Local Political Business Cycles: Evidence from Philippine Municipalities*

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Abstract

This paper establishes the existence of short-term political business cycles in the Philippines over the period 2003 - 2009. Examining a balanced panel of 1,143 municipalities shows that employment levels increase in the two pre-electoral quarters and drop sharply in the two quarters following elections. Further results are consistent with the cycles being generated by incumbents' attempts to increase their chances of re-election. Cycles are stronger in sectors that incumbents are more able to influence, and when they expect stronger electoral competition. Evidence suggests these cycles are detrimental to development.

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1 Introduction

It is often speculated that incumbents will manipulate the economy, especially employment, in order to increase their chances of re-election (Nordhaus, 1975; Alesina, Roubini and Cohen, 1997). These manipulations would create political business cycles.

Despite the fact that the ingredients required for political business cycles have been well documented, evidence for political business cycles themselves is less conclusive. Specifically, voters evaluate incumbents according to economic performance (Lewis-Beck and Paldam, 2000), and incumbents have the ability to affect economic outcomes (Coulomb and Sangnier, 2014; Snowberg, Wolfers and Zitzewitz, 2007*a,b*). Yet, researchers have been able to identify only moderate cycles in economic policies and, at best, weak cycles in real outcomes (Franzese, 2002).

A plausible reason for the difficulty in identifying political business cycles is the use of data aggregated across time and space. Yearly data obscures the fact that voters seem to be most sensitive to very recent economic outcomes (Fair, 1978, 2002; Healy and Lenz, 2014). However, while early studies were carried out at the national-level, recent analyses of local governments' spending in election years yield similarly mixed results.¹

Analysis of quarterly data from Philippine municipalities shows the existence of robust political employment cycles. The data comes from 1,143 cities and municipalities over the period 2003 - 2009. The share of the working-age population that is employed increases by 1.5 percent (0.88 percentage points) in the two quarters before elections, and is 0.49 percentage points lower in the two post-election quarters in election years than in non-election years. This effect is only apparent in quarterly data due to the fact that increases in employment before the election are canceled out by declines after the election.²

The data also contain evidence that the employment cycles described above are caused by incumbents' attempts to increase their, or their family members, chances of re-election. First, there is no evidence of an employment cycle when the incumbent is running unopposed, and thus the incumbent lacks an incentive to manipulate the economy. Second, cycles are partic-

¹See, for example, Brender (2003), Akhmedov and Zhuravskaya (2004), Khemani (2004), Drazen and Eslava (2010), Sakurai and Menezes-Filho (2010), Aidt, Veiga and Veiga (2011), Jones, Meloni and Tommasi (2012) and Sjahrir, Kis-Katos and Schulze (2013).

²The results presented in this paper suggest that, where possible, future analyses of political business cycles should use monthly or quarterly data. A similar point is implicit in results presented by Akhmedov and Zhuravskaya (2004) for analyzing political business cycles, but it appears that data constraints have prevented researchers from doing so.

ularly strong around elections in which the incumbent is term limited, and one of his or her relatives is running. As strong challengers in the Philippines often wait until the incumbent is term limited, relatives tend to face much more competitive races (Querubin, 2011). Third, cycles are much stronger in private employment than in public employment. This is consistent with the fact that incumbents face legal hiring constraints in the public sector right before elections, but can invest in last-minute infrastructure with private companies.

There is also suggestive evidence that political business cycles are detrimental to development. Philippine municipalities with more pronounced cycles in 2004 and 2007 had lower employment levels in 2009. This is robust to controlling for a number of socio-economic and electoral municipal-level characteristics.

2 Setting

Before turning to the analysis, it is useful to understand details of the political economy of the Philippines. It is important to emphasize that the institutional context varies extremely little across the country, implicitly controlling for differences that may make political business cycles difficult to detect and understand (Drazen and Eslava, 2010).

Most decisions regarding municipal budgets in the Philippines are made by mayors who use available funds with limited oversight. This is despite the fact that the 1991 Local Government Code established municipal councils and gave them decision-making powers. Mayors control both how budgets are spent and hiring decisions in the local bureaucracy (Hodder, 2009; Hutchcroft, 2012). As incumbent mayors are known to exert significant control over the local economy, they are likely to be held responsible for local economic performance. This, in turn, creates incentives for mayors to distort the economy ahead of elections (Anderson, 1995).

The fiscal and calendar years coincide in the Philippines, and mayoral elections take place every three years in May. Elections follow an established schedule set out in the 1987 constitution. As incumbents cannot control election timing, this eliminates concerns that the economy may affect election timing. Two elections were held during the sample period, in May 2004 and May 2007.

Incumbents in the Philippines face legal constraints on increasing public sector employment in the 45 days before an election. They cannot appoint or hire new employees; create or fill

new positions; give promotions or increases in salary, other remuneration, or privileges.³ The Commission on Elections (COMELEC) also bans public works 45 days before elections, but the ruling has a number of exemptions. For example, the maintenance of existing projects is allowed, as well as work allocated through public bidding before the 45-day period. Thus, local politicians can circumvent these restrictions by hiring private contractors to work on existing infrastructure projects.

Qualitative evidence suggests that incumbents attempt to time public spending in order to increase employment just before elections (Hollnsteiner, 1963; Wurfel, 1963). These new jobs are publicized to ensure that voters are aware of their political obligations (Wurfel, 1963).

Local politicians often use the power of their office to increase their local business holdings, which enables them to directly employ their constituents (Sidel, 1999). Further, in a number of Philippine municipalities, mayors act as employment brokers, helping their constituents find jobs. For example, in a province near Manila, job applicants in local factories were required to provide letters of recommendation from local officials (Sidel, 1999, pp 76-77). There is qualitative evidence that this role intensifies before elections as voters have more bargaining power (Kawanaka, 2002).

3 Data and Descriptive Statistics

Data largely comes from the Philippine Labor Force Surveys (LFS) collected by the National Statistics Office (NSO). This is used to compute official employment statistics. The surveys are conducted four times a year (January, April, July and October), and the data contains the response to all 26 surveys between July 2003 and October 2009.⁴ Each survey has a sample size of approximately 200,000 individuals in 50,000 households, and contains 1,143 cities and municipalities, out of 1,634 in the country.⁵ A person is considered employed if he or she reported to work for at least an hour during the week prior to the survey. In addition, information is collected on the total number of hours worked during the past week, the sector of employment and the daily wage. For each municipality/survey wave, the employment rate is computed as a share of the working-age population.⁶

³COMELEC resolution no. 6620 (25 November 2003) and COMELEC resolution no. 7707 (30 August 2006).

