Università degli Studi di Messina

Phd programme in
Economics, management and statistics
XXX cycle

ESSAYS IN PUBLIC ECONOMICS AND POLICY

S.D.D. SECS-P/03

Phd thesis by
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Academic year 2016/2017

Contents

Preliminary note

This dissertation is composed of three autonomous chapters that empirically investigate issues related to public economics exploiting micro level data. Chapter 1 and chapter 2 evaluate spending and political behavior of Italian local institutions, Chapter 3, instead, concentrates on firms' profitability and employment level. A common ground in all the three chapters is the effort to identify conditions that enable to estimate "causal effects". In particular, in the first analysis I estimate the effect of neighboring municipalities' spending on municipality. The second chapter focuses on the effect of Political budget cycle on the European structural funds' municipal utilization. The third chapter investigates the effect of Employment legislation constraints on firms' profitability and employment level.

As exemplified in Angrist, J.D., Pischke, J.S. (2009) the causal effect is "what would happen to a given individual not hospitalized in a hypothetical comparison of alternative hospitalization (scenarios)"; in order to identify the effect of a scenario on the individual we have to compare it to the effect on individual in the opposite situation. The problem is that we cannot observe the individual in the two scenarios because only one realized. In presence of randomization the selection problem is avoided, such as for experiments in medicine when is random if an individual experiment the scenario 1 or the opposite one. If we are not in presence of randomization, we have to solve the problem of lack of information, facing with the concept of "counterfactual" (Loi, M. and Rodrigues, M.,2012). A counterfactual is for the individual in the happened scenario the potential outcome for the alternative state, and for the individual that do not experiment the scenario the potential outcome if they would be experimented it. When we think to this type of analysis the framework is always composed by a part of the population or of the sample which is subjected to the *treatment*. The researcher's effort consists on finding a reliable control group. In fact, it is not always available or it is not easy to identify a reliable one that is not affected by the *selection bias*, a type of distortion for which an individual belongs to a group which receive a treatment or experiment a scenario because of its characteristics and not randomly assigned. The econometric theory formulates techniques to construct an adequate control group, this process is known as *identification strategy* and consent to simulate the counterfactual outcome, solving the problem of selection bias. These identification strategies allow us identifying the causal effect; in absence of this identification, we can only talk about correlation between the *treatment* (scenario) and the outcome.

Chapter 1 explores the existence of spatial effect on Italian municipalities' spending decisions. I estimate a spatial autoregressive dynamic panel data model, using information on 5,564 Italian municipalities over the period 2001-2011, exploiting their border contiguity as a measure of spatial neighborhood. I find a positive and statistically significant effect of neighboring expenditures on total, capital and current expenditures of

a given municipality. These results are robust to the use of alternative weighting matrices for the definition of spatial neighbors. Anyway, this type of analysis, within the framework of spatial econometrics that use internal instruments, does not offer, in general, a valid identification of causal relation, and in turn, might lead to biased estimates. For this reason, I also employ a *quasi-natural experimental approach* to estimate a causal effect. The randomization of the data can not be exploited, for this reason it is necessary taking advantage of a particular condition in the observable data which enables to set up a quasi-natural experiment. In my case I exploit the exogenous variation in the neighbors' spending induced by the devastating earthquake occurred in 2009 in Abruzzo. The natural event hits municipalities belonging to the L'Aquila area, but not all municipalities of Abruzzo region. I can identify a group hit by the earthquake, whose average expenditure increased with the earthquake intensity. In this way, the new instrument used in the analysis is based on an unexpected natural disaster and it is reasonable to consider it as exogenous for any single municipality. The results confirm the presence of interdependence between municipalities' spending. Moreover, I do not find any evidence of yardstick competition when I take into account political effects, while I do find a negative relationship between spatial interaction and the size of the municipality. Thus, I conclude that spillover effects drive the strategic interactions in spending decisions among Italian municipalities.

Chapter 2 focuses on the dynamic of the European Structural funds' utilization at municipal level. I investigate the presence of Political Budget Cycle (PBC) demonstrating that mayors exploit the possibility to implement projects in their territory in order to send out a signal for the electorate. I estimate the existence of a causal relationship between PBC and the probability to start or end at least one EU projects, between PBC and the number of projects started or ended and between PBC and fragmentation of the EU projects realized. I use information on 3,102 Italian municipalities over the period 2007-2014, also distinguishing between municipalities of *Convergence* objective regions and *Competition and employment objective* regions (objective's European classification for EU Structural Funds). In this case the causal relationship between the variables tested is assured by the randomly assignment distribution of the dummy independent variable PBC equal to 1 if the year is the year before the election. The year of election, and consequently the year before the election, cannot be modified by the mayors and depends on a historical dynamic at national level and it does not depend on the characteristics of municipalities. We can say that belonging to the group of municipalities going to election in a particular year is random. I find evidence of the existence of PBC in the dynamic of EU structural funds' process, with differences related to the nature of projects realized linked to EU Objectives.

Finally, Chapter 3 concentrates on firm level data, trying to identify a causal effect through an experimental econometric approach. I test the effect of the Employment Protection Legislation on Italian firms' performance and on the level of employment. I use a panel dataset at firm-level for the period 2005-2011 which contains data of corporations. I set up an identification strategy based on a discontinuity regression design and a difference-

in-differences approach, even with an unconventional use of the dummy variable pre and post. I exploit the discontinuity on EPL at the 15 employees' threshold for Italian firms, established by the *Articolo 18* of the *Statuto dei lavoratori* (law n. 300/1970), that set different level of regulations for firms above the limit of 15 employees and below this limit. I also exploit a tax rate cut occurred in 2008 (the Ires tax) to fix a *pre* and a *post* period, to test the reaction of these group of firms in term of profitability and employment level to a decreasing in taxation. I find a negative and statistically significant effect of EPL on profitability, measured by Roe and Roa, and on the level of employment. Firms less constraints by EPL, below the limit of 15 employees, show a positive reaction to the tax rate cut if compared to the firms above the limit of 15 employees.

Key words: Local public spending interactions, spillovers, yardstick competition, spatial econometrics, dynamic panel data, natural disaster, internal and external instruments.

Political business cycles; European structural funds; local governments

Employment protection legislation, firms, profitability, employment

JEL codes: C23, H72 D72; H76 H32, D22, J48

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Chapter 1

Spatial interaction in local expenditures among Italian municipalities: evidence from Italy 2001-2011

Abstract

I estimate a spatial autoregressive dynamic panel data model, using information on 5,564 Italian municipalities over the period 2001-2011, exploiting their border contiguity as a measure of spatial neighborhood. I find a positive and statistically significant effect of neighboring expenditures on total, capital and current expenditures of a given municipality. These results are robust to the use of alternative weighting matrices for the definition of spatial neighbors and also when I employ a quasi-natural experimental approach, which exploits the exogenous variation in the neighbors' spending induced by the devastating earthquake that in 2009 hit municipalities belonging to the Abruzzo region. Moreover, I do not find any evidence of yardstick competition when I take into account political effects, while I do find a negative relationship between spatial interaction and the size of the municipality. Thus, I conclude that spillover effects drive the strategic interactions in spending decisions among Italian municipalities.

Key words: Local public spending interactions, spillovers, yardstick competition, spatial econometrics, dynamic panel data, natural disaster, internal and external instruments.

JEL codes: C23, H72

1.1 Introduction

Many studies in the last two decades aimed to assess the existence of spatial effects influencing local expenditure decisions. In particular, there is a line of works[1], both theoretical and empirical, that investigates whether governments make their spending decisions by taking into account the behavior of their neighbors. In such a framework, decisions on expenditures would depend not only on the traditional determinants of local spending, such as income, grants, socio-demographic and political characteristics of municipalities, but also on spending decision of neighboring municipalities. Indeed, if municipalities choose their expenditures/taxes – which can affect the welfare of their neighbor's – by maximizing their own welfare and so not taking into account their neighbor's welfare, they end up into inefficient levels of expenditure and/or taxes (Gordon, 1983).

The existence of strategic interactions between local governments is theoretically explained by several models, e.g. yardstick competition, tax and welfare competition, spillover effects and, more recently, political trend models. In the yardstick competition model, voters with no complete information on the cost of public goods and services compare expenditures and taxes in their jurisdiction with those of nearby jurisdictions (Salomon, 1987) and, hence, voters punish the incumbent politician if her tax rate decisions are not in line with those of neighbors. Starting from the seminal work of Besley and Case (1995) – who show that neighbors' tax rates impact on the probability of re-election for the incumbent in US states – a substantial body of literature has developed documenting and empirically testing yardstick competition (see, among others, Revelli, 2002a; Bordignon et al., 2003; Solé-Ollé, 2003; Allers and Elhrost, 2005; Padovano and Petrarca, 2014). The second source of spatial interdependence arises in tax competition models. Municipalities face mobile tax bases, which depend on both their own tax rate and their neighbors' tax rate giving rise to tax competition (Kanbur and Keen, 1993; Devereux et al., 2008; Rizzo, 2008). As far as the traditional "spillover" model is concerned, public expenditures of a municipality may have positive or negative effects beyond its own boundary, thus affecting the welfare of residents in neighboring municipalities. As a result, municipalities might decide the level of their own expenditure, by strategically taking into account the expenditures of their neighbors (Case et al 1993; Revelli, 2002b; Revelli, 2003; Baicker, 2005; Solé-Ollé, 2003; Werck et al., 2008; Costa et al., 2015). Finally, strategic interactions among local governments can be also explained by political interactions. This idea is based on the assumption that the local incumbent politician, taking into account the common ideology, makes her decisions on taxes and expenditure by looking only to those neighbors belonging to the same political party (Geys and Vermier, 2008; Santolini, 2009). Empirical findings support this hypothesis. In particular, Foucault et al., (2008), by using a panel data-set on French municipalities over

[1] For a theoretical survey on horizontal strategic interaction see, for example, Wilson (1999), while for an empirical survey on fiscal strategic interactions see, among others, Brueckner (2003), Revelli (2006) and, more recently, Delgado et al., (2014).

the period 1983-2002, show that spending interactions exist between municipalities that have the same political affiliation. The same results are confirmed for Spanish municipalities by Delgado et al., (2014), while for the Italian case, political ideology is a relevant determinant of fiscal interaction only for right-wing and centrist parties (Santolini, 2008)[2].

Most of the empirical literature estimates fiscal strategic interactions by considering the tax side of the local budget. Indeed, there are only few papers that focus explicitly on public expenditures (Case et al., 1993; Figlio et al., 1999; Baicker, 2005; Revelli 2002 and 2003, Foucault et al., 2008 and Costa et al., 2015) and, among these, only two works deal with the Italian framework (Ermini and Santolini, 2011; Bartolini and Santolini, 2012). However, these studies are conducted on sample of sub-national Italian jurisdictions (municipalities belonging to Marche region[3]) and they focus only on current expenditure, so that, at the best of my knowledge, no one has investigated strategic interactions in both current and capital expenditure decisions, by using a comprehensive dataset on Italian municipalities.

In this work, I aim to fill this gap by assessing the existence of spatial effects influencing the spending decisions of Italian municipalities and identifying the source of such interdependences. I use information on all Italian municipalities (except for those in autonomous regions) over the period 2001-2011. By employing the Arellano-Bond estimator; I estimate an empirical model where the public expenditure in a given municipality depends on the average of their own border municipalities' expenditures and on a set of control variables, including the lagged value of expenditure. I find a positive horizontal interdependence in spending decisions among Italian municipalities. However, some political variables turn out to be important determinants of local expenditure. In fact, the election year positively affects both total and capital expenditure, implying the presence of the political budget cycle among Italian municipalities. Moreover, the level of expenditure is higher among those municipalities where the mayor wins the election with a strong majority. Interestingly, I also find that the population size of the municipality negatively affects the impact of neighbors' expenditure on its own expenditure, such that, above a certain population level, the positive horizontal interdependence in the municipal expenditure vanishes. This last finding together with the no significant interactions with political variables (i.e., electoral and pre-electoral years, the political power of the mayor and mayors that, according to the Italian law, cannot be re-elected) let me argue that the strategic interaction is due to spillover effects and it is not driven by yardstick competition.

[2] The role of political ideology has been found to be an important driver also at the country level, as shown by Cassette and Exbrayat (2009), who conduct an analysis on 27 European countries over the period 1995-2007 finding that ideology on tax interactions holds only for contiguous countries.

[3] Ermini and Santolini (2011) found a positive and significant spillover effect for current expenditure, while, Bartolini and Santolini (2012) found evidence of yardstick competition, when they control for both the domestic stability pact and pre-electoral years.

The main contribution of this paper derives from the properties of the dataset. Since it includes all Italian municipalities for the period 2001-2011, it allows testing the existence of local spending interactions by also controlling for the persistency in local expenditures. Such a feature has been exploited only by few papers, including Foucault at al. (2008); Bartolini and Santolini (2012) and Costa et al. (2015)[4]. Moreover, this study is the first that investigate the source of interactions on capital expenditure for the Italian case. The local policy maker uses these investments as a way to attract economic activities, firms and households, and hence it is highly likely to observe strategic interactions between municipalities: if two municipalities are neighboring and one of them invests in roads, there is an incentive also for the other municipality to invest in roads, as the benefits from road usage are expected to be higher for the residents of the two municipalities if roads are provided on both jurisdictions than in the case in which only one jurisdiction provides good roads. Finally, I test the robustness of my results not only by using alternative weighting matrices - as it is common in the applied literature of spatial econometrics -, but also by employing a quasi-natural experimental approach. In particular, I focus on the period 2009-2010, since 49 municipalities of Abruzzo region were hit in year 2009 by a dreadful earthquake that caused economic losses of more than 14.7 billion euros. I thus build a dummy variable equals to one for municipalities hit by the earthquake, and then I convert it in a measure of the intensity of the earthquake by using the Mercalli-Carcani-Sieberge scale. The neighbors' value of this variable is then used to instrument the change in the average expenditure of neighboring municipalities from 2009 to 2010. The estimates of spatial interactions in municipal expenditure obtained in this experimental context confirm those obtained by relying on internal instruments (using the same sample of municipalities belonging to Abruzzo region), and so pointing to the existence of interactions in public expenditure among local governments.

The rest of paper is organized as follows: Section 1.2 illustrates the institutional framework; Section 1.3 discusses the econometric strategy and Section 1.4 describes the data. Section 1.5 presents the main results, while robustness tests are shown in Section 1.6. Then, in Section 1.7, I investigate more in depth the source of spatial interaction, by testing yardstick competition and spillover hypotheses. Finally, Section 1.8 concludes.

[4] It is worth noting that the dataset includes 61,204 observations, making it the largest sample ever examined in applied work on strategic interactions in spending decisions at the local level. In fact, among those papers analyzing the existence of interactions related to public expenditure, Foucault at al., (2008) use a panel dataset of 90 French municipalities over the period 1983-2002, leading to 1,710 observations; Bartolini and Santolini (2011) rely on a panel dataset of 246 Italian municipalities of Marche region during the period 1994-2003, for a total of 2,460 observations and Costa et al., (2015) use a panel dataset of 278 Portuguese municipalities for the period 1986-2006, summing up to 5,560 observations.

1.2 Institutional framework: a brief analysis of Italian municipalities' spending

The Italian Constitution defines four administrative government layers: central government, regions, provinces and municipalities. While most regions and provinces are ruled by ordinary statutes, some of them – the autonomous regions and provinces – are ruled by special statutes[5]. Furthermore, Italy counts 110 provinces, that have recently been reformed by the law 56/2014, which reduced their public competences and eliminated the possibility of direct elections of their own representatives. Finally, municipalities are the smallest level of jurisdiction and are around 8,000, although this number is decreasing because the law 56/2014 is incentivizing amalgamation. Most municipalities (around 90%) have less than 15,000 inhabitants and the average size is around 6,400 inhabitants.

Municipalities in Italy are responsible for several public functions, such as social welfare services, territorial development, local transport, infant school education, sports and cultural facilities, local police services, water delivery, rubbish as well as most infrastructural spending. According to the data, municipalities' total expenditure accounts, on average, for about 8.7% of all total public expenditure in Italy during the period 2001-2011.

Municipalities' current expenditure, on average, accounts for 71% of the municipalities' total expenditure, which corresponds to 63 billion of euros per year during 2001-2011. Among current expenditure, approximately 75% is concentrated on four main functions: *Administration and Management*, *Roads & Transport Services*, *Planning and Environment* and *Social welfare*. The remaining 25% of the current expenditure is allocated to the *Municipal police*, *Education*, *Culture*, *Sport, and Tourism*. Finally, a very low amount of resources goes to three functions, *Economic development*, *In-house production services* and *Justice*, managed by many medium-sized and small municipalities networking with other municipalities.

Municipalities are also responsible for investments, which are on average 29% of the total expenditure in the period 2001-2011. However, it is worth noting that the share of these expenditures sharply decreased in the period 2006-2011, switching from 34% to 21% of total expenditures. At the same time, the share of current expenditure has increased. Looking at the specific functions, municipalities allocate resources for investments mainly to *Administration and Management* (16.7% of the capital expenditure) *Roads and Transport Services* (26%), *Planning and Environment* (27.5%) and *Education* (9%).

[5] In Italy there are five autonomous regions (Sicilia and Sardegna, which are insular territories, and Valle d'Aosta, Trentino Alto Adige and Friuli Venezia Giulia, which are northern boundary territories) and two autonomous provinces (Trento and Bolzano).

1.3 Empirical framework

The econometric strategy is based on the estimation of a spatial autoregressive dynamic panel model (Anselin et al., 2008), which takes the following form:

$$G_{it} = \alpha + \beta G_{(it-1)} + \gamma WG_{-it} + \rho X_{it} + \mu_i + \tau_t + \varepsilon_{it}, \qquad (1)$$

where G_{it} is the per capita expenditure of municipality *i* in year *t*, and $G_{(it-1)}$ is its one year lagged value.

$WG_{-i,t} = \sum_{j \neq i} \omega_{ij} G_{jt}$ is the weighted per capita average expenditure of the neighboring municipalities *j* at time *t*; ω_{ij} are exogenously chosen weights that aggregate the per capita expenditure of neighboring municipalities into a single variable $WG_{-i,t}$. The ω_{ij} are normalized so that $\sum_{j \neq i} \omega_{ij} = 1$. X_{it} is a matrix of demographic, socio-economic and political characteristics of municipality *i* at time *t*, and it also includes per capita transfers (current, capital or total grants, according to the dependent variable adopted in the estimation) from upper tiers of governments ($transfers_{it}$).[6] μ_i is an unobserved municipal specific effect, τ_t is a year specific intercept and ε_{it} is a mean zero, normally distributed random error.

In equation (1), the coefficient β measures the degree of inertia of the municipal expenditure, whereas the coefficient γ captures the horizontal interdependence in the municipal expenditure, that is the reaction of the expenditure of a given municipality to a one-euro increase in the average expenditure of its neighbors. The interpretation of the coefficient γ, as capturing the horizontal interdependence in the municipal expenditure, is very common in the literature (see, among others, Foucault et al., 2008; and Costa et al., 2015, who explicitly interpret it as a spillover effect). As far as the spillover effect is concerned, there are three possible cases, which are related to the degree of complementarity and substitutability in the provision of public goods and/or services:

i) $\gamma = 0$: no horizontal interdependence, namely municipalities do not imitate each other in setting local public spending.

ii) $\gamma < 0$: negative horizontal interdependence, that is a one-euro increase in the average expenditure of neighboring municipalities leads to a reduction in the municipal expenditure. This case holds when public goods/services provided by neighbors' municipalities are substitutes of the municipality's own goods/services. For example, two swimming pools, one located in each municipality, are likely to be substitutes and, hence, there is no incentive for a given municipality to increase its expenditure as a response to an increase in neighbors' expenditure.

iii) $\gamma > 0$: positive horizontal interdependence, that is a one-euro increase in the average expenditure of neighboring municipalities leads to an increase in the

[6] In the years 2008-2011 we subtract the compensative transfer from the central state that has been introduced to replace the missing revenue from the abolished property tax on owner-occupied dwellings.

municipal expenditure. This case holds when public goods/services provided by neighbors' municipalities are complements of the municipality's own goods/services. For example, road services provided by the two municipalities are likely to be complements and, hence, there might be an incentive for a given municipality to increase its expenditure as a response to an increase in neighbors' expenditure.

Since equation (1) includes endogenous variables, the OLS estimation is inappropriate as it generates biased estimates. The average neighboring expenditure, WG_{-it}, is endogenous because expenditure interactions are symmetric and simultaneous: each municipalities' behavior affects that of its neighbors and it is affected by their behavior in the same way. The lagged dependent variable, $G_{(it-1)}$, which is an important determinant of the municipal expenditure (Veiga and Veiga, 2007; Larcinese et al., 2013), is correlated with the municipality fixed effects in the error term, leading to biased and inconsistent fixed effects estimations (Nickell, 1981). The variable $transfers_{it}$ is also endogenous, as simultaneously decided with municipalities' expenditures. Thus, I use the system GMM (SYS-GMM) dynamic panel estimator (Arellano and Bover,1995; Blundell and Bond, 1998).

This estimator is an augmented version of the difference GMM (Arellano and Bond, 1991) and, hence, is considered more efficient with respect to the difference GMM (Blundell and Bond, 1998). The SYS-GMM, differently from the difference GMM which just employs the difference equation, builds a stacked dataset, one in levels and one in differences. Then the differences equations are instrumented with levels, while the levels equations are instrumented with differences[7].

The consistency of the GMM estimator depends on the assumption that the error term is serially uncorrelated, otherwise the instruments are not valid. Hence, to check for the absence of first-order serial correlation in levels in a dataset expressed in differences, as that used in a SYS-GMM, I need to check for the absence of second order correlation in differences. In fact, I am able to detect first order serial correlation in level between ε_{it-1} and ε_{it-2} by looking at the correlation between $\Delta\varepsilon_{it}$ ($\Delta\varepsilon_{it} = \varepsilon_{it} - \varepsilon_{it-1}$) and $\Delta\varepsilon_{it-2}$ ($\Delta\varepsilon_{it-2} = \varepsilon_{it-2} - \varepsilon_{it-3}$). For this reason, I test, using the differenced estimating equation, for first order autoregressive (AR(1)) serial correlation in the residuals, which I expect negative and significant[8] and for second order autoregressive (AR(2)) serial

[7] In terms of equation (1) we take the first difference, then the term G_{it-1} in ΔG_{it-1} ($\Delta G_{it-1} = G_{it-1} - G_{it-2}$) is correlated with the term ε_{it-1} in $\Delta\varepsilon_{it}$ ($\Delta\varepsilon_{it} = \varepsilon_{it} - \varepsilon_{it-1}$), so the choice of G_{it-1} as instrument would bias the estimates. As a results, for the equation in differences, we may use lagged values of G_{it} to form instruments as long as G_{it} is lagged two periods or more (G_{it-2}, G_{it-3} ,…). As concerns the level equations, the lagged endogenous variables (G_{it-1}) can be instrumented with ΔG_{it-1} since it is not correlated with ε_{it}. The same approach is followed for the other 2 endogenous variables, in particular ΔWG_{-it} is instrumented with two (or more) periods lags (WG_{-it-2}, WG_{-it-3} ,…) and $\Delta transfers_{it}$ is instrumented with two (or more) periods lags ($transfers_{it-2}$, $transfers_{it-3}$,…).

[8] Since $\Delta\varepsilon_{it}$ is analytically related to $\Delta\varepsilon_{it-1}$ via the term ε_{it-1}, a negative first-order serial correlation is always expected in differences. In fact, E($\Delta\varepsilon_{it}$, $\Delta\varepsilon_{it-1}$)=E($\varepsilon_{it} - \varepsilon_{it-1}$) E ($\varepsilon_{it-1} - \varepsilon_{it-2}$) = -Var$\varepsilon_{it-1}$.

correlation in the residuals, which I expect not significant (Arellano and Bond, 1991), where in both tests the null hypothesis is the absence of serial correlation in the residuals.[9]

In order to check the validity of the instruments, I employ the standard Hansen test whose null hypothesis is the exogeneity of the corresponding instrument (or group of instruments). However, as Roodman (2009) points out, the power of the Hansen test might be weakened if the number of instruments is high. Consequently, I test the validity of a subset of instruments by using a C-test (Baum, 2006). The C-test estimates the SYS-GMM with and without a subset of instruments and uses the difference between the two respective Hansen tests distributed as a *chi2* and, allowing to test the null hypothesis that the excluded instruments are valid, namely they are exogenous.

The SYS-GMM requires an additional assumption with respect to the difference GMM: the first differenced instruments for the level equations must be not correlated with the fixed effects. For this reason, I apply the C-test to the level equation and so comparing the Hansen test of this last equation with that of the SYS-GMM. The null hypothesis is that the instruments (which are taken in difference) for the level equations are valid and so the SYS-GMM is preferred to the difference GMM.

Finally, I use a two-step SYS-GMM, which makes the covariance matrix more robust to panel specific autocorrelation and heteroskedasticity, so the estimator is more efficient (Arellano and Bond, 1991; Blundell and Bond 1998). However, by using this procedure the standard errors might be severely downward biased (Roodman, 2009), hence, in order to correct the bias, I apply the correction made by Windmeijer (2005).

1.4 Data

The data on Italian municipalities used in my work result from a combination of different archives provided by the Italian Ministry of Internal Affairs, the Ministry of Economy and the Institute of National Statistics.

The data include a full range of information for the period 2001-2011 and are organized into two sections: 1) municipality financial data and 2) municipality demographic, socio-economic and electoral data, such as population size, age structure, average income of inhabitants, election years. I restrict the sample to municipalities located in ordinary statute regions. I exclude municipalities that have a specific status of metropolitan areas (law 56/2014)[10], because they usually provide a wider range of services compared to other municipalities. The final sample includes 5,564 municipalities[11], observed from 2001 to 2011, which generates a balanced panel data set of 61,204 observations. It is worth noting that all financial variables are expressed in 2011 real per capita value.

The Italian municipality balance sheet reports expenditures either in accrual basis or in cash basis. In this system of public accountability there is usually a gap (exceeding,

[9] In fact E($\Delta\varepsilon_{it}$, $\Delta\varepsilon_{it-2}$)=E($\varepsilon_{it} - \varepsilon_{it-1}$) E ($\varepsilon_{it-2} - \varepsilon_{it-3}$) =0.
[10] Milano, Roma, Napoli, Torino, Bari, Firenze, Bologna, Genova, Venezia and Reggio Calabria.
[11] We did not consider municipalities with missing values in the dependent variables defined at section 1.4.l

sometimes, more than one financial year) between the payment (registered on cash basis) and the commitment to it (registered on accrual basis). For this reason, I prefer to use the cash basis classification, since the value is reported only if the payment has effectively been made.

1.4.1 Dependent variables and variables of interest.

I estimate equation (1) using three different dependent variables: the per capita total expenditure (*total expenditure*), the per capita current expenditure (*current expenditure*) and, the per capita capital expenditure (*capital expenditure*). I use these aggregate measures of expenditure and not those disaggregated by functions, because many municipalities (especially the small ones) have expenditure crossing more than one function, but often registered only in one function.

To isolate the independent impact of neighboring expenditures on the expenditure of a given municipality, I use the neighbors' expenditures variable (*neigh expenditure*). In order to obtain this variable, as mentioned in Section 1.3, I use a contiguity matrix, implying $\omega_{ij} = 1/m_i$ where m_i is the number of municipalities contiguous to *i* and $\omega_{ij} = 0$ otherwise. Hence, for each municipality *i* in period *t*, the average value of its own neighbors' per capita expenditure is given by $WG_{-i,t} = \sum_{j \neq i} \omega_{ij} G_{jt}$.

1.4.2 Control variables

The municipality expenditure can be affected by other factors, accounting for demographic, socio-economic and electoral characteristics. In particular, I include a set of time-varying variables, which characterizes the municipality's demographic and economic situation. I include the municipal population (*population*10^{-4}*) and per capita area (*area*10^{3}*) - square kilometers divided by population – as these variables can capture the presence of scale economies and/or congestion effects. The proportion of citizens aged between 0 and 5 (*children*10^{3}*) and the proportion of citizens aged over 65 (*aged*10^{3}*) can control for some specific public needs (e.g., nursery school, nursing homes for the elderly) and hence may influence the composition of public spending.

