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ACCOUNTABILITY, POLITICAL CAPTURE AND SELECTION INTO POLITICS: EVIDENCE FROM PERUVIAN MUNICIPALITIES

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Abstract

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JEL Classification: O10, D72, O53, D71

Keywords: accountability, Selection into Politics, Peru

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Accountability, Political Capture and Selection into Politics: Evidence from Peruvian Municipalities*

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1 Introduction

In most democratic systems, different mechanisms and institutions can be used to increase voters' control over politicians', e.g., re-election incentives, free press, impeachment and recall mechanisms, etc. The objective of these institutions is to improve government quality and public good provision by both disciplining elected politicians and/or punishing the inefficient or corrupt ones (Persson and Tabellini (2000), Barro (1973) and Ferejohn (1986)). These mechanisms not only have effects over politicians' actions, but also on their selection: by holding them accountable, they affect the expected value of office (see eg. Besley (2007)). However, in countries with low state capacity, where accountability institutions are at risk of being captured or manipulated by political elites or special interest groups, these objectives can be distorted. For example, if the press is controlled by economic elites, it can highlight information that punishes efficient and/or honest politicians, which could discourage potentially good candidates to run for office, but who might have otherwise considered the post in the future.

In this paper, we study how accountability institutions affect the type of politicians who decide to run for office, and show the way in which these institutions can lead to a negative selection when they are misused by a group with political interests by legal channels, i.e. when they are prone to political capture. Using a comprehensive dataset on the characteristics and background of candidates running for district mayor in the last three rounds of municipal elections in Peru, we implement a close election regression discontinuity design, comparing the characteristics of candidates who decide to run for mayor in districts where the incumbent lost a recall referendum by a small margin with those running in places where the mayor barely survived the recall. Having a mayor recalled from office in a municipality affects the expected value of office for potential candidates by updating their beliefs about the probability that they are recalled for political purposes, i.e. irrespective of their performance in office (a similar mechanism as in e.g. Avis, Ferraz, and Finan (2018)). In this framework, high ability politicians and those who derive a high level of utility from providing public goods (especially to their coethnics) refrain from running, lowering the quality of the pool of candidates in the next regular election cycle and negatively affecting representation.

Our empirical results show that having a mayor ousted through a referendum in the previous period causes a reduction in the quality of candidates running for mayor in the current term: candidates in treated municipalities have about half a year less education, are 22 percent less likely to be university educated, and instead the proportion of candidates with only secondary education is higher. Looking at other dimensions that in our setting correlate with politicians' quality and policy making, we find that candidates in municipalities where a mayor was recalled also have less experience in elected office, and in particular, have 0.4 less years serving as district mayor, have less experience working in the public sector, and

are less likely to have served in a party office. Importantly, these candidates are not less likely to belong to a national political party. Finally, these candidates are slightly younger. These characteristics are negatively correlated with policy outcomes in our setting, suggesting that overall high ability candidates are selecting out of the race, and instead lower quality candidates are entering politics. Additionally, we show that candidates in districts that had the mayor ousted from office are not less likely to have a Quechua or Aymara background (as measured by a novel classification of surnames), but are less representative of indigenous populations. Finally, we show that despite the negative selection of candidates, elections still play an important role, and the negative effects on the pool of candidates are mostly offset by voters, who select the most qualified politicians among a lower quality pool. However, relevant policy outcomes are still negatively affected by the lower quality of candidates.

Recall referenda are a direct democracy institution that allows voters to hold politicians accountable outside the regular election terms. This accountability mechanism is used around the world in countries as diverse as Uganda, Colombia, Poland, the US, and Ecuador (Serdült and Welp (2012)). In Peru, recall referenda are widely used at the local level (Welp (2015)). For example, in the 2010 electoral period, 20 percent of mayors in the country faced a recall election, of which one fourth were ousted from office. Importantly, it has been documented that recall referenda are often used as a political tool, both around the world (Altman (2010); McCoy (2006); Welp and Milanese (2018)) and in our study setting (Welp (2015); Holland and Incio (2019); Soldevilla (2014)). For example, candidates who lost a previous election are often promoters of recall processes, an indication that this institution is used as a tool to undermine the incumbent. Additionally, the main predictors of the presence of a recall election in our setting are political variables, as the closeness of the elections, instead of other variables related to the incumbent's quality.

We present a simple framework where, in general, an increase in accountability allows voters to punish low quality and corrupt politicians, therefore reducing their expected term length and generating a positive selection. However, the political use of the accountability mechanism generates that high valence and policy-motivated candidates may also be punished by voters, regardless of their performance, deterring some of them from running for office, and generating a negative selection. When accountability institutions are at risk of being captured, as is often the case in countries with low state capacity, well-intended institutions can backfire. In the analysis, we show evidence consistent with the hypothesized mechanisms. For instance, we show suggestive evidence that the negative selection of candidates is almost entirely driven by districts where the main promoter of the recall referendum is a politician who ran for office in the previous election. On the other hand, this selection is unaffected by the previous mayor's performance, as measured by the percent of the budget executed, the expenditures or revenues collected during her tenure. These findings bolster the idea that the negative selection is mainly driven by politically motivated recalls. We provide additional evidence

that candidates who decide to run for office after a mayor is recalled have a lower opportunity cost, as measured by their predicted wages in the private sector. Our results are not driven by pre-existing differences in the characteristics of the incumbents or their opponents, time-variant characteristics of the political situation of the district at the moment of the recall referendum, or related to the absence of an incumbent mayor in municipalities where she was recalled.

Importantly, our results rely on the assumption that potential candidates are uninformed about the prevalence of politically motivated recalls and update their beliefs when they observe a mayor being recalled in their municipality. In Peru, more than 90 percent of recall elections take place in municipalities with less than 5,000 voters (Welp (2015)), which are hardly ever covered by national media. Moreover, once a recall took place in a major city in 2012, regulations were introduced to reduce the benefits of political opposers to gain power in the municipality after a recall, which speaks about the low information environment, even for policymakers and experts. We empirically provide evidence consistent with this information transmission mechanism. We analyze the patterns of candidate selection in municipalities that did not have a recall election, but had a close neighbor (within 2 hrs of travel time) where a close recall election took place, and compare districts in which the neighbor had the mayor ousted with those where the neighbor's mayor survived the recall. The results are entirely consistent with our main hypotheses.

Finally, in our empirical analysis, we investigate whether having a lower quality pool of candidates leads to lower quality elected mayors. Our findings, though suggestive due to a reduced sample size, show that elections mostly offset the negative effect of recalls on the candidate pool, and elected mayors in treated areas are only slightly less educated (not significant) than those who win the election in districts where a mayor barely survived the recall referendum. Even though there are no major differences in the mayors' characteristics, we observe that municipalities that had the incumbent recalled in the previous period spend less and collect less revenues, which could be explained by lower quality of the opposition, less oversight, etc.

Peruvian municipalities provide an ideal setting for studying the effects of accountability institutions on candidate selection. First, unlike other contexts where information on the characteristics of politicians is only released for the ones who get elected, the national electoral office collects and publishes detailed data on all candidates running for any public office, from the presidency down to the municipal council. These data allow us to look not only at the effects on the number of candidates and political competition, as previous studies, but importantly to who decides to run for office and who is deterred, emphasizing those characteristics that are likely to cause better performance in office, as education and previous experience in the public and private sector (Besley, Montalvo, and Reynal-Querol (2011), Besley, Pande, and Rao (2005), Martinez-Bravo (2017)). Second, it is not often the case that

one can observe variation in accountability (and the missuse of these institutions) at the local level, and when one does, it is not easy to disentangle between observed or unobserved factors that determine the level of accountability and other outcomes that one wants to study.[1] In our setting, close results in recall elections allow us to identify the effect of being exposed to (and learning from) an accountability institution that can be used for political purposes, and therefore causes a shorter expected term in office that is unrelated to performance or individual characteristics. Finally, recall referenda in Peru are at risk of being captured by political interest groups, a claim supported by anecdotal and statistical evidence as well as previous work documenting it, allowing us to shed light on the mechanisms that explain why accountability can lead to a negative selection of candidates.

Our work contributes to and bridges the literatures studying the effects of voter control mechanisms and the one analyzing the motivations and selection of politicians. First, we contribute to the literature looking at the broad question of politicians' motivations and selection. In an early paper, Diermeier, Keane, and Merlo (2005) estimate a model of the behavior of members of the US Congress, and simulate the effects of imposing term limits. They find that term limits substantially increase early voluntary exit from the House. Dal Bó et al. (2017) document several stages of the selection of politicians using extremely detailed and rich data from Sweden. Their findings demonstrate that politicians are on average smarter than the rest of the population, but are representative in terms of their social background, and that material and intrinsic motives matter for selection. In this paper, we show empirically a specific mechanism that affects the selection of politicians, which sheds light on their motivation for running for office.

Second, a large body of theoretical literature shows that increases in accountability allow voters to discipline politicians, for instance in the form of reelection incentives, term limits, information availability etc. Barro (1973) and Ferejohn (1986) in their seminal work highlighted the relevance of the information asymmetry between voters and politicians.[2] Most political agency models predict that these information asymmetries have effects on the incidence of both moral hazard and adverse selection, however the empirical work analyzing the effects of accountability institutions have focused on the former.[3] In a recent paper, Avis, Ferraz,

[1] Notable exceptions of random variation in accountability can be found in Ferraz and Finan (2008), Ferraz and Finan (2011), Avis, Ferraz, and Finan (2018) and Litschig and Zamboni (2016).

[2] Besley (2007) and Persson and Tabellini (2000) provide great reviews of these models.

[3] Besley and Case (1995) and Ferraz and Finan (2011) estimate the impact of term limits on the performance of governors and mayors in the US and Brazil, respectively, providing empirical evidence that lame ducks are more likely to have worse performance in office and higher incidence of corruption. List and Sturm. (2006) study term limits in US elections and find that they affect the expected quality of incumbents and environmental policy. Ferraz and Finan (2008) investigate how the release of information on corruption audits affects the reelection prospects of politicians in Brazil, Besley and Burgess (2002) study the effects of press availability on government responses in India, while Bobonis, Fuertes, and Schwabe (2016) look at the impact of timely corruption monitoring on corruption levels. Finally, Casey (2015) analyzes the effects of information availability on redistributive politics.

and Finan (2018) show that Brazilian mayors exposed to a random audit are less likely to engage in corruption or miss-management. As the model in this paper shows, despite the fact that audits are independent draws, and being audited should not affect the probability of this happening again, mayors are miss-informed and when an audit takes place, they update their priors. The main hypothesis presented in our paper is in a similar vein, and assumes that mayors do not have perfect information about the probability of having a politically motivated recall, and update their believes when they observe a mayor being recalled in their municipality. In this paper, we provide empirical evidence consistent with this mechanism.[4]

The selection of politicians who decide to run for office is as important as their behavior, since their honesty, competence and motivation determine the quality of public policies implemented, either directly (Martinez-Bravo (2017), Besley, Montalvo, and Reynal-Querol (2011), Besley, Pande, and Rao (2005)) or through its effects on political competition and more generally on the political equilibrium (Besley (2007), Acemoglu, Egorov, and Sonin (2013), Besley, Persson, and Strum (2010)). Few empirical papers so far have looked at the effects of accountability institutions on the selection of candidates. Alt, Bueno de Mesquita, and Rose (2011) empirically disentangle the effort and selection effects of term limits for state governors in the US, finding that part of the disappearance of the effect of term limits on gubernatorial performance over time is explained by low performing politicians failing to get reelected or choosing not to run. Fisman, Schulz, and Vig (2017) look at the effects of financial disclosure laws on the selection of candidates in India. Using the staggered implementation of disclosure laws, they find that potentially corrupt politicians self-select out of the electoral race. Cavalcanti et al. (2018) analyze the effects of the disclosure of information about corruption in Brazil on the selection of politicians, demonstrating that parties play a large role in selecting candidates based on the information that is known about them. In our analysis, we directly observe the characteristics of all candidates running for the mayor seat. In examining newspaper entry in Italian municipalities, Drago, Nannicini, and Sobbrio (2014) do not find an effect on political selection, while Gamalerio (2019) shows that the introduction of fiscal rules lowered the level of education of Italian candidates in local elections. Unlike these papers, we analyze an accountability institution that is used for political purposes, which distorts its objectives and hence generates negative selection.[5]

More closely related to the predictions of agency models, as well as highlighting the impor-

[4]Our results are also consistent with a story where the salience of politically motivated recalls increases with a recent recall of a mayor in the district, which in turn affects potential candidates' perceived probability of being ousted from office due to political grievances. Recent events have been shown to affect the perceived probability of the event happening again in the near future. For example, flood insurance sales spike right after a hurricane, or air ticket sales decrease after a plane accident.

[5]Malesky, Schuler, and Tran (2012) use an experiment to explore the effects of legislative transparency on the performance of Vietnamese parliamentarians. They find that, unlike in a democratic setting, co-optation and limited power sharing in an authoritarian regime, which would normally increase accountability, can have negative consequences in terms of curtailed participation.

tance of considering endogenous selection into politics (in the spirit of the citizen candidate models, e.g. Osborne and Silvinski (1996) and Besley and Coate (1997)), a group of papers analyze the effects of monetary incentives on politicians' selection and performance. Ferraz and Finan (2016) and Gagliarducci and Nannicini (2013) use detailed data on the characteristics of candidates running in local elections, and exploit regression discontinuity designs to identify the effects of politicians' salaries on their selection and their actions once in office in Brazil and Italy, respectively. Both papers find that higher wages generate a positive selection of politicians. Further, they manage to separate the incentive and selection effects, and document better performances of politicians who receive higher wages.[6] Brollo et al. (2013) study how additional resource revenues from natural resources affect political corruption and the quality of politicians. Their findings show that larger transfers increase corruption and reduce the average education of candidates for mayor. Beath et al. (2016) use a randomized control trial in Afghanistan to look at the effects of different electoral processes on the selection of politicians. They show formally and empirically that representatives elected in elections with a single multi-member district have higher educational levels and less extreme policy views.[7] Similar to these papers, we use detailed data on candidates to analyze the effects of a treatment that affects the expected value of office. In the context we analyze, potential candidates exposed to a successful recall of the local mayor in the previous electoral period have a lower expected length of term. To some extent, our results also complement those from Dal Bó and Rossi (2011), who show the effects of different length in office on the performance of legislators in Argentina, holding selection constant. We add to this literature by documenting that institutions intended to increase citizen control of politicians can have negative consequences when they are likely to be captured by specific interest groups.

2 Institutional Background

2.1 Local Governments in Perú

Municipalities (districts) are the lowest administrative level in Peru. The highly decentralized structure of the country gives a significant amount of decision power to municipalities, which execute a large share of the national budget, and are in charge of basic public good provision, e.g. street pavement, local security, trash collection, street cleaning, etc. Since 2002, municipalities have recorded a five-fold increase in their budgets, now accounting for more than

[6] Pique (2017) also evaluates the impact of mayors' salaries, and using the same data sources as our paper, documents that higher wages do not affect the selection of politicians, but have a robust negative effect on public investment performance.

[7] Galasso and Nannicini (2011), Galasso and Nannicini (2017), Galasso and Nannicini (2015) analyze the effects of electoral rules on the selection of politicians, emphasizing the role played by political parties in this selection process. Also related, the model in Caselli and Morelli (2004) explains the persistence of bad politicians in office.

20 percent of the national budget and around 45 percent of Peru's total public investment budget (Pique (2017)).

District mayors and their councilors are elected for four year terms with the option of reelection (reelection was banned in 2015, at the end of our analysis period). The mayor is elected by a first past the post system in which the mayor's party automatically gets a majority of seats in the council, with the rest of them being assigned to other political parties, proportional to their vote shares. Mayors are full time employees that receive a wage, while councilors are paid by the number of hours they serve in the council.

The political landscape at the local level is extremely fragmented, with a significant number of candidates running for independent parties, which have few links outside the district and are often seen as an election vehicles centered around the candidate, rather than an ideology or political program (see eg. Bland and Chirinos (2014)). For example, in the 2014 municipal elections, the average district had 7.26 candidates running for office, and only 36.9 percent of them represented a national political party.

2.2 Recall Elections

Peruvian citizens have the right to recall any local elected official (mayors, councilors, and regional presidents, but not MPs or the president). The introduction of this direct democracy mechanism in the 1993 Constitution followed a set of similar democratizing reforms in other Andean countries (Colombia, Ecuador, Bolivia) and emulated ones already existing elsewhere in the world (eg. US, Poland, Uganda, among others). The main objective of this institution is to hold politicians accountable on a constant basis, rather than only in regular election times. Detractors argue that direct democracy mechanisms embedded in a representative democracy undermine governance, keeping incumbents occupied in constant campaigning to avoid being recalled, and provide incentives to invest in projects with a shorter time scope, rather than larger reforms with a longer time horizon.

