Australian National University

Working Papers in Trade and Development

Research Note: The Impact of Pre-Electoral Coalitions on Mayoral Election Outcomes in Indonesia

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August 2018
Working Paper No. 2018/20

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Abstract

The extent to which pre-electoral coalitions (PECs) influence executive elections in presidential systems has not been subject to rigorous empirical study. This paper uses regression discontinuity methods to identify the causal effect of PEC size on mayoral election outcomes in Indonesia. The study finds that mayoral candidates backed by PECs comprising political parties that control council seat shares exceeding first-round electoral vote thresholds are around 18-24 percentage points more likely to win those elections than their counterparts supported by smaller-sized PECs.

Key words

Pre-election coalitions, presidential systems, subnational elections

JEL:

C21, C31, D72

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1. Introduction

Research on pre-electoral coalitions (PECs) is limited, at least as compared to that on governing coalitions (Golder, 2006b; Muller et al, 2008; Chiru, 2015). Most work on PECs has focused on determining political party rationales for forming coalitions. In parliamentary systems, research has consistently shown that political parties are motivated to join PECs in order to both influence policy and secure office position (Golder, 2005; Golder, 2006a; Shin, 2015). In presidential democracies, recent work has emphasized that the unreliability of executive candidate promises to potential political party partners dampens office incentives for the latter to form PECs. Policy incentives may still obtain, however, at least when policy agreements among PEC members are made public during the campaign (Kellam, 2015).

The electoral impact of PECs has received less attention. Research on parliamentary systems has studied PEC effects on voter turnout and voter choice in legislative elections (Tillman, 2015; Plescia, 2017; Gschwend and Hooghe, 2008; Fortunato, 2017). In presidential frameworks, the common assumption is that PECs can assist executive candidates in winning elections and a reasonable amount of largely descriptive evidence and/or qualitative analysis has been offered to support the assumption (Golder, 2006b; Machado, 2009; Kellam, 2015; Shin, 2015).[1] However, PEC impact on executive electoral outcomes has so far not be been empirically examined in a thorough and rigorous manner.

This note explores the causal impact of PEC size on mayoral election outcomes in Indonesia. Indonesia is an interesting case in this context. Local executive elections are hotly contested, with an average of five candidates competing in any given poll; and party systems are very fragmented: the average absolute and effective numbers of political parties represented on local councils are around eleven and eight, respectively. Mayoral candidate

competition to establish PECs is intense. Nevertheless, the extent to which PECs assist candidates in winning elections is quite uncertain. To the best of my knowledge, this is the first empirical investigation to assess the causal effects of PECs on executive election outcomes anywhere.

2. Local political context

Indonesia is a unitary country, comprising central, provincial, and local governments.[2] There are currently 34 provinces and 535 districts. Provinces and districts are autonomous, non-overlapping units of government, and both are directly accountable to the central government. Districts are not formally answerable to the provinces within which they are located, although the latter play a coordinating role among districts within their borders.

Prior to 1999, subnational government executives were appointed and subnational legislatures (*Dewan Perwakilan Rakyat Daerah,* DPRD) were formed by central government. In 1999, the popular election of DPRD representatives was introduced. Starting in that year, DPRDs also began to appoint subnational government heads, as central government selected executives' terms expired. Indonesia initiated direct elections of subnational government heads in 2005. Direct executive elections have since been implemented in a gradual manner, as DPRD-appointed heads' terms finished.

Local executive elections are based on a two-round system. Between 2005 and 2008 a mayoral candidate, along with his/her vice mayoral candidate, needed at least 25 percent of the vote to win office in the first round; national legislation increased the first-round vote share threshold to 30 percent in 2009. If no candidate pair gained the minimum vote share in the first round, then the two candidate pairs with the highest vote shares contested the second-round run-off (Hillman, 2011). The two-round election system was discontinued in 2015, at which point single round plurality voting was adopted.

DPRD elections are held across multiple voting districts (*daerah pilihan—dapils*) within subnational government jurisdictions, where each *dapil* is allocated seats in the DPRD based on its relative population size (given the DPRD's total number of seats). Over the period of study, the number of *dapils* per local jurisdiction ranged from two to seven and the number of seats per *dapil* varied from three to 12. Voting is based on proportional representation and open-list electoral rules. There is no electoral threshold for political parties participating in DPRD polls (Tomsa, 2014).

