Essays on the economics of land use regulation and political cycle

PhD thesis

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Academic Year 2017/18

Acknowledgments

I would like to thank my supervisor Marco Leonardi for the guidance, support and motivation he has provided throughout these three years. I benefited greatly from many detailed comments and fruitful discussions with Sauro Mocetti. I would also thank the director of the phd program Alessandro Missale for his kind support.

I am grateful to seminar participants at the University of Milan and Pavia. I also thank two anonymous reviewers, Paolo Balduzzi, Massimo Bordignon, Emanuele Padovani, Emanuele Vendramini. A special thank to Massimo Tatarelli and Carmine La Vita (Italian Ministry of the Interior), Simonetta Rosa and Monica Crivellari (Italian Court of Auditors), Michele Munafo' (ISPRA), Romain Bocognani and Giovanna Altieri (Associazione Nazionale Costruttori Edili) for help with data.

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Chapter 1

Introduction

This thesis is an empirical analysis of the effects of relaxing land use regulation on the housing market and local economic activity. The existence of a political cycle and a term-limit effect on the issue of building permits to benefit voters is also investigated.

The first paper analyses how land use regulation may affect the residential and non-residential development of cities. The extent and restrictiveness of these rules may lower the elasticity of local housing supply and creating entry barriers that may discourage entrepreneurship or reduce competition. Indeed, land use regulation serves social purposes but has substantial economic costs as far as constructors have to comply with housing and building codes and follow administrative procedures to obtain authorisation to build. The higher the complexity of the overall process, the higher the financial and time costs and uncertainty that builders incur.

This work applies a quasi-experimental design exploiting the fact that in 1999 the Italian government imposed fiscal rules on municipal governments and in 2001 relaxed them for municipalities below 5,000 inhabitants; from 2003 the law also enabled municipalities to finance current expenditures with urbanization revenues. Therefore, we use a difference-in-discontinuity design to assess whether Italian cities rely on urbanization revenues they collect from releasing building permits to meet fiscal rules and how these budgeting choices affect the real estate market and economic activity.

The analysis shows that tightening fiscal rules translates into higher fiscal revenues: targeted municipalities systematically collect more urbanization revenues (about 20%) likely to finance current expenditure. This increase may be interpreted as indirect evidence of a relaxation of land use regulation and is accompanied by an increase in the released building permits, particularly for the non-residential market (+35%). Moreover, we show how the ease of land use regulation has affected business dynamics relaxing entry barriers, i.e. favouring firms' entry and employment growth at the local level: firms' branches have grown by 2% and, correspondingly, the employment level by 5%.

The second paper focuses on the within-term political cycle effect and term-limit impact on the issue of building permits. The evidence of political cycle in the empirical literature often result to be mixed since macroeconomic variables may not capture the opportunistic behaviour of incumbent politicians close to the election since they have a limited influence on few spending items. Moreover, fiscal benefits can be lagged and are not always targetable, thus providing fewer chances to favour potential voters. Nonetheless, mayors can manipulate both duration and outcomes of the administrative processes that provide immediate benefit to voters, as for the release of building permits.

Exploiting the exogeneity of regular election dates - i.e. those following completed full-terms -

and the availability of high-frequency data, the analysis shows that the issue of building permits is systematically higher than the trend during the period just before an election, then the activity slows down to lower values. The political cycle increases the monthly amount of released permits by 0.011 to 0.062 standard deviations from the average monthly value over the considered period (between +3% and +16%), and it is mainly concentrated within 12 months around the election date. This effect is significant for cities below 15,000 inhabitants, and it is detected both in the northern and southern regions. Furthermore, the analysis allows disentangling the impact of the last-terms, finding that the incentive effect of possible re-election makes room for a political cycle and then excluding a reputational argument for the behaviour of the mayor.

Chapter 2

The real effects of land use regulation: quasi-experimental evidence from a discontinuous policy variation [1]

[1]This chapter is co-authored with Marco Leonardi (University of Milan) and Sauro Mocetti (Bank of Italy)

Abstract

We provide quasi-experimental evidence on the effects of a relaxation of land use constraints on the housing market and local economic activity. We exploit the fact that in 1999 the central government imposed fiscal rules on municipal governments and in 2001 relaxed them for municipalities below 5,000 inhabitants. We find that municipalities rely on urban revenues they collect from releasing building permits to avoid fiscal distress and finance current expenditure. The rise of building permits is concentrated in the non-residential market and has represented de facto a reduction of entry barriers. As downstream effects, we find an increase of employment and firms' entry.

JEL Codes: D73, H72, R52, R31, R33

Keywords: urbanization revenues, land use regulation, building permits, firms' entry, employment

2.1 Introduction

Countries differ significantly in land use regulation. In Italy an entrepreneur spend 228 days to complete all the procedures to build a warehouse; the corresponding figures for the US, the UK, Germany and France are respectively 81, 86, 126 and 183. The ease of dealing with construction permits is also highly heterogeneous within countries, reflecting a wide array of local government regulations. Among the main Italian cities, the number of days ranges from about 150 in Milan to more than 300 in Palermo[2]. Large differences arise also in terms of number, complexity and monetary costs of the procedures and explicit land use restrictions. Such restrictions often serve valuable social purposes but might also have significant economic costs.

The extent and restrictiveness of land use regulation crucially shapes the form of cities and the amount of residential and non-residential development. Concerning the residential market, regulation appears to be the single most important factor affecting local housing supply (Gyourko and Molloy, 2015). Simple theoretical models predict that regulation reduces the elasticity of housing supply, raises the equilibrium level of house prices and reduce the equilibrium quantity of housing. Concerning the non-residential market, land use restrictions create entry barriers thus discouraging entrepreneurship and reducing competition (OECD, 2010). However, examining the causal effect of land use regulation is extremely challenging from an empirical point of view as (i) land use policies are multidimensional and difficult to measure and (ii) the extent of regulation is shaped by the interests of homeowners, developers and the local community.

In the present paper we provide quasi-experimental evidence on the effects of a relaxation of regulatory constraints on the housing market and local economic activity. More specifically we exploit the fact that in 1999 the Italian government imposed fiscal rules on municipal governments and in 2001 relaxed them for municipalities below 5,000 inhabitants; interestingly, from 2003 the law also enabled municipalities to finance current expenditures with urbanization revenues. Therefore, we use a difference-in-discontinuity design to address the following research question: do Italian municipalities rely on urbanization revenues they collect from releasing building permits to meet fiscal rules? How do these budgeting choices affect land consumption, real estate market and economic activity?

We find that tighter fiscal rules translates into higher urbanization revenues (about 20%), likely used to finance current expenditures because such effect becomes significant (from a statistical and economic point of view) after 2003. Second, we find that the increase in urbanization revenues - that we interpret as indirect evidence of a relaxation of land use regulation - is (unsurprisingly) accompanied by a similar increase of the construction permits, particularly for the non-residential market (more than 35%). Indeed, non-residential permits are more profitable for municipalities since they are more expensive for applicants and require a lower amount of resources for public services. Moreover, business activities that request for authorization to build also provide additional tax revenues that make the issue of this type of permits more desirable.

Finally, we show that lower land restrictions likely reduced entry barriers thus favouring firm entry and employment growth at the local level (about 2% and 5% respectively), in particular for the commercial activities.

This paper relates to three strands of literature. First, we contribute to the literature that has analyzed the effects of fiscal rules. They are seen as devices to ensure fiscal disciplines though they might also have undesired effects such as limitations to the countercyclical fiscal policy and dubious

[2] These figures are drawn from the World Bank Doing Business reports.

accounting practices to meet the numerical values established by the rules (Milesi-Ferretti, 2004). In this paper we show that fiscal rules might also lead to other unintended effects: the increase of urbanization revenues they collect from releasing building permits to avoid fiscal distress. We call these effects unintended because the fiscal rules were not introduced with the aim of increasing the land consumption.

Second, we contribute to the literature on the effects of a relaxation of urban planning regulation on land use (and values). Most of the existing papers do find a strong negative relationship between regulation and housing supply (and a strong positive relationship between regulation and housing values). However, these results are mostly drawn from cross-sectional evidence and they are subject to endogeneity concerns, thus failing to establish a strong and direct causal effect (Glaeser and Gyourko, 2018)[3]. With only cross-sectional evidence, it is very difficult to disentangle the causes and effects of regulation from local demographic and socioeconomic characteristics that might be correlated with regulation. Moreover, existing estimates that do not take account of reverse causation are likely to be biased. Indeed, land use policies are likely shaped by preferences of the local community. In this paper we implement a difference-in-discontinuities design by combining before/after and discontinuous policy variation and we do find a causal nexus from regulation to building permits and land consumption.

Third, there is an abundant recent literature suggesting that excessive regulation (or a heavy administrative burden on firms) is bad for competition and growth. Starting from Djankov, La Porta, Lopez-de Silanes, and Shleifer, 2002, several studies exploit cross-country heterogeneity in terms of the stringency of entry regulation (e.g. as measured by the World Bank Doing Business indicators) and examine the correlation with measures of economic performance. Other studies exploit variation in firm entry costs using time, region and/or industry-specific variation in entry costs created by policies within particular countries (Branstetter, Lima, Taylor, and Venancio, 2014). More related to our paper, Bertrand and Kramarz, 2002 show that the introduction of zoning permits at the discretion of municipal councils for retail stores in France in the 1970s had a negative impact on employment. We complement this evidence in two directions. First, we get closer to the identification of a causal nexus. Second, we look at firm dynamics and employment growth for the overall economy and for specific subsectors of economic activities.

The remainder of the paper is organized as follows. In section 2.2 we review the three strands of literature. In Section 3 we describe in more details the Italian institutional setting. In section 3.4 and section 3.5 we discuss the data and the empirical strategy, respectively. In section 3.6 we show the results and section 2.7 concludes the paper.

2.2 Review of the literature

2.2.1 Fiscal rules and their effects

Fiscal rules are often seen as devices to ensure fiscal discipline. Indeed, as far as subnational governments are concerned, a number of factors create adverse incentives to overspend, under-tax, and/or borrow excessively - all root causes of fiscal imbalances. A first factor is the common pool problem. In an integrated economy, the costs of fiscal indiscipline by one or more subnational

[3] One notable exception is represented by Libecap and Lueck, 2011. They use a natural experiment in nineteenth-century Ohio to analyze the economic effects of two dominant land demarcation regimes. The first is characterized by plot shapes, alignment, and sizes defined individually; the second is a centralized grid of uniform square plots that does not vary with topography. They find that the latter increase the land values.

governments are likely to spill over to the others. This, in turn, might generate an incentive for local governments to excessively increase local expenditure (Rodden, Rodden, Eskeland, and Litvack, 2003).The second factor is related to the fact that whenever a local administration defaults, the national level generally intervenes with transfers of more resources to the local level. In such cases, the incentives for fiscal responsibility are weakened, as the costs of indiscipline are transferred to the national budget, thus generating a problem of moral hazard[4].

The imposition of numerical rules on budget deficits or public spending is viewed as one possible way to reduce or eliminate these distortions. While some studies find that fiscal rules do indeed result in lower budget imbalances, others stress the reasons why they might not be effective in restraining fiscal policy[5].

Using the terminology of Milesi-Ferretti, 2004, fiscal rules might lead local governments to produce also bad or ugly outcomes, as opposed to good outcomes (i.e. the ones the rules are designed for). Namely, rules can lead to bad outcomes, as they hinder the use of counter cyclical fiscal policy and hamper the operation of automatic stabilizers, thus exacerbating macroeconomic fluctuations. They can also lead ugly outcomes such as window dressing and creative accounting, i.e. the use of dubious accounting practices to meet the numerical rules that however might have no overall effect on fiscal imbalances (and reduce the degree of transparency in the government budget).[6]

2.2.2 Regulation and housing market

Local housing supply crucially affects the shape of urban development and the evolution of housing values. Among its determinants, the existing literature has mostly focused on the role of land use regulations, though it varies widely in quality of research method and strength of result. Caldera and Johansson, 2013 show significant differences in housing supply elasticities across OECD countries[7]. They also show that cumbersome land use and planning regulations are associated with a less responsive housing supply, though they also acknowledge that this link is hard to be established empirically through their data.

What is true across countries may also be true across cities. Indeed, Green, Malpezzi, and Mayo, 2005 adopt a within country perspective and show significant housing supply heterogeneity across US cities. They also show that heavily regulated cities exhibit low elasticities and that, in spite of the crudeness of the index of regulatory stringency they use, it helps predict housing supply elasticity in a statistically significant and economically important manner[8].

Glaeser and Ward, 2009 and Kok, Monkkonen, and Quigley, 2014 move the analysis within the metropolitan area. Glaeser and Ward, 2009 use extremely detailed data for municipalities

[4]Moreover, even an explicit central government's commitment to a no bailout policy may lack credibility, if local governments are responsible for the provision of essential public goods and services, and a disruption in such provision is likely to have strong political and social consequences.

[5]See the reviews by Alesina and Perotti, 1996 and Wyplosz, 2012. As far as Italian municipalities are concerned, Grembi, Nannicini, and Troiano, 2016 show that relaxing fiscal rules increases deficits and lowers taxes.

[6]Milesi-Ferretti, 2004 defines creative accounting as a *measure implying the improvement of the fiscal balance (without) an improvement in the intertemporal budgetary position of the government sector at large.* As far as local governments are concerned, these measures may entail the use of out-of-budget debts that allows municipalities to shadow some of their expenditures and/or refer to the possibility that municipalities have not cashed yet some of the revenues they should use to cover current expenditures.

[7]Housing supply tends to be relatively flexible in North America and some Nordic countries, while it is more rigid in continental European countries (e.g. Switzerland, the Netherlands, Austria and Italy).

[8]Level of land-use regulation is measured as (unweighted) sum of seven variables describing the regulatory environment for 56 MSAs. These comprised answers to survey questions regarding, for example, the approval time (zoning and subdivision) for different kinds of residential projects, the percentage of zoning changes approved, and the like.

in the Boston area and show that minimum lot size and other land use controls are associated with reductions in new construction activity and to higher prices. Kok, Monkkonen, and Quigley, 2014 use survey data on land use regulations in the jurisdictions in the San Francisco Bay Area, they investigate the linkage between these regulations and land and house prices. They find that the restrictiveness of the legal and regulatory environment, measured by the number of approvals needed to obtain permits or zoning changes, is strongly and positively correlated with land and house prices.

However, the existing evidence has arguably not fully established a causal link. First, there is a measurement issues as zoning and other land-use policies are multidimensional and difficult to measure. Second, most of previous studies refer to cross-section analysis, thus making difficult to disentangle the causes and effects of regulation from local demographic and socioeconomic characteristics that might be correlated with regulation. Third, there might be a reverse causation issues as regulation reflect the preferences and interests of local voters.

As far as measurement is concerned, regulation restrictiveness measures have generally been weak and indirect, with the standard approach being the use of a summary index of regulatory indicators. Moreover, regulatory surveys are administered sparsely and infrequently. With some simplification we can distinguish three main approach in the measurement of regulation restrictiveness. First, Glaeser and Ward, 2009 measure the difference between houses' market value and the production cost of an extra floor in Manhattan condominiums. They interpret this gap as evidence for government regulation. However, like any residual factor, it is hard to establish whether the difference can be entirely attributable to regulation. Indeed, the gap between prices and costs can arise for other reasons such as, for example, some degree of monopoly power of the construction firms or the existence of physical and/or technological barriers to building. Second, Glaeser and Ward, 2009 and Kok, Monkkonen, and Quigley, 2014 use extremely detailed data that are, however, city-specific (they refer to localities in the Boston area and in the San Francisco bay area, respectively). Third, a more shallow but wider approach is the one used by Gyourko, Saiz, and Summers, 2008 who collected survey information for 2,611 U.S. communities. The data were then used to create a summary measure of the stringency of the local regulatory environment in each community - the Wharton Residential Land Use Regulation Index. This index has been widely used in other papers on this topic.

As far as endogeneity is concerned a statistical association between regulation and house prices might capture a causal effect the former on the latter but might also be the results of spurious correlation (whenever we are not able to control for omitted variable correlated with both) or of a reverse causation (if residents of communities with higher housing values demand more restrictive regulations). Indeed, the dominant political economics view suggests that local land use regulations correspond to the wishes of a majority of local voters. Homeowners, in particular, have stronger incentives to protect their housing investments where land values are high initially. The homevoter hypothesis (Fischel, 2001) implies a reverse causal relationship from initially high land values to increased regulations[9].

Available empirical evidence is strongly suggestive that homeowners (and conservationists) are influential in regulating land use locally (Dehring, Depken, and Ward, 2008). Saiz, 2010 find that antigrowth local land policies are more likely to arise in growing, land-constrained metropolitan areas and in cities where preexisting land values were high and worth protecting. Some papers

[9]See also Hilber and Robert-Nicoud, 2013 and Ortalo-Magné and Prat, 2014 for theoretical models on the political economy of land regulation.

deal with endogeneity of regulation. Ihlanfeldt et al., 2007 uses the lag value of the community characteristics at the time the land use plan was approved by the State as instruments for the restrictiveness of regulation in Florida cities. Saiz, 2010 use local public expenditure share in protective inspection and the nontraditional Christian share in 1970 as instruments for the current regulation index. However, as pointed out by Kok, Monkkonen, and Quigley, 2014, any historical measure of demographics or urban form is likely to be correlated with contemporary measures, and thus will not satisfy the exclusion restriction.

2.2.3 Regulation and economic activity

Land use restrictions can serve legitimate purposes but they might also represent entry barriers and raise costs for entrepreneurs initiatives and firms dynamics (OECD, 2010). They affect the development of economic activity through zoning, planning and location-specific rules and approval processes.

A substantial literature has developed on the relationship between entry barriers and economic performance. Namely, we can distinguish between two strands of empirical work providing evidence on the issue. The first group of studies is substantially based on cross-country comparison, examining the correlation between the stringency of entry regulation with various measures of economic performance. For example, Djankov, La Porta, Lopez-de Silanes, and Shleifer, 2002 show that onerous entry regulation is associated with higher corruption and a higher concentration of activity in the informal sector. Djankov, McLiesh, and Ramalho, 2006 provide cross-section evidence of a positive relationship between GDP growth and regulation across 135 countries using the World Bank Doing Business Indicator, a composite measure that captures relevant dimensions of establishing and running business according to the number of required procedures, their costs and length. Other papers have used similar empirical strategies to evaluate the impact of business entry regulation on job creation and entry of new firms in the market (Ciccone and Papaioannou, 2007; Klapper, Laeven, and Rajan, 2006). However, cross-country evidence is hardly interpretable in causal terms as countries with heavy entry regulation also are less likely to have good institutions along a number of dimensions.

The second group of studies exploits within-country variation in policies across time, region and/or industries. Bertrand and Kramarz, 2002 show that zoning regulation introduced in France in the early 1970s to restrain the development of large retail stores has had a negative impact on employment in that sector. As far as the Italian retail trade sector is concerned, Schivardi and Viviano, 2011 exploits a reform in the late 1990s that delegated the regulation of entry of large stores to the regional governments. They use the local variation in regulation to determine the effects of entry barriers on sectoral performance. Branstetter, Lima, Taylor, and Venancio, 2014 evaluate the consequences of a regulatory reform in Portugal, which substantially reduced the cost of firm entry. They find that the reform resulted in increased firm formation and employment, but mostly among marginal firms that would have been most readily deterred by existing heavy entry regulations. Amici, Giacomelli, Manaresi, and Tonello, 2016 find that a simplification in the bureaucratic regulation for doing business in Italy is associated to an increase in the firm entry rate.

2.3 Institutional setting

2.3.1 The Domestic Stability Pact

The fiscal discipline imposed by the Maastricht Treaty and later by the Stability and Growth Pact forced European countries to engage in budget consolidation. As a consequence, many European countries have also introduced fiscal rules to limit the expenditures of local administrations. In Italy the law 448/1998 prescribes the implementation of a Domestic Stability Pact (DSP) which limits the budget deficit of local governments from the year 1999 onwards. Starting from 2001, small municipalities (i.e. those with a population below 5,000 residents) were excluded from the DSP[10]. The rationale for the exemption of those municipalities was to avoid burdening very small towns with onerous requirements.

Italian municipalities basically handle the direct provision of local services. They are responsible, for instance, for the provision of creches, care of the elderly, welfare programs, as well as street maintenance and public transportation, among the others [11].

Because of the DSP municipalities (as well as Regions and Provinces) need to meet a series of caps and constraints on their expenditures. Rules of the DSP for our reference period are reported in Table 1. The penalties put in place for not complying with the DSP (though they slightly changed year-by-year) included a cut in the annual transfers from the central government, limitation on new hires, and a cut on reimbursement and other bonuses, among other things. Patrizii, Rapallini, and Zito, 2006 show that the large majority of local governments met the DSP requirements.

2.3.2 Land use regulation and legal framework for building activities

Urban planning is a political and technical process aiming at design and regulate the use and development of land and urban environment. Within these functions land use regulation plays a crucial role, including several stages for the construction process: obtaining subdivision permits; re-zoning existing parcels, from agricultural use and subdivide it into lots appropriate for individual houses (development permits); filing environmental impact statements; applying for building permits prior to constructing houses on the finished lots; comply with housing and building codes. This process adds explicit financial and time costs (and also some uncertainty about the construction activity).

There is a consensus in the literature that easing the release of building permits, shortening delays and removing cumbersome procedures would directly reduce the costs of construction and thus increasing the amount of new buildings (Gyourko and Molloy, 2015). Mayer and Somerville, 2000 show that development or impact fees have relatively little impact on supply, but regulations that lengthen or constrain new development have larger and more significant effects.

Then regulation of building activities seems to be one of the primary task within urban planning: indeed, a general framework has been set in Italy since the 1940s providing a high degree of autonomy to the local level concerning the definition of specific rules. In a historical perspective the main change to building regulation, that still affects how it works nowadays, has been introduced by

[10] Resident population is calculated as that one of two years before; e.g., for 2003, the resident population at the end of 2001 applied.