A recall referendum can be called in the second or third year of the mayor's term. To initiate a recall procedure, the promoter has to (i) buy a "recall kit," which includes the official forms to collect signatures from supporters,[8] (ii) name the authorities subject to the recall and provide a reason for recalling officials,[9] and (iii) collect valid signatures of 25 percent of

[8]The representative of the recall petition has to be registered in the district where she wants to recall an incumbent and must have no outstanding fines. The cost of purchasing a recall kit is relatively low, at about US$30.

[9]Multiple names can be included in the petition, e.g. the mayor and a group of councilors. The proposer needs to present an argument backing up the reasons for the recall attempt, but she does not need proof. No recall attempt has been stopped because of an invalid reason so far. Welp (2015) mentions that "Quintanilla (2013) cites as the most common reasons to activate a recall in 2012 were (more than one reason could be given): (1) The lack of fulfillment of the working plan and/or electoral promises (143 requests); (2) The misuse of resources or funds for private gains (119 requests); (3) Bad management or moral incapacity (114 requests); (4) Nepotism, abuse of power and/or usurpation of functions (110 requests); (5) Lack of transparency, lack

eligible voters in the constituency.[10] Figure 1 shows the timing of elections, and the steps required to call a recall referendum.

When the signature threshold is achieved, the national electoral commission (JNE) calls for a recall referendum. Voters are able to vote for the recall of each individual authority under scrutiny. An incumbent is recalled if (i) turnout is at least 50 percent, and (ii) at least 50 percent plus one of the valid votes are cast in favor of recalling the authority. Despite the cumbersome procedure, recall referenda are very common in Peruvian politics. Between 1997 and 2013, there have been more than 20,000 recall attempts (kits purchased), and more than 5,000 officials have faced a recall referendum in 45 percent of all districts in the country (747 out of 1645 districts).[11]

When a mayor is recalled, unless at least one third of the council is also recalled, the first councilor from the list takes office until the next regular election cycle. On the other hand, if the mayor and at least a third of the council are recalled, there are new elections, and the elected mayor serves in office until the original term is done. Over our sample period, these new elections in the middle of the term take place in 71 instances (less than 18 percent of cases). In practice, this means that the new mayor is in power for less than two years.

Recall Elections and Political Capture

While recall elections are direct democracy mechanisms intended to increase accountability, it has been widely documented that they are often used as political tools. Given the large number of candidates running for office and the absence of run-off elections, it is not uncommon that mayors are elected with a very low percentage of votes. For example, in the 2014 election, the

of accountability, does not convene cabildos (city councils) (79 requests); (6) Does not execute public works or does so inadequately, does not finish or execute non-prioritized works (49 requests); (7) Does not respect agreements made through participatory budgeting, does not call for participatory budgeting or executes works not approved for in the participatory budget (47 requests); (8) Does not supervise local management (46 requests); (9) Negligence (42 requests); and (10) Non-Fulfillment of duties (39 requests)."

[10] The signatures submitted are examined by the national registry (RENIEC, a national level, technical and independent institution,) which checks for their validity, e.g. if all the individual information is correct, if the person is registered in the district where the recall is to take place, if she has not signed for other recall petition, etc. Once the signatures are checked, RENIEC gets back to the proposers and lets them know the final percentage of valid signatures. In cases in which this percentage falls below 25 percent of eligible voters, they have the chance to submit extra batches of signatures, which are checked until a pre-established deadline arrives or the threshold is achieved. The fact that that proponents of the recall have a chance to resubmit signatures creates bunching of petitions just above the 25 percent threshold, therefore preventing us from exploiting this discontinuity in an RD framework.

[11] Peru is the country in the world where recall referenda are used most often, followed by the US and Poland, where this institution has been in place for more than 100 and 25 years, respectively. Welp (2015) reports that "recall referendums have become one of the most intensively used mechanisms of citizen participation in South America, particularly in the Andean countries. To give just a few examples, between 2008 and 2010 more than 700 recall attempts were registered in Ecuador of which more than 100 resulted in a referendum. Hundreds of attempts have been registered in Bolivia since 2012 and Colombia has seen a large number of recall attempts since its legal introduction in 1991, including a process against the Mayor of Bogota, Gustavo Petro, in 2012. The mechanisms is also provided in some Mexican states and Argentine provinces."

average mayor was elected with 35.1 percent of the votes, and in districts with above median political competition (as measured by the number of candidates), this number goes down to 29.4 percent. After a mayor is elected, it is not hard to put together a coalition between loosing candidates that gather enough support to promote a recall referendum (Bland and Chirinos (2014)). For instance, as the JNE shows, in the 2012 recall cycle, 22 percent of the promoters of a recall referendum were candidates who lost in the preceding election. If one considers that many times politicians have political operators representing them as the official person promoting the recall, we should expect the true number to be even larger. Holland and Incio (2019) show a similar statistic for the full period of our study (2002-14). These statistics, on top of the fact that the number of recall referenda has varied widely across years, add to the uncertainty any candidate has about the probability of being ousted from office through a recall referendum due to political grievances (i.e. independent of their performance in office).

The political use of impeachment and recall procedures and their failure to achieve effective accountability are not unique to Peruvian politics. A number of examples of high-profile recalls have been linked to elite power grabs.[12] Altman (2010) (as cited in Holland and Incio (2019)), in a global study of direct democracy mechanisms, describes recalls as "motivated by political reasons" (p. 16).[13]

Two recent papers provide detailed accounts of how Peruvian recall elections are very often used as a tool to pursue political goals rather than to achieve real accountability. Welp (2015) argues that the combination of low institutionalization of political parties and the relative ease with which recall referenda are activated generate incentives for political opposers to use these mechanisms to undermine the incumbent and have chances of more frequent political campaigning, solidifying their names in the political arena. On the other hand, Holland and Incio (2019) quantitatively analyze the determinants of recall elections. Using data for the same period as the one covered in our study, they find that "losing politicians organize recall referenda, but office performance matters when citizens vote to retain their politicians." As evidence of the political use of recalls, they document that (i) 18% of the recall requests are filed by former political competitors, (ii) the most common words used in these recall requests are related to unverifiable claims (e.g. failure to fulfill electoral promises, incompetence, poor management, etc.), and (iii) more than 50% of recall petitions are filed as soon as

[12] See: Altman (2010), McCoy (2006), Welp and Milanese (2018) and Miro-Quesada Rada (2013)

[13] Helmke (2017) and Perez-Linan (2007) argue that recalls and impeachments are more often related to elite conflicts and serve to strengthen power groups. Breuer (2007) studies direct democracy accountability institutions in Latin America, arguing that despite the introduction of referenda and recall mechanisms, structures of vertical accountability have been hardly affected. Likewise, Qvortrup (2011) conducts a comparative study analyzing qualitative and quantitative information from recall elections in the US and Canada, and concludes that recall elections have hardly improved trust in the government, and moreover, have tended to strengthen political elites. Serdült and Welp (2012) runs a comprehensive analysis of bottom-up direct democracy worldwide and finds that these referenda could be used to concentrate power and serve as partisan strategies.

legally possible, hardly providing enough time to evaluate the incumbent's performance. Other qualitative studies support these conclusions. For example, after the 2012 recall election against the mayor of Lima, Miro-Quesada Rada (2013) and Vasquez Oruna (2014) document that political grievances were behind this recall campaign. Soldevilla (2014), more generally, characterize the system of recall elections against local politicians in Peru as "instruments of revenge."

The evidence presented above documenting the political use of recall elections in Peru has been mostly generated in the past few years, after the very salient recall attempt against the mayor of Lima in 2012. In this event, it was very clear that the proposer of the recall was a political proxy of the runner-up in the previous election and former mayor of the city (who was in turn elected in 2014), and that the motivation for the recall was political rather than based on the mayor's performance in office. In turn, this recall process made analysts and policymakers in Lima aware of the political use of recall elections and this led to a change in the regulation that was directly intended to reduce the benefits for political opposers from having a mayor recalled. This new regulation allowed every elected mayor to hand pick her successor in case of a recall, therefore blocking political opponents from gaining access to the municipal seat. The effect of this change was that in the electoral period 2014-18 only 29 districts in the whole country had a recall election.

It is important to highlight that after almost 20 years recall elections had been in place and that hundreds of politicians had faced recall referenda, policy makers and experts in the area in Lima were not aware of the prevalence of the political capture of recall elections. This is mostly due to the fact that most of these recall referenda take place in small municipalities and national media hardly ever covers these processes. The JNE documents that between 1997 and 2012, 1,015 districts held a recall referendum and 91.7 percent of them had less than 5,000 registered voters (Welp (2015)). If politicy makers and experts were not aware of these details of the effective implementation of the recall elections, it is safe to assume that potential candidates are also uninformed about the prevalence of political capture.

In Table 1, we provide quantitative evidence supporting the claims presented above, and regress the presence of a recall referendum on different covariates that presumably predict recall elections, namely, the observable characteristics of the mayor and variables describing the political scenario of the previous election (turnout, number of candidates and win margin). After including in the regression district and election fixed effects, the variables that have more predictive power are those related to the level of political competition: recall elections are more likely to take place after a close election and in districts where turnout was higher (though the magnitude of the latter is small). Importantly, none of the mayor's characteristics have economically or statistically significant effects on the probability of a recall election taking place. This is consistent with the claim that the recall referenda are used as a political tool,

rather than as a citizen control mechanism.[14]

Presumably, the political objective of a recall referendum is to weaken the incumbent for a future election. In districts where a recall petition was initiated through a signature collection, the incumbent runs for reelection in 79.7 percent of cases (compared to 68 percent, where there was no signature collection at all). Incumbents who faced a referendum and survived it, run for reelection 72 percent of the times, and 18 percent of them win the reelection. In contrast, 48 percent of incumbents who were recalled do run for reelection, but only 4.8 percent of them win these elections (see Appendix Table 15).

3 Conceptual Framework and Empirical Strategy

3.1 Conceptual Framework

In this section, we outline a simple framework to help conceptualize the expected effects of a politically captured accountability institution on political selection and guide the empirical analysis.

In our framework, there are three dimensions that characterize politicians: ability, office motivation and corruption. High ability politicians are also more productive in the private sector and hence their opportunity cost of entering politics is higher (Besley (2005)). Politicians derive utility from delivering public goods, and this level of utility increases with co-ethnicity. This is consistent with empirical evidence on policies targeted towards the politician's identity group, both for welfare transfers and public goods (see e.g. Pande (2003) and Burgess et al. (2015).) Finally, corrupt politicians are motivated by extracting rents from office (e.g. Ferraz and Finan (2011)). The introduction of a well functioning control and accountability mechanism increases the cost of rent extraction and inefficient policy making, therefore deterring corrupt and low quality candidates from entering the political arena (Persson and Tabellini (2000)).

As documented in Section 2, recall elections for local politicians in Peru have been shown to be used for political purposes very often. How does the political capture of this accountability institution affect the decision to run for office? Effectively, when a politician learns about the political capture of accountability institutions, there is a reduction in her expected term length, and importantly it is unrelated to her potential performance in office. This in turn leads to a decline in the direct benefits from office, in the form of e.g. wages earned or ego rents. The first implication derived from this simple framework is that potential politicians

[14] In a similar analysis, we consider the correlates of recall attempts (i.e. buying a "kit"). Here, the results also point in the same direction. Municipalities with a lower win margin have a higher probability of having someone attempting to recall the incumbent. On top of this, we see that some individual characteristics that are unlikely to be related to performance in office, as being a woman, the number of years in elected office or party affiliation are also significantly associated with the probability of a recall attempt. These results are shown in Appendix Table 14.

with a higher opportunity cost are more likely to be deterred from running for office when they learn about the political use of recall elections. Conversely, we would see low ability politicians entering the race. One important proxy for the opportunity cost is the level of education.

A shorter term in office implies that the opportunities to take on public works are reduced. There is a mechanical effect in that with a shorter period of time available, the ability to deliver public goods is lower. On top of this, the fact that there is a recall referendum implies that the incumbent has to spend time campaigning and this displaces time devoted to policymaking. The second implication of the framework is that upon learning about the political use of recall elections, policy motivated politicians would be less likely to run for office. Moreover, if we consider that the utility politicians derive from public good delivery is increasing in the level of co-ethnicity, we should expect that more representative politicians (i.e. indigenous politicians in localities with high proportion of Quechua or Aymara speakers) are less likely to run for office. Finally, the prediction in terms of the selection of corrupt politicians is ambiguous, since a shorter term implies less opportunities for rent extraction, but on the other hand, the recall itself lowers reelection incentives, lowering the cost of engaging in corruption.

3.2 Empirical Strategy

The ideal experiment to test the predictions of our simple framework would be one in which we randomly allocate information about the political capture of an existing institution that allows voters to hold elected officials accountable. Obviously, random variation of this sort is nearly impossible to find, because to be able to run such an information experiment, we would need to know ex-ante the set of municipalities where the institution has actually been captured and provide evidence on this, which is extremely hard to do. Instead, in the empirical section we rely on the observation that the political use of the accountability institution is not likely to be public knowledge (see Section 2), and exploit quasi random variation in events that are likely to reveal information about the political capture of this accountability institution.

Our empirical strategy is designed to resemble the ideal experiment. In a sample of municipalities where the accountability institution is active, i.e. where a recall election took place in the previous period, we compare the characteristics of candidates running for office in districts where the mayor barely survived the recall with those running in a district where the mayor was ousted from office by a small margin. The underlying assumption is that the fact that a mayor is recalled is a salient event that reveals information about the political capture of the accountability institution. In additional tests, we will provide evidence consistent with the information acquisition story.[15]

[15]The revelation of information about the political use of recall elections can take place at two stages: when the referendum takes place and when the mayor is recalled. We indeed see these two steps as a continuous signal revelation process in which potential candidates learn something about the motives for the recall at both

Following the description of the ideal experiment above, to test our main hypotheses, our identification strategy uses a sharp regression discontinuity design (Lee and Lemieux (2010), Imbens and Lemieux (2008)), and relies on the assumption that districts in which the mayor was barely ousted are similar in observable and unobservable characteristics to those in which the mayor barely managed to stay in office.

Our main regression equation is as follows:

$$Y_{ijt} = \alpha + \beta Recalled_{jt-1} + \gamma f(VoteShare_{jt-1}) + \varepsilon_{ijt} \tag{1}$$

where, Y_{ijt} are characteristics of candidate i running for office in district j in election t. In our main regressions, these characteristics include their educational level, years of experience in private and public office, demographic characteristics, whether the candidate is of an indigenous group and if she is representative. Our main interest lies in β, the coefficient associated with having a mayor recalled in electoral term $t-1$. The running variable is the share of votes in favor of the recall, and thus we include in all of our regressions a flexible polynomial of this variable $f(VoteShare_{jt-1})$. Our preferred specification uses a local linear regression with triangle kernel weights (we also show the results for other specifications for robustness). Finally, ε is the error term, which we cluster at the level of the treatment, district×election level.

Given that we are comparing candidates in elections where a recall election was barely won or lost, our analysis sample is restricted to district×election observations in which a recall election was held. In addition, we only consider observations at the district×election dimension for which the vote share in favor of the recall is close enough to the threshold and present robustness checks for multiple bandwidths, i.e. $VoteShare_{jt-1} \in [0.5-\epsilon, 0.5+\epsilon]$, where ϵ is determined with optimal bandwidth selection procedures. In our preferred specification, we use optimal bandwidths based on Imbens and Kalyanaraman (2012). In the Appendix, we also present results with the bandwidths as suggested by Calonico, Cattaneo, and Titiunik (2014) as well as results with 3 percentage points as bandwidth.

To provide evidence that the reduced form effects are consistent with our main hypotheses, namely, that a successful recall of a mayor informs potential candidates about the political capture of the accountability institutions, we will use different strategies. First, we explore the heterogeneity of the results by variables that indicate that the recall was initiated for political

stages. However, we view the actual recall of a mayor as a more salient event that highlights the potential consequences of the recall, and in which the performance of the incumbent is observed and the motives of the proponents of the recall are evident. Additionally, note that a comparison between municipalities where there was a recall referendum with those where the referenda did not take place would confound the effects of the referenda in itself and (limited information of) political capture. Instead, the comparison we make isolates the effect of the information on political capture. Given that some information is revealed when the recall election is initiated, our estimates should be considered as a lower bound for the real effect of a captured political institution on the selection of candidates.

purposes and not related to the mayor's performance in office. For example, we identify recall elections that were initiated by someone who was a candidate in the previous election (as these have been identified by Welp (2015) and Holland and Incio (2019) to be politically captured) and use proxies of the mayor's performance (measured by the municipality's revenues and spending during the mayor's tenure). Second, potential candidates are likely to learn about the political use of recall elections not only through electoral results in their own municipality, but also from those that are close by. We test this by looking at the selection of candidates in municipalities that *did not have a recall election*, but had one occurring in a neighboring municipality (within two hours of travel time) and do a similar exercise as above.

