

Public good under appointed versus elected mayors: policing and crime in Belgium*

Andrea Colombo, Ilan Tojerow[†]

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[PRELIMINARY DRAFT. PLEASE, DO NOT QUOTE]

Abstract

This paper studies how the method by which mayors gain their positions impacts the provision of local public goods and, most specifically, policing. Our identification strategy exploits a natural experiment provided by the introduction in 2005 of direct election to mayorship in only one region of Belgium, Wallonia; while mayors from elsewhere are still appointed by the locally elected City Councils. Particularly, we compare crime incidence under mayors that are directly elected by voters and those that are appointed by an elected body. Conducting a difference-in-difference analysis with a rich dataset registering locally-reported crimes from 2000 to 2012, our results show a post-reform decrease in overall crime from 5% to 8%, depending on the specification. We provide evidence that the reallocation of efforts towards fighting specific types of crimes by directly-elected mayors drives this effect, rather than a general increase in police efficiency. Moreover, the “increased accountability” effect for each directly-elected mayor dilutes when the management of local police has to be coordinated with other neighboring peers.

Keywords: Mayor elections; local government; electoral incentives; crime; Belgium; difference-in-difference

JEL codes: D72, H11, H72, H77, R5

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[†] **Andrea Colombo** (corresponding author): Université libre de Bruxelles, SBS-EM, ECARES. Email: andrea.colombo@ulb.ac.be. **Ilan Tojerow**: Université libre de Bruxelles, SBS-EM, CEB, DULBEA, IZA. Email: ilan.tojerow@ulb.ac.be.

1 Introduction

"It is incredibly important that people can identify who is accountable locally. People, whether they are for you or against you, know there is a Mr London out there and that is incredibly important. One of the problems with the political process is that that people do feel disempowered, shut out from it. We have therefore got to be imaginative about how we bring decision-making closer to them."

[Tony Blair, former Prime Minister of the United Kingdom – October 2006]

In decentralized institutional settings, a mayor is the executive leader of the local government, and is in charge of several categories of expenditure and of the provision of public goods. Simplifying, mayors can be either directly elected by their citizens or appointed by more or less accountable political bodies. In the past twenty years, a discussion on the expediency of having directly elected rather than appointed mayors has sparked around the world. In the United Kingdom, the first directly elected mayor was created as the executive of the Greater London Authority in 2000. Since then, similar reforms involving other British cities followed - the latest set was implemented in 2016. Other examples include Italy, which introduced the direct election of mayors in 1993, Croatia (2009) and Ireland (2011). Proposals in this sense have been put forward by federal governments in non-OECD countries too, like in India (2016). Back to Europe, in Belgium a 2005 reform introduced the direct election of mayors in only one of the country's three regions; elsewhere, mayors are still appointed by the City Council.

This paper will compare policy outcomes between municipalities with directly elected mayors to municipalities with appointed mayors. In particular, it tests the hypothesis that directly elected mayors are more accountable to citizens than appointed mayors, who owe allegiance to parties in the City Council; they therefore exert more effort in fighting criminality. To do so, we exploit two interesting features of local government in Belgium: first, the 2005 reform that introduced the direct election of mayors in Wallonia, the Southern region of Belgium; second, Belgian mayors - contrarily to other European countries - are the chief of local police, and have the responsibility for keeping public order in their municipalities.¹ We utilize a difference-in-difference strategy to compare crime incidence in municipalities with directly elected mayors (Wallonia, or the treatment group) to those with appointed mayors, before and after the 2005 reform. Data on the policy outcome incidence come from a rich dataset on crime - aggregated and disentangled by type - reported during two full local legislatures in 589 Belgian municipalities from 2000 to 2012. Baseline results

¹Crime, moreover, can be a very salient issues for local politicians, especially during electoral campaigns. In this sense, Belgium is no exception. According to the 2010 Eurobarometer round of surveys, 35% and 25% of Belgians worry about burglary and violent crime, respectively.

indicate a post-reform significant decrease in the incidence of aggregated crime in the treatment group by 4.9 to 8.2 percent, depending on the specification.

According to predictions from the political economic literature, there are two theoretical explanations to expect crime incidence - and other policy outcomes - to differ under directly elected mayors. First, if fully informed voters are assumed to perfectly distinguish between types of candidates, they will punish the incompetent one by either not voting for her or, in case of incumbent leaders, by not re-electing her to mayorship; as a result, only competent mayors will be selected into office. Second, given re-election incentives, mayors are less likely to slack off once in charge. Either way, directly elected mayors face incentives to provide better quality public goods. In our case, theory suggests that under directly elected mayors, overall crime incidence is expected to decrease. This may happen because of a general increase in police efficiency, or because of the reallocation of resources in fighting certain types of crime rather than others. Our difference-in-difference analysis rules out the first option: in municipalities that introduced the direct election of mayors, we find a decrease in the incidence of those types of crime that are politically sensitive - like robbery and violence - and an increase for less “relevant” ones.

These results are in line with previous empirical research on the behavior of directly elected regulators and judges. [Besley and Coate \(2003\)](#) finds that elected regulators tend to be more pro-consumer candidates than appointed regulators if they aim at running for re-election.² [Huber and Gordon \(2004\)](#) finds that elected judges issue lengthier sentences as an election nears. [Choi et al. \(2010\)](#) argue that elected judges are more productive, while the appointed judges’ work quality is higher. [Besley and Payne \(2013\)](#) finds that elected judges tend to be more career-concerned and thereby file more employment anti-discrimination charges in order to pander voters and to seize re-election.

There are circumstances, however, that might attenuate the benefits from having directly elected mayors; for example, when the provision of a local public good has to be coordinated across different municipalities. In Belgium, the control over a police district is shared, on average, among four mayors-chiefs of police. A moral hazard issue therefore arises. Each mayor in a police district could blame their peers for possible policy failures or gain credit in case of success, especially when she is directly elected. The signal on her competency would then be blurred and not easily

²[Besley and Coate \(2003\)](#)’s argument is built around the agenda that regulators run for. When regulators are selected by political bodies, regulatory policy becomes bundled with other policy issues that the appointing politicians are responsible for. Because voters have only one vote to cast and regulatory issues are not salient for most voters, electoral incentives will lead politicians and their pick for 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 as pro-consumer candidate.

detectible by voters. As a result, incompetent mayors would not be selected out of office, and electoral incentives would not be enough to keep them accountable once in charge. This mechanism suggests that the larger is the number of mayors-chiefs of police in charge of a police district in Belgium, the less accountable they will be held, the lower will be the difference in crime incidence between municipalities with directly elected mayors and appointed ones. Marginal effects of the 2005 reform interacted with the size of the police districts will support this hypothesis.