[11] More specifically, the actual functions of the municipalities are the following: general administration; justice; local police; public education (up to kindergarten, primary school and part of secondary school); culture; sport; tourism; local public transportation; urban development; social sector; economic development; productive local services.

the law 765/1967 (*Legge ponte*): it charges the release of building licenses with an impact fee upon the builder. This fee is imposed by municipalities and is due by new building projects to finance the costs of providing infrastructures and public services in the newly developed areas, reducing the burden on local authorities. These so-called urbanization revenues are devoted both to primary infrastructures (e.g. roads, parks, sewer, water treatment, utilities, network infrastructures) and to secondary ones (e.g. schools, social centres, public gyms, place of worship).

This setting has been further strengthened with the law 10/1977 (*Legge Bucalossi*) that fills some gaps in land use and building regulation[12]. Urbanization revenues are due according to the foreseen urbanization investment and construction costs that are also related to the dimension of the building, its economic purpose and the size of the city. Moreover, these revenues and those collected as urban sanctions entered the balance sheet as a restricted item that cannot be used except for primary and secondary type infrastructures, restructuring of the historical city centers, expropriation of land for public social programs and the ordinary maintenance of municipalities building stock. To prevent misallocation of urbanization revenues under the broad category of ordinary maintenance that by its nature allows for the inclusion of a wide range of expenses, the legislator has stated with the legislative decree 318/1986 and law 488/1986 that this item must not exceed the 30% of the total amount.

In the following years, the ongoing process of fiscal decentralisation and the subsequent reduction of central transfers has lead municipalities to rely more on autonomous revenues. Then, to safeguard the local finance sustainability, the legislator has relaxed the limit on the use of urbanization revenues with the law 449/1997, removing the restriction on the amount available for maintenance of the public building stock and also allowing for both ordinary and extraordinary types of interventions. Three years later the reorganisation of the functions of local authorities stated by the legislative decree 267/2000 (*Testo Unico sull'ordinamento degli enti locali*, TUEL), has marked a further step to loosen budget regulation on this item. It has allowed municipalities not experiencing a fiscal distress to temporarily borrow urbanization revenues to fund current expenses. They only have the legal obligation to reallocate the borrowed amount to the original budget item by the end of the fiscal year. Then the legal constraint on the use of urbanization revenues has been totally removed with the new Act on Construction Building DPR 380/2001 (*Testo unico sull'edilizia*, TUE) binding since the mid of 2003 [13].

The release of building permits still requires the payment of a fee according to the urbanization investments and construction costs, keeping virtually unchanged the fee's nature and aims but substantially distorting them in terms of budget allocation. Indeed, although the unplanned and once-off nature of these capital revenues, they can also be used now to fill current expenses and then contributing to the budget equilibrium[14].

After a public debate on the consequences of the new framework on the local finance sustainability and urban environment, the legislator has mitigated the availability of these revenues with the law 311/2004 that reduces the allowed share of urbanization revenues contributing to current

[12]See Court of Auditors, Lombardy Region, Legal advice n.1/2004

[13]TUE should have been enforced by 2002/01/01, but the overlap of several measures with those included within the law 433/2001 (the so-called *Lunardi* or *Objective Law*), being expression of the new government at that time, imposed a lengthy process of legal harmonization that requires to postpone TUEs enforcement by the law 411/2001, the legislative decree 122/2002 and the law 185/2002 that set the final date of 2003/06/30.

[14]The debate following the approval of DPR 380/2001 has required the release of official interpretations provided with Note n.108321 of 2003/10/07 and Note n.39656 of 2004/04/07 by the Ministry of Finance. In the same years additional interpretations has been provided by some regional Courts of Auditors to clarify the proper budget allocation, see Piedmont region, Legal advice n.2/2005, Tuscany region, Legal advice n.1/2005 and Lombardy region, Legal advice n.1/2004.

expenditures to 75% in 2005 and 50% in 2006[15]. The extensive and relaxed use of urbanization fees concerning the initial motivation they were introduced for has been going on, with frequent changes, until 2017 when the law 232/2016 has excluded the possibility to use them as a tool for budget balancing, restoring their original function.

2.4 Data

According to the particular design adopted, we select a sample of municipalities between 3,000 and 7,000 inhabitants, excluding cities from regions with special autonomy, since they were partially exempt from the reform. The resulting sample is approximately composed of 1,300 municipalities, 700 that are treated (since they have more than 5,000 inhabitants) and 600 belonging to the control group (with population less than 5,000)[16]. The reference population is its legal value in 2001, corresponding to the official number of resident people in 1999 as provided by the General Register Offices.

We consider both the urbanization revenues and the floor area of building permits as the two outcomes of the first part of our analysis, since they describe both the budget and urban planning sides of this setting.

Concerning the analysis at the budget level, we use municipal financial reports that are administrative data provided by the Ministry of Interior for the universe of all Italian cities, available since 1998. They contain detailed information about all the items that municipalities have to declared according to the annual budget law. We focus on the category that jointly measures revenues coming from both urbanization fees and urban sanctions, from now on urbanization revenues, as included in the capital revenues section of the balance sheets (nominal per capita values on accrual basis). While revenues that arise as urban sanctions represent an additional resource for making budget-balancing through relaxing the urban planning regulations, they are not strictly related to the release of building permits. The average value of urbanization revenues registered before the reform for both the treated and untreated cities is 39 euros per inhabitants, accounting for about 7% of total current revenues and 16% of total capital revenues.

The analysis of building permits is based on administrative data provided by the Italian National Institute of Statistics (ISTAT) for the universe of all Italian cities since 1997. Every month all municipalities must communicate detailed information concerning the release of building permits. They mainly include the total floor area (square meters), the volume (cubic meters) allowed to be built, the type of allowed interventions (ex-novo or extension of pre-existing buildings), the nature of the building (residential or non-residential) and its economic destinations. We aggregate these data into annual cumulative measures stated in per capita terms, the main variable of interest is floor area (total, residential, non residential). The groups of treated and untreated cities do not present statistically significant differences in the pre-reform mean values (i.e. 1999-2000) of floor area released with permits which equals 1.7 square meter per inhabitant (Table 3.1).

Our outcomes are the average growth rates of urbanization revenues and building permits, before and after the reform, defined as the difference between the final and initial values over their

[15]For 2007 the law 299/2006 has confirmed the set of limitations adopted in the previous year, introducing an expenditure cap on the ordinary maintenance of public building stock that cannot be greater than (an additional) 25% of urbanization revenues (50% + 25%). Then the law 244/2007 has re-stated the 50% cap on the amount allocated to current expenditures for the period 2008-2010, while broadening the additional 25% committed to ordinary maintenance, including green area and roads.

[16]The exact dimension of the sample is defined according to an optimal data-driven rule for each outcome and model specification. See empirical strategy (section 3.5).

means: while for small variations it provides values similar to those of a standard growth rate, it has the advantage of being symmetric and bounded between [-2;+2]. Moreover by averaging size at the denominator over two periods, it avoids biases due to regression-to-the-mean that may emerge when dealing with repeated measurements where relatively high (low) values are likely to be followed by observations with less extreme ones, closer to the true mean: a situation that is typical of the issue of building permits when small municipalities are considered.

We use data related to time-invariant features of municipalities (geographic location, area size, slope, coastal city) made available by ISTAT (Table 3.1). We also include structural factors related to the housing market as provided by 2001 Census which mainly describe socio-economic dimensions that cannot be affected by the reform in the short run. Concerning housing demand determinants our dataset includes the share of people older than 65 years and the share of foreign people over all residents at the municipal level and the population density. About the housing supply factors, we include the number of houses per capita and the share of rented and empty houses at the city level. For all these factors which are relevant to our outcomes, we provide a set of t-tests of means of the two groups of treated and untreated cities to test the validity of our design (Table 3.1). The balance of these covariates around the threshold is remarkable: indeed, the differences are mostly statistically not significant, while for few cases they are statistically relevant but negligible regarding the magnitude or economic interpretation.

In the second part of our analysis we consider other outcomes that may capture side-effects of the main mechanism of releasing permits as reaction to increasing fiscal rules that we have described above. In particular we analyze whether easing and speed-up the release of building permits for non residential constructions may have positively affected the set-up or expansion of enterprises, measured by the number of firms' branches and individual workers at municipal level.

We use census data from the Business Register ASIA that has been set-up in 1996 and is based on a process of integration of administrative and statistical sources. It covers all enterprises carrying on economic activities in the fields of industry, trade and services, providing identification (name and address) and stratification (e.g. main economic activity, size, legal form, date of creation and date of cessation, turnover) variables. We use all the cross section waves available within the relevant period, i.e. those collected in 1996, 2001 and 2004. We have aggregated ATECO 1991 - Nace rev.1 divisions classification into 10 high-level categories of macro economic sector defined by ISIC rev.4 following ISTAT(2009)[17]. Then we furtherly aggregate into 3 main categories: private sector (G-H-I) that includes sections G-H-I, i.e. wholesale trade, hotel, restaurant and catering services, transport and storage; private sector (all other sections) that includes B-C-D-E (fishing, mining manifacturing, energy production), F (construction), J (financial and insurance sector), K (real estate, renting and leasing, research and development sector, business services); and public sector that includes M-N (education, health and other social services), O-P-Q (Others).

2.5 Empirical strategy

The identification of the average treatment effect of the DSP on the release of building permits and urbanization revenues is the first step of our analysis; then we analyse its downstream effects on economic performance. We exploit the fact that targeted municipalities has changed in the reference period. In particular, the DPS first applied to every city, but from 2001 smaller cities

[17] ISTAT(2009) Classificazione delle attività economiche Ateco 2007 derivata dalla Nace Rev.2, Metodi e Norme n.40

(those below the 5,000 citizens thresholds) have been exempted [18].

We combine two sources of variation, before/after 2001 and just below/above 5,000 inhabitants, and implement a difference-in-discontinuities design, taking the difference between the pre-treatment and the post-treatment discontinuity at 5,000 inhabitants (Grembi, Nannicini, and Troiano, 2016).

The combination of a difference-in-difference approach with a regression discontinuity design requires some assumptions to guarantee identification (Lee and Lemieux, 2010). First, as in a general RDD framework, all the potential outcomes have to be continuous around the threshold which is a non-testable assumption that we will show to be plausible for this setting (Hahn, Todd, and Van der Klaauw, 2001). Second, according to the DID setting, the exploitation of time variation requires that, before the reform, municipalities just above or below the population limit are (locally) on a parallel trend: a condition that we will positively test. Third, to generalise the effect to all the cities in the neighbourhood of the threshold (and not only those being treated), we must also assume that the treatment effect is locally homogeneous. In other words, we expect the absence of interaction between the treatment and any other pre-existing confounding policy that can make the impact of the reform different on the two sides of the threshold. We implement a falsification test in the next section to show this condition is not violated.

The causal effect of the DSP can be estimated with the following cross-section model by local polynomial regression, i.e. by fitting linear regressions within a bandwidth on either side of the threshold:

$$
\begin{aligned}
&\Delta Y_i = \alpha + \beta_1 D + \beta_2 f(pop_i - c) + \beta_3[D \times f(pop_i - c)] + \beta_4 X_i + \epsilon_i \\
&D = 1[\text{municipality size} \geq 5,000 \text{ inhabitants in 2001}] \\
&\text{pop} = \text{legal population in 2001} \\
&\text{c} = \text{population thereshold} = 5,000
\end{aligned}
$$

Where ΔY is the average growth rate of the outcome before and after the reform, D is a dummy equals to one if the legal population of the municipality is greater than 5,000 inhabitants in the years after the reform and zero otherwise, the matrix X includes a set of housing demand and supply factors, geographic location and budget indexes at the city level. The coefficient β_1 represents the estimate of the impact of the DSP reform.

This specification includes a polynomial of the first degree in the normalised population (the running variable) and its interactions with the treatment dummy D. This non-parametric and local low-order polynomial approximation in a neighborhood of the cutoff features a potential misspecification but is more robust and less sensitive to overfitting and boundary issues (Cattaneo, Idrobo, and Titiunik, 2018). Indeed, in general, the local linear estimator delivers a good trade-off between accuracy, variability and stability of the treatment effect in the RD setting.

To implement the local polynomial point estimator of the RD treatment effect we employ a data-driven choice of the optimal bandwidth according to a bias-variance trade-off, the so-called minimum square error (MSE) approach. This choice imposes to consider how the existence of a

[18] It is worth noting that the requirements that need to be fulfilled by local authorities are (unilaterally) established by the central government through the National Budget Law and municipalities have no voice in writing these rules. Therefore fiscal rules at the local level can be correctly regarded as exogenous.

misspecification error in the approximation can affect inference procedures (Cattaneo, Idrobo, and Titiunik, 2018). We then report three different specifications: (i) *Conventional* estimates with MSE bandwidths follow a parametric weighted OLS estimation whose inference validity is questioned; (ii) *Bias-corrected* estimates allow for misspecification correction and deliver valid inferences when MSE optimal bandwidth are used; (iii) *Robust* estimates adopt the same misspecification correction as for the *Bias-corrected* estimates but redefine a new asymptotic variance that captures the contribution of the bias correction procedure to the variability of the estimator. Since OLS point estimates are optimal when MSE bandwidths are used (and so far widely used) but confidence interval are not reliable, while bias-corrected and robust approaches provide sub-optimal point estimates with valid confidence intervals, we keep all of them in our tables of estimates. The reported standard errors account for possible error correlations at the province layer since relevant factors characterizing the housing market exist at that level (alternative specifications are provided in the Appendix).

2.6 Results

2.6.1 Effects on urbanization revenues and construction permits

The graphical evidence of discontinuities in the distribution of the considered outcomes is given by their global approximation and local behaviour in Figure 2.1. For all per capita outcomes we draw a 4th-order polynomial fit and local sample means of the average growth rate for the period before and after the reform. We analyze outcomes variations over different time windows to highlight the importance of the increasing relaxation of legal constraints on the use of urbanization revenues (as depicted in the subsection 2.3.2 and summarized with Table 2.1) on the behavior of treated municipalities. In particular, we compare the average value of the first two years preceding the reform (1998-1999), the baseline, with the average value of the first two years later (2001-2002), the biennial mean after two years (2003-2004) and the overall mean over the four years after the reform (2001-2004) (Timeline 1).

Within the first two years after the DSP reform (2001-2002) there is no clear evidence of discontinuity for any outcomes (Figure 2.1, 1st column): this visual analysis is also confirmed by the corresponding estimates of the model (Table 2.3). Nonetheless, a clear discontinuity in the variation of urbanization revenues after two years since the policy change (2003-2004) is visible in Figure 2.1 (mid-column, 1st row). Similarly, in the same period, a jump is also evident for total building permits (2nd row), which is driven by the non-residential (NR) channel (3rd row). The overall variation over the first four years following the reform (third column) is then driven by the variation registered in 2003-2004.

The impact of the reform may have been delayed by two factors. First, while the possibility to use urbanization revenues has been extended since 1997 (Table 2.1), the effective entering into law of the Act on Construction Building (TUE) in 2003 has further (and critically) relaxed the framework. Indeed, TUE has allowed to use these revenues to fund current expenditure and then strengthening the perception that municipalities could have systematically resorted to them as a tool for budget balancing only two years after the reform. Second, easing the issue of building permits by changes to the local housing and building code or the local strategic plan may be lengthy since it has to be carefully discussed and assessed, having to comply with the normative framework at province and regional levels, and it may also require the Council's approval. Moreover, any modification to administrative processes may require time to be practically implemented and to get people aware of the change.

Therefore the estimates of the diff-in-disc model focus on the outcomes variation over the period 1999-2000 vs 2003-2004 (Table 2.4). The effect of not relaxing the DSP is an increase of 23.4% of collected urbanization revenues[19], which corresponds to an increase of about 23.6% of the issue of building permits which is driven by a +36.6% for non residential constructions, while the variation through the residential channel is not significant (see Figure 2.2 for a visual representation of the estimated impact).

Several aspects may explain the importance of the non-residential channel. Firstly, the release of permits for non-residential buildings is more costly for applicants, and then it guarantees higher revenues for municipalities. Also, local governments may bear a lower level of investment for public infrastructures and social services when dealing with commercial buildings: indeed, costs for schools, kindergartens, health and care for elderly, may increase with the number of new residents. Furthermore, the higher the number of business activities the higher the tax revenues for municipalities. An additional motivation to favour the non-residential type of permits may be due to the homeowners' preference for a limited issuing of permits that can preserve the value of their properties.

The standard errors are clustered at the province level (NUTS3 in the Eurostat definition) which is the smallest administrative unit after the municipality, since the homogeneity of the housing market rules, policies and factors is visible at that level[20].

These results are quite robust to the adoption of different polynomial degree of the running variable (Table 2.5): our preferred specification is linear since it provides a good trade-off between bias and variability of the estimates of the causal parameter, while we provide a polynomial of zero and second degree to avoid poor approximation at the boundary point and unreasonable weighting scheme typical of higher-degrees approximations (Gelman and Imbens, 2018).

These estimates remain stable after the inclusion of predetermined covariates (Table 2.6) for geographic location and budget indexes, housing supply and demand factors at the municipal level (whose descriptives are provided in Table 3.1). The magnitude of the estimates is slightly affected while the precision increases whenever the MSE-optimal bandwidths adjusted for covariates imply a larger sample. The treatment effects are always statistically significant for the bias-corrected and robust specifications.

We provide a set of validity and falsification tests to check whether the assumptions required to identify the causal effect are satisfied. First, we consider the possibility of manipulative sorting to affect the assignment mechanism which is based on population. The density tests on the running variable over several years do not show any discontinuity around the threshold (Figure 2.3). The possibility of manipulation seems to be unrealistic for two reasons. Since the article 156 of the legislative decree 267/2000 (TUEL) defines the legal population at year t as the number of city residents at year $(t-2)$, mayors should have been able to diminish the level of population or falsify their declarations to the National Statistical Office one year before the enactment of the reform. Moreover, given that the wage policy for the mayors and executive committees is based on population thresholds, any attempt to voluntarily reduce the number of residents would have ended up with a salary reduction.

[19]These estimates are re-estimated using the standard log-growth rate: results mainly confirm the sign and magnitude (Table 2.15).

[20]We also provide estimates clustering at other levels (Table 2.16): (i) at the running variable (i.e. population) and (ii) at the level of the Local Labor Market (LLM), that is a cluster of municipalities, defined on the basis of the commuting patterns and that represents a self-contained labor market. Both the two alternative ways of clustering deliver comparable standard errors.

Then we examine the presence of systematic differences in predetermined covariates just above or below the threshold that may affect the non-testable assumption of continuity of the potential outcomes (Table 2.7). Using pre-treatment characteristics as outcomes of the model, we are not able to estimate significant treatment effects for almost all of them. Since the share of municipalities located in the southern regions is slightly higher beyond the threshold, we re-estimate the model limiting the sample to the cities in the north and centre of Italy (since the sample restricted to the southern regions would have been too small to provide reliable estimates). The estimates for the two main outcomes, i.e. variations of urbanization revenues and released building permits, deliver treatment effects that are significant and comparable to those obtained using the original sample which includes southern cities (Table 2.8). We can then exclude issue for the identification coming from geographic factors.

Our estimates are weakly sensitive to the chosen bandwidth, being positive and significant for a sufficient range of the running variable values around the threshold (Figure 2.4). They tend to become not significant for smaller bandwidths because fewer observations, while reducing misspecification error of the local approximation, tend to increase the variance of the estimated coefficients.

As anticipated in the previous section, the validity of our identification strategy requires, in particular, the absence of interaction between the DSP reform and other confounding policy at the threshold. The actual introduction of the DSP in 1999 for all municipalities represents a significant test to determine whether cities just above or below the population limit react differently to the introduction of fiscal rules. We compare the outcome growth rate of the considered outcomes between 1997-1998 vs 1999-2000 (Timeline 2): estimates of the model reject the violation of the homogeneity assumption, all of them being strongly not significant (Table 2.9).

Another crucial assumption for the diff-in-diff approach of our identification strategy, namely the existence of a parallel trend of both outcomes before the reform, results to hold: indeed, all the year-by-year variations of the considered variables are not significant[21] (Figure 2.5).

Since the RD identification relies on the continuity of the regression function for treated and control at the threshold, we test for the existence of remarkable discontinuities away from it to exclude the possibility that our estimates are significant only by chance. While this test is neither necessary nor sufficient being the continuity assumption untestable, it can support the adoption of this specific design. We perform several estimations of the model with artificial (placebo) thresholds (Figure 2.6) allowing for contamination (i.e. we do not exclude actual-treated from the control group when the cut-off is beyond the actual one and analogously for cut-off below the actual one). The scatterplot of resulting causal parameters shows an expected inverse U-shape: the most substantial and most significant estimated ATE is that at the correct threshold, furtherly strengthening our identification strategy.

2.6.2 Downstream effects on economic performance

The identification of the reform's unintended impact on business dynamics represents the second step of this analysis. Based on the discussion above, the DSP modification has caused an increase of the released building permits, easing a relevant aspect of the regulation governing economic activities at local level and making a more business-friendly environment for existing firms and start-ups.

[21] Due to limitations of data, the graph for urbanization revenues is available only for 1999 and 2000.

We measure the impact of the DSP reform on the variation of firms (branches) and workers before and after the reform estimating the diff-in-disc model we already used in the subsection 2.6.1, performing the usual set of robustness checks. According to the available waves of survey released by the Business Register ASIA we can consider the outcomes' variations over two possible periods, i.e. 1996-2001 and 2001-2004. While the two periods are slightly different to that we consider in the previous section, where we deal with flow measures of permits and revenues per year, the comparison is still suitable since the numbers of firms and workers are stock measures representing quantities existing at that point in time, which may have accumulated in the past.

The impact of relaxing the issue of building permits for non-residential purposes has been positive for local businesses in term of employment level over both the two periods, while firms demography has been positively affected by this change in 2001-2004 only, as suggested by the visual representation (Figure 2.7). According to our diff-in-disc estimates (Table 2.10), the number of workers has systematically increased more for treated municipalities (+4.6%) in 2001-2004 and, analogously, the number of firms' branches has raised by +2.4% in the same period (see Figure 2.8 for a visual representation of the estimated impact).

The increasing level of employment that we also estimate for the period 1996-2001, even though at a lower rate (+3.4%) than that in the following period, does not match with a similar variation of firms' growth. Expanding firms may start hiring additional workforce before the actual establishment of new units: technical procedures required for the set-up and the construction of new buildings may be time-demanding and contribute to restrain business to become operative. Moreover, as seen in the previous section, relaxing the issue of building permits has been stronger when the possibility to fund current expenditure with urbanization revenues has been extended, i.e. from 2003. For this reason, from now on, we focus on the period over 2001-2004 as the more relevant one.