In terms of economic and financial controls, I include the per capita personal income tax base (*income*10^{-3}*), i.e. a proxy of per capita average income and, per capita transfers (current, capital or total grants) from upper tiers of governments (*transfers*), that vary according to the dependent variable adopted in the estimation.[12] These variables should have a positive impact on expenditure. On the one side, higher levels of local expenditure

[12] Transfers from upper level of government represent a significant part, appoximatley around 25%, of the Italian municipal financing system. There is a well-known literature on the effects of grants on public expenditure (see, among others, Gramlich,1977; Hines and Thaler, 1995; Gamkhar and Shaw, 2007 and Inman, 2009) usually finding that grants can stimulate government expenditures more than monetary transfers to individuals of the same amount—the so-called flypaper effect, whereby a quota of the federal money sticks to the public sector instead of being distributed to citizens.

might be associate with high level of local economic development (proxied by the per-capita personal income tax base) and, on the other side, an increase in the municipal revenue (proxied by transfers) should lead to an increase in expenditure.

Furthermore, following the literature (Bordignon et al, 2003; Foucault at al., 2008; Bartolini and Santolini, 2012) I use a set of political variables that may influence the local budget. In particular, I define a dummy variable (*election*), which, during the period 2001-2011, is equal to 1 for a given municipality in the year of election. The coefficient of this variable is expected to be positive as the incumbent might have an incentive to expand the expenditure during the election period in order to be re-elected. I then measure the political power of the mayor by using the percentage of votes that have been necessary to win an election (*vote-share*): the stronger the power of the local policy maker, the grater is her capacity to influence the budget. Since Italian law establishes a limit of no more than two consecutive terms in office for a mayor, I use a dummy variable (*term-limit*) which is equal to 1 for all the years a mayor is at her second term (and hence she cannot be re-elected) and it is equal to 0 when the mayor is at her first term: the impossibility of further re-election may significantly bias the budget-related decisions of a municipality.

Since 2001[13], the Italian central government – in order to fulfill the obligations of the European Stability and Growth Pact – imposes to each municipality above 5,000 inhabitants the so-called Domestic Stability Pact. Depending on the year, it implies either a constrained municipal deficit or a threshold on the municipal expenditure. Hence, I include a dummy (*domestic stability pact*) equal to one if a municipality has to fulfill the Domestic Stability Pact (i.e. it has more than 5,000 inhabitants) and 0 otherwise: this variable should lead to lower level of expenditure. The summary statistics of all the variables used in the analysis are reported in Table 1.

As discussed in section 1.3, the dynamic model I estimate includes the lagged endogenous dependent variable, $G_{(it-1)}$ and two further endogenous variables, namely the average neighboring expenditure, $\mathrm{WG}_{-\mathrm{it}}$, and per capita transfers (current, capital or total grants) from upper tiers of governments ($transfers_{it}$). Therefore, all these variables are instrumented by using their lags[14].

[13] See law 388/2000, article 53.
[14] Information about lags used for instrumenting all endogenous variables are provided for each estimated specification.

Table 1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Total expenditure	61,204	1251.95	1101.98	30.17	43906.23
WTotal expenditure	61,204	1226.65	719.84	0.00	15280.65
Current expenditure	61,204	742.04	390.06	5.63	13023.92
WCurrent expenditure	61,204	734.71	264.41	0.00	5750.50
Capital expenditure	61,204	509.91	851.44	0.00	42127.01
WCapital expenditure	61,204	491.94	532.12	0.00	14174.68
Total transfers	61,204	620.43	834.36	7.14	33814.22
Current transfers	61,204	277.61	234.39	1.10	14177.54
Capital transfers	61,204	342.82	714.18	0.00	32906.61
Population$*10^{-4}$	61,204	0.65	1.40	0.00	26.54
Children$*10^{3}$	61,204	51.17	13.32	0.00	126.58
Aged$*10^{3}$	61,204	220.53	61.43	40.93	634.78
Area$*10^{3}$	61,204	18.53	44.19	0.08	1148.94
Income$*10^{-3}$	61,204	10.93	3.68	0.21	196.58
Domestic stability pact	61,204	0.31	0.46	0.00	1.00
Election	61,204	0.20	0.40	0.00	1.00
Term-limit	61,204	0.38	0.49	0.00	1.00
Vote-share	61,204	0.59	0.16	0.16	1.00
Pre-election	61,204	0.19	0.39	0.00	1.00
Total expenditure($\Delta_{2010\text{-}2009}$)	195	112.326	637.775	-1372.652	4026.659
WTotal expenditure($\Delta_{2010\text{-}2009}$)	195	125.448	547.080	-998.671	3281.798
WTotal expenditure($\Delta_{2009\text{-}2008}$)	195	58.578	249.200	-1220.460	1209.863
Current expenditure($\Delta_{2010\text{-}2009}$)	195	58.987	281.617	-321.453	2084.869
WCurrent expenditure($\Delta_{2010\text{-}2009}$)	195	91.297	333.790	-305.833	2084.869
WCurrent expenditure($\Delta_{2009\text{-}2008}$)	195	36.257	144.764	-96.688	964.193
Capital expenditure($\Delta_{2010\text{-}2009}$)	195	53.339	465.387	-1692.400	2822.363
WCapital expenditure($\Delta_{2010\text{-}2009}$)	195	34.151	286.161	-1144.008	1559.060
WCapital expenditure($\Delta_{2009\text{-}2008}$)	195	22.322	203.728	-1533.329	665.972
Earthquake	195	0.113	0.317	0.000	1.000
Earthquake intensity	195	0.703	1.983	0.000	8.000
Population($\Delta_{2010\text{-}2009}$)	195	-156.892	651.587	-6231.000	258.000
Children($\Delta_{2010\text{-}2009}$)	195	0.000	0.004	-0.012	0.014
Aged($\Delta_{2010\text{-}2009}$)	195	0.002	0.007	-0.029	0.030
Area($\Delta_{2010\text{-}2009}$)	195	0.001	0.003	-0.001	0.022
Income($\Delta_{2010\text{-}2009}$)	195	-12.715	262.007	-848.711	913.008
Total transfers ($\Delta_{2010\text{-}2009}$)	195	-22.187	551.906	-3086.409	3269.272
Current transfers ($\Delta_{2010\text{-}2009}$)	195	82.914	389.003	-607.956	2747.905
Capital transfers ($\Delta_{2010\text{-}2009}$)	195	-105.102	614.373	-4853.103	1718.377
WEarthquake intensity	195	0.726	1.691	0.000	8.000

Notes: period 2001-2011. The financial variables are in real, per capita and cash flows terms. *Children, aged, area and income* are divided by *population*. The spatial matrix used to compute the neighbors' variables is binary, contiguity-based, by which two municipalities are neighbors if they share a border, and it is row-standardized.

1.5 Results

I first estimate equation (1) by using the OLS estimator (Table 2, col. 1), then I replicate the previous estimation by applying the FE estimator (Table 2, col. 2) and, finally, I perform the SYS-GMM estimator (Table 2, col. 3).

The coefficient of the lagged dependent variable is found to be positive and always significant in all specifications, and thus suggesting a certain degree of inertia of public expenditure. In particular, the estimated coefficient of *expenditure (-1)* ranges between 0.25 and 0.50. These values are in line to the findings of Veiga and Veiga (2007) and Foucault et al., (2008), but, however, are slightly lower with respect to those found by Bartolini and Santolini (2012) for Marche region.

Turning to the results associated with the presence of spending interactions, I find that the coefficient of *neigh expenditure* is always positive and significant in all specifications and so pointing to the existence of a positive horizontal interdependence in the expenditure of Italian municipalities, that is public goods/services provided by neighbors' municipalities are substitutes of the municipality's own goods/services provision. In particular, by using the estimated coefficient of the SYS-GMM (Table 2, col. 3), I found that a one-euro increase in the average expenditure of the neighbors generates, *ceteris paribus*, an increase in the expenditure of municipality *i* of 0.16 euro.

Looking at the other control variables (Table 2, col, 3), I find that the coefficients have the expected sign. In particular, by considering the preferred specification (SYS-GMM), the coefficients of both *transfers* and *income*10*$^{-3}$ are positive (0.38 and 17.93, respectively) and signficant, implying that total expenditure incrases as income and also grants increase. Municipalities' geographic and demographic characteristics have also an effect on total expenditure. The positive coefficient of *area*10*^{3} (4.31 and significant at 1%) suggests the presence of economies of scale, since the lower the municpal area per capita the higher the level of expendiutue; while the postive cofficient found for *population*10*$^{-4}$ (11.58 and statsictally significant at 1%) accounts for the presence of congestion effects. Moreovoer, the municipal spending decreases as the proportion of children increase, being the coeffcient of *children*10*^{3} negative (-2.02) and significant.

All the specifications include political and institutional variables as well. Focusing on the SYS-GMM, the dummy variable *election* has a positive and significant coefficient (20.24), implying the presence of the political budget cycle, as the incumbent mayor has an incentive to expand the expenditure in order to be re-elected. In addition, an higher level of expenditure is associated with high value of *vote-share* (75.86), suggesting that mayors supported by a large counsensus have more power to influence the local budget.

Finally, the dummy variable *domestic stability pact* shows a negative and significant coefficient (-38.08) and confirming recent findings (Grembi et al., 2016[15]) on the effectiveness of the Domestic Stability Pact in constraining local expenditures.

Table 2: Estimation results for total expenditure with OLS, FE and SYS-GMM estimator

Dependent variable	Total Expenditure		
Model	OLS	FE	SYS-GMM
	(1)	(2)	(3)
Expenditure (-1)	0.50***	0.25***	0.31***
	(0.03)	(0.03)	(0.06)
Neigh expenditure	0.08***	0.11***	0.16*
	(0.01)	(0.02)	(0.10)
Transfers	0.54***	0.60***	0.38***
	(0.03)	(0.04)	(0.10)
Population*10^{-4}	6.09***	-189.54***	11.58***
	(0.85)	(38.80)	(1.79)
Children*10^{3}	-1.41***	-1.07	-2.02***
	(0.41)	(0.77)	(0.52)
Aged*10^{3}	-0.31***	0.10	0.23
	(0.11)	(0.30)	(0.24)
Area*10^{3}	1.47***	8.64***	4.31***
	(0.31)	(3.28)	(0.95)
Income*10^{-3}	21.45***	33.30***	17.93***
	(1.29)	(8.23)	(3.86)
Domestic stability pact	-6.23	-73.39**	-38.08***
	(4.08)	(29.09)	(11.04)
Election	11.66**	22.13***	20.24***
	(4.85)	(3.92)	(4.87)
Term-limit	3.11	7.15*	3.03
	(3.99)	(4.14)	(3.96)
Vote-share	4.32	21.43	75.86***
	(15.82)	(20.73)	(28.11)
Constant	104.70***	17.20	181.99**
	(36.37)	(150.67)	(73.11)
Observations	55,640	55,640	55,640
R-squared	0.84	0.46	
Number of municipalities	5,564	5,564	5,564
ar1p			0.000
hansenp			0.497
ar2p			0.727
Number of instruments			29

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors, clustered at the municipal level, are shown in parentheses. In all regressions I control for time fixed effects, while, in col. (2) and (3) I also include municipal fixed effects. In col. (3) the variable *Expenditure (-1)* is instrumented applying difference GMM, by using lags 1, 2 and 3; the variable *neigh expenditure* is instrumented applying SYS-GMM, by using lags 3 and 4; the variable *transfers* (total transfers) is instrumented applying SYS-GMM by using lags 3 and 4. The validity of the instruments is checked by using the standard Hansen test and the C test (results are available upon request).

[15] This result should be read with some warning. In fact, the variable *domestic stability pact* (which is 0 if population is lower than 5,000 inhabitants and 1 otherwise) also accounts for other municipal rules. For example the mayor's salary and the amount of transfers received from the central government change if the municipality is above the threshold of 5,000 inhabitants.

In Table 3 I report the results of the estimations using as dependent variable the two components of total expenditures: current expenditure (col. 1, 2 and 3) and capital expenditure (col. 4, 5 and 6). I apply OLS, FE and SYS-GMM estimators. In the latter case, as before, I instrument the lagged dependent variable and the other endogenous variables (*neigh expenditure* and *transfers*) with their lags.

As for the current expenditure, following the estimates of the SYS-GMM, my preferred specification, (Table 3, col. 3), I find a certain degree of persistency in the expenditure, but weakly significant and more modest (0.11) than the one estimated for the total expenditure. The estimated coefficient associated with current expenditure of neighboring muncipalities (*neigh expenditure*) is positive (0.65), statistically significant at 1% and larger with respect to the one estimated for total expenditure. Such a postive effect suggests that the interaction in current spending decision at the local level is driven by public goods and/or services that are of the complement type.

Moving to capital expenditure, the results - following the estimations of the SYS-GMM (Table 3, col. 6) - show that the estimated coefficient of the lagged dependent variable is positive (0.31), statistically significant at 1%, and very similar to the one estimated for total expenditure (Table 2, col. 3) indicating that capital expenditure at the municipal level in Italy is likely to change slowly over time. The coefficient of capital expenditure of neighboring muncipalities (*neigh expenditure*) is positive (0.10), signficant at 5%, and lower than the one estimated for total expenditure. The positive coefficient associated to the neighboring expenditure reveals that spatial interactions on capital expenditure at the local level are driven by those investments that are complements in usage. Control variables are also very informative about the determinants of both current and capital municipal expenditure. In particular, the coefficient associated with $population*10^{-4}$ is positive and significant and thus confirming the presence of congestion effects; the coefficient of per-capita area ($area*10^{3}$) is positive and statistically significant, implying the presence of economies of scale, and the coefficient of *vote-share* is positive and significant as I found for the total expenditure. On the contrary, the variable *domestic stability pact*, which captures financial constraints imposed by the central government to municipalities, is negative and significant both for current and capital expenditure. Moreover, for the specific case of current expenditure, the coefficient of $aged*10^{3}$ turns out to be positive and significant, indicating that an higher share of elderly people is associated with higher level of current expenditure. On the side of capital expenditure, instead, the coefficients of *transfers* (0.47), $income*10^{-3}$ (2.59) and *election* (-33.89) play an important role in explaining investment decisions at the municipal level.

Thus far, the empirical evidence leads to three main findings that can be summarized as follows. Firstly, local expenditure of Italian municipalities turn out to be persistent - especially for the case of capital expenditure - and such a result is in line with the evidence found in other countries, such as France (Foucault et al., 2008) and Portugal (Veiga and Veiga, 2007; Costa et al., 2015). Moreover, the results show the presence of a positive horizontal interdependence in spending decision among Italian municipalities, with the effect being more pronounced for current expenditure: a one-euro increase in the average current expenditure of the neighbors generates, *ceteris paribus*, an increase of 0.65 euro

in the municipality's current expenditure; whereas, a one-euro increase in the average capital expenditures of the neighbors generates, *ceteris paribus*, an increase of 0.10 euro in municipality's capital expenditure. While the presence of horizontal interactions in current spending decisions is a well known result for Italian municipalities (Ermini and Santolini, 2011; Bartolini and Santolini, 2012), the findings of a positve interaction in capital expenditure – and thus of a complementarity relationship in the provision of local public goods - represents a novel result for the Italian case. Finally, political variables are important factors of municipal expenditure. In particular, the power of the mayor, in terms of political consensus, leads to higher expenditure – both current and capital – while being in an electoral year positively impacts only on capital expenditure, as spending on infrastructures is usually seen as the most visible expenditure (Drazen and Eslava, 2010).

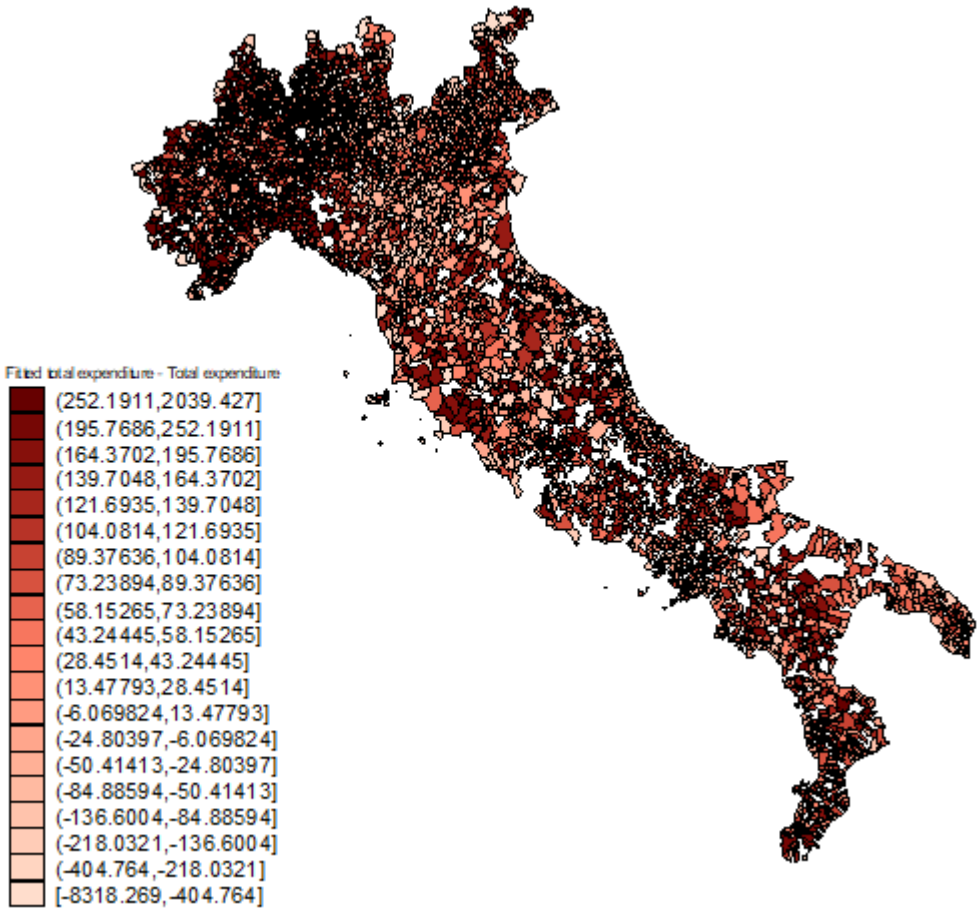
I present the results obtained in the analysis through a colored map in order to show how the spatial effect is distributed across Italian municipalities. Looking at the Figure 1, I notice that the spatial effect is higher in the Northern-east Italy, where there is a high concentration of small municipality in term of land area. This intuition, coming from the map, has been detected in the following analysis, described in the section 1.7.2: the relationship between the spatial effect and the municipality's dimension.

Table 3: Estimation results for current and capital expenditures with the SYS-GMM estimator

Dependent variable	Current Expenditure			Capital Expenditure		
Model	OLS	FE	SYS-GMM	OLS	FE	SYS-GMM
	(1)	(2)	(3)	(4)	(5)	(6)
Expenditure (-1)	0.92***	0.41***	0.11*	0.37***	0.24***	0.31***
	(0.01)	(0.02)	(0.06)	(0.03)	(0.03)	(0.05)
Neigh expenditure	0.06***	0.27***	0.65***	0.06***	0.10***	0.10**
	(0.01)	(0.03)	(0.18)	(0.01)	(0.02)	(0.04)
Transfers	0.10***	0.15***	-0.33	0.61***	0.60***	0.47***
	(0.02)	(0.04)	(0.22)	(0.04)	(0.04)	(0.09)
Population*10^{-4}	1.33***	-94.36***	22.70***	2.51***	14.79	2.27***
	(0.42)	(17.87)	(3.33)	(0.55)	(20.13)	(0.84)
Children*10^{3}	-0.28***	-0.42***	-1.31***	-0.55	-0.30	-0.68*
	(0.08)	(0.16)	(0.46)	(0.38)	(0.68)	(0.37)
Aged*10^{3}	-0.05*	-0.07	0.62**	-0.10	0.48*	0.07
	(0.03)	(0.08)	(0.29)	(0.10)	(0.28)	(0.09)
Area*10^{3}	0.26***	4.88***	2.49***	0.85***	2.97	2.09***
	(0.06)	(0.80)	(0.69)	(0.25)	(2.74)	(0.36)
Income*10^{-3}	2.48***	9.59***	-1.20	3.65***	18.34**	2.59***
	(0.60)	(1.69)	(5.46)	(0.56)	(7.67)	(0.74)
Domestic stability pact	-2.71*	-7.11**	-43.26***	-23.07***	-55.88*	-33.89***
	(1.54)	(3.55)	(15.38)	(3.20)	(29.50)	(4.68)
Election	-1.40	-0.46	-0.52	15.03***	21.27***	17.60***
	(1.36)	(1.16)	(1.90)	(4.22)	(3.69)	(4.07)
Term-limit	-0.22	0.57	-2.62	4.48	7.16*	3.40
	(1.09)	(1.09)	(2.60)	(3.46)	(3.92)	(3.23)
Vote-share	4.34	8.77*	56.46***	21.82	16.44	51.61***
	(3.87)	(4.88)	(19.54)	(13.95)	(20.07)	(18.02)
Constant	-11.48	65.80	120.06	86.98***	-199.46	94.16***
	(10.02)	(40.29)	(109.93)	(32.19)	(127.26)	(33.19)
Observations	55,640	55,640	55,640	55,640	55,640	55,640
R-squared	0.92	0.47		0.78	0.46	
Number of municipalities	5,564	5,564	5,564	5,564	5,564	5,564
ar1p			0.000			0.000
Hansenp			0.259			0.795
ar2p			0.581			0.769
Number of instruments			28			28

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors, clustered at the municipal level, are shown in parentheses. In all regressions I control for time fixed effects, while, in col. (2), (3), (5) and (6) I also include municipal fixed effects. In col. (3) the variable *Expenditure (-1)* is instrumented applying difference GMM, by using lags 1, 2, 3 and 4; the variable *neigh expenditure* is instrumented applying SYS-GMM by using lags 7 and 8; the variable *transfers* (current transfers) is instrumented applying SYS-GMM by using lag 4. In col. (6) the variable *Expenditure (-1)* is instrumented applying difference GMM by using lags 1 and 2; the variable *neigh expenditure* is instrumented applying SYS-GMM by using lags 2 and 3; the variable *transfers* (capital transfers) is instrumented applying SYS-GMM by using lags 3 and 4. The validity of the instruments is checked by using the standard Hansen test and the C tests (results are available upon request).

Figure 1: Maps of spatial interaction effect through Italian municipalities



Notes: the effect is calculated as a mean for the period 2001-2011 by municipality, and as difference between the value of the expenditure of municipality *i* and the fitted value of the expenditure deriving from the regression of table 2, col. 3 (the stata command used for estimating fitted values is *predict x, xb*).

1.6 Robustness tests

In order to cofirm the results found in the previous Section, I run two set of robustness test. Firstly, I replicated regressions by using alternatve wighting matrices. Then, I employ a quasi-natural experiment, which consists of comparing regression results obtained by relying on an external instrument to the results obtained by using an internal instrument.

1.6.1 Different weighting matrices

As a first set of robustness test, I re-estimate the previous models by using different neighbor's matrices. In particular, I define this new neighbors' variable (*neigh expenditure*) by using three weighted matrices. We, first, consider neighbors all municipalities distant up to 25 km from a given municipality and I weigh the corresponding expenditure with the inverse of that distance; above 25 km the weight is 0. Then, by using the same procedure, I classify as neighbors all municipalities whose distance from a given municipality is no more than 50 km and, finally, I use a broader defintion of neighbors, namely I define neighbors all municiplaities whose distance is within 100 km.

I perform the estimations for the total expenditure and its two components (current expenditure and capital expenditure) using the three spatial matrices, separately. The estimates obtained using a neighbor's distance less than 25 km confirm the results I have obtained in the previous analysis. The strategic interaction between expenditures persists for each type of expenditure. The coefficient of neighboring expenditure (*neigh expenditure*) is 0.22 and significant at 10% when using total expenditure (Table 4, col. 1), and it is very similar (0.20) for capital expenditure (Table 4, col. 3); however it becomes larger and increases up to 0.77 with current expenditure (Table 4, col. 2).

When I use a neighbor's distance up to 50 km, the estimates confirm again my previous results for all types of expenditure In particular, when I use total expenditure (Table 4.col. 4) the coefficient of neighbor's expenditure (*neigh expenditure*) is 0.34 and statistically significant at 1%. In the other two cases, the neighboring expenditure coefficient takes on the value of 0.88 (at 1% significance) for current expenditure (Table 4, col. 5) and of 0.21 for capital expenditure (Table 4, col. 6).

Finally, when I allow for a wider defintion of neighborhood (100km) the spillover effect vanishes, in fact the coefficient of neighbors expenditure is not stastically significant for total expenditure (Table 4, col. 7), neither for current expenditure (Table 4, col. 8), nor for capital expenditure (Table 4, col. 9).

It is worth noting that although the estimated coefficients are found to be larger as the distance increases[16], they are not stastically different between each others, leading us to

[16] Similar results are found by Costa et al. (2015), who justify the increase in the size of the estimated coefficient by saying that "when allowing for a broader definition of neighborhood, a higher effect of neighbors' expenditure is captured" (pag. 1451). We then

conclude that the spillover effects is not statistically sensitive to the definition of "neighborhood". In fact, for each of the weighting matrix adopted (25, 50 and 100 km neighborhood) and for the dependent variables (total, current and capital expenditure), I plot the estimated coefficients of the variable *neigh expenditure* as of Table 4, and its confidence interval at the 10% significance level (Figure A1, Appendix).

As it regards current expenditure, I find that the *neigh expenditure* coefficient (0.77, and statistically significant at 1%) obtained by using the definition of 25 km neighborhood (Table 4, col. 2) is not statistically different from the coefficient of *neigh expenditure* (0.88, and statistically significant at 1%) obtained by using the definition of 50 km neighborhood (Table 4, col. 5) since their confidence intervals overlap, while, on the contrary, there is not overlapping between these two coefficients and the estimation of the coefficient of *neigh expenditure* obtained by using the definition of 100 km neighborhood (Table 4, col. 8), which - as discussed above - turns out to be not statistically different from zero (Figure A1, Panel B, Appendix).

The same picture emerges from both total expenditure (Figure A1, Panel A, Appendix) and capital expenditure (Figure A1, Panel C, Appendix). In particular, for the case of total expenditure, I find that the *neigh expenditure* coefficient (0.22, and statistically significant at 10%) obtained by using the definition of 25 km neighborhood (Table 4, col. 1) is not statistically different from the coefficient of *neigh expenditure* (0.34, and statistically significant at 1%) obtained by using the definition of 50 km neighborhood (Table 4, col. 4) since their confidence intervals overlap, while, on the contrary, there is not overlapping between these two coefficients and the estimation of the coefficient of *neigh expenditure* obtained by using the definition of 100 km neighborhood (Table 4, col. 1), which - as discussed above - turns out to be not statistically different from zero (Figure A1, Panel A, Appendix). Finally, for the case of capital expenditure, the *neigh expenditure* coefficient (0.20, and statistically significant at 10%) obtained by using the definition of 25 km neighborhood (Table 4, col. 3) is not statistically different from the coefficient of *neigh expenditure* (0.21, and statistically significant at 5%) obtained by using the definition of 50 km neighborhood (Table 4, col. 6) as their confidence intervals overlap, while, on the contrary, there is not overlapping between these two coefficients and the estimation of the coefficient of *neigh expenditure* obtained by using the definition of 100 km neighborhood

investigated further whether the increase in the size of the coefficient is due to heterogeneity across municipalities. In fact, the spillover effect found in my analysis is an average effect of all Italian municipalities and the increase in the size of the spillover effects observed when the distance increases might be linked to some geographical characteristics of municipalities, being the idea that the definition of neighborhood (and so of the distance) is different between municipalities that are located in plain and municipalities that are located in mountain. To address this point we divided municipalities into two groups: the first one contains only municipalities located in plains (2,969 municipalities, 53% of the sample) and the other one contains municipalities located either in hill or in mountains (2,595 municipalities), and we run for these two sub-samples the previous regressions. Interestingly, we found that spillover effects for municipalities located in plain hold only when we use the contiguity matrix, while, for municipalities located in mountain/hill the spillover effects hold with all the weighted matrices and thus suggesting that the size of the spillover effect varies according to definition of neighborhood (and hence of distance), which, in turn, depends on the geographical characteristics of municipalities. Results are available upon request.

(Table 4, col. 9), which - as discussed above - turns out to be not statistically different from zero (Figure A1, Panel C, Appendix).