This paper contributes to three juxtaposing strands of literature.³ The first one investigates whether voters through direct elections select out of office incompetent (or, generally speaking, “bad” type) politicians, and discipline them once elected. [Ferraz and Finan \(2008\)](#), [Banerjee et al. \(2011\)](#) show that when voters are given enough information about the quality of the candidates, they will vote accordingly and rule out the unfit ones. [Ferraz and Finan \(2011\)](#) investigate whether politicians are less corrupt if they are up for re-election. This paper departs from the existing field literature by not looking at whether increased information shapes voters’ behavior; instead, we question whether mayors signaling higher committed to provide better quality public goods - contextually, lower crime incidence - are more likely to be selected into office and to behave accordingly once elected. Differently from [Ferraz and Finan \(2011\)](#), our contribution is made unique by a reform that make a subgroup of mayors more accountable than elsewhere within the same country. Then, we follow [Estache et al. \(2016\)](#) and question whether increased accountability might be attenuated when several local politicians are in charge for the provision of local public goods.

This work interacts with a second strand of literature that provides evidence on how the modality of selection of mayors might feed back into policy. [Levin and Tadelis \(2010\)](#) find that appointed municipality managers are more likely to privatize services than directly elected ones. [Coate and Knight \(2011\)](#) show that spending falls (rises) following switches to directly elected mayors, relative to jurisdictions not changing their form of government. On the contrary, [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.⁴ [Hessami \(2017\)](#) looks at whether mayors’ incentives and policy choices depend on whether they are appointed by an elected body or directly

³At the national level, 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. \(2007\)](#)

⁴This literature is mostly US-based and recognize two main forms of government in 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, a mayor and the city council are independently elected by voters and jointly develop policy. It is our interpretation that council-manager (mayor-council) municipalities are close in nature to those with appointed (directly elected) mayors.

elected by voters. Similarly, we will exploit a quasi-natural experiment to investigate - to our knowledge - for the first time in the literature whether the different methods of selecting mayors may affect crime incidence.

Thirdly, this paper contributes to the literature on economics of crime. Since [Becker \(1968\)](#)'s seminal economic model of crime, economists have devoted considerable effort to determining its empirical validity. [Dills et al. \(2010\)](#) provides a modern and very complete review of the effectiveness of various deterrence variables (like arrest, incarceration rate, and size of the police force). These variables are in principle controllable by policy, and they affect crime either by raising the expected cost of crime or by incapacitating criminals. This paper is among the few works that look at the interaction between the management of the police force and local institutional characteristics. In his seminal research, [Levitt \(1997\)](#), for instance, shows that the size of police forces increases disproportionately in mayoral election years. [Ater et al. \(2014\)](#), in a paper very close in spirit to ours, investigate the role played by the organizational structure of law enforcement agencies in determining police activity and crime.

In sum, this paper aims at advancing the existing literature in two ways. First, we show that directly elected mayors do provide better quality public goods than the appointed ones. In particular, their increased accountability will make them spending more effort in fighting crime. Second, the "accountability effect" might be diluted when the provision of public goods is shared among a large number of mayors. We therefore hope to shed more light on the virtues and constraints of increased accountability and public good provision, and thereby weight in the worldwide debate around the possibility of enfranchising voters to directly elect their mayors.

In order to support our conclusions, this paper will unfold along the following structure. The next section will discuss the two Belgian institutional features that the empirical analysis will leverage on: the 2005 reform that introduced direct election to mayorship in Wallonia, and the structure of local police in Belgium. Data supporting the analysis are presented in Section 3. Section 4 discusses the benchmark results and insists on their robustness by proposing a number of different specifications. Section 5 reconciles the baseline results with the aforementioned theoretical predictions. In particular, we will disentangle the effect of increased accountability by type of crime, and we will look at how it interacts with the number of mayors sharing the mandate of chief of police. Section 6 will conclude and draw some policy insights.

2 Institutional Background

2.1 The 2005 reform in Wallonia: from appointed to directly elected mayors

Belgium is a federal country, fragmented along different administrative and linguistic dimensions. It consists of three regions - Flanders, Brussels, and Wallonia - in charge of matters related to territories, and of three communities in charge of matters related to persons. Regions and communities overlap. Brussels serves as the capital of both the Flemish region and Community. Brussels is also the capital city of the French-speaking community, while that of the Wallonian region is Namur.

Municipalities, the primary unit of our analysis, are the smallest significant administrative subdivision of Belgium. There are 589 of them: 19 in the Brussels-Capital region, 308 in Flanders and 262 in Wallonia. Municipalities are led by mayors, who are responsible for the execution of laws, decrees, ordinances and orders. Each mayor is supported by the Municipal Council, the representative assembly of the municipality, and they are both renewed every six years. Besides running the local administration (which costs represent 21% of the municipal overall expenditures), mayors are mostly in charge for everything concerning education, cultural and recreational activities, and sanitation. As we will discussed later, mayors are pivotal in the management of the local police, which represents on average 8.7% of the municipal overall expenditures.

While the rules for electing city councilors are the same across the country, the modality of selection of mayors is not. In 1999, the organization of mayoral elections was devolved to the regions, an attempt of the Belgian federal government to further decentralizing the governance architecture of the country. Until 2005, mayors were all appointed by the local city councils across all the three regions. At the end of 2005, under voters' pressure, Wallonia was the only region to introduce the direct election of mayors, which would have been first implemented during the 2006 local elections.⁵ According to the reform, the new "Code for local democracy and decentralization" now establishes that mayor is the candidate that gathered most of the preferences on the most voted list within the winning coalition.⁶ In other words, the 2005 reform made the selection of candidates and the political scenario in Wallonia more transparent and less volatile, when compared to the

⁵The French-speaking magazine *Vers l'Avenir* announced in 2002 that 71% of Wallonian voters wished a direct election of their mayors. The Flemish-speaking newspaper *De Morgen* announced in 2004 that this proportion rose up to 74%. In September 2005, the eruption of a scandalous abuse of social housing that involved several administrators and aldermen from several important cities in Wallonia accelerated the need for such reform.

⁶Theoretically, the "winning coalition" can be formalized through a contract signed within three months from the validation of the electoral results. In practice, strategic cartels of parties formed in the months preceding the 2006 elections.

Brussels-Region and Flanders: Wallonian mayors are now more accountable to voters rather than to political allegiances. We will exploit this institutional discontinuity to test whether the different modalities of selection of mayors may ultimately affect the quality of public goods and services, and in particular the local crime rate.

2.2 The organization of the police force in Belgium

Belgian mayors are officially the chief of local police and are particularly crucial maintaining order in their municipalities. Mayors steer armed local policemen that patrol streets and public places, answer citizens’ requests at least 12 hours per day and intervenes 24/7 in case of need. The local police also manage local traffic and conduct crime investigation under the authority of a magistrate. Mayors could then exert concrete and direct control over local policing and may eventually have a meaningful impact on local crime incidence.⁷

Table 1: Distribution of police districts by size (no. of municipalities) and region

	No. ZP	No. Municipalities			
		Mean	Std. Dev.	Min	Max
Flanders	117	3.54	1.85	1	10
Wallonia	72	5.14	2.57	1	10
Brussels	6	3.42	0.99	2	5
All Belgium	195	4.25	2.33	1	10

