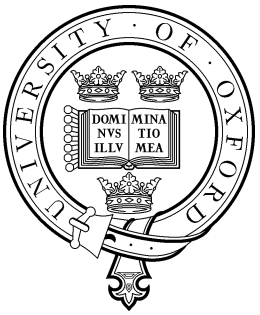
Essays on the Political Economy of Development: the Role of Democracy and Institutions



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Abstract

Political institutions shape the structure and incentive systems of economies and are therefore essential for development. This thesis consists of three chapters that analyse and empirically study questions of different topics on the political economy of economic development, focussing on accountability, political capture, and democracy more generally. Chapter 1 analyses the effects of increases in the salience of electoral accountability on selection into politics in a context where accountability mechanisms are prone to political capture. The analysis examines recall referenda in Peruvian municipalities and demonstrates that candidates in municipalities where a recall election took place are on average less educated and less experienced. Furthermore, elected mayors in those municipalities exhibit less effort in office. The findings can be explained by political capture of recall referenda, which discourages high-quality individuals from competing for office and also reduces incentives for incumbents in office. Chapter 2 explores strategic behaviour of non-state actors during a prolonged transition period. The chapter provides evidence that non-state actors in Colombia strategically exploit the period between peace negotiations and demobilisation to expropriate land in order to consolidate their future economic and political power. Weak state capacity, relatively far-reaching impunity, and the multi-year length of the transition period allow non-state actors to influence the transition process in their interest and to pursue personal gain. In Chapter 3, I empirically examine the relationship between democracy, government size, and redistribution in a global country panel. Using within-country time series variation in dynamic panel model regressions and instrumental variable estimation, I find that democratisation is associated with significant increases in redistribution but not with higher government expenditure or taxation per se. The effects are considerable in the long run, leading to an increase in redistributive taxes and expenditure of up to 20%. Democratisation's impact is, however, heterogeneous. I explore and discuss a number of dimensions along which the effects on redistribution vary.

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Contents

1 Accountability, Political Capture, and Selection into Politics 9

1.1 Introduction . . . 9

1.2 Institutional background . . . 15

1.2.1 Local governments in Perú . . . 15

1.2.2 Recall elections . . . 16

1.3 Data and empirical strategy . . . 20

1.3.1 Data sources . . . 20

1.3.2 Descriptive statistics . . . 22

1.3.3 Empirical strategy . . . 24

1.4 Results: accountability and candidate selection . . . 27

1.4.1 Candidates' education and experience . . . 27

1.4.2 Quality of candidates: suitability of education . . . 29

1.4.3 Robustness checks . . . 32

1.4.4 Regression discontinuity of recall election vote shares . . . 35

1.5 Theoretical framework and mechanisms . . . 36

1.5.1 Theoretical framework . . . 36

1.5.2 Candidate entry or exit? . . . 38

1.5.3 Mechanisms . . . 39

1.5.4 Do recall referenda lead to lower quality mayors? . . . 41

1.5.5 Are politicians' policy choices altered? . . . 42

1.6 Summary and discussion . . . 44

Tables and figures . . . 74

Appendix . . . 75

2 Strategic Expropriation and Civil War 81

2.1 Introduction . . . 81

2.2 Background . . . 87

2.2.1 Paramilitaries in Colombia: history, evolution, and motivation 88

2.2.2 The peace negotiations and demobilisation process . . . 90

2.3 Data: description and summary statistics . . . 93

2.3.1 Main outcome variable: land restitution claims . . . 93

2.3.2 Explanatory variables: timing of demobilisation and presence of paramilitaries . . . 97

2.3.3 Descriptive statistics . . . 100

2.3.4 Conceptual framework . . . 100

2.4 Empirical strategy . . . 103

2.4.1 Demobilisation: a natural experiment? . . . 103

2.4.2 Difference-in-differences estimation . . . 106

2.5 Discussion of results . . . 110

2.5.1 Strategic expropriations . . . 110

2.5.2 Expropriations and land characteristics . . . 112

2.5.3 Mechanisms: plot size, military competition, and centrality . . 114

2.6 Robustness checks . . . 117

2.6.1 Extended sample of municipalities . . . 120

2.7 Conclusion . . . 122

Tables and figures . . . 159

Appendix . . . 160

3 Democracy, Government Size, and Redistribution 168

3.1 Introduction . . . 168

3.2 Democracy in the literature . . . 171

3.2.1 Democracy, political institutions, and the size of government . 172

3.2.2 Economic, demographic, and social determinants of government size . . . 175

3.3 Data and definitions . . . 178

3.3.1 Government finance statistics 178
3.3.2 Democracy and democratisation 180
3.3.3 Other variables 181
3.3.4 Descriptive statistics 182
3.4 Empirical framework and identification 184
3.4.1 Fixed effects estimation 184
3.4.2 Instrumental variable estimation 187
3.5 Results and discussion 188
3.5.1 Fixed effects estimation results 189
3.5.2 Instrumental variable approach 193
3.5.3 Heterogeneity in the impact of democracy 196
3.6 Summary and discussion 201
Tables and figures 231
Appendix 232

Introduction

> *Institutions provide the incentive structure of an economy; as that structure evolves, it shapes the direction of economic change towards growth, stagnation, or decline.* — Douglas North, *Institutions*

Institutions play an essential role for economic development. In the old quest of explaining why some countries are rich and others poor, the literature in economic history, political economy, and development economics has identified differences in current institutions and different legacies of historical institutions as an important part of the answer.

State institutions help to explain the rise of successful economies (North, 1991; North and Thomas, 1973) and divergence between former colonies, where different colonial institutions where installed (Acemoglu et al., 2001). Even within the same country, differences in the quality of local institutions or persistence of past institutions can explain economic and socio-economic discrepancies (Dell, 2010; Michalopoulos and Papaioannou, 2013). Institutions, as defined by North (1990), encompass both informal constraints as well as formal rules. Such rules and constraints are definitive characteristics of any political system, but especially of democracies, which have clear established norms, laws, and constitutions.

In this thesis, I examine the role of different political institutions in democracy and also analyse the political economy of regime transitions. The former influence the way democracies function and, ultimately, affect their economic success (Acemoglu and Robinson, 2012). The latter are critical junctures. On the one hand,

institutions are often shaped when new regimes emerge, on the other hand, periods of transition are particularly vulnerable to institutional weaknesses that can be exploited by vested interest groups or elites who aim to consolidate their power.

This thesis consists of three self-contained chapters. The first chapter analyses the effects of increases in the salience of electoral accountability on selection into politics, i.e. the decision of potential candidates whether they should run for political office or not. In its analysis, Chapter 1 investigates how accountability mechanisms can be prone to be captured in environments of weak state capacity so that they backfire despite being well-intentioned. The second chapter examines how prior knowledge of a pending peace settlement to bring civil war to an end can have an impact on strategic behaviour of demobilising non-state actors. The analysis focuses on the Colombian civil war, during which non-state actors' economic, military, and political influence undermine the state's monopoly on power and the rule of law, in many cases supplanting the state in specific areas of the country. As a consequence, non-state actors appear to be able to exploit a prolonged transition to further consolidate their influence. The third chapter analyses the relationship between democracy and redistribution. If democracy is associated with an enfranchisement of poorer segments of society, then democratisation should lead to higher levels of redistribution. Yet, we observe that democracy is often compatible with high levels of inequality. I empirically study this question, while clearly distinguishing measures of redistribution and measures of government size, which can represent government spending or service provision that do not pursue a redistributive or equalising objective.

Although each chapter is self-contained and addresses distinct political economy questions, all three chapters have a number of overarching topics in common. In particular, they highlight the importance of political institutions. Furthermore, they demonstrate that (i) accountability mechanisms are relevant in democracies but need to be designed wisely and (ii) all types of state institutions and political systems can be captured by special interest groups such as elites. In a broader sense, each

chapter, in its own way, also poses the question of who actually influences politics and by what means.

In Chapter 1, I estimate the effects of the salience of political accountability on the selection of politicians when accountability mechanisms are prone to political capture. I use a comprehensive and novel dataset that records characteristics of candidates for mayor in the last three local elections in Peru. Exploiting within-district time variation and a regression discontinuity, I compare candidates that decide to run for mayor in districts where a recall referendum took place in the previous electoral term - increasing the salience of accountability and decreasing the expected term length in office - with those who run in districts that did not have a recall election. Candidates running for office in districts where the risk of recall is perceived to be higher are less educated and have less experience working in the public sector and holding public office. Furthermore, they have lower opportunity costs, as measured by imputed wages.

These findings are consistent with a political agency model in which accountability mechanisms are prone to capture, which distorts their main objective of improving the quality of government. The chapter provides anecdotal and statistical support for such political capture of recall elections. Despite the negative selection, voters still select the most qualified politicians among the available options. However, the use of recall elections as a political tool creates an environment that not only incentivises mayors (and politicians more generally) to constantly campaign but that also discourages long-term planning and investments since short-term results become more valuable politically. In fact, evidence suggests that the political capture of recall elections has negative consequences on mayors' policies in Peruvian municipalities.

Accountability is at the heart of any functioning democracy. It establishes clear rules for those who govern and sets limits for capriciousness of those in office. When fundamental accountability mechanisms get captured and misapplied, as documented in Chapter 1 with regard to recall elections in Peru, it therefore

amounts to not only a weakening of voters' control over politicians but also erodes the very basis of democracy.

The second chapter examines the behaviour of non-state actors during the transition from civil war to peace in a context with weak state capacity and where the state's monopoly on violence is severely undermined. In particular, the chapter analyses what happened at the end of the civil conflict in Colombia with non-state actors, while peace negotiations were ongoing.

The Colombian civil war has been the longest lasting conflict in recent history and is thus the epitome of persistent wars. The analysis in Chapter 2 investigates whether the nature of the conflict termination inadvertently created incentives for non-state actors to commit violence. In particular, it analyses whether right-wing paramilitaries strategically responded to peace negotiations in anticipation of their inevitable subsequent demobilisation. Using completely novel data on the precise demobilisation process of paramilitaries, I employ a difference-in-differences estimation to demonstrate that paramilitaries take strategic actions to consolidate their power before laying down their arms. I find that they increase their expropriation of land in the years preceding their demobilisation. They focus such violence on areas with higher quality land, i.e. agriculturally more productive land. These results are consistent with the notion that their actions and the violence are primarily motivated by economic incentives.

The findings in Chapter 2 are important as the nature of conflict termination is decisive for the sustainability of peace agreements and the likelihood of conflict recurrence, which would constitute an enormous impediment to economic development. The findings of the chapter also emphasise the sensitive nature of regime transition periods. I demonstrate that prolonged transition periods can give rise to strategic actions of forces such as former elites or non-state actors through which they weaken and capture state institutions.

The third chapter of this thesis investigates the relationship between democracy, government size, and redistribution. Using detailed disaggregated public finance data for around 150 countries, I exploit within-country time series variation to identify the effects of democratisation. My findings point out that the distinction between crude measures of government size and measures of redistribution matters, a point mostly overlooked in existing work, which equates total expenditure or total tax revenue with redistribution. While democratisation leads to an increase in redistribution, both through higher taxes on income, profits, and capital gains as well as expenditure on education, health, and housing, total government size is unaffected. Neither total tax revenue nor total government expenditure change following democratisation. In contrast, my results suggest that democratisation's long-run effects on redistributive taxation and spending approximately correspond to increases of 20% and 15%, respectively.

I address concerns about the potential endogeneity of democratisations by also employing an instrumental variable estimation, which qualitatively confirms the results obtained from fixed effects dynamic panel estimation. Additionally, the chapter reveals that democratisation's impact on public finance policies is heterogeneous across countries, underlining the relevance of different forms of democratisation, design of political institutions, and cohesion in society. I demonstrate that factors such as electoral accountability, ethnic fractionalisation, or persistent influence of former elites all have an impact on whether a regime change to democracy is associated with higher redistribution or not. In ethnically fractionalised societies, democratisation's impact on redistribution is subdued. A lack of electoral accountability, arising from irregular transfers of power, appears to lead to increased but less redistributive government expenditure. Finally, democratisations only cause higher redistribution if they were preceded by a revolution that weakened former elites' grip on power.

Chapter 3 revisits the inconclusive empirical literature on democracy and redistribution and provides new robust and causal evidence based on the most comprehensive and reliable data available. Beyond the questions of redistribution, the chapter's analysis sheds light on and alludes to multiple dimensions that effectively

co-determine electoral influence and decision-making in democracies such as the distribution of de-facto political power, the link between economic and political influence, the persistence of elites, ethnic and social cohesion, and the design of institutional constraints on the executive.

While the three chapters of this thesis examine different political economy questions, they have in common that each of them highlights fragile aspects of political institutions and vulnerability of democracy in general. In democracies, political institutions can be captured or undermined by pervasive conflict or grievances, all of which potentially result in adverse consequences such as impairing the functioning of democracy, inhibiting economic growth, or eroding trust in the state and public institutions. As a consequence, the questions that the chapters of this thesis aim to answer are relevant for a number of reasons. First, they elicit new insights, in the form of empirical evidence, that can further our understanding of the political economy of economic development. Additionally, the underlying analysis and resulting findings also have implications for future research as well as for policy making.

In various democracies, high levels of inequality are attributed to fostering discontent. Studying what forces determine or inhibit redistribution is therefore important. Many democratic countries also recently experienced a rise in electoral support for populist parties as voters turn away from traditional political parties. One reason for such disillusionment seems to be that citizens feel that there is a disconnect between the electorate and politicians, in other words there seems to be a lack of accountability. This thesis studies recall elections as a mechanism to enhance accountability. The lessons learned from the empirical evidence can contribute to improving existing mechanisms or contribute to designing more robust new mechanisms that are less prone to capture. For instance, recall processes could be combined with a monitoring system for alleged reasons for a recall initiative, putting the burden of proof on the promoter. Such a modification could yield a fall in politically motivated recall elections. Similarly, state capture is a problem that is pervasive in many democracies. The work presented in Chapter 2 shows that transition periods,

here illustrated in the form of a transition from civil conflict to a peace settlement in Colombia, are particularly vulnerable to such capture. The experience in Colombia, which set low constraints for paramilitaries to commit crimes, might be instructive for future peace settlements, especially with regard to land restitution processes and criminal procedures against former insurgents. Overall, the evidence from Chapters 1 and 2 could be used by policymakers to build more resilient institutions.

Future research is needed to assess the long-term impact of the phenomena of political capture that we document. In particular, the following two questions could inform future research: (i) Are there negative long-term effects through "perverted" recall processes on factors such investments that yield long-term rather than short-term rents? (ii) Does the documented appropriation of land still today yield economic and political influence to ex-paramilitaries and if so, has this undermined general trust and attitudes towards the central government that has let perpetrators escape with impunity? With regard to Chapter 3, it would be worthwhile to devote more research to factors that influence demand for redistribution in democracies. For example, investigating the role of inequality and perception of inequality could reveal valuable insights on how redistribution is determined in societies.

Overall, the findings of this thesis contribute to our understanding of political economy elements that can stimulate or inhibit economic development and thus welfare.

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Chapter 1

Accountability, Political Capture, and Selection into Politics

1.1 Introduction

In most democratic systems, different mechanisms and institutions can be used to increase voters' control over politicians' actions, 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 or punishing the inefficient or corrupt ones. However, these institutions not only affect politicians' actions, but also their selection: by holding them accountable, they also change the expected value of office (e.g. less rent extraction opportunities). The way accountability institutions are designed and the extent to which they can be captured or manipulated by political elites or interest groups can distort these objectives. For example, if the press is controlled by economic elites, it can highlight information that punishes efficient and honest politicians, which is taken as a signal for others not to consider the post, but who might have otherwise run for office in the future.

While most political agency models predict that the information asymmetry between politicians and voters has effects on the incidence of both moral hazard and adverse selection[1], the empirical work analysing the effects of accountability institutions have focused on the former. However, the selection of politicians who decide

[1]See the seminal works of Barro (1973) and Ferejohn (1986); Besley (2007) and Persson and Tabellini (2000) provide great reviews of these models.

to run for office is as important as their behaviour, since their honesty, competence and motivation determine the quality of public policies implemented, either directly (Martinez-Bravo, 2017; Besley et al., 2011, 2005) or through its effects on political competition and more generally on the political equilibrium (Besley, 2007; Acemoglu et al., 2013; Besley et al., 2010). In this paper, we study the effects of political accountability on the selection of politicians who decide to run for office, and show how these institutions can lead to negative selection when they are prone to capture by political interest groups.

Using a comprehensive dataset on the characteristics and background of candidates running for mayor in the last three rounds of the municipal elections in Peru and exogenous variation in the level of accountability, we investigate the effects of an increase in the expected probability of being recalled from elected office on the selection of politicians. Recall referenda are a direct democracy mechanism that allows voters to oust elected mayors and their councillors. In the Peruvian context, it has been documented that this institution is often used as a political tool, with candidates that lost in a previous election often being the initiators of recalls. A recall referendum in a municipality signals potential candidates that they could be removed from office, even independent of their actions, thus affecting their priors on the expected length of their term.

Our estimation strategy compares the characteristics of candidates running for mayor in districts that had a recall referendum in the previous electoral period with those that did not. Recall promoters need to collect valid signatures of 25 percent of eligible voters. The signatures have to be validated by the national registry, and even after multiple attempts, some requests fall just short of having enough signatures, therefore not leading to a recall referendum, while others are barely successful. The fact that we have multiple electoral periods allows us to make within district comparisons between periods in which a recall referendum barely took place with those in which it did not. Our findings show that in treated municipalities, candidates who decide to run for office have, on average, up to one year less education, are between 12 and 15 percentage points less likely to be university educated, and instead the

proportion of candidates with technical or secondary education is higher. Looking at other dimensions that are presumably correlated with a politician's quality, we find that candidates in municipalities where a recall referendum took place also have less experience in elected office and in the public sector, and in particular, they are less likely to have been mayors in the past. Furthermore, politicians who decide to run for office face less political competition, and win elections by a larger margin in districts where a recall elections took place in the previous electoral period.

We argue that the negative selection takes place because recall referenda can be captured and used to benefit politicians in the opposition. The political use of this direct democracy mechanism reduces the correlation between politicians' actions and the probability that they are recalled from office, hence candidates with high opportunity costs of holding office, or those who derive high levels of utility from carrying out a policy agenda, select out of the pool of candidates. 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.

Finally, in our empirical analysis, we investigate whether having a lower quality pool of candidates leads to actually having lower quality mayors and worse policy outcomes. Our findings, though suggestive due to a reduced sample size, show that on average, voters still select the most qualified politicians from a pool of lower average quality candidates, underscoring the important role played by elections in choosing the best candidates for the job. However, local public service provision and incumbent mayors' effort in office are negatively affected.

The specific design of the recall process generates threats to our identification strategy. Recall proposers who end up with valid signatures that amount to less than 25 percent of the electorate (after the registry's revision) have a chance of submitting new batches of signatures until either they reach the required number or a deadline is reached. While all proposers have incentives to collect as many signatures as possible to hold a recall referendum, some fall just short of achieving the goal, hence the mass of observations above the 25 percent threshold is larger than the one below. Sorting into treatment would represent a threat to our identification strategy

if there are time-variant characteristics of the recall election that cause both that the threshold is met and affect the quality of the candidates running for elections. We run a battery of robustness checks to specifically address this concern. Covariates are balanced around the threshold, which rules out selection on observables. More importantly, in our main results we use district specific fixed effects, which allows us to rule out the possibility of consistently having districts with high probability of recalls or low ability politicians. Our results are also robust to the exclusion of all incumbents running for reelection, which alleviates concerns that there is a mechanical effect of eliminating high quality politicians from the race that drives our result. Furthermore, our results are robust to the inclusion of incumbent characteristics and variables related to political competition. Finally, we also examine the second stage of recall processes, the recall election itself, with another regression discontinuity design. This setup is not affected by a potential sorting into treatment and confirms our findings that mayoral candidates in districts with a recall election in the previous period - in this case a recalled mayor in the previous period - are of lower quality. Overall, the set of robustness checks implemented provide assurance that our results are not driven by selection.

Peruvian municipalities provide an ideal setting for studying the effects of accountability on candidate selection. First, unlike other contexts in which information on the characteristics of politicians is only released for the ones who get elected, the national electoral office collects and publishes detailed information on all the 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 the level of political competition, but also at who decides to run for office and who is deterred, emphasising those characteristics that are likely to be correlated with performance in office, as education and previous experience in the public and private sector (Besley et al., 2011; Martinez-Bravo, 2017). Second, it is not often the case that one can observe variation in accountability 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.[2] In our setting, the procedure through which recall elections are carried out allows us to identify the effects of being exposed to a higher perceived probability of being recalled from office. Finally, recall referenda in Peru are an institution that is at risk of being captured by political interest groups, a claim supported by anecdotal and statistical evidence, 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 analysing 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 et al. (2005) estimate a structural model of the behaviour 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) in a recent study, 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 still representative in terms of their social background, and that material and intrinsic motives matter for selection. In this paper, we analyse empirically a specific mechanism that affects the selection of politicians, which sheds light on their motivations 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, free press, etc. Barro (1973) and Ferejohn (1986) in their seminal works highlighted the relevance of the information asymmetry between voters and politicians. As in any agency model, incentives have effects on moral hazard and adverse selection, yet most of the empirical literature analysing this agency problem have focused on the effort and performance of politicians, while neglecting the selection dimension. Empirical work on these issues has analysed the impacts of term limits

[2]Notable exceptions of random variation in accountability can be found in Ferraz and Finan (2008) and Ferraz and Finan (2011).

and information availability on the behaviour of elected politicians.[3] Unlike this work, in a recent paper which is closely related to ours, Fisman et al. (2017) look at the effects of financial disclosure laws on the selection of politicians in India. Using the staggered implementation of disclosure laws, they find that potentially corrupt politicians self-select out of the electoral race. In our analysis, we not only look at the chances of reelection of incumbents, but we are able to directly observe the characteristics of all candidates running for the mayor seat. Further, we analyse an accountability institution which is used for political purposes, hence distorting its objectives and ultimately generating negative selection.

More closely related to the predictions of agency models, as well as highlighting the importance of considering endogenous selection into politics (in the spirit of the citizen candidate models, e.g. in Osborne and Silvinski (1996) and Besley and Coate (1997)), a group of recent papers analyses the effects of monetary incentives on politicians' selection and performance.[4] 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. Moreover, they manage to separate the incentive and selection effects, and document a better performances of politicians who receive higher wages.[5] Brollo et al. (2013) study how additional natural resource revenues affect political corruption and the quality of politicians. Their findings show that

[3]Besley and Case (1995) and Ferraz and Finan (2011) estimate the impacts 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 have higher incidence of corruption. Alt et al. (2011) and List and Sturm (2006) study term limits in US elections, and find that they affect the expected quality of incumbents, and also that they affect environmental policy. Ferraz and Finan (2008) investigate how the release of information on corruption audits affect the reelection prospects of politicians in Brazil, Besley and Burgess (2002) study the effects of press availability on government responses in India, while Bobonis et al. (2016) look at the effects of timely corruption monitoring on corruption levels. Casey (2015) analyses the effects of information availability on redistributive politics.

[4]In related work, Caselli and Morelli (2004) explain through a model the persistence of bad politicians in office.

[5]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.

larger resource transfers to municipalities increase corruption and reduce the average level of education of candidates for mayor.[6] Similar to these papers, we use detailed data on candidates to analyse the impact of a treatment that affects the expected value of office. In the context we analyse, potential candidates exposed to a recall referendum in the previous electoral period have a lower expected length of term.[7] We add to this literature by documenting that institutions intended to increase citizen control of politicians can have negative consequences, if they deter high quality politicians from running for office as a consequence of political capture of such institutions.

In the next section, we provide background information on recall elections in Peruvian politics. Section 1.3 presents our main datasets, descriptive statistics, and outlines our identification strategy, which is used in the empirical analysis, shown in Section 1.4. Section 1.5 explores and discusses a theoretical framework and mechanisms that could explain our findings. Section 1.6 concludes with a discussion of the results and their external validity.

1.2 Institutional background

1.2.1 Local governments in Perú

Municipalities (districts) are the lower administrative level in Peru. The highly decentralised 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, as well as construction of local public infrastructure such as roads or sanitary facilities.[8] Since 2002, municipalities have recorded a five-fold increase

[6] In a somewhat related paper, Beath et al. (2016) use a randomised 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 levels of education, and their policy views are less extreme.

[7] To some extent, our treatment is similar to the one analysed in Dal Bó and Rossi (2011), where they look at the effects of different length in office on the performance of legislators in Argentina, holding selection constant.

[8] More recently, municipalities have started to also administer health insurance cards. However, due to this relatively recent development, we do not have suitable data for our period of analysis.

in their budgets, now accounting for more than 20 percent of the national budget and around 45 percent of Peru's total public investment budget (Pique, 2017).

District mayors and their councillors are elected to serve for four year terms with the option of reelection. The mayor is elected by a first past the post mechanism.[9] The political landscape at the local level is extremely fragmented, with a significant number of candidates running for "independent" parties (i.e. citizen candidates) that have few links outside the district and are often seen as an election vehicle centred around the candidate rather than an ideology or political programme (see eg. Bland and Chirinos (2014).) For example, in the 2014 municipal elections, the average district had about 7.26 candidates running for office, and only 36.9 percent of them represented a national political party.

1.2.2 Recall elections

Peruvian citizens have the right to recall any local elected official (mayors, councillors, and regional presidents). The introduction of this direct democracy mechanism in the 1993 Constitution followed a set of similar democratising reforms introduced in other Andean countries (Colombia, Ecuador, Bolivia) and emulated ones already existing elsewhere in the world (eg. US, Poland, Uganda, among others). This accountability mechanism holds politicians accountable on a more constant basis (rather than only in regular election times), and provides incentives for voters to be informed outside of electoral periods. 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 scope, rather than larger reforms and projects.[10]

[9] The mayor's party automatically gets a majority of seats in the council, with the rest of seats being assigned to the other political parties, proportional to their vote shares. Mayors are full time employees with a wage, while councillors are paid by the number of hours they serve in the council.

[10] Other arguments in favour of recall referenda are: (i) they help to check undue influence by narrow special interests, (ii) offer a safety-valve mechanism for polarised contexts and (iii) they provide a sensible alternative to impeachment. Additional arguments against recall referenda include: (i) recalls make public office less attractive to the most able individuals, (ii) the very premise of recall is antagonistic to republican principles, especially to the idea of electing good lawmakers and officials and then allowing them a chance to govern until the next election, (iii) they can be costly, unnecessary and directed at the wrong target and (iv) they may generate more

A recall referendum can be called in the second or third year of a 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,[11] (ii) name the authorities subject to the recall and argue a reason,[12] and (iii) collect valid signatures of 25 percent of the eligible voters in the jurisdiction. The signatures submitted are examined by the national registry (RENIEC),[13] which checks for their validity, e.g. if the name, signature and finger print match the records, if the person is registered in the district where the recall is to take place, if he/she has not signed for another 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, promoters have the chance to submit extra batches of signatures, which are checked until a pre-established deadline arrives, or the threshold is achieved. Figure 1.1 shows a plot of the submitted and valid signatures. All submissions have more than 25 percent of signatures, but there is wide variation in the number of signatures which are declared valid. Figure 1.2 shows the timing of elections, and the steps required to call a recall referendum.

polarisation in elections due to increased political tensions and grievances.

[11]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.

[12]Multiple names can be included in the petition, e.g. the mayor and a group of councillors. The proposer needs to present an argument backing up the reasons for the recall attempt, but they do not need any 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 (more than one reason could be given): (1) The lack of fulfilment 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 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-Fulfilment of duties (39 requests)." The data on reasons for a recall initiation are not systematically available at a disaggregated municipal level.

[13]RENIEC is a national technical and independent institution. There have never been any claims that the process of validating signatures for recall processes has been influenced by political interest groups.

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 favour of recalling the authority. Despite the cumbersome procedure, recall procedures 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, covering 45 percent of all districts in the country (747 out of 1645 districts).[14]

When a mayor is recalled, unless at least one third of the council is also recalled, the first councillor 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. In practice, this means that the new mayor is in power for less than two years.[15]

While this direct democracy mechanism is intended to increase accountability, devolving power to voters outside the regular electoral cycle, and enabling them to punish inefficient or corrupt politicians, it has been documented that it is often used as a political tool. Given the large number of candidates running for office in each election 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 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%.

[14]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 mechanism is also provided in some Mexican states and Argentine provinces."

[15]Since the 2014 electoral cycle, changes in the legislation were introduced, and recall referenda can only be called in the second year of tenure and new elections are never called in case of a successful recall.

After a mayor is elected, it is therefore not hard to put together a coalition between loosing candidates that have enough support to promote a recall referendum (Bland and Chirinos, 2014). For example, as the JNE states, in the 2012 recall cycle, 22 percent of the promoters of a recall referendum were former candidates who had lost an election. If one considers that many times politicians have political operators representing them as the person officially promoting the recall, as was the case of the high-profile 2013 recall referendum of the mayor of Lima, we should expect the true number to be even larger.

In Table 1.1, we 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 election (turnout, number of candidates, and win margin). After including district and election fixed effects in the regression, the only variables that have a significant 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 the number of candidates was higher. None of the mayors' characteristics have statistically significant effects on the probability of a recall election taking place. This is consistent with the claim that recall referenda are used as a political tool, rather than as a citizen control mechanism to punish corrupt or low-performing politicians.

Presumably, the political objective of a recall referendum is to weaken the incumbent for a future election. In districts in which there was a recall referendum (even if unsuccessful), the incumbent runs for reelection only in 52.6 percent of cases (compared to 59.3 percent, where there was no recall referendum), moreover, conditional on running, they are 10.7 percentage points less likely to be reelected (from a base of 31 percent.) Hence, it seems that recall referenda are used as a political strategy to weaken the incumbent's electoral profile.

1.3 Data and empirical strategy

1.3.1 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 in particular, they publish candidates' Curriculum Vitae (*Hoja de vida*). We scraped the website to assemble a novel and comprehensive dataset with the characteristics 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 are able to compute a series of variables related to each candidate's schooling: (i) ever attended university, (ii) attended only up to a technical education centre, (iii) attended up to secondary school, (iv) attended primary school. 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, allow us to impute the number of years of education.[16] Additionally, the dataset includes information on each candidate's work and political experience as well as party service, from which we can compute the number of years of experience in (i) elected public office (mayor, councillor or regional councillor), (ii) the position of mayor, (iii) service in party office, as well as (vi) whether he/she is a member of a national political party, (v) has work experience in the public sector or (vi) private sector. We also obtained information on candidates' demographic characteristics, e.g. gender and age.[17] Finally, we retrieved information on wages from official

[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. 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. University degrees were capped at 6 years and technical degrees were capped at 3 years. Postgraduate degrees were capped at 6 years for PhDs and equivalent qualifications, at 2 years for Master degrees and equivalent, and 1 year otherwise. 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 respectively. Second, attending or completing technical education are coded as 1 or 3 years of education respectively.

[17] While the CVs online have fields for filling out previous convictions or open trials, and wealth, these are rarely filled, and therefore we cannot use them for our analysis.

national survey data (ENAHO), which allows us to impute wages for all candidates based on a large set of observable characteristics such as education, gender, age or location.

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 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]

The national registry (RENIEC) shared with us information on the number of signatures submitted by the supporters of each recall, the date of the submission, and the number of valid signatures. This information is available starting with the first recall attempt for the 2002 electoral cycle. Data on the submitted reasons for initiating a recall process of the local mayor are not available in a disaggregated way, i.e. a format that would allow to identify recall reasons in each municipality. Therefore, we cannot use data on recall reasons for the purpose of our analysis in this paper.

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 allow us to compute the win margin of the elected mayor, as well as a measure of political competition that is constructed via a Herfindahl index of vote shares.[19] Additionally, they also gave us access to data on the number of kits bought to attempt a recall, the names of the authorities whom promoters attempt to recall, whether a recall referendum took place in a district (and its date), and its outcome.

[18] Although 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 centred 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] The measure is normalised to continuous values between 0 and 1 and is equivalent to 1 minus the Herfindahl index of concentration of vote shares in municipal elections. Thus, larger values indicate electoral decisions that were more contested.

We complement these data sources with information on the population of each district, which we obtained from the national statistical agency (INEI).

1.3.2 Descriptive statistics

As mentioned in Section 1.2, recall referenda are fairly common in Peru. Figure 1.3 shows the incidence of recall referenda over the last three electoral periods. Recalls have been attempted ("kit" purchased) in 35 to more than 60 percent of districts, with a clear upward trend over 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 referendum, leading to between 2 to 6 percent of districts having a recalled mayor. The number of municipalities with recall attempts or recall referenda is depicted in more detail in Figure 1.4. In total, 804 municipalities recorded at least 1 recall attempt in the form of signature collections. Out of those municipalities, 659 experienced a recall election. Figure 1.4 also gives a detailed overview of the distribution of municipalities according to the number that (i) recall attempts resulted in recall elections, and (ii) recall elections led to a successful recall of the incumbent mayor.

Table 1.2 provides the basic descriptive statistics of our data, both for the full sample, and for the restricted sample of district-election observations in which the number of valid signatures was close to the 25% threshold.[20] Candidates running for mayor in Peruvian municipalities have a relatively high level of education. The majority of them (59%) attended university, and they have on average 14.8 years of education. Similarly, only around 3 percent of candidates during the analysis period have primary education or less while around 23 percent have only attended secondary education. Those candidates that end up elected as mayors have on average similar levels of education. They have slightly more years of education but are on average comparable in terms of university, technical, secondary, and primary education. In terms of their previous experience, there are some noticeable differences. Elected mayors have on average 2.8 years of experience of holding an elected

[20]To select this sample, in our preferred specification, we use the optimal bandwidth proposed by Imbens and Kalyanaraman (2012) for regression discontinuity analyses.

office such as mayor or councilman, while the average candidate has slightly less experience. On average winning candidates have previously served 1.5 years as mayor beforehand and they have held other positions within their political parties for 2.5 years. For all candidates, these figures are lower. Their experience of having held elected office amounts to around 1.6 years; more than one year less than electoral winners. Moreover, their average years of experience as mayor or in a position in their political party are 1.2 and 1.3 years, respectively. With regards to experience in the private and public sector, the differences between elected mayors and candidates for mayor are somewhat less pronounced. Around 64.5 percent of mayors have work experience in the public sector, which is 4 percentage points higher than the proportion of all candidates with public sector work experience. In contrast, for private sector experience the proportion of candidates lies at 45.5 percent, two percentage points higher than the respective figure for elected mayors.

Finally, for both candidates and elected mayors, a relatively low number (around 44 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, in particular at the municipal level. In this context, individuals matter more than party platforms, emphasising the significance of their qualifications, experience and personality for local politics and public goods provision. Demographically, candidates and mayors are of similar average age (44 and 46 years) but there is one striking observation. The share of women among those who get elected into office is half the share among all candidates that compete in municipal mayoral elections.

In the last five rows of Table 1.2, we present a brief overview of five 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, a standardised measure of political competition and the election turnout. Additionally, we document the population size of the districts. 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, a number that rises up to 9.5 for the sample of our analysis. The level of political competition, measured by the distribution of vote shares among candidates, indicates the severity of competition for mayoral positions. The average win margin, of around 9 or 8 percentage points depending on the sample, 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 in 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 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 mobilisation (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 small number of votes.

1.3.3 Empirical strategy

The empirical strategy is designed to identify the causal effect of the salience of a political accountability mechanism - as measured by the presence of a recall referendum in the previous electoral period - on the selection of politicians running for office. To do this, we compare candidates running for the mayor position in the same district in elections when a recall referendum barely took place, with those in which the proponent fell just short of having enough signatures to call the referendum. Our identification strategy relies on district and election fixed effects, and exploits specific features of the recall process that generates variation in the presence of a recall referendum, in the spirit of a sharp regression discontinuity design (Lee and Lemieux, 2010; Imbens and Lemieux, 2008).

As described in Section 1.2, each time a recall petition is filed, the national registry (RENIEC) checks each of the signatures submitted, and declares them to be either valid or invalid. A recall referendum will take place only if the valid signatures amount to 25 percent of the eligible voters in the district. The process allows failed petitioners to resubmit new signatures until either a deadline arrives,

or the minimum number of signatures is reached. While many of the petitions succeed, a number of them still barely fall short of reaching the threshold, and thus, we compare the characteristics of candidates running for mayor in districts where proposers barely had enough signatures to hold a recall referendum in the previous electoral period with those running in districts that did not. The fact that resubmissions are allowed and the number of successful petitions is larger than the unsuccessful ones around the threshold generates concerns about selection on unobservables. This is partially addressed by the inclusion of the district fixed effects, but we include a battery of robustness and placebo checks that specifically address this concern. All in all, our identification strategy is based on a within district sharp regression discontinuity design, and the main regression equation is as follows:

$$Y_{ijt} = \alpha + \beta Recall_{jt-1} + \gamma f(ValidSignatures_{jt-1}) + \delta X_{jt-1} + \nu_j + \mu_t + \varepsilon_{ijt} \quad (1.1)$$

where, Y_{ijt} are characteristics of candidates 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, and demographic characteristics. Our main interest lies in β, the coefficient associated with the presence of a recall referendum in the electoral period $t-1$. Given that the presence of a recall referendum is determined by the number of valid signatures, we include in all of our regressions a flexible polynomial of the number of valid signatures in the recall petition as a proportion of the number of eligible voters, $f(ValidSignatures_{jt-1})$, our running variable.

A key component of our identification strategy relies on the inclusion of ν_j, a district-specific fixed effect. By doing this, we are not only making comparisons between districts at both sides of the threshold, but also exploiting within-district variation in the presence of a recall referendum in t-1, effectively identifying our coefficients of interest by comparing districts that at different points in time fall on both sides of the threshold for holding a recall referendum. As shown in Figure

1.3, there are clear differences and patterns, thus, we include μ_t, which represents election fixed effects and controls for any election-specific unobserved factors. X_{jt-1} is a matrix of district-specific, time-varying variables. For robustness checks, we also include (i) political variables (turnout, win margin, etc.), and (ii) characteristics of the mayor in X_{jt-1}. Finally, ε is the error term, which we cluster at the level at which the treatment is applied, *district* $\times$ *election* level.[21]

Given that we are comparing municipalities in which a recall election barely takes place or not, our analysis sample is restricted to *district* $\times$ *election* in which at least one recall petition was filed. In addition, we only consider observations in the *district* $\times$ *election* dimension for which the number of valid signatures was close enough to the threshold, and present robustness checks for multiple bandwidths, i.e. $ValidSigntures \in [0.25-\epsilon, 0.25+\epsilon]$, where ϵ is determined with optimal bandwidth selection procedures. In our preferred specification, we use optimal bandwidths based on Imbens and Kalyanaraman (2012), but in the Appendix, we also present results with 3 percentage points as bandwidth.

While the focus of our empirical analysis is placed on the regression discontinuity of collected valid signatures, we supplement our findings with a second regression discontinuity design. We exploit the fact that the second stage of recall election processes again include a sharp threshold that determines the outcome. Only if more than fifty percent of casted votes support a recall motion, the incumbent will be recalled. If the vote share falls short of that mark, the mayor stays in office. Since the referendum can occur only once and no additional vote can be taken in any given electoral cycle, contrary to the signature collection process that allows multiple submissions, the density of recall election vote shares is smooth around the threshold value. This strongly suggests that there is no gaming of the threshold so that a plain, sharp regression discontinuity design can be applied. The regression equation is as follows:

$$Y_{ijt} = \alpha + \beta RecalledMayor_{jt-1} + \gamma f(VoteShare_{jt-1})[+\mu_t] + \varepsilon_{ijt} \qquad (1.2)$$

[21] Our results are robust to more conservative clustering, i.e. at the district level.

where, Y_{ijt} are characteristics of candidates i running for office in district j in election t. Again, our main interest lies in β, the coefficient associated with the incidence of a successful recall referendum in the electoral period $t-1$, the result of which saw the incumbent mayor recalled from office. Given that the outcome of the recall referendum is determined by the vote share in favour of the recall, we include in all of our regressions a flexible polynomial of the vote share, $f(VoteShare_{jt-1})$, our running variable. Our preferred specification excludes time fixed effects but we also consider regressions including them to control for election period-specific influences that might affect the composition of political candidates.

1.4 Results: accountability and candidate selection

1.4.1 Candidates' education and experience

As a first pass of the data, Figure 1.5 shows graphically our main results using unconditional non-parametric plots with breaks at the 25 percent signature threshold. This graphical evidence indicates that candidates who run in elections in districts where a recall referendum took place in the previous electoral period have fewer years of education, are less likely to have attended university, and are more likely to have gone to a technical education centre or secondary school.

In Table 1.3 we formally test for the magnitude and significance of the observed effects from Figure 1.5, reporting the results of regression equation 1.1. It presents the results using a simple specification, controlling for district and election fixed effects, and including a linear polynomial of the running variable. All the 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.[22] The main message from the graphical evidence holds: candidates running in districts above the threshold have fewer years of education (not significant), and are 12 percentage

[22]The differences between the sample sizes used in the different regressions is due to the optimal bandwidth obtained, which go between 3 and 8 percentage points above and below the threshold. In the Appendix, we show the main regressions for alternative bandwidth specifications, namely a smaller one of 3 percentage points above and below the threshold. All the results are quantitatively and qualitatively similar.

points less likely to have attended university, and more likely to have only attended a technical education centre or attended up to secondary school.

One potential concern for results without election fixed effects would be that, by pooling the data from three elections (2006, 2010 and 2014), we might be comparing districts from different electoral periods across the thresholds, and as we have seen in Figure 1.3, there are clear trends and differences in the recall processes across years, thus making them not comparable. The inclusion of election intercepts controls for election specific unobservables common to all districts in our sample. Additionally, we further tighten our identification by including district fixed effects, which effectively allow us to exploit within-district variation for identification.

Following the work of Gelman and Imbens (2017), our preferred specification is based on a linear polynomial. However, many regression discontinuity design studies use more flexible higher order polynomials of the running variable. We show that our results also hold if we use higher order polynomials such as a quartic polynomial (Table 1.17 in the Appendix). The qualitative results of the regression in Panel A of Table 1.17 resemble the ones in Table 1.3. Candidates running in a district that had a recall referendum in the preceding electoral cycle are less educated than in districts where no recall referendum took place. They have significantly fewer years of schooling and are significantly less likely to have attended university. The magnitudes of these effects are sizeable and somewhat larger than in the linear polynomial specification reported in Table 1.3. Candidates that decide to run after a recall election has taken place are almost 20 percentage points less likely to have attended university, and 7 percent percentage points more likely to have attended at most secondary education. The proportion of candidates who attended a technical education centre is also higher, but not significantly so. Overall, they have on average around 1 year less of schooling. Hence, our main results are qualitatively not driven by functional form assumptions.[23] In Panel B of Table 1.17, we show the main results controlling for a range of variables that describe the political environment in t-1 and that could therefore affect our results (turnout, win margin, number of candidates for mayor, population size), and the results remain unchanged.

[23]Similar results are obtained with quadratic and cubic polynomials of the running variable.

Higher leader's education has 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 likely to have available more health centres, doctors and safe drinking water. Using data from southern India, Besley et al. (2005) show that the educational level of the village heads is correlated with lower levels of corruption. In a cross-country setting, Besley et al. (2011) document that the exogenous removal of a highly educated head of state has a negative impact on economic growth.

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 to look at other characteristics, beyond educational attainment, that are also presumably related to the mayor's performance in office. Using our preferred specification from Table 1.3, the results in Table 1.4 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 was held in the previous period have 0.2 fewer years of experience serving as a district mayor, though the effect is insignificant. There is suggestive evidence that they are less likely to have experience holding an office in a political party (0.6 fewer years, not statistically significant). Panel B of Table 1.4 bolsters these results. Candidates in the treatment group are 10 percentage points less likely to have any experience working in the public sector. Finally, while we do not see that they are younger or older, the possibility of being recalled from office is more likely to deter women for running for office, decreasing the already low representation of women in elections.

1.4.2 Quality of candidates: suitability of education

One question that naturally arises when analysing political selection is what constitutes a good politician. One might even ask whether citizens' main objective is to choose qualified politicians or ones that are more representative of society as a whole. Could there be a trade-off between quality and representativeness of political candidates in the Peruvian context that we study?

This does not seem to be the case. Elected mayors are at least as if not more qualified than other candidates. The fact that voters choose actually more educated individuals than average candidates seems to reveal that they have a preference for higher education in candidates running for the office of mayor. Furthermore, both average candidates as well as elected mayors are much more educated and politically experienced than average citizens in Peru, suggesting that these characteristics are valued for political office. Moreover, as reported in Table 1.1, incumbents' education and experience are negative, albeit insignificant, predictors of recall elections. All of these facts indicate that education is not only a characteristic that voters associate with mayoral capacity but that there is also no clear trade-off with other factors that would render voters preferring less educated mayors.

Nonetheless, we want to re-address the vital question: what are the key factors for the quality of politicians? The default in the literature is to assess a politician's capacity by using his/her education as a proxy. Another dimension that has been considered is candidates' experience, both in office as well as other professional experience. A potentially even better measure for the quality or capacity of political candidates could be retrieved from their earnings, which should proxy for their skill sets. Wages should be seen as measuring candidates' opportunity costs, i.e. their outside earning potential. Following that argument, higher opportunity costs are a characteristic of more qualified candidates that might have more lucrative outside options.

While information on previous wages of political candidates cannot be systematically and reliably extracted from their CV data, candidates' wages can be imputed. We extract information from Peru's national survey (ENAHO) to impute wages of political candidates based on their demographic, geographic, and educational background. For each election year (2014, 2010, 2006, and 2002), we extracted the ENAHO survey data on income from individuals' primary jobs as well as information on observable characteristics that are also documented in the CV data or can at least be created. The list of variables consists of the candidates' age, the square of their age, their gender, their level of education (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. We then run for each year a regression on primary income with the following explanatory variables: age, age squared, University, Technical, Secondary, gender, and urban. We save the coefficient results (including a constant) and, after appending those datasets for wages to the election years, we merge them with the candidates dataset and generate the opportunity costs/ imputed wages by using the regression coefficients and the individuals' respective characteristics.[24] The imputation regression is presented in Table 1.5. In an additional exercise, we impute wages while accounting for rural-urban differences in the explanatory variables' effects on wages.[25]

We probe the effects of accountability on the average opportunity costs of candidates running for office in the following period. The results, depicted in Panel A and B of Table 1.6, provide convincing support for our earlier findings. The imputed wages for candidates are significantly lower in districts where a recall election narrowly took place compared to districts where the signature collection just fell short of the 25 percent threshold. Given the time-varying nature of income, our preferred specifications are presented in column 2 of Table 1.6. Regardless of whether we choose a linear or a quartic polynomial, the results demonstrate the negative effects of observing a recall election on average opportunity costs of political candidates. The magnitude of the effects amounts to around 95 Soles per month (in a specification with election period fixed effects), which translates into a reduction of more than 10 percent of the average income of candidates in our analysis (Table 1.6). Even if we allow for all variables to have a different impact on wages in rural and urban districts, the results prevail qualitatively (Table 1.20 in the Appendix). These results provide further support to our baseline findings of adverse selection effects of accountability in the context of Peruvian municipal recall elections.

[24]
$$wage = Uni * wage_{Uni} + age * wage_{age} + ... + wage_{Constant} \tag{1.3}$$

[25] All explanatory variables are additionally interacted with the dichotomous indicator of a municipality's rurality. The regression results based on that imputation are qualitatively similar (Panel B of Table 1.20 in the Appendix).

1.4.3 Robustness checks

The identification assumption in our empirical design is that observations at both sides of the threshold are comparable along observable and unobservable characteristics. Figures 1.6, 1.7, and 1.8 show that observable characteristics are continuous along the threshold. We focus on variables related to (i) the educational level (Figure 1.6) and (ii) experience and characteristics (Figure 1.7) of the incumbent during the period when the recall referendum was attempted, and (iii) we focus on variables related to the political process in the electoral period where the recall referendum was attempted (Figure 1.8). There are no significant jumps along the threshold in most of the variables of interest. Particularly important are the continuity tests shown in Figures 1.6 and 1.7, since they reinforce the evidence from Table 1.1, allowing us to rule out the possibility that recall elections consistently take place in locations where the quality of mayors is lower.

A second important assumption of standard regression discontinuity designs 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). In our case, the design of the recall petition mechanism generates this discontinuity, since proposers who end up with valid signatures that amount to less than 25 percent of the electorate (after the registry's revision) have a chance of submitting new batches of signatures until either they reach the required number or a deadline is reached. Hence, the mass of observations just above the 25 percent threshold is larger than the one just below (Figure 1.9). The main concern for identification is that the incentives that lead proposers to collect just enough signatures to generate a recall referendum in an election period, also affect the decision of candidates to run in the subsequent elections. Below, we provide a battery of robustness checks and placebo tests that help rule out that the bunching of observations above the threshold causes the selection effects observed.

First, it could be the case that there are unobserved district characteristics that consistently cause certain locations to have recall referenda and also to have low quality candidates. Our main results are robust to the inclusion of district fixed

effects (Tables 1.3, 1.4 and 1.17), hence exploiting within-district variation in the presence of recall elections, which rules out that any district-specific, time-invariant, unobserved characteristics are driving the results.