As for results presented before, we check the sensitivity of our estimates to parsimonious specifications of the polynomial degree of the running variable (i.e. zero and second degrees, to avoid poor inference related to higher order degrees as explained in section 3.5). The estimated ATE for both the two outcomes is particularly stable and almost always significant for all specifications (Table 2.11). In addition, we provide covariates-adjusted estimates through the additive inclusion of factors related to geographic location and budget indexes, housing supply and demand determinants: while the magnitude of the estimates is almost not affected, the precision increases whenever the optimal data-driven bandwidth which accounts for the presence of additional regressors defines a larger sample [22] (Table 2.12).

Evidence of heterogeneous effects within specif sectors of the economy is also provided. The causal impact of the DSP on employment and firms can be decomposed according to 10 macro economic sectors we defined in section 3.4. The macro sector G-H-I which is composed by wholesale trade, hotel, restaurant and catering services, transport and storage is the only one to be affected by a quite significant and positive increase over the reference period. Considering this macro-sector the DSP reform has indirectly determined a positive variation around 3.2% for firms (Table 2.13, column 2) and 5.5% for workers (Table 2.14, column 2).

[22] These results are quite robust to different definition of the outcome (i.e. the standard log-growth rate, see Table 2.15) and different ways of clustering (i.e. at the forcing variable and at the local labor market levels, see Table 2.16).

2.7 Conclusions

The use of fiscal rules as devices to ensure fiscal discipline may also have unintended effects. We provide quasi-experimental evidence that relaxing fiscal constraints has forced non-targeted municipalities to rely on urbanization revenues they collect from issuing building permits to comply with tighten budget rules.

Constrained cities have increased the total amount of urbanization fees collected in the post-reform period (about +20%), having a similar increase in the corresponding release of authorisations to build, in particular for non-residential user (more than +35%). Although builders are charged with construction and impact fees to make them share with the local administration the burden of providing infrastructures to the newly-built area, from 2003 municipalities have been enabled to finance even current expenditures with those revenues: a change that has provided further incentive to use urbanization revenues for dubious accounting practices.

The relaxation of urban planning regulation on land use has affected business dynamics, making easier the expansion or establishment of new firms with the corresponding growth of employment: the number of firms' branches and of workers in treated municipalities has increased around 2% and 5% respectively, with a stronger effect for the macro-sector including wholesale trade, hotel, restaurant and catering services, transport and storage.

2.8 Tables and figures

Table 2.1: Institutional framework: timeline

DSP		Share of urbanization revenues allowed to be used for (%)		
year	binding for	ordinary maintenance	extraordinary maintenance	current outlays
1997	none	100	100	0
1998	none	100	100	0
1999	ALL	100	100	0
2000	ALL	100	100	0
2001	$\geq 5,000$	100	100	0
2002	$\geq 5,000$	100	100	0
2003	$\geq 5,000$	100	100	100
2004	$\geq 5,000$	100	100	100

Main sources: Annual budget laws, Law 449/1997, D.P.R. 380/2001 (TUE)

Table 2.2: Descriptives and two-sample t-tests

	(3,500-5)	(5-6,500)	Δ	T-test (p)	Obs
Main outcomes					
building permits: floor area (m^2/inhab)	1.71	1.69	0.019	0.841	949
building permits: floor area, res (m^2/inhab)	0.87	0.90	-0.027	0.575	935
building permits: floor area, non res (m^2/inhab)	1.01	0.93	0.083	0.317	874
urban revenues (€/inhab)	40.33	38.73	1.604	0.500	949
firms (number of branches/inhab, y=2001)	0.07	0.07	-0.003*	0.052	949
workers (number/inhab, y=2001)	0.26	0.27	-0.010	0.331	949
Budget					
current revenues (€/inhab)	602.01	598.51	3.504	0.824	948
current spending (€/inhab)	557.38	556.32	1.060	0.937	948
capital revenues (€/inhab)	395.34	367.99	27.344	0.501	948
capital spending (€/inhab)	345.80	295.39	50.404	0.214	948
urban revenues/current revenues (%)	0.07	0.07	0.001	0.823	949
urban revenues/capital revenues (%)	0.16	0.17	-0.009	0.327	949
Budget indexes					
personnel/current spending (%)	0.19	0.18	0.010***	0.003	947
loan repayment/total spending (%)	0.05	0.05	-0.002	0.499	947
Geographic					
north	0.62	0.57	0.043	0.190	949
centre	0.14	0.13	0.018	0.435	949
south	0.24	0.30	-0.061**	0.039	949
coastal city	0.06	0.09	-0.028	0.109	949
slope	19.85	16.22	3.637**	0.033	949
area (km^2)	35.66	38.03	-2.376	0.366	949
House supply (determinants)					
stock of houses (houses/inhab)	0.49	0.48	0.016	0.186	949
empty houses (%)	0.18	0.17	0.012	0.239	949
houses for rent (%)	0.14	0.14	-0.005	0.177	949
House demand (determinants)					
foreign people (%)	0.03	0.02	0.001	0.599	949
people over 65 years (%)	0.19	0.18	0.009***	0.001	949
employment rate 15-64 (%)	0.46	0.46	0.002	0.756	949
population density (inhab/km^2)	282.02	348.96	-66.947***	0.003	949

Notes. Two-sample t-test on the equality of means for municipalities between $(3,500 - 5,000)$ and $(5,000 - 6,500)$ inhabitants. Average values over 1999 and 2000. Floor area is the total amount of useful area per capita allowed to be constructed by the release of building permits. Urbanization revenues per capita are provided both on accrual basis, nominal values. Sources: Istat, Bank of Italy

2.8.1 Effects on urbanization revenues and construction permits

Timeline 1: Relevant periods for the preliminary analysis: variation over three different periods as considered for the visual analysis with global smooth approximation and local behaviour represented by Figure 2.1.

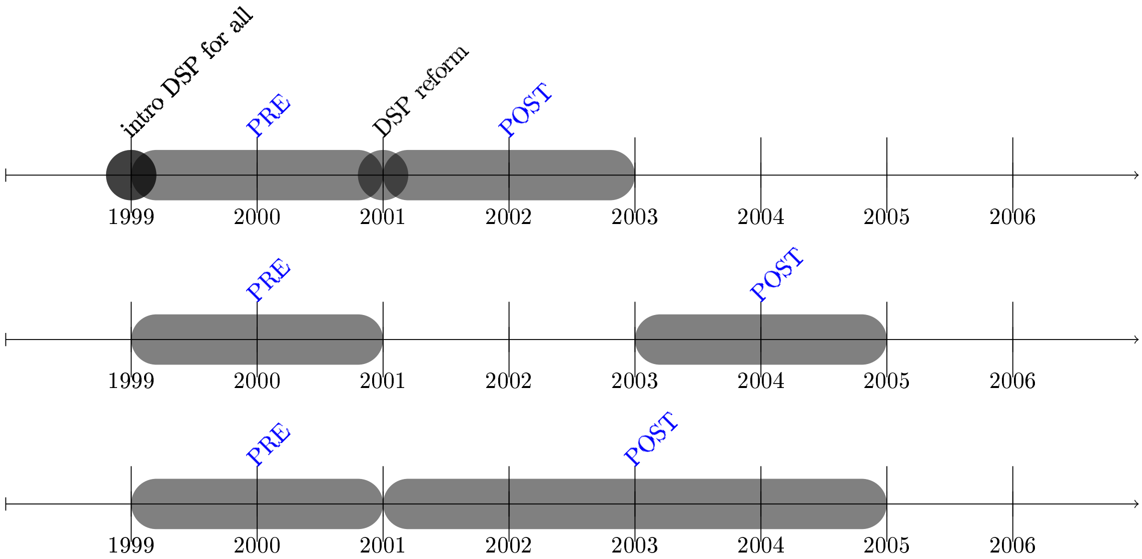
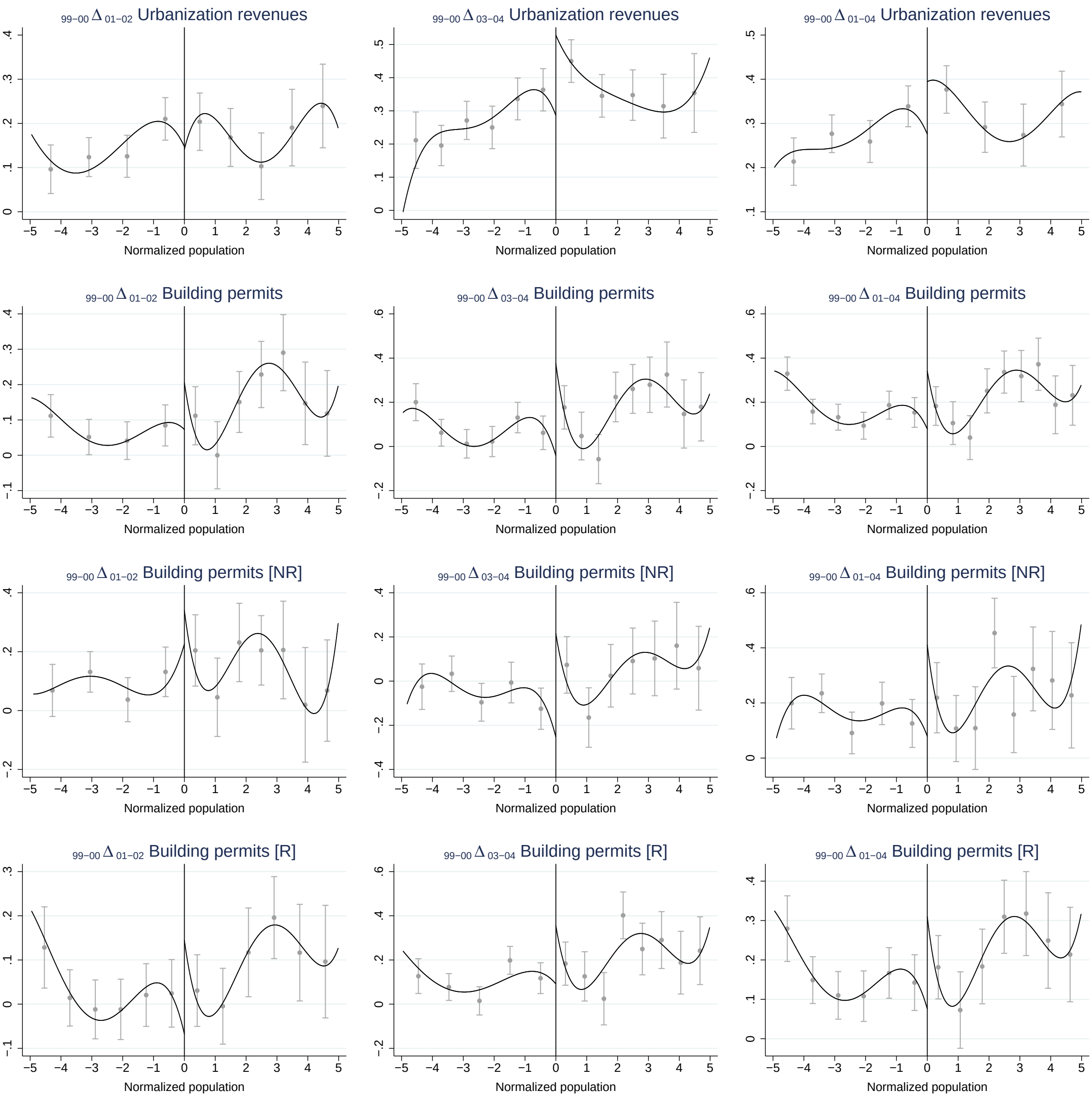


Figure 2.1: Visual analysis: global smooth approximation and local behaviour



Notes. Global polynomial fit (4^{th} order) and local sample means with bin selected with IMSE-optimal evenly-spaced method using spacings estimators (confidence interval at 90%). Y-axis: average growth rate of the outcome variable (before and after the introduction of the DSP). X-axis: legal population at 2001 normalized at the threshold = 5,000.

Table 2.3: Effect of the DSP for municipalities larger than 5,000 inhab over 1999 – 2000 vs 2001 – 2002.

	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	.025	.031	.074	.052
	(0.792)	(0.721)	(0.576)	(0.600)
Bias-corrected	.029	.06	.112	.087
	(0.758)	(0.499)	(0.400)	(0.387)
Robust	.029	.06	.112	.087
	(0.799)	(0.559)	(0.473)	(0.446)
Estimation				
Bandwidth	1.322	1.904	1.735	1.694
Obs	853	1263	993	1077
Bias-correction				
Bandwidth	2.129	3.388	2.758	3.041
Obs	1427	2422	1639	2046

p-values in parentheses

Notes. RD estimates of the impact of the DSP on policy outcomes (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the period 1999 – 2000 vs 2001 – 2002, $\Delta Y = (\bar{Y}_{01-02} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{01-02} + \frac{1}{2}\bar{Y}_{99-00})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Table 2.4: Effect of the DSP for municipalities larger than 5,000 inhab over $1999-2000$ vs $2003-2004$.

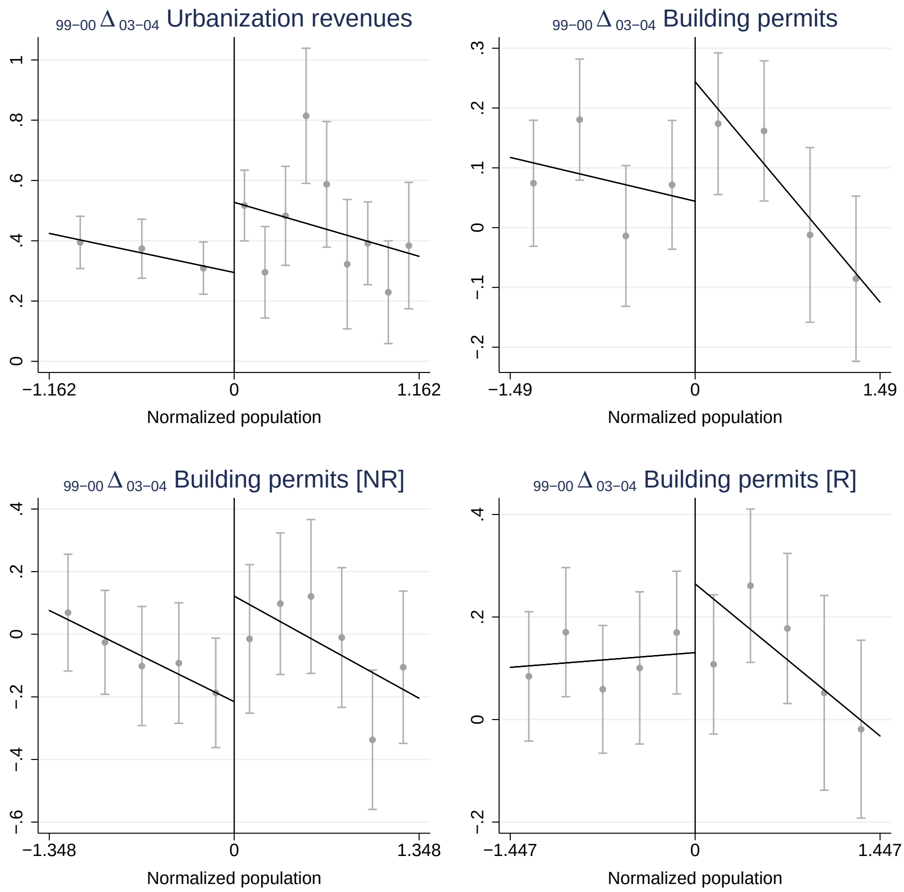
	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	.214**	.182	.304*	.045
	(0.014)	(0.121)	(0.079)	(0.716)
Bias-corrected	.234***	.236**	.366**	.031
	(0.007)	(0.044)	(0.034)	(0.803)
Robust	.234**	.236*	.366*	.031
	(0.022)	(0.064)	(0.069)	(0.834)
Estimation				
Bandwidth	1.162	1.490	1.348	1.447
Obs	751	947	789	909
Bias-correction				
Bandwidth	2.006	3.102	2.340	2.199
Obs	1360	2164	1396	1456

p-values in parentheses

Notes. RD estimates of the impact of the DSP on policy outcomes (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the period $1999-2000$ vs $2003-2004$, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Figure 2.2: Visual analysis: estimated effects



Notes. Local linear fit with optimal-MSE bandwidth and local sample means with bin selected with an IMSE-optimal evenly-spaced method using spacings estimators (confidence interval at 90%). Y-axis: average growth rate of the outcome variable (before and after the introduction of the DSP). X-axis: legal population at 2001 normalized at the threshold = 5,000.

Table 2.5: Sensitivity to the polynomial order specifications. Effect of the DSP for municipalities larger than 5,000 inhab.

	Δ Urbanization revenues			Δ Building permits [NR]		
	poly=0	poly=1	poly=2	poly=0	poly=1	poly=2
Conventional	.116**	.214**	.203*	.2*	.304*	.346*
	(0.019)	(0.014)	(0.062)	(0.097)	(0.079)	(0.096)
Bias-corrected	.136***	.234***	.194*	.272**	.366**	.357*
	(0.005)	(0.007)	(0.074)	(0.024)	(0.034)	(0.085)
Robust	.136**	.234**	.194	.272*	.366*	.357
	(0.030)	(0.022)	(0.113)	(0.066)	(0.069)	(0.141)
Estimation						
Bandwidth	1.292	1.162	1.555	0.818	1.348	2.107
Obs	840	751	1008	463	789	1271
Bias-correction						
Bandwidth	2.624	2.006	2.116	2.076	2.340	2.915
Obs	1791	1360	1432	1255	1396	1757

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes over different periods (cross section model). Method: local polynomial regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the considered period e.g. for $1999-2000$ vs $2003-2004$, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Table 2.6: Covariates-adjusted estimates. Effect of the DSP for municipalities larger than 5,000 inhab. over 1999 − 2000 vs 2003 − 2004.

	Δ Urbanization revenues			Δ Building permits [NR]		
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	.207**	.206**	.201**	.271	.288*	.303*
	(0.017)	(0.019)	(0.022)	(0.112)	(0.087)	(0.067)
Bias-corrected	.228***	.227***	.224**	.333*	.343**	.368**
	(0.009)	(0.010)	(0.011)	(0.051)	(0.042)	(0.026)
Robust	.228**	.227**	.224**	.333*	.343*	.368*
	(0.025)	(0.029)	(0.029)	(0.091)	(0.081)	(0.053)
Estimation						
Bandwidth	1.084	1.077	1.045	1.331	1.285	1.321
Obs	707	704	683	779	749	772
Bias-correction						
Bandwidth	1.927	1.897	1.877	2.387	2.296	2.499
Obs	1281	1256	1230	1425	1368	1489
Covariates						
Geo and Budget	Yes	Yes	Yes	Yes	Yes	Yes
House supply	No	Yes	Yes	No	Yes	Yes
House demand	No	No	Yes	No	No	Yes

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes over different periods (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the considered period e.g. for $1999-2000$ vs $2003-2004$, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Table 2.7: Validation test using placebo outcomes. Effect of the DSP for municipalities larger than 5,000 inhab.

	north	center	south	coastal	slope	area
Conventional	-.122	-.039	.166*	.021	3.88	-7.51
	(0.269)	(0.547)	(0.093)	(0.609)	(0.316)	(0.244)
Bias-corrected	-.144	-.038	.178*	.021	4.15	-8.64
	(0.194)	(0.562)	(0.071)	(0.598)	(0.284)	(0.180)
Robust	-.144	-.038	.178*	.021	4.15	-8.64
	(0.236)	(0.605)	(0.097)	(0.640)	(0.333)	(0.243)
Estimation						
Bandwidth	1.967	2.210	1.786	2.220	1.518	1.673
Obs	1322	1498	1162	1501	972	1087
Bias-correction						
Bandwidth	2.387	2.664	2.099	3.003	2.108	2.532
Obs	1609	1827	1424	2089	1424	1710

p-values in parentheses

Note. RD estimates of the impact of the DSP on placebo outcomes (predetermined covariates) over different periods (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Table 2.8: Effect of the DSP for municipalities larger than 5,000 inhab over $1999-2000$ vs $2003-2004$ (excluding southern municipalities).

