidence, we find that the tax base change mirrors the change in the stock of top taxpayers, indicating tax-induced sorting of top incomes. Comparing pre- and post-decentralization period, the figure presents a discontinuity of $9.439 - 0.759 = 8.68$ rich taxpayers. In tax base terms, the discontinuity in the regional tax rate (which amounts to .74 percentage points, on average, see Figure A15) generates a discontinuity of $1,899 - 23 = 1,877$ thousand of euros.[29]

In Table A7, we use the BD estimate to derive tax base and population stock elasticities. On average, we find that the tax base (population stock) reported in the top bracket increased by 9.7 (9.4) percent in the low-tax side of the border when tax rate differentials began to emerge. These estimates translate in a net-of-tax population stock elasticity of 2.6 and a tax base elasticity of 1.4 (although the latter is less precisely estimated).[30] As expected, the BD elasticity estimate is larger than the elasticity computed using tax variation across brackets within a municipality (see Table 1.5), as the former

[29]In Figure A14, we replicate the BD analysis for the bracket just below the top. We find a positive effect, although not statistically significant and of lower magnitude relative to the impact uncovered at the very top bracket.

[30]The elasticity is computed by regressing log tax base (or population stock) on municipality dummies, post-decentralization reform dummy and log marginal net-of-tax rate differential.

is a LATE effect, that is relevant for municipalities close to the regional border where the cost of moving the tax base is relatively lower.

1.4.4 Marche tax reform

Finally, to dig deeper into the identification of the effects of local tax rates, we leverage the quasi-experimental variation provided by a tax cut imposed by the central government following an "illegal" tax increase. In 2002, the region of Marche increased the income tax rate from 0.9 to 4 percentage points. In 2005, a tax commission declared the tax rate increase to be not legal, as it violated the 1.4 percent cap on the regional tax rate established by the national government during that period. This tax cut was sudden and unanticipated, as was the result of a legal dispute advanced by a citizen.[31]

Following this episode, we can compare the evolution of the tax base between Marche and a similar control region, before and after the tax rate cut. We construct a control region by using the synthetic control method algorithm, developed by Abadie et al. (2010), and we plot the result of this exercise in Figure A17. The figure depicts a positive and persistent increase in the tax base following the tax cut imposed by the national government. On average, the tax base was 2.62 percentage points higher in Marche compared to the synthetic control region. This tax cut translates into a long-run tax base elasticity of 0.968, which is fairly similar to the baseline elasticity that we estimate using cross-bracket within-municipality variations.

1.5 Local taxation and transfer of tax residence

We have shown that local taxation affects the location of the tax base. Yet, we do not know exactly towards where taxpayers (and tax bases) are moving. Although the nature of the mobility response did not matter for a municipality-specific tax revenue perspective, it is instead crucial for global welfare. Aggregate welfare losses of local

[31]See *Ordinanza dalla Commissione tributaria provinciale di Ascoli Piceno*, N.270, 18 March 2005.

taxation depend on whether the tax base is moved in another (less-taxed) place that it is still within the country or not, such as in cases when it is shifted into the informal sector, to the internet, or in tax havens. In order to analyze whether local taxation induces relocation of the tax base across places within the country, we now study mobility by using individual-level data from transfers of tax residence and labor force survey.

1.5.1 Location pair analysis

If taxpayers are mobile, differences in local tax rates have the potential to significantly affect the geographical allocation of taxpayers within the country. Standard models of migration (see, e.g., Kennan and Walker 2011) show the decision of moving depends on expected (net-of-tax) income as well as on any difference in amenities and cost of moving. In fact, even in a simple model with low mobility costs, location-specific amenities might be so strong to completely offset any tax incentive.

To identify the effect of taxes on the probability of changing the tax residence net of any fixed mobility cost and amenities, we conduct a *location pair analysis* following Moretti and Wilson (2017). Specifically, we first compute the outmigration odds-ratio relative to each pair of provinces in Italy and for every year. Then, we relate changes in transfers of tax residence across province pairs with changes in the net-of-tax rate differential between the two provinces. Formally, we run regressions as the following:

$$log(P_{o,d,t}/P_{o,o,t}) = \beta \cdot log[(1-\tau_{d,t})/(1-\tau_{o,t})] + \gamma_{o,d} + \delta_t + u_{o,d,t}, \tag{1.6}$$

where $P_{o,d,t}/P_{o,o,t}$ is the population share that moves from an origin province o to a destination province d, $P_{o,d,t}$, relative to the population share in o that does not move, $P_{o,o,t}$. $log[(1-\tau_{d,t})/(1-\tau_{o,t})]$ is the net-of-average tax rate differential within the province pair considered. Our baseline tax rate is the average tax rate defined at the income level equal to the top percentile of the pre-tax national distribution. The rationale for using this tax measure is driven by the evidence presented in Section 1.4, where we showed that most of the response to local taxation comes from the top tail of the income distribution. In alternative specifications, we will also use the top marginal tax rate. The parameter of interest is β, which computes the mobility elasticity with respect to the net-of-tax rate differential across location pairs.[32] $\delta_{o,d}$ are province pair fixed effects, which capture the cost of moving for each province pair and differences in consumption and production amenities within the province pair. Moreover, these fixed effects capture any time-invariant policy of the provinces or secular patterns in migration across provinces. For example, if individuals tend to move from one origin province located in the South of Italy to cities in the North of Italy, because the latter has higher labor demand, then province pair effects will account for these factors as long as they are permanent. In estimating standard errors, we follow Moretti and Wilson (2017) to allow for three-way clustering by origin $\times$ year, destination $\times$ year, and origin-destination pair.[33]

Equation (1.6) is our baseline model, although, in alternative specifications, we will include origin or destination province $\times$ year fixed effects or origin-destination time trends to account for non-tax related factors driving mobility and any time-varying shocks in origin or destination location. We will also control for the differential in local public spending and property tax rates (on both the main residence and second homes) to account for any change that might ameliorate amenities or might make a location more attractive.

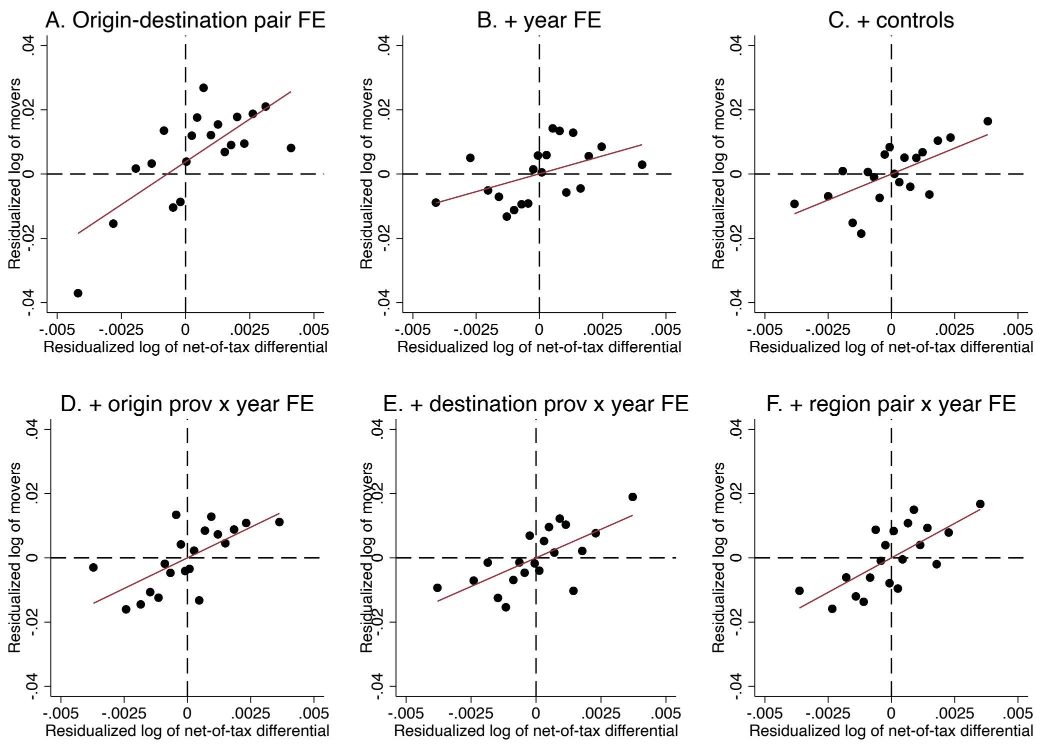
Figure 1.7 presents our main results as a series of bin scatter-plots of the log of out-migration odds-ratio on the log of net-of-tax rate differential. We start by presenting

[32] The resulting average elasticity of the probability of moving with respect to the net-of-tax rate will be equal to: $E[dlog(P_{o,d,t})/dlog(1-\tau_{o,t})] = \beta(1-P)$, where P is the weighted average of $P_{o,d,t}$ observations (where each combination is weighted by the number of individuals in that observation cell). As in our sample $P < 0.001$, the elasticity is very close to β.

[33] Formally, we 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 we assume that $corr(u_{o,d,t}, u_{p,q,t+j}) = 0$ if $p \neq o$ or $q \neq d$. This assumption seems consistent with the data. We test for first-order serial correlation between the residual for a given origin-destination pair in year t and the residual for each other pair in year $t-1$ (even in cases when they share a common origin or destination province.) A regression of the former residual on the latter residual yields a statistically insignificant coefficient of 0.008 (p-value 0.421).

the impact of local taxation on the probability of changing the tax residence after conditioning on province-pair fixed effects. In other words, we depict the slope estimated by regressing the log of the odds-ratio and net-of-tax rate differential demeaned by the province pairs dummies. We then cumulatively add year fixed effects and pair-specific time trends, the differential in spending and property taxes, origin or destination province-year fixed effects, and region pair-year fixed effects. All the graphs depict a positive relation between the probability of changing the tax residence and the net-of-tax rate differential. This suggests that higher destination-origin net-of-tax rate (after-tax income) differentials are associated with higher origin-to-destination transfers of tax residence.

Figure 1.7: Tax-induced transfer of tax residence



Note: The figure compares the log outmigration odds-ratio from an origin location *o* to a destination location *d* (vertical axis) with the differential in the log net-of-average tax rate differential between *d* and *o* (horizontal axis). We depict the residuals obtained by (cumulatively) regressing the two variables on origin-destination location pair fixed effects, year fixed effects and pair-specific time trends, differential in property taxes and public spending, origin province × year fixed effects, destination province × year fixed effects, and region pair × year fixed effects. The figure plots the residuals in 20 equal sized bins and shows the line of best fit. The positive slope suggests that, on average, mobility from *o* to *d* increases as the tax rate in *o* becomes larger than in *d*. The sample includes 4,549,111 transfers of residence moving within 11,449 province pairs over the 2007-2015 period.

Regression results are shown in Table 1.6, which reports the coefficient β estimated from variants of equation (1.6). Each column shows the effect of log of net-of-tax rate differential on the outmigration log odds-ratio and it is equivalent to the fit lines shown in Figure 1.7. All the coefficients are positive and statistically significant at convention levels. Our baseline coefficient, which absorbs year fixed effects, location pairs fixed effects, and pair-specific trends (column 2), is 2.2. Given that the average number of transfers of tax residence within a location pair is 49, our most conservative estimate suggests that a 1 percent increase in the net-of-tax differential would induce 0.98 individuals, on average, to transfer the tax residence within a province pair.

Table 1.6: Local income taxation and transfers of tax residence

	Outcome: log of outmigration odds-ratio					
	(1)	(2)	(3)	(4)	(5)	(6)
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	6.180***	2.230*	3.226***	3.810***	3.560***	4.295***
	(1.039)	(1.173)	(1.229)	(1.290)	(1.233)	(1.308)
Origin-Destination pair FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Group trend	No	Yes	Yes	Yes	Yes	Yes
Spending and pr tax controls	No	No	Yes	Yes	Yes	Yes
Origin province x year FE	No	No	No	Yes	No	No
Destination province x year FE	No	No	No	No	Yes	No
Region pair x year FE	No	No	No	No	No	Yes

Note: This table presents the effect of net-of-average tax rate differential on the probability of transferring the tax residence. Our outcome is the log outmigration odds-ratio: the probability of an individual moving from an origin province to a given destination province relative to the probability of not moving at all. The sample includes 4,549,111 transfers of residence moving within 11,449 province pairs 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.

When comparing the cross-location mobility elasticity with the population *stock* elasticity (previously shown in Table 1.5), it is natural that the former is larger because the base (that is, the number of individuals who moves each year within a province pair) is smaller.[34] The elasticity estimate is in line with the existing evidence on *within-country* migration in countries applying the residence-based tax (see, e.g., Agrawal and Foremny (2019) for Spanish regions; Martinez (2017) for Swiss cantons). As stressed

[34]The average number of individuals moving in an origin province - destination province - year cell is 49, while the average population stock in a municipality - income bracket - year cell is 846.

previously, the fact that these elasticities are larger than *cross-country* migration elasticity estimates (see, e.g., Kleven et al. 2013; Akcigit et al. 2016) reflects the mechanical relationship between mobility elasticity and jurisdiction size (Kanbur and Keen 1993).

Next, we test the robustness of our findings. First, notice that our identification strategy relies on the assumption that, absent any tax rate change, changes in transfers of residence across location pairs would have been constant over time, conditional on our set of fixed effects. To test this possibility, we perform an event study where we compare the log outmigration odds-ratio between observations in which the destination-origin tax differential changes and those in which it does not, relative to the year prior to the tax rate change. Our identifying assumption would be valid if mobility patterns for these two groups were similar over the period before a tax change.

Formally, we estimate the coefficients β_j by regressing the log outmigration odds-ratio on an event indicator $D_{o,d}$, which takes value 1 if the destination-origin differential in the net-of-tax rate *increases* between t and $t+1$; -1 if the tax differential *decreases* between t and $t+1$; and 0 if the tax differential does not change:[35]

$$log(P_{o,d,t}/P_{o,o,t}) = \sum_{j \neq -1} \beta_j \cdot D_{o,d} \cdot 1(t = t_j) + \gamma_{o,d} + \delta_t + u_{o,d,t}. \quad (1.7)$$

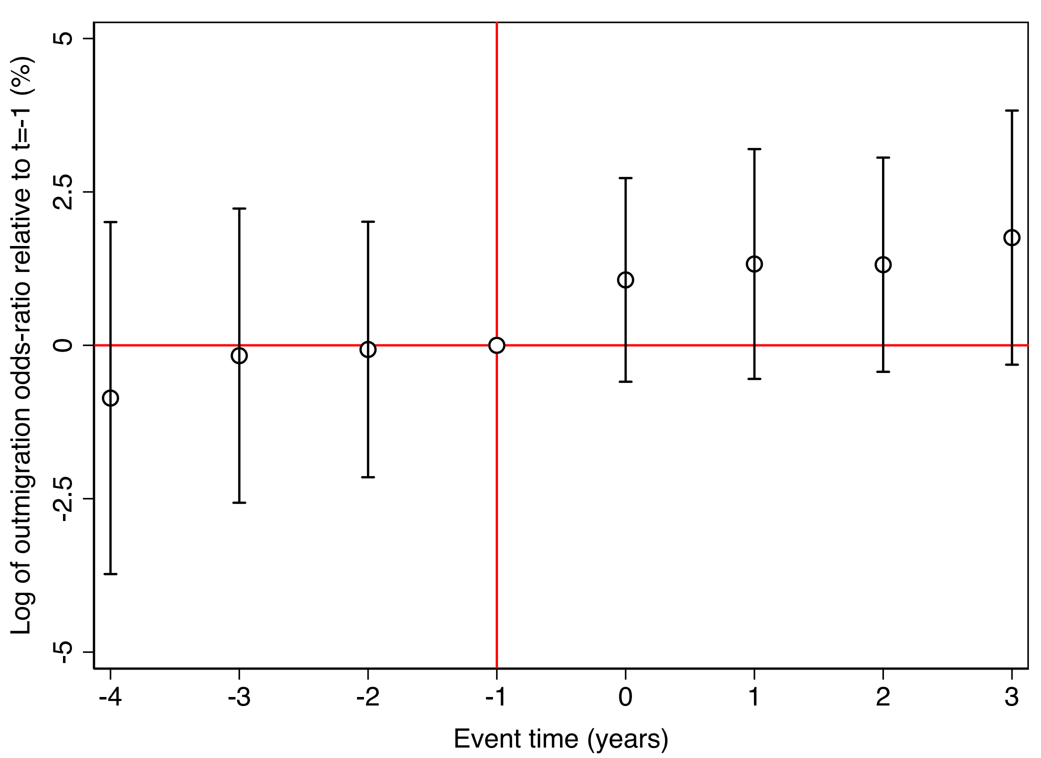
We present the β_j coefficients and 95 percent confidence intervals in Figure 1.8. Each point shows the difference in log outmigration odds-ratio between observations in which the destination-origin tax differential changes and those in which it does not, relative to the year before the tax change.[36] The figure shows that migration patterns were not systematically correlated with tax rate changes: we do not uncover any obvious pre-trend in the period before the tax rate change. Then, in the years following

[35] For parsimony, we impose symmetry by restricting the transfer of residence response to a tax differential increase to be equal, but of opposite sign, to the response to a tax decrease. Results are similar if we re-weight the observations by the magnitude of the tax change. In the case of multiple tax changes within an origin-destination pair, we select the largest in absolute terms.

[36] Since our data cover the 2007-2015 period and the vast majority of tax changes happened after the tax decentralization reform, we are able to plot this difference for up to 4 years before and 3 after a tax rate change.

the change in the tax differential, taxpayers appear more likely to transfer their tax residence from a province to another when taxes in a destination province fall relative to the origin province. In line with earlier municipality-specific evidence, the effect seems to manifest immediately after the tax change. On average, transfers of residence increase by 1.3 percent following a tax change event.[37]

Figure 1.8: Outmigration before and after a tax change event



Note: The figure depicts the difference in outmigration odds-ratio between observations in which the destination-origin tax differential changes and those in which it does not, relative to the year before the tax change. We plot estimated coefficients and the 95 percent confidence intervals. Regressions include province pair fixed effects, year fixed effects, and pair-specific trends. Standard errors are three-way clustered by origin-province × year, destination-province × year and province-pair.

Second, our results so far have been proved robust to allowing for origin or destination-specific shocks, and not systematically correlated with underlying mobility patterns. Since tax rate changes are not random, one may still be concerned about other location pair-specific shocks that might drive tax rate variations. For this end, we exploit the 2011 decentralization reform to instrument the net-of-tax rate differential by a dummy equal to 1 if the tax rate differential changes in 2012, 0 otherwise. Although not per-

[37] We do not find any significant heterogeneous response when we look separately at tax raise vs tax cut or large tax changes vs small tax adjustments.

fect, this instrument is useful since it allows to exploit tax rate changes that followed a policy change rather than any time-varying pair-specific factors. In Table A8, we show that our baseline elasticity estimates are remarkably similar to those estimated by using this instrumental variable approach.

Finally, one caveat of the transfer of residence data is that we do not observe income of movers. In our baseline analysis, we use the average tax rate computed by simulating taxes in all years and provinces for a representative taxpayer in the top percentile of the pre-tax national income distribution. In Table A9, we test the sensitivity of our results to using the top statutory marginal tax rate. Although migration decisions should theoretically depend on the average tax rate, the marginal tax rate might be more salient and easy to retrieve. We find that our estimates remain substantially similar.

1.5.2 Is mobility real or "the mere stroke of a pen?"

Does mobility reflects a change in the (tax) residence or is it a real (i.e., job-related) change? Transfers of tax residence data do not allow us to disentangle a real from a fraudulent move, where a taxpayer changes the tax residence to a second home without physically moving. Distinguishing between a labor market-driven response from a simple change in home address does not matter under a municipality-specific tax revenue perspective, but it is crucial in terms of welfare conclusions and policy recommendations (Chetty 2009).

To shed light on this question, we perform two exercises. First, we focus on workplace mobility - using information from labor force survey data. For this end, we first compute the outmigration odds-ratio relative to workplace changes for each province pair. Then, we regress the odds-ratio on the net-of-tax rate differential as in our baseline specification presented in equation (1.6). Top panel in Table 1.7 displays the coefficient estimate. We estimate a positive effect of net-of-tax rate differentials on the probability of changing the workplace, although we are unable to uncover any statistically significant estimate. Even in the less conservative scenario, our elasticity estimate is always lower than the elasticity estimated from transfers of tax residence data (and although the average number of observations in an origin province-destination province-year cell is always smaller in labor force survey data). This result suggests that the tax-induced mobility response involved small, if any, real responses.

Table 1.7: "Real" mobility response to taxation

	Outcome: log of outmigration odds-ratio					
	(1)	(2)	(3)	(4)	(5)	(6)
	A. *Sample*: Labor force survey					
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	-1.013	1.368	1.358	1.379	1.585	1.696
	(2.693)	(2.560)	(2.542)	(2.577)	(2.511)	(2.565)
	B. *Sample*: Football players					
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	0.267	-0.062	0.129	-0.401	0.703	0.291
	(2.755)	(2.152)	(2.440)	(2.531)	(2.459)	(2.587)
Orig-dest pair FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Group trend	No	Yes	Yes	Yes	Yes	Yes
Sp and pr tax controls	No	No	Yes	Yes	Yes	Yes
Origin province x year FE	No	No	No	Yes	No	No
Destination province x year FE	No	No	No	No	Yes	No
Region pair x year FE	No	No	No	No	No	Yes

Note: This table presents the effect of net-of-average tax rate differential on the probability of changing province (panel A) or region (panel B) of workplace. Our outcome is the outmigration odds-ratio: the probability of an individual moving from an origin location to a given destination location relative to the probability of not moving at all. Standard errors in parentheses, with three-way clustering by origin-location $\times$ year, destination-location $\times$ year and location-pair.

Second, we recognize the fact that our job-related mobility elasticity estimated from labor force survey data provides a lower bound, given that response rates from the rich - which are usually more sensitive to tax changes - are comparatively lower. To account for this issue, we focus on internal mobility within the Italian football labor market following Kleven et al. (2013) and using data on football clubs' players transfers.[38]

[38] Data on football clubs' player transfers are scraped from https://www.transfermarkt.it/. To increase the average number of transfers within each cell, we group transfers at the origin-destination *region* pair.

The reason of using the football market for the study of (real) mobility and taxation is twofold. First, mobility is high in the professional football market, making it a valuable and visible laboratory to study mobility responses. Second, football players cannot live far away from their club as they are required to train almost daily with their teammates. Hence, football players almost always face the (top) local marginal tax rate of the place in which they work. Following these considerations, we believe this exercise likely provides an upper bound on the job-related migration response to taxation. Using the same specification as in equation (1.6), we do not find any significant effect (see panel B in Table 1.7). In light of this evidence, we conclude that job-related reasons explain only a modest part of the migration response.

1.5.3 Separation between residence and workplace

The fact that the migration response mostly involved a transfer of tax residence would suggest a separation of place residence from the workplace. To what extent taxes affects the probability of living close to the workplace? Is the possibility to live far away from the workplace only accessible to specific workers or jobs? We study these questions by leveraging workplace and residence information from labor force survey data.[39]

If taxation matters, the probability of living in the same municipality of the workplace will positively depend on the net-of-tax rate. To test this hypothesis, we estimate how the probability of having the tax residence in the same municipality of the workplace varies with respect to the log of net-of-tax rate observed in the municipality of residence by running the following equation:

[39] A small literature has studied the impact of tax policies on the spatial structures of cities (see, e.g., Wildasin 1985 and Schmidheiny 2006). Agrawal and Hoyt (2018) provide a notable contribution to the literature focusing on the impact of taxes on commuting by exploiting the discontinuous change in the tax system at geographic borders in the US. They show that taxpayers are willing to accept longer commute times in return for lower income tax rates.

$$1(Residence = workplace)_{j,i,t} = \beta \cdot log(1 - \tau_{j,i,t}) + \gamma_{i,t} + \delta_{o(j),i} + \eta_{o(j),p(i),t} + \varphi X_{j,i,t} + u_{j,i,t}, \tag{1.8}$$

where $1(Residence = workplace)_{j,i,t}$ is a dummy equal to 1 if the municipality of residence i coincides with the location of the workplace for individual j in cohort (i.e., observation year) t. $log(1 - \tau_{j,i,t})$ is the log of the net-of-average tax rate, which varies across individuals within a municipality depending on the individual's occupational group. Recalling that we impute the income group (and then the local tax rate) of each taxpayer based on her occupational group, we are thus able to estimate the effect of local tax rates on mobility by exploiting variation across groups of taxpayers within a municipality. Our coefficient of interest, β, measures the percentage change in the probability of (fiscally) living in the same municipality of the workplace when the net-of-tax rate changes by 1 percent.

We account for a wide set of fixed effects and individual-specific characteristics. The inclusion of municipality fixed effects allows us to exploit variation *across consecutive cohorts within a municipality*. In this way, we account for any time-invariant municipality-specific factors that might make commuting more costly (including geographical factors). Municipality $\times$ cohort fixed effects, $\gamma_{i,t}$, and occupational group $\times$ municipality fixed effects, $\delta_{o(j),i}$, allow to account for unobserved heterogeneity across cohorts and occupational groups (and, thus, income groups) within a municipality. We also add occupational group $\times$ province $\times$ cohort fixed effects, $\eta_{o(j),p(i),t}$, to account for any local shocks or policies that directly affect an occupational group. Finally, $X_{j,i,t}$ controls for sex, age, civil status and years of education of each individual.

Coefficient estimates are reported in Table 1.8. We first consider a specification that controls only for municipality fixed effects, cohort fixed effects and individual characteristics. In column (2), we introduce municipality-cohort fixed effects. The estimates are large and highly significant. Column (3) includes occupation-municipality fixed effects and occupation-cohort fixed effects. The point estimate, although statistically significant, significantly drops when we include these fixed effects, suggesting that failing to control for local labor market characteristics as well as any (common) occupation-specific shocks creates an upward bias. Column (4) further controls for occupation-cohort fixed effects interacted with provincial dummies in order to test for potential shocks or policies that affect systematically a labor market in a given area. The coefficient estimate remains similar. Finally, in column (5), we allow the effect of individual characteristics to vary by provinces. In this way, we can flexibly control for any policies (or the business cycle) varying across local labor markets not only over time or across occupations, but also depending on socio-economic and demographic characteristics of the workforce. The results remain qualitatively similar.

Table 1.8: Taxation and probability of living close to workplace

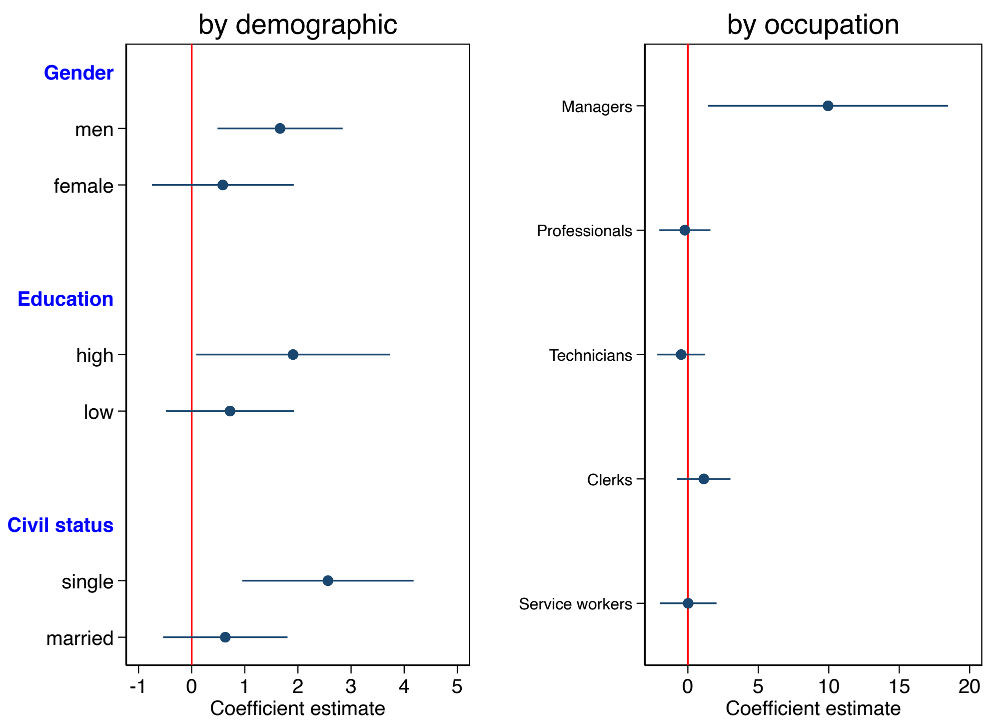
	$1(Residence = workplace)$				
	(1)	(2)	(3)	(4)	(5)
$log(1 - \tau_{j,i,t})$	3.887***	3.977***	1.567***	1.167**	0.962*
	(0.334)	(0.318)	(0.495)	(0.580)	(0.585)
Observations	1,992,686	1,992,686	1,992,686	1,992,686	1,992,686
Municipality FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Municipality × cohort FE	No	Yes	Yes	Yes	Yes
Municipality × occupation FE	No	No	Yes	Yes	Yes
Occupation × cohort FE	No	No	Yes	Yes	Yes
Occupation × cohort × province FE	No	No	No	Yes	Yes
Controls × province FE	No	No	No	No	Yes
Mean outcome (%)	0.582	0.582	0.582	0.582	0.582

Note: This table presents the effect of log net-of-tax rate on the probability of living in the municipality where the workplace is located.

Our most conservative estimate suggests that a 1 percent increase in the net-of-tax rate raises the share of workers living in the same municipality of the workplace by 0.962 percentage points. Given the share of individuals living in a municipality that it is the same of the workplace is 58.32 percent, this estimate translate in an elasticity of moving the residence in the same place of workplace with respect to the net-of-tax rate of 1.788.

It is important to highlight that migration responses are not necessarily the same across segments of the labor markets. For instance, workers employed in high-skill jobs might be more mobile than other workers, because their skills can easily adapt to new locations, workplace and peers. In Figure 1.9, we depict the β coefficient estimate and 95 percent confidence intervals obtained by running equation (1.8) separately by gender, skill level, civil status and occupations.

Figure 1.9: Heterogeneous influence of taxes on location choice



Note: This figure presents coefficient estimate and 95 percent confidence intervals on the effect of net-of-tax rate on the probability of living in the municipality where the workplace is located. We run separate regressions on the following subsamples of individuals: i. men vs women; ii. high vs low educated; iii. single vs married; iv. occupations.

We find the following heterogeneous effects. First, the mobility response is not statistically significant for women. This result implies that women face larger commuting costs, that offset any tax incentives. This is plausible when women are involved in home duties or childcare that significantly raise the cost of not living close to the workplace (see, e.g., Manning 2003).

Second, influence of taxes on location choices is stronger for individuals with the higher education. This may be consistent with the fact that high-educated individuals have fewer job constraints and thus have a larger feasible set of locations to choose from. High-educated individuals might also be more likely to seek the guidance of a tax consultant for advice on low-tax residential location.

Third, we find that married households' residential choices are not affected by local taxes. One interpretation is that married household value public goods and services, such as schools, relatively more.

Looking on heterogeneity by occupation, we identify a very strong effect for managers. This can be explained by their ability to change (or choose) their tax residence because their work location might be flexible or not fixed over the year. This strong responsiveness of chief executive officers to taxation is in line with Goolsbee (2000), which uses compensation data from corporate executives in the US, and with cross-country evidence provided by Piketty et al. (2014). Most of the other occupations presents, instead, smaller or zero effect.

1.6 Implications for tax revenue

Following Saez (2001), we use the tax base elasticity estimate to forecast the revenue effect of local income tax changes.[40] Let us assume that there are N taxpayers in the top bracket facing a constant marginal tax rate τ for incomes above y^*. If a local government increases the top tax rate by a small amount $d\tau$, there are two effects to consider. First, there is a mechanical increase in revenue by $dM = N(y - y^*)d\tau$, where y is the average income in the top bracket. Second, the mobility response would reduce the tax base in the top bracket by $dy = -\beta y d\tau/(1-\tau)$, where $\beta = [(1-\tau)/y]\partial y/\partial(1-\tau)$ is the elasticity of the tax base with respect to the net-of-tax rate. Therefore, the loss in tax revenue coming from mobility responses is $dB = -N\beta y\tau/(1-\tau)d\tau$.

[40] See also Saez (2004) and Diamond and Saez (2011) for applications.

The total change in tax revenue is thus:

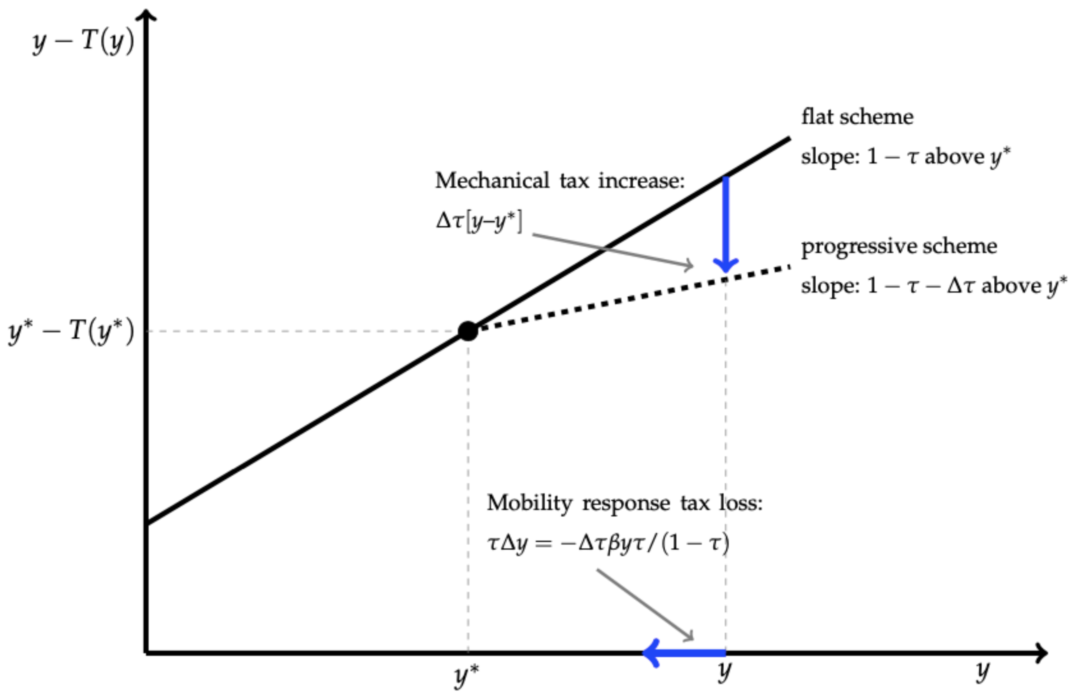
$$dR = dM + dB = \underbrace{N \cdot (y - y^*) \cdot d\tau}_{\text{mechanical effect}} \cdot \underbrace{[1 - \beta \cdot \alpha \cdot \tau/(1-\tau)]}_{\text{mobility effect}}, \tag{1.9}$$

where $\alpha = y/(y - y^*)$ is the Pareto parameter in the top bracket of the income distribution. Equation (1.9) is equal to the marginal deadweight burden created by the increase in the tax rate and shows that the fraction of tax revenue lost through mobility response is an increasing function of the tax rate, the elasticity and the Pareto parameter. Using the tax base elasticity estimate obtained by running equation (1.3), we can forecast the fraction of the projected mechanical revenue lost through mobility responses as $\beta \cdot \alpha \cdot \tau/(1-\tau)$.

Figure 1.10 shows how equation (1.9) is derived. The horizontal axis shows pre-tax income, while the vertical axis shows disposable income. The solid line shows the original flat tax schedule. As depicted, we assume that implementing a progressive tax schedule raised the top tax rate τ by $\Delta\tau$ above the income level y^*. To evaluate this change, we need to consider the expected effects on revenue. Ignoring behavioral responses at first, this reform mechanically raises additional revenue by an amount equal to the change in the tax rate ($\Delta\tau$) multiplied by the average income that is above the cutoff income level ($y - y^*$). Summing up these effects for all the taxpayers N in the bracket above y^*, we get the mechanical effect of implementing a progressive tax schedule. This effect is dampened by the mobility response, which is captured by the elasticity, β, of the tax base with respect to the net-of-tax rate $1 - \tau$.

As an application, consider the adoption of a progressive tax schedule in Lazio region and the city of Rome. We have β = 2.602 (see column 4 in Table 1.5), α = 2.014 (based on tax returns data for Rome), and τ = 4.23 (as in 2015). It turns out that only 23.15 percent of the projected tax revenue is lost through mobility response. In general, when we focus on all the municipalities with a progressive tax schedule, we find that

Figure 1.10: Efficiency costs of adopting a progressive tax scheme



Note: The figure depicts the derivation of the optimal top tax rate $\tau = 1/(1+\alpha\beta)$ by considering a small reform around the optimum which increases the top marginal tax rate τ by $\Delta\tau$ above $y*$. A taxpayer with income y mechanically pays $\Delta\tau[y–y^*]$ extra taxes but, by definition of the elasticity β of income with respect to the net-of-tax rate $1–\tau$, also reduces her income by $\Delta y = \beta y \Delta\tau/(1–\tau)$, leading to a loss in tax revenue equal to $\Delta\tau\beta y\tau/(1–\tau)$. Summing across all top bracket taxpayers and denoting by y^m the average income above y^* and $\alpha = y^m/(y^m–y*)$, we obtain the revenue maximizing tax rate $\tau^* = 1/(1+\alpha\beta)$. This is the optimum tax rate when the government sets zero marginal welfare weights on top income earners.

migration responses dampen projected revenue by 20.13 percent.[41] Holding constant α and τ, for mobility responses to completely offset the mechanical revenue gains, we would need a tax base elasticity of 12.92.

Ignoring the social value of marginal consumption of top incomes, it follows an optimal revenue-maximizing local tax rate for the top bracket equal to:

$$\tau^* = \frac{1}{1+\alpha\cdot\beta} = 12.08\%, \tag{1.10}$$

which is well above any existing local top tax rate (summing both the regional and municipal tax rate). The implication of this finding is that local governments are on the

[41]The mean α and τ (as in 2015) in places where a progressive tax is in force are 2.796 and 2.693.

left-hand side of the Laffer curve: raising the tax rate would increase tax revenue.

The analysis has assumed so far that the local reduction in incomes due to the tax rate increase is equal to the *global* effect on tax revenue. This assumption would be reasonable if the drop in the tax base was due to real (i.e., labor supply) effects or if the tax base was shifted in the informal sector or out of the country. As we showed in Section 1.5, our estimates are consistent with *within-country cross-location* mobility. Therefore, it is plausible that incomes disappearing from the tax base of location A, following a tax increase in A, are shifted towards a location B with a lower tax rate, τ^B.[42] It is then straightforward to show that equation (1.9) becomes:

$$dR = dM\left[1 - \frac{\tau^A - \tau^B}{1 - \tau^A} \cdot \beta \cdot \alpha\right]. \tag{1.11}$$

Going back to our initial example of a taxpayer moving her residence from Rome (location A) to Costa Smeralda (location B) and assuming that all the mobility response took place over the $A - B$ pair, we can use the elasticity estimate β, obtained from the location-pair analysis (see column 2 in Table 1.6), to derive the fraction of revenue lost due to mobility drops. In this case, we find that the fraction of revenue lost due to mobility response drops from 23.15 to 15.4 percent.[43] The resulting revenue-maximizing optimal local tax rate would increase from 12.08% to:

$$\tau^A = \frac{1 + \tau^B \cdot \alpha \cdot \beta}{1 + \alpha \cdot \beta} = 16.15\%. \tag{1.12}$$

Therefore, this simple theoretical analysis shows that, in addition to estimating the elasticity β, it is critical to analyze the nature of the mobility response.

[42] Chetty (2009) develops a generalized formula that allows to account for the fact that some of the costs of evasion and avoidance are transfers to other tax bases or economic agents.

[43] This is computed as $(\tau^A - \tau^B)/(1 - \tau^A) \cdot \beta \cdot \alpha$ = (.0423-.009)/(1-.0423)*2.2*2.014. Overall, the fraction of revenue lost due to mobility responses would further decrease to 7.9 percent when we use the average top tax rate of all the potential destination locations.

1.7 Conclusion

In this paper, we address one of the most long-standing questions in public economics: do individuals move across places in response to tax differences? The answer to this question has crucial implications for policy-makers, encompassing the resulting changes in expected tax revenue and the level and location of local economic activity. Despite a growing literature has focused on tax-induced mobility response of individuals, a recent survey of the literature (Kleven et al., 2020) has emphasized that "direct empirical evidence on the responsiveness of individual locations to taxes has been remarkably scant". In particular, most of the existing literature has focused on specific segments of the population that might be substantially sensitive to taxes, both because they tend to be less tied to specific firms and because their skills are less likely to be location-specific. There are major empirical and data challenges related to both measurement and identification of how individuals respond to taxes, which limit our knowledge on this topic.

We combine administrative data with several identification strategies to study the effect of local income taxation on tax base and individual mobility. Our laboratory is Italy, which offers both temporal and spatial variation in the local income tax rate. The tax rate was substantially similar across places in the early 2000s. Following a series of recent tax decentralization reforms, which have granted more autonomy to local governments in setting taxes, larger dispersion in the local income tax rate has emerged both across places for a given income group and across income groups for a given place. This policy change gives us a unique opportunity to study how taxation affects location choices in a country where income taxes are purely residence-based and several local public goods (e.g., education, public healthcare, voting) are exclusively provided to their residents. In addition, a key advantage of the Italian setting is that the authorities have been collecting micro-level data on tax residence's transfers for the entire population.

We find that local income tax changes affects the location of the tax base and the probability of changing tax residence. Our preferred model shows that a 1 percent increase in the net-of-local tax rate on personal income would raise the tax base by around 1.2 percent. Relating changes in tax residence's transfers with changes in the local income tax rate differential across places, we provide clear evidence that taxpayers actively move their tax residence across places to minimize their tax liability. On average, a 1 percent increase in the net-of-tax rate differential raises tax residence's transfers by around 2.2 percent (from a baseline of around 49 individuals moving within a location pair). Comparing workplace changes vis-à-vis with the place of tax residence, we find that this result involved limited, if any, real (i.e., job-related) mobility responses, but significantly raised the probability of having the tax residence in a municipality different from where the workplace is located. This mobility response is mostly concentrated among chief executive officers, high-skill men, and unmarried individuals.

In the last part of the paper, we study the efficiency costs of local income taxation and discuss the implications of our results for tax revenue and the revenue-maximizing local income (top) tax rate. Although migration is an often-cited justification in proposals to avoid tax progressivity at local level, we find that the benefit of additional revenue from adopting a local progressive tax scheme greatly exceeds the cost of foregone revenue due to relocation. Our results, at least over the medium run, are consistent with Epple and Romer (1991) and Agrawal and Foremny (2019), who show that local redistribution is feasible with migration, but in contrast with the analysis in Feldstein and Wrobel (1998), who show that local redistribution involves large efficiency costs. Building on the elasticity estimate, we find that the optimal income tax-revenue maximizing rate would be larger than any existing ones set by local governments in Italy. A possible caveat is that mobility could rise in the long-run given demographic shifts and technological innovations, which may impose additional constraints on redistributive policy.

Chapter 2

Tax Enforcement, Public Spending and Tax Rates: Evidence from the Ghost Buildings Program

2.1 Introduction

Economists and policy makers often advocate tackling tax evasion as a key policy for the development of fiscal capacity (Besley and Persson 2009; Besley and Persson 2013), to finance worthy government projects (Myles 2000; Lindert 2004), and to set tax instruments more efficiently (Saez et al. 2012; Keen and Slemrod 2017). The interest in fighting tax evasion ramps up routinely during economic downturns, when governments face challenges in raising revenue and financing public spending. Despite technological development has enhanced the ability of governments to retrieve reliable information and monitor tax payments, relatively little is known on the economic returns from anti-tax evasion policies.

Whether curbing tax evasion is a successful strategy for improving the fiscal budget and global welfare is not obvious (Slemrod 2007; Slemrod 2019). First, the effectiveness of anti-tax evasion policies can be limited when tax authorities face constraints in enforcing tax payments. Enforcement may in fact be difficult and costly, in particular in weak institutional environments (Carrillo et al. 2017) and when the decision to punish evaders overlaps with political considerations (Casaburi and Troiano 2016). Second, even if stricter tax enforcement would eventually raise revenue, overall welfare depends on how revenue are spent and whether tax rates complement or substitute stricter enforcement (Keen and Slemrod 2017). Additional revenue might indeed not improve welfare if they are diverted in political rents (see, e.g., Brollo et al. 2013; Caselli and Michaels 2013).

In this paper, we ask the following questions: How successful are anti-tax evasion policies when tax authorities face enforcement constraints? Is curbing tax evasion an effective strategy for financing public goods provision? Does the threat of tax evasion deter the desired degree of tax progressivity? We study these questions in the context of the "Ghost Buildings" program: an anti-tax evasion policy implemented in Italy aimed to identify buildings not registered on the land registry maps and thus missing from the tax base.

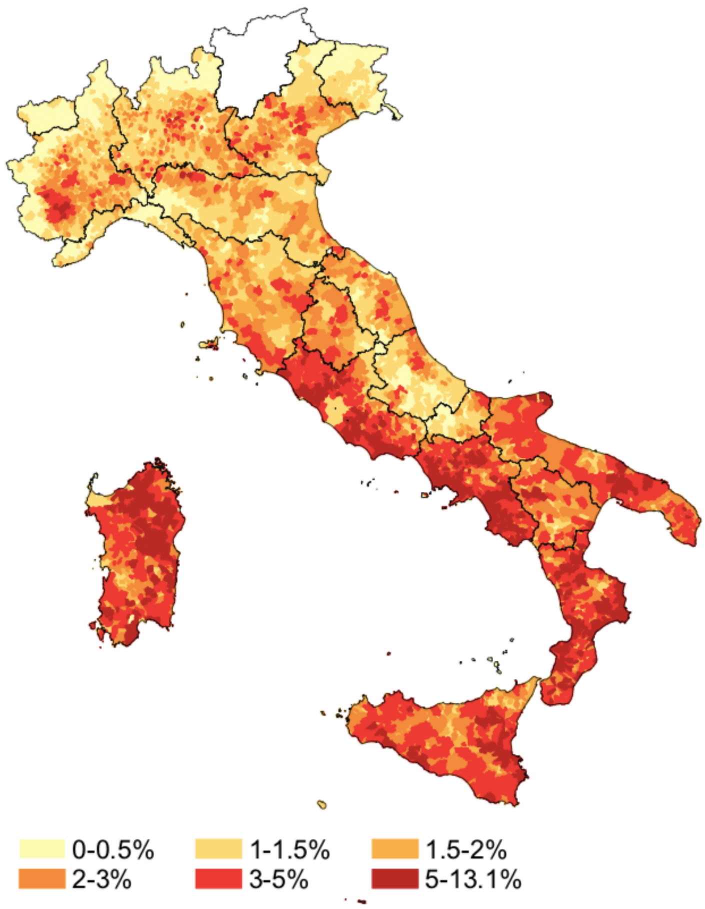
Using an innovative monitoring technique, the program detected more than 2 millions of unregistered buildings. The central government identified the location of each unregistered building and transmitted detailed information to municipalities. Municipal administrators were then required to enforce the registration of previously unregistered buildings and to collect taxes. To incentivize compliance, the central government retained transfers to municipalities based on the *projected* increase in tax revenue that municipalities would have experienced by fully enforcing ghost buildings' registration.

