It Takes a Village Election: Turnover and Performance in Local Bureaucracies*

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Abstract

In many countries, local governments struggle with inefficiency and inaction, often perpetuated by entrenched elites. This paper examines how leadership changes affect local bureaucratic performance. Combining personnel and citizen surveys with a regression discontinuity design in a large sample of Indonesian villages, we show that electoral turnover revitalizes local bureaucracies and improves the flow of information about citizen preferences. Bureaucrats serving new leaders interact more with citizens and are less connected to past or present village officials, resulting in a more responsive village government that better incorporates citizens' demands in policy-making. This improves local service provision, as measured in both administrative data and citizen surveys. Overall, our findings suggest that leadership turnover can mitigate elite capture and improve governance in local administrations.

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1 Introduction

In decentralized democracies, citizens can periodically replace their local leaders through competitive elections. Officials elected at this level typically manage a small bureaucracy responsible for engaging with citizens and delivering services that respond to community needs. Often, these local leaders and bureaucracies enjoy considerable discretion in carrying out such responsibilities. While effective bureaucracies are key ingredients of state effectiveness (Besley et al., 2022; Finan et al., 2017), there is limited evidence on the consequences of turnover for bureaucratic performance in local governments.

Turnover can influence local governance through competing forces. Recent research highlights the disruptions caused by bureaucratic turnover following elections (Akhtari et al., 2022; Toral, 2024). Other studies examine the tradeoffs between merit-based and discretionary appointments in bureaucracies (Colonnelli et al., 2020; Moreira and Pérez, 2024; Xu, 2018). This work suggests that turnover can cause instability, distort incentives, and undermine performance. At the same time, excessively rigid bureaucracies may develop a business-as-usual culture, struggle to attract new talent, and face organizational inertia. These dynamics may be especially pronounced in local administrations, where the pool of qualified bureaucrats is limited and leaders operate with minimal oversight, making such systems vulnerable to elite capture (Cruz et al., 2017). In these settings, local elections inducing changes in leadership could disrupt entrenched patronage networks and improve governance.

This paper explores how electoral turnover shapes bureaucratic performance. We focus on local governments in Indonesia, where village heads are elected every six years and wield significant executive authority. Indonesia has more than 75,000 rural villages where the local administration is often the first, if not the only, interface between citizens and the state. This setting is well suited to studying how leadership changes affect bureaucratic behavior and service delivery: bureaucrats operate under strong top-down and bottom-up pressures, since their tenure is highly contingent on local leadership and, as frontline providers, they are in regular contact with citizens.

Our analysis relies on data from a large survey that we conducted in 2022 with village heads, bureaucrats, and citizens in 852 villages spanning 17 provinces across Indonesia. We designed this survey to collect rich data on bureaucrats' characteristics and citizens' attitudes, and to understand how the policy priorities of village officials align with the preferences of citizens. We also use detailed administrative records on village-level public goods provision. Together, these data sources allow us to study what citizens want from their village government, what bureaucrats know about these preferences, and how they act upon them. To our knowledge, this paper is among the first to study bureaucratic performance from the dual perspective of bureaucrats and the citizens they serve.

Using this dataset, we employ a regression discontinuity design (RDD) leveraging variation from close village elections where the incumbent narrowly won or lost. Incumbency plays a central role in shaping electoral competition in this context since village elections are non-partisan by law. Across the 852 villages we surveyed, 512 conducted an election featuring an incumbent between 2015 and 2022. Incumbents won a slight majority (52%) of these elections, allowing us to identify the effects of turnovers on village- and individual-level outcomes. We also use variation from the staggered timing of village elections inherited from Indonesia's democratic transition (Martinez-Bravo et al., 2017). Supporting our

identification strategy, we find no systematic evidence of manipulation of election results by incumbents, and we show that turnovers are uncorrelated with a wide range of predetermined village characteristics.

We begin by highlighting three interrelated shifts triggered by electoral turnovers. First, personnel changes within village administrations result in higher staff morale and increased bureaucratic effort, as reflected in greater engagement with citizens. While the structure of village governments is set by law, new leaders can reshuffle personnel by reallocating staff across positions and encouraging some officials to step down to appoint others. We show that new leaders engage in more appointments, promotions, demotions, and reassignments. These changes translate into higher morale and effort after electoral transitions: bureaucrats report greater enthusiasm and interact more frequently with constituents, consistent with stronger bottom-up accountability.

Second, turnover reduces nepotistic appointments. The share of officials embedded in nepotistic networks declines after a turnover: new leaders are less likely to have relatives employed by the village, and bureaucrats are less likely to have a parent who served in the village government. This is consequential, as nepotistic appointments correlate negatively with service quality in our data. These shifts affect all bureaucrats. While new hires—who are less likely to be nepotistic appointees—show the largest gains in morale, long-standing bureaucrats drive the increase in citizen interactions. This pattern may reflect both selection and incentives: new leaders are more likely to retain high-performing bureaucrats, while longer-serving bureaucrats face stronger pressure to demonstrate their value to new village leaders.

Third, turnover improves information flows between bureaucrats and citizens, allowing village officials to better understand and act upon local preferences. After an electoral turnover, bureaucrats are more likely to identify which services citizens view as priorities or of lower quality and to report receiving complaints about those same services. Village heads, in turn, report taking action on these priorities. These effects extend to preferences expressed by citizens more socially distant from village officials, suggesting that improved information flows make policy more representative of the entire community. We illustrate these accountability dynamics by tracing the full causal chain from policy diagnosis to action for one public good: in villages with worse transport costs at baseline, a proxy for road quality, turnover increases the likelihood that bureaucrats recognize road deficiencies and report citizen complaints, and that village governments respond with road investments.

We next examine whether these shifts in bureaucratic processes and engagement improve the performance of village governments. Consistent with this, turnovers improve the quality of service provision, as measured in both administrative and survey data. Restricting attention to villages that held elections before 2021, the most recent year with administrative service-provision data, we find an increase of about 0.5 standard deviations in a standardized service index. This effect is driven by locally managed services such as garbage collection and street lighting. These gains appear only in villages where the head has no relatives employed in the village government, illustrating the role of declining bureaucratic nepotism as an important channel. They are also larger in villages whose last election occurred several years earlier (2015–2017) rather than more recently (2018–2020). Thus, the benefits of leader turnover may take time to materialize, perhaps because they must first offset the short-run disruptions associated with bureaucratic turnover, as in Akhtari et al. (2022).

Importantly, the citizens we surveyed also report improved perceptions of service access and quality, and village heads are more likely to report that their government has taken action or made investments to improve local service quality after an electoral turnover. The services for which citizens perceive the largest improvements in quality are the same as those on which the village government has reportedly taken action or made investments. Consistent with village bureaucracies becoming less beholden to local elites, the gains in perceived service quality are at least as large among citizens who are more socially distant from members of the village government. However, despite these improvements, citizens overall do not report higher satisfaction with or trust in their village government. This null effect suggests that improvements in bureaucratic performance caused by turnovers may not be immediately observable or may be misattributed to other forces, as argued in other work (Cruz and Schneider, 2017; Guiteras and Mobarak, 2015; Khan et al., 2021).

In the final section of the paper, we consider several alternative explanations for our main findings. First, we find little change in leaders' demographic and socioeconomic characteristics after electoral turnovers. Moreover, the results also hold in a subsample of elections featuring "unlucky" incumbents who stood for reelection after a local natural disaster, helping rule out negative selection into close elections (as in Marx et al., 2024). Second, we find similar results when excluding villages with term-limited incumbents, who may have weaker incentives to perform than those able to run again. Third, using a survey experiment, we rule out social desirability bias in data reported by village officials.

Our paper makes several contributions to the literature on bureaucratic performance in local government. Local elites and bureaucrats play an essential role for service delivery in developing countries—see Gulzar and Pasquale (2017) as well as Dunning et al. (2019) for a comprehensive review of the literature on political accountability. Most related to our paper is the seminal study by Akhtari et al. (2022), who find that bureaucratic turnover negatively impacts performance in the education sector in Brazilian municipalities. In contrast, we show, in a different setting with more limited state capacity, that turnover enables the emergence of more responsive and effective local bureaucracies.

We highlight several ways through which turnover may improve bureaucratic performance. Reshuffling within village governments raises morale and effort among local agents of the state. Evidence from the private sector highlights motivation (Oswald et al., 2015; Segal, 2012) and management (Bender et al., 2018; Bloom et al., 2012) as key drivers of productivity, but comparable evidence from public bureaucracies remains scarce (exceptions include Muñoz and Prem, 2024; Rasul and Rogger, 2018). In our setting, bureaucrats serving new leaders work harder to understand citizens' preferences, including those among groups more socially distant from governing elites. This bottom-up information flow empowers village governments to implement policies that better reflect community needs.

We also show that turnover can disrupt nepotistic networks controlled by local elites. Prior research finds that nepotism and family dynasties undermine local governance and public goods provision (Cruz et al., 2017; George, 2024), including in Indonesia (Aspinall and As'ad, 2016; Berenschot et al., 2021; Kenawas, 2023). Related work highlights the persistence of nepotistic practices in Colombia (Riaño, 2023) and Brazil (Cardoso et al., 2023). While challengers may not be inherently less inclined to appoint friends and relatives, our findings suggest that electoral turnover disrupts the processes that entrench

such networks. These disruptions can, in turn, foster greater meritocracy and improve governance in the short to medium run. Given the prevalence of bureaucratic nepotism in many countries, this mechanism is likely to be of relevance in other settings.

Our results further underscore that bureaucrat–citizen interactions are an important channel through which information about citizen needs reaches the state. This mechanism complements a growing body of work showing that policymakers often hold inaccurate beliefs about citizen preferences (Jablonski and Seim, 2024; Lucas et al., 2025; Pilet et al., 2024; Walgrave et al., 2023), in part because their beliefs disproportionately reflect the views of more privileged groups (Broockman and Skovron, 2018; Pereira, 2021). Consistent with this perspective, evidence shows that improving information about citizen preferences can improve policy performance (Liaqat, 2020) and that information gathered through direct interactions can improve the targeting of aid to the most disadvantaged (Bergeron et al., 2024). More broadly, our results complement evidence that accountability mechanisms shape the performance of locally embedded bureaucrats (Bhavnani and Lee, 2018).

Finally, our paper builds upon the vast literature on principal-agent problems in bureaucracies led by elected officials. Prior work highlights the benefits of meritocracy relative to patronage appointments, which have been largely phased out of bureaucracies in high-income countries (Aneja and Xu, 2024; Besley et al., 2022; Moreira and Pérez, 2024). However, meritocratic hiring and promotion systems can also constrain newly elected leaders' ability to reshape bureaucratic performance and set a new direction for their administration (Spenkuch et al., 2023). This tradeoff has fueled ongoing debate over how much discretion executive leaders should have in appointing and removing bureaucrats. Our findings show that electoral turnovers can enhance public goods provision by breaking nepotistic networks, increasing bureaucratic effort, and strengthening engagement between officials and the citizens they serve.

The rest of the paper is organized as follows. Section 2 provides relevant institutional background on village governance and village elections in Indonesia. Section 3 presents our data, empirical strategy, and identification checks. Section 4 presents our results on bureaucratic organization and bureaucrat-citizen knowledge flows. Section 5 studies impacts on local service provision. Section 6 addresses alternative explanations, and Section 7 concludes.

2 Background: Village Governance and Elections

Indonesia's democratic and decentralized system provides a rich setting to study the effects of turnover in local governments. In this section, we outline the key features of this institutional context.

Local Democracy in Indonesia. Since 1999, village heads have been elected through popular vote every six years. The legal framework for village elections is provided by the Village Law of 2014 (UU Desa 6/2014), under which village heads can serve at most three consecutive or non-consecutive terms. Like other local elections in Indonesia, village elections are staggered across districts and held simultaneously within each district, as described in Section 3.1.

Under Indonesia's Village Law, significant resources and responsibilities are devolved to village governments. These small bureaucracies manage relatively large budgets by international standards,

amounting to 3% of government spending nationally. Between 2015 and 2018, the central government transferred approximately US$14 billion to more than 75,000 villages across Indonesia, and transfers to villages increased nearly five-fold between 2013 and 2018 (World Bank, 2020). In our data, village heads report annual village budgets averaging 1.26 billion IDR (approximately USD 83,000). Budgets require agreement between the village head and the village consultative body (*Badan Perwakilan Desa* or BPD), and are subsequently submitted for approval to the district government.

The vast majority of village heads in our data are male (95%). On average, they are 48 years old and have 13.2 years of education. Most (96%) were elected as required by law, while the rest were appointed. The average village head reported having served for 5.2 years.

Composition of Village Governments. Village heads appoint members of the village government (*aparatur desa*), which includes four key positions: a village secretary and three heads of affairs responsible for general matters, finances, and planning (see Appendix Figure B.1 for an illustration). These officials are selected by the village head among village residents after consultation with the subdistrict head. By law, they can only leave their positions under specific circumstances, such as death, resignation, retirement, or criminal conviction. In our sample, village officials, on average, are 38.5 years old, have 13.6 years of education, and have served in the village bureaucracy for 5.4 years. Most (76%) report having permanent tenure. Family ties play a notable role in appointments: 22% have a parent who served in the village government, and 5% have a parent who was a village head.[1]

Political Economy of Village Governance. Local democracy is vibrant across Indonesia. Aspinall and Rohman (2017) and Berenschot et al. (2021) provide extensive qualitative evidence and case studies highlighting the intensity of electoral competition in village elections. These elections are highly contested, and incumbents do not enjoy a systematic advantage. Even before the Village Law era, the democratic transition in 1998 created new pathways for individuals outside traditional elite networks—in particular, aristocratic elites associated with the prior Suharto regime (Berenschot et al., 2021)—to attain leadership positions: *"The breakdown of centralised mechanisms of control has opened space for sometimes unruly political contestation in the villages . . . established elites have lost their former monopoly on village power"* (Aspinall and Rohman, 2017, p.32). Other evidence highlights that *"village politics [are] sometimes marked by intense political competition and close margins of victory in village head elections"* (World Bank, 2023, p.v).

However, despite intense electoral competition at the local level, significant challenges remain in establishing village governments that operate with full accountability and transparency. The ongoing process of elite renewal initiated in 1998 remains incomplete due to the resilience of strong patronage networks associated with well-established family dynasties. Despite substantial variation across villages, a key challenge stems from the continued practice of village heads appointing friends and relatives in the village government, reflecting broader patterns of elite capture in formal deliberative institutions. Consistent with the figures discussed above, a recent qualitative study conducted across 18 Indonesian

[1]Beyond the village secretariat, the village governance structure also includes the chairperson of village representative bodies (BPD) and BPD members, as well as local leaders of hamlets or neighborhoods (*dusun*). Our analysis focuses on the main officials in the village government: village secretaries and heads of affairs for general, financial, and planning matters.

villages found widespread evidence of nepotism in village bureaucracies. These qualitative accounts suggest that nepotism may be central to understanding bureaucratic practice and performance:

> "As a result of considerable, albeit narrowing, discretionary powers of the village head, we found that the village bureaucracy is often made up of friends and, particularly, family members of the village head. In 8 of our 18 villages at least some ... village officials were related to the village head. Not surprisingly, the villages where officials were family members of the village head are also the villages with more unresponsive and factionalized village governments" (World Bank, 2023, p.17).

3 Empirical Framework

This section describes the survey and administrative data we use, develops our empirical strategy, and validates the key assumptions underlying the regression discontinuity design.

3.1 Data

We describe here the numerous sources of primary and secondary data on village governance, elections, and bureaucracies that underpin our empirical design.

Survey of Village Officials and Citizens. We conducted a large-scale survey of village officials and citizens in Indonesia between March and August 2022. The survey covered 852 villages across 23 districts in 17 provinces spanning the archipelago. Our sampling strategy focused on districts with good internet coverage and aimed to achieve broad national representativeness within this set. The primary targets were active village officials, including elected village heads, non-elected members of the village government, hamlet heads, and BPD chairpersons and representatives. In addition, we surveyed 8 to 12 adult citizens in each village. The survey aimed to inform the design of a future bureaucrat training intervention, to deepen our understanding of village governance, and to provide new insights into perceptions of village governance and development among officials and citizens. We collected perceptions of service access and quality, as well as priorities for future development spending, separately from citizens and officials. This allows us to study information flows from the former to the latter and to understand how knowledge of citizens' preferences informs policy decisions.

Due to Covid-19 restrictions, all surveys were conducted by phone. We sampled citizens using a snowball procedure in which initial respondents (typically members of the village government) were asked to provide three contacts whose names began with a randomly drawn letter of the alphabet (see Appendix Figure B.2). This process continued until the target sample size was reached in each village. As a result, some citizens in our sample may be more connected to the village government than the average citizen. However, the extent of these connections does not vary discontinuously at the RD threshold and therefore does not represent a threat to our empirical strategy (see Appendix Table A.3).

Our survey covered a total of 744 village heads, 1,793 village bureaucrats, and 14,378 citizens. Restricting these figures to the 512 villages in which an incumbent candidate competed in the last election (see below), our final sample includes 444 village heads, 1,067 village bureaucrats, and 8,880 citizens. Appendix B provides additional details on our survey design.

Electoral Data. As part of our survey, we collected official voting tallies for all candidates running in the last village head election. We obtained complete electoral data for 799 among the 852 villages in our sample (94%). Under the Village Law, village heads are elected every six years via first-past-the-post voting, and local elections are staggered across districts, with all village elections occurring in the same year within a district. Thus, elections were held in different years across villages in our sample: less than 1% were held before 2016, 11% in 2016, 13% in 2017, 16% in 2018, 28% in 2019, 2% in 2020, 27% in 2021, and 1% in 2022. On average, 3.6 candidates competed in these elections with a turnout of 82% (calculated as votes cast divided by the number of registered voters in each village).[2] We report various checks on the electoral data in Section 3.3 (see also Appendix Table A.1 and Appendix Figures A.1–A.2).

The voting data indicates which candidate was the incumbent at the time of the last election. Overall, 512 village elections featured an incumbent candidate. These villages constitute the main sample for our empirical analysis.[3] Women comprised only 5% of incumbent candidates, and 6% of all candidates. Figure 1 (panel a) plots the density of the difference between the vote share received by the highest-ranking challenger candidate and the incumbent's vote share. We use this difference as the running variable in our regression discontinuity (RD) design, described in Section 3.2.

Administrative Data. To measure bureaucratic performance, in addition to survey outcomes, we use administrative data from the 2014 and 2021 rounds of *Podes*, a triennial village census, which we match to our survey sample. We use the 2014 data as the baseline and the 2021 data as the endline. When studying endline outcomes, we restrict the sample to villages that held their last election before 2021 and use the remaining villages for placebo checks. We also use predetermined geographic and socioeconomic characteristics from *Podes* to conduct balance and other validity checks.

3.2 Regression Discontinuity Design

Our goal is to identify how bureaucratic processes and service provision are influenced by turnover in the most recent village election. Before introducing our identification strategy, Table 1 presents correlations between some of these outcomes and the presence or the victory of an incumbent candidate in the last election. We first examine two measures of bureaucratic performance: the quality of public goods in the 2021 *Podes* data, and the growth in service quality between the 2014 and 2021 *Podes* waves. While there is no significant correlation between these outcomes and whether an incumbent runs (columns 1 and 3), incumbent wins are followed by a decline in public goods provision (columns 2 and 4). As one might expect, incumbent reelections are also associated with lower bureaucratic turnover (column 6). Furthermore, village bureaucracies led by a reelected incumbent have more nepotistic appointees in their ranks: bureaucrats in these villages are more likely to be connected to the village head (column 8).

While this suggests that prolonged leadership tenure may limit bureaucratic turnover, fuel nepotism, and hinder performance, the estimates in Table 1 should not be interpreted as causal. The probability

[2] A small fraction (4%) of elections in our sample featured turnout greater than 100%. We later use this as a measure of data quality and show that this is uncorrelated with the occurrence of an electoral turnover.

