

In the Line of Fire: Political Violence and Fiscal Decentralization in Colombia*

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Abstract

Policies aimed at increasing the resources and autonomy of subnational governments are often proposed as a conflict resolution strategy. This paper explores a particular threat to the effectiveness of such reforms in war-torn countries, namely the capture of local governments by non-state armed actors. These groups are reliant on rents and may increase their violence in an attempt to capture new public resources. This is explored using subnational-level data from Colombia during the 1990s, when local fiscal capacity was greatly expanded. I exploit a key reform in the system fiscal transfers to identify the effect of automatic transfers on selective political violence. The evidence shows that the increase in the fiscal resources is associated with higher murder rates of local authorities and politicians. Moreover, this effect is conditional on local state capacity. These results are robust and consistent with the rent-seeking strategy of paramilitaries and guerrillas during the period.

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Introduction

Decentralization processes have become increasingly common among developing democracies around the world. Latin America for instance, a region characterized historically by its centralized governments, elected more than 70 per cent of all sub-national tier executives during the 1990s (Treisman, 2002). Decentralization has also proven particularly appealing to countries either currently in conflict or with a history of regional strife. Devolving power to local governments, it is argued, can help mitigate grievances by allowing local authorities an extent of autonomy. For example, if local governments are better equipped to offer public services and decentralization improves this capacity, citizens may be less likely to support or join a rebel group.¹ On the basis of this idea, decentralizing reforms have been advocated by development organizations, and many countries including Indonesia, Colombia, and Bolivia have utilized some form of decentralization in part to offer an alternative means of political expression to would-be-rebellious regions.

Are reforms that redistribute authority and resources between national and subnational governments convenient for conflict-prone democracies? Although there are many ways in which decentralization may help local authorities win their constituents' loyalty and promote better social outcomes, the empowering of local governments can also provide new opportunities for capture, coercion, and increased strife. In particular, the influx of economic resources associated with decentralizing reforms can increase the importance of subnational governments in the overall strategy of insurgencies. This way, non-state armed groups may be incentivized to use their fighting capacity in an attempt to exploit public resources in a decentralized fiscal system.²

This paper contributes to this debate by analyzing this overlooked threat to the effectiveness of decentralization as a conflict resolution strategy. Namely, I study how decentralization under war influences the incentives of armed actors in a way that is detrimental to the security of the local administrations involved in the process. The main idea behind my argument is simple. In a civil war, non-state armed actors are reliant on rents in their fight against the central state or against other armed militia groups. Since decentralization usually involves not only new administrative responsibilities but also new sources of funding

¹Under a similar principle, the improvement of social conditions through development aid has become one of the cornerstones of the US military's counterinsurgency doctrine in recent times (US Army/Marine Corps 2007). This is what some scholars have labeled as the "heart and minds" theory of counterinsurgency (see e.g., Beath, Christia, and Enkolpov 2013; Berman, Shapiro and Felter 2011). Although this theory is relevant to the evidence presented in the current paper, development programs aimed at reducing violence and combating insurgencies may or may not involve decentralizing reforms.

²Similar mechanisms could be also present during peace and may even trigger a civil war or a secessionist movement. For instance, studies have shown how some forms of political decentralization are indirectly associated with ethnic conflict, secessionism, and terrorism (e.g., Bakke and Wibbels 2006; Brancati 2009).

for subnational governments, which often lack the “repressive capacity” (Fearon and Laitin 2003) to contain insurgencies, these governments become an attractive target for non-state actors. Thus, decentralization under a state which does not fully control its territory opens the possibility for non-state armed actors to capture public resources using selective political violence as an instrument of coercion.

I formalize this argument in a simple rent seeking model in which multiple armed actors compete for a public resource rent. Armed groups invest in violence in the hope of capturing a local government, in which case they will secure the appropriation of the public rent (or at least part of it). In this setting, an exogenous increase in the level of public resources generates a “greed” effect in the sense that actors will make bigger investments in violence. In addition, I assume that some key locality-specific characteristics influence the likelihood of capture. Specifically, high levels of state capacity makes capture less feasible so this will condition the impact of public rents on the likelihood of violence. This implies that while an increase in public rents has a positive impact on the resources invested in violence, this impact decreases as localities have higher levels of state capacity. Hence the model makes two distinct empirical predictions. It predicts a positive effect of public rents on violence and an opposite negative effect of public rents interacted with local state capacity.

I test these predictions using subnational-level data from Colombia during the 1990s, decade in which a series of decentralizing reforms were sequentially implemented in the midst of an escalating conflict between left-wing guerrillas, right-wing paramilitaries, and the central government. My identification strategy uses a constitutional reform in 1993 that increased the fiscal resources transferred automatically to the municipalities and modified as well the formula used to distribute these resources. This reform provides a plausible exogenous source of variation in the amount of public resources available to municipalities and is ideal to test my predictions for various reasons. First, the reform was designed and administered by national agencies so there was little room for manipulation by localities in the allocation of new fiscal resources. Second, transfers were based on a few socio-demographic characteristics and were not allocated selectively to municipalities based on their levels of violence. Lastly, the decentralization process did not include a defense component, so the responsibility for security remained centralized in the hands of the National Police and the National Army. This means that the effect of the fiscal reform introduced can be isolated from other national-level policies aimed at decreasing conflict.

Using data on selective political murders (i.e., murders of elected officials, candidates, politicians and local leaders) during the 1990s, I find that fiscal transfers from the central government to municipalities are positively associated with this form of violence. Moreover, the impact of transfers and its interaction with various measures of state capacity have the

opposite sign and are both statistically significant. For instance, the evidence demonstrates how the presence of local state agencies (e.g., police stations, courts, and notaries) can offset the positive effect of fiscal transfers on violence. This means that the way in which the fiscal reform influenced the conflict between the various armed actors and the local authorities depended critically on the capacity of local governments. These findings are consistent with the motivating theory and robust to a variety of specifications, including an empirical strategy which uses historical measures of colonial state development as proxies for modern state capacity.

The estimated relationship between transfers from the central government and political violence is interpreted as an increase in the level of coercion of local governments during the post-reform period. This exercise of political influence through violence is what some scholars have labeled as “armed clientelism,” which is simply “the private appropriation of public goods through violence or the threat of violence” (Eaton 2010, p. 535). In the Colombian context, this form of clientelism is related to a shared incentive non-state armed actors have to control economic resources, shape policies, and consolidate their power locally. Thus, the effect of the fiscal reform of the early 1990s on the subsequent levels of political violence is indicative of a perverse dynamic in which local governments became more vulnerable as they became more autonomous and funded.

By focusing on one specific reform in one specific country, I am able to identify a specific dimension of decentralization which is relevant for conflict. This is important since fiscal, administrative and political reforms are often implemented simultaneously, making the identification of a particular mechanism challenging. Another advantage of using subnational variation is that I am able to hold constant many unobserved factors which potentially influence both the level of decentralization and conflict across countries. Lastly, the fiscal resources involved were very substantial (14% of total central government revenue in 1993) and were part of a national scale reform. Thus this paper complements recent studies estimating the political impact of lower-scale development programs in other conflict environments (e.g., Crost, Felter and Johnson 2014) and foreign aid (e.g., Nielsen *et al.*, 2011; Nunn and Quian 2013).