4 Data

4.1 Data Sources

For the empirical analysis, we put together data from different sources. Our main outcome variables are compiled from www.Infogob.com.pe, a government website that publishes electoral information, and candidates' Curriculum Vitae (*Hoja de vida*). An example of the CVs posted on line can be found in the Appendix. We scraped the website to assemble a novel and comprehensive dataset with the characteristics and background of candidates who ran for mayor in the 2002, 2006, 2010 and 2014 elections.

Despite the differences in the format and level of detail provided in the original datasets for different years, we compute a series of consistent variables related to the candidates' schooling: (i) ever attended to the university, (ii) attended only a technical education center, (iii) attended up to secondary school, (iv) attended at most primary school. From these variables, together with information on whether each level of schooling was completed or not, plus the number of years of schooling at the post secondary level, we can impute the number of years of education.[16] Additionally, the dataset includes information on the candidates' work and political experience as well as political party service, from where we can compute the number of years of experience in (i) elected public office (mayor, councilor or regional counselor), (ii) the position of mayor, (iii) service in party office, as well as (vi) whether a candidate is a member of a national political party, (v) has work experience in the public sector or (vi) private sector. Finally, we obtained information on the candidates' demographic characteristics, e.g.

[16] In accordance with the Peruvian educational system, we impute 6 and 5 years of education if a candidate completed primary or secondary education, respectively. For attending but not graduating from primary or secondary school, 4 and 3 years of education are imputed, respectively. To avoid mistakenly giving too much weight to individuals who stretched their degrees beyond the regularly required degree period, we assign caps on the maximum amount of years for post secondary degrees. Finally, for the rare cases where the years studied for post secondary degrees are not reported, we impute years of education as follows. First, attending or completing university are imputed as 4 or 6 years of education. Second, attending or completing technical education are coded as 1 or 3 years of education.

gender and age.[17]

While candidates are not legally mandated to submit their CVs to the national electoral office, conditional on reporting it, the information has to be truthful or else they could face legal charges. The coverage of our dataset is large: we have information on educational attainment for 94.7 percent of candidates running in the 2014 election, and 93.9, 84.8, 84.1 for those in contention for the mayor's seat for 2010, 2006 and 2002, respectively.[18]

To construct a measure of the ethnic background of candidates and the degree to which they are representative of local population, we first do a text analysis of all candidate's surnames, and classify them as indigenous or other (Spanish or foreign). We identify Hispanic surnames using the dictionary suggested by the Biblioteca Nacional de España (Platt (1996)), which includes an index of Hispanic surnames developed in Latin America and the United States.[19] This source includes the list of surnames in Carraffa and Carraffa (1920–1963), the traditional reference for Hispanic surnames.[20] We complement Basque surnames using a list of surnames provided by the Real Academia de la Lengua Vasca. On the other hand, for the identification of native surnames, we look for the presence of linguistic roots from the Quechua and Aymara language families (the two most popular ethnic groups in Peru) within surnames. For the Quechuan language family, the main sources are the classic dictionary by González Holguín (1608)[1952] and a recent dictionary compiled by the Academia Mayor de la Lengua Quechua (2005). We also include the list of names provided by the Peruvian Registro Nacional de Identificación y Estado Civil (RENIEC (2012)). For the Aymara family, the main sources are the classic dictionary by Bertonio (1612)[2011], the list of surnames provided by De Lucca (1983), and a recent dictionary compiled by CONADI (2011). Finally, the analysis is complemented with two additional sources: (1) *Vocabulario Políglota Incaico*, originally compiled by Franciscan missionaries in Peru, which provides an extensive list of words in four dialects of Quechua (varieties of Cuzco, Ayacucho, Junín and Ancash) and Aymara, see Fide (1905)[1998]; and (2) the *An Crúbadán-Corpus Building for Minority Languages* project, which provides downloadable text datasets for different dialects of Quechua and Aymara based on online text resources, including translations of the Bible and the Universal Declaration of Human Rights.

Once identified the origin of all candidates' surnames, we classify them as indigenous if

[17] While the CVs online have fields for filling out previous convictions or open trails and wealth, these are seldomly filled, and therefore we can't use them for our analysis.

[18] While the website does not provide a direct link to the CVs of candidates running for the 2002 elections, we do have the list of their ID numbers. The information for the 2002 candidates is taken from the CVs reported in subsequent elections. Our main analysis is centered on the characteristics of candidates running in the 2006, 2010 and 2014 elections, and we use the information from 2002 for robustness and validity checks.

[19] As stated by the author, "the word Hispanic refers to individuals born in Latin America or the United States, whose parents speak Spanish and whose principal cultural background was Spanish."

[20] The list of surnames in Carraffa and Carraffa (1920–1963) can also be accessed through The Library of Congress.

they have at least one or two Quechua or Aymara surnames. To measure how representative candidates are of the local population, we use the 2007 census to compute the percentage of the district's population that has an indigenous language as their mother tongue and define a candidate as representative is she has indigenous surnames and at least 25, 50 or 75 percent of the municipality's population speaks an indigenous language.

Finally, we obtained from the national electoral office (ONPE) information on all relevant political outcomes at the district level, namely, the list of candidates running for each election, their party affiliations and vote shares. These data allowed us to compute the win margin of the elected mayor. Additionally, they also gave us access to data on the number of kits bought to attempt a recall, the names of the authorities who they attempt to recall, the name and ID number of the person who filed the recall petition, and whether a recall referendum took place in a district (and its date), and its outcome. Data on the percent of budget executed, revenues and expenditures of the municipality were obtained from the Ministry of Economy and Finance (MEF).

4.2 Descriptive statistics

As mentioned in Section 2, recall referenda are fairly common in Peru. Figure 2 shows the incidence of recall referenda over the last three electoral periods. Recalls have been attempted (i.e., "kits" purchased) in 35 to more than 60 percent of districts, with a clear upward trend in time. These attempts have been successful in about 35 percent of cases in each period, meaning that between 10 to 20 percent of districts in the country had a recall referenda, leading to between 2 to 6 percent of districts having a recalled mayor. Our main analysis sample is drawn from the subset of districts×elections in which a recall referendum was held. Overall, the statistics from Figure 2 reinforce the fact that there is wide time variation in the incidence of recall referenda, and that the probability of being recalled is quite uncertain.

Table 2 provides the basic descriptive statistics of our data, both for the full sample, and for the restricted sample of districts×elections in which the vote share in favor of recalling the mayor was around the 50% threshold. To select this sample, in our preferred specification, we use the optimal bandwidth proposed by Imbens and Kalyanaraman (2012) for regression discontinuity analyses. Candidates running for mayors in Peruvian municipalities have a relatively high level of education. 39 percent of candidates in our RD sample attended university, and they have on average 14 years of education. Similarly, around 7 percent of candidates during the analysis period have primary education or less while around 34 percent have only attended secondary. Those candidates that end up elected as mayors have on average extremely similar educational levels. In terms of their previous experience, elected mayors are also similar to the ones facing a recall election. They have on average 1.9 years of experience in elected office, of which about 70 percent comes from their experience as mayors in the past.

Finally, for both candidates and elected mayors, a relatively low number (slightly less than 40 percent) belong to a political party that nationally competes in elections. The fact that the majority of candidates runs for a regional or local party or movement illustrates the fragmentation of the political and party system in Peru at the municipal level. In this context, individuals matter more than party platforms, emphasizing the significance of their qualifications, experience and personality for local politics and public goods provision. Demographically, candidates and mayors are of similar average age (46 years). The share of women among those who get elected into office is extremely low (5 percent), and is two thirds the share of women who run for office. Finally, 33 and 6 percent of candidates running for office have at least one or at least two indigenous surnames, respectively.

In the last rows of Table 2, we present a brief overview of variables at the district level. These consist of political and electoral outcomes, such as the number of candidates that run for the office of mayor, the win margin (in percentage points) of the elected candidate and the election turnout, as well as the percentage of the population in the district that speaks a native language, and the log total revenues and expenditures. Table 2 draws a clear picture of the nature of Peruvian municipal elections. The elections are strongly contested, with on average more than 7 candidates running for mayor. We compute a measure of political competition as the effective number of candidates. For this, we take the inverse of the sum of squared vote shares of each running candidate within an electoral race. If all candidates have the same vote share, then this measure is equal to the actual number of candidates, on the other hand, if one candidate wins all votes, then the effective number of candidates is one. Note that the average effective number of candidates (below 5) is smaller than the actual number of candidates. The average win margin, of around 9 percentage points, appears at first glance relatively large in comparison to the number of candidates and the proxy for political competition. However, a closer look at the distribution conveys a more nuanced view. 50 percent of electoral races were decided by at most 6 percentage points and at the top the win margin is significantly reduced. For instance, the average win margin for the 50 percent closest electoral races is below 3 percentage points, for the the most competitive third of elections the win margin drops even below 1.9 percentage points. In conjunction with the other electoral measures and the high level of voter mobilization (around 85 percent of all registered voters participate in elections), this demonstrates that elections for mayoral office are in many instances extremely competitive and often decided by a marginal number of votes.

Overall, the average district in the country has around 29 percent of people who speak a native language as their mother tongue. A comparison between the average share of indigenous people, as measured by the share of population with a native mother tongue (29 percent), and the average share of elected mayors that are classified as having an indigenous background according to their two surnames (only 8 percent) speaks about the political under-representation of this historically disadvantaged group.

5 Results: Accountability and Candidate Selection

5.1 Candidate Education, Experience and Representativeness

Figure 3 shows graphically our main results using non-parametric plots with breaks at the 50 percent vote share. As is clear from the graphical evidence, candidates who run in elections in districts where a mayor was recalled in the previous electoral period have less years of education, are less likely to have attended university, and more likely to only have attended up to secondary education.

In Table 3, we formally test for the magnitude and significance of the observed effects from Figure 3, showing the results of regression equation (1). Panel A shows our preferred specification, in which we run the regression discontinuity using a local linear regression for the running variable and triangle kernel weights. All results are shown restricting the sample to an optimal bandwidth (following Imbens and Kalyanaraman (2012)) but they are not sensitive to the choice of bandwidth.[21,22]The main message from the graphical evidence holds: candidates running in districts with higher salience of the recall institution have 0.5 less years of education and are 22 percent less likely to have attended university. The proportion of candidates with just a technical education center diploma is unchanged, but there is a sharp increase of 23 percent in the proportion of candidates who only attended secondary education. Panels B and C of Table 3 shows a specification check in which instead of using a weighted local linear regression of the running variable, we use a linear or quadratic polynomial and the results remain unchanged. Generally, the qualitative and quantitative results are not sensitive to the choice of bandwidth or polynomial specification.[23]

While there seems to be a robust relationship between the leader's educational level and economic performance, a leader's quality is a multidimensional concept. Our data allow us

[21]The differences between the sample sizes used in the different regressions are due to the optimal bandwidth obtained..

[22]Appendix Table 17 shows the results of a naïve OLS regression on the sample of districts×elections where a recall referendum took place (Panel A). The results are of a similar magnitude as the ones in our RD approach (if anything, slightly larger). We also run a similar regression where we analyze the correlation between having a recall election and the educational level of candidates running in the following election. These results are shown in Panel B of 17. Consistent with the idea that the different steps of the recall process is a continuous signal revelation process, we see that places where a recall election took place have candidates with lower levels of education in the next election. Using an alternative identification strategy exploiting the discontinuity provided by the number of signatures needed to hold a recall referendum we find similar effects than the OLS regression. However, this identification is weaker than the one shown for the main results of this paper since opposers had the chance of submitting signatures to the electoral office multiple times, thus generating a larger mass of observations at one side of the discontinuity and raising concerns about selection into the treatment. These results are available upon request.

[23]In Appendix Table 18 we also show the robustness of the results to using higher order polynomials. The qualitative results and the magnitude of the coefficients is similar to our preferred specification, but we lose statistical significance as we increase the flexibility of the polynomial. We also show the main regressions for alternative bandwidth specifications, namely the one suggested in Calonico, Cattaneo, and Titiunik (2014) (Appendix Table 19), or an arbitrary bandwidth of 3 percentage points above and below the threshold (Appendix Table 20). All the results are quantitatively and qualitatively similar.

to look at other characteristics that are also presumably related to the mayor's performance in office, beyond educational attainment. Using our preferred specification from Panel A in Table 3, the results in Table 4 show the selection effects for the candidates' experience before deciding to stand for office and their demographic characteristics.

Our results demonstrate that candidates who decide to stand for elections in municipalities where a recall referendum recalled the incumbent in the previous period have less years of experience in elected office (not significant), and in particular, they have 0.4 less years serving as a district mayor, and there is suggestive evidence that they are less likely to have experience holding an office in a political party (0.2 years less, not statistically significant). Importantly, having a recalled mayor in the past does not have differential effects on the proportion of candidates affiliated with national political parties. We also find that candidates in the treatment group are 11 percentage points less likely to have any experience working in the public sector (from a base of 55 percent), and they are one and a half years younger (not significant).

Office motivated politicians derive utility from the delivery of public services, and the level of utility derived is higher when the public goods are provided to coethnics. Unfortunately, we do not have a good measure of public service motivation in our data. However, we can study whether having a mayor recalled in the previous period affects the proportion of candidates coming from historically disadvantaged ethnic groups (Quechua and Aymara) and if the effects are more or less important in municipalities where they are more representative, namely, where there is a larger share of the population of the same ethnic background. To do this, we create a new measure of the politician's ethnicity based on their last names and exploit information from the census on the percentage of people who speaks Quechua or Aymara as their mother tongue. Columns 1 and 2 in Table 5 show that having a mayor recalled in the previous electoral cycle does not affect the number of candidates with at least one or two indigenous surnames, respectively. We further analyze whether the proportion of representative candidates is affected by learning about a politically motivated recall. We define an indigenous candidate (candidate with at least one indigenous surname) as being representative if she is running for mayor in a municipality where more than 25, 50 or 75 percent of the population has the Quechua and/or Aymara languages as mother tongue (Cols 3-5, respectively). In municipalities with a large proportion of Quechua and/or Aymara population ($>$ 75 percent), there is a large negative and significant effect of having a mayor recalled on the number of candidates with an indigenous background. This result speaks to the effects of politically motivated recalls on the representativeness of candidates, especially of those coming from historically worse off groups, as is the case for indigenous population in Peru.

Overall, the results indicate that candidates who decide to run in elections after a mayor was recalled are not only less educated, but they also seem to be new entrants to politics and

to the public sector in general: they have less experience in elected office, have less experience working in the public sector and are younger. Additionally, they are less representative of indigenous population.

More educated leaders have been shown to cause better public good provision and economic growth. Exploiting a natural experiment in Indonesia, Martinez-Bravo (2017) shows that villages with a head who has an additional year of education are more likely to have available more health centers, doctors and safe drinking water. Using data from southern India, Besley, Pande, and Rao (2005) show that the educational level of the village heads is correlated with lower levels of corruption. In a cross country setting, Besley, Montalvo, and Reynal-Querol (2011) document that the exogenous removal of a highly educated head of state has a negative impact on economic growth. On the other hand, the relationship between the other observed characteristics of politicians and their performance in office is less clear. We use data on municipal finances for the period of analysis to evaluate at the correlational level how are candidate characteristics associated with relevant policy outcomes. Appendix Table 16 shows the result of OLS regressions with district and electoral period fixed effects in which we include variables related to the mayor's education and experience. The dependent variables are the log of expenditures and revenues over the last three years of the mayors' term. Mayors with higher levels of education (measured either in years or as level dummies) on average show higher levels of expenditures and revenues during their tenure. A mayor with one more year of education spends and collects 0.8% more, on average. Likewise, having attended to university (compared to only primary education) is associated with 8% higher expenditures and 8.9% more revenues. Similarly, more years of experience serving in a political party and experience working in the public sector are associated with more revenues and spending, while younger politicians tend to spend and collect less money. While correlational, this evidence suggests that the characteristics that we find to be relevant in our selection analysis are associated with important policy outcomes at the district level, and therefore support the idea that generally the quality of candidates falls in districts where the previous mayor was recalled by a small margin.

5.2 Identification Assumptions

The identification assumption in our empirical design is that observations at both sides of the threshold are comparable along observable and unobservable characteristics. Figures 4 through 8 show the continuity tests for different district×election observable characteristics. We focus on variables related to (i) the educational level (Figure 4), and (ii) previous experience and characteristics (Figure 5) of the incumbent during the period when the recall referendum took place, (iii) variables related to the political process in the previous electoral period (Figure 6), and (iv) educational level and the characteristics of the runner-up in the

previous electoral period (Figure 7 and 8, respectively). There are no significant jumps along the threshold in most of the variables of interest. We formally test the continuity assumption in a regression framework in Appendix Table 21.