[3] In Table 1, discussed in Section 3.2, we use the full sample of villages to document how villages where an incumbent competed in the last election differ from other villages. In this table and the RD analysis, we exclude 8 villages where the incumbent and the highest-ranked challenger candidate are tied in the electoral data.

of an incumbent winning the last election likely correlates with observable and unobservable candidate and village characteristics, and incumbents may be particularly likely to lose elections in villages where bureaucratic performance has been poor. To address these concerns, we turn to a regression discontinuity (RD) design comparing villages where the incumbent barely won or lost the most recent election. The main identifying assumption required for this design to be valid is that potential outcomes be smooth across the RD cutoff. In particular, there should be no ex ante differences between villages where elections are won by incumbents and villages where elections are won by challengers. We probe the validity of these assumptions in Section 3.3.

We estimate the effects of an electoral defeat of the incumbent with the following RD equation:

$$y_{ijt} = \alpha + \beta_1 \cdot \text{margin}_{jt} + \beta_2 \cdot \text{margin}_{jt} \times \mathbb{1}(\text{margin}_{jt} > 0) + \gamma \cdot \mathbb{1}(\text{margin}_{jt} > 0) + \delta_t + \varepsilon_{ijt}, \quad (1)$$

where y_{ijt} is an outcome for respondent i (village head, bureaucrat, or citizen) residing in village j that held its election in year t. $margin_{jt}$, the running variable, is the victory margin of the highest-ranked challenger candidate in the election conducted in village j at time t, and $\mathbb{1}(margin_{jt} > 0)$ equals one if the challenger won. γ is the coefficient of interest. We include election-year fixed effects, δ_t, to account for the fact that villages hold their elections in different years.[4] When examining administrative outcomes, we estimate equation (1) at the level of village j; in this case, the regression has exactly N=512 observations, the number of villages in which an incumbent competed in the most recent village election.

We estimate equation (1) using the non-parametric method of Calonico et al. (2014) with a triangular kernel, and we cluster standard errors by village. Using this approach, in all our main tables we report the conventional RD point estimate γ, the conventional standard error in parentheses, and the robust bias-corrected standard error from Calonico et al. (2014) in brackets. Statistical significance levels are based on the robust bias-corrected standard errors, and we also report the p-value associated with the robust confidence interval for γ. When conducting heterogeneity analyses across different subsamples, we use the estimator from Calonico et al. (2025) to test for significant differences between subsamples. Finally, we present RD plots separately for our main outcomes of interest.

Our analysis examines the impact of electoral turnover on outcomes across several domains: bureaucratic organization, nepotism, bureaucrat–citizen alignment, and service provision. To mitigate concerns about multiple hypothesis testing, we group related outcomes into standardized indices where possible and focus on patterns across outcome families rather than individual estimates. We do not apply formal corrections across domains, as these correspond to distinct theoretical channels rather than repeated tests of the same hypothesis.

3.3 Identification Checks

We describe here key tests that support a causal interpretation of the RD estimate, γ, in equation (1).

[4] In all specifications where we look at bureaucrat outcomes, we also control for a treatment indicator associated with a survey experiment embedded in our survey. This experiment provided a messaging intervention designed to estimate the magnitude of social desirability bias. The randomization was conducted at the village level and treatment assignment in this experiment is uncorrelated with the turnover effect in equation (1): the RD point estimate is τ=-0.093 (robust SE 0.131, p=0.372).

Density Test. Incumbent village heads may be able to systematically manipulate local election results, tilting electoral outcomes in their favor on average. If this occurred, we would observe a discontinuous drop in the density of our running variable (the victory margin of the best-ranked challenger) across the threshold (McCrary, 2008). We address this concern in Figure 1 (panel b), which implements the density test from Cattaneo et al. (2018). There is no evidence of manipulation or sorting at the threshold: the p-value from this test is 0.856.

Balance Checks. We then report a range of balance tests to probe the validity of our RD strategy. First, Appendix Table A.1 shows balance along various predetermined village characteristics from the survey and electoral data: the number of neighborhoods or hamlets (column 1), log number of households in the village (column 2), separate dummies for the village being located in each of Indonesia's major islands (columns 3-7), the number of registered voters (column 8), and the number of candidates competing in the most recent election (column 9). Only one of these variables (the likelihood that the village is located in NTB-Bali) is significantly correlated with turnover, at the 10% level.

Second, Appendix Table A.2 shows balance along ten predetermined village characteristics from the *Podes* data: latitude, longitude, altitude, coastal location, forest location, a dummy indicating that agriculture is the main economic activity in the village, and four dummies indicating the dominant agricultural activity (rice, corn, rubber, or palm oil). Only one out of these ten characteristics (corn cultivation) is significantly correlated with turnover, as one would expect by chance. Appendix Table A.3 shows balance on whether a citizen's contact information is provided by a village official or BPD member (columns 1–2) and the degree of connection between citizens and these officials (column 3). These balance checks support our analysis of the differential effects of turnovers with respect to social distance, discussed in Section 5. Finally, Appendix Table A.4 shows balance on citizens' demographic characteristics (gender, age, disability, education, employment status, and monthly income).

Electoral Data Checks. We further report several checks on the validity of the electoral data. Appendix Figure A.1 plots the raw turnout data and turnout winsorized at 100% against the vote share of the incumbent (panels a and b) and against our running variable, the margin of victory of the highest-ranked challenger (panel c and d).[5] There is no systematic evidence of turnout manipulation in favor of incumbents, as the few instances of excessive turnout are located on both sides of the RD threshold. We confirm this in Appendix Table A.1, where we estimate equation (1) using voter turnout and a dummy for turnout being greater than 100% as dependent variables. There is no evidence that turnovers are associated with differential turnout at the threshold (column 10), nor that they are associated with suspiciously high or low turnout (column 11). Turnovers also have a null effect on an alternative measure of electoral competition, a Herfindahl index of vote shares (column 12).

Finally, we implement a test inspired by Benford's law to detect electoral manipulation in villages won by the incumbent (see Mebane, 2006, 2011). In Appendix Figure A.2, we plot the distribution of the first, second, third, and last digits of candidate vote tallies separately for villages won and villages lost by the incumbent. Using a Kolmogorov-Smirnov test, we cannot reject the null of equal distributions

[5]Recall that 4% of villages in our sample, i.e. 21 out of 512 villages, report turnout over 100%.

across the two types of villages for any of the four digit distributions—the p-values from these tests are reported at the bottom of each panel. Nonetheless, panels (c) and (d) of Appendix Figure A.2 show significant heaping of candidate vote tallies at zero, plausibly as a result of rounding. Thus, in Appendix Table A.1, we also show that the number of candidate vote tallies with a trailing zero is not significantly associated with turnovers (column 13). Overall, we find no evidence of manipulation of election results.

4 Impacts on Bureaucratic Organization and Information Flows

We now present our estimates of the effects of turnover in village elections. In this section, we highlight three main shifts. First, turnover reshapes village bureaucracies, improves staff morale, and increases effort exerted by bureaucrats, as reflected in more frequent interactions with citizens (Section 4.1). Second, turnover reduces the presence of bureaucrats with nepotistic ties (Section 4.2). Third, turnover improves information flows between officials and citizens. This improves the bureaucracy's understanding of citizens' preferences and leads to stronger alignment regarding policy priorities (Section 4.3).

4.1 Organizational Changes

Leader Turnover. Our main specification examines the impact of an incumbent's electoral defeat in the most recent election on village-level and individual-level outcomes. Using our survey data, we first verify that these electoral results induce a leadership change, as expected. Specifically, we show that an incumbent's defeat increases the likelihood that the village head in our survey is someone different from the incumbent candidate who competed in the most recent election. The RD point estimate is 83.5 p.p., significant at the 1% level (see Table 2, column 1, and Figure 2, panel a). We also estimate the effect of a turnover on the village head's tenure. The RD point estimate is roughly (minus) five years, slightly less than the *de jure* term of six years (see Table 2, column 2, and Figure 2, panel b).

Thus, electoral turnovers translate into leader turnovers at the village level, but there is imperfect compliance. While our baseline specification is a sharp RD estimation of the effect of turnovers (γ in equation 1), in the Appendix we also report fuzzy RD estimates where we use $\mathbb{1}(margin_{jt} > 0)$ as an instrument for village head turnover to account for this imperfect compliance. In this case, the endogenous regressor is a dummy equal to 1 if the village head in our survey sample is a different individual from the incumbent who competed in the most recent election—the dependent variable in column 1 of Table 2. Thus, the sample for this fuzzy RDD estimation is restricted to the N=443 villages in which an incumbent competed in the most recent election and we were able to survey the current village head.

Bureaucratic Turnover. Although the majority of village officials have tenured positions, newly elected village heads may seek to reorganize the village government by appointing new officials or by reshuffling existing staff across positions.[6] Bureaucrats appointed under previous leadership may also be more likely to step down following a leadership change. In column 3 of Table 2, we estimate the effects of turnover on the share of non-elected village bureaucrats appointed to their current position since

[6] 76% of bureaucrats report having permanent tenure, or report a planned retirement date as the scheduled end of their tenure.

the last election. This share is 33% in the control group (i.e., villages within the RD bandwidth on the left-hand side of the cutoff, in which the incumbent narrowly won). At the RD cutoff, it increases by 18 p.p., significant at the 5% level (Table 2, column 3). Panel (c) of Figure 2 provides corresponding visual evidence. Note that this effect captures higher replacement rates holding size constant, since the composition of village governments is constant and set by law, as described in Appendix Figure B.1.

Village heads can also reshuffle the village government by promoting and demoting existing staff, or by laterally moving bureaucrats from one secretariat position to another. Column 4 of Table 2 shows that leader turnovers increase the fraction of village bureaucrats who experienced a promotion, demotion, or lateral move since the last election. The mean of this variable is 3.6% in the control group, and this increases by 8.9 p.p. (significant at the 10% level) in villages that experienced an electoral turnover.[7] Thus, electoral turnovers induce more bureaucratic turnover: relative to reelected incumbents, newly elected leaders are more likely to make new appointments and to reshuffle the existing staff.

These effects may impact bureaucratic selection along observable demographic characteristics. Appendix Table A.5 examines the effect of turnovers on bureaucrats' age, education, and gender. While the officials serving in turnover villages are slightly older (by 1.1 years, column 1) and less likely to be women (by 15.2 p.p., column 3), these estimates are imprecisely estimated, and there is little evidence that newly appointed bureaucrats differ in terms of such characteristics. Thus, any changes in bureaucratic performance are unlikely to come from changes in bureaucratic selection along these dimensions.

Morale and Effort. The inauguration of a new leader and the staff changes they implement may invigorate non-elected village officials, boosting morale and injecting fresh momentum into the village bureaucracy. Table 3 examines the impact of turnovers on bureaucratic morale and effort.

We first explore self-reported measures of enthusiasm and motivation. Column 1 of Table 3 shows that turnovers improve bureaucrats' job enthusiasm, with an effect size of 0.49 standard deviations on a 5-point Likert scale (see Figure 3, panel a, for graphical evidence). Next, in column 2, we analyze a continuous measure of motivation, benchmarked against bureaucrats' initial motivation when they first joined the village government. Our survey asked: *"Imagine that your motivation was 100 when you started. What number would you say your motivation is now relative to that?"* Respondents could report values above 100, and the average response was 105.6 (100.6 in the control group) with a standard deviation of 62.6. While we estimate a sizable positive effect of turnovers on motivation (22.9 from the baseline of 100), the RD estimate falls short of conventional significance levels (see Figure 3, panel b, for graphical evidence).

This enhanced staff morale is accompanied by more frequent interactions between village officials and citizens (Table 3, columns 3–4). These interactions are consistent with greater bureaucratic effort and stronger bottom-up accountability. Bureaucrats in villages that experienced a turnover are significantly more likely to interact with citizens on a daily basis (column 3). This finding holds when using a standardized measure of interaction frequency (column 4), as illustrated in Figure 3, panels (c) and (d). These results suggest that bureaucrats under newly elected leaders are more proactive in engaging with constituents. Notably, however, these interactions appear to occur outside formal settings, as we find no

[7] Promotions are transitions from head of affairs to village secretary, while demotions are the converse (from village secretary to head of affairs). Lateral moves refer to transitions between different head of affairs positions.

evidence that turnover increases attendance at village assemblies (*Badan Permusyawaratan Desa* or BPD) after a turnover (RD estimate = 0.0003, p-value = 0.941).

Robustness Checks. Appendix Tables A.6 and A.7 consider alternative specifications for the estimates in Table 2 and Table 3, respectively. The first row of each table reports our baseline estimate. In the second row, we remove from our baseline equation (1) the controls for election-year dummies and the survey experiment treatment indicator described in footnote 4. In the third row, we include region fixed effects in addition to dummies for pairs of election years (2015-2016, 2017-2018, etc.). In the fourth row, we use a third degree polynomial in the running variable to construct the RD point estimate, instead of the local linear regression in our baseline. In the fifth, sixth, and seventh rows, we vary the RD bandwidth to half, three-fourths, and two times the MSE-optimal bandwidth from Calonico et al. (2014), respectively. In the eighth row we also report RD estimates when excluding 27 villages with at least one bureaucrat appointed under a Suharto-appointed district mayor (Martinez-Bravo et al., 2017). The ninth row reports the fuzzy RD specification described above, instrumenting for leadership changes in our survey data with the victory dummy from equation (1). Overall, these specification changes leave unchanged the main takeaways from Tables 2 and 3.

4.2 Reduced Nepotism

In many villages, entrenched nepotistic practices sustain the dominance of old village elites, undermining good governance and local democracy (Simanihuruk and Sihombing, 2019; World Bank, 2023). Appendix Table A.8 shows that nepotistic hiring practices in the village bureaucracy correlate negatively with the quality of service provision measured in administrative *Podes* data (column 1) as well as perceptions of service access and quality in our surveys conducted with citizens (columns 2–3).

In Table 4, we examine the likelihood that relatives of the village head are employed in the village government, as reported by the village heads themselves. At the RD cutoff, we find a substantial, statistically significant decline in this measure of nepotism (column 1). This effect may partly reflect the continued presence of bureaucrats who were relatives of the previous village head (the defeated incumbent) but are unrelated to the new leader, and retained their positions after a turnover. As a result, the presence of bureaucrats related to the village head would be lower in turnover villages than in villages where the incumbent won. The estimate in column 1 of Table 4 suggests that, at a minimum, new leaders do not systematically replace these prior nepotistic appointees with their own relatives.

In columns 2 and 3 of Table 4, we consider two additional measures of nepotism using data collected from bureaucrats, addressing the concern above. We look at the probability that bureaucrats had a parent who ever served as village head (column 2) or a parent who ever served in the village government (column 3). Overall, a large fraction of bureaucrats (27%) had a parent who served in the village government. We find that fewer individuals with such family connections serve under newly elected leaders: the point estimate in column 3 is -16.8 p.p., significant at the 5% level (see Figure 2, panel d, for visual evidence). This could be driven by both a lower probability of making nepotistic appointments and a higher probability of a staff shakeup, i.e., removing incumbent bureaucrats with family connections. We

explicitly consider this possibility in Section 5.

Appendix Table A.9 reports robustness checks on Table 4. The results are robust to alternative specifications (removing controls, varying the polynomial in the running variable and the size of the RD bandwidth, fuzzy RD specification) as well as excluding villages with at least one bureaucrat appointed under a Suharto-appointed district mayor. This check suggests that the decline in nepotism in turnover villages is not primarily driven by villages with elites entrenched since the Suharto era, and therefore is not simply a long-run repercussion of Indonesia's democratic transition.

Crucially, our findings on nepotism do not necessarily suggest that challengers are inherently less inclined to appoint friends and relatives to bureaucratic positions. Instead, establishing and maintaining nepotistic networks may be a gradual process, requiring time in office to systematically place favored individuals in key roles. As shown in Table 4, electoral turnovers disrupt these entrenched networks, occasionally dismantling patronage systems that took years to build. In turn, these disruptions may foster greater meritocracy and improve governance in the short to medium term, as we discuss below.

Heterogeneity by Time of Appointment. The results thus far suggest that turnovers reshape village bureaucracies, reducing nepotism, and increasing morale and effort. In Appendix Table A.10, we show that these effects stem from continuing bureaucrats (those appointed before the last election and retained by the victorious challenger) as well as new bureaucrats appointed since the last election. Panels A and B report estimates for continuing bureaucrats and new bureaucrats, respectively, and the bottom panel reports the p-value from the test of Calonico et al. (2025) that robust RD estimates are significantly different across these two subsamples.

Comparing estimates across both panels, we find that bureaucrats appointed since the last election are substantially less likely to have nepotistic ties in turnover villages (panel B, columns 1–2). While new appointees exhibit the largest gains in enthusiasm and motivation, long-serving bureaucrats retained after a turnover also exhibit higher—though not statistically significant—enthusiasm and motivation levels (columns 3–4). For the latter, turnovers significantly increase the frequency of interactions with citizens, while newly appointed bureaucrats exhibit no such differences (columns 5–6). Thus, the improvements in morale and effort are not driven solely by new hires, but also stem from enhanced performance among long-standing bureaucrats.

There are two potential, complementary sets of explanations for the findings in Appendix Table A.10. One involves improved bureaucratic selection among old-serving and new employees alike. New village heads may be more likely to retain high-performing bureaucrats, and to hire more competent and motivated new bureaucrats, relative to reelected incumbents. The other potential explanation involves enhanced incentives and performance among both old and new bureaucrats. For example, bureaucrats appointed before the election and eager to retain their position may face greater pressure to demonstrate their worth to the new leader they now serve. The large and significant coefficients in panel A, columns 5–6 of Appendix Table A.10 are consistent with both enhanced selection and incentives among long-serving bureaucrats.

4.3 Information Flows: From Diagnosis to Policy Action

Bureaucrat–Citizen Alignment. The more frequent interactions between citizens and bureaucrats may improve the village government's understanding of citizens' policy preferences. Figure 4 and Table 5 show that turnovers lead to improved alignment between bureaucrats and citizens regarding local priorities. Our survey separately asked bureaucrats and citizens which services they considered top priorities for future development spending in the village,[8] as well as their perceptions of the quality of ten key local services: garbage collection, water access, electricity provision, roads, cell phone coverage, healthcare, kindergartens, primary schools, disability services, and local safety.

We first examine whether bureaucrats and citizens agree on investment priorities—specifically, whether a bureaucrat names at least one public service as a priority that village citizens also identify as a top-three priority (column 1). The control group mean for this measure is high (0.75), increasing by 10.5 percentage points at the RD cutoff, though this effect is not statistically significant. Next, we consider whether bureaucrats correctly identify as a priority one of the top-three services that citizens perceive to be of the lowest quality (column 2). Here, we find robust evidence of increased alignment, suggesting that bureaucrats in turnover villages do a better job recognizing citizens' most pressing needs in terms of service provision (see also Figure 4, panels a and b).

Moreover, bureaucrats in turnover villages are better at identifying services that are priorities for citizens, as evidenced by their increased awareness of constituent complaints in the community. In columns 3 and 4 of Table 5 and panels (c) and (d) of Figure 4, we examine whether bureaucrats report receiving complaints about services that citizens rank as top-three priorities or bottom-three quality services. These measures reflect bureaucrats' ability to accurately recognize future spending priorities, based on citizens' grievances. We find strong evidence that bureaucrats in turnover villages were more likely to receive complaints about services identified as priorities by citizens, with RD estimates of 16.2 p.p. and 17.4 p.p. (significant at the 1% and 5% levels, respectively).

Bureaucrats Acting Upon Citizens' Preferences. In the final two columns of Table 5, we examine whether village governments act on the information gathered from citizens. The dependent variable indicates whether the public services that village heads report taking action on correspond to either a top-three priority identified by citizens (column 5) or a bottom-three quality service according to most citizens (column 6). Electoral turnovers increase both measures by 13 p.p. (significant at the 1% level) and 21 p.p. (significant at the 5% level), respectively. Together with previous findings, these estimates suggest that increased interactions with citizens after turnovers allow village bureaucracies to better understand what citizens want, and this information is conveyed to village heads, allowing them to take policy actions congruent with citizens' preferences (see also Figure 4, panels e and f).