We first focus on the role of municipal administrators in collecting tax revenue. Our empirical approach is twofold. First, we relate cross-municipality variation in the share of detected unregistered buildings and the staggered introduction of the program with municipality-level panel data on tax revenue. The program gave rise to stark geographical heterogeneity in the scope for fighting tax evasion. As shown by Figure 2.1, the share of detected unregistered buildings ranges from 0 to 13 percent of the stock of total buildings across the almost 8,000 Italian municipalities. By using official estimates on the projected increase in tax revenue that municipalities would have experienced in a scenario of perfect compliance, we are able to infer what share of projected revenue was actually raised by municipal administrators.

Second, we exploit a discontinuity in the possibility for accumulating debt and run deficit to study whether the program spurred stricter enforcement in municipalities subject to a balanced budget rule.[1] Intuitively, to offset lower government grants, municipalities eligible for fiscal restraints would have more binding incentives to enforce tax collections compared to municipalities not subject to fiscal restraints. We implement a difference-in-discontinuity design, which exploits variation in tax revenue

[1]Over the period of interest, around 5,600 municipalities (that is, around 71 percent of the sample) have not been subject to a balanced budget rule.

Figure 2.1: Geographical representation of ghost buildings



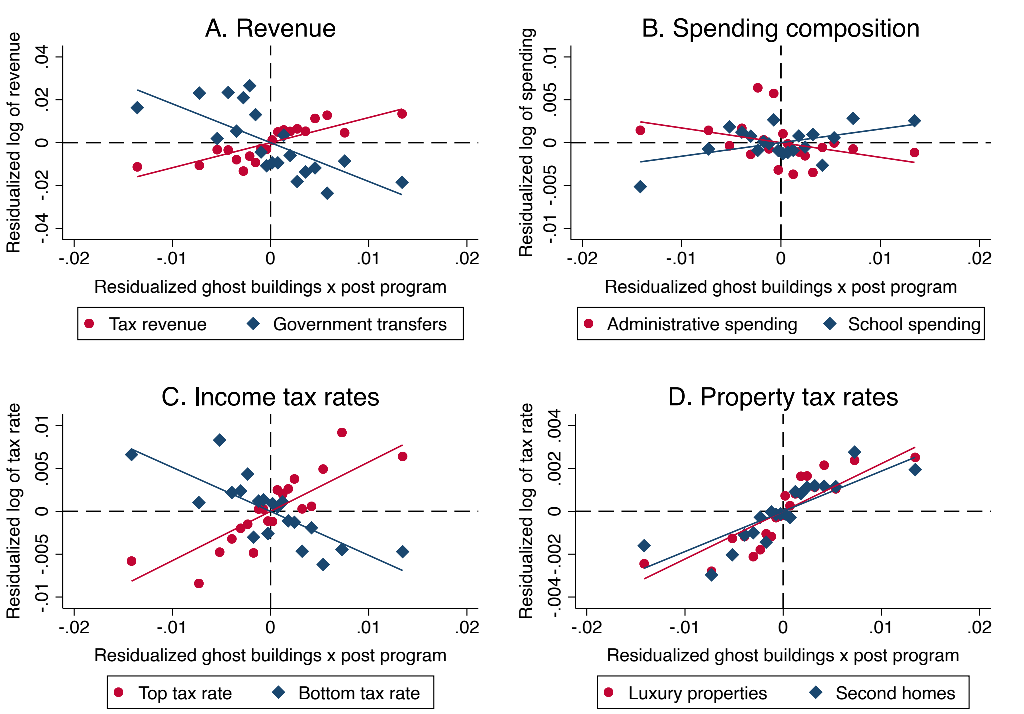
Note: This figure presents the share of detected ghost buildings in each municipality. Yellow (red) area depicts municipalities with a lower (larger) share of ghost buildings. The black line refers to regional boundaries. Break points are the quintile intervals in the share of ghost buildings. Trentino Alto-Adige region (the white area in the North-East) was not part of the program. Data from the Italian Internal Revenue Agency.

across municipalities whose population size is close to the threshold defining eligibility for a balanced budget rule, before and after the introduction of the Ghost Buildings program. This strategy allows to absorb any systematic cross-municipality difference that eligibility for fiscal restraints might have generated, such as the fact that municipalities with a balanced budget rule adopt more rigid fiscal policies (Grembi et al. 2016).[2]

[2]This strategy also allows us to control for any confounding policy changing at the same cutoff, such as the wage paid to mayors, and any national policy implemented over the period of interest.

We find that the program increased tax revenue by around three-fourth of the projected mechanical variation in the tax base generated by ghost buildings' registration. This result is depicted in the top left graph of Figure 2.2, where we present a binned scatter-plot comparing (log of) tax revenue (red dots) or government grants (blue dots) with the interaction between the share of detected unregistered buildings and a dummy for the post-program period, net of municipality and province-year fixed effects. On average, a 1 standard deviation increase in the share of ghost buildings (corresponding to a 1.7 percentage points increase) raised municipal tax revenue by around 2.1 percent of the sample mean (i.e., around 42,000 euros), while government grants fell by around 2.7 percent.

Figure 2.2: Baseline results



Note: This figure shows the effect of the Ghost Buildings program on (log of) the following municipality-level outcomes: i) tax revenue vs government grants (top left graph); ii) share of municipal public spending in administration vs education (top right); iii) top vs bottom statutory marginal tax rate on personal income (bottom left); iv) tax rate on luxury properties vs second homes (bottom right). We depict the residuals obtained by regressing each of these outcome variables (vertical axis) and the interaction between share of ghost buildings and the post-program dummy (horizontal axis) on municipality fixed effects, province-year fixed effects, election year-year fixed effects and time-varying municipality controls. The figure plots the residuals in 20 equal sized bins and shows the line of best fit. The sample includes 7,709 municipalities over the 2001-2015 period.

Missing revenue are consistent with imperfect compliance by municipal administrators in enforcing ghost buildings' registration. Our difference-in-discontinuity estimate clearly shows that revenue are anomalously lower in municipalities not eligible for fiscal restraints. Ceteris paribus, municipalities subject to fiscal restraints collect around 11.1 percent more revenue, compared to those with similar scope for enforcing tax collection but not eligible for fiscal restraints. These results survive to several robustness checks. Among those, we also show that, conditional on scope for stricter enforcement, municipalities were on very similar trend before the introduction of the Ghost Buildings program.

We then focus on the effect of revenue composition on allocation of public spending. That is, we ask whether substituting government grants with tax revenue affected public spending choices. Political agency models of public finance imply that relying more on tax revenue as a source of public spending decreases moral hazard and rent-seeking behavior by politicians when there are asymmetries of information across sources of revenue (see, e.g., Besley 2006; Besley and Smart 2007). This informational asymmetry arises because taxation is in itself informative about government revenue, while information on non-tax revenue must be acquired at a cost. As a result, voters are more likely to keep politicians accountable and demand improvement in public services when spending is mostly financed through taxes rather than by government transfers.[3]

We test this hypothesis by relating the shift in the composition of local revenue engendered by the Ghost Buildings program with rich information on public expenditures from municipal balance sheets since the early 2000s. Italian local governments are the ideal testing ground for the causal evaluation of revenue composition on public

[3]The idea that increasing transparency may stimulate the demand for public goods can be traced back at least to John Stuart Mill's *Principles of Economy* in 1848. In more recent times, a series of seminal papers by James Buchanan and co-authors has argued that citizens systematically underestimate the tax price of public sector activities (see, e.g., Buchanan 1967 and Buchanan and Wagner 1977). This "fiscal illusion" might be exploited by governments to reach a size that is larger than an informed citizen would want.

spending allocation. Municipalities manage around 10 percent of total current public expenditure and are in charge of providing a large array of essential public services, such as public transportation, school facilities, local police, town planning, and manage public utilities. This environment - which is common to many other countries with a certain degree of decentralization - allows us to study whether the way local governments are financed affects the allocation of revenue.

Top right graph of Figure 2.2 shows that municipalities are more likely to invest in schools and less on administration in response to the Ghost Buildings program. On average, a 1 standard deviation increase in the share of ghost buildings raises investments in schools by around 1.6 percent, while administrative spending declines by nearly 2.6 percent. This reallocation of public spending is in line with the idea that politicians attempted to pleasure voters by improving public goods provision, while reducing budget items (including diversified rents) that would undermine their chances to be re-elected. If voters are treated as rational principals, they would be sensitive to the welfare implications of this policy and reward the incumbent in the voting booth. Public spending improvements can thus be an important channel explaining the success of the program in increasing mayors' probability to be re-elected, previously shown by Casaburi and Troiano (2016).[4]

Next, we study whether municipalities change local tax rates in response to a broader and relatively more enforced tax base. The two main local taxes are on personal income (where the marginal tax rates can differ across income brackets) and on property. Since the value of a building enters the tax base for both the property and the income tax (as imputed rent), local policy makers have two basic alternatives: to raise taxes to benefit from the "mechanical gains" of relying on a broader tax base (Keen and Slemrod 2017) or to cut them to compensate non-evaders and perhaps gain political consensus. Moreover, the possibility of choosing different marginal tax rates across income brack-

[4]This finding connects with Weigel (2020), showing that voters demand better public infrastructure in exchange for larger taxes.

ets give them the opportunity to change the distribution of the tax burden across the income distribution.

We provide clear evidence that tax evasion deters the progressivity of the local income tax schedule. As shown in panel c of Figure 2.2, we find a significant increase in the top marginal tax rate and, by contrast, a fall in the bottom marginal tax rate on personal income. This change in income tax progression was more intense in places with higher pre-program level of income inequality; lower in places where the tax base is more likely to flee. For property taxes, we find that the increase in the tax base brought about by tighter enforcement led to larger tax rates on both owner-occupied luxury properties reported as main residence and second homes (see panel d of Figure 2.2).[5] This increase in property and top income tax rates suggests that tax evasion led to suboptimal tax rates. One implication is that base broadening reduces the marginal efficiency cost of taxation, thus making raising taxes less costly (Slemrod and Kopczuk 2002; Kopczuk 2005).

This paper contributes to three strands of the literature. First, it relates to the empirical literature studying the public finance effects of anti-tax evasion policy (see Alm 2012 and Slemrod and Weber 2012 for reviews). In particular, our findings contribute to the literature on tax enforcement (e.g., Andreoni et al. 1998 and Slemrod and Yitzhaki 2002) and the role of digitalization to tackle tax evasion (see Gupta et al. 2017 and Pomeranz and Vila-Belda 2019 for recent reviews).[6] Our results show that incentivizing tax collectors' compliance can be effective to harness third-party information in a context with extensive opportunities for tax evasion. This is an old idea: historically, states from the Roman empire to the French monarchy granted special autonomy to "tax farmers", who were subject to a set of monetary incentives aimed to bolster their

[5] Properties reported as main residence and different from "luxury" properties have been tax exempted over the period analyzed.

[6] A recent literature has focused on the role of other enforcement technologies, such as third-party reporting (Slemrod et al. 2001; Saez 2010; Kleven et al. 2011; Chetty et al. 2013; Naritomi 2019), cross-checking (Carrillo et al. 2017), paper trails (Pomeranz 2015; Kumler et al. 2020) and targeted auditing strategies (Almunia and Lopez-Rodriguez 2018).

loyalty. Yet, as pointed out by Slemrod and Gillitzer (2013), the focus on tax collectors in this literature has been neglected.[7] Our results thus offer evidence on the expected returns of anti-tax evasion policies in countries that have begun to reconsider incentives for tax staff (see, e.g., Kahn et al. 2001 for the case of Brazil and Das-Gupta and Mookherjee 1998 for several developing countries).

Second, we study the linkage between revenue sources and public spending composition. To our knowledge, this is the first study to consider the impact of tax revenues on public spending composition at the local level in the context of a high-tax evasion and developed country.[8] Two recent notable exceptions are the papers by Gadenne (2017) and Martinez (2019), who reach similar results by investigating the effect of an exogenous increase in tax revenue on provision of public goods in Brazil and Colombia. By contrast, there is a rich and growing literature reporting null or small effects on both quantity and quality of public goods when additional resources stem from *non-tax* revenue. Windfall gains from non-tax revenue, such as natural resources, international aid or grants, have been described as a source of "disease" or even as a "curse" that negatively affect relative prices, corruption and rent seeking, thus dissipating any possible benefits.[9] This article emphasizes the notion that what matters is the source of fiscal windfall: public spending financed through taxes has a positive effect on school spending. Since a large fraction of public goods is provided locally, improvements in tax collections have thus the potential to improve residents' welfare.

[7]One notable exception is Khan et al. (2016), which conduct a large-scale field experiment in Pakistan showing that incentivized tax collectors raise 9.4 log points higher revenue than those not facing any incentives.

[8]Italians have been accused by some of making tax evasion a "national sport" (Povoledo 2011). The tax gap is estimated at 34 percent by the European Commission (see *Study to quantify and analyse the VAT Gap in the EU Member States*, 2015) and at 30 percent by the Italian Ministry of Economy and Finance (see Annex 2 to the *Nota di Aggiornamento del documento di economia e finanza*, 2015).

[9]Vicente (2010) and Caselli and Michaels (2013) show that oil discoveries increase corruption and have little or no effect on the quality of public good provision. Borge et al. (2015) provide negative evidence of additional rents from hydro-power production on efficiency of public goods in Norwegian municipalities. Brollo et al. (2013) emphasize the link between non-tax revenues and rent-seeking behavior by politicians focusing on Brazilian municipalities. They show that the electoral punishment of corruption decreases when transfers are larger, since with a larger budget size the incumbent has more room to grab political rents without loosing popularity among voters.

Third, we provide the first empirical evidence on the deterrent effect of tax evasion on tax progressivity. The complementarity between tax progressivity and tax base broadness induced by stricter enforcement relates to the theoretical predictions of Slemrod (1994) and Keen and Slemrod (2017). Moreover, our results connect with a recent empirical finding by Jensen (2019), which studies historical episodes of shifting from self-employment to wage labor in the US. Consistent with our finding, his results show that increasing information about individuals' income for the government substantially raised state-level income taxes. In the Italian context, Bordignon et al. (2017) show that information shocks affect tax choices of local governments, while Rubolino (2020) finds limited mobility responses to higher local income taxes, thus suggesting that local taxes are an efficient way to finance local public goods.

The rest of the paper is organized as follows. Section 2.2 illustrates the institutional framework. Section 2.3 sets out the hypotheses to be tested in the empirical analysis. Section 2.4 presents the data. Section 2.5 describes the empirical strategies. Section 2.6 presents the results. Section 2.7 concludes.

2.2 Background

2.2.1 Local public finance in Italy

The Italian constitution devolves substantial autonomy to the 20 regions and 7,910 municipalities. Municipalities manage around 10 percent of total public expenditure and are responsible for providing a large array of public goods and services to citizens. To finance these services, they set taxes on properties and a surtax on personal income, which raise nearly 15 percent of total revenue. Moreover, both the central and regional governments transfer resources to municipalities to cover ordinary running costs. Transfers are determined by law on the basis of a municipality's population, density, surface, age composition and previous expenses (see Decreto Legislativo n.

504/1992).[10]

The national government keeps municipalities accountable through a set of subnational fiscal rules. Since 1998, the Domestic Stability Pact (*Patto di Stabilità Interno*) has constrained municipalities in terms of fiscal discipline by limiting their possibility to accumulate fiscal deficit (see *Legge Finanziaria* 23 December 1998, no. 448, Article 28). To enforce these rules, the national government reduces interest payments for municipalities that complied and cut transfers for those who did not.[11] The operational target of the rule and the population cutoff defining eligibility have frequently changed from year to year (see Grembi et al. 2016). Importantly for our analysis, over the 2005-2012 period only municipalities with less than 5,000 residents were exempted from the Domestic Stability Pact.[12] This implies that around 5,600 municipalities (i.e., around 71 percent of the sample) have not been eligible for fiscal rules during this period. Starting from 2013, the rule has been also extend to municipalities with population above 1,000 inhabitants, and eventually extended to every municipality.

The municipal government is composed of a mayor and an executive committee. Any change in fiscal policy, such as local tax rates and public goods provision, is proposed by the mayor and the executive committee. An elected municipal council endorses the annual budget proposed by the mayor.

2.2.2 The "Ghost Buildings" program

Italian law requires new buildings to be reported to the land registry within thirty days after their completion (*Regio Decreto Legge 13 Aprile 1939*, N. 652). All buildings require

[10] Horizontal (non earmarked) equalization grants were allocated with a system based on historical expenditure until 2014. Starting from 2015, a reform has gradually introduced an equalization system based on the difference between standard expenditure needs and fiscal capacity (see Marattin et al. 2020 for the fiscal policy reaction of Italian cities to transfer cuts).

[11] Noncompliers are subject to the following penalties: i. a 5 percent cut in the government transfers; ii. a ban on hires; iii. a 30 percent cut on reimbursement and non-absenteeism bonuses for employees of the municipal administration. By contrast, municipalities complying with fiscal rules benefit from interest rate cuts for loans from the central government. Patrizii et al. (2006) provide evidence of a very large compliance rate in meeting the Domestic Stability Pact requirements.

[12] The rationale for this exemption cutoff lies on the presence of economies of scale in managing municipal governments.

a building permit before starting the construction to make them part of the City Plan.[13] Yet, granting a permit does not automatically imply the registration of the building in the land registry, since the two processes are independently administered and data are not cross-checked. This anomaly gave rise to the phenomenon of "ghost buildings": buildings physically existing, but missing from land registry and thus invisible for the tax authorities.[14]

Failing to register a building is tax evasion: buildings enter the tax base for property tax, income tax (as imputed rent), waste disposal tax and require the payment of registration fees. To detect unregistered buildings, the *Agenzia del Territorio* - the government agency managing the land registry - carried out the "Ghost Buildings" program. The program started in 2006 and consisted of two steps. First, land and registry maps were juxtaposed to obtain the Official Building Map. Then, Official Building Maps were overlapped with high resolution (50 cm) aerial photographs of the entire country. A building is identified as "ghost" when it appears in the aerial photographs but not in the Official Building Map (see Appendix B for details). Using this technique, the *Agenzia del Territorio* detected 2.238 million ghost buildings, including commercial, industrial, and residential stand-alone buildings, as well as any unreported extension of previously registered buildings.

The *Agenzia del Territorio* published information on the unregistered properties in the *Gazzetta Ufficiale della Repubblica Italiana* (the official journal of record of the Italian government). The publication process lasted three years (from August 2007 to September 2010): this difference in timing of publication rested on the availability of digitized land registry maps. At the time the program started, only 60 percent of the land registry maps of the Italian territory was available. Then, the *Agenzia del Territorio*

[13] If a building is not part of the City Plan, then the law requires its demolition.

[14] Property tax evasion in Italy dates back centuries. For instance, the creation of dry stone buildings called *trulli*, located in some South-East villages and still inhabited, was a response to the 1466 *Prammatica de Baronibus* edict, which forced tax payments of *lime* houses. Conversano's count resorted to cunning: building *dry stone* houses to avoid taxes and made them easy to disassemble before tax inspectors approached.

digitized the remaining land registry maps proceeding by province (i.e., by simultaneously coding different municipalities in the same province). Therefore, the publication year mostly varies by province: only one-tenth of municipalities has a program start year that differs from the provincial modal publication date (see Figure B2).

Buildings' registration was not automatic and involved the active participation of local administrators. In particular, they were required to: i. disseminate information about the ghost buildings; ii. proceed, with the support of municipal police, to follow-up inspections and imputation of the tax base of properties not voluntarily registered; iii. collect overdue taxes; iv. check whether the building was conform with the City Plan and local zoning restrictions. To incentivize local administrators' compliance in the enforcement process, the national government cut transfers to municipalities based on the *projected* increase in tax revenue that municipalities would experience by fully enforcing ghost buildings' registration.

As shown by Table B1, the total cadastral rent of the ghost buildings was 825.6 million euros. The Italian Internal Revenue Agency (*Agenzia delle Entrate*) calculates that registering the buildings would increase the tax base by approximately 600 million of euros, summing up nearly 444 million of euros for the property tax, about 137 millions of euros for the income tax (including both central and local taxes), and around 7.5 million of euros from registration fees (Agenzia delle Entrate 2012). Agenzia delle Entrate (2012) and Casaburi and Troiano (2016) calculate that the owner of a ghost building will face, on average, an additional tax burden of nearly 528 euros per year, and that 65 percent of the burden is paid in local taxes. If confirmed, this projected increase in the tax base would raise municipal tax revenues by 5 percent of the pre-program sample mean.[15] In other terms, the mechanical variation in the tax base implies that a 1 standard deviation increase in ghost buildings would raise local tax revenue by 3

[15]Mean (unweighted) municipal tax revenue was of around 2 million euros per year. The average (unweighted) number of detected ghost buildings in a municipality is 287. The predicted average increase in municipal tax revenue is 287(0.65*528) = 98,498 euros per year.

percent of the sample mean. For instance, the program would increase tax revenues in the city of Rome - where 3,990 ghost buildings were detected - by around 1.4 million of euros. The town with the largest share of detected ghost-buildings - Isola di Capo Rizzuto (Calabria) - would experience an increase in tax revenues of two-thirds of its average pre-program level of tax revenues (around 1 million euros).[16]

2.3 Empirical hypotheses

This section formalizes the hypotheses tested in the empirical analysis. Our goal is to the study the effect of the Ghost Buildings program on three municipality-level outcomes: i. tax revenue; ii. public spending; iii. tax rates.

The program crucially leaned on the active participation of local administrators to enforce registration of ghost buildings. Anti-tax evasion policies are canonical examples of policies that are asymmetric in their concentration of costs and benefits (Tullock 1959; Olson 1965). If politicians are not willing to curb tax evasion, then registration rates and tax revenue would be lower than in the case all the buildings were registered. Yet, since failing to enforce ghost buildings' registration would reduce government grants and eventually increase budget deficit, eligibility for fiscal rules creates a discontinuity in the cost of not enforcing ghost buildings' registration. Therefore, we can estimate the compliance gap (i.e., the difference between the amount of projected and collected revenue) by comparing municipalities eligible for fiscal rules with those not eligible.

Hypothesis 1: The program increases tax revenue. Conditional on scope for stricter enforcement, tax collections are relatively larger in municipalities eligible for fiscal restraints.

[16] Some discrepancy between projected and actual revenue might be due to measurement errors in estimating the mechanical variation in the tax base. These errors might arise, e.g., when policy makers imputed the rental value of buildings not voluntarily registered. Yet, ghost buildings' owners had the right to appeal for a re-evaluation if not convinced by the imputed rental value of the building.

Second, the change in the composition of local revenue generated by the Ghost Buildings program allows us to explore whether how (local) governments are financed affects the allocation of revenue. The concept that the source of revenue might affect public spending's decision is developed in political agency models of public finance (Besley 2006; Besley and Smart 2007). In these models, the incumbent faces a trade-off between spending on public goods, which please voters, versus grabbing "rents", which are personal perks that are unobservable to voters. Incumbents that displease voters by extracting excessive rents will not be re-elected. In principle, voters should be able to observe the size of government and make inference about the incumbent based on observed policy outcomes. Yet, if there are informational asymmetries across sources of revenue, voters would have distorted information on the actual resources available to the incumbent.

Switching from a less to a more transparent source of revenue has thus the potential to reduce incumbent's moral hazard. Intuitively, citizens paying more taxes would be more likely to monitor the incumbent in order to check how their money are spent. By contrast, any increase in non-tax revenue does not have any (direct) cost on citizens and, thus, would not stimulate accountability. In our context, the program would enhance accountability by substituting government grants with tax revenue.[17]

Hypothesis 2: An increase (decrease) in tax revenue (government transfers) leads to more provision of public infrastructure and lower political rents.

Third, we study the effect of relying on a broader and relatively more enforced tax base on local statutory tax rates on property and income. How tax rates should vary when tax evasion is curbed is not a-priori obvious.[18] On the one hand, the increase

[17] The concept that taxation improves governance can be already found in the literature focusing on the development of modern Europe (North and Weingast 1989). In public economics, it is related to the idea of "fiscal illusion" (Puviani 1903) and at the core of the "second generation" approach to fiscal federalism (Oates 2005; Weingast 2009).

[18] For instance, Sandmo (2012) argued that "the joint analysis of tax design and compliance policy is too complex to result in a simple and intuitive characterization."

in the tax base brought about by tighter enforcement increases the mechanical benefit (i.e., revenue gain) of increasing the tax rate. Using a simple framework to analyze efficient level of tax administration, Keen and Slemrod (2017) show that the relation between tax base and tax rate is towards strategic complementarity. On the other hand, a potential ambiguity is introduced by the impact that tighter enforcement might have on the responsiveness of other forms of tax evasion to the tax increase (or if politicians want to cut taxes to compensate non-evaders and gain political consensus). For instance, taxpayers may respond to stricter enforcement for property income by making offsetting adjustments on less enforced tax bases, thereby reducing the total effect on tax revenue (Saez et al. 2012; Slemrod and Gillitzer 2013; Carrillo et al. 2017). In this case, the relation between tax base and tax rate would be of strategic substitutability.

The theory of optimal taxation defines optimal taxes by means of the Ramsey's inverse elasticity rule (see Cremer and Gahvari (1993) for an analysis on the validity of these rules in the presence of tax evasion). In our context, local administrators would choose property and income statutory tax rates taking as given the (time-varying) probability of detection, which affects the "expected" tax rates, that are crucial for evaluating the welfare effects of a tax rate change.[19] In the special case of independent demands, Cremer and Gahvari (1993) show that tax evasion leads to a modification of the Ramsey rule: optimal expected tax rates are lower in markets where evasion is more spread. This suggests that anti-tax evasion policies and optimal taxation should be consider in conjunction with each other. It is thus plausible to assume that the program would raise the statutory tax rates, given that stricter tax enforcement (and the possibility to rely on a broader tax base) reduces the marginal efficiency cost of taxation, thus making raising taxes less costly (Slemrod and Kopczuk 2002; Kopczuk 2005).

Hypothesis 3: Statutory tax rates and enforcement are strategic complements.

[19]The "expected" tax rate is a weighted average of the regular (statutory) tax rate and the penalty rate, where weights are defined by the probability of detection and non-detection.

2.4 Data

This section presents the data used in this paper. Table 2.1 shows the summary statistics of the main variables employed.

Table 2.1: Summary statistics

	Obs (1)	Mean (2)	Std. Dev. (3)	Min (4)	Max (5)
Ghost buildings (%)	115,635	0.018	0.017	0	0.131
Post (0/1)	115,635	0.544	0.498	0	1
Ghost buildings × Post	115,635	0.010	0.015	0	0.131
Tax revenue (€1,000)	115,635	1,972	18,375	0	1,751,331
Government grants (€1,000)	115,635	1,535	15,659	0	1,838,914
Administrative spending per-capita	115,635	358.58	893.04	0	42,923
School spending per-capita	115,635	11.998	51.189	0	3,845
Income tax - top rate (%)	115,635	0.352	0.264	0	0.900
Income tax - bottom rate (%)	115,635	0.278	0.254	0	0.800
Tax rate on luxury properties (%)	115,635	0.495	0.090	0	0.860
Tax rate on second homes (%)	115,635	0.677	0.152	0.300	1.110
Tax base (1,000€)	115,635	94,210	71,851	269	4.94e+07

Note: The sample covers 7,709 municipalities over the 2001-2015 period. Variables are expressed in 2015 euros.

2.4.1 Ghost Buildings program

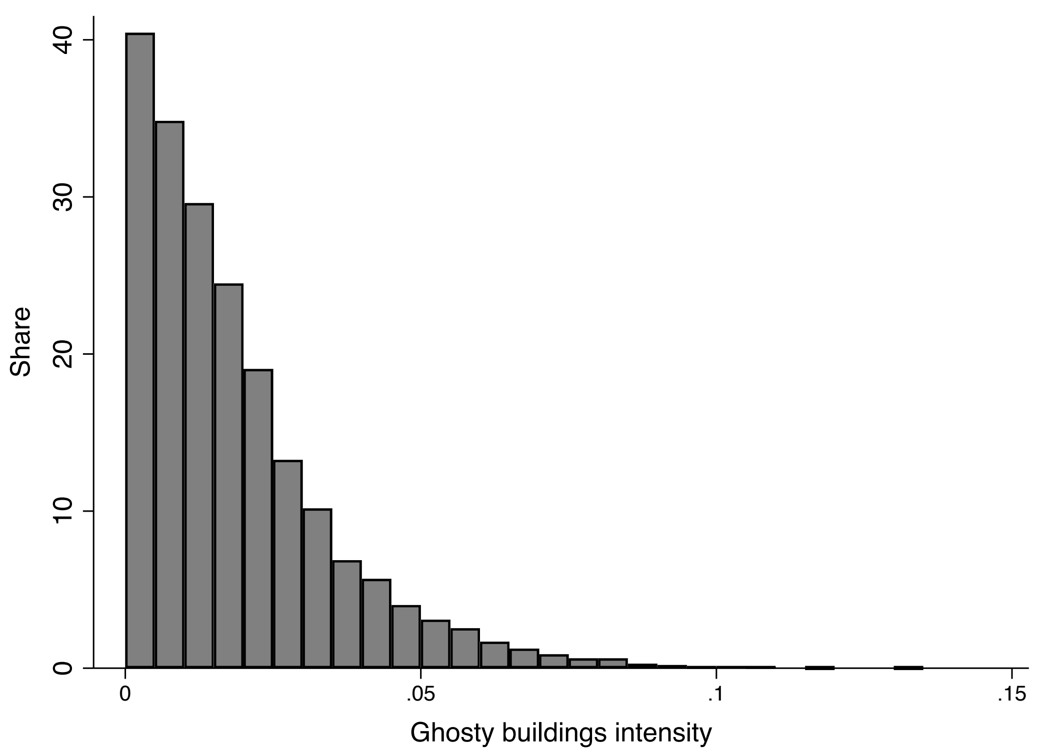
The Italian Internal Revenue Agency provides data on the Ghost Buildings program. We access to municipality-level information on the total number of parcels, the number of parcels containing ghost buildings, and the program start date.[20] This information allows us to define *ghost buildings intensity* as the ratio between the number of parcels containing ghost buildings and the total number of parcels, net of buildings that did not required to be reported on land registry. We construct this indicator for the population of 7,709 out of the 7,910 municipalities, since the program did not cover Trentino Alto-Adige region, where land registry maps are autonomously administered.

Figure 2.3 displays the density of ghost buildings intensity. The indicator varies from 0 to 13 percent and it has a mean (median) value of 1.8 (1.4). Geographically,

[20]Parcels are defined at the municipality-level. According to the law (Reggio Decreto no. 1952, 1931), they can vary in size, but they capture portions of lands (or buildings) that belong to the same owner, and their cadastral definition and quality is the same.

tax evasion is significantly higher in the South and, although to a lower extent, in the Center (see previous Figure 2.1). Figure B3 shows that ghost buildings intensity is highly correlated with other regional level proxies for tax evasion derived from tax data. Casaburi and Troiano (2016) find that several geographical and socioeconomic characteristics are strongly associated with the share of detected ghost buildings in a municipality. Moreover, they find that initial registration rates are correlated with mayor characteristics, such as gender, age, birthplace, and education.

Figure 2.3: Distribution of ghost buildings intensity



Note: The histograms shows the distribution of ghost buildings intensity, defined as the ratio between the number of land registry parcels containing ghost buildings and the total number of parcels in each municipality.

Differently from Casaburi and Troiano (2016), which use data on registered ghost buildings up to April 30, 2011, our dataset is based on the final update of the program. Following Casaburi and Troiano (2016), our empirical analysis focuses on the ex ante program scope to measure the impact of enforcement rather than on the final registration rates. The rationale is that the program scope predicts the exogenous increase in enforcement induced by the program, while the actual registration rate could depend on imperfect compliance of local administrators in ensuring buildings' registration. Moreover, registration rates are sensitive to measurement issues, given that we miss information on the number of detected unregistered buildings that were not conform with local zoning or environmental restrictions and were *actually* demolished.[21]

2.4.2 Municipal balance sheets

We collect panel data on tax revenue and public expenditures from the balance sheets of Italian municipalities. Municipal balance sheets have been introduced with the aim to better monitor local public spending in the frame of the Domestic Stability Pact. They are approved by the town council by the 30th of April of the following year. The current accounting models are authorized by the Italian Ministry of Interior and are harmonized both across municipalities and over time.

The dataset covers the 2001-2015 period and includes detailed yearly information on the composition of revenue and its allocation across several budget items. As shown by Figure B4, revenue are mostly composed of own taxes (39 percent) and government grants (34 percent), while a relatively lower part arises from assets disposal, capital transfers, loans and mortgages and transfers from the European Union. On average, municipalities raise around 2 million of 2015 euros from local taxes and receive grants for around 1.5 million. In the rest of the paper, we refer to tax revenue as the sum of revenue collected from municipal taxes, while revenue from government grants include transfers from both the central and regional governments.

The balance sheets provide detailed information on how municipalities invest their

[21] Anecdotal evidence confirms the absence of a hard-line stance by local administrators in demolishing abusive unregistered buildings (see, e.g., Il Sole 24 Ore, 5 November 2018). Several detected ghost buildings are located in environmentally constrained area (e.g., when the distance to seaside or a river is lower than 150 meters). In these situations, municipal administrators had to initiate a urban infringement procedure, which is a legal requirement to obtain a demolition permit. In practice, most demolitions were postponed and eventually condoned. For instance, an unregistered villa in Casteldaccia (Sicily) was overwhelmed by a flood of the Milizia river, which killed 8 people in 2018. Local administrators had already obtained the permit to demolish the villa, but the order had remained on paper since 2008.

revenue among several budget items. The right-hand side of Figure B4 depicts the composition of capital spending: almost half of total spending is allocated to administration (e.g., financial administration, economic planning, performance audit), waste disposal and environment protection. Public transports, urban roads maintenance and social services account for around one-third, while around 8 percent of spending is assigned to finance school facilities.[22]

Education is the main measure of public expenditure outcome that we consider. Municipalities are in charge of financing nursery, primary, and early secondary schools. In particular, they finance school facilities, lunches, transportation and staff. Municipalities receive government grants earmarked for *current* expenditures on most basic budget items. By contrast, physical school infrastructures have been particularly underfunded until a recent large-scale reform (see law 107/2015), which is after the period covered in our empirical analysis. Therefore, we focus on physical school infrastructures as the type of input that is the most likely to be affected by changes in non-earmarked revenues, but we also discuss and present the effect on other budget items. There is ample anecdotal evidence that the supply of municipal education infrastructure has not kept up with the increase in the demand over the last decade in Italy.[23] Furthermore, there is causal evidence that increases in municipal school spending raise standardized test scores in Italian primary schools (Pavese and Rubolino 2020).

One potential way through which municipalities could improve public goods provision is by reallocation of expenditures. Municipalities have room for adjustment because about one-third of expenditures are not rigid. Furthermore, Bandiera et al. (2009) show how similar municipalities can pay very differently for similar goods, which the authors interpret as evidence of passive waste. Moreover, a better management of existing resources might also be associated with lower political rents (see, e.g., Persson

[22]To gain precision, we impute missing data with provincial average values. Missing values account for 7 percent of total observations. All the results presented in the empirical strategy are not sensitive to these interpolations.

[23]See, e.g., OECD, "Education at a Glance 2016".

and Tabellini 2000). We attempt to capture these possibilities by studying the effect of the program on spending per-capita in administration, which is our proxy for inefficient spending (including political rents).

2.4.3 Local tax rates

To finance spending, municipalities set two main taxes. First, they charge a surtax on personal income on top of tax rates set by the national and regional governments. The income tax rates can differ across income brackets and vary between 0 and 0.9 percent. Moreover, since 2007 municipalities can set an exemption cutoff for the income tax. In our analysis, we focus on two measures: i. the bottom statutory marginal tax rate; ii. the top statutory marginal tax rate, which is faced by around the top 5 percent of the income distribution. These series have been collected by Rubolino (2020) from administrative sources since 2001. Importantly, the definition of the tax base is constant across municipalities, given that it is defined by the central government and any national reform would be absorbed as a common effect. Likewise, the definition of the income intervals to which the tax rates applied has been constant both over time and across municipalities.

Second, municipalities raise revenue from a local property tax. This tax was introduced in 1993 (formerly called *Imposta Comunale sugli Immobili*) in an attempt to raise municipalities' administrative power and accountability. Municipalities can choose two distinct property tax rates: i. the tax rate on "luxury" homes (e.g., stately homes, villas, maisonettes, cottages) reported as main residence[24]; ii. the tax rate for second homes and commercial buildings. The tax base for the property tax depends on a function of the cadastral value of the residence (net of deductions), determined by the

[24]The 2008 reform abolished the property tax on owner-occupied residence, with the exception of cadastral units A1, A8 and A9 (referred as luxury homes in the text), that were taxed even if reported as main residence. These luxury homes are real estate units belonging to buildings located in prestigious area presenting constructive, technological and fittings features than are of higher level than that of residential buildings. In 2012, the property tax was substantially reformed and renamed "*Imposta Municipale Propria*" (see Messina and Savegnano 2014 for a review on local property taxation in Italy).

national government. We collect information on property tax rates from the Italian Institute of Finance and Local Economy (*Fondazione IFEL*). Over the period of interest, the tax rate on luxury properties ranges from 0 to 0.86 percent, while the tax rate on second homes between 0.3 and 1.11 percent.

2.4.4 Other data

In addition to the core data, we collect municipality-level time-varying information on demographical and socio-economic data (i.e., population, share of 65+, share of 15-, share of foreign, unemployment rate) from the Italian National Statistical Office, detailed demographic and socio-economic information on local politicians from the Italian Department of the Interior (*Ministero degli Interni*), taxable income from the Ministry of Economy and Finance, elections and turnout data from *Ministero degli Interni* and survey information on tolerance toward tax cheaters from *European Values Survey*.

2.5 Empirical strategy

The aim of the paper is to study the effects of the Ghost Buildings program on local public finance outcomes. To this end, we build on two distinct empirical strategies. First, we exploit cross-municipality variation in ghost buildings intensity and the staggered introduction of the program to implement a difference-in-differences approach. Second, we exploit the discontinuous change in eligibility for fiscal restraints and the introduction of the program to implement a difference-in-discontinuity approach.

2.5.1 Difference-in-differences approach

We examine how ghost buildings intensity is associated with municipality-level public finance outcomes by implementing a difference-in-differences event study. Specifically, we run specifications as the following:

$$y_{i,t} = \sum_{j \neq 0} \beta_j \cdot GB_i \cdot 1(t = t_j) + \gamma_i + \delta_t + u_{i,t}, \tag{2.1}$$

where the dependent variable, $y_{i,t}$, is log of tax revenue, public spending or tax rate in municipality i at year t. GB_i denotes ghost buildings intensity. The interaction term, $GB_i \cdot 1(t = t_j)$, omits the program inception year (denoted by $j = 0$), so that the difference-in-differences coefficient β_j can be interpreted as the effect in year t relative to the program inception year. The inclusion of municipality fixed effects, γ_i, and year fixed effects, δ_t, allows to control for municipality-specific time-invariant unobserved characteristics and common shocks. In some specifications, we also include time-varying municipality characteristics, province-year fixed effects and election year-year fixed effects. These fixed effects allow to capture any change in regional or provincial policy, local business cycle, and the fact that electoral incentives for pursuing policies aiming to capture voters, such as lower taxes or higher public goods provision, are stronger when legislative elections approach (and voters' attention increases). Throughout the analysis, we cluster the standard errors at the municipality level.[25]

The identifying assumption requires that the timing of program inception and its interaction with ghost buildings intensity are quasi-random. The quasi-randomness is equivalent to the "parallel trends assumption" of standard difference-in-differences specifications. Therefore, we will validate this assumption by showing that $\beta_j = 0$ $\forall j < 0$.

Since the program starting year is staggered in time, municipalities might have influence over when the program would start. We then might be worried that the original date of the program could be a response to local economic shocks. If this is the

[25]To account for the possibility of spatial correlation in the error term, we follow the suggestions by Angrist and Pischke (2009) to "pass the buck up one level" and cluster standard errors on a higher level of aggregation, which in our case is the province or the region. As we will show in Section 2.6, our baseline results remain valid.

case, then β would capture a selection effect. However, as discussed earlier, the timing of the program rested on the availability of digital land registry and was highly clustered at the provincial level. Indeed, nearly one-tenth of the post-program dummies has value different from the one it would have had based on the modal date of publication in the province (see Figure B2). To deal with this discrepancy, we follow Casaburi and Troiano (2016) to implement an instrumental variable approach. We first compute the modal program starting year for each province, then we instrument the actual municipality-specific program inception year dummy with this binary variable defined at the provincial level.

To assess the magnitude of the effect and relate actual with projected revenue, we run the following empirical specification:

$$y_{i,t} = \alpha Post_{i,t} + \beta(GB_i \cdot Post_{i,t}) + \gamma_i + \delta_t + u_{i,t}, \tag{2.2}$$

where $Post_{i,t}$ is a dummy for the post-program period. This reduced form specification is equivalent to a difference-in-differences strategy where we compare the change in the outcome variable with ghost buildings intensity, before and after the staggered introduction of the program.

2.5.2 Difference-in-discontinuity approach

To study whether budget constraints stimulate local administrators to enforce ghost buildings' registration, we exploit the discontinuous change in eligibility for the Domestic Stability Pact, which limits the possibility of accumulating debt and run deficits. We focus on the period 2005-2012, when only municipalities above 5,000 inhabitants were subject to fiscal rules. Since we expect fiscal rules to have an independent effect on local public finance outcomes (Grembi et al. 2016), we implement a "difference-in-discontinuity" design, which allows to difference out any systematic difference across eligible and not eligible municipalities by exploiting the staggered introduction of the program.

Defining p_i as the population size in municipality i (as computed in national Census), p_c as the population cutoff above which fiscal rules applied, T_i as a dummy equal to 1 if $p_i > p_c$, and $Post_{i,t}$ as a dummy equal to 1 for each year t after the inception of the Ghost Buildings program, the difference-in-discontinuity estimator can be implemented by estimating the boundary points of four regressions functions of $y_{i,t}$ on p_c: on both sides of p_c and for both $Post_{i,t}$ = 0 and = 1.

We follow Imbens and Lemieux (2008) and Gelman and Imbens (2019) to run local linear regressions: this method consists in fitting linear regression functions to the observations distributed within a distance h on either side of p_c. We restrict the sample to municipalities in the interval $p_i \in [p_c - h, p_c + h]$[26] to run the following regression:

$$\begin{aligned} log(y_{i,t}) = \alpha P_i^* + T_i(\beta_0 + \beta_1 P_{it}^*) + Post_{i,t}[\gamma_0 + \gamma_1 P_i^* \\ + T_i(\delta_0 + \delta_1 P_i^*)] + u_{i,t}, \end{aligned} \tag{2.3}$$

where $P_i^* = p_i - p_c$ is the normalized population size. The coefficient of interest is δ_0, which identifies the local average effect of eligibility for fiscal restraints on tax collections, as the treatment is given by the interaction between the post-program dummy variable and eligibility for the Domestic Stability Pact, $Post_{i,t} \times T_i$. A positive δ_0 would suggest that local administrators in municipalities subject to fiscal rules collect larger revenue as a result of the Ghost Buildings program, compared to municipalities not eligible for fiscal rules but with similar scope for raising revenue.

This empirical approach yields the causal effect of eligibility for fiscal restraints on local administrators' compliance under plausible assumptions. First, any difference in tax revenue would have not systematically changed at p_c in the absence of the Ghost

[26] We first restrict the sample to the interval $\pm$ 2,000 since other policies change discontinuously at the 3,000 population threshold. The optimal bandwidth, h, is then computed using the algorithm developed by Calonico et al. (2014).

Buildings program. In other terms, we assume (local) parallel trends across municipalities just below and above p_c. To validate this assumption, we will estimate the δ_0 coefficient year-by-year. We will show that municipalities were on similar trends before the introduction of the Ghost Buildings program, thus strongly supporting the validity of this design.

Second, we assume the absence of any manipulation of the running variable. We test for the continuity of the density at p_c (McCrary 2008). Figure B5 shows no evidence of systematic manipulation; the discontinuity estimate is -.061 (.120).

Third, δ_0 provides an unbiased estimate of the effect of ghost buildings' registration on the outcome of interest under the assumption that there is no interaction between the Ghost Buildings program and other policies or factors that changed discontinuously at the 5,000 cutoff. This assumption might be violated if local administrators just below and just above p_c, who are paid differently, are systematically different in their ability to enforce registration of ghost buildings. We directly test this assumption by performing two exercises. First, in Figure B6, we show that the distribution of mayor and components of town council's ability (measured by a dummy for having a college degree) does not significantly change at p_c (discontinuity estimates are .017 (.057) and .019 (.019), respectively). Second, we exploit the discontinuous change in mayors' salary at other population cutoffs (i.e., at 10,000 and 15,000) to analyze whether there is any change in collected revenue, holding constant the eligibility for fiscal rules. Figure B7 shows that tax collections do not present any significant jump in these cases, thus indirectly suggesting that the contemporaneous change in mayor's salary would not confound the effect that we assign to eligibility for fiscal restraints.

Similarly, we require municipalities around p_c to face the same scope in raising tax revenue. This assumption might be violated if either scope for registering ghost buildings changes discontinuously at the population cutoff or, conditional on scope for stricter enforcement, if buildings' characteristics and tax rates differ as well. If this is the case, then any difference in tax collections would reflect a systematic difference in revenue gains from any mechanical increase in the tax base. We test this possibility by comparing the distribution of ghost buildings intensity (Figure B8), pre-program tax rates (Figure B9), rental value and buildings' characteristics (Figure B10). In each of these cases, we do not find any significant difference as one crosses the cutoff defining eligibility for fiscal restraints, thus suggesting that expected revenue gains from enforcing ghost buildings' registration would be similar for the two groups of municipalities.

2.6 Results

This section presents the results on the impact of the Ghost Buildings program on municipality-level tax revenue (Hypothesis 1), public spending (Hypothesis 2) and tax rates (Hypothesis 3). For the sake of space, we report additional results and robustness checks in Appendix B.

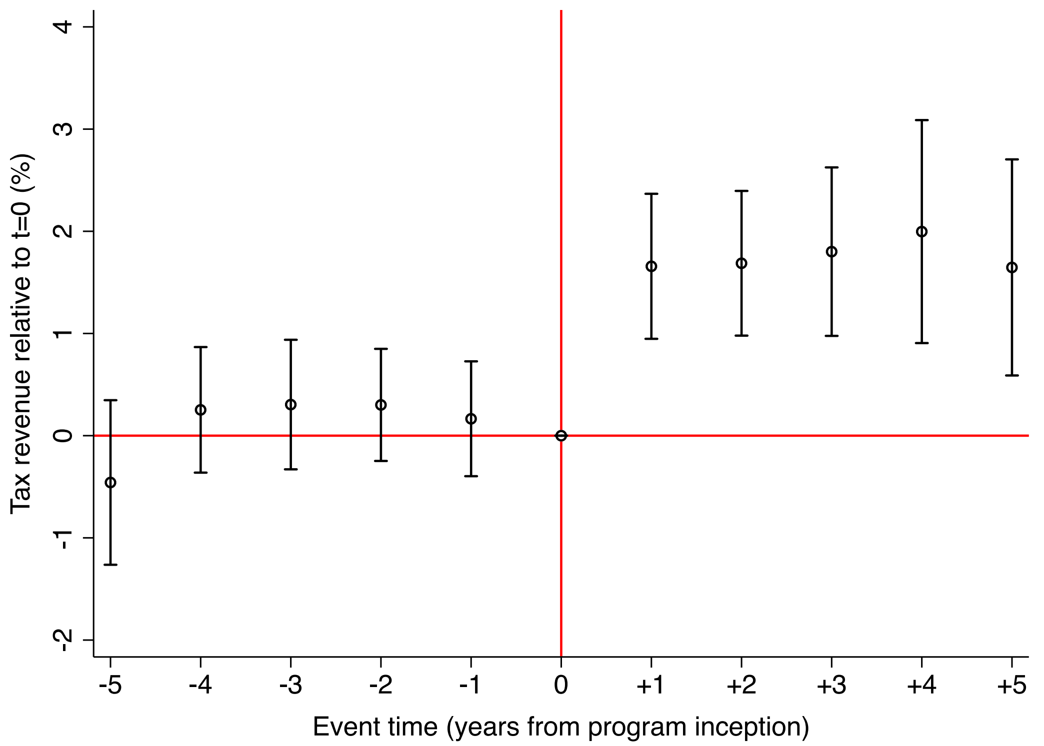
2.6.1 Tax revenue

Baseline results. We start by presenting the effect of the Ghost Buildings program on tax collections. Figure 2.4 presents the estimated β_j coefficients from equation (2.1): each point shows the effect of having implemented the Ghost Buildings program for j years (if $j > 0$) or of starting the policy j years before (if $j < 0$) relative to the actual inception year. Three main findings emerge. First, there are no pre-existing differences in the tax revenue trend across municipalities: the β_j are not significantly different from zero for at least five pre-program years.[27] The parallel trend assumption is thus satisfied. Second, tax revenue have started to increase exactly from the time the program was put in force. Third, the revenue increase is sharp, stable and persistent up to five

[27] Hereafter, we only plot estimates up to 5 years before and 5 years after the program inception year because the vast majority of municipalities is observed during this time window. However, the regressions include the full set of dummies as specified in equation (2.1). Results are not sensitive to excluding municipalities that are not observed over this time window.

years after the implementation of the program.

