

Fiscal Windfalls and Misallocation: Evidence from Mexico

By DANIEL VALDERRAMA*

GEORGETOWN UNIVERSITY

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Abstract

A law that creates a new intergovernmental transfer to fund local public infrastructure caused a large and unexpected change in local public spending in Mexico. I combine this natural experiment with a close-elections discontinuity design to estimate the effect of government spending on the private-formal employment at the municipal level. I show spending shocks appear to significantly deter the growth of private-formal employment. An increase of 10 percentage points in the growth rate of local spending leads to a decline of 9 percentage points in the growth rate of formal private jobs. This effect cannot be explained by public sector enlargement. The fact that overall economic activity, as measured by night lights, does not react to the spending shocks may suggest that public spending has unintended consequences in the reallocation of employment between the formal and the informal sector.

JEL:R10, G20, H10

1. Introduction

Violence related to the drug trade has escalated dramatically in Mexico since 2007, claiming over 60,000 lives and raising concerns about the capacity of the state to monopolize violence. Recent years have also witnessed large-scale efforts to combat drug trafficking, spearheaded by Mexico's conservative National Action Party (PAN). These efforts have cost around 9 billion USD per annum, nearly as much as the government expends on social development.¹ Yet there is limited causal evidence about the impacts of crackdowns. This study uses plausibly exogenous variation from close Mexican mayoral elections, a network model of drug trafficking, and confidential data on the drug trade to identify how crackdowns have affected violence and trafficking. It examines both the direct effects of crackdowns in the places experiencing them and the spillover effects they exert by diverting drug traffic elsewhere.

Mexico is the largest supplier to the U.S. illicit drug market, with Mexican traffickers earning approximately 25 billion USD each year in wholesale U.S. drug markets (U.N. World Drug Report, 2011). Official data described later in this study document that in 2008, drug trafficking organizations maintained operations in two thirds of Mexico's municipalities, and illicit drugs were cultivated in 14% of municipalities.

While Mexico is a major player in the drug trade, its high levels of drug violence and drug enforcement expenditures are not unique. Global annual drug enforcement spending exceeds 100 billion USD, and traffickers in Central America, West Africa, and elsewhere use violent tactics and often belong to the same transnational trafficking organizations that operate in Mexico (Economics Briefing, 2013). Because law enforcement does not randomly decide where to crack down, the existing evidence on drug enforcement impacts consists primarily of correlations. While often the best evidence available, these can be non-trivial to interpret. For example, a positive cross-sectional correlation between violence and drug enforcement could result because areas with higher violence attribute it to drug consumption and thus expend more fighting the drug trade, and a positive correlation in a panel could occur because governments crack down in places where they expect violence to later increase.

This study isolates plausibly exogenous variation in drug enforcement policy by exploit-

ing the outcomes of 2007-2010 close mayoral elections involving the PAN party.² The PAN federal government's role in spearheading the war on drug trafficking, as well as qualitative evidence that PAN mayors have contributed to these efforts, motivate this empirical strategy. While municipalities where PAN candidates win and lose by wide margins are likely to be

Government transfers are a standard policy tool to boost development in lagging economic areas. The efficacy of these transfers in promoting long-term economic growth depends on how they complement private-sector economic activity. The estimation of these policies' impact is always problematic because their redistributive and countercyclical nature means that higher spending is directed to places in a downward economic trend or recently suffered from an economic shock. Therefore any naive estimate is biased towards zero when the confounder is related to the downward trend, or upward biased in case the confounder is related to mean reversion.

This paper circumvents this issue by exploiting the creation of sizeable and unexpected intergovernmental transfer from the federal government to the municipalities in Mexico. This transfer led to a two-fold increase in average municipal public spending between 1998 to 2003.

The amounts that each municipality had to receive were defined by a precise formula. Once calculated, the transfers were made to state governors and they were in charge of distributing them to the municipalities. We use this policy characteristic to answer two questions: Does political alignment between the state governors and the municipal mayors affect the spatial distribution of the recently enacted intergovernmental transfer? If it does, what is the effect of these politically induced intergovernmental transfers on private economic activity?

To overcome the challenge that the allocation of the transfer responds to local characteristics, we use the fact that the transfers' enactment delegated the allocation role to state governors. We exploit this feature and use variation in partisan alignment between the state governor and municipal mayor as a source of exogenous variation in transfers received from the federal government. However, partisan alignment is an endogenous decision; voters decide who to vote for considering the effect of alignment on their welfare.

To tease out the endogeneity of alignment, we limit the analysis to municipalities with close elections. The identification assumption is that in close elections the result of being aligned or not is unrelated to previous trends or characteristics of the local economies. We use variation from 467 close elections that took place between 1998 and 2003, out of which 226 elected a mayor from the same party as the state governor’s party, while the other 241 elections elected a mayor from the other political parties.

We find that intergovernmental transfers’ growth rate is 46 percentage points larger in aligned municipalities than in their non-aligned counterparts. The positive effects of receiving more transfers is not offset by reductions in other revenue sources or public programs in which the states intervene. The lack of crowding out implies that the growth rate of local public spending increases as a result of political alignment. The average growth rate of aligned municipalities increases 10 percentage points compared to their non-aligned counterparts during the mayor’s term. Moreover, our estimates find that partisan alignment translated into local and state incumbency advantage. This rationalizes this tactical allocation of public resources within a framework where politicians seek electoral returns from the intergovernmental transfers they allocate. The probability of an aligned mayor to win the next increases 17 percentage points compared to the counterfactual of not being aligned.

This increase in spending is economically meaningful when measured over the municipal economy’s total production; we see that political alignment increases public spending as a fraction of local gross production by 2 percentage points.¹ The size of these spending shocks makes it relevant to ask about their impacts on the private sector economy.

We find that municipalities where the mayor belongs to the governor’s party experience lower private sector employment growth. The growth rate of employment in municipalities aligned with the state governor declines by 10 percentage points compared to the value of the non-aligned municipalities. The increase of public spending is free to the recipient’s perspective, i.e. the local citizens are not expecting to receive increases in their taxes or interest rates as response to the fiscal shock. This implies that the traditional channels

¹There is no yearly information on gross production at the municipality level. However, the Economic census allows us to measure every five years. We take local gross production from the 1999 Economic Census.

that ameliorate the impact of the fiscal policy, such as taxes and interest rates, are not playing a role in offsetting the response of private-sector economic activity.

