Fiscal windfalls and missing jobs: Evidence from close elections in Mexico

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Abstract

This paper studies the economic consequences of politically induced public spending. I combine the increase of intergovernmental transfers in Mexico with a close-elections discontinuity design to estimate the causal effect of political alignment on public spending and private sector employment at the local level. I find political alignment increases local public spending but slows down the creation of jobs. I show this is consistent with public spending crowding out private-sector economic activity. The slowdown in job creation does not translate into lower economic output or consumption. It is stronger in economies where production factors are scarce before they become politically aligned. The evidence suggests that a potential mechanism through which this crowding-out occurs is an income effect rather than public sector enlargement or private investment reduction that may result from higher infrastructure investment.

JEL: R10, G20, H10

1. Introduction

Public-sector spending can be an effective tool to boost economic growth in distress economic areas or in places that suffer from temporary economic downturns (Chodorow-Reich, 2019). However, the everyday public spending does not occur during recessions nor aims to be allocated where it can be more effective. A large body of research on political economy documents that politicians skew resources to places in response to political factors (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 these same political factors affect local economic activity.

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

Two reasons explain the scant evidence on the economic effects of politically induced 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 usually detects the effect of politics on public-spending tend to do it over a fraction of public resources that are not sufficiently large to have any effect on local economic growth.

I circumvent these problems by taking advantage of the political economy behind the implementation of a Ramo-33 in Mexico, a policy that led to a substantial increase in local public spending. In 1998, the government of Mexico expanded intergovernmental transfers to local governments, which led to an unexpected and economic sizable increase in local public spending. Although Ramo-33 transfers were earmarked and conceived as an

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[1]This may lead to a downward bias if the confounder is related to the deep economic factors behind the downward economic trend or an upward biased if a mean reversion component confounds the policy impact.

entitlement program with its own statutory allocation rules, politics had a sharp influence on where and when allocating the transfers. In particular, during the former years of the policy, state-governors took advantage of their (weakly supervised) role in allocating the transfers to skew public resources towards municipalities with political traits that offer them high political return, one of these traits was political alignment, which motivates the empirical strategy developed in this paper.[2]

I take advantage of plausibly exogenous variation in political alignment to identify the effects of spending on employment. I use a close election regression discontinuity design,[3] 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 randomly allocated for razor-close elections implies that aligned municipalities are not systematically different that misaligned municipalities in terms of the economic shocks, secular trends in economic activity or past public spending.

As a first step toward understanding how political alignment affects local economic activity, I assemble an unique dataset that provides information at the municipality-year level. This level of disaggregation allows me to measure the dynamics of economic and public finance outcomes before and after a municipality becomes politically aligned. I use detailed public finance data to measure the effect of alignment on public spending. To construct my primary measure of economic activity I use the employer-employee data for the universe of the Mexican formal sector, which allows me to measure total employment and wage bill for each municipality and distinguish across several characteristics: sector, firm size, age and gender. I combine this data with other sources of information that allow me to measure the Mexican informal sector (household surveys, and firm level data from economic censuses) and total economic activity (night lights and electricity consumption).

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[2]This is not the first study that explores the political economy of the allocation of Ramo-33 transfers, several papers have look the correlation of several political factors with the allocation of transfers across Mexico (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.

[3]This research design is 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)

The first part of the paper focuses on the political economy behind the allocation of the earmarked transfers. I document how partisan alignment affected the allocation of public resources. I find that transfers' growth rate 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. I do not find that this increase in transfer crowds-out alternative source of revenue (other intergovernmental transfers, taxes, or debt). This implies that total revenues should increase. In line with this logic, I find that the growth rate of total spending increase is 10 percentage points higher in politically aligned municipalities relative the non-aligned municipalities.

The second part of the paper explores whether the alignment-induced spending crowd in or crowd out the private sector economic activity. To do so, I use measures of employment and wages from the universe of formal workers. I find political alignment affects employment but not wages. In particular, the employment growth rate is 10 percentage points lower in politically aligned municipalities than non politically aligned counterparts. A back of the envelope calculation suggests that the 10 percentage point decline is equivalent to reducing one percentage point in the formal employment rate. This effect is persistent after the mayor leaves the office, and it seems to be explained by a slowdown in the creation of formal jobs in politically aligned municipalities.

Before moving into exploring the mechanisms, I explore to what extent the decline in formal employment affects total employment or if the result 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 data on total (formal and informal) private employment from Economic Censuses. Using the household surveys, I find the probability of being employed declines by 2.7 percentage points. This decline happens only in the formal sector, and at most, one-fifth of the decline (0.6 percentage points) is related to shifts to the informal sector. Additionally, total employment counts from the economic census suggest a decrease of 5 percent of total employment, albeit estimates using this data have less power to provide precise confidence intervals.

There are two potential mechanisms through which politically induced fiscal windfalls can deter the creation of formal employment: The first is a rent seeking effect, which

would suggest that fiscal windfalls affect negatively political institutions and spur corruption (Brollo et al., 2013), which has been proven can deter local economic growth(Colonnelli and Prem, 2020). The second is that public spending crowds out private sector economic activity, competing for production factors that otherwise would have been employed in the private sector.

To test the first hypothesis, I use data on the results of fiscal audits to local governments, which is performed by an autonomous watchdog agency (Auditoria Superior de la Federación). 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 or corruption.

I find suggestive evidence that the main channel through which public spending deters employment growth is a crowding-out effect. A crowding-out effect takes place only when the economy is not at full capacity. I find that political alignment is strongly affected by tight labor markets, measured as municipalities with a high pre-electoral employment growth rate. Also, I find that political alignment has stronger effects on tradeable sectors than non-tradeable industries, which is consistent with the crowding-out effect because the non-tradeable industries rely more on local demand, which increases due to the public spending, than industries on the tradeable sector. Finally, I find that two different measures of aggregate economic activity, night lights and electricity consumption, do not decline because of political alignment, which is consistent with total consumption not declining and only a reduction of product not declining and, to some extent, higher public spending compensating the reduction in jobs.

A unique feature of this finding is that I can rule the traditional ways through which theory argues that crowding out happens, 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 through which crowding out can affect the economy: public sector enlargement, economic disruption of infrastructure investment, and income effects.

I argue that the evidence suggests an income effect channel based on three arguments suggesting that voters' welfare did not decrease. First, voters of politically aligned mu-

nicipalities tend to re-elect the same political party. I find that the probability of being re-elected next period increases by 15 percentage points. Second, the decrease in the formal employment rate (2.7 percentage points) is explained by decreased labor force participation. The fact the decline is not explained by higher unemployment rates, suggesting that the reductions in employment are not involuntary. Third, I find no effects on night lights or electricity consumption, which is consistent with the idea that voters' welfare did not decrease.

I do not find strong evidence suggesting that public sector enlargement or spending on infrastructure projects explain the main results. One crowding-out mechanism is that higher public expenditure increases public sector employment at the expense of the private sector. This mechanism cannot explain my results because public employment is very stable and does not increase disproportionally in politically aligned municipalities. Another crowding-out mechanism that can negatively affect employment in the short run is infrastructure investment. Ramey (2020) suggested that infrastructure investment can crowd-out the private sector because construction disrupts economic activity (disruption effect) and also substitute private investment in the short-run (delay effect). The evidence in favor of this channel is not conclusive. Although the additional revenues are disproportionally spent on infrastructure projects, I do not find clear evidence of construction occurring in aligned municipalities when measuring using either input (construction jobs) or outputs (higher infrastructure).

RELATED LITERATURE. — This paper contributes to several strands of the literature. First, it contributes to the literature that ask 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) to increase in the long run as a result of the effect that a higher stock of public capital have on labor productivity (Kline and Moretti, 2014; Yaffe, 2020; Leduc and Wilson, 2013; Allen and Arkolakis, 2019). Similar to this literature, I find negative contemporaneous effect of infrastructure investment on employment; but contrary to the previous evidence, I do not observe a positive effect of employment in the medium run, which I argue is

explained because of the broken link between infrastructure spending and public capital stock I find in the data.

Second, it contributes to the literature of distribute politics that focus on how the political distortions affect the allocation of public resources. The bulk of the literature have 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). The transfers studied in all this papers focus on discretionary spending because the conventional wisdom suggest that entitlement programs can not be politically manipulated. I provide suggestive evidence that entitlements can also suffer from political manipulation when the allocation rules are not salient and monitored by depoliticized institutions.

Third, I contribute to the literature that studies the economic effects of political favoritism. This literature agrees that ethnic and regional favoritism lead to higher economic growth(Hodler and Raschky, 2014; Alesina, Michalopoulos and Papaioannou, 2016); but has opposite findings regarding the effects of partisan favoritism. For example, Cohen, Coval and Malloy (2011) finds that employment *decreases* in U.S. states who had a senator leading the congressional committee and argue it is because of higher federal spending; while Asher and Novosad (2017), using data from India, finds that employment *increases* more in districts who are aligned with the state ruling party because of less strict regulation.[4] I propose two arguments to reconcile this findings: First, the policy lever that politicians are allowed to manipulate affects the answer one gets: my findings are in line Cohen, Coval and Malloy (2011) because in my setting politicians manipulate the same policy lever, namely, public spending. Second, I find that labor misallocation is an unintended and overlooked cost of politically-induced benefits which could deter growth. We expect this cost to be less relevant in India, an economy with high levels of factor misallocation relative to an economy with low levels of misallocation as U.S.

