

Indirect Political Budget Cycles: Evidence from Chilean Municipalities

ALEJANDRO CORVALAN^{*a}, PAULO COX^{†b}, and RODRIGO OSORIO^{‡c}

^a*Universidad Diego Portales*

^b*Universidad de Talca*

^c*Universidad de Santiago de Chile*

Abstract

Political budget cycles have been studied at the national and local level, separately. However, when local governments depend on national resources, the central government may have incentives to manipulate intergovernmental transfers in order to influence the reelection prospects of local incumbents, with an eye on their own future reelection. This generates an indirect Political Budget Cycle (iPBC). This paper documents the presence of an iPBC in Chile. We show that during municipal election years the government increases the overall amount of transfers to municipalities (local governments), and that these additional resources are allocated to the politically aligned mayors. We also report that transfers to aligned mayors are higher when the local races are tighter, suggesting that the government prefers to deviate resources towards swing municipalities. Finally, we show that transfers are correlated with municipal spending during electoral years, and with both local and national political electoral outcomes. (JEL: H72, D72, D78, C23, C25).

Keywords. Political budget cycles, Intergovernmental Transfers, Local governments, Elections, Chile.

*Alejandro Corvalan, Universidad Diego Portales, Av. Santa Clara 797, Huechuraba, Santiago, Chile. Email: alejandro.corvalan@udp.cl.

†Paulo Cox, Universidad de Talca. Santa Elena 2222, Santiago, Chile; Tel. (562) 2 414 8327 (6051); Email: paulocox@utalca.cl

‡Rodrigo Osorio, Facultad de Humanidades, Universidad de Santiago de Chile. Av. Alameda Libertador Bernardo O'Higgins 3363. Santiago, Chile; Tell. (562) 2718 2474; Email: rosoriof@gmail.com

1 Introduction

Political budget cycles have extensively been studied both at the national (Brender and Drazen (2005), Shi and Svensson (2006)) and local (Khemani (2004), Veiga and Veiga (2007), Labonne (2016)) level. However, these studies have been conducted following neat separate branches of the literature. This, in spite of the fact that a political cycle combining decisions and incentives that operate taking both levels of government together, may emerge quite naturally in certain institutional environments. Usually, political officials at the national level have limited information on how resources are allocated at the local level. This is why they typically use political brokers who intermediate between the government they represent and grassroots constituencies (Szwarcberg (2012), Stokes et al. (2013)). Local political officials are natural candidates for intermediation. On one hand, they are usually in charge of channeling national resources towards their local communities. On the other, they may have the power to use these resources to foster the reelection of the incumbent national government to whom they are politically aligned. As a matter of fact, recent literature has documented both that intergovernmental transfers are politically motivated (Johansson (2003), Arulampalam et al. (2009), Banful (2011), Brollo and Nannicini (2012)) and that appointed local officials do influence voters in national elections (Martinez-Bravo (2014)).

In this paper we claim that when the local governments' finance depends on the national government's fiscal policy, national officials have incentives to manipulate the allocation of intergovernmental resources during local elections years. In doing so, they induce politically aligned mayors to mobilize local constituencies to vote in favor of them in future elections. The increase of transfers from national to local governments during municipal election years generates a political budget cycle (PBC), but crucially, the mechanism here is *indirect* as compared to the traditional PBC. In the present case the government does not manipulate fiscal policy for its own sake, directly, but to influence the appointment of another political official in an electoral contest other than its own. By doing so, the government indirectly influences its own reelection prospects. Hence, we refer to this particular cycle as an indirect

political budget cycle (iPBC).

We provide evidence of the existence of an iPBC in Chile. Our case study presents several advantages for the study of this particular type of cycle. Chile has the peculiarity of having municipal elections held right a year before presidential ones, which makes incentives for this mechanism to take place starker. As is also the case in most countries, there is far more public scrutiny and attention from public opinion and institutions, to fiscal variables set at the central government, than at the local one; and far more legal constraints over their management. This is the more relevant the larger is the central government's spending as a fraction of total public resources. Indeed, transfers to municipalities represent a small fraction (about 1%) of total public spending, while amounting to a meaningful share of what local incumbents usually spend during election campaigns. From the point of view of incumbents of national governments, therefore, iPBC is less costly than the manipulation of other fiscal variables, and thus more likely to take place. Finally, it should be noticed that Chilean municipalities have limited fiscal autonomy, and thus mayors cannot easily manipulate other funding sources.

Our paper empirically tests the hypothesis that intergovernmental transfers, from the central government to local municipalities, are systematically distorted for electoral purposes. First we show that transfers increase during local election years in about 10% compared to the long-run transfers' trend. On the contrary, we do not observe an increase on transfers during Presidential and Legislative election years, which are held a year later. These results are robust to a variety of specifications.

We provide some tests in order to further elucidate the iPBC's underlying mechanisms. Our interpretation of the cycle is that the government influences local elections in favor of mayors belonging to its same coalition, because local officials can then influence the national election the following year. This interpretation has several testable implications. First and most importantly, we expect to see a positive cycle only in municipalities where mayors are politically aligned with the government. Indeed, we show that the iPBC is only noticeable in

municipalities where mayors are politically aligned. During electoral years transfers increase more than 25% in aligned municipalities. In addition, we report that transfers towards aligned mayors are larger the tighter is the local election contest. That is, we show evidence that the government manipulates transfers to target swing municipalities.

Secondly, under our hypothesis transfers should have an effect on municipal spending and, consequently, on electoral outcomes. Regarding the first point, we show that municipal spending indeed increases in local election years, and that this increment is larger in aligned municipalities. Interestingly, the larger increment in aligned municipalities turns to be insignificant when we control for the central government's transfers. As for the second point, we find a positive correlation between intergovernmental transfers and the probability of winning the election of the incumbent mayor. We also study the relation between transfers and the results of the following national presidential election. We show that the ruling coalition's vote share in national elections is higher in municipalities receiving larger transfers, even after controlling for alignment. We are aware that the amount of transfers is endogenous, and probably depends on municipal spending or prospective electoral outcomes. We are therefore cautious about these results and we present them as insightful correlations. Overall, however, we argue that the mechanism we are proposing is plausible and coherent with our data.

This paper is related to several topics discussed in the literature. First, it is related to studies on political budget cycles (PBC). The large literature on national PBCs has shown how typically pervasive these cycles are in developing economies and new democracies (Brender and Drazen (2005), Shi and Svensson (2006)). PBCs have also been analyzed at the local level. There is much evidence of PBCs in local European governments, such as in Germany (Seitz, 2000, Galli and Rossi (2002), Foremny and Riedel (2014)), Sweden (Pettersson-Lidbom (2010)), France (Foucault et al. (2008) and Portugal (Veiga and Veiga (2007).), and in less developed countries as India (Khemani (2004)), Brazil (Sakurai and Menezes-Filho (2008)), Colombia (Drazen and Eslava (2010)) and the Philippines (Labonne

(2016)). Our paper studies a cycle which is, at the same time, national and local. Indeed, in our case national politicians use intergovernmental transfers in order to influence the appointment of local politicians.

Secondly, our work is related to several works documenting the political motivation behind the allocation of intergovernmental transfers from national to local governments. The fact that intergovernmental grants have been used to win votes has been reported, among others, for Albania (Case (2001)), Sweden (Johansson (2003)), Spain (Sole-Olle and Sorribas-Navarro (2008)), India (Arulampalam et al. (2009)), Ghana (Banful (2011)) and Brazil (Brollo and Nannicini (2012)). In particular, this latter work shows that aligned mayors receive larger transfers from the government during the last two years before local elections are held. However, they do not separate the local from the national effects when studying the cycle, which is the focus of our work.

Thirdly, our findings also shed some light on the discussion regarding the incentives and practices of local officials. Several articles discuss the differences between elected and appointed officials. Due to career concerns, appointed officials have more incentives to influence voters in national elections (Martinez-Bravo (2014)), and fewer incentives to increase the provision of public goods (Martinez-Bravo et al. (2014)). However, elected leaders may also deviate resources towards their electoral basis (Dahlberg and Johansson (2007)) or use targeted redistribution in order to achieve their own goals (Aidt and Mooney (2014)). In this paper, we consider the role of local officials under limited fiscal autonomy. We claim that under fiscal centralization, the government does not require appointments to discipline local mayors, but merely the use of its fiscal capacity.

Finally, our work is related to the literature on political cycles and political brokers in Chile. On the first theme, Larrain and Assael (1997) find evidence that policy instruments, both monetary and fiscal, were used for electoral purposes during the second half of the past century, while at the local level Letelier (2011) concludes that Chilean municipalities increase fiscal borrowing during electoral years. In addition, Cerda and Vergara (2007) show

that the increase of governmental subsidies during election years had a significant effect on the voting share of the government coalition in the 90s. On the second matter, that is the role of mayors as political brokers, the seminal work by (Valenzuela (1977)) the author argues how local political leaders in Chile extracted resources from the central government through their contacts at the national level, in exchange for higher turnout of the local vote in favor of national representatives. While his study focuses on the 60s, several authors show that clientelistic practices by Chilean mayors has persisted to this day (Barozet (2008), Arriagada (2013), and Toro (2017)).

The article is structured as follows. In the following section we discuss the conditions and incentives under which there's scope for the role of local mayors as political brokers, with a particular description of the Chilean case. In section 3 we describe the institutional framework that shapes the economic and political context on which our study is based. Section 4 describes the data. Sections 5 and 6 discuss our baseline estimation results and the particular findings on the role of political alignment and close elections, respectively. The last section concludes.