It is important to mention that our measure of private sector economy is based on administrative records from the Mexican Institute of Social Security (IMSS). This implies that our results can only speak about the formal private employment and are mute regarding what happens in the informal sector.²

We examine three potential mechanisms that could explain the slowdown in the formal private sector: public sector enlargement, reallocation to informal activities, and increase in malfeasance as a result of the fiscal resource abundance.

We do not find robust evidence of public sector enlargement happening disproportionately in aligned municipalities. If anything, public employment starts to increase during the last year of the mayor's term, which does not explain the dynamics we find in the effect of alignment in private sector employment, where the decline starts in the first year. Neither do we observe that aligned municipalities are more likely to be accused from malfeasance or corruption.

This leaves two potential mechanisms that may explain why these fiscal windfalls do not translate into higher employment in the private sector: i) unexpected government transfers increase consumption and leisure and reduce labor supply in the economy, which leads to firm retrenchment ii) These government transfers increase demand in economic activities which are intensive in informal labor such as construction and retail. Therefore, there is a partial reallocation of jobs from the formal to the informal sector (Cohen, Coval and Malloy, 2011).

We find that the growth rate of spending in public infrastructure increases 25 percentage points more in aligned municipalities. Since construction is a sector intensive in informal labor, it may be that an increase in the informal sector compensates for the reduction of private-formal employment.³ To test this hypothesis further, we use night lights to proxy for overall economic activity. Suppose the decline in private formal activities follows from

²We use other strategies to capture what is happening in total economic such as night light measures, household surveys, and establishment information from the economic census. However, they did not offer reliable estimates. The formal economy explains about 70% of total production, although only 40% of the workers.

³In a further test of the results, we estimate the effect of the probability of working in an informal sector using the quarterly labor force survey (ENOE), results are still under review.

a reallocation of workers, with milder effects on workers' productivity. In that case, we should not observe a decline in our proxies for economic activity. Our estimates can not reject a null hypothesis of the null effect of alignment on economic activity proxies, although the estimates are noisy.⁴ Moreover, we see a positive, albeit non statistically significant, increase in the number of establishments using data from the Economic Census.

Related literature. This paper contributes to three strands of literature. First, the literature of fiscal federalism in Mexico. Early studies from Diaz-Cayeros (2004), Langston (2005), Hernandez-Trillo, and Jarillo-Rabling (2008), among others, did extensive research about the political discretion in the allocation of intergovernmental transfers from Ramo 33. This study contributes to this literature by providing causal evidence, as the previous studies focused more on statistical correlations. We provide evidence on how this politically induced transfer affected total local spending. We also contribute to the general literature of partisan favoritism and intergovernmental transfers (Brollo and Nannicini (2012), Curto-Grau, Solé-Ollé and Sorribas-Navarro (2018) and Bracco et al. (2015), and Fiva and Halse (2016)) by documenting how favoritism can be curbed after intense regulation over the allocation of the resources took place.

Second, we contribute to the literature on the economic consequences of political favoritism. This literature is inconclusive. Cohen, Coval and Malloy (2011), Asher and Novosad (2017) and Hodler and Raschky (2014) are papers that provided quasi-experimental evidence but find opposite results. While Cohen, Coval and Malloy (2011) find that political alignment harms private employment and does not affect economic growth in the U.S, Asher and Novosad (2017) and Hodler and Raschky (2014) find sizeable positive impacts for India and a large subset of African countries, respectively. We find negative impacts in line with what Cohen, Coval and Malloy (2011) found. One potential reason for this is that we focus on the same policy lever, namely, spending.

The third strand that this paper contributes to is the literature on the resource curse of fiscal windfalls. It documents that an excess of fiscal resources can lead to more rent-seeking and lower quality of politicians (Ganong and Shoag (2017), Brollo et al. (2013)). This paper shows another way through which these transfers can also affect the local

⁴Other tests with the Economic census are not able to reject the null hypothesis of different growth rates in the overall economic activity, albeit the estimated impacts are noisy

economy, namely formal-sector retrenchment. This formal sector downsizing not only may offset part of the benefits from the fiscal windfalls but also may potentially lead to factor misallocation and lower tax collection due to a lower formality rate.

2. Institutional Context

Mexico follows a revenue-sharing system where the federal government collects most of the taxes and redistributes them to lower levels of government—states and municipalities. This system is characterized by high vertical imbalances with low local tax collection and high dependence on intergovernmental transfers to fund sub-national public spending. We focus on the allocation of intergovernmental transfers to municipalities during a period in which they expanded dramatically and where the control over its distribution was done by state governments.

After the Tequila crises of 1994 the recently elected federal government started a process of decentralization. Intergovernmental transfer to municipalities increased by xx fold as a result of this decentralization. This increase local public spending by xx percent in real terms. The increase in average local public spending was not result of simple change in the label (federal vs local) of who take the spending decisions for three reasons: The bulk of increase in spending happened years after the enactment of transfer took place. National public spending also increase during this period by XX. Average local spending increase also as a result or larger reallocation of spending towards places with virtually nonexistent public resources.⁵ compare DDD for two reasons: First, public spending increased strongly during this period and second, the new intergovernmental transfer were not correlated with previous allocation patterns, new allocation followed more redistribute purposes.

This new earmarked transfers change the allocation of public spending and also happened in a context of large economic growth of public revenues.⁶ About half of these transfers intended to act as a placed based policy distributed to distress areas with the objective of develop infrastructure projects. The other half was distributed based on pop-

⁵While before 1997 xx municipalities reported cero intergovernmental transfers

⁶In December of 1997, as part of the decentralization process, the congress created Ramo 33, a intergovernmental transfer fund composed of eight different spending subcomponents. We focus on two of them: FISM and FORTAMUN-DF, which are earmarked intergovernmental transfers to fund infrastructure projects and promote regional development. In contrast to other subcomponents, these ones were not tied to any wage bill and therefore were easier to reallocate across constituencies.

ulation criteria. It is worth to mention that that the re-distributive transfer were part of the anti-poverty policies implemented at the end of th 90's by Mexico are were equally important than PROGRESA in terms of budget.