Also, this paper revisits the literature on the resource curse. There is a established consensus on the negative effects of fiscal windfalls on political institutions. Independent of the origin of fiscal windfalls, higher fiscal resources tend to increases corruption and

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[4]Other studies of political alignment with null results are Bonilla-Mejía (2017)

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 trough which fiscal windfalls may affect local economies. The demand shocks induced by the fiscal windfall may distort the optimal allocation of labor across sectors, in a similar fashion that the *ducth disease* reallocates labor towards natural resource industries.[5] Particularly, in this context I observe a shrinkage of the formal sector, which mechanically reduces taxes and local aggregate productivity, the latter depends on the extent to which higher informality implies larger factor misallocation (La Porta and Shleifer, 2014; Hanson, 2010).

2. Institutional Context

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

A. The national expansion of intergovernmental transfers in Mexico

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

This paper takes advantage of the expansion of intergovernmental transfer that followed the creation of Ramo-33 in December of 1997. In particular, it focuses on the two subcomponents of Ramo-33 that are allocated to municipalities: FORTAMUN-DF and

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[5]The main difference with respect to the dutch disease is that in this setting the demand shock comes from the government spending decisions.

FISM; the rest of the subcomponents of Ramo-33 are transfers directed to state governments, and we do not observe the specific location of the projects funded by each state; from now on I refer to these two municipal subcomponents as Ramo-33.[6] These two funds are desirable for identification purposes for two reasons: First, they provide unexpected increases in local public spending; Figure 1 shows the increase in spending that resulted from these two subcomponents of Ramo-33 was weakly correlated with the historical allocation of government spending.[7] Second, they meant a relatively large shock for the average municipality. Figure 2 shows that, between 1998 and 2006, local spending increased by 20 percentage points of municipal's GDP. These relatively large magnitudes resulted from the redistributive nature of Ramo-33. In particular, one of the core objectives of these transfers was to allocate disproportional public resources to less developed municipalities, which explains the significant increases for the average municipality.

A second important aspect of Ramo-33 is that these transfers were earmarked to infrastructure projects and build local institutional capacity. As a result, it can be observed in Figure 3 that the rise in public investment mimicked the increase of Ramo-33 transfers. Specifically, FISM is earmark to a broad set of infrastructure projects: from social infrastructure (e.g., health and school facilities) to core economic infrastructure (e.g., electrification and construction of dams or municipal roads). In comparison, FORTAMUN-DF is earmark to either infrastructure (e.g., maintenance of urban infrastructure) or non-infrastructure projects (public security, debt payments, and state capacity).

To summarize, Ramo-33 implied an unexpected, economically substantial increase in local public spending. This spending shock is explained by a large 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 increases is expected to be stronger.

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[6]Both sub-components uses 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: coverage to electricity, poverty, education, 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

[7]The other sub-components or Ramo-33 were strongly correlated with the previous allocation because they were created to facilitate the decentralization of public expenditure rather than to increase local public spending.

B. *The political economy behind the local exposure to 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. To do so, these intergovernmental transfers were designed as entitlements programs, with an allocation across municipalities decided by a fixed formula that was a function of predetermined municipality characteristics. Also, by law, these federal transfers to municipal governments can not bypass the state governments. This restriction aimed to prevent municipalities with low bargaining power with respect to the federal government from systematically fewer transfers.

However, these safeguards created their own distortions; they transfer disproportional power from the federal to the state government, during a period where the federal government could not dominate this governments As a result, anecdotal evidence suggests that state governments did not follow strictly the guidelines defined by the allocation formula when distributing the resources of Ramo-33. Some governors publicly stated that they should be allowed to skip the allocation defined by law and replace it with their own allocation.[8]

Figure 4 shows two pieces of evidence to substantiate the claim of manipulation of intergovernmental transfers. First, it shows a large proportion of municipalities that reported not receiving intergovernmental transfers between 1998 and 2002. This result is odd since the statutory allocation mandate a positive amount of transfers for all municipalities. Second, it shows the dispersion in the growth rate of intergovernmental transfers. Since 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.[9] Figure 4, shows not only a positive coefficient of variation but also that it increases after the enaction of Ramo-33 until 2005. It is not clear what explains the substantial decline after 2005. Still, I find it correlates with the increases in fines imposed by the ASF (Autonomous watchdog agency) to municipal and state governments

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

[9]Although several reasons can explain a positive dispersion on yearly growth rates we would not expect these reasons to create the observed pattern in the data. Some of these reasons are measurement error, political business cycle, or a change in formula's inputs (every time a population census is released, in our period either in 1992, 2002, 2012).

for malfeasance and waste of public resources.

To summarize, the evidence points out to significant discretion in the allocation of intergovernmental transfers across municipalities. Since the distribution is in charge of state governors, this discretion is likely explained by distinct political factors that state governors care about, such as: political competition, turnout, partisan favoritism.

This paper focuses on the role of partisan favoritism in the allocation of transfer at local level. Where partisan favoritism is defined as the tendency of central governments to favor local constituencies ruled by their same political party. This political factor is convenient for two reasons: first, it connects our research question with one of the most prevalent political distortion found in democracies; second, it provides a transparent identification strategy based on close elections which will be explained in detail in the following section.

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 any electoral outcome it results from the aggregation of voters decisions when casting their ballots. Since voters' decisions are influenced by a myriad of political and economic factors that affect the outcomes of interest, I should expect strong biases from any naive OLS estimate biased.[10] To address this identification problem, I exploit variation from razor-close elections, a research design known as close election regression discontinuity design (RDD).

A. Identification assumption

This identification strategy takes advantage of the variation provided by close elections to get causal estimates of political alignment. To implement it, I construct the vote margin

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[10]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 bias.

as my *running variable* (Hahn, Todd and Van der Klaauw, 2001), this variable is computed for every municipal election indexed by municipality m and election year e. The vote margin is defined as the difference, in votes between the candidate of the governor's party—$v_{m,e}^g$, and the main opposition's party— $v_{m,e}^o$, normalized by the total number of votes—$v_{m,e}$.[11]

$$(1) \qquad\qquad VM_{m,e} = \frac{v_{m,e}^g - v_{m,e}^o}{v_{m,e}}$$

Consequently, above (below) zero corresponds to municipalities where the elected candidate does (does not) belong to the state governor's party. Candidate's political affiliation is measured before elections take 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 cutoff.[12]. The main intuition of the *continuity* assumption is that municipalities where the politically aligned candidate barely lost are valid counterfactual of municipalities where the politically aligned candidate barely 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 would be obtained by the difference between the expected value of aligned and misaligned municipalities at the cut-off. Therefore the precision of the

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[11]I consider both individual candidates and coalitions to define political alignment. When a political coalition forms the governor's party, I consider politically aligned, any mayor that belongs to any of the parties that are part of the political coalition. Moreover, the main opposition's party 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.

[12]This assumptions simply states that $E[y(1)_{m,e} \mid VM_{m,e}]$ and $E[y(0)_{m,e} \mid VM_{m,e}]$ are continuous at the cut-off (i.e. $VM_{m,e} = 0$). where 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

estimates will increase with the amount of observations at the cut-off.

B. RDD specification

I estimate the parameter of interest using a 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 lower or equal to five percentage points.[13] The regression pools the observations of the post-electoral years that correspond to the mayor's ruling period that took place during our period of study (1998-2006). In particular, I estimate the following equation:

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

(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 obeys to the fact that three years is the mayors term limit, therefore we measure a change between a period before and a period after a municipality is politically aligned.[14] $aligned_{m,e}$ is an indicator variable that identifies whether the current mayor belongs to the governor's party and is 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 by state–$\delta_{s(m)}$ and election-period–$\xi_{e,k}$ trends; and $X_{m,e-1}$ by political characteristics from the previous election cycle: political alignment, political party, governors vote margin . Alignment is defined at the municipality election level (Abadie et al.,

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[13]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 a 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.

[14]Most of the mayoral election have a three year term limit with few exceptions (2 percent of the elections have 4 years term limit). When municipalities have larger term limits we 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.

2017), but since we expect correlation political alignment within the same municipality I cluster standard errors at the municipality level (Bertrand, Duflo and Mullainathan, 2004).

