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THE REAL EFFECTS OF LAND USE REGULATION: QUASI-EXPERIMENTAL EVIDENCE FROM A DISCONTINUOUS POLICY VARIATION

by Marco Fregoni[*], Marco Leonardi[*] and Sauro Mocetti[§]

Abstract

We provide quasi-experimental evidence of the effects of a relaxation of land use constraints on local economic activity. We exploit the fact that in 1999 the central government imposed fiscal rules on municipal governments and in 2001 it relaxed them for municipalities with less than 5,000 inhabitants. First, we show that municipalities rely on the urbanization revenues that they collect from issuing building permits to avoid fiscal distress and to finance current expenditure. The rise of building permits is concentrated in the non-residential market and is stronger after 2003, when urban revenues were allowed to pay for municipalities' current expenditures. Second, we exploit this *de facto* reduction of firms' entry barriers to examine downstream effects. We find a positive impact on employment growth and firms' entry that is concentrated in non-tradable sectors.

JEL Classification: D73, H72, R52, R31, R33.
Keywords: fiscal rules, urbanization revenues, land use regulation, building permits, firms' entry, employment.
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1 Introduction *

The land use regulations differ significantly between countries. For example, an Italian entrepreneur spends on average 190 days to complete all the procedures to build a warehouse; the corresponding figures for the US and the UK are respectively 81 and 86. The ease of dealing with construction permits is also highly heterogeneous within countries, which reflects a wide array of local government regulations. Among Italian cities, the number of days ranges from 105 in Milan to more than 200 in Palermo and around 300 in Naples.[1] Large differences also arise in terms of number, complexity and monetary costs of the procedures and explicit land use restrictions.

The extent and restrictiveness of land use regulation shape the form of cities and the amount of residential and non-residential development. In the residential market, regulation appears to be the single most important factor affecting local housing supply (the literature is very big, but for brevity see Gyourko and Molloy, 2015 for the microeconomic implications and Hsieh and Moretti, 2019 for the macroeconomic implications). In the non-residential market, land use restrictions create entry barriers, thus discouraging entrepreneurship and reducing competition (OECD, 2010).

Examining the causal effect of land use regulation on economic activity is extremely challenging from an empirical point of view. First, land use policies are multidimensional and difficult to measure. Second, the extent of regulation is shaped by the interests of the homeowners, developers and the local community. We address this endogeneity problem by exploiting a discontinuity in these cities budget rules, which induced larger cities to release more building permits (i.e., a relaxation of land use regulation).

Our identification strategy exploits two policy changes. In 1999 the Italian government imposed strict fiscal rules on all municipal governments; in 2001, these rules were relaxed for municipalities below 5,000 inhabitants. Although the urbanization revenues that accrue from releasing building permits have always been an important source of revenues for municipalities, they could only be used to pay for capital investment. In 2003, the government relaxed this constraint and this enabled municipalities to finance current expenditures with urbanization revenues, thus increasing the incentive to use building permits to balance the budget. Indeed, the amount of urbanization revenues per inhabitant increased from 39 euros in 1999–2000 to 62 euros in 2003–2004. We use a difference-in-discontinuity design in time (before and after 2001) and size (around the 5000 population threshold) to examine the effect of fiscal rules and of land use regulation on local economic activity.

First, we find that tighter fiscal rules lead to higher urbanization revenues. This effect is statistically and economically significant after 2003, when urbanization revenues were allowed to pay for current expenditures. The increase in urbanization revenues is (unsurprisingly) accompanied by a similar increase of the construction permits, which is concentrated in the non-residential market.

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[1]These figures are drawn from the World Bank's Doing Business 2020.

In fact, non-residential permits are more profitable for municipalities because they are more expensive for applicants and usually require lower expenditures for public services. Second, the implied relaxation of land use regulation had a positive impact on economic growth. In particular, we find that lower land restrictions increase employment and firm growth at the local level (about 2 and 5 percentage points, respectively), and the impact is concentrated in the non-tradable sector.

Using the same discontinuity on Italian municipal budget rules, Grembi, Nannicini, and Troiano, 2016 show that cities which were relieved from fiscal constraints increased deficits and lowered taxes relative to the group of larger cities who were kept under the constraints.[2] Starting from this evidence and looking inside municipalities' balance sheets, we show that this last group of cities took to releasing more building permits to increase tax revenues and to balance the budget.[3] Then we exploit this exogenous source of variation to examine the real effects of land use regulation.

We contribute to two different strands of literature. First, many papers in the literature on urban planning regulation find a negative correlation between regulation and land use (and land values). However, these results are mostly drawn from cross-sectional evidence and they are subject to endogeneity concerns, thus failing to establish a strong causal nexus (Kok, Monkkonen, and Quigley, 2014 and Glaeser and Gyourko, 2018). Land regulation may be both correlated to other socioeconomic factors that are difficult to isolate with cross-sectional data and likely to be shaped by preferences of the local community (Fischel, 2001). Therefore, omitted variable biases and reverse causality are both rampant issues. Using a quasi-experimental setting, we provide clean evidence on the relaxation of the land regulation on land use. Moreover, there is also a measurement issue because zoning and other land-use policies are multidimensional and difficult to measure. Previous studies have attempted to address this problem with survey information on the local regulatory environment (Gyourko, Saiz, and Summers, 2008), while we exploit a within-city (exogenous) variation of the extent of regulation (i.e., the variation in the number of construction permits).[4]

Second, we contribute to the literature that examines the impact of regulation (or administrative burden) on business activities and economic growth. Starting from Djankov, La Porta, Lopez-de Silanes, and Shleifer, 2002, several cross-country studies look at the correlation of (indexes of) of firm entry regulation with measures of economic performance (Djankov, McLiesh, and Ramalho, 2006; Ciccone and Papaioannou, 2007 and Klapper, Laeven, and Rajan, 2006). Other studies, which are more closely related to our approach, exploit time variation in firm entry costs across regions and/or industries. In particular, 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. Branstetter, Lima, Taylor, and Venancio, 2014 show that the (staggered) reduction of entry costs across counties in Portugal resulted in increased firm formation and employment.[5] We exploit the discontinuous policy variation, to examine the impact of the relaxation of land use regulation in the non-residential market on firm creation and employment.

[2]Marattin, Nannicini, and Porcelli, 2019 examine the changes in budgetsâ composition due to the fiscal consolidation of 2012 and find that municipalities above 5000 inhabitants increased property taxes.

[3]From this point of view, and in the spirit of work by Milesi-Ferretti, 2004, we show that fiscal rules may lead to unintended effects, such as increasing building permits and thus land consumption to balance the budget. See also Wyplosz, 2013 for a review of the impact of fiscal rules.

[4]Namely, Gyourko, Saiz, and Summers, 2008 collected survey information for 2,611 U.S. communities and created 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.

[5]See Amici, Giacomelli, Manaresi, and Tonello, 2016 and Schivardi and Viviano, 2011 for evidence on Italy.

This source of variation is arguably more exogenous with respect to other variation of regulation that are subject to the discretion of the local authorities and/or to the rent-seeking activities of certain industries.

The remainder of the paper is organized as follows. In section 2, we describe the Italian institutional setting in more detail. In section 3 and section 4, we discuss the data and the empirical strategy, respectively. In section 5 we give the results and section 6 concludes the paper.

2 Institutional setting

The fiscal discipline imposed by the Maastricht Treaty and later by the Stability and Growth Pact forced European countries to engage in budget consolidation. Consequently, many European countries also introduced fiscal rules to limit the expenditures of local administrations. In Italy, law 448/1998 prescribes the implementation of a Domestic Stability Pact (DSP), which limits the budget deficit of local governments. However, after two years, starting from 2001, small municipalities (i.e. those with a resident population below 5,000 residents) were excluded from the DSP to avoid burdening very small towns with onerous requirements. To avoid manipulation of the population threshold, resident population is calculated as of two years earlier.

According to the DSP, municipalities (as well as regions and provinces) must meet a series of caps and constraints on their expenditures.[6] Rules of the DSP for our reference period are reported in Table 1. The penalties for non-compliance (although 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.

Urbanization revenues are one of the most important items in the municipalities' budgets and before 2001 they generated more than one tenth of the total revenues.[7] Namely, the Italian building regulation charges the release of building licenses with an impact fee upon the builder; the amount of the fee is an autonomous decision of the municipality.[8] This fee aims to finance the costs of providing infrastructure and public services in the newly developed areas, which reduces the burden on local authorities.

Urbanization revenues enter the balance sheet as a specific item that cannot be used except for "capital expenses"; that is, primary (e.g. roads, parks, sewer, water treatment, utilities, network infrastructures) and secondary infrastructures (e.g. schools, social centres, public gyms, place of worship) and the ordinary maintenance of municipality's 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, since 1986 the legislator has stated that this item must not exceed the 30% of the total amount. In 1997, to safeguard the local finance

[6] The actual functions of the municipalities are: general administration; 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.

[7] Urbanization revenues accrue at several stages for the construction process: obtaining subdivision permits; re-zoning existing parcels from agricultural use into lots appropriate for housing (development permits); filing environmental impact statements; applying for building permits prior to constructing houses on the finished lots; complying with housing and building codes.