Appendix Table A.11 reports robustness checks on bureaucrat–citizen alignment outcomes: removing controls, adding region fixed effects, including a third-degree polynomial in the running variable, varying the RD bandwidth, estimating the fuzzy RD, and removing villages with Suharto-era elites.

[8] Village officials were asked: "For the village funds that are not earmarked for direct cash assistance, in your opinion, what should be the top three services prioritized for improvement?"

Bureaucrat-Citizen Information Flows: Application to Roads. Bureaucrats may invest in the services prioritized by citizens for at least two different, complementary reasons: because they have collected accurate information about what citizens want, and because they have gained better knowledge of service quality through direct observation. To probe these channels, we focus on one dimension of service quality—the quality of roads in the village—and examine the full causal chain going from the policy diagnosis to the relevant policy action. We find that both channels likely matter: in villages with high road quality, turnovers increase accurate perceptions of road quality among bureaucrats, while in villages with poor road quality, turnovers increase the likelihood that bureaucrats have heard citizens' complaints about roads, and that the village government has taken relevant action on roads.

To perform this test, we measure baseline road quality using the 2014 *Podes* survey. Road quality is proxied by an index of transportation costs (per km) to the subdistrict office and the regent or mayor's office, where lower transportation costs indicate better road quality.[9] Table 6 splits villages into those with higher road quality (transportation costs below the sample median; panel A) and lower road quality (transportation costs above the sample median; panel B). This baseline measure of road quality is balanced at the RD threshold (Table 6, column 1). This balance is critical, as transportation costs may reflect a range of factors beyond village-level investment, including geography, remoteness, and higher-tier infrastructure. The RD design therefore isolates the effect of turnovers on bureaucratic decisions shaping road provision within the village's purview, net of these confounding sources of variation.

In villages with high road quality, bureaucrats serving new leaders are more likely to accurately report that roads are in good condition, while in villages with poor road quality, they are more likely to accurately report worse quality (column 2). Thus, in both types of villages, turnovers improve bureaucratic knowledge of the state of local roads. In villages with high baseline road quality, turnovers have no effect on citizen complaints received about roads, while in villages with low road quality, bureaucrats have received more complaints about roads (column 3). Finally, turnovers increase the likelihood that village leaders have taken action on roads, but this is only the case in villages with poor road quality (column 4, panel B). Thus, turnovers increase the likelihood that bureaucrats have accurate perceptions of road quality, that they communicate effectively with citizens on the need to improve roads, and that this translates into relevant policy actions on roads by the village government—suggesting that overall, the flow of information about citizens' preferences has improved in those villages, with meaningful consequences for local policy.

Other Bureaucratic Knowledge. The changes in bureaucratic organization and information flows that we document may be related to other knowledge acquisition, broadly defined, within the bureaucracy. Appendix Table A.12 examines this possibility by assessing three measures: whether bureaucrats received training in the past 12 months (column 1), their ability to correctly answer a policy-relevant question about a recent regulation (an objective knowledge measure, column 2), and a standardized index of self-reported knowledge across five domains: development management and accountability, financial management, village regulations, drafting development plans, and the Village Law (column 3). We find

[9] We use transportation costs in IDR per kilometer as a proxy for baseline road quality rather than road type (asphalt roads vs. other roads), given the lack of meaningful variation in the latter measure.

no significant effects of electoral turnovers on these outcomes. Furthermore, bureaucrats serving new leaders may also be more skilled at securing government transfers. In Appendix Table A.13, we explore how turnovers impact transfers received by a subset of villages (423 out of 512) for which we could obtain administrative data on Village Funds Allocations (known by the Indonesian acronym ADD), a type of transfer allocated by district governments to fund staff salaries, benefits, and village operations. We find no evidence that turnovers affect the amount of ADD funds allocated or utilized (columns 1–2) or the share of funds spent (columns 3–4). These null effects mirror those we find on the budget amounts reported by village heads in our own survey data (column 5). Together, these results suggest that turnovers reshape bureaucratic organization without materially altering broader institutional know-how.

5 Impacts on Service Provision

Our findings so far indicate that bureaucrats serving new village heads report higher enthusiasm, exert greater effort, and engage more frequently with citizens. This increased engagement helps them better understand community priorities, allowing village governments to respond more effectively to citizens' demands. We hypothesize that these forces ultimately generate improvements in local service quality.

In this section, we first document effects of turnover on service provision in administrative data, as well as perceived service access and quality and actions taken by village governments to improve service provision, as measured in survey data. We examine the dynamic timing of these effects and we probe the robustness of these results. We then show that these gains are especially pronounced for socially distant citizens and in villages where turnover weakens nepotistic networks. Finally, we document their limited pass-through to citizen satisfaction and trust in the village government.

5.1 Service Provision in Administrative Data

Main Results. Consistent with our findings on bureaucrat-level outcomes, turnover in village elections improves public service provision, as reflected in administrative *Podes* data. For this analysis, we restrict the sample to villages that held their last election before 2021 (378 out of 512 villages), the year of the most recent *Podes* survey. We construct a standardized index of service quality, encompassing all public goods managed by village governments: drinking water, sewage, garbage collection, street lighting, paved roads, public transit, kindergartens, primary schools, village maternity clinics (*polindes*), and community health centers (*puskesmas*). In Table 7, column 1, we find a substantial 0.50 standard deviation (s.d.) increase in this index of service provision at the RD cutoff (see also Figure 5, panel a). This effect is primarily driven by garbage collection, street lighting, and to a lesser extent, drinking water and public transit—Appendix Table A.14 reports RD estimates for each component.

The magnitude of these effects is large relative to the existing literature.[10] Two features of our setting may help explain this large magnitude. First, village governments in Indonesia exercise considerable

[10]For comparison, Akhtari et al. (2022) find effects of bureaucratic turnover on educational outcomes in Brazil below 0.10 s.d., albeit in a setting with substantially higher baseline state capacity.

discretion over locally managed services, so leadership changes can have direct and immediate consequences for service delivery. Second, our index captures services such as garbage collection and street lighting, where substantial improvements may be achieved through relatively low-cost organizational changes rather than major capital investments. As a balance check, the last column of Table 7 presents the same service provision index from the 2014 *Podes* wave, excluding garbage collection, which was not recorded that year (see also Figure 5, panel b). Prior to the most recent village head turnover, service provision was 0.06 s.d. lower (non-significant) in turnover villages.

Heterogeneity by Nepotism. Our findings align with qualitative evidence emphasizing the crucial role new village heads play in shaping local development outcomes.[11] In particular, the staff replacements and reduction in nepotism we observe may contribute to the emergence of more responsive local bureaucracies. Officials in turnover villages exert greater effort to engage with citizens and understand their priorities, fostering investments aligned with citizen preferences and ultimately improving service provision—an effect we observe in both administrative data and citizen surveys.

In Appendix Table A.15, we provide evidence that the positive effects of turnover are driven by villages where the electoral outcome disrupts nepotistic networks. An important caveat is that the measures of nepotism used to partition the sample are potentially endogenous. In columns 1–2, the split is based on whether the current village head employs a relative, which is itself directly affected by turnover (Table 4). Bearing this in mind, the improvements in service provision appear to only occur in villages where the village head does not employ a relative. In these villages, turnovers lead to a 0.77 s.d. increase in service provision (significant at the 5% level, column 1), while no improvement is observed when at least one bureaucrat is related to the village head (-0.069 s.d., not significant, column 2).

In columns 3–4 of Appendix Table A.15, we divide villages into those with no bureaucrats appointed before the last election who report having a parent who previously served as village official, and those with at least one such bureaucrat, respectively. This measure of nepotism is potentially less endogenous to the treatment (Appendix Table A.16 shows that it is not directly affected by electoral turnovers) and captures the persistence of family-based ties in village governance. The effect of turnovers on service provision is larger in villages without these long-serving nepotistic appointees (0.60 s.d., significant at the 5% level), though the difference between groups is not statistically significant. Together, these patterns suggest that dismantling nepotistic networks is associated with larger improvements in service provision, though caution is warranted given the potential endogeneity of the sample splits.

Dynamic Effects. In Appendix Table A.17, we show that improvements in service provision measured in *Podes* take time to materialize after electoral turnovers. We group villages by the timing of their most recent election: 2015–17 (122 villages), 2018–20 (256 villages), and 2021–22 (134 villages). The staggered timing stems from Indonesia's democratic transition, as district mayors appointed in the final years of the Suharto era were allowed to complete their terms (Martinez-Bravo et al., 2017). The 2021–22 group serves as a placebo, as elections held after the 2021 *Podes* survey should not affect service outcomes measured in 2021.

[11] A recent qualitative study of Indonesian villages (see Section 2) highlights that *"a responsive and reform-oriented village head can exercise considerable agency in ensuring a well-run village even without high levels of citizen demand"* (World Bank, 2023, p.11).

We find that improvements in service provision grow over time. Effects are nearly twice as large in villages that held elections in 2015–17 (column 2) compared to those with more recent elections (column 3), while, as expected, there is no evidence of improvements in villages where elections occurred after the *Podes* wave (column 4). This pattern is consistent with the idea that the benefits of turnover take time to materialize, potentially reflecting short-run adjustment costs (Akhtari et al., 2022). While these comparisons are based on variation in election timing that is plausibly exogenous, they should be interpreted as suggestive given the possibility of residual variation across cohorts (e.g., differences in baseline institutional quality or geographic composition).

5.2 Perceived Service Provision and Village Government Actions in Survey Data

Main Results. Our survey data provides further evidence that perceptions of service access and quality improved, and village governments were more likely to take action on public services when the village experienced an electoral turnover. We examine citizens' views and reported village investments on the public goods that most closely correspond to those enumerated in *Podes*, namely water access, garbage collection, roads, electricity (for street lighting), kindergartens, primary schools, and local healthcare delivery. Here, we look at service provision along both the extensive margin (whether the service is accessible in the village) and the intensive margin (reported service quality, based on citizens' ratings of service provision for each type of service on a 5-point Likert scale). Table 7, columns 2–3 report the results for citizens' perceptions, while column 4 examines actions taken by the village government to improve service provision, as reported in our surveys of village heads. Figure 6 presents RD plots.

Overall, citizens in turnover villages report an increase in both service access and quality: averaging across all services, the reported access rate is 78% in control villages, and this increases by 8 p.p. at the RD threshold (column 2), while the perceived quality of these services increases by 0.21 s.d. (column 3). Both estimates are significant at the 5% level.[12] Consistent with these effects, village governments in turnover villages were more likely to have taken action or made investments in the past twelve months to improve local service quality. Across services, 76% of villages have taken such action to improve service quality, and this increases by 16 p.p. at the threshold (column 4).

Appendix Table A.18 reports effects on the individual components of the two indices of service access and service quality examined in Table 7, columns 2–3. In turnover villages, the most improved service according to citizens is garbage collection, in line with the findings in Appendix Table A.14. Citizens also report improvements in the condition of roads, whereas in the administrative data, we observe improvements in street lighting (Appendix Table A.14, column 5), paved roads (column 6), and public transit (column 7), but only the first of these estimates is statistically significant. Appendix Table A.19 shows that the effect on village governments taking action to improve services (Table 7, column 4) is also primarily driven by garbage collection (column 3).

[12] One concern could be that the sample of citizens, drawn from a snowball process with village officials, may be more favorably inclined towards the government. However, this bias would naturally arise on both sides of the RD cutoff. In Appendix Table A.3, we report balance checks on whether a citizen's contact information is provided by a village official or BPD member. Neither of these variables is statistically significant.

Heterogeneity by Social Proximity to the Bureaucracy. Our sampling strategy allows us to explore the effects of turnovers on perceived service provision separately for citizens with varying degrees of social proximity to village officials. To construct our sample of citizens, we employed a snowball procedure in which respondents were asked to provide three contact persons whose names began with a randomly drawn letter of the alphabet. This procedure started with the village heads and BPD chairpersons and continued with citizen respondents until we reached the target sample size (8 to 12 citizens) in each village. As a result, our sample captures a spectrum of social distances from the village government: 30% of respondents are directly connected to a member of the administration, 25% have a second-degree connection (they were referred by a directly connected individual), and 45% have a third- or higher-degree connection. Importantly, social proximity to the village government does not change discontinuously at the RD cutoff, and the demographic characteristics of citizens are also balanced at the threshold.[13]

In Appendix Table A.20, we show that individuals more socially distant from the government are relatively younger and more likely to be women, less likely to be working and to have a tertiary education, and earn lower incomes on average. As expected, these individuals also interact less frequently with bureaucrats: in the control group, the standardized index of interaction frequency is 0.21 s.d. for citizens directly connected to the government, compared to -0.24 s.d. for those with three or more degrees of separation (see Appendix Table A.23).

Appendix Table A.21 shows that electoral turnovers lead to greater alignment between bureaucrats and citizens, including those with fewer social connections to village officials. In Section 4.3, we showed that turnovers improve bureaucrats' understanding of citizens' priorities. When we disaggregate these effects by social distance, we find that the gains in alignment are, if anything, more pronounced for citizens with weaker ties to village officials. In turnover villages, bureaucrats are more likely to identify investment priorities that align with socially distant citizens (columns 1–2) and to receive complaints about services prioritized by these citizens (columns 3–4). Additionally, village governments are more likely to act on the priorities of these most socially distant and marginalized constituents (columns 5–6).

Finally, socially distant citizens drive the improvements in perceptions of service access and quality following a turnover (columns 7–8). While turnovers improve perceptions across all subsamples, the largest and most precisely estimated gains (at the 1% level) occur among those least connected to the administration. Consistent with village bureaucracies becoming less beholden to local elites, these results suggest greater responsiveness to the preferences of more marginalized constituents.

Robustness Checks. Appendix Table A.22 reports robustness checks for the results in Table 7, including removing election-year dummies, adding region fixed effects, using a third-degree polynomial in the running variable, varying the RD bandwidth, and excluding villages with Suharto-era elites. In column 1, although the smaller administrative data sample (N=378 villages that held their last election before 2021) limits precision in some specifications, the estimated effects on service provision in the administrative data remain consistently positive and large, ranging from 0.28 to 0.68 s.d. increases in the overall index of public services. We also report the fuzzy RD estimate, which yields a similar result with slightly larger magnitude. Columns 2–4 present analogous robustness checks for citizens' percep-

[13]See Appendix Table A.3 and Appendix Table A.4, discussed in Section 3.3.

tions of service access and quality as well as village governments taking action to improve services. We again find consistent evidence that turnovers improve perceived service access, quality, and village-level investments on service provision.

5.3 Downstream Effects on Attitudes

Despite the improvements in service provision observed in both administrative and survey data, turnovers do not improve citizen satisfaction or trust in the village government. Appendix Table A.23 shows that this finding holds in the full sample (panel A) and across subsamples of citizens who are more or less socially distant from the village government (panels B through D). Consistent with bureaucrats' reports (see Table 3), citizens overall indicate more frequent interactions with bureaucrats after a turnover (column 1, though not statistically significant). However, they do not express greater satisfaction with the village government (column 2) or higher trust in it (column 3). Panels B through D show that less socially distant individuals are driving the increase in bureaucrat–citizen interactions after a turnover, while changes in trust towards the village government do not vary substantially between citizens at varying degrees of social distance.

Overall, these null effects on attitudes—despite significant gains in service provision—suggest that citizens may not immediately recognize or attribute these improvements to the village government, limiting short-run gains in trust or satisfaction. Improvements in service provision may be misattributed to other actors, such as higher levels of government or foreign donors (Cruz and Schneider, 2017; Guiteras and Mobarak, 2015), or trust may adjust only slowly to new information (Khan et al., 2021). In addition, our broad measures of trust and satisfaction may not fully capture welfare gains from improvements in specific services. While administrative data and citizen surveys point to meaningful gains in service access and quality (Table 7, columns 2–3), attitudes toward the village government may reflect additional considerations, including fairness, expectations, and experiences beyond local service delivery. This disconnect is also consistent with rising expectations following turnover, which may offset the effects of improved service provision on reported satisfaction.

6 Alternative Explanations

Our results show that turnover in village elections shakes up village bureaucracies, fosters increased engagement and policy alignment between bureaucrats and citizens, and improves service provision. In this section, we discuss potential explanations for the shifts in bureaucratic organization and improvements in service quality that we observe. We focus on four possible explanations: positive selection of new leaders, lame-duck village heads driving down bureaucratic effort, patronage appointments by newly elected leaders, and social desirability bias in survey data collected from bureaucrats.

Leader Selection. Village governance may improve due to a selection channel: the challengers who win elections might be more able leaders than reelected incumbents, on average. We show here that such selection is unlikely to explain our findings.

In Appendix Table A.24, we examine how village head characteristics vary at the RD threshold. New leaders are no less likely than reelected incumbents to be connected to prior village leadership, including having a parent who previously served as village head (column 1) or as a member of the village government (column 2). Thus, electoral turnovers do not improve governance by selecting less connected leaders. These findings are consistent with qualitative accounts that village elections are often contested between rival families or clans, implying that challengers are not necessarily less likely to come from local elites (Aspinall and Rohman, 2017). At the same time, elected challengers may still outperform incumbents despite hailing from similar elite backgrounds, as a result of the "founder effect" described in George (2024).

We also find little evidence that elected challengers differ from reelected incumbents along observable characteristics: their age (column 3), gender (column 4), level of education (column 5), or language ability (column 7). The point estimate for religion (column 6) is negative and significant at the 10% level, which we interpret as a chance finding. Overall, the average leader in the control group is 49.9 years old, overwhelmingly likely to be male, and has completed 13 years of schooling, and none of these characteristics differs for elected challengers at the threshold. Similarly, we find no evidence that elected challengers differ from reelected incumbents in their enthusiasm (RD estimate = 0.043, p-value = 0.861) or motivation (RD estimate = -18.217, p-value = 0.257) for the job.

While our data shows no measurable differences between elected challengers and reelected incumbents at the RD threshold, it remains possible that challengers who won the most recent election are, on average, unobservably more competent than incumbents who secured reelection. Furthermore, if the gap in unobservable quality between elected challengers and elected incumbents is larger in close village elections than in typical elections outside our sample, then the findings we obtain in our sample of close elections may not generalize to other, less competitive elections.

To probe these concerns, we implement an empirical exercise designed to distinguish elections featuring less competent incumbents from those involving "unlucky incumbents" who faced adverse external circumstances as they competed for another term. Building on Marx et al. (2024), this approach identifies incumbents who faced a close reelection battle not mainly as a result of their poor past performance, but because of unfavorable external factors that created an electoral playing field hostile to incumbents, such as a natural disaster affecting the local community. Intuitively, natural disasters provide exogenous variation that reduces an incumbent's reelection chances for reasons beyond their control. Using the administrative *Podes* data, we identify a subsample of 242 villages that experienced a natural disaster—a major landslide, flood event, ocean tide, hurricane, or drought—during an incumbent's term prior to the election observed in our data. In these elections, incumbents earned a vote share 6.5 p.p. lower on average, and they were 14 p.p. less likely to be reelected overall (Appendix Table A.25).[14]

In Appendix Table A.26, we show that turnovers in elections held after a local natural disaster lead to similar effects as those observed in the full sample. While we lack statistical power in some of these

[14] A stricter test would involve only considering natural disasters occurring the year of, or the year prior to an election. This would help rule out that an incumbent's disaster response could also shift reelection probabilities. Unfortunately, sample size concerns prevent us from implementing this more stringent restriction, and we instead consider all disasters occurring in the five years prior to the election.

regressions due to the smaller sample size, turnovers in those elections reduce nepotism (columns 1–2) and improve bureaucrats' morale and effort (columns 3–4) as well as bureaucrat-citizen alignment (columns 5–6). They also significantly increase actual and perceived service provision in the village (columns 7–9). Thus, the positive effects of turnovers do not mainly stem from negative selection of incumbents into the sample of close elections used in the RD estimation, but instead hold when we only consider elections that were closely contested due to exogenous shocks.