A second important assumption of a regression discontinuity design is that there is no sorting into the treatment. One indication that units could be sorting into the treatment is that the density of observations is discontinuous at the threshold (McCrary (2008)). Figure 9 shows a graphical depiction of the McCrary (2008) test, and as expected, the density of observations is continuous around the 50 percent vote share threshold. This ensures that selection into treatment is not a concern.

Overall, the set of robustness and specification checks implemented provide assurance that our results are not driven by selection, and that there is a causal relationship between having a recalled mayor in the district in the previous electoral period and the quality of the candidates that decide to run for office.

6 What Drives the Negative Selection of Candidates?

How could it be that an institution that increases voters' ability to hold politicians accountable while in office generate a negative selection of candidates? As discussed in our conceptual framework in Section 3, we argue that having a mayor recalled in a certain municipality updates potential candidates' priors about the probability that they are recalled from office for political reasons, and unrelated to their performance (a similar mechanism as in Avis, Ferraz, and Finan (2018)).[24] An increase in the perceived probability of being recalled decreases the expected value of office and therefore affects the selection of candidates.[25]

While there could be other mechanisms at play in this selection process, in this section, we provide evidence that the incentives given by the expected rents from office for potential candidates are the main drivers of the reduced form effects. We first test whether the main effects shown in Section 5 are driven by politically motivated recall referenda. Testing for a hypothesis involving the *intentions* of the recall promoters is inherently difficult, therefore we proxy for this using data on whether the recall petition was initiated by someone who was a political contender in the previous electoral period (as argued in Welp (2015) and Holland and Incio (2019)). In Panel A of Table 6 we show the results of our baseline regression, interacting

[24] It is important to note that having a mayor recalled in a district does not affect the probability of the next mayor being recalled (especially in locations where the recall was decided by a small margin of votes), since this probability should reflect voters' preferences. However, our argument is based on candidates' learning about the probability of a politically motivated event.

[25] An alternative interpretation with similar reduced form predictions is that the salience of the accountability institution, and more specifically, politically motivated recalls, raises the perceived probability of being removed from office. This is consistent with evidence showing that people overestimate the probability of an event right after it has occurred, e.g. sales of flood insurance increase after a hurricane, or attendance to a certain beach is reduced after a shark attack.

the main treatment variable with a dummy for whether the recall petition was initiated by a former political contender. A large share of the main effect of the presence of a recalled mayor on the educational level of the candidates running in the next election is driven precisely by those elections where the recall was promoted by a political opponent.

Our hypothesis implies that the negative selection should be driven by elections where the potential candidates perceive that they could be recalled from office regardless of their performance. In Panels B, C and D of Table 6, we directly test this implication by interacting our main treatment variable with different proxies for the performance in office of the incumbent subject to a recall. The policy variables used in this exercise are the average percentage of the budget that she ends up spending at the end of each of the last three fiscal years (Panel B), the log total revenues collected (Panel C) and the log total expenditures during the last three years of the term (Panel D). [26] The results in Panels B, C and D show that the negative selection of candidates is unrelated to the performance of the incumbent in office, since the coefficient of the interactions are small in magnitude and statistically insignificant.

An additional piece of evidence consistent with our main hypothesis is presented in Table 7, where we check if it is indeed the case that potential candidates with high ability, and therefore high opportunity cost, are the ones who are self-selecting out of the electoral race. In the absence of data on the opportunity cost of the candidate, we assume that the wage that one would earn in the private sector represents this opportunity cost. We use information from the Peruvian LSMS (ENAHO) to generate a predicted wage in the private sector for each candidate. To do this, we run a Mincer regression on the LSMS data for people who report working in the private sector, and use as regressors all the variables that are also available on the candidates' CVs (age, age squared, gender, rural/urban, and education dummies). Using the coefficients from this regression, we generate a predicted value of the opportunity cost for each candidate, which we use as the dependent variable in the regressions in Table 7. The results using specifications with different functional forms show that candidates running for office in districts that had a mayor recalled in the previous term have a lower opportunity cost of around 11 percent.

An important assumption in our analysis is that the reason why the observed candidate selection pattern arises after observing a mayor being recalled is because potential candidates learn that the accountability institution is used for political purposes, updating their beliefs about their own expected term length. To indirectly test this, we analyze other events in which this information can be transmitted. For example, when a neighboring municipality holds a

[26] While imperfect, the percentage of the budget executed is commonly used in the popular press as an indicator of performance. Budget execution is typically low, and it is not rare to see that a local government manages to spend only half of their budget by the end of the fiscal year. For some examples of press reports highlighting this issue and explicitly taking the percent of the budget execution as a proxy for performance, see e.g.: https://elcomercio.pe/lima/invirtio-obras-distrito-contamos-155429 or http://larepublica.pe/sociedad/1155111-regiones-y-municipios-no-pudieron-gastar-todo-su-presupuesto-este-ano.

recall election and the mayor is ousted, the information travels, and potential candidates in other municipalities learn about political capture. We define neighboring municipalities as those that share a border and for which the travel time by car between the two district capitals is lower than 2 hours (the results are robust to using other definitions, e.g. 1 hour). We collect the travel time information using the Google Maps API.[27]

In Table 8 we run our main RD regression, using a sample of districts×elections that *did not have a recall process*, but for which at least one of her neighbors had a close recall election, and compare the selection pattern that arises when the mayor of the neighboring municipality was barely recalled vs. when she survived the referendum. The running variable in these RD regressions therefore correspond to the vote share in favor of the recall in the neighboring municipality with the closest result.[28] The results shows that regardless of the functional form used, there is a robust negative effect of having a recalled mayor in a neighboring municipality on the level of education of the candidates that decide to run for office in the next election. Moreover, the magnitude of these effects is even larger than the ones shown in Table 3, which is consistent with the hypothesis that having a mayor recalled is a more salient event, but yet the recall election in itself also reveals information about the motives for the recall. This evidence is hard to reconcile with other alternative mechanisms that are unrelated to the information being revealed through the recall of a mayor.[29,30]

6.1 Alternative Mechanisms

While many alternative mechanisms are consistent with our reduced form results from Section 5, in this subsection we provide evidence rejecting some of these potential stories and in support of our main hypothesis.

A first concern with our results is that in municipalities that had a mayor recalled, the

[27] Note that the definition of neighboring municipalities is time invariant. Information on travel time using the Google Maps Distance Matrix API was accessed in May 2019.

[28] The results remain unchanged if we limit the sample to municipalities that only had one neighbor with a recall referendum, that is, excluding municipalities that received conflicting signals (more than one neighboring municipality with recall referendum in t−1, resulting in one successful recall and one non-successful recall). See Appendix Table 23.

[29] We can use this neighbors specification to replicate all the main tables in the paper, obtaining very similar results and leaving all the qualitative conclusions of the study unchanged. These results are available upon request.

[30] One concern with identification is that the effects of having a recall referenda are persistent over time, and therefore we could have districts in which the quality of candidates for mayor is decreasing systematically, and this could be driving the results. This amounts to a violation of the parallel trends assumption in a difference in differences setting. To alleviate this concern, in Appendix Table 22 we present a placebo test, where we test if the presence of a recall referendum in $t-2$ affects the selection of candidates running for mayor in period t. Notice that this test significantly reduces our sample size, since the inclusion of a lag of our treatment variable effectively forces us to restrict the analysis to only two electoral periods. The results show that the main effects of a recall in $t-1$ are similar, both in magnitude and statistical significance, to the ones in Table 3. Note that these regressions are run using a linear polynomial, since the local linear regressions with kernel weighting do not allow for the inclusion of additional controls in the regression.

mayors' seat was taken by someone else, generating policy changes that could have affected the incentives for potential candidates to enter the race. However, this argument does not hold for municipalities that did not have a recall election, as those included in the results shown in Table 8. Instead, in these municipalities, the only thing that changes between the treatment and control municipalities is the recall of the mayor of a neighboring municipality.

Second, in districts where there is a lower quality mayor, the chances that voters oppose her in a referendum are higher, and thus opponents have larger incentives to campaign for a recall. This implies that districts with low quality mayors are more likely to lose a recall election, introducing concerns about omitted variables and selection. However, as we have seen in Figures 4 and 5, incumbents in districts at both sides of the threshold are similar in terms of their educational achievement, previous job experience and demographics. In Figure 9, we showed that the density is continuous around the threshold, hence candidates are not sorting differentially at the threshold. Further, in Panel A of Table 9 we include in our preferred specification controls for the characteristics of incumbent mayors (educational level, experience, age and gender), and our main results are not only qualitatively similar, but also the magnitude of the coefficients is very stable (though, some coefficients are no longer significant).

Third, certain political scenarios might increase the chances of a successful recall and at the same time deter specific types of candidates to run for election. For example, when an election was more contested, the chances of a successful recall are higher and promoters will work harder to get the mayor recalled. Again, all available political controls are balanced across the threshold (Figure 6), and including these variables in the main regression (Panel B in Table 9) do not significantly affect our results.

Fourth, the presence of a strong incumbent who has high chances of being reelected might provide more incentives for proponents to campaign against the mayor, and therefore weaken the incumbent's reelection prospects. If high quality incumbents decide not to run for office because they have been recalled, while other low quality incumbents who barely survived a recall referendum are still up for reelection, we would mechanically have a lower quality pool of candidates in places where a mayor was recalled. Panel A of Table 10 excludes from the regression sample all incumbents (i.e. including those who survived the recall), and the results are robust to this exclusion. In a related argument, some people could be better at running campaigns to recall mayors. If politically motivated recalls are run by those who lost previous elections, we should expect that including these characteristics affect the main estimates. First, we observe that the characteristics of the runner-ups are continuous across the threshold (Figure 7 and 8), and including these characteristics in the main regressions keep the results unchanged (Panel B in Table 10).

Finally, an alternative hypothesis explaining our results is that political competition determines the quality of candidates who run for office. Lower quality politicians are deterred

from running when an incumbent is in the race. Instead, when the incumbent loses the recall election, they face an open seat election and decide to run for office. Unfortunately, we are unable to to test empirically this conjecture, since only 4.8 percent of recalled mayors run for office. However, it is unlikely that this hypothesis explains our results. Unlike in the US, incumbents in Peru (and in many developing countries) do not seem to have an incumbency advantage (Uppal (2009); Titiunik and Klasnja (2015); Cordova and Incio (2013)). While between 60 and 80 percent of mayors run for reelection, a very low proportion of those get reelected (18-20 percent).

6.2 Candidate Entry or Exit?

Candidates running in elections after a mayor was recalled in a referendum are, on average, less educated, have less experience in the public sector, are less representative of indigenous population, and the evidence from Section 6 suggests that the effect runs through a reduction in the expected term length, which differentially affects the incentives to run for different types of politicians. One question that remains is whether it is indeed the case that high quality candidates who would have otherwise run are not entering the race, or instead that lower quality candidates are the ones entering the political arena.

To look into this question, as well as how the political landscape is affected in districts that had a recall election in the previous period, in Table 11 we analyze the effects of having a recalled mayor on turnout, the number of candidates, win margin, and political competition. Voter participation in elections does not change significantly after a recall referendum. The results in Column (2) show that the number of candidates in these elections do not change significantly either, suggesting that there is a reshuffling in the candidate pool: while high ability candidates are being deterred from running, some low-ability ones are entering races that they would have otherwise not participated in. Consistent with the entry of low ability candidates in the pool, we observe that the win margin does not change significantly, but the level of political competition remains unchanged. These results are consisten with the framework discussed above.

7 Do Recalls Lead to Lower Quality Mayors?

Does the lower average quality of the pool of candidates imply that the elected mayor will also be of lower quality? To explore this question, we run a similar analysis as before, comparing the characteristics of *elected* mayors in districts that had a mayor recalled or not in the previous term. The results of this analysis are reported in Table 12. Due to the lower number of observations, we have lower statistical power. Overall, the point estimates in these regressions indicate that, if anything, the effects of having a recalled mayor in the past on the

selection of candidates is significantly reduced when looking at the characteristics of elected mayors. For example, elected mayors have 0.15 less years of education and are 3 percentage points less likely to have attended university (not significant).

Panels B and C of Table 12 explore the effects on past political and job experience, as well as other characteristics. Overall, we do not see that having a mayor recalled in the past leads to elected mayors who have lower experience in public office. If anything, there is some weak evidence that elected mayors are younger and are less likely to have worked in the public or private sector. Despite the lower average quality of the pool of candidates, it seems like elections still serve as a mechanism to elect high quality politicians.

Finally, in Table 13 we study whether having a mayor recalled in the past causally affects relevant policy outcomes. In particular, we analyze the effects on total expenditures and revenues. Municipalities where the previous mayor was recalled have lower expenditures and revenues. However, the interpretation of these results is not obvious, since the reduced form effects could reflect the causal effect of having a (marginally) lower quality mayor, less political competition or lower quality of the opposition.

8 Summary and Discussion

All democratic systems have mechanisms intended to allow citizens to hold politicians accountable for their actions in office. The basic form of accountability are reelection incentives, through which voters punish or reward politicians with reelection depending on their performance. However, accountability institutions not only affect the behavior of politicians while in office, but also have an effect on potential politicians' decision of whether to run or not.

Most of the empirical literature analyzing the effects of accountability institutions have focused their attention on their discipling effects. Unlike these studies, in this paper we analyze how accountability affects the selection of politicians (candidates) and highlight the pervasive effects generated by the capture of accountability institutions by political interest groups. We study the effects of recall referenda in Peru, a direct democracy mechanism that allows voters to recall elected mayors from office, and compare the characteristics of candidates who decide to run in districts that had a mayor recalled from office in the previous term with those who run in districts where the mayor was not recalled. The fact that a mayor was recalled in a referendum in a district updates potential candidates' information about the prevalence of politically motivated recalls and therefore increases the perceived probability that, if elected, one could be ousted from office independent of the performance.

We identify our results using a close election regression discontinuity design. Our results show that candidates who run in districts that had a recall referendum in the last period are of lower quality, as measured by their educational attainment and previous experience. In particular, they have about half a year less education, are 8 percentage points less likely to

have attended university, and instead 8 percentage points more likely to have attended only up to secondary education. They are also less likely to have held elected office in the past, and in particular to have served as mayor. Likewise, these candidates have a lower likelihood to have worked in the public sector and are slightly younger. All in all, the results suggest that having a recalled mayor in the past lowers the quality of the candidate pool, while new entrants to politics are more likely to run. Additional results indicate that in municipalities that had a recalled mayor, candidates are less representative of the indigenous population.

How could it be that an institution that increases citizen control over politicians generates a negative selection? We provide qualitative and quantitative evidence that recall elections are often used as a political tool, with candidates who lost the elections in the previous period being the promoters of the recall election. If this is the case, the probability of being ousted is independent of the elected mayor's performance, hence discouraging politicians who have a high opportunity cost or who are motivated by public good provision (and especially to their coethnics).

Finally, we analyze whether the availability of an average pool of candidates of poorer quality leads to the election of lower quality mayors. Our results show that despite having a pool of candidates that is on average lower, elections are still doing their job, and voters select the best out of the available candidates, hence mayors in districts where an incumbent was recalled in the previous period have similar levels of education and experience to those who run in districts where the mayor barely survived the recall referendum. However, policies are still affected, and in these municipalities, expenditures and revenues are lower.

Our results have far reaching consequences for the design of citizen control mechanisms. While these institutions are supposed to increase the chances that voters exert control over public and elected office, and deter poor quality and corrupt politicians from standing for office, when they are at risk of being captured, their initial objectives can be distorted, leading to a poorer quality of the government and public service provision. These institutions should incorporate safeguards to prevent capture. For example, as in the cases of presidential impeachment, promoters have to present plausible evidence of miss-management or poor performance, which is evaluated before proceeding to the vote. These types of mechanisms could help avoid the political use of an otherwise well intended mechanism of citizen control.