Second, there could be other time-variant district characteristics that determine the presence of recall elections close to the threshold, as well as candidates' characteristics. Such characteristics might be related to the incumbents during the period in which the recall was initiated. One might think that in districts with weaker or lower quality mayors, recall referenda have higher chances of succeeding, and therefore promoters have larger incentives to exert effort in collecting signatures, thus sorting into the treatment. However, as we have seen in Figures 1.6 and 1.7, incumbents in districts at both sides of the threshold are similar in terms of their educational achievement, previous job experience and demographics. Further, in Panel A and B of Table 1.7 we include 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.

Third, certain political scenarios might increase the chances of a successful recall referendum 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 a recall referendum to take place. Again, all available political controls are balanced across the threshold (Figure 1.8), and including these variables in the main regression (Panel B in Table 1.7 and Panel B in Table 1.17 in the Appendix) does not significantly affect our results.

Furthermore, the presence of a strong incumbent who also has higher chances of being reelected might provide more incentives for proponents to collect more signatures, and therefore weaken the incumbent's chances to get reelected. If high quality incumbents decide not to run for office because of a weakened popularity due to the recall, while other incumbents who barely did not have to face a recall do run, we would mechanically have a lower quality pool of candidates in places where a recall took place. To deal with this concern, Panel A of Table 1.8 excludes all incumbents from the regression sample, and the results are robust to this exclusion.

Finally, differences in the capabilities and qualifications of those likely to be behind promoting recall processes might undermine our empirical strategy and bias our results. While it is for the most part not possible to make a complete statement about the promoters' characteristics due to data limitations, it has been demonstrated that a lot of recalls are triggered by political rivals who lost in preceding elections. Therefore, we identify those candidates who finished as runners-up in the previous elections. We include their characteristics as controls in Panel B of Table 1.8 in order to make sure that significant differences between runners-up explain neither the treatment status nor the overall effects. Again, our qualitative results hold. Moreover, we document in Figures 1.11 and 1.12 in the Appendix that their education and characteristics are very comparable for districts below and above the treatment threshold.

Another concern regarding our identification strategy could be that the effects of having a recall referendum could be persistent over time, and therefore even after controlling for district fixed effects, we could have districts in which the quality of candidates for mayor is decreasing systematically. This amounts to a violation of the parallel trends assumption in a difference in differences setting. To alleviate this concern, in Panel A of Table 1.9 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. Only the presence of a recall referendum in $t-1$ causes a decrease in candidate quality in period t. Panel B of Table 1.9 on the other hand, examines the effect of recall elections on political selection in municipalities that experience a recall election for the first time. Overall, the effects are fairly comparable to the effects presented in our preferred specification for all municipalities (Table 1.3), suggesting that the negative selection effects of recall elections are not necessarily more pronounced the first time they take place. While the marginal increases to the salience of recall elections might be more powerful when they take place for the first time, a repeated occurrence of recall elections

could be indicative of a political capture of recall elections, discussed in detail in Section 1.5.3, which could aggravate the adverse effects on political selection.

Overall, the robustness checks and placebo tests implemented provide assurance that our results are not driven by selection, and that the causal mechanism behind the negative selection observed are the incentives for potential candidates.

1.4.4 Regression discontinuity of recall election vote shares

The recall process in Peru includes two stages, the second of which consists of the actual referendum. If an initiative to recall a mayor has successfully collected signatures of more than 25 percent of the electorate, the referendum itself takes place. Similar to the signature collection, a clear cut-off line is defined, in this case a simple majority of votes. If at least 50 percent of all all voters plus one vote support the recall, the mayor will be recalled. Otherwise, he stays in office and no further recall process can be issued against him for the remaining electoral term. This setup allows us to pursue another regression discontinuity (RD) analysis to test the existence of adverse effects of perceived accountability at the institution's second stage. Again, the argument is that electoral accountability becomes more salient in municipalities where a recall process leads to the successful ousting of an incumbent. As a consequence, if the salience of accountability via recall elections has adverse effects on the pool of candidates running for office, those effects should be significantly more pronounced in districts where an incumbent was marginally recalled than in districts where an incumbent narrowly survived the recall referendum.

Figure 1.10 displays the McCrary test for the density of vote shares in favour of recalling a mayor in recall referenda in Peruvian municipalities. The density is comparable on the two sides of the threshold, satisfying the test and alleviating concerns about any manipulation or sorting that could undermine the causal interpretation of RD estimates. For this reason, we follow the standard RD approach and omit fixed effects from our regressions on the effects of a recalled incumbent in the preceding period on political selection. The corresponding regression is given by Equation 1.2.

While this identification strategy has the advantage that we have a clean discontinuity, with all covariates and the density being continuous at the threshold, it

has the drawback that we are identifying potentially more marginal increases in the salience of the recalls, since the moment in which the referendum is called is when the campaigning starts, and therefore the mechanism becomes very visible. With this caveat in mind, we present the results of this analysis in Table 1.10.

Our findings show that having a mayor recalled causes a negative selection of candidates. However, as expected, the magnitude of the effects is lower than the one shown in our main regressions. In Table 1.10, the reported regressions for candidates' education use Imbens-Kalyanaraman bandwidths. The pattern that we observed for the signature RD is corroborated and our main hypothesis is supported. On average, political candidates are less qualified in districts where the salience of electoral accountability is increased as a consequence of mayor having been recalled. They have on average 0.6 fewer years of education and their proportion with university education is around than 7 percentage points lower. Instead, their share with at most secondary education is around 8 percent higher. Furthermore, the candidate pool in districts with a recalled incumbent in the previous election appears also to be of worse quality in terms of non-education related characteristics. Though the effect on experience in the public sector is weaker than in the signature RD regression (Table 1.4), evidence suggests that having locally observed a recall of an incumbent reduces the mayoral experience of candidates by more than 0.4 years. Finally, the opportunity costs of candidates are also significantly reduced in districts where a mayor was previously recalled (Table 1.11).

In combination with the results discussed in Sections 1.4.1, 1.4.2 and 1.4.3, the additional evidence on the second stage of the recall process demonstrates the adverse selection effects of increased salience of recall elections on political candidates in municipal elections.

1.5 Theoretical framework and mechanisms

1.5.1 Theoretical framework

How could it be that an institution that increases voters' ability to hold politicians accountable while in office generates negative selection? As mentioned in Section

1.2, recall elections in Peru have been shown to be often used as a political tool, with losers in the previous electoral period often being the promoters of recall elections. As shown in Table 1.1, the strongest predictor of the presence of a recall election is the closeness of the election, rather than the district's or mayor's characteristics. In this subsection, we propose a model that helps us understand the main reasons why an increase in perceived accountability can lead to negative selection of politicians when there is a risk of political capture.

We assume that there are three dimensions that characterise politicians: corruption, opportunity costs (ability), and public sector motivation. While the first two types of politicians derive utility from the rents they can generate, either by extracting rents from office or the wage they get, public sector motivated politicians derive utility from the completion of public works and projects while in office. The type of the politician is drawn before they decide to run, and is known to the politician but not to the voters. Once the politician is elected, nature draws a state of the world and the politician chooses an action.

After the mayor is elected, voters have the option of recalling them from office. They observe the outcomes in their constituencies (high or low), and vote to recall them if they observe a low outcome. However, they observe a noisy signal of politicians' actions. If the noise to signal ratio in voters' observations is low, an increase in the probability that the mayor is recalled should reduce the proportion of corrupt politicians running for office (since they expect to be able to extract less rents).

On the other hand, the introduction of distortions in the accountability mechanism increases the amount of noise, and therefore voters' ability to distinguish good from bad politicians is reduced, increasing the chances that a good politician gets recalled. If this is the case, we will observe a lower number of candidates running for office, and the reduction in the number of candidates is driven by high quality politicians who drop out of the race.

While the intention of this direct democracy mechanism is to be able to punish corrupt or poor performing politicians, capture by political interest groups increases the probability that an honest politician or one who is performing well in office is

recalled. Hence, candidates who have high opportunity costs, or those who derive utility from carrying out a policy agenda could be deterred from running for office, generating a negative selection of candidates.

1.5.2 Candidate entry or exit?

Candidates running in elections after a recall referenda took place are, on average, less educated and have less experience in the public sector. 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 enter the race. The theoretical framework in Section 1.5.1 predicts that the number of candidates should go down, and therefore the level of political competition should be reduced.

To look into this question, as well as how the political landscape is being affected in districts that had a recall election in the previous period, in Table 1.12 we analyse the effects of having a recall referendum in the previous period on turnout, the number of candidates, the win margin, and the level of political competition. Voter participation in elections does not change significantly after a recall referendum. In contrast, the results in Column (2) show that the number of candidates in these elections drop significantly by 12 percent (from a base of 6.9 candidates). This suggests that it is not the case that more low quality politicians are entering the race and thus lower the average quality. Rather, consistent with our hypothesis, it seems to be the case that at least a few high quality politicians are being deterred from running for office by observing a recall election in the previous election.

Finally, columns 3 and 4 show the results for the win margin and the Herfindahl index of political competition. Given the lower number of candidates, it is not surprising to see that elections become less competitive: winners have a margin of victory of about 2 more percentage points and the the overall vote in the election is more concentrated (lower value of the Herfindahl index), though these results are not statistically significant.

1.5.3 Mechanisms

As previously discussed, there is suggestive evidence that a lot of recall processes in Peru are initiated for political reasons not necessarily related to the incumbent's behaviour, effort, or performance in office. The evidence rather indicates that recalls are motivated by electoral gains of political opponents and rival parties. As discussed in Section 1.2, there are plenty of anecdotal observations of such politically captured recalls. We substantiate this by providing more detailed information on the nature of recall promoters. We use information on promoters' identity to match that information with our dataset of political candidates for municipal elections from 2002 to 2014.[26]

In our matching exercise, we only consider political rivals who ran in the same municipality at any of the elections in our sample period.[27] In total, we can successfully match 1779 of all 5722 DNI-municipality combinations that identify the different recall processes' promoters. Thus, we can directly identify more than 31 percent of all promoters as political rivals. 946 were candidates in an earlier election, 833 ran for office at a later election. Our estimate of 31 percent should be considered as a very conservative lower bound of the actual number of recalls triggered by political opponents, as we must assume that many processes will also be triggered by a straw man. Hence, we can conclude that a considerable proportion of recalls is used as a political tool. The straw man caveat induces a major impediment to pursuing any heterogeneity analysis based on the identification of some recall processes as political. Therefore, it is not surprising that the effects of negative selection of accountability are not significantly stronger as reported in Table 1.13. These estimates are likely to be biased since we cannot assure an unproblematic classification into political and non-political reasons for promoting a recall election. Nonetheless, the

[26] We make use of the fact that each individual in Peru is assigned a unique national identity number, DNI, which we have for both recall promoters and political candidates. This allows us to securely match observations from different datasets.

[27] We consider both candidates for mayor as well as for the city council ("regidor") since councilmen also have incentives to weaken incumbents and have also been documented to be behind some of the high-profile cases of recall elections in Peru.

estimates appear to be pointing in the expected direction: places with recall elections that were politically charged, i.e. triggered by political rivals of the incumbent mayor, experience a more pronounced drop in the quality of candidates running for election in the next period.[28]

We pursue two other sets of heterogeneity analysis. First, we look at the performance of the incumbent during the first year in office. Second, we examine the heterogeneity of effects by the level of political competition. The former can lend empirical support to our theoretical and conceptual idea that political capture of recall elections introduces a lot of noise in the processes which undermines the efficacy and objectives of accountability. The results are presented in Panel A of Table 1.14. The interaction of an incumbent's effort, measured by the proposed and available budget for expenditure that he successfully executed, and the incidence of recall elections is significantly negative. In districts where a mayor performed relatively well and still faced a recall election, the share of university educated candidates decreases even further.

Panel B of Table 1.14 takes a closer look at heterogeneity along the level of political competition. We use the number of contesting candidates for the office of mayor as a proxy for electoral competition. First, we demean the number of candidates, i.e. we subtract from the respective number the average number of candidates standing for office and then we interact it with the incidence of a recall election. Effectively, we examine deviations from the mean level of competition. The underlying idea for this exercise is that highly competitive elections are more likely to be used for political purposes as the incentives for opposing candidates to weaken the incumbent are greater. In fact, we find that the negative effects of accountability on political selection are more pronounced in places with more competitive elections in the previous period, as can be seen on the significantly negative effect on the share of candidates with university education.

[28]The coefficient of interest captures the interaction effect of having a recall election in t-1 and the dichotomous variable "Political Opponent" that is equal to 1 if the promoter was a rival candidate in any previous election (Panel A) or the directly preceding election (Panel B).

Taken together, the findings presented in this subsection give support to our proposed mechanisms. Political capture of recall elections dilutes their effectiveness and even "perverts" their original objective. Instead of allowing citizens to sanction bad or corrupt politicians and thus to raise the quality of politicians, the average quality of political candidates deteriorates.

1.5.4 Do recall referenda lead to lower quality mayors?

In the preceding sections, we have shown that an increase in the perceived probability of being recalled from office causes highly educated and experienced candidates to abstain from running from office, generating a smaller pool of candidates, of lower average quality, and causing less political competition. However, 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 and did not have a recall referendum in the previous period. The results of this analysis are shown in Table 1.15. Surprisingly, the results from our preferred specification show that elected mayors do not have lower levels of education. If anything, they are slightly more likely to be university educated, but for none of our regressions can we reject that the coefficients are different from zero.

Panels B and C of Table 1.15 explore the effects on past political and job experience, as well as mayors' characteristics. Mayors elected in districts that had a recall referendum are neither more nor less likely to have experience as mayors or holding other elected office. Moreover, they are not different in terms of party office experience. They appear a bit less likely to have worked in the public or private sector in the past, compared to mayors elected in municipalities where there was no recall election, although the effects are clearly insignificant. Finally, they seem to be similar in terms of age and gender to mayors elected in districts without a recall election in the previous electoral cycle. It must be noted that due to the reduced sample size, all these estimates are very noisy.

Overall, despite the lower average quality of the pool of candidates, it seems like elections still serve as a mechanism to elect high-quality politicians.

1.5.5 Are politicians' policy choices altered?

Even though we do not find any differential effects on the average mayor's qualities between places that had a recall election and those that did not, recall elections might still affect policy choices. Mayors in places with a recall election during the last electoral period face less competition, as measured by the number of candidates competing for office, and also run against an on average less qualified pool of opponents as documented in Tables 1.3, 1.4, 1.6 and 1.12. Consequently, the incentives of mayors in such municipalities might very well be affected since their pressure to perform well or make an effort to pursue the right set of policies is likely to be subdued.

It is beyond the scope of this paper to examine effects on policy choices at great length. Furthermore, data availability at the level of municipalities is a greater concern for policies than for the profiles of political candidates as policy decisions at the local level are either not regularly recorded or not systematically and consistently compiled for all municipalities. For those reasons, our analysis needs to be limited to a few observable and documented measures for policy choices, as well as effort and performance in office.

We have retrieved and processed data on municipal policies recorded in the RENAMU datasets (the National Registry of Municipalities). In general, we can examine data on the following categories:

- budget spending, total expenditure, total revenue, and the share of budgeted expenditure that was actually spent;
- local services and amenities: trash collection, construction of public facilities etc.

Mayors in Peru are responsible for the planning of small, local infrastructure projects and ultimately manage the provision and delivery of a number of local services. In

order to assess whether mayors' policy choices are altered, we choose indicators for service delivery that are both relatively important at the municipal level and can at the same time be empirically examined. More precisely, we investigate the impact on the construction of public sanitation facilities and the quality and regularity of public services in the form of the provision of the number of local patrol guards and the frequency of waste collection. Furthermore, we assess whether there are any effects on the type of employment contracts (permanent or temporary) of municipal employees since mayors who face less fierce political competition could also tend to award permanent contracts to loyal supporters. These types of services are among the most relevant at the municipal level in Peru following the decentralisation reforms of 2002 and 2004.[29] The provision of other public goods and services such as education or general health services is determined at the regional rather than the municipal level and is therefore not affected by local recall elections.

In Peru, municipal finances are largely derived from central government transfers. Therefore, budgets are mostly out of the control of mayors. However, they manage the realisation of budgeted projects and thus affect realised expenditure of municipalities. For that reason, a mayor's motivation can be proxied by looking at the ratio of realised expenditure to revenue on one hand, and realised to budgeted expenditure on the other. The results are reported in Table 1.16.

In summary, the reduced level of competition for political office appears to affect mayors' policy choices and performance in office. While revenue or trash collection are not affected, other categories such as the construction of public toilets, the number of patrol guards employed or the actual execution of budgeted, i.e. already available, funds for public expenditure are reduced. The estimated effects suggest that not only the pool of candidates is lower in municipalities with a recall election during the previous electoral cycle but that this also translates into worse policy outcomes at the local level. However, these results need to be seen with caution due to a reduced sample size. Overall, we believe that more research needs to be directed

[29]The limited amount of reliable and multi-temporal data spanning more than a single electoral term makes the analysis of other potential indicators such as the administration of health cards infeasible. We do not have sufficient time coverage in the policy data to assess effects on the administration of national health cards, which are now issued by local authorities.

to the effects on policy choices. In particular, it would be worthwhile to investigate whether the increased salience of accountability also has any long term effects as it is likely that mayors' decisions to invest in projects that promise long-term rather than short-term benefits will be affected.

1.6 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 behaviour of politicians while in office, but they also have an effect on potential politicians' decision of whether to run for office or not.

Most of the empirical literature analysing the effects of accountability institutions has focused attention on disciplining effects. Unlike these studies, in this paper we analyse how accountability affects the selection of politicians. We highlight the "perverted" 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 for election in districts that had a recall referendum in the previous term with those who run in districts where no recall referendum was held. The fact that a recall referendum took place in a district increases the salience of recall elections and the perceived probability that, if elected, one could be ousted from office, therefore reducing the expected term length.

We identify our results using a regression discontinuity design that exploits specific features of the process of calling recall referenda. Promoters of the recall have to submit the valid signatures of at least 25 percent of eligible voters to be able to hold a recall referendum. Submitted signatures are checked by the national registry and some submissions end up falling just short of having enough signatures and

others are approved, allowing us to compare candidates running for election using a regression discontinuity design.

Our results show that candidates who run in districts that had a recall referendum in the last period are of a lower quality, as measured by their educational attainment and previous experience. In particular, they have about half a year less education, are 14 percentage points less likely to have attended university, and instead attended a technical education programme or just have secondary education. They also have significantly lower opportunity costs, measured by their income. Likewise, these candidates have a lower likelihood to have worked in the public sector and are more likely to come from the private sector instead. The probability of having a woman running for office is also reduced in districts where a recall election took place. Further, our results show that the number of candidates and political competition is reduced, therefore it seems that high quality politicians who would have run for office in the absence of an increase in the salience of this accountability institution are being discouraged and do not run.

How could it be that an institution that increases citizen control over politicians generates a negative selection? We provide anecdotal and statistical 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. The adverse effects of recall elections are significantly more pronounced in municipalities with high levels of political competitions, i.e. in places where the incentives for rivals to exploit recall elections for their own interests are particularly high. We also show that the performance of incumbent mayors in office before recall elections can be initiated does not dampen the negative effects. In fact, our heterogeneity analysis shows that places with well-performing incumbents who faced a recall election are affected even more severely: their mayoral candidate pools record even larger deterioration in terms of their quality. Moreover, we document that almost a third of all recall election processes in our sample are directly promoted by political rivals, an estimate that must be seen as a conservative lower bound of the true proportion. All these observations support the notion that recall elections are used as a

political tool. If this is the case, the probability of being ousted is independent of the elected mayor's performance or honesty, hence discouraging politicians who have high opportunity costs or who are committed to a certain policy agenda.

Finally, we analyse 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, as voters select the best out of the available candidates, hence mayors in districts where a recall election took place in the previous period are not more likely to have a lower level of education or experience than those who run in districts where no recall referendum took place. Nonetheless, we find that recall elections appear to have a negative impact on mayors' policy choices and performance in office.

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 low 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 impeachments, promoters should have to present plausible evidence of mismanagement 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-intentioned mechanism of citizen control.

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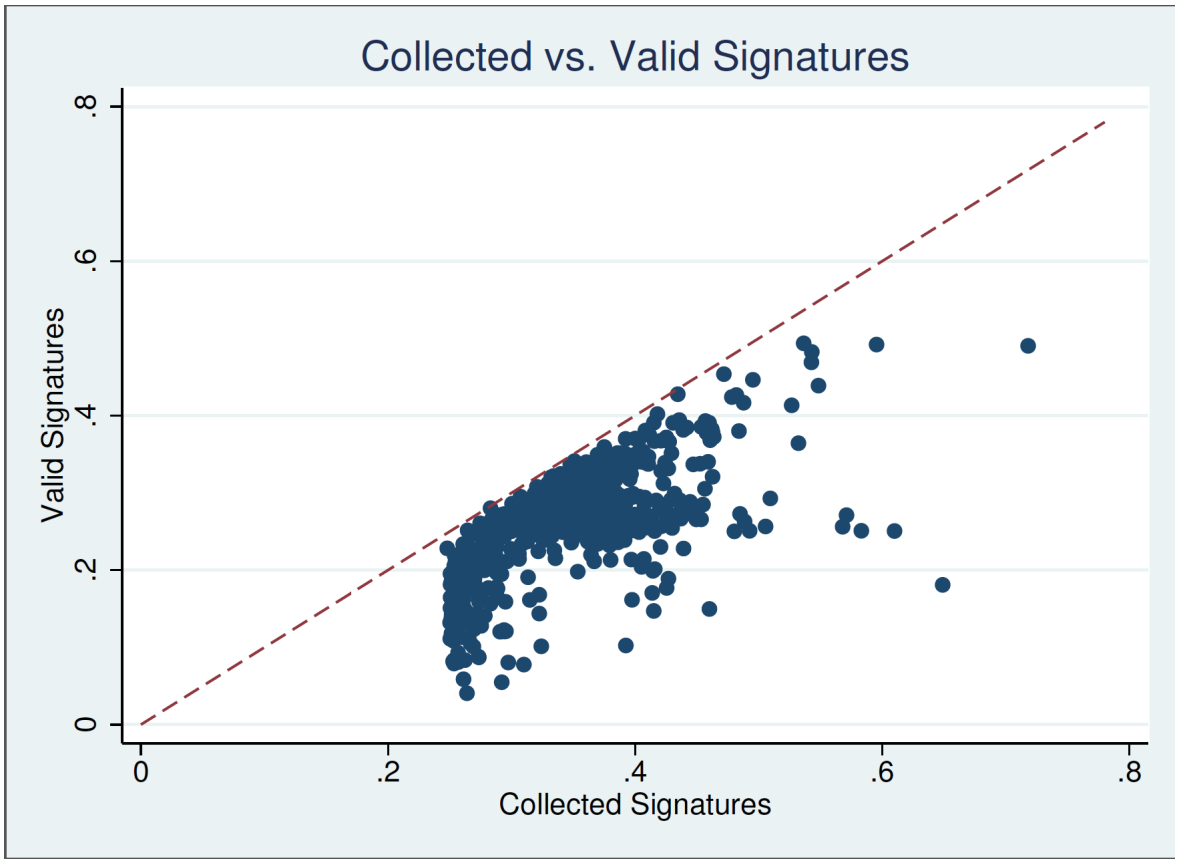
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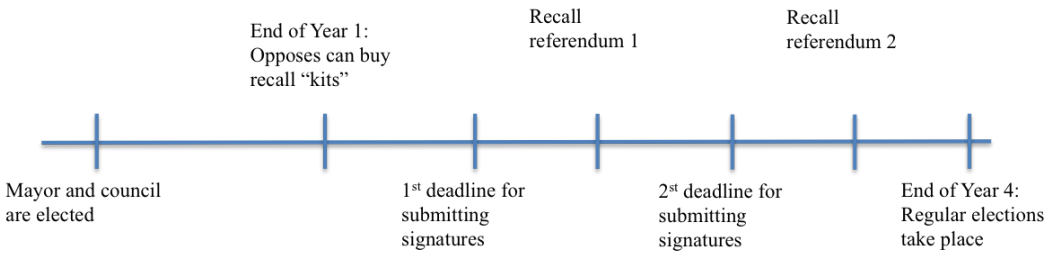
Tables and figures

Graph 1.1: Collected and valid Signatures



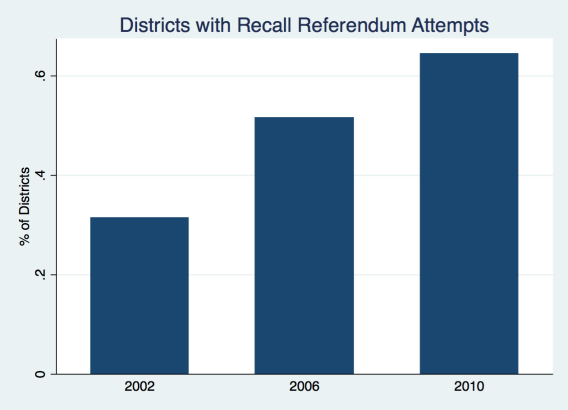
Note: The figure presents the ratio of collected and validated collected signatures for a recall election referenda of municipal mayors in Peru.

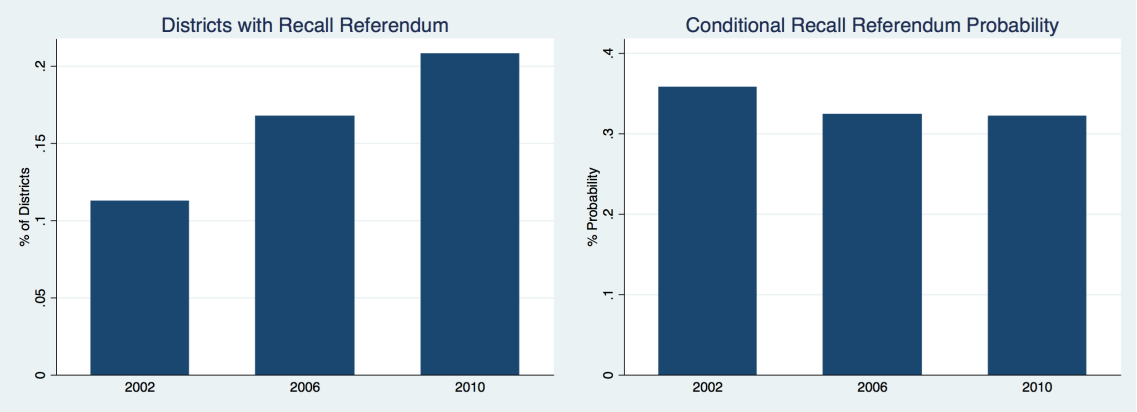
Graph 1.2: Timing of recall referenda

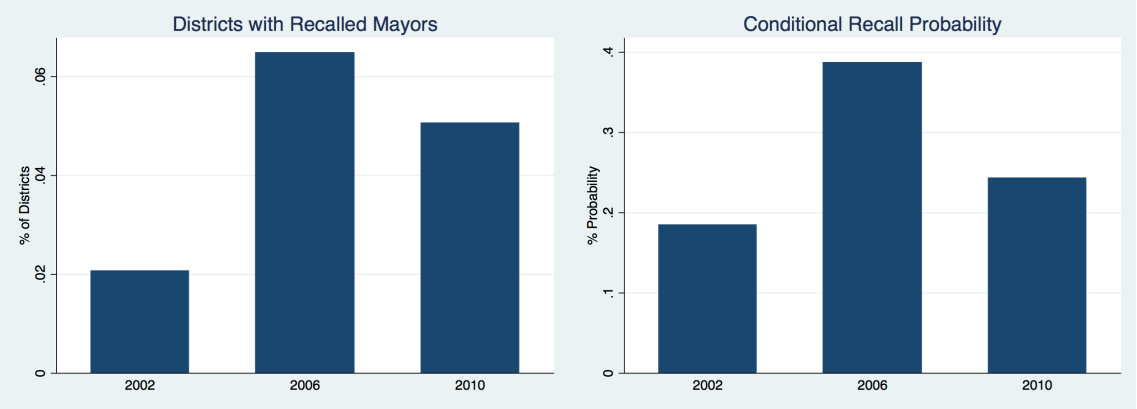


Note: The figure presents the timeline of the different steps of recall processes in Peru.

Graph 1.3: Incidence of recall referenda

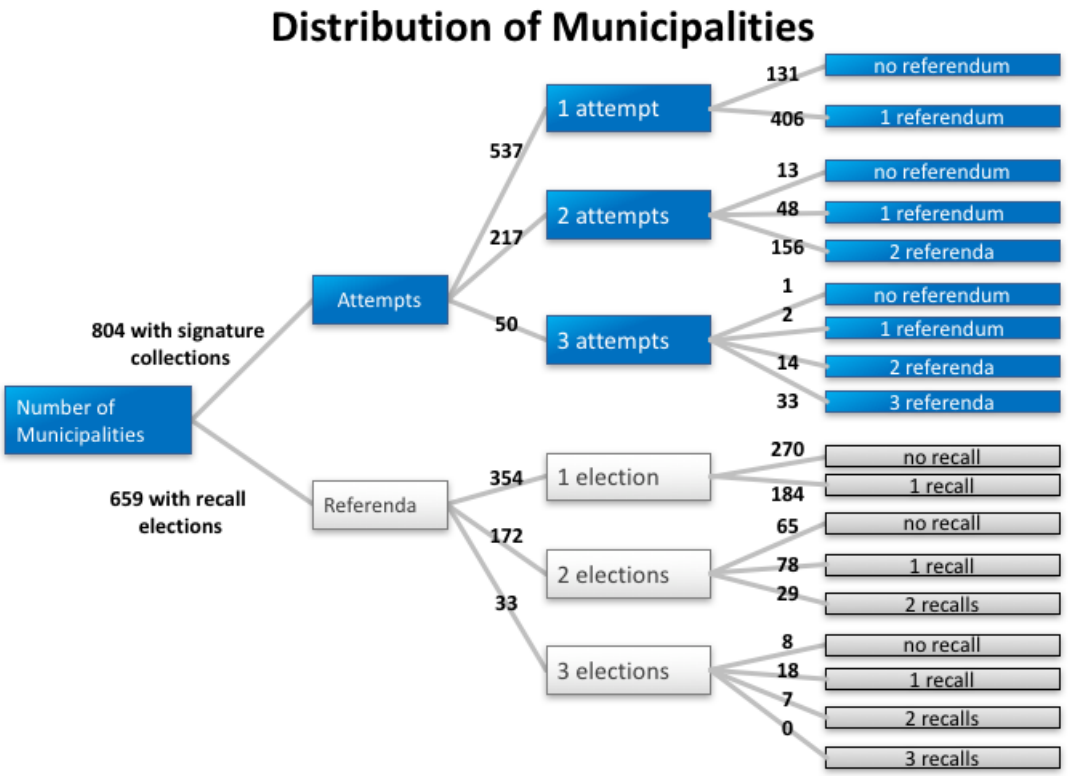






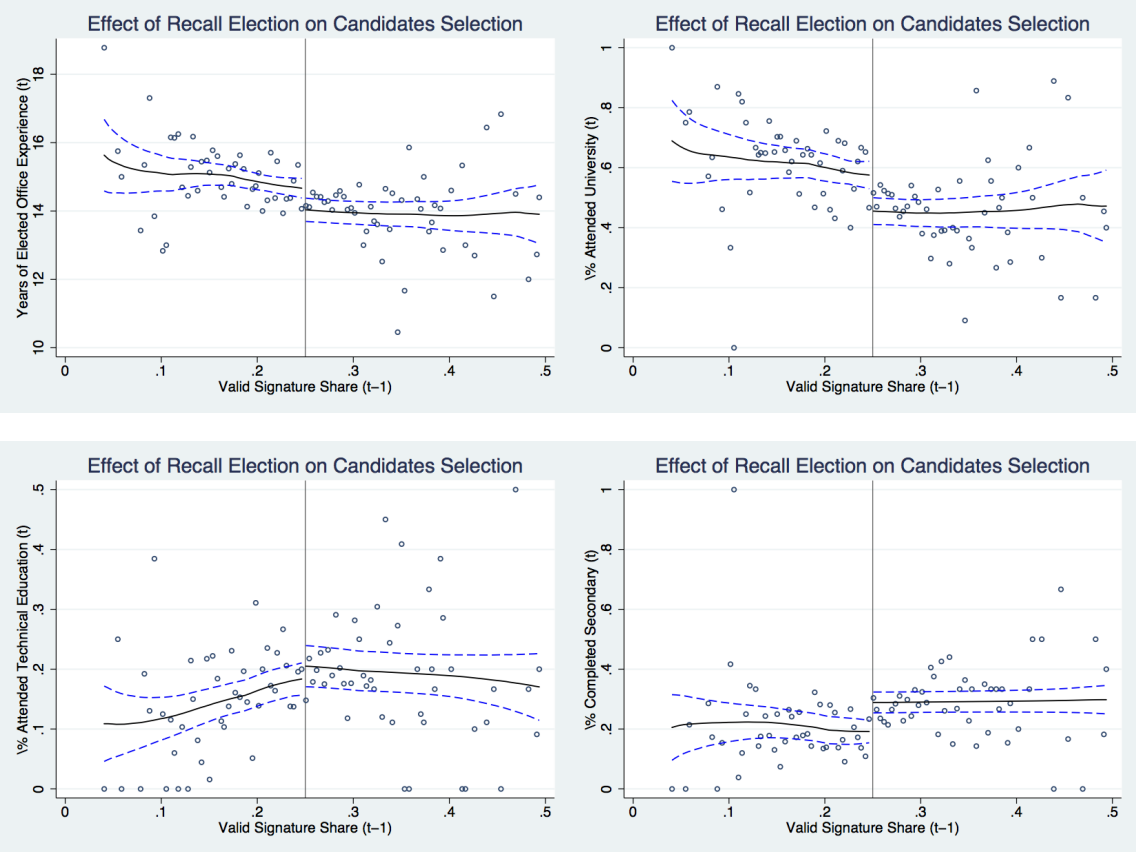
Note: The figures show for each electoral term (i) the proportion of districts in which a recall kit was purchased in order to initiate a recall process against the incumbent mayor, (ii) the proportion of districts in which the incumbent mayor faced a recall referendum, (iii) the conditional probability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

Graph 1.4: Distribution of municipalities



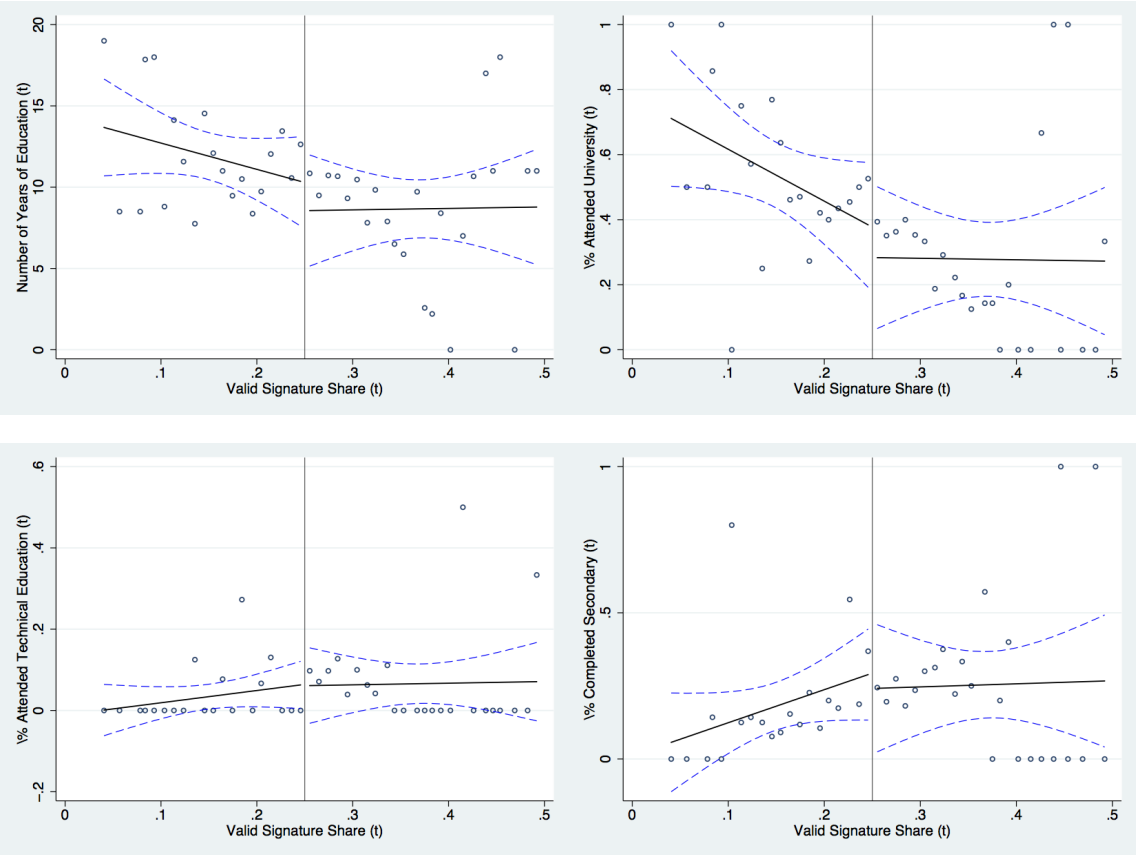
Note: The figure presents the distribution of municipalities according to the number of recall attempts and recall referenda they observed during the three electoral cycles commencing in 2002, 2006 and 2010. The districts are then classified based on how many times the signature collection resulted in a recall referendum or, alternatively, how many times a recall referendum resulted in a successful recall of an incumbent mayor.

Graph 1.5: Non-parametric RD plot: candidate's education



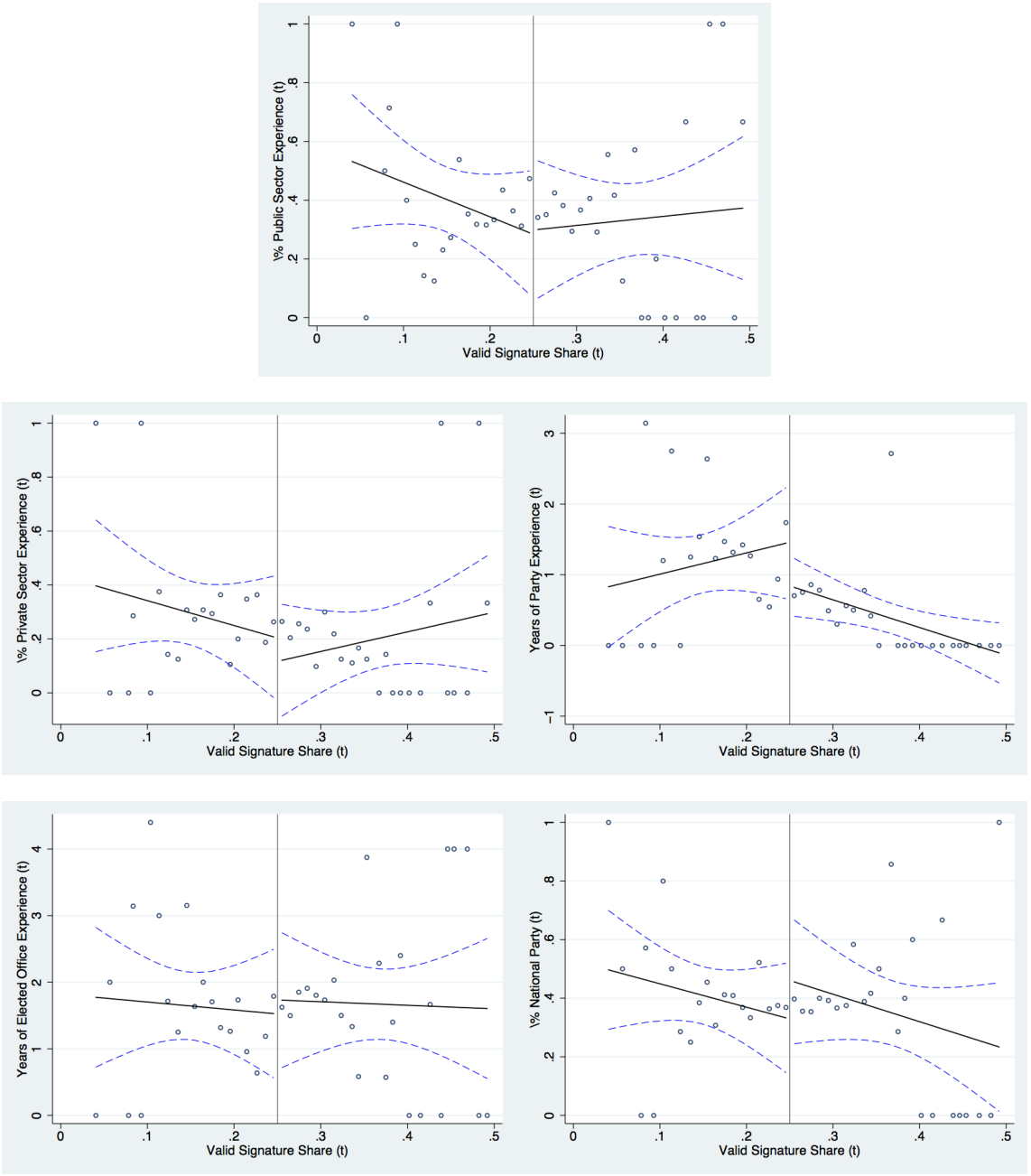
Note: The figures show the results from kernel-weighted local polynomial smoothing plots with Epanechnikov kernels for our main outcome variables and the 95% confidence intervals. The vertical line marks the threshold for a recall referendum, which requires at least valid signatures of 25% of the municipality's electorate.

Graph 1.6: Continuity test: incumbents' education



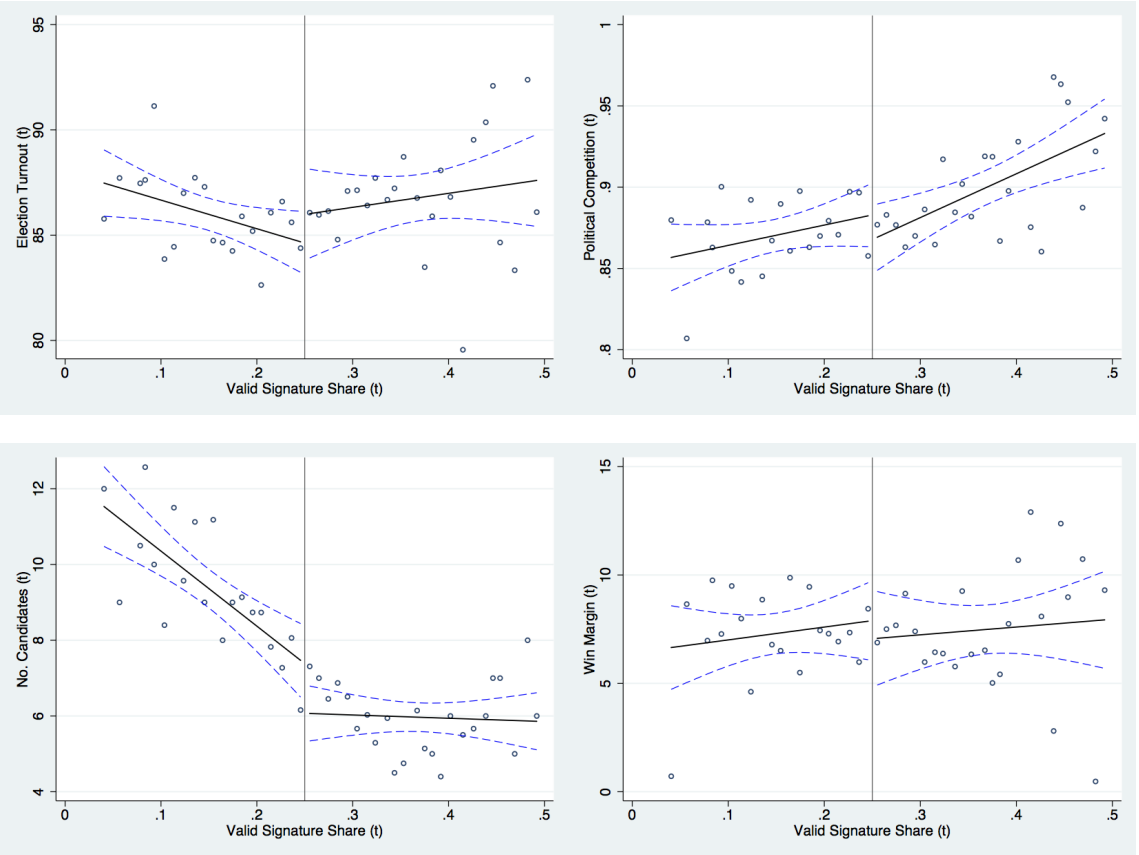
Note: The figures show the continuity tests of incumbents' education based on a linear polynomial plot of the share of valid signatures for a recall election in the same municipality. The vertical line marks the threshold for a recall referendum, which requires at least valid signatures of 25% of the municipality's electorate.

Graph 1.7: Continuity test: incumbents' experience



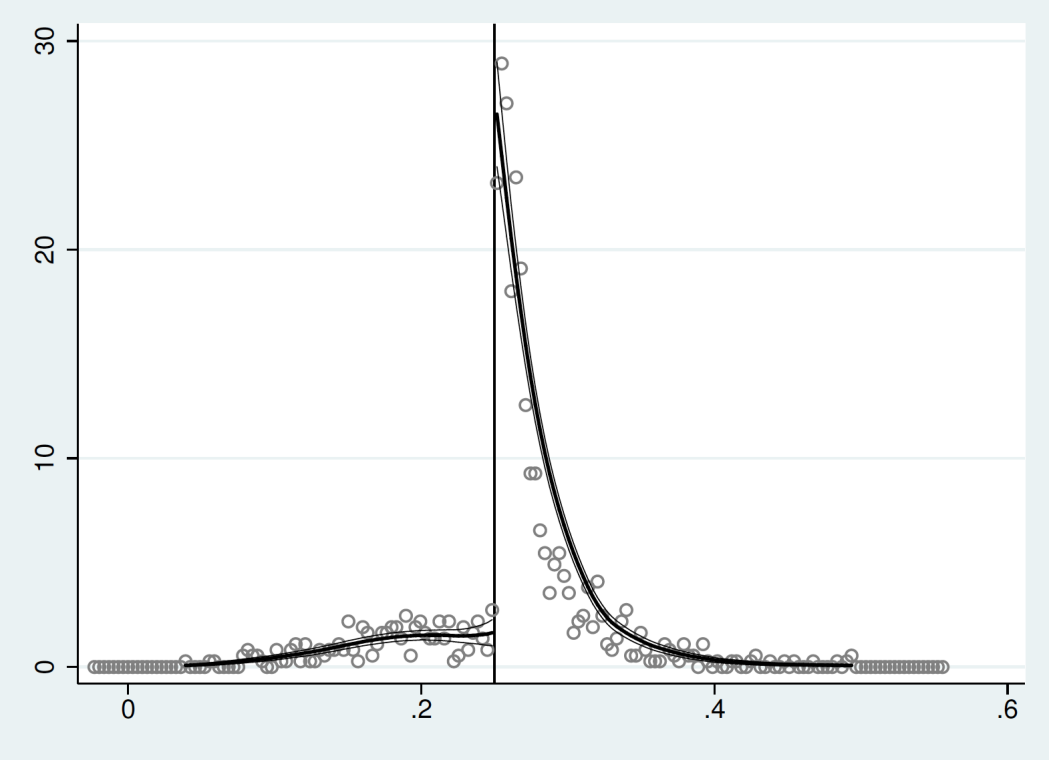
Note: The figures show the continuity tests of incumbents' characteristics based on a linear polynomial plot of the share of valid signatures for a recall election in the same municipality. The vertical line marks the threshold for a recall referendum, which requires at least valid signatures of 25% of the municipality's electorate.