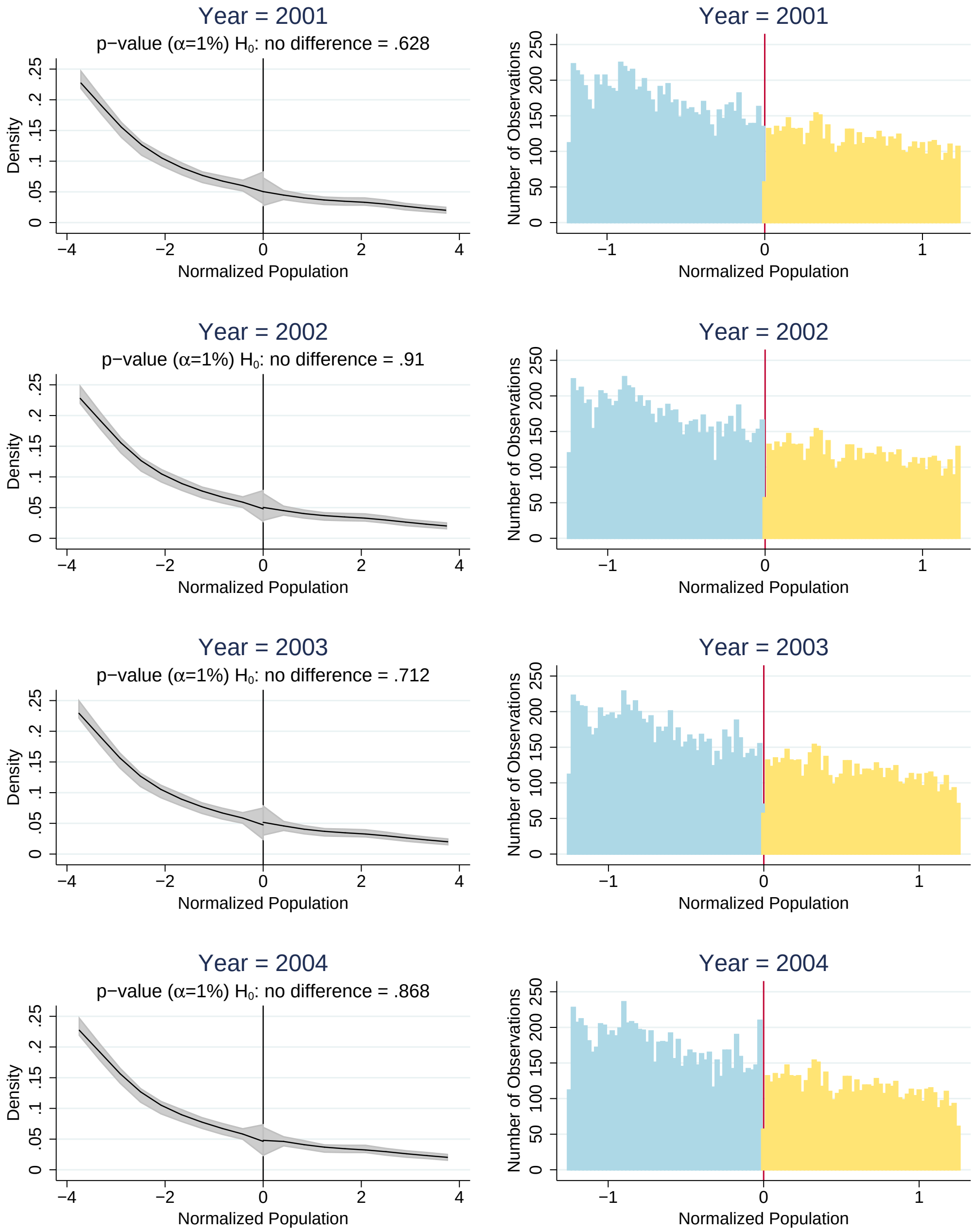
	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	.15*	.253*	.414**	.105
	(0.099)	(0.073)	(0.046)	(0.457)
Bias-corrected	.159*	.317**	.481**	.117
	(0.081)	(0.024)	(0.021)	(0.408)
Robust	.159	.317**	.481**	.117
	(0.142)	(0.043)	(0.047)	(0.486)
Estimation				
Bandwidth	1.307	1.365	1.316	1.521
Obs	632	650	568	707
Bias-correction				
Bandwidth	2.085	2.553	2.291	2.276
Obs	1040	1269	1004	1112

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the period $1999-2000$ vs $2003-2004$, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

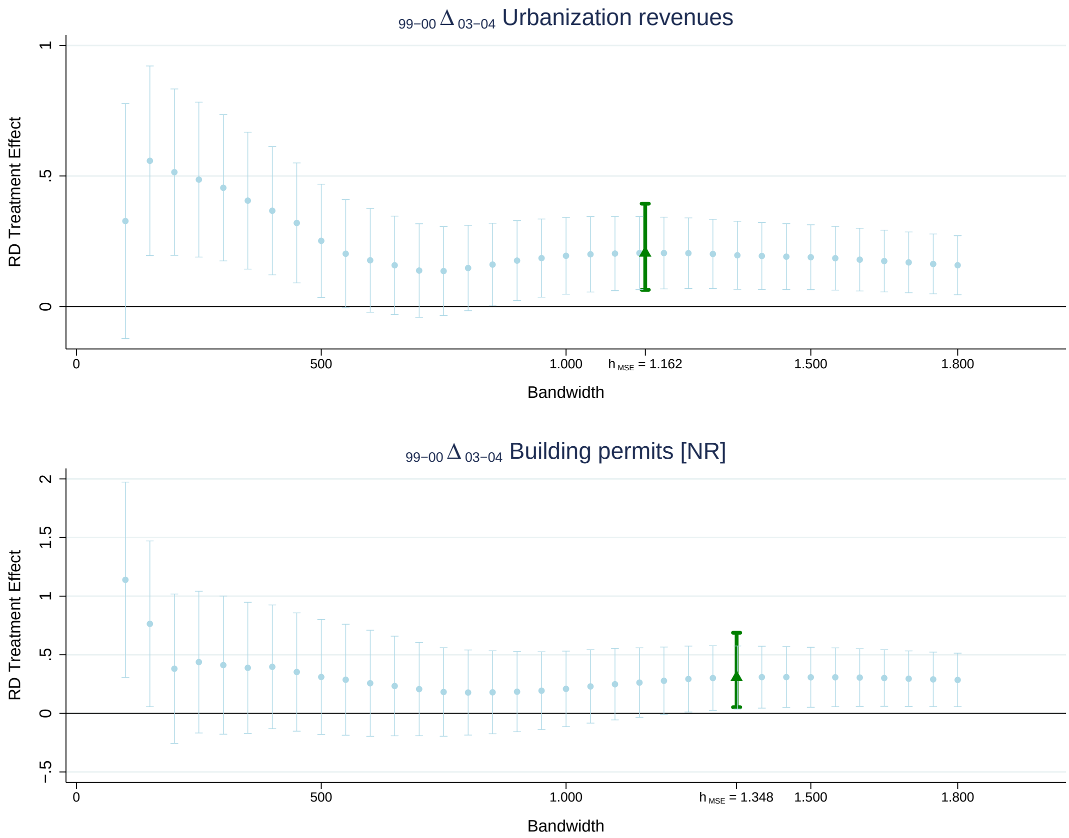
* p<0.10, ** p<0.05, *** p<0.01

Figure 2.3: Density test: manipulation of the running variable



Notes. On the left: Local polynomial fit (2^{nd} order) with confidence interval at 99% and p-value for the bias-corrected density test (H_0: *No difference in the density of treated and control observations at the cutoff*) according to Cattaneo, Jansson, and Ma (2018). Y-axis: density of observation X-axis: Normalized population. On the right: histogram of population distribution (bin width = 0.025). Y-Axis: Number of observations. X-axis: Normalized population (between the optimal bandwidth computed for the corresponding local polynomial fit on the left) .

Figure 2.4: Estimates sensitivity to bandwidth



Notes. Scatterplot of RD treatment effects for different values of the bandwidth (with the confidence interval at 90%: conventional [light blue] and robust [green]). Y-axis: RD estimates of the impact of the DSP for municipalities larger than 5,000 inhab. Method: local linear regression with triangular kernel. X-axis: Bandwidth (absolute values, thousands of inhabitants); optimal MSE (Minimum Square Error) bandwidth [green and bold] (see Cattaneo et al., 2014, 2017).

Table 2.9: Effect of a fake DSP relaxation in 1999 for municipalities larger than 5,000 inhab. over 1997-1998 vs 1999-2000

	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	-.042	-.081	-.101	-8.5e-03
	(0.709)	(0.578)	(0.570)	(0.953)
Bias-corrected	-.032	-.04	-.09	.033
	(0.774)	(0.783)	(0.612)	(0.819)
Robust	-.032	-.04	-.09	.033
	(0.814)	(0.815)	(0.673)	(0.846)
Estimation				
Bandwidth	1.964	1.406	1.538	1.319
Obs	1230	844	819	794
Bias-correction				
Bandwidth	3.010	2.315	2.296	2.103
Obs	1962	1452	1253	1299

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes over different periods (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the district level. The outcome is the average growth rate over the considered period e.g. for $1999-2000$ vs $1997-1998$, $\Delta Y = (\bar{Y}_{99-00} - \bar{Y}_{97-98})/(\frac{1}{2}\bar{Y}_{99-00} + \frac{1}{2}\bar{Y}_{97-98})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Timeline 2: a fake DSP relaxation in 1999 for municipalities larger than 5,000 inhab., while in that year DSP was introduced and binding for all cities (considered variation: 1997-1998 vs 1999-2000)

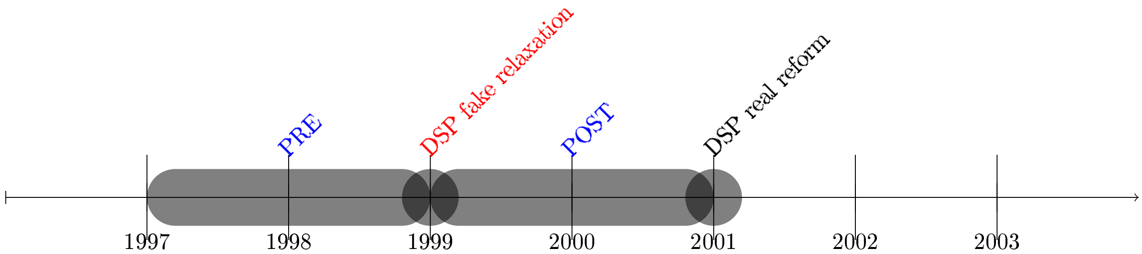
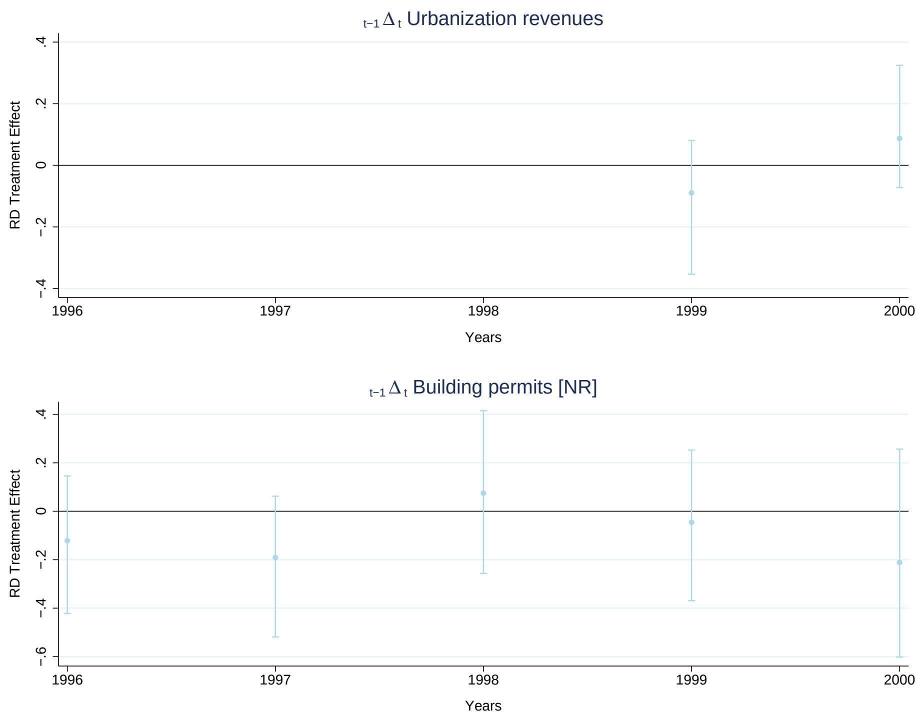
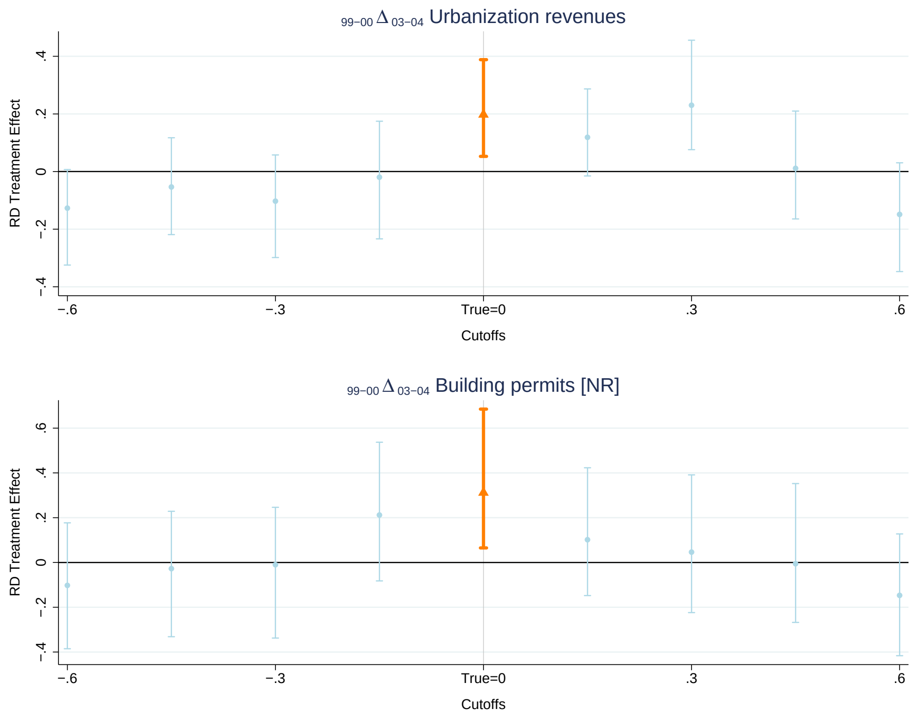


Figure 2.5: Validity test: parallel trend before the reform



Notes. Scatterplot of RD treatment effects on the year-by-year average growth rate with optimal-MSE bandwidth (with the confidence interval at 90 percent). Y-axis (1): RD estimates of the impact of the DSP for municipalities larger than 5,000 inhab. X-axis: Year

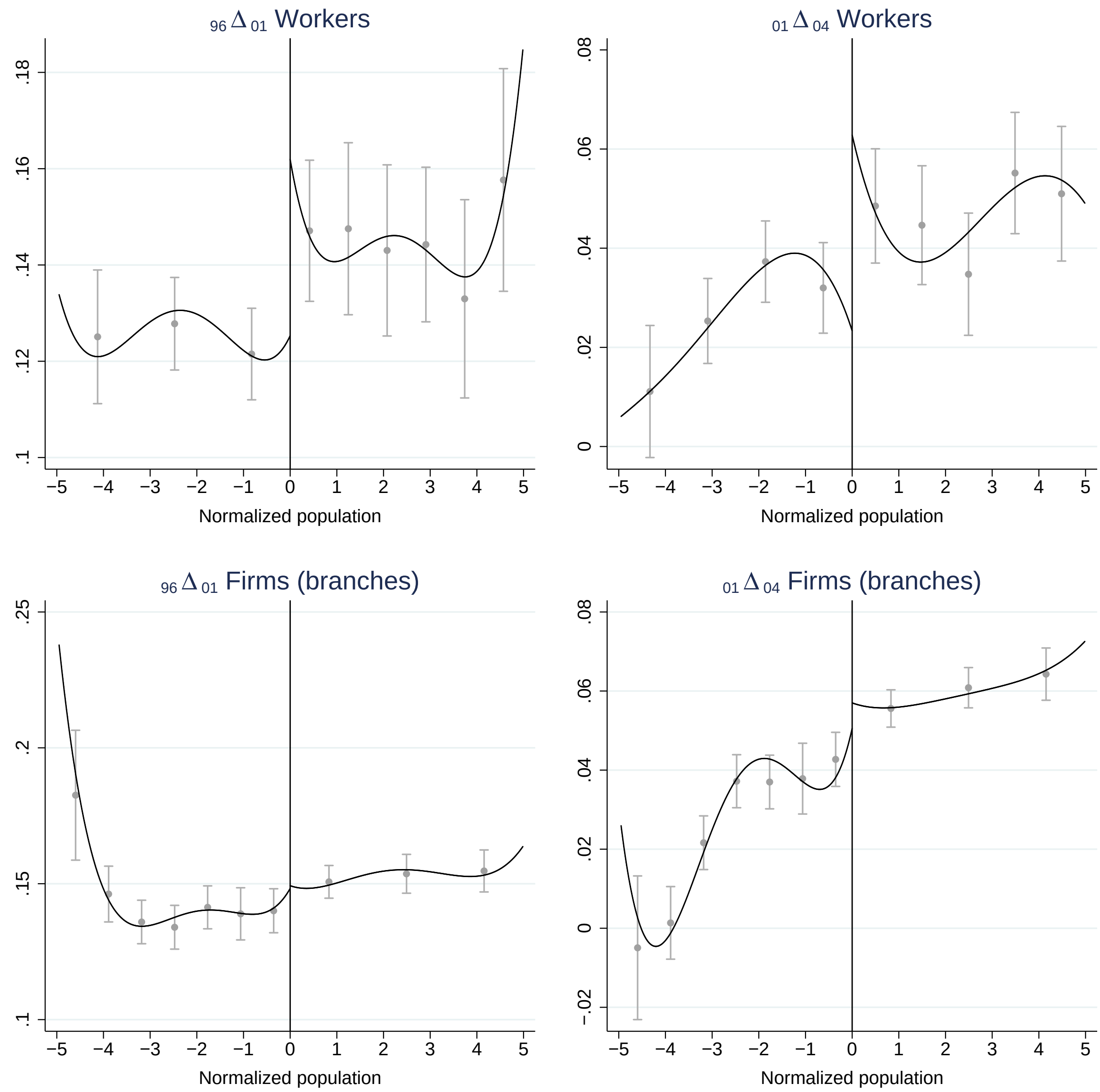
Figure 2.6: Validity test: false cutoffs (allowing contamination)



Notes. Scatterplot of RD treatment effects with variable cutoffs and optimal-MSE bandwidth (with the confidence interval at 90 percent). Y-axis (1): RD estimates of the impact of the DSP for municipalities larger than 5,000 inhab. X-axis: Cutoffs at the normalized population (thousands of inhabitants)

2.8.2 Downstream effects on economic performance

Figure 2.7: Visual analysis: global smooth approximation and local behaviour



Notes. Global polynomial fit (4^{th} order) and local sample means with bin selected with IMSE-optimal evenly-spaced method using spacings estimators (confidence interval at 90%). Y-axis: average growth rate of the outcome variable (before and after the introduction of the DSP). X-axis: legal population at 2001 normalized at the threshold = 5,000.

Table 2.10: Effect of the DSP for municipalities larger than 5,000 inhab.

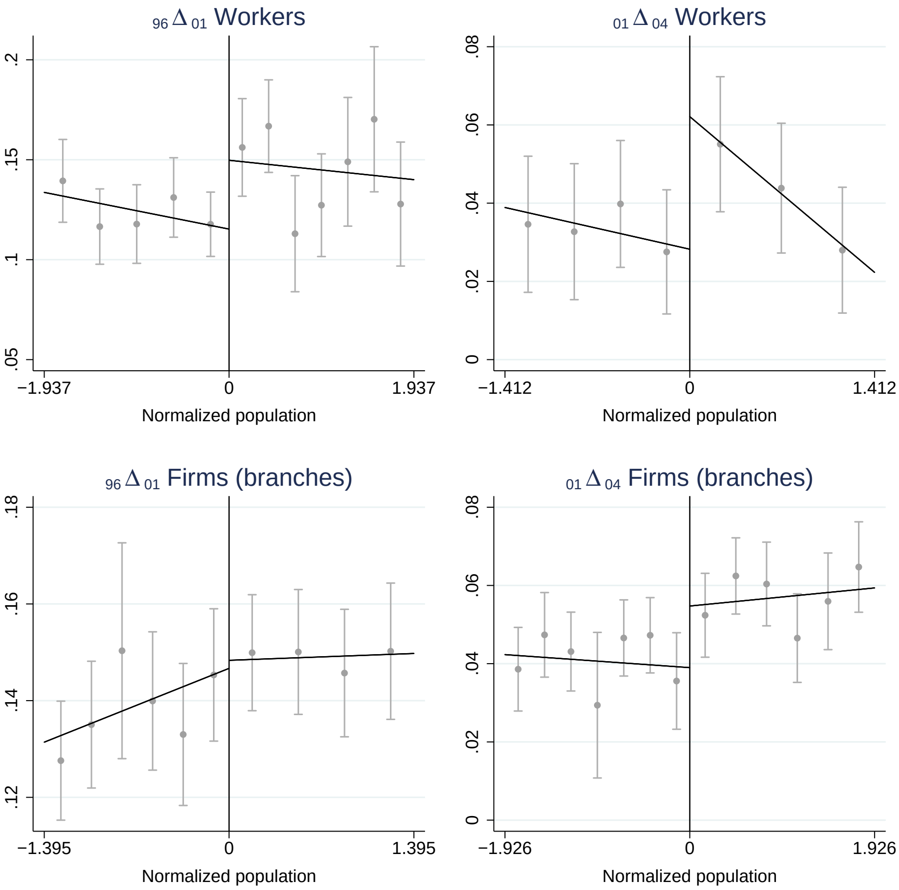
	Δ Workers		Δ Firms (Branches)	
	Δ_{96}^{01}	Δ_{01}^{04}	Δ_{96}^{01}	Δ_{01}^{04}
Conventional	.03*	.041**	3.7e-04	.021**
	(0.071)	(0.041)	(0.978)	(0.022)
Bias-corrected	.034**	.046**	-1.6e-03	.024***
	(0.041)	(0.019)	(0.903)	(0.009)
Robust	.034*	.046**	-1.6e-03	.024**
	(0.081)	(0.047)	(0.919)	(0.022)
Estimation				
Bandwidth	1.937	1.412	1.395	1.926
Obs	1295	906	899	1282
Bias-correction				
Bandwidth	3.224	2.328	2.173	3.125
Obs	2269	1564	1469	2180

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes over different periods (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the considered period $2004-1996$ $\Delta Y = (\bar{Y}_{04} - \bar{Y}_{96})/(\frac{1}{2}\bar{Y}_{04} + \frac{1}{2}\bar{Y}_{96})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Figure 2.8: Visual analysis: estimated effects



Notes. Local linear fit with optimal-MSE bandwidth and local sample means with bin selected with an IMSE-optimal evenly-spaced method using spacings estimators (confidence interval at 90%). Y-axis: average growth rate of the outcome variable (before and after the introduction of the DSP). X-axis: legal population at 2001 normalized at the threshold = 5,000.

Table 2.11: Sensitivity to the polynomial order specifications. Effect of the DSP for municipalities larger than 5,000 inhab.

