Does Informality Deter Tax Progressivity?*

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Abstract

In contexts with weak enforcement and widespread informality, the threat of tax evasion may constrain policy makers' power to set tax policies optimally. This paper studies whether reducing informality by tackling tax evasion leads policy makers to increase statutory tax progressivity. I take advantage of an Italian policy that generated cross-municipality variation in the scope for tackling income and property tax evasion through stricter tax enforcement. Combining an event study design with municipality-level panel data on statutory tax rates, I show that the ability of the government to change the size of the informal sector tips the balance in favor of higher marginal tax rates for middle and top earners, lower for the poor. The tax hike was larger in places with higher pre-program inequality and where intrinsic tax compliance attitudes were weaker. As a result of larger tax collections, municipalities hired more workers and raised public spending. These results suggest that policies enforcing legal rules and payment of taxes have not only the power to foster tax capacity, but also to enhance the ability to pursue redistributive policies.

Keywords: tax evasion; informality; tax enforcement; local taxes; tax progressivity.
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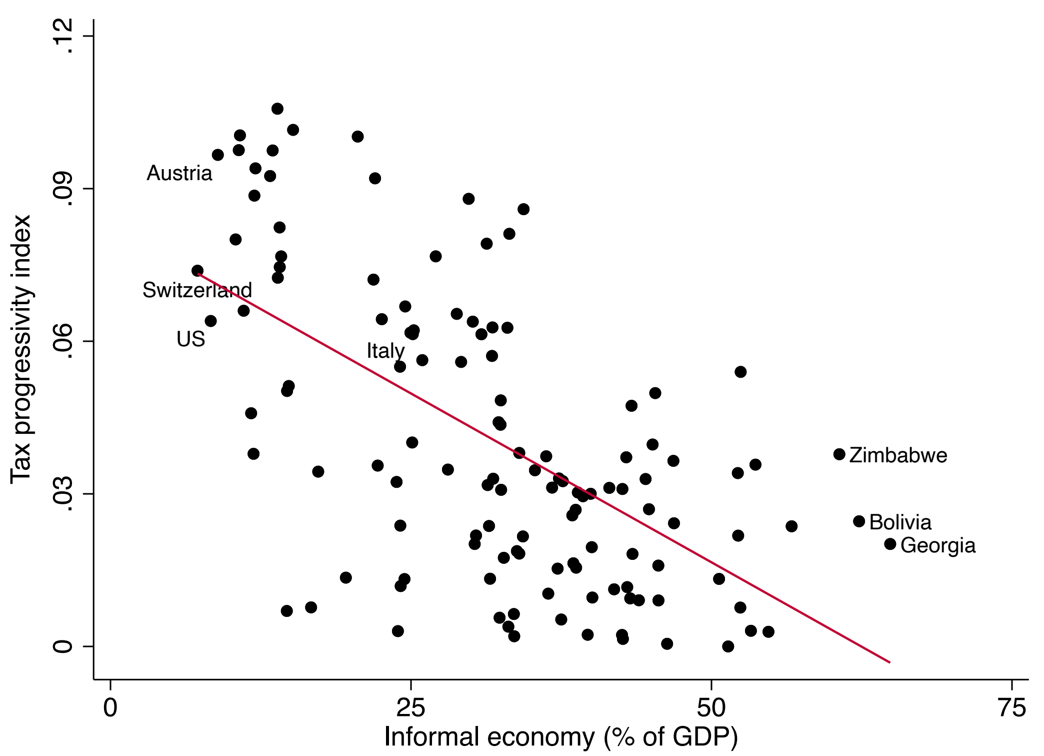
1 Introduction

Does informality hold back tax progressivity - the capacity to tax the rich at an higher rate than the poor? The optimal (and fair) distribution of the tax burden has long been a key issue in both academic and policy circles. A well-known result in optimal tax theory is that the revenue-maximizing top tax rate is negatively related with the tax base elasticity (Mankiw et al. 2009; Diamond and Saez 2011; Saez et al. 2012; Heathcote et al. 2017). When informality is widespread, tenuous tax enforcement and weak tax compliance attitudes would lead to an overflow of tax evasion, a large tax base elasticity, and thus a lower level of tax progressivity. In fact, a simple cross-country analysis suggests a negative correlation between informality and tax progressivity (see Figure 1). The income tax schedule is more progressive in Western economies, such as Austria, Switzerland and the United States, where informality is limited. By contrast, in countries such as Bolivia, Georgia and Zimbabwe, where informality accounts for almost two-thirds of GDP, tax progressivity is very low.

Figure 1: The relationship between tax progressivity and informality



Note: This figure relates the average rate progression of the personal income tax schedule (from Peter et al. 2010) with the size of the informal economy as a share of GDP (from Medina and Schneider 2018). The average rate progression is computed by regressing the average tax rate on different income levels, from zero to an income level equivalent to four times the GDP per-capita. The size of the shadow economy is measured using a Multiple Indicators Multiple Causes (MIMIC) approach. These variables are measured around the 2000s for 137 countries that appear in both data sources.

The received wisdow in public economics is that behavioral responses to taxes are shaped by the enforcement environment (see, e.g., the recent review by Slemrod 2019). A broader and more enforced tax base is generally associated with a low marginal cost of redistributing income through the tax system, that would imply more progressive tax structures. This suggests that the tax base elasticity should not be interpreted as a constant (structural) parameter, but as a policy choice. If informality can be tackled through policy, then the tax rate schedule could be adjusted to achieve whatever degree of progressivity is deemed optimal. Would policies curbing informality then led policy makers to increase tax progressivity? In other words, do governments change the distribution of the tax burden in the presence of a broader and more enforced tax base?

Despite a growing literature in public finance and political economy has emphasized that political institutions affect tax capacity (Acemoglu 2005; Besley and Persson 2009; Gordon and Li 2009; Besley and Persson 2013; Besley 2020; Weigel 2020), there is limited empirical evidence on whether tackling informality affects the distribution of the tax burden.[1] There are two main empirical challenges that limit our understanding on this topic. The first is related to the well-known difficulty of observing - and then measuring - informality (Schneider and Enste 2000; Slemrod and Weber 2012). The second empirical challenge is that policies aiming at tackling informality and tax policies are usually set simultaneously by the same government (see, e.g., Loayza 1996 and Friedman et al. 2000). This makes difficult to go beyond simple correlational evidence.[2]

To break new ground on this topic, I leverage variation in scope for fighting tax evasion generated by the "Ghost Buildings" program: a policy implemented by the Italian national government that detected buildings not registered in the land registry. Using an innovative monitoring technique, the program identified more than 2 million unregistered buildings, corresponding to missing cadastral incomes of more than 825 million euros. By making tax enforcement stricter, the policy enabled municipalities not only to "mechanically" collect more revenue by enforcing ghost buildings' registration, but also to reduce the elasticity of the tax base, thus lowering the efficiency costs of raising tax rates (Slemrod and Kopczuk 2002; Kopczuk 2005).

I study the impact of the Ghost Buildings program on *structural* tax progressivity (Musgrave and Thin 1948; Jakobsson 1976): how policy makers respond to a broader and more enforced tax base by changing statutory marginal tax rates. Italy is an attractive context for two main reasons. First, the Ghost Buildings program provides exogenous cross-municipality variation in scope for curbing informality by fighting tax evasion. Namely, detected unregistered buildings range from 0 to 13 percent of

[1]Notable exceptions include Piketty and Saez (2007), Alstadsæter et al. (2019), Saez and Zucman (2019), Bachas et al. (2020), and Alstadsæter et al. (2022), which study the effect of tax evasion on *effective* tax progressivity. This literature shows that the tax schedule is de facto less progressive when opportunities (or predilections) for evasion are systematically correlated with income. By contrast, this paper focuses on *structural* tax progressivity: the change in statutory marginal tax rates along the income distribution.

[2]These difficulties are well summarized by Keen (2012): "One conclusion to draw from all this is surely the need for more effective evaluation of initiatives in this area. The lack of careful studies partly reflects weaknesses of data - even on basic revenue information - and, perhaps, a lack of interest and/or awareness in the academic community. It also reflects an absence of controlled experiments, partly because they are intrinsically difficult to design for many major reforms."

the stock of total buildings across the almost 8,000 Italian municipalities.[3] Importantly, neither the scope for fighting tax evasion nor the timing of program inception was the result of an effort or specific policy adopted by local tax authorities. From a municipality perspective, the program can thus be seen as a random event. Second, Italian municipalities have large fiscal autonomy. Specifically, they manage around 10 percent of total current public expenditure and are in charge of providing a large array of essential public services, such as local public transportation, school facilities, town planning, and manage public utilities. To finance these services, municipalities set taxes on personal income and on property. This environment, which is common to many other countries with a certain degree of decentralization, makes the Italian context a propitious testing ground for the causal evaluation of whether informality deters tax progressivity.

The empirical analysis rests on administrative data on the Ghost Buildings program provided by the Italian Internal Revenue Agency. The dataset includes municipality-specific information on the total number of buildings, the number of parcels containing detected unregistered buildings, and the program inception year. Following Casaburi and Troiano (2016), I construct a measure of ghost buildings intensity as the share of parcels containing unregistered buildings. I then merge this information with a newly constructed panel dataset containing information on income and property tax rates, tax revenue, public spending, public sector employees, and the tax base (as reported in tax returns) for each municipality since the early 2000s. The empirical strategy is based on an event study approach, combining cross-municipality variation in ghost buildings intensity with the staggered implementation of the program.

As a first step, I study whether the policy was successful in broadening the tax base. Any (mechanical) increase in the tax base due to ghost buildings registration could, in principle, be circumvented by adjustments in other margins, such as labor supply responses, or other avoidance or evasion technologies. I show that the program broadened the tax base: a 1 standard deviation increase in the municipality-level program intensity (corresponding to a 1.662 percentage points increase in the share of unregistered buildings) raised the tax base by about 0.628 percent. This corresponds to an average increase of nearly 675,000 euros per municipality. Inspecting the dynamics of the treatment effects, I find a flat trend in the post-program tax base increase: it appeared immediately and remained constant up to 8 years. Most of the tax base increase stemmed from high incomes (defined as those reporting more than 75,000 euros of taxable income per year), while significant, but relatively lower, effects were also detected for low and medium incomes.

I then show that the policy led municipalities to adjust the statutory marginal tax

[3]For instance, the program would increase tax revenues in Rome - where 3,990 unregistered 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 about two-thirds of its average pre-program level of tax revenues (around 1 million euros).

rates on both personal income and property. I find that a 1 standard deviation increase in ghost buildings intensity led municipalities to raise the top statutory marginal tax rate on income by 1.1 percent, while reducing the bottom marginal tax rate by 0.9 percent. Statutory marginal tax rates monotonically increased for tax brackets above 15,000 euros, raising the marginal rate progression of the municipal income tax schedule. I also find an increase in property tax rates on both luxury properties and second homes. Taken together, these changes in statutory tax rates shifted the distribution of the local tax burden toward middle and top incomes, and toward wealthy property owners. The joint change in tax base broadness and statutory tax rates led to a significant increase in tax revenue: a 1 standard deviation increase in ghost buildings intensity raised municipal tax revenue by around 4.9 percent.[4] The income tax rate hike was mostly concentrated among municipalities with higher pre-program level of inequality and weaker attitudes toward tax compliance. This heterogeneity suggests that policies creating enforcement of legal rules and payment of taxes can not only foster tax capacity, but they can also enhance the ability of local governments to tackle local inequalities, accounting for weaker tax compliance norms. These estimates do not reflect pre-existing cross-municipality differences or underlying trends, neither are the results of differential efforts in starting the program earlier or in enforcing ghost buildings' registration.

The last part of the paper explores the implications of increasing tax capacity for the size of the public sector. I focus on two outcomes. First, using municipal balance sheets' information, I construct a measure of public spending as the sum of expenditures on schools, environment, cultural, social and sport activities. Second, I use a novel administrative dataset collecting information on public sector employees. I find a positive impact of the program on both local public expenditures and public sector employment. On average, a 1 standard deviation increase in program intensity raised public spending by 1.7 percent (i.e., around 69,000 euros of the pre-program mean), part-time female (male) employment by around 2.5 (1.7) percent, while the impact on full-time public sector employment was positive but not statistically significant. This result suggests that improving the capacity of (local) governments to collect and set taxes increases public services' provision and public sector labor demand.

1.1 Literature Contribution

The main contribution of this paper is to provide empirical evidence on the deterrent effect of informality on tax progressivity. The natural implication of this finding is the following: stricter tax enforcement broadens the tax base and reduces the marginal efficiency cost of taxation, thus making any tax rate change less costly (Slemrod and

[4] To put these estimates in perspective, if we moved from Trento (where no ghost building was detected) to Crotone (where one-tenth of the stock of buildings was unregistered), we would observe an increase in the Crotone's tax base by around 4.2 percent, the Crotone's top statutory marginal income tax rate would increase by 7.4 percent, and Crotone's tax revenue would raise by 33 percent.

Kopczuk 2002; Kopczuk 2005). 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). Cremer and Gahvari (1994) incorporate tax evasion into the classical Mirrlees (1971) optimal taxation framework. They provide sufficient conditions under which tax evasion might deter the optimal tax rate and show that tax evasion can affect the progressivity of a tax system depending on the "concealment technology."[5] Zoutman and Jacobs (2016) study the joint determination of monitoring and tax schedules, and the conditions under which these can be implemented. Building from this literature, I show that whether statutory tax rates and tax enforcement are strategic complements or substitutes depends on the impact that stricter tax enforcement has on the responsiveness of evasion to the marginal tax rate. As long as opportunities (or preferences) for tax evasion are systematically correlated with income, then the responsiveness of tax base to tax rate change might differ along the income distribution. My empirical findings show that the relationship between tax rates and tax enforcement leans towards strategic complementarity for middle and top earners, but towards strategic substitutability for low incomes. This implies that stricter tax enforcement significantly affects the optimal shape of the tax schedule.

My results are germane to a recent stream of the empirical literature providing evidence on the returns from making tax collection more effective. Jensen (2021) studied historical episodes of shifting from self-employment to wage labor in the US. His results underscore the key role of third-party information to spur tax compliance, which is a particular challenge in countries with weak tax enforcement and widespread informality (Kleven et al. 2016). Bergeron et al. (2021) provide experimental evidence on complementarity between enforcement and tax rate from a collaboration with tax authorities in D.R. Congo. According to their estimates, taking into account this complementarity would raise revenue from 61 to 77 percent. In the same context, Balan et al. (2021) emphasize the role of tax collectors: delegating tax collection to local chiefs, rather than state agents, significantly improved tax compliance and property tax collections. Basri et al. (2021) show that improved tax administration - higher staff-to-taxpayer ratios - significantly spurred tax collections from large firms in Indonesia. Comparing tax revenue effect from stricter enforcement with tax rate-induced changes, they show that improved tax administration is equivalent to a relatively large increase in tax rates (i.e., around 8 percentage points). I contribute to this literature by providing evidence that weak enforcement constraints (local) policy makers' power to optimally set both income and property taxes. Since local taxes have important distributional consequences (Brülhart et al. 2020) and property taxation is the key revenue source for the vast majority of local governments around the world (Brülhart et al. 2015; Agrawal et al. 2021), my findings suggest that weak enforcement limits the size of the public sector and distorts the distribution of the local tax burden.

[5] Cremer and Gahvari (1996) extend this finding in the context of a two-type economy with non-linear taxation and monitoring.

This paper also relates to the literature studying the impact of different policies aiming at curbing tax evasion (see surveys by Andreoni et al. 1998; Slemrod and Yitzhaki 2002; Alm 2012; Slemrod and Weber 2012; Slemrod 2019). A growing recent literature has provided evidence on the public finance and labor market returns from improving enforcement, 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), e-filing (Okunogbe and Pouliquen 2021), and targeted auditing strategies (Almunia and Lopez-Rodriguez 2018).[6]

The paper also offers evidence on the impact of improving tax capacity on public spending and public sector employment. To my knowledge, this is the first study to provide evidence on the returns of improving tax capacity at the local level in the context of a high tax evasion and developed country. In the developing world, Gadenne (2017) and Martinez (2019) reached similar conclusions by leveraging quasi-experimental variation in tax revenue on provision of public goods in Colombia and Brazil, respectively. Focusing on property taxation in Mexico City, Brockmeyer et al. (2021) show that governments can finance public goods though higher tax rates even in settings with significant under-compliance and liquidity constrained individuals. By contrast, there is a rich and growing literature reporting null or small effects on the 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.[7] This article emphasizes the notion that what matters is the source of fiscal windfall: extra revenue coming from fighting tax evasion raise public services' provision and increase public sector employment. Since a large fraction of public goods is provided locally, improvements in tax collections have thus the potential to improve residents' welfare.[8]

The rest of the paper is organized as follows. Section 2 illustrates the institutional framework and describes the data. Section 3 sets out the conceptual framework and the empirical strategy. Section 4 and 5 present the results, some mechanisms and implications. Section 7 concludes.

[6]For the role of digitalization to tackle tax evasion, see the surveys by Gupta et al. (2017) and Pomeranz and Vila-Belda (2019).

[7]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 revenue and rent-seeking behavior by politicians.