To protect the allocation of intergovernmental transfer from presidential decision the Congress designed both a formula and This period of increase in intergovernmental transfers During the period studied Mexico experienced a regime change from a one party rule to a competitive multi-party system. Since the hegemonic party was in control of the federal goverment the As product of this transition the law that enacted the distribution of new intergovernmental

These two intergovernmental were allocated based on formulas defined by law. FOR-TAMUN is allocated on the basis of population, and FISM is distributed according to a formula that uses a multidimensional deprivation index (see Appendix A). The formulas are not frequently updated because they require information collected through the population census.⁷ The law that enacted these federal transfers specified that they could not be directly allocated to the municipalities and assigned the final role of distributing them to the state governments. Although the federal government's allocation to the states uses the formula described by law (article 34 of the Fiscal coordination Law), the states are not obligated to use the same formula but an alternative one described by the article 35 of the FCL.⁸

This paper focuses on these funds (FISM and FORTAMUN-DF) because they were easier to divert by state politicians since they aimed to fund non-programmatic spending, contrary to the health and education components of Ramo 33, which were restricted by the pre-existent distribution of public hospitals and schools. Also, by not being tied to the wage bill or the previous structure of the government, they create a significant variation to the status quo allocation of public resources across the municipalities.⁹

Local revenues multiplied as a consequence of the creation of FORTAMUN-DF and FISM. In less than three years, between 1998 and 2000, this new revenue source went from accounting to nothing to making up 40% of local spending. The average local spending

⁷The population counts have not been used to update these formulas except for 2015.

⁸This formula gives more weight to populated municipalities than the original formula.

⁹This transfer were part of the strategy to reduce poverty that together with the well-know conditional cash transfer program (PROGRESA) used fund from the extinct Ramo 26.

grew six-fold across municipalities with a wide variation. This considerable variation could be due to differences in the needs of public infrastructure across municipalities or to political factors. This paper aims to measure the role of a particular political factor, partisan alignment, in explaining that variance.

There are three reasons why despite the existence of the formula, there may be some discretion in the assignment of the transfers: First, the fiscal watchdog agency that monitors these resources only started to implement audits in 2000 and to impose severe fines in 2004. Two, the mayors and voters were not fully aware of how the formula worked to make state governors accountable. Three, there was a disagreement about how the transfer should be distributed and some delay in implementing the census information from the side of the governors. These three factors lead to a significantly considerable variation in the allocation of FORTAMUN and FISM over time within a state. As Table 1 and 2 show, the effect of alignment on intergovernmental transfer from Ramo 33 is positive before 2004 and almost zero afterwards.

The creation of these transfers coincides with the loss of hegemonic power of PRI in the federal government and the rise of governors as central politicians in the allocation of different public goods. This context may explain why they did not comply with the federal regulations during that time period (Langston 2005). This is not the first study in exploring the political determinants of these transfers. Several studies have found a strong correlation between these transfers and political variables such as political competition and political alignment (Diaz-Cayeros, 2004; Langston, 2005; Hernandez-Trillo and Jarillo-Rabling, 2008 among others). This is the first that test this idea in a causal framework by using a close election design.

3. Identification

The main objective is to estimate the effect of political alignment between municipality-mayors and state-governors on a wide variety of economic outcomes. Political alignment is not randomly allocated but rather it results from a conscious decision made by voters when casting their ballots. We expect this decision to be affected by a myriad of unobserved political and economic factors that are also important determinants of my outcomes of

interest. Therefore, a naive OLS estimate of the effect of political alignment may also pick up the effect of these unobservables. To obtain causal estimates of the effect of political alignment on economic outcomes, I exploit information of elections decided by a close margin, a common research design known as a close-election regression discontinuity design—RDD.

A close election RDD uses as *running* variable the vote margin of each mayoral election. The vote margin— $VM_{m,e}$, 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— $v_{m,e}^o$, measured as a share of the total number of votes— $v_{m,e}$, where m and e index the municipality and election cycle.¹⁰

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

By definition, positive (negative) values of $VM_{m,e}$ correspond to municipalities where the elected candidate does (does not) belong to the state-governor’s party, i.e. it is (it is not) politically aligned. Our data on political affiliation of the candidate is measured before elections takes place, which rule out any concern regarding endogenous selection of candidates into political alignment after election results are known.¹¹

As explained by XX, the RDD allows to estimate the average treatment effect (ATE) of political alignment on the outcome y conditional on having a tied election (e.g. $VM_{m,e} = 0$), described by the equation below:

$$(2) \quad E[\beta^y \mid VM_{m,e} = 0] = E[y_{m,e}(1) \mid VM_{m,e} = 0] - E[y_{m,e}(0) \mid VM_{m,e} = 0]$$

where $y_{m,e}(1)$ is the potential outcome of municipality m when the winner candidate of

¹⁰The candidate of the main opposition party is defined at the municipality-election level, as the opposition’s candidate that gets the highest number of votes in a mayoral election. During our period of study Mexico has three strong political parties (e.g. PRI, PAN and PRD). This implies that when a local election is won by the main opposition party, the vote margin is not necessarily computed as the difference between the winner and the runner up.

¹¹We consider both candidates supported by a single party or by political coalitions. we classified a coalition as politically aligned if the state-governor’s party is part of such coalition and not politically aligned otherwise. Similar to the single party candidates, the coalitions we consider are announced during the political campaign well before election results are known.

the mayoral election e is politically aligned with the state-ruling party and $y_{m,e}(0)$ is the counterfactual when the winner is not politically aligned. Following ? and De la Cuesta and Imai (2016), the identification assumption needed to estimate β^y is the following:

Assumption 1 (Continuity). $E[y(1)_{m,e} \mid VM_{m,e}]$ and $E[y(0)_{m,e} \mid VM_{m,e}]$ are continuous at the cut-off, i.e. $VM_{m,e} = 0$.

