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The Impact of Mayor-Council Coalitions on Local Government Spending, Service Delivery, and Corruption in Indonesia

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This study examines the impact of majority coalitions on local government spending, service delivery, and corruption in Indonesia. The investigation finds that majority coalitions, i.e. those coalitions for which participating political parties control greater than half of council seats, cause a shift in local government spending towards health sector activities and induce improvements in citizen health service access—but only for a year or two, after which the positive effects disappear. The study shows that budget fraud starts to become problematic in the last two years of the coalition's life. Majority coalition support for the local health spending and service agenda dissipates quickly as attention turns to corrupting the budget, via increased infrastructure outlays and associated rent-seeking. We hypothesize that budget fraud serves, in part, to finance subsequent rounds of local parliamentary and executive elections.

1. Introduction

This study examines the impact of majority coalitions on public finance and governance outcomes in Indonesia. We employ regression discontinuity methods to identify the causal effects of majority coalitions—i.e. those coalitions for which political parties control more than half of the council seats—on local government spending, service delivery, and corruption. We investigate majority coalition impact both on average and across the years of coalition lives.

Both mayors and councilors are directly elected in Indonesia. Executive administrative offices are headed by civil servants and not used as a means of rewarding local parliamentarians that join coalitions. The local political system in Indonesia is therefore "presidential" in nature. Mayoral candidates establish coalitions with political parties represented on councils in the run-up to popular elections as a signaling device to voters to maximize election vote shares, and to potentially facilitate 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 coalitions: parties make themselves available to the highest candidate-bidder and would-be mayors and their financial backers pay those parties to join their coalitions. Coalition formation at the local level in Indonesia is 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).

Party motivations for joining pre-electoral coalitions (PECs) in Indonesia differ substantially from those identified in the literature. In parliamentary systems, research has consistently shown that political parties are encouraged to join PECs as a function of both the desire to influence policy and to gain office (e.g. cabinet appointments) (Golder, 2005; Golder, 2006a; Golder, 2006b; Bandyopadhyay & Oak, 2008; and Shin, 2018). These motivations are, in turn, based on a judgement by parties comprising PECs that they are likely be involved in government formation post-election (Martin and Stevenson, 2001; Golder, 2006a; Carrol and Cox, 2007). The belief seems reasonably well-founded. Debus (2009), for example, has demonstrated that pre-electoral alliances significantly influence post-election government formation in parliamentary systems in Europe.

In multi-party presidential systems, executive candidate agreements established with potential political party PEC partners are often argued to lack credibility (Mainwaring and Shugart, 1997). The unreliability of executive candidates' promises dampens office incentives for parties to form pre-election alliances. Policy motivations may still be important, however, if policy agreements among executive candidate-political party coalition partners are made public during the campaign (Kellam, 2017). Untenable commitments notwithstanding, some largely descriptive evidence suggests that presidential PECs often precede the formation of governing coalitions (Mainwaring and Scully, 1995). More analytically, Freudenreich (2016) has shown

that government formation in Latin American presidential systems is largely a function of pre-election agreements made between executive candidates and political parties in the legislature.

We assume here that coalitions established prior to elections have some role in governing after elections. Case study research in Indonesia suggests that this is indeed the case, at least some of the time (Buehler & Tan, 2007; Tans, 2012). Alternatively, this study can be interpreted as a test of the hypothesis that local election coalitions matter for decision-making, post-election.

We make a second assumption related to how local legislative decisions are made. Unlike in many legislatures around the world, decision-making in both national and local parliaments in Indonesia is based on deliberation and consensus (*musyawarah untuk mencapai mufakat*) and not vote-taking. Sherlock (2012) argues that in the Indonesian national parliament, consensus is essentially an agreement among party leaders. In this study, we assume that consensus in local parliament is driven by political party leaders in the coalition and that majority status of the coalition facilitates consensus. This position is consistent with recent research by Tans (2012) who notes that the most effective local governments in Indonesia are those that secure a majority in the local council. We test the assumption that council majorities matter later in the analysis.

The investigation finds that majority coalitions cause a shift in local government spending towards health sector activities and induce improvements in citizen access to health services—but only during the first year or two of the post-election period, after which the positive effects disappear. The finding that local governments with majority coalitions initially shift their focus to health spending and service delivery is consistent with recent qualitative case study research that argues that decentralized democracy in Indonesia has encouraged local governments to concentrate on healthcare policy to satisfy voter demands (Aspinall, 2013).

On the other hand, our study demonstrates that budget fraud and abuse start to become problematic in the last two years of majority coalitions' lives. Majority coalition support for the health spending and service agenda dissipates quickly, as attention turns to corrupting the budget, via increased infrastructure outlays and associated rent-seeking.[1] We hypothesize that budget fraud serves, in part, to finance subsequent rounds of local parliamentary and executive elections.[2]

This paper makes three contributions to the literature on the determination and consequences of majority coalitions. First, to the best of the authors' knowledge this is the first study to empirically examine the impact of local coalitions on corruption. Second, the analysis here also appears to be the first to investigate the temporal effects of local majority coalitions on outcomes of any kind. Third, this examination contributes in a more general way to the political economy literature on the effectiveness of decentralized institutions in delivering sound local public finance and governance outcomes (Bardhan, 2002; Zhang et al., 2004; Enikolopov & Zhuravskaya, 2007; Martinez-Bravo et al., 2011; Nye et al, 2014; Gonschorek et al., 2018).

The rest of the article is organized as follows. First, some background on fiscal and political decentralization and coalition building in Indonesia is provided. Second, the data and identification strategy used in the study are discussed. Third, the causal impact of majority coalitions on local government spending, service access, and corruption is examined. Fourth, the robustness of the empirical results is tested. Fifth, public finance and governance effects of majority coalitions over time are explored. The last section summarizes and concludes.

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[1] See Fisman and Gatti (2002), Dincer et al (2010), and Zakharov (2018) for analyses of corruption in other fiscally and politically decentralized contexts, the first two in US and the latter in Russia.

[2] Hanusch (2010) provides a review of the literature on political budget cycles and the different incentives faced by coalition political parties for abusing budgets for re-election purposes. For recent examples of local political budget cycles see Garmann (2017 and 2018).

2. Fiscal and political decentralization and coalition building in Indonesia

2.1 Fiscal decentralization

Indonesia is a unitary country and throughout most of its history it had one of the most centralized systems of government in the world. In 2001, however, the country began an ambitious effort to decentralize authority over public service delivery to subnational governments, comprising provinces and districts, where the latter have received the bulk of new service responsibilities.[3, 4]

District service assignments focus on health, education, and infrastructure. On average, local governments spend about 10, 35, and 20 percent of their budgets on these three functions, respectively. Additional important tasks include those related to agriculture, social protection, environment, low-income housing, and security, among others. Subnational government responsibility for service delivery is considerable. Subnational expenditure makes up approximately one-half of total public-sector spending net of subsidies and interest payments; local government expenditure comprises about 75 percent of the subnational total (Lewis, 2017b).

