Ghost-House Busters: The Electoral Response to a Large Anti Tax Evasion Program

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12 December 2013

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First Version: October 2011

This Version: December 2013

Abstract

The incentives of political agents to enforce tax collection are key determinants of the levels of compliance. We study the electoral response to the Ghost Buildings program, a nationwide anti-tax evasion policy in Italy that used innovative monitoring technologies to target buildings hidden from tax authorities. Two million buildings were registered as a result of the program. Our difference-in-differences identification strategy exploits both variation across towns in the ex-ante program scope to increase enforcement as well as administrative data on actual building registrations. Local incumbents experience an increase in their reelection likelihood as a consequence of the policy. In addition, these political returns are higher in areas with lower tax evasion tolerance and with higher speed of public good provision, implying complementarity among enforcement policies, the underlying tax culture, and government efficiency.

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1 Introduction

A government's ability to enforce tax collection efficiently is one of the fundamental components of state capacity and, in turn, has historically been an important driver of economic development. Tax evasion generates significant losses and distortions in government revenues.[1] The literature (e.g., Slemrod (2007); Besley and Persson (2013)) describes three main determinants of tax compliance: enforcement technology, political incentives, and cultural norms. This paper illustrates the interaction among these three factors. We estimate the electoral returns – the change in reelection likelihood – that local policymakers obtain from a nationwide anti-tax evasion policy in Italy based on an innovation in tax-compliance monitoring technology. In addition, we study how these electoral returns depend on underlying social preferences for tax compliance and on local government efficiency in the provision of public goods. To the best of our knowledge, this paper provides the first empirical evidence concerning voters' responses to anti-tax evasion policies.

Measures to reduce tax evasion generate a conflict between voters. The measures hurt tax evaders, typically a minority of voters, while the majority of the population is likely to benefit from additional government expenditures, lower tax rates, or even directly from the punishment of former shirkers. For instance, a recent survey administered by Bank of Italy finds that approximately 76 percent of the respondents believed that fighting tax evasion should be among the foremost priorities for Italian public policy (Cannari and D'Alessio, 2006). However, the magnitude of the individual costs tax evaders incur from enforcement is potentially higher than the individual benefit non-evaders derive from it. Anti-tax evasion policies are thus canonical examples of policies that are asymmetric in their concentration of costs and benefits (Tullock (1959); Olson (1965)). Due to this asymmetry, fighting tax evasion may either benefit or harm politicians who seek reelection. The sign of this impact is ex ante ambiguous and, therefore, an empirical question.

In 2007, the Italian government instituted a nationwide anti-tax evasion policy, the *Ghost*

[1] Slemrod (2007) states that, according to existing evidence: "the overall net noncompliance rate for all U.S. federal taxes and the individual income tax seems to stand at about 14 percent". Estimates from other developed countries deliver similar figures (for Italy, see Marino and Zizza (2008)). In developing countries, where the share of the informal economy is typically larger, the figures are much higher (Gordon and Li (2009)). La Porta and Shleifer (2008) focus on the relationship between economic development and the size of the informal economy.

Buildings program. The program identified ghost buildings – properties not included in the land registry and thus hidden from tax authorities – by overlaying aerial photographs and digital land registry maps. The intervention detected more than two million parcels (the unit of the land registry) with ghost buildings. A large registration program targeting the identified ghost buildings followed the completion of the mapping exercise. While the central government began the program and coordinated registration activities, municipal administrations circulated information about the program, collaborated with follow-up inspections, and enforced payment of overdue local taxes. Media reports highlight both the importance of local administrations in the registration process and that local governments took credit for the enforcement that followed the mapping exercise (Dell'Oste and Trovati (2011), Bernardini (2011) and Barca (2008)).

The policy induced a large shift in tax enforcement, the intensity of which varied significantly across towns. In towns with a higher prevalence of detected ghost buildings, the program was more likely to affect the amount of building registration. We use a measure of *Ghost Building Intensity*, the ratio of the number of land registry parcels with ghost buildings identified by the program to the total number of land registry parcels in the town, to proxy for the scope of the program. Ceteris paribus, there is a higher opportunity to reduce tax evasion in towns with a larger fraction of ghost buildings detected by the program.

The data show that the increase in enforcement is stronger in towns with higher ghost building intensity. Using a difference-in-differences approach, we test the impact of the anti-evasion policy on the reelection of local incumbents by exploiting the cross-municipality variation in this intensity. Our strategy controls for time-invariant town characteristics through the use of town fixed effects. It identifies the causal effect of the policy scope to increase enforcement on electoral outcomes under standard assumptions (i.e., no contemporary differential changes and no differential pre-trends in the outcome variable by treatment intensity) that we verify in the data. In particular, it isolates the effect of an exogenous change in enforcement from other mayor or voter characteristics that might have affected the actual levels of ghost building registration in the town.

The results show sizable political responses. In local elections occurring after the beginning of the program, an increase of one standard deviation in the ghost building intensity raises the likelihood of reelection of the local incumbent relative to pre-program elections

by approximately 2.2 percentage points, approximately 4.8 percent of the average reelection rate (45.3 percent). Higher town-level ghost building intensity also lowers several measures of competitiveness in local elections. In particular, it reduces the number of candidates running for election, increases the margin of victory for the winner, and reduces the likelihood of a runoff.

Guiding our empirical models is a retrospective voting framework of political agency (Barro (1973); Ferejohn (1986)), which we adapt to study tax evasion. This theoretical framework helps us to predict how a change in tax enforcement can impact voter choices and which factors affect this response. Additional analysis of the actual building registrations induced by the program complements the reduced-form analysis described above. For a given town-level program scope, a higher registration rate of ghost buildings under the incumbent local administration (i.e., the share of ghost buildings that are registered prior to the local election date) has a positive effect on the likelihood of reelection. The result is robust to the inclusion of mayors' characteristics as controls and to an instrumental variable approach, based on the time elapsed between the program start date and the town election date.

We provide evidence for two channels that could drive the observed electoral response. First, using survey data on the self-reported tolerance for tax evasion among voters, we show that the program's positive impact on incumbent reelection is significantly higher in areas with lower tolerance for tax evasion. Second, towns where the government is more efficient in delivering public goods show a larger electoral response to the program. We also verify that towns with higher ghost building intensity experienced a differential increase in local government expenditures following the program's inception. Finally, the empirical findings are inconsistent with two potential alternative interpretations of the impact of the program on voter support for incumbents. In the first, the program changes voter behavior by providing information on the existing stock of ghost buildings. In the second, it gives the incumbent an electoral rent by giving her the option to *not* register identified ghost buildings.

Our approach has the potential to be applied in different settings to study the political feasibility of upgrading tax administrations around the world using new electronic data, cross-checking technologies, and other monitoring devices.[2] Additionally, our analysis points

[2]For instance, other countries, such as Greece and Rwanda, have recently implemented policies using similar technologies.

at a complementarity between technological innovations in tax enforcement and political incentives. When exposed to a reduction in monitoring costs, politicians can exploit the new technologies and experience political gains. These findings have a direct bearing on the political feasibility of upgrading tax administrations around the world using new electronic data, cross-checking technologies, and other monitoring devices (Bird and Zolt (2008)). In addition, our study provides evidence that the underlying tax culture shapes the political incentives for tax enforcement and the political returns to these innovations (Torgler (2007); Rothstein (2000)).

Finally, access to the program's town-level nationwide administrative data allows us to provide evidence on two additional fronts. First, we study the correlates of tax evasion at the town level. We find that geographical features such as town size are important determinants of tax evasion, consistent with Saiz (2010), and that social capital is negatively correlated with tax evasion (Putnam (2001)). Second, we document that mayors' characteristics, such as education, gender, and age, do affect the extent to which the Ghost Buildings program increased tax enforcement (consistent with Alesina (1988); Besley and Coate (1997); Besley, Montalvo and Reynal-Querol (2012)).

This paper relates to several strands of literature. First, a recent set of studies uses microdata to shed light on enforcement technologies such as third-party reporting (Slemrod, Blumenthal, and Christian (2001), Saez (2010), Kleven et al. (2011), Chetty, Friedman, and Saez (2012)), paper trails (Pomeranz (2012); Kumler, Verhoogen, and Frías (2011)), cross-checking (Carrillo, Pomeranz, and Singhal (2012)), targeted auditing strategies (Almunia and Lopez-Rodriguez (2012); Aparicio (2012)) and third-party monitoring of customs duty collection (Yang (2008a, 2008b)).[3] Second, this paper is related to the political agency literature (Barro (1973); Ferejohn (1986)). Retrospective voting models have received considerable empirical support, in the context of fiscal stabilization, for example, which is the setting of this study (Brender, 2003; Brender and Drazen (2008); Alesina, Carloni, and Lecce (2011)). By studying how technology-driven enforcement policies affect policymakers, we build a bridge between the political agency and the tax evasion literature. In addition, by delving into the relationship between the incentives of political agents and tax evasion, our paper is related

[3]For a review of the literature, see Andreoni, Erard, and Feinstein (1998) and Slemrod and Yitzhaki (2002).

to the work of Artavanis, Morse, and Tsoutsoura (2012), who find that tax evasion is higher in industries supported by parliamentarians. Finally, our results provide support to existing literature that highlights the roles of culture and social norms as determinants of tax evasion, either via cross-country analysis (Torgler (2003); Slemrod (2003)) or lab experiments (Spicer and Becker (1980); Alm, Jackson, and McKee (1992)).

The remainder of the paper is organized as follows. Section 2 describes the Ghost Buildings program. Section 3 presents a simple framework that guides our empirical analysis. Section 4 describes the data and presents descriptive evidence. Section 5 lays out our empirical strategy to estimate the electoral response to the policy. Section 6 presents the results. Section 7 presents the study's conclusions.

2 The Ghost Buildings Program

The value of the buildings registered in the land registry enter the tax base for several national and local taxes, including ICI (the local property tax), IRPEF (the personal income tax, which is both national and local), and the local waste management tax. Italian legislation requires that owners register new buildings at the local office of the *Agenzia del Territorio*, the agency managing the land registry, within thirty days after their completion.[4]

In 2006, the national government approved new anti-tax evasion legislation, the Ghost Buildings program, aimed at detecting buildings not registered on the land registry maps.[5]. Italy's national politics were unstable during the years in which the program was implemented: Silvio Berlusconi (right-wing) was the Prime Minister in the first half of 2006, Romano Prodi (left-wing) was the Prime Minister from the second half of 2006 to 2008, and Silvio Berlusconi was again Prime Minister between 2008 and 2011. Therefore, the attribution of the policy to one specific national party is not obvious.

The *Agenzia del Territorio*, the national agency managing the land registry, coordinated

[4]See *Legge 9 Marzo 2006 n.80 - Art. 34-quinquies*. All buildings in Italy require a building permit before construction can begin, and obtaining a building permit makes the new building part of the City Plan. The process of obtaining building permits is administered independently from the registration in the land registry maps. Buildings not in the City Plan are required to be demolished.

[5]See *Legge 24 novembre 2006, n. 286* subsequently modified by *Legge 30 Luglio 2010, n. 122* The detection exercise did not cover one of the semi-autonomous regions, Trentino Alto-Adige because land registry maps are autonomously administered in that region. The region contains less than two percent of the total population of Italy.

the effort. The agency first juxtaposed the land and building registry maps to obtain the Official Building Map. It subsequently compiled high-resolution (50 cm) aerial photographs of the entire country to identify the ghost buildings. Appendix Figures A.1A-A.1C summarize the identification steps. First, the aerial photograph of a particular location was created. Then, the pictures were matched with the official building map for the corresponding area. Finally, the ghost buildings were identified.[6] Ghost buildings include commercial, industrial, and residential stand-alone buildings, as well as substantial extensions of previously registered buildings that should have been reported to the land registry.

Through this process, the *Agenzia del Territorio* identified approximately two million land registry parcels containing unregistered buildings. Beginning in August 2007, the *Agenzia del Territorio* started publishing parcel-level data on unregistered properties in the *Gazzetta Ufficiale*, the official bulletin promulgating Italian laws and decrees, to induce registrations of the ghost buildings. Within three years, it coded detailed information on the number of ghost buildings in the universe of Italian municipalities (with the exception of Trentino Alto-Adige). The order of publication relied on the availability of digitized land registry maps at the time when the program started. The *Agenzia del Territorio* had 60 percent of the land registry maps of the Italian territory in digitized form before the Ghost Buildings program was approved. After 2006, the *Agenzia del Territorio* began digitizing the remaining land registry maps, proceeding by province (i.e., they simultaneously coded different municipalities in the same province). It completed the identification exercise by the end of 2010.[7]

According to the initial legislation, owners could register the detected ghost building with the land registry by April 30, 2011.[8] Widespread media campaigns and local administrations' efforts contributed to achieve high registration rates. In particular, local administrators a) disseminated information about targeted parcels; b) collaborated on follow-up building inspections; c) proceeded with the collection of overdue local taxes up to five years before the program began; and d) verified the conformity of ghost buildings to the city plan and

[6] According to the Law *Decreto Ministero delle Finanze 2 gennaio 1998, n.28.Art. 3*, the following buildings do not increase the tax base of their owners and thus are not subject to registration requirements: (i) buildings that are incomplete; (ii) buildings that are particularly degraded; (iii) solar collectors; (iv) greenhouses; and (v) henhouses or other buildings reserved for animals.