2 Mayors as political brokers

Even when all politicians seek to influence the outcome of an election by distributing resources and favors, (Dixit and Londregan (1996)) most of the clientelistic spending or allocation of goods and services takes place at the local level. Naturally, in this context national politicians cannot easily identify target voters at the local level. In practice, they enlist the assistance of local brokers, middlemen or intermediaries, who have private information about grassroots constituencies (Szwarcberg (2012), Stokes et al. (2013)). Local political brokers are broadly used by national parties, even when incentives faced by the former ones are not directly aligned with those of the parties (Larreguy et al. (2016)).

Local officials are good candidates for local brokers that are meant to serve the interest

of national politicians. They have better knowledge about the preferences of grassroots constituencies and occasionally many of them are politically aligned with the government or the incumbent party or coalition. It is optimal therefore to delegate on them the implementation of local voter mobilization strategies. When local officials are directly appointed by the central government, their incentive compatibility constraints are straightforwardly achieved. A recent literature has pointed out to the fact that appointed officials have stronger incentives to signal their alignment towards upper hierarchies because their careers depend on them (Martinez-Bravo (2014), Aidt and Mooney (2014), Mu and Zhang (2014)). This literature emphasizes that elected officials have weaker incentives to influence voters during national elections because their careers do not depend on the government but on their own future electoral success.

However, appointment is not the only mechanism by which local mayors can be disciplined. Elected officials can also be influenced by the government through the provision of economic resources that go from the central government to the local one, and especially when these resources can influence the mayors own reelection chances. . That is, fiscal centralization generates an indirect mechanism of control of local politicians. We notice that career concerns may also be playing a role upon elected mayors, since their loyalty to national parties eases their transit from the local to the national political arena. However, transfers are the most important channel through which the central government can induce the electoral alignment of mayors in national elections.

Chile is a good case study of the role of mayors as political brokers. In the seminal work by Valenzuela (1977), the author provides the first systematic research on the intermediation role that local officials played in Chile during the late 60s. His work argued how Chilean mayors extracted resources from the central government through their contacts at the national level, in exchange for higher turnout of the local vote in favor of national representatives. Valenzuela argued that the clientelistic role of local officials was twofold. First, they generated a complex network of relationships in order to create an electoral base and

in return satisfy their economic and legal demands. Secondly, they created strong ties with national legislative and government officials, which served to collect resources in return for their local electoral support.¹ That is, mayors served as local-level brokers on behalf of the government and members of the congress, in their electoral campaigns; while congress members served as national-level brokers that supported mayors and allocated resources to their municipalities.

The Chilean dictatorship (1973-1989) tried to finish these clientelistic practices in order to reduce the influence of political parties in local politics, and thus created a new legislation in which the local administration was integrated with the national government. Municipalities gained more administrative autonomy, and more attributions in the provision of public services – as health and education – at the local level. Financial resources, however, remained in the hands of the government. These regulations triggered a change in the role of local officials, but scholars coincide in the fact that parties were able to adapt themselves to the new scenario and mayors continued to be electoral machines (Rehren (1996)). In a context of more administrative autonomy, fiscal employment and local bureaucracy became additional tools at the mayors’ disposal. These tools were used to generate a voter mobilization network within municipalities (Toro (2017)).

Several authors report that current local officials in Chile keep on exhibiting clientelistic patterns. For instance, when analyzing two different contests to win the mayorship— in Santiago and Iquique —², Barozet (2008) shows that support for candidates relies on the operation of social linkages through the delivery of goods and services — such as housing or employment —by the municipal administration. In the same vein, it is shown that the role of mayors is to generate, through the provision of municipal goods, strong clientelistic relationships with different social organizations in exchange for mobilization of the electorate in

¹The mayors’ comments compiled by Valenzuela are illustrative of their intermediation roles: “we are the work tools of *Parlamentarios*”, “the political parties depend on the mayor I tell my people how they have to vote”, “The *parlamentarios* are the people who open the doors” (Valenzuela (1977): 123).

²Santiago is the main municipality within the Chilean Capital, and Iquique is one of the largest cities in the Northern region of Chile.

Santiago ([Arriagada \(2013\)](#)) and Caldera, and La Florida and Talcahuano [Espinoza \(2006\)](#).³ [Durstun \(2005\)](#), finally, shows that in most rural municipalities we also find brokers who intermediate the provision of goods and services to communities, in order to encourage the mobilization of the electorate.

3 Institutional Background

Democracy was reestablished in Chile in 1990, after 17 years of dictatorship. Since then, two coalitions have ruled the country. The center-left “Concertación” governed during four consecutive terms, from 1990 to 2009. In 2009, the right-wing coalition “Alianza” won the presidency, but in 2013 the Concertación won office back again. Since 2005, the presidential term is 4 years long, without reelection. The last three presidential elections, which are the relevant ones for our analysis, took place in 2005, 2009 and 2013.

The municipalities are the smallest administrative units in Chile.⁴ Currently the number of municipalities is 345. In Chile, local decentralization is political and administrative, but not fiscal. Municipalities elect their own authorities and, to some extent, administer their own resources.⁵ However, municipalities cannot levy taxes nor fully decide the size of their overall budget ([Eaton \(2004\)](#), [Mardones \(2007\)](#)).

Chilean municipalities are ruled by the Municipal Council, at the head of which is a Mayor who detains executive power. Mayors are elected, since 2004, by a majoritarian voting system, and the Council is elected by a proportional system following a DHondt rule. The first municipal election during the post-authoritarian period took place in 1992, and since then elections have been held every 4 years. Until 2000, all local elections were indirect; a Municipal Council was voted by citizens and the council appointed one of its

³Caldera is a small city in the North of Chile, La Florida is one of the largest municipalities in the Capital Santiago, and Talcahuano is a large port in the South of Chile.

⁴A municipality resembles the role of a county in the United States. The largest cities, such as Santiago, are divided in several municipalities.

⁵Notwithstanding, several spending categories managed at the local level, such as health, housing and security, are decided by the national government.

members as Mayor. A reform for direct elections was passed in 2004. Data is available for all the period with direct elections, from 2001 to 2013, and thus our sample comprises the three municipal elections during this period, which were held at 2004, 2008 and 2012 . The first two were held while the center-left Concertacin detained the country's executive power; in the 2012 election, the right wing Alianza was in power. Our sample also includes the presidential elections, which were held a year after each municipal election, in 2005, 2009 and 2013.

Municipal finance has three main funding sources: own permanent revenues, the Common Municipal Fund (CMF) and transfers from the central government. The first two sources are generated by the municipalities themselves, mainly through the recollection of property and vehicle taxes receipts; these resources are divided into a mandatory contribution to the CMF and the residual is left as autonomous revenues. The purpose of the CMF is to homogenize the available resources across municipalities, in an attempt to narrow the large socioeconomic disparities across municipalities. The criteria defining the rules by which this redistributive instrument is implemented, are tightly regulated by Law,⁶ which sets the contribution of each municipality to the CMF's pool and the amount that each one receives back from this pool, according to predetermined parameters such as their population size, poverty rate, and several other indicators.

In this work we focus on the third source of municipal funding: transfers from the central government. These are decided and allocated by the Office of Regional Development (Subdere) mainly, which depends on the Minister of Interior or Home Affairs, and is in charge of coordinating the management of regional public funds. These funds are given to municipalities to enhance their local management, for infrastructure and improvement of specific neighborhoods, to foster local employment, prepare the national census, and also as aid in case of natural disasters.

As mentioned in the introduction, at least two conditions must be fulfilled in order to

⁶Ley de Rentas Municipales 3063, enacted in 1996.

make the case for an iPBC. Firstly, however big transfers as a whole may be as compared to the central government’s overall budget (plausibly small and maybe for this reason not drawing much attention from public opinion), they should represent a large share of the budget or campaigning expenditures at the local level. Secondly, the rules defining both the overall amount of transfers and how these transfers are distributed across local governments, should be to some extent discretionary. As we argue next, these two conditions are met in the Chilean case.

Table 1 shows the total amount of transfers to municipalities per year in our sample.⁷ In order to gauge their significance, we compare transfers as a fraction of both the national and local budget respectively.

*** TABLE 1 HERE ****

We notice that Municipal Transfers are about 1% of the central government’s total budget. In practice, this means that it is arguably easier for the government to increase transfers in electoral years as opposed to other larger budgetary items. At the same time, although transfers represent only about 7% of total municipal funding, the mean of transfers across municipalities is as large as 20%. This is due to the fact that the transfers share of the total local budget varies significantly across municipalities depending on their socioeconomic characteristics. While transfers are not important in the extremely rich municipalities, who manage a large fraction of their total municipal budget, they are quite relevant in practically all the remaining ones. For instance, while transfers represent less than 2% of the budget in Vitacura, the richest municipality in Chile, they represent 68% of the relatively poor municipality of Tocopilla. In conclusion, the central government’s transfers are sizable for a significant number of municipalities.

During the period of our study, there was a legislation change. In 2005, a new “Ley de Rentas Municipales” (Law of Municipal Rents) was enacted in order to be implemented the

⁷Details and discussion of the sources used in this table are provided in the next section.

following year, that is, in 2006.⁸ However, as is shown in the table, the Law did not imply a sharp increase of resources in 2006. In fact transfers turned out to be just a little more than 1% of the total budget in 2008, that is, in the following local election year after the reform.