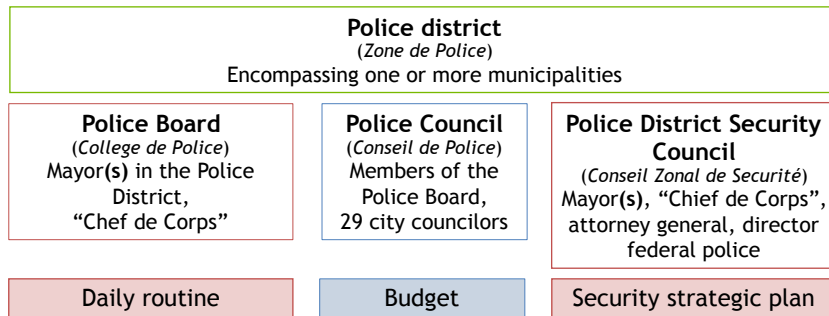
Since the 1st of January 2001, there had been in total 192 police districts in Belgium, each organized with its own police force; 43 of them cover the territory of one municipality (hereafter, one-city zones), while 149 districts encompass several municipalities (more-city zones). The size of a police districts can vary greatly, depending, for instance, on the surface of cities and towns and on their rate of urbanization. On average there are 4 municipalities in a police districts; in some extreme cases, there are 10 of them (Table 1). In one-city police districts, the mayor is the sole chief of local police. In “more-city” police districts, the executive power is delegated to the police college, gathering the mayors from each of the member municipality.

Mayors and the police college are checked and balanced by the police board, which is made of selected city councilors from all the municipalities in a police district. The police board is in charge of the organization and the management of the local police force, including its size and budget (see Figure 1 for a detail organogram of the police district). In each police district, a commanding

⁷Since the dawn of its existence, Belgium was often contested between neighboring States. Such precariousness eventually imprinted both its current federal nature and the the organization of its local police. As a matter of fact, local gendarmeries were the first defense of the new-born Belgium borders and were therefore accordingly organized and equipped. This pattern survived through modern times and contributes to explain the exceptional importance that local police still has in enforcing law and order at the municipal level.

officer is designated for a 5-year term by Royal decree under the proposal of the Police Board. The term can be renewed only once. The commanding officer is responsible for the organization and the redistribution of tasks within the police force, and broadly for the management of policemen. The commanding officer is also the first to draft every four years the security plan for the district. The plan is then discussed and evaluated by the Police Board, and finally approved by all the mayors of a police district.

Figure 1: Simplified organigram of a police district



3 Data sources and descriptive statistics

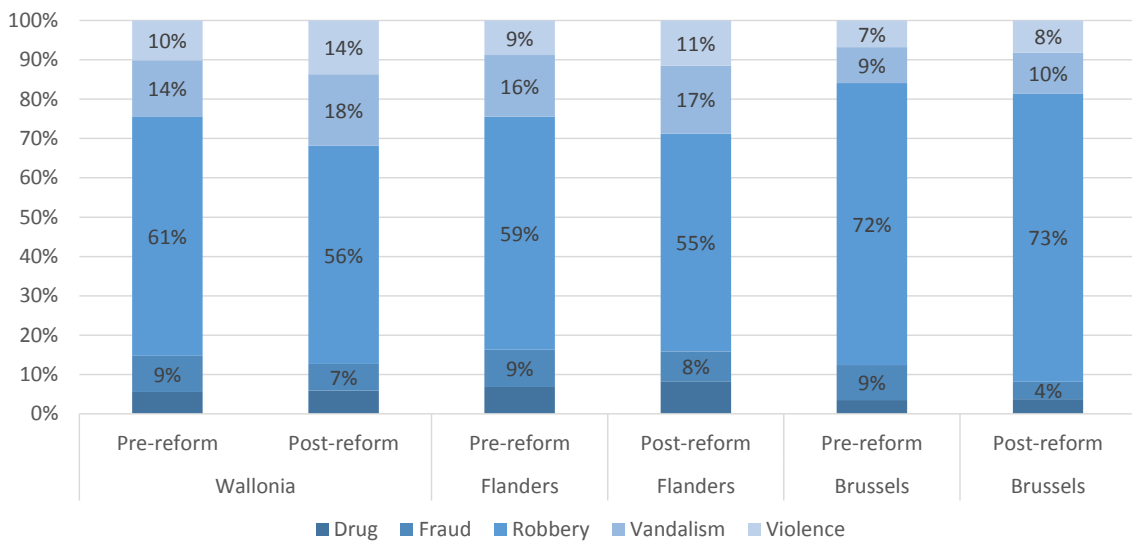
Crime incidence. The policy outcome of interest in our analysis is the local crime incidence, that is the number of crime events observed in each municipality per 1,000 inhabitants. We derived the crime incidence from statistics gathered by the Belgian Federal Police. Their database is a balanced panel of 589 Belgian municipalities, providing detailed yearly information about 7,644 crime events reported in each municipality between 2000 and 2012.⁸ Reports are compiled by the local police agents every time a criminal act is either attempted or realized. They include the exact location and the day the criminal act occurs and classifies it according to categories uniformly defined at the federal level. When multiple offenses occur in the perpetration of a single crime event, the agent only records the most serious of them. The report is then transmitted on a paper version to a federal database (*Banque de données nationale générale*, BNG) within three weeks from the acknowledgment of the criminal act.

The criminal acts that are not known by the police are not included in the data. To this extent, it is useful to recall a major distinction in the nature of criminal events. The Federal Police itself distinguishes between “proactive crime” and “reactive crime”. The number of issued reports concerning “proactive crime” might be positively correlated with the policing effort: the more controls

⁸Data on crime were provided by the Belgian Federal Police, “Police fédérale – Direction de l’information policière et des moyens ICT (DRI) – service Politique et gestion (BIPOL)”. An extract of the data used is available in Flemish and French on the website dedicated to criminal statistics by the Federal Police (<http://www.stat.policefederale.be>).

on drogue abuse and trafficking are conducted, the more observations about that kind of crime pop up in the data. An increase in drogue-related crime, then, would not necessary imply an actual increase in the abuse or trafficking of drogue. However, such an evolution would be an indicator of the pro-activity of the local police forces in tackling drogue-related crime. For what concerns “reactive crime” events, instead, reports are produced following a victim’s complaint. An increase in burglaries in the data, then, can reflect either an actual surge in this kind of crime event or the citizens’ increased propensity to complaint about it (Police Fédérale, 2015). These considerations have three main implications. First, the data used in this article do not provide information about the origin of criminals. Second, the figures might underestimate the true crime rate. Third, it is plausible to assume that not all crimes are reported by victims. Similar limitations are nevertheless common in the literature of crime and, most importantly, we argue that there are no reasons to believe that they may affect crime incidence across the Belgian regions differently.

Figure 2: Distribution of main types of crime by category (in %), across regions and period



Source: Belgian Federal Police. Authors’ own computations.

