Colombo, Andrea

Thesis

Why do local institutions matter? : the political economy of decentralization

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Why do local institutions matter?
The political economy of decentralization

Thesis presented by Andrea COLOMBO
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Supervisor: Professor Antonio ESTACHE

Thesis jury:
Marjorie GASSNER (Université libre de Bruxelles, Chair)
Gany ALDASHEV (Université libre de Bruxelles, Secretary)
Ilan TOJEROW (Université libre de Bruxelles)
Francesc TRILLAS (Universitat Autònoma de Barcelona)
Ana MORENO MONROY (Organisation for Economic Co-operation and Development)

ECARES

To Florentine

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Chapter 1

The role of local institutions in the decentralization process

1.1 Introduction

This thesis aims at better understanding how local institutions and political incentives condition the decentralized provision of public goods. I argue that the impact of decentralization reforms depends on local political dynamics as well as on the type of decentralized public goods. The thesis provides evidence from three countries – Burundi, Brazil and Belgium – that differ with respect to their stage of development as well as to the "intensity" of their decentralized institutions. I consider a "global" public good – peace and stability in a post-conflict country – and two local public goods – sanitation and crime.

Decentralization reforms aim at improving access to public services by redistributing power, resources and authority from the central level to subnational administrative units (Faguet, 2012). According to the first generation theories of federalism, in decentralized settings the government can match resources to the demand and needs of the local population more precisely and efficiently. As a result, the provision of public goods such as water and sanitation, and policing could improve significantly. In addition – assuming the perfect mobility of capital and labor – subnational governments would compete to improve services and attract individuals and investments (Faguet et al., 2015).

Because of their virtues, decentralization reforms often come to the fore of the policy debate in both developing and developed countries. In Belgium, Germany and Italy, decentralization aimed at accounting for political equilibria that are deeply rooted in marked regional identities. Indonesia and the Philippines empowered local authorities in order to respond to urgent economic and political crises. In Burundi, Colombia and Rwanda, decentralization was intended to bolster the legitimacy of the state after long-lasting conflicts. In Brazil, Mexico and South Africa, the objective of decentralization was to facilitate the transition from authoritarian to democratic rule (Eaton et al., 2011).

In reality, the outcomes of decentralization reforms may depart from theoretical predictions. One of the reasons is their halfhearted endorsement by central politicians and bureaucrats. In unitary states, any decentralization reform must inevitably come from the top of the governmental hierarchy. Yet, central administrators have little incentive to give up their power and authority. As a result, decentralization reforms usually hide details and caveats that allow the central state to keep a grip on local administrators, thus disempowering them. For instance, the central governmental may transfer authority without equipping subnational governors with the fiscal means and capabilities to operate independently. Decentralization is therefore a rather nuanced concept, an institutional

equilibrium where both centrifugal and centripetal forces coexist and might balance each other out.

Another reason is that decentralization reforms often overlook pre-existing local institutions, government forms and traditions. For instance, local grievances and political rivalries may impede administrators from coordinating the provision of large-scale public services. As long as a country is centralized, these local characteristics can be considered as "background noise", with little relevance for policy design. However, in decentralized states this "background noise" ends up directing political incentives, driving local politicians' decisions and determining the adaptability of decentralization reforms to local contexts.

The reasons behind failing decentralization also have implications for the way these reforms are evaluated. Traditionally, scholars assess decentralization reforms as if they were unidimensional: a country is either decentralized or not. The actual implementation of decentralization policies instead depends on the historical, political and economic local drivers, and in general good local governance (Faguet, 2012). Moreover, these pre-existing institutions and the associated policy impact may vary across space and time. Overlooking such heterogeneities would capture only the aggregate effect of decentralization reforms, thereby failing to measure precisely their local impact. It is therefore more appropriate to go beyond simplistic assessment of decentralization policies and characterize them by their "intensity" – or "completeness" (Devarajan et al., 2009).

In order to get a more complete assessment of decentralization, the next three chapters study the political and institutional factors of success of decentralizing reforms.

Chapter 2 focuses on political decentralization reform in a post-conflict country, Burundi, and its consequences for the access to a "global" public good: peace and stability. In 2009, international organizations advocated political decentralization as a first step towards democratic transition after years of dictatorship and a devastating civil war. The country signed peace agreements that demobilized rebels and scheduled local elections for 2010. Local elections were part of a broader agenda that was deemed to increase the "intensity" of decentralization by bringing higher accountability, inclusion of minorities and ultimately stability. Contrary to expectations, Olivia D'Aoust, Olivier Sterck and I show that the reform did not always facilitate a peaceful democratic transition. The open conflict in the battlefield evolved into stiff political competition, where parties used demobilized soldiers to pollute elections with violence. Wartime heritage and local political dynamics took a heavy toll and deviated decentralization reforms from a successful trajectory.

Chapter 3 investigates how decentralization may fall short of theoretical predictions when local authorities jointly provide a local public good, in this case: sanitation services in Brazil. While sanitation is a responsibility of mayors, the scale of the public good and the spillovers that it entails, as well as poor local capacity, are such that local authorities usually coordinate its provision. The "intensity" of decentralization, here, depends on the capacity of mayors to coordinate proceedings. Even if institutions are by design decentralized, the intensity of decentralization is low when municipalities do not cooperate. This increases with the capacity of municipalities to coordinate the provision of sanitation infrastructure – and similar local public goods.

Coordination raises accountability issues, however. When local public goods are provided cooperatively, voters cannot observe clearly the performance of their politicians

and punish any morally hazardous behavior. Blurred accountability may make it easier for mayors to free-ride on each other, thereby minimizing their own effort in the cooperative provision of public goods. In the case of Brazil and the local management of sewers, I argue that free-riding is easier when mayors come from the same political family: party discipline or the potential loss of the party's reputation may discourage punishment of shirkers. The result may be a quantity and quality of public good that is lower than what the society wishes for. Instead, politically misaligned administrators may be less lenient with free riders and more vigilant over the conduct of their peers. Tighter monitoring would then improve access to sanitation.

Chapter 4 bridges the first two, focusing on the provision of cross-border public goods in Belgium when the accountability of local politicians increases. A reform in 2005 introduced the direct election of mayors in one of the country's regions. In the rest of the country, mayors remained appointed by the local city councils. By further decentralizing political power, the reform increased the "intensity" of decentralization.

Ilan Tojerow and I measure the impact of increased accountability on the provision of an inter-jurisdictional public good: policing. We show that crime incidence decreases in municipalities affected by the reform, with higher accountability leading to better policing. However, the beneficial effects of the reform fade when the management of local police has to be coordinated among numerous neighboring mayors. Once again, decentralization reforms may derail when they overlook inter-jurisdictional spillovers and the transaction costs of implementation.

The rest of this chapter contextualizes the three case studies and the related. It reviews two interrelated strands of political and public economics literature to which this thesis aims to contribute. The first strand studies to what extent decentralization increases the accountability of local authorities, especially in fragile countries; the second studies the conditions under which decentralization is still preferable in the context of inter-jurisdictional spillovers.

1.2 Decentralization in fragile states

Electoral competition is usually a competition where only few contenders are able to extra oligopolistic rents. If democracy were a market, parties would be suppliers of public services and compete for rents that they can extract once in power. Democratic political competition should motivate parties to offer their constituents better public services while minimizing rents. Yet, competition is rarely open. National government is so complex that only large parties can exploit economies of scale and minimize management costs. Moreover, voters can find it hard to entrust the national government to any political group that has no proven record of administrative competence. In this context, parties in power may try to collude, share oligopolistic profits from democratic competition and exclude new potential competitors (Myerson, 2014).

Decentralization may break the oligopoly of the ruling elite by increasing turnover and competition. Political decentralization creates a multitude of local political markets where administrators compete for power and popular local leaders may aspire to roles in the upper tiers of government. These aspirations represent a short-term threat for incumbent national leaders, but are also a long-term opportunity. National leaders can hold power only if they win voters' support, which popular local agents are best positioned

to catalyze because of their proximity to constituents. Bold national leaders may find it convenient to encourage local politicians' campaigning efforts by promising democratic advancement. Political decentralization can therefore promote good local leaders in the national elite and further break incumbent national leaders' oligopoly (Myerson, 2014).

Decentralization may theoretically improve citizens' well-being by increasing the accountability of local politicians, although not much evidence exists in support of this argument. Opening up electoral competition to local parties would yield higher-quality public goods that better suit citizens' needs (Myerson, 2014). At the same time, decentralization enfranchises local voters, who can now monitor the conduct of local administrators more easily because of proximity (Finan and Ferraz, 2011; Ramanathan, 2007). However, there is not much empirical evidence to support this direct relationship. For instance, in Bolivia decentralization reforms increased enrollment rates in public schools and access to public health amongst the poorest through an increase in the accountability of local politicians (Faguet and Sánchez, 2014). Public expenditure in Brazilian municipalities have followed popular preferences more closely since the introduction of participatory budgeting (Gonçalves, 2014).

Nonetheless, political decentralization could lead to unintended outcomes that may harm accountability. The overall risk is that – to use Myerson's terminology – local elites emerge and monopolize newly established local political markets. In Argentina, for example, entrenched elites dominate most provinces, foil democratic competition and prosper through clientelism (Ardanaz et al., 2014). Moreover, since provincial actors weigh heavily on national decisions, local dynamics dragged the whole country into repeated economic failures (Weingast, 2014). In decentralized Pakistan, local political dynasties set high barriers of access to local "political markets" (Cheema et al., 2006). In Uganda, the beneficial effects of decentralization are questionable because of the lack of a culture of transparency and civic engagement that would otherwise ensure downward accountability (Francis and James, 2003). Finally, the introduction of local democracy can be just a way for military rulers to gain a measure of democratic legitimacy for their regime (Bardhan and Mookherjee, 2006; Cheema et al., 2004).

Political decentralization reforms can effectively raise accountability if they adapt to pre-existing institutions, local norms and capacity (Acemoglu and Jackson, 2014, 2017). In Rwanda, decentralization reforms succeeded because two set of old traditions were revived in order to educate citizens and politicians about the virtues of "democratic accountability". The first tradition motivates citizens to get actively involved in community development. The second tradition challenges leaders to commit to local service delivery through a specific public ceremony (Andrews, 2013). In 2004, the newly signed constitution of Afghanistan called for the establishment of the first village councils and government. Some villages organized local elections to select council members, others formalized the pre-existing *shuras*, whose members are selected on the basis of their community standing. As a result, democratic-related outcomes improved more in the first group of villages than in the second. However, voters trusted *shuras* to represent their interests in front of higher levels of government and local Taliban groups than the elected councils (Jochem et al., 2016).

Political decentralization reforms can build trust, accountability and peace in post-conflict contexts if they account for the drivers of the conflict itself and the inherited political dynamics. The first chapter of this thesis contributes to this line of research, as

it investigates how pre-existing polarization among rebel groups might have undermined the implementation of political decentralization reforms and fueled instability in post-civil war Burundi.

1.3 Decentralization and inter-jurisdictional coordination

When local authorities are in charge of the provision of local public goods, they may free-ride on each other's resources, generating negative spillovers for neighboring authorities. This occurs because local leaders lack incentives to moderate the use of cross-border resources if neighbors bear the social costs of over-exploitation. For example, water quality is worse in rivers that cross international borders than in rivers that flow within the same country (Sigman, 2002, 2005). In the United States, air polluters are more likely to locate themselves near a state's downwind border (Monogan et al., 2017). In Brazil, water pollution in rivers that cross several municipalities increases as the stream approaches the downstream exit point, with downstream residents bearing the consequences of upstream residents' behavior (Lipscomb and Mobarak, 2016).

The Oates' Decentralization Theorem argues that the central government is best placed to minimize the social costs from externalities because of its scale and capacity. In particular, centralization is an optimal solution when spillovers affect municipalities that have preferences for similar public goods (Oates, 1972).

Yet, decentralization may be preferable when central coordination could threaten the accountability of the policy-making process. Because of their scope and nature, central governments tend to aggregate preferences, overlooking the needs of specific local communities (Cremer et al., 1994). Moreover, central governments may not act as the theory predicts. The underlying assumption of the Oates' decentralization theorem is that a central government provides uniform and unbiased policies to internalize cross-border externalities. However, career-concerned legislators can earmark funds for specific projects in their districts. Even when a formula dictates the allocation of funds, the latter is not impermeable to manipulations that would hijack resources to specific jurisdictions. All in all, centralization does not necessarily guarantee an adequate, uniform and political unbiased answer to inter-jurisdictional spillovers (Besley and Coate, 2003). Decentralization, however, can guarantee tailor-made solutions to local needs, at the cost of potentially higher externalities.

Policy-makers therefore face the following dilemma: how to tackle cross-boundary externalities without giving up the benefits of decentralized institutions in terms of increased accountability? There are three possible solutions.

First, incentives for local bureaucrats can help achieve a good balance between accountability and internalization of inter-jurisdictional spillovers. The Chinese government enacted effective decentralization reforms by promising career advancements to local leaders that preserved the quality of water in cross-jurisdiction rivers (Kahn et al., 2015). The transparent measurement of bureaucrats' jobs coupled with mutual monitoring, information sharing and innovation may be powerful incentive mechanisms to overcome lagging local capacity and to increase productivity (Che and Yoo, 2001).

Second, tasks could be divided across layers of government depending on the type of externalities that they are best at tackling. Investments in large infrastructure may entail territorial externalities and policy externalities, arising from the interdependence of

policy objectives and instruments. The federal level has been found to better internalize territorial externalities, while subnational governments is in the best position to tackle policy externalities (Montolio and Trillas, 2013).

Hybrid institutional arrangements are a third solution to the coordination-accountability trade-off. In countries with "partial decentralization", the central and local governments share the power and mandate over the provision of certain public goods (Devarajan et al., 2009). This hybrid arrangement can be effective if the benefits from jointly providing complementary public goods outweigh the costs of reduced accountability (Jametti and Joanis, 2010; Joanis, 2014). In this regard, the design of fiscal transfers can create the right incentives for involved decision-makers to coordinate provision and remain transparent (Brueckner, 2009). Moreover, it is crucial to ensure a well-working system to monitor the decision-makers' activity.

When responsibilities are shared, it is difficult to keep track of the performance of every layer of government. In fact, information about the vertical distribution of power may be so imperfect that voters hold national governments accountable for inadequate local services (Devarajan et al., 2009; Joanis, 2014). Blurred accountability has two consequences. On the one hand, unaccountable local governments may divert resources away from the public goods for which they are (co-)responsible. On the other hand, as a reaction to the moral hazard issue, higher tier of governments may divert resources towards districts where they could gain an electoral advantage.

Political alignment between and across layers of government can determine the success or failure of partial decentralization. Federal and local politicians who come from the same party rely on each other to win or keep their respective offices, and might endorse each other's activities accordingly. Jametti and Joanis (2010) and Joanis (2014), in their aforementioned studies, argue that the benefits of joint provision of public goods trump the costs of blurred accountability only when federal and subnational governments are politically aligned, as reciprocity dominates political calculus. In Brazil, water pollution is found to decrease when state governors and mayors – jointly responsible for sanitation policies – are from the same party (Estache et al., 2016). Politically aligned politicians are, moreover, more likely to interact and coordinate, implying a mutual deterrence to defection on which credible commitments can develop (de Figueiredo Jr. and Weingast, 2005; Ostrom, 1998).

Political alignment may have its drawbacks too. For instance, it might influence the way that the central government transfers grants to subnational governments. Municipalities ruled by the same parties as the center tend to receive relatively more transfers than municipalities ruled by competing parties. In this way, the central government can "tie the hands of its enemies" and increase the advantage of locally aligned governments by giving them more fiscal space to lower local taxes, provide more public goods and improve municipal services (Baskaran and Hessami, 2017; Brollo et al., 2013; Solé-Ollé and Sorribas-Navarro, 2008).

Partial decentralization is about interaction not only across layers of government, but also between administrators at the same level of governance. In a federal system, the scale of certain types of public goods often requires coordination between mayors or other subnational administrators. In fact, the lack of coordination can exacerbate the costs of cross-border externalities. For instance, waterborne epidemics are more likely to

break out if individual municipalities manage the sanitation infrastructure on their own, without coordination (Brannstrom, 2004; Formiga-Johnsson and Kemper, 2005).

Horizontal inter-jurisdictional coordination poses similar accountability issues to those seen with "vertical" partial decentralization. Inter-local agreements shift service responsibility from individual mandates to shared mandates, blurring administrators' accountability and undermining the quality of services (Feiock, 2007; Zeemering, 2012). Consider a group of mayors that coordinate the provision of an inter-jurisdictional infrastructure, such as water and waste-water pipes, or roads. Mayors may take advantage of inter-local agreements to free-ride on their respective peers or collude with them in order to minimize their effort and maximize rents. This morally hazardous behavior may eventually lead to the under provision of the common public good. Local administrators could opt for cooperative behaviors if offered the right incentives: lower transaction costs of negotiating, monitoring and enforcing an agreement.

Political alignment can still determine the success of horizontal inter-jurisdictional coordination. Durante and Gutierrez (2015) find that homicide rates in Mexican municipalities decrease when neighboring mayors come from the same party and therefore interact more easily. Gerber et al. (2013), Hawkins et al. (2016) and related public administration literature find that political homophily could facilitate coordination of services in a metropolitan area, but there is no real assessment of the direct outcome in terms of access to public services. Few studies directly address the benefits and problems entailed by the interaction between local governments.

The literature has only recently started investigating the role of horizontal coordination and political alignment in decentralized settings. The ambition of this thesis is to contribute to this emerging and promising field of research. Chapter 3 assesses the extent to which coordination among mayors from the same political party may improve (or not) access to an inter-jurisdictional sewerage network. Chapter 4 moves one step forward and studies how coordination issues may jeopardize the potential beneficial effects of increased local accountability on the management of the Belgian local police.

Chapter 2

From rebellion to electoral violence: evidence from Burundi[1]

Joint work with Olivia D'Aoust (World Bank) and Olivier Sterck (University of Oxford)

> *"Democratic governance - by protecting minorities, encouraging political pluralism, and upholding the rule of law - can channel internal dissent peacefully, and thus help avert civil wars. Conversely, authoritarian and highly personalized forms of governance, ethnic discrimination, human rights violations and corruption are among the root causes of many of today's internal conflicts."*
> U.N. Secretary-General Kofi Annan (2000)[2]

Promoting democratization and elections has been at the core of peace-building missions in post-conflict societies since the end of the Cold War. Early post-war elections should indeed increase legitimacy and accountability of the newly emergent governments and foster social trust in war-torn societies, thus contributing to long-lasting peace and sustainable development. Nonetheless, elections failed to be implemented properly in a vast majority of post-conflict countries in Africa. During the 1975-2011 period, 80% of African polls were spoiled by violence, bribery, intimidation or inequitable government interference, compared to 40% in non-African countries (Bishop and Hoeffler, 2014). Developing a better understanding of the causes of failed elections, and more generally democratic transitions, is all the more important in view of their devastating effects on the living conditions of civilians (Dupas and Robinson, 2010, 2012; Omotola, 2010). Failed transitions undermine the legitimacy of the States (Berman et al., 2014), destroy social capital (Dercon and Gutiérrez-Romero, 2012), determine poor economic and political performance (Chauvet and Collier, 2009; Collier and Hoeffler, 2015; Kudamatsu, 2012) and can plunge fragile countries back into war (Brancati and Snyder, 2012; Hegre et al., 2001; Henderson and Singer, 2000).

In which context is electoral violence likely to emerge? On the one hand, a vast literature has studied how ethnic grievances may foster violence or affect the provision of public goods – see e.g. Alesina et al. (1999); Easterly and Levine (1997); Montalvo and Reynal-Querol (2005). Popular resentment is likely to be exacerbated during electoral periods as elites have been shown to exploit ethnic allegiances to seize political power (Eifert et al., 2010; Wilkinson, 2004). On the other hand, economists have shown theoretically that electoral violence may be an optimal strategy for political actors, depending on their position (incumbent versus opponent), their relative strength and their support among the population (Chaturvedi, 2005; Collier and Vicente, 2012; Ellman and Wantchekon, 2000; Robinson and Torvik, 2009).

[1] Based on Colombo et al. (2019). Secondary tables and figures are reported in the Online Appendix to the published version of the article. They are nonetheless available upon request.

[2] Quote from the address delivered by Secretary-General Kofi Annan in Warsaw at the International Conference: "Towards a Community of Democracies".

In this chapter, we show empirically that the exploitation of ethnic belonging and political competition are not sufficient for triggering electoral violence. For political competition to turn violent, the presence of local perpetrators is necessary. These can be referred to as *"specialists in violence"* (Chaturvedi, 2005) or *"hardcore supporters"* (Collier and Vicente, 2012). Demobilized rebels, whose factions often became political parties after civil war, are good candidates for such a role during electoral campaigns. As such, they might be more likely to resort to violence and rely on non-democratic persuasion tools, hence undermining the conduct of free and fair elections. We test this theory in the context of the 2010 electoral cycle in Burundi and find that polarization between ex-rebel groups - capturing the presence of groups with equal support - is associated with more electoral violence.

This chapter draws three main conclusions. First, we show that a one standard deviation increase in polarization among rebel groups leads to an increase of 38% of violent events. Namely, going from the lowest polarized municipality in Burundi to the highest one, we predict a four-fold increase in the incidence of violent events. Second, political competition between parties matters in triggering electoral violence as well. An increase of one standard deviation in political fractionalization leads to 45% more violent events. Moreover, when disaggregating electoral violence by type, we find that political and demobilized rebels' competition explain different types of events. In particular, political fractionalization is significantly associated with targeted violence, such as arbitrary detention or murder while demobilized rebels' polarization is causing more clashes and destruction of properties. Third, ethnic grievance and Hutu-Tutsi antagonism, traditionally indicated as one of the main drivers of violence, does not determine any significant increase in the incidence of violence. The results suggest that the triggers of electoral violence in Burundi have disengaged from their original inter-ethnic roots. Electoral violence was higher in municipalities with a larger proportion of Hutu, which suggests that tensions became anchored in the intra-Hutu rivalry, manifested by Hutu ex-rebel groups and political parties competing to seize power. This study concludes that demobilization programs alone may not be sufficient to prevent the resurgence of violence.

This chapter contributes to three strands of the literature.

First, it contributes to the literature on the non-monotonic benefits of democratization. In post-war countries, the extent to which citizens can gain from democratic transition depends on a complex set of incentives and institutions forcing politicians to compete legally. For instance, democratic transition from dictatorship is successful in countries with some earlier history of democratic regimes (Eaton et al., 2011). If these incentives were missing, the democratic transition could fail with disastrous consequences on political instability, ethnic conflict, and poor economic outcomes (Rodrik and Wacziarg, 2005). These negative spillovers are amplified in the presence of variability in the quality of local institutions at the subnational level. In Burundi, the distribution of demobilized soldiers, their incentives to reintegrate into society or to serve illicit local political dynamics jeopardize the homogeneous unfolding of democratization across the country.

Second, it complements the few existing micro-quantitative studies on electoral violence. Dercon and Gutiérrez-Romero (2012) use micro-level data from Kenya to study the electoral violence that erupted in 2007. They find that violence emerged in areas prone to land disputes and with politically connected gangs. In the same context,

Gutiérrez-Romero (2012) finds that political parties engaged in vote-buying in areas where they were less likely to win, in order *"to weaken the support of their political rivals and to mobilize their own"*. Complementing these results, Collier and Vicente (2014) recently evaluated a randomized anti-violence community campaign in Nigeria. They find that the intervention decreased the intensity and the perceptions of violence, and increased turnout. Fafchamps and Vicente (2013) further show that the effects of the campaign were also transmitted indirectly through kinship and geographical proximity. In this chapter, the empirical investigation complements this scarce literature by focusing on a post-conflict context and by directly studying the long-term impact of Burundi's civil war on post-conflict elections. This focus is particularly relevant since conflict-affected countries have been shown to be more likely to return to war (Collier, 2008).

Importantly, this study also contributes to the body of research that assesses the effectiveness of demobilization programs in post-conflict societies (D'Aoust et al., 2016; Gilligan et al., 2013; Humphreys and Weinstein, 2007; Verwimp and Bundervoet, 2009). This is the first empirical study that evaluates the impact of ex-combatants' demobilization on the recurrence of outbursts of violence. More generally, we claim that understanding the causes of electoral violence is an important preliminary step before implementing and assessing policies aiming to reduce this anti-democratic phenomenon.

The chapter is organized as follows. The history of Burundi and the context of the 2010 elections are introduced in Section 2.1. In Section 2.2, we lay out and justify research hypotheses by referring to the theoretical literature on conflict and electoral violence. Section 2.3 describes the dataset and the econometric methods used in the empirical analysis. Results are presented in Section 2.4 and are discussed in Section 2.5. Section 2.6 concludes.

2.1 Decentralization as a tool for post-war reconstruction in Burundi

Since its independence from Belgium in 1961, the political situation in Burundi has been unstable and disrupted by recurrent episodes of violence between the country's two major ethnic groups, the Hutu majority and the Tutsi minority.[3] After a short democratic transition, a group of Tutsi officers seized power in 1966 following contested elections won by the Hutu majority. From 1966 to 1988, three Tutsi presidents from the same party[4] and the same village in the Southwestern province of Bururi governed the country and violently repressed any Hutu rebellion.

In 1988, a democratization process was initiated under the pressure of the international community. A new constitution introduced multiparty competition after 20 years of Tutsi regime. Melchior Ndadaye, from the Hutu-based party FRODEBU[5], triumphed in the 1993 elections and set up a government of power-sharing. His assassination a few months later by Tutsi officers and the death of the *ad interim* President, Cyprien

[3]According to the 2012 Afrobarometer survey, the Hutu and the Tutsi represented 82% and 17% of the population respectively. A third group, the Twa, was reported to account for less than 1% of the population.

[4]The Union for National Progress (Union pour le Progrès National, or UPRONA)

[5]The Front for Democracy in Burundi (Front pour la Démocratie du Burundi)

Ntaryamina in the 1994 plane crash in Rwanda[6] triggered a civil war opposing the Tutsi-controlled army and radical Hutu groups. The already doomed democratization process was definitely buried in July 1996 when the Tutsi-controlled army led by former Tutsi president Buyoya overthrew the power-sharing government.

A fragile peace was reached through the Arusha Peace and Reconciliation Agreement on August 28, 2000. The Arusha Agreement institutionalized power sharing across ethnic groups by designing an ethnically-mixed transitional government and imposing ethnic quotas in institutions and political parties.[7] However, the two largest Hutu rebel groups, the CNDD-FDD and the FNL-Palipehutu[8] rejected the peace accords and continued to fight the transitional government. The CNDD-FDD eventually signed a Comprehensive Ceasefire Agreement in 2003 and joined the power-sharing government. Combatants from the Burundian Armed Forces (Forces Armées Burundaise, or FAB) and from the CNDD-FDD were selected to form the new national army (National Defense Force or Forces de Défense Nationale – FDN). Those who did not fulfill selection criteria based on age, health status and experience were demobilized according to a "Disarmament, Demobilization and Reinsertion" (DDR) program. Approximately 23,000 units from both sides (almost three demobilized soldiers per thousand inhabitants) benefited from a sequence of reinsertion and reintegration grants in order to be able *"to return to their community and to sustain themselves and their families for a limited period following demobilization"* (The World Bank Group, 2004).

The CNDD-FDD won the elections held in 2005, and its leader, Pierre Nkurunziza, became President. Despite the appointment of a Hutu exponent as head of the country, the FNL-Palipehutu continued to fight the government, definitively transforming what had been an inter-ethnic war into an intra-Hutu struggle for power.[9] After a first attempt at a ceasefire agreement in 2006, the FNL-Palipehutu finally agreed to give up its weapons and turned into a political party in 2009. Minor administrative posts were attributed to the FNL leadership. As for the CNDD-FDD four year earlier, its combatants either joined the national army or benefited from the DDR program.

As part of a broader agenda of political decentralization, elections were scheduled in 2010, only few months after the epilogue of the civil war. Five consecutive ballots were organized, starting with the election of municipal representatives on May 24, 2010, followed by the presidential election on June 28, the parliamentary and senatorial elections at the end of July, and ending with the election of the hills' representatives in early September. Even if several opposition parties seemed confident in their success, the

[6]The airplane carrying the Burundian president Cyprien Ntaryamira and its Rwandan counterpart, Juvénal Habyarimana was shot down as it prepared to land in Kigali on April 6, 1994. This attack triggered the Rwandan Genocide.

[7]According to Van Acker (2015), "as much as political elites stirred and instrumentalized ethnic antagonism during the violent conflict, growing inter-elite trust and socialization in the atmosphere of the post-conflict "spirit of Arusha" trickled down beyond formal state institutions as a driver of political and social reconciliation." In line with the Arusha Agreement, the 2005 constitution guarantees 60/40 percent representation of Hutu and Tutsi ethnic groups in Parliament, in the government, in the local administration, and a 50/50 percent representation in the army. The president must appoint two vice-presidents, one Hutu and the other Tutsi.

[8]The National Council for the Defense of Democracy - Forces for the Defense of Democracy (Conseil National de Défense de la Démocratie - Forces de Défense de la Démocratie, or CNDD-FDD) and the National Forces of Liberation (Forces Nationales de Libération, or FNL-Palipehutu)

[9]See e.g. Palmans (2012), Vandeginste (2011, 2012, 2014) and International Crisis Group (2011).

FNL party was seen as the most serious opposition to the CNDD-FDD of the incumbent president Pierre Nkurunziza (International Crisis Group, 2011). Table 2.1 displays the names of main parties, their creation dates, their ethnic origin, their results at the 2010 municipal election and, for ex-rebels groups, their number of demobilized rebels.

Table 2.1: Parties and ex-rebel groups in post-war Burundi

Party	Year of creation	Ethnicity of leader	% of votes in 2010	Ex-rebel group	No. of demob. (share)	Correlation votes & share demob.
CNDD-FDD	1998	Hutu	64.03	Yes	6874 (39%)	0.59
FNL	1999	Hutu	14.15	Yes	6029 (34.2%)	0.57
UPRONA	1957	Tutsi	6.25	No		
FRODEBU	1986	Hutu	5.43	No		
MSD	2009	Tutsi	3.75	No		
UPD	2003	Hutu	2.21	No		
FRODEBU-Nyakuri	2008	Hutu	1.36	No		
CNDD	1994	Hutu	1.26	Yes	1372 (7.8%)	0.76
MRC	2001	Tutsi	0.62	No		
PALIPE-Agakiza	1980	Hutu	0.24	Yes	578 (3.3%)	0.02
FROLINA	1990	Hutu	0.20	Yes	540 (3.1%)	0.31
KAZE-FDD	2005	Hutu	0.00	Yes	361 (2%)	-0.04
FNL dissidents		Hutu	-	Yes	1594 (9%)	-
FNL Icanzo	2001	Hutu	-	Yes	278 (1.6%)	-

National Council for the Defense of Democracy-Forces for the Defense of Democracy, National Front for Liberation (FNL), Union for National Progress (UPRONA), Front for Democracy in Burundi (FRODEBU), Movement for Solidarity and Democracy (MSD), Union for Peace and Democracy-Zigamibanga (UPD), Front for Democracy in Burundi-Nyakuri (FRODEBU-Nyakuri), National Council for the Defense of Democracy (CNDD), Movement for the Rehabilitation of Citizens-Rurenzangemero (MRC), Party for the Liberation of People-Agakiza (PALIPE-Agakiza), National Liberation Front (FROLINA), Kaze-Forces for the Defense of Democracy (KAZE-FDD)

The pre-electoral climate was spoiled by numerous violent episodes, claims of intimidation and suspicions of fraud. In such a context, the CNDD-FDD party won the first municipal ballot outright, capturing 64% of the votes and 62% of the seats in municipal assemblies. The FNL ended up as the second largest force, with only 14% of the votes. FRODEBU and UPRONA obtained 5 and 6% of the votes, respectively. The international community recognized the electoral results to be free and fair. Nevertheless, the resounding defeat pushed the opposition parties to boycott the four following ballots, accusing Nkurunziza of massive fraud and irregularities (Helbig de Balzac et al., 2011; International Crisis Group, 2011; Vandeginste, 2012). The incumbent president, Nkurunziza, who was therefore the only candidate running for presidency, was re-elected with 95% of the votes. Violence continued to be pervasive until the end of the electoral process.

The 2010 elections legitimized a quasi-return to the single-party rule, with the CNDD-FDD having obtained a three-quarters majority in the National Assembly.[10] The tensions that followed, however, were political, rather than ethnic, and lead to the resurgence of rebel groups - among whom were the FNL- aiming to fight the government (International Crisis Group, 2012; Van Acker, 2015). Many opposition leaders left the country after complaining about constant harassment and threats on their lives. Several of those who

[10]Despite the boycott, two parties decided to take part in the legislative elections in addition to the CNDD-FDD: Frodebu Nyakuri and UPRONA. They could send deputies at the National Assembly (Helbig de Balzac et al., 2011).

remained politically active were arrested or assassinated. Media and civil society were threatened, increasing the risk of instability and insecurity (Vandeginste, 2012). In this volatile context, the country went through a new electoral round in 2015.[11]

2.2 Conceptual framework

This chapter aims at understanding the causes of violence that perturbed the electoral process in Burundi in 2010. We test whether electoral violence was driven by (1) ethnic composition and ethnic grievances, (2) political competition or (3) the presence and the distribution of "specialists in violence." This section draws on the literature on conflicts and electoral violence to explain why these channels could be relevant in triggering electoral violence in Burundi.[12]

The literature on conflict has shown that ethnic distribution is likely to be correlated with violence. As explained in the previous section, the post-colonial history of Burundi has been plagued by ethnic tensions between the Hutu and the Tutsi. This inter-ethnic rivalry eventually triggered a bloody civil war in the nineties, partially resolved with the Arusha Peace Agreements. It is reasonable to assume that such deeply rooted ethnic cleavages survived the democratic transition and were exploited by the candidates during their electoral campaigns (Eifert et al., 2010; Wilkinson, 2004).

Two mechanisms could be at play. First, the theoretical literature has shown that violence is more likely to emerge if the distribution of ethnic groups is either polarized or fractionalized (Esteban and Ray, 2008, 2011). The ethnic distribution is said to be polarized if there are only two groups of similar size, and fractionalized if there are many groups of similar importance. Empirical evidence is mixed. While Garcia-Montalvo and Reynal-Querol (2005); Montalvo and Reynal-Querol (2005) argue that ethnic polarization best captures the likelihood of conflict, Schneider and Wiesehomeier (2008) and Esteban et al. (2012a,b) find that both ethnic fractionalization and ethnic polarization are conducive to conflict, depending on the political regime and the nature of the conflict.

However, a second mechanism could also be at play. Since around 85% of the population in Burundi is of the Hutu ethnic origin, a Hutu president is expected to rule the country after the elections.[13] Given the high stakes involved, electoral violence could be more likely in areas where the Hutu are the majority. This is also in line with the fact that the civil war in Burundi evolved from an inter-ethnic conflict to an intra-

[11] On April 2015, the CNDD-FDD designated incumbent President Pierre Nkurunziza as its candidate for the forthcoming presidential elections, despite the fact that he had already completed the two terms in office allowed by the constitution. The announcement triggered demonstrations and violence, a failed coup d'etat, and repeated delays of the elections. It should be noted, however, that the political opposition to President Nkurunziza's third mandate transcended ethnic boundaries. The plotters of the failed coup d'etat, for instance, were a Hutu and former CNDD-FDD rebel, and a Tutsi and former minister of defense under President Pierre Buyoya. This anecdotal evidence reinforces a key argument of this chapter: since 2005, identity politics in Burundi has unfolded along political partisanship rather than ethnic ties (Vandeginste, 2015).

[12] The literature supports the pivotal role of these three channels in triggering electoral violence. However, the three hypotheses do not provide an exhaustive list of possible channels of electoral violence.

[13] Palmans (2012) and Vandeginste (2011) report that *"for the majority of the population (an estimated 85% of whom are Hutu) legitimacy goes hand in hand with an ethnically representative leader."*

hutu struggle for power. On the contrary, Tutsi parties and Tutsi municipalities are less likely to be key in determining who would win the elections, making them less prone to engaging in violence.

We consider two indicators to capture the two mechanisms: an indicator of ethnic fractionalization[14] and the proportion of Hutu. If the first mechanism prevails, ethnic fractionalization should be positively correlated with violence. If the second mechanism dominates, violence should be positively correlated with the proportion of Hutu.

We test political competition as a second potential channel of electoral violence. As explained in the historical review, the Arusha Agreements institutionalized power sharing across ethnicities and imposed ethnic quotas in political parties. The 2010 elections might therefore have been spoiled by intense political competition, rather than by ethnic tensions. As for ethnicity, two mechanisms may be at play. On one hand, parties might engage in violence if the political context is heavily fragmented or polarized. In these situations, violence or intimidation could indeed be beneficial in gaining a small political advantage, which can ultimately be decisive for winning the elections (Sterck, 2015). On the other hand, the theoretical models of Collier and Vicente (2012) and Chaturvedi (2005) conclude that violent campaigning is used by a weak party to discourage the supporters of its rival from voting. In their framework, violent campaigning increases in the initial support for the rival. These two mechanisms lead to very different predictions regarding political competition, which we measure by polarization and fractionalization indexes. If the first mechanism is stronger, political competition should be positively correlated with electoral violence. On the contrary, political competition should be negatively correlated with electoral violence if the second mechanism prevails.

As for the third channel of electoral violence examined in this chapter, we study the role played by "hardcore supporters" of parties or "specialists in violence." According to Chaturvedi (2005), Collier and Vicente (2012) and Sterck (2015), conflictual campaigning is more effective in the presence of numerous "specialists in violence." In the context of post-conflict Burundi, demobilized ex-combatants are likely to play this role. This is plausible for at least three reasons. After the civil war, most rebel groups turned into political parties, and the literature suggests that demobilized soldiers are active in the post-war political life of their country (Annan et al., 2011; Gilligan et al., 2013; Goose and Smyth, 1994). Demobilized ex-combatants might also be more likely to engage in violence given their past histories. Finally, in a context of widespread poverty and unemployment, they may have higher incentives to support their former leader (Human Rights Watch, 2010). We therefore test if the number and the distribution of demobilized ex-rebels is correlated with electoral violence. However, given the limitation of our data, we are not be able to determine when violence is used to gain electoral advantage, and when it is the mere consequence of frustrations among demobilized rebels that are resurfacing during the elections. We measure the distribution of former combatants by using indexes of polarization and fractionalization of demobilized combatants.

While the literature agrees that the distribution of religious, ethnic or political groups

[14]There are only two main ethnic groups in Burundi: the Hutu and the Tutsi. The third ethnic group, the Twa, represents only 1% of the population. In the analysis, we only consider Hutu and Tutsi, implying that ethnic fractionalization and polarization indexes are proportional and hence perfectly multicollinear. Even if we would consider the Twa, it would not solve the multicollinearity problem as this group is marginally represented.

has an impact on the likelihood of conflict, it has not reached a consensus on whether conflict is the result of fractionalization or polarization.[15] In this chapter, we therefore compare the predictive power of indexes of fractionalization and polarization related to political parties and demobilized rebel groups. The recent findings of Esteban and Ray (2011) and Esteban et al. (2012a,b) are however worth emphasizing. They show, theoretically and empirically, that polarization is more conducive of violence than fractionalization *"when the winners enjoy a public prize,"* such as political power or religious hegemony. The contrary is true when the prize is "private," such as looted resources.

2.3 Identification strategy

In order to test the three hypotheses, we estimate the following model:

$$\begin{aligned} \text{Violent episodes}_m = \alpha_m &+ \beta_1 \text{ Hutu share}_m + \beta_2 \text{ ethnic frac.}_m \\ &+ \phi_1 \text{ political pol.}_m + \phi_2 \text{ political frac.}_m \\ &+ \gamma_1 \text{demob. rebels' pol.}_m + \gamma_2 \text{ demob. rebels' frac.}_m \\ &+ \gamma_3 \text{ number demob. rebels'}_m + \mathbf{X}'_m\delta + Z_k + \epsilon_m, \end{aligned} \tag{2.1}$$

where *Violent episodes*$_m$ is the number of episodes of electoral violence that occurred in each municipality $m \in [1, 129]$. *Hutu share*$_m$ and *ethnic frac.*$_m$ capture the ethnic distribution between Hutu and Tutsi. Political competition is measured by fractionalization and polarization indexes based on the result of the 2010 municipal elections (*political frac.*$_m$ and *political pol.*$_m$). *Demob. rebels' frac.*$_m$ and *demob. rebels' pol.*$_m$ are indexes of fractionalization and polarization between demobilized rebels at the municipal level. *Number demob. rebels'*$_m$ captures the number of demobilized rebels who returned to the municipality per 1000 inhabitants. $\mathbf{X}_m$ is a vector of covariates which includes a median wealth index, population, population density and past violence[16]. Z_k are fixed effects. Summary statistics are shown in Table 2.2.