Another contribution of this paper is to provide quantitative evidence on the mixed consequences of decentralization. In Colombia, this process is associated with positive economic outcomes, in particular with improvements in public education (Faguet and Sanchez 2008). Yet, the evidence presented suggests that improvements in social indicators are no guarantee of improvements in security. This is consistent with recent empirical studies looking at the impact of external resources from development programs on conflict in other countries such

as the Philippines (Crost, Felter and Johnson 2014).³

In addition to the works cited above, this paper is related to other literatures. First, several studies have explored the relationship between political decentralization, ethnic conflict, and secessionism. Although there is an ongoing empirical debate, many of these studies agree on the benefits of some form of decentralization arguing that a deepening of local democracy reduces conflict by bringing citizens closer to their representatives and by giving regional parties the opportunity to influence national-level politics (see e.g., Lijphart 1977; Horowitz, 1985; Diamond and Tsalik 1999). Perhaps closer to my findings are works showing that in some cases decentralization may actually promote conflict (e.g., Leff 1999; Bunce 1999; Snyder 2000; Brancati 2006), or may have a detrimental effect on the quality of democracy and public services (e.g., Fan, Lin and Treisman 2009; Machado 2013).⁴ In the Colombian case, recent empirical studies (Acemoglu *et al.*, 2013, Gallego 2016), explore how the activity of armed actors has influenced local political competition, turnout, and some national-level policies. None of these works study the relationship between fiscal decentralization and political violence.⁵

This paper also contributes to a large economics literature focusing on the relationship between decentralization and public services (e.g., Besley and Coate 2003; Faguet 2004) and on the political economy of local governance more generally (e.g., Enikolopov and Zhuravskaya 2007). While some of these studies have explored how under decentralization local elites could capture subnational governments more easily, they focus on more “traditional” mechanisms, such as lack of political competition and corruption (e.g., Bardhan and Mookherjee, 2000; Bardhan 2002), assuming a full monopoly of violence by the central state. Thus, the capture of democratically elected local governments by force remains largely unexplored both in the political economy and public economics literature.

³Yet, the mechanism suggested by Crost *et al.*, (2013) is different. They find that an important development program lead to an increase in insurgent-initiated violence, particularly during the early stages of the program. Hence they conclude insurgencies sabotaged the program anticipating its success could weaken their support in the population.

⁴There is also a political economy literature exploring the negative effects of federalism on macroeconomic outcomes (e.g., inflation) and on the quality of fiscal policies (see e.g., Rodden 2002).

⁵Sanchez and Chacón (2006) explores descriptively the determinants of armed conflict in Colombia during the 1974-2002 period and find that transfers are positively correlated with the expansion of some armed groups. The evidence presented in Section 6 shows that this correlation is not robust to the inclusion of municipality and time fixed-effects. The focus on the impact of the 1993 reform on selective political violence during the 1990s is exclusive to this paper.

Theoretical Framework

In this section, I present a simple model to help interpret the empirical findings. The model is in the spirit of the formal conflict and rent-seeking literature which is based on the canonical “contest model” (e.g., Hirshleifer 1989, Skaperdas 1992) originated with Tullock (1975). In the model, various armed groups compete for a public rent and exogenous factors such as the technology of violence will determine the resources invested in the struggle. I make a simple extension to the standard model to allow for the possibility a “draw” in the sense that a local government may resist the violence and keep its resources. This framework highlights how an exogenous change in the value of the rent influences the incentives to fight and how the strength of local authorities conditions this influence.

Consider $N \geq 2$ non-state armed groups competing for some public rents of value R^l in locality l . These actors simultaneously invest an amount e_j ($j = 1, 2, \dots, N$) in violence towards local officials in the hope of capturing the public resource. Competition between groups is “imperfectly discriminatory” in the sense that the highest fighting effort may lead to the highest probability of capture but it does not necessarily secure capture. Following Tullock’s original formulation, the success of each group is proportional to the ratio of resources invested in violence. Specifically, given a vector of efforts $e = (e_1, e_2, \dots, e_N)$, the probability of j ’s victory takes the logit form

$$p_j(e) = \begin{cases} e_j^{m^l} / \left(\sum_{i=1}^N e_i^{m^l} \right) & \text{if } \sum_{i=1}^N e_i^{m^l} > 0 \\ 1/N & \text{otherwise} \end{cases}, \quad (1)$$

where $m^l > 0$ represents the effectiveness of violence in l .⁶ To simplify the analysis suppose $m^l \leq N/(N-1)$ so there could be some degree of increasing returns in the conflict technology but there is a limit to these.

In addition to the fighting technology, other locality-specific factors could influence the marginal expected returns of fighting expenditures. In particular, the institutional characteristics of each locality could make harder (or easier) for armed actors to influence the political system (e.g., the level of state capacity, the strength of civic society, or the presence of local media). These characteristics are modeled in a reduced-form manner by a parameter $\phi^l \in [0, 1]$ which represents the likelihood that a locality successfully resists the capture. This means that even if all groups make positive fighting efforts there is still a chance that none of them will be able to appropriate the public resources of the locality.⁷ If groups fail

⁶The conflict literature has emphasized factors such as skillful rebel leaders, the type of military technology of armed groups and other exogenous characteristics (e.g., rugged terrain), as important determinants of this fighting effectiveness (Blattman and Miguel 2010).

⁷The behavior of local (and national) authorities is therefore non-strategic. Although localities could

to capture the locality they all get zero and lose their fighting expenditures. Assuming that actors are risk neutral, the expected payoff of j is then given by

$$\mathcal{W}^j(e_j, e_{-j}) = (1 - \phi^l) \left(\frac{e_j^{m^l}}{e_j^{m^l} + \sum_{i \neq j} e_i^{m^l}} \right) R^l - e_j, \quad (2)$$

where $e_{-j} = (e_1, \dots, e_{j-1}, e_{j+1}, \dots, e_N)$.

As it is well known, the unique pure-strategy Nash equilibrium of this setting is symmetric and involves all groups investing a positive amount in fighting (see Appendix for all proofs).⁸ The equilibrium investments depends crucially on the value of the rent, the fighting technology effectiveness and on the feasibility of capture. Higher returns in the conflict technology will lead naturally to higher fighting expenditures. Similarly, an increase in R^l will lead to an increase in the level of violence by a typical “greed effect” arising from having a higher prize in the locality; the more public resources the locality has, the more are groups willing to spend to obtain it. This is the first and most basic testable prediction explored in the data.

Furthermore, the strength of the local government conditions the effect of R^l and m^l on the level of violence. In particular, higher values of ϕ^l will decrease the magnitude of the greed effect induced by an increase in the public rent. In other words

$$\frac{\partial e_j^*}{\partial R^l} > 0 \text{ and } \frac{\partial^2 e_j^*}{\partial \phi^l \partial R^l} < 0,$$

where e_j^* is the equilibrium level of investment for any group j .

The intuition for the second comparative static result is straightforward. When the locality is more able to resist the attack of armed groups, the marginal effect of rents on equilibrium investments decreases since each group will have a lower probability of capturing the public rent. Hence, groups will focus their efforts in “weak” municipalities (having low values of ϕ) given an exogenous increase in R . The immediate implication is that in municipalities where the government is strong (weak), the amount of fiscal resources available should have a smaller (larger) impact on the levels of political violence. This conditional effect of fiscal rents is the second empirical prediction explored by interacting fiscal transfers

and do indeed allocate their defense expenditures strategically, the particular fiscal reform studied did not include a defense component and was introduced by the national government unilaterally. For these reasons local governments are not active players in the game.

⁸In this model, peace, defined as having zero fighting investments by all players, is never an equilibrium. This is because there is a strong incentive to invest a minimal amount when others are not (the probability of winning in this case jumps discontinuously from $1/N$ to 1).

to the municipalities with various proxies of local state capacity.⁹

In summary, this framework generates two testable predictions. First, as a result of making local governments more valuable, an increase in the amount of fiscal resources transferred by the central government to the municipalities should increase the violence against local officials. The intuition is that fiscal transfers generate a local “greed” effect among the different armed groups active in each municipality. Second, this effect should be lower in municipalities with “stronger” local states which are better equipped to deal with challenges from non-state actors. In such municipalities, the expected return of targeted political violence is lower hence armed actors are less likely to control the public administration and focus instead in capturing the resources of “weaker” municipalities. Therefore, this simple model predicts that the effect of fiscal decentralization on the levels of political violence depends crucially on the capacity and strength of local governments.

Background

Timing of Reforms

The decentralization process in Colombia started in the mid-1980s and was part of a wave of political and economic liberalization in the region. The devolution of power was gradual and involved mayor political reforms. First, a constitutional amendment in 1986 introduced for the first time the popular elections of mayors from 1988 onwards.¹⁰ This process was deepened with a new constitution in 1991 which introduced the direct election of the departmental governors and increased the tax base and rate of fiscal resources transferred to municipal governments.¹¹ Specifically, the 1991 Constitution stipulated that gradually half of total national revenues had to be directly and automatically transferred to the subnational governments and that the bulk of these resources had to be spent on education and health services. The main administrative responsibilities of municipalities in the new system were established in 1993 (Law 60). In particular, municipal governments were endowed with the responsibility of administering the public education system, primary healthcare, water and sanitation services. Importantly, these responsibilities were introduced uniformly and independently of the pre-existing strength of local governments.