This is a weaker identification assumption than *local unconfoundedness* because it allows a systematic correlation between unobservables and political alignment as long as it is continuous at the alignment threshold (De la Cuesta and Imai, 2016). This identification assumption has three implications that are important to validate, interpret and obtain the parameter of interest: First, any confounder that systematically correlates with alignment should vary smoothly around the cut-off. Second, in the absence of homogeneous effects of political alignment, the estimate obtained should be interpreted as local average treatment effect. Third, one can reliable sample analogue of $E[\beta^y \mid VM_{m,e} = 0]$?? by exploiting the variation provided by the elections decided by a close margin on either side of the threshold ($VM_{me} = 0$) as shown in equation below:

$$E[\beta^y \mid VM = 0] = \lim_{vm \rightarrow 0^+} E[y_{m,e} \mid VM_{m,e} = vm] - \lim_{vm \rightarrow 0^-} E[y_{m,e} \mid VM_{m,e} = vm] \quad (3)$$

The intuition of our identification assumption is that municipalities close to $VM_{me} = 0$ are comparable, therefore misaligned municipalities below the cut-off provide valid information to estimate the counterfactual of aligned municipalities that are just above the cut-off. This is equivalent to assume that the sole discontinuous change that occurs at $VM_{me} = 0$ is the shift in political alignment. The regression equation that allow us to get a sample analogue of our parameter of interest is defined as follows:

$$\begin{aligned} y_{m,e,k} = & \alpha + \beta \text{aligned}_{m,e} \\ (4) \quad & + \sum_{k=1}^{K(m)} \theta^k (VM_{m,e} \times \text{aligned}_{m,e}) + \sum_{k=1}^{K(m)} \gamma^k (VM_{m,e} \times (1 - \text{aligned}_{m,e})) \\ & + \delta_{s(m)} + \xi_{e,k} + \psi y_{m,e-1,k} + \epsilon_{m,e,k} \quad \forall VM_{m,e} \in (-h, h) \end{aligned}$$

where $y_{m,e,k}$ is measure our economic outcomes k years after election e in municipality m took place. The total number of k years included in the regression depends on the length of the term limits of local governments $K(m)$, which varies across Mexico.¹² The coefficient β measures the local average treatment effect of partisan alignment over the k year post-election. To identify β under the RDD setting we need to control by our running variable γ . The second line of equation 4 estimates a flexibly parametric function of the running variable on either side of the cut-off at each year k , described by θ^k and γ^k .

We identify β from the cross-sectional variation in y across municipalities where elections were defined by a narrow vote margin, i.e. $VM_{m,e} \in (-h, h)$.¹³ Particularly, we define as close elections as those where the vote margin of the winner over its main opposition was less than five percentage points difference.¹⁴ I estimate equation 4 using a local linear regression with triangular kernel weights Calonico, Cattaneo and Farrell (2020) and a first order polynomial of the running variable Gelman and Imbens (2019).¹⁵ I cluster standard errors at the municipality level because treatment is assigned at the election-municipality γ level but also there is correlation of the effects of political alignment within a municipality over time γ .

We also include state- $\delta_{s(m)}$, and election-period- $\xi_{e,k}$ fixed effects. Finally, we control by the values of our outcome of interest in the same electoral year k in the previous election cycle $e - 1$, $y_{m,e-1,k}$. This control provides a growth rate interpretation to our estimates

¹²Albeit all state governor's have six year term limit, the term limit of mayors varies across states. During our period of study, 27 out of 32 states have three years term limit, 2 states have a four years term limit and the rest have a two years term limit. Results are qualitatively similar when either: i) we re-weight our estimates by the inverse of the term limits or ii) when we focus only on the sub-sample of states for which local elections have a three-year term limit.

¹³This specification do not control by unobserved heterogeneity (i.e. municipality fixed effects) for two reasons: First, our identification assumption allows to have systematic correlation between unobserved municipalities traits and political alignment, as long as this traits do not change discontinuously at the cut-off. Second, a consistent causal estimate in a RDD use a local linear regression over the subset of close elections, adding a municipality fixed effect limits inference over the subsample of municipalities with more than one close election (less than 10% of the close elections sample) which may lead to small sample bias in our estimates. A more saturated approach that includes a municipality fixed effect can be seen in our difference in discontinuities specification provided below.

¹⁴Since the optimal bandwidth (h) suggested by Calonico, Cattaneo and Farrell (2020) is sensible to the outcome of interest and the sub-sample of data analyzed, we decided to keep the same bandwidth across different specifications. In Appendix XXX you can see the sensibility of our results to different bandwidths and also the estimates obtained by using optimal bandwidth for each outcome Calonico, Cattaneo and Farrell (2020). Our results remain qualitatively similar.

¹⁵See Appendix XX, where we show the robustness of our results to alternative kernel-weights and other order polynomials.

and improves efficiency.¹⁶

To observe the dynamics of the effect and support our identification assumptions, we follow Cellini, Ferreira and Rothstein (2010) and frame our close election RDD estimates into as an event study. This specification allows us to dissect the contemporary from the lagged effects of alignment and indirectly test the presence of parallel trends before alignment occurs. In our particular setting treatment changes over time, i.e. aligned municipalities at election e may become misaligned at election $e + 1$. This may complicate interpretation of the estimates of lagged effects for any period k in which treatment status could change as a result of the subsequent election $e + 1$.¹⁷ To have a consistent interpretation of the estimates and balance panel across municipalities we limit the estimation of lagged effects of alignment to a three-year window, i.e. $k = 3$, which corresponds to the most common length of term limits of local governments in Mexico. This implies that our estimates can be interpreted as a local average treatment effect. The dynamic specification we estimate is the following:

$$\begin{aligned}
 (5) \quad y_{m,e,k} = & \alpha_1 + \sum_{k=-3, k \neq -1}^3 \beta^k aligned_{m,e} \\
 & + \sum_{k=-3, k \neq -1}^3 \left[\theta^k (MV_{m,e} \times aligned_{m,e}) + \gamma^k (MV_{m,e} \times (1 - aligned_{m,e})) \right] \\
 & + \delta_{m,e} + \psi_k + \xi_e + \epsilon_{m,e,k} \quad \forall \quad VM_{m,e} \in (-h, h)
 \end{aligned}$$

To estimate this equation we built a dataset for the subset of close elections where the the unit of observation is a municipality election pair.¹⁸ Correspondingly, $\delta_{m,e}$ are

¹⁶This estimate does not suffer from Nickel bias because we are not including municipality fixed effects. This specification is equivalent to a model in first differences where we control for heterogeneous trends based on initial values of the outcome of interest. Controlling by the initial values of the outcome variable is valid as long as it varies smoothly around the cut-off and mimics the most standard control in RCT designs that exploit cross sectional variation ?.