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Secondly, a key aspect of our study is the discretionary nature of the allocation of transfers to municipalities. As we do not have direct measures in order to assess the central governments degree of arbitrariness when setting these transfers, we must rely on indirect arguments. First, we note that the laws that regulate these transfers are particularly ambiguous regarding the instruments scope and transfers' overall amount. This is particularly notorious when compared to the Law that defines with great detail how the allocation of the CMF should be performed.¹⁰ Secondly, the academic literature on the topic recognizes municipal funding policies have been typically driven by partisan motives (Eaton (2004), Mardones (2007)). For instance, the Public Reform Commission in 2009 indicated that municipal transfers “were typically very bureaucratic, discretionary and not always focused on the local realities” (Commission for Public Reform, 2009, pp169). Thirdly, the Subdere has been accused several times of using its funds for political intervention.¹¹

Finally, two additional points are worth noting ahead of our discussion. The first one regards the timing of both local and national elections, and decisions on the municipal budget. Local elections are held in October, while national elections are held in December. Intergovernmental transfers are part of the annual national budget, which is decided a year before its execution. Accordingly, the previous year the budget is executed, the government discusses the budget. This discussion starts every April, and the budget must be approved before December of that year.¹² Accordingly, the government decides municipal transfers

⁸Ley 20,033.

⁹While the annual change in transfers in 2005-2006 was 17%, in 2007-2008 it reached 38%. In any case, the existence of the iPBC is robust to the inclusion of a post-reform dummy.

¹⁰Decreto N 3063.

¹¹Parties of the right wing coalition Alianza denounced the Subdere of political intervention in 2009, under the Concertación's government (see La Tercera, January 14, 2009), and even in 2013, when their own coalition was in power (see La Tercera, January 20, 2014).

¹²Law 1263.

the year before the local election is held. This implies that the budget can effectively be used for electoral purposes during its execution.

The second concern is that Chile does not have particular restrictions on the overall amount of transfers that are spent during an electoral year.¹³ That means that the government is not particularly bounded in the allocation of funds in either local or national electoral years.

4 Data

Our dataset includes information of the 345 municipalities in Chile over period 2001-2013. We obtain information from the National System of Municipal Information (SINIM). The SINIM includes data on income transfers from the central government, that we refer to hereafter simply as ‘transfers’. Transfers are reported on a yearly basis and in constant 2013 prices. In order to account for population size wide differences across municipalities, we normalize transfers by population, obtaining transfers per capita. The municipalities’ population size is also obtained from the SINIM, which uses demographic forecasts by the Chilean Statistical Office (INE). We use population size also as a control to allow for other indirect effects that this variable may have on transfers. The dependent variable is thus the log of per capita transfers.

Figure 1 describes total transfers per capita for period 2000-2013, in millions of real Chilean pesos as for 2013. The dashed line represents the long-run trend, which corresponds to the predicted value of a fourth-degree polynomial estimation.

*** FIGURE 1 HERE ****

We observe that transfers increase systematically throughout. There is a sharp increase during the second half of the period.

¹³The “Ley de Rentas Municipales” (Law of Municipal Rents) does not establish any type of restriction for the total budget or its allocation during electoral years.

The main explanatory variables consist of a set of election year dummies. We define a dummy for local - municipal - election year, *LOC*, that equals one if year is either 2004, 2008 or 2012, and zero otherwise. And we define a dummy for national - presidential and legislative - election year, *NAT*, that equals one if year is either 2005, 2009 or 2013, and zero otherwise.

Other controls include several variables typically used in the literature on local government PBCs. Aside population size itself, the demographic composition of the population may have an impact on transfers. Accordingly, we consider the fraction of young population, defined as the percentage of individuals aged 15 and below, and the fraction of old population, defined as the percentage of individuals aged 65 and above. This information is based on demographic forecasts by the INE. As for other socioeconomic characteristics, we control for the poverty rate. This variable is obtained from the National Socioeconomic Survey (CASEN), which is conducted by the Chilean government every two or three years. For each year, we use the data from the latest CASEN survey.¹⁴ Finally, we also include a measure of the fraction of households in the municipality that are considered to be placed in rural areas, which we obtain from the INE.

We also build a “disaster” dummy variable that equals one if a municipality suffered a natural disaster at a particular time, and zero otherwise. Natural disasters are important determinants of transfers, and this is particularly relevant in a country such as Chile, where earthquakes abound.¹⁵ Information on disasters is collected from press records.¹⁶

Finally, we also collect electoral data from the Chilean Electoral Office (SERVEL) to build political variables. In order to study the interaction between transfers and the candidates’ political affiliation, we include information about the mayor’s political party and coalition every year. We create a dummy variable, *ALIGN*, that indicates whether the mayor is a member of any party belonging to the governments coalition, or not. We also use candidates’

¹⁴The CASEN was conducted in 2000, 2003, 2006, 2009 and 2011.

¹⁵The most damaging earthquake, worldwide, on record, took place in Chile in 2010.

¹⁶We use a word search (disaster, earthquake, tsunami, flood and alluvium) in “El Mercurio”, the main newspaper in the country, for the period of our study.

vote share in local elections, in order to build the difference between the winner and the runner up competitor. This variable, which we call *MARGIN*, indicates whether races were tight or not, and allows us to test whether the government is allocating resources towards swing voter municipalities or not. Finally, we collect data on local and national elections as well. In particular, we consider data on the reelection of incumbent mayor's in municipal elections, and the ruling coalition's vote share in national elections, in order to test whether these figures are correlated with transfers, or not. Given that in some specifications we use *ALIGN* and *MARGIN* from the past elections (that is the lag of election years), we consider electoral data from the municipal election in 2000 onwards.

5 Evidence of an iPBC

Before discussing our estimation results, we provide some graphical evidence of the iPBC. We consider the difference between effective transfers and its long run trend - a fourth-degree polynomial - shown in Figure 1, which is the short run trend. We compute the percentage deviations defined as the ratio between the short and long run trends, and we plot them against time in Figure 2.

*** FIGURE 2 HERE ****

Figure 2 shows that deviations of per capita transfers with respect to the long run trend are systematic and significantly larger during local election years (which are marked by the dashed vertical lines). The percentage change is on average 15% higher during years 2004, 2008, and 2012. We also note that transfers do not increase during national election years, each of which was held the following year a municipal election took place.

5.1 Specification

In order to provide formal evidence on the existence of an iPBC, we consider the following empirical specification:

$$y_{it} = \beta_\ell LOC_t + \beta_n NAT_t + \gamma x_{it} + f(t) + \delta_i + \varepsilon_{it} \quad (1)$$

where y_{it} is the log of the per capita transfer for municipality i at time t , LOC_t and NAT_t are dummies for municipal and presidential election years, respectively, x_{it} is a set of controls, $f(t)$ is a fourth-degree polynomial time trend,¹⁷ and δ_i is a municipality specific term. In our baseline estimation, we assume that the error term ε_{it} is clustered by municipality.

We first estimate equation (1) using ordinary least squares (OLS) with no fixed effects. Our preferred estimation, however, uses fixed effects (WITHIN) in order to control for municipal non observables that do not change over time. In addition, if transfers exhibit some degree of serial correlation we need to include lags of the dependent variable on the right hand side of equation (1). As a robustness check, our main table of results also reports the WITHIN estimation of a dynamic panel with the lag of transfers added as an explanatory variable.

5.2 Baseline Results

Table 2 shows our baseline results. In the first two columns we report the OLS estimation, and the second two columns show the WITHIN estimation results. All regressions control for transfers' time trend.

*** TABLE 2 HERE ****

¹⁷We build a trend from our time variable because funds delivered by the central government increase over time, as we note from Table 1. We notice that the presence of time dummies precludes the identification of our dependent variables LOC and NAT . We provide several robustness checks for this polynomial trend.

Table 2 shows that during local election years transfers increase. This is a robust result. Coefficient β_ℓ is positive and statistically significant at a 1% level, for all estimators and specifications.

The effect is as large as 6% (see last column), which is meaningful. To gauge its magnitude we compare this increase with the amount that mayors spend during electoral campaigns. Indeed, the implied additional amount on transfers during local election years is twice as large as the amount that mayors spent on average in the 2008 electoral campaign (Edwards et al. (2012), Rivera (2012)).¹⁸

Contrary to the local election case, transfers do not increase during national election years. This is clear in the estimation of the static specifications reported in the second and third columns. In the case of column (4), the coefficient for NAT has a significant negative effect. However, its sign only indicates that mean reversion - after the increase in local election years - is automatic the following year after the local election takes place.¹⁹ In addition, we test whether the coefficients on the LOC and NAT dummies are statistically different or not. We report the p-value of the null hypothesis that the coefficients are the same. P-values are close to zero in all estimations. This reassures that the two coefficients are different.

Most control variables' coefficients are not statistically significant and therefore should not be taken conclusively.

5.3 Robustness Checks

In this subsection we discuss several robustness checks on our main findings on a number of distinct dimensions. Estimation and tables displaying these results are in the Appendix.

In Table 2, column (4), we show that our main result is robust to the inclusion of lags of transfers as explanatory variables. We are aware that in this case the WITHIN

¹⁸On average transfers are increased during a municipal election year by about 30 million Chilean pesos while the elected mayors spent about 10 million Chilean pesos on average in their electoral campaign during the 2008 election year.