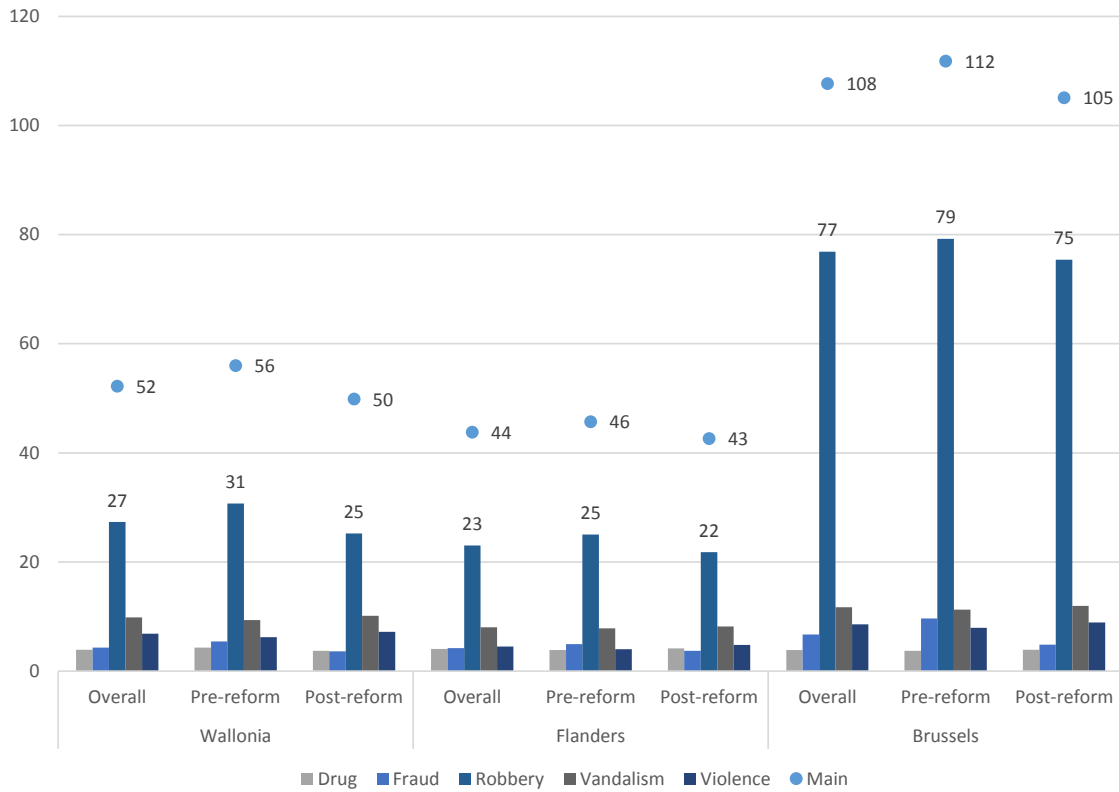
The baseline analysis of this paper will focus on five main categories of crime and their aggregated value: drug, vandalism, fraud, violence, and robbery.⁹ These five categories aggregated represent more than 70% of the total crime reported in the country for the whole period of interest¹⁰ The Federal Police recognizes these five categories as the most serious and concerning sources of crime

⁹An analysis at a more detailed level is more likely subjected to reported mistakes and is heavily complicated by the presence of missing values. From the information we have, we know that drug crimes are mostly related to import and export, usage, and detention; vandalism to destruction of private and public property; fraud to abuse of trust, “con man”, and misappropriation of public funds; violence includes family violence; robbery is reported either broadly or depending on the aggravated circumstances.

¹⁰Before (after) the reform, the aggregated value of the main categories represent 76% (70%), 76% (72%) and 81% (76%) of the total registered crime in Wallonia, Flanders, and Brussels, respectively.

all over Belgium (Van Den Steen et al., 2009). Figure 2 provides more information about the magnitude of each of the five main categories and how they evolve before and after the reform in the three regions. Robbery is the type of criminal activity that was reported most of the times in both periods and in all three regions. Interestingly, it is the only category, together with fraudulent activities (which are nonetheless smaller in proportional magnitude) that considerably decreased in Wallonia and Flanders, while it remained quite stable in Brussels. All the other crimes are increasing in all the three regions, especially in Wallonia. We will see that this fact will drive the main results of our analysis. Reassuringly, Figure 2 does not show any anomalous patterns in the composition of crime across time and regions. As a consequence, any potential change in the variable of interest in our baseline analysis is unlikely to be driven by peculiar trend in sub-categories of crime.

Figure 3: Crime incidence by category, across regions and period



Note: Crime incidence is measured as the number of registered crime events per 1,000 inhabitants.
Source: Belgian Federal Police. Authors' own computations.

Throughout our analysis, and following the literature on crime, we will use as dependent variable the crime ratio for each category of crime and municipality. The crime ratio provides information about the number of registered crime events per 1,000 inhabitants and gives a better idea about the incidence of criminal activity. Figure 3 presents the crime incidence for each of the five categories

and for their aggregated values, by region and period. Once controlled for the population, patterns are similar to those presented in Figure 2 for what concerns both overall crime and sub-categories of crime. Brussels is the region with the highest crime incidence (108 per thousand of inhabitants), Flanders with the lowest one (44 per thousand of inhabitants). Wallonia is in between, with 52 reported crimes per thousand of inhabitants. Interestingly, the effect of the reform appears to be even more consistent in Wallonia with respect to other regions.

Socio-economic variables. In addition to the data on crime, certain specifications of our analysis will control for a number of socio-economic characteristics available at the municipality level. Data about population, density, mean and median income per declaration are available from 2000 throughout 2012 and are retrieved from the “Federal Public Service Economy, SMEs, Self-Employed and Energy”.¹¹ They will be included in the analysis with their logarithmic values. Following the literature on crime, the quadratic term of the logarithm of density will be included: we assume, in this way, that density might affect crime in a non-linear fashion. In particular, small and very large municipalities are sometimes characterized by higher crime than medium-sized urban areas. The only convenient data source for the proportion of population with low or no education degree is the decennial census, carried in 2001 and 2011. Consequently, a linear interpolation of that variable is made for noncensus years. Data about the share of public expenditure devolved to justice and police by each municipalities were obtained from Belfius but are unfortunately incomplete for certain years and municipalities. They will be nonetheless introduced in one of the specifications of our benchmark model.

4 Empirical strategy and results

4.1 Identification strategy

Our objective is to identify the average effect of the 2005 reform of mayoral accountability on crime incidence in those municipality in which mayors became directly elected (i.e., 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 should the reform have never taken place. As the counterfactual is impossible to be observed, we need to identify a number of 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.

¹¹Data are publicly available on <http://statbel.fgov.be/>.

However, the 2005 reform did not involve municipalities randomly, but split Belgium in two groups along the country administrative borders. As discussed in Section 2.1., Wallonia was targeted by the reform; mayors in Flanders and the Brussels-Region, instead, were not affected by the policy and kept on being appointed even after 2005. In a quasi-experimental setting, Wallonian municipalities would constitute the “treatment group”, while the others would act as the “control group”. The identification assumption of our analysis is that Flemish and Brussels municipalities are so similar to Wallonian ones in terms of observed and unobserved characteristics, that any change in crime incidence in the control group after the reform is an estimate of the true counterfactual: they mimic what would have happened to the treatment group if there had been no reform. Comparing the differences in crime incidence between groups before and after the reform will provide a causal estimation of the effect of increased mayoral accountability on local crime incidence in Wallonia.