⁹If instead of the ratio in efforts, p_j is a function of the difference in efforts between groups, peace could be an equilibrium (see Hirschleifer 1989). In such model, similar comparative static results to the ones discussed will hold in the sense that an increase in $R(\phi)$ would make peace less (more) likely.

¹⁰Before 1988, mayors were appointed by departmental governors which in turned were appointed directly by the president.

¹¹For an account of how the popular election of governors was bargained in the Constitutional Assembly see Falletti (2010; 135-139).

Fiscal Reform

Although the 1991 Constitution gave new responsibilities and resources to the municipalities, the distribution of these resources was not regulated. Thus, new sources of funding for municipalities were created (e.g., the *Sistema General de Participaciones* (SGP) in Article 356), with the specific goal of improving health, education and social services, but no exact guidelines about how these should be assigned or expended was put in place. This meant that the fiscal transfers to the regions and municipalities continued to follow the rule of Law 12 of 1986 which was based almost entirely on population size.¹² The general principle laid out in the Constitution was that the new fiscal transfers system had to address the severe problems of inequality and poverty of the country.

As a result, Law 60 of 1993 (known as the *Law of Responsibilities and Resources* and implemented in 1994) defined a new intergovernmental system of transfers and set the fiscal restrictions of local governments. For example, according to this law, municipalities were mandated to expend all transfers from the central government on the following categories: education (30%), health (25%), water and basic sanitation (20%), discretionary investment (20%), and sports and culture (5%). This law also stipulated a gradual increase in the total amount allocated to the municipalities from 14 per cent of total national fiscal revenues in 1993 to a maximum of 22 per cent in 2001. Figure 1 presents the evolution of fiscal transfers from the center to all the *entidades territoriales* (i.e., municipal and regional governments) as a percentage of (nominal) GDP for the 19860-2002 period. As seen, top-down transfers increased dramatically during this period. In particular, direct transfers to the municipalities increased from an average of less than 1 per cent in the late 1980s to more than 3 percent of GDP in 2002.¹³

Law 60 provided not only an important increase in the public resources decentralized but also introduced a key change in the distribution of top-down transfers. Specifically, the law modified the transfer formula and gave, on average, more resources per capita to poor municipalities. For this purpose, both the number of poor people and the overall relative poverty level of the municipality were included as weights in the new formula (these two variables determined approximately 60% of the “participation” of each municipality in the SPG system until 2001). This was a substantial change since before 1993 central

¹²This law was the first to set restrictions on the discretionality of municipal expenditures, which were founded primarily from the national sales tax. Law 12 also increased gradually the share transferred from 30% of the total national sales tax revenue to 50% in 1992.

¹³The rate of transfers to the regional (departmental) governments was also increased by Law 60. These were set to increase from 22.5 per cent of the total national revenue in 1993 to 24.5 per cent in 1996-2001. As a consequence, automatic transfers to lower-level governments reached more than 38 per cent of total national fiscal revenues in 2000 (Baron and Meisel 2003 and DNP).

government's transfers were based almost exclusively on population size according to Law 12, 1986.¹⁴ This change in the fiscal system is depicted in Figure 2 which shows the evolution of real transfers per capita for municipalities below and above the pre-reform national 75th poverty percentile. As seen, in the pre-reform period, both the trend and mean size of transfers per capita of relatively rich and relatively poor municipalities are identical. Yet the transfers going to the poorest municipalities increased comparatively faster in the post-1993 period. The immediate consequence of this law was then a substantial redistribution of fiscal resources from big, relatively rich, to small, relatively poor, municipalities.

Therefore, an important source of cross-sectional variation in the fiscal resources received by municipalities during the period comes from this specific change in the transferring system used. This is an ideal testing ground for the predictions outlined since this change in the allocation of public resources was exogenous to the local activity of armed groups. More importantly, the process of decentralization did not include a defense component and the responsibility for local security remained centralized in the hands of the National Police and the National Army, both of which fall under the authority of the president. Hence, the new transfer rule introduced in 1993 did not allocate fiscal resources selectively to municipalities based on their (pre-reform) levels of violence, armed groups presence, or potential security threats.

The Rise of Armed Clientelism

Surprisingly, as the Colombian state became less centralized, local elections were introduced, and local governments received more funding, the conflict erupted (Sánchez and Chacón 2006). In particular, the first local elections were accompanied by very high levels of violence against local officials and politicians.¹⁵ While this violence may be partly explained by the emergence of left-wing parties, which were accused of being political instruments of left-wing insurgent groups, and paramilitary organizations, the case study literature on the political economy of the conflict suggests that some of the reforms implemented provided a political environment conducive to the increase in violence (e.g., Rangel 1997; Peñate 1999; Restrepo 2003).

¹⁴Article 20, Law 20, stated that 25.8% of the annual sale tax revenue, one of the main sources of public revenue, had to be distributed to municipalities and to the district of Bogotá D.C. "in proportion to the population of each of these territorial entities" (own translation).

¹⁵For instance, during the mayoral elections of 1988, there was a wave of violence against the communist-based party *Union Patriótica* (UP), which was funded by former FARC members in the early 1980s. This party won 16 mayoral elections and obtained more than 250 seats in local councils across the country in 1988. During the first rounds of local elections (1988, 1990 and 1992) it is believed that as many as 3,000 UP leaders and supporters were assassinated by paramilitaries and right-wing squads (Dudley 2003).

The conventional interpretation of how decentralization deteriorated the security of municipalities is that both left-wing guerrillas and right-wing paramilitaries used violence in their attempt to capture new economic and political resources and this way strengthen their local control.¹⁶ Fiscal decentralization allowed these non-state actors to influence directly the expenditure decisions of municipalities, administrative autonomy facilitated the coercion of officials, and the introduction of local elections gave these actors the opportunity to manipulate the electoral process and run their own candidates (Eaton 2010). This form of local capture is what some scholars have defined as “armed clientelism,” which is simply “the private appropriation of public goods through violence or the threat of violence.” (Eaton, 2010, 535). For example, describing this process Rangel (2004, 245) argues that

“Thanks to armed clientelism, paramilitaries influenced the planning and execution of the local budgets. In addition, in many municipalities they are involved in the selection of the local bureaucracies. In this process, armed actors exercise pressure over authorities so that they fill vacancies with their followers and sympathizers. In other cases, the influence takes place in the selection of potential contractors of the locality.”

In addition, the case study evidence suggests that the strategy of armed clientelism was particularly common among right-wing paramilitary groups. During the decade studied, these organizations based their military strategy against guerrillas on the capture of local governments in regions of the country where guerrillas had a strong presence (Romero 2002). In many cases, paramilitary groups used this local control not only to combat guerrillas but also to appropriate public resources (Alvaro 2009). The increased influence of paramilitaries in municipal governments is also consistent with the electoral manipulation and corruption exercised during the late 1990s and early 2000s by the Autodefensas Unidas de Colombia (AUC), an umbrella organization of different paramilitary groups and drug lords formed in 1997. Recent studies show that the AUC had a determinant influence over the political process of some regions and in many cases majors, municipal council members, and even governors had family ties and close links with paramilitaries (see e.g., Lopez 2010 and Acemoglu *et al.*, 2013).

¹⁶Some studies even argue that decentralization promoted the expansion of armed groups into new territories (e.g., Sanchez and Chacón 2006; Eaton 2010).

Data and Empirical Strategy

Data

The main explanatory variable is the annual per capita fiscal transfers from Colombia's central government to the municipal governments. The National Planning Department (DNP) records this information for every fiscal year since 1984. I focus the analysis on the years 1988 through 2001, and deflate the series using constant pesos of 2000.¹⁷ As explained, these funds are earmarked for education, health, water and sanitation services. A small percentage was also denominated as a "discretionary investment" fund which is used to finance small development projects proposed by the community and the municipal council (e.g., roads and public facilities). According to a fixed formula, administered by the DNP and based mainly on population and poverty, these amounts are transferred automatically by the Ministry of Finance (*Hacienda*) to municipalities in several installments during each fiscal year.