¹⁹In Appendix A, where we discuss several dynamic specifications, we explain this result in detail.

estimators are biased. This bias may be significant if the panel is a short one as ours (Nickell (1981)), in which $N=345$ but $T=10$. The usual strategy in order to correct the bias is to use GMM estimators, which are built with further lags of the dependent variables as instruments (Anderson and Hsiao (1981), Arellano and Bond (1991)). In the appendix, we provide a discussion and results for General Method of Moments (GMM) estimation. Table A1, in columns (1) and (2), shows that our results are robust to this dynamic specification, with LOC positive and statistically significant at a 1% level. We also reject the null hypothesis that LOC and NAT coefficients are the same.

Secondly, we provide several robustness checks on results in Table 2. These checks tackle both the time trend and the error structure. First, we estimate the effect of LOC_t using two, three and four-degree polynomial time trends. Second, we consider clustering not at the municipal but at the province level, and we also consider multidimensional clustering (Cameron et al. (2011)) with a province-year cluster robust variance. Thirdly, we consider an electoral cycle fixed effect, and we enhance this specification including a dummy for the second presidential year. Finally, we allow the time trend to vary across provinces and regions. Table A2 reports all these results. We obtain significant results in our main variables in almost all these checks, and all estimations but one reject the null hypothesis that the coefficients for LOC and NAT are the same. Overall, we take these results as evidence that the existence of an iPBC is robust to several specifications with different time term and error structures.

Finally, we implement a placebo exercise. We already argued that rules defining both the overall amount and the distribution of transfers are to some extent discretionary. However, there some other resources financing the municipal budget, which are more tightly defined and which cannot be easily manipulated by neither the local nor the national government. This is the case, for instance, of local tax revenues. These revenues are decided by law, and cannot be modified by the majors. We use tax revenues as a placebo. The LOC and NAT variables should not have any bearing on this variable. We expect the iPBC only to be

visible on the discretionary part of the budget. This exercise also reduce concerns that the results are merely picking some unusual time trends. In Table A3, column (1), we reproduce the baseline specification but using the log on per capita tax revenues as dependent variable instead of transfers. The table shows indeed that local and national elections have no effect on tax revenues. This reassures that our results are not due to the particular specification.

6 The iPBC's Mechanism

In the previous section we provide evidence on the existence of an iPBC in Chile. Our interpretation of the cycle is that the government influences local elections in favor of mayors belonging to its own coalition, with an eye on the influence and role they can have on the national election the following year. This interpretation has several testable implications.

First and most obviously, we expect to see a positive cycle only on aligned mayors. It is reasonable to expect a higher increase in local transfers during electoral years, in municipalities where incumbent mayors belong to the government's political coalition. In non-aligned municipalities, on the contrary, we do not expect an increase of transfers but rather a decrease in the allocation of funds if any.

Secondly, we also expect transfers allocated to aligned mayors to increase the tighter is the local race, or in swing municipalities. Finally, we conjecture that local election results may have an effect on national election results the following year. In particular, we expect the ruling coalition vote share in national elections to be higher in aligned municipalities than in non-aligned ones. We discuss these two implications in what follows.

After having discussed the patterns that describe the evolution of intergovernmental transfers during election years, we move to the question on whether transfers have an effect on municipal spending and electoral outcomes, or not. In both cases transfers are the explanatory variable. We are aware that the amount of transfers is endogenous, and they probably depend on either spending or prospective electoral outcomes. We make clear, there-

fore, that we take our results in this part only as insightful correlations. Notwithstanding, the fact that these relations show to be significant reassures that the mechanism we are proposing is plausible and coherent with our data.

First, we study whether transfers have an effect on total municipal spending or not. In particular, we expect overall municipal spending to be larger in aligned municipalities than in non-aligned ones during local election years. Secondly, we investigate whether transfers are somewhat related to local election results. We consider the correlation of transfers with the probability of winning of the incumbent mayors. We also check whether transfers are related with the ruling coalitions vote share in the following national election, or not.

As before, our empirical strategy takes advantage of our panel’s within-municipality and within-time variance. An alternative method that provides a causal identification for this result is to use close races in order to implement a Regression Discontinuity Design (RDD).²⁰ However, the use of close elections mightily reduces the sample size. When we impose the difference between the winner and the runner up to be less than 5% of their votes, the number of close elections is about 25 contests each year, with our sample summing up to 75 observations under this restriction.²¹ A larger window size, with the margin defined as less than 10%, renders only 140 observations. In any case, the sample size is not large enough to provide reliable inference on a causal effect.

6.1 Political Alignment

Herein, we provide some graphical evidence on the impact of political alignment. We compute the short run percentage deviations, as the ones depicted in Figure 2, but now we separate between politically aligned and non-aligned municipalities. Figure 3 shows the results.

²⁰Brollo and Nannicini (2012) use this approach with data from Brazil to deal with a similar problem. We observe that Brazil has about 5,500 municipalities while Chile has about only 350.

²¹We notice that each tight contest is only used once, in the next municipal election. Accordingly, close elections in 2000, 2004 and 2008, which are 21, 27 and 27, respectively, provide 75 observations in which we can observe changes to transfers during the 2004, 2008 and 2012 municipal elections.

*** FIGURE 3 HERE ****

In Figure 3 the dark line shows percentage deviations of transfers for municipalities where mayors were aligned with the government, while the light line displays the same for non-aligned mayors. We observe that the political cycle is only noticeable for the aligned ones, while quite irregular for municipalities not belonging to the incumbent coalition. At the same time, the size of the cycle for aligned municipalities is higher than the one reported in Figure 2, with a percentage increase of 25% in transfers on average for municipalities belonging to this subgroup.

In order to study the alignment hypothesis we define $ALIGN_{it}$ as a dummy variable that indicates whether the mayor in municipality i is aligned with the government coalition at time t , or not. We include this variable, $ALIGN$, in (equation 1) and an interaction term between LOC and $ALIGN$ to see its effect throughout the political cycle. Estimation results are shown in Table 3 below.

*** TABLE 3 HERE ****

The baseline results for alignment are shown in the first three columns of Table 3. We note that $ALIGN$ is positive and statistically significant for all specifications. When we introduce LOC , in the second column, we verify that transfers increase during election years, irrespective of the political coalition to which mayors belong. However, when the interactive term is introduced LOC is no longer significant, while the interactive term is. This means that the cycle is only noticeable for politically aligned incumbents.

The last two columns correspond to robustness checks. Note that in the present case, the relevant variable in our equation is an interaction term. We can therefore use year fixed effects. In the fourth column we consider year dummies, and in the last one we consider province-specific time dummies.²² We verify that the cycle is meaningful for politically aligned mayors.

²²This implies the introduction of $54 \times 13 = 702$ dummies.

In the appendix, we provide some additional robustness checks on the role of alignment. Table A1, which reports a dynamic specification, also shows that $ALIGN \times LOC$ is significant while LOC is not, reassuring that the existence of the cycle is only significant in aligned municipalities. In Table A3, we verify that in the placebo exercise using tax revenues as the dependent variable, alignment is irrelevant.

6.2 Margin and Swing Voter Municipalities

In addition to targeted transfers to aligned municipalities, we conjecture that the government manipulates these transfers to enhance its reelection probability, in order to have them as incumbent mayors in the following national election. If so, we expect transfers to increase in swing municipalities where elections have been historically tight.²³ The alternative hypothesis, namely that the government allocates resources to their core supporters (Cox and McCubbins (1986)), is less appealing in our particular context, because the government can favor core supporters during non-electoral years while concentrating its efforts towards swing municipalities during local election years.

We define $MARGIN$, our measure of electoral competitiveness, as the difference between the winner and the runner up candidate during the latest election.²⁴ To test the effect of $MARGIN$, we notice that the effect is mediated by alignment. Among aligned municipalities, the government has incentives to allocate resources in places where the incumbent won with a narrow margin, because revenues can make a difference on tighter races. Conversely, unaligned municipalities whose incumbent won with a narrow margin received less transfers for the very same reason.

Table 4 show the results.²⁵

²³As mentioned further above, there exists evidence on the targeting of intergovernmental transfers to swing voter districts in several countries (Johansson (2003), Arulampalam et al. (2009), Banful (2011), Brollo and Nannicini (2012)).

²⁴This measure can be misleading when there exists a third important candidate. As Chilean electoral races were for most if not all electoral contests between the two main coalitions, this is not problematic here. Nevertheless, all the results are robust to the normalized version of $MARGIN$.

²⁵All the results in Table 4 are robust to the inclusion of year fixed effects instead of the time trend.

*** TABLE 4 HERE ****

In the first two columns, we show that *MARGIN* itself is not significant, and that it has a positive and significant sign when we control for local election years. This means that the government, during non-electoral years, favors core supporters belonging to the two main coalitions. However, we already explained that the results must be conditioned on alignment. Accordingly, in columns (3) and (4) we consider the separate samples containing aligned and unaligned municipalities, respectively. For aligned municipalities, we observe that the government increases significantly the amount of transfers in narrow-margin municipalities. The effect, on the contrary, is not observed in unaligned municipalities. In column (5), we introduce a triple interaction term. We would like to see the effect of *MARGIN* in aligned municipalities in local election years. The effect is also significant although at the 10% level.

Finally, we deviate from our assumption that the role of margin enters through a linear interaction, since the effects might not be linear. In the last two columns, we distinguish between municipalities where the incumbent was elected with a narrow margin - less than 5 percent points - versus municipalities where the margin was higher than 5 percent. We observe that the relevant coefficient, which is the interaction between *LOC* and *ALIGN*, is twice as large for tight electoral races as compared to non-tight ones.

