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Legislature Size, Local Government Expenditure and Taxation, and Public Service Access in Indonesia

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Abstract
This study examines the impact of legislature size on local public finance and service outcomes in Indonesia. The investigation employs both continuity- and randomization-based regression discontinuity methods to accommodate the endogeneity of council size and to identify its causal effects on local government spending, service delivery, and own-source revenue mobilization. Many studies have examined the influence of increasing legislature size on expenditures, but no consensus has emerged on the direction of impacts. Moreover, interpretation of the efficiency of derived spending effects has remained elusive and reliant on ad hoc theorizing. This is the first study to examine the causal impact of council size on service outcomes, thereby facilitating an empirically based understanding of efficiency effects. The study finds that increasing legislature size negatively affects local government total and capital spending. The investigation also shows that rising legislature size has a negative influence on citizen access to public services. Finally, the examination offers evidence to suggest that an increasing number of legislators have no impact on local own-source revenues. Taken together the results imply a decline in local efficiency: residents pay the same amount in taxes but receive fewer services. The findings in this investigation contradict recent theoretical predictions and empirical results from other research.

Keywords Legislature size · Local government · Pork barrel spending · Service delivery · Efficiency · Indonesia

JEL Classification H72 · H75 · H76

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Introduction

This study examines the causal effects of legislature size on local government spending, service delivery, and own-source revenue generation in Indonesia. It also investigates the question of the extent to which such effects can be judged as efficient. In the defining, the scope and substance of its research this study draws on the theoretical propositions of two related literatures and the empirical work that has tested hypotheses derived from theory.

The starting point for much of the recent theory on the impact of local legislature size is the well-known article by Weingast et al. (1981). These authors conjecture that larger councils lead to the implementation of capital projects that are “too large” and increased local spending. In this context, a growing number of legislators exacerbate the fiscal commons problem, whereby individual councilors seek to target project benefits to their home districts but where those projects are funded from a common pool of cross-district resources. As the number of legislators rises, any given councilor absorbs a smaller share of an additional project’s tax costs, leading to higher demand for inefficiently large projects and increased spending. It is fair to say that this so-called “pork barrel” theory has become the conventional wisdom on the topic (Hohmann 2017).

A second strand of research focuses more on the institutional relations between councils and local government executives. Pettersson-Lidbom (2012) posits the existence of an agency problem between time-constrained councilors and local government bureaucrats; whereby, the former seek to restrain the budget maximizing tendencies of the latter. In this case, the argument is that a growing number of legislators lead to a decline in inefficient local spending. Coate and Knight (2011) theorize that the divergence of preferences between elected councilors and the local executive leads to difficulties in reaching budget decisions and that such delays force a decrease in project spending. While the authors do not explicitly make the argument, it is plausible to conjecture that an increasing number of legislators might further exacerbate difficulties associated with mayor–council decision-making.

The empirical research to date that has tested theoretical predictions related to the impact of legislature size offers decidedly mixed results. Gilligan and Matsusaka (1995, 2001), Bradbury and Crain (2001), Baqir (2002), Perotti and Kontopoulos (2002), and Egger and Köthenbürger (2010) all find that council size has a positive impact on local spending. Contrarily, De Figueiredo (2003), Pettersson-Lidbom (2012), Garmann (2015), and Hohmann (2017) determine that legislature size negatively affects expenditure at the local level.

Researchers face two significant challenges in investigating the causal effects of council size on local spending and efficiency. The first difficulty concerns the endogeneity of legislature size. The most recent work in this area recognizes that the size of the local council is endogenous and that a failure to accommodate that endogeneity in model specification leads to biased estimation results. Of the studies mentioned above, only Egger and Köthenbürger (2010), Pettersson-Lidbom (2012), Garmann (2015), and Hohmann (2017) adopt methods that allow for the endogeneity of legislature size.

The second hurdle relates to determining whether the estimated spending effects are efficient or not. None of the studies mentioned above provides direct empirical evidence on that count; researchers instead have relied on simple assumptions or ad hoc theorizing to make the case one way or the other. Of the studies enumerated above, those that find expenditure increases argue that the result is inefficient while those that find spending decreases claim that the outcome is efficient.[1] In all instances, however, the inferences are derived from theory and not explicit evidence. These judgements may be correct, of course, but it would be useful to have some empirical confirmation of the theory-based assertions.

Indonesia offers an interesting laboratory in which to examine the effects of legislature size on public finance and service outcomes. The country launched a very ambitious program of fiscal decentralization in 2001. Local governments have become responsible for the delivery of most key public services, including those in the education, health, and infrastructure sectors. In 1999, the popular election of local councils was re-introduced, and Indonesia initiated direct elections of district heads in 2005.

The investigation in this paper employs both continuity- and randomization-based regression discontinuity methods to overcome problems related to the endogeneity of council size and identify its causal effects on district spending, service access, and taxation. The examination of service impacts allows for an empirically based assessment of the extent to which effects are efficient or not. This appears to be the first study to assess the impact of endogenous legislature size on services and the first to employ two different approaches to regression discontinuity design in the investigation of legislature size effects of any kind.

The study finds that rising council size negatively affects local government total and capital (project) spending. In addition, the investigation shows that rising legislature size has a negative impact on citizen access to key public services as well. Finally, the examination provides evidence to suggest that increasing council size has no effect on the mobilization of local government own-source revenues. Together, the results imply a decline in local government efficiency: residents pay the same amount in taxes but receive fewer services. The derived outcomes are inconsistent with much of the relevant theory, as outlined above, and contradict the interpretations of recent and similar empirical findings (De Figueiredo 2003; Pettersson-Lidbom 2012; Garmann 2015).

The rest of the paper proceeds as follows. First, some of the standard theory is discussed in greater depth; an alternative theoretical framework is introduced, and hypotheses to be tested in the study are outlined. Second, some background information on local fiscal and political affairs and service delivery in Indonesia is provided. Third, the variables and data used in the examination are reviewed. Fourth, the methods and identification strategy are explained. Fifth, the main empirical results of the study are presented and discussed. Sixth, the robustness of the results is interrogated. A closing section summarizes and concludes.

[1] The exception to the rule is Hohmann (2017). He finds negative spending impacts of increasing council size in Germany but reasonably suggests that further research would be required before a judgment can be made about the interpretation of effects.

Theory and Hypotheses

Pork barrel models assume single-member electoral districts, each of which is represented in a legislature. The analytical focus of such models is on distributive policies, that is, those policies or projects for which benefits are geographically concentrated in specific districts but for which costs are spread across all districts and funded through generalized taxation. In this framework, all districts have a project and individual legislators care only about projects that are executed in their districts; the legislature is assumed to operate under the principle of universalism, in which all proposed projects are adopted (Weingast et al. 1981).

The key driving factor in these models is the number of districts or the number of legislators, which are equivalent in the set-up as described above. In this context, an increase in the number of districts/legislators leads to higher demand for new project spending in home districts, since each additional legislator understands that project benefits will accrue only to his or her constituents but that project costs—financed automatically and in full by increased taxes—will be allocated across all districts.

More formally, each legislator i prefers project spending level X_i so that

$$B^{'}(X_i) = C^{'}(X_i)/n \tag{1}$$

where $B(X_i)$ and $C(X_i)$ are the benefits and costs associated with project spending in district i, where it is usually assumed that $B' > 0$, $B'' \leq 0$, $C' > 0$, and $C'' > 0$, and n is the number of legislators (Weingast et al. 1981).

