Politics, Spending and Local Economic Growth: Evidence from Mexico

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December, 2021

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Abstract

This paper studies the economic consequences of partisan favoritism when politicians can manipulate public spending. I use quasi-experimental variation from the expansion of intergovernmental transfers in Mexico and a regression discontinuity design to estimate the effects of political alignment between governors and mayors on public spending and private-sector jobs. I find that political alignment increases the growth rate of public spending by 12 percentage points and reduces the growth rate of private-sector jobs by 11.6 percentage points. The effect of political alignment on the private sector is stronger when production factors are in high demand, during local economic expansions, and in local economies with a relatively small supply of firms and workers that can provide goods and services to local governments. I find suggestive evidence that these results could be explained by an increase in rent-seeking in public contracts offered to the private sector. Politically aligned municipalities increase the rents of contracting with the public sector to funnel resources to improve the reelection probabilities for their party. The increase in rents reduces the actual production and, therefore, the demand for private labor and investment.

JEL Codes: D72, O17, O,18, R11

* I am indebted to Laurent Bouton for multiple discussions on this project. I am also grateful to Martin Ravallion, Garance Genicot and Toshihiko Mukoyama for detailed comments to this document. I also want to give special thanks for detailed comments to Carlos Rodriguez-Castellan, Jorge Pérez Pérez, Pedro Juarros, Mary Ann Bronson, Frank Vella, Maria Hernandez de Benito, Carolina Concha Arraigada, Ana Maria Mayda, Andrew Zeitlin, Rodimiro Rodrigo, Umberto Muratori, Juan Margitic, JJ Nadeo, Kevin Ankey, Minji Kim, Lina Yu, Deniz Sanin, Carlo Alcaraz, Lorenzo Aldeco, David Jaume, Maria José Orraca, Alejandrina Salcedo, Enrico Moretti, Dix Carneiro, Vincent Pons, Paul Novosad, Leopoldo Fergusson, Alberto Bisin.

1. Introduction

Public-sector spending can be an effective tool to boost economic growth in distressed economic areas or in places that suffer from temporary economic downturns (Chodorow-Reich, 2019). However, everyday public spending is not targeted to maximize the efficiency of fiscal policy. A large body of research on political economy documents that political factors drive the allocation of public spending (Golden and Min, 2013). Despite the ubiquitous role of political factors in defining the allocation of public-sector spending, there is limited evidence on how politically motivated spending affects local economic activity.

In this paper, I take advantage of the unique context of Mexico and focus on a salient political factor that distorts the allocation of public resources to ask: How does partisan favoritism affect local economic activity? Understanding the effects of public spending when it is channeled through partisan favoritism is essential to see if the problem of unequal allocation of public resources goes beyond equity concerns and creates its own distortions.

Two reasons may explain the scant evidence on the economic effects of politically motivated spending. First, the redistributive and countercyclical nature of fiscal policy makes it difficult to obtain causal estimates. The targeting of these policies implies that one would typically observe public spending directed to places already in a downward economic trend or affected by a temporary economic shock, which would bias any OLS estimate.[1] Second, politically induced spending is everywhere but hard to detect; most of the research that manages to identify the causal effect of political factors on public spending tends to do it over a fraction of public resources that are not sufficiently large to affect local economic aggregates.[2]

I circumvent these problems by taking advantage of the political economy behind the expansion of intergovernmental transfers in Mexico. In 1998, the government of Mexico created Ramo-33, a law that expanded intergovernmental transfers to local governments, leading to an unexpected and economically sizable increase in local public spending. Although Ramo-33 transfers were earmarked and designed to be allocated based on an objective formula, politics did have a sharp influence on where and when these transfers were

[1] An OLS estimate would be downward bias if the confounder is related to the deep economic factors behind the downward economic trend; also, it would be upward biased if a mean reversion component confounds the policy impact.

[2] Curto-Grau, Solé-Ollé and Sorribas-Navarro (2018) focus on regional capital transfers (8% of total spending); while Brollo and Nannicini (2012) focus on federal infrastructure transfers (15% of infrastructure investment). The effects found in both papers imply relatively low increases in total spending.

distributed. It has been documented that state-governors took advantage of their role in allocating the transfers to skew public resources towards municipalities with political traits that offer them high political returns.[3]

This evidence motivates the empirical strategy deployed in this paper. I study the effect of politically motivated spending by leveraging variation in political alignment between governor and mayor during the period of expansion of Ramo-33 transfers. To do so, I use a regression discontinuity design that takes advantage of plausibly exogenous variation in political alignment that occurs when elections are decided by a close margin.[4] My identification assumption is that municipalities where the governor's party candidate narrowly won are valid counterfactuals to municipalities where the candidate narrowly lost. The fact that political alignment is unpredictable among razor-close elections implies that aligned municipalities are not systematically different from unaligned municipalities in terms of unobservables (e.g. economic shocks or particular political traits) that explain the current economic activity, intergovernmental transfers, or public spending patterns.

As a first step toward understanding how political alignment affects local economic activity, I assemble a unique dataset that provides information at the municipality-year level on several measures of local economic activity and public finance. This level of disaggregation allows me to precisely measure the dynamics of the economy before and after a municipality becomes politically aligned. To construct my primary measure of economic activity, I use the employer-employee data to measure total employment and average wages for the universe of formal sector workers. I combine this with additional information on employment in the informal sector (household surveys and economic censuses), consumption (night lights and electricity consumption), public employment (social security records of public employees) corruption (audit reports from an anti-corruption agency), and homicides (vital statistics). I also use public finance data to observe how alignment increases public revenues and spending; the detailed nature of this data allows me to decompose revenues and spending into a wide set of subcategories.

[3] Several studies about Ramo-33 transfers have suggested that turnout, political competition and political alignment resulted in a higher allocation of Ramo-33 (Díaz Cayeros and Silva Castañeda, 2004; Langston, 2010; Trillo and Rabling, 2007). One of the goals of this paper is to obtain a causal estimate of the effect of political alignment on the allocation of Ramo 33 transfers.

[4] This research design is known as a close election regression discontinuity design and it has been widely used to uncover the effects of partisan favoritism on the allocation of public resources on Brazil (Brollo and Nannicini, 2012), Italy (Bracco et al., 2015), Spain (Curto-Grau, Solé-Ollé and Sorribas-Navarro, 2018), and U.S. (Albouy, 2013)

I study the economic consequences of politically motivated spending in three steps. In the first step, I provide causal evidence that politically aligned mayors receive higher intergovernmental transfers and increased total public spending than municipalities that are not politically aligned. The second step studies the effects of political alignment on private-sector employment and wages. I find that the growth rate of private-sector employment slows down in politically aligned municipalities. I rule out that this result is explained by workers moving from the formal to the informal sector, implying that total employment is lower in politically aligned municipalities. In the third step, I investigate the potential channels that may explain these results. I rule out that additional resources increased violence or corruption due to interest groups or politicians trying to capture the additional intergovernmental transfers. I find suggestive evidence that this increase in public spending leads to higher rents for public sector contractors—the reallocation of production factors to rent-seeking activities has negative externalities on total employment growth.

The first part of the paper focuses on the political economy behind the allocation of the earmarked transfers and its effects on total spending. I find that the three-year growth rate of intergovernmental transfers increased by 46 percentage points in municipalities where the mayor belongs to the governor's party compared to where the mayor belongs to the opposition's. I do not find that this increase in transfers crowds-out alternative revenue sources (other intergovernmental transfers, taxes, or debt), implying that total revenues should increase. In line with this logic, political alignment increases the three-year growth rate of public revenues/spending (local governments run a balanced budget) by 10 percentage points.

The second part of the paper explores how politically motivated spending affects private-sector economic activity. My primary measures of economic activity are employment and the average wages of the universe of formal sector workers. I find political alignment affects employment but not wages. In particular, the three-year employment growth rate is 12 percentage points lower in politically aligned municipalities. This negative coefficient is explained by a slowdown, rather than a reduction, of private-sector jobs in politically aligned municipalities. The point estimates are equivalent to a decline in the employment rate of 2.8 percentage points, assuming that neither public nor informal sector employment is affected by political alignment.[5]

[5]The formal employment ratio in our sample is about 28%, which suggests that the observed decline in formal employment is equivalent to a reduction.

Before exploring the mechanisms, I evaluate to what extent the reduction of formal employment is explained by a shift from the formal to the informal sector. To do so, I use two coarser data sources of local employment: household surveys and the quinquennial economic census. The results from the household surveys suggest that alignment reduces the probability of being employed by 3.4 percentage points. This decline in total employment seems to be driven by declines in the formal sector. In particular, the likelihood of being employed in the formal sector explains about two-thirds of this decline (2.1 percentage points). The results from the economic census, although not precisely estimated, confirm a negative effect of political alignment on total employment. Overall, I cannot rule out an increase in informality. Still, I can confidently say that shifts towards the informal sector cannot explain the bulk of my results.

I explore three potential mechanisms that may explain my results. First, I ask whether public spending crowded out private-sector jobs. This mechanism would suggest that the slowdown in private-employment results from production factors (labor and capital) being diverted towards goods and services provided to the public sector.[6] Second, I use homicide data to test whether the reduction in total employment is driven by an increase in violence that results from interest groups fighting to capture economic rents provided by the higher public spending. Third, I use data on audits of local governments to test whether the additional rents increased the probability of elected politicians engaging in corrupt behavior; undermining local economic growth.

I find pieces of evidence suggesting that the crowding-out effect is explained by the reallocation of production factors towards rent-seeking activities that deter aggregate employment. A crowding-out effect occurs only when the economy is at full capacity and in sectors that benefit less from increases in local demand due to higher public sector spending. Specifically, the effect of political alignment on employment is stronger on tight labor markets, the tradable sector, and economies with a low share of government-dependent sectors. I do not find evidence that the slowdown in job creation results from increases in violence[7] or corruption.[8]

[6] This crowding-out effect could lead to lower aggregate employment when the activities demanded by the government are less labor-intensive and have lower employment multipliers than the activities from which the resources are drawn. Despite being less productive, they may provide sufficient rents to attract entrepreneurs and capital investment (Torvik, 2002).

[7] This result is consistent with the fact that the period studied, 1998-2006, had historically low levels of homicides, and it was before the well-documented increase in violence that took place after 2006.

[8] I use data on anti-corruption audits performed by an autonomous watchdog agency (Auditoria Superior de

Moreover, consistent with a crowding-out effect, I do not observe a decline in citizens' welfare. I use three indirect measures of welfare all of which suggest that citizens are better off despite the decline in private-sector jobs. In particular, I find that the incumbent party is 40% (13 percentage points) more likely to be re-elected in the subsequent election when it is politically aligned. Also, I find that two-thirds of the decrease in the formal employment rate (2.8 out of 3.4 percentage points) is explained by decreased labor force participation rather than an increase in unemployment. This implies that people are not losing jobs as would be the case if politically aligned municipalities were experiencing heightened violence or corruption shocks. Third, I do not find statistically significant evidence that neither the growth rate of night lights nor electricity consumption is lower in politically aligned municipalities. If anything, the point estimates suggest positive and economically significant effects.

A unique feature of this finding is that I can rule out the traditional ways in which theory argues that crowding out occurs, namely through higher taxes or interest rates. The disproportional amount of transfers is nationally funded, and interest rates are only affected at the national level. I explore three mechanisms by which crowding-out can happen around this context: public sector enlargement, economic disruption caused by infrastructure investment, and increase in rent-seeking contracts which I measure as spending that is not backed up by proportional increases in employment.

I rule out that the crowding is caused by a disproportionate increase in public sector employment[9] or by construction projects disrupting economic activity.[10] I suggest that the rise in rent-seeking activities could explain the findings. In particular, I find that politically aligned municipalities experience a disproportionate increase in the growth rate of infrastructure investment (40 percentage points) and general service contracts (25 percentage points). When looking at the growth rate of private-sector jobs in construction or government-dependent industries, I fail to find statistically or economically significant changes. This implies that additional contracts do not generate jobs and are more likely to provide rents to citizens. This result is consistent with people leaving the labor force and

la Federación) to test whether the fiscal windfalls spur higher levels of corruption. I do not observe that aligned municipalities are more likely to be accused of malfeasance or corruption. On the contrary, conditional on being audited, politically aligned municipalities are less likely to be accused of malfeasance.

[9]This is consistent with evidence that the size of the public sector is relatively stable in Mexico and difficult to be affected by local politicians.

[10]Infrastructure spending could decrease total employment through the negative spillovers of building infrastructure projects Ramey (2020). I rule out this mechanism because I fail to find statistically or economically significant increases on either construction jobs or public capital stock. Therefore, I cannot conclude that more construction projects are taking place in politically aligned municipalities.

citizens being more willing to re-elect the politically aligned candidate.

RELATED LITERATURE.— This paper contributes to several strands of the literature. First, it contributes to the literature that asks about the local employment effects of infrastructure spending. Most of the studies that focus on the short-run find that employment dips negative during the first few years after the infrastructure spending took place (Garin, 2019; Leduc and Wilson, 2013; Dupor, 2017; Buchheim and Watzinger, 2017), but increases in the long run as higher stock of public capital can boost labor productivity (Kline and Moretti, 2014; Yaffe, 2020; Leduc and Wilson, 2013; Allen and Arkolakis, 2019). This literature suggests that the construction of infrastructure projects disrupts economic activity and delays private investment. Although I find negative effects on employment after an exogenous increase in infrastructure spending, I fail to find concrete evidence that construction is increasing, which suggests that other mechanisms could be at play. In particular, infrastructure spending may allow politicians to divert resources to unproductive activities, which may deter private investment.

Second, it contributes to the literature of distributive politics that focuses on how partisan favoritism affects the allocation of public resources. The bulk of the literature has found that central politicians skew resources to politically aligned municipalities (Brollo and Nannicini, 2012; Curto-Grau, Solé-Ollé and Sorribas-Navarro, 2018; Bracco et al., 2015; Fiva and Halse, 2016; Albouy, 2013). While this literature focuses on discretionary transfers, I provide suggestive evidence that political alignment can substantially distort the allocation of resources even in circumstances where transfers are meant to be allocated with a predetermined allocation formula. Also, different from this literature, I focus on the economic consequences of political alignment.

Third, I contribute to the literature that studies the economic effects of political favoritism. This literature agrees that ethnic and regional favoritism generally leads to higher economic growth (Hodler and Raschky, 2014; Alesina, Michalopoulos and Papaioannou, 2016), but has conflicting findings regarding the effects of partisan favoritism. On one hand, Cohen, Coval and Malloy (2011) find that political alignment with the chair of any congressional committee *decreases* employment in private-sector firms in the U.S. The authors argue that this effect is explained by larger public spending crowding out private-sector economic activity. On the other hand, Asher and Novosad (2017), using data from India, finds that employment *increases* more in districts that are politically aligned with the state ruling party

compared to unaligned districts. They argue that the main mechanism is the discretionary power that politicians have over-regulation. My findings are similar to Cohen, Coval and Malloy (2011) because I also focus on the same policy lever, namely, public spending. In this sense, my results show that the effect of political alignment on economic growth is sensitive to the policy lever that the politicians manipulate, implying that the context on which one decides to focus defines the answer one gets.

This paper also revisits the literature on the resource curse. There is an established consensus that fiscal windfalls can negatively impact political institutions and increase conflict. Independent of their origin, higher fiscal resources tend to soar corruption and deteriorate the quality of political candidates (Brollo et al., 2013; Asher and Novosad, 2020; Chen and Kung, 2016; Vogel, 2021). This paper proposes a different channel by which fiscal windfalls may negatively affect local economies. In a similar fashion that the *Dutch disease* reallocates labor towards resource-extractive industries, the politically motivated fiscal windfalls reallocate labor towards the non-tradable sector, which tend to be less productive and suffer from higher informality rates. The most impressive decline in the formal sector has two principal negative consequences; it mechanically reduces taxes and the capacity of the workforce to contribute to the health and pension system.