The second important source of information quantifies the activity of non-state armed organizations according to national police records, the central government, and the main newspapers. This information is compiled by the Observatory of Human Rights and International Humanitarian Law of the Vice-presidency and contains many categories of events.¹⁸ The analysis focuses on homicidal violence against popularly elected officials, local politicians, leaders, and public employees, which can be unambiguously attributed to non-state actors. The main dependent variable includes murders of elected officials (mayors, council and JAL members, deputies, candidates, and ex-politicians), and non-elected municipal officers and public officials (e.g., judges, other justice officials, correctional officers, public teachers, among others).¹⁹ According to the Observatory, between 1988 and 2001, a total of 186 mayors, 651 council and deputies members, 291 politicians, 214 local leaders, and 550 state officials were murdered by armed groups.

To preview the results, Figure 3 shows the evolution of political murders during the sample period grouping municipalities by their level of pre-reform poverty (using the 1985 poverty index). As seen, there is a general downward trend in these murders during the period, a fact which could be partly explained by the very high levels of political violence during the first rounds of local elections at the beginning of the sample period.²⁰ As mentioned, this violence was mostly targeted toward new left-wing parties. Interestingly, political violence

¹⁷There was a major reform to the transfers system in 2001, implemented in 2002, which modified the funds received directly by municipalities. Hence I limit the analysis to the pre-2002 period.

¹⁸<http://www.derechoshumanos.gov.co/Observatorio/Paginas/Observatorio.aspx>

¹⁹These categories exclude police and army casualties which are analyzed separately.

²⁰Local and regional elections occurred in 1988, 1990, 1992, 1994, 1997 and 2000.

was on average *lower* in the relatively poor municipalities during the pre-1993 years.²¹ More importantly, the gap in political murders between relatively rich and relatively poor municipalities vanished at the end of the reform period. As seen, this because the downward trend is steeper for the relatively rich municipalities. In the next section, I explore the statistical significance of these differences assuming that in the absence of the reform the pre-reform trends across rich and poor municipalities would have remained constant.

To test the prediction that the political impact of transfers should depend on local state capacity, I use a set of indicators based on the number of state agencies and their employees registered in each municipality. Namely, I use two variables: (i) the total number of local public employees (“funcionarios municipales”), and ii) the total number of state agencies in the main urban center (“cabecera”) of the municipality, which includes notaries, postal offices, police stations, jails, judicial offices, and tax offices, among others. These variables are taken from Fundacion Social (1998), and are available only for 1995.²² Since these measures are from 1995, after the main decentralizing reforms were already implemented, they could be partly explained by the reaction of municipalities to the new political system. Hence, the interaction of transfers with local state presence in the 1990s contains a potentially endogenous variable. To address this, I use two additional measures based on the historical development of the Colombian state. Namely, based on Acemoglu, Garcia-Jimeno and Robinson (2015), I use the presence of colonial offices and officials in the late 18th century as proxy for modern state capacity. These data was originally compiled by Duran y Diaz (1794), and contains the total number of crown officials and several state agencies throughout the Viceroyalty of Nueva Granada.²³ As Acemoglu *et al.* (2015) demonstrate, these historical measures are correlated with modern capacity and provide a potential exogenous source of variation in local state capacity.

In various specification tests, I analyze the impact of fiscal transfers on acts of violence against civilians (e.g., massacres), and acts of indiscriminate violence, which include terrorist attacks with explosive devices and attacks on the infrastructure of the municipality (e.g., the electric grid or an oil pipeline). To explore a potential effect of transfers on the military strategy and the presence of armed groups across time and space, I use armed clashes between the military and the guerrillas, ambushes, and military casualties as proxies for changes in

²¹This provides further proof that the 1993 reform did not give resource disproportionately to municipalities having already higher levels of violence.

²²My empirical approach is to interact these time-invariant measures with lagged transfers (in real per capita terms). This is preferable than using a contemporaneous time-varying proxy of state capacity since capacity is likely endogenous to the conflict and correlated with the main determinants of transfers (e.g., poverty).

²³Specifically, the measure used includes the agencies in charge of administering: the *alcabala* (sales tax), the tobacco, liquor, cards and gunpowder *estancos* (state monopolies), and post offices.

the presence and territorial expansion of armed groups. See Appendix B for a detailed description of these variables and their sources.

My theoretical model assumes multiple armed actors in each locality. During the 1990s, the Colombian conflict is better characterized as two-sided battle, with guerrillas on one side and a conjunction of paramilitaries and government forces on the other. Naturally, many municipalities were free from the conflict, especially in regions around the country’s main urban centers. To capture this source of heterogeneity, I use the armed group presence indicators from the Conflict Analysis Resource Center (CERAC). These yearly (dummy) indicators of guerrilla and paramilitary presence are based on unilateral actions and episodes of combats between these groups. Even though these events are based also on newspaper reports (from 25 different sources), they are produced independently from the events recorded by the Observatory and supplemented by reports from a national network of Catholic priests. Thus this data offers an added check on the soundness of the Observatory data.

Lastly, the main control variables used are the factors employed in the transfer rule, namely population (from the National Administrative Department of Statistics, DANE), and poverty rate measured by the NBI index of “unsatisfied basic needs” from the Centro de Estudios sobre Desarrollo Economico (CEDE) at the Universidad de los Andes. In addition, I include interactions between time dummies and various time-invariant characteristics which could condition the “technology of conflict” (i.e., influence m^l) in each municipality. These include altitude, area, and distance from the departmental capital, all from the CEDE. Table 1 presents summary statistics for the main variables used in the analysis.

Empirical Strategy

My empirical approach is based on a simple difference-in-differences model where changes in the level of central government’s transfers have an impact on violence that depends on some (time-invariant) characteristics of the municipality. Ideally, the analysis would contain a “first stage” to purge the transfer series from any potential source of endogeneity. Yet, the exclusion restriction for any of the determinants of the transfer rule during the period (e.g., population, poverty or a post-reform dummy) would not be valid since these could have an independent impact on the conflict. Instead, I follow a control-based approach which controls flexibly for the characteristics determining the transfers and a set of differential time trends.

Specifically, I employ a model of the following form:

$$y_{it} = \alpha_i + \lambda_t + \gamma r_{it-1} + \delta(w_i \times r_{it-1}) + \mathbf{x}'_{it-1}\boldsymbol{\beta} + \varepsilon_{it}, \quad (3)$$

where y_{it} denotes violence in municipality i at time t ; α_i is a municipality fixed effect and

λ_t is a year fixed effect that captures the national trend of the conflict. r_{it} is the log value of per capita central transfers and w_i is a time-invariant measure of state capacity. Hence, the coefficients γ and δ capture the main predictions of the theoretical model. \mathbf{x}_{it-1} is a vector of control variables which includes log population, the poverty index (NBI), a set of interactions between various time-invariant geographic factors and year dummies, and a full set of departmental fixed effects interacted with year dummies as well. Lastly, ε_{it} is an error term capturing all other unobserved factors influencing conflict.²⁴

To check the robustness of model (3) I also estimate a model taking two data points, one before the 1993 reform and one years after. Specifically, I estimate a series of long-difference models of the form:

$$\Delta y_i = d_{j(i)} + \gamma \Delta r_i + \delta(w_i \times \Delta r_i) + \Delta \mathbf{x}_i \beta + v_i. \quad (4)$$

In this model, $d_{j(i)}$ is a department fixed effect and Δy_i is the change in political violence (or other conflict outcome) in municipality i , located in department j , between the pre-reform period (years 1988-1991) and the post-reform period (years 1998-2001). Similarly, Δr_i is the real growth rate of transfers between these two periods. w_i is the same time-invariant measure of state capacity, $\Delta \mathbf{x}_i$ represents the change in the vector of covariates, and v_i an error term representing all omitted factors.