Lame-duck Village Heads. Under Indonesia's Village Law, village heads are allowed to serve a maximum of three consecutive or non-consecutive terms. Our empirical strategy, which consists of comparing outcomes in villages where the incumbent barely won or lost the most recent election, naturally raises questions pertaining to the role of these *de jure* term limits: lame-duck village heads serving their third and final term might face poorer incentives to perform, and this could, in turn, undermine bureaucratic effort and performance. A large literature has documented the negative effects of term limits on policy performance (e.g., Ferraz and Finan, 2011; Fouirnaies and Hall, 2021).

However, across the 512 villages in our sample, only 31 village heads (6%) are serving a third term. This small share is consistent with low rates of incumbent reelection: among 852 villages in our sample (including those where incumbents did not run), only 265 (31%) saw an incumbent victory. Term-limited incumbents therefore represent a small fraction of villages. In the bottom row of Appendix Tables A.6, A.7, A.9, A.11, and A.22, we exclude the 31 villages where the current head is serving a third term. This restriction is mechanically asymmetric: while we exclude third-term incumbents, we cannot similarly exclude close elections where a defeated incumbent would have entered a third term due to data limitations. If anything, this asymmetry should attenuate the estimated effects if term-limited incumbents were driving the results. Reassuringly, our findings are unchanged under this restriction.

Patronage Appointments. One potential explanation for the increase in bureaucrat morale and citizen engagement is the appointment of campaign activists following an electoral turnover. Individuals who campaigned for the new village head may be more likely to secure positions in the village government, and, in turn, exhibit higher enthusiasm and have greater knowledge of citizens' preferences. However, campaign networks often overlap with family and kinship ties, and we find that nepotistic hiring declines after turnovers (Table 4). In addition, and more importantly, it is unlikely that patronage appointments alone account for the improvements in public service provision observed in administrative data and citizen perceptions (Table 7).

Social Desirability. Several of the outcomes we examine are reported by the bureaucrats themselves. This is, to some extent, a strength of our empirical setting: we collected measures of morale and policy preferences directly from the bureaucrats themselves, measures which are typically unavailable in administrative data. However, this also raises concerns about social desirability bias in the bureaucrat-level outcomes, if such bias is correlated with electoral turnovers.

To address this, we also included in our instrument a survey experiment designed to quantify experimenter demand effects in the responses of village officials. This experiment provided a randomized priming treatment that made more salient the ongoing data collection effort; the message emphasized

either (i) that data collection was part of a research collaboration with the Indonesian Ministry of Home Affairs or (ii) that data collection was simultaneously ongoing with citizens residing in the same village. The randomization was conducted at the village level. Treatment assignment in this survey experiment is uncorrelated with turnover in equation (1): the RD point estimate is τ=-0.093 (robust SE: 0.131, p=0.372). Nonetheless, we control for this treatment assignment in all our specifications. We report the takeaways from this survey experiment in a companion paper, and, in general, we find limited effects of our priming intervention on a wide range of bureaucrat-level outcomes and attitudes.

7 Conclusion

This paper studies Indonesian villages as laboratories of local democracy. We use electoral turnovers—instances in which an incumbent leader failed to secure reelection in the most recent village election—as natural experiments that disrupt the status quo in local governance. These turnovers typically bring new leaders to power with a mandate to improve governance and development outcomes. Because village bureaucracies serve as the key link between citizens and frontline service delivery, they are a central instrument through which such political change may affect everyday bureaucratic performance.

We show that turnover in local elections reshapes the bureaucracy, most notably through staff reshuffling and a reduction in the presence of bureaucrats with nepotistic ties. Bureaucrats serving under new leaders report higher morale and exert greater effort: they interact more frequently with citizens, become better informed about constituents' priorities, and are more likely to take actions aligned with those priorities. Improvements in the flow of policy-relevant information between officials and citizens translate into meaningful improvements in public service provision, visible in both administrative records and citizen reports of service access and quality, as well as village-level actions taken to improve local services. These gains reach even individuals who are socially distant from the village administration, and they are especially pronounced in villages where new leaders weaken entrenched nepotistic networks.

More broadly, our findings point to the importance of local accountability in shaping how democracy functions in practice. At a time when democratic institutions face growing skepticism in many settings, our results suggest that regular elections can help improve the quality of local governance by making bureaucracies more responsive and service delivery more effective. In that sense, the ability of elections to periodically shift power—even at very local levels—may be one important channel through which democracy delivers tangible benefits to citizens.

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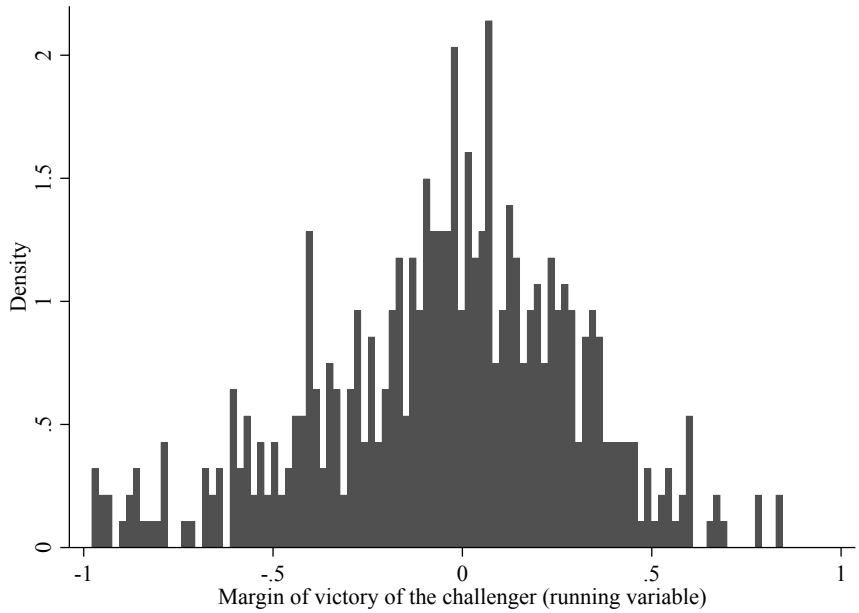
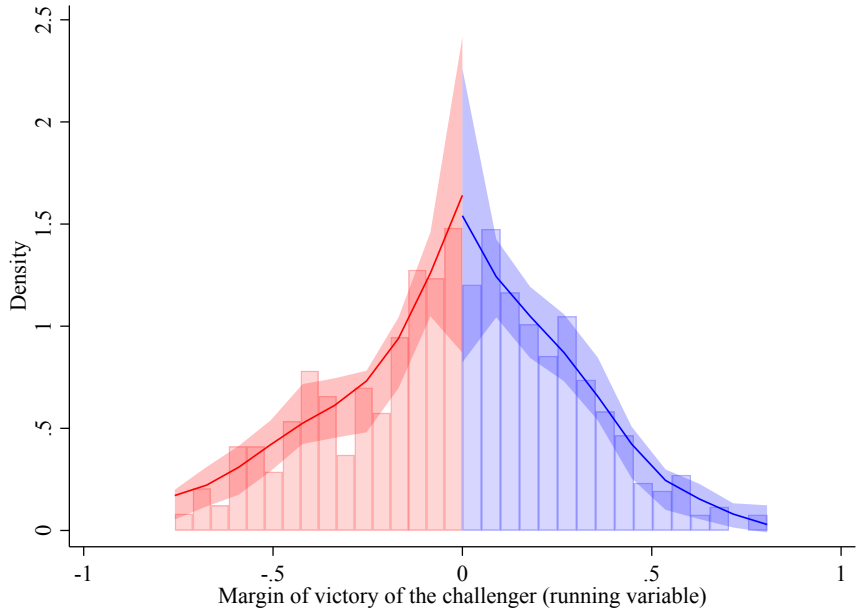
Figures

Figure 1: Density Test

(a) Distribution of the Victory Margin



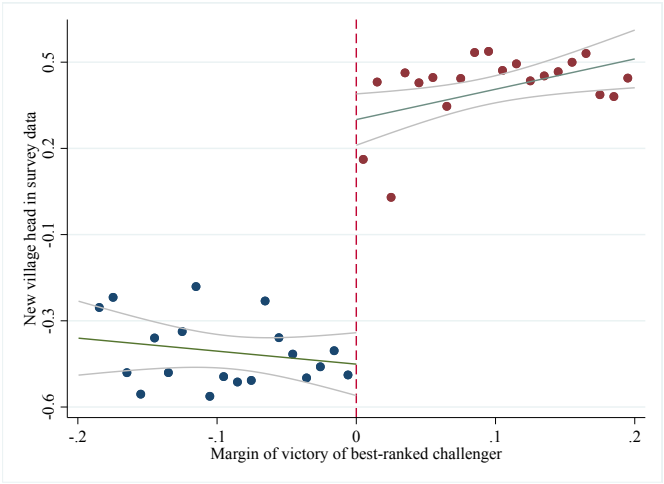
(b) Testing the Continuity of the Victory Margin



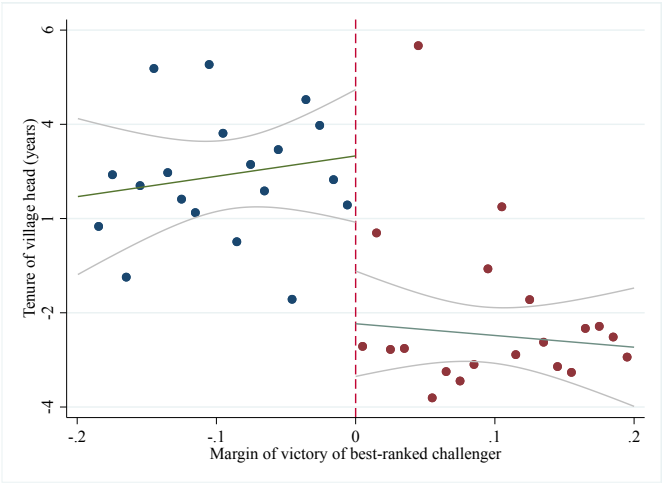
Notes: Panel (a) plots the density of the running variable in our RD estimation, defined as the difference between the vote share received by the highest-ranked challenger and the incumbent's vote share in the most recent village election. Panel (b) implements the density test from Cattaneo et al. (2018) using the margin of victory of the challenger as the running variable. The p-value from this test is 0.856.

Figure 2: Electoral and Bureaucratic Turnover

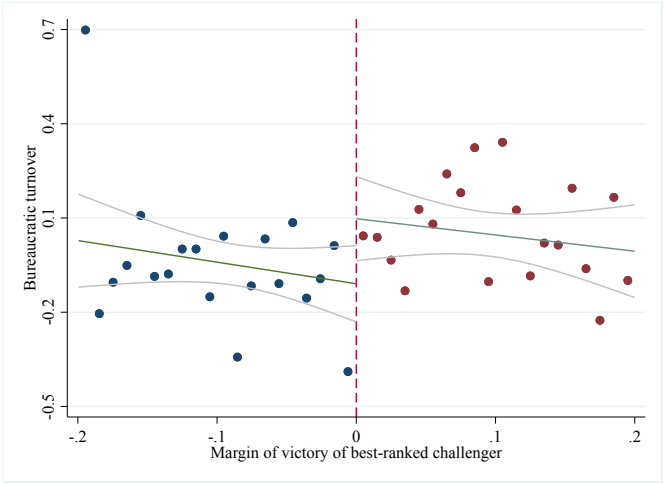
(a) New village head in survey data



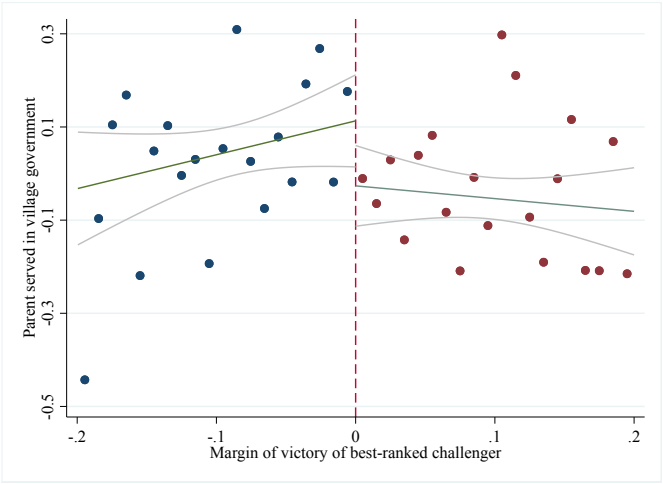
(b) Tenure of village head (years)



(c) % New appointments



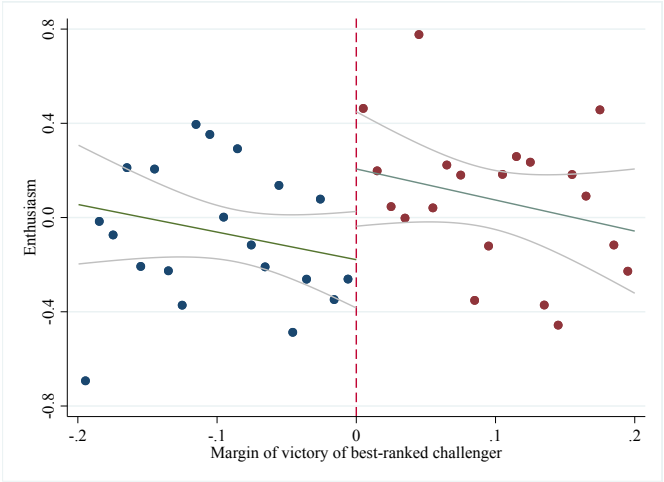
(d) Bureaucrat parent served in village gov.



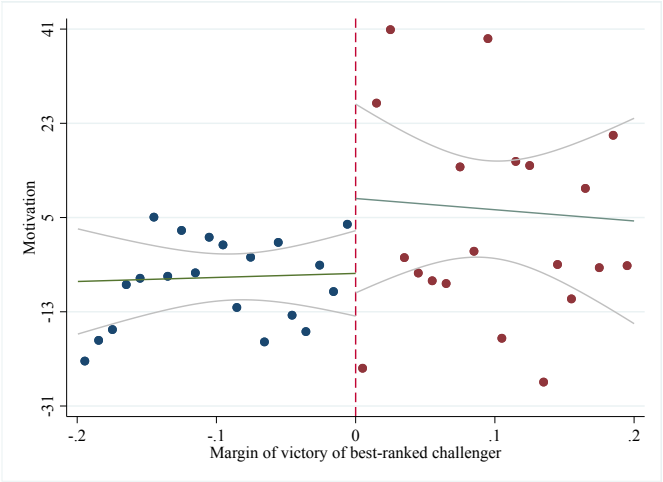
Notes: Panel (a) looks at the probability that the village head in our survey sample is a different individual from the incumbent candidate competing in the most recent village election. Panel (b) looks at the number of years in office of the village head in our sample. Panel (c) looks at the village-level fraction of bureaucrats (excluding the village head) who began in their current position since the last election. Panel (d) looks at the probability that bureaucrats have a parent who served in the village government. The dots are conditional means of each outcome across binned intervals of the margin of victory of the best-ranked challenger on each side of the RD threshold, with 95% confidence intervals in solid gray lines.

Figure 3: Bureaucratic Morale and Effort

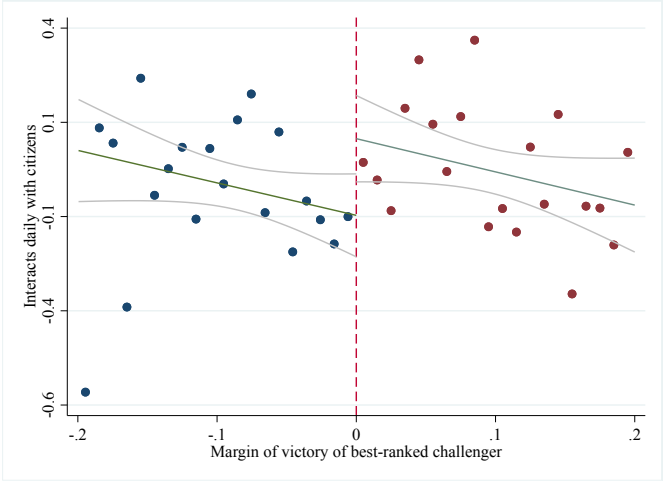
(a) Enthusiasm



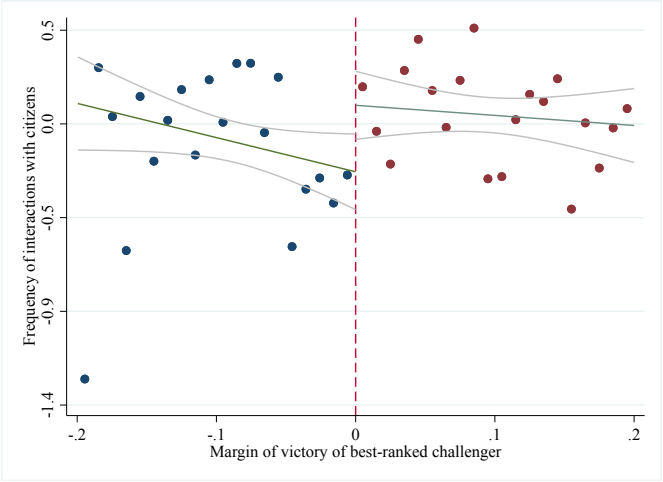
(b) Motivation



(c) Interacts daily with citizens



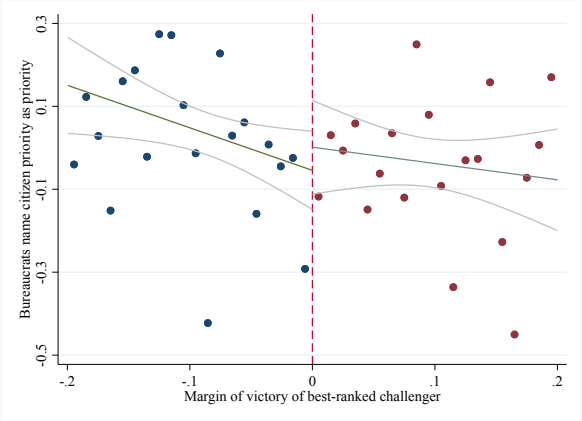
(d) Frequency of interactions



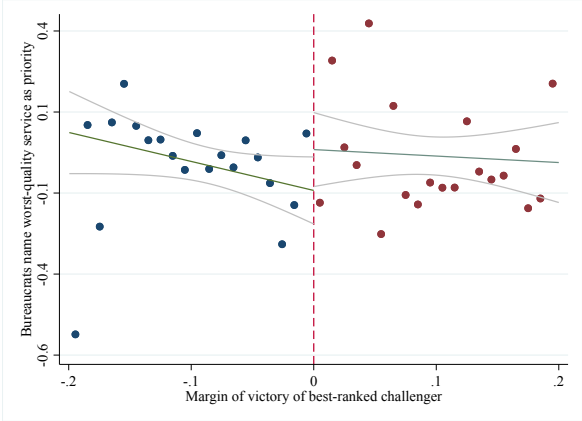
Notes: The top two figures look at morale outcomes. Panel (a) looks at a standardized z-score of self-reported enthusiasm. Panel (b) looks at a continuous measure of motivation anchored at a baseline of 100 and winsorized at the top percentile. The bottom two figures look at measures of the frequency of interactions between bureaucrats and citizens. Panel (c) looks at a dummy equal to 1 if the bureaucrat reports interacting with village citizens on a daily basis. Panel (d) looks at a standardized measure of the frequency of citizen interactions, computed from a categorical variable measured on a 1-5 scale. The dots are conditional means of each outcome across binned intervals of the margin of victory of the best-ranked challenger on each side of the RD threshold, with 95% confidence intervals in solid gray lines.