As Primo and Snyder Jr. (2008) demonstrate, totally differentiating the first order condition in Eq. (1) with respect to n gives

$$\frac{\partial X_i}{\partial n} = \frac{B^{'}(X_i)}{C^{''}(X_i) - nB^{''}(X_i)} > 0 \tag{2}$$

Thus, pork barrel logic, or the "law of $1/n$," means that an increase in the number of legislators leads to higher individual district project spending, X_i, and therefore rising total government spending, $nC(X_i)$.[2] A comparison of the net benefits associated with an additional district's project with optimal net benefits, as derived by maximizing total benefits minus the total costs of spending (i.e., taxes) across all districts, allows for a judgement about individual district project efficiency. Weingast et al. (1981) show that a rising number of districts/legislators lead to district project spending that is relatively more inefficient.

The situation in Indonesia differs from the stylized theoretical framework laid out above in two important regards. First, as will be discussed in more detail below, local government jurisdictions comprise voting districts with multiple council members. In this context, it is not clear why a councilor would zealously focus on returning capital

[2] Interestingly, Primo and Snyder Jr. (2008), using the same framework as Weingast et al. (1981), show that if home district project costs are partially subsidized by central government, the impact of the number of legislators on total local spending depends on the pureness of the public good and the initial number of legislators. In particular, if public goods are somewhat congested and/or the initial number of legislators is relatively large then an increase in the latter may result in lower local government spending.

projects to his or her district, which includes other legislators: he or she may have to share credit with possible electoral opponents among voters for doing so. The existence of multiple member districts would seem to generally dampen demand for home-based capital projects.

Second, and perhaps more importantly, local tax instruments and rates are quite constrained by national legislation. Taxes cannot be expected to automatically rise to finance increased councilor demand for capital spending.[3] So, even if a rise in the number of legislators were to stimulate more demand for home-based projects, it is very unlikely that such demand could be funded from local own-sources, either directly or indirectly to repay project loans.[4]

These differences cast doubt on the relevance of pork barrel models in the Indonesia context. An increase in the number of councilors cannot be assumed to mechanically result in an increase in demand for capital projects, everywhere and all the time, and the presumption that any such rise in demand necessarily results in the financing and implementation of projects is dubious.[5]

I, therefore, adopt a different theoretical approach in this paper. I posit that the number of legislators on the local council simultaneously and directly influences local government spending, both in total and for capital projects, as well as the delivery of local services. Local jurisdictions have broad discretion over their spending and the way that public services are delivered. Mayors and council members jointly determine the allocation of spending across functions each fiscal year and make decisions about whether and how to tender out capital projects. As regards, the manner in which services is delivered, mayors and councils decide together about the extent to which management of services may be devolved to front line providers (schools and health centers, for instance) or whether special external local service units (*badan pelayanan umum daerah*) or local state-owned enterprises (*perusahaan daerah*) should be established to manage service delivery (especially for water and sanitation) and determine what their exact responsibilities are.

Borrowing from Coate and Knight (2011), I assume that elected council members and mayors have heterogeneous preferences regarding spending and service delivery decisions. Furthermore, I hypothesize that reaching agreement on these issues depends on the number of decision-makers involved. I presume that an increasing number of legislators on the council constrain timely and sound decision-making. I expect that a growing number of councilors will lead to reduced local capital (and total) spending and abridged local public service access, because necessary mayor–council decisions related to efficient project spending and service delivery are not made in a timely fashion and/or are poorly made.

On the other hand, local governments and parliaments have quite restricted decision-making authority over locally generated revenues. The local tax that mobilizes most revenue is that on personal electricity consumption, and it is administered by the National Electricity Agency (PLN). Other allowable tax instruments and maximum

[3] Intergovernmental transfers, the main source of revenue in local budgets, are exogenously determined by central government. Mayors and councilors have no influence on the amount of transfers received.

[4] In any case, local governments rarely borrow to finance capital investments (Lewis and Oosterman 2011).

[5] Neither are local councilors time-constrained in Indonesia in the sense described by Pettersson-Lidbom (2012) for Sweden and Finland, making the associated agency framework plausibly relevant in those countries inappropriate in this case.

tax rates (which are automatically adopted by most local governments) are both fixed in national law. Constrained policy choices limit mayor and councilor capacity to independently affect local own-source revenues, in any manner, even if they were able to reach agreement. I anticipate, therefore, that an increase in the number of local legislators will have no effect on the generation of local tax revenue, all else equal.

In sum, I offer three hypotheses:

H1. An increase in the number of legislators leads to a decrease in capital and total spending.

H2. An increase in the number of legislators leads to a decrease in local public service access.

H3. An increase in the number of legislators has no impact on local own-source revenues.

Note that if these hypotheses hold, then it can be concluded that an increase in the number of legislators results in reduced local project efficiency: residents pay the same amount of local taxes but at the same time receive fewer public services.[6]

Local Fiscal and Political Affairs and Service Provision

Local Fiscal Affairs

Indonesia is a unitary country, and throughout most of its history, its public sector was one of the most centralized in the world. In 2001, however, the country began a determined effort to decentralize authority over service delivery to subnational governments—provinces and local governments (districts). District service assignments focus on education, health, and infrastructure functions but also include tasks related to agriculture, social protection, environment, low-income housing, and security, among others. Local expenditure budget shares for education, health, and infrastructure are approximately 35%, 10%, and 25%, respectively. Subnational government responsibility for service delivery is significant. Provincial and district expenditure makes up about one half of total public–sector spending net of subsidies and interest payments. Local government expenditure encompasses about 75% of the subnational total (Lewis 2017b).

District revenues include those from: own-sources, shared tax, and non-tax revenue, a general-purpose grant, a specific-purpose grant, and others. Own-sources are quite constrained, as mentioned; allowable taxes are enumerated, and maximum tax rates are prescribed in national legislation. The most important local taxes are those on electricity consumption (administered by PLN, as noted) and hotel and restaurant sales. Districts may create their own (minor) user charges based on rules set in law. But, central government reserves the right to cancel charges and fees that it believes breach legislative principles and it often does so (Lewis 2003).

[6] This assumes that other (non-local) taxes paid by residents (e.g., personal income taxes) that form the basis for funding some intergovernmental transfers are not affected by increasing council size either. Later, I will show that such payments-cum-transfers do not confound hypothesized effects.

Shared taxes consist of the property tax (through 2013, now decentralized to districts) and the personal income tax. Shared non-taxes derive from national forestry, fisheries, mining, and gas and oil revenues. The general-purpose grant is an equalization mechanism, which allocates funds based on a fiscal gap formula. The specific-purpose transfer is a matching capital grant; it spans numerous sectors but is concentrated in education, health, and infrastructure.[7] Other revenues include special autonomy funds for districts in Aceh, Papua, and West Papua; grants from the central government for teacher certification and other education operations; and transfers from provinces. Taken together intergovernmental fiscal transfers comprise more than 90% of local revenue budgets (Lewis 2017b).