2.3.1 Data

Electoral violence. The measure of electoral violence is constructed using the Burundi *Ushahidi* electoral violence dataset. The *Ushahidi* ("testimony") software was developed to map reports of violence in Kenya after the post-election fallout in 2007-2008. It was then adapted to the Burundian context through the *Amatora mu Mahoro* ("Elections in Peace") project. It also draws on the Elections Violence Education and Resolution (EVER) project which gathers information on incidents of violence and peace activities and has been conducted in a dozen countries since 2003.

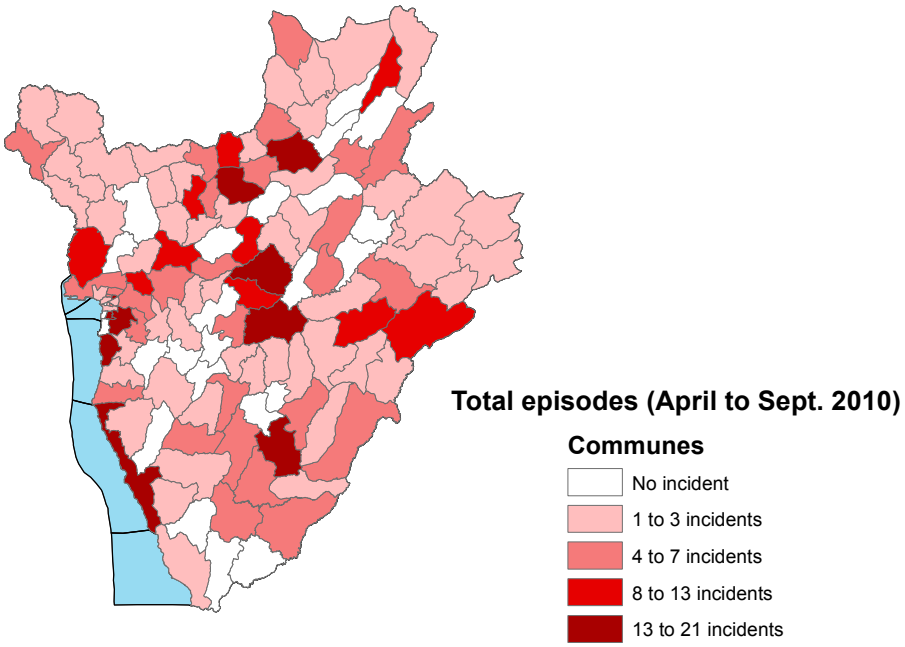
[15] Most of the theoretical literature has been written by Joan Esteban and Debraj Ray (see Esteban and Ray (2008, 2011) for recent examples comparing fractionalization and polarization indexes). See e.g. Esteban and Ray (2008), Schneider and Wiesehomeier (2008) and Blattman and Miguel (2010) for reviews of the literature. See Garcia-Montalvo and Reynal-Querol (2005); Montalvo and Reynal-Querol (2005), Schneider and Wiesehomeier (2008) and Esteban et al. (2012a,b) for contradictory evidence on the effect of ethnic polarization and ethnic fractionalization on conflict.

[16] Past violence, population and population density are expressed in log given their high dispersion. The indexes of ethnic, political and ex-soldiers' fractionalization, the indexes of political and demobilized combatants' polarization, as well as the wealth index are standardized.

During the 2010 electoral process in Burundi, 450 well-vetted and trained monitors - on average 3.5 per municipality - had to verify and provide detailed information about violent incidents.[17] The procedure for reporting and registering such incidents involved three steps. First, election monitors witnessing a violent episode had to signal it by sending an SMS to the Ushahidi platform. After receiving the SMS, Ushahidi agents had to call the election monitors back to obtain more information about the incident. A detailed report was then written for the Ushaidi website. Finally, election monitors were requested to submit a form with detailed information on the timing, the location and type of incident within one week after the episode.[18] To ensure information accuracy, incidents had to be reported by at least two different sources to be recorded in the database.

Information about physical violence, destruction of property, clashes between groups and intimidation during the electoral cycle were recorded between April, 26 and September 12, 2010. In the majority of cases, monitors had been unable to identify the perpetrator. This is particularly true for cases of murder or attempted murder (n=67) and cases of destruction (n=62). In 25 municipalities no episodes of electoral violence were signaled. The dependent variable in our analysis is the total number of violent episodes that occurred at the municipal level. Descriptive statistics are presented in Table 2.2. The geographical distribution of the total number of episodes is additionally presented in Figure 2.1. We do not observe evidence of spatial correlation in the dependent variable.

Figure 2.1: Distribution of electoral violence



[17] IFES endorsed the *"Declaraction of Global Principles for Non-Partisan Election Observation and Monitoring By Citizen Organizations"* and of the *"Code of Conduct for Non-Partisan Citizen Election Observers and Monitors"* which require the highest ethical standards for impartiality and accuracy in the witnessing, reporting and analysis of election processes and political environment.

[18] See NDI (2014) for more details on the methodology.

Table 2.2: Summary Statistics

	Mean	St. Dev.	Min	Median	Max	N
Violent episodes	4.02	4.52	0	3	21	129
Demobilized rebels						
Demobilized rebels' polarization	0.58	0.18	0	0.60	1	129
Demobilized rebels' fractionalization	0.54	0.16	0	0.58	0.78	129
Total no. demobilized rebels (/1000)	2.13	2.13	0.14	1.43	12.72	129
CNDD-FDD	53.29	61.08	0	35	350	129
CNDD	10.64	35.76	0	2	348	129
Palipe Agazika	4.48	11.66	0	0	74	129
Frolina	4.19	20.09	0	0	208	129
KAZE-FDD	2.80	6.23	0	1	58	129
FNL Icanzo	2.16	8.16	0	0	74	129
FNL - Rwasa	46.74	55.61	1	29	275	129
FNL Dissidents	12.36	37.42	0	3	326	129
Ethnic cleavages						
Hutu share	0.81	0.14	0.47	0.84	0.98	129
Ethnic fractionalization	0.27	0.14	0.03	0.27	0.50	129
Political competition						
2010 Political fractionalization	0.50	0.18	0.15	0.47	0.81	129
2010 Political polarization	0.44	0.13	0.15	0.43	0.87	129
2005 Political fractionalization	0.48	0.18	0.08	0.49	0.83	129
2005 Political polarization	0.48	0.17	0.08	0.50	0.92	129
Other covariates						
Median Wealth Index	-15144	91514	-55170	-41004	545315	128
Total violence (1997-2009)	20.40	37.68	0	9	357	129
Attacks on civilians (1997-2009)	10.97	21.09	0	3	177	129
Battles (1997-2009)	9.43	18.12	0	5	180	129
Population (2008 census)	62431	26454	17481	57284	155005	129
Population Density	1235	4174	72.34	352	33831	129

Ethnic cleavages. Ethnicity is a sensitive matter. It is therefore challenging to obtain ethnic data either because it is no longer collected or publicly released. According to the Belgian census of 1959, three ethnic groups coexist in Burundi: the Hutu (85%), the Tutsi (14%) and the Twa (1%). Data from this census is only available at the national level.

The 2012 Afrobarometer survey did, however, collect and release data on ethnicity in Burundi. The survey is representative at the province level. We thus proxy ethnic composition by taking the average proportion of Hutu at the province level.[19] Descriptive

[19]The 2012 Afrobarometer survey has data available for 111 out of 129 municipalities. In each municipality, between 8 and 32 individuals were interviewed. Given the low number of individuals interviewed in sampled municipalities, an indicator of ethnicity computed at the municipal level would suffer from a mismeasurement problem, leading to attenuation bias (Hausman, 2001).

statistics in Table 2.2 show that the Afrobarometer figure for the proportion of Hutu - 81% - is close to the 85% reported in the 1959 Belgian census.

We use two indicators in order to capture ethnic tensions. First, we consider the proportion of Hutu itself. Second, we construct an index of ethnic fractionalization following Alesina et al. (2003):

$$\text{Ethnic Fractionalization}_m = \sum_{i=1}^{N} (1 - \pi_i)\, \pi_i \tag{2.2}$$

where π_i is the proportion of people belonging to ethnic group i. The index of ethnic fractionalization can simply be interpreted as the probability that two randomly selected individuals from a given municipality belong to different ethnic groups.[20]

Political competition. We construct an index of political fractionalization and an index of political polarization to capture the intensity of political competition at the municipal level. The index of political fractionalization is constructed according to equation (2.2) and can be interpreted as the probability that two randomly selected individuals from a given municipality had voted for a different party in the municipal elections.

For the index of political polarization, we use the index of polarization derived by Sterck (2015): [21]

$$\text{Political polarization}_m = 1 - \sum_{i=1}^{N} \left| \frac{0.5 - \pi_i}{0.5} \right| \pi_i \tag{2.3}$$

where π_i is the proportion of votes obtained by each party. The index captures how far the political distribution is from being bipolar, with *Political polarization*$_m = 1$ indicating a bipolar political scenario.

To construct these indexes, we use electoral results from the 2005 and 2010 municipal elections. With data from the 2010 elections, estimates could potentially suffer from a reverse causality bias, as the occurrence of electoral violence before the municipal election may have affected voting behavior - and hence indexes of political competition - in a non-random way. We therefore rely on the data from the 2005 elections in our benchmark regressions. Nonetheless, we show that results are not significantly affected when political competition is measured using the results of the 2010 municipal elections.

Demobilized rebels' fractionalization, polarization and number. We construct fractionalization – Equation (2.2) – and polarization – Equation (2.3) – indexes based on ex-rebels' affiliations. We use data from official registers containing information on the

[20] In our sample, N=2. Because $\pi_i = (1 - \pi_j)$, the formula becomes: ethnic frac. $= (1 - \pi_i)\, \pi_i + (1 - \pi_j)\, \pi_j = (1 - \pi_i)\, \pi_i + (1 - (1 - \pi_i))\, (1 - \pi_i) = 2(1 - \pi_i)\pi_i$.

[21] In the case of two parties 1 and 2 with voting shares π_1 and π_2 such that $\pi_1 + \pi_2 = 1$, elections are defined as close if π_1 and π_2 are sufficiently close to 50%, that is, if $|0.5 - \pi_1| = |0.5 - \pi_2| < \epsilon$. This is equivalent to: $\left|\frac{0.5-\pi_1}{0.5}\right| < 2\epsilon \Leftrightarrow \left|\frac{0.5-\pi_1}{0.5}\right| \pi_1 + \left|\frac{0.5-\pi_1}{0.5}\right| (1 - \pi_1) < 2\epsilon \Leftrightarrow \left|\frac{0.5-\pi_1}{0.5}\right| \pi_1 + \left|\frac{0.5-\pi_2}{0.5}\right| \pi_2 < 2\epsilon$ $\Leftrightarrow 1 - \sum_{i=1,2} \left|\frac{0.5-\pi_i}{0.5}\right| \pi_i > 1 - 2\epsilon$. The left-hand side of the latter inequality is the indicator of polarization proposed by Sterck (2015). Similar results are obtained with the Garcia-Montalvo and Reynal-Querol (2005) index of ethnic polarization, which consider the quadratic value of the term in the sum rather than its absolute value (results available upon request).

return of approximately 30,000 combatants from 10 armed groups demobilized between 2004 and 2009.[22] We also control for the number of demobilized rebels per municipality per 1000 inhabitants.

Most of the rebels were demobilized from the traditionally Hutu CNDD-FDD, led by the incumbent President, Pierre Nkurunziza (12,000 demobilized soldiers).[23] The second largest group was the FNL-Palipehutu, whose leader is Agathon Rwasa, Nkurunziza's main opponent. The remaining 4,500 demobilized ex-combatants are shared among the remaining six Hutu rebel groups.

Other covariates. We compute a median wealth index for each municipality from the household data from the 2010 Demographic and Health Survey (DHS).[24] The DHS wealth index uses information on the household's ownership of assets (e.g. bicycle and radios), environmental conditions and housing characteristics (e.g. type of water source, sanitation facilities, materials used for housing construction) and uses a principal components analysis to assign weights to the different components of the index (Rutstein and Johnson, 2004). We control for the history of violence experienced by the municipalities from 1997 to 2009 by relying on the Armed Conflict Location & Event Data (ACLED) dataset. ACLED contains records on 2669 events, among which are battles and attacks against civilians[25] that occurred throughout Burundi from 1997 to 2009 (Raleigh et al., 2010). Population size comes from the last available census, conducted in 2008 by the *Institut de Statistiques et d'Etudes Economiques du Burundi* (ISTEEBU).

2.3.2 Estimation method

Given the count and non-normal nature characterizing the occurrence of electoral violence, Hilbe (2011) recommends estimating equation (2.1) with Poisson or Negative Binomial models. A Poisson distribution assumes that the mean and the conditional variance of the dependent variable are equal. This assumption does not hold for the distribution of violent episodes. Its conditional variance is 20.41, which is five times higher than its conditional mean (4.02), indicating over-dispersion in the data. This diagnosis is confirmed by chi-square goodness-of-fit tests, which strongly reject the null hypothesis that the data follow a Poisson distribution (p-value = 0.00). Given strong over-dispersion in the dependent variable, we opt for a negative binomial regression model (Hilbe, 2011). The model specification is validated by Pearson's dispersion tests and link tests. Figure 2.2 plots the distribution of violent episodes against a Poisson distribution and a Negative Binomial distribution with the same mean and variance. It further confirms how the

[22] The National Commission for Demobilization, Reinsertion and Reintegration kindly shared the data.

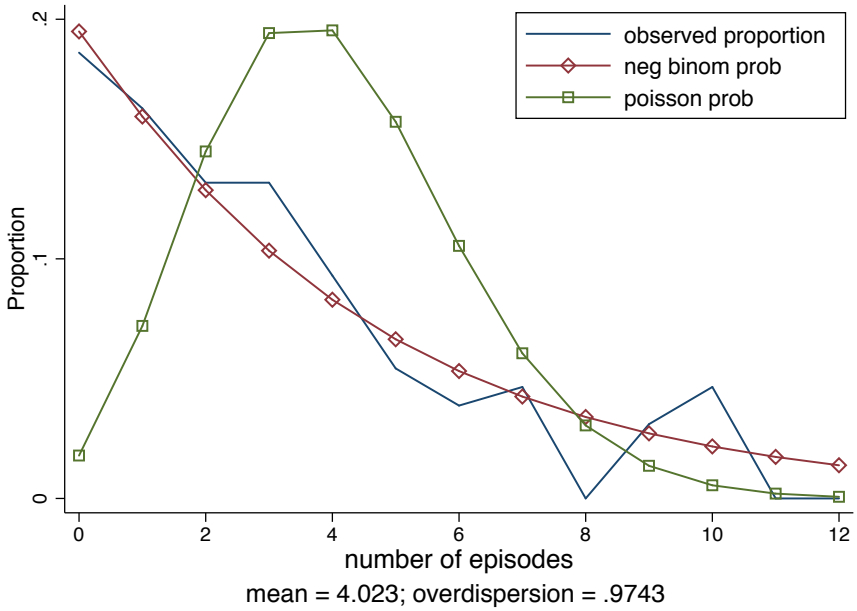
[23] It should be noted that most of the demobilized soldiers come from the former national army (FAB) and the current national forces of defense (FDN) (13,000 demobilized soldiers). Nevertheless, since FAB did not turn into a political party and its soldiers may be affiliated to different political group, we exclude these demobilized soldiers when we construct indexes of polarization and fractionalization. Controlling for the demobilization of FAB and FDN does not significantly affect the results (results available upon request).

[24] The survey was conducted in 128 municipalities (out of 129). Sampling weights are accounted for.

[25] Battles are *"violent interaction between two politically organized armed groups at a particular time and location"*. We exclude non-violent events, riots and protests.

latter performs better than the former in explaining the data on electoral violence. OLS and Poisson estimates give similar results.[26]

Figure 2.2: Goodness of fit of Negative Binomial and Poisson models



We undertake a matching exercise between neighboring municipalities to minimize the risk of omitted variable bias. We do so by constructing a database that identifies each neighborhood of municipalities by a dummy variable, which we then include as fixed effects in the regressions. These "geographic", or "tight", fixed effects should capture unobserved characteristics common among neighboring municipalities. This approach relies on the assumption that neighboring municipalities are likely to be similar.[27] This assumption is valid if unobservable characteristics are geographically distributed and if borders between municipalities are exogenous enough to ensure no important differences between unobservable characteristics of neighboring municipalities (Huillery, 2009).

We rely on three definitions of neighborhood to compute the fixed effects that are included in the model.[28]

All pairs. We built a new dataset by matching each municipality with each of its neighbors. Municipalities have between 2 and 10 neighbors (mean = 5.44 neighbors). This dataset contains 1404 observations, i.e. 702 pairs of neighbors.[29] Each pair is identified by a dummy. We then estimate equation (2.1) by including pair fixed effects.

[26] Results are presented in the Online Appendix of Colombo et al. (2019).

[27] Neighboring is defined as sharing a common border.

[28] Our approach is close to Naidu (2012) and Goldstein and Udry (2008) who take the difference of the variables from the mean of the adjacent administrative units. In our case, the differentiation would lead to negative outcome values, which cannot be fitted by a negative binomial regression.

[29] Data on wealth is missing for one municipality, which has 8 neighbors, implying that regressions controlling for wealth and with pairs fixed effects rely on 1388 observations. Pairs of observations in 112 of 702 clusters (7.98%) are characterized by the same number of violent episodes. For these

Since municipalities have many neighbors and are neighbors of many other municipalities, standard errors are clustered at three levels (Cameron et al., 2011). The first level is the neighborhood. The second level accounts for the fact that each municipality may be the neighbor of several other municipalities. The third level captures the fact that municipalities have duplicates in the sample.

Neighborhood. We define the neighborhood of municipality as its set of neighbors plus the municipality itself. We built a dataset listing all municipalities of the 129 neighborhoods; each neighborhood being then identified by a dummy variable. This new dataset contains 831 lines.[30] We then re-estimate equation (2.1) by including these neighborhood fixed effects. The estimation of this specification requires clustering standard errors at two levels to account for the fact that municipalities have multiple neighbors and are neighbors of multiple municipalities.

Random pairs. The third method uses the same sample as the "all pairs" fixed effects method. However, instead of considering all pairs of neighbors simultaneously, we constituted a subsample of pairs by matching each municipality with one of its neighbors, selected randomly. We then estimate equation (2.1) using this sample of 2×129 observations.[31] To avoid the effect being driven by particular neighborhood designs, we repeate the sampling procedure and re-estimate the regression 200 times. We report the average of estimated coefficients and standard errors. As the same municipality can appear within different pairs, we clustered standard errors at the municipal level.

In Tables 2.10 and 2.11 in Appendix, we show that the inclusion of "geographic" fixed effects improves the balance on observables. We compare regressions of control variables on variables of interest with and without fixed effects matching. The absolute value of regression coefficients either decreases or is not significantly affected by the inclusion of fixed effects.[32] Even if the balance remains imperfect after fixed effect matching (some coefficients remain significant), which is not surprising given the absence of randomized treatment, these results show that municipalities with different "treatment intensity" look more similar with "geographic" fixed effects.[33]

observations, electoral violence is fully explained by the fixed effects. The intra-cluster correlation is not statistically different from 0, implying that fixed effects do not capture all the variation in the dependent variable.

[30]There are 129 municipalities having on average 5.44 neighbors. $129 + (5.44 \times 129) = 831$. Data on wealth is missing for one municipality, which has 8 neighbors, implying that regressions controlling for wealth with neighborhood fixed effects rely on 822 observations. In all neighborhoods, there is variation in the number of violent episodes. The intra-cluster correlation is not statistically different from 0, implying that fixed effects only capture a small proportion of the variation in the dependent variable.

[31]Data on wealth is missing for one municipality, implying that regressions controlling for wealth with random pairs fixed effects rely on 128 observations.

[32]The balance is improved with fixed effects (that is, the absolute value of coefficients is reduced) for 77% of coefficients tested, although the difference is significant only for 7% of coefficients. For 23% of coefficients, the absolute value of coefficients increases but not significantly. While 68% of coefficients are significant at conventional levels without fixed effects matching, this proportion is reduced to 35% thanks to fixed effects.

[33]We reproduced maps that show spatial correlation across municipalities in terms of the quality and composition of their soil. Such a geographic correlation further justifies the use of "geographic" fixed effects. Maps are available upon request.

2.3.3 Causal chain and bad controls

The three hypotheses are related to each other in the context of Burundi. As shown in Table 2.1, most political parties and rebel groups are ethnically rooted, and most parties are issued from demobilized rebel groups. Nevertheless, the three hypotheses capture different aspects of the political environment in Burundi that should be disentangled in the empirical analysis. The correlation between electoral results of former rebel groups and their share of demobilized rebels is high but far from perfect. Many parties have no demobilized ex-combatants and their ethnic orientations are heterogeneous. This implies that the correlations between our variables of interest are rather limited. For example, the correlation between political fractionalization and the proportion of Hutu at the municipal level is equal to -0.42. The correlation between political fractionalization and demobilized rebels' fractionalization is only equal to 0.01, and only 0.09 between political polarization and demobilized rebels' polarization (Figure 2.4 in Appendix). The three hypotheses are therefore distinguished in the empirical analysis.

It is interesting to test the three hypotheses simultaneously as well as separately. Testing all hypotheses simultaneously limits the risk of omitted variable bias. However, it may also induce a problem of bad controls (Angrist and Pischke, 2008): a variable of interest X_i may appear insignificant if another variable included in the regression, X_j, is in fact a channel of the impact of X_i rather than being a primary factor of violence. If hypotheses are tested separately, both variables X_i and X_j will appear to be significant. In this case, theoretical reasoning is needed to determine whether the correlation between X_i and electoral violence when X_j is not controlled for is due to omitted variable bias, or whether X_i is not significant when all hypotheses are tested simultaneously because X_j is actually a channel of the impact of X_i on electoral violence, that is, a bad control. In what follows, we test the three hypotheses separately and simultaneously, and compared regression coefficients across specifications.

We control for past violence and wealth in equation (2.1). These variables could also be considered as "bad controls." Violence during the civil war, for instance, was partly driven by ethnic grievances. If past violence triggered electoral violence in 2010, it could therefore be considered as a channel, or a bad control. Likewise, wealth, which could explain electoral violence, may have been affected by ethnic distribution. We therefore face a trade-off: on the one hand, the inclusion of past violence and wealth in the regressions may attenuate the impact of variables of interest; on the other hand, removing these variables from the list of controls could induce an omitted variable bias. We show that the problems from omitted variable bias and bad controls are marginal in our case study. The results do not significantly change when past violence and wealth are included in or removed from the list of controls.

2.4 Results

In Section 2.4.1, we examine if electoral violence is correlated with ethnic composition, political competition and the distribution of demobilized rebels. In Section 2.4.2, we study how these factors interact together to favor the emergence of electoral violence.

2.4.1 Benchmark results

In columns (1) to (8) of Table 2.3, we explore one-by-one the three hypotheses which may explain why violence spoiled the 2010 elections in Burundi. In columns (9) and (10), the three hypotheses are tested simultaneously.[34] Neighborhood fixed effects are included in these specifications.[35] Control variables include the number of demobilized combatants, population size and population density. In Table 2.4, we show that results are robust to the different types of fixed effects. We also show that controlling for past violence and for the wealth index do not significantly change the results, suggesting that the problem of "bad controls" is marginal for these variables.

[34] As mentioned in the conceptual framework, these three hypotheses do not provide an exhaustive list of possible channels of electoral violence. As a matter of fact, Tables 2.8 and 2.9 in Appendix show that the adjusted R-squared of OLS regressions ranges between 0.08 and 0.19 without fixed effects (between 0.21 and 0.33 with fixed effects). Such results show that our hypotheses are not exhaustive and part of the variation is left unexplained. The R-squared are nonetheless in line with those of similar papers (see e.g. Garcia-Montalvo and Reynal-Querol (2005), in which pseudo R-squared range between 0.09 and 0.13).

[35] For the sake of brevity, we focus on neighborhood fixed effects, as this method leads to more conservative estimates, and no cluster of municipalities is excluded.

Table 2.3: Testing hypotheses separately and simultaneously

Dependent variable: total episodes of electoral violence

	Ethnic grievances		Political competition				Demob. rebels		All channels	
			2010		2005					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Hutu share 2012	1.212								2.074*	3.060***
	(0.822)								(1.129)	(1.071)
Ethnic fractionalization 2012 (st.)		-0.098							-0.024	0.139
		(0.104)							(0.130)	(0.134)
Political fractionalization 2010 (st.)			0.243**						0.100	
			(0.108)						(0.120)	
Political polarization 2010 (st.)				0.305***					0.264***	
				(0.089)					(0.099)	
Political fractionalization 2005 (st.)					0.192**					0.207
					(0.080)					(0.141)
Political polarization 2005 (st.)						0.225***				0.129
						(0.083)				(0.137)
Demob. rebels' polarization (st.)							0.217***		0.257**	0.342***
							(0.083)		(0.110)	(0.109)
Demob. rebels' fractionalization (st.)								0.095	-0.105	-0.209*
								(0.083)	(0.116)	(0.123)
Observations	831	831	831	831	831	831	831	831	831	831
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	NFE	NFE	NFE	NFE	NFE	NFE	NFE	NFE	NFE	NFE

Negative binomial regressions. Controls include the number of demobilized rebels per 1000 inhabitants, the $\log$ of population size, and the $\log$ of population density. All estimations include neighborhood fixed effects (NFE). Standard errors are robust and clustered at two levels as described in Section 2.3.2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In columns (1) and (2) of Table 2.3, we assess whether the ethnic composition of municipalities is correlated with the occurrence of electoral violence. From column (1), we see that the share of Hutu is positively correlated with electoral violence. The coefficient is large, but the relationship is not significant at conventional levels (p-value=0.14). As shown in columns (9) and (10), the coefficient becomes larger and significant when controlling for political competition. Indeed, the coefficient associated with the proportion of Hutu is likely to be downward biased when political fractionalization is omitted from the list of independent variables, as political fractionalization is positively correlated with electoral violence and negatively correlated with the proportion of Hutu.[36] Ethnic diversity, measured by ethnic fractionalization, is not correlated with electoral violence.

The relevance of political competition is examined in columns (3) to (6) of Table 2.3. Given the aforementioned risk of reverse causality, we compare indexes of political competition based on the results of the 2010 and 2005 municipal elections. Both political fractionalization and polarization have a positive and significant effect on electoral violence when they are considered separately. If both indicators are included in the regressions – columns (9) and (10) –, the polarization index is significant when political competition is build on 2010 electoral data, while the fractionalization index captures most of the effect when political competition is measured using 2005 electoral data. These ambiguous results can be explained by the multicollinearity between political polarization and fractionalization: both indicators capture the same mechanism in this case study. Multicollinearity is not surprising given the high correlation between political fractionalization and polarization (ρ =0.65). The two variables are in fact very similar when fractionalization is below 0.6, which is the case for more than 75% of the observations. Interestingly, Alesina et al. (2003) faced the same issue with indexes of ethnic, linguistic and religious fractionalization and polarization. In what follows, we focus on the political fractionalization index using 2005 data to mitigate multicollinearity and reverse causality.[37] Similar results are however obtained with the indicator of political polarization and when political competition indicators are constructed using data from the 2010 elections.[38]

Columns (7) to (8) of Table 2.3 examine whether the distribution of demobilized rebels is correlated with electoral violence in Burundi. The coefficient associated with the polarization index is positive and significant at the 1% threshold. On the contrary, the coefficient associated with the rebels' fractionalization index is small and not significant. Similar results are obtained when the three hypotheses are tested simultaneously – columns (9) to (10). Based on column (10), an increase of one standard deviation in demobilized rebels' polarization induces a 41% increase in the number of episodes of

[36] In places where the proportion of Hutu is low, the political landscape is more fragmented because both Hutu and Tutsi parties obtain a high share of votes (e.g. in Bujumbura Mairie or in the provinces of Cankuzo and Mwaro).

[37] We focus on fractionalization rather than polarization because it is the fractionalization index which captures most of the effect when political competition is measured in 2005. Fractionalization is also negatively correlated with the proportion of Hutu in municipalities; ignoring this indicator may therefore bias downward the coefficient associated the proportion of Hutu.

[38] Results are presented in the Online Appendix to the published version of this chapter.

electoral violence.[39] These results show that demobilized rebel groups were more likely to be involved in electoral violence when competing with another group of similar size.

The three hypotheses are jointly tested in Table 2.4 without fixed effects – column (1), with pair fixed effects – column (2), with neighborhood fixed effects – column (3) – and with random pairs fixed effects – column (4). Our findings are stronger when the three hypotheses are jointly tested, thereby minimizing the risk of omitted variable bias. In columns (5) to (8), the indicators of past violence and median wealth are included in the regressions to minimize omitted variable bias. Results are robust to these alternative specifications.

Table 2.4: Benchmark regressions - comparison of different sets of controls and different sorts of geographic fixed effects

Dependent variable: total episodes of electoral violence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Without violence and wealth				With violence and wealth			
Hutu share 2012	1.615*	3.721***	3.351***	3.924***	1.253	2.236**	2.290**	2.485*
	(0.973)	(1.173)	(1.103)	(1.299)	(0.920)	(1.140)	(1.151)	(1.291)
Ethnic fractionalization 2012 (st.)	0.012	0.227	0.172	0.263	-0.020	0.049	0.056	0.085
	(0.129)	(0.159)	(0.140)	(0.167)	(0.122)	(0.161)	(0.144)	(0.169)
Political fractionalization 2005 (st.)	0.165*	0.348***	0.320***	0.369***	0.080	0.271***	0.241***	0.290***
	(0.100)	(0.087)	(0.091)	(0.106)	(0.099)	(0.080)	(0.088)	(0.103)
Demob. rebels' polarization (st.)	0.287**	0.393***	0.357***	0.397***	0.415***	0.417***	0.384***	0.418***
	(0.112)	(0.108)	(0.112)	(0.129)	(0.133)	(0.108)	(0.111)	(0.128)
Demob. rebels' fractionalization (st.)	-0.349***	-0.198	-0.213*	-0.193	-0.293**	-0.173	-0.199	-0.173
	(0.130)	(0.121)	(0.123)	(0.142)	(0.143)	(0.114)	(0.123)	(0.140)
No. demob. rebels (/1000)	0.030	0.046	0.037	0.069	-0.061	-0.025	-0.029	-0.013
	(0.056)	(0.059)	(0.058)	(0.059)	(0.056)	(0.053)	(0.057)	(0.054)
Population (log)	0.955***	1.444***	1.409***	1.467***	0.771***	0.827***	0.897***	0.875***
	(0.224)	(0.196)	(0.199)	(0.224)	(0.227)	(0.208)	(0.216)	(0.237)
Population density (log)	0.284***	0.198	0.204	0.129	0.304***	0.251	0.261*	0.191
	(0.097)	(0.205)	(0.165)	(0.192)	(0.102)	(0.160)	(0.136)	(0.179)
Past violence (log)					0.340***	0.405***	0.387***	0.395***
					(0.080)	(0.096)	(0.089)	(0.093)
Median Wealth Index (st.)					0.068	-0.001	0.081	-0.034
					(0.128)	(0.113)	(0.120)	(0.140)
Observations	129	1404	831	258	128	1388	822	256
Fixed Effects	.	All	NFE	Random	.	All	NFE	Random

Negative binomial regressions. No fixed effects are signaled by a dot. "All" refers to "All pairs", "NFE" to Neighborhood Fixed Effects", and "Random" to random pairs. Standard errors are robust and clustered at the level relevant to the fixed effects included in the regression (as described in Section 2.3.2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

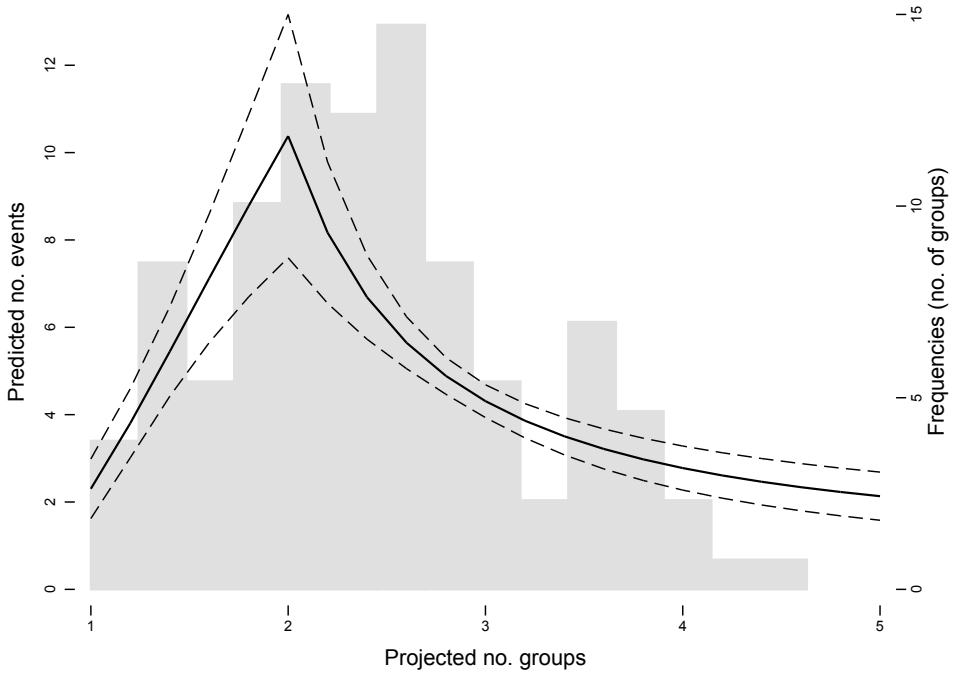
From Table 2.4, we conclude that ethnic grievances between Hutu and Tutsi did not cause electoral violence in 2010. Instead, violence was more likely to occur in municipalities characterized by a high proportion of Hutu. The coefficients associated with the share of Hutu are positive and large in all specifications. They are significant at the 5% threshold with all types of fixed effects but not significant at conventional thresholds in a simple cross-section regression when controlling for past violence and the median wealth index. Based on column (7), our model predicts an increase of about 5.3 violent events between the municipalities characterized by the lowest and the highest proportion of Hutu.

[39] To interpret the results of the negative binomial regressions in percentage terms, one should take $\exp^{\beta} - 1$.

Political competition is also correlated with electoral violence. In Table 2.4, political competition is captured by the index of political fractionalization in 2005. The coefficients associated with political fractionalization are positive and significant at the 1% level when fixed effects are included in the regression. With fixed effects, the predicted effect of a one standard deviation increase in political fractionalization on electoral violence ranges between 27% to 45%, depending on the specification.

The coefficients associated with demobilized rebels' polarization are positive and strongly significant in all specifications. The size of the effect is similar to that of political fractionalization. With fixed effects, the predicted effect of a one standard deviation increase in demobilized rebels' polarization on violent events ranges between 43% and 52%, depending on the specification. This effect is large, representing a fourfold increase in the incidence of events between the lowest- and the highest-polarized municipalities – considering column (7).

Figure 2.3: Predicted number of events in function of projected number of groups of the same size



It is important to consider the simultaneous effect of demobilized rebels' fractionalization and polarization since both indicators are constructed based on the distribution of rebel groups. Figure 2.3 shows the number of predicted violent episodes in each municipality as a function of the number of groups of similar size.[40] The predicted number

[40]Considering n groups of similar size, the index of demobilized rebels' fractionalization as calculated in equation (2.2) would collapse to demobilized rebels' fractionalization$_m = \sum_{i=1}^{N} (1 - \pi_i)\, \pi_i = 1 - \frac{1}{n}$. Consequently, the "projected" number of groups of equal size in each municipality is given by $n = \frac{1}{(1 - \text{frac})}$.

of episodes reaches its maximum when there are two groups of former rebels of similar size, i.e. when the distribution of demobilized rebels is polarized into two groups.

Importantly, the number of demobilized rebels that returned to the municipality is not associated with electoral violence. We conclude that the number of "hardcore supporters" does not directly explain the occurrence of electoral violence. Rather, electoral violence is more likely to occur in a municipality characterized by a bipolar distribution of demobilized rebel groups.

Table 2.4 also provides some evidence that past violence is positively correlated with the electoral violence in 2010. This relationship is in line with the reports from International Crisis Group (2011, 2012) which points out that, *"[...] rumors circulated from July about the presence of armed groups gradually settling themselves in Kibira forest, a traditional sanctuary for rebel movements.* These rumors where confirmed by households which reported looting, clashes between groups and attacks against the military.

The comparison between Tables 2.3 and 2.4 shows that ethnic composition, political competition and the distribution of demobilized rebels are separate factors of violence. The coefficients associated with the share of Hutu, political fractionalization and demobilized polarization are larger when the three hypotheses are tested simultaneously (although not significantly) and their p-values are lower, showing that these factors are not channels of impact or bad controls. The inclusion of past violence (and the index of median wealth) in the list of controls slightly reduces the coefficients associated with the share of Hutu and political fractionalization, although these differences are not statistically significant (Table 2.4). Overall, this suggests that the problem of bad controls is marginal in our study.

2.4.2 Heterogenous effects

In this section, we allow for interactions between the variables of interest.[41] The results are shown in Table 2.5. Marginal and total predicted effects are represented graphically in the Online Appendix to the published version of this chapter.

Demobilized rebels' polarization and proportion of Hutu. In line with the conceptual framework, it is intuitively plausible that polarization between demobilized rebel groups is a better predictor of electoral violence in pro-Hutu municipalities. The expected return from violence should indeed be higher in pro-Hutu municipalities as the Hutu are the majority and hence more likely to rule the country after the elections. We test this hypothesis by interacting demobilized rebels' polarization with the proportion of Hutu. We expect the marginal impact of the demobilized rebels' polarization index to be close to zero in Tutsi municipalities, and then to increase with the proportion of Hutu. Similarly, the marginal impact of the proportion of Hutu should be close to zero in municipalities that are not polarized, and increase with the demobilized rebels' polarization index. Results from column (1) of Table 2.5 confirm this intuition, although the coeffi-

[41] We also tested the interactions between variables of interest and past violence, of which none are significant at conventional thresholds. We conduct the same analysis with political competition constructed using data from the 2010 municipal election. Results are reported in the Online Appendix to the published version of this chapter.

cient of the interaction term is not significant at conventional thresholds. Demobilized rebels' polarization and the proportion of Hutu seem to be complementary explanations of electoral violence.

Table 2.5: Heterogenous effects

Dependent variable: total episodes of electoral violence

	(1)	(2)	(3)	(4)	(5)
Hutu share 2012	2.985**	2.695**	3.305***	3.661***	2.919**
	(1.214)	(1.220)	(1.104)	(1.076)	(1.240)
Ethnic fractionalization 2012 (st.)	0.154	0.090	0.192	0.220*	0.167
	(0.145)	(0.156)	(0.139)	(0.131)	(0.154)
Political fractionalization 2005 (st.)	0.311***	-0.719	0.285***	0.108	-0.500
	(0.091)	(0.548)	(0.093)	(0.109)	(0.473)
Demob. rebels' polarization (st.)	-0.144	0.353***	0.346***	0.372***	-0.058
	(0.610)	(0.111)	(0.107)	(0.110)	(0.605)
Demob. rebels' fractionalization (st.)	-0.233*	-0.220*	-0.211*	-0.228*	-0.245*
	(0.128)	(0.120)	(0.120)	(0.126)	(0.129)
No. demob. rebels (/1000)	0.034	0.021	0.026	0.097	0.069
	(0.057)	(0.054)	(0.057)	(0.060)	(0.063)
Population (log)	1.419***	1.434***	1.492***	1.326***	1.420***
	(0.202)	(0.204)	(0.204)	(0.199)	(0.210)
Population density (log)	0.170	0.181	0.171	0.248*	0.178
	(0.176)	(0.172)	(0.173)	(0.143)	(0.163)
Demob. rebels' polarization × Hutu share	0.619				0.514
	(0.761)				(0.759)
Political frac. × Hutu share		1.286*			0.741
		(0.681)			(0.611)
Demob. rebels' polarization × Political frac.			-0.118*		-0.089
			(0.063)		(0.061)
Demob. rebels (/1000) × Political frac.				0.127***	0.113**
				(0.046)	(0.048)
Observations	831	831	831	831	831
Fixed Effects	NFE	NFE	NFE	NFE	NFE

Negative binomial regressions. All estimations include neighborhood fixed effects (NFE). Standard errors are robust and clustered at two levels as described in Section 2.3.2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Political fractionalization and proportion of Hutu. A similar reasoning applies to the interaction between political fractionalization and the proportion of Hutu. The Hutu are the majority and have the most to gain or lose from electoral competition. It is therefore likely that the correlation between political competition and electoral violence will be higher in pro-Hutu municipalities and lower in pro-Tutsi municipalities. Results confirm this intuition: the coefficient of the interaction term in column (2) of Table 2.5 is positive and significant. Electoral violence is particularly high in Hutu strongholds where political competition is high, confirming the intra-Hutu dimension of post-war electoral competition.