[7] Publication in the *Gazzetta Ufficiale* occurred in the following waves: August 2007, October 2007, December 2007, December 2008, December 2009, December 2010.

[8] This was the result of two previous deadlines of ninety days and seven months since the publication in the *Gazzetta Ufficiale*.

local zoning restrictions. In addition, there is extensive evidence from the media that local governments often attempted to take full credit for the registration process. In Section 3, we highlight this is an important element for understanding the voter responses we document in this paper.

Owners of ghost buildings who registered prior to the April 2011 deadline were required to pay overdue taxes dating back to 2007, or to the construction date for post-2007 cases, and to pay penalties for delayed payments. After April 2011, the *Agenzia del Territorio*, with the support of local administrations and contractors, proceeded with follow-up inspections to impute the tax base for the remaining unregistered buildings.[9] Additional penalties and a fee for the extra inspection were assessed to the owners of buildings for which the *Agenzia del Territorio* imputed the tax base after April 2011. The *Agenzia del Territorio* published a report on the detailed fiscal impact of the program for the year 2011. The program led to a substantial wave of registrations. According to the administrative data, approximately 40 percent of the ghost buildings were registered as of December 30th, 2011. According to the figures provided, the program increased total tax revenues by 472 million euros in that year.[10] Local administrations received a large share of the additional tax revenues generated by the program, and we estimate that approximately 65 percent of those revenues come from local taxes. We run a back-of-the-envelope calculation using figures on the number of land parcels with ghost buildings, the registration rates, and the total additional tax revenues from the program. A one standard deviation increase in ghost buildings targeted by the program increased the tax revenues by approximately 3 percent of the median value. Using the same information, we find that, on average, the owner of a registered ghost building now faces an additional yearly tax burden of approximately 528 Euros.

3 Tax Enforcement and Retrospective Voting: A Conceptual Framework

This section outlines the impact of a change in tax enforcement on voters' electoral choices. We provide a simple framework based on the modeling of retrospective voting (Barro (1973);

[9]To further increase incentives for the local administrations, an additional bonus was introduced in 2011 for each registered ghost building. See also *Decreto Legge 79/2010, art. 10, 11.*

[10]This figure does not include payment for overdue taxes from previous years.

Ferejohn (1986)) and tax evasion (Allingham and Sandmo (1972)). The main intuition of retrospective voting models is that citizens decide whether to reelect the incumbent based on their welfare in the most recent political term.[11] While the discussion in this section relies heavily on intuition from existing models of political agency, their specific application to tax evasion is novel and provides new comparative statics.

The economy is populated by a unit mass of voters and by politicians. Voters are heterogeneous in their ability to evade. This ability could be a function of occupation type (employed vs. self-dependent) or evasion costs (economic and psychological). We consider a simple case with two fixed types of voters: evaders and non-evaders. Evaders pay taxes only if enforcement occurs, while non-evaders always pay taxes.[12] The population share of evaders is λ. The enforcement of tax collection for each evader occurs with probability p. Enforcement draws are independent across evaders, and thus p is the share of evaders for which enforcement occurs. This is assumed to be a function of the politician type (a) and of an idiosyncratic component (υ), whose distributions are $G(a)$ and $G(\upsilon)$, respectively. Voters do not observe the two components and are uncertain over the politician type as in Banks and Sundaram (1998). They use previous realizations to form expectations $\hat{a}$ and $\hat{p}$, in the spirit of Holmstrom (1982).

We assume an exogenous income level, normalized to 1, and tax rate, τ, constant across the population. Voters derive utility from disposable income and from the overall level of enforcement, for instance, through the increased provision of public goods and deficit reduction. This implies that enforcement has two effects on evaders' utility, which go in opposite directions. First, enforcement decreases the disposable income for evaders; however, cracking down on tax evasion increases the size of the government, which benefits all citizens, including evaders.[13] The expected utility for evaders, V_E is defined as:

$$V_E(\hat{p}) = \hat{p}(U(1-\tau)) + (1-\hat{p})U(1) + \hat{p}W_E(\lambda, e), \tag{1}$$

[11] For empirical applications based on the retrospective voting model framework see: Brender (2003); Besley and Pratt (2006); Brender and Drazen (2008); Ferraz and Finan (2008); Alesina, Carloni, and Lecce (2011); Nannicini, Stella, Tabellini and Troiano (2012).

[12] For simplicity, we ignore the extra fines evaders pay when audited and, thus, the optimal individual evasion level they choose.

[13] While we do not model this explicitly, it is possible that non-evaders can incur costs from higher tax enforcement. For instance, previously evading firms may now charge higher prices to non-evading consumers because of their increased costs.

where we highlight that V_E depends on the expected level of enforcement, $\hat{p}$. In Equation 1, $U(\cdot)$ is the monetary utility from disposable income and $W_E(\cdot)$ is the utility from tax collection enforcement.[14] W_E is increasing in λ, the share of evaders in the population, and e, government efficiency in using tax revenues to produce public goods.

We allow non-evaders to obtain an additional non-monetary benefit from enforcement. One example is the case where, because of fairness concerns, non-evaders derive direct utility from the enforcement of evaders' tax payments, independently from their monetary returns.[15] Thus, the expected utility function for the non-evaders is:

$$V_N(\hat{p}) = \hat{p}W_N(\lambda, e, b) + U(1-\tau) \tag{2}$$

We notice that, in addition to λ and e, V_N also depends on b, a shifter that affects the non-monetary benefits from increases in enforcement. For instance, b captures the extent to which voters are averse to tax evasion (tax culture). In the model, we abstract from the utility arising from government services financed by the tax payments of the non-evaders because that does not depend on $\hat{p}$, the core variable of interest for our argument.

We now consider the voters' choice between an incumbent and a contender. We adopt a standard probabilistic voting approach (Lindbeck and Weibull, 1987). In the text below, $\hat{a}$ and $\hat{p}$ denote the voters' beliefs about incumbent type and enforcement, respectively. On the other hand, $\overline{a}$ and $\overline{p}$ capture the expectations about the contender. In deciding whether to reelect the incumbent, the two groups of voters compare the utility under the expected incumbent's type with an average opponent. Voter i in group $j = \{E, N\}$ will reelect the incumbent if $V_j(\hat{p}) > V_j(\overline{p}) + \epsilon_{ij} + \delta$. The parameter ϵ_{ij} is an individual ideological bias with respect to the contender, distributed uniformly over $[-\frac{1}{2\phi^j}, \frac{1}{2\phi^j}]$.[16] The parameter δ measures the average popularity of the contender in the population and is distributed uniformly over $U[-\frac{1}{2}, \frac{1}{2}]$. Under the above assumptions, the ex-ante incumbent reelection probability (i.e.,

[14] To simplify the presentation, we assume that the utility from enforcement is proportional to the expected level of enforcement.

[15] For experimental evidence on this channel, see Carpenter et al. (2009); Casari and Luini (2009); Ouss and Peysakhovich (2012).

[16] The parameters ϕ^E and ϕ^N should be interpreted as proxies for the responsiveness of voters in each group to tax evasion enforcement. For example, they might reflect the fact that a group's political power can change depending on its size or ability to self-organize (Olson(1965)).

before the realization of δ) is:

$$\pi = (\hat{p} - \overline{p}) \left[\lambda \phi_E(-U(1) + U(1-\tau) + W_E) + (1-\lambda)\phi_N W_N\right] \tag{3}$$

The following equation presents the electoral impact of an increase in the expected enforcement level under the incumbent, $\hat{p}$:

$$\frac{\partial \pi}{\partial \hat{p}} = \lambda \phi_E\big(- U(1) + U(1-\tau) + W_E\big) + (1-\lambda)\phi_N W_N \tag{4}$$

The first term represents the net electoral gains coming from evaders voting. These will be negative whenever the utility cost of the expected loss in disposable income, $U(1) - U(1-\tau)$, more than offsets the benefits from enforcement, W_E. The second term is the electoral gain from non-evaders (always positive). This duality is consistent with the discussion in Section 1: an increase in the perception of the enforcement type of the incumbent has ambiguous effects. The change generates a conflict across voters and the model parameters determine which channel prevails. In addition, the model delivers intuitive comparative statics on the heterogeneity of the electoral impact arising from an increase in expected enforcement under the incumbent. Both government efficiency in public good provision and the intensity of non-monetary benefits from the additional enforcement matter play a role. Specifically:

$$\frac{\partial^2 \pi}{\partial \hat{p} \partial e} = \lambda \phi_E \frac{\partial W_E}{\partial e} + (1-\lambda)\phi_N \frac{\partial W_N}{\partial e} \tag{5}$$

and

$$\frac{\partial^2 \pi}{\partial \hat{p} \partial b} = (1-\lambda)\phi_N \frac{\partial W_N}{\partial b}, \tag{6}$$

which are both positive. To summarize, the simple model predicts that an exogenous increase in the perceived level of enforcement under the incumbent:

i) has an ambiguous impact on the likelihood that the incumbent is reelected.

This impact:

ii) is larger when government is more efficient in the provision of public goods;

iii) is larger when there are greater non-monetary returns from enforcement.

The Ghost Buildings program allows us to shed light on these predictions. The program, initiated by the central government, can be considered as a positive shock to enforcement. We argue that voters observe the increase in building registrations but have limited information about the specific production function of enforcement (i.e., information collected by the central government, the efforts of local administrations, and complementarity between the two sources). This in turn increases the belief voters hold about the local incumbent type, $\hat{a}$ (and thus on $\hat{p}$), and, according to the model, generates an ambiguous effect on the incumbent reelection probability.[17]

Crucially, this result relies on the assumption that voters have limited information about the details of the Ghost Buildings program. They observe the change in enforcement and still attribute a part of it to the incumbent, thus extracting a signal about her type. Models with *rational but poorly informed voters* have received growing attention in the literature. They can provide theoretical support for the empirical findings that voters' electoral choices respond to economic conditions (Wolfers (2009)), natural disasters (Cole, Healy, and Werker (2012)), and quasi-random targeted transfers (Manacorda, Miguel, and Vigorito (2011)). In addition, a recent wave of randomized experiments shows that information provision can significantly affect voter choices and political outcomes (for a review, see Pande (2011)). Poor voter information is particularly relevant for the Ghost Buildings program. It was likely difficult for voters to make inferences about who exactly was ramping up enforcement. Local administrations' efforts complemented the initial identification process. In addition, evidence from media reports and town bulletins suggest that mayors often took credit for the enforcement following the program, and in some cases, even for the initial stages of building identification through aerial pictures (Cavallaro (2011), Corriere della Città (2012), Gazzetta del Mezzogiorno (2012)).

Finally, we notice that an alternative model where voters perfectly observe the nature of the Ghost Buildings program (while they are still uncertain about the type of mayor) can also predict an impact of the program on the voters' support for the incumbent. In this alternative setting, the program provides an opportunity for voters to extract a more precise signal about the incumbent type, as in Bubb (2008). This can either benefit or hurt the incumbent in

[17]We thus assume that the voter belief about the mayor enforcement-type is increasing in the observed level of enforcement. We do not, however, delve into the specifics of the process of changing voter beliefs.

turn, depending, for instance, on voter risk preferences (Quattrone and Tversky (1988)) or on the skewness of the distribution of incumbent types (Caselli et al. (2013)). In the empirical analysis, we do not aim to differentiate the two classes of models. Rather, the insight that the net voter response to an enforcement policy is theoretically ambiguous, which is common to both models, motivates our empirical investigation.

4 Data and Descriptive Evidence

4.1 Data

The main database for the analysis includes information on the number of parcels containing ghost buildings in each town. The aerial photographs detected more than two million such parcels. We target the population of 7,720 of the 8,092 Italian towns for which we can define the measure of ghost building intensity. Additionally, we obtain data on registered ghost buildings up to the deadline of April 30, 2011. To analyze the electoral response to ghost building registration, we construct a measure of registration imputable to the incumbent administration. Specifically, we multiply the registration rate by the ratio between a) the time elapsed between program start date and election date and b) the time elapsed between program start date and April 2011.[18]

We complement this information with data from the Italian Department of the Interior (*Ministero degli Interni*) on the universe of municipal elections from 1993 to 2011.[19] In Figure 1, we plot the number of elections per year. Towns vote in different years according to predetermined waves. We distinguish between elections before and after the beginning of the Ghost Buildings program. There are almost 5,200 municipalities for which we have data on an election that occurred after program inception (approximately 67 percent of the total number of towns targeted by the program). Two institutional reforms that occurred in the time span of our sample were relevant. First, in 1993, the starting year for our election

[18]In one of our robustness checks, we also compute a second measure of registration imputable to the incumbent under the assumption of a constant growth rate of 50 percent in the registration levels over years.

