Housing booms and local spending

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Abstract:

This paper examines how local governments adjust their spending in response to a temporary revenue windfall generated by a housing boom. We focus on Spanish local governments because of the intensity of the last housing boom-bust experienced there and the large share of construction-related revenues they obtain. We find that windfall revenues were mostly used to increase expenditures (above all, current). We seek to determine whether this behaviour was due to political myopia (incumbents in contested elections increasing expenditures to convince uninformed voters about their competence) or to extrapolation bias (leading to the overstatement of the persistence of revenue shocks). We find evidence for both mechanisms: the propensity to spend is higher where local incumbents were elected by a narrow vote margin and lower in places with past volatility experience. Finally, we also examine what happens during the bust, and find that governments enjoying large windfalls during the boom had to cut their spending abruptly (above all, capital) and raise taxes. The adjustment during the bust was actually greater in those places that overspent more during the crisis.

Key words: housing booms and busts; tax volatility; policy myopia

JEL Classification: H72, R2, R5

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1. Introduction

News of unexpected increases in tax returns is generally considered good. Yet, a revenue windfall is often only temporary, turning into a shortfall with little warning. This is frequently the case when a government is overly reliant on volatile revenue sources. A good example is that of California and the state budget's excessive reliance on capital gains taxes, which makes the evolution of revenues especially sensitive to stock market developments. Likewise, the collapse of the housing market in Spain generated similar problems for local governments that had become excessively reliant on construction-related revenues. Such situations require prudent fiscal management: windfall revenues obtained during a boom should, for the most part, be saved. The consequence of failing to save during the boom is fiscal stress during the bust when these revenues vanish, leading to abrupt cuts in spending and to deterioration in public services and/or to tax increases. Two excerpts from the Spanish media illustrate the situation during the boom-bust cycle, respectively:

> '*The municipalities (…) see in housing construction their main and most tempting source of finance*.' (El País, 10/6/2006).
>
> '*The bursting of the real estate bubble hit the local council hard and construction revenues fell by 96%, leading to bankruptcy. Services were paralyzed; payments to workers and suppliers were delayed. Even council members went unpaid'*" (Eleconomista.es, 10/2/2011).

Here, we study how local governments react to temporary revenue windfalls attributable to asset booms. Earlier papers examining the ability of local governments to smooth spending faced the challenge of ensuring that actual revenue changes were not capturing the effects of permanent revenue shocks. Our main contribution in this paper, therefore, is to overcome this problem by confirming the temporary nature of the windfall revenues. To do so, we draw on data from Spanish local governments for the period 1995-2011, years in which these governments were affected by a huge, unexpected, temporary fluctuation in their revenues, attributable to the housing boom and bust. The reason for this abrupt swing lies in the fact that Spanish municipalities obtain a large share of their revenues from property transaction taxes, including a betterment tax, a tax on construction budgets, development and building permit fees, and land sales income (see section 3).

Using data from local governments in Spain's largest urban areas, we perform three different analyses. First, we study the average reaction to the boom windfall. To this end, we estimate a first-differences equation that relates the change in expenditures from the start to the peak of the boom to the change in construction-related revenues during the same period. Our preferred result is obtained from an equation estimated by two-stage least squares (2SLS), using the amount of vacant land (i.e., land legally classified as developable but not yet developed) at the beginning of the boom period as an instrument. Our results show that a large share of the windfall (around 70%) was used to fund spending increases. The remainder was saved for the lean times ahead, as we do not find any evidence of a significant adjustment in other margins (e.g., taxes or grants). Current spending was the most heavily affected item (accounting for 50% of the windfall) and, in particular, spending on personnel (30%).

Second, we seek to identify which local governments are most prone to spending their windfall revenues during booms. Specifically, we investigate whether this behaviour is attributable to political myopia or extrapolation bias. To examine the first mechanism, we consider the possibility that local incumbents increase expenditures to convince uninformed voters about their competence when elections are highly competitive. Our results provide support for this mechanism: local incumbents that won the elections by a narrow margin have a higher propensity to spend. To assess the second mechanism, we examine whether the propensity to spend is due to the overstatement of the long-run persistence of the revenue shock. We find that municipalities that experienced high volatility in the housing construction sector in previous boom-bust cycles do have a lower propensity to spend.

Finally, we also examine how local governments cope with the loss of construction revenues during a bust. We find that construction revenue shortfalls were followed by abrupt cuts in spending (around 70%), tax increases (10%) and deficit spending (the remaining 20%). Personnel expenditures were not cut at all, the bulk of the adjustment falling on capital spending. Thus, after the complete housing boom-bust cycle, cities that had enjoyed larger construction revenue windfalls ended up with larger deficits, higher taxes, and an expenditure budget biased towards current (especially personnel) spending. Moreover, the adjustment seems to have been stronger in those municipalities that overspent most during the boom (those without past experience of volatility and with narrow margins of victory). As such, it would appear that overspending during the boom years created considerable stress during the bust.

The paper contributes to various strands in the literature. First, there are the works that study whether local governments are forward-looking agents (see Holtz-Eakin et al., 1994; Dahlberg and Lindström, 1998; Børge and Tovmo, 2009; Persson, 2016). These papers estimate the response of public consumption to actual changes in tax revenues using dynamic panel data methods to isolate the temporary shock. We enhance this approach by using a new identification strategy. We also explore in greater depth the mechanisms that might account for the results. For instance, these papers do not take into account the effect of the persistence of the revenue shock (as is now customary in the private consumption literature, see, e.g., Sørensen and Luengo-Prado, 2008) and rarely consider political economy explanations (though see Børge and Tovmo, 2009, by way of exception).

Second, the paper is related to recent studies in political economy examining electorally induced policy myopia. See, for example, Bonfiglioni and Gancia (2013) on the incentives to delay reforms, Matsen et al. (2016) on the over-exploitation of natural resources, Bagchi (2016) on pension under-funding, and Pérignon and Vallée (2017) on local government loan choices that conceal the cost of debt[1]. In these papers, contested elections generate incentives to focus on the short-run policy effects. We extend this literature by studying the effects of electoral competition on the fiscal response to volatility.

Finally, the paper also contributes to our understanding of the causes and effects of housing booms and busts. Several papers have already highlighted the role of buyers' expectations during booms (Glaeser and Nathanson, 2015 and 2017). Other papers study the effects of the ups and downs of housing prices on household consumption (Mian et al., 2013). There are also several studies that examine the effect of the last housing boom on local public finances in the U.S.: Vlaicu and Whalley (2011), Alm et al. (2011), Lutz et al. (2011) and Ihlanfeldt and Doerner (2011). The paper most similar to ours is the one by Davis and Ferreira (2018). The authors leverage variation from the trend break in housing prices across U.S. metro areas during the boom (see also Ferreira and Gyourko, 2018) to study the effect of increases in property tax collections on school spending.

The rest of the paper is organized as follows. Section 2 presents a theoretical framework that helps interpret the findings. Section 3 provides some institutional background on Spain. Section 4 presents the data and describes the empirical specification. Section 5 presents the results. The last section discusses policy implications and concludes.

2. Theoretical framework

We present a framework aimed at organizing the different stories that might explain the response of local spending to a temporary windfall. First, we present a baseline setting in which the incumbent is informed about the nature of the shock and concerned with voter welfare. Second, we examine the story of political myopia, focusing on an informed politician who cares about winning elections. Third, we analyse the case of an incumbent who is unable to assess the persistence of the revenue shock. Finally, we discuss a few additional stories.

2.1 Baseline setting

Let's assume that the local incumbent is fully aware of the temporary nature of the shock and cares solely about voter welfare. To keep the problem simple, the welfare of a representative voter is expressed as:

$$W = \ln(e_1) + \ln(e_2) \quad (1)$$

where e_1 and e_2 denote local spending in periods 1 and 2, that is, the periods of 'boom' and 'bust', respectively. The budget constraint of the government in periods 1 and 2 is

[1] The literature on the effects of electoral competition on policy outcomes is more extensive. Some of the papers suggest that competition helps curb rent extraction (e.g., Besley et al., 2010;) while others suggest it might be less benign if voters are uninformed (e.g., Ashworth, 2012).

$e_1 = r_1 - s$ and $e_2 = r_2 + s$, where r_1 and r_2 are exogenous revenues, and s are savings. The inter-temporal budget constraint can be written as:

$$e_1 + e_2 = r_1 + r_2 = r \tag{2}$$

Revenues are ordinary revenues t (i.e., that do not fluctuate) plus the total temporary windfall of construction revenues c, so that $r_1 = t + c$ and $r_2 = t$.

Finding $e_2 = r - e_1$, substituting this in (1) and maximizing w.r.t. e_1 we obtain:

$$e_1^* = e_2^* = t + \frac{1}{2}c \tag{3}$$

which indicates that spending should be constant over time and in each period equal to the permanent revenues t plus ½ of the temporary windfall. So, in this case the marginal propensity to spend (and save) out of the temporary windfall is:

$$\frac{\partial e_1^*}{\partial c} = \frac{1}{2} \quad \text{and} \quad \frac{\partial s^*}{\partial c} = \frac{1}{2} \tag{4}$$

This simply says that the marginal propensity to spend is far from unity and that a substantial share of a temporary windfall should be saved. In fact, in an infinite-period model the marginal propensity to spend out of a temporary windfall will be close to zero. This is a well-known result in consumption theory (see Hall, 1978; Jappelli and Pistaferri, 2010). Of course, the prediction may change if we relax some assumptions implicit in the framework as, for example, the absence of liquidity constraints. We discuss this and other possibilities in section 2.4.

2.2. Political myopia

Another possible explanation for overspending (i.e., spending a larger share than that suggested above) is political myopia. The intuition is that local incumbents overspend the windfall to convince uninformed voters of their competence and so as to win the next election. This prediction can be generated by a dynamic career-concerns model similar to those developed in Hölstrom (1999) and Bonfiglioni and Gancia (2013).

In the Online Appendix we extend our baseline model in this direction. The model assumes that the local incumbent cares about the residents' welfare but also about their own re-election. Voters are uninformed in the sense that while they observe the quality of public services, they are ignorant of local government spending and savings decisions. Therefore, voters tend to re-elect profligate politicians (who are seen to be more capable), providing incentives to the incumbent to spend in the first period to get re-elected.

The model generates two main predictions. First, the marginal propensity of an office-seeking politician to spend (save) out of a temporary windfall is greater (smaller) than that of a benevolent politician. Second, the propensity to spend (save) decreases (increases) the less competitive the elections are. This provides the basis for our empirical analysis that studies whether political competition (proxied by the incumbent's winning margin of victory) influences the impact of the temporary windfall on spending.

2.3. Extrapolation bias

A local incumbent might be overspending out of a temporary windfall because she may not be fully aware of the degree of persistence of the revenue shock. Various studies demonstrate that people are not good at forecasting mean reversion processes of hump-shaped macro variables. They tend to extrapolate future growth from recent experiences and, as a result, are prone to believe that shocks are more persistent than they really are (Fuster *et al*., 2010). Extrapolative beliefs of this kind are a plausible explanation of the behaviour of economic agents during housing booms (Glaeser and Nathanson, 2017). Notice that housing prices and construction exhibit short-run momentum and long-run mean reversion (Glaeser and Nathanson, 2015) and that construction revenues in Spain, which mimic the evolution of housing construction (see section 3), also exhibit these properties.

It is easy to see that allowing for a behaviour of this type would modify the predicted reactions of spending and savings to a windfall. Assume, for example, that revenues in period 2 are $r_2 = t + \rho c$ where $\rho \in [0,1]$ is the persistence of the construction windfall 'as perceived by the local incumbent'. This means the propensity to spend will be $(1+\rho)/2$, which is larger than ½. In order to understand this mechanism better, we seek to determine whether the reaction to the windfall is influenced by past experiences of construction revenue volatility. Our intuition is that, during previous housing boom-bust cycles, Spanish local governments may have learned something about the long-run persistence of construction revenues, meaning that their spending and saving decisions during the current boom might not be fully conditioned by short-run dynamics. On the contrary, the municipalities experiencing the greatest swings in construction revenues in the past should predict greater swings in the future and, thus, spend a lower share of the windfall[2]. We show how this idea can be operationalized in section 5.

2.4. Other stories

Liquidity constraints. The baseline model assumes that local governments are able to access credit free of constraint. It is known, however, that the propensity to spend out of transitory income is higher when there are liquidity constraints (Flavin, 1985) and/or when consumers are prudent and keep a savings buffer for leaner times (Carroll, 1997). See Persson (2016) for evidence that liquidity constraints do matter for local spending behaviour in Sweden and Craig *et al*. (2016) for evidence that the behaviour of U.S. state governments is consistent with the buffer-stock model of savings. To account for this, we examine whether the debt burden influences the spending response to the windfall.

Catching-up. Local spending might stimulate the local economy (e.g., Suárez-Serrato and Wingender, 2016) and so increase future ordinary tax revenues, suggesting it might

[2] For example, some papers claim that while it is true that individuals adapt their forecasts to new data, the information derived from prominent episodes experienced during their lifetime may outweigh this. For evidence in the case inflation, see Nagel and Malmendier (2016).

be optimal to spend a share of the windfall with this goal in mind. However, the small size of Spanish municipalities and the share that local taxes represent in their income suggest this effect will be limited. In any case, so as to take this possibility into account we examine whether per capita income influences the effects of the windfall on spending. Note that this story applies primarily to infrastructure spending so that the results by spending category should help us tease out the relevance of this explanation.

3. Institutional context

Spain's local public finances. Spain has more than 8,000 municipalities, although most are quite small. The municipalities constitute multi-purpose governments, and their main spending categories coincide with responsibilities that elsewhere are typically assigned to local governments (i.e., environmental services, planning, urban infrastructure, transportation, welfare, etc.) with the exception of education, a responsibility assigned to Spain's regional governments. Local spending amounts to around fifteen per cent of public spending. Own revenues account for more than two thirds of current revenues, the remaining third being met by grants. Two thirds of the municipalities' own revenues come from taxes with the remaining third originating from user charges. The main taxes are the *Property tax*, the *Local vehicle tax* and the *Local business tax*, which account for 50, 25, and 10% of tax revenues, respectively. Local governments have the autonomy to fix their own tax rates, albeit within certain limits. Maximum tax rates are generally non-binding, although minimum tax rates might be binding for affluent municipalities. This means that some municipalities that receive a huge windfall and which react by cutting taxes might hit the minimum and so, in practice, may be constrained in their reaction. In any case, in the empirical analysis, we also study the role of taxes in the reaction to the windfall.

Construction revenues. Spain's municipalities also generate extraordinary revenues, related in the main to housing construction. Chief among these is the *Betterment tax,* which is a capital gains tax on the land portion of a real estate transaction. The tax base is estimated using the current assessed value of land and the number of years since acquisition. Second, there is the *Construction tax*, which is paid by the owner of a construction project (i.e., either the developer or the homeowner). The tax base is the construction budget. Both taxes have a single *ad valorem* tax rate, set by municipalities between certain limits. Most municipalities fix the maximum tax rate as allowed by law. Third, there are *Developer's fees*, including *Building permits* and payments in exchange for development duties. Developers have the duty to contribute a share of developed land (the part required for streets plus 10% of the developed land), or to provide the equivalent value in money (see Riera *et al*., 1991, for a detailed explanation). Fourth, there are revenues from the *Sales of land plots* contributed by the developers.

[Insert Figure 1]

Panel (a) in Figure 1 – which tracks the evolution of housing construction and of transactions – shows how these revenues fluctuated over the housing cycle. Note that construction revenues more than doubled during the boom (1995-2007) but then virtually disappeared in the bust (2008-2011). This behaviour was unique to revenue instruments of this type; ordinary taxes (e.g., property tax) were unaffected by these fluctua-tions (see Panel (b) in the same figure). The swing was clearly abrupt and had a major impact on local budgets. The revenue share jumped from 12% in 1995 to 21% in 2007, before falling again to 9% in 2011 (see Table A.1 in the Online Appendix).

Some comments on the nature of these revenues are in order. First, some of these items are in fact intended to finance infrastructure, which means, in theory, the amount raised should be used to offset the building cost (Slack, 2002). If this were the case, however, no windfall would materialise. In practice, this is of limited application, first, because developers' duties also include the direct provision of the main infrastructure (including, street paving, lighting, and sewage), meaning that revenues tend to exceed by several factors the needs created by the urbanization process; second, because developers also provide lump-sum contributions (in land or in money) so that the community can obtain a share of urbanization profits[3] (these contributions are not designed to match urbanization costs and thus constitute a windfall); and, third, because the regulations that should help to keep these revenues out of the current budget are either nonexistent (in the case of taxes) or often circumvented (in the case of land sales).

Debt limits. Spanish municipalities also have the autonomy to use debt to cover their capital spending. Capital projects are funded from current account savings, with earmarked capital transfers, extraordinary revenues and with debt. During the period under study, legally binding debt limits were in place. The debt burden and debt stock could not exceed 25 and 115% of current revenues, respectively. Reaching these thresholds does in theory trigger the imposition of a local adjustment plan that forces the accumulation of savings during a period of years. However, very few municipalities had reached this limit by the start of the boom. Despite this, many governments entered the boom period with a sizeable amount of debt inherited from the previous crisis. It is unclear as to whether this hindered access to finance in a period of plentiful credit.

Risk-sharing grants. Most of the intergovernmental grants received by Spanish local governments are unconditional formula transfers (e.g., 80% of all current grants are of this kind). The main transfer of this type has a component in its formula that compensates municipalities with low fiscal capacity. Note, however, that only ordinary taxes (e.g., the property tax) are included in the calculation of fiscal capacity. Therefore, there is no element embedded in the formulation of grants that is designed to compensate for losses in construction revenues. Moreover, we did not find any anecdotal evidence of

[3] See Peterson (2008) for a review of 'land value capture' policies. Several of the construction-related sources used in Spain follow the logic of sharing urbanization profits with citizens. This precept is in fact included in the Spanish Constitution (see article 47: "the community shall participate in the capital gains generated by the urban regulations.")

shortfalls in construction revenues being compensated by transfers. In any case, in the empirical analysis below we check whether grants played some role in the reaction to the windfall.