⁴More information on the survey design is available at http://www.census.gov.ph/data/technotes/notelfs_new.html last visited on March 26, 2012.

⁵<http://www.nscb.gov.ph/activestats/psgc/default.asp> last visited on July 3, 2014.

⁶The NSO changed the definition of the economically active population in April 2005; individuals now have to be "available to work" in order to be counted as unemployed. This information was not collected previously, and so

The employment data is supplemented by annual data on municipal budgets from the Department of Budget and Management.⁷ The data are all expressed in 2000 Pesos using regional consumer price indices. Descriptive statistics are available in Table A.1 in the online appendix.

Political employment cycles are easily detectable in the quarterly data. Average employment rate in Q1/Q2 was higher in election years than in non-election years. Specifically, the average municipal employment rate in Q1/Q2 in election years is 59.6 percent, while it is only 58.7 percent in non-election years. Similarly, the average employment rate in Q3/Q4 was lower in election years than in non-election years. The average municipal employment rate in Q3/Q4 in election years is 58.9 percent, while it is 59.3 percent in non-election years. Finally, Figure 1 shows the difference between Q1/Q2 and Q3/Q4 in election years and non-election years. As shown in the figure, in non-election years between 2003 and 2009, the employment rate in the last two quarters is higher than in the first two. In election years, this pattern is reversed: employment is higher before the elections than after.

4 Estimation strategy

To empirically test for the presence of political employment cycles, we estimate equations of the form:

$$Y_{ijqt} = \alpha E_t + \beta X_{ijt} + u_{ij} + v_q + w_{ijqt} \quad (1)$$

where Y_{ijqt} is employment rate in municipality i in province j in quarter q at time t , E_t is a vector of variables capturing election timing, X_{ijt} is a vector of municipal-level characteristics that vary over time, u_{ij} is a municipality-specific unobservable, v_q is a quarter-specific unobservable and w_{ijqt} is the usual idiosyncratic term.⁸ For most of the paper, E_t includes a dummy equal to 1 for the January and April waves of 2004 and 2007 and another dummy equal to 1 for the July and October waves of 2004 and 2007. The vector X_{ijt} includes controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15) and the share of women (for those older than 15) in the sample, all computed using the LFS data. X_{ijt} also includes controls for population levels and per capita fiscal transfers.

we are unable to adjust the data for the previous quarters. As a result, estimates in this paper combine the effects of elections on the decision to enter/exit the labor force and of being employed (for those in the labor force).

⁷The data are available from <http://www.blgf.gov.ph/#>, last visited on March 26, 2012.

⁸The online appendix also reports results where Y_{ijqt} is the average number of hours worked over the past week (for those with a job) or the average log daily wage (for those with a job).

The quarter-specific dummies capture the seasonal nature of employment in the Philippines, which is driven by weather patterns. Each observation is weighted using the sum of individual survey weights in the municipality at that time period.⁹ Elections are held simultaneously around the country, which prevents us from including time fixed effects. But we also report regressions in which we flexibly control for time trends by including either region-specific or province-specific quadratic time trends.

Given the data structure, the error terms are not independent, and are likely to be correlated within municipalities, provinces and time periods. As a result, we use a method developed by Cameron, Gelbach and Miller (2011) and cluster standard errors across both time and provinces (as municipalities are nested within provinces).¹⁰

Section 6 estimates Equation (1) separately for the public and private sectors, and for casual and permanent employment. As the surveys were designed to provide representative estimates at the regional level, the municipal-level estimates used in the paper are noisy. This is a particular concern for sectoral analyses, as those estimates have larger variances that decrease power. As such, sectoral results should be interpreted with caution.

5 Political cycles: Does employment increase shortly before elections?

In line with the literature, we start by estimating Equation (1) using annual data. In those regressions, E_t is a dummy equal to 1 for the years 2004 and 2007. Contrary to the clear patterns found in Figures 1 - 3, this analysis, shown in the first panel of Table 1, finds no evidence of political employment cycles. Indeed, the proportion of the working-age population that is employed is neither higher nor lower in election years vs. non-election years. The point estimates are small (0.13 percentage points), which suggests that the failure to reject the null hypothesis of no effect is not due to imprecise estimates.¹¹ Therefore for the rest of the paper we examine quarterly data.

⁹The online appendix also presents results from unweighted regressions.

¹⁰An added complication arises from the fact that we only have 26 time periods; thus standard errors are downward biased and lead to the over-rejection of the null hypothesis of no effect (Cameron, Gelbach and Miller, 2008). As we have six time-invariant regressors, one solution is to use critical values from a T distribution with 20 degrees of freedom. The results discussed in the paper are robust to using such critical values.

¹¹To account for the fact that we only have data for the July and October waves in 2003 (a different sample was used prior to July 2003), we re-estimate the models using the 2004 - 2009 sample. This does not affect the results (Table A.3).

Point estimates for the overall effects are seven times larger when considering quarterly data. The results in Panel B of Table 1 indicate that there is an increase in employment in the two quarters preceding elections of 0.88 - 0.90 percentage points. Moreover, employment in the two post-election quarters is 0.46 - 0.49 percentage points lower than it would have been had the elections not taken place. This explains why the clear electoral cycles in Figure 1 are not found in annual data: a post-election decline in employment cancels out earlier gains. The results are robust to the inclusion of a number of control variables and region-specific or province-specific quadratic time trends.

The effects do not appear to be driven by one specific election. Separating the effects into point estimates for 2004 and 2007, the pre-election increase in employment rate is 0.78 and 0.96 percentage points, respectively (p-value on the difference = 0.764).¹² The post-election decrease is also statistically identical in 2004 and 2007. Here the point estimates are -0.41 and -0.55, respectively (p-value on the difference = 0.737).