[8]For instance, Suárez Serrato and Wingender (2014) estimate that, on average, an additional dollar of government spending increases welfare by 1.45 dollars. A fully-fledged analysis of the welfare effect of the Ghost Buildings program is out of the scope of this paper.

2 Institutional Background and Data

2.1 The "Ghost Buildings" program

Similarly to other countries, 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 a building permit before starting construction to make them part of the City Plan.[9] 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 to the tax authorities.[10]

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.[11] 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. 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 extensions of previously registered buildings.[12] Since the program inception, the cadastral information system has been integrated with these high resolution images; the procedure to detect unregistered building has then been implemented every year to check any novel evidence of "ghost buildings." The program can thus be perceived as a permanent improvement in the enforcement technology.

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 reg-

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

[10] Eluding the registration of buildings or portions of land is a behavior observed both in the developed and developing world. For instance, 14 percent of land in England and Wales was unregistered because registration was not compulsory until 1990 (HM Land Registry, Annual Report and Accounts 2012/2013). In Portugal, land registry covers only 80 percent of the land (European Land Registry Association). Other countries have followed the Italian example to digitize cadastral maps in order to detect unregistered buildings and make monitoring more effective. For instance, Hungary has introduced a 3D land registry starting from 1 July 2018.

[11] Moreover, once sold or transferred, buildings are subject to transfer tax, value added tax, registration and stamp duties, and land registry tax.

[12] See Appendix A for details on the identification process.

istry maps of the Italian territory was available. Then, the *Agenzia del Territorio* digitized the remaining land registry maps proceeding by province. Therefore, the publication year mostly varies across municipalities located in different provinces: only one-tenth of municipalities had a program start year that differed from the provincial modal publication date (see Figure A2).

As shown by Table A1, 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 euros, summing up nearly 444 million euros for the property tax, about 137 millions euros for the income tax (including both central and local taxes), and around 7.5 million euros from registration fees (Agenzia delle Entrate 2012). Agenzia delle Entrate (2012) calculates 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.

2.2 Local Fiscal Policy in Italy

The Italian constitution devolves substantial autonomy to the 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. 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.

To finance expenditures, municipalities receive revenue from different sources, including taxes, fees, and grants from the national, regional and provincial government. In this paper, I focus on the two main sources of municipal tax revenue: property taxes and a surcharge on the personal income tax.

2.2.1 Local Property Taxation

Similarly to many industrialized countries, property taxes are the key source of funding for local governments. According to OECD Global Revenue Statistics, revenue from taxes on property account for around 2.5 percent of the Italian GDP in 2019, and nearly 6 percent of total taxation. In Italy, a local property tax was introduced in 1993 (formerly called *Imposta Comunale sugli Immobili*) in an attempt to raise municipalities' administrative power and accountability.[13] Municipalities choose two tax rates: i) on homes reported as "main residence"; ii) on second (vacation) homes and commercial buildings.[14] Each municipality has the power to diverge by as much as 0.3 percentage points from the basic statutory tax rate of 0.76 percent. The tax base is computed as the

[13] Agenzia delle Entrate (2019) provides information on the evolution of property taxation in Italy.

[14] The Italian law defines "main residence" the buildings reported in cadastral registers as single real estate units where the owner resides habitually (at least 186 days in a year).

rental value of the building, scaled by a nationwide multiplier, and net of deductions that can be set by both the national and local government.

Several nationwide reforms have changed the power of municipalities to set property taxes (see Messina and Savegnano (2014) for a review). In particular, owner-occupied residences reported as main residence are not subject to the property tax rate (see law 93/2008). This exemption does not apply to "luxury" properties: real estate units belonging to buildings located in prestigious area presenting constructive, technological and fittings features that are of higher level than that of residential buildings. Stately homes, villas, maisonettes and cottages are examples of these buildings.[15] In the empirical analysis, I will thus focus on two property tax rates set by municipalities: the rate applied to luxury properties, the rate for second homes and commercial buildings.

2.2.2 Local Income Taxation

Municipalities also charge a surtax on personal income on top of the tax rates set by the national and regional governments. Statutory income tax rates set by municipalities can differ across income brackets and vary between 0 and 0.8 percent.[16] To simplify the tax collection process, the tax base is defined by the national government. This has the advantage of ensuring comparability in tax base definition both over time and across municipalities. Municipalities can implement either a graduated tax scheme or a flat rate. In the former case, rates are required to be structured according to the same income brackets defined by the national personal income tax and to be diversified and increasing with income. Over the period covered in the analysis, there are the following five income intervals (in euros): i) $\leq$ 15,000; ii) 15,001-28,000; iii) 28,001-55,000; iv) 55,001-75,000; v) $>$ 75,000. Municipalities have also the power to set a tax exemption cutoff, below which income is not subject to local income taxation. As Italy has a residence-based tax system, the tax rate applies to the taxpayer's tax residence at the beginning of the year.

2.3 Data

The Italian Internal Revenue Agency provided data on the Ghost Buildings program. I access to municipality-level information on the total number of parcels, the number of parcels containing ghost buildings, and the program start date.[17] Following Casaburi and Troiano (2016), I construct a measure of *ghost buildings intensity* as the ratio between the number of parcels containing ghost buildings and the total number

[15] More precisely, all buildings classified as cadastral units A1, A8 and A9 are not tax exempted.

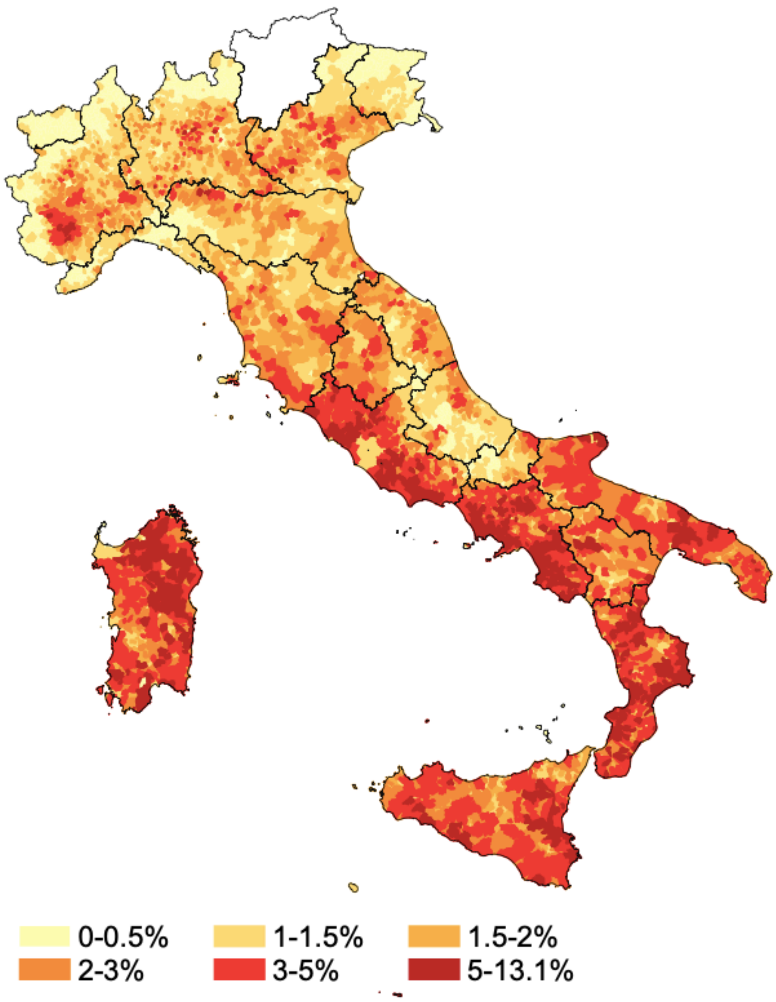
[16] One exception is the city of Rome that is allowed to set a municipal income tax rate up to 0.9 percent. See Appendix A in Rubolino (2020) for a review on local income taxation in Italy.

[17] 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.

of parcels, net of buildings that were not required to be reported to the land registry. I construct this indicator for the population of 7,709 out of the 7,910 municipalities, since the program does not cover Trentino Alto-Adige region, where land registry maps are autonomously administered.

Figure 2 depicts ghost buildings intensity across municipalities. Geographically, the South of Italy presented a larger share of detected unregistered buildings, in particular along the Tirrenean and Jonic sea. Figure A3 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. Appendix Figure B1 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).

Figure 2: 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.

Income tax rate series come from Rubolino (2020), who collected these data from administrative sources since 2001. The dataset reports information on statutory marginal tax rates on personal income set by municipalities, specifically for each income interval. In the empirical analysis below, I will investigate the impact of the Ghost Buildings program on each statutory marginal tax rate set by a municipality in each of the six income intervals where they can apply a different tax rate. Moreover, as the modal value of the tax exemption cutoff is 10,000 euros, I will also investigate the program impact on the tax rate applied to an income level $\leq$ 10,000 euros, and to the exemption cutoff as well. Following classical public finance literature on measurement of structural tax progressivity (see Musgrave and Thin 1948 and Jakobsson 1976), I also compute the marginal rate progression (MRP): the derivative of the marginal tax rate with respect to income before tax.[18] I develop a measure for each municipality and year using the procedure proposed by Peter et al. (2010), which consists in regressing marginal rates on the log of the statutory income thresholds. A larger value of MRP is associated with a higher degree of progression.

Property tax rates come from the Italian Institute of Finance and Local Economy (*Fondazione IFEL*), which provides data on both the tax rate for owner-occupied luxury properties reported as main residence, and the tax rate for second homes and commercial buildings. 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.

Data on tax revenues are from the balance sheets of Italian municipalities, which are annual reports provided by the Italian Ministry of Interior since 2001 (*Ministero degli Interni*). Municipal balance sheets have been introduced with the aim to better monitor local public spending in the frame of the Domestic Stability Pact. The accounting models have been homogeneous both across municipalities and over time. The average municipality collects 2.7 million euros from taxes (around 350 euros per capita).

Using balance sheet data, I also collect information on public spending on several budget items. I define a measure of municipal public spending as the log of total current expenditure earmarking schools, environment, and cultural, social and sport activities. Average total local public spending amounts to 4.1 million euros, which is around 535 euros per capita.

I also compute the municipal tax base subject to income taxation using publicly available data on taxable income from the Italian Ministry of Economy and Finance. These data are based on tax returns and are available for each municipalities since 2001. The tax base as well as the number of taxpayers are reported in several income intervals, allowing me to study the evolution of taxable income along the income distribution across municipalities over time.

[18] Musgrave and Thin (1948) introduce the definition of *structural progressivity* as changes in the calculated (nominal) tax burden along the income distribution. This measure of progressivity is different from measures of effective progressivity (e.g., Raynolds-Smolensky or Kakwani index) that aim at capturing changes in actual income inequality.

Administrative data on public employment for each municipality is provided by the *Ragioneria Generale dello Stato* from the Italian Ministry of Economy and Finance. The dataset collects information on public administration employees. From these data, I compute the count of public administration employees for each municipality in each year over the 2001-2015 period. Employees hired by a municipality are around 53, on average.

I compute several municipality-level time-varying demographics and labor market indicators collecting information from the Italian Institute of Statistics. These include population, share of population 65+, share of population 15-, share of foreign, and unemployment rate. From the Ministry of Interior, I collect demographic information on 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), and the year when an election took place. Finally, I use a region-level standardized score from the European Values Study to the question "Do you justify tax cheating?" Table B1 presents summary statistics for the variables used in the empirical analysis.

3 Conceptual Framework and Identification Strategy

3.1 Conceptual Framework

This section sets out a simple framework to study how tackling informality through stricter enforcement affects the tax schedule set by governments. Building from Keen and Slemrod (2017), I assume that informality arises from the behavior of some individuals that conceal a portion of their tax base to the tax authority.[19]

Consider a representative individual that chooses labor supply and the amount of income to conceal from the tax authorities by maximizing the following quasi-linear utility function:

$$U = x - \psi(l) + v(g), \tag{1}$$

where x denotes private consumption, l is labor supply (hours of work) and $v(g)$ is the utility obtained from public spending, g. ψ is strictly increasing and convex; v is strictly increasing and concave. To finance public spending, the government sets a tax schedule $T(z)$ that applies to the reported taxable income, $z = wl - e$. The tax schedule can be either linear, where $T'(z_0) = T'(z_1)$, or be increasing with income, $T'(z_0) < T'(z_1)$, with $z_0 < z_1$. Consumption is given by the following equation:

$$x = wl - T(z) \cdot (wl - e) - c(e, \alpha), \tag{2}$$

[19] An early contribution of this issue is Sandmo (1981), who studies enforcement and optimal progressivity in the presence of tax evasion. See also Feldstein (1999), Chetty (2009), Saez et al. (2012) and Slemrod and Gillitzer (2013) for other analyses on how tax enforcement and tax rates affects individual evasion choices.

where w denotes a fixed wage rate, e is the amount of income that is concealed, and c represents the cost that the consumer bears when decides to hide some of tax base.

α is the enforcement parameter that can be actively manipulated by the tax authority. In this context, it captures the technological improvement to detect unregistered buildings. Because, ceteris paribus, stricter monitoring should raise the probability to be caught evading, larger α raises the private costs of evasion, so that $c_\alpha > 0$ and $c_{e\alpha} > 0$. [20] The government budget constraint can be written as:

$$g + \alpha(\alpha) = T(z) \cdot (wl - e), \tag{3}$$

where $\alpha(\alpha)$ captures some positive and strictly increasing costs associated with the policy intervention. In this model, taxation involves shifting consumption from the private good, x, to public spending, g, via tax revenue (net of any administration costs). Substituting equations (2) and (3) in (1), it gives the following concave social welfare function that government maximizes:[21]

$$W(T(z)) = wl - T(z) \cdot (wl - e) - c(e, \alpha) - \psi(l) + v(T(z) \cdot (wl - e) - \alpha(\alpha)). \tag{4}$$

Differentiating equation (4) with respect to $T(z)$ and using the envelope property, I can define the necessary condition for the local government's choice of the optimal marginal tax rate at income level z:

$$W_{T'(z)} = -z + v'(z + T'(z) \cdot z_{T'(z)}) = 0, \tag{5}$$

Assuming that $v' > 1$ and rearranging equation (5), the optimal marginal tax rate at income level z is a function of the tax base elasticity:

$$\frac{T'(z)}{1 - T'(z)} = \left(\frac{v' - 1}{v'}\right) \frac{1}{\eta_{z,1-T'(z)}}, \tag{6}$$

where $\eta_{z,1-T'(z)} = \left(\frac{T'(z)}{1-T'(z)}\right) \frac{\partial z}{\partial(1-T'(z))}$ is the elasticity of taxable income with respect to the net-of-marginal tax rate at income level z.

How stricter tax enforcement (i.e., a larger α) affects the marginal tax rate, $T'(z)$, set by the government? Applying the implicit function theorem to equation (5) and

[20] In this basic model, α does not vary across income groups: the program did not target taxpayers based on their incomes since information on ghost buildings' owners are ex ante unknown. However, it is likely to differentially affect groups of taxpayers within a given municipality depending on where ghost buildings' owners were located along the (pre-tax) municipal income distribution. I will return to this point in section 4.2.2.

[21] I made two simplifications. First, since the focus in this paper is on the tax setting behavior of the *local* government, which takes the enforcement parameter as given (being set by the *national* government), I abstract from maximizing the social welfare function with respect to the enforcement parameter α. Second, I do not embed any taste for egalitarianism in the social welfare function. For the interested reader, see Slemrod (1994) for analyzing how the optimal tax rate and enforcement solution would vary with different measures of taste for egalitarianism.

assuming that v' is constant, the sign of the slope of the best response function $T'(z)(\alpha)$ is given by:

$$W_{T'(z)\alpha} = -(v'-1)e_\alpha - v'T'(z)e_{T'(z)\alpha}. \tag{7}$$

Because $v' > 1$ and $e_\alpha < 0$,[22] the first term in equation (7) yields an unambiguous positive relationship between enforcement and the marginal tax rate. The argument behind this result is that any reduction in evasion achieved through stricter tax enforcement would increase the "mechanical" benefit (i.e., revenue gains) of a tax hike.

The sign of the second term of the equation, $e_{T'(z)\alpha}$, is instead not a-priori obvious. The parameter $e_{T'(z)\alpha}$ captures the impact that stricter tax enforcement has on the responsiveness of evasion to the marginal tax rate. If stricter tax enforcement dampens the marginal impact of a more progressive tax schedule in encouraging evasion, then $e_{T'(z)\alpha} < 0$ and $W_{T'(z)\alpha} > 0$. This would imply that the structural progressivity of statutory tax rates and enforcement are *strategic complements*. The presence of the term $e_{T'(z)\alpha}$ in equation (7) is germane to a basic public economics argument: the tax base elasticity is endogenous to enforcement (Slemrod 1994; Slemrod and Kopczuk 2002; Kopczuk 2005; Saez et al. 2012; Slemrod 2019). As behavioral responses to taxes are shaped by the enforcement environment, the revenue-maximizing marginal tax rate should not be interpreted as a constant (structural) parameter, but as a policy choice.