Local government revenues include those from: own-sources, shared taxes, shared non-taxes, a general-purpose grant, a specific-purpose grant, and others. The most important own-source revenues are taxes on electricity consumption and hotel and restaurant receipts and health service fees. Shared taxes consist of the property tax (through 2013, now decentralized) and the income tax. Shared non-taxes are derived from national forestry, fisheries, mining, and gas and oil revenues. The general-purpose grant is an equalization mechanism, which allocates funds

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[3] There are currently 34 provinces and 529 districts (not including Jakarta), comprising 430 mixed rural-urban places (*kabupaten*) and 99 municipalities (*kota*). Districts are also referred to as local governments in this paper.
[4] The major pieces of decentralization legislation—Laws 22 and 25—were promulgated in 1999. Implementation began in 2001.

based on a fiscal gap formula. The specific-purpose transfer is a matching capital grant (through

2015, now a proposal-based transfer). It spans numerous sectors but is concentrated in health,

education, and infrastructure. Other revenues include grants from the central government for

teachers and schools and transfers from provinces. Taken together transfers comprise just greater

than 90 percent of local revenue budgets (Lewis, 2017b).[5]

2.2   Political decentralization

Indonesian fiscal decentralization has been complemented by significant political developments

at the local level. Through 1998, both members of subnational parliaments (*Dewan Perwakilan*

Rakyat Daerah, DPRD) and subnational government executives were effectively appointed by

central government. In 1999, with the resignation of then President Suharto, the popular election

of DPRD representatives was (re-)introduced. DPRD members are elected for five-year terms.

Since the first polls, legislative elections have been held in 2004, 2009, and 2014.

DPRD elections are held across multiple voting districts (*dapils*) within subnational

government jurisdictions, where each *dapil* is allocated seats in the DPRD based on its relative

pre-election population size, given the DPRD's total number of seats. (The total number of

DPRD seats is also a function of pre-election population size, where the number of seats at the

district level varies from 20 to 50.) Over the period of study here, the number of *dapils* per local

jurisdiction ranged from two to seven and the number of seats per *dapil* varied from three to 12.

Subnational parliamentary elections are based on an open-list proportional representation

system. There is no electoral threshold for DPRD polls. The system is "permissive"—it seeks to

encourage strong linkages between politicians and citizens but at the same time facilitates

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[5] Central government also makes in-kind transfers to districts, called co-assistance (*tugas pembantuan*) funds. See Gonschorek et al (2018) for an analysis of the political motivations underlying the allocation of these in-kind transfers.

increased fragmentation of DPRDs across political parties (Tomsa, 2014). 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.

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 fiscal 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 local government responsibilities and play a key role

in formulating local medium-term development strategies, as well.

A very limited amount of empirical research has been undertaken on the public finance

impacts of local parliaments in Indonesia. Sjahrir et al (2014) find that growing political

fragmentation (as measured by the effective number of political parties) within DPRDs is

associated with (but does not necessarily cause) less administrative expenditure. On the other

hand, Lewis (2018b) determines that an increasing number of council members causes

significant decreases in capital project spending and leads to deteriorating service access.

In 1999 DPRDs began to appoint subnational government executives, as central

government appointed heads' terms expired. Indonesia initiated the direct election of subnational

government heads in 2005. Direct elections have since been implemented in a gradual manner, as

indirectly elected (DPRD-appointed) executives' terms terminated. The proportion of districts

with directly elected heads increased from around 40 percent in 2005 to just less than 100

percent in 2012. The process by which the popular election of subnational government

executives was instituted can be interpreted as an exogenously imposed transition from a

parliamentary style of government to a presidential one (Skoufias et al, 2014; Sjahrir et al, 2014; Lewis, 2018a).

There is a growing body of empirical research on the impact of direct local elections in Indonesia. Sjahrir et al (2013) find that incumbents running for re-election at the district level reallocate budgets (towards administrative spending) with a view to raising their popularity but not necessarily to improving overall fiscal outcomes. Skoufias et al (2014) determine that direct elections have had a positive influence on health expenditure but no impact on spending in other sectors or service outcomes. The authors also show that in the run-up to elections expenditure patterns for those districts with incumbents running for re-election shift towards the kinds of spending (personnel in this case) that are designed to garner votes but not to improve service delivery. Pierskalla and Sacks (2018) demonstrate that districts with elected executives spend less on capital as elections draw near.

Sjahrir et al (2014) show that direct district elections do not constrain the typically inefficient spending of local governments. On the other hand, Lewis (2017b) demonstrates that the spending of local governments with directly elected executives is more consistently associated with improvements to service delivery in education, health, and infrastructure sectors. Kis-Katos and Sjahrir (2017) provide evidence suggesting that direct elections have led to decreases in district infrastructure investment and they interpret this to mean the absence of beneficial effects. Lewis (2018a) also finds that districts with directly elected heads spend less, especially on infrastructure, compared to their counterparts with indirectly elected executives. In addition, he shows that local governments with directly elected heads spend more efficiently in pursuit of service outcomes than districts with indirectly elected officials. The author argues that districts led by directly elected executives are relatively less corrupt and that this reduced

corruption leads to reduced spending on rent-seeking intensive infrastructure projects and more efficient use of fiscal resources in general.

2.3 Coalition building

In the run-up to elections, subnational government executive aspirants solicit the support of political parties represented in associated DPRDs. As discussed above, they do so to create a base of (potential) voters that identifies with supportive political parties and to facilitate implementation of their policy and political agendas after taking office. The number of political parties backing the successful candidate—the winning coalition—varies widely across districts. For direct district elections held between 2005 and 2012 the number of political parties forming coalitions ranged from zero to 15 across districts. The share of parliamentary seats held by coalitions varied between zero and 95 percent during the period.

Recent research on local coalitions in Indonesia has concentrated on deriving and examining coalition typologies. Tans (2012) argues that three types of political coalitions predominate in Indonesia, which he terms: party machines, political mafias, and mobilizing coalitions. Machines are more vertically oriented, i.e. towards national level political institutions; mafias are more focused on their relationships with local elites; while mobilizing coalitions concentrate on popular citizen concerns. In case studies of three districts in North Sumatra the author finds that different coalition types are persistently in competition with another and that coalitions may change type over time to achieve and maintain power.

3. Data and identification

3.1 Data

The independent variable of main concern in this study is the coalition's share of seats in the DPRD. (This is the running variable in the RD analysis.) We have unique data on the political

parties that form the winning coalition and the share of seats held by those parties in local

parliament for 488 districts over the period 2005-2012. We focus attention on majority

coalitions, defined as those for which the share of seats exceeds 50 percent. During the period of

study, just less than 30 percent of districts had majority coalitions, as defined here.[6] The relevant

data have been provided by the Indonesian General Elections Committee (KPU).