[19]The Italian municipal government (*Comune*) is composed of a mayor (*Sindaco*), an executive committee (*Giunta*) appointed by the mayor, and an elected city council (*Consiglio Comunale*) responsible for authorizing the annual budget proposed by the mayor. The mayor and the executive committee, whose members' appointments can be terminated by the mayor at will, propose changes in policies, such as reductions in the tax rates or expenditures. Subsequently, the city council votes on the proposed modifications.

sample, Italian municipal politics were overhauled: a new electoral law changed the mayoral electoral system from party to individual ballot. It also introduced a two-term limit. Second, in 2000, the length of the mayoral term was extended from four to five years.[20]

In addition to the core data, we collect geographic and socio-economic data at the municipality level from the Italian National Statistical Office. Finally, we use two additional data sources to test the channels driving the electoral response: town-level government expenditures (from the *Ministero degli Interni*) and a region-level standardized score to the question Do you justify tax cheating? from the *European Values Study* for each of the 20 Italian regions.

Table 1 presents summary statistics for the variables used in the paper. Panel A presents the main variables related to the Ghost Buildings program. Panels B and C include town-level geographical and socio-economic covariates, respectively. These are measured prior to the inception of the Ghost Buildings program, mostly in the 2001 Population Census. Panel D (Mayor variables) summarizes the characteristics of the mayor in office at the time of the program's inception in a particular town.[21] In Panel E, we summarize the local election panel variables. Tables A.1 and A.2 provide a detailed description of data sources and variable definitions.

4.2 The Correlates of Tax Evasion

We use data from the Ghost Buildings program to study the correlates of tax evasion at the town level. Figure 2 presents our measure of ghost building intensity across Italian towns. Notably, ghost buildings are more prevalent in Southern Italy, and less widespread

[20] In limited cases, the term can last less than is provided by the law and elections can take place earlier. Those limited cases include the resignation of the mayor, the resignation of the majority of the council or a no-confidence vote in the council. Early termination is relatively infrequent in Italian local politics. In the post-program period, only approximately 7 percent of the towns have an election in a year different than the fifth one after the previous and for only 2.5 percent of the towns is the difference between the two larger than one year. Consistent with the previous points, we verify that our results are not affected when we drop the sample of towns with an election year in the post-program period that is different from the one scheduled by the law.

[21] Only about half of mayors are matched to national parties, and the other parties are difficult to categorize within a left-right spectrum. We therefore choose not to focus on this variable in our analysis. We note that political party dummies among the controls in the regressions (for those mayors for which we have this information) do not affect the results we present later in the paper.

in the North.[22] Table 2 presents the correlates of ghost building intensity (per 1,000 land registry parcels). In Column (1), we first study whether geographical factors (altitude, area of the town, number of land registry parcels) are correlated with tax evasion. In Column (2), we add socio-economic controls (population, income per capita, social capital, number of firms, urbanization rate). Finally, in Column (3), we show that our results are unaffected by the inclusion of regional fixed effects.[23]

We find that several geographic characteristics are strongly associated with tax evasion. In particular, controlling for other variables, tax evasion is higher in geographically larger municipalities. Plausibly, in cities with wide geographical extension, there are greater opportunities to hide unregistered buildings as the enforcement of building registration is more difficult and resource-intensive. However, we cannot decisively interpret this evidence as causal. Previous literature has shown, for example, that borders are endogenously determined (see, among others, Alesina and Spolaore (1997), Alesina, Baqir and Hoxby (2004), Alesina, Easterly, and Matuszeski (2011)).

Finally, as expected, tax evasion is negatively associated with both social capital and income. In particular, the finding on social capital is consistent with Putnam (2001), who finds that the percentage of tax evasion, as measured by the Internal Revenue Service, is strongly related to differences in social capital at the state level.

4.3 The Political Determinants of Ghost Building Registration

We now provide more details on the wave of registration of ghost buildings induced by the program. First, we show that the number of ghost buildings detected by the program is a good predictor of the number of ghost buildings that were registered in response to the policy. Figure 3 displays the relationship between the number of land parcels with ghost buildings eventually registered by the April 2011 deadline (registered ghost building intensity) and the

[22]The *Agenzia del Territorio* conducted its detection activities homogeneously throughout the country. Thus, heterogeneity in the number of detected unregistered buildings captures differences in actual levels of non-registration at the time of the aerial photographing, as opposed to differential intensity in detection activity.

[23]For 3.5 percent of the towns in our sample we are missing at least one town-level control. In our regressions throughout the paper, for each control we include a binary indicator which is equal to one if the control is missing. In addition, we replace missing values with an arbitrary unique value. The results are unchanged when we undo this and simply drop observations with missing values for the control variables.

number of parcels that were identified as containing ghost buildings (ghost building intensity), both as a share of the total number of land registry parcels. In the graph, the x-axis variable is partitioned into percentiles. The scatter plot shows a clear increasing relationship. In a linear regression analysis, an increase of one standard deviation in the detected intensity of ghost buildings raises the *registered* ghost building intensity at April 2011 by approximately 0.75 standard deviations ($p < 0.01$). To summarize, the program scope at the town-level strongly predicts the program's actual impact on tax enforcement. This premise motivates the strategy that we introduce in Section 5 to estimate the impact of the Ghost Buildings program on electoral outcomes.

Second, we analyze the ghost building registration rate, defined as the percentage of ghost building parcels that were registered by the April 2011 deadline. Figure 4 summarizes the ghost building registration rate and documents a substantial dispersion across towns. Table 3 documents the impact of the characteristics of the mayor at the time of the program inception on this outcome. For a given level of the other covariates, the registration rate is higher when mayors are male, younger, more educated, or were born in the same city in which they serve as mayor. The correlation between gender and policies in Italian municipality is potentially consistent with the results of Gagliarducci and Paserman (2012), who find that female policymakers usually face greater difficulty in implementing policies while in office. To the extent that education can be considered a proxy for politicians' quality (see, for example, Besley, Montalvo and Reynal-Querol (2011)), this set of results also supports the view that better policymakers fight tax evasion more. We highlight the correlation between the mayor's birthplace and tax evasion enforcement. One possible explanation could be that mayors who are born in the same city have access to additional information that can facilitate tax evasion enforcement. We acknowledge that this evidence relies on cross-sectional correlation analysis and thus should be interpreted with caution. However, we also notice that the results are robust to the inclusion of geographical and socio-economic controls, in Columns (2) and (3), respectively. With these caveats in mind, the findings of this section suggest that the mayors' characteristics did have a role in shaping registration activities across towns.

5 Empirical Strategy

5.1 The Electoral Response to the Ghost Buildings Program

In this section, we outline our approach to estimate the voter response to the Ghost Buildings program. We also aim to isolate the channels that drive this response. Our empirical strategy exploits variation across towns in the program scope to increase tax enforcement. We implement a difference-in-differences approach based on town-level ghost building intensity, which we defined above as the ratio of the number of land registry parcels with ghost buildings to the total number of land registry parcels in each town, measured at program inception.

In Section 4.3, we documented that mayors' characteristics, such as age, education, and gender, predict the registration rate of the detected ghost buildings. However, the actual levels of registration could depend on voter preferences and responsiveness to the program. Thus, a naive analysis examining the relationship between actual ghost building registrations and reelection outcomes will suffer from standard omitted variable bias. This motivates our focus on *ex ante* program scope to measure the impact of enforcement.

The rationale for our identification approach is that program scope at the town level predicts the exogenous increase in enforcement induced by the Ghost Buildings program, as shown in Figure 3. Towns with a higher share of parcels containing *detected* ghost buildings also have, on average, a higher share of parcels with *registered* ghost buildings, as measured in April 2011. Importantly the intensity of ghost buildings is not a valid instrument for actual registration intensity. The program can affect incumbent reelection probability through other channels besides registration, for instance, by giving the mayors an opportunity to extract rents from low program enforcement or by providing voters with information regarding past enforcement. In section 6.3, we discuss these alternative interpretations and show that they cannot drive our results; however, it could still be the case that these alternative mechanisms partially affected the voter response, which would violate the standard exclusion restriction

required for an instrumental variable approach.[24] Our baseline specification is therefore:

$$R_{imet} = \beta_0 + \beta_1 Post_{ie} \cdot Ghost\ Building\ Intensity_i + \eta_m \cdot Post_{ie} + \phi_i + \phi_t + \epsilon_{imet} \qquad (7)$$

The dependent variable R_{imet} is a dummy that indicates whether the incumbent of town i in macro-area m is re-elected in election e in year t.[25] Observations where the incumbent cannot be reelected because of a binding term limit are excluded from the regression sample. The dummy *Post* is equal to one when election e occurs after the beginning of the Ghost Buildings program in the town. The coefficients η_m capture post-program period fixed effects that are specific to the four Italian macro-areas m where town i is located.[26] We also include town fixed effects, ϕ_i, and election year fixed effects, ϕ_t. Town fixed effects would capture any time-invariant difference across cities that may be correlated with ghost building intensity. Finally, $Ghost\ Building\ Intensity_i$ is the intensity of ghost buildings in town i. The coefficient of interest, β_1, thus captures the differential impact of the Ghost Buildings program on incumbent reelection by ghost building intensity. Throughout the paper, we cluster standard errors at the provincial level to allow for spatial correlation in the error term. It should also be noted that because of the existence of a two-term limit, our identification relies on mayors in their first term. In a recent contribution, Mian and Sufi (2012) adopted a similar empirical approach to study the effects of the fiscal stimulus in the US.

We adopt a similar regression model to study the impact of the program on other electoral competitiveness outcomes. We focus on four variables: i) the number of candidates running for mayor; ii) a binary indicator to capture re-candidacy (i.e., whether the incumbent runs for election a second time); iii) the difference in the percentage of votes between the first and the second candidate; and iv) a binary indicator equal to one if a runoff takes place, which occurs in towns with more than 15,000 inhabitants when none of the candidates obtain an absolute majority in the first-round.[27]

One potential challenge to our identification strategy may arise from the town-specific

[24]The coefficient of registration rate when instrumented by ghost building intensity is 6.21, significant at the 1% level.

[25]The variable equals zero both if a mayor eligible for reelection does not run again or if she runs again and is not elected.

[26]Macro-areas are North, Center, South, Islands.

[27]For the analysis of the difference in percentage votes between the first and second candidate, we always use first-round results, even for elections when a runoff occurs.

timing of publications of the unauthorized buildings lists. On the one hand, if local administrators had influence over publication dates, unpopular mayors in cities with high evasion rates might lobby to delay publication. On the other hand, the central government might push to start the program earlier in those towns where mayors set a lower level of tax enforcement. In both these cases, our estimates of the impact of the Ghost Building program on reelection likelihood may capture a selection effect. We address this concern in several ways. First, as discussed in Section 2, we note that the timing of the publication was primarily determined at the provincial level by the availability of digital land registry maps and was highly clustered by province.[28] Only approximately 7 percent of the post-program elections have values for the post-program indicator different from the one they would have had based on the modal date of publication in the province.

To address these discrepancies, we implement an instrumental variable approach. We code elections based on whether they occur before or after the modal date of publication of the unauthorized building lists in the province. We then instrument the actual *Post* dummy with this binary indicator at the provincial level. The rationale for this choice is that the provincial level modal inception year may be correlated with historic characteristics of towns in the province (captured by our town fixed effects), but is unlikely to be driven by specific mayor characteristics. On the other hand, these may be driving the discrepancies between the town and the provincial program inception year.

We adopt this strategy for our main specifications.[29] In addition, in Appendix Table A.3, we present robustness checks using an alternative instrument for the post-program indicator using the *national* modal program inception year. [30] As is standard in difference-in-differences estimation, the identification of the coefficient of interest relies on two assumptions. The first is the absence of contemporary events that differentially affected towns with a higher ghost building intensity. We are not aware of other policies targeting this form of tax evasion that occurred concurrently with the Ghost Buildings program; however, it is

[28] Appendix Figure A.2 emphasizes the high level of provincial clustering in the publication years.

[29] The towns targeted by the program belonged to 101 provinces.

[30] We also note that our identification does not rely on comparison across towns with different publication years. In addition, we perform two additional checks related to the timing of program inception. First, we control for the interaction between the program inception year and the post program variable. Second, we check that there is no evidence of differential pre-trends in prior reelection rates across towns with different program inception years. Results are available on request.

still possible that other events, which differ in intensity by other variables correlated with ghost building intensity, occurred at the same time. We address this concern by presenting alternative specifications where we include interactions between a comprehensive set of geographical, socio-economic, and political controls, all measured before the beginning of the program, and the post-program binary indicator. The second assumption is the presence of parallel trends in the outcome variable. We assess this assumption using several tests and placebo exercises.