Demobilized rebels' polarization and political fractionalization. In column (3) of Table 2.5, we investigate the extent to which the interaction between demobilized rebels' polarization and political fractionalization determines electoral violence. We have no prior assumptions about the sign of the interaction term. It could be positive if both factors are jointly required to generate electoral violence, or negative if only one of these factors is sufficient for causing violence. Results show that the coefficient associated with the interaction term is negative and significant. As shown in Figure 2.7 in Appendix, the total predicted effect of these two variables is similar if only demobilized rebel's polarization is high, if only political fractionalization is high or if both variables are high. Only one of these conditions is needed to increase the prevalence of electoral violence.

Number of demobilized rebels and political fractionalization. The literature suggests that the presence of "specialists in violence" or "hardcore supporters," such as demobilized rebels, is likely to increase the return from violent campaigning (Chaturvedi, 2005; Collier and Vicente, 2012). We therefore test whether the number of demobilized rebels and political competition are complementary explanations for electoral violence. In column (4) of Table 2.5, the coefficient associated with this interaction is positive and significant. The presence of demobilized rebels increases the likelihood of violence in places where political competition is intense.

2.5 Discussion

In this section, we further discuss the results and put them in perspective with the mechanisms identified in the conceptual framework. We then discuss whether electoral violence in 2010 is different from other types of violence. Finally, we show that our results are robust to alternative specifications.

2.5.1 Reconciling theoretical and empirical evidence

While the economic literature point to ethnic grievances in explaining violence, our results show that this explanation is not relevant for explaining electoral violence during the 2010 electoral process in Burundi. Violence rather emerged as a result of tensions between political parties and between demobilized rebel groups in pro-Hutu areas. As explained in the conceptual framework, the stakes of the election were indeed higher for Hutu parties, thereby generating higher incentives to engage in violent campaigning. Such outcomes were also described in the recent literature on the political landscape in Burundi, confirming the evolution from inter-ethnic conflict to an intra-Hutu competition for power.[42] Palmans (2012) writes *"unlike in 1993, electoral competition [in 2010] is no longer dominated by ethnic rivalry."* Similarly, Vandeginste (2014) reports that *"there is general agreement [...] that the 2010 Burundian elections were no longer a matter of primarily ethnic competition - which is quite an achievement in a country torn apart by politico-ethnic strife for decades."*

We also find that high political competition between parties increased the likelihood of electoral violence. Contrary to the theoretical predictions of Collier and Vicente (2012)

[42] See e.g. Palmans (2012), Vandeginste (2011, 2014) or International Crisis Group (2011).

and Chaturvedi (2005), illicit campaigning was not a strategy of weak parties against the strong ones during the 2010 elections in Burundi. Our results rather show that electoral violence emerged where electoral competition was tight, in line with the theoretical model of Sterck (2015). The effect of political competition is stronger in Hutu-dominated municipalities, showing that ethnic power-sharing, which has been institutionalized in Burundi through the Arusha peace agreement and the 2005 Constitution, *"contributed to reducing the ethnic divide in Burundian society which today is shaken by tensions based on what are essentially political cleavages within the Hutu majority"* (International Crisis Group, 2011).

Finally, we find that "hardcore supporters," or "specialists in violence," played a role in sparking violence, as suggested by Collier and Vicente (2012) and Chaturvedi (2005). Our empirical analysis identifies two mechanisms. First, electoral violence is more likely to emerge in the presence of two main groups of demobilized rebels of similar size. Second, the presence of numerous demobilized rebels amplifies the impact of political competition on violence. The instability due to the presence of demobilized rebels and their association with parties has been reported by observers and NGOs on the ground. Human Rights Watch (2010) reports that *"The presence of partisan youth groups adds to political volatility. A number of parties have made significant efforts to recruit demobilized combatants, raising concerns that such youth could easily be manipulated into carrying out acts of violence."* In addition, many demobilized combatants were the victims of violence (Amatora Mu Mahoro, 2010; Human Rights Watch, 2012).

In line with the work of Horowitz (1985), we find that tensions between demobilized rebel groups are better captured by an indicator of polarization between demobilized groups than by an indicator of fractionalization. This is also in accordance with Esteban et al. (2012b) who conclude that polarization is more important *"when the winners enjoy a public prize (such as political power or religious hegemony)."* Our study is not as conclusive about the relative performance of political fractionalization and polarization indexes.[43]

2.5.2 Is electoral violence different from other types of violence?

To answer this question, we undertake a falsification exercise in which we replace the dependent variable, electoral violence, by other variables which are related in nature but which are not expected to be affected by the same regressors of interest. Results are presented in Table 2.6. This exercise does not aim to uncover causal relationships, but rather to provide suggestive evidence that electoral violence is different from other types of violence.

In columns (1) and (2), the dependent variables are respectively the attacks against civilians and battles[44] which occurred between 1997 and 2006, as recorded in ACLED data. In column (3), we used data from the 2010 DHS survey[45] to compute the pro-

[43] As explained in Section 2.4, this is due to the high degree of multicollinearity between these two indicators in the context of Burundi. Interestingly, Alesina et al. (2003) encountered the same problem when studying the effects of ethnic, linguistic and religious heterogeneity on the quality of institutions and growth.

[44] Battles are defined as *"a violent interaction between two politically organized armed groups at a particular time and location"* (Raleigh et al., 2010).

[45] Results are available for 128 out of 129 municipalities.

portion of individuals who think that domestic violence is justified in at least one of the five following situations: the wife goes out without telling her husband, she neglects children, she argues with her husband, she refuses to have sex with him or she burns the food. The last two falsification tests used proxies of crime prevalence based on the 2012 Afrobarometer survey. The first indicator measures how often households have feared crime in their own house.[46] The second indicator is the proportion of people that have reported "crime and security" as one of the three most important problems in Burundi.[47]

Table 2.6: Falsification tests

	Attacks civilians (1)	Battles (2)	Domestic violence (3)	Fearing crime (4)	Crime issue (5)
Hutu share 2012	0.061	1.997	0.233	1.140	-0.561
	(0.993)	(1.317)	(0.344)	(1.105)	(0.908)
Ethnic fractionalization 2012 (st.)	0.087	0.409***	0.052	-0.027	-0.183*
	(0.112)	(0.117)	(0.043)	(0.124)	(0.100)
Political fractionalization 2005 (st.)	0.185*	0.025	-0.051**	-0.160*	0.177**
	(0.097)	(0.131)	(0.021)	(0.090)	(0.079)
Demob. rebels' polarization (st.)	-0.071	0.167	0.006	-0.033	0.120
	(0.093)	(0.115)	(0.026)	(0.124)	(0.074)
Demob. rebels' fractionalization (st.)	-0.120	-0.158	0.001	-0.018	-0.051
	(0.104)	(0.131)	(0.026)	(0.095)	(0.095)
No. demob. rebels (/1000)	0.079*	0.118***	0.013	0.029	0.000
	(0.044)	(0.045)	(0.011)	(0.031)	(0.023)
Population (log)	1.725***	1.687***	0.043	-0.415***	0.205
	(0.184)	(0.198)	(0.048)	(0.155)	(0.163)
Population density (log)	-0.052	-0.119	-0.021	-0.024	-0.167*
	(0.136)	(0.170)	(0.059)	(0.185)	(0.098)
Observations	779	779	822	725	725
Fixed Effects	NFE	NFE	NFE	NFE	NFE

Negative binomial regressions. The number of demobilized is by municipality of origin in columns (1) and (2), and by municipality of return in columns (3) to (5). Attacks against civilians and battles aggregate all ACLED records by type between 1997 and 2009. All estimations include neighborhood fixed effects (NFE). Standard errors are robust and clustered at two levels as described in Section 2.3.2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Electoral violence differs from other types of violence. Demobilized rebels' polarization is never significantly correlated with the five dependent variables. Contrary to our benchmark results, ethnic fractionalization is strongly correlated with battles, in line with the literature on ethnic grievances and civil war (Blattman and Miguel, 2010). The relationship between political fractionalization and other types of violence is ambiguous. The coefficient associated with political fractionalization is positive and significant for attacks against civilians and perception of criminality, and negative and significant for

[46] Possible responses are never, just once or twice, few times, many times or always. The construction of the proxy for crime, we created a dummy variable equal to one if they already feared crime at least a few times.

[47] Only 111 municipalities were surveyed in the Afrobarometer.

domestic violence. Overall, these regressions suggest that the determinants of electoral violence are different, particularly regarding the role played by demobilized groups and the absence of relationship with ethnic fragmentation.

2.5.3 Are the results robust?

We further tested the robustness of our results to numerous specifications. We estimated our model with OLS, Poisson and Negative Binomial estimation methods, with three sorts of "geographic fixed effects", with alternative vectors of covariates, without outliers, with standard-errors adjusted for spatial correlation following Conley (1999), and controlling for spatial dependence. Our results are robust to these alternative estimation strategies. In addition, placebo tests suggest that our results are not driven by the specific nature of our variable of interest. We further show that results are unlikely to be driven by unobservable characteristics of municipalities.[48]

2.6 Concluding remarks

In the last four decades, 80% of elections in Sub-Saharan Africa suffered from violence, bribery, intimidation or inequitable government interference (Bishop and Hoeffler, 2014). Using a unique dataset on electoral violence in Burundi in 2010, we tested whether electoral violence is driven by (1) ethnic composition and ethnic grievances, (2) political competition or (3) the presence and distribution of demobilized combatants. Understanding the causes of electoral misconduct in Burundi is of crucial importance for the stability of the Great Lake region.

The nature of our research question made a randomized controlled trial (RCT) virtually impossible. To minimize the risk of bias, our identification strategy exploits variations between neighboring municipalities, relying on the fact that these are more likely to have similar unobserved characteristics. We show that electoral violence was more prevalent in municipalities with two equally large groups of demobilized rebels (a one standard deviation increase in polarization among rebel groups leads to a 43% to 52% increase in violent events, depending on the specification) and characterized by a fierce political competition (an increase of one standard deviation in political fractionalization leads to an increase in violent events ranging between 27% and 45%, depending on the specification). The latter effect is stronger in municipalities with numerous former rebels. Politicians willing to seize power through illicit means seem to have exploited their former allegiances with demobilized rebels to commit violence. Interestingly, cleavages between ethnic groups, which were the main causes of violence in post-independence Burundi, did not fuel electoral violence in 2010. Violent campaigning was rifer in municipalities populated by a high proportion of Hutu.

The findings and limitations of this study suggest several important avenues for future research. First, more theoretical work is needed to understand the determinant of electoral violence. In particular, the models of Collier and Vicente (2012) and Chaturvedi (2005) should be extended to meet our empirical findings: rather than being a desirable

[48]The description of these tests and their results are presented in Appendix. The related results are instead reported in the Online Appendix to the published version of this article. They are available upon request.

strategy of the weakest parties, electoral violence in Burundi emerged in places where political competition was tight. Second, empirical research should be conducted to test the external validity of our findings and to distinguish different types of electoral malpractices. Future analysis should also determine when intimidation and violence are used as electoral tools and when they are the mere consequence of frustrations that flare up when tensions run high during elections. Finally, more research is necessary to understand the optimal demobilization process that should guide a country through democratic transition. In particular, the localization of campaigns for better integrating soldiers in the local economic and social fabric could successfully reduce the risk of electoral violence.

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Appendix

Overview of robustness tests implemented

Results presented above are robust to a battery of tests. I overview them below. The outcomes of these tests are reported in the Online Appendix of the published version of this article. They are nonetheless available upon request.

Estimation methods and "geographic fixed effects." We estimated OLS and Poisson models, which do not change our conclusions. Our results are robust to all types of "geographic fixed effects," which control for the unobserved factors that are similar in neighboring municipalities. We obtain similar results with standard errors clustered at the provincial level, and when observations are weighted to take into account the fact that municipalities have multiple neighbors and that some observations are duplicated.[49]

Alternative set of controls. Results are not significantly affected when additional control variables are included in the regressions. In particular, results hold when we control for latitude and longitude and their powers, for electoral results, for the share of demobilized rebels from each group in the population, for the presence of demobilized soldiers from the national army (FAB and FDN) or for municipalities in the capital city.

Selection on observables and unobservables. Our regressions control for observed and unobserved heterogeneity thanks to control variables and geographic fixed effects. In spite of these precautions, we cannot completely rule out that some unobservable characteristics could bias the results in one way or another.

Two types of unobservable characteristics could induce a spurious correlation between demobilized rebels' polarization and electoral violence. First, some unobserved factors, A, may have directly affected electoral violence. If these factors were determinants of the size of demobilized rebel groups (e.g. reasons for joining the rebellion), such as to affect demobilized rebel polarization in a non-random way, the coefficient associated with demobilized rebels' polarization would partly capture the impact of these unobserved variables, A. One way to control for this is to include measures of the relative size of demobilized rebel groups in the model. By doing so, we indirectly control for all unobserved factors affecting the composition of demobilized rebel groups that could potentially impact electoral violence via another route. For example, this strategy allows us to control for factors explaining where the recruitment of ex-rebels took place. In one specifications, we included the proportions of demobilized rebels belonging to each faction; in another, we included the proportions of the population belonging to each faction

[49] For all pairs fixed effects, the weights associated with a municipality i and its neighbors are equal to the inverse of its number of neighbors times two. For neighborhood fixed effects, the weights associated with a municipality i and its neighbors are equal to the inverse its number of neighbors plus one. For random pairs fixed effects, the weights are equal to 1/2.

are included in the regressions. Including these proportions as control variables does not change our results. The relationship between the polarization index of demobilized rebel groups and the occurrence of electoral violence remains positive and significant.

Second, some unobserved factors, B, may have influenced the location where demobilized combatants resettled after the conflict. If these factors also had a direct impact on the occurrence of electoral violence, the coefficient associated with demobilized rebels' polarization could partly capture the effect of unobserved variables, B. This type of bias is unlikely to affect our estimates for three reasons. First, a large majority of demobilized rebels, 83%, returned to their municipality of origin, and 86% of them returned to their province of origin. These statistics show that most demobilized combatants returned home after the conflict and that unobserved variables, B, only influenced a minority of them. Second, it is worth noting that 52% of those who did not return home went to Bujumbura Mairie.[50] In fact, 70% of those who returned to Bujumbura Mairie are originally from another municipality. We test whether estimates are affected when the capital city is removed from the sample. The coefficient associated with demobilized rebels' polarization is not statistically different. The coefficient associated with political fractionalization is lower and not significant at conventional thresholds. Removing Bujumbura Mairie reduces the variability of political fractionalization as the stronghold of political competition is located in the capital: political fractionalization is on average 1.5 standard deviation higher in Bujumbura Mairie than in the rest of the country. Finally, we tested if the results change when information on the origin of demobilized rebels is used to compute fractionalization and polarization indexes. As shown in Table 2.7, using the origin instead of the return municipality does not affect the significance nor the size of the coefficient associated with demobilized rebels' polarization. We therefore conclude that the correlation between demobilized rebels' polarization and electoral violence is robust and unlikely to be driven by unobserved factors.

As for demobilized rebels' polarization, we also tested whether some unobservable characteristics, C, could have impacted electoral violence directly, and have influenced votes via a separate route. If votes are affected such as to alter political competition in a non-random way, our regressions could partly capture these unobserved factors, C. In order to test this hypothesis, we included the proportion of voters for each party in the regression. This increases the point estimates of the political competition index.[51]

More generally, Altonji et al. (2005) proposed a method for estimating the relative influence of unobservables by analyzing how coefficients of interest are affected by the inclusion of control variables. The method requires estimating a regression with a restricted set of control variables and one with the full set of controls. By denoting $\hat{\beta}_R$ as the coefficient of interest measured in the former regression, and $\hat{\beta}_F$ as the coefficient measured in the latter regression, the ratio $\hat{\beta}_F/(\hat{\beta}_R - \hat{\beta}_F)$ quantifies how strong the selection on unobservables should be relative to the selection on observables to explain

[50] The capital is very different from rural municipalities. It hosts most Burundian institutions as well as the headquarters of International Organizations working in Burundi. Municipalities located in Bujumbura Mairie are by far richer than rural ones ($p < 0.00$). It is also worth noting that municipalities of Bujumbura Mairie host on average more demobilized demobilized combatants than rural municipalities ($p = 0.079$).

[51] By controlling for the proportion of voters, we include components of the political fractionalization index. This can cause multicollinearity, which explains the increase in standard errors and related loss of significance.

away the estimated effect of the variable of interest (Bellows and Miguel, 2009; Nunn and Wantchekon, 2011). In our case, coefficients of interest measured in the regressions with control variables and fixed effects are higher than the coefficients measured in regressions with a restricted set of controls, leading to a negative ratio $\hat{\beta}_F/(\hat{\beta}_R - \hat{\beta}_F)$. Intuitively, controlling for more unobservables should result in higher coefficients of interest. Our estimates are therefore likely to be lower bounds.

Removing outliers. Our results are robust to the removal of outliers from the sample. Outliers are defined as the observations whose standardized deviance residuals are greater than two (Hilbe, 2011).

Spatial dependence. We also tested if spatial correlation in the dependent variable could bias our estimates and thereby drive the results. Figure 2.1 shows no evidence of spatial correlation in electoral violence, which is confirmed quantitatively by the negative and non-significant Moran's statistic, associated with the indicator of electoral violence (p-value=0.330). As a robustness check, we nevertheless estimated our model by accounting for spatial dependence. We are not aware of any studies that demonstrated how to obtain consistent estimates for negative binomial models with spatial correlation. We therefore relied on four second-best approaches. First, we controlled for geographic coordinates of municipalities and for their squares and cubes. Second, we assessed how results are affected by the introduction of a spatial lag in the negative binomial model (Neumayer and Plümper, 2010). Third, we estimated the models developed by Pisati (2010) for linear regression models. We used two different weighting matrices: one identifying neighboring municipalities and one based on latitude and longitude data. Fourth, we estimated the OLS model and adjusted standard-errors for spatial correlation (Conley, 1999). These different estimation strategies and the two different weighting matrices yield similar results. Overall, we do not find any evidence that spatial dependence drives the results.

Placebo test. We carried out placebo tests to check if our results are driven by the nature of our explanatory variables. The placebo test consists in replacing the main regressor of interest by a variable of similar nature (that is, polarization indexes), but which is not expected to have predictive power on the dependent variable. We used two different polarization indexes based on age-groups[52] and religion.[53] The former stems from the hypothesis that youth bulges may be a source of conflict (Urdal, 2006).[54]

[52] In order to construct the age polarization index, we first divided the DHS sample into alternative age-group scenarios. Starting from individual ages, we assigned every individual in a group, and computed the proportion of individual in each group at the municipality level. These proportions were then used to compute an index of age polarization at the municipality level. Results are reported for a distribution of individuals according to the following categories: $[0, 15[$, $[15, 40[$, $[40, 60[$, $[60, 80[$, $[80, 99[$. Alternative scenarios give the same results.

[53] The religious polarization index also relies on DHS data, which classifies men and women into seven groups according to their religion (no religion, catholic, protestant, Muslim, adventist, jehova witness and other). It is computed at the municipality level by following the same steps as for age-group polarization.

[54] We additionally tested that it is not the bulge itself, by controlling for the number of young people, which had no impact on electoral violence when controlling for population size.

Religious diversity has been explored alongside ethnic diversity in the literature on the causes of civil conflict (Blattman and Miguel, 2010). Neither religious beliefs nor the resulting polarization index should affect electoral violence in the context of Burundi, where ethnicity rather than religion fueled violence in the past. Reassuringly, none of the placebo polarization indexes enter significantly in the regressions.

Supplementary tables

Table 2.7: The results of Table 2.4 with demobilized rebels' variables constructed using the municipality of origin of demobilized rebels

Dependent variable: total episodes of electoral violence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Without violence and wealth				With violence and wealth			
Hutu share 2012	1.691*	2.992***	2.759***	3.119**	1.266	1.836*	1.949*	2.262
	(0.927)	(1.133)	(1.048)	(1.485)	(0.907)	(1.101)	(1.101)	(1.450)
Ethnic fractionalization 2012 (st.)	0.034	0.193	0.140	0.206	-0.000	0.056	0.051	0.084
	(0.126)	(0.153)	(0.135)	(0.196)	(0.119)	(0.151)	(0.137)	(0.193)
Political fractionalization 2005 (st.)	0.156	0.341***	0.314***	0.333***	0.081	0.274***	0.236***	0.280**
	(0.108)	(0.085)	(0.091)	(0.120)	(0.102)	(0.075)	(0.086)	(0.116)
Demob. rebels' polarization (st.)	0.296***	0.414***	0.381***	0.391***	0.403***	0.388***	0.372***	0.365***
	(0.101)	(0.112)	(0.106)	(0.142)	(0.122)	(0.105)	(0.107)	(0.141)
Demob. rebels' fractionalization (st.)	-0.297**	-0.206*	-0.220*	-0.159	-0.236*	-0.157	-0.171	-0.120
	(0.135)	(0.109)	(0.114)	(0.149)	(0.140)	(0.105)	(0.116)	(0.149)
No. demob. (/1000)	0.002	0.015	0.006	0.042	-0.090	-0.049	-0.057	-0.023
	(0.064)	(0.058)	(0.061)	(0.072)	(0.058)	(0.049)	(0.057)	(0.064)
Population (log)	0.870***	1.373***	1.353***	1.356***	0.668***	0.758***	0.817***	0.806***
	(0.222)	(0.190)	(0.192)	(0.251)	(0.228)	(0.214)	(0.220)	(0.275)
Population density (log)	0.295***	0.163	0.173	0.110	0.285***	0.218	0.231*	0.182
	(0.098)	(0.221)	(0.172)	(0.233)	(0.108)	(0.161)	(0.135)	(0.203)
Past violence (log)					0.340***	0.395***	0.381***	0.371***
					(0.079)	(0.087)	(0.086)	(0.102)
Median Wealth Index (st.)					0.048	0.026	0.083	0.020
					(0.132)	(0.129)	(0.124)	(0.164)
Observations		1404	831	258		1388	822	256
Fixed Effects	.	All	NFE	Random	.	All	NFE	Random

Negative binomial regressions. No fixed effects are signaled by a dot. "All" refers to "All pairs", "NFE" to Neighborhood Fixed Effects", and "Random" to random pairs. Standard errors are robust and clustered at the level relevant to the fixed effects included in the regression (as described in Section 2.3.2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.8: Testing hypotheses separately and simultaneously - OLS estimates

Dependent variable: total episodes of electoral violence (log)

	Ethnic grievances		Political competition				Demob. rebels		All channels	
			2010		2005					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Hutu share 2012	0.584								1.610	2.125**
	(0.714)								(1.028)	(1.041)
Ethnic fractionalization 2012 (st.)		-0.060							0.018	0.133
		(0.092)							(0.127)	(0.134)
Political fractionalization 2010 (st.)			0.227**						0.150	
			(0.093)						(0.099)	
Political polarization 2010 (st.)				0.224***					0.171*	
				(0.081)					(0.090)	
Political fractionalization 2005 (st.)					0.157**					0.204*
					(0.071)					(0.114)
Political polarization 2005 (st.)						0.135*				0.027
						(0.074)				(0.113)
Demob. rebels' polarization (st.)							0.124*		0.158*	0.211**
							(0.068)		(0.092)	(0.090)
Demob. rebels' fractionalization (st.)								0.048	-0.070	-0.127
								(0.067)	(0.090)	(0.092)
Observations	831	831	831	831	831	831	831	831	831	831
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	NFE	NFE	NFE	NFE	NFE	NFE	NFE	NFE	NFE	NFE
R^2	0.32	.32	.34	.36	.33	.33	.33	.32	.38	.36
$AdjustedR^2$	0.19	.19	.21	.23	.2	.2	.2	.19	.26	.24

Negative binomial regressions. Controls include the number of demobilized rebels per 1000 inhabitants, the $\log$ of population size, and the $\log$ of population density. All estimations include neighborhood fixed effects (NFE). Standard errors are robust and clustered at two levels as described in Section 2.3.2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.9: The results of Table 2.4 using OLS estimation

Dependent variable: total episodes of electoral violence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Without violence and wealth				With violence and wealth			
Hutu share 2012	0.672	2.522*	2.173**	2.621*	0.660	1.731	1.617	1.861
	(0.829)	(1.392)	(1.070)	(1.474)	(0.780)	(1.402)	(1.064)	(1.484)
Ethnic fractionalization 2012 (st.)	0.033	0.182	0.139	0.204	0.014	0.091	0.073	0.106
	(0.107)	(0.193)	(0.137)	(0.203)	(0.104)	(0.190)	(0.137)	(0.206)
Political fractionalization 2005 (st.)	0.092	0.270***	0.227***	0.273**	0.019	0.208**	0.173**	0.218*
	(0.075)	(0.096)	(0.077)	(0.113)	(0.076)	(0.098)	(0.077)	(0.116)
Demob. rebels' polarization (st.)	0.168*	0.252**	0.215**	0.255**	0.251**	0.243**	0.233**	0.242*
	(0.093)	(0.110)	(0.093)	(0.125)	(0.101)	(0.116)	(0.096)	(0.127)
Demob. rebels' fractionalization (st.)	-0.216**	-0.118	-0.130	-0.114	-0.180*	-0.081	-0.113	-0.082
	(0.096)	(0.114)	(0.093)	(0.132)	(0.101)	(0.115)	(0.094)	(0.133)
No. demob. rebels (/1000)	0.018	0.020	0.005	0.036	-0.050	-0.025	-0.036	-0.011
	(0.045)	(0.065)	(0.050)	(0.063)	(0.045)	(0.063)	(0.049)	(0.061)
Population (log)	0.759***	1.216***	1.173***	1.194***	0.640***	0.823***	0.820***	0.818***
	(0.204)	(0.235)	(0.193)	(0.242)	(0.200)	(0.247)	(0.200)	(0.253)
Population density (log)	0.164*	0.032	0.072	-0.013	0.173*	0.097	0.133	0.048
	(0.089)	(0.221)	(0.163)	(0.196)	(0.101)	(0.184)	(0.131)	(0.186)
Past violence (log)					0.265***	0.280***	0.277***	0.264***
					(0.064)	(0.097)	(0.077)	(0.094)
Median Wealth Index (st.)					0.080	0.034	0.077	-0.008
					(0.113)	(0.148)	(0.115)	(0.150)
Observations	129	1404	831	258	128	1388	822	256
Fixed Effects	.	All	NFE	Random	.	All	NFE	Random
R-squared	.14	.63	.36	.63	.25	.67	.43	.66
Adj. R-squared	.08	.25	.24	.21	.19	.33	.31	.27

OLS regressions. No fixed effects are signaled by a dot. "All" refers to "All pairs", "NFE" to Neighborhood Fixed Effects", and "Random" to random pairs. Standard errors are robust and clustered at the level relevant to the fixed effects included in the regression (as described in Section 2.3.2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.10: Balancing test - regressions of variables of interest on controls, with and without geographic fixed effects

	Demob. polarization				Demob. fractionalization			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No. demob. rebels (/1000)	0.058	-0.034	-0.013	-0.036	0.092**	-0.013	0.006	-0.017
	(0.040)	(0.048)	(0.040)	(0.050)	(0.039)	(0.045)	(0.037)	(0.043)
		[0.702]	[0.428]	[0.732]		[0.187]	[0.112]	[0.199]
Past violence (log)	-0.266***	-0.038	-0.092	-0.010	-0.208***	-0.030	-0.071	-0.002
	(0.076)	(0.066)	(0.068)	(0.078)	(0.075)	(0.066)	(0.063)	(0.073)
		[0.025]	[0.09]	[0.02]		[0.077]	[0.164]	[0.051]
Median Wealth Index (st.)	-0.079	0.132	0.101	0.099	0.090	0.233**	0.205***	0.213**
	(0.103)	(0.121)	(0.079)	(0.120)	(0.072)	(0.092)	(0.063)	(0.094)
		[0.739]	[0.866]	[0.9]		[0.223]	[0.232]	[0.301]
Population (log)	0.234	0.124	0.108	0.066	0.857***	0.410*	0.417*	0.392
	(0.280)	(0.253)	(0.263)	(0.270)	(0.228)	(0.243)	(0.231)	(0.243)
		[0.771]	[0.743]	[0.667]		[0.182]	[0.178]	[0.165]
Population density (log)	0.173*	0.060	0.094	0.030	0.085	0.215*	0.232**	0.205
	(0.100)	(0.151)	(0.112)	(0.175)	(0.079)	(0.113)	(0.095)	(0.132)
		[0.534]	[0.6]	[0.479]		[0.348]	[0.236]	[0.437]
Observations	128	1388	822	256	128	1388	822	256
Fixed Effects	.	All	NFE	Random	.	All	NFE	Random

OLS regressions. No fixed effects are signaled by a dot. "All" refers to "All pairs", "NFE" to Neighborhood Fixed Effects", and "Random" to random pairs. Standard errors are reported in parenthesis; they are robust and clustered at the level relevant to the fixed effects included in the regression (as described in Section 2.3.2). In brackets, we report the p-value of a t-test assessing if the absolute value of coefficients without and with fixed effects are different . * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

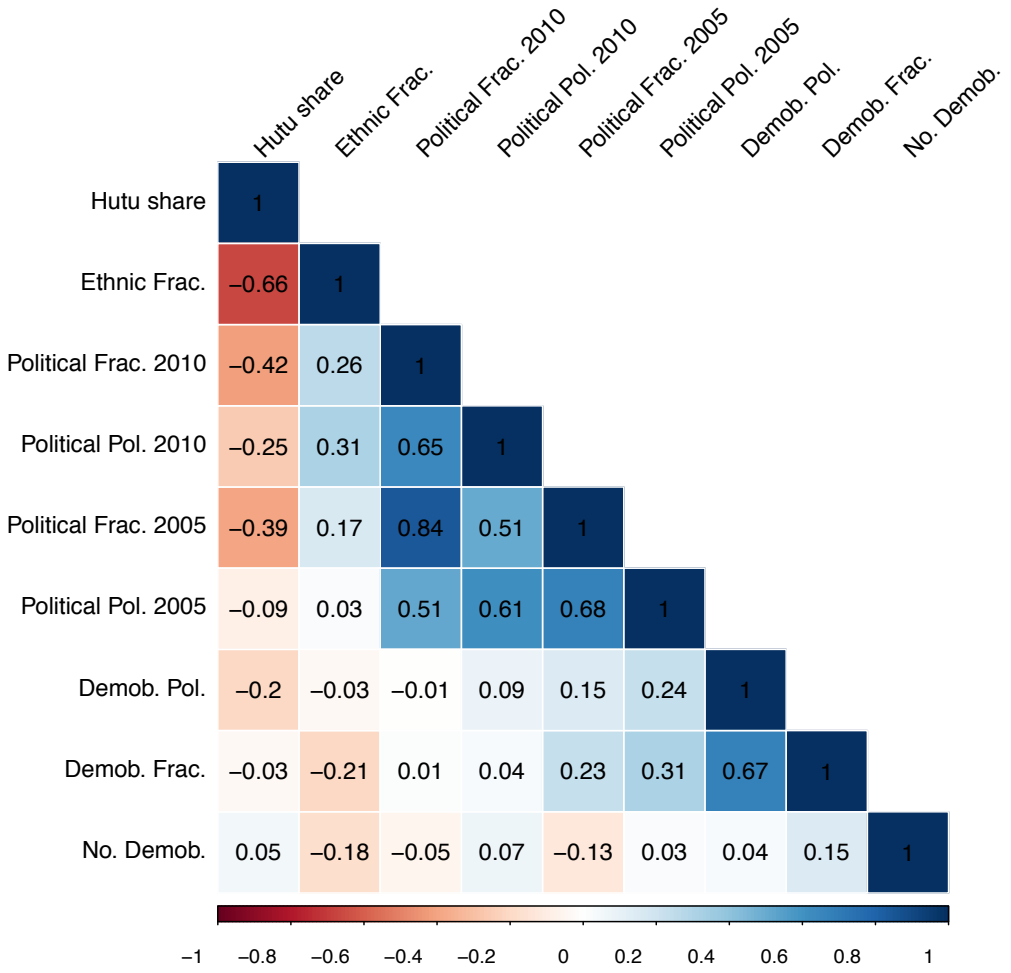
Table 2.11: Balancing test - regressions of variables of interest on controls, with and without geographic fixed effects (Cont'd)

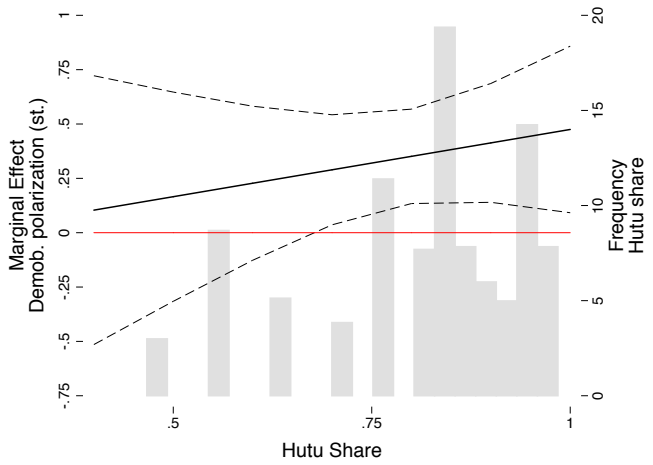
	Hutu share				Ethnic fractionalization				Political fractionalization			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
No. demob. rebels (/1000)	0.007*	0.001	0.003	0.001	-0.119***	-0.074	-0.100***	-0.069	-0.074*	-0.150***	-0.135***	-0.154***
	(0.004)	(0.006)	(0.004)	(0.005)	(0.035)	(0.057)	(0.038)	(0.054)	(0.039)	(0.032)	(0.031)	(0.036)
		[0.407]	[0.481]	[0.351]		[0.502]	[0.714]	[0.439]		[0.134]	[0.223]	[0.134]
Past violence (log)	-0.004	-0.001	-0.002	-0.003	0.151**	0.098	0.126**	0.095	0.064	0.029	0.030	0.037
	(0.007)	(0.007)	(0.006)	(0.008)	(0.075)	(0.069)	(0.060)	(0.068)	(0.080)	(0.075)	(0.067)	(0.071)
		[0.762]	[0.829]	[0.925]		[0.604]	[0.795]	[0.581]		[0.75]	[0.745]	[0.801]
Median Wealth Index (st.)	-0.027***	-0.019	-0.022***	-0.015	-0.199**	-0.190***	-0.202***	-0.168**	0.374***	0.369***	0.357***	0.316***
	(0.009)	(0.012)	(0.008)	(0.012)	(0.080)	(0.056)	(0.045)	(0.069)	(0.102)	(0.106)	(0.075)	(0.119)
		[0.595]	[0.679]	[0.425]		[0.927]	[0.974]	[0.77]		[0.973]	[0.893]	[0.712]
Population (log)	0.091***	0.053**	0.061***	0.049*	-0.764***	-0.404*	-0.481**	-0.352	-0.393*	-0.343	-0.422**	-0.324
	(0.027)	(0.021)	(0.021)	(0.027)	(0.227)	(0.223)	(0.194)	(0.228)	(0.207)	(0.222)	(0.199)	(0.224)
		[0.269]	[0.382]	[0.273]		[0.26]	[0.345]	[0.203]		[0.869]	[0.92]	[0.821]
Population density (log)	-0.040***	-0.028	-0.035**	-0.024	0.161**	-0.144	-0.102	-0.138	0.004	0.097	0.071	0.064
	(0.013)	(0.020)	(0.015)	(0.021)	(0.079)	(0.101)	(0.084)	(0.123)	(0.086)	(0.179)	(0.125)	(0.166)
		[0.616]	[0.802]	[0.518]		[0.895]	[0.61]	[0.875]		[0.64]	[0.66]	[0.749]
Observations	128	1388	822	256	128	1388	822	256	128	1388	822	256
Fixed Effects	.	All	NFE	Random	.	All	NFE	Random	.	All	NFE	Random

OLS regressions. No fixed effects are signaled by a dot. "All" refers to "All pairs", "NFE" to Neighborhood Fixed Effects", and "Random" to random pairs. Standard errors are reported in parenthesis; they are robust and clustered at the level relevant to the fixed effects included in the regression (as described in Section 2.3.2). In brackets, we report the p-value of a t-test assessing if the absolute value of coefficients without and with fixed effects are different. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

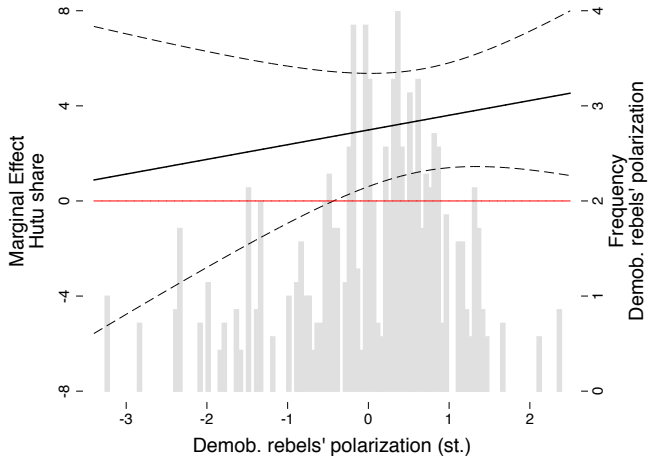
Supplementary figures

Figure 2.4: Correlation matrix between variables of interest

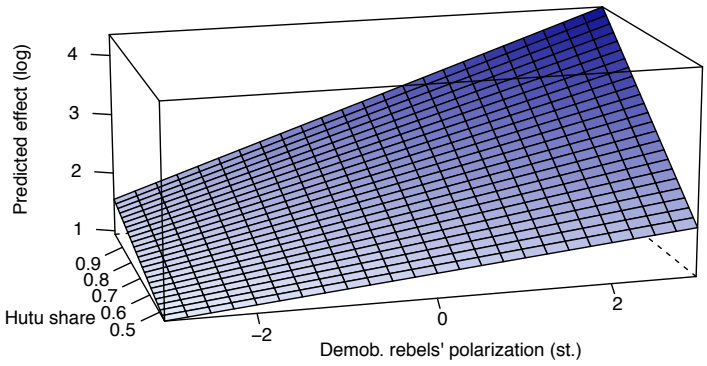




(a) Marginal effect of demob. rebels' polarization as a function of Hutu share (95% CI)

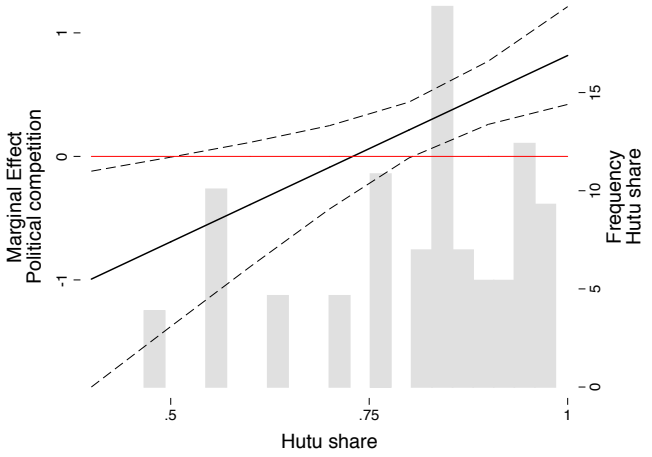


(b) Marginal effect of Hutu share as a function of demob. rebels' polarization (95% CI)

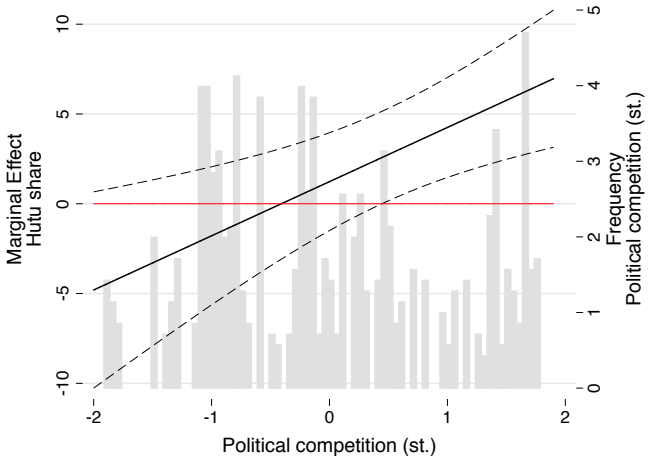


(c) Joint predicted effect of Hutu share and demob. rebels' polarization

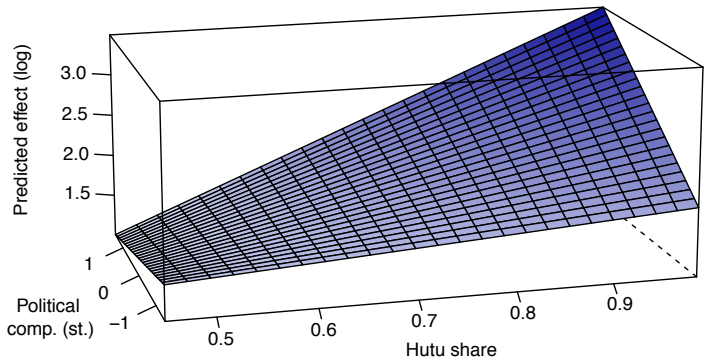
Figure 2.5: Interactions: demob. rebels' polarization and Hutu share