4. Data and empirical specification

4.1. Sample

We estimate the effects of construction revenues on the local spending of Spanish municipalities. We focus specifically on municipalities in Spain's largest urban areas connected to the highway network. Our choice is guided by the following motives. First, the housing boom was much more intense in these locations than in the rest of the country[4]. This means that land regulation constraints are probably more binding there and that, as a result, the amount of vacant land – our regulatory indicator – should be a more powerful driver of housing construction (and of construction revenues) in this sample[5]. Second, the choice helps reduce the heterogeneity in our data and makes municipalities receiving large vs. small windfalls (or with large vs. small amounts of vacant land) much more comparable[6]. See section 4.3 for a detailed discussion of the identification strategy.

The urban area delimitations are provided by AUDES (http://alarcos. esi.uclmes/per/fruiz/audes/) and are based on commuting patterns and the physical continuity of the build-out area. This project defines a large urban area as one that has a central city of at least 50,000 residents and a sizeable conurbation. This gives us a total of 69 urban areas[7]. Access to the highway network is defined as having a highway ramp or a direct connection to a major two-lane road as of 1995. The road data are taken from Garcia-López *et al.* (2015). For reasons of data availability, we are obliged to focus on municipalities with a population greater than 1,000 residents in all the years. This gives us a total of 410 municipalities. We have been able to assemble the required budget data for the whole period of analysis (1993-2011) for 314 of these. These data are drawn from a survey that covers all the largest municipalities (i.e., above 5,000 residents) and a sample of the smaller ones and was undertaken by the Spanish Ministry of Finance[8].

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[4] Both housing construction and the amount of land zoned for development grew much more during the last boom in urban municipalities (Solé-Ollé and Viladecans-Marsal, 2012) and in municipalities connected to the highway network (García-López *et al.*, 2015).

[5] Municipalities in rural and/or less accessible locations have plenty of available land and face little opposition to development (see Hilber and Robert-Nicoud, 2013). For this reason, they tend to have low levels of regulation. The amount of vacant land is meaningless in such contexts.

[6] We compare urban municipalities connected to a highway to other municipalities with these same traits located in the same urban area and at the same distance from the central city. We consider these municipalities as being highly likely to be similar in terms both of availability of land and past and future demand for construction.

[7] The urban areas of the Canary Islands, the Balearic Islands, the Basque Country and Navarra had to be excluded from the analysis due to data availability issues.

[8] The selection of the small municipalities in this survey was in theory random so our final sample should be representative. This is in fact confirmed by comparing the values in the restricted sample to those of the original sample. Results are available upon request.

4.2. Data sources

Construction revenues. These are revenues derived from the *Betterment tax* and the *Construction tax* and from *Developer's fees* and *Sales of land* (see section 3). These revenues (as all the budget data) have been computed from outlay data, have been deflated using a provincial price index, and are expressed in per capita terms. Both the price index and the yearly official population data are provided by the National Institute of Statistics (www. ine.es) . Windfalls are increases in per capita revenues from these taxes between a base pre-boom period (1993-95) and a period covering the years at the peak of the boom (2004-07). Shortfalls are defined as decreases in revenues per capita from these taxes between the peak of the boom (2004-07) and the bust (2008-11)[10,11].

Budget data. Changes in expenditures are defined in the same way as for construction revenues: differences in real average outlays per capita between terms. Spending is total non-financial spending. We also analyse the breakdown of spending into Current and Capital spending, and the breakdown of current spending into Personnel, Purchases and Transfers[12]. Ordinary revenues are Tax revenues (excluding construction related taxes) and Grants. In addition to these data, we use initial budget data from the period 1986-95 to analyse spending pre-trends. The data are provided by the Spanish Ministry of Finance.

Land use data. Our identification strategy relies on the use of the amount of vacant land at the beginning of the boom (land zoned for development but not yet developed) as an instrument (see section 4.3 for details). The amount of vacant land has been obtained from a database provided by the National Property Assessment Office, which assesses property values across Spain. A by-product of the work undertaken to update its property register is a complete database on the status of all land plots in Spain. This database can be accessed online (http://www.catastro.meh.es/esp/estadisticas.asp) and provides information of the amount of land classified, since 1995, as developed, developable (not

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[9] Table A.2 in the Online Appendix reports the data sources. Table A.3 shows the descriptive statistics for the construction windfalls and for the other variables used in the paper.

[10] We fix the pre-boom period to the years 1993 to 1995 so as to ensure that vacant land is measured before housing construction started to increase. The boom did not begin earlier than 1997 and was not perceptible in most urban areas before 1999. The results are robust to the use of slightly different periods.

[11] Figures A.1 and A.2 in the Online Appendix provide additional descriptive evidence on the size distribution of windfalls during the boom and the bust.

[12] *Personnel* expenditures include wages of workers directly employed by local government. These are workers deployed in the provision of local services (e.g., kindergarten teachers, police officers) and bureaucrats. Purchases are expenditures on consumables (e.g., school material, gas for police cars, cultural venues, etc.). Transfers include subsidies to families (e.g., scholarships, poverty relief) or to NGOs (e.g., soccer clubs, cultural associations) that also provide some kind of service to the population. Capital expenditures include spending on non-durables (e.g., police cars), on infrastructure (e.g., school buildings, streets), and on maintenance.

yet developed but legally developable), and not developable. The amount of developable land is what we refer to here as vacant land; the value of this variable in 1995, in per capita terms, is what we use as our instrument[13].

Control variables. We use a variety of control variables from a number of sources, the most important being the Housing Census for 2001 and 1991. This data source provides us with annual housing construction statistics, insofar as it records the year of construction of all housing in the country. This data is used to compute our volatility measure. The Housing and the Population Censuses furnish information on the socio-demographic variables used here as controls (including, % renters and % commuters). Building density has been computed as the build-out area (according to the database) and resident population. Political controls (% vote margin, % left voting, coalition dummy) are computed from information on votes and seats at several local elections provided by the Spanish Ministry of Home Affairs.

4.3. Empirical specification

4.3.1. First-differences

In order to study the reaction of local budgets to construction windfalls during the boom we estimate an equation that relates the change in the average value of expenditures between a pre-boom period and a period corresponding to the peak of the boom to the change in construction-related revenues during the same period. This approach allows us to cope with the year-to-year variability in construction revenues and to avoid modelling the complex short-run dynamics of fiscal decisions. A first estimation approach consists of using OLS on an equation of the form:

$$\Delta e_i^{boom} = \alpha * \Delta c_i^{boom} + \beta * x_i^0 + \gamma * \Delta y_i^{boom} + \lambda_j + \lambda_k + \varepsilon_i \qquad (5)$$

where *e* denotes spending per capita and *c* denotes construction revenues (also per capita). Δ is the first-differences operator, the super index *boom* indicating that the difference has been computed between the pre-boom and the peak of the boom. The sub index *i* indicates municipality, *j* indicates urban area (i.e., *j*=1…69) and *k* indicates the distance-to-central city interval (i.e., *k*=1…4, <5km, 5-10km, 10-15km, and ≥ 15km). Note that by taking first differences we are eliminating permanent differences across municipalities in the level of spending and of construction revenues. In addition to that, the equation in (5) controls for base-period municipality characteristics that might be correlated with trends in the evolution of construction revenues (x_i^0). A similar role is played by λ_j and λ_k, which represent a set of urban area dummies and a set of distance-to-central-city interval dummies. The fixed effects aim at capturing trends in local spending common to municipalities in the same urban area (and located at the same distance from the central city) that are also correlated with housing market developments.

[13] See Figure A.3 in the Online Appendix for a graphical illustration of the concept of vacant land.

The equation also controls for changes during the boom period in post-treatment confounders (Δy_i), that is, variables representing alternative channels of influence of housing construction on expenditures. Two variables that stand out here are changes over the period in tax capacity from ordinary taxes and in population growth. Note, for instance, that housing construction might also have an effect on the growth in property tax revenues (per capita) if the new houses are larger, more expensive or assessed at higher values. Controlling for shocks in ordinary revenues (which tend to be much more persistent) is also important to correctly interpret the shocks in construction revenues as truly temporary. Similarly, population growth may be related to additional infrastructural needs related to urbanization. Other variables we consider are changes in personal income, building density (new construction might be more land intensive), changes in the share of immigrants and of young residents (which might influence the demand for local services), and changes in political variables (% vote margin, left-wing vote, coalition government dummy).

4.3.2. Instrumental variables

The main threat to the estimation of the above equation by OLS is the possible endogeneity of construction revenues. Note that in Spain local governments are responsible for zoning regulations. Municipalities are in charge of drafting the Master Plan, which specifies the areas under municipal jurisdiction where building is allowed, as well as the many regulations related to type of activity or building densities (see Solé-Ollé and Viladecans-Marsal, 2012 and 2013)[14]. Local governments determine the amount of new land to be converted from rural to urban uses and this affects the amount of new construction in the coming years (see Garcia-López *et al*., 2015, for evidence) and, thus, the amount of construction-related revenues. This suggests causality might actually run from spending to construction revenues: municipalities might decide to allow for more construction in order to obtain funds to expand their expenditures. This would bias the OLS coefficient of the spending equation upwards. In addition to this, a problem of omitted variables might remain should we not be fully able to account for influences on spending that are correlated with differential trends across municipalities in construction revenues.

The difficulty in dealing with these issues within an OLS framework justifies the use of a different identification approach. We estimate the equation in (5) by 2SLS, using as the instrument the amount of vacant land at the start of the boom, that is the amount

[14] Regional governments are responsible for the design and enforcement of the basic legislation on this matter and can block local plans if they do not comply with the law or interfere with the planning of infrastructure or with the protection of the environment. However, they do not have the power to force local government to develop land.

of land already zoned for development in the past but not yet developed at the start of the boom[15]. The first-stage equation looks like this:

$$\Delta c_i^{boom} = \tau * v_i^0 + \mu * x_i^0 + \delta * \Delta y_i^{boom} + \zeta_j + \zeta_k + \omega_i \quad (6)$$

Three main assumptions have to be fulfilled for v_i^0 to be a valid instrument (see Angrist *et al.*, 1996). First, vacant land needs to be able to predict the increase in construction revenues (i.e., $\tau \neq 0$). Moreover, we know that in order to avoid biased inferences the explanatory power of the instrument has to be substantial, that is the instrument has to be strong (see Staiger and Stock, 1997). Second, vacant land needs to be assigned in a quasi-random way, which means that municipalities with high and low amounts of vacant land should not differ systematically from one another (this is the so-called ignorability assumption). Third, the effect of vacant land on spending needs to be channelled exclusively through construction revenues (this is the so-called exclusion restriction assumption). In this paper, we argue that vacant land fulfils the second and third assumptions conditional on controlling for the two sets of fixed effects mentioned above. Other controls proved not to be relevant. Below we explain the logic of the instrument and discuss why we think it fulfils the three assumptions.

Instrument intuition. For vacant land to explain the change in construction revenues, these revenues need to be sensitive to housing construction and, at the same time, there has to be more housing construction in those places where there is more vacant land to start with. We consider the first part of this statement plausible given that most tax bases included under the concept of construction revenues are computed with information on quantities (i.e., number of housing units built, transactions)[16]. The second part of the statement is also plausible because we know that some cities began the boom with more vacant land than others and because in Spain it takes time for the latter to amend their land use regulations.

This set of circumstances is attributable to the nature of land use regulations in Spain. First, the main regulatory instrument, the Master Plan, specifies the amount of vacant land required to accommodate the municipality's growth needs for a given period of time (i.e., between one and two decades). If we assume that this plan is approved at the beginning of a housing boom-bust cycle, the amount of vacant land at the end of that

[15] This instrument has been previously used by Ihlanfeldt and Mayock (2014) in their study of the effect of land use regulations on the elasticity of housing supply in Florida. They authors found that the amount of land classified as developable has a larger effect on housing supply elasticity. Florida's system of land use regulation is based on comprehensive Master Plans and so is similar to the system employed in Spain. We could have used as an alternative instrument the amount of land suitable for development (see Saiz, 2010). However, this measure (computed by detracting rivers, wetlands and high-slope areas from the land under the municipal jurisdiction) is a weak predictor of construction revenues (the F-stat is around three), and so does not work well as an instrument. Hilber and Vermeulen (2016) also show that regulations matter more than topography for the supply of land in the UK.

[16] Prices also play a role in the computation of tax bases but, in most cases, they are based on the assessed value of the property and are outdated due to property value reassessment lags.

cycle will be equal to the amount of vacant land specified in the plan less the new land build during the whole cycle. This suggests that the vacant land at the end of the housing cycle might be considered as a forecast error (i.e., the difference between forecasted construction and real construction). As a result, a given municipality might start a housing boom with a larger amount of vacant land because politicians were overly optimistic in their forecasts of housing demand in the previous housing cycle. Note that whether a local development project materializes or not is an uncertain event, there being plenty of anecdotal evidence of unfulfilled expectations in this respect[17]. Thus, it might also be the case that housing construction falls short of expectations some years after the plan has been put into implementation.

Second, the Master Plan is a legally binding document that creates rights for landowners. This means that it is very difficult to revert the development status of a land plot cannot to its previous use without adequately compensating its owner (see Riera *et al.*, 1991), something that Spanish local governments are generally not able to do due to their lack of financial resources. This being the case, past planning forecast errors are not easily rectified. Similarly, the process of amending the plan is especially complex and lengthy and the subsequent process of land assembly is also very slow[18]. During this process housing construction tends to slow down, either because of legal provisions (i.e., permitting stops while the plan is discussed), or because when the local government initiates the amendment process the old plan is already obsolete, which means that the amount (or type) of vacant land remaining is insufficient to sustain the current rate of construction (see Martínez Mora and Sáez-Fernández, 2009, for evidence). For all these reasons, municipalities starting the boom with more vacant land will end up building more. Figure A.4 in the Online Appendix provides evidence of the correlation between these two variables.

Strength. The amount of vacant land has a clear positive and significant impact on windfall revenues during the boom. The F-statistic is close to twenty in all specifications, and this holds despite the addition of urban area and distance-to-central-city interval fixed effects and of different types of control variables (see next section for details).

Ignorability. Since our equation is estimated in first-differences the fulfilment of this condition requires that, prior to the period of study, local spending (or other budget items) does not evolve differently in municipalities with high vs. low amounts of vacant land. We can check this assumption by estimating placebo equations that examine the effect of vacant land as of 1995 on changes in local spending (and other budget items) in previous periods. More specifically, we examine the effect on the change in spending, tax revenues and grants (which are the variables we study later on) during the previous

[17] Many anecdotes concern failed attempts to attract big industrial plants and the consequences of this for the amount of vacant land ("The industrial estate bubble explodes", *El Economista* 22/10/2012).

[18] See Brooks and Lutz (2016) on how land use regulations affect land assembly in the US.

boom-bust cycle (1986-87 to 1993-95) and also during the previous boom (1986-87 to 1990-91) and bust (1990-91 to 1993-95).

[Insert Table 1]

The results are shown in Table 1. All equations include the two sets of aforementioned fixed effects. We find that the amount of vacant land at the beginning of the recent boom-bust cycle (that is, in 1995) does not have any effect on the change in budget variables in previous periods – the coefficients are never statistically significant and they are very close to zero. The table also reports the effect of vacant land during the recent boom (i.e., our reduced form equation). Here, in line with our later findings, the effect is statistically significant and quantitatively meaningful for local spending. Thus, local spending seems to have been on the same path prior to the recent boom-bust period in municipalities that had either high or low amounts of vacant land as of 1995. While it is true that finding a zero effect for the bust (1990-91 to 1993-95) is not especially informative (given that there was not much construction activity during that period), the result for the previous boom period is more relevant. If there is a determinant of both vacant land and spending increases that is persistent from one boom to the next, then vacant land should be correlated with changes in spending during both booms, but we do not find that.

One possible limitation of this analysis is that – due to data availability issues – the budget data employed do not refer to outlays but rather to initial budgets. For this reason, we complement the above evidence by determining whether the amount of vacant land is, effectively, uncorrelated with pre-determined covariates in levels. As discussed earlier, in our context, this assumption might not be strictly necessary. However, its fulfilment might be reassuring. There are various reasons as to why the amount of vacant land and the start of the boom might differ from one place to another. First, some urban areas may systematically experience larger housing demand shocks than others. In these areas, local governments might fear more the risk of falling short of land (which means that interesting projects are likely to flight to another location) than the possibility of supplying too much land (with the risk this represents of haphazard or 'leapfrog' development). This suggests that local planners in these places (taking these matters into consideration) will plausibly convert more land than will their counterparts in places that are not expected to grow so much. The results shown in Table A.4 in the Online Appendix (first two columns) suggest that construction during the housing boom-bust cycle of the 1980s does have some ability to explain differences in vacant land in 1995. However, this effect vanishes after we include urban area and distance-to-central-city interval dummies. Second, we also explore the possibility that differences in preferences for development and in the fiscal situation of local governments in the 1980s correlate with the amount of vacant land in 1995[19]. We find that these variables have a low ex-

[19] We control for political variables such as the vote margin, the vote share of left-wing parties and a coalition dummy (pre-shock), variables measuring voter preferences (i.e., income per capi-

planatory power and that neither of them is statistically significant (see also Table A.4)[20].

It seems, therefore, that the municipalities that entered the housing boom-bust cycle of the 1990s with a larger amount of vacant land do not differ systematically from those that had access to a smaller amount of land, at least after including fixed effects. Both groups of municipalities are very similar in terms of their political traits and, also, of their respective financial situations. To further corroborate this idea, we examine the correlation between the amount of vacant land at the end of two housing boom-bust cycles (i.e., in 1995 and 2015). If vacant land is randomly assigned across municipalities, these two variables should be uncorrelated. Figure A.5 in the Online Appendix shows that there is some correlation between the two variables, but that this vanishes once we condition on the two sets of fixed effects. Note that by including these fixed effects in the estimation, we are considering within area differences. We believe that the differences between places with high and low vacant land vanish because, although some urban areas systematically grow more than others, this is less true for different locations within the same urban area (and located at the same distance from the CBD). Land supply is more constrained locally than across areas and, as some places fill up, development moves to places that still have vacant land. Additionally, many locations within a given urban area are close substitutes, especially if they are adjacent or located at the same distance from the central city.