Finally, I contribute to the literature on distributive politics in Mexico. The literature that focuses on intergovernmental transfers has documented strong correlations between the allocation of transfers and several political variables like political competition, partisan alignment, and voter turnout (Díaz Cayeros and Silva Castañeda, 2004; Langston, 2010; Trillo and Rabling, 2007). My contribution to this extensive literature is to quantify the *causal* effect of political alignment on the allocation of Ramo 33 transfers. Another strand of literature uses the same research design employed in this paper to study the consequences of political alignment on access to loans and the implementation of crackdowns (de la Garza and Lopez-Videla, 2020; Dell, 2015). These studies focus on the effects of political alignment with the president. My paper centers on the role of state governors in the discretionary allocation of intergovernmental transfers which, according to my results, has a large impact on economic outcomes.

2. Institutional Context

This section describes the functioning of public finances in Mexico, focusing on the creation of new intergovernmental transfers in 1998. This expansion of transfers, known as Ramo-33, led to an unexpected and sizable increase in local public spending. Also, I explain how the institutional design of the newly enacted transfers gave state governors disproportional power and, therefore, left room for political favoritism.

A. The Expansion of Earmarked Transfers

Mexico has a revenue-sharing system in which the federal government collects most of the taxes and later redistributes them across states and municipalities. Thus, the fiscal capacity of sub-national governments is limited severely by the number of intergovernmental transfers they receive. Therefore, any policy that affects intergovernmental transfers to a great extent can affect the total of local public spending substantially.

This paper takes advantage of the expansion of intergovernmental transfer that followed the creation of Ramo-33 in 1998. It focuses on the two subcomponents of Ramo-33 that allocated funds to municipalities: FORTAMUN-DF and FISM. I do not include the rest of the subcomponents of Ramo-33 because those transfers are managed by state governments; therefore, I cannot observe the municipalities where they are allocated. From now on, I refer to these two subcomponents as Ramo-33.[11] These two funds are desirable for identification purposes for two reasons: First, they provide unexpected increases in local public spending. As Figure 1 shows, there is a weak correlation between the growth rate of Ramo-33 municipal transfers and the growth rate of public spending before 1998. Second, these transfers are economically significant. Figure 2 shows that, between 1998 and 2006, local spending as a share of municipal GDP increased by 20 percentage points. These relatively large magnitudes resulted from the redistributive nature of Ramo-33. One of the core objectives of the transfers was to allocate disproportional public resources to less developed municipalities, which explains the significant increases for the average municipality.

Additionally, it is crucial to highlight that a substantial fraction of Ramo-33 is earmarked

[11] Both sub-components use explicit allocation formulas: FORTAMUN-DF is allocated based on population, and FISM is distributed according to a formula that uses a multidimensional deprivation index that considers access to electricity, poverty, education, and sewerage among others. The formulas are updated every time new information from the most recent population census is released. See guidelines of these sub-components here https://www.coneval.org.mx/Informes/Evaluacion/Estrategicas/Ramo_33_PDF_02032011

to infrastructure projects. As a result, it can be observed in Figure 3 that the expansion of funds also led to an increase in public investment. Specifically, FISM is earmarked for a broad set of infrastructure projects: from social infrastructure (e.g., health and school facilities) to core economic infrastructure (e.g., electrification, construction of dams, sewerage, and municipal roads). In comparison, FORTAMUN-DF is earmarked for either infrastructure (e.g., maintenance of urban infrastructure) or non-infrastructure projects (public security, debt payments, and acquisition of goods to strengthen the productivity of public workers). Overall, more than two-thirds of the transfers are earmarked to infrastructure projects.

To summarize, Ramo-33 implied an unexpected and economically substantial increase in local public spending. This spending shock is explained by a sizable growth of intergovernmental transfers to local economies and by a shift in the allocation of resources towards less developed areas, where the economic size of this spending increase is expected to be larger.

B. The Political Economy of Earmarked Transfers

One of the main motivations behind the creation of Ramo-33 was to protect the intergovernmental transfers from the political discretion of the Federal government. For that purpose, these intergovernmental transfers were designed as entitlement programs, meaning: the transfers were to be received by municipalities every year according to a fixed formula define by predetermined municipal characteristics. Also, the law that enacted these transfers assigned state governments the role of allocating these transfers at municipal level. This restriction aimed to prevent municipalities, with low bargaining power regarding the federal's government, from receiving systematically fewer transfers.

However, these safeguards created numerous related distortions by transferring disproportional power from the federal to the state government. As a result, anecdotal evidence suggests that state governments did not strictly follow the guidelines defined by the allocation formula when distributing the resources of Ramo-33. Some governors publicly stated that they should be authorized to skip the allocation defined by law and replace it with theirs.[12]

Figure 4 shows two pieces of evidence to substantiate the claim of manipulation of intergovernmental transfers. First, it shows that a large proportion of municipalities reported not receiving intergovernmental transfers between 1998 and 2002. This result is odd since

[12] See Díaz Cayeros and Silva Castañeda (2004), Trillo and Rabling (2007), (Langston, 2010).

the statutory allocation mandated a positive amount of transfers for all municipalities. Second, it shows the dispersion in the growth rate of intergovernmental transfers. Given that these transfers worked as entitlement programs that allocated resources across municipalities based on a fixed formula, one should expect a coefficient of variation close to zero.[13] However, Figure 4 shows not only a positive coefficient of variation but that it increases after the enactment of Ramo-33 until 2005 as well. I find the substantial decline after 2005 is correlated with a relatively large increase in fines imposed by the ASF (Autonomous watchdog agency) to municipal and state governments for malfeasance and waste of public resources.

To summarize, the evidence points towards significant discretion in the allocation of intergovernmental transfers across municipalities. Since state governors were in charge of distributing these funds, distinct political factors they care about, such as political competition, turnout, and partisan favoritism, could explain their allocation.

3. Identification

This section explains how my identification strategy allows me to estimate the effect of political alignment between municipality-mayors and state-governors on a wide variety of economic and public finance outcomes.

Political alignment is not random, as an electoral outcome is a direct result of the aggregation of voters' decisions when casting their ballots. Since these decisions are influenced by a myriad of political and economic factors that also affect the outcomes of interest, I should expect strong biases from any naive OLS estimate.[14] To address this identification problem, I exploit variation from razor-close elections, a research design known as close election regression discontinuity design (RDD).

[13] Several reasons can explain a positive dispersion on yearly growth rates, namely: measurement error, political business cycle, or a change in formula's inputs, which takes place every time a new population census is released (1992, 2002, 2012).

[14] For example, state governors can invest in winning specific municipal elections; therefore, the effect of political alignment on economic growth could be confounded by the independent effect of past governor's efforts on economic growth. Also, if voters who live in municipalities that are in a downward economic trend tend to elect politically aligned candidates, the effects of alignment on economic growth could be downward biased.

A. Identification Assumption

This identification strategy takes advantage of the variation provided by close elections to obtain causal estimates of political alignment. To implement the estimator, I compute the vote margin for every municipal election indexed by municipality m and election cycle e. The vote margin is defined as the difference in votes between the candidate of the governor's party—$v^g_{m,e}$, and the main opposition's party— $v^o_{m,e}$, normalized by the total number of votes—$v_{m,e}$.[15]

$$VM_{m,e} = \frac{v^g_{m,e} - v^o_{m,e}}{v_{m,e}} \tag{1}$$

Consequently, a vote margin above (below) zero corresponds to municipalities where the elected candidate does (does not) belong to the state governor's party. A candidate's political affiliation is measured before the election takes place, which rules out any concern regarding unobserved characteristics affecting the running variable.

As explained by Hahn, Todd and Van der Klaauw (2001) and De la Cuesta and Imai (2016) the identification assumption is continuity of the potential outcomes at the cut-off.[16] The main intuition of the *continuity* assumption is that municipalities where the politically aligned candidate narrowly lost are valid counterfactuals for municipalities where the politically aligned candidate narrowly won.

This identification assumption has three critical implications for validating, interpreting, and computing the parameter of interest. First, any confounder that systematically correlates with alignment should vary smoothly around the cut-off. Second, in the presence of heterogeneous treatment effects, the estimate obtained should be interpreted as a local average treatment effect (the effect of alignment at the cut-off). Third, the sample analog estimator can be computed as the difference between the sample mean of aligned and misaligned municipalities at the cut-off. Therefore, the precision of the estimates will increase

[15] I consider both individual candidates and coalitions to define political alignment. When a political coalition forms the governor's party, I consider any mayoral candidate that belongs to any of the parties that are part of the political coalition to be politically aligned. Moreover, the main opposition's candidate is the candidate/coalition that does not belong to the governor's party/coalition and has the highest number of votes. This implies that the vote margin is not necessarily computed as the difference between the winner and the runner-up.

[16] This assumption simply states that $\mathrm{E}[y(1)_{m,e} \mid VM_{m,e}]$ and $\mathrm{E}[y(0)_{m,e} \mid VM_{m,e}]$ are continuous at the cut-off (i.e. $VM_{m,e} = 0$) where $y_{m,e}(1)$ is the value of y when the candidate elected is politically aligned with the central government, and $y_{m,e}(0)$ when is not politically aligned. See De la Cuesta and Imai (2016) for a clear explanation of why the *continuity* assumption is weaker than the usually claimed *local unconfoundedness* within a bandwidth

with the number of observations at the cut-off.

B. Difference in Discontinuities Specification

I estimate the parameter of interest using local linear regression (Gelman and Imbens, 2019) with triangular kernel weights (Calonico, Cattaneo and Farrell, 2020) over the sub-sample of close elections (i.e. $VM_{m,e} \in (-h, h)$), defined as those with a vote margin whose absolute value is less than or equal to five percentage points.[17] The regression pools the observations of the post-electoral years that correspond to the mayor's ruling span during our period of study (1998-2006). In particular, I estimate the following equation:

$$
\begin{aligned}
\Delta^3 y_{m,e,k} = \alpha + \beta\ aligned_{m,e} \\
+ \theta VM_{m,e} \times aligned_{m,e} + \gamma VM_{m,e} \times (1 - aligned_{m,e}) \\
+ \delta_{s(m)} + \xi_{e,k} + \psi X_{m,e-1} + \epsilon_{m,e,k} \quad \forall\ VM_{m,e} \in (-h, h)
\end{aligned} \tag{2}
$$

where $\Delta^3 y_{m,e,k}$ is a three-year log points difference of the outcome y measured k years after the latest election indexed by municipality–m and year e; the three-year difference corresponds to the three-year mayoral.[18] $aligned_{m,e}$ is an indicator variable that identifies whether the current mayor belongs to the governor's party and does not vary across k. The specification also includes a linear function of the running variable, estimated separately on either side of the cut-off. Finally, I control for state fixed effects ($\delta_{s(m)}$) and the interaction of election and election-time ($\xi_{e,k}$) fixed effects. Also, I control for the vector $X_{m,e-1}$, which contains a set of political characteristics from the previous election cycle: political alignment, political party, and the governor's vote margin. I cluster standard errors at the municipality level to allow for correlation over time within a municipality, the level at which treatment occurs. (Abadie et al., 2017; Bertrand, Duflo and Mullainathan, 2004).

In this specification, β measures the effect of political alignment across all k post-election years and is identified from variation in political alignment across municipalities that had a

[17] The preference for an ad-hoc bandwidth (h=5) lies in the fact that any data-driven bandwidth (Calonico, Cattaneo and Farrell, 2020) would lead to compositional problems when comparing results across different outcomes or subsamples. In the presentation of my results, I also show estimates with an 11 percentage points bandwidth defined by Calonico, Cattaneo and Farrell (2020). The Appendix shows that results are qualitatively similar when using alternative bandwidth sizes and kernel weights.

[18] Most of the mayoral elections have a three-year term limit with few exceptions (2 percent of the elections have a 4 years term limit). When municipalities have larger term limits, I normalize the difference to be a three-year growth rate equivalent. Also, results are qualitatively similar when either I re-weight our estimates by the inverse of the term limit or when I focus only on the sub-sample of municipalities with a three-year term limit.

close election within the same state and during the same election cycle.[19]

C. Dynamic Difference in Discontinuities

To observe the dynamics of the effect of political alignment and support the identification assumption, I follow Cellini, Ferreira and Rothstein (2010) and frame the close election RDD as an event study. This specification allows me to disentangle the contemporary from the lagged effects of alignment and indirectly test the presence of the parallel trends assumption.

This specification recast the dataset such that the unit of observation is a close election, defined by a municipality-electoral cycle pair for which the election's vote margin satisfies a specific threshold. For each observation, I proceed to track the evolution of the outcomes of interest three years before and four years after the year of the election.[20] With this new dataset I estimate the following equation:

$$
\begin{aligned}
y_{m,e,k} = \alpha &+ \sum_{j=-3, j\neq -1}^{3} \beta^j \; 1(k=j) \times aligned_{m,e} \\
&+ \sum_{j=-3, j\neq -1}^{3} 1(k=j)\Big[\theta^j VM_{m,e} \times aligned_{m,e} + \gamma^j \; VM_{m,e} \times (1 - aligned_{m,e})\Big] \\
&+ \delta_{m,e} + \psi_{e,k} + \epsilon_{m,e,k} \quad \forall \;\; VM_{m,e} \in (-h, h)
\end{aligned} \tag{3}
$$

where $y_{m,e,k}$ is the inverse hyperbolic sine transformation of outcome y measure at k years from the election cycle e. Notice that this specification includes years before and after the election took place, while in equation (2) I focus only on growth rates in the post-election period. Correspondingly, $\delta_{m,e}$ are municipality-election cycle fixed effects, while $\psi_{k,e}$ controls non-linearly for macroeconomic shocks and the trend of each election cycle e. Similar to the main specification, I estimate equation (3) using a local linear regression of the running variable with triangular kernel weights and a five percentage point bandwidth. Standard

[19] The standard approach in the empirical literature is to exploit the richness of the cross-sectional variation to properly estimate the effect of political alignment at the cut-off; some examples: Brollo and Nannicini (2012); Curto-Grau, Solé-Ollé and Sorribas-Navarro (2018); Asher and Novosad (2017). Including municipality fixed effects while holding the bandwidth would limit inference to municipalities with more than one close election, changing political status (aligned, not-aligned) over time, and small sample biases emerge. The dynamic specification that I propose next includes municipality-election fixed effects after recasting the data to provide the variance needed.

[20] More formally, if the same municipality has two close elections, I duplicate all its observations and follows the outcomes of interest over a specific window. This implies each copy (municipality-electoral cycle) as a separate unit that gets treated only once.

errors are clustered at the municipality level to account for the serial correlation of political alignment over time (Bertrand, Duflo and Mullainathan, 2004).

Notice that the identifying variation comes from variation in trends among the subset of close elections that took place within a specific election cycle (i.e. cohort). This ensures that my estimates do not suffer from the standard critiques that apply to two-way fixed effects models. This estimator can also be seen as an analog of the identification strategy proposed by (Cengiz et al., 2019) that circumvents problems related to event studies with staggered adoption, which is relevant in this case because the timing of elections is staggered across states.

The identification assumption of this specification is that conditional on the election cycle, the trends among aligned and non-aligned municipalities are continuous at the alignment threshold.[21] Although one cannot test this assumption directly, it is possible to perform indirect tests to support their plausibility. Section 5 presents the results of such examinations.

4. Data and sample

The objective is to estimate the economic effects of political alignment when politicians can distort the allocation of intergovernmental transfers. To do so, I assemble a municipality-year dataset that combines data on local public finance, electoral results, employment, and wages (public and private) from social security records. I also use alternative datasets to complement the main analysis, namely: labor force surveys, economic censuses, remote sensing data, and federal audits to local governments. This section describes each source of information and the sample used to obtain the main estimates.