Table 4: Estimation results for total, current and capital expenditure with SYS-GMM using different type of neighbors' matrices

Weighting matrix	W^{25km}			W^{50km}			W^{100km}		
Dependent variable	Total	Current	Capital	Total	Current	Capital	Total	Current	Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Expenditure (-1)	0.35***	0.10*	0.34***	0.31***	0.15	0.33***	0.32***	0.70	0.24***
	(0.06)	(0.06)	(0.07)	(0.06)	(0.22)	(0.05)	(0.05)	(0.51)	(0.07)
Neigh expenditure	0.22*	0.77***	0.20*	0.34***	0.88***	0.21**	0.12	0.44	0.06
	(0.12)	(0.20)	(0.11)	(0.12)	(0.29)	(0.10)	(0.10)	(0.47)	(0.17)
Transfers	0.31***	-0.22	0.40***	0.36***	-0.20	0.47***	0.45***	-0.30	0.66***
	(0.10)	(0.23)	(0.11)	(0.09)	(0.37)	(0.09)	(0.07)	(0.51)	(0.15)
Population*10^{-4}	11.56**	21.58**	2.56***	11.27**	19.39**	1.94*	9.07***	9.62	2.34**
	(1.82)	(3.33)	(0.96)	(2.00)	(7.38)	(1.17)	(1.73)	(9.32)	(1.19)
Children*10^{3}	-2.27***	-1.32***	-0.61	-1.95***	-1.24*	-0.84**	-2.59***	-0.55	-0.44
	(0.57)	(0.44)	(0.39)	(0.53)	(0.68)	(0.39)	(0.51)	(1.03)	(0.37)
Aged*10^{3}	0.15	0.45	0.05	0.03	0.33	-0.04	0.10	0.37	0.07
	(0.25)	(0.30)	(0.12)	(0.23)	(0.41)	(0.12)	(0.16)	(0.46)	(0.09)
Area*10^{3}	4.65***	2.63***	2.22***	4.92***	3.07***	2.04***	4.77***	1.50	1.73***
	(0.83)	(0.61)	(0.44)	(0.77)	(1.15)	(0.41)	(0.65)	(1.89)	(0.43)
Income*10^{-3}	17.23**	2.12	3.18***	19.14**	1.44	3.82***	21.27***	-5.77	3.60***
	(3.61)	(5.66)	(0.98)	(3.08)	(8.43)	(1.01)	(2.53)	(13.98)	(1.03)
Domestic stability pact	-33.69**	-28.14*	26.63***	-28.33**	-27.12	26.93***	40.98***	-23.24	34.90***
	(13.66)	(15.90)	(8.11)	(12.37)	(22.48)	(7.25)	(9.45)	(28.83)	(5.47)
Election	19.36**	-0.96	17.17***	20.51**	-0.84	18.15***	20.73***	-2.92	19.28***
	(4.71)	(2.14)	(4.28)	(4.67)	(2.75)	(4.09)	(4.19)	(3.62)	(4.06)
Term-limit	4.13	-2.63	3.66	3.59	-3.71	3.10	1.00	-2.20	3.55
	(3.91)	(2.56)	(3.25)	(3.91)	(4.28)	(3.24)	(3.80)	(4.39)	(3.16)
Vote-share	94.28**	45.12**	50.91***	80.98**	37.46	50.38***	99.12***	21.17	32.75
	(26.43)	(20.30)	(17.23)	(27.31)	(32.73)	(18.33)	(26.01)	(35.93)	(19.93)
Constant	56.28	19.46	39.36	-74.37	-65.69	-11.86	202.78*	-47.39	77.38
	(102.74)	(118.79)	(51.86)	(105.24)	(96.09)	(45.26)	(113.57)	(140.01)	(93.45)
Observations	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640
Number of municipalities	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564
ar1p	0.000	0.000	0.000	0.000	0.043	0.000	0.000	0.085	0.000
Hansenp	0.826	0.176	0.500	0.376	0.229	0.578	0.224	0.164	0.233
ar2p	0.764	0.885	0.721	0.704	0.886	0.836	0.926	0.345	0.729
Number of instruments	28	29	29	28	27	29	29	26	27

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors, clustered at the municipal level, are shown in parentheses. I control for time and municipal fixed effects. The variables *neigh expenditure* and *transfers* are always instrumented using SYS-GMM, instead the variable *Expenditure (-1)* in all regressions is instrumented by using difference GMM. Instruments: (1) lags 1 and 2 for the variable *Expenditure (-1)*, lags 4 and 5 for the variables *neigh expenditure* and *transfers* (total transfers); (2) lags 1, 2, 3 and for the variable *Expenditure (-1)*, lags 7 and 8 for the variable *neigh expenditure*, lags 4 and 5 for the variable *transfers* (current transfers); (3) lags 1, 2, 3 and 4 for the variable *Expenditure (-1)*, lags 4 and 5 for the variable *neigh expenditure* and lag 4 for the variable *transfers* (capital transfers); (4) lags 1 and 2 for the variable *Expenditure (-1)*, lags 3 and 4 for the variable *neigh expenditure* and for the variable *transfers* (total transfers)*;* (5) lags 1 for the variable *Expenditure (-1)*, lags 4, 5 and 6 for the variable *neigh expenditure* and lag 5 for the variable *transfers* (current transfers); (6) lags 1, 2 and 3 for the variable *Expenditure (-1)*, 4 and 5 for the variable *neigh expenditure* and lags 3

and 4 for the variable *transfers* (capital transfers)*;* (7) lag 1for the variable *Expenditure (-1)*, 2, 3 and 4 for the variable *neigh expenditure* and lags 2, 3 and 4 for the variable *transfers* (total transfers)*; (*8) lags 1 for the variable *Expenditure (-1)*, lags 8 and 9 for the variable *neigh expenditure* and lags 8 and 9 for the variable *transfers* (current transfers); (9) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lag 2 for the variable *neigh expenditure* and lag 2 for the variable *transfers* (capital transfers). The validity of the instruments is checked by using the standard Hansen test and the C tests (results are available upon request).

1.6.2 An experimental setting

Municipal expenditures (total, current and capital) are found to be positively linked to those of their neighbors'. To obtain these results I instrumented the neighbors' averages of the dependent variable by using its lagged values. However, recent studies (Gibbons and Overman, 2012; Lyytikäinen, 2012; Baskaran, 2014; Isen, 2014; Di Porto et al., 2017) have pointed out that, within the framework of spatial econometrics, the use of internal instruments does not offer, in general, a valid identification of causal relation, which, in turn, might lead to biased estimates. Consequently, they suggest relaying on the "experimental paradigm", by exploiting exogenous variation in the neighbors' dependent variable. Therefore, I follow this approach to test the robustness of my results.

As a source of exogenous variation in the municipal spending I consider the severe earthquake that occurred in Abruzzo region in the year 2009. In particular, during the period April-October 2009, 49 municipalities (corresponding to approximately 15% of the total municipalities of Abruzzo region) were hit by the tremors that caused the collapse of houses, churches, schools, buildings and cracks of sidewalks and roads[17]. In addition, around 40,000 people were made homeless by the earthquake, and they found accommodation in tented camps and a further 10,000 people were housed in hotels, making this natural disaster one of the worse earthquake hitting Italy. The list of municipalities involved, as well as the estimates of the economic losses, amounting to more than 14.7 billion of euros, can be found in official documents of the Protezione Civile (the Italian Civil Defense)[18].

What is important for my purposes is that the municipalities were hit by the earthquake in different ways, that is the intensity of the earthquake that differs across municipalities. And in order to measure these differences, I collected data on the intensity of the earthquake, by relaying on the Mercalli-Carcani-Sieberge (MSC) scale registered in each municipality hit by the earthquake the day 6th of April 2009.[19]

I then restrict the analysis on the municipalities belonging to Abruzzo region in the period 2009-2010, and I test whether the change in the average expenditure of neighboring municipalities from 2009 to 2010 affects the change in the expenditure of a given municipality during the same period. Therefore, I rewrite equation (1) in first difference - without considering the lagged value of the dependent variable - as follows:

[17] In the dataset we have information on only 195 out of 305 municipalities belonging to Abruzzo region. However, within the sample, the share of municipalities hit by the tremors is 12% (22 municipalities on 195), a percentage that is very close to real percentage of municipalities affected by the earthquake on the total number of municipalities belonging to Abruzzo region (49 municipalities on 305).

[18] The estimates of losses are reported in Law 97/2009. For a detailed account of damages, see reports by Protezione Civile at http://www.protezionecivile.gov.it/jcms/it/emergenza_abruzzo.wp.

[19] Data on the Mercalli-Carcani-Sieberge (MSC) scale can be found at http://www.protezionecivile.gov.it/resources/cms/documents/Elenco_centri_abitati_danneggiati.pdf.

$$\Delta G_i = \gamma \Delta \sum_{j \neq i} \omega_{ij} G_j \; + \Delta x_i \rho + \varepsilon_i, \qquad (2)$$

where ΔG_i is the change in the per capita expenditure of a given municipality *i* from 2009 to 2010, $\Delta \sum_{j \neq i} \omega_{ij} G_j$ is the change, from 2009 to 2010, in the average per capita expenditure of neighbors' municipalities, where ω_{ij} are exogenously chosen weights matrix, row-standardized, that gives the value 1 to municipalities that share a border and 0 otherwise, Δx_i is the change, from 2009 to 2010, of the explanatory variables described in Section 1.4.2[20], and ε_i is the error term. I deal with endogeneity issues by employing two strategies, which are then compared in order to establish whether they lead to different results.

The first strategy consists in estimating Equation (2) by instrumenting the endogenous variables using an internal instrument. Therefore, the change in the average per capita expenditure of neighbors, $\Delta \sum_{j \neq i} \omega_{ij} G_j$, from 2009 to 2010, is instrumented by using its lags, i.e., the change in the average per capita expenditure of neighbors from 2008 to 2009.

The second strategy relies on the exogenous variation in the neighbors' expenditure induced by the natural disaster, which provides "external" instrument. In particular, and adopting an approach similar to that of Lyytikäinen (2012), I set a dummy variable (*Earthquake*) equal to one if a municipality was hit by the earthquake in 2009 and zero otherwise. I then build a new variable, *Earthquake intensity*, expressing the intensity of the earthquake, obtained by the product of *Earthquake* dummy with the Mercalli-Carcani-Sieberge (MSC) scale registered in each municipality hit by the earthquake. Finally, I use the variable *Earthquake intensity* to build the variable W*Earthquake intensity*, measuring the intensity of the earthquake in the neighboring municipalities. This latter variable is then used to instrument the change in the average expenditure of neighbors' municipalities.

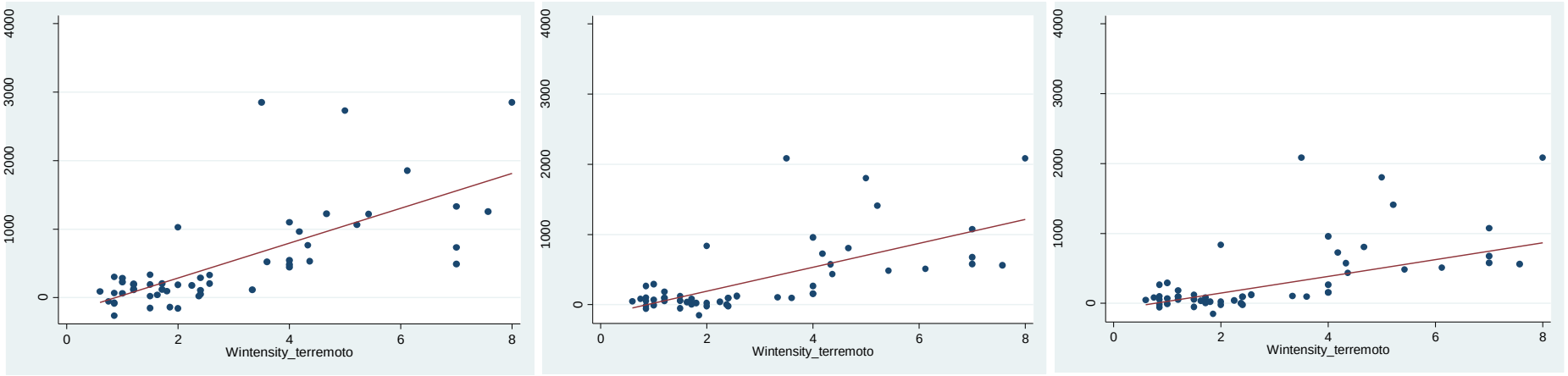
An example is useful to illustrate how the instrument works. Consider a municipality A that has two neighbors, B and C. Suppose that in the year 2009 only municipality B is hit by the earthquake, and that its intensity, measured by MSC scale, is equal to 4. Suppose also that the corresponding variable is equal to zero for municipality C, which is not affected by the earthquake. Hence, the value of the instrument for neighbors of municipality A is equal to 2, the weighted average of earthquake intensity for its neighbors. The rationale behind this instrument is that the variation in the neighbors' expenditure can be explained by the intensity of the earthquake they experienced; and since this instrument is based on an unexpected natural disaster, it is reasonable to consider it as exogenous for any single municipality.

Before examining the results, it is interesting to note that the relationship between the change, from 2009 to 2010, in the neighboring expenditure (total, current and capital) and the variable measuring the average intensity of the earthquake in neighboring municipalities, i.e., the relationship between the instrumented variable and its instrument,

[20] Note that for political variables, namely *election*, *term-limit* and *vote-share* we use the 2010 value expressed in levels, instead of taking the first difference as these variables are related to the political cycle and therefore they do not show the classical panel dimension.

is positive, suggesting that higher earthquake intensity on neighbors is associated with higher changes in their average expenditure (see Figure 2). A possible explanation is that the higher was the intensity of the flood, the higher were the damages. Clearly, while damages in infrastructures of small entity can be recovered in short time, the process of recovery spending for severe damages usually takes several years. Therefore, it is plausible that the increase in spending on infrastructures immediately after the flood was lower in municipality heavily hit by the event with respect to other municipalities. The difference, from 2010 to 2011 (two years after the flood), in the per-capita spending on infrastructures for the municipalities hit by the flood is, on average, equal to 27.78 euros in per-capita terms, while the corresponding figure for the municipalities not hit by the flood is, on average, equals -12.24, suggesting that two years after the flood the former municipalities spent more on infrastructures than the latter. Moreover, I checked whether this increase in per-capita spending on infrastructures between 2010 and 2011 was driven by those municipalities more severely affected by the flood. In particular, I used the intensity of the flood to classify municipalities into two groups: a) those strongly affected by the flood and b) those slightly affected by the flood, with the former group containing those municipalities for which the intensity of the flood is above the value identifying the third quartile (483 millimeters). Interestingly, I found that the increase in the per-capita spending on infrastructures between 2010 and 2011 is more marked for those municipalities strongly affected by the flood (76.68 per capita euros) than for those slightly affected (8.22 per capita euros).

Figure 2: Relationship between instrument and instrumented variable



Turning to the results, I find that, in the specification that employs the internal instruments (Table 5, col. 1, 3 and 5), for the change in total expenditure as dependent variable (col.1) the *neigh expenditure* coefficient is equal to 0.69, statistically significant at 10%. Similar results are obtained when I rely on the external instrument (col. 2), even though the estimated coefficient of neighboring muncipalities is lower (0.24, statictally significant at 5%) than that found by using the internal instrument, and thus suggesting that estimations carried out with the internal instrument might lead to upward biased estimates. As far as the first stage concerns, I notice that both my internal (the change in the average per capita expenditure of neighbors from 2008 to 2009) and external (intensity of the earthquake in the neighboring municipalities) instruments are positive and highly significant with a coefficient of 0.67 and 266.71, respectively (Table A1, col. 1 and 2, Appendix). In addition, the Kleibergen-Paap F statistics indicate that *WEarthquake intensity* is a very strong instrument. For the case of current expenditure, I find that estimations carried out by using internal instruments lead to very similar results to those obtained by relying on external instruments. Indeed, the *neigh expenditure* coefficient is equal to 0.12, statistically significant at 10% in the specification where I use the internal instrument (Table 5, col. 3), while, in the specification where I use the external instrument the *neigh expenditure* coefficient is positive (0.19) and statistically significant at 5%. Also in this case, both internal and external instrument have a positive and highly significant impact on the change in the neighbors' expenditure (Table A1, col. 3 and 4), and thus indicating that are good instruments. Finally, as for capital expenditure, I find that coefficients are not statistically significant using both the external (Table 5, col. 5) and the internal instruments (Table 5, col. 5), but with the same (positive) sign.

All in all I showed that, within the experimental setting, the estimations carried out by using internal instruments led to similar results (with the only exception being the case of total expenditure, where the estimated coefficients obtained with the internal instrument is found to be larger than those obtained with the external instrument) to those obtained by relying on external instruments, and so reinforcing the evidence of a positive horizontal interdependence in spending decision among Italian municipalities.

Table 5 Estimation results for total, current and capital expenditure: internal vs external instrument

Dependent variable	Total expenditure		Current expenditure		Capital expenditure	
Type of instrument	Internal	External	Internal	External	Internal	External
	(1)	(2)	(3)	(4)	(5)	(6)
Neigh expenditure ($\Delta_{2010-2009}$)	0.69*	0.24**	0.12*	0.19**	7.83	0.36
	(0.41)	(0.11)	(0.07)	(0.08)	(25.77)	(0.24)
Earthquake	-4,728.58*	-6,107.02***	-2,260.94*	-2,133.97*	11,126.46	-1,478.05
	(2,468.84)	(1,801.35)	(1,185.43)	(1,228.67)	(44,748.00)	(2,217.79)
Earthquake intensity	863.11**	1,136.32***	374.27*	348.13*	-2,024.62	317.60
	(418.62)	(279.83)	(200.86)	(208.30)	(8,269.06)	(358.74)
Transfers ($\Delta_{2010-2009}$)	0.04	0.02	0.40***	0.41***	0.06	-0.11
	(0.09)	(0.08)	(0.15)	(0.15)	(0.83)	(0.07)
Population ($\Delta_{2010-2009}$)	0.05	0.06	-0.02	-0.03	0.05	0.07
	(0.04)	(0.05)	(0.03)	(0.03)	(0.20)	(0.07)
Children ($\Delta_{2010-2009}$)	5,358.95	-7,125.90	-1,569.05	-1,012.48	147,943.18	-956.22
	(13,919.46)	(10,095.25)	(4,476.20)	(4,216.01)	(532,368.89)	(11,384.84)
Aged ($\Delta_{2010-2009}$)	-1,387.59	-3,031.26	4,165.09**	4,100.80**	28,194.01	3,775.31
	(8,364.48)	(7,655.17)	(1,877.04)	(1,871.88)	(99,143.31)	(7,073.54)
Area ($\Delta_{2010-2009}$)	27,352.72	35,969.33	1,092.36	140.83	-22,337.85	26,523.40*
	(21,593.06)	(22,673.77)	(11,946.86)	(11,965.64)	(174,982.33)	(15,440.00)
Income ($\Delta_{2010-2009}$)	-0.21	0.02	0.05	0.03	-1.36	0.03
	(0.28)	(0.18)	(0.06)	(0.05)	(5.01)	(0.16)
Domestic stability pact	107.99	44.01	20.50	25.36	486.35	12.46
	(66.70)	(46.70)	(20.94)	(19.94)	(1,694.45)	(39.91)
Election	222.19**	228.56**	-10.05	-10.82	288.72	262.47**
	(107.41)	(107.69)	(35.20)	(35.50)	(744.45)	(129.83)
Term-limit	83.98	73.78	47.20**	46.77**	315.77	53.02
	(76.62)	(72.13)	(19.63)	(19.69)	(922.95)	(72.08)
Vote-share	62.11	27.26	83.39	85.16	327.02	-50.86
	(230.10)	(251.81)	(106.61)	(104.74)	(1,640.51)	(220.63)
Costant	-167.56	-112.92	-59.71	-63.70	-498.77	-55.28
	(133.99)	(139.91)	(71.62)	(71.02)	(1,772.88)	(108.12)
Observations	195	195	195	195	195	195
R-squared	0.49	0.57	0.72	0.72	0.36	0.36
Kleibergen-Paap F	13.834	75.08	33.148	56.644	0.072	30.006

Notes: *** significant at 1%; ** significant at 5%; significant at 10%. Columns (1), (3) and (5) display the results by using internal instruments, namely the change in the average per capita expenditure of neighbors from 2008 to 2009. Columns (2), (4) and (6) show the second stage results of the previous regressions by using the neighbor's earthquake intensity variable as instruments for the change in the average expenditure of neighboring municipalities. The spatial weighting matrix (W) used is of the type: contiguity-based and it is row-standardized. Robust standard errors are shown in parentheses.

1.7 Testing for sources of spatial interdependence

The estimates carried out so far have shown that there is a strategic interaction between spending decisions at local level, without, however, revealing the source of such interdependence. Therefore, I investigate further these spatial interactions in order to test whether the municipal interdependence is driven by yardstick competition and/or spillover

effects. On the one hand, indeed, strategic interactions can be justified in electoral periods, when policymakers are concerned about re-election and hence they might look at what neighboring municipalities do (yardstick competition hypothesis). On the contrary, the presence of strategic interactions might be reasonably due to the fact that citizens living in the neighboring municipalities can enjoy the provision of local public goods in a given municipality (spillovers effect hypothesis).

1.7.1 Yardstick competition hypothesis

The idea of yardstick competition is due to the fact that voters have no complete information about the type of the policy maker, hence they compare policies carried out in their municipality with those of nearby municipalities (Salmon, 1987). As results, the local policy maker has an incentive to look at what her neighboring municipalities do in order to get (or not to lose) political consensus. Then, starting from these insights, the empirical literature has linked fiscal interactions with the political process. In particular, fiscal interdependences might be effective in both electoral (Sollé-Ollé, 2003) and pre-electoral (Bartolini and Santolini, 2012) years, when politicians mimic the behavior of their neighbor's to capture voters' preferences and to win elections. This behavior is found to be more pronounced when politicians are not lame duck, which implies that they are interested in obtaining voters' confidence (Case, 1993; Bordignon et al., 2003), and it might depend further on the power of the policy maker, in terms of electoral consensus (Bordignon et al., 2003; Allers and Elhrost, 2005).

In order to test the existence of yardstick competition I interact the neighboring expenditure variable with political variables, such that the model I estimate takes the following form:

$$G_{it} = \alpha + \beta G_{(it-1)} + \gamma WG_{-it} + \delta(political\_variables_{it} * WG_{-it}) + \rho X_{it} + \mu_i + \tau_t + \varepsilon_{it}. \qquad (3)$$

I estimate equation (3) using SYS-GMM and instrumenting the lagged dependent variable and the other endogenous variables (neighboring expenditure, neigboring expenditure interacted with political variables and transfers). I separately estimate equation (3) allowing for four different specifications of political variables and I test the robustness of my results by considering alternative weighting matrices based on geographical distance (I consider neighbors all municipalities distant up to *i)* 25 km and *ii)* 50 km, respectively.

In the first specification I use, as a political variable, the election year dummy (*election*): a positive and significant coefficient of the interaction between spending decision and the electoral dummy would imply the presence of yardsdtick competition. However, the interaction term (*neigh expenditure*election*) is never statistically significant (Table 6) for any of dependent variables and for any adopted weighting scheme matrices (contiguity and geographical distance – 25 and 50 km).

Table 6: Estimation results for yardstick competition model with the interaction between neigh expenditure and Election

Weighting matrix	$W^{contiguity}$			W^{25km}			W^{50km}		
Dependent variable	Total	Current	Capital	Total	Current	Capital	Total	Current	Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Expenditure (-1)	0.33***	0.10*	0.31***	0.37***	0.09*	0.33***	0.38***	0.07	0.32***
	(0.05)	(0.06)	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)
Neigh expenditure	0.12*	0.62***	0.11***	0.25*	0.71***	0.13*	0.23**	0.84***	0.15*
	(0.07)	(0.16)	(0.04)	(0.13)	(0.16)	(0.07)	(0.12)	(0.17)	(0.09)
Neigh expenditure * election	0.10	0.02	-0.03	-0.06	0.02	-0.00	0.08	0.02	0.06
	(0.06)	(0.02)	(0.04)	(0.04)	(0.02)	(0.05)	(0.05)	(0.03)	(0.07)
Transfers	0.33***	-0.27	0.46***	0.32***	-0.11	0.44***	0.19**	-0.04	0.46***
	(0.08)	(0.20)	(0.09)	(0.10)	(0.22)	(0.10)	(0.09)	(0.19)	(0.09)
Election	-100.58	-11.92	31.16*	90.00*	-11.93	19.37	-77.19	-13.07	-11.90
	(75.23)	(16.82)	(à18.46)	(50.47)	(18.85)	(25.07)	(60.61)	(22.44)	(33.93)
Constant	236.41***	120.28	90.13***	28.62	50.46	78.28**	56.61	-61.95	-9.35
	(62.06)	(96.98)	(32.77)	(110.48)	(133.16)	(39.39)	(105.99)	(103.93)	(40.66)
Observations	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640
Number of municipalities	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564
ar1p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
hansenp	0.341	0.315	0.919	0.510	0.153	0.428	0.28	0.138	0.397
ar2p	0.777	0.646	0.757	0.841	0.694	0.803	0.689	0.537	0.806
Number of instruments	31	30	31	31	30	31	34	31	31

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors, clustered at the municipal level, are shown in parentheses. I control for time and municipal fixed effects, *population*10^{-4}*, *children*10^{3}*, *aged*10^{3}*, *area*10^{3}*, *income*10^{-3}*, *domestic stability pact*, *term-limit*, *vote-share*. The variables *Expenditure (-1)*, *neigh expenditure*, the interaction *neigh expenditure * election* and *transfers* are always instrumented using SYS-GMM, excluding for *Expenditure (-1)* in regression (2), (5) and (8), which is instrumented by using difference GMM. Instruments: (1) lags 1 and 2 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure*, *neigh expenditure*election* and *transfers* (total transfers); (2) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 7 and 8 for the variable *neigh expenditure*, lags 5 and 6 for the variable *neigh expenditure*election*, and lag 4 for the variable *transfers* (current transfers); (3) lags 1 and 2 for the variable *Expenditure (-1)*, lags 2 and 3 for the variable *neigh expenditure*, lags 6 and 7 for the variable *neigh expenditure*election*, and lags 3 and 4 for the variable *transfers* (capital transfers); (4) lags 1 and 2 for the variable *Expenditure (-1)*, lags 4 and 5 for the variables *neigh expenditure*, *neigh expenditure*election* and *transfers* (total transfers); (5) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 7 and 8 for the variable *neigh expenditure*, lags 5 and 6 for the variable *neigh expenditure*election*, and lag 4 for the variable *transfers* (current transfers); (6) lags 1 and 2 for the variable *Expenditure (-1)*, lags 2 and 3 for the variable *neigh expenditure*, lags 6 and 7 for the variable *neigh expenditure*election*, and lags 3 and 4 for the variable *transfers* (capital transfers); (7) lags 1 and 2 for the variable *Expenditure (-1)*, lags 5, 6 and 7 for the variables *neigh expenditure*, lags 6, 7 and 8 for the variables *neigh expenditure*election* and *transfers* (total transfers); (8) lags 1 and 3 for the variable *Expenditure (-1)*, lags 7 and 8 for the variable *neigh expenditure*, lags 5 and 6 for the variable *neigh expenditure*election*, and lags 4 and 5 for the variable *transfers* (current transfers); (9) lags 1 and 2 for the variable *Expenditure (-1)*, lags 2 and 3 for the variable *neigh expenditure*, lags 6 and 7 for the variable *neigh expenditure*election*, and lags 3 and 4 for the variable *transfers* (capital transfers). The validity of the instruments is checked by using the standard Hansen test and the C tests (results are available upon request).

I also explore another possibility, by interacting the neighboring expenditure variable with the dummy variable *term-limit*, if there is yardstick competition I expect this interaction to be negative, because a lame-duck mayor should not have any electoral concern (Esteller-Moré and Rizzo, 2014). However, the results shown in Table 7 do not support

the existence of yardstick competition in municipal spending decisions, as the interaction term (*neigh expenditure*term-limit*) turns out to be never statistically significant for any of dependent variables and for any weighting scheme matrices.