[8] The main change to building regulation, which still affects how it works nowadays, was introduced by law 765/1967 (*Legge Ponte*) and law 10/1977 (*Legge Bucalossi*).

sustainability, the legislator relaxed the limit on the use of urbanization revenues removing the restriction on the amount available for maintenance of the public building stock and allowing for both ordinary and extraordinary types of interventions.[9] In 2003 the legal constraint on the use of urbanization revenues has been totally removed with the *Testo unico sull'edilizia* (TUE henceforth), which allows municipalities to use these revenues to fund current expenditures.[10] In our analysis, we compare the pre-treatment period 1998–1999 to the post-treatment period 2001–2004 (divided in two sub-periods of 2001–2002 and 2003–2004, the last of which is the period when urbanization revenues could be used to pay for current expenditures). The new framework allowed the use of urbanization revenues to pay for current expenses, which sparked a public debate on the consequences to local finance's sustainability and urban environment.[11]

3 Data

In the first part of our analysis, which looks at the effects of DSP application around the threshold of 5000 inhabitants on municipalities' budget choices, we consider both the urbanization revenues and the floor area of building permits as outcome variables.

According to the design that we adopted, we select a sample of municipalities between 3,000 and 7,000 inhabitants, excluding cities from regions with special autonomy because 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). The exact dimension of the sample is defined according to an optimal data-driven rule for each outcome and model specification (as discussed in section 4). In most of the cases the sample of municipalities is restricted to those between 3,500 and 6,500 inhabitants and, therefore, Table 2 reports main descriptive statistics for this subset of municipalities (and referring to the years before the treatment). 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.

For urbanization revenues (and all other variables belonging to municipal budgets), we use municipal financial reports, including administrative data provided by the Ministry of Interior for the universe of all Italian cities, which are available since 1998. These reports contain detailed information about all the items that municipalities have to declare according to the annual budget law. We focus on the category "urbanization revenues" (that jointly measures revenues coming

[9]In 2000, the reorganisation of the functions of local authorities (*Testo Unico sull'ordinamento degli enti locali*) allowed municipalities that were not experiencing a fiscal distress to temporarily "borrow" urbanization revenues to fund current expenses—they only had the legal obligation to reallocate the borrowed amount to the original budget item by the end of the fiscal year.

[10]In particular, the act was approved in 2001 but the change of government imposed a lengthy process of legal harmonization that set the final date of 30/06/2003.

[11]Consequently, the legislator introduced a limit to the share of urbanization revenues contributing to current expenditures at 75% in 2005 and 50% in 2006. 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%). Later on, law 244/2007 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. The extensive and relaxed use of urbanization fees continued, with frequent changes, until 2017 when the law excluded the possibility to use them as a tool for budget balancing, restoring their original function.

from both urbanization fees and urban sanctions) as reported in the capital revenues section of the balance sheets (nominal per capita values on accrual basis). The average value of urbanization revenues registered before the reform is 39 euros per inhabitant and there is no statistical difference between municipalities above and below the population threshold. Treated and untreated cities are also equal in terms of the incidence of the urban revenues on the total current and capital revenues (Table 2).

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 municipalities since 1997. Every month all municipalities must communicate detailed information concerning the release of building permits. They include, among other things, the total floor area (square meters) allowed to be built and the nature of the building (residential or non-residential). 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 of floor area released with permits, which equals 1.7 square meter per inhabitant.

We also use data related to geographical and socio-demographic characteristics of the municipalities, drawn from the National Institute of Statistics (ISTAT), which might affect both housing demand and supply. For these variables, we provide again a set of t-tests of means of the two groups of treated and untreated cities to test the validity of our design. 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 analyse whether increasing the release of building permits for non residential constructions may have positively affected the set-up or expansion of enterprises, as measured by the number of firms' branches and individual workers at municipal level. Namely, we use census data from the Business Register, including information on the number of employees and (branches of) firms carrying on economic activities in the fields of industry, trade and services. We use all the cross-section waves available within the relevant period (i.e. those collected in 1996, 2001 and 2004). We obtain data for the missing years through interpolation. For these variables, we also know the sector of economic activity.

4 Empirical strategy

In the first step of our analysis we identify the average treatment effect of the DSP on the release of building permits and urbanization revenues. We exploit the fact that the targeted municipalities have changed in the reference period. In particular, the DPS first applied to every city, but from 2001 smaller cities (those below the 5,000 citizens thresholds) have been exempted.[12]

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

[12] 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.

Troiano, 2016).

This empirical strategy has a clear advantage with respect to a difference-in-differences design because large and small municipalities might be on differential trends in public policies. This strategy is also preferable to a regression discontinuity design because there is another policy—the salary of the mayor (and of the executive officers)—that changes sharply at the threshold of 5,000 inhabitants. Indeed, one might argue that mayors just below and just above the threshold (i.e., who are paid differently) can adopt different fiscal rules (Eggers, Freier, Grembi, and Nannicini, 2018).

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 of 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, which 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 (Grembi, Nannicini, and Troiano, 2016). 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; that is, by fitting linear regressions within a bandwidth on either side of the threshold:

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

where ΔY is the average growth rate of the outcome before and after the reform (1999-2000 vs. 2001-2003 or 1999-2000 vs. 2003-2004)[13]; D is a dummy that equals one if the legal population of the municipality is greater than 5,000 inhabitants in 2001 and is zero otherwise (i.e. we look at the effect on cities that kept the DSP relative to those that were relieved); and 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 (f) 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

[13]Our outcomes are the growth rates of urbanization revenues and building permits defined as $\Delta = \frac{x_1 - x_0}{\frac{1}{2}x_1 + \frac{1}{2}x_0}$. 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 putting an average at the denominator, we can avoid biases due to regression-to-the-mean that may emerge when dealing with data series that may exhibit very high (low) values, which is typical of building permits in small municipalities.

robust and less sensitive to overfitting and boundary issues (Cattaneo, Idrobo, and Titiunik, 2019). 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 (Calonico, Cattaneo, and Titiunik, 2015). This choice requires us to consider how the existence of a misspecification error in the approximation can affect inference procedures (Cattaneo, Idrobo, and Titiunik, 2019). 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. Because 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 them in our tables of estimates. The standard errors are clustered at the province level (NUTS3 in the Eurostat definition), which is the smallest administrative unit after the municipality because the homogeneity of the housing market rules, policies and factors is visible at that level.[14]

5 Results

5.1 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: 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 on the behavior of treated municipalities (Timeline 1). 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) and the following two years (2003–2004).[15]

Within the first two years after the DSP reform (2001–2002) there is no clear evidence of a discontinuity in the variation of the urbanization revenues (Figure 1, first column). In contrast, a clear discontinuity arises if we consider the following two years (2003–2004) since the policy change (Figure 1, second column). Similarly, in the same period, we observe also a jump for total building permits, which is driven by the non-residential (NR) market while the effect on the residential (R) market is less significant.

[14] We also provide estimates clustering at other levels (Table A.1): (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 alternative ways of clustering deliver comparable standard errors.

[15] The results are qualitatively similar if we compare the years preceding the reform period with the average value of the four years after the reform.

The visual evidence is confirmed in a regression setting. Namely, the estimates of the diff-in-disc model are not statistically different from zero when we examine the variation in the first two years after the DSP reform (Table 3), while they become significant in the subsequent two years (Table 4).[16] The impact is also sizeable from an economic point of view: according to our findings, the effect on municipalities with more than 5000 inhabitants which kept the DSP after 2001 is an increase of urbanization revenues growth by 23 percentage points. The increase in the urbanization revenues corresponds to a similar increase of the issue of building permits. Interestingly, this increase is driven by the non-residential constructions (37 percentage points), while the increase in the residential market is much smaller (3 percentage points) and not significant at the conventional levels. A visual representation of the estimated impact for each variable is reported in Figure 2.

The delayed impact of the reform (in 2003 rather than in 2001) may be attributed to two main factors. First, the TUE (i.e., the Act on construction building) that allowed to use of urbanization revenues to fund current expenditure (thus strengthening the perception that municipalities could have systematically resorted to them as a tool for budget balancing) entered into law in 2003. Second, municipalities wanting to ease the issue of building permits needed to change the urban plan and to comply with the normative framework at province and regional levels, all of which requires the City Council's approval. Moreover, any modification to the administrative process takes time to be practically implemented and to make people aware of the change.

Concerning the nature of the building permits, several factors may explain while the impact is mostly concentrated in the non-residential (rather than residential) market. First, the release of permits for non-residential buildings is more costly for applicants and it guarantees higher revenues for municipalities. Second, commercial buildings pay more in urbanization revenues destined to public infrastructures and social services and, all things being equal, municipalities need to spend less for schools, kindergartens, health and care for elderly when there are lower numbers of new residents. Finally, there is a political economy motivation for municipalities to favour non-residential construction permits: the homeowners' preference for a limited issuing of residential permits to preserve the value of their properties (Glaeser and Ward, 2009, Hsieh and Moretti, 2019).

5.2 Validity checks

Our results are robust to the adoption of polynomials of various degrees of the running variable (Table 5): our preferred specification is linear because it provides a good trade-off between bias and variability of the estimates of the causal parameter, but we also provide a polynomial of zero degree (to avoid the unreasonable weighting scheme typical of higher-degrees approximations) and of second degree (to avoid poor approximation at the boundary point, Gelman and Imbens, 2018). The estimates remain stable after the inclusion of predetermined covariates (Table 6) for geographic location and budget indexes, housing supply and demand factors at the municipal level. The magnitude of the estimates is slightly affected while the precision increases whenever the MSE-optimal bandwidth adjusted for covariates implies a larger sample. The treatment effects are always statistically significant for the bias-corrected and robust specifications.