Our objective is to examine the influence of majority coalitions on post-election local

government spending, in the aggregate and by function—health, education, and infrastructure—

as well as the impacts on relevant service outcomes and corruption. For reasons that will become

apparent we focus on health service access: percent of births attended by a health professional

and the percent of children less than five years of age with proper immunizations.[7] Local

government spending data come from the Ministry of Finance (MoF) and service access data

have been provided by the Central Bureau of Statistics (BPS) annual household socio-economic

survey (SUSENAS).

We use external local government audit opinions as our proxy for budget corruption.[8]

The Indonesian Supreme Audit Agency (BPK) carries out audits of district budget execution on

an annual basis. Standard BPK local government audits examine: the adequacy of internal

controls; compliance with laws and regulations; and the clarity and accuracy of information

contained in official cash budgets, operating statements, and balance sheets. BPK is also required

to report any evidence of local government corruption to the police, attorney general, or the

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[6] Only 10 districts in our sample (two percent of the total) were led by "majority governments"—that is, districts for which the majority election coalition comprised just one party (i.e. that of the elected mayor). We include these local governments in the analysis; they form part of the set of majority coalitions. The main results of the analysis do not change if majority governments, as defined here, are dropped.
[7] Health service access variables are selected based on the availability of data. There are no comprehensive data on health service quality for Indonesia.
[8] See Brollo et al (2013) for the similar use of financial audits as an indicator of "broad corruption" among Brazilian municipalities.

corruption eradication commission (KPK) (McLeod & Harun, 2014). BPK offers four main types

of opinions with respect to its investigations: disclaimer, adverse, qualified, and unqualified

(ranging from least to most satisfactory).[9]

Researchers have found that a major cause of substandard district audit performance in

Indonesia is corrupt budget execution practices, especially as tied to contracting out public works

projects (Sacks et al, 2014). In addition, Lewis (2017b) shows that the spending of local

governments that do poorly on their audits is less consistently and positively related to service

access than the expenditure of districts that perform well on their audit reports. He argues that

unsatisfactory audit opinions are a reasonable proxy for budget-related corruption among local

governments in Indonesia and that such corruption leads to declining service outcomes. In

another study Lewis (2017a) shows that deficient audit results are strongly associated with weak

governance outcomes as proxied by a general economic governance index developed by the

well-known Indonesian NGO Regional Autonomy Watch (KPPOD, 2008).

In this paper, we construct a dummy variable for audit outcomes to proxy budget

corruption by setting disclaimer and adverse opinions equal to zero and qualified and unqualified

opinions equal to one. The assumption is that budget corruption decreases as audit outcomes

change from the former to the latter. Data on audit opinions used to create the bivariate variable

come directly from BPK.

We make use of several other covariates in our analyses: number of candidates in the

mayoral election, number of political parties in the local parliament, population, population

density, ethnic fractionalization, religious fractionalization, intergovernmental transfers, poverty

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[9] Two recent peer reviews of BPK operations have been largely positive about the institution's independence and proficiency. For a discussion of relevant issues and suggestions for improvements see Netherlands Court of Audit (2009) and Supreme Audit Office of Poland (2014).

rate, and gross regional domestic product (GRDP). These variables are used to test the robustness of key derived results; the relevant (balance) tests are described in more detail below. Data on the numbers of candidates and political parties are from KPU; data on transfers have been provided by MoF; data on ethnic and religious fractionalization derive from the national census[10]; and data on all other variables come from BPS/SUSENAS.

Data on all variables used in this study are available for the period 2005-2012, except for child immunization rates for which we have data just through 2011. Summary statistics for all variables used in the investigation are presented in Table 1.

[Table 1]

3.2   Identification

Political coalitions are likely to be endogenous, since outcomes of interest (local government spending, service delivery, and audits) and coalition size and structure may both be determined by omitted (unobservable) variables—voter preferences, for example (Garmann, 2015). To assess the causal impact of majority coalitions on fiscal, public service, and governance outcomes we adopt a regression discontinuity (RD) design.

An RD set-up requires a running variable and a cut-off. The running variable determines the cut-off and the cut-off divides the sample into treatment and control units. The basic intuition behind the RD methodology is that, within narrow bounds on either side of the cut-off, units are the same in all regards, except for treatment assignment. We can say, heuristically at least, that within those bounds, the assignment of units to treatment and control is "as good as random". As

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[10] The religious and ethnic fractionalization indices are constructed using the following expression. $Fractionalization = 1 - \sum_{m} s_{mi}^2$, where s_{mi} is the population share of the religious or ethnic group m in the total number of religious or ethnic groups in district i. The index varies between zero (perfect homogeneity) and one (perfect fractionalization).

such, RD designs are very effective at reducing selection bias associated with the endogeneity (or the non-random assignment) of treatment (Cattaneo et al, 2018).

In the current context, the coalition's share of DPRD seats is the running variable. The cut-off value of the running variable is 0.50. When a coalition's share of seats exceeds 0.50 a majority coalition exists. Majority coalitions represent the treatment in our RD set-up.

Following Imbens and Lemieux (2008), define $Y_i(0)$ and $Y_i(1)$ to be a potential spending, service, or audit outcome of interest for district i where $Y_i(0)$ is the outcome without exposure to majority coalitions and $Y_i(1)$ is the outcome under majority coalitions. In this case, the impact of majority coalitions 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 majority coalitions, $Y_i(1) - Y_i(0)$, across subgroups of the relevant population. Let $D_i = 0$ if a district was not exposed to a majority coalition and $D_i = 1$ if it was exposed to a majority coalition. 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 majority coalitions, τ, at the cut-off, c=0.50, is given by:

$$\tau = \mathrm{E}\left[Y_i(1) - Y_i(0) \mid X_i = c\right] = \mathrm{E}\left[(Y_i(1) \mid X_i = c\right] - \mathrm{E}\left[Y_i(0) \mid X_i = c\right] \tag{1}$$

The key identifying assumption in this framework is that $\mathrm{E}[Y_i(1) \mid X_i]$ and $\mathrm{E}[Y_i(0) \mid X_i]$ are continuous in X, coalition share of seats. This implies that all other unobserved determinants of public finance and governance outcomes, Y, are also continuously related to X (Imbens and Lemieux, 2008). The implication allows one to use outcomes just to the left of the cut-off as valid counterfactuals for those just to the right of the cut-off (Cattaneo et al, 2018).

The general form of the estimating equation is:

$$Y_i = \tau D_i + g(X_i) + \mu_i \tag{2}$$

In equation (2) the subscript i refers to the district; Y is the outcome; g(X) is a polynomial function of the running variable X, coalition share of seats; μ is the error term, and τ is the treatment effect, which is to be estimated.

We estimate the treatment effect using non-parametric regression techniques within narrow windows (bandwidths) on each side of the cut-off. In this context three choices must be made: the degree of polynomial of the regression equation, the type of kernel, and the bandwidth. Recent research argues for the use of lower order polynomials and we employ polynomials of degree one and two (Gelman & Imbens, 2014; Cattaneo et al, 2018). We use a rectangular (or uniform) kernel[11] and choose our bandwidths in a data-driven fashion to minimize the mean squared error (MSE) of the RD point estimator[12] as the latest work in this area advocates (Imbens & Kalyanaraman, 2012; Cattaneo et al, 2018).