By contrast, if stricter tax enforcement induces individuals to be more sensitive to a tax rate change, then there is *strategic substitutability* between tax enforcement and tax rates. Higher responses to tax changes in the presence of a more enforced tax base might happen for several reasons. For instance, liquidity constrained individuals might be more sensitive to changes in their tax burden if they do not have enough money for paying taxes (see, e.g., Brockmeyer et al. 2021). Furthermore, if opportunities (or predilections) for tax evasion are systematically correlated with income, then $e_{T'(z)\alpha}$ might differ along the income distribution. If this happens, any change in the enforcement environment would thus lead to a differential impact in optimal marginal tax rates along the income distribution. This motivates me to investigate the impact of stricter tax enforcement on the entire tax function by looking at marginal tax rates at different portions of the income distribution.

For the sake of simplicity, this model rests on the assumption that taxpayers conceal a portion of the tax base to the tax authority. More precisely, not registering a building should be represented as a decision on the extensive margin of reporting a specific source of wealth. However, the sign of the slope of the best response function $\tau(\alpha)$, where τ is the property tax rate, would depend on the same parameters as those present in equation (7). A key difference between the optimal income vs property tax choice is that the property tax rate applies to a base that is immovable and

[22] This is derived by the optimal individual's choices of concealment with respect to the enforcement parameter: $e_\alpha = -c_{e\alpha}/c_{ee} < 0$.

largely inelastic, while taxes on incomes tend to bring about behavioral changes. This implies that, ceteris paribus, $e_{\tau\alpha} \geq e_{T'(z)\alpha}$, making the relationship between stricter tax enforcement and property tax rate to lean toward strategic complementarity. In terms of property tax progressivity, while it depends on tax incidence and the distribution of property assets across income groups, the fact that owner-occupied main residences are tax exempted suggests that any increase in the property tax rate can be considered as an increase in progressivity. In fact, luxury homes and second homes, which are the only types of properties subject to the local property tax, are heavily concentrated among higher income earners.

3.2 Empirical Strategy

The goal of my empirical work is understanding whether reducing informality through stricter tax enforcement (i.e., a higher α), affects the tax schedule, $T(z)$, set by municipalities. The main empirical obstacle is that tax enforcement is not randomly allocated across municipalities. This happens for two main reasons. First, tax enforcement may be stricter in municipalities where tax evasion is more prevalent. In this case, stricter tax enforcement is triggered mainly because of missing tax revenue. Second, tax enforcement may be stricter in municipalities with lower quality institutions and low civic and social capital. Intrinsic attitudes towards tax compliance and more developed institutions might also be correlated with tax rates.

To account for these issues, I identify the impact of tackling informality on tax rates by leveraging the differential exposure across municipalities in scope for fighting tax evasion triggered by the Ghost Buildings program. The empirical approach leverages two sources of variations. First, the staggered implementation of the policy implies that each municipality receives the treatment at different times. Namely, depending on the availability of digitized land registry, each municipality i started the program at (calendar) year E_i during the period $t \in [2007, 2010]$, and remained treated thereafter. Second, each municipality is differentially exposed to the program depending on the share of detected unregistered buildings, GB_i.

To exploit these two sources of identifying variations, I implement an event study approach by running regressions as the following:

$$\tau_{i,t} = \sum_{k=-6}^{8} \alpha_k \cdot 1(K_{i,t} = k) + \sum_{k=-6}^{8} \beta_k \cdot 1(K_{i,t} = k) \cdot GB_i + \gamma_i + \delta_{p(i),t} + \eta_{e(i),t} + u_{i,t}, \quad (8)$$

where the dependent variable, $\tau_{i,t}$, is the log of the marginal tax rate (plus one) or other public finance outcomes observed in municipality i at year t. The variable $K_{i,t} = t - E_i$ denotes the "event time", that is the number of years relative to the program inception year. Since the outcome variables of interest are observed over the 2001-2015 period and the earlier program inception year is 2007, I am able to include observations with $K_{i,t} \in [-6, 8]$.[23]

Municipality fixed effects, γ_i, control for the fact that municipalities with different share of unregistered buildings may be different in other dimensions, such as in their technology for collecting tax revenue or in preferences for redistributive policies. Province-year fixed effects, $\delta_{p(i),t}$ capture any change in provincial (or regional) policy and the local business cycle. Since trends in taxation and other public finance and labor market outcomes are likely to differ geographically, the inclusion of these fixed effects allows me to construct potentially more realistic counterfactuals by comparing tax rate changes across municipalities with different ghost buildings share within a given province.[24] Given the staggered timing of elections across municipalities, adding election year-year fixed effects, $\eta_{e(i),t}$, allows to account for the fact that electoral incentives for pursuing policies aiming to capture voters, such as lower taxes, are stronger when legislative elections approach. Finally, $u_{i,t}$ is the error term. Throughout the analysis, I cluster the standard errors at the municipality level.

β is the parameter of interest and measures the differential impact of the Ghost Buildings program on tax rates across municipalities with different share of unregistered buildings. The interaction term, $1(K_{i,t} = k) \cdot GB_i$, omits the year before the program inception year, $k = -1$, so that the coefficient β_k can be interpreted as the effect at the event year k relative to the year before the program inception year. One advantage of this approach is that it allows to scrutiny the dynamic effects for each year $k \geq 0$ relative to $k = -1$. Consider, for instance, to inspect the effect of the program on the tax base. If the program was successful in enforcing ghost buildings registration, I should observe an *immediate* mechanical increase in the tax base. Then, the pattern of the β_k for $k \geq 0$ would indicate whether the tax base fell, increased or stayed constant over the post-program period.

To estimate the average treatment effect (ATE), I will also present the coefficient β estimated from the following canonical (static) difference-in-difference (DiD) specification:

$$\tau_{i,t} = \alpha \cdot 1(K_{i,t} \geq 0) + \beta \cdot 1(K_{i,t} \geq 0) \cdot GB_i + \gamma_i + \delta_{p(i),t} + \eta_{e(i),t} + u_{i,t}, \tag{9}$$

where $1(K_{i,t} \geq 0)$ is the treatment indicator. Differently to the model presented in (8), the canonical DiD specification imposes a constant treatment effect for all $k > 0$.

The coefficient of interest, β, measures the causal effect of the Ghost Buildings program on the outcome variable of interest by ghost buildings intensity. A recent growing literature (see, e.g., Athey and Imbens 2018; Callaway et al. 2021; de Chaisemartin and D'Haultfoeuille 2020; Sun and Abraham 2020; Callaway and Sant'Anna 2021) has

[23] Coefficient estimates obtained from a balanced sample around the event time (i.e., by including only observations with $K_{i,t} \in [-5, 5]$) are highly similar.

[24] Because the policy inception year is strongly correlated at the province-level, the addition of province-year fixed effects implies that I exploit variation across municipalities within the group of municipalities that is treated at the same point in time. This leads to interpret the coefficient estimate as the *group-year* average treatment effect (Callaway and Sant'Anna 2021).

studied the estimation of and inference of ATE in similar settings. The basic assumption to identify the ATE is 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. I will validate this assumption by scrutinizing that $\beta_k = 0$ for each $k < -1$.

One concern advanced by the literature is that the presence of negative weights might affect the computation of ATE. In this context, negative weights arise because β is a weighted sum of several DiD, each comparing the evolution of the outcome variable between consecutive time periods across pairs of municipalities. Given the staggered adoption of the program, the "control" group in some of the comparisons would become treated at a later period. Then, its treatment effect at a later period gets differenced out, thus generating the negative weights. Following the recommendations of de Chaisemartin and D'Haultfoeuille (2020) and Borusyak et al. (2021), I test the sensitivity of my estimate to the presence of negative weights. I find that only 0.2 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. In the robustness section below, I will show that coefficient estimates are fairly similar when I remove municipalities that presented a negative weight. I thus conclude that negative weights are not a strong concern in this setup.

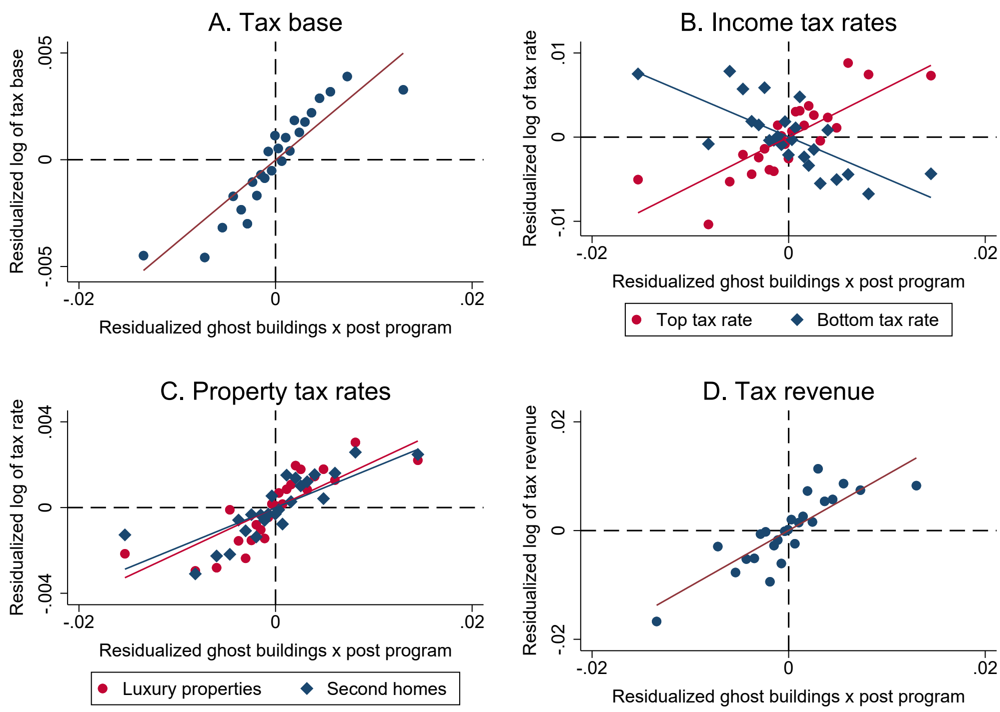
4 The Impact of The Ghost Buildings Program

4.1 Graphical Evidence

Figure 3 provides graphical evidence on the impact of the Ghost Buildings on local public finance outcomes. It presents a series of bin scatter-plots of the log of the outcome variable of interest on the interaction between ghost buildings intensity and the post-program dummy, net of the fixed effects presented in equations (8) and (9). The slope is equal to the β coefficient obtained from running equation (9); it computes the average treatment effect of the Ghost Buildings program on the outcome variable of interest by ghost buildings intensity.

The figure provides the following findings. First, the positive relation depicted in panel A suggests that the tax base increased relatively more in municipalities with a larger share of detected unregistered buildings. This provides prima facie reassuring evidence that the policy led to a significant increase in the municipal tax base by inducing ghost buildings' registration. Second, panel B shows that there is a positive relationship between ghost buildings intensity and the top statutory marginal tax rate on personal income, while the slope is negative for the bottom statutory tax rate. Property tax changes also appear to be associated with ghost buildings intensity: panel C shows that the tax rate on both luxury and second homes became relatively higher in municipalities with a larger share of detected unregistered buildings. Finally, panel D

Figure 3: The Impact of the Ghost Buildings Program on Public Finance Outcomes



Note: The figure provides graphical evidence on the impact of the Ghost Buildings program on the log of the following municipality-level variables: tax base (panel A), top vs bottom statutory marginal tax rate on personal income (panel B), the property tax rate on luxury homes vs second homes (panel C), and tax revenue (panel D). The vertical axis depicts the residuals obtained by regressing the outcome variable of interest on municipality fixed effects, province-year fixed effects, and election year-year fixed effects. The horizontal axis presents the residuals from regressing the interaction between ghost buildings intensity and the post-program dummy on the same set of fixed effects. The figure plots the residuals in 25 equal sized bins and shows the line of best fit. The slope corresponds to the β coefficient estimate obtained by running equation (9). The sample includes 7,709 municipalities observed over the 2001-2015 period.

shows that the overall effect of the program on tax revenue was positive. In the next section, I will present and discuss the event study estimates.

4.2 The Tax Base Effect Induced by Ghost Buildings' Registration

This section investigates the effect of ghost buildings' registration on the municipal tax base. For this end, I will first present the event study estimates of the Ghost Buildings program on the aggregated municipal tax base. Then, I will study specific income groups to test whether the tax base effect differs along the municipal income distribution.

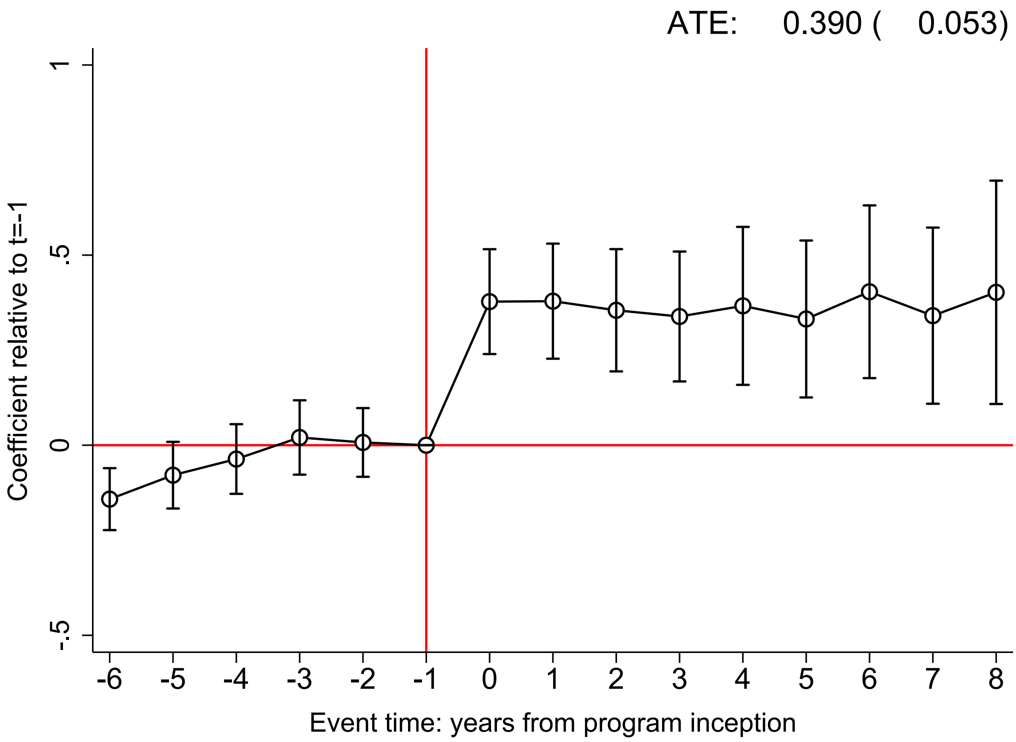
4.2.1 Overall Impact

Inspecting the effect on the tax base allows to evaluate whether the program mechanically broadens the tax base by inducing registration of ghost buildings. The virtue of the event study design is that it provides a transparent way to identify the effect of the

Ghost Building program and to inspect the dynamics of the effect.

Figure 4 plots the estimated β_j coefficients from equation (8) and 95 percent confidence intervals: each point on the solid line shows the effect of having implemented the Ghost Buildings program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the policy inception year. The figure also reports the ATE obtained by estimating β from equation (9).

Figure 4: The Impact on the Tax Base



Note: This graph presents the impact of the Ghost Buildings program on the log of the tax base. The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard error clustered at municipality level in parenthesis. Data covering 7,709 municipalities observed over the 2001-2015 period.

Three main findings emerge from the figure. First, there are no pre-existing differences in the tax base trend across municipalities with different ghost buildings intensity: the β_j are not significantly different from zero over the pre-policy years (with the exception of event time = -6). Second, the tax base increased exactly in the year the program took place. Third, the tax base increase is sharp and persistent up to eight years after the program implementation. While this result does not rule some behavioral responses around the program inception, the pattern of β_j estimates over the post-policy period suggests that the estimated effects mostly reflected a mechanical increase in the tax base due to registration of ghost buildings.

The ATE, computed from equation (9), is highly significant and implies that a 1 standard deviation increase in the municipality-level program intensity (corresponding to a 1.662 percentage points increase in the share of unregistered buildings) raised, on average, the tax base by 0.628 percent. This corresponds to an average increase of nearly 0.675 million euros per-municipality. To put this number in perspective, if we moved 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 the tax base by around 4.187 percent in Crotone compared to Trento, which corresponds to broadening the Crotone's tax base by around 24 million euros.[25]

4.2.2 The Impact by Income Group

This section zooms on the tax base impact by income groups. This exercise allows to provide indirect evidence on the distribution of ghost buildings' owners. Ceteris paribus, we should observe a relatively larger increase in the post-program income group-specific tax base where most ghost buildings' owners were located. This means that even if the Ghost Buildings program program did not specifically target taxpayers based on their income, it is likely to differently affect taxpayers within a municipality depending on where ghost buildings' owners were located along the (pre-tax) municipal income distribution.

I first divide the aggregate tax base in 6 income intervals, following the same income intervals to which different income tax rates can be set by municipalities. Then, I compute ATE and event study coefficient estimate on the log of the tax base reported in each income interval. The results are reported in Appendix Figure B2, which depicts the β coefficient estimate and 95 percent confidence intervals computed from equation (9). The figure shows that the tax base increase was mostly concentrated in the top income group (i.e., those reporting annual taxable incomes larger than 75,000 euros).[26] On average, a 1 standard deviation increase in ghost buildings intensity raised the top income group's tax base by around 31 percent. For mid earners (15,000-26,000 euros) and mid-high earners (26,000-55,000 euros), the corresponding tax base increase was of nearly 2.2 and 2.8 percent, respectively. Appendix Figure B3 presents the event study estimates. Consistent with the aggregate evidence displayed above, the figure provides reassuring evidence that the tax base increase appeared at the time of program inception, and then stayed constant over the post-program years.