	Δ_{01}^{04} Workers			Δ_{01}^{04} Firms (branches)		
	poly=0	poly=1	poly=2	poly=0	poly=1	poly=2
Conventional	.027**	.041**	.047*	.019***	.021**	.017
	(0.047)	(0.041)	(0.052)	(0.007)	(0.022)	(0.227)
Bias-corrected	.035***	.046**	.049**	.02***	.024***	.015
	(0.009)	(0.019)	(0.044)	(0.003)	(0.009)	(0.299)
Robust	.035**	.046**	.049*	.02**	.024**	.015
	(0.035)	(0.047)	(0.084)	(0.020)	(0.022)	(0.366)
Estimation						
Bandwidth	0.892	1.412	2.055	1.274	1.926	1.732
Obs	580	906	1391	828	1282	1130
Bias-correction						
Bandwidth	2.100	2.328	2.673	2.146	3.125	2.304
Obs	1424	1564	1834	1449	2180	1550

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes over different periods (cross section model). Method: local polynomial regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. ***Robust*** estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the considered period e.g. for $2004-2001$ $\Delta Y = (\bar{Y}_{04} - \bar{Y}_{01})/(\frac{1}{2}\bar{Y}_{04} + \frac{1}{2}\bar{Y}_{01})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Table 2.12: Covariates-adjusted estimates. Effect of the DSP for municipalities larger than 5,000 inhab.

	Δ_{01}^{04} Workers			Δ_{01}^{04} Firms (branches)		
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	.037*	.039*	.041**	.018**	.017**	.021**
	(0.058)	(0.052)	(0.033)	(0.041)	(0.036)	(0.016)
Bias-corrected	.042**	.045**	.047**	.021**	.019**	.023***
	(0.029)	(0.026)	(0.015)	(0.020)	(0.021)	(0.008)
Robust	.042*	.045*	.047**	.021**	.019**	.023**
	(0.064)	(0.058)	(0.037)	(0.042)	(0.044)	(0.024)
Estimation						
Bandwidth	1.426	1.379	1.330	1.902	1.846	1.602
Obs	913	890	864	1261	1208	1035
Bias-correction						
Bandwidth	2.321	2.261	2.200	3.114	2.976	2.626
Obs	1560	1525	1484	2171	2067	1784
Covariates						
Geo and Budget	Yes	Yes	Yes	Yes	Yes	Yes
House supply	No	Yes	Yes	No	Yes	Yes
House demand	No	No	Yes	No	No	Yes

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes over different periods (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the considered period e.g. for $2004 - 2001$ $\Delta Y = (\bar{Y}_{04} - \bar{Y}_{01})/(\frac{1}{2}\bar{Y}_{04} + \frac{1}{2}\bar{Y}_{01})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Table 2.13: Effect of the DSP for municipalities larger than 5,000 inhab. Δ_{2001}^{2004} Firms (branches) by economic sector (NACE rev.1 - ATECO 1991)

	All sectors	Private (G-H-I)	Private (Others)	Public (All)
Conventional	.016*	.029**	5.6e-03	.011
	(0.086)	(0.010)	(0.619)	(0.570)
Bias-corrected	.017*	.032***	7.8e-03	7.8e-03
	(0.059)	(0.005)	(0.486)	(0.677)
Robust	.017	.032**	7.8e-03	7.8e-03
	(0.109)	(0.018)	(0.548)	(0.725)
Estimation				
Bandwidth	1.254	1.101	1.397	1.526
Obs	808	713	898	979
Bias-correction				
Bandwidth	2.080	1.852	2.505	2.534
Obs	1403	1211	1691	1708

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes over different periods (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the period $2004-2001$ $\Delta Y = (\bar{Y}_{04} - \bar{Y}_{01})/(\frac{1}{2}\bar{Y}_{04} + \frac{1}{2}\bar{Y}_{01})$. Economic sectors (macro-areas, NACE rev.1.1 - ATECO 2002): B-C-D-E: fishing, mining manifacturing, energy production; F: Construction; G-H-I: wholesale trade, hotel, restaurant and catering services, transport and storage; J: financial and insurance sector; K: real estate, renting and leasing, research and development sector, business services; M-N: education, health and other social services; O-P-Q: Others. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Table 2.14: Effect of the DSP for municipalities larger than 5,000 inhab. Δ_{2001}^{2004} Workers by economic sector (NACE rev.1 - ATECO 1991)

	All sectors	Private (G-H-I)	Private (Others)	Public (All)
Conventional	.041**	.048**	.02	.05
	(0.038)	(0.023)	(0.418)	(0.219)
Bias-corrected	.047**	.055***	.023	.056
	(0.018)	(0.009)	(0.355)	(0.172)
Robust	.047**	.055**	.023	.056
	(0.041)	(0.020)	(0.433)	(0.253)
Estimation				
Bandwidth	1.276	1.398	1.487	1.678
Obs	827	898	944	1088
Bias-correction				
Bandwidth	2.105	2.891	2.220	2.669
Obs	1419	1993	1497	1825

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes over different periods (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the average growth rate over the period $2004-2001$ $\Delta Y = (\bar{Y}_{04} - \bar{Y}_{01})/(\frac{1}{2}\bar{Y}_{04} + \frac{1}{2}\bar{Y}_{01})$. Economic sectors (macro-areas, NACE rev.1.1 - ATECO 2002): B-C-D-E: fishing, mining manifacturing, energy production; F: Construction; G-H-I: wholesale trade, hotel, restaurant and catering services, transport and storage; J: financial and insurance sector; K: real estate, renting and leasing, research and development sector, business services; M-N: education, health and other social services; O-P-Q: Others. Public sector: M-N-O-P-Q Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

2.9 Appendix

Table 2.15: Effect of the DSP for municipalities larger than 5,000 inhab. over $1999-2000$ vs $2003-2004$. Alternative outcome definition: log growth rate.

	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	.182*	.149	.439	.103
	(0.054)	(0.315)	(0.105)	(0.495)
Bias-corrected	.205**	.147	.536**	.11
	(0.031)	(0.322)	(0.047)	(0.467)
Robust	.205*	.147	.536*	.11
	(0.066)	(0.409)	(0.087)	(0.543)
Estimation				
Bandwidth	1.328	1.427	1.333	1.445
Obs	835	931	794	923
Bias-correction				
Bandwidth	2.248	2.104	2.276	2.199
Obs	1460	1455	1390	1483

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes (cross section model). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression of order 2 and ad-hoc optimal-MSE bandwidth) according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. *Robust* estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the province level. The outcome is the log growth rate over the period $1999-2000$ vs $2003-2004$, $\Delta Y = logY_{03-04} - logY_{99-00}$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

Table 2.16: Effect of the DSP for municipalities larger than 5,000 inhab. over 1999 – 2000 vs 2003 – 2004. Different clusters for variance-covariance matrix.

	Δ urb rev		Δ permits		Δ permits [NR]		Δ permits [R]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	.227***	.225***	.305*	.307*	.305*	.307*	.076	.075
	(0.008)	(0.007)	(0.051)	(0.070)	(0.051)	(0.070)	(0.500)	(0.494)
Bias-corrected	.252***	.251***	.332**	.337**	.332**	.337**	.078	.076
	(0.003)	(0.003)	(0.033)	(0.047)	(0.033)	(0.047)	(0.484)	(0.489)
Robust	.252**	.251***	.332*	.337*	.332*	.337*	.078	.076
	(0.011)	(0.009)	(0.074)	(0.098)	(0.074)	(0.098)	(0.560)	(0.563)
Estimation								
Bandwidth	1.146	1.110	1.302	1.318	1.302	1.318	1.447	1.439
Obs	759	736	789	799	789	799	935	932
Bias-correction								
Bandwidth	1.924	1.921	2.002	2.034	2.002	2.034	2.236	2.223
Obs	1318	1314	1244	1267	1244	1267	1518	1510
Cluster	POP	LLM	POP	LLM	POP	LLM	POP	LLM

p-values in parentheses

Note. RD estimates of the impact of the DSP on policy outcomes over different periods (cross section model). Method: local polynomial regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction according to Calonico et al. (2014, 2017). Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. *Bias-Corrected* estimates allow for error misspecification correction, without incorporating the variability introduced in the estimator's variance when the bias is estimated. ***Robust*** estimates include bias correction and corrected estimator's variance. Standard errors are clustered at the population (POP) level or at the Local Labor Market (LLM) level. The outcome is the average growth rate over the considered period e.g. for 1999 – 2000 vs 2003 – 2004, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. Treatment: d(pop>5,000). Variables are in per capita terms, nominal values. Population is the legal value at 2001 (i.e. administrative population at 1999) normalized at the threshold = 5,000 inhab.

* p<0.10, ** p<0.05, *** p<0.01

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Chapter 3

Political cycle and term limit effect on land use regulation: evidence for Italian municipalities in 1995-2014

Abstract

This paper provides evidence on the existence of an opportunistic electoral cycle in the issue of building permits at the local level in Italy from 1995 to 2014, using monthly data for the universe of municipalities. Mayors tend to exert pressure on administrative offices to ease or speed-up the release of permits, an outcome which is more controllable and targetable than fiscal ones. A within-term analyses shows the absence of cycle in the last-term, since the lack of incentive for the incumbents that cannot run for re-election: this allows to exclude any reputation incentive. The political cycle is significant for cities below 15,000 inhabitants and it is visible both in the northern and southern regions.

JEL Codes: D72, D73, H83, R52
Keywords: electoral cycle, term limit, land use regulation, building permits, local government

3.1 Introduction

Despite a vast empirical literature, the evidence for the existence of the political cycle is still mixed, in particular for developed countries (see Drazen, 2000 for early literature). Applied studies have mainly focused on monetary and fiscal outcomes at different levels of government, while only recently attention has been given to other types of outcomes, heterogeneity and contextual determinants of the cycle across countries (De Haan and Klomp, 2013).

Evidence of political cycle is often weak since macroeconomic variables may not account for the opportunistic behaviour of incumbent politicians close to the election, in particular at the local level. Mayors, despite having a minimal influence on few spending items, can manipulate both duration and outcomes of the administrative processes that provide immediate benefit to voters, as for the release of building permits, while fiscal benefits can be lagged and are not always targetable (Garmann, 2017). Speeding up the authorisation process to create or extend an existing building may reduce explicit financial and time costs for constructors since both the outcome and the length of this regulatory process are uncertain (Mayer and Somerville, 2000). Moreover, the cycle could be magnified as predicted by moral hazard models of electoral competition (Shi and Svensson, 2006) since the very nature of issuing permits does not allow the voters to distinguish between competence and opportunistic manipulation.

This work adds to the empirical literature by investigating the within-term electoral cycle effect and term-limit impact on the administrative process of issuing building permits for Italian municipalities over the period from 1995 to 2014 using high frequency (monthly) data. Indeed, the release of authorisations to build is a non-fiscal outcome which has been understudied so far except for Garmann, 2017 who analyses the cycle at the local level in Germany with annual data over 2001-2010. Moreover, this work contributes to the existing literature an analysis of the within-terms cycles and their possible different characteristics across general and last terms (Klein and Sakurai, 2015; Dalle Nogare and Kauder, 2017). The use of high frequency data allows solving the underestimation of cycles potentially affecting the previous studies (Akhmedov and Zhuravskaya, 2004), since manipulation may be concentrated near the electoral month. Furthermore, analysing the universe of municipalities in a homogeneous context such as a single country, allows to control for cultural and institutional characteristics that can potentially threaten the identification of causal effects in a cross-country analysis. Finally, the long-time horizon (1995 to 2014) allows disentangling the election year-month effect from pure time effects exploiting non-simultaneous elections.

This paper shows that, when incumbent mayors can be re-elected, the release of permits increases by 0.011 to 0.062 standard deviations from the average monthly value over the considered period (between +3% and +16%), the stronger effect, the closer to the electoral month. This cycle is evident for small municipalities (below 15,000 inhabitants) which account for 90% of all Italian cities, and it is detected both in the northern and southern regions.

The rest of the paper is organised as follows. In section 3.2 a review of the literature is provided. Then section 3.3 describes the institutional set-up including the organisation of municipalities, electoral rules and issuing of building permits. In section 3.4 a detailed description of data is provided, section 3.5 describes the empirical strategy and section 3.6 comments the results. The conclusion follows in the section 3.7.

3.2 Review of the literature

Since the pioneering contribution of Nordhaus, 1975 on opportunistic pre-electoral manipulation, and after its reconciliation with the rational expectation revolution (Rogoff and Sibert, 1988, Rogoff, 1990, Shi and Svensson, 2006) and the contemporary development of partisan cycle theories (Hibbs, 1977, Alesina, 1987, Alesina, 1988), a vast applied literature on political cycle has been developed. However, such tradition has not been able to provide concluding evidence.

Most of the empirical tests have been focused on detecting the cycle via instruments manipulation, rather than macroeconomic outcomes, these being more directly controllable. Indeed, to signal his competence which is the ability to provide public goods, the incumbent needs to generate clear and immediate economic benefits to the constituency (Tufte, 1980). For this reason the existence of growing constraints on the use of monetary and fiscal variables may lead governments to change the distribution of the public expenditure rather than its aggregate (Drazen and Eslava, 2010): targeted spending is more visible and can benefit only pivotal group of voters.

Numerous empirical studies have found evidence of manipulation for several categories: public employment (Levitt, 1997; Katsimi, 1998), public wages (Klein, 2004), government support to agricultural sector (Klomp and De Haan, 2013), health expenditure (Potrafke, 2010), and culture (Dalle Nogare and Galizzi, 2011). Beyond considering alternative outcomes (see Dubois, 2016 for a review), the current research focuses on the contextual determinants of the presence and magnitude of the political cycle, identifying political institutions and voters characteristics as relevant, e.g. level of development and democracy, institutional quality and media access, constitutions (De Haan and Klomp, 2013).

The rules governing the electoral calendar – such as a term-limit which prevents politicians from competing for re-election – have been the most common aspect exploited by literature to shed light on the incentive mechanism for the incumbent. The theory of conditional political business cycle predicts that the absence of incentives for the incumbent in his last term will deviate him from the equilibrium since he cannot be re-elected.

The evidence is usually based on the US data: in recent works List and Sturm, 2006 find an opportunistic electoral behaviour wich is limited to secondary policies while Aidt and Shvets, 2012 find evidence in favour of the incumbents' deviation for pork-barrel spending. In contrast with previous works based on US and international data, Dalle Nogare and Ricciuti, 2011 do not evidence of opportunistic spending in a cross-country study except for presidential countries. The incentive effect of term limit is also used to discriminate between yardistick competition from competing theory of strategic interaction: Bordignon, Cerniglia, and Revelli, 2003 find spatial-correlation in local tax rates where mayors compete for re-election in Italy. In more recent studies, Klein and Sakurai, 2015, analyse the term-limit effect on spending within-term framework for Brazilian municipalities and Dalle Nogare and Kauder, 2017 uncover incumbents' incentive to obtain intergovernmental grants.

However, different are the equilibriums of principal-agent models of reputation - see Barro, 1973 - which predict a disciplining role of the election when the incumbent can still run for re-election. The seminal work of Besley and Case, 1995 finds evidence of the election effect on accountability by analysing the behavior of US governors on different policy instruments and so do more recently Johnson and Crain, 2004 in a cross-country study of fiscal performance under alternative term-limit rules. More recently, Alt, Bueno de Mesquita, and Rose (2011) have focused on disentangling the accountability and competence effects of elections by exploiting the variation in the length of the term limit, but providing uncertain predictions.

3.3 Institutional setting

The Italian administrative framework is based on 20 regions (NUTS3 in the Eurostat definition), 110 provinces (NUTS4) and more than 8,000 municipalities which represent the lower level of government. Municipalities are composed by an elected mayor, an executive committee whose members are appointed by the mayor and an elected city council that is responsible for the approval of the annual budget. Since the reform of local government framework in 1993, both the mayor and the city council are elected directly and by plurality rule with majority premium, with a run-off for cities above 15,000 inhabitants and single-round below. Moreover, the duration of the legal term was reduced from five to four years (and then restored after 2000) and a term-limit rules out the possibility to compete for re-election after two consecutive terms.

Several public services are provided at the local level as kindergartens and care for elderly, social housing, public transportation and road maintenance, local police, cultural services, water supply and waste management and other environmental related services. Municipalities are responsible for about 10% of total primary expenditures, funded by own taxes and fees and by transfers from regional or national governments.

Beyond the provision of services, municipalities are also in charge of the land-use regulation and city development: in particular, they set building rules within their regional legislative framework. They request constructors to comply with several requirements and potentially costly changes to obtain a formal authorisation to build, making this regulatory process lengthy and uncertain. Since 1968 the release of building licenses requires the builder to pay a fee, in order to make him share with the local administration the financial burden of providing infrastructures to the newly built area. Since then, the Italian legislators have amended the destination of this revenue periodically while keeping unchanged the rationale for its introduction.

The official or manager in charge of the administrative-technical unit that supervisions constructors' compliance with building regulations is legally responsible for the issuing of permits. While the mayor and executive committee decide the orientation, objects, policies and programs, officials are responsible for the administrative, financial and technical management required to implement them, having independent power on expenses, personnel management and control. Indeed, since the 1993's Law on the rationalisation of public administration, the principle of separation between political objects and administrative management was set and then carried out in 2000 with the Act on local authorities (TUEL) that clarifies specific functions, tasks and responsibilities. Nonetheless, the possibility for the mayor to exert some form of suasion or soft coercion on some aspects of municipal administration (or speed-up some mechanisms) exists, being the mayor in charge of vigilance of administrative activities to check their consistencies with executive committee's goals and potentially suspend the adoption of administration deeds. Moreover, the mayor can attribute or remove tasks to the officials, a further element that reveals the actual ability of the mayor to determine desired outcomes.

3.4 Data

The analysis considers the universe of Italian municipalities over the period between 1995 and 2014 (more than 8,000). It includes cities that have experienced territorial or administrative variations such as the constitution after the split of a pre-existing one, the legal administrative abolition due to the merge with other municipalities or the association to new or different provinces (NUTS4 level) or regions (NUTS3 level). The publication of any administrative-related information concerning Italian cities is provided by the Italian National Institute of Statistics (ISTAT) as well as data regarding the dimension and structure of the population (i.e. number of inhabitants, the share of young and old population and population density), which are based on census and inter-census surveys.

As for the issue of building permits, the analysis is based on administrative data collected each month by all Italian municipalities and then processed by ISTAT. Data includes detailed information as the intervention's total floor area (square meters) and the volume (cubic meters) authorised, the type of activities (creation of a new building or extension), the nature of the building (residential or non-residential) and its category of economic activity. The outcome of interest is the interventions' total floor area that has been approved on a monthly basis since the requested time to process an application is proportional to the complexity of a building project that can be proxied by its extension. The outcome is normalised to account for high variability (even within population classes) and also to obtain comparable coefficients across population classes.

The analysis of elections is based on the open-access public administrators' data provided by the Ministry of Internal Affairs. They are released every year and provide the composition of the executive committees, including date of the election, date of appointment and term-end, the date and place of birth for all the members. This data allows the construction of a term-limit dummy, electoral dummies for each month within n months before and after the electoral date and also a regular-term dummy for terms that ended at their original deadline (and not prematurely).

Homogeneous population classes are defined according to institutional rules (Gagliarducci and Nannicini, 2013) and homogeneity of the real estate market: they include cities below 5 thousands inhabitants, between 5-15, 15-50, 50-250 and beyond 250 (Table 3.1). More than 70% of the Italian cities is below 5,000 inhabitants and account for almost 11 million citizens, while the 12 biggest cities represent almost 8 million people. The smallest municipalities are averagely older, and with a lower level of population density than bigger cities, almost 60% of them is located in the north. The issue of building permits is an administrative activity with remarkable different intensity across cities' dimension: while municipalities with less than 5,000 inhabitants effectively release permits one month every four, the 12 biggest cities in Italy are active in more than 80% of the overall period. Considering the duration of electoral periods, the incidence of regular terms over the considered period is high and quite stable across cities' population classes, ranging from 92% of small municipalities to 85% of the biggest cities.

It is noteworthy to also look at the distribution of elections and released building permits throughout the considered period, and in particular how they are allocated over the calendar months. Elections at the local level are slightly centred around 1995 and the following full terms' end (i.e. 1999, 2004, 2009, 2014), with a similar pattern for the distribution of the term-limit dummy (Figure 3.1). The Ministry of Internal Affairs decides the date of the election according to the Law n.182/1991 that sets the electoral period between April 15 and June 15 of the term's final year if the term's deadline is within the first semester or, otherwise, in the same period but the following year. In case of anticipated end of the term with respect to the original deadline, election will take place in the same period of the current year if the event that have determined the need for a new election has happened before February 24, otherwise in the same period but the following year. For this reason, elections are concentrated between April and June (Figure 3.2), except for a limited number of extraordinary rounds occurred in other months as decided by the Ministry of Internal Affairs; the share of last term elections over the total number of electoral dates by calendar month is balanced.

Concerning the release of permits, the overall trend reaches a peak in the early 2000s and a progressive decline up to 2013 because of the long-lasting consequences of the crisis that prevent to restore the previous level of demand. The northern regions experience a higher level of released permits while they start suffering the effect of the recession before than the rest of Italy; nonetheless, the overall cycle is very similar across regions (Figure 3.3). The issue of permits is concentrated between March and July, while it registers a substantial fall in August due to the summer break that affects administrative activities, and a slowdown during winter (Figure 3.4).

3.5 Empirical strategy

The identification of a political cycle is based on the exogeneity of the election date. The Ministry of Internal Affairs decides upon it according to the Law, as described above, preventing the potential issue of opportunistic setting by incumbent mayors. I estimate the following model on a monthly panel data at municipal level with individual and time fixed effects:

$$y_{it} = \sum_{j=-18}^{+18} \alpha_j \text{month}_{jit} + \beta(L)y_{it-h} + \gamma X_i + \lambda_t + \eta_i + \epsilon_{it} \tag{3.1}$$

Where i identifies municipalities, t is current time in months, and y is the monthly floor area authorised by the issue of building permits, normalised to its mean and standard deviation over time at the city level, to account for the different level of variability registered across population dimensions[1]. The dummies *month* identifies the relative position of month t with respect to the election month (j=0), within a range of 18 months before and 18 months later than election. This specific time window is consistent with existing works analysing short-lived cycles within an electoral term (Akhmedov and Zhuravskaya, 2004). Since the dependent variable is sluggish, the model includes an optimal lag structure to account for residuals autocorrelation (8 lags according to the Akaike criterion).

The set of variables X includes controls[2] that vary on an annual basis as total population (thousands of inhabitants), its density (inhabitants/km2) and the share of young (%) and old population (%). The model includes municipalities-specific fixed effects (η_i) and monthly time fixed effects (λ_t) to control for trend and macroeconomic shocks. The standard errors account for possible error correlations at the municipal level.