The observed effects are consistent with a within-year reallocation of resources by incumbent politicians. As is apparent from comparing the annual and quarterly data, the null hypothesis that the sum of the coefficients on the pre-election and post-election dummies is equal to 0 cannot be rejected ($\chi^2 = 1.23$, p-value = 0.267). Unfortunately, data constraints preclude a direct test as the budget data are only available at the annual-level. However, this still suggests that the failure to identify political business cycles in some previous studies could be partly driven by examining only yearly data.¹³

If political business cycles occur on the quarterly, or even monthly, scale, then the ability to detect political business cycles using annual data will be diminished. For example, in Portugal, annual data leads to estimated effects of elections of an increase of only 2.5 jobs municipal jobs (Coelho, Veiga and Veiga, 2006). Similarly, in Sweden and Finland there are 0.6 more full-time public employees per 1,000 residents in election years. For the average municipality in Sweden, this translates into only 15.6 additional public sector jobs (Dahlberg and Mork, 2011).

Interestingly, there is no evidence that elections affect either the number of working hours

¹²See Panel B of Table A.2 in the online appendix. Only the post-election quarters dummies included in E_t are also interacted with the year dummies. Thus, we have separate dummies for the January / April quarters in 2004 and 2007. Similarly, we have separate dummies for the July / October quarters in 2004 and 2007.

¹³These results are robust to alternative specifications, including those with up to four lagged values of the dependent variable (estimated using both fixed effects and generalized method of moments (GMM) regressions), quarter*municipality fixed effects and alternative estimation samples (excluding either outliers in a number of distributions or the most conflict-affected region in the country). Those results are discussed in more detail in the online Appendix (Tables A.6-A.10).

or the wages of employed individuals.¹⁴ This suggests that, along those two dimensions, jobs created as a result of elections are similar to average jobs in the municipalities.

6 Are the employment cycles politically induced?

This section provides evidence that the cycles identified in the previous section are caused by politicians' attempts to win elections. The argument proceeds in two steps. First, by showing that political business cycles are concentrated in sectors incumbents can directly influence. Second, by demonstrating that the strength of the cycle is a function of the incumbent's electoral incentives.

6.1 The cycles are stronger in sectors that incumbents can influence

As noted above, incumbents face legal constraints on public sector hiring in the 45 days before an election. As such, there should be little evidence of electoral cycles in public employment. Indeed, the results in Table 2 show that most of the variation in employment is concentrated in the private sector. In the public sector, elections are associated with a 0.15-percentage-point increase in employment before the election, and no meaningful decrease in employment after the election. In contrast, in the private sector, there is a 0.73-percentage-point increase in employment before the election, and a 0.48-percentage-point decrease in employment after the election. That is, 83 percent of the increase in employment levels, and effectively all of the decrease, occurs in the private sector.

Additionally, electoral incentives may lead to different types of employment in the public and private sector. In the public sector, incumbents will want to tie bureaucrats to their own electoral prospects by hiring them on non-casual contracts. In this case, the bureaucrats job tenure is implicitly linked to the incumbent's electoral success (Robinson and Verdier, 2013; Iyer and Mani, 2012). However, in the private sector, there is no such possibility, and so employment should ramp up using casual, short-term contracts. Table 2 shows these patterns do, indeed, obtain, by analyzing employment by contract type (casual or non-casual) and sector. The only statistically significant effects are on non-casual employment in the public sector, and casual private employment.

Overall, this set of results provides evidence that the employment cycles identified in the

¹⁴See Tables A.4 and A.5 in the online appendix.

paper are concentrated in sectors over which incumbents have some control. This leads to the second part of the argument outlined above: showing that employment cycles vary in ways predicted by incumbents' incentives.

6.2 The cycles are stronger when incumbents expect greater electoral competition

We next examine elections with different levels of political competition to establish that politicians attempt to temporarily increase employment levels before elections when they have electoral incentives to do so. In particular, the analysis considers two sources of variation in political competition: unopposed candidates, and term-limited incumbents who are trying to transfer the office to a family member. In the former case there are no electoral cycles, while in the latter, cycles are particularly strong.

First, as there are no write-in candidates in the Philippines, unopposed candidates need only one vote to be elected. Candidates have to declare their intention to run a few months before an election, so incumbents know when they are running unopposed well ahead of time, and can therefore refrain from costly manipulation of the labor market. The results of Table 3 shows that, consistent with politically induced cycles, they are concentrated in municipalities where the incumbent (or one of his or her relatives) faced at least one challenger.

Second, although mayors face a three-term limit, they often circumvent this rule by having a family member run for the office after them (Querubin, forthcoming). Forced open-seat races – elections in which the incumbent is unable to run because she is term limited – tend to be more competitive. As a result, a relative of an incumbent running in a forced open-seat race is likely to face stronger opposition than an incumbent running for re-election. Therefore, cycles should be pronounced in forced open-seat races, especially when an incumbent's relative is running. Consistent with this prediction, the results of Table 4 indicate that the increase in the employment rate is twice as large in forced open-seat races when one of the incumbent's relatives is running.

The fact that the point estimate for pre-election quarters when the incumbent is not term limited (1.09) is only half of that for term-limited incumbents with a relative running ($0.98 + 1.52 = 2.6$) might appear surprising at first. One would expect incumbents to exert more effort to benefit themselves than to benefit one of their relatives. However, Querubin (2011) argues that strong challengers often wait until the incumbent is term limited, so the relatives face much more competitive races. In addition, employment in pre-election quarters is higher even

when an incumbent is term limited but does not have a relative running – perhaps because the incumbent is still associated with one of the candidates. These results are consistent with the view that incumbents exert efforts to benefit their anointed successor.

The employment cycles detected in Section 5 are stronger ahead of elections in which the incumbent or one of her relatives expect to face stronger competition. This, in addition to the fact that cycles are non-existent when the incumbent runs unopposed, provides powerful evidence that these cycles are driven by the political incentives and actions of incumbents.

7 Are the cycles detrimental to development?

A remaining question is whether the political business cycles identified in the paper are detrimental to development. Due to the difficulties in identifying exogenous sources of variation in the strength of the political business cycles, we can only provide *suggestive* evidence that is consistent with this interpretation. We proceed in two steps. First, using data for 2003 - 2008, for each municipality in the sample, we run a quarterly-level regression of the share of the working-age population that is employed on quarter dummies and our dummies for the two pre-election quarters and the two post-election quarters. More specifically, for each municipality m , we estimate:

$$Y_{mqt} = \alpha_m E_t^{pre} + \beta_m E_t^{post} + v_{mq} + w_{mqt} \quad (2)$$

where Y_{mqt} is employment rate in municipality m in quarter q at time t , E_t^{pre} is a dummy equal to one for the January and April waves of 2004 and 2007, E_t^{post} is a dummy equal to one for the July and October waves of 2004 and 2007, v_{mq} is a quarter-specific unobservable and w_{mqt} is the usual idiosyncratic error term.