4.3 The Impact on Statutory Tax Rates

The previous results provide evidence that the Ghost Buildings program broadened the tax base, mostly due to an increase in the reported taxable income by top income

[25]Moving from Trento to Crotone implies a 6.667 standard deviation increase in ghost buildings intensity. Crotone's tax base over the pre-program period was nearly 578 million euros.

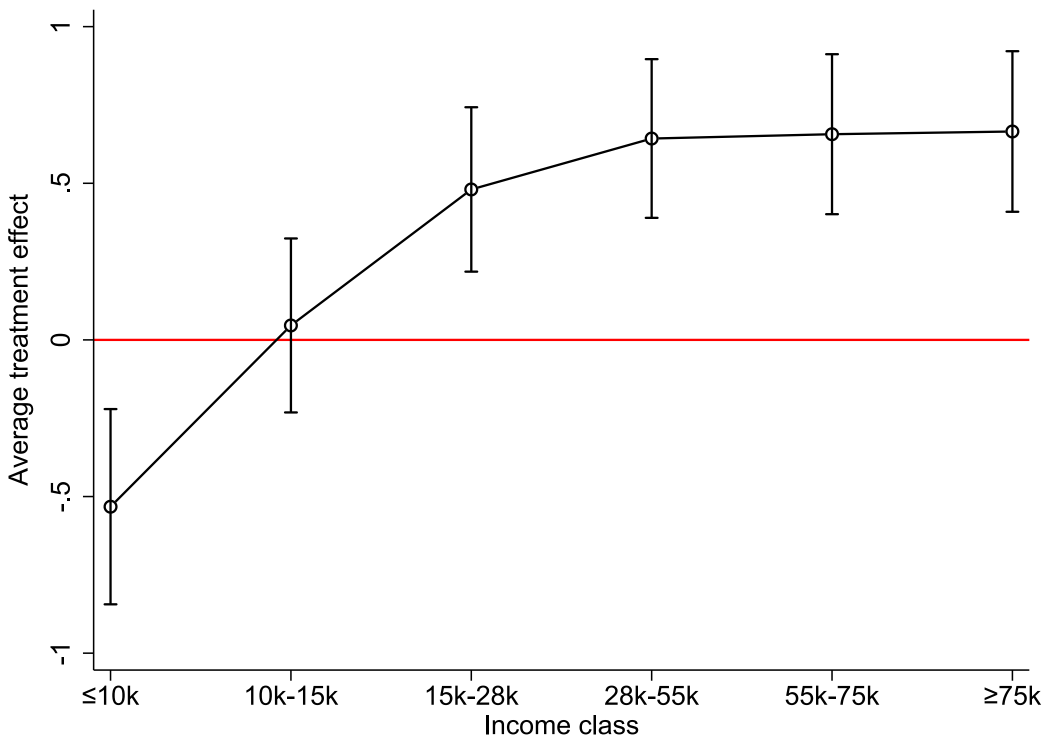
[26]These patterns suggest an increase in the *effective* progressivity of the Italian tax system: by affecting richer taxpayers relatively more, the effective tax rate (ratio of taxes paid to total (true) income) increases with income. Due to the lack of individual data on ghost buildings' owners, a fully-fledged evaluation of the impact of the program on effective tax progressivity is out of the scope of this paper.

individuals. By making tax enforcement stricter, the policy enabled municipalities not only to "mechanically" collect more revenue by enforcing ghost buildings' registration, but also to reduce the elasticity of the tax base, thus lowering the efficiency costs of raising tax rates (Slemrod 1994; Slemrod and Kopczuk 2002; Kopczuk 2005; Saez et al. 2012; Slemrod 2019). This section shows the impact of the Ghost Buildings program on statutory marginal tax rates set by municipalities on personal income (4.3.1) and on property (4.3.2).

4.3.1 Income Tax Rates

Figure 5 displays the impact of the Ghost Buildings program on statutory marginal tax rates on personal income set by municipalities. It depicts the β coefficient estimate and 95 percent confidence intervals obtained from equation (9). The estimates suggest that stricter tax enforcement leads municipalities to raise the marginal tax rates above 15,000 euros. By contrast, municipalities cut the bottom tax rate, applying to taxpayers reporting income below 10,000 euros. Incomes ranging from 10,000 to 15,000 euros were instead not affected by the policy. This results suggest that there is complementarity between stricter enforcement and statutory tax rates for an income level above 15,000 euros; substitutability for an income level below 10,000 euros.

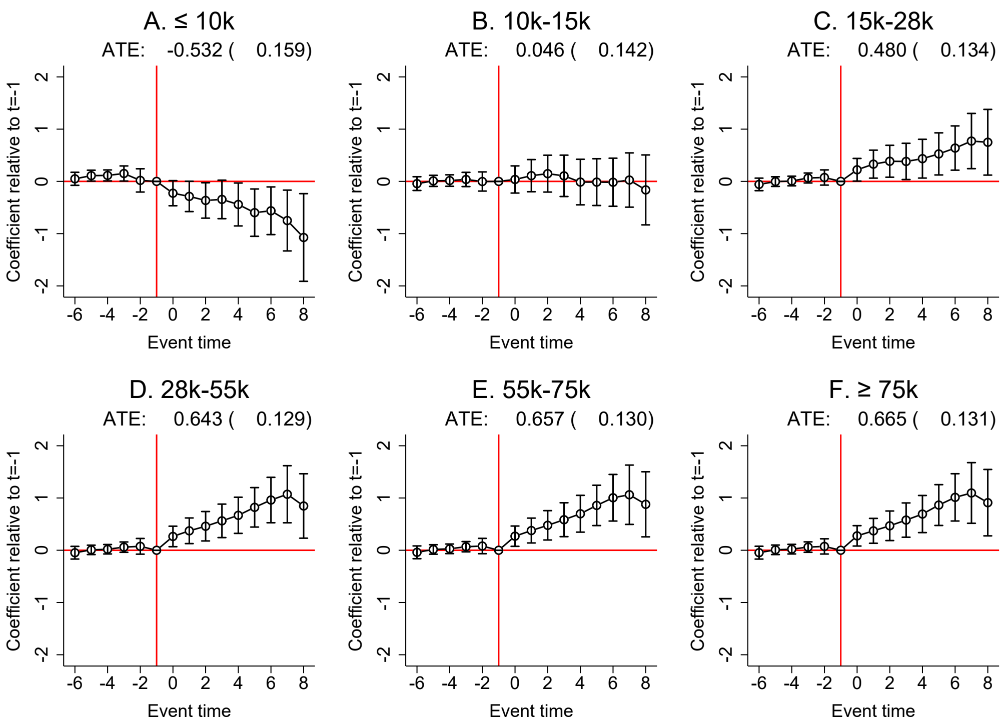
Figure 5: The Impact On Statutory Income Tax Rates



Note: This figure depicts the β coefficient estimate and 95 percent confidence intervals obtained from equation (9). It presents the average treatment effect (vertical axis) of the Ghost Buildings program on statutory marginal tax rate on personal income (horizontal axis). Data covering 7,709 municipalities observed over the 2001-2015 period.

Figure 6 displays the event study coefficient estimates and confidence intervals. The results point to a gradual adjustment in the marginal income tax rate progression. A 1 standard deviation increase in ghost buildings intensity led to an increase (decrease) by 1.105 (0.884) percent in the top (bottom) statutory marginal tax rate over the post-program period. This change in statutory marginal tax rate translates into a higher marginal rate progression: Figure B5 shows a positive and significant impact of the program on the marginal rate progression, computed by regressing the marginal tax rate on statutory tax thresholds. This shift in the distribution of the local income tax burden was achieved by a combination of higher statutory marginal tax rates for an income level above 15,000 euros and an higher tax exemption cutoff. Figure B4 shows that the program led municipalities to raise the no tax area: a 1 standard deviation increase in program intensity raised the tax exemption cutoff, on average, by around 21,807 euros. The increase was persistent and gradual, resembling the pattern of adjustment found for the statutory marginal tax rates.

Figure 6: Event Study Estimates on Statutory Income Tax Rates

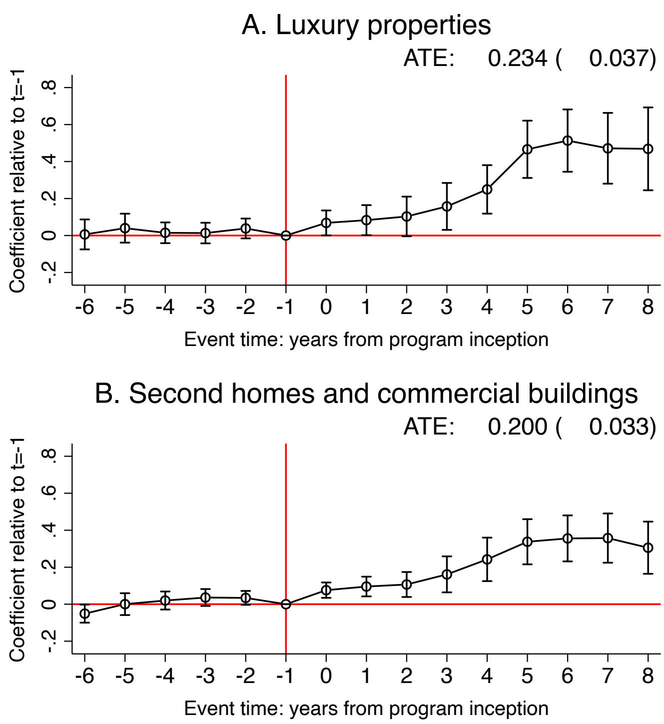


Note: This figure presents the impact of the Ghost Buildings program on log of statutory marginal tax rate on personal income set by municipalities. Each graph reports estimates relative to the marginal tax rate applied to a different income interval. The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality level. Data covering 7,709 municipalities observed over the 2001-2015 period.

4.3.2 Property Tax Rates

Figure 7 shows the event study estimates on the tax rate on luxury properties (top panel), and on second homes and commercial buildings (bottom panel). A gradual increase in both rates emerged over time. ATE estimates are highly significant in both cases and suggest that a 1 standard deviation increase in municipality-level program intensity raised the tax rate on luxury properties by around 0.39 percent, and by 0.33 percent on second homes and commercial buildings. This result suggests that municipalities raise the tax rates in response to a broader and more enforced tax base.

Figure 7: Event Study Estimates on Property Tax Rates



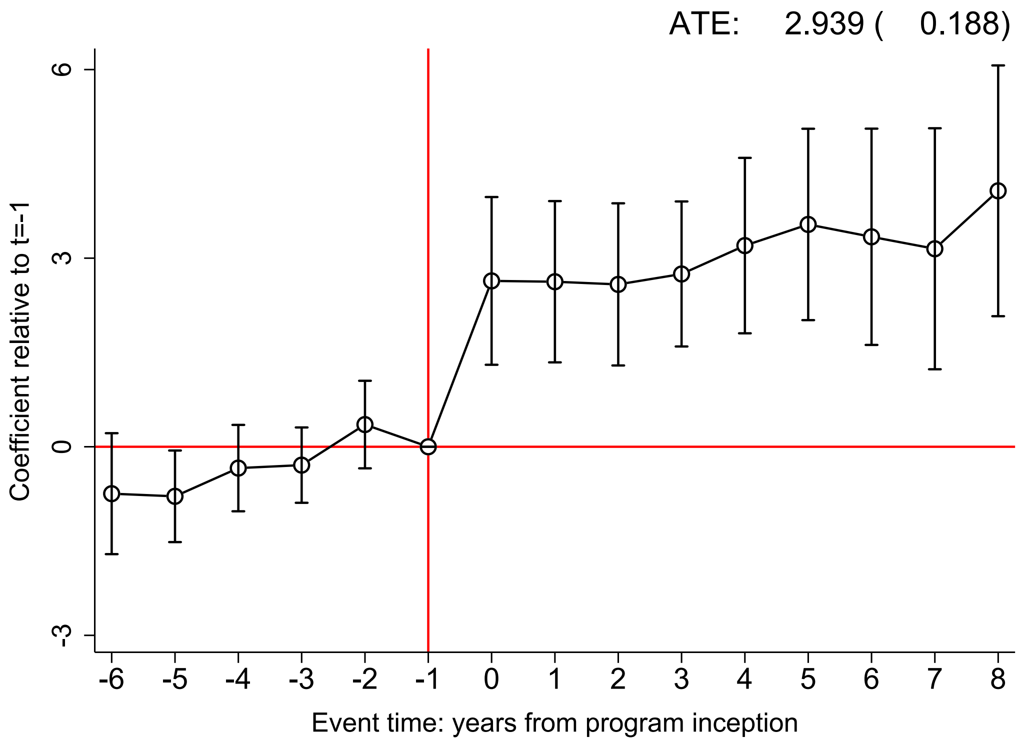
Note: This figure presents the impact of the Ghost Buildings program on the log of the tax rate set by municipalities on luxury properties reported as main residence (panel A) and on second homes and commercial buildings (panel B). The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality level. Data covering 7,709 municipalities observed over the 2001-2015 period.

Taken together, two main implications emerge from this analysis. First, the results suggest that fighting tax evasion shifts the distribution of the local income tax burden toward middle and top incomes, and toward wealthy property owners. Second, the joint change in statutory tax rates and tax base should lead to a substantial increase in tax revenue. I will now study the impact of the program on local tax revenue.

4.4 The Impact on Tax Revenue

Figure 8 displays the event study coefficient estimate and 95 percent confidence intervals for municipal tax revenue. Prior to the program, there was no significant association between ghost buildings intensity and tax revenue: differences in tax revenue were nearly constant and never significantly different from zero. Following the Ghost Buildings program inception, a 1 standard deviation increase in the share of detected unregistered buildings led to a nearly 4.9 percent increase in tax revenue. Using the previous example, if we moved from Trento to Crotone, we would observe an increase in tax revenue by around 33 percent in Crotone compared to Trento.

Figure 8: Event Study Estimates on Tax Revenue



Note: This figure presents the impact of the Ghost Buildings program on log of tax revenue. The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality-level. Data covering 7,709 municipalities observed over the 2001-2015 period.

This revenue increase emerged immediately and is the product of the mechanical increase in the tax base brought about ghost buildings' registration and the revenue increase driven by the tax hikes, net of any behavioral response to the program. In fact, the pattern of the β_j estimates is consistent with the constant tax base effect depicted in Figure 4.

4.5 Robustness Checks

This section tests potential threats to identification and the robustness of the results to alternative specifications. The main identifying assumption is that both the timing of program inception and the scope for inducing ghost buildings' registration was not the result of an effort adopted by municipalities. Since the program starting year is staggered in time, municipalities might have influence over when the program would start. One then might be worried that the original date of the program could be a response to local economic shocks. If this is the case, then my estimates would capture, at least in part, 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 values different from the one it would have had based on the modal date of publication in the province (see Figure A2). To deal with this discrepancy, I follow Casaburi and Troiano (2016) to implement an instrumental variable approach. I first compute the modal program starting year for each province, then I instrument the actual municipality-specific program inception year dummy with this binary variable defined at the provincial level. Table B2 compares my baseline ATE estimates, shown in column 1 and obtained by regressing equation (9), with the coefficient estimates obtained from a two-stage least squares (2SLS) model where the dummy for the actual program inception year is instrumented by the the provincial modal year. The second stage specification is similar to equation (9), but using the instrumented inception year. The table shows that 2SLS and baseline OLS estimates are highly similar, thus suggesting that selection in the program does not represent a serious threat for my findings.

I conduct an additional robustness check exploiting the fact that the policy was not implemented in the Trentino Alto-Adige region. I first impute the program inception year as the modal program year. Then, I perform a triple difference approach, where I augment my baseline approach with the difference in each outcome of interest with respect to Trentino Alto-Adige, before and after the program. Column 3 of Table B2 shows that the coefficient estimate from the triple difference approach are substantially similar to my baseline estimate based on a sample without Trentino Alto-Adige.

An additional concern is that the program intensity - measured as the share of detected unregistered buildings - would not be a good predictor of the number of ghost buildings that were actually registered in response to the policy. For example, one can be worried by a systematic cross-municipality association between the detected share of unregistered buildings and the ability to enforce registration. To investigate this issue, I first retrieve municipality-level data on registration rates up to the final wave of the program (April 2011). Then, I compare registration rates with the share of detected unregistered buildings. Figure B6 shows that the number of detected ghost buildings is a good predictor of the number of ghost buildings that were registered in response to the policy (correlation coefficient equal to 0.602). The figure shows a clear positive association between the two variables, thus suggesting that the municipality-level program scope strongly predicts the program actual impact. Using the program actual impact instead of the share of detected unregistered buildings yields relatively larger estimates on all the outcome variables explored in the main analysis (see Column 4 in Table B2). I thus interpret the coefficient estimated using the program scope as the lower bound effect of the program.