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FIGURE 1: Timing for Recall Referenda

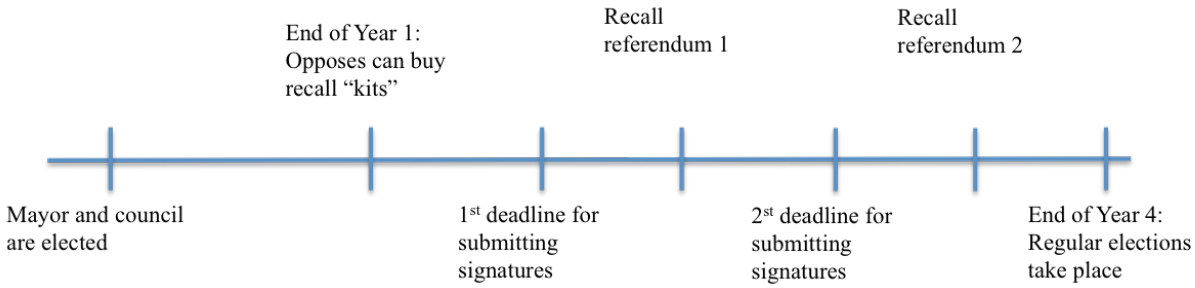
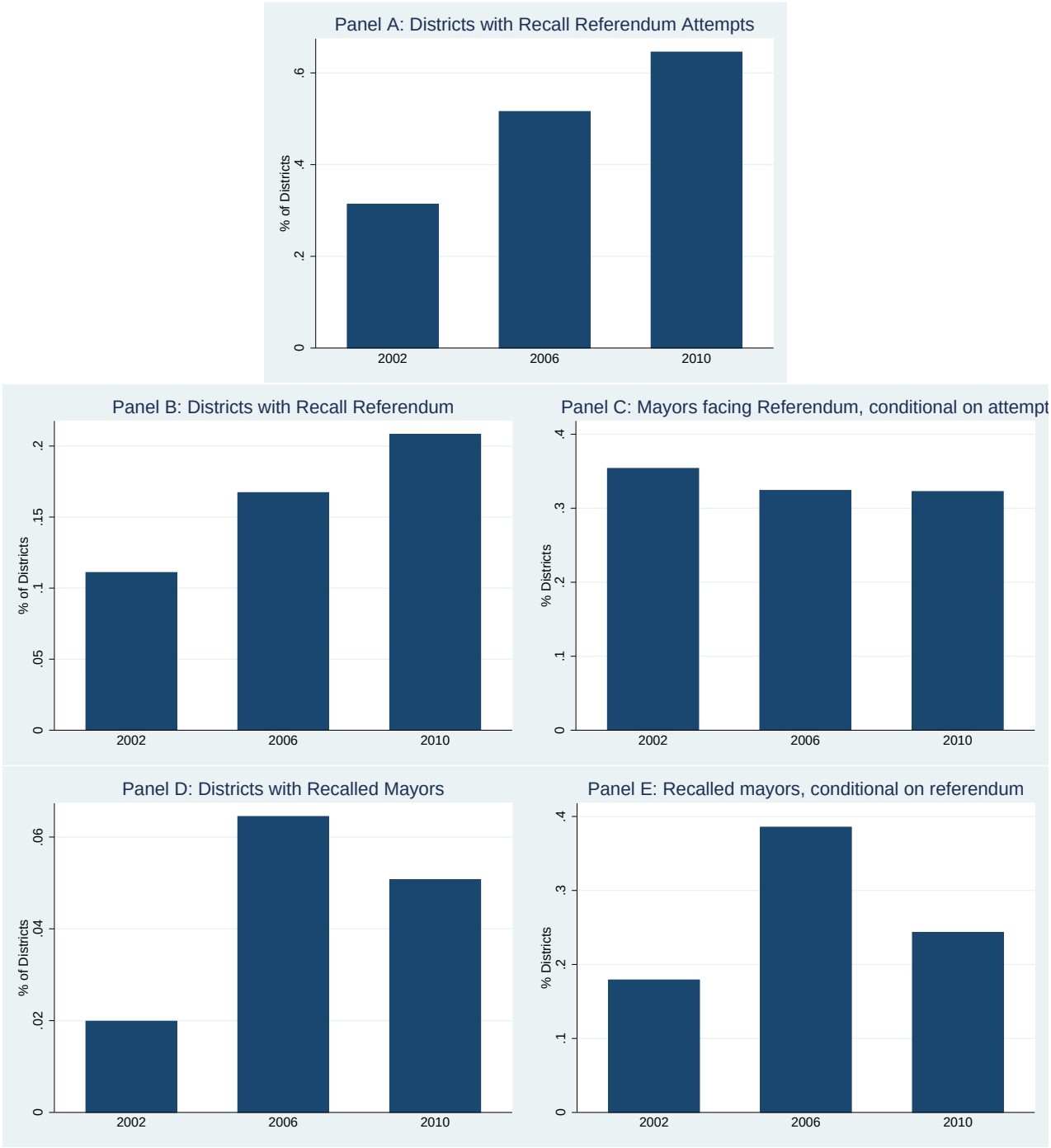
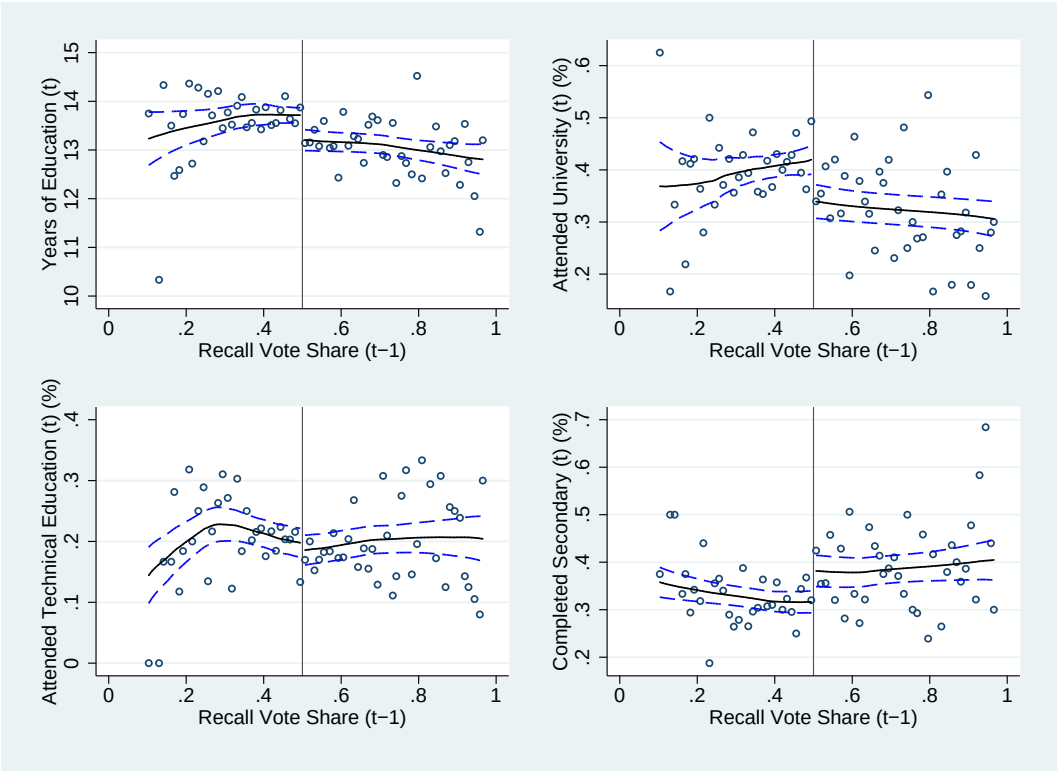


FIGURE 2: Incidence of Recall Referenda



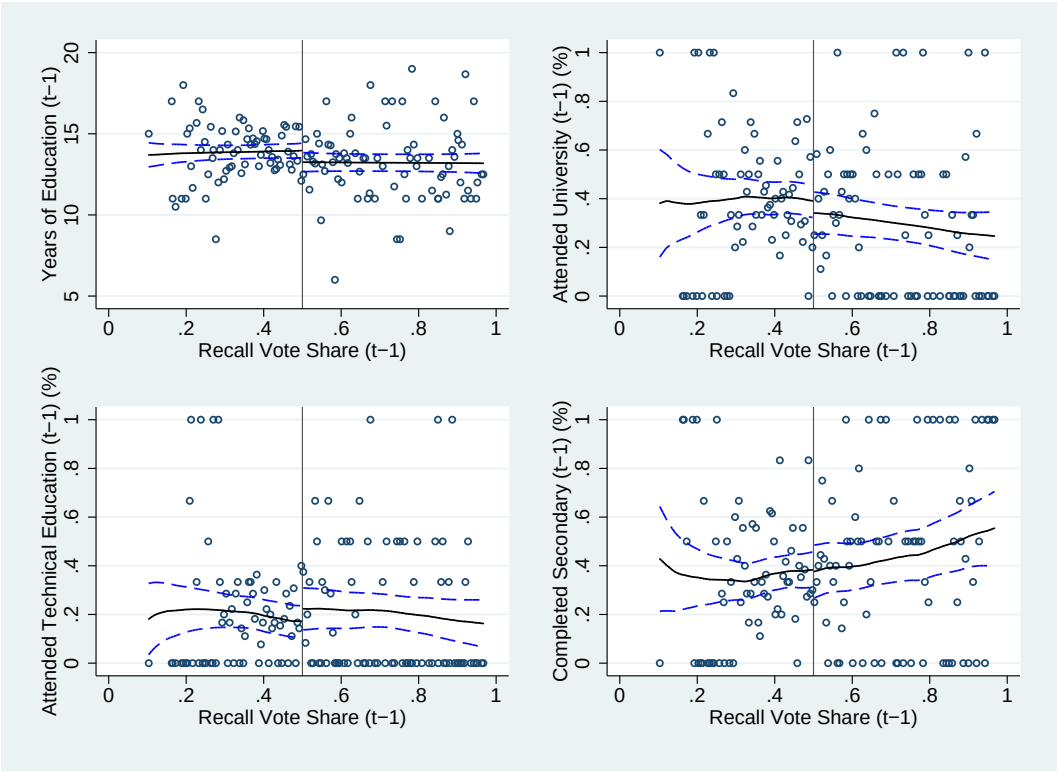
Note: The figures show for each electoral term (A) the proportion of districts in which a recall kit was purchased in order to initiate a recall process against the incumbent mayor, (B) the proportion of districts in which the incumbent mayor faced a recall referendum, (C) the conditional probability of having a recall referendum on the mayor if a recall kit was purchased, (D) the proportion of districts in which the mayor was recalled, and (E) the conditional probability of an incumbent mayor being recalled if a recall referendum took place.

FIGURE 3: Non-Parametric RD Plot: Candidate's Education



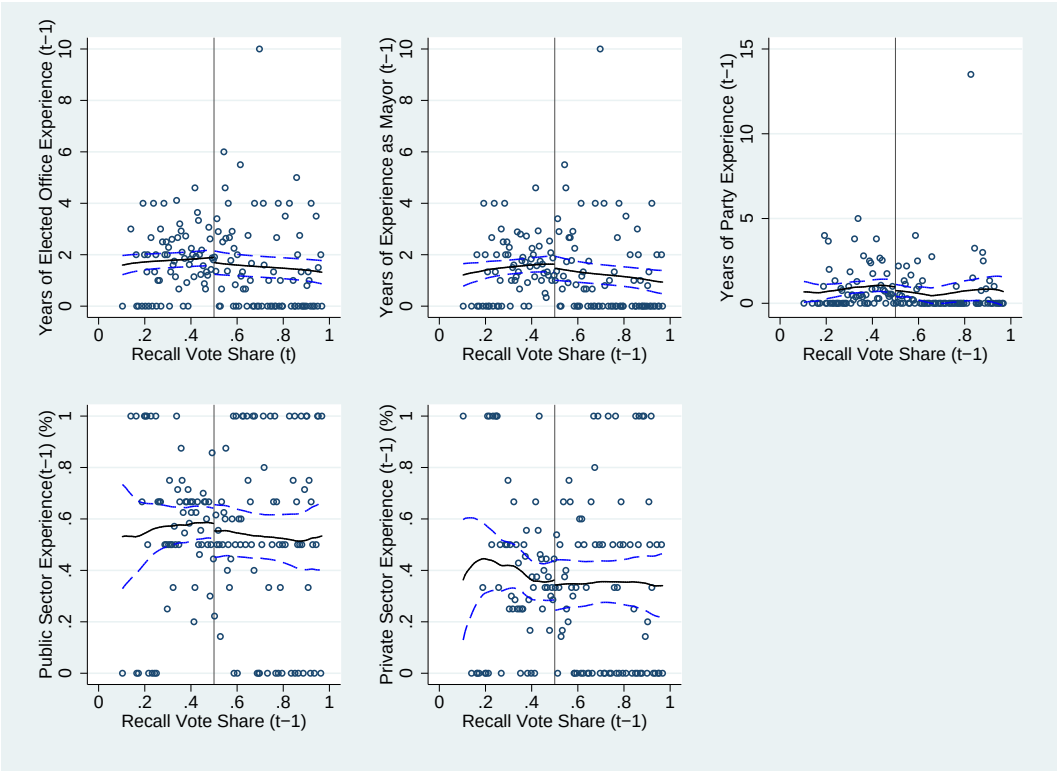
Note: The figures show the results from kernel-weighted local polynomial smoothing plots with epanechnikov kernels and the 95% confidence intervals for our main outcome variables.

FIGURE 4: Continuity Test: Incumbent's Education



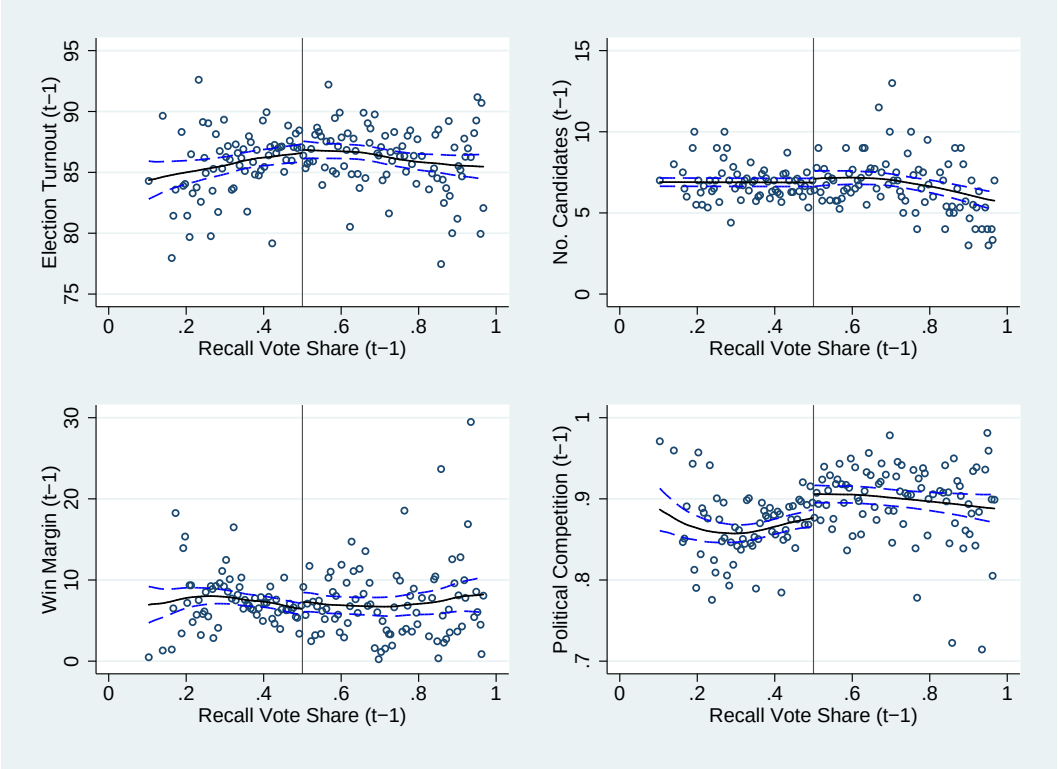
Note: The figures show the results from kernel-weighted local polynomial smoothing plots with epanechnikov kernels and the 95% confidence intervals for incumbents' education.

FIGURE 5: Continuity Test: Incumbent's Experience



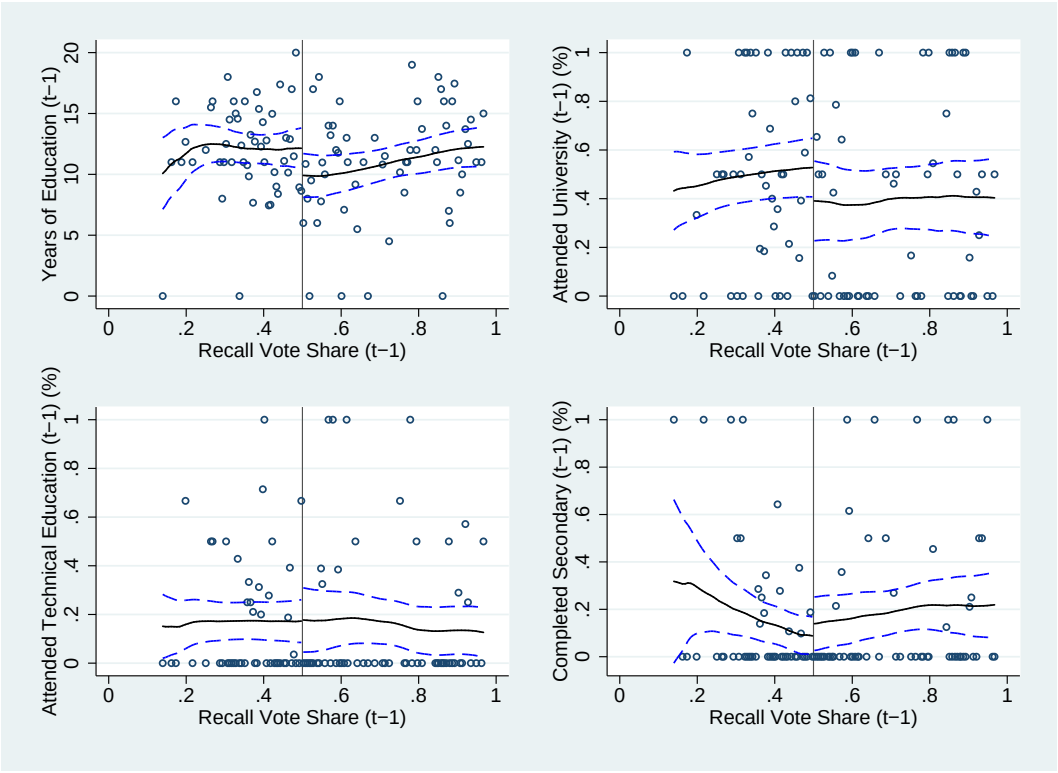
Note: The figures show the results from kernel-weighted local polynomial smoothing plots with epanechnikov kernels and the 95% confidence intervals for incumbents' experience.