¹⁷As Cellini, Ferreira and Rothstein (2010) suggest, for any year k before the end of the local mayor term we can safely interpret the estimates of β^k as the cumulative local average treatment effect k , or treatment on the treated while for any k after the end of the term limit the estimate of β^k is a compound of the alignment effect of both election e and election $e + 1$ and therefore can only be interpreted as a intention to treat of the lagged- k effect of partisan alignment in the former election.

¹⁸This means that for each municipality election pair, we track the the outcomes of interest for several (3) years before and after this election while keeping political aligned according to the result in the election year e .

municipality-election fixed effects to control for unobserved heterogeneity, while ψ_k and ξ_t are event time and year fixed effects.¹⁹ We take the same decisions as in equation 4 regarding the order of the polynomials (first order) of the running variable, the bandwidth to define our sample (5 percentage points) and the kernel-weights used in the local liner regression estimates (triangular). We cluster our standard errors by municipality because there is a obvious correlation across municipality-election pairs that come from the same municipality.

This strategy is analogous to what the rising literature of staggered adoption designs suggests, which applies to our context as long as have irreversibility of treatment, which means treatment is not changing over time. This property is achieve because political alignment does not change up for our three year window, i.e. $k = 3$.

The firs implication of our identification assumption allow us to estimate the effect of political alignment as long as we control by our running variable. The second implication was that the sole thing that changes at the cut-off is political alignment therefore any observed and unobserved characteristics that systematically correlate with alignment should be continuous at the cut-off. This assumption can not be directly tested, but we can perform some indirect tests that may assure the plausibility of this assumption in the following section.

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4. Validity of the research design

This section presents a set of formal tests about the internal validity of our estimates and also descriptive statistics that allow us to evaluate the external validity of our findings.

A. Internal validity

The internal validity of our research design depends on unobservable factors changing smoothly along the vote margin for municipalities were elections were close. Although we can not directly test this assumption we can implement two indirect tests that may

¹⁹Mexican states implement local elections in a staggered fashion. Therefore, we can control for event-time and year fixed simultaneously

reassure the internal validity of our estimates. Particularly, we evaluate if there is evidence of manipulation of the electoral outcomes or if there are discontinuities in pre-election characteristics around the cut-off.

MANIPULATION OF RUNNING VARIABLE. — The idea of this test is to evaluate if there is evidence of sorting on either side of the cut-off which may suggest that local governments select into being politically aligned. A exploratory test is to look at the density function. Figure XX shows the histogram of close elections around the cut-off for our period of study. There are xx municipalities where the local election was decided by less than 1 percent, among those XX were misaligned and XX were aligned. This symmetry among aligned and misaligned municipalities remain when we look other bandwidths around the cut-off.²⁰.

Figure XX present the results of McCrary (2008) test. This test aim to validate the continuity of the density of the vote margin. To do so, it basically estimate a density function on each side of the cut-off and evaluates if it is discontinuous at the cut-off. Figure XX suggest no discontinuity in the density function around the cut-off. The p-value of the McCrary-test is XXXX.

Calonico, Cattaneo and Farrell (2020) and Bugni and Canay (2021) have proposed variation of this test that rely of more strict assumptions. The results of these test are presented in Table XX together with the McCrary test. Overall all different test fail to reject a hypothesis of no sorting of municipalities around the cut-off.

It is possible that the manipulation of the running variable depends on the party who is in power at central level or in the incumbency status of either the state-governor or the municipal-mayor, for example if manipulating a election requires of strong political connections it is reasonable to think that incumbent governors have a comparative advantage with respect to new entrants. Figure XX shows the results of McCrary (2008) test across different parties and across different status of incumbency advantage for both state-governor and local mayors. Overall there is no strong evidence of particular parties

²⁰We observe xx (YY) municipalities where the local election was decided by less than 3(5) percentage points, among those XX(yy) were misaligned and XX(yy) were aligned.

being able to manipulate elections nor incumbency status being a key factor that allows to observe sorting around the cut-off.

DISCONTINUITY OF PRE-DETERMINED CHARACTERISTICS. — Figure XX performs a balance test of baseline characteristics between aligned and misaligned municipalities around the cut-off, i.e. $VM = 0$. Particularly, we test difference in means on XX economic, socio-demographic, geographic, and political characteristics, all measured before the election takes place. We standardize all nonbinary 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 β as defined by the equation below, where the outcome of interest is the baseline characteristics measure at 1990.²¹

$$\begin{aligned}
 y_{m,90} = & \alpha + \beta \text{aligned}_{m,e} \\
 (6) \quad & + \theta(VM_{m,e} \times \text{aligned}_{m,e}) + \gamma(VM_{m,e} \times (1 - \text{aligned}_{m,e})) \\
 & + \delta_{s(m)} + \epsilon_{m,e,k} \quad \forall VM_{m,e} \in (-h, h)
 \end{aligned}$$

where all the coefficients and indexes follow directly from equation 4, the only difference is that our outcomes of interest are baseline characteristics measured at 1990s. Similar to the body of the study we estimate this equation using a local linear regression using a 5 percentage points bandwidth, triangular kernel weights and a 1 order polynomial of the running variable. The Online Appendix provides analogous results for alternative bandwidths, kernels and polynomials specifications.

It is reassuring that there is not evidence of any discontinuous jump in the baseline characteristics. All the confidence intervals cross the zero except in the case of XXX. The p-value of the joint hypothesis test that all baseline characteristics are statistically equal to zero is XXXX, which is presented in the left corner of the figure XX.

While conducting several placebo test allows to be more confident about the no presence

²¹The notation of this equation is the same as in equation 4. The only difference is that we do not control for any pre-electoral measure of our outcome of interest because in this test the baseline characteristics are our main outcome of interests. Most of the baseline characteristics are measured from the 1990s Population and 1989 Economic Censuses.

of omitted variable that jumps at the aligned-misaligned threshold it comes at the price of false discoveries. When testing multiple baseline variables it is always possible that we find false positives even when our identification assumption holds [De la Cuesta and Imai \(2016\)](#).²² To control for multiple testing, appendix XX shows the the Family-Wise-Error p-values (Westfall and Young, 1993 and Jones et al., 2019). Overall we find that characteristics are balance around the cut-off.

Similarly to Mc-Crry test, [Canay and Kamat \(2018\)](#) propose a permutation test that validates that the distribution, instead the mean, of baseline characteristics is continuous around the cut-off. The results are presented in Appendix XXX and suggest that

In the section of we extend this balance test to the pre-election trends in our outcomes of interest.