5.2 Tax Enforcement and Heterogeneity Analysis

The reduced form approach presented thus far tests whether a higher program scope to increase tax enforcement at the town level affects incumbent reelection likelihood in the post-program period. We complement this baseline regression with further analysis. First, we show that it is the tax enforcement induced by the program that drives the electoral response, as opposed to other potential interpretations. For this purpose, we use actual ghost building registration data. In Section 4, we emphasized several important measurement limitations of these data that warrant caution. With this caveat in mind, we test whether, for a given intensity of ghost buildings, a higher ghost building registration rate (*Registration Rate*) induced by the program has a positive effect on incumbent reelection likelihood:

$$\begin{aligned} R_{imet} = \gamma_0 + \gamma_1 Post_{ie} \cdot Ghost\ Building\ Intensity_i \\ + \gamma_2 Post_{ie} \cdot Registration\ Rate_i + \zeta_m \cdot Post_{ie} + \mu_i + \mu_t + \upsilon_{imet} \end{aligned} \tag{8}$$

As discussed above, an obvious threat to the identification of γ_2 in Equation 8 arises from the fact that the registration effort is potentially correlated with many town-level confounders. We first check the robustness of the results to the inclusion of mayoral controls. In addition, the timing of the program provides a strategy that can alleviate this concern. Even if the program began in the same year in most of the towns, we can exploit the variation generated by the fact that Italian municipalities hold elections in different years. A longer time period between the beginning of the program and the election date naturally leads to more registration activities. This generates variation across towns in the registration rate achieved prior to the local election date that is plausibly uncorrelated with mayor quality. We use this instrumental variable strategy to examine the impact of a change in the registration rate on

incumbent reelection likelihood.

Second, we shed light on the channels through which the program could affect voters' political preferences. Consistent with the theoretical framework, we investigate the interaction among the political returns to an increase in tax enforcement, tax culture – the stigma associated with evading taxes – and local government efficiency in delivering public goods. We use data from the *European Value Study*, the European component of the *World Values Survey*, to study the role of tax culture. Specifically, we use the answers to the question: Do you justify cheating on tax? Slemrod (2003) uses a similar variable to study the relationship between tax culture and social capital. We are not aware of other variables that can plausibly capture tax culture available at the sub-national level in Italy. In this dataset, geographical identification of respondents is available only at the regional level (20 regions). We thus compute and standardize region-level means. The following regression model tests whether the electoral response to the Ghost Building program varies by tax evasion tolerance:

$$\begin{aligned} R_{imet} = \delta_0 + \delta_1 Post_{ie} \cdot Ghost\ Building\ Intensity_i + \delta_2 Post_{ie} \cdot Tax\ Evasion\ Tolerance_i \\ + \delta_3 \cdot Post_{ie} * GB_i \cdot Tax\ Evasion\ Tolerance_i + \xi_r \cdot Post_{ie} + \lambda_i + \lambda_t + \nu_{imet}, \end{aligned} \tag{9}$$

where δ_3 is the coefficient of interest.

We use the speed of public good provision as a proxy for the quality of the delivery at the *municipal* level. This indicator is measured as the ratio of paid outlays in the municipal financial report over the total outlays committed in the budget. The intuition is that the provision of public goods is more effective in places where the actual allocation delivered to citizens is closer to the amount allocated in the budget. This proxy has already been used to measure the quality of public goods delivery (Gagliarducci and Nannicini (2013) and Grembi, Nannicini and Troiano (2013)). We are not aware of other proxies that can plausibly capture the efficiency of the universe of municipal governments in Italy. We compute the speed of public good provision as the average across two pre-treatment years.[31] The regression model to capture heterogeneity by this variable is similar to the one presented in Equation 9. Finally, we also assess the impact of the program on town-level public expenditures. To test whether the program scope to increase tax enforcement affected these expenditures, we

[31]The results, available on request, are similar with alternative definitions.

adopt a specification similar to that which we presented in Equation 7, using the natural logarithm of the local government expenditures as the dependent variable.

6 Results

6.1 Baseline Results

In this section, we investigate the electoral consequences of the Ghost Buildings program. Figure 5 provides a visual analysis of the relationship between ghost building intensity and changes in the incumbent reelection likelihood – our main outcome variable – after the beginning of the program. On the x-axis, the ghost building intensity is partitioned into percentiles. The scatterplot displays a clear increasing relationship.[32]

Table 4 formalizes the analysis above and presents the results of the difference-in-differences estimation discussed in Section 5. Column (1) reports the basic OLS specification ("Reduced Form") using the provincial post-program indicator. The coefficient remains stable with the addition of town fixed effects (Column (2)) and election year fixed effects (Column (3)). Including a rich set of town-level covariates interacted with the post-program dummy does not change the results (Column (4)).[33] Starting in Column (5), we instrument the post-program indicator with the provincial post-program indicator. The coefficient is stable across the different specifications. Again, the results are robust to the inclusion of year fixed effects, town fixed effects, and interaction among town controls and the post-program indicator, in Columns (6)-(8), respectively.[34] In Column (7), the baseline specification for the rest of the analysis, the reported coefficient on the interaction between ghost building intensity and the post-program indicator is 1.042, significant at 1 percent. This magnitude implies that a one standard deviation increase in the town-level program scope to increase enforcement, as measured by the ghost building intensity, raises the likelihood of the incumbent's reelection by approximately 2.2 percentage points in post-program elections, relative to pre-program ones (from a sample mean of 45.3 percent). A back-of-the-envelope calculation suggests that the

[32] Appendix Figure A.3 presents a placebo version of Figure 5.

[33] Our results are similar when using natural logarithms instead of levels for some of the controls.

[34] Appendix Table A.3 presents several additional robustness checks. We show that the results are robust to the inclusion of additional town controls (interacted with the post-program indicator), trimming procedures and to alternative sample definitions, instrumentation strategies, and normalization measures.

effect of a one standard deviation increase in Ghost Buildings program scope on incumbent reelection probability is on the order of magnitude of 6 percent of the incumbency effect in U.S. House elections (Lee (2008)).

In Figure 6, we check whether towns with different levels of evasion were on different trends in the probability of incumbent reelection before the treatment. We report point estimates and confidence intervals on ghost building intensity for each of the elections pre- and post-program. The figures show that, before the Ghost Buildings program started, the probability of reelection of the incumbent was independent of tax evasion. However, after the beginning of the program there is a statistically and economically significant impact. Thus, the coefficient pattern in Figure 6 suggests that the common trend assumption holds in our setting. One potential concern is that because of the term limit rule, we only include towns with first-term mayors; however, if the results were purely explained by composition, one would expect large jumps in reelection rates even in pre-program elections. The lack of such cyclical changes in our pre-trends graph attenuates the concerns arising from the fact that the composition of towns change. Additionally, it is encouraging that our effect is robust to the inclusion of a rich set of controls interacted with the post-program dummy: if the sample composition was driving the observed effects, we would expect the inclusion of the town covariates to substantially reduce the estimates.

We adopt an analogous regression strategy to study the impact of the program on other measures of election competitiveness as described in Section 5. For each of these variables, we report the specification used in Column (7) of Table 4. Table 5 presents a clear picture. An increase in ghost building intensity raises the likelihood that the incumbent runs again, and decreases the competitiveness of local elections. Specifically, a one standard deviation increase in ghost building intensity reduces the number of candidates by 1.8 percent of the sample mean, increases the likelihood that the incumbent runs again by 4.1 percent, increases the margin of victory by 3.4 percent (although this last result is not statistically significant at conventional levels), and reduces the likelihood of a runoff by 18 percent. This is consistent with the idea that potential mayoral candidates correctly anticipate a stronger incumbent advantage as a result of the program.[35]

[35]Appendix Figure A.4 shows that the parallel trend assumptions also hold for the other political outcome variables described above.

6.2 Tax Enforcement, Tax Culture and Efficiency of Local Governments

This section elaborates on some of the potential channels through which the anti-tax evasion program could increase voter support for the incumbent. Table 6 presents the results from the estimation of Equation 8. This step aims to show that the increase in tax enforcement induced by the program – the ghost buildings registration – drove the electoral response. In Column (1), we present the correlation between ghost building registration rates and the likelihood of incumbent reelection. We find that, controlling for ghost building intensity, a one standard deviation increase in ghost building registration rate raises reelection likelihood by 1.3 percentage points. We use the registration rate reached by the election year computed as described in Section 4. In Column (2), we show that adding the interaction between town- and mayor-level controls and the post-program indicator does not change the results. In Column (3), we show that in a cross-city regression, the number of years elapsed from the program start date are a strong predictor of the city-level registration rate. This can be interpreted as a first stage for our instrumental variable approach. In Column (4) we use the years elapsed since the program start date as an instrument for registration rate.[36] In the IV specification, a one standard deviation increase in the registration rate (.079) raises the reelection likelihood by 4 percentage points in post-program elections. Finally, in Column (5) we show that the IV estimate is unchanged when adding the interaction between town level controls and the post-program indicator.

In Columns (4) and (5), we notice that the IV estimates are larger than the respective OLS estimates. This can be explained either by OLS attenuation bias due to measurement error, or by the fact that in the set of cities affected by the IV – that is, cities where the registration activity depends on program duration – the political returns to registration may be larger than in the rest of the cities (i.e., we are estimating a LATE). Even if our instrument is uncorrelated with any idiosyncratic city-specific characteristics, we are unable to rule out the possibility that having the program for longer time has an independent effect on its impact on the probability of reelection, which would invalidate the IV strategy. While we acknowledge

[36]In our IV specification we do not control for year fixed effects. Three quarters of the post-program elections come from cities that started the program in 2007. Thus, we lose statistical significance when running this specification, although it is reassuring that the coefficient of interest remains of a similar size. Results are available upon request.

this possibility, we still believe that our instrument performs well in addressing the main endogeneity concern for the registration efforts of the mayors (town-specific characteristics, such as the mayor's ability or effort). We then provide empirical support for two channels affecting the electoral response to the program. Our simple theoretical framework predicted that this should be higher in towns where the non-monetary returns to tax enforcement are higher and where the local government delivers public goods more effectively. We provide evidence about these hypotheses by estimating Equation 9. The coefficient of interest δ_3 captures the impact of a standard-deviation increase in the variables measuring either the tolerance for tax evasion or the municipal speed of public goods provision on the electoral response to the program. Table 7 presents the results. We then examine the role of tax culture. We exploit variations across regions to the extent to which respondents justify tax cheating in the *European Values Study*. These results provide clear evidence the tax culture matters. In Column (1), we show that a one standard deviation increase in the tolerance score reduces the point estimate of the impact of ghost buildings on reelection by .63, (significant at the ten percent level). Column (2) shows that the magnitude of the coefficient is stable, or if anything, increases (in absolute value) when adding the triple interactions with macro-area dummies.[37] These results provide suggestive evidence that the positive effect of the Ghost Buildings program on incumbent reelection likelihood is larger in localities where voters have, on average, stronger preferences for tax enforcement and where the delivery of public goods is more effective.

We then focus on the role of local government efficiency. In Column (3) we find that a one standard deviation increase in the speed of public good provision increases the point estimate of the impact of ghost buildings on reelection by 0.63 and that this coefficient is statistically significant at the ten percent level. We then confirm that this interaction effect does not simply capture geographical variation in the responsiveness across different parts of Italy by adding triple interactions across the post-program indicator, the ghost building intensity, and the macro area dummies. The sign and economic significance of the coefficient is robust (Column (4)), although estimated less precisely (p=.137). While the limited power in these estimates should evoke some caution, the analysis presents evidence that voters'

[37]Standard errors are similar when performing region-level cluster bootstrapping following Cameron, Gelbach and Miller (2008).

responses depend on underlying tax morale and government spending efficiency.

Finally, Table 8 presents the results of the estimation of the baseline regression model in Equation 7, using the log of town-level government expenditures.[38] Column (1) presents the reduced-form results, using the post-program indicator based on the provincial mode. The point estimate is .436 (significant at 10 percent). The coefficient is stable when instrumenting the post-program indicator with the provincial one and is slightly larger when including interactions among controls and the post-program indicator (Columns (2) and (3)).

While the effect of the program is statistically significant, we also note that it is fairly small. A one standard-deviation increase in ghost-building intensity increases expenditures by approximately 1 percent. We believe that it is unlikely that this effect explains the entirety of the incumbent reelection effect we documented earlier in the paper. Consistent with the suggestive evidence provided by the heterogeneity in tax culture, we suggest that non-monetary factors (e.g., the direct utility non-evaders derive from catching the shirkers) must play an important role.

6.3 Alternative Explanations

In this section, we argue that the increase in tax enforcement arising from the program drives the results on voting support for the local incumbents and that this channel more than offsets several alternative potential explanations about the impact of the Ghost Buildings program.

First, the publication of the number of ghost buildings could generate information about the incumbent. We believe this to be both unlikely and inconsistent with our findings. First, the number of ghost buildings is a slow moving stock variable that is likely to have accumulated over decades, rather than a reflection of only the most recent years. Most of the buildings found by the *Agenzia del Territorio* were not newly constructed. The existence of a term limit, paired with the fact that the time it takes to complete a building in Italy is generally longer than in most other OECD countries, suggests that most of these buildings could not have been built while the incumbent was in office. Second, we note that voters who could potentially receive information from the publication are most likely the ones who

[38] As for the remainder of the paper, the regression model includes fixed effects, and we therefore obtain identical results when using expenses per capita.

were not evading before the program, as evaders were already aware of their own evasion.