Exclusion restriction. Vacant land, by fuelling housing construction, may also have some effect on property tax revenues, which in turn might fuel spending increases. In addition to this, housing construction might also have an effect on employment, which could, in turn, have an effect on vehicle and business tax bases. We, therefore, need to recognise the possibility that the estimated effect of temporary windfalls impacts other (perhaps more permanent) sources of revenues. Construction activity might also impact personal incomes and building density, thus affecting the demand for and/or the costs of providing local services. We believe that all these effects might also be attenuated by the inclusion of urban area and distance-to-central-city interval dummies. Note that the effect of vacant land on the property tax base depends on the response of housing prices, which is determined at the urban area level, and of the frequency of property reassessments, which are performed close in time for cities in the same housing market. The same can

ta, % college education, % renters and % commuters, all pre-shock), and budgetary variables (i.e., spending and assessed property value per capita, and debt burden ratio, also pre-shock).

[20] Note that this does not contradict the fact that land conversion during a given cycle responds to demand shocks or to preference variables. Recall that the amount of vacant land at the end of the cycle is equal to the amount of land reserved for development at the start minus the amount of land built during the cycle. There is evidence that demand shocks result in an increase in both margins, although land conversion tends to respond earlier than housing construction (see Garcia-López et al., 2015, for evidence in the case of a demand shock fuelled by road construction). Something similar occurs with the effect of ideology: right-wing mayors facilitate both land conversion and housing construction during a boom (see Solé-Ollé and Viladecans-Marsal, 2013).

be said of the effects on employment or income, which tend to spill over to other localities in the area.

To assess the importance of alternative channels of influence of vacant land over local spending (and so as to validate the exclusion restriction), we run different regressions using vacant land as the explanatory variable and several potential post-treatment confounders as the dependent variable: namely, changes in ordinary revenues (i.e., ordinary taxes plus grants), in population growth, in personal income per capita, in building density, in the share of immigrants and young residents, and in the vote margin, in the left-vote share and in the coalition status of the government. In all these reduced form regressions, we control for urban area and distance-to-central-city dummies. We show (see Table A.5 in the Online Appendix) that vacant land at the start of the boom has a statistically significant effect on changes over the boom cycle in ordinary revenues (at the 5% level) and in population growth (at the 10% level), but not on changes in the other outcomes. However, the effect on the growth of ordinary revenues is quite small (i.e., one tenth of the effect on construction revenues) and the effect on population growth is also very small. As such, the exclusion of these two variables hardly affects our estimates.

5. Results

5.1. Main results

Before going into a detailed exposition of the effects of construction revenue windfalls we present a graphical summary of the main results in Figure 2. In Panel (a) we report the bivariate relationship between the increase in expenditures per capita Δe_i^{boom} and the increase in construction revenues per capita Δc_i^{boom}, both during the boom. The two variables have been residualized, meaning that we have purged the influence of the fixed effects. This corresponds to the First-differenced model presented in equation (5). The slope of the relationship is 0.984 and so virtually equal to one, suggesting that most windfall revenues were spent. In Panel (b) we report the relationship between Δc_i^{boom} and v_i^0 –the amount of vacant land per capita at the start of the boom. This equation corresponds to the First-stage of the 2SLS estimation. Panel (c) reports the bivariate relationships between vacant land and the increase in expenditures during the boom (Δe_i^{boom}. This relationship corresponds to the reduced form specification. The 2SLS estimate (our treatment effect) can be obtained as the ratio between the slope of the reduced form and that of the first stage. The estimated effect of the construction windfall during the boom is thus 0.745=0.176/0.236, a number below one but still sizeable. In the next section we will show that this effect is still statistically different from zero. Finally, in Panel (d) we also display the reduced form relationship for the bust period (i.e., the relationship between $\Delta e_i^{bust}$and v_i^0). The 2SLS coefficient can be computed as -0.784=-0.185/0.236. This suggest that the construction windfall generates expenditure cuts during the bust that are similar in size than the expenditure increases it fuelled during the boom.

[Insert Figure 2]

5.2. The boom

In Table 2 we present more detailed estimates of the average response of local spending to construction revenues during the boom: panel (a) reports the 2SLS estimates, panel (b) reports the first-stage, and panel (c) reports the OLS results. In column (1), we do not include any controls; in column (2), we include urban area and distance-to-central-city interval fixed effects; in column (3), we also control for housing demand (i.e., housing construction in the previous housing boom-bust cycle and population size) and for variables that proxy for fiscal stress pre-shock (the spending and property tax base per capita, and the debt burden ratio) and political (% voting margin, % left voting, coalition) and citizen preferences also pre-shock (personal income, % renters, % commuters, and % college educated); finally, in column (4), we control for changes over the boom period in variables that might constitute alternative channels of influence of housing construction on local budgets (i.e., growth in ordinary revenues and population).

[Insert Table 2]

The 2SLS coefficient falls from around 0.9 when no controls are included to around 0.7 when we add the fixed effects. This coefficient is statistically significant at the 1% level. Neither the value of the coefficient nor its statistical significance changes when the different groups of controls are included. The first stage coefficient is also very stable. Only after including the fixed effects does the first-stage coefficient fall slightly. The introduction of the other controls does not change that coefficient at all. The instrument appears to be strong in all cases, given that the Kleibergen-Paap Wald rk F-statistic is always higher than the Stock-Yogo weak ID test critical value at a 10% maximal IV bias.

The results suggest that for every 100 euros of construction revenues, around 70 were used to fund spending increases. The OLS coefficient is close to one, making it higher than the 2SLS coefficient. The upward bias of the OLS coefficient might be due to the fact that municipalities that want to expand their budget use zoning as a 'revenue machine', and thus convert more land to fuel construction revenues.

A natural question is whether a marginal propensity to spend out of a temporary windfall of 0.7 is high or low. Clearly, when compared to the prediction of our baseline model, this is a high number, since a fully temporary shock should be saved in full for leaner times. The evidence from private consumption suggests that this might be the case. For example, Blundell *et al*. (2008) show that consumption is not at all sensitive to transitory shocks. Cerletti and Pijoan-Mas (2014) obtain a propensity to spend out of a transitory shock of around 0.2 in the case of durable goods, and they attribute this result to borrowing constraints. Our expenditure data include capital spending, so this last number is perhaps a reasonable benchmark for this type of spending if Spanish municipalities were liquidity constrained during this period. However, less clear is what the benchmark might be in the case of public consumption. Previous papers on the ability of governments to smooth spending over time relied on a different methodology so results are not readily comparable. Moreover, these results are very much dependent on country institutions: while U.S. governments seem perfectly capable of smoothing spending (Holtz-Eakin *et al*., 1994), the same cannot be said for their Nordic counterparts (Dahlberg and Lindström, 1998) or rather it depends on whether local governments are actually credit constrained and/or subject to debt limits (Børge and Tovmo, 2009; Persson, 2016).

5.3. Other margins

In Table 3 we explore the possibility that the response of the windfall is channelled through other margins. Specifically, local governments might have used the windfall revenues obtained during the boom to fund tax reductions. Note that it is straightforward to introduce taxes in the model developed in section 3. The tax-smoothing model (see e.g., Barro, 1979) also predicts that a (benevolent) government would not change tax rates in response to a temporary shock. We can show that introducing taxes as a decision variable in our model has no effect at all on the response of savings to the temporary windfall, the prior reaction of spending being split between spending increases and tax cuts (results available upon request). The results show that an increase in construction revenues of 100 euro is accompanied, roughly, by a 5-euro reduction in (ordinary) tax revenues. This effect is, however, imprecisely estimated. Most of it comes from a reduction in property tax revenues, but also in this case the coefficient is not statistically significant. So, the adjustment through taxes does not seem to be very relevant.

[Insert Table 3]

Table 3 also explores the possibility that the windfall has an effect on the amount of grants received from other tiers of government. It could be, for example, that some formula grants automatically react to increases in tax revenues, or that grantors feel less compelled to help municipalities that are already revenue buoyant. Or the opposite might be the case, i.e., higher construction revenues make it easier for municipalities to match the co-funding requirements of earmarked grants. It is important that we rule out this possibility because a notable impact on grants would lead to our misinterpreting our results: the propensity to spend out of the windfall would identify not just the effect of construction revenues on spending but also the effect of both small and large grants. Note, however, that the results reported in Table 4 indicate that grants are totally insensitive to the windfall. This applies both to current grants (most of them formulated) and to capital grants (which are specific). As explained in section 3 above, given the characteristics of these grants in the Spanish case, we believe these results to be coherent.

5.4. Expenditure type

In this section, we present our results for different categories of expenditures. Yet, what exactly do we expect to learn from this exercise? Notice that in the model presented in section 2 the reason why overspending in period 1 reduces welfare is simply the result of the decreasing marginal utility of public services. This generally holds true for current spending: for example, hiring more teachers to increase the quality of kindergartens in one period only to have to lay them off again in the following period (with the corresponding negative effect on quality) reduces welfare, compared to maintaining a constant number of teachers over the two periods.

In the case of capital spending, this is less of a concern: building a new school will provide benefits in the future, so spending more today does not necessarily reduce welfare. Thus, in theory, the allocation of construction revenues to the current budget would provide more compelling evidence for inefficiency than would allocation to the capital budget. Note, however, that there are various reasons why excessive volatility of capital spending may also be problematic. First, capital spending includes both new investment and maintenance so that a fall in the latter may impact the quality of services (Kahn and Levinson, 2011). Second, a reduction in capital spending might have an effect on the local economy, thus exacerbating the crisis (Alloza and Sanz, 2019). Finally, concentrating all this capital spending in a few years during the boom may affect the ability to select the best projects and their costs (Presbitero, 2018).

[Insert Table 4]

Table 4 presents the 2SLS results for a detailed breakdown of expenditure categories. We present results for capital and current expenditures, and for three different categories of current spending, namely personnel, purchases and transfers. The results in Table 4 suggest that the spending response is allocated as follows: around 70% goes to current spending (50 out of every 100 euros of windfall) and around 30% to capital spending (20 euros). Note that even if we consider the capital spending response as not being inherently inefficient (because of the durability of the benefits it provides), the effect on current spending is still quite large. Note also that the spending response is dedicated mainly to personnel (28 euros or around 40% of the spending response). Due to a lack of data, we are unable to say whether this spending is made up of raises in salaries, new permanent hires or just temporary jobs. However, since both cutting salaries and the laying off of workers are politically costly, assuming this type of commitment out of a temporary windfall may entail certain financial risks. Since it is difficult to adjust this type of expenditure down, the effect of the loss of construction revenues will probably be detected in other budget items. We come back to this question later when we examine the effects during the bust (see section 5.7).

5.5. Robustness

The results are robust to several methodological variations. First, we repeated the analysis but changed the years used to define the periods of boom and bust (using either two years or even one year) but the results remain basically the same. Second, we examined whether vacant land is equally capable of predicting the different types of construction revenues. Here, we found that the coefficient of the regression between the increase in revenues and the predictive capacity is basically the same for construction tax revenues as it is for fees and for land sales. The results are also insensitive to the exclusion from the analysis of those cities that derive their construction revenues from just one of these sources.

We also ran a couple of additional analyses to verify the reliability of our instrumental variables strategy. First, we examined if the results are dependent on whether the Master Plan was amended in the early, mid- or late 1980s. The Master Plans amended earlier in the decade (that is, before the housing boom of the late 1980s) may have been less contaminated by housing demand shocks and, therefore, the vacant land in these municipalities may have more plausibly been random. However, we find that both the strength of the first-stage and the marginal propensity to spend are more or less the same in the two samples (see Table A.6 in the Online Appendix).

Second, we performed an analysis to see how sensitive our 2SLS results are to the omission of variables. Note that, while we had already ruled out many possible channels that could invalidate the exclusion restriction, there may be other channels that we are simply unable to measure. Our test is based on the idea of Altonji *et al*. (2011) that if the estimated coefficient is robust to the inclusion of observable controls, then this is also an indication that the coefficient would not be greatly affected by the failure to control for unobservables. We use a refined version of this test, in line with Oster (2018), which takes into account the explanatory capacity of the controls included. Table A.7 in the Online Appendix shows that the results are quite robust to the omission of variables.

Finally, we examine our results when using the entire urban sample (see Table A.8 in the Online Appendix). The estimates are quantitatively similar than those already presented (the propensity to spend is around 0.7) but somewhat less robust to the introduction of controls. Moreover, in this sample, the instrument seems to present some correlation both with budget spending pre-trends (see Table A.9 in the Online Appendix) and with pre-determined covariates in levels (columns 3 and 4 in Table A.4). It seems that the amount of heterogeneity in this sample is larger than that in the sample used to carry out our main analysis. Also, the instrument is less strong, especially when adding covariates. However, using weak instrument inference, we still find that the coefficient is statistically significant at the 10% level.

5.6. Mechanisms

In this section, we investigate whether the inability to smooth spending and taxes over time is a generalized problem of all Spanish municipalities or, rather, their behaviour in this regard differs. We focus primarily on two mechanisms: political myopia and extrapolation bias, but we also consider the possibility that other stories might be relevant. To account for each mechanism, we select a variable labelled w_i, which we introduce in the regression interacted with windfall revenues. The equation takes the following form:

$$\Delta e_i^{boom} = \left[\alpha_j + \delta * w_i + \sigma * z_i\right] * \Delta c_i^{boom} + \theta * w_i + \vartheta * z_i + \psi_j + \psi_k + \xi_i \qquad (7)$$

The w_i variables are demeaned, so that the α parameters inform us of the effect of a variable at the mean while the δ parameter measures the effect at different levels above or below the mean. We use three different approaches to identify δ. First, we control for interactions between the urban area fixed effects and windfall revenues. This means that the interaction effect is also identified with within-area variation. Second, we aim to select interacting variables that are plausibly exogenous. Below we describe the variables chosen for each mechanism. Third, we control for interactions between a set of potential confounders z_i and windfall revenues.

Political myopia. Following on from the discussion in section 2, we consider whether the propensity to spend out of a temporary windfall rises as the level of competition at the local elections increases. We measure political competition using the incumbent's vote margin (% *Vote margin*), a measure used in other papers (e.g., Besley *et al*., 2010). We compute this variable as the difference between the vote share obtained by the parties making up the left- and right-wing political blocs at the two local elections held during the boom (i.e., the 1999 and 2003 polls). The classification of parties by ideology follows a scale used in the authors' previous work (Solé-Ollé and Viladecans-Marsal, 2013). This variable might be correlated with some other municipal traits, such as income or education, which casts certain doubts on its exogeneity. For this reason, we also present some 2SLS results, using as our instrument the *Predicted % Vote margin*, computed with the hypothetical vote shares of the parties at the local elections of 1999 and 2003 resulting from the projection of the party votes at the 1979 elections when using the national growth rate in the votes for each party since that year. This method has been previously used by the authors (see Solé-Ollé and Viladecans-Marsal, 2012).

Extrapolation bias. We examine whether the propensity to spend is greater in municipalities with prior experience of housing construction volatility. Our intuition is that places where the housing boom-busts of the 1980s were more volatile can expect to experience a higher level of volatility in the 1990s and, hence, of construction revenues, which, as explained above, closely follow the evolution taken by housing construction.

To test this mechanism, we measure volatility as follows. We rely on a long series of housing construction data (defined as housing units built/housing stock) at the municipality level for the period 1981-2011, which covers both the boom-bust cycle we are studying here (1996-2011) and the earlier cycles. For each municipality, we isolate both business cycle and trend components of the series using the Hodrick-Prescott filter, with a filter parameter equal to 400, the value suggested by Ravn and Uhlig (2002) for yearly data. The results are robust to using other values or even to simply using a linear trend or the average over the whole period. Figure A.6 in the Appendix shows the cyclical components for the whole sample, and for selected municipalities. The graph shows that the volatility of the boom-bust cycle of 1996-2011 is quite high. The two boom-bust cycles of the 1980s seem to be shorter and less pronounced than the more recent one, but they are still quite remarkable. The graph also illustrates the geographical heterogeneity in the intensity of the cycles and in the differences between the recent and past boom-bust cycles.

We can now compute our *Volatility* measure as the standard deviation of the cyclical component (relative to trend). We compute this measure for two sub-periods: 1981-1995 and 1996-2011. The correlation coefficient between the two measures is 0.423 and a one-unit increase in past volatility translates to an increase of 0.402 in the volatility during the recent boom-bust (see Figure A.7 in the Online Appendix). This suggests that the long-run persistence of housing construction is to some extent predictable. The variable we interact with the revenue shock is the predictable component of volatility, that is, the prediction of the regression between the volatility in the second and first periods (*Predictable Volatility*). We also present our results when interacting the windfall variable with the unpredicted part or the residual of this regression (*Unpredictable Volatility*).

A caveat should be made at this point. Note that we measure volatility by using data on housing construction rather than data on housing construction revenues. This is due to problems of data availability. We do not have a detailed breakdown of revenue categories for the period prior to 1995 to measure housing construction revenues for the first period. However, the correlation between past volatility in housing construction and the standard deviation of housing construction revenues in the recent boom is also high (i.e., equal to 0.37 and statistically significant). This makes sense because, as we have already demonstrated, construction revenues are highly correlated with housing construction.

Other stories. To account for the possibility of liquidity constraints we include an interaction with the *Debt burden* (= interest + repayment), measured per capita. Using the debt burden is almost the equivalent of using the level of debt, data that are not available as they were not published until 2008. To account for the possibility that a city might be spending its windfall in order to develop the local economy and catch up with richer cities, we include an interaction between the boom windfalls and the level of income per capita pre-boom. Controlling for the level of income is important because some of the behaviours that are consistent with the other mechanisms might actually be confounded with the level of development. For example, it might be the case that development is correlated with political competition or with the intensity of the housing bubble. Richer places might also be more able to collect taxes and, hence, have lower levels of debt.

We also control for interactions between windfall revenues and other variables that might be correlated either with the %*Vote margin* or with the *Volatility* index. These include population size (*Population*), share of people with a college education (% *College*), share of votes won by parties on the left (%*Left vote*) and a dummy indicating whether the government does not have a majority of seats in the council (*Coalition*). In the same way as for the vote margin, they have been computed as an average of the values of the 1999 and 2003 local elections.