Formally, we plan to test a difference-in-differences model that can be specified as a two-way fixed-effect linear regression model:

$$y_{jist} = \alpha + \gamma WAL_s + \lambda d_t + \beta D_{st} + \delta \mathbf{X}_{it} + \tau_t + \epsilon_{it} \quad (1)$$

where y_{jit} the logarithm of the rate of crime of j in municipality i and in year t (benchmark results and their robustness will be tested on the aggregate values of observed robbery, violence, fraud, drug, and vandalism); $D_{st} = WAL_s * d_t$ is a dummy that switches on for crime episodes observed in Wallonian municipalities ($WAL_s = 1$) in the post-reform period, from 2005 onwards ($d_t = 1$); \mathbf{X}_{it} is a set of time-varying (observable) municipal characteristics: as reported in Section 4 we control for (the logarithm values of) population, density (and its squared value), mean income, median income, total taxable income, unemployment, proportion of inhabitants with no or low educational degree. Due to its incompleteness, we will include municipal spending in justice and police as the share of overall local expenditure in one specification only. Finally, τ_t is a time variable incremented every year; municipal and year fixed-effects are also included in the benchmark specification.

The error ϵ_{it} is a municipality time-varying error and is assumed to be independently distributed with respect to θ_i and λ_t for every municipality and year. One common problem to panel data is that ϵ_{it} might be correlated across time and space: first, some municipality characteristics correlated with crime incidence (e.g., being a tourist locality) might be persistent and could thereby induce time-series correlation at the municipal level; second, the same characteristics could also

affect neighboring municipalities. To minimize these problems, we allow for an arbitrary covariance structure within municipalities over time and compute our standard errors cluster at the municipality level (Galiani et al., 2005).

In the model described by equation (1), β captures the difference-in-difference estimator of the average impact of the 2005 reform on crime incidence, and it is therefore the parameter of interest. Formally:

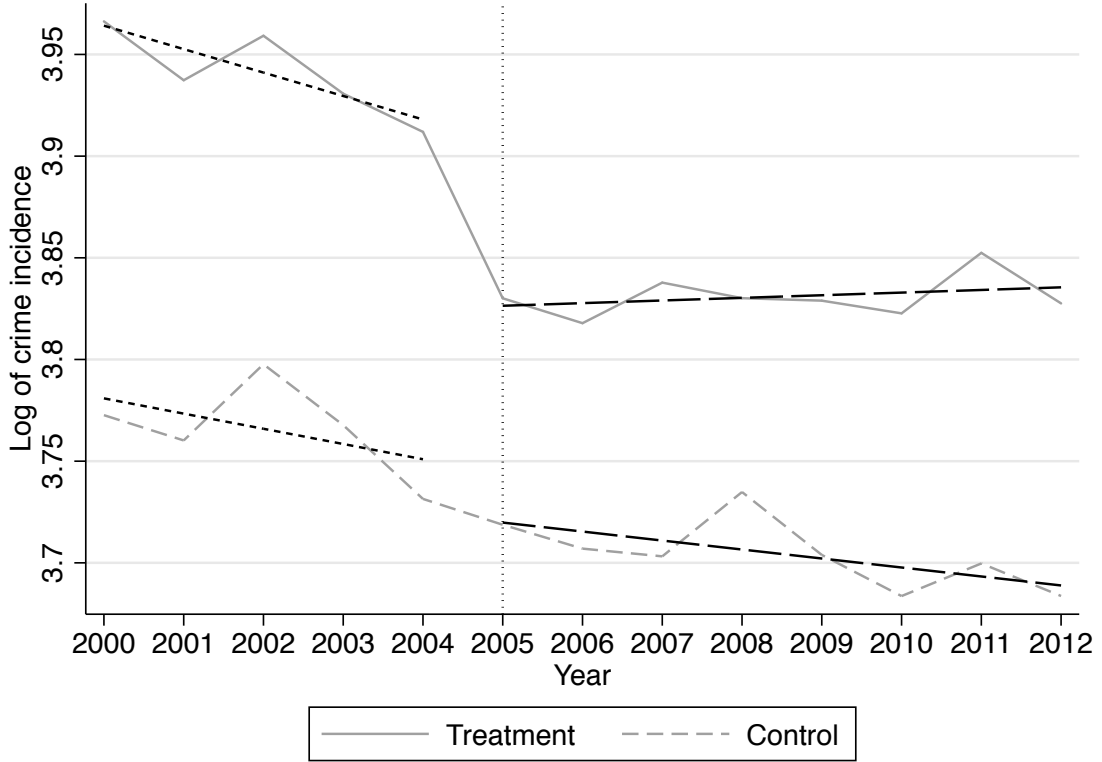
$$\begin{aligned} \hat{\beta} = & (\mathbb{E}[y_{jist}|d_t = 1, WAL_s = 1] - \mathbb{E}[y_{jist}|d_t = 1, WAL_s = 0]) \\ & - (\mathbb{E}[y_{jist}|d_t = 0, WAL_s = 1] - \mathbb{E}[y_{jist}|d_t = 0, WAL_s = 0]) \end{aligned} \quad (2)$$

The first and the second differences compares the change in crime incidence between treatment and control groups after and before the reform, respectively. A negative value of the coefficient of interest would validate our first hypothesis: after the 2005 reform, crime incidence is lower under elected - and therefore accountable - mayors than elsewhere.

If the main identification assumption holds and the change in crime incidence in “control” municipalities is an unbiased estimate of the counterfactual, crime incidence before the reform should develop along parallel trends before the reform across the treatment and the control group; moreover, for the reform to have affected only the treatment group, crime incidence in the control group after the reform should not deviate from the pre-reform trend. We will therefore amass evidence of the substantial pre-treatment parallel trend in the dependent variable and that any change in the crime incidence in control municipalities is an unbiased estimate of the counterfactual. A graphical inspection of the evolution of the average value of the dependent variable by group suggests that trends were indeed parallel before the reform was implemented. While the evolution of crime incidence in Wallonia diverges after the reform, the same trend for the control group remains substantially unaltered (Figure 4).

A potential source of concern is the discrete jump in the average crime incidence the year of the reform that involves not only the treatment group (as we should expect in order to validate our main hypothesis), but the control group as well. For the reform to have a causal impact on Walloon municipalities and for the control group to mimic correctly the counterfactual, no deviation of the dependent variable from the pre-reform trend should be observed in Flanders and in Brussels. Figure 4 might be pointing at unobserved explanations for a change in crime incidence in both treatment and control group that could water down the overall treatment effect. In other words,

Figure 4: Trend of crime incidence (main categories aggregated) by treatment and control group



Note: Crime incidence is measured as the number of registered crime events per 1,000 inhabitants.
 Source: Belgian Federal Police. Authors' own computations.

if this discrete change in the crime incidence in the control group were statistically 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 the dependent variable between one and two years around the reform is not significant for the control group, while it is for the treatment group.

We moreover use pre-reform data from 2000 to 2004 to formally estimate differential time trends in the dependent variable for treated and control municipalities. First, we interact WAL_s - the dummy identifying treated municipalities from control - with a constant linear trend. If the pre-treatment trend were not parallel between the two groups, the difference captured by the interaction should be significantly different from zero. Secondly, in order to detect potential differences in trend for each of the pre-treatment years, we estimate a new model where WAL_s is interacted with a series of year dummies (Abramitzky and Lavy, 2014). Results of the estimation of these two models are displayed in Table 2.