Keeping this premise in mind, we believe our results rule out this alternative explanation. In one version of this alternative story, voters, after learning about *low* levels of evasion detected by the program, reward the current mayor for having properly enforced tax payment in the past. This hypothesis predicts a *negative* impact of the detected ghost building intensity on incumbent reelection in post-program elections, and as such it is obviously inconsistent with our baseline results.

In another version of this alternative explanation, voters reward an incumbent mayor for having allowed *high* levels of evasion in the past. First, this contradicts the intuition that non-evaders, rather than those who previously evaded, are the ones who are potentially acquiring new information. Second, this is unlikely because the purpose of the program, and therefore the publication, was to shut down the evasion opportunity. Third, it is at odds with the fact that the positive impact of program intensity on incumbent reelection is lower in regions with higher tolerance for tax evasion. Fourth, it is also inconsistent with our results showing that towns with higher registration levels are more likely, rather than less likely, to reelect an incumbent mayor.

In a third potential alternative explanation, the program gives an incumbent an electoral rent by allowing her *to not register* the targeted ghost buildings, for instance, by reporting errors in the results generated by the mapping process.[39] If this were the prevailing mechanism, we would expect the positive impact of the program to be stronger in regions with a higher tolerance for tax evasion, but we find the opposite to be the case. In addition, such an explanation is inconsistent with the result that a higher share of registered ghost buildings at the time of a local election increases reelection likelihood. The empirical results thus provide strong evidence that it is the additional tax enforcement induced by the program, as opposed to these alternative explanations, that drives the increase in the reelection prospects of the incumbent.

[39]For example, the press agency of the mayor of a city in our sample, Capaccio Paestum, explicitly criticized the excessive media attention to the program, indicating that the unregistered buildings in that city were unregistered due to citizens' needs (Comune di Capaccio Paestum, 2010).

7 Conclusion

A rapidly growing literature shows that interventions that improve the technology of tax enforcement – third-party reporting, cross-checking, or better auditing algorithms – can substantially reduce tax evasion. Yet, political incentives to adopt these technologies are also of crucial importance. Policymakers will delay or prevent enforcement policies if they are bound to lose support because of them. In spite of this, little is known about the electoral impact of fighting tax evasion. This paper provides evidence of a positive interaction between technological improvements in tax-payer monitoring and political incentives. Specifically, local incumbents are shown to obtain positive political returns, namely, an increase in their reelection likelihood, from the Ghost Building program, a nationwide anti-tax evasion policy in Italy that was based on a new enforcement technology.

The underlying tax culture, broadly defined as the individual propensity and social norms determining evasion for a given level of technology, is another important determinant of tax compliance. It shapes the enforcement level a government can achieve for a given enforcement technology. We show that tax culture affects the political returns to undertaking anti-tax evasion policies. The increase in incumbent reelection probabilities in response to the Ghost Buildings program is larger in areas with a lower self-reported tolerance for tax evasion. Finally, we document that the political returns to enforcement policies are higher when the government is more efficient in providing public goods. This paper's findings have two important policy implications. First, they provide a framework for thinking about the political feasibility of policies that increase the visibility of tax evasion, thus lowering monitoring costs and increasing policymakers' incentives to improve enforcement. This has immediate relevance for special interest politics. Concentrated evader groups might effectively lobby to keep evasion hidden from the public, but they are unlikely to be able to punish an incumbent who enforces tax compliance after the evasion becomes broadly visible.

Second, there is potential complementarity among anti-tax evasion policies, government responsiveness, and social preferences for tax compliance. Governments that plan to implement novel enforcement policies should concurrently attempt to strengthen their capabilities, for instance, by improving the speed at which they respond to citizen's needs, or by increasing the social stigma associated with tax evasion. This complementarity will likely increase

the returns politicians obtain from anti-tax evasion policies and will thus make such policies better aligned with political agent incentives.

We are aware that using an identification strategy based on a specific natural experiment enhances the internal validity of our study but may come at the price of lower external validity, for instance, on shedding light on similar programs in other countries or on programs targeting other taxes. Yet, we speculate that evidence of positive political returns to anti-tax evasion policies in Italy, a country often cited as an example of poor tax culture, will be a lower bound for other OECD countries. We believe an interesting goal for future work would be to elucidate the potential non-linearity in the relationship between the prevalence of tax evasion and political returns to enforcement policies. In addition, we believe that complementarity between enforcement policies and social norms on evasion could potentially be relevant for policy design in other regions of the world.

Another important dimension of external validity concerns enforcement policies targeting other types of evasion. One of the merits of the Ghost Buildings program is that it detected the entire stock of evasion. In contrast, the effectiveness of policies targeting other tax-concealing activities might vary according to the ability of the specific evader to hide, which might, in turn, affect how the public would respond. We hope future work will shed light on the political returns to other enforcement policies around the world.

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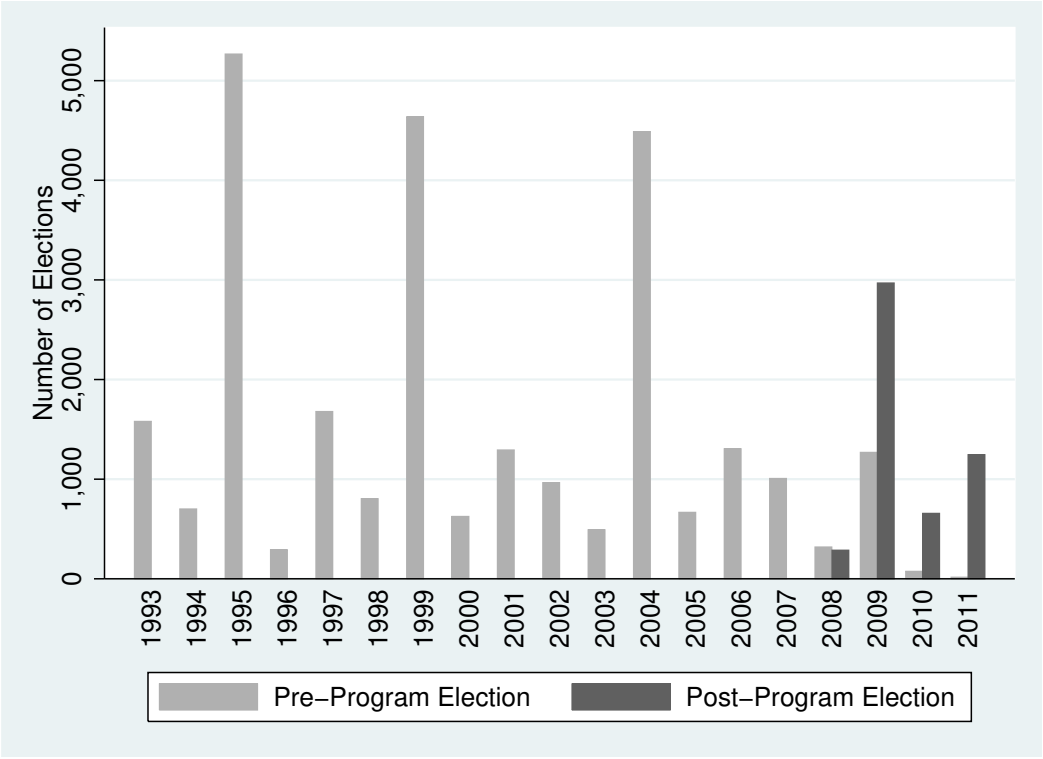
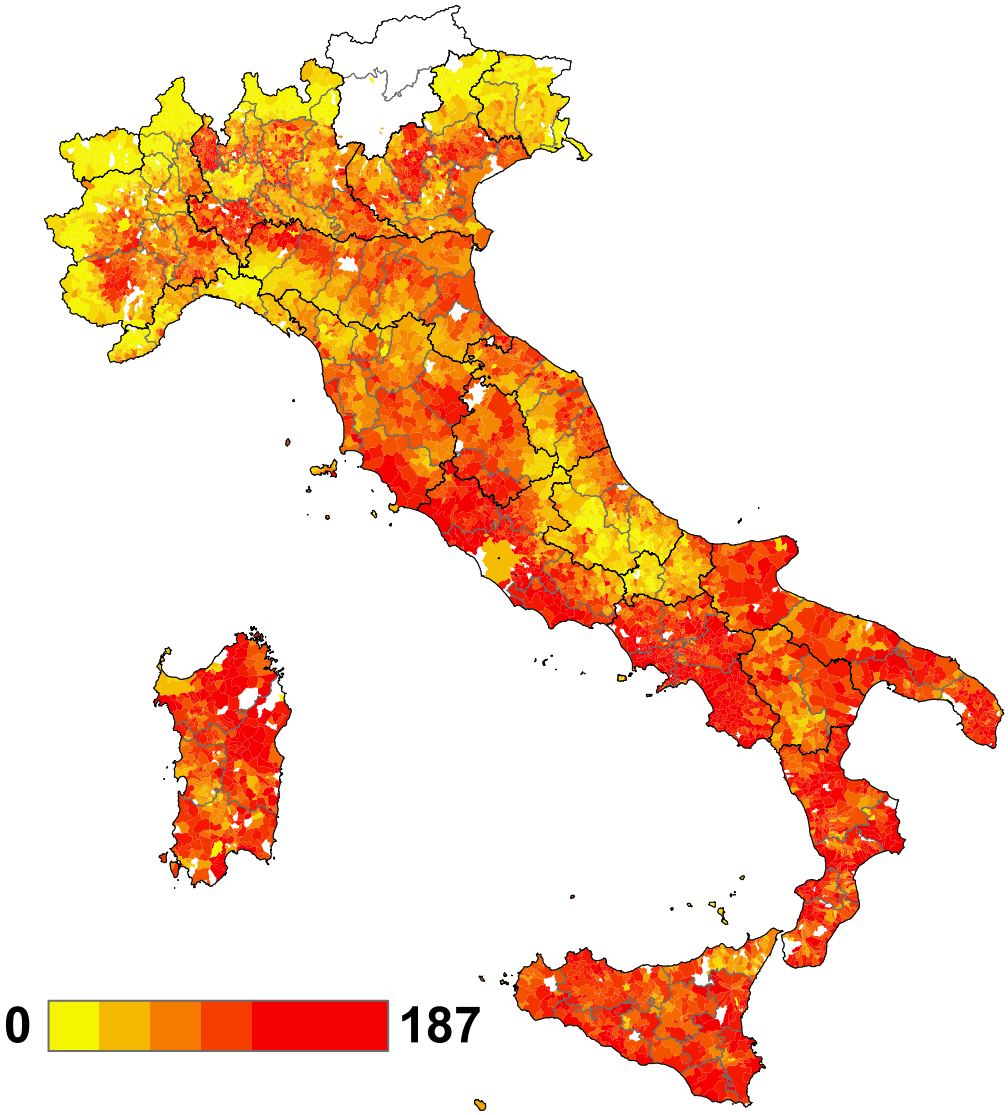
Figures

Figure 1: Number of Elections per Year



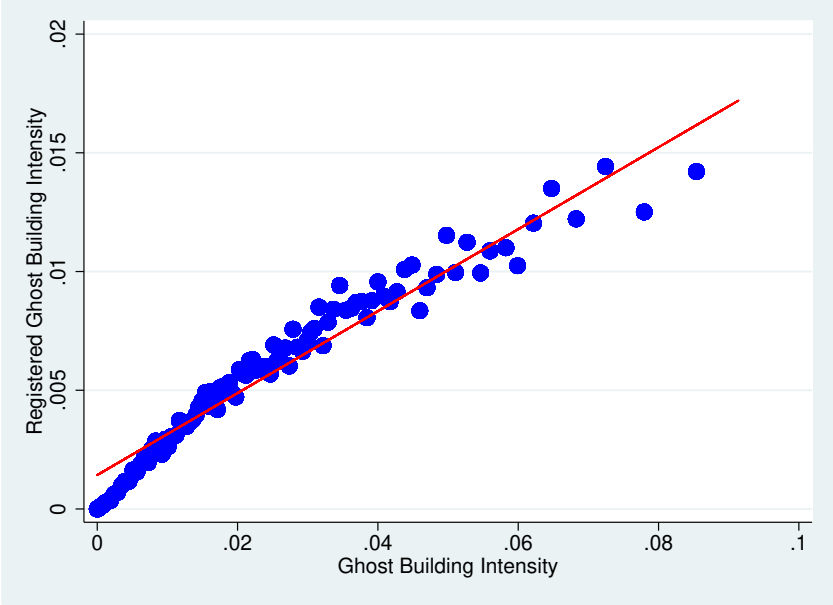
Notes: The figure shows, for each calendar year, the number of elections held before and after the inception of the Ghost Buildings program.