The methods described here identify a local average treatment effect (LATE) (Lee & 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 coalition government impact at values of the forcing variable outside a narrow range around the cut-off.

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[11] Rectangular kernels weight all observations equally. Some analysts prefer to use a triangular kernel, which weights the observations closer to the cut-off more heavily in the non-parametric regressions. We use a rectangular kernel so that the non-parametric regression results we derive are more comparable with those generated by the parametric (OLS) and probit models (i.e. which employ a rectangular kernel) used later in the study to examine the effects of coalition duration on outcomes. In fact, empirical results shown in this investigation for the non-parametric regressions are very robust with respect to kernel choice.

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

4.   Majority coalition impact on local spending, service delivery, and budget corruption

4.1   Identifying assumption

For the RD approach to be valid there must be no precise manipulation of the running variable—

the coalition's share of council seats—around the cut-off. Since political parties determine the

coalitions in which they participate it might reasonably be expected that parties would

collaborate with one another to establish majority coalitions, which, in turn, would assist them in

influencing local policy formulation. As previously noted, other studies have found that political

parties are "policy motivated" in this regard (e.g. Kellam, 2017). The argument in this paper,

however, is that local political parties in Indonesia are typically not inspired by policy concerns

in forming coalitions. They are instead "money motivated". That is, what parties care about is the

magnitude of the payoff offered to join a coalition; they are largely indifferent about coalition

seat share. As such, manipulation in this case is improbable.

On the other hand, mayoral candidates are certainly motivated to establish majority

coalitions to influence policy, among other things, after being elected. The question in this

instance is whether candidates have *precise* control over the coalition's share of seats at the cut-

off. We argue that they do not have such control. Typically, many candidates run in any single

election and all candidates know the rules of the game, have access to the same information, and

attempt to establish as large a coalition as possible. Given candidate competition in this context,

it seems unlikely that any single candidate would be able to precisely or completely control its

coalition's share of seats at the cut-off. Such control would imply that a candidate with a

coalition comprising, say, 49 percent of council seats would easily be able to enlist another party

to join the coalition, thereby covering 51 percent of the seats, or more, for example. This seems

implausible in the institutional environment outlined above. All things considered, therefore we assume that there is no precise manipulation of coalition share of seats.[13]

We now test that assumption. Figure 1 shows the frequency distribution of the coalition's share of seats. Visual inspection does not suggest any discontinuity around the cut-off (shown by the vertical line at 0.50). 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, with jack-knifed standard errors (the default procedures) is -0.838 and the p value is 0.402. The evidence therefore implies no manipulation of the forcing variable at the threshold.

[Figure 1]

4.2 District spending, service delivery, and corruption effects

We begin the treatment effects analysis by examining standard RD plots. Figure 2 shows the plots for log total spending per capita and spending budget shares for health, education, and infrastructure, relative to the coalition's share of seats on the local council. Each dot in the figure represents the average value of the spending outcome for a data-driven selected range (bin) of the coalition share of seats. Local polynomial regressions of order three are superimposed on the data on both sides of the cut-off to best illustrate the relationships (Cattaneo et al, 2018).

Attention is drawn to the relationships at the cut-off. The figure shows a pronounced jump around the cut-off for health budget shares but no apparent effects for total spending or for education and infrastructure budgets. We tentatively conclude that majority coalitions have no

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[13] Of course, mayoral candidates may be money motivated as well, and as such it is plausible to argue that they would be motivated to set up majority coalitions in order to facilitate their rent-seeking agendas if they were elected. Nevertheless, as argued above, it is unlikely that candidates would have sufficiently precise control to implement such an agenda and this, again, argues against manipulation.

impact on the total amount of expenditure, but that they may lead to a shift in spending towards the health sector and away for all other functions, collectively.

[Figure 2]

We now provide formal empirical estimates of majority coalition treatment effects as illustrated in the above RD plots, by estimating equation (2). As previously noted we employ non-parametric estimation procedures, using a rectangular kernel and MSE optimal bandwidths. We estimate effects using polynomials of both degree one and two. Estimated standard errors are clustered at the district level.

Table 2 provides the results. For each regression the table shows: total observations, MSE-optimal bandwidths, the number of observations to the left and right of the cut-off that are used in estimation procedures, the conventional estimated treatment effect (τ) and robust bias-corrected p-values and 95 percent confidence intervals (Cattaneo et al, 2018).

[Table 2]

The estimation output confirms the suspected positive impact of majority coalitions on health sector budget shares. The results for the non-parametric regression using a polynomial of degree one implies that majority coalitions lead to a 2.5 percentage point increase in health budget shares, on average. Given average health budget shares of 9.6 percent, this implies that coalitions cause an approximate 26 percent rise in health spending (i.e. 2.5/9.6), a non-trivial increase. The second-degree polynomial regression results show a slightly smaller treatment effects point estimate, indicating that majority coalitions cause a 2.1 percentage point rise in health budget shares. On the other hand, and as the RD plots imply, the treatment effects regressions demonstrate that majority coalitions have no statistically significant impact on either total local government spending or on education or infrastructure expenditure budget shares.

The output in Table 2 also provides the estimated effects of majority coalitions on health service access indicators and audit opinions as well. The table shows that the estimated impact of majority coalitions on attended births is positive and statistically significant at the 10 percent level. The sign of the majority coalition effect is positive for child immunizations, as well, but the estimated impact is not statistically significant by the usual standards. The associated p value is 0.13. The sign of the treatment effect coefficient on audit outcomes is negative, but again the estimated impact is not strictly statistically significant. In this case the p value is 0.14. Estimation results for regressions with second degree polynomials show no statistically significant effects for attended births, child immunizations, or audits.

In broad summary, the empirical analysis thus far suggests that local majority coalitions have no effect on the total amount of district spending but that they do apparently cause a shift in spending towards the health sector and away from all other functions combined. The finding that districts with majority coalitions reallocate spending towards health functions is consistent with recent qualitative case study research by Aspinall (2013), who argues that decentralized democracy in Indonesia has encouraged local government executives and councilors to concentrate on healthcare policy, as a means of getting elected. The average effects of majority coalitions on local health service access and budget corruption are less clear, however. Although the direction of coalition impact on health service access variables is positive, the estimated treatment effects are not strongly significant, in general. The direction of coalition impact for audit opinions is negative but it too is statistically insignificant by the usual standards.