Figure 2.4: The impact of the Ghost Buildings program on tax collections



Note: This graph presents the impact of the Ghost Buildings program on log of tax revenue. The figure plots the estimated β_j coefficients from equation (2.1) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > 0$) or of starting the policy j years before (if $j < 0$) relative to the actual program starting year. Standard errors clustered at municipality-level.

To assess the magnitude of these effects and compare the estimated coefficient with the projected increase based on a scenario of full compliance, Table 2.2 presents coefficient estimates and standard errors obtained by running equation (2.2) . We start from a basic model including municipality fixed effects (column 1), and then we cumulatively include year fixed effects, time-varying municipalities controls,[28] province-year fixed effects and election year-year fixed effects. Column 5 (our preferred specification) presents the coefficient estimated from a two-stage least squares (2SLS) model where the dummy for the actual program inception year is instrumented by the the

[28]We control for demographic characteristics (population, share of population 65+, share of population 15-, share of foreign), unemployment rate, and individual-level characteristics of the mayor and other members of the town council (i.e., gender, age and years of education of the mayor and average value of the same variables within the town council). We also include the interaction between each of these variables and a dummy for the post-program period.

provincial modal year. In the last column, we exploit the fact that the program was not implemented in Trentino Alto-Adige region to run a triple difference analysis. The coefficient remains substantially similar. Overall, this table shows that the impact of the program on tax revenue is consistently positive in each specification.

Table 2.2: Baseline effects on tax revenue

	Outcome: log of tax revenue					
	OLS	OLS	OLS	OLS	2SLS	2SLS & DDD
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{i,t} \times GB_i$	2.477***	2.359***	1.686***	1.342***	1.283***	1.229***
	(0.206)	(0.205)	(0.202)	(0.278)	(0.295)	(0.295)
Observations	115,635	115,635	115,635	115,635	115,635	119,670
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes
Province × year FE	No	No	No	Yes	Yes	Yes
Election year × year FE	No	No	No	Yes	Yes	Yes
Mean dependent (€1,000)	1,972	1,972	1,972	1,972	1,972	1,937

Note: This table shows the effect of the Ghost Buildings program on log of municipal tax revenue. $Post_{i,t} \times GB_i$ is the interaction between a dummy for the (municipality-specific) post-program period and the share of ghost buildings detected in a municipality. Column (5) reports estimates from an instrumental variable approach where the post-program dummy is instrumented by the provincial modal year of the program inception year. Column (6) combines the 2SLS approach with a triple difference approach that exploits the fact that one region did not participate into the program. The sample covers 7,709 municipalities over the 2001-2015 period. First-stage coefficient is 0.958 (0.007). Standard errors clustered at municipality-level.

The baseline effect implies that a 1 standard deviation increase in municipality-level program intensity (corresponding to a 1.662 percentage points increase in the share of unregistered buildings) raises tax revenue by 2.1 percent of the sample mean, which amounts to nearly 42,043 euros of additional revenue per year. This effect accounts for 70 percent (that is, 2.1 percent of the predicted 3 percent) of the projected tax revenue increase that the program would have generated in a scenario with perfect compliance.

To put this number in perspective, if we move from Trento (where ghost buildings intensity was 0) to Crotone (where one-tenth of the stock of buildings was unregistered), we would observe an increase in tax revenue by around 14 percent in Crotone compared to Trento, which corresponds to nearly 1,140 thousand euros of additional revenue per-year.[29]

The estimated impact can be ascribed to the Ghost Buildings program if we observed a contemporaneous offsetting change in government grants. In line with the graphical evidence presented above, Figure B11 shows a clear fall in government grants. Table B2 presents the same specifications as above, but using the log of government grants as outcome. Our preferred specification (column 5) implies that a 1 standard deviation increase in ghost buildings intensity decreases government grants by 2.7 percent, which is fairly close to the projected reduction based on the back-of-the-envelope calculations presented in section 2.2. Moreover, we also show that the timing of the effect is consistent with an increase in the tax base (see Figure B12 and the bottom panel in Table B2), while we do not find any significant change in other potential revenue sources, such as loans or mortgages (see Figure B13). Overall, these tests provide evidence that it is the additional tax base generated by ghost buildings' registration that drives the variation in tax revenue. In line with *Hypothesis 1*, these results suggest that the Ghost Buildings program raises tax revenue.

Finally, we account for the possibility that the error term was spatially correlated across municipalities located in the same local labor market. Following the suggestions by Angrist and Pischke (2009) to "pass the buck up one level", we cluster the standard errors on a higher level of aggregation. Figure B14 shows that our estimates remain statistically significant at usual confidence intervals when we employ standard errors clustered at province- or region-level.

Local administrators' compliance. The results presented in the previous paragraphs showed that actual tax revenue were lower than projected tax revenue. From a policy perspective, it is important to understand whether missing revenue can be attributed to local administrators' imperfect compliance.

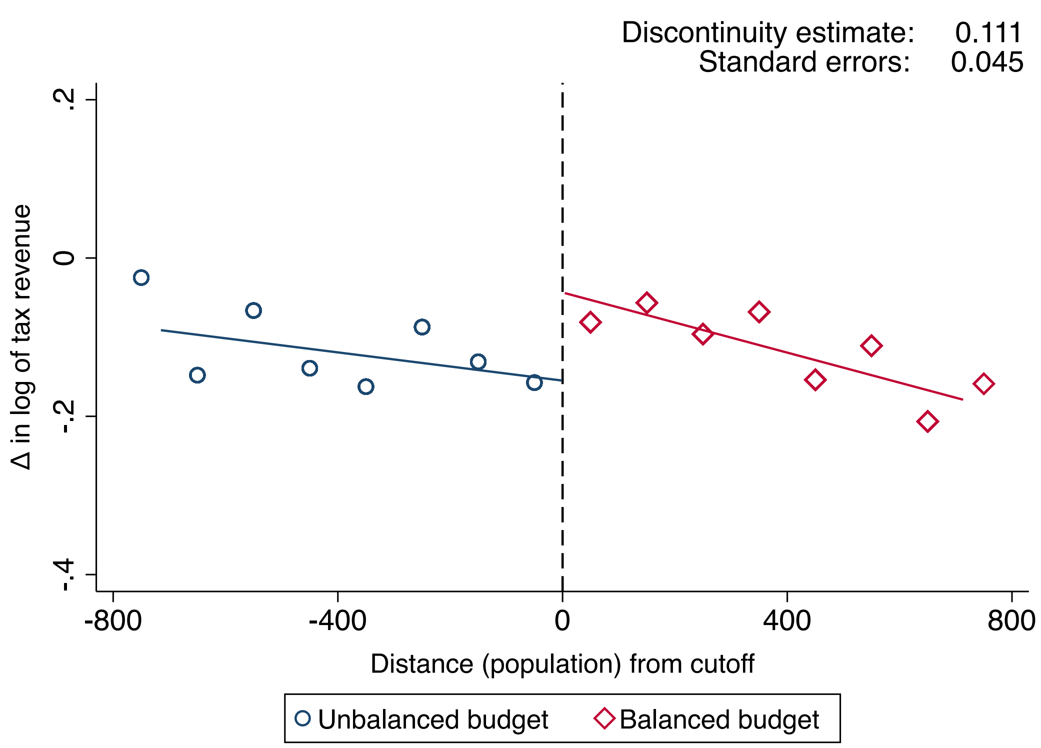
[29]Moving from Trento to Crotone implies a 6.667 standard deviation increase in ghost buildings intensity. Tax revenue in Crotone over the pre-program period were 8,137,109 euros.

We study the role of local administrators by exploiting a discontinuity in the possibility of accumulating debt and run deficit. Since failing to enforce ghost buildings' registration would reduce government grants and eventually increase budget deficit, we expect larger revenue in municipalities eligible for fiscal rules. Therefore, we aim to identify the role of local administrators by comparing tax revenue before and after the Ghost Buildings program (to cancel out any systematic cross-municipality heterogeneity, including any (time-invariant) policy changing at the same cutoff) and just below and above the cutoff defining eligibility for fiscal rules.

Figure 2.5 presents the difference in log of tax revenue in 20 equal size bins and a linear fit for observations at both sides of the cutoff. The sample is composed of 549 municipalities, having a population size between 4,283 and 5,717, as defined by employing the data driven choice of the optimal bandwidth proposed by Calonico et al. (2014) (we will present results for different range of bandwidths below). We also report the δ_0 coefficient estimate and municipality-level clustered standard errors obtained by running regression (2.3). The figure shows that eligibility for fiscal restraints creates a revenue gap: local administrators in municipalities with a balanced budget rule collect, on average, 11.1 percent more revenue. This finding confirms *Hypothesis 1*: ceteris paribus, tax revenue are higher in municipalities subject to fiscal restraints.

We then allow for treatment intensity by multiplying the interaction between the dummy for post-reform period and the dummy for fiscal rules ' eligibility, $Post_{i,t} \cdot D_i$, with ghost buildings intensity, GB_i. Table 2.3 shows the results. Our baseline estimate (column 1) shows that a 1 standard deviation increase in ghost buildings intensity (corresponding to an increase of 1.577 ghost buildings share in this sample) leads to a tax revenue increase that is 6.764 percent larger in municipalities eligible for fiscal restraints, compared to municipalities not eligible for fiscal restraints. In the rest of the table, we test the sensitivity of this result to polynomial order's choice and the inclusion of municipality-specific time-varying control variables. The coefficient estimate

Figure 2.5: Imperfect compliance in ensuring enforcement



Note: This graph shows the impact of eligibility for a balanced budget rules on local administrators' compliance in ensuring enforcement. The vertical axis is the difference in log of tax revenue before and after the Ghost Buildings program. The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins. The optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014).

remains substantially similar across specifications.

We conduct three additional robustness checks to corroborate this finding. First, we validate the (local) parallel trend assumption in Figure B15, where we depict year-by-year coefficient estimates and confidence intervals. We do not find any significant difference in tax revenue across municipalities below and above the cutoff for up to 5 years before the introduction of the Ghost Buildings program, thus suggesting that municipalities were on similar trends. By contrast, a clear and persistent discontinuity arises immediately after the introduction of the program. This result unambiguously shows that the increase in tax revenue was driven by the Ghost Building program and not by pre-existing trends across municipalities.

Second, the difference-in-discontinuity estimate rests on the assumption that other demographic and political economy factors are not varying systematically across mu-

Table 2.3: Local administrators' imperfect compliance

	Outcome: log of tax revenue					
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{i,t} \times D_i \times GB_i$	4.292***	3.427***	3.975***	3.196***	4.210***	3.366***
	(0.786)	(0.819)	(0.817)	(0.873)	(0.824)	(0.901)
Observations	549	549	549	549	549	549
Polynomial order	1	1	2	2	3	3
Controls	No	Yes	No	Yes	No	Yes

Note: This table reports difference-in-discontinuity estimates of the impact of the Ghost Buildings program on the difference in log of tax revenue across municipalities below and above the cutoff defining eligibility for fiscal rules. In Columns (1) and (2), we run local linear regressions. Columns (3) and (4) use a second-order polynomial, while we use a third-order polynomial in columns (5) and (6). Column (2), (4) and (6) include municipality-specific control variables. Optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014). Standard errors clustered at municipality-level.

nicipalities above and below the cutoff in response to the Ghost Buildings program. If, for instance, the introduction of the Ghost Buildings program led high skill local administrators to be appointed, we would capture a selection effect. We explore this possibility by running equation (2.3) on several mayor and town council-specific characteristics, including their ability, age and sex. Table B3 reports the results. We do not find any significant effect of the program on each of these variables.

Finally, we test whether our results are sensitive to bandwidth choice. Figure B16 displays coefficient estimates and 95 percent confidence intervals obtained by estimating equation (2.3) using different bandwidths around the threshold. The figure shows that our coefficient estimate is unaffected by bandwidth choice for a reasonable range of bandwidths.

Heterogeneity analysis. We now study whether, conditional on scope for ghost buildings' registration, collected revenue significantly differ across municipalities according to specific characteristics of local administrators, citizens, and housing sector. First, we study whether local administrators' ability matters. Ability might reflect, e.g., the capacity to persuade evaders to register the buildings. We create the dummies $LowAbility_i$ and $HighAbility_i$, equal to 1 for municipalities where the share of graduated local administrators lies in the bottom or top decile, respectively, of the national distribution. Column (1) in Table 2.4 shows that, conditional on scope for stricter enforcement, low ability local administrators were relatively less successful in enforcing ghost buildings' registration.

Table 2.4: Heterogeneous effects on tax revenue

	Outcome: log of tax revenue		
	(1)	(2)	(3)
$Post_{i,t} \times GB_i$	1.293***	0.804**	1.318***
	(0.433)	(0.336)	(0.411)
$Post_{i,t} \times GB_i \times LowAbility_i$	-1,796***		
	(0.711)		
$Post_{i,t} \times GB_i \times HighAbility_i$	0.175		
	(0.404)		
$Post_{i,t} \times GB_i \times LowTaxCheat_i$		3.514***	
		(0.920)	
$Post_{i,t} \times GB_i \times HighTaxCheat_i$		-2.734**	
		(1.212)	
$Post_{i,t} \times GB_i \times LowOwnerOcc_i$			0.291
			(0.364)
$Post_{i,t} \times GB_i \times HighOwnerOcc_i$			-1.468**
			(0.705)
Observations	115,635	115,635	115,635
Baseline controls	Yes	Yes	Yes

Note: This table presents heterogeneous responses of the Ghost Buildings program on tax revenue. Each specification includes municipality fixed effects, province-year fixed effects, election year-year fixed effects and municipality controls. The interaction between the dummy for the post-program period and the share of ghost buildings in a municipality, $Post_{i,t} \times GB_i$, is interacted with dummy variables for politicians with low or high ability as measured by the town council's average years of education (column 1), low or high tolerance toward tax cheating behaviors (column 2), and municipalities with a low or high share of owner-occupied homes (column 3). A municipality is classified as a high (low) level of a specific indicator if it has a value in the top (bottom) decile of the national distribution of that indicator. Standard errors clustered at municipality-level.

Second, we study whether the program was more successful in places where the underlying preferences for curbing tax evasion are more spread. As emphasized by political scientists, social and civic capital are key values for the successful operation of government policies (see, e.g., Lipset 1960; Almond and Verba 1963; Putnam 1993). We use information on tolerance toward tax cheating behavior from survey data to construct the dummies $LowTaxCheat_i$ and $HighTaxCheat_i$, that are equal to 1 in places where the average score is in the bottom or top decile, respectively, of the national distribution. Column (2) in Table 2.4 shows that the program was relatively more successful in municipalities with a lower level of tolerance for tax cheaters. By contrast, places where residents are less likely to blame tax evaders have significantly lower tax intakes as a result of the Ghost Buildings program. A simple interpretation for this result is that local administrators' compliance rate is larger in places where voters are more likely to blame evaders.

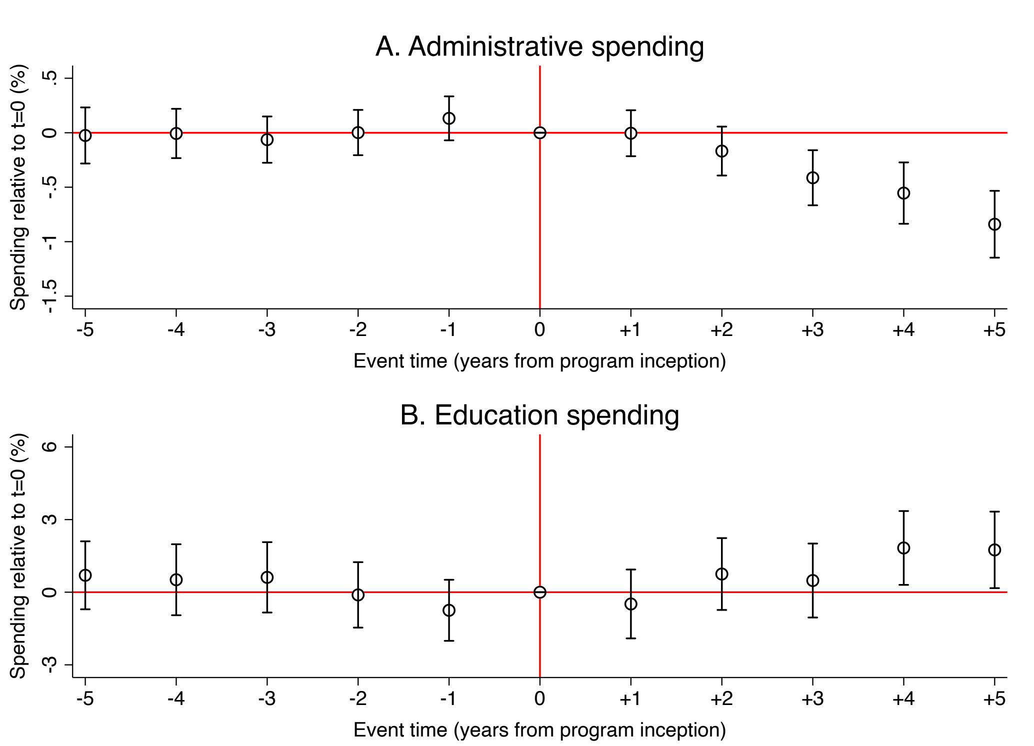
Finally, we explore whether local administrators are less likely to raise tax collections in places where the buildings' stock is mostly composed of owner-occupied homes. Since enforcing ghost buildings' registration raises the tax burden and might possibly harm re-electoral chances, compliance rate can be lower in place with a larger share of owner-occupied homes. To explore this possibility, we create the dummies $HighOwnerOcc_i$ and $LowOwnerOcc_i$, which take value 1 in a municipality when the share of owner-occupied homes lies in the top or bottom decile, respectively, of the national distribution. Column (3) consistently shows that tax collections are significantly lower in places where the stock of owner-occupied homes is relatively larger. One explanation is that local administrators are less willing to enforce ghost buildings' registration in places where a large number of residents (i.e., voters) would be affected by the program.

2.6.2 Public spending

Baseline results. This section presents the effect of the Ghost Buildings program on municipality-level public spending. Namely, we study whether shifting from government grants to tax revenue as a source of funding for municipalities had any impact on public spending allocation.

Figure 2.6 depicts the β_j coefficients and standard errors estimated by running equation (2.1) on two outcomes: administrative spending per-capita (top panel) and education spending per-capita (bottom panel). As in the previous figure, the event time is set relative to the program inception year, and dynamic treatment effects are rescaled by the baseline spending at $t = 0$. The evidence provided in this figure is consistent with the concept that switching from a less to a more transparent source of revenue raises politicians' accountability and public goods provision. In accordance with *Hypothesis 2*, we observe a substantial and gradual decline in administration expenditure and an increase in school spending.[30]

Figure 2.6: Event study for public spending allocation



Note: This graph presents the effects of the Ghost Buildings program on log of administrative spending per-capita (top figure) and education spending per-capita (bottom figure). The figure plots the estimated β_j coefficients from equation (2.1) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > 0$) or of starting the policy j years before (if $j < 0$) relative to the actual program starting year. Standard errors clustered at municipality level.

Table 2.5 quantifies the average effect of the Ghost Buildings program on log of administrative spending per-capita (top panel) and education spending per-capita (bottom panel). The table reports coefficients estimates and standard errors obtained by running equation (2.2).[31] The coefficient estimates for administrative spending are

[30]Differently from tax revenue, the effect appears to start from $t = +2$. This temporal lag might reflect the fact that public spending decisions are approved by the town council by the 30th of April of the following year.

[31] Figure B17 presents alternative results using standard errors clustered at province- or region-level.

consistently negative, although the magnitude of the effect varies somewhat across specifications. In particular, the effect becomes larger once we include province-year fixed effects and election year-year fixed effects, which account for any local economic shocks or the fact that electoral incentives for pursuing policies aiming to capture voters are stronger when legislative elections approach (and voters' attention increases). On average, our preferred specification (column 5) suggests that a 1 standard deviation increase in ghost buildings intensity decreases administrative spending by 2.6 percent, which corresponds to a reduction of around 9.3 euros per-capita (baseline administrative spending per-capita is 359 euros).

By contrast, we estimate that the program has a positive effect on school spending per-capita. Although the estimated coefficient does not vary substantially across specifications, we obtain less precise estimates in specifications with province-year and election year-year fixed effects. The baseline specification suggests that a 1 standard deviation increase in scope for enforcement raises school spending per-capita by 1.6 percent (from a baseline of around 6 euros per-capita). In aggregate terms, our estimates suggest that a 1 standard deviation increase in ghost buildings intensity would increase, on average, education spending by around 484 euros per-municipality. According to the estimate provided by Pavese and Rubolino (2020), a 1 standard deviation increase in ghost buildings intensity would raise standardized test scores by around 0.06 of a standard deviation.[32]

The intuition behind this observed change in public spending allocation is that any revenue increase obtained through higher taxes is obviously visible by taxpayers, while an increase in revenue financed by government transfers might not be observed at all by citizens. As a result, any increase in tax revenue would make politicians more accountable and ultimately leads to a more efficient allocation of public spending. This

[32]Pavese and Rubolino (2020) estimate that a 100 euros increase in school spending per-pupil raises standardized math test scores by 0.119 of a standard deviation. A back-of-the-envelope calculation implies that a 1 standard deviation increase in ghost buildings intensity would raise school spending by around 46.15 euros per-pupil.

Table 2.5: Baseline effects on public spending

	OLS	OLS	OLS	OLS	2SLS	2SLS & DDD
	(1)	(2)	(3)	(4)	(5)	(6)
	A. *Outcome*: log of administrative spending per-capita					
$Post_{i,t} \times GB_i$	-0.282**	-0.224*	-0.594***	-1.445***	-1.541***	-1.542***
	(0.118)	(0.118)	(0.119)	(0.158)	(0.163)	(0.163)
Mean dependent	358.71	358.71	358.71	358.71	358.71	374.92
	B. *Outcome*: log of education spending per-capita					
$Post_{i,t} \times GB_i$	1.023**	1.045**	1.369***	1.090*	0.958	0.920
	(0.440)	(0.440)	(0.458)	(0.659)	(0.693)	(0.693)
Mean dependent	5.98	5.98	5.98	5.98	5.98	6.84
Observations	115,635	115,635	115,635	115,635	115,635	119,670
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes
Province $\times$ year FE	No	No	No	Yes	Yes	Yes
Elec. year $\times$ year FE	No	No	No	Yes	Yes	Yes

Note: This table shows the effect of the Ghost Buildings program on log of administrative spending per-capita (top panel) and education spending per-capita (bottom panel). $Post_{i,t} \times GB_i$ is the interaction between a dummy for the (municipality-specific) post-program period and the share of ghost buildings detected in a municipality. The sample covers 7,709 municipalities (7,978 in last column) over the 2001-2015 period. First-stage coefficient is 0.958 (0.007). Standard errors clustered at municipality-level.

finding is closely related to the evidence presented by Gadenne (2017), showing that improvements in tax collections raise school investments in Brazilian municipalities. Similarly, Martinez (2019) exploits the timing of cadastral updates and fluctuations in oil prices to show that larger property tax revenue raises local public goods provision in Colombian municipalities.

In Table B4, we replicate the same analysis on other budget items. We do not find any significant effect on all the other public expenditure categories, with the only exception of spending to finance municipal police.[33] This spending increase might be motivated by the fact that municipal policies played a key role, along with municipal administrators, in the enforcement process. In fact, municipal police was required

[33]Crowding out of national or regional spending in education is unlikely to drive this effect: the "division of labor" between different levels of government in Italy is well defined by the Constitutional law. We can therefore rule out the concern that national and regional bodies withdraw funding in areas in which they are aware of increased spending by municipalities.

to proceed to inspections and imputation of the tax base of properties not voluntarily registered. Yet, although statistically significant, the effect appears rather small in economic terms: on average, we find that a 1 standard deviation increase in ghost buildings intensity raises municipal police spending by around 1.8 euros per-capita (see Figure B18 for the event study graph).

Heterogeneity analysis. Table 2.6 casts light on the mechanisms behind the observed effects by studying heterogeneous responses regarding i) voters' likelihood to be informed; ii) politicians' ability; iii) mayor's gender.

Table 2.6: Heterogeneous effects on public spending

	log of administrative spending per-capita			log of education spending per-capita		
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{i,t} \times GB_i$	-1.736***	-1.451***	-1.419***	0.718	1.128*	0.879
	(0.210)	(0.160)	(0.161)	(0.923)	(0.666)	(0.665)
... $\times LowTurnout_i$	0.470**			0.584		
	(0.217)			(0.906)		
... $\times LowSkill_i$		0.792*			-0.489	
		(0.423)			(1.960)	
... $\times Woman_i$			-0.239			2.129**
			(0.286)			(1.075)
Observations	115,635	115,635	115,635	115,635	115,635	115,635
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table studies heterogeneous responses of the Ghost Buildings program on municipal public spending per-capita in administration and education. All specifications include municipality fixed effects, province-year fixed effects, election year-year fixed effects and municipality controls. The post-program ghost buildings intensity indicator is interacted with a dummy variable for municipalities where political participation is lower than the median (columns 1-4), a dummy for those where the average number of years of education of town council's members is lower than the median (columns 2-5), and a dummy for municipalities with female mayors (columns 3-6). Standard errors clustered at municipality-level.

We first study heterogeneity effects with respect to voters' likelihood to be informed about how public spending is allocated, as proxied by the electoral turnout for the national election in 2013. Intuitively, places where citizens are less informed and less active in the political debate might be less willing to monitor local politicians and, thus, to limit rent-seeking behaviors (Ferraz and Finan 2008; Gadenne 2017). Consistent with this view, we find that the administrative spending cut is significantly dampened in municipalities where political participation is lower than the median value (column 1). This result underlines the role of having a more informed electorate to exert pressure on politicians for providing a more efficient allocation of public budget.

The increase in the tax burden faced by ghost buildings' owners could make them more willing to demand more from politicians. Politicians ability to translate extra revenue in worth government spending might be crucial to accommodate the electorate. In column 2, we show that municipalities where town council's members are less able (proxied by the share of members with a college degree) were less likely to reduce administrative spending.

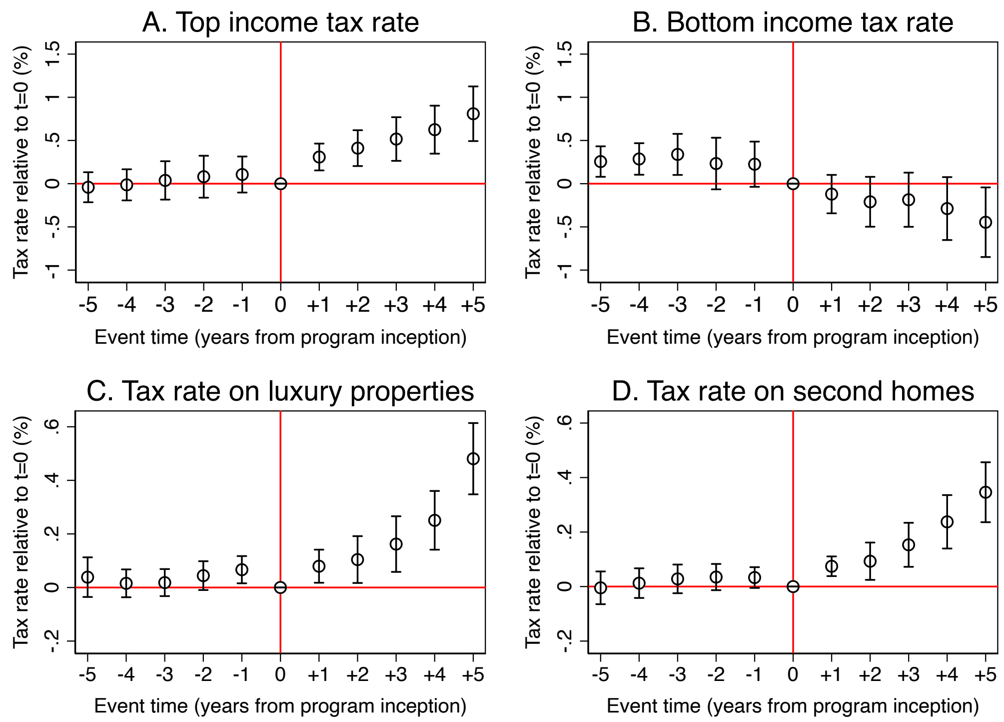
Finally, we study whether the gender of the mayor matters for public spending decision. Previous literature has provided suggestive evidence that women politicians are more likely to promote redistributive public policies both because they are less corrupt (Brollo and Troiano 2016) and more effective than men to spur economic growth through public spending (Edlund and Pande 2002; Edlund et al. 2005). Consistent with this literature, we find that the increase in education spending is significantly larger in places where mayors are women.

2.6.3 Tax rates

Baseline results. This section studies whether the Ghost Buildings program has any impact on local tax rates. Specifically, the program offers the opportunity to study the tax rate response to a broaden and relatively more enforced tax base. We focus on four (statutory) tax rates set by municipalities: i) the top marginal tax rate on personal income; ii) the bottom marginal tax rate on personal income; iii) the tax rate applied to luxury properties reported as main residence; iv) the tax rate applied to buildings different from the main residence.

We start by plotting the event study estimates from equation (2.1) in Figure 2.7. This figure provides two main findings. First, there is a clear change in the distribution of the income tax burden as a response to the program. Top graph depicts a significant gradual increase in the top tax rate and, by contrast, a reduction in the income tax rate faced by poorer taxpayers (although the pre-program coefficients are significantly different from zero in some years). This result points to a positive increase in *structural progressivity* of the municipal income tax schedule.

Figure 2.7: Event study for local tax rates



Note: This graph presents the effects of the Ghost Buildings program on local statutory tax rates. The figure plots the estimated β_j coefficients from equation (2.1) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > 0$) or of starting the policy j years before (if $j < 0$) relative to the actual program starting year. Standard errors clustered at municipality-level.

Second, we find that the program led to larger tax rates on both luxury properties and second homes. These results are consistent with our *Hypothesis 3*: a broader and more enforced tax base leads local administrators to exploit the "mechanical gains" of raising taxes. In all cases, we find significant effects up to 5 years after the program inception year.

Table 2.7 presents coefficient estimates and standard errors on the effect of the program on local tax rates, obtained by running equation (2.2).[34] The table shows that estimates are hardly affected when controlling for municipality-specific controls, province-year fixed effects and election year-year fixed effects or by using different econometric models. Our preferred specification (column 5) shows that a 1 standard deviation increase in ghost buildings intensity raises the top marginal tax rate on income by 0.6 percent, while the bottom marginal tax rate falls by 0.7 percent.

Table 2.7: Baseline effects on local marginal tax rates

	OLS	OLS	OLS	OLS	2SLS	2SLS & DDD
	(1)	(2)	(3)	(4)	(5)	(6)
	A. *Outcome*: log of top income tax rate					
$Post_{i,t} \times GB_i$	0.557***	0.569***	0.471***	0.339***	0.341**	0.347***
	(0.093)	(0.093)	(0.092)	(0.127)	(0.134)	(0.133)
Mean dependent (%)	0.352	0.352	0.352	0.352	0.352	0.340
	B. *Outcome*: log of bottom income tax rate					
$Post_{i,t} \times GB_i$	-0.389***	-0.410***	-0.335***	-0.422***	-0.427***	-0.431***
	(0.109)	(0.109)	(0.110)	(0.155)	(0.164)	(0.164)
Mean dependent (%)	0.278	0.278	0.278	0.278	0.278	0.269
	C. *Outcome*: log of property tax on luxury buildings					
$Post_{i,t} \times GB_i$	0.089***	0.111***	0.070***	0.157***	0.166***	0.170***
	(0.026)	(0.026)	(0.026)	(0.037)	(0.038)	(0.038)
Mean dependent (%)	0.495	0.495	0.495	0.495	0.495	0.492
	D. *Outcome*: log of property tax on second homes					
$Post_{i,t} \times GB_i$	0.146***	0.107***	0.090***	0.154***	0.158***	0.162***
	(0.023)	(0.022)	(0.022)	(0.032)	(0.034)	(0.034)
Mean dependent (%)	0.676	0.676	0.676	0.676	0.676	0.671
Observations	115,635	115,635	115,635	115,635	115,635	119,670
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes
Province × year FE	No	No	No	Yes	Yes	Yes
Election × year FE	No	No	No	Yes	Yes	Yes

Note: This table displays the effect of the Ghost Buildings program on the (log of) tax rates set by municipalities. The sample covers 7,709 municipalities (7,978 in the last column) over the 2001-2015 period. Standard errors clustered at municipality-level.

We find that the program leads to higher property taxes. Point estimate in column (5) implies that a 1 standard deviation increase in the Ghost Buildings program leads to an increase by around 0.3 percent in property tax rates. This clear change in property

[34] Figure B19 displays alternative results using standard errors clustered at province- or region-level.

taxes suggests that the absence of a monitoring technique and the possibility of not registering a property led to suboptimal tax rates.

Heterogeneity analysis. This section explores heterogeneity responses to the tax rate increase. First, we ask whether policy makers raise taxes for redistributive purposes. If growing inequality was the main concern, the tax rate increase should be relatively larger in places where the pre-program distribution of income was more unequal. We shed light on this mechanisms by using data on the municipality-specific pre-tax Gini index.[35] We split municipalities according to their pre-program level of local income inequality to create the dummy $HighIneq_i$, equal to 1 for municipalities with a Gini index above the median. Top panel in Table 2.8 shows that the impact of the program on the top income tax rate was significantly larger in more unequal municipalities.

Second, if optimal tax rules are defined by means of the Ramsey's inverse elasticity rule, local administrators might take into account the responsiveness of the reported tax base to larger statutory tax rates. If, for instance, taxpayers in certain places had access to better evasion or avoidance technologies or their labor supply is more sensitive to tax rate changes, then local policy makers would be less willing to raise taxes. To summarize how much taxpayers are likely to respond to a tax rate change, we estimate the (pre-program) tax base elasticity of a municipality with respect to its net-of-tax rate. This elasticity incorporates all the behavioral responses of taxpayers to any tax rate change set by municipalities (Saez et al. 2012) and it measures the efficiency cost of raising taxes in terms of missing revenue.[36] Using the estimated elasticity, we cre-

[35] The Gini index is computed from municipality-specific tabulated data on taxable income from 7 income intervals, following the procedure proposed by Milanovic (1994) and Abounoori and McCloughan (2003).

[36] Empirically, the tax base elasticity is estimated as the β parameter from regressions of the following form: $log(y_{i,t}) = \beta log(1 - \tau_{i,t}) + \gamma X_{i,t} + \delta_i + \epsilon_{p(i),t} + \eta_{e(i),t} + u_{i,t}$, where the outcomes is the tax base of municipality i at year t; τ is the tax rate (the top marginal tax rate, the bottom marginal tax rate or the property tax rate depending on the specification). $X_{i,t}$ are municipality-level time-varying controls; δ_i, $\epsilon_{p(i),t}$, and $\eta_{e(i),t}$ are municipality fixed effects, province-year fixed effects, and election year-year fixed effects, respectively.

Table 2.8: Heterogeneous effects on local tax rates

	log(Inc tax) Top rate (%) (1)	log(Inc tax) Bottom rate (%) (2)	log(Prop tax) Luxury pr. (%) (3)	log(Prop tax) Second pr. (%) (4)
	by pre-program income inequality level			
$Post_{i,t} \times GB_i$	0.399**	-0.315*	0.172***	0.186***
	(0.157)	(0.176)	(0.033)	(0.041)
$Post_{i,t} \times GB_i \times HighIneq_i$	0.189**	-0.148	-0.030	-0.010
	(0.096)	(0.117)	(0.031)	(0.028)
	by cost of raising taxes			
$Post_{i,t} \times GB_i$	1.615***	0.460**	0.241***	0.318***
	(0.170)	(0.226)	(0.055)	(0.051)
$Post_{i,t} \times GB_i \times HighCost_i$	-1.350***	-1.326***	0.001	-0.102**
	(0.154)	(0.209)	(0.050)	(0.048)
Observations	115,635	115,635	115,635	115,635
Baseline controls	Yes	Yes	Yes	Yes

Note: This table studies heterogeneous responses of the Ghost Buildings program on local marginal tax rates. All specifications include municipality fixed effects, province-year fixed effects, election year-year fixed effects and municipality controls. The interaction between the dummy for the post-program period and the share of ghost buildings in a municipality, $Post_{i,t} \times GB_i$, is interacted with dummy variables for municipalities with a pre-program Gini index higher than the median value (panel a) and for those where the cost of raising taxes (captured by the net-of-tax tax base elasticity) is larger than the median value (panel b). The sample covers 7,709 municipalities over the 2001-2015 period. Standard errors clustered at municipality-level.

ate the dummy $HighCost_i$, equal to 1 if the cost of raising taxes in a municipality (i.e., its tax base elasticity) is larger than the median value. The bottom panel in Table 2.8 shows that policy makers do discount for the expected behavioral response of taxpayers when deciding on tax policy. The increase in the top marginal tax rate and property tax on second homes was substantially dampened in places with larger tax base elasticity, suggesting that the threat of tax base flee prevents policy makers by changing taxes. Accordingly, the tax rate reduction is significantly larger in places where taxpayers are more sensitive to the tax rate. This might suggest that policy makers attempt to spur economic growth by lowering the tax burden faced by the poor.

2.7 Conclusion

How successful are anti-tax evasion policies in raising revenues? Do politicians react to exogenous increase in revenue by seeking rents or spending it on public goods? Does tax evasion hinder tax progressivity by raising the cost of setting higher taxes on the rich? This paper aims to answer these questions by focusing on the Ghost Buildings program: an anti-tax evasion program implemented in Italy that detected more than 2 millions of buildings hidden to tax authorities.

Exploiting cross-municipality variation in the share of detected unregistered buildings and the staggered introduction of the program, we uncover a stable and persistent increase in tax revenue, corresponding to nearly three-fourth of the projected mechanical increase in the tax base. This discrepancy is mostly due to imperfect compliance by local administrators in financially unconstrained municipalities. We also show that decisions regarding how to finance local governments are a key feature for ensuring a more efficient allocation of public spending. Finally, we provide evidence that stricter tax enforcement leads to larger property tax rates and to an increase in the progressivity of the local income tax schedule.

Chapter 3

Knocking on Parents' Doors: Regulation and Intergenerational Mobility

3.1 Introduction

The literature on intergenerational mobility documents that socioeconomic status persists over generations in all countries studied so far, although to varying degrees (Black and Devereux 2011; Corak 2013). A growing number of papers also document persistence within specific occupations, such as doctors (Lentz and Laband 1989), lawyers (Laband and Lentz 1992), academic professors (Durante et al. 2011), pharmacists (Mocetti 2016) and liberal professions (Aina and Nicoletti 2018). The literature on the causes of intergenerational persistence has been largely dominated by the debate on the relative importance of an individual's innate qualities (nature) versus environmental factors (nurture),[1] while the role of the functioning of the labor market remains substantially under-investigated.

Another strand of literature is aimed at understanding the economic effects of regulation of occupations (Kleiner 2000). One of the main justifications for regulation in certain professions is the existence of asymmetric information between suppliers and clients that, in turn, may lead to a market failure. However, excessive regulation may hinder competition and generate monopoly rents, especially when regulation is mainly shaped by the interests of the incumbents. Empirical studies usually find higher earnings for individuals in regulated occupations (Kleiner and Krueger 2013), while the evidence regarding the effects on the selection of practitioners is scant and based on peculiar case studies.

The present paper stands at the junction between these two strands of literature. Our aim is to provide a first thorough analysis of how regulation affects intergenerational persistence in occupations and therefore entry opportunities and allocative mechanisms of these labor markets.

Distinguishing a career following that is motivated by an intergenerational trans-

[1]See, among others, Bowles et al. (2005), Björklund et al. (2006) and Sacerdote (2011).

fer of occupation-specific human capital (through either nature or nurture) from that caused by regulation and positional rents is empirically challenging. To address this issue, we exploit two reforms relating to the regulation of professional services that have been implemented in Italy since the 2000s: the "Bersani decree" in 2006 and the "Monti reform" in 2011. Although the liberalization of Italian professional services was remarkable in some respects, initial conditions differed substantially across occupations, while the pace and extent of regulatory reform also varied substantially. To measure the strictness of regulation, we build an OECD-style index for 14 occupations and for three different cohorts (i.e., before and after each reform). The propensity of children to follow their parents' career is measured using data from the Labor Force Survey (LFS), matching the (2-digits) degree program on which they are enrolled with the (4-digits) occupation of their parents. Namely, we proxy occupational persistence with an indicator that is equal to 1 if children pursue a course of study that naturally leads to the same occupation as their parents.[2] Then, we exploit the differential effect of regulation on career following for professionals (treated group) and employees in similar occupations (control group), before and after each reform.

We find significant heterogeneity in intergenerational persistence across occupations: career following is remarkably high among lawyers and pharmacists, whereas it is relatively lower among natural scientists. We also find that regulation does affect the extent of occupational persistence. According to our estimate, the combined effect of the two regulatory reforms (that corresponds to a 1.7 decrease in our index of the strictness of regulation on a 0-6 scale) reduced the propensity of career following by about 4 percentage points (more than one-third of the sample mean). The impact is stronger for occupations in the social sciences (e.g., lawyers, accountants, etc.) and in areas where the local economy is more dependent upon professional services (i.e.,

[2]It is worth noting that our main outcome variable captures the *likelihood* of career following and not occupational persistence, as we observe the degree program and not the final occupation of the children. However, for the sake of simplicity, we also refer to it as (risk of) occupation persistence.

where economic rents are higher). Interestingly, at the individual level, the impact of regulation on occupational persistence is stronger for less able children, thus further confirming the existence of allocative inefficiencies in the distribution of talents across occupations. As far as the domains of regulation are concerned, the effect of regulation is entirely driven by restriction on market conduct (e.g., restrictions on prices and advertising); by contrast stricter entry requirements are associated with fairer entry opportunities in certain occupations.

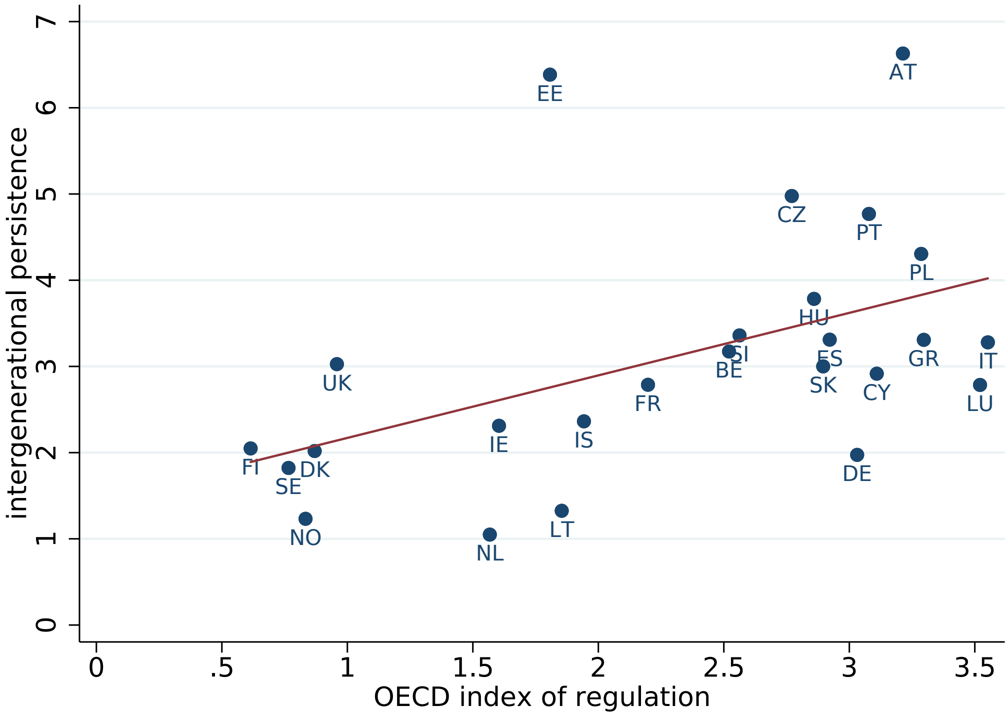
Our paper contributes to the literature in three main ways. First, the overwhelming majority of existing empirical studies that examine the factors responsible for the observed intergenerational persistence has focused on nature versus nurture and on the mediating role of the education system (Black and Devereux 2011).[3] Surprisingly, the role of regulation, which may heavily influence economic returns and barriers to entry in certain occupations, is largely neglected. However, a simple cross-country evidence shows that the strictness of regulation in professional services is positively associated to intergenerational persistence in related occupations (see Figure 3.1).[4] Moreover, regulation is not a second-order issue as a large proportion of workers is employed in regulated sectors (Kleiner 2000; Koumenta and Pagliero 2019); as far as Italy is concerned, individuals employed in occupations whose activity requires membership of a professional body (*professioni ordinistiche*) represent about 10 percent of the total employment and 31 percent of those with a college degree (Mocetti et al. 2019).

Second, our empirical strategy allows the identification of a causal nexus (from anti-competitive regulation to career following), thus overcoming the descriptive approach

[3]As far as occupational persistence is concerned, Lindquist et al. (2015) find, using Swedish administrative data on adoptees and on their biological and adoptive parents, that post-birth factors matter twice as much as pre-birth factors in explaining intergenerational transmission of occupations. However, they are not able to identify the underlying mechanisms behind such a large post-birth effect.

[4]In countries with a lower index of regulation (i.e., with more market-friendly regulatory environments) the probability of being employed in a certain occupation if one's parent is employed in the same occupation is considerably lower. However, this cross-section association needs to be interpreted with caution as regulation and intergenerational mobility might likely have common correlates that cannot all be credibly controlled for.