Model (4) uses the medium-run variation in the data while keeping a close resemblance to the panel model (3)—in effect, this specification is algebraically equivalent to a panel model with only two periods and a full set of municipality and time effects.²⁵

The key assumption of the empirical approach is that conditioning on all time-invariant characteristics, the key time-varying variables of the transfer rule, and various differential time trends, the transfers received by the municipalities in a given year is uncorrelated to all other time-varying omitted factors that may influence local conflict. While this assumption cannot be attested, is plausible for several reasons. First, transfers are pre-determined by a fixed formula administered by the National Planning Department and beyond control of municipal governments. Second, the main source of temporal and cross-sectional variation in the transfers comes from the 1993 reform which did not give resources selectively to municipalities. Lastly, the administrative and fiscal reforms implemented during the sample period did not coincide with similar reforms in other state institutions that could have a direct impact on the conflict (e.g., reform to the judicial system).

²⁴Because the potential effect of transfers depends on local characteristics standard errors are clustered at the municipal level.

²⁵Although this specification ignores the annual variation in the data, is possibly more robust since the year-to-year variation in the violence reported could be explained in part by measurement error.

Econometric Results

Tables 2 present the estimates using the annual number of political homicides as the dependent variable. Thus, these represent the effect of fiscal transfers on selective political violence, conditional on local state capacity. The standard errors reported are robust to arbitrary heteroskedasticity and are clustered at the municipal level. Columns 1-4 use the total number of state agencies and columns 5-8 the total number of municipal-level public employees as proxy for state capacity, respectively. In all models I control for (ln) population and year effects (when possible).

In line with the theoretical predictions, the level of per capita transfers and its interaction with each measure of state capacity have the expected opposite signs. For example, the point estimate of 0.501 (s.e. = 0.188) for γ in column 1 represents the effect of transfers on violence in a municipality with no state agencies. The magnitude of this estimate implies that holding all other factors constant, a one standard deviation increase in central transfers increases the number of political murders in the next year by around 0.36. For the median municipality in the sample (having 18 state agencies), the marginal effect significantly decreases to approximately 0.04 ($0.501 - 0.16 * \ln(18) = 0.039$). For municipalities having a number of agencies one-standard deviation above the national mean, this marginal impact becomes negative ($0.501 - 0.16 * \ln(225) = -0.36$).²⁶ Columns 2-4 explore the robustness of these results. In column 2, instead of controlling for the contemporaneous (lagged) poverty rate the model includes a set of time effects interacted with the pre-reform (1990) level of poverty. The model in column 3 includes in addition a set of differential time trends based on key time-invariant characteristics of municipalities (e.g., altitude and distance to capital), and the one in column 4 a full set of department-specific time trends.²⁷ As seen, the inclusion of these controls has a negligible effect on the estimates and their statistical significance.

Columns 5-8 present the same specifications but use the number of municipal-level public employees as a proxy for w_i . These models yield almost identical results. For instance, the estimates of column 5 imply that in a municipality with 20 public employees (median number in the sample), a one point increase in transfers per capita leads to an increase of approximately 0.04 in the number of political murders. Relative to the sample mean, this effect represents an increase in murders of more than 25 percent. For a municipality having 950 public employees (one-standard deviation above the national mean), this effect becomes negative ($0.196 - 0.051 * \ln(950) = -0.15$). These estimates are somewhat less precise when

²⁶This negative effect is valid for approximately 25% of the municipalities in the sample. The impact of transfers changes sign at $\exp(\hat{\gamma}/\hat{\delta}) \approx 23$ agencies (75% percentile is equal to 26).

²⁷This model drops the year effects (i.e., the global trend) since these are perfectly collinear with the department-specific trends.

we control for the different trends (columns 6-8) but overall consistent and robust. For example, in the model containing all the differential trends (column 8), a one point increase in transfers has a positive impact on future homicides of 0.03 for the median municipality.

Table 3 presents long-difference estimates for γ and δ from model (2), which includes a full set of department fixed effects. Using the number of state agencies, the estimate of γ of 0.822 (s.e. = 0.292) in column 1 implies that for the median municipality (having 18 agencies) the impact of transfer is approximately zero. For a municipality having a total number of agencies one standard deviation above the national mean, the impact of transfers is equal to -0.738. The specification checks in columns 2-4 are analogous to ones presented in Table 2 and the results are similar. Lastly, columns 5-8 use the interaction of municipal-level public employees and transfers. The estimate of γ in these models is highly significant and implies a large effect. For instance, the estimate of 0.288 (s.e. = 0.086) in column 8 implies that a municipality having a state capacity one standard deviation below the national mean and the mean increase in transfers (126%) will have an increase in political homicides of approximately 0.12. This effect represents more than 100% of the mean (absolute) change in political violence over the period.

Overall, the panel and long-difference specifications are robust and consistent with the motivating theory. This theory predicts that in a situation of civil war, an exogenous increase in the public resources of subnational governments can have a detrimental impact on local security and that this depends crucially on the local capacity of these. As shown, both the fiscal transfers to the municipalities and their interaction with various measures of local state capacity have the predicted opposite signs and are statistically significant at conventional levels.

Robustness

Historical State Capacity

The models in Table 4 address the potential endogeneity of the state capacity measures by using a set of historical state presence variables which are positively associated with modern state capacity. Namely, I use the number of colonial agencies and the number of colonial officials in the late 18th century directly as proxy for the pre-reform state capacity of municipalities. These variables are historically predetermined and potentially more exogenous than the measures available from the mid-1990s.²⁸

²⁸Acemoglu et al. (2015) use these variables to estimate the impact of local and neighboring state capacity on modern development in Colombia.

Using these historical variables, the panel estimates for γ and δ are less precise but quantitatively similar (long-difference estimates are presented in the Online Appendix, Table 1A). For example, the estimate of γ in column 1 (0.081, s.e. = 0.024) implies that in a municipality with no colonial agencies (60% of the municipalities in the sample), a one point increase in lagged transfers leads to an increase in the number of murders of 0.08. For municipalities having one agency (23% of the sample), the magnitude of this effect is reduced by almost 50 percent and for towns having two or more agencies (12.5% of the sample) the effect becomes negative.²⁹ The interaction term of interest is not significant at standard levels when controlling for the differential trends (columns 3-4). Yet, in the models using the number of colonial officials (columns 5-8), the interaction of interest has the predicted sign and is significant at the 5% and 10% level.

Armed Group Presence

The motivating theory of Section 2 assumes the presence of at least two armed actors in each locality. In the 1990s, the local presence of armed groups in Colombia differed across regions and departments. Many regions of the country were free from nonstate actors and under the control of the central government. Other municipalities were controlled by a single nonstate actor (either a guerrilla or a paramilitary group), and some others were contested between the state and a single nonstate actor or by two non-state actors.³⁰ Hence, the local presence and activity of different factions is an importance source of variation in the potential effects of transfers on violence. Naturally, the theorized effects are only valid for places where multiple armed groups were active so in municipalities where the state has a monopoly of violence we should observe no relationship between fiscal transfers and selective political violence.

To explore the validity of the model with respect to this heterogeneity, I use the yearly conflict indicators of CERAC to capture the presence of different armed groups during the period. Using these indicators, I perform a simple sensitivity analysis test splitting the sample between municipalities having low and high armed group presence based on the number of years in which guerrilla and paramilitary groups are reported as being jointly present in each locality. Specifically, municipalities are classified as having a low (high) armed group presence if the number of years each group is registered as present is below (above) the group-specific mean over the period.³¹ According to this taxonomy, during the

²⁹In the sample, only 43 municipalities (around 4% of the sample) have more than 2 colonial agencies.

³⁰The main paramilitary organization, the AUC, was demobilized between 2003-2006. The FARC, the main guerrilla group, began a peace process in 2012 and demobilized collectively in 2016-2017.

³¹During the 14-year sample period, guerrillas are registered on average 3.5 years (s.d.=3.37) and paramil-

sample period, 477 municipalities had low armed groups presence while in 258 there was a high presence of both paramilitaries and guerrilla groups.