DPRD members are elected for five-year terms. Since the first polls in 1999, legislative elections have been held in 2004, 2009, and 2014. The exact number of seats allocated to a DPRD is a deterministic and discontinuous function of jurisdiction population size. The legislative electoral system in Indonesia is "permissive": it seeks to encourage strong linkages between politicians and citizens but at the same time facilitates increased political party fragmentation in DPRDs (Tomsa, 2014). Party fragmentation is indeed high at the local level. The average absolute and effective numbers of political parties represented in local councils during the study period were approximately eleven and eight, respectively.

Mayoral candidates form coalitions with political parties represented in councils in the run-up to popular executive elections as a signaling device to voters to maximize the probability of electoral success, and to potentially enable execution of their political and policy agendas post-election. On the other hand, a substantial body of research shows that political parties are often motivated by rent-seeking to join election coalitions: parties make themselves available to the highest candidate-bidder and would-be mayors and their financial backers pay those parties to join PECs. PEC formation at the local level in Indonesia is thus an integral part of the pervasive phenomenon of "money politics" that exists in the country (Vel, 2005; Mietzner, 2007; Buehler & Tan, 2007; Choi, 2007, Aspinall, 2013).

3. Data

I have good quality data on 728 district head first-round elections between 2005 and 2013. This represents about 80 percent of the total number of such elections held over the period. Data on the remaining 20 percent of elections were either missing or of dubious quality and these cases were excluded. I also drop local polls in the province of Aceh, because of its considerably different electoral environment.[3] That leaves data on 709 elections. Of these, only 57 polls were not concluded during the first round.[4] I retain these latter cases in the analysis. The results presented here are robust with respect to their inclusion.

For each of the elections in the final data set, I have information on the number of mayoral candidates who competed in the polls, the share of council seats held by each candidate's PEC, and the winning candidate. Data have been accessed through the General Elections Commission (KPU) and are provided in the Supplementary Online Appendix.

Identification

The objective of this examination is to assess the causal impact of PEC size, as measured by its share of seats on the council, on district executive candidates' chances of winning an election. The share of seats held by a PEC is likely to be endogenous to the probability that a candidate gets elected because of omitted voter preferences, which influence both political party composition of councils, and therefore PECs, and mayoral election outcomes. To accommodate that endogeneity and identify the causal effects of PEC size on mayoral candidates' chances of winning elections I use regression discontinuity (RD) methods.

I employ a sharp RD design with multiple cut-offs, where the latter are defined as a function of the two different electoral share thresholds used to determine first-round winning executive candidates during the period of study, i.e. either 25 or 30 percent. To operationalize the multiple cut-off RD approach, I normalize PEC share of seats around the winning electoral thresholds, by subtracting the relevant first-round threshold from the PEC share of

council seats, to form a single cut-off at zero. In the RD framework, the normalized share of PEC council seats is termed the forcing variable. To the left of the cut-off, candidate PECs have seat shares that are smaller than the first-round winning electoral threshold and to the right of the cut-off, candidate PECs have seat shares that are larger than the threshold.

I assume that PEC council seat shares indicate the proportion of voters that identify with the parties comprising the PEC and thus the extent to which they might be inclined to vote for a mayoral candidate supported by the PEC. I hypothesize that candidates backed by PECs comprising political parties whose normalized seat shares exceed zero are more likely to win elections compared to those with normalized seat shares less than zero. The former should have sufficient voter backing to surpass first round electoral thresholds and win elections. I term PECs with a normalized share of seats greater than zero "treatment PECs".