Graph 1.8: Continuity test: political variables



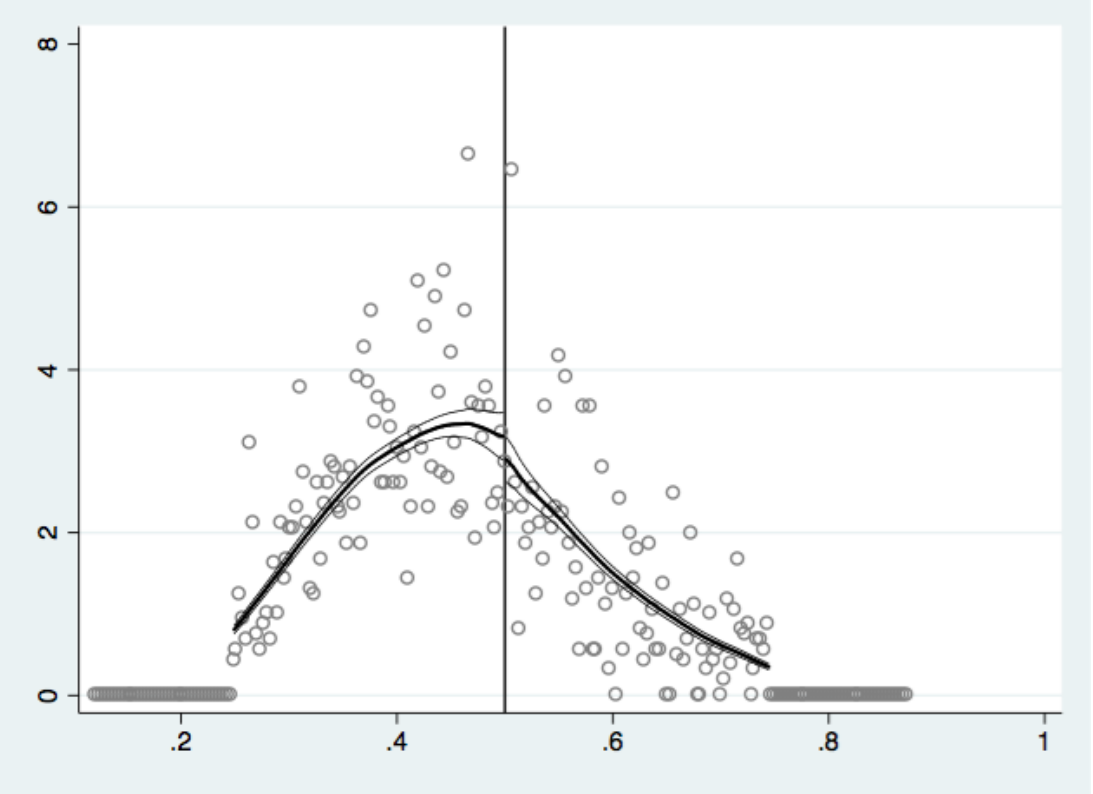
Note: The figures show the continuity tests of political variables based on a linear polynomial plot of the share of valid signatures for a recall election in the same municipality. The vertical line marks the threshold for a recall referendum, which requires at least valid signatures of 25% of the municipality's electorate.

Graph 1.9: McCrary density test - valid signatures



Note: The figure shows the McCrary test for discontinuities in the density of the valid collected signatures in order to hold a recall election referendum at the 25% value (McCrary, 2008). The estimated density is depicted by the thick black line.

Graph 1.10: McCrary density test - recall election vote shares



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 1.1: Predicting recall elections

	Dependent Variable: Recall Referendum		
	Political	Mayor's Edu.	Mayor's Char.
Win Margin (%)	-0.0046***	-0.0046***	-0.0046***
	(0.0005)	(0.0005)	(0.0005)
Turnout (%)	0.0039***	0.0039***	0.0038***
	(0.0012)	(0.0012)	(0.0013)
Number of Candidates	-0.0037	-0.0038	-0.0039
	(0.0024)	(0.0024)	(0.0024)
Mayor's Characteristics			
University		-0.0128	-0.0076
		(0.0123)	(0.0155)
Technical		0.0055	0.0099
		(0.0197)	(0.0220)
Secondary		-0.0139	-0.0102
		(0.0144)	(0.0177)
Age			0.0000
			(0.0004)
Female			0.0587**
			(0.0278)
Public Sector Experience			-0.0068
			(0.0118)
Private Sector Experience			-0.0145
			(0.0124)
Num. years elected office			-0.0028
			(0.0026)
Num. years as mayor			-0.0004
			(0.0021)
Num. years party experience			0.0002
			(0.0032)
National Party Affiliation			-0.0049
			(0.0105)
Election FEs	Yes	Yes	Yes
District FEs	Yes	Yes	Yes
Observations	27816	27816	27816
Districts	1839	1839	1839
Mean Dep.	0.150	0.150	0.150

Note: Robust to clustered standard errors at the district*election level. Mayors' characteristics include the following variables on the mayor's experience and demographics: experience in the public and private sector, years of experience as mayor, years of experience in an elected office, years of experience in a party office, age and gender.

Table 1.2: Descriptive statistics

		Full Sample	RD Sample	Full Sample	RD Sample
		Winners' Characteristics		*Candidates' Characteristics*	
Primary or less	Mean	0.033	0.035	0.031	0.024
	N	5496	115	36526	1183
Secondary	Mean	0.243	0.209	0.227	0.200
	N	5496	115	36526	1183
Technical	Mean	0.144	0.096	0.152	0.144
	N	5496	115	36526	1183
University	Mean	0.580	0.661	0.590	0.632
	N	5496	115	36526	1183
Years of Education	Mean	14.868	15.139	14.804	15.117
	N	5496	115	36526	1183
Num. years elected office	Mean	2.782	2.232	1.629	1.411
	N	6082	125	39765	1256
Num. years as mayor	Mean	1.480	1.768	1.191	1.230
	N	4085	82	26498	813
Num. years party experience	Mean	2.539	1.767	1.280	0.978
	N	5251	103	32695	1021
National Party	Mean	0.447	0.397	0.431	0.399
	N	6136	126	40544	1272
Public Sector	Mean	0.645	0.554	0.605	0.569
	N	4640	121	31771	1171
Private Sector	Mean	0.425	0.562	0.455	0.557
	N	4640	121	31771	1171
Age	Mean	44.083	45.786	45.793	46.775
	N	6136	126	40544	1272
Female	Mean	0.032	0.008	0.067	0.082
	N	6136	126	40544	1272
		District Characteristics			
Number of Candidates	Mean	7.426	9.486		
	N	7293	148		
Win Margin (%)	Mean	8.952	7.938		
	N	7227	146		
Political Competition	Mean	0.868	0.896		
	N	7232	146		
Turnout (%)	Mean	84.570	85.804		
	N	7292	148		
Population (1000s)	Mean	25.966	54.384		
	N	7281	148		

Note: Information on incumbent's characteristics is derived from the CV data of political candidates in Peruvian municipal elections provided by government sources, as described in Section 1.3. The source for the district characteristics is the Peruvian national electoral office (ONPE). The samples of the four columns are defined as follows: (i) all districts and (ii) districts in which a signature collection effort took place in the previous election cycle and the number of collected valid signatures totalled between 20.77% and 29.23% of the districts' electorate. The bandwidth of 4.23% corresponds to the optimally chosen Imbens-Kalyanaraman bandwidth for the variable years of education.

Table 1.3: Accountability and candidates' education

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
PANEL A				
Recall election in t-1	-0.0928	-0.1177**	0.0425	0.1015***
	(0.2758)	(0.0517)	(0.0683)	(0.0290)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	4694	4373	4078	5865
Districts	598	569	532	716
Mean Dep.	13.531	0.400	0.206	0.324
R^2	0.059	0.051	0.067	0.103
Bandwidth	0.042	0.036	0.030	0.087

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to clustered standard errors at the district*election level.

Table 1.4: Accountability and candidates' characteristics

	PANEL A			
	Dependent Variable:			
	Num. years elected office	Num. years as mayor	Num. years party experience	National Party Affiliation
Recall election in t-1	0.3799	-0.1913	-0.6250	-0.0791
	(0.4154)	(0.3128)	(0.9579)	(0.0549)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	4151	4400	3406	4205
Districts	522	583	622	518
Mean Dep.	1.230	0.839	1.024	0.396
R^2	0.114	0.052	0.091	0.031
Bandwidth	0.028	0.039	0.024	0.029
	PANEL B			
	Dependent Variable:			
	Public Sector Experience	Private Sector Experience	Age	Female
Recall election in t-1	-0.1008**	0.0462	-1.1902	-0.0656***
	(0.0435)	(0.0578)	(1.0345)	(0.0208)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	4949	4500	4404	6053
Districts	615	523	533	697
Mean Dep.	0.556	0.442	46.156	0.074
R^2	0.029	0.069	0.042	0.025
Bandwidth	0.048	0.031	0.030	0.059

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to clustered standard errors at the district*election level.

Table 1.5: Opportunity costs imputation

	Dependent Variable: Wages in:			
	2014	2010	2006	2002
	Predictors of Wage Income			
University degree	1394.562***	1061.289***	1019.571***	935.5306***
	(18.0094)	(19.8955)	(16.76031)	(18.71043)
Technical degree	704.008***	516.4129***	500.447***	420.4438***
	(19.4818)	(19.7699)	(16.02192)	(17.50094)
Secondary education	352.1533***	225.7426***	205.3534***	170.2353***
	(16.9574)	(17.4676)	(13.86638)	(15.23986)
Age	54.6524***	38.3112***	29.48052***	30.50162***
	(2.3997)	(2.5940)	(2.17484)	(2.382771)
Age^2	-.4730***	-.3054***	-.2172381***	-.2486141***
	(.0298)	(.0332)	(.0281928)	(.0308864)
Male	268.0153***	190.0213***	114.1772***	104.5481***
	(12.8551)	(13.5912)	(11.2026)	(12.44457)
Urban	-158.1711***	-101.6397***	-63.84002***	-62.32843***
	(17.1407)	(17.5684)	(13.83571)	(15.3332)
constant	-996.595***	-682.8358***	-582.5559***	-569.783***
	(45.7813)	(46.9846)	(38.96525)	(42.27536)
Observations	25,061	17,173	15,011	13,536
Mean Dep.	979.6258	684.5102	779.4675	455.6146
R^2	0.2975	0.2436	0.3074	0.2513

Note: The table depicts the regression of primary income on observable characteristics of Peruvian individuals. The regressions are based on the Peruvian household survey ENAHO that collects information on a nationally representative sample of Peruvian adults. For each survey year, primary income is regressed on the set of explanatory variables.

Table 1.6: Accountability and candidates' opportunity costs: imputed wages

	Dependent Variable:	
	Wage	Wage
	PANEL A: Linear Polynomial	
	(1)	(2)
	Wage	Wage
Recall election in t-1	-408.0584***	-95.9029**
	(156.8435)	(46.0745)
Linear Polynomial	Yes	Yes
District FEs	No	Yes
Election FEs	Yes	Yes
Observations	3356	3356
Districts	453	453
Mean Dep.	1280.100	1280.100
R^2	0.042	0.039
Bandwidth	0.029	0.029

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. A linear polynomial is used in the regressions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to clustered standard errors at the district*election level.

Table 1.7: Robustness checks

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
PANEL A				
Recall election in t-1	-0.1758	-0.1370***	0.0694	0.1121***
	(0.2901)	(0.0488)	(0.0651)	(0.0295)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Mayor's Characteristics	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Observations	4723	4402	4102	5911
Districts	601	572	534	720
Mean Dep.	13.531	0.399	0.207	0.325
R^2	0.063	0.059	0.072	0.109
Bandwidth	0.043	0.037	0.031	0.088
PANEL B				
Recall election in t-1	-0.3204	-0.1223**	0.0482	0.1232***
	(0.2946)	(0.0526)	(0.0665)	(0.0307)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Mayor's Characteristics	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	4694	4373	4078	5865
Districts	598	569	532	716
Mean Dep.	13.531	0.400	0.206	0.324
R^2	0.068	0.064	0.076	0.113
Bandwidth	0.042	0.036	0.030	0.087

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Panel A and B control for a wide set of the incumbent mayors' characteristics such as education, political and professional experience, and demographic information. Panel B additionally controls for the political environment in t-1. Robust to clustered standard errors at the district*election level.

Table 1.8: Robustness check: characteristics of runners-up

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
PANEL A: Dropping re-running Incumbents				
Recall election in t-1	-0.2284	-0.1320**	0.0426	0.0807**
	(0.3167)	(0.0584)	(0.0726)	(0.0326)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Incumbents	No	No	No	No
Observations	4222	3955	3678	5320
Districts	600	569	531	718
Mean Dep.	13.514	0.393	0.209	0.328
R^2	0.052	0.048	0.062	0.902
Bandwidth	0.042	0.036	0.030	0.087
PANEL B: Controlling for Characteristics of Runners-up				
Recall election in t-1	-0.3215	-0.1133**	-0.0104	0.1556***
	(0.4113)	(0.0576)	(0.0774)	(0.0504)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Characteristics Runners-Up	Yes	Yes	Yes	Yes
Observations	3542	3379	3157	4482
Districts	451	432	407	545
Mean Dep.	13.489	0.390	0.194	0.337
R^2	0.067	0.053	0.072	0.112
Bandwidth	0.042	0.036	0.030	0.087

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Panel A excludes all incumbents who re-run for office. Panel B controls for a wide set of characteristics of the second and third placed competing candidates in the preceding election such as education, political and professional experience, and demographic information. Robust to clustered standard errors at the district*election level.

Table 1.9: Accountability and education: placebo test and first time recalls

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
PANEL A: Placebo Test				
Recall election in t-1	-0.5967*	-0.1318**	0.1675***	0.0255
	(0.3341)	(0.0635)	(0.0559)	(0.0415)
Recall election in t-2	0.0631	0.0005	0.0246	-0.0340
	(0.1593)	(0.0275)	(0.0222)	(0.0217)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	4077	3853	3607	5237
Mean Dep.	14.390	0.512	0.198	0.248
R^2	0.060	0.052	0.069	0.106
Bandwidth	0.042	0.036	0.030	0.087
PANEL B: First Time Recall				
First time recall	-0.3371	-0.1184***	0.0057	0.0425
election in t-1	(0.2055)	(0.0258)	(0.0260)	(0.0357)
Linear Polynomial	Yes	Yes	Yes	Yes
Controls	4753	5676	4102	3954
Observations	13.603	0.400	0.206	0.328
R^2	0.054	0.051	0.068	0.102
Bandwidth	0.050	0.082	0.032	0.029

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to clustered standard errors at the district*election level.

Table 1.10: Accountability and candidates' education and characteristics: election RDD

	Dependent Variable:			
PANEL A				
	Years Edu	University	Technical	Secondary
Recalled Incumbent in t-1	-0.5568**	-0.0650*	-0.0178	0.0797**
	(0.2554)	(0.0352)	(0.0279)	(0.0399)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	3597	3853	3479	3495
Mean Dep.	13.506	0.389	0.194	0.344
R^2	0.048	0.032	0.025	0.029
Bandwidth	0.150	0.168	0.142	0.143
PANEL B				
	Num. years elected office	Num. years as mayor	Num. years party experience	National Party Affiliation
Recalled Incumbent in t-1	-0.2040	-0.4184***	-0.0708	0.0065
	(0.2820)	(0.1609)	(0.1736)	(0.0380)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	2520	3792	3745	3457
Mean Dep.	1.341	0.891	0.598	0.394
R^2	0.015	0.019	0.013	0.018
Bandwidth	0.089	0.149	0.146	0.126
PANEL C				
	Public Sector Experience	Private Sector Experience	Age	Female
Recalled Incumbent in t-1	-0.0650	-0.0678	-1.2077	0.0067
	(0.0460)	(0.0471)	(0.8553)	(0.0163)
Linear Polynomial	Yes	Yes	Yes	Yes
Observations	2093	2703	3069	4393
Mean Dep.	0.566	0.427	45.977	0.078
R^2	0.011	0.013	0.016	0.03
Bandwidth	0.079	0.113	0.119	0.182

Note: Regression equations follow Equation 1.2 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01. Robust to clustered standard errors at the district*election level.

Table 1.11: Accountability and candidates' opportunity costs: election RDD

	Dependent Variable:	
	Wage	Wage
Recalled Mayor in t-1	-126.8179**	-55.7545*
	(54.4755)	(32.1049)
Linear Polynomial	Yes	Yes
Election FEs	No	Yes
Observations	3223	3223
Mean Dep.	1235.93	1235.93
R^2	0.053	0.067
Bandwidth	0.129	0.129

Note: Regression equations follow Equation 1.2 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01. Robust to clustered standard errors at the district*election level.

Table 1.12: Accountability and political outcomes

	Dependent Variable:			
	Turnout	Candidates	Win Margin	Pol. Comp.
PANEL A				
Recall election in t-1	0.0092	-0.8200*	2.1053	-0.0318
	(1.3460)	(0.4330)	(2.0727)	(0.0206)
Linear Polynomial	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Observations	619	755	843	742
Districts	501	592	638	584
Mean Dep.	85.587	6.909	8.668	0.895
R^2	0.021	0.016	0.024	0.013
Bandwidth	0.024	0.040	0.056	0.040

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01. Robust to heterogenous and serially correlated standard errors.

Table 1.13: Mechanisms: political opponents as recall promoters, signature RD

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
PANEL A: Political Opponents previous Elections				
Recall election in t-1	0.0092	-0.1092	0.0623	0.0701**
	(0.3547)	(0.0741)	(0.0909)	(0.0328)
Treated * Political Opponent	-0.3365	-0.0229	-0.0499	0.1080***
	(0.4132)	(0.0772)	(0.1004)	(0.0363)
Promotor Political Opponent	0.1356	0.0290	0.0067	-0.0907***
	(0.4108)	(0.0750)	(0.0986)	(0.0330)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	4694	4373	4078	5865
Districts	598	569	532	716
Mean Dep.	13.531	0.399	0.207	0.325
R^2	0.062	0.052	0.068	0.106
Bandwidth	0.042	0.036	0.030	0.087
PANEL B: Political Opponents in t-1				
Recall election in t-1	-0.0076	-0.1073	0.0545	0.0721**
	(0.3547)	(0.0745)	(0.0926)	(0.0330)
Treated * Political Opponent in t-1	-0.2782	-0.0316	-0.0339	0.1101***
	(0.4089)	(0.0786)	(0.1029)	(0.0358)
Promotor Political Opponent in t-1	0.1835	0.0162	0.0154	-0.1062***
	(0.4145)	(0.0759)	(0.1022)	(0.0322)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	4694	4373	4078	5865
Districts	598	569	532	716
Mean Dep.	13.531	0.399	0.207	0.325
R^2	0.061	0.051	0.069	0.108
Bandwidth	0.042	0.036	0.030	0.087

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to heterogenous and serially correlated standard errors.

Table 1.14: Mechanisms: competitiveness and performance before recall Initiation

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
PANEL A: Performance prior Recall				
Recall election in t-1	-0.3574	0.1433**	-0.2925***	-0.0799
	(0.7470)	(0.0649)	(0.0874)	(0.0700)
Treated * % Expense Budget Executed	-0.0418	-0.1893***	0.2561***	0.1072***
	(0.3443)	(0.0318)	(0.0390)	(0.0352)
% Expense Budget Executed	-0.1666	0.1511***	-0.2559***	-0.0838***
	(0.2981)	(0.0222)	(0.0327)	(0.0309)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	3769	3581	3331	4814
Districts	476	457	427	580
Mean Dep.	13.496	0.388	0.198	0.340
R^2	0.060	0.053	0.067	0.106
Bandwidth	0.042	0.036	0.030	0.087
PANEL B: Competitiveness of Election				
Recall election in t-1	0.0485	-0.1304***	0.0891*	0.0293
	(0.3354)	(0.0415)	(0.0486)	(0.0220)
Treated * Difference from				
Mean No. Candidates in t-1	0.0108	-0.0518***	0.0606***	-0.0073
	(0.1264)	(0.0110)	(0.0142)	(0.0081)
Mean No. Candidates in t-1	0.0193	0.0576***	-0.0694***	0.0121
	(0.1363)	(0.0121)	(0.0147)	(0.0081)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	4694	4373	4078	5865
Districts	598	569	532	716
Mean Dep.	13.040	0.380	0.197	0.309
R^2	0.066	0.054	0.070	0.108
Bandwidth	0.042	0.036	0.030	0.087

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to heterogenous and serially correlated standard errors.

Table 1.15: Accountability and winners' characteristics

PANEL A				
	Dependent Variable:			
	Years Edu	University	Technical	Secondary
Recall election in t-1	0.8383	0.0817	-0.1950	0.0191
	(1.2423)	(0.2021)	(0.1834)	(0.1328)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	749	695	632	841
R^2	0.076	0.085	0.106	0.113
Bandwidth	0.042	0.036	0.030	0.087
PANEL B				
	Dependent Variable:			
	Num. years elected office	Num. years as mayor	Num. years party experience	National Party Affiliation
Recall election in t-1	-0.1447	-0.5409	0.8209	0.0012
	(1.2538)	(1.0424)	(1.6818)	(0.2447)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	598	702	575	587
R^2	0.116	0.066	0.073	0.020
Bandwidth	0.028	0.039	0.024	0.026
PANEL C				
	Dependent Variable:			
	Public Sector Experience	Private Sector Experience	Age	Female
Recall election in t-1	-1.2461	-0.0595	0.0343	0.2688
	(4.1965)	(0.0633)	(0.1709)	(0.2416)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	630	872	750	608
Districts	503	662	579	489
Mean Dep.	44.579	0.039	0.583	0.442
R^2	0.02	0.062	0.030	0.018
Bandwidth	0.030	0.048	0.043	0.059

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to heterogenous and serially correlated standard errors.

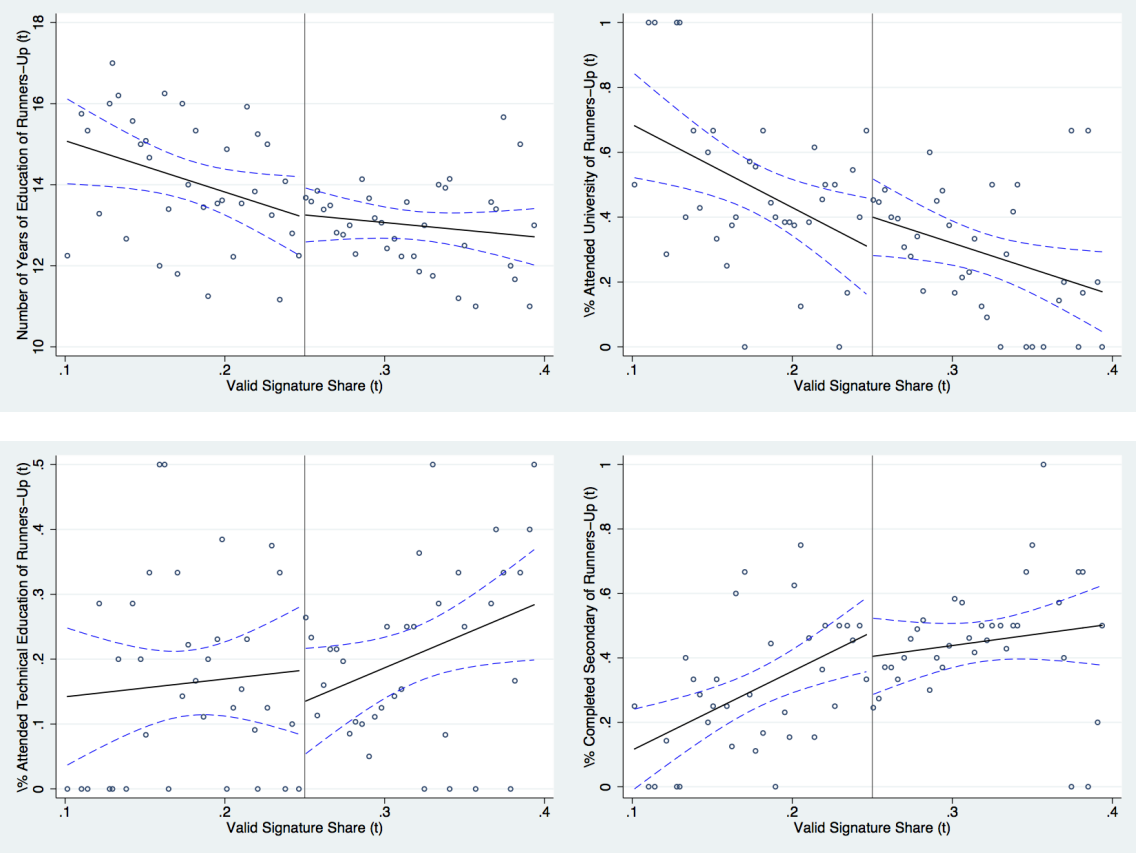
Table 1.16: Public services: effect of accountability on policy choices

	Dependent Variable:			
	Expenses/ Revenue	Executed Expenses/ Budgeted	Revenue	Expenses
		PANEL A		
Recall election in t-1	-0.5496***	-0.4755**	-2769613.2704	-1.3673e+07*
	(0.2047)	(0.2297)	(2111425.6855)	(7780487.0251)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	2345	2345	2345	2345
R^2	0.021	0.031	0.013	0.021
Bandwidth	0.042	0.042	0.042	0.042
		PANEL B		
	Trash Collection	Share Perm. Employees	No. Public Toilets	No. Patrol Guards
Recall election in t-1	0.0673	0.1268	-0.8123*	-2.1567*
	(0.3836)	(0.1264)	(0.4169)	(1.1244)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	2341	2341	2345	2345
Districts	339	339	340	340
R^2	0.178	0.067	0.051	0.021
Bandwidth	0.042	0.042	0.042	0.042

Note: Regression equations follow Equation 1.1 in the paper. In each regression, the sample considered is based on the optimal bandwidth selected for the years of education of candidates, following Imbens and Kalyanaraman (2012). Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01. Robust to heterogenous and serially correlated standard errors.

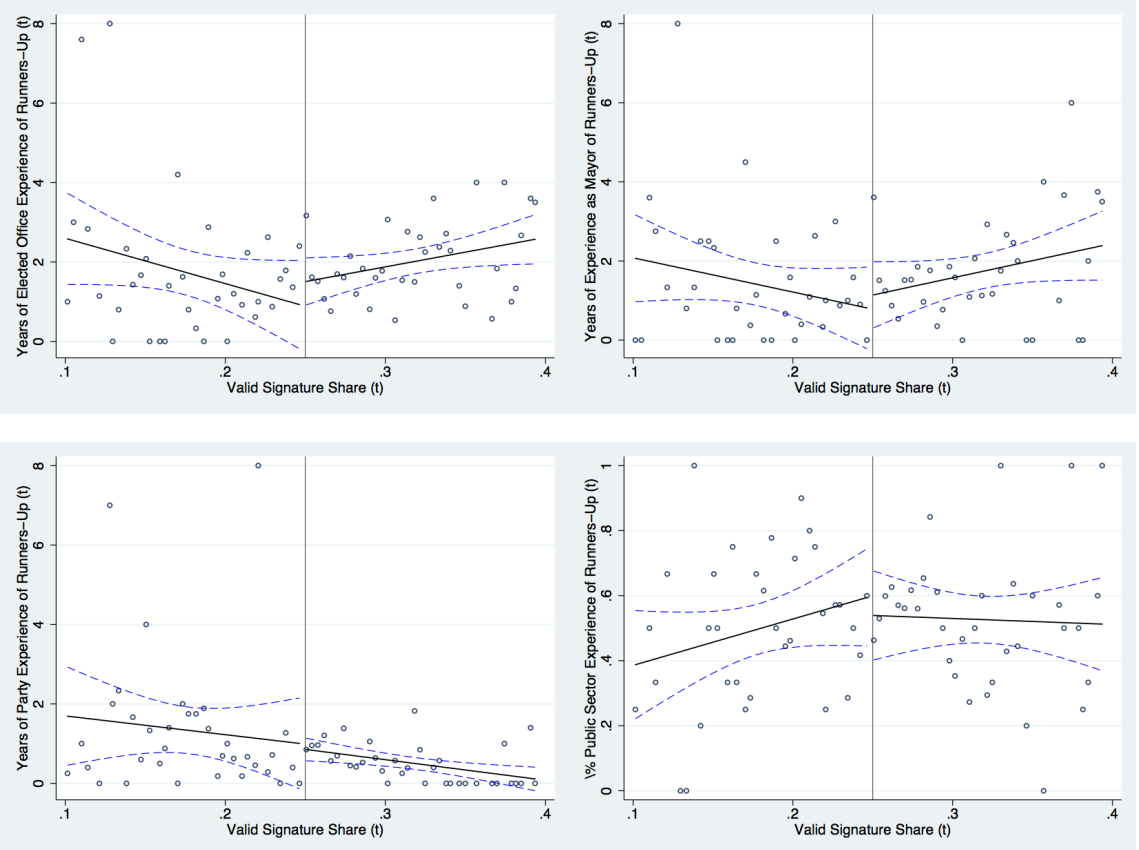
Appendix

Graph 1.11: Linear RD plot of signature collection: election runners-ups' education



Note: The figures show the results from linear polynomial RDD plots for our the education variables of candidates that finished second or third in the preceding election and the corresponding 95% confidence intervals. The x-axis displays the share of valid signatures for a recall election of the incumbent mayor in the preceding electoral cycle.

Graph 1.12: Linear RD plot of signature collection: election runners-ups' characteristics



Note: The figures show the results from linear polynomial RDD plots for our the experience variables of candidates that finished second or third in the preceding election and the corresponding 95% confidence intervals. The x-axis displays the share of valid signatures for a recall election of the incumbent mayor in the preceding electoral cycle.

Table 1.17: Accountability and candidates' education: quartic polynomial

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
		PANEL A		
Recall election in t-1	-1.0346**	-0.1998***	0.0659	0.0736*
	(0.4250)	(0.0711)	(0.0868)	(0.0411)
4th Order Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
R^2	0.058	0.055	0.070	0.102
Bandwidth	0.042	0.036	0.030	0.087
		PANEL B		
Recall election in t-1	-1.1201***	-0.2193***	0.0854	0.0873**
	(0.4140)	(0.0695)	(0.0841)	(0.0414)
4th Order Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	4694	4373	4078	5865
Districts	598	569	532	716
Mean Dep.	13.531	0.400	0.206	0.324
R^2	0.060	0.056	0.071	0.104
Bandwidth	0.042	0.036	0.030	0.087

Note: Regression equations follow Equation 1.1 in the chapter. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). Controls include lagged turnout, lagged win margin, lagged number of candidates, and population. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to clustered standard errors at the district*election level.

Table 1.18: Accountability and candidates' education: 3 percentage points bandwidths

	Dependent Variable:			
	Years Edu	University	Technical	Secondary
PANEL A				
Recall election in t-1	-0.1342	-0.1275**	0.0425	0.0910**
	(0.2913)	(0.0556)	(0.0683)	(0.0416)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
R^2	0.038	0.041	0.051	0.079
Bandwidth	0.03	0.03	0.03	0.03
PANEL B				
Recall election in t-1	-0.8363**	-0.2423***	0.0854	0.1255***
	(0.3777)	(0.0725)	(0.0841)	(0.0475)
4th Order Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	4064	4064	4064	4064
Districts	529	529	529	529
Mean Dep.	13.508	0.400	0.206	0.322
R^2	0.039	0.042	0.052	0.081
Bandwidth	0.03	0.03	0.03	0.03

Note: Regression equations follow Equation 1.1 in the chapter. In each regression, the sample considered is based on a 3 percentage point bandwidth. Controls include lagged turnout, lagged win margin, lagged number of candidates, and population. Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01. Robust to clustered standard errors at the district*election level.

Table 1.19: Accountability and candidates' characteristics: 3 percentage points bandwidths

		PANEL A		
		Dependent Variable:		
	Num. years elected office	Num. years as mayor	Num. years party experience	National Party Affiliation
Recall election in t-1	0.1219	-0.4371	-1.5284	-0.1491***
	(0.3833)	(0.3558)	(1.6127)	(0.0534)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	4299	4003	3680	4400
Mean Dep.	1.233	0.842	1.045	0.399
R^2	0.094	0.047	0.081	0.030
Bandwidth	0.03	0.03	0.03	0.03
		PANEL B		
		Dependent Variable:		
	Public Sector Experience	Private Sector Experience	Age	Female
Recall election in t-1	-1.1902	-0.0806**	-0.1137**	0.0358
	(1.1652)	(0.0319)	(0.0491)	(0.0602)
Linear Polynomial	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Election FEs	Yes	Yes	Yes	Yes
Observations	4400	4400	4400	4400
Mean Dep.	46.162	0.074	0.555	0.441
R^2	0.023	0.051	0.033	0.021
Bandwidth	0.03	0.03	0.03	0.03

Note: Regression equations follow Equation 1.1 in the chapter. In each regression, the sample considered is based on bandwidths of 3 percentage points. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust to clustered standard errors at the district*election level.

Table 1.20: Accountability and candidates' opportunity costs: imputed wages

	Dependent Variable:	
	Wage	Wage
	PANEL A: Quartic Polynomial	
Recall election in t-1	-276.7147	-132.7757**
	(203.1707)	(52.7926)
4th Order Polynomial	Yes	Yes
District FEs	Yes	Yes
Election FEs	No	Yes
Observations	3356	3356
Districts	453	453
Mean Dep.	1280.10	1280.10
R^2	0.044	0.040
Bandwidth	0.029	0.029
	(1)	(2)
	PANEL B: Wage imputed with rural interactions	
Recall election in t-1	-35.5773	-76.9135*
	(45.0865)	(46.2339)
Linear Polynomial	Yes	
4th Order Polynomial		Yes
District FEs	Yes	Yes
Election FEs	Yes	Yes
Observations	3356	3356
Districts	453	453
Mean Dep.	1099.47	1099.47
R^2	0.046	0.042
Bandwidth	0.029	0.029

Note: Regression equations follow Equation 1.1 in the chapter. In each regression, the sample considered is based on the optimal bandwidth, following Imbens and Kalyanaraman (2012). In Panel A, a quartic polynomial is applied. Panel B is based on the imputed wages that account for rural-urban differences in the explanatory variables. Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01. Robust to clustered standard errors at the district*election level.

Chapter 2

Strategic Expropriation and Civil War

2.1 Introduction

Civil wars are a major impediment to economic development. Depending on the definition in terms of the annual number of battle-related casualties, internal civil conflicts have occurred in more than a third or half of the world's countries since World War II.[1] Yet, such internal conflicts are particularly frequent among poorer developing countries. The relationship between poverty and internal conflicts is not unidirectional. Conflicts and wars are detrimental for human welfare, they reduce human capital and wealth, and inhibit growth, while low income per capita or low economic growth are correlated with higher propensities of war (Collier, 1999; Collier et al., 2003). Scholars of the literature on civil war have recently emphasised two stylised facts common to many examples of internal conflicts. First, there is a strong link between civil wars and conflicts over land. Second, war afflicted countries are extremely vulnerable to conflict recidivism.

Civil wars and conflicts over land are inextricably linked, with the causation going both directions (UN-Habitat, 2012). On the one hand, land-related issues may cause conflict. On the other hand, civil conflicts can give rise and spill over to become struggles over land. Scholars have shown that land-related grievances and inequality

[1]Most works rely on the widely disseminated dataset provided by UCDP/ PRIO, based on work by Gleditsch et al. (2002) and Harbom and Wallensteen (2010). The most common definition of conflict articulated by UCDP/PRIO is founded on armed contests that entail at least twenty-five battle-relate deaths in a given year.

in terms of land possession can fuel violence and conflict (Albertus and Kaplan, 2013; Deininger and Feder, 2001; Barraclough, 1970). Other works such as Esteban and Ray (2011) or Grossman (1994) analyse theoretically the role of disputes over land and its distribution in driving the onset of civil wars. Land inequality might not only matter for the onset of civil war but can also be a determinant of the intensity of conflict (Luca and Sekeris, 2010). At the same time, violent conflicts and civil wars affect land tenure in a variety of ways. Most notably, otherwise unrelated conflicts can spill over to struggles over the control and possession of land. During conflicts land and other assets get appropriated by rebels or other factions involved. Such appropriation can often be attributed to greed for economic rents rather than ideological reasons. This notion has been a constant in the debate on the inherent motivation of rebels to fight and participate in civil wars that is often referred to as greed versus grievances.[2] Several works such as Mesquita (2013), Blattman and Miguel (2010), Korf (2005), or Kalyvas (2006) offer theoretical forays and empirical investigations of the relationship between land or assets and conflict. The relevance of land with respect to studying scenarios of civil war and conflicts does not stop here. Expropriations and displacements are tragically common in many conflicts. The question of how such injustice is dealt with is of fundamental importance. For instance, land restitution can be essential for the sustainability of peace once a conflict has ended (Beyers, 2013; Rhodri, 2006; Leckie, 2009).

In this paper, we investigate the impact of demobilisation on strategic behaviour of non-state actors, using the case of the Colombian civil war. In 2002, following the election of Álvaro Uribe as president, the Colombian government entered official peace negotiations with right-wing paramilitaries that resulted in the unilateral demobilisation of those insurgents.[3] Between 2003 and 2006 more than 20,000 fighters

[2]Blattman and Miguel (2010) give a brief overview on these two competing views. Two seminal works in this debate are Fearon and Laitin (2003) and Collier and Hoeffler (2004). Both studies use cross-country observations to evaluate the importance of grievances and economic factors in explaining civil war onset and incidence. They find that it is not ethnic or religious diversity or polarisation that could approximate grievances but instead economic factors that explain which countries are more likely to experience civil wars. Specifically, they point out that economic conditions related to the feasibility of rebellions are of crucial importance.

[3]Uribe ran on a clear agenda of containing non-state actors' power and stopping their violent activities. While he proposed military action against left-wing guerrillas, he advocated a negotiated solution with the leaders of the right-wing paramilitary groups.

were disarmed in a process that included thirty-seven different paramilitary groups. Most paramilitaries, who had locally acquired considerable political and economic influence, had several years of clear knowledge of their upcoming demobilisation and thus sufficient time to prepare for and anticipate the post-conflict era. The election of Uribe as well as the soon initiated peace negotiations marked a dramatic shift in the paramilitaries' time horizon and incentives. Our paper analyses the time gap between peace agreement and actual demobilisation to assess the presence of systematic changes in the behaviour and actions of paramilitaries. We focus on expropriation of land in anticipation of the agreed demobilisation, i.e. the years paramilitaries eventually laid down their arms and lost their military power. The Colombian context assigns special meaning to land as it has long been the source of political and economic influence (Mazzuca and Robinson, 2009).

Our approach is novel in a number of ways. First, we provide evidence on what happens at the twilight of conflict using unique and detailed information on the key features of the demobilisation process. Thanks to so far unused data on land restitution claims, we are able to distinguish between deliberate expropriations and, potentially unintended, displacements. Displacements can be the side effect of violence whereas expropriation requires targeted action. By exploiting variation in the timing of the demobilisation process of paramilitaries, we provide evidence of strategic violence and patterns of insurgent activities. We show that paramilitaries systematically expropriate land in the years prior to their demobilisation. Furthermore, such expropriations disproportionately affect areas with greater economic value, confirming our hypothesis that paramilitaries' actions, at least at that stage of the conflict, are dictated by economic greed. Such anticipatory violence does not extend to the appropriation of assets other than land. We interpret our findings as evidence of paramilitaries' endeavour to consolidate their economic and political power. Control and possession of land is the decisive mechanism that allows them both to secure future rent flows as well as exhibit political and electoral influence.

Our findings matter for a variety of reasons. First, they contribute to the study of risks of conflict recurrence as well as the wider literature on conflict and its

economics causes and consequences. Previous work has shown that the nature of conflict termination affects the propensity of conflict recurrence and the stability of peace.[4] Second, we link up with the political economy literature on state capacity and its importance for economic development. Our findings provide evidence of non-state actors' ability to maintain economic and political power even in a post-war environment. Such state capture via appropriating land can significantly undermine the state's monopoly on power, with potentially manifold adverse consequences.[5]

Our analysis sheds further light on the greed versus grievances debate on the causes of conflicts. Although our study focuses on economic reasons for expropriations and provides new evidence on the materialistic incentives for violence, the overarching conflict cannot be understood without also considering long-standing grievances. Thus, our work links up to Collier and Hoeffler (2004) who articulate the idea of economic factors driving individual decisions to participate in violence whereas social or ethnic grievances give rise to a sense of unity within factions and create clear separating lines to outsiders, effectively providing the necessary emotional and social conditions under which factions can exist.[6]

Apart from the aforementioned topics, our analysis also contributes to several other related literatures. A burgeoning strand in the conflict literature focuses on within-country studies. Their main advantage over earlier cross-country examinations is the opportunities with which they enable more robust and causal empirical analysis of the determinants of conflict. A lot of recent work in this literature has focussed on the mechanisms through which economic conditions drive or reduce violence. At the core of this literature are studies that investigate the impact of income

[4]Since WWII, more than half of countries with a previous civil war experienced conflict recidivism (Caplan and Hoeffler, 2017).

[5]A nice illustration of this constellation is given by Acemoglu et al. (2013).

[6]Our study also contributes to the broad literature on the economic significance of property rights. Unresolved displacement and expropriation erode the rule of law and the notion of property rights. As a result, economic costs might incur, ranging from disincentives for investment in human and physical capital to stalling economic development or persistence of poverty. Our findings also matter due to their implications for inequality, specifically land inequality, in Colombia, where land inequality is among the highest in the world. Hence, large scale expropriations of predominantly small landowners exacerbate inequality. As in many developing countries, land is a fundamental asset for economic gains and subsistence. Its distribution is therefore vital for economic welfare of large groups of society and economic development in general. For those reasons, expropriations can incite new grievances and be the motivation for potential future conflicts.

shocks. They distinguish between two channels through which such shocks can affect violence: (i) the returns to violence channel and (ii) the opportunity cost channel. The predominant causes for changes in these channels examined in the literature are driven by income shocks, most prominently exogenous shocks to commodity prices.[7] Our investigation enriches this strand in the literature by showing that expropriations, and violence more generally, can also be caused by a change in incentives and expectations about the future state of the world. We provide new insights into systematic behaviour of non-state actors at the dawn of a conflict. Previous micro data work has looked at causal explanations for violence at the onset or during conflicts. We demonstrate that targeted violence can also systematically occur and even intensify at the end of a conflict.

Furthermore, our analysis is fundamentally motivated by earlier work on the economic legacy of civil war (Abadie and Gardeazabal, 2003; Serneels and Verpoorten, 2015; Singhal and Nilakantan, 2016; Collier and Duponchel, 2013). Such work identifies political institutions as a vital mechanism. Civil wars affect, often eradicate, and sometimes reform pre-existing institutions that are a key determinant of post-conflict economic performance. Some authors argue that internal wars can in fact lead to *better* institutions as they can be a catalyst to state-building, raising state capacity (Powell, 2013; Weber, 1965). However, a necessary condition for such a positive impact is that the state successfully establishes a monopoly of violence. Under many circumstances this might not be the case. For instance, stratified systems of local control can emerge in civil wars or sectarian or ethnic tensions can stimulate electoral violence, both undermining the state's monopoly on power and weakening political institutions (Toft, 2010; Joshi et al., 2017; Brancati and Snyder, 2013). Despite the variety of views on consequences of internal wars, it is consensus that civil wars matter for post-conflict institutions and it is evident that how civil wars end is of extraordinary importance for shaping institutions (Toft, 2010). We focus exactly on the very nature of conflict termination and provide evidence

[7] Alternatively, the effect of climatic shocks with implications on agricultural income, for instance rainfall shocks, provide exogenous variation that is used to study the effects on conflict (Dube and Vargas, 2013; Verwimp, 2003; Brückner and Ciccone, 2010).

of widespread, mostly, unpunished land expropriations that might potentially have undermined the national government's power as well as diminished society's trust in the country's legal justice system. Hence, our analysis complements the literature on political institutions.[8]

Finally, our work emphasises the sensitive nature of political transitions. Roland (2002) offers an overview of the the literature on the political economy of transitions. If political transitions are slow and the transition period takes too long, then actors such as elites of the former regime can exploit their de facto power to either undermine reforms or take strategic anticipatory actions to strengthen their own position in the new regime. In a recent study, González and Prem (2017) examine firms' productivity before, during, and after democratisation in Chile. They demonstrate that politically connected firms exploited their links to the Pinochet regime to receive more loans from state-owned banks during the transition period, which lasted for seventeen months, and thus enhanced their productive capacity with a lasting effect. In related work, Martinez-Bravo et al. (2017) demonstrate that slower political transitions can be captured by elites, leading to worse governance outcomes, worse public goods provision, lower political competition, and higher persistence of pre-existing elites. Our work investigates a different type of political transition, that from a state of civil war to peace, but it equally studies the strategic behaviour that takes place during a prolonged transition period. We show that paramilitaries use the period between the beginning of peace negotiations and their demobilisation to fundamentally strengthen their future economic and political power. Therefore, we demonstrate that slow transition periods cannot only be captured in the case of democratisations but may also have adverse consequences in the case of civil war termination.

The remainder of the paper is organised as follows. Section 2.2 explains and describes the historical context of the civil conflict in Colombia and discusses the

[8]It also illustrates the significance of historical roots and colonial legacies for long-term development, the literature of which came to prominence with the works of Acemoglu et al. (2001) and Sokoloff and Engerman (2000), by providing illustrative examples of the long-lasting shadow of Spanish colonial rule with respect to division of land and power.

emergence and evolution of paramilitaries as non-state actors in the conflict. Furthermore, it provides an overview of the most important aspects of the peace treaty negotiations between the national government and the paramilitaries as well as the demobilisation process of the latter. Section 2.3 presents our data, visually illustrates the conflict intensity and dispersion of non-state actors in Colombia, and presents our conceptional framework and research questions. In Section 2.4, we introduce and explain our empirical strategy. Section 2.5 discusses our baseline estimates and presents our main results as well as heterogeneity of effects across different types of land and their economic attractiveness. Moreover, we briefly discuss some potential mechanisms for our findings and explain our preferred interpretation. Section 2.6 addresses some of the concerns about the results and reports a number of robustness checks that corroborate our findings, support the underlying assumptions, and rule out alternative explanations. Finally, in Section 2.7 we conclude, reappraise our contribution to the wider literature, and also discuss the importance and relevant implications of our findings.

2.2 Background

Explaining the history of violence and the persistence of civil conflict in Colombia with all its intricacies is a formidable task and beyond the scope of this paper. As a consequence, in describing the historical context we do not make any claims of completeness. In the following, we will delineate a selected, brief overview of the historical and political background of the main elements that matter for our study. In the Appendix 2.7, we provide a short summary of the civil war in Colombia since World War II. In doing so, the importance of land in the Colombian context as a source of power and cause of violence is described in detail. Below, we will outline the emergence and characteristics of paramilitaries as a powerful non-state actor. Furthermore, we illustrate the main aspects of the peace negotiations and demobilisation of the paramilitaries that took place between 2002 and 2006.

2.2.1 Paramilitaries in Colombia: history, evolution, and motivation

Until the 1980s, the armed conflict in Colombia was characterised as a low-impact internal conflict (see Figure 2.1 for a simple timeline of the civil conflict in Colombia). Guerrillas, with communist ideology,[9], sought to take over the government through the means of violence. As a side effect, paramilitary groups were created as self-defence militias to provide protection against looting by and victimisation from guerrilla groups. A broad illustration of their history and evolution is given by Figure 2.2.

As the value of coca cultivation grew, the conflict's impact increased. Territories that were under control of guerrillas became ideal locations to plant and trade illicit crops. In parallel, drug dealers and traditional landowners that did not share interests with guerrillas organised the expansion of small self-defence militias throughout Colombian territory. The first high-scale paramilitary manifestation took place in 1981, when a sister of a powerful drug dealer was kidnapped by guerrillas. As a response, the main drug barons created a paramilitary group to liberate her. This structure later became typical for paramilitary groups (Ibáñez and Muñoz-Mora, 2010; Offstein, 2003).

In the 1990s, the course of the Colombian conflict changed. At first, the "war on drugs" became the main focus of military operations as a response to the assassinations of political leaders and presidential candidates ordered by the drug cartels (GMH, 2013). Guerrillas took advantage of this situation by increasing their territorial control and military capacity. While the government was fighting drug cartels, the rebels quickly conquered new territories, took in dissidents from demobilised rebel groups (i.e. EPL and M-19), and strengthened their illegal financial channels (e.g. extortions, kidnappings, drug industry) (GMH, 2013; Peña, 2013). Meanwhile, paramilitaries took the place of the main cartels and started a military offensive against the expansion of guerrilla groups.

[9]In Colombia, there had been several guerrilla groups since middle of the 20th century. The most important are: the Revolutionary Armed Forces of Colombia "FARC" (Spanish acronym), founded in 1964 as the military wing of the Colombian Communist Party and the National Liberation Army "ELN" (Spanish acronym), founded in 1965 as a radicalisation of the liberal party (Offstein, 2003).

After 1994, the conflict between the paramilitaries and guerrillas was characterised by the use of several strategies: direct military confrontation; selective and indiscriminate violence against those suspected of aiding the enemy; displacement and repopulation of vacated areas; and alliances with other actors, such as the state and industrialists. In 1997, several paramilitary groups from different regions allied and united into one large umbrella organisation for military purposes: the United Self-Defence Forces of Colombia (AUC). This provided them with the required strength to push the rebels further into the periphery, allowing them to control the main coca production centres. Guerrillas fought back and the actions of both sides resulted in a major humanitarian crisis, with thousands dead or displaced from their land.

In zones under paramilitary control, paramilitaries filled the void left by a weak state. They imparted justice, resolved citizen disputes, and provided security, thus creating an alternate order. In 2002, paramilitary leaders entered into negotiations with the government that led to their demobilisation and reintegration into civil society. The last of the paramilitary units turned in their weapons in 2006. Militants were granted amnesty, while leaders and those in commanding positions, were offered reduced incarceration sentences.

Paramilitaries took advantage of their increased control to dispossess, annex, resettle, buy, merge, and exploit land with relative ease. They used such land to develop agro-industrial projects and to produce or traffic cocaine. As demobilisation loomed closer, AUC leaders started buying land and investing in agro-industrial enterprises as a way of keeping their assets safe from the state.[10]

An important aspect of paramilitary history in Colombia is their evolution. Starting out as protection or self-defence units, they soon expanded their actions and control to other areas. They entered and eventually took over the lucrative drug production and narco trafficking from the large drug cartels. Individual materialistic

[10]They vied to make this process appear legal, by buying off local state officials, forging documents, and formalising their acquisitions. This led to a penetration of illegality into otherwise legal activities, especially palm oil production.

incentives for profits became more important and replaced political ideology as the main motivation for continuing to fight.[11]

Another essential characteristic of paramilitaries is their large degree of fractionalisation. They were divided into many different subgroups, called blocks, each of which exhibited local control over a different geographic area. A potential explanation of this particularisation could lie in the limited capacity to both control and uphold loyalty among troops with increasing number of fighters (Weinstein, 2005). Similarly, the existence of large geographic distances can give rise to a splintering of factions (Gates, 2002).[12] In total, there were almost 40 separate paramilitary blocks with independent command structures, leaderships, and geographic zones of influence. These blocks were not only geographically dispersed but constituted an extremely heterogenous collection of independent groups. They consisted of local warlords, drug barons, organised crime, local political or economic elite, and counter-insurgent groups. In essence, the blocks were idiosyncratic and their actions were largely based on the respective local leadership (International Crisis Group, 2007).