Figure 2: *Ghost Building Intensity* (per 1,000 land registry parcels)



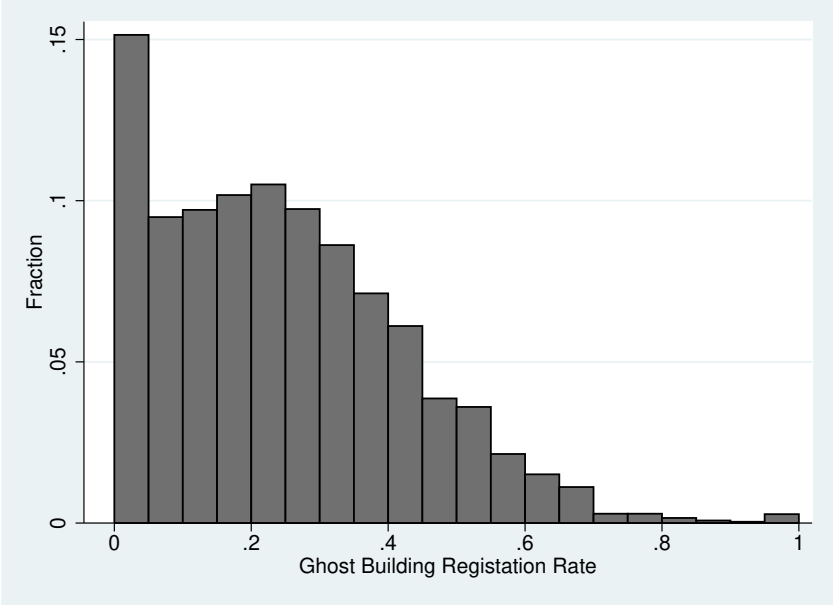
Notes: In this figure, Ghost Building Intensity is defined as the number of land registry parcels with ghost buildings *per thousand* of land registry parcels. White areas identify towns with missing data.

Figure 3: Registered Ghost Building Intensity



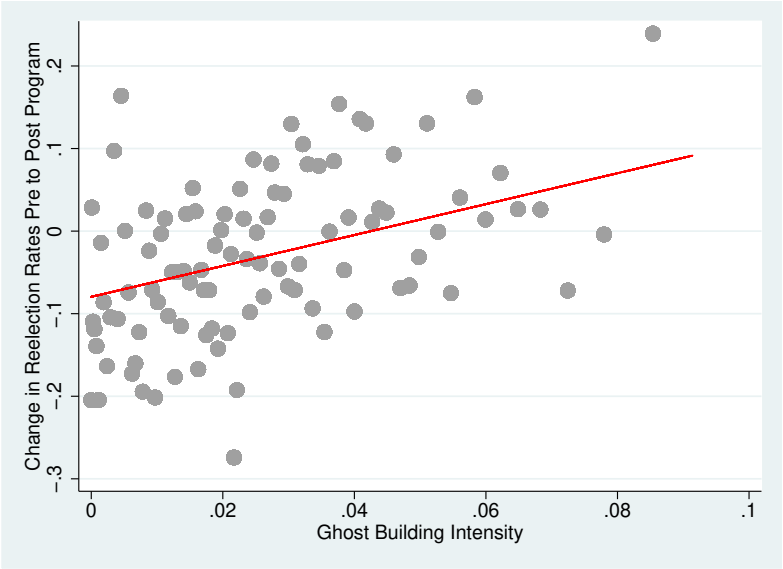
Notes: The scatter plots the relation between *Registered Ghost Building Intensity* (i.e., the fraction of land parcels with ghost buildings that get registered by April 2011) and *Ghost Building Intensity* (i.e., the fraction of land parcels with ghost buildings identified by the program). The x-axis is partitioned into percentiles. The x-axis of each dot is the median value of the ghost building intensity in the percentile. The y-axis is the average value of the registered ghost building intensity in the percentile. We cut the top 1% of the x-axis values from the graph. The line plots the predicted values from a linear regression model.

Figure 4: Ghost Building Registration Rate



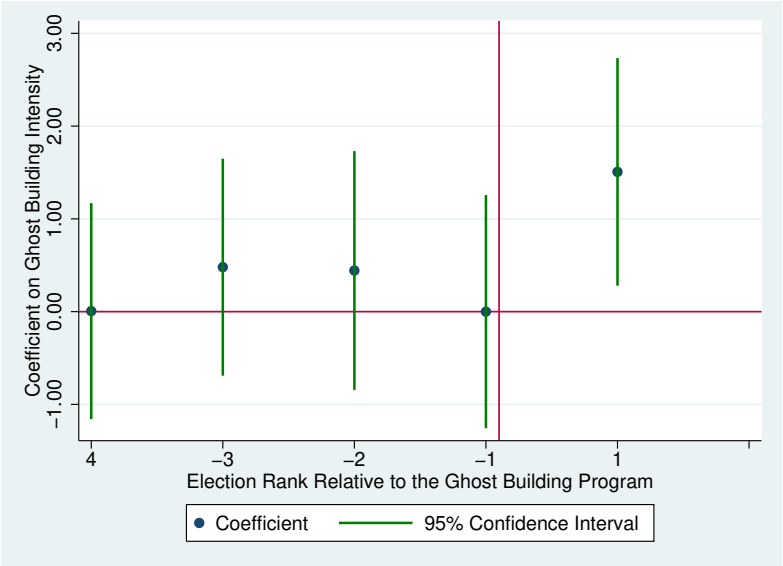
Notes: The histogram shows the distribution of the ghost building registration rate at April 30, 2011, defined as the ratio between the number of land registry parcels with ghost buildings that get registered by this date and the number of land registry parcels with ghost buildings identified by the program.

Figure 5: Difference in reelection rates pre- to post- Ghost Buildings program



Notes: The scatter plots the relation between the change in the average (year-demeaned) reelection rate between the pre-program and the post-program periods and the *Ghost Building Intensity*. The x-axis is partitioned into percentiles. The x-axis of each dot is the median value of the ghost building intensity in the percentile. The y-axis is the average value of the registered ghost building intensity in the percentile. We cut the top 1% of the x-axis values from the graph. The sample includes elections in which the incumbent does not face a binding term-limit. The line plots the predicted values from a linear regression model.

Figure 6: Ghost Building Intensity Coefficient by Election Pre/Post Program



Notes: The graph reports the coefficients on the ghost building intensity for each election before and after the beginning of the Ghost Buildings program. On the x-axis, elections are ranked based on their occurrence relative to the program. The regression includes town and year fixed effects. The sample includes elections in which the incumbent does not face a binding term-limit. For each election rank, we report the point estimate and the 95% confidence interval. The election before the program ("-1") is the omitted category, for which confidence interval is obtained as the mean of the confidence interval width in election -2 and election +1. The modal number of years between elections is five years between 1993 and 2001, and four afterwards.

Tables

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Panel A: Ghost Building Town Variables					
Ghost Building Intensity	0.027	0.021	0	0.187	7720
Registered Ghost Building Intensity (Apr 2011)	0.006	0.006	0	0.051	7720
Ghost Building Registration Rate (Apr 2011)	0.243	0.181	0	1	7720
Panel B: Geographic Town Variables					
Town Area Size (sq km)	37.044	50.096	0.2	1307.71	7720
Altitude (mt)	510.584	461.487	0	3072.5	7720
Land Registry Parcels (1,000)	10.776	13.278	0.001	514.372	7720
Panel C: Socio-Economic Town Variables					
Population (1,000)	7.225	40.23	0.033	2546.804	7720
Disposable Income per capita (1,000 Euros)	13.449	3.042	5.013	44.949	7720
Urbanization Index	1.619	0.684	1	3	7720
Non-Profit Associations/1,000 pop	5.293	3.912	0.212	81.218	7720
Number of Firms per capita	0.076	0.027	0.018	0.344	7720
Panel D: Mayor Variables					
Mayor Age	49.03	9.5	21	83	7720
Mayor Education	3.29	0.69	1	5	7720
Mayor Born Same City (0/1)	0.47	0.49	0	1	7720
Mayor Term Number	1.3	0.46	1	2	7720
Mayor Woman (0/1)	0.1	0.3	0	1	7720
Panel E: Election Panel Variables					
Term Limit Indicator (0/1)	0.201	0.401	0	1	32422
Election Rank Relative to Publication	-2.017	1.57	-8	1	25893
Post Program Election (0/1)	0.143	0.351	0	1	25893
Years Elapsed since Program Inception (= 0 if ≤ 0)	0.299	0.802	0	4	25893
Incumbent Reelection (0/1)	0.454	0.498	0	1	25893
N. Candidates	2.761	1.301	1	17	24585
Incumbent Rerun (0/1)	0.572	0.495	0	1	25525
Victory Margin	25.999	26.942	0	100	23933
Runoff (0/1)	0.525	0.499	0	1	2285

Notes: **Socio-Economic Town Variables** are collected before the Ghost Buildings program inception. **Mayor Variables** refer to characteristics of the incumbent mayor at the time of program inception. Summary statistics for the **Election Panel Variables** are reported for the subsample of elections with no binding term limit, except for *Term Limit Indicator*. In all the tables, we replace missing values for the town-level controls with regional means, so to retain a constant sample size. The range of missing values across variables spans from 0 to 3.8%. The results are unchanged if, for each covariate, we add a dummy equal to one for a missing value, instead. A detailed description and source of each variable is provided in Appendix Tables A.1 and A.2.

Table 2: The Determinants of Ghost Building Intensity (per 1,000 land parcels)

	(1)	(2)	(3)
Town Area Size (sq km)	0.102***	0.123***	0.098***
	(0.021)	(0.015)	(0.012)
Altitude (mt)	-0.015***	-0.009***	-0.011***
	(0.002)	(0.002)	(0.002)
Land Registry Parcels (1,000)	-0.236***	-0.328***	-0.270***
	(0.065)	(0.070)	(0.047)
Population (1,000)		0.020	0.004
		(0.016)	(0.011)
Disposable Income per capita (1,000 Euros)		-2.598***	-1.224***
		(0.363)	(0.292)
Urbanization Index		5.940***	4.404***
		(1.837)	(1.652)
Non-Profit Associations/1,000 pop		-0.463***	-0.207***
		(0.147)	(0.074)
Number of Firms per capita		56.244***	89.888***
		(20.100)	(17.828)
Region FE			X
Observations	7720	7720	7720

Notes: The dependent variable is the town-level ghost building intensity per thousand of parcels, defined as the ratio between the number of land registry parcels with ghost buildings and the total number of land registry parcels, multiplied by one thousand. Standard errors are clustered at provincial level. *p<0.1, **p<0.05, ***p<0.01.

Table 3: The Determinants of the Ghost Building Registration Rate

	(1)	(2)	(3)
Mayor Age	-0.066***	-0.066***	-0.054***
	(0.021)	(0.021)	(0.019)
Mayor Education	0.802***	0.703**	0.806***
	(0.286)	(0.271)	(0.239)
Mayor Born Same City (0/1)	1.054**	1.127***	0.946**
	(0.424)	(0.425)	(0.411)
Mayor Term Number	-0.201	-0.084	-0.082
	(0.355)	(0.341)	(0.351)
Mayor Woman (0/1)	-0.915	-1.223*	-1.185*
	(0.637)	(0.625)	(0.608)
Geograpic Controls		X	X
Socio-Economic Controls			X
Observations	7720	7720	7720

Notes: The dependent variable is the town-level ghost building registration rate, defined as the ratio between the number of land registry parcels with ghost buildings that get registered by April 2011 and the number of land registry parcels with ghost buildings identified at the beginning of the program. Refer to Table 1 for a description of the *Geographic* and *Socio-Economic* Controls. All the regressions include regional fixed effects and year-of-program-inception fixed effects. Standard errors are clustered at provincial level. *p<0.1, **p<0.05, ***p<0.01.

Table 4: Ghost Building Intensity and Incumbent Reelection: Baseline Results

	Reduced Form				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ghost Building Intensity*Post					1.097***	1.114***	1.042***	1.074***
					(0.358)	(0.373)	(0.378)	(0.366)
Ghost Building Intensity*Province Post	1.083***	1.061***	0.953***	0.958***				
	(0.343)	(0.358)	(0.360)	(0.347)				
Town FE		X	X	X		X	X	X
Election Year FE			X	X			X	X
Town Controls*Post				X				X
Observations	25893	25893	25893	25893	25893	25893	25893	25893

Notes: The dependent variable is a binary indicator equal to one if the incumbent mayor is reelected (mean 0.453). **Post** is a binary indicator equal to one if the election occurs after the Ghost Buildings program inception. **Province Post** is a binary indicator equal to one if the election occurs after the Ghost Buildings program modal inception year in the province. In the columns grouped under the header "2SLS", *Post* is instrumented with *Post Province*. **Ghost Building Intensity** is defined as the ratio between the number of land registry parcels with ghost buildings and the total number of land registry parcels. All the columns include an interaction between macro-areas fixed effects and either *Province Post* (Columns (1)-(4)) or *Post* (Columns (5)-(8)). Columns (1) and (5) include the Ghost Building Intensity level. **Extra Controls*Post** include town-level controls interacted with the *Post* dummy. Refer to Table 1 for a list of these variables. The regression sample includes all the elections between 1993 and 2011 in which the incumbent does not face a binding term-limit. Standard errors are clustered at provincial level. *p<0.1, **p<0.05, ***p<0.01.