Following Imbens and Lemieux (2007), define $Y_{ij}(0)$ and $Y_{ij}(1)$ to be a potential election outcome for candidate i in district j where $Y_{ij}(0)$ is the outcome to the left of the cut-off (control) and $Y_{ij}(1)$ is the outcome to the right of the cut-off (treatment). In this case, the impact of the PEC's share of council seats is given by $Y_{ij}(1) - Y_{ij}(0)$. Unfortunately, $Y_{ij}(0)$ and $Y_{ij}(1)$ cannot be observed simultaneously and so attention turns to the average effects, $Y_{ij}(1) - Y_{ij}(0)$, across subgroups of the relevant population. Let $D_{ij} = 0$ if a candidate is in the control group and $D_{ij} = 1$ if s/he is subject to treatment. Observed outcomes, Y_{ij}, are therefore $= Y_{ij}(0)$ if $D_{ij} = 0$ and $= Y_{ij}(1)$ if $D_{ij} = 1$; and the average causal effect of PEC's share of seats, τ, at the cut-off, c=0, is given by:

$$\tau = \mathrm{E}\left[Y_{ij}(1) - Y_{ij}(0) \mid X_{nij} = c \right] = \mathrm{E}\left[(Y_{ij}(1) \mid X_{nij} = c \right] - \mathrm{E}\left[Y_{ij}(0) \mid X_{nij} = c \right] \quad (1)$$

The key identifying assumption in this framework is that $\mathrm{E}[Y_{ij}(1) \mid X_{nij}]$ and $\mathrm{E}[Y_{ij}(0) \mid X_{nij}]$ are continuous in X_{nij}, the normalized PEC share of seats. This implies that all other unobserved determinants of election outcomes, Y_{ij}, are also continuously related to X_{nij}

(Imbens and Lemieux, 2007). The implication allows one to use outcomes just below the cut-off as valid counterfactuals for those just above the cut-off (Skovron and Titiunik, 2015).

The general form of the standard RD estimating equation is:

$$Y_{ij} = \tau D_{ij} + g(X_{nij}) + \mu_{ij} \quad (2)$$

In equation (2), g(X) is a polynomial function of the running variable; μ is the error term; τ is the treatment effect, which is to be estimated; and all other variables are defined as above. If treatment PECs are important for winning elections, then τ should be positive and statistically significant. More specifically, estimated τ gives the candidate's increased chances of winning an election when s/he is backed by a treatment PEC.

I estimate the treatment effect using non-parametric regression techniques within narrow windows (bandwidths) on each side of the cut-off point. In this context, three choices must be made: the degree of the polynomial in the regression equation, the kernel type, and the bandwidth (Calonico, Cattaneo, and Titiunik, 2014). Recent research argues for the use of lower order polynomials (Gelman and Imbens, 2017) and I employ polynomials of degree one and two in the estimation procedures below. I use a triangular kernel (that weights observations closer to the cut-off point more heavily) and choose bandwidths in a data-driven fashion to minimize the coverage error rate of the robust biased-corrected confidence interval, which is preferable for inference purposes. (Calonico, Cattaneo, and Farrell, 2018).

The RD methods described here identify a local average treatment effect (LATE) (Lee and Lemieux, 2010). It is perhaps useful to emphasize the local character of estimated treatment effects. While the internal validity of effects estimated in the described manner is typically argued to be strong, external validity is usually thought to be relatively weak. This suggests that it may be unreasonable to generalize about the impact of a treatment PEC at values of the forcing variable outside a narrow range around the cut-off.

4. Results

For the RD approach to be valid there must be no precise manipulation of the forcing variable—PEC normalized share of council seats—around the cut-off. Since political parties determine the PECs in which they participate it might reasonably be expected that parties would collaborate with one another to establish treatment PECs, which, in turn, might assist them in influencing local policy formulation. [5] The argument in this paper is that local political parties in Indonesia are typically not inspired by policy concerns in forming PECs. They are instead "money motivated". That is, what parties care about is the magnitude of the payoff offered to join a coalition; they are indifferent about PEC seat share. As such, manipulation in this case is improbable.