FIGURE 6: Continuity Test: Political Variables



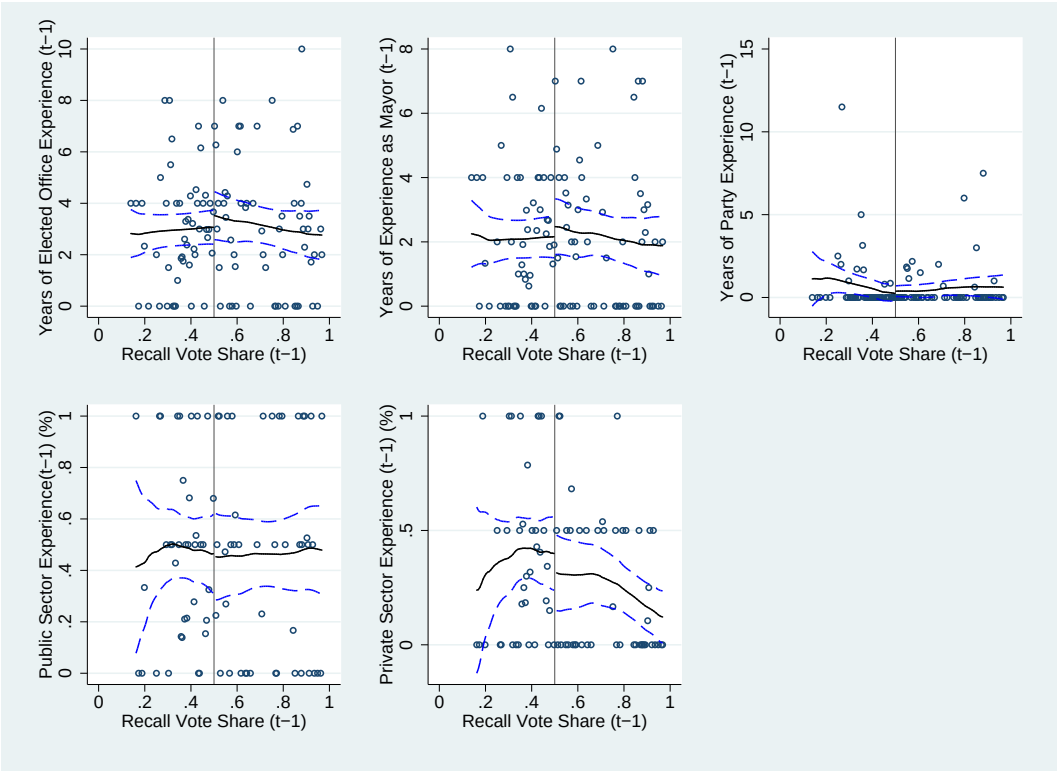
Note: The figures show the results from kernel-weighted local polynomial smoothing plots with epanechnikov kernels and the 95% confidence intervals for political variables.

FIGURE 7: Continuity Test: Runners Up, Education



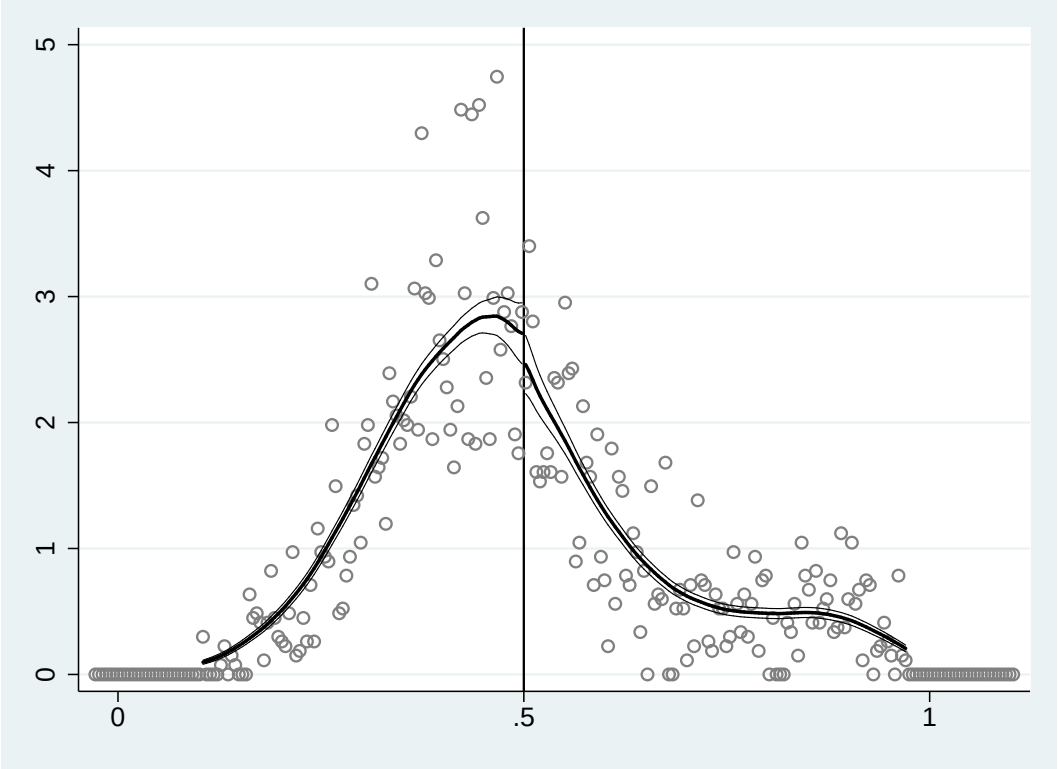
Note: The figures show the results from kernel-weighted local polynomial smoothing plots with epanechnikov kernels and the 95% confidence intervals for the education of candidates who finished second or third in the previous election.

FIGURE 8: Continuity Test: Runners Up, Experience



Note: The figures show the results from kernel-weighted local polynomial smoothing plots with epanechnikov kernels and the 95% confidence intervals for the experience of candidates who finished second or third in the previous election.

FIGURE 9: McCrary Density Test



Note: The figure shows the McCrary test for discontinuities in the density of the running variable (referendum vote share in favour of a recall of the mayor) at the 50% value (McCrary 2008). The estimated density is depicted by the thick black line.

TABLE 1: Predicting Recall Elections

	Dependent Variable: Recall Referendum		
Political Variables			
Win Margin (%)	-0.0042***	-0.0043***	-0.0042***
	(0.0007)	(0.0007)	(0.0007)
Turnout (%)	0.0076**	0.0076**	0.0076**
	(0.0033)	(0.0033)	(0.0033)
Number of Candidates	-0.0081**	-0.0081**	-0.0082**
	(0.0033)	(0.0033)	(0.0033)
Incumbent's Characteristics			
University		0.0083	0.0105
		(0.0069)	(0.0072)
Technical		0.0078	0.0093
		(0.0076)	(0.0077)
Secondary		0.0076	0.0084
		(0.0066)	(0.0067)
Age			0.0002
			(0.0001)
Female			-0.0055
			(0.0038)
Public sector experience			-0.0028
			(0.0029)
Private sector experience			-0.0034
			(0.0027)
Num. years elected office			-0.0005
			(0.0015)
Num. years party experience			0.0001
			(0.0005)
Num. years as mayor			-0.0013
			(0.0018)
National party affiliation			0.0076***
			(0.0029)
Election FEs	Yes	Yes	Yes
District FEs	Yes	Yes	Yes
Observations	17517	17517	17517
Number Districts	1832	1832	1832
Number District×Election	3555	3555	3555

Note: The dependent variable takes value 1 if there was a recall referendum, and 0 otherwise. Clustered standard errors at the district*election level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2: Descriptive Statistics

		Full Sample	RD Sample	Full Sample	RD Sample
		Winners' Characteristics		Candidates' Characteristics	
Primary or less	Mean	0.051	0.059	0.055	0.070
	N	6076	424	37854	2801
Secondary	Mean	0.290	0.317	0.291	0.342
	N	6076	479	37854	3394
Technical	Mean	0.183	0.204	0.186	0.191
	N	6076	597	37854	2962
University	Mean	0.476	0.417	0.468	0.388
	N	6076	593	37854	3698
Years of Education	Mean	14.181	13.833	14.068	13.511
	N	6076	706	37854	3390
Num. years elected office	Mean	2.501	1.937	1.548	1.329
	N	6521	572	41115	2502
Num. years as mayor	Mean	0.908	1.683	0.760	0.897
	N	6521	818	41115	3849
Num. years party experience	Mean	1.966	0.667	0.999	0.618
	N	6521	588	41115	2902
National party affiliation	Mean	0.410	0.375	0.433	0.394
	N	6578	435	42557	3047
Public sector experience	Mean	0.630	0.605	0.588	0.566
	N	5056	522	33818	2093
Private sector experience	Mean	0.417	0.404	0.445	0.427
	N	5056	670	33818	2703
Age	Mean	43.993	44.258	45.629	45.984
	N	6578	539	42557	3058
Female	Mean	0.030	0.050	0.064	0.075
	N	6578	661	42557	4865
At least one native surname	Mean	0.301	0.334	0.301	0.331
	N	6572	605	41641	2028
Two native surnames	Mean	0.080	0.074	0.079	0.062
	N	6572	541	41641	2478
		District Characteristics			
Number of Candidates	Mean	7.415	6.820		
	N	7316	748		
Win Margin (%)	Mean	8.983	8.784		
	N	7250	476		
Eff. Number of candidates	Mean	4.658	4.447		
	N	7243	833		
Turnout (%)	Mean	84.565	86.040		
	N	7315	527		
Ln(Revenues in N. Soles)	Mean	16.502	16.259		
	N	6542	421		
Ln(Expenditures in N. Soles)	Mean	16.252	15.964		
	N	6542	491		
Native mother tongue (%)	Mean	28.792	23.710		
	N	7300	560		

Note: Information on incumbent's characteristics is taken from the CV data of political candidates in Peruvian municipal elections provided by government sources, as described in the data section (3.1). The source for the district characteristics is the Peruvian national electoral office (ONPE). The four columns present the number of observations and the mean values for the main dependent and control variables. Columns 1 and 2 show the characteristics of elected mayors for (i) the full sample and (ii) the RD sample. Columns 3 and 4 show the characteristics of candidates running for mayor (i) in the full sample and (ii) the RD sample. At the bottom of the table, district characteristics are presented for the (i) full sample and (ii) the RD sample. In each case, the RD sample is based on the optimal bandwidth proposed by Imbens and Kalyanaraman (2012).

TABLE 3: Accountability and Candidates' Education

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Local Linear Regression			
Recalled Incumbent in t-1	-0.5241*	-0.0849**	-0.0006	0.0795*
	(0.2964)	(0.0410)	(0.0356)	(0.0479)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	3390	3698	2962	3394
Mean Dep.	13.511	0.388	0.191	0.342
	PANEL B: Linear Polynomial Regression			
Recalled Incumbent in t-1	-0.5398**	-0.0744**	-0.0198	0.0788*
	(0.2655)	(0.0363)	(0.0296)	(0.0405)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	3390	3698	2962	3394
Mean Dep.	13.511	0.388	0.191	0.342
	PANEL C: Quadratic Polynomial Regression			
Recalled Incumbent in t-1	-0.5183*	-0.0652*	-0.0222	0.0882**
	(0.2679)	(0.0365)	(0.0300)	(0.0417)
Quadratic Polynomial	Yes	Yes	Yes	Yes
Observations	3390	3698	2962	3394
Number District×Election	611	679	538	612
Mean Dep.	13.511	0.388	0.191	0.342

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 4: Accountability and Candidate Characteristics

	PANEL A			
	Dependent Variable:			
	Num. years elected office	Num. years as mayor	Num. years party experience	National Party Affiliation
Recalled Incumbent in t-1	-0.3035	-0.3711**	-0.2260	0.0212
	(0.3362)	(0.1859)	(0.2308)	(0.0492)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	2502	3849	2902	3047
Number District×Election	430	666	500	514
Mean Dep.	1.329	0.897	0.618	0.394
	PANEL B			
	Dependent Variable:			
	Public Sector Experience	Private Sector Experience	Age	Female
Recalled Incumbent in t-1	-0.1133**	-0.0389	-1.5026	0.0134
	(0.0522)	(0.0551)	(0.9998)	(0.0178)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	2093	2703	3058	4865
Number District×Election	347	453	515	842
Mean Dep.	0.566	0.427	45.984	0.075

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 5: Accountability and Representation

	Dependent Variable:				
	At least one native surname	Two native surnames	Representative (25 percent)	Representative (50 percent)	Representative (75 percent)
Recalled Incumbent in t-1	-0.0178	-0.0231	-0.0459	-0.0605	-0.1143**
	(0.0709)	(0.0283)	(0.0637)	(0.0693)	(0.0450)
Triangle Kernel	Yes	Yes	Yes	Yes	Yes
Observations	2028	2478	2478	1892	2466
Number District×Election	341	425	425	316	422
Mean Dep.	0.331	0.062	0.151	0.132	0.078

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Local linear non-parametric regressions. In columns 1 and 2, the dependent variables are dummies indicating whether the candidate has at least one or two native surnames (surnames with roots from the Quechuan or Aymaran language families), respectively. In columns 3, 4 and 5, the dependent variables are dummies indicating whether the candidate has at least one native surname in a district where more than 25, 50 or 75 percent of the population has native mother tongue (Quechua or Aymara) as reported in the population census of 2007, respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 6: Mechanisms: Political Opponents and Performance in Office

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Political Opponents preceding Election			
Recalled Incumbent in t-1	-0.2346	-0.0360	-0.0163	0.0473
	(0.2526)	(0.0347)	(0.0312)	(0.0423)
Recalled in t-1 * Political Opponent in t-1	-0.5696*	-0.0793*	-0.0229	0.0704
	(0.3088)	(0.0436)	(0.0349)	(0.0449)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	3385	3693	2957	3389
Number District×Election	610	678	537	611
Mean Dep.	13516	388	191	341
	PANEL B: Performance prior Recall – Budget Executed			
Recalled Incumbent in t-1	-0.5860**	-0.0875**	-0.0339	0.0942**
	(0.2748)	(0.0379)	(0.0305)	(0.0458)
Recalled in t-1 * % Expense Budget Executed	0.1343	0.0276	-0.0203	-0.0068
	(0.1337)	(0.0250)	(0.0161)	(0.0268)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	2565	2791	2237	2565
Number District×Election	384	422	336	384
Mean Dep.	13.439	0.381	0.180	0.355
	PANEL C: Performance prior Recall – Revenues			
Recalled Incumbent in t-1	-0.4393*	-0.0739**	-0.0181	0.0848**
	(0.2525)	(0.0354)	(0.0297)	(0.0415)
Recalled in t-1 * Ln(Revenues)	0.0803	-0.0132	0.0077	0.0131
	(0.1157)	(0.0186)	(0.0136)	(0.0184)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	3184	3450	2777	3188
Number District×Election	559	617	491	560
Mean Dep.	13.543	0.390	0.191	0.341
	PANEL D: Performance prior Recall – Expenditures			
Recalled Incumbent in t-1	-0.4556*	-0.0763**	-0.0185	0.0878**
	(0.2531)	(0.0356)	(0.0297)	(0.0415)
Recalled in t-1 * Ln(Expenditures)	0.0470	-0.0156	0.0065	0.0172
	(0.1202)	(0.0192)	(0.0138)	(0.0192)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	3184	3450	2777	3188
Number District×Election	559	617	491	560
Mean Dep.	13.543	0.390	0.191	0.341

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Linear polynomial regressions. In panels B, C and D, the share of budget executed, ln(revenues) and ln(expenditures) refer to the demeaned version of the variables. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 7: Accountability and Opportunity Costs