Results. In Table 5 we examine the different mechanisms[21]. In columns (1) to (4) we present the results obtained when including the interactions with %*Vote margin*. The interaction with the %*Vote margin* is found to be statistically significant. The sign of the interaction is negative, indicating that local incumbents that face more competition at the polls spend a larger portion of the boom windfall (see column (1)). To gauge the quantitative relevance of these findings it is more informative to consider the marginal effects, which are displayed in panel (a) of Figure 3. According to the graph, the effect of electoral competition is huge. If we consider one standard deviation in the vote margin below/above the average (indicated by the long-dashed vertical lines), the marginal propensity to spend moves from 1.264 (=0.672+0.037x16) to 0.080 (=0.672-0.037x16). These two values are not statistically different from one and from zero, respectively. Incumbents faced with highly competitive elections spend the whole windfall. Incumbents that feel quite unthreatened save the entire windfall. Moreover, this conclusion is quite robust. In column (2) we re-estimate the equation allowing for an interaction between the urban area fixed effects and the windfall variable (i.e., exploiting only within-area differences in electoral competition). In column (3) we instrument the % *Vote margin* with the *Predicted % Vote margin* (i.e., computed using 1979 vote shares at the city level and evolution of party vote shares at the national level). In all these cases, the coefficient on the interaction is statistically significant and the magnitude is stable. In column (4) we control for interactions between the revenue shock and other variables. The coefficients of these variables are omitted for reasons of space but are included in the Online Appendix (see Table A.12). The results do not change. None of the other interactions is statistically significant and some (*Debt burden* and *Income*) are extremely small.

[Insert Table 5 and Figure 3]

Columns (5) to (10) present the results of the interaction with volatility. The interaction with *Predictable Volatility* is statistically significant and negative (see column (5)), suggesting that the marginal propensity to spend is lower when incumbents have previous experience of housing market volatility. This result is robust to changes in the specification. In column (6) we control for an interaction with %*Vote margin*, in column (7) we include interactions with urban area fixed effects, and in column (8) with additional covariates (the same as in column (4)). The results remain more or less unchanged. Finally, in column (9) we use the real amount of volatility experienced during the boom and obtain a coefficient that is positive but not statistically significant. In column (10) we include interactions with both *Predictable Volatility* and *Unpredictable Volatility*. The results for the predicted component remain unchanged, but the coefficient for the unpredicted component is not statistically significant. Finally, the marginal effect (see panel (b) in Figure 3) suggests that this mechanism is also quantitatively relevant. If we measure the effect at one standard deviation below/above the average (indicated by the

[21] The table only reports the second-stage results. The first-stage results for the main regressions are reported in Table A.11 in the Online Appendix.

long-dashed vertical lines), the marginal propensity to spend moves from 1.278 (=0.640 + 0.58 x 11) to -0.056 (=0.640 - 0.58 x 11). Again, these two values are not statistically different from one and zero, respectively.

It seems, therefore, that both mechanisms, *Political Myopia* and *Extrapolation bias*, are able to account for the degree to which municipalities overspend their construction revenue windfalls. Some municipalities overspend because they are unaware of the real long-term persistence of this revenue shock, given that they have no prior experience of such a huge swing. Other incumbents, while aware of the volatility of the shock, may nevertheless opt to spend the windfall because they know it will help them win a competitive election. The question might be posed as to which type of expenditures are more sensitive to each of these two mechanisms. The answer to this question is provided in Figure A.8 in the Online Appendix, which shows the marginal effects for Current and Capital spending separately. Thus, while both mechanisms seem to operate for both types of spending, in the case of *Volatility*, the interaction effects seem to be virtually of the same strength for the two spending categories. In the case of %*Vote margin*, the effect is present for both types of spending, but it is much stronger for Capital spending. A possible interpretation of these results is that, in the first case, the local incumbent is not very well-informed and so she tends to overspend in a similar proportion in all types of spending. In the second case, the local incumbent is informed but decides to overspend anyway because she needs to win the election. She is aware, however, that if she wins, it will be difficult to cut Current spending, so she opts to use Capital spending to try to win extra votes.

5.7. The bust

In this section, we present the results of the estimation of the effect of the construction revenue windfall obtained during the boom on the evolution of expenditures during the bust. We estimate the following equation:

$$\Delta e_i^{bust} = \alpha * \Delta c_i^{boom} + \beta * x_i^0 + \gamma * \Delta y_i^{boom} + \varphi * \Delta y_i^{bust} + \lambda_j + \lambda_k + u_i \qquad (8)$$

where Δe_i^{bust} is the change in expenditures per capita from the peak of the boom (i.e., average of the term 2003-07) to the crisis (i.e., average of the term 2008-11). Note that, as before, the treatment is defined as the change in construction revenues per capita during the boom. This is justified by the fact that these revenues are totally mean reverting (i.e., $\Delta c_i^{bust} \cong -\Delta c_i^{boom}$) as is shown in Figure 4 below. In the equation, we still control for the two sets of fixed effects and for post-treatment covariates, the only novelty being that we now control for changes in confounders that occur both during the boom and during the bust. In practice, however, and as occurred with the boom results, once we include the fixed effects the results are not affected by the inclusion of the different sets of controls (either pre- or post-treatment).

[Insert Figure 4]

Results. The full set of results including the different controls is presented in Table A.13 in the Online Appendix. Here, we comment solely on the results obtained when the fixed effects are included. Table 6 summarizes the results for the spending, taxation and grants margins, and for the different spending components.

[Insert Table 6]

Several results are worth highlighting. First, for every 100 euros of construction windfalls obtained during the boom, spending is cut by 78 euros during the subsequent bust. Second, nearly 80% of this cut corresponds to capital spending (59 euros) and just 30% corresponds to current spending (21 euros). Recall that the increase in total spending during the boom was of a similar dimension but was allocated primarily to current rather than to capital spending. Note also that personnel spending is not adjusted at all during the bust, while it absorbed a considerable proportion of the windfall during the boom. This can be explained by the high political cost of cutting this type of spending during the bust[22]. Third, the rest of the adjustment during the bust corresponds to a tax increase of around 10 euros. Recall also that the windfall had a smaller (and not statistically significant) effect on tax cuts during the boom. The remaining corresponds to an increase in the deficit (around 20 euros). Finally, note that intergovernmental grants play no role in the adjustment during the bust. There is, therefore, no evidence that governments that spent their windfall during the boom were compensated either by an automatic increase in formula transfers or by way of a bailout grant when these revenues vanished during the bust.

We also examine the effect of electoral competition and volatility on the size of the adjustments made during the bust. The marginal effects are shown in Figure 5 (see also Table A.14 in the Online Appendix). The marginal effects during the bust mirror those recorded during the boom: the municipalities with past experience of volatility overspent less during the boom and thus had to cut their spending less during the bust. The same occurs with the vote margin variable: local incumbents facing highly competitive elections during the boom reacted by spending most of the windfall and later had to implement large spending cuts. In the Appendix (see Figure A.9), we show that these effects are there both for current and capital spending. Thus, the municipalities that overspent during the boom (due either to their failure to assess the persistence of the shock or to electoral incentives) are those that had to cut spending most during the crisis.

[Insert Figures 5 and 6]

In Figure 6, we also examine the effect of the electoral competition faced during the bust. To do so, we interact the windfall variable with the vote margin for the 2007 local elec-

[22] Alternatively, this result might be due to the complementarity of capital and personnel spending: the facilities build during the boom need personnel to operate. If this is the case, it is less clear that the failure to cut personnel expenditures during the bust can be qualified as inefficient.

tions (%*Vote margin*(Bust)). We find that this variable has a strong influence on the size of the spending cuts implemented during the crisis (even after controlling for the degree of electoral competition faced during the boom and for volatility, see Table A.14). Here again this holds both for current and for capital spending (see Figure A.9 in the Online Appendix). This suggests that policy myopia might also have an influence during the bust: local incumbents facing stiffer electoral competition might be reluctant to implement fiscal adjustments. This is consistent with the results of previous papers on the political economy of fiscal adjustment and reforms (see, e.g., Bonfiglioni and Gancia, 2013).

6. Conclusions

In this paper, we have studied the effect of a large, temporary revenue windfall on local expenditures, using data from Spanish municipalities during the housing boom-bust of 1995-2011. As we have shown, Spanish local governments are quite reliant on taxes and other revenue sources associated with the construction sector. Revenues from these sources are, moreover, highly unstable, rising steeply during booms but virtually disappearing during busts. We have documented that, in addition to their temporary nature, these windfalls had a massive impact on local expenditures both during the boom and du-ring the bust. Further, our results suggest that, on average, local governments saved only a very small proportion of these windfalls for leaner times, preferring to fund spending increases, with a marked impact on current spending and, especially, personnel spending. During the bust, when the windfall was converted into a shortfall, capital spending was cut abruptly while current spending proved resistant to cuts. Likewise, during the bust, taxes were raised, and deficits appeared.

When exploring the mechanisms behind these results, we also document how local government behaviour during the boom differed greatly depending on the level of electoral competition and on the municipality's previous experience of housing construction volatility. In municipalities with less competitive elections, the boom windfalls were largely saved, while in municipalities with highly contested elections, these extraordinary revenues were mostly spent. Similarly, municipalities with volatile housing markets in the past tended to save a larger proportion of the windfall. We can, thus, conclude that temporary windfalls were spent during the boom because of the municipalities' inability to foresee the future and because of electoral incentives. We have also shown that the municipalities that had to implement the largest adjustments during the bust were those that overspent most during the boom and also those facing the most competitive elections during the crisis.

Given the findings, the question arises as to whether policy actions need to be implemented to address this issue. Note that Spanish local governments have actually been able to adjust to the impact of the construction-revenue crisis in a relatively short period of time. However, despite these adjustments, the excessive cyclical volatility of revenues and spending can constitute a real threat to stability. For instance, abrupt cuts to infrastructure spending during a bust can exacerbate the crisis, while excessive spending on personnel may also constitute a burden for the future. What solutions, therefore, are available? First, given that the problem derives from the excessive volatility of revenues, actions need to be taken to ensure that revenues related to land use and the construction sector do not become a source of windfall gains. One suggestion that has already been made in this regard is the setting up of a mandatory rainy-day or stabilization fund (Lago and Solé-Ollé, 2016)[23]. Second, it is of paramount importance that greater transparency in the management of these revenues, and of finances in general, is achieved.

Our results should be of interest to other countries whose governments also rely on volatile sources of revenue and, in particular, to developing countries that make extensive use of construction revenues. For example, there is evidence that land conversion revenues generated during the recent housing boom in China have had an adverse effect on fiscal management and governance at the local level (Kung and Chen, 2016). The World Bank and other institutions have expressed concerns about the spread of this problem to other countries, including, for example, to Brazil (World Bank, 2014). Policies to address this situation might include both the diversification of the revenue portfolio (i.e., revamping the more stable property tax) and increases in the transparency of the management of construction revenues.

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* We are grateful for comments received from Jan Brueckner, Christian Hilber, Byron Lutz and Thomas Stratmann and from participants at seminars held at SERC-LSE, U. of California-Irvine, KU-Leuven and Banco de España, and at the ZEW Public Finance Conference (Mannheim, Germany), the IIPF Conference (Lake Tahoe), the1st Brüneck Workshop on the Political Economy of Federalism (Brüneck), the Meeting of the Urban Economic Association (Vancouver) and the AREUA International Conference (Guangzhou). We also acknowledge the excellent research assistance provided by Ilias Pasidis and Pierre Magontier. We also acknowledge the support of projects ECO2015-68311-R and ECO2016-75941-R (Ministerio de Economía y Competitividad) and 2017SGR796 (Generalitat de Catalunya).

[23] Although our results might be informative about the optimal size of the fund needed to stabilize expenditures, it is hard to be precise about that. The size of fund might depend on: (a) the share of the windfall that end up being allocated to current spending during the boom (our results suggest that this is a 50% of the windfall on average), (b) the relative frequency of booms v. busts (here one could consider for example that booms are very infrequent or that they are equally frequent than busts), and (c) whether the fund is designed for the average municipality or for the more profligate ones (in this last case, the degree of overspending might reach a 100% of the windfall). This suggests a size of the fund going from a 25% of the windfall (for the average municipality and a high frequency boom) to approximately a 75-100% (for a profligate municipality and an infrequent boom).

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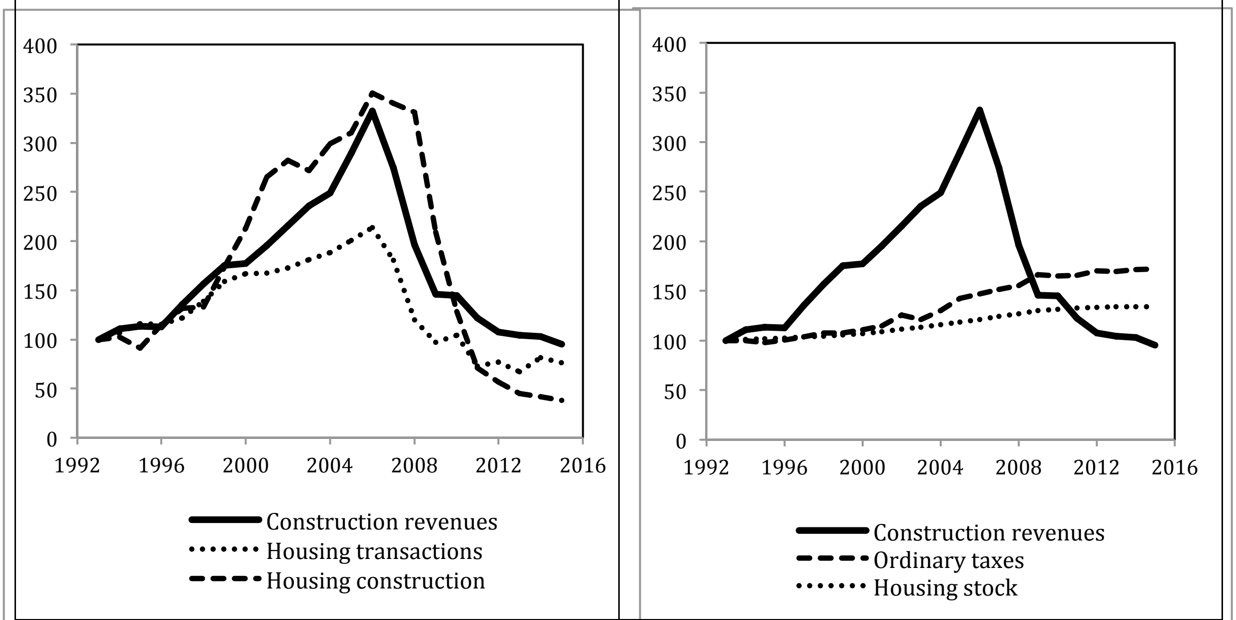
Figures

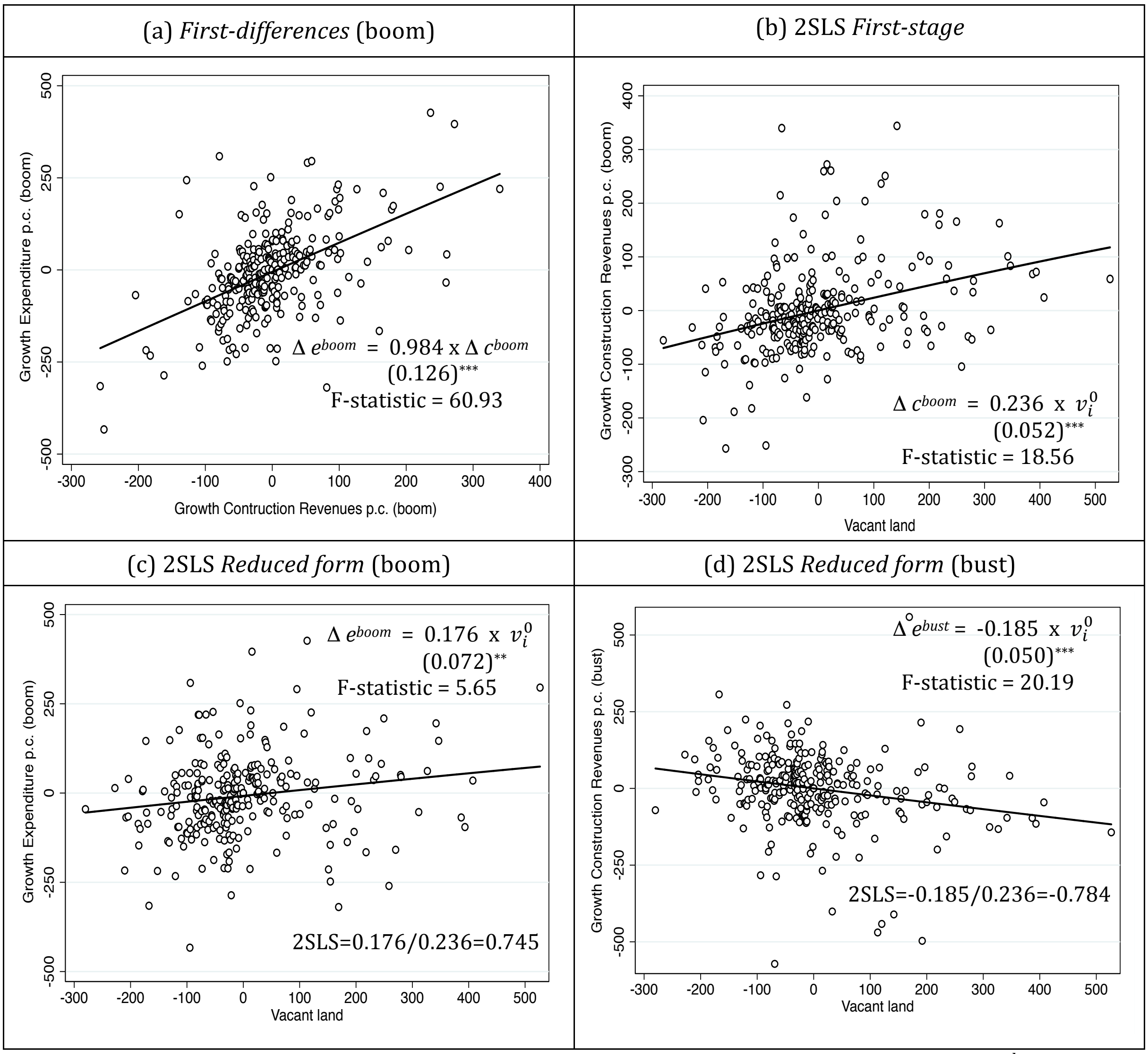
Figure 1: *Construction revenues during the Housing boom & bust*