A. Data and Measurement

PUBLIC FINANCE.— Municipalities produce yearly balance sheets classifying both revenues and spending across different subcategories. The Mexican National Institute of Statistics and Geography (INEGI) collects this data. The revenues side provides information about distinct categories, from intergovernmental transfers to local property taxes and fees for services provided by local governments. On average, 82% of local revenues come from

[21] Notice that this is a weaker identification assumption than the standard parallel trends assumption, which states that aligned and misaligned municipalities would have the same trends in the absence of the treatment. This setting allows these trends to be systematically different as long as they are continuous at the cut-off.

intergovernmental transfers. In comparison, about 10% come from taxes (mostly property taxes) and other public services provided by local governments. The revenue data allows me to distinguish which sources of revenue increase due to political alignment. Also, I can observe different components of spending data. The two most important in terms of their average share in total spending are the wage bill (40%) and the public investment (30%).

ELECTIONS.— Electoral data comes from Centro de Investigación para el Desarrollo (CIDAC) and the state electoral authorities. The election data provides information on the number of votes for each party or coalition for the universe of municipal, state, and presidential elections.[22] It is important to note that the party affiliation recorded in the electoral data is defined months before the election. Therefore, the measure of alignment is not affected by politicians deciding their political party affiliation after knowing the electoral results. The latter alleviates any concern related to the manipulation of alignment status.

MAIN EMPLOYMENT AND WAGE DATA.— To measure employment at the municipal-year level, I combine administrative records from the Mexican Institute of Social Security (IMSS) and the Institute of Social Security of Public Workers (ISSTE). Since both data sources correspond to social security records, they capture the universe of formal employees and employers. IMSS collects data on formal private-sector workers/employers and the ISSTE on formal public sector employees. Formal workers, in this context, are all workers who contribute to the social security system and, therefore, receive health insurance and pension contributions.

In addition to employment counts, the IMSS data allows for the measurement of aggregate wage bills and, therefore, average wages. Also, it provides this data by sector, firm size, gender, and age groups. The data from ISSTE does not report wages; I circumvent this by using the total wage bill of local governments from the public finance data.

ALTERNATIVE EMPLOYMENT DATA.— The main drawback of the data from IMSS and ISSTE is that they remain mute about what is happening to the informal sector and, therefore, to total employment. I use two alternative sources of information to infer the effects of alignment on aggregate (formal and informal) employment and, to some extent, on the informal sector. The first source of information is the Mexican Economic Census collected every five years by INEGI. It provides detailed municipal-level information for the universe of non-

[22] I manually collect the elections not provided by CIDAC by requesting the data from the electoral institutions of each state.

agricultural establishments, both formal and informal. I use this dataset to measure employment and wage bill growth between the 1998 and 2008 rounds of the economic census. I follow Asher and Novosad (2017) by assigning the result of the earliest election that took place between the two rounds of the economic census to each intercensal growth rate. The main drawback of the economic census is that it does not allow us to measure the change in employment precisely before and after every election, which leads estimates to be biased towards zero.

The second source of information corresponds to the labor force surveys collected by INEGI. In particular, I use the National Urban Employment Survey (ENEU), which is available at a quarterly level for 1998-2004. This survey is representative of about 48 metropolitan areas and collects a wide variety of socio-demographic and labor market information for both formal and informal workers. I use this data source to complement the analysis of the administrative records, mainly regarding the effects of political alignment on informality and labor force participation.

The household surveys are not the preferred dataset for two main reasons. The first reason is that ENEU has little overlap with the primary sample used in the estimates. Specifically, it only provides 40% of the municipality-year observations used in the main estimates. When I limit the sample to municipalities that appear before and after a close election, this percentage declines to 20%. The second reason is that household surveys are not representative at the municipality level; this implies a large within municipality variation that would limit the ability to detect small effects.[23]

ECONOMIC ACTIVITY.— To measure aggregate economic activity I use night lights luminosity and electricity consumption. Both are scaled by population, measured in log points, and available at the municipality level. The data on night lights comes from the National Oceanic and Atmospheric Administration (NOAA). Night lights data provides a luminosity measure for every square kilometer of Mexican territory on a scale from 0 to 63. The fact that this information is censored from above may limit the power to find statistically significant effects in cities with several pixels censored at 63. To circumvent this problem, I measure total municipal luminosity growth considering only those pixels that were below 63 by 2003. This is equivalent to assuming that the censored pixels' growth rate is equivalent

[23] I compute the correlation between the growth rate of formal employment captured by IMSS and the ENEU between 1998-2003, uncovering that at the state level, the correlation is 0.57, while at the municipal level, the correlation is 0.2; Bosch and Campos-Vazquez (2014) find similar results.

to neighboring non-censored pixels.

The second source of data is the aggregate consumption of electricity, which comes from the ministry of energy and regulation. This information corresponds to the total energy consumed by both establishments and households. It can be interpreted as a local measure of economic activity that can increase either because residents work more or because they increase their consumption from higher government transfers.

OTHER DATASETS.— I also use other datasets to explore some potential mechanisms and perform balance tests over the data. The sources include: i) The rollout of the number of beneficiaries from Seguro Popular, ii) Monthly payments and beneficiaries from PROGRESA, iii) Official reports from audits of local governments performed by an autonomous watchdog agency (Auditoría Superior de la Federación), and iv) Annual homicides data from INEGI.

B. Sample

The final dataset provides variation at the municipal-year level for the sample of elections between 1998 and 2003. Since mayors have three-year term limits and the outcomes are measured as a three-year growth rate, this implies that I follow the dynamics of employment and public finance outcomes for the period of 1996-2006. The key advantages of this study period are: First, it allows me to estimate the effects of political alignment during the expansion and weak oversight of intergovernmental transfers. Second, it ended one year before the sudden and steep increase in violence experienced by Mexico after 2006, which some have argued was the result of political alignment with the president's party.

I exclude municipalities from the state of Oaxaca because most do not choose their mayor through elections; rather, they use a traditional governance structure, which makes it infeasible to construct the running variable.[24] Also, I limit the observations to those municipalities for which there is information available on formal private-sector employment for the study period 1998-2006. The final sample considers 1097 out of 2446 municipalities, which employ 99% of all formal employees in Mexico and host 80% of the Mexican population. This filter implies that the estimates are representative of mid-sized and large municipalities making

[24] Traditional governance structures in Oaxaca are valid after a constitutional reform that took place in 1995. This governance structure is known as *"Usos y costumbres"*. The municipalities organized by this type of "polity" are ruled by assemblies rather than mayors. The election of these assemblies is informal and local leadership can be arranged by rotating appointments without elections taking place.

the results of this study relevant from a macroeconomic point of view.[25]

5. Validity of the Research Design

This section evaluates the internal validity of the identification strategy, which hinges on the fact that any other variables that affect my outcomes of interest change smoothly along the threshold. I perform two indirect tests for that purpose. I evaluate whether there are discontinuities along the cut-off on either the density of the running variable or baseline characteristics. I also provide a raw look at the spatial clustering of the data.

A. Manipulation Test

The fact that political alignment brings benefits to local officials is a sufficient reason to think that local governments may self-select into political alignment with the governor. I test for this by evaluating whether the density of the vote margin changes abruptly at the cut-off. Figure 6 shows the histogram of the vote margin for the elections in which the governor's party compete. Overall, I do not find evidence of municipalities sorting on either side of the alignment threshold. This strong symmetry in the result of close elections is also apparent in the raw data. Particularly, I find that 467 out of 1867 elections were decided by a margin of less than five percentage points and, among those, 241 were won by the opposition and 226 by the governor's party. [26]

I implement the McCrary (2008) test to formally evaluate if there are discontinuities in the vote margin at the threshold. Specifically, I estimate the density function of the vote margin separately on each side of the cut-off and test if the two expected values of the density function at the cut-off are statistically different from zero. The results, presented in Figure 7, precisely suggest that there is no discontinuity of the density function around the cut-off. The p-value of the McCrary-test is 0.7, which indicates that neither the governor's party nor the opposition systematically wins close elections.[27]

[25] IMSS data records employment for 1,850 out of 2446 municipalities in Mexico. The rest of the municipalities either have none or few formal employees (e.g., less than 10) and therefore that IMSS group them into a larger neighboring municipality.

[26] This symmetry remains when I look at narrower bandwidths: I observe 282 (189) municipalities that were decided by a margin of less than three (one) percentage points, and among those, 149 (100) were won by the opposition and 133 (89) by the governor's party.

[27] Calonico, Cattaneo and Farrell (2020) and Bugni and Canay (2021) have proposed variations of the McCrary (2008) test. I obtain the same conclusion with any of these results (available upon request).

B. Discontinuity of Baseline Characteristics

Another indirect test to the identification assumption is to evaluate whether baseline characteristics jump discontinuously at the alignment threshold. A discontinuity on baseline characteristics would suggest that municipalities where the politically aligned candidate narrowly won are systematically different from municipalities where the politically aligned candidate narrowly lost. To perform this test, I estimate the causal effect of political alignment using a variant of equation (2). The main difference is that now the baseline characteristics (measured in levels) are the main outcomes of interest, implying that they are excluded from the set of controls. Since I use the sample of close elections that took place during the study period (1998-2003),[28] and the main outcomes are measured before 1998, I should not find discontinuities of these outcomes at the cut-off.

Figure 8 shows the results of this continuity test on several economic, socio-demographic, geographic, and political characteristics measured several years before every election took place.[29] I standardize all non-binary variables and present estimates in terms of standard deviation units to facilitate comparison across variables. The figure reports the point estimates and 95% confidence intervals of each regression. It is reassuring to observe that there is no evidence of any discontinuous jump in the baseline characteristics. The confidence interval for each variable crosses zero, except for the share of workers in manufacturing. The p-value of the joint hypothesis test that all baseline characteristics are statistically equal to zero is 0.8.

C. Spatial Concentration of Close Elections

Another concern is that the close elections subsample is geographically clustered. This would bias the estimates in the presence of either spatial spillover effects or heterogeneous effects by state characteristics. Figure 9 shows the map of close elections during the study period using a bandwidth of 5 percentage points. If a municipality has more than one close election, I map the result of the first election. As it can be observed, close elections are spread throughout the country. All 32 states had at least one close election won by either the governor's party or the main opposition.

[28] The data set to perform this test is at the close election level, therefore municipalities with more than one close election show up more than once. I account for this by clustering standard errors at the municipality level; results are robust to only including the first close election for each municipality.

[29] All socioeconomic characteristics are measured from the 1990s Population and 1989 Economic Censuses. The political characteristics are measured from the previous electoral period.

6. Main Results

This section presents our main estimates. First, it explores the extent to which political alignment affects the allocation of intergovernmental transfers and total public spending. Second, it shows the effects of political alignment on employment and wages for the universe of formal private-sector workers. Third, it uses coarser measures of total employment (both formal and informal) to evaluate whether the results on formal employment affect total employment or are explained by shifts between the formal and the informal sector. Unless otherwise indicated, I use equation (2) and equation (3) to obtain all results presented in this section.

A. Public Revenues and Spending

TRANSFERS.— The first-order question is whether being politically aligned during the period of expansion of earmarked transfers benefits municipalities. I use two measures to answer this question. The first measure is the probability of receiving transfers (extensive margin), and the second is the three-year growth rate of transfers per capita (intensive margin). The first measure aims to identify if state governments punish non-aligned municipalities by holding up intergovernmental transfers. The second measure tests whether there is a difference in the total amount of transfers received.

Table 1 shows that politically aligned municipalities receive a higher amount of transfer, but alignment does not significantly increase the probability of receiving transfers. In particular, columns 1 through 4 show that political alignment increased the growth rate of earmarked transfers between 29 and 65 percentage points depending on the specification. Column 2 is my preferred specification since it uses the narrower bandwidth and includes the set of controls. This column indicates that the growth rate of earmarked transfers was 42 percentage points higher in aligned municipalities compared to their non-aligned counterparts. To put this into context, the average growth rate of earmarked transfers, for the non-aligned municipalities is 138 percent, which implies that political alignment increased the growth rate of intergovernmental transfers by one-third (= 42/138). These relatively large growth rates in the control groups are explained by the context of the study period, during which earmarked transfers expanded from near zero (as a fraction of local revenue) to accounting for almost 30% of total revenues. Columns 5 to 8 of Table 1 show that political alignment does not consistently increase the likelihood of receiving earmarked transfers

during the years of the mayor's term. This null result could be explained by the fact that 96 percent of municipalities in the control group (non-aligned municipalities) report receiving transfers leaving little room for discretion between aligned and misaligned municipalities.

Figure 10 plots the growth rate of earmarked transfers around the alignment threshold for the post-election and pre-election periods. The discontinuity for the post-election period is evident and implies that aligned municipalities have higher growth rates than their non-aligned counterparts (Panel A). Moreover, the fact that there are no discontinuities in the observed pre-election growth rates at the alignment threshold validates the identification assumption. This test is analogous to what is referred to in the literature of difference and difference as the parallel trends test.

OTHER TRANSFERS, TAXES, AND DEBT.— Other revenue sources may offset the effect of earmarked transfers on total public revenues. Governors may compensate non-aligned municipalities with another type of public resources, leading to a misleading conclusion when studying the net effect of political alignment Kramon and Posner (2013). Another mechanism that could offset the effect of earmarked transfer on total spending is the response of taxes and debt. Local governments may change their optimal decisions regarding taxes and debt as a result of higher intergovernmental transfers. For example, a well-established theoretical result suggests that governments should reduce their taxes after a fiscal windfall.[30] The impact of fiscal windfalls on debt is less clear. On the one hand, higher transfers imply higher collateral for local governments; on the other hand, the fiscal windfalls imply lower borrowing needs.[31]

Table 2 uses the detailed categories of revenues collected in the public finance data to test if any of the mechanisms mentioned above amplify or offset the effects of political alignment on local public resources.

For ease of comparison, column 1 again presents the effect of political alignment on the growth rate of total earmarked transfers. Column 2 shows the impact on revenue sharing transfers, which are the main revenue source (representing 53% of total revenues) and are also administrated by state governors. I do not find robust evidence of revenue transfers

[30] This prediction has motivated extensive empirical literature around the flypaper effect, which has found mixed results on this prediction. See Inman (2008) for a review

[31] Also, political alignment could directly affect debt if central politicians can influence the access to credit. de la Garza and Lopez-Videla (2020) shows that in Mexico, political alignment with the president between 2009 and 2013 explains higher access to debt. It is important to validate is his findings also apply to our context, which not only focuses on a different period but a different measure of political alignment.

changing at the alignment threshold; results are statistically significant with an 11 percentage point bandwidth but disappear when using a 5 percentage point bandwidth. Although this coefficient estimate is non-statistically significant, it is economically significant, and I cannot rule out that it plays a role in increasing overall spending.

Column 3 of Table 2 reports the effect of alignment on taxes, which represent about 5.9 to 6.2 percent of total revenues. I find that the growth rate of taxes for aligned municipalities is higher but not in a statistically significant manner. Moreover, the point estimates in both Panel A and B suggest that the effect of alignment on taxes would be positive. Column 4 of Table 2 shows the result for debt. During the study period, debt constitutes a relatively small fraction of local revenues; in our sample, it represents at most 3.3 percent of total revenues. The point estimates for the effect of political alignment on debts are positive but neither statistically significant nor consistent across different bandwidths. To summarize, alignment-induced transfers do not seem to significantly affect local governments' decisions regarding taxes and debt. This evidence is in line with the idea of a flypaper effect; grants increase total spending, which has been validated by Bracco et al. (2015) using data from Italy and a similar research design as this paper.

This implies that, if anything, political alignment brings more rather than fewer resources to local economies. Yet, I have not entirely ruled out that other public resources that I can not observe with the public finance data are not responding to political alignment. I will go back to this in the robustness section, where I explore the effect of political alignment on the allocation of other nationwide programs unrelated to intergovernmental transfers.