To analyze the different incentive effect for the incumbent in the last term, the model specification is modified to include a dummy that takes value 1 for months that belong to the last terms (LT) e its interaction with dummies *month*, relaxing the assumption that the electoral cycle is constant through the term.

[1]The econometrics analysis has also been performed using an alternative definition of the outcome, i.e. according to an inverse hyperbolic sine transformation (Burbidge, Magee, and Robb, 1988), $ihs(x) = ln(x + \sqrt{x^2+1})$, that while being very similar to the logarithm transformation (as well as in terms of interpretation), allows the inclusion of zero values that are many in our data. Main results do not change.

[2]The use of electoral data as the margin of victory or the seat shares of political parties in the city council to account for political ideology has not been possible due to data limitation over the period of analysis.

$$y_{it} = \sum_{j=-18}^{+18} \alpha_j \text{month}_{jit} + \sum_{j=-18}^{+18} \delta_j \text{month}_{jit} \times \text{LT} + \theta \text{LT} + \beta(L) y_{it-h} + \gamma X_i + \lambda_t + \eta_i + \epsilon_{it} \quad (3.2)$$

Both models include static and dynamic specifications that are estimated by OLS. Although the presence of fixed effects and lags of the dependent variable causes biases when using a within-estimator, it vanishes as the number of periods of the panel is sufficiently large (Nickell, 1981), a condition satisfied by the considered time-frame (T=240 months). Alternative estimator for dynamic panels as the difference and system generalized-method-of-moments (GMM) are estimated and provide comparable estimates (Arellano and Bond, 1991; Blundell and Bond, 1998; Windmeijer, 2005; Roodman, 2009).

3.6 Results

Preliminary evidence about the occurrence of political cycle is shown by a set of t-tests on the average values of released building permits for each calendar months before and after an election: they are systematically and significantly higher in the pre-voting period (Table 3.2).

The static model is firstly estimated using all electoral months within a range of 18 months before and after the election date (Table 3.3, 1st column). There is a clear pattern of positive and significant estimates for all the five months before the election and negative and significant ones after four an five months that serve as evidence of the electoral cycle. Indeed, in the electoral month ($t = 0$) the release of permits is 0.062 standard deviations higher than the monthly average over the considered period (about +16%). The cycle is also remarkable at month $t = -1$ with an effect equals to 0.039 (+10%) and at month $t = -2$ with 0.025 (+6%). Overall, in the six months up to the election, including the voting month, the issue of permits is averagely higher by 0.028 standard deviations (+7%). Similarly, four and five months after the election the outcome is, respectively, -0.012 and -0.016 standard deviations lower than the monthly average value (-3% and -4%).

Since causal interpretation of time dummies relies on the exogeneity of electoral date, observations belonging to terms ended up prematurely are removed to avoid potential endogeneity (due to reverse causality between issuing of permits and the mayor or the executive committee step down). The model is then estimated considering only not-regular terms (Table 3.3, 2nd column): the cycle shrinks to an even stronger effect (+0.097) but limited to the election month only. Results of the baseline model including all observations hold when the sample is limited to all valid terms (Table 3.3, 3rd column): there are strong and similar estimates for six consecutive months up to the election, when the release of permits is 0.054 standard deviations (+14%) higher than its overall monthly average value and three significant negative coefficients over the post-electoral semester. The additive inclusion of controls as population, the share of young and old population and density do not affect the magnitude of the estimates (Table 3.3, 4th column). Results are also unchanged with the dynamic version of the model[3] which includes an optimal lag structure (Akaike criterion), i.e. 8 periods (Table 3.3, 5th column and Figure 3.5 for a visual representation[4]).

[3]Similar estimates are also obtained with the inclusion of an additional term for province seasonality: results on opportunistic cycle hold using both city and monthly time fixed effects and the interaction between provinces' identifiers and dummies for each of the twelve calendar months.

[4]From now on model's estimates are visually represented to improve results readability.

The analysis across population-classes shows that the cycle is mainly driven by small municipalities, in particular, those below 15,000 inhabitants, that are characterised by a significant electoral cycle with the stronger effect registered in the election month (Table 3.4 and Figure 3.6 for a visual representation). Indeed, for municipalities below 5,000 inhabitants, seven out of twelve months before election feature a positive and significant deviation beyond the average monthly value, while cities between 5,000 and 15,000 show significant coefficients for five months in the year preceding the election (Figure 3.6, first two sub-figures from the top). A systematic positive deviation from the trend is also concentrated in the 2nd and 4th months before the election for cities between 15,000 and 50,000 inhabitants, while the analysis reveals some unexpected positive coefficients one year after the election (Figure 3.6, middle sub-figure). Municipalities between 50 and 250,000 show a more complex pattern. Here, the issuing of permits does decrease significantly just after the election; however there is no evidence of symmetrical increase before the election and the reduction one year before voting could be unrelated to any opportunist cycle argument (Figure 3.6, second sub-figure from below). The twelve biggest cities show no evidence of deviation for the incumbents' behaviour (Figure 3.6, first sub-figure from below).

The lack of clear evidence for bigger municipalities may be due to the the higher workload the administrative unit has to deal with (see the intensity of permits release, Table 3.1, 1st panel), and its more complex and larger organization, that make the manipulation to be difficult and not easily targetable to the period just before the election. Moreover, bigger cities usually feature higher level of awareness and civic participation among citizens (since the higher average level of education) and because the stronger presence of local media that may reduce the asymmetry of information regarding the incumbents' competence vs manipulation, whose existence is needed for the cycle.

The analysis of heterogeneous effects related to geographic location shows the presence of an electoral cycle both for North and South of Italy, while municipalities in the Centre's regions do not show any clear pattern but a systematic decrease five and eighteen months after the election (Table 3.4 and Figure 3.7 for a visual representation).

The analysis of the incentive effect of term-limit rule supports the hypothesis of opportunistic behaviour of mayors rather than a reputation argument. Indeed, estimates of equation (3.2) clearly show how the incentive effect of possible re-election makes room for political cycle (Table 3.5, 1st column, for monthly coefficients only), while there is no systematic effect in the last term (Table 3.6, 1st column, for monthly coefficients interacted with the last-term dummy). Figure 3.8 provides visual representation of the two effects. Moreover, these estimates reject any possible claim that electoral cycle could be driven by the demand of constructors who, being averse to the uncertainty stemming from political change and potential post-electoral variations to land use regulation (including requirements for issuing permits), may decide to apply just before the election.

The analysis of heterogenous effects (Table 3.5 and Table 3.6) relative to population classes or geographic location support the general patterns commented above, i.e. stronger magnitude for smaller municipalities and for cities in the North and South of Italy (respectively, Figure 3.9 and Figure 3.10 for visual representations). Interestingly, disentangling general and last-term effects, allows to show the presence of cycle when the mayors can run for re-election for cities in the Centre.

To prevent the evidence in favour of the existence of the cycle to be driven by electoral periods overlapping with months traditionally characterized by higher demand of permits (and then an higher intensity of issuing by municipalities), the model is estimated considering electoral events occurring in April, May and June separately (Table 3.7, for monthly coefficients only and Table 3.8, for monthly coefficients interacted with the last-term dummy): the visual analysis confirms both findings on electoral cycle and last-term effects as commented above (Figure 3.11 for a visual representation). Moreover, to disentangle the incidence of political cycle over different time horizons, the sample is divided in three periods: from 1995 to 2000 (before regime change in the domestic stability pact that affects fiscal rules for municipalities), from 2001 to 2007 (before the crisis) and from 2008 to 2014 (after the crisis) (Table 3.7 and Table 3.8, last three columns). Results are mainly confirmed for the first and last periods, while the cycle is limited to the electoral month when we consider the period before the crisis (Figure 3.12 for a visual representation).

3.7 Conclusions

This paper shows the existence of an opportunistic electoral cycle in the administrative process of issuing building permits at the local level in Italy from 1995 to 2014. The analysis is based on the large monthly panel of the released building authorisations for the universe of the Italian municipalities (more than 8,000). A within-term analysis, using a panel data model with city and month fixed effects, provides evidence of an opportunistic incumbent behaviour in the first term and the lack of reputation effect of election. Mayors tend to exert pressure on administrative offices to ease or speed-up the issue of permits: this may create immediate and targeted benefits for voters, reducing uncertainty and delay. Moreover, voters cannot distinguish between competence and manipulation as far as administrative processes are concerned, making the cycle stronger.

Result show that, when mayors run for re-election, the release of permits increases by 0.011 to 0.062 standard deviations from the average monthly value over the considered period, i.e. from +3% to +16%. This effect is registered mainly within twelve months before the election, and it is stronger the closer the voting month. The effect is clear and significant for municipalities below 15,000 inhabitants which account for 90% of all the Italian cities and more than 25 million of citizens (40% of the total population). Indeed, manipulating the release of permits may be difficult for bigger cities because of the higher complexity of this administrative process, the presence of media and higher awareness of citizens. The existence of an opportunistic behaviour for incumbent mayors is detected both in the northern and southern regions.

The successful detection of an electoral cycle at municipal level may be explained by the focus on a non-fiscal outcome that is more easily controllable by politicians. Besides this, the use of high-frequency data that clearly shows how the monthly increase of the outcome just before an election, and the following reduction after voting, would offset each other if using annual data.

The study of a homogenous context has allowed to control for unobservable characteristics that may potentially threaten the identification of causal effects, making estimates more credible. The reliability of results has also benefited from the possibility to use year-month effect from pure time effects exploiting non-simultaneous elections staggered over 20 years.

3.8 Tables and figures

Table 3.1: Descriptives by population classes

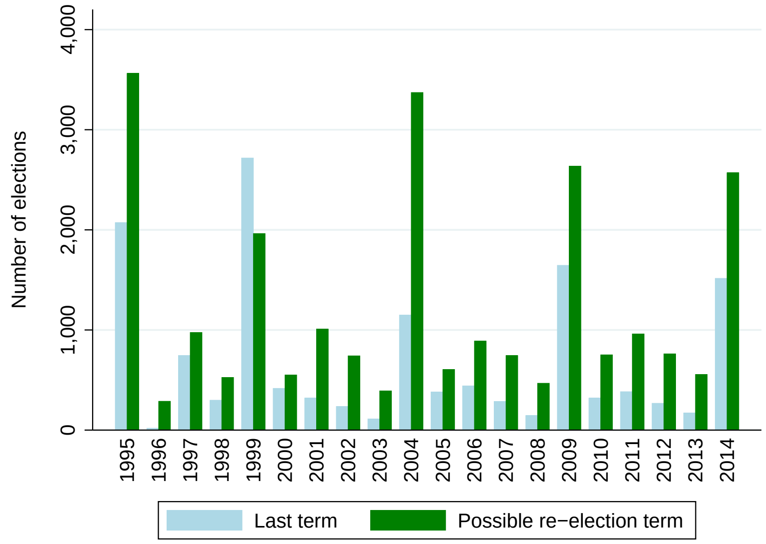
	All	0-5	5-15	15-50	50-250	250+
Building permits						
Released building permits (m^2)	506.9	183.9	836.5	1707.4	4013.0	17064.2
	(1326.5)	(208.1)	(557.5)	(1055.1)	(2983.3)	(22313.9)
Intensity of release (% of months)	0.375	0.266	0.601	0.722	0.776	0.830
	(0.248)	(0.174)	(0.182)	(0.179)	(0.220)	(0.223)
Population						
Number of inhabitants (thousands)	7.214	1.821	8.459	25.56	85.85	742.6
	(39.42)	(1.268)	(2.711)	(9.049)	(40.31)	(670.0)
% of young population (<15years)	0.134	0.129	0.147	0.150	0.144	0.129
	(0.0276)	(0.0272)	(0.0227)	(0.0271)	(0.0287)	(0.0248)
% of old population (>65years)	0.215	0.229	0.183	0.177	0.189	0.216
	(0.0603)	(0.0622)	(0.0378)	(0.0399)	(0.0420)	(0.0391)
Population density (inhab/Km^2)	293.2	133.4	483.3	1075.6	1323.0	3612.9
	(639.1)	(224.3)	(588.1)	(1418.2)	(1893.7)	(2470.7)
Geographics						
North	0.552	0.577	0.525	0.419	0.323	0.500
	(0.497)	(0.494)	(0.500)	(0.494)	(0.470)	(0.522)
Centre	0.125	0.112	0.138	0.198	0.195	0.167
	(0.331)	(0.316)	(0.345)	(0.399)	(0.398)	(0.389)
South	0.324	0.310	0.337	0.382	0.481	0.333
	(0.468)	(0.463)	(0.473)	(0.486)	(0.502)	(0.492)
Elections						
Regular terms (% of months)	0.899	0.924	0.876	0.751	0.716	0.853
	(0.165)	(0.133)	(0.182)	(0.252)	(0.237)	(0.174)
Last terms (% of months)	0.349	0.374	0.308	0.240	0.245	0.281
	(0.194)	(0.190)	(0.188)	(0.185)	(0.181)	(0.138)
Regular 1st terms (% of months)	0.565	0.563	0.582	0.540	0.543	0.660
	(0.189)	(0.188)	(0.186)	(0.199)	(0.186)	(0.114)
Regular 2nd terms (% of months)	0.264	0.275	0.257	0.192	0.163	0.193
	(0.165)	(0.160)	(0.171)	(0.170)	(0.174)	(0.189)
Regular 3rd terms (% of months)	0.0524	0.0636	0.0298	0.0153	0.00950	0
	(0.0987)	(0.105)	(0.0786)	(0.0583)	(0.0484)	(0)
Regular 4th terms (% of months)	0.0186	0.0237	0.00727	0.00367	0	0
	(0.0703)	(0.0789)	(0.0443)	(0.0307)	(0)	(0)
Observations	8299	5891	1698	565	133	12

mean coefficients; sd in parentheses

Notes. Average values over 1995-2014 (240 months). Population classes in thousands of inhabitants. *Intensity of permits release* is the ratio bwteen the number of month in which permits has been effectively released over the total number of considered months. *Regular terms* is the ratio between the number of month belonging to electoral term ended regularly (at their natural closing date) over the total number of considered months. Similar interpretation for electoral variables defined as a share of months. Sources: Ministry of Internal Affairs, ISTAT

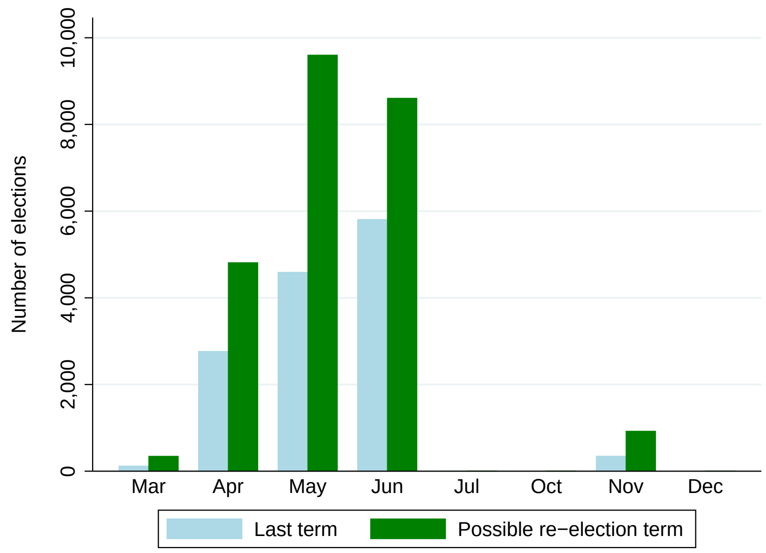
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 3.1: Electoral dates by year and binding terms



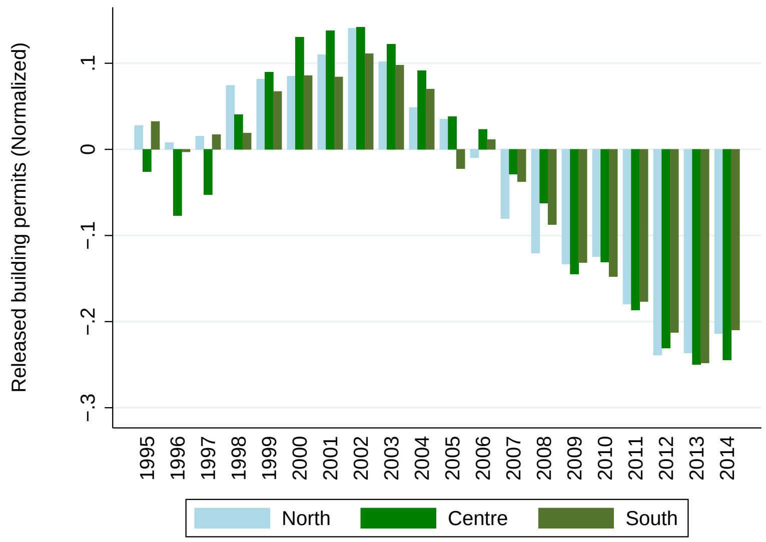
Notes. Absolute number of elections. Last-term elections when the incumbent mayor cannot run for re-election.

Figure 3.2: Electoral months by calendar month and binding terms



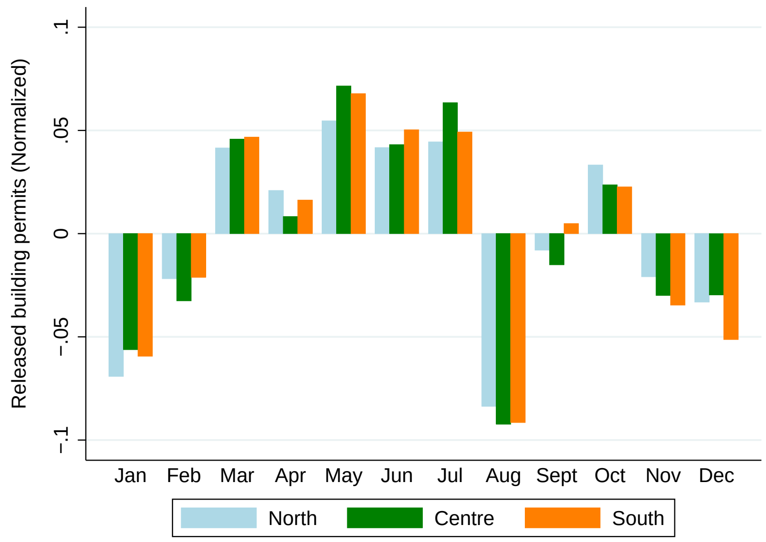
Notes. Absolute number of elections. Last-term elections when the incumbent mayor cannot run for re-election.

Figure 3.3: Released building permits by year over geographic location



Notes. Building permits as total floor area authorized to be built or extended (m^2, normalized , mean=0, sd=1)

Figure 3.4: Released building permits by calendar month over geographic location



Notes. Building permits as total floor area authorized to be built or extended (m^2, normalized, mean=0, sd=1)

Table 3.2: Realised building permits: descriptives and two-sample t-tests

	Before election	After election	Δ	Std.Err	T-test (p)	Obs
January	-0.056	-0.073	0.018***	0.006	0.003	101,301
February	-0.010	-0.029	0.019***	0.006	0.003	101,290
March	0.069	0.037	0.032***	0.007	0.000	101,342
April	0.038	0.020	0.018***	0.007	0.006	104,298
May	0.095	0.070	0.025***	0.007	0.001	101,113
June	0.065	0.049	0.016**	0.007	0.021	100,042
July	0.066	0.051	0.014*	0.007	0.054	100,262
August	-0.068	-0.103	0.035***	0.006	0.000	100,298
September	0.008	0.000	0.008	0.007	0.234	100,753
October	0.047	0.022	0.025***	0.007	0.000	103,194
November	-0.004	-0.041	0.037***	0.006	0.000	109,101
December	-0.030	-0.041	0.011*	0.006	0.055	111,458

Notes. Average value of released building permits (m^2, normalized, mean=0, sd=1) by calendar month before and after an election

Table 3.3: Electoral cycle in realising building permits at local level

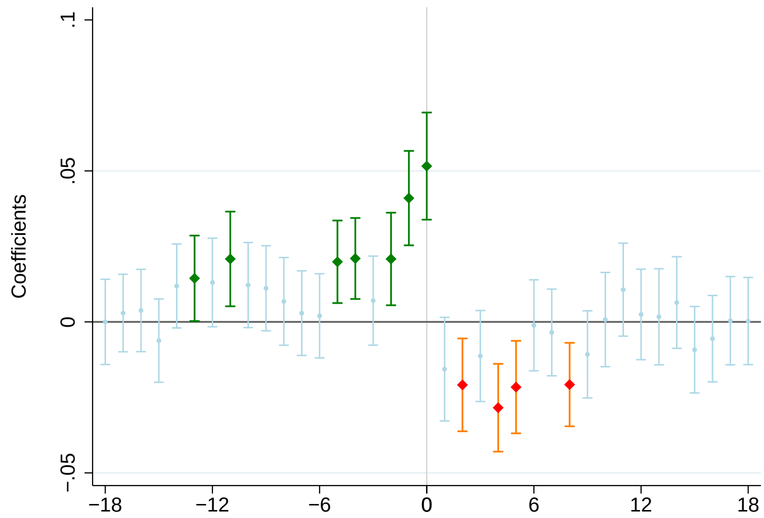
	Electoral terms			Regular terms	
	(1)	(2)	(3)	(4)	(5)
	All	Not regular	Regular	With covariates	Dynamic
month -6	2.9e-03	-.02	5.1e-03	5.4e-03	1.9e-03
	(0.651)	(0.238)	(0.477)	(0.451)	(0.790)
month -5	.016**	-1.3e-03	.02***	.02***	.02***
	(0.013)	(0.944)	(0.005)	(0.004)	(0.004)
month -4	.019***	6.0e-03	.022***	.022***	.021***
	(0.002)	(0.744)	(0.001)	(0.002)	(0.002)
month -3	.011*	4.7e-03	.014*	.013*	7.1e-03
	(0.088)	(0.791)	(0.066)	(0.069)	(0.347)
month -2	.025***	.028	.025***	.025***	.022***
	(0.000)	(0.151)	(0.001)	(0.001)	(0.006)
month -1	.039***	.023	.041***	.041***	.041***
	(0.000)	(0.237)	(0.000)	(0.000)	(0.000)
month 0	.062***	.097***	.054***	.054***	.051***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
month 1	-6.5e-03	7.3e-03	-.014	-.014*	-.016*
	(0.357)	(0.704)	(0.105)	(0.088)	(0.069)
month 2	-.01	-3.7e-03	-.016**	-.016**	-.021***
	(0.125)	(0.839)	(0.037)	(0.042)	(0.007)
month 3	-5.6e-04	.021	-7.9e-03	-8.4e-03	-.012
	(0.930)	(0.232)	(0.291)	(0.265)	(0.120)
month 4	-.012*	.029	-.027***	-.028***	-.029***
	(0.052)	(0.154)	(0.000)	(0.000)	(0.000)
month 5	-.016**	-.017	-.023***	-.023***	-.022***
	(0.014)	(0.384)	(0.004)	(0.004)	(0.006)
month 6	-2.0e-03	-.016	-2.8e-03	-2.9e-03	-1.5e-03
	(0.767)	(0.420)	(0.721)	(0.712)	(0.850)
R^2	0.016	0.016	0.013	0.014	0.028
Obs	1,956,930	194,356	1,718,402	1,714,544	1,655,857
N. Cities	8349	3711	8298	8270	8270
Cities F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	Yes	Yes
Lags	No	No	No	No	8

p-values in parentheses

Note. Estimates of electoral cycle effects within 18 months before and after the election (panel data model with individual and time fixed effects). Method: OLS. The outcome is the total floor area authorized with the building permits released within a calendar month (normalized). Standard errors are robust and clustered at the city level. Complete estimates of all the time dummies are visually provided in Figure 3.5.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Figure 3.5: Electoral cycle



Notes. Scatterplot of electoral cycle treatments over time (see Table 3.3, 5th column for model specification). Y-axis: Regression coefficients of time dummies with confidence interval at 95%. X-axis: months before and after the electoral month (t=0). Green solid range (for positive and significant estimates) on the left and orange solid range (for negative and significant estimates) on the right serve as evidence of electoral cycle.