On the other hand, mayoral candidates are certainly motivated to establish PECs with seat shares that exceed winning electoral thresholds to assist themselves in getting elected. The question in this instance is whether candidates have *precise* control over the PEC seat shares at the cut-off. I argue that they do not have such control. Typically, many candidates run in any single election (on average, just greater than five during the period of study here) and all candidates know the rules of the game, have access to the same information, and attempt to establish as large a PEC as possible.[6] Given candidate competition in this context, it seems unlikely that any single candidate would be able to precisely or completely control its PEC's share of seats at the cut-off. Such control would imply that a candidate with a coalition comprising, say, 29 percent of council seats (relative to a 30 percent threshold, for instance) would easily be able to enlist another party to join the PEC, thereby comprising 31 percent of the seats, or more. This seems implausible in the competitive electoral environment outlined above. All things considered, therefore, I argue that there is no precise manipulation of the forcing variable, PEC share of council seats, by candidates.

I now empirically test that assumption. Figure 1 shows the density distribution of PEC normalized share of seats.[7] Visual inspection does not suggest any significant discontinuity around the cut-off (shown by the vertical line at zero). In any case, a formal test of the null hypothesis that no discontinuity exists at the cut-off, using a procedure developed by Cattaneo, Jansson, and Ma (2016), indicates that the null cannot be rejected. Specifically, the robust bias-corrected test statistic, using a polynomial of degree two, a triangular kernel, jack-knifed standard errors, and allowing for different densities on either side of the cut-off is -0.291 and the p value is 0.771. The evidence therefore implies no manipulation of the forcing variable at the normalized cut-off.[8]

[Figure 1 here]

I continue the treatment effects analysis by examining standard RD plots. Figure 2 shows the plots for the probability that a candidate wins an election relative to its PEC's normalized share of seats on the local council, for polynomials of both degree one and two. Each dot in the plots represents the average value of the election outcome for a data-driven selected range (bin) of the PEC seat share. Attention is drawn to the relationships at the cut-off. The figure shows a reasonably pronounced jump around the cut-off for both plots. I tentatively conclude that treatment PECs may lead to an increase in the probability that a candidate wins an election. The plots are merely suggestive of impacts, however; a firm and more exact conclusion can only be reached after a formal estimation of treatment effects, i.e. using data-driven bandwidths and properly estimated standard errors.

[Figure 2 here]

I now provide formal empirical estimates of PEC treatment effects, as illustrated in the above RD plots, by estimating equation (2). As previously noted, I employ non-parametric estimation procedures, using a triangular kernel, and optimal confidence interval

bandwidths. I estimate effects using polynomials of both degree one and two. Standard errors are clustered at the district level.

Table 1 provides the results. For each of the two regressions the table shows: total observations; optimal bandwidths (measured in PEC seat shares); the number of observations to the left and right of the cut-off that are used in estimation procedures; the robust bias-corrected estimated treatment effect (τ); estimated (cluster robust) standard errors, and an indication of the statistical significance of the estimated treatment effect.

[Table 1 here]

The estimation output confirms the suspected positive impact of treatment PECs on the probability that a candidate wins an election, as illustrated in the RD plots. The results for the non-parametric regression using a polynomial of degree one implies that treatment PECs lead to an 18-percentage point increase in the chances of winning the election. The second-degree polynomial regression results show a slightly larger effect, indicating that treatment PECs cause a 24-percentage point increase in the probability of winning. Both outcomes are statistically significant at the five percent level.

I have also tested the robustness of the above model and the derived results along two dimensions: covariate balance and fake cut-offs, as is common in the RD literature. I find that a large number of potential confounders are balanced around the cut-off and that estimated treatment effects are null across a wide range of fake cut-offs. The tests provide significant support for the identification strategy employed in the analysis and the robustness of the empirical results. Detailed output is provided in the Supplementary Online Appendix.

5. Conclusions

To what extent do PECs influence executive election outcomes in presidential systems? Despite its fundamental relevance, the question has not been subject to rigorous empirical

analysis. This study has investigated the causal impact of PEC size on mayoral election outcomes in Indonesia. The examination finds that mayoral candidates backed by PECs comprising political parties that control council seat shares that exceed first-round electoral vote thresholds are approximately 18 to 24 percentage points more likely to win those elections than their counterparts backed by smaller-sized PECs. Given the dearth of research in this area, it is difficult to judge the degree to which an increase in the probability of winning an election of that estimated magnitude is substantively meaningful or not. More work on the topic would assist in making such a determination.