PUBLIC SPENDING.— The second main result is presented in the last column of Table 2. Total public spending increase in politically aligned municipalities. This is consistent with the fact that I did not find crowding-out effects from other sources of revenue (transfers, taxes, or debt). Both Panel A and B illustrate a consistent story; political alignment increases the total public spending growth rate by 10 to 12 percentage points. Since the growth rate of spending for the control was about 56 percent, this suggests that the effect of alignment on total resources is about 7 percent (1.07=164/156). This estimate can be interpreted as a net effect of partisan alignment after any compensation and behavioral effects induced by the increase in intergovernmental transfers have been netted out. Panel A of Figure 11 confirms the discontinuity, while Panel B reassures our identification assumption.

B. Employment and Wages

This subsection explores whether employment and wages evolve differently in politically aligned municipalities, which, as it was explained above, receive a disproportional amount of governmental resources. To do so, I use data on total jobs and the aggregate wage bill for the universe of formal sector jobs recorded by the Mexican Institute of Social Security. [32] I compute two outcome measures with this data that can be observed at the municipal-year level and disaggregated by sector and firm size. The first is the absolute number of formal jobs, and the second is a measure of wages that I compute as a total wage bill divided by the number of jobs.

Columns 1 to 4 of Table 3 show the impact of political alignment on the growth rate of private employment. Overall, the results show that aligned municipalities have a slower growth rate than their non-aligned counterparts. The growth rate of formal employment is between 9.5 to 12.1 percentage points lower in politically aligned municipalities. This effect is robust to the choice of bandwidth and is not sensitive to the inclusion of different controls.

To provide an interpretation of the coefficient, I look at the sample mean for non-aligned municipalities presented in the table and the plots of the outcome variation against the running variable. Since the mean growth rate of private formal employment in non-aligned municipalities is between 7 and 9.1 percent, the coefficient estimate suggests that the negative coefficient should be interpreted more as a slowdown in job creation in aligned places compared to non-aligned municipalities. Both Panel A and B of Figure 13 provide the same interpretation. The intersection of each slope with the alignment threshold, when the vote margin is equal to zero, could be interpreted as the conditional growth rates at the threshold. Panel A indicates that employment grew by more than 10 percent in non-aligned municipalities while slightly above zero for aligned municipalities. Panel B shows that before the election, both aligned and non-aligned municipalities were experiencing private employment growth at the threshold.

Columns 5 to 8 of Table 3 show that political alignment did not affect average wages. The point estimates are not statistically significant and relatively small (less than 0.1 percent) compared with the average wage growth of non-aligned municipalities (between 7 to

[32] Formal workers represent 40% of the jobs and account for about 70% of the output. I interpret the result here as relevant for the formal sector and do not extrapolate its conclusion to the informal sector. In the next subsection, I study the effects on aggregate employment using coarser sources of information like household surveys or economic censuses

8 percent). For the sake of completeness, Figure 15 plots the growth rate of wages for both the post and pre-election periods, and as expected, I do not observe discontinuities in wage growth either before or after alignment takes place.

C. *Informality*

The previous analysis relies on social security records. Therefore, it remains mute about the impact of political alignment on employment in the informal sector and, consequently, on the total number of jobs.

A potential alternative explanation to the results is that public spending increases demand in sectors that disproportionally hire informal workers, like construction and services. In that case, the reduction of formal jobs is explained by a shift of workers from the formal to the informal sector rather than a slowdown in total employment. This explanation would be consistent with the evidence that the bulk of workers in the informal sector are informal by choice and not by a lack of opportunities for being employed in the formal sector (Alcaraz, Chiquiar and Salcedo, 2015; Maloney, 1999).

By definition, informal jobs are illegal; therefore, administrative records that would allow me to measure informal or total employment (informal and formal) at the municipal-year level do not exist. To circumvent this measurement problem, I use two alternative sources of data: household force surveys and economic censuses.

The household survey data collects comprehensive individual-level information for a cross-section of individuals on a quarterly basis. This data allows me to measure the conditional probability of being employed and decompose it into the formal and informal sectors. [33] In particular, I use this rich individual-level data to estimate the effect of political alignment on the probability of belonging to one of the following mutually exclusive labor statuses: formal worker, informal worker, unemployed, and out of the labor force. To do so, I estimate separate regression for each labor market status on an augmented version of equation (2) that includes a rich set of individual-level controls: age, education, gender, and household size. The data from the economic census reports the total number of jobs, among other establishment-level characteristics.[34] I use the same specification of equation

[33] I cannot compute municipal level aggregates with the household surveys because the data collects information for a non-representative subsample of households living in each municipality. The survey design makes the data representative at the metropolitan area level, which is a larger geographical unit than the municipality level

[34] I only observe municipal-sector level aggregates of this establishment-level information.

(2) where the outcome variable is the five-year change in total employment. This equation is estimated for all the close elections that took place in between any close election year.

Panel A of Table 4 reports the results for total employment. Columns 1 and 2 present the effect of political alignment using the household surveys. They show that political alignment reduces the probability of being employed by 3.3 to 3.4 percentage points. This result suggests that the effects observed using only the formal sector have consequences on total employment. Columns 3 and 4 present the estimated effects of alignment on the change in total employment between 1998 and 2008. Although the estimates are not statistically significant, they are economically meaningful, suggesting that political alignment reduces the total employment growth rate by 5-7 percentage points.[35]

Table 5 decomposes the result from household surveys into two exclusive components, each computed as a probability among the population between 15 and 65 years of age. Columns 1 and 2 show the effects of political alignment on the likelihood of working in the formal sector; in particular, Panel A shows that political alignment reduces the formal employment rate by 2.1-2.3 percentage points. In contrast, columns 3 and 4 show that the impact of political alignment on the informal employment rate is also negative, although imprecisely estimated. The sum of these two effects corresponds to the effect on the total employment, presented again for illustrative purposes in columns 5 and 6. Overall, the results suggest that alignment reduces the size of both the formal and the informal sector. Therefore, I can rule out that my main results are explained by shifts of workers from the formal to the informal sector.

7. Mechanisms

This section investigates the potential mechanisms that explain why politically aligned municipalities show a slower increase in job creation despite receiving a disproportional increase in public revenues. I argue that public spending crowded out private-sector jobs by reallocating production factors (labor and capital) towards rent-seeking activities. The

[35]The lower precision from the estimates that use data from the economic census may be explained by the fact that the outcome measures cannot match to measure employment before and after each mayor's term. In particular, two different measurement problems may lead a downward bias in the estimates: first, the outcome of interest is measure as a ten-year growth rate, while the treatment's period (mayor's term) is defined as a three-year window. Second, the difference between treatment year (election year) and the baseline year (1998 round of the economic census) varies across municipalities.

basic argument is that the additional revenues received by aligned municipalities are spent on contracts to the private sector that increase profits rather than the quantity of goods or services provided. The increase of profits is consistent with aligned politicians funneling private transfers to voters as strategy to increase their parties' re-election probabilities. This increase in rents attracts entrepreneurs and capital from non-rent seeking industries. This reallocation has an opportunity cost to the aggregate economy if non-rent seeking industries have a positive externality on the aggregate growth rate of total employment. The higher profits from reallocating jobs to the formal sector increase the likelihood that municipalities attract high productivity entrepreneurs who would otherwise have been employed in different industries. In turn, this increases investment and employment in the local economy.

This channel has two implications: First, the effect should be weaker in economies where production factors did not reallocate because of the increase in the public sector demand. Second, the increase in spending on contracts to the private sector does not materialize into a proportional increase in production of goods or services to the public sector as part of the increased spending is diverted towards higher rents.

A. Reallocation and Rent seeking

REALLOCATION EFFECTS.— A standard Keynesian macroeconomic framework predicts that increases in public spending lead to higher private employment when production factors are underutilized. In this case, the increase in public sector demand would not reallocate production factors from non-rent seeking industries, but it would put production factors into work that would otherwise not be utilized in the economy. On the contrary, if production factors are scarce, one would expect a higher opportunity cost from the increase in public sector demand that results from the reallocation of economic activity towards rent seeking activities.

I perform several tests to argue that reallocation is one of the main reasons behind the slowdown in the private-sector economic growth. The first test evaluates whether the impact of politically motivated spending is different in expansion, when production factors are scarce, than in recessions, when production factors are idle. To measure expansions and recessions at the municipal level, I use the growth rate of formal employment during the three years before an election takes place. I define expansions (recessions) as those municipalities where the pre-electoral growth rate was above (below) the median pre-election employment growth rate. Panel A of Table 6 shows the estimates over the expansions subsample, while

Panel B shows the estimates over the recessions subsample. On one hand, the results of columns 1 and 2 of both panel A and B suggest that the amount of politically motivated spending is relatively similar in both subsamples. On the other hand, I find a substantial difference in the impact of alignment on employment depending on the economic cycle. In particular, columns 3 and 4 show that alignment reduces the employment growth rate by 13 percentage points during economic expansions, while the point estimate is halved, 6.7 percentage points, during non-expansionary periods.

A second test is to split the results between the tradeable and non-tradeable sectors. Non-tradeable industries rely more on local demand. Therefore, it is expected that they benefit from increases in local public spending. This demand effect may partially offset the crowding-out effect that results from the reallocation of production factors towards rent seeking activities. On the other hand, tradeable industries, which rely less on local demand and depend on international markets, are expected to be fully affected by the crowding-out effects.

Table 7 shows the effect of political alignment for tradeable and non-tradable industries. consistent with a competition for production factors mechanism, the tradable sector is the one that experiences the bulk of the negative effect of political alignment. The results suggest that the effect of alignment on employment is about six times larger in the tradeable sector. Column 2 shows that political alignment reduces the employment growth rate by 14.6 percentage points in the tradeable sector but only 2.3 percentage points in the non-tradeable sector.

The third test of this hypothesis is to evaluate whether the pre-election size of the industries that supply the public sector is important in explaining my results. I compute the pre-election share of private-sector jobs that work in industries that disproportionally supply the public sector. I use the input-output matrix of Mexico to separate the sectors between those that dis-proportionally supply the government and those with relatively low dependence on government demand. Based on this sector categorization, I split the municipalities into two groups, those with a high and low pre-election share of industries that supply the public sector. The idea is that the larger the share of government-dependent industries in an economy, the better the capacity of the local economy to accommodate to the increase in public spending without demanding a severe reallocation of workers from other highly productive industries.

Panel A and B of Table 8 show the effect of alignment on public spending and employment growth on local economies with a relatively high and low share of government-dependent (GD) sectors. The results of the effect of political alignment on spending are similar across both sets of municipalities, which indicates that central politicians do not consider the size of the GD sectors when allocating transfers. Specifically, I find that political alignment increases the spending growth rate by 9.9 percentage points in municipalities with a high share of GD jobs (Column 2 of Panel A) and 11.4 percentage points in municipalities with a relatively low share of GD jobs (Column 4 of Panel B). Although both types of municipalities receive a similar spending shock, I find that the impact of alignment on the employment growth rate tends to be stronger (point estimate is 57% stronger) in municipalities with a relatively low share of GD jobs. In particular, alignment reduces the employment growth rate by 11.8 (7.5) percentage points in municipalities with a low (high) share of GD sectors. This provides suggestive evidence that the impact of alignment on employment is related to the relative size of the GD sector in a municipality; a relatively large GD sector may imply less competition for production factors with other productive sectors in the economy.

RENT SEEKING.— The main argument behind this mechanism is that the reallocation of production factors lead to rent seeking. By definition, the idea of rent seeking is that certain production factors obtain higher rents without providing equivalent value added in the goods and/or services that they produce. Providing direct evidence on rent seeking would require contract level data on the goods and services produced by public sector contractors, which is not available in Mexico for the study period.

However, I perform three indirect test that suggest that the contracts were directed to provide higher private rents that could increase political returns rather than using the spending to increase employment in sectors that provide higher externalities for the whole economy. Three facts confirm this story. First, I do not observe a reduction in measures of total consumption. Table 9 reports the effect of political alignment on two different measures that capture total (public and private) consumption at a local level: night lights luminosity and electricity consumption. It is reassuring to see similar point estimates in both measures. Both points towards positive but non-statistically significant changes in aggregate consumption. This is consistent with the hypothesis that the disproportional resources from higher intergovernmental transfers are being allocated to households through means other than formal employment.

A second argument suggesting that voters are better off is presented in Table 10. It estimates the effect of alignment on the probability of being employed, unemployed, and part of the labor force. Each measure is computed using the population between 15 and 65 years of age. The decline in employment discussed in section 5 is decomposed into two types of transitions: from employment to out of the labor force (columns 5 and 6), and from employment to unemployment (columns 3 and 4). Panel A suggest that political alignment reduced labor force participation by 2.8-2.9 percentage points, but had no significant effect on unemployment. This implies that the observed negative effects of political alignment on total employment correspond to an increase in the labor force participation rather than an increase in the probability of being unemployed. This takeaway is consistent with interpreting the negative effect of alignment on employment as a slowdown in the net job creation rather than a destruction of jobs, as one would expect the destruction of jobs to generate a larger increase in the probability of being unemployed.

Table 11 presents a third piece of evidence suggesting that money from intragovernmental transfers may have been used to generate private rents with political purposes. Specifically, this table displays the estimates of the effect of political alignment on the probability of winning the subsequent elections. Table 11 shows the probability of winning the next election (column 1 and 2) and two subsequent elections in a row (column 3 and 4). Although the estimates in panel A are not statistically significant, the consistency between the estimates is remarkable. In the case of panel B, I find a strong incumbency advantage for mayors who were initially politically aligned. Looking at panel B, I find that the probability of winning the next election is 40 percent (=13/31) higher for politically aligned municipalities. Also this municipalities duplicate the probability of winning two elections in a row (13/11).

B. Ruling Out Public Employment and Infrastructure Construction

PUBLIC EMPLOYMENT.— One potential crowding-out mechanism is that higher public spending leads to an increase in public sector employment, which may deter private sector employment growth. To test this hypothesis, I construct a measure of total jobs and an aggregate wage bill for the public sector.

To measure public sector jobs at the municipality-year level, I combine administrative data from the institute of social security institute for state employees (ISSTE), and the institute

of social security for private employees (IMSS).[36] It is important to clarify that this employment measure does not capture contractors hired by the public sector, as these individuals are counted as private-sector workers. I also use the public finance data to measure total spending on salaries and work benefits for public employees, which I define as the total wage bill of public sector employees, which represents about 30% of total public spending.

Table 12 suggests that political alignment is not systematically associated with higher public employment. In particular, Columns 1 and 3 show positive and mildly statistically significant effects of alignment on public-sector employment and wage bills. However, these effects are unstable and change abruptly in columns 2 and 4, with the inclusion of the controls defined in equation (2). The lack of coefficient stability between the estimates with and without controls is not a characteristic of our main results (Section 6). This implies that the variation between public employment and alignment is not strongly correlated with the increase in the public sector. Another aspect to highlight from the table is that the R-squared of the regression on public employment with controls is relatively high (0.9). This fact is consistent with the low turnover of the public sector in Mexico and may be why I do not find changes in public sector employment because of political alignment.

INFRASTRUCTURE SPENDING.— An increase of infrastructure projects may be a potential mechanism behind the observed slowdown on local employment. A recent literature review by Ramey (2020) concludes that the short-term effects of infrastructure spending on employment are either negative or zero. Two suggestive explanations for this negative effect are a *disruption effect* and a *delay effect*. The disruption effect refers to the fact that, during the construction phase, infrastructure projects can increase traffic or even reduce sales in specific areas like retail or tourism. The delay effect suggests that agents may decide to delay any private investment until after an infrastructure project is built. They find it optimal to delay investment because the returns to private capital will be higher in the future once the stock of public capital increases. I test this hypothesis from several angles in table 13,15, and 16.