5.   Robustness tests

5.1   Majority coalitions and consensus decision-making

We begin by testing the robustness of our derived results to the assumption that majority

coalitions matter for local executive-legislative decisions. If majorities are unimportant in the

context of consensus type decision-making, then the 50 percent cut-off is arbitrary, and the

statistically significant results found at that cut-off are spurious. Furthermore, if the majority

threshold and derived results are false we might well expect to find significant results at other

cut-offs too. To test this conjecture, we examine coalition effects on health budget shares at

different coalition seat share thresholds: 30, 35, 40, 45, 55, 60, 65, and 70 percent.

Table 3 presents the relevant output. The results show that coalitions have no significant

impact on health budget shares at any of the alternative seat share thresholds, for either

polynomial degree one or two regressions. The fact that statistically significant impacts are found

uniquely at the 50 percent cut-off provides reasonable support for the argument that majority

coalitions matter for legislative consensus-type decision-making related to budgets at the local

level in Indonesia.

[Table 3]

5.2   Covariate balance

We now test the robustness of our results to the assumption of covariate balance. The treatment

effects analysis carried out here assumes that pre-determined covariates or, alternatively, placebo

outcomes are balanced around the majority coalition threshold. That is, they do not exhibit jumps

around the cut-off. If they were not balanced in this manner, then our identification strategy

would be called into question. We test the balance assumption using several important covariates

on which data are available: number of candidates in the local executive election, number of

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political parties represented in the local parliament, log population, log population density, ethnic

fractionalization, religious fractionalization, log intergovernmental transfers per capita, poverty,

and log GRDP per capita.

The number of candidates in the local executive election and the number of political

parties in the DPRD were determined before treatment was assigned and thus we would not

expect to see any significant treatment effect. All other variables were determined at the same

time as or after coalitions were formed but we would not anticipate treatment to affect these

variables either since those outcomes are largely outside district control. Nevertheless, all these

variables have the potential at least to influence local government spending, service, and audit

outcomes in some manner and thus confound the estimation of treatment effects.

Table 4 supplies the formal treatment effects estimation results for the balance tests.

None of the estimated treatment effects is statistically significant at even the 10 percent level.

Overall the results support the conclusion that the shift in spending towards health and the

possible improvements to health service access and negative effects on budget corruption are a

consequence of majority coalitions and not driven by the influence of other covariates. [14]

[Table 4]

The general conclusion regarding positive majority coalition effects on health spending

notwithstanding it may be useful to reiterate that the estimated results probably do not have

significant external validity. Derived spending, service, and audit effects are best thought of as

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[14] As one reviewer remarked, the number of political parties on the council may also influence the ability of mayoral candidates to form majority coalitions and the effectiveness of those coalitions, post-election. To test the heterogeneity of treatment effects with respect to the number of political parties on the council, we re-estimate the model across two subsets of data, one for which the number of political parties on the council is less than or equal to nine and the other for which the number of parties is greater than nine. We choose nine political parties to divide the sample into sub-samples to assure an adequate number of effective observations for estimation across the two groups. We find that the treatment effect estimates are very robust with respect to the two sub-samples defined in this way, suggesting that majority council treatment effects are not heterogeneous with respect to the number of council political parties. The full results are presented in Table A1 in the online Appendix.

being valid only for political coalitions that control just greater than 50 percent of the seats in parliament compared to those that hold just fewer than 50 percent of the seats. More substantively, this suggests perhaps that estimated impacts are valid only for those coalitions that operate in highly contested political environments.

6. Majority coalition effects over time

This section of the paper explores the impact of majority coalition over post-election coalition lives. To examine the impact of local majority coalitions over time we estimate another (this time parametric) version of the treatment effects model used above. We posit the following relationship between local government spending, service outcomes, and audit opinions and majority coalition treatment.

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 CY_i + \beta_3 D_i \cdot CY_i + \beta_4 X_i + \beta_5 CY_i \cdot X_i + \varepsilon_i \tag{3}$$

In equation (3) the subscript i represents districts; Y is a fiscal, service, or audit outcome; D is treatment, i.e. a dummy variable that takes on the value one if the coalition's share of seats is greater than or equal to 0.50, else zero; CY is the coalition year, discussed in greater detail below; X is the coalition's share of seats; and ε is the usual error term.

Coalition year, CY, indicates the year in the life of a pre-electoral coalition. It varies between one and four, the maximum life of a coalition in our sample. DPRD elections were held in 2004 and 2009. Local government executive elections started in 2005 and were implemented gradually throughout the study period (i.e. 2005 through 2012). Local executives that were elected in 2005, for example, will have formed election coalitions with DPRD parties that were elected in 2004. The coalition endures until 2009, i.e. for a period of four years, when new DPRD elections were held and at which time the executive will have had one year left in his/her term. District heads elected in 2009 will have established coalitions with DPRD members elected

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the same year. This coalition continues for four years as well i.e. through the end of the study period. The lives of political coalitions associated with district executives elected in all other years will be shorter, varying between one and three years.

We take coalition year to be exogenous. Coalition year is a function of the exogenously imposed start dates of local legislative and executive elections (2004 and 2005, respectively), exogenously determined term limits (five years), and, more generally, the timing of direct executive elections. Empirical evidence regarding the exogeneity of the latter has been provided by other researchers (Skoufias et al 2014; Lewis 2018a) and we adopt the same exogeneity assumption here. Given that coalition year is completely determined by three exogenous variables it is reasonable to treat it as exogenous as well.

The model as written in equation (3) can be interpreted as combining elements of a difference-in-differences approach with an RD design. Note that in this specification coalition year is interacted with the coalition's share of seats (i.e. the running variable) as recent research demonstrates should be the case in such a set-up (Egger & Kothenburger, 2010; Garmann, 2015). The model essentially constructs four natural experiments, one for each of four coalition years. The main coefficients of interest are β_1, β_2, and β_3, which provide the direct treatment effect (ignoring interactions), the direct impact of coalition year (holding interactions constant), and the influence of treatment and coalition year interactions on outcomes of concern, respectively.

In estimating equation (3) we focus on health budget shares, health service access—attended births and child immunizations—and budget corruption, as proxied by external audit opinions, as well as infrastructure budget shares. Estimation is by ordinary least squares for the budget share and service equations and by probit for the audit specification.[15] Standard errors are

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[15] Estimation by OLS and probit implies the use of a rectangular kernel, for which all observations within the estimation bandwidth are weighted equally.

clustered at the district level. As before, the specification is estimated within narrow bandwidths around the cut-off. In each of the five regressions here we use the same bandwidths as derived in the data-driven procedures for the relevant non-parametric regressions estimated above.

Table 5 presents the output of main interest. The table shows the estimated treatment effects for health budgets, health service access, audit opinions, and infrastructure spending budget shares (at average values of all other variables), and those effects at each of the four years of the majority coalition's life.

[Table 5]

The results in the table demonstrate four main points. First, the sign, magnitude, and statistical significance of the overall treatment effects in each of the five equations are quite comparable to those estimated using non-parametric techniques as reported in Table 1. The consistency of estimation results inspires confidence in their robustness.