Figure 3.1: Occupational persistence and regulation across countries



Note: Intergenerational persistence is measured with the odds ratio, i.e., the probability of becoming member of a profession if one's parent is a member of the same profession relative to the corresponding probability for the overall population. Data are drawn from EU-SILC survey conducted in 2005 containing a specific section concerning intergenerational mobility with information on occupation (at the 2-digit level) for both children and parents (recorded in a retrospective fashion). Regulation is measured with the OECD product market indicator for professional services (i.e., accountants, lawyers, engineers and architects).

that has prevailed in previous studies on occupational persistence.[5] In contrast, our empirical strategy allows us to control for occupation-specific transmission channels while exploiting occupation-specific variation in the regulatory environment. Finally, we contribute to the literature on the impact that regulation has on the selection of practitioners. We show that, beyond a *natural* degree of persistence (due, for example, to intergenerational transmission of occupation-specific skills), regulation generates rents that bias the allocation of individuals across occupations depending on their family background. This effect is larger for less able children, thus reinforcing the idea

[5]Indeed, other papers have examined the role of non-market factors, such as *nepotism*, on occupational persistence but they do not provide *causal* evidence. One exception is Mocetti (2016), which focuses on the Italian pharmacists' labor market and exploits (cross-sectional) discontinuity produced by regulation in the ratio between the number of pharmacies allowed and the population.

of a potential negative impact in terms of selection of practitioners.[6]

The rest of the paper is organized as follows. In the next section, we provide a deeper discussion on the economics of regulation (Section 3.2.1) and on the channels through which it might favor the intergenerational transmission of occupations (3.2.2). In Section 3.3, we describe the data and the main variables. In Section 3.4, we discuss the empirical strategy. Section 3.5 shows and analyzes the main results. Section 3.6 concludes.

3.2 Background

3.2.1 Institutional framework

In each country, a complex set of laws and institutions regulates the functioning of the product and labor markets. As far as professional services are concerned, regulation might affect entry into a given market, the supply of services and the prices applied to consumers. Entry requirements generally include: having a university degree in a field of studies relevant for the specific occupation (e.g., a degree in law for becoming a lawyer); the acquisition of professional experience (e.g., through a practice period spent under the supervision of a professional and/or attendance on specialized courses); passing a state examination to obtain a license; becoming a member of a relevant formal professional body (*albo professionale*); and for some economic activities there are also restrictions on the number of firms that are allowed to operate in a given market (e.g., pharmacies and notaries). As far as the code of conduct is concerned, the professional body generally imposes rules and restrictions on pricing, advertising and business structure and is endowed with a disciplinary power to guarantee enforcement of these rules.

[6] Raitano and Vona (2018) find a positive impact of regulation on law background returns for (incumbent) lawyers, thus highlighting one potential channel through which regulation impacts on (self-) selection across occupations.

The economic rationale for regulation lies in reducing problems associated with asymmetric information. If suppliers are heterogeneous in markets with asymmetric information, consumers might not have the ability to discern or even collect the information needed to evaluate the quality of the services they consume. As a remedy for this market failure, the regulator may decide to introduce entry barriers and other forms of regulation to guarantee a better selection of practitioners and a higher level of average quality of services and, therefore, welfare gains for consumers (Akerlof 1970; Leland 1979; Law and Kim 2005). However, regulation might also lead to negative outcomes. In particular, it might limit competition by impeding free entry into the market and reduces consumer welfare by inducing higher prices and lower supply than in a perfectly competitive equilibrium. Moreover, it might bias the allocation of resources across occupations. This is even more the case when regulation is shaped by the professional body and is designed primarily for its benefit (Stigler 1971; Pagliero 2011).[7]

Empirical evidence shows that regulation has a significant effect on the labor market outcomes of the regulated occupations. Kleiner and Krueger (2013) for the U.S. and Koumenta and Pagliero (2019) for European countries both find that licensing is associated with a significant wage premium. Reliable evidence on the impact of regulation on measures of practitioner quality is scant, partly due to identification issues and the difficulty of finding accurate measures of the service quality. The case studies examined so far do not find that regulation increase the quality of service.[8]

In Italy, professional services are historically subject to strict regulation (Pellizzari

[7]The debate between proponents and opponents of licensing dates back over centuries. Smith (1776) described the ability of the crafts to lengthen apprenticeship programs and limit the number of apprentices per master, thus ensuring higher prices and, therefore, higher earnings in these occupations. Friedman and Kuznets (1945) described occupational licensing as an institution that allows practitioners to capture monopoly rents, with some professions characterized by similarities with the medieval guilds.

[8]Kleiner (2000) exploit cross-sectional variation in licensing stringency for dentists in the U.S. and find that tougher licensing does not improve dental health (although it raises the earnings of practitioners). Using data on physicians in Israel, Kugler and Sauer (2005) investigate variation induced by a policy rule, and find that stricter licensing requirements lead to higher practitioner rents but also to a lower quality of the service. Angrist and Guryan (2008) find that state-mandated teacher testing is associated with increases in teacher wages without a corresponding increase in teaching quality (as measured by their educational background).

et al. 2011). However, since the 2000s, various reforms have been carried out in order to open professional services to competition. Two main legislative actions were taken in 2006 (so-called Bersani decree) and 2011 (so-called Monti reform). The two reforms can be regarded as a sudden and unexpected change in the Italian legislation. They were approved by two different governments, shortly after they took office, via emergency decrees. The Monti reform was notably adopted as a response to the financial and sovereign debt crisis, which urged taking swift and vigorous action. Both reforms dealt with practitioners' conduct and, to a lesser extent, with entry requirements. First, minimum prices and restrictions on advertising and inter-professional cooperation were withdrawn by the Bersani decree, which also took action on the reserves of the activities of notaries and the sale of medicinal products by pharmacies. In the second wave of reforms, regulated tariffs were completely abolished and continuing education and other conduct obligations were introduced. A cap on the duration of initial training was also fixed at up to 18 months. The number of notaries has since been increased and the demographic criteria for the establishment of pharmacies loosened.

Every five years, the OECD releases the Indicators of Product Market Regulation (the "PMR Indicators"), which measure the degree of openness to competition allowed by the regulatory environment for each country and for different economic sectors (network industries, retail trade, professional services). With respect to professional services, until 2013 regulations on architecture, engineering, legal and accounting services were taken into account.[9] By using such indicator, the OECD countries can be ranked on the basis of the restrictiveness of their legal framework with respect to competition. This indicator certifies the progressive liberalization of Italian professional services: between 2003 and 2013, Italy moved from 2nd (out of 27 OECD countries) to 19th (out of 34) position with respect to the restrictiveness of regulation in this field. Figure 3.2

[9]In the 2018 wave, the indicator was expanded and revised and, therefore, is not comparable with the previous one.

provides a clear visual evidence of these figures: Italy was one of the more regulated countries in professional services before the reforms, but it has also experienced the largest variation in the strictness of regulation during the next decade.

Figure 3.2: OECD indicator of regulation in professional services

Note: Data from OECD product market regulation website.

3.2.2 Regulation and occupational persistence

Intergenerational persistence has been documented in several occupations studied so far, although to a varying extent. This stylized fact might be attributed to a number of reasons that are difficult to isolate from each other. Parents may influence their children through the genetic transmission of characteristics, such as innate abilities and personality traits that are more valued in certain labor markets (e.g. memory, locus of control, risk aversion, confidence, etc.). Moreover, parents may subtly influence the lifetime prospects of their children through family culture and other monetary and non-monetary investments that shape skills, aptitudes, beliefs and behaviors.[10]

[10]See Mogstad (2017) for a review on the human capital approach to intergenerational mobility.

However, intergenerational occupational persistence might also be shaped by regulation. First, children of parents who are professionals might have privileged access *ex lege*. For example, in Italy entry into the pharmacies market is highly regulated (the law establishes the number of pharmacies that should operate in a city as a function of the existing population) and inheriting the family business is one of the most common ways of owning a pharmacy.

Second, compulsory practice might be easier to complete for a child whose parent will hire him/her, as he/she does not have to spend time in looking for it and can receive a financial support which is more limited for the other children. Working with a parent can also make easier to comply with the requirements that can be set during the compulsory practice, regarding the number and kind of activities that shall be carried out.

Third, having a parent already in the business might help the young practitioner to create a portfolio of clients, and this is clearly even more important when other instruments to attract potential clients (such as advertising or competitive tariffs) are constrained by regulation. In addition to this, in a context in which the form of business is restricted (i.e., only specific kinds of companies or sole practitioner are admitted), it is harder for a child of a non-professional to run its business - while he or she could be hired as professional by a company in a more liberalized context.

Fourth, parents might exploit their positional advantage (and their connections) to obtain privileged information that, in turn, might facilitate their children gaining admission to a college or passing the state exam.

Finally, and importantly, the interest in exploiting these positional rents is clearly greater when also the economic returns of the occupation (which in turn depends on the extent of regulation) are larger. Stated differently, consider two individuals who have to decide whether to run the business of their parents or opt for a different occupational choice. The only difference is that in the first case the business activity is regulated and, therefore, it has higher returns. The consequence is that, while both individuals have an equal positional advantage in inheriting the family business, the incentive to exploit this advantage is larger in the former case.

Nevertheless, stricter regulation does not necessarily imply a higher propensity of career following. For example, strict but fair entry requirements (e.g., in terms of educational requirements or characteristics of the state exam) might also increase the role of individual merit and decrease that of less fair mechanisms, such as nepotism, family networks, etc.

The relevance of each channel could clearly vary to a large extent across occupations and the characteristics of the corresponding labor market. Aina and Nicoletti (2018) show that having a liberal professional father affects the different steps required for the child to become a liberal professional to a varying extent. They show that the impact is stronger on the probability of completing a compulsory period of practice and entering a liberal profession, whereas there are no effects (after controlling for children's and parents' formal human capital) on passing the licensing examination. Raitano and Vona (2018) find that the liberalization measures in the lawyers labor market squeezed the law background returns (and interpret this as evidence of nepotism).

3.3 Data

3.3.1 Labor Force Survey

Data for the Italian labor market of occupations are drawn from the Labor Force Survey (LFS). This survey is conducted by the National Institute of Statistics (ISTAT) during every week of a year. The annual sample is composed of over 250,000 households (about 600,000 individuals). The survey represents the leading source of statistical information for estimating the main aggregates of the Italian labor market at the national and local levels.

We retrieved data since 2004 on people aged 19-25 who are recorded as children in the survey and have at least one parent whose occupation is one of the following: accountants, agronomists, architects, biologists, chemists, doctors, engineers, geologists, lawyers, notaries, pharmacists, psychologists, social assistants and veterinarians.[11] These occupations are identified on the basis of the 4-digit ISCO occupational classification. Having co-residing children and parents allows us to match each child to their parents and therefore to observe two generations.

This strategy has two main drawbacks. First, focusing on children still living with their parents might lead to a sample selection bias. However, the proportion of people aged 19-25 still living with their parents is approximately 92 percent.[12] Therefore, we argue that this issue is negligible. Second, the vast majority of children are not in the labor market yet and, therefore, we do not observe their occupation. However, we observe whether they are enrolled at a university and, if so, their degree program.

To construct our measure of intergenerational persistence, we match children's educational choice with their parents' occupation. Hence, we measure the individual propensity of children to follow their parents' occupation with an indicator that is equal to 1 if children pursue a course of study that naturally leads to the parents' occupation. We illustrate the matching between each occupation and the corresponding degree program in Table 3.1 (top panel).[13]

As we will discuss in the following section, the empirical strategy requires us to

[11] We select this subset of occupations following two main criteria. First, we select occupations that require a specific degree program, thus allowing us to build our measure of propensity of career following. This is why, for example, we exclude journalists. Second, we select only occupations above a minimum population threshold. This is why we exclude the profession of actuary, which is very rarely surveyed in the LFS.

[12] In the LFS, the sample units are "de facto" households, composed of people living together even if with no formal arrangement. Members who are temporarily absent (e.g., for job or training reasons) are still considered members of the family. The family background of youngsters who do not co-reside with their parents is not known by construction. In the majority of cases, these youngsters are already active in the labor market; conversely, the proportion of students is rather low (13 percent, which is about one-third of the corresponding figure for co-residing children).

[13] Less than 1 percent of the children in our sample already has a college degree and is employed in one of the occupations considered in our study. In this case, occupational persistence is directly measured by comparing the occupation of the parent with that of the children.

create a proper control group, i.e., children whose parents are employed in occupations similar to those of the treated ones but not exposed to entry requirements and conduct rules established by a professional body (or, at most, exposed to milder and *time-invariant* occupational licensing). These control units have been chosen using the following criteria. First, we restrict the analysis to cognitive non-routine occupations that are comparable to those in the treated group in terms of skill content. Namely, we consider the ISCO major groups 1, 2 and 3, corresponding to managers, professionals and technicians and associate professionals. Second, within these ISCO groups, we select the occupations that have a similar education careers with respect to that of the treated group. The choice of which specific occupation to consider has been dictated by the educational content of each job description (using a semantic criterion) and/or the employment opportunities associated with each degree course as described in the university guides. For example, legal experts and magistrates represent the control group for lawyers and notaries; finance technicians are the counterparts of accountants; building technicians, computer scientists or mathematicians are the control units for engineers; etc. See Table 3.1 (bottom panel) for a complete list of the control occupations and their corresponding degree program.

Table 3.2 displays the summary statistics of our main variables, i.e., the indicator of occupational persistence and the main socio-demographic variables at both individual and household level. The sample of the children in the treated group includes 28,796 children-parents pairs. The average occupational persistence is 0.18, which means nearly one-fifth of the children is enrolled in a degree program that represents a prerequisite for entry into the occupation of their parents. This figure is higher among children whose parents are self-employed professionals (0.25), while it is remarkably lower among children of parents employed in similarly skilled occupations (0.06). It is also noteworthy that 71 percent of this sample of children is enrolled at a university, a percentage much higher than that for the overall population in the same age bracket.

3.3.2 Regulation index

Since the 1990s, the OECD has been constructing a system of indicators to measure stringency and ongoing development in product market regulation across the OECD countries (Nicoletti et al. 1999; Conway and Nicoletti 2006; Koske et al. 2015).[14] The basic idea is to turn qualitative data on laws and regulation that may affect competition into quantitative indicators. The indicator covers different sectors (network industries, professional services, retail trade) and has a pyramidal structure. The index is then computed as a simple average among the various occupations covered among the professional services (i.e., legal, accounting, engineering and architecture services). For each occupation, different aspects are taken into account and noted on a 0-6 scale (from the less restrictive to the most restrictive one).[15] The answers (from 0 to 6) to the different questions are then averaged and aggregated into sub-indicators. The simple average of the sub-indicators then leads to the measure of regulation for a specific sector or occupation. As far as professional services are concerned, the indicator includes questions regarding entry (exclusive rights, educational requirements, compulsory chamber membership and quotas) and conduct regulation (prices and fees, marketing and advertising, form of business, inter-professional cooperation).

We follow this simple methodology to develop a novel measure of the stringency of regulation for a selected sample of occupations in Italy. We consider different aspects of the relevant legal framework of each occupation for the years 2003, 2008 and 2013. This allows us to cover the effects of the Bersani decree in 2006 and of the Monti reform in 2011 on the stringency of regulation of each occupation

Our indicator differs from the OECD index regarding the number of occupations covered and the content of the relevant regulation taken into account. Our contribu-

[14] These indicators have been widely used in the literature to examine, for example, the impact on growth and productivity in downstream sectors (Barone and Cingano 2011; Bourlès et al. 2013).

[15] For example, the indicators consider whether it is compulsory to be member of a professional organization in order to legally practice: if so, the question is noted ”6”; if not, it is noted ”0”.

tion is twofold. First, we enlarge the set of occupations to the following: accountants, agronomists, architects, biologists, chemists, doctors, engineers, geologists, lawyers, notaries, pharmacists, psychologists, social assistants, and veterinarians.

Second, we include a wider range of information in the construction of the indicators. Specifically, for entry regulation we consider the following five sub-topics: exclusive rights (i.e., reserves of activities), education requirements, professional exam, compulsory chamber membership and quantitative restrictions. For conduct regulation, we consider the following five sub-topics: prices and fees, advertising, form of business, inter-professional cooperation and disciplinary power. Moreover, with regard to entry, we refine the extent of exclusive rights by considering both the number and the value of reserves of activities of each occupation. We have also extended the set of information related to educational requirements, with reference to the characteristics of the university courses that grant access to the professional exams, and to the professional exams themselves (e.g., we consider whether there is limited enrollment at university and the composition of the examining board). With regard to conduct, we add more details regarding the effect of regulation on: i. quantitative restriction concerning the business activities allowed to operate in a market; ii. the possibility of advertising the business; iii. the legal form of the companies allowed to operate in a market; iv. the disciplinary powers of chambers; v. the costs related to chamber membership. Detailed information about the construction of the indicator are provided in the Appendix.[16]

Table 3.3 shows the extent of regulation for the 14 occupations and how this has varied over time. The occupations subject to more stringent regulation are those of notaries and pharmacists, while those of engineers and geologists are relatively less regulated. Moreover, the indicator has decreased over time, reflecting the liberalization effects of the Bersani decree and the Monti reform, although to a differing extent across

[16]See Figure B1 in the Appendix for a graphical illustration of the pyramidal structure of the index, and Table C1 for a complete description of the items included in the construction of the indicator.

occupations.

3.3.3 Descriptive evidence

During the recent years, Italy has experienced a significant increase in the number of workers employed in regulated occupations; moreover, wages in these occupations have also markedly decreased (Figure 3.3).[17] The increase in the supply and the decrease in rents are consistent with the liberalization process that occurred in the same temporal window.[18]

Figure 3.3: Employment and income patterns

Note: Dashed vertical lines represent the years of the Bersani decree and the Monti reform, respectively. Data from LFS and tax returns from the Italian Ministry of Economy and Finance.

These occupations are characterized by a significant intergenerational occupational persistence. We find that, on average, the probability of being enrolled in a degree program that naturally leads to the same occupation as the child's parents is nearly 8 times higher among the children of professionals compared to the rest of the population. We also find substantial heterogeneity across occupations. In particular, the odds ratios

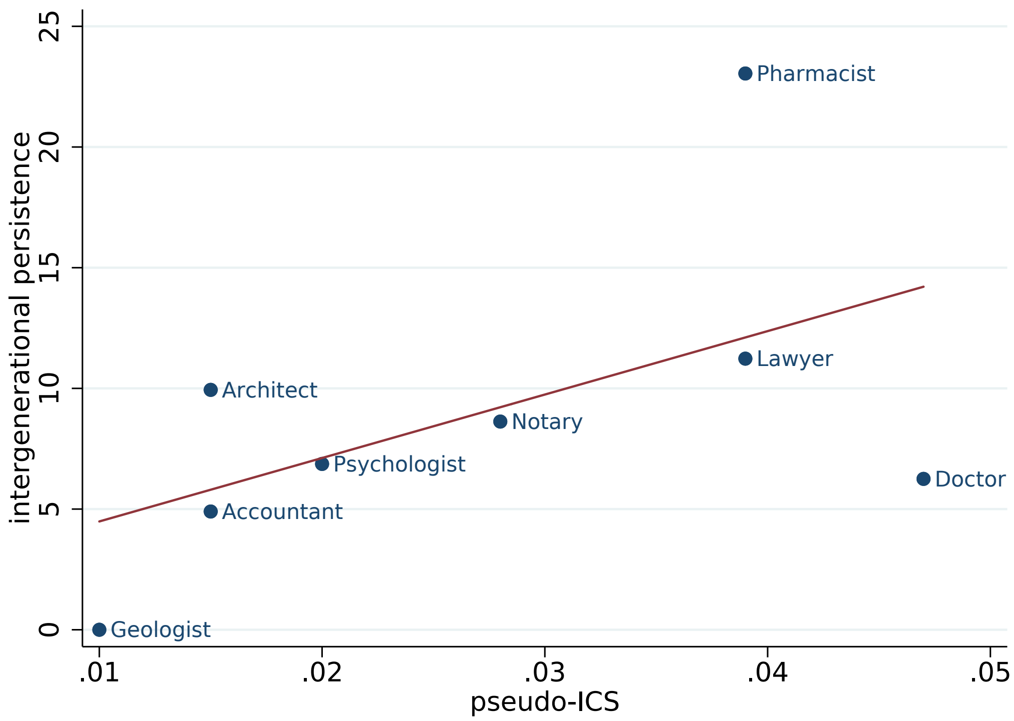
[17] See Mocetti et al. (2019) for a more detailed descriptive analysis of the labor market of regulated occupations in Italy and the economic effects of the reforms.

[18] Similar findings are obtained looking at the cross-country evidence (Figure C2). In more regulated countries the share of workers employed in professional services is significantly lower and the wage premium is significantly higher, consistent with the idea that anti-competitive regulation hampers entry in the occupations and increases economic rents.

are higher among the children of lawyers and, especially, among those of pharmacists. In contrast, career following is much lower among geologists and biologists.

In order to validate the goodness of our indicator of intergenerational persistence, we examine whether and to what extent it is correlated with other available measures. At the geographical level, occupation persistence is significantly higher in the South and, although to a lesser extent, in the Center. Similar patterns are observed by Güell et al. (2018). At the occupation level, Basso and Labartino (2011) exploit the informative content of surnames to capture the strength of family connections across professionals in Italy. Figure 3.4 shows that the two indicators are positively associated with a coefficient of correlation equal to 0.54.

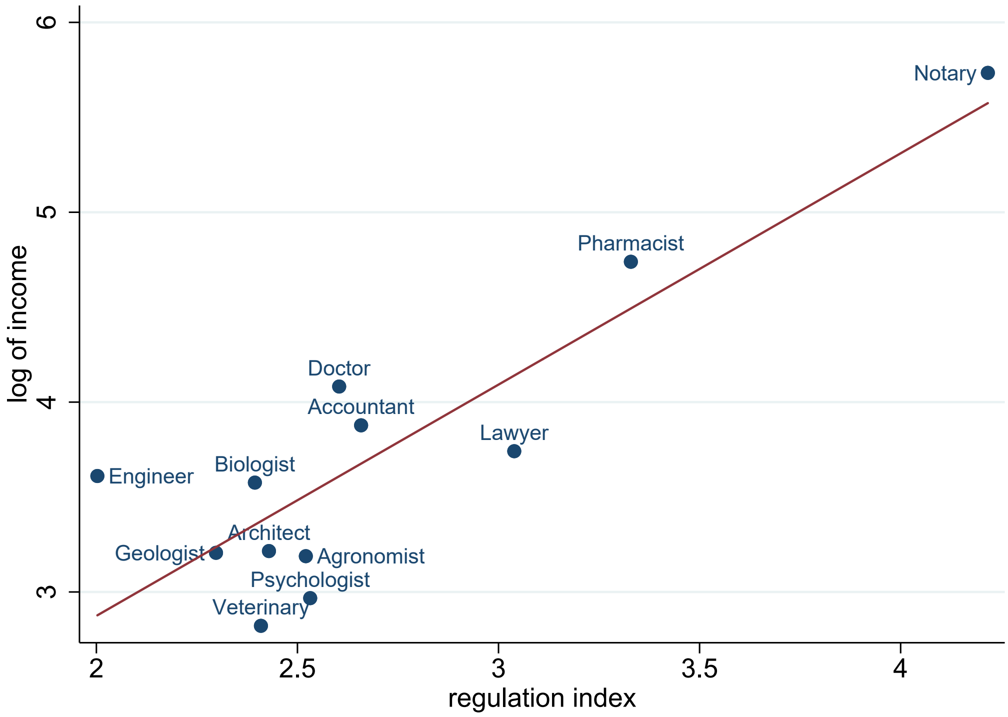
Figure 3.4: Occupational persistence across professions



Note: Intergenerational persistence is measured with the odds ratios, i.e., the probability of becoming a member of a profession if one's parent is a member of the same profession relative to the corresponding probability for the overall population. The pseudo-ICS measures the strength of intergenerational links within professions exploiting the informational contents of surnames, as computed by Basso and Labartino (2011). The intuition behind the latter measure is that if socioeconomic status is strongly transmitted then surnames should also explain a large share of its variance, i.e. the R-squared in a regression of socioeconomic status on surname dummies is increasing in the strength of the intergenerational process.

Figure 3.5 shows that there is also a strong and positive association between the extent of regulation and incomes (as declared in tax records), with a coefficient of correlation equal to 0.87. Economic returns, in particular, are significantly higher among pharmacists and notaries which are also the two most regulated occupations according to our indicator. This is somewhat reassuring since we know from previous studies that regulation does affect economic rents (Kleiner 2000; Kleiner and Krueger 2013).

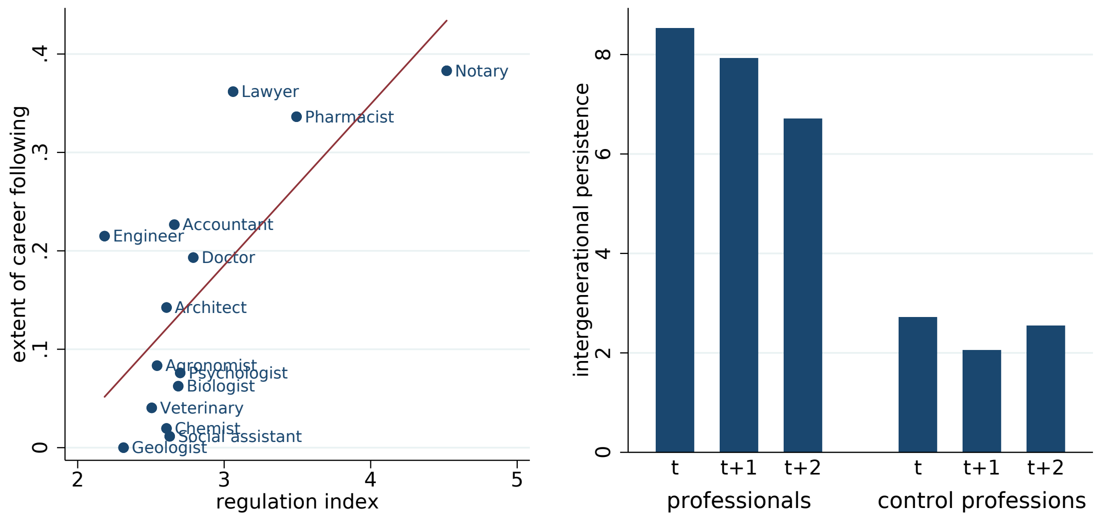
Figure 3.5: Wage premium and regulation in Italy



Note: Stringency of regulation is measured with a 0-6 OECD-style index, with higher values indicating stricter regulation; see the Appendix for more details. Income data from tax returns provided by the Italian Ministry of Economy and Finance.

Figure 3.6 provides *prima facie* positive evidence on the strong relationship between the extent of career following and the stringency of regulation across occupations (left-hand side graph) and over time (right-hand side graph). The coefficient of correlation is equal to 0.72. It is worth noting that this positive correlation is not entirely driven by pharmacists and notaries that visually appear as two outliers in the scatter plot; indeed, if we exclude these two occupations the correlation continues to be sizable (0.46). Consistent with the progressive process of liberalization of professional services, we show that intergenerational transmission of occupation has decreased over time in occupations subject to regulation, while it has remained fairly stable in control occupations. An obvious interpretation of these findings is that economic regulations provide market power to incumbents, constitute barriers to entry and ultimately generate rents. Thus, the children might benefit from their parents' positional rents and are more likely to follow in their parents' footsteps.

Figure 3.6: Occupational persistence and regulation



Note: Intergenerational persistence is measured with the odds ratio, i.e., the probability of becoming member of a profession if one's parent is member of the same profession relative to the corresponding probability for the overall population. Stringency of regulation is measured with 0-6 OECD-style index, with higher values indicating stricter regulation (see the Appendix for details on the construction of this indicator).

However, these correlations, although suggestive, may be affected by the omission of relevant variables (e.g., sector-specific factors that might affect both the degree of competition and the extent of career following) and therefore cannot be interpreted as constituting a causal nexus. In the following section, we describe how we deal with this identification challenge.

Table 3.1: Occupations and corresponding university degree

Occupation:	Degree:
a. Treated group	
Accountant	Economics
Agronomist	Agriculture and veterinary
Architect	Architecture
Biologist	Biology
Chemist	Chemistry
Doctor	Medicine
Engineer	Engineering
Geologist	Geology
Lawyer	Law
Notary	Law
Pharmacist	Pharmacy
Psychologist	Psychology
Social assistant	Social services
Veterinary	Agriculture and veterinary
b. Control group	
Financial associate professional	Economics
Manager or entrepreneur in agriculture	Agriculture and veterinary
Designer or art teacher	Architecture
Biochemical technician	Biology
Chemical technician	Chemistry
Paramedical professional	Medicine
Civil engineering technician or ICT professional	Engineering
Physic and geologic technician	Geology
Legal expert	Law
Magistrate	Law
Pharmacologist or pharmaceutical technician	Pharmacy
Personnel and staff development professional	Psychology
Primary, pre-primary and special needs teacher	Social services
Other life science technician	Agriculture and veterinary

Note: For each occupation - identified on the basis of the 4-digit ISCO classification of occupations - the table reports the corresponding university degree.

Table 3.2: LFS: descriptive statistics

	Children of:		
	Professionals	Self-employed professionals	Control professions
Occupational persistence	0.184 (0.388)	0.248 (0.432)	0.060 (0.237)
Female	0.490 (0.500)	0.488 (0.500)	0.484 (0.500)
Age	21.72 (1.977)	21.73 (1.980)	21.76 (1.981)
Number of siblings	2.027 (0.808)	2.026 (0.796)	2.009 (0.793)
Enrolled at university	0.713 (0.452)	0.720 (0.449)	0.550 (0.498)
Parents' age	54.96 (4.754)	55.50 (4.981)	52.04 (4.903)
# parent-child pairs	28,796	14,324	69,682

Note: The table shows mean values and standard deviation (in parenthesis) of the main variables. The sample refers to individuals aged 19-25 who are recorded as children in the survey (and therefore they still co-reside with their parents) and have at least one parent who is a professional or employed in a control profession (see Table 1 for the list of occupations). Data are drawn from the Italian LFS (years 2004 to 2018).

Table 3.3: Regulation index in selected occupations in Italy

	Year:		
	2003	2008	2013
Accountant	3.563	2.775	1.638
Agronomist	3.413	2.613	1.537
Architect	3.325	2.525	1.438
Biologist	3.278	2.490	1.415
Chemist	3.235	2.473	1.422
Doctor	3.473	2.573	1.766
Engineer	2.827	2.027	1.153
Geologist	3.185	2.398	1.310
Lawyer	3.735	3.048	2.335
Notary	5.013	4.026	3.613
Pharmacist	4.010	3.412	2.565
Psychologist	3.315	2.528	1.753
Social assistant	3.245	2.458	1.370
Veterinary	3.318	2.505	1.405

Note: The table shows the measures of regulation (with a 0-6 OECD-style index, with higher values indicating stricter regulation) across occupations and over time; see the Appendix for more details.

3.4 Empirical strategy

The goal of our study is to identify the effect of regulation on occupational persistence. To address this issue, we adopt a strategy that exploits the differential effect of the treatment (the changes in regulation) on occupational persistence for the treated group and a proper control group. We consider as control units the children of parents employed in highly-skilled occupations that are characterized by a similar educational career with respect to that of the treated occupations but which are not subject to the entry requirements and conduct rules established by a professional body.

We define our dependent variable as an indicator that is equal to 1 if children are enrolled at a university on a degree program that could lead them to follow their parents' career; for example, children of doctors have a high propensity to become doctors themselves if they are enrolled in a medical degree course. According to the diverse regulatory environment that may affect children's educational and occupational choices, we divide our sample of children into three cohorts depending on the year, t, in which they enrolled at the university: i. $t < 2007$ (pre-Bersani decree); ii. $2007 \leq t < 2012$ (post-Bersani decree and pre-Monti reform); iii. $t \geq 2012$ (post-Monti reform).

Then, we measure the effect of regulation on the the propensity to follow the parents' career by running regressions as the following:

$$Y_{i,p,t} = \alpha + \beta R_{p,t} + \gamma X_{i,t} + \phi_p + \delta_t + \rho_{e(p),t} \times \delta_t + e_{i,p,t}, \tag{3.1}$$

where $Y_{i,t}$ is the propensity of the child i (whose parent is employed in occupation p) in the cohort t to follow the parents' career; this variable is equal to 1 if there is career following and 0 otherwise. The main explanatory variable is $R_{p,t}$, which measures the strictness of regulation in occupation p at time t (obviously, $R_{p,t} = 0$ for the control units). The specification also includes the main socio-demographic variables, $X_{i,t}$, as controls. Crucially, we add a wide array of fixed effects in order to address the omitted variable bias. Namely, we control for occupation-fixed effects, ϕ_p, to capture any unobservable that may systematically affect intergenerational persistence in an occupation, such as the fact that in certain jobs the heritability of occupation-specific skills (in the pre-birth stage or in the family environment) might be larger[19] or that the ability of our proxy to capture career following might vary across occupations. Time fixed effects, δ_t captures common shock. Importantly, we also include degree program $\times$ time fixed effects, $\rho_{e(p)} \times \delta_t$, with the degree-program e that we use as predictor of occupation p. This last set of fixed effects is aimed at capturing the fact that enrollment on a certain degree program might vary across time due to supply factors or demand factors (e.g., an increase in the employment opportunities for the graduates in a certain degree program). Finally, $e_{i,p,t}$ is the error term. Throughout the analysis, we cluster the standard errors at the employment status-occupation group level. By allowing for an arbitrary covariance structure within groups over time, we account for the presence of common unobserved random shocks at the group level that would lead to correlation between all observations within each group.[20]

3.5 Results

3.5.1 Main results

This section sets out our main findings on the effects of regulation on intergenerational persistence among occupations.

We start with Table 3.4, which shows our baseline results. In the first column, we control for time-, profession- and region-fixed effects, thus accounting for the common

[19] Indeed, one may plausibly argue that the cost of acquiring occupation-specific human capital is lower for children who follow their father's occupation. Where the direct and indirect transmission of job-specific knowledge and abilities is more relevant, there would presumably be a higher percentage of children following their father's occupation.

[20] The choice to cluster the standard errors at the employment status-occupation group level is motivated by the fact that the effect of regulation varies both with the employment status (e.g., between self-employed and employee) and the occupation level (as regulation is occupation-specific).

trends to which children belonging to the same cohort are exposed, for occupation-specific (time-invariant) factors affecting occupation persistence and for unobserved local variables that might affect both regulation and employment opportunities. According to these estimates, a 1 point decrease in the regulation index leads to a decrease of 2.3 percentage points in the propensity of children to follow their parent's professional career (relative to the control group). In the second column, we add the main socio-demographic characteristics, both at the individual and household level. The estimated parameter is unchanged: this is not surprising as the two groups are well balanced across these characteristics. In the third column, we add degree program fixed effects to account for the fact that certain degree-programs structurally attract more students. Finally, in the fourth column (our preferred specification), we include degree program-time fixed effects to capture asymmetric shocks such as time-varying demand for certain educational profiles. The coefficient is unaffected and it remains highly significant. According to this estimate, the combined effect of the two regulatory reforms (corresponding to a 1.7 decrease in our index of the strictness of regulation) reduced the propensity of career following by about 4 percentage points, more than one-third of the sample average.[21]

Although we strictly follow the OECD methodology, turning qualitative information into quantitative evidence is still subject to a number of arbitrary choices. One important choice is the weights structure: the overall indicator is obtained as a simple average of the indicators of each sub-domain, thus implicitly assuming that, say, exclusive rights are as important as educational requirements or that limitations on prices are as important as those on the form of business. One might guess that these sub-domains are not all equally important but any different weights structure can appear

[21] Table 3.4 also contains standard errors clustered at the province level (second row) as some regulatory domains are shaped by local (provincial) professional bodies and the demand for professional services is highly heterogeneous over the territory. In this case, standard errors are slightly lower. Hereafter, standard errors clustered at the province-level are omitted as we prefer to have more conservative estimates.

Table 3.4: Impact of regulation: main results

	Dependent variable: Likelihood of occupational persistence			
	A. Measure of regulation: simple average			
Regulation index	0.023**	0.023**	0.020**	0.023**
	(0.008)	(0.008)	(0.007)	(0.010)
	(0.005)	(0.005)	(0.005)	(0.005)
	B. Measure of regulation: principal component			
Regulation index	0.023**	0.023**	0.021**	0.024**
	(0.007)	(0.007)	(0.007)	(0.009)
	(0.005)	(0.005)	(0.005)	(0.004)
Time FEs	Yes	Yes	Yes	Yes
Parent's profession FEs	Yes	Yes	Yes	Yes
Region FEs	Yes	Yes	Yes	Yes
Socio-demographic controls	No	Yes	Yes	Yes
Child's degree program FEs	No	No	Yes	Yes
Child's degree program $\times$ Time FEs	No	No	No	Yes
Observations	98,478	98,478	98,478	98,478

Note: The table shows the effect of the strictness of regulation on the likelihood of career following, i.e., an indicator equal to 1 when the children attend a degree program that naturally leads to the same occupation of their parents. The measure of regulation is obtained as a simple average across all regulation items in the top panel and with the principal component analysis in the bottom panel. The sample includes all children having at least one parent who is a professional (treated group) or employed in a similar profession (control group). Socio-demographic controls include: gender, children's age, number of siblings, birth order, gender of the parent, parents' age. Standard errors (in parentheses) are clustered at the employment status and occupation-level (first row) and at the province-level (second row).

arbitrary. Therefore, in the bottom panel of Table 3.4, we use the principal component analysis as an alternative strategy to extract information from each sub-domain. The first principal component, which we use as a synthetic measure, explains more than 50 percent of the total variance of the underlying variables. This synthetic measure has the advantage of using a different weights structure that, however, is chosen by the statistical algorithm in a transparent way.[22] The results are largely confirmed.

Our identifying variation comes from two different sources. On the one hand, we exploit variation comparing treated and control groups before and after the two re-

[22]Specifically, more weights are assigned to extent and value of exclusive rights and strictness and fairness of the professional exam among the entry requirements, and to restriction on prices and advertising among the conduct requirements.

forms. On the other hand, we have a continuous measure of the strictness of the regulation that varies over time and across (treated) occupations. In Table 3.5, we exploit separately the two sources of variation. In the first two columns, we examine the variation of the likelihood of occupation persistence between treated and control group in a standard difference-in-differences setting, with a different set of controls. According to these results, the first reform slightly decreased the likelihood of career following for the children having at least one parent who is a professional, although the effect is not significant from a statistical point of view; the second reform, in contrast, decreased the likelihood of occupation persistence by about 4 percentage points. The more significant effect of the second reform can be attributed to different factors. First, this reform shortened the period of compulsory practice, thus reducing the positional advantage of the children of professionals in a crucial step to start a liberal profession (Aina and Nicoletti 2018). Second, the same reform significantly reduced the wage premium of regulated professions (Mocetti et al. 2019), thus decreasing the incentives of the children of professionals to exploit their positional advantage. In the last two columns of Table 3.5, we exploit variation of regulation with the subset of regulated (treated) occupations. We find again that the strictness of regulation is positively associated with the extent of career following.

The credibility of a difference-in-differences strategy crucially relies on the assumption that, in the absence of the treatment, the occupational persistence for the treated and the control group would have followed parallel paths over time. In our setting, we expect that the treated and the control groups have a different level of occupational persistence (as they were exposed to different regulatory environments) and follow a similar trend over the pre-reform period. A parallel trend between treated and control groups would suggest the absence of an anticipatory effect and/or of divergent patterns between the two groups before the policy reforms were implemented. These assumptions are visually examined in Figure 3.7, which plots the difference in occu-

Table 3.5: Impact of regulation: exploiting different sources of variation

	Dependent variable: Likelihood of occupational persistence			
	Between groups		Within group	
Treated × post-2006	-0.010	-0.012		
	(0.009)	(0.010)		
Treated × post-2011	-0.041***	-0.045**		
	(0.015)	(0.019)		
Regulation index			0.101**	0.123*
			(0.049)	(0.065)
Time FEs	Yes	Yes	Yes	Yes
Parent's profession FEs	Yes	Yes	Yes	Yes
Region FEs	Yes	Yes	Yes	Yes
Full set of controls	No	Yes	No	Yes
Observations	98,478	98,478	28,796	28,796

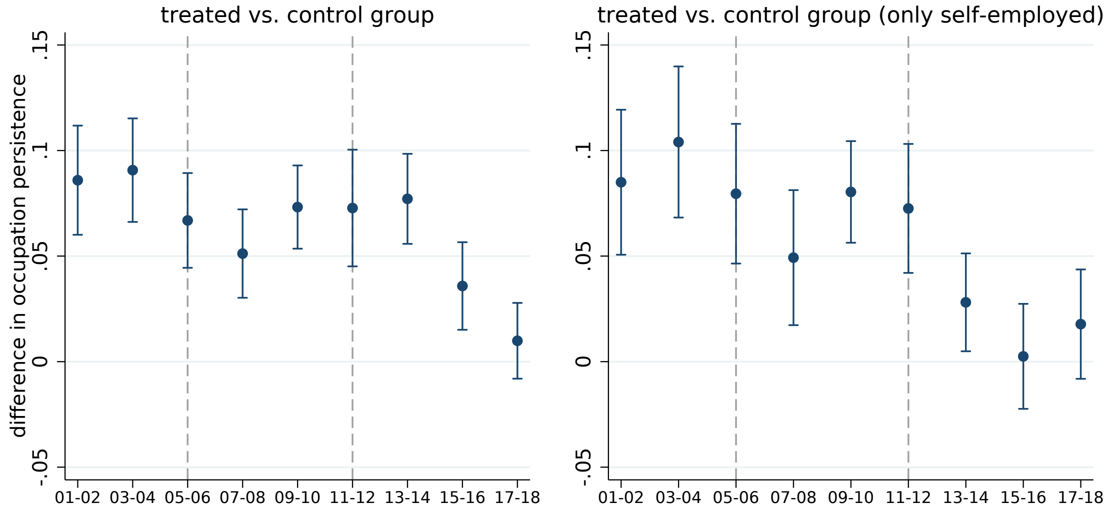
Note: The table shows the effect of regulation on the likelihood of occupational persistence exploiting two different sources of variation: i. variation across treated and control occupations over time due to the liberalization reforms; ii. variation in the intensity of the reforms (captured by the regulation index) across treated occupations over time. In the first case, we report the results of a difference-in-differences empirical strategy where the parameter of interest is obtained from an interaction term between a dummy for treatment group and dummies for the period after the 2006 and 2011 reforms, respectively. The coefficient is negative, suggesting that the reforms negatively affected intergenerational occupational persistence in treated occupations over the post-reform period compared to control group occupations. In the last two columns, instead, we expected and find a positive effect of the regulation index on the outcome, given that our regulation index takes higher values as regulation is stricter. The sample includes all children having at least one parent who is a professional (treated group) or employed in a similar profession (control group) in the first two columns and only children having at least one parent who is a professional (treated group) in the last two columns. Full set of controls include socio-demographic controls (gender, children's age, number of siblings, birth order, gender of the parent, parents' age) and child-s degree program-time fixed effects. Standard errors (in parentheses) are clustered at the employment status and occupation level.

pational persistence between the treated and the control groups for different cohorts of children. In the left panel, we consider the entire sample, while in the right panel we focus on the subset of children of self-employed individuals. In both figures, the coefficients are fairly stable over time and slightly decrease later, in particular after the second reform (consistently with the results shown in Table 3.5). Hence, the parallel trend assumption is empirically satisfied.

3.5.2 Robustness

This section tests the validity of our main findings. First, we control whether our results are robust to the inclusion of further controls (Table 3.6). If treated and control

Figure 3.7: Parallel trend assumption



Note: Each point represents the estimated difference in occupation persistence between the treated and the control group. Vertical bands represent + / - 1.96 time the standard error for each point estimate. Dashed vertical lines refer to the Bersani reform and Monti decree.

units operate in different markets, they might be exposed to distinct macro shocks. To address this issue, we enrich the specification with a set of geographical area-, sector- and firm size-time fixed effects (included separately and jointly) to account for different economic cycles and enrollment patterns along these dimensions. The results are qualitatively similar to those estimated by the baseline model, although the magnitude is somewhat lessened when we control for different trends across (parents') firms of different size.

Second, we examine whether our results hold for different specific subsamples of the population. Namely, in Table 3.7, we replicate the analysis using only children of self-employed parents (top panel), only children of parents employed in micro-firms, i.e., with less than 5 employees (middle panel) or only children enrolled at a university (bottom panel). The restriction on the children of self-employed parents is motivated by the fact that occupational persistence might differ markedly between employees and self-employed professionals. Indeed, they are differently exposed to regulation depending on their occupational status; e.g., lawyers who own their legal firm are subject to the regulation for professional services, while lawyers who are employed as, say,

Table 3.6: Impact of regulation: robustness to the inclusion of further controls

	Dependent variable: Likelihood of occupational persistence			
Regulation index	0.024***	0.024***	0.019**	0.018**
	(0.009)	(0.009)	(0.008)	(0.007)
Time FEs	Yes	Yes	Yes	Yes
Parent's profession FEs	Yes	Yes	Yes	Yes
Region FEs	Yes	Yes	Yes	Yes
Socio-demographic controls	Yes	Yes	Yes	Yes
Child's degree program FEs	Yes	Yes	Yes	Yes
Child's degree program × Time FEs	Yes	Yes	Yes	Yes
Geographical area × Time FEs	Yes	No	No	Yes
Sector of activity × Time FEs	No	Yes	No	Yes
Firm's size × Time FEs	No	No	Yes	Yes
Observations	98,478	98,478	98,478	98,478

Note: The table shows the effect of the strictness of regulation on the likelihood of career following, i.e., an indicator equal to 1 when the children attend a degree program that naturally leads to the same occupation of their parents. The sample includes all children having at least one parent who is a professional (treated group) or employed in a similar profession (control group). Socio-demographic controls include: gender, children's age, number of siblings, birth order, gender of the parent, parents' age. Standard errors (in parentheses) are clustered at the employment status and occupation level.

a legal adviser for a commercial bank are less subject to the regulation, as they work in a sector not (or, at least, less) affected by restrictions on market conduct. Therefore, we might expect that the impact of regulation on occupational persistence is stronger among the self-employed. The results confirm this expectation, while they remain highly significant from a statistical point of view. The restriction on the subsample of children of parents working in micro-firms exploits the fact that smaller firms are more likely family-managed firms where recruitment or succession decisions are taken more often within the family.[23] Larger firms, in contrast, are more likely to resort to more formal selection mechanisms. Our results show that the impact of regulation on occupation persistence is larger for smaller firms. Finally, the restriction on the subsample of children enrolled at a university might help to discriminate whether enrollment on a degree program naturally leading to the parents' occupation is mostly driven by higher enrollment *per se* or to a preference for that specific degree program among

[23]See Bennedsen et al. (2007) for an empirical analysis on the role of families in succession decisions and firm performance.

those enrolled. One could argue that parents' economic rents might have a direct effect on the probability that their children enroll at university, independently from the specific degree chosen. Indeed, our summary statistics show that enrollment is significantly higher among professionals' children. However, our results are substantially confirmed when we replicate the analysis for the subsample of enrolled children.

Table 3.7: Impact of regulation: robustness to sample selection

	Dependent variable: Likelihood of occupational persistence			
	A. Subsample of self-employed parents			
Regulation index	0.037***	0.038***	0.034***	0.043***
	(0.010)	(0.010)	(0.009)	(0.011)
Observations	26,213	26,213	26,213	26,213
	B. Subsample of parents working in micro-firms			
Regulation index	0.040***	0.040***	0.035***	0.041***
	(0.010)	(0.010)	(0.009)	(0.011)
Observations	26,955	26,955	26,955	26,955
	C. Subsample of enrolled children			
Regulation index	0.023**	0.022**	0.024**	0.024**
	(0.010)	(0.010)	(0.009)	(0.011)
Observations	58,281	58,281	58,281	58,281
Time FEs	Yes	Yes	Yes	Yes
Parent's profession FEs	Yes	Yes	Yes	Yes
Region FEs	Yes	Yes	Yes	Yes
Socio-demographic controls	No	Yes	Yes	Yes
Child's degree program FEs	No	No	Yes	Yes
Child's degree program × Time FEs	No	No	No	Yes

Note: The table shows the effect of the strictness of regulation on the likelihood of career following, i.e., an indicator equal to 1 when the children attend a degree program that naturally leads to the same occupation of their parents. The samples include: in the top, panel all children having at least one parent who is a *self-employed* professional (treated group) of *self-employed* in a similar profession (control group); in the middle panel, all children having at least one parent who is a professional (treated group) in a micro-firm (with at most 5 employees) or employed in a similar profession (control group) in a micro-firm; in the bottom panel, all children *enrolled at a university* having at least one parent who is a professional (treated group) or employed in a similar profession (control group). Socio-demographic controls include: gender, children's age, number of siblings, birth order, gender of the parent, parents' age. Standard errors (in parentheses) are clustered at the employment status and occupation level.