6.3 Spending

We have shown how transfers increase during local election years. Our main hypothesis is that this obeys to political motivations and objectives. Indeed, if larger transfers do not imply larger spending during election years, our hypothesis would be flawed. It is precisely in order to foster the likelihood of winning the election at the local level that national government target resources towards politically aligned municipalities. And the mechanism through which mobilization operates is through targeted spending to which root constituencies are particularly sensitive electorally speaking. Overall municipal spending, therefore, were our

hypothesis right, should increase during local election years. Recall that municipalities have limited fiscal autonomy. An increase in the central government’s transfers is likely to have an effect on the municipalities’ spending pattern. In order to test this crucial step, we estimate the political cycle’s effect on the log of total local per capita spending. In Table 5 we report these estimations.

*** TABLE 5 HERE ****

Table 5 shows several interesting results. First, in column (1), we notice that spending increases during both municipal and national elections. This means that in spite of municipalities’ limited fiscal autonomy, they still manage to create a local political budget cycle and are able to increase spending during national election years. The local election effect is three times higher than the national one, however. In column (2), we report that *ALIGN* is positive and statistically significant for local spending as well. However, when the interactive term $LOC \times ALIGN$ is introduced, in column (3), *ALIGN* is no longer significant, while the *LOC* and the interactive term are significant. This means that the local cycle in spending is higher for align municipalities, as expected.

Our conjecture is that the increase in spending in aligned municipalities is induced by intergovernmental transfers. Accordingly, in the last two columns in Table 5, we introduce the log of per capita transfers on the right hand side of the specification. Recall that the log of per capita transfers is the dependent variable in Tables 2 and 3. We observe the both *ALIGN* and $LOC \times ALIGN$ coefficients show to be statistically insignificant after introducing transfers. This suggests that the effect of alignment is due to intergovernmental transfers. We take these results cautiously however, in consideration of the likely presence of endogeneity between both transfers and overall spending.

Unfortunately, we do not have disaggregated data on municipal spending. However, a recent report by the Chilean Accountability Office²⁶ sheds some light on municipal spending

²⁶ “Estudios sobre Gastos en Publicidad Difusión en el Sector Municipal” (2016).

on several items during election years. According to the report, “advertising and diffusion” increase systematically during election years. The Office also reports that in the latest local election, that is 2012, only one third of the municipalities increased advertising spending in less than 5%; another third increased this item between 5% and 50%, and the last third increased this item in more than 50%.

6.4 Transfers and Political Electoral Outcomes

We have reached the final step in the mechanism through which intergovernmental transfers create a political budget cycle. Naturally, one expects national governments to implement this strategy only if it pays to do so. Why would they bother systematically and through several election years to manipulate transfers if transfers did not matter for their final goal, which is their own reelection. Under our hypothesis, thus, transfers should influence national electoral outcomes. And if this is so, we should expect transfers to be somewhat correlated with two different types of political outcomes. First, aligned municipalities that receive more transfers should outperform electorally those that don't. This means that extra transfers should effectively pay off in local elections. Secondly, we expect transfers to be correlated with the incumbent government or coalition vote share in the presidential. In recent contributions [Martinez-Bravo \(2014\)](#) and [Brollo and Nannicini \(2012\)](#) show that local mayors do influence voters in national elections in Indonesia and Brazil, respectively. For the Chilean case, there exists qualitative evidence that mayors mobilize voters at the local level in legislative and presidential elections, ²⁷ and [Izquierdo et al. \(2009\)](#) report a positive correlation between the parties' local and legislative election results. Accordingly, if transfers increase the politically aligned mayors' probability of winning their electoral race, they should also increase the vote share of the ruling coalition in the following presidential election.

In order to provide additional evidence on these additional conjectures, we first consider

²⁷As we have discussed in detail in Section 2.

whether transfers are related to the probability of winning the local election, or not. In particular, we consider the following empirical specification:

$$E_{it} = \alpha y_{it} + \gamma x_{it} + \delta_t + \delta_i + \varepsilon_{it} \quad (2)$$

where E_{it} is a dummy that equals one if the incumbent mayor's coalition wins the local election in municipality i at time t , y_{it} is the log of per capita transfers, x_{it} is a set of controls, and δ_t and δ_i are year and municipality dummies respectively. Notice that the exercise uses a panel which includes only local election years: $t = 2004, 2008$ and 2012 . The error term ε_{it} is clustered by municipality.

As for the second correlation, between transfers and presidential outcomes, we use again equation (2) but applied to presidential elections: that is, $t = 2005, 2009$ and 2013 . Now the dependent variable V_{it} is the vote share of the ruling coalition in municipality i in the presidential election t . Among the controls we include $ALIGN_{it}$, which indicates whether the mayor is politically aligned to the incumbent coalition at the time of the national contest, or not, given that alignment is likely attenuating the effect of transfers on vote shares.

The estimation results are described in Table 6.

*** TABLE 6 HERE ****

The first three columns in Table 6 estimate directly equation (2) with E_{it} as the dependent variable. Given that the dependent variable is discrete, in columns (1) and (2), we estimate a PROBIT model without municipality fixed effects. In the table we report the estimated coefficients. The marginal effects are about one half of the respective coefficients, in both specifications. The introduction of fixed effects, however, generates an incidental parameter problem. To address this problem we estimate in column (3) a linear probability model.²⁸ In all regressions, the coefficient on log of per capita transfers is positive and significant.

²⁸In any case, the PROBIT estimation with fixed effects also delivers significant results. The coefficients are much higher than those in the table. This is because the estimator only exploits the part of the sample that varies over time.

Next we consider the presidential vote share V_{it} as the dependent variable, and we provide results for a WITHIN fixed effect estimation. We observe that transfers have a positive effect on the presidential vote share. We also test if this result is robust to the inclusion of *ALIGN*. As expected, the coefficient for *ALIGN* is positive and significant, meaning that aligned mayors are effectively mobilizing voters in the presidential contest. But at the same time, its inclusion decreases the coefficient on the log of per capita transfers. That is, alignment is attenuating the direct effect of transfers on the incumbent coalition vote share. Either way, the effect remains significant after including these further controls.

Summing up, we find a positive correlation between transfers and both local and national electoral outcomes. However, we don't want to overemphasize the point. Fixed effects are controlling for non observables that do not changed over time, but any other determinant which changes over time has not been controlled for. All in all, the results are consistent with the proposed mechanism behind the reported iPBC.

7 Conclusions

In this paper we show evidence of a political budget cycle during local elections in Chile. The budgetary item that is manipulated by the central government consists of transfers to local governments. Crucially, transfers are set following soft budgetary constraints and discretionary allocation rules. Using a panel consisting of 345 municipalities and 13 years, throughout which 3 municipal and 3 presidential elections took place, we show robust evidence that transfers to municipalities increase during municipal elections, and that this increase is larger and significant when incumbent local governments are politically aligned with the national government. Among aligned mayors, transfers are larger the tighter is the election contest.

The work's main contribution is to point out to, measure, and describe, an indirect PBC where both national and local governmental levels interact. In contrast to traditional

political budget cycles, in which politicians use policy instruments to have an impact on her own reelection prospects, in our case the incumbent manipulates fiscal policy to influence the election of another public official, in an election other than her own.

The article is also shedding some light on the negative effects of fiscal centralization. Much of the literature focuses on the fact that appointed local officials have strong incentives to serve as political brokers of national politicians due to career concerns. We claim in this work that these detrimental practices may also be present in contexts where elected officials face limited fiscal autonomy.

A Appendix: Robustness

A.1 GMM Estimation

Transfers might exhibit some degree of serial correlation. If this were the case, lags of the dependent variable should be considered as explanatory variables on the right hand side of equation (1). Additionally, the WITHIN estimator, as the one reported in the last column of Table 2, would be biased. Indeed, consider the first difference estimator, $y_{i,t-1}$ in $\Delta y_{i,t-1} = y_{i,t-1} - y_{i,t-2}$. This variable is correlated with $\varepsilon_{i,t-1}$ in $\Delta \varepsilon_{it} = \varepsilon_{it} - \varepsilon_{i,t-1}$. The bias can be problematic in a short panel as ours (Nickell (1981)), in which $N = 345$ but $T = 10$ (which is not large enough). To address this problem, Anderson and Hsiao (1981) recommend using $y_{i,t-2}$ as an instrument variable for $\Delta y_{i,t-1}$. Several estimators improve the efficiency of the Anderson-Hsiao strategy using additional lags of the dependent variable as additional instruments, and the use of the General Method of Moments (GMM) to deal with over-identification (Arellano and Bond (1991)).

Yet, the inclusion of additional instruments may create problems as well. A key assumption of the GMM estimator is that the instrument matrix is exogenous. As is the case in specifications with too many instruments, we can test the validity of the moment conditions jointly. The Sargan test, which has the null hypothesis that over-identification restrictions are valid, need to not be rejected in order to increase the number of instruments. In our case, however, the use of the complete set of instruments proposed by Arellano and Bond (1991) creates an over-identification problem.²⁹ To deal with this problem, we reduce the numbers of lags and collapse the instrument matrix (Roodman (2006)). The reported results are the ones with the best performance in terms of the Sargan test and the autocorrelation statistics. As usual, the reported Sargan test corresponds to the one obtained in the second step estimation, while the explanatory variables' coefficient estimation is obtained from the one-step estimation.