2.2.2 The peace negotiations and demobilisation process

The beginnings of the peace negotiations between the Colombian national governments and the paramilitaries date back to the events that unfolded prior to the presidential election in 2002. During president Andres Pastrana's term (1998-2002), concerted efforts to reach a separate peace deal with the left-wing guerrillas failed.[13]

[11]Such an evolution of non-state actors over their lifecycle has been discussed in the literature, e.g. in Weinstein (2005) who articulates that rebels over time turn their focus to material benefits regardless of their original motivation.

[12]Recent work by Mayoral and Ray (2017) shows that small groups may be more likely to be involved in conflict against the state when the prize/returns are private, which is the case in the Colombian civil war.

[13]In October 1998, shortly after coming into office, the new president accepted the demands the FARC set for resuming and reinitiating peace talks: (i) the official recognition of the FARC as a political actor in Colombia; and (ii) the demilitarisation of a large territory in the southwest of Colombia of more than 42.000 km2, an area roughly of the size of the Netherlands. While the former offered the FARC increased legitimacy, the latter had the official purpose of establishing a neutral zone where the different actors involved in the peace process, the government, the members of FARC, civil society and representatives of the international community, could pursue negotiations in a safe environment. Whereas the government soon withdrew its security forces and institutions from the zone, the FARC reneged on the agreed terms. Instead it consolidated its grip over the area and the wider South of Colombia to create a safe haven for its fighters. At the same time, it used the government withdrawal to increase its illegal crop activities (Peña, 2013) and utilised

The unsuccessful endeavour of securing a peace deal with the FARC paved the way for Alvaro Uribe's bid for the presidential election in 2002, who took a much harder stand on guerrillas. Uribe announced his candidacy in 2001 and pursued a clear agenda with regard to non-state actors in Colombia. He ran on a platform that promised a far-reaching containment of armed groups. His strategy consisted of two key elements. On the one hand, he favoured sealing a partial peace deal with the paramilitaries resulting in their demobilisation and disarmament. On the other hand, he proposed to fight the guerrillas by curtailing their power and financial resources through military action rather than through further negotiations. Following his election, Uribe started the largest counter-insurgency initiative of Colombia's history and, propped up by international aid predominantly provided by the United States under "Plan Colombia", the Colombian military was strengthened significantly and regained control of vast areas from the guerrillas (Dube and Naidu, 2015).

Uribe's election and his publicly expressed priority of resolving the violent conflict by all means constituted a clear shift of the paramilitaries' incentives and motivation to continue their military activities. In 2002, they agreed to a unilateral cease fire, which was a prerequisite for negotiations with the government. On July 15th 2003, the government reached an agreement with most AUC groups that required them to disarm by the end of 2005. The legal foundation of the demobilisation and the subsequent treatment of demobilised militants was based on Law 418 of 1997 and its amendments.[14] Although nominally addressed to all members of illegal armed groups who voluntarily desert or demobilise, its later amendments were specifically created to appeal to paramilitaries. In June 2005 the "Justice and Peace Law" (JPL) was passed. It limited prison sentences for any human rights violation, however severe it might have been, to 5 to 10 years. Lesser crimes were not to be punished at all. These provisions gave a de facto amnesty for 92% of disarmed paramilitaries, the only exception being individuals under investigation for serious violations of international

the strategic location of the demilitarised area, only 100 km from the country's capital Bogotá, as a launching zone for further military expansion and attacks both on civilians as well as state institutions (Muñoz-Mora, 2016).

[14]Colombian Law 782 of 2002 and Decrees 128 of 2003, 3360 of 2003, and 2767 of 2004.

human rights or humanitarian law, such as massacres or kidnappings, which was criticised by various human rights groups. Nevertheless, despite an intervention of the constitutional court, upon public pressure and demands for legal justice for the numerous victims of paramilitary violence, only mildly modified the terms for demobilising combatants. In order to qualify for the programme or reduced prison sentences they were required to give a full confession of their activities.

There were several reasons why paramilitaries agreed to participate in the peace negotiations. First, they faced a newly elected government with a firm commitment to restoring the state's monopoly on violence and plenty of resources through international aid that would allow it to pursue the necessary actions to achieve this goal, peacefully or, if needed, by force. Furthermore, the government's large-scale operations against left-wing guerrillas undermined the paramilitaries' alleged raison d'être: fighting the ideological socialist nemesis and protecting citizens from its reach. Second, by entering joint negotiations the paramilitaries were hoping to be able to effectively tailor some of the deal's agreements to their advantage.[15] Foremost concerns were preventing a potential extradition to the US, minimising any possible prison term drastically and laundering illegally acquired wealth (Human Rights Watch, 2005). Finally, the design of the process gave plenty of other incentives to militants to lay down their arms. They were offered a diverse set of economic benefits that among others consisted of shelter, health care, education and vocational training in order to facilitate their reintegration into society.[16] Crucially, the question of returning seized assets such as occupied or expropriated land was mostly absent from the negotiations. Hence, demobilising combatants could hope to walk free and retain their assets despite of the illegal and violent nature with which they came into their possession.

Gradually, faction after faction laid down arms, handed themselves in and demobilised. In total, 37 blocks demobilised between December 2002 and early 2006. The

[15]Recorded tapes from unofficial discussions between paramilitary leaders and the government's chief negotiator suggest that the former at times seem to suggest the agenda.

[16]Other benefits included clothing for combatants and their families, psychological assistance, support in finding job or government funding of productive projects (Human Rights Watch, 2005).

negotiations that settled the specific conditions were conducted on a one-to-one basis between the government's representatives and each individual paramilitary block. As discussed in 2.2.1, individual insurgent blocks were heterogenous and independent, with idiosyncratic command structures and leaderships, operating in mostly non-overlapping geographic zones of influence (International Crisis Group, 2007). In conjunction with the arduous bureaucratic task of brokering peace on an individual basis, the blocks' idiosyncrasies and the fact that their actions and strategic decisions were largely based on the respective local leadership appears to have led to the staggered timing of demobilisation across blocks.

2.3 Data: description and summary statistics

This section starts out with a description of the data collection process. It discusses and explains the main variables used in our empirical analysis and their sources. The focus will be on a detailed description and presentation of our preferred dependent variable, the number of expropriated plots of land, on the one hand, and the two decisive variables for our identification strategy on the other: the location of paramilitaries, which defines their territorial control; as well as the timing of their demobilisation. Finally, we present a brief conceptual framework that gives rise to the main testable hypotheses on paramilitaries' actions during political transition.

2.3.1 Main outcome variable: land restitution claims

We use a novel and unique dataset that allows us to extract unprecedentedly reliable and detailed information on land expropriation during the recent conflict in Colombia. As the main source of information, we collected data on land expropriation in Colombia from the Special Administrative Management Unit Restitution of Dispossessed Land (URT) of the Colombian government.[17]

[17]This unit was created as a part of the Law 1448 of 2013 (also known as the Law of Victims) with a clearly defined legal procedure to restore and formalise the land of victims of dispossession. Victims are invited to register their land claims, which are physically and legally reviewed by the URT before starting an actual process of restitution.

Each land restitution claim describes the expropriation event: location and general characteristics of the plot as well as the year of the loss of land. There are different types of land claims based on the nature of expulsion:

- Victims that were forced to flee because of military confrontation between actors. For these cases, the victims' land was not the primary motivation for the underlying source of violence.
- Victims that were direct victims of expropriation.
- Victims that were forced to sell their land to an illegal actor at a very low price.
- Victims that, after being forced to flee, lost their titles due to a coercion of institutions (e.g. forgery) or identity theft.

In our analysis, we only include those claims where a land expropriation took place (types 2 to 4 from the list above). We disregard those claims where the victims just abandoned their plot. Such data could be sensitive to the fact that the displacement of civilians was a byproduct of fighting or persistent violence. In contrast, our objective is to elicit strategic actions of non-state actors that specifically pursued the goal of appropriating assets in the form of land.[18]

Our investigation is restricted to valid claims, that is claims that have been checked and found to be correct, as prospects of material compensation could drive false claims and bias our analysis. Systematic data on invalid claims is not available in any disaggregated way that we could meaningfully use for our analysis. For that reason, we cannot analyse potential under-reporting in specific municipalities. However, we believe that it would not compromise our main analysis as we restrict the sample of municipalities to include only areas that are comparable in terms of previous presence of non-state actors (see below for more details). Throughout the

[18] A similar distinction of violence strategies during civil wars is discussed in Azam and Hoeffler (2002), who differentiate between looting and terror. In our setting, terror could be seen as equivalent to displacement whereas looting, requiring targeted violence and actions, is comparable to expropriations.

paper, our sample consists of an annual panel of Colombian municipalities from 1997 to 2010.

In this paper, we discard any municipality for which no land expropriation has been recorded, according to the register of land restitution claims. Those municipalities are likely to differ substantially from areas where some or even a lot of land expropriations took place. While it might be of interest to illustrate those differences, we neither consider it important for our analysis nor to be in the scope of this paper. Furthermore, plenty of previous work has looked into geographic, socioeconomic or historic determinants of conflict propensity, both theoretically as well as empirically, see for instance Vargas (2012). As a result, our baseline sample is made up of 625 out of a total of 1122 municipalities, constituting around fifty-six percent of Colombian municipalities. Figure 2.3 graphically explains our sample in the wider context of all Colombian municipalities. We later restrict our sample further, which is explained below.

As primary dependent variable, we compute the aggregate number of land plots that were expropriated per municipality per year. Alternatively, the data we have collected would also allow us to scrutinise the total area of land, in hectares, expropriated in a municipality in a given year. We prefer the number of plots over the total size of land expropriated for various reasons. Firstly, the former comes with an enormous advantage of accuracy. The question of whether an expropriation took place is at the core of the restitution procedure and is therefore thoroughly checked. In contrast, the information on the size of a land plot is only verified and corrected at the very end of the restitution process, which means that the available data might be flawed. Secondly, some claims might pertain to public land, which in turn is constituted of extremely large plots, potentially biasing the results.

Despite restricting our attention to the 625 municipalities in our sample, not every municipality recorded expropriations in every year. In fact, the number of municipality-year observations with zero values is large. They make up roughly eighty percent of all observations. If we limit our analysis further to areas with clearly documented presence of paramilitaries, explained in detail in Section 2.3.2, this

proportion falls to around seventy percent.[19] In essence, our main dependent variable needs to be seen as a flow variable, not as a stock variable. That is, we analyse the number of expropriated land plots and not the total stock of land expropriated. [20]

2.3.1.1 Alternative dependent variables

In addition to information on expropriations, we consider a number of alternative dependent variables both in order to prove the robustness of our results as well as to investigate some of the mechanisms at play. We use data on official victimisation statistics, collected by the Colombian government, that provide detailed information on a number of committed crimes and atrocities such as forced displacement.

To complement our analysis on land expropriation, we use data on other forms of appropriation such as general theft or assaults on private properties as well as other forms of violence. All these variables are derived from a database published by Centro de Estudios sobre Desarrollo Económico (CEDE) at the Facultad de Economía at the Universidad de Los Andes in Bogotá.[21] In addition to CEDE's database, we also make use of two measures of non-state actor presence constructed by Acemoglu et al. (2013) and computed with the number of attacks led by the respective group.

Finally, we use detailed information on the concentration of land possession in Colombia, first calculated by Ibáñez and Muñoz-Mora (2010). Based on their calculations, we derive annual land inequality data, measured by a Gini index, that allow us also to examine the anticipatory effects of violence on land tenure.

[19]The novelty of our principal data source comes with one drawback. Law 1448 is a relatively recent phenomenon. At the same time the legal procedure of reviewing submitted claims is an extensive process. In combination, this means that not all submitted claims have already been processed but a considerable proportion of claims is still under review. Despite this pitfall, restitution claims offer the best and most accurate analysis of investigating expropriations during the Colombian civil conflict. Our analysis is based on the most updated and complete data that are available. Moreover, the fact that victims' compensation may also occur in pecuniary means rather than land restitution also alleviates concerns that victims systematically under-report their expropriations in a way that could compromise our analysis.

[20]Our interest lies not in the total stock of land that gets expropriated but rather in shifts that occur in anticipation of an event.

[21]CEDE's data are originally sourced from newspaper articles and police reports, systematically compiled by the Observatory of Human Rights of the Vice-presidency.

2.3.2 Explanatory variables: timing of demobilisation and presence of paramilitaries

As discussed in section 2.2.1, paramilitaries exercised considerable influence in many parts of Colombia at the peak of their power. While in some places they were only active for temporary spells, they maintained sustainable and persistent territorial presence in other areas. Not distinguishing between these two types of territorial activity has been a major caveat of previous work on civil conflict and non-state actors in Colombia. Lacking precise information, prior work has tended to approximate territorial control of insurgents by using the number of military activities in a given area (Acemoglu et al., 2013; Dube and Naidu, 2015; Dube and Vargas, 2013). Such an approximation might be suitable to gauge the consequences of violence in a conflict but it is likely to be a problematic choice to truly capture territorial dominance of non-state actors over a given area. Proposed measures such as attacks or crimes committed by a military faction can in fact be misleading. Instead of capturing the sphere of influence of factions, they could simply indicate the very opposite. Battles and violent struggle will often be located in areas over which there is a multi-interest competition. Hence, recording violent activities could confuse contested areas with dominated areas. On the contrary, asset appropriation activities, to which land expropriation belongs, are more likely to be found in areas where a non-state actor exhibits military dominance rather than in contested areas. After all, land expropriation in the Colombian civil war served as a means to consolidate economic and political power and was not solely used for short-term looting goals.

In contrast to the previous economic literature on the Colombian civil war, we use information that gives us the exact location of paramilitaries at the beginning of the peace process in Colombia in 2002. Based on official documents provided by the Colombian High Commission for the Peace Process, we digitise maps and then geo-code the exact location of each of the 37 paramilitary blocks that demobilised between 2003 and 2006. This procedure allows us to distinguish between areas where paramilitaries periodically engaged in battles or other activities and areas where they kept a strong military presence and thus exhibited greater territorial

control. Figure 2.4 gives a detailed graphic illustration of each of the 37 blocks' geographic presence. As will be explained in more detail below in Section 2.4, it is essential for our analysis to use this precise spatial information. The location of each paramilitary block is validated through various accounts. Primarily, it is based on the reports of testimonies of the leaders of the paramilitary blocks. Their statements were additionally cross-checked with government and military accounts on paramilitary presence. Finally, victims' statements were also used to validate this information.

The resulting measure of paramilitary control presented in Figure 2.4 can be compared to the intensity of paramilitary attacks. Figure 2.5 displays the map of Colombia and shows whether a municipality was controlled by a paramilitary group as well as the number of paramilitary attacks that took place in the municipality between 1997 and 2001. Areas that are at the border of paramilitary territory recorded, on average, a larger number of attacks than areas that are more centrally located within the territory controlled by paramilitaries. Furthermore, the intensity of paramilitary military activities is also higher in frontier regions just outside the rebel territory than in central paramilitary municipalities, indicating that it is foremost a suitable measure for fought-over territory. Attacks are likely the results of military excursions into areas that are contested by both paramilitary and left-wing guerrillas, i.e. areas where no clear territorial dominance has been established. As a consequence, the precise information on paramilitary presence that we have constructed is more suitable for the purpose of this paper. We will investigate the role of location within paramilitary territory further in Section 2.5.3.

We use the same source to extract the timing of demobilisation of each of the 37 blocks that participated in the peace negotiations. This information allows us to infer when the municipalities in our sample were exactly affected by the disarmament of paramilitaries. Thus, we can deduce the stage of the demobilisation process in every location at any time in our sample. Furthermore, a distinction can be made between municipalities where (i) demobilisation has already taken place, (ii) peace negotiations are in process or (iii) peace negotiations have not yet commenced.

As laid out in Section 2.2.2, paramilitary demobilisations did not take place simultaneously. Their timing was staggered, with early demobilisers disarming in 2003 and the last demobilisations occurring in early 2006. The bulk of blocks signed peace treaties and concluded their demobilisation between 2004 and 2006. Out of all municipalities with paramilitary presence, around 98% recorded a demobilisation between 2004 and 2006. Figure 2.6 depicts the spatial variation of the different demobilisation years while Figure 2.7 provides a very basic statistical overview of the shares of areas along demobilisation years.[22] In over seventy percent of municipalities, the local dominant insurgent block demobilised in 2005 or 2006, three respectively four years after Uribe had been elected in 2002 and two respectively three years after the peace negotiations had passed a significant hurdle, the official accord on paramilitary demobilisation.

2.3.2.1 Other variables

Using satellite data and geographic information, we generate a number of variables on the geography and climate of each municipality. We geocode altitude and latitude of each municipality. Additionally, we compute its remoteness or accessibility by considering geographic distances as averages within the municipality: (i) the distance to the coast line, and (ii) the distance to the district capital.[23] Temperature and precipitation data are approximated by using twenty-year annual averages.[24]

We use several other municipality level variables that are taken from CEDE on demographics and socioeconomics.[25] Last, we compute data on mining activities and average soil quality in municipalities.[26]

[22]For illustration Figure 2.8 depicts the resulting number of combatants that officially demobilised over those years. It describes a similar picture in that the largest chunk of demobilisation occurred in 2005 and 2006. Figure 2.9 shows the number of demobilised combatants by paramilitary block, illustrating the highly fragmented nature of paramilitaries.

[23]In order to capture the information for urban centres we use the SDCW data set. It provides the exact location of urban centres (either as points or polygons). Based on this information, we built the mentioned variables as Harvesine distance from the centroid of our cell to the cell of the object of interest.

[24]We calculate these averages based on data provided by Harris et al. (2014).

[25]Presence of coca cultivation; historical land inequality data measured by a Gini index; population size; rurality of populated areas in the form of a rurality index; a municipality's area's share used for agriculture.

[26]For mining, we use the Seamless Digital Chart of the World (SDCW; `www.worldgeodatasets.com/basemaps/`), which provides information on areas with natural resource extraction. It includes

2.3.3 Descriptive statistics

Table 2.1 describes the key dependent variables of the municipalities during our sample period from 1997 to 2010 at the municipality-year level, namely the number of expropriated plots of land as well as other forms of violence and victimisation. Furthermore, it includes the most important control variables. The table is divided into top part for time-invariant variables and a lower part for time-variant ones.[27]

On average, 49.528 hectares and 0.61 plots of land were expropriated throughout this period. Interestingly, there is enormous underlying variation in these figures. For instance, as stated above, plenty of year-municipality observations are without any reported land expropriation, which can be seen by the fact that the median is 0 for both measures. Also, the standard deviations are fairly large in relation to the averages. On average municipalities in our sample recorded 313 displaced citizens per year, a testimony to the large toll the conflict took on the Colombian people. In around two percent of municipalities in our sample coca is cultivated and the share of municipalities with mining activities is close to around 10 percent. In comparison, in municipalities where paramilitaries were located (presented in Table 2.2) these figures are larger. The average number of expropriated land plots, the associated total area, as well as the number of displacements, are all greater.

2.3.4 Conceptual framework

Rather than developing a complete theoretical model, we build our empirical analysis on a few conceptual arguments and considerations derived from the history of the conflict, the nature of the peace agreement and the principal motivation of paramilitaries. As discussed in Section 2.2.1, paramilitaries started out being politically

mines/quarries, oil/gas fields, and salt evaporators. For soil quality, we use information used by Nunn and Puga (2012), which determines whether each cell on a 5-minute grid of the earth is subject to constraints for growing rain-fed crops. The information is originally created by Fischer et al. (2012), based on the FAO/UNESCO Digital Soil Map of the World, the soil association composition table and climatic data compiled by the Climate Research Unit of the University of East Anglia. We calculate the percentage of the land surface area of each municipality that has fertile soil.

[27]Some of the measures above, for instance historical land inequality or coca cultivation, are only observed once due to data restrictions such as limited frequency of Censuses but in essence are not completely constant over time.

motivated by an anti-communist ideology but quickly underwent a change of their primary objectives, turning into militant organisations that primarily sought material gains and rent-extraction opportunities via violence or political intimidation.

Such behaviour is in line with economic theory that predicts economic rents as main motivation of non-state actors. In this setting, the election of Uribe and his firm commitment to contain non-state actor violence by - among other means - demobilising paramilitaries, constituted a news shock. It changed the paramilitaries' incentives and motivation drastically since their eventual demobilisation and loss of military power became extremely likely. As informal talks on the terms of a peace agreement began early and Uribe's election campaign articulated an explicit agenda for his future policies, they had clear knowledge and information several years in advance of their demobilisation. As a consequence, if we consider paramilitaries to act rationally, we would expect a systematic change or reaction in their behaviour in response to this new situation in order to maximise their future profits. The question is then what type of effects, i.e. what type of strategic actions, should we expect to observe as paramilitaries aim to consolidate their power in a post-conflict world?

In Colombia, most of the fighting and non-state actor violence has been concentrated in rural areas with considerable reliance on agriculture. In those areas, land is not only used for economic production but also yields enormous political influence. Local elites consisted historically of large landowners, a legacy of Spanish colonial times (see Appendix 2.7). The power structures in Colombia persisted to a large degree for centuries (Mazzuca and Robinson, 2009). Economic power was equivalent to political power and the largest of landowners belonged to the economically most powerful groups in the country. Additionally, land possession has been seen as politically pivotal because landowners provide employment and therefore the economic subsistence basis to tenant farmers or otherwise landless groups, thus wielding a lot of influence over these individuals.[28]

[28]Baland and Robinson (2008) and Baland and Robinson (2012) advance this idea analytically in the context of a simple model of patron-client relationships, representative of the relationship between landlord and worker, and empirically validate their model's prediction with data from Chile. In their models, landlords exhibit strong political influence over their workers/clients, par-

For this reason, land is of pivotal importance in any interest group's endeavour to exhibit (legal) local control or even dominance over such areas. One way to obtain land is through expropriation, via different sources of coercion, violence or persuasion.[29] We argue that by expropriating civilians, paramilitaries can ensure their future political influence and control of economic gains stemming from their victims' plots of land. As a consequence, we expect to see a rise in paramilitary expropriations of land in anticipation of demobilisation. Furthermore, our argument can be extended to predict what type of land should be affected more severely. If economic rents are the main motivation of rebels' action, as proposed in various studies in the theoretical literature (see for instance Grossman (1995) or Besley and Persson (2011)), then paramilitaries should focus their actions on the most lucrative land, i.e. systematically appropriate "high quality" land.

The theoretical considerations stated above give rise to a number of research questions and testable predictions. We take these predictions to the data in order to empirically probe them. In the next section, we present an econometric approach to identify the causal effects of demobilisation on more frequent land expropriations in the years preceding the demobilisation. We argue that such increased expropriation activities are not random but take place systematically in the years preceding the demobilisation of paramilitary groups, at a time when they already anticipate their disarmament.

Our empirical analysis will be based on a set of principal research questions:

Main Questions:

1. Do paramilitaries intensify their expropriation efforts in the years preceding their demobilisation?

2. When exactly do such anticipatory actions take place?

3. What type of land is primarily expropriated?

ticularly, but not exclusively, when voting can be observed (through open ballot). However, even within closed ballot systems, patrons maintain some political clout over their workers.

[29] In this paper, we are unable to exactly determine the means by which expropriations are enforced. However, we examine whether there were any systematic changes in types of violence in affected municipalities.

We propose three hypotheses that we empirically test:

Hypothesis 1: *In anticipation of their demobilisation, paramilitaries will increasingly expropriate land to consolidate their economic and political influence.*

Hypothesis 2: *Areas without paramilitary presence should experience fewer cases of land expropriations in the years preceding a demobilisation process.*

Hypothesis 3: *Economically profitable land should be affected disproportionally more by expropriation.*

2.4 Empirical strategy

We employ a difference-in-differences strategy in order to empirically assess whether expropriation was strategically used by paramilitaries in anticipation of their respective demobilisation. The specific pattern of demobilisation across paramilitary blocks plays a pivotal role for identifying such anticipation effects. As a consequence, we will first elaborate the nature and significance of demobilisation of paramilitaries for our empirical investigation. Then we introduce a difference-in-differences strategy to estimate causal effects of anticipation of demobilisation on land expropriation.

2.4.1 Demobilisation: a natural experiment?

The demobilisation of paramilitaries in Colombia was by no means homogeneous, nor did all paramilitaries form a uniform entity. Instead, Colombian paramilitaries consisted of several distinct groups.[30] Each of these groups itself consisted of various subgroups, so called paramilitary blocks. As Map 2.4 illustrates, these groups were spatially spread out through vast parts of Colombia.

Across paramilitary groups and even within each paramilitary group, demobilisation did not occur for all blocks at the same time. The first few blocks started demobilising in 2003. By 2006, all major paramilitary blocks had laid down their

[30]The ACCU (Autodefensas Campesinas de Córdoba y Urabá), ACMM (Autodefensas Campesinas de Magdalena Medio), BCB (Bloque Central Bolivar), and other independent groups.

arms. The largest demobilisation waves took place in 2005 and 2006. Nonetheless, as can be seen in Map 2.10, there was a lot of variation with regards to the specific demobilisation year both spatially as well within paramilitary groups.[31] We exploit this temporal variation in the timing of demobilisation for our empirical analysis in order to gauge whether paramilitaries increased their expropriation efforts of landowners in the years preceding their eventual disarmament. By having precise information of the presence and location of each paramilitary block, we know when each municipality experienced a paramilitary demobilisation. As a consequence, if we assume comparable behaviour across paramilitary actors, the incidence of anticipation effects will differ across municipalities. This temporal variation enables us to control for year-specific fixed effects. In addition to areas that experienced demobilisation, we can determine locations and hence municipalities that were not directly affected by any paramilitary demobilisation. Analogously to the former, we can establish for the latter the timing of demobilisation of the geographically closest paramilitary block.[32]

Since the temporal variation of demobilisation constitutes the core element of our empirical analysis, it is prudent to examine if there are any specific factors that determine this variation. In particular, we probe whether the timing of demobilisation processes is truly exogenous or not. As a first step, we take a closer look at the determinants of such a timing. We employ an ordered probit regression to elicit any observable factors that might explain the year of demobilisation. There are several potential explanatory factors. For instance, the intensity of previous violence - measured by the number of attacks between 1997 to 2001 - or the political preferences in a given municipality might explain why the decision to demobilise was delayed or taken earlier. Similarly, we consider a number of socioeconomic and demographic variables that could explain differences in the timing of demobilisation.[33] Acemoglu et al. (2013) argue and provide suggestive evidence that paramilitary

[31] For more details, see Section 2.3

[32] In Section 2.6, we make use of this information to probe any spatial spillover effects.

[33] We consider the inequality of land possession, measured by a Gini index in 1985, the degree to which a municipality can be classified as rural, and the municipality's average population size between 1997 and 2001.

groups in Colombia were able to influence legislative elections. For this reason, we also consider the vote share president Uribe won in the election of 2002 as a proxy for the political and thus economic influence of non-state actors in a district. Furthermore, we test the importance of various geographic and accessibility measures such as altitude, distance to the coast or distance to the state capital. Finally, we include measures for the climate conditions in each municipality in the form of average precipitation data.

None of the aforementioned variables appear to be significantly correlated with the timing of demobilisation (Table 2.3). The only notable exceptions are the size of municipalities and average rainfall. Given the length of our sample period, 1997 to 2010, such measures can confidently be assumed to be fixed over time. In all following empirical specifications, we will include municipality fixed effects that will effectively control for such a difference. Additionally, municipality fixed effects allow us to ensure that no other time-invariant municipality-specific factors either fundamentally differ between municipalities with different demobilisation years, thus undermining the validity of our estimates, or even drive the effects of our estimation.

We have conducted extensive research to uncover any determinants of the timing of the demobilisation process. There appears to be no evidence of any systematic pattern that caused the staggered timing in the way different paramilitary blocks disarmed, instead it appears to have largely been based on factors exogenous to our analysis. For instance, it is (in part) attributed to coordination problems of the government and armed blocks (Restrepo, 2013). As explained in detail in Section 2.2, the peace negotiations were conducted on an individual level between the national government and each paramilitary block separately. As the 37 demobilised blocks were idiosyncratic and their strategic decisions were greatly influenced by their respective leaders, this momentous task for the national government gave rise to various coordination problems. It is noteworthy that even though the Colombian civil conflict and the peace processes with the non-state actors have received wide-spread attention by economics, political science scholars and human rights organisations,

there is neither qualitative support for any specific reasons for the differential demobilisation timing of paramilitaries nor any speculative inference about non-random explanations. For all these reasons, we conclude that the timing of demobilisation of paramilitary blocks can be viewed in our analysis as exogenous.

A relatively blank spot in the literature on the Colombian paramilitaries is the analysis of differences in characteristics of the separate paramilitary blocks. Mainly, this is due to the scarcity of comprehensive data. As far as possible, we have collected systematic information on those blocks and their respective leaders. The leaders are decisive for their block's actions and thus of great interest when assessing the demobilisation process.[34] Therefore, we use those data to further validate our assumption that the exact demobilisation timing is exogenous, here with respect to paramilitary leader characteristics. Originally, paramilitaries started as a reaction to guerrilla violence (see Section 2.2). Some early paramilitary leaders had been personally affected by such violence, e.g. in the form of the loss of relatives. Paramilitary leaders with such grievances against the guerrillas might be more reluctant to give up arms and demobilise while the guerrillas have not been defeated or contained, delaying their block's demobilisation. Similarly, leaders that faced greater risk of a harsh legal punishment following their demobilisation could be unwilling to disarm and thus defer it until more favourable terms are found in negotiation with the government. We use information on leaders' personal grievances against guerrillas as well as subsequent requests for their extradition to the US to assess the impact of these factors on the demobilisation timing.[35] Both factors appear not to be correlated with the timing of demobilisation of the different paramilitary blocks, a fact that further increases confidence in the timing's exogeneity (Table 2.4).

2.4.2 Difference-in-differences estimation

As shown in Maps 2.4 and 2.6, the demobilisation of paramilitaries in Colombia varied both temporally as well as spatially. As a consequence, we can exploit this

[34] We have also conducted interviews with ex-combatants who confirmed the extraordinary importance of a block's leader in the decision making and demobilisation process.

[35] We use extradition requests not actual extraditions as the latter might be endogenous if paramilitary leaders can influence them through prolonging their armed resistance.

two-dimensional variation to use a difference-in-differences approach focussing exclusively on municipalities where paramilitaries maintained a territorial presence throughout the conflict and were present at the time of their demobilisation in order to enhance comparability (335 municipalities). We compare expropriations during the years preceding the demobilisation of the locally dominant paramilitary block between municipalities where different paramilitary blocks exhibited control.[36] As a robustness check, we extend this exercise by also including municipalities where no paramilitaries maintained a sustained presence, comparing them to those municipalities with paramilitary presence, which is discussed in detail in Section 2.6.1.

Our preferred specification includes dummies for the three years before demobilisation since we consider those years to be the decisive years for any systematic behaviour in anticipation of the eventual demobilisation and peace agreement. For the overwhelming majority of districts in our sample, totalling to around 98% of observations, the locally dominant paramilitary group demobilised in 2004 or later (2005 or 2006). In 2001, Álvaro Uribe decided to run for presidential office. He was elected president in 2002. While Uribe ran on a decisively anti-Guerrilla appeasement platform, he made clear his intentions to contain and reach an agreement with the paramilitary head organisation AUC (Autodefensas Unidas de Colombia) in order to disarm them.[37] Therefore, three years before their respective disarmament (almost all) paramilitary blocks had clear information on the government's agenda and soon informal and eventually formal peace negotiations ensued.[38] As argued in Section 2.3.4, our empirical analysis builds on the insight that paramilitaries are profit-maximising organisations that are primarily interested in economic benefits and at the same time have clear foresight of their future limited military capacity to pursue such benefits. Equation 2.1, specified below, probes our hypothesis that paramilitaries take anticipatory actions before they lay down their arms.

[36] Our difference-in-differences estimation excludes areas without any paramilitary activity at all. We drop municipalities where paramilitaries have not been reported to have committed any expropriation throughout our sample period.

[37] Section 2.2 elaborates the paramilitary history and the events that unfolded at the time of Uribe's election and during the demobilisation negotiations.

[38] Thus, by 2001 and especially 2002 or 2003 they can reasonably be assumed to have anticipated and expected the eventual need to demobilise. More importantly, they were aware of the fact that their military actions and other forms of violence would soon be severely limited.

The main regression equation, estimated by OLS, is as follows:

$$Exp_{it} = \alpha + ANT'_{it}\beta + \mu_i + \delta_t + X'_{it}\gamma + \epsilon_{it} \tag{2.1}$$

where the dependent variable Exp_{it} refers to the number of land plots that were expropriated in municipality (i) in year (t). The vector ANT_{it} consists of a set of binary variables that indicate

the years before demobilisation, i.e. $ANT_{it} = \begin{pmatrix} Demobilisation_{it} \\ Demobilisation_{it-1} \\ Demobilisation_{it-2} \\ Demobilisation_{it-3} \end{pmatrix}$.

These indicators, $Demobilisation_{it-s}$ for $s \in [0, 3]$, are only equal to one s years before the locally dominant paramilitary block in municipality i demobilised, and zero otherwise.[39]

Our main coefficients of interests are presented by the vector $\boldsymbol{\beta} = (\beta_0, \beta_1, \beta_2, \beta_3)$, the elements of which measure the difference in the anticipation effect on expropriations between municipalities with different demobilisation years of the local paramilitaries. Crucially, our analysis constitutes an event-time study not a calendar-time study in the sense that our estimates go beyond simply picking up variation from specific years by exploiting the fact that demobilisations occurred in different municipalities and years. On the contrary, the variation in demobilisation across paramilitary blocks and municipalities allows us to include year fixed effects, δ_t, in all our specifications. These control for any dynamic in the number of expropriations over time that is common to all municipalities. The panel structure of our data additionally enables us to include municipality fixed effects, μ_i, in order to account for any mean differences in expropriations across municipalities. Moreover, municipality fixed effects ensure that our results are not driven by any time-invariant characteristics of municipalities, such as geography. Finally, we include a rich set of additional controls, represented by X_{it}. The controls range from geographic variables and climate data to demographic and socioeconomic information. We group

[39]For instance, if paramilitaries demobilised in 2005 in an arbitrary municipality (j), then $Demobilisation_{jt-1}$ would be equal to one in 2004 only and zero otherwise.

them accordingly in the regression tables. As annual data at the municipality level in Colombia is sparse due to the infrequency of censuses, we only have annual data for a couple of variables (rurality of a district, population). All other time-invariant variables are interacted with a full set of time dummies. These interactions control for flexible time pattern or any time trend related to these municipality characteristics that could otherwise be driving our results. In all our baseline regressions we include flexible time patterns for the following controls: precipitation, altitude, distance to coast, distance to district capital, latitude, soil quality, land Gini index, and the share of a district used for mining activities.

The error term is represented by ε_{it}. As default, we cluster ε_{it} at the municipality level since previous work by Bertrand et al. (2004) and Donald and Lang (2007) demonstrates that serial correlation within geographic units of analysis in difference-in-differences specifications can lead to significantly downward-biased standard errors and thus yield flawed inference. The way we cluster the standard errors allows them to be arbitrarily correlated over time within municipalities, while maintaining a robust estimation of the model's coefficients. Our results are also robust to more conservative clustering such as clustering at the departmental level.

The premise of any difference-in-differences evaluation is the vital assumption of common trends. It posits that the treatment group would have followed the control group's path if there had not been a treatment. This implies that the average change of the outcome variable in the control group represents the counterfactual change of the outcome variable in the treatment group. We test the parallel trend assumption by checking differences of the pre-trends in the number of expropriated land plots between comparison and treatment groups, here constituted by the set of municipalities with different demobilisation years, before paramilitaries start to anticipate their demobilisation. In addition, we also compare the post-demobilisation trends. The visualisation of the parallel trend test is presented in Figure 2.11. The test supports the assumption of parallel trends and boosts the confidence in the validity of our model estimation. The presented evidence of the common trend assumption

provides support of and increases the confidence in the validity of the estimates resulting from Equation 2.1.

2.5 Discussion of results

In the section below, we present and discuss results from regression analysis to assess any systematic anticipation effects of demobilisation on paramilitary activities in Colombia. The results are structured into three parts. First, baseline results are explored. Next, we pursue a number of exercises to scrutinise the link between expropriations and land characteristics. Finally, we examine a number of mechanisms that could explain how and where such crimes took place.

2.5.1 Strategic expropriations

The results of the difference-in-differences regressions estimated by Equation 2.1 are presented in Table 2.5.[40] The first four rows report the difference-in-differences estimates for the number of expropriated plots of land in the three years preceding the demobilisation and the year of the demobilisation.

For reasons of space and practicality, we do not report the coefficients of individual control variables. In addition to year fixed effects and municipality fixed effects, our control variables include the following: the average proportion of fertile land (soil), the distance to the district capital, shortest distance to the coast, the municipality's altitude and latitude (Geography), average annual precipitation over twenty years (climate), historical land inequality (measured by a Gini coefficient) and the share of area with mining activities (socioeconomic). All of these controls are interacted with a full set of flexible time dummies. These interactions provide a powerful mechanism to probe whether the baseline results could be driven by any of the mentioned control variables, since it accounts for any potential temporal dynamic through which these factors might matter. Moreover, we control for the rurality of each municipality by including the rurality index (socioeconomic), which is time-variant.

[40] Below the main coefficients, a set of rows specify which groups of control variables are included in each regression, marked by separate columns.

The presented coefficients show that more land expropriations take place in the years before paramilitaries demobilise. The coefficient on the dummy that indicates the respective year three years before the local paramilitary block's demobilisation is statistically significant, with p-values below 0.05. The coefficient's magnitude, 0.284, is equivalent to an increase of expropriations three years before paramilitaries demobilise of more than 33.5 percent relative to the mean number of expropriations throughout the sample period. Furthermore, all coefficients and in particular the coefficient on $Demobilisation_{t-3}$ are extremely stable regardless of the inclusion of the large number of soil quality related, geographic, climatic or socio-economic control variables.

Contrary to the three-year anticipation dummy variable, the coefficients for expropriations two years or one year before demobilisation are insignificant as is the coefficient on expropriations during the year of demobilisation. Taken together, these findings suggest that there indeed was strategic behaviour by paramilitaries: in light of their upcoming demobilisation and resulting loss of military power they systematically appropriated land before they eventually disarmed. These results are consistent with our prediction specified in Hypothesis 1 that paramilitaries will act decisively in response to an upcoming disarmament in order to consolidate their economic and political influence. The bulk of expropriations were made three years in anticipation. In interpreting these results, one has to keep in mind that our dependent variable is a flow variable not a stock variable. In other words, we measure the number of expropriations per year and not the stock of total expropriations made. Given that we document a large increase in expropriation three years before demobilisation, the lack of significance of the other coefficients for the subsequent anticipation years might be a consequence of the already large number of expropriated plots.[41]

[41] The F-test on the joint significance of all these coefficients, reported in the lower half of the table, shows that the coefficients are jointly not significantly larger than zero despite the significance of the dummy on three years before demobilisation. This is explained by the fact that expropriations are not increased shortly, two or fewer years before demobilisation, cancelling out the effect in the preceding year.

In all five specifications we include year fixed effects courtesy of the staggered timing of demobilisations across paramilitary blocks that is translated to temporal variation of affected municipalities, depicted in Figure 2.7. Since we find evidence of significant anticipation effects despite the inclusion of year fixed effects, we can conclude that expropriations are indeed driven by the event of demobilisations and not by any arbitrary competing event.[42] As pointed out in Section 2.3, our outcome variable is a count variable with a large number of zeroes, i.e. there are many municipality-year observations with no recorded land expropriation. As a consequence, we probe our linear regression results (Table 2.5) with negative binomial regressions. The results confirm our finding that expropriations are significantly increased three years preceding the demobilisation of the paramilitaries (Table 2.6).

2.5.2 Expropriations and land characteristics

A natural question that has to be addressed is what type of land paramilitaries expropriate in anticipation of their disarmament. Can we say anything about the characteristics of expropriated land? If our Hypothesis 3 is correct, non-state actors driven by economic rent-extraction opportunities should primarily be interested in economically attractive land. We will refer to such land as "high quality land". To gauge the economic attractiveness of land, we have collected data on a number of land characteristics of Colombian municipalities. Agriculture remains an important sector and hence possessing agriculturally productive land is economically attractive. We use information on the average soil quality in each municipality to infer the agricultural productivity of land. Additionally, we have information on the share of each municipality's land used for agricultural purposes.[43] The results are presented in Tables 2.7 and 2.8.

[42] Our preferred specification directly rules out calendar year effects driving our results. All empirical results presented in this paper include year fixed effects. Though not reported, we have additionally assessed our results without controlling for year fixed effects. The results are qualitatively similar.

[43] All land characteristics are observed at the municipality level not at the land plot level. Unsurprisingly, there is no precise information available for the latter due to the multitude of land plots. We use the municipality level information to approximate the average land characteristics of the area's land plots.

In assessing which land characteristics matter in terms of expropriation by paramilitaries, we interact the anticipation dummies for the pre-demobilisation years with the municipality level variables of land. If a particular type of land matters more to paramilitaries, we should expect it to be expropriated relatively more, which would be indicated by significant coefficients on the interaction.[44] We established earlier that the anticipation effect is most salient three years before demobilisation. Therefore, we concentrate our analysis on that variable and its interaction with land characteristics.[45]

The regression results show that the interaction of the anticipation variable and soil quality is positive and statistically significant. Without interpreting the exact size of the effect, it becomes clear that land plot expropriations in anticipation of paramilitary demobilisation occurred relatively more in municipalities with on average better soil quality. Put differently, the better the average soil quality, the more expropriations by paramilitaries took place, which seems to provide evidence in support of our Hypothesis 3. This finding does not extend to agriculture more generally. The share of a district that is used for agriculture is not significantly correlated with anticipatory expropriations (Table 2.8). Thus, our finding only pertains to high quality agricultural land not agricultural land in general. But is soil quality the only economic aspect of land that drives expropriations? Other rent-extracting opportunities inherent to a municipality's land characteristics could equally explain which municipalities are affected relatively more by expropriations. Due to Colombia's recent history regarding international narco traffic, illegal crop cultivation is a natural alternative candidate. Likewise, natural resources can be decisive in determining the economic benefits that accrue to possessing land.[46] For those reasons, we examine the heterogeneity of anticipatory expropriations in terms of coca production

[44] All variables on land characteristics are time-invariant. Thus, they are uninformative when including municipality fixed effects and therefore not reported in the tables. For all variables we consider, it is very unlikely that any will have changed considerably over the relatively short time period we consider.

[45] We keep the regression specification as before, i.e. including dummies for three pre-demobilisation years and the demobilisation year itself. For brevity, we only discuss the dummy for $Demobilisation_{t-3}$ and its interaction with land characteristics.

[46] The literature on civil war is fraught with evidence of the close link between natural resources and conflict.

and mining opportunities (see Tables 2.9 and 2.10). We do not find any significant effects for either coca or mining. While expropriations in areas with coca cultivation appear to be mildly more numerous than in areas without coca plantations, the coefficient is insignificant.

Are the effects of anticipating demobilisation only limited to land or are there similar patterns for other types of assets? After all, other assets could also be used by paramilitaries for economic gain. Contrary to land expropriations, neither thefts nor assaults on private property (other than land) increase in the years preceding paramilitary demobilisation (Tables 2.11 and 2.12). The explanation for this finding could lie in the vital importance of land in the Colombian context: it not only offers material gains from agriculture and other forms of economic activity but it also confers powerful political clout, which in turn can be used for rent extraction through various means.

2.5.3 Mechanisms: plot size, military competition, and centrality

Due to data limitations we cannot empirically test the exact means and mechanisms that were employed by paramilitaries. However, we have collected data on self-reported victimisation, categorised in various forms of violence such as sexual abuse, threats, or homicides. None of these types of violence increased significantly in the pre-demobilisation years, which might indicate more subtle strategies exploited by paramilitaries in appropriating land (Table 2.22 in the Appendix).

As discussed in Section 2.3, the data on the size of expropriated land is noisy and less reliable than the number of expropriations. Keeping this caveat in mind, we can exploit the underlying information to examine two things. First, we can test whether the overall area of expropriated land increased in addition to the number of expropriations. Second, we can use those results to shed light on whether it is predominantly small or large land plots that are appropriated. Table 2.13 presents the results. Overall, there is no statistically significant increase in the total area of land expropriated in anticipation of the pending demobilisation. This finding could be explained by the imprecise nature of the data. Alternatively, it might

reveal that the rise in the number of expropriations occurred at the cost of small and more vulnerable landowners, while no significant increase in expropriations from larger landowners took place. From the paramilitaries' perspective, such a strategy could be more efficient by avoiding any conflicts with larger landowners and instead focussing on easier prey, small and defenceless landowners.

Although the paramilitaries eventually signed a peace agreement with the Colombian government after committing countless atrocities against civilians, one of the original founding motives was fighting its ideological adversary from the far-left: the guerrillas of the FARC and ELN. As with paramilitaries, these non-state actors' primary motivations also evolved from political to economic. Consequently, the two types of non-state actors can also be seen as competitors fighting for economic and political influence as well as rents. In fact, we find evidence of such a competitive relationship. Municipalities with relatively more active guerrillas experienced less expropriations of land three years prior to paramilitary demobilisation (Table 2.14), pointing to a reduced capacity of paramilitaries to successfully appropriate land when they simultaneously fight guerrillas for influence and military control.[47]

A related factor that appears to be relevant is the location, more precisely the centrality, of municipalities within paramilitary controlled territory. As pointed out in Figure 2.5, paramilitary attacks seem to have occurred mostly in peripheral areas that were outside or at the border of the territory controlled by the local paramilitary block. A simple regression exercise provides tentative support to this observation. Being located at the border of paramilitary territory ("frontier areas") was positively associated with a larger number of paramilitary attacks between 1997 and 2006 (Table 2.15). These areas tended to be more militarily contested than centrally located areas within the paramilitary territory. Therefore, we estimate for each municipality the distance of its most central point to the closest boundary that marks the end of the territory of the paramilitary block that is in control in that respective municipality.[48] Using this measure of centrality to determine

[47]For this exercise, we rely on the full sample of municipalities in order not only encompass paramilitary strongholds but also areas with frequent guerrilla activities.

[48]The most central point is the centroid of the polygon that represents municipalities' area on a map.

central and frontier municipalities, we can examine whether anticipated strategic behaviour varied by the distance to the territorial frontier. In municipalities that were relatively central more expropriations took place than in municipalities that were closer to the boundary of the paramilitary territory (Table 2.16). The result seems to suggest that paramilitaries anticipated their demobilisation by strategically expropriating land in areas that had an above-median distance to the boundary of the area of control (Columns 1 and 3 of Table 2.16). In contrast, they did not increase the number of expropriations in areas with below-median distance to the boundary (Columns 2 and 4 of Table 2.16). Central areas were more shielded from external threats such as attacks by other insurgents and thus more firmly under control of the respective paramilitary block, which likely explains why expropriations of land were concentrated in central areas where the insurgents were unlikely to face any significant resistance. This finding succinctly points out that our data on paramilitary location is more informative than standard measures of rebel presence (e.g military attacks) that tend to focus on areas at the boundary or outside of rebel territory. As a consequence, analyses with those measures would miss such heterogeneous behaviour within paramilitary territory.

2.5.3.1 Land inequality

Colombia is notorious for its large economic inequality, especially in terms of land-ownership. Land inequality and resulting grievances have been at the core of the Colombian Civil War. Therefore, we analyse whether the rise in expropriations that we document also aggravated inequality. We use data on land inequality in the form of a Gini index to pursue this question. Our results are reported in Table 2.17. If expropriations aggravated inequality, we should observe a positive effect of our main explanatory variable on the Gini index. This change should arise in the year following the rise in expropriations, marked by $Demobilisation_{t-2}$. Our findings, however, point in the opposite direction. In the year following the rise in paramilitary expropriations, land inequality significantly falls. This result suggests that, either through expropriations or subsequent sale or redistribution, the scale of land inequality is reduced in districts with demobilising paramilitary blocks.

However, the precise factors that lead to the fall in the Gini index are unobserved. One potential explanation could be that mostly small landowners are targeted. If expropriated land is subsequently consolidated in the hands of larger landowners, to which many paramilitaries belong, then land inequality would indeed fall. We are, however, unable to test probe this potential explanation. Of more relevance is the fact that the change in the ownership structure lends strong support to our findings that a significant number of expropriations took place in anticipation of paramilitaries' demobilisation.

2.6 Robustness checks

The results presented lend support for our main hypotheses, that paramilitaries exploited the prolonged transition period to consolidate their power by expropriating land, especially economically attractive land. Below, we will respond to a number of alternative narratives and arguments that could invalidate or undermine our interpretation of the evidence presented.