	Dependent Variable: Predicted Wage (opportunity cost)	
Recalled Incumbent in t-1	-139.0638***	-137.8319**
	(52.2649)	(58.7260)
Linear Polynomial	Yes	No
Local Linear Regression	No	Yes
Observations	3608	3608
Number District×Election	661	661
Mean Dep.	1234.929	1234.929

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Opportunity costs are imputed based on Enaho survey data on income from individuals' primary job as well as information on observable characteristics that are also available in the mayoral candidates' CV data or can at least be created: age, age-squared, gender, their education level (which can be broken down into categories that correspond to our variables, University, Technical, Secondary, or everything below), as well as a variable on whether they are from an urban or rural area. Column (1) presents linear polynomial regressions. In Column (2), we use a local linear non-parametric regression with triangle kernels. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 8: Accountability and Candidates' Education - Recalled Neighbours

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Local Linear Regression			
Recalled Neighbour Incumbent in t-1	-0.9264***	-0.1327***	-0.0015	0.1301***
	(0.2541)	(0.0400)	(0.0180)	(0.0333)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	6225	5902	10003	5591
Mean Dep.	14.289	0.498	0.185	0.270
	PANEL B: Linear Polynomial Regression			
Recalled Neighbour Incumbent in t-1	-0.8695***	-0.1257***	-0.0076	0.1152***
	(0.2306)	(0.0361)	(0.0156)	(0.0296)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	6225	5902	10003	5591
Mean Dep.	14.289	0.498	0.185	0.270
	PANEL C: Quadratic Polynomial Regression			
Recalled Neighbour Incumbent in t-1	-0.7925***	-0.1094***	-0.0071	0.1033***
	(0.2362)	(0.0364)	(0.0159)	(0.0300)
Quadratic Polynomial	Yes	Yes	Yes	Yes
Observations	6225	5902	10003	5591
Number District×Election	1018	958	1704	895
Mean Dep.	14.289	0.498	0.185	0.270

Note: In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 9: Robustness Checks

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Controlling for Incumbent's Characteristics			
Recalled Incumbent in t-1	-0.4308	-0.0646*	-0.0058	0.0595
	(0.2649)	(0.0382)	(0.0346)	(0.0452)
Triangle Kernel	Yes	Yes	Yes	Yes
Incumbent's Characteristics	Yes	Yes	Yes	Yes
Observations	3377	3685	2949	3381
Number District×Election	610	678	537	611
Mean Dep.	13.497	0.386	0.192	0.343
	PANEL B: Controlling for Political Situation in t-1			
Recalled Incumbent in t-1	-0.4443*	-0.0735*	-0.0055	0.0728
	(0.2563)	(0.0376)	(0.0352)	(0.0451)
Triangle Kernel	Yes	Yes	Yes	Yes
Political Controls	Yes	Yes	Yes	Yes
Observations	3372	3677	2944	3376
Number District×Election	608	675	535	609
Mean Dep.	13.512	0.388	0.190	0.341

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Both panels present local linear non-parametric regression with triangle kernels. Panel A controls for the incumbent's education, experience (political and work experience) and other characteristics (age, gender). Panel B controls for the previous election's turnout, win margin and number of candidates running for mayor, as well as the municipality's population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 10: Robustness Checks

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Dropping re-running Incumbents			
Recalled Incumbent in t-1	-0.4733	-0.0763*	-0.0130	0.0818*
	(0.2906)	(0.0400)	(0.0349)	(0.0476)
Triangle Kernel	Yes	Yes	Yes	Yes
Re-running Incumbents	No	No	No	No
Observations	3063	3460	2939	3060
Number District×Election	609	711	584	608
Mean Dep.	13.488	0.384	0.195	0.348
	PANEL B: Controlling for Characteristics of Runners-up			
Recalled Incumbent in t-1	-0.4622	-0.0837**	-0.0121	0.0878*
	(0.2884)	(0.0411)	(0.0342)	(0.0510)
Triangle Kernel	Yes	Yes	Yes	Yes
Runners Up Characteristics	Yes	Yes	Yes	Yes
Observations	2382	2573	2086	2382
Number District×Election	351	384	309	351
Mean Dep.	13.453	0.381	0.183	0.346

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Both panels present local linear non-parametric regression with triangle kernels. Panel A drops incumbents who return for election from the sample. Panel B controls for the education, experience (political and work experience) and other characteristics (age, gender) of the two runners up in the preceding election. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 11: Accountability and Political Outcomes

	Dependent Variable:			
	Turnout	Candidates	Win Margin	Eff. Num. Candidates
Recalled Incumbent in t-1	-0.7000	0.0947	-1.4477	0.1431
	(1.0067)	(0.3907)	(1.2230)	(0.1839)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	527	748	476	833
Number Districts	425	563	390	608
Mean Dep.	86.040	6.820	8.784	4.447

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Local linear non-parametric regressions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to heterogeneous and serially correlated standard errors.

TABLE 12: Accountability and Winners' Characteristics

	Years of Education	University	Technical	Secondary
	PANEL A	Dependent Variable:		
Recalled Incumbent in t-1	-0.1557	-0.0301	-0.0728	0.1422
	(0.4469)	(0.0861)	(0.0712)	(0.0981)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	706	593	597	479
Number Districts	547	478	480	398
Mean Dep.	13.833	0.417	0.204	0.317
	PANEL B	Dependent Variable:		
	Num. years elected office	Num. years as mayor	Num. years party experience	National Party Affiliation
Recalled Incumbent in t-1	-0.2253	-0.4170	-0.2209	0.0225
	(0.6352)	(0.4208)	(0.3455)	(0.1034)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	572	818	588	435
Number Districts	455	607	466	362
Mean Dep.	1.937	1.683	0.667	0.375
	PANEL C	Dependent Variable:		
	Public Sector Experience.	Private Sector Experience	Age	Female
Recalled Incumbent in t-1	-0.0403	-0.0985	-1.0264	0.0560*
	(0.0772)	(0.0663)	(1.4998)	(0.0318)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	522	670	539	661
Number Districts	430	526	432	516
Mean Dep.	0.605	0.404	44.258	0.050

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Local linear non-parametric regressions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to heterogeneous and serially correlated standard errors.

TABLE 13: Accountability and Policy Outcomes

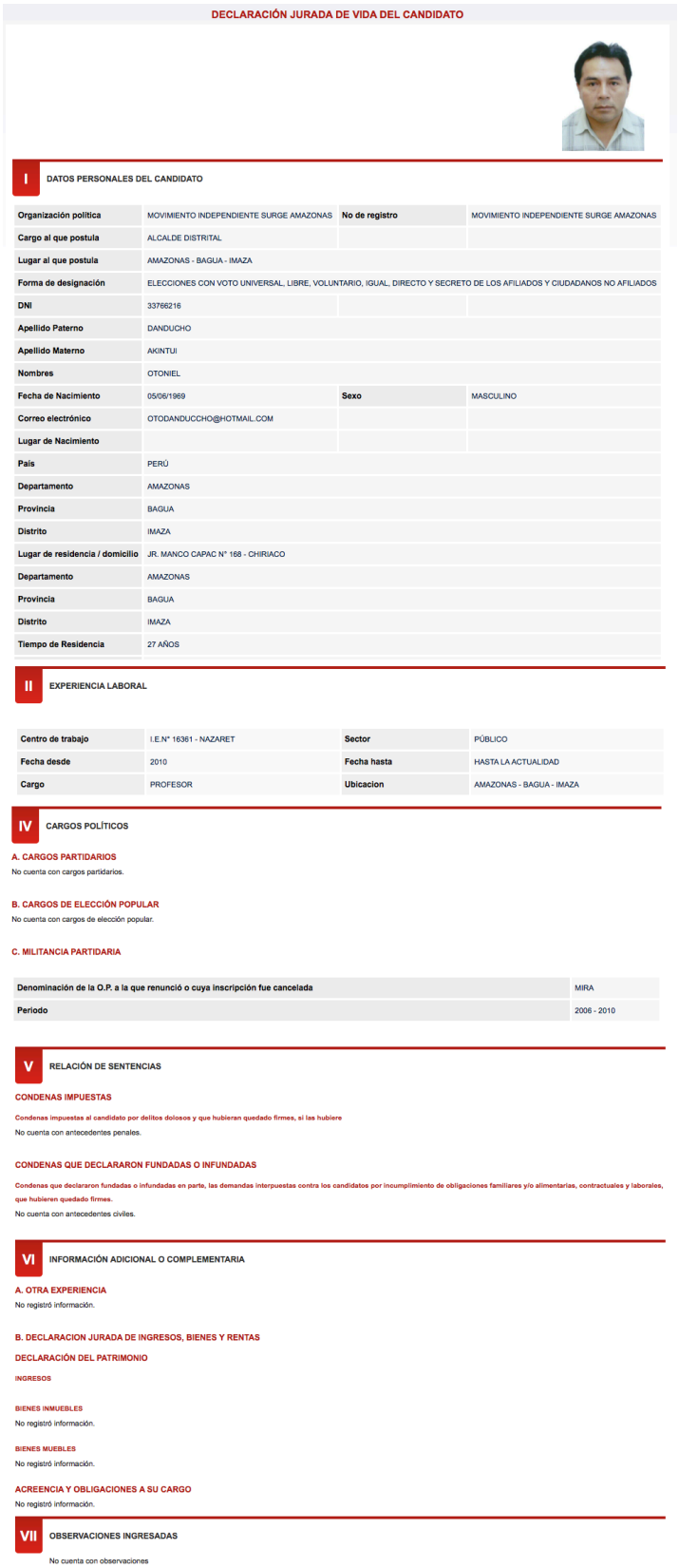
	Dependent Variable:	
	Ln(Expenditures)	Ln(Revenues)
Recalled Incumbent in t-1	-0.4019**	-0.4902**
	(0.1821)	(0.2062)
Triangle Kernel	Yes	Yes
Observations	491	421
Number Districts	402	356
Mean Dep.	15.964	16.259

Note: In column (1), the dependent variable is the log of the budget executed during the last three years of the mayor's term. In column (2), the dependent variable is the log of the total revenues received during the last three years of the mayor's term. The information is provided by the Peruvian Ministry of Economy and Finance. All regressions include district and election fixed effects .* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to heterogeneous and serially correlated standard errors.

Appendix (not intended for publication)

A Appendix - Figures

FIGURE 10: Example from CV data 1 (2014 elections)



DECLARACIÓN JURADA DE VIDA DEL CANDIDATO

I DATOS PERSONALES DEL CANDIDATO

Organización política	MOVIMIENTO INDEPENDIENTE SURGE AMAZONAS	No de registro	MOVIMIENTO INDEPENDIENTE SURGE AMAZONAS
Cargo al que postula	ALCALDE DISTRITAL		
Lugar al que postula	AMAZONAS - BAGUA - IMAZA		
Forma de designación	ELECCIONES CON VOTO UNIVERSAL, LIBRE, VOLUNTARIO, IGUAL, DIRECTO Y SECRETO DE LOS AFILIADOS Y CIUDADANOS NO AFILIADOS		
DNI	33766216		
Apellido Paterno	DANDUCHO		
Apellido Materno	AKINTUI		
Nombres	OTONIEL		
Fecha de Nacimiento	05/06/1969	Sexo	MASCULINO
Correo electrónico	OTODANDUCCHO@HOTMAIL.COM		
Lugar de Nacimiento			
País	PERÚ		
Departamento	AMAZONAS		
Provincia	BAGUA		
Distrito	IMAZA		
Lugar de residencia / domicilio	JR. MANCO CAPAC N° 168 - CHIRIACO		
Departamento	AMAZONAS		
Provincia	BAGUA		
Distrito	IMAZA		
Tiempo de Residencia	27 AÑOS		

II EXPERIENCIA LABORAL

Centro de trabajo	I.E.N° 16361 - NAZARET	Sector	PÚBLICO
Fecha desde	2010	Fecha hasta	HASTA LA ACTUALIDAD
Cargo	PROFESOR	Ubicacion	AMAZONAS - BAGUA - IMAZA

IV CARGOS POLÍTICOS

A. CARGOS PARTIDARIOS

No cuenta con cargos partidarios.

B. CARGOS DE ELECCIÓN POPULAR

No cuenta con cargos de elección popular.

C. MILITANCIA PARTIDARIA

Denominación de la O.P. a la que renunció o cuya inscripción fue cancelada	MIRA
Periodo	2006 - 2010

V RELACIÓN DE SENTENCIAS

CONDENAS IMPUESTAS

Condenas impuestas al candidato por delitos dolosos y que hubieran quedado firmes, si las hubiere

No cuenta con antecedentes penales.

CONDENAS QUE DECLARARON FUNDADAS O INFUNDADAS

Condenas que declararon fundadas o infundadas en parte, las demandas interpuestas contra los candidatos por incumplimiento de obligaciones familiares y/o alimentarias, contractuales y laborales, que hubieren quedado firmes.

No cuenta con antecedentes civiles.

VI INFORMACIÓN ADICIONAL O COMPLEMENTARIA

A. OTRA EXPERIENCIA

No registró información.

B. DECLARACION JURADA DE INGRESOS, BIENES Y RENTAS

DECLARACIÓN DEL PATRIMONIO

INGRESOS

BIENES INMUEBLES

No registró información.

BIENES MUEBLES

No registró información.

ACREENCIA Y OBLIGACIONES A SU CARGO

No registró información.

VII OBSERVACIONES INGRESADAS

No cuenta con observaciones

Source: Example extracted from Infogob.

FIGURE 11: Example from CV data 2 (2010 elections)

DECLARACIÓN JURADA DE VIDA DEL CANDIDATO

I DATOS PERSONALES DEL CANDIDATO

Organización política	DECIDE	**No de registro**	
Cargo al que postula	ALCALDE DISTRITAL		
Lugar al que postula	AREQUIPA - CARAVELI - ACARI		
Forma de designación	ELECCIONES CON VOTO UNIVERSAL, LIBRE, VOLUNTARIO, IGUAL, DIRECTO Y SECRETO DE LOS AFILIADOS Y CIUDADANOS NO AFILIADOS		
DNI	30482933		
Apellido Paterno	YTO		
Apellido Materno	SUCAPUCA		
Nombres	DIONISIO		
Fecha de Nacimiento	26/03/1951	**Sexo**	MASCULINO
Portal del candidato		**Correo electrónico**	
Lugar de Nacimiento			
País	PERÚ		
Departamento	PUNO		
Provincia	SAN ROMAN		
Distrito	JULIACA		
Lugar de residencia / domicilio	AV. RICARDO PALMA 254, MZ-Z, LTE 7		
Departamento	AREQUIPA		
Provincia	CARAVELI		
Distrito	ACARI		
Tiempo de Residencia	42		

II EXPERIENCIA LABORAL

Centro de trabajo	GRIFO EL PORVENIR	**Sector**	PRIVADO
Fecha desde	ENERO - 1975	**Fecha hasta**	OCTUBRE - 1995
Cargo	CHOFER TANQUE SISTERNA	**Detalles adicionales**	
Centro de trabajo	MUNICIPALIDAD DE ACARI	**Sector**	PÚBLICO
Fecha desde	ENERO - 1996	**Fecha hasta**	JULIO - 1998
Cargo	REGIDOR	**Detalles adicionales**	
Centro de trabajo	ANTAMINA TRANSLI	**Sector**	PRIVADO
Fecha desde	OCTUBRE - 1998	**Fecha hasta**	FEBRERO - 2001
Cargo	CHOFER VOLQUETE	**Detalles adicionales**	
Centro de trabajo	CAJAMARCA - TRANSLI	**Sector**	PRIVADO
Fecha desde	MARZO - 2001	**Fecha hasta**	OCTUBRE - 2002
Cargo	CHOFER VOLQUETE	**Detalles adicionales**	
Centro de trabajo	MUNICIPALIDAD DISTRITAL DE ACARI	**Sector**	PÚBLICO
Fecha desde	ENERO - 2003	**Fecha hasta**	DICIEMBRE - 2006
Cargo	ALCALDE	**Detalles adicionales**	

III FORMACIÓN ACADÉMICA

EDUCACIÓN BÁSICA REGULAR

	Estado	Centro educativo
Primaria	CONCLUIDO	MUNICHICO 9427
Secundaria	CONCLUIDO	COLEGIO NACIONAL NICOLAS PIEROLA

EDUCACIÓN SUPERIOR

IV CARGOS POLÍTICOS

A. CARGOS PARTIDARIOS

(No ha ocupado ningún cargo político)

B. CARGOS DE ELECCIÓN POPULAR

(No ha ocupado cargos de elección popular)

V RELACIÓN DE SENTENCIAS CONDENATORIAS IMPUESTAS AL CANDIDATO POR DELITOS DOLOSOS Y QUE HUBIERAN QUEDADO FIRMES, SI LAS HUBIERE

(Sin antecedentes penales)

VI RELACIÓN DE SENTENCIAS QUE DECLARARON FUNDADAS O INFUNDADAS EN PARTE, LAS DEMANDAS INTERPUESTAS CONTRA LOS CANDIDATOS POR INCUMPLIMIENTO DE OBLIGACIONES FAMILIARES Y/O ALIMENTARIAS, CONTRACTUALES Y LABORALES, QUE HUBIERAN QUEDADO FIRMES

(No se cuenta con sentencia firme)

DECLARACIÓN JURADA DE NO TENER SENTENCIA CONDENATORIA VIGENTE

(No tiene condena vigente)

VII MENCIÓN DE LAS RENUNCIAS EFECTUADAS A OTROS PARTIDOS, MOVIMIENTOS DE ALCANCE REGIONAL O DEPARTAMENTA U ORGANIZACIONES POLITICAS DE ALCANCE PROVINCIAL Y DISTRITAL, DE SER EL CASO

Denominación del cargo	SECRETARIO PROVENCIONAL	**Organización política**	PPC
Fecha de ingreso	JULIO - 2003	**Fecha de salida**	FEBRERO - 2010

Source: Example extracted from Infogob.