[16] To corroborate this evidence, we have also run a sort of a triple difference-in-discontinuity where the outcome variable is the difference between the variation between 2003–2004 and 1999–2000 and that between 2001–2002 and 1999–2000. The results confirm that the impact of the DSP reform becomes significant after 2003–2004 (Table A.2).

We also provide a set of validity and falsification tests to check whether the assumptions that are 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 3). The possibility of manipulation seems to be unrealistic for two reasons. Because the law defines the legal population at year t as the number of city residents at year $(t-2)$, mayors should have been able to reduce 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 (Grembi, Nannicini, and Troiano, 2016).

Second, the analysis of the balancing properties shows that treated and control municipalities (i.e. those just above and below the threshold) are almost indistinguishable before the DSP reform for most of the socio-demographic characteristics considered, although the share of municipalities located in the southern regions is slightly higher beyond the threshold. Therefore, one potential concern is that our results are driven by the traditional North-South divide (i.e., the fact that the municipalities in the two areas are exposed to different economic shocks over the same period). Our doubts are dispelled once we re-estimate the model while limiting the sample to the municipalities located in the Centre and North of Italy, and we find that the estimated treatment effects are still significant and comparable to those obtained using the original sample (Table 7).

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 4). They tend to become insignificant for smaller bandwidths because the low number of observations reduces the misspecification error of the local approximation but tends 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 (e.g. the mayor's salary changes at the 5000 inhabitants threshold). The 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 (thus violating the identification assumption). Because the 1999 introduction of DSP was valid for municipalities of all sizes, we expect no difference at the 5000 threshold. We compare the growth rate of the various outcomes between 1997–1998 vs 1999–2000 (Timeline 2) and find that estimates of the model reject the violation of the homogeneity assumption (Table 8).

Another crucial assumption for our identification strategy is the existence of a parallel trends in treated and controls. The assumption seems to hold: all the year-by-year variations of the considered variables are insignificant (Figure 5).[17]

Because the RD identification relies on the continuity of the regression function for treated and controls 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 because the continuity assumption is untestable, it can support the adoption of this specific design. We perform several estimations of the model with artificial

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

(placebo) thresholds (Figure 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 scatter plot of the resulting causal parameters shows an expected inverse U-shape: the most substantial and most significant estimated ATE is that at the correct threshold, further strengthening our identification strategy.

Finally, we take account of potential spatial spillovers that might bias our estimates. For example, if a relaxation of land regulation in a certain municipality leads to a relocation of the construction activities at the local level, with neighbor municipalities being displaced and, therefore, issuing a reduced number of building permits, then this would overestimate the effect of the DSP reform. To account for potential spatial spillovers, we re-estimate the model giving less weight to control municipalities that are closer to the treated municipalities. The underlying idea is that this structure of weights should attenuate the role of displacement effects between neighbor municipalities. The results continue to be highly significant and with an order of magnitude that is close to that of the baseline specification, which suggests that the spatial spillovers do not play a significant role in this setting (Table 9).

5.3 Downstream effects on economic performance

The second step of this analysis is to identify the reform's unintended impact on business dynamics. Based on the previous discussion, the DSP modification has caused an increase in building permits, easing *de facto* a relevant aspect of the regulation of economic activities at local level and making a more business-friendly environment for firms (particularly for start-ups). In fact, land use regulation is often structured to protect the interests of incumbent firms by restricting the possibilities for new businesses to enter markets and this effect is plausibly stronger for products that have narrow geographic markets (i.e. the non-tradable sectors). In contrast, products without an inherent local geographic content (i.e. tradables) are less likely to be impacted by local land use restrictions because in principle firms producing tradable products can be located anywhere.

We measure the impact of the DSP reform on changes in the number of firms (the data record firms' branches, not only the legal entity) and workers before and after the reform estimating the diff-in-disc model we already used in subsection 5.1. It is also worth noting that while in the previous section we deal with flow measures of permits and revenues per year, the information on the number of firms branches and workers are stock measures representing quantities existing at that point in time.

We start again with the graphical evidence showing a discontinuity around the threshold in the change in the number of firms and workers between 2000 and 2004 (Figure 7). According to our diff-in-disc estimates (Table 10), employment growth has been higher for treated municipalities (around 5 percentage points) between 2000 and 2004, while for firms the impact was somewhat lower but still positive (2 percentage points) and highly significant. Therefore, average firm size also increases. In Figure 8, we show a visual representation of the estimated impact.

In the following, we provide a battery of sensitivity checks, in line with what we have done in the previous section. First, we do not find any significant increase in the previous period (1996–2000) for both workers and firms, thus suggesting that the parallel trend assumption is satisfied. Then, we check the sensitivity of our estimates to parsimonious specifications of the polynomial degree of the running variable (i.e. zero and second degrees). The estimated ATE for both the two outcomes is stable and almost always significant for all specifications (Table 11). We also 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 barely affected, the precision increases whenever the optimal data-driven bandwidth which accounts for the presence of additional regressors defines a larger sample (Table 12).[18] Finally, we account again for potential spatial spillovers because the non-residential market activity in one municipality might have some effects on the same variable in close municipalities. Again, the results are qualitatively confirmed (Table 13).

These results rely on the implicit assumption that the joint effect of the DSP-Act on Construction Building on the real economic activity of municipalities (firm entry and employment) only passes through the channel of a relaxation of land use regulation. However, the exclusion restriction would be violated if the reform also affected other budget items and, through these, the level of economic activity. From Grembi, Nannicini, and Troiano, 2016, we know that municipalities that were relieved from the DSP (i.e., our control group) increased deficits and lowered taxes. Consistently, we find that the municipalities that kept the DSP (the treatment group) increased in particular urbanization revenues (a form of tax) issuing more building permits and this indirectly boosted local economic activity. Normally increasing taxes would cause a reduction in economic activity while we find an increase. Had it not been that the increase in revenues was obtained through an increasing issue of building permits, then we would probably have observed a reduction rather that an increase in the number of firms and workers in the treatment group. Stated differently, our results can be interpreted as a lower bound if we are willing to assume that direct impact of fiscal constraints (i.e. less resources and more constraints on their allocation) are unlikely to boost economic activity.

Finally, we examine whether the increase in the level of activity was heterogeneous across sectors of economic activity. To examine this issue, we distinguish workers and firms branches in different sectors. Namely, we distinguish between commercial non-tradable activities (i.e., retail trade, hotel and restaurants, transports and communication), public sector (i.e., education, health and other social services) and the remaining private activities (mainly manufacturing i.e. tradables). According to our findings, the increase in the number of firms is entirely driven by the commercial non-tradable activities (Table 14) and the same is true, unsurprisingly, for the increase in the number of workers (Table 15). This result squares with the observation that non-tradable activities are more responsive to changes in land regulation while tradables are less because they can be located anywhere and that public services are not sensitive because they are located where they are needed independently from the regulation of building permits.

6 Conclusions

The use of fiscal rules as devices to ensure fiscal discipline may also have unintended effects. We provide quasi-experimental evidence that fiscal constraints induced targeted municipalities to rely on urbanization revenues that they collect from issuing building permits to comply with tighter budget rules. We exploit this exogenous source of variation to examine the real effects of land use

[18] These results are also robust to different ways of clustering (i.e. at the forcing variable and at the local labor market levels, see Table A.1).

regulation.

The growth of the total amount of urbanization revenues has been about 20 percentage points higher in the municipalities that have remained subject to the DSP. A similar positive impact has been recorded for building permits, particularly for the non-residential market. 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 growth in the number of firms and workers in treated municipalities has been around 2 and 5 percentage points higher, respectively. Interestingly, the effect was concentrated in the non-tradable sector, which is more likely to be impacted by local land use restrictions.

Tables and figures

Table 1: Institutional framework: timeline

Domestic Stability Pact (DSP)		Share of urbanization revenues allowed to be used for (%)		
year	binding for municipalities	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	$\geq 5,000$ inhab.	100	100	0
2002	$\geq 5,000$ inhab.	100	100	0
2003	$\geq 5,000$ inhab.	100	100	100
2004	$\geq 5,000$ inhab.	100	100	100

Main sources: Annual budget laws, Testo Unico dell'edilizia (TUE).

Table 2: Descriptives and two-sample t-tests by municipality population groups.

	control (3,500-5,000)	treated (5,000-6,500)	Δ	T-test (p)	Obs
Main outcomes					
building permits (m^2/inhab.)	1.71	1.69	0.019	0.841	949
building permits: residential (m^2/inhab.)	0.87	0.90	-0.027	0.575	935
building permits: non residential (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.)	0.07	0.07	-0.002*	0.057	949
workers (number/inhab.)	0.25	0.26	-0.008	0.405	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
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
area (km^2)	35.66	38.03	-2.376	0.366	949
House supply (determinants)					
stock of houses (n. of 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

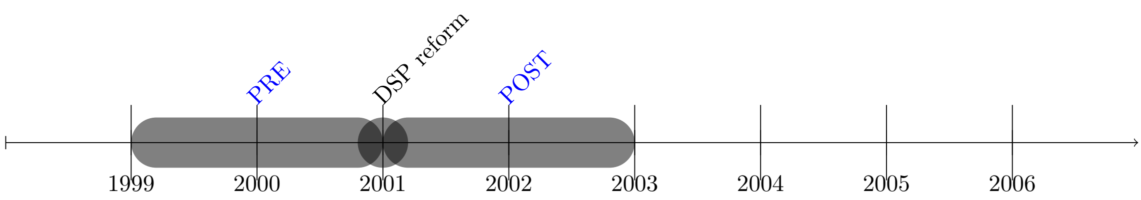
Notes. Two-sample t-test on the equality of means for municipalities between $(3,500-5,000)$ and $(5,000-6,500)$ inhabitants. Average values over 1999 and 2000. Building permits refer to the the total amount of useful area per capita allowed to be constructed by the release of building permits. Urbanization revenues per capita are provided on accrual basis, nominal values. Confidence interval: 90%. Sources: Istat, Bank of Italy

Timeline 1: The two time windows adopted in the analysis.