Notes

[1] On the other hand, Resnick (2011) argues that PECs comprising strictly opposition parties have had little success in reducing presidential incumbents' chances of winning elections in Africa.
[2] I use the term "subnational" to refer to provinces and local governments together and the term "local" to refer to local governments. The latter are also called districts.
[3] Aceh is the only province in Indonesia for which the central government allows the formation of local political parties.
[4] Nationwide, about 93 percent of local executive elections were finalized in the first round during the period of study here.
[5] Office benefits do not exist for political parties at the local level in Indonesia as executive branch administrative positions are staffed by civil servants and not used by mayors as a means of rewarding local councilors or their political parties that join PECs.
[6] The exception to this rule is candidates that run as independents and do not seek to form PECs. Nationwide, independent candidates were allowed to stand for election for the first time in 2008. (Independent candidates were permitted to run for election in the province of Aceh starting in 2006.) Just fewer than 20 percent of mayoral candidates in our sample ran as independents. The results presented here do not change if independent candidates are deleted from the analysis.
[7] The high-density bar on the far left of the figure represents independent candidates who do not form PECs.
[8] The Cattaneo, Jansson, and Ma second generation density test is preferable to the often-used McCrary first generation test. The latter is known to over-reject the null hypothesis of no manipulation due to its practice of pre-binning the data and use of local linear regressions.