During the period of study here, local governments had substantial discretion over the expenditure of revenues to which they had access. Districts are required to spend 20% of their budgets on education but had full spending autonomy over the remainder of their funds. The education spending mandate is not particularly onerous since it includes teacher salaries, and this makes the target rather easily achieved for most local governments. Moreover, the spending mandate is not enforced by central government anyway (Lewis and Nguyen 2018). In recent years, district spending authority has been curtailed to a certain extent. In addition to compulsory education expenditure, since 2015 districts have become obliged to spend at least 5% of their funds on health and transfer another 10% of their resources to the villages within their jurisdictions (Lewis 2015; Antlöv et al. 2016). These changes were implemented after the period of study here.

Local Political Affairs

Indonesian fiscal decentralization has been complemented by significant political developments at the local level. Prior to 1999, both subnational government executives and councils (*Dewan Perwakilan Rakyat Daerah*, DPRD) were effectively appointed by central government. The (full) democratic election of DPRD representatives was introduced in 1999. Starting in that year, DPRDs also began to appoint subnational government heads, as central government appointed executives' terms expired. Indonesia initiated direct elections of provincial and district government heads in 2005. Direct elections have since been implemented in a gradual manner, as DPRD-appointed executives' terms finished (Lewis 2017b).

DPRD elections are held across multiple voting subdistricts (*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 here, the number of *dapils* per local government jurisdiction ranged from 2 to 7 and the number of seats per *dapil* varied from 3 to 12.

Voting is based on proportional representation and open-list electoral rules. There was no electoral threshold for political parties participating in DPRD polls during the period of study (Tomsa 2014). The system is "permissive"—it seeks to encourage strong linkages between politicians and citizens but at the same time facilitates

[7] In 2016, the specific-purpose grant was reconstructed as a proposal-based transfer to provinces and districts. The matching component was expunged.

increased fragmentation of DPRDs across political parties. Political fragmentation is indeed high in Indonesia. During the period of study here, the average number of political parties represented in DPRDs was 11.2 and the effective number of parties was 7.6.[8]

DPRD members are elected for five-year terms. Since the first polls in 1999, legislative elections have been held in 2004, 2009, and 2014.[9] The exact number of seats allocated to a DPRD is a deterministic and discontinuous function of jurisdiction population size. Table 1 shows how the number of local DPRD seats was fixed in the 2004 and 2009 elections, which span the period of investigation of this study.

Note that the number of seats in the legislature for districts with population sizes greater than one million was increased from 45 to 50 just prior to the 2009 elections. The change was based on a proposal to the national legislature (*Dewan Perwakilan Rakyat*, DPR) from one of the largest and most powerful political parties in Indonesia, GOLKAR.[10] The suggestion was agreed to by most of the other parties in the DPR, and the appropriate change was made in revised electoral legislation, Law No. 10/2008.

In any case, there are no exceptions to the rule relating pre-election population size to total number of DPRD seats in the data used in this study. Figure 1 illustrates the deterministic relationship between population size and the total number of council seats for the 2004 and 2009 elections.[11]

DPRDs are tasked with significant responsibility over budget-related matters. They assist the subnational executive in planning fiscal year revenues and expenditures, have authority for approving annual budgets and mid-year budget revisions, and oversee budget execution. At the end of the budget year the executive presents a financial accountability report to the DPRD, which the latter evaluates. The DPRDs are also meaningfully involved in day-to-day policy- and regulation-making over the entire range of district service responsibilities and play a key role in formulating local medium-term development strategies.

Local Service Provision

Local government education, health, and infrastructure service delivery has gradually improved since decentralization began. Table 2 shows the extent to which access to major services in those three sectors changed from 2001 to 2012. Service access has risen across all indicators. While progress has not been particularly rapid, it has, in general, been steady.[12]

[8] Political parties are weak at the local level in Indonesia. They are essentially vehicles for individuals to seek and gain access to political office. See Aspinall (2013) for a discussion and analysis of the fragmentation and personalization of Indonesian political parties at the local level.

[9] Comprehensive data on the 2014 elections are not yet available.

[10] GOLKAR is the party of ex-president Suharto.

[11] The figure shows districts with populations up to two million persons to best illustrate the relationship. There are 13 local jurisdictions with population greater than two million; the largest place (Surabaya) has a population of just over four million. Jakarta, population (approximately eight million) is not included in the study due to lack of data.

[12] Service access variables are selected based on the availability of data. Data also exist on child immunization rates, but the variable has not been consistently measured across the years and so it cannot be used in the analysis here. There are no comprehensive data on service quality for Indonesia.

Table 1 Population size and number of DPRD seats

		Number of seats
Population thresholds for 2004 elections		
1	≤ 100,000	20
2	> 100,000 and ≤ 200,000	25
3	> 200,000 and ≤ 300,000	30
4	> 300,000 and ≤ 400,000	35
5	> 400,000 and ≤ 500,000	40
6	> 500,000	45
Population thresholds for 2009 elections		
1	≤ 100,000	20
2	> 100,000 and ≤ 200,000	25
3	> 200,000 and ≤ 300,000	30
4	> 300,000 and ≤ 400,000	35
5	> 400,000 and ≤ 500,000	40
6	> 500,000 and ≤ 1,000,000	45
7	> 1,000,000	50

Source: General Elections Commission (KPU)

Variables and Data

Dependent variables of interest to this study include local government fiscal and service outcomes. Fiscal variables comprise district total and capital expenditure

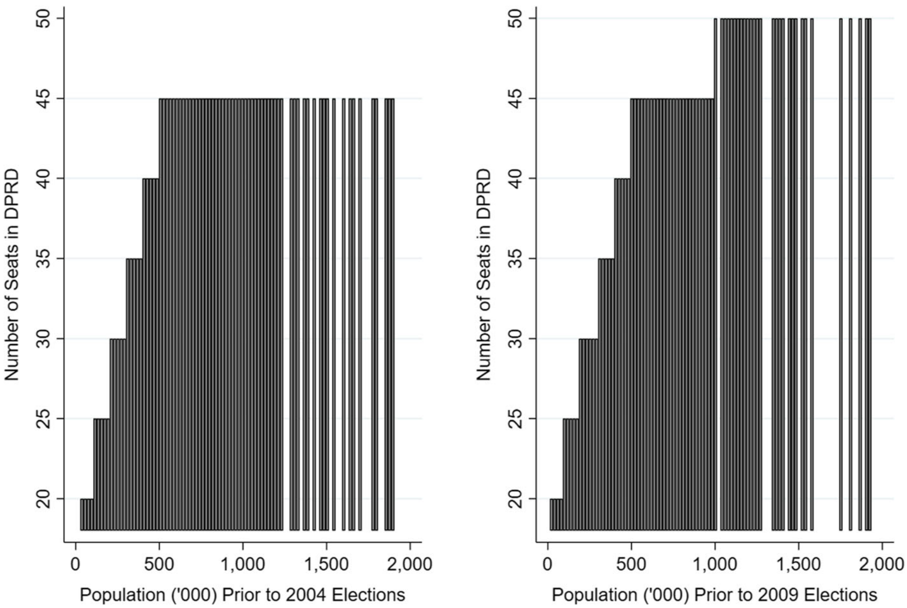

Fig. 1 Pre-election population and number of DPRD seats

Table 2 Service access, 2001–2012

Year	Junior secondary enrolment	Senior secondary enrolment	Professionally attended births	Household water access	Household sanitation access	Village road access
2001	59.6	36.8	48.9	44.0	54.6	63.6
2002	60.7	37.8	67.9	44.8	57.6	60.1
2003	62.8	40.9	68.6	44.3	58.7	57.6
2004	65.3	44.0	71.5	45.4	61.2	60.5
2005	61.9	41.2	68.3	43.9	57.3	62.5
2006	66.0	44.6	69.2	44.7	56.7	64.1
2007	65.0	45.7	69.1	46.9	56.6	65.6
2008	64.2	45.2	71.0	48.5	58.7	67.2
2009	65.1	46.2	72.1	50.6	59.2	68.9
2010	65.7	46.4	74.2	52.5	60.5	69.7
2011	65.4	48.7	75.5	54.3	61.0	70.5
2012	67.5	50.4	78.1	56.6	63.1	71.2

Source: Central Bureau of Statistics (BPS) SUSENAS and PODES

and local own-source revenue. Local government fiscal data come from the Ministry of Finance (MoF), which compiles the information from district executed budgets in the first instance.