Table 3.4: Electoral cycle in realising building permits at local level

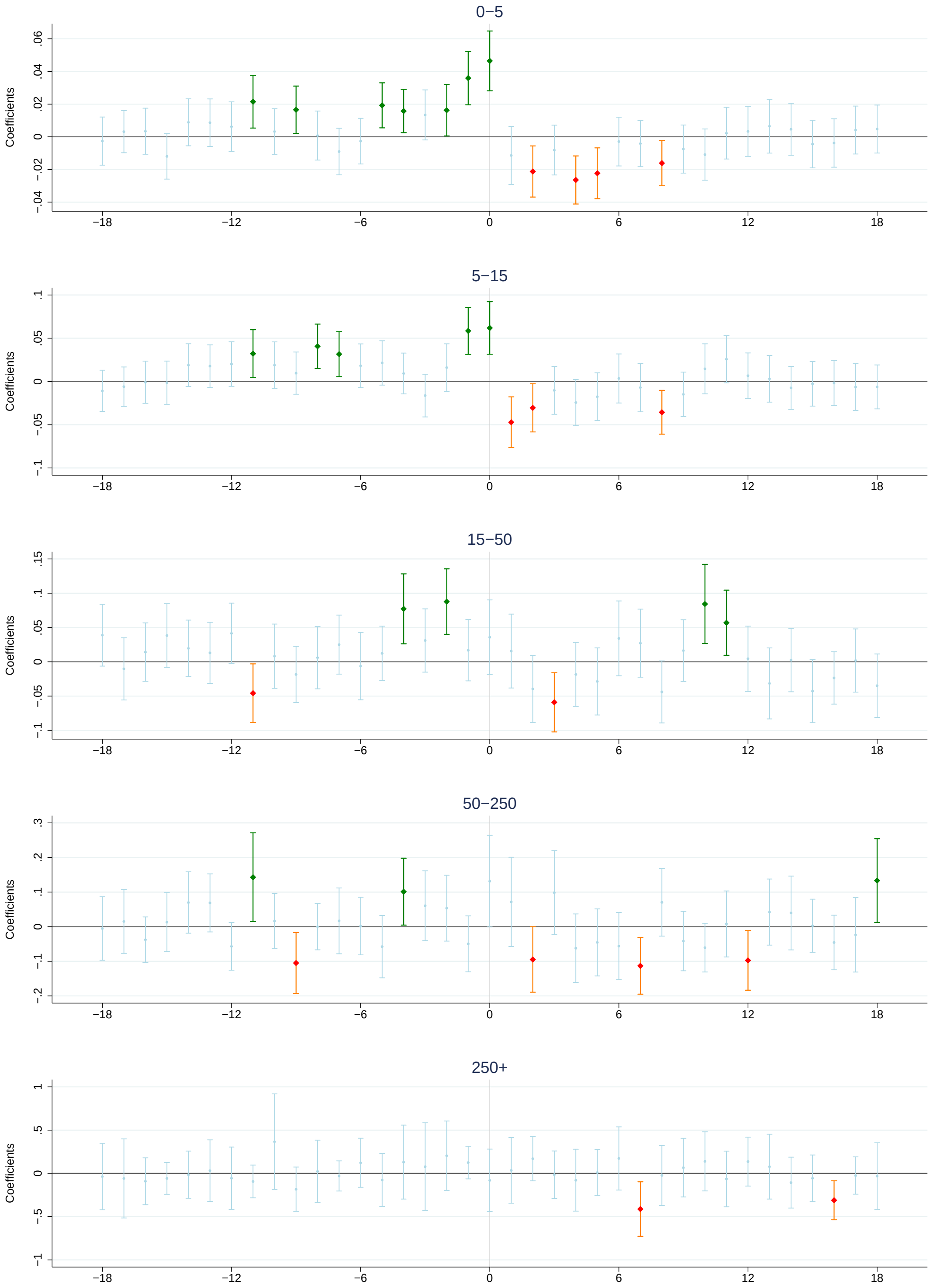
	Population classes					Geographics		
	(1) 0-5	(2) 5-15	(3) 15-50	(4) 50-250	(5) 250+	(6) North	(7) Centre	(8) South
month -6	-2.7e-03	.018	-6.3e-03	1.8e-03	.122	.021**	-.036	-.017
	(0.753)	(0.236)	(0.833)	(0.971)	(0.456)	(0.043)	(0.124)	(0.134)
month -5	.019**	.021	.012	-.058	-.077	.024**	5.5e-03	.02*
	(0.021)	(0.169)	(0.607)	(0.291)	(0.664)	(0.024)	(0.798)	(0.090)
month -4	.016*	9.2e-03	.077**	.101*	.13	.018*	.036*	.027**
	(0.050)	(0.520)	(0.013)	(0.085)	(0.597)	(0.079)	(0.078)	(0.016)
month -3	.013	-.016	.031	.061	.078	5.4e-03	-4.7e-03	.017
	(0.150)	(0.277)	(0.268)	(0.322)	(0.789)	(0.622)	(0.846)	(0.164)
month -2	.016*	.016	.088***	.054	.204	.033***	.023	7.3e-03
	(0.089)	(0.336)	(0.003)	(0.353)	(0.382)	(0.005)	(0.319)	(0.556)
month -1	.036***	.058***	.017	-.05	.125	.055***	.015	.04***
	(0.000)	(0.000)	(0.534)	(0.312)	(0.261)	(0.000)	(0.520)	(0.003)
month 0	.046***	.062***	.036	.132	-.081	.06***	.026	.056***
	(0.000)	(0.001)	(0.276)	(0.102)	(0.698)	(0.000)	(0.326)	(0.000)
month 1	-.011	-.047***	.016	.072	.035	-.023*	.018	-.011
	(0.292)	(0.008)	(0.633)	(0.359)	(0.873)	(0.063)	(0.507)	(0.464)
month 2	-.021**	-.03*	-.04	-.095*	.17	-.018	.011	-.033**
	(0.026)	(0.072)	(0.184)	(0.100)	(0.259)	(0.123)	(0.631)	(0.011)
month 3	-8.1e-03	-.01	-.059**	.098	-.015	-.017	.022	-.015
	(0.381)	(0.541)	(0.025)	(0.182)	(0.923)	(0.119)	(0.360)	(0.247)
month 4	-.026***	-.024	-.018	-.062	-.079	-.024**	-4.0e-03	-.052***
	(0.003)	(0.133)	(0.516)	(0.301)	(0.701)	(0.026)	(0.864)	(0.000)
month 5	-.022**	-.018	-.029	-.045	.01	-.028**	-.051**	-9.6e-03
	(0.018)	(0.295)	(0.336)	(0.440)	(0.946)	(0.014)	(0.016)	(0.467)
month 6	-2.9e-03	3.5e-03	.034	-.056	.173	-.011	.017	-4.2e-03
	(0.750)	(0.840)	(0.302)	(0.342)	(0.415)	(0.316)	(0.483)	(0.737)
R^2	0.020	0.050	0.066	0.106	0.198	0.024	0.039	0.035
Obs	1,212,313	325,945	94,584	20,882	2,350	960,376	207,704	487,994
N. Cities	6035	1871	631	145	13	4571	1034	2666
Cities F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lags	8	8	8	8	8	8	8	8

p-values in parentheses

Note. Estimates of electoral cycle effects within 18 months before and after the election by population classes and geographic location (panel data model with individual and time fixed effects). Method: OLS. The outcome is the total floor area authorized with the building permits released within a calendar month (normalized). Standard errors are robust and clustered at the city level. Complete estimates of all the time dummies are visually provided in Figure 3.6 and Figure 3.7

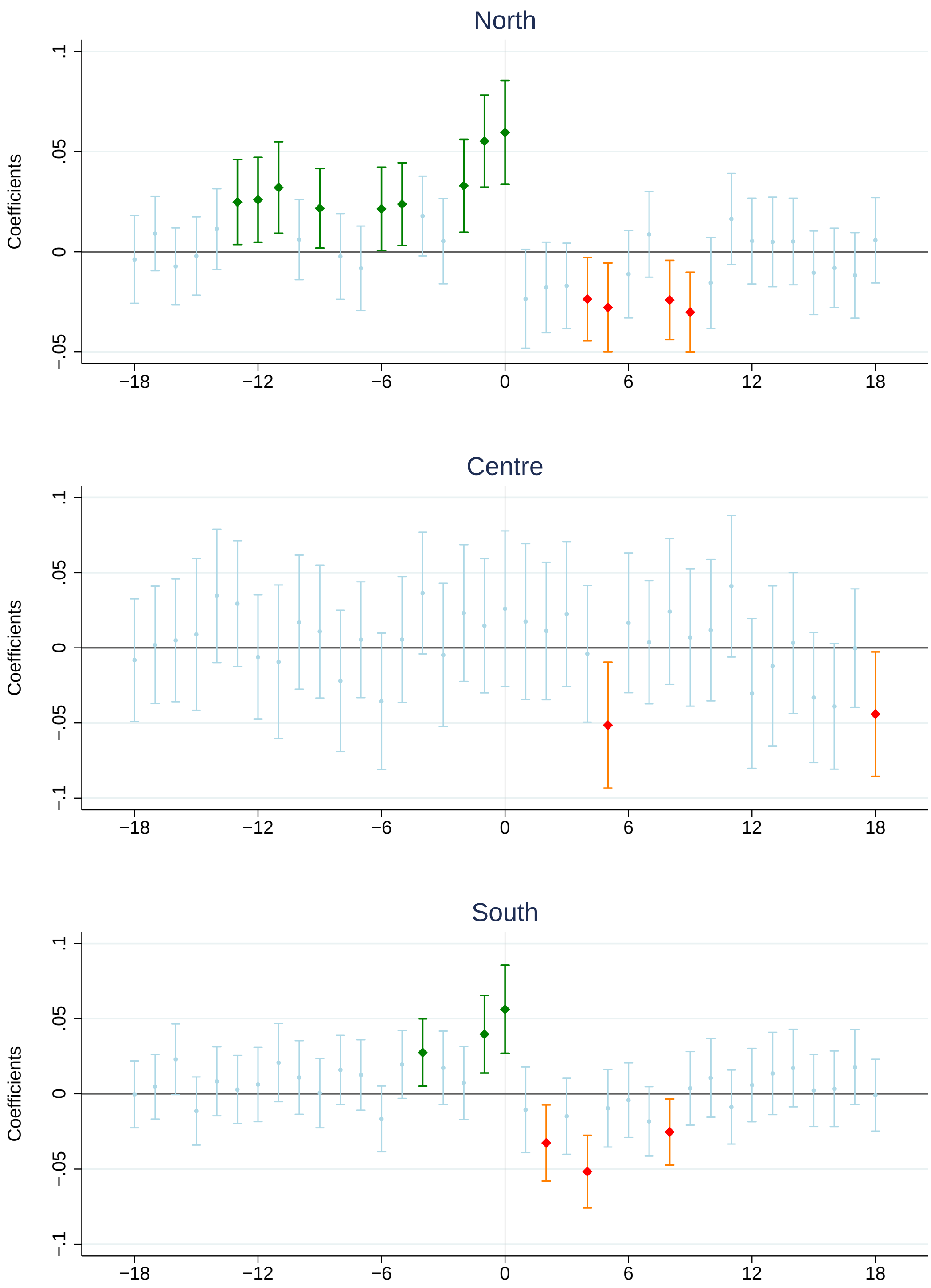
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Figure 3.6: Electoral cycle by population classes



Notes. Scatterplot of electoral cycle treatments over time by population classes (see Table 3.4 for model specification). Y-axis: Regression coefficients of time dummies with confidence interval at 95%. X-axis: months before and after the electoral month (t=0). Green solid range (for positive and significant estimates) on the left and orange solid range (for negative and significant estimates) on the right serve as evidence of electoral cycle.

Figure 3.7: Electoral cycle by geographic location



Notes. Scatterplot of electoral cycle treatments over time by population classes (see Table 3.4 for model specification). Y-axis: Regression coefficients of time dummies with confidence interval at 95%. X-axis: months before and after the electoral month (t=0). Green solid range (for positive and significant estimates) on the left and orange solid range (for negative and significant estimates) on the right serve as evidence of electoral cycle.

Table 3.5: Electoral cycle and last term limit in realising building permits by population classes: general cycle

	All	Population classes					Geographics		
		0-5	5-15	15-50	50-250	250+	North	Centre	South
month -6	-3.0e-03	-8.9e-03	.013	-4.6e-03	.011	.031	.023*	-.027	-.032***
	(0.704)	(0.349)	(0.450)	(0.889)	(0.855)	(0.851)	(0.058)	(0.295)	(0.008)
month -5	.021***	.022**	.02	.01	-.091*	-.013	.031**	-4.8e-03	.015
	(0.010)	(0.022)	(0.257)	(0.712)	(0.086)	(0.955)	(0.011)	(0.840)	(0.256)
month -4	.021***	.017*	3.9e-03	.083**	.101	.085	.015	.06**	.023*
	(0.008)	(0.068)	(0.811)	(0.022)	(0.124)	(0.762)	(0.197)	(0.015)	(0.068)
month -3	.01	.018*	-.017	.047	.056	.152	6.2e-03	.016	.015
	(0.237)	(0.099)	(0.297)	(0.144)	(0.392)	(0.690)	(0.625)	(0.568)	(0.280)
month -2	.022**	.017	.017	.098***	.03	.249	.038***	.043	-4.0e-03
	(0.013)	(0.128)	(0.364)	(0.004)	(0.612)	(0.329)	(0.005)	(0.116)	(0.771)
month -1	.047***	.042***	.077***	.012	-.069	.045	.056***	.039	.046***
	(0.000)	(0.000)	(0.000)	(0.696)	(0.223)	(0.803)	(0.000)	(0.152)	(0.002)
month 0	.053***	.058***	.046**	-5.4e-03	.138	-.174	.059***	.025	.063***
	(0.000)	(0.000)	(0.033)	(0.883)	(0.151)	(0.319)	(0.000)	(0.414)	(0.001)
month 1	-.02*	-.017	-.056***	.046	.035	.199	-.023	.015	-.024
	(0.052)	(0.173)	(0.007)	(0.233)	(0.633)	(0.450)	(0.123)	(0.615)	(0.142)
month 2	-.021**	-.02*	-.046**	-7.0e-03	-.07	.314	-.019	.028	-.037**
	(0.022)	(0.085)	(0.013)	(0.851)	(0.318)	(0.164)	(0.154)	(0.309)	(0.016)
month 3	-.014	-8.8e-03	-.018	-.047	.075	-4.0e-03	-.018	.024	-.024
	(0.120)	(0.420)	(0.384)	(0.138)	(0.284)	(0.985)	(0.170)	(0.379)	(0.123)
month 4	-.023**	-.024**	-.015	9.2e-03	-.027	.064	-.018	-7.7e-03	-.043***
	(0.010)	(0.023)	(0.466)	(0.794)	(0.716)	(0.810)	(0.153)	(0.782)	(0.004)
month 5	-.022**	-.025**	-.014	-.025	-.033	3.0e-03	-.03**	-.053**	-8.6e-03
	(0.017)	(0.028)	(0.502)	(0.489)	(0.621)	(0.982)	(0.023)	(0.035)	(0.587)
month 6	-4.5e-03	-.013	.021	.023	-.011	.194	-.022*	.04	-3.5e-03
	(0.616)	(0.225)	(0.298)	(0.558)	(0.881)	(0.494)	(0.082)	(0.157)	(0.816)
R^2	0.028	0.020	0.050	0.066	0.108	0.212	0.024	0.039	0.036
Obs	1,656,074	1,212,313	325,945	94,584	20,882	2,350	960,376	207,704	487,994
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lags	8	8	8	8	8	8	8	8	8
Cities	8271	6035	1871	631	145	13	4571	1034	2666

p-values in parentheses

Note. Estimates of electoral cycle and term limit effects within 18 months before and after the election by population classes and geographic location (panel data model with individual and time fixed effects). Method: OLS. The outcome is the total floor area authorized with the building permits released within a calendar month (normalized). Standard errors are robust and clustered at the city level. Complete estimates of all the time dummies are visually provided in Figure 3.8 (for all cities), Figure 3.9 (by population classes) and Figure 3.10 (by geographic location)

* p<0.10, ** p<0.05, *** p<0.01

Table 3.6: Electoral cycle and last term limit in realising building permits by population classes: Lat term

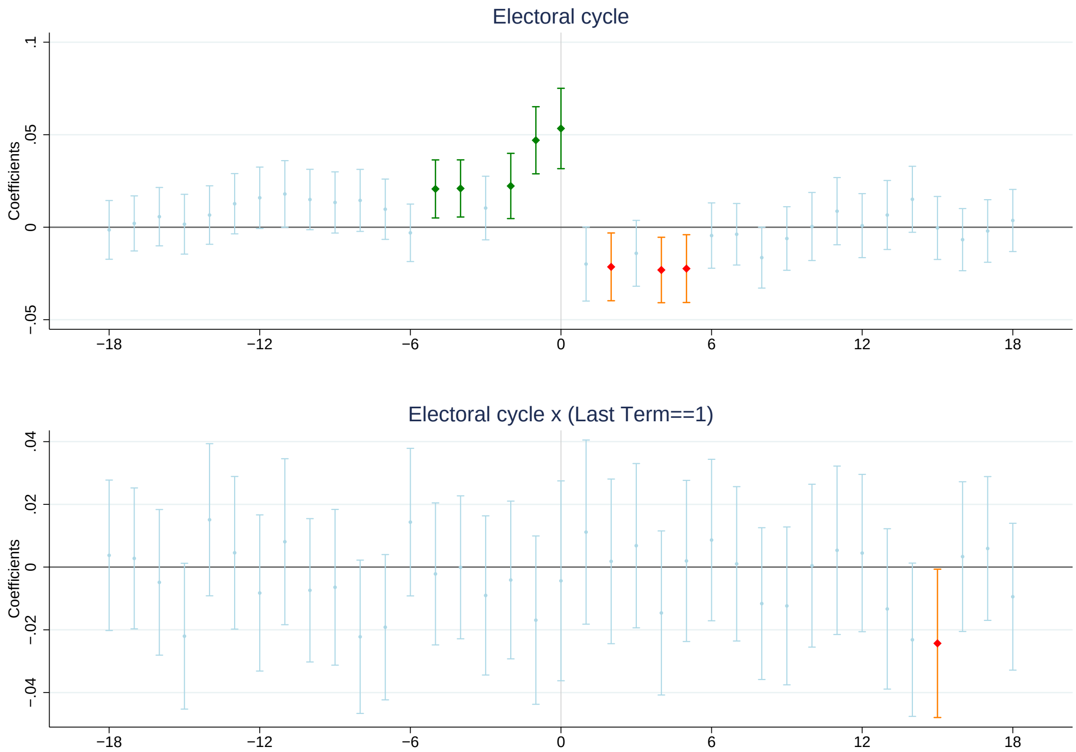
	All	Population classes					Geographics		
		0-5	5-15	15-50	50-250	250+	North	Centre	South
month -5 x LT	-2.2e-03	-7.5e-03	6.1e-03	9.5e-03	.156	-.334	-.018	.028	.017
	(0.851)	(0.570)	(0.831)	(0.842)	(0.247)	(0.324)	(0.226)	(0.372)	(0.493)
month -4 x LT	-7.5e-05	-3.0e-03	.018	-.024	-1.5e-03	.195	6.6e-03	-.064**	.015
	(0.995)	(0.823)	(0.491)	(0.643)	(0.990)	(0.712)	(0.661)	(0.026)	(0.542)
month -3 x LT	-9.0e-03	-.012	4.1e-03	-.06	.018	-.408	-1.3e-03	-.056	6.6e-03
	(0.485)	(0.431)	(0.883)	(0.282)	(0.897)	(0.325)	(0.941)	(0.111)	(0.791)
month -2 x LT	-4.1e-03	-1.4e-03	-3.5e-03	-.04	.113	-.19	-.013	-.054	.039
	(0.748)	(0.923)	(0.907)	(0.516)	(0.405)	(0.625)	(0.420)	(0.117)	(0.141)
month -1 x LT	-.017	-.016	-.058**	.024	.098	.376	-3.8e-03	-.067*	-.022
	(0.217)	(0.339)	(0.041)	(0.669)	(0.372)	(0.443)	(0.830)	(0.052)	(0.428)
month 0 x LT	-4.4e-03	-.028	.043	.129*	-.022	.325	1.6e-03	7.5e-04	-.022
	(0.787)	(0.142)	(0.231)	(0.079)	(0.878)	(0.671)	(0.943)	(0.986)	(0.461)
month 1 x LT	.011	.014	.024	-.097*	.126	-.578*	-4.2e-04	6.5e-03	.04
	(0.456)	(0.413)	(0.463)	(0.097)	(0.489)	(0.068)	(0.982)	(0.883)	(0.176)
month 2 x LT	1.8e-03	-3.5e-03	.044	-.1*	-.087	-.497*	4.2e-03	-.043	.014
	(0.891)	(0.821)	(0.161)	(0.068)	(0.468)	(0.074)	(0.809)	(0.255)	(0.598)
month 3 x LT	6.8e-03	1.1e-03	.021	-.037	.084	-.062	2.4e-04	-4.9e-03	.026
	(0.609)	(0.943)	(0.480)	(0.420)	(0.578)	(0.812)	(0.989)	(0.903)	(0.278)
month 4 x LT	-.015	-6.0e-03	-.028	-.091*	-.125	-.527	-.014	9.8e-03	-.025
	(0.273)	(0.698)	(0.378)	(0.089)	(0.272)	(0.117)	(0.409)	(0.826)	(0.290)
month 5 x LT	2.0e-03	5.9e-03	-.012	-.012	-.051	-.023	6.4e-03	2.3e-03	-3.6e-03
	(0.881)	(0.703)	(0.675)	(0.825)	(0.641)	(0.955)	(0.716)	(0.945)	(0.883)
last term	5.9e-03	-.017	.051	.15**	-.015	.325	.014	.017	-.017
	(0.710)	(0.370)	(0.157)	(0.038)	(0.912)	(0.657)	(0.499)	(0.692)	(0.546)
R^2	0.028	0.020	0.050	0.066	0.108	0.212	0.024	0.039	0.036
Obs	1,656,074	1,212,313	325,945	94,584	20,882	2,350	960,376	207,704	487,994
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lags	8	8	8	8	8	8	8	8	8
Cities	8271	6035	1871	631	145	13	4571	1034	2666

p-values in parentheses

Note. Estimates of electoral cycle and term limit effects within 18 months before and after the election by population classes and geographic location (panel data model with individual and time fixed effects). Method: OLS. The outcome is the total floor area authorized with the building permits released within a calendar month (normalized). Standard errors are robust and clustered at the city level. Complete estimates of all the time dummies are visually provided in Figure 3.8 (for all cities), Figure 3.9 (by population classes) and Figure 3.10 (by geographic location)