Table 5: Ghost Building Intensity and Election Competitiveness

	N. Candidates	Incumbent Rerun	Victory Margin	Runoff
	(1)	(2)	(3)	(4)
Ghost Building Intensity*Post Program	-2.383**	1.115**	42.063	-4.502***
	(1.057)	(0.457)	(25.984)	(1.383)
Dependent Variable Mean	2.761	0.572	25.999	0.525
Observations	24441	25483	23562	2216

Notes: **N. Candidates** is the number of candidates running for election. **Incumbent Rerun** is a binary indicator equal to one when the current incumbent runs for reelection. **Victory Margin** is the percentage point difference between the first and the second candidate in the elections (we use first-round percentages even for towns with a runoff). **Runoff** is a binary indicator, defined only for towns with more than 15,000 inhabitants, equal to one if the election requires a runoff. This occurs if the first candidate in the first round receives less than 50% of the votes. **Post** is a binary indicator equal to one if the election occurs after the Ghost Buildings program inception. In all the columns, *Post* is instrumented by *Province Post*, a binary indicator equal to one if the election occurs after the modal program inception year in the province. **Ghost Building Intensity** is defined as the ratio between the number of land registry parcels with ghost buildings and the total number of land registry parcels. All the regressions include town fixed effects, election-year fixed effects and an interaction between macro-areas fixed effects and *Post*. The regression sample includes all the elections between 1993 and 2011 in which the incumbent does not face a binding term-limit. Standard errors are clustered at provincial level. *p<0.1, **p<0.05, ***p<0.01.

Table 6: Ghost Building Registration and Incumbent Reelection

	(1) OLS	(2) OLS	(3) 1st Stage	(4) 2SLS	(5) 2SLS
Ghost Building Registration Rate*Post	0.225***	0.206***		0.625***	0.674***
	(0.085)	(0.078)		(0.196)	(0.195)
Years Elapsed since Program Inception			0.076***		
			(0.009)		
Ghost Building Intensity*Post	1.275***	1.359***		1.561***	1.889***
	(0.374)	(0.409)		(0.391)	(0.448)
Town Controls*Post		X			X
Observations	25893	25893	7720	25893	25893

Notes: Columns (1), (2), (4), and (5) present election-panel regressions where the dependent variable is a binary indicator equal to one if the incumbent mayor is reelected (mean 0.453). **Post** is a binary indicator equal to one if the election occurs after the Ghost Buildings program inception. In all the columns, *Post* is instrumented by *Province Post*, a binary indicator equal to one if the election occurs after the modal program inception year in the province. **Ghost Building Intensity** is defined as the ratio between the number of land registry parcels with ghost buildings and the total number of land registry parcels. **Registration Rate** refers to the imputed registration rate at the time of the election using the April 2011 rate as a starting point and assuming a constant yearly registration rate. **Extra Controls*Post** include town-level controls interacted with the *Post* dummy. Refer to Table 1 for a list of these variables. The regressions include town fixed effects, election-year fixed effects and an interaction between macro-areas fixed effects and *Post*. The regression sample for these columns includes all the elections between 1993 and 2011 in which the incumbent does not face a binding term-limit.

Column (3), **First Stage**, presents cross-town regressions where the dependent variable is the ghost building registration rate at April 2011. The variable **Years Elapsed since Program Inception** measures the years elapsed between the program inception year and 2011. The regression includes the levels of the above controls, the level of the ghost building intensity and macro-areas fixed effects. Standard errors are clustered at provincial level. *p<0.1, **p<0.05, ***p<0.01.

Table 7: Ghost Building Intensity and Incumbent Reelection: Heterogeneity Analysis

	(1)	(2)	(3)	(4)
Ghost Building Intensity*Post	1.063***	1.311**	1.174***	1.229*
	(0.380)	(0.668)	(0.391)	(0.682)
...*Justify Tax Cheating	-0.639*	-0.734*		
	(0.364)	(0.404)		
...*Speed of Public Good Provision			0.627*	0.592
			(0.380)	(0.397)
GBI*Macro Area*Post	No	Yes	No	Yes
Observations	25893	25893	25893	25893

Notes: The dependent variable is a binary indicator equal to one if the incumbent mayor is reelected (mean 0.453). **Post** is a binary indicator equal to one if the election occurs after the Ghost Buildings program inception. In all the columns, *Post* is instrumented by *Province Post*, a binary indicator equal to one if the election occurs after the modal program inception year in the province. **Ghost Building Intensity** is defined as the ratio between the number of land registry parcels with ghost buildings and the total number of land registry parcels. **GBI*Macro Area*Post** is the triple interaction among macro-areas fixed effect, ghost building intensity and *Post* All the regressions include town fixed effects, election-year fixed effects, interactions between macro-areas fixed effects and *Post*, and an interaction between the relevant heterogeneity variable for the column and *Post*. The regression sample includes all the elections between 1993 and 2011 in which the incumbent does not face a binding term-limit. Standard errors are clustered at provincial level. *p<0.1, **p<0.05, ***p<0.01.

Table 8: Local Government Expenditures

	OLS	2SLS	
	(1)	(2)	(3)
Ghost Buildings Intensity * Post		0.497*	0.621***
		(0.263)	(0.206)
Ghost Building Intensity*Post Province	0.441*		
	(0.253)		
Extra Controls*Post	No	No	Yes
Observations	74664	74664	74664

Notes: The dependent variable is the natural logarithm of municipal government expenditures. **Post** is a binary indicator equal to one if the election occurs after the Ghost Buildings program inception. In all the columns, *Post* is instrumented by *Province Post*, a binary indicator equal to one if the election occurs after the modal program inception year in the province. **Ghost Building Intensity** is defined as the ratio between the number of land registry parcels with ghost buildings and the total number of land registry parcels. **Extra Controls*Post** include town level controls interacted with the *Post* dummy. Refer to Table 1 for a list of these variables. All the regressions include town fixed effects, election-year fixed effects and an interaction between macro-areas fixed effects and *Post*. Standard errors are clustered at provincial level. * p<0.1, **p<0.05, ***p<0.01.

A Appendix (For Online Publication)

Appendix Figures

Figure A.1: The Ghost Building Identification Process

Figure A.1A: Aerial Picture

Figure A.1B: Digital Land Registry Map

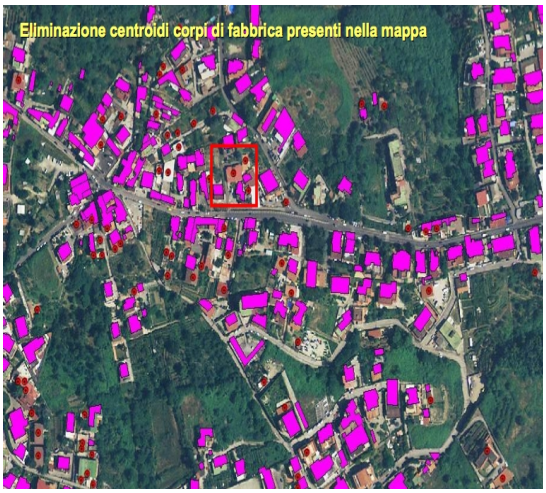
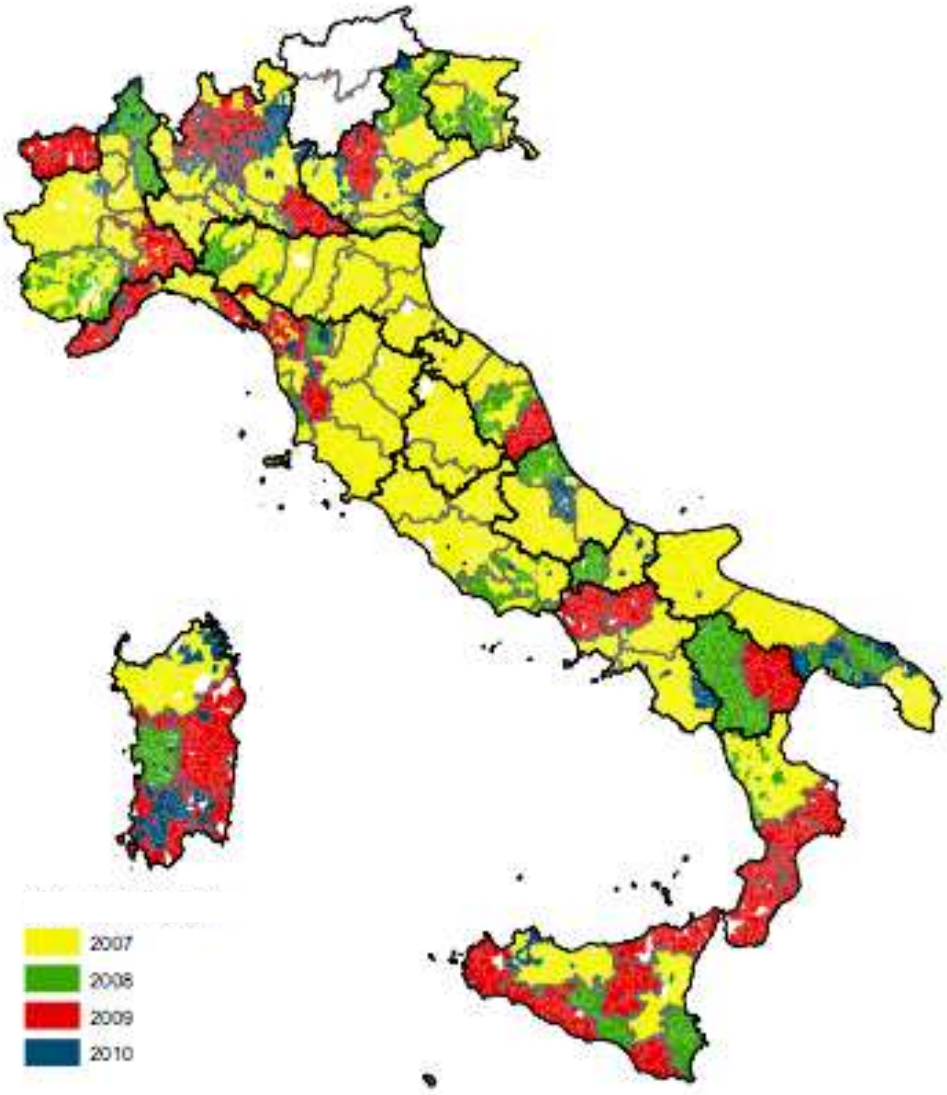