Table 7: Estimation results for yardstick competition model with the interaction between neigh expenditure and Term-limit

Weighting matrix	$W^{contiguity}$			W^{25km}			W^{50km}		
Dependent variable	Total	Current	Capital	Total	Current	Capital	Total	Current	Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Expenditure (-1)	0.30***	0.11*	0.31***	0.30***	0.09	0.31***	0.31***	0.07	0.30***
	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.06)
Neigh expenditure	0.16*	0.70***	0.08**	0.26**	0.81***	0.13*	0.34***	0.86***	0.14
	(0.09)	(0.17)	(0.04)	(0.13)	(0.18)	(0.08)	(0.12)	(0.18)	(0.10)
Neigh expenditure * term-limit	0.03	-0.01	0.06	0.00	0.02	0.06	-0.02	0.04	0.09
	(0.04)	(0.05)	(0.05)	(0.06)	(0.07)	(0.06)	(0.09)	(0.08)	(0.07)
Transfers	0.38***	-0.29	0.48***	0.36***	-0.17	0.47***	0.36***	-0.11	0.50***
	(0.09)	(0.21)	(0.09)	(0.09)	(0.23)	(0.10)	(0.08)	(0.22)	(0.10)
Term-limit	-36.50	5.82	-23.25	0.09	-17.44	-24.31	23.59	-35.17	-41.74
	(53.28)	(37.60)	(22.48)	(74.67)	(53.53)	(28.58)	(103.52)	(58.84)	(34.86)
Constant	135.00**	86.25	53.95*	35.35	-4.53	88.84**	-62.40	-47.85	6.62
	(65.49)	(104.33)	(32.47)	(104.74)	(110.62)	(40.13)	(115.57)	(113.54)	(42.55)
Observations	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640
Number of municipalities	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564
ar1p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
hansenp	0.593	0.376	0.763	0.515	0.174	0.205	0.487	0.135	0.135
ar2p	0.683	0.668	0.750	0.615	0.983	0.739	0.708	0.832	0.780
Number of instruments	31	31	33	31	31	33	31	30	33

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors, clustered at the municipal level, are shown in parentheses. I control for time and municipal fixed effects, *population*10^{-4}*, *children*10^{3}*, *aged*10^{3}*, *area*10^{3}*, *income*10^{-3}*, *domestic stability pact*, *election vote-share*. The variables *Expenditure (-1)*, *neigh expenditure*, the interaction *neigh expenditure*term-limit* and *transfers* are always instrumented using SYS-GMM, excluding for *Expenditure (-1)* in regression (2), (5) and (8), which is instrumented using difference GMM. Instruments: (1) lags 1 and 2 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure*, *neigh expenditure*term-limit* and *transfers* (total transfers); (2) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 7 and 8 for the variable *neigh expenditure*, lags 3 and 4 for the variable *neigh expenditure*term-limit*, and lag 4 for the variable *transfers* (current transfers); (3) lags 1 and 2 for the variable *Expenditure (-1)*, lags 2 and 3 for the variable *neigh expenditure*, lags 3, 4 and 5 for the variable *neigh expenditure*term-limit* and for the variable *transfers* (capital transfers); (4) lags 1 and 2 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure*, *neigh expenditure*term-limit* and *transfers* (total transfers); (5) lags 1, 2, 3 and 4 for the variable *Expenditure (-1)*, lags 7 and 8 for the variable *neigh expenditure*, lags 3 and 4 for the variable *neigh expenditure*term-limit*, and lag 4 for the variable *transfers* (current transfers); (6) lags 1 and 2 for the variable *Expenditure (-1)*, lags 2 and 3 for the variable *neigh expenditure*, lags 3, 4 and 5 for the variable *neigh expenditure*term-limit*, and lags 3, 4 and 5 for the variable *transfers* (capital transfers); (7) lags 1 and 2 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure*, *neigh expenditure*term-limit* and *transfers* (total transfers); (8) lags 1 and 2 for the variable *Expenditure (-1)*, lags 7 and 8 for the variable *neigh expenditure*, lags 3 and 4 for the variable *neigh expenditure*term-limit*, and lags 4 and 5 for the variable *transfers* (current transfers); (9) lags 1 and 2 for the variable *Expenditure (-1)*, lags 2 and 3 for the variable *neigh expenditure*, lags 3, 4 and 5 for the variable *neigh expenditure*term-limit*, and lags 3, 4 and 5 for the variable *transfers* (capital transfers). The validity of the instruments is checked by using the standard Hansen test and the C tests (results are available upon request).

Furthermore, I test for the presence of yardstick competition by considering the interaction between the year before election (*pre-election*) with neighboours' spending decision, as the opportuisc behavior can take place in the pre-electoral year. The results (Table A2, Panel A, Appendix) show that the coefficient of the interaction term (*neigh expenditure*pre-election*) is not statistically significant for any of dependent variables, nor for any weighting schemes (contiguity and geographical weight distance). Finally, I tried to capture yardstick competion by looking at size of the majoirty supporting the mayors, namely I interact the neighbors' spending with the share of votes obtained by the mayor (*vote-share*) which, differently from previous cases, is a continuos variable ranging from 0 to 1. Therefore, testing for yardstick competition in this case implies to evalute the combined coefficients of $\gamma + \lambda$*vote-share*, which depends on the value of *vote-share*. According to the estimates, such combined coefficient turns out to be not statistically significant for any value of *vote-share*, for any of dependent variables and for any weighting schemes (Table A2, Panel B, Appendix).

All the results indicate that yardstick competition is not a source of spatial interaction, that municipalities do not mimic each other to get votes and that the spatial interdependence is not sensitive to the electoral cycle.

1.7.2 Spillover Hypothesis and the size of municipalities

The absence of yardstick competition reveals that the source of spatial interactions in spending decisions among Italian municipalities is likely due to spillover effects. Therefore, I perform an additional test to verify whether the municipality size influences the spatial interdependence. The hypothesis is based on recent findings of Ferraresi et al., (2017), who show – both theoretically and empirically – that the size of a municipality affects spatial interactions, the intuition being that a highly populated municipality hardly reacts to changes in expenditure by a neighboring municipality, because spillover effects on its residents are negligible.

In order to consider the size of the municipality I include in the model the interaction of the neighbor's expenditure with the variable $population_{it} * 10^{-4}$ and I estimate the following model:

$$G_{it} = \alpha + \beta G_{(it-1)} + \gamma WG_{-it} + \lambda(WG_{-it} * population_{it} * 10^{-4}) + \rho X_{it} + \mu_i + \tau_t + \varepsilon_{it}. \quad (4)$$

Note that the estimate of the spatial interaction is given by $\gamma + \lambda$*population*10^{-4}, which depends on the size of the municipality, expressed by its population. The estimation of eq. (4) is carried out by applying the SYS-GMM estimator and instrumenting all the endogenous variables (neighboring expenditure, neigboring expenditure interacted with *population**10^{-4} and transfers). I also test the robustness of results by considering alternative weighting matrices based on geographical distance (25 and 50 km, respectively).

As far as the total expenditure regards, the coefficient of neigh expenditure is positive (0.45) and statistically significant at 1%, while the coefficient of *neigh expenditure*population*10^{-4}* is negative (-0.02), but not statically significant (Table 8, col. 1). Most importantly, the estimated coefficient of the spatial interaction ($\gamma + \lambda$**population*), is positive and statistically significant for any level of population below 90,000 inhabitants, suggesting that municipalities expand their expenditure in response to an increase in the spending of the neighbors, but such effect holds only below a given population threshold. Note that results are almost the same if I use different spatial weighting matrices (Table 8, col. 4 and 7).

Turning to current expenditure, the neighboring expenditure coefficient is positive (0.66) and statistically significant at 1% and the coefficient of the interaction between population and neighbor's expenditure (*neigh expenditure*population*10^{-4})* is negative (-0.06) and significant at 10% (Table 8, col. 4). According to these coefficients, I find that the spatial interaction is positive and significant, but only for municipalities with less than 50,000 inhabitants, that is municipalities with a population larger than 50,000 inhabitants do not strategically interact with their neighbors. Again, if I change the weighting the results do not change (Table 8, col. 5 and 8).

As for capital expenditure (Table 8, col. 7), the estimated coefficient of the neighboring expenditure is positive (0.47) and significant at 1%, and that of the interaction term (*neigh expenditure*population*10^{-4}*) is negative (-0.02), but not significant. In this case, municipalities positively react to an increase in capital expenditure of neighboring communities as long as the level of population is lower than 85,000 inhabitants. These results are robust to different specifications of the weighting matrices (Table 8, col. 6 and 9).

Table 8: Estimation results of the spending interdependence and size of municipalities

Weighting matrix	$W^{contiguity}$			W^{25km}			W^{50km}		
Dependent variable	Total	Current	Capital	Total	Current	Capital	Total	Current	Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Expenditure (-1)	0.45***	0.08	0.43***	0.46***	0.06	0.43***	0.86***	0.12	0.40***
	(0.07)	(0.07)	(0.11)	(0.07)	(0.07)	(0.11)	(0.23)	(0.08)	(0.09)
Neigh expenditure	0.45***	0.66***	0.47***	0.53***	0.82***	0.54***	0.67**	0.77***	0.50***
	(0.14)	(0.07)	(0.14)	(0.15)	(0.07)	(0.16)	(0.29)	(0.14)	(0.15)
Neigh expenditure * (population*10^{-4})	-0.02	-0.06*	-0.02	-0.02	-0.08***	-0.03	-0.10	-0.09***	-0.01
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.09)	(0.03)	(0.02)
Transfers	0.12	-0.17	0.10	0.06	0.10	0.11	-0.13	-0.27	0.20
	(0.09)	(0.22)	(0.17)	(0.08)	(0.18)	(0.17)	(0.24)	(0.20)	(0.16)
Population*10^{-4}	37.64	63.11***	8.95	34.11	75.52***	12.86	118.86	81.11***	3.76
	(24.02)	(20.51)	(9.40)	(23.92)	(14.66)	(10.37)	(102.05)	(21.00)	(7.58)
Constant	14.46	70.96	0.90	-152.88	-83.74	-126.92**	-485.19	-2.06	-154.43**
	(102.81)	(65.17)	(47.36)	(122.32)	(62.32)	(55.54)	(340.89)	(91.49)	(62.50)
Observations	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640
Number of municipalities	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564
ar1p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Hansenp	0.214	0.525	0.479	0.637	0.138	0.375	0.865	0.166	0.316
ar2p	0.636	0.834	0.488	0.498	0.358	0.581	0.987	0.828	0.667
Number of instruments	31	31	32	31	31	31	28	30	33

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1 Robust standard errors, clustered at the municipal level, are shown in parentheses. I control for time and municipal fixed effects, *children*10^{3}*, *aged*10^{3}*, *area*10^{3}*, *income*10^{-3}*, *domestic stability pact*, *election*, *term-limit* and *vote-share*. The variables *Expenditure (-1)*, *neigh expenditure*, the interaction *neigh expenditure*population*10^{-4}* and *transfers* are always instrumented using SYS-GMM, excluding *Expenditure (-1)* in regression (2), (5) and (8), which is instrumented using difference GMM. Instruments: (1) lags 1 and 2 for the variable *Expenditure (-1)*, lags 5 and 6 for the variable *neigh expenditure*, and lags 6 and 7 for the variables *neigh expenditure*population*10^{-4}* and *transfers*; (2) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 5 and 6 for the variables *neigh expenditure* and *neigh expenditure*population*10^{-4}*, lags 4 and 5 for the variable *transfers* (current transfers); (3) lags 1, 2, 3 and 4 order lags for the variable *Expenditure (-1)*, lag 5 for the variable *neigh expenditure*, and lags 6 and 7 for the variables *neigh expenditure*population*10^{-4}* and *transfers* (capital transfers); (4) lags 1 and 2 for the variable *Expenditure (-1)*, lags 5 and 6 for the variable *neigh expenditure*, and lags 6 and 7 for the variables *neigh expenditure*population*10^{-4}* and *transfers*; (5) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 5 and 6 for the variables *neigh expenditure* and *neigh expenditure*population*10^{-4}*, lags 4 and 5 for the variable *transfers* (current transfers); (6) lags 1, 2, 3 and 4 order lags for the variable *Expenditure (-1)*, lag 5 for the variable *neigh expenditure*, lag 2 for the variable *neigh expenditure*population*10^{-4}* and lags 6 and 7 for the variable *transfers* (capital transfers); (7) lag 3 for the variables *Expenditure (-1)* and *neigh expenditure*, lag 2 for tne variable variables *neigh expenditure*population*10^{-4}* and lags 2 and 3 for the variable *transfers*; (8) lags 1 and 2 for the variable *Expenditure (-1)*, lags 8 and 9 for the variables *neigh expenditure* and *neigh expenditure*population*10^{-4}*, lags 4 and 5 for the variable *transfers* (current transfers); (9) lags 1, 2, 3, 4 and 5 order lags for the variable *Expenditure (-1)*, lags 5 and 6 for the variable *neigh expenditure*, lag 2 for the variable *neigh expenditure*population*10^{-4}* and lags 6 and 7 for the variable *transfers* (capital transfers). The validity of the instruments is checked by using the standard Hansen test and the C tests (results are available upon request).

Our estimations suggest that the municipal expenditure - current and capital - is positively linked to that of their neighbors, but only for population levels below a given threshold, that is the complementarity relationship in the provision of local public goods and services holds only for a given population level. In fact, it is very likely that a highly populated municipality hardly reacts to changes in per capita expenditure of a small neighboring

municipality since, in terms of public goods spillovers, these changes have a negligible per capita impact on the residents of a large municipality.

1.8 Conclusion

In this paper I explored the existence of spatial interactions in spending decisions among Italian municipalities. I estimated a spatial autoregressive dynamic panel data model, by using data on 5,564 Italian municipalities for the period 2001-2011, and exploiting their border contiguity. I found a positive and statistically significant effect of neighbors' expenditure on the expenditure of a given municipality, for total, capital and current expenditure. These results turned out to be robust not only by using different weighting matrices, but also by employing an experimental approach. I did not find any evidence of yardstick competition, and therefore I are confident that spillover effects drive the strategic interaction. This conclusion is confirmed by the results of a negative relationship between spatial interaction and municipality's size for current expenditures. A highly populated municipality should hardly react to changes in per capita expenditure of a small municipality, because public goods spillovers are negligible on the residents of a large municipality.

1.9 Appendix

Table A1: First stage regression results of Table 6

Dependent variable	Neigh total expenditure ($\Delta_{2010-2009}$)		Neigh current expenditure ($\Delta_{2010-2009}$)		Neigh capital expenditure ($\Delta_{2010-2009}$)	
Type of instrument	Internal	External	Internal	External	Internal	External
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbors' earthquake intensity		266.71***		161.15***		104.87***
		(30.78)		(21.41)		(19.15)
Neigh expenditure ($\Delta_{2009-2008}$)	0.67***		1.50***		0.04	
	(0.18)		(0.26)		(0.16)	
Earthquake	-2,789.34**	-1,812.41***	-430.28	-1,045.61	-1,690.94**	-1,180.77
	(1,229.74)	(575.09)	(802.45)	(1,279.13)	(649.56)	(756.71)
Earthquake intensity	536.88***	294.20***	97.24	186.38	313.50***	187.21
	(192.83)	(91.15)	(139.24)	(218.12)	(106.39)	(123.88)
Transfers ($\Delta_{2010-2009}$)	-0.04	-0.06	0.00	-0.12	-0.02	-0.03
	(0.10)	(0.04)	(0.08)	(0.13)	(0.08)	(0.07)
Population ($\Delta_{2010-2009}$)	0.02	0.00	-0.01	0.01	0.00	-0.01
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Children ($\Delta_{2010-2009}$)	-18,765.94*	-12,091.08**	-3,716.65	716.67	-19,447.52***	-13,574.88***
	(10,520.03)	(4,839.95)	(3,956.32)	(3,378.20)	(7,341.56)	(4,942.05)
Aged ($\Delta_{2010-2009}$)	-9,508.14	-2,412.08	-1,631.89	-130.38	-3,589.68	-2,735.94
	(7,568.87)	(4,237.46)	(3,396.30)	(2,365.82)	(4,026.47)	(3,323.62)
Area ($\Delta_{2010-2009}$)	1,948.23	4,915.05	3,112.29	6,027.60	5,686.59	734.45
	(13,965.53)	(6,721.22)	(7,472.83)	(9,135.79)	(11,297.57)	(7,613.08)
Income ($\Delta_{2010-2009}$)	0.40*	0.13	0.16	0.09	0.18	0.03
	(0.23)	(0.09)	(0.10)	(0.06)	(0.11)	(0.06)
Domestic stability pact	-122.23**	-29.45	-43.94	-9.10	-63.13**	-18.80
	(48.24)	(39.50)	(32.44)	(26.44)	(29.21)	(25.66)
Election	0.20	-110.52	13.47	-63.15	-4.14	-51.88
	(128.33)	(87.12)	(54.75)	(56.49)	(100.18)	(73.73)
Term-limit	-55.96	-27.49	-10.53	5.28	-36.52	-36.75
	(58.34)	(37.69)	(30.21)	(24.13)	(38.51)	(34.90)
Vote-share	-36.64	-21.25	13.69	8.53	-49.24	-29.03
	(188.13)	(108.54)	(90.24)	(77.35)	(99.25)	(84.23)
Costant	116.46	-37.20	22.31	-63.70	60.16	-3.05
	(138.01)	(72.77)	(60.41)	(71.02)	(68.93)	(53.53)
Observations	195	195	195	195	195	195
R-squared	0.50	0.81	0.69	0.78	0.31	0.52
Kleibergen-Paap F	13.834	75.08	33.148	56.644	0.072	30.006

Notes: *** significant at 1%; ** significant at 5%; significant at 10%. Column (1) displays the first stage results of the specification reported in Table 6 (col. 1); col. (2) displays the first stage results of the specification reported in Table 6 (col. 2); col. (3) displays the first stage results of the specification reported in Table 6 (col. 3); col. (4) displays the first stage results of the specification reported in Table 6 (col. 4); col. (5) displays the first stage results of the specification reported in Table 6 (col. 5) and col. (6) displays the first stage results of the specification reported in Table 6 (col. 6). The spatial weight matrix (W) used is of the type: contiguity-based and it is row-standardized. Robust standard errors are shown in parentheses.

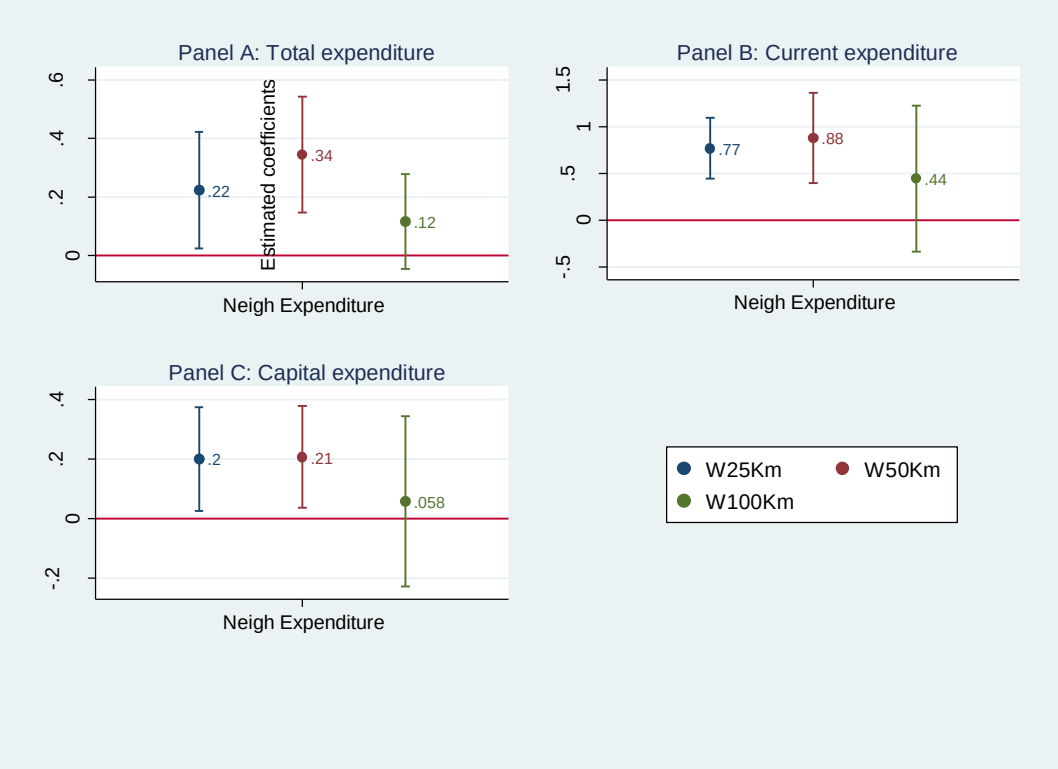
Table A2: Estimation results for yardstick competition model with the interaction between neigh expenditure and pre-Election and vote-share

PANEL A									
Weighting matrix	$W^{contiguity}$			W^{25km}			W^{50km}		
Dependent variable	Total	Current	Capital	Total	Current	Capital	Total	Current	Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Expenditure (-1)	0.32***	0.11*	0.50***	0.30***	0.12*	0.60***	0.31***	0.11	0.57***
	(0.05)	(0.06)	(0.09)	(0.05)	(0.06)	(0.11)	(0.05)	(0.07)	(0.10)
Neigh expenditure	0.21***	0.65***	0.39*	0.29***	0.75***	0.60**	0.33***	0.96***	0.40
	(0.05)	(0.18)	(0.21)	(0.08)	(0.18)	(0.28)	(0.08)	(0.23)	(0.28)
Neigh expenditure *pre-election	-0.04	-0.01	-0.07	-0.06	-0.01	-0.15	-0.08	-0.02	-0.17
	(0.04)	(0.02)	(0.08)	(0.05)	(0.03)	(0.10)	(0.07)	(0.06)	(0.13)
Transfers	0.36***	-0.30	0.08	0.36***	-0.31	-0.11	0.37***	-0.48	0.07
	(0.08)	(0.22)	(0.24)	(0.08)	(0.26)	(0.27)	(0.08)	(0.37)	(0.22)
Pre-election	87.43**	9.84	80.62**	113.37*	6.67	123.26*	136.75*	16.41	132.56*
	(44.20)	(17.15)	(38.01)	(61.61)	(23.81)	(50.77)	(79.60)	(42.47)	(62.57)
Constant	150.91**	101.03	11.21	55.77	39.43	-132.49	-62.81	-38.69	-92.39
	(54.16)	(109.26)	(65.66)	(75.27)	(114.52)	(117.41)	(79.14)	(118.20)	(131.92)
Observations	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640
Number of municipalities	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564
ar1p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
hansenp	0.278	0.394	0.278	0.267	0.208	0.707	0.165	0.135	0.677
ar2p	0.718	0.635	0.726	0.654	0.693	0.844	0.736	0.45	0.945
Number of instruments	32	32	32	32	32	32	32	32	32
PANEL B									
Weighting matrix	$W^{contiguity}$			W^{25km}			W^{50km}		
Dependent variable	Total	Current	Capital	Total	Current	Capital	Total	Current	Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Expenditure (-1)	0.28***	0.22**	0.29***	0.32***	0.21***	0.29***	0.33***	0.21*	0.33***
	(0.05)	(0.10)	(0.06)	(0.06)	(0.08)	(0.05)	(0.06)	(0.12)	(0.06)
Neigh expenditure	0.14	0.52	0.39	0.32	0.54	0.16	0.74*	-0.57	0.67
	(0.09)	(0.44)	(0.28)	(0.26)	(0.46)	(0.38)	(0.38)	(1.49)	(0.85)
Neigh expenditure *vote-share	-0.23	0.43	-0.43	0.05	0.80	0.17	-0.59	2.46	-0.72
	(0.15)	(0.58)	(0.45)	(0.43)	(0.62)	(0.65)	(0.64)	(2.03)	(1.43)
Transfers	0.52***	-0.72*	0.53***	0.35***	-0.64*	0.50***	0.36***	-0.47	0.48***
	(0.09)	(0.42)	(0.11)	(0.10)	(0.35)	(0.11)	(0.09)	(0.45)	(0.09)
Vote-share	396.61**	-254.00	273.31	5.21	-550.72	-46.65	814.35	-1,773.38	417.51
	(198.97)	(432.94)	(238.81)	(549.92)	(462.99)	(345.55)	(811.17)	(1,501.17)	(736.04)
Constant	29.63	309.64	-64.57	65.04	328.73	96.99	-549.86	1,058.86	-245.45
	(125.92)	(321.66)	(167.94)	(336.81)	(320.72)	(226.72)	(483.80)	(1,001.44)	(457.83)
Observations	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640	55,640
Number of municipalities	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564	5,564
ar1p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
hansenp	0.338	0.522	0.125	0.321	0.441	0.119	0.218	0.152	0.862
ar2p	0.900	0.418	0.730	0.679	0.436	0.641	0.794	0.520	0.339
Number of instruments	31	30	32	30	30	32	30	28	30

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors, clustered at the municipal level, are shown in parentheses. I control for time and municipal fixed effects, *children*10^3, aged*10^3, population*10^{-4} , area*10^3, income*10^{-3}, domestic stability pact, election, term-limit* and *vote-share* in all specifications in Panel A; while I control for time and municipal fixed effects, *children*10^3, aged*10^3, population*10^{-4} , area*10^3, income*10^{-3}, domestic stability pact, election, term-limit* in all specifications in Panel B. The variables *Expenditure (-1), neigh expenditure*, the interaction *neigh expenditure *pre-election* and *transfers* are always instrumented using SYS-

GMM, excluding for *Expenditure (-1)* in regression (2), (5) and (8) of Panel A which is instrumented using difference GMM. Instruments for specification in Panel A: (1) lags 1 and 2 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure* and *transfers* (total transfers), lags 4 and 5 for the variable *neigh expenditure*pre-election*; (2) lags 1, 2, 3 and 4 for the variable *Expenditure (-1)*, lags 7 and 8 for the variables *neigh expenditure*, lags 6 and 7 for the variable *neigh expenditure*pre-election* and lag 4 for the variable *transfers* (current transfers); (3) lag 4 for the variable *Expenditure (-1)*, lags 5, 6 and 7 for the variables *neigh expenditure*, lags 6 and 7 for the variables *neigh expenditure*pre-election* and *transfers* (capital transfers); (4) lags 1 and 2 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure* and *transfers* (total transfers), lags 4 and 5 for the variable *neigh expenditure*pre-election*; (5) lags 1, 2, 3 and 4 for the variable *Expenditure (-1)*, lags 7 and 8 for the variables *neigh expenditure*, lags 6 and 7 for the variable *neigh expenditure*pre-election* and lag 4 for the variable *transfers* (current transfers); (6) lag 4 for the variable *Expenditure (-1)*, lags 5, 6 and 7 for the variables *neigh expenditure*, lags 6 and 7 for the variables *neigh expenditure*pre-election* and *transfers* (capital transfers); (7) lags 1 and 2 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure* and *transfers* (total transfers), lags 4 and 5 for the variable *neigh expenditure*pre-election*; (8) lags 1, 2, 3 and 4 for the variable *Expenditure (-1)*, lags 7 and 8 for the variables *neigh expenditure*, lags 6 and 7 for the variable *neigh expenditure*pre-election* and lag 5 for the variable *transfers* (current transfers); (9) lag 4 for the variable *Expenditure (-1)*, lags 5, 6 and 7 for the variables *neigh expenditure*, lags 6 and 7 for the variables *neigh expenditure*pre-election* and *transfers* (capital transfers). The variables *Expenditure (-1)*, *neigh expenditure*, the interaction *neigh expenditure *vote-share* and *transfers* are always instrumented using SYS-GMM, excluding for *Expenditure (-1)* in regression (2), (5) and (8) of Panel B which is instrumented using difference GMM. Instruments for specification in Panel B: (1) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 2 and 3 for the variables *neigh expenditure* and *neigh expenditure*vote-share*, lag 2 for the variable *transfers* (total transfers); (2) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 8 and 9 for the variables *neigh expenditure*, lags 5 and 6 for the variable *neigh expenditure*vote-share*, lag 5 for the variable *transfers* (current transfers); (3) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure* and *transfers* (capital transfers), lags 4 and 5 for the variable *neigh expenditure*vote-share*; *(4) lags 1 and 2 for the variable* Expenditure (-1), *lags 3 and 4 for the variables neigh expenditure* and *neigh expenditure*vote-share*, lag 3 for the variable *transfers (*total transfers*);* (5) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 8 and 9 for the variable *neigh expenditure*, lags 5 and 6 for the variable *neigh expenditure*vote-share*, lag 5 for the variable *transfers* (current transfers); (6) lags 1, 2 and 3 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure* and *transfers* (capital transfers), lags 4 and 5 for the variable *neigh expenditure vote-share*; (7) lags 1 and 2 for the variable *Expenditure (-1)*, lags 3 and 4 for the variables *neigh expenditure* and *neigh expenditure*vote-share*, lag 3 for the variable *transfers* (total transfers); (8) lags 1 and 2 for the variable *Expenditure (-1)*, lags 8 and 9 for the variable *neigh expenditure*, lag 6 for the variable *neigh expenditure*vote-share*, lag 5 for the variable *transfers* (current transfers); (9) lags 1 and 2 for the variable *Expenditure (-1)*, lags 5 and 6 for the variables *neigh expenditure* and *neigh expenditure*vote-share*, lag 3 for the variable *transfers* (capital transfers). The validity of the instruments is checked by using the standard Hansen test and the C tests (results are available upon request).