(a) *Construction revenues* | (b) *Ordinary tax revenues*



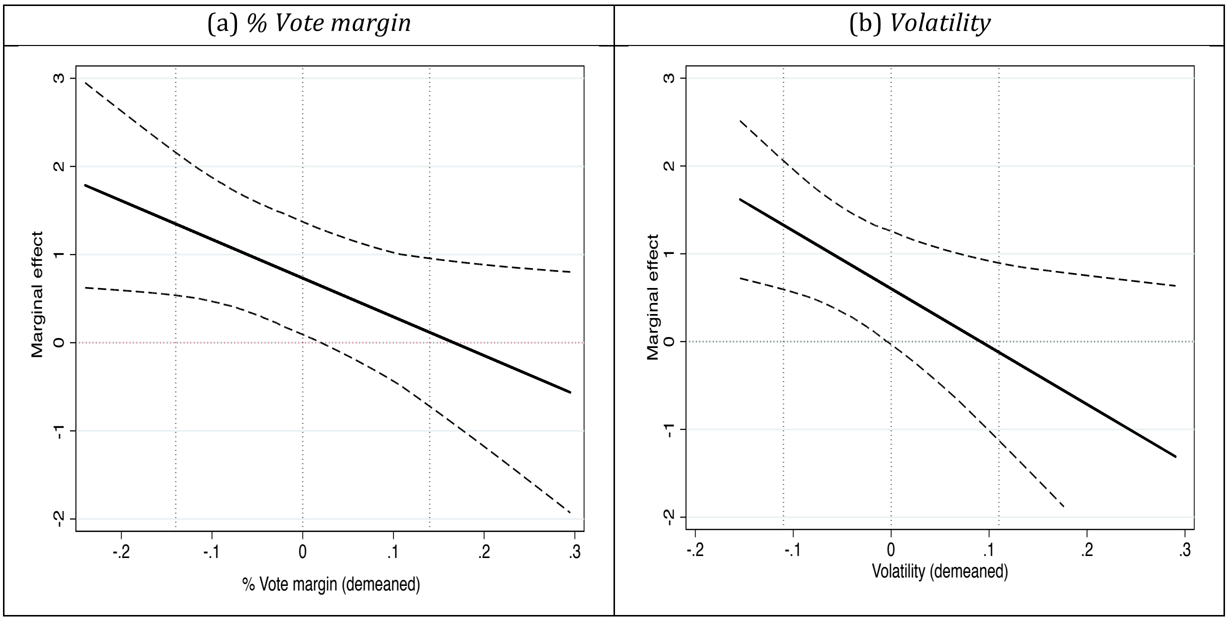
Notes: (1) Ordinary taxes include revenues from taxes and fees not included in Construction revenues and are computed with constant tax rates; construction revenues and Ordinary tax revenues in real terms; outlay data until 2011, budget forecasts for the remaining years. (2) All variables expressed as an index (1993=100). (3) Sources: Housing construction and Transactions form Ministerio de Fomento (http://www.fomento. gob.es). Construction revenues: Ministerio de Hacienda y Administraciones Publicas (http:// www.minhap.gob.es) and own elaboration.

Figure 2: *Main results*



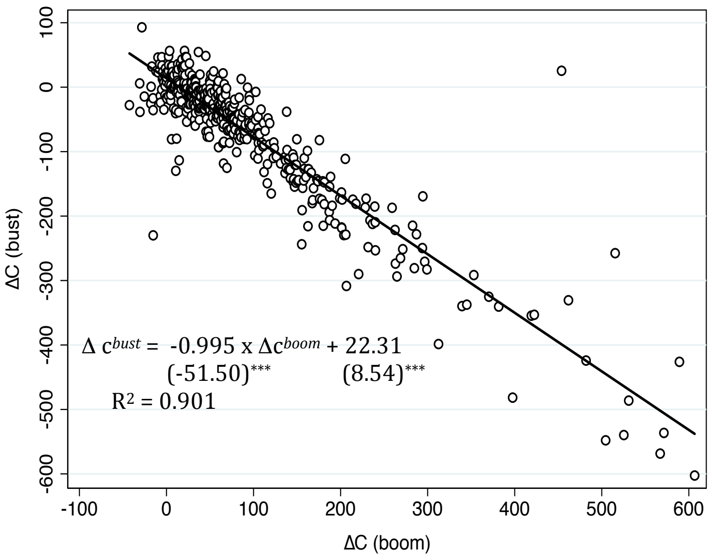
Notes: (1) Panel (a): Relationship between the increase in expenditure per capita during the boom (Δe_i^{boom}) and the increase in construction revenues per capita (Δc_i^{boom}); Panel (b) First-stage equation: relationship between the increase in construction revenues during the boom and vacant land per capita at the start of the boom (v_i^0); Panel (c): reduced form equation for the boom: relationship between increase in expenditure per capita during the boom and vacant land per capita; Panel (d): the same for the bust period. (2) All variables have bee residualized, that is the variable plotted is the residual of a regression between the original variable and the two sets of fixed effects already mentioned. (3) Inside each box we display the equation estimated; standard errors in parenthesis, ***, ** and *: statistically significant at the 1, 5 and 10% levels; at the bottom of Panels (c) and (d) we also display the computation of the 2SLS coefficient, which is the ratio between the reduced form and first-stage coefficients.

Figure 3: *Marginal effects*



Notes: (1) Effect of an increase in construction revenues during the boom on Spending during the boom at different levels of the interaction variable, either the %Vote margin or the Predictable Volatility (bold line) and 95% confidence interval (dashed lines). (2) The interaction variables have been demeaned, so that the 0 indicates the marginal effect evaluated at the sample mean; the other two vertical lines indicate -1,+1 s.d. of the interaction variable.

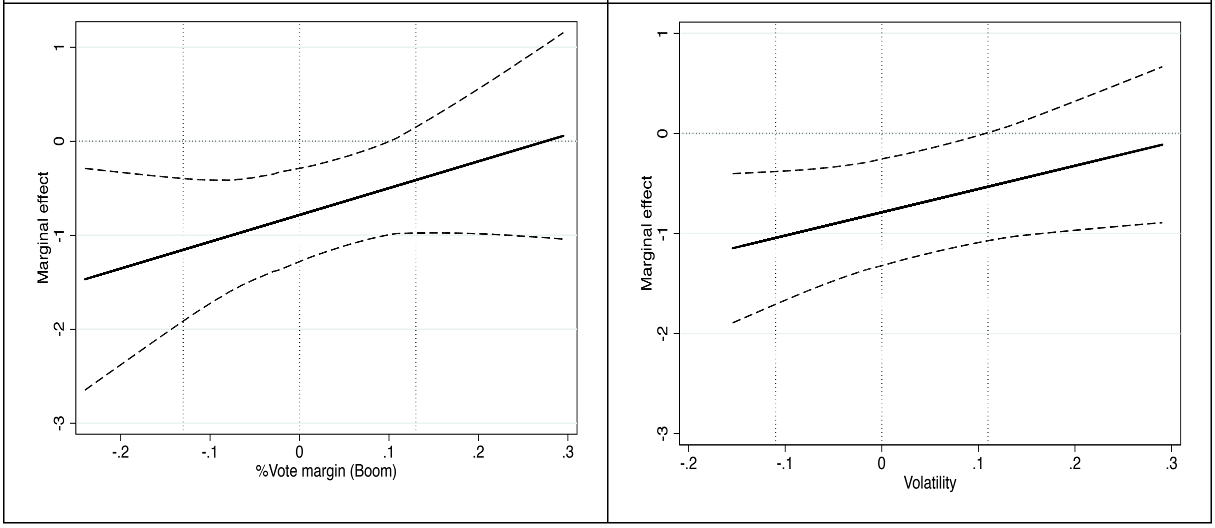
Figure 4: *Mean reversion in Construction Revenues*



Notes: (1) y-axis: Growth in construction revenues per capita during the boom; x-axis= Growth in construction revenues per capita during the bust. (2) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the highway network (N=314). (3) t-values in parentheses, ***: significant at the 1% level; standard errors clustered by urban area.

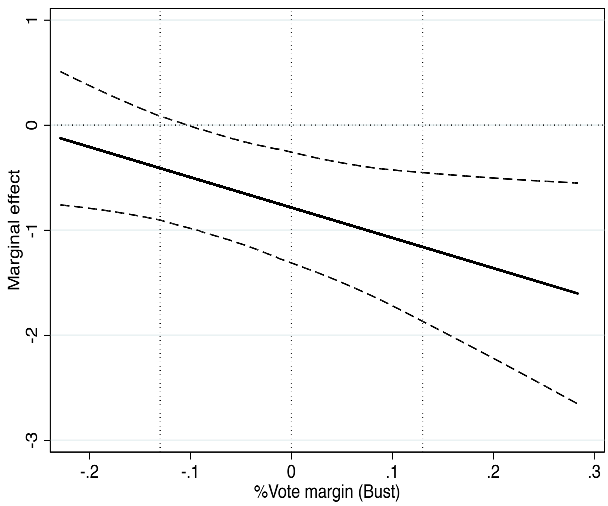
Figure 5: *Marginal effects during the crisis*

(a) *% Vote margin* (Boom)	(b) *Volatility*



Notes: (1) Effect of an increase in construction revenues during the boom on Spending during the crisis at different levels of the interaction variable, either the %Vote margin (during the boom) or the Predictable Volatility (bold line) and 95% confidence interval (dashed lines). (2) See Figure 4.

Figure 6:
Marginal effect of % Vote margin in the crisis



Notes: (1) Effect of an increase in construction revenues during the boom on Spending during the crisis at different levels of the %Vote margin obtained in the elections held just before the crisis (bold line) and 95% confidence interval (dashed lines). (2) See Figure 4.

Table 1: *Vacant land and budget pre-trends*

	(1)	(2)	(3)	(4)	(5)	(6)

	(a) *Previous boom* (1986-87 to 1990-91)			(b) *Previous bust* (1990-91 to 1993-95)		
	Expenditures	*Tax revenues*	*Grants*	*Expenditures*	*Tax revenues*	*Grants*
Vacant land p.c. (v^0)	0.039 (0.084)	-0.017 (0.016)	-0.083 (0.163)	0.014 (0.017)	-0.001 (0.001)	-0.016 (0.176)
R^2 (adj.)	0.245	0.180	0.271	0.219	0.329	0.265
	(c) *Previous boom-bust cycle* (1986-87 to 1993-95)			(d) *Current boom period* (1993-95 to 2004-07)		
	Expenditures	*Tax revenues*	*Grants*	*Expenditures*	*Tax revenues*	*Grants*
Vacant land p.c. (v^0)	0.041 (0.069)	-0.013 (0.042)	-0.042 (0.112)	0.176*** (0.063)	-0.022 (0.023)	0.005 (0.011)
R^2 (adj.)	0.212	0.175	0.155	0.362	0.199	0.232

Notes: (1) Effects on changes in different budget variables during the previous boom and bust periods and during the current boom; (2) Outlay data used for the current boom period (Panel (d)) and Initial budget data used for the other periods (Panels (a) to (c)). (3) All equations include urban area and distance-to-central-city interval fixed effects; (4) Sample: municipalities larger than 1,000 inhabitants and with access to the main road network in larger Spanish urban areas (N=314); (5) Standard errors clustered by urban area in parenthesis; ***. *** & *= statistically significant at the 1%, 5% and 10% levels.

Table 2: *Effect on Expenditures in the boom* (Δe^{boom})

	(1)	(2)	(3)	(4)
	(a) 2SLS			
Construction revenues p.c. (Δc^{boom})	0.922*** (0.215)	0.745*** (0.214)	0.711*** (0.306)	0.723** (0.327)
	(b) *First-stage*			
Vacant land p.c. (v^0)	0.271*** (0.062)	0.236*** (0.055)	0.235*** (0.056)	0.230*** (0.057)
R^2 (adj.)	0.147	0.152	0.433	0.488
First stage F-statistic	19.17	18.56	18.80	17.21
	[16.38 / 8.96 / 6.66]			
	(c) OLS			
Construction revenues p.c. (Δc^{boom})	1.061*** (0.093)	0.976*** (0.094)	0.944*** (0.115)	0.992*** (0.114)
R^2 (adj.)	0.433	0.467	0.620	0.622
Urban area & Distance to CBD f.e..	NO	YES	YES	YES
Pre-determined controls	NO	NO	YES	YES
Alternative channels	NO	NO	NO	YES

Notes: (1) Dependent variable is increase in total spending per capita during the boom period; (2) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the highway network (N=314); (3) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels; (4) *First stage F-statistic*: Kleiberger-Paap Wald rk F-statistic; in brackets Stock-Yogo weak identification test critical values at 10%/15% and 20% maximal IV bias.

Table 3: *Effect on Taxes and Grants in the boom.* 2SLS *results.*

	Tax Revenues			*Grants*	
	Total	*Property*	*Other*	*Current*	*Capital*
Construction revenues p.c (Δc^{boom})	-0.058 (0.066)	-0.042 (0.033)	-0.003 (0.022)	0.012 (0.024)	0.020 (0.067)

Notes: (1) Dependent variables measured as increases in per capita amounts during the boom period; *Tax revenues* include only revenues from *Ordinary taxes* (Property tax, Business tax, and Vehicle tax); (2) All equations include urban area and distance-to-central-city interval fixed effects. (3) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the main road network (N=314); (4) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels; (5) See Table A.1 for definitions of the different budget items analysed.

Table 4: *Effect on Expenditure components in the boom.* 2SLS *results*.

	Expenditures			
	Capital	*Current*		
		Total	*Personnel*	*Other*
Construction revenues p.c (Δc^{boom})	0.298^{**} (0.150)	0.428^{***} (0.148)	0.206^{**} (0.093)	0.213^{*} (0.114)

Notes: (1) Dependent variables measured as increases in per capita amounts during the boom period; All equations include urban area and distance-to-central-city interval fixed effects. (2) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the main road network (N=314); (3) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels; (4) See Table A.1 for definitions of the different budget items analysed.

Table 6: *Effect on Expenditures and other budget items in the Bust.* 2SLS *results*.

	Expenditures				
	Total	*Capital*	*Current*		
			Total	*Personnel*	*Other*
Construction rev. p.c (Δc^{boom})	-0.784^{***} (0.189)	-0.587^{***} (0.139)	-0.207^{***} (0.099)	0.007 (0.040)	-0.209^{***} (0.066)
	Tax revenues			*Grants*	
	Total	*Property*	*Other*	*Current*	*Capital*
Construction rev. p.c (Δc^{boom})	0.149^{*} (0.075)	0.141^{*} (0.068)	0.007 (0.023)	0.012 (0.054)	-0.024 (0.056)

Notes: (1) The table reports 2SLS estimates of the effects of the increase in construction revenues p.c. during the boom (1995-2007) on several budget items during the bust (2008-2011); dependent variables measured as increases in per capita amounts during the bust period. (2) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the main road network (N=314); (3) The instrument used is the amount of vacant land p.c. in 1995; in the estimation, we include urban area and distance-to-central-city interval fixed effects. (4) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels.

Table 5: *Mechanisms*. 2SLS *results.*

	(a) Policy myopia				(b) Extrapolation bias					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Construction revenues p.c (Δc^{boom})	0.672** (0.287)	--.--	0.710** (0.348)	0.697** (0.303)	0.685** (0.311)	0.649** (0.279)	--.--	0.704** (0.294)	0.628** (0.255)	0.643** (0.283)
× *% Vote margin*	-0.037** (0.019)	-0.047** (0.019)	-0.041** (-0.018)	-0.032* (0.017)	--.--	-0.035* (0.021)	-0.045** (0.021)	-0.039** (0.022)	-0.035** (0.013)	-0.037** (0.018)
× *(Predictable) Volatility*	--.--	--.--	--.--	--.--	-0.033* (0.017)	-0.048* (0.028)	-0.041* (0.022)	-0.056** (0.027)	--.--	-0.058* (0.026)
× *(Un-predictable) Volatility*	--.--	--.--	--.--	--.--	--.--	--.--	--.--	--.--	--.--	0.102 (0.069)
× *Volatility*	--.--	--.--	--.--	--.--	--.--	--.--	--.--	--.--	0.068 (0.178)	--.--
First stage F-statistic	8.47	--.--	--.--	--.--	7.33	--.--	--.--	--.--	--.--	--.--
Interaction fixed effects?	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO
Vote margin instrumented?	NO	NO	YES	NO	NO	NO	NO	NO	NO	NO
Additional interactions?	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO

Notes: (1) Dependent variable is increase in total spending per capita during the boom period. (2) All equations include urban area and distance-to-central-city interval fixed effects. See Tables 2 & 3. (3) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the main road network (N=314); (4) In column 2, we control for interactions between windfalls and urban area fixed effects; in column 3 we instrument the %Vote margin with the Predicted % Vote Margin; in columns (4) and (7) we controls for interactions between windfalls and other variables: *Debt burden p.c., Income p.c., % Vote left, Coalition* dummy, *Population* and % *College educated*. (5) See Table A.1 for definitions and sources. (6) First stage F-statistic: Kleiberger-Paap Wald rk F-statistic; Stock-Yogo weak identification test critical values at 10%/15% and 20% maximal IV bias: 7.03, 4.58 and 3.95. (7) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels.

Housing booms and local spending

Online Appendix

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Figure A.5: *Quasi-randomness of the Vacant land instrument*
Figure A.6: *Cyclical component of Housing construction*
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i. A model of Political myopia

Here, we present a simple political agency model that captures the intuition that overspending might be due to the incentives that elections provide to pander to the preferences of voters. The model is a dynamic career concerns model similar to Holmström (1999) and Bonfiglioni and Gancia (2013).

We depart from our baseline setting (section 2.1) and modify the model to account for the effect of elections. To begin with, we modify the expression of voter welfare:

$$W = \mathbb{E}[\ln(y_1) + \ln(y_2)]$$

(A.1)

Where $\mathbb{E}$ is the expectations operator, $y_1 = \eta_1 e_1$ and $y_2 = \eta_2 e_2$ are the quality of local public services in periods 1 and 2, e_1 and e_2 are local spending in periods 1 and 2, and η_1 and η_2 denote the quality of the politicians.

Let's assume that it is the function of elections to select the politician with the greatest quality. Voters do not observe quality but know η is distributed $U[1 - 1/2\phi, 1 + 1/2\phi]$, with $\mathbb{E}(\eta) = 1$ and density ϕ. They also know that quality persists over time and that incum-bents losing the election are substituted by opponents of average quality. The objective function of the incumbent is:

$$U = W + R + pR$$

(A.2)

where W is (expected) voter's utility, R are the exogenous office rents in each period, and p is the (expected) probability of re-election. Before the elections, voters observe c and y_1 but are unable to observe s and e_1[24] Thus, they are uncertain as to whether the high quality of public services is the result of profligacy or of the incumbent's quality.