B. External validity

As any regression discontinuity design we use a subsample of data to estimate the parameters of interest. Particularly, we use in our estimates municipalities with high political competition, which implies that, in the presence of heterogeneous treatment effects, our estimates are only able to capture a local average treatment effect.

HOW MANY CLOSE ELECTIONS. — Table XX asses the sample size of close elections and compare it with other papers of the literature. Panel B shows to what extent municipalities have more than one close election during our period of study.

CHARACTERISTICS OF CONTESTED PLACES. — Table XX asses the implications of using only close elections to identify our parameters of interest. This table compares the difference in means in a wide set of pre-policy characteristics between municipalities with close-elections and all the municipalities in Mexico. both types of municipalities conditional on having reliable information on formal employment (See section XX). As we can see

²²[De la Cuesta and Imai \(2016\)](#) estimates that when we test for discontinuities in 25 variables and all null hypotheses are true at 5% level of significance we will have will have on aveage 1.25 false rejections.

contested municipalities differ strongly from uncontested municipalities: they tend to have 2 percentage point more XXX, 3 percentage point more of XXXX and 3 percentage point more of XXXX.

This suggest that we can not extrapolate our estimates to municipalities outside the cut-off. That being said, we we would abstain from extrapolate our estimates, even if characteristics presented in Table XX indicated no difference in means in those pre-determined characteristics. The fundamental reason, is that political alignment in a non-contested place do not create enough incentives to the central government to reallocate public resources. The strong political preferences of uncontested places reduce the probability that any partisan action of the central government affects switch the electoral results in their favor. Therefore the optimal response of central government with uncontested places does not vary with partisan alignment. See [Brollo and Nannicini \(2012\)](#) for a formal proof of this argument.

SPATIAL CONCENTRATION OF CLOSE ELECTIONS. — Another concern regarding of the close elections subsample, is that it exhibits strong spatial clustering. This may bias our estimates in the presence of spatial spillover effects. Moreover, if close elections are concentrated in few states it will create an additional problem of external validity than the one related about the type of municipalities we observe. It will limit our inference regarding the type of central government for which we estimates the effects of partisan alignment. The Figure XX shows the map of close elections during our period of study using a bandwidth of 5 percentage points. In the case a municipality have had more than one close during our period we report the result of the first. As it can be observed close elections are spread all over the place. All 32 elections have had close election with aligned and misaligned municipalities during our period of study.

5. Data

This paper combines 7 different datasets using administrative records, remote sensing data and, Population and Economic Censuses from Mexico. Below we describe the main datasets used.

PUBLIC FINANCE. — We use official balance sheets reports to create different public finance outcomes. Municipal governments must report to the Federal government a detail accounting of their revenues and spending across different set of categories and subcategories. The data is compiled and harmonized by the Mexican National Institute of Statistics and Geography (INEGI). This dataset allows to construct a rich set of public finance outcomes at the municipality-year to employ in our research design.

We exploit both the spending and revenue data to construct different outcomes of interest. The revenue data is disaggregated in the following categories: **local taxes, fees and tariffs, transfers from upper layers of government, and fiscal deficit**. We use all these revenue categories to understand which type of revenue at local level respond to political alignment with the ruling party at state level. We also exploit the detailed information on spending provided to see if local governments change their spending patterns due the the alignment-induced additional resources. Spending is classified in the following categories: **personal services, capital investment, interest payments, inputs, and general services, and other expenditures**.

The summary statistics of revenues categories for our period of study are presented in panel A of Table 2. Regarding municipal revenues we have that most of the municipal revenues are received trough intergovernmental transfers. In the average municipality-year about XX% of income comes from intergovernmental transfers, when dividing this share between earmarked and discretionary transfer we have that XX percent of municipal revenues comes from discretionary while XX percent correspond to earmarked transfers. The collection of own revenue is relatively small and account for XX% of total revenues. most of this revenues comes from taxes (mostly property taxes) XX%, and from services provided by the municipal government, xx%.

The summary statistics of spending categories for our period of study are presented in Panel B of Table 2. After the decentralization process that was implemented in 1998 municipalities were assigned a wide variety of responsibilities related to provide public services, security, health and school related investments and pay the public personal at

municipal level²³. As it can be seen the bulk of spending is allocated across the municipal wage bill (xx%), public investment (xx%) and XXXXX (xx%).

we abstain from further disaggregate revenues and spending, since it is likely that the disaggregation across subcategories produces non-random measurement error, where institutional capacity is correlated with the share of spending misreported across subcategories.

ELECTORAL RESULTS. — Electoral data for state and presidential elections come from the National Electoral Authority (INE). Municipal elections were collected across different electoral authorities at state level (State INE) and complemented with data collected from Centro de Investigación para el Desarrollo A.C (CIDAC). The election data provides information on the number of votes cast by party or coalition in the universe of municipal, state and presidential elections hold during our period of study.

Election data allow us to compute the political alignment status and our to measure the vote margin between the ruling party at state level and the opposition's party who obtained the highest number of votes, see ?? for details. Information of coalitions is considered when defining political alignment.²⁴ It is important to note that the coalitions recorded by electoral authorities are defined before elections takes place, this eliminates any concerns regarding some unobserved factors affecting our measurement of political alignment.²⁵

Figure XX shows the term limits of different layers of the executive government in Mexico. Presidential and state elections take place every 6 years, while elections of local mayors take place every three years in most of the municipalities.²⁶ During our period

²³except for the public workers in the education and health sector for who the state government is responsible

²⁴In a multiparty system political parties may run for election forming coalitions with other minority parties. We defined alignment broadly considering all state level coalitions formed before elections take place. Let's consider an example: The winner candidate of Puebla's governor election in XXXX ran under a coalition made between PAN and PRD parties. All municipalities of Puebla who elected a mayor from either PAN or PRD during the term of the governor elected in 200XXX were considered politically aligned. When the coalitions were made at local level if one of the parties of the coalition belong to the winner at state level we consider the parties are politically aligned.

²⁵Any coalition between mayors and governors formed after election would invalidate our identification assumptions. It is easy to think in a case where winner candidates of certain traits select themselves into being politically aligned after winning an election and observing the winner at state level. By using only pre-electoral coalitions we block one channel through which manipulation of treatment could happen.