Second, the estimation output suggests that majority coalition health sector effects vary over the duration of the executive-legislative coalition. For health budget shares, treatment effects are positive and statistically significant for the first two years of the coalition and then become insignificant. The point estimates suggest that majority coalitions lead to a 3.1 to 2.5 percentage point increase in health budgets during the first two years only. For attended births and child immunizations, majority coalition impacts are significant (at the 10 percent level for births) and positive for just for the first year and then turn insignificant. In these cases, majority coalitions lead to a 10.2 percentage point increase in attended births and 3.9 percentage point rise in child immunizations in the first year of a coalition's existence.[16] Overall, these estimation

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[16] Treatment effect estimates for the health service regressions are imprecise, as judged by the associated standard errors. Imprecise coefficient estimates are a function of measurement error associated with the health service indicators, which is turn is caused by small district-level sample sizes used by BPS in implementing the SUSENAS survey (Lewis et al, 2016).

results imply that coalitions become increasingly ineffective in delivering positive health spending and service outcomes over time.

Third, the table shows that the impact of majority coalitions on audit outcomes also varies over the life of the coalition. The effects are statistically insignificant in the first two years of the coalition's existence but become strongly significant and negative in the last two years. Specifically, the results imply that majority coalitions lead to a 36.8 and 48.4 percentage point decrease in the probability that districts receive a qualified or unqualified opinion on their external audits in the last two years of the coalition, respectively. Given our assumptions in this study, the result implies that majority coalitions eventually lead to rising budget corruption.

Finally, the table shows that majority coalitions have no initial impact on infrastructure expenditure budget shares but that these effects become positive and statistically significant (at 10 percent level at least) during the later years. Specifically, majority coalitions lead to around 3.2 and 5.1 percentage point increases in infrastructure spending in the final two years of the coalition. Given that infrastructure is the sector in which much of the local budget fraud takes place, via falsified contracting and kick-backs (Sacks et al, 2014), such outcomes offer corroborating evidence that corruption becomes problematic towards the end of coalition lives.[17]

7. Conclusions

This study has examined the impact of majority coalitions on district spending, service delivery, and budget corruption. The investigation finds that majority coalitions, i.e. those coalitions that control greater than half of council seats, cause a shift in spending towards health sector activities and induce improvements in citizen health service access—but only during the first year or two of a coalition's existence, after which the positive effects disappear. On the other

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[17] We find no treatment significant effects across the years of coalition lives for aggregate spending, education spending, or service outcomes other than those for health.

hand, the study shows that budget fraud and abuse, as evidenced by worsening audit outcomes and increased spending in the corruption-prone infrastructure sector, become especially evident in the last two years of the majority coalition after elections.

In the end, therefore, local political coalitions disappoint. Majority coalition support for the local health spending and service agenda dissipates quickly as attention turns to corrupting the budget, via increased infrastructure outlays and associated rent-seeking. It is useful to emphasize that budget corruption starts to become particularly problematic in the final years of a coalition's life, i.e. in the run-up to new council and mayoral elections. Although we cannot be certain given the data at hand, we hypothesize that budget fraud serves, in part, to finance subsequent rounds of local legislative and executive elections. This conjecture is consistent, at least, with other recent work on Indonesia that finds the existence of strong local political business cycles (Sjahrir et al, 2013; Skoufias et al, 2014; Pierskalla & Sacks, 2017).

It is difficult to discern the extent to which the majority coalition effects found in this study might be more widely applicable. Indonesia certainly engages in what appear to be quite unique coalition building practices, to say the least. It may be that Indonesia constitutes an outlier in this context and that the derived results are specific to Indonesia. Only additional empirical research can answer this question. This study suggests that further work along these lines would be useful, especially in other developing countries where local political institutions are not yet well developed, and governance issues are a major concern.

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Table 1

Summary statistics

Variable	No. Obs.	Mean	SD	Minimum	Maximum
Total expenditure per capita	2,426	4,236	5,184	14	69,100
Health expenditure budget share	2,175	0.096	0.033	0.008	0.269
Education expenditure budget share	2,175	0.332	0.116	0.010	0.925
Infrastructure expenditure budget share	2,353	0.192	0.045	0.068	0.476
Percent of births attended by a health worker	3,015	75.1	20.5	1.6	100.0
Percent of children under age five with immunizations	1,838	80.2	11.3	4.7	100.0
Audit opinions	2,778	0.460	0.499	0.000	1.000
Coalition share of seats	3,031	0.315	0.173	0.000	0.950
Number of candidates in local executive election	3,026	4.1	1.5	2.0	10.0
Number of political parties in local parliament	3,031	11.3	3.0	4.0	22.0
Population	3,026	508,026	567,815	12,587	5,202,097
Population density	2,278	954	2,082	1	16,575
Ethnic fractionalization	2,982	0.456	0.321	0.004	0.998
Religious fractionalization	2,789	0.187	0.188	0.002	0.700
Intergovernmental transfers per capita	2,933	4,163	5,675	24	71,100
Percent of population below poverty line	3,016	0.153	0.102	0.012	0.651
GRDP per capita	2,283	61,264.0	1,148.2	59,012.9	63,515.0

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

Table 2

Majority coalition treatment effects: Fiscal, service, audit outcomes

	Polynomial Degree One								Polynomial Degree Two							
Dependent Variable	Tot. Obs.	Bandwidth	Obs. Left	Obs. Right	τ	p	95% CI		Tot. Obs.	Bandwidth	Obs. Left	Obs. Right	τ	p	95% CI	
Log total expenditure per capita	2,426	0.081	217	147	0.139	0.479	-0.370	0.789	2,426	0.122	332	212	0.608	0.117	-0.107	1.376
Health budget share	2,175	0.116	293	190	0.025	0.010	0.007	0.050	2,175	0.137	362	207	0.021	0.026	0.005	0.069
Education budget share	2,175	0.105	293	190	0.019	0.629	-0.054	0.089	2,175	0.178	518	257	0.009	0.962	-0.088	0.084
Infrastructure budget share	2,353	0.100	210	145	-0.005	0.816	-0.031	0.025	2,353	0.144	422	236	0.010	0.474	-0.020	0.044
Attended births	3,015	0.096	274	191	10.400	0.094	-1.801	22.96	3,015	0.088	274	191	2.130	0.759	-16.86	23.12
Child immunizations	1,838	0.069	153	98	1.817	0.133	-0.634	4.784	1,838	0.095	176	110	0.567	0.749	-3.288	4.568
Audits	2,252	0.070	175	123	-0.177	0.137	-0.545	0.074	2,252	0.087	205	140	0.078	0.531	-0.282	0.549

Determination of bandwidths is data driven. Number of observations to the left and right of the cut-off point is the effective number used in estimation. Estimated treatment effect is conventional, and the p-value and 95% confidence interval are robust and bias-corrected. Standard errors are clustered at the district level. All fiscal and economic variables are measured in constant 2014 terms.