²⁹This is also the case with the Blundell and Bond (1998) estimators.

Table A1 show our GMM estimation results. The first two columns are related to the baseline results in Table 2. The last three columns are related to estimation of the alignment hypothesis as shown in Table 3.

*** TABLE A1 HERE ***

Table A1 shows that our main findings are robust to several dynamic specifications. For the baseline specification, the coefficient β_ℓ is positive and statistically significant at a 1% level, for all GMM estimation results. As indicated by the Sargan test, the GMM estimation models are well specified. The estimated GMM coefficient for the lagged dependent variable lies within the bounds defined by the dynamic versions estimated with the OLS and WITHIN estimators (not reported here), which satisfies the ‘rule of thumb’ specification test. The second autocorrelation test also is passed in favor of the GMM specification.

The estimated coefficients on *NAT*, which are negative and significant in all regressions, require further discussion. Recall that presidential and legislative elections take place, in all cases in our sample, the following year a municipal election is held. Accordingly, in a dynamic setup there are two effects to consider in a national election year. First there is a direct effect captured by β_n (reported in table 2). Secondly, due to the dynamic specification including a lagged term of the dependent variable, a positive value of β_ℓ implies that transfers should also increase in the following years because mean reversion is not automatic. This effect is captured by $\alpha\beta_\ell$. The overall estimated effect, thus, is $\beta_n + \alpha\beta_\ell$. Accordingly, we report whether we can reject the null hypothesis that $\beta_n + \alpha\beta_\ell = 0$, or not ($Prob > F$). In all cases, we cannot reject the null hypothesis that the overall effect is zero in national election years. Overall, the fact that transfers do not increase in national election years is also robust to the dynamic specification.

Table A1 also shows the cycle for aligned versus non aligned municipalities. Columns (3) to (5) replicate the results of columns (1) to (3) in Table 3. The main result again, in column (5), is that $ALIGN \times LOC$ is significant while LOC is not, which reassures that

the cycle is only meaningful for aligned municipalities. The GMM estimation satisfies the Sargan and second autocorrelation statistical tests.

A.2 Time Trends and Clustering

Here we provide a number of robustness checks for the results in Table 2. Here we tackle both the specific time trend and error structure. First, we discuss our time trend polynomial choice in Table 2. Table A2 reports our estimation results. In columns (1), (2) and (3), we estimate the effect of LOC_t using two, three and four-degree polynomial time trends, and we report the Akaike informational criterion (AIC) for each one of them. Secondly, we consider other possible ways of clustering observations in the error term. In column (4) we cluster observations at the provincial level, and in column (5) we implement a multidimensional clustering (Cameron et al. (2011)) at a provincial and year level.³⁰ Thirdly, we consider additional time controls. In column (5), we include an electoral cycle fixed effect, i.e. dummies for the 2002-2008 and 2009-2012 periods, leaving 2001-2004 as the omitted category. In column (6), we enhance this specification including a dummy for the second presidential year.³¹ Finally, we allow the time trend to vary across provinces and regions. We use specific time trends at a higher level of aggregation (provincial and regional instead of municipal) because the use of municipality-specific time trends reduces our sample in one third. In all estimations we report the hypothesis test that the estimated coefficients on LOC and NAT are the same.

*** TABLE A2 HERE ****

The first three columns in A2 test different polynomial trends. We notice that the effect of LOC_t on transfers is elusive for the quadratic polynomial trend, while it is positive and

³⁰The 345 Chilean municipalities are partitioned into 54 provinces and 15 regions.

³¹Given that our specifications already include dummies for NAT and LOC, this specification includes a full set of electoral year dummies.

significant for three and four-degree polynomial trends.³² The informational criterion indicates that the fourth-degree polynomial is the specification that minimizes the information loss. In the fourth column, the significance is robust to clustering at the provincial level, but the effect is no longer significant for the provincial and year multidimensional clustering because the standard error is twice as large than in the case of single provincial clustering. In columns (6) and (7), we show that our results are kept after having added electoral period and second term year dummies, although the coefficient are slightly lower. Finally, in the last two columns we show that the results are robust to the inclusion of province or region-specific time trends, respectively. Overall, we interpret the results in Table A2 as evidence that the existence of an iPBC is robust to several specifications with different time trends and the error structure.

A.3 Placebo Exercise

A natural concern regarding the existence of the cycle is that it may be driven by a particular election of the time trend or error structure. While we have controlled for a variety of specifications in the previous subsection, a placebo exercise will reassure that the iPBC is not merely picking some spurious and specific phenomenon laying hid in some peculiar error structure or time trend. In order to discard this possibility, we run the same specifications on some dependent variable influencing municipal funding and finance, but on which there are tighter rules and less scope for discretionary allocation. That is, we use a dependent variable that is more strictly regulated by law, with few scope for manipulation by either local or national governments. If our previous results are evidence of a political business cycle emerging from limited fiscal autonomy and discretionary allocation, then we should not observe any effect of either LOC, NAT or ALIGN on this variable.

We use the log of per capita tax revenues as our placebo variable. Local tax revenues mainly consist of taxes which are determined by law, and on which there's little scope for

³²The effect is also positive and significant for higher order trends, not reported here.

manipulation either by the local or national government. In table [A3](#) we show our results.

*** TABLE [A3](#) HERE ***

In the table, the first column reproduces the baseline specifications used in our main results, using here the log of local per capita tax revenues as the dependent variable instead of transfers. We observe that neither LOC nor NAT are significant. This implies that there is no evidence of a cycle at any level. The last three columns are related to estimation of the alignment effect, as shown in Table [A3](#). Again, there is no evidence of a political cycle pattern. In column (3) the interaction between ALIGN and LOC is significant though negative. However, the effect is removed when year dummies are included. Overall, and as expected, we find no evidence of a political cycle pattern on local tax revenues.

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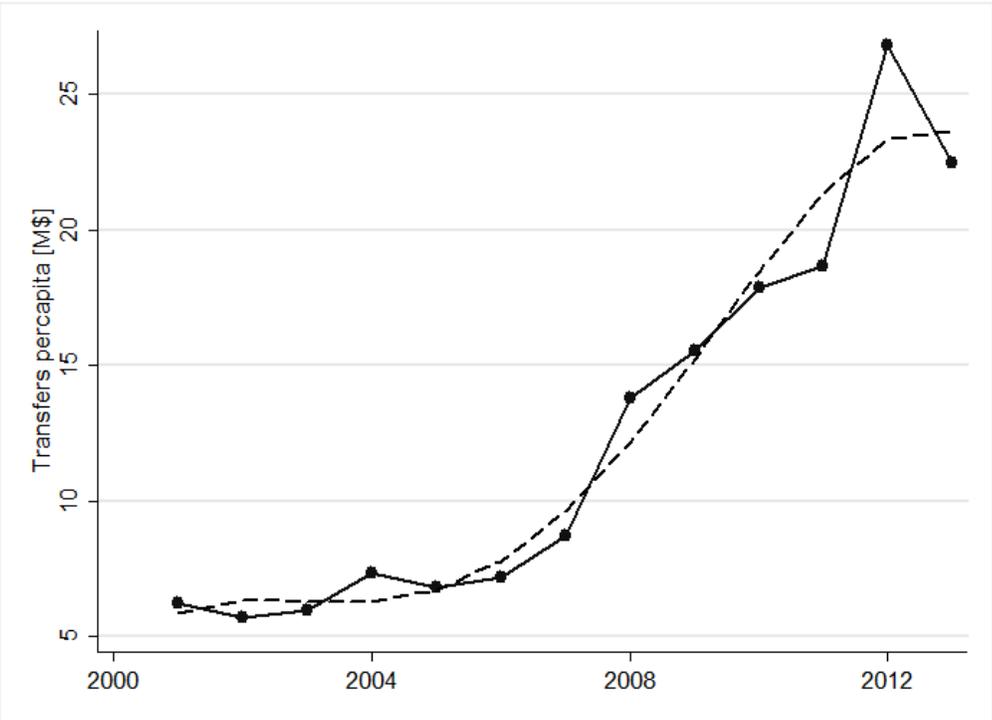


Figure 1: Per capita transfers (millions of Chilean Pesos as of 2013)

Note: Total transfers per capita for period 2000-2013, in millions of real Chilean pesos as for 2013. The dashed line represents the long-run trend, which is the predicted value of a fourth-degree polynomial estimation.