In settings with variation in treatment timing across units, the coefficient on a given lead or lag estimated from equations as in equation (8) can be contaminated by effects from other periods. A growing literature has proposed alternative estimators that are free of this source of contamination. Using the methodology developed by Borusyak et al. (2021), I test the sensitivity of my estimates to this source of bias. Appendix Figure B7 and Figure B8 shows that coefficient estimates remain fairly similar.[27]

Finally, I 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", I cluster the standard errors on a higher level of aggregation. Figure B9-Figure B12 show that estimates remain statistically significant at usual confidence intervals when I employ standard errors clustered at province- or region-level.

5 Mechanisms

This section analyzes the mechanisms driving the tax rate change in response to the Ghost Buildings program. For the sake of space, I report income tax estimates exclusively for the bottom and the top marginal tax rate. I explore three families of mechanisms.

First, I study whether the tax rate changes reflect an attempt of tackling local inequalities in income and/or home ownership.[28] If local policy makers aimed at fighting inequality through higher tax rates on top incomes, then the income tax progressivity increase would be relatively larger in places where the pre-program income distribution was more unequal. Likewise, if municipalities aimed at tackling inequalities in home ownership, then we should observe a larger tax rate increase in places where home ownership rate is lagging. I study these mechanisms by computing the municipality-specific pre-tax Gini index and the share of owner-occupied houses.[29] I

[27] Specifically, I first calculate the weights underlying the linear combination of treatment effects using an auxiliary regression. Examining the weights allows to measure how large the amount of treatment effects heterogeneity can be contaminated by treatment effects from other periods. Then, I estimate a model for non-treated potential outcomes using the non-treated (i.e., never-treated or not-yet-treated) observations only (including never-treated municipalities located in Trentino Alto-Adige region) that received positive weights.

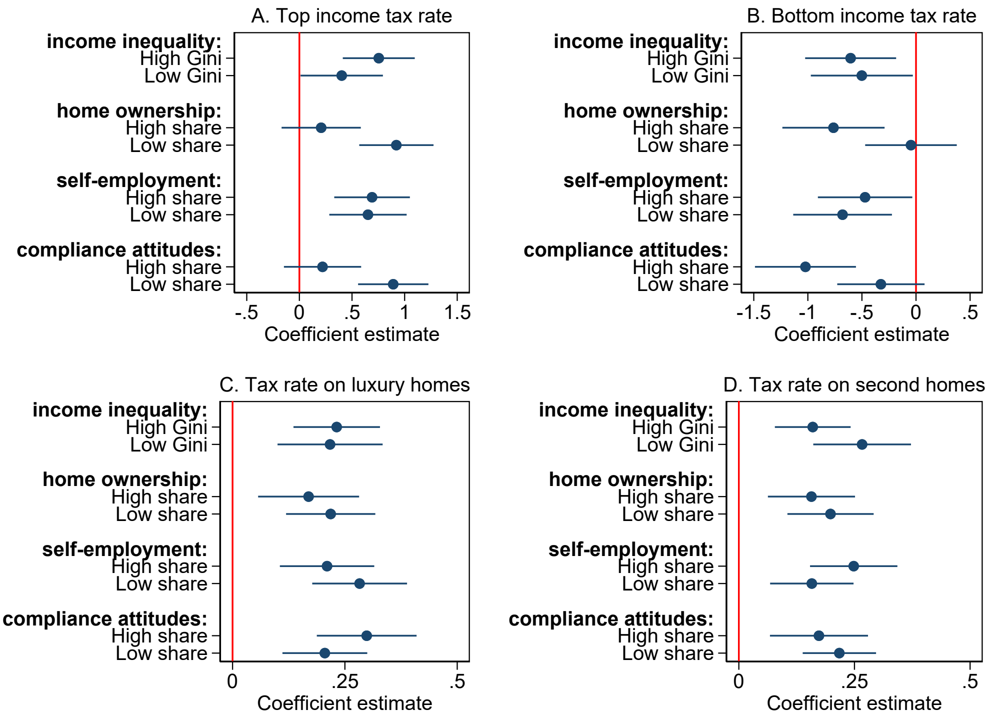
[28] A few studies have provided cross-country evidence of a negative association between tax progressivity and pre-tax income inequality (see, e.g., Piketty et al. 2014; Rubolino and Waldenström 2020).

[29] The Gini index is computed from municipality-specific tabulated data on taxable income from 7

split municipalities according to their pre-program level of income inequality - above (*High Gini*) versus below (*Low Gini*) the median - and their rate of home ownership as well.

Figure 9 reports the ATE computed from equation (9) and 95 percent confidence intervals. The figure shows that municipalities with higher ex ante level of pre-tax inequality were relatively more responsive in raising the top statutory marginal tax rate on income (panel A). However, point estimates do not significantly differ between the two municipality groups. The effect based on home ownership rate was instead stronger and statistically significant; it suggests that the tax hike was mostly concentrated in places where a relatively smaller share of the population was home owner. This would suggest that municipalities attempted to tax relatively more incomes of richer individuals located in places with higher property inequality.

Figure 9: Drivers of local tax rates choice



Note: This figure reports coefficient estimates and 95 percent confidence intervals obtained from regressing equation (9). Each estimate is based on splitting municipalities according to whether they are below or above the median value of the following municipality-specific variables (calculated over the pre-program period): i. *income inequality* refers to the Gini index calculated from grouped tax returns data; ii. *home ownership* is computed as the share of owner-occupied homes in a municipality, as reported in census data; iii. *self-employment* is the share of self-employment income, retrieved from tax returns; iv. *compliance attitudes* are computed from a region-level standardized score to the question "Do you justify tax cheating?" from the European Values Study.

A second potential channel that might influence the tax progressivity increase is the threat of larger responsiveness to evasion in response to the program. As illustrated by

income intervals, following the procedure proposed by Milanovic (1994) and Abounoori and McCloughan (2003). Home ownership rate is retrieved from the 2011 census.

equation (7) in the conceptual framework, the impact that stricter tax enforcement has on the responsiveness of a higher marginal tax rate in encouraging evasion determines the optimal slope of the best response function $T'(z)(\alpha)$. To proxy for the possibility of underreporting incomes, I use tax returns data to compute the share of aggregate taxable income in a municipality coming from self-employment. Intuitively, places where self-employment is a relatively more important source of income would present structural characteristics that facilitate manipulation of reported income, conditional on the scope for raising taxes through stricter tax enforcement.[30] The threat of this kind of behavioral responses might thus deter policy makers from raising tax progressivity in response to the program. I split municipalities according to the median level of self-employment income share, dividing them into above (*High share*) versus below (*Low share*) the median value of self-employment income share. The figure shows that this potential channel is not likely to be a mechanism behind the tax rate choice: the two coefficient estimates are highly similar.

Finally, I investigate the role of intrinsic attitudes toward tax compliance. For this end, I compute a region-level standardized score to the question "Do you justify tax cheating?" from the European Values Study. The figure shows that the top tax rate hike was mostly implemented in municipalities where a larger share of the population was more inclined to justify tax cheating. My interpretation for this result is that weaker intrinsic motivation for tax compliance deterred tax progressivity before the program inception. This interpretation is in line with the studies emphasizing the role of norms in determining tax evasion (see, e.g., Besley et al. 2021), suggesting that policies enforcing tax payments have the potential for substituting weaker tax compliance attitudes.

The bottom graphs display property tax estimates. None of the mechanisms presented above had any significant bear on property tax choice. Why property tax hikes are only relatively affected by inequality levels, threat of tax evasion, and/or tax compliance attitudes? A simple answer to this question is that property tax setting is structurally different from income tax setting. As a form of taxation, property tax is considered efficient to the extent it both broadens the tax base and applies to a base that is immovable and largely inelastic. In contrast, taxes on incomes and business activities tend to bring about behavioral change. However, I cannot exclude that other socio-economic and political factors that I am not able to capture were likely to explain the property tax rate increase.

6 Implications for the Size of the Public Sector

Do additional tax revenues increase public services' provision? Or are they diverted into political rents, thus dissipating any potential benefits of improved tax capacity? The existing literature has provided contrasting answers to this question. On the one

[30]See, e.g., Saez (2010) and Kleven et al. (2011) for evidence showing that tax evasion is much more likely to take place when income is not third-party reported.

hand, a rich literature has found little, if any, effects on both quantity and quality of public goods from windfall revenue gains (see, e.g., Vicente 2010; Caselli and Michaels 2013; Brollo et al. 2013; Borge et al. 2015). On the other hand, political agency models of public finance emphasize the notion of revenue sources: relying more on *tax* revenue instead of *non-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).[31] To shed light on this question, this section studies the effect of the program on municipality-level public spending and public employment.

6.1 The Impact on Public Spending

Figure 10 shows the impact of the Ghost Buildings program on municipality-level public spending, computed as the sum of current expenditures in schools, environment, and cultural, social, and sport activities. It depicts the event study coefficient estimates and 95 percent confidence intervals from equation (8). The figure shows a gradual and persistent impact of the program on public spending. On average, a 1 standard deviation increase in program intensity raised public spending by 1.7 percent. This corresponds to around 69,000 euros of the pre-program mean, that is an increase of nearly about 9 euros per-capita.

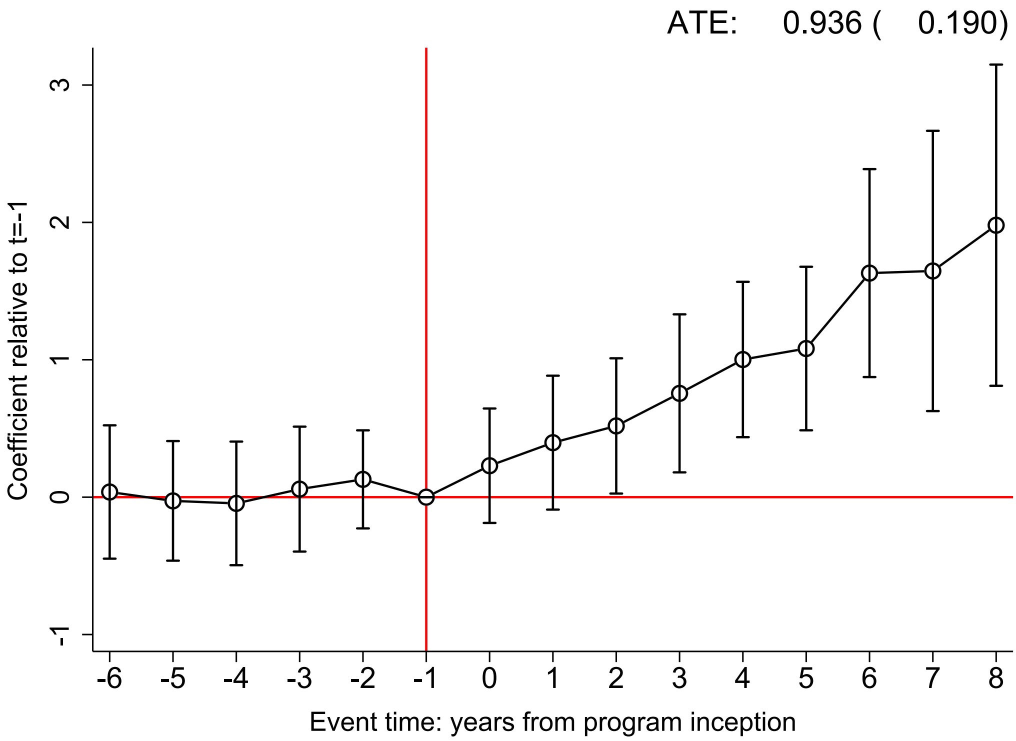
This result confirms the findings previously shown in Casaburi and Troiano (2016) using a 2SLS model, and it is in line with a recent stream of the public finance empirical literature suggesting that tax capacity improvement may be able to finance public goods' provision. For instance, Gadenne (2017) shows that improvements in tax collections raise school investments in Brazilian municipalities. Martinez (2019) exploited 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. Focusing on property taxation in Mexico City, Brockmeyer et al. (2021) show that governments can actively finance public goods though higher tax rates even in settings with significant under-compliance and liquidity constrained individuals.

6.2 The Impact on Public Sector Employment

A few papers have provided both empirical and theoretical evidence that stricter law and tax enforcement has the potential to affect labor market outcomes. For instance, by leveraging variation in enforcement of labor inspections cross Brazilian cities, Almeida and Carneiro (2012) find that stricter enforcement leads to an increase in the share of the population in formal employment. Ulyssea (2018) develops and estimates an equilibrium model for understanding informality. In the model, heterogeneous firms can

[31] Informational asymmetries arise 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.

Figure 10: The Impact on Public Spending

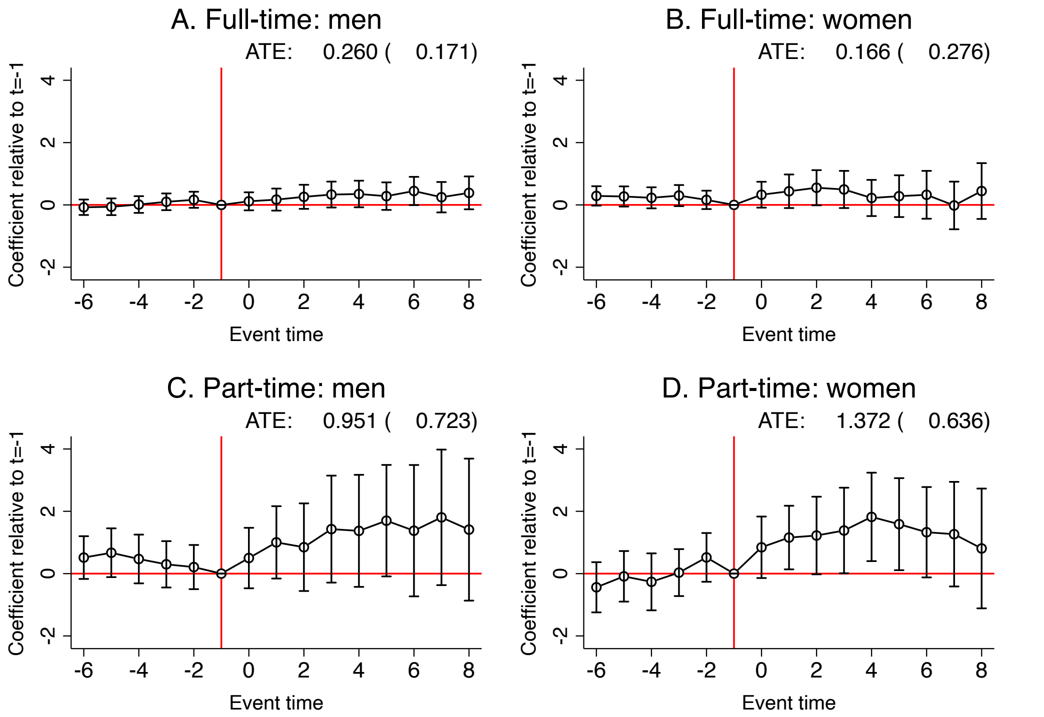


Note: This figure presents the impact of the Ghost Buildings program on log of tax revenue. The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality-level. Data covering 7,709 municipalities observed over the 2001-2015 period.

either not register their business or hire workers "off the books." He shows that, under some assumptions, curbing informality can raise output, total factor productivity and welfare.

I contribute to this literature by studying the impact of the Ghost Buildings program on public sector employment. Figure 11 shows the event study coefficient estimates. Top graphs focus on full-time workers, dividing them by gender, while bottom graphs reports the estimates for part-time employment. The figure shows a positive effect of the program on public employment, but coefficient estimates are statistically significant only for part-time female employment. These estimates imply that a 1 standard deviation increase in ghost buildings intensity raised part-time female employment in the public sector by around 2.5 percent, while male employment raised by around 1.7 percent. Figure B13 shows that this employment effect reflects new hires. A natural interpretation for this result is that tax revenue raises public sector labor demand, perhaps by simply suggesting that some of the program-induced extra tax revenue were invested to hire new workers.

Figure 11: The Impact on Public Sector Employment



Note: This figure presents the impact of the Ghost Buildings program on log of public sector employment, separately for full-time vs part-time workers, and by gender. The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality-level. Data covering 7,709 municipalities observed over the 2001-2015 period.

7 Concluding Remarks

The power to tax is at the heart of creating an effective state. This paper provides quasi-experimental evidence that curbing informality by tackling tax evasion increases tax progressivity and the size of the public sector. My research design exploits cross-municipality variation in the scope for curbing income and property tax evasion by enforcing registration of previously unregistered (taxable) buildings. The policy significantly broadened the tax base and raised tax collections, inducing many taxpayers, in particular top incomes, to register their buildings. In response to a broadened and better enforced tax base, municipalities adjusted the distribution of the local tax burden in favor of higher statutory marginal tax rates for middle and top incomes, and for owners of second homes and commercial buildings. As a result of larger tax collections, municipalities hired more workers and raised public spending.

These results suggest that informality deters tax progressivity and the size of the public sector. Policies enforcing payment of taxes and proposing new technologies for tax collections have thus not only the power to foster tax capacity - a stylized fact in the existing literature (see, e.g., Besley and Persson 2009; Gordon and Li 2009; Besley and Persson 2013; Besley 2020) - but also to enhance the ability to pursue redistributive policies. Despite the specific peculiarity of the Ghost Buildings program, the conclusions emerging from this paper can be of wide applicability for understanding the development of tax systems in countries where tax evasion is a long-standing policy concern.