(a) Marginal effect of political competition as a function of Hutu share (95% CI)

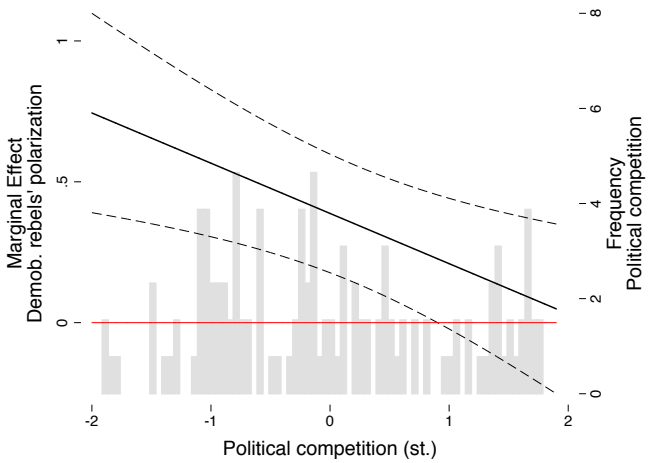


(b) Marginal effect of Hutu share as a function of political competition (95% CI)

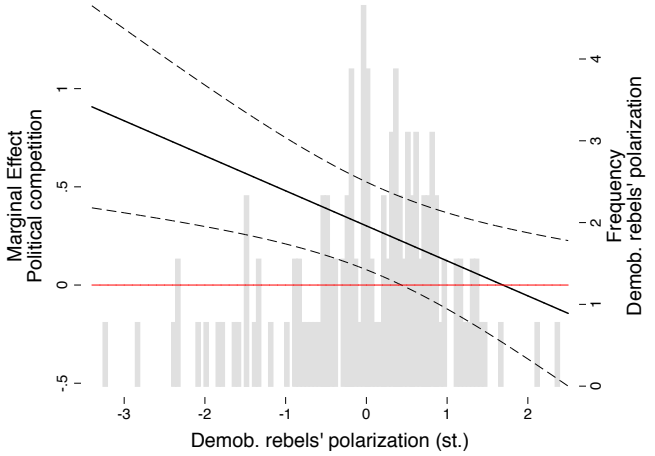


(c) Joint predicted effect of Hutu share and political competition

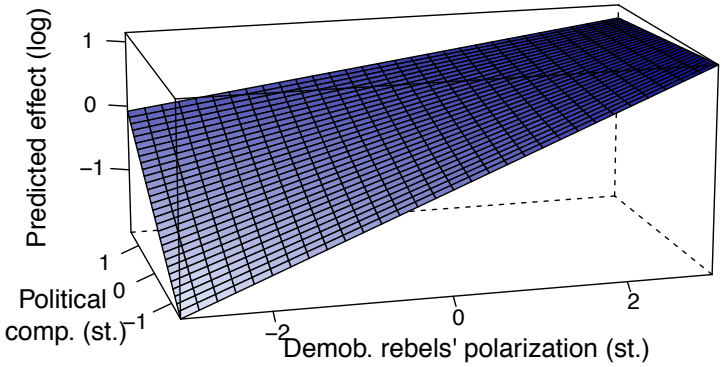
Figure 2.6: Interactions: Political competition and Hutu share



(a) Marginal effect of demob. rebels' polarization as a function of political competition (95% CI)

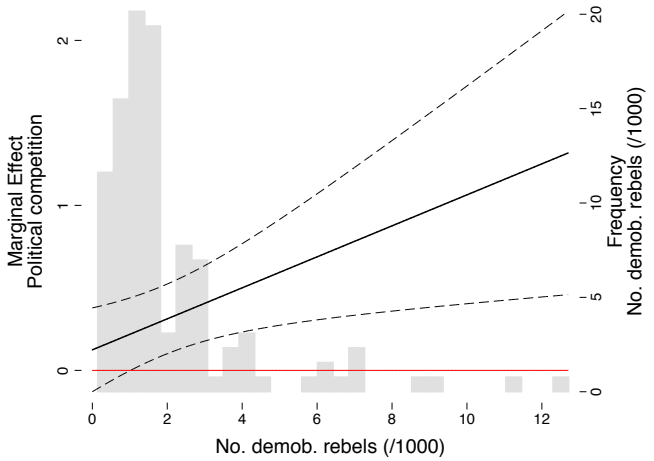


(b) Marginal effect of political competition as a function of demob. rebels' polarization (95% CI)

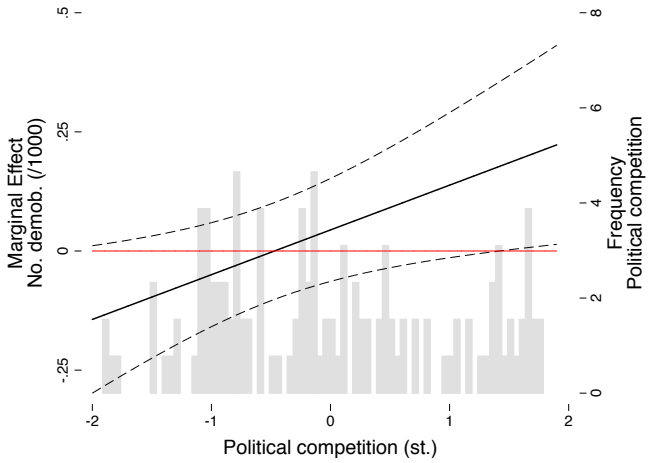


(c) Joint predicted effect of demobilized rebels' polarization and political competition

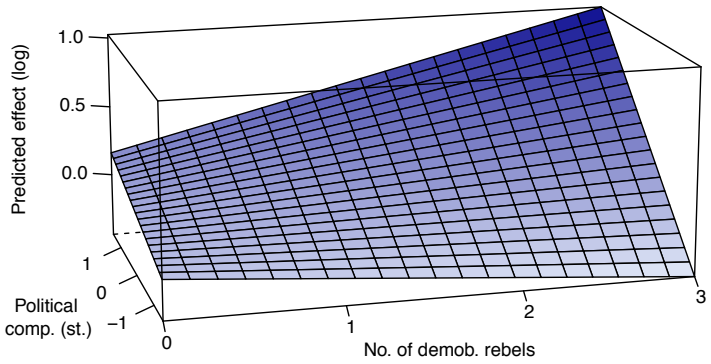
Figure 2.7: Interactions: demob. rebels' polarization and political competition



(a) Marginal effect of political competition as a function of the no. of demob. rebels. (95% CI)



(b) Marginal effect of the no. of demob. rebels as a function of political competition (95% CI)



(c) Joint predicted effect of demobilized rebels' polarization and political competition

Figure 2.8: Interactions: number of demobilized rebels and political competition

Chapter 3

Interjurisdictional coordination and access to sanitation in Brazil

3.1 Introduction

In decentralized settings, individual municipalities need to coordinate certain policies in order to avoid undesirable spillover effects or to obtain benefits from positive externalities. For example, uncoordinated and insufficient investments in sanitation infrastructure may provoke the outbreak of regional epidemics. In Brazil – where local governments are in charge of water and waste-water treatment – the Zika virus in 2015 broke out in municipalities with poor sanitation services and then spread across the rest of the Northeast (Human Rights Watch, 2017). To prevent similar epidemics from happening in the future, Brazilian authorities have been encouraging more coordination between local providers of water-related infrastructures (Brannstrom, 2004; Formiga-Johnsson and Kemper, 2005).

Jurisdictions coordinate the provision of public services when the benefits of cooperation outweigh the transaction costs. On the one hand, coordination pushes local governments to work together, share information and develop policies that tackle externalities going beyond individual administrative boundaries. On the other hand, coordination requires costly searching for bargaining over mutually beneficial solutions, and monitoring and enforcing any resulting agreement. In addition, local political leaders engage in cooperation agreements are held accountable by their constituents for collective decisions over which they do not have sole control (Gerber et al., 2013). Blurred accountability creates room for morally hazardous behavior that ultimately undermines the access to public services.

Coordination may be less costly in cases where decision-makers are politically aligned. When several layers of government share a mandate over the provision of public good, repeated interaction between parties across levels of government and with a common political agenda may facilitate policy coherency (de Figueiredo Jr. and Weingast, 2005; Weingast, 2014). Political alignment between administrators at the same level of governance matters as well. In Brazil, surveys reveal that neighboring mayors are more likely to coordinate the provision of water-related services when they come from the same party or coalition. They also show that mayors find negotiations with political leaders from opposing parties costly (Formiga-Johnsson and Kemper, 2005; Mobarak Mushfiq and Lipscomb, 2009).

This chapter argues that, under certain circumstances, coordination among politically aligned local decision-makers may jeopardize access to interjurisdictional public goods. Both literatures on incentives in teams and on the network of regulators suggest that co-providers who come from the same party may collude and deliver a worse outcome than cooperative one. For example, a local politician may hide the underperformance

of a neighboring comrade due to fear of damage to the party reputation, which could eventually spillover. Likewise, a mayor may have more incentive to denounce an underperforming peer when the two belong to different parties.

I test this hypothesis in Brazil where the management of sanitation services is decentralized at the municipal level, and focus on municipalities that could benefit from co-managing a common network of sewers. I find that the share of households with access to an interjurisdictional sanitation network is *lower* in municipalities where mayors who share the mandate over local sanitation infrastructure are politically aligned.

The hypothesis relies on two main assumptions.

First, I assume that the proximity between municipalities is a sufficient condition for their mayors to coordinate a common network of sewers. Since there exists no comprehensive dataset on cooperative management of public services in Brazil, I use the distance between the densest clusters of municipalities - as identified through geospatial data - as a proxy for the scope of coordination between two mayors. I then argue that mayors of communities whose densest neighborhoods are close together are more likely to co-manage a common network of sewers.

Second, I isolate the causal impact of coordination and political alignment on access to sanitation by focusing on network coverage in groups of neighboring municipalities that are very politically competitive. In particular, I make a comparison of the share of households with access to the sewerage network in 2010 between municipalities where the party ruling the majority of the neighboring jurisdictions barely wins the 2008 elections and those municipalities where it loses. Municipalities just above and just below the cutoff tend to be comparable in terms of all characteristics, with the exception of their political alignment with neighboring jurisdictions.

This chapter contributes to a rising literature on horizontal interjurisdictional coordination and access to public services. Ostrom et al. (1978) were among the first scholars to study the positive implications of interjurisdictional cooperation for the efficient management and organization of local public goods – in particular, the administration of justice. Dell (2015) shows that (the lack of) coordination among highly competitive neighboring municipalities significantly shapes the geography of drug traffic in Mexico. In Argentina, Di Tella and Schargrodsky (2004) computes negative spillovers in policing from one part of Buenos Aires to neighboring areas in the aftermath of a terrorist attack. In Colombia, Acemoglu et al. (2015) use instrumental variables and a network model to show that local state capacity can improve when neighboring municipalities keep on interacting.

Few papers in this literature have looked at the joint effect of coordination and political alignment on local public goods. Among those that have, the results are mixed. Durante and Gutierrez (2015) find that homicide rates in Mexican municipalities decrease when neighboring mayors come from the same party and therefore interact more easily. In Brazil, Lipscomb and Mobarak (2016) exploit sudden changes in borders of municipalities to analyze spatial patterns of water pollution as rivers cross locations. The authors find that as a river approaches the downstream exit border, water pollution significantly increases. Political alignment, however, does not significantly affect the magnitude of cross-border externalities. Gerber et al. (2013), Hawkins et al. (2016) and related public administration literature find that political homophily could facilitate coordination of

services in a metropolitan area, but there is no real assessment of the direct outcome in terms of access to public services.

This chapter is related to a second strand of literature that investigates vertical – rather than horizontal – political alignment as a determinant of policy output. Jametti and Joanis (2010) argue that vertical externalities among politicians arise when the provision of a public good is shared among different mandates, arguing that mandataries might react strategically to the contribution to the public good of another level of government. Interaction and the size of externalities might depend on similarities in political agendas along the chain of local providers. Estache et al. (2016) and Kresch (2017) successfully test this prediction in the case of regulation of water pollution in Brazil. In a similar fashion, Solé-Ollé and Sorribas-Navarro (2008), Joanis (2011) and Brollo et al. (2013) find that municipalities ruled by the same parties as the center tend to receive relatively more transfers than municipalities ruled by competing parties. In this way, the central government can "tie the hands of its enemies" and increase the advantage of locally aligned governments by giving them more fiscal space to lower local taxes, provide more public goods and improve municipal services.[1]

Section 3.2 reviews the main institutional features of local sanitation provision in Brazil. Section 3.3 frames the main hypothesis of this chapter in a theoretical framework drawing insights from the predictions of the principal-two agents model and from the theories of network of regulators. Section 3.4 presents descriptive statistics of dependent and control variables used in the regression discontinuity (RD) analysis. Section 3.5 elaborates the identification strategy and discusses local polynomial methods to estimate the RD effect. Results are then presented in Section 3.6 and discussed in Section 3.7. Section 3.8 offers a conclusion, acknowledges the limitations of the analysis and suggests avenues for further research.

3.2 Decentralization and coordinated provision of sanitation services in Brazil

Access to a sanitation network remains problematic in both rural and urban municipalities in Brazil. In rural areas, access is challenging especially in small local administrations, although financial resources have been mobilized in recent years. In urban informal settlements, the population is so high and land availability so limited that sewers are often a scarce resource (Stepping, 2016).

Decentralization reforms in Brazil assigned the management of sanitation to municipalities without setting a clear framework of operation. Originally, the military regime – in power between 1964 and 1985 – created 26 state companies (*Companhias Estaduais de Saneamento Básico*, CESB) to serve this purpose. After the collapse of the regime, the new Constitution promulgated in 1988 decentralized the provision of a number of public services – including water and waste-water treatment – to municipalities. Most of the new providers terminated their contracts with the CESB and began relying on

[1] This chapter is related to a third strand of literature that studies the effects of yardstick competition between local administrators on tax and welfare outcomes (Brueckner, 2003). Case et al. (1993) and Besley and Case (1995) are seminal studies in the field. Besley and Smart (2007); Bordignon et al. (2003); Revelli (2006) are other notable milestones. However, this literature does not specifically address the benefits of political alignment and coordination.

local structures (either public or private) for the provision of water and sanitation services. However, for two decades the related legislative framework remained opaque and uncertain, discouraging investments in sanitation infrastructure (Heller, 2007).

A new framework came into force at the beginning of the 2000s. In 2002 President Ignacio Lula da Silva gave a boost to decentralization reforms through the institution of the Ministry of Cities (*Ministério das Cidades*). The new ministry became responsible for investments in water and sanitation projects in municipalities with more than 50,000 inhabitants.[2] In 2005, the government presented National Sanitation Law No. 11, establishing a new regulatory framework for the sanitation sector in Brazil. The law declares access to sewerage – as well as to drinkable water, solid waste management and drainage of rainwater – as a universal right. Municipalities are in charge of regulating service provision, monitoring compliance and enforcement, and have to develop sanitation plans coherent with those elaborated at the river basin, state or federal levels.

Importantly for this study, Sanitation Law 11.445/2007 establishes that municipalities can cooperate to provide water and sanitation services. In practice, they can delegate their obligations to public consortia (e.g., inter-municipal consortia or State consortia) under public or private law (Stepping, 2016). Delegation does not relieve mayors of the responsibility to ensure access to adequate sanitation services for their constituents. Throughout the chapter, I argue that voters keep mayors accountable with regard to the access to a network of sewers independent of the form of governance of sanitation provision. In fact, delegation may alter the capacity of voters to observe the actual capacity of mayors to deliver universal access to sewers.

The lack of a systematic database on the forms of governance of sanitation services necessitates assumptions about the scope of coordination. An annual survey by the National Information System on Water and Sanitation (*Sistema Nacional de Informações sobre Saneamento*, SNIS) records whether municipalities engage in consortia or manage sanitation networks by themselves. However, the database is incomplete as it covers only 34% of municipalities in the country. Moreover, these municipalities are not necessarily contiguous, making it difficult to map any form of interjurisdictional cooperation. In order to close this informational gap, I assume that municipalities with close dense neighborhoods are likely to coordinate. This assumption helps focus the analysis on a specific sample of municipalities. Section 3.4 presents this sampling strategy in detail.

3.3 Conceptual framework

Neighboring local governments have three main good reasons for managing sanitation infrastructure together. First, lagging access to sanitation in one community may produce negative spillovers affecting neighboring jurisdictions, as mentioned at the beginning of this chapter. Coordination may help prevent the outbreak of diseases by facilitating information flows among decision-makers. Second, municipalities that cooperate could achieve financial economies of scale: as the number of users of the network increases, the marginal unit cost of waste-water treatment and disposal will decrease, yielding lower operation costs. Coordination may contribute to easing the fiscal position of rural areas, where population shrinks and the per capita costs of sanitation services increase

[2]Below this limit, the responsibility was left to the Ministry of Health.

as a consequence. Third, local governments that team up may have stronger bargaining power to negotiate sanitation-related funding with state and federal authorities. In what follows, I focus on the role that mayors have in coordinating public services.

Morally hazardous mayors could capture the benefits of coordination, nonetheless. Mayors with particularly strong career-related concerns could free-ride on the infrastructure provided by others in order to spend more energies and resources on services that are more idiosyncratic and are thus more likely to appeal to the local electorate. Moreover, coordination may blur accountability and further motivate free-riding behavior. Local constituents may hold mayors accountable for access to the sanitation network over which the collective - and not just individual mayors - have ultimate control. Sluggish mayors could then capitalize on the performance of diligent ones when the level of the public good is satisfactory, but can also hide personal inefficiencies by blaming their neighbors when voters are unsatisfied. In sum, under coordination, free-riding could create incentives for all types of mayors to slack off, and access to sanitation would be lower than in the social optimal equilibrium.

Increasing interaction between mayors could discourage moral hazard across municipal borders. Constant interaction improves information flow, forcing mayors to monitor and assist each other. For instance, river basin committees gather all actors with a stake in the management of a river crossing several jurisdictions. The function as spaces for decision-makers to negotiate and exchange information over access to water quantity and quality, ensuring economically productive, socially equitable and environmentally sustainable usage of natural resources (Lipscomb and Mobarak, 2016; Mody, 2004).

A regulatory network theory to explain the coordinated provision of sanitation services

By institutionalizing cooperation, mayors could monitor each other and report to their voters any misconduct among their neighboring peers. Monitoring thus influences the individual costs of engaging in desirable and undesirable behavior. It might curb or create incentives for shirking, depending on the circumstances.

To illustrate this point, consider a principal-two agent model where agents supervise each other. In this chapter, the citizens of two neighboring municipalities (the principal) entrust their mayors (the agents) with the provision of a cross-border sanitation network (Figure 3.1).[3] The agents supervise each other's activity and may report to the principal any inefficiency of their peer. In return, the constituents reward mayors with their votes if they are satisfied with the quality of the waste-water treatment facilities.

Under this theoretical framework, cooperation is expected to yield a superior outcome than in a situation where each agent acts on her own. In his seminal work, Holmström (1979) shows that when the performance of two agents are correlated, the optimal incentive scheme consists of rewarding one agent conditional on the output of the other agent. Varian (1990) uses this framework to explain the functioning of micro-credit in developing countries. In this case, banks make loans to a group of small entrepreneurs

[3] I assume here that the citizens of the two municipalities form a homogeneous body with the same preferences concerning access to adequate sanitation. They can therefore be considered as one principal. It would be interesting to model multiple principals with diverging preferences, either within the municipalities or across border.

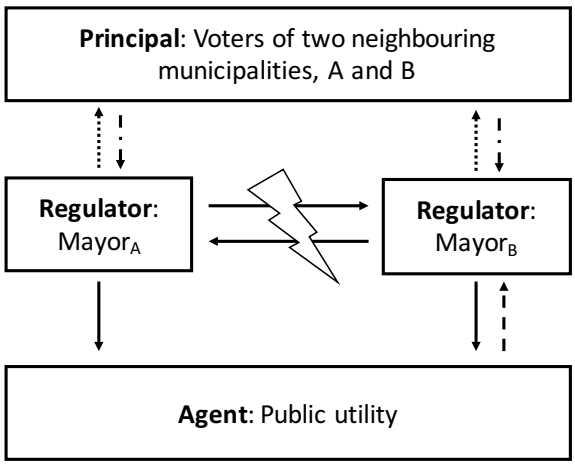
that could not otherwise obtain access to larger capital markets. Although the loan is individual, the entrepreneurs act as co-guarantors. The resulting peer group pressure on any potential slacker contributes to the sustainability of the loan.

Monitoring could fail if mayors found side contracts more profitable. A side-contract is any implicit or explicit exchange between agents entailing more lenient supervision of one or both agents' activity. The principal has no control over side-contracts because she cannot observe them (Holmström and Milgrom, 1990). For example, a slacking agent could bribe a second agent in order not to have her poor performance revealed. Alternatively, both agents could collude in order to minimize their effort.

The decision of agents to collude depends on the transaction costs of side-contracts. For example, accepting to collude and be lenient might have reputation costs, if this deception is leaked to the public. Transaction costs are moreover non-linear. They are high only when one agent is willing to bribe her peer, and might decrease when both agents exchange favors and agree not to report each other's underperformance to the principal (Laffont and Meleu, 1997). When the costs of side-contracting are low, preventing collusion between the two mayors become more difficult and cooperation may lead to a suboptimal level of public good in equilibrium.

Now, consider an augmented version of the principal-two agents model where the two mayors are regulators of an inter-municipal public utility and collusion is possible. In this context, each mayor has two tasks: first, to monitor the performance of the public utility and report it to constituents; second, to monitor the peer's regulatory efficiency (Figure 3.1). Like in a traditional principal-regulator-agency model, the public utility might wish to bribe the regulator in order to minimize effort and maximize profits. The existence of multiple regulators, though, might complicate the illicit plans of the agent.

Figure 3.1: A schematic representation of the conceptual framework



Laffont and Martimort (1999) show that corruption is less likely when the number of regulators increases. The authors' model is based on the assumption that each regulator observes the signal that the other receives. If both supervisors receive the same informative signal about the firm's performance, each will anticipate that the other will report to the principal and hence any collusion would be ineffective. In fact, even if each supervisor does not observe which signal the other receives, having more than one

regulator still reduces capture. Estache et al. (2000) argue that when each supervisor is only partially informed, her ability to extract bribes from the public utility is reduced.

Increasing the number of supervisors might not help to minimize collusion if mayor-regulators collude. In line with the principal-two agents model, the cooperative management of the sewerage network might create incentives for mayors to collude, limiting their accountability to voters. Mayors-regulators might have less incentives to stipulate side-contracts and extract rents from the public utility if they were competing against each other (Estache and Wren-Lewis, 2010). In the context of this analysis, a group of mayors managing a common sanitation network might face less incentives to collude if they were coming from different parties.

Political alignment and local coordination of public services

Political similarities could facilitate interactions between subnational governments, whether they are benevolent or not. The costs of searching for mutually beneficial policy decisions may decline when constituents from neighboring jurisdictions share similar policy preferences about the appropriate role of government. Cooperative behavior and information sharing may be due to closer personal connections between fellow party members operating in neighboring jurisdictions (Durante and Gutierrez, 2015). Party discipline – that is, the ability of a political party to align the preferences and behavior of its members preferences – could also play a role. In Brazil, while party fragmentation remains elevated, partisan discipline is consistently high (de Melo, Pedro OS Vaz, 2015).

Yet, political similarities may undermine the benefits of the collective management of a public good. Within the conceptual framework discussed above, this study assumes that the transaction costs of collusive side-contracts are particularly low when neighboring mayors are politically aligned. I therefore argue mayors co-managing a public service collude when the transaction costs of side-contracting decrease faster than the costs of coordination because of political similarities. In this case, the agents would hide each other performance from the principal's scrutiny, worsening the access to and quality of sanitation.

As an example, consider two parties, one right wing and one left wing. The public utility tries to bribe the left-wing mayor in order to minimize effort and maximize profits. A right-wing mayor is more likely to openly condemn collusion by neighboring left-wing mayor because of the ongoing political competition between the two parties.[4] Whistle-blowing would yield electoral consequences, with the principal punishing the sluggish mayor. Vice-versa, a leftist mayor may be reticent about reporting to voters the underperformance of a peer from the same party. Leaking underperformance to voters would undermine the party's reputation and ultimately backfire, unintentionally threatening the popularity of the diligent mayor as well. Party discipline may also discourage any accusation towards neighboring fellows.

From the perspective of the public utility, bribing can become expensive when two mayor-regulators are political competitors. A right-wing mayor discovering side-contracts may blackmail the leftist mayor and the public utility, asking for a larger share of the bribe

[4]It would be interesting to study whether dynamics that go beyond the local level and stem from state-level or federal-level rivalries might fuel political competition at the local level and further minimize collusion possibilities.

in order not to report the illicit behavior to the voters. A left-wing mayor, conversely, could be less demanding, as exposing the collusive behavior would not pose a credible threat because of the aforementioned political stakes.

The theoretical framework helps me formulate the following testable hypothesis:

Hypothesis *Household access to adequate local sanitation services decreases when municipalities co-managing the sewerage network are politically aligned.*

I will test whether groups of municipalities that are "barely" aligned perform worse than group of municipalities that are "barely" misaligned with respect to coverage of the sewage system. (Mis)alignment would occur because of "almost random" victories (or defeats) of the parties ruling the groups of municipalities. To validate my hypothesis, I should find that access to sanitation network is lower in a group of municipalities that coordinate the management of sewers and are politically aligned than in politically heterogeneous clusters. In particular,

3.4 Data

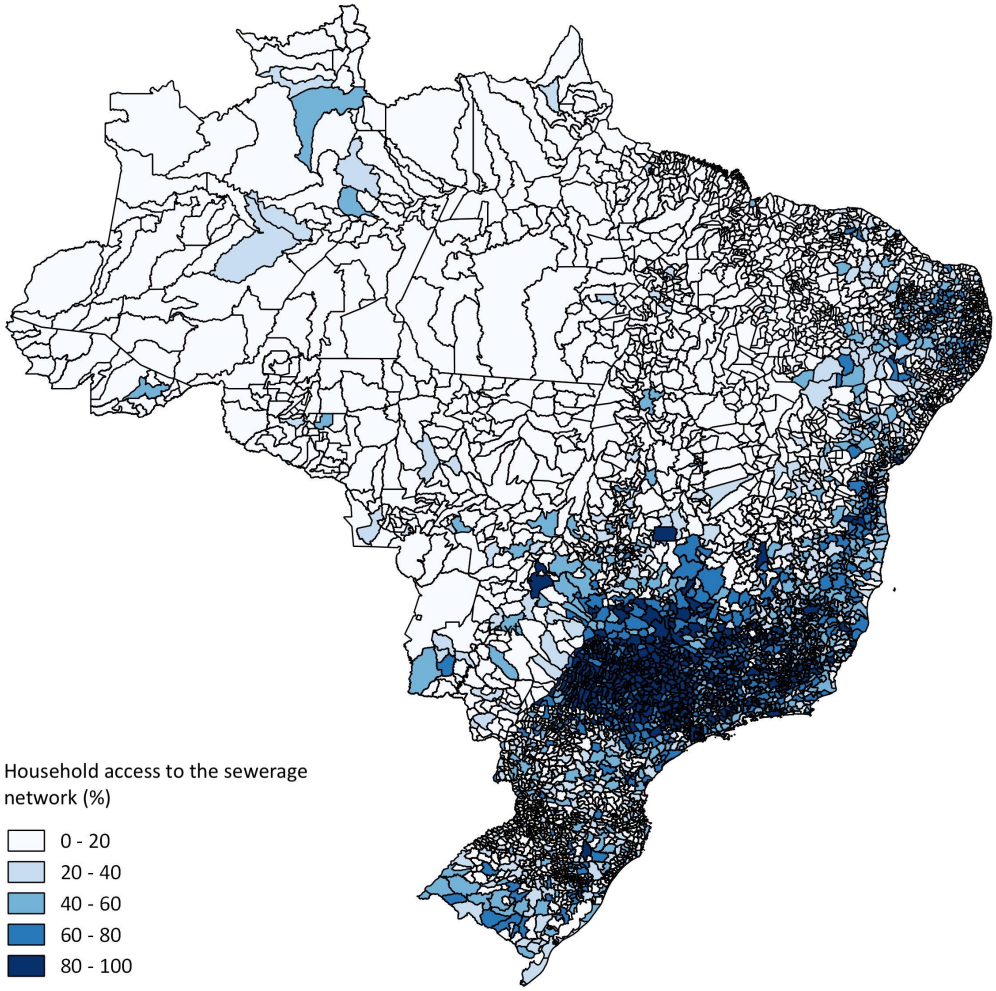
This section presents the main descriptive statistics of the variable of interest – the share of households with access to sewerage system – as well as covariates used throughout the analysis. It also provides some details about one of the crucial assumptions of the analysis – that municipalities with dense neighborhoods are in sufficiently close proximity, coordinate the management of sewers.

3.4.1 Dependent and independent variables

Outcome variable: household access to the sewerage network Data on the accessibility of sewerage network come from the 2010 Census. On average, 29% of Brazilian households have access to sewers; the proportion is slightly higher in urban areas (39%) and dramatically lower in rural areas (3%). Standard deviation is overall quite high. Across states, accessibility varies greatly: the highest average share is registered in southeastern states – 79% in Sao Paulo; the lowest is found in the northeastern region – less than 2% in Piauì (Figure 3.2). Within states, access is on average higher among urban households than rural ones.[5]

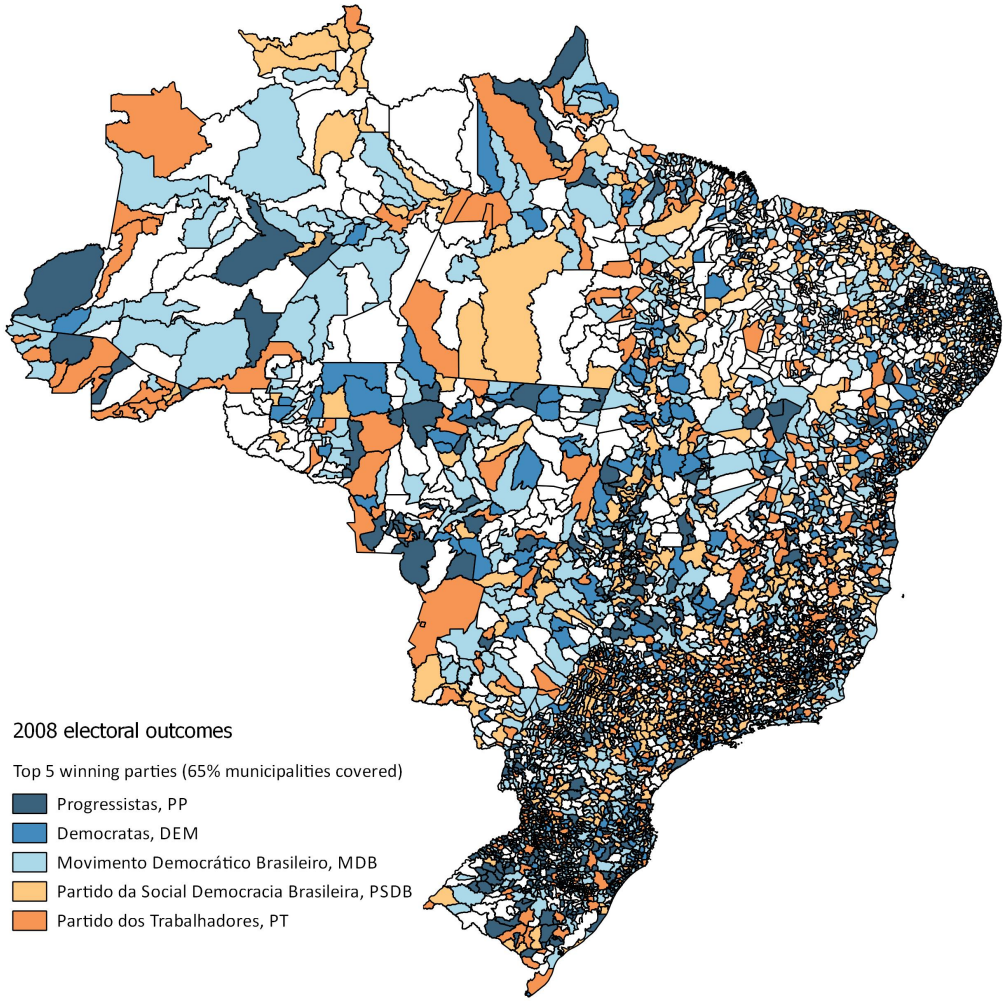
[5]Rates in 2010 were on average higher in Brazilian urban areas than in most other Latin American cities. Yet, Brazil still lagged behind other benchmarking countries in the region, like Chile (67% of urban population using safely managed sanitation services) and Uruguay (64%). Nowadays, the share of urban households with access to adequate sanitation has increased to 40%, the fourth-highest value in the Latin American region after Chile, Uruguay and Mexico.

Figure 3.2: Map of access to sewerage network, municipality level



Running variable: vote margin The running (or score) variable in the regression discontinuity (RD) analysis is the difference in vote shares obtained by the winning party and the runner-up at the 2008 municipal elections. Local electoral data are available from the *Tribunal Superior Eleitoral* and include information about the total number of votes cast for each candidate and party in each municipality. The local political landscape is particularly fragmented, with over 20 parties. Figure 3.3 maps the distribution of the top five most performing parties in the 2008 local elections across Brazilian municipalities: the Brazilian Democratic Movement (*Partido do Movimento Democrático Brasileiro*, PMDB), the Brazilian Social Democratic Party (*Partido da Social Democracia Brasileira*, PSDB), the Workers' Party (*Partido dos Trabalhadores*, PT), the Progressive Party (*Progressistas*, PP) and the Democrats (*Democratas*, DEM). The five parties together controlled over 60% of the municipalities.

Figure 3.3: Outcomes of the 2008 elections, municipality level



Other control variables Alongside electoral data, throughout the specifications I control for the following demographic characteristics of municipalities: the number of households, the average size of households and population density. For each municipality, I compute the average number of neighbors – as defined below – and average access to sanitation among neighbors. Data come from the 2010 Census. I moreover employ the following municipal fiscal characteristics: the average GDP per capita in 2010, average overall expenditure and average overall revenues between 2009 and 2010. Data come from the *Finbra* dataset of the Ministry of the Economy (*Ministério da Fazenda, Tesouro Nacional*).

3.4.2 Definition of "neighboring municipalities" and sample selection

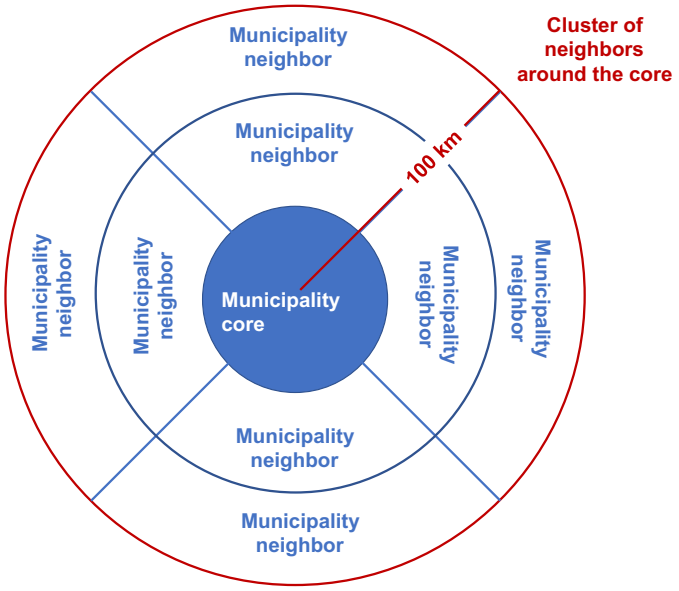
Complete information about the governance of sanitation networks is unavailable at the local level. Ideally, I would test the effect of coordination among aligned mayors on access to public goods by focusing on a sample of municipalities that co-manage sewers. However, the SNIS survey does not allow that strategy to be pursued. The survey covers

only a random sub-sample of municipalities; it is possible in some cases to retrieve details on forms of cooperative engagement for some, but not for the partner-in-cooperation.

To overcome the data limitation, I assume that municipalities whose densest human settlements are sufficiently close, are likely to coordinate the local sewage infrastructure. I use geospatial data of population distribution to identify the densest human settlement in each municipality and compute Euclidian distances thereafter. In sum, the scope of coordination depends on whether municipalities are neighbors; the "neighborliness" does not depend only on border sharing, but rather on the distance between dense human settlements.

The rationale behind this assumption is twofold. First, as recalled in the introduction, poor sanitation infrastructure often lead to the outbreak of deadly waterborne diseases. The impact of these diseases is particularly devastating within highly dense clusters of people. Municipalities whose dense human settlements are sufficiently close would co-manage sanitation infrastructure to avoid the negative externalities of poor sanitation network. Second, the costs of construction and management of sewerage network may decrease with the density of the population. A group of municipalities displaying high concentration of residents across border may have the incentive to join forces and exploit such economies of scale.

Figure 3.4: Schematic representation of an average cluster of neighbors within 100 km from the core



In defining clusters of neighboring jurisdictions, I impose the following three rules.

First, each cluster consists of a municipality core and surrounding municipality neighbors that are at most 100 km away from the core.[6] The distance between the core and each neighbor is defined in an Euclidean sense, as a straight line between the coordi-

[6]The area of the average Brazilian municipality amounts to 1,528 kilometers square (km^2), with a standard deviation of 5,623 km^2, a minimum of 3.5 and a maximum 159,534. Differences in the average municipal surface is present at the state level as well. In the state of Sao Paulo (Southeast region), where the access to sanitation is the highest, there are 645 municipalities with an average area of 385

nates (latitude and longitude) of two points: the centroid of the core and that of the neighbor within the same cluster. Figure 3.4 schematizes the difference between cores and neighbors.

Second, the distance between centroids determining whether municipalities are "neighbors" depends on the population distribution within a municipality. Sharing a common border is a necessary but not sufficient condition for two municipalities to be "neighbors". The "neighborliness" is rather a function of the distance between the municipalities' highest dense human settlements, as identified through satellite data.[7]

Computing distances between municipalities starting from their geographical centroids would be misleading from both an econometric perspective and a policy perspective. People are not necessarily uniformly distributed within a jurisdiction; they are rather concentrated in a non-random fashion. For example, proximity to natural resources, transportation networks or centers of business are key drivers of agglomeration. Geographical centroids do not take into account population distribution. Any definition of proximity based on geographic centroids, and therefore the scope for interjurisdictional coordination, would be biased by endogenous drivers of agglomeration.[8]

Third, municipality cores and municipality neighbors must belong to the same state. As Brazil is a federal country, its institutional and political background can vary significantly between regions. Any attempt to assess coordination between neighboring municipalities that belong to two different states might be spoiled by potential endogeneity issues.

As a result, I obtain a cross-section of 5,536 clusters of neighboring municipalities with on average 70 municipalities within 100 km from the core.[9] To exploit as much as possible the power of the sample, I define ten buffers of 10 km each around the core of the clusters, which gives me ten different subsamples that could be used for the RD analysis.[10] Not all of these subsamples are suitable for analysis; the next section defines an extra rule of eligibility that is based on the treatment status of each of the municipality cores.

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km^2. At the other extreme, in the state of Amapá there are only 16 municipalities with an average surface of 8,927 km^2.

[7] In practice, I adjust the geographical centroids by overlaying polygons of municipalities with the Global Human Settlement layer in *R*. The GHSL combine information from population censuses with information from built-up areas in small grid cells of 38 km^2. The result is a layer representing the presence and density of population in a grid of cells of 1 km resolution that disregards administrative boundaries. These human settlements are then classified according to certain rules of population and built-up density and the contiguity of grid cells into three categories: urban center, urban cluster and rural area (Pesaresi et al., 2016). For each Brazilian municipality I first identify urban centers and urban clusters. Then, starting from this adjusted centroid, I compute the distance from neighboring municipalities within 100 km.