For the remainder of the analysis, we use the point estimate on the pre-election quarters ($\widehat{\alpha}_m$) as our municipality-specific measure of cycle intensity.¹⁵ Consistent the results discussed so far, the average municipal-level cycle is .91 percentage points with a median of 1.10 percentage points.

Second, we run municipal-level regressions of the share of the working-age population that was employed at the end of the sample period (in 2009) on our measure of cycle intensity:

¹⁵We obtain qualitatively similar results if we use the difference between the point estimates on the pre-election quarters and the point estimates on the post-election quarters ($\widehat{\alpha}_m - \widehat{\beta}_m$) instead.

$$Y_m^{2009} = a(\widehat{\alpha}_m) + bX_m + \epsilon_m \quad (3)$$

where Y_m^{2009} is average employment in municipality m over the four 2009 quarters, X_m is a vector of municipal characteristics and ϵ_m is the usual idiosyncratic error term. The parameter of interest is a . To facilitate interpretation we normalise the coefficients ($\widehat{\alpha}_m$) so they are mean zero and standard deviation one. This allows us to test whether the cycles are detrimental to development by lowering employment levels.

There is a negative correlation between the strength of the cycles in the 2004 and 2007 elections and employment levels in 2009 (Table 5). A one-standard-deviation increase in the strength of the cycle translates into a 1.2-percentage-point drop in employment levels in 2009. This correlation is robust to controlling for province fixed-effects, lagged employment rate, the 2009 share of the respondents that were male, the 2009 average education levels of male respondents, the 2009 average education levels of female respondents, the incumbent's vote margin in 2007, whether the incumbent is term limited and the number of candidates in the mayoral race in 2007.

8 Conclusion

A balanced panel of 1,143 Phillipine municipalities over 26 quarters between 2003 and 2009 reveals the existence of within-year political employment cycles. In 2004 and 2007, employment increased in the two pre-electoral quarters and dropped sharply after the elections. These cycles are more pronounced in municipalities where the incumbent is term limited and trying to transfer the mayoral position to one of his or her relatives, and less pronounced in municipalities where the incumbent faces no challengers. This, along with other evidence compiled in this paper and the online appendix, strongly suggests that these cycles in employment are caused by incumbent politicians shifting resources within a fiscal year to increase their chance of re-election.

The paper suggests a number of potentially fruitful areas for further research. First, it is important to understand how politicians manage to increase employment ahead of elections if one would like to reduce welfare-decreasing political manipulation of the economy. For example, are politicians putting pressure on bureaucrats to increase the rate of hiring private contractors? Second, does increasing the magnitude of employment shifting increase incumbent vote share?

How does this compare to the ability to shift resources across years within a term? Third, how does increased short-term economic activity translate into more votes? For example, do voters who are employed on short-term pre-election projects feel the need to repay the incumbent with their support? Or do they interpret cycles as a signal of the incumbent's type? Fourth, and finally, are voters aware that these effects are only short term, and overall detrimental to the economy? Does making them aware of this reduce the magnitude of electoral cycles?

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Figure 1: Difference between Q3/Q4 and Q1/Q2 employment rates in election years and in non-elections years

Table 1: Testing for political business cycles: annual vs. quarterly data

Dependent Variable: Employment Rate				
Panel A : Annual data				
Election year	0.17 (0.13)	0.13 (0.13)	0.13 (0.14)	0.13 (0.15)
Municipal fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Time trends	No	Yes	Region	Province
Observations	8,001	7,893	7,893	7,893
R-squared	0.82	0.83	0.83	0.84
Panel B: Quarterly data				
Pre-election quarters	0.90*** (0.31)	0.87*** (0.33)	0.88*** (0.32)	0.88*** (0.26)
Post-election quarters	-0.46** (0.22)	-0.46** (0.22)	-0.49** (0.23)	-0.49** (0.20)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	29,710	29,278	29,278	29,278
R-squared	0.63	0.64	0.64	0.65

Notes: The regressions in Columns 2–4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and per capita fiscal transfers. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table 2: Testing for political business cycles in various sectors and contract types

Dependent Variable: Employment Rate in	Public Sector			Private Sector		
	Overall	Casual	Permanent	Overall	Casual	Permanent
Pre-election quarters	0.15* (0.09)	0.03 (0.05)	0.12** (0.05)	0.73*** (0.26)	1.58** (0.75)	-0.85 (0.63)
Post-election quarters	-0.00 (0.04)	0.03 (0.05)	-0.04 (0.04)	-0.48** (0.21)	-0.45 (0.65)	-0.04 (0.76)
Municipal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic time trends	Province	Province	Province	Province	Province	Province
Observations	29,278	29,278	29,278	29,278	29,278	29,278
R-squared	0.62	0.67	0.27	0.60	0.41	0.55

Notes: All regressions include controls for, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population, per capita fiscal transfers, a dummy for whether or not the previous municipal election led to a change in local leadership, and province-specific quadratic time trends. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table 3: Are political business cycles present when the incumbent runs unopposed?

Dependent Variable: Employment Rate				
Pre-election quarters X				
Opposed	0.99*** (0.27)	0.92*** (0.26)	0.92*** (0.27)	0.92*** (0.26)
Unopposed	0.34 (0.94)	0.19 (0.91)	0.23 (0.83)	0.31 (0.71)
Post-election quarters X				
Opposed	-0.40* (0.22)	-0.42** (0.22)	-0.44* (0.22)	-0.44** (0.20)
Unopposed	-0.84 (0.67)	-0.96 (0.67)	-0.90 (0.78)	-0.81 (0.61)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	27,581	27,189	27,189	27,189
R-squared	0.63	0.64	0.64	0.65

Notes: The regressions in Columns 2–4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and per capita fiscal transfers. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table 4: Are political business cycles stronger when a relative of a term-limited incumbent runs?