Service outcomes incorporate those for education, health, and infrastructure sectors: net enrolment rates in junior and senior secondary school, percent of births attended by a health professional, and household access to water and sanitation facilities and village access to paved roads. In the analysis below, I employ the mean of the annual change in these six variables as an indicator of average citizen access to public services provided by local government.[13] Data on all variables except road access have been supplied by the Central Statistics Agency (BPS) annual socioeconomic survey (SUSENAS). Data on village access to paved roads have been accessed through BPS's Village Survey (PODES). BPS/PODES survey results are available for 2003, 2005, 2008, 2011, and 2014. Variable values for missing years have been linearly interpolated.

The key independent variable in the analysis is pre-election population size (the forcing variable in the regression discontinuity analysis). Data on pre-election population have been supplied by the General Elections Commission (KPU).

The study also employs several other covariates. These include the following: a dummy variable indicating if the district is newly created or not, ethnic fractionalization, intergovernmental transfers, Gini coefficient for personal (household)

[13] Alternatively, the service access index could be constructed by employing principal components analysis. This procedure was also carried out. The empirical results do not change appreciably; the qualitative conclusions reached here based on the analysis are robust with respect to the service access measure. I prefer to use average service access because it is more easily interpreted.

consumption, personal (household) consumption, and a dummy for those districts from Eastern Indonesia.[14] More detail on the use of these variables will be provided below.

New districts are created when a single jurisdiction splits into two or more administrative units. In Indonesia, the process is known as *pemekeran* and it is a ubiquitous phenomenon.[15] Data on pemekeran have been provided by the Ministry of Home Affairs.

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

$$EF = 1 - \sum_{m} s_{mi}^2 \tag{3}$$

In Eq. (3), s_{mi} is the population share of ethnic group m in the total number of ethnic groups in local jurisdiction 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 BPS in 2000 and 2010.

Data on intergovernmental transfers come from MoF, while data on the personal (household) consumption (Gini and level) have been accessed through BPS/SUSENAS.

The analysis in this paper focuses on districts with directly elected heads, which make up the vast bulk of districts during the study period.[16] Between 2005 and 2012, 390 local governments with directly elected executives are represented in the data set. This comprises about 80% of the total number of such districts that existed during the study period. The total number of these local governments, by population size category, at the start of each electoral period, is shown in Online Table 7 in Appendix. Summary statistics are provided for all variables used in the analysis are provided in Table 3.

Methods and Identification

The objective of this examination is to assess the impact of legislature size on district spending and public service access. In this context, the size of the legislature is likely to be endogenous, because of reverse causality or due to omitted unobservables—citizen or political party preferences, for example (Egger and Köthenbürger 2010; Pettersson-Lidbom 2012; Garmann 2015; Hohmann 2017). To accommodate that endogeneity and identify the causal spending and service effects, I use regression discontinuity (RD) methods.

[14] Districts in Eastern Indonesia comprise those in the provinces of Maluku, Maluku Utara, Nusa Tenggara Barat, Nusa Tenggara Timur, Papua, and Papua Barat. Eastern Indonesia is the least developed region of the country, and spending and service access in districts in there are significantly different from those in the rest of Indonesia.

[15] For an up-to-date analysis of the determinants of district splitting, see Pierskalla (2016), and for recent investigations of the (largely deleterious) effects of *pemekaran*, see Burgess et al. (2012), Bazzi and Gudgeon (2016), and Lewis (2017a).

[16] Garmann (2015) distinguishes between local governments with elected and appointed heads in his examination of council size effects in Germany. He finds a negative council size effect on spending for the former but no statistically significant results either way for the latter. It is not analytically feasible to draw that distinction in this study, owing to the small sample of districts with appointed heads (52).

Table 3 Summary statistics

Variable	Number of observations	Mean	Standard deviation	Minimum	Maximum
Pre-election population ('000)	2254	233.1	118.4	50.6	549.8
Number of DPRD seats	2254	29.3	6.1	20.0	45.0
Local government total spending per capita	2251	3,029,295	1,743,604	291,448	16,900,000
Local government capital spending per capita	2106	901,403	881,152	62,065	11,300,000
Annual change in average service access index	1875	1.8	2.8	− 3.9	7.3
Local government own-source revenue per capita	2346	169,969	219,250	5409	4,669,815
New district dummy	2254	0.47	0.50	0.00	1.00
Ethnic fractionalization	1882	0.52	0.30	0.01	0.99
Intergovernmental transfers per capita	2254	2,818,071	1,531,889	406,533	11,300,000
Gini coefficient for household personal consumption per capita (× 100)	2254	17.3	14.0	0.2	60.2
Household personal consumption per capita	2254	626,703	199,488	246,986	1,988,474
Eastern Indonesia dummy	2254	0.19	0.40	0.00	1.00

All economic and fiscal variables are measured in rupiah in constant 2010 terms

I employ a sharp RD design with multiple cutoffs, where the latter are defined as a function of the various population thresholds. In this framework, pre-election population is the forcing variable. Pre-election population determines exactly the number of seats in the DPRD. To operationalize the multiple cutoff RD approach, I normalize and pool pre-election populations to form a single cutoff. I normalize population around thresholds as follows:

$$
\begin{aligned}
\mathrm{X_{ni}} = {} & \mathrm{X_i}\text{–}100,000 \quad \text{if } 50,000 < \mathrm{X_i} \leq 150,000 \text{ (for both electoral periods)} \\
& \mathrm{X_i}\text{–}200,000 \quad \text{if } 150,000 < \mathrm{X_i} \leq 250,000 \text{ (for both electoral periods)} \\
& \mathrm{X_i}\text{–}300,000 \quad \text{if } 250,000 < \mathrm{X_i} \leq 350,000 \text{ (for both electoral periods)} \\
& \mathrm{X_i}\text{–}400,000 \quad \text{if } 350,000 < \mathrm{X_i} \leq 450,000 \text{ (for both electoral periods)} \\
& \mathrm{X_i}\text{–}500,000 \quad \text{if } 450,000 < \mathrm{X_i} \leq 550,000 \text{ (for first electoral period)} \\
& \mathrm{X_i}\text{–}500,000 \quad \text{if } 450,000 < \mathrm{X_i} \leq 550,000 \text{ (for second electoral period)} \\
& \mathrm{X_i}\text{–}1,000,000 \quad \text{if } 950,000 < \mathrm{X_i} \leq 1,050,000 \text{ (for second electoral period)}
\end{aligned}
$$