²⁶States by type term limit of mayor elections XXXX

of study we have data on XXX and XXX presidential and state elections, while there is data on XXX municipal elections. [cool tikz on elections and close elections, timeline](#)

EMPLOYMENT AND WAGES. — . To construct a panel of employment at municipal-year level we combine administrative records from the Mexican Institute of Social Security (IMSS) and the Institute of Social Security of Public Workers (ISSTE). IMSS allows us to observe the number of employees and average wages of worker in the formal sector. Similarly, ISSTE measure employment for most of the workers in the public sector. Between IMSS and ISSTE we are able to observe the dynamics of almost all formal workers in Mexico.

The data on wages [This speaks about the formal sector, but we have to say it as we really wanted or skip it as the papers on Brazil](#)

This implies that in our data there are about XX municipalities with less than 100 workers and for who any growth rate measure will induce to high measurement error. Therefore we limit our analysis to those municipalities that report at least 100 employees on average for the period for which the data is available. This filter prevents small municipalities with considerable variation in the percentage of employees from driving our results.

Our analysis is restricted to 986 municipalities representing 80 percent of Mexico's population, and 95 percent of workers registered at IMSS. However, in the presence of heterogeneous effects it limits our the external validity of our results. Particularly, our findings can not be extrapolated to the effects of alignment on public finance outcomes to municipalities with high levels of informality.²⁷

Our research design demands employment data at municipality-year level. Therefore data provided by household surveys, censuses is not sufficient to mimic the level of variation that our close elections design requires.

Aggregate Economic Activity The main drawback of IMSS and ISSTE is that they remain mute about what is happening to the informal sector and therefore we can infer aggregate consequences at local level. To proxy for annual municipal level economic

²⁷Regarding our results for private employment, the lack of external validity is irrelevant because we do not care about what would have happened with a formal sector in municipalities where the formal sector does not exist.

activity, we use the intensity of night lights from XXXXXX and the total consumption of electricity from XXXXX. The main idea is that these measures are elastic to changes that take place both in the formal and informal sector. To validate these two proxies of economic activity we employ data from the Economic Census.

Data on night light composites is produced by the National Oceanic and Atmospheric Administration (NOAA). NOAA produced images for Mexico where each pixel covers approximately 1 square kilometer. Each pixel contains a digital number that measures the light intensity on a scale from 0 to 63. Therefore, data could not be suitable to measure growth for places that are already censored at 63. We impute growth rates over censored pixels using the growth rate of neighboring pixels of non-censored pixels. The implicit assumption is that the growth rate is highly correlated across neighboring pixels of each municipality, which is corroborated by the data.

Other alternative data we use is consumption of electricity. dsdaaxsdaeacacac

The main objective of these two data sources is to proxy for aggregate economic activity. To validate that they are good proxies we regress actual measures of economic activity collected every five years from the economic censuses on both measures in the same spirit of XXX and XXX papers.

Three main conclusions can be obtained from this exercise. XXXXXXXX

We did not directly use the Economic Census data since it provides information every 5 years which does not match with the variation in political alignment that takes place every three years. The only way to employ this different variation in a quasi-experimental setting would be to use close election that takes place

OTHER DATASETS. — We also use other datasets to explore some potential channels that explain our main findings and balance test that validate our identification assumptions. Particularly we use population census audits xxxx data on large federal programs such as zzzzzz Household surveys zzzzzzzzz used to perform balance test on several economic and socio-demographic characteristics such as: i) The roll out of the number of beneficiaries from Seguro Popular, ii) Monthly payments of PROGRESA, iii) Population and Economic Censuses for the years 1999 and 2003, iv) Electricity consumption, v) Official audit reports

from Auditoría Superior de la Federación.

6. Main Results

This section presents our main estimates. We first report local average treatment effects of political alignment on a wide set of public finance outcomes. Second, we explore the effects on labor market outcomes for both public and private sector. Third we use these estimates to construct estimates of crowding out of private sector economic activity. Finally, we present a complementary analysis where we use a measure of alignment as an instrument for local public spending. All estimates in this section are obtained using equation (2) and equation (3) unless otherwise is indicated.

A. Public Revenues

RAMO-33 TRANSFERS. — The first order question is if being politically aligned matters at all for the allocation transfers, even in the presence of the allocation formulas that were designed to protect this transfer from political manipulation. In other words, do intergovernmental transfers change when municipalities cross the political alignment threshold?. To answer this question we use two measures of Ramo 33 intergovernmental transfers, the probability of receiving transfers and the log of the amount of per-capita transfers. The first measure aims to identify if state governments punish non-aligned municipalities by holding up the transfers, which does not necessary imply that by the end of the mayor term they received less transfers. The second measure test whether there is difference in the total transfers received by aligned municipalities.

Figure ?? shows the results of the effect of political alignment on Ramo 33 transfers both after (panel A and B) and before (panel C and D) a close election takes place (see Table ??). While political alignment does not significantly increase the probability of receiving transfers during the mayor’s subsequent term (panel A), it increases the amount of transfers received substantially (panel B). Transfers increased by about XX following a close victory of the state-governor’s party with respect to a close victory of state-governor’s opposition (panel B). Consistent with our identification assumptions the probability of

receiving transfers before a close election takes place do not discontinuously change at the alignment threshold.

It is interesting to explore if the coefficients we observe on transfers follow a logic outlined by the literature on political economy. Particularly, we estimate if the effect of political-induced transfers is more prevalent in state governments who face high political competition? and whether the effect of alignment on transfers follows the electoral cycle???, i.e. whether the effects of alignment on transfers are higher the year before the election takes place. To answer this questions, Table ?? shows the heterogeneity of the effect of political alignment along these two dimensions. Column (1) and (2) present the benchmark results with no heterogeneity. Column (3) and (4) show the heterogeneity of the effects of alignment along the political competition dimension,²⁸ while column (5) to (8) presents the effects of alignment on the electoral cycle of mayors and governors.²⁹

According to column (3) and (4) of Table ?? the effect of political alignment is XX times stronger in states that have lower political competition. This suggest that political accountability can limit the room the state-governors have to favor their co-partisans at local level with transfers. This result goes in line with the findings by ?? who find that partisan favoritism in non-contested state-governors is XX times higher than in contested governments.