Dependent Variable: Employment Rate				
Pre-election quarters X				
Non-term limited	1.12** (0.46)	1.03** (0.46)	1.02** (0.49)	1.09** (0.47)
Term limited	1.16*** (0.30)	1.04*** (0.30)	1.06*** (0.34)	0.98*** (0.31)
Family running	-0.32 (0.36)	-0.30 (0.37)	-0.30 (0.43)	-0.35 (0.39)
Term limited & Family running	1.39* (0.84)	1.44 (0.89)	1.51 (1.09)	1.52* (0.83)
Post-election quarters X				
Non-term limited	-0.13 (0.41)	-0.16 (0.41)	-0.20 (0.43)	-0.12 (0.42)
Term limited	-0.37 (0.37)	-0.40 (0.37)	-0.36 (0.39)	-0.40 (0.34)
Family running	-0.41 (0.35)	-0.41 (0.37)	-0.40 (0.39)	-0.47 (0.38)
Term limited & family running	0.72 (1.16)	0.82 (1.32)	0.90 (1.44)	0.86 (1.08)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	27,581	27,189	27,189	27,189
R-squared	0.63	0.64	0.64	0.65

Notes: The regressions in Columns 2–4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and per capita fiscal transfers. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table 5: Correlations between electoral cycles and employment

Dependent Variable: Employment Rate in 2009					
Electoral cycle	-0.012*** (0.0036)	-0.012*** (0.003)	-0.018*** (0.003)	-0.019*** (0.003)	-0.013*** (0.003)
Province fixed effects	No	Yes	Yes	Yes	Yes
Employment rate in 2003	No	No	Yes	Yes	Yes
Education controls	No	No	No	Yes	Yes
Electoral controls	No	No	No	No	Yes
Observations	1,132	1,132	1,132	1,132	1,056
R-squared	0.011	0.510	0.619	0.622	0.702

Notes: Regressions in Columns 2–5 include province fixed effects. In Column 3–5, the regressions control for the 2003 share of the working-age population that has a job in the week before the surveys. In Columns 4–5, regressions control for the share of the respondents that were male, the average education levels of male respondents and the average education levels of female respondents. In Column 5, the regression controls for the incumbent's vote margin in 2007, whether the incumbent is term limited and the number of candidates in the mayoral race in 2007. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Online Appendix

Robustness checks

We now present results from a number of robustness checks. First, we assume a dynamic model of the form:

$$Y_{ijt} = \sum_{p=1}^P \alpha_p Y_{ijt-p} + \beta X_{ijt} + u_{ij} + w_{ijt}. \quad (4)$$

We estimate Equation (4) for $P = 1, 2, 3, 4$ using the fixed-effects estimator. The results, available in Panel A of Table A.6, are consistent with previous results. As an additional robustness check, to deal with the fact that the lags of the dependent variable are not exogenous, we also estimate Equation (4) using either the one-step or two-step GMM (Arellano and Bond, 1991; Blundell and Bond, 1998). Again, the results, available in Panels B and C of Table A.6, confirm the previous set of results.

Second, to deal with concerns that our results are driven by particular observations, we estimate Equation (1) on sub-samples that exclude outliers. We focus on outliers along three distributions: the quarterly*municipality share of the working-age population that is employed, its municipal average (over the sample period) and the municipal coefficient of variation (estimated over the sample period). The results, presented in Table A.7, are consistent with those discussed previously.¹⁶ There is an increase in employment in the two pre-election quarters followed by a decline in the two post-election quarters. The only exception is the coefficient on the post-election dummy in the sample that excludes the top and bottom 10 percent of the quarterly*municipality share of the working-age population that is employed. The coefficient is no longer significant, but the null hypothesis that it is equal to its value on the full sample is not rejected at the usual level of confidence (p-value = 0.162).

Third, we also present results from unweighted regressions (Solon, Haider and Wooldridge, 2013). The results, available in Column 1 of Table A.10, are consistent with those obtained previously. Fourth, the sample includes municipalities in the Autonomous Region of Muslim Mindanao (ARMM), which has been affected by conflict and thus the quality of public statistics might be lower there. To check that this is not affecting the main results, we estimate Equation (1) on the non-ARMM sample with and without weights. The results are consistent with those

¹⁶We obtain similar results when we exclude the top and bottom of the distributions separately (Tables A.8 and A.9).

obtained previously (Columns 2 and 3 of Table A.10).

Fifth, to control for potential variations in the degree of seasonality across municipalities, we also estimate Equation (1) using quarter/municipality fixed effects on samples with and without ARMM as well as with and without weights. Including this alternative set of fixed effects does not affect our results (Columns 4–7 of Table A.10).

Overall, the results discussed earlier are robust to alternative specifications, alternative estimation samples and estimation procedures. This reinforces our confidence that they are capturing the presence of PBCs in Philippine municipalities.

Ruling out alternative explanations of our findings

While the findings discussed in the paper provide strong evidence that our results are caused by incumbents' actions to increase their chances of re-election, some could argue that the rise in employment levels might simply reflect temporary jobs created to conduct the election. This seems unlikely, as the increase is larger for private sector employment than it is for public sector employment. In addition, the scale of the increase is difficult to reconcile with the number of jobs needed to organize elections. For example, the point estimates for 2004 suggest that the pre-election increase in employment represents 1.1 percent of registered voters. Yet since most election-related jobs, such as poll workers, are performed by existing civil servants (usually public school teachers), and there are no last-minute surveys in the month before an election, this level of job creation cannot be explained by hiring election workers.

We also use employment sector data to test whether the cycles are driven by sectors that could be differently affected by campaign activities. For example, one can imagine that the demand for printing and publishing services goes up as candidates print out campaign materials. Such an increase would be unrelated to PBCs, as these cycles would not be driven solely by incumbents' actions. We estimate Equation (1), where Y_{ijqt} is the share of the working-age population that is employed in sectors that may be affected by the election.¹⁷ The results reported in Panel A of Table A.11 do not suggest that our results merely capture an increase in economic activity due to campaigning. The point estimates are not statistically significant, and are orders of magnitude smaller than the point estimates obtained using the full employment

¹⁷These sectors are: manufacture of textiles, manufacture of apparel, manufacture of paper and paper products, publishing, printing and reproduction of recorded media, and activities of membership organizations. These sectors were selected because they either produce goods and services that are used during the campaign (e.g., flyers or promotional t-shirts) or because they include political party employees.

rate variables.