Table 4 presents this sensitivity analysis. Each two columns corresponds to the specifications and controls reported in previous tables. As expected, in municipalities reporting a low presence of armed actors (columns 1-2 and 5-6), the conditional impact of transfers is significantly smaller compared to the effect found in municipalities with a high presence (columns 3-4 and 7-8). For instance, the estimates in column 2 imply in municipalities with low presence and median state capacity a one point increase in transfers, holding all else constant, has an effect on violence close to zero ($0.119 - 0.04 * \ln(18) = 0.003$). In the sample of municipalities reporting high presence (column 4), this effect increases to 0.205 ($1.115 - 0.315 * \ln(18)$), which represents a six-fold increase compared to national effect estimated previously (column 4, Table 2). The models using the number of public employees (columns 5-8) yield almost identical results.

In sum, these results are consistent with the idea that the conditional association between fiscal transfers and political violence is only valid for municipalities in dispute by multiple non-state actors. Namely, the impact of central transfers conditional on local state capacity in the high presence municipalities is significantly greater compared to one obtained in municipalities with low presence. This provides a “reality check” to the previous estimations and validates one key assumption of the motivating theory.

Transfers and Other Conflict Outcomes

To verify that the estimated effect of municipal transfers on political violence is not explained by a trend in the conflict not modeled properly (the 1990s was a decade in which the violence of armed actors exhibited a strong upward trend, see for example Dube and Vargas 2013), in Table 6 I explore the relationship between transfers and a variety of other conflict-related outcomes. I divide these outcomes in: acts of violence against civilians (columns 1-2), outcomes related to the military confrontation between the National Army and the guerrillas (columns 3-5),³² and terrorist attacks (columns 6-7). All models include the same set of differential trends and controls included previously in the panel specifications.

Columns 1 and 2 show that transfers per capita have no statistically significant effect on the number of civilian homicides by armed groups and the number of civilian massacres respectively. Similarly, transfers have no significant impact on the number of military casualties (column 3) or on the number of military confrontations between the National Army

ities 0.73 years (s.d.= 1.26).

³²In the CEDE dataset there is not a single military confrontation between the Army and paramilitary groups. Similarly, there are no records of confrontations between guerrillas and paramilitaries.

and the guerrillas (column 4).³³ This impact is marginally significant and positive for cases of guerrilla ambushes but the interaction term with local state capacity is not statistically significant different from zero (column 5). Lastly, there is no significant relationship between transfers or its interaction with state capacity in the model explaining the number of explosive artifacts (column 6). The estimates for the model explaining the attacks on the infrastructure of the municipalities (e.g., bridges and government buildings) have the same sign as in the model of political violence, yet these are only significant at the 10% level. The results for the models taking instead the public employees measure are presented in the Online Appendix, Table 2A.

Why Decentralize?

Given the negative impact that the fiscal reform of 1993 had on the subsequent levels of political violence, and the failure of decentralization to stop the conflict more generally, the natural questions are: What were the causes of the decentralization? Why did the elites in control of the national government voluntarily agreed to empower lower levels of government? Were the negative consequences of decentralization unintended?

In this section I briefly present some qualitative evidence addressing these issues. The case study evidence on the historical context of this process generally agrees on the underlying principle of these reforms, which was that local democracy would lead to an increase in efficiency and transparency of public expenditures by strengthening accountability of local representatives (e.g., Alesina, Carrasquilla, and Echevarria 2002).³⁴ Similarly, although the political economy of the fiscal decentralization process implemented in the early 1990s is largely unexplored, some case study works mention the great need to improve the social conditions of poor municipalities, and the incentives to redistribute the fiscal revenues of big cities, as the main factors explaining this reform (see e.g., Angell *et al.*, 2001).³⁵

Hence it would appear that the negative consequences of fiscal decentralization on the dynamics of violence were indeed unintended. Interestingly, some of the main political figures at the time who were part of the Constitutional Assembly of 1991 argued that decentralization was the best way to pacify the country. As Horacio Serpa, leader of the Liberal Party

³³These results may suggest that although the increase in fiscal transfers had a local impact on the levels of political violence, it did not had an “expansion” effect in the sense that it did not lead to a territorial expansion of armed groups.

³⁴A different interpretation of the political decentralization process is given by Escobar-Lemmon (2003) who focuses on the incentives of parties and individual politicians. She finds that Conservatives were more likely to support local elections presumably because they were unlikely to win national elections in the future.

³⁵Falleti (2010) argues instead that the timing of these reforms was a crucial determinant explaining the nature of these fiscal reforms.

and prominent member of the Constitutional Assembly, explained:

What did we wanted with that constitution? We wanted to make peace and that constitution is a constitution to make peace. It contained all the political reforms that any revolutionary movement aspires and much more...The insurgencies demanded political reforms and with the 1991 Constitution lets say they were left with no platform. (Serpa, Horacio. Interviewed by author, Bucaramanga, Colombia).

On the issue of why the decentralization process did not include a defense component experts argue that the main security threat at the time where the drug cartels from Cali and Medellin, which started fighting a war between themselves and against the national government in their attempt to eliminate the extradition treaty with the US, and not the guerrilla and paramilitary groups. As Antonio Navarro, another prominent member of the Assembly answered when asked about the whether or not the issue of territorial control and political violence was discussed in the Assembly:

I don't recall that well a discussion like the one you ask in the Assembly. The issue of security was focused mainly on how to deal with drugtracking and on how to recover the possibility of exercising justice. We created the Fiscalia with a semi-accusatory regime and then a special commission in the Legislature . . . [W]e structured the "no-face" justice system to protect justices from the risk that the mafias represented. (Navarro, Antonio. Personal communication with author).

The argument about the preponderance of drug-related violence in the public debate and the relatively minor importance of how guerrillas and paramilitaries could undermine the effectiveness of the decentralizing reforms is expressed by many other political leaders, even from the Conservative party, a collectivity historically having a hard-line stance towards armed actors. As Gustavo Zafra Roldan, a representative of the Conservative party in the Constitutional Assembly argued:

We clearly underestimated the capacity of the paramilitaries. In our defense, paramilitaries were not part of the national debate and the main security issue in the country was the violence of the drug cartels. (Zafra, Gustavo. Interviewed by author, Bogota, Colombia).

Lastly, on the issue of the timing of the main reforms, there are two main arguments. The first is based on an increasing popular pressure to democratize local politics. This discontent

is consistent with hundreds of civic strikes during the 1980s (Falleti 2010). Second, there seems to be genuine believe that decentralization was only solution to the deep social inequalities of the country and to the increasing threat of the guerrillas (Castro 1988). As Horacio Serpa argued about why they introduced these set of reforms in the new Constitution:

Because we thought peace was going to come. I mean, the constitution was not made for a country at war but for a country in peace. We introduced a series of reforms which were the ones the guerrilla demanded. So we said, with these reforms we will make the peace, the guerrilla will be over, and we will live in peace, as we always wanted. That is the only explanation. (Serpa, Horacio. Interviewed by author, Bucaramanga, Colombia).

Conclusion

Based on a specific reform in Colombia during the nineties, this paper revisited the effectiveness of fiscal decentralization as a strategy of conflict resolution. In this particular case, political, fiscal and administrative reforms, empowering lower levels of government, were implemented partly to improve the security situation of the country. The implicit assumption was that a decentralized system would better represent the interest of the poor and improve local public goods.

My findings, however, indicate that this logic may be flawed in some conflict situations. Using a series of difference-in-difference models and subnational variation in conflict-related events across Colombian municipalities, I find a positive, statistically significant relationship between (automatic) fiscal transfers from the center and violence against local authorities and politicians. Moreover, I find that this effect is conditional on the state capacity of each locality. In places where the state has very low capacity (e.g., in municipalities having no local police), the impact of transfers on violence is significantly higher. These findings are interpreted in the context of a simple rent seeking model in which armed actors use political violence to appropriate the public resources of municipalities. A provocative conclusion is then that decentralization made local governments more vulnerable to the coercion and violence of nonstate armed actors.