In the above formulation, i represents individual local governments; X_i is pre-election population, and X_{ni} is normalized pre-election population. The lower and upper bounds used to establish the normalized pre-election populations are, in general, fixed at the midpoints of the relevant pre-election population categories as defined in Table 1. The exceptions to this rule concern the last population size class in the each of the two electoral periods, where the bound endpoints were chosen to make the interval length consistent with that of the other bounds (i.e., a population size of plus or minus 50,000). Pooling the normalized pre-election populations in the manner here creates a single cutoff at zero.[17]

Following Imbens and Lemieux (2008), define Y_i (0) and Y_i (1) to be a potential spending or service outcome for district i where Y_i (0) is the outcome to the left of the threshold (control) and Y_i (1) is the outcome to the right of the cutoff (treatment). In this case, the impact of increased legislature size is given by Y_i (1) - Y_i (0). Unfortunately, Y_i (0) and Y_i (1) cannot be observed simultaneously and so attention turns to the average effects of council size, Y_i (1) - Y_i (0), across subgroups of the relevant population. Let $D_i = 0$ if a district is in the control group, and $D_i = 1$ if a district is subject to treatment. Observed outcomes, Y_i, are therefore = Y_i (0) if $D_i = 0$ and = Y_i (1) if $D_i = 1$; and the average causal effect of the size of the legislature, τ, at the cutoff, $c = 0$, is given by:

$$\tau = E[Y_i(1) - Y_i(0)|X_{ni} = c] = \mathrm{E}\Big[\Big(Y_i(1)|X_{ni} = c\Big] - \mathrm{E}[Y_i(0)|X_{ni} = c] \tag{4}$$

The key identifying assumption in this framework is that E [Y_i (1) | X_{ni}] and E [Y_i (0) | X_{ni}] are continuous in X_{ni}, normalized pre-election population. This implies that all other unobserved determinants of spending and service outcomes, Y_i, are also continuously related to X_{ni} (Imbens and Lemieux 2008). The implication allows one to use outcomes just below the cutoff as valid counterfactuals for those just above the cutoff (Cattaneo et al. 2018a; De la Cuesta and Imai 2016). Note that in the set-up here, crossing the threshold from the left to the right implies an additional five seats on the council (see Table 1.) Thus, treatment is defined as a marginal increase of five legislators.

The general form of the estimating equation is:

$$Y_i = \tau D_i + g(X_{ni}) + \mu_i \tag{5}$$

In Eq. (5) subscript i refers to the district; Y is the outcome; D is the treatment dummy variable, as defined above; $g(X)$ is a polynomial function of the running variable X_{ni}; μ is the error term; and τ is the treatment effect, which is to be estimated.

I estimate the treatment effect using non-parametric regression techniques within narrow windows (bandwidths) on each side of the cutoff.[18] In this context, three choices must be made: the degree of polynomial of the regression equation, the kernel type, and the bandwidth. Recent research argues for the use of lower order polynomials (Gelman and Imbens 2017; Cattaneo et al. 2018a) and I employ a polynomial of degree one in the estimation procedures below. I use a triangular kernel (that weights observations closer to the cutoff point more heavily) and choose bandwidths in a data-driven fashion to minimize the mean squared error (MSE) of the RD point estimator,[19] as the latest work in this area advocates (Imbens and Kalyanaraman 2012; Cattaneo et al. 2018a). The RD methods described here identify a local average treatment effect (LATE) (Lee and Lemieux 2010).

[17] See Brollo et al. (2013) for the use of similar pooling and normalization procedures in the context of multiple cutoff RD designs.

[18] I use the Stata command "rdrobust" to estimate treatment effects in this article. See Calonico et al. (2017).

[19] The MSE of the estimator is the sum of the bias squared plus the variance. As such, the bandwidth selection procedure optimizes the bias-variance trade-off. (Cattaneo et al. 2018a).

Empirical Results

The treatment effect analysis begins by examining the standard RD plots. Figure 2 shows the RD plots for log spending per capita, in total and for capital, annual change in average service access, and log local taxes per capita, all relative to pooled pre-election population. Each dot in the figure represents the average value of the outcome in question for a data-driven selected range (bin) of pooled population. Local polynomial regressions of order four are superimposed on the data on both sides of the cutoff to best illustrate the global relationships (Cattaneo et al. 2018a).

Attention is drawn to variable relationships at the threshold. The plots appear to show a pronounced downward break in spending and service outcome variables but no discontinuity for own-source revenues around the cutoff. This implies that increasing legislature size may negatively affect district total and capital spending and service access but that it has no effect on the generation of own taxes. The plots are merely suggestive of impacts (or lack thereof), however; a firm conclusion can only be reached after a formal estimation of treatment effects (i.e., using data-driven bandwidths and properly estimated standard errors).

I now provide formal empirical estimates of council size treatment effects as illustrated in the above RD plots, by estimating Eq. (5). As previously noted, I employ non-parametric estimation procedures, using a triangular kernel and MSE optimal bandwidths. I estimate effects using a polynomial of degree one. Estimated standard errors are clustered at the district level.

Table 4 provides the results. Each regression in the table shows the following: total number of observations, MSE-optimal bandwidths (in thousands of persons), number of observations to the left and right of the cutoff used in estimation procedures, the conventional estimated treatment effect (τ), robust p values,[20] and the robust bias-corrected 95% confidence intervals as recent work suggests (Cattaneo et al. 2018a).

The estimation output confirms the suspected negative impact of rising legislature size on district total and capital spending and on service access, as illustrated in the RD plots. The results imply that a marginal increase of five councilors leads to an approximate 17% decline in total spending and a 22% decrease in capital expenditure. In addition, the output indicates that the specified increase in council size causes a 0.8 percentage point decline in average service access, all else being equal. Finally, the results show that increasing council size has no causal effect on own-source revenue mobilization.[21]

These results confirm the hypotheses laid out in the second section of the paper and provide support for the conceptual framework developed there.[22]

[20] I present robust p values to be consistent with p values associated with the randomization-based RD procedures used later in the paper.

[21] I find no impact of increasing council size other types of spending—personnel and goods and services. To save space, I do not present those results here.

[22] Note that a decrease in total spending together with no change to own-source and transfer revenues implies that districts run a surplus. It might plausibly be argued that this surplus could be used augment fiscal reserves and increase capital spending and improve service delivery in the future. However, local governments in Indonesia tend to finance their capital spending out of gross operating budgets and do not use fiscal reserves (or borrowing) to any significant extent for such purposes (Lewis and Oosterman 2011).