Finally, we replicate the regressions excluding one treated occupation (and its corresponding control occupation) at a time (Table 3.8) to examine whether the estimates are sensitive to the particular performance of a single occupation. Again, the results are qualitatively confirmed.

Table 3.8: Impact of regulation: robustness to exclusion of professions

	Dependent variable: Likelihood of occupational persistence			
Profession excluded:	β	SE	Full set of controls	# observations
Accountant	0.022**	(0.009)	Yes	89,803
Agronomist	0.023***	(0.009)	Yes	94,395
Architect	0.023**	(0.010)	Yes	93,550
Biologist	0.024***	(0.009)	Yes	97,105
Chemist	0.024***	(0.009)	Yes	96,717
Doctor	0.033***	(0.009)	Yes	66,678
Engineer	0.023**	(0.009)	Yes	87,398
Geologist	0.024***	(0.009)	Yes	98,101
Lawyer	0.020**	(0.008)	Yes	95,925
Notary	0.024***	(0.009)	Yes	98,000
Pharmacist	0.019**	(0.009)	Yes	96,548
Psychologist	0.024***	(0.009)	Yes	96,714
Social assistant	0.025***	(0.009)	Yes	71,620
Veterinary	0.026***	(0.009)	Yes	97,660

Note: The table shows the effect of the strictness of regulation on the likelihood of career following, i.e., an indicator equal to 1 when the children attend a degree program that naturally leads to the same occupation of their parents. The sample includes all children having at least one parent who is a professional (treated group) or employed in a similar profession (control group), with the exclusion in each row of the listed treated profession (and its corresponding control profession). Full set of controls includes time-, parent's profession- and region-fixed effects, socio-demographic controls (gender, children's age, number of siblings, birth order, gender of the parent, parents' age) and child's degree program-time fixed effects. Standard errors (in parentheses) are clustered at the employment status and occupation level.

3.5.3 Heterogeneous effects

In this section, we explore the heterogeneous effects of regulation, distinguishing between individual characteristics and domains of regulation.

In Table 3.9, we examine whether the impact of regulation varies on the basis of children (top panel) or occupational (bottom panel) characteristics. As far as children characteristics are concerned, we examine whether the impact varies depending on the gender of the children, the birth order and a measure of individual ability. We find that the impact is somewhat stronger for males and first born children and significantly stronger for less able children, with the latter identified as those who obtain their highest school qualification with (at least) a year's delay. Although partial and rough, this is an objective and comparable measure of the practitioner's quality across occupations. This suggests that the less able individuals are those who benefit most from positional rents induced by regulation of entry into the occupation. More generally, this result suggests that anti-competitive regulations bias the allocation of individuals across occupations, favoring family background over individual merit.

Table 3.9: Impact of regulation: heterogeneous effects

	Dependent variable: Likelihood of occupational persistence					
	A. by children's characteristics					
Regulation index	0.028**	0.023***	0.026**	0.019**	0.047***	0.022**
	(0.011)	(0.008)	(0.011)	(0.008)	(0.013)	(0.009)
Subsample:	*Male*	*Female*	*First born*	*Later born*	*Less able*	*More able*
Observations	50,663	47,815	65,679	32,799	13,729	84,749
	B. by occupation's characteristics					
Regulation index	0.028**	0.014*	0.038***	-0.001	0.020*	0.028***
	(0.011)	(0.007)	(0.009)	(0.009)	(0.011)	(0.009)
Subsample:	*Soft sciences*	*Hard sciences*	*Private services*	*Public services*	*Low local demand*	*High local demand*
Observations	50,157	48,321	34,771	63,707	49,104	49,374
Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table shows the effect of the strictness of regulation on the likelihood of career following, i.e., an indicator equal to 1 when the children attend a degree program that naturally leads to the same occupation of their parents, for different subsamples of the population as indicated in each column. The sample includes all children having at least one parent who is a professional (treated group) or employed in a similar profession (control group). Full set of controls includes time-, parent's profession- and region-fixed effects, socio-demographic controls (gender, children's age, number of siblings, birth order, gender of the parent, parents' age) and child-s degree program-time fixed effects. Standard errors (in parentheses) are clustered at the employment status and occupation level.

As occupational characteristics, we first distinguish two groups depending on whether they refer to social sciences (e.g., economics, law, etc.) or hard sciences (e.g., engineering, medicine, natural sciences, etc.). We find that the impact of regulation on intergenerational occupational persistence is higher for social sciences. This finding might be due to the fact that entry into these occupations is based on more subjective evaluation and/or to the fact that the output of these services is more difficult to evaluate in a comparative manner. Both factors might increase the positional rents generated by regulation. The distinction between social and hard sciences also reflects the distinction between occupations which require a training period prior to the professional exam (e.g. lawyer, accountant, etc.) and those that do not (e.g. engineer, doctor, etc.). Consistent with Aina and Nicoletti (2018) - who find that having a liberal professional father has a significant effect on the probability to complete a compulsory period of practice - such a requirement may explain the differing impact of regulation on these two groups.

Second, we examine whether the results vary between private services and public services (i.e., public administration, health, education) as they might be exposed to different entry and demand conditions. We find that the impact is largely concentrated in the private services where restrictions on market conduct might be more effective. The differing effect might also be explained by the higher proportion of self-employed professionals (that, in turn, are more exposed to regulation) working in private services compared to public services.

Finally, we examine whether the impact of regulation is different across areas characterized by a differing demand for professional services. The underlying idea is that, although regulation of professional services is homogeneous over the territory - and geographical mobility is historically low (Faini et al. 1997) and professionals are largely *local* (Michelacci and Silva 2007) -, heterogeneity in the demand for these services might increase rents at the local level. In other words, supply constraints are more binding (and rents higher) where the demand is higher. We build a measure at the province level capturing dependence of the economy on professional services. This measure is computed in two steps. First, using the input-output matrix, we compute the dependence on professional services for each sector of economic activity. Second, we translate these figures at the province level using the sector composition of the local economy (i.e., the distribution of employees across sectors at the province level as recorded by the 2001 Census).[24] Interestingly, we find that the impact of regulation on occupational persistence is stronger in the provinces where the demand for professional services is higher (i.e., where the measure of dependence is above the median).

In Table 3.10, we distinguish the effect by domains of regulation (and occupational characteristics). Namely, we replicate the results of Table 3.9 (bottom panel) using the index of regulation for entry and conduct rules separately. According to these findings, the impact is mostly driven by conduct regulation, although this result should be interpreted with caution due to the small variability in entry regulation observed in the reference period. Moreover, as expected, restrictions on market conduct are more effective in the private services and in areas with higher demand for professional services. Interestingly, strictness of entry regulation seems to favor intergenerational mobility in hard sciences.

Table 3.10: Impact of different types of regulation

	Dependent variable: Likelihood of occupational persistence					
Entry requirements	0.330	-0.890***	0.053	-0.460*	0.083	-0.080
	(0.138)	(0.326)	(0.167)	(0.239)	(0.198)	(0.339)
Conduct rules	0.016**	0.006	0.021**	-0.002	0.004	0.014**
	(0.006)	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)
Subsample:	*Soft sciences*	*Hard sciences*	*Private services*	*Public services*	*Low local demand*	*High local demand*
Observations	50,157	48,321	34,771	63,707	49,104	49,374
Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table shows the effect of the strictness of regulation (distinguishing between entry requirements and conduct rules) on the likelihood of career following, i.e., an indicator equal to 1 when the children attend a degree program that naturally leads to the same occupation of their parents, for different subsamples of the population as indicated in each column. The sample includes all children having at least one parent who is a professional (treated group) or employed in a similar profession (control group). Full set of controls includes time-, parent's profession- and region-fixed effects, sociodemographic controls (gender, children's age, number of siblings, birth order, gender of the parent, parents' age) and child's degree program-time fixed effects. Standard errors (in parentheses) are clustered at the employment status and occupation level.

[24] According to our analysis, the financial sectors and manufacturing activities with a higher technology content are more dependent on professional services; in contrast, agriculture and services to households essentially do not involve demand for professional services. Therefore, we expect that the demand for these services is heterogeneous across provinces depending on the sector composition of the local economy.

3.6 Conclusion

Does regulation affect the allocation of individuals across occupations? Does regulation affect intergenerational persistence in certain (high-income) occupations? Answering these questions could contribute to two strands of literature. The first examines the factors behind intergenerational persistence of earnings and occupations, where the role of regulation has been substantially under-investigated. The second examines the effects of regulation, where evidence on the characteristics of the practitioners who enter the profession (and, more generally, on allocative efficiency) is scant.

To answer these research questions, we exploited two main reforms in the regulation of professional services that were introduced in Italy since the 2000s. Italy was one of the more regulated economies in these sectors until the first half of the 2000s, while the combined effect of the two reforms led to a significant liberalization in the next decade.

We find that the progress toward a more market-friendly regulatory environment leads to a substantial decrease in the propensity for career following. These results suggest that intergenerational persistence in certain occupations depends to a large extent on the existence of positional rents generated by lack of competition. In other words, our findings suggest that regulation significantly biases the allocation of individuals across occupations based on the parental occupation. The impact is stronger for professions in social sciences and in areas where the demand for professional services is higher. Moreover, the impact of regulation on occupational persistence is stronger for less able children, thus confirming allocative inefficiencies in the distribution of talents across occupations.

Conclusion

This dissertation focuses on three main public policy reforms that took place in Italy around the turn of this century. First, I study the mobility response to larger geographical dispersion in tax rates on personal income. Using data from the universe of tax residence's transfers, I implement several empirical approaches, resting on tax variation both across locations over time and across income bracket within a location. I provide evidence that local income taxes distort the location of the tax base. This result has important implications for policy-makers, encompassing the resulting changes in expected tax revenue and the level and location of local economic activity.

The second chapter focuses on the local public finance effects of curbing tax evasion. Focusing on an anti-tax evasion program implemented in Italy that detected more than 2 millions of buildings hidden to tax authorities, I provide evidence of a stable and persistent increase in municipalities' tax revenue. Moreover, I show that decisions regarding how to finance local governments are a key feature for ensuring a more efficient allocation of public spending. Finally, I provide evidence that stricter tax enforcement leads to larger property tax rates and to an increase in the progressivity of the local income tax schedule.

The third chapter studies whether regulation of professional services affects intergenerational persistence of occupations. To answer this question, we exploit two reforms that were introduced in Italy since the 2000s and differently affected occupational groups. Italy was one of the more regulated economies in these sectors until the first half of the 2000s, while the combined effect of the two reforms led to a significant liberalization in the next decade. We find that the progress toward a more market-friendly regulatory environment leads to a substantial decrease in the propensity for career following. These results suggest that intergenerational persistence in certain occupations depends to a large extent on the existence of positional rents generated by lack of competition. In other words, our findings suggest that regulation significantly biases the allocation of individuals across occupations based on the parental occupation.

Bibliography

Abadie, A., Diamond, A., and Hainmueller, J. (2010). Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association*, 105(490):493–505. 42

Abounoori, E. and McCloughan, P. (2003). A Simple Way to Calculate the Gini Coefficient for Grouped as well as Ungrouped Data. *Applied Economics Letters*, 10(8):505–509. 104

Agenzia delle Entrate (2012). Regolarizzazione degli Immobili Fantasma. *Comunicato Stampa*. 74

Agrawal, D. R. and Foremny, D. (2019). Relocation of the Rich: Migration in Response to Top Tax Rate Changes from Spanish Reforms. *The Review of Economics and Statistics*, 101(2):214–232. 4, 16, 46, 60

Agrawal, D. R. and Hoyt, W. H. (2018). Commuting and Taxes: Theory, Empirics and Welfare Implications. *The Economic Journal*, 128(616):2969–3007. 51

Aina, C. and Nicoletti, C. (2018). The intergenerational transmission of liberal professions. *Labour Economics*, 51:108–120. 108, 117, 132, 139

Akcigit, U., Baslandze, S., and Stantcheva, S. (2016). Taxation and the International Mobility of Inventors. *American Economic Review*, 106(10):2930–2981. 16, 33, 47

Akcigit, U., Grigsby, J., Nicholas, T., and Stantcheva, S. (2018). Taxation and Innovation in the 20th Century. *SSRN Electronic Journal*. 17

Akerlof, G. A. (1970). The Market for "Lemons": Quality Uncertainty and the Market Mechanism. *The Quarterly Journal of Economics*, 84(3):488. 113

Alm, J. (2012). Measuring, Explaining, and Controlling Tax Evasion: Lessons from Theory, Experiments, and Field Studies. *International Tax and Public Finance*, 19(1):54–77. 69

Almond, G. and Verba, S. (1963). *The Civic Culture: Political Attitudes and Democracy in Five Nations*. New York: Sage Publications. 95

Almunia, M. and Lopez-Rodriguez, D. (2018). Under the Radar: The Effects of Monitoring Firms on Tax Compliance. *American Economic Journal: Economic Policy*, 10(1):1–38. 69

Andreoni, J., Erard, B., and Feinstein, J. (1998). Tax Compliance. *Journal of Economic Literature*, 36(2):818–860. 69

Angrist, J. D. and Guryan, J. (2008). Does Teacher Testing Raise Teacher Quality? Evidence from State Certification Requirements. *Economics of Education Review*, 27(5):483–503. 113

Angrist, J. D. and Pischke, J.-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press, Princeton, NJ. 31, 84, 91, 183

Bakija, J. and Slemrod, J. (2004). Do the Rich Flee from High State Taxes? Evidence from Federal Estate Tax Returns. *NBER Working Paper No. 8158*. 16

Bandiera, O., Prat, A., and Valletti, T. (2009). Active and Passive Waste in Government Spending: Evidence from a Policy Experiment. *American Economic Review*, 99(4):1278–1308. 81

Barone, G. and Cingano, F. (2011). Service Regulation and Growth: Evidence from OECD Countries. *The Economic Journal*, 121(555):931–957. 120

Basso, G. and Labartino, G. (2011). Family Connections in Accessing Licensed Occupations in Italy. Technical report, Fondazione Roldofo Debenedetti. 123

Bennedsen, M., Nielsen, K. M., Perez-Gonzalez, F., and Wolfenzon, D. (2007). Inside the Family Firm: The Role of Families in Succession Decisions and Performance. *The Quarterly Journal of Economics*, 122(2):647–691. 135

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How Much Should We Trust Differences-In-Differences Estimates? *The Quarterly Journal of Economics*, 119(1):249–275. 36

Besley, T. (2006). *Principled Agents? The Political Economy of Good Government*. Oxford University Press, Oxford. 67, 76

Besley, T. and Persson, T. (2009). The Origins of State Capacity: Property Rights, Taxation, and Politics. *American Economic Review*, 99(4):1218–1244. 5, 63

Besley, T. and Persson, T. (2013). Taxation and Development. In Auerbach, A. J., Chetty, R., Feldstein, M., and Saez, E., editors, *Handbook of Public Economics*, pages 51–110. Amsterdam: Elsevier B. V. 5, 63

Besley, T. and Smart, M. (2007). Fiscal Restraints and Voter Welfare. *Journal of Public Economics*, 91(3-4):755–773. 67, 76

Björklund, A., Lindahl, M., and Plug, E. (2006). The Origins of Intergenerational Associations: Lessons from Swedish Adoption Data. *Quarterly Journal of Economics*, 121(3):999–1028. 108

Black, S. E. and Devereux, P. J. (2011). Recent Developments in Intergenerational Mobility. In Ashenfelter, O. and Card, D., editors, *Handbook of Labor Economics*, pages 1487–1541. North Holland. 108, 110

Boeri, T., Ichino, A., Moretti, E., and Posch, J. (2019). Wages Equalization and Regional Misallocation: Evidence from Italian and German Provinces. *NBER Working Paper No. 25612*. 21

Bordignon, M., Grembi, V., and Piazza, S. (2017). Who Do you Blame in Local Finance? An Analysis of Municipal Financing in Italy. *European Journal of Political Economy*, 49:146–163. 71

Borge, L.-E., Parmer, P., and Torvik, R. (2015). Local Natural Resource Curse? *Journal of Public Economics*, 131:101–114. 70

Bourlès, R., Cette, G., Lopez, J., Mairesse, J., and Nicoletti, G. (2013). Do Product Market Regulations In Upstream Sectors Curb Productivity Growth? Panel Data Evidence For Oecd Countries. *Review of Economics and Statistics*, 95(5):1750–1768. 120

Bowles, S., Gintis, H., and Osborne Groves, M. (2005). Unequal Chances: Family Background and Economic Success. Princeton, N.J.: Princeton University Press. 108

Brollo, F., Nannicini, T., Perotti, R., and Tabellini, G. (2013). The Political Resource Curse. *American Economic Review*, 103(5):1759–1796. 5, 63, 70

Brollo, F. and Troiano, U. (2016). What Happens when a Woman Wins an Election? Evidence from Close Races in Brazil. *Journal of Development Economics*, 122:28–45. 101

Brülhart, M., Bucovetsky, S., and Schmidheiny, K. (2015). Taxes in Cities: Interdependence, Asymmetry and Agglomeration. In Duranton, G., Henderson, J. V., and Strange, J. C., editors, *Handbook of Regional and Urban Economics*, pages 1123–1196. 17

Brülhart, M., Danton, J., Parchet, R., and Schläpfer, J. (2020). Who Bears the Burden of Local Taxes? *Unpublished manuscript*. 17

Buchanan, J. M. (1967). *Public Finance in Democratic Process*. University of North Carolina Press, Chapel Hill. 67

Buchanan, J. M. and Wagner, R. E. (1977). *Democracy in Deficit: The Political Legacy of Lord Keynes*. Academic Press, New York. 67

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, 82(6):2295–2326. 40, 86, 92, 93, 94, 189, 190, 191, 193, 203

Cameron, A. C., Gelbach, J. B., and Miller, D. L. (2011). Robust Inference With Multiway Clustering. *Journal of Business & Economic Statistics*, 29(2):238–249. 37

Carfora, A., Pansini, R. V., and Pisani, S. (2018). Regional Tax Evasion and Audit Enforcement. *Regional Studies*, 52(3):362–373. 197

Carrillo, P., Pomeranz, D., and Singhal, M. (2017). Dodging the Taxman: Firm Misreporting and Limits to Tax Enforcement. *American Economic Journal: Applied Economics*, 9(2):144–164. 5, 63, 69, 77

Casaburi, L. and Troiano, U. (2016). Ghost-House Busters: The Electoral Response to a Large Anti–Tax Evasion Program. *The Quarterly Journal of Economics*, 131(1):273–314. 5, 63, 68, 74, 79, 85

Caselli, F. and Michaels, G. (2013). Do Oil Windfalls Improve Living Standards? Evidence from Brazil. *American Economic Journal: Applied Economics*, 5(1):208–238. 5, 63, 70

Chetty, R. (2009). Is the Taxable Income Elasticity Sufficient to Calculate Deadweight

Loss? The Implications of Evasion and Avoidance. *American Economic Journal: Economic Policy*, 1(2):31–52. 14, 49, 58

Chetty, R., Friedman, J. N., and Saez, E. (2013). Using Differences in Knowledge Across Neighborhoods to Uncover the Impacts of the EITC on Earnings. *American Economic Review*, 103(7):2683–2721. 69

Conway, P. and Nicoletti, G. (2006). Product Market Regulation in the Non-Manufacturing Sectors of OECD Countries. Technical report, OECD Working Paper No. 530. 120, 221

Corak, M. (2013). Income Inequality, Equality of Opportunity, and Intergenerational Mobility. *Journal of Economic Perspectives*, 27(3):79–102. 108

Cremer, H. and Gahvari, F. (1993). Tax Evasion and Optimal Commodity Taxation. *Journal of Public Economics*, 50:261–275. 77

Das-Gupta, A. and Mookherjee, D. (1998). *Incentives and Institutional Reform in Tax Enforcement: An Analysis of Development Country Experience*. Oxford University Press, New York. 70

de Chaisemartin, C. and D'Haultfoeuille, X. (2020). Two-way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review, forthcoming*. 30

Diamond, P. and Saez, E. (2011). The Case for a Progressive Tax: From Basic Research to Policy Recommendation. *Journal of Economic Perspectives*, 25(4):165–190. 55

Driscoll, J. C. and Kraay, A. C. (1998). Consistent Covariance Matrix Estimation with Spatially Dependent Panel Data. *Review of Economics and Statistics*, 80(4):549–560. 37

Dube, A., Lester, T. W., and Reich, M. (2010). Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties. *Review of Economics and Statistics*, 92(4):945–964. 38, 39

Durante, R., Labartino, G., and Perotti, R. (2011). Academic Dynasties: Decentralization and Familism in the Italian Academia. Technical report, National Bureau of Economic Research, Cambridge, MA. 108

Edlund, L., Haider, L., and Pande, R. (2005). Unmarried Parenthood and Redistributive Politics. *Journal of the European Economic Association*, 3(1):95–119. 101

Edlund, L. and Pande, R. (2002). Why Have Women Become Left-Wing? The Political Gender Gap and the Decline in Marriage. *The Quarterly Journal of Economics*, 117(3):917–961. 101

Eissa, N. (1995). Taxation and Labor Supply of Married Women: The Tax Reform Act of 1986 as a Natural Experiment. Technical report, National Bureau of Economic Research, Cambridge, MA. 33

Eissa, N. and Hoynes, H. W. (2006). Behavioral Responses to Taxes: Lessons from the EITC and Labor Supply. *Tax Policy and the Economy*, 20:73–110. 33

Epple, D. and Romer, T. (1991). Mobility and Redistribution. *Journal of Political Economy*, 99(4):828–858. 4, 15, 60

Faini, R., Galli, G., Gennari, P., and Rossi, F. (1997). An empirical puzzle: Falling migration and growing unemployment differentials among Italian regions. *European Economic Review*, 41(3-5):571–579. 139

Fajgelbaum, P. D., Morales, E., Serrato, J. C. S., and Zidar, O. (2019). State Taxes and Spatial Misallocation. *The Review of Economic Studies*, 86(1):333–376. 17

Feldstein, M. and Wrobel, M. V. (1998). Can State Taxes Redistribute Income? *Journal of Public Economics*, 68(3):369–396. 4, 16, 17, 60

Ferraz, C. and Finan, F. (2008). Exposing Corrupt Politicians: The Effects of Brazil's

Publicly Released Audits on Electoral Outcomes. *Quarterly Journal of Economics*, 123(2):703–745. 100

Friedman, M. and Kuznets, S. (1945). *Income from Independent Professional Practice*. National Bureau of Economic Research, New York. 113

Fuest, C., Peichl, A., and Siegloch, S. (2018). Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany. *American Economic Review*, 108(2):393–418. 16

Gadenne, L. (2017). Tax Me, but Spend Wisely? Sources of Public Finance and Government Accountability. *American Economic Journal: Applied Economics*, 9(1):274–314. 70, 99, 100

Galbiati, R. and Zanella, G. (2012). The Tax Evasion Social Multiplier: Evidence from Italy. *Journal of Public Economics*, 96(5-6):485–494. 197

Gelman, A. and Imbens, G. (2019). Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs. *Journal of Business & Economic Statistics*, 37(3):447–456. 40, 86

Giroud, X. and Rauh, J. (2019). State Taxation and the Reallocation of Business Activity: Evidence from Establishment-Level Data. *Journal of Political Economy*, 127(3):1262–1316. 16

Glaeser, E. L. (2013). Urban Public Finance. In Auerbach, A. J., Chetty, R., Feldstein, M., and Saez, E., editors, *Handbook of Public Economics*, pages 195–256. Elvesier, North Holland. 17

Goolsbee, A. (2000). What Happens When You Tax the Rich? Evidence from Executive Compensation. *Journal of Political Economy*, 108(2):352–378. 55

Grembi, V., Nannicini, T., and Troiano, U. (2016). Do Fiscal Rules Matter? *American Economic Journal: Applied Economics*, 8(3):1–30. 65, 72, 85

Güell, M., Pellizzari, M., Pica, G., and Rodríguez Mora, J. V. (2018). Correlating Social Mobility and Economic Outcomes. *The Economic Journal*, 128(612):F353–F403. 123

Gupta, S., Keen, M., Shah, A., and Verdier, G. (2017). *Digital Revolutions in Public Finance*. International Monetary Fund, Washington, DC. 69

Haughwout, A., Inman, R., Craig, S., and Luce, T. (2004). Local Revenue Hills: Evidence from Four U.S. Cities. *Review of Economics and Statistics*, 86(2):570–585. 17

Hines, J. R. (1996). Altered States: Taxes and the Location of Foreign Direct Investment in America. *American Economic Review*, 86(5):1076–1094. 17

Holmes, T. J. (1998). The Effect of State Policies on the Location of Manufacturing: Evidence from State Borders. *Journal of Political Economy*, 106(4):667–705. 38

Imbens, G. W. and Lemieux, T. (2008). Regression Discontinuity Designs: A Guide to Practice. *Journal of Econometrics*, 142(2):615–635. 40, 86

Jensen, A. (2019). Employment Structure and the Rise of the Modern Tax System. *Working Paper, Harvard University*. 71

Kahn, C. M., Silva, E. C. D., and Ziliak, J. P. (2001). Performance-based Wages in Tax Collection: The Brazilian Tax Collection Reform and its Effects. *The Economic Journal*, 111(468):188–205. 70

Kanbur, R. and Keen, M. (1993). Jeux Sans Frontieres: Tax Competition and Tax Coordination when Countries Differ in Size. *American Economic Review*, 83(4):877–92. 13, 36, 47

Keen, M. and Slemrod, J. (2017). Optimal Tax Administration. *Journal of Public Economics*, 152:133–142. 5, 63, 68, 71, 77

Kennan, J. and Walker, J. R. (2011). The Effect of Expected Income on Individual Migration Decisions. *Econometrica*, 79(1):211–251. 43

Khan, A. Q., Khwaja, A. I., and Olken, B. A. (2016). Tax Farming Redux: Experimental Evidence on Performance Pay for Tax Collectors. *The Quarterly Journal of Economics*, 131(1):219–271. 70

Kleiner, M. M. (2000). Occupational Licensing. *Journal of Economic Perspectives*, 14(4):189–202. 108, 110, 113, 124

Kleiner, M. M. and Krueger, A. B. (2013). Analyzing the Extent and Influence of Occupational Licensing on the Labor Market. *Journal of Labor Economics*, 31(S1):S173–S202. 108, 113, 124

Kleven, H. J., Knudsen, M., Kreiner, C., Pedersen, S., and Saez, E. (2011). Unwilling or Unable to Cheat? Evidence from a Randomized Tax Audit Experiment in Denmark. *Econometrica*, 79:651–692. 69

Kleven, H. J., Landais, C., Muñoz, M., and Stantcheva, S. (2020). Taxation and Migration: Evidence and Policy Implications. *Journal of Economic Perspectives*, 34(2):119–142. 4, 9, 16, 59

Kleven, H. J., Landais, C., and Saez, E. (2013). Taxation and International Migration of Superstars: Evidence from the European Football Market. *American Economic Review*, 103(5):1892–1924. 16, 27, 47, 50

Kleven, H. J., Landais, C., Saez, E., and Schultz, E. (2014). Migration and Wage Effects of Taxing Top Earners: Evidence from the Foreigners' Tax Scheme in Denmark. *The Quarterly Journal of Economics*, 129(1):333–378. 16

Kleven, H. J. and Schultz, E. A. (2014). Estimating Taxable Income Responses Using Danish Tax Reforms. *American Economic Journal: Economic Policy*, 6(4):271–301. 33

Kopczuk, W. (2005). Tax Bases, Tax Rates and the Elasticity of Reported Income. *Journal of Public Economics*, 89(11-12):2093–2119. 69, 77

Koske, I., Wanner, I., Bitetti, R., and Barbiero, O. (2015). The 2013 Update of the OECD Product Market Regulation Indicators: Policy Insights for OECD and non-OECD Countries. Technical report, OECD Working Paper No. 1200. 120, 221

Koumenta, M. and Pagliero, M. (2019). Occupational Regulation in the European Union: Coverage and Wage Effects. *British Journal of Industrial Relations*, 57(4):818–849. 110, 113

Kugler, A. D. and Sauer, R. M. (2005). Doctors without Borders? Relicensing Requirements and Negative Selection in the Market for Physicians. *Journal of Labor Economics*, 23(3):437–465. 113

Kumler, T., Verhoogen, E., and Frías, J. (2020). Enlisting Employees in Improving Payroll-Tax Compliance: Evidence from Mexico. *The Review of Economics and Statistics*, forthcomin:1–45. 69

Laband, D. N. and Lentz, B. F. (1992). Self-Recruitment in the Legal Profession. *Journal of Labor Economics*, 10(2):182–201. 108

Law, M. T. and Kim, S. (2005). Specialization and Regulation: The Rise of Professionals and the Emergence of Occupational Licensing Regulation. *The Journal of Economic History*, 65(03). 113

Lehmann, E., Simula, L., and Trannoy, A. (2014). Tax Me if You Can! Optimal Nonlinear Income Tax Between Competing Governments. *The Quarterly Journal of Economics*, 129(4):1995–2030. 4, 9

Leland, H. E. (1979). Quacks, Lemons, and Licensing: A Theory of Minimum Quality Standards. *Journal of Political Economy*, 87(6):1328–1346. 113

Lentz, B. F. and Laband, D. N. (1989). Why So Many Children of Doctors Become Doctors: Nepotism vs. Human Capital Transfers. *The Journal of Human Resources*, 24(3):396. 108

Lindert, P. H. (2004). *Growing Public*. Cambridge University Press, Cambridge. 5, 63

Lindquist, M. J., Sol, J., and Van Praag, M. (2015). Why Do Entrepreneurial Parents Have Entrepreneurial Children? *Journal of Labor Economics*, 33(2):269–296. 110

Lipset, S. M. (1960). *Political Man*. London: Heinemann. 95

Manning, A. (2003). *Monopsony in Motion: Imperfect Competition in Labor Markets*. Princeton University Press. 54

Marattin, L., Nannicini, T., and Porcelli, F. (2020). Revenue vs Expenditure Based Fiscal Consolidation: The Pass-Through from Federal Cuts to Local Taxes. *Working paper, IGIER, Bocconi University*. 72

Marino, M. R. and Zizza, R. (2012). Personal Income Tax Evasion in Italy: An Estimate by Taxpayer Type. In *Tax Evasion and the Shadow Economy*. Edward Elgar Publishing. 32

Martinez, I. Z. (2017). Beggar-Thy-Neighbour Tax Cuts: Mobility after a Local Income and Wealth Tax Reform in Switzerland. *SSRN Electronic Journal*. 16, 46

Martinez, L. (2019). Sources of Revenue and Government Performance: Evidence from Colombia. *Working paper, University of Chicago*. 70, 99

McCrary, J. (2008). Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test. *Journal of Econometrics*, 142(2):698–714. 87

Messina, G. and Savegnano, M. (2014). A prova di acronimo: i tributi locali sulla casa in Italia. *Banca d'Italia, Working paper No. 250*. 82

Michelacci, C. and Silva, O. (2007). Why So Many Local Entrepreneurs? *Review of Economics and Statistics*, 89(4):615–633. 139

Milanovic, B. (1994). The Gini-type Functions: An Alternative Derivations. *Bulletin of Economic Research*, 46(1):81–90. 104

Milligan, K. and Smart, M. (2019). An Estimable Model of Income Redistribution in a Federation: Musgrave Meets Oates. *American Economic Journal: Economic Policy*, 11(1):406–434. 17

Mirrlees, J. (1982). Migration and Optimal Income Taxes. *Journal of Public Economics*, 18(3):319–341. 3, 9

Mocetti, S. (2016). Dynasties in Professions and the Role of Rents and Regulation: Evidence from Italian Pharmacies. *Journal of Public Economics*, 133:1–10. 108, 111

Mocetti, S., Rizzica, L., and Roma, G. (2019). Regulated Occupations in Italy: Extent and Labor Market Effects. *Banca d'Italia, Working paper*. 110, 122, 132

Mogstad, M. (2017). The Human Capital Approach to Intergenerational Mobility. *Journal of Political Economy*, 125(6):1862–1868. 115

Moretti, E. and Wilson, D. J. (2014). State Incentives for Innovation, Star Scientists and Jobs: Evidence from Biotech. *Journal of Urban Economics*, 79:20–38. 17

Moretti, E. and Wilson, D. J. (2017). The Effect of State Taxes on the Geographical Location of Top Earners: Evidence from Star Scientists. *American Economic Review*, 107(7):1858–1903. 11, 16, 43, 44

Moulton, B. R. (1990). An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables on Micro Units. *The Review of Economics and Statistics*, 72(2):334. 37

Musgrave, R. A. (1959). The Theory of Public Finance: A Study in Public Economy. London: McGraw-Hill. 17

Musgrave, R. A. and Thin, T. (1948). Income Tax Progression, 1929-48. *Journal of Political Economy*, 56(6):498–514. 180

Myles, G. D. (2000). Wasteful Government, Tax Evasion, and the Provision of Public Goods. *European Journal of Political Economy*, 16(1):51–74. 5, 63

Naritomi, J. (2019). Consumers as Tax Auditors. *American Economic Review*, 109(9):3031–3072. 69

Nicoletti, G., Scarpetta, S., and Boylaud, O. (1999). Summary Indicators of Product Market Regulation with an Extension to Employment Protection Legislation. Technical report, OECD Working Paper No. 226. 120, 221

North, D. C. and Weingast, B. R. (1989). Constitutions and Commitment: The Evolution of Institutions Governing Public Choice in Seventeenth-Century England. *The Journal of Economic History*, 49(4):803–832. 76

Oates, W. E. (1972). *Fiscal Federalism*. NY: Harcourt Brace Jovanovich. 17

Oates, W. E. (2005). Toward A Second-Generation Theory of Fiscal Federalism. *International Tax and Public Finance*, 12(4):349–373. 76

Olson, M. (1965). *The Logic of Collective Action: Public Goods and Theory of Groups*. Harvard University Press, Cambridge, MA. 75

Pagliero, M. (2011). What is the Objective of Professional Licensing? Evidence from the US Market for Lawyers. *International Journal of Industrial Organization*, 29(4):473–483. 113

Patrizii, V., Rapallini, C., and Zito, G. (2006). I Patti di Stabilità Interni. *Rivista di Diritto Finanziario e Scienza delle Finanze*, 65(1):156–189. 72

Pavese, C. and Rubolino, E. (2020). Do Fiscal Restraints Harm Test Scores? Evidence from Italy. *Working paper, University of Venice & University of Essex*. 81, 98

Pellizzari, M., Basso, G., Catania, A., Labartino, G., Malacrino, D., and Monti, P. (2011). Family Ties in Licensed Professions in Italy. Technical report, Fondazione Rodolfo Debenedetti. 113

Persson, T. and Tabellini, G. (2000). *Political Economics: Explaining Economic Policy*. MIT Press, Cambridge, MA. 81

Piketty, T. and Saez, E. (2013). Optimal Labor Income Taxation. In Auerbach, A. J., Chetty, R., Feldstein, M., and Saez, E., editors, *Handbook of Public Economics*, pages 391–474. Elvesier-North, Amsterdam. 3, 9

Piketty, T., Saez, E., and Stantcheva, S. (2014). Optimal Taxation of Top Labor Incomes: A Tale of Three Elasticities. *American Economic Journal: Economic Policy*, 6(1):230–271. 55

Pissarides, C. A. and Weber, G. (1989). An Expenditure-Based Estimate of Britain's Black Economy. *Journal of Public Economics*, 39(1):17–32. 27

Pomeranz, D. (2015). No Taxation Without Information: Deterrence and Self-Enforcement in the Value Added Tax. *American Economic Review*, 105(8):2539–2569. 69

Pomeranz, D. and Vila-Belda, J. (2019). Taking State-Capacity Research to the Field: Insights from Collaborations with Tax Authorities. *Annual Review of Economics*, 11(1):755–781. 69

Povoledo, E. (2011). Italy Tries to Get Tax Cheats to Pay Up, New York Times. 70

Putnam, R. (1993). *Making Democracy Work: Civic Tradition in Modern Italy*. Princeton, N.J.: Princeton University Press. 95

Puviani, A. (1903). *Teoria dell'Illusione Finanziaria*. Remo Sandon, Milan. 76

Raitano, M. and Vona, F. (2018). Nepotism vs Specific Skills: The Effect of Professional Liberalization on Returns to Parental Background of Italian Lawyers. 112, 117

Rubolino, E. (2020). Tax-Induced Transfer of Residence: Evidence from Tax Decentralization in Italy. *Working paper, University of Essex*. 71, 82

Rubolino, E. and Waldenström, D. (2019). Trends and Gradients in Top Tax Elasticities: Cross-Country Evidence, 1900–2014. *International Tax and Public Finance*, 26(3):457–485. 16

Rubolino, E. and Waldenström, D. (2020). Tax Progressivity and Top Incomes Evidence from Tax Reforms. *The Journal of Economic Inequality*, 18(3):261–289. 180

Sacerdote, B. (2011). Nature and nurture effects on children's outcomes: What have we learned from studies of twins and adoptees? In Benhabib, J., Jackson, M. O., and Bisin, A., editors, *Handbook of Social Economics*. Elvesier, North Holland. 108

Saez, E. (2001). Using Elasticities to Derive Optimal Income Tax Rates. *The Review of Economic Studies*, 68(1):205–229. 55

Saez, E. (2003). The Effect of Marginal Tax Rates on Income: A Panel Study of 'Bracket Creep'. *Journal of Public Economics*, 87(5-6):1231–1258. 21

Saez, E. (2004). Reported Incomes and Marginal Tax Rates, 1960-2000: Evidence and Policy Implications. *Tax Policy and the Economy*, 18:117–173. 55

Saez, E. (2010). Do Taxpayers Bunch at Kink Points? *American Economic Journal: Economic Policy*, 2(3):180–212. 69

Saez, E., Slemrod, J., and Giertz, S. H. (2012). The Elasticity of Taxable Income with Respect to Marginal Tax Rates: A Critical Review. *Journal of Economic Literature*, 50(1):3–50. 5, 13, 16, 63, 77, 104

Sandmo, A. (2012). An Evasive Topic: Theorizing About the Hidden Economy. *International Tax and Public Finance*, 19:5–24. 76

Schmidheiny, K. (2006). Income Segregation and Local Progressive Taxation: Empirical Evidence from Switzerland. *Journal of Public Economics*, 90(3):429–458. 16, 51

Schmidheiny, K. and Slotwinski, M. (2018). Tax-induced Mobility: Evidence from a Foreigners' Tax Scheme in Switzerland. *Journal of Public Economics*, 167:293–324. 16

Slattery, C. and Zidar, O. (2020). Evaluating State and Local Business Incentives. *Journal of Economic Perspectives*, 34(2):90–118. 17

Slemrod, J. (1994). Fixing the Leak in Okun's Bucket Optimal Tax Progressivity when Avoidance Can Be Controlled. *Journal of Public Economics*, 55(1):41–51. 71

Slemrod, J. (2007). Cheating Ourselves: The Economics of Tax Evasion. *Journal of Economic Perspectives*, 21(1):25–48. 5, 63

Slemrod, J. (2010). Location, (Real) Location, (Tax) Location: An Essay on Mobility's Place in Optimal Taxation. *National Tax Journal*, 63(4, Part 2):843–864. 15

Slemrod, J. (2019). Tax Compliance and Enforcement. *Journal of Economic Literature*, 57(4):904–954. 5, 63

Slemrod, J., Blumenthal, M., and Christian, C. (2001). Taxpayer response to an increased probability of audit: evidence from a controlled experiment in Minnesota. *Journal of Public Economics*, 79(3):455–483. 69

Slemrod, J. and Gillitzer, C. (2013). Tax Systems. MIT Press, Cambridge, MA. 70, 77

Slemrod, J. and Kopczuk, W. (2002). The Optimal Elasticity of Taxable Income. *Journal of Public Economics*, 84(1):91–112. 69, 77

Slemrod, J. and Weber, C. (2012). Evidence of the Invisible: Toward a Credibility Revolution in the Empirical Analysis of Tax Evasion and the Informal Economy. *International Tax and Public Finance*, 19(1):25–53. 69

Slemrod, J. and Yitzhaki, S. (2002). Tax Avoidance, Evasion, and Administration. In Auerbach, A. J. and Feldstein, M., editors, *Handbook of Public Economics*, pages 1423–1470. Elvesier, Amsterdam, 3 edition. 69

Smith, A. (1776). *Wealth of Nations*. Modern Library Edition. 113

Stigler, G. J. (1971). The Theory of Economic Regulation. *The Bell Journal of Economics and Management Science*, 2(1):3. 113

Suárez Serrato, J. C. and Zidar, O. (2016). Who Benefits from State Corporate Tax Cuts? A Local Labor Markets Approach with Heterogeneous Firms. *American Economic Review*, 106(9):2582–2624. 16

Tiebout, C. M. (1956). A Pure Theory of Local Expenditures. *Journal of Political Economy*, 64(5):416–424. 3, 9, 17

Tullock, G. (1959). Problems of Majority Voting. *Journal of Political Economy*, 67(6):571–579. 75

Vicente, P. C. (2010). Does Oil Corrupt? Evidence from a Natural Experiment in West Africa. *Journal of Development Economics*, 92(1):28–38. 70

Weigel, J. L. (2020). The Participation Dividend of Taxation: How Tax Collection in Congo Engenders Citizen Engagement with the State. *Quarterly Journal of Economics*, (forthcoming). 68

Weingast, B. R. (2009). Second Generation Fiscal Federalism: The implications of Fiscal Incentives. *Journal of Urban Economics*, 65(3):279–293. 76

Wildasin, D. E. (1985). Income Taxes and Urban Spatial Structure. *Journal of Urban Economics*, 18(3):313–333. 51

Young, C., Varner, C., Lurie, I. Z., and Prisinzano, R. (2016). Millionaire Migration and Taxation of the Elite. *American Sociological Review*, 81(3):421–446. 16, 38

Appendices

A Appendix for Tax-Induced Transfer of Residence: Evidence from Tax Decentralization in Italy

A1 Local income taxation in Italy

This Appendix provides detailed information on the set of rules regulating the income tax set by municipalities and regions in Italy.

Municipal tax on personal income. Municipalities can establish, according to Article 1 of Legislative Decree 360/1998, an income tax on top of the tax rate set by the central and regional government. Such tax rate cannot exceed the 0.8 percent, except for cases expressly provided by the law, such as the case of the city of Rome, which, starting from 2011, can establish a rate of up to 0.9 percent.

Starting from 2007, municipalities have been granted the right to introduce an exemption threshold from the tax in the presence of specific income requirements. In this case, the municipal income tax is not due if the income is lower or equal to the limit established by the municipality. From 2011, municipalities can establish a single rate or a plurality of different rates. If a municipality implements a graduated tax scheme, tax rates must be articulated according to the same income brackets established for the national income tax, *Imposta sul Reddito delle Persone Fisiche* (IRPEF), as well as diversified and increasing with income.

The municipal income tax (as well as the income tax set by the regional and national government) depends on the municipality in which the taxpayer has her tax residence on 1st of January of the year to which the payment of the tax refers. The tax is calculated by applying the rate set by the municipality to the total income determined for *IRPEF* purposes, net of deductible costs and tax credits for income generated abroad. The payment of the municipal income tax is made on account and balance, together with the payment of the income tax for the regional and national government.

For the year 2016 and 2017, municipalities cannot change the income tax rate(s). The same law states that the local tax freeze does not apply to the municipal waste disposal tax (*TARI*). Furthermore, the tax freeze does not apply for local authorities that are on *predissesto* or a situation of financial instability, as referred by Article 243-bis and Article 246 of Legislative Decree no. 267/2000.

Municipalities retain revenue from the municipal income tax. Tax intakes are mostly used to deliver public goods provisions, such as public transportation, waste management and to finance school facilities.

In the following, we list the laws concerning the income tax set by municipalities:

- Law 15 December 1997, n. 446, Article 52. *Istituzione dell'imposta regionale sulle attività produttive, revisione degli scaglioni, delle aliquote e delle detrazioni dell'Irpef e istituzione di una addizionale regionale a tale imposta, nonché riordino della disciplina dei tributi locali*
- Law 28 September 1998, n. 360. *Istituzione di una addizionale comunale all'IRPEF*.
- Law 27 December 2006, n. 296. *Disposizioni per la formazione del bilancio annuale e pluriennale dello Stato*.
  - Article 1, comma 161. *Modalità e termini per l'accertamento, da parte degli enti locali, dei tributi di propria competenza.*
  - Article 1, comma 162. *Requisiti minimi che devono possedere gli atti di accertamento di tributi locali.*
  - Article 1, comma 163. *Termine per la notifica degli atti esecutivi relativi a tributi locali.*
  - Article 1, comma 164. *Termine per la richiesta di rimborso, da parte del contribuente, di tributi locali non dovuti.*
  - Article 1, comma 165. *Misura degli interessi sui rimborsi di imposta.*

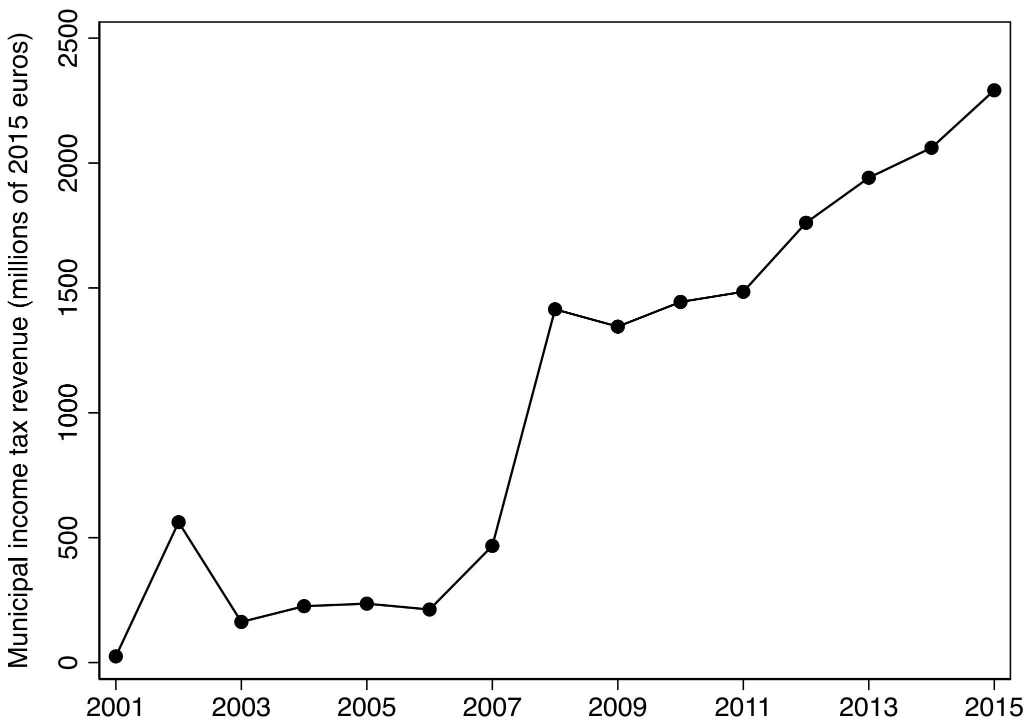
  - Article 1, comma 166. *Arrotondamento del versamento di tributi locali*.
  - Article 1, comma 167. *Modalità di compensazione di tributi locali*.
  - Article 1, comma 168. *Soglie minime per l'esigibilità di tributi locali*.
  - Article 1, comma 169. *Proroga automatica delle aliquote vigenti in mancanza di nuova delibera*.