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Figures and Table

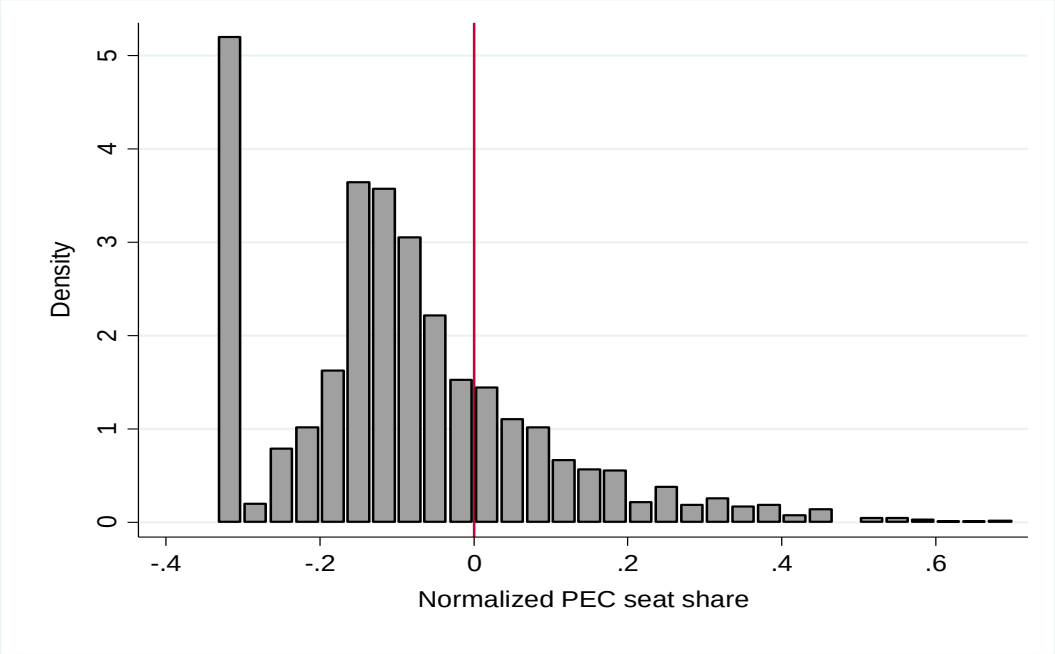

Figure 1. Density of normalized PEC share of seats on council

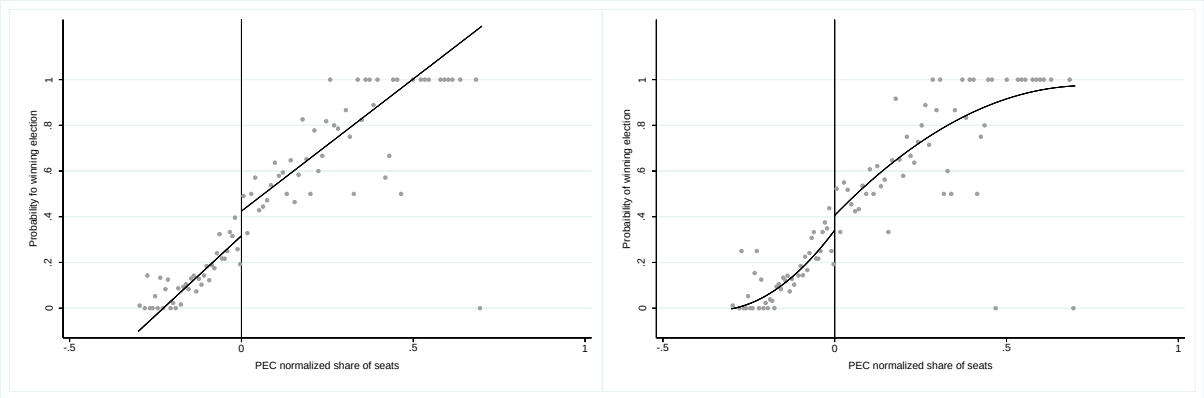

Figure 2. RD plots, polynomials of degree one and two

Table 1 Impact of PEC size on the probability a mayoral candidate wins the election

	Tot. Obs.	Bandwidth	Obs. Left	Obs. Right	τ	SE	
Polynomial degree one	3,007	0.054	307	223	0.184	0.090	**
Polynomial degree two	3,007	0.077	459	286	0.242	0.104	**

The election winner dummy is the dependent variable. PEC share of seats dummy is the treatment variable. Tot Obs is the total number of observations. Selection of bandwidths is data driven and they are measured by PEC seat share. Obs Left and Obs Right are the effective numbers of observations used in estimation. τ is the estimated treatment effect, which is cluster-robust and bias-corrected. ** indicates statistical significance at the 0.05 level.

Supplementary Online Appendix

This appendix presents and briefly discusses two robustness tests related to the empirical results presented in the text: covariate balance and fake cut-offs.

The treatment effects analysis carried out in this study assumes that other potential determinants (confounders) of election outcomes (or, alternatively, placebo outcomes) are balanced around the normalized PEC seat share cut-off. If they were not balanced in this manner, then the identification strategy would be called into question. I test the balance assumption using several important confounders on which data are available: a dummy variable indicating whether a candidate is an incumbent (=1) or not (=0); a dummy designating newly created districts (=1) or not (=0), an ethnic fractionalization index, log intergovernmental transfer revenue of districts, an index of average service access, and log GRDP per capita.

The ethnic fractionalization (EF) variable is constructed using the following expression.

$$EF = 1 - \sum_{m} s_{mi}^{2} \qquad \text{(A1)}$$

In equation (A1) s_{mi} is the population share of ethnic group m in the total number of ethnic groups in district i. The index varies between zero (perfect homogeneity) and one (perfect fractionalization). The data used to calculate the index come from the national census, which was conducted by the Central Bureau of Statistics (BPS) in 2000 and 2010.

The service access index is formed by taking the simple average of five service access variables on which data exist: net enrolment in junior secondary school, net enrolment in senior secondary school, the percentage of births that were assisted by a health professional, percentage of households with access to water, and percentage of households with access to sanitary facilities. Data used to derive this index are also from BPS.

The incumbency variable is from the General Elections Commission (KPU). New district data were provided by the Ministry of Home Affairs. Information on intergovernmental transfers has been supplied by Ministry of Finance. And GRDP data have been accessed through BPS. Summary statistics for all variables are provided in Table A1.

Table A1 Summary statistics.

Variable	Number of observations	Mean	Standard deviation	Minimum	Maximum
Incumbent dummy	3,222	0.142	0.349	0	1
New district dummy	3,222	0.388	0.487	0	1
Ethnic fractionalization	3,222	0.453	0.318	0.004	0.998
District transfers per capita ('000)	2,645	2,931	3,772	43,606	43,100,000
Average service access (pct.)	3,220	61.025	13.027	4.552	88.412
District poverty rate (pct.)	3,222	14.812	8.312	1.209	52.560
District GRDP per capita ('000)	2,645	45,007	37,596	1,165	726,039

All economic and fiscal variables are measured in rupiah in constant 2014 terms.