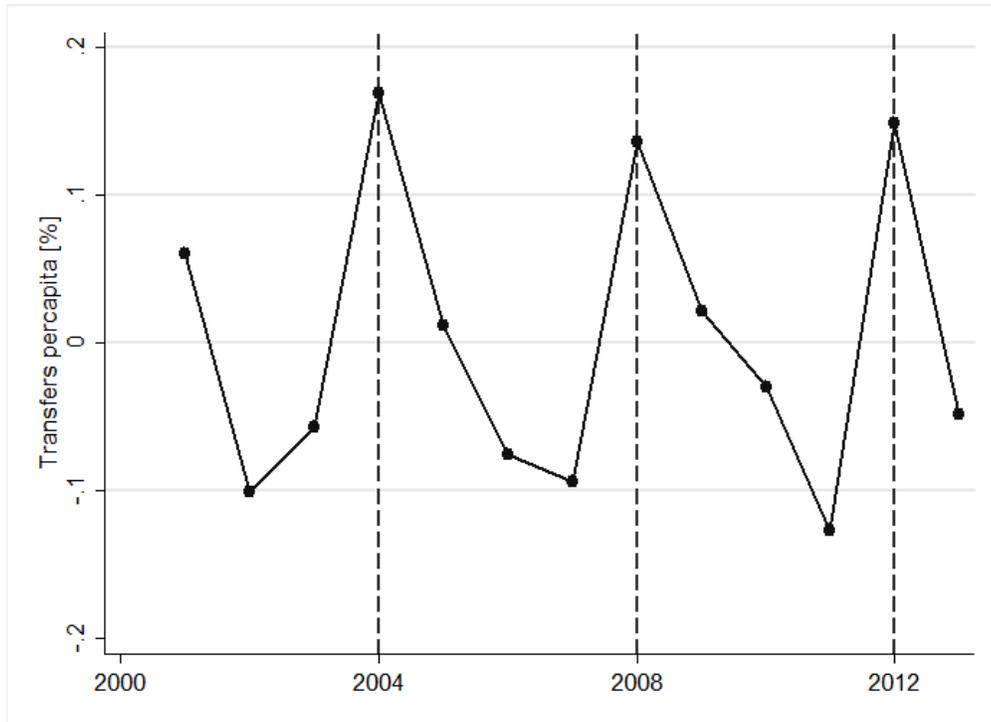


Figure 2: Per capita transfers (percentage)

Note: The figure is the percentage deviation between effective transfers and its long run trend - a fourth-degree polynomial - plotted over time. The dashed lines are municipal election years.

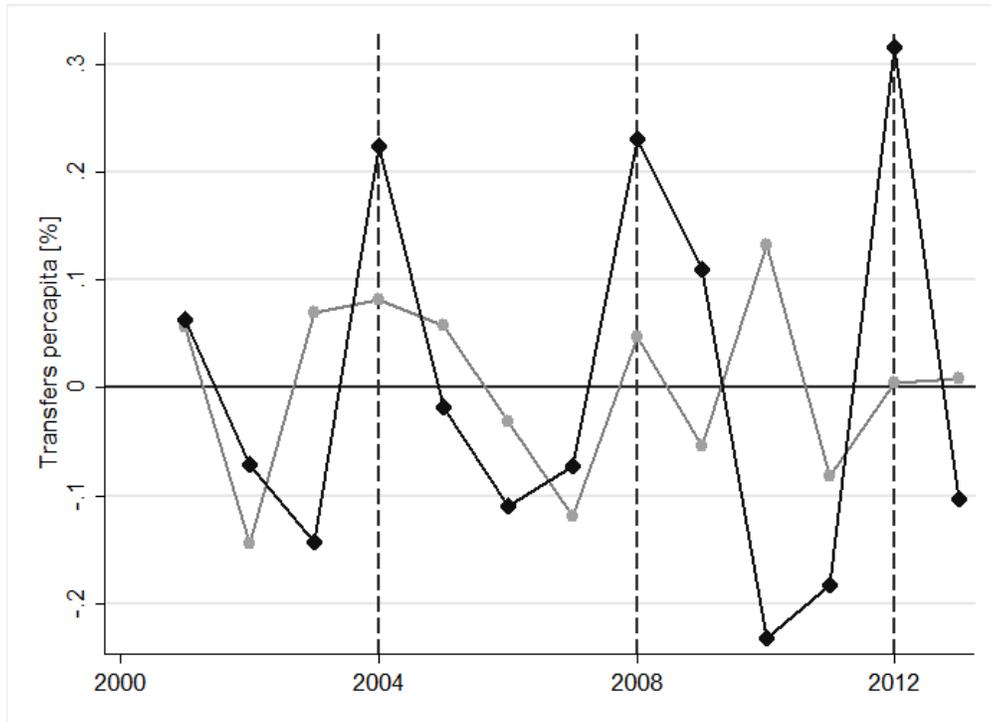


Figure 3: Per capita transfers (percentage)

Note: The figure is the percentage deviation between effective transfers and its long run trend - a fourth-degree polynomial - plotted over time for aligned (black) and unaligned (gray) mayors. The dashed lines are municipal election years.

Table 1: Transfers, and Central and Local Budget

Year	T[mM\$]	CGB[mM\$]	T/CGB[%]	LB[mM\$]	T/LB [%]	Av.[%]	Min[%]	Max[%]
2001	114	10939	1.0	1498	7.6	11.0	0.0	56.5
2002	115	12226	0.9	1563	7.4	10.6	0.0	52.6
2003	98	12849	0.8	1690	5.8	9.0	0.0	48.2
2004	112	12989	0.9	1794	6.2	9.8	0.0	71.2
2005	98	13599	0.7	1786	5.5	9.1	0.0	70.9
2006	115	15298	0.8	1935	6.0	9.6	0.0	76.9
2007	146	17883	0.8	2023	7.2	11.8	0.0	62.5
2008	201	20651	1.0	1955	10.3	16.6	0.1	63.5
2009	249	23407	1.1	2220	11.2	17.5	0.1	63.6
2010	294	25652	1.2	2314	12.7	19.1	0.1	69.0
2011	286	28984	1.0	2485	11.5	17.0	0.4	71.4
2012	394	30890	1.3	2744	14.4	20.1	1.4	71.2
2013	402	33247	1.2	2807	14.4	18.8	1.4	67.8

Sources: Transfers (T) and Local Budget (LB) are obtained from the System of Municipal Information, SINIM (<http://www.sinim.gob.cl>). Central Government Budget (CGB) is obtained from the National Budget Office, DIPRES (<http://www.dipres.gob.cl>).

Note: Av., Min and Max, correspond to the average, minimum and maximum of T/LB.

Table 2: Transfers in Local and National Election Years

	Log per capita Transfers			
	OLS		WITHIN	
	(1)	(2)	(3)	(4)
LOC	0.099*** (0.016)	0.040*** (0.014)	0.033** (0.013)	0.061*** (0.014)
NAT		-0.001 (0.017)	-0.006 (0.017)	-0.038** (0.017)
Log Population		-0.677*** (0.045)	-0.746 (0.522)	-0.282 (0.414)
Rural Dummy		-0.001 (0.001)	-0.004 (0.003)	-0.001 (0.003)
Population below 15 [%]		-0.811 (0.689)	-3.367 (2.501)	-2.301 (2.139)
Population above 65 [%]		-2.763*** (0.851)	-1.256 (3.148)	-0.917 (2.233)
Disaster Dummy		0.02 (0.018)	0.047*** (0.015)	-0.014 (0.016)
Poor [%]		-0.105 (0.191)	0.139 (0.220)	-0.013 (0.187)
Log per capita Transfers (-1)				0.340*** (0.029)
Observations	4,366	4,079	4,079	3,753
Municipalities	345	345	345	345
Prob > F		0.014	0.017	0.005
R-squared	0.003	0.463	0.366	0.470

All regressions except the first one include a fourth-degree polynomial time trend. Prob > F is the probability of the type-I error of the null that coefficients LOC and NAT are statistically equal. Errors are clustered at the municipality level in all estimations. *, **, ***, significance at the 10%, 5% and 1% levels, respectively.

Table 3: The iPBC and Political Alignment

	Log per capita Transfers				
	(1)	(2)	(3)	(4)	(5)
ALIGN	0.101*** (0.019)	0.100*** (0.019)	0.080*** (0.021)	0.085*** (0.021)	0.091*** (0.022)
LOC		0.032** (0.013)	(0.014) (0.019)		
NAT		-0.002 (0.017)	-0.003 (0.017)		
LOC x ALIGN			0.090*** (0.026)	0.086*** (0.026)	0.090*** (0.030)
Log Population	-0.825 (0.518)	-0.825 (0.519)	-0.824 (0.518)	-0.900* (0.518)	-0.616 (0.570)
Rural Dummy	-0.004 (0.003)	-0.004 (0.003)	-0.004 (0.003)	-0.004 (0.003)	-0.005 (0.004)
Population below 15 [%]	-3.175 (2.434)	-3.32 (2.445)	-3.288 (2.448)	-3.86 (2.485)	-3.853 (3.014)
Population above 65 [%]	-1.337 (3.153)	-1.41 (3.154)	-1.397 (3.149)	-1.739 (3.132)	-2.25 (3.918)
Disaster Dummy	0.053*** (0.014)	0.046*** (0.015)	0.043*** (0.015)	0.003 (0.022)	0.106 (0.086)
Poor [%]	0.120 (0.211)	0.125 (0.216)	0.124 (0.216)	0.257 (0.220)	-0.037 (0.272)
Time Trend	YES	YES	YES	NO	NO
Year FE	NO	NO	NO	YES	NO
Year-Province FE	NO	NO	NO	NO	YES
Observations	4,079	4,079	4,079	4,079	4,079
Municipalities	345	345	345	345	345
Prob > F		0.041	0.627		
R-squared	0.373	0.374	0.375	0.387	0.514

The first three columns include a fourth-degree polynomial time trend. Column (4) and (5) include a full set of year and year-province dummies, respectively. Prob > F is the probability of the type-I error of the null that coefficients LOC and NAT are statistically equal. Errors are clustered at the municipality level in all estimations. *, **, ***, significance at the 10%, 5% and 1% levels, respectively.