When we focus on the relation between the political alignment effect and the electoral cycle, we find that alignment is more sensitive to the electoral cycle of state-governors rather than the electoral cycle of municipal mayors. The effect of mayor's electoral cycle (column 6) is XX higher than the effect of alignment in any other year, while in the case of the governor's electoral cycle (column 8) we find that the effect of alignment is XX times higher. Overall, this suggest that favoring their co-partisans is a decision that respond to a vote-seeking behavior from the side of state governors. This matches the results found

²⁸To obtain municipal level measures of political competition we measure the historical average vote margin between the winner and the runner up before the close election of reference take place, those municipalities with a historical average above the median of our subset of close elections are labelled as places with relatively high political competition.

²⁹To measure the local political business cycle we use an indicator variable for the last year of the mayors term (three year term limit) while the state business cycle is measure by an indicator variable for the last two years of the governor's term (six year term limit). Notice that since we are asking heterogeneity along this dimension, we estimate equation 4 without the cohort-time dummy, only cohort dummy is used in the specification.

in ? and ?.

ALTERNATIVE STATE AND FEDERAL POLICIES. —

ALTERNATIVE STATE AND FEDERAL PROGRAMS. — dsad?? suggests the estimates on the effects of political alignment may suffer from upward bias because of its tendency to focus on one single public good or transfer, we rule this possibility in our context by looking at other intergovernmental transfers provided by state and federal government. The main point of ?? is that by focusing on one single public good (e.g. Ramo 33) we ignore potential crowding-out effects that the transfer one observe may have over the transfers we implicitly decide to not observe. In other words, politicians can compensate the non-politically aligned municipalities with other transfers. To rule out that alignment operates through other transfers we explore the effect of alignment on unconditional transfers made by state governments, known as Ramo 28, and also we look at two largest initiative of the federal government that increased during our period of study, namely, Progresas and Seguro Popular.

A crowding out effect would suggest that misaligned places, who received less Ramo-33 transfer, are compensated with higher resources from the alternative transfer and public programs we look at. The results of the effect of alignment on these programs are not statistically significant but the point estimates suggest that if anything alignment just meant higher resources for aligned municipalities (see in Figure). Moreover the point estimates are XX times lower than what we found for Ramo 33. In the absences of crowding out effects and given that municipalities run a balance budget, we must expect that alignment affects local public spending. Moreover, this gives confidence that during the period of study the main driver through which state-governors rewarded the municipalities with a co-partisan mayors was through, the weakly enforced, Ramo 33 transfers.

TAX AND DEBT. — Above we rule out that Ramo 33 did not crowd out other state and federal programs. Still, crowding out effects can happen through the decision of local governments over taxes and debt. Particularly, local governments with higher amount of Ramo 33 transfers may reduce their taxation efforts or be less prone to acquire public debt. Any of those behaviors would lead to.... finds that politically induced transfers reduce by 16 percent local taxation in Italy. finds that transfer shock We explore if this is the case For example decide higher transfers from Ramo 33 can affect the decision of local governments about how much they borrow a tax.

local government's from either enforcing taxation or or governments since money is fungible, local governments can decide the effect of transfer can be crowded out if the effect of Ramo 33 can crowd out can crowd out state and federal government The transfers can also affect the decision Besides the effect Flypaper effect As expected in alignment increased recently enacted transfer and did not have significant effect on other state and federal transfers

LOCAL PUBLIC SPENDING. — Table XX consolidates all the results presented until now by looking at the effect of political alignment on public local spending. Local governments run a balanced budget. since we found that political alignment lead to higher transfer and if anything those lead to crowding in we should expect positive estimates of alignment on total public spending. there are not clear crowding out If we found a strong effect of political alignment on transfer from Ramo 33 As expected in alignment increased recently enacted transfer and did not have significant effect on other state and federal transfers

Table 3 still finds being aligned with the governor increases the growth rate of public resources by about 10 percentage points. The positive effect on local revenues can be interpreted as an estimate of partisan alignment's net effect once any compensation and behavioral effects induced by the increase in intergovernmental transfers have been netted out. Figure 6-7 shows that this effect lasts during the whole period of mayors' term.

B. Employment

Mexico is well known for its large informality rates, which have been blamed as one of the main reasons for the reluctant economic growth (Hanson 2010). The formal sector employs about 40% of the workforce and produces about 70% of the total output. Formal employment is not sparse across all municipalities; only about 1300 out of 2496 municipalities register at least one private-formal employee during our period of study (1997-2006). Our estimates consider only municipalities where we see at least 100 private-formal employees on average for the period for which information is available. This allows having less noisy estimates of the effect of alignment on formal employment growth.

Table 4 and Figure 8 show the effect of political alignment on the growth rate of private-formal workers. According to these estimates, alignment implies an average reduction of 10 percentage points in private-formal employment growth. The descriptive statistics indicate that this reduction comes from lower net creation of jobs rather than job destruction. The dynamic estimation suggests that the reduction in private-formal employment is short-lived and vanish when the mayor's term is over—Figure 9.

We estimate separate equations of these effects by characteristics of the sector, firm size, and worker earnings (Tables 4-14). There is no regular pattern, and we cannot statistically reject that the effect is particularly strong in one sector or other.

7. Conclusions

This paper estimates the effect of fiscal windfalls in local private-sector economic activity. To do so, we causally estimate the effect of partisan alignment with state governors on local private economic activity; in a context in which governors controlled a new intergovernmental transfer that represented a sizeable amount of local public resources in Mexico.

We find that political alignment with state governors increases public spending about 10 percentage points due to larger intergovernmental transfers received by aligned municipalities. This increase seems to negatively affect the net creation of formal-private jobs in a similar magnitude to the increase in spending. Under the assumption that the main policy lever that the governors had during our study period is to increase the local

public spending, our estimates imply an elasticity of private-formal employment to local spending of -0.89.

We did not find evidence of higher corruption or public sector enlargement due to partisan alignment (Tables 17-19). The lack of a similar negative effect on our proxy of total economic activity, measured by nighttime light, suggests that reallocation to the informal sector is a plausible collateral effect of the spending shocks.

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 development of the private economic sector.

These findings imply that government spending on local economies can create unintended consequences, namely, lower productivity and lower tax collection from personal income taxes. This does not imply that government spending leads to lower welfare. We can not reject the null hypothesis that this spending did not affect welfare significantly by using night light, although point estimates suggest a consistent decline over time (Figure 16).

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