The entirety of the empirical analysis of this paper uses the total number of expropriated land plots in a given municipality in a given year. That means we do not observe or use information on the precise party who appropriated the land.[49] In theory, the documented expropriations need not have been committed by paramilitaries but could instead have been caused by the competing large non-state actor in Colombia, the guerrillas (ELN and FARC).[50] In our analysis, we both focus on the years that are decisive for paramilitaries' preparation for the time following their disarmament and limit the sample to areas with paramilitary military presence. Therefore, it is unlikely that our findings are actually driven by guerrillas because they were, if they were present in the same areas at all, in an inferior military position than the paramilitaries.

[49] We abstain from using information on the alleged actor because it is very noisy and has many missing observations. Even in the cases where the data is available there are well-founded reasons to doubt its reliability as non-state actors often hired external individuals to do such jobs.

[50] The FARC, by far the most important guerrilla group with previously more than 20,000 fighters under command, signed a peace deal with the national government in August 2016. For the period of our investigation the FARC was fully active and involved in various acts of violence and crimes.

The precise location of guerrilla camps, especially for the time period of our study, is not available. Alternatively, we can approximate contested areas. We have information on the incidence and frequency of guerrilla violence against civilians. Acemoglu et al. (2013) use data on the number of guerrilla attacks from 1997 to 2001 and also classify municipalities according to the intensity of attacks into areas with a high or low number of guerrilla attacks. We take those data and control for these measures to account for guerrillas and their military influence. In addition to this, we also control for data for other violent and illegal activities by the FARC and ELN derived from the CEDE database. Results are presented in Table 2.18. If guerrillas were behind a large proportion of the variation in our outcome variable, expropriated land plots, then we would expect areas with a larger concentration of guerrilla violence to be affected the most. By controlling for the aforementioned factors we account for this. The results are effectively unchanged when compared to the baseline specification of Table 2.5. The coefficient on $Demobilisation_{t-3}$ remains statistically significant whereas none of the measures of guerrilla presence matter in explaining expropriations of land. Referring back to the heterogeneity results based on guerrilla activity (Table 2.14), we notice that the interactions between $Demobilisation_{t-3}$ and guerrilla attacks are significantly negative, which further supports the claim that our main findings are driven by paramilitary anticipatory behaviour. In Table 2.14, we use the full set of municipalities in order to include areas with a significant number of guerrilla attacks.

A major contribution of our paper is that it offers novel insights on strategic expropriations. Work on land-related victimisation has so far focussed mostly on displacement as no data on explicit expropriation has been available. However, displacements need not be caused by targeted actions of non-state actors but can also be a side-effect of violence and inter-group fightings. Nonetheless, some of those individuals are displaced because of their land and insurgents' desire to appropriate it. To test if our story truly is about expropriation and not merely an artefact of increased displacements that then trickle down to a rise in cases of expropriations, we run our preferred specification on government provided figures of displacement

as a dependent variable. In contrast to the number of expropriated land plots, we do not observe a rise in the number of displaced in the pre-demobilisation years, which we take as evidence of strategic actions causing expropriations rather than other factors (Table 2.19).

We also show that strategic land expropriations by paramilitaries are not solely explicable by their political and electoral influence. There is widespread evidence that, starting with the 2002 election, paramilitaries actively sought to have an impact on elections of local politicians. Acemoglu et al. (2013) give evidence that they use both violence and intimidation as well as other forms of manipulation to boost the chances of candidates that were sympathetic to their cause. Fergusson et al. (2017) convincingly show that violence is used by paramilitaries before Colombian elections to affect the final outcome. There are two threats to our identification that arise from these observations: local electoral and thus political influence might be both correlated with the differences in demobilisation years and the local insurgents' capacity to appropriate land. We address this concern as follows. First, we show in Table 2.3 that the vote share for president Uribe in the 2002 election is not significantly correlated with the timing of demobilisation.[51] Second, we show in Table 2.20 that our results hold even if we reduce our sample by dropping municipalities from the analysis where parties that were closely aligned with paramilitary interests collected a large share of votes in the 2006 congressional elections.[52]

As another robustness check, we test whether the results persist if we collapse the sample to the pre-demobilisation years. Dropping post-demobilisation years does not change our findings. We also prove empirically that further lags, i.e. coefficients on $Demobilisation_{t-4}$ etc. are insignificant and their inclusion does not qualitatively alter the results.[53] Finally, we demonstrate that our results hold and the anticipation effects on strategic expropriations become even more salient if we include group

[51] As a candidate, Uribe took a much more lenient stand towards paramilitaries than his political competitors from the left, whose candidates were the primary target of paramilitary aggression.

[52] The chosen threshold is a twenty-five percent vote share. These third parties emerged in Colombia in the 2000s. They cannot be categorised into any of the pre-existing political movements. Instead, they pursued an agenda closely aligned with paramilitary interests. See Fergusson et al. (2017) for further details.

[53] For brevity, these results are not reported.

fixed effects for the four major groups of paramilitary units (Table 2.21), alleviating concerns that the timing of demobilisation or the main findings are driven by any particular group.[54]

2.6.1 Extended sample of municipalities

The results presented so far are estimated for the sample of Colombian municipalities that were controlled by the paramilitaries. However, those were not the only areas where land expropriations took place, nor were the military actions of paramilitaries confined to them. The main argument for limiting the sample of our analysis exclusively to areas with paramilitary presence at the beginning of the demobilisation process are concerns about the potential endogeneity in the presence of paramilitaries. By construction, such a limited sample of municipalities should be more internally comparable.

As a robustness check, we extend our sample beyond the territory controlled by paramilitaries and additionally include all Colombian municipalities where at least one case of land expropriation was documented throughout the period of analysis. We cannot rule out that paramilitaries were active in those areas at some point. To control for any spillover of strategic expropriations to those municipalities, we use the information on the geographic control and demobilisation year of the paramilitary block that is located closest to each municipality. Hence, the anticipation effect indicators from Equation 2.1, $Demobilisation_{it-s}$ for $s \in [0, 3]$, capture the demobilisation timing of the next closest paramilitary block. As for the restricted sample, we find that expropriations are significantly increased three years before demobilisation (Appendix Table 2.23). The coefficient's magnitude, 0.238, is equivalent to an increase in expropriations three years before paramilitaries demobilise of more than 39 percent of the mean number of expropriations throughout the sample period.

These results are very similar to the ones for the difference-in-differences estimation on the restricted sample (Table 2.5). Again, expropriations are only significantly

[54]Since local paramilitary dominance does not switch between different factions and groups for the same municipality over the course of our sample, we need to exclude municipality fixed effects in those specifications.

increased three years before demobilisation and even the coefficients' magnitudes are not drastically altered.[55] The large comparability of results between the two estimation approaches enhances our confidence in the extended sample analysis.

Further support for the validity of these findings is presented in Table 2.24. We interact binary variables denoting years 2001, 2002 and 2003 with the indicator for paramilitary presence. Not only does the coefficient on $Demobilisation_{t-3}$ remain significant but all results are also remarkably stable in terms of their magnitude. This allows us to conclude that it was not competing events in those years that caused the increase in expropriations but instead is driven by the pending demobilisation.

For the validity of any causal interpretation of a difference-in-differences approach, it is crucial that the units of observation in the treatment and comparison group are comparable. This issue is particularly pressing in the extended sample analysis since it includes municipalities that were under permanent control of paramilitaries as well as municipalities that were not. If there are systematic differences between those municipalities, the validity of the estimated effects becomes questionable. Such differences might explain why paramilitary groups maintained a permanent presence in the treated municipalities and could potentially also be meaningful for the process of expropriation.

To mitigate those concerns, we pursue a number of strategies. First, we exclude any municipalities where no recorded expropriation took place. Second, we provide an overview of summary statistics of a set of observables and assess whether there are any significant statistical differences. For a large majority of observables there are not statistically significant differences (Table 2.25 in the Appendix). The three variables for which we find significant discrepancies are area size, distance to coast

[55] For instance, comparing column (5) of Table 2.5 with that of Table 2.23 instead of an increase of 0.131 the restricted sample estimation yields an increase of 0.149 in the number of expropriated land plots in the year of demobilisation (both being clearly statistically insignificant). For our main variable of interest, $Demobilisation_{t-3}$, the coefficient rises slightly from 0.238 to 0.284. Unsurprisingly, the average number of expropriated plots of land per year is greater in the subsample of municipalities that had permanent paramilitary presence, namely 0.849 as opposed to 0.609 plots per year. As a result, we need to put the larger coefficient on $Demobilisation_{t-3}$ into perspective. The effect is equivalent to an increase of 33.5 percent relative to the mean number of expropriations per municipality per year, again similar to increase of 39 percent in the larger sample.

line, and average precipitation, which are either constant over time or explicitly controlled for (precipitation) in all our regressions. In Colombia, political and economic grievances historically played a pivotal role in inciting violence. To rule out that such grievances differ considerably between municipalities with or without paramilitary presence, we conduct t-tests on a number of measures that capture historical violence and conflicts (Table 2.26 in the Appendix) and do not find any significant mean differences in terms of land conflicts.[56] To this extent we can rule out that any historical grievances, which might explain paramilitary presence or land expropriations, are not comparable between the two sets of municipalities. Furthermore, we also find no significant differences in ethnic grievances, proxied by the historical presence of indigenous populations.

Despite the fact that we have suggestive supporting evidence for the comparability of municipalities as well as the great overlap of results in Tables 2.5 and 2.23, it is impossible to completely rule out alternative unobservable factors that drive paramilitary presence. If such factors exist and vary over time, so that fixed effects cannot control for them, our difference-in-differences estimation on the extended sample will be flawed. Therefore, the majority of results presented in this paper are estimated on the restricted sample of municipalities with a maintained presence of paramilitaries.

2.7 Conclusion

This paper studies how illegal actors might strategically increase asset appropriation as a mechanism to guarantee political and economic power when peace negotiations start. We argue that when insurgents can tailor the conditions of the peace agreement, they will look for strategies to appropriate assets that may guarantee economic and political rents during peacetime. Thus, a badly designed peace agreement might lead to weak institutional arrangements that trigger long term legacies of civil war such as an increase in the risk of a recurring conflict, manipulation of electoral competition or organised crime.

[56] We test for difference in grievance between 1901 to 1917, 1918 to 1931, or during the high violence wave between 1948 to 1953.

To empirically test our hypothesis, we use the case of peace negotiations between the Colombian government and right-wing paramilitaries between 2002 and 2006. After being actively involved in politics by pressuring the population to vote for a particular agenda, paramilitaries were able to manipulate the policymakers to create a peace agreement tailored to their needs. As a consequence, paramilitaries strategically and illegally appropriated an asset historically associated with political and economic power in Colombia: land.

Our empirical strategy exploits a presumably exogenous source variation in the timing of the final year of the demobilisation. The lack of coordination and the extremely disparate characteristics of paramilitaries' factions at the local level led to a spatial and temporal variation that was orthogonal to the idiosyncratic characteristics of municipalities. In this setting, we can test whether an anticipation effect may take place using a difference-in-differences strategy. We use two unique data sources to test this hypothesis. First, we geo-code a data set with the exact locations where the paramilitary blocks used to have permanent control. Furthermore, based on different sources we build a comprehensive profile for each paramilitary leader. Second, we use novel information on land claims reported by victims of expropriation.

Our results confirm that an anticipation effect on asset expropriation took place as a consequence of the expectation of the design and implementation of a tailored peace agreement. We show that this anticipation effect occurred between three important events: the senate election, presidency election and the formalisation of the peace agreement. We provide different robustness checks as well as an alternative measures of land transaction to validate our baseline results. Furthermore, we demonstrate that paramilitaries concentrated their expropriations in municipalities that were centrally located in their territories and thus firmly under their control. Finally, we document that these anticipatory expropriations are particularly pronounced for agriculturally productive land, supporting the hypothesis that these actions are driven by economic factors.

While further research is needed to identify the exact economic and political legacies of civil war, our results suggest that institutions may be an important chan-

nel through which negative effects of conflict persist in the long term. This paper also reveals that the nature and length of any transition period from war to peace can give rise to adverse effects when they are captured by non-state actors or elites. Similar to democratic transitions, peace negotiations as periods of transition can be used for strategic and anticipatory behaviour of actors if the transition is too long. Our work thus highlights the importance of careful design of peace treaties and conflict terminating agreements.

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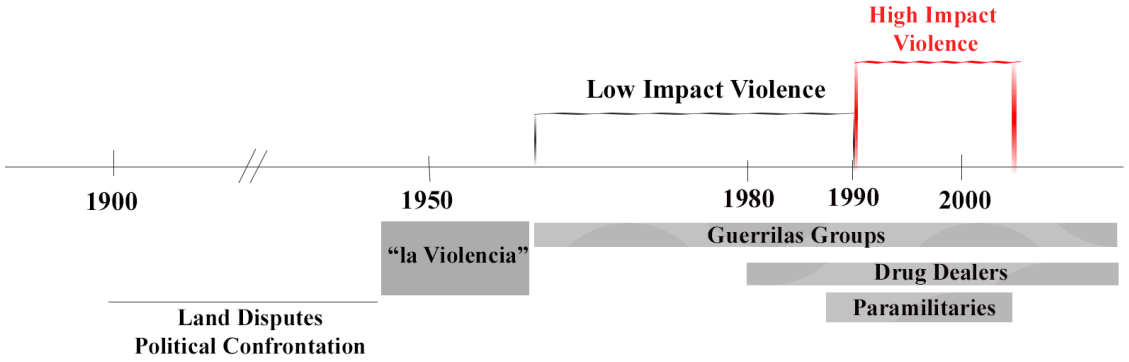
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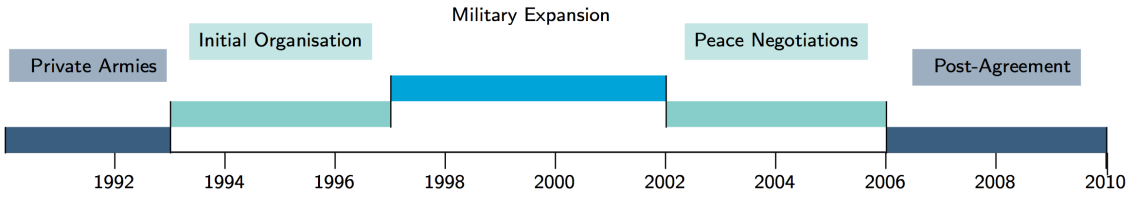
Tables and figures

Graph 2.1: Timeline of civil conflict in Colombia



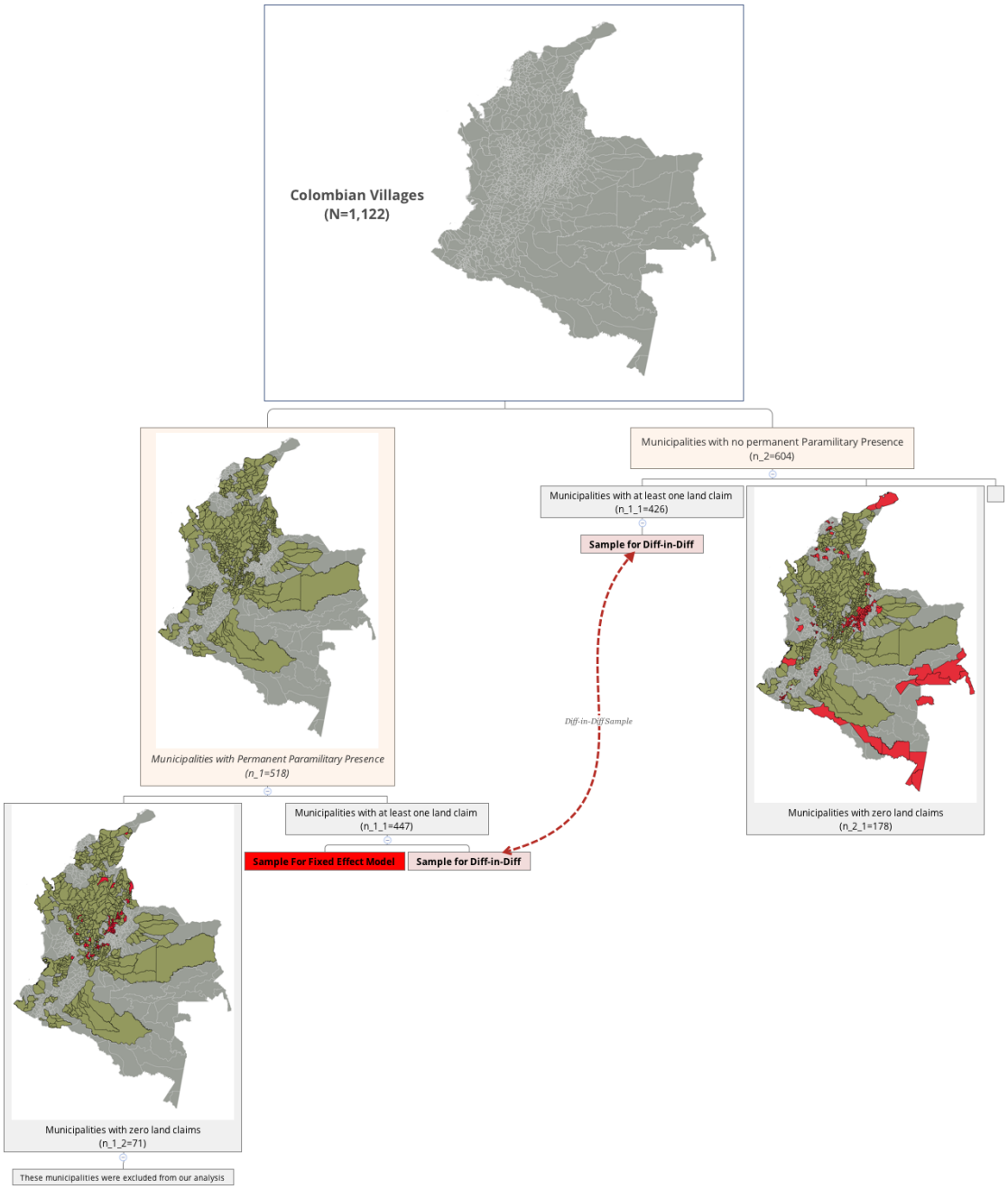
Notes– This graph shows the timeline of the conflict in Colombia. *Source*: Kleine-Rueschkamp and Muñoz-Mora, 2017.

Graph 2.2: Timeline paramilitaries



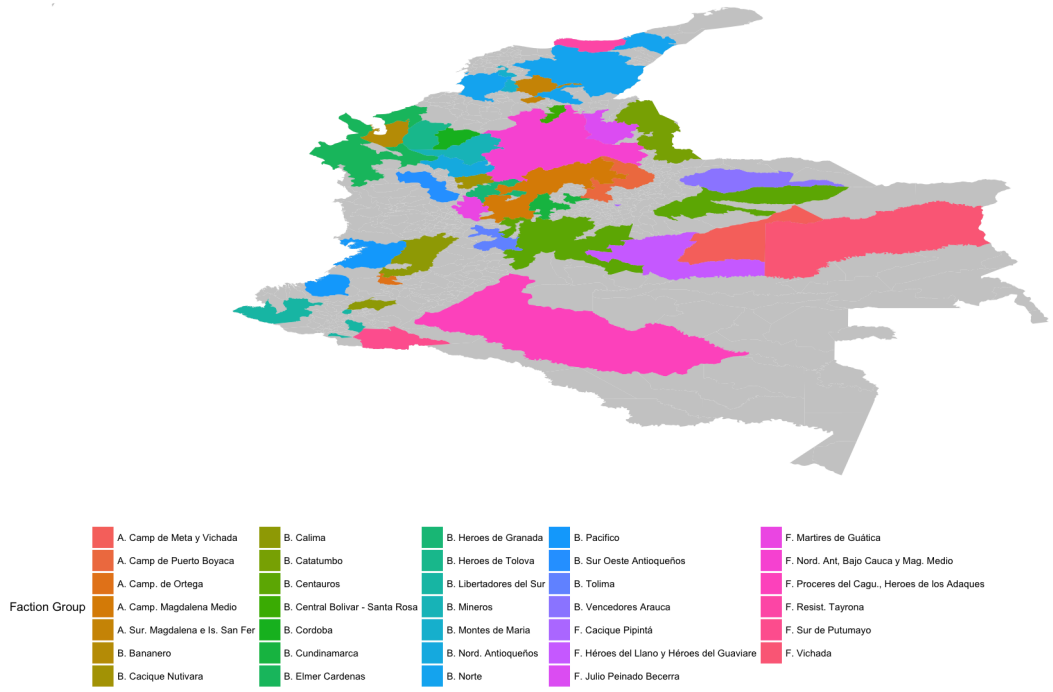
Source - Kleine-Rueschkamp and Muñoz-Mora, 2017.

Graph 2.3: Sampling tree: illustration of sample municipalities



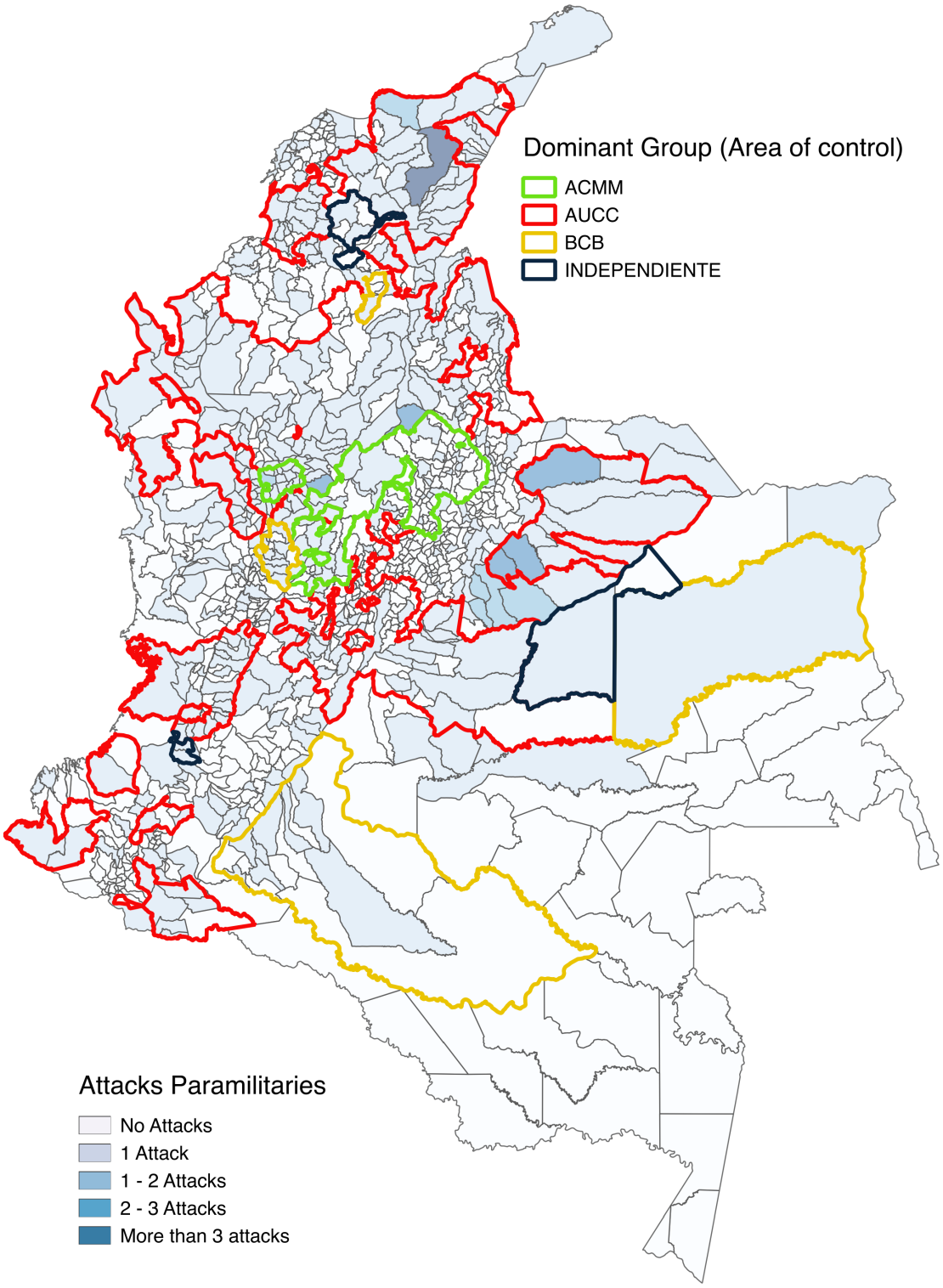
Notes– The figure explains the set of municipalities in our sample in the wider context of all Colombian municipalities.

Graph 2.4: Spatial distribution of the paramilitaries blocks in Colombia



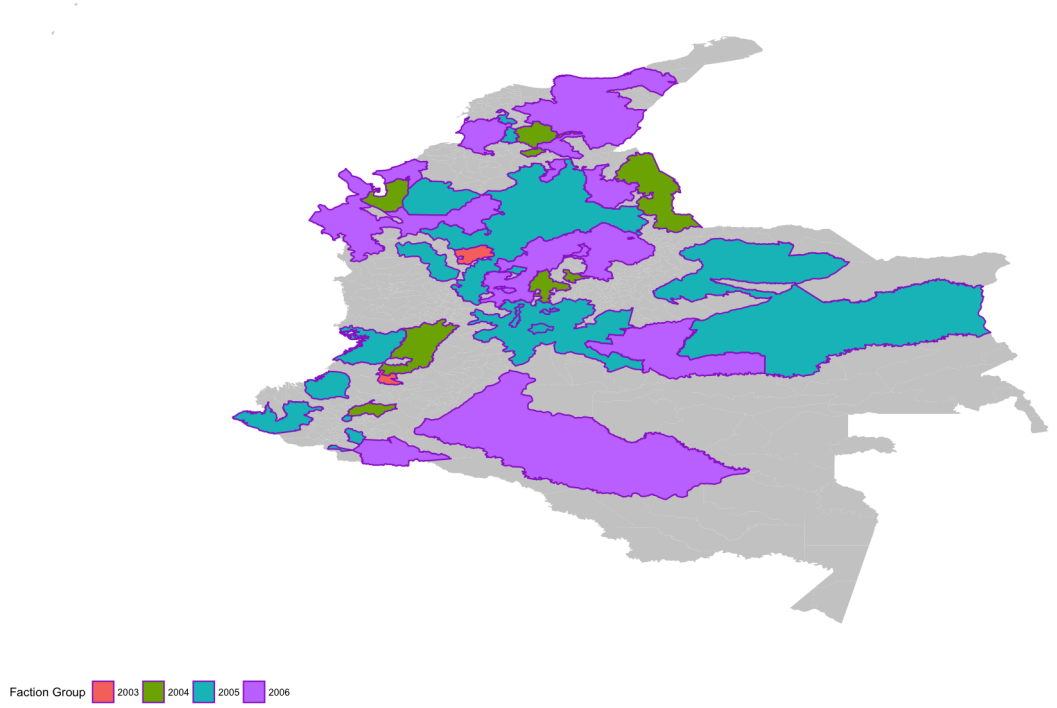
Notes– This map shows the spatial distribution of paramilitary blocks in Colombia based on the testimony of Paramilitaries Leaders and Government information. The capital letters represent abbreviations of common terms in names of paramilitary blocks (these terms can be seen as synonyms). A stands for "Autodefensas", B for "Bloque", and F for "Frente". *Source:* Kleine-Rueschkamp and Muñoz-Mora, 2017; based on information from the Colombian government, 2016.

Graph 2.5: Comparison of paramilitary control and paramilitary attacks



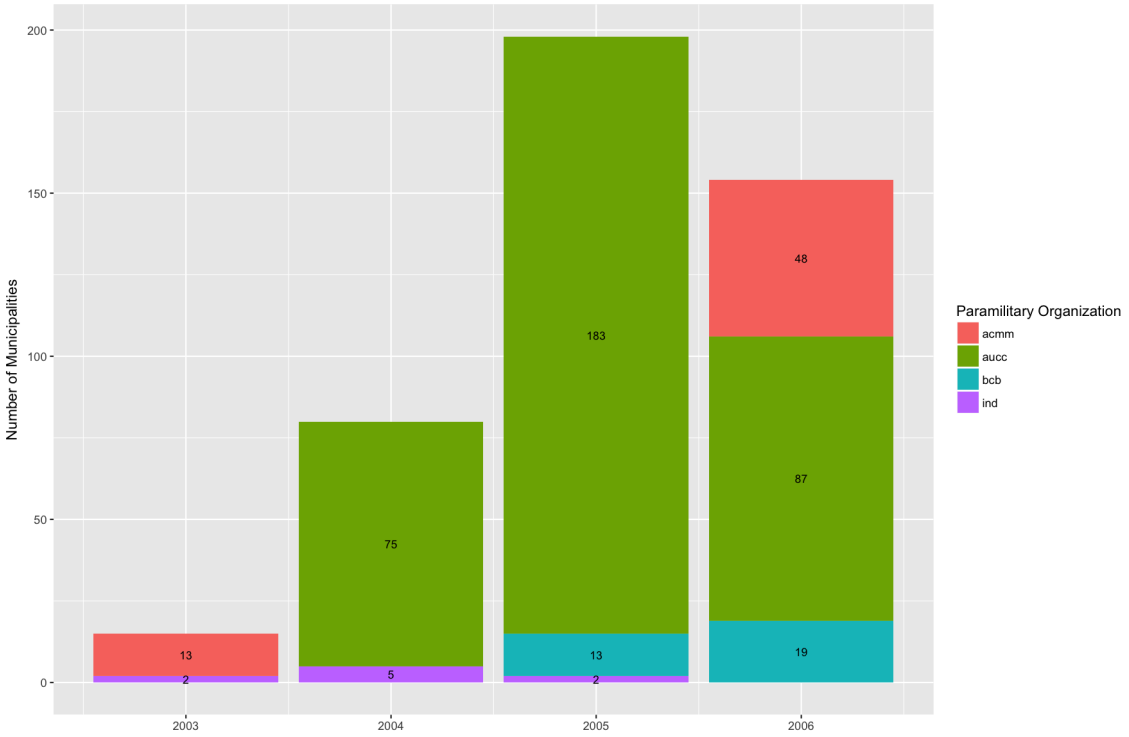
Notes– This map shows the area of paramilitary control in Colombia based on the testimony of Paramilitaries Leaders and Government information and contrasts it with the intensity of paramilitary attacks. The coloured lines define the areas under control of paramilitary blocks. *Source:* Colombian Government (2016) and Dube and Vargas (2013).

Graph 2.6: Spatial distribution of paramilitaries demobilisation timing



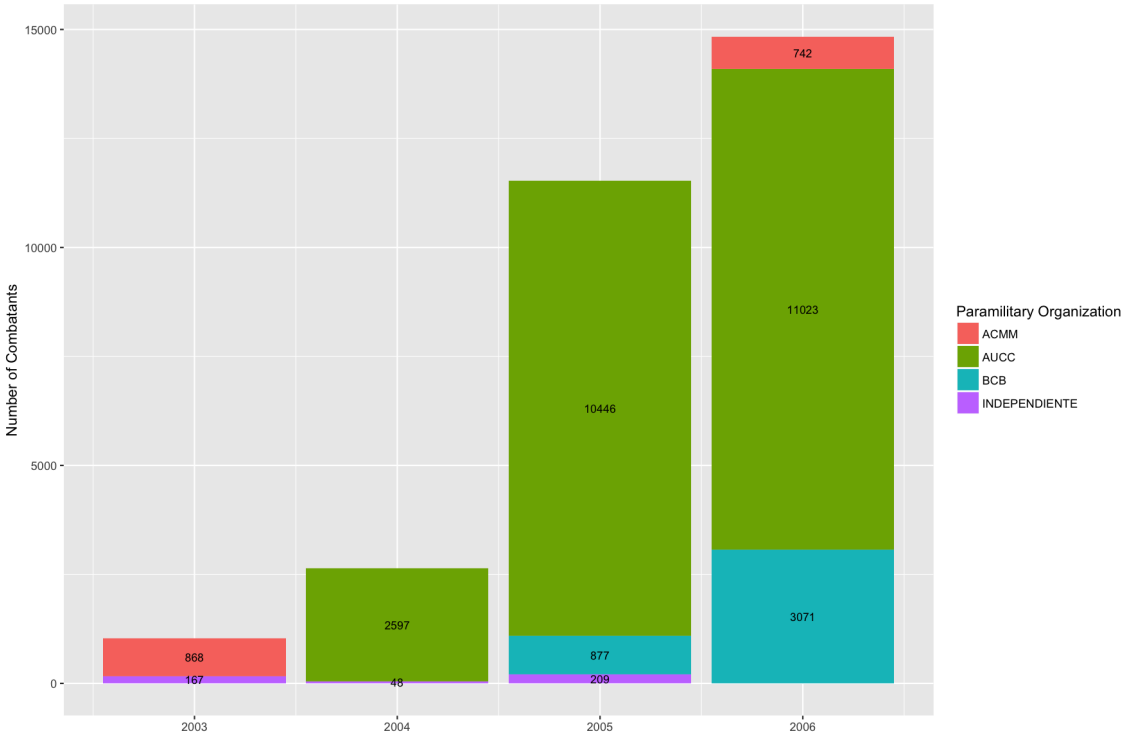
Notes– This map shows the spatial distribution of the timing of demobilisation across Colombian Municipalities. *Source:* Kleine-Rueschkamp and Muñoz-Mora, 2017; based on information from the Colombian government, 2016.

Graph 2.7: Number municipalities affected by demobilisation over time



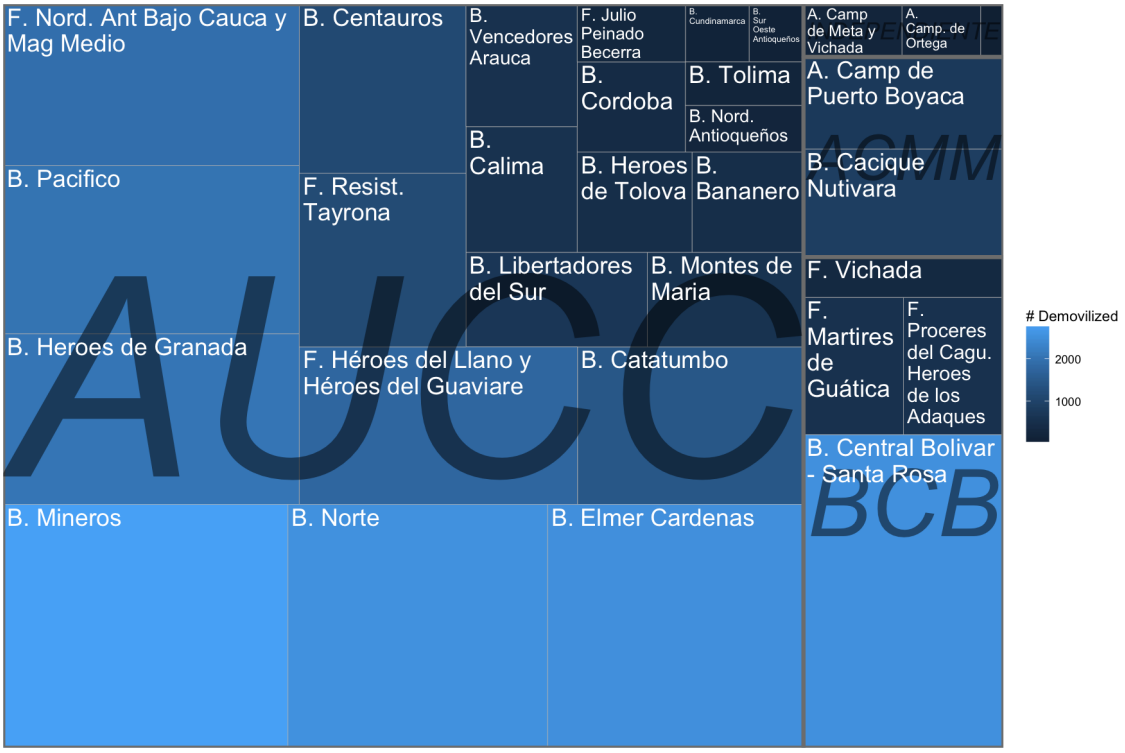
Notes– This map shows the number of municipalities in where paramilitaries demobilised by year. *Source:* Kleine-Rueschkamp and Muñoz-Mora, 2017; based on information from the Colombian government, 2016.

Graph 2.8: Number of combatants demobilised by main paramilitary groups over years



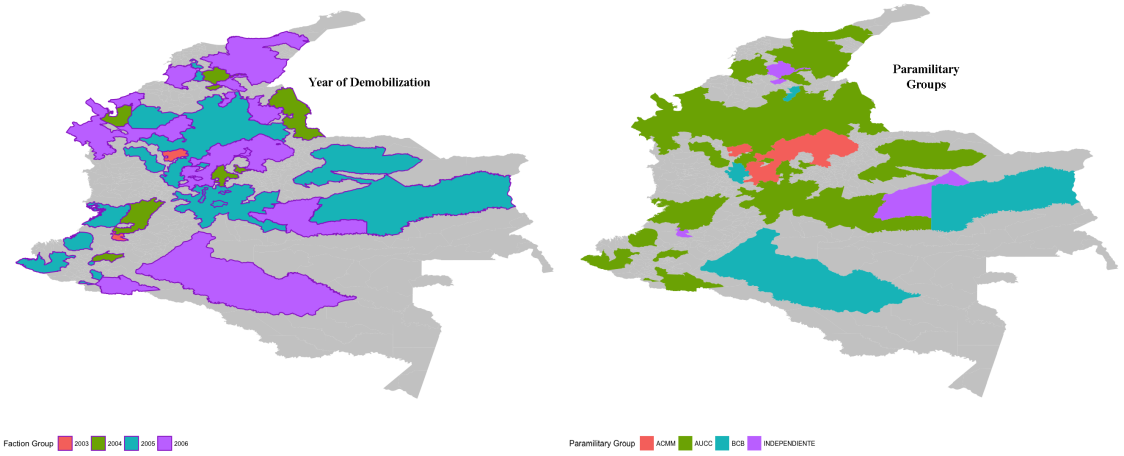
Notes– This map shows the number of demobilised combatants by paramilitary organisation over year. ACMM="Autodefensas Campesinas del Magdalena Medio". AUCC="Autodefensas Campesinas de Cordoba y Uraba". BCB="Bloque Central Bolivar". INDEPENDIENTE="Independents paramilitary groups". *Source:* Kleine-Rueschkamp and Muñoz-Mora, 2017; based on information from the Colombian government, 2016.

Graph 2.9: Number of combatants demobilised by paramilitary faction



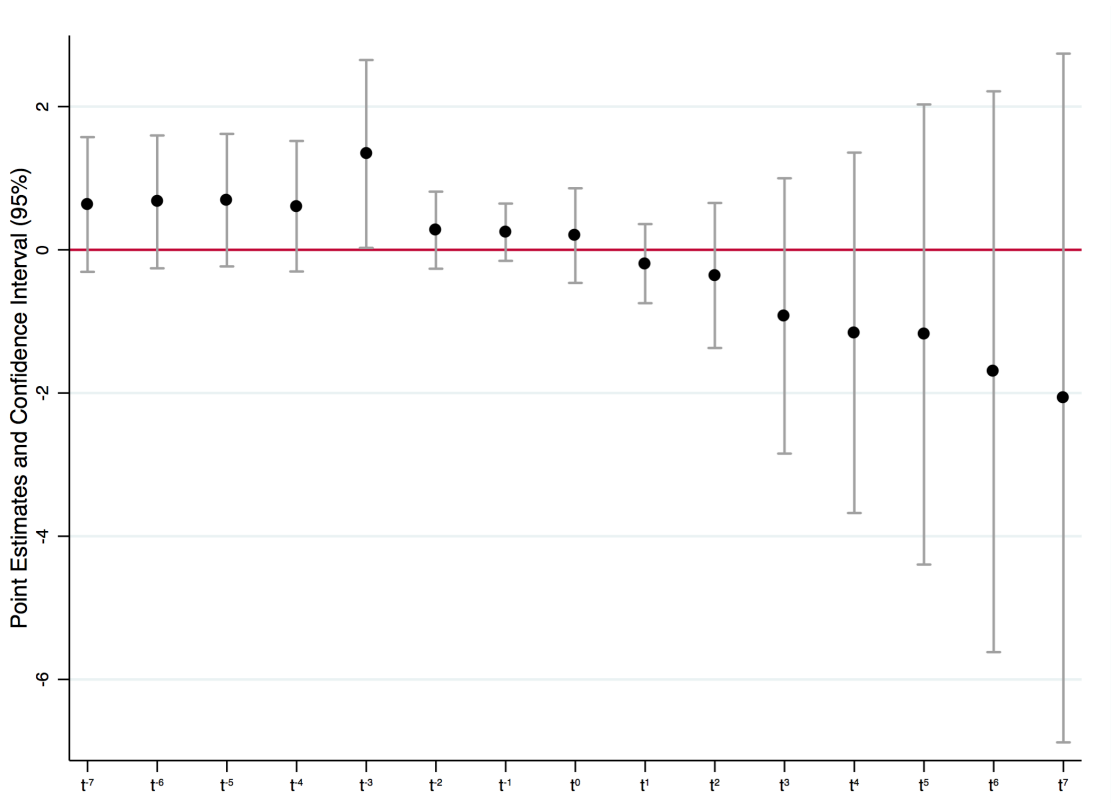
Notes– The figure displays the number of demobilised combatants and their affiliated block.
Source: Kleine-Rueschkamp and Muñoz-Mora, 2017; based on information from the Colombian government, 2016.

Graph 2.10: Comparison: spatial distribution of paramilitaries and demobilisation timing



Notes– This map shows the spatial distribution of the timing of demobilisation across Colombian Municipalities. *Source:* Kleine-Rueschkamp and Muñoz-Mora, 2017; based on information from the Colombian government, 2016.

Graph 2.11: Difference-in-differences validity check



Notes– The figure shows the parallel trend validity check of expropriations of land plots before and after the demobilisation of the paramilitaries. t_0 marks the year of demobilisation. The figure presents the point estimates and 95% confidence intervals of the effect of the respective year on the number of land expropriations.

Table 2.1: Summary statistics, all sample municipalities

	Observations	Mean	Std. Deviation	Q50	Q90
Time-invariant variables					
Area (ha)	625	45751.494	56023.052	27600.000	99300.000
% Fertile Soil	625	52.456	35.185	57.143	100.000
Average precipitation from 1961-1980	625	21.381	4.877	21.208	27.889
Average temperature from 1961-1980	625	161.685	52.556	153.411	227.708
Altitude	625	1122.891	818.434	1185.000	2180.000
Distance to State Capital (km)	625	75.863	52.567	65.628	143.708
Land Gini, 1985	596	70.621	9.199	70.980	82.280
Local voting share for Alvaro Uribe	625	69.496	15.551	73.024	87.881
Dummy for Coca Cultivation	625	0.019	0.138	0.000	0.000
Preferences for the Right	574	43.659	27.700	37.703	84.283
Distance to Coastline (km)	625	243.761	133.589	232.832	423.490
Share of Area in Mining	625	0.095	0.136	0.040	0.252
Time-variant variables					
Expropriated Area (Ha) - Total	8750	49.528	526.144	0.000	30.000
Expropriated Plots - Total	8750	0.610	2.976	0.000	1.000
Number of Land Claims - Total	8750	0.721	3.249	0.000	2.000
Number of displacements	8750	0.313	0.924	0.073	0.685
Rurality Index	8750	2.365	2.466	1.634	5.419
log population	8710	9.594	1.061	9.479	10.767

Notes - This table shows the basic descriptive statistics for municipalities that recorded at least one expropriation throughout the sample period. We consider information from 1997 till 2010. We excluded those paramilitaries blocks that demobilised in 2003 and municipalities with presence of indigenous. *Data source:* CEDE (2016) and Colombian Government (2016).

Table 2.2: Summary statistics, municipalities under paramilitaries control

	Observations	Mean	Std. Deviation	Q50	Q90
Time-invariant variables					
Area (ha)	335	51472.990	57315.704	36500.000	1.11e+05
% Fertile Soil	335	50.962	34.402	54.396	99.606
Average precipitation from 1961-1980	335	21.566	4.842	21.597	27.883
Average temperature from 1961-1980	335	171.080	52.424	168.583	232.100
Altitude	335	1085.743	773.594	1115.000	2113.000
Distance to State Capital (km)	335	77.357	56.877	68.127	139.552
Land Gini, 1985	324	70.650	9.220	71.040	82.710
Local voting share for Alvaro Uribe	335	70.559	14.462	73.533	87.618
Dummy for Coca Cultivation	335	0.016	0.126	0.000	0.000
Preferences for the Right	315	43.241	27.735	37.888	85.404
Distance to Coastline (km)	335	257.643	114.504	265.090	391.508
Share of Area in Mining	335	0.097	0.125	0.054	0.230
Time-variant variables					
Expropriated Area (Ha) - Total	4690	65.775	657.952	0.000	75.650
Expropriated Plots - Total	4690	0.851	3.612	0.000	2.000
Number of Land Claims - Total	4690	1.009	3.959	0.000	3.000
Number of displacements	4690	0.427	1.177	0.110	0.952
Rurality Index	4690	2.334	2.397	1.580	5.280
log population	4679	9.681	1.084	9.573	10.910

Notes - This table shows the basic descriptive statistics for municipalities that were defined as a permanent control of paramilitaries. We consider information from 1997 till 2010. We excluded those paramilitaries blocks that demobilised in 2003 and municipalities with presence of indigenous. *Data source:* CEDE (2016) and Colombian Government (2016).