Table 4: The iPBC and Past Margin of Victory

	Log per capita Transfers						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Margin (-1)	0.144 (0.095)	0.166* (0.098)	0.183 (0.129)	0.097 (0.144)	0.135 (0.117)		
LOC		0.049** (0.021)	-0.036 (0.030)	0.128*** (0.031)	-0.024 (0.029)	-0.043 (0.050)	-0.009 (0.020)
LOC x Margin (-1)		-0.094 (0.090)	0.082 (0.127)	-0.261** (0.127)	0.058 (0.128)		
ALIGN					0.073** (0.031)	0.016 (0.050)	0.089*** (0.022)
ALIGN x Margin (-1)					0.028 (0.132)		
LOC x ALIGN					0.147*** (0.043)	0.161*** (0.068)	0.073*** (0.028)
LOC x ALIGN x Margin (-1)					-0.324* (0.179)		
Sample	ALL	ALL	ALIGN=0	ALIGN=1	ALL	M <5	M >5
Observations	4,074	4,074	2,033	2,041	4,074	800	3,279
Municipalities	345	345	342	333	345	193	341
R-squared	0.366	0.367	0.386	0.288	0.377	0.313	0.361

All regressions include a fourth-degree polynomial time trend and the controls reported in Table (2). In column (3) and (4), the sample consists of aligned and non-aligned municipalities, respectively. In column (6) and (7), $|M| < 5$ and $|M| > 5$ indicate that $\text{Margin}(-1)$ is lower and higher than 5%, respectively. Errors are clustered at the municipality level in all estimations. *, **, ***, significance at the 10%, 5% and 1% levels, respectively.

Table 5: Municipal Spending

	<i>Log per capita Spending</i>				
	(1)	(2)	(3)	(4)	(5)
Log pc Transfers				0.090*** (0.008)	0.090*** (0.008)
ALIGN		0.008** (0.004)	0.005 (0.004)	-0.001 (0.003)	-0.002 (0.003)
LOC	0.022*** (0.002)	0.022*** (0.002)	0.015*** (0.003)	0.019*** (0.002)	0.016*** (0.003)
NAT	0.008*** (0.003)	0.008*** (0.003)	0.008*** (0.003)	0.008*** (0.002)	0.008*** (0.002)
LOC x ALIGN			0.013*** (0.005)		0.005 (0.004)
Log Population	-0.859*** (0.115)	-0.865*** (0.115)	-0.865*** (0.115)	-0.791*** (0.128)	-0.791*** (0.128)
Rural Dummy	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
Population below 15 [%]	-0.495 (0.500)	-0.491 (0.504)	-0.487 (0.503)	-0.197 (0.497)	-0.196 (0.497)
Population above 65 [%]	-1.838*** (0.644)	-1.852*** (0.636)	-1.849*** (0.639)	-1.725** (0.667)	-1.724** (0.668)
Disaster Dummy	-0.003 (0.003)	-0.003 (0.003)	-0.004 (0.003)	-0.008*** (0.003)	-0.008*** (0.003)
Poor [%]	-0.012 (0.041)	-0.013 (0.041)	-0.013 (0.041)	-0.023 (0.035)	-0.023 (0.035)
Observations	4,074	4,074	4,074	4,074	4,074
Municipalities	345	345	345	345	345
R-squared	0.809	0.809	0.809	0.855	0.855

All estimations include a fourth-degree polynomial time trend. Errors are clustered at the municipality level in all estimations. *, **, ***, significance at the 10%, 5% and 1% levels, respectively.

Table 6: Correlation between Transfers and Local/National Outcomes

	Municipal: Prob. Winning			Presidential: Vote Share		
	(1)	(2)	(3)	(4)	(5)	(6)
Log pc Transfers	0.123** (0.060)	0.184** (0.087)	0.141*** (0.046)	1.940*** (0.875)	3.180*** (0.720)	2.716*** (0.704)
ALIGN		0.135 (0.085)	0.003 (0.041)			3.228*** (0.811)
Log Population		0.220* (0.129)	0.722 (0.860)		46.420** (23.370)	43.054* (22.518)
Rural Dummy		-0.002 (0.002)	0 (0.005)		-0.126 (0.147)	-0.135 (0.138)
Population below 15 [%]		-0.246** (0.110)	-0.103* (0.057)		-7.359 (4.620)	-7.2 (4.415)
Population above 65 [%]		-1.084** (0.515)	0.179 (0.442)		1.516 (7.477)	0.859 (7.192)
Disaster Dummy		-1.02 (1.701)	-4.056 (4.511)		-29.725 (133.988)	-34.359 (129.624)
Poor [%]		2.291 (2.003)	6.183 (5.272)		193.006 (176.483)	181.881 (168.121)
Observations	1,008	961	961	1,008	961	961
R-squared			0.040	0.630	0.634	0.647
Municipalities	345	344	344	345	344	344

Estimations (1), (2) and (3) include data on local election years (2004, 2008 and 2012). Estimations (4), (5) and (6) include data in presidential elections years (2005, 2009, 2013) for all variables, except Log pc Transfers which is from the previous year (local elections). Columns (1) and (2) use PROBIT estimation, and (3) corresponds to the estimation of a linear probability model with municipality fixed effects. Columns (4) to (6) use the fixed effects within estimator. All estimations include year fixed effects. Errors are clustered at the municipality level. *, **, ***, significance at the 10%, 5% and 1% levels, respectively.

Table A1: GMM Estimations

	Log per capita Transfers				
	(1)	(2)	(3)	(4)	(5)
Log per capita Transfers (-1)	0.731*** (0.090)	1.523*** (0.458)	0.702*** (0.088)	0.726*** (0.089)	0.716*** (0.090)
Log per capita Transfers (-2)		-0.292 (0.186)			
ALIGN			0.039* (0.021)	0.035* (0.021)	0.017 (0.022)
LOC	0.073*** (0.019)	0.100*** (0.025)		0.072*** (0.019)	0.039 (0.025)
NAT	-0.062** (0.025)	-0.090** (0.040)		-0.061** (0.025)	-0.061** (0.025)
ALIGN x LOC					0.065* (0.034)
Observations	3,366	3,054	3,366	3,366	3,366
Municipalities	345	344	345	345	345
Prob > F	0.337	0.657		0.326	0.895
Number of Instruments	15	16	14	16	17
Sargan Test	0.117	0.887	0.110	0.119	0.136
AR(2)	0.936	0.169	0.919	0.948	0.954

All regressions include a fourth-degree polynomial time trend and the set of controls reported in Table 2. GMM specifications include 2, 3 and 4 lags as instruments; all instruments are collapsed. Prob>F is the probability of the type-I error of the null of the mean reversion hypothesis. Errors are clustered at the municipality level in all estimations. *, **, ***, significance at the 10%, 5% and 1% levels, respectively.

Table A2: Time Trends and Clustering Robustness Checks

	<i>Dependent Variable is Log of per capita Transfers (N=4,019)</i>								
	TR=2 (1)	TR=3 (2)	TR=4 (3)	PROVC (4)	2DC (5)	EFE (6)	EYFE (7)	PROVTR (8)	REGTR (9)
LOC	-0.022 (0.014)	0.033** (0.013)	0.033** (0.013)	0.033** (0.016)	0.033 (0.028)	0.027** (0.013)	0.028* (0.016)	0.030** (0.014)	0.035** (0.014)
NAC	-0.070*** (0.014)	0.006 (0.015)	-0.006 (0.017)	-0.006 (0.017)	-0.006 (0.067)	-0.028 (0.020)	-0.027 (0.020)	-0.006 (0.017)	-0.002 (0.017)
AIC	3965.7	3805.0	3802.7						
Prob>F	0.001	0.073	0.017	0.053	0.521	0.005	0.006	0.033	0.033
R-squared	0.34	0.365	0.366	0.366	0.686	0.37	0.37	0.396	0.452

Estimation results reported in columns (1), (2) and (3) include a quadratic (TR=2), a third-degree (TR=3) and a fourth-degree (TR=4) polynomial time trend, respectively. All other regressions include a fourth-degree polynomial time trend. Errors are clustered at the municipality level in all estimations, except in column (4) and (5) in which errors are clustered at province (PROVC) and province-year (2DC) levels, respectively. Column (6) includes an electoral period dummy (EFE), while column (7) also includes a second year term dummy (EYFE). Time trends are province (PROVTR) and region-specific (REGTR) in columns (8) and (9), respectively. Prob > F is the probability of the type-I error of the null that coefficients LOC and NAT are statistically equal. All estimations include the set of controls reported in Table 2. *, **, ***, significance at the 10%, 5% and 1% levels, respectively.

Table A3: Placebo Exercise: Tax Revenues

	<i>Log per capita Tax Revenues</i>			
	(1)	(2)	(3)	(4)
ALIGN		-0.003 (0.005)	0.000 (0.005)	-0.001 (0.005)
LOC	-0.004 (0.003)	-0.004 (0.003)	0.001 (0.004)	
NAT	-0.003 (0.004)	-0.003 (0.003)	-0.003 (0.003)	
ALIGN x LOC			-0.010* (0.006)	-0.006 (0.006)
Time Trend	YES	YES	YES	NO
Year FE	NO	NO	NO	YES
Observations	4,140	4,140	4,140	4,140
R-squared	0.355	0.355	0.356	0.361
Municipalities	344	344	344	344

All regressions include the set of controls reported in Table 2. Columns (1), (2) and (3) include a fourth-degree polynomial time trend, while column (4) includes a full set of year dummies. Errors are clustered at the municipality level in all estimations. *, **, ***, significance at the 10%, 5% and 1% levels, respectively.