In this specification β which measures the effect of political alignment across all k post-election years; and it is identified from variation in political alignment across municipalities who had a close election within the same state.[15]

C. Dynamic RDD specification

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

In our setting, treatment is allowed to change over time, i.e. and aligned municipality in the electoral cycle e can become misaligned in the next electoral cycle $e + 1$. Following Cellini, Ferreira and Rothstein (2010), I recast the dataset such that the unit of observation is every close election (i.e. municipality-electoral cycle pair)[16] and for each unit I track the evolution of the outcomes of interest three years before and four years after the close election took place.[17]

Different to a standard event study, I control by the running variable linearly allowing that the function varies flexible on either side of the cut-off and also for each year k. Also I limit the inference to the subsample of close elections, which make of this estimation a dynamic regression discontinuity design. The dynamic specification I estimate is the following:

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[15]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 effect while holding the bandwidth would limit inference to municipalities with more than one close election and that change of political status (aligned not aligned) over time, resulting in small sample biases. The dynamic specification that I propose next, include municipality -election fixed effects after recasting the data to provide the variance needed.

[16]More formally, if a municipality has two close elections with different treatments, I duplicate all its observations and treat each copy (municipality-electoral year) as a separate unit that gets treated only once.

[17]Since I limit my inference to the duration of the mayoral term limit (3 years after the each close election), I can interpret my estimates as the effect of treatment on the treated.

(3)

$$y_{m,e,k} = \alpha + \sum_{j=-3, j \neq -1}^{3} \beta^j \, 1(k=j) \times aligned_{m,e}$$

$$+ \sum_{j=-3, j \neq -1}^{3} 1(k=j) \Big[\theta^j MV_{m,e} \times aligned_{m,e} + \gamma^j \, MV_{m,e} \times (1 - aligned_{m,e}) \Big]$$

$$+ \delta_{m,e} + \psi_{e,k} + \epsilon_{m,e,k} \quad \forall \ VM_{m,e} \in (-h, h)$$

whereas before $y_{m,e,k}$ is the outcome y measure k years, now before and after the close election m, e took place. Correspondingly, $\delta_{m,e}$ are municipality-election fixed effects to control for unobserved heterogeneity, while $\psi_{k,e}$ are electoral cycle-time event fixed effects controlling for the political business cycle, cohort effects and any macroeconomic shock.[18] Similarly that the main specification I estimate equation (3) using a local linear regression of the running variable with triangular kernel weights and a 5 percentage point bandwidth. Standard errors are clustered at the municipality level to account for the serial correlation of political alignment over time (Bertrand, Duflo and Mullainathan, 2004).

The identification assumption of this specification is that among the subset of close elections that took place during the electoral year e, the potential outcomes for the municipalities who elected an aligned candidate and for the municipalities who elected a non-aligned candidate are continuous at the cut-off.[19] Although one can not test this assumption directly, it is possible to perform indirect tests to ensure their plausibility. The following section presents the results of such examination.

4. Validity of the research design

This section evaluates the internal validity of the identification strategy, which hinges on the fact that any other variable that affect my outcomes of interest change smoothly

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[18]The set of fixed effects defined by $\psi_{k,e}$ subsume any year fixed effect

[19]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 that these trends to be systematically different as long as they are continuous at the cut-off.

along the threshold. I perform two indirect test for that purpose, I evaluate whether there are discontinuities along the cut-off on either the density of the running variable or baseline characteristics. Also I provide a raw look at the spatial clustering of the data.

A. Manipulation of running variable

The fact that political alignment brings benefits to local governments is a sufficient reason to think that local governments may select themselves into being politically aligned. I test for this by evaluating if the density of the vote margin changes abruptly at the cut-off. Figure 6 shows the histogram of the vote margin for the elections where the governor's party compete. Overall we can conclude that there is no evidence of sorting of municipalities on either side of the alignment threshold. This strong symmetry in the result of close elections can be appreciated by looking at the raw data. In particular, I find that 467 out of 1867 elections were decided by a margin of less than five percentage points, among those, 241 were won by the opposition and 226 by the governors party.[20]

I implement the McCrary (2008) test to formally evaluate if there are discontinuities in the vote margin at the threshold. In particular, 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, suggests a very precise 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.[21]

B. Discontinuity of predetermined characteristics

Another indirect test to the identification assumption is to observe if baseline characteristics jump discontinuously at the alignment threshold. This would suggest that municipalities where the aligned candidate barely won are different from municipalities

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

[21]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).

where she barely lost. To perform this test I estimate the causal effect of political align-
ment on predetermined characteristic, measured before the election takes place. I use
a variant of equation (2) and estimate the effect political alignment using a local linear
regression that controls linearly for the running variable on either side of the cut-off, sim-
ilar than before I use a triangular kernel and pool the data of the municipalities with a
vote margin of less than five percentage points. Notice I use the sample of close elec-
tions that took place during the study period (1998-2003), since the outcomes of interest
are measured well before these elections took place, I should no find discontinuities in
political alignment.

Figure 8 shows the results of this continuity test on several economic, socio-demographic,
geographic, and political characteristics measured in 1990 or the pre-election period.[22] I
standardize all non-binary variables and present estimates in terms of standard devia-
tion 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. All the confidence
intervals cross zero except in the case of 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 either in the presence of spatial spillover effects or heteroge-
neous 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 all over the place. All 32 states have had close election won by either
governor's or main opposition party.

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[22]All except the political characteristics are measured from the 1990s Population and 1989 Economic Cen-
suses. The political characteristics are measured from the previous electoral period.

5.   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 government. This section describes each source of information in detail, 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. This data is collected by the Mexican National Institute of Statistics and Geography (INEGI). The revenues side provides information about distinct categories, from intergovernmental transfers to local property taxes, to fees for services provided by local governments. On average, 82% of local revenues come from intergovernmental transfers (mostly Ramo 33 and Ramo28) . In comparison, about 10% come from taxes (mostly property taxes) and other public services provided by local governments. I use revenues data to estimate which source of revenue increase because of political alignment. Also, I can observe different components of spending data. The two most important because of their average share in total spending are the wage bill (40%) and public investment (30%).

ELECTORAL RESULTS. — Electoral data come 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.[23] 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 the politicians deciding their political party affiliation after knowing

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

the electoral results. This ameliorates any concern related to politicians manipulating the running variable.

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. The definition of formal workers here is all those workers who contribute to the social security system and therefore received health insurance and pension contributions.

In addition to employment counts, the IMSS data allows the measure 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 that by using the total wage bill of local governments from the public finance data.

ALTERNATIVE EMPLOYMENT DATA. — The main drawback of 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-agricultural establishments, both formal and informal. I use this dataset to measure employment and wage bill growth between the 1998 and 2003 round of the economic census. I follow Asher and Novosad (2017) and assign to each intercensal growth rate the result of the earliest election that took place in between the two rounds of the economic census.

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. This implies less precise

estimates[24] and downward bias in our estimates.[25]

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 it collects a wide variety of socio-demographic and labor market information for both formal and informal workers. I used 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. In particular, it only provides information for 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 limits the ability to detect small effects.[26]

ECONOMIC ACTIVITY. — To measure aggregate economic activity, I use night lights luminosity and electricity consumption, both 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 the Mexican territory. This measure goes 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

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[24]The lower precision in the estimates surge from the fact that the effect of alignment is estimated across units with different exposure lengths and base years. For example, a municipality that elected its mayor in 1998 (2000) will have 5 (3) years exposure by 2003; if treatment effects vary with time of exposure, this will increase the dispersion of the estimates. Similarly, for municipalities where the earliest election occurred in 1998 will have as base year an electoral year, while a municipality where the earliest election occurred in 2000 will use as base year a non-electoral year. The fact that electoral years are different from non-electoral years implies that one should expect more imprecise estimates when using different base years across specifications.

[25]The downward bias comes from the fact that treatment changes in between the two census rounds, for example: a municipality that is politically aligned right after 1998 may become non politically aligned before 2003,, creating a downward bias on the effect of being politically aligned the entire period in between the two census rounds.

[26]I compute the correlation between the growth rate of formal employment captured by IMSS and that captured by the ENEU between 1998-2003; I find 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) finds similar results.

63. To circumvent this problem, I measure total municipal luminosity growth considering only those pixels that by 2003 were below 63. This is equivalent to imputing the neighboring non-censored pixels' growth rate to those already censored.

The second source of data is the aggregate consumption of electricity. This data 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 from the government.

OTHER DATASETS. — I also use other datasets to explore some potential mechanisms and perform balance tests over the data: i) The rollout of the number of beneficiaries from Seguro Popular, ii) Monthly payments and beneficiaries from PROGRESA, iii) Official audits data from the main watchdog agency (Auditoría Superior de la Federación).

B. Sample

The final dataset provides variation at the municipal year level for the sample of elections between 1998 and 2003. Since the mayor have a three year term limit 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. This study period has three advantages: First, it allows to estimate the effects of political alignment during the expansion and weak oversight of intergovernmental transfers. Second, it ends one year before the sudden and steep increase in violence experienced by Mexico after 2006, and that has been argued resulted from political alignment with the president's political party.