Figure 4: Understanding of Citizen Preferences

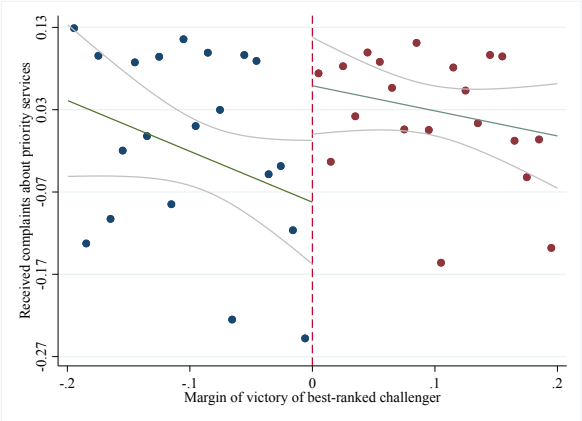
(a) Officials/citizens agree: Priority services



(b) Officials/citizens agree: Worst-quality services

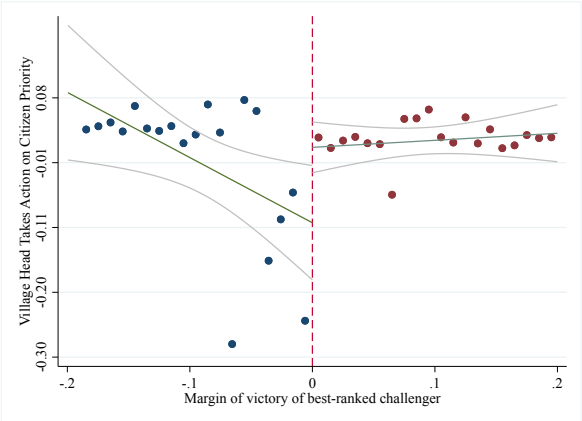


(c) Complaints received: Priority services

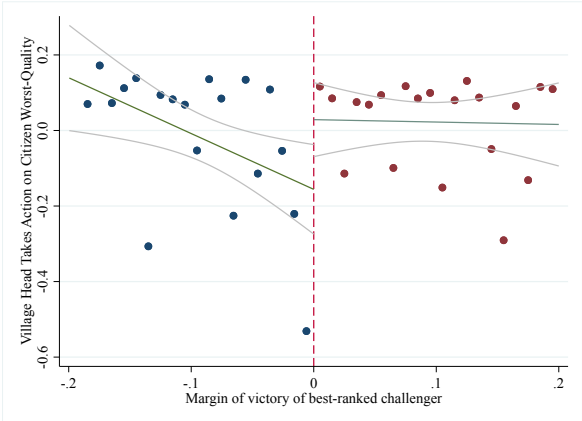


(d) Complaints received: Worst-quality services

(e) Village takes action: Priority services



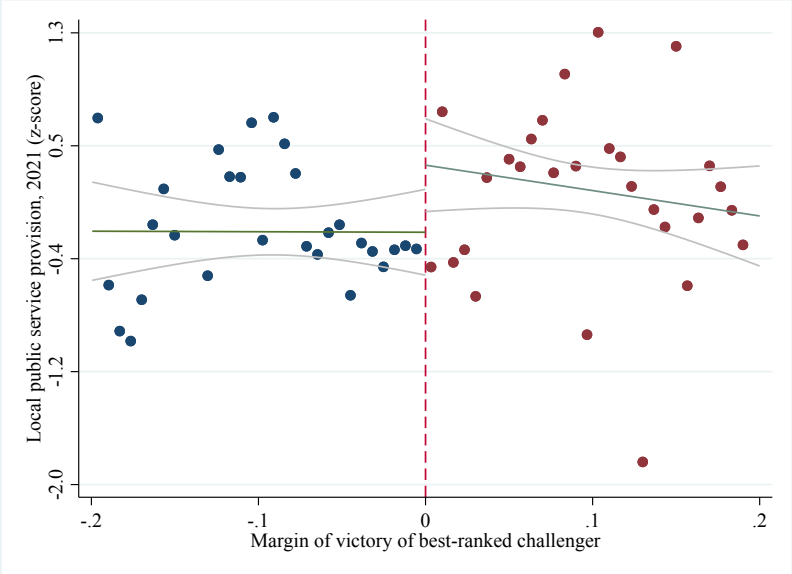
(f) Village takes action: Worst-quality services



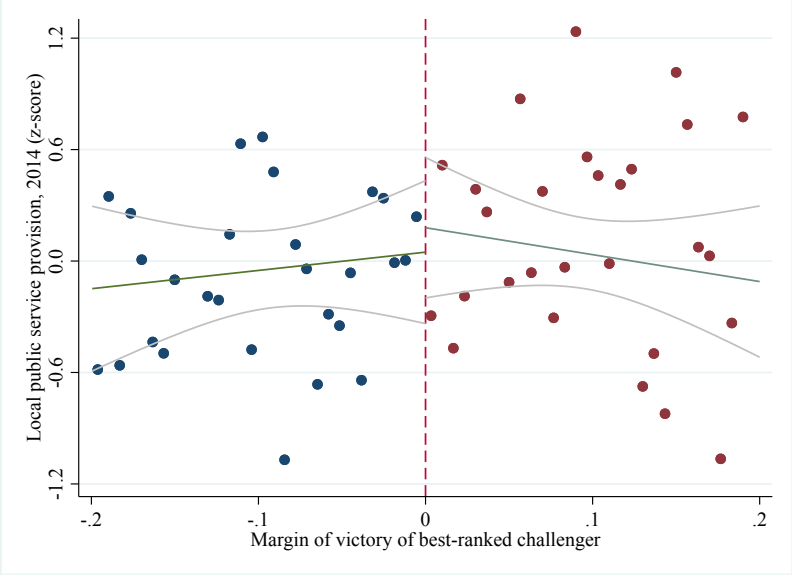
Notes: Panel (a) looks at an indicator equal to 1 if the bureaucrat names as priority for future spending a public service which village citizens identify as a top-3 priority. Panel (b) looks at an indicator equal to 1 if the bureaucrat names as priority for future spending a service which citizens rank as a bottom-3 quality public service. Panel (c) looks at an indicator equal to 1 if the bureaucrat reports receiving complaints about at least one public service that most citizens identify as a top-3 priority. Panel (d) looks at an indicator equal to 1 if the bureaucrat reports receiving complaints about at least one public service that most citizens believe is a bottom-3 quality service. Panel (e) looks at an indicator equal to 1 if the village head took action on a public service which citizens identify as a top-3 priority. Panel (f) looks at an indicator equal to 1 if the village head took action on a public service that most citizens believe is a bottom-3 quality service. See Section 4 for details. The dots are conditional means of each outcome across binned intervals of the margin of victory of the best-ranked challenger on each side of the RD threshold, with 95% confidence intervals in solid gray lines.

Figure 5: Effects on Public Goods Provision (Administrative Data)

(a) Administrative data, 2021



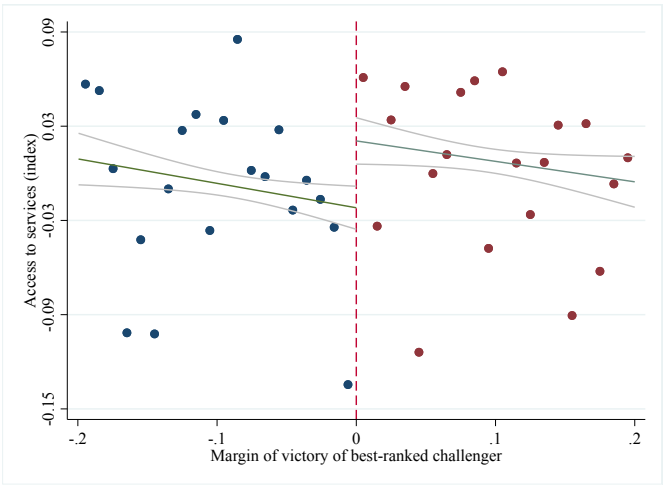
(b) Administrative data, 2014 (balance)



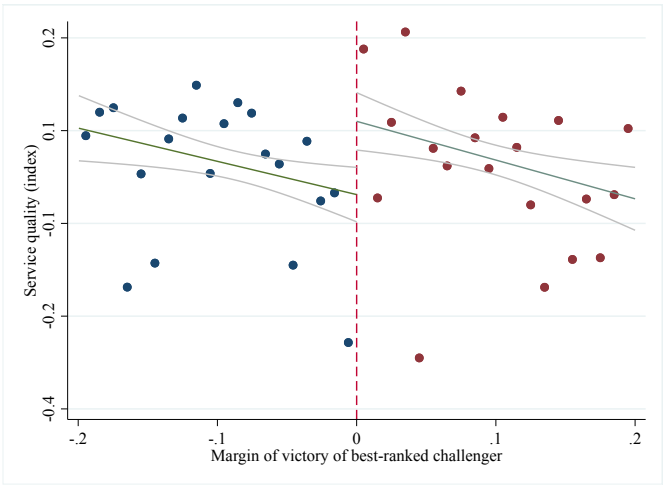
Notes: In panel (a), the dependent variable is a standardized index of local service provision constructed using the 2021 *Podes* survey. The index has the following 10 components: drinking water, sewage, garbage collection, street lighting, kindergartens, primary schools, village maternities (*polindes*), community health centers (*puskesmas*), paved roads, and public transit. We first standardize each individual component before taking the village-level average of all components. The sample includes all villages in our sample that conducted their last election before 2021. In panel (b), the dependent variable is a standardized index of local service provision constructed using the 2014 *Podes* survey. The 2014 index has the same components except garbage collection, which was not collected in 2014, and a different coding for street lighting, which was categorized into two groups in 2014 (*no lights*, *any lights*), compared to three groups in 2021 (*no lights*, *some lights*, and *many lights*). The dots are conditional means of each outcome across binned intervals of the margin of victory of the best-ranked challenger on each side of the RD threshold, with 95% confidence intervals in solid gray lines.

Figure 6: Effects on Public Goods Provision & Village Actions (Survey Data)

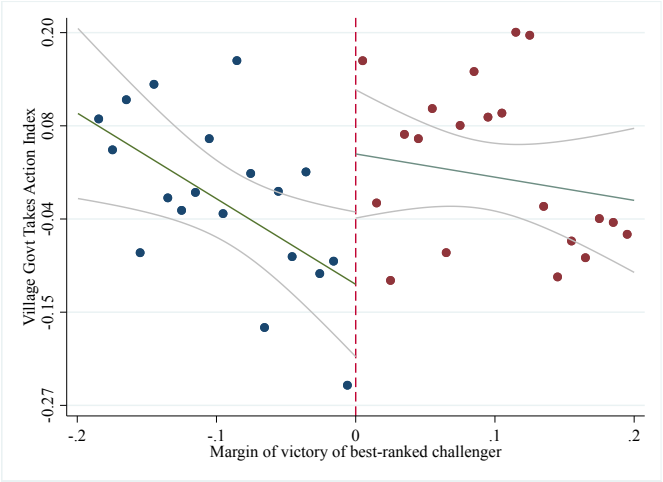
(a) Access to services



(b) Service quality



(c) Village actions to improve services



Notes: In panel (a), the dependent variable is an index of access to local services constructed using our survey data. In panel (b), the dependent variable is a standardized index of service quality. In panel (c), the dependent variable is an index indicating whether the village government took action or made investments on a public service. All indices have the following components: water access, garbage collection, paved roads, electricity, kindergartens, primary schools, and community healthcare. The dots are conditional means of each outcome across binned intervals of the margin of victory of the best-ranked challenger on each side of the RD threshold, with 95% confidence intervals in solid gray lines.

Tables

Table 1: Turnover, Public Goods, and Nepotism: Correlations (OLS)

	Public Goods Index				**Village government**		**Village head survey**	
	Podes 2021		*Podes* 2021-2014 growth		% New appts		Village head relative	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent runs	0.006	0.102	0.046	0.643	-0.035	0.031	0.031	-0.024
	(0.064)	(0.077)	(0.377)	(0.511)	(0.029)	(0.034)	(0.040)	(0.047)
Incumbent wins		-0.181**		-1.121**		-0.123***		0.102**
		(0.080)		(0.492)		(0.033)		(0.046)
P-value, total effect		0.29		0.22		0.0055		0.091
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample mean	0.31	0.31	1.33	1.33	0.39	0.39	0.37	0.37
Observations	565	565	562	562	784	784	679	679

Notes: This table reports OLS estimates of each outcome on two binary variables: *incumbent runs*, indicating whether the incumbent village head competed in the most recent election and *incumbent wins*, indicating whether the incumbent won that election, respectively. Units of observation are villages in columns 1-6 and village heads in columns 7-8. The dependent variable is: in columns 1 and 2, a standardized index of local public service provision constructed using the 2021 Podes data. The index has the following 10 components: drinking water, sewage, garbage collection, street lighting, kindergartens, primary schools, village maternities (polindes), community health centers (puskesmas), paved roads, and public transit. In columns 3 and 4, the growth in service quality between the 2014 and 2021 Podes waves. In columns 5 and 6, the rate of bureaucratic turnover at the village level since the last election, defined as the fraction of new bureaucrats appointed to their current position since the last election. In columns 7 and 8, a dummy equal to 1 if relatives of the village head are employed in the village government. Regressions include region fixed effects. The main regions in our sample are Java, Sulawesi, Sumatra, Kalimantan, and NTB-Bali.
* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 2: Effects of Turnovers on Bureaucratic Organization

	Village heads		Village government	
	New leader	Tenure (yrs)	% New appts	% Reshuffling
	(1)	(2)	(3)	(4)
New village head	0.835***	-4.908***	0.182**	0.089*
	(0.084)	(1.236)	(0.085)	(0.045)
	[0.101]	[1.527]	[0.100]	[0.052]
Observations	442	443	510	510
Control mean	0.035	7.96	0.33	0.036
Robust p-value	0.000	0.001	0.042	0.073
MSE-opt. bandwidth	15.8	31.2	22.0	18.9
Effective obs.	172	285	256	235

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). Units of observations are village heads in columns 1-2 and villages in columns 3-4. The dependent variable is: in column 1, a dummy equal to 1 if the village head in our survey data is a different individual from the incumbent competing in the most recent village election; in column 2, the number of years spent in office by the current village head; in column 3, the rate of bureaucratic turnover at the village level since the last election, defined as the fraction of new bureaucrats appointed to their current position since the last election; in column 4, the share of bureaucrats "reshuffled" within the village secretariat, defined as the fraction of bureaucrats who experienced a promotion, demotion, or lateral move since the last election. See Section 4 for details.
* p<0.1, ** p<0.05, *** p<0.01. Conventional standard errors in parentheses and robust bias-corrected standard errors in brackets. Significance levels are based on the robust bias-corrected standard errors.

Table 3: Effects of Turnovers on Morale and Effort

	Bureaucrat survey			
	Enthusiasm	Motivation	Interacts daily w/ citizens	Frequency index
	(1)	(2)	(3)	(4)
New village head	0.487***	22.868	0.197**	0.405**
	(0.143)	(15.03)	(0.088)	(0.156)
	[0.165]	[18.495]	[0.102]	[0.183]
Observations	1064	1062	1064	1064
Control mean	-0.057	100.6	0.57	0.32
Robust p-value	0.001	0.153	0.029	0.012
Bandwidth size (%)	20.8	21.5	18.5	16.3
Effective obs.	522	533	487	441

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). Units of observation are bureaucrats in all columns. The dependent variable is: in column 1, a standardized z-score of self-reported enthusiasm; in column 2, a continuous measure of motivation anchored at 100 at baseline and winsorized at the top 1%; in column 3, a dummy variable equal to 1 if the bureaucrat reports interacting with citizens on a daily basis; in column 4, a standardized z-score of the frequency of bureaucrat-citizen interactions measured on a 1-5 scale. See Section 4 for details.

* p<0.1, ** p<0.05, *** p<0.01. Conventional standard errors in parentheses and robust bias-corrected standard errors clustered by village in brackets. Significance levels are based on the robust bias-corrected standard errors clustered by village.

Table 4: Effects of Turnovers on Nepotism

	Village head survey	**Bureaucrat survey**	
	Employs relative	Parent was leader	Parent served in govt
	(1)	(2)	(3)
New village head	-0.385***	-0.066	-0.168**
	(0.155)	(0.043)	(0.069)
	[0.178]	[0.051]	[0.082]
Observations	442	1067	1067
Control mean	0.36	0.054	0.27
Robust p-value	0.008	0.109	0.0340
Bandwidth size (%)	12.7	17.5	22.5
Effective obs.	149	466	550

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). Units of observation are village heads in column 1 and bureaucrats in columns 2-3. The dependent variable is: in column 1, a dummy equal to 1 if relatives of the village head are employed in the village government; in column 2, a dummy equal to 1 if the bureaucrat reports having a parent who served as village head; in column 3, a dummy equal to 1 if the bureaucrat reports having a parent who served in the village government. See Section 4 for details.
* p<0.1, ** p<0.05, *** p<0.01. Conventional standard errors in parentheses and robust bias-corrected standard errors in brackets. In columns 2–3, standard errors are additionally clustered by village. Significance levels are based on the robust bias-corrected standard errors (clustered by village in columns 2–3).

Table 5: Effects of Turnovers on Alignment with Citizens' Preferences

	Bureaucrat survey				Village head survey	
	Officials/citizens agree on:		**Complaints received about:**		**Village head takes action on:**	
Village Services:	Priority	Worst-quality	Priority	Worst-quality	Priority	Worst-quality
	(1)	(2)	(3)	(4)	(5)	(6)
New village head	0.105	0.236**	0.162***	0.174**	0.127*	0.213**
	(0.102)	(0.103)	(0.059)	(0.067)	(0.065)	(0.087)
	[0.116]	[0.117]	[0.068]	[0.079]	[0.079]	[0.103]
Observations	1067	1067	1067	1067	443	443
Control mean	0.75	0.32	0.87	0.71	0.94	0.88
Robust p-value	0.204	0.015	0.006	0.028	0.073	0.019
MSE-opt. bandwidth	17.5	17.0	18.4	25.7	22.7	26.4
Effective obs.	467	457	484	606	223	253

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). Units of observation are bureaucrats in columns 1–4 and village heads in columns 5–6. In column 1, the dependent variable is an indicator equal to 1 if the bureaucrat names as priority for future development spending a public service which village citizens identify as a top-3 priority. In column 2, the dependent variable is an indicator equal to 1 if the bureaucrat names as priority for future development spending a service which citizens rank as a bottom-3 quality public service. In column 3, the dependent variable is an indicator equal to 1 if the bureaucrat reports receiving complaints about at least one public service the majority of village citizens identify as a top-3 priority. In column 4, the dependent variable is an indicator equal to 1 if the bureaucrat reports receiving complaints about at least one public service the majority of village citizens believe is a bottom-3 quality public service. In column 5, the dependent variable is an indicator equal to 1 if the village head took action on a public service which village citizens identify as a top-3 priority. In column 6, the dependent variable is an indicator equal to 1 if the village head took action on at least one public service the majority of village citizens believe is a bottom-3 quality public service. See Section 4 for details.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table 6: Knowledge of Public Goods Needs, Heterogeneity by Baseline Road Quality

	Balance *Podes* 2014	Knowledge Roads Quality	Receive Roads Complaint	Take Action on Roads
	(1)	(2)	(3)	(4)
		Panel A: Baseline Roads Quality = High		
New village head	0.117	0.635**	0.052	-0.149
	(0.108)	(0.292)	(0.116)	(0.161)
	[0.128]	[0.332]	[0.137]	[0.196]
Observations	496	525	525	213
Control mean	0.49	2.49	0.68	0.76
Robust p-value	0.391	0.0155	0.870	0.344
MSE-opt. bandwidth	28.2	12.3	18.8	21.8
Effective obs.	306	187	250	106
		Panel B: Baseline Roads Quality = Low		
New village head		-0.226	0.387***	0.355*
		(0.491)	(0.112)	(0.166)
		[0.590]	[0.134]	[0.204]
Observations		510	510	215
Control mean		2.42	0.67	0.70
Robust p-value		0.563	0.001	0.086
MSE-opt. bandwidth		16.2	17.3	17.1
Effective obs.		205	219	90
High vs. low p-value		0.245	0.145	0.144

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). Units of observation are villages in column 1, bureaucrats in columns 2 and 3, and village heads in column 4. In column 1, the dependent variable is an indicator for whether a village is below or above the median of a standardized index of transportation costs constructed using the 2014 Podes data. In column 2, the dependent variable is bureaucrats' rating of road quality measured on a 5-point Likert scale. In column 3, the dependent variable is an indicator equal to 1 if the bureaucrat reports receiving complaints about roads from village citizens. In column 4, the dependent variable is an indicator equal to 1 if the village head took action on roads. Road quality is measured by a standardized index of transportation costs to the subdistrict office and the regent or mayor's office, where lower transportation costs indicate better road quality. Panel A restricts the sample to villages with high baseline road quality (transportation costs below the sample median), while Panel B restricts the sample to villages with low baseline road quality (transportation costs above the sample median). The bottom row reports the p-value from the test of Calonico et al. (2025) that robust RD estimates are significantly different across the two panels. See Section 4 for details.
* p<0.1, ** p<0.05, *** p<0.01. Conventional standard errors in parentheses and robust bias-corrected standard errors in brackets. Significance levels are based on the robust bias-corrected standard errors.