Voters infer the incumbent's quality as:

$$\tilde{\eta}_1 = \frac{y_1}{r_1 - \tilde{s}_1} \qquad (A.3)$$

Where the $\sim$ over a variable represents a belief. The voter will re-elect the incumbent if she expects her quality to be greater than that of an opponent of average quality, so if $\tilde{\eta}_1 \geq 1$, that is if:

$$\eta \geq \frac{r_1 - s}{r_1 - \tilde{s}} \qquad (A.4)$$

[24] We can think of an uninformed voter that knows the local housing market is booming and that as a result of on-going projects the government will obtain a revenue windfall. However, she is unable to ascertain before the elections how much of the windfall the local government has spent. This assumption is justified on the grounds that the incumbent may employ accounting tricks to conceal the real amount of her spending commitments.

So, the probability of a voter voting for the incumbent is:

$$p = \frac{1}{2} + \phi \left[1 - \frac{r_1 - s}{r_1 - \tilde{s}}\right]$$

$$= \frac{1}{2} + \phi \left[1 - \frac{\tilde{e}_1}{e_1}\right] \qquad (A.5)$$

And the effect of e_1 (and s) on the probability of re-election is:

$$\frac{\partial p}{\partial e_1} = -\frac{\partial p}{\partial s} = \frac{\phi}{e_1} \qquad (A.6)$$

where we used the fact that, in equilibrium with rational expectations, voters are able to perfectly infer e_1 (and s): $\tilde{e}_1 = e_1$ and $\tilde{s} = s$ (see Holmström, 1999).

Maximizing U with respect to e_1, the F.O.C. is:

$$\frac{\partial W}{\partial e_1} + \frac{\partial p}{\partial e_1} R = \frac{1}{e_1^*} - \frac{1}{r - e_1^*} + \frac{\phi R}{e_1^*}$$

$$= 0 \qquad (A.7)$$

So, we can obtain the expression for e_1^*:

$$e_1^*$$

$$= \frac{1 + \phi R}{2 + \phi R} r \qquad (A.8)$$

The responses of spending and savings to a construction-revenue windfall are:

$$\frac{\partial e_1^*}{\partial c} = \frac{1 + \phi R}{2 + \phi R} \quad \text{and} \quad \frac{\partial s^*}{\partial c}$$

$$= \frac{1}{2 + \phi R} \qquad (A.9)$$

An office-seeking politician has a marginal propensity to spend (save) out of a temporary windfall greater than that of a benevolent politician. Note that the propensity to spend (save) now extends from ½ to one (zero). Note that the propensity to spend (save) decreases (increases) the less competitive the elections are, represented by the ϕ parameter, which measures the sensitivity of votes to policy:

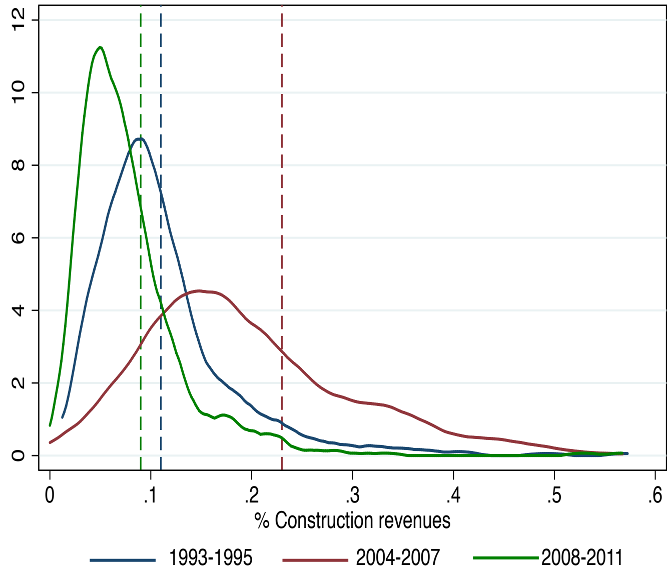
$$\frac{\partial^2 e_1^*}{\partial c \partial \phi} = -\frac{\partial^2 s^*}{\partial c \partial \phi} = \frac{R}{(2 + \phi R)^2}$$

$$\geq 0 \qquad (A.10)$$

This provides the basis for our empirical analysis that will study whether political competition (proxied by the winning margin of victory of the incumbent) does influence the effect of the temporary windfall on spending.

ii. Additional figures:

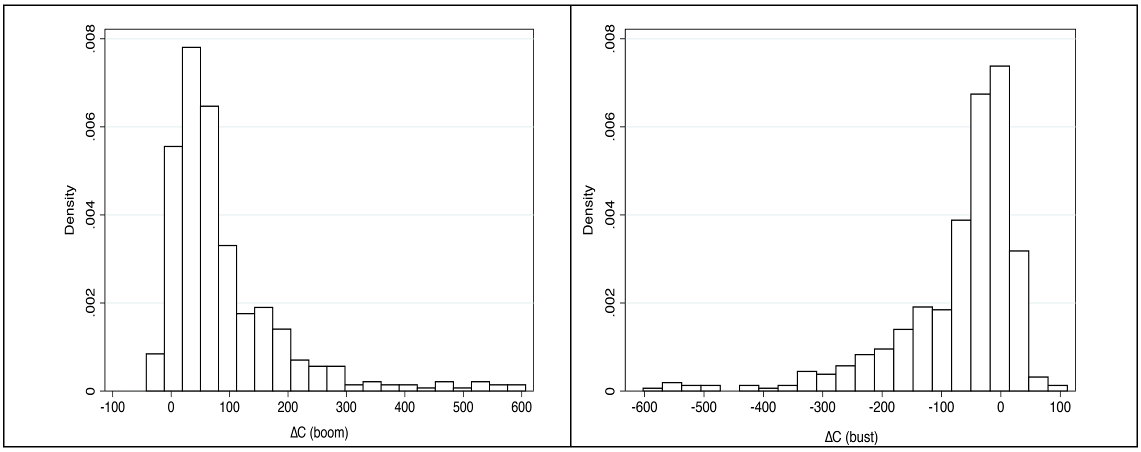
Figure A.1: *Share of construction revenues in the budget in %*



Notes: (1) Share of construction revenues measured over non-financial revenues; (2) Solid line is a Kernel Epachenikov fit; dashed line is the mean of the respective period; (3) Outlay data; (4) See Table A.2 for definitions and data sources.
Source: Ministerio de Economía y Hacienda (www.minhac.es), "Base de datos de liquidaciones de los presupuestos de las Entidades Locales".

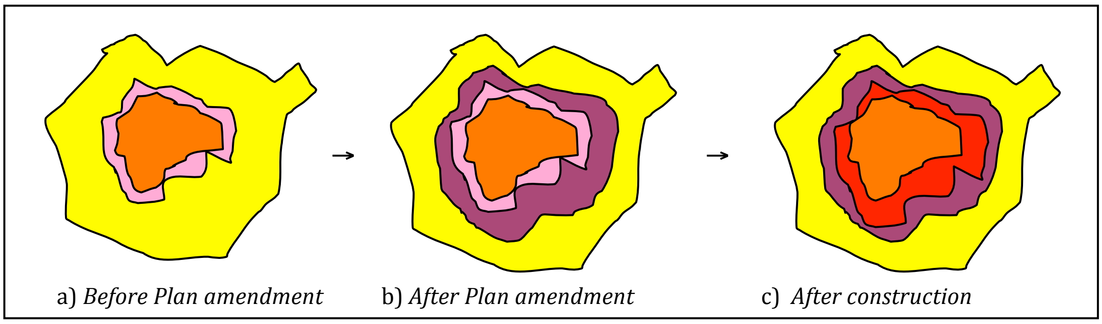
Figure A.2: *Growth of construction revenues. Boom & Bust.*

Boom	Bust



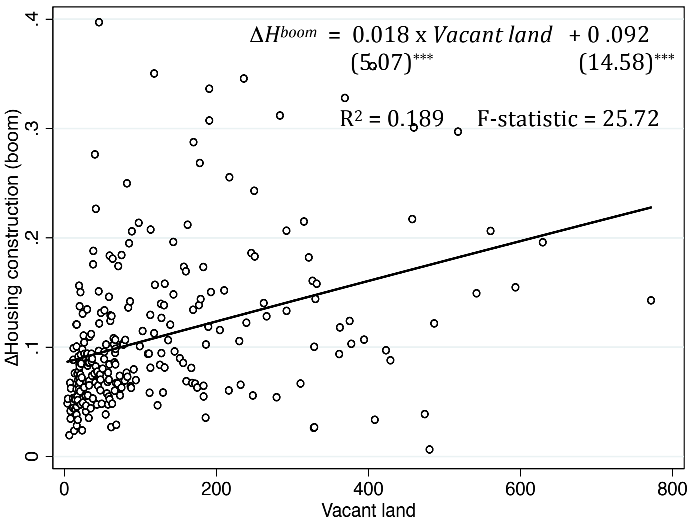
Notes: Growth in construction revenues per capita. Boom: revenues in period 2004-2007 minus revenues in period 1993-1995; Bust: revenues in period 2008-11 minus revenues in period 2004-2007.

Figure A.3: *Land use categories in Spain*



Notes: (1) *Yellow*: 'Non-developable' land (i.e., rural uses or protected); *Orange*: 'Developed' land before the amendment of the Master Plan (and before the construction that follows the implementation of the new plan); *Pink*: 'Developable' land before the amendment of the Master Plan (and also after); *Purple:* Developable land after the amendment of the plan but not before (i.e., amount of land converted from rural to urban uses between as a result of the amendment): Red: 'Developed' land with the new plan (i.e., construction that takes places once the new plan has been implemented). (2) The amount of vacant land at different moments is denoted with different colours: in graph (a) vacant land is denoted with *Pink*, in Panel (b) is *Pink* + *Purple*, and in Panel (c) is only *Purple*.

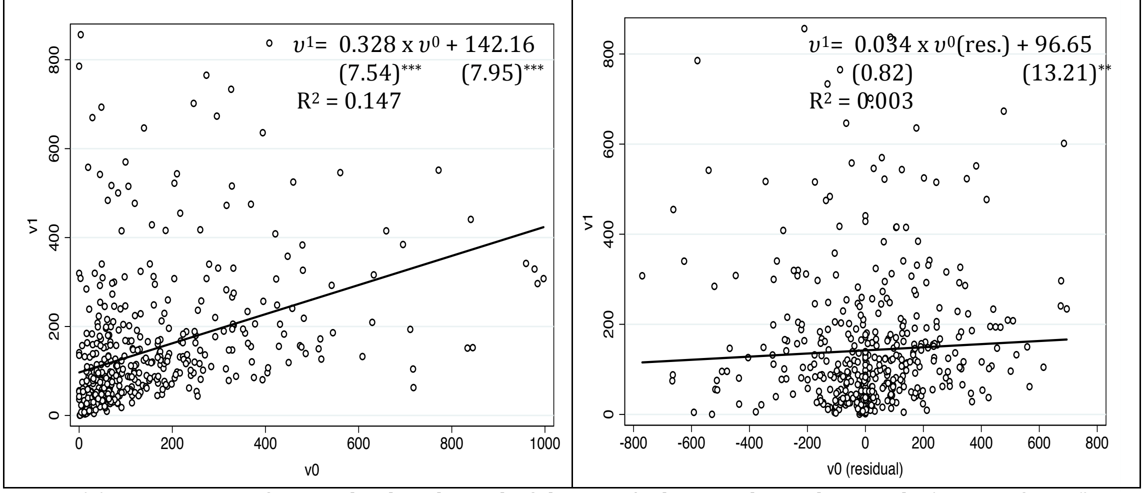
Figure A.4: *Housing Construction v. Vacant land*



Notes: (1) y-axis: Growth in Housing construction per capita (*H*) between base period (1993-1995) and peak of the boom (2004-2007); x-axis= vacant land per capita in 1995. (2) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the main road network (N=314); (3) t-values in parentheses, ***: significant at the 1% level; standard errors clustered by urban area.

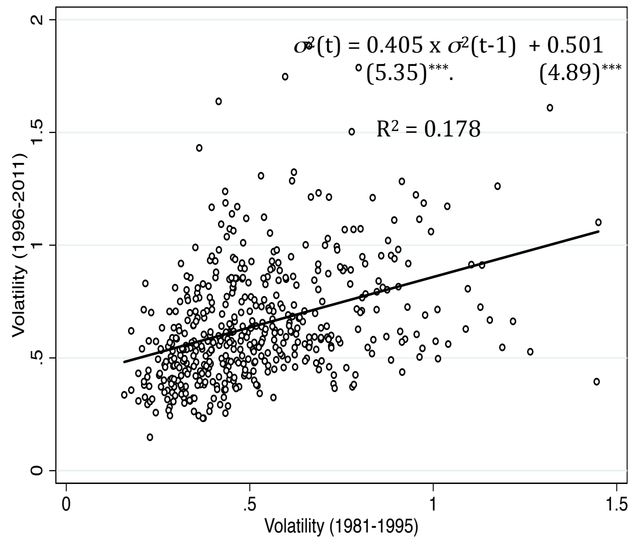
Figure A.5: *Quasi-randomness of the Vacant land instrument.*

(i) υ^1 vs. υ^0	(ii) υ^1 vs. $\hat{\upsilon}^0$ (residual)



Notes: (1) υ^1 = amount of vacant land at the end of the 1990' s housing boom-bust cycle (i.e., as of 2015); υ^0= amount of vacant land at the end of the 1980's housing boom-bust cycle (i.e., as 1995); υ^0 (residual) difference between this variable and its forecast: $\hat{\upsilon}^0_{ijk} = \hat{\lambda}_j + \hat{\lambda}_k + \hat{\beta} * x^0_{ijk}$ (see Table A.4); (2) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to highway network (N=314); (3) t-values in parentheses, ***: significant at the 1% level; standard errors clustered by urban area.

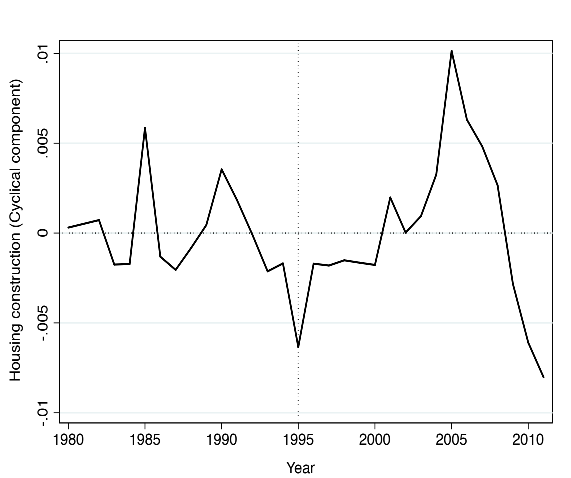
Figure A.7: *Correlation between past and current Volatility*



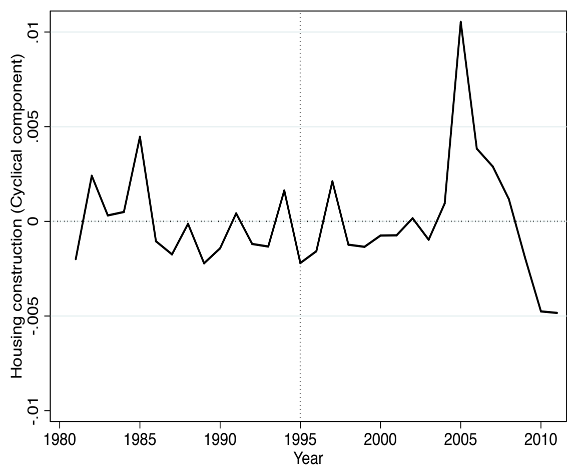
Notes: (1) Volatility calculated as the standard deviation of the Cyclical component of Housing construction, obtained after detrending the series using an HP filter with a filter parameter equal to 400. (2) The volatility is computed for the period studied in the paper (1996-2001), referred to as 'present volatility', and for the period 1980-1995, referred to as 'past volatility'. (3) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to highway network (N=314); (4) t-values in parentheses, ***: significant at the 1% level; standard errors clustered by urban area.