The estimates from first model (Columns 1 and 2) suggest that there is no difference in the

pretreatment trend of crime incidences across municipalities. The mean trend is significantly different from zero, but the annual decrease amounts to 0.6% in (the logarithm of) crime per 1000 inhabitants, only. The estimated coefficient on the interaction of the constant trend with the treatment indicator is also not statistically significant. According to the second model, instead (Columns 3 and 4) the interaction terms of the treatment indicator with the year dummies are all small and sometimes significant when municipality fixed effects are included. For years 2002 and 2003 the trend of crime in treatment municipalities may diverge from control municipalities, although weakly.

Table 2: Testing pre-treatment parallel assumption

	(1)	(2)	(3)	(4)
WAL	0.187*** (0.042)	.	0.194*** (0.040)	.
Time Trend	-0.007* (0.004)	-0.007* (0.004)		
Time Trend X WAL	-0.004 (0.006)	-0.004 (0.007)		
WAL x 2001			-0.016 (0.015)	-0.016 (0.016)
WAL x 2002			-0.032* (0.017)	-0.032 (0.020)
WAL x 2003			-0.031 (0.020)	-0.031 (0.023)
WAL x 2004			-0.013 (0.023)	-0.013 (0.026)
Municipality FE	No	Yes	No	Yes
No. municipalities	589	589	589	589
Observations	2945	2945	2945	2945

Notes: This table presents the results from OLS regressions run at the municipal level predicting the logarithm of crime incidence (crime rate per 1000 inhabitants). In the regressions in Columns 1 and 2, differences in crime between treatment and control municipalities are allowed to vary according to a linear time trend that differs in treatment and control municipalities. Treatment municipalities are the only ones that implemented the 2005 reform. In the regressions in Columns 3 and 4, outcomes are allowed to vary freely for each pre-reform year. Estimates in columns 2 and 4 include municipalities fixed effects; as a result, treatment coefficients in this case are omitted because of collinearity. Significance levels are denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Results and robustness

Table 3 presents the results from the estimation of the benchmark model - equation (1) - for the logarithmic value of the incidence of the aggregated main types of crime (drug, fraud, robbery, vandalism, and violence). Column 1 reports the results for the specification that includes no covariates except for municipality and year fixed effects. We find that the average number of main crime events per 1,000 inhabitants has decreased in municipalities where the mayors are elected

following the 2005 reform. In particular, overall crime incidence has decreased by 4.8 percent with respect to the control group and the pre-reform period.

The average impact of the treatment on the treated in Column 1, however, may be endangered by socio-economic and geographic heterogeneities across Belgium. If such differences were correlated with crime incidence, and at the same time made the reform on mayoral accountability more likely in Wallonia than anywhere else, an endogeneity issue would arise and bias our estimations.¹² In order to handle these concerns, we propose six different variations of the main specification.

Table 3: Benchmark results and robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.049*** (0.012)	-0.053*** (0.015)	-0.049*** (0.016)	-0.053*** (0.015)	-0.065* (0.034)	-0.082*** (0.025)	-0.074*** (0.015)
Controls	No	All, no G	All	All, no G	All, no G	All, no G	All, no G
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Heterogeneous FE	No	No	No	No	No	Regio YFE	Regio trend
Sample	Full	Full	Full	No BXL	Border+	Full	Full
No. municipalities	589	589	589	570	152	589	589
Observations	7657	7657	6965	7410	1976	7657	7657

Notes: This table presents the results from OLS regressions run at the municipal level predicting the logarithm of crime incidence (crime rate per 1,000 inhabitants). Standard errors are clustered at the municipal level and presented in parentheses. Significance levels are denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

First, we control for control variables that may confound the effect of the treatment on the dependent variable. Column 2 shows that the inclusion of these control variables does not affect the statistical significance of the coefficient of interests; in fact, they even increase its magnitude. Due to the incompleteness of the information about municipal expenditure in justice and police, we add this variable to the other covariates in a third specification. The results are presented in Column 3 and do not show substantial differences in magnitude and significance with Columns 1 and 2.

Second, we restrict the sample size to find a subset of municipalities in the treatment and control groups that are as comparable as possible with respect to observable and unobservable characteristics. As a start, we exclude the region of Brussels from the analysis: given its status as capital of the country and headquarter of the main European institutions, one may argue that Brussels could be a formidable magnet of criminal events as compared to other cities in the country. Column 4 shows that robustness of the results are not particularly sensitive to this specification. Then, we restrict the original sample of 589 municipalities to a subsample comprising municipalities that are located along the administrative border between the treatment and the control group, and

¹²In Section 2.1., for instance, we discussed that a particular need for more accountable mayors in Wallonia arose following pressure by its citizens, who at the turn of the century demanded more transparency in the local public budget management. This pressure might be related to observable or unobservable characteristics that might make Wallonian municipalities unique with respect to other parts of the country.

their neighbors. Following a common practice in the quasi-experimental economic literature, we assume that administrative units just across the border tend to be similar in terms of observed and unobserved characteristics, in spite of possible fundamental and historical differences between the three Belgian regions. Moreover, we account for the neighbors of the border municipalities in order to partly control for potential spillovers that usually characterize criminal activity.

Table 4 shows a t-test for the mean difference in each of the control variables between the treatment and the control group. Before the 2005 reform, all control variables but the level of education were on average statistically different between the treatment and the control group (Column 1). Such imbalances attenuate once we restricted the sample to those municipalities at the border and their neighbors, except for unemployment - which is 3.5 percent higher in the treated sub-sample than in the control group (Column 2).

Table 4: Robustness check: restricting the sample to municipalities along the border

	Pre-treatment (2000-04)		Post-treatment (2005-12)	
	(1) Full sample	(2) Restricted sample	(3) Full sample	(4) Restricted sample
Population	-8,447.52 (2157.19)***	-1,545.65 (3311.55)	-8,947.89 (2,259.82)***	-1,370.67 (3408.97)
Density	-690.55 (130.54)***	-65.78 (72.53)	-746.00 (143.18)***	-62.05 (74.50)
Unemployment rate	3.92 (0.27)***	3.5 (0.32)***	4.06 (0.28)***	3.68 (0.32)***
Total taxable income	-125,000,000 (25,300,000)***	-34,500,000 (38,800,000)	-159,000,000 (31,700,000)***	-39,600,000 (49,600,000)
Mean income	-2,106.97 (273.62)***	-163.3 (575.32)	-2,254.73 (306.43)***	-607.86 (615.44)
Median income	-1,481.29 (151.81)***	-292.77 (258.22)	-1,700.59 (171.31)***	-728.08 (277.99)*
% low (or no) education	0.43 (0.40)	-1.19 (0.89)	1.26 (0.39)***	-0.43 (0.82)
%G in justice&police	-1.4 (0.22)***	0.06 (0.32)	-1.40 (0.22)***	0.35 (0.32)
No. Municipalites	589	152	589	152

Standard errors are in parenthesis and are clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We further test for statistically significant differences in the same control variables, but after the treatment. This comparison is particularly important to detect any important deviation from the pre-treatment trend in the observables between treatment and control groups. In that case, one may argue that the behavior of diverging control variables might ultimately be endogenous to our analysis. Column 4 shows that once the sample is restricted, there are no significant average differences between the treatment and the control group except for two cases. First, unemployment remains statistically significant between the treatment and the control sub-samples; however, this post-treatment difference is not statistically significant from the pre-treatment one. Second, the

two groups differ in terms of median income, while they did not before the reform. We argue that median income is not endogenous to the detection of the average treatment effect on the treated: mayors have no power to shape wealth in their municipalities, and therefore the reform might have hardly had any effect on that. However, we recognize that as a potential driver of criminality, median income is a covariate to account for in the benchmark regression.