Relevant institutional changes:

- 1999: Introduction of the Domestic Stability Pact (DSP) for all municipalities.
- 2001: The reform of the DSP relaxes fiscal rules for municipalities below 5,000 inhabitants.
- 2003: Introduction of the Testo Unico dell'Edilizia (TUE) allows municipalities to finance current expenditure with urbanization revenues.

- The first time window



- The second time window

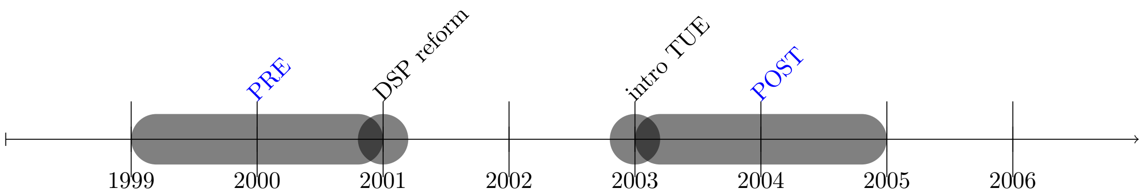
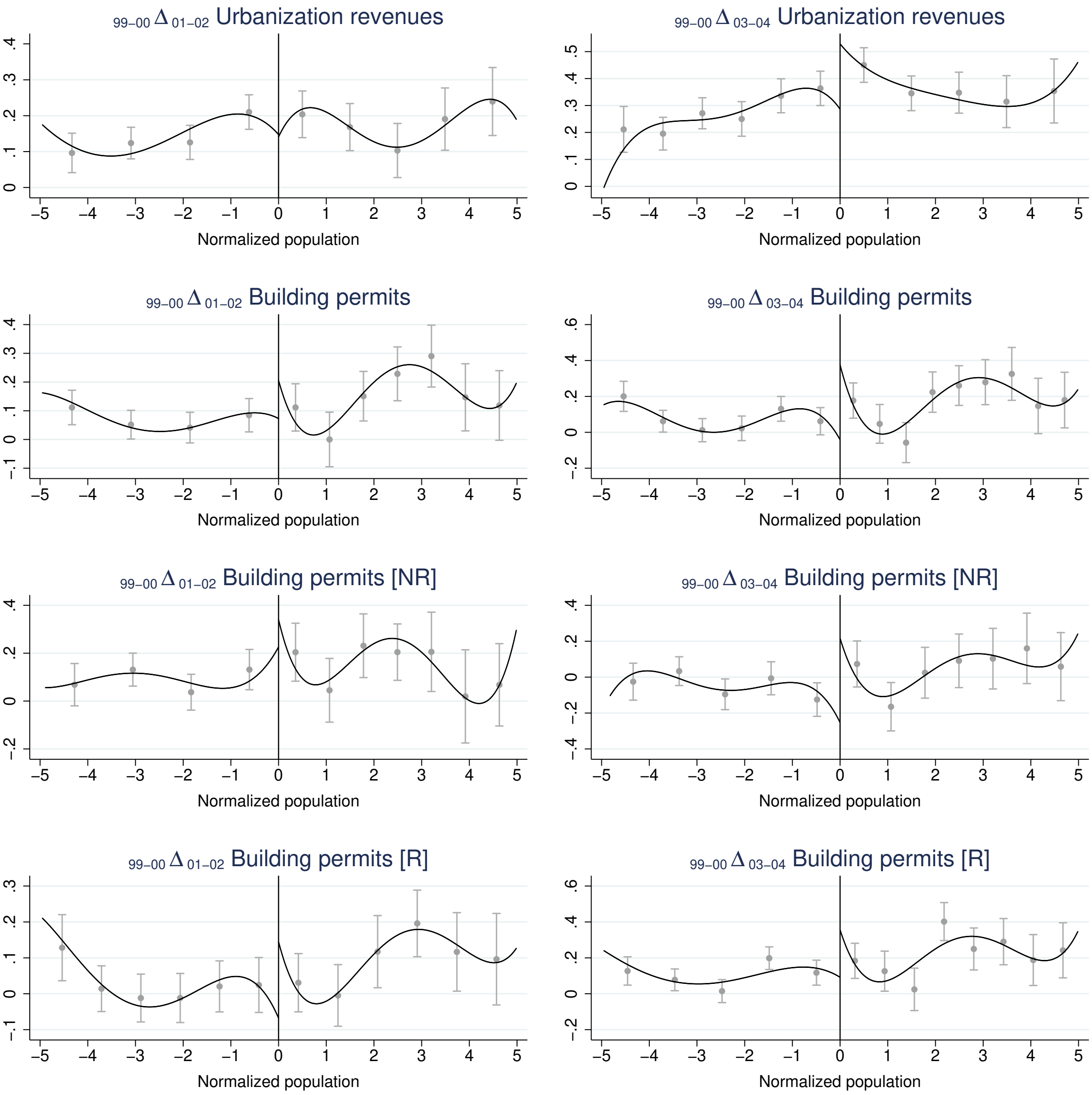


Figure 1: Visual analysis of the effects of the DSP on municipalities larger than 5,000 inhab: global smooth approximation and local behaviour.



Notes. Global polynomial fit (4^{th} order) and local sample means with bin selected with IMSE-optimal evenly-spaced method using spacings estimators according to Calonico, Cattaneo, and Titiunik, 2015 (confidence interval at 90%). Y-axis: average growth rate of the outcome variable (before and after the introduction of the DSP), e.g. $\Delta Y_{99-00}^{03-04} = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. X-axis: legal population at 2001 normalized at the threshold = 5,000 (thousands of inhabitants). [NR] stands for non residential (building permits), [R] stands for residential.

Table 3: Effect of the DSP on municipalities larger than 5,000 inhab. over 1999−2000 vs 2001−2002 (the first time window).

	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	0.025	0.031	0.074	0.052
	(0.095)	(0.088)	(0.133)	(0.100)
Bias-corrected	0.029	0.060	0.112	0.087
	(0.095)	(0.088)	(0.133)	(0.100)
Robust	0.029	0.060	0.112	0.087
	(0.115)	(0.102)	(0.156)	(0.113)
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

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1$[municipality size $\geq$ $5,000$ inhabitants in 2001]. The outcome is the average growth rate over the period 1999−2000 vs 2001 − 2002, $\Delta Y = (\bar{Y}_{01-02} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{01-02} + \frac{1}{2}\bar{Y}_{99-00})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits), [R] stands for residential. 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, Cattaneo, Farrell, and Titiunik, 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.

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

Table 4: Effect of the DSP for municipalities larger than 5,000 inhab. over 1999−2000 vs 2003−2004 (the second time window).

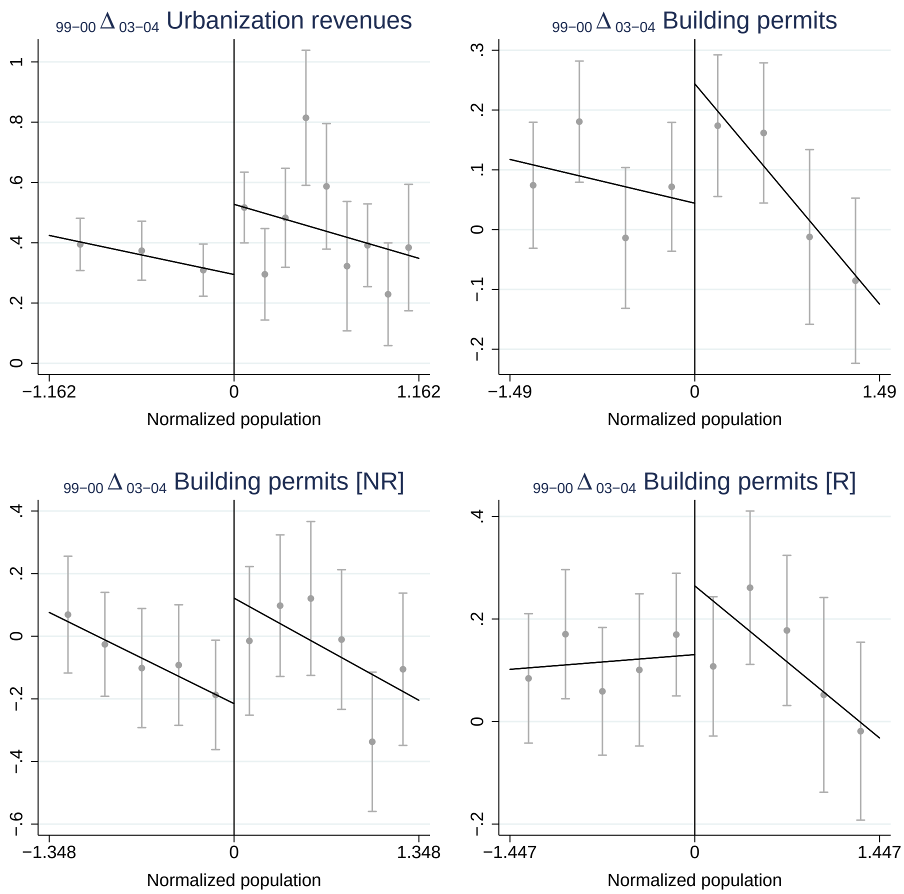
	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	0.214**	0.182	0.304*	0.045
	(0.087)	(0.118)	(0.173)	(0.123)
Bias-corrected	0.234***	0.236**	0.366**	0.031
	(0.087)	(0.118)	(0.173)	(0.123)
Robust	0.234**	0.236*	0.366*	0.031
	(0.102)	(0.128)	(0.201)	(0.147)
Estimation				
Bandwidth	1.162	1.490	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

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1$[municipality size $\geq$ 5,000 inhabitants in 2001]. The outcome is the average growth rate over the period 1999−2000 vs 2003 − 2004, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits), [R] stands for residential. 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, Cattaneo, Farrell, and Titiunik, 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.