Figure A1: Point estimates end confidence interval of neighbor expenditure with different weighting matrices



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Chapter 2

The political budget cycle of EU Funds' spending process: Evidence from Italian municipalities.

Abstract

This article focuses on the dynamic of the European Structural funds' utilization at municipal level. I investigate the presence of Political Budget Cycle (PBC) demonstrating that mayors exploit the possibility to implement projects in their territory in order to send out a signal for the electorate. I estimate the existence of a causal relationship between PBC and the probability to start or end at least one EU projects, between PBC and the number of projects started or ended and between PBC and fragmentation of the EU projects realized. I use information on 3,102 Italian municipalities over the period 2007-2014, also distinguishing between municipalities of *Convergence* objective regions and *Competition and employment objective* regions (objective's European classification for EU Structural Funds). I find evidence of the existence of PBC in the dynamic of EU structural funds' process, with differences related to the nature of projects realized linked to EU Objectives.

Key words: Political business cycles; European structural funds; local governments

JEL codes: D72; H76

2.1 Introduction

Since 1988 the European Union with the Single European Act has fixed, as one of its main objectives, the economic and social cohesion of European regions fostering the "harmonious and sustainable development of the Community" (Regulation 1083/2006). It has translated it in a series of policies, generally known as Cohesion policy, starting in 1989 and periodically re-confirmed with a progressive resources augmentation, trough the allocation of the Structural funds to member states.

The EU Structural funds have been the subject of many empirical works investigating different aspect of the theme. The most relevant strand of the empirical analysis on European Structural funds focused on the evaluation of the policy, studying if it achieved its desired goals (Becker, Egger and Von Ehlrich, 2010; Ederveen et al., 2006; Giua, 2016); anyway, other strand of research tries to put in evidence the existence of elements that enter in the process of allocation of Structural funds, maybe influencing their effectiveness (Becker, Egger and Von Ehlrich, 2012; Bähr, 2008).

In this work, I focus on a different aspect of the EU structural policy. I want to understand if Structural funds, such as other expenditure policy, can be influenced by the Political Budget Cycle. My goal is to assess the causal effect of Political Budget Cycle on the administration of Structural Funds at the municipal level distinguishing between municipalities of *Convergence* objective regions and *Competition and employment objective* regions. I believe that this analysis can fill a gap in the literature of Structural funds, along the way of the studies that want to identify elements influencing the dynamic of spending (Carrubba, 1997; Bouvet and Dall'Erba, 2010).

The efficacy in the use of Structural Funds can be difficult determined without taking in consideration the way the resources are used. Understanding the mechanism of spending can be helpful for national and regional policy makers to better allocate resources, also improving their efficacy. In accordance to the pattern established by the EU Funds regulation, the allocation process within the member states consists on a bargaining process that take place between sub-national governments: in the Italian context between National governments, Regions and other actors, the final funds recipients. Among recipient actors, a consistent group is composed by public entities, such as municipalities. For this reason, municipalities have an important role in the timing of use of Structural funds, they have gained participating to the allocation process. Furthermore, the use of Structural Funds in Italy has been often criticized[21], because of delays in the implementation of handled projects and consequently in the spending of the linked resources, even with the risk of incurring in the automatic decommitment consisting on the loss of the assigned resources.

I investigate the presence of Political Budget Cycle (PBC) to demonstrate that mayors exploit the possibility to implement projects in their territory in order to send out a signal for the electorate. I estimate the existence of a causal relationship between PBC and different variable accounting for the dynamic of investment of the structural funds

[21] Vanuzzo, 2012: http://linkiesta.it/italia-fondi-europei-cultura-spesi-male; Virno, 2014

resources: the probability to start or end at least one EU projects, the number of projects started or ended and the fragmentation of the EU projects realized.

The rest of the paper is organized as follows: Section 2.2 describes the regulatory framework of the Structural funds; Section 2.3 makes a review of the most important lines of literature on the EU structural funds; Section 2.4 illustrates the theoretical framework of the PBC applied in the context of this analysis. Section 2.5 describes the Dataset. Section 2.6, 2.7 and 2.8 show the empirical framework, the results and the robustness tests. Section 2.9 concludes.

2.2 The design of Structural Funds Allocation through Italian municipalities

The European Cohesion policy organizes its intervention to reach the economic and social cohesion of European regions with financial-transfers' plans that have a several-years financial perspective. The first action started in 1988 with a five-years plan (1988-1992) successively replicated in 1994 (1994-1999 plan). Later from the year 2000 the policy perspective extended to a seven-years framework and it has started the 2000-2006 plan, the 2007-2013 plan and the underway 2014-2020 plan. Every plan establishes the distribution of the Structural funds to the European National States and to Regions and it fixes different objectives they must reach.

Our period of interest is the 2007-2013 plan. The EU regulation named three objectives to be achieved in the period (2007-2013) with the use of the Structural Funds: the *Convergence*, the *Regional competitiveness and employment* and the *European territorial cooperation*. The *Convergence* objective was the priority of the funds in terms of amount of resources dedicated to it. Its scope was to speed up the convergence of the least-developed Member States and Regions by improving conditions for growth and employment with the investment in physical and human capital, the development of innovation and of the knowledge society, adaptability to economic and social changes, the protection and improvement of the environment, and administrative efficiency. The *Regional competitiveness and employment* objective, outside the least-developed regions, was aimed at strengthening regions' competitiveness and attractiveness as well as employment by anticipating economic and social changes, including those linked to the opening of trade, through the increasing and improvement of the quality of investment in human capital, innovation and the promotion of the knowledge society, entrepreneurship, the protection and improvement of the environment, and the improvement of accessibility, adaptability of workers and businesses as well as the development of inclusive job markets. Finally, the *European territorial cooperation* objective was aimed at strengthening cross-border cooperation through joint local and regional initiatives, strengthening transnational cooperation by means of actions conductive to integrated

territorial development linked to the Community priorities, and strengthening interregional cooperation and exchange of experience at the appropriate territorial level.[22]

The intervention level was identified with the NUTS 2[23] administrative level, which corresponds for Italy to the administrative Regions. The regulation established the criteria for eligibility in one or in other objectives. For the 2007-2013 plan, Regions were eligible in the *Convergence* objective if its per capita GDP in purchasing power parities was less than 75% of the EU-25 average GDP in the period 2000-2002 (Regulation 1083/2006 (17) and (18)). The *Regional competitiveness and employment* objective had the aim to cover the territory of the Community outside the Convergence objective, so Regions eligible in this objective were Regions with GDP over 75% of the Community average in the years 2000-2002. Finally, the regulation identified regions in a "transitional support": regions which would have had been eligible for *Convergence* objective status if the average GDP had been calculated for the EU-15 (as for the previous plan, the 2000-2006 plan), but which lose the eligibility because their nominal GDP per capita level exceeded 75 % of the average GDP calculated for the EU-25 country (Phasing out region). In the opposite case, Regions that won the eligibility because of the new formula, were defined Phasing-in regions. In Italy for the considered 2007-2013 plan, I have for the *Convergence* objective, Regions of Puglia, Calabria, Sicilia and Campania, the remaining Regions participated in the *Competitiveness and employment* objective, except for Sardegna that was a Phasing in region and for Basilicata who was a Phasing out region.

At the financial level, European Union settled different instruments to transfer resources to national and regional governments. I can identify the European Regional Development Fund (ERDF), the European Social Fund (ESF) and the Social Cohesion Fund, all organized to finance the objectives described above. The *Convergence* objective was financed by the ERDF, the ESF and the Cohesion Fund; the *Regional competitiveness and employment objective* by the ERDF and the ESF; the *European territorial cooperation* objective by the ERDF. Furthermore, it was up to each Member State providing co-financing resources: the European regulation established that Europe's financing share might not exceed the ceiling of 75 per cent of public expenditure for the *Convergence* objective and 50 per cent for the *Competitiveness and employment* objective. The total amount of European resources allocated to Italy for the 2007 – 2013 plan was 28.7 billion euro, split into 21.6 billion in the *Convergence* objective, 6.3 billion euro in the *Regional competitiveness and employment* objective and 0.8 billion euro in the *European territorial cooperation* objective.

At the organization level, each Member State of the European Union implemented a program formalized in a document: the National Strategic Reference Framework (NSRF). The NSRF was declined in Regional Operational Programs (POR), in National Operational Programs (PON) and in Inter-Regional Operational Programs (POIn), which are competence of different administrative actors. In Italy, the Comitato interministeriale

[22] Regulation (EC) No 1080/2006 of the European Parliament and of the Council of 5 July 2006 on the European Regional Development Fund and repealing Regulation (EC) No 1783/1999 [Official Journal L 210 of 31.7.2006]

[23] European Union divides Europe in three territory level: NUTS1, macro areas, NUTS2, corresponding to Italian Regions and NUTS3 corresponding to Italian provinces.

per la programmazione economica (CIPE) coordinated National and European economic policies defining priorities and strategies to reach the macro-objective fixed by the European Union in the regulation of Structural Funds, instead each Regions set operational programs. The Italian NSRF for the 2007-2013 considered 4 macro-objectives declined in 10 priority lines[24]. Each region, with the POR, planned tools to detail the way in which they intended to reach the priority axis of the NSRF, coherently with the master objectives. Players of the territories (privates, profit and no profit organizations, firms and public entities) are generally invited to participate to projects fixed at regional level. These recipient actors can obtain funds through invitations to tender ending with ranking list, for public entities generally realized as call for projects. Regions can establish the type of projects and decide who can participate to invitations to tender. Public entities can participate to the tender to implement projects related to the main priorities fixed by the Region. Municipalities were free to participate or not to participate to the invitation to tender, depending on its political objectives or on its finances.

As described above European structural funds had to be co-financed by the member states. The co-financing resources can be find at different level of governments: national, regional, and municipal. The Cipe resolution determined new financing quote, with respect to the European regulation: all the projects should be financed by the nation at the 50%, thus providing for a much higher co-financing in the Convergence objective compared to the national minimum ceiling of 25% established by the European regulation.

In Italy, the delay in the utilization of European resources has assumed relevance also in the public opinion, considering that many resources risked being lost because of the automatic decommitment rule[25] (*disimpegno automatico*). Municipalities in Italy play an important role in the Structural Funds utilization. Analyzing information deriving from the dataset (Table A1-A2-A3 in the Appendix), almost 40% of Italian municipalities receive resources to develop at least one project in the considered period. This percentage is composed by 78% of municipalities belonging to the *Convergence* objective, the leftover 22% is municipalities belonging to *Competition and employment* regions. Overall, municipalities were defined project implementer in the 2007-2013 plan for about

24 Priority axis 1: Improving and enhancing human resources (Miglioramento e valorizzazione delle risorse umane). Priority axis 2: Promotion, valorisation and dissemination of research and innovation for competitiveness - Promozione, valorizzazione e diffusione della ricerca e dell'innovazione per la competitività. Priority axis 3: Energy and environment: Sustainable and efficient use of development resources - Energia e ambiente: uso sostenibile e efficiente delle risorse per lo sviluppo. Priority axis 4: Social Inclusion and Services for Quality of Life and Territorial Attractiveness - Inclusione sociale e servizi per la qualità della vita e l'attrattività territoriale. Priority axis 5: : Valorization of natural and cultural resources for attractiveness and development - Valorizzazione delle risorse naturali e culturali per l'attrattività e lo sviluppo. Priority axis 6: Network and mobility - Reti e collegamenti per la mobilità. Priority axis 7: Competitiveness of production systems and employment - Competitività dei sistemi produttivi e occupazione. Priority axis 8: Competitiveness and attractiveness of cities and urban systems - Competitività e attrattività delle città e dei sistemi urbani. Priority axis 9: International opening and attraction of investment, consumption and resources - Apertura internazionale e attrazione di investimenti, consumi e risorse. Priority axis 10: Governance, institutional capacity and competitive and effective markets - Governance, capacità istituzionali e mercati concorrenziali ed efficaci.

25 COUNCIL REGULATION (EC) No 1083/2006, article 93. "The Commission shall automatically decommit any part of a budget commitment in an operational programme that has not been used for payment of the pre-financing or interim payments or for which an application for payment has not been sent in conformity with Article 86 by 31 December of the second year following the year of budget commitment under the programme…"

12,000 projects[26] realized with the co-financing part of the European funds. I can notice some differences in the type of project realized and in their nature if I consider municipalities of *Convergence* regions and others. For example, in case of municipalities belonging to *Competition and employment* regions, the 29.39% of projects belong to the priority axis 7 *Competitiveness of production systems and employment*, with respect to 3.97% in case of municipalities belonging to *Convergence* regions (Table A2, Appendix). These differences are mainly related to the EU structural objective's vocation: in fact, *Convergence* objective can be realized implementing public investments capable to reduce differences of GDP between regions.

2.3 Brief literature review of EU structural funds

The topic of EU Structural funds has been object of many studies. An important branch of this literature focused on quantifying the impact of structural funds on the growth process of European regions, that is the main declared objective of the Cohesion policy, measured in term of GDP or employment growth.

The literature has not been completely in agreement to the effectiveness of the Structural funds. Mohl and Hagen (2010) show in their work a complete literature review of the most important studies starting from the year 2001. They confirmed that the empirical evidence has provided mixed and contradictory results, in fact "while some authors do find evidence of a positive impact of structural funds on economic growth others only find a weakly positive, no statistically significant or even a negative impact". Anyway, they underline that the estimation results described until that moment might be biased due to the use of imprecise data or because of the reverse causality. In Giua (2017) I can find a complete survey of the literature on the topic classifying papers by estimation model and technicalities used in the analysis. Some studies used causal relationship in the estimations to investigate the effectiveness. An example is Becker, Egger and Von Ehrlich (2010) who tested the effectiveness of the Convergence objective for EU regions with a lower GDP than European average. They found positive per capita GDP effects of Objective 1 funds, but no employment growth effect, analyzing three programming periods (1989–1993, 1994–1999, 2000–2006), for NUTS2 and NUTS3, with a regression discontinuity design. Giua (2017) shows that the EU Structural funds produced a positive impact on employment levels, without any displacement of economic activities away from nontreated regions and a concentration of the impact in key economic sectors. This paper exploits administrative boundaries as spatial discontinuity to estimate the causal effect of this policy on the Italian Objective 1 regions' employment. In a second work Becker, Egger and Von Ehrlich (2012) use data at the NUTS3 level for the period plans 1994–1999 and 2000–2006 to understand weather more transfers caused stronger growth effects, to understand if there are determinants that foster growth. They find that some reallocation

26 This information derived from the row data collected from the open-source database "Opencoesione", where are freely published all the information linked to the projects realized in Italy. See Section 2.5 for information about the transformation of row data to the set of values used in the paper's analysis.

of the funds across target regions would lead to higher aggregate growth in the EU and could foster convergence in a higher way than the current allocation scheme. In conclusion, a part of the literature tends to be confident in the effectiveness of Structural Fund in reaching their goals.

The effect become stronger when I consider that there are some variables conditioning the effect of the policy. In Ederveen et al., (2006) the effect of Structural funds on growth at national level is estimated to be effective when they introduce in the analysis some conditioning variables measuring institutional quality such as trade openness, inflation, trust, corruption. They find a significant and positive effect on the interaction term between growth and the just defined variables. A different point of view of the phenomenon comes from Bähr (2008) who introduces in the analysis of structural funds effectiveness the level of sub-national autonomy, with the idea that decentralization can have an influence on it. Regions, in fact, have a crucial role in the allocation and utilization of the communitarian resources and at the same time they are more sensitive to specific needs of their communities and the way to stimulate an economic growth; for this reason, his analysis investigated whether decentralization fosters the funds effectiveness, finding a significant positive effect of Structural funds on growth *when interacted with a decentralized structure of country*.

Given the allocation process above described, politics can have a certain influence on Structural funds. In a seminal paper, Carrubba (1997) tests the existence of political explanation in the transfers' decisions applied to the Structural funds. The paper concentrates on identifying motivations that push politicians to demand for structural funds, especially in the case of the richest member states. He supposes and tests the existence of a pareto efficiency equilibrium with money transfers and integration of the European Union, finding that domestic political conditions influence transfer level. This implies that transfers foster integration process. Anyway, I must consider that governing politicians are interested in the effect that its decisions have on their chance of re-election, which is strictly related to its electorate's beliefs. Bouvet and Dall'Erba (2010) want to identify economic or political variables that are bases of the funds allocation process. A key element of their study is the presence of political bargaining in the allocation; they focus on the two-stages process. Their results suggest that funds allocation is influenced by political considerations, even if it varies across different objectives. The allocation process of the structural funds between different level of governments can "deviate from neutral allocations solely based on economic considerations" (Bouvet and Dall'Erba, 2010), considering that political bargaining is part of it. A typical example of distortion is due to the political alignment between level of governments: the funds are used to capture votes in regions where the position of the party in charge in the first level is weaker (Solé-Ollé and Sorribas-Navarro, 2008).

The aim of my paper is to show that the spending dynamic of the structural funds by the final institutional users can be influenced by different elements, and in this specific case I test the mayors' behavior with respect to the political budget cycle. I believe that testing the presence of this dynamic in the use of Structural funds' resources by municipalities

can be an important key-element in further analysis in the effectiveness of the Structural Funds.

2.4 The political budget cycle

Generally, when we talk about PBC, I refer to the "opportunistic politician's behaviour aiming to increase their re-election probabilities" through fiscal and monetary manipulation (Snowdon & Vane, 2005). Furthermore, the Political Budget Cycle Theory stated that some components of the public budget, independently from the level of government, are influenced by the electoral cycle; consequently, the year nearest the elections may cause an increase in government spending or a decrease in taxes, even leading to larger fiscal deficit, because the incumbent uses the fiscal manipulation as a tool for influencing voters and increasing their changes for re-election (Youssef, 2012).

The literature proposed different model to explain the PBC theory. The PBC can arise due to information asymmetries about the incumbent's competence in administrating the production of public goods. In this model observed pre-election expenditures may serve as signal of the incumbent's competence. Voters are initially uninformed about the type of incumbent, whereas the incumbent knows his own type. This means that the incumbent has a temporary information advantage over voters, in this sense that he sees his competence shock contemporaneously (Rogoff, 1990).

Another PBC model is the moral hazard model. In this case voters are rational and they understand the incumbent's incentive to increase the deficit before an election. Also, governments are more rational actors than voters, they will comply with the voters' expectations and indeed increase the deficit. The main idea in this model is that voters expect increases in deficit before the election. In this circumstance, the questions that whether incumbent will increase the deficit or not, and what it will spend the borrowed funds on, become very important. If voters do not observe an increase in government expenditures, they will assume that the resources were spent inefficiently (Shi & Svensson, 2006).

Many researchers empirically investigate the existence of the PBC at the local level starting from the Rogoff's model (Rogoff, 1990). One of the first study is that of Blais and Nadeau (1992) which test the PBC in the Canadian provinces, finding its presence for the expansion of social and road expenditures. Veiga and Veiga (2007) test prediction of PBC using Portuguese municipalities using the GMM estimator to avoid estimation's inconsistency; they find strong evidence of rational opportunistic PBC in all the dependent variables used (budget balance, taxes and total, capital, investment expenditures). In the Italian context Alesina and Paradisi (2017) find evidence of PBC at the municipal level studying the effect of the mayors' opportunistic behaviour in the tax rate. They exploit a natural experiment, such as the introduction of a new local tax in the year 2011, finding that the PBC budget is stronger in smaller municipalities and in the South. Other authors focus on the topic of Italy: Bonfatti and Forni (2016), Bartolini and Santolini (2009).

Anyway, as this behaviour causes a cyclical fluctuation in fiscal policies induced by the timing of elections, I expect that the same effect can be tracked in the dynamic of administration of the EU Structural funds at the municipal level, where mayors exploit the possibility to develop projects financed by external resources to improve their probability of being re-elected. The Italian electoral reform established that mayors and city council are directly elected for five-year terms and are subject to a two-term limit. In the sample period, coinciding with the 2007-2013 plan of the Structural funds, for each municipality I observe generally two pre-election years, in case of regular election period.

Considering that my research concentrates on the investigation of the Political Budget Cycle (PBC) in the dynamic of municipal EU projects and in the relation between mayors' decisions in the context of European structural funds and the dynamic of election, characterized by the competition on citizens' consensus in their campaign; municipalities play an important role as final consumers in the value chain of the structural funds.

Municipalities in Italy are responsible for a large array of important public programs in the field of welfare services, territorial development, local transport, infant school education, sports and cultural facilities, local police services, as well as most infrastructural spending. They can use the European funds, participating to the Regional operational program as described in the previous section, to develop their territory, reinforce the action in their core functions and in other objective established by regions. In my intuition, mayors exploit the possibility to develop projects partially financed by external resources to satisfy their own citizens and gaining political objectives, such as the re-election. They can have some degrees of freedom in the use of the funds. They decide to participate to the invitation to tender augmenting the probability of receive financing resources and to start projects in a fixed year and its number. They can decide the timing of realization of the project: when the European project had to start or to finish, given some general dynamics fixed by the region. So, they can manipulate the launch date, after having won the tender, deciding to when effectively starting the project and they can speed up or slow down the evolution of the projects following the electoral cycle. In fact, even the end date of the project can have an important relevance: mayors that finish a project show effectiveness in realizing her policy and can show a tangent result to their citizens.

The political economy of structural funds could be complex to be identified because of the presence of highly complex institutional process in its allocation between three level of governments: national, regional and municipal level (Bondestein, Thilo and Achin Kemmerling, 2011). Anyway, in this paper I am not interested in the inter-relationship between the level of governments and the allocation of funds across them; I want to focus on the timing-use of funds by the final user, in this case municipalities. This moment, it has been reached after a bargaining process. So, given that the municipality *i* receives a certain quantity of funds in the period of the plan (2007-2012), I am interested in the moment she decides to start using it and/or she decides to close the project. I believe that this aspect can be the cause of the delay in Structural Funds resources' expenditure, often causing the loss of the European financing.

2.5 Dataset and variables

My empirical analysis is based on data on European projects managed by municipalities and financed by Structural Funds. I collected these data from the open-source database "Opencoesione"[27], where are freely published all the information linked to the projects realized in Italy. Such as the amount of resources invested by Europe and the level of co-financing by funders EU, State, Region, Province, Municipality, the type of objective to whom the project belongs, the priority axis under which the project is realized, progress in term of payment and in term of realization. For the aim of my research I do note that key-elements of this database are the launch date, the end-date of projects and the type of implementer. After having selected as implementer only municipalities, excluding all other entities developing projects, I made a re-elaboration of information to obtain a panel dataset running from 2007 to 2014[28] for Italian municipalities which have at least activated one EU project financed by Structural funds.

The re-elaboration of the data consists on using as year of panel, the year of the launch date of each project to impute it (I will refer to this dataset in the analysis as *Dataset 1 – LAUNCH date project year*). Alternatively, in a second analysis I re-built another dataset starting from the end-date of the project (I will refer to this dataset in the analysis as *Dataset 2 – END-date project year*). Independently from the date I decide to use, the launch date or the end date of the project, once each project was related to a year and to a municipality implementer I can build a panel collapsing data by year and municipality. The variables of interest at municipal aggregate level per year that I obtain elaborating the information from the initial dataset downloaded from Opencoesione, are the number of project started/ended in year *t* by municipality *i*, the probability of started/ended projects in year *t* by municipality *i*, the amount of funds obtained by the municipality and invested in the project.

To complete the dataset, I implement it with electoral data covering the results of elections in which the mayors in office during the period covered by the dataset were elected. I build up a dummy variable equal to one in the year before the election to use it as independent variable, as proxy of the Political Budget Cycle. The referred period is 2007-2014 that corresponds to the 7-year – 2007-2013 - EU budget program plus one year in which municipalities can yet spend funds already attributed to them by regions.

I included some municipal demographic and socio-economic characteristics such as population size, age structure, average income of inhabitants, a dummy accounting for the Domestic stability pact, in order to control for municipalities' characteristics. I excluded data of autonomous regions, except for Sardegna (Valle d'Aosta, Trentino Alto Adige, Friuli Venezia Giulia and Sicilia), for which I did not have available electoral data.

[27] http://www.opencoesione.gov.it/
[28] 2014 is out of the 2008-2013 plan, anyway municipalities can yet spend funds already attributed to them by regions.

Table 1. Distribution of observations per year.

Year	*Number of observations*	*Municipalities going to election*	*Percentage of municipalities going to election*
2007	2,602	500	3,102
2008	2,827	275	3,102
2009	1,740	1,362	3,102
2010	2,643	459	3,102
2011	2,477	625	3,102
2012	2,611	491	3,102
2013	2,792	310	3,102
2014	1,781	1,311	3,092
Total	19,473	5,333	24,806

At the end, I obtained a sample of 3,102 municipalities including 24,806 observations from 2007 to 2014 (*Table 1*). I introduce a dummy variable equal to 1 if the municipality belongs to a region eligible in the *Convergence* objective, and equal to 0 if the region belongs to the *Competitiveness and employment* objective to evaluate the causal effect of PBC in the two group of municipalities, considered that there are some differences between them in term of amount of resources and objectives to reach. Observations for the Convergence recipient regions were 16,195, and 8,611 for Competition recipient regions.

2.5.1 Dependent variables

I am interested in identifying the effect of the Political Budget Cycle on the use of structural funds by municipalities. Hence, as dependent variables I use variables[29] obtained collapsing data in the analysis:

- a dummy variable equal to one if the municipality *i* started at least one project in the year *t*, probability to start/end;
- the number of projects started in year *t* by the municipality *i*;
- an index of fragmentation; the Herfindahl index: $Index_{HH} = \sum_{i=0}^{n}(\frac{value\ of\ project_{it}}{\sum value\ of\ project_{it}})^2$. It varies from 0 to 1: when the limit tends to 0 I have high fragmentation and if the limit is equal to 1 there is no fragmentation.

[29] We have to mention that for variables described (except for the fragmentation index) we replaced the missing value with 0. We retained that if municipality *i* in year *t* does not start a project we need to consider it as a year with 0 number of started projects, 0 probability to start at least one project. On the contrary, when we use the fragmentation index we exclude the missing values.

I report summary statistics for the dependent variables described above in Table 2. The first section of the table shows results for the *Dataset 1 – LAUNCH date project year*, instead the second section for the *Dataset 2 – END-date project year.*

The probability of starting at least one project is in mean for the period equal to 0.177, higher than the probability of ending a project equal to 0.112. If I look at the statistics per year (Table 3) I notice that the probability of starting or ending a project grows up until the year 2012, from 0.038 in 2007 to 0.302 in 2012 for the *Dataset 1 – LAUNCH date project year* and from 0.001 in 2007 to 0.205 in 2012 for the *Dataset 2 – END-date project year.* This dynamic is certainly due to organizational and administrative procedure of funds allocation and management. In the first years of the 2007-2013 plan the national governments and regions must implement the procedure to allocate the funds and successively municipalities must take necessary actions to reach the goal. The probability of ending a project is general lower than that of starting it, this put in evidence a certain inability of municipality to conclude the project started in the allowed time of the 7-year plan.

The number of project started per 1,000 inhabitants is in mean equal to 0.109 (Table 2, section 1), instead of a mean of 0.064 for the project ended per 1,000 inhabitants (Table 2, section 2). If I analyse the mean per year, also for this variable, I notice that the value grows up until a certain year: 0.28 is the mean value of the started project in 2010 (Table 3, section 1), and 0.184 is the mean value of the ended project in 2011 (Table 3, section 2). The fragmentation index is likely to be around the 0.90, this means that there is low fragmentation in the use of structural funds. It is in mean equal to 0.925 (Table 2, section 1), instead of a mean of 0.941 for the project ended (Table 2, section 2); the fragmentation is higher in the lunch date project.