Table 13 confirms that the disproportional resources received by aligned municipalities led to an increase in public investment. In particular, column 2 of panel A suggests that political alignment increases the growth rate of infrastructure spending by 40 percentage points. This effect is relatively large compared to the growth rate of non-aligned munic-

[36] About 92% of public workers are affiliated to ISSTE. I identify public workers affiliated with IMSS by taking advantage that IMSS data allows me to observe employment at the sector level. I classify a worker in IMSS as a public worker when it works in sector 94 or 99 defined by the 2-digit NAIC code.

ipalities (120 percent) and the point estimate of political alignment on total spending (12 percentage points).[37] This result could be interpreted in two ways; first, a mechanical effect of higher compliance with the earmarks' spending rules, which assigns spending to infrastructure projects.[38] Second, it has been argued by Robinson and Torvik (2005) that politicians may prefer to build infrastructure, even white elephants, to signal their power and obtain higher electoral returns.

However, the increase in infrastructure spending is insufficient to argue that the rise in infrastructure projects explains the slowdown in private-sector jobs. As is stated by Garin (2019) and Ramey (2020), one should observe either higher inputs used by infrastructure projects, i.e., construction jobs, or higher outputs that result from infrastructure projects, i.e., increases in the stock of public capital.

Table 15 evaluates whether politically aligned municipalities experienced an increase in construction jobs. Columns 1 and 2 explore the effects of political alignment on the number of construction jobs using the social security records, while Columns 3 and 4 explore the results on wages. The results in Panel A do not show a consistent result. The more conservative interpretation is that alignment does not have a statistically significant effect on jobs or wages in the construction sector. However, when looking at the results in Panel B, the point estimates for employment turn to be negative and statistically significant. The higher spending on infrastructure with missing construction jobs may be suggestive evidence that corruption is taking place. I will test this interpretation with data on corruption in the next subsection. However, the fact that construction jobs are not positive suggests that neither the disruption nor the delay effect explains my results.[39]

Finally, Table 16 examines the effect of alignment on long-term (1995-2010) changes in distinct public infrastructure measures, the proportion of households with access to electricity, water, and sewerage. Looking at the long term allows me to rule out that I cannot see results because of the expected delays of construction projects. The results suggest relatively small and not statistically significant infrastructure improvements in politically aligned places, which is in line with no higher construction jobs taking place.

[37] See results in Table 2.

[38] Ramo-33 has two components FISM and FORTAMUN, the former was earmark to infrastructure projects, See section 2 for more details.

[39] Since about 85% of the workers on infrastructure are informal, I complement this analysis using the subsample of municipalities present in the household surveys. Results suggest that political alignment increases the probability of working in the construction sector as informal workers. However, the estimates are not statistically significant (results available upon request).

C. Ruling Out Violence and Corruption

VIOLENCE.— Resource abundance is associated with increased conflict between population subgroups trying to capture rents from the fiscal windfalls. An increase in violence could undermine the positive effects of higher spending and explain why I observe a slowdown in total employment. Table 17 estimate the effect of political alignment on homicides. Columns 1 and 2 measures the effect of alignment on the probability of a homicide during each year of the mayor's term; while columns 3 and 4 study the effects on the growth of the homicide rate. In summary, I do not find consistent and statistically significant evidence that alignment increases homicides. However, the standard errors are relatively large, so I cannot reject that political alignment increases the homicide rate.

To conclude about the effect of alignment on violence, I rely on the institutional context of Mexico. During my study period, 1998-2006, homicides were on a declining trend. Therefore, the abrupt changes that I observe in economic activity from increases in political alignment are not likely to be explained by historically low levels of homicides. This test also rules out that political alignment increases violence because it facilitates the implementation of anti-drug crackdowns.[40]

CORRUPTION.— Another channel through which higher spending could create lower economic growth is corruption. Brollo et al. (2013) shows that fiscal windfalls spur corruption, which can reduce local economic growth Colonnelli and Prem (2020). The basic argument is that fiscal windfalls increase corruption either because the incumbent politicians tend to be more corrupt (moral hazard effect), or because past fiscal windfalls attract low-quality politicians in the subsequent elections (selection effect).

The effect of alignment on employment takes place immediately after the increase in transfers. Therefore, changes in the quality of candidates of subsequent elections cannot explain my results.[41] Still, the results could be explained by a moral hazard effect; that is, it could be that the elected mayor changes his or her behavior in response to the excess of resources and becomes more corrupt. This increase in corruption may be sufficiently large to undermine

[40] (Dell, 2015) finds that political alignment facilitated the implementation of anti-drug crackdown policies, which ended up increasing violence. I do not find this effect because I focus on a different period (2007-2009) and under a different definition of political alignment, i.e. she focused on the alignment between mayors and the president's party.

[41] The research design assumes no systematic difference between aligned and misaligned municipalities in baseline characteristics, among those the pool of candidates before alignment is decided. Although I do not have information on the valence of candidates to test this assumption, I use a wide set of baseline characteristics in Figure 8.

the potentially positive effect obtained through additional resources.

I use data on audits to local governments to test if corruption is a mediation mechanism that explains the negative impact of political alignment on total employment. The audits are performed by an autonomous watchdog agency (Auditoria Superior de la Federación) that is part of the federal government and out of reach of the state governors.

The audits are conducted on a subsample of municipalities and report continuous measures of corruption and malfeasance for each municipality audited.[42]

Table 18 shows the results of political alignment on the probability of being audited (columns 1 and 2); the likelihood that more than 10% of the audited spending is not documented, i.e., being found guilty of corruption (columns 3 and 4); and the probability that more than 10% of audited spending does not correspond with the purpose of the earmarks, i.e., malfeasance (columns 5 and 6). Three conclusions emerge from this table: First, it is reassuring that political alignment does not affect the probability of being audited (columns 1 and 2). The point estimates are positive (1.9 percentage points in Panel A column 2) but not statistically significant. This is in line with the fact that the watchdog agency that performs the audits is autonomous and not influenced by state governors. Second, I find that alignment does not increase the probability of being accused of corruption (columns 3 and 4), but it does reduce the likelihood of being accused of malfeasance (columns 5 and 6). The latter effect suggests that the likelihood of being accused of malfeasance falls by 42 percentage points at the cut-off, which is particularly strong especially considering that the control's mean is 22 percentage points.

The surprising result that political alignment reduces malfeasance could be interpreted in two opposing ways. One interpretation is that governors exert more control and provide more guidelines to aligned mayors, which improves their management practices and the results of the audits. A second interpretation is that state governors influence the audits' results, asking the watchdog agency to be more lenient with politically aligned mayors.

Since distinguishing between these two hypotheses is not possible unless other data is available,[44] I argue that audit reports are not politically manipulated for three reasons. First,

[42]The selection of municipalities to be audited is not random, it obeys to population and total municipal budget.[43] They report the percentage of audited spending that is not supported by receipts, flagged as corruption. Also, the municipal audits focus on evaluating the malfeasance of Ramo-33 transfers; in particular, they focus on the FISM transfers. Their measure of malfeasance is defined as the percentage of spending that is not spent on the goods or services that are in line with the purpose of the earmarked transfers.

[44]For example, Chu et al. (2020) obtains a similar result. They find that auditors reduce the proportion of questionable spending reports when evaluating their hometowns. The authors collect firm-level data of state-

the constitution gives economic and political independence to the watchdog agency. By being a centralized institution, it is also more difficult for governors to exert control over the audit's results. Second, several studies consider this data a valid corruption measure (Chong et al., 2015; Arias et al., 2018; Ajzenman, 2021). Third, suppose aligned municipalities are actually less likely to be accused of malfeasance. In that case, I should observe more spending on the infrastructure projects (the primary purpose of the earmarks), which is confirmed by the data.

8. Conclusions

This paper studies the impact of politically induced public spending on private-sector economic activity. To do so, I causally estimate the effect of political alignment on private employment in a context in which governors are able to disproportionally allocate an economically significant amount of public spending to municipalities that elect a mayor from the governor's own political party.

I find that political alignment with state governors increases public spending by about 10 percentage points due to larger intergovernmental transfers received by aligned municipalities. Municipalities that experience the disproportional increase in spending suffer from a slowdown in private sector job creation. The results imply that the growth rate of private sector jobs in non-aligned municipalities is 10 percentage point lower in politically aligned municipalities.

I do not find evidence that higher corruption, a larger public sector enlargement, or an increase in the construction of infrastructure projects explain the observed results. The lack of a similar negative effect on our proxy of total economic activity, measured by nighttime light and electricity consumption, suggests that the results can be interpret as a crowding out effect, where there is not effects on output but some substitution between public sector spending and private economic activity.

The existing literature that studies the impact of political alignment on economic welfare has found positive effects for economies where politicians' policy levers are related to regulation rather than higher spending (Asher and Novosad, 2017). In our context, the main

owned enterprises and compute real activity manipulation measures to disentangle between a discipline or a manipulation effect. Their evidence supports a manipulation effect. This data is not available for the case of Mexico. Still, it is definitively an area of future research.

policy lever is spending which, in line with the findings of Cohen, Coval and Malloy (2011), seems to crowd out private sector economic activity.

These findings imply that politically induced public spending can have unintended consequences that may negatively affect welfare. Although the result does not imply lower welfare in the short term (at least when measure trough nigh lights and electricity consumption), it may affect welfare in the long term trough three channels. First, lower formal employment implies lower tax collection, which leads to a higher national deficit in the long run. Second, less workers in the formal sector implies lower pension savings. Third, higher informality can reduce the future incentives of firms to expand in order to remain informal.

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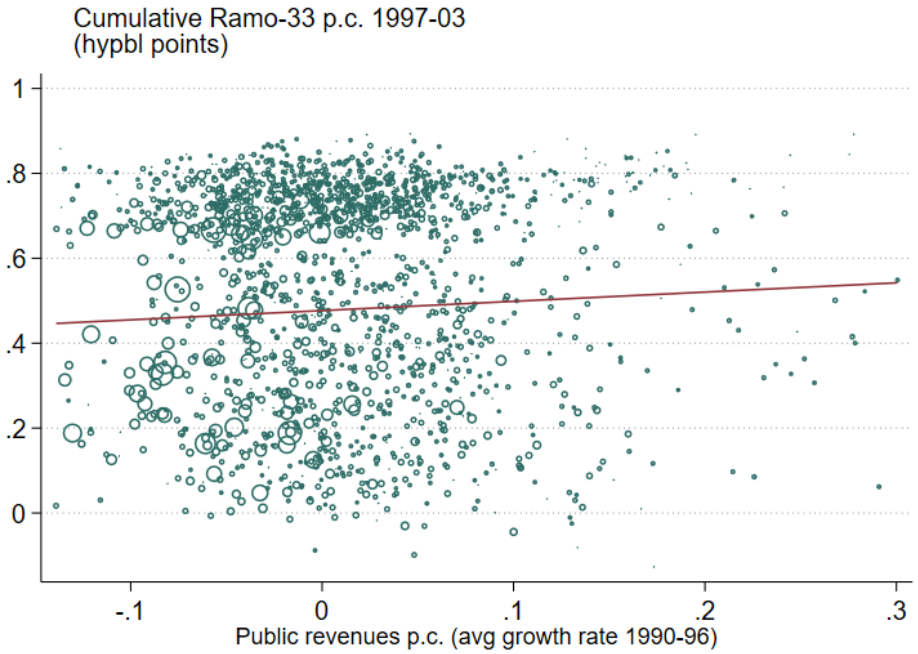
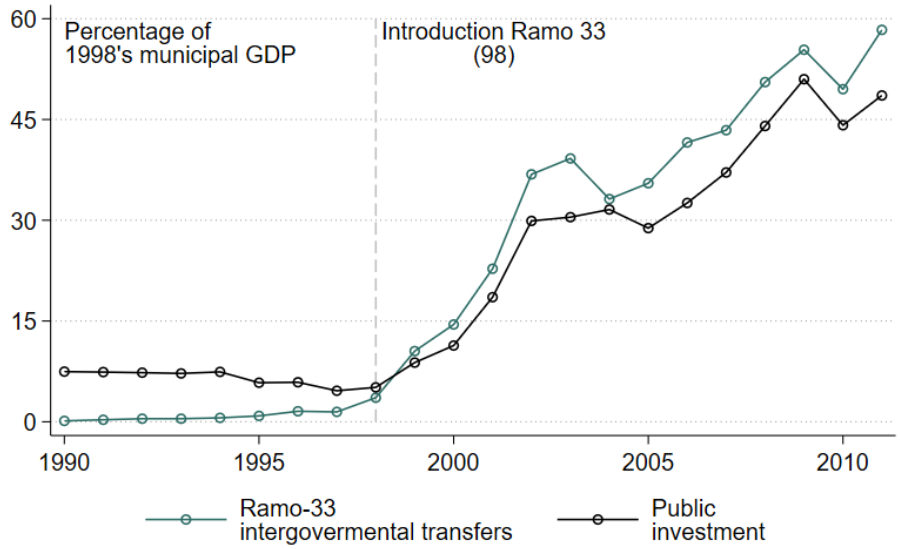
9. Figures

Figure 1: The economic size of Ramo-33



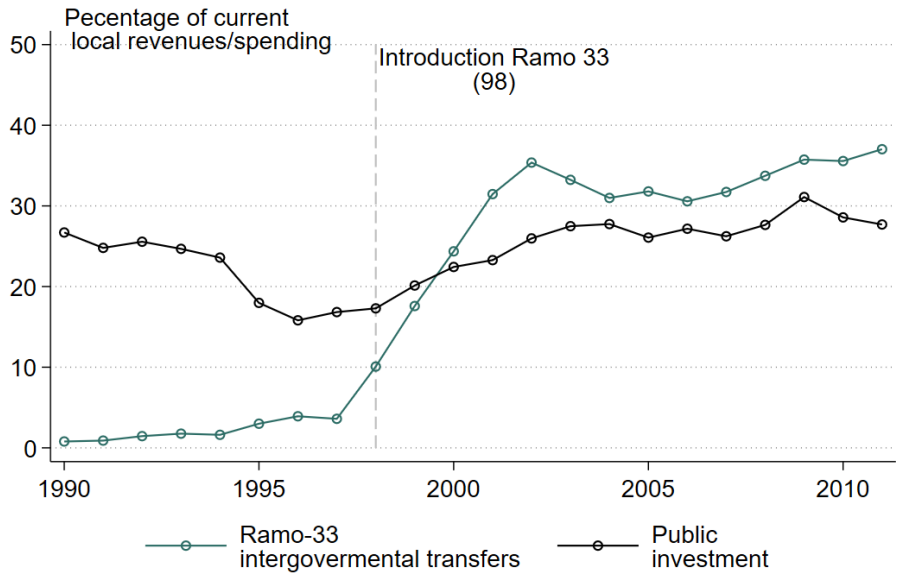
NOTE— The figure show the population weighted average of revenues as a share of local GDP in 1998. Ramo-33 for the year after 1998 and PRONASOL for years before 1998.

Figure 2: The economic size of Ramo-33



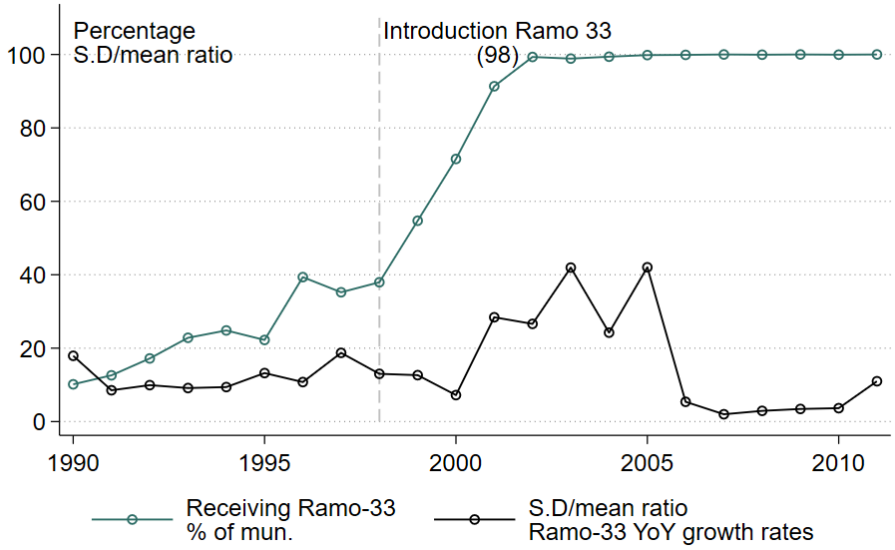
NOTE— The figure show the population weighted average of revenues as a share of local GDP in 1998. Ramo-33 for the year after 1998 and PRONASOL for years before 1998.