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

Figure 2: Visual analysis of the estimated effects of the DSP.



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 according to Calonico, Cattaneo, and Titiunik, 2015 (confidence interval at 90%). Y-axis: average growth rate of the outcome variable (before and after the introduction of the DSP): $\Delta Y_{99-00}^{03-04} = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$ X-axis: legal population at 2001 normalized at the threshold = 5,000 (thousands of inhabitants). [NR] stands for non residential (building permits), [R] stands for residential.

Table 5: Sensitivity to the polynomial order specifications.

	Δ Urbanization revenues			Δ Building permits [NR]		
	poly=0	poly=1	poly=2	poly=0	poly=1	poly=2
Conventional	0.116**	0.214**	0.203*	0.200*	0.304*	0.346*
	(0.049)	(0.087)	(0.108)	(0.120)	(0.173)	(0.207)
Bias-corrected	0.136***	0.234***	0.194*	0.272**	0.366**	0.357*
	(0.049)	(0.087)	(0.108)	(0.120)	(0.173)	(0.207)
Robust	0.136**	0.234**	0.194	0.272*	0.366*	0.357
	(0.063)	(0.102)	(0.122)	(0.148)	(0.201)	(0.243)
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

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1$[municipality size $\geq$ $5,000$ inhabitants in 2001]. The outcome is the average growth rate over the period $1999 - 2000$ vs $2003 - 2004$, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits). Method: local linear regression with triangular kernel and optimal-MSE bandwidth with cluster-robust variance estimation and bias-correction (by local polynomial regression and ad-hoc optimal-MSE bandwidth) according to Calonico, Cattaneo, Farrell, and Titiunik, 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.

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

Table 6: Covariates-adjusted estimates.

	Δ Urbanization revenues			Δ Building permits [NR]		
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	0.207**	0.206**	0.201**	0.271	0.288*	0.303*
	(0.087)	(0.088)	(0.087)	(0.171)	(0.168)	(0.166)
Bias-corrected	0.228***	0.227***	0.224**	0.333*	0.343**	0.368**
	(0.087)	(0.088)	(0.087)	(0.171)	(0.168)	(0.166)
Robust	0.228**	0.227**	0.224**	0.333*	0.343*	0.368*
	(0.102)	(0.104)	(0.103)	(0.197)	(0.196)	(0.190)
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

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1$[municipality size $\geq$ $5,000$ inhabitants in 2001]. The outcome is the average growth rate over the period $1999-2000$ vs $2003-2004$, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits). 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, Cattaneo, Farrell, and Titiunik, 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. Covariates (pre-reform values): Geographics (dummies for macro-regions), budget (personnel/current spending, loan repayment/total spending), house supply (houses per capita, share of empty houses, share of houses for rent, slope of terrain), house demand (share of services over economic activities, share of old and foreign people, employment rate, population density).

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

Table 7: Estimates excluding southern municipalities.

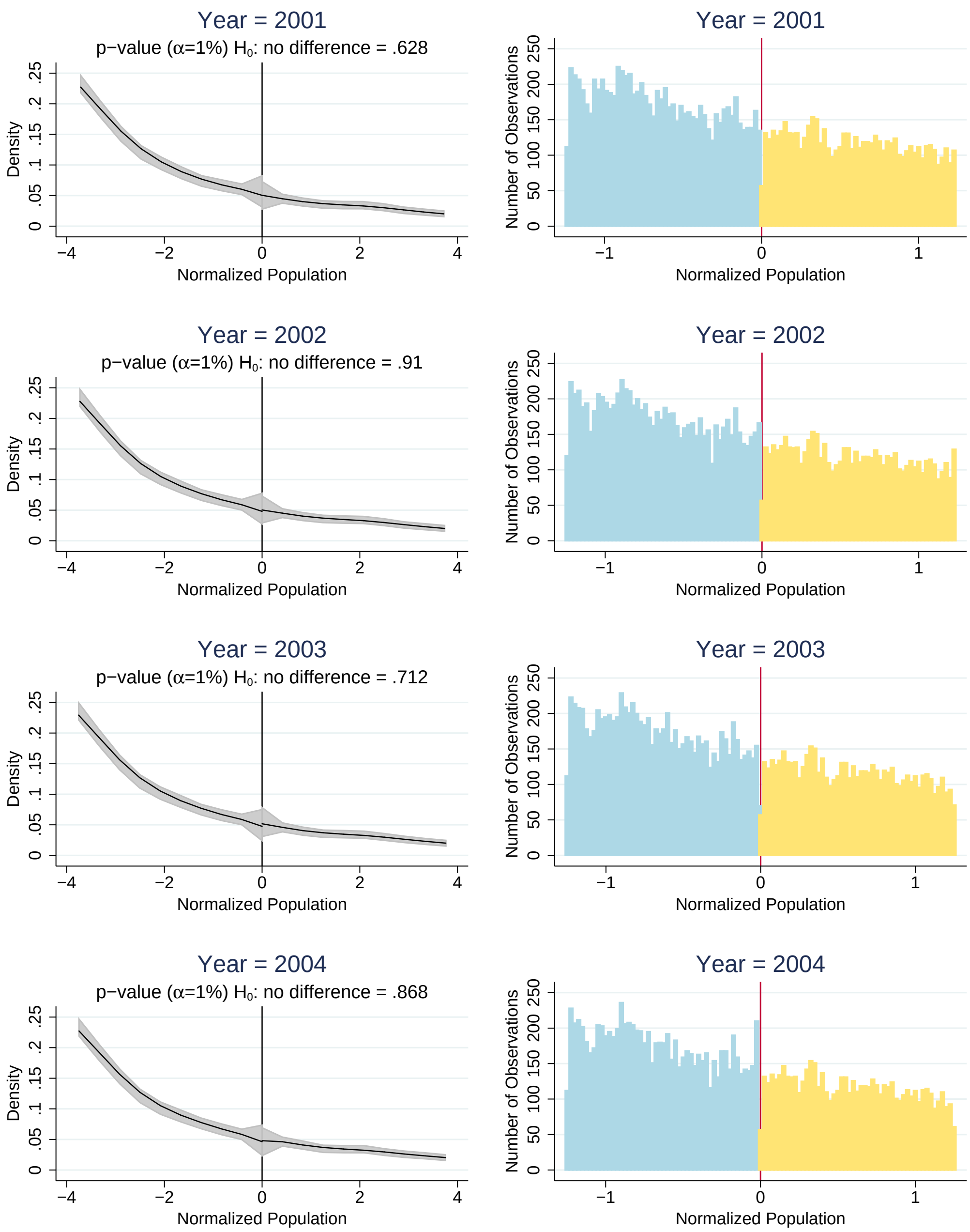
	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	0.150*	0.253*	0.414**	0.105
	(0.091)	(0.141)	(0.208)	(0.141)
Bias-corrected	0.159*	0.317**	0.481**	0.117
	(0.091)	(0.141)	(0.208)	(0.141)
Robust	0.159	0.317**	0.481**	0.117
	(0.108)	(0.156)	(0.243)	(0.167)
Estimation				
Bandwidth	1.307	1.365	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

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1[\text{municipality size} \geq 5,000 \text{ inhabitants in } 2001]$. The outcome is the average growth rate over the period 1999−2000 vs 2003 − 2004, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits), [R] stands for residential. 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, Cattaneo, Farrell, and Titiunik, 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.