Table 2. Summary statistics – Dependent variables

Variable	*Number of observations*	*Mean*	*Std . Dev.*	*Min*	*Max*
	Dataset 1 – LAUNCH date project year				
Dummy funds	24,806	0.177	0.382	0	1
Number of projects (per 1000 inhabitants)	24,806	0.109	0.491	0	17.241
Fragmentation Index	4,386	0.925	0.177	0.075	1
	Dataset 2 – END-date project year				
Dummy funds	24,806	0.112	0.315	0	1
Number of projects (per 1000 inhabitants)	24,806	0.064	0.362	0	12.500
Fragmentation Index	2,779	0.941	0.157	0.093	1

Table 3. Value mean by year and by objective – Dependent variables

Dataset 1 – LAUNCH date project year

						EU funds' Objective			
	Total			***Convergence***			***Competitiveness***		
Year	Dummy funds	Number of projects (per 1000 inhabitants)	Fragmentation Index	Dummy funds	Number of projects (per 1000 inhabitants)	Fragmentation Index	Dummy funds	Number of projects (per 1000 inhabitants)	Fragmentation Index
2007	0.038	0.022	0.949	0.054	0.019	0.946	0.03	0.024	0.952
2008	0.045	0.022	0.934	0.066	0.031	0.972	0.035	0.018	0.896
2009	0.167	0.074	0.944	0.209	0.080	0.909	0.144	0.071	0.97
2010	0.269	0.175	0.919	0.264	0.117	0.90	0.272	0.207	0.929
2011	0.372	0.28	0.935	0.33	0.155	0.897	0.395	0.347	0.952
2012	0.302	0.20	0.906	0.342	0.151	0.879	0.28	0.226	0.923
2013	0.173	0.076	0.918	0.215	0.087	0.919	0.151	0.071	0.917
2014	0.048	0.02	0.938	0.052	0.018	0.97	0.045	0.022	0.919

Dataset 2 – END-date project year

						EU funds' Objective			
	Total			***Convergence***			***Competitiveness***		
Year	*Dummy funds*	*Number of projects (per 1000 inhabitants)*	*Fragmentation Index*	*Dummy funds*	*Number of projects (per 1000 inhabitants)*	*Fragmentation Index*	*Dummy funds*	*Number of projects (per 1000 inhabitants)*	*Fragmentation Index*
2007	0.01	0.006	0.978	0.015	0.004	0.989	0.007	0.007	0.967
2008	0.018	0.013	0.968	0.031	0.015	0.986	0.012	0.012	0.942
2009	0.061	0.023	0.954	0.08	0.024	0.98	0.051	0.023	0.933
2010	0.124	0.053	0.914	0.249	0.104	0.91	0.057	0.026	0.922
2011	0.256	0.184	0.962	0.233	0.092	0.927	0.268	0.233	0.979
2012	0.205	0.126	0.935	0.254	0.112	0.905	0.179	0.134	0.958
2013	0.138	0.051	0.924	0.167	0.057	0.907	0.122	0.048	0.936
2014	*0.084*	*0.055*	*0.94*	*0.015*	*0.006*	*1*	*0.121*	*0.08*	*0.936*

2.6 Empirical analysis and results

Our economic strategy is based on the estimation of the PBC in the dynamic of EU municipal projects. I set up a regression equation of this form:

$$x_{it} = \alpha_i + \gamma_t + \beta_1 * PBC_{it} + \varepsilon_{it} \qquad (1)$$

The dependent variable x_{it} takes the values of alternative variables described in the section 2.5 (Dummy variable, number of project per capita, Fragmentation index starting or ending in the year *t* by the municipality *i*). The variable γ_t is the year fixed effects, α_i is the unobserved time-invariant municipal effects and ε_{it} is the error term. The variable PBC_{it} is the dummy identifying the pre-electoral year.

The analysis is based on the concept of causality. As described in Angrist and Pischke (2008) the causal relationship, in this case, between PBC and dependent variables such as the number of projects started by the municipality *i* at year *t*, tells us how many projects would start, on average, in the year of pre-election compared with other years of the electoral mandate. This comparison can be defined causal if the assumption of independence is guaranteed. If it is valid it ensures that the causal variable of interest is independent of potential outcomes so that the groups being compared, project started/ended in the year of pre-election and projects started/ended in year of no pre-election, are truly comparable. The independence assumption is guaranteed by the condition of random assignment. In this case the PBC, approximated by a dummy equal to 1 if the year is the year before the election, is randomly assigned. I retain that that the year of election cannot be modified by the mayors and depends on a historical dynamic at national level, moreover municipal elections are not scheduled in the same year for each municipality. I can confidentially interpret the coefficient of the regression as a consequent effect of PBC on dependent variables, namely causal effect. Furthermore, if the treatment is random, the point estimate should not change by adding more controls (Angrist and Pischke, 2008) for this reason I don't need to control for other variables, even if this can be useful in the robustness check on the claim of random assignment.

2.6.1 Estimation results - Dataset 1 – LAUNCH date project year

I first estimate equation (1) by using *Dataset 1 – LAUNCH-date project year* (Table 4) applying the FE estimator including controls for year dummies. The regression in column (1a, Table 4) shows the effect of the pre-electoral year (PBC) on the dummy variable equal to 1 if the municipality *i* in year *t* starts at least one project; the coefficient β is statistically significant at 1% and it is equal to 0.0238. This result says that the PBC influences directly the probability to start a project with respect to other years of the mayor mandate. I re-estimate equation (1) on the dummy variable by using the Probit model (Table 4, col. 1b) for a binary response, in this case the coefficient β is statistically significant at 10% and it is equal to 0.0491, the average marginal effect, I computed, is equal to 0.0113, meaning

that being in the year before the election increases the probability of start at least one project by 0.0113.

The causal effect of the pre-electoral year is also confirmed when I use as dependent variable the number of project per 1000 of inhabitants (col. 2 table 4): in the year before the election the number of started projects increases of 0.0208 per 1000 inhabitants (the coefficient is statistically significant at 5%). The regression in column (3) shows the causal effect of PBC on the fragmentation index, the coefficient β is negative, -0.0262 statistically significant at 1%, the fragmentation of projects increases in the pre-election year; it seems that PBC influences not only the quantity of the project but also its magnitude, to capture the consensus the quantity of projects to realize is important and instead of its magnitude.

These results suggest that it exists a causal relationship between the PBC and the dynamics of the use of European structural funds. My interpretation, in the context of the PBC literature, is that mayors use European resources and the possibility to make projects and investment on its own territory in order to influence voters, to obtain consensus and to win elections.

Table 4. Estimation results with FE estimator using the Dataset 1 – LAUNCH-date project year.

	(1a)	(1b)	(2)	(3)
VARIABLES	*Dummy Funds FE model*	*Dummy Funds Probit model*	*Number of projects (per capita)*	*Fragmentation Index*
PBC (pre_elec)	0.0238***	0.0491*	0.0208**	-0.0262***
	(0.00651)	(0.0263)	(0.0101)	(0.00962)
Year_2008	-0.000936	0.0656	-0.00751	-0.0392
	(0.00528)	(0.0579)	(0.00600)	(0.0309)
Year _2009	0.127***	0.803***	0.0505***	-0.0537**
	(0.00722)	(0.0495)	(0.00681)	(0.0256)
Year _2010	0.228***	1.153***	0.151***	-0.0962***
	(0.00869)	(0.0481)	(0.0125)	(0.0267)
Year _2011	0.333***	1.445***	0.256***	-0.116***
	(0.00946)	(0.0475)	(0.0149)	(0.0266)
Year _2012	0.263***	1.254***	0.178***	-0.122***
	(0.00884)	(0.0478)	(0.0130)	(0.0267)
Year _2013	0.127***	0.818***	0.0471***	-0.0842***
	(0.00780)	(0.0500)	(0.00787)	(0.0271)
Year _2014	0.0115**	0.109*	-1.15e-05	-0.0407
	(0.00505)	(0.0568)	(0.00529)	(0.0290)
Constant	0.0359***	-1.778***	0.0206***	1.024***
	(0.00395)	(0.0416)	(0.00514)	(0.0246)
Observations	24,806	24,806	24,806	4,386
R-squared	0.118		0.041	0.036
Number of codice	3,102		3,102	2,525

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1; all regressions present time and municipal fixed effect; population used in per capita variable is divided by 1000.

In table 5 I report the result of the estimations introducing an interaction term ($PBC_{it} * conv_i$) composed by the pre-electoral year dummy and the dummy *Convergence,* equal to one if a municipality belongs to a *Convergence* objective region. This variable does not vary in the time, but only in individuals. The equation assumes this form:

$$x_{it} = \alpha_i + \gamma_t + \beta_1 * PBC_{it} + \pi_1 * PBC_{it} * conv_i + \varepsilon_{it} \qquad (2)$$

In this way, I capture the differential effect in the behaviour of mayors in case its municipality belongs to a *Convergence* recipient region, such as Calabria, Campania, Puglia and Basilicata, or in case its municipality belongs to a *Competitiveness and employment* objective regions. Looking at the estimates for the Dummy funds (col. 1a, table 5), the pre-election dummy stays statistically significant at 1% and positive (0.0345),

but the estimated coefficient of the interaction which capture the differential effect of PBC on a Convergence municipality is negative and statistically significant at 5% (-0.0300). It means that for the municipalities of the Convergence regions the PBC have a lower effect on the probability of starting projects. Estimation of equation (2) when I use the dependent variable Dummy funds by using the Probit model (Table 5, col. 1b) for a binary response, shows not statistically significant results.

The same direction is suggested by the estimated coefficient of $PBC_{it} * conv_i$ when I use as dependent variable the number of project per 1000 inhabitants. Also in this case (col. 2, table 5) the pre-election dummy is positive (0.0385) and statistically significant at 1% but the differential results are negative and statistically significant at 1% (-0.0500).

The difference between municipalities belonging to *Convergence* regions and municipalities belonging to *Competitiveness and employment* regions in the estimation can be probably due to the type of implemented projects (Table A2). In municipalities belonging to *Convergence* regions the 83.31% (Table A3) of the projects are classified as *public works*, instead of the 47.17% of the projects of municipalities belonging to *Competitiveness and employment* regions. In fact, a great part of the projects developed by municipalities of *Competitiveness and employment* regions is related to *concessional contribution to economic actors* (37.79%), *purchase of goods* and *realization of services* (respectively 5.31% and 9.06%). This nature can have an influence on the duration of the projects but also in the possibility to be manipulated by mayors. If I think about the necessary procedure to implement and realize public works, I can imagine that the duration of this type of projects can be longer in time than contribution or purchase of goods or services. Furthermore, contributions or purchases of goods and services have a visible effect on citizens from the moment of their attribution that coincides with the beginning of the projects. On the contrary public works have a longer process and the announcement effect of starting a work could not be used by mayors to influence the electorate consensus.

Having regard to this analysis I can explain the results obtained in the regression of Table 5: the effect of PBC on EU structural projects is lower than in case of municipalities belonging to *Convergence* regions because mayors do not exploit investments for their mandate objectives in the year in whom projects started. On the contrary, in municipalities belonging to *Competitiveness and employment* regions the political effect of their type of projects is already visible in the year when the projects started and mayors exploit them to convince citizens to vote her.

Finally, if I consider the result reported in column 3 (table 5), I only found a negative statistically significant coefficient of the pre-electoral dummy (-0.0363) on the Fragmentation Index, confirming the result obtained in the basic analysis (col. 3 table 4). The interaction has not relevance in the estimation, this shows that the effect of PBC on fragmentation is the same in the two group of municipalities.

Table 5. Estimation results with FE estimator using the Dataset 1 – LAUNCH-date project year with Objective specification: Convergence and Competitiveness and employment regions.

	(1a)	(1b)	(2)	(3)
VARIABLES	*Dummy Funds FE model*	*Dummy Funds Probit model*	*Number of project*	*Fragmentation Index*
PBC (pre_elec)	0.0345***	0.0351	0.0385***	-0.0363***
	(0.00770)	(0.0315)	(0.0142)	(0.0121)
*PBC*conv (pre_ele_conv)*	-0.0300**	0.0379	-0.0500***	0.0238
	(0.0125)	(0.0467)	(0.0154)	(0.0196)
Year_2008	-0.00242	0.0678	-0.00998	-0.0368
	(0.00531)	(0.0579)	(0.00631)	(0.0308)
Year _2009	0.127***	0.804***	0.0498***	-0.0525**
	(0.00721)	(0.0495)	(0.00682)	(0.0256)
Year _2010	0.228***	1.153***	0.151***	-0.0962***
	(0.00870)	(0.0481)	(0.0125)	(0.0266)
Year _2011	0.333***	1.445***	0.256***	-0.116***
	(0.00946)	(0.0475)	(0.0149)	(0.0266)
Year _2012	0.263***	1.254***	0.177***	-0.122***
	(0.00884)	(0.0478)	(0.0130)	(0.0266)
Year _2013	0.126***	0.819***	0.0446***	-0.0846***
	(0.00785)	(0.0500)	(0.00814)	(0.0271)
Year _2014	0.0111**	0.110*	-0.000618	-0.0407
	(0.00505)	(0.0568)	(0.00527)	(0.0290)
Constant	0.0363***	-1.779***	0.0212***	1.024***
	(0.00395)	(0.0416)	(0.00509)	(0.0245)
Observations	24,806	24,806	24,806	4,386
R-squared	0.119		0.042	0.036
Number of codice	3,102		3,102	2,525

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1; all regressions present time and municipal fixed effect; population used in per capita variable is divided by 1000.

2.6.2 Estimation results - Dataset 2 – END date project year

The second part of the analysis consists on re-estimating the previous model by using the *Dataset 2 – END-date project year* (table 6) always applying the FE estimator (and the Probit model in case of a binary variable, such as the *Dummy funds*) including controls for year dummies. The regression in column (1a, table 6) shows the effect of the pre-electoral year (PBC) on the dummy variable project that it is equal to 1 if the municipality *i* in year *t* concludes at least one project; the coefficient β is statistically significant at 1% and it is equal to 0.0134. This result says that the PBC influences directly the probability of ending a project of about 1.34% respect to other years of the mayor mandate. The

regression in column (1b, table 6) estimating with a Probit model does not show any significant results. The causal effect of the pre-electoral year is not confirmed when I use as dependent variable the number of project per 1000 of inhabitants (col. 2 table 6) and the fragmentation index (col. 3, table 6), the coefficient β in the two cases is not statistically significant, meaning that the way in which project are fragmentated at their conclusion and the number of projects finished is not influenced by the PBC.

Table 6. Estimation results with FE estimator using the Dataset 2 – END-date project year

	(1a)	(1b)	(2)	(3)
VARIABLES	*Dummy Funds FE model*	*Dummy Funds Probit model*	*Number of project*	*Fragmentation Index*
PBC (pre_elec)	0.0134**	0.0332	0.00492	0.0121
	(0.00528)	(0.0303)	(0.00660)	(0.0144)
Year_2008	0.00369	0.226***	0.00569	0.0541
	(0.00346)	(0.0865)	(0.00402)	(0.0464)
Year _2009	0.0505***	0.780***	0.0170***	0.00911
	(0.00450)	(0.0759)	(0.00367)	(0.0361)
Year _2010	0.112***	1.166***	0.0468***	-0.0292
	(0.00616)	(0.0731)	(0.00514)	(0.0383)
Year _2011	0.245***	1.667***	0.178***	-0.0234
	(0.00799)	(0.0714)	(0.0120)	(0.0369)
Year _2012	0.195***	1.504***	0.121***	-0.0359
	(0.00746)	(0.0717)	(0.00900)	(0.0386)
Year _2013	0.123***	1.225***	0.0434***	-0.0393
	(0.00664)	(0.0734)	(0.00553)	(0.0396)
Year _2014	0.0753***	0.952***	0.0497***	-0.0275
	(0.00537)	(0.0745)	(0.00650)	(0.0400)
Constant	0.00881***	-2.330***	0.00523*	0.965***
	(0.00266)	(0.0671)	(0.00290)	(0.0362)
Observations	24,806	24,806	24,806	2,779
R-squared	0.078		0.029	0.015
Number of codice	3,102		3,102	1,865

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1; all regressions present time and municipal fixed effect; population used in per capita variable is divided by 1000.

In table 7 I report the result of the estimations introducing as independent variable the interaction (PBC*conv) between the pre-electoral year dummy and the dummy *Convergence*[30] which does not vary in time, but only between municipalities. In this way, I capture the differential effect in the behaviour of mayors in case their municipality belongs to Calabria, Campania, Puglia and Basilicata or in case their municipality belongs

[30] The equation assumes this form $\boldsymbol{x_{it} = \alpha_i + \gamma_t + \beta_1 * PBC_{it} + \pi_1 * PBC_{it} * conv_i + \varepsilon_{it}}$

to a *Competitiveness and employment* regions. Looking at the estimates for the Dummy funds (col. 1a, table 7), the pre-election dummy is not statistically significant, but the estimated coefficient of the interaction that captures the differential effect of PBC of a municipality belonging to *Convergence* regions is positive and statistically significant at 1% (0.0317). The same estimate of the regression with the use of the Probit model (col. 1b, table 7) shows significant results (the pre-election dummy has an estimated coefficient of -0.109 and the interaction term equal to 0.344, both significant at 1%). The average marginal effect of the interaction is 0.0591 (PBC -0.0186).
When I use as dependent variable the number of project per 1000 inhabitants (col. 2, table 7), I do not find evidence of PBC presence, differently from the the analysis with the *Dataset 1 – LAUNCH-date project year*. Finally, the result reported in column 3 (table 7) shows that for the Fragmentation Index of the ended projects the differential result of the interaction term is negative and statistically significant at 10% (-0.0497).

I notice that the effect of PBC on dependent variables in this second analysis is guided by municipalities belonging to *Convergence* regions. The results follow the same dynamic of the previous analysis (Table 4) even if only for this group of municipalities. The probability of ending a project in the pre-electoral year is statistically significant only in this case (col. 1, table 7). The fragmentation is higher in the pre-electoral year for municipalities belonging to *Convergence* regions, but the effect disappears if I consider all municipalities (col. 3, table 7: lincom 0.0448*-0.0497*=-0.0049 not statistically significant). These results put in evidence that municipalities belonging to *Convergence* regions show higher effect of the PBC on the probability of ending project and in their fragmentation, differently to the result obtained using the *Dataset 1 – LAUNCH-date project year* when I consider the launch date of projects instead of its conclusion. As described previously the nature of the projects, in my opinion, have a strong influence on the estimated results. I retain that municipalities belonging to *Convergence* regions show PBC effects in the year of its conclusions. This means that mayors exploit the electoral signal of the realization of a public work, realized with the EU Structural funds, in the moment known as the ribbon-cutting ceremony. For this type of projects, the announcement effect is not considered significant and enough visible for the electorate with respect to its effective realization such as the end of the project. The opposite reasoning can be done for the projects of the municipalities of *Competitiveness and employment* regions, this type of projects has relevance in term of PBC in their launch date, and they are not exploited by mayors in the year of their ending to convince the electorate.

Table 7. Estimation results with FE estimator using the Dataset 2 – END-date project year with Objective specification: Convergence and Competitiveness and employment regions.

	(1a)	(1b)	(2)	(3)
VARIABLES	*Dummy Funds FE model*	*Dummy Funds Probit model*	*Number of project*	*Fragmentation Index*
PBC (pre_elec)	0.00215	-0.109***	0.00615	0.0448*
	(0.00604)	(0.0381)	(0.00925)	(0.0229)
*PBC*conv (pre_ele_conv)*	0.0317***	0.344***	-0.00345	-0.0497*
	(0.0106)	(0.0527)	(0.0107)	(0.0290)
Year_2008	0.00525	0.242***	0.00552	0.0515
	(0.00348)	(0.0866)	(0.00415)	(0.0465)
Year _2009	0.0509***	0.785***	0.0170***	0.0117
	(0.00451)	(0.0760)	(0.00368)	(0.0363)
Year _2010	0.112***	1.165***	0.0468***	-0.0270
	(0.00616)	(0.0732)	(0.00514)	(0.0385)
Year _2011	0.245***	1.671***	0.178***	-0.0227
	(0.00799)	(0.0714)	(0.0120)	(0.0371)
Year _2012	0.195***	1.507***	0.121***	-0.0352
	(0.00746)	(0.0718)	(0.00900)	(0.0388)
Year _2013	0.125***	1.248***	0.0433***	-0.0404
	(0.00666)	(0.0735)	(0.00563)	(0.0397)
Year _2014	0.0757***	0.957***	0.0496***	-0.0218
	(0.00538)	(0.0746)	(0.00650)	(0.0403)
Constant	0.00842***	-2.335***	0.00528*	0.962***
	(0.00267)	(0.0672)	(0.00287)	(0.0364)
Observations	24,806	24,806	24,806	2,779
R-squared	0.078		0.029	0.018
Number of codice	3,102		3,102	1,865

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1; all regressions present time and municipal fixed effect; population used in per capita variable is divided by 1000.

2.6.3 Robustness tests

I test if the results obtained in the main analysis are robust. I repeat regressions of the main analysis introducing control variables in the equation used in the analysis (Table B1 – summary statistics). I include a set of time-varying variables, which characterizes the municipality's demographic and economic situation. I include municipality population (*population/1000*) and per capita area (*area*) - square kilometers divided by population - which can control for the size of the municipalities influencing the number of project obtained but also the organizational skills in the realization of projects. The proportion of citizens aged between 0 and 5 (*children*) and the proportion of citizens aged over 65 (*aged*)

that can control for some specific public needs which can influence the electorate. In terms of economic controls, I include the per capita personal income tax base (*income/100*). A political control is the dummy term-limit: since Italian law establishes a limit of no more than two consecutive terms in office for a mayor, I use a dummy variable (*term-limit*) which is equal to 1 for all the years a mayor is at her second term (and hence she cannot be re-elected) and it is equal to 0 when the mayor is at her first term. In case of second mandate the mayor could not be interested in giving signal to the electorate because she cannot be re-elected. Finally, I include a control for the Domestic Stability Pact that implies for municipalities to respect a constrained deficit or the level of expenditure. The dummy (*domestic stability pact*) is equal to one if a municipality must fulfill the Domestic Stability Pact (i.e. it has more than 5,000 inhabitants) and 0 otherwise.

I also exclude from the datasets (*Dataset 1 – LAUNCH-date project year* and *Dataset 2 – END date project year)* the year of election. Considering that elections are planned during the year (generally in the month between April and June) the electoral year can account for different effects: the before election effect and the post electoral effect. This can imply a confounding effect in the estimation. I also exclude from the datasets municipalities with an irregular cycle of election. In fact, in case of irregular political cycle mayors cannot predict the year of election and she do not give signals to the electorate and the PBC should not hold. In this case I obtain a sample of 19,618 observations.

I test this form $x_{it} = \alpha_i + \gamma_t + \beta_1 * PBC_{it} + \pi_1 * PBC_{it} * conv_i + \varepsilon_{it}$ for the two datasets. I reported the regression in the appendix (Table B2 and B3). I confirm the results obtained in the main analysis, also in case of the Probit model (col. 1b).

2.7 Conclusion

In this paper, I explore the existence of Political budget cycle in the dynamic of realization of projects financed by the European Structural funds at the municipal level. I exploit the role of Italian municipalities as project implementers, considering that after an allocation process trough different administrative level (from National level to regional and to local level) almost 40% of them receive resources to develop at least one project in the 2007-2013 plan period. I estimate the presence of PBC using two different panel datasets using as year of the panel the launch or the end date of the project. The relevance of the results obtained in the analysis suggests that it exists a causal relationship between the PBC and the dynamics of the use of European structural funds. My interpretation, in the context of the PBC literature, is that mayors use the resources and the possibility to make projects on its own territory in order to influence voters, to obtain consensus and win the elections. When I analyze the effect of PBC splitting the dataset by the EU objective (*Convergence* and *Competitiveness and* employment) I obtain different results, also when I consider the *Dataset 1 – LAUNCH-date project year* or the *Dataset 2 – END date project year*. I retain that mayors try to reach the goal of consensus in two different ways, related to the nature of the projects. In case of Contributions or purchase of good and services (typical projects

of municipalities belonging to *Competitiveness and employment* regions), mayors exploit the announcement effect of the project to convince the electorate. In case of projects with long duration such as public works (typical projects of municipalities belonging to *Convergence* regions), mayors exploit the effectiveness of the projects when they can publicly show its end, moment known as the ribbon-cutting ceremony (taglio del nastro). Anyway, my analysis confirms the existence of political effect on the use of structural funds in the Italian municipalities.

2.8 Appendix

Table A1. Projects financed by the EU structural funds with municipality as implementer – Categorized by type of Objective.

	EU funds' Objective			
Region	***Convergence***	***Competitiveness***		
	Freq.	***Freq.***	***Total***	***Perc.***
Basilicata	538		538	4.22
Calabria	2330		2330	18.28
Campania	861		861	6.75
Puglia	1600		1600	12.55
Sicilia	747		747	5.86
Abruzzo		372	372	2.92
Emilia Romagna		94	94	0.74
Friuli		552	552	4.33
Lazio		288	288	2.26
Liguria		295	295	2.31
Lombardia		301	301	2.36
Marche		347	347	2.72
Molise		190	190	1.49
Piemonte		386	386	3.03
Sardegna		1469	1469	11.52
Toscana		392	392	3.07
Trentino		1413	1413	11.08
Umbria		161	161	1.26
Valle d'Aosta		83	83	0.65
Veneto		330	330	2.59
Total	***6076***	***6673***	***12749***	***100***

Table A2. Projects financed by the EU structural funds with municipality as implementer – Categorized by Priority axis and type of Objective.

Priority axes	***Description of priority axes***	***EU funds' Objective***			
		Convergence		***Competitiveness***	
		Freq.	***Percent***	***Freq.***	***Percent***
1	*Improving and enhancing human resources*	219	3.6	60	0.90
2	*Promotion, valorization and dissemination of research and innovation for competitiveness*	51	0.84	434	6.5
3	*Energy and environment: Sustainable and efficient use of development resources*	2,200	36.21	2,011	30.14
4	*Social Inclusion and Services for Quality of Life and Territorial Attractiveness*	1,140	18.76	659	9.88
5	*Valorization of natural and cultural resources for attractiveness and development*	975	16.05	753	11.28
6	*Network and mobility*	205	3.37	136	2.04
7	*Competitiveness of production systems and employment*	241	3.97	1,961	29.39
8	*Competitiveness and attractiveness of cities and urban systems*	910	14.98	629	9.43
9	*International opening and attraction of investment, consumption and resources*	1	0.02	1	0.01
10	*Governance, institutional capacity and competitive and effective markets*	134	2.20	29	0.43
Total		***6,076***	***100***	***6,673***	***100***

Table A3. Projects financed by the EU structural funds with municipality as implementer – Categorized by Nature of the project and type of Objective.

Nature of the project	***EU funds' Objective***			
	Convergence		***Competitiveness***	
	Freq.	***Percent***	***Freq.***	***Percent***
Purchase of goods	252	4.16	354	5.31
Purchase of realization of services	700	11.55	604	9.06
Concessional contribution to economic actors (except for production unit)	55	0.91	2,519	37.79
Concessional contribution to production units	4	0.07	44	0.66
Public works (constructions and installations)	5048	83.31	3,144	47.17
Total	***6059***	***100***	***6665***	***100***

Table B1. Summary statistics of the control variables

Variable	*Number of observations*	*Mean*	*Std . Dev.*	*Min*	*Max*
PBC (Pre electoral year)	24,806	0.195	0.396	0	1
Term limit	24,806	0.664	0.472	0	1
Income	24,806	9,096.334	3,135.076	0	29,453.93
Area	24,806	0.018	0.025	0	0.315
Population	24,806	11,347.22	62,612.98	66	2,872,021
Aged	24,806	0.228	0.056	0.070	0.623
Children	24,806	0.050	0.012	0	0.111
Domestic stability pact	24,806	0.359	0.480	0	1

Table B2. Estimation results with FE estimator, control variables and without the year of election using the Dataset 1 – LAUNCH-date project year. Excluding municipalities with irregular election cycle.