[8] Scholars and policy advisors studying the governance of large and fragmented urban agglomerations have increasingly advocated the need for better tools to correctly define the scope of local spatial policies. For instance, see Brezzi et al. (2012) and OECD (2015) for a discussion of better coordination across jurisdictions that form a metropolitan area.

[9] 24 municipalities, mostly in the Amazonas, were dropped since the distance between their adjusted centroids and those of their potential neighbors was higher than 100 km.

[10] The first subsample encompasses municipalities that are within 10 km from the core; the second sample consider municipalities that are within 20 km from the core, and so on, up to the inclusion of all the possible neighbors within 100 km from the core.

3.5 Empirical analysis

3.5.1 Identifying assumption

In order to test whether interjurisdictional coordination and political homophily between neighboring mayors drives the coverage of the local sanitation network, one could simply compare accessibility in municipalities that are politically aligned with their neighbors with those that are not. However, such a comparison is likely to be biased. For instance, poor coverage of sanitary infrastructure may hamper the popularity of the incumbent local government; this could in turn affect the incumbent mayor's chances of success at the 2008 elections, and ultimately determine political alignment between neighboring municipalities.

To minimize the bias and isolate the causal impact, I employ an RD design. In particular, I look at elections where political alignment between municipalities follows a very close victory (or defeat) of the respective ruling parties (Calonico et al., 2014b; Imbens and Lemieux, 2008; Lee and Lemieux, 2010). Close elections are arguable sources of exogenous discontinuity; as such, they have been already used to study the impact of party identity on various socio-economic outcomes (Dell, 2015; Lee et al., 2004; Meyersson, 2014).

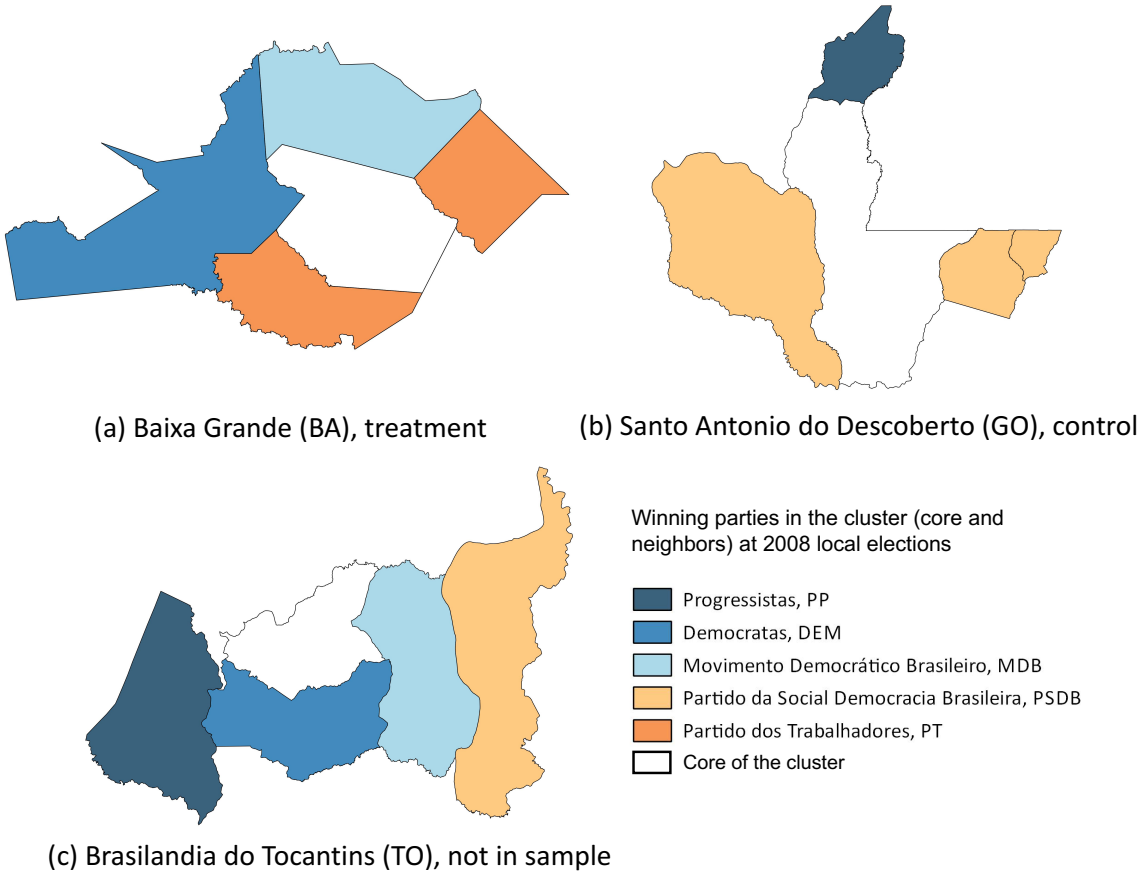
In the RD setup and within any cluster as defined in Section 3.4, the unit of observation is the municipality *i* at the centre of the cluster of neighbors (Figure 3.4). The treatment status is determined by the "score variable", the margin of victory. The margin of victory is the difference between the vote share obtained by the party ruling at least 50% of the neighbors of *i*, and the vote share obtained by the best runner-up in municipality *i* at the 2008 elections. Hence, the score is a continuous variable that can be positive or negative; the cutoff that determines whether a municipality is aligned with its neighbors is located at zero. A positive vote margin means that the party ruling the neighbors has won in municipality *i*: the parties governing municipality *i* and its neighbors are the same, and are therefore said to be "politically aligned". Municipality *i*, in this case, is part of the treatment group. A negative vote margin means that the party ruling the majority of neighbors has lost in municipality *i*: the parties governing in *i* is therefore misaligned with the neighbors, and is part of the "control group". In other words, it is not enough for a municipality to be politically aligned with one neighbor out of many to enjoy economies of scale from coordination. As proposed by Durante and Gutierrez (2015), political (mis)alignment across mayors must be more systemic within the cluster.

As an example, consider the following three clusters in Figure 3.5: Baixa Grande and its neighbors (Mundo Novo, Mairi, Macajuba and Pintadas) in the State of Bahia (Figure 3.5a); Santo Antônio do Descoberto and its neighbors (Águas Lindas de Goiás, Novo Gama, Alexânia and Valparaíso de Goiás) in the State of Goiás (Figure 3.5b); and Brasilandia do Tocantis and its neighbors (Presidente Kennedy, Tupiratins, Itaporã do Tocantins and Itapiratins) in the State of Tocantis (Figure 3.5c). All the clusters encompass neighbors that are within 40 km from the core.

Baixa Grande is ruled by PT like two of its four neighbors, while the others have mayors from the Democratas (DEM) and Movimento Democrático Brasileiro (MDB) parties. Baixa Grande is therefore politically aligned with the majority of its neighbors

and is part of the "treatment group" in the quasi-experimental setting described below. Santo Antonio do Descoberto is ruled by the Partido da República (PR), while three out of four neighboring mayors are from the Partido da Social Democracia Brasileira (PDB). Santo Antonio do Descoberto is thus politically misaligned with its neighbors and part of the "control group". Differently from the first two cases, the mayor of Brasilandia do Tocantis belongs to DEM, while there is no party prevailing in the majority of its neighbors. Brasilandia do Tocantis is not part of the analysis.

Figure 3.5: Examples of clusters by "treatment status"



(a) Baixa Grande (BA), treatment

(b) Santo Antonio do Descoberto (GO), control

(c) Brasilandia do Tocantins (TO), not in sample

Note: This figure represents three different examples of clusters, as defined in Section 3.4. The core is colored in white, while the neighbors are colored depending on the party that won the 2008 elections.

The goal of the RD analysis is to compare "treated" municipalities, where the party ruling the neighbors wins by a small margin, with "control" municipalities, where the party ruling the neighbors loses by a small margin. Under certain assumptions, the RD estimate should measure the causal (local) effect of political alignment between neighboring municipalities on access to sanitation. Assuming that parties cannot systematically manipulate their electoral outcomes, municipalities just above and just below the cutoff will tend to be comparable in terms of all characteristics, with the exception of their political alignment with neighboring jurisdictions. Thus, right at the cutoff, the comparison between politically aligned and misaligned municipalities (treatment and control

groups, respectively) will be orthogonal to observed and unobserved differences between the two groups.

In order to isolate the causal effect of interest, treatment and control groups should be similar with respect to observable characteristics. Table 3.1 presents the mean differences between the two groups of every covariate used in the analysis, for all samples as defined in Section 3.4. Municipalities in the treatment and control groups are not dissimilar *ex ante* for clusters where neighbors are within 40 km, 60 km, 90 km and 100 km from the core.

Table 3.1: Difference in means of control variables, all samples

	Distance from the core of the cluster									
	10 km	20 km	30 km	40 km	50 km	60 km	70 km	80 km	90 km	100 km
Covariate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Access to sewage (%) (neighbors)	2.51	1.04	-0.49	1.27	2.40	0.53	2.63	3.64	5.55*	4.88
Size of cluster	0.00	-0.10	-0.19	-0.01	0.13	-0.05	-0.77	-1.21	0.12	-0.16
Area (km^2)	-282.10	-115.80	-60.19	-242.80	-164.60	-187.00	145	338.80	499.50	1391.20
Population	484.40	21620.10	2279.90	166.80	143.50	-3950.30	3712.4	8287.40	5230.70	5833.90
Urban population (%)	5.264***	1.21	0.27	2.11	3.200*	0.62	4.801**	4.793**	4.35	0.88
Density	20.31	21.97	-5.65	0.48	2.14	-5.06	12.95	2.79	0.16	0.39
No. Households	13.08	6776.90	595.70	55.35	88.23	-1217.70	1038.4	2662.7*	1844.00	1829.80
Household size	0.04	0.01	0.03	-0.01	-0.05	-0.02	-0.0137	-0.04	0.00	0.12
GDP per capita	1371.30	72.34	-658.40	519.80	1751.90	1044.10	1370.5	900.00	1559.40	-702.80
Total expenditure per capita	-98.62	-77.85*	-104.3**	-45.54	63.17	9.69	59.28	115.70	109.90	86.19
Total revenues per capita	-79.95	-72.31	-110.2**	-36.88	62.85	18.47	66.12	127.00	95.23	86.46
DEM	0.07***	0.06***	0.05***	0.04**	0.05**	0.06***	0.09***	0.05*	0.02	0.06*
	(3.16)	(3.93)	(3.18)	(2.01)	(2.54)	(2.61)	(3.13)	(1.89)	(0.46)	(1.74)
PDT	-0.00	0.04***	0.04***	0.00	-0.02	0.00	0.00	-0.03	-0.04	-0.04
	(0.23)	(2.67)	(2.83)	(0.12)	(0.97)	(0.13)	(0.12)	(1.23)	(1.55)	(1.53)
PMDB	-0.18***	-0.17***	-0.25***	-0.24***	-0.24***	-0.25***	-0.25***	-0.24***	-0.20***	-0.30***
	(5.33)	(7.19)	(9.23)	(7.52)	(6.50)	(6.27)	(5.52)	(4.60)	(3.41)	(4.66)
PP	-0.05*	-0.01	0.01	0.06***	0.04*	0.02	0.02	0.01	0.02	0.05
	(1.93)	(0.50)	(0.79)	(2.84)	(1.73)	(0.80)	(0.45)	(0.35)	(0.46)	(1.38)
PR	0.02	0.04***	0.04**	0.03	0.03	0.04	0.04	0.01	0.05	0.03
	(1.54)	(3.27)	(2.48)	(1.32)	(1.41)	(1.47)	(1.18)	(0.30)	(1.10)	(0.71)
PSB	0.04*	-0.01	-0.00	-0.01	0.01	-0.02	0.01	-0.01	-0.04	-0.07**
	(1.79)	(0.93)	(0.08)	(0.69)	(0.27)	(1.18)	(0.55)	(0.31)	(1.05)	(2.01)
PSDB	-0.02	-0.06***	-0.07***	-0.05**	-0.06**	-0.03	-0.03	0.04	0.05	0.04
	(0.78)	(3.04)	(3.05)	(2.16)	(2.21)	(0.89)	(0.87)	(1.04)	(1.43)	(1.06)
PT	0.03	0.05***	0.08***	0.08***	0.11***	0.09***	0.06**	0.07*	0.02	0.05
	(1.27)	(3.42)	(4.71)	(4.22)	(4.38)	(3.31)	(2.03)	(1.96)	(0.48)	(0.91)
PTB	0.04*	0.01	0.01	-0.01	-0.02	0.00	0.01	0.01	0.02	0.03
	(1.82)	(0.50)	(0.66)	(0.42)	(0.97)	(0.03)	(0.30)	(0.40)	(0.47)	(0.74)
N	665	1309	1080	792	578	469	367	286	223	190
No. Obs	665	1309	1080	792	578	469	367	286	223	190
Control	358	717	572	443	310	248	190	145	106	90
Treatment	307	592	508	349	268	221	177	141	117	100

Note: The table reports the difference in the means of each variable between the control and the treatment groups for all the samples as defined in Section 3.4. The differences that are significant are indicated as follows:
* significant at 10%; ** significant at 5%; *** significant at 1%.

There are differences in the average political composition of the treatment and the control groups, but they might not jeopardize the identifying assumption. Table 3.1

reports the share of municipalities controlled by each party, for the nine major parties in Brazil – controlling more than 5% of the country's local mayorship. The PMDB seems to be more popular in the control group than in the treatment group across all specifications. In the sample of interest, treated municipalities have more mayors from the DEM and the PT than in the control group.

These differences might become a source of concern if the network of mayors belonging to the PMDB, DEM or PT is stronger and hence makes side-contracting easier. I have not found any evidence (scientific or anecdotal) in support of this argument and therefore conclude that skewed political distribution might not pose problems with respect to the robustness of the analysis.

The benchmark analysis will refer to the first of the three samples, as this is the one with the largest number of observations (column 4 in Table 3.1).

3.5.2 Estimation of the treatment effect

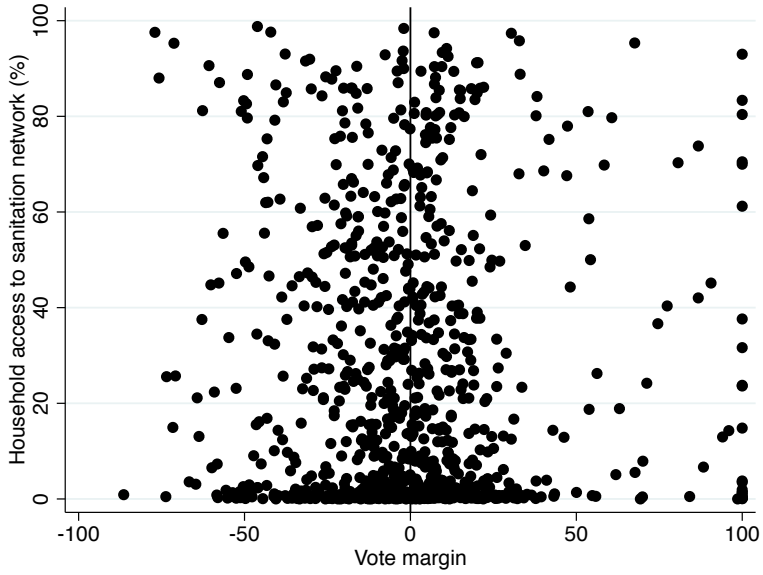
To begin, I plot the distribution of the outcome variable – the share of households with access to a sanitation network – against the running variable – the vote margin. In Figure (3.6a), each point corresponds to a raw municipality-level observation in the benchmark sample. There are 792 points in the scatter plot; 349 municipalities are to the right of the cutoff (treatment group) while the others (443) form the control group. There is a concentration around the cutoff of municipalities where less than 20% of households have access to the sewerage system. In very few municipalities, the candidate has won with an absolute vote margin higher than 50%.

Figure (3.6b) is a smoothened representation of the previous distribution. The score variable is partitioned in a number of quantile-spaced bins. The number is determined non-parametrically, such that the binned sample means have an asymptotic variability approximately equal to the amount of variability of the raw data (Calonico et al., 2015a). To visualize any discontinuity at the cutoff, I fit separately the outcomes in the treated and control groups as a fourth-order polynomial function of the running variable.

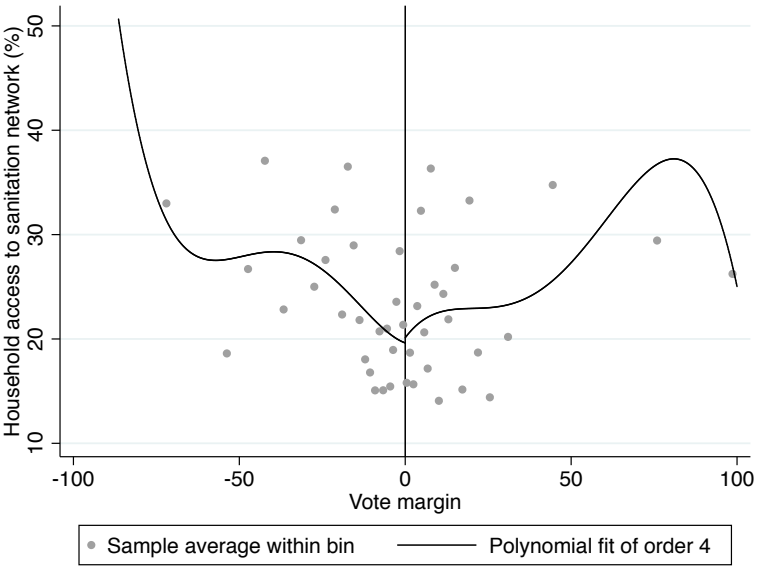
A visual analysis of raw data seems to suggest that political alignment may improve access to sanitation, in line with the existing literature. Figure (3.6b) shows that the share of households with access to the sewerage network increases (although slightly) right at the point at which the vote spread becomes positive, that is when the candidate of the party in power in the majority of neighbors is elected in the core. This result would thus prove the hypothesis that this study aims to test – that political alignment between mayors that coordinate inter-jurisdictional sewers could undermine the quality of a public service.

The non-linear behavior of the outcome-running variable relationship and the presence of outlying observations may prejudice the identification of a causal effect between the outcome and score variables. The distribution of raw data and the need to establish a causal relationship between access to sanitation and political alignment between co-providers requires the employment of local polynomial methods. Since these methods essentially localize the polynomial fit by using observations just around the cutoff, the outlier-driven bias is minimized and low-order polynomial approximation can be used.

Figure 3.6: Local mean estimates with different quantile-spaced bins



(a) Non-adjusted scatter plot



(b) Quantile-spaced non-parametric distribution with polynomial fit

Local polynomial point estimation

I test the relationship between the outcome of interest and the vote margin for a series of bandwidths h around the cutoff $\overline{x}$, where $h > 0$ and, in our case, $\overline{x} = 0$. By using a triangular kernel function, more weight is assigned to observations that are closer to the cutoff $\overline{x}$ rather than those that are further away within the bandwidth $[\overline{x} - h; \overline{x} + h]$.

I moreover use both a linear and quadratic polynomial to fit observations around the cutoff.

Formally, I estimate the following equation:

$$y_i = f(x_i) + \rho D_i + \beta \mathbf{X} + \epsilon_i \tag{3.1}$$

where y_i is the outcome of interest in municipality-core i – the share of households with access to a sanitation network in 2010; x_i is the vote margin – the difference between the party's vote share and that of its closest competitor in municipality i, after the 2008 elections; and D_i is the regressor of interest since it measures whether the party governing at least 50% of municipality i's neighbors won the election in municipality i in 2008. D_i is correlated and a deterministic function of x_i, since it is discontinuous in x_i at the cutoff $\overline{x}$ – formally, $D_i = \mathbb{I}(x_i \geq \overline{x})$; $f(x_i)$ is a p-th order polynomial that fits the local relationship between the outcome and the running variable on both sides of the cutoff.

Results for both first- and second-order polynomial function are computed. In order to better fit the unknown regression functions on each side of the cutoff, I include the covariates presented in Table 3.1, as well as a dummy that indicates whether municipality i is a State capital. Finally, standard errors are clustered at the level of the cluster of neighboring municipalities, encompassing the "core" and its neighbors, as defined in Section 3.4 (hereinafter, the *municipality-cluster* level).

As in the work of Brollo et al. (2013), the goal is to estimate the average treatment effect (ATE) in close elections:

$$\mathbb{E}\left[y_i(1) - y_i(0) | \overline{x_i} = \overline{x_i}\right] = \mathbb{E}_{h \to 0}\left[y_i | x_i = \overline{x_i} - h\right] - \mathbb{E}_{h \to 0}\left[y_i | x_i = \overline{x_i} + h\right] \tag{3.2}$$

The ATE will ultimately be captured by the estimation of parameter ρ in baseline Equation (3.1). A negative ρ would validate the first intuition inspired by Figure 3.6. Access to sanitation improves in neighboring municipalities that are politically aligned. The main issue now concerns choice of h and of the size of the bandwidth.

Selection of the bandwidth I estimate the baseline Equation (3.1) for municipalities where the parties ruling the neighbors won the 2008 elections in the core by less than 20, 10 and 5 percentage points. The following thresholds define three different bandwidths around the cutoff: $h = 20$, $h = 10$ and $h = 5$.[11] The smaller the bandwidth, the narrower the margin of victory in municipality *i* of the party ruling the majority of *i*'s neighbors. Shrinking the bandwidth helps minimize the bias error that would otherwise arise in the presence of outlying values and low-order polynomials. Moreover, as the bandwidth gets narrower, political alignment between municipalities can be considered as good as random. Narrow bandwidths are therefore crucial to establishing a "causal"

[11] The local polynomial estimations are implemented in the package `rdrobust`, available for both *R* and *Stata*. The package is presented in three companion software articles: Calonico et al. (2014a), Calonico et al. (2015b) and Calonico et al. (2017).

relationship between interjurisdictional coordination and access to sanitation. However, as the number of observations within bandwidth decreases, the variance of the estimated coefficients may increase. This "bias-variance trade-off" may be tackled by selecting bandwidth non-parametrically, driven by the data.

In this regard, Imbens and Kalyanaraman (2012) propose an RD estimator that solves the following minimization problem:

$$\min_{h>0} \left(h^{2(p+1)}\mathbb{B}^2 + \frac{1}{nh}\mathbb{V} \right) \tag{3.3}$$

where $\mathbb{B}$ and $\mathbb{V}$ are respectively the bias and the variance associated with the estimation of the coefficient of interest ρ in Equation (3.1). The solution to the minimization problem is the threshold h_{MSE} which minimizes the asymptotic means-squared error (MSE) of the local approximation. The MSE-optimal bandwidth is given by:

$$h_{\mathsf{MSE}} = \left(\frac{\mathbb{V}}{2(p+1)\mathbb{B}^2} \right)^{(2p+3)^{-1}} n^{(2p+3)^{-1}} \tag{3.4}$$

Equation (3.4) gives the intuition behind the choice of h_{MSE}. The bandwidth should be sufficiently large to employ enough observations to minimize the asymptotic variance of the point estimator. Since low-order polynomial functions are used, the larger the bandwidth, the higher the risk of approximation errors. The bandwidth should therefore be narrow enough to minimize the approximation bias of the RD estimator. In sum, the minimization of this "bias-variance" trade-off is at the core of the choice of h_{MSE} (Cattaneo et al., 2017).

Local polynomial inference The local polynomial point estimation is based on fitting two weighted least-square regressions around the cutoff. Regular ordinary least-square (OLS) inference methods cannot be used. OLS methods assume that the local polynomial regression model is correctly specified, while in this case I am just approximating the unknown regression functions. Although the size of the misspecification bias can be controlled through the size of the bandwidth, it will never be neutralized. Moreover, the "bias-variance trade-off" forbids considering bandwidths that are too small. Thus, inference analysis based on h_{MSE} cannot be conducted as if the bias were zero. Calonico et al. (2014b) recommend a robust-bias corrected confidence interval: the adjusted confidence interval combines the MSE-optimal bandwidth h_{MSE} with a modified test statistic accounting for the misspecification bias induced by h_{MSE}. Formally, the RD point estimator $\hat{\rho}$ is assumed to approximately follow the distribution:

$$\frac{\hat{\rho} - \rho - \mathbb{B}}{\sqrt{\mathbb{V}}} \sim N(0,1)$$

The asymptotic 95% confidence interval for $\hat{\rho}$ is therefore given by:

$$CI = \left[(\hat{\rho} - \mathbb{B}) \pm 1.96 \cdot \sqrt{\mathbb{V}} \right] \tag{3.5}$$

Based on Equation (3.5), three types of confidence intervals can be computed: "conventional", "bias-corrected" and "robust bias-corrected". "Conventional" confidence intervals assume the misspecification bias $\mathbb{B}$ in Equation (3.5) to be zero. In "bias-corrected" confidence intervals, the misspecification bias induced by the choice of bandwidth is estimated and removed from the distributional approximation – Equation (3.5). As a further improvement, "robust-bias corrected" confidence intervals include the extra variability brought by the estimation of the misspecification bias.

"Robust-bias corrected" confidence intervals must be preferred when using local polynomial estimation methods. Regardless, the related estimator $\left(\hat{\rho} - \widehat{\mathbb{B}}\right)$ remains suboptimal relative to the "conventional" estimator. As suggested by Cattaneo et al. (2017), in the following section I present the "conventional" MSE-optimal RD point estimator. For inference purposes, the p-values associated to the "robust bias-corrected" confidence intervals are reported in brackets.

3.6 Results

Baseline results The estimation of Equation (3.1) through a local linear regression are presented in Table 3.2 for each of the three aforementioned bandwidths. In this section, the dummy "Alignment$_i$" captures whether the mayor of municipality i comes from the same party as the mayors governing in the majority of neighboring municipalities. The "cluster characteristics" include the covariates average access to sewerage in neighbors within the cluster and size of cluster. The "population characteristics" of municipality i area: area, population, share of urban population, population density, average number of households and average size of households. The "fiscal characteristics" of municipality i include: GDP per capita, total expenditure per capita and total revenues per capita. A dummy variable for whether the municipality i is the capital of the State is also included.

Table 3.2: Parametric fit (local linear regressions) of access to sanitation limited to close elections

	Linear			Quadratic		
	(1)	(2)	(3)	(4)	(5)	(6)
	h=0.2	h=0.1	h=0.05	h=0.2	h=0.1	h=0.05
Alignment$_i$	-1.6	-3.72	-7.12	-4.90	-8.49	-13.07
	(0.27)	(0.13)	(0.08)	(0.15)	(0.11)	(0.03)
Cluster characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Population characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Observations	530	336	169	530	336	169
Treatment	253	161	84	253	161	84

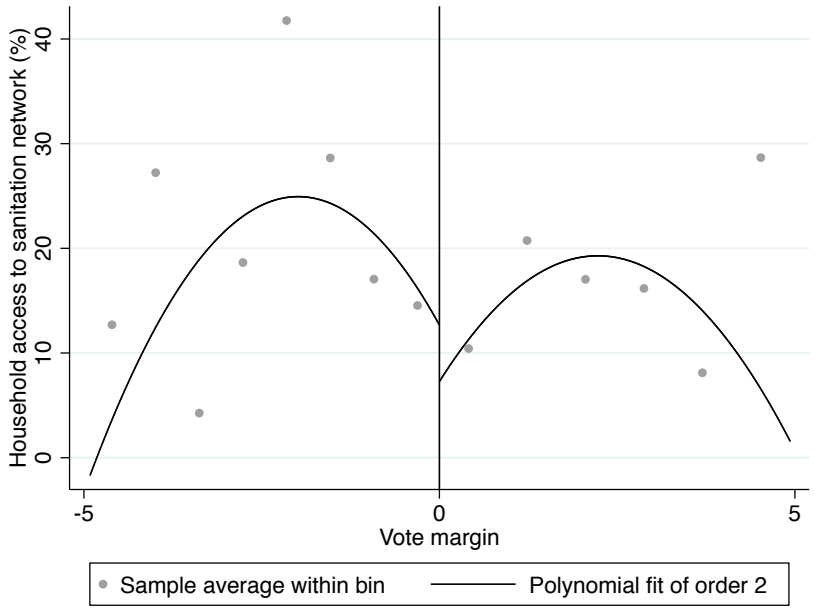
Note: The dependent variable is household access to the sewerage network in municipality i. Standard errors are clustered at the municipality-cluster level. "Robust-bias corrected" p-values are in brackets.

Access to sanitation decreases by 13% in municipalities that are aligned to neighboring

municipalities after very close elections. The robust bias-corrected results indicate that for $h = 0.05$, that is, very small margins of victory – and thus "bare" alignment – the coefficient of interest is negative and significant. This results is robust to the specification and validates the hypothesis discussed in Section 3.3.

The treatment effect is non-linear. Figure 3.7 offers a graphic representation of the estimated RD effect around the cutoff, for $h = 0.05$. The significant jump around the cutoff shows that access to sanitation falls discretely in neighboring municipalities that became politically aligned "almost by chance" after the 2008 elections.

Figure 3.7: The RD effect around the cutoff, for $h = 0.05$



Spatially heterogeneous results The costs and benefits of installing sewers might differ across Brazilian states and thus induce heterogeneity in the results. For example, northern municipalities are particularly large and have an abundance of natural obstacles that make sanitation networks particularly costly. In this case, the benefits from cooperation are so high that they could trump potential drawbacks from collusion among fellow mayors. One could expect political alignment to facilitate coordination and improve access to sanitation services, even at the cost of lower accountability and potential hidden side-contracting.

To identify spatial heterogeneities I test the same model as in Equation (3.1) for the five macro-regions of Brazil. I use a first-order local polynomial approximation with triangular kernel function as well standard errors clustered at the municipal cluster level. The model is tested only for municipalities where the party ruling the rest of the cluster has won or lost elections by 20% or less. Considering a smaller bandwidth for each sample-region would be too costly in terms of observation.

The results show that political alignment positively drives access to adequate sanitation infrastructures only in northern neighboring municipalities. The coefficient in

column 1 of Table 3.3 is indeed positive and significant at almost 10% level. This result is in line with the above intuition.

Table 3.3: The RD effect by region

	North	Northeast	Centre-west	Southeast	South
	(1)	(2)	(3)	(4)	(5)
Alignment$_i$	0.82	-6.39	1.88	2.26	2.84
	(0.09)	(0.18)	(0.27)	(0.98)	(0.68)
Cluster characteristics	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	Yes	Yes	Yes	Yes	Yes
Fiscal characteristics	Yes	Yes	Yes	Yes	Yes
Observations	123	271	94	136	163
Control	67	140	56	84	92
Treatment	56	131	38	52	71

Note: The dependent variable is household access to the sewerage network in municipality i. Standard errors are clustered at the municipality-cluster level. "Robust-bias corrected" p-values are in brackets. Official names of the regions are *Norde* (column 1), *Nordeste* (2), *Centro-Oeste* (3), *Sudeste* (4) and *Sul* (5).

3.7 Robustness test

Testing the exogeneity of the "treatment" The causality of the effect of political alignment on access to sewerage in neighboring municipalities depends on the exogeneity of the "treatment" – that is, political alignment itself. To begin with, access to sanitation in a municipality must vary smoothly with respect to the margin of victory (or loss) of the party governing the cluster of neighbors. Then, only political alignment can have an effect on access to sanitation at the discontinuity threshold (Caughey and Sekhon, 2011).

If treatment were not exogenous, other background characteristics of the municipalities affected by political alignment between neighbors might exist, and these and that in turn could spoil the causal relationship of interest. For example, if politically misaligned neighboring municipalities were fundamentally different with respect to GDP per capita, then underlying poverty levels might indirectly affect household access to the sewerage network.

I test these two assumptions for a sample of municipalities where the party that controls the majority of neighbors won or lost the elections by 5% or less. This is indeed a bandwidth for which a sound causal relationship between political alignment and provision of sanitation network can be established, at least locally. The results are presented in Table 3.4.

There are no significant differences in observable characteristics between the treatment and control groups, and political alignment is not correlated with almost any of them. Column 3 of Table 3.4 shows that the municipalities where the party ruling the cluster "barely" lost the elections are similar to those where it "barely" won, reaffirming the comparability of the two groups. Column 4 presents the results of a simple RD analysis where each control is as dependent variable and political alignment is the dependent variable; column 5 shows the corresponding robust bias-corrected t-statistics.

Here, the observable characteristics and the "treatment" are not significantly correlated. It seems instead that alignment is more likely when the PMDB Party governs the clusters of municipalities (column 5).[12] Overall, Table 3.4 hence reaffirms the validity of the two hypotheses on which the identification strategy relies.

Table 3.4: Descriptive statistics, means difference and RD estimates for covariates in restricted sample $(h = 0.05)$

	(1) Party ruling the cluster lost by <= 5 p.p.	(2) Party ruling the cluster won by <= 5 p.p.	(3) t-stat on means difference	(4) RD estimate	(5) t-stat on RD estimate
Access to sewerage (neighbour)	19.19	17.75	(0.36)	0.34	[1.43]
Size of cluster	6.01	6.94	(1.18)	-1.51	[-0.77]
Area (km^2)	1640.96	1401.01	(0.66)	-1487.50	[-0.80]
Population	18422.43	25604.23	(-1.17)	-5262.70	[-0.41]
Urban population (%)	61.46	59.15	(0.68)	-2.84	[-0.02]
Density	22.94	29.77	(-1.04)	-2.45	[-0.46]
No. Households	6770.08	6597.92	(0.10)	-7493.20	[-1.27]
Household size	3.38	3.47	(-1.30)	0.25	[1.27]
GDP per capita	12364.46	11717.36	(0.41)	-4331.60	[-1.12]
Total expenditure per capita	1598.63	1602.01	(-0.03)	-58.26	[-0.07]
Total revenues per capita	1628.03	1624.68	(0.03)	-16.44	[0.14]
DEM	0.08	0.05	(0.87)	-0.07	[-0.91]
PDT	0.06	0.08	(-0.66)	-0.06	[-1.21]
PMDB	0.21	0.38	(-2.53)	0.52	[2.53]
PP	0.12	0.10	(0.42)	-0.15	[-0.91]
PR	0.07	0.10	(-0.62)	-0.04	[-0.49]
PSB	0.08	0.08	(-0.07)	0.03	[0.21]
PSDB	0.13	0.12	(0.15)	0.13	[0.97]
PT	0.09	0.01	(2.37)	-0.06	[-0.99]
PTB	0.06	0.08	(-0.66)	-0.08	[0.06]
Observations	84	85			

Note: The table reports descriptive statistics for all covariates used in the regression analysis above, and in particular in Columns 3 and 6 of Table 3.2. Statistics are reported for municipalities where neighbors are within 40 km and where the party ruling the majority of the neighbors lost or won the election by 5% or less – for the control group and treatment group, respectively. The table reports the t-stat on the difference in the means of each covariate between the control and treatment group (Column 3), the RD estimates (Column 4) and the corresponding robust bias-corrected t-stat (Column 5). The RD estimation is based on a first-order polynomial, triangular kernel function, where errors are clustered at the municipality-cluster level.

Comparing neighboring municipalities Another source of concern relating to the robustness of the results could arise from comparability among neighboring municipalities between the treatment and the control group. The observable characteristics of municipalities surrounding the centroid of a cluster might explain the local electoral results, political alignment and access to the sanitation network.

[12] There are significantly more aligned municipalities that are governed by the PT. However, clusters ruled by PT are not as likely to be aligned as PMDB clusters.

I test the robustness of the baseline results against this possibility by comparing the average characteristics considered in Equation (3.1) in clusters where neighboring municipalities are politically aligned with the centroid against those where neighboring jurisdictions are not politically aligned. I concentrate on those clusters where alignment is almost random – that is, where the party controlling the majority of neighbors won or lost the elections by 5% or less in the cluster's centroid. The results are presented in Table A3.1 (in the Annex) and show no difference in observable characteristics between neighboring municipalities at the opposite side of the cut-off.

Non-parametric estimation of the baseline model Parametric bandwidths do not take into account bias and variance in the underlying data. Accordingly, I run a non-parametric RD analysis that relies on the MSE-optimal bandwidth choice. Table A3.2 (in the Annex) reports the results for different MSE-optimal bandwidths h_{MSE} and polynomial functions. In columns 1 and 3, the same h_{MSE} is used on both sides of the cut-off; and the RD effect is estimated for bandwidth $[-12.50, 12.50]$ and $[-19.79, 19.79]$, respectively. In columns 2 and 4, h_{MSE2} is allowed to be different on each side of the cut-off; the bandwidths considered are $[-17.56, 11.22]$ and $[-26.95, 21.07]$.

Results of the non-parametric RD analysis are in line with those from the baseline model, but lose significance. When the same MSE-optimal bandwidth is used on both sides of the cut-off, the RD effect of "bare" political alignment on households' access to the sewerage network is still negative and between 3% and 5% in magnitude. The effect is similar when we consider different MSE-optimal bandwidths on each side of the cut-off. The coefficient is not statistically significant for any type of bandwidth, as suggested by the robust bias-corrected p-value.

This test might sound discouraging but I cast doubts on its relevance. The coefficients obtained through non-parametric RD analysis are based on quite large bandwidths, for which the identifying assumption of causality would not hold (there are no close elections where a candidate wins with a 20% margin of victory). Baseline results, instead, are significant at the 5% level and for a very small margin of victory, thus ensuring randomness and causality in the results.

3.8 Conclusion

This chapter tests the causal effect of coordination between municipalities ruled by the same party on access to sanitation infrastructure in Brazilian municipalities. In Brazil, the constitution decentralizes management of sanitation services to the municipal level and encourages local administrators to coordinate the provision of sanitation services across borders.

The coordinated management of sanitary infrastructures may therefore be advantageous, especially if decision-makers come from the same party. Coordination among local providers, moreover, helps mitigate the negative externalities that could arise from lagging access to adequate sewerage. Most of the literature shows that political alignment between decision-makers facilitates coordination, as it reduces bargaining and other transaction costs.

In this chapter, I test whether political alignment in coordination may have drawbacks. In Brazil, mayors in charge of a common sewer may be more likely to collude if they

come from the same party, thus under-providing access to adequate sanitation for their voters. Collusion is more likely because of reciprocity or fear that any scandal involving a fellow peer might result in a backlash affecting personal careers. Collusion is instead less likely when two mayors come from different parties, as they may put more effort into policing each other as a result of political calculus. In sum, when cooperative mayors are political aligned the access and quality of a public good in a municipality may be lower than what the society finds optimal.

I validate this hypothesis by focusing on a group of neighboring Brazilian municipalities that became politically (mis)aligned because of narrow margins of victory after the 2008 local elections. The results are obtained through a local polynomial inference of an RD effect. They indicate that access to sanitation is *lower* in municipalities that are politically aligned with their neighbors. The results are nonetheless heterogeneous across the country. Political alignment seems to be beneficial for neighboring northern municipalities, whose geographical and demographic characteristics are such that the benefits of politically driven coordination trump potential drawbacks.

These results represent a first informative step towards understanding interjurisdictional dynamics across peers in emerging countries. In decentralized settings, local governments strengthen the relationship with neighbors in order to integrate local public services. In this context, correct assessment of local political incentives within clusters of municipalities might help minimize the risk of side-contracting and improve the quality of public goods. In the future, I plan to refine these results in two ways.

First, there is a need for an identification strategy that is less costly in terms of observation. The quest for an exogenous source of variation of political alignment requires a restrictive definition of treatment status. The threshold that determines whether a group of neighboring mayors is political aligned or not (50% of the neighboring municipalities) might be too high, especially given the particularly fragmented political scenario in Brazil. It may be just enough for a municipality to have one single key neighboring partner for coordination to be viable and access to sanitation networks to increase.

Second, I plan to gather more first-hand information about the local governance of sanitation. The lack of complete data on the actual form of local cooperation between municipalities motivated the use of within-jurisdiction population distribution as a proxy for coordination. Municipalities whose densest urban settlements are sufficiently close in proximity are assumed to be coordinating the management of sewers. The results would gain in precision and policy insight if it were clear which municipalities coordinate public good provision – and how.

Acknowledgments

This chapter benefited of the extremely useful insights by Elisabetta Cornago, Luisa Dressler, Antonio Estache, Gregoire Garsous and Ana Moreno Monroy. Participants to the OECD WiP seminar provided valuable inputs, too. I am moreover thankful to Nancy Lozano Gracia: her works on interjurisdictional coordination in Colombia and our discussions during my research visit to the World Bank in 2016 inspired this chapter.