A potential threat to the explanation that the post-electoral drop in employment is due to a within-year reallocation of resources is that incoming officials are learning how to use the bureaucracy, which might delay government spending. If learning about bureaucratic procedures explains the negative impact on employment in the post-election period, drops in employment should be concentrated in municipalities in which a challenger won. Yet the results in Panel B of Table A.11 show that this is not the case.

Table A.1: Descriptive statistics

	(1)	(2)
	Mean	Std. Dev.
Share of working-age population with a job in:		
Overall	59.14	(9.57)
Public sector	4.61	(3.15)
Private sector	54.53	(10.14)
Public sector (casual)	0.55	(0.91)
Public sector (permanent)	4.06	(2.88)
Private sector (casual)	44.34	(11.25)
Private sector (permanent)	10.19	(7.96)
Construction (casual)	1.61	(1.81)
Construction (permanent)	3.43	(2.67)
Other variables:		
No education	2.16	(4.60)
Some primary	13.94	(10.86)
Primary graduate	14.50	(7.73)
Some secondary	16.93	(5.53)
Secondary graduate	24.27	(9.34)
Some college +	28.19	(13.68)
Population	82,353	(207,676)
Share female	0.50	(0.04)
Age	35.82	(2.23)
Observations	29,715	

Table A.2: Testing for political business cycles: annual vs. quarterly data

Dependent Variable: Employment Rate				
Panel A : Annual data				
Year: 2004	0.17 (0.19)	0.04 (0.16)	0.04 (0.17)	0.05 (0.17)
Year: 2007	0.17 (0.16)	0.21 (0.17)	0.21 (0.19)	0.20 (0.20)
Municipal fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Time trends	No	Yes	Region	Province
Observations	8,001	7,893	7,893	7,893
R-squared	0.82	0.83	0.83	0.84
Panel B: Quarterly data				
Pre-election quarters (2004)	0.78** (0.39)	0.73* (0.44)	0.77 (0.51)	0.78 (0.50)
Pre-election quarters (2007)	1.00*** (0.34)	0.98*** (0.36)	0.96*** (0.37)	0.96*** (0.30)
Post-election quarters (2004)	-0.36 (0.27)	-0.41 (0.31)	-0.41 (0.37)	-0.41 (0.33)
Post-election quarters (2007)	-0.56** (0.24)	-0.52** (0.23)	-0.55** (0.26)	-0.55** (0.24)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	29,710	29,278	29,278	29,278
R-squared	0.63	0.64	0.64	0.65

Notes: Regressions in Columns 2–4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and per capita fiscal transfers. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table A.3: Testing for political business cycles: annual vs. quarterly data [excluding 2003]

Dependent Variable: Employment Rate				
Panel A : Annual data				
Election Year	0.20 (0.15)	0.15 (0.16)	0.15 (0.17)	0.14 (0.18)
Municipal fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Time trends	No	Yes	Region	Province
Observations	6,858	6,750	6,750	6,750
R-squared	0.86	0.86	0.87	0.87
Panel B - Quarterly Data				
Pre-election quarters	0.89*** (0.32)	0.84** (0.34)	0.83*** (0.31)	0.83*** (0.27)
Post-election quarters	-0.49** (0.21)	-0.53*** (0.19)	-0.53** (0.23)	-0.53*** (0.19)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	27,425	26,993	26,993	26,993
R-squared	0.63	0.64	0.65	0.65

Notes: Regressions in Columns 2–4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and per capita fiscal transfers. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table A.4: Testing for political business cycles in hours worked

Dependent Variable: Average hours worked for those employed				
Panel A: All sectors				
Pre-election quarters	-0.56 (0.48)	-0.56 (0.50)	-0.57 (0.51)	-0.58 (0.51)
Post-election quarters	0.13 (0.29)	0.12 (0.31)	0.15 (0.31)	0.15 (0.31)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	29,710	29,278	29,278	29,278
R-squared	0.71	0.71	0.71	0.72
Panel B: Public sector				
Pre-election quarters	-0.84 (0.69)	-1.21 (0.79)	-1.19 (0.77)	-1.20* (0.69)
Post-election quarters	0.28 (0.28)	0.09 (0.38)	0.04 (0.39)	0.05 (0.37)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	25,442	25,083	25,083	25,083
R-squared	0.31	0.31	0.31	0.32
Panel C: Private sector				
Pre-election quarters	-0.54 (0.47)	-0.50 (0.50)	-0.51 (0.50)	-0.52 (0.51)
Post-election quarters	0.12 (0.30)	0.12 (0.30)	0.17 (0.31)	0.17 (0.30)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	29,710	29,278	29,278	29,278
R-squared	0.70	0.70	0.71	0.71

Notes: Regressions in Columns 2–4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and per capita fiscal transfers. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table A.5: Testing for political business cycles in wages

Dependent Variable: Average log wage for those employed				
Panel A: All sectors				
Pre-election quarters	-0.07** (0.04)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Post-election quarters	-0.03 (0.04)	-0.00 (0.01)	0.00 (0.01)	0.00 (0.01)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	28,813	28,398	28,398	28,398
R-squared	0.70	0.76	0.76	0.76
Panel B: Public sector				
Pre-election quarters	-0.07** (0.03)	-0.02 (0.01)	-0.02 (0.02)	-0.02 (0.02)
Post-election Quarters	-0.04 (0.03)	-0.02 (0.02)	-0.02 (0.02)	-0.02 (0.02)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	23,324	23,002	23,002	23,002
R-squared	0.26	0.29	0.29	0.30
Panel C: Private sector				
Pre-election quarters	-0.07* (0.04)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Post-election quarters	-0.03 (0.04)	0.00 (0.01)	0.01 (0.01)	0.01 (0.01)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	28,099	27,694	27,694	27,694
R-squared	0.77	0.80	0.81	0.81

Notes: Regressions in Columns 2–4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and per capita fiscal transfers. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table A.6: Robustness checks: controlling for lagged employment rate