Table 7: Effects of Turnovers on Public Goods Provision

	Public Goods Index	**Citizen survey**		**Village head survey**	**Balance**
	Podes 2021	Access	Quality	Take Action	*Podes* 2014
	(1)	(2)	(3)	(4)	(5)
New village head	0.503*	0.073**	0.208**	0.165**	-0.058
	(0.216)	(0.037)	(0.090)	(0.071)	(0.341)
	[0.263]	[0.043]	[0.102]	[0.086]	[0.419]
Observations	378	8848	8846	443	375
Control mean	0.23	0.78	-0.028	0.76	0.018
Robust p-value	0.053	0.039	0.014	0.046	0.823
MSE-opt. bandwidth	18.7	15.2	14.9	24.9	19.0
Effective obs.	161	3479	3427	241	161

Notes: This table reports RD estimates of γ in equation (1). Units of observation are villages in columns 1, 4 and 5, and citizens in columns 2 and 3. In column 1, the dependent variable is a standardized index of local public service provision constructed using the 2021 Podes data. The index has the following components: drinking water, sewage, garbage collection, street lighting, paved roads, public transit, kindergartens, primary schools, village maternities (*polindes*), and community health centers (*puskesmas*). The index is standardized to have mean 0 and standard deviation 1. In column 2, the dependent variable is an index of access to local services constructed using our citizen survey data. The index ranges between 0 and 1 and is constructed as the average of binary variables denoting access to each type of service, where 1 denotes that the service is accessible in the village. In column 3, the dependent variable is a standardized index of service quality based on citizens' ratings of service quality for each service on a 5-point Likert scale. In column 4, the dependent variable is an index of village governments taking action on a public service, constructed as the average of binary variables indicating that the village government took action or made investments to improve each type of service, where 1 denotes that such action was taken. The indices in columns 2–4 each have the following components: water, garbage collection, paved roads, electricity, kindergartens, primary schools, and community healthcare. In column 5, the dependent variable is a standardized index of local public service provision constructed using the 2014 Podes data, and serves as a balance check. The 2014 index has the same components as in column 1, with the exception of garbage collection, which was not collected in 2014, and a different coding for street lighting, which was categorized into two groups in 2014 (*no lights*, *any lights*), compared to three groups in 2021 (*no lights*, *some lights*, and *many lights*). See Section 5 for details.
* p<0.1, ** p<0.05, *** p<0.01. Conventional standard errors in parentheses and robust bias-corrected standard errors in brackets. In columns 2–3, standard errors are additionally clustered by village. Significance levels are based on the robust bias-corrected standard errors (clustered by village in columns 2–3).

Appendix (For Online Publication)

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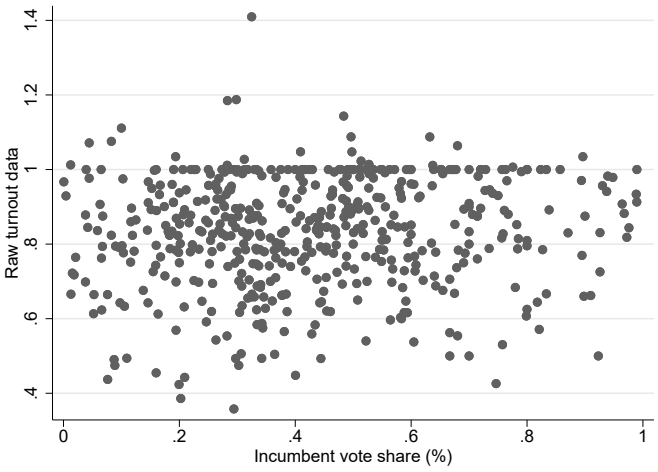
A Additional Results

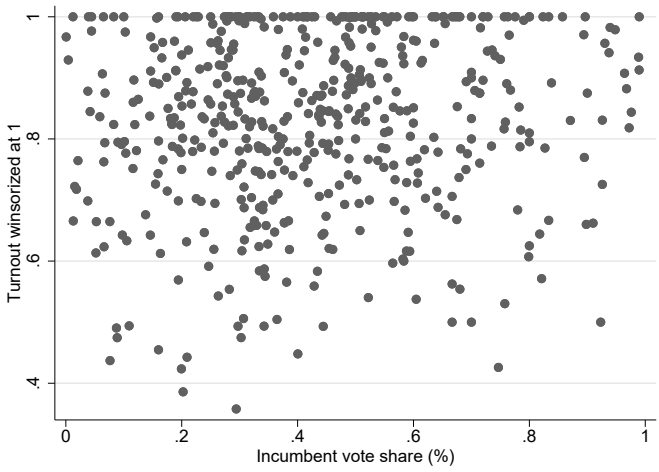
Figures

Figure A.1: Electoral Data Checks: Turnout

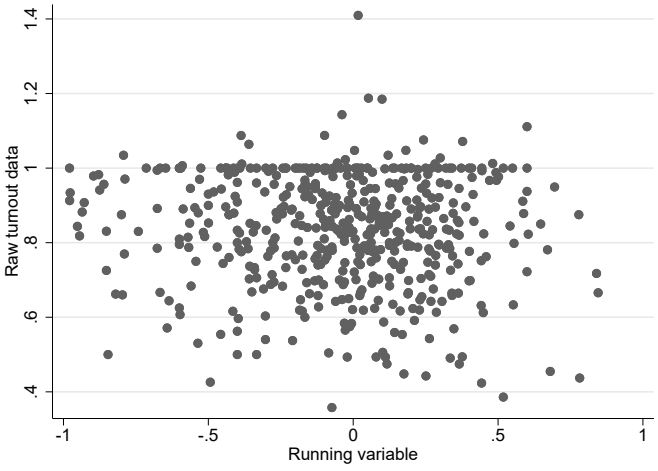
(a) Raw Turnout vs. incumbent vote



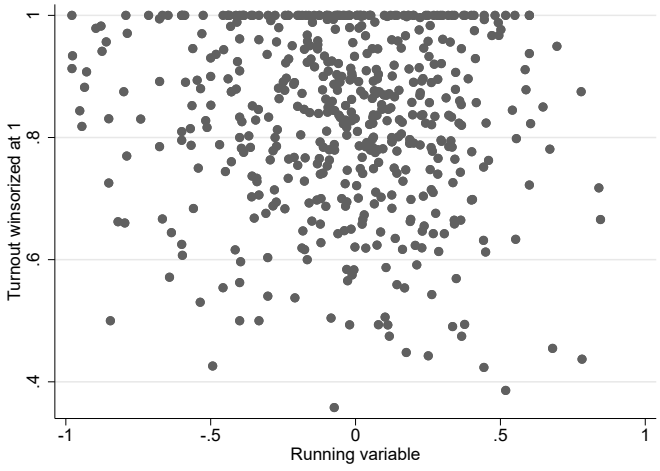
(b) Winsorized Turnout vs. incumbent vote



(c) Raw Turnout vs. running variable

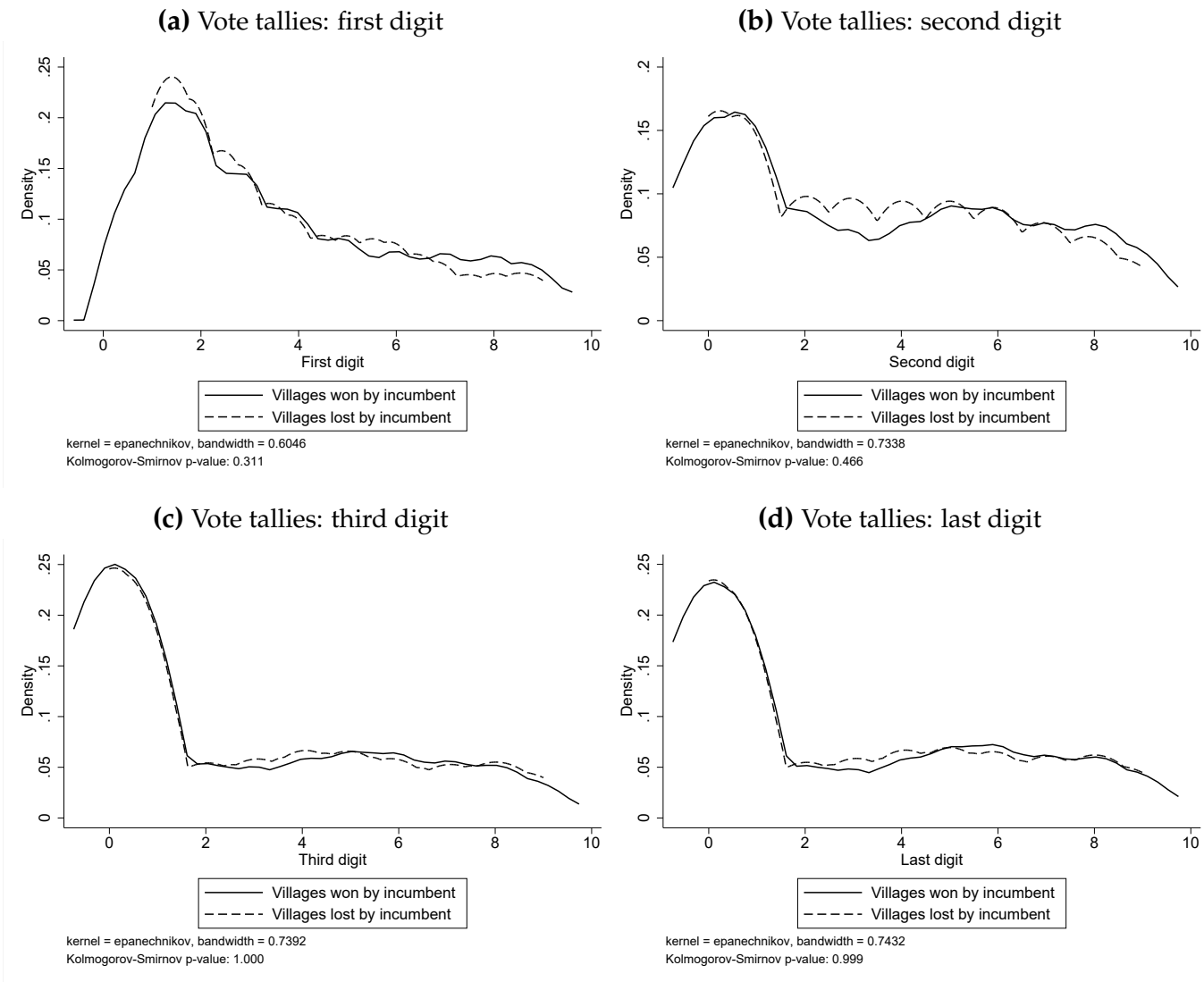


(d) Winsorized Turnout vs. running variable



Notes: Panels (a) and (b) plot raw turnout and turnout winsorized at 100% against the vote share of the incumbent candidate. Panels (c) and (d) plot raw turnout and turnout winsorized at 100% against our running variable in the RD analysis, namely the difference between the vote share of the highest-ranked challenger and the incumbent's vote share.

Figure A.2: Electoral Data Checks: Digit Distribution in Vote Tallies



Notes: This figure plots the distribution of the first, second, third, and last digits of candidate vote tallies, separately for villages won and villages lost by the incumbent. At the bottom of each panel, we report the p-value from a Kolmogorov-Smirnov test of equality of distributions across the two types of villages.

Tables

Table A.1: Balance Checks on Village Characteristics and Electoral Data

	Hamlets	HHs	Sumatra	Java	NTB-Bali	Kalimantan	Sulawesi	Reg. voters	Candidates	Turnout	Turnout≥1	Herfind.	Rounding
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
New village head	-0.599	-0.154	-0.057	-0.008	0.198*	0.058	-0.015	-512.870	-0.195	0.043	-0.002	0.001	-0.238
	(0.665)	(0.278)	(0.143)	(0.102)	(0.112)	(0.077)	(0.100)	(554.393)	(0.355)	(0.046)	(0.061)	(0.026)	(0.422)
Observations	512	509	512	512	512	512	512	512	512	512	512	512	512
Control mean	4.65	6.47	0.31	0.15	0.16	0.075	0.17	2229.8	3.43	0.84	0.025	0.39	1.62
Robust p-value	0.313	0.543	0.676	0.782	0.058	0.418	0.926	0.367	0.533	0.215	0.930	0.980	0.471
MSE-opt. bandwidth	19.6	20.5	21.0	27.2	19.8	19.1	22.2	20.4	22.0	21.6	31.5	31.3	18.4
Effective obs.	241	246	251	305	242	238	258	249	258	255	336	336	230

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). The dependent variable is: in column 1, the number of neighborhoods/hamlets in the village; in column 2, the log number of households residing in the village; in columns 3-7, a dummy equal to 1 if the village is located on the island of Sumatra, Java, Nusa Tenggara Barat/Bali, Kalimantan, and Sulawesi, respectively; in column 8, the number of registered voters in the most recent village election; in column 9, the number of candidates; in column 10, voter turnout (votes cast divided by the number of registered voters); in column 11, a dummy equal to 1 if reported turnout was greater than 100% in the most recent election; in column 12, a Herfindahl index of candidate vote shares; in column 13, the number of candidates with a trailing zero in their vote tally.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.2: Balance Checks on Village Characteristics: Administrative Data

	Latitude	Longitude	Altitude	Coastal	Forest	Agric.	Rice	Corn	Rubber	Palm oil
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
New village head	0.659	0.146	106.152	-0.009	0.013	-0.049	0.144	-0.190**	-0.026	-0.003
	(0.725)	(3.019)	(161.475)	(0.073)	(0.089)	(0.085)	(0.127)	(0.083)	(0.060)	(0.008)
Observations	512	512	512	512	512	512	512	512	512	512
Control mean	4.76	110.6	179.6	0.093	0.14	0.92	0.56	0.17	0.034	0.012
Robust p-value	0.327	0.994	0.377	0.991	0.930	0.453	0.228	0.015	0.733	0.540
MSE-opt. bandwidth	23.1	19.4	17.0	22.6	20.5	23.9	28.5	19.4	18.7	11.5
Effective obs.	266	239	216	262	249	276	316	240	234	158

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). The dependent variable is: in columns 1 through 3, the latitude, longitude, and altitude of the village, respectively; in columns 4 and 5, a dummy variable equal to 1 if the village is located in a coastal area or a forest area, respectively; in column 6, a dummy equal to 1 if agriculture is the main economic activity in the village; and in columns 7 through 10, a dummy equal to 1 if rice, corn, rubber, or palm oil is the main economic activity in the village, respectively. All dependent variables are measured in the 2021 wave of the Podes survey.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.3: Balance Checks on Sampling of Citizens

	Listed by village official	Listed by BPD member	Degree of Connection
	(1)	(2)	(3)
New village head	-0.006	-0.025	-0.213
	(0.022)	(0.020)	(0.251)
Observations	14484	14484	14484
Control mean	0.16	0.12	1.98
Robust p-value	0.869	0.118	0.354
MSE-opt. bandwidth	18.7	19.2	19.1
Effective obs.	6680	6842	6768

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). The dependent variable is: in column 1, a dummy equal to 1 if a village official directly provided a citizen's phone number; in column 2, a dummy equal to 1 if a BPD member directly provided a citizen's phone number; in column 3, a variable measuring the degree of connections between citizens and village/BPD officials.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.4: Balance Checks on Citizen Demographics

	Female	Age	Disability	Tertiary Educ.	Not Working	Monthly Income
	(1)	(2)	(3)	(4)	(5)	(6)
New village head	-0.051	-0.330	0.001	0.025	-0.016	-85.046
	(0.080)	(1.646)	(0.011)	(0.049)	(0.057)	(341.049)
Observations	8880	8816	8873	8850	8856	6536
Control mean	0.47	34.9	0.025	0.20	0.26	2198
Robust p-value	0.451	0.860	0.988	0.609	0.749	0.892
MSE-opt. bandwidth	19.4	17.2	26.5	20.3	17.6	18.3
Effective obs.	4196	3767	5195	4318	3879	2961

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). The dependent variable is: in column 1, a dummy equal to 1 for female citizens; in column 2, the age of citizens in years; in column 3, a dummy for whether the citizen reports having a disability; in column 4, a dummy for whether the citizen has a tertiary level education; in column 5, a dummy for whether the citizen reports not working; in column 6, the citizen's average monthly income in the last 6 months (conditional on working) and winsorized at the top 1% and reported in thousands of Indonesian rupiah.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.5: Effects on Bureaucrats' Demographic Characteristics

	Age	Years of education	Gender (female)
	(1)	(2)	(3)
New village head	1.055	-0.521	-0.152*
	(1.797)	(0.439)	(0.090)
Observations	1061	1066	1067
Control mean	38.6	13.6	0.28
Robust p-value	0.338	0.191	0.055
MSE-opt. bandwidth	17.8	20.5	16.1
Effective obs.	474	523	437

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). Units of observation are bureaucrats in all columns. The dependent variable is: in column 1, the age of bureaucrats in years; in column 2, years of education; in column 3, a dummy equal to one for female bureaucrats. See Section 4 for details.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.6: Robustness Checks on Bureaucratic Organization (Table 2)

	Village heads		Village government	
	New leader	Tenure (yrs)	% New appts	% Reshuffling
	(1)	(2)	(3)	(4)
1. Baseline	0.835***	-4.908***	0.182**	0.089*
	(0.101)	(1.527)	(0.100)	(0.052)
2. No controls	0.831***	-4.006**	0.139	0.068
	(0.098)	(1.930)	(0.102)	(0.053)
3. Region fixed effects	0.805***	-4.268***	0.160	0.087*
	(0.090)	(1.500)	(0.100)	(0.051)
4. 3rd-Degree polynomial	0.809***	-4.698*	0.127	0.106
	(0.126)	(2.452)	(0.152)	(0.065)
5. 1/2 MSE-opt. bandwidth	0.825***	-5.502*	0.094*	0.053
	(0.228)	(2.776)	(0.150)	(0.042)
6. 3/4 MSE-opt. bandwidth	0.813***	-5.145**	0.141	0.082
	(0.153)	(2.317)	(0.131)	(0.055)
7. 2x MSE-opt. bandwidth	0.888***	-4.369***	0.141**	0.092*
	(0.087)	(1.347)	(0.092)	(0.048)
8. Excl. Suharto villages	0.832***	-5.079***	0.199**	0.097*
	(0.103)	(1.752)	(0.100)	(0.054)
9. Fuzzy RD	—	-6.507***	0.115	0.120*
		(2.513)	(0.139)	(0.073)
10. Excl. lame-duck	0.839***	-4.701***	0.179**	0.092*
	(0.104)	(1.127)	(0.102)	(0.054)
Observations	442	443	510	510
Control mean	0.035	7.96	0.33	0.036
MSE-opt. bandwidth	15.8	31.2	22.0	18.9