I exclude municipalities from the state of Oaxaca because most of them do not choose their mayor through elections but using a traditional governance structure, which makes it infeasible to construct the running variable. Also, I limit the observations to those municipalities for which there is information available on private employment for the study period 1998-2006. The final sample considers 1097 out of 2446 municipalities, which employ 99 % of the formal employment and host 80% of the Mexican population. This filter

implies that the estimates are only representative for middle and large municipalities, which is more relevant from a macroeconomic point of view.[27]

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 labor market outcomes for the universe of private-sector workers. Third, it explores whether the influence of political alignment on formal employment is consequential for the aggregate levels of employment and different measures of total economic activity. Unless otherwise is indicated, I use equations (2) and equation (3) to obtain all the results presented in this section.

A.   Public Revenues

TRANSFERS. — The first-order question is if 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 test 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 rather than having a higher probability of receiving transfers. In particular, columns 1 to 4 show that political alignment increased the growth rate of earmarked transfers between 29 to 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. The point estimates of column 2 indicate that the growth rate of earmarked transfers was 42 percentage points higher in aligned municipalities than their non-aligned counterparts. To put this into context, the growth rate of earmarked transfer for the non-aligned

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[27]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.

municipalities used in that specification is 138 percent. Therefore we can infer that the growth rate of aligned municipalities increases by one-third (0.3= 42/138) because of political alignment. These relatively large growth rates are explained by the change induced by the creation of Ramo-33 transfers in 1998. Before Ramo-33 several municipalities received almost cero earmarked transfers, and after the reform, they started to received positive values that led to high growth rates. Columns 5 to 8 of Table 1 shows that political alignment does not consistently increase the likelihood of receiving earmarked transfers during the years of the mayor's term. The no-effect could be explained by the fact that almost all aligned municipalities report receiving transfers during the post-election period.

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 on the observed pre-election growth rates at the alignment threshold validates the identification assumption. This test is analog 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.[28] 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 fewer borrowing needs.[29]

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[28]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

[29]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

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, I present again in column 1 the effect of political alignment on the growth rate of earmarked transfers. Column 2 shows the impact on the revenue sharing transfers, which are the main revenue source, represented about 53% of local revenues, and are also administrated by state governors. They are such a large fraction of total revenues implies that even a small jump of this revenue source at the alignment threshold can lead to economically significant differences in total public spending. I do not find robust evidence of revenue transfers changing at the alignment threshold; results are statistically significant with an 11 percentage point bandwidth but disappear when using a 5 percentage point bandwidth. Still, I do not rule out that part of this non statistically significant, but economically significant coefficients, has a role in increasing overall spending.

Column 3 of Table 2 reports the effect of alignment on taxes. According to the table, taxes 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 statistically significant. Moreover, the point estimates of both Panel A and B suggest that the alignment effect on taxes would be positive, if anything. 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 of the effect of political alignment are positive but neither statistically significant nor consistent across different bandwidths. To sum up, alignment-induced transfers do not seem to affect local governments' decisions regarding taxes and debt strongly. This evidence goes in line with the idea of a flypaper effect; grants increase total spending and has also been validated by Bracco et al. (2015) using data from Italy and a similar research design.

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 align-

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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.

ment. I will go back to this in the robustness section, where I explore the effect of political alignment in allocating other nationwide programs unrelated to intergovernmental transfers.

LOCAL 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 show 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 once 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*

This subsection explores whether employment and wages evolve differently in politically aligned municipalities, which, as it was explained, receive a disproportional amount of resources. To do so, I use data on total jobs and aggregate wage bill for the universe of formal sector jobs recorded by the Mexican Institute of Social Security. [30] I compute two outcome measures with this data that can be observed at 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 shows 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

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[30]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

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 different controls.

To provide an interpretation of the coefficient, I will 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 for non-aligned municipalities is between 7 to 9.1 percent, this 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 provides the same interpretation. The intersection of each slope with the alignment threshold, when the vote margin is equal to zero, could be interpret 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 where growing 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 misaligned municipalities (between 7 to 8 percent). For the sake of completeness, Figure 15 plots the growth rate of wages for both the post and pre-election period, and as expected, I do not observe discontinuities in wage growth either before or after alignment takes place.

7. Conclusions

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

I find that political alignment with state governors increases public spending 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. This implies that the growth of private

sector jobs in non-aligned municipalities is 10 percentage point less in politically aligned municipalities.

I do not find evidence that higher corruption, a larger public sector enlargement or a increase in 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 literature about the impact of political alignment on economic welfare has found positive findings for economies where politicians' policy levers are not related to higher spending but regulation (Asher and Novosad, 2017). In our context, the main policy lever is spending, and in line with the findings of Cohen, Coval and Malloy (2011), it seems that it crowds out the private economic sector.

These findings imply that politically induced public spending can have unintended consequences that may affect negatively welfare. Although the results 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 n 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 grow expand in order to remain informal.

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8. Tables

Table 1—Effect of Alignment on intergovernmental transfers

	Δ^3 Earmarked transfers				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

	Δ^3 Employment				Δ^3 Wage			
	(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—Effect of Alignment on employment from Economic census

	PBT		Labor earnings		Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
Political alignment	-0.002	-0.037	-0.002	-0.058	-0.026	-0.047
	(0.093)	(0.092)	(0.111)	(0.105)	(0.035)	(0.034)
State and Election-year FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Obs.	915	915	915	915	915	915
R^2	0.01	0.05	0.01	0.09	0.14	0.18

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 5—Effect of alignment on public sector employment

	Wage bill			Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	0.164*	0.105	0.072	-0.030	-0.023	-0.010
	(0.093)	(0.077)	(0.053)	(0.021)	(0.020)	(0.015)
State and Election year FE		✓	✓		✓	✓
Baseline controls		✓	✓		✓	✓
Obs	1313	1313	2639	1346	1346	2690
R^2	0.00	0.36	0.35	0.00	0.21	0.21
Bandwith	5	5	11	5	5	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 6—Effect of alignment on employment by booms and recessions

	(1)	(2)	(3)	(4)
			RDD	
	Bad Times (bdh=11)		Good Times (bdh=11)	
Poitical Alignment	-0.043	-0.038	-0.11***	-0.11***
	(0.05)	(0.04)	(0.03)	(0.03)
Mean dep. var	- 0.26	- 0.26	0.32	0.32
Observations	740	740	2080	2080
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 formal and informal employment from household surveys

	Employment		Formal		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)
Total employment	-0.0239**	-0.019***	-0.030***	-0.029**	0.006	0.003
	(0.008)	(0.006)	(0.011)	(0.011)	(0.007)	(0.012)
Mllns Obs.	1.9	4.3	1.9	4.3	1.9	4.3
Bandwidth	5	11	5	11	5	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 8—Effect of alignment on infrastructure spending

	(1)	(2)	(3)	(4)	(5)
			RDD		
	Global	**Local h= $+\backslash-$ 5pp**		**Local h= $+\backslash-$ 11pp**	
Poitical Alignment	0.088*	0.36***	0.373***	0.309**	0.202**
	(0.04)	(0.12)	(0.12)	(0.14)	(0.082)
$\beta\backslash E[Y_0]$	0.08	0.20	0.32	0.25	0.15
Observations	5228	1313	1313	2639	2639
R^2	0.31	0.33	0.81	0.33	0.77
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 infrastructure and household assets

	Δ 1990-2010 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.

9. Figures

Figure 1: The economic size of Ramo-33
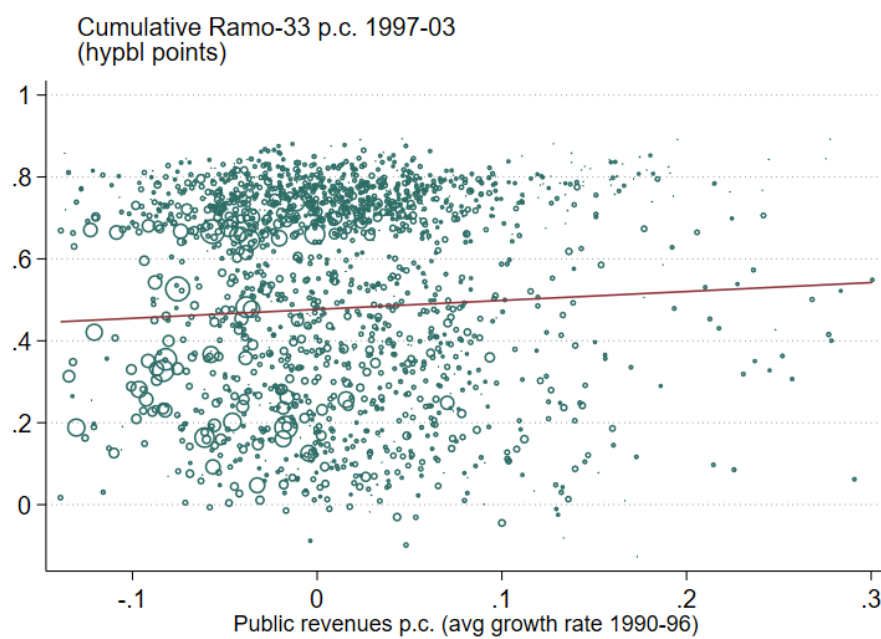
Cumulative Ramo-33 p.c. 1997-03
(hypbl points)