Figure 3: The size of Ramo-33 in the local public finances



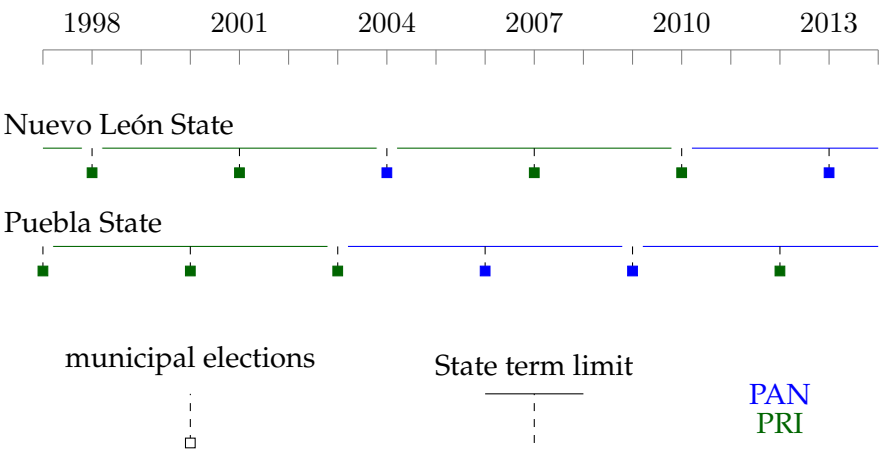
NOTE— The figure show the population weighted average of infrastructure investment as share of total spending or revenues. Ramo-33 for the year after 1998 and PRONASOL for years before 1998.

Figure 4: The non-compliance with Ramo-33 *de jure* allocation



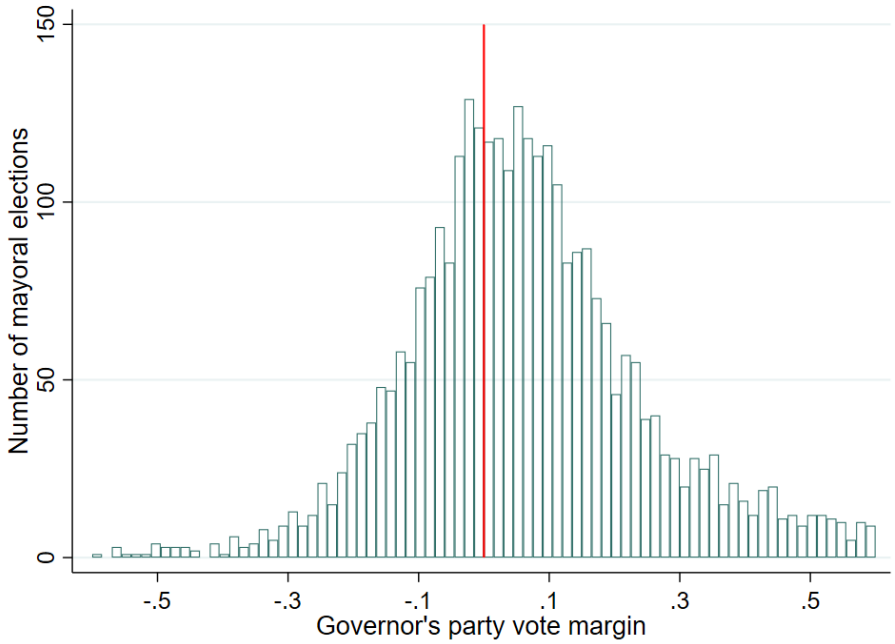
NOTE— The figure show the population weighted average or municipalities receiving transfers and the coefficient of variation (standard deviation / mean) of the distribution of yearly growth rates of Ramo-33 for the year after 1998 and PRONASOL for years before 1998.

Figure 5: Staggered elections and variation in political alignment



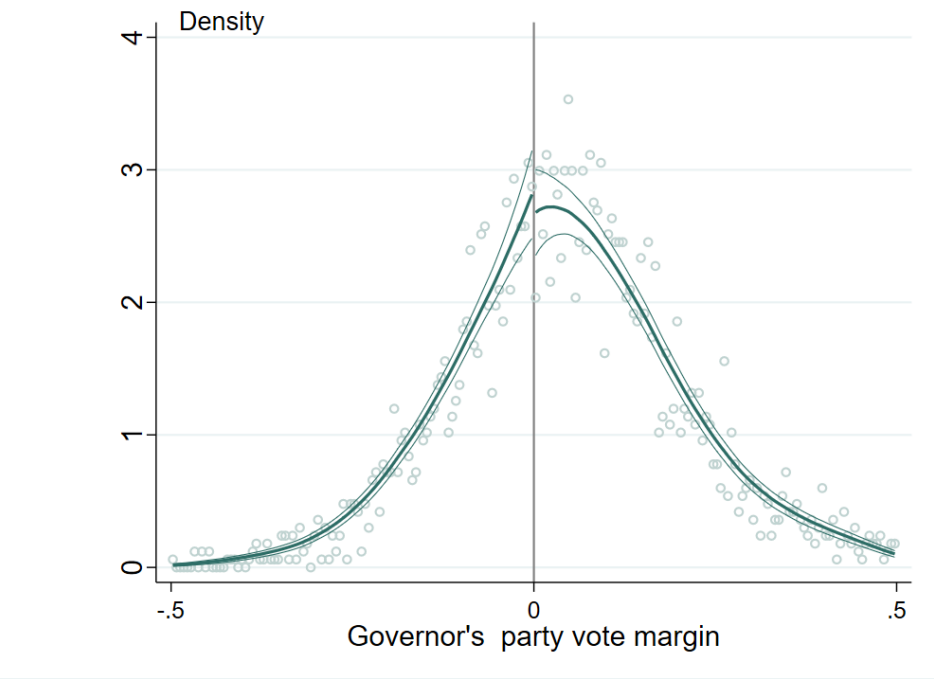
NOTE— This figure is an example of two states who have election cycles at different calendar years. The horizontal bar represent the governor's term limit (six years), while the space between squares is the mayoral term limit (three years). The vertical dashed represent election dates. The colors of the bar and squares represent the parties who won of each state and local election.

Figure 6: Distribution of mayoral elections along the vote margin $V_{m,e}$ 1998-2003



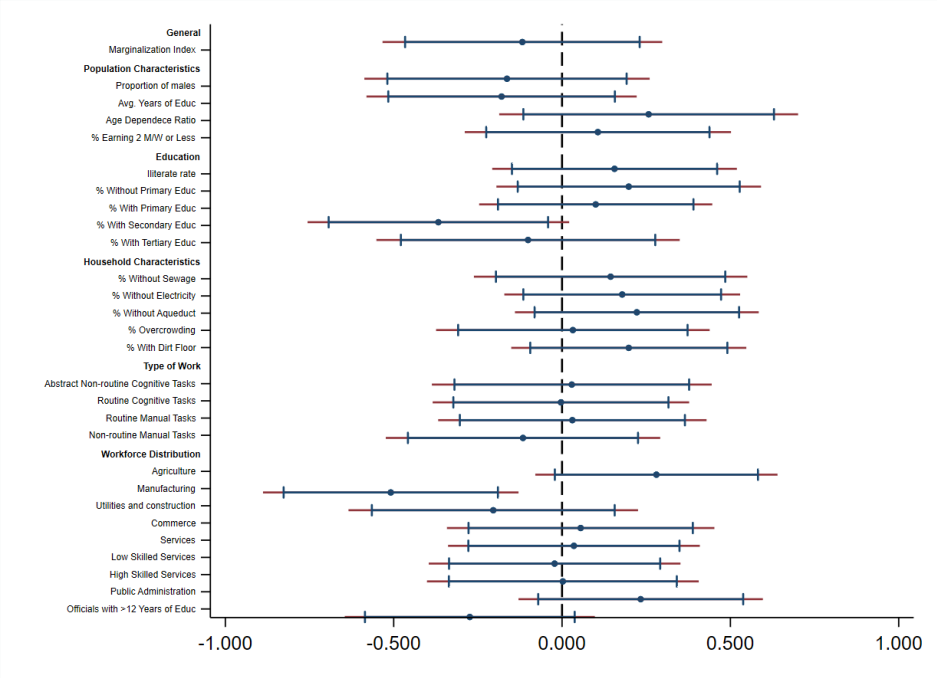
NOTE— This figure shows the histogram of the governor's party vote margin on the mayoral elections used in our estimates (1998-2003).

Figure 7: McCrary density estimates of the vote margin $V_{m,e}$ 1998-2004



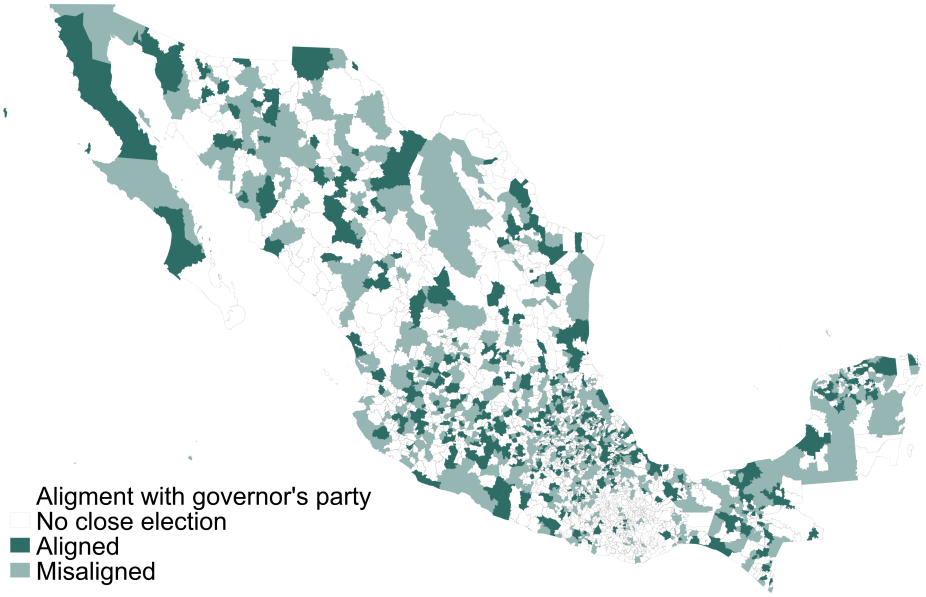
NOTE— This figure shows a estimate of the density of governor's party vote margin on mayoral elections used in our estimates (1998-2003). Each bubble groups all elections that took place in half percentage points spread bins. The dark lie is the point estimate of the density function and the light lines a 95% confidence interval.

Figure 8: Balance on predetermined (1990s) covariates



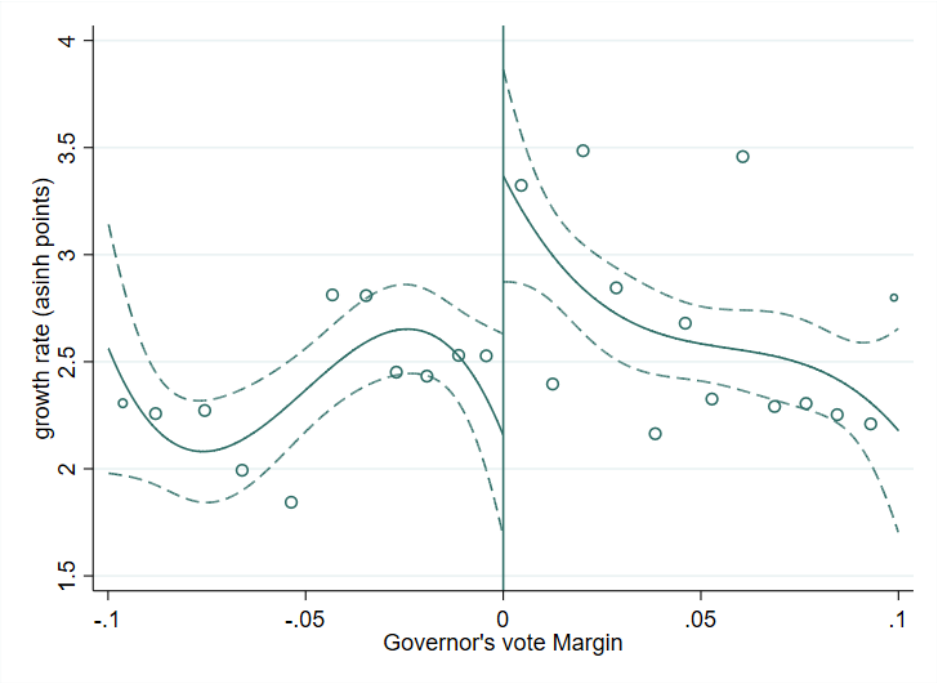
NOTE— The reported coefficients come from separated regressions that estimated the causal effect of political alignment on predetermined covariates using a variant of equation (2) that only controls linearly for the running variable on either side of the cut-off. When a municipality has more than one close election I consider only first reported election from the studied period (1998-2003). All reported outcomes are measure circa 1990 using populatin and economic census.

Figure 9: Spatial distribution of political alignment in close elections

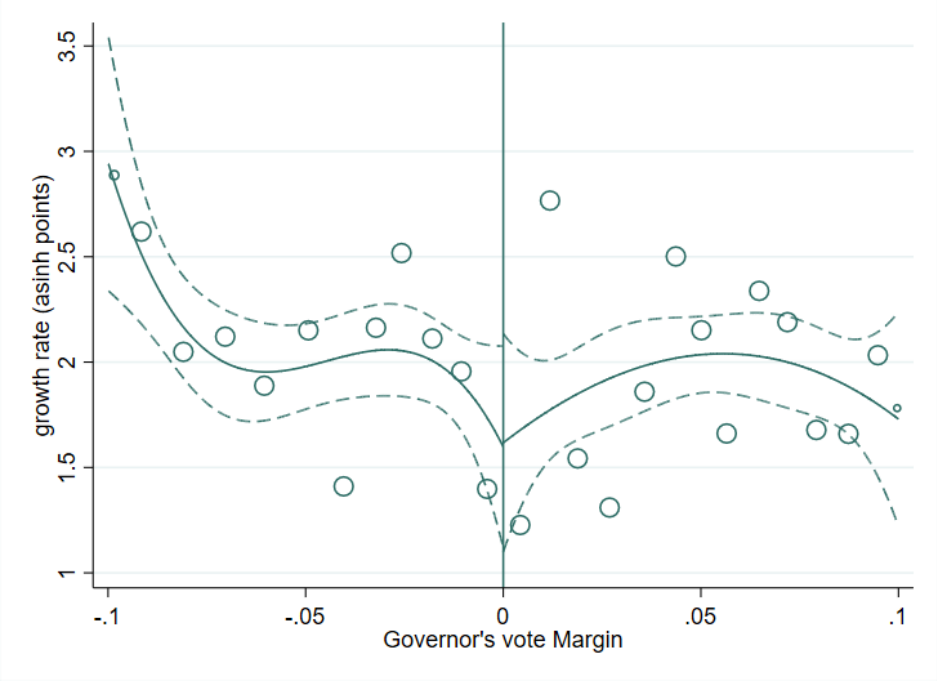


NOTE— The figure maps municipalities ruled by aligned and opposition parties for the sample used to obtain our main estimates (see section 4), where elections were decided by less than 5 percentage points.