Notes: This table reports robustness checks on Table 2. The first row reports our baseline estimates. In the second row, we remove election year dummies and our control for the survey experiment treatment, which are included in our baseline estimation. In the third row, we include region fixed effects and dummies for pairs of election years (2015-2016, 2017-2018, etc.). The main regions in our sample are Java, Sulawesi, Sumatra, Kalimantan, and NTB-Bali. In the fourth row, we use a 3rd-degree polynomial to construct the point estimator. In the fifth, sixth, and seventh rows, we use a RD bandwidth equal to 1/2, 3/4, and twice the MSE-optimal bandwidth from Calonico et al. (2014), respectively. In the eighth row, we exclude villages with bureaucrats appointed during the Suharto era. In the ninth row, we report fuzzy RD estimates of γ in equation (1). We use $\mathbb{1}(\text{margin}_{jt} > 0)$ from equation (1) to instrument for a dummy equal to 1 if the current village head in our survey sample is a different individual from the incumbent who competed in the last election. In the ninth row, we exclude villages where the current village head is serving in their third term. The dependent variables are identical to those in Table 2. The bottom panel reports the bandwidth size for the baseline estimation.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.7: Robustness Checks on Bureaucrat Outcomes (Table 3)

	Bureaucrat survey			
	Enthusiasm	Motivation	Interacts daily w/ citizens	Frequency index
	(1)	(2)	(3)	(4)
1. Baseline	0.487***	22.868	0.197**	0.405**
	(0.165)	(18.495)	(0.102)	(0.183)
2. No controls	0.343**	30.825*	0.198*	0.402**
	(0.155)	(20.756)	(0.111)	(0.195)
3. Region fixed effects	0.322**	25.430	0.148*	0.337**
	(0.152)	(18.557)	(0.096)	(0.174)
4. 3rd-Degree polynomial	0.705***	34.742	0.229	0.570**
	(0.236)	(26.132)	(0.136)	(0.246)
5. 1/2 MSE-opt. bandwidth	0.539**	30.067	0.205	0.452
	(0.287)	(28.168)	(0.165)	(0.281)
6. 3/4 MSE-opt. bandwidth	0.528**	27.447	0.194	0.455
	(0.233)	(26.313)	(0.134)	(0.254)
7. 2x MSE-opt. bandwidth	0.318***	20.093	0.161**	0.329***
	(0.156)	(15.826)	(0.094)	(0.169)
8. Excl. Suharto villages	0.384**	24.870	0.199**	0.403**
	(0.157)	(20.579)	(0.104)	(0.181)
9. Fuzzy RD	0.710***	37.908	0.229*	0.582***
	(0.235)	(25.172)	(0.140)	(0.249)
10. Excl. lame-duck	0.527***	22.989	0.188**	0.363**
	(0.179)	(17.014)	(0.103)	(0.181)
Observations	1064	1062	1064	1064
Control mean	-0.057	100.6	0.57	0.32
Bandwidth size (%)	20.8	21.5	18.5	16.3

Notes: This table reports robustness checks on Table 3. The first row reports our baseline estimates. In the second row, we remove election year dummies and our control for the survey experiment treatment, which are included in our baseline estimation. In the third row, we include region fixed effects and dummies for pairs of election years (2015-2016, 2017-2018, etc.). The main regions in our sample are Java, Sulawesi, Sumatra, Kalimantan, and NTB-Bali. In the fourth row, we use a 3rd-degree polynomial to construct the point estimator. In the fifth, sixth, and seventh rows, we use a RD bandwidth equal to 1/2, 3/4, and twice the MSE-optimal bandwidth from Calonico et al. (2014), respectively. In the eighth row, we exclude villages with bureaucrats appointed during the Suharto era. In the ninth row, we report fuzzy RD estimates of γ in equation (1). We use $\mathbb{1}(\text{margin}_{jt} > 0)$ from equation (1) to instrument for a dummy equal to 1 if the current village head in our survey sample is a different individual from the incumbent who competed in the last election. In the tenth row, we exclude villages where the current village head is serving in their third term. The dependent variables are identical to those in Table 3. The bottom panel reports the bandwidth size for the baseline estimation.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.8: Public Goods and Nepotism: Correlations (OLS)

	Public Goods Index	**Citizen Perceptions**	
	Podes 2021	Access	Quality
	(1)	(2)	(3)
Any nepotistic appointee	-0.226***	-0.051*	-0.017
	(0.086)	(0.029)	(0.011)
Sample mean	0.36	0.010	0.80
Observations	376	8819	8821

Notes: This table reports OLS estimates of each outcome on a dummy variable for whether a village has at least one bureaucrat who reports having a parent who served in the village government. The dependent variable is: in column 1, a standardized index of local public service provision constructed using the 2021 Podes data; in column 2, a standardized index of access to local services constructed using our citizen survey data; and in column 3, a standardized index of service quality constructed using our citizen survey data. See Section 4 for details.
* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses for column 1 and robust standard errors clustered by village in parentheses for columns 2-3.

Table A.9: Robustness Checks on Nepotism (Table 4)

	Village head survey	**Bureaucrat survey**	
	Employs relative	Parent was leader	Parent served in govt
	(1)	(2)	(3)
1. Baseline	-0.385***	-0.066	-0.168**
	(0.178)	(0.051)	(0.082)
2. No controls	-0.191	-0.040	-0.165**
	(0.179)	(0.046)	(0.084)
3. Region fixed effects	-0.208*	-0.051	-0.164**
	(0.161)	(0.046)	(0.084)
4. 3rd-Degree polynomial	-0.516**	-0.096	-0.209*
	(0.237)	(0.065)	(0.120)
5. 1/2 MSE-opt. bandwidth	-0.456	-0.080	-0.188
	(0.401)	(0.073)	(0.131)
6. 3/4 MSE-opt. bandwidth	-0.442	-0.076	-0.200*
	(0.326)	(0.064)	(0.109)
7. 2x MSE-opt. bandwidth	-0.067**	-0.027	-0.134**
	(0.165)	(0.046)	(0.073)
8. Excl. Suharto villages	-0.379**	-0.078*	-0.172**
	(0.182)	(0.049)	(0.085)
9. Fuzzy RD	-0.237**	-0.086*	-0.396***
	(0.178)	(0.062)	(0.129)
10. Excl. lame-duck	-0.419***	-0.079*	-0.150**
	(0.179)	(0.050)	(0.077)
Observations	442	1067	1067
Control mean	0.36	0.054	0.27
Bandwidth size (%)	12.7	17.5	22.5

Notes: This table reports robustness checks on Table 4. The first row reports our baseline estimates. In the second row, we remove election year dummies and our control for the survey experiment treatment, which are included in our baseline estimation. In the third row, we include region fixed effects and dummies for pairs of election years (2015-2016, 2017-2018, etc.). The main regions in our sample are Java, Sulawesi, Sumatra, Kalimantan, and NTB-Bali. In the fourth row, we use a 3rd-degree polynomial to construct the point estimator. In the fifth, sixth, and seventh rows, we use a RD bandwidth equal to 1/2, 3/4, and twice the MSE-optimal bandwidth from Calonico et al. (2014), respectively. In the eighth row, we exclude villages with bureaucrats appointed during the Suharto era. In the ninth row, we report fuzzy RD estimates of γ in equation (1). We use $\mathbb{1}(\text{margin}_{jt} > 0)$ from equation (1) to instrument for a dummy equal to 1 if the current village head in our survey sample is a different individual from the incumbent who competed in the last election. In the tenth row, we exclude villages where the current village head is serving in their third term. The dependent variables are identical to those in Table 4. The bottom panel reports the bandwidth size for the baseline estimation.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-correct standard errors in parentheses. In columns 2–3, standard errors are additionally clustered by village.

Table A.10: Bureaucratic Outcomes: Heterogeneity by Time of Appointment

	Bureaucrat survey					
	Parent Head	Parent Served	Enthusiasm	Motivation	Interacts daily w/ citizens	Freq. index
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Bureaucrats appointed before last election					
New village head	-0.051	-0.104	0.252	21.931	0.347***	0.544***
	(0.060)	(0.122)	(0.223)	(19.398)	(0.132)	(0.180)
Observations	531	531	529	527	529	529
Control mean	0.055	0.28	0.10	101.3	0.58	0.37
Robust p-value	0.237	0.427	0.175	0.218	0.00379	0.00106
MSE-opt. bandwidth	14.9	21.8	21.0	22.5	20.9	19.3
Effective obs.	214	279	269	287	269	256
	Panel B: Bureaucrats appointed after last election					
New village head	-0.103	-0.389***	0.926***	37.045	-0.051	0.139
	(0.090)	(0.162)	(0.291)	(26.698)	(0.200)	(0.425)
Observations	396	396	395	395	396	396
Control mean	0.045	0.25	-0.24	95.4	0.61	0.34
Robust p-value	0.177	0.00774	0.000777	0.115	0.860	0.599
MSE-opt. bandwidth	21.4	18.3	17.1	19.6	17.2	17.2
Effective obs.	186	172	164	179	165	165
Before vs After p	0.960	0.211	0.878	0.850	0.032	0.272

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). Units of observation are bureaucrats in all columns. The dependent variables in columns 1–2 are identical to those in columns 2–3 of Table 4. The dependent variables in columns 3–6 are identical to those in columns 1–4 of Table 3. Panel A looks at bureaucrats appointed to their current position before the last village election while panel B looks at bureaucrats appointed since the last election. The bottom row reports the p-value from the test of Calonico et al. (2025) that robust RD estimates are significantly different across the two panels. See Section 4 for details.
* p<0.1, ** p<0.05, *** p<0.01. Conventional standard errors in parentheses and robust bias-corrected standard errors clustered by village in brackets. Significance levels are based on the robust bias-corrected standard errors clustered by village.

Table A.11: Robustness Checks on Bureaucrat Alignment (Table 5)

Village Services:	**Officials/citizens agree on:**		**Complaints received about:**		**Village takes action on:**	
	Priority	Worst-quality	Priority	Worst-quality	Priority	Worst-quality
	(1)	(2)	(3)	(4)	(5)	(6)
1. Baseline	0.105	0.236**	0.162***	0.174**	0.127*	0.213**
	(0.116)	(0.117)	(0.068)	(0.079)	(0.079)	(0.103)
2. No controls	0.046	0.224**	0.192***	0.167**	0.139**	0.202*
	(0.132)	(0.120)	(0.070)	(0.083)	(0.076)	(0.122)
3. Region fixed effects	0.074	0.250**	0.159***	0.135*	0.138**	0.197**
	(0.117)	(0.119)	(0.063)	(0.079)	(0.071)	(0.110)
4. 3rd-Degree polynomial	0.129	0.337**	0.242**	0.202*	0.168	0.396**
	(0.178)	(0.165)	(0.102)	(0.115)	(0.125)	(0.180)
5. 1/2 MSE-opt. bandwidth	0.122	0.289	0.198*	0.163*	0.122	0.317***
	(0.248)	(0.248)	(0.167)	(0.106)	(0.144)	(0.183)
6. 3/4 MSE-opt. bandwidth	0.104	0.268	0.175**	0.163*	0.129	0.254**
	(0.178)	(0.186)	(0.114)	(0.097)	(0.112)	(0.149)
7. 2x MSE-opt. bandwidth	-0.005	0.099**	0.118***	0.182**	0.092**	0.140**
	(0.108)	(0.111)	(0.063)	(0.073)	(0.068)	(0.093)
8. Excl. Suharto villages	0.097	0.247**	0.147***	0.176**	0.124*	0.260***
	(0.116)	(0.122)	(0.059)	(0.081)	(0.074)	(0.111)
9. Fuzzy RD	-0.001	0.301***	0.190**	0.148	0.155*	0.321**
	(0.147)	(0.144)	(0.094)	(0.119)	(0.096)	(0.148)
10. Excl. lame-duck	0.125	0.277***	0.127**	0.189**	0.142**	0.153*
	(0.118)	(0.111)	(0.060)	(0.079)	(0.079)	(0.102)
Observations	1067	1067	1067	1067	443	443
Control mean	0.75	0.32	0.87	0.71	0.94	0.88
MSE-opt. bandwidth	17.5	17.0	18.4	25.7	22.7	26.4

Notes: This table reports robustness checks on Table 5. The first row reports our baseline estimates. In the second row, we remove election year dummies and our control for the survey experiment treatment, which are included in our baseline estimation. In the third row, we include region fixed effects and dummies for pairs of election years (2015-2016, 2017-2018, etc.). The main regions in our sample are Java, Sulawesi, Sumatra, Kalimantan, and NTB-Bali. In the fourth row, we use a 3rd-degree polynomial to construct the point estimator. In the fifth, sixth, and seventh rows, we use a RD bandwidth equal to 1/2, 3/4, and twice the MSE-optimal bandwidth from Calonico et al. (2014), respectively. In the eighth row, we exclude villages with bureaucrats appointed during the Suharto era. In the ninth row, we report fuzzy RD estimates of γ in equation (1). We use $\mathbb{1}(\text{margin}_{jt} > 0)$ from equation (1) to instrument for a dummy equal to 1 if the current village head in our survey sample is a different individual from the incumbent who competed in the last election. In the tenth row, we exclude villages where the current village head is serving in their third term. The dependent variables are identical to those in Table 5. The bottom panel reports the bandwidth size for the baseline estimation.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.12: Effects on Self-Reported Bureaucratic Knowledge

	Training	Village Law	Knowledge index
	(1)	(2)	(3)
New village head	-0.089	0.019	0.088
	(0.117)	(0.107)	(0.119)
Observations	1067	1065	1065
Control mean	0.61	0.76	0.12
Robust p-value	0.313	0.886	0.391
MSE-opt. bandwidth	19.1	17.8	28.4
Effective obs.	500	476	662

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). Units of observation are bureaucrats in all columns. The dependent variable is: in column 1, a dummy equal to 1 if the bureaucrat received any training in the past 12 months; in column 2, a dummy equal to 1 if the bureaucrat reports being informed about Village Law regulations; in column 3, a standardized index of self-reported knowledge across 5 topics: development management & accountability, financial management, village regulations, drafting development plans, and the Village Law. See Section 4 for details.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.13: Effects on Village Transfers and Budgets

	Administrative data				**Village head survey**
Village Funds:	Allocated	Utilized	% Spent	Fully Spent	Budget
	(1)	(2)	(3)	(4)	(5)
New village head	-0.367	-0.320	0.025	0.038	-0.347
	(0.625)	(0.704)	(0.062)	(0.140)	(1.205)
Observations	423	422	422	422	418
Control mean	2.04	1.91	0.87	0.58	2.47
Robust p-value	0.517	0.635	0.699	0.854	0.732
MSE-opt. bandwidth	28.0	23.7	31.6	28.6	24.7
Effective obs.	259	226	275	262	224

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). The sample includes all villages with available data. In column 1, the dependent variable is the 2022 Village Funds Allocation (ADD) in millions of Indonesian Rupiah per capita. The Village Funds Allocation includes funds allocated by district governments to fund village government salaries, benefits, and operations. In column 2, the dependent variable is the 2022 ADD amount utilized by the village for programs and activities in millions of Indonesian Rupiah per capita. In column 3, the dependent variable is the share of ADD utilized. In column 4, the dependent variable is a dummy equal to 1 if the amount allocated is fully utilized. Finally, in column 5, the dependent variable is the 2021 village budget reported by the village head in Indonesian Rupiah per capita. See Section 4 for details.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.14: Effects on Public Goods Provision: Index Components (2021 Administrative Data)

	Index	Water	Sewage	Garbage	Street Lighting	Asphalt road	Public transit	Kindergarten	Prim. Sch.	Polindes	Puskesmas
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
New village head	0.503*	0.212	0.064	0.825*	0.472**	0.059	0.328	0.194	-0.159	-0.025	0.044
	(0.263)	(0.357)	(0.392)	(0.404)	(0.256)	(0.127)	(0.331)	(0.332)	(0.304)	(0.354)	(0.289)
Observations	378	378	378	378	378	375	378	378	378	378	378
Control mean	0.23	0.15	0.14	-0.083	0.17	0.42	0.26	0.062	0.11	-0.099	-0.17
Robust p-value	0.053	0.453	0.997	0.079	0.046	0.587	0.235	0.407	0.534	0.696	0.957
MSE-opt. bandwidth	18.7	22.5	21.9	16.6	20.8	23.1	18.6	20.2	18.5	15.3	25.0
Effective obs.	161	181	177	141	173	182	160	172	160	133	196

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). In column 1, the dependent variable is a standardized index of local service provision constructed using the 2021 *Podes* survey. Remaining columns report RD estimates on the individual index components. The index has the following 10 components: drinking water, sewage, garbage collection, street lighting, paved roads, public transit, kindergartens, primary schools, village maternities (*polindes*), and community health centers (*puskesmas*). We first standardize each individual component before taking the village-level average of all components. The sample includes all villages in our sample that conducted their last election before 2021.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.15: Public Goods Provision, Heterogeneity by Nepotistic Networks

	Public Goods Index			
Villages where...	Village head has no relative in govt	Village head has at least 1 relative in govt	No bureaucrat w/ parent in govt	At least 1 bureaucrat w/ parent in govt
	(1)	(2)	(3)	(4)
New village head	0.773**	-0.069	0.603**	0.484
	(0.359)	(0.388)	(0.282)	(0.526)
Observations	191	133	295	81
Control mean	0.17	0.36	0.17	0.40
Robust p-value	0.020	0.683	0.022	0.309
MSE-opt. bandwidth	20.6	16.1	19.2	23.8
Effective obs.	86	47	129	38
Not nepotistic vs Nepotistic *p*		0.022		0.287

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). The dependent variable is a standardized index of local public service provision constructed using the 2021 Podes data, as in column 1 of Table 7. The sample includes: in column 1, all villages in which the village head reports having no relative in the village government; in column 2, all villages in which the village head reports having at least one relative in the village government; in column 3, all villages in which no bureaucrat who was appointed before the most recent election reports a parent previously served as a village official; and in column 4, all villages in which at least one bureaucrat who was appointed before the most recent election reports a parent previously served as a village official. See Section 5 for details.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.16: Effects on Old-Serving Nepotistic Appointees

	Proportion	Binary (=1 if any)
	(1)	(2)
New village head	-0.036	-0.017
	(0.068)	(0.130)
Observations	510	510
Control mean	0.15	0.25
Robust p-value	0.825	0.933
MSE-opt. bandwidth	22.9	21.3
Effective obs.	263	252

Notes: This table reports RD estimates of γ in equation (1). In column 1, the dependent variable is the share of bureaucrats who were appointed before the most recent election and report that a family member previously served as a village official. In column 2, the dependent variable is a dummy equal to one if at least one such bureaucrat is present in a village. See Section 5 for details.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.17: Dynamic Effects on Public Goods Provision

	2015-2020	2015-2017	2018-2020	2021-22 (placebo)
	(1)	(2)	(3)	(4)
New village head	0.503*	0.902*	0.440	-0.364
	(0.263)	(0.529)	(0.340)	(0.549)
Observations	378	122	256	134
Control mean	0.23	0.33	0.15	0.45
Robust p-value	0.053	0.063	0.165	0.615
MSE-opt. bandwidth	18.7	18.4	19.0	22.2
Effective obs.	161	52	109	80

Notes: This table reports RD estimates of γ in equation (1). The dependent variable is the index of local public service provision constructed using the 2021 Podes data. We restrict the sample to villages that conducted their most recent election between 2015-2020 (column 1); between 2015 and 2017 (column 2) or between 2018 and 2020 (column 3). In column 4, we restrict the sample to villages that conducted their most recent election in 2021 or 2022, namely after data collection for the 2021 Podes survey. Thus, these regressions can be interpreted as placebo checks.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.18: Effects on Citizens' Perceptions of Service Provision: Index Components