Column 5 of Table 3 shows that the parameter of interest loses significance when the sample of interest is restricted to municipalities at the border and their neighbor. In particular, the treatment effect is now different from being null at a 10 percent level, rather than 1 percent as in previous specifications. However, one should note that this effect might be partly due to the sensitive decrease in the number of observations. In fact, a significant - although weaker - parameter given the smaller sample size actually plays in defense of the robustness of the treatment effect we are estimating.

In Column 6 and 7 of Table 3 we test the robustness of our model by controlling for spatial heterogeneity in the trends of outcome variables. Heterogeneous patterns in observable and unobservable characteristics across the country could be persistent across time and endogenously shape the evolution of crime in the three regions. For instance, if Wallonia were less prone than Flanders and Brussels-Region to absorb economic shocks, unemployment and income volatility might eventually affect regional trends of crime incidence more in the treatment group than in the control group (Allegretto et al., 2011).¹³ Municipality and year fixed effects alone are not enough to tackle heterogeneities in underlying crime patterns. Following the literature, we propose two further specifications. The first one allows year effects to vary by regions: we include region-specific year effects defined as the interaction between the indicator of the time of the reform (d_t) and a categorical variable for the regions (ρ_s). We argue that this interaction term eliminates the between-regions variation and hence better controls for spatial heterogeneity in different crime patterns, including region-specific economic shocks:

$$y_{jist} = \alpha + \beta D_{st} + \delta \mathbf{X}_{it} + \phi \tau_t + \lambda d_t * \rho_s + \epsilon_{it} \quad (3)$$

In the second specification we introduce a region-specific linear trend variable controlling for heterogeneity in the underlying (long-term) evolution of crime rate. We argue that this further interaction

¹³Allegretto et al. (2011) accounts 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

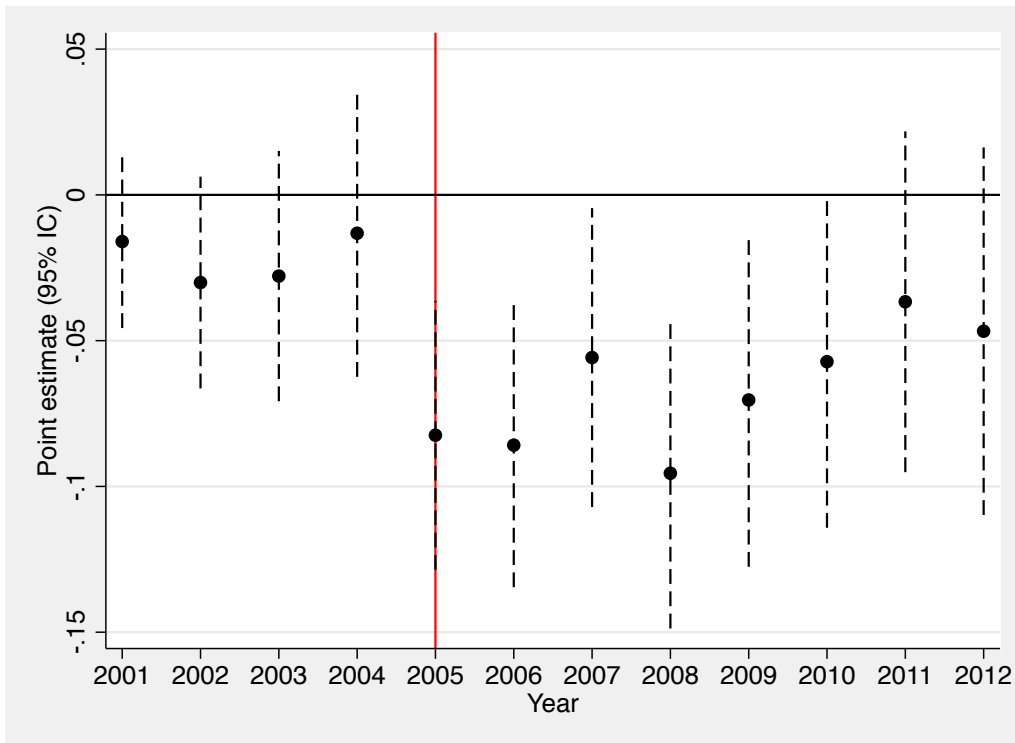
controls for unobserved regional characteristics that might persistently affect crime. Formally:

$$y_{jst} = \alpha + \beta D_{st} + \delta \mathbf{X}_{it} + \phi \tau_t * \rho_s + \lambda d_t + \epsilon_{it} \quad (4)$$

Independently on whether we add region-specific year fixed effects or region-specific time trends, the coefficients of interest in Columns 6 and 7 of Table 3 do not lose in significance. The magnitude is actually higher than in the previous specifications.

A final possible source of concern might be the anticipatory effects of the treatment. As discussed in Section 2, the 2005 reform of the accountability of mayors in Wallonia started to be discussed at the end of 2004. Given the wide political support, its later implementation might have been anticipated by Wallonian mayors, who had more time to adjust their crime-preventing policies (especially within sight of the 2006 elections). The discrete specifications in Table 3 provides no sense of the dynamics of the treatment effect: how quickly crime incidence reacts to the reform, and whether this impact accelerates, stabilizes, or reverts. If crime incidence leads to the adoption of the reform rather than vice versa, the previous estimates may hide this reverse causality. To explore these dynamics, Figure 5 disentangles the effect of the reform by year and shows little evidence of an anticipatory response in municipalities where the mayor is about to be directly elected.

Figure 5: Average treatment effect on the treated, disentangled by year



5 Understanding the channels through which enhanced accountability is (not) impactful

For what concerns the rest of our analysis, we will elect the specification reported in Column 2 in Table 3 - a fair mix of cautiousness, robustness, and explanatory power - as our favorite one. The coefficient of interest in this case shows that, following the 2005 reform, the number of crime per 1,000 inhabitants has decreased on average by 5.3 percent in Wallonian municipalities with respect to elsewhere and the pre-treatment period. Figure 5 shows that in the year of implementation, crime incidence decreased substantially by 8.2 percent. The negative effect is persistent over the following four years, during which it fluctuates between 5.5 and 9.5 percent. After 2009, the effect of the reform starts to fade away and loses statistical significance in the last two year of our panel. The goal of this section is twofold. First, we aim at unveiling possible mechanisms through which crime incidence decreases under elected mayors rather than appointed ones. Second, we investigate the reasons for which the treatment effect diminishes in intensity and relevance towards the end of the second legislature.