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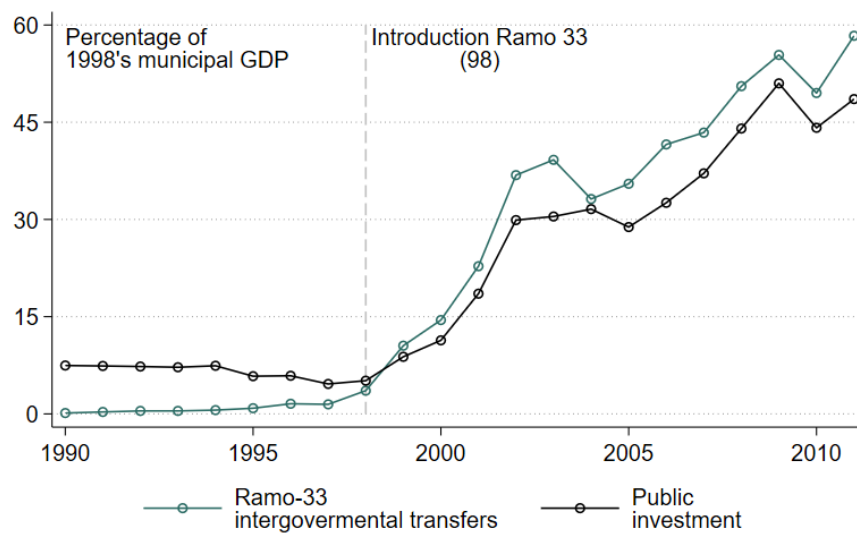
NOTE— The figure show the population weighted average of revenues as a share of local GDP in 1998. Ramo-33 for the year after 1998 and PRONASOL for years before 1998.

Figure 3: The size of Ramo-33 in the local public finances
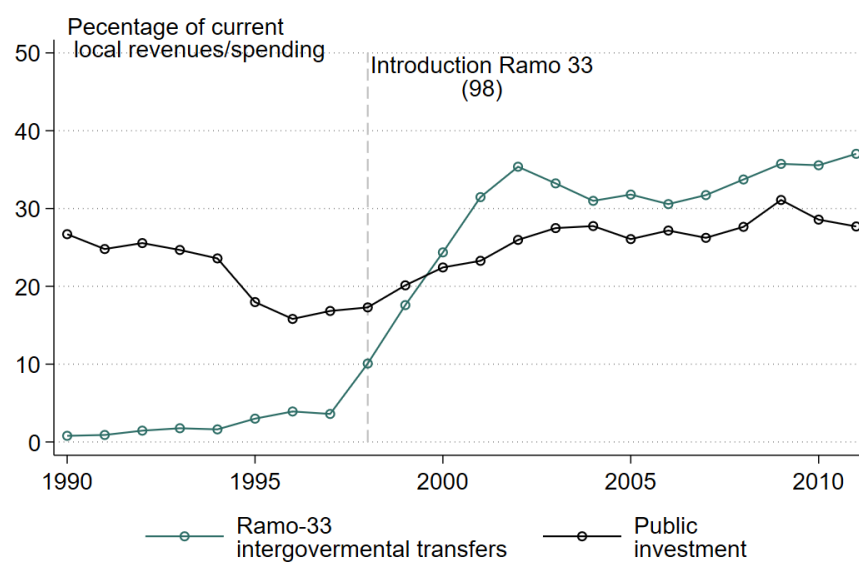
NOTE— The figure show the population weighted average of infrastructure investment as share of total spending or revenues. Ramo-33 for the year after 1998 and PRONASOL for years before 1998.

Figure 4: The non-compliance with Ramo-33 *de jure* allocation
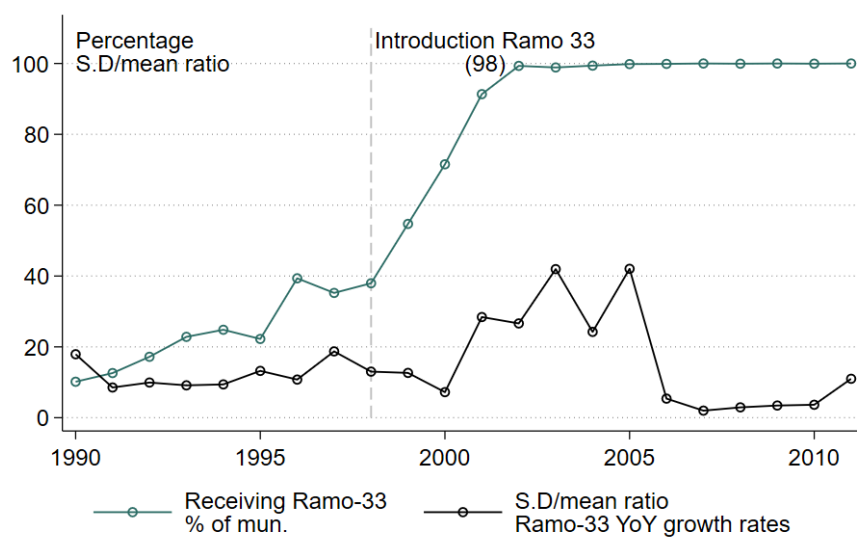
NOTE— The figure show the population weighted average or municipalities receiving transfers and the coeficient of variation (standard deviation / mean) of the distribution of yearly growth rates of Ramo-33 for the year after 1998 and PRONASOL for years before 1998.

Figure 5: Staggered elections and variation in political alignment

State and local
elections

1998   2001   2004   2007   2010   2013

Nuevo Leon State

Puebla State

3 yr
municipal   6 yr state   **PAN**
elections   elections   **PRI**
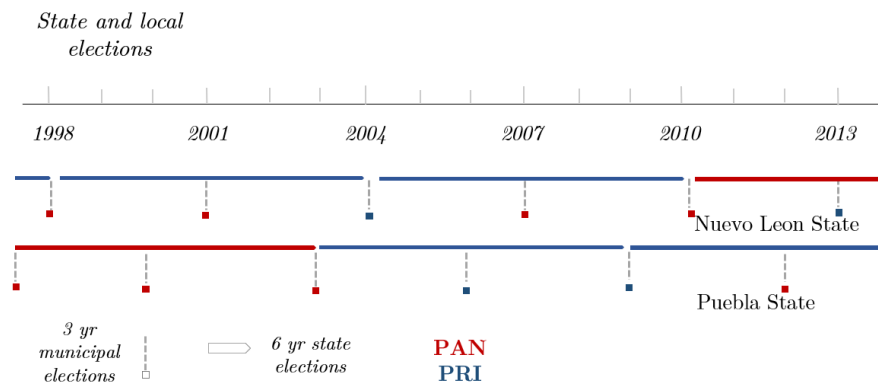
NOTE— This figure is an example of two states who have election cycles at different calendar years. The horizontal bar represent the governor's term limit (six years), while the space between squares is the mayoral term limit (three years). The vertical dashed represent election dates and the colors a different political party (PRI in blue and PAN in red). .

Figure 6: Distribution of mayoral elections along the vote margin $V_{m,e}$ 1998-2003
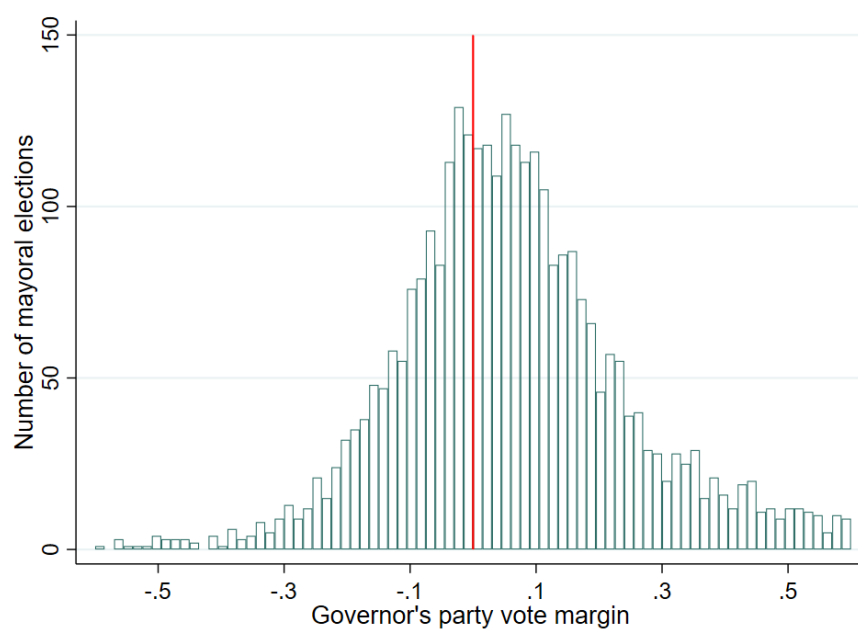
NOTE— This figure shows the histogram of the governor's party vote margin on the mayoral elections used in our estimates (1998-2003).