- Law 31 May 2010. *Misure urgenti in materia di stabilizzazione finanziaria e di competitività economica.*
- Law 14 March 2011, n. 23. *Disposizioni in materia di federalismo fiscale municipale.*
  - Article 14. *Ambito di applicazione del decreto legislativo, regolazioni finanziarie e norme transitorie*.
- Law 13 August 2011, n. 138. *Ulteriori misure urgenti per la stabilizzazione finanziaria e per lo sviluppo.*
  - Article 1. *Disposizioni per la riduzione della spesa pubblica*.
- Law 21 November 2014, n. 175, Article 8. *Semplificazione fiscale e dichiarazione dei redditi precompilata.*
- Law 28 December 2015, n. 208. *Disposizioni per la formazione del bilancio annuale e pluriennale dello Stato*.

Revenue from the regional and municipal income tax accounts for 28,302 millions of euros, that is nearly 16 percent of total revenue in 2015. Figure A1 shows the trend in total revenue raised from the municipal income tax over the 2001-2015 period. The figure clearly underlines the process of tax decentralization described above. Tax revenue were around 25 millions of euros in 2001, then they substantially rose after the first wave of decentralization in 2007 (to around

1,500 millions) and continue to gradually increase after the 2011 reform. In 2015, municipalities raised around 2,500 millions of euros from the municipal income tax. This accounts for around 1.5 percent of total personal income tax revenue raised by the Italy in 2015 (that amount to nearly 173,007 millions of euros).

Figure A1: Tax revenue from the municipal income tax



Note: This figure depicts the trend in total revenue (millions of 2015 euros) raised from the municipal income tax. The sample includes 7,960 municipalities over the 2001-2015 period.

Regional tax on personal income. The law 446/1998 introduced the regional surcharge to the income tax. The tax rate applied to total taxable income, net of deductible costs, deductions, the tax credit for profits distributed by companies and entities, and for income produced abroad. The basic rate of the tax was 0.9 percent initially, then raised to 1.23 percent from 2012. Each region (and autonomous province) can increase the basic rate within the limits set by the national law by modifying its own law, which is published in the Official Gazette no later than 31 December of the year preceding the one in which the tax refers.

The discipline of the regional additional income tax was substantially reformed by the Article 6 of Legislative Decree 68/2011, which established that any tax increase cannot be larger than 2.1 percentage points. Similarly to the municipal income tax, this reform established that regions can adopt a graduated tax scheme, where the tax rates must be articulated exclusively in relation to the same income brackets established for the national income tax, as well as diversified and increasing with income. The regions can arrange tax deductions in favor of the family and also adopt measures of direct economic support. The regions can also set deductions from the regional income tax itself in the place where subsidies, vouchers, service vouchers and other social support measures provided for by regional legislation are provided.[25]

The regional surcharge is paid in a single solution to the region in which the taxpayer has his tax domicile on 1st of January of the year in which the tax rate applied. Revenue from the regional income tax are held by the regions and mostly contribute to financing the National Health Service (administered by the regions).

In the following, we list the laws concerning the income tax set by regions:

- Law 15 December 1997, n. 446, Article 50. *Istituzione dell'imposta regionale sulle attività produttive, revisione degli scaglioni, delle aliquote e delle detrazioni dell'Irpef e istituzione di una addizionale regionale a tale imposta, nonché riordino della disciplina dei tributi locali.*
- Law 4 December 1997, n. 460, Article 21. *Riordino della disciplina tributaria degli enti non commerciali e delle organizzazioni non lucrative di utilità sociale*
- Law 4 May 2001, n. 207, Article 4. *Riordino del sistema delle istituzioni pubbliche di assistenza e beneficenza, a norma dell'articolo 10 della L. 8 novembre 2000, n. 328.*
- Law 30 December 2004, n. 311, Article 1. *Disposizioni per la formazione del bilancio an- nuale e pluriennale dello Stato.*

[25]These tax exemption measures cannot be adopted by the regions involved in the recovery plans from the health deficit.

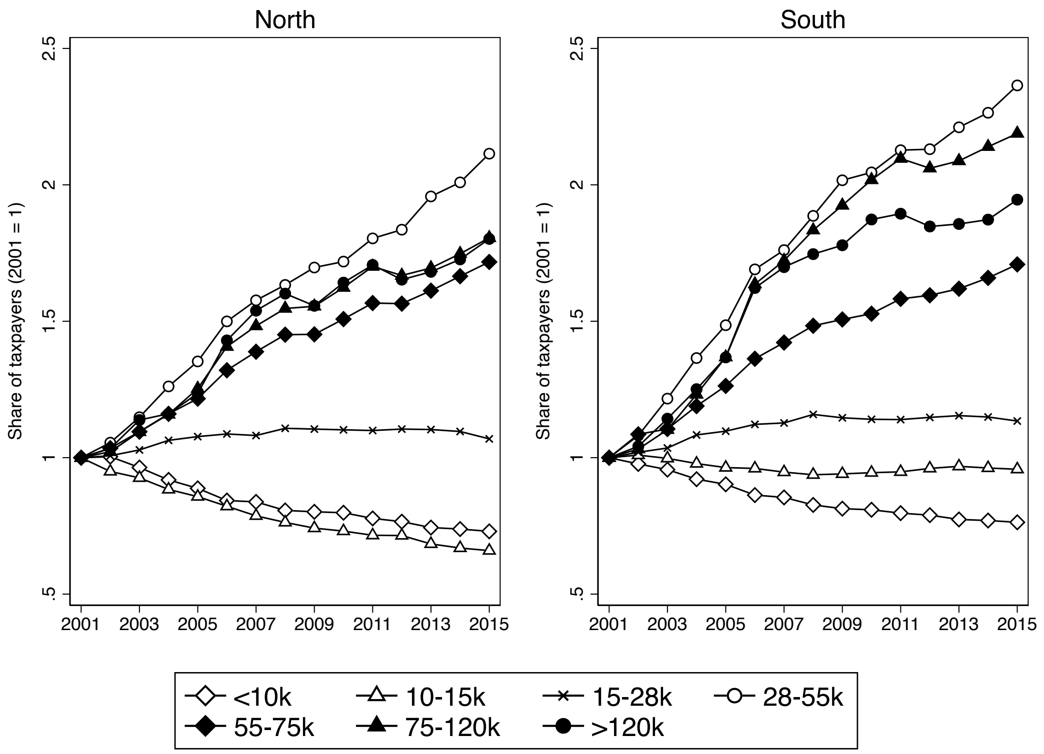
- Law 27 December 2006, n. 296, Article 1. *Disposizioni per la formazione del bilancio an- nuale e pluriennale dello Stato*
- Law 1 October 2007 n. 159, Article 4.
- Law 5 May 2009, n. 42, Article 7. *Delega al Governo in materia di federalismo fiscale, in attuazione dell'articolo 119 della Costituzione.*
- Law 23 December 2009, n. 191, Article 2. *Disposizioni per la formazione del bilancio annuale e pluriennale dello Stato.*
- Law 31 May 2010, n.78, Article 11. *Misure urgenti in materia di stabilizzazione finanziaria e di competitività economica.*
- Law 6 May 2011, n. 68, Articles 2 and 6. *Disposizioni in materia di autonomia di entrata delle regioni a statuto ordinario e delle province, nonché di determinazione dei costi e dei fabbisogni standard nel settore sanitario.*
- Law 6 December 2011 n. 201, Article 28. *Disposizioni urgenti per la crescita, l'equità e il consolidamento dei conti pubblici.*
- Law 29 December 2011, n. 216, Article 29. *Proroga di termini previsti da disposizioni legislative.*
- Law 22 June 2012, n. 83, Article 19 comma 9. *Possibilità per la regione Campania di destinare l' aumento dell'aliquota dell'addizionale regionale all'IRPEF previsto dall'art. 2, comma 86, della legge n. 191 del 2009 o anche il raddoppio dell'aumento stesso, alla copertura del Piano di rientro dal disavanzo nel settore del trasporto.*
- Law 6 July 2012, n. 95, Article 15. *Disposizioni urgenti per la revisione della spesa pubblica con invarianza dei servizi ai cittadini nonché misure di rafforzamento patrimoniale delle imprese del settore bancario.*

- Law 8 April 2013, n. 35, Article 3-ter. *Disposizioni urgenti per il pagamento dei debiti scaduti della pubblica amministrazione, per il riequilibrio finanziario degli enti territoriali, nonché in materia di versamento di tributi degli enti locali*

- Law 28 June 2013, n. 76, Article 11 comma 15. *Primi interventi urgenti per la promozione dell'occupazione, in particolare giovanile, della coesione sociale, nonché in materia di Imposta sul valore aggiunto (IVA) e altre misure finanziarie urgenti.*

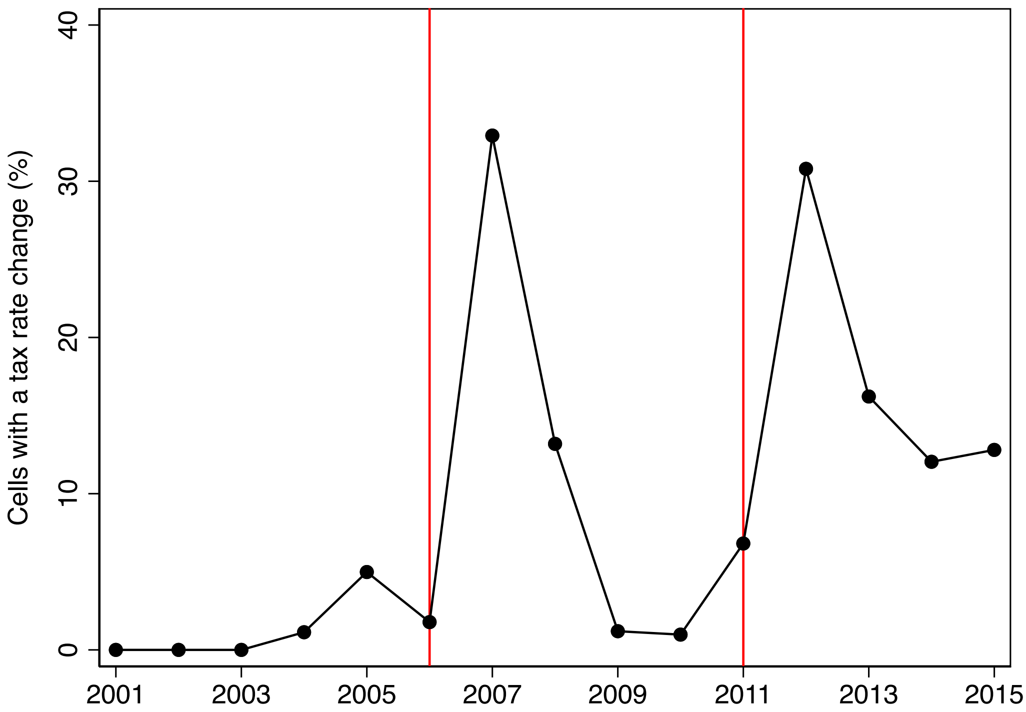
- Law 21 November 2014, n. 175, Article 8 comma 1 and 4. *Semplificazione fiscale e dichiarazione dei redditi precompilata.*

Figure A2: Share of taxpayers in each income group



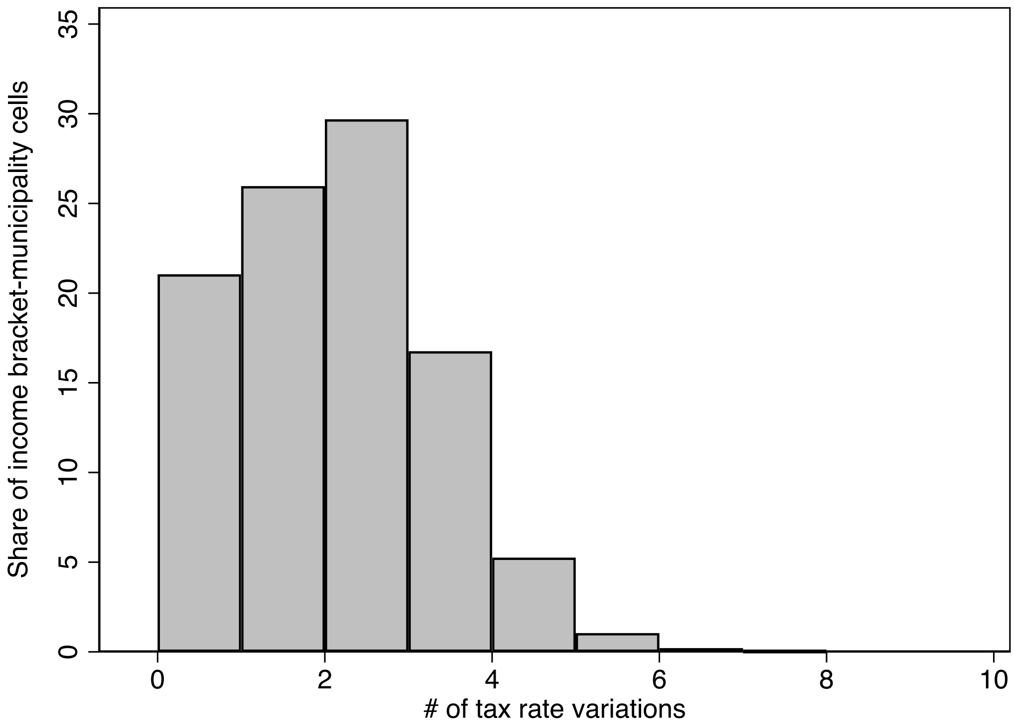
Note: The figure displays the evolution in the share of taxpayers in each income group in Northern Italy (left-hand side graph) and Southern Italy (right-hand side graph). Series are normalized to 1 in 2001.

Figure A3: Changes in the local tax rate



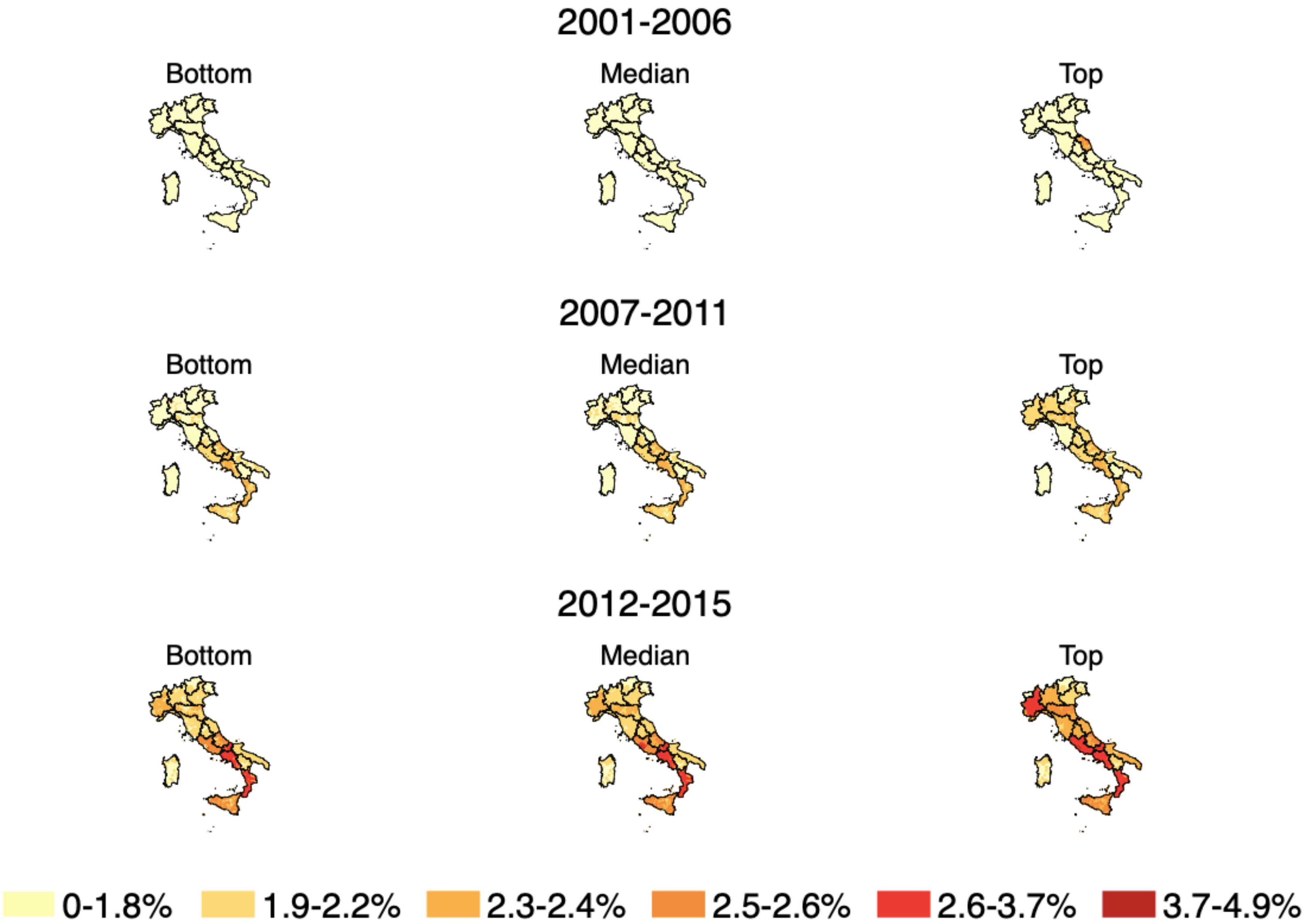
Note: The figure displays the evolution in the share of income bracket-municipality-year observations containing a tax rate change. Red vertical lines refer to the year before local governments were granted the possibility to implement a tax exemption cutoff (2006) and to set different tax rates across brackets (2011).

Figure A4: How many tax rate variations?



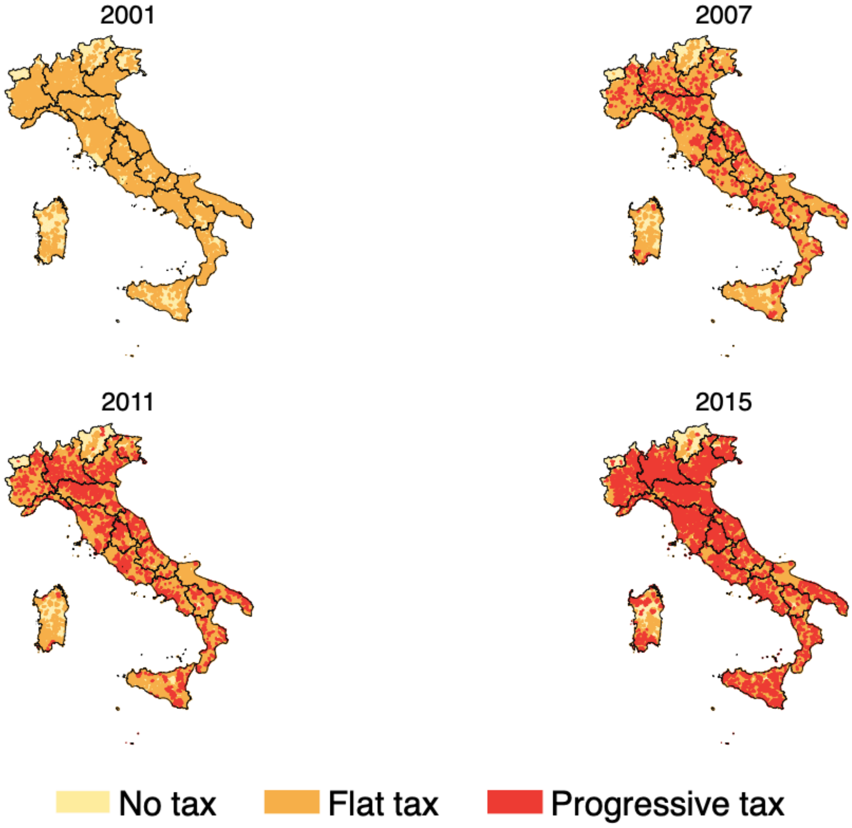
Note: The figure displays the share of income-bracket municipality cells (vertical axis) with a given number of tax rate change (horizontal axis) observed over the 2001-2015 period.

Figure A5: Local average tax rate by bracket



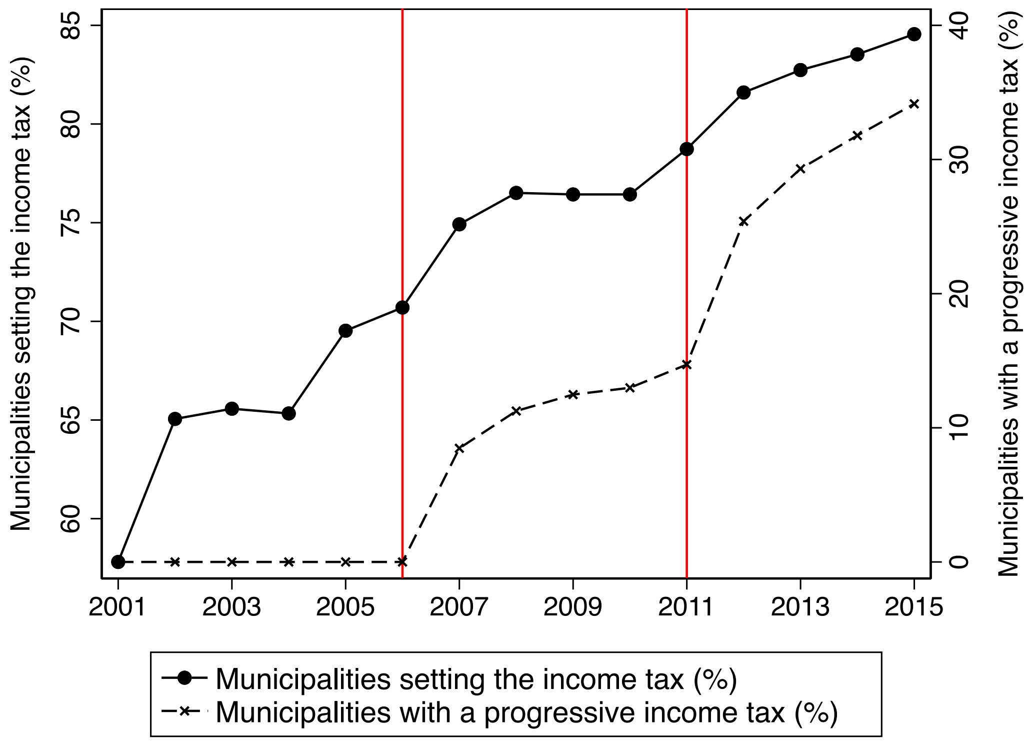
Note: This graph depicts the local average tax rate on personal income (%) for taxpayers in bottom, median and top income bracket. The black line indicates regional boundaries.

Figure A6: Municipality tax scheme



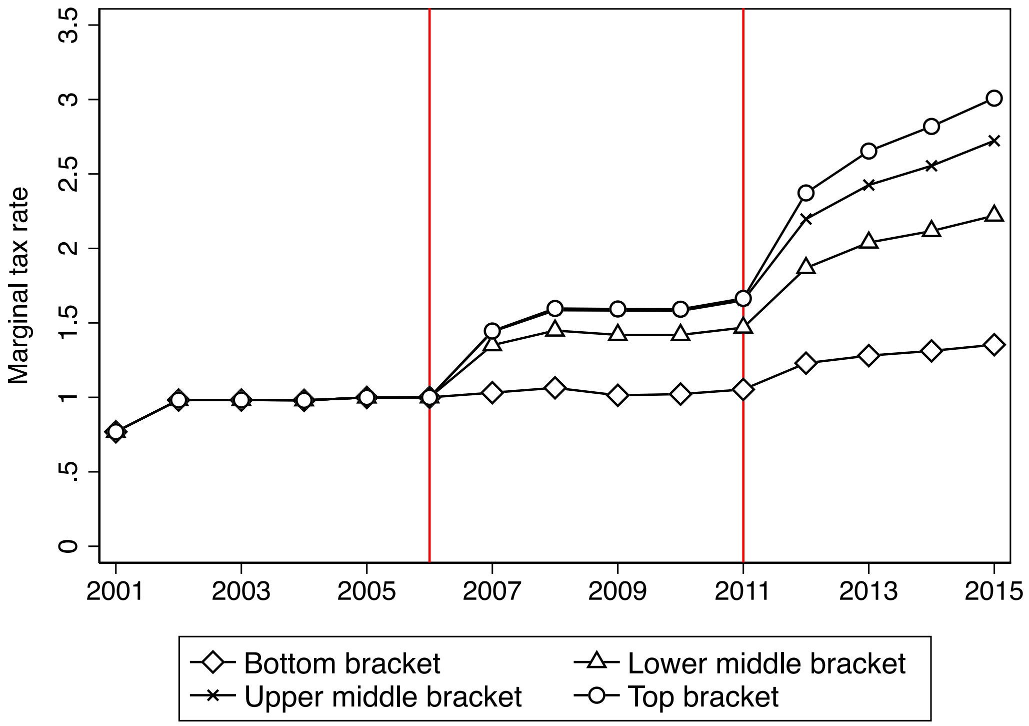
Note: The figure displays the evolution in the municipality tax scheme, by showing whether a municipality has set a tax rate on personal income and, if it did, whether it is a single flat rate or a progressive tax scheme (including both those with a tax exemption cutoff or graduated tax rates). The black line indicates regional boundaries.

Figure A7: Trend in municipal income taxation



Note: The figure displays the evolution in the share of municipalities with a nonzero income tax rate (left-hand side vertical axis) and in the share of municipalities with a graduated tax scheme (right-hand side vertical axis) over the 2001-2015 period. Red vertical lines refer to the year before local governments were granted the possibility to implement a tax exemption cutoff (2006) and to set different tax rates across brackets (2011).

Figure A8: Within-municipality cross-bracket tax rate variation



Note: The figure displays the evolution in the municipal marginal tax rate across income bracket over the 2001-2015 period for municipalities with a graduated tax rate. Red vertical lines refer to the year before local governments were granted the possibility to implement a tax exemption cutoff (2006) and to set different tax rates across brackets (2011). Data from the Italian Ministry of Economy and Finance.

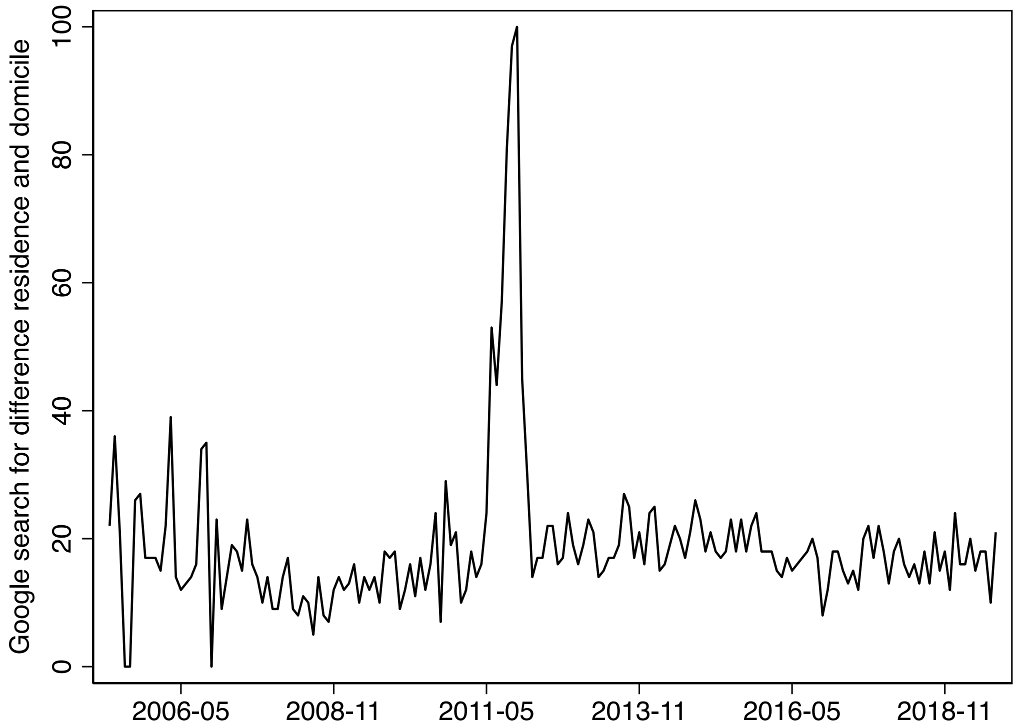
A2 Transfer of residence

Table A1: Average annual outflow of individuals, selected province pairs

Origin	Destination province:																			
province:	RM	MI	NA	TO	PA	BS	BA	CT	BG	SA	FI	BO	PD	CE	VR	VA	TV	VI	VE	GE
RM	49,047	1,629	1,040	605	326	242	325	247	182	419	471	416	240	403	261	218	252	161	236	239
MI	1,283	51,998	652	684	379	726	334	277	2,134	249	301	327	235	156	309	3,846	194	166	213	615
NA	2,949	1,919	45,726	560	151	375	174	96	272	2,504	654	838	191	5,582	282	330	233	177	275	251
TO	582	888	258	50,234	240	136	137	168	125	149	129	156	89	116	123	176	84	69	95	268
PA	638	806	144	362	16,629	140	53	251	158	46	190	275	92	58	143	176	93	110	110	138
BS	243	677	225	146	98	25,472	89	68	1,174	68	66	93	94	66	410	90	57	88	82	68
BA	704	878	135	238	53	136	10,791	34	95	53	121	264	97	53	120	110	92	84	109	50
CT	446	589	70	260	209	103	38	19,267	109	30	95	164	84	26	110	135	79	60	63	63
BG	163	1,578	144	114	100	1,269	55	70	22,971	62	46	63	49	49	89	132	41	46	44	66
SA	1,013	653	1,757	238	51	131	48	31	93	12,718	177	299	76	148	84	131	74	53	63	80
FI	426	329	240	107	150	50	54	59	43	74	15,465	233	59	66	56	51	39	34	54	67
BO	379	405	322	129	175	70	119	83	54	133	194	18,987	106	126	97	54	63	55	86	60
PD	215	294	96	86	47	81	43	40	48	32	63	122	16,983	34	319	42	726	1,146	1,697	35
CE	938	487	3,237	204	53	120	60	24	86	146	196	281	68	12,779	101	135	97	56	86	69
VR	212	340	160	104	95	387	63	68	85	35	55	106	313	56	18,379	48	112	560	129	42
VA	191	2,857	154	130	104	103	61	85	128	66	46	55	48	57	51	17,764	36	36	37	77
TV	224	229	120	77	65	71	48	44	43	40	60	89	832	58	134	42	17,345	625	1,674	31
VI	174	221	112	66	85	94	48	39	51	31	43	80	1,196	33	628	36	581	17,297	256	29
VE	205	252	137	82	69	62	52	37	40	25	58	104	1,756	46	150	43	1,832	258	10,225	34
GE	232	645	114	256	105	63	34	43	58	33	76	62	37	28	45	76	24	22	41	9,433

Note: This table shows the average annual outflow of individuals moving from an origin province (first column) to a destination province (columns 2-20) over the 2007-2015 period within the following 20 most populated provinces: Rome (RM), Milan (MI), Naples (NA), Turin (TO), Palermo (PA), Brescia (BS), Bari (BA), Catania (CT), Bergamo (BG), Salerno (SA), Florence (FI), Bologna (BO), Padua (PD), Caserta (CE), Verona (VE), Varese (VA), Treviso (TV), Vicenza (VI), Venice (VE), and Genua (GE). The full dataset covers 11,932,720 transfers of residence moving within 107 $\times$ 107 = 11,449 province pairs over the 2007-2015 period.

Figure A9: Google search for "difference domicile and residence"



Note: This figure depicts the trend in google search for "difference domicile and residence" (*differenza domicilio e residenza*). Searches are normalized to 100 in the peak period.

A3 Results appendix

Robustness analysis for the progressive tax analysis. This section provides alternative specifications and robustness checks to test the sensitivity of our baseline results on the impact of implementing a progressive local tax schedule. First, we account for different intensities in the implementation of a progressive local tax schedule. While our baseline estimates consider the decision of switching to a progressive tax as a simple discrete change in the local tax scheme, the intensity of the reform is likely to differ across municipalities depending on the progression of the marginal tax rates across brackets. We account for this possibility in two ways. First, we implement a triple difference strategy by further comparing municipalities introducing only a tax exemption for low incomes (which we expect to be relatively less affected) with municipalities with graduated tax rates. Column (2) in Table A2 shows that the impact is not not statistically significant in municipalities that implemented a tax exemption. Second, we compute the average rate progression (i.e., the derivative of the tax rate with respect to income levels), which we calculate at income level equal to four times the median tax base in a given year. This measure is an index of the structural progressivity of a tax schedule (Musgrave and Thin 1948; Rubolino and Waldenström 2020). In column (3) of Table A2, we show that the impact on the tax base is significantly larger when the tax schedule is more progressive, that is, when the slope of the tax rate progression is steeper.

Second, we examine the sensitivity of our results to the staggered implementation of the progressive tax schedule. One difference between equation (1.2) and the classical DiD approach is that our model accounts for the fact that there are many local tax scheme switches staggered over time. The staggered implementation of a progressive tax also means that our control group is not restricted to municipalities that never implement a progressive tax. In fact, equation (1.2) can be estimated even if all munic-

Table A2: Implementing a progressive tax schedule, alternative specifications

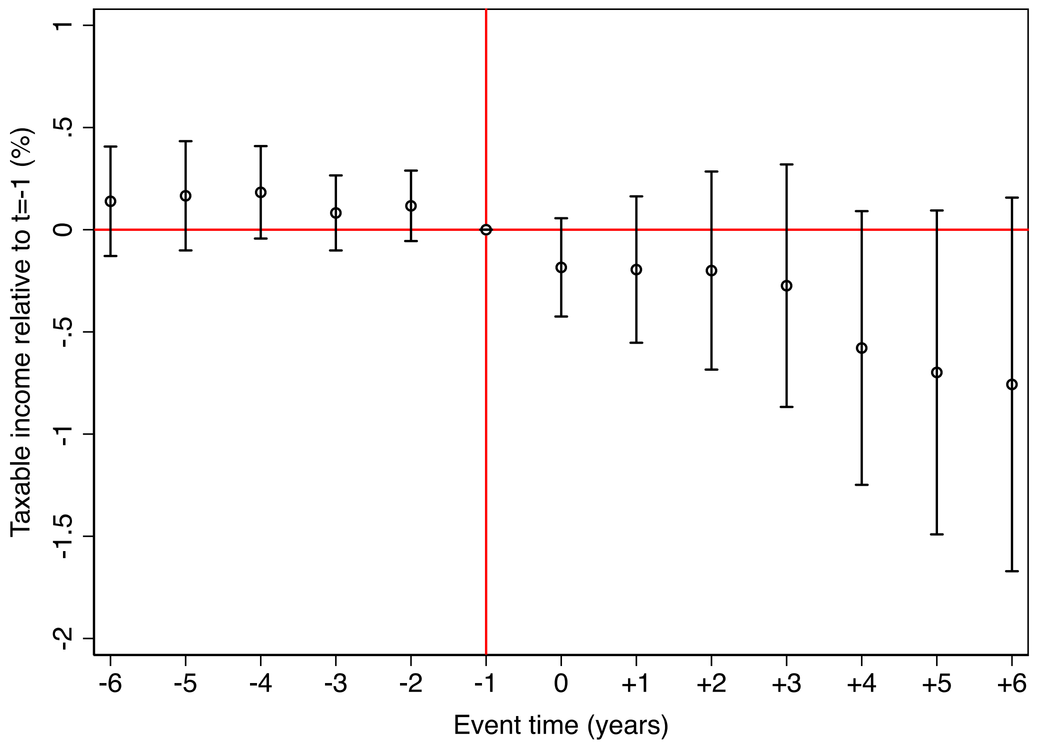
	log(taxable income)		
	(1)	(2)	(3)
$1(i \in Prog) \cdot 1(t \in Post)$	-0.012***		
	(0.003)		
$1(i \in Exemption) \cdot 1(t \in Post)$		-0.002	
		(0.007)	
$1(i \in Exemption) \cdot 1(i \in Gra) \cdot 1(t \in Post)$		-0.014***	
		(0.004)	
$log(AverageRateProgression)$			-0.040***
			(0.011)
Observations	118,770	118,770	118,770
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Tax base (€1,000)	93,659	93,659	93,659

Note: Column (1) shows the baseline effect of switching from a flat to a progressive income tax schedule. Column (2) reports the coefficient estimate separately for municipalities implementing only a tax exemption cutoff (and then a flat rate for incomes above the cutoff), $1(i \in Exemption)$, and those introducing also graduated tax rates in addition to a tax exemption cutoff, $1(i \in Exemption) \cdot 1(i \in Gra)$. In column (3), we use the average rate progression as a measure of the structural progressivity of the local income tax schedule. The sample is composed of 7,918 municipalities over the 2001-2015 period. Standard errors clustered at municipality-level in parentheses.

ipalities eventually switch to a progressive tax scheme by exploiting differences in the timing of the reform. We report this exercise in Figure A10 and we show coefficient estimate and confidence intervals in Table A3. The results are qualitatively similar when we rely only on variation in the timing of the local tax scheme switch.

Third, we explore whether the effect differs across municipalities that switched from a zero local tax rate to a progressive tax versus those switching from a flat to a progressive income tax schedule. In our sample, the share of municipalities taxing their residents (either through a flat or a progressive tax schedule) has steadily increased over time (see Figure A7), raising from around 55 percent to 85 percent over the 2001-2015 period. One may argue that the first-time introduction of an income tax might have a larger effect that a change in an existing tax rate, since the initial inception of a local tax might be more salient than any subsequent change. To investigate this possibility, we analyze the effect on the tax base stemming from a triple interaction: i. a dummy

Figure A10: Staggered impact of implementing a progressive local tax schedule



Note: The figure depicts the effects of switching from a flat to a progressive local income tax schedule by exploiting the staggered timing in the local tax scheme switch. The figure plots estimated coefficients and the 95 percent confidence intervals: each point shows the effect of having implemented a progressive tax schedule for j years (if j >-1) or of starting the policy in j years (if j < -1) relative to the year before the tax scheme switch was implemented. Regressions include municipality fixed effects and province × year fixed effects. The sample includes 2,828 municipalities over the 2001-2015 period. Standard errors clustered at municipality-level.

for municipalities that did not set an income tax originally ("later" municipalities); ii. a dummy for the period after introducing an income tax; iii. a dummy equal to 1 when a later municipality introduces a progressive tax. We report the result of this exercise in Table A4. We find that, conditional on introducing a local income tax, the effect is significantly larger when a later municipality chooses a progressive tax scheme.

Fourth, we test the sensitivity of our estimate to the differential trend in cost of living across municipalities. For this end, we deflate the tax base by the average housing price in a municipality, that allows to absorb, at least in part, any municipality-specific change in cost of living. Table A5 shows that the effect of introducing a progressive tax remains remarkably similar.

Finally, we allow for spatial correlation in the error term among municipalities by

Table A3: Staggered change in the tax scheme and tax base

	log(taxable income)			
	(1)	(2)	(3)	(4)
$1(i \in Prog) \cdot 1(t \in Post)$	-0.011***	-0.005**	-0.005**	-0.006**
	(0.004)	(0.003)	(0.002)	(0.003)
Observations	42,421	42,421	42,421	42,421
Municipality FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes
Province × year FE	No	No	Yes	Yes
LLM × province × year FE	No	No	No	Yes
Tax base (€1,000)	180,906	180,906	180,906	180,906

Note: This table shows the effect of switching from a flat to a progressive income tax. The sample is composed of 2,828 municipalities over the 2001-2015 period. The average tax base in progressive tax municipalities over the period before the tax scheme switch was around 169 million of 2015 euros. Standard errors clustered at municipality-level in parentheses.

clustering the standard errors on a higher level of aggregation (Angrist and Pischke 2009). Figure A11 shows that our estimates remain statistically significant at usual confidence intervals when we employ standard errors clustered at local labor market, provincial or regional level.

Table A4: Introduction of a local income tax

	log(taxable income)					
	(1)	(2)	(3)	(4)	(5)	(6)
$1(i \in Later) \cdot 1(t \in Post)$	0.004	0.007	0.006	0.001	0.004	0.007
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.007)
... $\cdot 1(i \in Prog)$		-0.013***	-0.017***		-0.012**	-0.017**
		(0.004)	(0.003)		(0.006)	(0.007)
Observations	118,770	118,770	118,770	49,227	49,227	49,227
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM × province × year FE	No	No	Yes	No	No	Yes
Sample	Full	Full	Full	Later	Later	Later

Note: This table shows the tax base effect of introducing a local income tax in municipalities that did not set any income tax initially ("later" municipalities). In columns (2) and (3), we interact the dummy equal to 1 for each year after the introduction of the local income tax in a later municipality $1(i \in Later) \cdot 1(t \in Post)$, with a dummy equal to 1 when the tax scheme is progressive. In columns (4)-(6), we restrict the sample to only later municipalities to exploit the timing in the introduction of the local income tax. Standard errors clustered at municipality-level in parentheses.

Cross-bracket analysis with fixed effects. Although suggestive, the graphical evidence presented in Figure 1.5 might be biased if there are municipality-specific economic changes affecting differently taxpayers in the very top bracket relative to the bracket just below, and which are happening simultaneously with the local tax change. We attempt to account for this issue by adding a wide set of fixed effects. Specifically, we run regressions of the following form:

$$\begin{aligned} log(y_{b,i,t}) = \beta \cdot 1(b \in TopBracket) \cdot 1(t \in Post) + \\ \gamma_{b,i} + \delta_{i,t} + \eta_{b,p(i),t} + u_{b,i,t}, \qquad \forall i \in Prog, \end{aligned} \tag{1}$$

where $y_{b,i,t}$ is the tax base or population stock in the income bracket b in municipality i at time t. $1(b \in TopBracket)$ and $1(t \in Post)$ are dummies for the top income bracket and the post-tax scheme switch period, respectively. β is the coefficient of interest, which measures the impact of implementing a progressive tax on the tax base or population stock in the top bracket. We include income bracket × municipality fixed effects,

Table A5: Accounting for differences in cost of living

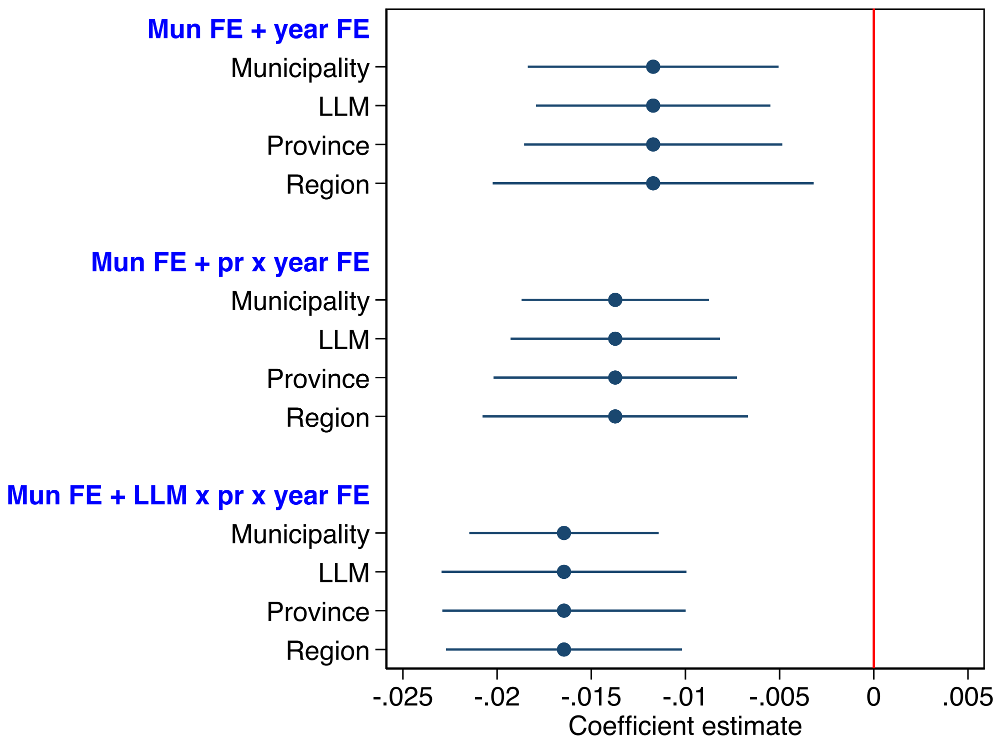
	log(taxable income)				
	(1)	(2)	(3)	(4)	(5)
$1(i \in Prog) \cdot 1(t \in Post)$	-0.012***	-0.012***	-0.011***	-0.010***	-0.008***
	(0.004)	(0.004)	(0.002)	(0.001)	(0.001)
Observations	86,874	86,874	86,874	86,874	86,874
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
Province × year FE	No	No	Yes	Yes	Yes
LLM × province × year FE	No	No	No	Yes	Yes
Controls	No	No	No	No	Yes
Tax base (€1,000)	94,375	94,375	94,375	94,375	94,375

Note: This table shows the effect of switching from a flat to a progressive income tax, where the tax base is deflated by the municipality-specific housing price. The sample is composed of 7,918 municipalities over the 2005-2015 period. Standard errors clustered at municipality-level in parentheses.

$\gamma_{b,i}$, to filter out permanent heterogeneity across taxpayers located in different municipality and/or different income brackets. Municipality × year fixed effects, $\delta_{i,t}$, account for municipality-specific time-varying amenities or economic shocks. If a municipality becomes more attractive after tax rate changes because of policy changes correlated with the change in taxes (e.g., improvement in public amenities or increase in public spending), then these fixed effects would absorb such difference. Income bracket × province × year fixed effects, $\eta_{b,p(i),t}$, control for any different reason for why individuals at different points in the income distribution and/or located in different places might experience different income growth rates, aside from tax changes.

Coefficient estimates are presented in Table A6. We start from a basic model with municipality, income bracket and year fixed effects. Consistent with the trend detected in the raw data, we find that the introduction of a progressive tax has a negative impact on both the tax base and the population stock in the top bracket. Point estimates remain qualitatively similar when we include municipality × income bracket fixed effects and municipality × year fixed effects. On average, our estimates imply that implementing a progressive tax would induce at most 3.904 top taxpayers to outmigrate: this would reduce the aggregate municipal tax base by around 854 thousand of euros. A rough comparison with the cross-municipality DiD estimate suggests that nearly fourth-fifth

Figure A11: Robustness to clustering choice



Note: The figure depicts coefficient estimates and confidence intervals for three models: i. municipality FE + year FE: ii. municipality FE + province × year FE; iii. municipality FE + local labor market × year FE. For each model, we compute standard errors at four different cluster levels: i. municipality (# = 7,910); ii. local labor market (# = 686); iii. province (# = 107); iv. region (# = 20).

of the erosion in the tax base is driven by the migration response in the top bracket.

One further challenge is represented by local shocks or policies affecting specifically the rich in a given local area. In column (3) and (6), we account for this possibility by including income bracket × year × province fixed effects. Our estimates hold up well across specifications, although the exact magnitude of the effect differs somewhat across models. Our most conservative estimate implies that implementing a progressive income tax reduces the tax base (stock of taxpayers) in the top bracket by 2.8 (0.8) percent.