Table 3

Coalition treatment impact on health budget shares: Alternative cut-offs

	Polynomial Degree One					95% CI		Polynomial Degree Two					95% CI	
Cut-Offs	Bandwidth	Obs. Left	Obs. Right	τ	p			Bandwidth	Obs. Left	Obs. Right	τ	p		
30 percent	0.084	443	776	-0.005	0.411	-0.019	0.005	0.099	443	376	0.003	0.844	-0.021	0.026
35 percent	0.133	714	381	0.008	0.229	-0.003	0.021	0.215	1245	510	0.007	0.272	-0.005	0.024
40 percent	0.135	553	370	-0.001	0.793	-0.016	0.012	0.214	986	483	0.000	0.906	-0.018	0.016
45 percent	0.149	517	209	-0.013	0.130	-0.030	0.001	0.262	1201	361	-0.009	0.217	-0.031	0.004
55 percent	0.123	252	165	-0.005	0.620	-0.026	0.014	0.217	597	222	-0.009	0.491	-0.035	0.015
60 percent	0.124	174	159	-0.015	0.096	-0.033	0.003	0.161	285	176	-0.013	0.377	-0.041	0.012
65 percent	0.700	102	50	0.011	0.695	-0.028	0.046	0.094	143	63	0.003	0.977	-0.047	0.045
70 percent	0.077	84	25	-0.029	0.243	-0.084	0.016	0.055	58	17	-0.092	0.529	-0.228	0.089

The total number of observations in each regression is 2,175. Determination of bandwidths is data driven. Number of observations to the left and right of the cut-off point is the effective number used in estimation. Estimated treatment effect is conventional, and the p-value and the 95% confidence interval are robust and bias-corrected. Standard errors are clustered at the district level. All fiscal and economic variables are measured in constant 2014 terms.

Table 4

Majority coalition treatment effects: Covariate balance

| | Polynomial Degree One | | | | | | | | Polynomial Degree Two | | | | | | | |
Dependent Variable	Tot. Obs.	Bandwidth	Obs. Left	Obs. Right	τ	p	95% CI		Tot. Obs.	Bandwidth	Obs. Left	Obs. Right	τ	p	95% CI	
No. of candidates in local election	3,026	0.086	276	193	-0.323	0.364	-1.254	0.407	3,026	0.112	420	269	-0.656	0.172	-1.926	0.302
No. of political parties in local parliament	3,031	0.157	276	193	-1.516	0.201	-3.665	0.684	3,031	0.106	420	269	-2.564	0.113	-6.160	0.406
Log of population	3,026	0.073	231	170	-0.491	0.168	-1.328	0.164	3,030	0.108	420	268	-0.490	0.291	-1.493	0.416
Log of population density	2,665	0.131	424	238	0.401	0.643	-0.988	1.748	2,665	0.177	621	304	0.601	0.472	-1.051	2.498
Ethnic fractionalization	2,982	0.090	272	187	0.107	0.292	-0.092	0.357	2,982	0.147	555	305	0.064	0.561	-0.172	0.329
Religious fractionalization	2,789	0.087	254	184	0.040	0.497	-0.081	0.179	2,789	0.141	513	290	0.055	0.373	-0.078	0.223
Intergovernmental transfers per capita	2,158	0.082	191	131	-0.020	0.935	-0.459	0.508	2,158	0.156	420	233	0.399	0.202	-0.183	1.159
Percent of people below poverty line	3,016	0.099	275	191	-0.017	0.482	-0.066	0.029	3,016	0.102	419	265	-0.027	0.495	-0.096	0.044
Log GRDP per capita ('000 000)	2,669	0.159	535	283	-0.073	0.736	-0.417	0.281	2,669	0.168	613	299	-0.200	0.338	-0.683	0.191

Determination of bandwidths is data driven. Number of observations to the left and right of the cut-off point is the effective number used in estimation. Estimated treatment effect is conventional, and the p-value and 95% confidence interval are robust and bias-corrected. Standard errors are clustered at the district level. All fiscal and economic variables are measured in constant 2014 terms.

Table 5

Majority coalition treatment effects by coalition year

Independent Variables	Hlth Budget Share			Births			Immunizations			Audits			Inf Budget Share		
	Coef.	SE		Coef.	SE		Coef.	SE		Coef.	SE		Coef.	SE	
Treatment effect	0.024	0.011	**	9.898	6.347		1.937	1.385		-0.178	0.138		0.012	0.012	
Treatment effect at coalition year=1	0.031	0.001	**	10.163	5.807	*	3.948	1.570	**	0.004	0.197		-0.008	0.012	
Treatment effect at coalition year=2	0.025	0.011	**	9.927	6.211		1.722	1.409		-0.202	0.150		0.012	0.012	
Treatment effect at coalition year=3	0.018	0.013		9.969	7.711		-0.534	2.015		-0.368	0.165	**	0.032	0.018	*
Treatment effect at coalition year=4	0.012	0.017		9.455	9.817		-2.790	2.949		-0.484	0.171	***	0.051	0.025	**
Number of observations	483			465			251			298			464		

Dependent variables are health budget share, percent of births attended by a health professional, percent of children under five years of age with proper immunizations, audit opinions, and infrastructure budget share and are listed across the second top row. Bandwidths for each regression are the same as the data-driven bandwidths derived for the relevant non-parametric regressions shown in Table 2. Estimation is by OLS for the health budget share, attended births, child immunization, and infrastructure budget share regressions and by probit for the audit regression. Standard errors are clustered at the district level. ***, **, and * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table A1

Majority coalition treatment effects: Fiscal, service, audit outcome heterogeneity with respect to number of political parties on the council

Dependent Variable	ANP≤9								ANP>9							
	Tot. Obs.	Bandwidth	Obs. Left	Obs. Right	τ	p	95% CI		Tot. Obs.	Bandwidth	Obs. Left	Obs. Right	τ	p	95% CI	
Log total expenditure per capita	783	0.141	167	118	-0.019	0.951	-0.617	0.656	1,643	0.106	201	110	0.029	0.775	-0.587	0.788
Health budget share	716	0.136	143	102	0.034	0.010	0.002	0.078	1,459	0.087	109	68	0.022	0.030	0.002	0.047
Education budget share	716	0.135	143	102	0.030	0.478	-0.056	0.119	1,459	0.079	100	65	-0.011	0.739	-0.149	0.106
Infrastructure budget share	753	0.079	87	61	0.017	0.192	-0.011	0.055	1,600	0.083	123	77	-0.022	0.188	-0.059	0.012
Attended births	912	0.096	104	81	2.230	0.708	-14.21	20.94	2,103	0.134	326	163	6.303	0.321	-6.004	18.34
Child immunizations	676	0.102	115	91	1.858	0.224	-1.137	4.852	1,162	0.132	172	67	1.006	0.614	-2.034	3.442
Audits	713	0.108	119	90	-0.036	0.988	-0.398	0.393	1,539	0.074	107	68	-0.240	0.164	-0.719	0.122