Figure 7: McCrary density estimates of the vote margin $V_{m,e}$ 1998-2004
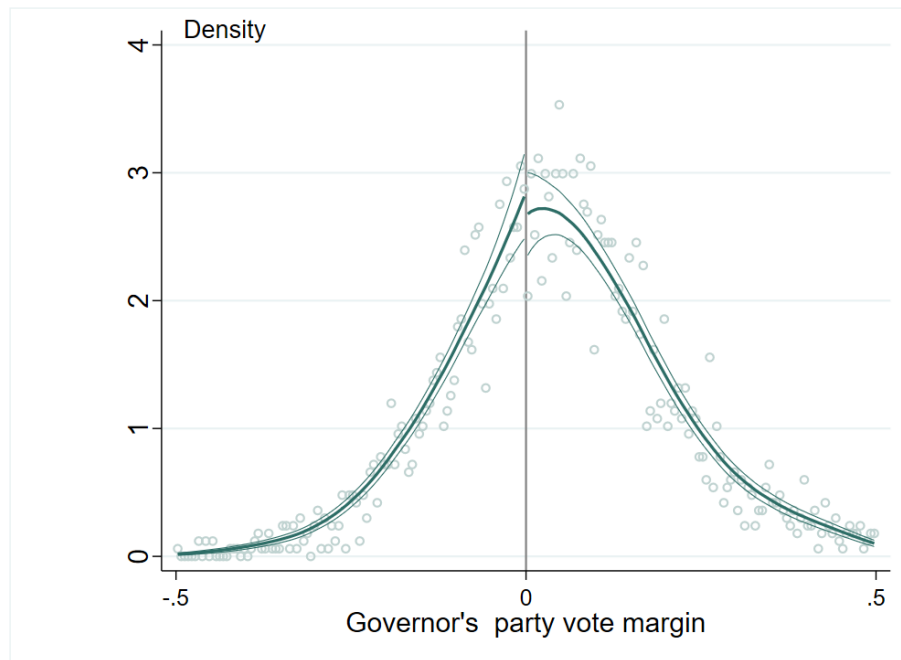
NOTE— This figure shows a estimate of the density of governor's party vote margin on mayoral elections used in our estimates (1998-2003). Each bubble groups all elections that took place in half percentage points spread bins. The dark lie is the point estimate of the density function and the light lines a 95% confidence interval.

Figure 8: Balance on predetermined (1990s) covariates
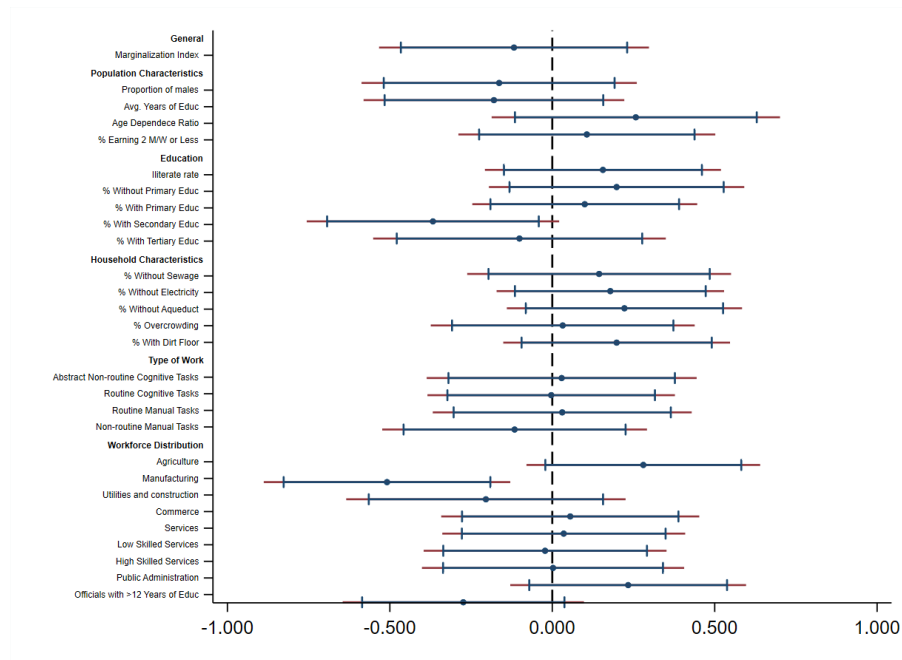
NOTE— The reported coefficients come from separated regressions that estimated the causal effect of political alignment on predetermined covariates using a variant of equation (2) that only controls linearly for the running variable on either side of the cut-off. When a municipality has more than one 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
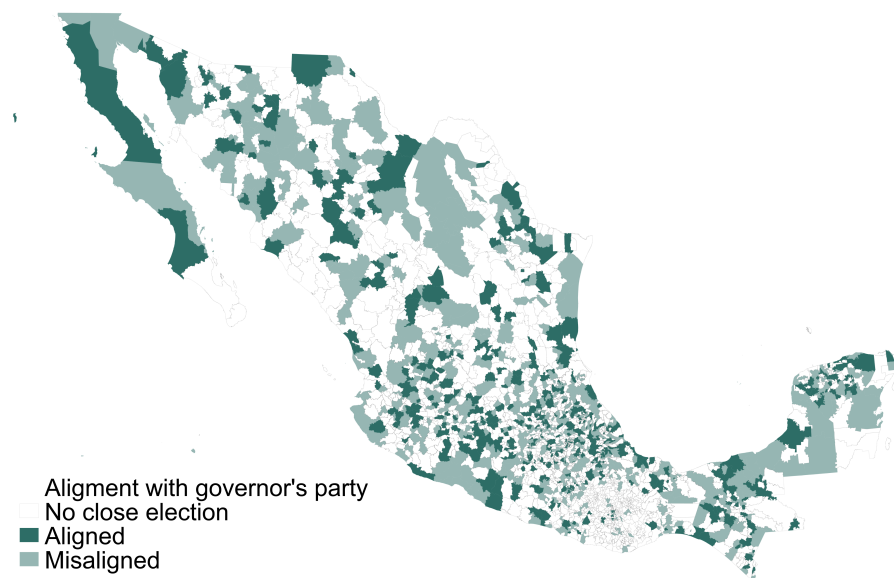
NOTE— The figure maps municipalities ruled by aligned and opposition parties for the sample used to obtain our main estimates (see section 4), where elections were decided by less than 5 percentage points.

Figure 10: Growth rate of earmarked transfers and Governor's vote margin
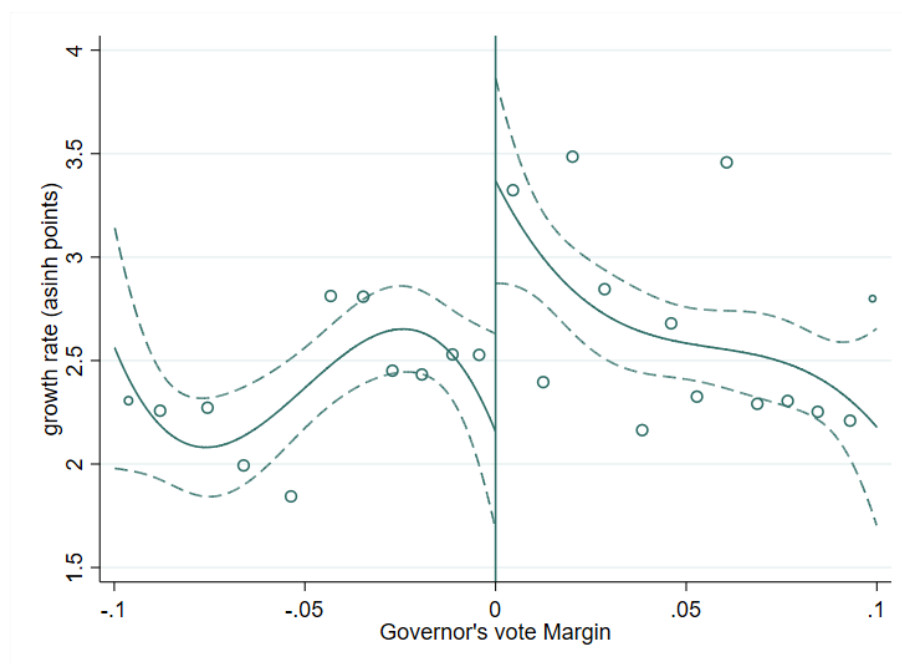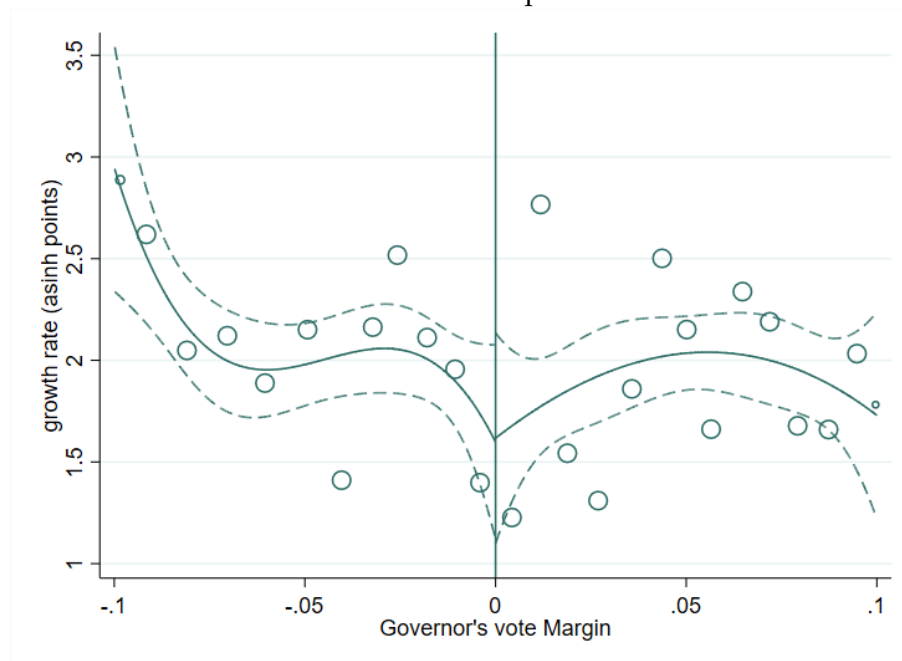
A. Post-election period