Figure A.6: *Cyclical component of Housing construction*

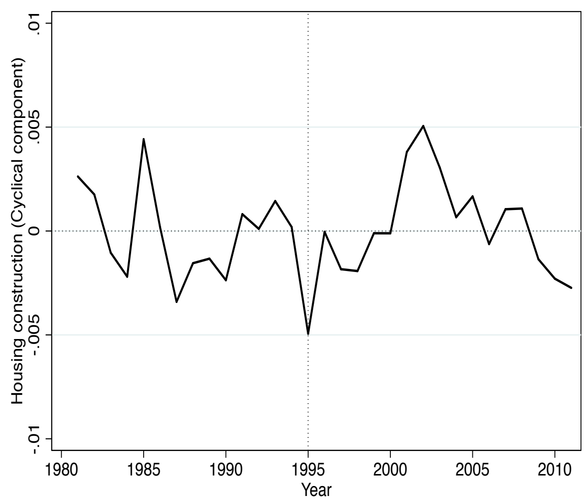
a) Urban areas



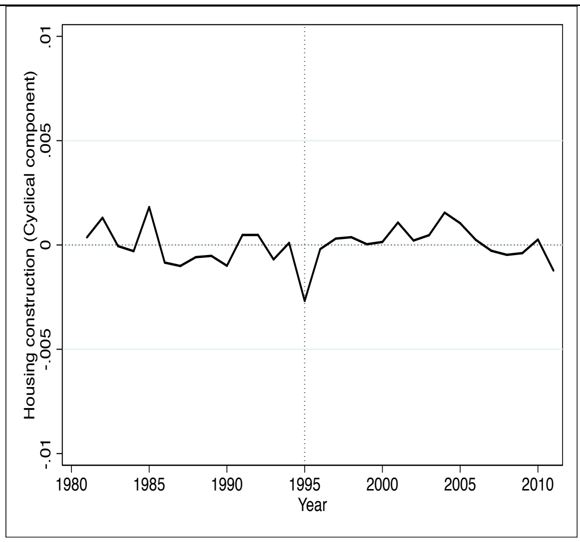
b) Madrid



c) Valencia

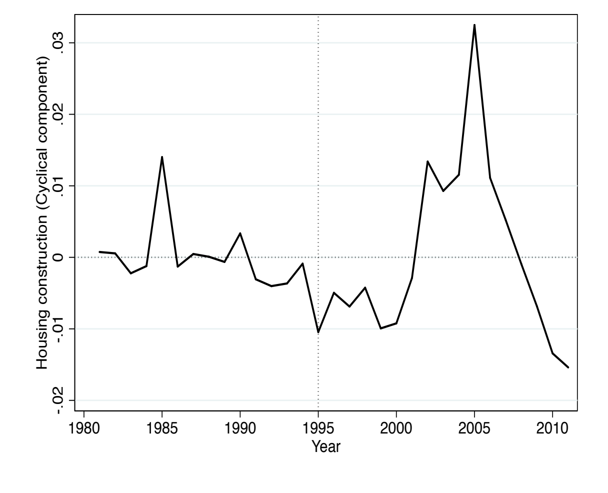


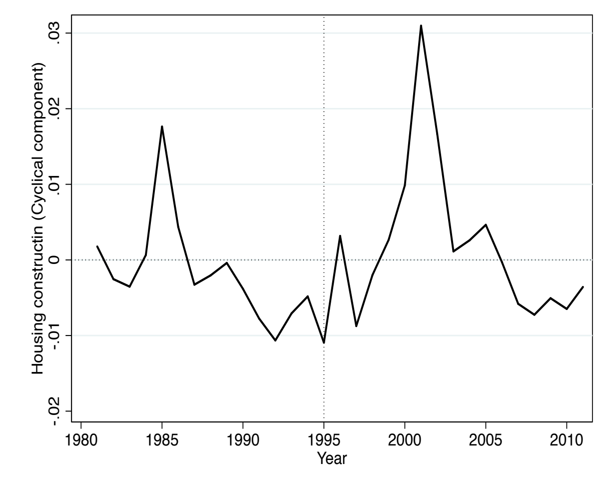
d) Barcelona



e) Alicante

f) Marbella



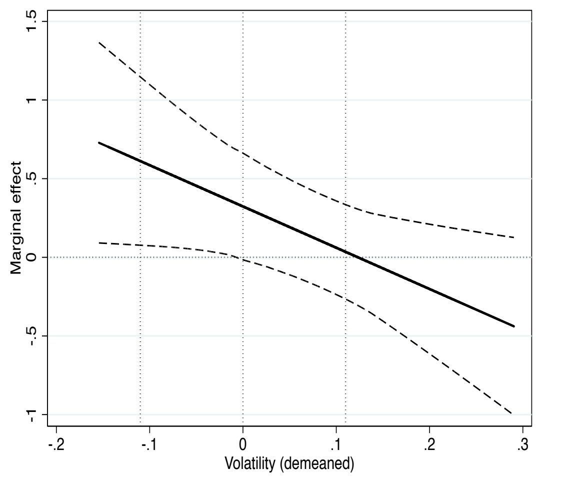


Notes: (1) Cyclical component of Housing construction (100 x (New housing units / Housing stock)) obtained after detrending the series using the HP filter with a filter parameter equal to 400. (2) Urban areas refer to the sample of urban municipalities used in the paper; the other graphs are for selected municipalities.

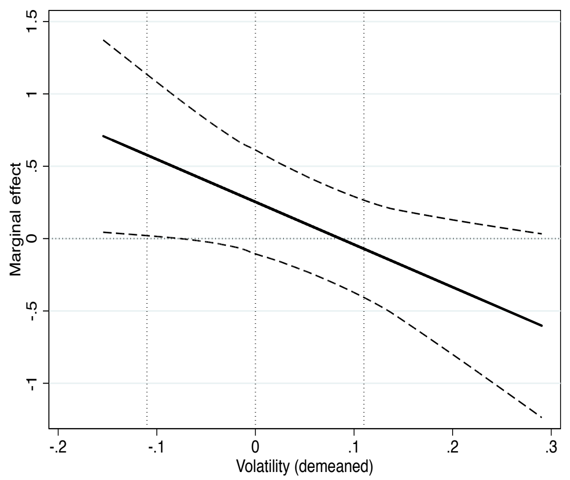
Figure A.8: *Marginal effects on Current and Capital spending*

a) *Volatility*

a.1) *Current spending*

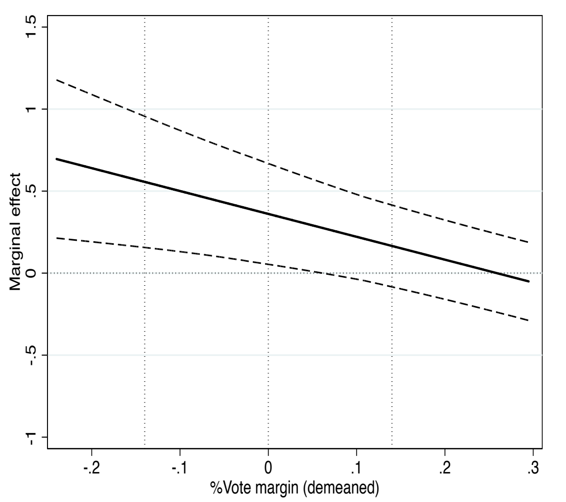


a.2) *Capital spending*

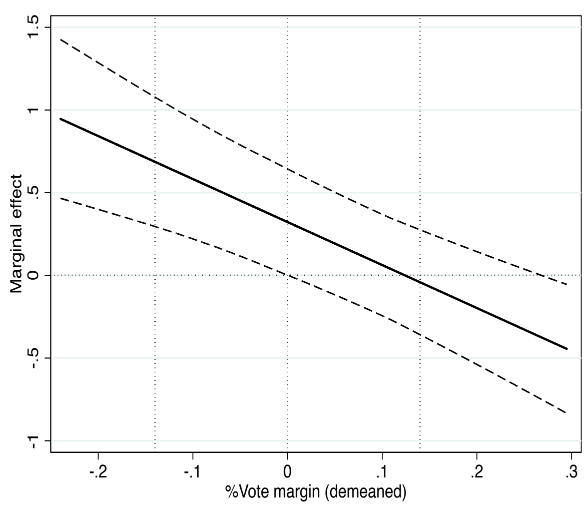


b) %*Vote margin*

b.1) *Current spending*



b.2) *Capital spending*

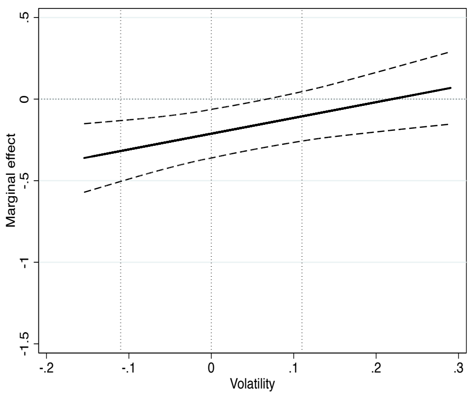


Notes: (1) Panel (a): Effect of an increase in construction revenues during the boom on Current and Capital spending during the boom at different levels of the Vote margin (bold line) and 95% confidence interval (dashed lines). (2) Panel (b): the same for different levels of (Predictable) Volatility. (3) Both the %Vote margin and the (Predictable) Volatility variables have been demeaned, so that the 0 indicates the marginal effect evaluated at the sample mean; the other two vertical lines indicate -1,+1 s.d. of the Vote margin.

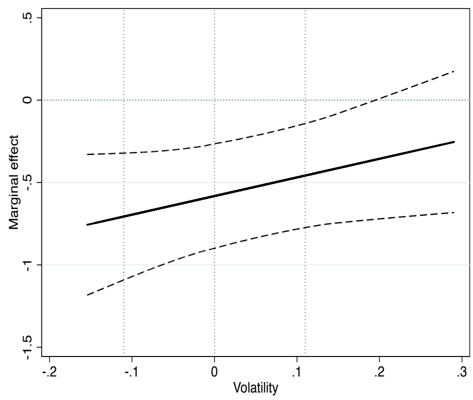
Figure A.9: *Marginal effects on Current and Capital spending. Bust period.*

a) *Volatility*

a.1) *Current spending*

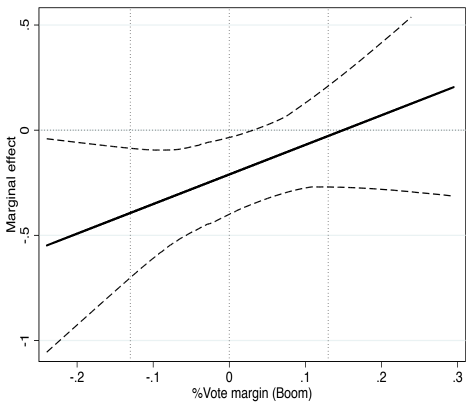


a.2) *Capital spending*

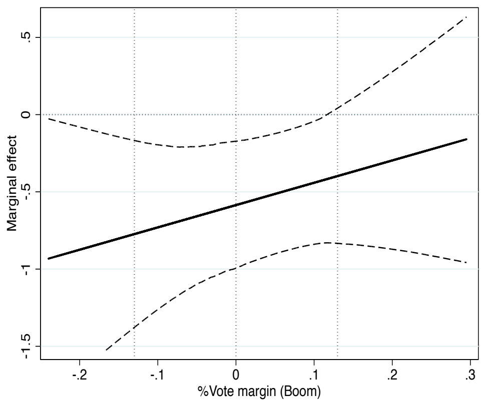


b) %*Vote margin* (Boom)

b.1) *Current spending*



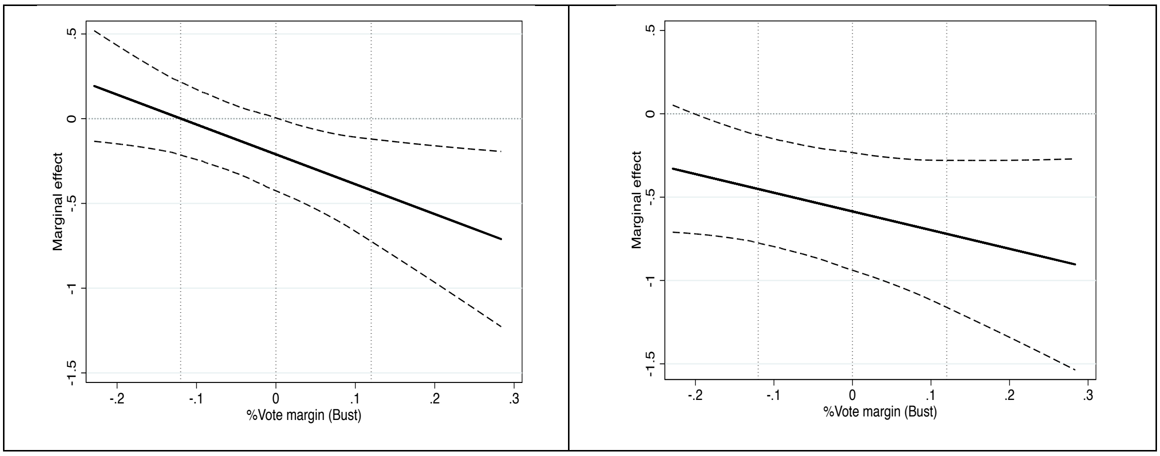
b.2) *Capital spending*



b) %*Vote margin* (Bust)

b.1) *Current spending*

b.2) *Capital spending*



Notes: (1) Panel (a): Effect of an increase in construction revenues during the boom on Current and Capital spending during the bust at different levels of (Predictable) Volatility (Bold line) and 95% confidence interval (dashed lines); Panels (b) and (c): the same for %Vote margin during the Boom and during the Bust, respectively. (2) Both (Predictable) Volatility and the %Vote margin variables have been demeaned, so that the 0 indicates the marginal effect evaluated at the sample mean; the other two vertical lines indicate -1,+1 s.d. of the Vote margin.

iii. Additional tables:

Table A.1: *Share of construction revenues in the budget in %*

	1995	2007	2011
Ordinary revenues	80.82	70.37	83.10
Ordinary taxes and fees	46.82	40.05	49.59
Transfers (current)	33.98	30.32	40.42
Extraordinary revenues	19.18	28.94	16.12
Construction revenues	**11.76**	**21.27**	**9.02**
Construction taxes	5.39	9.05	5.13
Construction fees	4.15	5.62	2.68
Sales of land plots	2.22	6.60	1.51
Transfers (capital)	7.42	7.67	7.10
	100.00	100.00	100.00

Notes: (1) Share of construction revenues measured over non-financial revenues; (2) Outlay data; (3) See Table A.2 for definitions and data sources.
Source: Ministerio de Economía y Hacienda (www.minhac.es), "Base de datos de liquidaciones de los presupuestos de las Entidades Locales".

Table A.2: *Variable definitions and data sources.*

	Definition	*Sources*
	(a) *Budget variables*	

Construction revenues p.c.	(Betterment tax ('Impuesto sobre Incremento del Valor de los Terrenos de Naturaleza Urbana') + Construction tax ('Impuesto sobre Construcciones Instalaciones y Obras) + Developers fees ('Licencia de obras' + 'Cuotas de promotor') + Land sales ('Enajenación de terrenos))/ Population	Ministerio de Hacienda y Administraciones Públicas: "Estadísticas sobre liquidaciones de los presupuestos de las Entidades Locales", http://www.minhafp.gob.es/, several years
Spending p.c.	(Total spending (current + capital)) / Population	
Current spending p.c.	(Spending on Personnel, Purchases and Current transfers) /Population	DCG, Dirección General del Catastro: "Estadísticas sobre ordenanzas fiscales del Impuesto sobre Bienes Inmuebles", http://www. catastro.meh.es /, several years
Capital spending p.c.	(Public investment + Capital transfers) / Pop.	
Tax revenues p.c.	(Revenues from Taxes + Fees – Construction taxes and Fees)/ Population	
Grants p.c.	(Current + Capital grants)/ Population	La Caixa: 'Anuario Económico de España', several years
Debt burden p.c.	(Interest + Debt principal)/ Population	
Current savings	(Current revenues – Current spending – Debt principal)/ Population	

Table A.2 (continued)

	Definition	*Sources*
	(b) *Housing variables*	
Vacant land p.c. (v^0)	Amount of land (hectares) qualified as developable in the Master Plan but not yet developed at the start of the boom (in 1995) / Population	DCG, Dirección General del Catastro: "Estadísticas sobre ordenanzas fiscales del Impuesto sobre Bienes Inmuebles", http://www. catastro.meh.es /, several years
Building density	Amount of developed land / Population	INE, Instituto Nacional de Estadística: "Censo de Población y Viviendas", several years
Property value p.c.	Assessed value of the housing stock / Population	
	(c) *Socio-economic variables*	

% Vote margin	abs(vote share left wing parties-vote share right wing parties), using data form the 1991 and 1995 elections (for Table A.2) or data from the 1999 and 2003 ones (for the main results)	Ministerio del Interior, *Base Histórica de Resultados Electorales*, http://www. elecciones.mir.es/MIR/jsp /resultados index.htm, several years
Predicted % Vote margin	abs(vote share left wing parties-vote share right wing parties), the vote share of each party is the vote share at the 1979 local elections x growth rate of vote of this party from 1979 to 1999 or 2003	
% Left vote	Vote share of left wing parties, computed as the average of the 1991 and 1995 elections (for Table A.2) or as the average of the 1999 and 2003 elections (for the main results)	
Coalition	Dummy equal to one if the most voted party does not have a majority of the seats in the local council. Computed as the average of the 1991 and 1995 elections (for Table A.2) or as the average of the 1999 and 2003 elections (for the main results)	
Income p.c.	Personal income / Population	La Caixa: 'Anuario Económico de España', several years
Population size	Population	INE, Instituto Nacional de Estadística: "Cifras oficiales de población de los municipios españoles", yearly data. http://www.ine.es/
% Old	Resident pop. older than 65 / Population	INE, Instituto Nacional de Estadística: "Censo de Población y Viviendas 1961, 1971, 1981, 1991, 2001 and 2011", decennial data http://www.ine.es/
% College education	Resident pop. with a college degree / Population	
% Immigrants	Resident pop. born outside the EU/Population	
%Renters	Rental housing units / housing units	
%Commuters	Resident population working outside the municipality / population	

Table A.3: *Descriptive statistics.*

	Mean	S.D.	Min.	Max
	(a) *Main variables*			
Construction revenues p.c.				
Base: 1993-95	43.01	31.23	3.07	333.08
Δ^{boom}= (2007-04) – (1993-95)	91.11	99.45	-71.74	606.69
Δ^{bust}= (2008-11) – (2007-04)	-85.24	97.12	-610.84	98.37
Expenditures p.c.				
Base: 1993-95	394.46	148.96	83.46	1,074.51
Δ^{boom}= (2007-04) – (1993-95)	219.99	163.55	-169.39	1180.13
Δ^{bust}= (2008-11) – (2007-04)	9.38	136.08	-816.53	314.17
Tax revenues p.c.				
Base: 1993-95	75.85	58.65	2.43	533.28
Δ^{boom}= (2007-04) – (1993-95)	36.13	34.78	-225.98	206.17

Δ^{bust}= (2008-11) – (2007-04)	28.13	80.03	-672.40	286.28
Grants p.c.				
Base: 1993-95	139.22	50.63	60.65	476.80
Δ^{boom}= (2007-04) – (1993-95)	68.52	68.23	-187.65	495.57
Δ^{bust}= (2008-11) – (2007-04)	2.37	18.95	-147.86	80.46
Current expenditures p.c.				
Base: 1993-95	297.14	102.99	71.97	736.76
Δ^{boom}= (2007-04) – (1993-95)	147.74	76.10	-54.09	512.98
Δ^{bust}= (2008-11) – (2007-04)	20.15	58.36	-450.31	151.01
Capital expenditures p.c.				
Base: 1993-95	97.21	74.91	10.49	485.14
Δ^{boom}= (2007-04) – (1993-95)	72.25	120.55	-266.58	951.59
Δ^{bust}= (2008-11) – (2007-04)	-10.77	103.89	-828.36	274.50
Vacant land p.c. (v^0)	122.36	136.26	0	771.67
	(b) *Interactions*			
% Vote margin	24.96	16.00	1.01	84.02
Volatility	0.63	0.11	0.44	0.97
Debt burden p.c.	44.77	46.21	0	456.93
Income p.c.	12,027	3,387	3,770	34,623

Notes: (1) Budget variables and Income measured in euro per capita; % Vote margin and Housing price break are in %, Vacant land is in Ha p.c. (2) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the main road network (N=314); (3) See Table A.2 in the Appendix for definitions and sources of the variables.