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Appendices

A The Ghost Buildings program

A.1 Identification Procedure

In the following, I 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. I graphically show this procedure in Figure A1 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 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).

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.

The initial legislation states that owners must register the ghost building on land registry by April 30, 2011. Accordingly, administrative data show that about 40 percent of the ghost buildings was registered by the end of April 2011. After such date, the *Agenzia del Territorio* proceeded with follow-up inspections to impute the tax base for the remaining unregistered buildings. Delayed registration charged additional penalties and a fee for the extra inspection to the owners of buildings.

A.2 Comparison With Other Indicators of Tax Evasion and Informality

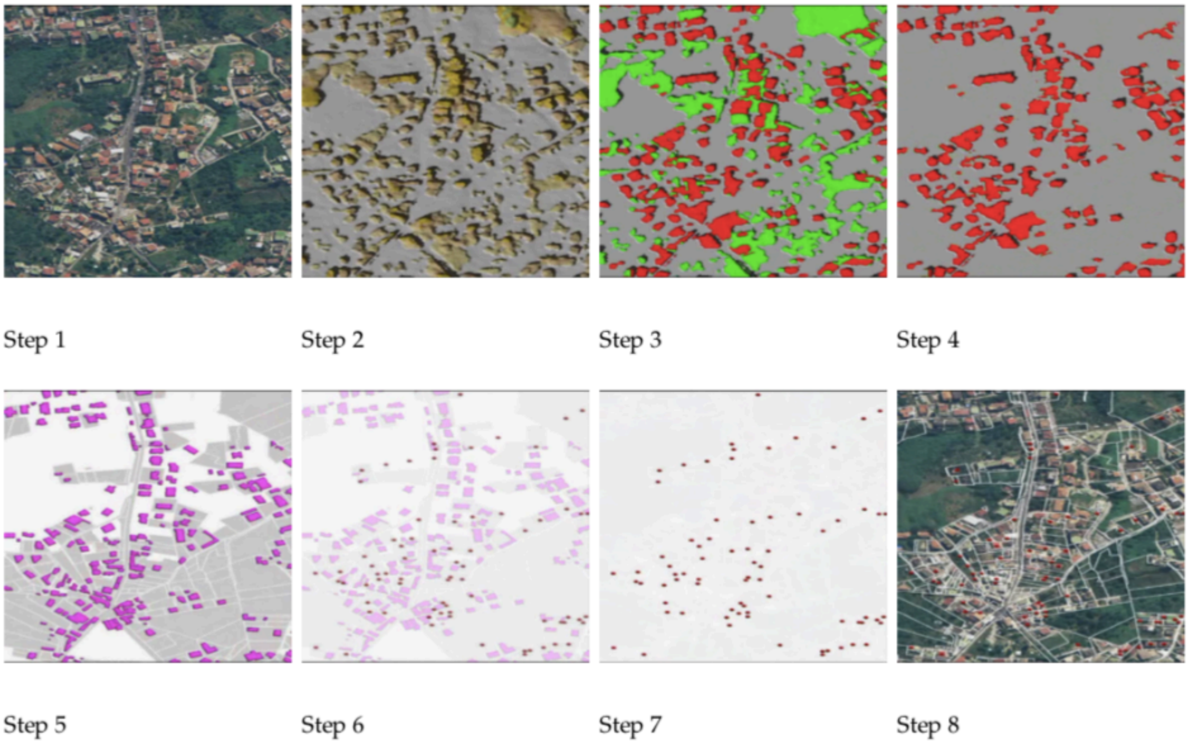
To validate this indicator as a proxy for tax evasion, Figure A3 compares ghost buildings intensity (y-axis) with two regional-level estimates of the tax gap (x-axis).[32] The left-hand side graph compares ghost buildings intensity with Galbiati and Zanella (2012)'s estimate of the tax gap, which uses 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, Vega Pansini and Pisani (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).

Informality might also be the result of low bureaucracy efficacy and weak enforcement at local level. A recent report by the World Bank, "Doing Business in the European Union 2020", has examined the role played by local authorities in determining how national regulations are implemented. Focusing on 13 Italian cities, they construct several city-level *Doing Business* indicators, including enforcing contracts, registering properties, and dealing with construction permits. In each indicator, geographical vari-

[32] In this figure, the ghost building intensity indicator is computed as the municipal population-weighted regional average.

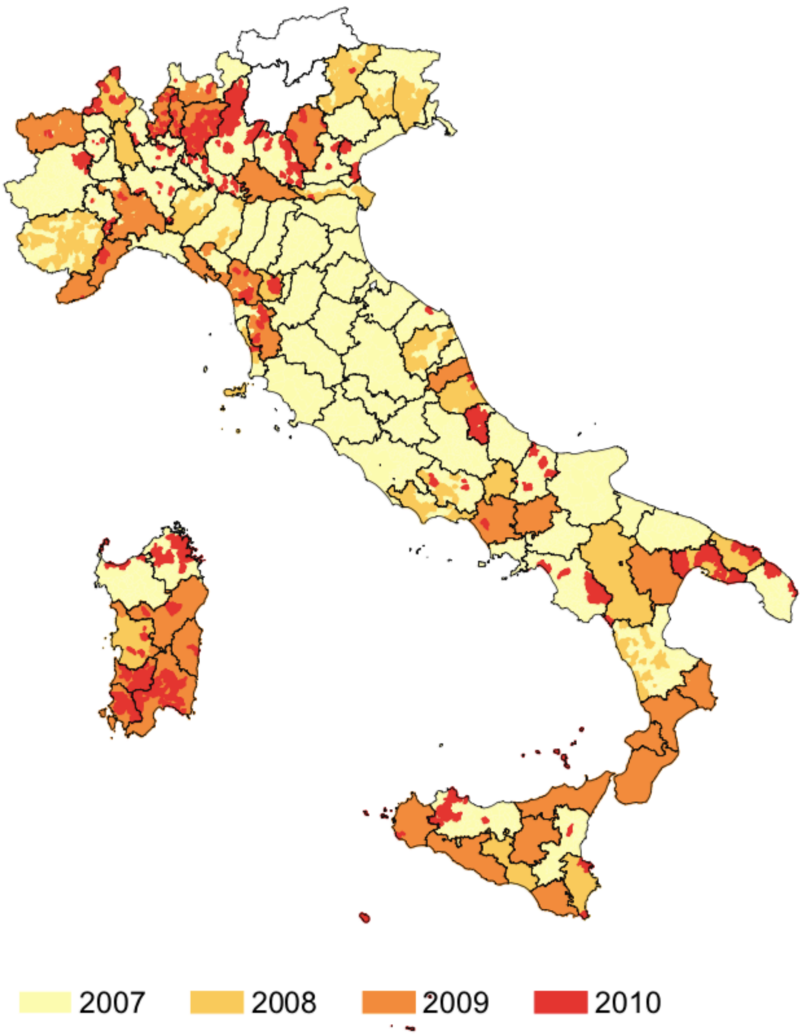
ations are particularly large. For instance, cities in the North of Italy, such as Milan, have the most streamlined and fastest processes for starting a business and enforcing contracts, while cities in the South of Italy, such as Naples, are the hardest in which to start a business or enforcing contracts. Appendix Figure A4 shows that a better performance in these indicators is associated with a lower share of detected unregistered buildings in a city (with a correlation coefficient of -0.62).

Figure A1: Identification process for ghost buildings



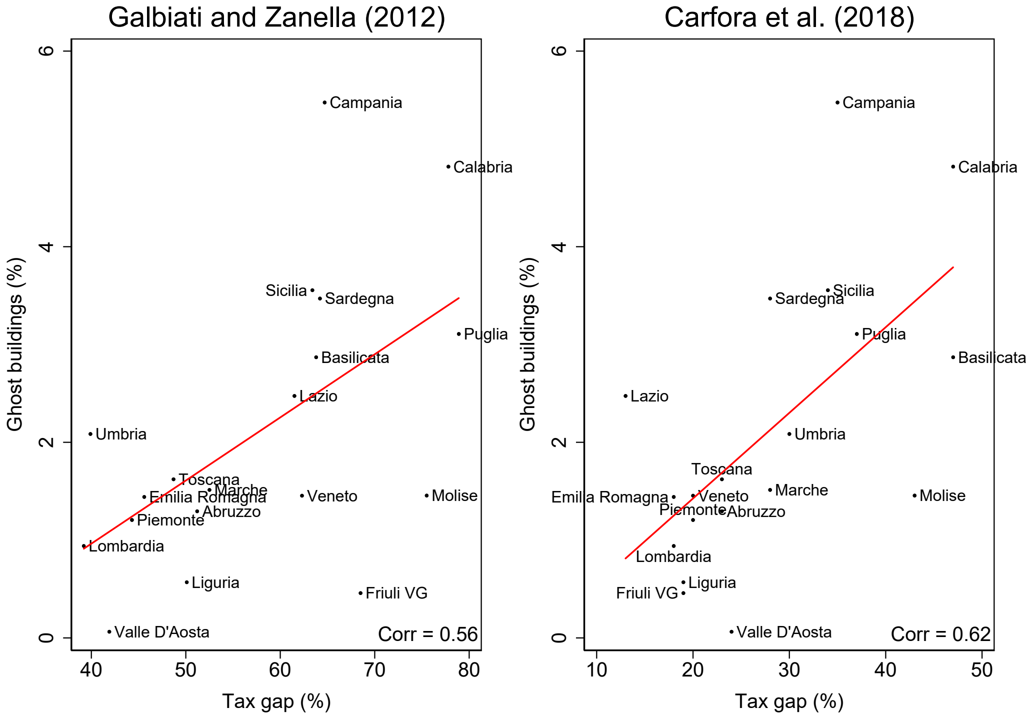
Note: See the text for details.

Figure A2: Timing of program inception



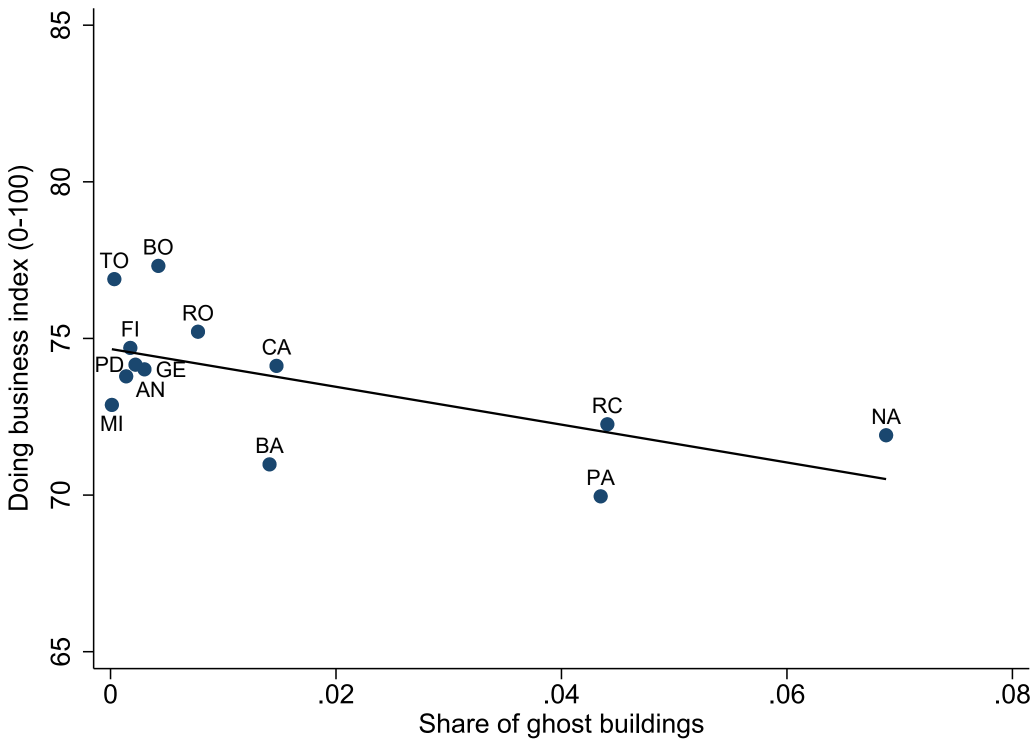
Note: This figure depicts the Ghost Buildings program inception year. Trentino Alto-Adige region (the white area in North-East) did not participate into the program. The exact dates are the following: August 2007; October 2007; December 2007; December 2008; December 2009; September 2010. Data from the Italian Internal Revenue Agency.

Figure A3: 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.

Figure A4: Comparing "Doing Business" indicators with share of unregistered buildings



Note: The figure scatters the city-level doing business index with the share of detected unregistered buildings. The *Doing Business* indicator includes information on enforcing contracts, registering properties, and dealing with construction permits. The figure also shows the line of best fit.

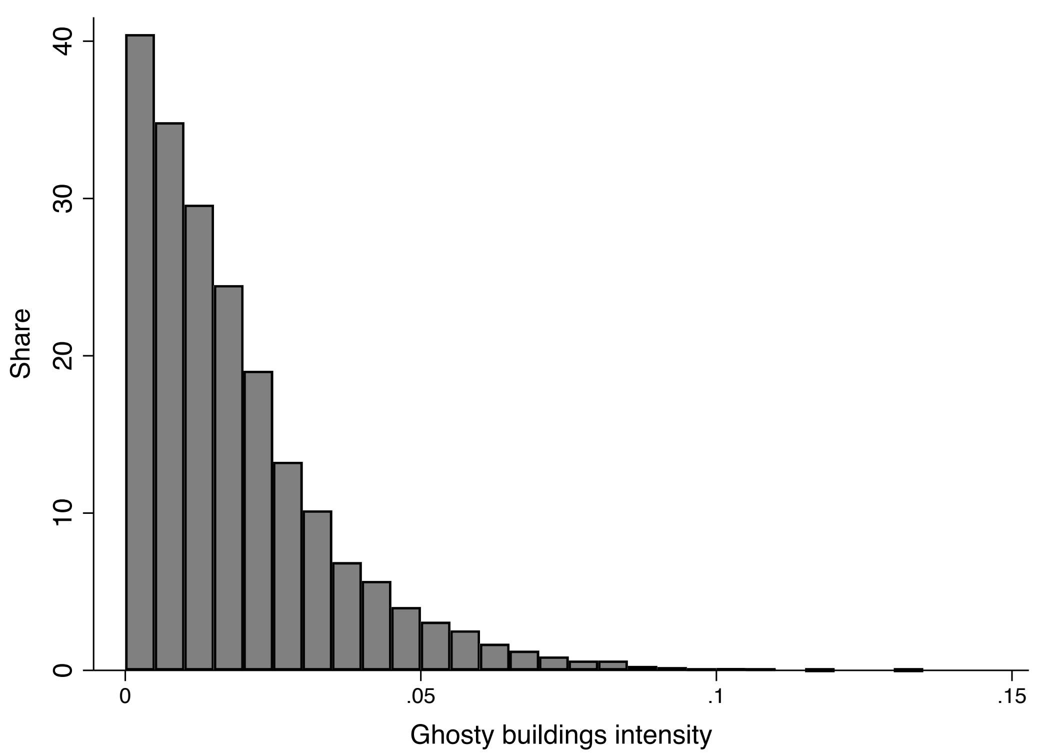
Table A1: The Ghost Buildings program

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*).