My findings suggest that the amount of public resources available for capture is a key determinant of local political violence. This complements recent studies on the economic determinants of other forms of civil war violence (e.g., Dube and Vargas 2013) and validates more generally studies focusing on how local conditions shape the final political outcomes of decentralization (e.g., Bardhan and Mookherjee 2006). Although this paper focuses only

on one case, it calls attention to theories placing local political empowerment as a simple, one-dimensional strategy, against insurgencies and terrorist organizations. The relative importance of public goods provision and local accountability in preventing and containing political violence under war is a promising area of further empirical research.

Appendix

A. Proof of Comparative Statics

Taking e_{-j} as given, the objective function for any given group $j \in \{1, 2, \dots, N\}$ is given by

$$\max_{e_j \in \mathbb{R}_+} \left\{ \left((1 - \phi) \left(\frac{e_j^m}{e_j^m + \sum_{i \neq j} e_i^m} \right) R - e_j \right) \right\}.$$

The interior solution to this program is given by

$$(1 - \phi) \left(\frac{m e_j^{m-1} \sum_{i=1}^N e_i^m - e_j^m (m e_j^{m-1})}{\left(\sum_{i=1}^N e_i^m \right)^2} \right) R = 1.$$

Imposing symmetry and solving for e_j we get

$$e_j^* = \frac{(1 - \phi)(N - 1)m}{N^2} R \equiv e^*. \quad (5)$$

Hence, the equilibrium level of effort for each player is a positive function of m and R and a negative function of ϕ .

Therefore, we have that

$$\begin{aligned} \frac{\partial e^*}{\partial R} &= \frac{(1 - \phi)(N - 1)m}{N^2} > 0 \\ \frac{\partial}{\partial \phi} \left(\frac{\partial e^*}{\partial R} \right) &= -\frac{(N - 1)m}{N^2} < 0. \end{aligned}$$

To verify the existence of the interior solution notice how for any j

$$\begin{aligned} \mathcal{W}^j(e^*) &= \frac{(1 - \phi)}{N} R - \frac{(N - 1)(1 - \phi)m}{N^2} R \\ &= \frac{(1 - \phi)}{N} \left(\frac{N(1 - m) + m}{N} \right) R \geq 0 \end{aligned}$$

for any $m \leq N/N - 1$.

B. Data Appendix

Political homicides: number of mayors, council and JAL members, ex-politicians, candidates, and local leaders murdered between 1988 and 2001. Data from the Programa de Derechos Humanos y Derecho Internacional Humanitario, Presidencia de la República (<http://www.derechoshumanos.gov.co>) and compiled by the Centro de Estudios sobre Desarrollo Economico (CEDE), Universidad de los Andes.

Central Government Fiscal Transfers: yearly real transfers per capita to municipal governments from the Departamento Nacional de Planeacion (<https://www.dnp.gov.co/>). Transfers are deflated using a price index of 2000 from the Banco de la República-Colombia (<http://www.banrep.gov.co/>). Total municipal population from the Departamento Administrativo Nacional de Estadística-DANE (<http://www.dane.gov.co>).

Poverty: annual NBI “unsatisfied basic needs” index, between 1988 to 2000. Data from the Centro de Estudios sobre Desarrollo Economico-CEDE, Universidad de los Andes.

Local State Capacity: local “institutional presence” of the state measured by the number of state agencies in the “cabecera” municipal and the total municipal-level public employees. From Fundacion Social (1998).

Conflict Indicators: Yearly terrorist activity by all non-state armed actors, measured by the number of small explosives and attacks to the infrastructure of the municipality; the intensity of military confrontation measured by the number of armed clashes between the National Army and guerrillas (FARC and ELN mostly), and by the number of military casualties. Violence indicators against civilians measured by civilian casualties (directly related to the armed conflict) and by the number of massacres (defined by the murder of more than 3 civilians in the same time and space). All indicators are from the Programa de Derechos Humanos y Derecho Internacional Humanitario and compiled by the CEDE and cover the 1988-2001 period.

Non-State Armed Actors Presence: Yearly dummy indicators of guerrilla presence and paramilitary presence based on conflict events, including armed clashes between groups or unilateral actions between 1990 and 2000. From the Conflict Analysis Resource Center CERAC (<http://www.cerac.org.co>).

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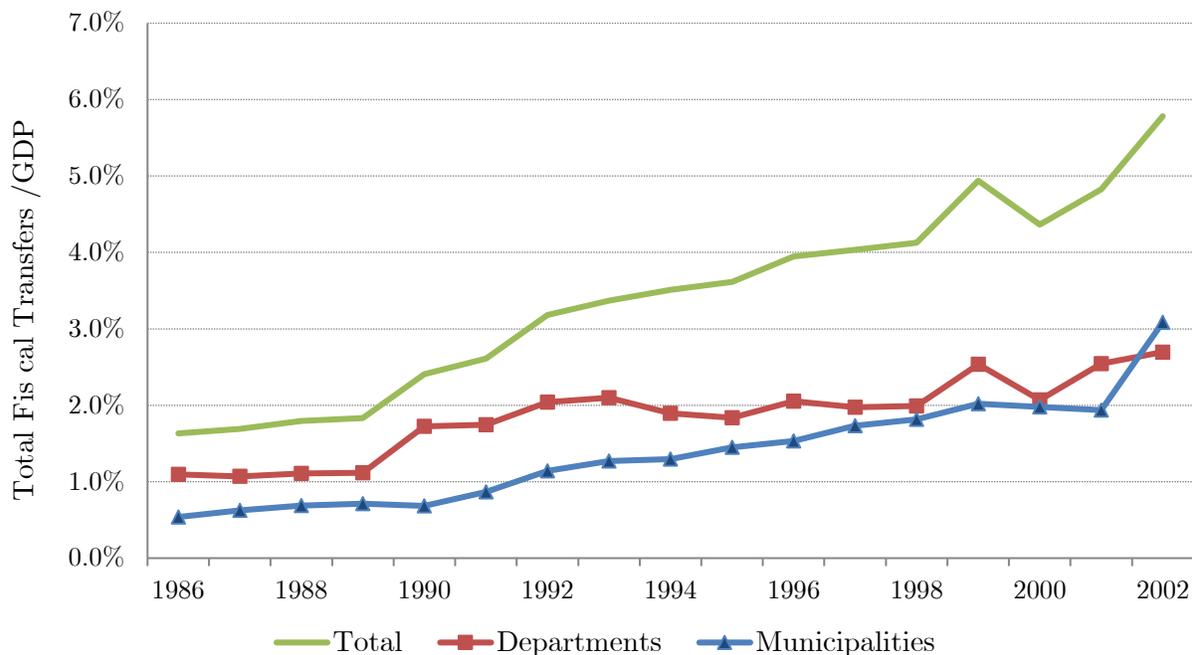
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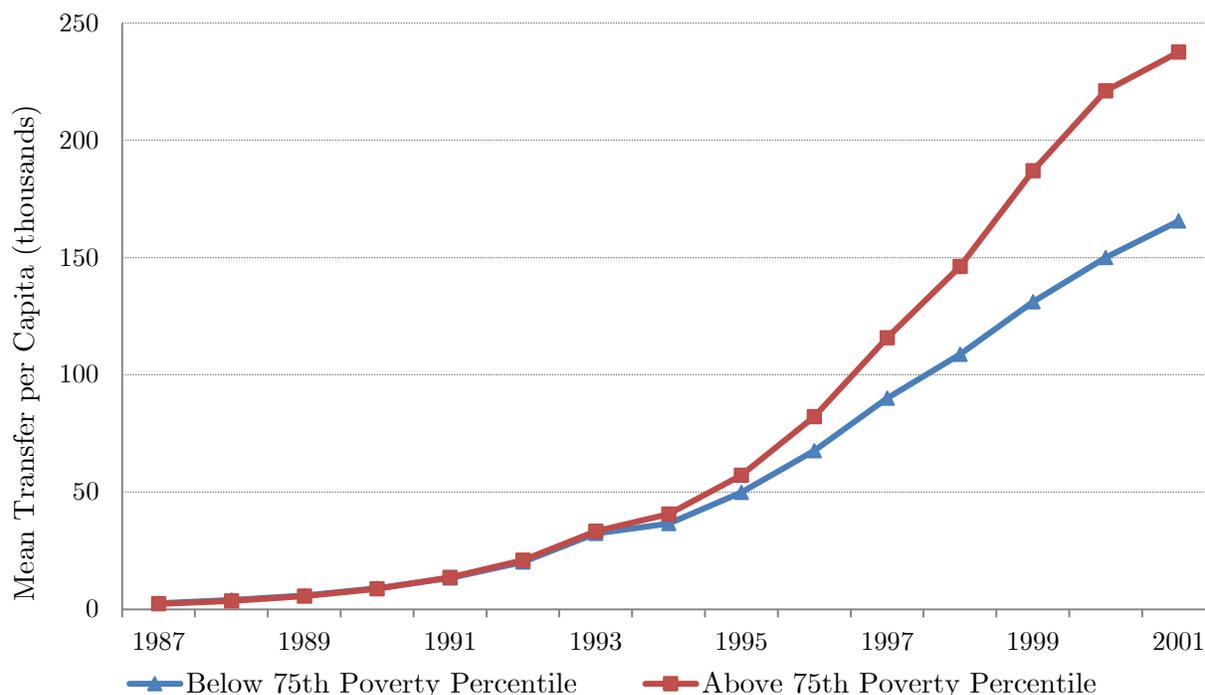
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Figure 1
Transfers to Lower Levels of Government and GDP, 1986-2002