Figure 10: Growth rate of earmarked transfers and Governor's vote margin



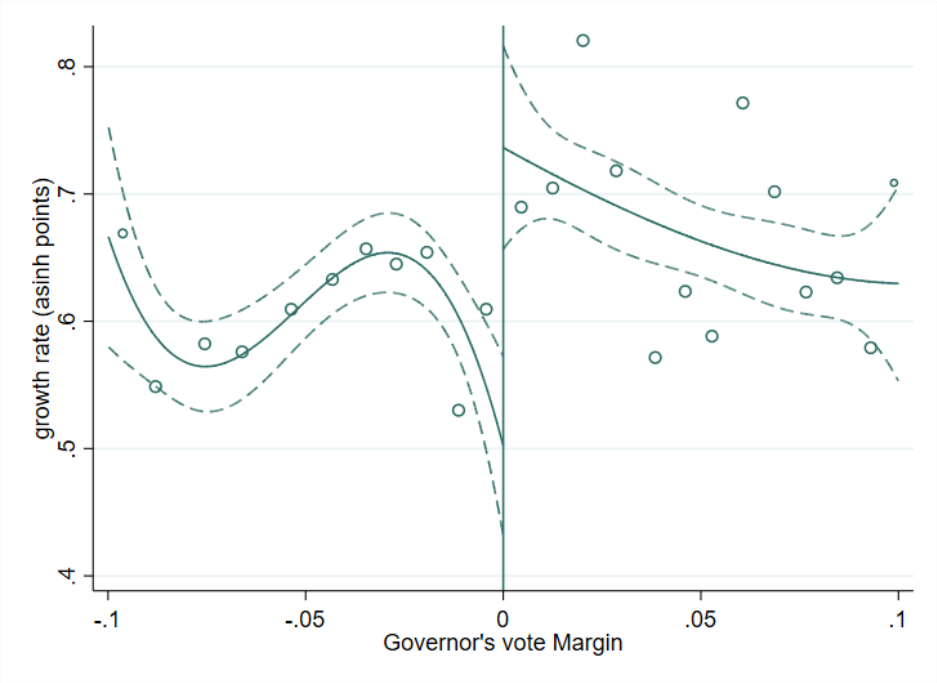
A. Post-election period



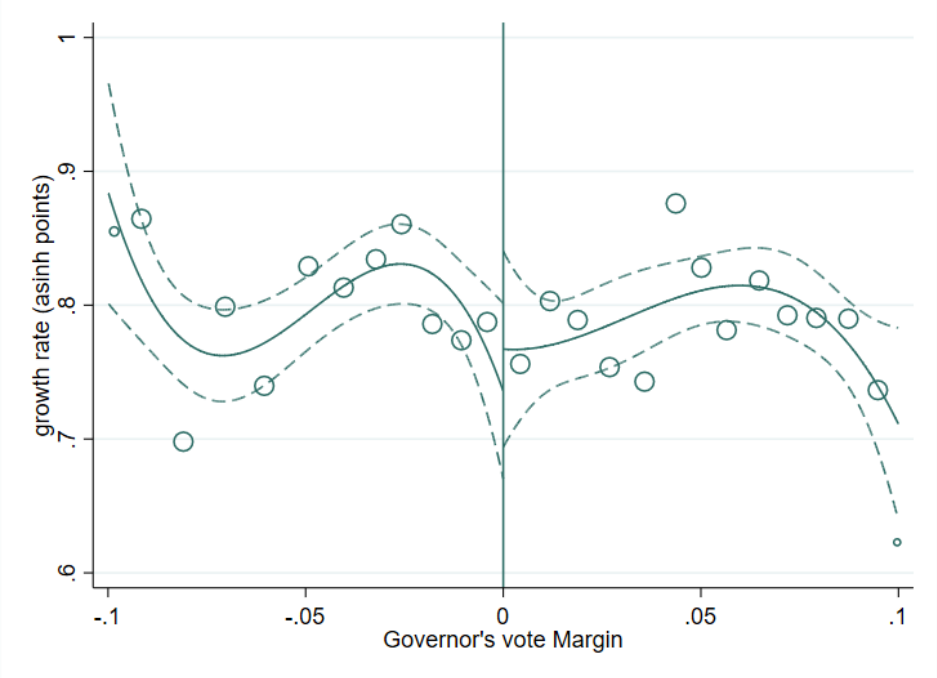
B. Pre-election period

NOTE— The lines correspond to the estimates of the outcome variable (defined in the y axis) on the a third order polynomial of the governor's vote margin. Each circle shows the mean of the observations that correspond to the specifc half percentage point bin.

Figure 11: Growth rate of total spending and Governor's vote margin



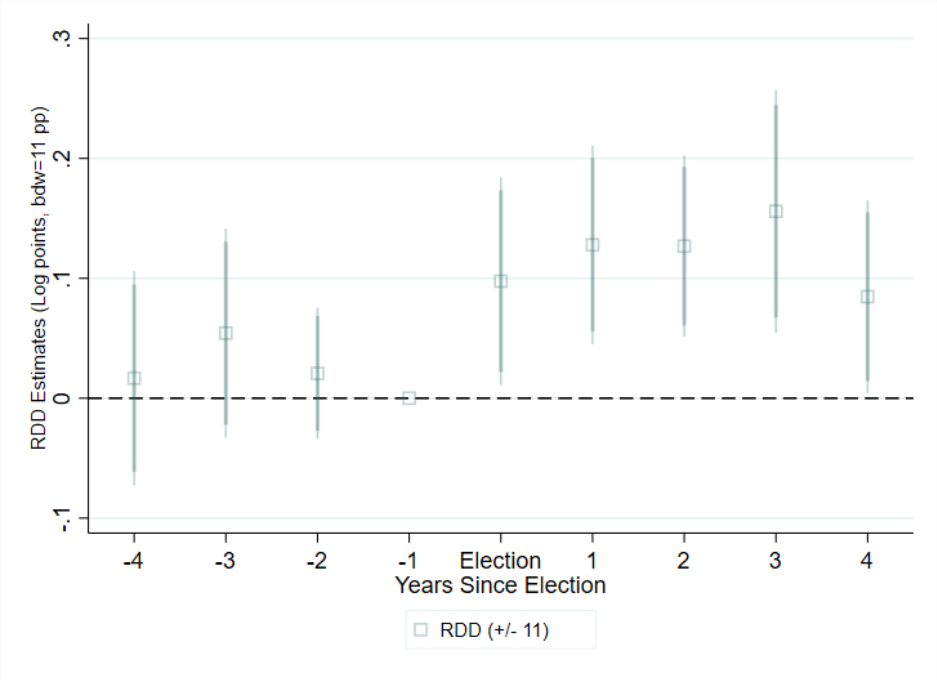
A. Post-election period



B. Pre-election period

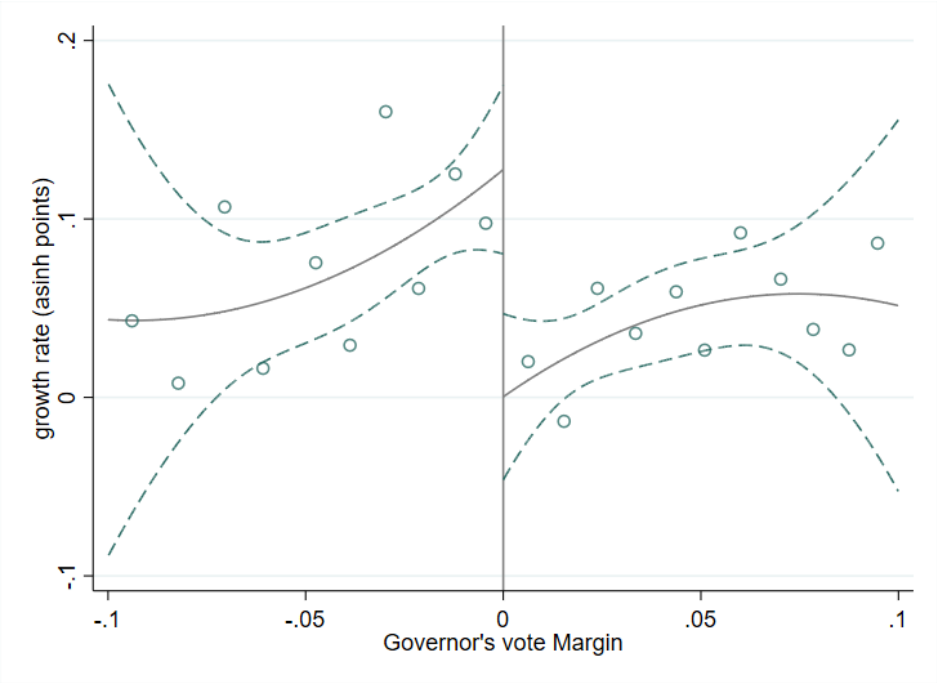
NOTE— The lines correspond to the estimates of the outcome variable (defined in the y axis) on the a third order polynomial of the governor's vote margin. Each circle shows the mean of the observations that correspond to the specific half percentage point bin.

Figure 12: Event study total spending after political alignment

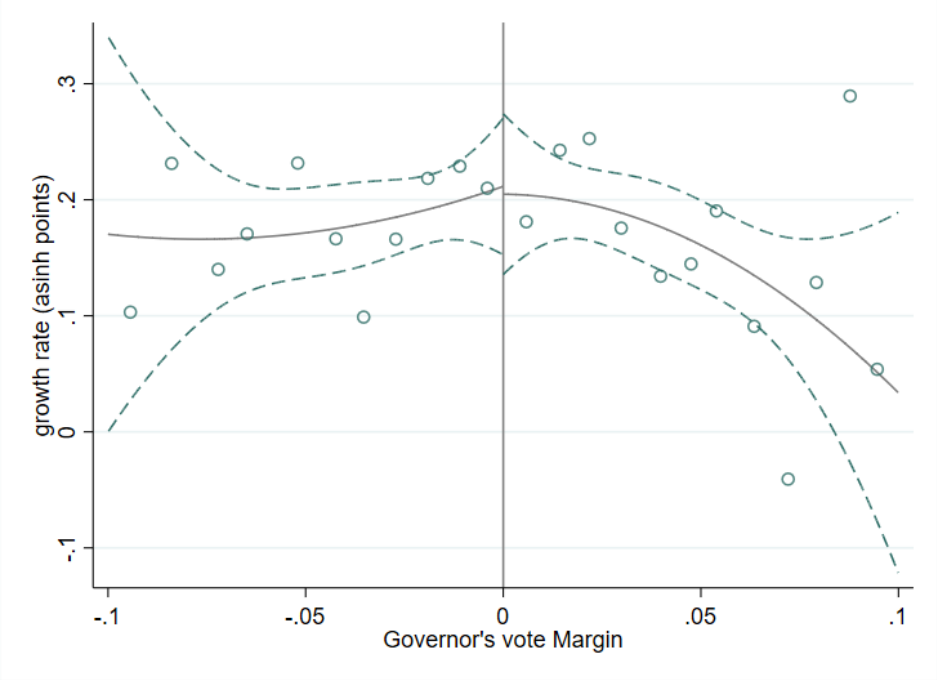


NOTE— This plot aggregate data into bins of half percentage points and estimate a third order polynomial regression between the running variable and the bins on each side of the cut-off.

Figure 13: Growth rate of formal employment and Governor's vote margin



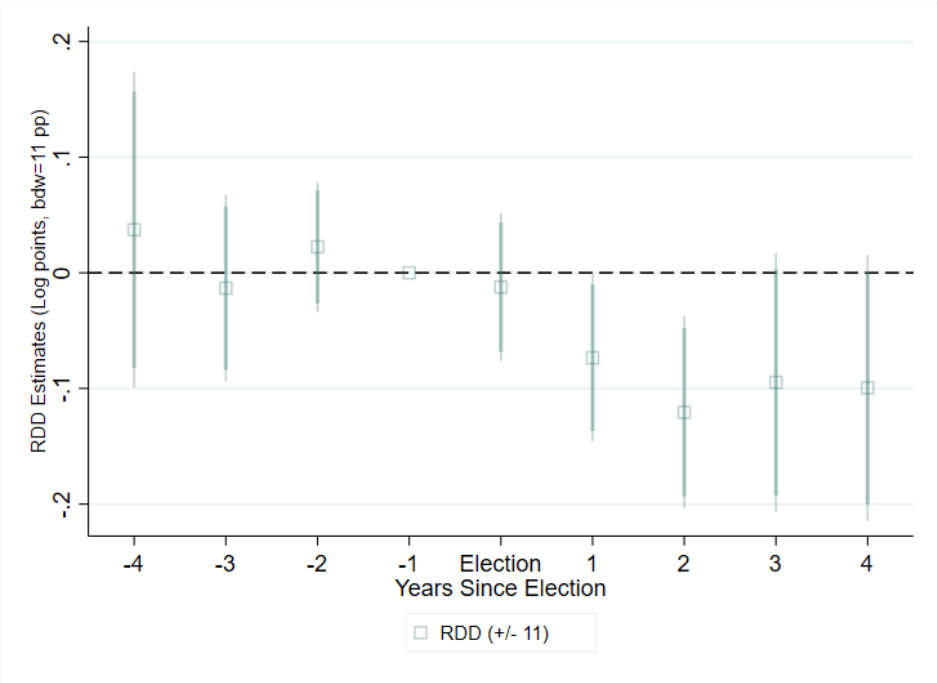
A. Post-election period



B. Pre-election period

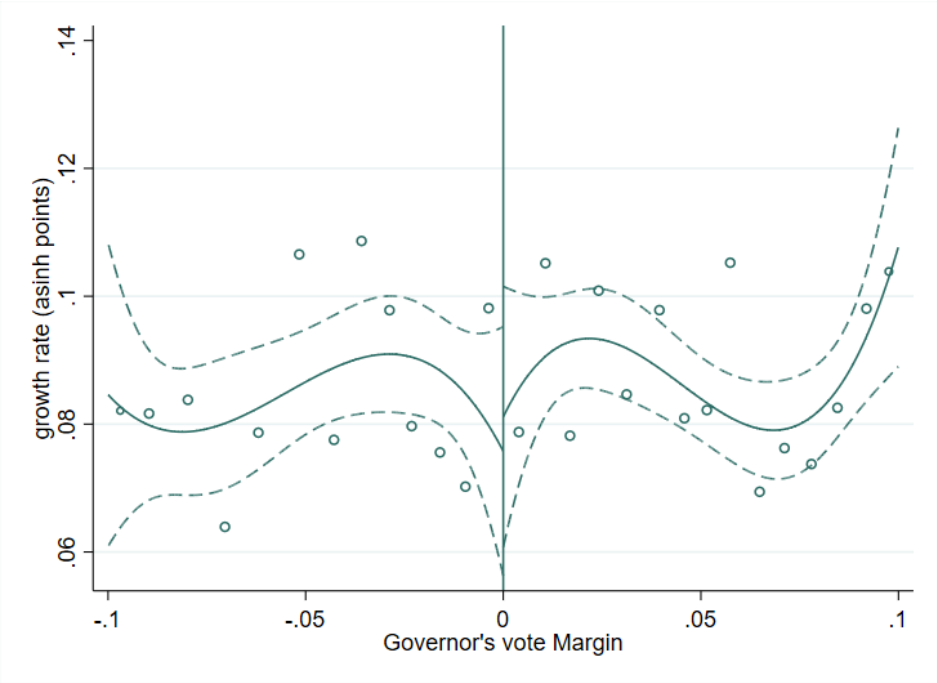
NOTE— The lines correspond to the estimates of the outcome variable (defined in the y axis) on the a third order polynomial of the governor's vote margin. Each circle shows the mean of the observations that correspond to the specific half percentage point bin.