	Index		Water		Garbage		Roads		Electricity		Kindergarten		Schools		Health	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
New village head	0.073**	0.208**	0.035	0.057	0.208**	0.364**	0.149**	0.351**	0.025	0.164	0.003	0.047	0.009	0.037	0.030	0.157
	(0.043)	(0.102)	(0.102)	(0.211)	(0.097)	(0.170)	(0.077)	(0.186)	(0.023)	(0.136)	(0.078)	(0.145)	(0.080)	(0.182)	(0.047)	(0.150)
Observations	8848	8846	8797	8771	8783	8817	8842	8836	8839	8837	8828	8741	8834	8794	8833	8798
Control mean	0.78	-0.028	0.72	0.027	0.37	-0.16	0.91	0.073	0.99	-0.058	0.79	-0.016	0.76	-0.043	0.93	-0.023
Robust p-value	0.039	0.014	0.624	0.630	0.017	0.022	0.017	0.021	0.231	0.117	0.917	0.630	0.863	0.730	0.347	0.163
MSE-opt. bandwidth	15.2	14.9	17.7	16.0	23.0	26.5	12.4	13.3	28.8	23.9	21.2	29.0	22.9	20.7	21.0	18.7
Effective obs.	3479	3427	3897	3533	4575	5140	2898	3165	5536	4801	4403	5465	4579	4309	4349	4057

Notes: This table reports RD estimates on the indices of service access and quality used in Table 7, columns 2 and 3, and individual components of the indices. Odd-numbered columns report effects on perceived access and even-numbered columns report effects on perceived quality.
p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.19: Effects on Village Taking Action on Public Goods: Index Components

	Index	Water	Garbage	Roads	Electricity	Kindergarten	Schools	Health
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New village head	0.165**	0.213	0.543***	-0.010	0.123	0.135	0.142	0.126
	(0.086)	(0.134)	(0.149)	(0.142)	(0.156)	(0.135)	(0.166)	(0.089)
Observations	443	443	443	443	443	443	443	443
Control mean	0.76	0.69	0.72	0.75	0.66	0.85	0.66	0.91
Robust p-value	0.046	0.115	0.000	0.819	0.523	0.275	0.495	0.118
MSE-opt. bandwidth	24.9	23.7	14.8	24.9	22.2	19.7	21.1	26.2
Effective obs.	241	235	167	241	221	207	216	250

Notes: This table reports RD estimates on the take action index used in Table 7, column 4, and individual components of the index.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.20: Citizen Demographics, by Social Distance to Village Government

	Female	Age	Disability	Tertiary Educ.	Not Working	Monthly Income
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First-Degree Connection						
Sample Mean	0.388	38.9	0.022	0.241	0.184	2572
Std. dev.	[0.487]	[11.4]	[0.146]	[0.428]	[0.388]	[3504]
Observations	2656	2637	2655	2647	2651	2146
Panel B: Second-Degree Connection						
Sample Mean	0.466	35.7	0.022	0.191	0.257	2405
Std. dev.	[0.499]	[10.9]	[0.147]	[0.393]	[0.437]	[3293]
Observations	2216	2198	2213	2207	2208	1629
Panel C: Third-Degree or More Connection						
Sample Mean	0.532	32.3	0.028	0.158	0.304	2220
Std. dev.	[0.499]	[10.5]	[0.165]	[0.365]	[0.460]	[3096]
Observations	4008	3981	4005	3996	3997	2761

Notes: This table reports summary statistics of citizen demographics by degree of connection to the village government. The dependent variable is: in column 1, a dummy equal to 1 for female citizens; in column 2, the age of citizens in years; in column 3, a dummy for whether the citizen reports having a disability; in column 4, a dummy for whether the citizen has a tertiary level education; in column 5, a dummy for whether the citizen reports not working; in column 6, the citizen's average monthly income in the last 6 months (conditional on working) winsorized at the top 1% and reported in thousands of Indonesian rupiah.

Table A.21: Alignment with Citizens' Preferences, by Social Distance to Village Government

	Bureaucrat survey				Village head survey		Citizen survey	
	Officials/citizens agree on:		Complaints received about:		Village head takes action on:		Citizen Perceptions	
Village Services:	Priority	Worst-quality	Priority	Worst-quality	Priority	Worst-quality	Access	Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: First-Degree Connection								
New village head	0.113	0.288***	0.192***	0.074	0.098	0.212**	0.046	0.130
	(0.110)	(0.114)	(0.063)	(0.084)	(0.071)	(0.112)	(0.040)	(0.109)
Observations	1067	1067	1067	1067	443	443	2647	2646
Control mean	0.72	0.40	0.86	0.77	0.96	0.86	0.76	-0.071
Panel B: Second-Degree Connection								
New village head	0.167*	0.242**	0.117**	0.147**	0.098	0.111	0.055	0.143
	(0.109)	(0.138)	(0.067)	(0.076)	(0.071)	(0.086)	(0.048)	(0.117)
Observations	1067	1067	1067	1067	443	443	2207	2207
Control mean	0.70	0.42	0.86	0.75	0.96	0.89	0.79	-0.018
Panel C: Third-Degree or More Connection								
New village head	0.173*	0.244**	0.069	0.139*	0.121**	0.194**	0.100**	0.276***
	(0.119)	(0.139)	(0.060)	(0.072)	(0.067)	(0.107)	(0.053)	(0.122)
Observations	1046	1046	1046	1046	436	436	3994	3993
Control mean	0.69	0.37	0.87	0.75	0.96	0.90	0.80	-0.022

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). Units of observation are bureaucrats in columns 1–4, village heads in columns 5–6, and citizens in columns 7–8. In columns 1 through 6, the outcomes are identical to those in columns 1–6 of Table 5. In columns 7–8, the outcomes are identical to those in columns 2–3 of Table 7. Each panel looks at a different subsample of citizens with varying degrees of social proximity to the village government, namely: citizens with first-degree (direct) connections in Panel A, second-degree connections in Panel B, and third- (or higher) degree connections in Panel C. See Section 5 for details.

* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.22: Robustness Checks on Public Goods Provision (Table 7)

	Public Goods Index	**Citizen survey**		**Village head survey**
	Podes 2021	Access	Quality	Take Action
	(1)	(2)	(3)	(4)
1. Baseline	0.503*	0.073**	0.208**	0.165**
	(0.263)	(0.043)	(0.102)	(0.086)
2. No controls	0.682***	0.061*	0.202**	0.126
	(0.276)	(0.039)	(0.098)	(0.098)
3. Region fixed effects	0.452**	0.054*	0.172**	0.117
	(0.238)	(0.040)	(0.098)	(0.087)
4. 3rd-degree polynomial	0.282	0.111*	0.271**	0.150
	(0.407)	(0.064)	(0.125)	(0.140)
5. 1/2 MSE-opt. bandwidth	0.293	0.082	0.202	0.158
	(0.477)	(0.085)	(0.189)	(0.157)
6. 3/4 MSE-opt. bandwidth	0.442	0.077*	0.212**	0.157
	(0.364)	(0.063)	(0.143)	(0.126)
7. 2x MSE-opt. bandwidth	0.458**	0.036*	0.097**	0.124**
	(0.235)	(0.039)	(0.095)	(0.075)
8. Excl. Suharto villages	0.511*	0.067*	0.189**	0.171**
	(0.272)	(0.043)	(0.100)	(0.089)
9. Fuzzy RD	0.688**	0.110**	0.308***	0.191**
	(0.320)	(0.050)	(0.124)	(0.097)
10. Excl. lame-duck	0.498*	0.055	0.175*	0.176**
	(0.272)	(0.044)	(0.107)	(0.092)
Observations	378	8848	8846	443
Control mean	0.23	0.78	-0.028	0.76
MSE-opt. bandwidth	18.7	15.2	14.9	24.9

Notes: This table reports robustness checks on Table 7. The first row reports our baseline estimates. In the second row, we remove election year dummies and our control for the survey experiment treatment, which are included in our baseline estimation. In the third row, we include region fixed effects and dummies for pairs of election years (2015-2016, 2017-2018, etc.). The main regions in our sample are Java, Sulawesi, Sumatra, Kalimantan, and NTB-Bali. In the fourth row, we use a 3rd-degree polynomial to construct the point estimator. In the fifth, sixth, and seventh rows, we use a RD bandwidth equal to 1/2, 3/4, and twice the MSE-optimal bandwidth from Calonico et al. (2014), respectively. In the eighth row, we exclude villages with bureaucrats appointed during the Suharto era. In the ninth row, we report fuzzy RD estimates of γ in equation (1). We use $\mathbb{1}(\text{margin}_{jt} > 0)$ from equation (1) to instrument for a dummy equal to 1 if the current village head in our survey sample is a different individual from the incumbent who competed in the last election. In the tenth row, we exclude villages where the current village head is serving in their third term. The bottom panel reports the bandwidth size for the baseline estimation. The dependent variables are identical to those examined in columns 2–4 of Table 7. * p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses. In columns 1–2, standard errors are additionally clustered by village.

Table A.23: Citizen Attitudes and Trust Towards the Village Government

	Citizens survey		
	Interactions with govt	Perceived govt quality	Trust in govt
	(1)	(2)	(3)
Panel A: Full Sample			
New village head	0.177	-0.018	0.027
	(0.159)	(0.142)	(0.126)
Observations	8815	8790	8789
Control mean	-0.12	-0.034	-0.029
Robust p-value	0.223	0.949	0.724
MSE-opt. bandwidth	17.1	17.7	17.5
Effective obs.	3752	3892	3812
Panel B: First-Degree Connection			
New village head	0.294*	-0.034	0.169
	(0.179)	(0.161)	(0.160)
Observations	2638	2631	2630
Control mean	0.21	0.23	0.23
Panel C: Second-Degree Connection			
New village head	0.398*	-0.105	-0.128
	(0.218)	(0.179)	(0.169)
Observations	2201	2196	2196
Control mean	-0.098	-0.012	-0.035
Panel D: Third-Degree or More Connection			
New village head	-0.016	0.033	-0.000
	(0.179)	(0.171)	(0.151)
Observations	3976	3963	3963
Control mean	-0.24	-0.14	-0.11

Notes: This table reports RD estimates of γ in equation (1) obtained via the non-parametric method from Calonico et al. (2014). The dependent variable is: in column 1, a z-score of the frequency of interactions with village officials, as reported by citizens; in column 2, a z-score of self-reported satisfaction with the village government; in column 3, a z-score of self-reported trust in the village government. Panel A reports results for the full sample of citizens. Panels B through D report estimates from subsamples of citizens at varying levels of social distance from the village government, analogous to Table A.21. See Section 5 for details.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses.

Table A.24: Effects on Village Head Characteristics

	Parent head	Parent served	Age	Male	Educ	Islam	Bahasa
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New village head	0.071	0.005	-2.124	-0.028	-0.182	-0.188*	0.100
	(0.096)	(0.113)	(2.969)	(0.059)	(0.582)	(0.135)	(0.117)
Observations	443	443	443	443	443	443	443
Control mean	0.10	0.24	49.9	0.95	13.1	0.86	0.17
Robust p-value	0.371	0.909	0.641	0.526	0.700	0.082	0.308
MSE-opt. bandwidth	24.5	28.3	16.0	22.0	28.2	15.9	20.8
Effective obs.	241	267	176	220	266	176	214

Notes: This table reports RD estimates of γ in equation (1). The sample includes all village heads. The dependent variable is: in column 1, a dummy equal to one if the village head's parent was also village head; in column 2, a dummy equal to one if the village head's parent served in the village government; in column 3, the age of village heads in years; in column 4, a dummy equal to one if the village head is male; in column 5, years of education; in column 6, a dummy equal to one if the village head's religion is Islam; in column 7, a dummy equal to one if the village head speaks Bahasa as the primary language.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.25: Turnover and Natural Disasters: Correlations (OLS)

	New Leader	Diff. Vote Share
	(1)	(2)
Any disaster	0.141***	0.065**
	(0.044)	(0.031)
Sample mean	0.47	0.47
Observations	512	512

Notes: This table reports OLS estimates of each outcome on an indicator variable for whether a village experienced a natural disaster (a major landslide, flood event, ocean tide, hurricane, or drought) during an incumbent's term prior to the election. The dependent variable is: in column 1, an indicator for whether a new leader was elected in the most recent election; and in column 2, the difference between the vote share received by the highest-ranking challenger candidate and the incumbent's vote share, which is the running variable in equation (1). See Section 6 for details.
* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors in parentheses.

Table A.26: Effect of Turnovers in Villages Impacted by Natural Disasters

	Bureaucrats				Alignment		Public Goods		
	Parent Head	Parent Served	Enthusiasm	Freq Interact	Agree	Receive Complaint	*Podes* 2021	Access	Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
New village head	-0.092*	-0.163	0.357*	0.393	0.105	0.201**	0.704**	0.116*	0.266**
	(0.069)	(0.111)	(0.244)	(0.279)	(0.194)	(0.094)	(0.392)	(0.079)	(0.160)
Observations	510	510	508	507	510	510	167	4336	4334
Control mean	0.052	0.28	0.079	0.37	0.72	0.90	0.36	0.81	0.019
Robust p-value	0.096	0.109	0.078	0.112	0.588	0.011	0.047	0.061	0.037
MSE-opt. bandwidth	14.4	17.9	21.8	21.1	17.0	15.8	17.3	12.3	13.7
Effective obs.	210	243	274	269	238	218	67	1489	1726

Notes: This table reports RD estimates of γ in equation (1). The sample is restricted to villages that experienced a natural disaster (a major landslide, flood event, ocean tide, hurricane, or drought) during an incumbent's term prior to the election. The dependent variables are: in columns 1 and 2, the same as those in Table 4; in columns 3 and 4, the same as those in Table 3; in columns 5 and 6, the same as those in Table 5; in columns 7-9, the same as those in Table 7. See Section 6 for details.

* p<0.1, ** p<0.05, *** p<0.01. Robust bias-corrected standard errors clustered by village in parentheses in columns 1-6 and 8-9. Robust bias-corrected standard errors in parentheses in column 7.

B Data Appendix: Details on Survey Design

We conducted a survey of village officials and citizens in Indonesia between March and August 2022, in partnership with the Indonesian Ministry of Home Affairs (MoHA) and the World Bank. The survey took place in 852 villages spread across 23 districts in 17 provinces. The primary targets were active village officials as well as 8 to 12 adult citizens residing in the same villages. The survey aimed to gain a better understanding of village governance and to provide a new window into the level of village development as perceived by both officials and citizens. As a result of the restrictions associated with the Covid-19 pandemic, we conducted all surveys over the phone. Below, we describe the sampling procedures we used to select villages, village officials, and citizens.

B.1 Sampling of villages

We constructed a large representative sample of villages spanning each of Indonesia's major islands. Since the survey was designed as the baseline of a future digital training intervention, this sample was restricted to districts with relatively high internet coverage. We first randomly selected districts after stratifying by region, and then randomly selected a fixed proportion of villages within each district.

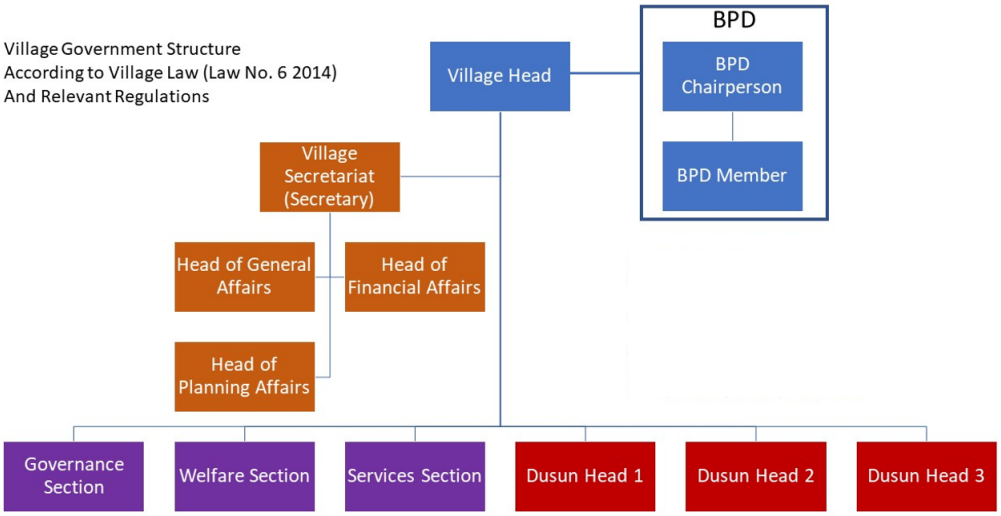
Our initial goal was to recruit a sample of 1,000 villages from a set of eligible villages in 20 districts. Given surveys were conducted over the phone, we expected a low consent rate. We thus sampled from a pool of around 1,700 villages across 20 districts and later added another 3 districts in order to reach a final target sample of 1,000 villages. Among these, we were able to administer the survey in 852 villages spread across the islands of Sumatra, Java, Bali and Nusa Tenggara (NT), Kalimantan, and Sulawesi.

Contact details for village heads and BPD chairpersons were obtained directly from MoHA. We started data collection by conducting a listing process to verify these phone numbers, obtaining village heads' consent. If a village was successfully listed, the survey team would proceed to interviews of village officials. We then marked the village as a "completed listing" once it had been confirmed that the village head's phone number could be called, and they had consented to be interviewed. This listing process resulted in a total of 865 villages in the final sample, consisting of 856 completed listing villages, 8 partially completed listing villages, and 1 incomplete listing village. Of these 865 villages, 852 villages were marked as "completed interviews", meaning we successfully completed the target number of interviews with village officials and citizens.

B.2 Sampling of village officials

In each village, we aimed to conduct interviews with the village head (*kepala desa*), the village secretary (*sekretaris desa*), the BPD chairperson (*ketua BPD*), one randomly selected member of the village bureaucracy, one randomly selected neighborhood/hamlet head (*kepala dusun*), and one randomly selected BPD member (*anggota BPD*). Phone numbers of village officials were obtained from the village heads themselves, or alternatively from the BPD chairperson if the village head could not be reached. Our sample size reached a total of 744 village heads, 864 BPD chairpersons, and 3,606 other village officials, including 1,793 village bureaucrats (members of the village secretariat).

Figure B.1: Composition of Village Governments



B.3 Sampling of citizens

We sampled citizens using a snowball procedure in which respondents were asked to provide three contact persons whose names began with a randomly drawn letter of the alphabet. This procedure started with the village heads and BPD chairpersons and continued with citizen respondents until we reached the target sample size (8 to 12 citizens) in each village. The random selection of a letter of the alphabet was designed to impose some constraints on the selection of potential respondents by the village officials. The figure below provides the corresponding section of our questionnaire. This process allowed us to interview 14,378 citizens across the 852 villages in our sample.

Figure B.2: Sampling of Citizens

H. PHONE NUMBER COLLECTION

ENUMERATOR: PLEASE REPEAT THE FOLLOWING PROCESS **TWO TIMES.** ONLY ASK FOR THE PHONE NUMBER OF PEOPLE AGED 18 YEARS OR OLDER.

1. I am going to tell you a letter: [XXX randomized according to census prevalence XXX]. Now, please look into the contact list on your phone.

D1a.	Is there anyone **living in your village** whose name starts with [XXX]?	1. Yes, name: _____ 3. No ➔ PROCEED TO NEXT RANDOMIZED LETTER 7. REFUSE TO ANSWER ➔ PROCEED TO NEXT RANDOMIZED LETTER
D1b.	Are you willing to share the contact number of this person?	1. Yes 3. No ➔ D2a
D1c.	What is the phone number of this person?	|_|_|_|_| - |_|_|_|_|_|_|_|_|