Table 2.3: Determinants of the demobilisation timing (ordered probit regression)

Dependent Variable: Timing of demobilisation timing (Early demobilisation 2003 to late demobilisation 2006)

	2003	2004	2005	2006
Paramilitary Attacks 1997-2001	0.030	0.078	0.019	-0.128
	(0.038)	(0.065)	(0.035)	(0.119)
Preferences for the Left	0.000	0.000	0.000	-0.000
	(0.001)	(0.002)	(0.000)	(0.003)
Vote share for Alvaro Uribe, 2002	0.001	0.002	0.001	-0.003
	(0.001)	(0.002)	(0.001)	(0.003)
Land gini, 1985	-0.042	-0.107	-0.026	0.174
	(0.121)	(0.252)	(0.078)	(0.439)
Rurality Index	0.012	0.030	0.007	-0.048
	(0.038)	(0.119)	(0.029)	(0.184)
log. Average Population	0.021	0.054	0.013	-0.088
	(0.015)	(0.037)	(0.022)	(0.055)
Log. Area (ha)	-0.023	-0.059*	-0.014	0.096***
	(0.018)	(0.023)	(0.019)	(0.026)
Log. Distance to coastline (km)	-0.016	-0.041	-0.010	0.068
	(0.034)	(0.062)	(0.022)	(0.112)
Log. Distance to state capital (km)	0.000	0.000	0.000	-0.000
	(0.000)	(0.001)	(0.000)	(0.001)
Average precipitation from 1961-1980	-0.004	-0.011*	-0.003	0.018**
	(0.004)	(0.005)	(0.004)	(0.007)
Average temperature from 1961-1980	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.001)
Observations	302	302	302	302

Notes - The table reports the marginal effects for the ordered probit. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The treatment groups are composed by those municipalities where paramilitaries groups had a permanent territorial control. *Data source:* Colombian Government, 2017

Table 2.4: Paramilitary characteristics as determinants of the demobilisation timing (ordered probit regression)

Dependent Variable: Timing of demobilisation timing (Early demobilisation 2003 to late demobilisation 2006)

	2003	2004	2005	2006
Leader with extradition request (yes=1)	-0.018	-0.044	-0.011	0.073
	(0.045)	(0.094)	(0.031)	(0.163)
Leader with grievances against guerrillas (yes=1)	-0.034	-0.083	-0.021	0.138
	(0.082)	(0.158)	(0.066)	(0.296)
Paramilitary Attacks 1997-2001	0.002	0.005	0.001	-0.008
	(0.038)	(0.089)	(0.023)	(0.150)
Observations	335	335	335	335

Notes - The table reports the marginal effects for the ordered probit. * p < 0.1, ** p < 0.05, *** p < 0.01. The treatment groups are composed by those municipalities where paramilitaries groups had a permanent territorial control. *Data source:* Colombian Government, 2017

Table 2.5: Baseline results

Dependent Variable: number of expropriated plots

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	0.065	0.077	0.097	0.151	0.149
	[0.222]	[0.223]	[0.214]	[0.215]	[0.215]
$Demobilisation_{t-1}$	0.157	0.172	0.198	0.156	0.153
	[0.191]	[0.192]	[0.183]	[0.183]	[0.183]
$Demobilisation_{t-2}$	0.152	0.167	0.193	0.078	0.075
	[0.228]	[0.228]	[0.219]	[0.177]	[0.177]
$Demobilisation_{t-3}$	0.280*	0.291**	0.311**	0.287**	0.284**
	[0.145]	[0.146]	[0.140]	[0.135]	[0.135]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic-Specific trends	No	No	No	No	Yes
Dep. Var Mean	0.849	0.849	0.849	0.849	0.849
F-test (p-value)	0.305	0.270	0.183	0.255	0.261
Observations	4690	4690	4690	4536	4536

Notes - All regressions control for municipality fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.6: Baseline results - negative binomial regression

Dependent Variable: number of expropriated plots

	(1)	(2)	(3)	(4)	(5)
$Demobilization_{t0}$	0.093	0.094	0.102	0.136	0.133
	[0.174]	[0.171]	[0.172]	[0.171]	[0.169]
$Demobilization_{t-1}$	0.123	0.142	0.073	0.123	0.118
	[0.172]	[0.170]	[0.173]	[0.173]	[0.172]
$Demobilization_{t-2}$	0.154	0.162	0.100	0.148	0.144
	[0.200]	[0.196]	[0.187]	[0.187]	[0.185]
$Demobilization_{t-3}$	0.312*	0.312*	0.308*	0.348**	0.343**
	[0.177]	[0.174]	[0.172]	[0.172]	[0.171]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic-Specific trends	No	No	No	No	Yes
Dep. Var Mean	0.849	0.849	0.849	0.849	0.608
Observations	4690	4690	4536	4536	4536

Notes - Negative binomial regressions. All regressions control for municipality fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.7: Expropriation and soil quality

Dependent Variable: number of expropriated plots

	(1)	(2)	(3)	(4)
$Demobilisation_{t0}$	0.566	0.575	0.638	0.633
	[0.431]	[0.438]	[0.444]	[0.442]
$Demobilisation_{t-1}$	0.017	0.042	0.121	0.116
	[0.211]	[0.210]	[0.211]	[0.210]
$Demobilisation_{t-2}$	0.004	0.036	0.055	0.050
	[0.175]	[0.171]	[0.176]	[0.175]
$Demobilisation_{t-3}$	-0.245	-0.214	-0.248	-0.251
	[0.215]	[0.217]	[0.216]	[0.217]
$Demobilisation_{t-1} * SoilQuality$	0.003	0.004	0.001	0.001
	[0.003]	[0.003]	[0.002]	[0.002]
$Demobilisation_{t-2} * SoilQuality$	0.003	0.004	0.001	0.001
	[0.004]	[0.004]	[0.003]	[0.003]
$Demobilisation_{t-3} * SoilQuality$	0.010**	0.010**	0.011**	0.011**
	[0.005]	[0.005]	[0.005]	[0.005]
Year FE	Yes	Yes	Yes	Yes
Geography-Specific trend	No	Yes	Yes	Yes
Climate-Specific trend	No	No	Yes	Yes
Socioeconomic variables	No	No	No	Yes
Dep. Var Mean	0.608	0.536	0.518	0.608
F-Test Interactions	0.052	0.049	0.104	0.103
Observations	4690	4690	4536	4536

Notes - All regressions control for municipality fixed effects. Soil quality is the average percentage of a municipality's land surface area that has fertile soil. Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.8: Expropriation and land quality: cropland

Dependent Variable: number of expropriated plots

	(1)	(2)	(3)	(4)
$Demobilisation_{t0}$	-0.104	-0.069	0.015	0.012
	[0.185]	[0.178]	[0.163]	[0.162]
$Demobilisation_{t-1}$	-0.184	-0.151	0.176	0.163
	[0.399]	[0.395]	[0.225]	[0.225]
$Demobilisation_{t-2}$	-0.295	-0.270	0.033	0.012
	[0.381]	[0.371]	[0.234]	[0.236]
$Demobilisation_{t-3}$	0.315	0.322	0.243	0.218
	[0.317]	[0.312]	[0.307]	[0.309]
$Demobilisation_{t-1} * ShareCropland$	4.247	4.282	0.087	0.154
	[4.358]	[4.382]	[1.222]	[1.232]
$Demobilisation_{t-2} * ShareCropland$	5.735	5.880	0.604	0.770
	[5.480]	[5.489]	[1.334]	[1.349]
$Demobilisation_{t-3} * ShareCropland$	0.285	0.546	0.816	1.070
	[2.675]	[2.672]	[2.766]	[2.790]
Year FE	Yes	Yes	Yes	Yes
Geography-Specific trend	No	Yes	Yes	Yes
Climate-Specific trend	No	No	Yes	Yes
Socioeconomic variables	No	No	No	Yes
Dep. Var Mean	0.608	0.514	0.496	0.608
F-Test Interactions	0.307	0.289	0.700	0.616
Observations	4494	4494	4340	4340

Notes - All regressions control for municipality fixed effects. The *ShareCropland* variable captures the share of land in a municipality that is used for growing crops. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.9: Expropriation and land quality: coca cultivation

Dependent Variable: number of expropriated plots

	(1)	(2)	(3)	(4)
$Demobilisation_{t0}$	0.051	0.081	0.135	0.133
	[0.256]	[0.245]	[0.247]	[0.247]
$Demobilisation_{t-1}$	0.236	0.276	0.207	0.205
	[0.205]	[0.198]	[0.194]	[0.194]
$Demobilisation_{t-2}$	0.183	0.223	0.076	0.074
	[0.246]	[0.239]	[0.184]	[0.183]
$Demobilisation_{t-3}$	0.140	0.172	0.136	0.134
	[0.136]	[0.131]	[0.125]	[0.125]
$Demobilisation_{t-1} * Coca$	-0.339	-0.352	-0.214	-0.221
	[0.257]	[0.264]	[0.255]	[0.254]
$Demobilisation_{t-2} * Coca$	-0.032	-0.049	0.117	0.109
	[0.373]	[0.375]	[0.372]	[0.371]
$Demobilisation_{t-3} * Coca$	0.955	0.935	1.012	1.001
	[0.653]	[0.653]	[0.699]	[0.700]
Year FE	Yes	Yes	Yes	Yes
Geography-Specific trend	No	Yes	Yes	Yes
Climate-Specific trend	No	No	Yes	Yes
Socioeconomic variables	No	No	No	Yes
Dep. Var Mean	0.608	0.535	0.518	0.608
F-Test Interactions	0.529	0.568	0.342	0.357
Observations	4679	4679	4536	4536

Notes - All regressions control for municipality fixed effects. *Coca* is a binary variable that indicates the presence of coca cultivation in a municipality. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.10: Expropriation and land quality: mining activities

Dependent Variable: number of expropriated plots

	(1)	(2)	(3)	(4)
$Demobilisation_{t0}$	0.007	0.036	0.078	0.076
	[0.277]	[0.269]	[0.273]	[0.272]
$Demobilisation_{t-1}$	0.265	0.304	0.197	0.193
	[0.214]	[0.207]	[0.193]	[0.192]
$Demobilisation_{t-2}$	0.086	0.126	-0.048	-0.053
	[0.264]	[0.256]	[0.180]	[0.180]
$Demobilisation_{t-3}$	0.343*	0.375**	0.344**	0.341**
	[0.177]	[0.174]	[0.172]	[0.173]
$Demobilisation_{t-1} * ShareMining$	-1.072	-1.087	-0.462	-0.463
	[0.804]	[0.804]	[0.693]	[0.693]
$Demobilisation_{t-2} * ShareMining$	0.630	0.605	1.203	1.203
	[0.925]	[0.927]	[0.816]	[0.816]
$Demobilisation_{t-3} * ShareMining$	-0.619	-0.655	-0.602	-0.603
	[0.780]	[0.787]	[0.818]	[0.817]
Year FE	Yes	Yes	Yes	Yes
Geography-Specific trend	No	Yes	Yes	Yes
Climate-Specific trend	No	No	Yes	Yes
Socioeconomic variables	No	No	No	Yes
Dep. Var Mean	0.608	0.536	0.518	0.608
F-Test Interactions	0.565	0.539	0.933	0.933
Observations	4690	4690	4536	4536

Notes - All regressions control for municipality fixed effects. *ShareMining* indicates whether there are mining activities in a municipality. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.11: Other forms of expropriation: thefts & robberies

Dependent Variable: number of thefts & robberies

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	6.751	7.421	-10.199	-16.410	-18.420
	[40.610]	[40.279]	[34.012]	[34.059]	[33.344]
$Demobilisation_{t-1}$	-0.252	1.118	-36.948	-51.106	-55.623
	[99.848]	[99.411]	[91.153]	[94.221]	[92.948]
$Demobilisation_{t-2}$	49.745	52.337	-9.659	-35.285	-41.806
	[155.664]	[155.001]	[134.554]	[136.738]	[134.086]
$Demobilisation_{t-3}$	74.819	78.333	-30.101	-65.997	-67.718
	[190.550]	[189.456]	[155.762]	[158.320]	[159.204]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes
Dep. Var Mean	168.516	168.516	168.516	168.516	168.516
F -test (p-value)	0.787	0.773	0.834	0.689	0.661
Observations	2664	2664	2664	2576	2576

Notes - All regressions control for municipality fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.12: Other forms of expropriation: private property

Dependent Variable: number of assaults on private property

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	0.001	0.001	0.001	0.001	0.001
	[0.003]	[0.003]	[0.003]	[0.004]	[0.004]
$Demobilisation_{t-1}$	0.003	0.003	0.003	0.003	0.003
	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]
$Demobilisation_{t-2}$	0.008	0.008	0.009	0.009	0.009
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]
$Demobilisation_{t-3}$	-0.000	-0.000	-0.000	0.000	0.000
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes
Dep. Var Mean	0.003	0.003	0.003	0.003	0.003
F-test (p-value)	0.462	0.456	0.436	0.413	0.417
Observations	4680	4680	4680	4536	4536

Notes - All regressions control for municipality fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.13: Effects on area of expropriated land

Dependent Variable: expropriated land in square km

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	45.689	46.790	44.561	47.041	46.675
	[48.532]	[48.797]	[50.553]	[48.187]	[48.121]
$Demobilisation_{t-1}$	41.206	42.652	41.975	45.225	44.718
	[45.318]	[46.289]	[47.158]	[48.758]	[48.595]
$Demobilisation_{t-2}$	8.057	9.503	8.738	11.988	11.415
	[44.575]	[45.311]	[45.949]	[46.198]	[46.071]
$Demobilisation_{t-3}$	12.919	14.020	16.023	18.502	18.005
	[40.258]	[40.734]	[41.116]	[41.669]	[41.628]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	No	Yes	Yes
Climate-Specific trend	No	No	Yes	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes
Dep. Var Mean	65.684	65.684	65.684	65.684	49.351
Observations	4690	4690	4536	4536	4536

Notes - All regressions control for municipality fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.14: Heterogeneity by guerrilla activity

Dependent Variable: number of expropriations

	(1)	(2)	(3)	(4)
$Demobilisation_{t-3}$	0.340*	0.290*	0.310**	0.263*
	[0.198]	[0.166]	[0.155]	[0.147]
$Demobilisation_{t-3}$* high Guerrilla attacks	-0.275			
	[0.306]			
$Demobilisation_{t-3}$* Guerrilla attacks 1997-2001		-0.084		
		[0.092]		
$Demobilisation_{t-3}$* FARC road attacks			-0.649*	
			[0.383]	
$Demobilisation_{t-3}$* ELN road attack				-0.314**
				[0.160]
Year FE	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	Yes	Yes	Yes	Yes
Geography-Specific trend	Yes	Yes	Yes	Yes
Climate-Specific trend	Yes	Yes	Yes	Yes
Socioeconomic variables	Yes	Yes	Yes	Yes
Dep. Var Mean	0.608	0.608	0.608	0.608
Observations	8344	8344	8344	8344

Notes - All regressions control for municipality fixed effects. "High guerrilla attacks" is a binary indicator for municipalities with above-median numbers of guerrilla attacks. The other measures give the average number of guerrilla attacks between 1997 and 2001, the average number of attacks on roads committed by the FARC between 1997 and 2001, and the average number of attacks on roads committed by the ELC between 1997 and 2001. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.15: Frontier areas and average number of paramilitary attacks

Dependent Variable: annual and average annual number of paramilitary attacks

	(1) Annual	(2) Mean
Frontier area	0.039	0.048**
	[0.028]	[0.022]
Year FE	Yes	No
Dep. Var Mean	0.12572	0.12572
Observations	8718	1122

Notes - The table shows the regression of paramilitary attacks on whether a municipality was at the frontier of paramilitary territory, i.e. whether it bordered the area controlled by a paramilitary block. Column (1) present the results for the annual number of attacks, Column (2) the results for mean number of attacks in each municipality between 1997 and 2006. Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.16: Distance to the boundary of paramilitary control

Dependent Variable: number of expropriations

	(1) central	(2) border	(3) central	(4) border
$Demobilisation_{t0}$	-0.042	0.170	0.122	0.198
	[0.279]	[0.353]	[0.237]	[0.361]
$Demobilisation_{t-1}$	0.218	0.095	0.218	0.116
	[0.275]	[0.264]	[0.251]	[0.267]
$Demobilisation_{t-2}$	0.349	-0.007	0.144	0.041
	[0.405]	[0.241]	[0.263]	[0.243]
$Demobilisation_{t-3}$	0.417	0.187	0.374*	0.232
	[0.253]	[0.184]	[0.217]	[0.190]
	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	No	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes
Climate-Specific trend	No	No	Yes	Yes
Socioeconomic variables	No	No	Yes	Yes
Dep. Var Mean	1.261	0.445	1.261	0.445
Observations	2324	2366	2240	2296

Notes - All regressions control for municipality fixed effects. The table presents heterogeneous effects of demobilisation on expropriation based on municipalities' location within the area controlled by each paramilitary block. Municipalities are divided into two sub-samples based on their distance to the border of the respective paramilitary block's control. Columns 1 and 3 present the results for the sub-sample of municipalities with above-median distance to the border (more central areas). Columns 2 and 4 present the results for the sub-sample of municipalities with below-median distance to the border (less central areas). Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.17: Effects on land tenure - Gini index

Dependent Variable: Land Gini index

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	-0.006*	-0.006*	-0.007**	-0.006**	-0.006**
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
$Demobilisation_{t-1}$	-0.003	-0.003	-0.004	-0.003	-0.003
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
$Demobilisation_{t-2}$	-0.006**	-0.007**	-0.009***	-0.007**	-0.008**
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
$Demobilisation_{t-3}$	-0.001	-0.001	-0.001	0.000	-0.000
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
$Demobilisation_{t-4}$	-0.001	-0.002	-0.002	-0.000	-0.001
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	No	Yes	Yes
Climate-Specific trend	No	No	Yes	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes
Dep. Var Mean	0.731	0.731	0.731	0.731	0.722
Observations	2582	2582	2490	2490	2490

Notes - All regressions control for municipality fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.18: Robustness check: controlling for guerrilla activity

Dependent Variable: number of expropriations

	(1)	(2)	(3)	(4)
$Demobilisation_{t0}$	0.132	0.130	0.131	0.131
	[0.150]	[0.150]	[0.150]	[0.150]
$Demobilisation_{t-1}$	0.119	0.118	0.120	0.117
	[0.103]	[0.103]	[0.103]	[0.103]
$Demobilisation_{t-2}$	0.057	0.057	0.057	0.057
	[0.099]	[0.099]	[0.099]	[0.099]
$Demobilisation_{t-3}$	0.237*	0.239*	0.238*	0.237*
	[0.142]	[0.142]	[0.142]	[0.143]
Guerrilla attacks 1997-2001 * trend	-0.007			
	[0.007]			
High guerrilla attacks dummy* trend		0.010		
		[0.025]		
FARC, attacks on roads			0.049	
			[0.154]	
ELN, attacks on roads				0.050
				[0.099]
Year FE	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	Yes	Yes	Yes	Yes
Geography-Specific trend	Yes	Yes	Yes	Yes
Climate-Specific trend	Yes	Yes	Yes	Yes
Socioeconomic variables	Yes	Yes	Yes	Yes
Dep. Var Mean	0.608	0.608	0.608	0.608
Observations	8344	8344	8344	8344

Notes - All regressions control for municipality fixed effects. "High guerrilla attacks" is a binary indicator for municipalities with above-median numbers of guerrilla attacks. The other measures give the average number of guerrilla attacks between 1997 and 2001, the average number of attacks on roads committed by the FARC between 1997 and 2001, and the average number of attacks on roads committed by the ELC between 1997 and 2001. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.19: Robustness check: number of displaced people

Dependent Variable: number of displaced people

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	22.336	25.421	84.064	96.848	102.154
	[43.885]	[45.646]	[60.848]	[67.660]	[69.559]
$Demobilisation_{t-1}$	108.782	112.707	194.032	210.895	221.068
	[120.507]	[124.274]	[142.688]	[150.083]	[153.401]
$Demobilisation_{t-2}$	141.865	143.835	224.233	242.131	244.787
	[153.472]	[156.721]	[179.106]	[187.770]	[189.159]
$Demobilisation_{t-3}$	80.179	80.561	139.564	149.090	157.839
	[94.104]	[96.066]	[128.316]	[139.145]	[142.708]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes
Dep. Var Mean	518.797	518.797	518.797	518.797	396.916
F-test (p-value)	0.349	0.352	0.186	0.179	0.171
Observations	3213	3213	3213	3114	3114

Notes - All regressions control for municipality fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.20: Power of paramilitaries: strong third parties in 2006 ($> 25\%$ in congress elections)

Dependent Variable: number of expropriations

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	0.074	0.079	0.103	0.166	0.167
	[0.262]	[0.261]	[0.251]	[0.253]	[0.253]
$Demobilisation_{t-1}$	0.208	0.214	0.246	0.201	0.202
	[0.218]	[0.218]	[0.208]	[0.210]	[0.209]
$Demobilisation_{t-2}$	0.137	0.143	0.175	0.038	0.039
	[0.265]	[0.264]	[0.254]	[0.206]	[0.205]
$Demobilisation_{t-3}$	0.296*	0.300*	0.325**	0.296*	0.296*
	[0.169]	[0.169]	[0.161]	[0.156]	[0.157]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes
Dep. Var Mean	0.952	0.952	0.952	0.952	0.608
F-test (p-value)	0.334	0.317	0.219	0.304	0.301
Observations	3934	3934	3934	3780	3780

Notes - All regressions control for municipality fixed effects. The table reports our baseline estimation results for a restricted sample that excludes municipalities where third parties, associated with paramilitary interests, received more than 25% of votes in the congressional elections in 2006. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.21: Controlling for group fixed effects of macro-paramilitary organisations

Dependent Variable: number of expropriations

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	0.065	0.070	0.048	0.091	0.096
	[0.222]	[0.224]	[0.221]	[0.222]	[0.223]
$Demobilisation_{t-1}$	0.157	0.162	0.133	0.078	0.083
	[0.191]	[0.193]	[0.188]	[0.189]	[0.189]
$Demobilisation_{t-2}$	0.152	0.157	0.128	0.000	0.005
	[0.228]	[0.229]	[0.225]	[0.188]	[0.189]
$Demobilisation_{t-3}$	0.280*	0.284*	0.262*	0.227	0.231*
	[0.145]	[0.146]	[0.143]	[0.140]	[0.140]
Year FE	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes
Dep. Var Mean	0.608	0.608	0.608	0.608	0.608
Observations	4690	4690	4690	4536	4536

Notes - Instead of municipality fixed effects the regressions control for paramilitary group fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Appendix

The following subsections provide additional background information and empirical evidence. The first subsection gives a more detailed overview of the complex historical and political background in Colombia. It thus enriches the information described in Section 2.2. The second subsection delineates the relevance of land in Colombia and its role throughout the conflict. The third subsection presents the regression tables and balance checks for the difference-in-differences estimation for the extended sample of municipalities (discussed in Section 2.6.1).

Civil conflict in Colombia

The civil war in Colombia was for a long time the longest-lasting civil conflict in the world.[57] Over the course of its history of more than fifty years, the conflict evolved numerous times (Figure 2.1). As a result, the patterns of violence varied over time. A broad set of different actors were involved throughout the conflict, most of them non-state armed actors. The beginning of the most recent phase of the civil war dates back to the period called "La Violencia" (the Violence), which approximately lasted from the late 1940s to the late 1950s. Despite the fact that this period of violence, causing the death of more than 200,000 people, was primarily caused by a partisan conflict between the dominant conservative and liberal political parties, it gave rise to the emergence of left-wing guerrilla groups. Among these groups were the two most important groups that recently were still in existence: the National Liberation Army (NLA) and the Revolutionary Armed Forces of Colombia (FARC). At first, rebels were mainly located in remote areas at the agricultural frontier. In those areas, they were widely supported as they took over the main functions and responsibilities of the state. With the sudden rise of coca cultivation and the accompanying lucrative drug trade the conflict changed and intensified notably. The gains from illegal crop cultivation and trade generated for the rebels' cause financial resources in unprecedented magnitude but also enticed new non-state actors into the conflict, namely paramilitaries and drug dealers.

Coca cartels emerged in the 1980s. Thereafter, in the late 1980s, a new set of actors, right-wing paramilitaries, were founded, originally as self-defence units to provide protection against guerrilla violence. The access to a large financial resources through illegal drugs aggravated the fight over land in some regions among those three main actors: (i) left-wing rebels; (ii) paramilitaries; and, (iii) drug cartels. Territorial control became the centre of the Colombian armed conflict in several ways: military disputes arose for territorial control, land was fought over in the need to establish corridors for transporting arms as well as illicit drugs, and land was used as war spoils.

As a result, thousands of hectares of land were either abandoned or illegally appropriated by armed groups. Using different mechanisms, some actors sought to formalise their dominance over appropriated land and gain property rights over it by taking advantage of their influence over local institutions. Such behaviour was

[57] In 2017, the government struck a peace deal with the FARC, effectively concluding the civil war.

presumably used as a mechanism to maintain economic and political power during times of potential changes in a post-conflict setting. This strategy was particularly employed by paramilitaries. Currently, as an attempt of establishing a transitory justice towards a post-conflict setting, the Colombian government has launched the "Law of Victims and Land Restitution". This law defines the conditions under which victims may initiate an administrative process for reclaiming their expropriated land. The poor definition of the original property rights and a lack of knowledge about the mechanisms whereby conflict affected the land market, however, have created an important hurdle for the application of such a policy.

Additional background information: land in Colombia

The distribution and definition of land property rights in Colombia have historical roots.[58] During Spanish colonial rule (from the 16th century until the early 19th century), land was assigned according to social or army status as a political asset.[59]. In a few years, the most productive and best located land was allocated to the most influential groups or families. It generated a semi-feudal structure based upon large plots called *Haciendas* (Machado, 2009; Ibáñez and Muñoz-Mora, 2010).[60] Until independence in 1810, authority over land property rights on private and public land was exclusively manifested in the hands of the Spanish Crown.[61].This also applied to all remaining land, which by definition equally belonged to the the Spanish realm (Machado, 2009). After independence, most colonial institutions remained in place, in particular, those related to the distribution and definition of land property rights, which perpetuated the Spanish institutional legacy (Machado, 2009; Ibáñez and Muñoz-Mora, 2010; LeGrand, 1988).

During this period, landless people used colonisation as an alternative to access land. Small peasants, freed slaves and entire communities ventured beyond the agricultural frontier to inhabit new land. This process and the abundance of unoccupied land kept under control the social tensions in Colombia during the nineteenth century (LeGrand, 1988). Several factors drove this seemingly spontaneous process, among which the conflicts between peasants and large landlords as well as the individual

[58]Ample of literature can be found on land issues in Colombia. Machado (2009) reviews the different land institutions and policies from the colonial period until the late twentieth century. Social confrontation and colonisation have been important topics in the works of LeGrand (1988), Oquist (1980), and Ibáñez and Muñoz-Mora (2010).

[59]For instance, in 1513, the Spanish introduced the concept of "morada y labor" (dwell and work), which defines that land should be assigned to the person who has dwelled and worked on the plot of land (LeGrand, 1988; Ibáñez and Muñoz-Mora, 2010). This could be considered as a first attempt of a land reform in Colombia. However, this reform was mainly to the advantage of large landlords who used political control to increase the total surface of land they owned (Machado, 2009).

[60]These territories were originally occupied by indigenous communities, who were evicted from their land or forced to work for the landlords who took over their land. Often, the appropriation of land took place under a colonial policy named *encomienda*, which gave certain groups the control over taxes and over the labour force of the indigenous communities but led to land appropriation (Guillén, 2008).

[61]The first land property rights provided by the Spanish crown were *capitulaciones* and *repartimientos* (Machado, 2009).

coloniser's motivation stand out.[62] Throughout Colombia the uncertainty over property rights and the absence of state institutions facilitated land seizures, increasing the number of disputes between large landowners and small-peasants (Ibáñez and Muñoz-Mora, 2010). These tensions became increasingly more coordinated and allowed local conflicts to develop into major social and regional ones (LeGrand, 1988, 1992).

The government of Colombia regarded land reforms as a potential solution to these growing social tensions (Machado, 2009).[63] Despite three attempts (in 1936, between 1958 and 1962, and between 1966 and 1970), land reforms largely failed in resolving the growing conflict. The power of large landowners wound up thwarting any attempt of reform (Ibáñez and Muñoz-Mora, 2010). The aforementioned conflict intensification in the 1980s further exacerbated struggles over land.

The strategies employed by armed groups to exert territorial control and accumulate land have led to the expulsion of millions of peasants from their lands. More than one million of people were displaced during 1995 and 2002 (Ibáñez and Muñoz-Mora, 2010). In order to seize their lands, armed groups have used several strategies, such as coercive transfers, fence-shifting to seize plots previously in the hands of people now displaced, the use of front men to keep plots from expropriation, and the acquisition of fore-closured plots, the owners of which failed to pay mortgages because they were displaced (Ibáñez and Muñoz-Mora, 2010).

After the deliberate displacement of its owners, abandoned land is being used in diverse ways. In some cases, it has been reassigned to peasants loyal to the dominant armed group in the regionReyes (2009). In others, the land has been appropriated by drug traffickers or by members of the armed groups. This is the case with the leaders of paramilitary groups, who have accumulated significant amounts of land (Reyes, 2009; Muñoz-Mora and Giraldo, 2011). Drug traffickers and paramilitary groups took advantage of the institutional void and the weakness of the state to guarantee the rights to buy lands in areas with a strong presence of the guerrilla, providing private security, raising property values, and profiting from the subsequent increases in land prices (Reyes, 2009; Muñoz-Mora and Giraldo, 2011).

[62]Nevertheless, some scholars have suggested that in some regions this process was actually organised by large landowners who took advantage of the lack of defined property rights to increase their land possession (LeGrand, 1988, 1992; Guillén, 2008).

[63]For a detailed land reform review in Colombia see Machado (2009).

Additional figures and tables

Table 2.22: Victimisation types: violence strategies

Dependent Variable: threats, sexual abuse, homicides

	(1)	(2)	(3)	(4)	(5)
			PANEL A: threats		
$Demobilisation_{t-3}$	-2.750	-2.711	-2.392	-2.293	-2.285
	[1.804]	[1.819]	[1.833]	[1.860]	[1.860]
			PANEL B: sexual abuse		
$Demobilisation_{t-3}$	-0.103	-0.099	-0.066	-0.077	-0.077
	[0.162]	[0.163]	[0.166]	[0.165]	[0.165]
			PANEL C: homicides		
$Demobilisation_{t-3}$	13.948	13.384	13.134	14.080	14.046
	[18.260]	[17.608]	[16.890]	[17.187]	[17.201]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes

Notes - All regressions control for municipality fixed effects. The table presents the difference-in-differences estimation for different types of violence that could have been used in the years preceding demobilisation in order to expropriate land. Panel A examines the effects on threats, Panel B on cases of sexual abuse, and Panel C on the number of homicides. All anticipation dummies are included in the regressions but for brevity only the ones on the three-year lag are reported. The other coefficients also do not report an increase in the respective type of violence. Robust standard error clustered at the municipality level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.23: Differences-in-differences for extended sample

Dependent Variable: number of expropriations

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	0.092	0.093	0.104	0.131	0.131
	[0.148]	[0.147]	[0.146]	[0.150]	[0.150]
$Demobilisation_{t-1}$	0.167	0.170	0.179	0.118	0.119
	[0.118]	[0.118]	[0.118]	[0.103]	[0.103]
$Demobilisation_{t-2}$	0.148	0.153	0.159	0.057	0.057
	[0.149]	[0.149]	[0.149]	[0.099]	[0.099]
$Demobilisation_{t-3}$	0.234*	0.240*	0.242*	0.238*	0.238*
	[0.139]	[0.139]	[0.139]	[0.142]	[0.142]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes
Dep. Var Mean	0.608	0.608	0.608	0.608	0.608
F-test (p-value)	0.070	0.064	0.052	0.084	0.083
Observations	8750	8750	8750	8344	8344

Notes - All regressions control for municipality fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.24: Controlling for competing events in 2001, 2002 or 2003

Dependent Variable: number of expropriations

	(1)	(2)	(3)	(4)	(5)
$Demobilisation_{t0}$	0.074	0.076	0.086	0.108	0.109
	[0.150]	[0.150]	[0.149]	[0.153]	[0.153]
$Demobilisation_{t-1}$	0.144	0.148	0.159	0.080	0.080
	[0.135]	[0.135]	[0.134]	[0.109]	[0.109]
$Demobilisation_{t-2}$	0.132	0.137	0.149	0.007	0.007
	[0.201]	[0.201]	[0.200]	[0.124]	[0.124]
$Demobilisation_{t-3}$	0.270*	0.275*	0.285**	0.243*	0.243*
	[0.145]	[0.145]	[0.144]	[0.133]	[0.133]
$ParamilitaryPresence * 2001$	-0.177	-0.171	-0.185	-0.178	-0.178
	[0.122]	[0.121]	[0.122]	[0.128]	[0.128]
$ParamilitaryPresence * 2002$	-0.194	-0.192	-0.206	-0.156	-0.156
	[0.169]	[0.169]	[0.168]	[0.161]	[0.161]
$ParamilitaryPresence * 2003$	0.113	0.111	0.098	0.203	0.204
	[0.212]	[0.212]	[0.211]	[0.182]	[0.182]
Year FE	Yes	Yes	Yes	Yes	Yes
Soil Quality-Specific trend	No	Yes	Yes	Yes	Yes
Geography-Specific trend	No	No	Yes	Yes	Yes
Climate-Specific trend	No	No	No	Yes	Yes
Socioeconomic variables	No	No	No	No	Yes
Dep. Var Mean	0.608	0.608	0.608	0.608	0.608
F-test (p-value)	0.177	0.165	0.134	0.223	0.221
Observations	8750	8750	8750	8344	8344

Notes - All regressions control for municipality fixed effects. Robust standard error clustered at the municipality level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.25: Mean tests for municipalities with at least one plot expropriated, by control of paramilitaries groups

	Comparison Group	Treatment Group	Difference
Area (ha)	36863.448	51695.224	-1.48e+04***
	(290)	(335)	[4368.011]
% Fertile Soil	54.517	51.580	2.936
	(290)	(335)	[2.829]
Average precipitation from 1961-1980	21.364	21.753	-0.389
	(290)	(335)	[0.390]
Altitude	1107.383	1052.337	55.045
	(290)	(335)	[66.346]
Distance to State Capital (km)	75.086	82.367	-7.281
	(290)	(335)	[4.552]
Average population	28155.303	57091.199	-2.89e+04
	(290)	(335)	[21469.188]
Land Gini, 1985	70.443	70.584	-0.140
	(272)	(324)	[0.755]
Local voting share for Alvaro Uribe	67.308	70.664	-3.356**
	(290)	(335)	[1.277]
Dummy for Coca Cultivation	0.021	0.015	0.006
	(290)	(335)	[0.011]
Preferences for the Right	44.076	43.054	1.022
	(259)	(315)	[2.321]
Distance to Coastline (km)	220.017	254.932	-34.915**
	(290)	(335)	[10.600]
Share of Area in Mining	0.095	0.103	-0.007
	(290)	(335)	[0.011]

Notes - Two-sided mean t-tests reported. The treatment group encompasses municipalities under paramilitary control. The treatment group consists of municipalities without permanent paramilitary presence. Standard errors of the difference between the two groups are in brackets, the number of observations for each variable are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: CEDE (2016) and Colombian Government (2016).

Table 2.26: Mean test in terms of historical grievances for municipalities with at least one plot expropriated, by control of paramilitaries groups

	Comparison Group	Treatment Group	Difference
Territorial conflicts 1901 - 1931	0.041	0.063	-0.021
	(290)	(335)	[0.018]
Territorial conflicts 1901 - 1917	0.090	0.096	-0.006
	(290)	(335)	[0.023]
Territorial conflicts 1918 - 1931	0.114	0.158	-0.044
	(290)	(335)	[0.028]
Presence of indigenous population 1535 - 1540	0.428	0.421	0.007
	(290)	(335)	[0.040]
Violence between 1948 and 1953	0.152	0.152	-0.001
	(290)	(335)	[0.029]

Notes - Two-sided mean t-tests reported. The treatment group encompasses municipalities under paramilitary control. The treatment group consists of municipalities without permanent paramilitary presence. Standard errors of the difference between the two groups are in brackets, the number of observations for each variable are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: CEDE (2016) and Colombian Government (2016).

Chapter 3

Democracy, Government Size, and Redistribution

3.1 Introduction

In the last forty-five years, the world has seen an almost unprecedented amount of political changes and regime transitions. The emergence of independent states that were previously colonised and subsequently democratised, the international decline of communism, the fall of military dictatorships, and the collapse of other autocratic regimes all have contributed to a host of democratisations that, according to Polity IV, has more than doubled the number of democracies in the world since 1970 (see Figure 3.1). More recently, a surge in *democratic reversals* has grasped the attention of scholars and policy makers alike, as countries in most regions of the world experienced a return to autocratic rule and authoritarian institutions.

The scale of such political transitions is not only notable in its own right, but these transitions may also have far-reaching economic and social consequences. The question whether some political systems are economically and socially optimal has undergone much scrutiny by both political scientists as well as economists. A natural starting point of such an analysis on differential economic or social outcomes is provided by the study on the effects of transitions that entail a regime change. In the economics literature, the main focus has been set on the examination of democratisations' impact on economic growth. The overwhelming majority of recent work suggests that democratisation causes increased economic growth in previously

autocratic countries (Acemoglu et al., Forthcoming; Giavazzi and Tabellini, 2005; Papaioannou and Siourounis, 2008; Persson and Tabellini, 2009).[1]

One direct channel through which governments may influence economic development and determine social outcomes is public finance, i.e. public spending and revenue. The total size of public revenue or expenditure is commonly referred to as government size. Similar to the number of democracies, the growth of government size has been rapid in the post World War II era (Lindert, 1994). The absolute size of government may matter for a variety of reasons: depending on its composition and timing it can be conducive or obstructive to economic growth (Afonso and Furceri, 2010), it can affect life satisfaction (Bjørnskov et al., 2007), and, importantly, it serves redistributive objectives through social welfare spending or taxation (Meltzer and Richard, 1981). In the last few years, reported evidence on rising inequality in democracies in the developed world has been widely discussed in the public discourse (Piketty, 2014). Therefore, it has been debated whether democratic governments can or even should pursue more redistributive policies, which also raises the question whether democratisation does actually lead to increased redistribution in the first place.

In political economy, a body of theoretical work exists that tries to connect the economic importance of both democratisation and government size, emphasising the role of democracy for public expenditure and taxation. Nevertheless, there is no consensus whether democratisation in fact causes an increase in government size. In particular, the empirical literature on this question is largely inconclusive. Common to a lot of the previous empirical studies are four points: First, they either employ a continuous democratisation measure or only account for discrete measures distinguishing democracies and non-democracies, which fails to take into account the causes and consequences of democratisation. Second, they place little importance on country-specific time-invariant characteristics as well as heterogeneity among individual countries and country-groups in the effects of democratisation. Third,

[1]These studies use innovative approaches to overcome previous identification challenges and potential reverse causality by employing instrumental variable regressions (Persson and Tabellini, 2009; Acemoglu et al., Forthcoming) or difference-in-differences estimation (Giavazzi and Tabellini, 2005; Papaioannou and Siourounis, 2008).

most existing studies elicit correlations rather than causal evidence.[2] Fourth, most studies look at measures of government size and equate them with redistribution, without considering whether they actually serve a redistributive purpose.

We propose a different approach to testing the relationship between democratisation and redistribution, both in the form of taxes and public spending. We use a global panel data set from 1972 to 2013 for disaggregated government finance data that captures variation between countries as well as variation over time. We suggest that the latter, within-country time series variation, is in fact decisive for measuring the effects of democratisation on public revenue and expenditure. Furthermore, we probe the importance of country-specific fixed effects with a set of different regression specifications. We also explore to what degree democratisation might alter not only total size but also the composition of tax revenue and government expenditure. Our work highlights a potential weakness in previous analyses that focused on aggregate measures of government size such as total government revenue. Our findings indicate that such measures might not be suitable measures for capturing redistribution. Instead, categories of public expenditure or taxes with a strong redistributive or progressive nature, respectively, need to be considered. Therefore, we examine the impact of democratisation on both aggregate measures of government size and on redistributive spending and taxation.

Another contribution we make consists of providing novel causal evidence on the impact of democratisation on redistribution, which has so far been missing at a global scale as such evidence has been limited to Europe or the United States. We use an instrumental variable estimation approach that addresses potential endogeneity problems. In addition to these points, we contribute to the literature in a variety of other ways. Methodologically, we present evidence from applying difference-in-differences and instrumental variable estimation, approaches rarely used in the literature. Conceptually, we highlight the significance of heterogeneity in the effects of democratisation and present evidence that different forms of democratisation processes increase or reduce redistribution. Relatedly, we show that the actual design

[2]Notable exceptions are Aidt and Jensen (2013) and especially Albertus and Menaldo (2014). We will discuss the work of Albertus and Menaldo (2014) in detail in Section 3.2.

and implementation of democratic principles in newly democratised countries matter for the extent to which fiscal redistribution will increase or not.

The remainder of this chapter is structured as follows. First, we provide a selective literature review for the purposes of our analysis in Section 3.2. We then discuss the sources and definitions of our chosen data and provide descriptive statistics for our main variables in Section 3.3. In the two following Sections 3.4 and 3.5, we first explain our empirical framework before we present and discuss our findings. Finally, we conclude in Section 3.6 by reflecting on our results, our contribution to the literature, and future areas of research.

3.2 Democracy in the literature

The emergence, the causes, and the consequences of democracy have been studied extensively in economics and other social sciences. The following survey of the literature on democracy draws on selected contributions that are most closely related to or relevant for our work.

Common to most explanations for changes in government size in the literature, is the attention and significance they attribute to political, economic, or demographic factors. The impact of these factors on the nature of government expenditure and taxation has been assessed and discussed in numerous examples of both theoretical and empirical research, with some studies attempting to reconcile theoretical and empirical concepts simultaneously. For the purpose of our analysis, we structure the survey into two core topics: (i) political institutions and democracy as determinants of government expenditure and taxation and (ii) economic and demographic factors that might influence public spending and revenue.[3]

[3]The primary objective of the literature review is to summarise vital stylised facts concerning these topics and to introduce the reader to the methodological angles from which previous work tried to embrace the underlying identification challenges. We refer to other strands of the literature on democracy if they are relevant for the work of this chapter.

3.2.1 Democracy, political institutions, and the size of government

The seminal study that initiated the literature on democracy as a vital determinant of government size was made by Meltzer and Richard (1981). Based on a voting model in which, under majority rule, the final outcome is decided by the median voter, they highlight that democratisation in the form of voting rights and political access to wider groups of society may induce a greater government size. In their model, this development is illustrated by extensions of the voting franchise, which change the specific position of the decisive median voter within the income distribution. Building on empirical evidence that income distributions are skewed, a voting franchise extension lowers the ratio of the median voter income to the mean income of the electorate. This should result in greater demand for redistribution and is therefore cited as the principal reason for the growth of government size. The model prediction has subsequently become known as *franchise extension hypothesis*.

Support for the franchise extension hypothesis is provided by several studies such as Husted and Kenny (1997) or Lott and Kenny (1999) that address the hypothesis by examining the effects of suffrage. Husted and Kenny (1997) find that a large increase in government expenditure on social welfare across 46 of the United States between 1950 and 1988 is associated with the removal of poll taxes and literacy tests that enfranchised poor voters. In apparent accordance, Lott and Kenny (1999) show that 20 % of public expenditure growth in the United States between 1870 and 1940 can be explained by greater election participation due to women's suffrage. In another analysis of the historical development of the public sector, Aidt and Jensen (2013) scrutinise both revenue and spending data. Using country fixed effects regressions and, alternatively, instrumental variable estimation for a sample of Western European countries throughout *the long 19th century* (1820 - 1913), they find robust empirical support for the franchise extension hypothesis with regards to the size of government expenditure per capita but no effect on public revenue. Further qualitative evidence on the franchise extension hypothesis is given by Lindert (2004), who elaborates how franchise extension in European democracies during the 19th

and 20th centuries induced demand for public education and thus higher spending. More recently, the degree of political rights has been found to be correlated with government expenditure on social protection (Shelton, 2007).

The political economy literature has also drawn attention to the particular design of political institutions in democracies as a determinant of size and composition of government expenditure and revenue. For instance, the distinction between majoritarian or proportional representation electoral rules on the one hand, and between presidential and parliamentary systems on the other, can have a significant impact on redistribution through social welfare programs as well as on public goods provision (Persson and Tabellini, 2002).[4] A number of studies demonstrate theoretically and empirically that proportional representation results in greater redistribution.[5] Yet another possible dimension of institutional design lies in the difference between centralised or federal government systems (Dincecco, 2009).

The question whether we see altogether larger government size in democracies relative to non-democracies has also been probed in the literature. Political regimes may have direct and, under particular circumstances, indirect effects on government size. For instance, democratic regimes can be associated with larger governments if they coincide with a sufficiently high level of economic development (Boix, 2001).[6][7] Most of the empirical work in this literature has been made on the basis of country cross-sectional comparisons, historical time series or panel-data analyses limited to

[4]Similar findings are provided by Persson et al. (2000) who apply a game-theoretic micro-political model that contrasts different political regimes and posits increased political competition between both voters and politicians. They predict less redistribution and lower public goods provision in presidential regimes and find strong empirical support for their hypothesis in a cross-country comparison of average central government expenditure in 54 countries from 1988 to 1992.

[5]Austen-Smith (2000) develops a political economy model that explains why average tax rates are higher and distributions of post-tax income more equal in countries with proportional representation. Milesi-Ferretti et al. (2002) come to the same prediction as Persson and Tabellini (2002) that countries under proportional representation will tend to have larger redistribution through transfers. However, with regards to public goods provision they come to a different conclusion, postulating that majoritarian systems will induce higher spending compared to proportional representation.

[6]Boix (2001) uses real GDP per capita to measure level of development and the presence of contested elections in previous years to distinguish between democracy and non-democracy.

[7]Directly comparing public policies in democracies and non-democracies, Mulligan et al. (2004) do not find significant differences with regard to fiscal policies. However, this may just give further weight to the argument of Boix (2001) that indirect effects of democracy are only apparent through their interaction with other factors.

only a few countries. Recently however, some studies have used panel-data estimation on disaggregated expenditure or tax revenue data for a larger number of countries (Shelton, 2007; Profeta et al., 2013).[8]

The most promising studies to examine the effect of democracy on redistribution, and the studies most closely related to our work, are Acemoglu et al. (2015) and Albertus and Menaldo (2014). Both studies use a global country panel with sufficiently large time coverage to include country fixed effects, mitigating a major caveat of previous work. Estimating dynamic panel models that aim to account for persistence in dependent variables, Acemoglu et al. (2015) find a strong positive relationship between democracy and total tax revenues as well as total government revenue, which they take for evidence of increased redistribution in democracies. However, in their estimation such increased redistribution does not translate into changes in inequality. Among potential explanations, they argue that a middle class bias in public finance can inhibit redistribution, leaving inequality effectively unchanged. Albertus and Menaldo (2014) use more nuanced measures of redistribution and employ instrumental variable estimations to show that redistribution is only increased under certain circumstances. They argue that democratisation only causes an increase in redistribution if elites of the old regime are relatively weak and thus unable to steer government policies in democracy.

Our work complements and refines these studies in a number of ways. First, we look at both total expenditure and revenue and their more redistributive subcomponents, which allows us to distinguish between government size and redistribution. Our findings point out that this distinction matters in assessing the impact of democratisation. Second, we provide novel and robust causal evidence on the effects of democracy, while also accounting for the strong persistence of fiscal policies, which allows us to consider the long-run effects of democracy.[9] Methodologically and empirically, we use the most reliable and precise data available and analyse it

[8]Nevertheless, those studies do not aim to elicit any causal link between democracy and redistribution. Furthermore, their analyses are limited by short time periods and small sample sizes.

[9]Albertus and Menaldo (2014) do not account for such persistence and do not study total government size. Acemoglu et al. (2015) in contrast, only examine crude government size measures as proxies for redistribution and do not address the potential endogeneity of democracy.

with different robust empirical strategies. In the Appendix, we lay out further how and why our work deviates from Acemoglu et al. (2015).

Despite these numerous studies on the relationship of democracy and government size, little attention has been given to the potential endogeneity of democratisations. Embodied by the famous slogan "no taxation without representation" of American colonists prior to the American revolution, several historians and sociologists have argued that taxation was historically followed by an emergence of more democratic institutions and representation. In his seminal work, Tilly (1975) makes the theoretical argument that taxation leads to the formation of Western nation states, which is accompanied with an extension of the voting franchise. In a simple cross-country regression framework, Ross (2004) finds some suggestive evidence that taxation increases representation.[10] While this argument appears to be mostly fitting in the context of historical democratisation processes of Western nations, we attempt to address the endogeneity concern by complementing our baseline analysis with instrumental variable estimation discussed in Sections 3.4.2 and 3.5.2. Furthermore, we scrutinise this endogeneity argument by examining the effects of democracy in situations where a government or regime is less reliant on taxes due to sudden and exogenous variation in large resource revenue (Section 3.4.1).

3.2.2 Economic, demographic, and social determinants of government size

A number of economic, demographic, and social factors compete with democracy as an explanation for the growth of government size. The most famous economic explanation is derived from the work of Wagner (1883) who postulated, in what is today known as *Wagner's Law*, that more economically developed societies experience greater demand for the provision of public services and goods, resulting in an overall expansion of the size of government. His explanation for the phenomenon is twofold.

[10]More precisely, Ross (2004) finds no effect of the tax burden (taxes/GDP) on democracy. Instead, he finds that high taxes have a positive effect on democracy if they concur with relatively little or inadequate provision of government services. In interpreting these results, he argues that citizens follow a cost-benefit analysis. They only demand greater representation for being taxed if the burden of paying taxes is unreasonably high given the amount a government spends on services and public goods.

First, income elasticity of demand for public services is greater than 1, caused by the fact that several goods and services funded by the public sector can be considered luxury goods. Second, he argues that wealthier societies are more complex because of their specialised economies, and consequently require more public regulation and intervention. *Wagner's Law* is frequently also referred to as the *modernisation hypothesis*.[11] Besides economic development, openness to trade has been pointed out to be related to the size of government (Cameron, 1978). Rodrik (1998) establishes that the correlation between openness to trade and the size of government holds for all different country income categories and all aspects of government consumption.[12]

Most of the more recent literature on the determinants of government expenditure and taxation emphasises the importance of demographics. Demographic factors are assumed to potentially codetermine the absolute size, as well as the relative composition, of public expenditure and tax revenue. In many countries, healthcare and public pensions are large components of total government expenditure. As a consequence, the share of elderly people in the population is an obvious, if not the dominant, driver of these items of public expenditure. Some studies additionally include the population share of young people, as this may bear on the composition of government expenditure with regards to education and social protection. Moreover, the size of a country in terms of its total population may also be relevant.[13] Another demographic factor to which the literature attributes significance is the degree of fractionalisation of a society along religious, ethnic, and linguistic lines. For instance, fractionalisation is found to be negatively correlated with expenditure on social welfare or public goods (Easterly and Levine, 1997; Alesina et al., 1999).

In economic theory, but also in empirical work, the concept of inequality is often considered as an important driver for redistributive public policies. It is at the

[11] A number of papers have tested the validity of Wagner's Law focusing on various indicators for economic development such as urbanisation, rising incomes, or educational levels and come to different conclusions (Shelton, 2007; Boix, 2001).

[12] Rodrik (1998) postulates that increased openness results in greater exposure to economic volatility induced by the international business cycle. Therefore, more government expenditure can be viewed as an insurance mechanism that aims to compensate for the additional risk associated with greater inter-connection with the global economy.

[13] Alesina and Wacziarg (1998) find that smaller countries have a larger share of public consumption in GDP.

heart of the mechanism that predicts redistribution in voting models that apply the median voter theorem, e.g. in Meltzer and Richard (1981) where an increasing ratio of the mean to median income induces higher taxes and larger government spending to satisfy demands for more redistribution. Acemoglu and Robinson (2006) highlight the importance that inequality may have in determining public policies. As pointed out by Boix (2003), inequality is at the heart of several political economy models of government size and should therefore, as an interaction with democracy, be included in government size analyses. In the most elaborate study to date on the impact of income inequality on public policies, Milanovic (2000) tests empirically the hypothesis that higher income inequality causes more taxation or social security. For a set of 24 democratic countries included in the Luxembourg Income Study, he compares the differences between factor income (pre-tax, pre-transfer income) with disposable income for the different deciles of the distribution of households in terms of factor income. Using fixed effects regressions, he concludes that higher inequality is strongly associated with more redistribution. However, his findings question whether the median voter (the fifth and sixth decile of the income distribution) is actually able to benefit from such redistribution, casting doubts on the presumption that median voter redistributive models can describe the data suitably.[14]

In our analysis, we incorporate the aforementioned alternative explanations for changes in government size and redistribution. We either control for factors that are thought to affect taxes or expenditure (e.g. openness to trade, level of development, or income per capita) or we investigate how such factors can have an impact in combination with democratisation. For instance, we will examine how democracy's effects on redistribution differ in societies that are ethnically fractionalised (3.5.3). As such, we believe that economic, social, and demographic factors complement rather than compete with democracy in explaining redistribution.