Dependent Variable: Employment Rate				
# of Lags:	1	2	3	4
Panel A: Fixed effects				
Pre-election quarters	0.74** (0.30)	0.80*** (0.28)	0.74** (0.34)	1.10*** (0.39)
Post-election quarters	-0.59*** (0.19)	-0.58*** (0.21)	-0.53** (0.22)	-0.57** (0.25)
Observations	28,129	26,982	25,836	24,690
R-squared	0.66	0.66	0.66	0.67
Panel B: Two-Step GMM				
Pre-election quarters	0.67*** (0.15)	0.78*** (0.16)	0.82*** (0.20)	1.00*** (0.22)
Post-election quarters	-0.56*** (0.14)	-0.69*** (0.15)	-0.24 (0.17)	-0.59*** (0.18)
Observations	26,982	25,836	24,690	23,545
Panel C: One-Step GMM				
Pre-election quarters	0.73*** (0.15)	0.78*** (0.14)	0.83*** (0.17)	1.14*** (0.18)
Post-election quarters	-0.69*** (0.13)	-0.77*** (0.14)	-0.63*** (0.15)	-0.87*** (0.15)
Observations	26,982	25,836	24,690	23,545

Notes: Results from municipal fixed-effects (Panel A) and GMM regressions (Panels B and C) with quarterly data (2003 Q3 to 2009 Q4). The dependent variable is the share of the working-age population that has a job in the week before the survey. The dependent variable is lagged once (Column 1), twice (Column 2), three times (Column 3) and four times (Column 4). All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population, per capita fiscal transfers and province-specific quadratic time trend. In Panel A, the standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table A.7: Robustness checks: excluding outliers

Dependent Variable: Employment Rate						
Exc. Outliers:	1%	2%	3%	4%	5%	10%
Panel A: quarterly*municipality distribution						
Pre-election quarters	0.87*** (0.27)	0.80*** (0.27)	0.81*** (0.26)	0.81*** (0.26)	0.79*** (0.26)	0.69*** (0.25)
Post-election quarters	-0.45** (0.20)	-0.42** (0.20)	-0.39* (0.21)	-0.35 (0.21)	-0.35* (0.21)	-0.24 (0.18)
Observations	28,705	28,118	27,534	26,947	26,362	23,425
R-squared	0.64	0.63	0.63	0.62	0.61	0.59
Panel B: municipal average distribution						
Pre-election quarters	0.87*** (0.26)	0.88*** (0.26)	0.89*** (0.26)	0.87*** (0.26)	0.88*** (0.26)	0.84** (0.33)
Post-election quarters	-0.49** (0.20)	-0.48** (0.20)	-0.49** (0.20)	-0.50** (0.20)	-0.49** (0.22)	-0.47* (0.25)
Observations	28,698	28,138	27,522	26,950	26,334	23,406
R-squared	0.63	0.62	0.61	0.59	0.58	0.50
Panel C: Municipal coefficient of variation distribution						
Pre-election quarters	0.85*** (0.26)	0.88*** (0.29)	0.91*** (0.28)	0.93*** (0.28)	0.88*** (0.29)	0.85*** (0.32)
Post-election quarters	-0.49** (0.22)	-0.53** (0.23)	-0.51** (0.25)	-0.52** (0.25)	-0.59** (0.24)	-0.58** (0.25)
Observations	28,697	28,125	27,506	26,938	26,326	23,394
R-squared	0.64	0.64	0.64	0.63	0.63	0.63

Notes: Results from municipal fixed-effects regressions with quarterly data (2003 Q3 to 2009 Q4). The dependent variable is the share of the working-age population that has a job in the week before the survey. The sample excludes the top and bottom 1 percent of the reference distribution (Column 1), the top and bottom 2 percent (Column 2), the top and bottom 3 percent (Column 3), the top and bottom 4 percent (Column 4), the top and bottom 5 percent (Column 5) and the top and bottom 10 percent (Column 6). In Panel A, the reference distribution is the share of the working-age population that has a job in the week before the survey. In Panel B, the reference distribution is the municipal-level average of the share of the working-age population that has a job in the week before the survey across all 26 quarters. In Panel C, the reference distribution is the municipal coefficient of variation of the share of the working-age population that has a job in the week before the survey across all 26 quarters. All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population, per capita fiscal transfers and province-specific quadratic time trends. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table A.8: Robustness Checks: Excluding Outliers (top)

Dependent Variable: Employment Rate						
Exc. Outliers:	1%	2%	3%	4%	5%	10%
Panel A: quarterly*municipality distribution						
Pre-election quarters	0.87*** (0.27)	0.85*** (0.27)	0.85*** (0.27)	0.85*** (0.26)	0.86*** (0.27)	0.82*** (0.27)
Post-election quarters	-0.47** (0.20)	-0.46** (0.20)	-0.45** (0.20)	-0.44** (0.21)	-0.44** (0.21)	-0.36* (0.22)
Observations	28,998	28,706	28,413	28,121	27,831	26,360
R-squared	0.64	0.63	0.62	0.61	0.61	0.58
Panel B: Municipal average distribution						
Pre-election quarters	0.87*** (0.26)	0.88*** (0.26)	0.88*** (0.26)	0.88*** (0.26)	0.88*** (0.27)	0.85*** (0.27)
Post-election quarters	-0.49** (0.20)	-0.48** (0.20)	-0.48** (0.20)	-0.48** (0.20)	-0.47** (0.20)	-0.44** (0.19)
Observations	28,982	28,704	28,396	28,110	27,802	26,344
R-squared	0.64	0.63	0.62	0.61	0.60	0.57
Panel C: Municipal coefficient of variation distribution						
Pre-election quarters	0.87*** (0.26)	0.86*** (0.26)	0.86*** (0.25)	0.86*** (0.25)	0.86*** (0.25)	0.85*** (0.25)
Post-election quarters	-0.49** (0.20)	-0.50** (0.20)	-0.50** (0.20)	-0.48** (0.20)	-0.47** (0.20)	-0.40** (0.20)
Observations	28,979	28,693	28,382	28,096	27,796	26,322
R-squared	0.65	0.65	0.66	0.66	0.66	0.67