B. Pre-election period

NOTE— 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 11: Growth rate of total spending and Governor's vote margin
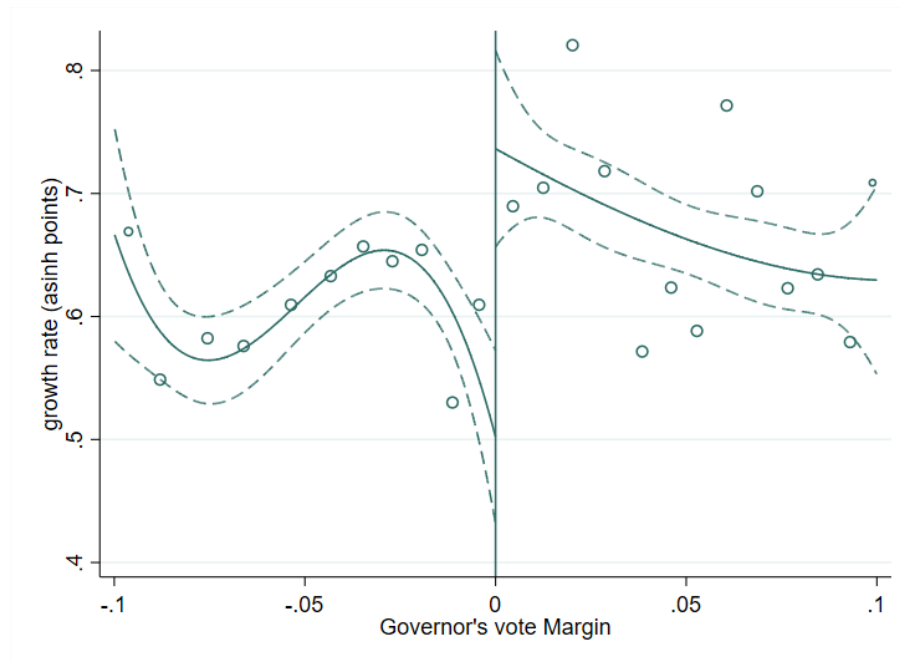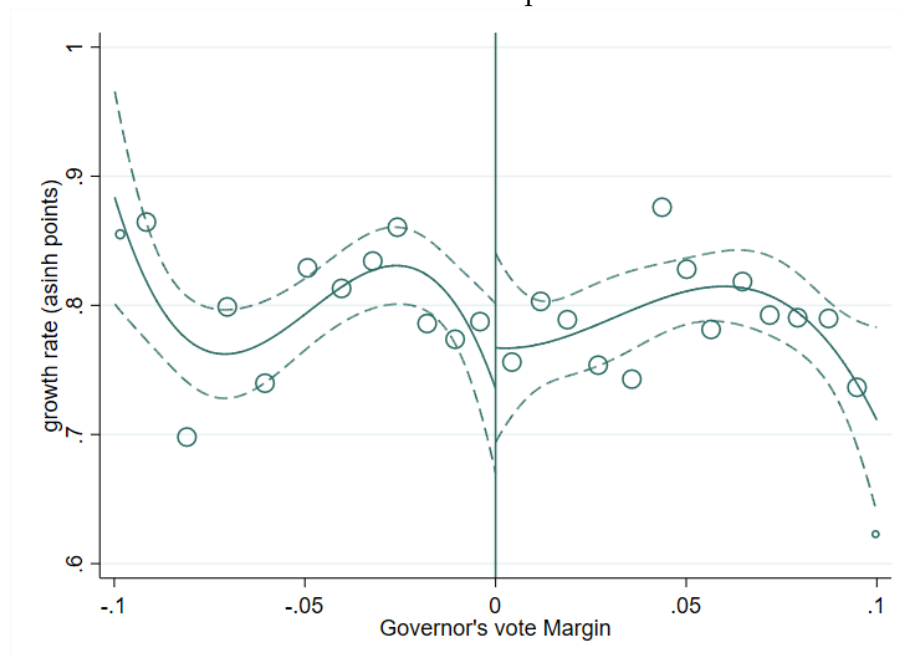
A. Post-election period

B. Pre-election period

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.

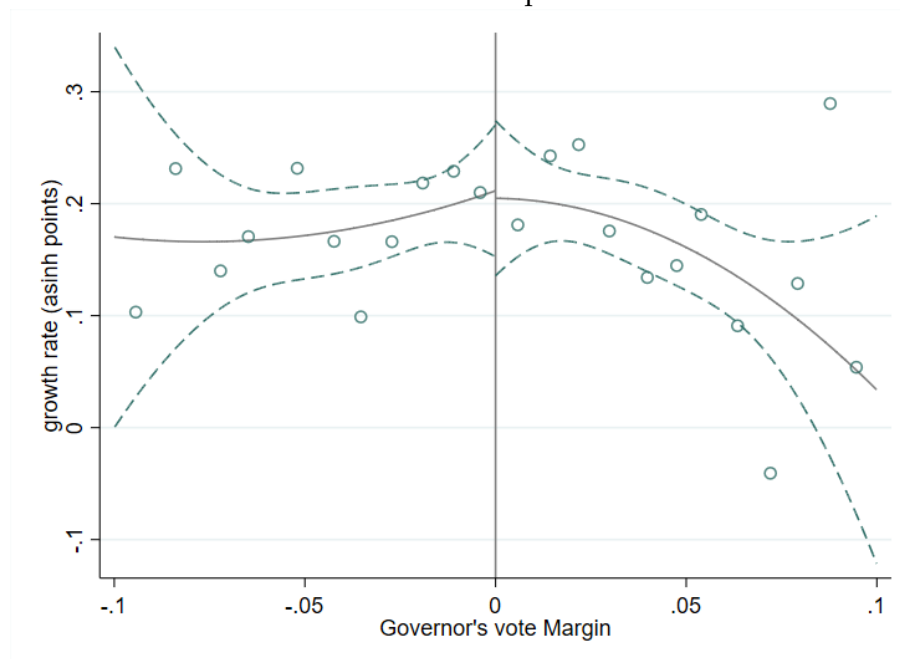Figure 12: Event study total spending after political alignment
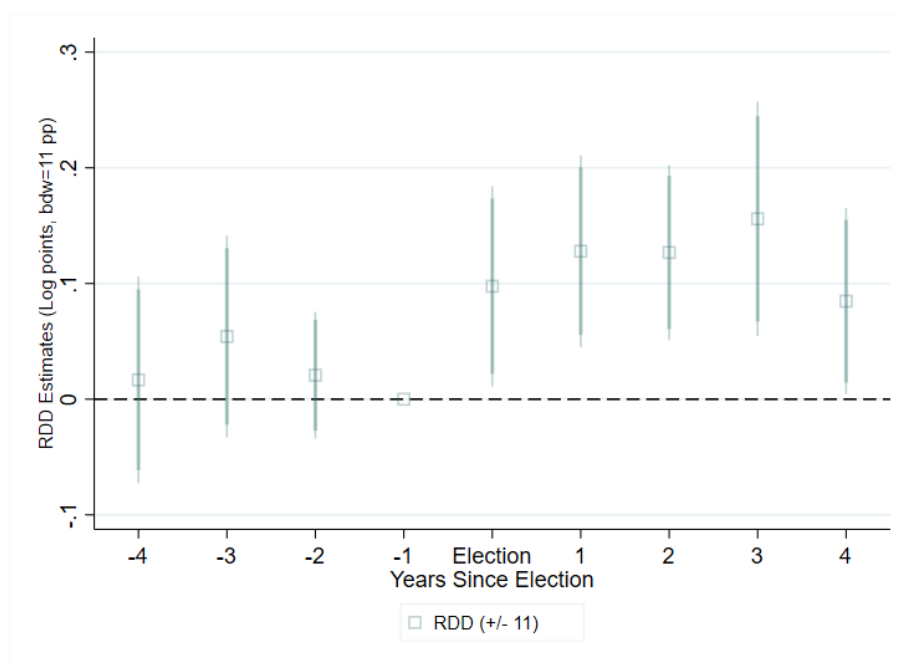
NOTE— This plot aggregate data into bins of half percentage points and estimate a third order polynomial regression between the running variable and the bins on each side of the cut-off.