Table A.4: *Determinants of Vacant land.*

	Highway sample (N=314)		*Whole urban sample* (N=452)	
	(1)	(2)	(3)	(4)

Housing construction p.c.	5.649** (2.810)	2.393 (1.492)	3.785** (1.565)	2.115 (1.359)
Population	-0.003 (0.002)	-0.002 (0.003)	-0.001 (0.003)	-0.001 (0.004)
Highway access	--.--	--.--	-13.412** (6.419)	-17.336** (8.432)
% Vote margin	-0.056 (0.082)	-0.043 (0.079)	-0.102* (0.048)	-0.123* (0.068)
% Left vote	-0.633* (0.326)	-0.520 (0.361)	-1.342** (0.409)	-1.026* (0.467)
Coalition	-0.009 (0.039)	-0.012 (0.109)	-0.011 (0.056)	-0.018 (0.066)
Income p.c.	0.011 (0.008)	0.005 (0.006)	0.008*** (0.001)	0.009*** (0.002)
% College education	-0.200 (0.230)	-0.202 (0.459)	-0.290** (0.135)	-0.277** (0.144)
%Renters	0.020 (0.125)	0.021 (0.190)	0.027 (0.101)	0.034 (0.089)
%Commuters	-0.003 (0.012)	-0.004 (0.011)	-0.012 (0.007)	-0.014 (0.009)
Debt burden p.c.	-0.177 (0.229)	-0.132 (0.388)	-0.122 (0.176)	-0.110 (0.154)
Spending p.c.	0.098 (0.297)	0.101 (1.044)	0.077 (0.276)	0.123 (0.176)
Property value p.c.	0.354 (0.229)	0.245 (0.583)	0.311 (0.651)	0.209 (0.981)
Distance to CBD >5 & <=10 Km.	--.--	28.39* (17.31)	--.--	25.34* (13.95)
Distance to CBD >10 & <=15 Km.	--.--	42.91** (18.79)	--.--	39.97** (19.17)
Distance to CBD >15 Km.	--.--	81.93*** (26.31)	--.--	77.20*** (27.10)
Constant	33.82 (35.381)	--.--	23.312 (40.775)	--.--
R^2 (adj.)	0.219	0.377	0.155	0.323
F-stat (*Urban area f.e.*)	--.--	2.473 [0.000]	--.--	3.257 [0.000]
F-stat (*Distance to* CBD *f.e.)*	--.--	3.892 (0.000]	--.--	4.365 [0.000]
Urban area f.e.	NO	YES	NO	YES

Notes: (1) The dependent variable is Vacant land per capita (as of 1995), measured as the amount zoned for development in 1995 but not yet developed, divided by resident population in 1995. (2) Housing construction is the number of housing units build during the 1986-1991 boom per capita; the rest of the variables are measured before 1995. (3) Highway sample: municipalities larger than 1,000 inhabitants and with access to the highway network in larger Spanish urban areas (N=314); Whole sample: includes municipalities with and without highway access (N=452); (4) Standard errors clustered by urban area in parenthesis; ***. *** & *= statistically significant at the 1%, 5% and 10% levels; (5) *F-stat* are the F-statistic used for the test of joint significance of the urban area and distance-to-central-city interval fixed effects (*Distance to* CBD <5 Km. is the base category); values in brackets are p-values. (6) Dependent variable is *Vacant land p.c.*, measured as the amount zoned for development in 1995 but not yet developed, divided by resident population in 1995. See Table A.2 for definitions and sources of the variables.

Table A.5: *Effect of Vacant land on alternative channels.*

	(1)	(2)	(3)	(4)	(5)
	a) Δ *over the boom period in:*				

	Construction revenues	*Ordinary revenues*	*Population*	*Income per capita*	*Building density*
Vacant land p.c. (v^0)	0.234*** (0.055)	0.016** (0.007)	0.004* (0.002)	0.001 (0.003)	-0.021 (0.048)
R^2 (adj.)	0.406	0.302	0.256	0.220	0.110
	b) Δ *over the boom period in:*				
	% Immigrants	*% Young*	*% Vote margin*	*% Left vote*	*Coalition*
Vacant land p.c. (v^0)	-0.001 (0.009)	0.002 (0.182)	0.002 (0.006)	-0.001 (0.001)	-0.024 (0.029)
R^2 (adj.)	0.162	0.154	0.324	0.337	0.251

Notes: (1) Effects on changes in different outcomes from the period prior to the boom (1993-95) to the peak of the boom (2004-2007); (2) See Table A.2 for definitions of the variables; (3) All equations include urban area and distance-to-central-city interval fixed effects; (4) Sample: municipalities larger than 1,000 inhabitants and with access to the main road network in larger Spanish urban areas (N=314); (5) Standard errors clustered by urban area in parenthesis; ***. *** & *= statistically significant at the 1%, 5% and 10% levels.

Table A.6: *Effect of the age of the Master Plan*

	First-stage		2SLS
	Construction revenues p.c. (Δw^{boom})		*Spending p.c.* (Δe^{boom})
	× *Old Plan* (<1986)	× *New Plan* (≥1987)	
Vacant land p.c. (v^0) × *Old Plan* (<1986)	0.267 (2.70)***	-0.021 (-0.58)	--.--
× *New Plan* (≥1986)	0.006 (0.11)	0.238 (2.55)***	--.--
$\Delta\widehat{w}^{boom}$ × *Old Plan* (<1986)	--.--	--.--	0.682 (2.48)**
× *New Plan* (≥1986)	--.--	--.--	0.751 (2.17)**
F-stat../K-P. stat.	8.23 (0.000)	7.94 (0.000)	7.93 (0.000) 7.03/4.68/3.95
Test Old = New	0.029 (0.853)		0.020 (0.900)

Notes: (1) 2SLS estimates allowing for an interaction with the age of the Master Plan. Old Plans are the ones approved before the start of the 1980s boom. Data on the year of the plan comes from Ministerio de Obras Públicas y Transporte: *Planeamiento Urbanístico Vigente en los Municipios Españoles. Informe num 2, Segundo Semestre 1993.* (2) Test Old = New is a $\chi 2$ test of equality of the coefficients for the Old and New cases, in both the first and second stage equations. (3) The estimation controls for the amount of developable land and includes urban area and distance-to-central-city interval fixed effects; (4) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to highway network (N=314); (5) t-statistic in parenthesis; ***. *** & *= statistically significant at the 1%, 5% and 10% levels; standard errors clustered by urban area.

Table A.7: *Sensitivity to omitted variables.*

	(1)	(2)
	Estimated coefficient ($\tilde{\beta}$) / *Unbiased coefficient* (β)	

Reduced form coefficient	0.167 / 0.161	0.166 / 0.163
First stage coefficient	0.235 / 0.230	0.233 / 0.228
2SLS *coefficient*	0.711 / 0.688	0.722 / 0.714
Urban area & Distance to CBD f.e.	YES	YES
Predetermined covariates	YES	YES
Alternative channels	NO	YES

Notes: (1) *Estimated coefficient* ($\tilde{\beta}$): coefficient estimated including the different sets of control variables indicated at the bottom of each column; (2) *Unbiased coefficient* (β): computed using Oster's formula: $\beta = \tilde{\beta} + (\tilde{\beta} - \beta^{\circ})*(1.3*\tilde{R}/(\tilde{R} - R^{\circ}))$, where β° is the coefficient obtained in the specification without controls, $\tilde{\beta}$ is the coefficient obtained when including the different sets of controls, R° and $\tilde{R}$ are the R-squared of these two cases, respectively (see Oster, 2018); (4) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the highway network (N=314).

Table A.8: *Effect on Expenditures. Whole urban sample.*

	(1)	(2)	(3)	(4)
	(a) 2SLS			
Construction revenues p.c. (Δw^{boom})	1.113*** (0.243)	0.934** (0.432)	0.887* (0.450)	0.772* (0.376)
AR *test* (*weak inst. Inference*)	9.01 [0.003]	4.58 [0.027]	2.29 [0.084]	3.59 [0.058]
AR *confidence interval* (95%)	[0.570, 1.656]	[0.101, 1.811]	[-0.144, 1.918]	[-0.041, 1.585]
	(b) *First-stage*			
Vacant land p.c. (υ^0)	0.200*** (0.044)	0.151*** (0.042)	0.127** (0.042)	0.141*** (0.042)
R^2 (adj.)	0.077	0.204	0.203	0.287
First stage F-statistic	17.78	11.58	5.66	4.95
	[16.38 / 8.96 / 6.66]			
	(c) OLS			
Construction revenues p.c. (Δc^{boom})	0.979*** (0.085)	0.906*** (0.094)	0.925*** (0.109)	0.900*** (0.112)
R^2 (adj.)	0.371	0.620	0.514	0.579
Urban area & Distance to CBD	NO	YES	YES	YES
Pre-determined covariates	NO	NO	YES	YES
Alternative channels	NO	NO	NO	YES

Notes: (1) Dependent variable is increase in total spending per capita during the boom period; (2) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants (N=492); (3) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels; (4) AR test: Anderson Rubin test (H0: Δw^{boom} coefficient = 0) robust to weak instruments; AR confidence interval: 95% c.i. robust to weak instruments, built using the AR test. (5) *First stage F-statistic*: Kleiberger -Paap Wald rk F-statistic; in brackets Stock-Yogo weak identification test critical values at 10%/15% and 20% maximal IV bias.

Table A.9: *Budget pre-trends. Whole urban sample.*

	(1)	(2)	(3)	(4)	(5)	(6)

	(a) *Previous boom* (1986-87 to 1990-91)			(b) *Previous bust* (1990-91 to 1993-95)		
	Expenditures	*Tax revenues*	*Grants*	*Expendi-tures*	*Tax revenues*	*Grants*
Vacant land p.c. (v^0)	0.055** (0.021)	0.010 (0.012)	-0.011 (0.055)	0.021 (0.033)	0.014 (0.014)	-0.037 (0.051)
R^2 (adj.)	0.405	0.470	0.313	0.235	0.239	0.244
	(c) *Previous boom-bust cycle* (1986-87 to 1993-95)			(d) *Current boom period* (1993-95 to 2004-07)		
	Expenditures	*Tax revenues*	*Grants*	*Expendi-tures*	*Tax revenues*	*Grants*
Vacant land p.c. (v^0)	0.042* (0.024)	0.025 (0.018)	-0.038 (0.112)	0.146*** (0.058)	-0.021** (0.060)	-0.028 (0.039)
R^2 (adj.)	0.366	0.467	0.327	0.383	0.333	0.261

Notes: (1) Effects on changes in different budget variables (per capita) during the previous boom and bust periods and during the current boom; (2) For the current boom period (Panel (d)) we use Outlay data; for the other periods (Panels (a) to (c)) we use Initial budget data for expenditures and grants and Outlay data on the property tax for tax revenues. (3) All equations include urban area and distance-to-central-city interval fixed effects; (4) Sample: municipalities larger than 1,000 inhabitants in larger Spanish urban areas (N=452); (5) Standard errors clustered by urban area in parenthesis; ***. *** & *= statistically significant at the 1%, 5% and 10% levels.

Table A.10: *Effect of Vacant land on alternative channels. Whole urban sample.*

	(1)	(2)	(3)	(4)	(5)
	a) Δ *over the boom period in:*				
	Construction revenues	*Ordinary revenues*	*Population*	*Income per capita*	*Building density*
Vacant land p.c. (v^0)	0.151*** (0.042)	0.017* (0.009)	0.002* (0.002)	0.004 (0.007)	-0.025* (0.014)
R^2 (adj.)	0.204	0.326	0.191	0.311	0.110
	b) Δ *over the boom period in:*				
	% Immigrants	*% Young*	*% Vote margin*	*% Left vote*	*Coalition*
Vacant land p.c. (v^0)	0.002 (0.208)	0.001 (0.203)	0.001 (0.003)	0.001 (0.006)	-0.021 (0.002)
R^2 (adj.)	0.183	0.143	0.341	0.235	0.245

Notes: (1) Effects on changes in different outcomes from the period prior to the boom (1993-95) to the peak of the boom (2004-2007); (2) See Table A.2 for definitions of the variables; (3) All equations include urban area and distance-to-central-city interval fixed effects; (4) Sample: municipalities larger than 1,000 inhabitants in larger Spanish urban areas (N=452); (5) Standard errors clustered by urban area in parenthesis; ***. *** & *= statistically significant at the 1%, 5% and 10% levels

Table A.11: *Mechanisms. First stage results.*

	(1)	(2)	(3)	(4)

	Δc^{boom}	Δc^{boom} × *% Vote margin*	Δc^{boom}	Δc^{boom} × *Predictable Volatility*
Vacant land p.c. (υ^0)	0.234** (0.112)	0.012 (0.089)	0.242** (0.101)	-0.091 (0.331)
× *% Vote margin*	-0.120 (0.221)	0.252*** (0.121)	--.--	--.--
× *Predictable Volatility*	--.--	--.--	0.234 (0.589)	0.251** (0.165)
K-P F-statistic	8.47		7.33	
	[7.03 / 4.58 / 3.95]			

Notes: (1) First stage of the 2SLS equations 1 and 5 in Table 6. (2) K-P F-statistic: Kleiberger-Paap Wald rk F-statistic; in brackets below we report the Stock-Yogo weak identification test critical values at 10%/15% and 20% maximal IV bias. (3) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels.

Table A.12: *Mechanisms: additional stories*. 2SLS *results*

	(1)	(2)	(3)	(4)	(5)	(6)
Construction revenues p.c (Δc^{boom})	0.640** (0.279)	0.652*** (0.235)	0.699*** (0.236)	0.667*** (0.239)	0.631** (0.279)	0.697** (0.319)
× *% Vote margin*	--.--	--.--	-0.033* (0.024)	-0.033** (0.017)	-0.030** (0.013)	-0.035* (0.018)
× *Predictable Volatility*	--.--	--.--	-0.045* (0.023)	-0.045* (0.023)	-0.045* (0.023)	-0.044 (0.025)
× *Debt burden p.c.*	0.016 (0.230)	--.--	0.021 (0.155)	--.--	0.050 (0.208)	0.050 (0.208)
× *Income per capita*	--.--	-0.009 (0.018)	--.--	-0.007 (0.011)	-0.010 (0.027)	-0.010 (0.027)
× *% Vote left*	--.--	--.--	--.--	--.--	--.--	-0.010 (0.054)
× *Coalition*	--.--	--.--	--.--	--.--	--.--	-0.075 (0.530)
× *Population*	--.--	--.--	--.--	--.--	--.--	0.030 (0.026)
× *% College educated*	--.--	--.--	--.--	--.--	--.--	-0.033 (0.021)
First stage F-statistic	8.33	8.01	--.--	--.--	--.--	--.--

Notes: (1) Dependent variable is increase in total spending per capita during the boom period. (2) All equations include urban area and distance-to-central-city interval fixed effects. See Tables 2 & 3. (3) See Table A.2 for definitions and sources of the different variables. (5) First stage F-statistic: Kleiberger-Paap Wald rk F-statistic Stock-Yogo weak identification test critical values at 10%/15% and 20% maximal IV bias: 7.03, 4.58 and 3.95 (6) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels.

Table A.13: *Effect on Expenditures during the bust* (Δe^{bust})

	(1)	(2)	(3)	(5)

	(a) 2SLS			
Construction revenues p.c. (Δc^{boom})	-0.986*** (0.174)	-0.790*** (0.187)	-0.786*** (0.254)	-0.784*** (0.189)
	(b) *First-stage*			
Vacant land p.c. (υ^0)	0.284*** (0.063)	0.262*** (0.064)	0.256*** (0.064)	0.244*** (0.059)
R^2 (adj.)	0.177	0.355	0.372	0.386
K-P F-statistic	19.23	19.11	19.12	18.35
	[16.38 / 8.96 / 6.66]			
	(c) OLS			
Construction revenues p.c. (Δc^{boom})	-1.231*** (0.098)	-1.045*** (0.103)	-1.101*** (0.119)	-0.993*** (0.112)
R^2 (adj.)	0.388	0.590	0.580	0.607
Urban area & Distance to CBD f.e..	NO	YES	YES	YES
Predetermined covariates	NO	NO	YES	YES
Alternative channels	NO	NO	NO	YES

Notes: (1) The dependent variable is the change in spending per capita during the bust period; (2) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to highway network (N=314); (3) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels; (4) K-P F statistic: Kleiberger-Paap Wald rk F-statistic; in brackets Stock-Yogo weak identification test critical values at 10%/15% and 20% maximal IV bias.

Table A.14: *Mechanisms. Bust period*. 2SLS *results.*

	(1)	(2)	(3)	(4)	(5)	(6)
Construction revenues p.c (Δc^{boom})	-0.649** (0.263)	-0.708** (0.277)	-0.698** (0.295)	-0.654** (0.273)	-0.788** (0.281)	-0.685** (0.311)
× *% Vote margin* (Boom)	0.015 (0.010)	--.--	0.013 (0.008)	--.--	0.029** (0.011)	0.025** (-0.012)
× *% Vote margin* (Bust)	--.--	--.--	--.--	-0.021** (0.010)	-0.028** (0.014)	-0.030** (-0.014)
× *(Predictable) Volatility*	--.--	0.019** (0.009)	0.018* (0.010)	--.--	--.--	0.017** (0.008)
First stage F-statistic	7.89	10.79	--.--	8.03	--.--	--.--

Notes: (1) Dependent variable is increase in total spending per capita during the bust period. (2) All equations include urban area and distance-to-central-city interval fixed effects. See Tables 2 & 3. (3) Sample: municipalities in larger Spanish urban areas larger than 1,000 inhabitants and with access to the main road network (N=314); (5) First stage F-statistic: Kleiberger-Paap Wald rk F-statistic; Stock-Yogo weak identification test critical values at 10%/15% and 20% maximal IV bias: 7.03, 4.58 and 3.95. (7) Standard errors clustered by urban area in parenthesis; ***. ** & *=statistically significant at the 1%, 5% and 10% levels.