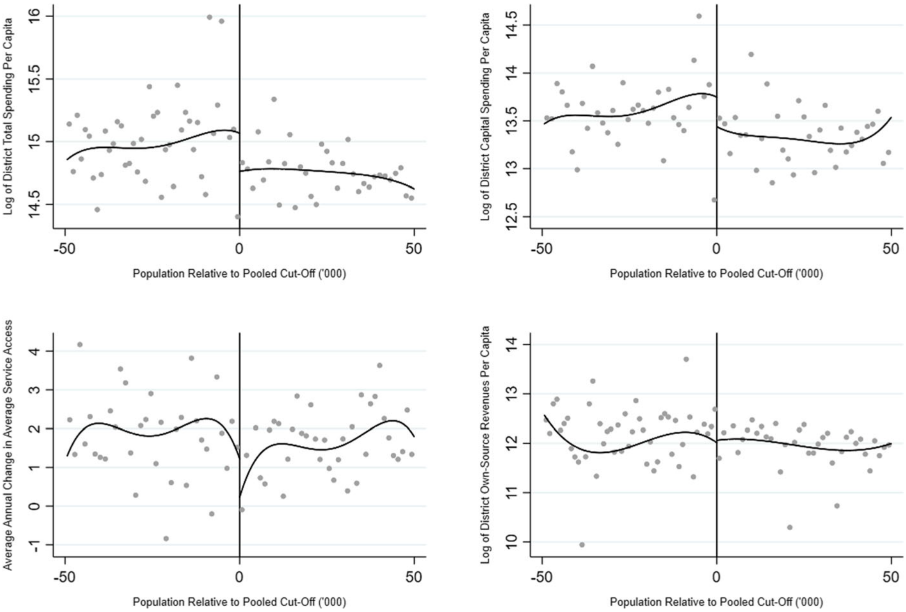

Fig. 2 RD plots for district spending, service access, and own-source revenues

Robustness Tests

I test the robustness of the above empirical results along three dimensions: running variable manipulation, covariate balance, and an alternative approach to RD designs.

Running Variable Manipulation

For the RD approach to be valid, there must be no precise manipulation of the forcing variable, pre-election population, near the cutoff points. This would be the case, for example, if BPS and/or KPU were to systematically falsify population estimates to

Table 4 Legislature size impact on district spending, service access, and own-source revenues

Dependent Variable	Tot. obs.	Bandwidth	Obs. left	Obs. right	τ	p	95% CI	
Log district total spending per capita	2251	14.0	235	335	− 0.174	0.000	− 0.245	− 0.120
Log district capital spending per capita	2106	15.7	239	339	− 0.218	0.000	− 0.300	− 0.116
Annual change in service access	1875	13.0	120	212	− 0.787	0.000	− 1.328	− 0.503
Log district own-source revenues per capita	2346	5.3	63	120	0.066	0.227	− 0.063	0.283

Legislative size is the treatment variable. Tot. obs. is the total number of observations. Bandwidths are in thousands of persons. Obs. left and Obs. right are the effective numbers of observations used in estimation. τ is the conventional estimated treatment effect; p is the robust p value, and CI is the robust bias-corrected 95% confidence interval. All fiscal variables are measured in constant 2010 terms allocate fewer or more DPRD seats to local jurisdictions than they would otherwise merit according to the rules. While there has been no suggestion that BPS and/or KPU act in such a manner, it might still be considered a possibility. Alternatively, manipulation of the forcing variable might occur if people were to migrate strategically to locate themselves in jurisdictions that match their desired number of DPRD seats. This also seems unlikely, although perhaps plausible.

Figure 3 shows the density of pre-election population around the pooled cutoff. The figure is not suggestive of any apparent discontinuities at the threshold, indicated by the vertical line at zero. A formal test of the null hypothesis that no discontinuity exists at the cutoff, using a procedure developed Cattaneo et al. (2016), indicates that the null cannot be rejected. Specifically, the robust bias-corrected test statistic, using a polynomial of degree two, a triangular kernel, with jack-knifed standard errors (the default procedures) is 0.088 and the p value is 0.930. The evidence implies no manipulation of the forcing variable.

Covariate Balance

The treatment effect analysis carried out here assumes that other predetermined covariates (or placebo outcomes) are balanced around the pooled pre-election population threshold. If they were not balanced, then such variables might confound the estimated council size treatment effects on outcomes of interest. I test the covariate balance assumption using several important potential confounders on which data are available: a dummy indicating whether the district is newly created, ethnic fractionalization in the jurisdiction, intergovernmental transfers per capita, Gini for personal consumption, personal consumption per capita, and a dummy variable for districts in Eastern Indonesia. All of these variables might be expected to influence district spending and/or service delivery outcomes.

Table 5 shows the formal treatment effect estimation results, taking each of the potential confounders as the dependent variable in turn, as is usual practice. The regression output demonstrates that none of the variables considered varies significantly around the pooled threshold. The results for intergovernmental transfers are particularly important in this regard. They demonstrate that increasing council size has no impact on such revenues, suggesting that any tax payments made by local jurisdiction residents that feed into transfers are not affected by council size. I conclude that none of the variables substantively confound the derived statistically significant treatment effect results related to local public finance outcomes. The conclusion supports the argument that the initially derived results are robust.[23]

Randomization-Based RD

A potential problem with the above analysis is that the number of observations used in the estimation procedures is rather small, perhaps casting doubt on the statistical power

[23] Eggers et al. (2018) argue that treatment effects may also be confounded when election population thresholds are used for other policy decisions. They show that election thresholds are used to make a variety of other relevant decisions in France, Italy, and Germany. None of the policy decisions enumerated by those authors are made as a function legislative-seat-determining population thresholds in Indonesia. I am unaware of any other policy decisions that depend on population thresholds.

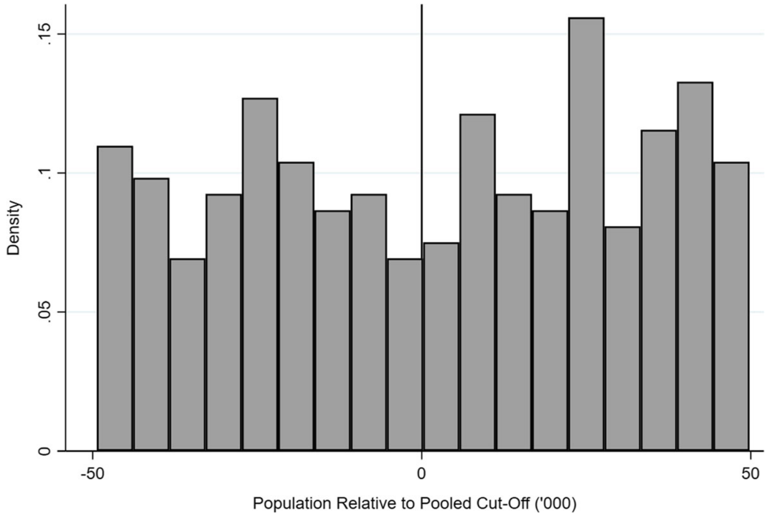

Fig. 3 Density of running variable

of the estimations. One way around this issue is to apply a different formulation of the RD methods; one that is better suited to smaller samples. This alternate framework is called randomization-based RD, as contrasted with the more standard continuity-based RD design that was used above. Randomization-based RD techniques are thought to be especially useful as a robustness test when the number of effective observations is relatively limited (Cattaneo et al. 2017; Cattaneo et al. 2018b).