Notes: Results from municipal fixed-effects regressions with quarterly data (2003 Q3 to 2009 Q4). The dependent variable is the share of the working age population with a job in the week before the survey. The sample excludes the top 1 percent of the reference distribution (Column 1), the top 2 percent (Column 2), the top 3 percent (Column 3), the top 4 percent (Column 4), the top 5 percent (Column 5) and the top 10 percent (Column 6). In Panel A, the reference distribution is the share of the working-age population that has a job in the week before the survey. In Panel B, the reference distribution is the municipal-level average of the share of the working-age population that has a job in the week before the survey across all 26 quarters. In Panel C, the reference distribution is the municipal coefficient of variation of the share of the working-age population that has a job in the week before the survey across all 26 quarters. All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population, per capita fiscal transfers and province-specific quadratic time trends. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table A.9: Robustness checks: excluding outliers (bottom)

Dependent Variable: Employment Rate						
Exc. Outliers:	1%	2%	3%	4%	5%	10%
Panel A: quarterly*municipality distribution						
Pre-election quarters	0.88*** (0.26)	0.83*** (0.25)	0.84*** (0.28)	0.84*** (0.26)	0.81*** (0.26)	0.74*** (0.24)
Post-election quarters	-0.47** (0.20)	-0.45** (0.20)	-0.43** (0.21)	-0.40* (0.22)	-0.40* (0.22)	-0.37** (0.19)
Observations	28,985	28,690	28,399	28,104	27,809	26,343
R-squared	0.65	0.65	0.65	0.66	0.66	0.66
Panel B: Municipal average distribution						
Pre-election quarters	0.87*** (0.26)	0.88*** (0.26)	0.88*** (0.26)	0.87*** (0.26)	0.88*** (0.25)	0.87*** (0.32)
Post-election quarters	-0.49** (0.20)	-0.49** (0.20)	-0.49** (0.20)	-0.50** (0.21)	-0.51** (0.22)	-0.52** (0.26)
Observations	28,994	28,712	28,404	28,118	27,810	26,340
R-squared	0.64	0.64	0.64	0.63	0.63	0.60
Panel C: Municipal coefficient of variation distribution						
Pre-election quarters	0.86*** (0.26)	0.90*** (0.28)	0.93*** (0.29)	0.95*** (0.29)	0.90*** (0.30)	0.90*** (0.32)
Post-election quarters	-0.48** (0.22)	-0.51** (0.23)	-0.50** (0.24)	-0.53** (0.24)	-0.61*** (0.23)	-0.70*** (0.24)
Observations	28,996	28,710	28,402	28,120	27,808	26,350
R-squared	0.64	0.63	0.63	0.62	0.62	0.60

Notes: Results from municipal fixed-effects regressions with quarterly data (2003 Q3 to 2009 Q4). The dependent variable is the share of the working-age population that has a job in the week before the survey. The sample excludes the bottom 1 percent of the reference distribution (Column 1), the bottom 2 percent (Column 2), the bottom 3 percent (Column 3), the bottom 4 percent (Column 4), the bottom 5 percent (Column 5) and the bottom 10 percent (Column 6). In Panel A, the reference distribution is the share of the working-age population that has a job in the week before the survey. In Panel B, the reference distribution is the municipal-level average of the share of the working-age population that has a job in the week before the survey across all 26 quarters. In Panel C, the reference distribution is the municipal coefficient of variation of the share of the working-age population that has a job in the week before the survey across all 26 quarters. All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population, per capita fiscal transfers and province-specific quadratic time trends. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table A.10: Robustness checks: alternative specifications and estimation samples

Dependent Variable: Employment Rate									
Pre-election quarters	0.80***	0.85***	0.80***	0.87***	0.84***	0.81**	0.80**		
	(0.31)	(0.25)	(0.28)	(0.32)	(0.30)	(0.34)	(0.37)		
Post-election quarters	-0.71***	-0.48**	-0.69***	-0.48**	-0.47**	-0.71**	-0.70**		
	(0.27)	(0.20)	(0.25)	(0.22)	(0.23)	(0.30)	(0.33)		
Municipal fixed effects	Yes	Yes	Yes	No	No	No	No		
Quarter*municipal fixed effects	No	No	No	Yes	Yes	Yes	Yes		
Weights	No	Yes	No	Yes	Yes	No	No		
Includes ARMM	Yes	No	No	Yes	No	Yes	No		
Observations	29,278	27,675	27,675	29,278	27,675	29,278	27,675		
R-squared	0.60	0.65	0.59	0.70	0.70	0.66	0.65		

Notes: Results from municipal fixed-effects regressions with quarterly data (2003 Q3 to 2009 Q4). The dependent variable is the share of the working-age population that has a job in the week before the survey. In Columns 1–3 municipal fixed effects are used. In Columns 4–7 municipal*quarter fixed effects are used. In Columns 1, 3, 6 and 7 the regressions are estimated without weights. In Columns 2, 5 and 7, the sample excludes municipalities from ARMM. All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population, per capita fiscal transfers and province-specific quadratic time trends. The dependent variable is lagged once in Columns 1, 3 and 5 and twice in Columns 2, 4 and 6. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.

Table A.11: Ruling out alternative explanations for PBCs

	(1)	(2)	(3)	(4)
Panel A: Testing for PBCs of campaign workers				
Pre-election quarters	0.16 (0.11)	-0.00 (0.04)	0.00 (0.04)	0.00 (0.04)
Post-election quarters	0.09 (0.11)	0.02 (0.05)	0.01 (0.05)	0.01 (0.05)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	29,710	29,278	29,278	29,278
R-squared	0.71	0.72	0.73	0.73
Panel B: Showing that the post-election decline is not driven by new incumbents				
Pre-election quarters	0.85*** (0.33)	0.84** (0.34)	0.86*** (0.33)	0.86*** (0.26)
Post-election quarters	-0.49** (0.21)	-0.48** (0.21)	-0.50** (0.23)	-0.50** (0.19)
Post-election quarters X New incumbent	0.15 (0.28)	0.12 (0.30)	0.08 (0.30)	0.07 (0.30)
Municipal fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes
Quadratic time trends	No	Yes	Region	Province
Observations	29,710	29,278	29,278	29,278
R-squared	0.63	0.64	0.64	0.65

Notes: Regressions in Columns 2–4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and per capita fiscal transfers. The standard errors (in parentheses) account for potential correlations with the time periods and province. * denotes significance at the 10%, ** 5% and *** 1% levels.