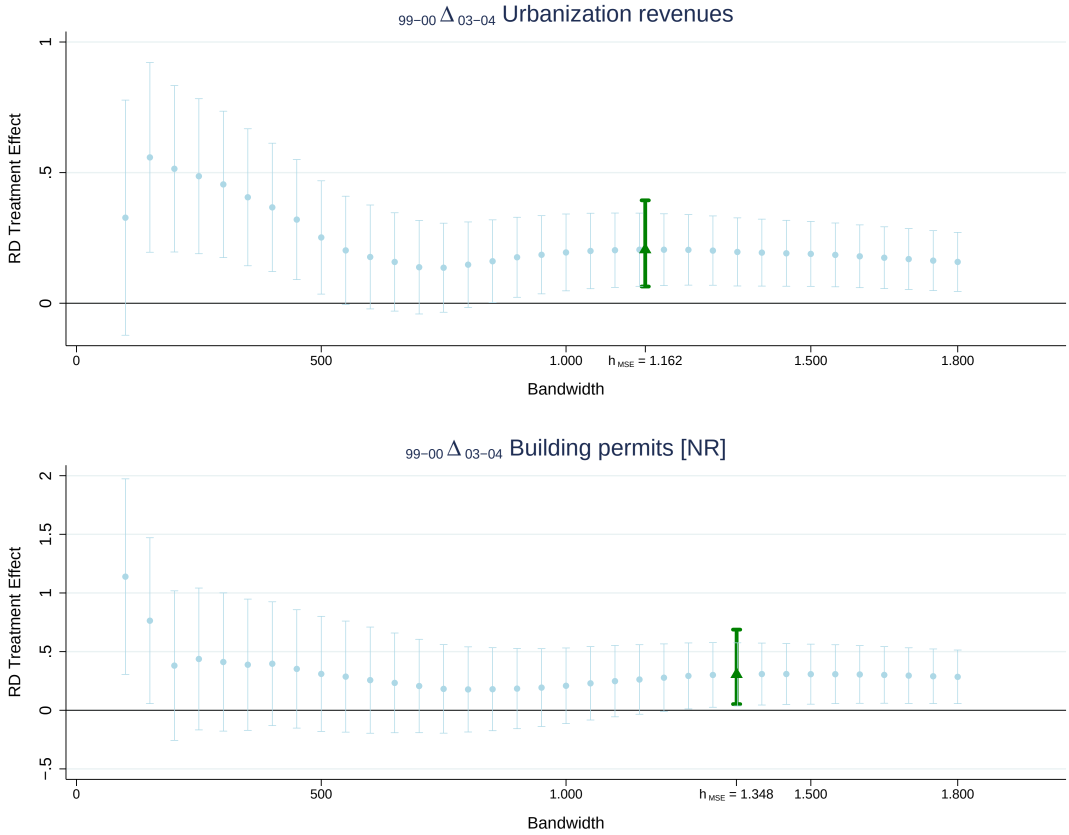
* p<0.10, ** p<0.05, *** p<0.01

Figure 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: legal population at 2001 normalized at the threshold = 5,000 (thousands of inhabitants). On the right: histogram of population distribution (bin width = 0.025). Y-axis: Number of observations. X-axis: legal population at 2001 normalized at the threshold = 5,000 (thousands of inhabitants) [within the optimal bandwidth computed for the corresponding local polynomial fit on the left].

Figure 4: Estimates sensitivity to the 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 coefficient on the treatment dummy $D = 1$[municipality size $\geq 5,000$ inhabitants in 2001]. The outcome is the average growth rate over the period $1999 - 2000$ vs $2003 - 2004$, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits). Method: local linear regression with triangular kernel. X-axis: bandwidth (absolute values); optimal MSE (Minimum Square Error) bandwidth [green and bold] (see Cattaneo, Idrobo, and Titiunik, 2019).

Timeline 2: a fake DSP relaxation in 1999 for municipalities larger than 5,000 inhab.

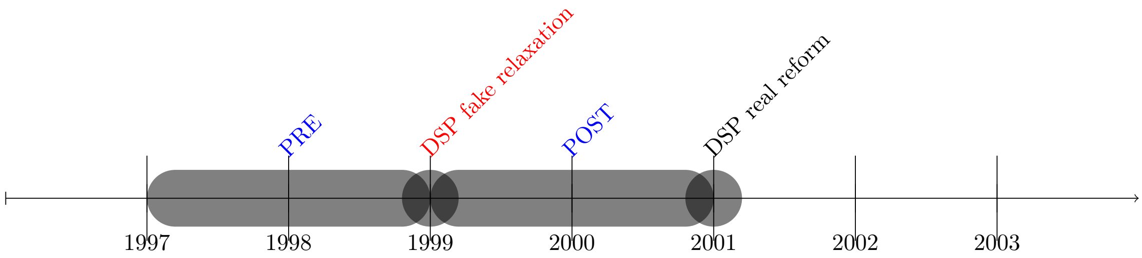


Table 8: Effect of a fake DSP introduction in 1999 on municipalities larger than 5,000 inhab.

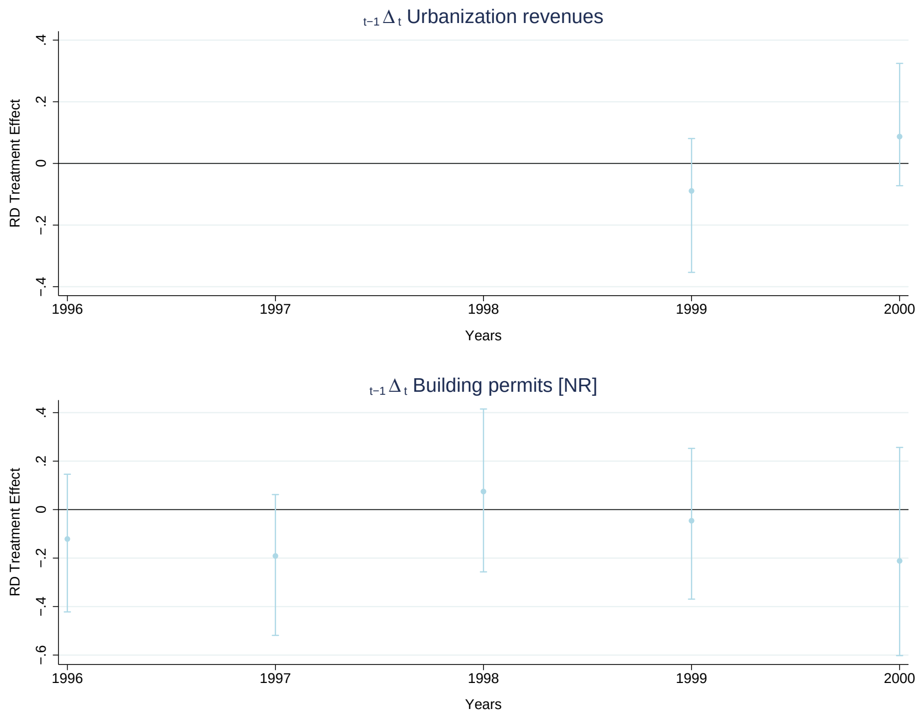
	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	-0.042	-0.081	-0.101	-0.008
	(0.113)	(0.145)	(0.177)	(0.143)
Bias-corrected	-0.032	-0.040	-0.090	0.033
	(0.113)	(0.145)	(0.177)	(0.143)
Robust	-0.032	-0.040	-0.090	0.033
	(0.138)	(0.170)	(0.213)	(0.168)
Estimation				
Bandwidth	1.964	1.406	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

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1$[municipality size $\geq 5,000$ inhabitants in 1999]. The outcome is the average growth rate over the period $1997-1998$ vs $1999-2000$, $\Delta Y = (\bar{Y}_{99-00} - \bar{Y}_{97-98})/(\frac{1}{2}\bar{Y}_{99-00} + \frac{1}{2}\bar{Y}_{97-98})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits), [R] stands for residential. 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, Cattaneo, Farrell, and Titiunik, 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.

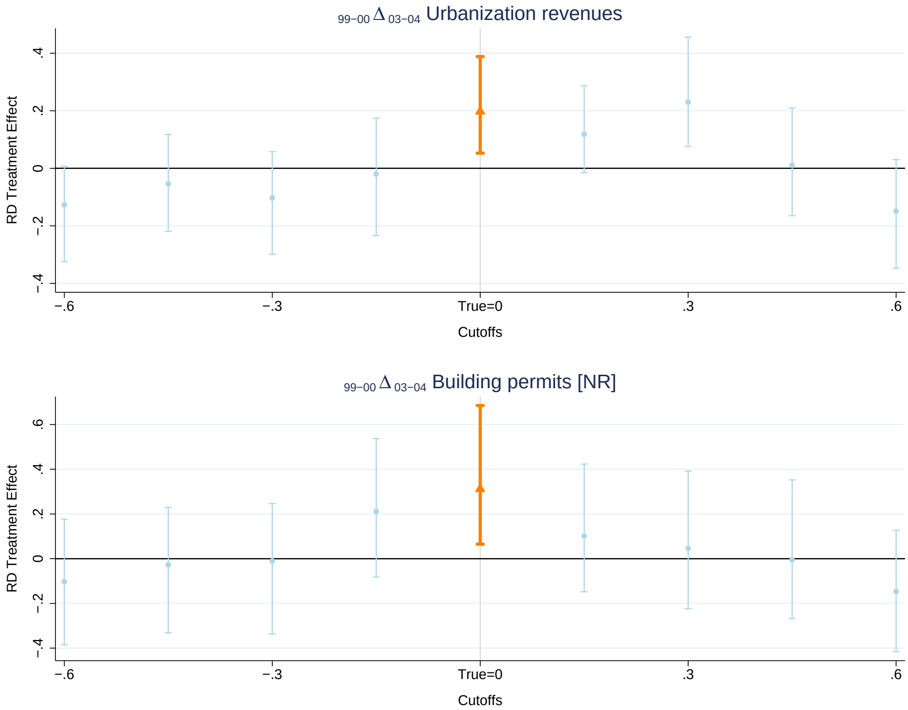
* p<0.10, ** p<0.05, *** p<0.01

Figure 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 according to Calonico, Cattaneo, Farrell, and Titiunik, 2017 (with the confidence interval at 90 percent). Y-axis (1): RD estimates of the impact of the DSP for municipalities larger than 5,000 inhabitants. X-axis: Year. [NR] stands for non residential (building permits).