FIGURE 12: Example from CV data 3 (2006 elections)

REGISTRO DE LAS HOJAS DE VIDA DE LOS CANDIDATOS Y PLANES DE GOBIERNO PARA LAS ELECCIONES REGIONALES Y MUNICIPALES 2006

Constancia generada el día 26/01/2017 a horas 12:01 PM

[Imprimir Reporte] [Cerrar Ventana]

DECLARACIÓN JURADA DE VIDA DEL CANDIDATO

Manifiesto bajo juramento que los datos ingresados en las páginas de mi Declaración Jurada de Vida son fidedignos.

I. Datos personales del candidato

Partido o alianza electoral	ACCION POPULAR
Nro de registro	No hay datos para colocar
Cargo al que postula	ALCALDE DISTRITAL
Lugar al que postula	AYACUCHO - HUAMANGA - CARMEN ALTO
Forma de designación de su candidatura	Designación por órgano conforme al estatuto
Nombres	WALTER RICHARD
Apellidos	ATAURIMA ATAURIMA
Sexo	Masculino
Documento de identidad	DNI 10330734
Fecha de nacimiento	10-03-1964
Correo electrónico	wraa_33@hotmail.com
Página web	No hay datos para colocar
Lugar de nacimiento	
País	PERÚ
Departamento	JUNIN
Provincia	HUANCAYO
Distrito	HUANCAYO
Lugar de residencia y domicilio	
Dirección	Jr. Nueva Generación 123- Vista Alegre
Departamento	AYACUCHO
Provincia	HUAMANGA
Distrito	CARMEN ALTO
Tiempo de Residencia	40 años

II. Formación académica

Educación básica regular :

Primaria completa
Secundaria concluida

Estudios realizados

1 . -

Tipo de centro de estudios	Universitario
Centro de estudios	UNIVERSIDAD NACIONAL SAN CRISTOBAL DE HUAMANGA
Nombre del curso o carrera	INGENIERIA QUIMICA Y METALURGIA
Lugar	AYACUCHO
Fecha de inicio	20-07-1983
Fecha de termino	06-01-1990
Se obtuvo	Grado obtenido
Nombre	BACHILLER EN INGENIERIA QUIMICA Y METALURGIA
Comentario	No hay datos para colocar

2 . -

Tipo de centro de estudios	Técnico
Centro de estudios	INSITUTO SUPERIOR TECNOLOGICO MONSEÑOR VICTOR ALVAREZ HUAPAYA
Nombre del curso o carrera	MECANICA
Lugar	AYACUCHO
Fecha de inicio	03-04-1979
Fecha de termino	12-12-1981
Se obtuvo	Grado obtenido
Nombre	BACHILLER EN MECANICA
Comentario	No hay datos para colocar

III. Experiencia laboral

	Centro de trabajo	Sector	Fecha de ingreso	Fecha de salida	Cargo
1	ORNAMENTOS PRETREOS E.I.R.L	PRIVADO	28-12-2003	11-09-2006	GERENTE GENERAL
2	EPSASA	PUBLICO	07-04-2002	20-04-2003	DIRECTOR

IV. Cargos políticos

a. Cargos partidarios

	Cargo	Ámbito o circunscripción	Partido o alianza electoral	Ingreso	Salida
1	SECRETARIO GENERAL	Distrital	ACCION POPULAR	24-06-2005	11-09-2006

b. Cargos de elección popular

	Cargo	Región/prov./dist.	Proceso electoral	Partido o alianza electoral por la que postulo	Ingreso	Salida
	No hay datos para colocar	No hay datos para colocar	No hay datos para colocar	No hay datos para colocar	No hay datos para colocar	No hay datos para colocar

V. Otra experiencia

	Cargo	Entidad / Institución	Ingreso	Salida
1	PRESIDENTE	CENTRO SOCIAL DEPORTIVO VISTA ALEGRE	02-12-2000	15-08-2002
2	PRESIDENTE	LIGA DISTRITAL DE FUTBOL CARMEN ALTO	20-08-2002	28-04-2006

VI. Antecedentes Judiciales y/o Penales

Materia de la demanda	No hay datos para colocar
Nro expediente	No hay datos para colocar
Juzgado	No hay datos para colocar
Fecha de interposición	No hay datos para colocar
Fecha de sentencia firme	No hay datos para colocar
Nombre del demandante	No hay datos para colocar
Petitorio de la demanda	No hay datos para colocar
Fallo	No hay datos para colocar
Observaciones	No hay datos para colocar

Source: Example extracted from Infogob.

B Appendix - Tables

Table 14: Predicting Recall Attempts

	Dependent Variable: Recall Attempt		
Political Variables			
Win Margin (%)	-0.0065***	-0.0065***	-0.0065***
	(0.0011)	(0.0011)	(0.0011)
Turnout (%)	0.0003	0.0003	0.0002
	(0.0040)	(0.0040)	(0.0040)
Number of Candidates	-0.0024	-0.0024	-0.0026
	(0.0051)	(0.0051)	(0.0051)
Incumbent's Characteristics			
University		0.0000	-0.0006
		(0.0083)	(0.0085)
Technical		0.0009	0.0007
		(0.0092)	(0.0093)
Secondary		0.0006	0.0001
		(0.0081)	(0.0081)
Age			-0.0001
			(0.0002)
Female			-0.0094*
			(0.0054)
Public sector experience			-0.0009
			(0.0038)
Private sector experience			0.0013
			(0.0036)
Num. years elected office			-0.0058***
			(0.0021)
Num. years party experience			-0.0000
			(0.0006)
Num. years as mayor			0.0052**
			(0.0025)
National party affiliation			0.0080**
			(0.0039)
Election FEs	Yes	Yes	Yes
District FEs	Yes	Yes	Yes
Observations	17517	17517	17517
Number Districts	1832	1832	1832
Number District×Election	3555	3555	3555

Note: The dependent variable takes value 1 if there was a recall attempt (the promoter buys a "recall kit"), and 0 otherwise. Clustered standard errors at the district*election level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 15: Overview of Incumbents and Reelection Probabilities

		Probability Running for Reelection	Probability Winning Reelection
Incumbent Recalled	Probability	48.4%	4.8%
	N	250	250
Incumbent survived Referendum	Probability	72.8%	18.6%
	N	644	644
Incumbent faced Recall Petition	Probability	79.7%	20.0%
	N	1806	1806
Incumbent without Recall Process	Probability	68.0%	22%
	N	2787	2787

Note: The table shows the probabilities of incumbents to re-run for election and to win such an election, conditional on various stages of the recall processes during the previous period. Row 1 shows the probabilities of incumbents who were recalled in the previous election term. Row 2 shows the probabilities of incumbents who survived a recall referendum during the previous election term. Row 3 shows the probabilities of incumbents against whom a recall process was initiated but no referendum took place during the previous election term. Row 4 then shows the probabilities for all other incumbents.

TABLE 16: Mayor Characteristics and Policy Outcomes

	Dependent Variable:			
	Ln(Expenditures)		Ln(Revenues)	
Years of Education		0.008***		0.008***
		(0.002)		(0.003)
Secondary	0.036		0.043	
	(0.033)		(0.036)	
Technical	0.055		0.055	
	(0.036)		(0.038)	
University	0.077**		0.085**	
	(0.034)		(0.037)	
Num. years elected office	0.005	0.005	0.009**	0.009**
	(0.004)	(0.004)	(0.004)	(0.004)
Num. years party experience	0.006***	0.006***	0.007***	0.007***
	(0.002)	(0.002)	(0.002)	(0.002)
Num. years as mayor	-0.005	-0.005	-0.008	-0.008
	(0.004)	(0.004)	(0.005)	(0.005)
National party affiliation	-0.004	-0.003	0.005	0.006
	(0.013)	(0.013)	(0.015)	(0.015)
Public sector experience	0.053***	0.049***	0.055***	0.049***
	(0.014)	(0.014)	(0.015)	(0.015)
Private sector experience	0.016	0.016	0.017	0.018
	(0.013)	(0.013)	(0.015)	(0.015)
Age	-0.004***	-0.004***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.002	-0.006	0.015	0.012
	(0.037)	(0.036)	(0.040)	(0.040)
Election FEs	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Observations	4747	4747	4747	4747
Number Districts	1831	1831	1831	1831

Note: In columns (1) and (2), the dependent variable is the log of the budget executed during the last three years of the mayor's term. In columns (3) and (4), the dependent variable is the log of the total revenues received during the last three years of the mayor's term. The information is provided by the Peruvian Ministry of Economy and Finance. All regressions include district and election fixed effects .* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to heterogeneous and serially correlated standard errors.

TABLE 17: Accountability and Candidates' Education - Correlation

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Recalled Incumbent			
Recalled Incumbent in t-1	-0.8955***	-0.1102***	0.0061	0.0739***
	(0.0883)	(0.0126)	(0.0090)	(0.0132)
Observations	37371	37371	37371	37371
Mean Dep.	14.072	0.468	0.185	0.292
	PANEL B: Recall Referendum			
Recall Referendum in t-1	-0.5721***	-0.0754***	0.0194***	0.0379***
	(0.0651)	(0.0087)	(0.0065)	(0.0086)
Observations	37371	37371	37371	37371
Mean Dep.	14.072	0.468	0.185	0.292

Note: OLS estimates. All regressions control for the previous election's turnout, win margin and number of candidates running for mayor. Clustered standard errors at the district*election level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 18: Accountability and Candidates' Education - Specification Checks

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Cubic Polynomial Regression			
Recalled Incumbent in t-1	-0.4090	-0.0777	0.0050	0.0417
	(0.3469)	(0.0479)	(0.0405)	(0.0548)
Cubic Polynomial	Yes	Yes	Yes	Yes
Observations	3390	3698	2962	3394
Mean Dep.	13.511	0.388	0.191	0.342
	PANEL B: Quartic Polynomial Regression			
Recalled Incumbent in t-1	-0.4087	-0.0818*	0.0015	0.0447
	(0.3469)	(0.0481)	(0.0410)	(0.0559)
Quartic Polynomial	Yes	Yes	Yes	Yes
Observations	3390	3698	2962	3394
Number District×Election	611	679	538	612
Mean Dep.	13.511	0.388	0.191	0.342

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 19: Accountability and Candidates' Education - Calonico et al. (2014)

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Bias-Corrected RD Estimates			
Recalled Incumbent in t-1	-0.5231	-0.1176*	0.0610	0.0510
	(0.3953)	(0.0631)	(0.0527)	(0.0635)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	3417	3143	2637	3568
Mean Dep.	13.519	0.393	0.192	0.344
	PANEL B: Linear Polynomial Regression			
Recalled Incumbent in t-1	-0.5449**	-0.0740*	-0.0100	0.0712*
	(0.2636)	(0.0400)	(0.0322)	(0.0392)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	3417	3143	2637	3568
Mean Dep.	13.519	0.393	0.192	0.344
	PANEL C: Quadratic Polynomial Regression			
Recalled Incumbent in t-1	-0.5415**	-0.0736*	-0.0139	0.0740*
	(0.2657)	(0.0407)	(0.0326)	(0.0402)
Quadratic Polynomial	Yes	Yes	Yes	Yes
Observations	3417	3143	2637	3568
Number District×Election	617	567	486	654
Mean Dep.	13.519	0.393	0.192	0.344

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Calonico et al. (2014). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 20: Accountability and Candidates' Education - Arbitrary Bandwidth

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Linear Polynomial Regression			
Recalled Incumbent in t-1	-0.5415	-0.1556*	0.0532	0.1065
	(0.5074)	(0.0801)	(0.0640)	(0.0847)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	792	792	792	792
Mean Dep.	13.451	0.383	0.186	0.366
	PANEL B: Quadratic Polynomial Regression			
Recalled Incumbent in t-1	-0.5599	-0.1495*	0.0447	0.1107
	(0.5034)	(0.0804)	(0.0644)	(0.0852)
Quadratic Polynomial	Yes	Yes	Yes	Yes
Observations	792	792	792	792
Mean Dep.	13.451	0.383	0.186	0.366
	PANEL C: Cubic Polynomial Regression			
Recalled Incumbent in t-1	-0.4218	-0.1940*	0.0632	0.1526
	(0.6156)	(0.1040)	(0.0883)	(0.1090)
Cubic Polynomial	Yes	Yes	Yes	Yes
Observations	792	792	792	792
Number District×Election	137	137	137	137
Mean Dep.	13.451	0.383	0.186	0.366

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on an arbitrary bandwidth of 3 percentage points above and below the threshold. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 21: Continuity Tests: Incumbent's Education and Experience

	PANEL A			
	Dependent Variable:			
	Years Edu	University	Technical	Secondary
Recalled Incumbent	-0.3745	-0.0240	0.0229	-0.0436
	(0.5922)	(0.1054)	(0.1050)	(0.0845)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	552	401	340	552
Number Districts	458	350	302	458
Mean Dep.	13.763	0.379	0.179	0.368
	PANEL B			
	Dependent Variable:			
	Num. years elected office	Num. years as mayor	Num. years party experience	National Party Affiliation
Recalled Incumbent	0.2110	0.1949	-0.1317	-0.1521
	(0.7045)	(0.5710)	(0.3648)	(0.1250)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	447	423	484	339
Number Districts	378	362	403	299
Mean Dep.	1.949	1.704	0.808	0.490
	PANEL C			
	Dependent Variable:			
	Public Sector Experience	Private Sector Experience	Age	Female
Recalled Incumbent	-0.0709	0.0309	-2.1327	-0.0164
	(0.1026)	(0.1141)	(1.7533)	(0.0537)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	420	355	400	458
Number Districts	379	326	346	387
Mean Dep.	0.576	0.346	46.002	0.059

Note: In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Local linear non-parametric regressions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to heterogeneous and serially correlated standard errors.

TABLE 22: Robustness Checks

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	Placebo: Recalled Incumbent in t-2			
Recalled Incumbent in t-1	-1.3093***	-0.0849	-0.0753	0.0754
	(0.4898)	(0.0768)	(0.0538)	(0.0818)
Recalled Incumbent in t-2	0.1773	0.0689*	-0.0397	-0.0434
	(0.2918)	(0.0383)	(0.0301)	(0.0392)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	780	854	692	784
Number District×Election	134	150	121	135
Mean Dep.	13.447	0.390	0.188	0.329

Note: Regression equations follow Equation (1) in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Linear polynomial regressions. The regressions control for the lag of the explanatory variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.

TABLE 23: Accountability and Candidates' Education - Robustness for Recalled Neighbours

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
	PANEL A: Local Linear Regression			
Recalled Neighbour Incumbent in t-1	-1.0810***	-0.1625***	-0.0109	0.1726***
	(0.2743)	(0.0423)	(0.0202)	(0.0346)
Triangle Kernel	Yes	Yes	Yes	Yes
Observations	6147	6364	8388	5618
Mean Dep.	14.395	0.514	0.184	0.257
	PANEL B: Linear Polynomial Regression			
Recalled Neighbour Incumbent in t-1	-0.8391***	-0.1329***	-0.0210	0.1620***
	(0.2405)	(0.0353)	(0.0173)	(0.0306)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	6147	6364	8388	5618
Mean Dep.	14.395	0.514	0.184	0.257
	PANEL C: Quadratic Polynomial Regression			
Recalled Neighbour Incumbent in t-1	-0.9536***	-0.1394***	-0.0295*	0.1708***
	(0.2627)	(0.0390)	(0.0176)	(0.0337)
Quadratic Polynomial	Yes	Yes	Yes	Yes
Observations	6147	6364	8388	5618
Number District×Election	1042	1090	1449	929
Mean Dep.	14.395	0.514	0.184	0.257

Note: In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Districts that received conflicting signals (more than one neighbour district with recall referendum in $t-1$, resulting in one recalled incumbent and one non-recalled incumbent) are excluded from the sample. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the district*election level.