A brief comparison of the two RD methods may be helpful. The continuity-based approach, which was applied to generate the main results above, selects bandwidths by minimizing the mean square error of the treatment effects point estimate. Treatment effects are then derived as the difference between average outcomes at the cutoff. Covariate balance is subsequently tested by determining if selected covariates exhibit

Table 5 Test of covariate balance

Dependent Variable	Tot. obs.	Bandwidth	Obs. left	Obs. right	τ	p	95% CI	
Newly created local government	2254	8.1	117	210	0.054	0.468	− 0.058	0.148
Ethnic fractionalization	1882	16.0	201	334	0.044	0.515	− 0.056	0.130
Log of intergovernmental transfers per capita	2254	12.5	201	302	0.001	0.770	− 0.047	0.068
Gini coefficient for personal consumption per capita	2254	13.6	218	320	− 1.561	0.151	− 3.960	0.252
Log of personal consumption per capita	2254	4.9	58	117	0.094	0.120	− 0.020	0.232
Eastern Indonesia	2254	9.9	144	268	− 0.054	0.375	− 0.161	0.046

Legislative size is the treatment variable. Tot. obs. is the total number of observations. Bandwidths are in thousands of persons. Obs. left and Obs. right are the effective numbers of observations used in estimation. τ is the conventional estimated treatment effect; p is the robust p value, and CI is the robust bias-corrected 95% confidence interval. All fiscal variables are measured in constant 2010 terms jumps around the cutoff, as was done here. If those regressions are smooth across the cutoff, then it is assumed that covariates do not confound the estimated treatment effects. In these circumstances, estimation and inference are based on large-sample approximations (Cattaneo et al. 2015; Cattaneo et al. 2017; Cattaneo et al. 2018b).

On the other hand, the randomization-based method selects bandwidths to assure that covariate balance is achieved in the first instance. In this case, a covariate is defined as balanced if its mean values on either side of the cutoff are not statistically significantly different from one another. Once the appropriate bandwidth has been determined—i.e., the maximum bandwidth for which all covariates are balanced—the treatment effect is estimated as the difference in means of the outcome variable on either side of the cutoff (within the bandwidth). In this situation, the estimated treatment effect is unconfounded by construction. Here, estimation and inference more closely approximate that of randomized experiments (Cattaneo et al. 2017; Cattaneo et al. 2018b).

I now apply the randomization-based RD technique to examine the effects of increasing council size on district spending and service delivery. I employ the same covariates used in the continuity-based RD balance test above to determine the appropriate bandwidth here: a dummy for new districts, ethnic fractionalization, log intergovernmental transfers per capita, Gini for personal consumption per capita, log personal consumption per capita, and a dummy variable representing districts in Eastern Indonesia. The significance level used to test whether the randomization assumption is satisfied within bandwidths is 0.15, as suggested by Cattaneo et al. (2015). The last window (in a series of ordered trials, starting with a minimum bandwidth) for which the randomization assumption holds is the bandwidth within which the treatment effects are estimated. Implementation of the procedure results in a bandwidth of 6.88 (in thousands of persons) on either side of the cutoff.[24] This bandwidth is narrower than those used in the continuity-based RD estimation of treatment effects above and therefore the effective number of observations employed to estimate effects is smaller.

Table 6 provides the estimation results of employing the randomization-based RD technique, as described just above. The table presents the same output as before (except robust bias-corrected confidence intervals, which are not pertinent in this set-up). The results indicate that an increase of five councilors in the DPRD leads to a 21% decrease in total district spending, a 41% decline in local government capital spending, a 1.7 percentage point annual decrease in average service access, and no statistically significant effect for own-source revenues. These treatment effect estimates for spending and service access are somewhat larger (in absolute value) compared to those earlier derived but still broadly consistent. I conclude that the qualitative inferences reached above concerning the negative impact of legislature size on district spending and public service access and the lack of influence on own-taxes are robust. [25]

[24] To illustrate the window selection algorithm, Table 12 in the Online Appendix reports the full output using log of total expenditure per capita as an example. The results for all other outcome variables are similar.

[25] I also test the robustness of the derived results with respect to the use of an alternative kernel (uniform) and different bandwidths. Regarding the latter, I choose bandwidths that are 25% narrower and 25% wider than the bandwidths used in the baseline regressions. I also test the robustness of the main results by splitting the sample of districts into those that did and did not experience an increase in council seats between the two electoral periods. Finally, I examine the treatment effects for all individual services that make up the service index used in the main regressions. I find that the baseline results presented in the text are robust with respect to all these tests. Relevant output is presented in the Online Appendix in Tables 8,9,10, and 11.

Table 6 Legislature size impact on district spending, service access, and own-source revenues—randomization-based RD approach

Dependent Variable	Tot. obs.	Bandwidth	Obs. left	Obs. right	τ	p
Log district total spending per capita	2251	6.88	91	142	− 0.208	0.001
Log district capital spending per capita	2106	6.88	88	135	− 0.414	0.000
Annual change in service access	1875	6.88	78	121	− 1.662	0.000
Log district own-source revenues per capita	2346	6.88	91	143	− 0.070	0.582

Legislative size is the treatment variable. Tot. obs. is the total number of observations. Bandwidths are in thousands of persons. Obs. left and Obs. right are the effective numbers of observations used in estimation. τ is the estimated treatment effect, and p is the robust p value. All fiscal variables are measured in constant 2010 terms

Summary and Conclusions

Previous empirical research on the effects of increasing local legislature size has resulted in a diverse and inconsistent set of results. Some researchers have found that increasing council size has a positive impact on local spending while others have determined that marginal increases to the size of the local legislature lead to a decline in expenditure. Analysts have typically just assumed rising spending is inefficient while reduced expenditure is efficient.

This study examines the impact of rising legislature size on local government spending and taxation and public service access in Indonesia. The study finds that rising council size negatively affects district total and capital spending. In addition, the investigation shows that increasing council size has a concurrent negative impact on citizen access to key public services. Finally, the examination demonstrates that an increasing number of councilors have no effect on the mobilization of own-source revenues. Together, the empirical results imply a decline in local efficiency. These are unique results in the empirical literature.

More specifically, the investigation determines that an increase of five legislators on the DPRD leads to a decrease in local government total spending of between 17 and 21%, a decline in capital expenditure of between 22 and 41%, a reduction in annual average public service access of between 0.8 to 1.7 percentage points, and no statistically significant effect on local tax revenues. The range in magnitude of effects for any given outcome is a function of the different RD methods used in their estimation. While magnitudes vary across techniques, derived qualitative conclusions based on the estimates do not.

What of policy consequences? One implication of the research in this study, specifically for Indonesia, is that government may wish to consider decreasing the size of local councils (or at least not increasing them as was done prior to the 2009 elections). Smaller council sizes would, in theory, encourage local governments to spend more, especially on capital, which in turn, would be expected to lead to improved service access for citizens. The adoption of such a policy stance seems unlikely; however, since it would confront significant political interests associated with holding positions of power at the local level. But, if such a strategy is not embraced, average legislature size will persist in rising, along with population growth, and fiscal and service outcomes will continue to suffer.

This investigation has offered a conceptual framework that differs from the standard pork barrel model, one that focuses instead on mayor–council interactions as the main mechanism through which increasing council size affects public finance and service outcomes. This framework may have wide applicability across other countries, especially those in the developing world where joint executive–legislature fiscal and service delivery decisions are constrained by very diverse political agendas and limited local revenue autonomy and finances. It is hoped that the research here will stimulate additional analyses of a similar kind in other countries. Such studies would be useful in determining the extent to which the unique empirical results presented here can be generalized beyond a single case.

Compliance with Ethical Standards

Conflict of Interest The author declares that he has no conflict of interest.