ANP is the number of political parties on the council. Determination of bandwidths is data driven. Number of observations to the left and right of the cut-off is the effective number used in estimation. Estimated treatment effect is conventional, and the p-value and 95% confidence interval are robust and bias-corrected. Standard errors are clustered at the district level. All fiscal and economic variables are measured in constant 2014 terms.
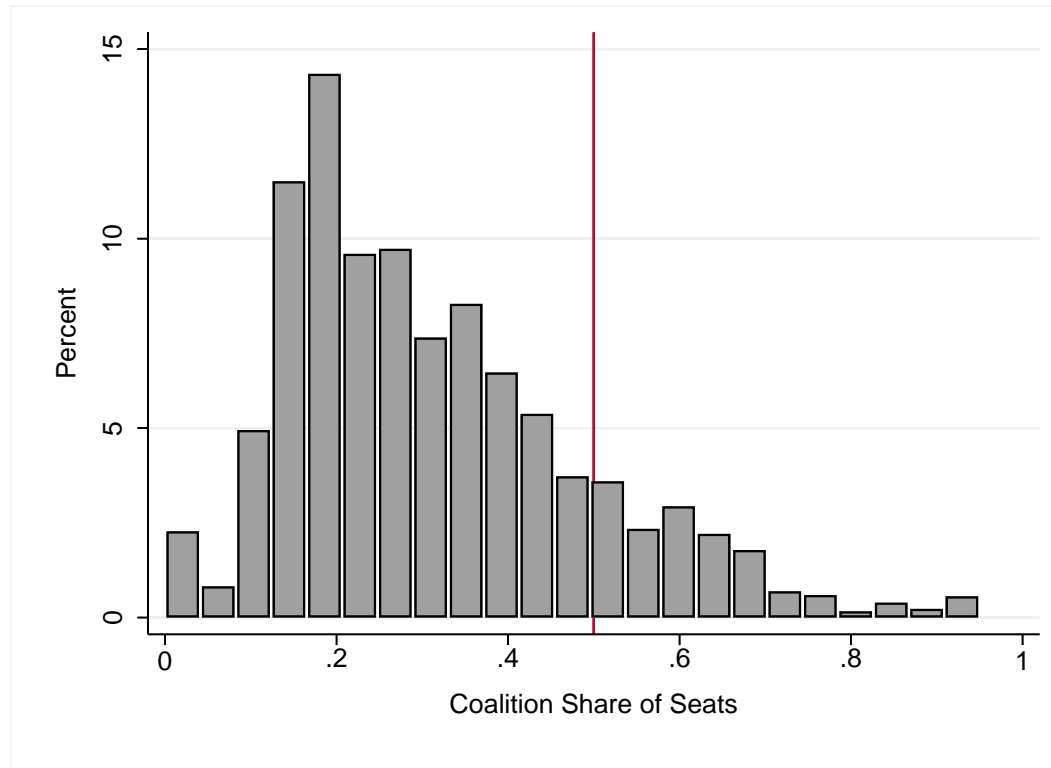
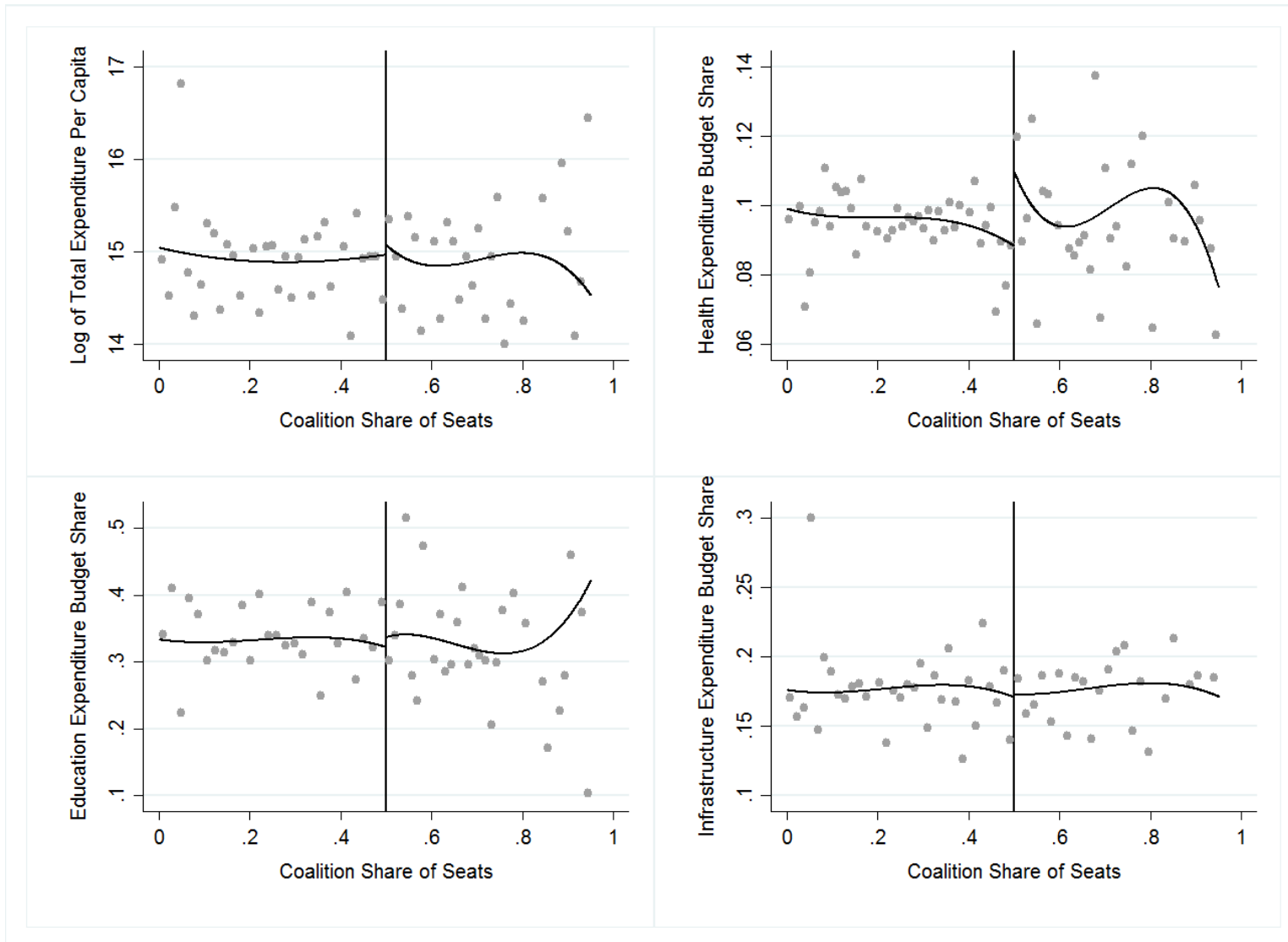**Figure 1.** Frequency distribution of coalition share of seats

Figure 2. Coalition share of seats and spending outcomes