The covariate balance test proceeds by estimating equation (2) from the text, using the same methods as previously applied, employing each of the covariates as the dependent variable, in turn. Table A2 supplies the formal treatment effects estimation results for the balance tests for polynomials of degree one and two. As the table shows, none of the estimated treatment effects is statistically significant at even the 10 percent level. As such, all potential confounders are balanced around the cut-off. Overall, the results support the conclusion that estimated impact of treatment PECs on candidate chances of winning elections have not been influenced by other potential confounders.

Table A2. Covariate balance tests

		Polynomial Degree One					Polynomial Degree Two				
Dependent Variable	Tot. Obs.	Bandwidth	Obs. Left	Obs. Right	τ	SE	Bandwidth	Obs. Left	Obs. Right	τ	SE
Incumbent dummy	3,007	0.072	407	282	-0.083	0.079	0.107	845	367	-0.073	0.090
New district dummy	3,007	0.035	153	173	0.182	0.129	0.055	307	223	0.191	0.145
Ethnic fractionalization	3,007	0.056	330	229	0.069	0.069	0.145	1,283	442	0.037	0.067
Log transfers per capita	2,444	0.035	131	150	0.304	0.209	0.060	294	198	0.207	0.230
Average service access	3,005	0.076	459	286	-0.367	3.022	0.064	375	254	4.136	4.952
District poverty rate	3,007	0.059	330	229	0.526	1.844	0.080	490	298	0.187	2.244
Log GRDP per capita	2,444	0.089	461	276	-0.155	0.134	0.084	461	276	-0.016	0.187

Determination of bandwidths is data driven and they are measured by PEC seat share. Number of observations to the left and right of the cut-off point is the effective number used in estimation. Estimated treatment effect is bias corrected and robust where standard errors are clustered at the district level. Fiscal and economic variables are measured in constant 2014 terms. None of the estimated coefficients is statistically significant at even the 10 percent level.

Testing treatment effects at fake cut-offs helps to gauge the extent to which estimated treatment effects at the actual cut-off (i.e. zero) are spurious. In this context, I test for significant treatment effects using the main empirical model from the text at six different fake cut-offs: -0.20, -0.15, -0.10, 0.10, 0.15, and 0.20. Cut-offs beyond either of the end points in the series cannot be estimated due to insufficient observations. Table A3 supplies the results.

Table A3. Fake cut-off tests

	Polynomial Degree One					Polynomial Degree Two				
Cut-Offs	Bandwidth	Obs. Left	Obs. Right	τ	SE	Bandwidth	Obs. Left	Obs. Right	τ	SE
-0.20	0.015	72	29	-0.061	0.073	0.017	74	37	0.176	0.156
-0.15	0.036	270	431	0.029	0.054	0.028	192	363	0.021	0.137
-0.10	0.048	602	374	-0.053	0.046	0.052	692	508	-0.053	0.053
0.10	0.099	314	170	-0.074	0.108	0.131	487	202	-0.072	0.132
0.15	0.156	472	186	-0.098	0.097	0.219	847	215	-0.100	0.115
0.20	0.101	222	94	-0.044	0.162	0.135	282	105	-0.065	0.218

Total number of observations is 3,077 for all regressions. Determination of bandwidths is data driven and they are measured by PEC seat share. Number of observations to the left and right of the cut-off point is the effective number used in estimation. Estimated treatment effect is bias corrected and robust where standard errors are clustered at the district level. Fiscal and economic variables are measured in constant 2014 terms. None of the coefficients is statistically at even the 10 percent level.

The output indicates that none of the fake cut-offs generates statistically significant results at even the 10 percent level of significance, supporting the main estimation results at the zero cut-off.

In sum, the results from these two robustness tests support the validity of the identification strategy and provide confidence in empirical treatment effect estimates presented and discussed in this article.