VARIABLES	(1a) *Dummy Funds FE model*	(1b) *Dummy Funds Probit model*	(2) *Number of project*	(3) *Fragmentation Index*
PBC (pre_elec)	0.0328***	0.0836**	0.0428***	-0.0434***
	(0.00842)	(0.0331)	(0.0157)	(0.0132)
*PBC*conv (pre_ele_conv)*	-0.0297**	-0.118**	-0.0569***	0.0290
	(0.0134)	(0.0498)	(0.0169)	(0.0216)
Year_2008	-0.0164**	0.00823	-0.0227**	-0.0406
	(0.00648)	(0.0237)	(0.00964)	(0.0352)
Year _2009	0.130***	-3.51e-05***	0.0304**	-0.0600*
	(0.0103)	(4.32e-06)	(0.0137)	(0.0320)
Year _2010	0.204***	0.128	0.123***	-0.109***
	(0.00959)	(0.579)	(0.0151)	(0.0333)
Year _2011	0.306***	-1.455***	0.226***	-0.121***
	(0.0112)	(0.338)	(0.0171)	(0.0335)
Year _2012	0.233***	-11.99***	0.153***	-0.133***
	(0.0100)	(1.454)	(0.0167)	(0.0344)
Year _2013	0.0929***	1.20e-06***	0.00710	-0.0804**
	(0.00953)	(1.54e-07)	(0.0176)	(0.0344)
Year _2014	-0.0392***	0.318***	-0.0637***	-0.0359
	(0.00891)	(0.0274)	(0.0198)	(0.0402)
Term limit	0.00117	0.0869	-0.00509	0.00977
	(0.00587)	(0.0619)	(0.00805)	(0.0136)
Income	1.76e-05***	0.949***	1.24e-05*	1.89e-05*
	(2.61e-06)	(0.0568)	(6.48e-06)	(1.14e-05)
Area	-4.265***	1.141***	12.63**	9.182
	(1.271)	(0.0529)	(5.051)	(5.857)
Aged	1.197***	1.472***	1.179	-0.839
	(0.304)	(0.0525)	(0.932)	(0.869)
Children	0.187	1.274***	0.614	1.659
	(0.559)	(0.0525)	(1.581)	(1.552)
Population	3.45e-06***	0.828***	1.03e-06***	-1.58e-06***
	(1.08e-06)	(0.0543)	(3.69e-07)	(3.19e-07)
Domestic stability pact	-0.0224	0.140**	8.22e-05	0.0130
	(0.0662)	(0.0665)	(0.0211)	(0.0845)
Constant	-0.335***	-0.698***	-0.605**	0.825***
	(0.0862)	(0.140)	(0.267)	(0.264)
Observations	19,618	19,618	19,618	3,563
R-squared	0.118		0.043	0.045
Number of codice	3,102		3,102	2,266

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1; all regressions present time and municipal fixed effect; population used in per capita variable is divided by 1000.

Table B3. **Estimation results with FE estimator, control variables and without the year of election using the *Dataset 2 – END-date project year. Excluding municipalities with irregular election cycle.***

VARIABLES	(1a) *Dummy Funds FE model*	(1b) *Dummy Funds Probit model*	(2) *Number of project*	(3) *Fragmentation Index*
PBC (pre_elec)	0.00659	-0.0281	0.00835	0.0347
	(0.00653)	(0.0400)	(0.0105)	(0.0262)
*PBC*conv (pre_ele_conv)*	0.0305***	0.179***	-0.00746	-0.0385
	(0.0115)	(0.0565)	(0.0122)	(0.0329)
Year_2008	-0.00282	-0.0456*	0.000271	0.0412
	(0.00428)	(0.0265)	(0.00662)	(0.0554)
Year _2009	0.0266***	-3.71e-05***	0.000569	0.000438
	(0.00663)	(4.96e-06)	(0.00849)	(0.0394)
Year _2010	0.110***	-0.684	0.0398***	-0.0646
	(0.00703)	(0.674)	(0.00816)	(0.0397)
Year _2011	0.226***	-0.930**	0.159***	-0.0561
	(0.00930)	(0.391)	(0.0135)	(0.0379)
Year _2012	0.174***	-9.550***	0.105***	-0.0579
	(0.00833)	(1.676)	(0.0120)	(0.0414)
Year _2013	0.0970***	8.96e-07***	0.0252*	-0.0569
	(0.00785)	(1.46e-07)	(0.0129)	(0.0430)
Year _2014	0.0743***	0.334***	0.0557***	-0.00973
	(0.00910)	(0.0309)	(0.0187)	(0.0487)
Term limit	-0.00611	0.260***	-0.00890	0.0322
	(0.00525)	(0.0947)	(0.00678)	(0.0235)
Income	6.40e-06***	0.776***	3.40e-06	1.70e-05
	(2.16e-06)	(0.0889)	(5.02e-06)	(1.36e-05)
Area	-3.977***	1.219***	20.95***	-3.870
	(1.380)	(0.0814)	(6.526)	(10.63)
Aged	1.851***	1.717***	-0.436	-2.697*
	(0.264)	(0.0800)	(0.711)	(1.425)
Children	0.562	1.548***	-0.447	-1.697
	(0.489)	(0.0801)	(1.070)	(2.176)
Population	-4.98e-07	1.278***	-1.84e-07	2.11e-06
	(6.50e-07)	(0.0814)	(2.07e-07)	(4.08e-06)
Domestic stability pact	0.0162	1.186***	0.0162	-0.102**
	(0.0596)	(0.0840)	(0.0157)	(0.0491)
Constant	-0.408***	-1.465***	-0.275	1.573***
	(0.0760)	(0.169)	(0.203)	(0.437)
Observations	19,618	19,618	19,618	2,270
R-squared	0.081		0.034	0.040
Number of codice	3,102		3,102	1,652

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1; all regressions present time and municipal fixed effect; population used in per capita variable is divided by 1000.

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Chapter 3

The employment protection legislation in Italy: different restricted firms' reaction to a tax rate cut

Abstract

This article investigates the effect that Italian Employment Protection Legislation can have on firms' performance and in the level of their employment. I use a panel dataset at firm-level for the period 2005-2011 which contains data of Italian corporations. I exploit a tax rate cut occurred in 2008 (the Ires tax) to fix a *pre* and a *post* period and divide the dataset in *treated* and *control group*. Italian EPL, regulated by the *Articolo 18* of the *Statuto dei lavoratori* (law n. 300/1970) fixes high level of constraints for firms over the threshold of 15 employees and lower level of constraints for firms above the threshold of 15 employees. I implement an identification strategy based on a discontinuity regression design and a difference-in-differences approach, even with an unconventional use of the dummy variable pre and post. I find a negative and statistically significant effect of EPL on profitability, measured by Roe and Roa, and on the level of employment. Firms less constraints by EPL, below the limit of 15 employees, show a positive reaction to the tax rate cut if compared to the firms above the limit of 15 employees.

Key words: Employment protection legislation, firms, profitability, employment

JEL codes: H32, D22, J48

3.1 Introduction

The Employment protection legislation (EPL) is a set of regulations limiting the faculty of firms to hire and fire workers. These restrictions on firing can be established by law but also by standard employment contracts (Young, 2003), such as severance payments, mandatory notice periods, administrative procedures and delays. The scope of this type of regulation is protecting employees against dismissals activated by their employers, in a political vision of workers as the weaker party of the labor market. Following this policy goal, in the second half of the twentieth century, many European policy makers issued legislation with the scope of limiting the phenomenon. In Italy, in 1970 it has been enacted the *Statuto dei lavoratori*, law n. 300, which established limits and faculties of firms in hiring and firing employees.

Researchers extensively debated the effect of this type of policies with macro level analysis. They put in evidence (Leazer, 1990) the negative effects of EPL on the efficiency of markets, it influences employment's firm choices and the level of competition. This type of restrictions causes augmented costs for firms and I can consider them as a tax on firing. The EPL, as a taxation, produces distortions in the efficiency of the economy, the so-called "deadweight loss" or "excess burden". It represents the added cost to taxpayers and society of raising revenue through taxes (Auerbach, 2002). This distortion is assumed to cause changes in economic agents' decisions. In the last three decades, this evidence pushes policy makers to activate liberalization policies, progressively eliminating the EPL regulations. For example, the Italian article 18 of the *Statuto dei lavoratori*, law n. 300, has been object of a progressive revision starting with the Fornero's reform in 2012 and culminating in 2015 with the reform, known as *Job's act*, that introduced a fewer constricting regulation of labour, the work contract with increasing protections.

A second branch of research analyses the effect of EPL on firms exploiting micro level data, finding effect on firms' choices in term of employment level or on productivity (Bassanini et al., 2007). Firms' profitability, instead, has not been widely debated.

Our article focuses on the effects of EPL on the Italian firms' performance. I want to verify if the Italian employment protection legislation had been effect on the profitability of firms, measured through some indicators, such as the Return on equity (Roe) and the Return on assets (Roa), and on the employment level using variable accounting for variation in the number of employees from one year to another. I use characteristics of EPL, regulated by the *Articolo 18* of the *Statuto dei lavoratori* (law n. 300/1970), to identify a group of firms which is constrained by a high level of regulation and a group less constrained. Moreover, I use a variation in the corporate tax rate (*Ires tax, Imposta sul reddito delle società)* to test how the two groups of firms react to it. So, I set up an experiment exploiting the variation of tax rate, with a reform realized in 2008, that reduces the rate for corporations from 33% to 27%, for the two groups of firms, constrained and not constrained by EPL. I expect that firms affected in different way by the EPL, react differently to the decrease of the taxation on profits and on employment decisions.

The paper will be structured as follows. Section 3.2 gives a picture of the Italian institutional background , Section 3.3 makes a survey of the related literature, with a specific focus on the relation between EPL and profitability, the aspect I want to test in the model. Section 3.4 introduces the dataset, gives some preliminary evidence, describes the econometric strategy and then discusses the results. A subsection shows the robustness controls. Section 3.5 concludes.

3.2 Institutional background

The Italian labour market has been characterized by strong regulation of firing for years. The *Articolo 18* of the *Statuto dei lavoratori* (law n. 300/1970) has been the legislator's most important tool for this scope and it has established how firms could fire open-end contract workers. It entered into force in the 1970 with Law n. 300 and it constrained only firms above 15 employees. In 1990 the legislator tightens up the discipline extending the unfair dismissal to the firms below the limit of 15 employees (Law n. 108), even if it has been maintained some differences of treatment: small firms had a favourable treatment in respect to the big ones.

The law established that "the judge cancels the dismissal filed without just-cause base or justified reason or declares nullity ordering the employer to reinstate the worker in the workplace. The worker is entitled to compensation for the loss or damage suffered as a result of the dismissal for which the ineffectiveness or invalidity has been established in accordance with the preceding paragraph. In any case, the compensation measure shall be determined in accordance with the criteria of art. 2121 of the Italian Civil Code."

In recent years, starting from 2012, the *Articolo 18* has been object of an important debate on the liberalization of labour market that must necessarily pass through the revision of the EPL system. The Fornero job's reform rewrote it deleting the differences between small and big firms in the discipline of firing and it established that the employer had to reinstate the worker or to pay the unemployment compensation only in case of unfair dismissal due to the discrimination or if the just cause-basis are "strongly unfounded" (*manifestamente insussistente*). In 2015 Job's Act reform further limited the case of reinstatement, introducing the work contract with increasing protections. This trend of reforms over the last years confirms that the relaxation of EPL constraints is still one of the policy stimulus used to foster growth.

Considering the reforms occurred in recent years in Italy, my research concentrates in the years 2005-2011, where EPL stayed unchanged. In the considered period, the *Articolo 18,* as described above, established that individual and collective dismissals of workers were only allowed on a just cause basis. In the Italian system legislation just-cause basis could assume different meanings: employers could fire a worker for misbehaviour (*giusta causa* o *giustificato motivo oggettivo*), or if the firm had economic problem and had to reorganize its activities or to downsize. If these requirements did not subsist, after the judge's rule of unfair dismissal, the firms had to

pay a sort of "penalty". This penalty varied between firms, linked to the firms' size. In fact, the *Articolo 18* fixed a size threshold under which dismissal was easier: firms with less or equal to 15 employees even if they fired without a just-cause basis, they were not forced to reinstate the worker but at most they had to pay an allowance between 2.5 and 6 months of salary. Above the threshold of 15 employees, in case of unfair dismissal stated by the judge, companies were obliged to reinstate the worker or to pay until 15 months of salary, if the worker opted for it. This type of regulation created different treatment and different level of firing costs: firms above 15 employees had to stand higher cost of unfair dismissal. Another aspect to consider is highlighted by Schivardi and Torrini (2008), they put in evidence that the critical variable of Italian EPL system is not the legislation but the uncertainty about the result of the trial, especially when the object of controversy is the employee's misbehaviour. In this last case, the firm had to demonstrate it to the judge. Moreover, as they notice, large firms had to compensate the workers for the forgone wages in the time elapsing between the firing moment and the judge sentence, with no limitation. Considering the Italian trial's last, these costs could be very high, reaching even 5 years of salary.

Finally, I want to give a clarification related to the legislation and the available data. The limit of 15 employees of the law did not refer to the firm, but more precisely to the establishment. Data on establishment are not available, I use data at firm-level, so when I interpret the results I have to pay attention at this type of distortion. Also Cingano et al (2014) consider this specification of the Law and assess they solve this problem focusing on a sample composed by firms with a number of employees between 10 and 20, that are plausibly single-plant firms; so in that case, there is no difference between data on establishments or on firms. I believe this condition should be maintained until the number of 30 employees, for this reason I decide to use in the main analysis a dataset including firms between 1 and 30 employees. I am interested in analysing firms' behaviour below and above the limit of 15 employees: I expect that firms under 15 employees were more flexible than the other ones. I test if this reform affected differently firms with a number of employees under or equal to 15 and firms with number of employees above 15 with regard to their economic performances.

I grasp the opportunity offered by the 2008 reform of the corporation tax (*Ires tax, Imposta sul reddito delle società*). The reform of the Ires tax implied the reduction of the tax rate from 33% to 27.5%. The Ires is a tax on the corporation profit (*società di capitali*). Partnerships (*Società di persone*) are not subjected to this type of taxation but to the Irpef tax system. I have to mention that also corporations in the Italian fiscal system have the possibility to choose another tax regime (Irpef), proper of the partnerships. According to this pattern I selected a sample including only Società per azioni (Spa are a type of coporation); I decide to use it in the analysis and to exclude other types of legal form such as S.r.l. or Consorzi, because it was plausible that these last ones are similar to the partnerships and they could opt for Irpef taxation with more probability. Anyway, this reduction of tax influences firms' behaviour; as described in Wu (2010): every firm's scope is the profit maximization, obtaining trough the general formula of maximizing profits and minimizing costs., where in the complex of costs compares also taxes, which act as additional cost. So if higher taxation "reduce a firm's

profitability and its ability to invest and hire more workers", its reduction should conduce to the opposite result.

3.3 Theoretical consideration on EPL and empirical studies

Literature considers Employment protection legislation a "tax on hiring and firing" (Leazer, 1990). The impact on firing is the most evident effect, anyway EPL influences also hiring since firms actualize potential future costs of dismissal already in this preliminary phase of the recruitment. Moreover, this type of legislation creates rules that constrain the labour market. Researchers usually assess that a strict regulation has a negative impact on the efficiency of markets and causes poor performances in term of aggregate country employment and competition. Regulation in the labour market has had the aim of increasing employees' welfare and improving workers' conditions, but, more rights for employees translate into costs for firms and employers and maybe into negative effects on hiring. This second aspect has been stressed a lot in the last two decades: regulation has blamed for diminishing the efficiency and for causing the poor level of employment of European countries that had implemented in the past strict rules for firing and other type of labour market regulation (unemployment benefit). Evidence from USA performance, country with a typical less regulated labour market, enforce this preliminary evidence (Autor et al, 2007). The literature tried to produce empirical demonstration of this trend, and theoretical models about the deregulation of markets (Blanchard and Portugal, 2001; Blanchard and Giavazzi, 2003). So most of the literature stresses the impact that dismissal costs – or EPL - have on the labour markets and leaves in a second stage the impact on firms.

One of the first work in this sense is that of Leazer (1990) that faces the aspect of the market efficiency in presence of an EPL. He assesses that, in a perfect market, a properly designed labour contract undoes any state-mandated severance pay and there are no effects. He builds a two-period model, where in the first period employer and employee sign the contract and in the second period the employee works. The regulation of the market is represented as a government severance pay that workers receive in the second period if they are not employed by the firm. To reach the equilibrium workers have to pay a fee to the firms equal to the amount of the severance pay, and they accept this condition because they are sure to obtain it in the second period. In this case, the mandated transfer, which corresponds to a firing cost, is completely offset by the fee (private transfer from the workers to the firms): it is an optimal contract that evolves in the competitive market. This complete offsetting is ensured by the absence of imperfections in the market (as limit on borrowing, lack of trust of workers, presence of third part intermediary) that induces inefficiencies.

In fact, the theory predictions in absence of efficiency are ambiguous and empirical studies tried to find some evidence from the data: the effect could vary over time, or across the state of the demand. If I consider a non-perfect labour market, in presence of imperfections, EPL becomes a tax burden and it causes inefficiency.

Many researchers focus on the impact that EPL can have on the labour market, studying the relationship between EPL and unemployment rate. For example, Blanchard and Portugal (2001) propose a theoretical model to explain the difference between Portuguese and U.S. labour market, where they find the same unemployment rate but different unemployment duration; layoffs and quits are lower in Portugal than in the U.S. market. They pay attention on three effects of EPL on the equilibrium. First, firing costs decrease job flows, making layoffs more expensive. Second, firms keep less productive workers and then costs of production arises. Third, costs of firing strengthen bargaining power of workers and lead to an increase in employment duration. Thus, EPL generates effects on job flexibility and flows and passing through the production function of firms. Bentolila and Bertola (1990) had stressed this concept assessing that firing costs "provide incentives to retain workers whose wage exceeds their productivity during bad time and not to hire workers whose wage lies below their marginal productivity during good times".

Research less concentrate on the effects that EPL have on firms' production function, even if the link between cost of labour and productivity is very narrow. The reduction of workers flows and the retention of unproductive workers, caused by the firing costs distortion, can create movements on the firms' production choices and changes in the productivity. If the marginal product of labour falls below wage (the case of retention of unproductive workers), dismissal costs translates into a deadweight loss. Autor et al. (2007) assert that deadweight loss pushes firm and worker to maintain their relationship until the present value of the productivity gap is less than the deadweight loss, restricting efficient job separation and accession. Looking out for firms' productivity, if all other variables stay fixed, a reduction of labour productivity drives to a reduction of total productivity. Firms could improve different actions to pass over this limit: they could differentiate the type of hiring or replace labour with other factors of production. For these reasons, it is difficult to identify the net impact on the total productivity. So the truly effect on costs of production may be ambiguous. Bird and Knopf (2009) summarize them. On the one hand, firms may be more reluctant to dismiss unproductive workers because of the increased costs associated with discharge. On the other hand, dismissal protections may cause firms to hire employees more stringently and thus to select more productive employees, although implementing such hiring measures may have costs of their own. Another factor may be that dismissal protections promote capital deepening, a shift towards capital over labour as a choice for improving productivity. Finally, employees could demand for higher wages because of the employer's reduced power to fire. The result might be an increase in productivity because firms do not hire less productive workers, because of a more conservative behaviour. Furthermore, EPL could reduce firms' flexibility, impeding quickly reaction to changes in technology or product demand that require reallocation of staff or downsizing (Bassanini and Venn, 2007).

A third branch of research completes the pictures of firms' behaviour by focusing on the economic impact of firm-level cost and profitability effects by EPL (Birds and Knopf, 2009). Despite of this, any theoretical framework explains the link between economic performance and productivity. As Bird and Kopf assert, firms constrained

by EPL may use resources to increased productivity in another way (substituting labor with capital), or an increase in productivity may lead to lower profitability because of the constrained environment compare with firms operating in a non–regulated market. Considering that from a theoretical point of view, the total effect of EPL on productivity and on profitability is ambiguous, in recent years some empirical studies tried to give an answer to this question. I mention without demanding completeness, Bassanini and Venn. (2007), Cingano et al (2010), Autor et al. (2007), Cingano et al. (2014), Bird and Knopf (2009).

Bassanini and Venn (2007) examine the impact of different labour market policies, and also of EPL on productivity, on the OECD county with aggregate data from 1979 to 2003. They mainly find a positive impact on capital deepening, which results are consistent with Autor et al (2007).

Cingano, Leonardi, Messina and Pica (2010) study the joint effect of EPL and financial market imperfection on labour productivity, investment and labour-capital substitution at a firm-level data of EU countries. They find that on average EPL reduces capital per worker, investment per worker and labour productivity in an economic significant way. Thus, they suggest, "the debate on the role of EPL needs to consider not only the direct effect on employment flows but also the indirect impact due to distorted incentives on capital accumulation and investment". Then they explore the role of credit market imperfection on the EPL's effects; their results show weak evidence of a differential effect of EPL on investment in firms with different level of liquid resources even if they expected that firms with difficult access to credit had a lower capital stock per worker. Finally, they confirm that "exogenous improvements in credit markets may alleviate the negative impact of labour market restrictions on capital deepening and technology adoption".

Autor et al. (2007)'s study evaluates the empirical link between firing costs and productivity in the US context using micro-data. They start studying the effect of EPL in employment flows, then they focus on employment level and finally whether the observed effects on employment have productivity consequences, total effect that can be split in choices of capital and labour inputs. They find that the wrongful-discharge protection, by state courts in the US, reduces job flows, lowered the entry of new establishment and increases firms' adjustments costs. Afterwards, given this first results they try to evaluate the link between these evidence and productivity. They use as dependent variables: total investment and capital-labour ratio for capital, labour productivity and TPF. The results show a positive effect on capital investment, increasing the capital deepening, which lead to an increase in labour productivity. Otherwise, they measure a decline in total factor productivity, especially in a short-run period (three years after the adoption of EPL).

Cingano et al (2014) estimate the effect of EPL on capital deepening and productivity, exploiting the 1990 reform of the Italian *Articolo 18*, that extended unfair dismissal to the firms below the limit of 15 employees, leaving it unchanged for firms above the limit. They find a positive effect of EPL on capital deepening, which can be interpreted

as a capital-labour substitution effect, even if more pronounced for firms with lower capital and a large amount of liquid resources. The effect on TPF is negative.

Bird and Knopf (2009), focusing on the impact on firm-level labor expenses, capital investments and profitability, investigated the economic impact of EPL, on company-level costs and profitability using as sample 18,000 U.S. banks over the 1977-1999 period. They find that the constraints of wrongful discharge laws might decrease profitability of companies, even though the productivity increased. Namely, if wrongful discharge laws induce higher wages and greater investment in capital without a commensurate increase in income, the ratio of expenses to income would increase, resulting in lower profitability.

I want to contribute to this literature by testing the impact of EPL on firms' employment decisions and on profitability.

3.3.1 EPL and profitability

Economists has devoted few studies to the effect of EPL on profits, even if it is one of the most important determinants of the firm's activity. As already mentioned in the previous paragraph, EPL can influence the reallocation of productivity factors and accordingly to it decisions of companies to invest. This aspect may have reflection on profitability. The level of profits is dependent on several factors, including the investments. In the literature, there is an intense debate on the causality between profits and investments. I can see profits as an internal source applied to investment, along with external capital resources; from the other side, investments try to achieve the goal of increase the value for the owners. This put an item on the direction of causality. As summarized in Laporsek (2012) who makes an interesting list of the literature on profits and on profits and restrictions regulation, some studies find the existence of causality running from profits to investment (Little, 1962; Baumol et al, 1970; Lee and Suh, 2009), other from investment to profits (McFetridge 1978) and, also, a two-way causality (Lee and Nohel 1997). So I can affirm that the relationship between profits and investment has been proved, anyway I believe that profitability could depend also on other factors, related to the institutional background and the environment in which firms act, including EPL. EPL can influence profits from different point of view and this impact is definitely ambiguous. EPL increases the costs of employees, causing a decrease of the labor demand and a reduction of the effectiveness of resource allocation. These effects have an impact on the firm's efficiency and secondary on the level of profits (Boeri and van Ours, 2013). Firms tend to evade paying costs of dismissal, and consequently they are disinclined to fire workers, even if they are not productive, and also reluctant about hiring new ones. This behavior, as discussed in Autor et al. (2007), translate in lower labor productivity that could reduce profits. Other researchers put in evidence that employers could compensate this loss of productivity and profits with a reduction of workers' wages, moving the cost of EPL from them to the workers, like a cost of "being insured from dismissal" (Laporsek, 2012). Even if a wages reduction could affect labor productivity, Barone (2001)

affirms that assuring workers stable and secure employment and incomes, strict EPL would therefore not result in the reduction of workers' effort and it would not necessarily have unfavorable effects on profits. Another aspect to be considered is a substitution effect: EPL could stimulate employers to substitute labor with capital, buying "capital-intensive technologies and production processes", or to hire only high productive workers.

Concluding the reasoning I make on the effect of EPL on profits, I believe that firms' profitability generally decreases especially if compared to a situation without constrictions, if I compared firms constrained by EPL to firms acting in an environment with flexible employment protection regulations (Cingano et al., 2010).

3.4 Empirical analysis

3.4.1 Dataset, sample selection and preliminary evidence

In this analysis I use a panel data on private firms collected from the database Aida. The database Aida is realized and distributed by Bureau van Dijk S.p.a. and contains the Italian firms' data of balance sheets, registry and commodities sector. The dataset covers the period 2005-2011; I decide to exclude years starting from 2012 because it occurred reforms that affected EPL (the Fornero job's reform and the Job's act, see paragraph 3.2 for more information). I obtained financial information on corporations such as balance sheets' data, performance indicators (Return on equity and Return on assets), number of employees, corporate activity and legal form. The initial dataset was about 253,371 firm-year observations. I eliminated observations for firms with negative costs, negative revenues, negative equity and with zero employees and salaries to avoid missing data and outliers. I selected only *Società per azioni S.P.A.* in order to identify firms subject to Ires taxation. I make the main analysis with a subsample of firms from 1 to 30 employees, in order to avoid the possibility to have firms with more than a single-plant, considering that the EPLlaw did not refer to the firm, but more precisely to the establishment. In this way I do not plausibly have difference between data on establishments and data on firms. Furthermore, I decide to use this subsample to obtain comparable firms, because I am confident that over 30 employees, firms' behaviour is different from the other ones. I also test hypothesis on different sample dimensions: a maximum of 20 and a maximum of 40 employees, to control that the choice of the sample does not influence estimation results. I obtain respectively samples of 36,076 (1-20 employees), 49,844 (1-30 employees) and 61,922 observations (1-40 employees).

I want to investigate the effects of EPL on firm's performance using the Ires tax rate cut in 2008. Even if this policy influences all the firms in the dataset without distinction between treated or control group, namely, above or below the threshold of 15 employees, which is effective for the validity of EPL constraints, I believe that the tax rate cut could be a valid instrument to set a model specification for the estimates,

considering that EPL and taxation, as described in the literature, have the same effect on the dependent variable I use in my analysis.

The first variables, I want to use, are those which approximate profitability: ROA and ROE. ROA is the Return on Assets computed as the ratio between Net income and Assets; ROE is the Return on Equity, computed as the ratio between Profits and Equity. Secondary, I want to verify if a decrease in taxation leads to different variations in the employment level for firms above or under the limit of 15 employees. To test this effect, in the main analysis I use two dummy variables (*y* and *y2*) accounting for an increase of employees from year *t* to year *t+1* and from year *t* to *t+2*.

I report basic statistical information in case of the sub-sample of 1-30 employees. I divide the sample in treated and control group: I identify firms that have more than 15 employees, which have a strong regulation of firing and firms that stay above this limit. I obtain the group of treated firms (>15 employees) with 20,954 observations, and the group of no-treated firms (<=15 employees) with 28,890 observations. The mean number of employees for the treated is 22 employees, instead of 8 for the no-treated (Table 1). The ROE assumed the values of 6.15 for firms under 15 employees and of 5.53 for the ones over 15; the ROA is 4.38 for the firms under 15 employees and 4.63 for the ones over 15. The variable *y* has a mean of 0.42 in the entire sample, meaning that the probability of having an increase in the number of employees from one year to the next one in the period 2007-2014 is about 42%, the probability is higher (51%) if we considered (*y2*) a variation of employees from one year to two years later. I have a differentiation of the results only if I consider the variable *y:* if a firm is under 15 employees have an higher probability of increasing its employees in the considered period (0.44).

Table 1. Descriptive statistics: number of observation and mean values. 1<=employees<=30

	<=15	>15	Total	Min	Max
Number of observations	28,890	20,954	49,844		
Employees	8	22	14	1	30
ROE	6.15	5.53	5.89	-149.48	145.35
ROA	4.38	4.63	4.48	-261.44	126.76
y	0.44	0.41	0.42	0	1
y2	0.51	0.51	0.51	0	1

Table 2. Descriptive statistics: mean values pre and post reform. 1<=employees<=30

	Pre-reform		Post-reform	
	<=15	>15	<=15	>15
Employees	8.506	22.985	8.287	22.806
ROE	7.889	7.332	5.075	4.248
ROA	5.258	5.659	3.824	3.872
y	0.400	0.471	0.412	0.47
y2	0.514	0.541	0.496	0.541

Now, I look at the difference ante and post the Ires tax rate cut occurred in 2008. As I can see in Table 2, the ROA is increasing with the number of employees (pre-reform: 5.26 under 15 employees, 5.66 above 15 employees; post reform: 3.82 under 15 employees, 3.87 above 15 employees). Anyway, variables show a negative trend if we compare the pre and the post tax rate cut.