Figure A.1C: Overlay

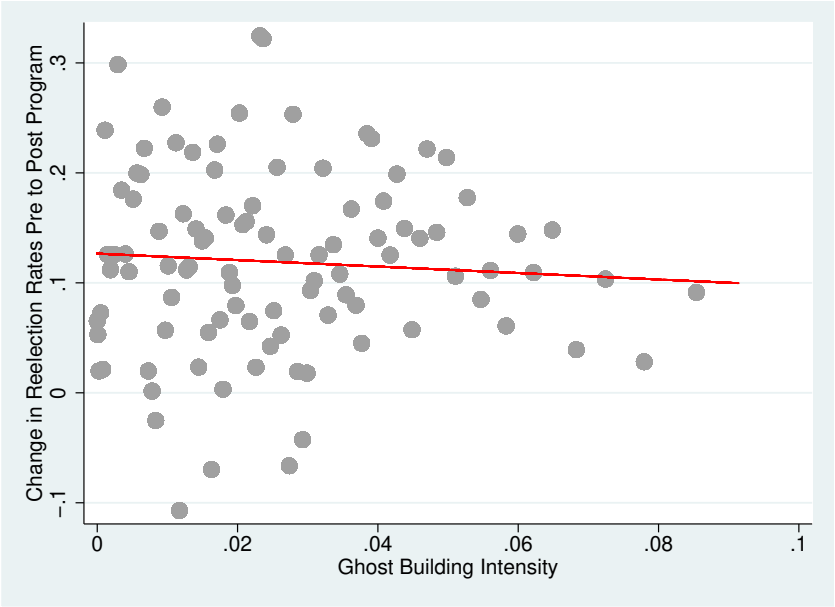
Source: Agenzia del Territorio

Figure A.2: Ghost Buildings Program Inception Year



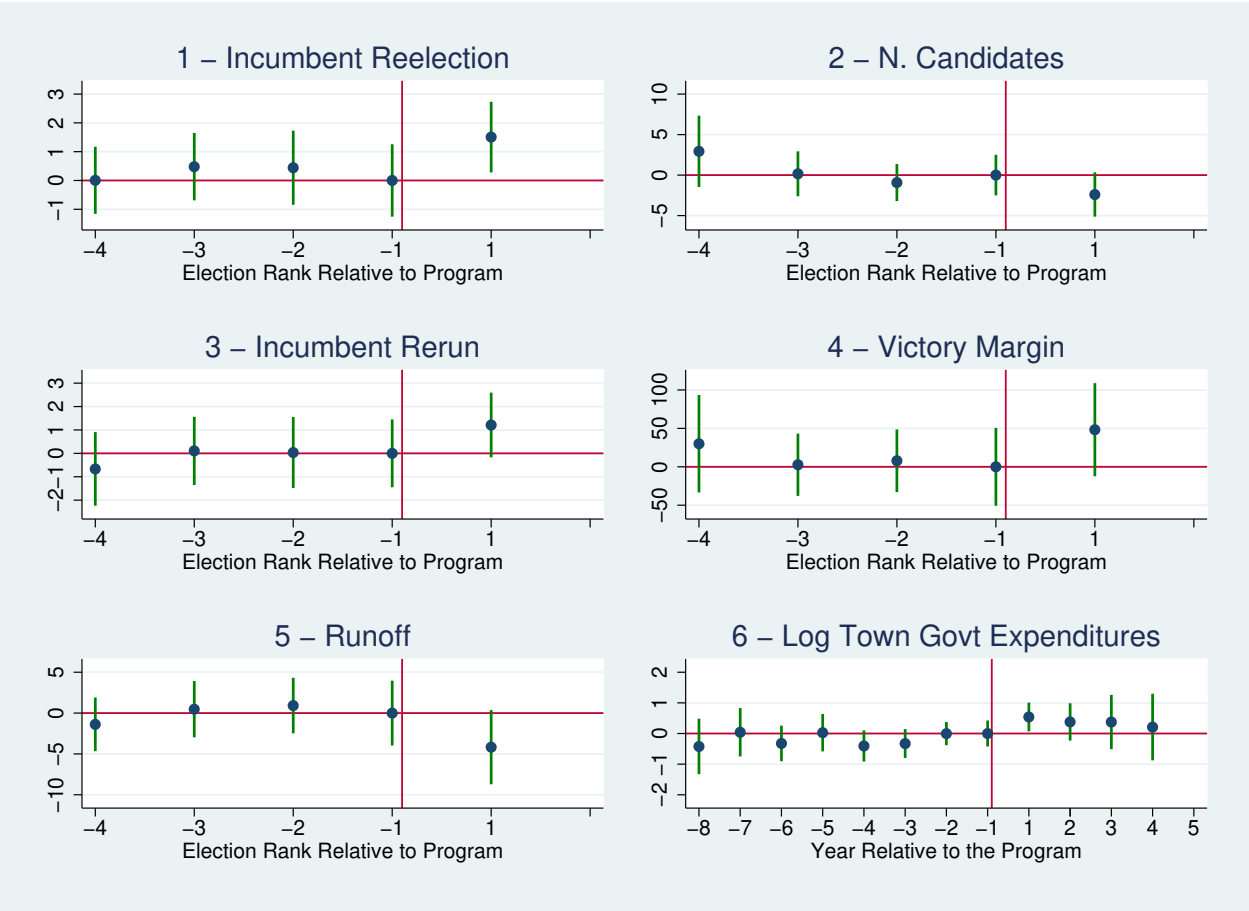
Notes: The figure shows the year of inception of the Ghost Building program (i.e., the year of publication of the list of ghost buildings) in each town. White areas identify towns with missing data.

Figure A.3: Difference in reelection rates pre- to post- *Placebo* Ghost Buildings program



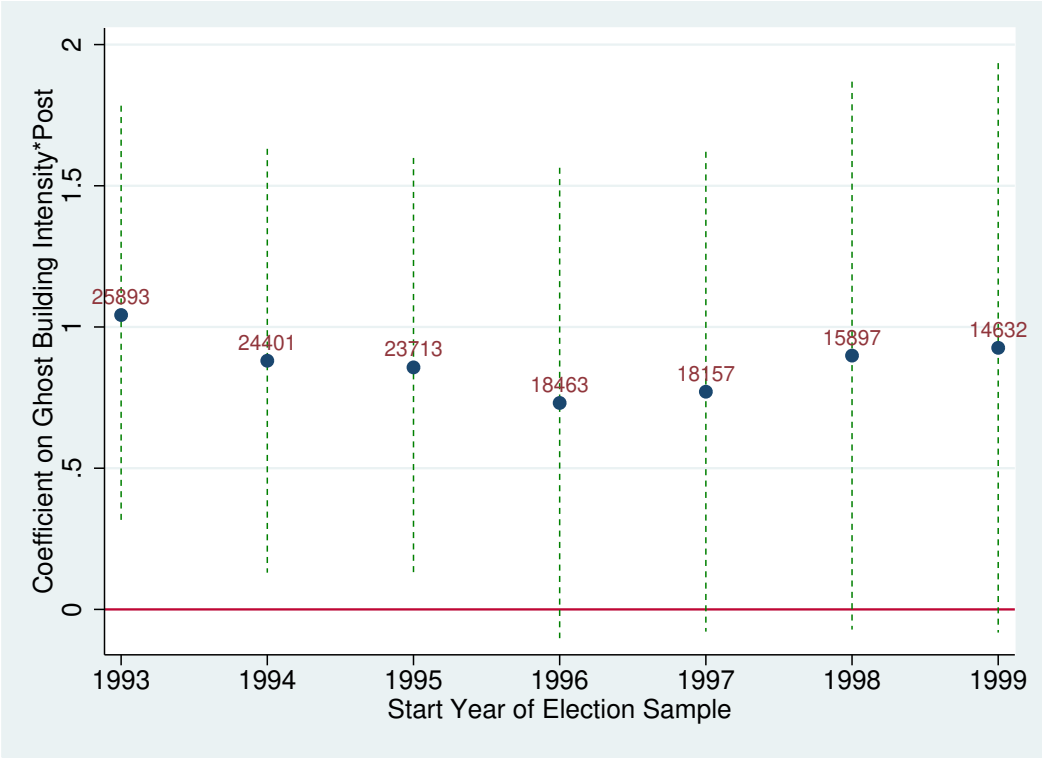
Notes: The scatter plots the relation between the change in the average (year-demeaned) reelection rate between the pre-placebo-program and the post-placebo-program periods and the *Ghost Building Intensity* (i.e., the ratio between the number of land registry parcels with ghost buildings and the total number of land registry parcels). The placebo subsample of observations used for this graph only includes election that occurred before the actual program inception. In each town, the year of the placebo program start is defined as nine years before the actual publication. This roughly divides the graph sample in two equally sized groups of pre-placebo and post-placebo elections. The x-axis is partitioned into percentiles. The x-axis of each dot is the median value of the ghost building intensity in the percentile. The y-axis is the average value of the registered ghost building intensity in the percentile. We cut the top 1% of the x-axis values from the graph. The sample includes elections in which the incumbent does not face a binding term-limit. The line plots the predicted values from a linear regression model.

Figure A.4: Ghost Building Intensity Coefficient by Election Pre/Post Program



Notes: **Graphs 1 to 5** report the coefficients on the ghost building intensity *for each election* before and after the beginning of the Ghost Buildings program. The modal number of years between elections is five years between 1993 and 2001, and four afterwards. On the x-axis, elections are ranked based on their occurrence relative to the program. The sample includes elections in which the incumbent does not face a binding term-limit. **Graph 6** reports the coefficients on the ghost building intensity *for each calendar year* before and after the beginning of the Ghost Buildings program. The dependent variable is in natural logarithm. On the x-axis, years are ranked based on their occurrence relative to the program. We drop the year of program inception due to its ambiguous treatment status. In all the graphs, the regression includes town and year fixed effects. We report the point estimate and the 95% confidence interval. The last election/year before the program ("-1") is the omitted category. The coefficient on ghost building intensity for this group is normalized to zero. Confidence interval width for this election is obtained as the mean of the confidence interval width in election/year -2 and election/year +1.

Figure A.5: Robustness to changes in election sample time span



Notes: The figure presents robustness of the results on incumbent reelection to changes in sample years. The y-axis shows the coefficient (and 95% confidence intervals) on the interaction between ghost building intensity and the post-program indicator as estimated in our baseline specification (Table 4, Column 7). The x-axis is the start year of the alternative election samples we use (the final year is 2011 for all the samples). The first sample, 1993-2011, is the baseline sample. In 1993, an electoral law reform introduced mayor individual ballot election. The marker label shows the number of observations in each sample.

Appendix Tables

Table A.1: Variable description and sources

Variable	Definition and measure	Sample	Source
Ghost Building Intensity	Ghost Building Intensity Ratio between the number of land registry parcels with ghost buildings and the total number of parcels.	Program inception	ATD
Registration Rate	Registration Rate Percentage of ghost building parcels that get registered by the April 2011	2011	ATD
Total expenditures	Total local expenditures Per-resident	2000-2011	IMI Financial reports, *Quadro 3*
Town Area Size	Area Size of the town, in square km	2001	SAIM
Altitude	Altitude Altitude of the city, in meters	2001	SAIM
Population	Population Population, in thousand of inhabitants	2001	Census
Disposable Income per capita	Disposable income per capita at the municipal level, in thousand of euros	2005	SAIM
Urbanization Index	Index is equal to one if density is less than 100 people per sq. km; it is equal to two if density is between 100 and 500 people per sq. km; it is equal to three if density is above 500 people per sq.km.	2001	SAIM
Non-Profit Associations	Non profit association per thousand of inhabitants	2001	SAIM
Number of Firms per capita	Number of firms per capita thousand of inhabitants	2001	SAIM
Justify Tax Cheating	Answers to the question "Do you Justify Tax Cheating?", originally coded on a scale 1 (never justifiable) to 10 (always justifiable). Normalized variable.	1981-2008	EVS
Speed of Public Good Provision	Speed of current expenditures Ratio between paid over committed current expenditures	2005-2006	IMI

Notes: ATD stands for Agenzia del Territorio Database. IMI stands for Italian Ministry of the Interior. SAIM stands for Statistical Atlas of Italian Municipalities. EVS stands for European Values Survey.

Table A.2: Political Variables description and sources

Variable	Definition and measure	Sample	Source
Mayor Age	Age of the mayor Age of the mayor, in number of years	Program inception	IMI
Mayor Education	Education of the mayor Categories: Primary Education, High school education, University degree, Postgraduate professional schooling, GED equivalent schooling, Vocational schooling	Program inception	IMI
Mayor Born Same City	Place of birth of the mayor Dummy variable equal to 1 if mayor is born in the same city	Program inception	IMI
Mayor Term Number	Tenure of the mayor Number of the mayoral's term, in number of years	Program inception	IMI
Mayor Woman	Gender of the mayor Dummy variable equal to 1 if the mayor is a woman	Program inception	IMI
Term Limit Indicator	Mayor is not eligible for reelection Equal to 1 if mayor has a binding term limit	1993-2011	IMI
Incumbent Reelection	Incumbent mayor is reelected Equal to 1 if mayor is reelected	1993-2011	IMI
Incumbent Rerun	Incumbent mayor decides to run for office again Equal to 1 if mayor re-runs	1993-2011	IMI
N. Candidates	Number of candidates Number of candidates	1993-2011	IMI
Victory Margin	Margin of victory Margin of victory of the incumbent mayor	1993-2011	IMI
Runoff	Election has a runoff Runoff	1993-2011	IMI

Notes: IMI stands for Italian Ministry of the Interior.

Table A.3: Ghost Building Intensity and Incumbent Reelection: Robustness

	(1) Baseline	(2) Region Interactions	(3) Region and Controls Interactions	(4) Town w/ post-election	(5) Trim top 1% Intensity	(6) Drop Small Towns	(7) Post 2007 Instrument	(8) Alternative Normalization
Ghost Building Intensity*Post	1.042***	1.162***	1.320***	0.932**	1.255**	1.195***	1.345**	0.119*
	(0.378)	(0.386)	(0.459)	(0.368)	(0.493)	(0.390)	(0.563)	(0.067)
Observations	25893	25893	25893	17566	25618	23337	25893	25893

Notes: The dependent variable is a binary indicator equal to one if the incumbent mayor is reelected (mean 0.453). **Post** is a binary indicator equal to one if the election occurs after the Ghost Buildings program inception. In all the columns, *Post* is instrumented by *Province Post*, a binary indicator equal to one if the election occurs after the modal program inception year in the province. All the regressions include town fixed effects, election-year fixed effects and an interaction between macro-areas fixed effects and *Post*. In the specification **Region Interactions**, we add interactions between the post-program indicator and regional fixed effects. In the specification **Region and Controls Interactions**, we add interactions between the post-program indicator and regional fixed effects and between the post-program indicator and town-controls (refer to Table 1 for a list of these variables). In the specification **Town w/post-election** we only retain in our sample the towns with one post-program election. In the specification **Trim top 1% Intensity**, we drop towns in the first percentile of ghost building intensity (0.0928). In the specification **Drop Small Towns**, we exclude from the sample towns within the bottom 10% of the distribution of land registry parcels (2633). In Columns (1)-(3) and (5) *Post* is instrumented by *Province Post*, a binary indicator equal to one if the election occurs after the modal program inception year in the province. In Column (7) — **Post 2007**— we instrument it with a binary indicator equal to one if the election occurs after 2007, the start date for the first (and modal) round of the program in the country. **Ghost Building Intensity** is defined as the ratio between the number of land registry parcels with ghost buildings and the total number of land registry parcels. except in Column (8) — **Alternative Normalization** — where it is defined as the ratio between the number of land registry parcels with ghost buildings and the total number of buildings. All the regressions include town fixed effects, election-year fixed effects and an interaction between macro-areas fixed effects and *Post*. The regression sample includes all the elections between 1993 and 2011 in which the incumbent does not face a binding term-limit. Standard errors are clustered at provincial level. * p<0.1, **p<0.05, ***p<0.01.