Appendix

Table A3.1: Testing the exogeneity of the reform

	(1) Party ruling the cluster won by <= 5 p.p.	(2) Party ruling the cluster lost by <= 5 p.p.	(3) t-stat on means difference	(4) RD estimate	(5) t-stat on RD estimate
Access to sewage	19.19	17.75	0.36	0.34	1.42
Area (km^2)	1284.37	1438.34	-0.50	-736.68	-0.65
Population	23350.95	22641.17	0.12	-22605.00	-1.14
Urban population (%)	60.14	60.84	-0.21	-6.07	0.03
Density	29.53	31.46	-0.28	7.50	1.03
No. Households	6770.08	6597.92	0.10	-7493.20	-1.27
Household size	3.41	3.45	-0.46	0.19	1.08
GDP per capita	12347.64	13907.30	-0.44	9362.60	0.61
Total expenditure per capita	3229.47	2777.97	0.55	-2507.40	-0.59
Total revenues per capita	3407.44	2901.32	0.53	-2916.00	-0.64
Observations	85	84			

Note: The table reports descriptive statistics for all covariates used in the regression analysis above, and in particular in columns 3 and 6 of Table 3.2. Statistics are reported for municipalities whose neighbors are within 40 km and where the party ruling the majority of the neighbors lost or won the election by 5% or less – for the control group and treatment group, respectively. The table reports the t-stat on the difference in the means of each covariate between the control and treatment groups (column 3), the RD estimates (column 4) and the corresponding robust bias-corrected t-stat (column 5). The RD estimation is based on a first-order polynomial, triangular kernel function, where errors are clustered at the municipality-cluster level.

Table A3.2: Non-parametric fit (local linear regressions) of access to sanitation

	Linear		Quadratic	
	(1) h_{MSE}	(2) h_{MSE2}	(3) h_{MSE}	(4) h_{MSE2}
Alignment$_i$	-2.66	-3.72	-5.23	-3.88
	(0.369)	(0.27)	(0.20)	(0.33)
Cluster characteristics	Yes	Yes	Yes	Yes
Population characteristics	Yes	Yes	Yes	Yes
Fiscal characteristics	Yes	Yes	Yes	Yes
Observations	391	433	538	547
Control	204	256	281	301
Treatment	187	177	257	246

Note: The dependent variable is household access to the sewerage network in municipality i. Standard errors are clustered at the municipality-cluster level. "Robust-bias corrected" p-values are in brackets.

Chapter 4

Public goods under appointed versus elected mayors: evidence from policing and crime prevention

Joint work with Ilan Tojerow (Université libre de Bruxelles)

4.1 Introduction

Mayors are the backbones of decentralized institutions. They are in the best position to improve local well-being, given their proximity to citizens' needs. Hence, mayors need the right incentives to honor their mandates and provide local communities with high-quality public goods. In particular, the selection of mayors for office is crucial to determining to whom their decisions are accountable. Broadly, there are two ways to select a mayor. City councils can appoint them and intermediate between voters and the mayorship. Alternatively, voters can speak out and elect their mayors directly.

Reforms that introduced directly elected mayorships have become more common in both OECD and non-OECD countries. To name a few examples, Italy institutionalized the direct election of mayors in 1993; in the United Kingdom, the Greater London Authority had its first directly elected mayor in 2000; and Croatia and Ireland followed in 2009 and 2011. Belgium introduced the direct election of mayors in 2005, but only for one of its regions, Wallonia – a reform that is at the core of this chapter. Burundi inaugurated the post-civil war democratic transition by implementing local elections in 2010. In India, citizens have been directly choosing their mayors since 2016. Academics and policy-makers still need to assess the impact of this institutional setup on local welfare and citizens' access to public goods.

The goal of this chapter is to test whether switching to directly elected mayors affects the quality of a specific public good: safety. We exploit three central features of local government in Belgium. First, in 2005 a reform introduced the direct election of mayors in one of the Belgian regions, Wallonia, while mayors in other regions remained appointed by the respective city councils. Second, contrary to other European countries, all Belgian mayors are chiefs of local police and are responsible for law enforcement in their municipalities. Third, local police is formally the key player in fighting criminality in Belgium.[1] We use a difference-in-difference strategy to compare crime incidence in municipalities with directly elected mayors to those with appointed mayors, before and after the 2005 reform. Data on policy outcome incidence come from a rich dataset on crime events in 589 Belgian municipalities between 2000 and 2012. We observe a post-

[1] Crime is moreover a salient issue for local politicians, especially during electoral campaigns. According to the 2010 round of Eurobarometer surveys, 35% and 25% of Belgians worry about burglary and violent crime, respectively.

reform statistically significant decrease in crime incidence by 4.9% 5.7% in the treatment group. The magnitude and significance of the treatment effect decrease when various mayors share a common local police force, and in particular when these mayors belong to different political parties.

The 2005 reform increased mayors' accountability to citizens and efforts to fight criminality by making local elections more competitive. Starting from the 2006 elections, incumbent mayors who run for re-election face a wider pool of potential opponents, both from other parties and from the same list. In order to stand out from the crowd of candidates and gain support, their campaigns "specialize" in issues they can control directly – such as those related to safety. As a result, voters acquire more information on candidates' priorities and vote for candidates with clear attitudes towards criminality. Increased electoral competition, moreover, motivates incumbent mayors to tighten control over their police commissioners – the actual law enforcers. Once elected, mayors are less likely to slack off, especially if they pursue their career. Hence, we expect the direct election of mayors to create incentives for mayors to provide better quality public goods.

These results are in line with predictions from the political-economic literature on electoral rules and citizens' welfare, and with the literature on electoral competition and bureaucratic performance. Besley and Coate (2003) find that elected regulators tend to be more pro-consumer candidates than appointed regulators if they seek re-election.[2] Huber and Gordon (2004) find that elected judges issue lengthier sentences as elections near. Choi et al. (2010) argue that elected judges are more productive than appointed judges, at the cost of lower quality output. Besley and Payne (2003) find that elected judges tend to be more career-concerned: they file more employment discrimination charges in order to pander to voters and to seize re-election. Fiorina and Noll (1978) were the first to make a link between electoral competition and size as well as the efficiency of bureaucracy, but this relationship has been investigated more closely only recently.

Shared mandates over local police undermine the beneficial effects of the reform. Police districts in Belgium usually encompass several municipalities and require several mayor-chiefs of police to coordinate law enforcement. While coordination is necessary to exploit economies of scale, it entails costs as well. Moreover, coordination may raise moral hazard issues that could lead to under-provision of the common public good (Dixit, 2002). We predict that the larger the number of mayor-chiefs of police in charge of a police district in Belgium, the less accountable the mayors will be and the lower the difference in crime incidence between municipalities with directly elected mayors and those with appointed ones.

This chapter contributes to four juxtaposing strands of literature.[3] The first strand

[2]According to Besley and Coate (2003), when political bodies select regulators, regulatory policy becomes bundled with other policy issues for which the appointing politicians are responsible. Because voters have only one vote to cast and regulatory issues are not salient for most of them, electoral incentives will lead politicians and their pick for the regulator to respond to stakeholders' interests, rather than voters'. If regulators are elected instead, their stance on regulation is the only salient issue, and they will run being pro-consumer candidates.

[3]A vast political-economic literature has highlighted the role of electoral incentives in shaping politicians' decisions over fiscal variables and a broad spectrum of policy outcomes. See, among others, Alesina and Tabellini (2008), Alesina and Tabellini (2007) and Persson et al. (2007). This literature focuses on national case studies, while this chapter looks at local dynamics.

investigates the relationship between accountability and voters' choice. Scandals leaked to the media and anti-corruption audits often expose voters to more information about the quality of local politicians. During elections, voters may use this information to vote in higher-quality politicians and to vote out lower-quality ones (Banerjee et al., 2011; Ferraz and Finan, 2008). As a result, Ferraz and Finan (2011) find that incumbent politicians that seek re-election tend to be less corrupt. We argue that the introduction of direct election of mayors in Belgium increases the information available to voters by stiffening electoral competition. In tight races, local politicians' campaigns specialize in visible issues over which they have control – contextually, crime. In addition, higher commitment exposes candidates to more accurate screening by the electorate.

Second, this chapter contributes to the literature on electoral rules and their consequences for policy-making. Levin and Tadelis (2010) find that appointed (rather than elected) municipality leaders are more likely to privatize services than directly elected ones. Coate and Knight (2011) show that spending falls following switches to directly elected mayors, relative to jurisdictions not changing their form of government. Conversely, MacDonald (2008) argues that the form of government and the election method of city councilors (together with the size of the city council) does not have a significant impact on public expenditure. Enikolopov (2014) explains that since elected mayors are more likely to value patronage jobs, the number of full-time employees is significantly higher in mayor-council cities. All these articles use evidence from the United States, where institutions differ from those in Belgium.[4] Nonetheless, it provides insights into the mechanisms that motivate elected local politicians to provide better public services. Few papers study the issue in European countries. Hessami (2018) assesses the impact of the gradual introduction of mayoral elections on public choices in a German state. The present chapter studies a similar reform and its effect on local crime incidence.

Third, we contribute to the literature that studies the provision of public goods across jurisdictions. Estache et al. (2016); Soares and Viveiros (2010) show that interaction between elected local administrators at a different level of government may affect politicians' accountability and citizens' rights to quality water and local police. Very few studies focus on the interaction between elected local leaders at the same level of government. Feiock (2007) and Zeemering (2012) argue that the efficient provision of local public goods often relies on voluntary agreements between local administrations. However, shared mandates shift service responsibility from the individual mandate to "collective" mandates. Local leaders may free-ride on their neighbors, ultimately undermining the quality of the shared public good (Dell, 2015; Durante and Gutierrez, 2015). This chapter joins this growing and still limited literature.

Lastly, this chapter contributes to the literature on the economics of crime. Since Becker (1968), economists have tested different instruments to control criminality. Dills et al. (2010) provide a modern and very complete review of the effectiveness of criminal deterrence through arrest, incarceration and the size of the police force. Policies that control deterrence measures affect criminal activity by raising the expected cost of

[4]There are two primary forms of government in U.S. cities: council-manager and mayor-council. Under the council-manager, policy-making power resides within the city council, and the mayor is therefore accountable to appointing politicians. Under the mayor-council form, voters elect separately the mayor and the city council, who then have to govern together. It is our interpretation that council-manager (mayor-council) municipalities are close to those with appointed (directly elected) mayors.

crime or by incapacitating criminals. Few works, however, have studied how political institutions shape the management of the police force and thus the use of deterrence variables. In his seminal research, Levitt (1997), for instance, shows that the size of police forces increases disproportionately in mayoral election years. Ater et al. (2014) investigate the role played by the organizational structure of law enforcement agencies in determining police activity and crime. However, neither accountability of law enforcers nor inter-jurisdictional coordination are the focus of the authors' work.

In sum, this chapter sheds light on the complex effects of increased accountability on the provision of public goods, especially in the presence of coordination across local jurisdictions. We show that directly elected mayors provide better quality public goods than appointed ones. Increased electoral competition motivates candidates to focus their campaign on safety-related issues. Increased accountability forces directly elected mayors to uphold their electoral promises. However, the "accountability effect" might be diluted when a large number of mayors share the provision of public goods.

In order to support our conclusions, this chapter unfolds along the following structure. The next section discusses the two Belgian institutional features at the core of the empirical analysis and identification strategy. Section 4.3 draws from theoretical predictions of the political-economic literature to lay down the main hypothesis tested in this chapter. Section 4.4 describes the data used in the empirical strategy, detailed in Section 4.5. Section 4.6 presents the benchmark results and tests their robustness. Section 4.7 discusses the heterogeneous effects of the reform on crime incidence. Section 4.8 provides some conclusions.

4.2 Institutional framework: mayors and police management in Belgium

This paper explore five crucial features of Belgian institutions. First, municipalities are the backbones of the country's multi-level governance, bear essential responsibilities and are the closest administrative unit to citizens. Second, municipalities are crucial for national and regional parties to gain or preserve power, given their importance and proximity to voters. The battle for the city council and the mayorship is therefore highly competitive and unique to each municipality. Third, a 2005 reform approved by the Wallonian parliament has made the election of mayors in the region more transparent to voters. Flanders and the Brussels region did not implement a similar reform, thereby creating the institutional discontinuity at the core of our analysis. Fourth, local police function as the principal law enforcement body in Belgium, leaving residual tasks to the federal police. Finally, mayors have a crucial role in the management of local police. Therefore, local criminality is one of the most visible outcomes of the performance of a mayor. The management of local law enforcement is a sensitive topic during electoral campaigns, and incumbent burgomasters can use the police as a tool to consolidate power and seek re-election. This section describes these five features and sets the stage for the analysis that follows.

4.2.1 Background: local institutions and politics in Belgium

Belgium is a decentralized country, with municipalities at the core of its multi-level governance. Below the federal level, three regional governments (Flanders, Wallonia and the Brussels capital-region) and three community governments (Flemish, German and French-speaking Communities) coexist and may geographically overlap. Regions are responsible for territorial matters – including infrastructure, industrial policy, employment and taxation. Communities are responsible for people-related matters – including education, welfare and health. Regions are then organized into provinces, arrondissements and municipalities. Municipalities are the closest administrative level to citizens and function as the unit of analysis of this paper. Belgium has 589 municipalities: 308 in Flanders, 262 in Wallonia and 19 in the Brussels-Capital region.

Municipalities bear numerous responsibilities. Between 2000 and 2012 – the period of analysis of this paper – the priority areas of expenditure of municipalities were public administration (17% of total expenditure), road maintenance (15%), school infrastructure (13%), social protection (10%), security and crime prevention (9%) and garbage collection (7%). As explained later, the provision of public utilities is often shared between municipalities, introducing an extra layer of governance, the "inter-municipalities".

In each municipality, a city council holds the legislative power, while a mayor holds the executive power. City councilors are elected every six years by Belgian and European residents of at least 18 years of age, and there is no term limit. The mayor guiding the municipal government comes from the party or coalition of parties that occupy the majority of seats in the city council. Importantly for the analysis that follows, there are differences in the modalities of selection of mayors across regions, and in particular in the weight assigned to citizens' votes.

City councilors are elected according to a proportional system that accommodates both party logic and voters' preferences. Parties present lists of candidates, with as many candidates as the seats in the city council. Eligible citizens can vote only for one party-list by either marking their preference for the whole list of candidates – "list vote" – or for one or several candidates on the list – "preferential vote". Once ballot boxes close, votes are converted into seats in the city council based on the performance of each party-list and each candidate. First, the number of votes obtained by each party is divided by a series of divisors as determined by law. Seats are allocated to the parties that obtain the highest resulting quotients (or, the "highest average"), up to the total number of seats available (Norris et al., 2004). In a second stage, seats are assigned to candidates with the highest electoral score in each list. The candidate with the highest number of "preferential votes" gains a seat. The election of the "head of list" is likely as well, since they directly benefit from "list votes" on top of "preferential votes". Additionally, the complex electoral system is such that candidates that rank higher in the list also have a significant chance of getting elected, independent of their personal performance.[5] The party can still shape the city council by ranking candidates strategically. However,

[5]Candidates are required to meet an "eligibility thresholds", which in turn depends on the party's overall score and seats attributed. The votes for the head of the list constitute a "common pot" which lower-ranked candidates can draw from in order to meet the "eligibility threshold". If the votes obtained by the head of the list – both "preferential" and "list" – exceed the "eligibility threshold", the difference is added to the score of the second-ranked candidate. Any further residual votes are then assigned to the third-ranked candidate, and so forth, up to the full distribution of seats obtained by the list.

voters can reshuffle priorities with "preferential votes". Voters have often resorted to "preferential votes" in local elections in Belgium, given the weight, visibility and direct impact of local policy-makers' decisions. The share of valid ballots that cast at least one preferential vote has been above 70% since 2000, and increased to almost 85% in 2006 (André et al., 2014).

The post-electoral composition of the city council determines the mayorship, although the way in which mayors are selected differs across regions. After the elections, if no party has an absolute majority in the city council, a coalition must be formed. Parties that agree on a policy agenda sign a "majority agreement" and select the mayor. Before 2005, the choice of the burgomaster was also part of the political bargain and did not depend on the mayor's preferential votes (Schamp and Devos, 2016). Any Belgian citizen – even those not elected – could become mayor if backed by a sound majority in the city council. In 2005, Wallonia regulated the election to mayorship, making it automatic and directly dependent on the "preferential votes" obtained by elected councilors.

4.2.2 The 2005 reform introducing direct election of mayors in Wallonia

At the beginning of the 2000s, sharp institutional reforms in Belgium encouraged regions to empower mayors and to increase their accountability towards voters. Citizens from all regions increasingly demanded more accountable burgomasters and local policy-making: 71% of Wallonian voters wished for direct election of mayors, and support was equally widespread in the other regions (Pilet, 2007). In 2001, the fifth State reform created the opportunity to meet this pressing demand. The reform decentralized the organization of local elections and local administrative functions to the regions. The primary goal was to encourage regional governments and parliaments to bring institutions closer to citizens, to broaden mayors' capabilities and to make their choices more transparent to voters.

Political leaders viewed the direct election of mayors as the key to strengthening local democracy. Regional parliaments and local administrators have long debated the advantages and the disadvantages of such a reform. Direct election creates a "chain of delegation" that runs linearly from voters to candidates: politicians who are directly elected would behave in the interest of their voters (Persson and Tabellini, 2005). Citizens' preferences and willingness to finance public expenditure are more likely to shape decisions of directly elected community leaders. Instead, politicians' interests tend to determine the provision of public goods, when intermediary institutions – such as the city council – appoint leaders (Frey, 1994). The consequences of direct elections of community leaders are nonetheless nuanced. Political confrontation may shift from contents towards individual characteristics, as the personality of candidates becomes louder than their messages. Moreover, the overall benefits of the direct election – such as curbing corruption and discouraging the misbehavior of local politicians – may strongly depend on local capabilities (Henderson and Kuncoro, 2011). If mayorship were just a mere step in a candidates' political career, directly elected local politicians might still deviate from their voters' mandate and use their popularity and visibility to climb the regional and national institutions (Micozzi, 2012).

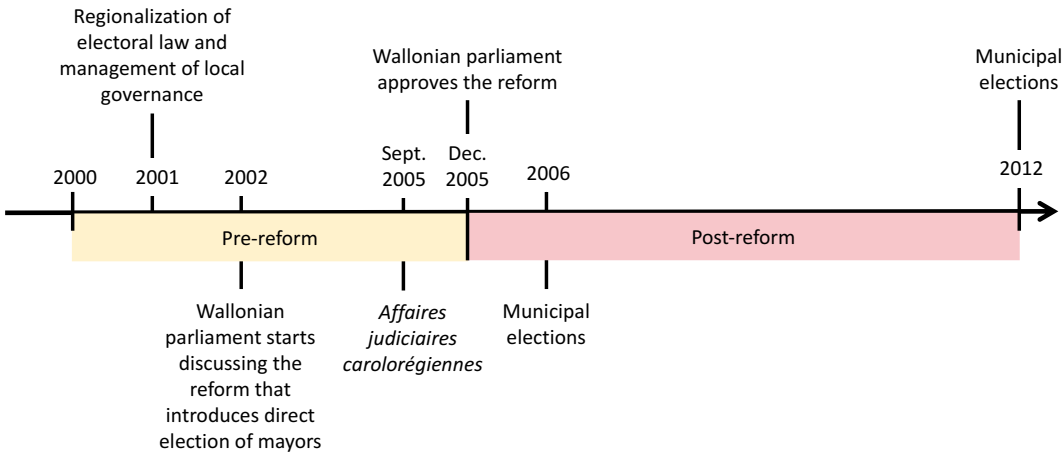
In Flanders, the parliament discussed an ambitious reform that would have introduced a "presidential-type" system of local government. In 2002, Flemish liberals proposed to dissociate elections to city councils from elections of mayors. Seats in the city

council would have still been distributed proportionally, while – similar to presidential elections in the United States – voters would have had the possibility to vote directly for a candidate-mayor "ticket". The second member of the ticket would have stepped in if the first member happened to be ineligible or was impeded from taking up the mayorship throughout the mandate. Like the French presidential system, the reform envisaged a voting runoff: if no "ticket" received the required number of votes in the first ballot, candidates with the largest share of votes would compete in a second round.

The reform lacked the support of most Flemish parties, however proportionally elected city council combined with a two-round mayoral election system might have led to divided local governments, where executive and legislative power refer to two different political areas. Most parties were concerned about the risk of potentially inefficient divided governments as well as the potential personalization of the electoral campaign and the mayorship (Schamp and Devos, 2016). Eventually, the reform did not pass. Voters still cannot directly influence the election of mayors; instead, city councilors name mayors, usually after post-electoral political bargaining.

In Wallonia, a more participative reform process made the confrontation between parties easier but not fruitful. Discussion of the reform began in 2002 and, unlike Flanders, involved both the regional parliament and the subregional governments. The majority of municipal councilors supported the direct election of mayors, albeit within a proportional electoral system. A special committee with representatives of the regional parliament and government, as well as subregional governments, drafted the guidelines of the reform. Nevertheless, for years a political consensus around the actual content of the reform was hard to reach: within the government coalition, the liberals, strong advocates of the reform, were opposed by the Socialist Party, which traditionally leaned more towards representative democracy. The Green Party stood halfway between the two (Steyvers et al., 2004).

Figure 4.1: Direct election of mayors had been discussed since 2002, was approved in 2005 and implemented during the 2006 municipal elections



Local scandals were a game-changer and helped accelerate the introduction of the direct election of mayors in Wallonia. The mismanagement of social housing in one of the main Wallonian cities – Charleroi – leaked to the press in September 2005, resulted

in widespread outrage, made proponents of direct election more vocal and pushed the parties in the regional parliament to find a legislative compromise. Political forces with a majoritarian vocation and those leaning more towards a proportional system reached an agreement and passed the reform at the end of 2005 (Matagne et al., 2011).[6] The reform made its debut at the 2006 municipal elections (Figure 4.1).

Since 2005, the election of a mayoral candidate in Wallonia is automatic and conditional on the performance of the candidate and of her party. The electoral law regulating the distribution of seats in the city council remains proportional. Once polls close and seats are redistributed, a candidate to the city council becomes mayor automatically under three conditions. First, she must belong to the coalition of parties – or "majority agreement" – with the highest share of votes. Second, she must come from the best performing list within the "majority agreement". Third, she must obtain the highest share of votes within the list (Blaise et al., 2006). A mayor who resigns becomes no longer eligible to sit on the city council. She is replaced by the candidate who belongs to the best-performing party in the "majority agreement" and has the second highest share of votes. The 2005 reform introduced elections of mayors that are more automatic and direct than in Flanders and Brussels - where mayors remain appointed by the city council independent from personal electoral performance.

The fundamental hypothesis of this paper is that the reform increased the accountability of local mayors. Direct election of mayors has contributed to changing the behaviors of candidates, parties and voters. In particular, we claim that personalization of the electoral campaign, as well as increased competition, contributed to bringing decision-makers and voters closer.

Direct election of mayors favors the personalization of the electoral campaign. Prior to the reform the political destiny of candidates to Wallonian city councils and the mayorship responded to party logic; virtually every candidate now has a chance to become mayor. All a candidate needs is to maximize the electoral score, pending the performance of the list as a whole. As a result, since 2005 candidates have been participating more actively in capturing preferential votes (Matagne et al., 2011).[7]

Increased electoral competition is another consequence of the 2005 reform. Before the reform, the proportional character of local electoral law encouraged parties to form alliances only at the end of the electoral process and the redistribution of seats in the council. Mayors usually sealed the "majority pact" between the ruling parties – a pact that was not validated by popular vote; mayors' decisions were accountable to the contractors of the pact rather than voters. Following the reform, parties must ponder better their

[6] Decree of 8 December 2005 that modified the *Code de la démocratie locale et de la décentralisation*, art. 14, section 1. The decree is publicly available on *Moniteur belge* of 2 January 2006.

[7] To validate this hypothesis, between June and September 2017 we interviewed several Flemish and Wallonian mayors covering the whole political spectrum. We met administrators from Gosselies, Namur, Neupré and Tintigny in Wallonia, Genk in Flanders, and Anderlecht in Brussels. In some cases, police chiefs attended the meeting as well. Interviews lasted on average one hour and aimed at understanding the impact of the 2005 reform, and mayors' role in the management of local police and other local affairs. The guideline questions used are listed in Annex 8. This strategy is particularly rewarding during local elections, where citizens are more likely to cast their votes for individuals compared to regional or national ballots. Competition within lists has therefore become fiercer, placing candidates under more intense scrutiny and thereby increasing the elected mayors' accountability to voters, rather than to the city council (Ferraz and Finan, 2011).

strategies, forecast first the expected performance at the upcoming elections and evaluate the popularity of candidates from other lists. In uncontested elections, the party in power could run alone - with or without popular parties: an outstanding performance would guarantee an absolute majority in the city council and therefore the mayorship. In more contested elections the hegemony of traditional parties might be threatened by a coalition of opponents led by a popular candidate. At the same time, smaller parties have more chances to break traditional systems of power by forming alliances around popular champions. Hence, parties have been converging into cartels to either consolidate or seize power, polarizing the local political arena. This polarization has further encouraged the preferential vote (Matagne et al., 2011).

The 2005 reform did not apply to municipalities in Flanders and the Brussels regions because of the objections raised by parties in the respective regional parliaments, as discussed above. There is no reason to believe that the behavior of politicians, parties and voters in those municipalities has changed as a consequence of the 2005 Wallonian reform. Hence, parties in these regions are still the power-makers and determine the mayorship, without necessarily accounting for the preferential votes of candidates. Electoral law does not create incentives to form cartels around popular candidates before the elections, and political fragmentation remains a dominant feature. Mayors in these regions still seal the majority pact, and their policies are under the scrutiny of the parties in the coalition, rather than being directly accountable to voters. Moreover, in those regions, any candidate can be appointed into office, irrespective of their personal electoral scope and actual participation in local elections (Matagne et al., 2011).

The direct election of mayors was not the only novelty of the 2006 municipal elections in Wallonia. In all three regions, non-European citizens were allowed to vote (but not to compete) conditional on the residence period in the country and their commitment to the Constitution, the laws and the European Convention on Human Rights. Regional parliaments tightened rules to exclude candidates with racist behavior from the competition and to impose gender balance in the lists. In Wallonia, members of national, regional and European parliaments were not allowed to run for municipal elections (Blaise et al., 2006). Importantly for this paper and its identification strategy, we assume that these measures do not question the exogeneity of the 2005 reform or affect local crime incidence.

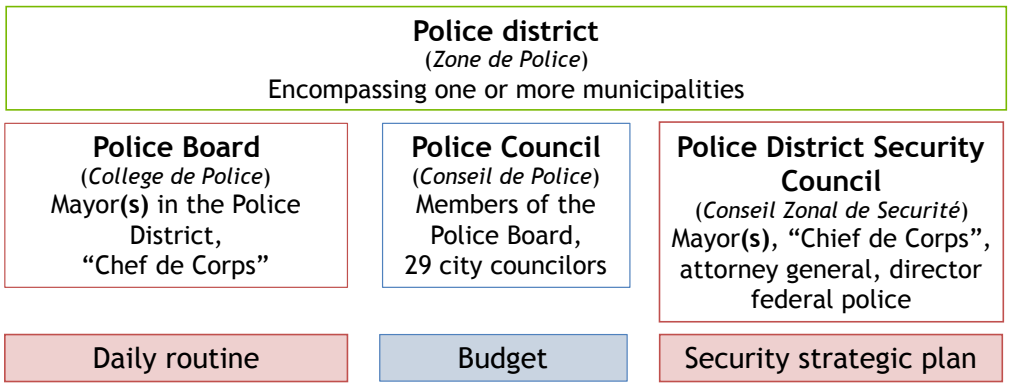
4.2.3 The governance of local police

Local police are the main form of law enforcement within pre-defined districts. There are 195 police districts, each equipped with a police force: 43 of them cover the territory of one municipality, while 152 districts encompass several municipalities. On average, there are four municipalities in a police district, although the size can range from 1 municipality to 10 municipalities. The size of a police district depends on the surface of cities and towns, their density, the rate of urbanization, and social as well as economic conditions. Within the police district, armed local police officers patrol streets and public places, answer citizens' requests at least 12 hours per day and intervene at any time in case of

need. Local police also manage local traffic and conduct criminal investigations under the authority of a magistrate.[8]

Mayors act as the chief of local police and can steer police activity due to their privileged relationship with the police commissioner. Mayors are responsible for the safety in their municipalities and coordinate law enforcement. Interviews conducted in the context of this paper suggest that mayors dedicate between one to four hours a day to dealing with crime-related issues. Police commissioners are designated for a five-year term by the Ministry of Interior under the proposal of the mayor-chief of police. They are responsible for the organization and the redistribution of tasks within the police force, and broadly for the management of police officers. As chiefs of police, mayors directly liaise with the police commissioners to draft and implement long-term crime plans. The police council – made up of a group of city councilors – validates the crime plans (Figure 4.2). In practice, counter-criminal daily actions require timely decisions, jointly taken by the mayor and the commissioner. The close relationship that necessarily develops leaves the chief of police and the police commissioner enough discretionary power to contribute to the consolidation of power of the former, and the career of the latter.

Figure 4.2: Police districts have their own governance framework which requires mayors to interact when several municipalities are involved



In a police district encompassing several municipalities, mayors need to share and coordinate law enforcement. All of them are chiefs of police and meet with the district police commissioner within a "police board". Mayors in the police district informally elect the president of the police board and take decisions unanimously. When mayors share the mandate, it is more difficult for voters to keep them accountable. Each chief of police can free-ride on neighbors within the same police district, ultimately affecting crime incidence. When police districts are very large, and coordination costs are high,

[8]The federal police in Belgium guarantee safety on motorways, railways and stations, waterways and airports, as well as the safety of the royal family. They deal with criminal activity that transcends the boundaries of a police district or the country, such as economic crimes, drug-related crimes and terrorism. The federal police also coordinates and supports the local police. Local police can at any time request the intervention of the federal police when specialized personnel and equipment are needed. Finally, the federal police are responsible for non-operational matters such as human resources, logistics and information technology, and the training of personnel. Mayors do not have any role in the management of the federal police.

mayors may completely delegate the management of police to the commissioner, which in no case is accountable to voters. All in all, the shared provision of policing may ultimately spoil the benefits from the direct election of mayors in Wallonia. The second part of this paper investigates the interaction between the governance of local police and the 2005 reform.

Nevertheless, even with shared mandates, policing remains one of the few policies that mayors can shape directly. Counter-crime policies are effective leverage for incumbent mayors to consolidate popularity and for opponents to gain consensus. Mayors may find it more challenging to affect directly other policy outcomes, instead, as only few public enterprises (or "inter-municipalities") manage waste collection and disposal, and water treatment. These public enterprises cover several administrations, leaving mayors with insufficient room room to steer policy-making.[9] Inter-municipalities are moreover formally independent from the mayoral power, have their own council - appointed by the city council - and president. Mayors cannot exert direct control over social services either; these are managed by ad-hoc municipal institutions that are accountable to the city council only.

The crucial role of local police together with the steering power of mayors is unique in Europe and resembles law enforcement in the United States. In Germany and France, the establishment of local police is an option left to local authorities. In Germany, the law entrusts states with the organization of security and police services. In France, mayors can establish a local police force that nonetheless complements (and sometimes overlaps with) the state police and does not monopolize local law enforcement. Belgium has a setup similar to the United States, where local police account for the majority of police services (Seron, 2004).

4.3 Conceptual framework

This chapter tests whether the modality used to select mayors for office affects the supply of a specific public good: safety. It moreover argues that this effect (if any) depends on local governance, that is how key decision-makers share responsibilities.

4.3.1 The effect of the reform on crime incidence in police districts with one municipality and chief of police

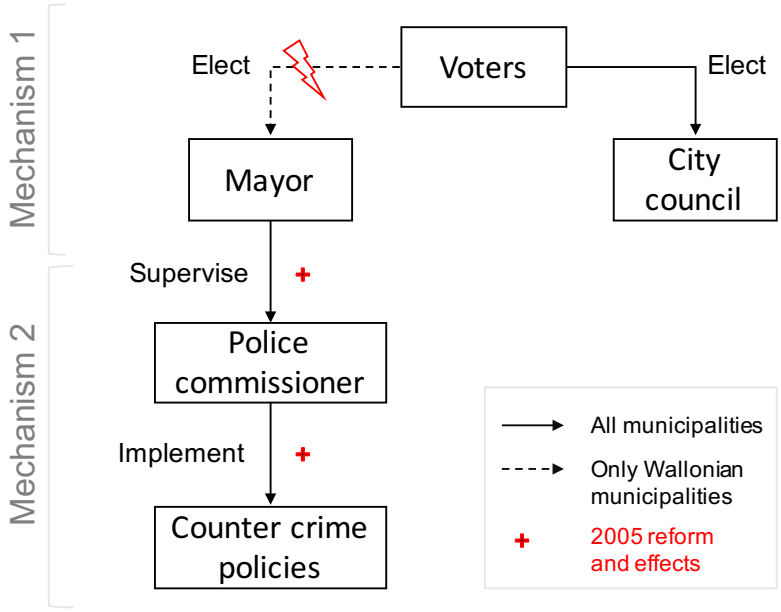
To start, we compare trends of crime incidence between municipalities ruled by directly elected burgomasters and municipalities ruled by appointed mayors, by exploiting the 2005 reform that introduced the direct election of mayors. We predict the following:

Hypothesis 1 *After the 2005 reform, crime incidence decreased faster in municipalities where mayors became directly elected than in other municipalities where mayors remained appointed by the city council.*

[9] For instance, only 31 inter-municipalities manage garbage collection across the country: 5 in Wallonia (one every 52 municipalities), 26 in Flanders (one every 12 municipalities) and 1 for the 19 municipalities in the Brussels region. For more details, see Goethals, Christophe (2017) "La physionomie des intercommunales en Belgique". *Mimeo.*

Two complementary mechanisms support Hypothesis 1 (Figure 4.3). To conceptualize them, we outline a classical principal-agent model, in which voters are the principal and the mayor is the agent (Mechanism 1), and the mayor-chief of police is the principal, and the police commissioner is the agent (Mechanism 2).

Figure 4.3: The positive effect of direct election on crime incidence (hypothesis 1 can be explained through two mechanisms, both of which draw from the principal-agent framework



Consider a two-period model in a municipality with an incumbent mayor-chief of police (*she*) and a police commissioner (*he*). We start by considering a "single-municipality" police district (we will relax this assumption later and account for districts that encompass more than one municipality and several mayor-chiefs of police). The mayor and the commissioner gather at the beginning of the first period to outline a "crime plan" for the two periods. The commissioner enacts the plan, while the mayor oversees the correct implementation. Both the mayor's and the commissioner's mandate last one period and can seek reconfirmation for the second: at the end of the first period the mayor faces elections; the commissioner is re-appointed on the mayor's recommendation. During the second period, both the mayor's and commissioner's performance contributes to their legacy which ultimately determine their future career and income.

Between the first and the second period, electoral rules suddenly change and competition for mayorship harshens. In the context of this chapter, the 2005 reform forces Wallonian mayors to race against competitors from other lists as well as members of the same list, since now popular vote can elect anyone to the mayorship (Section 4.2).

Mechanism 1: Tighter electoral competition increases the accountability of mayors to voters

Because of tighter competition, competitors would prime voters and design their campaign on specific issues. The incumbent mayor running for re-election would specialize

on issues she can have a direct control, such as criminality and need for tougher law enforcement (Aragonès et al., 2015). The mayor would then steer local police more incisively to tackle crime incidence and thereby gain approval among voters. Opponents may also decide to race on safety-related issues, suggesting alternative solutions to those proposed by the incumbent mayor.

More competitive elections thus narrow information asymmetries and favor candidates with stronger policy messages. Voters have more incentive to scrutinize the competitors' platforms and are exposed to clearer signals about the competitors' skills. In particular, voters who are concerned about criminality, would vote into office "law-and-order" type of candidates.

Ultimately, tighter electoral competition keeps mayors more accountable and pushes them to fight criminality effectively. After the elections, winning candidates come under intense scrutiny, given their strategic campaign and the importance they gave to crime prevention. Mayors must uphold their promise and meet voters' expectations in order to build a legacy that would then determine their future career ambitions.

It is important to note that Mechanism 1 does not predict any increase in accountability in Flanders or Brussels. The 2005 reforms did not affect these two regions, where political destiny of candidate-mayors remains in the city councilors' hands. There is less incentives for candidates to prime voters like in Wallonia because electoral competition between candidates does not exist. Without personalization of the campaign, safety remains bundled with other topics in campaigning platforms (Besley and Coate, 2003). Mayors therefore remain much less accountable than in Wallonia, and the effects of their counter-criminal policies less tangible.

Mechanism 2: Tighter electoral competition motivates mayors to monitor police commissioner's activities more closely

The second mechanism behind Hypothesis 1 explores the relationship between an incumbent mayor seeking re-election and the police commissioner. In so doing, it examines a "black box" that the political-economic literature has only recently addressed: the interaction between politicians and bureaucrats.

Within the above theoretical framework, the payoffs of the mayor increase with the probability of being re-elected and retaining power during the second period, and with the legacy left at the end of the second period. Re-election and legacy depend on the perceived safety in a municipality. Although criminal events often embed a random component that makes them partially unpredictable, the mayor can still minimize crime incidence or signal their stand against criminality – for instance, by increasing the police forces patrolling the streets. To counter crime, she has to rely on a network of bureaucrats – the policemen – and their head – the police commissioner. The coordination effort of the police commissioner is fundamental to grease the functioning of the whole bureaucratic machine, leading to effective law enforcement.

The police commissioner maximizes his payoff function. On the one hand, he is concerned about preserving his current position during the second period and a potential rent to cash in at the end of the second period. Both rewards depend on the mayor's rec-

ommendation.[10] On the other hand, the coordination of police officers entails costs that the commissioner will try to minimize. For the purpose of simplicity, the commissioner solves this trade-off by adopting two different strategies: he commits to the crime plan agreed with the mayor at the beginning of the first period, or he shirks his responsibilities and prefers to minimize coordination costs. Eventually, the commissioner's strategy will affect crime incidence and thereby endanger the mayor's re-election (at the end of the first period) or legacy (at the end of the second period).

Ideally, the mayor would need to monitor the implementation of the crime plan to prevent the commissioner from shirking. Monitoring has, however, a cost-opportunity: the mayor could spend time and energies on other projects that could increase her popularity. Alternatively, the mayor can audit the commissioner's activity randomly at the end of the first period and the second period. The mayor rewards the commissioner if she finds that he has committed to the plan. If the mayor instead finds evidence that the audited commissioner has been shirking his responsibilities, she will not reward him.

The credibility of the reward depends, however, on the re-election of the incumbent mayor. In a way, the commissioner discounts future rewards by assessing the intensity of the electoral competition. Tight elections threaten the probability of re-election of the incumbent mayor, undermining any promise of promotion or re-appointment – or any punishment for misbehavior. The more competitive the local elections, the more binding the commissioner's incentives not to pursue the agreed crime plan. At the same time, intense electoral competition may motivate the mayor to exert tighter control over the commissioner's performance.

The commissioner's effort and commitment to the crime plan, the mayor's intensity of monitoring and ultimately the level of crime incidence in equilibrium depends on the competitiveness of elections. There is no consensus on the circumstances that allow the mayor's tighter control to prevail over the commissioner's incentive to shirk (and *vice-versa*). On the one hand, the performance of bureaucrats decreases in tightly contested districts. As re-election becomes less secure, bureaucrats judge rewards and promotion as less likely and therefore have no incentives to increase their effort (Nath, 2015). On the other hand, highly competitive politicians desire quick results in order to pander to voters. They may therefore put extra pressure on bureaucrats to improve their productivity (Bloom et al., 2015). Additionally, bureaucrats may speed up the implementation of local projects, perhaps at the cost of decreasing quality and misuse of resources (Rogger, 2018). This chapter argues that the 2005 reform contributed to decreasing crime incidence by increasing local competition and thereby motivating career-concerned mayors to control more closely the local police commissioners' record.[11]

[10]For example, the police commissioner could seek for a promotion to another administrative level at the end of the second period that would guarantee him a future rent.

[11]Measurement of the performance of bureaucrats in the political economy literature is controversial. Usually, performance is measured *vis-à-vis* the achievement of predetermined and transparent targets. As noted by Bloom et al. (2015), smarter managers may merely game the system, meeting formal targets without improving clinical outcomes. In our case, a lower crime incidence may be the consequence of commissioners that hide crime reports in order to cheat on their performance or to cover their failures. Moreover, it is challenging to measure the relationship between politicians and bureaucrats. Rogger (2018) collected detailed data that follow the delivery of a representative set of public projects in Nigeria, from their initiation in Congress, through the bureaucratic body that implements them, to the independent authority evaluating the final output.