Figure 14: Event study formal employment after political alignment

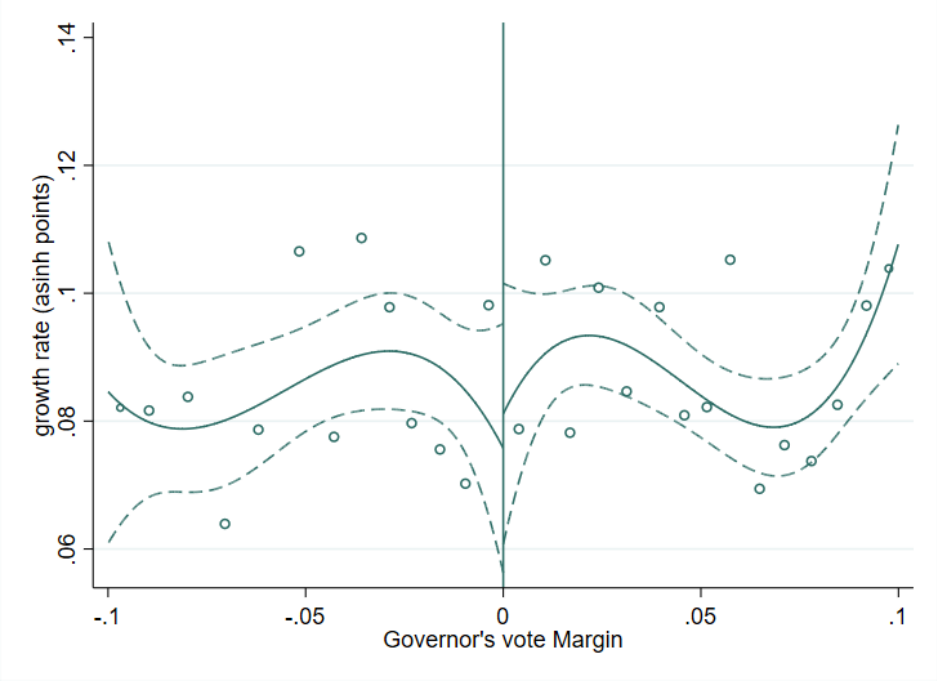


NOTE— This plot aggregate data into bins of half percentage points and estimate a third order polynomial regression between the running variable and the bins on each side of the cut-off.

Figure 15: Growth rate of wages and Governor's vote margin



A. Post-election period



B. Pre-election period

NOTE— The lines correspond to the estimates of the outcome variable (defined in the y axis) on the a third order polynomial of the governor's vote margin. Each circle shows the mean of the observations that correspond to the specific half percentage point bin.

10. Tables

Table 1–Effect of Alignment on intergovernmental transfers

	Earmarked Transfers Growth				Prb(Earmarked Transfers > 0)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Political alignment	.651*	.420**	.594**	.290**	-.0170	.00239	.00901	.0224**
	(.38)	(.18)	(.26)	(.12)	(.01)	(.01)	(.01)	(.01)
Mean dep var	1.38	1.38	1.44	1.44	.97	.97	.98	.98
R^2	.01	.79	.01	.80	.01	.21	.00	.22
Controls		✓		✓		✓		✓
Bandwidth	5	5	11	11	5	5	11	11
Obs	1313	1313	2639	2639	1313	1313	2639	2639

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 2–Effect of Alignment on source of revenue

	Earmarked transfers	Revenue transfers	Taxes & services	Public debt	Other revenues	Total revenues
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Bandwith 5 pp N=1313						
Political alignment	.420**	.0422	.0808	.287	.229	.121***
	(.18)	(.07)	(.10)	(.36)	(.15)	(.05)
Control mean dep var	1.38	.33	.36	.61	.36	.56
% of revenues	.22	.53	.062	.033	.15	1
R^2	.79	.24	.22	.48	.41	.68
Panel B. Bandwith 11 pp N=2639						
Political alignment	.290**	.0921*	.0496	.0478	.0759	.103***
	(.12)	(.05)	(.07)	(.23)	(.09)	(.03)
Control mean dep var	1.44	.35	.38	.60	.38	.57
% of revenues	.22	.54	.059	.029	.15	1
R^2	.80	.31	.18	.46	.39	.67

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 3–Effect of Alignment on Private formal employment and earnings

	Private Employment				Private Wages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Political alignment	-.121**	-.116**	-.105***	-.0950**	.00192	-.00213	.00159	-.00243
	(.05)	(.05)	(.04)	(.04)	(.01)	(.01)	(.01)	(.01)
Mean dep var	.091	.091	.071	.071	.082	.082	.079	.079
R^2	.01	.10	.01	.06	.00	.48	.00	.44
Controls		✓		✓		✓		✓
Bandwidth	5	5	11	11	5	5	11	11
Obs	1294	1294	2587	2587	1297	1297	2595	2595

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 4–Effects on 100 × probability of employment by formal and informal sector

	100 × Probability of being employed		Private total employment growth (1998-2008)	
	(1)	(2)	(3)	(4)
Panel A. Bandwith 5 pp				
Political Alignment	-3.384***	-3.412***	-.0765	-.0598
	(1.06)	(.93)	(.08)	(.06)
R^2	.02	.10	.00	.27
Observations	1,004424	1,004424	467	467
Panel B. Bandwith 11 pp				
Political Alignment	-3.061***	-3.156***	-.0614	-.0347
	(.95)	(.85)	(.04)	(.03)
R^2	.00	.24	.01	.20
Observations	2,102277	2,102277	945	945
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The estimates include municipalities with close elections between 1998 and 2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities. Columns 3 and 4 use as outcome variable the change in private municipal employment between the 2008 and 1998 round of the economic census.

Table 5–Effects on 100 × probability of employment by formal and informal sector

	Formal employment		Informal employment		Total employment	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Bandwith 5 pp N=1004424						
Political alignment	-2.360***	-2.100***	-1.024	-1.312	-3.384***	-3.412***
	(.48)	(.61)	(.86)	(.79)	(1.06)	(.93)
R^2	.02	.15	.02	.10	.00	.24
Panel B. Bandwith 11 pp N=2102277						
Political alignment	-1.707***	-1.976***	-1.354*	-1.180*	-3.061***	-3.156***
	(.42)	(.39)	(.70)	(.69)	(.95)	(.85)
R^2	.02	.15	.02	.10	.00	.24
Municipality FE	✓	✓	✓	✓	✓	✓
Election Year × Year FE	✓	✓	✓	✓	✓	✓
Individual × Year FE		✓		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 6–Effect of Alignment by pre-election economic cycle

	Public spending		Private-Employment	
	(1)	(2)	(3)	(4)
Panel A. High pre-election growth				
Political Alignment	.113	.131**	-.130**	-.137**
	(.08)	(.06)	(.06)	(.06)
Control mean dep var	.63	.63	.050	.050
R^2	.01	.49	.01	.10
Observations	1198	1146	1248	1248
Panel B. Low pre-election growth				
Political Alignment	.155**	.0934**	-.0711	-.0670*
	(.06)	(.05)	(.04)	(.04)
Control mean dep var	.51	.51	.089	.089
R^2	.01	.52	.00	.09
Observations	1441	1441	1568	1568
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 7–Effect of Alignment on employment by tradable and non tradable

	Private-Employment			
	Tradable		Non-Tradable	
	(1)	(2)	(3)	(4)
Panel A. Bandwidth 5 pp, N= 1220				
Political Alignment	-.157**	-.146**	-.0145	-.0235
	(.07)	(.07)	(.06)	(.06)
Control mean dep var	.020	.020	.15	.15
R^2	.01	.09	.00	.07
Panel B. Bandwidth 11 pp, N= 2421				
Political Alignment	-.134***	-.139***	-.00995	.00153
	(.05)	(.05)	(.04)	(.04)
Control mean dep var	-.0026	-.0026	.16	.16
R^2	.01	.05	.00	.05
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 8–Effect of Alignment on employment by pre-election share of government dependent sectors

	Public spending		Private-Employment	
	(1)	(2)	(3)	(4)
Panel A. High share of government dependent sectors, N=1300				
Political Alignment	.134*	.0995*	-.0682	-.0754*
	(.08)	(.06)	(.05)	(.04)
Control mean dep var	.57	.57	.067	.067
R^2	.01	.50	.00	.08
Panel B. Low share of government dependent sectors, N=1275				
Political Alignment	.154*	.114**	-.110**	-.118**
	(.08)	(.05)	(.05)	(.06)
Control mean dep var	.56	.56	.077	.077
R^2	.01	.50	.01	.12
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 9–Effect of Alignment on consumption

	Night lights		Electricity consumption	
	(1)	(2)	(3)	(4)
Panel A. Bandwidth 5 pp				
Political Alignment	.0738*	.0376	.0905	.0359
	(.04)	(.03)	(.07)	(.06)
Control mean dep var	.028	.028	.11	.11
R^2	.00	.75	.01	.12
Observations	1313	1313	887	887
Panel B. Bandwidth 11 pp				
Political Alignment	.0363	.0171	.0328	.000
	(.03)	(.02)	(.05)	(.05)
Control mean dep var	.022	.022	.13	.13
R^2	.00	.73	.00	.07
Observations	2638	2638	1704	1704
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 10–Effects on 100 × probability of being employed, unemployed and part of the labor force

	Total employment		Unemployment		Labor force	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Bandwith 5 pp N=1004424						
Political alignment	-3.384***	-3.412***	.448	.534	-2.936***	-2.878***
	(1.06)	(.93)	(.36)	(.35)	(.84)	(.70)
R^2	.00	.24	.00	.01	.00	.25
Panel B. Bandwith 11 pp N=2102277						
Political alignment	-3.061***	-3.156***	.509*	.557*	-2.552***	-2.599***
	(.95)	(.85)	(.29)	(.28)	(.79)	(.70)
R^2	.00	.24	.00	.01	.00	.25
Municipality FE	✓	✓	✓	✓	✓	✓
Election Year × Year FE	✓	✓	✓	✓	✓	✓
Individual × Year FE		✓		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 11–Effect of Alignment on probability of winning subsequent elections

	Prob of winning next election		Prob of winning next two elections	
	(1)	(2)	(3)	(4)
Panel A. Bandwidth 5 pp, N=1313				
Political Alignment	.0648	.0410	.0828	.0925
	(.09)	(.09)	(.06)	(.06)
Control mean dep var	.25	.25	.077	.077
R^2	.03	.15	.04	.19
Panel B. Bandwidth 11 pp, N=2639				
Political Alignment	.156**	.134**	.128***	.130***
	(.06)	(.06)	(.05)	(.05)
Control mean dep var	.31	.31	.11	.11
R^2	.03	.11	.03	.12
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 12–Effect of alignment on public employment and wages

	Public Employment		Wage bill Public Employees	
	(1)	(2)	(3)	(4)
Panel A. Bandwidth 5 pp, N= 1313				
Political Alignment	.309*	-.0243	.208*	.128
	(.18)	(.05)	(.11)	(.10)
Control mean dep var	.46	.46	.51	.51
R^2	.00	.90	.01	.32
Panel B. Bandwidth 11 pp, N= 2639				
Political Alignment	.191	-.00319	.121	.0935
	(.14)	(.05)	(.08)	(.07)
Control mean dep var	.50	.50	.54	.54
R^2	.00	.89	.00	.35
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 13–Effect of alignment on public investment

	Bandwith 5pp		Bandwith 11pp	
	(1)	(2)	(3)	(4)
Political Alignment	.383	.406**	.363	.282***
	(.33)	(.16)	(.22)	(.10)
Control mean dep var	1.20	1.20	1.14	1.14
R^2	.19	.19	.20	.20
Observations	1313	1313	2639	2639
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 14–Effect of alignment on public spending by categories

	Infrastructure Investment	Service Contracts	Subsidies & Transfers	Public workers salaries	Other spending	Total spending
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Bandwith 5 pp N=1313						
Political alignment	.406**	.252**	.270**	.128	.0525	.121***
	(.16)	(.11)	(.11)	(.10)	(.09)	(.05)
Control mean dep var	1.20	.28	.64	.51	.45	.56
% of spending	.19	.15	.14	.32	.22	1
R^2	.83	.42	.71	.36	.57	.68
Panel B. Bandwith 11 pp N=2639						
Political alignment	.282***	.159**	.0650	.0935	.0739	.103***
	(.10)	(.07)	(.08)	(.07)	(.06)	(.03)
Control mean dep var	1.14	.31	.57	.54	.46	.57
% of revenues	.20	.15	.14	.31	.21	1
R^2	.81	.40	.69	.35	.54	.67

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 15–Effect of alignment on inputs of infrastructure spending: construction jobs

	Employment		Wages	
	(1)	(2)	(3)	(4)
Panel A. Bandwidth 5 pp, N= 1313				
Political Alignment	-.211	-.0930	.299**	.0769
	(.16)	(.15)	(.15)	(.06)
Control mean dep var	.18	.18	.40	.40
% total jobs	.10	.10		
R^2	.01	.16	.01	.77
Panel B. Bandwidth 11 pp, N= 2639				
Political Alignment	-.256**	-.222**	.111	.0366
	(.10)	(.10)	(.09)	(.04)
Control mean dep var	.15	.15	.40	.40
% total jobs	.11	.11		
R^2	.01	.12	.00	.64
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 16–Effect of alignment on outputs of infrastructure spending: stock of public infrastructure

	Δ 1995-2010 log points difference			
	(1)	(2)	(3)	(4)
Public infrastructure				
Sewerage service	2.765	0.841	0.551	-0.248
	(3.575)	(2.930)	(2.369)	(1.940)
Electric lighting	3.014	3.147	3.278*	2.368
	(2.682)	(2.808)	(1.739)	(1.721)
Piped water	5.398*	4.378	2.860	2.329
	(2.840)	(3.136)	(1.907)	(1.912)
Private assets				
Overcrowding	-2.620***	-1.925**	-1.747**	-1.320**
	(0.982)	(0.941)	(0.706)	(0.616)
Concrete floor	3.857	1.887	3.319*	1.594
	(2.494)	(2.395)	(1.738)	(1.600)
Observations	461	461	934	934
Controls		✓		✓
Bandwith	5	5	11	11

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 17–Effect of Alignment on Homicides

	Prob (Homicide>0)		Homicide rate	
	(1)	(2)	(3)	(4)
Panel A. Bandwidth 5 pp, N= 1428				
Political Alignment	-.0599	.0313	-.178	.0963
	(.05)	(.05)	(.16)	(.15)
Control mean dep var	.0054	.0054	-.099	-.099
R^2	.00	.34	.00	.31
Panel B. Bandwidth 11 pp, N= 2871				
Political Alignment	-.0132	.0406	-.0840	.0754
	(.04)	(.04)	(.11)	(.11)
Control mean dep var	.0081	.0081	-.086	-.086
R^2	.00	.32	.00	.29
Controls		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.

Table 18–Effect of Alignment on Corruption

	Prb(Audited)		Prb(corruption>10%)		Prb(malfeaseance>10%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Bandwidth 5 pp						
Political Alignment	.0112	.0126	.111	.085	-.580***	-.455**
	(.01)	(.01)	(.21)	(.19)	(.19)	(.22)
Control mean dep var	.017	.017	.30	.30	.35	.35
R^2	.00	.15	.04	.53	.19	.69
Observations	4137	4134	83	83	83	83
Panel B. Bandwidth 11 pp						
Political Alignment	.00551	.00433	-.0774	-.0853	-.420***	-.439***
	(.01)	(.01)	(.13)	(.12)	(.14)	(.13)
Control mean dep var	.018	.018	.30	.30	.22	.22
R^2	.00	.15	.02	.40	.10	.42
Observations	8652	8640	186	186	186	186
Controls		✓		✓		✓

NOTE— This table reports the estimates of political alignment from equation (2). The sample includes post electoral years of all municipalities with close elections during the period 1998-2003. The outcome variables are measure as a three year changes. Controls refers to state fixed effects, election-year fixed effects, and baseline political characteristics (incumbency status, previous political alignment, previous political party). Mean dep var refers to the sample average of the outcome variable for the non-aligned municipalities.