The point I want to focus is if the trend of the variable above and below the limit of 15 employees is the same before and after the Ires tax rate cut. I investigate further this aspect and to test if there is a statistical difference in the performance according to the EPL constraint of *Articolo 18*, given the 2008 tax rate cut, I apply a Difference-in-differences approach (DD). I divide firms in the dataset as *treated* if they are below the limit (<= 15 employees) and *no-treated* if they are above it (>15 employees), as previously described. Then, I keep the previous variable *post* if I am in the years after the reform (2008-2011) and *ante* if I am in the years before it (2005-2007). In this way, I obtain four groups: No-treated Ante tax rate cut, No-treated Post tax rate cut, Treated Ante tax rate cut and Treated Post tax rate cut. Even if it is not the conventional method to use as pre and post a policy that affected in the same way the two groups.

Before the Ires tax rate cut, the difference in mean between the ROA for Treated and Non-treated firms is -0.0431 (Table 3). After the Ires tax rate cut, the difference in mean between the ROA for Treated and Non-treated firms in this last case is -0.048. If I consider the difference between the mean ante 2008 and the mean post 2008 I see that there is a general decrease, probably due to the financial crisis of 2008. Nevertheless, the mean-ROA reduction of treated firms (-1.435) is lower than the mean-ROA reduction of large firms (-1.818). The difference of the differences in ROA between Treated and Non-treated firms, before and after the tax rate cut leads to an estimate of 0.383 statistically significant at 5%. This result shows that firms belonging to the two subsamples decrease both their ROA, but firms under 15 employees decrease their ROA less than firms above 15 employees.

Table 3. DD on ROA. Dataset including SPA until 30 employees for the period 2005-2011.

ROA	Treated (<=15 employees)	Non-Treated (>15 employees)	Difference (treated-nontreated)
Ante 2008	5.258	5.689	-0.431***
Post 2008	3.842	3.872	-0.048
Difference (Post-ante)	-1.435***	-1.818***	0.383**

I decide to run controls for the analysis of Table 3. Firstly, I exclude from the sub-sample manufacturing companies, because I believe they are less flexible to changes. Results remain significant and it is higher than before, in this case (Table 4), the difference of the differences is 0.561 and 5% significant. Furthermore, I run the same statistics varying the year of the tax rate cut used to construct the variable *Post;* in this way I run a robustness check in order control for a fake year reform (Table A1, Appendix). I find that the Difference-in-Differences values are never statistically significant: only with the year of the real reform, the 2008, I find that the effect is significant (Table 3).

Table 4. DD on ROA. Dataset including SPA until 30 employees of the no-manufacturing sector for the period 2005-2011.

ROA	Treated (<=15 employees)	Non-Treated (>15 employees)	Difference (treated-nontreated)
Ante 2008	5.122	5.574	-0.452***
Post 2008	3.875	3.765	0.109
Difference (Post-ante)	-1.248***	-1.809***	0.561**

I repeated the DD estimates for the other variables I consider *ROE* and *number of employees*, with the sub-sample of firms with 1-30 employees (including the manufacturing sector), I did not find any significant results (Table A2, Appendix).

I run the DD also for the other sample dimensions (20 and 40 employees). If I consider the sample of firms with maximum 20 employees the only significant DD value is that of the variable *employees* (-0.262 significant at 5%, Table A2, Appendix); instead if I consider the sample of firms with a maximum of 40 employees the ROA maintains its significance (0.524 significant at 1%).

Even if the results do not show always significant evidences, the trend is in line with my expectations. Variables measuring profitability (ROA and ROE) shows a negative relationship with EPL (DD values in Table A2, Appendix, are always positive), this means that firms below 15 employees that are less constrained by EPL regulation decrease their profitability less than firms above 15 employees.

3.4.2 Identification strategy and regression model

Our research interest is on the effects that the Ires tax rate cut can have on firms' profitability and on employment level with respect to the EPL constraints.

In this main analysis I exploit the discontinuity on EPL at the 15 employees' threshold for Italian firms, established by the *Articolo 18*, and the Ires tax rate cut occurred in 2008, to build a regression model that can estimate the causal effect of EPL on different variables. I want to compare variations in dependent variables, below and above the limit of 15 employees. Furthermore, I add the variation before and after the year of the tax rate cut. I expect, as descriptive analysis has shown, that profitability, measured by index such as Return On Asset and Return On Equity, has a negative relationship with EPL; I expect the same for variables measuring the employment level. A decrease of the taxation level should induce firms having more profits, more investment and even hire employees; anyway, I expect that firms subject to strict EPL do not have the flexibility to quickly react to the tax relaxation.

I implement the equation (1), operating an identification strategy based on a discontinuity regression design and a difference-in-differences approach, even with an unconventional use of the dummy variable pre and post, because the policy (the tax rate cut) affected in the same way the two groups. I construct a fixed effect model, where I control also for year effects. This model allows the intercept to vary over individuals and over time:

$$x_{it} = \alpha_i + \gamma_t + \beta_1 * treated_{it} + \beta_2 * treated_{it} * post_{it} + \beta_3 * size\_polinom_{it} + \varepsilon_{it} \quad (1)$$

Our dependent variable x_{it} takes the values of alternative variables:

- Profitability: Return On Assets and Return On Equity
- Employment level:
    - *y1* - a dummy variable equal to 1 if the number of employees of the year *t+1* of firm *i* is higher than the number of employees in the year *t* of the firm *i*, and it is equal to 0 otherwise.
    - *y2* - a dummy variable equal to 1 if the number of employees of the year *t+2* of firm *i* is higher than the number of employees in the year *t* of the firm *i*, and it is equal to 0 otherwise.

The variable γ_t is the year fixed effects. The variable $treated_{it}$ is a dummy that identifies the size two group of firms with respect to the threshold of 15 employees (EPL constraints – *Articolo 18*): it is equal to 1 if the firm is below the limit of 15 employees and it is equal to 0 if it is above it. $Post_{it}$ is a dummy variable equal to 1 in years after the Ires tax rate cut (2008-2011) and 0 in years before the Ires tax rate cut (2005-2007). The coefficient β_3 contains a third-degree polynomial on the firm size. Cingano et al (2014) recommended the use of the polynomial in this type of model and to avoid the use of dependent variables in per-worker term because of the identification strategy that selects firms by the number of employees. If I use the polynomial all the effects on the coefficient can be read as holding labor constant. Finally, α_i is the unobserved individual effects and ε_{it} the idiosyncratic error term.

Moreover, I base the identification strategy on the assumption of the absence of self-selection bias, in particular in this analysis there could be the possibility that firms decide to stay above or below the limit of 15 employees, in order to exploit the favor of the legislation. In order to check if my assumption is robust I need to investigate whether firms tend to sort above and below the 15-employee threshold, running a linear probability model (Schivardi and Torrini, 2007; Cingano et al., 2014). I describe later this aspect in the paragraph 3.4.4.

3.4.3 Profitability and employment level variation

I estimate equation (1) using the sub-sample of corporate firms with a maximum of 30 employees applying the FE estimator including controls for year dummies. The coefficient β_1, that captures the effect on the dependent variable of being a less constrained firm and the coefficient β_2 , which captures the effect of being a less constrained firm after the Ires tax rate cut of 2008.

In column 1 and 2 (Table 5), I run the regression using as dependent variables the ROA and the ROE, testing for profitability. The effect of β_1 is never statistically significant in all the two cases. On the contrary, the effect of β_2 is positive and statistically significant; this indicates that after the reform the effect on the ROA and on the ROE of being a less constrained firm is positive with a value of 0.619 (significant at 1%) for the first one and of 0.815 (significant at 5%) for the second one. This means that a firm under 15 employees after the tax rate cut increases its profitability if compared to firms above 15 employees. The annual fixed effects, instead, captures significantly the decrease of profitability, probably due to the crisis after 2008.

I run the same model (1) using as dependent variable the employment level exploiting the two dummy variables: *y1* equal to 1 if the number of employees in the year *t+1* is higher than the number of employees in the current year (*t*) and *y2* if the number of employees in year *t+2* is higher than in the year *t*, I obtain a statistically significant positive effect of the Ires tax rate cut with EPL. After the tax rate cut a firm under 15 employees with respect to a firm above 15 employees has an higher probability of 0.101 of being a firm who increases the numbers of its workers in the year *t+1* (with a significance of 1%, col. 3, Table 5). The same result is obtained if I consider the variation of employees in two years, in this case firms under the limit have a higher probability of 0.095 (with a significance of 1%, col. 4, Table 5).

Table 5. Fixed effects regression on profitability and employment level. 1<employees<=30

	(1)	(2)	(3)	(4)
VARIABLES	*Roa*	*Roe*	*y1*	*y2*
β_1Treated (<=15 employees)	-0.198	-0.031	-0.044**	-0.024
	(0.207)	(0.573)	(0.018)	(0.019)
*β_2Treated*post*	0.619***	0.815**	0.101***	0.095***
	(0.154)	(0.401)	(0.011)	(0.013)
Size	0.159	-0.301	-0.086***	-0.119***
	(0.102)	(0.251)	(0.007)	(0.008)
size2	-0.005	0.033*	0.003***	0.004***
	(0.007)	(0.018)	(0.001)	(0.001)
size3	0.000	-0.001**	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Year_2006	0.469***	-0.121	0.090***	0.069***
	(0.094)	(0.290)	(0.009)	(0.008)
Year_2007	0.772***	0.253	0.146***	0.093***
	(0.114)	(0.313)	(0.009)	(0.009)
Year_2008	-0.921***	-2.431***	0.097***	0.033***
	(0.151)	(0.394)	(0.011)	(0.012)
Year_2009	-1.696***	-4.239***	0.050***	0.043***
	(0.146)	(0.404)	(0.012)	(0.012)
Year_2010	-1.321***	-3.029***	0.071***	
	(0.144)	(0.404)	(0.012)	
Year_2011	-1.310***	-3.976***		
	(0.145)	(0.410)		
Constant	3.684***	7.402***	1.119***	1.485***
	(0.465)	(1.128)	(0.035)	(0.039)
Observations	49,147	49,264	37,919	30,756
R-squared	0.016	0.010	0.086	0.136
Number of cf	11,302	11,251	9,852	9,261
r-squared	0.016	0.010	0.086	0.136

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

I repeat this analysis using different sample dimensions (Table A3, Appendix), all the variables confirm the results obtained in the main analysis except for the dependent variable ROE in the sample of firms between 1 and 20 employees which is not statistically significant (col. 3, table A3).

Finally, I repeated these exercises one more time in order to check my results for the type of industry; in particular I split the sample in manufacturing and no manufacturing sector. Estimates for no-manufacturing (Table A4/1, Appendix) are positive and significant for all the four dependent variables used. The coefficient β_2 that accounts for the effect of being a firm below the threshold of 15 employees after the tax rate cut, is positive with a value of 0.817 for ROA (col. 1, Table A4/1, statistically significant at 1%); a value of 1.505 for the variable ROE (col. 2, Table A4/1,statistically significant at 1%). Also the probability of increase the employees in one year and in two years are statistically significant, respectively 0.113 and 0.115

(col. 3-4, Table A4/1). If I consider the manufacturing industry (Table A4/2, Appendix) I don't get any significant effects of the interaction on the measures of profitability (ROA and ROE). But the results for employment level stay significant with a value of 0.0795 in case of an increase of employees for firms above the limit of 15 employees from one year to another (variable *y*, statistically significant at 1%, col. 3, Table A4/2) and of 0.0756 in case an increase in two years (*y2*, statistically significant at 1%, col. 4, Table A4/2). These results suggest that service corporations have the capabilities of reorganize their own structure to obtain profits in a more flexible way, due to the characteristics of the business; so that firms below the limit of 15 employees reacts to the tax rate cut and succeed in increasing their profitability and the probability of increasing the level of employment with respect to the firms above the threshold. Conversely, manufacturing corporations have a rigid organizational structure that it is difficult to be modified also for small dimensions, for this reason the EPL constraints have no effect on the profitability of firm. Anyway the probability of hiring workers is not influenced by the type of the business.

3.4.4 Robustness tests

As I discussed before, I reported in this section a series of test to check the robustness of my findings.

First of all, I test if the selection assumption of absence of self-selection bias used for the discontinuity design is robust. I have to verify that firms do not select to stay under the limit of 15 employees in order to benefit of the less constrained legislation. So I run a linear probability model (2):

$$y_{it} = \alpha_i + \gamma_t + \beta_1 * sizeDummy_{it} + \beta_2 * sizeDummy_{it} * post_{it} +$$

$$+\beta_3 * size\_polinom_{it} + \varepsilon_{it} \qquad (2)$$

Where y_{it} is a dummy variable equal to one if the employees increase from one year to the other and 0 otherwise. The variable $sizeDummy_{it}$ indicates if the firm's size is equal to 13, 14 or 15. The variable $post_{it}$ takes the value of one in the year after the tax rate cut (2008, 2009, 2010 and 2011). I include firm fixed effects, year effects (γ_t), and a polynomial in lagged firm size.

Table 6. Linear probability model. 1<employees<=30

	(1)	(2)
VARIABLES	*y*	*Y*
size	0.080***	0.080***
	(0.011)	(0.011)
size2	0.000	0.000
	(0.001)	(0.001)
size3	-0.000*	-0.000*
	(0.000)	(0.000)
size4	0.000***	0.000***
	(0.000)	(0.000)
D13	-0.003	0.011
	(0.015)	(0.025)
D14	-0.019	-0.026
	(0.015)	(0.023)
D15	-0.051***	-0.047*
	(0.016)	(0.025)
int13		-0.022
		(0.028)
int14		0.010
		(0.026)
int15		-0.006
		(0.028)
Year_2006	0.099***	0.098***
	(0.010)	(0.010)
Year_2007	0.050***	0.049***
	(0.008)	(0.009)
Year_2008	0.084***	0.084***
	(0.008)	(0.008)
Year_2009	0.084***	0.084***
	(0.008)	(0.008)
Year_2010	0.034***	0.034***
	(0.008)	(0.008)
Year_2011	-	-
Constant	-0.644***	-0.643***
	(0.033)	(0.033)
Observations	35,584	35,584
R-squared	0.123	0.123
Number of cf	9,120	9,120
r-squared	0.123	0.123

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

In the first regression (col. 1, Table 6) I exclude the interaction (*int13, int14, int15*) between the size Dummy and the variable Post and I obtain that there is a trend between the firms below the limit of 15 employees that avoid to increase the number of employees. But when I control for the year of tha tax rate cut (*int*, interaction between Size dummy and Post) I notice that this trend is not evident in the considered year. This is important to confirm the robustness of my assumptions because it means that even if there is a sorting in the neighborhood of the limit this is not influenced by the reform I use to study the phenomenon.

In order to support the main results of my research I decided also to control the behavior of firms that don't have to pay the Ires tax, but that are exposed to the Irpef fiscal system. In this way I control if the effect estimated in the main analysis were

due to the Ires tax rate cut or were due to other causes. I change the sample, by taking only the "società di persone" (partnerships). The new sample is composed by 791 observations, considering the firms with maximum 30 employees; so I obtain a treated group of 616 observations and a no-treated group of 175 observations. The variable *treated* is the "società di persone" with a number of employees below the limit of 15. The variable *interaction* is the effect on the dependent variable (ROA, ROE, *y*, *y2*) of being a firm under 15 employees. less constrained and in the period after the fake (in this case) tax rate cut. The coefficient β_2, is positive, as before, but not significant for the variable ROA and ROE (Table 7, col. 1-2), confirming that the effect of EPL caused by the tax rate cut obtained in the main analysis is reliable. Anyway, I have to consider that in case of employment level (*y* and *y2*) I find significant coefficient for the variable *y* (col. 3, Table 7, 0.249, significant at 5%) The result is not significant for *y2*, that represents the increase of employees in two years.

Table 7. Fixed effects regression on "società di persone". 1<employees<=30

	(1)	(2)	(3)	(4)
VARIABLES	*Roa*	*Roe*	*y*	*y2*
β_1*Treated (<=15 employees)*	-2.184	-6.526	0.110	0.392
	(2.359)	(11.816)	(0.178)	(0.240)
β_2*Treated*post*	0.599	10.988	0.249**	-0.013
	(1.518)	(10.019)	(0.096)	(0.133)
size	0.347	0.118	-0.188***	-0.333***
	(0.528)	(2.849)	(0.065)	(0.070)
size2	-0.037	-0.075	0.008	0.022***
	(0.045)	(0.238)	(0.005)	(0.005)
size3	0.001	0.004	-0.000	-0.000***
	(0.001)	(0.005)	(0.000)	(0.000)
Year_2006	1.506*	-1.608	0.054	-0.102
	(0.841)	(4.119)	(0.101)	(0.109)
Year_2007	0.883	-3.716	0.092	0.022
	(0.976)	(4.680)	(0.100)	(0.113)
Year_2008	-1.544	-16.161	0.003	0.046
	(1.528)	(10.572)	(0.108)	(0.139)
Year_2009	-1.797	-26.917**	0.024	-0.054
	(1.660)	(11.081)	(0.119)	(0.145)
Year_2010	-2.360	-30.007***	0.000	
	(1.583)	(11.038)	(0.124)	
Year_2011	-2.226	-30.690***		
	(1.602)	(10.716)		
Constant	6.797***	20.666*	1.274***	1.574***
	(2.519)	(12.345)	(0.308)	(0.291)
Observations	789	737	445	360
R-squared	0.049	0.088	0.149	0.195
Number of cf	252	245	160	148
r-squared	0.049	0.088	0.149	0.195

*Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1*

3.5 Conclusion

This article investigates the effect that Italian Employment Protection Legislation can have on firms' performance and in the level of their employment. I use a panel dataset at firm-level for the period 2005-2011 which contains data of Italian corporations. I exploit a tax rate cut occurred in 2008 (the Ires tax) to fix a *pre* and a *post* period and divide the dataset in *treated* and *control group.* Italian EPL, regulated by the *Articolo 18* of the *Statuto dei lavoratori* (law n. 300/1970) fixes high level of constraints for firms over the threshold of 15 employees and lower level of constraints for firms above the threshold of 15 employees. I implement an identification strategy based on a discontinuity regression design and a difference-in-differences approach, even with an unconventional use of the dummy variable pre and post. I find a negative and statistically significant effect of EPL on profitability, measured by Roe and Roa, and on the level of employment. Firms less constraints by EPL, below the limit of 15 employees, show a positive reaction to the tax rate cut if compared to the firms above the limit of 15 employees. Anyway, I have also to take in consideration that the robustness analysis does not completely reinforce the analysis, in particular when I use the variable related to the employment level.

3.6 Appendix

Table A1. DD on ROA. Dataset including SPA until 30 employees for the period 2005-2011, varying the year of the reform to test for a fake reform.

	Post 2006-2011			Post 2007-2011			Post 2009-2011			Post 2010-2011		
ROA	Treated	Non treated	Difference (treated-non treated)	Treated	Non treated	Difference (treated-non treated)	Treated	Non treated	Difference (treated-non treated)	Treated	Non treated	Difference (treated-non treated)
Ante	4.792	5.354	0.562**	5.112	5.485	0.373**	5.067	5.321	0.254**	4.691	4.927	0.237**
Post	4.339	4.527	0.189**	4.154	4.313	0.159	3.609	3.774	0.164	3.72	3.956	0.236
Post-ante	-0.454**	-0.827***	**-0.373**	-0.958***	-1.172***	**-0.214**	-1.457***	-1.547***	**-0.089**	-0.97***	-0.971***	**-0.001**

*** p<0.01, ** p<0.05, * p<0.1

Table A2. DD on ROA, ROE, Employees. Dataset including different firms' dimensions (20, 30 or 40 employees) and only the SPA type. Period 2005-2011.

Sample dimension	*20 Employees*			*30 Employees*			*40 Employees*		
ROA	Treated	Non treated	Difference (treated-non treated)	Treated	Non treated	Difference (treated-non treated)	Treated	Non treated	Difference (treated-non treated)
Ante 2008	5.258	5.661	-0.403**	5.258	5.689	-0.431***	5.258	5.777	-0.519***
Post 2008	3.842	4.001	0.178	3.842	3.872	-0.048	3.842	3.818	0.006
Post-ante	-1.435***	-1.66	0.225	-1.435***	-1.818***	**0.383****	-1.435***	-1.959***	**0.524*****
ROE									
Ante 2008	7.889	8.022	-0.133	7.889	7.322	0.557*	7.889	7.249	0.64**
Post 2008	5.075	4.922	0.083	5.075	4.248	0.827***	5.075	4.035	1.040***
Post-ante	-2.814***	-3.030	0.216	-2.814***	-3.084***	0.270	-2.814***	-3.214***	0.400
Employees									
Ante 2008	8.506	17.897	-9.391***	8.506	22.985	-14.479***	8.506	27.578	-19.073***
Post 2008	8.288	17.941	-9.653***	8.288	22.806	-14.519***	8.288	27.316	-19.029***
Post-ante	-0.218***	0.044	**-0.262****	-0.218***	-0.179***	-0.039	-0.218***	-0.262***	0.044

*** p<0.01, ** p<0.05, * p<0.1

Table A3. Fixed effects regression on corporates by different samples' dimension

Samples	1<=employess<=20				1<=employess<=40			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Roa	Roe	y	y2	Roa	Roe	y	y2
β_1*Treated (<=15 employees)*	-0.140	-0.0507	-0.0461**	-0.0340	-0.300	-0.527	-0.0400**	-0.0205
	(0.255)	(0.685)	(0.0216)	(0.0222)	(0.184)	(0.515)	(0.0155)	(0.0165)
β_2*Treated*post*	0.460**	0.836	0.0467***	0.0527***	0.714***	0.596*	0.115***	0.107***
	(0.223)	(0.584)	(0.0175)	(0.0190)	(0.139)	(0.355)	(0.00983)	(0.0110)
Size	-0.136	-0.776*	-0.0929***	-0.110***	0.128*	0.0162	-0.0830***	-0.111***
	(0.164)	(0.405)	(0.0113)	(0.0124)	(0.0676)	(0.170)	(0.00481)	(0.00548)
size2	0.0284*	0.0889**	0.00338***	0.00297**	-0.00296	0.00312	0.00263***	0.00352***
	(0.0159)	(0.0412)	(0.00114)	(0.00124)	(0.00336)	(0.00896)	(0.000249)	(0.000279)
size3	-0.000987**	-0.00247**	-0.000109***	-8.12e-05**	2.09e-05	-9.16e-05	-3.79e-05***	-5.05e-05***
	(0.000469)	(0.00126)	(3.49e-05)	(3.77e-05)	(5.11e-05)	(0.000140)	(3.92e-06)	(4.35e-06)
Year_2006	0.501***	-0.277	0.0890***	0.0723***	0.461***	0.0648	0.0926***	0.0637***
	(0.122)	(0.360)	(0.0105)	(0.00952)	(0.0802)	(0.251)	(0.00787)	(0.00694)
Year_2007	0.833***	-0.115	0.162***	0.116***	0.745***	0.449	0.141***	0.0746***
	(0.150)	(0.390)	(0.0109)	(0.0104)	(0.0978)	(0.274)	(0.00805)	(0.00777)
Year_2008	-0.639***	-2.634***	0.174***	0.0972***	-1.072***	-2.103***	0.0730***	0.00381
	(0.233)	(0.610)	(0.0185)	(0.0191)	(0.125)	(0.327)	(0.00934)	(0.00957)
Year_2009	-1.497***	-4.344***	0.116***	0.0947***	-1.754***	-3.935***	0.0378***	0.0206**
	(0.230)	(0.627)	(0.0188)	(0.0194)	(0.121)	(0.338)	(0.00951)	(0.00985)
Year_2010	-1.200***	-3.571***	0.131***		-1.394***	-2.688***	0.0579***	
	(0.227)	(0.620)	(0.0191)		(0.118)	(0.334)	(0.00967)	
Year_2011	-1.164***	-4.174***			-1.427***	-3.514***		
	(0.232)	(0.630)			(0.120)	(0.340)		
Constant	4.330***	8.951***	1.039***	1.339***	3.843***	7.070***	1.177***	1.561***
	(0.550)	(1.331)	(0.0404)	(0.0440)	(0.458)	(1.109)	(0.0347)	(0.0396)
Observations	35,443	35,661	27,305	22,213	61,188	61,211	47,423	38,398
R-squared	0.012	0.009	0.106	0.157	0.019	0.010	0.081	0.134
Number of cf	8,737	8,704	7,525	7,091	13,355	13,304	11,827	11,105

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table A4/1. Fixed effects regression on corporations for non-manufacturing subsample

Sample	*Non-manufactoring firms*			
	(1)	(2)	(3)	(4)
VARIABLES	*Roa*	*Roe*	*y*	*y2*
β_1*Treated (<=15 employees)*	-0.228	-0.191	-0.0549***	-0.0349
	(0.250)	(0.694)	(0.0213)	(0.0223)
β_2*Treated*post*	0.817***	1.505***	0.113***	0.115***
	(0.193)	(0.493)	(0.0139)	(0.0155)
size	0.185	-0.390	-0.0884***	-0.122***
	(0.113)	(0.278)	(0.00820)	(0.00900)
size2	-0.00666	0.0457**	0.00278***	0.00410***
	(0.00829)	(0.0207)	(0.000621)	(0.000671)
size3	0.000113	-0.00105**	-4.51e-05***	-6.71e-05***
	(0.000169)	(0.000431)	(1.30e-05)	(1.40e-05)
Year_2006	0.371***	-0.513	0.0886***	0.0665***
	(0.117)	(0.345)	(0.0106)	(0.00930)
Year_2007	0.664***	-0.385	0.143***	0.0949***
	(0.143)	(0.368)	(0.0107)	(0.0103)
Year_2008	-0.941***	-3.272***	0.0932***	0.00983
	(0.199)	(0.498)	(0.0143)	(0.0149)
Year_2009	-1.758***	-5.077***	0.0363**	0.0149
	(0.190)	(0.512)	(0.0146)	(0.0153)
Year_2010	-1.472***	-4.167***	0.0509***	
	(0.187)	(0.509)	(0.0148)	
Year_2011	-1.564***	-5.249***		
	(0.186)	(0.518)		
Constant	3.536***	8.184***	1.074***	1.420***
	(0.494)	(1.216)	(0.0367)	(0.0411)
Observations	35,568	35,761	27,409	22,213
R-squared	0.013	0.011	0.088	0.138
Number of cf	8,162	8,120	7,080	6,674

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table A4/2. Fixed effects regression on corporations for manufacturing subsample

Sample	Manufacturing firms			
	(1)	(2)	(3)	(4)
VARIABLES	Roa	Roe	y	y2
β_1*Treated (<=15 employees)*	0.00543	0.0967	-0.0220	-0.0122
	(0.370)	(1.046)	(0.0326)	(0.0352)
β_2*Treated*post*	-0.0643	-0.384	0.0795***	0.0756***
	(0.277)	(0.790)	(0.0214)	(0.0235)
size	0.00549	-0.348	-0.0678***	-0.105***
	(0.257)	(0.613)	(0.0179)	(0.0203)
size2	0.00186	0.0162	0.00175	0.00316**
	(0.0159)	(0.0404)	(0.00115)	(0.00128)
size3	-4.72e-05	-0.000229	-3.52e-05	-6.01e-05**
	(0.000297)	(0.000779)	(2.22e-05)	(2.46e-05)
Year_2006	0.712***	0.843	0.0937***	0.0747***
	(0.150)	(0.536)	(0.0165)	(0.0151)
Year_2007	1.049***	1.825***	0.151***	0.0859***
	(0.179)	(0.592)	(0.0171)	(0.0167)
Year_2008	-0.906***	-1.064	0.0929***	0.0635***
	(0.220)	(0.661)	(0.0192)	(0.0200)
Year_2009	-1.610***	-2.866***	0.0722***	0.0854***
	(0.224)	(0.669)	(0.0199)	(0.0208)
Year_2010	-0.992***	-0.829	0.112***	
	(0.226)	(0.677)	(0.0207)	
Year_2011	-0.697***	-1.403**		
	(0.240)	(0.689)		
Constant	4.653***	7.494**	1.202***	1.658***
	(1.307)	(2.991)	(0.0969)	(0.115)
Observations	13,579	13,503	10,510	8,543
R-squared	0.032	0.011	0.083	0.138
Number of cf	3,140	3,131	2,772	2,587

*Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1*

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