[14]In considering inequality a channel through which democracy has an effect, Tavares and Wacziarg (2001) argue that "democratic institutions are responsive to the demands of the poor by expanding access to education and lowering income inequality", effectively altering the nature of the expenditure and revenue sides of the public sector in terms of size and composition.

3.3 Data and definitions

In order to examine the relationship between democracy and taxes as well as expenditure, we construct a global panel dataset consisting of all countries for which detailed government finance data are available. The sample is based on annual observations from 1972 to 2013 and includes more than 150 countries, including advanced, emerging, and developing economies. Data from various sources are put together for the purpose of our analysis. Our main outcome variables are compiled from the IMF Government Finance Statistics (GFS) database. The time period from 1972 to 2013 is entirely dictated by the lack of detailed data on public expenditure and revenue for years prior to 1972[15], rather than chosen with potential results in mind. However, due to the considerable number of countries that underwent democratic transitions throughout those 40 years, the time period is relevant for the questions about the public policy effects of democratisation that this chapter aims to answer.

3.3.1 Government finance statistics

The IMF Government Finance Statistics (GFS) database is one of the international workhorses for modern comparative public finance analysis. It provides the most detailed and thorough source of public finance measures, based on a consistent accounting methodology that ensures better inter-temporal and cross-country comparability than other datasets. GFS is the primary source for the dependent variables in this chapter. As a result of its detailed decomposition of government expenditure and revenue, GFS allows examining not only the total size of the public sector but also specific categories of expenditure or revenue that are likely to better capture redistribution than mere aggregate figures for expenditure or revenue. In common with the literature, all dependent variables in this chapter are expressed in shares of GDP.

Public revenue is divided into four major categories in the GFS: tax revenue, social contributions, grants, and other revenue. For the purpose of our analysis, we

[15]Similarly, data availability of polity2 scores from the Polity IV initiative determines the final year of our sample as 2013.

will focus on total government revenue as well as on tax revenue. More precisely, we collect data on total government revenue and total tax revenue as natural measures for government size on the public revenue side. Additionally, we use information on revenue from different forms of taxation. We use taxes on income, profits, and capital gains (henceforth referred to as *income taxes*) as a measure of fiscal redistribution since such taxes can be highly progressive, with higher tax burdens for more affluent or high-income individuals.[16]

The choice of these indicators is based on the variables' causal interpretation concerning their distributive impact and their relevance to redistributive political economy theories of democracy. While other components such as rents from natural resources may, in some cases, contribute substantially to total public revenue, their origin and essence makes them less suitable in an analytical inquiry of the public finance effects of democratisation.[17] Although such types of revenue may have a political element, it is difficult to imagine them as policy tools, the outcome of which can be seen as the result of a political process. In contrast, tax revenue seems to be the natural choice for policy tools to generate government revenue.

On the government expenditure side, we use data based on the functional approach (classification of functions of government, COFOG), which allocates government expenditure to functional categories such as general public services, healthcare, education, defence etc.[18] Concentrating on the functional classification facilitates interpretation and understanding of the data. Similar to government revenue, we focus on total expenditure and a subcomponent, the sum of expenditure on education, health, and housing.

[16]Other tax categories include taxes on goods and services, international trade and transactions, and property. Only taxes on property are of similar progressive nature as income taxes. However, the underlying data suffer from missing observations for many developing countries. Therefore, we abstain from using it in this study.

[17]We probe the robustness of our revenue regression results when controlling for resource rents in Section 3.5.1. Its inclusion does not change any results qualitatively.

[18]GFS also contains a second approach that assigns public expenditure to economic categories. In this study, we focus on the functional classification as this provides a more meaningful disaggregation of total expenditure into categories that are potentially associated with democratisation. A complete explanation of the expenditure classification methods into economic and functional categories can be found in the GFS Manual 2001 International Monetary Fund (2001).

In total, this leaves us with five indicators, all of which are expressed as GDP shares: total government revenue, total tax revenue, total government expenditure, taxes on income, profits, and capital gains (*income taxes*), and expenditure on education, health, and housing (*social spending*).[19] There are a number of reasons for focussing on these indicators. First, aggregate measures such as total taxes, revenue or expenditure, have been of greatest relevance to the majority of work in the literature on government size. Second, better and more consistent data availability now facilitate cross-country comparisons for more detailed expenditure and tax categories. Third, income taxes and social spending are more informative and revealing about redistributive policies than aggregate figures. As a consequence, we henceforth make the following distinction. On the one hand, we consider total revenue, total tax revenue, and total expenditure as measures of government size. On the other hand, we treat income taxes and social spending as measures of redistribution, which is at the core of most political economy models on the consequences of democratisation. Appendix 3.6 describes in detail how we construct these five measures and ensure temporal as well as cross-country comparability.

3.3.2 Democracy and democratisation

In empirical political economy studies, the revised polity2 score from the Polity IV project is the most widely used concept in assessing the democratic nature of a country's institutions. The polity2 score is a measure that describes countries along criteria such as political competition, voting access and rules, constraints on executive authority, and characteristics of executive recruitment. An advantage of the Polity IV project is that it accounts for improvements that establish or improve existing democratic institutions and rights, which might not show up in dichotomous regime type indicators if they did not entail a regime change. The Polity IV database

[19] We gratefully acknowledge the generous sharing of data by Albertus and Menaldo (2014), whose data on income taxes and social spending for 1972 to 2008 we use. In cases where the historical GFS database, the online GFS database, and the Global Development Network Growth Database do not provide information on disaggregated expenditure and taxes, we use their observations, which were additionally assembled from GFS yearbooks etc. We extend the data with more recent observations that we extract from the IMF GFS to increase the time dimension of the panel up to 2013. Section 3.6 in the Appendix provides further information on the shares of observations for income taxes and social spending that were used from Albertus and Menaldo (2014).

also provides a democratic transition variable that is based on the polity2 score. Finally, the polity2 score exceeds most other democracy measures in terms of country and time-period coverage.

The score consists of two sub-scores, one score capturing democratic features and the other autocratic features of a regime, both ranging from 0 to 10, higher numbers indicating more democratic or more autocratic characteristics.[20] The polity2 score is thus a composite measure of these two distinct authority concepts and is computed by subtracting the autocracy score from the democracy score.[21] The resulting scores range from 10 to -10. Building on this range, we classify democracies as countries that record a polity2 score strictly above 0, and autocracies as those that have a polity2 score in the range of -10 to 0. This bisection rule has been widely used in the literature, for instance in Brückner et al. (2012), Jones and Olken (2009) or Persson et al. (2007). The main benefit of this particular threshold is the large amount of associated significant institutional changes that follow once that score number is surpassed (Giavazzi and Tabellini, 2005). Table 3.1 gives an overview of democratisations in our sample.[22]

3.3.3 Other variables

Demographic data are taken from the World Bank World Development Indicators (WDI) - share of population over the age of 65, percentage of people younger than 15, and share of the population living in urban areas. Fractionalisation data along religious, ethnic, and linguistic lines come from Alesina et al. (2003). A description of their measurement methodology is given in their data appendix. It is worthwhile to note that the fractionalisation data are constant over time for each country. Following the literature, we define openness to trade as the sum of exports and

[20] An explanation is given in the Polity IV user manual (Marshall et al., 2011):"In an attempt to facilitate empirical analysis of these and other historical trends, Polity IV includes constructed annual measures for both institutionalised democracy (DEMOC) and autocracy (AUTOC), as many polities exhibit mixed qualities of both of these distinct authority patterns."

[21] The polity2 score deviates from this simple aggregation rule for special situations such as cases of foreign intervention or cases of anarchy. Please see the Polity IV user manual for more details on the coding.

[22] Table 3.24 in the Appendix offers the complementary overview of democratic reversals in our sample.

imports over GDP. Income per capita is approximated by GDP per capita in constant 2010 US$. Openness to trade, current GDP in national currency, and income per capita are derived from the World Bank World Development Indicators. All public finance data, which are mostly documented in national currencies, are normalised into GDP shares by dividing it by current GDP in national currencies. We also make use of the WDI for estimating revenue from natural resources.[23]

Data on wars and civil conflicts come from the PRIO armed conflict database first described in Gleditsch et al. (2002). The dataset documents armed conflicts, both internal and external, since 1946. We make use of it in such a way that a dummy variable indicates a country to be involved in a war or civil conflict if it is listed among the primary parties on either side of a conflict in which at least 25 battle-related deaths were documented in a given year. For variables on educational attainment, we use data from Barro and Lee (2013) who constructed long lasting time series for most countries in the world. Their data are documented in five year averages. We interpolate these observations linearly to derive annual data. Data on political institutions and competitive elections are derived from Cruz and Scartascini (2016) who update the work of Beck et al. (2001). Finally, characteristics of national leaders and their entry to as well as exit from power are constructed from information in the Archigos database made available by Goemans et al. (2009).

3.3.4 Descriptive statistics

A number of studies have documented the rapid rise of the relative size of government activity over the 19th century up to the post World War II era (Lindert, 2004; Aidt and Jensen, 2013). Our analysis examines a later time period. It starts in the 1970s, around the time of the first oil crisis and concurrent with an increase in public debt ratios in most advanced economies. Figures 3.2 and 3.3 illustrate the evolution of average total expenditure and total tax revenue relative to the country GDP over this period. Both average expenditure and tax revenue have grown between 1972 and 2013 relative to the size of the respective economy. This growth does not seem

[23]We calculate the GNI share of the sum of revenue from carbon, energy, minerals, and deforestation.

to be driven by significant entry or exit of countries over the sample period, as the number of countries with available fiscal data is relatively stable or only changes gradually after 1973 (Table 3.25 in the Appendix). While the average amount of government expenditure in 1972 was equivalent to around 21% of GDP, it surpassed 27 % of GDP in 2013 (Figure 3.2). Similarly, tax revenue outpaced the growth of the economy, increasing from 17% to more than 20% of GDP (Figure 3.3). The evolution of these two measures, however, did not simply follow a continuous trend. After rising significantly in the 1970s and early 1980s, their growth abated and taxes and expenditure remained largely stable or even decreased in the 1990s. Subsequently, they resumed to grow in the 2000s. Interestingly, the different phases of growth in either measure does not seem to correspond (or follow) temporally the pattern of democratisations. The proportion of democracies, though rising throughout our period of analysis, experienced the largest jump with the fall of communism in the late 1980s and early 1990s. Neither average tax nor expenditure GDP shares display significant growth following this large democratisation wave, which begs the following question: Did this particular wave of democratisation really result in an increase in the size of government? We will return to this question in the discussion of our empirical framework and results.

Table 3.2 gives an overview of the basic summary statistics of our data. The first five rows describe the dependent variables for the 168 countries in our sample. Total revenue and expenditure are, on average, of similar magnitude accounting for 24% and 26% of GDP, respectively. Tax revenue, a component of total revenue, averages around 19% of GDP, out of which 7% is generated by taxes on income, capital gains, and profits. Expenditure on education, health, and housing accounts for more than a quarter of total government expenditure, with an average size of 6% of GDP. What stands out in Table 3.2 is that all these measures have a high variance probably due to significant country differences. Around two-thirds of country-year observations are in democracies. We observe 97 regime changes. 63 democratisations occur throughout this period. In the other direction, 34 democracies experience democratic reversals back towards autocracy.

On average, democracies are characterised by larger government size and more redistribution than autocracies (Table 3.3). In particular, there is a large difference in total tax revenue and income tax revenue, which are on average around 50% higher in democracies. Total expenditure and social spending relative to GDP are more comparable in democracies and autocracies, only being 10% lower in autocracies. Unsurprisingly, democracies and autocracies also differ in other characteristics. Democracies are on average richer, their citizens more educated, and their populations are older and more urbanised. Conflict episodes are more frequent in autocracies. Economically, democracies and autocracies in our sample appear similarly open to trade, as measured in terms of the sum of exports and imports relative to GDP.

While these differences are only descriptive, they offer a first glimpse of systematic differences between democracies and autocracies not only in terms of our dependent variables but also with regards to other economic, demographic and socioeconomic factors.[24]

3.4 Empirical framework and identification

Our empirical strategy for estimating the effects of democracy on redistribution is twofold. Firstly, we use different panel regression specifications that account for country fixed effects and control for a number of potentially confounding covariates (Section 3.4.1). Secondly, we employ instrumental variable (IV) estimation to alleviate concerns about a potential endogeneity of democratisation that could bias our fixed effects estimates. We elaborate our IV strategy below in Section 3.4.2.

3.4.1 Fixed effects estimation

Following the example of Papaioannou and Siourounis (2008), we construct an indicator for democratisation and, more generally, regime changes for all country year observations (as described in Section 3.3.2). This allows us to create a dichotomous variable for democracy, $Democracy_{it}$, which is equal to 1 in country i at time t

[24] In our regression analysis, we will account for some of those differences to rule out that they undermine our empirical strategy.

if the country is a democracy and 0 otherwise. As such $Democracy_{it}$ is equal to one in democratised former autocracies or in countries that always were democratic. Analogously, $Democracy_{it}$ is equal to zero in the years following a democratic reversal in previously democratic countries as well as in countries that stay autocratic throughout our sample period.

As such our baseline specification is as follows,

$$y_{it} = \alpha + \beta Democracy_{it} + \lambda X_{it-1} + \rho y_{it-1} + \nu_i + \mu_t + \varepsilon_{it} \tag{3.1}$$

where the dependent variable y_{it} represents GDP shares of total government expenditure, total government revenue, total tax revenue on the one hand, and social expenditure and income taxes on the other. Our main interest lies in β, the coefficient that indicates whether a country is a democracy or not. Equation 3.1 includes country fixed effects, ν_i, and year fixed effects, μ_t. The former account for any time-invariant country-specific characteristics that could affect y_{it}. Note that, given that we include country fixed effects, β is identified from within-country time series variation in $Democracy_{it}$, i.e. from democratisations (switch from 0 to 1) and autocratic reversals (switch from 1 to 0). The full set of country fixed effects allows us to control for any fixed characteristics that systematically differ between democracies and autocracies and potentially affect y_{it}.[25] The latter, year fixed effects, control for any year specific unobserved factors as well as temporal patterns in our dependent variables. We allow the error term, ε_{it}, to exhibit country-level clustered autocorrelation and heteroskedasticity, in order to take into account that serial correlation of the error terms ε_{it} could cause a downward bias of standard errors of fixed effects estimators (Bertrand et al., 2004). We also account for the autoregressive dynamics and persistence of y_{it} in explaining contemporaneous values of the dependent variable, which is captured by the coefficient ρ. In some specifications, we omit y_{it-1} to examine the static version of Equation 3.1.

[25] More generally, in our setting with country being the cross-sectional unit, it is likely that unobserved time-invariant country-specific factors are correlated with our set of explanatory and control variables $\text{Cov}(X_{it-1}, \nu_i) \neq 0$. We employ country fixed effects estimation for that particular reason because it is capable to accommodate such non-zero correlation.

X_{it-1} denotes a number of covariates for which we control, including several variables that are mentioned in the literature as potential drivers of y_{it} such as economic (openness to trade), socio-economic (educational attainment as measured by population with secondary or tertiary education), or demographic factors (population share of the elderly and young, respectively). Furthermore, we control for whether a country was involved in a war or civil conflict in year $t-1$. All these variables are lagged by one period to account for the fact that their predetermined values could affect democracy in period t. In all our specifications, we control for lagged GDP per capita for two reasons. First, different levels of development could affect political institutions and democratisation.[26] Second, economic prosperity could directly change redistribution in a country (see Section 3.2 for a discussion of *Wagner's Law*) or indirectly if democracy influences economic growth, which then affects y_{it} (Papaioannou and Siourounis, 2008; Acemoglu et al., Forthcoming).

In panel settings with only a small number of time periods, consistency of our estimates for the effect of $Democracy_{it}$ would hinge on the assumption that the explanatory variables are strictly exogenous, conditional on ν_i. Furthermore, the set-up of our regression model with both fixed effects estimation and lagged dependent variables would give rise to the so-called *Nickell bias*, which is caused by the mechanical correlation of the demeaned lagged dependent variable and the error term (Nickell, 1981). However, any inconsistency as a result of a violation of strict exogeneity as well as the Nickell bias diminishes with increasing number of time periods T. While the panel we use is unbalanced, with time series of our dependent variables ranging from 5 to 41 years, we have observations of more than 20 years for the vast majority of countries. The overall average time series length per country is approximately 25 years. Therefore, our estimates should be relatively unaffected by such issues.

A general concern of all empirical models is their proneness to omitted variable bias. Omission of variables that affect y_{it} can confound estimates. The inclusion of country-specific fixed effects and year fixed effects corrects for two sources of such

[26] As a robustness check, we additionally control for lagged GDP per capita growth because economic crises could trigger regime changes (Burke and Leigh, 2010).

bias. Furthermore, we collect and include a comprehensive set of alternative variables, X_{it}, covering the most popular explanations in the literature for government size changes and redistribution, thus controlling for their impact on y_{it}. Finally, we control for unrelated sudden spikes in our dependent variable that are due to war or armed conflict involvement.

3.4.2 Instrumental variable estimation

One potential threat to our identification is that democratisations might be endogenous to changes in government size and redistribution. Furthermore, both processes could be determined by the same unobserved factors. To mitigate such concerns, we use instrumental variable (IV) regressions that make use of presumably exogenous variation of democracy. Our instrument is based on the insight that democratisations (as well as democratic reversals) occur in regional waves, which appear not to be driven by region-wide economic factors (O'Loughlin et al., 1998). Hence, the share of democracies in a region is a suitable predictor of democratisation of any given country in that region.[27] Following the example of Acemoglu et al. (Forthcoming), we compute for each country the share of democracies among its neighbour countries in the same region, excluding the country itself from the computation.[28] Furthermore, we use lagged values of *Democracy* as additional instruments (see Section 3.5.2 for an explanation that motivates and justifies this inclusion).

The first stage that elicits the variation in democracy that is due to our instrument is as follows:

$$Democracy_{it} = \alpha + \gamma Instruments_{it} + \delta X_{it-1} + \phi y_{it-1} + \nu_i + \mu_t + \varepsilon_{it} \qquad (3.2)$$

[27] Acemoglu et al. (Forthcoming) show convincingly that a democratisation in a given region is followed by faster growth in the share of democracies in the same region than in the share of democracies in other regions.

[28] Persson and Tabellini (2009) pioneered the idea of using neighbour countries, in their case the neighbours' democratic capital, as instruments for democracy. Further support to the notion that democratisations occur in regional waves in given by Aidt and Jensen (2014), who examine how revolutions in neighbour countries diffuse a threat of regime contention and lead to democratisation.

where $Instruments_{it}$ represent the share of democracies in country i's region in time $t-1$ and the lagged value of $Democracy_{it}$.[29]

As an alternative to $DemocraticEnvironment_{itr}$, we also consider using the number of years a country has been independent to instrument democracy.[30] X_{it-1} includes the same set of covariates discussed in Section 3.4. Analogous to Equation 3.1, we include country and year fixed effects and in most specifications the lagged dependent variable. Next, we use predicted values of *Democracy* from the first stage to estimate the effect on redistribution via Two Stage Least Squares in the following second stage regression:

$$y_{it} = \alpha + \beta \hat{Democracy}_{it} + \lambda X_{it-1} + \rho y_{it-1} + \nu_i + \mu_t + \varepsilon_{it} \tag{3.3}$$

Instrumental variable strategies rest on two assumptions that, when satisfied, ensure that they produce causal inference. First, instruments need to be informative about the endogenous explanatory variables. In this case, they need to be correlated with *Democracy*. Second, instruments need to be valid, i.e. they need to affect the outcome of interest only through their effect on *Democracy*, thus being uncorrelated with the error term ε_{it}. In Section 3.5.2, we demonstrate that our instruments are informative. The exclusion restriction, however, cannot be tested as a consequence of the existence of numerous unobservables. Nonetheless, we pursue a number of exercises that give credence to the exclusion restriction being satisfied (Section 3.5.2).

3.5 Results and discussion

This section reports the main results for the effect of democracy on redistribution. We first present the fixed effects regressions results. We then describe the IV estimation results. Finally, we complement those results with (i) a number of robustness

[29] Democratic environment for country i in year t in region r is defined as

$$DemocraticEnvironment_{itr} = \sum_{j \neq i} (Democracy_{itr} / Countries_{tr})$$

where $Countries_{tr}$ is the number of countries in region r excluding country i.

[30] We cap the maximum number of years since independence at 200 years and then divide the number of years a country has been independent by 200. Consequently, the value of this instrument ranges from 0 to 1.

checks and (ii) various exercises that explore heterogeneous and non-linear effects of democracy.

3.5.1 Fixed effects estimation results

In Table 3.4, we formally examine whether democratisation has an effect on redistribution, showing regression results of Equation 3.1. The Table displays the results of the static panel model without the lagged dependent variable as an explanatory variable. Contrary to recent evidence in Acemoglu et al. (2015), we do not find any significant effect on government size measures. Democratisation is neither associated with increases in total government revenue nor with total tax revenue (as a percentage of GDP). Instead, the coefficients, albeit insignificant, are negative. The same picture holds if we examine government size from the expenditure perspective. Total government expenditure is not affected by democracy. In contrast, democratisation is associated with a significant increase in income tax revenue. The estimated effect of 0.65 percentage points corresponds to an increase of almost 9.5% relative the mean of income taxes.

As Figures 3.2 and 3.3 revealed, public finance measures have been relatively stable over the past 40 years. They are persistent, with changes from one year to another being relatively minor. Thus, past values are useful for explaining contemporaneous levels of tax revenue or government expenditure. Furthermore, there is strong, positive residual autocorrelation in the static model as demonstrated by the Arellano-Bond (Arellano and Bond, 1991) autocorrelation test in Table 3.4. The null hypotheses of no first- and second-order serial correlation are rejected. This requires democracy to not only be uncorrelated with current but also past unobserved influences (shocks) of our outcome variables in order for β to be estimated consistently, which might be a strong assumption as it is plausible that past fiscal variables have had an effect on democracy. In contrast, the residual autocorrelation is significantly reduced when the lagged dependent variable is included. As shown by the Arellano-Bond autocorrelation tests for the dynamic model in Table 3.5, the null hypothesis of no second order serial correlation is not rejected (except for the specification with total expenditure as dependent variable). As a consequence, democracy only needs

to be orthogonal to contemporaneous shocks in fiscal variables, a requirement that is not as restrictive as the requirement for the static model. For those reasons, we suggest to treat the findings of Table 3.4 with caution and instead prefer the dynamic regression specification that includes the lagged value of the outcome variable as an explanatory variable.

Table 3.5 presents the baseline estimates for our dynamic fixed effects model. As a result of controlling for the persistence of the dependent variable, we allow the effects of democratisation to accumulate over time. Therefore, β from Equation 3.1 now only captures the short-run effects of democratisation. The long-run effects of democratisation are obtained by aggregating the effects over time:

$$\text{Long-run effect} = \frac{\beta}{1-\rho} \tag{3.4}$$

Democratisation has no effect on any of the government size measures (total revenue, total taxes, total expenditure), neither in the short run nor in the long run. In contrast, we find significant short-term and long-run effects on income taxes and, contrary to Table 3.4, also on social spending. The long-run effects are sizeable, amounting to increases of 1.4 percentage points in income taxes and almost 1 percentage point in social spending. These figures correspond to increases equivalent to around 20% and 16% of average income taxes and average social spending, respectively. The effects are significantly larger than in the static model, which imposes that the effects do not increase over time, suggesting that the static model underestimates the total effect of democratisation on redistribution. Furthermore, they surpass the effects estimated in other studies.[31] For all five outcome variables, the coefficient of the lagged dependent variable, ρ, is highly significant. ρ ranges from 0.69 to 0.83, which demonstrates the strong persistence in the government size and redistribution measures. In all following specifications, we include the lag of the dependent variable and the lag of GDP per capita but will not report their coefficients and standard errors. Instead, we report the long-run effects of democratisation and corresponding standard errors.

[31] For example, Albertus and Menaldo (2014) estimate a 0.5 percentage point increase in income taxes.

In Panel A of Table 3.6, we include the lag of GDP per capita growth to account for any economic crises that could trigger regime changes (Burke and Leigh, 2010). In Panel B, we additionally control for social conflict and war as well as educational attainment in terms of the population share that is secondary educated.[32] In both cases, our results remain qualitatively unchanged and the estimated short-run and long-run effects of democratisation are also fairly stable.

We pursue a number of additional robustness checks in order to rule out alternative explanations for our findings. First, data coverage differs for the set of outcome variables we consider. Unsurprisingly, the IMF GFS database offers broader and more consistent coverage for aggregate measures, which we refer to as *measures of government size*, than for disaggregated and more detailed expenditure or tax revenue. As a consequence, the fact that we observe an increase in redistribution but not in absolute government size could be driven by the fact that the samples are different. Table 3.7 reports the dynamic regression specification estimates for the joint sample of all dependent variables. Our results remain qualitatively unchanged, demonstrating that the significant effect of democratisation on income taxes and social spending but not on government size measures is not purely caused by different samples.

Besides political institutions and level of development, the literature has also earmarked a country's demographic composition and its openness to trade as potential drivers of government size. Panel A of Table 3.8 controls for the size of each country's exports and imports relative to GDP. Neither the coefficients of democracy nor their significance level are greatly altered compared to our baseline dynamic model specification. If we additionally control for the population share of elderly (65 years or older), our results remain again largely the same, though the short-run and long-run effect of democratisation on social spending become borderline insignificant.[33]

[32]Most of the empirical literature controls for conflicts. As pointed out by Gates et al. (2006), controlling for conflict might be problematic because part of the polity2 score takes civil war already into consideration. However, its exclusion has virtually no effect on our estimates.

[33]Though not reported here, we also test the inclusion of the population share of people aged below 15, the share of urban population, natural resource revenue, and tertiary school attainment. None of these additional controls affects our results (see Appendix 3.6 Table 3.26).

As previously mentioned in Section 3.1, some historians and sociologists partially attribute the historical rise of democratic institutions in Western countries to the need to raise taxes, which could be levied in exchange for greater political representation and enfranchisement (Tilly, 1975). If this was also the case in more recent democratisations, then democracy could be endogenous and estimated results of democratisation on redistribution and government size could be biased. This endogeneity concern does not seem to apply to our specification that includes the lag of the dependent variable because the residuals are not serially correlated (Table 3.5). Nonetheless, we aim to address this potential endogeneity concern by probing our results through controlling for discoveries of giant oil or gas fields in the five to ten years preceding democratisation (Table 3.9).[34] If the need to raise taxes, which we expect to change with large natural resource windfalls, pressured regimes to democratic reforms, then the discovery of large oil and gas fields with considerable resulting resource rents should lower such pressure, and controlling for it should significantly alter our results.[35] Reassuringly, our estimates are not strongly affected when controlling for giant oil or gas field discoveries, alleviating concerns about reverse causality in our sample.

By default, a binary democracy indicator such as *Democracy* that draws a sharp distinction between political regimes based on a threshold for democracy scores might exaggerate potentially small observable differences between for instance an autocratic regime that almost has the necessary institutional characteristics for being categorised democratic and a democracy whose institutions only barely satisfy the minimum requirements for such a classification.[36] Therefore, we probe our results

[34]Data on giant gas and oil fields are from Horn (2014). Newly discovered oil or gas fields cannot be exploited immediately. They require some years of investments before production starts. Therefore, in line with the literature, we use a time lag of five years between discovery and the effect on revenue (Arezki et al., 2017).

[35]Collier (2006) provides a succinct overview of reasons why resource rents undermine democracy. Compared to taxes, resource rents are associated with lower public scrutiny. Since resource rents reduce the need for taxation, they also weaken accountability of governments with regard to how they spend such revenue. Collier (2006) argues that this mechanism erodes governance in resource rich countries.

[36]Democratisations are defined as passing a polity2 score threshold of 0 from one year to the next, which signifies that the democratic features in a country outweigh the autocratic ones. This choice is fairly uncontroversial, having been widely used in the literature, for instance in Brückner et al. (2012); Jones and Olken (2009).

by using a continuous measure of democracy, the polity 2 score, as the explanatory variable (Table 3.10). The results indicate that the distinction between continuous and discretised democracy measures, and thus concerns about any bias caused by introducing a binary distinction, are not of great importance in this context.

Our results on total government size deviate from recent findings (Acemoglu et al., 2015). In the Appendix 3.6, we explore in detail why our results for total revenue and total tax revenue differ from the findings in Acemoglu et al. (2015). In summary, the difference in our findings does not seem to be due to different empirical approaches or different time coverage but appears to be caused by significant discrepancies in the dependent variables. Following our baseline specification from Equation 3.1 with lagged dependent variables, we show that using their dependent variables for total revenue and total taxes does yield a significant positive effect of Democracy (Table 3.22 in the Appendix). In the Appendix 3.6, we discuss the potentially problematic nature of their choice of dependent variables.

Finally, we test the robustness of our results by using an alternative democracy measure from a different source. Table 3.11 reports our fixed effects estimates for the dynamic model but uses as explanatory variable the democracy indicator from Cheibub et al. (2010).[37] Again, the results are consistent with our earlier findings.

The combination of all of these results enhance the confidence in our findings that democracy does indeed increase redistribution but does not affect the absolute size of government.

3.5.2 Instrumental variable approach

The results we have presented so far assume democratisations to be exogenous. However, we cannot completely rule out endogeneity as a result of (i) omitted variables driving changes in both democracy and the dependent variables or (ii) simultaneity

[37]The classification of countries into democracies and non-democracies in Cheibub et al. (2010) is available until 2008. To make use of the full set of observations on our outcome variables, we extend their democracy indicator until 2013. We use their coding of democracy in 2008 for observations in the following years. If our democracy score reveals a democratisation in a country after 2008, we change the democracy variable to 1.

(for instance redistribution potentially having a direct or indirect effect on the probability of democratisation). To address such concerns, we make use of an instrumental variable (IV) estimation the results of which are discussed below.

Table 3.12 presents the IV estimates of the impact of democracy on our outcome variables. The IV results are based on instrumenting whether a country is democratic by its democratic environment (share of democratic countries in the region) and the first lag of $Democracy_{it}$. Lagged values of $Democracy_{it}$ can be used as instruments because there does not appear to be second order autocorrelation in the residuals if we include the lagged dependent variable, as discussed earlier and formally examined in Table 3.5. The reported AR(2) test statistic is not significant (except for expenditure), which justifies using lagged values of democracy as instruments.

The IV regression results provide support for our earlier findings based on fixed effects estimation. Our first stage results show that both our instruments are significant. Furthermore, the p-values of the Kleinbergen-Paap test of underidentification demonstrate that our instruments are informative (Panel B Table 3.12). Democratisation does not increase total government size but causes an increase in redistribution via income taxation (Panel A Table 3.12). The short-run and long-run effects of democratisation on income taxes are slightly larger, but also slightly noisier, than in the baseline dynamic fixed effects estimation reported in Table 3.5. In the short run, democratisation is associated with a 0.38 percentage point increase in income taxes, which accumulates up to 1.8 percentage points in the long run.

In contrast to several previous studies, we do not find any effect of democracy on government size (total revenue, taxes, and expenditure). However, in accordance with theoretical predictions such as the median voter model by Meltzer and Richard (1981), we do find a positive effect of democratisation on measures that better capture redistribution: revenue from income, profits, and capital gain taxes. We believe that this evidence emphasises that crude government size measures, which were predominantly used in previous studies in the literature, do not fully capture

redistribution. The choice of those variables appears to be driven by data availability rather than any conceptually compelling reason.

Since we use two instruments, our model is overidentified, i.e. we have more instruments than endogenous explanatory variables. This enables us to do Hansen overidentification tests. Such tests probe whether, under the assumption that one of the instruments is exogenous, the other instruments are also uncorrelated with the error term. They cannot fully test the validity of the exclusion restriction but they can at least give suggestive support for the assumption that instruments are exogenous. The reported p-values for the Hansen test statistic enhance confidence in the validity of our instruments (Table 3.12). With the exception of the specification for total tax revenue (column 2), they are insignificant. Consequently, we drop total tax revenue from the following IV specifications and focus on total revenue and total expenditure as measures of government size.

We probe the robustness of our IV estimates by gradually controlling for additional factors that could affect government size and perhaps also democracy (i.e. the same control variables that we used in the fixed effects estimation). In Table 3.13, we include the share of the population that is secondary educated (Panel A), openness to trade (Panel B), and the population share of people 65 or older (Panel C). In all specifications, our estimates are fairly stable and results remain qualitatively the same.[38]

Finally, to further increase confidence in the validity of our estimates, we control for a number of factors that could undermine the exogeneity of our democratic environment instrument. We include regional time trends to rule out that region-wide shocks affect democratisation and redistribution at the same time (Panel A Table 3.14), average regional conflict intensity, which takes into consideration region-wide waves of conflict that affect both political regimes and government size (or

[38] As an alternative to regional democratic environment, we exploit the observation that the longer a country has been independent and self-ruled, the more likely it is to develop democratic institutions (Persson, 2005; Eichengreen and Leblang, 2008). The results, reported in Table 3.27 in the Appendix, point in the same direction as our preferred IV specification: democratisation increases redistribution but has no effect on government size. Additionally, they have the advantage that all Hansen tests, including the one for the specification with taxes as dependent variable, reject the null hypothesis that the model is overidentified.

redistribution) (Panel B Table 3.14), and average regional growth of per capita GDP to account for economic dynamics that could stimulate democratisation and might be correlated with government expenditure or taxes (Panel C Table 3.14). All these additional specifications yield estimates that are very similar to those obtained in Table 3.12. Jointly with the overidentification tests, these results increase our confidence in the validity of the causal nature of our estimated effects of democracy on redistribution and government size.

3.5.3 Heterogeneity in the impact of democracy

The impact of democracy on redistribution is unlikely to be homogenous in countries that democratised. Its effects may vary along a number of dimensions. In this section, we shed light on several factors that appear (i) to influence whether democracy leads to higher redistribution in the first place, and (ii) determine the magnitude of the effects. Considering different types of democratisations as well as political characteristics of newly established democracies also helps to elicit potential mechanisms through which democracy affects redistribution.

The main theoretical argument for increased redistribution in democracy compared to autocracy is built on median voter models. These models predict that democratisation, in the form of enfranchisement of new voter groups, will change the position of the pivotal voter in the income distribution as the inclusion of economically disadvantaged groups shifts the ratio of median to mean income in society. An underlying assumption of this prediction is that voters (or citizens more generally) only differ in terms of economic means such as income or wealth. This assumption, while appealing for analytical simplicity, is quite restrictive. Societies are often diverse in terms of religion, ethnicity, or linguistic groups. Those divisions can give rise to cleavages or create group identities along those lines, which could undermine the redistribution mechanism in models such as in Meltzer and Richard (1981) if ethnicity or religion rather than economic means become the main defining criterion of group identities. In fact, we find that the effects of democratisation on redistribution and government size are significantly lower in countries that are relatively more ethnically fractionalised (Table 3.15), corroborating evidence of earlier work

based on cross-country regressions (Easterly and Levine, 1997) and city-level analysis in the United States (Alesina et al., 1999). The data on ethnic fractionalisation are time-constant and are therefore not reported in Table 3.15. In countries with above median ethnic fractionalisation, the effects of democratisation on taxes and income taxes are 0.8 percentage points lower than in countries with below median levels of ethnic fractionalisation, respectively.[39] Interestingly, democratisation is associated with a significant increase in tax revenue, both in the short and long run, in ethnically homogenous societies (or societies with low ethnic fractionalisation) as is shown by the coefficient on Democracy, which points out that stratification of a society along ethnic lines is relevant for explaining whether democratisation leads to larger government size or not.

Fiscal redistribution in the form of progressive taxation or redistributive spending has strong implications for (economic) elites that tend to benefit less from additional spending but contribute a relatively high proportion of revenue from progressive taxes. Therefore, economic elites are incentivised to subdue redistribution in democracies. They may achieve this if they manage to politically capture the institutions of a newly created democracy by exerting disproportionate influence (e.g. through vote-buying or wielding influence over individuals that economically rely on them such as employees, tenant farmers etc.). For instance, Tridimas and Winer (2005) show that, if political power is not equally distributed but correlated with income, the rich exhibit more political power than the poor, which results in lower income tax rates. In contrast, democracies may lead to increased redistribution if elites fail to control the transition to a new regime as demonstrated by Albertus and Menaldo (2014). We revisit the mechanism studied by Albertus and Menaldo (2014), additionally accounting for the dynamics of the dependent variable, and assess whether the effect of democracy differs if a democratisation took place as a result of revolution, which is likely to have shaken up the previously prevailing order of political

[39]Ethnic fractionalisation is standardised by a measure between 0 and 1, larger values indicating higher fractionalisation. In our sample of countries, ethnic fractionalisation ranges from 0.01 to 0.96. Both the observed average and median level are around 0.44.

power.[40] Our results confirm that redistribution through income taxes and social spending is only increased in a newly established democracy if they were caused or at least accompanied by a revolution (Table 3.16). Democratisations that were not caused or preceded by revolutions are not associated with higher redistribution.

At the core of functioning democracies lies the notion of accountability. Politicians can be held accountable for their (in-)action by the electorate and, if they fail to pursue policies in the interest of voters, be punished by not being re-elected. In the context of our analysis, this implies that elected politicians should deliver increased public goods provision or pursue further redistribution if those policies correspond to the broad electorate's preferences. There are, however, a number of scenarios under which the notion of accountability can break down, yielding less responsive governments or leaders. For instance, circumstances where transfer of power does not follow regular procedures could weaken the accountability mechanisms. Leaders that entered office irregularly, i.e. "not in the manner prescribed by either explicit rules or established conventions" (Goemans et al., 2009), might feel less inclined to pursue policies in the wider interest of large segments of the electorate. At the same time, irregular accession to power might also indicate that a government or leader is backed by a powerful coalition, the interest of which they might prioritise in their political agenda. Using data from the Archigos database (Goemans et al., 2016) on characteristics of leaders of all independent states, we examine whether the nature of leaders' access to or exit from power alter the effects of democracy on government spending and revenue. Irregular transfer of power in democracy is positively associated with higher government revenue and expenditure but not, or even negatively, associated with redistribution (Table 3.17). Although government's fiscal activity is increased, it is not used to redistribute funds widely. One possible explanation is that funds are allocated to a small group of political allies, the leader's political power base. Bueno de Mesquita et al. (2002) make a related point about

[40] We use data on revolutions provided by Banks and Wilson (2017) and define a democracy to be triggered by a revolution if a revolution took place during or in the year preceding a democratisation. Revolutions are defined as "any illegal or forced change in the top government elite, any attempt at such a change, or any successful or unsuccessful armed rebellion whose aim is independence from the central government." In total, we identify around 25 democratisations in our sample that took place after a revolution.

the expenditure targeted to a so-called "selectorate", which constitutes a leader's or government's support base.[41] Our results could indicate that the provisions for such a selectorate may be quite expensive, which could cause increases in government expenditure while more redistributive measures that would benefit larger groups remain constant.

A similar argument can be made about democracies with limited constraints on the executive. If the ruling coalition is less constrained by entities such as the parliament, then it might be less responsive to the electorate. We use the sub-components of the polity2 score to examine what effects a democratisation in the sense of a strengthening of institutionalised executive constraints has on redistribution and government size (Panel A Table 3.18). Similarly, we examine if and how democratisation defined as moving to established political competition between and participation of various political parties affects our outcome variables (Panel B Table 3.18).[42] While democratisation in terms of establishing substantial executive constraints is associated with significant short- and long-run increases in redistribution (but not in government size), democratisation as defined by political competition does neither have an impact on redistribution nor on government size. An explanation for these findings could be that political institutions that provide constraints on leaders' decision-making may provide more meaningful mechanisms to hold leaders accountable than mere participation of various parties in elections.

Finally, we examine (i) whether democratisation's impact on redistribution differs in former soviet countries and (ii) if preceding economic turmoil alters the effects of democratisation. Previous studies have tended to exclude countries that formed part of the Soviet Union and its satellite states in Central and Eastern Europe (e.g. Acemoglu et al. (2015)).[43] Based on their socialist ideology, former soviet countries are

[41] Bueno de Mesquita et al. (2002) theoretically investigate how leaders can survive by relying on the selectorate, the support of which they ensure through the provision of private goods.

[42] We define democracy in terms of executive constraints if a country is considered to have at least "substantial limitation on executive authority" by the legislature (Marshall et al., 2011). Democracy in terms of competitiveness of participation is defined as reaching a status that is categorised as "competitive" by Polity IV, which denotes regular electoral competition among parties and voluntary transfer of power.

[43] With the term "soviet" we refer to both countries that were part of the Soviet Union and its satellites.

likely to have had a different history of redistribution than other non-democracies. While the number of observations is small due to the limited number of countries and limited time coverage, our results indeed suggest that the effect of democracy differs in former soviet countries. In contrast to non-soviet countries, there is no significant effect on social spending (the coefficient is in fact negative, albeit insignificant), which is probably a result of high government activity and spending on social causes, paired with an emphasis on social equality in formerly socialist countries (Panel A Table 3.19). However, even in soviet countries democratisation is associated with an increase in revenue from income taxes. Importantly, our results also confirm that our previously reported findings of increased income tax revenue and social spending hold if we exclude soviet countries from the analysis (Panel B Table 3.19).

One reason why regimes might collapse is economic turmoil, which can cause discontent. The distinction between economically caused democratisations and democratisation that have predominantly other causes can be relevant for various reasons. First, economic crises can severely affect a government's ability to spend and might also erode taxable economic activities. Second, economic crises directly reduce GDP and can thus have an indirect effect on our dependent variables, which are expressed as GDP shares, if for instance government spending is more rigid than a country's total economic output. We scrutinise these issues in Table 3.20.[44] Overall, our results alleviate concerns pertaining to the second issue. Democracy remains positively associated with redistribution through income tax. However, we do find that democracies that are created after economic crises differ in terms of spending. They are associated with significantly higher total expenditure and the increase in social spending is more pronounced. While we cannot explicitly investigate the reasons for higher expenditure, there are potential political explanations for

[44]We build the variable "democracy after economic crises" based on information collected by Julia et al. (2016), who surveyed more than 100 democracy experts to collect information on the predominant reasons for democratisations. If economic factors are listed as the main or one of the most important reasons for democratic reform, then we code the process as one caused by an economic crisis.

this result. Following an economic crisis, governments might want to stimulate economic recovery by increased spending while holding taxes constant or even reducing them. Alternatively, or perhaps even in conjunction, new governments in recently established democracies whose citizens underwent severe economic hardship could be inclined to significantly increase public expenditure to consolidate their power by appeasing voters with political gifts in the form of (deficit-) spending.

The list above is not exhaustive as there might be other factors that can shape democratisation's effects on redistribution. Nonetheless, the results presented illustrate that democratisation's impact varies along social factors and political institutions such as accountability.

3.6 Summary and discussion

Modern democracies have many tools at their disposal to manage fiscal redistribution, yet many countries have recently experienced rising levels of inequality. In political economy models, democratisations are predicted to lead to greater fiscal activity of governments. In this chapter, we revisit this hypothesis and, using detailed fiscal government data for more than 150 countries between 1972 and 2013, empirically examine whether democratisation causes an increase in general government size and redistribution.

Most of the existing literature have focused on measures of government size such as total government revenue or total tax revenue in order to study the effects of democratisation on redistribution. In this paper, we use disaggregated public finance data that allow us to take a more nuanced look at this question. We differentiate between measures of government size (total government revenue and expenditure as well as total tax revenue) and measures of redistribution (taxes on income, profits, and capital gains as well as expenditure on education, health, and housing). Our analysis suggests that this distinction is of importance.

Our identification strategy is twofold. First, we use dynamic panel fixed effects estimation that identifies the effects of democratisation on government size and redistribution from within-country time series variation in countries that experience

a democratisation or a democratic reversal. Unlike most previous studies, we allow for the effects of democratisation to increase over time, which enables us to estimate both short- and long-run effects. Second, we use an instrumental variable approach to address potential endogeneity concerns with respect to democracy. To instrument democracy, we use a country's environment, more precisely the share of democracies among countries in the same region, as well as past values of democracy.

Our results suggest that democratisation causes an increase in redistribution. Progressive taxation in the form of income taxes increases by around 1.4 percentage points in the long run when a previously autocratic country democratises. Democratisation also leads to an increase in redistributive government expenditure on education, health, and housing of almost 1 percentage point. These effects are substantial, corresponding to approximately 20% and 16% of average documented income taxes and average social spending, respectively. In contrast, we do not find any effects of democracy on government size. Neither total revenue, total taxes, or total expenditure are significantly affected by democratisation. While overall spending and revenue remain constant, public policies become more redistributive in democracy. These findings appear to provide empirical support to the theoretical predictions of political economy models on the effects of democracy on redistribution. However, they also indicate that one needs to take a more nuanced approach with detailed disaggregated fiscal data to empirically test them.

We uncover a number of dimensions that appear to influence the magnitude of democratisations' effects. First, we show that societal cohesion, or the lack of it, matters. The impact of democratisation on redistribution is significantly lower in societies that are more ethnically fractionalised. At the same time, ethnic fractionalisation also inhibits taxation in democratised countries. In contrast, democratisation appears to lead to increases in total taxation in countries where the population is ethnically relatively homogeneous. A second dimension we examine is the nature of democratisation. Democracy is only associated with higher redistribution in countries that democratised after a revolution. If political power is not equally distributed but correlated with economic means, then elites of the old regime are likely

to maintain disproportionate political influence even after a country has democratised and will tend to undermine redistributive policies. A revolution can weaken former elites' political influence and thus lead to higher levels of redistribution in democracy.

Furthermore, our analysis indicates the relevance of accountability and installed political institutions in explaining the effects of democratisation. Irregular transfers of power in newly established democracies reduce leaders' accountability. As a consequence, they may be less responsive to the general electorate. Democracies with such undermined accountability increase overall government expenditure but this additional spending appears not to be used for redistributive purposes but may benefit only selective groups that constitute the leaders' power base. Similarly, we find that executive constraints, which impose substantial checks and balances on the executive, are an important factor in explaining increased redistribution in democracy.

These are only a number of factors that appear to determine whether democracy is associated with redistribution or not. Future work could investigate these aspects in more detail. For instance, the relationship between accountability and redistribution merits further research. Equally, future research could investigate in more detail how and through which mechanisms stratification of societies and inequality in terms of political influence affect policymaking, as these are likely to have consequences that reach beyond fiscal policies. Finally, our results on economic crises and revolutions that precede democratisation might only be suggestive due to a small number of events we capture in our sample. Consequently, future research could further scrutinise these two types of democratisations and how they affect redistribution.