4.3.2 The effect of the reform on crime incidence in police districts with more than one municipality and chief of police

The predictions of the conceptual framework may differ for large and politically heterogeneous police districts. To illustrate Mechanism 1 and 2, we considered a "one-city" police district, where only one mayor oversees the police commissioner. Section 4.2 shows that on average four mayors-chiefs of police share the mandate over local police. When multiple principals are involved, the outcomes of both mechanisms may change (Dixit, 2002). In the context of this chapter, we argue the following:

Hypothesis 2 *The beneficial effect of the direct election of mayors on crime incidence is diluted when several mayors need to coordinate local policing, mainly because of the increasing costs of coordination and political heterogeneity.*

Sharing mandates may be an obstacle to mayoral accountability. The larger the police district, the easier it is for a mayor to blame her peers for unexpected spikes in crime incidence. She can also free-ride on neighbors' policing efforts and campaign on others' performance. Widespread free-riding would ultimately lead to high crime rates (Gailmard, 2009). In Wallonia, voters would not be able to keep mayors accountable, since they would receive only imprecise (or "blurred") information about their performance in fighting criminality. Despite the 2005 reform, information asymmetries in large Wallonian police district remain significant. Because of incentives to free-ride and decreasing accountability, Mechanism 1 suggests that post-reform crime incidence in Wallonia decreases less in municipalities within large police districts than in municipalities within "mono-city" districts.

Sharing mandates undermines oversight of the commissioner's activity as well. Systematic coordination among all mayors involved in the management of a police district can make monitoring of the police commissioner more effective. However, coordination entails costs that in turn depend on the different political allegiance of mayors, due to tighter personal connections, shared views regarding crime prevention strategies and priorities, or party discipline (Durante and Gutierrez, 2015). We argue that coordination costs increase with political fractionalization and polarization of mayorships. Increasing coordination costs make monitoring less effective and leave the police commissioner without incentives to commit to the crime plan. Moreover, interviews conducted in the context of this chapter revealed that mayors tend to delegate police management to the commissioner – a non-elected bureaucrat – when coordination costs are high. Either way and despite the reform, Mechanism 2 predicts that there are no differences in post-reform crime incidence between Wallonian municipalities in politically heterogeneous districts, and Flemish or Brussels municipalities.

4.4 Data sources and descriptive statistics

We created a unique dataset that, based on a variety of sources, combines the following information at the municipal level in Belgium: crime incidence, social and economic characteristics, and public expenditure and size of the police force. We moreover compiled the first dataset to gather the identity of mayors in Belgium together with their political affiliation and score in the 2006 and 2012 elections (Colombo and Tojerow, 2019).

Crime incidence. We define crime incidence as the number of crime events per 1,000 inhabitants in a municipality. Our sample consists of 7,644 crime events reported in 589 Belgian municipalities between 2000 and 2012. The Belgian Federal Police ("Police fédérale – Direction de l'information policiére et des moyens ICT (DRI) – service Politique et gestion (BIPOL)") provided detailed information about the types of crime. Each entry in the database represents a criminal act that was either attempted or realized and was registered by officers of the local police through a report. All reports are then transmitted to a federal database (*Banque de données nationale générale*, BNG) within three weeks from the acknowledgment of the criminal act. The federal police classifies each criminal act according to pre-defined categories. When multiple offenses occur in the perpetration of a single crime event, the agent records the most serious of them.[12]

The main variable of interest in the following analysis is the aggregated value of the five main categories of crime: drug, fraud, robbery, vandalism and violence. These five categories represent more than 70% of the total crime reported in each region and the country (Table 4.1) and present the least number of zeros (Van Den Steen et al., 2009). Half of the drug-related crime episodes concern usage; the most recurrent fraudulent crimes are embezzlement, scams, and misappropriation; robbery mostly comprises pick-pocketing or is committed without aggravating circumstances; vandalism mostly consists of deliberate property destruction and arson. The average composition of crime is very similar across municipalities from the three Belgian regions. Robbery incidence is the highest throughout the reference period (2000–12), followed by vandalism and violence. Average crime incidence is the highest in municipalities in the Brussels region. However, Figure B4.1 in the Annex shows great within-region variation. The municipalities with the highest average crime incidence over the period 2000-2012 are Liège (183 episodes per 1,000 inhabitants), Charleroi (126) and Visé (122) in Wallonia; Fourons (183), Blankenberge (125) and Antwerpen (124) in Flanders; and Bruxelles (286), Saint-Gilles (221) and Saint-Josse-ten-Noode (123) in the Brussels region.[13]

Social and economic variables. Certain specifications in the following analysis control for several social and economic characteristics available at the municipality level. Their descriptive statistics are presented in Table 4.2. Data about density, mean and median income per declaration, and proportion of foreigner residents are available from 2000 throughout 2012 and come from StatBel, "Federal Public Service Economy, SMEs, Self-Employed and Energy" department. The quadratic term of the logarithm of density will be included as well, in order to consider the possibility that crime incidence evolves exponentially with population concentration. Wallonia is the second region with the highest density, employment and unemployment rate. It is the least dense and has the second highest average and median income.

[12]Crime data usually suffer from two main limitations. First, they report only criminal acts that are not known by the police. Second, the number of reported crime events might depend on the effort of local officers (Police Fédérale, 2015). Most importantly for this research, we do not believe that these limitations affect crime incidence heterogeneously across Belgian regions and municipalities. The federal police database provides no information about the identity of criminals, their socio-economic and demographic characteristics, or the geolocation of a criminal event.

[13]Visé and Fourons are two municipalities on the border with the Netherlands. According to some mayors and police officers interviewed in the context of this chapter, they are gateways for drug and goods smugglers.

Table 4.1: Average crime incidence between 2000 and 2012 by region and type of crime, municipal level

	Vandalism	Robbery	Violence	Fraud	Drug	Main	All
Wallonia							
Mean	8.0	23.0	4.5	4.2	4.0	43.8	60.2
St.Dev	3.5	13.4	2.3	4.6	10.3	22.8	28.7
Min	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Max	34.0	131.0	18.6	56.1	339.7	381.3	423.4
Observations	4004	4004	4004	4004	4004	4004	4004
Brussels							
Mean	11.7	76.9	8.5	6.7	3.9	107.7	140.1
St.Dev	3.3	45.3	3.4	4.9	3.0	55.7	68.9
Min	5.5	30.8	3.0	1.7	0.3	45.9	66.0
Max	23.4	234.0	21.4	28.6	18.6	306.0	364.8
Observations	247	247	247	247	247	247	247
Flanders							
Mean	9.8	27.3	6.8	4.3	3.9	52.1	73.6
St.Dev	4.0	13.6	2.9	4.4	7.8	22.3	29.1
Min	0.0	4.6	0.7	0.0	0.0	11.3	16.3
Max	25.4	161.7	19.2	52.0	214.3	265.7	304.6
Observations	3406	3406	3406	3406	3406	3406	3406
Total							
Mean	8.9	26.7	5.7	4.3	4.0	49.6	68.7
St.Dev	3.9	18.2	2.9	4.6	9.1	26.9	34.2
Min	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Max	34.0	234.0	21.4	56.1	339.7	381.3	423.4
Observations	7657	7657	7657	7657	7657	7657	7657

Source: Authors' calculation based on data from "Police fédérale – Direction de l'information policiére et des moyens ICT (DRI) – service Politique et gestion (BIPOL)". "Main" stands for the aggregate value of the five main types of crime by municipality.

Public expenditure and size of the police force. We retrieved information on public expenditure at the municipal level from 2000 to 2012 from the Belfius database. In particular, we extracted information concerning expenditure in local administration, garbage collection, social assistance, and safety and crime prevention. Over the sampled period, Flemish and Wallonian municipalities spent the highest share of total expenditure (17% and 19%, respectively) on public administration. Concerning local police management, the share is highest in Brussels (17% - €243 per capita) and amounts to around 9% in Flanders and Wallonia (€80 per capita and €72 per capita, respectively).

In Section 5.7, we will study how the treatment effect varies with the size of the police force. We were able to gather information about the number of officials in force in each of the police districts from 2002 to 2012 only. The number of police officers in each police district depends on a federal rule that takes into account 75 indicators from each police district. These indicators account for the size of the municipality (area and population), degree of urbanization, income, employment, unemployment, school-age

population, enrollment rate, age and household structure, nationality, migration inflows, housing characteristics, cadastral income, crime, road accidents, frequency of football matches and the prison population. On average, the distribution of police force does not vary across regions: there are 11 officers per 1,000 inhabitants in Flanders, 14 in Wallonia and 16 in Brussels. In each police district, then, officers are redistributed among member municipalities according to local agreements between mayors.

Table 4.2: Average social and economic characteristics and public expenditure between 2000 and 2012, municipal level

	Wallonia	Flanders	Brussels-Region	Total
Social and economic variables				
Density	307.30	523.29	9,278.13	709.63
	(429.12)	(449.27)	(5,332.34)	(1,886.77)
Mean income	25,355.74	27,747.08	24,417.86	26,575.96
	(4,183.49)	(4,000.51)	(4,584.86)	(4,284.52)
Median income	19,449.06	21,269.50	17,755.26	20,346.37
	(2,618.06)	(2,243.)	(2,469.74)	(2,625.91)
Foreigners (%)	6.34	4.16	26.30	5.85
	(5.73)	(5.41)	(8.77)	(6.89)
Employment	59.64	66.24	55.88	62.97
	(5.42)	(3.85)	(5.23)	(5.82)
Unemployment	8.48	3.83	14.91	6.26
	(3.54)	(1.4)	(5.52)	(3.91)
Public expenditure (% of overall local expenditure)				
Social assistance	11.13	10.29	14.84	10.84
	(3.69)	(3.51)	(3.00)	(3.68)
Garbage collection	6.13	7.93	1.20	6.85
	(1.74)	(2.01)	(1.34)	(2.34)
Public admin	19.45	16.91	12.78	17.93
	(4.38)	(4.36)	(6.17)	(4.73)
Police	7.97	8.83	17.28	8.75
	(2.67)	(2.12)	(3.25)	(2.99)
Political landscape				
Number of parties	2.46	2.12	2.06	2.27
	(0.96)	(0.94)	(0.68)	(0.96)
Fractionalization index	0.45	0.38	0.41	0.41
	(0.24)	(0.25)	(0.22)	(0.24)
Polarization index	0.69	0.64	0.71	0.67
	(0.34)	(0.39)	(0.36)	(0.37)
Number of municipalities	262	308	19	589

Source: Authors' calculation based on data from StatBel, Belfius and Colombo and Tojerow (2019). Standard deviations are in brackets.

Political landscape. There is no harmonized and complete dataset on local election outcomes and mayors in Belgium. This chapter contributes to closing this gap. We build a dataset of mayors in Belgium from 2000 to 2012 by merging two sources of information. The identities of most of the mayors come from a register of positions and assets self-declared by each Belgian public officer - at both national and local level - to the Court of Auditors. The register is then made public through the "Belgian official

journal" and information was re-organized and made more accessible to the public by the website *Cumuleo*. We complemented the register with information from other sources (e.g. local and national newspapers, and official websites of local administrations) when the register was not complete.[14] We then matched the list of mayors with their electoral performance during two rounds of local elections, in 2006 and 2012. We scrapped this information from websites dedicated to local elections.

The dataset thus created contains information of all 939 mayors who were in office between 2000 and 2012. Overall, 45% of mayors remained in charge of both mandates between 2000 and 2012. The share is the lowest in Wallonia, suggesting a partial renovation of mayorships in the region after the 2006 elections. Since there is no term limit, incumbent mayors often seek re-election. In 2006, virtually all mayors were actively campaigning (94%), while the share decreased to 72% in 2012 (Table 4.3).

Table 4.3: Share of mayors that ran for re-election in 2006 and 2012, and that served for 1 or 2 legislatures.

	Wallonia	Flanders	Brussels	Belgium
Share of mayors by running decision				
Re-running in 2006	93.05%	94.03%	100.00 %	93.76%
Re-running in 2012	70.18%	73.02%	91.30%	72.18%
Share of mayors by number of terms served				
2000-2006 term only	29.79%	21.17%	13.74%	24.92%
Both terms	37.88%	49.48%	62.07%	44.52%

Source: Data collected by the authors and presented in Colombo and Tojerow (2019).

Mayors differ across regions with respect to their party affiliation. During the period under investigation, three parties shared the political arena in Wallonia almost equally: the Social-Democratic Party (PS), the Liberal Party (MR) and the Christian-Democrats (cdH). In Flanders, half of the mayors were from the Christian-Democratic party (CD&V), followed by the liberals (Open VLD) and the Social-Democratic Party (sp.a). In Brussels, 40% of the mayors in the sampled period were from the Social-Democratic Party. The other two largest parties in the capital-region were of liberal origin (the previously mentioned MR and the Brussels-rooted Défi).

In order to quantify the political heterogeneity within a police district, we propose two indicators. We construct an index of political fractionalization following Alesina et al. (1999):

$$ELF = 1 - \sum_{k=1}^{K} \pi_k^2 \tag{4.1}$$

where π is the proportion of mayors within the same police district belonging to party k. The index of political fractionalization measures the probability that two randomly selected mayors from a police district belong to different political parties. The indicator increases with the political fractionalization of the police district.

[14] *Le Dico des communes* published by the Belgian newspaper *La Libre* on 3 October 2006 was particularly useful in closing some knowledge gaps concerning the 2006 elections.

We construct the index of political polarization following Montalvo and Reynal-Querol (2005):

$$pola = 1 - \sum_{k=1}^{K} \pi_k \left(\frac{0.5 - \pi_k}{0.5} \right)^2 \tag{4.2}$$

where, again, π has the same meaning as before. The index captures how far the political landscape within a police district is from being bipolar, with $pola = 1$ indicating a bipolar political scenario.

4.5 Empirical strategy

4.5.1 Identification strategy

The objective of this chapter is to identify the average effect of the 2005 reform of mayoral accountability on crime incidence in those municipalities in which mayors became directly elected – the average impact of treatment on the treated. Ideally, we would compare crime incidence in municipalities where the reform occurred to the counterfactual, that is those very same administrative units if the reform had never taken place. Since the counterfactual is impossible to observe, we need to identify municipalities similar to the treatment group in their observed and unobserved characteristics, but unaffected by the reform. If this were a randomized control trial, we would assign the modality of selection of mayors randomly across treatment and control groups, and then compare their average criminal outcomes

However, the 2005 reform did not involve municipalities randomly; the scope of its implementation followed administrative borders. As discussed in Section 4.2, Wallonia was the only region to pass the reform; mayors in Flanders and the Brussels region were not affected and remained appointed by the respective city councils. We define Wallonian municipalities as the "treatment group" and the others the "control group".

The identification assumption of our analysis is that Flemish and Brussels municipalities mimic what would have happened to Wallonia if the 2005 reform had not taken place. Comparison of the differences in crime incidence between groups before and after the reform provides a causal estimation of the effect of increased mayoral accountability on local crime incidence in Wallonia.

Formally, we test a difference-in-differences model, hereafter specified as a two-way fixed-effect linear regression model:

$$y_{it} = \alpha + \beta_0 WAL_i + \beta_1 d_t + \beta_2 D_{it} + \mathbf{\Gamma}'\mathbf{X}_{it} + \delta_i + \zeta_t + \epsilon_{it} \tag{4.3}$$

where y_{it} is the logarithm of the aggregated crime incidence in municipality i and year t; and crime incidence equals the number of crime episodes per 1,000 inhabitants. $WAL_i = 1$ if a municipality is in Wallonia; and $d_t = 1$ if a crime episode was observed at time t since 2005 (it takes the value of 0 before 2005). As a result, $D_{it} = WAL_i \times d_t = 1$ if there was a crime observed at time t since 2005, in municipality i.

Concerning the coefficients: β_0 informs on the extent to which there are more crimes in the treatment group (Wallonia) than in the control group (Flanders and Brussels); β_1 on the incidence of criminality since 2005 with respect to the previous years; the coefficient associated to D_{it}, β_2, captures the effect of the reform. In particular, it

functions as the difference-in-difference estimator of the average impact of the 2005 reform on crime incidence. Formally:

$$\begin{aligned}\hat{\beta}_2 = (\mathbb{E}\left[y_{it}|d_t = 1, WAL_s = 1\right]) - \mathbb{E}\left[y_{it}|d_t = 1, WAL_s = 0\right]) \\ - (\mathbb{E}\left[y_{it}|d_t = 0, WAL_s = 1\right] - \mathbb{E}\left[y_{it}|d_t = 0, WAL_s = 0\right])\end{aligned} \tag{4.4}$$

The first and the second differences compare the change in crime incidence between treatment and control groups, after and before the reform, respectively. A negative value of the coefficient of interest validates Hypothesis 1; that is, post-reform crime incidence is lower under elected mayors (in Wallonia, the treatment group) than elsewhere (Flanders and the Brussels region, the control group).

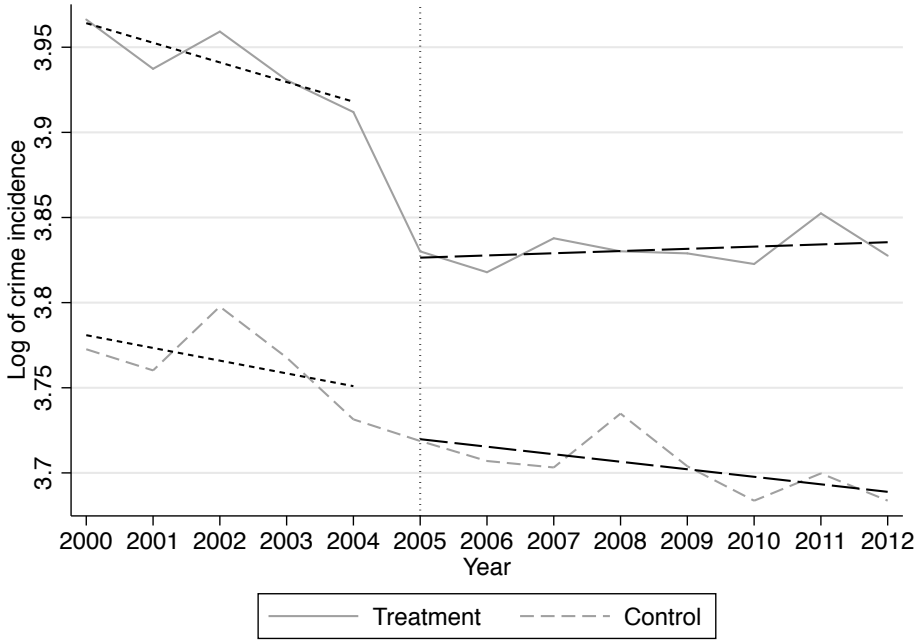
We augment Equation (4.3) with a series of covariates to strengthen the model and minimize endogeneity issues. $\mathbf{X_{it}}$ is a set of time-varying (observable) municipal characteristics. We control for socio-economic characteristics (the logarithm values of density and its squared value, mean income and median income; the proportions of inhabitants unemployed, employed and foreigners) and data on selected public expenditures (the logarithm values of expenditure in public administration, garbage collection, social assistance, and safety and crime prevention). Finally, we include municipal fixed-effects (δ_i) and year fixed-effects (ζ_t). In some benchmark specifications, we consider a trend variable that increments every year across all municipalities

We assume the error ϵ_{it} is a municipality time-varying error independently distributed for every municipality and year. However, one common problem with panel data is that ϵ_{it} might be correlated across time and space. Some municipality characteristics correlated with crime incidence (e.g. being a tourist locality) might be endemic and could thereby induce time-series correlation at the municipal level. Furthermore, the same characteristics could also affect neighboring municipalities. To minimize these problems, we cluster the standard errors at the municipality level to allow for an arbitrary covariance structure within municipalities over time (Galiani et al., 2005).

The identification assumption holds if the pre-reform trend of crime incidence is comparable between treatment and control groups. A simple graphical inspection suggests that trends were indeed parallel before the approval in 2005 of the Wallonian reform. The trend for the control group remains substantially unaltered, whereas the evolution of crime incidence in Wallonia diverges after the reform (Figure 4.4). This would seem to validate the identification assumption.

Moreover, the identification assumption holds if the reform affects only the treatment group. Crime incidence after the reform should deviate from the pre-reform trend in Wallonia only. A potential source of concern in Figure 4.4 is the discrete jump in crime incidence in the year of the reform involving not only the treatment group but the control group as well. There might be unobserved explanations for a change in crime incidence in both the treatment and control group that could threaten the identification assumption. If this jump were significant, we would not be able to disentangle the effect of the reform on crime incidence from other endogenous explanations correlated with crime in the control group. A standard t-test shows that the mean difference in crime incidence one and two years before and after the reform is not significant. The jump is instead significant in the treatment group. Hence, we believe that the identification assumption is valid.

Figure 4.4: Trend of average crime incidence before and after the reform and between treatment and control groups



Note: The graph shows the trend of average crime incidence (in log values) throughout the period of interest, in the treatment group (Wallonia) and in the control group (Flanders and Brussels region).

Following Abramitzky and Lavy (2014), we formally estimate differential time trends in the dependent variable for treated and control municipalities. First, we use pre-reform data from 2000 to 2004 to interact the dummy WAL_i with a constant linear trend. If the pre-treatment trend were not parallel between the two groups, the difference captured by the interaction would be significantly different from zero. Panel A of Table 4.4 reports the results. Whether control variables are included (column 1) or not (columns 2 and 3), the mean trend is not significantly different from zero. The estimated coefficient on the interaction of the constant trend with the treatment indicator is also not statistically significant, suggesting that there is no difference in the pre-treatment trend of crime incidences across treatment and control groups.

Finally, we interact WAL_i with a series of year dummies in order to detect potential differences in trends for each of the pre-treatment years (Autor, 2003). Panel B of Table 4.4 displays the results of the estimation of these two models. Before 2005, the interaction terms of the treatment indicator with the year dummies are all small, no matter the specification chosen. Once again, these tests prove the validity of the identification strategy and the robustness of the results presented in the next section.

The rest of Panel B of Table 4.4 provides details about the post-reform period. These details are discussed in the following section.

Table 4.4: Testing pre-treatment parallel assumption - interaction $WAL_i \times$ *year dummy*

	(1)	(2)	(3)
Panel A			
Trend	-0.006	0.002	-0.000
	(0.004)	(0.005)	(0.006)
WAL × Trend	-0.006	-0.006	-0.006
	(0.006)	(0.007)	(0.008)
Panel B			
WAL × year=2001	-0.016	-0.014	-0.022
	(0.015)	(0.015)	(0.016)
WAL × year=2002	-0.032*	-0.024	-0.032
	(0.018)	(0.019)	(0.020)
WAL × year=2003	-0.032	-0.025	-0.032
	(0.021)	(0.021)	(0.022)
WAL × year=2004	-0.023	-0.014	-0.018
	(0.023)	(0.024)	(0.026)
WAL × year=2005	-0.083***	-0.073***	-0.074***
	(0.021)	(0.022)	(0.025)
WAL × year=2006	-0.086***	-0.079***	-0.074***
	(0.022)	(0.023)	(0.026)
WAL × year=2007	-0.070***	-0.059**	-0.052**
	(0.023)	(0.025)	(0.026)
WAL × year=2008	-0.099***	-0.087***	-0.082***
	(0.023)	(0.025)	(0.027)
WAL × year=2009	-0.069***	-0.060**	-0.055*
	(0.024)	(0.027)	(0.029)
WAL × year=2010	-0.056**	-0.047*	-0.036
	(0.023)	(0.027)	(0.029)
WAL × year=2011	-0.042*	-0.026	-0.020
	(0.023)	(0.027)	(0.029)
WAL × year=2012	-0.051**	-0.036	-0.036
	(0.024)	(0.028)	(0.030)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	No	Socio-economic	All
Observations	7654	7654	6565

Notes: OLS regressions. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants), in log values. All specifications include municipality fixed effects and year fixed effects. Covariates at the municipal level are added in columns (2) and (3). In column (3) we include only municipalities for which we have budgetary information for the whole period of interest (2000-2012), as explained in Section 4.4. Standard errors are clustered at the municipality level and are presented in parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

4.6 Results and robustness tests

The introduction of the direct election of mayors contributed to a decrease in crime incidence in Wallonian municipalities. This section presents the results from the bench-

mark model – detailed in Equation (4.3). It then tests their robustness against a series of further specifications. The next section reconciles the results with the conceptual framework and examines the heterogeneous effects of the reform.

4.6.1 Benchmark results

Table 4.5 presents the average post-reform effect as computed through the benchmark model – Equation (4.3). Column 1 presents the results for the specification that includes no covariates except for municipality and year fixed effects. Columns 2 and 3 show the results obtained by including socio-economic characteristics of the municipalities and year fixed effects or a time trend. Columns 4 and 5 integrate the previous two specifications with information on categories of public expenditure at our disposal (Section 4.4).

Table 4.5: Benchmark results

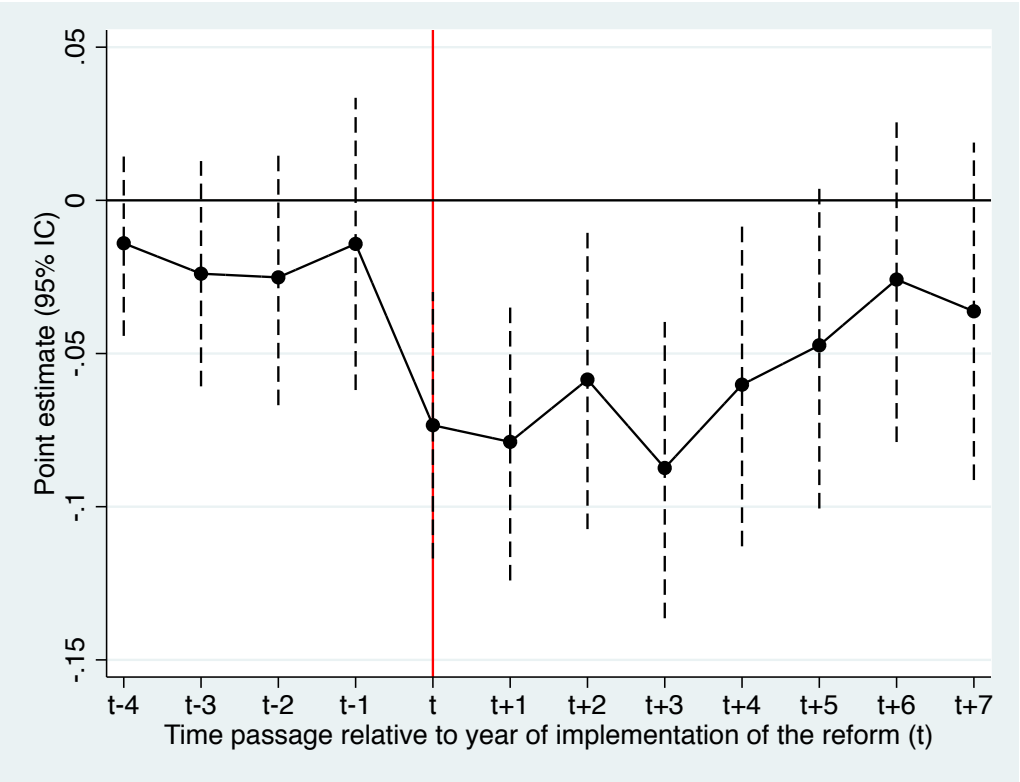
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.049***	-0.046***	-0.057***	-0.037**	-0.053***
	(0.013)	(0.014)	(0.011)	(0.015)	(0.012)
Social economic controls	No	Yes	Yes	Yes	Yes
Public expenditure controls	No	No	No	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	No
Trend	No	No	Yes	No	Yes
Observations	7654	7654	7654	6565	6565

Notes: OLS regressions. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants), in log values. All specifications include municipality fixed effects and year fixed effects. Covariates at the municipal level are added in columns (2) to (5). In columns (4) and (5) we include only municipalities for which we have budgetary information for the whole period of interest (2000-2012), as explained in Section 4.4. For an extended version of this table - including the coefficients for all controls - see Table A4.1. Standard errors are clustered at the municipality level and are presented in parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Following the reform, crime incidence in Wallonia decreased by on average 5% between 2005 and 2012. The results are coherent and statistically significant across all specifications. We refer to column 2 as our favorite specification because it is the most complete and the choice of covariates does not imply restricting the dataset. A back-of-the-envelope calculation suggests that, after the reform, municipalities where mayors became directly elected registered 2 episodes per 1,000 inhabitants less than elsewhere in the country. Safety – or the quality of the public good – increases because of the 2005 reform. These results validate hypothesis 1.

Table A4.1 in the Annex presents the estimated coefficients for each of the covariates in the baseline equation. The coefficients of labor market variables and the share of foreigners are not significantly different from zero. Crime incidence increases significantly with the median income in line with existing crime literature. The variable of interest increases exponentially with municipal density and in municipalities with higher per capita expenditure in garbage collections – which are usually wealthier. Expenditure on police and crime prevention does not have any effect on crime incidence.

Figure 4.5: Treatment effect by year (2000-2012)



Notes: this graph represents the difference in crime incidence between treatment and control group by year. The associated coefficients are reported in Table 4.4.

The magnitude and significance of the treatment effect vary throughout the post-reform period. Panel B of Table 4.4 and Figure 4.5 show the effects of the reform on crime incidence for each post-reform year. Three main results emerge. First, the swift decline in crime incidence in Wallonian municipalities occurred without delay during the year in which the reform was implemented. However, mayors might have anticipated the implementation of the reform, as discussed later. Second, crime incidence in Wallonia decreases for five to six years after the reform (depending on the specification). Third, after 2010 the effects of the reform fade away.

Section 4.7 reconciles these results with the conceptual framework. However first, we stress the robustness of the results.

4.6.2 Robustness tests

To test the benchmark results, we run three main sets of alternative specifications. First, we control for sample balance, that is whether the coefficient of interest is sensitive to the differences in observable characteristics between the treatment and the control group. Then, we discuss the clustering of standard errors and geographical trends. To conclude, we check whether local characteristics sharply change their behavior around the implementation of the reform; if that were the case, mechanisms other than the reform itself would explain the changes in crime incidence.

a. Balance test and sample size

The identification assumption of this analysis may be endangered by the actual capacity of the control group (Flanders and the Brussels region) to mimic the counterfactual. Unbalanced groups might be a consequence of socio-economic and geographic heterogeneities across Belgium, and in particular between Wallonian and Flemish municipalities. If such differences significantly correlated with crime incidence, and at the same time made the reform of mayoral accountability more likely in Wallonia than anywhere else, estimations in Table 4.5 might suffer from an endogeneity issue.

In order to minimize potential bias, we restrict the original sample of 589 municipalities to a subset of municipalities in the treatment and control groups that are as comparable as possible with respect to observable and unobservable characteristics. First, we exclude the region of Brussels from the analysis: Brussels being the federal capital of the country and headquarter of the core European institutions, it could be an outstanding magnet of criminal events as compared to other cities in the country. Second, we assume that administrative units just across the border tend to be similar in terms of observed and unobserved characteristics (Dube et al., 2010). Hence, we consider a subsample of 152 municipalities along the administrative border between the treatment and the control group. Moreover, we include the neighbors of municipalities at the border in order to partly control for usual spillovers of criminal activity.

Table A4.2 shows a t-test for the mean differences in each of the control variables between the treatment and the control group, and for the pre-treatment period (2000-2004). When considering the full sample of municipalities (column 1), all control variables are on average statistically different between the treatment and the control group. Such imbalances attenuate once we restrict the sample to those municipalities at the border and their neighbors (column 2). Notable exceptions are labor market covariates, the proportion of foreign inhabitants and expenditure in garbage collection. Employment is 5 percentage points (p.p.) significantly lower in the sub-sample of Wallonian municipalities than elsewhere; however, unemployment is 3.5 p.p. higher. The proportion of foreign inhabitants is 3 p.p. higher, possibly fueled by the prominence of French speakers commuting to the neighboring Brussels region. Expenditure on garbage collection is 0.43 logarithm points lower in Wallonian municipalities, possibly proxying for lower wealth.

Results for the restricted subsamples are reported in Table 4.6. Columns 1 and 2 report regression outcomes when we exclude Brussels from the analysis. The effect of the reform on crime incidence remains negative and significant, and only mildly different than the main outcome of our main specification - column 2, Table 4.5. In columns 3 and 4, we consider only 152 municipalities. Despite the smaller sample size, the coefficient of interest remains significant, with fixed effects (column 3) or with the trend variable (column 4). This result reinforces the robustness of the benchmarking treatment effect.

Table 4.6: Robustness test - Benchmarking results with restricted sample

	No Brussels		Border+	
	(1)	(2)	(3)	(4)
Treatment	-0.047***	-0.058***	-0.055*	-0.077***
	(0.014)	(0.011)	(0.031)	(0.024)
Density (hab./sq.km, in log)	0.337	0.536	1.827	1.949
	(0.786)	(0.781)	(2.097)	(2.132)
Density^2 (in log)	-0.114	-0.130*	-0.272	-0.272
	(0.070)	(0.070)	(0.195)	(0.199)
Employment (%)	-0.002	-0.005	-0.009	-0.015*
	(0.005)	(0.003)	(0.011)	(0.008)
Unemployment (%)	0.002	-0.004	-0.030	-0.032***
	(0.008)	(0.005)	(0.019)	(0.011)
Foreigners (%)	0.005	0.004	-0.006	-0.006
	(0.005)	(0.005)	(0.011)	(0.011)
Median income (in log)	0.370**	0.418***	0.210	0.476*
	(0.162)	(0.151)	(0.276)	(0.267)
Mean income (in log)	-0.181	-0.185	-0.331	-0.419
	(0.182)	(0.171)	(0.304)	(0.289)
Constant	3.900	3.078	4.333	2.240
	(2.604)	(2.465)	(5.912)	(5.781)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Trend	No	Yes	No	Yes
Observations	7407	7407	1973	1973

Notes: OLS regressions with treatment status as the independent variable and each covariate as the variable of interest. In column (2) we restricted the sample to those municipalities at the border between Wallonia and Flanders and their neighbors, only. Standard errors are presented in parentheses. Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

b. Clustered errors

Differences-in-differences estimations of long panel data series may suffer from a possibly severe serial correlation problem. Two factors make serial correlation an especially important issue in our setting. First, there might be serial correlation across years - that is, unobservable trends that might affect the impact of the treatment on the variable of interest. Second, there might be spill-over effects of criminality across administrative borders within police districts (Bertrand et al., 2004).

To solve for potential serial correlation, it is common practice to cluster errors at the level of the treatment. In our case, the treatment is formally assigned to regions rather than individual municipalities. In the benchmark regressions in Table 4.5 we nonetheless cluster at the municipal level. Although the treatment is regional, the elections that, through a change of behavior of local politicians, affect crime incidence are local. Moreover, the decision concerning crime prevention is taken at the municipal level, if not at the police district level. Finally, if we were clustering at the regional level, standard errors would be unnecessarily conservative, given that there are only three regions in Belgium (Abadie et al., 2017).

We nonetheless check whether benchmark estimations vary when clustering standard errors at different institutional levels. In practice, we estimate Equation (4.3) with standard errors clustered at a progressively higher level (Cameron and Miller, 2015). We assume, in particular, the serial correlation of errors within police district arrondissements, and provinces. The results are reported in Table A4.3. As expected, the coefficient of interest loses significance as standard errors become conservative. However, the impact of the reform on crime incidence is still significant at a confidence level of at least 10% in four out of six specifications.

Finally, following Conley (1999), we account for the possible serial correlation of errors across space. In particular, we control for correlation within 5 km, 15 km, and 25 km from the geographical centroid of each municipality. We also test for serial correlation of errors across time, with three-years lags. Results in Table A4.4 show that the coefficient of measuring the effect of the reform remains statistically significant.

c. Trends at different geographic levels

Heterogenous patterns in observable and unobservable characteristics could be persistent across time and endogenously shape the evolution of crime at different administrative levels. For instance, Wallonia may be less prone than Flanders and the Brussels Region to absorb economic shocks, unemployment and income volatility. If that were the case, we could not disentangle the effect of the reform on crime from the effects of regional structural features.[15] Hence, we need to rule out any specific trends in crime incidence at the different institutional levels that could spoil the benchmark results. We introduce local-specific linear trends and quadratic trends at the municipal level, police district level and regional level (Autor, 2003). In particular, three alternative specifications of Equation (4.3) include: municipal trends in Table A4.5, police district-level trends in Table A4.6 and regional trends in Table A4.7. The first two columns of each table include only socio-economic covariates, while the last two columns include expenditure covariates as well. Odd columns present results from specification with linear trends while even columns present results from specification with quadratic trends. In all 12 specifications, the coefficient capturing the impact of the 2005 reform on local crime incidence is always significant.

d. Behavior of municipal characteristics

Side-effects of the results on observable municipal characteristics may bias the benchmark estimations. Another possibility is that along the period of analysis (2000-2012), other exogenous shocks could affect municipal observable characteristics and therefore impact on crime incidence. In this case, it would be harder to identify the direct effect of the reform from possible indirect effects following changes in municipal social and economic characteristics. Figure B4.3 plots the evolution of the main covariates considered in Equation (4.3) by treatment and control group.

[15] Allegretto et al. (2011), for instance, account for spatial heterogeneity when assessing the impact of minimum wages on teen employment. Their main argument concerns the possible correlation between heterogeneous patterns in low-wage employment across US States and the decision by some States to implement minimum wage increases.

Figure B4.3 highlights two potential issues: the plunging median income and mean income in the treatment group between 2003 and 2004, and the evolution of the proportion of foreigners in the control group. In the first case, the anomalous behavior of income variables is due to a reform that introduced the digital submission of personal income tax reports in Wallonia in the first half of the 2000s. Due to this reform, low or no income earners were automatically included in the system, thereby biasing income statistics in this region. There is no evidence suggesting that this reform could have an impact on crime incidence. It does not affect the actual taxable income of citizens, but rather the value registered by tax authorities.

In the second case, we acknowledge the potential bias that could arise from the anomalous behavior of the proportion of foreign residents, especially in the control group. However, in the benchmark specification (column 2 in Table 4.5), and all specifications seen so far, the coefficient of this variable is always close to zero: crime incidence does not react to the share of foreigners. Moreover, when disentangling the evolution of the share of foreign residents by region, most of the variation in the control group is captured by the region of Brussels (Figure B4.2). The robust results obtained by excluding Brussels from the analysis (columns 1 and 2 of Table 4.6) address concerns from this potential bias.

In sum, this section shows that benchmark results are robust when tested through alternative specifications. Crime incidence in the treatment group decreases more than elsewhere after the implementation of the 2005 reform. Differences in the labor market fundamentals between treatment and control group are a source of concern. We minimize the potential impact of unemployment on crime incidence by controlling for it in the benchmark model. Section 4.2 discussed the lack of tools for mayors to tackle local structural unemployment. There is no reason to believe that the reform could have affected mayors' effort to address labor market outcomes and thereby crime incidence. At the same time, voters do not see mayors as responsible for local unemployment and would not vote them out of office on this basis. Further research should investigate the relationship between endemic regional differences in labor market's characteristics and their effect on local crime incidence. Yet, in the context of this chapter, we argue that this relationship does not invalidate the identification strategy.

4.7 Discussion

4.7.1 Reconciling the results with the first hypothesis in the conceptual framework

The main benchmark result shows that crime incidence decreases faster in municipalities led by directly elected mayors than in municipalities with appointed mayors. As discussed in Section 4.3 two mechanisms are at play.

The 2005 reform projects Wallonian mayors who seek re-election into a harsher competition. Now, burgomasters need to defend their seat from opponents in other lists as well as candidates within the same list. Hence, their campaigns specialize in strategic issues that are visible and over which they have direct control. Section 4.2 discusses why safety is one of those issues. At the same time, opponents compete with alternative solutions to counter criminality, sending voters clearer signals about their priorities. As

a result, only candidates who strongly commit against criminality will gain the mayorship. Once in power, they have to uphold their campaign promises, given the increased scrutiny by voters. This prediction is particularly valid for mayors that secure their seat for another legislature, from 2012 onwards.

The causal effect identified in the benchmarking results is in line with the predictions of this first mechanism, especially when it is broken down into years. First of all, mayors anticipate the effects of the reform. The regional parliament started discussing the introduction of the direct election of mayors in 2002. The debate stagnated for years before accelerating in 2005. Months before the reform passed, mayors could already predict its implementation. Since virtually all mayors run for re-election in 2006 (as discussed in Section 4.4), they all act accordingly and tackle criminality more incisively to pander voters. Once re-elected, the consequences of their commitment and campaign platform persist across time. However, as the legislature comes to an end, the effects of the reform on crime incidence fades away. Almost 30% of mayors in Wallonia do not run for re-election in 2012. Under these circumstances, "lame-duck" burgomasters would concentrate on issues other than visible and electorally appealing topics, like policing. After the initial beneficial effect of the reform, the level of criminality converges to the local equilibrium determined by municipal structural characteristics.