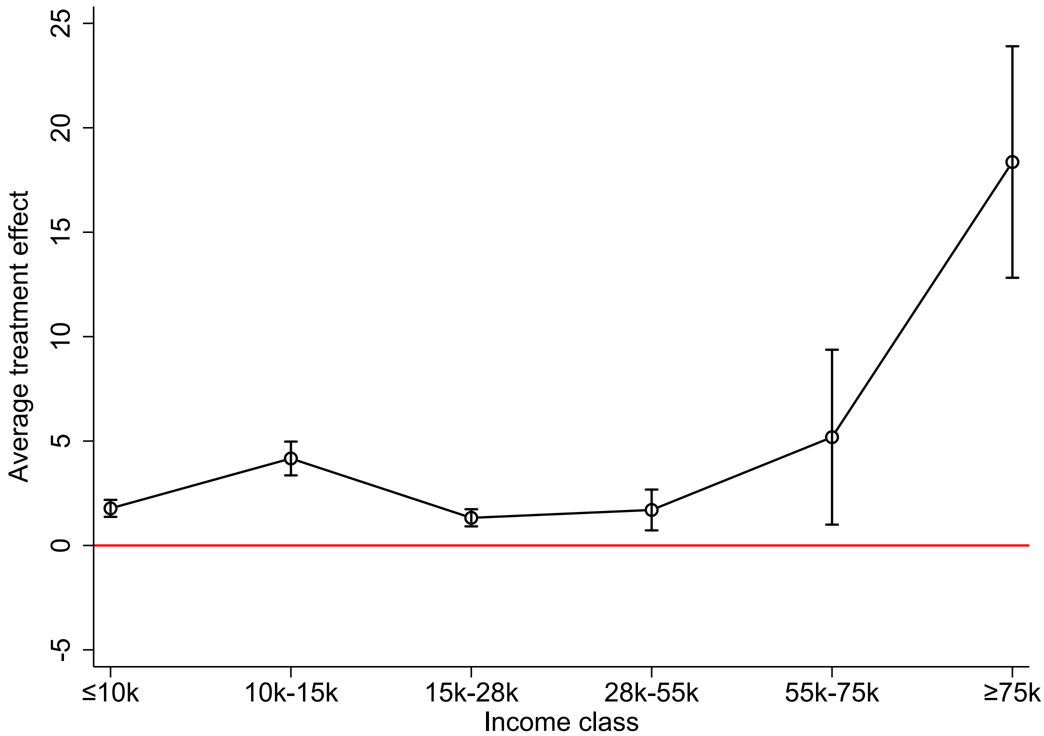
B Additional Figures and Tables

Figure B1: Distribution of Ghost Buildings Intensity



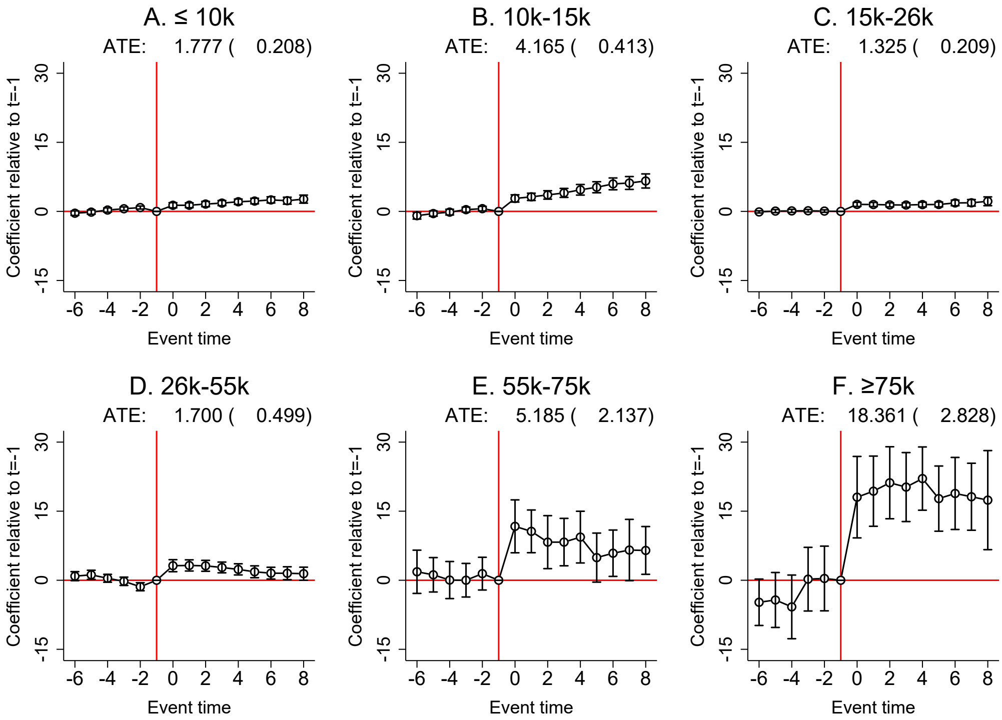
Note: The histograms shows the distribution of ghost buildings intensity, defined as the ratio between the number of land registry parcels containing ghost buildings and the total number of parcels in each municipality.

Figure B2: The Impact On the Tax Base by Income Bracket



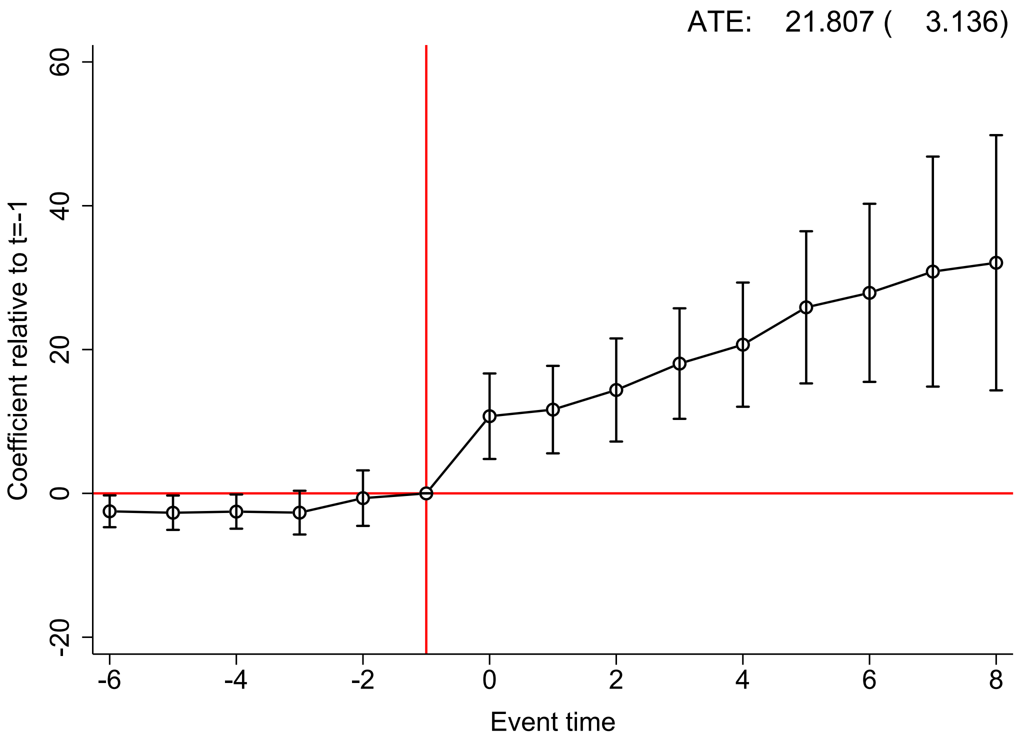
Note: This figure depicts β coefficient estimate and 95 percent confidence intervals obtained from equation (9). It presents the average treatment effect (vertical axis) of the Ghost Buildings program on the income group-specific tax base (horizontal axis). Data covering 7,709 municipalities observed over the 2001-2015 period.

Figure B3: Event Study Estimates on Tax Base



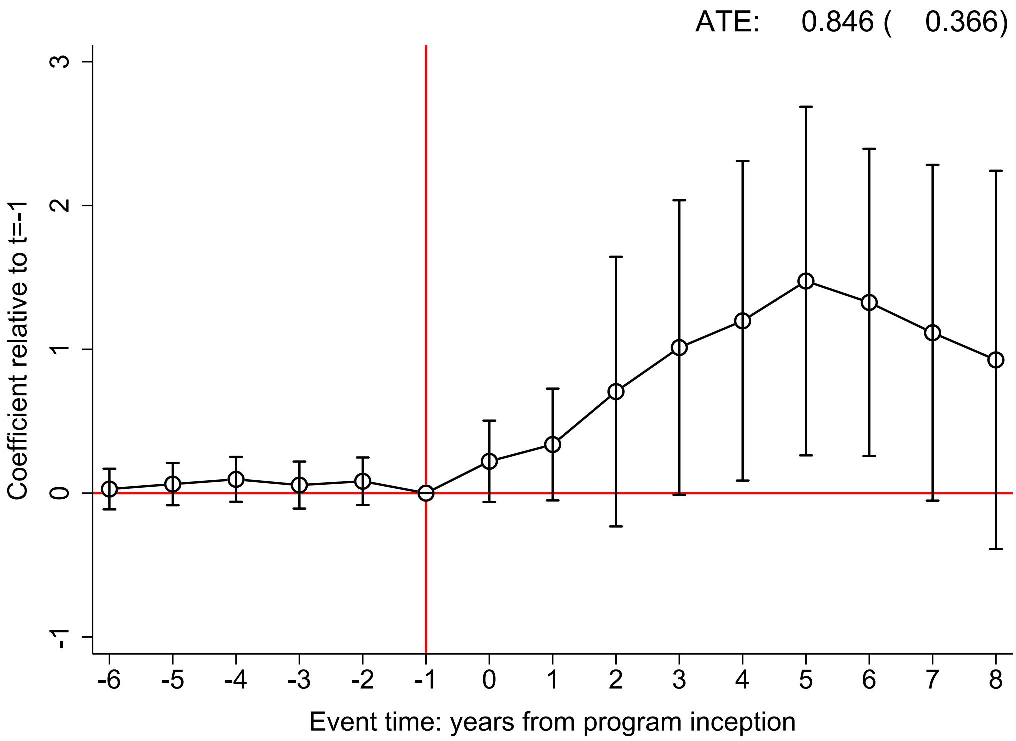
Note: This figure presents the impact of the Ghost Buildings program on log of the municipal tax base, divided in six different income intervals. The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality-level. Data covering 7,709 municipalities observed over the 2001-2015 period.

Figure B4: Event Study Estimates on the Tax Exemption Cutoff



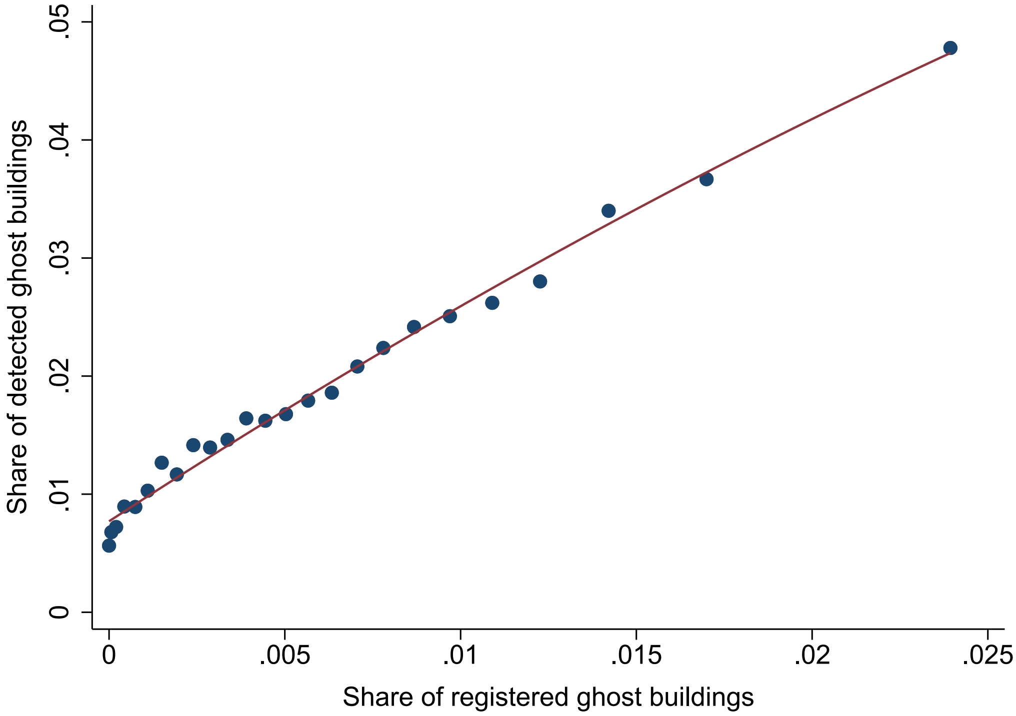
Note: This figure presents the impact of the Ghost Buildings program on the income tax exemption cutoff set by municipalities (in thousands euros). The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality-level. Data covering 7,709 municipalities observed over the 2001-2015 period.

Figure B5: Event Study Estimates on the Marginal Rate Progression



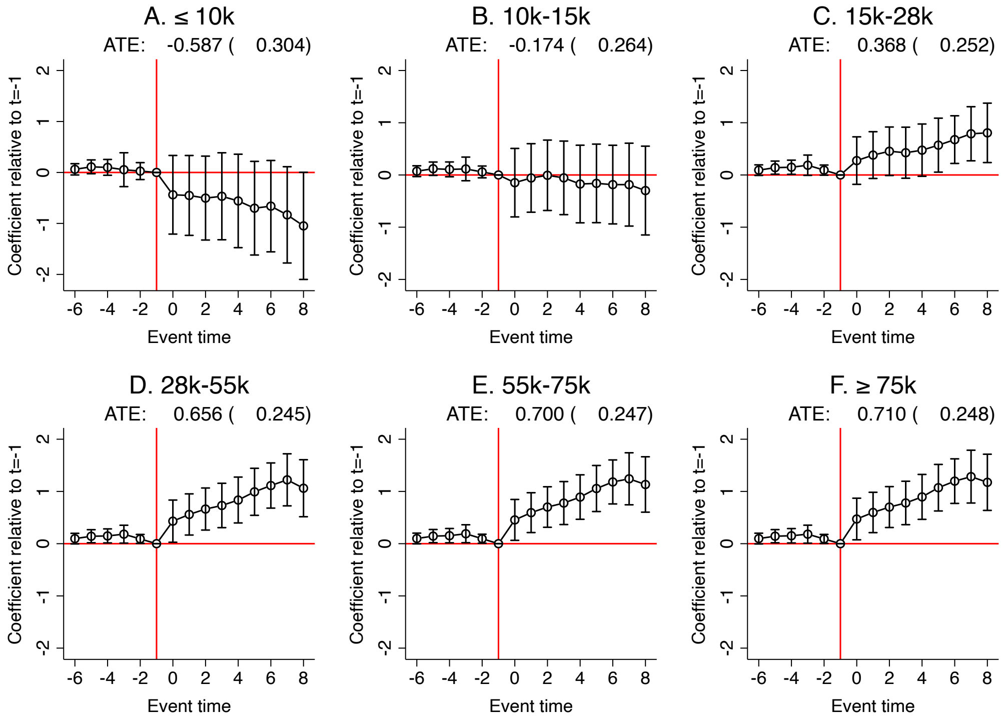
Note: This figure presents the impact of the Ghost Buildings program on log of the marginal rate progression of the income tax, computed by regressing marginal rates on the log of statutory income thresholds. The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality-level. Data covering 7,709 municipalities observed over the 2001-2015 period.

Figure B6: Registered Ghost Buildings Intensity



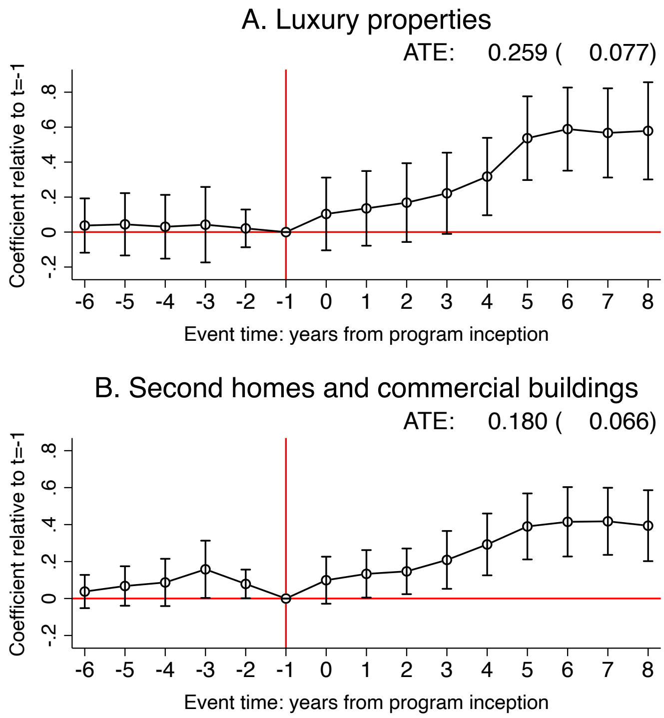
Note: The scatter plots the relation between ghost buildings intensity (the share of land parcels containing detected ghost buildings) with the share of ghost buildings that get registered by April 2011. The figure plots the relationship in 25 equal sized bins and shows the line of best fit.

Figure B7: Robustness to Negative Weights: Income Tax Rates



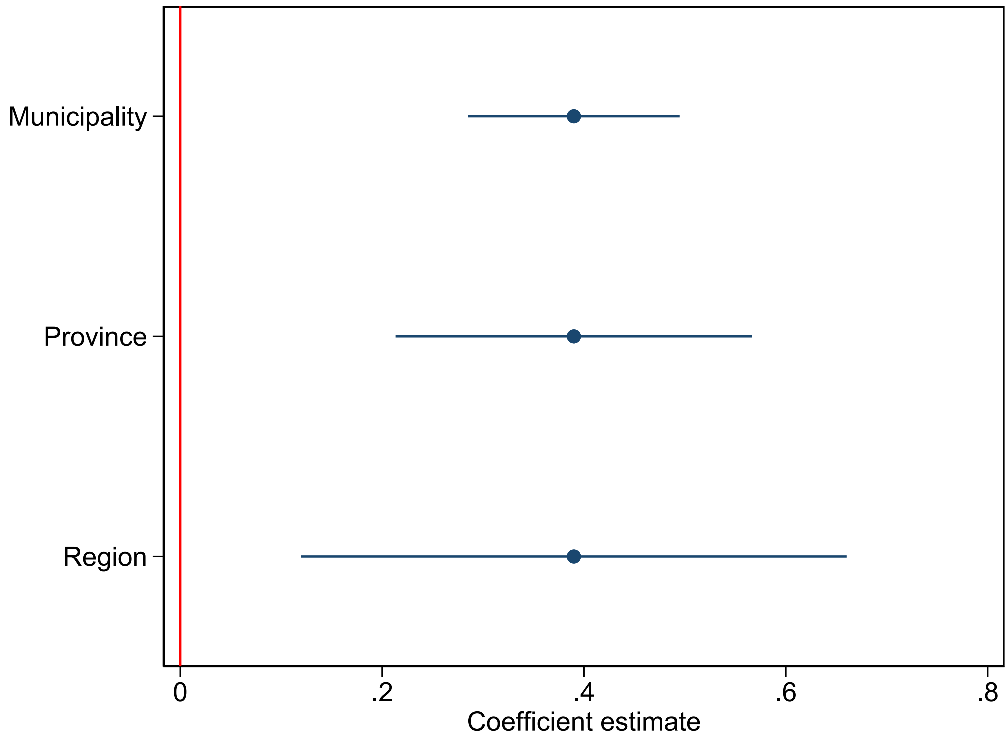
Note: This figure presents the impact of the Ghost Buildings program on log of statutory marginal tax rate on personal income set by municipalities. Each graph reports estimates relative to the marginal tax rate applied to a different income interval. The sample excludes municipalities that receive a negative weight (using the methodology proposed by Borusyak et al. (2021)). The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality level.