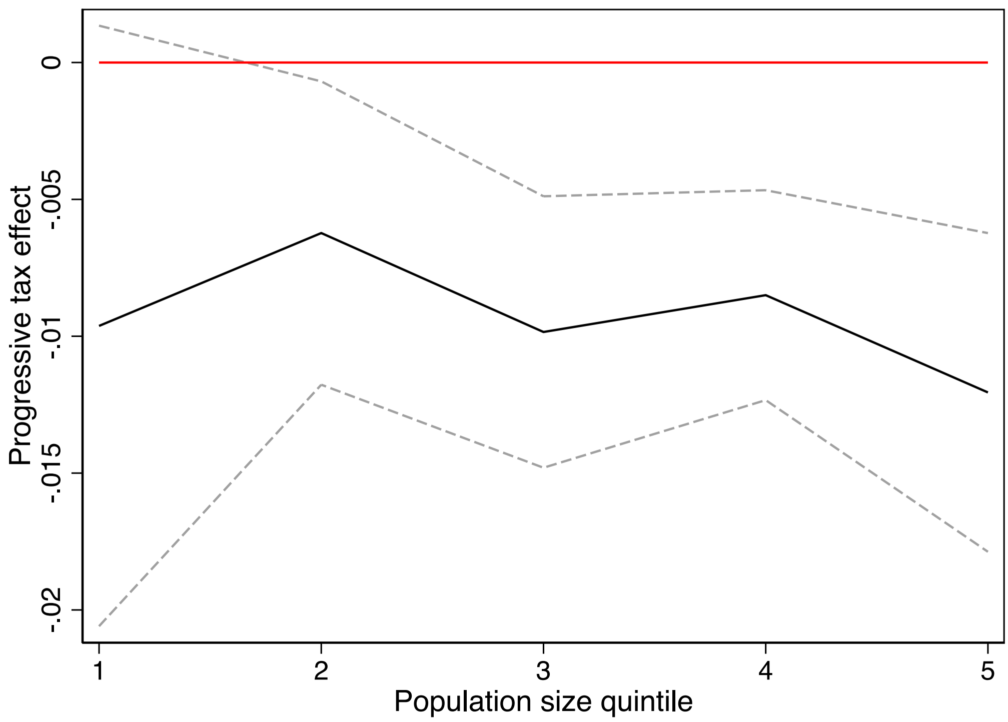
Table A6: The impact of implementing a progressive tax on top brackets

	log(taxable income)			log(population stock)		
	(1)	(2)	(3)	(4)	(5)	(6)
$1(b \in Top) \cdot 1(t \in Post)$	-0.045***	-0.052***	-0.028***	-0.029***	-0.032***	-0.008
	(0.014)	(0.008)	(0.009)	(0.010)	(0.006)	(0.006)
Observations	67,997	67,997	67,997	67,997	67,997	67,997
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income bracket FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality × bracket FE	No	Yes	Yes	No	Yes	Yes
Municipality × year FE	No	Yes	Yes	No	Yes	Yes
Bracket × year × province FE	No	No	Yes	No	No	Yes
Mean dependent (1,000€ or #)	16,429	16,429	16,429	122	122	122

Note: This table presents the impact of introducing a progressive local tax scheme on taxable income and population stock of taxpayers in the top income bracket, compared to the bracket just below. Standard errors in parenthesis, with three-way clustering by municipality × income bracket, income bracket × year and municipality × year.

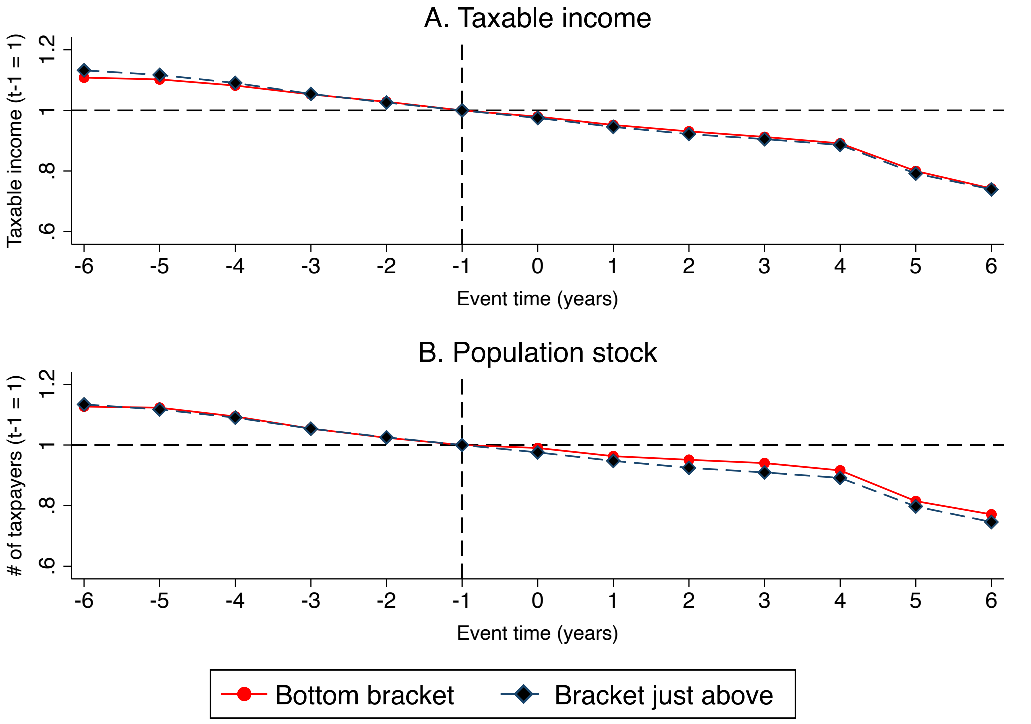
Other figures and tables

Figure A12: Heterogeneous effect by population size



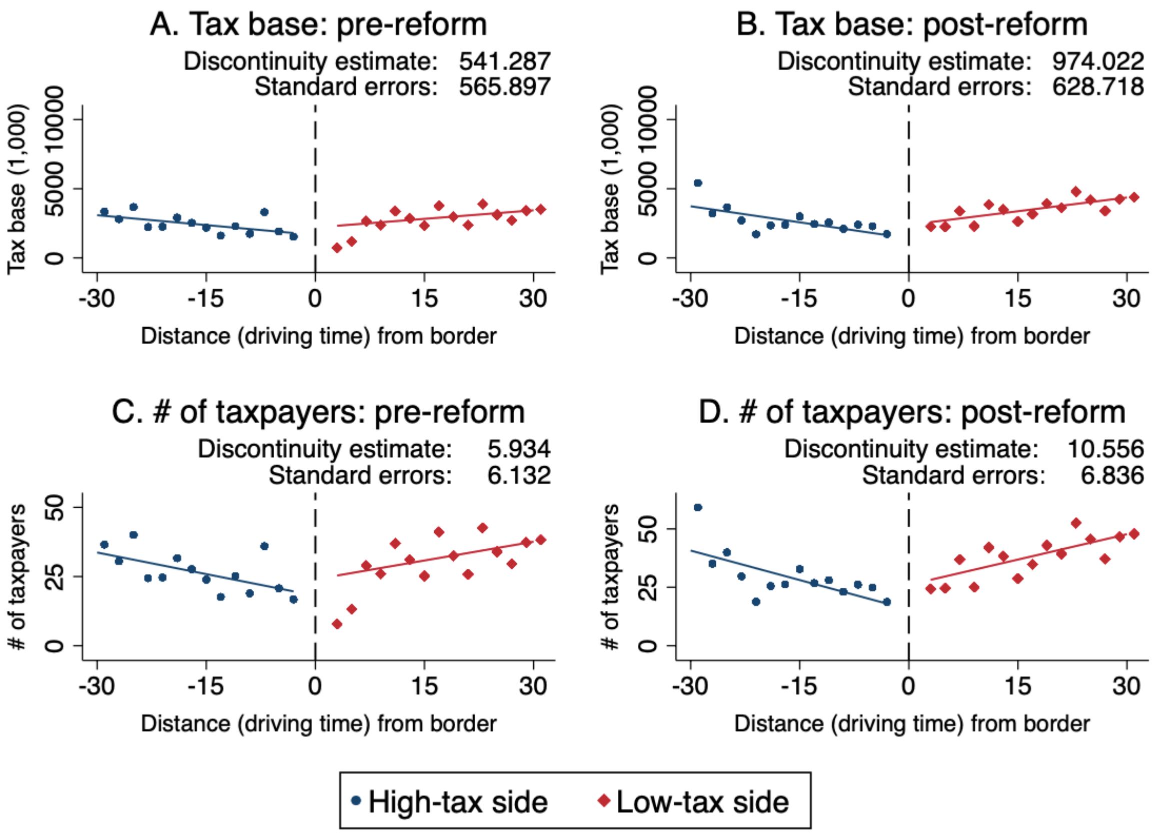
Note: The figure depicts point estimate (central line) and 95 confidence intervals (lateral lines) of the effect of switching from a flat to a progressive local income tax schedule on the tax base in each quintile of the population size of a municipality. The sample includes 7,918 municipalities over the 2001-2015 period.

Figure A13: Tax base and population response in bottom brackets



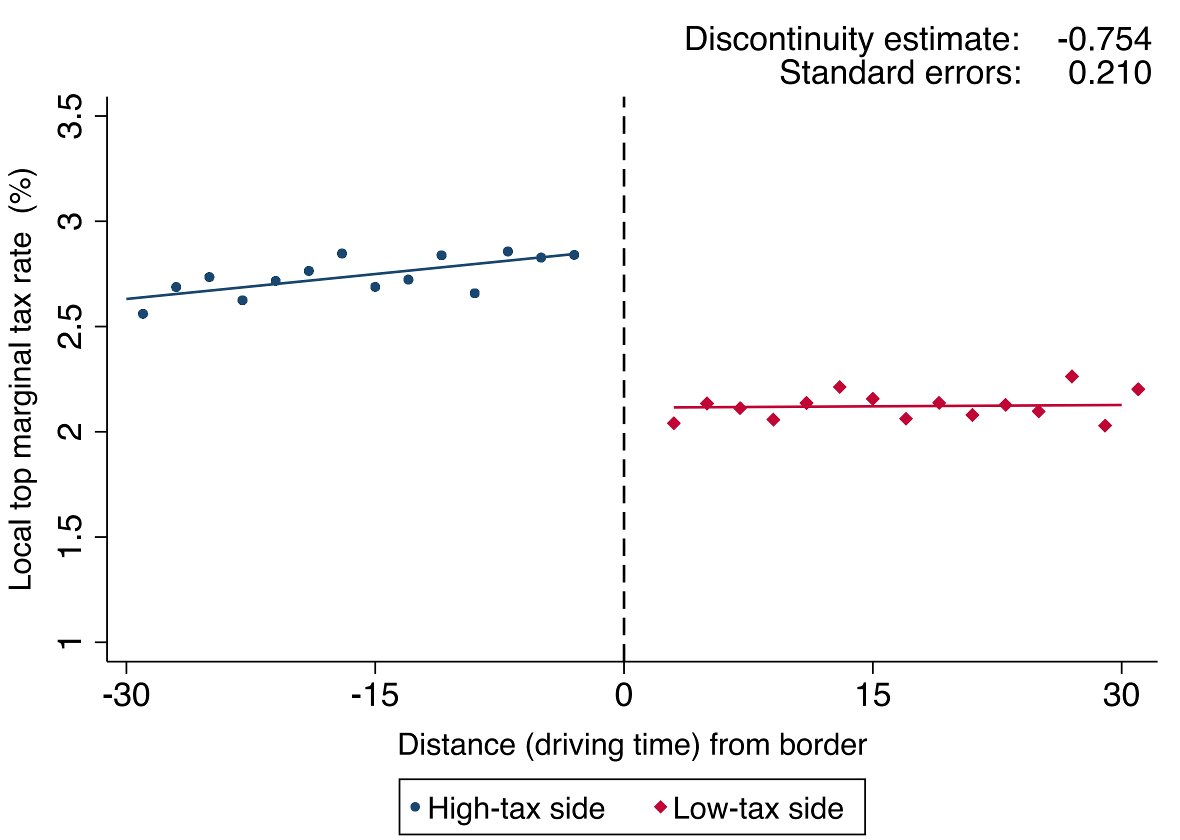
Note: The figure compares the evolution in taxable income (top panel) and number of taxpayers (bottom panel) in the very bottom bracket and in the bracket just above. The dashed vertical line refers to the year before a municipality switched from a flat to a progressive local income tax.

Figure A14: Relocation of the tax base at regional border, bracket below the top



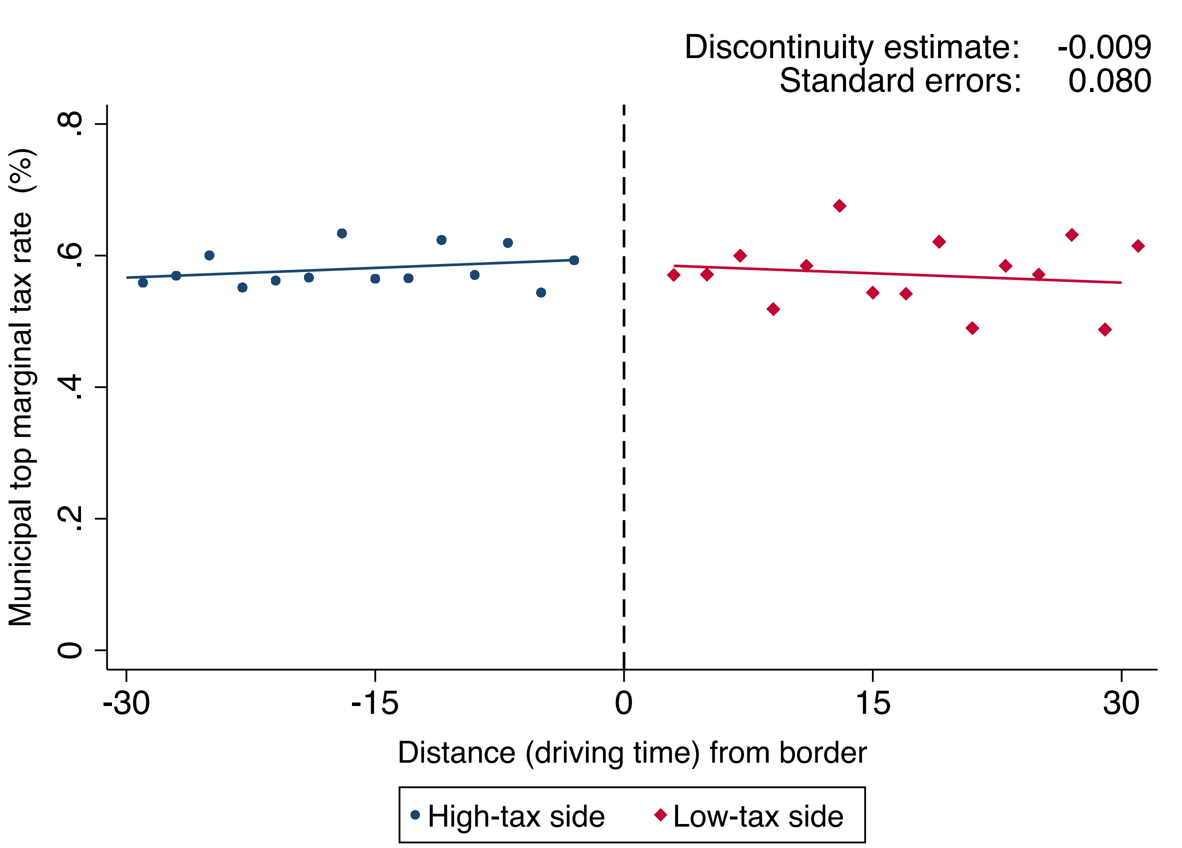
Note: The figure shows the relationship between the differential in the regional (top) tax rate on personal income and mobility. We implement a border discontinuity approach on the sample of municipalities located close to the regional border for two periods: i. the pre-reform (left-hand side graphs), where the tax rate differential was zero or negligible; ii. the post-reform period (right-hand side graphs), where spatial differences began to emerge. The vertical axis in top graphs is the total tax base reported in the bracket just below the top (in 2015 euros); bottom graphs show the number of taxpayers. The horizontal axis is the algebraic distance (in driving time) of a municipality from the regional border. Scatter points are sample average over intervals of 2-driving time minutes bins. Optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014). We report discontinuity estimate and standard errors with two-way clustering by municipality and regional border pairs standard errors.

Figure A15: Tax rate differential at regional border



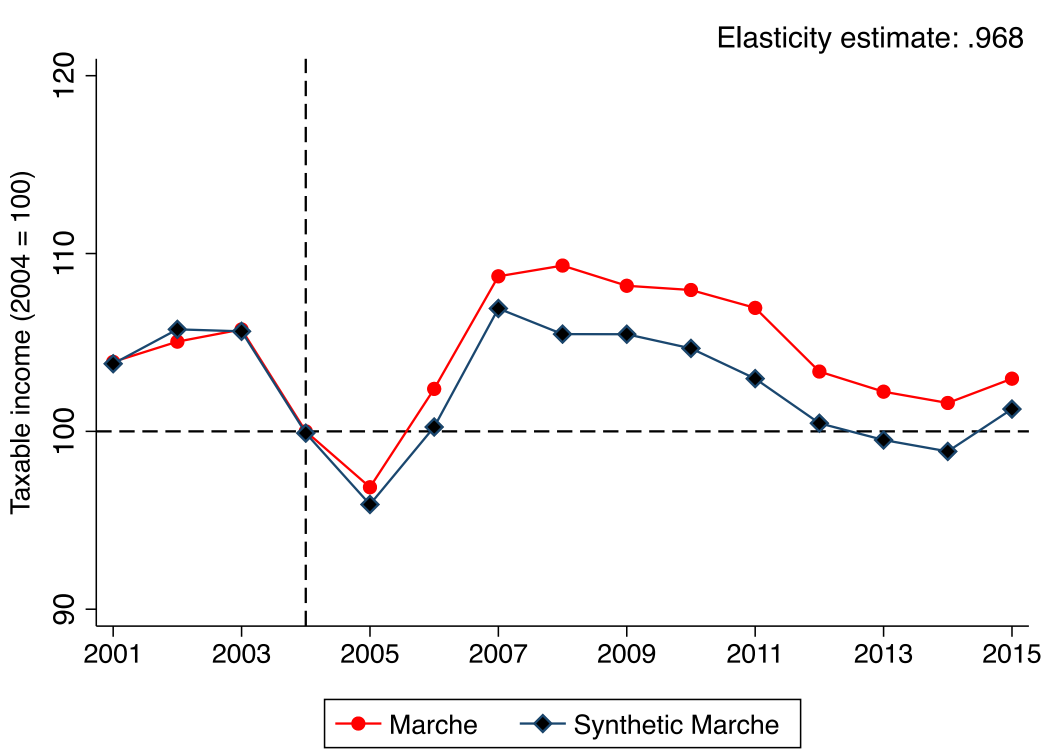
Note: The figure shows the difference in the regional (top) tax rate on personal income (%) at regional border. The horizontal axis is the algebraic distance (in driving time) of a municipality from the regional border. Scatter points are sample average over intervals of 2-driving time minutes bins. Optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014). We report discontinuity estimate and standard errors with two-way clustering by municipality and regional border pairs standard errors.

Figure A16: Municipal tax rate differential at regional border



Note: The figure shows the difference in the municipal (top) tax rate on personal income (%) at regional border. The horizontal axis is the algebraic distance (in driving time) of a municipality from the regional border. Scatter points are sample average over intervals of 2-driving time minutes bins. Optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014). We report discontinuity estimate and standard errors with two-way clustering by municipality and regional border pairs standard errors.

Figure A17: Marche 2005 ex lege tax cut



Note: The figure compares the evolution in tax base in the region of Marche (solid line) and a synthetic control group (dashed line), aiming to resemble the evolution of the tax base in Marche over the pre-reform period. The red vertical line refers to the year before Marche was forced to cut its tax rate from 4 to 1.4 percent. The two series are normalized to match the Marche series in the years before the tax cut. The elasticity is computed as the ratio between the log of the mean tax base change and the log of the net-of-tax rate change = log(102.620/100)/log((100-1.4)/(100-4)). Root Mean Squared Prediction Error = .460. The synthetic control is mostly composed of Piedmont, Basilicata, and Molise regions.

Table A7: Border discontinuity approach

	log(tax base)		log(population stock)	
	BD	BD-FE	BD	BD-FE
	(1)	(2)	(3)	(4)
$1(LowTaxSide_i)$	0.155*	0.097***	0.137*	0.094***
	(0.089)	(0.024)	(0.079)	(0.018)
Observations	3,548	8,870	3,548	8,870
Municipality FE	No	Yes	No	Yes
Year FE	No	Yes	No	Yes
Mean dependent (1,000€ or #)	4,768	4,768	23	23
Elasticity	9.406	1.402	10.403	2.635
	(3.820)	(1.421)	(3.378)	(0.832)

Note: This table reports the discontinuous change in log of tax base (columns 1 and 2) and log of population stock (columns 3 and 4) in the top bracket as one crosses from high-tax to low-tax side of the regional border. Each specification controls for the distance from the regional border at both sides. The optimal bandwidth (driving time from the border) is computed using the selection criterion proposed by Calonico et al. (2014). Column (1) and (3) report estimates only relative to the post-decentralization period, while columns (2) and (4) are based on the full sample period and allow to control for municipality and year fixed effects to absorb any pre-existing difference in the outcome over the period before tax rate differentials began to emerge. We weight observations by the difference in the regional tax rate differential, so to weight more heavily observations where the net-of-tax income gains would be larger as one crosses the regional border. Standard errors with two-way clustering by municipality and regional border pairs in parenthesis.

Table A8: Local income taxation and transfers of tax residence, 2SLS model

	Outcome: log of outmigration odds-ratio						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. *Tax rate:* 99th percentile average tax rate						
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	9.159***	2.650**	3.996**	5.349***	6.882***	3.223	6.242***
	(1.056)	(1.125)	(1.559)	(1.749)	(1.898)	(3.646)	(1.886)
	B. *Tax rate:* top marginal tax rate						
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	7.061***	2.049**	2.858**	3.716***	4.745***	2.273	4.260***
	(0.824)	(0.871)	(1.116)	(1.216)	(1.311)	(2.567)	(1.284)
Orig-dest pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Group trend	No	No	Yes	Yes	Yes	Yes	Yes
Sp and pr tax controls	No	No	No	Yes	Yes	Yes	Yes
Origin pr x year FE	No	No	No	No	Yes	No	No
Destination pr x year FE	No	No	No	No	No	Yes	No
Region pair x year FE	No	No	No	No	No	No	Yes

Note: This table presents the effect of net-of-average tax rate differential on the probability of transferring the tax residence by instrumenting tax rate changes by a dummy for the post-2011 period. Our outcome is the outmigration odds-ratio: the probability of an individual moving from an origin province to a given destination province relative to the probability of not moving at all. Our tax rate measure is the average tax rate computed at 99th percentile in the top panel, and the top marginal tax rate in the bottom panel. The sample includes 4,549,111 transfers of residence moving within 11,449 province pairs 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.

Table A9: Migration elasticity using the top marginal tax rate

	log of outmigration odds-ratio					
	(1)	(2)	(3)	(4)	(5)	(6)
$log[(1-\tau_{d,t})/(1-\tau_{o,t})]$	4.508***	1.589*	2.231***	2.676***	2.624***	3.149***
	(0.758)	(0.830)	(0.843)	(0.878)	(0.834)	(0.874)
Origin-Destination pair FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Group trend	No	Yes	Yes	Yes	Yes	Yes
Spending and pr tax controls	No	No	Yes	Yes	Yes	Yes
Origin province x year FE	No	No	No	Yes	No	No
Destination province x year FE	No	No	No	No	Yes	No
Region pair x year FE	No	No	No	No	No	Yes

Note: This table presents the effect of net-of-top marginal tax rate differential on the probability of transferring the tax residence. Our outcome is the outmigration odds-ratio: the probability of an individual moving from an origin province to a given destination province relative to the probability of not moving at all. The sample includes 4,549,111 transfers of residence moving within 11,449 province pairs 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.

B Appendix for Tax Enforcement, Public Spending and Tax Rates: Evidence from the Ghost Buildings Program

B1 The Ghost Buildings program

Identification procedure. In the following, we describe the procedure implemented by *Agenzia del Territorio* to detect buildings not registered on the land registry maps and thus missed from the tax base. We graphically show this procedure in Figure B1 and summarize this process in detail below:

- Step 1: Take high-resolution satellite images of the country;
- Step 2: Point out any area covered by physical objects by using of ground and surface's altimetric models;
- Step 3: Distinguish vegetation (in green) from objects (in red) by exploiting light frequency data within the short-wave infra-red spectral range;
- Step 4: Remove (untaxed) vegetation;
- Step 5: Compare red objects (i.e., buildings emerging from satellite data) with the cadastral maps' vectorial cartography.
- Step 6: Any object existing in satellite data but not in cadastral maps will be reported as a red ball;
- Step 7: The exact location (address) of each red ball (i.e., ghost building) will be communicated to local administrators;
- Step 8: Local administrators will be in charge to:
    1. disseminate information about the ghost buildings;

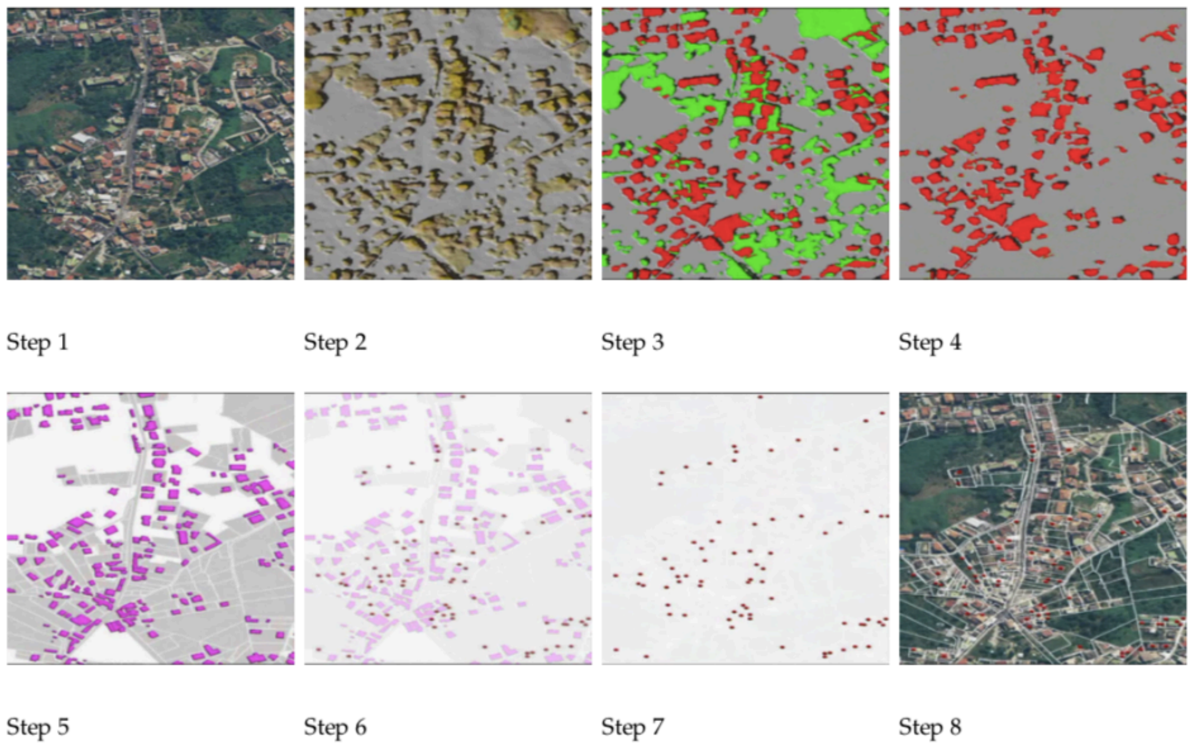
2. proceed, with the support of municipal police, to follow-up inspections and imputation of the tax base of properties not voluntarily registered;
3. collect overdue taxes;
4. check whether the building was conform with the City Plan and local zoning restrictions.

Using this technique, the *Agenzia del Territorio* detected 2.238 million ghost buildings, including commercial, industrial, and residential stand-alone buildings, as well as any unreported extension of previously registered buildings. Among these, over 1.260 millions of buildings required to be reported to the registry and thus were missed from the tax base. The following buildings do not enter the tax base and are not required to be registered: i) buildings that are incomplete; ii) buildings that are particularly degraded; iii) solar collectors; iv) greenhouses; v) henhouses or other buildings reserved for animals (*Decreto Ministero delle Finanze, 2 Gennaio 1998*, n. 28, Art. 3).

Comparison with other measures of tax evasion. To validate this indicator as a proxy for tax evasion, Figure B3 compares ghost buildings intensity (y-axis) with two regional-level estimates of the tax gap (x-axis).[26] The left-hand side graph compares ghost buildings intensity with Galbiati and Zanella (2012)'s estimate of the tax gap, which uses tax audits data on self-employed individuals (small individual businesses, including farmers and professionals) in the late 1980s. The right-hand side graph relates ghost buildings intensity with a measure of evasion developed by Carfora et al. (2018) by using data from the Italian Internal Revenue Agency over the 2001-2011 period and calculating the tax gap as the ratio between potential and actual tax revenue. The figure shows that both these two proxies for tax evasion are positively associated with the ghost buildings indicator (coefficients of correlation equal to 0.57 and 0.62).

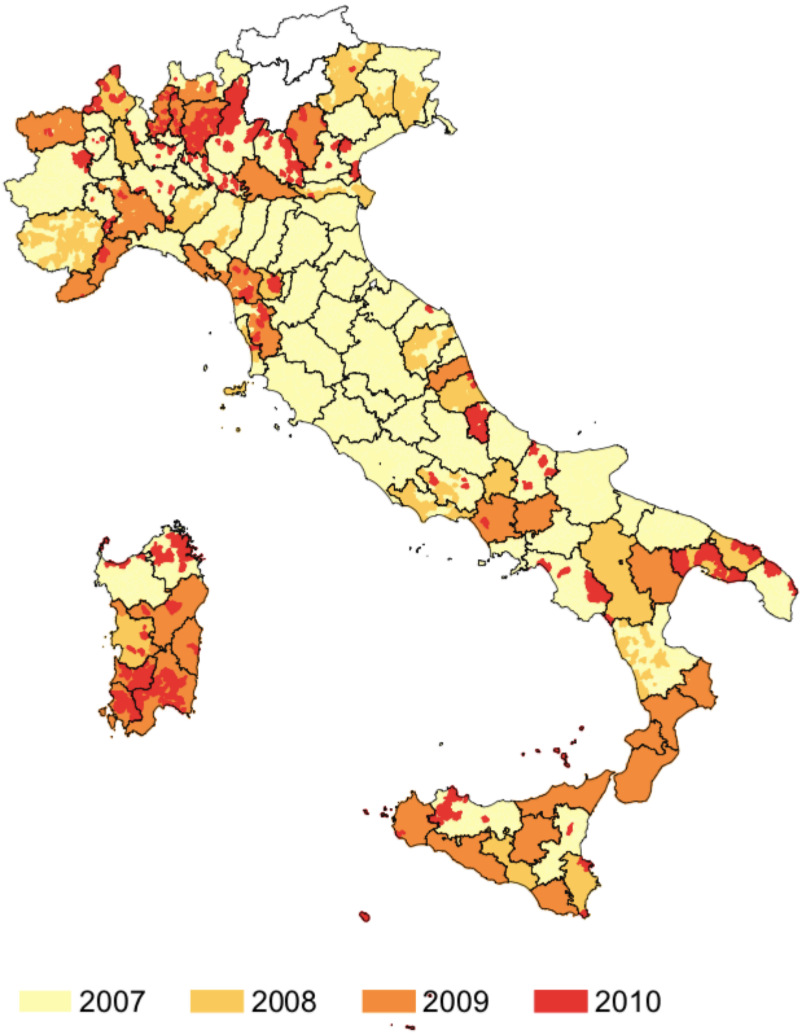
[26] In this figure, the ghost building intensity indicator is computed as the municipal population-weighted regional average.

Figure B1: Identification process for ghost buildings



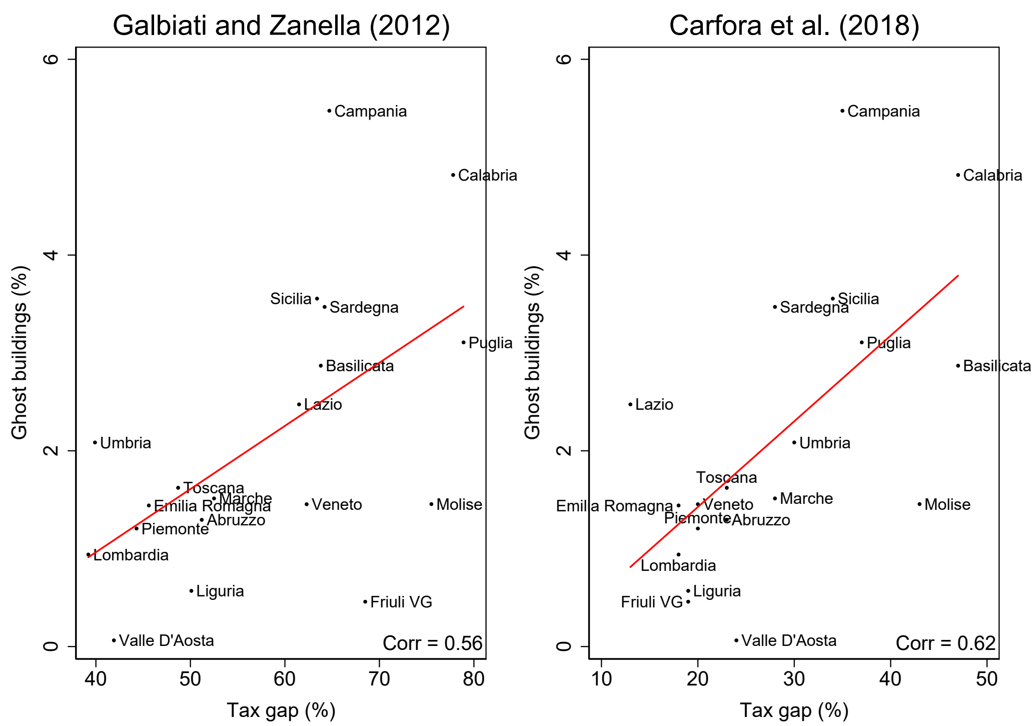
Note: See the text for details.

Figure B2: Timing of program inception



Note: This figure depicts the Ghost Buildings program inception year. Missing values are imputed with the provincial average. Trentino Alto-Adige region (the white area in North-East) did not participate into the program. The exact dates are the following: August 2007; October 2007; December 2007; December 2008; December 2009; September 2010. Data from the Italian Internal Revenue Agency.

Figure B3: Comparison with other estimates of tax evasion



Note: This figure compares the ghost building intensity indicator (y-axis) with regional-level measures of the tax gap (x-axis) as computed by Galbiati and Zanella (2012) and Carfora, Pansini Vega and Pisani (2018). The ghost building indicator is the municipal population weighted regional average.

Table B1: The Ghost Buildings program

Building type	# of registered buildings	Total rental value (euros)	Average rental value (euros)
Residential	446,093 (35%)	181.337.943 (22%)	407
Warehouse	395,482 (31%)	60,447,057 (7%)	153
Garage	215,601 (17%)	28,887,614 (3%)	134
Other	203,920 (16%)	554,592,000 (67%)	2,721
Total	1,261,096	825,624,614	655

Note: This table presents information on the type of buildings detected by the Ghost Buildings program and subject to registration requirement. Data from the Italian Internal Revenue Service (*Agenzia delle Entrate*).

B2 Data and results appendix

Table B2: The impact of the Ghost Buildings program on government grants and tax base

	OLS	OLS	OLS	OLS	2SLS	2SLS & DDD
	(1)	(2)	(3)	(4)	(5)	(6)
			log of government grants			
$Post_{i,t} \times GB_i$	-2.607***	-1.775***	-1.560***	-1.915***	-1.618***	-1.555***
	(0.516)	(0.514)	(0.512)	(0.529)	(0.549)	(0.551)
			log of tax base			
$Post_{i,t} \times GB_i$	0.629***	0.672***	0.237***	0.378***	0.387***	0.382***
	(0.068)	(0.068)	(0.050)	(0.065)	(0.067)	(0.067)
Observations	115,635	115,635	115,635	115,635	115,635	119,670
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes
Controls × Post	No	No	No	Yes	Yes	Yes
Province × year FE	No	No	No	Yes	Yes	Yes
Election year × year FE	No	No	No	Yes	Yes	Yes

Note: This table shows the effect of the Ghost Buildings program on log of government grants (top panel) and log of tax base (bottom panel). $Post_{i,t} \times GB_i$ is the interaction between a dummy for the (municipality-specific) post-program period and the share of ghost buildings detected in a municipality. Column (5) reports estimates from an instrumental variable approach where the post-program dummy is instrumented by the provincial modal year of the program inception year. Column (6) combines the 2SLS approach with a triple difference approach that exploits the fact that one region did not participate into the program. The sample covers 7,709 municipalities (7,978 in last column) over the 2001-2015 period. First-stage coefficient is 0.958 (0.007). Standard errors clustered at municipality-level.

Table B3: Validity of Difference-in-discontinuity

	Pop	For	Mayor			Town council		
			female	college	age	female	college	age
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Post_{i,t} \times D_i$	-0.010	-0.001	-0.037	-0.004	1.971	0.007	-0.020	0.420
	(0.008)	(0.002)	(0.042)	(0.072)	(1.410)	(0.011)	(0.015)	(0.429)
Observations	549	549	549	549	549	549	549	549
... $\times GB_i$	-0.308	-0.003	-0.720	-1.355	34.526	0.016	-0.299	2.685
	(0.185)	(0.039)	(0.665)	(1.175)	(27.719)	(0.193)	(0.336)	(10.620)
Observations	549	549	549	549	549	549	549	549

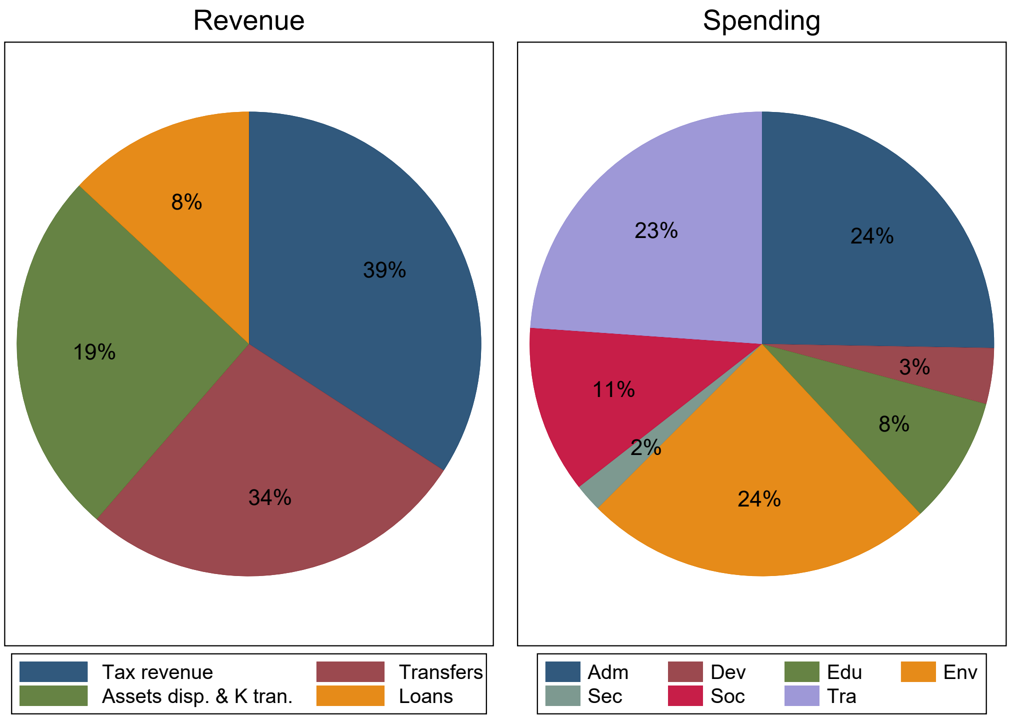
Note: This table reports difference-in-discontinuity estimates of the impact of the Ghost Buildings program on the difference in population (column 1), share of foreign residents (column 2), mayor sex (column 3), mayor education (column 4), mayor age (column 5), share of women in town council (column 6), share of college degree in town council (column 7), average age in town council (column 8) across municipalities below and above the cutoff defining eligibility for fiscal rules. We run local linear regressions with optimal bandwidth estimated following Calonico et al. (2014). Standard errors clustered at municipality-level.

Table B4: Effects on other items of public spending

	log of per-capita spending on:								
	Police (1)	Culture (2)	Sport (3)	Tourism (4)	Transp. (5)	Envir. (6)	Social (7)	Develop. (8)	Services (9)
$Post_{i,t} \times GB_i$	2.252***	-0.517	0.186	-1.038	-0.295	0.090	-0.050	-0.673	-0.109
	(0.630)	(0.601)	(0.603)	(0.595)	(0.362)	(0.371)	(0.553)	(0.549)	(0.739)
Observations	115,635	115,635	115,635	115,635	115,635	115,635	115,635	115,635	115,635
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election year × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS

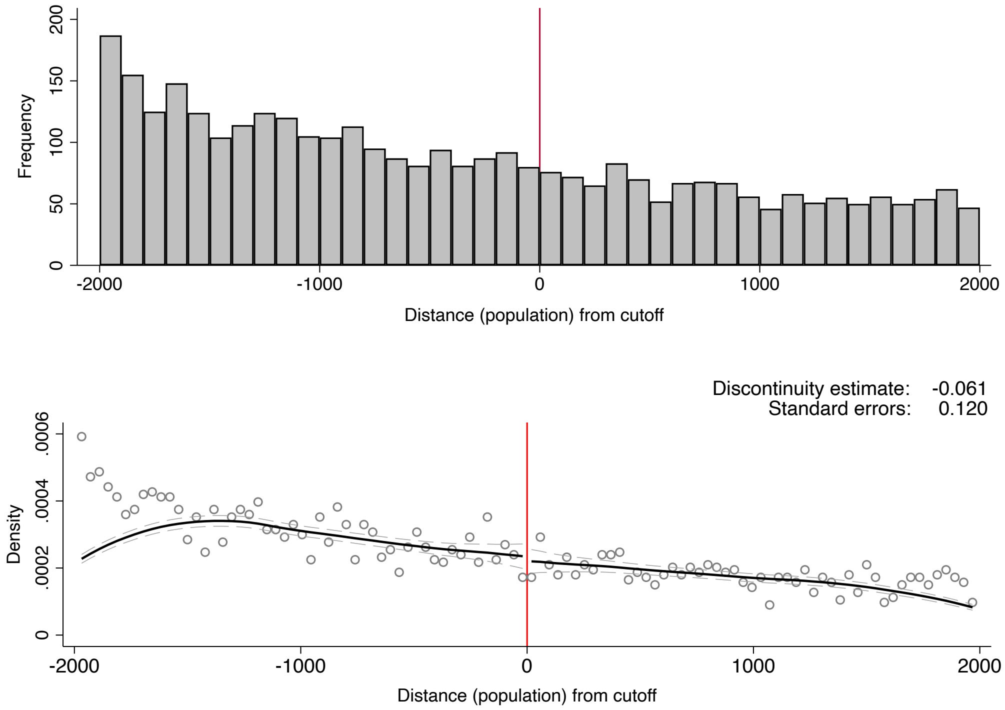
Note: This table shows the effect of the Ghost Buildings program on the log of municipal spending per-capita on police, culture, sport activities, tourism, transportation, environment and waste disposal, social activities, development activities, and services. $Post_{i,t} \times GB_i$ is the interaction between a dummy for the (municipality-specific) post-program period and the share of ghost buildings detected in a municipality. The sample covers 7,709 municipalities over the 2001-2015 period. Standard errors clustered at municipality-level.

Figure B4: Composition of municipality revenue and spending



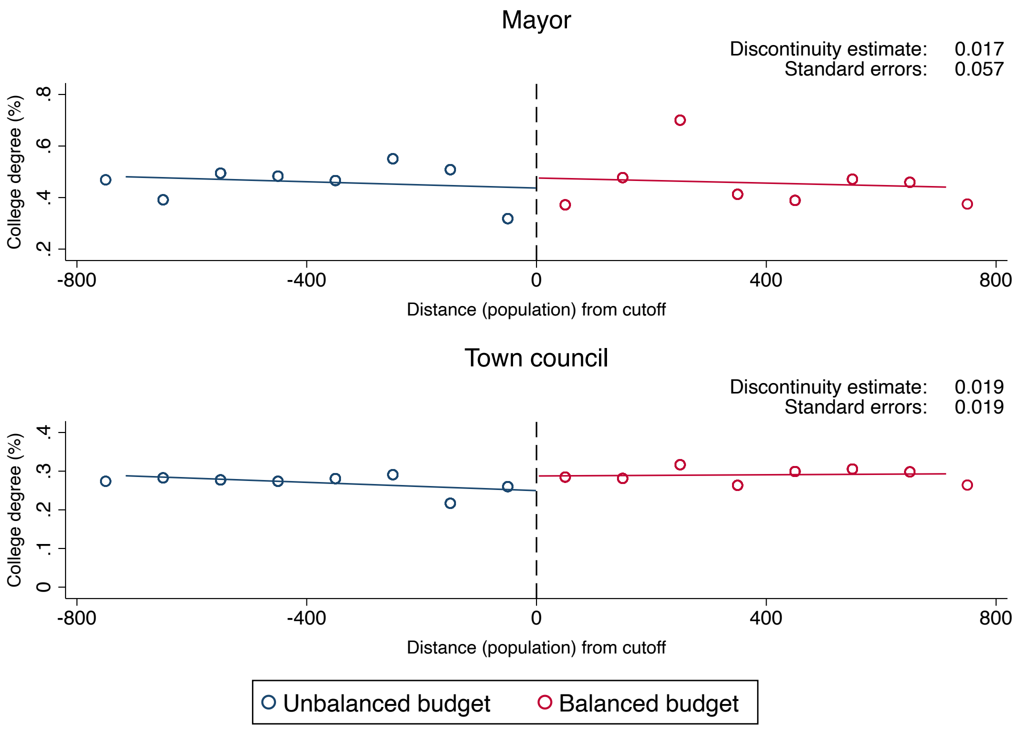
Note: This figure depicts the composition of municipality revenue (left-hand side graph) and investment spending (right-hand side graph). The following budget items are illustrated: administration ("Adm"); development ("Dev"); education ("Edu"); waste management and environment protection ("Env"); security ("Sec"); social, cultural and sport activities ("Soc"); public transportation and roads ("Tra"). Values are mean values over the 2001-2015 period. These budget items refer to the following 4-digit code in the municipal balance sheets (2014 format): administration (4190); development (4290 and 4357); education (4240); environment protection (4125); security (4225 and 4230); social, cultural and sport activities (4080, 4090, and 4150); public transportation and roads (4110 and 4180). Data from balance sheets of Italian municipalities.