The second mechanism in Section 4.3 predicts that mayors under direct scrutiny tighten their control over the police commissioner's activities. The intense monitoring leaves commissioners with no other choices than to enact the crime plan and exert more effort in fighting criminality. We try to measure "effort" indirectly, since there are no direct indicators available. The argument is the following: mayors do not need to increase spending or the size of the police force when it comes to pandering to voters about safety. Interviews conducted in the context of this chapter suggest that a mayor can strengthen control over local police by either meeting or following up with commissioners more regularly.

We therefore augment Equation (4.3) by examining the interaction between the size of the local police force, and the expenditure in activities related to crime-prevention. If our intuition is correct, we should expect the following results: either the effect of the reform does not change as these two indicators increase, or the effect of the reform is stronger for small police forces and for smaller amounts of expenditure.

The interactions validate the predictions of the second mechanism from the conceptual framework. We find that accountable mayors with small police forces are the most effective in fighting criminality (Table 4.7). Moreover, the coefficients of the treatment effect and the expenditure variable lose significance when interacted. In particular, more detailed investigation of marginal effects reveals that crime incidence decreases faster in municipalities with directly elected mayors who spend a higher share of the local budget on safety and prevention (Table 4.8).

Data on local police and public expenditure on safety are limited. Since we could not cover the whole sample, we focused on municipalities that self-select in the sample given their endogenous propensity to catalyze criminality. The above results initiate a discussion, nonetheless. More data and information are needed to investigate the intensive and extensive margins of the reform.

Table 4.7: Interaction between treatment and size of the local police force

	(1)	(2)	(3)
Treatment	-0.039***	-0.117	-0.260**
	(0.013)	(0.077)	(0.118)
Size local police (log)		0.043	0.031
		(0.028)	(0.029)
Treatment × Size local police		0.017	0.045*
		(0.017)	(0.026)
Social economic controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	No
Year FE	Yes	Yes	Yes
PD FE	No	No	Yes
Cluster level	Municipality	Municipality	Police district
Observations	6454	6454	6454

Notes: OLS regressions based on specification in column (2) of Table 4.5. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. In column (2), we restricted the sample to only those municipalities for which we have budgetary information for the whole period of interest (2000-2012), as explained in Section 4.4.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table 4.8: Interaction between treatment and local expenditure in safety and crime prevention

	(1)	(2)
Treatment	0.085	0.092
	(0.140)	(0.126)
Safety and crime prevention (log)	-0.027	-0.023
	(0.018)	(0.018)
Treatment × Safety and crime prevention	-0.009	-0.010
	(0.010)	(0.009)
Social economic controls	No	Yes
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Observations	6695	6695

Notes: OLS regressions based on specification in column (2) of Table 4.5. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. In column (2), we restricted the sample to only those municipalities for which we have budgetary information for the whole period of interest (2000-2012), as explained in Section 4.4.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

4.7.2 Reconciling the results with the second hypothesis in the conceptual framework

In this section, we investigate how the costs of coordination of local police between neighboring mayors can neutralize any benefits from the introduction of direct elections. In so doing, we address Hypothesis 2 in the conceptual framework.

Municipalities are organized in police districts. Districts may encompass one or several

municipalities, and their size varies significantly across the country. The average police district is made up of four municipalities and is larger in Wallonia than elsewhere. In some instances, up to ten municipalities are covered by police districts, both in Flanders and in Wallonia. Variability in size is moreover highest in the treatment group. These descriptive statistics imply that, on average, more than one mayor sits on the board of the police district, with several leaders usually sharing the mandate of the chief of police – Section 4.2.

Table 4.9: Interaction of the treatment effect with the size of police districts

	(1)	(2)	(3)
Treatment	-0.098***	-0.109***	-0.115***
	(0.025)	(0.026)	(0.025)
Treatment× Small PD	0.036	0.050*	0.055*
	(0.027)	(0.028)	(0.028)
Treatment× Medium PD	0.068**	0.077**	0.078**
	(0.033)	(0.034)	(0.034)
Treatment× Large PD	0.041	0.061*	0.063*
	(0.034)	(0.034)	(0.034)
Treatment× Very large PD	0.075**	0.082***	0.085***
	(0.030)	(0.030)	(0.030)
Marginal effects			
Treatment× Mono PD		-0.109***	
		(0.026)	
Treatment× Small PD		-0.059***	
		(0.016)	
Treatment× Medium PD		-0.032	
		(0.025)	
Treatment× Large PD		-0.048**	
		(0.024)	
Treatment× Very large PD		-0.027	
		(0.019)	
Social economic controls	Yes	Yes	Yes
Municipal FE	Random	Random	Random
Year FE	No	Yes	No
Trend	No	No	Yes
Observations	7654	7654	7654

Notes: OLS regressions. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. Standard errors are clustered at the municipality level and are presented in parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

There are two reasons to believe that the reform would have different effects in police districts where mayors share the mandate. First, as argued in Section 4.3, more effective oversight by one mayor can motivate other mayors in the district to free-ride. Despite the reform, voters do not have enough information to either punish the official responsible for enhancing criminality or to award those mayors that contributed to safer

municipalities. We would thus expect the moral hazard issue to ultimately undermine the collective effort and water down the beneficial impact of increased accountability on crime incidence.

The second reason depends on the costs of coordination. While coordination of police activity is optimal, it may also entail significant monetary and non-monetary effort. High coordination costs may discourage mayors from jointly supervising the police commissioner, ultimately undermining the quality of the public good in equilibrium. We expect the post-2005 reform difference in crime incidence between treatment and control groups to narrow in police districts that are larger than the average and politically more fractionalized and polarized.

We test these predictions with a random fixed effect panel data model similar to the main benchmark specification. In Equation (4.3) we introduce the interaction between the treatment effect and the size of the police district. The interaction term will capture potential variation in the impact of the reform on crime incidence along the different size of police districts. Note that, due to the unequal distribution of the number of municipalities belonging to the same police district, we reclassified the latter into "single-city" districts (composed of only one municipality), "small" districts (with two to three municipalities), "average" districts (with four to five municipalities), "large" districts (with six to seven municipalities), and "very large" districts (with more than seven municipalities).

Figure 4.6: Marginal effects of the interaction between treatment effects and the size of police districts

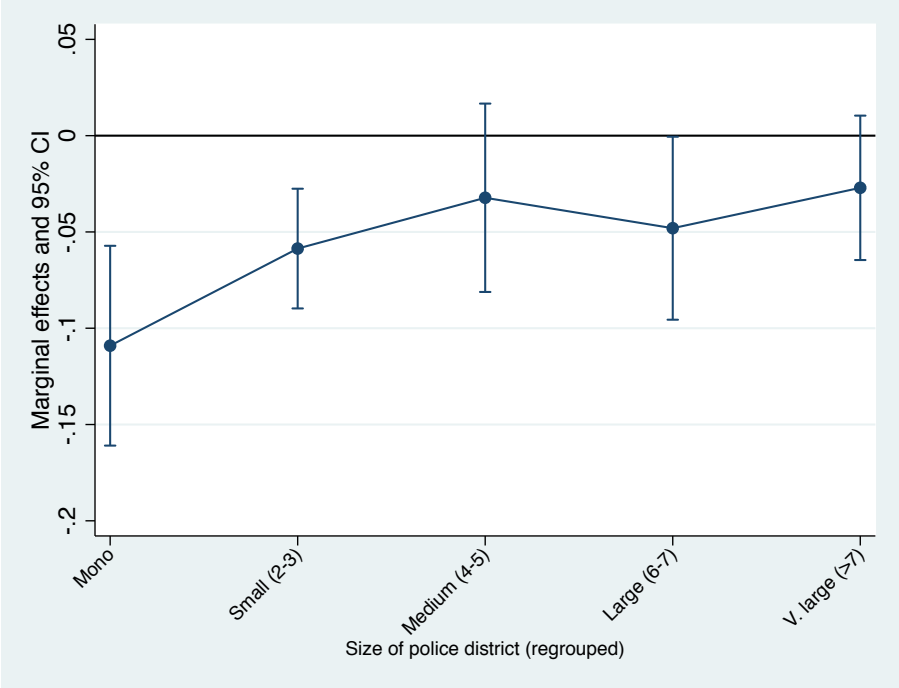


Table 4.9 and Figure 4.6 show the marginal treatment effect by the size of police district. This predicts how much post-reform crime incidence decreases in treated municipalities by comparing police districts by size across treatment and control groups. A positive trend seems to validate Hypothesis 2 in the conceptual framework: the larger

the police districts, the more numerous the mayors sharing the mandate of chiefs of police, and thus the more difficult it will be for voters to keep these mayors accountable for their performance.

Next, we test the interaction between the treatment effect and political diversities in a police district. As detailed in Section 4.4, we proxy for political heterogeneity with two indicators: the Herfindahl index of political fractionalization – as specified in Equation (4.1) – and an index of political polarization – as specified in Equation (4.2) –. We then analyze the interaction between the treatment effect and the indexes of polarization and fractionalization within Equation 4.3.

Marginal effects of the interactions show that when political diversity increases, the beneficial impact of the 2005 reform on crime incidence fades away (Table 4.10). The lack of cooperation between mayors from different parties undermines any reform that aims at increasing the transparency of service provision.

Table 4.10: Interaction of the treatment effect with indexes of political diversity

	(1)	(2)	(3)	(4)
Treatment	-0.075***	-0.053**	-0.091***	-0.065***
	(0.018)	(0.022)	(0.017)	(0.022)
Fractionalization index	-0.006	0.040		
	(0.030)	(0.039)		
Treatment × Fractionalization index	0.067**	0.064*		
	(0.030)	(0.037)		
Polarization index			0.004	0.035
			(0.018)	(0.024)
Treatment × Polarization index			0.066***	0.062**
			(0.020)	(0.025)
Social economic controls	Yes	Yes	Yes	Yes
Public expenditure controls	No	Yes	No	Yes
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	7654	4641	7654	4641

Notes: OLS regressions based on specification in Column (2) of Table 4.5. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. In even columns, we restricted the sample to only those police districts for which we have budgetary information from all member municipalities for the whole period of interest (2000-2012), as explained in Section 4.4.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

4.7.3 Benchmark results by type of crime

Finally, the size and significance of the treatment effect may be a function of the nature of the crime event and consequently of the preventive policies that mayor-chiefs of police decide to implement accordingly. Table 4.11 reports the treatment effect by type of main crime events, as described in Section 4.4: violence, robbery, fraud, vandalism and drugs. The largest and most significant effect concerns violence and robbery: under elected

mayors, the post-reform incidence of these types of crime decreases on average by 4.1% and 6.7%, respectively.

Table 4.11: Benchmark results by type of crime

	Violence	Robbery	Fraud	Vandalism	Drug
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.041**	-0.067***	-0.093*	0.023	-0.074
	(0.016)	(0.014)	(0.049)	(0.018)	(0.051)
Social economic controls	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	7657	7657	7657	7657	7657

Notes: OLS regressions based on specification in Column (2) of Table 4.5. Variable of interest: incidence by type of crime (number of crime episodes per 1,000 inhabitants), in log-values. Standard errors are clustered at the municipality level and are presented in parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

4.8 Conclusion and external validity

This chapter analyzes how the method of selecting local public officials may affect policy making and its outcomes. To do so, we focus on a reform that in 2005 introduced the direct election of mayors in one region of Belgium. Mayors from elsewhere in Belgium are still appointed and are thus accountable to the city councils rather than to voters. We argue that, due to such a reform, Belgian mayors face now different electoral incentives that might ultimately affect their commitment to fighting crime, a sensitive issue in the agenda of local politicians. The complex organization of police in Belgium is indeed such that mayors have significant leeway and interest in steering local policing, especially for electoral purposes.

The analysis draws from a rich and unique dataset on reported crime (disentangled by type) in 589 Belgian municipalities from 2000 to 2012, an observational period spanning two full legislatures. We utilize a difference-in-difference strategy to compare crime rates in Wallonian municipalities as opposed to other Belgian municipalities before and after the 2005 reform.

We find that post-reform crime incidence decreases by 4.9% to 5.7% – two episodes every 1,000 inhabitants – in municipalities where the mayor became directly elected. We argue that direct election tightens the electoral competition. Stiff elections have two consequences: first, candidates prime voters by focusing their campaign on issues they can directly control – such as safety-related ones. Hence, voters have access to more information about the candidates and vote into office only those who take a clear stand concerning criminality. Second, stiff electoral races motivate mayors to increase their monitoring of the activity of police commissioner.

The treatment effect loses significance and magnitude as the number of mayors who share the mandate of the chief of police within the same police district increases. Shared mandates might create free-riding incentives that ultimately affect the effectiveness of the local police force and the extent to which mayors are kept accountable by voters.

Moreover, shared mandates increase the cost of coordination among mayors, especially in large police districts and very politically diverse ones.

The contribution of this chapter to the literature is fourfold. First, we question whether mayors who signal a higher commitment to provide better quality public goods - contextually, lower crime incidence - are more likely to be voted into office and to behave accordingly once elected. Second, this is one of the few papers that looks at whether the modality of selection of mayors might feed into policy-making. Third, we contribute to the literature on the economics of crime by studying how political institutions impact on the management of the police force and crime incidence. Finally, we join a growing literature that investigates accountability and local public service provision in the presence of horizontal agreements between local politicians.

The scope of this chapter is not limited to Belgian municipalities. The interaction between electoral cycles and the local management of public services is a particular concern in countries where local administrators are to some degree accountable to voters and need to coordinate across jurisdictions. Moreover, several other European countries (e.g. France, Scotland and England) have been debating devolution of the governance of police and its potential impact on local criminality.

To conclude, we acknowledge some limitations of the analysis. The proposed empirical strategy is not immune to possible endogeneity issues. The results passed several robustness tests, but there are still some observable and unobservable differences between treatment and control groups that might confound the effect of the reform. Because of data limitations, we were not able to assess the intensive or extensive margins of the reform. How do mayors influence the effort of police commissioners? What incentives do commissioners need to commit to the crime plan? Future research should address these questions and thereby contribute to a growing and promising literature on the relationship between politicians and bureaucrats.

Acknowledgments

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Stockholm University, the European Public Choice Society, the Institut d'Economia de Barcelona, the European Political Science Association, the Society for Institutional and Organizational Economics, and the Transatlantic Workshop on the Economics of Crime. All remaining errors are ours.

Appendix

Table A4.1: Benchmark results, all coefficients

	(1)	(2)	(3)	(4)	(5)
Treatment	-0.049***	-0.046***	-0.057***	-0.037**	-0.053***
	(0.013)	(0.014)	(0.011)	(0.015)	(0.012)
Density (hab./km^2, in log)		-0.243	-0.226	0.094	0.160
		(0.506)	(0.499)	(0.653)	(0.652)
Density^2 (in log)		-0.049	-0.045	-0.091*	-0.093*
		(0.035)	(0.035)	(0.051)	(0.052)
Employment (%)		-0.001	-0.005	0.004	-0.001
		(0.004)	(0.003)	(0.004)	(0.003)
Unemployment (%)		0.006	-0.002	0.007	-0.003
		(0.007)	(0.004)	(0.007)	(0.005)
Foreigners (%)		0.003	0.003	0.005	0.005
		(0.004)	(0.004)	(0.005)	(0.005)
Median income (in log)		0.340**	0.378**	0.295	0.393**
		(0.159)	(0.148)	(0.181)	(0.167)
Mean income (in log)		-0.178	-0.150	-0.217	-0.212
		(0.181)	(0.170)	(0.206)	(0.197)
Trend			-0.006**		-0.007***
			(0.002)		(0.003)
Social assistance (in log)				0.015	0.019*
				(0.010)	(0.010)
Garbage collection (in log)				0.053**	0.064***
				(0.023)	(0.023)
Public administration (in log)				-0.004	-0.007
				(0.019)	(0.018)
Safety and crime prevention (in log)				-0.020	-0.010
				(0.018)	(0.017)
Constant	3.858***	5.349**	4.735**	4.623*	3.458
	(0.008)	(2.115)	(1.992)	(2.385)	(2.269)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	No
Trend	No	No	Yes	No	Yes
Observations	7654	7654	7654	6565	6565

Notes: OLS regressions. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. All specifications include municipality fixed effects and year fixed effects. Covariates at the municipal level are added in columns (2) to (5). In columns (4) and (5) we include only municipalities for which we have budgetary information for the whole period of interest (2000-2012), as explained in Section 4.4. Standard errors are clustered at the municipality level and are presented in parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A4.2: Robustness test - Balancing test for subsamples

	All sample (1)	Border+ (2)
Density (log)	-1.06	-0.19
	(0.09)***	(0.13)
Employment (%)	-5.56	-5.00
	(0.40)***	(0.60)***
Unemployment (%)	3.92	3.50
	(0.27)***	(0.32)***
Foreign (%)	1.40	3.09
	(0.54)***	(0.99)***
Mean income (log)	-0.09	-0.01
	(0.01)***	(0.02)
Median income (log)	-0.08	-0.01
	(0.01)***	(0.01)
Social assistance (log)	-0.56	-0.12
	(0.09)***	(0.19)
Garbage collection (log)	-0.78	-0.43
	(0.08)***	(0.18)**
Public administration (log)	-0.42	-0.04
	(0.07)***	(0.14)
Safety and crime prevention (log)	-0.73	-0.14
	(0.10)***	(0.20)
No. municipalities	589	152

Notes: OLS regressions based on specification in Column (2) of Table 4.5. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. In columns (1) and (2) we excluded the municipalities belonging to the Brussels capital region from the sample. In columns (3) and (4) we restricted the sample to those municipalities at the border between Wallonia and Flanders and their neighbors, only. Standard errors are clustered at the municipality level and are presented in parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A4.3: Robustness test - Clustering standard errors at different administrative levels

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.046**	-0.037*	-0.046**	-0.037	-0.046*	-0.037
	(0.018)	(0.019)	(0.022)	(0.022)	(0.023)	(0.022)
Density (hab./km^2, log)	-0.243	0.094	-0.243	0.094	-0.243	0.094
	(0.516)	(0.737)	(0.503)	(0.731)	(0.284)	(0.517)
Density^2 (log)	-0.049	-0.091	-0.049	-0.091	-0.049	-0.091
	(0.039)	(0.059)	(0.040)	(0.063)	(0.034)	(0.057)
Employment (%)	-0.001	0.004	-0.001	0.004	-0.001	0.004
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)
Unemployment (%)	0.006	0.007	0.006	0.007	0.006	0.007
	(0.008)	(0.007)	(0.008)	(0.007)	(0.009)	(0.008)
Foreigners (%)	0.003	0.005	0.003	0.005	0.003	0.005
	(0.005)	(0.006)	(0.004)	(0.004)	(0.003)	(0.004)
Median income (log)	0.340*	0.295	0.340**	0.295*	0.340	0.295
	(0.175)	(0.191)	(0.161)	(0.172)	(0.237)	(0.228)
Mean income (log)	-0.178	-0.217	-0.178	-0.217	-0.178	-0.217
	(0.186)	(0.212)	(0.173)	(0.186)	(0.214)	(0.206)
Social assistance (log)		0.015		0.015		0.015
		(0.010)		(0.011)		(0.009)
Garbage collection (log)		0.053*		0.053*		0.053
		(0.029)		(0.027)		(0.037)
Public administration (log)		-0.004		-0.004		-0.004
		(0.020)		(0.020)		(0.028)
Safety and crime prevention (log)		-0.020		-0.020		-0.020
		(0.019)		(0.022)		(0.020)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster level	PD	PD	Arr.ment	Arr.ment	Province	Province
Observations	7654	6565	7654	6565	7654	6565

Notes: OLS regressions based on specification in Column (2) of Table 4.5. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. Standard errors of the OLS model clustered the level of the police district, arrondissement and province. Standard errors are presented in parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A4.4: Robustness test - Spatially clustered standard errors

	(1) 5km	(2) 15km	(3) 25km
Treatment	-0.086***	-0.086***	-0.086***
	(0.007)	(0.013)	(0.017)
Density (hab./km^2, log)	0.002	0.002	0.002
	(0.027)	(0.040)	(0.043)
Density^2 (log)	0.009***	0.009**	0.009**
	(0.002)	(0.003)	(0.004)
Employment (%)	-0.027***	-0.027***	-0.027***
	(0.001)	(0.002)	(0.002)
Unemployment (%)	0.040***	0.040***	0.040***
	(0.001)	(0.002)	(0.002)
Foreigners (%)	-0.002***	-0.002**	-0.002**
	(0.000)	(0.001)	(0.001)
Median income (log)	0.697***	0.697***	0.697***
	(0.052)	(0.096)	(0.134)
Mean income (log)	-0.189***	-0.189**	-0.189
	(0.048)	(0.090)	(0.133)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	7654	7654	7654

Notes: OLS regressions based on specification in column (2) of Table 4.5. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. Standard errors of the OLS model are adjusted for spatial correlation following Conley (1999), for different distance cutoffs. A three-year lag is also included in all three specifications in order to take into account the spatial nature of data. Standard errors are in presented parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A4.5: Robustness test - Municipal trend (linear and quadratic)

	(1)	(2)	(3)	(4)
Treatment	-0.075***	-0.071***	-0.075***	-0.068***
	(0.013)	(0.012)	(0.014)	(0.013)
Density (hab./sq.km, in log)	-0.262	-1.770	-0.708	-0.514
	(1.347)	(1.517)	(1.716)	(2.071)
Density^2 (in log)	-0.126	0.032	-0.097	-0.111
	(0.111)	(0.123)	(0.150)	(0.180)
Employement (%)	-0.008***	-0.007**	-0.005*	-0.006*
	(0.003)	(0.003)	(0.003)	(0.003)
Unemployement (%)	-0.014***	-0.010***	-0.015***	-0.012***
	(0.004)	(0.004)	(0.004)	(0.004)
Foreigners (%)	0.005	0.010	0.006	0.017
	(0.007)	(0.007)	(0.011)	(0.012)
Median income (in log)	0.486***	0.404***	0.508***	0.442***
	(0.148)	(0.144)	(0.165)	(0.159)
Mean income (in log)	-0.359**	-0.260	-0.435**	-0.361**
	(0.168)	(0.162)	(0.179)	(0.174)
Social assistance (in log)			0.013	0.009
			(0.009)	(0.010)
Garbage collection (in log)			0.012	0.022
			(0.027)	(0.027)
Public administration (in log)			-0.012	-0.016
			(0.012)	(0.013)
Safety and crime prevention (in log)			0.007	0.007
			(0.017)	(0.017)
Municipality FE	Yes	Yes	Yes	Yes
Trend	Linear	Quadratic	Linear	Quadratic
Observations	7654	7654	6565	6565

Notes: OLS regressions. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. All specifications include municipality fixed effects. Municipal linear trends are in odd columns; municipal quadratic trends are in even columns. Regressions were run through Stata command *reghdfe*. In columns (3) and (4) we include only municipalities for which we have budgetary information for the whole period of interest (2000-2012), as explained in Section 4.4. Standard errors are clustered at the municipality level and are presented in parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A4.6: Robustness test - Police district trend (linear and quadratic)

	(1)	(2)	(3)	(4)
Treatment	-0.140***	-0.149***	-0.092***	-0.114***
	(0.016)	(0.016)	(0.021)	(0.021)
Density (hab./km^2, in log)	0.597***	0.612***	0.155	0.173
	(0.139)	(0.140)	(0.174)	(0.175)
Density^2 (in log)	-0.033***	-0.034***	-0.010	-0.011
	(0.012)	(0.012)	(0.015)	(0.015)
Employment (%)	-0.009*	-0.009**	-0.014***	-0.015***
	(0.005)	(0.004)	(0.005)	(0.004)
Unemployment (%)	0.029***	0.025***	-0.003	-0.007
	(0.009)	(0.008)	(0.010)	(0.010)
Foreigners (%)	0.001	0.001	0.001	0.000
	(0.003)	(0.003)	(0.005)	(0.005)
Median income (in log)	-0.473*	-0.654**	-0.301	-0.428
	(0.258)	(0.270)	(0.321)	(0.328)
Mean income (in log)	0.069	0.070	-0.126	-0.155
	(0.283)	(0.271)	(0.380)	(0.363)
Social assistance (in log)			0.075***	0.074***
			(0.020)	(0.020)
Garbage collection (in log)			0.011	0.013
			(0.045)	(0.045)
Public administration (in log)			-0.116***	-0.121***
			(0.042)	(0.041)
Safety and crime prevention (in log)			0.243***	0.242***
			(0.039)	(0.038)
Municipality FE	Yes	Yes	Yes	Yes
Trend	Linear	Quadratic	Linear	Quadratic
Observations	7654	7654	4641	4641

Notes: OLS regressions. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. All specifications include municipality fixed effects. Linear trends at the police-district level are in odd columns; quadratic trends are in even columns. Regressions were run through Stata command *reghdfe*. In columns (3) and (4) we include only municipalities for which we have budgetary information for the whole period of interest (2000-2012), as explained in Section 4.4. Standard errors are clustered at the municipality level and are presented in parentheses.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A4.7: Robustness test - Regional trend (linear and quadratic)

	(1)	(2)	(3)	(4)
Treatment	-0.110***	-0.116***	-0.078***	-0.095***
	(0.020)	(0.020)	(0.020)	(0.019)
Density (hab./km^2, in log)	-0.068	-0.057	-0.335***	-0.329***
	(0.112)	(0.112)	(0.098)	(0.098)
Density^2 (in log)	0.019*	0.018*	0.031***	0.031***
	(0.010)	(0.010)	(0.009)	(0.009)
Employment (%)	-0.024***	-0.024***	-0.020***	-0.020***
	(0.006)	(0.006)	(0.005)	(0.005)
Unemployment (%)	0.014	0.012	-0.002	-0.004
	(0.011)	(0.011)	(0.010)	(0.009)
Foreigners (%)	-0.000	-0.000	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Median income (in log)	-0.417	-0.542*	-0.064	-0.116
	(0.279)	(0.303)	(0.251)	(0.272)
Mean income (in log)	0.035	0.075	-0.168	-0.162
	(0.220)	(0.219)	(0.202)	(0.201)
Social assistance (in log)			0.072***	0.073***
			(0.024)	(0.024)
Garbage collection (in log)			0.045	0.045
			(0.036)	(0.036)
Public administration (in log)			-0.096**	-0.099**
			(0.039)	(0.039)
Safety and crime prevention (in log)			0.178***	0.178***
			(0.033)	(0.033)
Municipality FE	Yes	Yes	Yes	Yes
Trend	Linear	Quadratic	Linear	Quadratic
Observations	7654	7654	6565	6565

Notes: OLS regressions. Variable of interest: crime incidence (number of crime episodes per 1,000 inhabitants) in log values. All specifications include municipality fixed effects. Linear trends at the regional level are in odd columns; quadratic trends are in even columns. In columns (3) and (4) we include only municipalities for which we have budgetary information for the whole period of interest (2000-2012), as explained in Section 4.4. Standard errors are clustered at the municipality level and are in parenthesis.
Significance levels are denoted as follows: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Annex figures

Figure B4.1: Spatial distribution of crime incidence by municipality, average 2000-2012

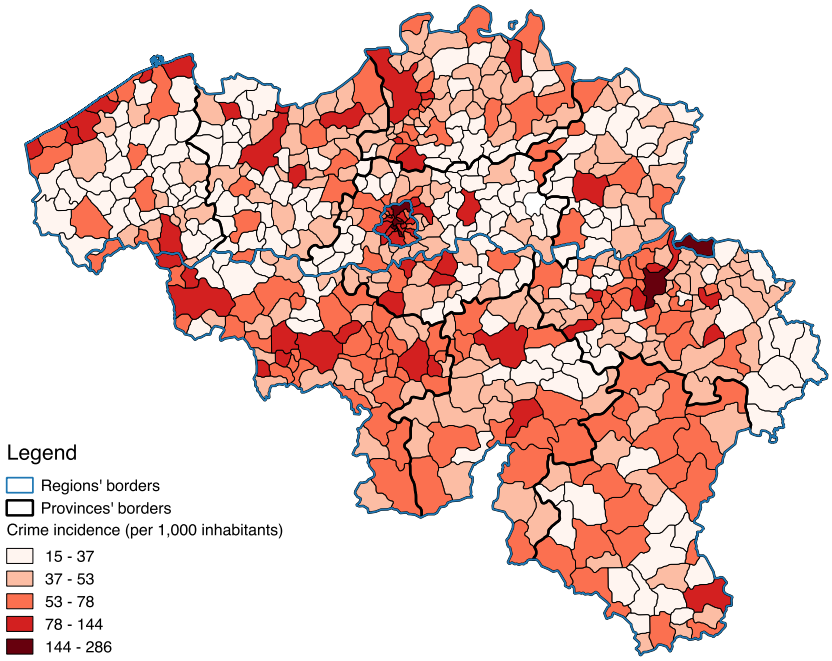


Figure B4.2: Evolution of the local proportion of non-Belgian residents (2000-2012), by region

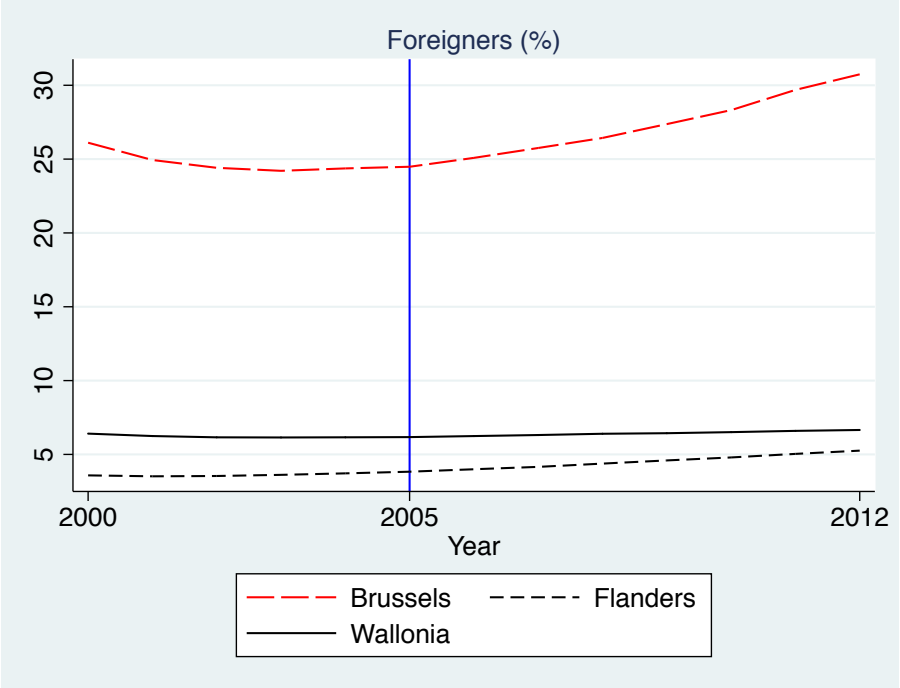
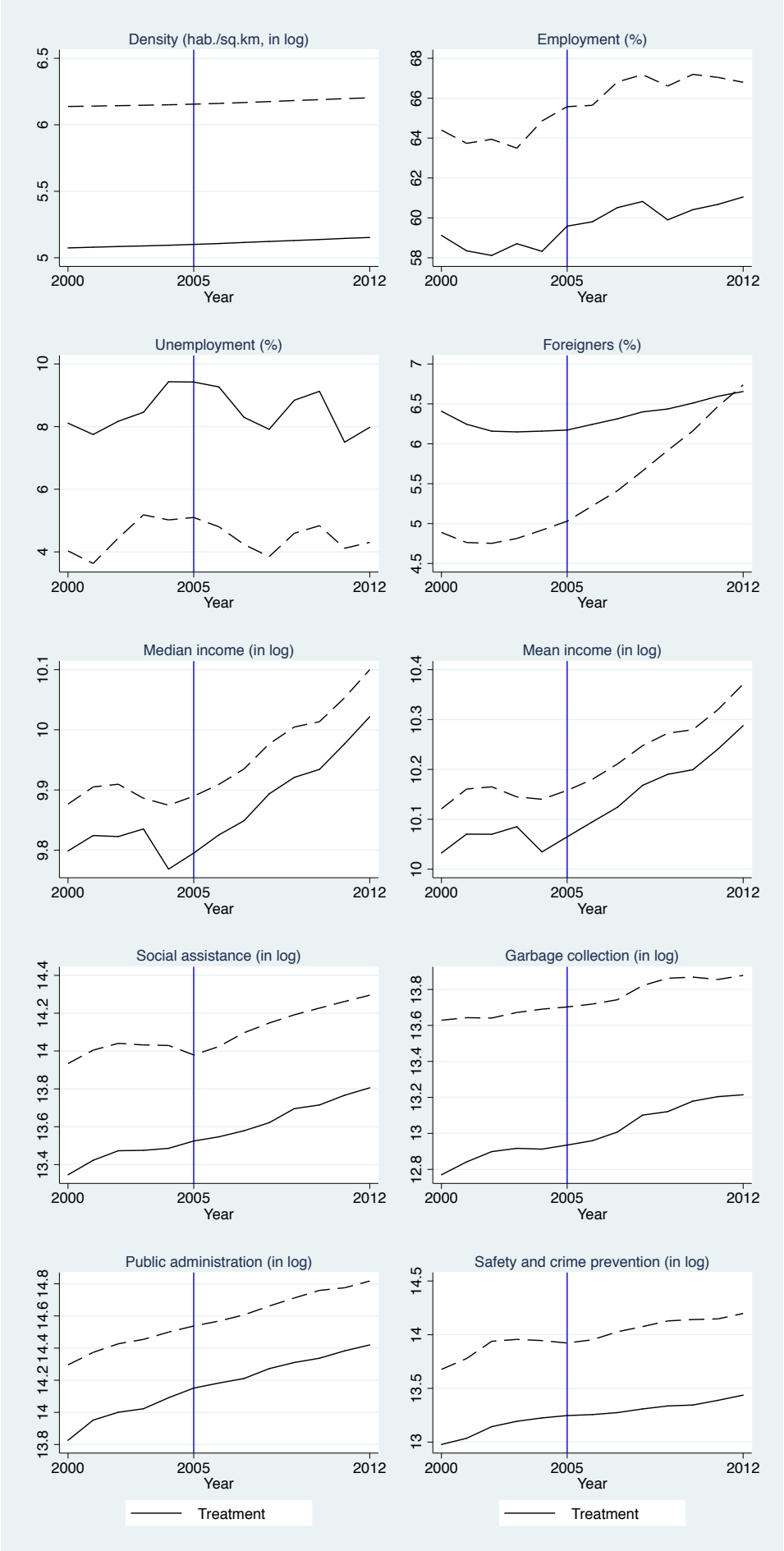


Figure B4.3: Evolution of covariates (2000-2012), by treatment and control groups



Questions for interviews to mayors

Interviews were conducted between August and September 2017, usually in the mayoral office, in either French or English. They lasted between 1 and 2 hours.

Interviewee:
Municipality:
Party:
Police District:

Importance of safety issues in mayor's agenda and effort in fighting crime

1. How much time do you dedicate to police management every day/week? (quantify in terms of hour and %)
2. How importance do you think your voters give to safety?
3. To what extent do voters recognize you as chief of police?
4. How much importance did you give to safety issues during your last electoral campaign?
   (a) Does your effort to fight crime change during the legislature?
   (b) Is there a particular period where you maximize your effort in fighting crime?
5. What do you usually do when it comes to fighting crime? What are the policy instruments in your hands?
6. Is there any specific type of crime you spend more time to fight against? Among: violence, robbery, drugs, fraud, vandalism.

The mayor and the governance of local police

1. How would you define your relationship with the head of police? (How often do you talk to each other? For what reasons?...)
2. How are the decisions taken in the College de Police, Conseil de Police, Conseil Zonal de Securité? - Especially when governance is shared among several mayors
   (a) How do you coordinate with other mayors?
   (b) Is coordination more difficult when your peers come from different parties?
   (c) Does someone have a final word when there are too many divergent opinions and agendas?
   (d) How do you decide to redistribute police within the police district?
3. What factors determine the shape of the police district in which your municipality is located?

The 2005 reform in Wallonia

The reform in 2005 in Wallonia aimed at making the election of mayors more direct.[16]

1. Would you consider yourself (and your peers in Flanders) less directly elected than in Wallonia?
   (a) If yes: how does this affect your policies and work as a mayor?
   (b) If no: do you think the reform has brought about any relevant change in Wallonia? If not, why was the reform implemented in the first place?
2. Has the reform changed the way electoral campaigns are conducted?

[16]The following questions were asked to mayors or local administrators that were not affected by the reform. The goal was to understand how representatives from municipalities in the "control" group perceived the reform.

Chapter 5

Final remarks

Decentralization reforms aim to improve access to public services by redistributing power, resources and authority from the central level to subnational administrative units. The underlying assumption is that local governments can target local needs more precisely and efficiently than the central government, because of the proximity to their constituents. First generation decentralization theories predict that in a decentralized setting the access and quality of public goods could improve significantly. In addition, decentralization could trigger competition between local jurisdictions, which would provide better services in order to attract individuals and investments.

While decentralization policies work on paper, empirical evidence shows that their implementation is more complex. The success of decentralization reforms largely depends on factoring in the type and quality of local institutions, such as pre-existing sets of norms, customs and political dynamics. Yet, these local institutions may not be taken into account sufficiently in the design of top-down policies, hampering the smooth provision of local public goods.

In addition, scholars often interpret decentralization as a unidimensional policy, while in reality it has several facets. They usually assess it by comparing countries that are decentralized to those that are not, as if decentralization were a dummy variable. Instead, decentralization may succeed at different degrees and "intensities" because of the type and quality of local institutions. It is therefore more appropriate to think of decentralization as a "continuous" variable.

This thesis investigates political economic reasons behind the success or failure of decentralization reforms. It aims to better understand how local institutions and political incentives condition the provision of local public goods. By providing evidence from three case studies – Burundi, Brazil and Belgium – I argue that the effects of decentralized institutions depend essentially on two factors: first, their "intensity", that is the degree with respect to which a country is decentralized; second, the type of decentralized public services. I will consider three types of public goods: a "global" or "national" one – peace and stability – and two local ones – sanitation and crime – whose provision may entail inter-jurisdictional spillovers.

In Chapter 2, I test whether political decentralization can deliver political stability at the beginning of the democratic transition in a fragile country. Using a unique dataset on electoral violence in Burundi in 2010, colleagues and I tested whether (1) ethnic composition and ethnic grievances, (2) political competition or (3) the presence and distribution of demobilized combatants drove violence in the first local elections to be organized after the end of the civil war.

Political decentralization failed in municipalities that bore the most consequences of the war. These municipalities hosted two equally large groups of demobilized rebels and were characterized by fierce political competition. Our interpretation is that politicians willing to seize power through illicit means exploited the former allegiances of demobilized

rebels to spoil the ballot. Interestingly, cleavages between ethnic groups, which were the main causes of violence in post-independence Burundi, did not fuel electoral violence in 2010. Violent campaigning was more frequent in municipalities populated by a high proportion of Hutu.

In Chapter 3, I discuss whether political alignment between local authorities spoils the cooperative management of local public goods. Decentralized institutions in Brazil are such that mayors are responsible for the provision of sanitation services. Usually mayors coordinate the management of sewers in order to exploit economies of scale and because of the risk of negative spillovers if decisions are taken by individual municipalities. The literature suggests that local administrators from the same party find it easier to coordinate with each other because similar ideologies or a solid common political network decrease the cost of cooperative decision-making.

In contrast to predictions made in the literature, I find that local access to sewers decreases when the mayors coordinating the network come from the same party. Fellow mayors may collude and underperform because of reciprocity or due to the fear that any scandal involving a peer may result in a backlash on their personal careers. Collusion is instead less likely when two mayors come from different parties, as political competition creates incentives to monitor each other.

In Chapter 4, I analyze the impact of increasing accountability on the delivery of local public goods entailing some form of interjurisdictional coordination. Theory suggests that one of the benefits of decentralization is the increased accountability of local politicians to constituents. If voters were called to directly vote their mayor, they would review more attentively the skills of the candidates. At the same time, candidates would send a stronger signal about their capacities. Over the long term, the accountability of elected mayors is expected to increase.

By exploiting a unique reform and institutional features of Belgium, Ilan Tojerow and I find that increased accountability may improve the quality of a local public good – police, measured through crime incidence – but that the effect fades when the provision is shared among several mayors. Our interpretation is that when mayors manage a common local police force, accountability is blurred. Citizens from interested jurisdictions cannot distinguish the performance of their mayor from that of the neighbor. At the same time, the mayor can free-ride on her peers and hide poor performance. In equilibrium, cooperative efforts would decrease and the quality of the public good would worsen.

In conclusion, this thesis shows that decentralization forces are beneficial in terms of increased accountability and the quality of local public goods, under certain conditions. One-size-fit-all decentralization agenda are deemed to fail because they do not take into account local social and political equilibria. A different type of decentralization can succeed, nonetheless, but it must factor in the quality of local institutions and the capacity of local politicians and bureaucrats to interact and pursue the public good.

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