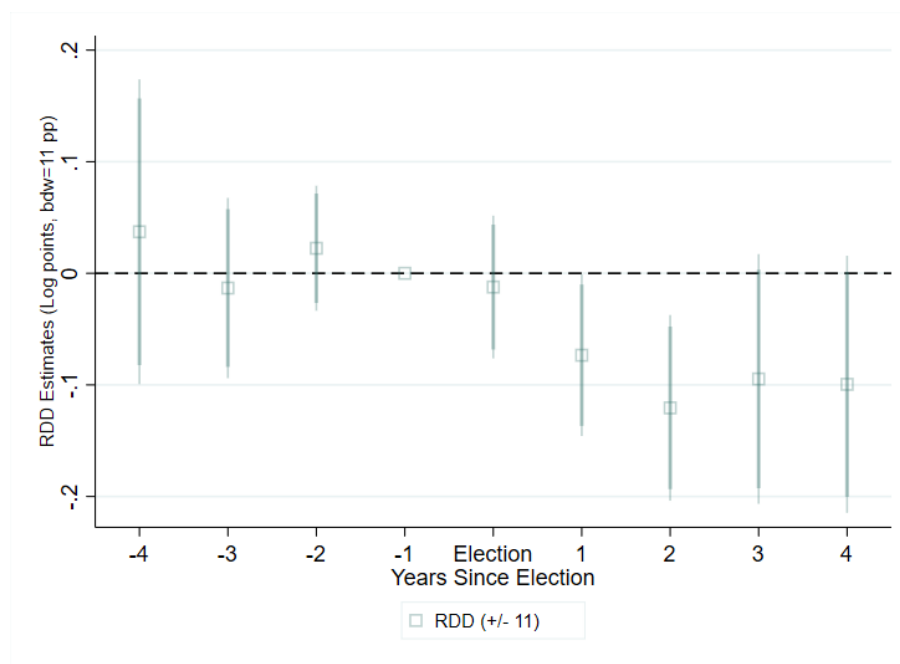Figure 13: Growth rate of formal employment and Governor's vote margin
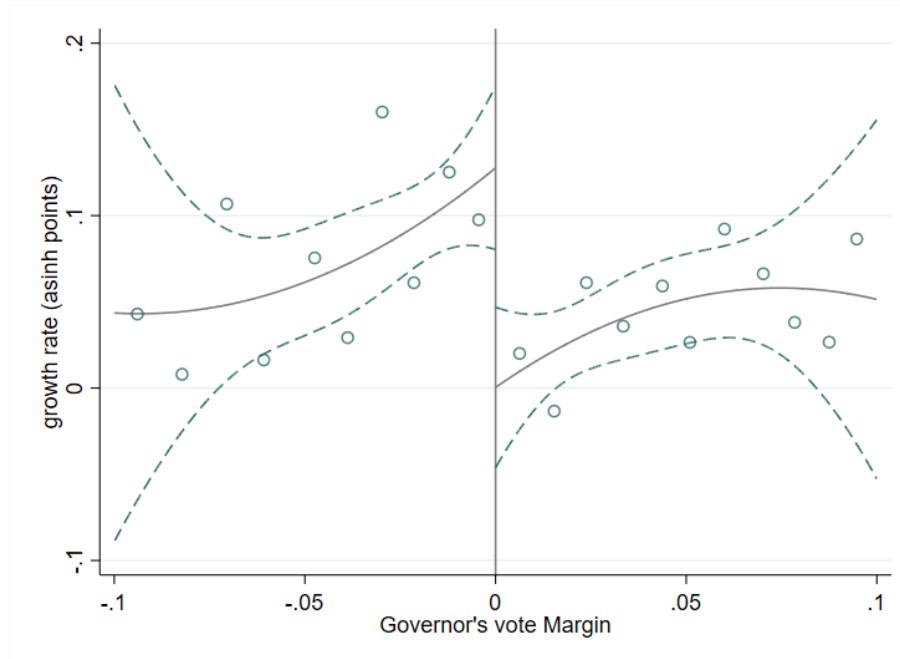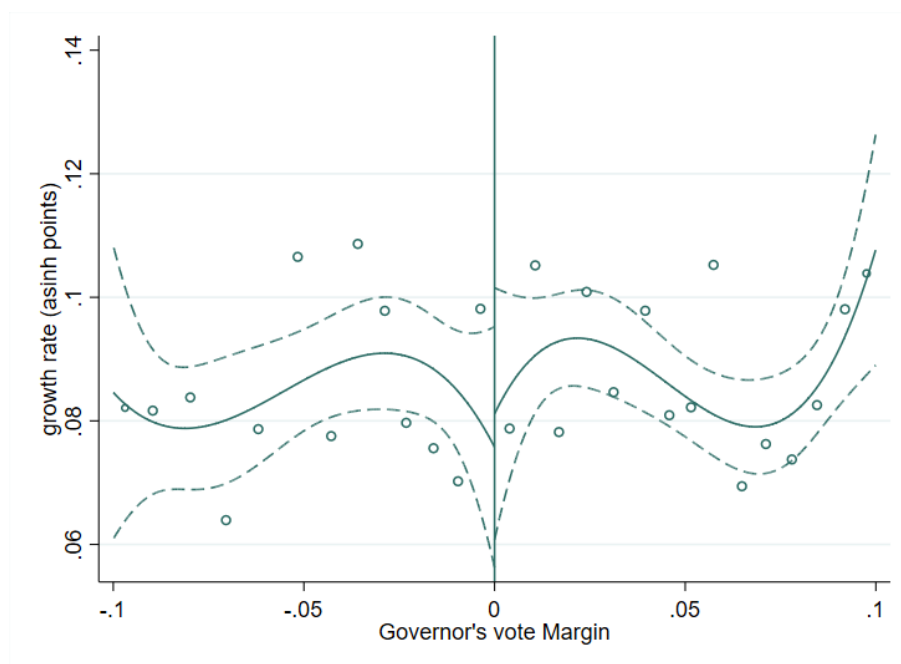

A. Post-election period

B. Pre-election period

NOTE— 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 14: Event study formal employment after political alignment
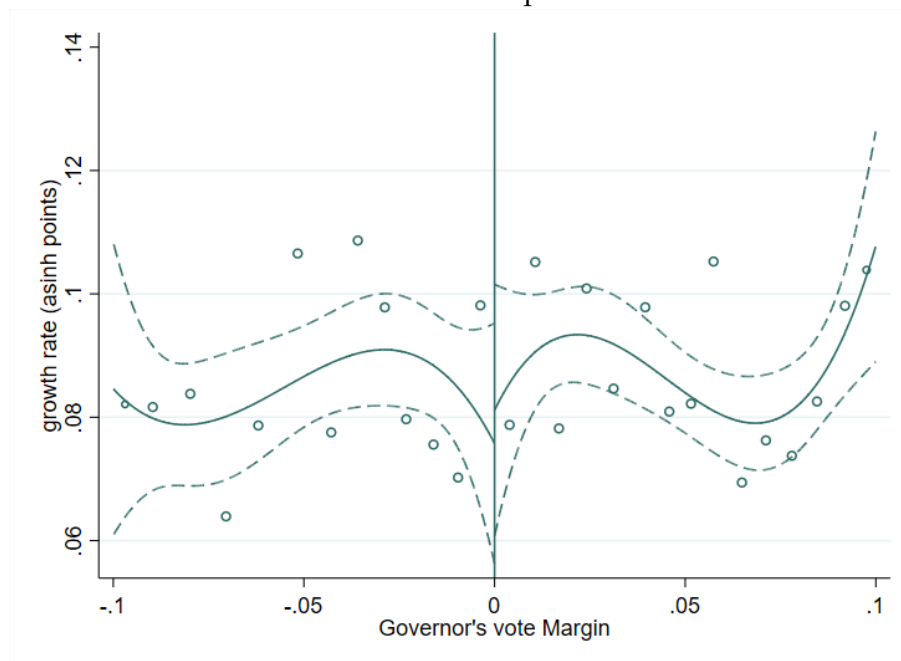

NOTE— This plot aggregate data into bins of half percentage points and estimate a third order polynomial regression between the running variable and the bins on each side of the cut-off.

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

A. Post-election period

B. Pre-election period

NOTE— 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.