Figure 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) (see Cattaneo, Idrobo, and Titiunik, 2019). Y-axis: RD estimates of the impact of the DSP for municipalities larger than a specific cutoff. X-axis: legal population at 2001 normalized at the threshold = 5,000 (thousands of inhabitants). [NR] stands for non residential (building permits).

Table 9: Testing for spillovers.

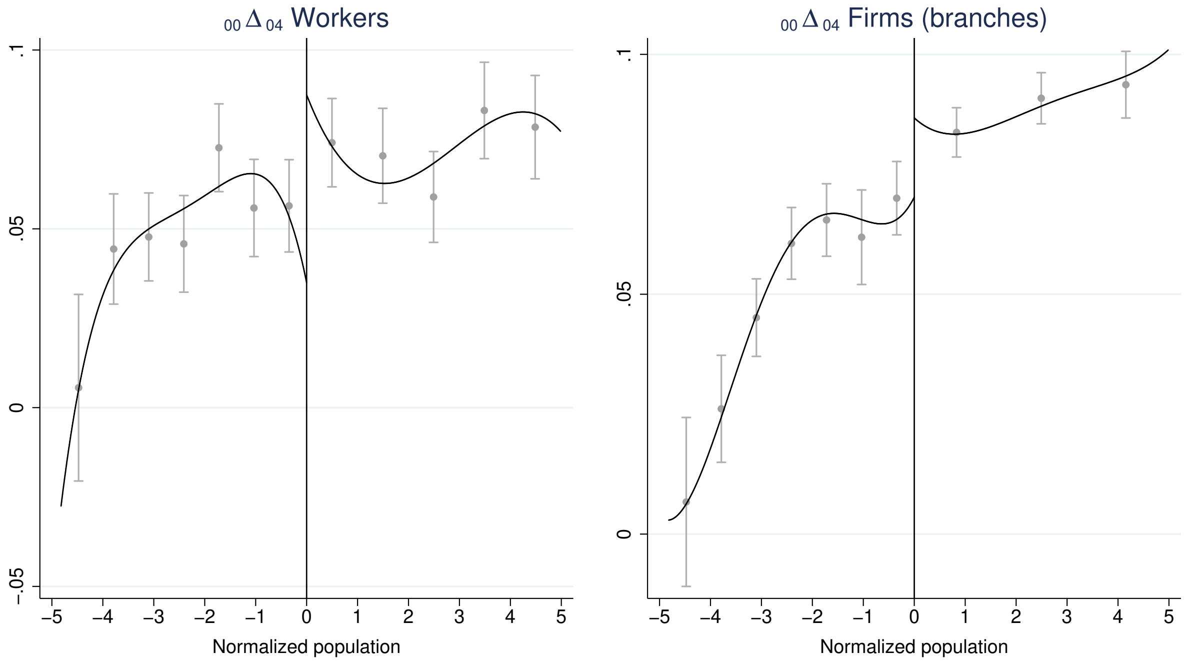
	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	0.220***	0.200*	0.287*	0.126
	(0.082)	(0.118)	(0.154)	(0.112)
Bandwidth	1.167	1.362	1.317	1.451
Obs	776	911	799	938
adj-R^2	0.003	0.005	0.002	0

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1$[municipality size $\geq 5,000$ inhabitants in 2001]. The outcome is the average growth rate over the period $1999-2000$ vs $2003-2004$, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits), [R] stands for residential. Method: local linear regression and optimal-MSE bandwidth. Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. Standard errors are clustered at the province level. Municipalities are weighted: weights for control municipalities are defined according to the log-distance (minutes) from the closest treated municipality; while for treated municipalities the distance is equal to the average distance registered by control municipalities.

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

Figure 7: Visual analysis of the downstream effects of the DSP: 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 according to Calonico, Cattaneo, and Titiunik, 2015 (confidence interval at 90%). Y-axis: average growth rate of the outcome variable (before and after the introduction of the DSP): $\Delta Y_{04-00} = (Y_{2004} - Y_{2000})/(\frac{1}{2}Y_{2004} + \frac{1}{2}Y_{2000})$. X-axis: legal population at 2001 normalized at the threshold = 5,000 (thousands of inhabitants).

Table 10: Downstream effects of the DSP.

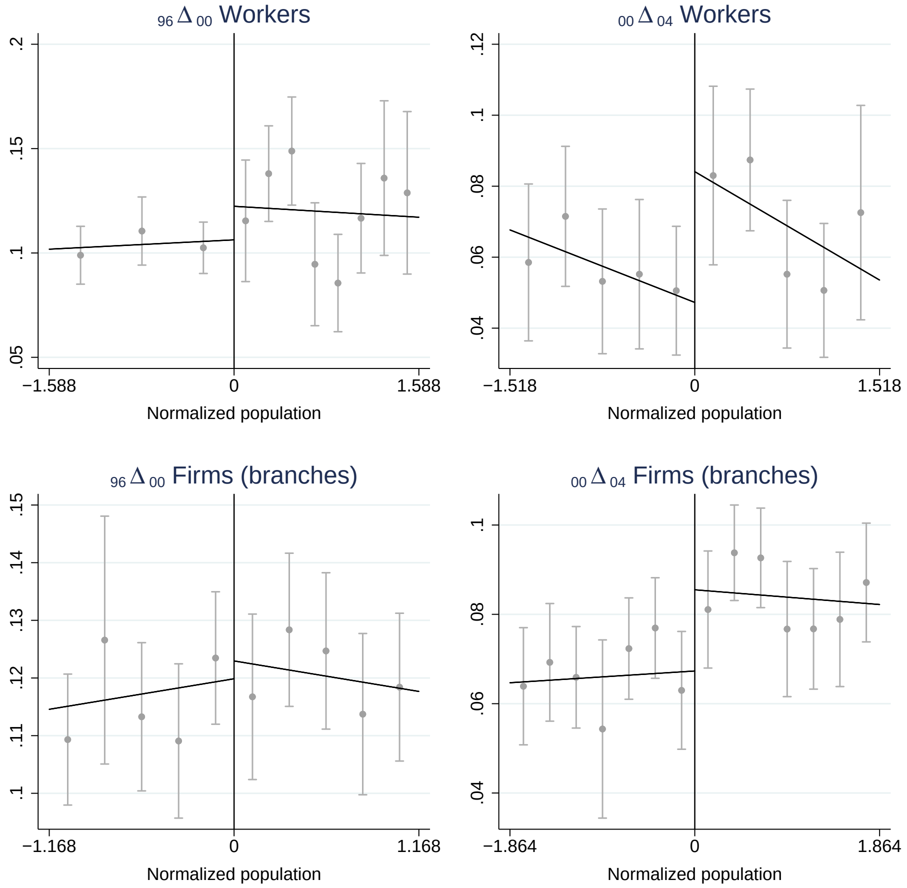
	Δ Workers		Δ Firms (Branches)	
	Δ_{96}^{00}	Δ_{00}^{04}	Δ_{96}^{00}	Δ_{00}^{04}
Conventional	0.022	0.046**	-0.006	0.022**
	(0.016)	(0.021)	(0.013)	(0.010)
Bias-corrected	0.022	0.052**	-0.009	0.024**
	(0.016)	(0.021)	(0.013)	(0.010)
Robust	0.022	0.052**	-0.009	0.024**
	(0.018)	(0.024)	(0.015)	(0.012)
Estimation				
Bandwidth	1.588	1.518	1.168	1.864
Obs	924	873	673	1092
Bias-correction				
Bandwidth	2.310	2.417	1.820	2.853
Obs	1380	1442	1050	1728

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1[\text{municipality size} \geq 5,000 \text{ inhabitants in } 2001]$. The outcome is the average growth rate over the period, e.g. for $2000 - 2004$, $\Delta Y_{04-00} = (Y_{2004} - Y_{2000})/(\frac{1}{2}Y_{2004} + \frac{1}{2}Y_{2000})$. All variables Y are in per capita terms and nominal values. 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, Cattaneo, Farrell, and Titiunik, 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.

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

Figure 8: Visual analysis of the estimated downstream effects of the DSP.



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 according to Calonico, Cattaneo, and Titiunik, 2015 (confidence interval at 90%). Y-axis: average growth rate of the outcome variable (before and after the introduction of the DSP), e.g. $\Delta Y_{04-00} = (Y_{2004} - Y_{2000})/(\frac{1}{2}Y_{2004} + \frac{1}{2}Y_{2000})$. X-axis: legal population at 2001 normalized at the threshold = 5,000 (thousands of inhabitants).

Table 11: Downstream effects of the DSP. Sensitivity to the polynomial order specifications.