Figure B8: Robustness to Negative Weights: Property Tax Rates



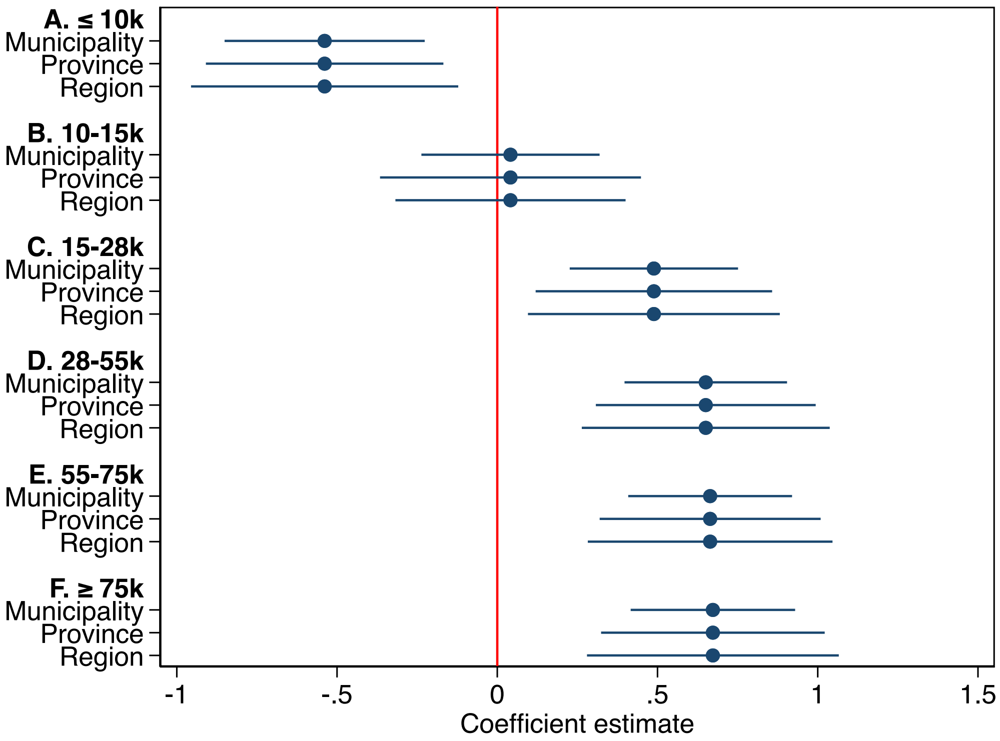
Note: This figure presents the impact of the Ghost Buildings program on the log of the tax rate set by municipalities on luxury properties reported as main residence (panel A) and on second homes and commercial buildings (panel B). The sample excludes municipalities that receive a negative weight (using the methodology proposed by Borusyak et al. (2021)). The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality level.

Figure B9: Robustness to clustering choice: tax base



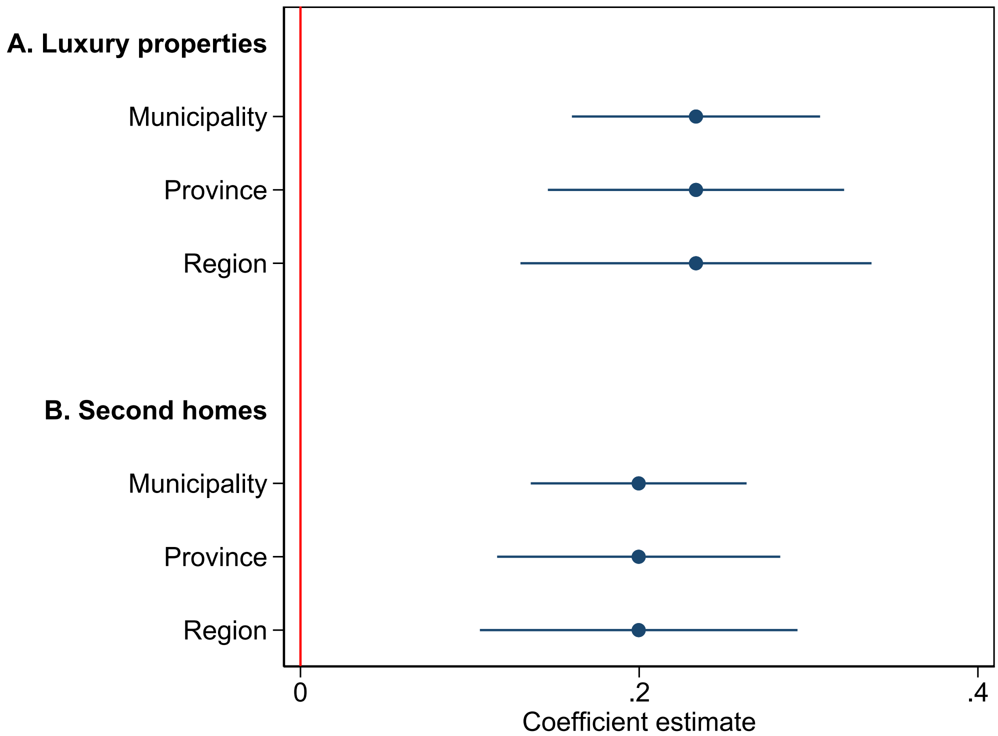
Note: The figure reports the β coefficient estimates and 95 percent confidence intervals obtained by regressing equation (9). The only difference across specifications is the clustering level: I start from the baseline municipality level, then I cluster at province and region level.

Figure B10: Robustness to clustering choice: income tax rates



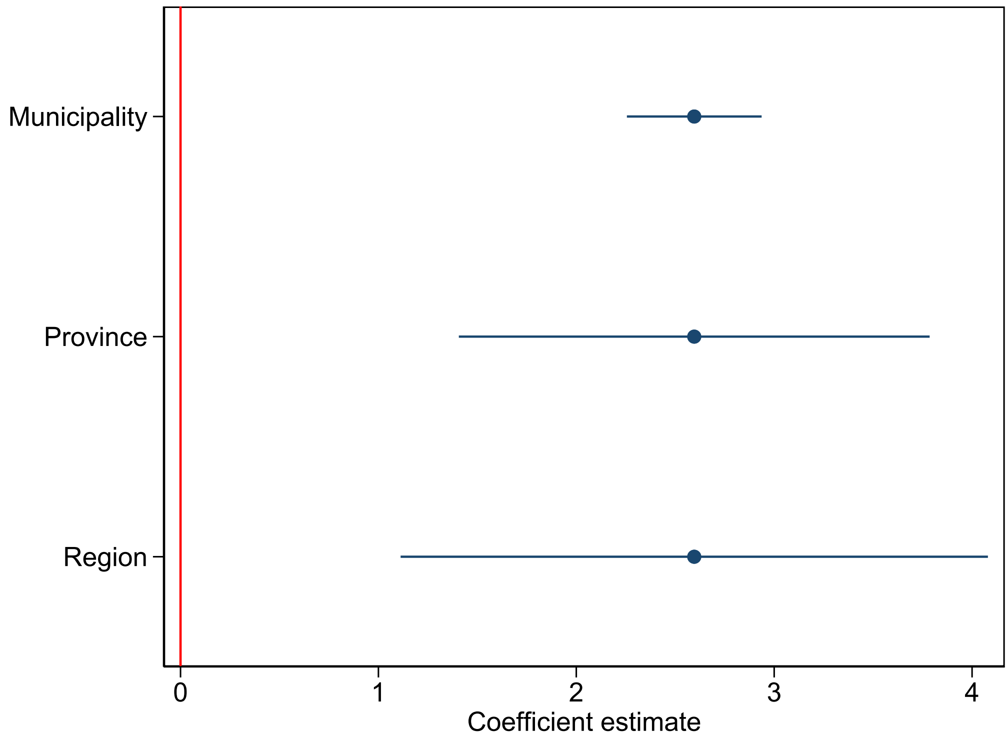
Note: The figure reports the β coefficient estimates and 95 percent confidence intervals obtained by regressing equation (9). The only difference across specifications is the clustering level: I start from the baseline municipality level, then I cluster at province and region level.

Figure B11: Robustness to clustering choice: property tax rates



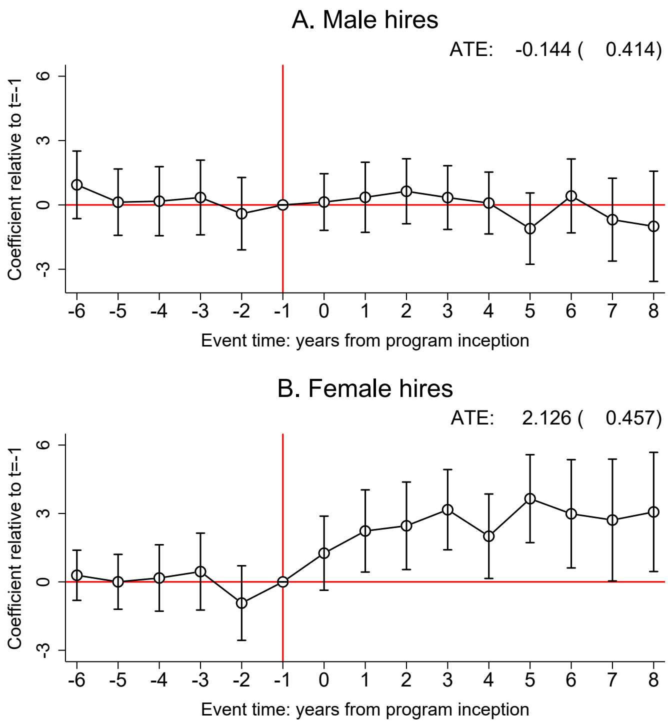
Note: The figure reports the β coefficient estimates and 95 percent confidence intervals obtained by regressing equation (9). The only difference across specifications is the clustering level: I start from the baseline municipality level, then I cluster at province and region level.

Figure B12: Robustness to clustering choice: tax revenue



Note: The figure reports the β coefficient estimates and 95 percent confidence intervals obtained by regressing equation (9). The only difference across specifications is the clustering level: I start from the baseline municipality level, then I cluster at province and region level.

Figure B13: The Impact on Public Employment by Contract



Note: This figure presents the impact of the Ghost Buildings program on log of public sector new hires, separately by gender. The figure plots the estimated β_j coefficients from equation (8) and the 95 percent confidence intervals: each point shows the effect of having implemented the program for j years (if $j > -1$) or of starting the policy j years before (if $j < -1$) relative to the year before the actual program starting year. The figure also reports the average treatment effect obtained by estimating β from equation (9) and standard errors clustered at municipality-level. Data covering 7,709 municipalities observed over the 2001-2015 period.

Table B1: Summary Statistics

	Obs (1)	Mean (2)	SD (3)	Min (4)	Max (5)
			A. Ghost Buildings program		
Detected unregistered buildings	113,788	289	479	0	6,891
Program inception year = 2007 (0/1)	113,788	0.560	0.469	0	1
Program inception year = 2008 (0/1)	113,788	0.119	0.324	0	1
Program inception year = 2009 (0/1)	113,788	0.245	0.430	0	1
Program inception year = 2010 (0/1)	113,788	0.076	0.264	0	1
Ghost buildings intensity (%)	113,788	0.019	0.017	0	0.131
			B. Tax rates		
Tax rate on luxury properties (%)	113,784	0.494	0.090	0	0.860
Tax rate on second homes (%)	113,784	0.675	0.152	0.300	1.110
Tax exemption cutoff (euros)	113,784	1,160	3,548	0	75,000
Marginal tax rate for income $\leq$10k (%)	113,784	0.279	0.254	0	0.800
Marginal tax rate for income 10-15k (%)	113,784	0.308	0.257	0	0.900
Marginal tax rate for income 15-28k (%)	113,784	0.338	0.258	0	0.900
Marginal tax rate for income 28-55k (%)	113,784	0.349	0.259	0	0.900
Marginal tax rate for income 55-75k (%)	113,784	0.351	0.262	0	0.900
Marginal tax rate for income $\geq$75k (%)	113,784	0.352	0.264	0	0.900
			C. Tax base		
Tax base (1,000 euros)	113,784	107,493	797,746	136	54,554,224
Tax base in range 1-10k (1,000 euros)	113,784	10,022	46,988	18,051	3,598,862
Tax base in range 10-15k (1,000 euros)	113,784	11,982	57,456	0	4,535,847
Tax base in range 15-28k (1,000 euros)	113,784	34,811	191,873	0	13,990,394
Tax base in range 28-55k (1,000 euros)	113,784	30,869	253,633	0	18,825,265
Tax base in range 55-75k (1,000 euros)	113,784	6,281	67,560	0	5,048,042
Tax base in range $\geq$75k (1,000 euros)	113,784	13,526	198,288	0	13,567,566
			D. Tax revenue, public spending, and public employees		
Tax revenue (1,000 euros)	111,009	2,668	24,517	0	2,177,848
Public spending (1,000 euros)	110,999	4,081	36,253	0	3,138,471
Full-time male employees	111,567	25.875	185.175	0	10,810
Full-time female employees	111,567	23,471	242,228	0	15,983
Part-time male employees	111,567	1.151	8.914	0	914
Part-time female employees	111,567	3,459	26,548	0	1,359
			E. Other variables		
Population	111,031	7,625	41,584	30	2,872,021
Female mayor (0/1)	109,256	0.103	0.304	0	1
Mayor has college degree (0/1)	109,256	0.425	0.494	0	1
Mayor age	109,256	49.087	9,846	19	94
Share of female in town council	109,256	0.191	0.123	0	1
Share of college degree in town council	109,256	0.253	0.166	0	1
Average age in town council	109,256	44.202	4.220	22	80

Note: This table reports summary statistics of the variables used in the empirical analysis.

Table B2: Robustness Checks

	Baseline (1)	2SLS (2)	Triple difference (3)	Registered share (4)
	A. *Outcome*: Tax base			
$1(K_{i,t} > 0) \cdot GB_i$	0.390***	0.398***	0.398***	0.620***
	(0.053)	(0.057)	(0.053)	(0.132)
	B. *Outcome*: Tax rate on income class $\leq$ 10k			
$1(K_{i,t} > 0) \cdot GB_i$	-0.539***	-0.558***	-0.539***	-1.091***
	(0.159)	(0.167)	(0.159)	(0.407)
	C. *Outcome*: Tax rate on income class 10-15k			
$1(K_{i,t} > 0) \cdot GB_i$	0.041	0.034	0.041	0.305
	(0.142)	(0.149)	(0.142)	(0.357)
	D. *Outcome*: Tax rate on income class 15-28k			
$1(K_{i,t} > 0) \cdot GB_i$	0.489***	0.491***	0.489***	1.353***
	(0.134)	(0.141)	(0.134)	(0.312)
	E. *Outcome*: Tax rate on income class 28-55k			
$1(K_{i,t} > 0) \cdot GB_i$	0.651***	0.658***	0.651***	1.479***
	(0.129)	(0.135)	(0.129)	(0.310)
	F. *Outcome*: Tax rate on income class 55-75k			
$1(K_{i,t} > 0) \cdot GB_i$	0.664***	0.669***	0.665***	1.491***
	(0.130)	(0.136)	(0.130)	(0.312)
	G. *Outcome*: Tax rate on income class $\geq$ 75k			
$1(K_{i,t} > 0) \cdot GB_i$	0.673***	0.679***	0.673***	1.531***
	(0.131)	(0.137)	(0.131)	(0.314)
	H. *Outcome*: Tax revenue			
$1(K_{i,t} > 0) \cdot GB_i$	2.939***	2.885***	2.214***	3.371***
	(0.188)	(0.193)	(0.188)	(0.494)
Observations	111,714	111,714	115,785	111,714
Baseline controls	Yes	Yes	Yes	Yes

Note: Column (1) presents β coefficient estimates (baseline effects) obtained by regressing equation (9) and municipality level clustered standard errors. Column (2) reports 2SLS estimates where the program inception year is instrumented by the provincial modal inception year. Column (3) rests on a triple difference approach, exploiting the fact that the program was not implemented in the Trentino Alto-Adige region. Column (4) reports estimates based on using actual registration rates as measure of program intensity.