Appendix

Table 7 Population size, number of council seats, and districts in sample

		Number of seats	Number of districts in sample
Population thresholds 2004 elections			
1	≤ 100,000	20	22
2	> 100,000 and ≤ 200,000	25	74
3	> 200,000 and ≤ 300,000	30	50
4	> 300,000 and ≤ 400,000	35	31
5	> 400,000 and ≤ 500,000	40	25
6	> 500,000	45	110
Population threshold 2009 elections			
1	≤ 100,000	20	36
2	> 100,000 and ≤ 200,000	25	95
3	> 200,000 and ≤ 300,000	30	68
4	> 300,000 and ≤ 400,000	35	35
5	> 400,000 and ≤ 500,000	40	23
6	> 500,000 and ≤ 1,000,000	45	67
7	> 1,000,000	50	66

Source: General Elections Commission (KPU)

Table 8 Legislature size impact on district spending, service access, and own-source revenue—uniform kernel

Dependent variable	Tot. obs.	Bandwidth	Obs. left	Obs. right	τ	p	95% CI
Log district total spending per capita	2251	10.5	156	256	− 0.290	0.000	− 0.367 to 0.217
Log district capital spending per capita	2106	10.7	154	251	− 0.342	0.000	− 0.468 to 0.264
Annual change in service access	1875	9.3	92	220	− 0.964	0.000	− 1.442 to 0.507
Log district own-source revenues per capita	2346	3.1	29	106	− 0.071	0.957	− 0.717 to 0.679

Legislative size is the treatment variable. Tot. obs. is the total number of observations. Bandwidths are in thousands of persons. Obs. left and Obs. right are the effective numbers of observations used in estimation. τ is the conventional estimated treatment effect; p is the robust p value, and CI is the robust bias-corrected 95% confidence interval. All fiscal variables are measured in constant 2010 terms

Table 9 Legislature size impact on district spending, service access, and own-source revenue—alternative bandwidths

Dependent variable	Tot. obs.	Bandwidth	Obs. left	Obs. right	τ	p	95% CI	
25% narrower bandwidths								
Log district total spending per capita	2251	10.5	176	285	− 0.036	0.031	− 0.165	− 0.052
Log district capital spending per capita	2106	11.8	172	275	− 0.126	0.009	− 0.259	− 0.010
Annual change in service access	1875	9.8	111	222	− 0.861	0.037	− 1.149	− 0.662
Log district own-source revenues per capita	2346	4.0	58	106	0.050	0.645	− 0.161	0.261
25% wider bandwidths								
Log district total spending per capita	2251	17.5	314	415	− 0.186	0.008	− 0.122	− 0.018
Log district capital spending per capita	2106	19.6	341	447	− 0.212	0.002	− 0.219	− 0.035
Annual change in service access	1875	16.3	201	315	− 1.009	0.000	− 1.720	− 0.949
Log district own-source revenues per capita	2346	6.6	99	155	− 0.119	0.118	− 0.089	0.438

Legislative size is the treatment variable. Tot. obs. is the total number of observations. Bandwidths are in thousands of persons. Narrower and wider bandwidths are 75% and 125% of the bandwidths used to present the results in Table 4, respectively. Obs. left and Obs. right are the effective numbers of observations used in estimation. τ is the conventional estimated treatment effect; p is the robust p value, and CI is the robust bias-corrected 95% confidence interval. All fiscal variables are measured in constant 2010 terms

Table 10 Legislature size impact on district spending, service access, and own-source revenue—no change or increase in number seats across electoral periods

Dependent variable	Tot. obs.	Bandwidth	Obs. left	Obs. right	τ	p value
No change in number of seats across electoral periods						
Log district total spending per capita	915	6.88	31	40	− 0.204	0.084
Log district capital spending per capita	915	6.88	30	34	− 0.295	0.083
Annual change in service access	868	6.88	27	32	− 1.236	0.060
Log district own-source revenues per capita	991	6.88	31	40	0.111	0.453
Increase in number of seats across electoral periods						
Log district total spending per capita	1336	6.88	63	104	− 0.297	0.000
Log district capital spending per capita	1191	6.88	60	102	− 0.387	0.000
Annual change in service access	1007	6.88	53	91	− 1.331	0.005
Log district own-source revenues per capita	1355	6.88	63	105	0.088	0.244

Legislative size is the treatment variable. Treatment effects are estimated using randomization-based techniques due to the limited number of observations available after splitting the sample. Tot. obs. is the total number of observations. Bandwidths are in thousands of persons. Obs. left and Obs. right are the effective numbers of observations used in estimation. τ is the estimated treatment effect, and p is the robust p value. All fiscal variables are measured in constant 2010 terms

Table 11 Legislature size impact on individual service access variables

Dependent variable	Tot. obs.	Bandwidth	Obs. left	Obs. right	τ	p	95% CI	
Annual change in junior secondary school enrolment	1875	4.1	56	101	− 2.824	0.010	− 4.035	− 1.722
Annual change in senior secondary school enrolment	1875	9.3	120	248	− 1.318	0.002	− 1.933	− 0.434
Annual change percent attended births	1875	3.5	56	101	− 4.968	0.000	− 7.777	− 4.691
Annual change in percent household access to water	1875	14.5	275	364	0.493	0.269	− 0.324	1.357
Annual change in percent household access to sanitation	1875	8.2	132	241	− 0.551	0.091	− 1.571	− 0.000
Annual change in percent village access to paved roads	1875	7.9	104	197	− 0.369	0.099	− 1.026	0.036

Legislative size is the treatment variable. Tot. obs. is the total number of observations. Bandwidths are in thousands of persons. Obs. left and Obs. right are the effective numbers of observations used in estimation. τ is the conventional estimated treatment effect; p is the robust p value, and CI is the robust bias-corrected 95% confidence interval. All fiscal variables are measured in constant 2010 terms

Table 12 Window selection algorithm results for randomization-based RD treatment effect estimation

Window ±	*p* value	Covariate with minimum *p* value	Obs. left	Obs. right
1.81	0.211	Eastern Indonesia	21	41
2.07	0.233	Eastern Indonesia	26	67
2.34	0.176	Ethnic fractionalization	26	85
2.61	0.239	Newly created district	26	95
2.87	0.300	Gini coefficient	27	95
3.14	0.313	Gini coefficient	27	95
3.41	0.339	Eastern Indonesia	45	95
3.67	0.339	Eastern Indonesia	45	95
3.94	0.346	Eastern Indonesia	45	95
4.21	0.333	Eastern Indonesia	45	95
4.48	0.361	Eastern Indonesia	45	95
4.74	0.257	Eastern Indonesia	45	99
5.01	0.452	Log transfers per capita	45	104
5.28	0.411	Log transfers per capita	45	105
5.54	0.370	Newly created district	45	115
5.81	0.549	Ethnic fractionalization	53	115
6.08	0.520	Ethnic fractionalization	53	115
6.34	0.412	Ethnic fractionalization	53	121
6.61	0.224	Newly created district	59	128
6.88	0.200	Newly created district	59	128
7.15	0.041	Log household consumption per capita	61	137
7.42	0.051	Log household consumption per capita	68	157
7.68	0.002	Log household consumption per capita	72	174

Window is the size of window around the cutoff in months; *p* value is the minimum *p* value among all covariates used in the procedure; covariate with minimum *p* value is the name of the variable with the minimum *p* value, and Obs left and Obs right are the number of observations to the left and right of the cutoff within the window. The table reports the results of the window selection algorithm for log total spending per capita. Results for other outcome variables are similar

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