	Δ_{04}^{00} Workers			Δ_{04}^{00} Firms (branches)		
	poly=0	poly=1	poly=2	poly=0	poly=1	poly=2
Conventional	0.031**	0.046**	0.052**	0.020***	0.022**	0.014
	(0.014)	(0.021)	(0.026)	(0.007)	(0.010)	(0.016)
Bias-corrected	0.040***	0.052**	0.053**	0.022***	0.024**	0.010
	(0.014)	(0.021)	(0.026)	(0.007)	(0.010)	(0.016)
Robust	0.040**	0.052**	0.053*	0.022**	0.024**	0.010
	(0.018)	(0.024)	(0.030)	(0.009)	(0.012)	(0.018)
Estimation						
Bandwidth	0.959	1.518	2.033	1.275	1.864	1.652
Obs	560	873	1231	742	1092	963
Bias-correction						
Bandwidth	2.221	2.417	2.615	2.091	2.853	2.243
Obs	1341	1442	1565	1265	1728	1348

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1$[municipality size $\geq$ $5,000$ inhabitants in 2001]. The outcome is the average growth rate over the period $2000 - 2004$, i.e. $\Delta Y_{04-00} = (Y_{2004} - Y_{2000})/(\frac{1}{2}Y_{2004} + \frac{1}{2}Y_{2000})$. All variables Y are in per capita terms and nominal values. 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, Cattaneo, Farrell, and Titiunik, 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.

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

Table 12: Downstream effects of the DSP. Covariates-adjusted estimates.

	Δ_{00}^{04} Workers			Δ_{00}^{04} Firms (branches)		
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	0.039*	0.042**	0.044**	0.020**	0.020**	0.023***
	(0.020)	(0.021)	(0.020)	(0.010)	(0.009)	(0.009)
Bias-corrected	0.045**	0.048**	0.049**	0.022**	0.022**	0.026***
	(0.020)	(0.021)	(0.020)	(0.010)	(0.009)	(0.009)
Robust	0.045*	0.048**	0.049**	0.022*	0.022**	0.026**
	(0.024)	(0.024)	(0.023)	(0.012)	(0.011)	(0.010)
Estimation						
Bandwidth	1.566	1.530	1.494	1.731	1.705	1.826
Obs	908	883	854	1005	987	1051
Bias-correction						
Bandwidth	2.478	2.442	2.346	2.677	2.653	2.967
Obs	1477	1456	1394	1606	1590	1788
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

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1$[municipality size $\geq 5,000$ inhabitants in 2001]. The outcome is the average growth rate over the period for $2000 - 2004$, i.e. $\Delta Y_{04-00} = (Y_{2004} - Y_{2000})/(\frac{1}{2}Y_{2004} + \frac{1}{2}Y_{2000})$. All variables Y are in per capita terms and nominal values. 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, Cattaneo, Farrell, and Titiunik, 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. Covariates (pre-reform values): Geographics (dummies for macro-regions), budget (personnel/current spending, loan repayment/total spending), house supply (houses per capita, share of empty houses, share of houses for rent, slope of terrain), house demand (share of services over economic activities, share of old and foreign people, employment rate, population density).

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

Table 13: Downstream effects of the DSP. Testing for spillovers.

	Δ Workers		Δ Firms (Branches)	
	Δ_{96}^{00}	Δ_{00}^{04}	Δ_{96}^{00}	Δ_{00}^{04}
Conventional	0.019	0.043**	-0.002	0.027***
	(0.013)	(0.019)	(0.009)	(0.010)
Bandwidth	1.799	1.384	1.38	1.833
Obs	1204	922	917	1234
adj-R^2	0.002	0.006	0.004	0.018

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1$[municipality size $\geq 5,000$ inhabitants in 2001]. The outcome is the average growth rate over the period, e.g. for $2000 - 2004$, $\Delta Y_{04-00} = (Y_{2004} - Y_{2000})/(\frac{1}{2}Y_{2004} + \frac{1}{2}Y_{2000})$. All variables Y are in per capita terms and nominal values. Method: local linear regression and optimal-MSE bandwidth. Estimates: *Conventional* estimates follow from parametric weighted least-square estimation, ignoring the smoothing bias. Standard errors are clustered at the province level. Municipalities are weighted: weights for control municipalities are defined according to the log-distance (minutes) from the closest treated municipality; while for treated municipalities the distance is equal to the average distance registered by control municipalities.

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

Table 14: Downstream effects of the DSP. Δ_{2000}^{2004} Firms (branches) by economic sector.

	Private (Trade)	Private (Others)	Public services
Conventional	0.026**	0.015	0.008
	(0.012)	(0.011)	(0.019)
Bias-corrected	0.028**	0.018	0.004
	(0.012)	(0.011)	(0.019)
Robust	0.028**	0.018	0.004
	(0.014)	(0.013)	(0.022)
Estimation			
Bandwidth	1.185	1.827	1.488
Obs	685	1053	847
Bias-correction			
Bandwidth	1.917	3.156	2.238
Obs	1130	1901	1338

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1[\text{municipality size} \geq 5,000 \text{ inhabitants in 2001}]$. The outcome is the average growth rate over the period $2000-2004$, i.e. $\Delta Y_{04-00} = (Y_{2004} - Y_{2000})/(\frac{1}{2}Y_{2004} + \frac{1}{2}Y_{2000})$. All variables Y are in per capita terms and nominal values. 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, Cattaneo, Farrell, and Titiunik, 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. Economic sectors (macro-areas): Private (Trade) includes wholesale trade, hotel, restaurant and catering services, transport and storage; Private (Others) includes all the remaining private sectors; Public services includes education, health and other social services (public administration is not included).

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

Table 15: Downstream effects of the DSP. Δ_{2000}^{2004} Workers by economic sector.

	Private (Trade)	Private (Others)	Public Services
Conventional	0.044**	0.032	0.017
	(0.022)	(0.026)	(0.043)
Bias-corrected	0.051**	0.035	0.017
	(0.022)	(0.026)	(0.043)
Robust	0.051**	0.035	0.017
	(0.025)	(0.03)	(0.052)
Estimation			
Bandwidth	1.562	1.533	1.521
Obs	905	883	875
Bias-correction			
Bandwidth	3.039	2.253	2.318
Obs	1824	1349	1375

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy D = 1[municipality size $\geq$ 5,000 inhabitants in 2001]. The outcome is the average growth rate over the period 2000 − 2004, i.e. $\Delta Y_{04-00} = (Y_{2004} - Y_{2000})/(\frac{1}{2}Y_{2004} + \frac{1}{2}Y_{2000})$. All variables Y are in per capita terms and nominal values. 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, Cattaneo, Farrell, and Titiunik, 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. Economic sectors (macro-areas): Private (Trade) includes wholesale trade, hotel, restaurant and catering services, transport and storage; Private (Others) includes all the remaining private sectors; Public services includes education, health and other social services (public administration is not included) 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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A Appendix

Table A.1: Effect of the DSP on 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	0.227***	0.225***	0.305*	0.307*	0.305*	0.307*	0.076	0.075
	(0.086)	(0.083)	(0.156)	(0.169)	(0.156)	(0.169)	(0.112)	(0.109)
Bias-corrected	0.252***	0.251***	0.332**	0.337**	0.332**	0.337**	0.078	0.076
	(0.086)	(0.083)	(0.156)	(0.169)	(0.156)	(0.169)	(0.112)	(0.109)
Robust	0.252**	0.251***	0.332*	0.337*	0.332*	0.337*	0.078	0.076
	(0.099)	(0.097)	(0.186)	(0.203)	(0.186)	(0.203)	(0.135)	(0.131)
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

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1[\text{municipality size} \geq 5{,}000 \text{ inhabitants in } 2001]$. The outcome is the average growth rate over the period 1999 – 2000 vs 2003 – 2004, $\Delta Y = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits), [R] stands for residential. 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, Cattaneo, Farrell, and Titiunik, 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 level of the running variable, i.e. population (POP), or at the level of the Local Labor Market (LLM).

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

Table A.2: Effect of the DSP on municipalities larger than 5,000 inhab: difference between Δ^{03-04}_{99-00} and Δ^{01-02}_{99-00} (triple difference).

	Δ urb rev	Δ permits	Δ permits [NR]	Δ permits [R]
Conventional	0.187**	0.172*	0.291*	0.002
	(0.092)	(0.094)	(0.153)	(0.097)
Bias-corrected	0.210**	0.200**	0.328**	-0.030
	(0.092)	(0.094)	(0.153)	(0.097)
Robust	0.210**	0.200*	0.328*	-0.030
	(0.105)	(0.103)	(0.185)	(0.116)
Estimation				
Bandwidth	1.162	1.490	1.348	1.447
Obs	744	947	761	905
Bias-correction				
Bandwidth	2.006	3.102	2.340	2.199
Obs	1348	2164	1332	1449

Standard errors in parentheses

Notes. RD estimates of the coefficient on the treatment dummy $D = 1[\text{municipality size} \geq 5,000 \text{ inhabitants in } 2001]$. The outcome is the difference between the average growth rate over the period (1999 − 2000 vs 2003 − 2004) and (1999 − 2000 vs 2002 − 2001), where $\Delta^{03-04}_{99-00} = (\bar{Y}_{03-04} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{03-04} + \frac{1}{2}\bar{Y}_{99-00})$ and $\Delta^{01-02}_{99-00} = (\bar{Y}_{01-02} - \bar{Y}_{99-00})/(\frac{1}{2}\bar{Y}_{01-02} + \frac{1}{2}\bar{Y}_{99-00})$. All variables Y are in per capita terms and nominal values. [NR] stands for non residential (building permits), [R] stands for residential. 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, Cattaneo, Farrell, and Titiunik, 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.

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

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