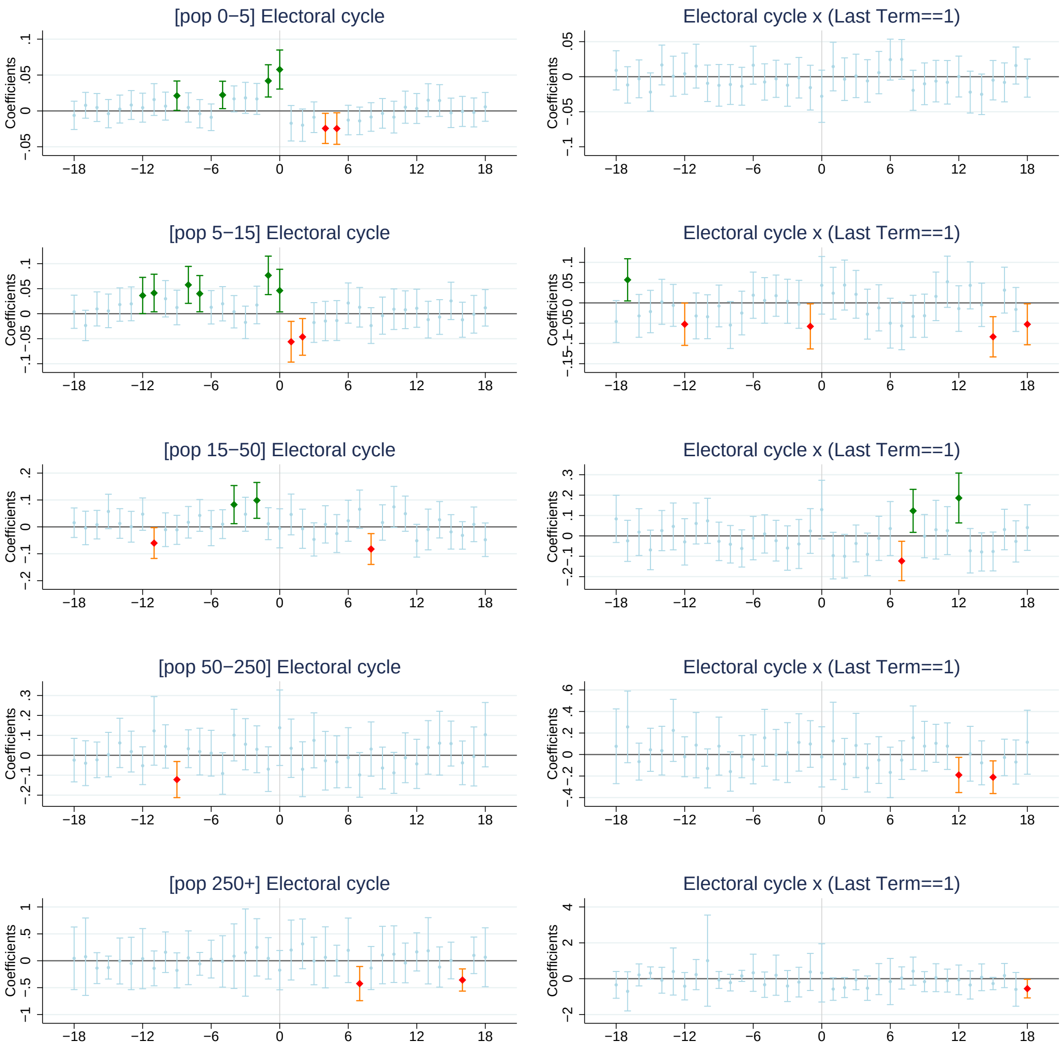
* p<0.10, ** p<0.05, *** p<0.01

Figure 3.8: Electoral cycle and term limit



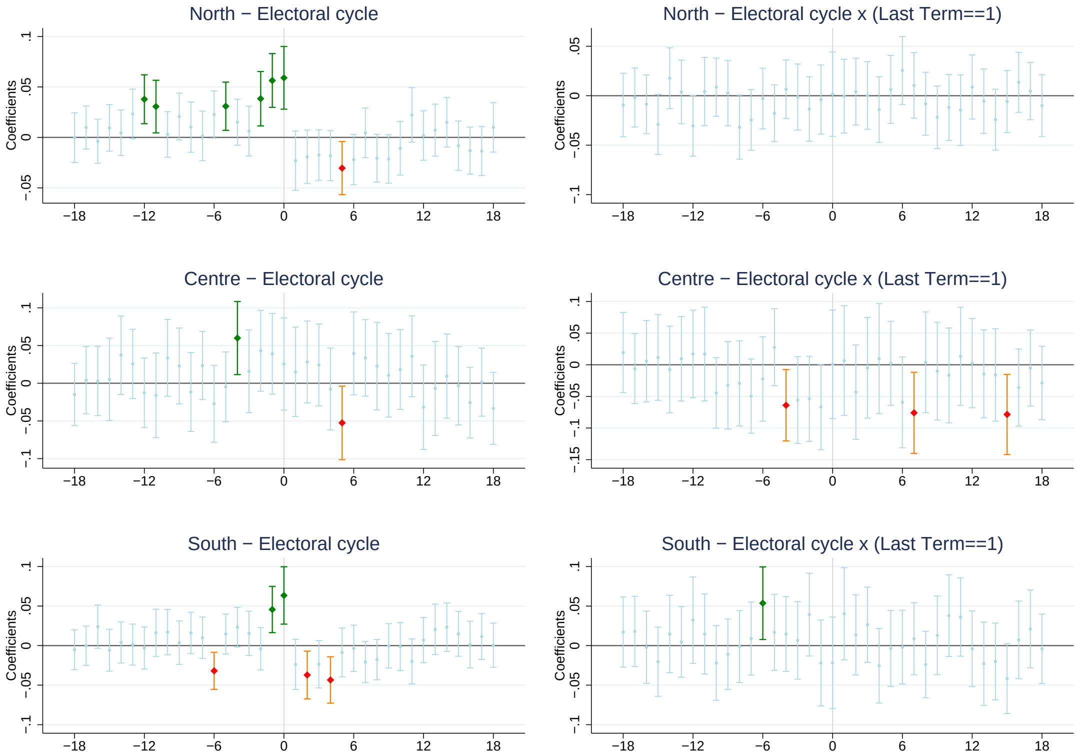
Notes. Scatterplot of electoral cycle and term limit effect (see Table 3.5 and Table 3.6 for model specification). Y-axis: Regression coefficients of time dummies with confidence interval at 95%. X-axis: months before and after the electoral month (t=0). Green solid range (for positive and significant estimates) on the left and orange solid range (for negative and significant estimates) on the right serve as evidence of electoral cycle.

Figure 3.9: Electoral cycle and term limit by population classes



Notes. Scatterplot of electoral cycle and term limit treatments over time by population classes (see Table 3.5 and Table 3.6 for model specification). Y-axis: Regression coefficients of time dummies with confidence interval at 95%. X-axis: months before and after the electoral month (t=0). Green solid range (for positive and significant estimates) on the left and orange solid range (for negative and significant estimates) on the right serve as evidence of electoral cycle.

Figure 3.10: Electoral cycle and term limit by geographic location



Notes. Scatterplot of electoral cycle and term limit treatments over time by geographic location (see Table 3.5 and Table 3.6 for model specification). Y-axis: Regression coefficients of time dummies with confidence interval at 95%. X-axis: months before and after the electoral month (t=0). Green solid range (for positive and significant estimates) on the left and orange solid range (for negative and significant estimates) on the right serve as evidence of electoral cycle.

Table 3.7: Electoral cycle and last term limit in realising building permits by population classes: general term

	Election months			Periods		
	April	May	June	1995-2000	2001-2007	2008-2014
month -6	-.01	-8.5e-03	9.1e-03	1.1e-03	8.5e-03	-.017*
	(0.607)	(0.440)	(0.538)	(0.942)	(0.669)	(0.089)
month -5	.026	.016	.015	.031*	6.7e-03	.014
	(0.233)	(0.172)	(0.249)	(0.064)	(0.720)	(0.177)
month -4	-2.1e-03	.014	.047***	.017	8.5e-03	.027***
	(0.912)	(0.211)	(0.001)	(0.280)	(0.639)	(0.007)
month -3	.032	4.4e-03	7.6e-03	.029*	-.023	.017
	(0.220)	(0.705)	(0.646)	(0.095)	(0.244)	(0.134)
month -2	-8.3e-03	.048***	1.2e-03	.017	.023	.021*
	(0.713)	(0.000)	(0.943)	(0.334)	(0.272)	(0.067)
month -1	.069**	.029**	.056***	.065***	.032	.041***
	(0.021)	(0.014)	(0.001)	(0.000)	(0.132)	(0.001)
month 0	.09**	.082***	.015	.079***	.043**	.05***
	(0.012)	(0.000)	(0.376)	(0.001)	(0.028)	(0.004)
month 1	-.054	3.1e-03	-.047***	-.027	-.018	-.011
	(0.113)	(0.837)	(0.004)	(0.189)	(0.349)	(0.445)
month 2	-.077***	4.0e-04	-.018	-.054***	-.024	9.1e-03
	(0.005)	(0.981)	(0.165)	(0.003)	(0.162)	(0.504)
month 3	-.045	-5.2e-03	-.017	-.042**	-.021	.015
	(0.163)	(0.692)	(0.253)	(0.019)	(0.187)	(0.308)
month 4	4.3e-03	-.028**	-8.0e-03	-.044**	-.023	-5.5e-03
	(0.865)	(0.036)	(0.605)	(0.011)	(0.169)	(0.676)
month 5	-.026	-.03**	-.021	-.049***	-.033*	8.7e-03
	(0.244)	(0.050)	(0.164)	(0.003)	(0.071)	(0.542)
month 6	2.8e-03	2.9e-03	-6.7e-03	-.025	-2.4e-03	8.3e-03
	(0.899)	(0.841)	(0.631)	(0.114)	(0.892)	(0.557)
R^2	0.028	0.028	0.028	0.009	0.008	0.018
Obs	1,656,074	1,656,074	1,656,074	466,353	524,776	664,945
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Lags	8	8	8	8	8	8
Cities	8271	8271	8271	7940	7970	8122

p-values in parentheses

Note. Estimates of electoral cycle and term limit effects within 18 months before and after the election: general term (panel data model with individual and time fixed effects). Method: OLS. The outcome is the total floor area authorized with the building permits released within a calendar month (normalized). Standard errors are robust and clustered at the city level. Complete estimates of all the time dummies are visually provided in Figure 3.11 and Figure 3.12.

* p<0.10, ** p<0.05, *** p<0.01

Table 3.8: Electoral cycle and last term limit in realising building permits by population classes: last term

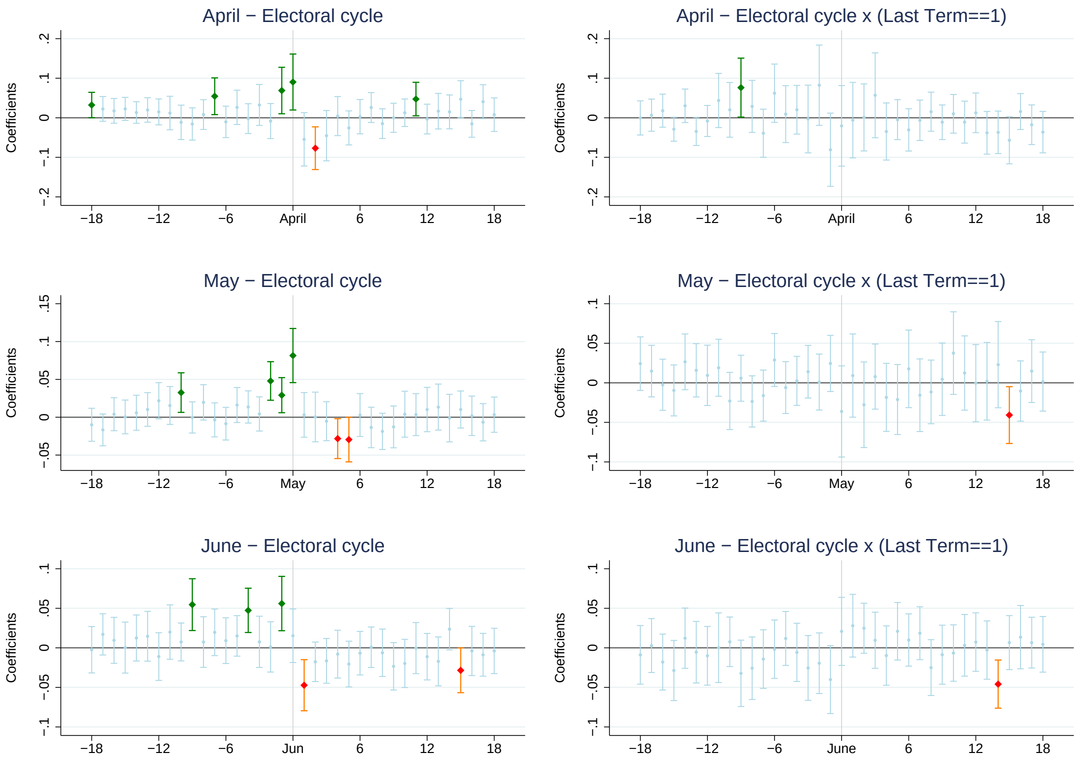
	Election months			Periods		
	April	May	June	1995-2000	2001-2007	2008-2014
month -5 x LT	9.3e-03	-6.0e-03	.012	-4.8e-03	.012	-.01
	(0.800)	(0.718)	(0.496)	(0.838)	(0.641)	(0.477)
month -4 x LT	.02	2.4e-03	-5.7e-03	3.0e-03	4.6e-03	-8.4e-03
	(0.519)	(0.878)	(0.760)	(0.908)	(0.858)	(0.533)
month -3 x LT	-3.1e-03	.014	-.025	-.014	-6.0e-03	-2.0e-03
	(0.944)	(0.409)	(0.223)	(0.604)	(0.826)	(0.905)
month -2 x LT	.082	9.3e-04	-.019	.024	8.6e-03	-.036**
	(0.112)	(0.959)	(0.320)	(0.395)	(0.757)	(0.014)
month -1 x LT	-.081*	.024	-.04*	-.011	-2.8e-03	-.031*
	(0.087)	(0.176)	(0.067)	(0.736)	(0.921)	(0.061)
month 0 x LT	-.021	-.036	.021	-.014	4.7e-03	-3.8e-03
	(0.693)	(0.218)	(0.344)	(0.619)	(0.881)	(0.885)
month 1 x LT	-5.9e-03	9.1e-03	.028	.028	.01	3.4e-03
	(0.903)	(0.734)	(0.167)	(0.294)	(0.746)	(0.878)
month 2 x LT	9.7e-04	-.028	.025	.013	-6.5e-04	.011
	(0.982)	(0.316)	(0.125)	(0.562)	(0.981)	(0.588)
month 3 x LT	.057	7.9e-03	9.6e-03	.023	-1.4e-03	9.1e-03
	(0.299)	(0.708)	(0.598)	(0.329)	(0.953)	(0.686)
month 4 x LT	-.035	-.018	-9.8e-03	.021	-.034	-.028
	(0.346)	(0.401)	(0.608)	(0.377)	(0.208)	(0.158)
month 5 x LT	-4.7e-03	-.021	.021	.016	-1.9e-03	5.0e-03
	(0.855)	(0.350)	(0.260)	(0.451)	(0.946)	(0.819)
last term	-.012	-.029	.03	-8.5e-03	.018	6.2e-03
	(0.813)	(0.323)	(0.174)	(0.760)	(0.549)	(0.810)
R^2	0.028	0.028	0.028	0.009	0.008	0.018
Obs	1,656,074	1,656,074	1,656,074	466,353	524,776	664,945
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Lags	8	8	8	8	8	8
Cities	8271	8271	8271	7940	7970	8122

p-values in parentheses

Note. Estimates of electoral cycle and term limit effects within 18 months before and after the election: general term (panel data model with individual and time fixed effects). Method: OLS. The outcome is the total floor area authorized with the building permits released within a calendar month (normalized). Standard errors are robust and clustered at the city level. Complete estimates of all the time dummies are visually provided in Figure 3.11 and Figure 3.12.

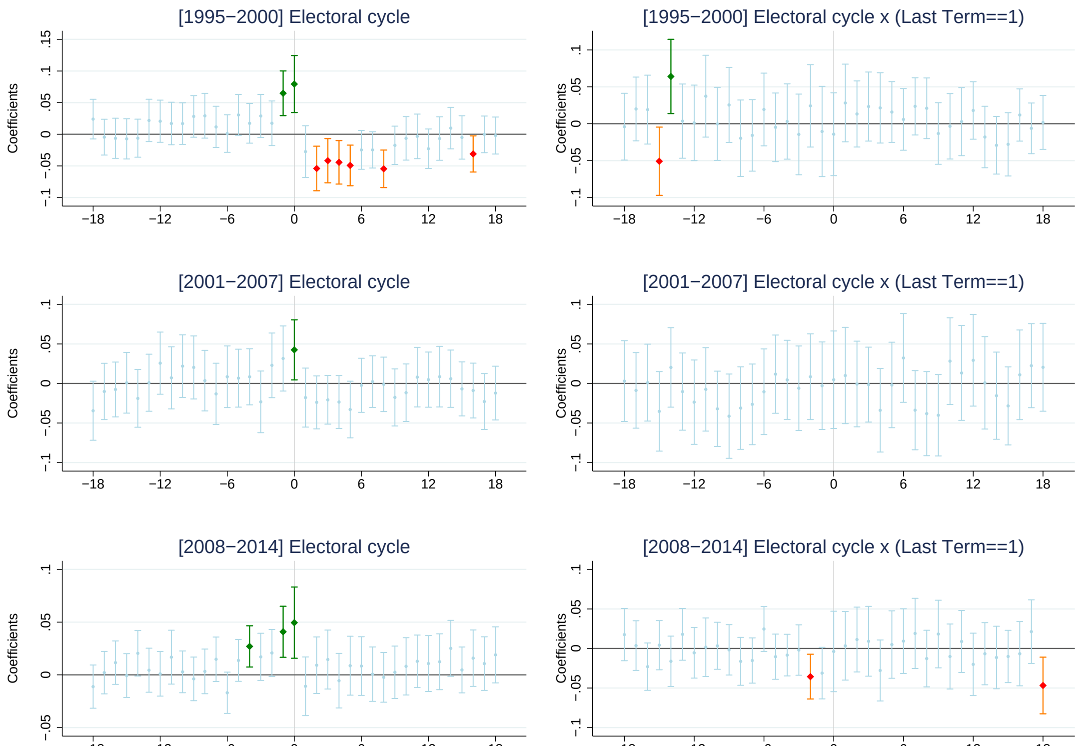
* p<0.10, ** p<0.05, *** p<0.01

Figure 3.11: Robustness check: electoral cycle and term limit considering elections in specific calendar month



Notes. Scatterplot of electoral cycle and term limit treatments over time (see Table 3.7 and Table 3.8 for model specification). Y-axis: Regression coefficients of time dummies with confidence interval at 95%. X-axis: months before and after the electoral month (t=0). Green solid range (for positive and significant estimates) on the left and orange solid range (for negative and significant estimates) on the right serve as evidence of electoral cycle.

Figure 3.12: Robustness check: electoral cycle and term limit considering elections over specific periods



Notes. Scatterplot of electoral cycle and term limit treatments over specific periods (see Table 3.7 and Table 3.8 for model specification). Y-axis: Regression coefficients of time dummies with confidence interval at 95%. X-axis: months before and after the electoral month (t=0). Green solid range (for positive and significant estimates) on the left and orange solid range (for negative and significant estimates) on the right serve as evidence of electoral cycle.

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