Figure B5: McCrary test



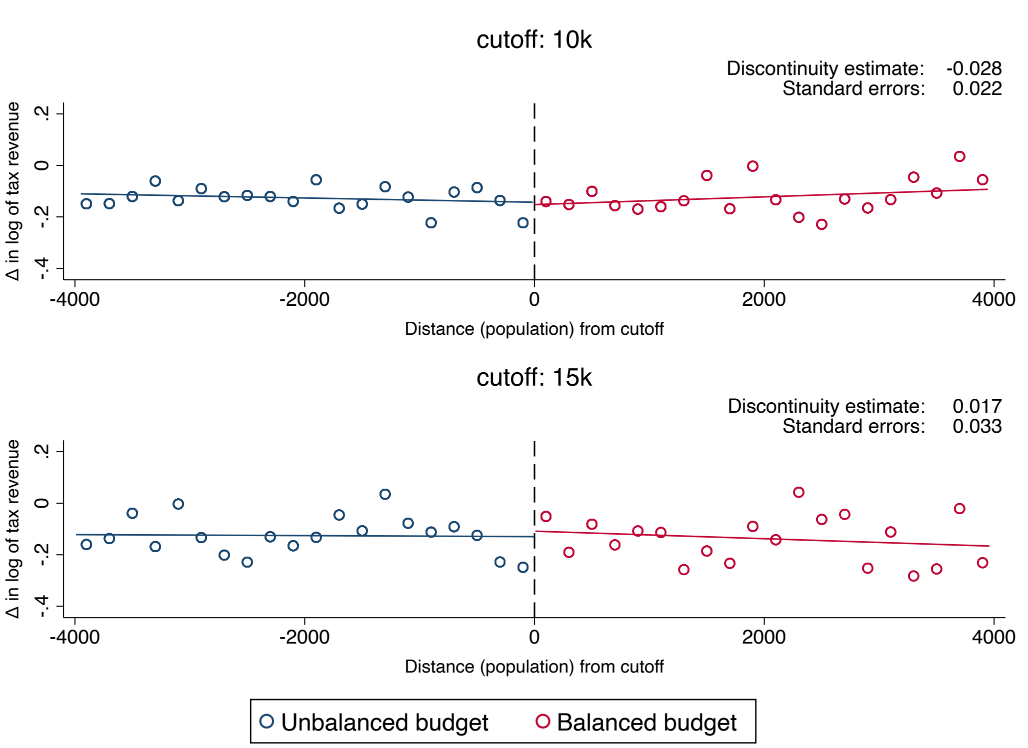
Note: The figure shows the distribution of the municipal population around the eligibility threshold for fiscal rules (red vertical line) in municipalities with population between 3,000 and 7,000 in 2001 and 2011 Census. Circles represent the difference between the municipal population and the 5,000 threshold. Circles are average observed values. The central solid line is a kernel estimate; the lateral lines represent the 95 percent confidence intervals. Discontinuity estimate (standard errors) is -.061 (.120).

Figure B6: Distribution of mayor and local council's ability



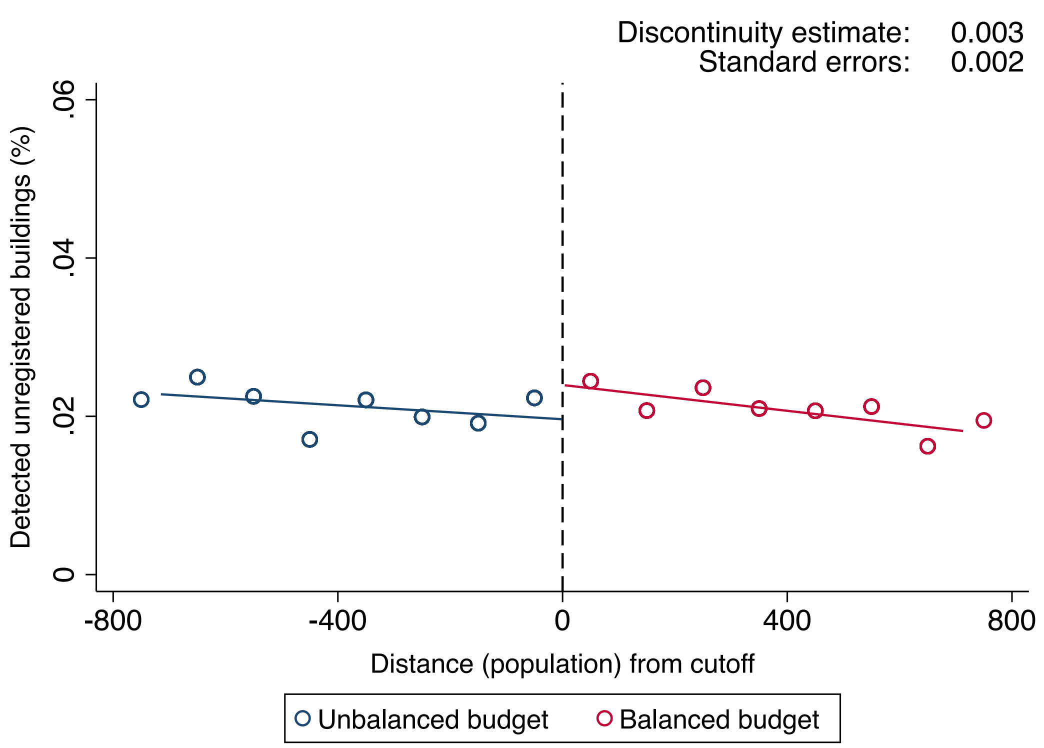
Note: This graph shows the distribution of mayor (top panel) and town council (bottom panel) probability of having a college degree, calculated during the period before the Ghost Buildings program's inception. The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins.

Figure B7: The impact of mayors' salary change



Note: This graph compares the effect of the Ghost Buildings program on tax revenue in municipalities having population size close to 10,000 (top graph) and 15,000 (bottom graph), where mayor' salary changes discontinuously. The horizontal axis is the normalized population size. Scatter points are sample average over intervals of 100 population size bins.

Figure B8: Scope for ghost buildings' registration

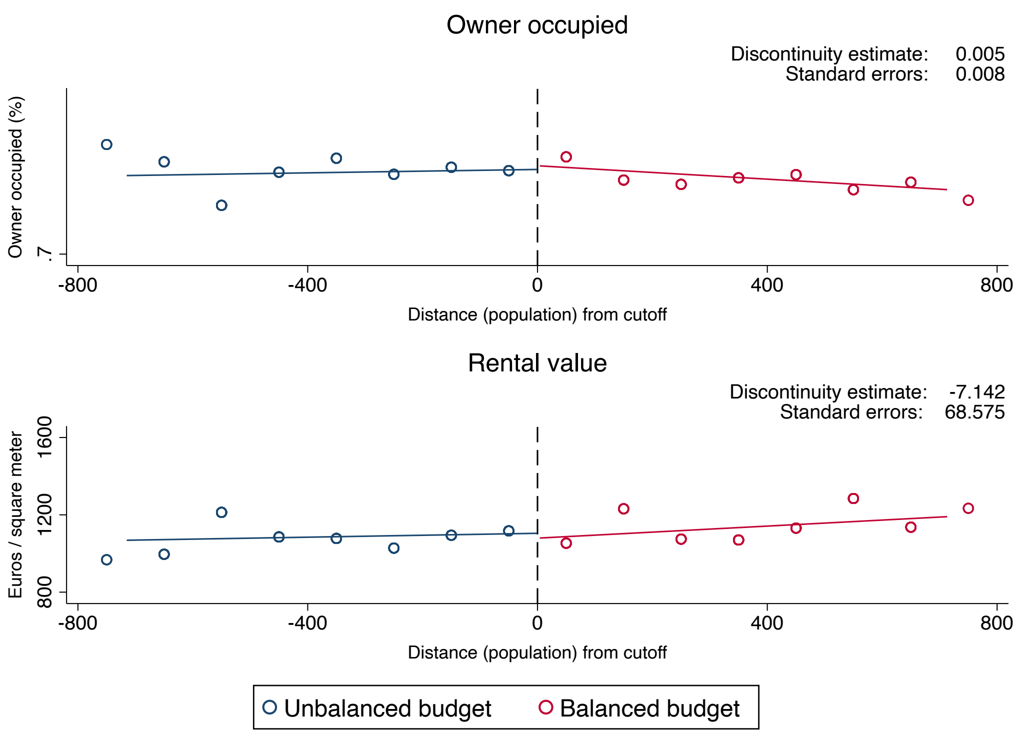


Note: This graph shows the distribution of ghost buildings intensity. The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins.

Figure B9: Distribution of tax rates before the program

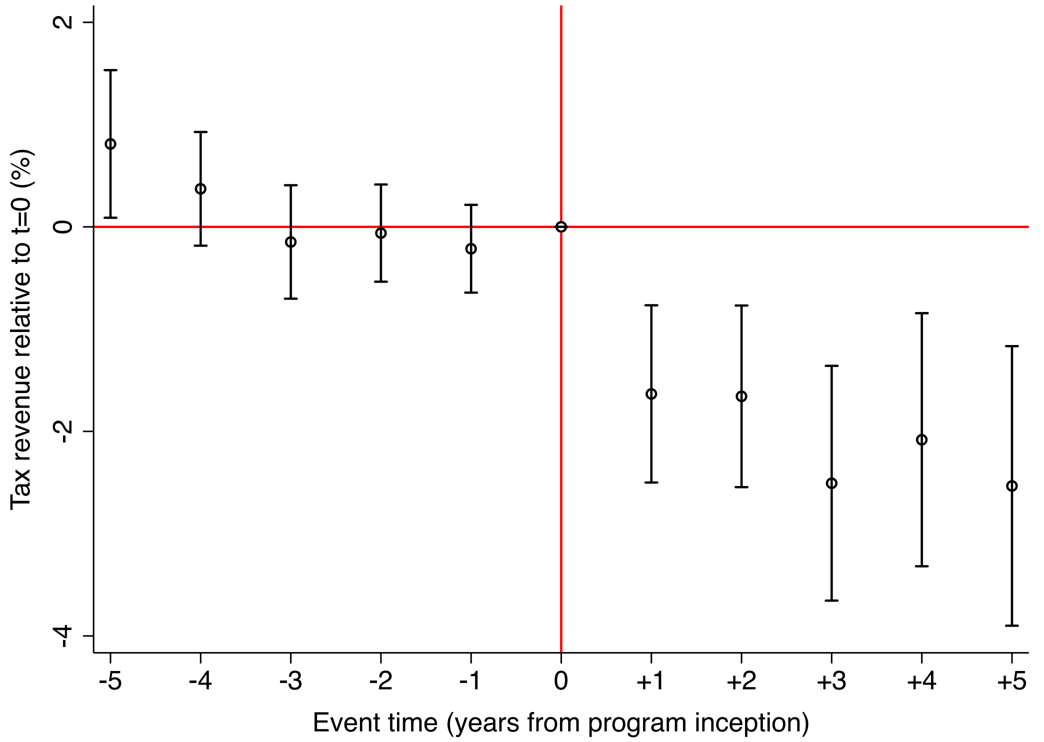
Note: This graph shows the distribution of local tax rates, calculated during the period before the Ghost Buildings program's inception. The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins.

Figure B10: Rental value and buildings' characteristics



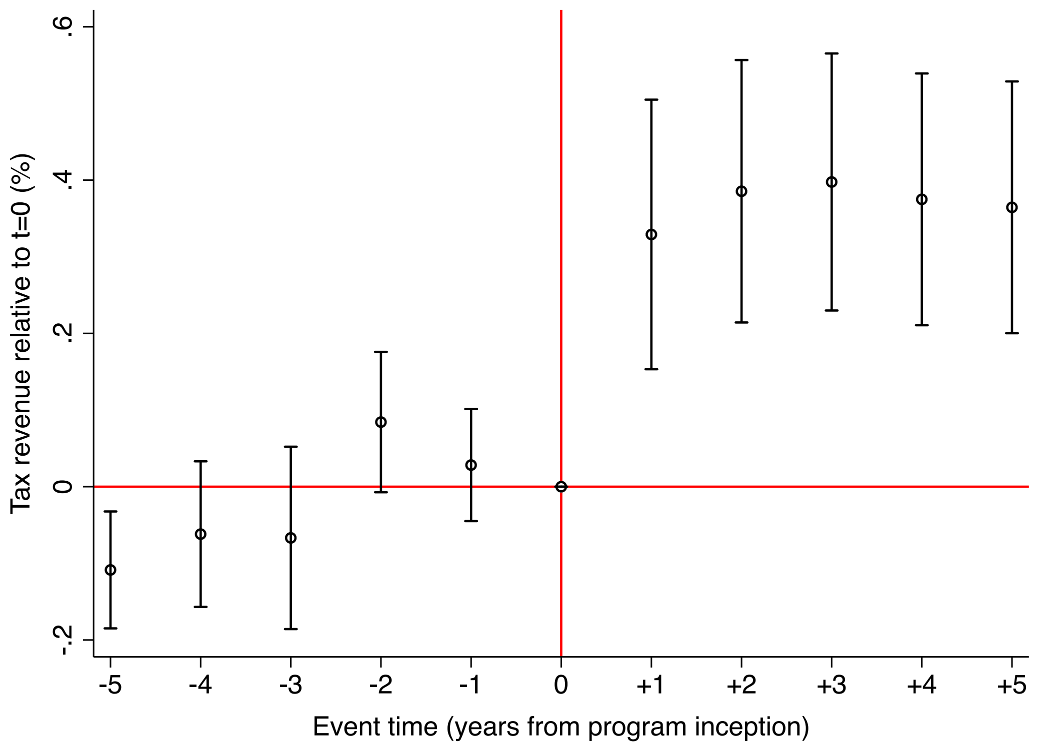
Note: This graph depicts rental value (proxied by the selling price in euros / square meters) and the share of owner occupied buildings. The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins.

Figure B11: The impact of the Ghost Buildings program on government grants



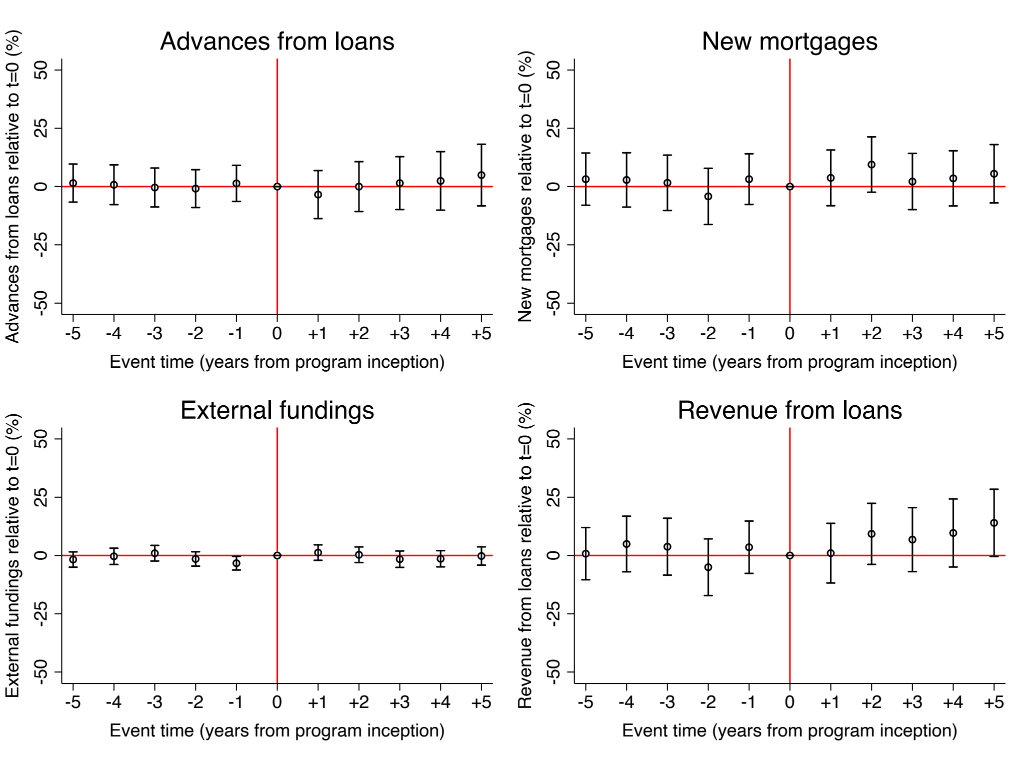
Note: This graph presents the effects of the Ghost buildings program on log of government grants. The figure plots the estimated β_j coefficients from equation (1) and the confidence intervals: each point shows the effect of having implemented the program for j years (if $j > 0$) or of starting the policy j years before (if $j < 0$) relative to the actual program starting year. Standard errors clustered at municipality-level.

Figure B12: The impact of the Ghost Buildings program on tax base



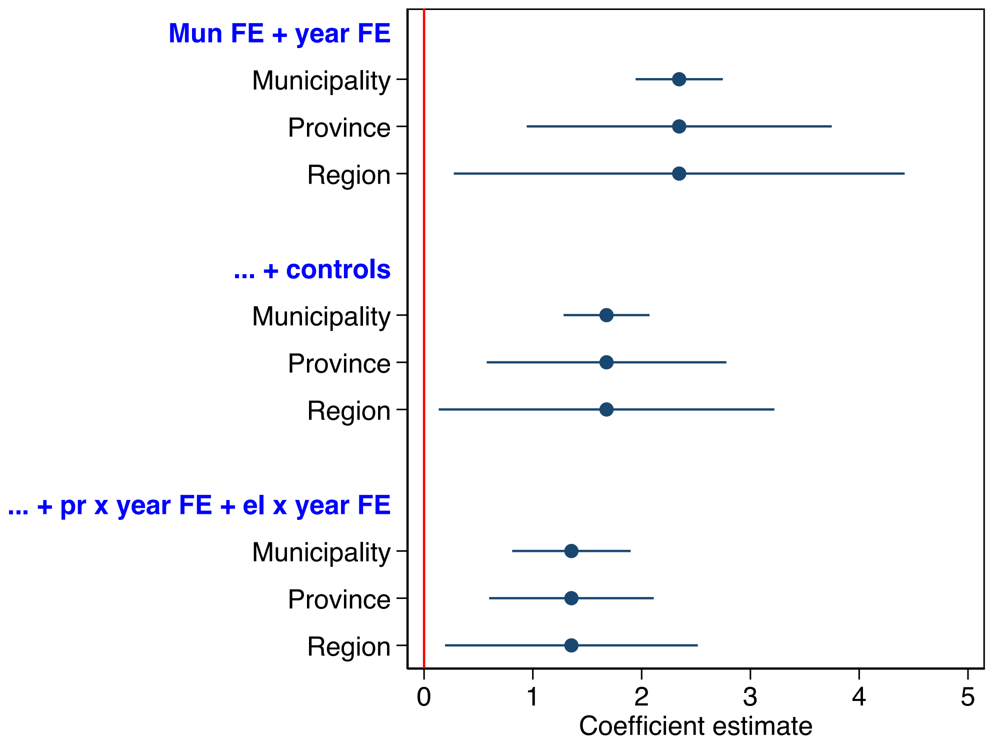
Note: This graph presents the effects of the Ghost buildings program on log of municipal tax base. The figure plots the estimated β_j coefficients from equation (1) and the confidence intervals: each point shows the effect of having implemented the program for j years (if $j > 0$) or of starting the policy in j years (if $j < 0$) relative to the actual program starting year. Standard errors clustered at municipality-level.

Figure B13: The impact of the Ghost Buildings program on other local public finance outcomes



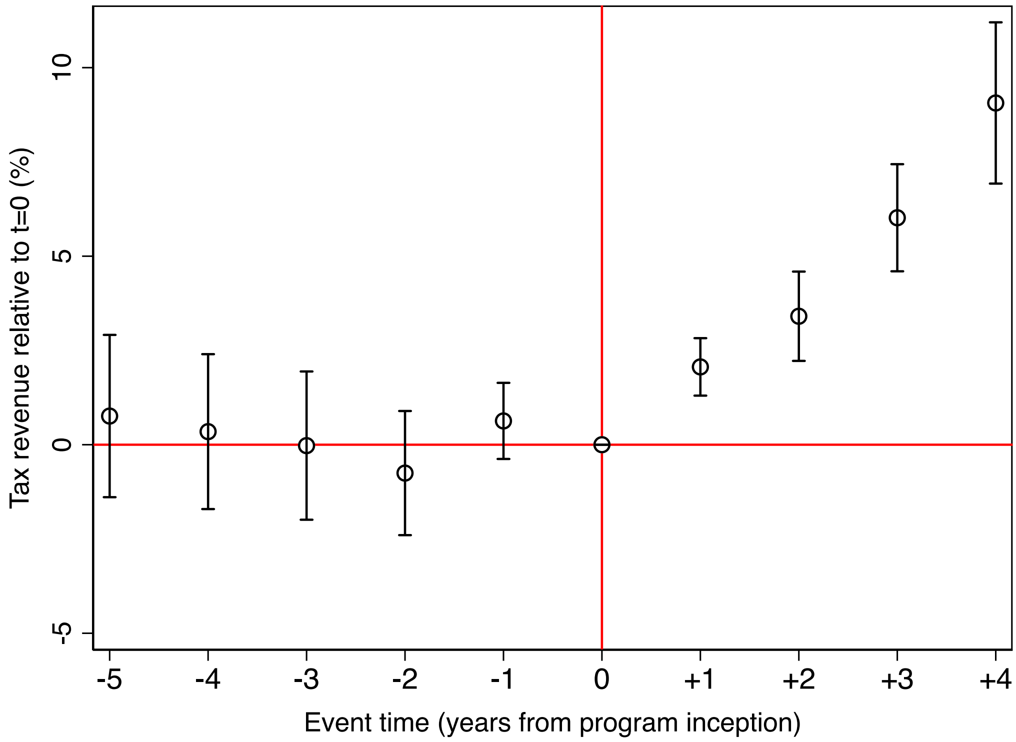
Note: This graph presents the effects of the Ghost buildings program on log of advances from loans; new mortgages; external fundings; loans. The figure plots the estimated β_j coefficients from equation (1) and the confidence intervals: each point shows the effect of having implemented the program for j years (if $j > 0$) or of starting the policy j years before (if $j < 0$) relative to the actual program starting year. Standard errors clustered at municipality-level.

Figure B14: Robustness to clustering choice, tax revenue



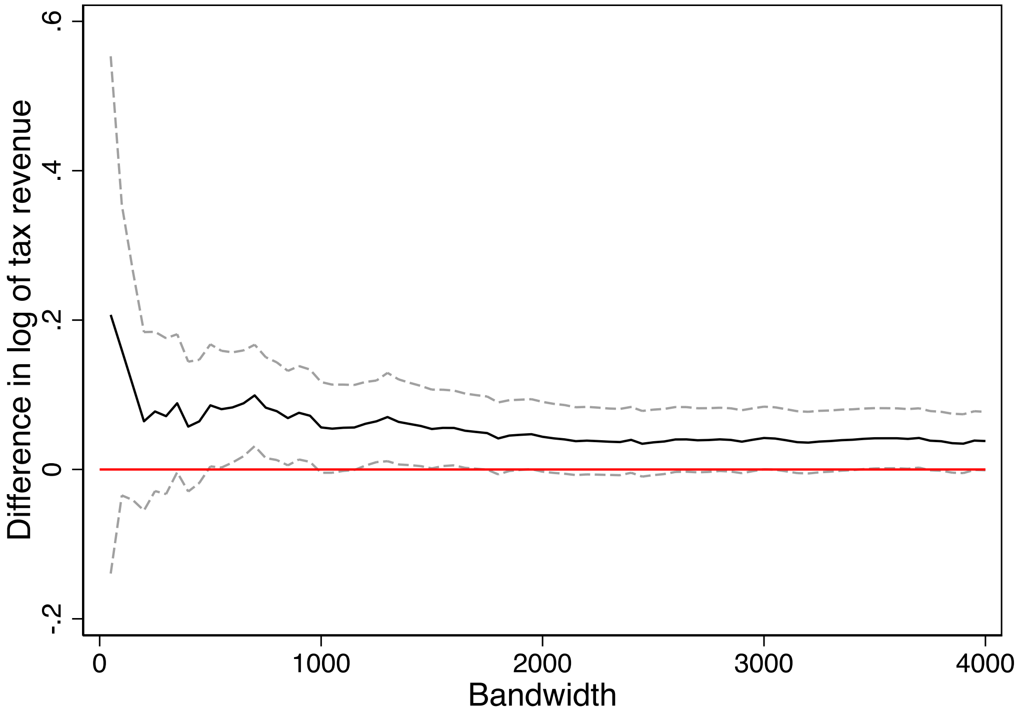
Note: The figure depicts coefficient estimates and 95 percent confidence intervals computed clustering the standard errors at three different levels: i. municipality (as used in the baseline analysis); ii. province; iii. region. We report these estimates for three different empirical models: i. municipality fixed effects and year fixed effects (top panel): ii. + time-varying municipality-level control variables (middle panel); iii. + province-year fixed effects and election year-year fixed effects (bottom panel).

Figure B15: Yearly RD estimates for tax revenue



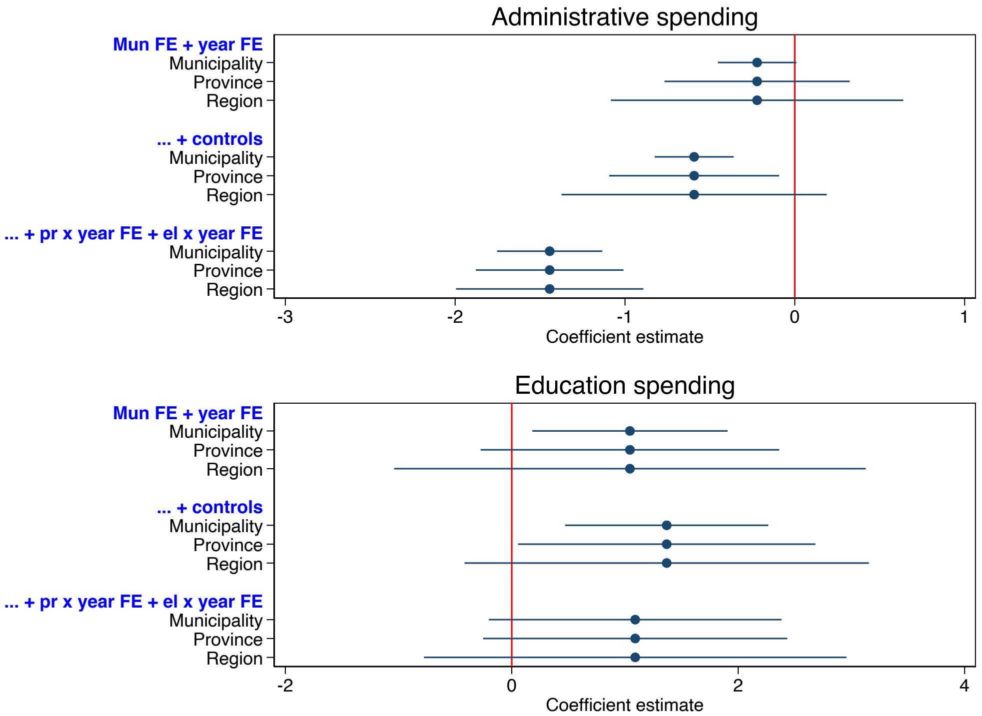
Note: This graph shows the effect of eligibility for balanced budget rules on tax revenue. The figure plots the estimated year-to-year δ_0 coefficient obtained from equation (3) and the confidence intervals: each point shows the effect of having implemented the program for j years (if $j > 0$) or of starting the policy j years before (if $j < 0$) relative to the actual program starting year. Standard errors clustered at municipality-level.

Figure B16: Bandwidth sensitivity



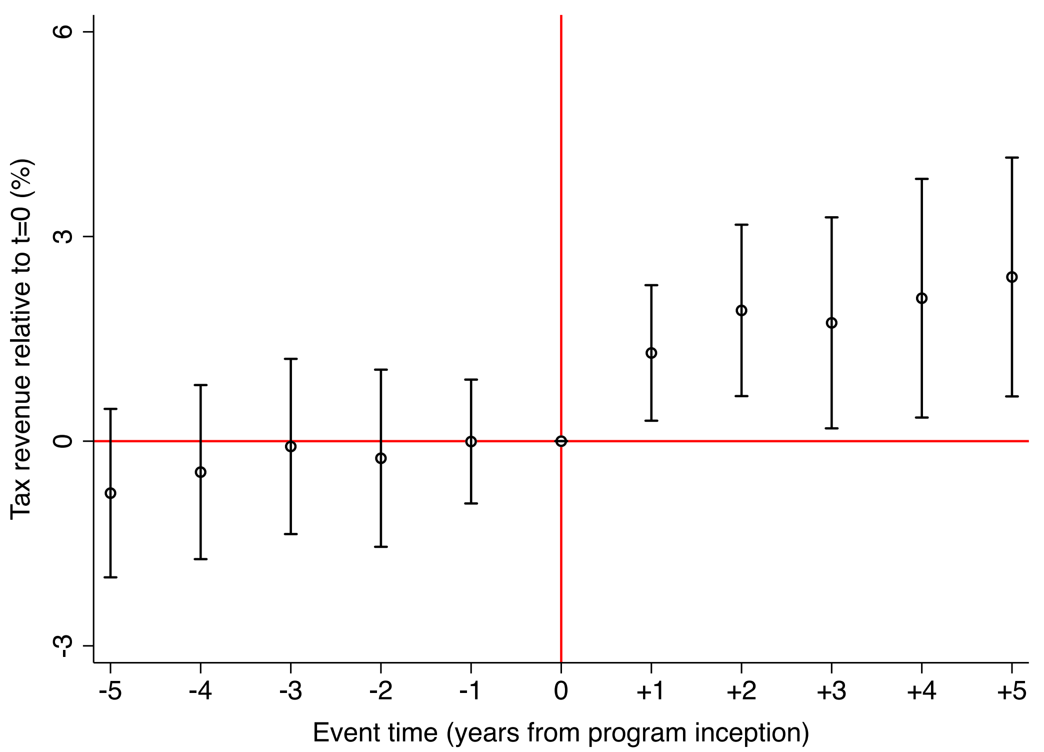
Note: The figure reports difference-in-discontinuities coefficient on the impact of the Ghost Buildings program on the difference in log of tax revenue. The horizontal axis is the bandwidth used to estimate the difference-in-discontinuities coefficient.

Figure B17: Robustness to clustering choice, public spending



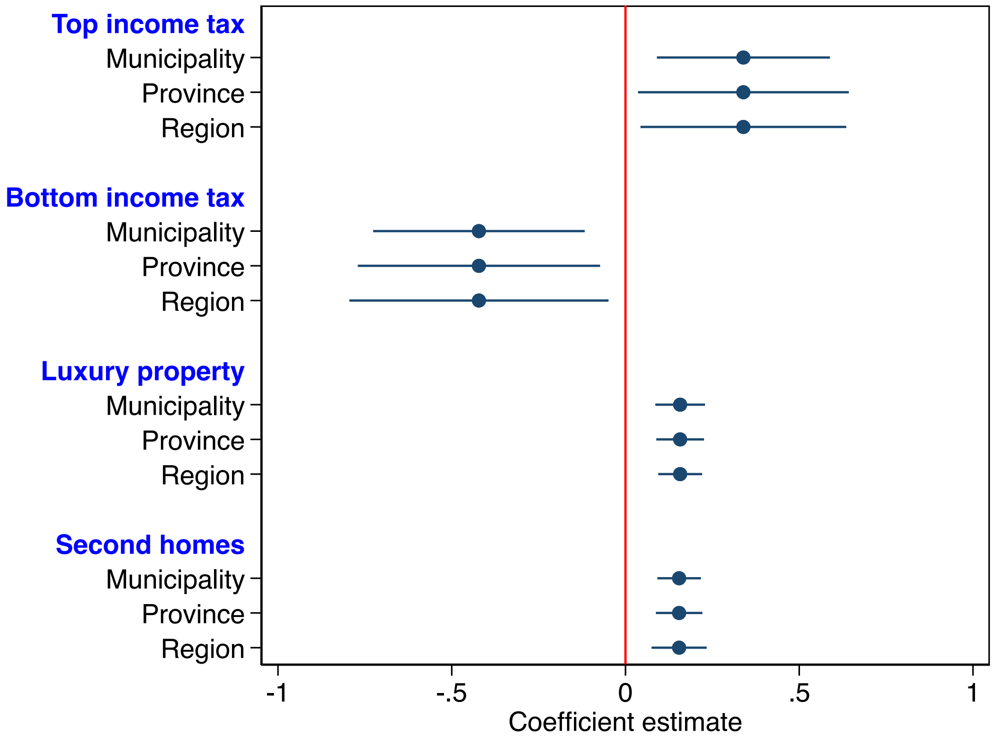
Note: The figure depicts coefficient estimates and 95 percent confidence intervals computed clustering the standard errors at three different levels: i. municipality (as used in the baseline analysis); ii. province; iii. region. We report these estimates for three different empirical models: i. municipality fixed effects and year fixed effects (top panel): ii. + time-varying municipality-level control variables (middle panel); iii. + province-year fixed effects and election year-year fixed effects (bottom panel).

Figure B18: The impact of the Ghost Buildings program on police spending



Note: This graph presents the effects of the Ghost Buildings program on log of municipal police spending per-capita. The figure plots the estimated β_j coefficients from equation (1) and the confidence intervals: each point shows the effect of having implemented the program for j years (if $j > 0$) or of starting the policy j years before (if $j < 0$) relative to the actual program starting year. Standard errors clustered at municipality-level.

Figure B19: Robustness to clustering choice, tax rates



Note: The figure depicts coefficient estimates and 95 percent confidence intervals computed clustering the standard errors at three different levels: i. municipality (as used in the baseline analysis); ii. province; iii. region. Each specification includes municipality fixed effects, time-varying municipality-level control variables, province-year fixed effects and election year-year fixed effects.

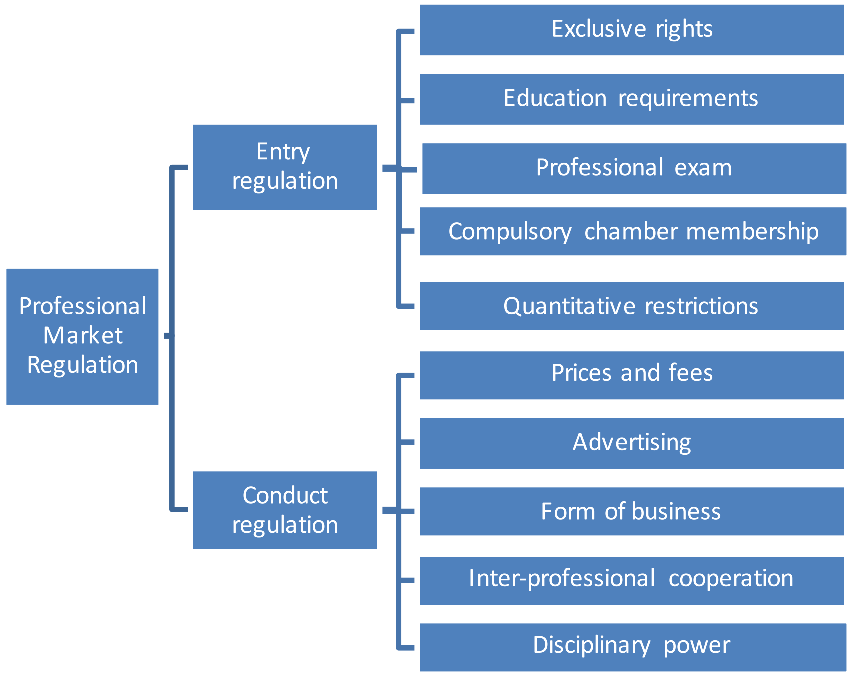
C Appendix for Knocking on Parents' Doors: Regulation and Intergenerational Mobility

Since the 1990s, the OECD has been constructing a system of indicators to measure stringency and ongoing development in product market regulation across the OECD countries. See Nicoletti et al. (1999), Conway and Nicoletti (2006) and Koske et al. (2015) for more details on the spirit of the indicator and on the methodology adopted to turn qualitative data on laws and regulation into quantitative indicators.

Following the OECD methodology, we develop a novel (time-varying) measure of regulation for 14 professions in Italy. Namely, the indicator has a pyramidal structure and it is aimed at summarizing regulations by regulatory domain.

At the top of the pyramid there is the overall regulatory environment of professional services that, in turn, is based on two main broad regulatory domains: the entry requirements into the profession and the regulation of the market behavior (i.e. conduct); these domains, finally, cover different sub-domains regarding specific classes of regulatory interventions, as shown in Figure C1.

Figure C1: Domains and sub-domains considered in the regulation index



For each sub-domain, different questions have been included in the analysis. The answers to these questions are all designed to express the stringency of regulations, from least to most restrictive (along a 0-6 scale), with regard to their impact on market competition. The aggregate indicators are built as mean of the values of the related sub-domains.

With respect to the OECD indicator we innovate along two main dimensions. First, we consider a broader set of professions: accountants, agronomists, architects, biologists, chemists, doctors, engineers, geologists, lawyers, notaries, pharmacists, psychologists, social assistants and veterinarians. Second, we enrich the sub-domains of the regulatory environment along several directions.

For entry regulation we consider the following five sub-topics: exclusive rights (i.e. reserves of activities), education requirements, professional exam, compulsory chamber membership and quantitative restrictions. For conduct regulation we consider the following five sub-topics: prices and fees, advertising, form of business, inter-professional cooperation and disciplinary power. The content of each sub-domain is reported in Table C1. In the following we provide further details and we discuss the main element of novelty.

For **entry regulation**, the first sub-domain concerns exclusive rights (1.1). The ministerial decrees report the reserves of activities for each profession and set the reference price for each of them (either a fixed price or price range). For example, the ministerial decree regarding veterinarians sets the price of an examination of a cat or a dog at 30 euros; with regard to lawyers, the compensation is based on the value of the litigation. In the indicator, we have considered how many groups of similar activities are mentioned in the decrees (1.1.1) and, for each group of activities, we have estimated the value of the most common activities, based on the price set by the decree (1.1.2).[27] For education requirements (1.2), we consider the length of the university degree (1.2.1), whether an undergraduate degree of 3 years enables to register at the chamber (1.2.2),[28] whether the university program which leads to pass the professional exam is free or with limited enrollment (1.2.3), the length of the compulsory practice (1.2.4). With regard to the professional exam, we have considered not only whether it exists or not but also – as a proxy of its difficulty and independence with respect to the local pressure of professional bodies – the number and types of tests it is composed of (1.3.1), the composition of the examining board (1.3.2), the national or local level of organization of the professional exam (1.3.3) and the pass rate (1.3.4).[29] With reference to chamber

[27]The ministerial decrees cover, for each profession, a number of groups of activities varying from two to twelve. In each group, different activities are listed and different value ranges are set. Let's consider the case of notaries. We identify five groups of activities (e.g. real estate deed, corporate deed, inheritances, etc.). For each group of activities, we consider different items. For example, as far as real estate sales are concerned, the compensation of the notary is parameterized with respect to the value range to which the sale belongs. Then, to build the value of the exclusive rights we proceed as follows. First, we select the most common activity within each group of activity. For real estate sales, we consider those in the value range 25,000-500,000 Euros. Second, we compute the compensation of the notary for the average sale in this bracket. Third, we replicate the exercise for other groups of activities. Finally, we get the simple average across the groups of activities as overall indicator of the values of the exclusive rights.

[28]This is a more accurate information than only the length of the university degree, as it allows to consider whether after three years it is possible to register at the chamber and, after that, how many years does the university degree last. People who register at the chamber after an undergraduate degree of 3 years are generally identified as "junior" professionals.

[29]These items have been added using the following assumptions. First, we assume that if the examining board is not composed of professionals the exam would be fairer and less subject to pressure by

membership (1.4), we consider whether it is compulsory or not (1.4.1) – this provides little information, because for all professions such membership is compulsory – but also the costs related to the membership itself (1.4.2). We have calculated the latter for each profession as a mean of the cost of first-five-year membership using information drawn from the websites of the professional bodies in each region's capital.[30] We also include the extent of quantitative restrictions (1.5). Namely, we include not only the quotas for foreign professionals or firms (1.5.1) as done by the OECD, but also whether the running of the business by the professional is subject to quotas within the country (1.5.2), [31] and, if so, to what extent (1.5.3). The latter is measured as the number of inhabitants for each business activities as in some professional activities (notably for notaries and pharmacists) the entry in the market is parameterized to the population following a demographic criterion.

For **conduct regulation**, the first sub-domain is represented by the regulation on prices and fees (2.1). The answer to this question strictly follow the OECD structure. As far as regulation on advertising (2.2) and that on legal form of business (2.3) are concerned, we enrich the answers to have them more tailored to the Italian context. We notably distinguish, on the one hand, the different kinds of advertising (comparative, on the characteristics of the professional and services or on the professional) and, on the other hand, the different legal forms of business that have been introduced in the Italian law (sole proprietorship, partnerships, capital companies) and the existence of restrictions on shareholders for capital companies. The sub-domain of inter-professional cooperation (2.4) has the same questions and answers of the OECD indicator, based on the number of forms of inter-professional cooperation allowed. Finally, we added

incumbents. Second, we assume that if there is a national examining board there is less risk of connections than in a local context. Third, a lower pass rate indicates higher difficulty to enter the profession (data drawn from CRESME).

[30]We consider the first-five-year average as for some professions the costs vary between the first and subsequent years. The average cost of membership varies from about 150 euros per year for social assistants to 1,500 euros per year for notaries

[31]This means that the running of the business is subject to a decision of the public authority (i.e. a license). We also consider whether such license can be inherited by the child of a professional or not.

a novel sub-domain on the disciplinary power (2.5). As a proxy of the effectiveness of such power, failing data on disciplinary proceedings run and penalties imposed by the chambers, we consider whether such power exists and, if so, whether it is entrusted to a specific body, that is deemed more independent, or not.

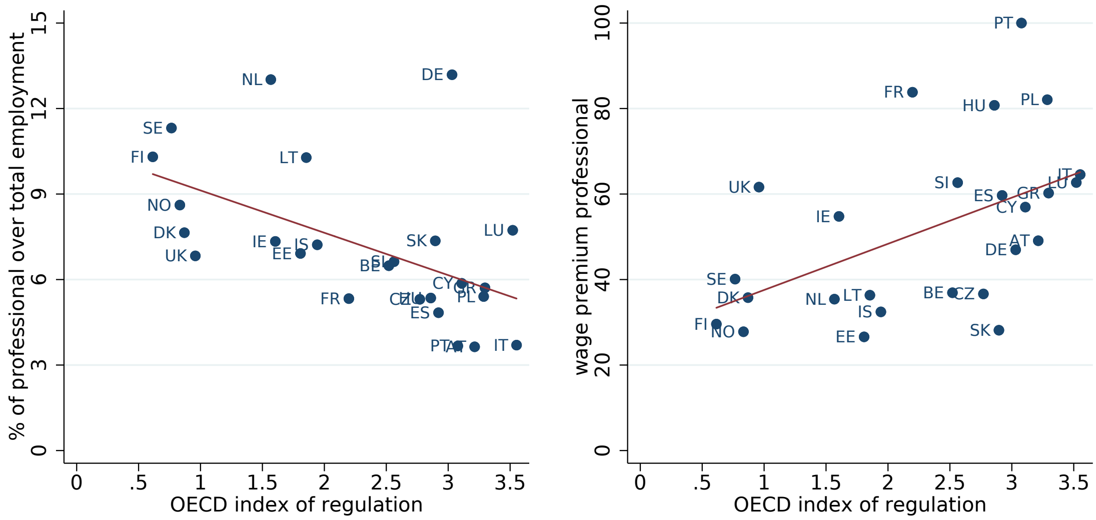
Table C1: Coding of answers and weight of the indicator

		Coding of answers		TW	STW	QW
1.	**Entry**			1/2		
1.1.	*Exclusive rights*				1/5	
1.1.1	How many services does the profession provide under an exclusive right?	Number of reserved activities (*)	0-6			1/2
1.1.2.	What is the average value of the most common reserved activities?	Average value (*)	0-6			1/2
1.2.	*Education and training requirements*				1/5	
1.2.1.	What is the duration of the University degree?	Number of years	0-6			1/4
1.2.2.	Does an undergraduate degree (3 years) enable passing of the professional exam?	Yes	0			1/4
		No	6			
1.2.3.	Is access to University program free or selective?	Percentage of Universities with entry restrictions (*)	0-6			1/4
1.2.4.	What is the length of compulsory practice / postgraduate education?	Number of years	0-6			1/4
1.3.	*Professional exam*				1/5	
1.3.1.	Which tests comprise the professional exam?	There are no professional exams	0			1/4
		One or more oral tests	1			
		A written test	2			
		A written test and an oral test	3			
		Two written tests and an oral test	4			
		Two written tests, an oral test and a practical test	5			
		Three or more written tests and a practical test	6			
1.3.2.	How is the examining board composed?	Mostly by non-professionals	0			1/4
		By members suggested by the chamber (also not professionals)	3			
		Mostly by professionals	6			
1.3.3.	Is the professional exam centralized or organized at local level?	Centralized	0			1/4
		Organized locally and evaluated by non-local examination boards	3			
		Entirely organized at the local level	6			
1.3.4.	What is the pass rate of the professional exam?	Percentage of candidates who pass the exam (*)	0-6			1/4
1.4.	*Compulsory chamber membership*				1/5	
1.4.1.	Is membership in a professional organization compulsory to legally practice?	No	0			1/2
		Yes	6			
1.4.2.	How much is the annual cost of the membership?	Average membership fee (*)	0-6			1/4
1.5.	*Quantitative restrictions*				1/5	
1.5.1.	Is the number of foreign professionals/firms restricted by quotas?	No	0			1/3
		Yes	6			
1.5.2.	Are quantitative restrictions on the number of businesses provided for?	No	0			1/3
		Yes	3			
		Yes with heritability of the business license	6			
1.5.3.	What is the extent of quantitative restrictions?	Strictness (*)	0-6			1/3

		Coding of answers		TW	STW	QW
2.	**Conduct**			1/2		
2.1.	*Prices and fees*				1/5	
2.1.1.	The charged fees or prices are regulated by the government or self-regulated?	No regulation	0			1
		Non-binding recommended prices for some services	1			
		Non-binding recommended prices for all services	2			
		Maximum prices for some services	3			
		Maximum prices for all services	4			
		Minimum prices for some services	5			
		Minimum prices for all services	6			
2.2.	*Advertising*				1/5	
2.2.1.	How is advertising and marketing of professional services regulated?	All kinds of advertising admitted	0			1
		Only advertising on professionals and services admitted	2			
		Only information on professionals admitted	4			
		Forbidden	6			
2.3.	*Form of business*				1/5	
2.3.1.	How is the legal form of business regulated?	Capital companies allowed with no restrictions on shareholders	0			1
		Capital companies allowed with restrictions on shareholders	2			
		Capital companies forbidden	4			
		Sole practitioners only	6			
2.4.	*Inter-professional cooperation*				1/5	
2.4.1.	How is inter-professional cooperation regulated?	All forms allowed	0			1
		Most forms allowed	2			
		Allowed between comparable professions	4			
		Generally forbidden	6			
2.5.	*Disciplinary power*				1/5	
2.5.1.	Is the chamber entitled with disciplinary power?	Yes, entrusted to a specific body	0			1
		Yes, entrusted to the chamber board	3			
		No	6			

TW = topic weight; STW = sub-topic weight; QW = question weight. (*) continuous values obtained normalizing each figure and letting the variable varies between 0 (minimum) and 6 (maximum).

Figure C2: Regulation and labor market outcomes in Europe



Authors' elaboration on data from the EU-SILC and OECD. We consider as professionals those employed in the ISCO group 21 and 24. Wage premium has been calculated on gross incomes, expect for Greece, Italy, Spain and Portugal for which we use net incomes; moreover, wage premium has been bounded to 100% for graphical reasons.