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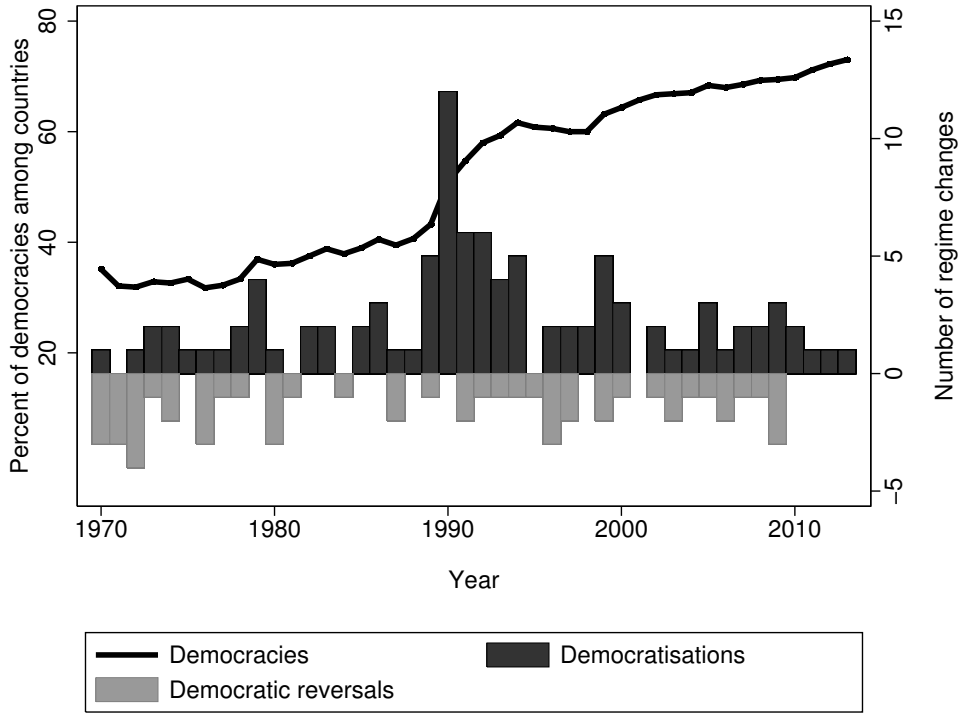
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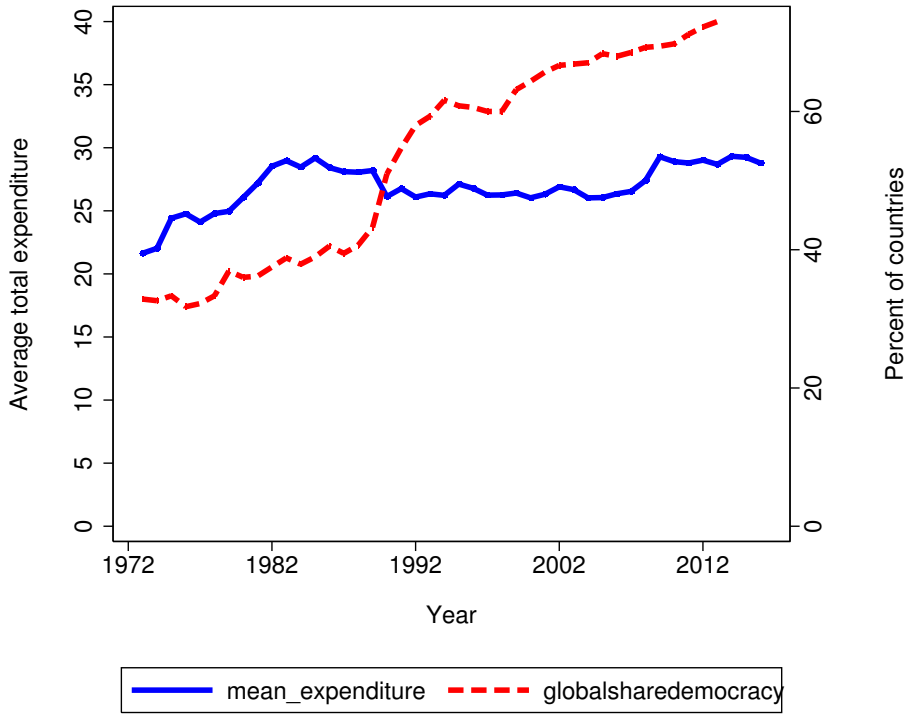
Tables and figures

Graph 3.1: Evolution of the number of democracies in the world over time



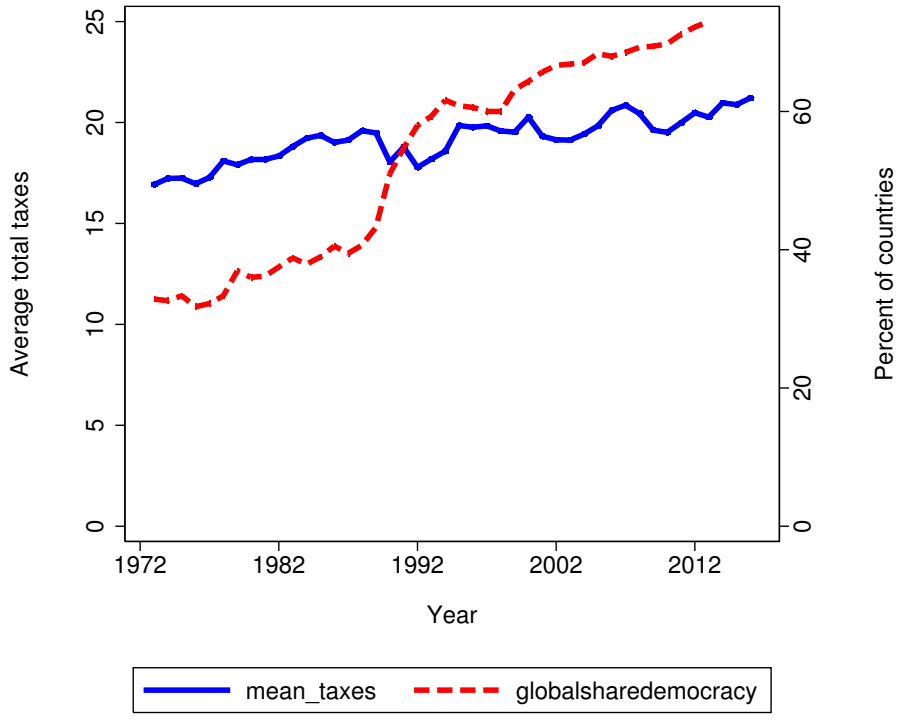
Note: The figure presents the share of democracies of countries in the world according to the polity2 score. Democracies are defined as countries that have a positive polity2 score. Democratisations refer to years when a country's polity2 changes from a non-positive to a positive value. Reversely, democratic reversals refer to years when a country records a negative change in its polity2 score from a positive to a non-positive value. *Source*: Polity IV and author's calculation.

Graph 3.2: Average expenditure and share of democracies over times



Note: The figure reports the average country tax revenue in % of GDP as well as the share of democracies in the world over time. *Source*: IMF Government Finance Statistics and Polity IV.

Graph 3.3: Average tax revenue and share of democracies over times



Note: The figure reports the average country tax revenue in % of GDP as well as the share of democracies in the world over time. *Source*: IMF Government Finance Statistics and Polity IV.

Table 3.1: Overview of democratisations

Democratisations by country and year			
country	year	country	year
Albania	1997	Guatemala	1986
Algeria	2004	Honduras	1980
Bangladesh	2009	Hungary	1989
Bhutan	2008	Indonesia	1999
Bolivia	1982	Iran, Islamic Rep.	1997
Brazil	1985	Kenya	2002
Bulgaria	1990	Korea, Rep.	1987
Burkina Faso	1977	Lebanon	2005
Chile	1989	Mali	2013
Congo, Dem. Rep.	2003	Mexico	1994
Congo, Rep.	1992	Nicaragua	1990
Croatia	1999	Panama	1989
Czechoslovakia	1990	Paraguay	1989
Dominican Republic	1978	Philippines	1986
Ecuador	1979	Portugal	1975
El Salvador	1982	Senegal	2000
Ethiopia	1993	Spain	1976
Fiji	1990	Uganda	1980
Ghana	1979	Uruguay	1985
Greece	1974	Zambia	1991
		Zimbabwe	2009
multiple democratisations			
Argentina	1973, 1983	Pakistan	1988, 2007
Cote d'Ivoire	2000, 2011	Peru	1979, 1993
Lesotho	1993, 1999	Sierra Leone	1996, 2001
Madagascar	1991, 2011	Thailand	1974, 1978, 1992, 2008
Nepal	1990, 2006	Turkey	1973, 1983

Note: The table reports democratisation by country and year. Democratisation is defined as a change from a from a negative or zero value to a positive value of the polity2 score.

Table 3.2: Summary statistics: main variables

	Summary statistics				
VARIABLES	N	mean	sd	min	max
Dependent variables					
Total expenditure as % of GDP	4,399	26.56	11.13	0	93.93
Total revenue as % of GDP	4,447	24.03	10.74	0	78.04
Total tax revenue as % of GDP	4,458	19.13	9.482	0	73.95
Income tax revenue as % of GDP	4,275	6.866	5.564	0	31.20
Social spending as % of GDP	3,127	6.092	3.448	0	22.48
Explanatory variables and controls					
Democracy	4,479	0.662	0.473	0	1
Democratisation	4,139	0.0128	0.112	0	1
Democratic reversal	4,139	0.00725	0.0848	0	1
GDP per capita, constant 2010 US$	4,479	8.476	1.526	5.098	11.64
Civil conflict or civil war	4,479	0.171	0.377	0	1
% with completed secondary education	3,616	18.00	14.63	0.0346	73.00
% with completed tertiary education	3,616	6.576	6.097	0.0126	34.79
Population share 65 or older	4,449	11.64	6.976	0.947	39.58
Urban population share	4,479	54.09	24.09	3.236	100
Trade openness, imports+exports relative to GDP	4,304	79.02	49.57	0.274	441.6
Number of countries	168	168	168	168	168

Table 3.3: Summary statistics: autocracy vs. democracy

	Autocracy		Democracy	
VARIABLES	N	mean	N	mean
Total expenditure as % of GDP	1,485	24.77	2,910	27.49
Total revenue as % of GDP	1,498	21.55	2,949	25.29
Total tax revenue as % of GDP	1,498	14.48	2,960	21.48
Income tax revenue as % of GDP	1,449	4.551	2,823	8.062
Social spending as % of GDP	1,146	5.689	1,978	6.330
GDP per capita, constant 2010 US$	1,511	7.592	2,964	8.925
Civil conflict or civil war	1,511	0.208	2,964	0.152
% with completed secondary education	1,190	9.502	2,422	22.18
% with completed tertiary education	1,190	3.174	2,422	8.251
Population share 65 or older	1,511	7.016	2,934	14.03
Urban population share	1,511	43.14	2,964	59.64
Trade openness	1,383	79.30	2,920	78.82
Population share 15 or younger	1,511	71.96	2,934	47.37

Note: Democracies are defined as countries with a positive value of the polity2 score.

Table 3.4: Baseline regressions - static model

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	-1.016	-0.917	0.654*	-1.695	0.092
	(1.084)	(0.853)	(0.389)	(1.135)	(0.299)
GDP p.c., t-1	3.729**	3.544***	2.513***	3.129**	1.183***
	(1.723)	(1.117)	(0.472)	(1.402)	(0.440)
Observations	4464	4473	4408	4413	3179
R^2	0.739	0.809	0.801	0.731	0.758
AR(1) p-value	0.0001	0.0001	0.0000	0.0000	0.0000
AR(2) p-value	0.0000	0.0000	0.0000	0.0000	0.0001
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Robust and clustered standard errors at the country level. All regressions control for lagged GDP per capita. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 3.5: Baseline regressions - dynamic model

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	-0.006	0.154	0.287***	0.120	0.163**
	(0.297)	(0.174)	(0.109)	(0.299)	(0.078)
Dependent variable, t-1	0.687***	0.799***	0.795***	0.769***	0.830***
	(0.087)	(0.019)	(0.031)	(0.020)	(0.030)
GDP pc, t-1	0.466	0.307*	0.419***	0.985***	0.452***
	(0.528)	(0.170)	(0.156)	(0.329)	(0.120)
Long-run effect	-0.020	0.763	1.400***	0.518	0.961**
	(0.947)	(0.844)	(0.517)	(1.295)	(0.479)
Observations	4231	4242	4184	4181	2950
R^2	0.858	0.936	0.914	0.876	0.872
AR(2) p-value	0.9773	0.6348	0.1511	0.0256	0.2487
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Robust and clustered standard errors at the country level. All regressions include the lag of the dependent variable and control for lagged GDP per capita. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.6: Dynamic model - controlling for economic crises, conflict, and education

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
PANEL A: controlling for GDP per capita growth					
Democracy	-0.001	0.137	0.288***	0.122	0.174**
	(0.301)	(0.175)	(0.110)	(0.305)	(0.081)
GDP pc, t-1	0.454	0.292*	0.409***	0.984***	0.481***
	(0.531)	(0.172)	(0.155)	(0.332)	(0.127)
GDP pc	-0.003	0.012	0.008	-0.013	-0.004
growth, t-1	(0.012)	(0.008)	(0.006)	(0.013)	(0.004)
Long-run	-0.003	0.681	1.407***	0.525	1.038**
effect	(0.958)	(0.852)	(0.517)	(1.315)	(0.501)
Observations	4208	4219	4165	4158	2931
R^2	0.858	0.936	0.915	0.875	0.867
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
PANEL B: controlling for conflict and education					
Democracy	0.077	0.112	0.244**	0.152	0.188**
	(0.206)	(0.159)	(0.115)	(0.279)	(0.087)
GDP pc, t-1	-0.066	0.224	0.333*	0.952***	0.411***
	(0.301)	(0.183)	(0.171)	(0.335)	(0.117)
Secondary	0.017	0.021**	-0.002	0.003	-0.008
educated, t-1	(0.012)	(0.009)	(0.007)	(0.012)	(0.006)
Conflict, t-1	-0.174	-0.059	-0.000	-0.085	-0.046
	(0.254)	(0.179)	(0.077)	(0.265)	(0.075)
Long-run	0.370	0.600	1.307**	0.708	1.150**
effect	(0.996)	(0.847)	(0.567)	(1.306)	(0.553)
Observations	3512	3526	3546	3476	2589
R^2	0.921	0.941	0.930	0.889	0.892
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Robust and clustered standard errors at the country level. All regressions include the lag of the dependent variable and control for lagged GDP per capita. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.7: Baseline regressions, joint sample of dependent variables

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
PANEL A					
Democracy	-0.089	0.095	0.337***	0.105	0.177**
	(0.290)	(0.252)	(0.126)	(0.438)	(0.077)
Long-run effect	-0.382	0.401	1.504***	0.359	0.994**
	(1.236)	(1.079)	(0.518)	(1.507)	(0.443)
Observations	2906	2908	2908	2905	2815
R^2	0.921	0.943	0.921	0.856	0.854
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
PANEL B					
Democracy	-0.173	-0.040	0.285**	-0.019	0.202**
	(0.278)	(0.270)	(0.134)	(0.444)	(0.084)
Secondary educated, t-1	0.014	0.022*	-0.004	0.007	-0.010
	(0.016)	(0.012)	(0.009)	(0.018)	(0.007)
Conflict, t-1	-0.747*	-0.493	-0.070	-0.842*	-0.080
	(0.402)	(0.354)	(0.089)	(0.450)	(0.079)
Long-run effect	-0.755	-0.163	1.242**	-0.068	1.168**
	(1.198)	(1.098)	(0.546)	(1.575)	(0.494)
Observations	2538	2541	2544	2537	2465
R^2	0.929	0.946	0.924	0.876	0.874
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Robust and clustered standard errors at the country level. All regressions include the lag of the dependent variable and control for lagged GDP per capita. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.8: Baseline regressions, additional controls

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
PANEL A					
Democracy	-0.024	0.061	0.243*	0.091	0.155*
	(0.211)	(0.165)	(0.123)	(0.277)	(0.090)
Secondary	0.014	0.018**	-0.002	0.003	-0.007
educated, t-1	(0.012)	(0.008)	(0.007)	(0.012)	(0.006)
Trade, t-1	0.001	0.002	0.002	0.001	-0.002*
	(0.003)	(0.003)	(0.002)	(0.004)	(0.001)
Conflict, t-1	-0.192	-0.081	-0.001	-0.115	-0.047
	(0.255)	(0.179)	(0.079)	(0.255)	(0.073)
Long-run	-0.115	0.323	1.314**	0.438	1.007*
effect	(1.011)	(0.877)	(0.610)	(1.338)	(0.595)
Observations	3395	3401	3417	3351	2476
R^2	0.917	0.940	0.931	0.893	0.885
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
PANEL B					
Democracy	-0.028	0.065	0.234*	0.088	0.146
	(0.211)	(0.166)	(0.121)	(0.277)	(0.091)
Secondary	0.014	0.018**	-0.002	0.003	-0.008
educated, t-1	(0.012)	(0.008)	(0.007)	(0.012)	(0.006)
Trade, t-1	0.001	0.002	0.002	0.001	-0.002
	(0.003)	(0.003)	(0.002)	(0.004)	(0.001)
Conflict, t-1	-0.195	-0.079	-0.009	-0.117	-0.060
	(0.253)	(0.178)	(0.076)	(0.252)	(0.071)
Old population	-0.015	0.016	-0.035	-0.013	-0.056***
share, t-1	(0.042)	(0.037)	(0.024)	(0.044)	(0.019)
Long-run	-0.136	0.347	1.259**	0.423	0.920
effect	(1.012)	(0.880)	(0.593)	(1.337)	(0.576)
Observations	3395	3401	3417	3351	2476
R^2	0.916	0.940	0.931	0.893	0.901
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Robust and clustered standard errors at the country level. All regressions include the lag of the dependent variable and control for lagged GDP per capita. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.9: Controlling for giant oil or gas field discoveries prior democratisation

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	-0.006	0.154	0.287***	0.117	0.164**
	(0.296)	(0.174)	(0.109)	(0.299)	(0.078)
Giant oil or gas field, t-1	0.033	-0.020	0.015	0.201	-0.019
	(0.275)	(0.168)	(0.151)	(0.212)	(0.068)
Long-run effect	-0.021	0.764	1.399***	0.506	0.965**
	(0.946)	(0.844)	(0.516)	(1.294)	(0.479)
Observations	4231	4242	4184	4181	2950
R^2	0.858	0.936	0.914	0.875	0.872
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Data on giant oil or gas field discoveries are from Horn (2014) and described in Section 3.5.1. Robust and clustered standard errors at the country level. All regressions include the lag of the dependent variable and control for lagged GDP per capita. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.10: Baseline regressions with polity 2 score instead of democracy indicator

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Polity 2	0.010	0.016	0.027***	0.014	0.016**
	(0.019)	(0.014)	(0.009)	(0.023)	(0.007)
Long-run effect	0.046	0.079	0.132***	0.062	0.097**
	(0.087)	(0.069)	(0.042)	(0.102)	(0.042)
Observations	3892	3904	3979	3849	2857
R^2	0.911	0.936	0.912	0.878	0.867
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Robust and clustered standard errors at the country level. All regressions include the lag of the dependent variable and control for lagged GDP per capita. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.11: Democracy based on Cheibub et al. (2010)

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy, CDG	0.108	0.209	0.291***	0.149	0.185**
	(0.265)	(0.165)	(0.101)	(0.312)	(0.074)
Long-run effect	0.347	1.034	1.415***	0.648	1.084**
	(0.866)	(0.775)	(0.521)	(1.335)	(0.502)
Observations	4238	4249	4190	4187	2952
R^2	0.549	0.709	0.658	0.649	0.708
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Democracy is defined according to the classification of Cheibub et al. (2010). Robust and clustered standard errors at the country level. All regressions include the lag of the dependent variable and control for lagged GDP per capita. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.12: IV estimation: share of democracies in the region and lagged democracy as instruments

	Panel A: Second Stage				
	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	0.109	0.235	0.375***	-0.089	0.116
	(0.256)	(0.185)	(0.137)	(0.369)	(0.102)
Long-run effect	0.465	1.164	1.824***	-0.395	0.686
	(1.078)	(0.880)	(0.640)	(1.630)	(0.607)
Observations	4218	4233	4181	4172	2947
R^2	0.667	0.707	0.658	0.654	0.708
Underidentification p-value	0.000	0.000	0.000	0.000	0.000
Hansen p-value	0.248	0.043	0.795	0.655	0.316
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

	Panel B: First Stage				
	(1)	(2)	(3)	(4)	(5)
Democratic environment, t-1	0.00128***	0.00123***	0.00041***	0.00127***	0.00182***
	(0.00039)	(0.00039)	(0.00135)	(0.00039)	(0.00053)
Democracy, t-1	0.8188***	0.8175***	0.8179***	0.8160***	0.7944***
	(0.0193)	(0.0194)	(0.0194)	(0.0196)	(0.0248)
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Regressions are based on Equation 3.3. All regressions include country and year fixed effects and control for lagged GDP per capita as well the lag of the dependent variable. Democracy is instrumented by the share of democracies in a country's region as well as lagged democracy. To test underidentification, the Kleinbergen-Paap statistic is used and its p-value is reported. The Sargan-Hansen test is used to test overidentifying restrictions and its p-value is reported. Robust and clustered standard errors at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.13: IV estimation: robustness to the inclusion of controls

	(1) Revenue	(2) Income taxes	(3) Expenditure	(4) Social spending
		PANEL A		
Democracy	0.032	0.319**	-0.083	0.163
	(0.212)	(0.144)	(0.387)	(0.115)
Secondary educated, t-1	0.018	-0.002	0.003	-0.008
	(0.011)	(0.007)	(0.012)	(0.006)
Long-run effect	0.153	1.705**	-0.397	1.002
	(1.024)	(0.701)	(1.851)	(0.709)
Observations	3506	3542	3470	2586
R^2	0.678	0.673	0.679	0.725
Underidentification p-value	0.000	0.000	0.000	0.000
Hansen p-value	0.188	0.907	0.661	0.343
		PANEL B		
Democracy	-0.009	0.376**	-0.103	0.108
	(0.269)	(0.148)	(0.334)	(0.111)
Trade, t-1	0.001	0.001	-0.002	-0.003**
	(0.004)	(0.001)	(0.005)	(0.001)
Long-run effect	-0.038	1.820***	-0.467	0.663
	(1.153)	(0.689)	(1.527)	(0.689)
Observations	4059	4012	4003	2806
R^2	0.669	0.663	0.663	0.720
Underidentification p-value	0.000	0.000	0.000	0.000
Hansen p-value	0.301	0.695	0.731	0.249
		PANEL C		
Democracy	0.118	0.368***	-0.081	0.107
	(0.256)	(0.135)	(0.371)	(0.103)
Old population share, t-1	0.015	-0.018	0.028	-0.052***
	(0.044)	(0.024)	(0.049)	(0.019)
Long-run effect	0.507	1.792***	-0.356	0.614
	(1.078)	(0.625)	(1.639)	(0.595)
Observations	4188	4157	4142	2947
R^2	0.666	0.659	0.655	0.709
Underidentification p-value	0.000	0.000	0.000	0.000
Hansen p-value	0.262	0.861	0.784	0.390

Note: Regressions are based on Equation 3.3. All regressions include country and year fixed effects and control for lagged GDP per capita as well the lag of the dependent variable. Democracy is instrumented by the share of democracies in a country's region as well as lagged democracy. To test underidentification, the Kleinbergen-Paap statistic is used and its p-value is reported. The Sargan-Hansen test is used to test overidentifying restrictions and its p-value is reported. Robust and clustered standard errors at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.14: IV estimation: controlling for region-wide trends, conflict, and economic growth

	(1) Revenue	(2) Income taxes	(3) Expenditure	(4) Social spending
	PANEL A: regional trends			
Democracy	0.195	0.324**	-0.111	0.056
	(0.272)	(0.157)	(0.385)	(0.107)
Long-run effect	0.810	1.564**	-0.468	0.326
	(1.108)	(0.716)	(1.625)	(0.626)
Observations	4218	4181	4172	2947
R^2	0.669	0.659	0.656	0.709
Underidentifcation p-value	0.000	0.000	0.000	0.000
Hansen p-value	0.309	0.656	0.309	0.930
Reg. trends	Yes	Yes	Yes	Yes
	PANEL B: regional conflict			
Democracy	0.233	0.342**	0.009	0.073
	(0.270)	(0.171)	(0.377)	(0.110)
Average regional conflict, t-1	-0.008	0.002	-0.006	0.002
	(0.008)	(0.004)	(0.009)	(0.003)
Long-run effect	0.998	1.664**	0.038	0.434
	(1.133)	(0.761)	(1.662)	(0.657)
Observations	4218	4181	4172	2947
R^2	0.667	0.658	0.654	0.708
Underidentifcation p-value	0.000	0.000	0.000	0.000
Hansen p-value	0.408	0.386	0.493	0.363
	PANEL C: average regional growth			
Democracy	0.112	0.373***	-0.080	0.117
	(0.256)	(0.137)	(0.369)	(0.102)
Average regional growth, t-1	-0.018	0.014	-0.065***	-0.007
	(0.024)	(0.011)	(0.025)	(0.010)
Long-run effect	0.480	1.814***	-0.351	0.686
	(1.077)	(0.638)	(1.624)	(0.608)
Observations	4218	4181	4172	2947
R^2	0.667	0.658	0.654	0.708
Underidentifcation p-value	0.000	0.000	0.000	0.000
Hansen p-value	0.249	0.797	0.659	0.325

Note: Regressions are based on Equation 3.3. All regressions include country and year fixed effects and control for lagged GDP per capita as well the lag of the dependent variable. Democracy is instrumented by the share of democracies in a country's region as well as lagged democracy. To test underidentification, the Kleinbergen-Paap statistic is used and its p-value is reported. The Sargan-Hansen test is used to test overidentifying restrictions and its p-value is reported. Robust and clustered standard errors at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.15: Heterogeneity: democracy and ethnic fractionalisation

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	0.661	0.742**	0.579***	0.644	0.151
	(0.468)	(0.371)	(0.201)	(0.488)	(0.097)
Democracy *	-0.732	-0.844*	-0.485**	-0.593	0.071
high fractionalisation	(0.567)	(0.464)	(0.224)	(0.623)	(0.131)
Long-run	3.079	3.789**	3.120***	2.932	0.954
effect Democracy	(2.070)	(1.662)	(0.913)	(2.132)	(0.625)
Long-run	-3.411	-4.310**	-2.613**	-2.700	0.448
effect interaction	(2.524)	(2.119)	(1.067)	(2.739)	(0.831)
Observations	3652	3662	3608	3604	2577
Countries	128	128	126	128	117
R^2	0.911	0.935	0.926	0.883	0.875
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Ethnic fractionalisation is ranges from 0 to 1, larger values indicating higher fractionalisation. The median level of fractionalisation in our sample is around 0.44. "High fractionalisation" is a dummy variable that is 1 if a country has above-median ethnic fractionalisation, and 0 otherwise. Robust and clustered standard errors at the country level. All regressions include country and year fixed effects and control for lagged GDP per capita as well the lag of the dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.16: Heterogeneity: democracies after revolution

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	0.250	0.266	0.098	0.368	0.125
	(0.258)	(0.202)	(0.101)	(0.356)	(0.094)
Democracy after revolution	-0.195	-0.233	0.345*	-0.626	0.253*
	(0.469)	(0.382)	(0.196)	(0.556)	(0.138)
Revolution, t-1	0.061	-0.099	-0.047	0.157	0.045
	(0.216)	(0.170)	(0.070)	(0.304)	(0.087)
Long-run effect Democracy	1.134	1.303	0.448	1.563	0.753
	(1.177)	(1.016)	(0.466)	(1.543)	(0.558)
Long-run effect interaction	-0.884	-1.143	1.576*	-2.661	1.519
	(2.161)	(1.929)	(0.867)	(2.440)	(0.929)
Observations	3361	3374	3477	3324	2644
Countries	139	139	140	139	130
R^2	0.908	0.932	0.914	0.869	0.856
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Revolutions are defined as "any illegal or forced change in the top government elite, any attempt at such a change, or any successful or unsuccessful armed rebellion whose aim is independence from the central government" (Banks and Wilson, 2017). Robust and clustered standard errors at the country level. All regressions include country and year fixed effects and control for lagged GDP per capita as well the lag of the dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.17: Democratisation and nature of leader's exit from and entry to power

	PANEL A: irregular exit				
	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	0.072	0.097	0.329***	-0.010	0.223**
	(0.301)	(0.226)	(0.116)	(0.342)	(0.109)
Democracy *	0.706*	0.610	-0.184	1.134**	-0.148
irregular exit	(0.420)	(0.378)	(0.143)	(0.468)	(0.156)
Irregular exit	0.136	0.020	0.056	-0.065	0.147
	(0.410)	(0.297)	(0.118)	(0.405)	(0.155)
Long-run	0.327	0.481	1.607***	-0.044	1.309**
effect Democracy	(1.363)	(1.102)	(0.575)	(1.497)	(0.635)
Long-run	3.218	3.019	-0.900	4.964**	-0.869
effect interaction	(1.962)	(1.923)	(0.708)	(2.052)	(0.883)
Observations	4060	4072	4091	4017	2926
Countries	155	155	156	155	143
R^2	0.911	0.936	0.914	0.877	0.871
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
	PANEL B: irregular entry				
Democracy	0.092	0.113	0.331***	-0.010	0.229**
	(0.304)	(0.229)	(0.116)	(0.342)	(0.111)
Democracy *	0.666	0.578	-0.189	1.134**	-0.163
irregular entry	(0.425)	(0.390)	(0.144)	(0.468)	(0.161)
Irregular entry	0.178	0.053	0.061	-0.065	0.163
	(0.414)	(0.298)	(0.119)	(0.405)	(0.161)
Long-run	0.420	0.562	1.619***	-0.044	1.345**
effect Democracy	(1.378)	(1.113)	(0.578)	(1.497)	(0.642)
Long-run	3.036	2.862	-0.925	4.964**	-0.954
effect interaction	(1.982)	(1.978)	(0.713)	(2.052)	(0.906)
Observations	4060	4072	4091	4017	2926
Countries	155	155	156	155	143
R^2	0.911	0.936	0.914	0.877	0.871
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Transfer of power is defined as irregular if it did not follow the manner prescribed by either explicit rules or established conventions (Goemans et al., 2009). Robust and clustered standard errors at the country level. All regressions include country and year fixed effects and control for lagged GDP per capita as well the lag of the dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.18: Democratisation based on polity2 subcomponents

	Revenue	Taxes	Income taxes	Expenditure	Social spending
			PANEL A: executive constraints		
Democracy	-0.030	0.094	0.302***	-0.128	0.186***
exec. constraints	(0.244)	(0.191)	(0.105)	(0.300)	(0.071)
Long-run	-0.134	0.460	1.465***	-0.559	1.125**
effect	(1.100)	(0.927)	(0.492)	(1.312)	(0.453)
Observations	3926	3938	4013	3883	2881
Countries	150	150	151	150	138
R^2	0.911	0.936	0.914	0.879	0.870
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
			PANEL B: competition of participation		
Democracy	0.108	0.038	0.224	-0.352	-0.025
participation	(0.415)	(0.273)	(0.271)	(0.397)	(0.104)
Long-run	0.487	0.187	1.099	-1.536	-0.148
effect	(1.872)	(1.337)	(1.334)	(1.737)	(0.634)
Observations	3926	3938	4013	3883	2881
Countries	150	150	151	150	138
R^2	0.911	0.936	0.916	0.879	0.880
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: We define democracy in terms of executive constraints if a country is considered to have at least substantial limitation on executive authority. Democracy in terms of competitiveness of participation is defined as reaching a status that is categorised as competitive by Polity IV, which denotes regular electoral competition among parties and voluntary transfer of power. Robust and clustered standard errors at the country level. All regressions include country and year fixed effects and control for lagged GDP per capita as well the lag of the dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.19: Heterogeneity: democratisation in formerly soviet vs. non-soviet countries

	PANEL A: Former soviet countries				
	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	-0.993	-0.657	1.157**	-1.273	-0.781
	(2.819)	(1.746)	(0.432)	(3.819)	(0.753)
Long-run effect	-3.025	-2.527	4.426**	-3.348	-2.290
	(8.441)	(6.246)	(1.723)	(9.981)	(2.007)
Observations	318	317	324	318	260
Countries	19	19	20	19	20
R^2	0.858	0.889	0.799	0.817	0.770
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
	PANEL B: Non-soviet countries				
	(1)	(2)	(3)	(4)	(5)
Democracy	0.018	0.179	0.275**	0.138	0.186**
	(0.298)	(0.175)	(0.110)	(0.298)	(0.077)
Long-run effect	0.056	0.898	1.346**	0.606	1.162**
	(0.952)	(0.849)	(0.520)	(1.316)	(0.507)
Observations	3913	3925	3860	3863	2690
Countries	147	147	145	147	127
R^2	0.856	0.934	0.917	0.877	0.880
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Robust and clustered standard errors at the country level. All regressions include country and year fixed effects and control for lagged GDP per capita as well the lag of the dependent variable.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.20: Heterogeneity: democratisation and economic crises

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	-0.101	0.079	0.295**	-0.035	0.116
	(0.334)	(0.189)	(0.122)	(0.316)	(0.080)
Democracy after economic crisis	0.833	0.489	-0.050	1.269**	0.470**
	(0.646)	(0.328)	(0.206)	(0.506)	(0.186)
GDP per capita growth growth, t-1	-0.002	0.012	0.008	-0.013	-0.004
	(0.012)	(0.008)	(0.006)	(0.013)	(0.004)
Long-run effect	-0.321	0.390	1.444**	-0.151	0.680
	(1.029)	(0.925)	(0.569)	(1.348)	(0.474)
Long-run effect interaction	2.649	2.417	-0.246	5.418**	2.757**
	(1.802)	(1.651)	(1.005)	(2.151)	(1.132)
Observations	4208	4219	4165	4158	2931
Countries	166	166	165	166	147
R^2	0.857	0.936	0.915	0.875	0.863
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Robust and clustered standard errors at the country level. All regressions include country and year fixed effects and control for lagged GDP per capita as well the lag of the dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

In the following subsections, we provide additional information on our main data source, IMF Government Finance Statistics, and further supporting, empirical evidence. The first subsection gives a more detailed overview of the GFS data, its compilation etc. The second subsection offers an analysis and explanation of the differences in our findings compared to Acemoglu et al. (2015). Finally, we include a number of additional regression tables.

Additional information on compiling government finance statistics

The IMF disseminates its government finance statistics in three ways. Data from 1990 onwards are accessible online through the IMF data portal. For some countries, especially high-income countries, earlier data can also be extracted from the online portal. In general, however, pre-1990 data are not available via the data portal or incomplete. Instead, pre-1990 data are included in a separate database called "Historical Government Finance Statistics". In order to get access to this database, which covers time series of annual data for 149 countries from 1972 to 1989, one needs to file and submit an application directly to the IMF data division. The data are then shared on CD-ROM. In addition, in some cases historical information on disaggregated taxes and expenditure are not fully available in the "Historical GFS" database. We therefore supplement those two sources with data from the Global Development Network Growth Database, which additionally compiled such information from IMF statistical year books, the third format in which the IMF disseminates its government finance statistics. Furthermore, we use data on income taxes and social spending shared by Albertus and Menaldo (2014) if none of the three sources above provide information. For income taxes, around 15% of observations are derived from Albertus and Menaldo (2014). For social spending, that share amounts to approximately 20% of observations.

Accounting, currency reforms, and monetary unions

There are a couple of issues that may impair intra-country comparability of GFS data. The vast majority of GFS observations are given in millions of national currency in current prices.[45] Since the GDP series with which we convert the GFS data into GDP shares (from the World Bank, World Development Indicators) is listed in today's currencies (i.e. Germany's and France's GDP 1990 is given in Euros), we had to convert all fiscal data recorded in the countries' previous currencies into its current currency. This entails two challenges. First, the formation of monetary unions that saw several countries change their currencies causes a discrepancy between past and current currency. The most prominent examples are European Union countries that adopted the Euro. We convert all fiscal data recorded in the countries' previ-

[45]For recent data, the IMF also directly reports statistics in % of GDP.

ous currencies into Euro series by applying the EU's irrevocable exchange rates.[46] A second issue emerges if and when countries pass currency reforms. In our sample, we account for currency reforms and convert data accordingly in the following countries: Belarus, Congo, Dem. Rep., Ecuador, El Salvador, Ghana, Romania, Sudan, Suriname, Turkey, Venezuela, RB, and Zambia.

Finally, the accounting system has changed in many countries. The historical convention to account public finance data on a cash basis, meaning activities were recorded at the time of a monetary exchange or payment, has been replaced, especially in advanced economies, by an accrual accounting system that records transactions when changes in economic value are documented. Generally, cash accounted measures are very close proxies for accrual measures but at times there might be small discrepancies due to timing differences. The GFS manual suggests using the accrual measures alongside the cash measures for cross-sectional time series research. Accounting switches can be marked by a dummy variable to control for any confounding effect.

Level of government: central versus general government

For our analysis, we use central government data on revenue and expenditure. At the central government level, data can be reported in two different accounting standards: Consolidated Central Government budget or Central Budgetary budget.[47] Not all countries report statistics for both standards. In general, we give preference to consolidated data. Where countries predominantly report Budgetary data, we use those data series instead. The key aspect is that we keep the accounting standard fixed for each country, which ensures temporal consistency for each country. Since the identifying variation in our empirical strategies is within-country variation, differences between countries in the accounting standard are less important and are accounted for by country fixed effects.[48]

If data availability were not a concern, general government would be the ideal choice regarding the level of government. It incorporates statistics from all different government levels, local, state, and central, and is therefore the most complete measure for both expenditure and taxation. In strongly federalist countries there could potentially be a discrepancy between central government and general government data because a lot of the expenditure made and tax revenue generated stem from the local or state levels. However, using data for general government restricts the analysis to a much smaller panel. It includes fewer countries and is limited to a shorter time span that does not cover a considerable proportion of democratisations. As a result, such an analysis would be vulnerable to severe selection bias problems.

[46]For a reference, please consult the statistical appendix to the IMF World Economic Outlook data set that provides a table of the exchange rates used for the GFS data series.

[47]The different accounting standards are explained in detail in International Monetary Fund (2014). Data consolidation is explained on page 62. Essentially, consolidation eliminates double-counting of flows and stocks by different government units.

[48]In a few countries such as Belgium, the accounting standard changes midway through our sample period. We show in Table 3.21 that these changes do not affect our results by including a dummy that indicates whether a country reports Central Budgetary data in a given year, which would pick up effects resulting from such a change.

Essentially, the probability of general government data availability is conspicuously lower for developing and emerging economies in comparison to availability of central government data. Thus using general government data would be less globally representative and mostly driven by evidence from advanced economies. Dziobek et al. (2011) show that using central government data might not be problematic in practice because fiscal data on the central government level are fairly good approximations for the general government statistcs, even more so in the case of developing economies. In general, most of the literature focuses on central government.

Table 3.21: Baseline fixed effects estimation including a dummy for budgetary data.

	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	-0.100	0.079	0.280**	0.106	0.151*
	(0.287)	(0.152)	(0.113)	(0.301)	(0.079)
Long-run effect	-0.333	0.437	1.300***	0.461	0.867*
	(0.923)	(0.845)	(0.495)	(1.304)	(0.452)
Observations	4196	4207	4074	4148	2876
R^2	0.861	0.941	0.918	0.872	0.866
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Robust and clustered standard errors at the country level. Fixed effects estimation following Equation 3.1. All regressions control for lagged dependent variable and lagged GDP per capita. The regressions include a binary indicator for Budgetary data. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Comparison with Acemoglu et al. (2015)

Acemoglu et al. (2015) focus in their analysis on total revenue and tax revenue as shares of GDP. In contrast to our work, they do not use government finance data directly from the IMF. Instead, they make use of data obtained from Hendrix (2010) based on a project by Arbetman-Rabinowitz et al. (2013). According to Acemoglu et al. (2015), those time series combine data from the World Bank (1960 to 1972), the IMF, and other national sources. The main advantage of using those integrated series is the prolonged time period that can be analysed. The data date back up to 1960, whereas IMF statistics are only available from 1972 onwards.

There are a number of reasons why our findings could differ from the results in Acemoglu et al. (2015). First, our empirical approaches are not identical. They prefer 5-year panels instead of an annual panel and focus on Arellano-Bond GMM estimation. However, they also report robustness checks for an annual data and

dynamic fixed effects panel regressions, which yield significant positive effects of democracy on taxes and revenue, too. Second, since their sample goes further back in time it also includes a number of regime changes that occurred between 1960 and 1972, which we cannot analyse. As such, the time dimension could drive the documented difference in results. Third, they construct a democracy measure that is slightly different from ours as they combine information from Polity IV and Freedom House to define democracies. Fourth, incongruence of the outcome variables could explain the discrepancy.

Table 3.22 below illustrates that it is in fact their choice of outcome variables that constitutes the main reason why our results deviate from theirs. Using our baseline regression specification, as presented in Equation 3.1, on their dependent variables, we obtain significant positive effects of democratisation.[49] This is particularly evident when we look at columns (3) and (4), which additionally control for tertiary educational attainment and past conflict, but also comes through in the pure baseline regressions in columns (1) and (2) that only include lagged GDP per capita and the lag of the dependent variable. The qualitative picture of the results neither changes if (i) we only consider observations from 1972 onwards (Panel B) or (ii) use democracy as coded by Acemoglu et al. (2015) (Panel C). Consequently, we can rule out that the different time coverage of the samples, a different estimation specification, or differences in the definition of democracy cause the discrepancy in findings between the two studies.

Based on this insight, we have taken a closer look at the dependent variables in Acemoglu et al. (2015). We have found non-negligible deviations from the official IMF GFS statistics, even in more recent years and in high-income countries where data should presumably be reported in a more consistent and reliable manner. For illustration, Table 3.23 contrasts tax revenue GDP shares from the IMF online portal and from the data used by Acemoglu et al. (2015) for Austria and Australia between 2000 and 2005. The discrepancies are large, regardless of the level of government we consider from IMF database. Since reporting standards and data consistency for older time periods are likely to be less rigorous than for such recent years as depicted in Table 3.23, we need to assume that similar or even greater discrepancies exist for more historical observations. All of this leads us to assume that there is considerable measurement error in the dependent variables used by Acemoglu et al. (2015) that could lead to imprecise estimates of the effect of democracy on government size.

[49] We thank Suresh Naidu for being willing to share the data used for the analysis in their paper.

Table 3.22: Fixed effects estimation with dependent variables from Acemoglu et al. (2015)

	PANEL A: same specification as Equation 3.1			
	(1)	(2)	(3)	(4)
	Revenue ACM	Taxes ACM	Revenue ACM	Taxes ACM
Democracy	25.280	0.005**	32.314*	0.005**
	(18.333)	(0.002)	(19.198)	(0.002)
Secondary educated, t-1			-1.412	-0.000
			(1.938)	(0.000)
Conflict, t-1			-9.746	-0.001
			(17.503)	(0.002)
Long-run effect	106.793	0.021**	146.820	0.023***
	(79.077)	(0.008)	(90.513)	(0.009)
Observations	3986	3986	3513	3513
R^2	0.913	0.887	0.916	0.893
	PANEL B: 1972 or later			
Democracy	23.393	0.005**	30.092	0.005**
	(18.778)	(0.002)	(19.653)	(0.002)
Secondary educated, t-1			-1.389	-0.000
			(2.006)	(0.000)
Conflict, t-1			-16.282	-0.001
			(18.246)	(0.002)
Long-run effect	96.686	0.021**	133.684	0.024***
	(78.928)	(0.008)	(90.053)	(0.009)
Observations	3893	3893	3425	3425
R^2	0.914	0.886	0.917	0.892
	PANEL C: Democracy as coded by Acemoglu et al. (2015)			
Democracy, ACM	25.889	0.005***	34.123*	0.006***
	(18.730)	(0.002)	(19.869)	(0.002)
Secondary educated, t-1			-1.323	-0.000
			(1.989)	(0.000)
Conflict, t-1			-14.379	-0.001
			(18.196)	(0.002)
Long-run effect	107.521	0.023***	151.153*	0.027***
	(78.669)	(0.008)	(89.796)	(0.009)
Observations	3926	3926	3443	3443
R^2	0.914	0.887	0.917	0.892

Note: All regressions include country and year fixed effects, the lag of the dependent variable and lagged GDP per capita. Fixed effects estimation following Equation 3.1. Robust and clustered standard errors at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.23: Comparison of revenue and taxes (% of GDP) for IMF data and Acemoglu et al. (2015)

	IMF				Acemoglu et al.	
	General government		Central government			
year	Revenue	Taxes	Revenue	Taxes	Revenue	Taxes
			PANEL A: Australia			
2000	35.4	28.6	25.0	22.3	25.9	13.8
2001	35.3	29.4	26.3	24.1	26.3	15.0
2002	33.7	27.9	25.2	22.8	26.4	14.8
2003	34.5	28.7	25.8	23.5	26.6	14.9
2004	34.8	28.9	25.7	23.5	26.4	14.5
2005	34.9	29.0	26	23.9	26.6	14.6
			PANEL A: Austria			
2000	48.6	28.1	44.1	26.3	39.3	16.8
2001	50.7	29.7	45.9	28.0	41.0	18.5
2002	49.7	28.7	44.9	27.0	40.2	17.7
2003	49.6	28.5	44.5	26.7	38.4	15.9
2004	49.0	28.1	44.2	26.4	38.4	16.1
2005	48.8	27.3	44.0	25.6	38.1	16.7

Additional results

Table 3.24: Overview of democratic reversals

Democratic reversals by country and year			
country	year	country	year
Albania	1996	Lesotho	1998
Argentina	1976	Madagascar	2009
Belarus	1995	Mali	2012
Burkina Faso	1980	Nepal	2002
Chile	1973	Nigeria	1984
Congo, Rep.	1997	Peru	1992
Cote d'Ivoire	2002	Philippines	1972
Ethiopia	2005	Sierra Leone	1997
Ghana	1981	Turkey	1980
Guatemala	1974	Uganda	1985
Guyana	1978	Uruguay	1972
Iran, Islamic Rep.	2004	Zimbabwe	1987
Korea, Rep.	1972		
multiple reversals			
Fiji	1987 and 2006	Thailand	1976, 1991, 2006
Pakistan	1977 and 1999	Bangladesh	1974 an 2007

Note: The figure reports democratic reversals by country and year. Democratic reversal is defined as a change from a from a positive to a non-positive value of the polity2 score.

Table 3.25: Overview of number of countries with tax and expenditure data per year

	Number of countries per year with				
year	**tax data**	**expenditure data**	year	**tax data**	**expenditure data**
1972	77	76	1993	107	107
1973	93	91	1994	111	110
1974	95	96	1995	114	113
1975	103	102	1996	112	110
1976	108	107	1997	113	111
1977	108	109	1998	115	114
1978	108	109	1999	115	111
1979	108	109	2000	111	106
1980	111	109	2001	120	114
1981	111	109	2002	123	119
1982	109	109	2003	128	125
1983	110	109	2004	131	129
1984	114	111	2005	133	129
1985	116	112	2006	137	133
1986	113	110	2007	137	133
1987	105	105	2008	145	140
1988	108	107	2009	149	144
1989	107	107	2010	153	149
1990	101	101	2011	156	152
1991	104	104	2012	151	147
1992	103	103	2013	142	138

Table 3.26: Fixed effects: additional controls

	Revenue	Taxes	Income taxes	Expenditure	Social spending
			PANEL A		
Democracy	-0.006	0.170	0.289***	0.111	0.145*
	(0.298)	(0.175)	(0.105)	(0.299)	(0.078)
Urban population, t-1	-0.000	-0.011	-0.001	0.005	0.008
	(0.027)	(0.014)	(0.011)	(0.023)	(0.008)
Long-run effect	-0.018	0.843	1.409***	0.481	0.846*
	(0.952)	(0.850)	(0.499)	(1.297)	(0.475)
Observations	4231	4242	4184	4181	2950
R^2	0.858	0.937	0.915	0.873	0.854
			PANEL B		
Democracy	-0.082	0.089	0.257**	0.137	0.156**
	(0.286)	(0.152)	(0.112)	(0.301)	(0.077)
Young population, t-1	0.009	0.006	-0.011*	0.020	0.002
	(0.015)	(0.007)	(0.006)	(0.012)	(0.004)
Long-run effect	-0.273	0.491	1.190**	0.589	0.899**
	(0.925)	(0.846)	(0.485)	(1.299)	(0.450)
Observations	4165	4176	4050	4117	2876
R^2	0.862	0.941	0.915	0.875	0.868
			PANEL C		
Democracy	0.007	0.050	0.212*	0.092	0.170**
	(0.208)	(0.156)	(0.115)	(0.273)	(0.085)
Tertiary educated, t-1	-0.021	0.004	-0.034*	-0.034	-0.008
	(0.031)	(0.022)	(0.017)	(0.035)	(0.017)
Long-run effect	0.036	0.268	1.060**	0.432	1.008*
	(1.000)	(0.831)	(0.511)	(1.287)	(0.525)
Observations	3481	3495	3443	3445	2515
R^2	0.922	0.941	0.932	0.890	0.887
			PANEL D		
Democracy	-0.247	0.061	0.285**	0.101	0.175**
	(0.354)	(0.174)	(0.123)	(0.316)	(0.086)
Resource revenue, t-1	0.067	0.022	0.013	0.092***	0.008
	(0.044)	(0.024)	(0.013)	(0.027)	(0.005)
Long-run effect	-0.685	0.304	1.290**	0.411	1.000**
	(0.917)	(0.860)	(0.529)	(1.270)	(0.473)
Observations	3785	3787	3717	3729	2616
R^2	0.843	0.939	0.918	0.859	0.889

Note: Fixed effects estimation following Equation 3.1. All regressions include country and year fixed effects, the lag of the dependent variable and lagged GDP per capita. Robust and clustered standard errors at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.27: IV estimation: years since independence and lagged democracy as instruments

	Panel A: Second Stage				
	(1) Revenue	(2) Taxes	(3) Income taxes	(4) Expenditure	(5) Social spending
Democracy	0.128	0.256	0.381***	-0.079	0.105
	(0.254)	(0.183)	(0.142)	(0.365)	(0.101)
Long-run effect	0.547	1.272	1.853***	-0.351	0.621
	(1.066)	(0.868)	(0.655)	(1.615)	(0.606)
Observations	4218	4233	4181	4172	2947
R^2	0.667	0.707	0.658	0.654	0.708
Underidentifcation p-value	0.000	0.000	0.000	0.000	0.000
p-value					
Hansen p-value	0.867	0.774	0.215	0.917	0.772
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
	Panel B: First Stage				
	(1)	(2)	(3)	(4)	(5)
Years	0.3127***	0.3543***	0.3754***	0.3555***	0.4229***
independent	(0.1180)	(0.1225)	(0.1238)	(0.1249)	(0.1596)
Democracy, t-1	0.8253***	0.8238***	0.8256***	0.8228***	0.8063***
	(0.0183)	(0.0183)	(0.0182)	(0.0186)	(0.0236)
Lagged dep. var.	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: Regressions are based on Equation 3.3. All regressions include country and year fixed effects, the lag of the dependent variable and lagged GDP per capita. Democracy is instrumented by the years a country has been independent, capped at 200 and then divided by 200, as well as lagged democracy. Robust and clustered standard errors at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.