5.1 Disentangling the effect of the reform type of crime

The size and significance of the treatment effect may be a function of the nature of the crime event and consequently of the approach that elected mayors decide to take with respect to it. Table 5 reports the treatment effect by type of main crime events, while Column 1 reproposes the results from our favorite specification. The largest and most significant effect concerns violence and robbery: under elected mayors and after the reform we expect on average 4.3 and 6.7 percent of observed events per 1,000 inhabitants less compared to elsewhere, respectively. The positive effect of the reform on vandalism and the slightly lower resulting significance for fraud and drug might suggest a change in the crime-preventing strategy: elected mayors might allocate a different amount of resources to fight certain types of crime than others.

Table 5: Average treatment effect on the treated, by type of crime

	(1)	(2)	(3)	(4)	(5)	(6)
	Main	Violence	Robbery	Fraud	Vandalism	Drug
Treatment	-0.053*** (0.015)	-0.043*** (0.016)	-0.067*** (0.015)	-0.109** (0.051)	0.030 (0.020)	-0.131** (0.055)
Controls	All, no G	All, no G	All, no G	All, no G	All, no G	All, no G
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	Full	Full
Observations	7657	7657	7657	7657	7657	7657

The negative treatment effect for visible crimes like violence and robbery might be explained in two

competing ways. First, following the enhanced accountability, mayors in Wallonia might decide to exert more effort in tackling crime in their municipalities. If the effort is measured by the number of policemen patrolling, its increase would prevent certain types of crime to occur. On the other hand, an inefficient police force - although upgraded - would fail in spotting and therefore report crime events. The negative coefficient of interest, then, may be the symptom of a lower propensity of victims to delate as a reaction to arguable inefficiency and ineffectiveness of the police force. Further data and analysis is needed to address these channels. To this extent, we plan to include in the panel the number of policemen allocated in each municipality every year.

5.2 Accountability, share mandates, and crime

In Section 2 we discussed the particular organization of Belgian police, which is articulated in police districts. Districts may encompass one or several municipalities, and their size varies greatly across the country. The average police district is made of 4 municipalities and is larger in Wallonia than elsewhere. In certain cases, there are up to ten municipalities by police districts, both in Flanders and in Wallonia. Variability in the size is moreover the highest in treatment group (Table 1).

The organization of the local police implies that, on average, there is more than one mayor sitting in the Police Board of the district. The mandate of chief of police, then, might be shared among several mayors at the same time. Conceptually, shared mandates may raise a moral hazard issue: the different mayors may have intrinsically conflicting electoral objectives depending, for instance, on their willing to seek re-election or on the party they belong to. Such distortions may create incentives for certain mayors to free-ride on others - perhaps the more efficient ones, or those heading a larger municipality. As for the classical public good, this perverse mechanism would ultimately trigger a Prisoners' dilemma, with underprovided policing and higher crime incidence in equilibrium. In our empirical analysis, the average treatment effect would then be lower in magnitude and statistical significance in municipalities belonging to numerous police districts.

We test this prediction with a random fixed effect panel data model similar to our favorite benchmark specification. Differently from equation 1, however, we include a term interacting the treatment and the size of the police district. The interaction term will capture the 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, I reclassified the latter into “mono”-districts (composed by 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.

Figure 6: Marginal treatment effect by size of police district

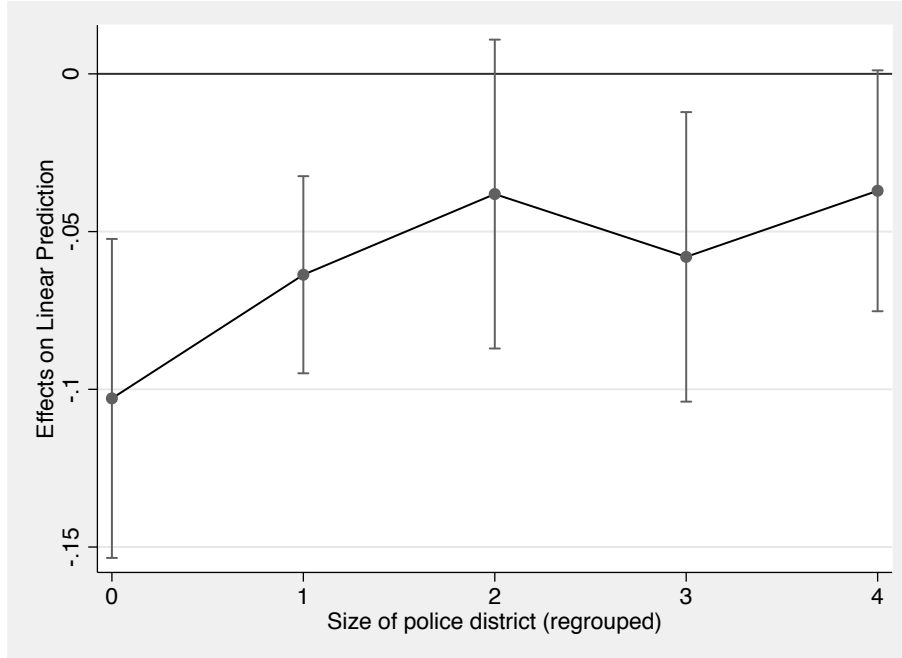


Figure 6 shows the marginal treatment effect by size of police district, that is by how much crime incidence is expected to decrease in treated municipalities following the 2005 reform. A clear positive trend seems to validate our intuition: the larger is the police districts - the higher will be the number of mayors among which the mandate of “chief of police” will be shared - the more difficult will be for voters to keep these mayors accountable for their management of police force and resulting crime incidence.

6 Conclusion

This paper analysis how the method of selecting local public officials may affect policy making and its outcomes. To do so, we focus on a reform introduced in one region of Belgium, Wallonia, in 2005. Following this reform, Wallonian mayors are directly elected and therefore more accountable to voters. Mayors from elsewhere in Belgium, instead, are still appointed and thus accountable to political parties. We argue that, due to such a reform, Belgian mayors face now different electoral incentives that might ultimately affect their commitment to fight crime, a sensitive issue in local politicians’ agenda. The complex organization of police in Belgium is indeed such that mayors have a 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. Results from the most conservative specification indicate a post-reform significant decrease by 4.8 to 8.2% in overall crime, and in “politically sensitive” types of crime (like, robbery) in Wallonian municipalities as compared to elsewhere and to the pre-treatment period. The treatment effect, however, loses significance and magnitude as the number of mayors sharing the mandate of 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.

The contribution of this paper to the literature is threefold. First, we contribute to the vast empirical and theoretical research about the impact of political decentralization on the provision of local and regional public goods and services. Second, multilevel governance and local politicians’ accountability are determinants of crime that remain overlooked by the existing literature. Third, the local management of police in Belgium has been particularly questioned after the recent terrorist attacks. We believe that our paper has also an external validity and its interest is not limited to the Belgian borders, only. First of all, the devolved governance of police and its impact on crime has been debated in several other European countries (e.g., France, Scotland and England). Finally, the interaction between electoral cycles, the local management of police and thereby its impact on crime, is a particular concern in countries where the local administrators of justice are accountable to voters (e.g. State attorneys in the US).

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