Notes: GDP and transfers data in nominal Colombian pesos. Data from the Dirección de Desarrollo Territorial, Departamento Nacional de Planeación (DPN), Colombia. Author's calculation.

Figure 2
Real Fiscal Transfers per Capita and Poverty, 1987-2001



Notes: Fiscal transfers data are from the Departamento Nacional de Planeacion (DPN). Poverty line, measured by the NBI "Unsatisfied Basic Need Index," and municipal population are from the Departamento Nacional de Estadística (DANE). Author's calculation. The average poverty rate in 1985 was 61.3% and the 75th percentile was 75%.

Figure 3
A
Homicides Elected Officials and Candidates, 1988-2000



B
Homicides Elected Officials, Candidates, Leaders and other public officers, 1988-2000



Notes: Graph A: Homicides of elected officials, politicians and candidates, Graph B homicides of all public employees (elected and non-elected) and other local-level politicians. Data from the Observatorio de DDHH y DIH, Vicepresidencia de la Nación. Poverty percentiles based on the NBI index of 1985 from the DANE. Author's calculations

Table 1-Descriptive Statistics

		Mean	Median	Std. Dev.
		(1)	(2)	(3)
<i>Main Outcome</i>	Political Homicides	0.16	0	0.69
<i>Central Gov. Transfers</i>	Per capita transfers	10.85	9.02	9.56
<i>(real COP × 1000)</i>	Per capita transfers _{t1=88-91}	4.73	4.17	4.97
	Per capita transfers _{t2=98-01}	17.35	14.83	10.98
	$\Delta \text{Ln(PC transfers)}_{t2,t1}$	1.26	1.25	0.57
<i>State Capacity</i>	Local Public Employees ₉₅	100.72	20	848.63
	State Agencies ₉₅	37.98	18	187.37
	Colonial Officials	5.81	0	124.08
	Colonial Agencies	0.58	0	0.88
<i>Other Conflict Outcomes</i>	Terrorists attacks (explosives)	0.40	0	2.63
	Attacks against infrastructure	0.28	0	2.38
	Military casualties	0.20	0	2.06
	Army combats with guerrillas	0.47	0	1.44
	Civilian Homicides	1.02	0	4.47
	Massacres	0.13	0	0.73
<i>Controls</i>	Population (× 1000)	36.20	13.14	203.17
	Poverty rate (NBI) ₉₀	56.04	56.40	17.85
	Urbanization Rate ₉₀	0.36	0.31	0.23
	Electoral Competition (ENP ₈₈)	1.78	1.80	0.50
	Distance Dpt. Capital (km)	117.41	96.50	95.70
	Altitude (mt)	1,198.47	1,250.00	897.65
	Observations (Municipalities)	1,009		

Note: see main text and Appendix B for variable definitions and sources.

Table 2
The Impact of Fiscal Transfers on Political Violence, FE Estimates

<i>Dependent variable</i>	Homicides of Public Officials, Politicians and Candidates							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln (per-capita CG transfers _{t-1})	0.501*** (0.188)	0.501*** (0.179)	0.478*** (0.180)	0.419** (0.171)	0.196*** (0.074)	0.189*** (0.065)	0.166*** (0.063)	0.143** (0.060)
Ln (per-capita CG transfers _{t-1}) × Ln (state agencies ₁₉₉₅)	-0.160** (0.066)	-0.159** (0.064)	-0.154** (0.065)	-0.133** (0.060)				
Ln (per-capita CG transfer _{t-1}) × Ln (municipality employees ₁₉₉₅)					-0.051** (0.025)	-0.049** (0.023)	-0.046** (0.023)	-0.038* (0.020)
Observations	13,846	13,846	13,678	13,678	14,096	14,096	13,914	13,914
Municipalities	989	989	977	977	1,007	1,007	994	994
R^2 (within)	0.030	0.031	0.037	0.127	0.024	0.025	0.031	0.121
Lagged poverty	yes	no	no	no	yes	no	no	no
Initial poverty × year dummies	no	yes	yes	yes	no	yes	yes	yes
Time invariant controls × year dummies	no	no	yes	yes	no	no	yes	yes
Department dummies × year dummies	no	no	no	yes	no	no	no	yes

Notes. Robust standard errors in parentheses, clustered at the municipality level. All models include municipality fixed-effects and (ln) total municipal population. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3
The Impact of Fiscal Transfers on Political Violence, LD Estimates

<i>Dependent variable</i>	Δ (Homicides of Public Officials, Politicians and Candidates)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta \text{ Ln (per-capita CG transfers}_{t-1})$	0.822*** (0.292)	0.836*** (0.279)	0.843*** (0.276)	0.768*** (0.289)	0.338*** (0.103)	0.325*** (0.086)	0.310*** (0.082)	0.288*** (0.086)
$\Delta \text{ Ln (per-capita CG transfers}_{t-1})$ $\times \text{ Ln (state agencies}_{1995})$	-0.288** (0.118)	-0.291** (0.115)	-0.293** (0.114)	-0.264** (0.117)				
$\Delta \text{ Ln (per-capita CG transfer}_{t-1})$ $\times \text{ Ln (municipality employees}_{1995})$					-0.108** (0.047)	-0.107** (0.044)	-0.105** (0.044)	-0.094** (0.042)
Municipalities	989	989	977	977	1,007	1,007	994	994
R^2	0.109	0.109	0.112	0.209	0.063	0.065	0.069	0.179
Δ poverty	yes	no	no	no	yes	no	no	no
Initial poverty	no	yes	yes	yes	no	yes	yes	yes
Time invariant controls	no	no	yes	yes	no	no	yes	yes
Department dummies	no	no	no	yes	no	no	no	yes

Notes: Robust standard errors in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 4
Robustness Tests. Colonial State Capacity Measures

<i>Dependent variable</i>	Homicides of Public Officials, Politicians and Candidates							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln (per-capita CG transfers _{t-1})	0.081*** (0.024)	0.079*** (0.021)	0.056*** (0.018)	0.054** (0.024)	0.079*** (0.021)	0.078*** (0.019)	0.061*** (0.018)	0.052** (0.023)
Ln (per-capita CG transfers _{t-1}) × Colonial state agencies	-0.052** (0.026)	-0.048* (0.025)	-0.038 (0.024)	-0.035 (0.023)				
Ln (per-capita CG transfer _{t-1}) × Ln (colonial officials)					-0.122** (0.049)	-0.119** (0.048)	-0.104** (0.047)	-0.068* (0.036)
Observations	13,984	13,984	13,830	13,830	13,984	13,984	13,830	13,830
Municipalities	999	999	988	988	999	999	988	988
R^2 (within)	0.022	0.024	0.030	0.120	0.028	0.029	0.034	0.121
Lagged poverty	yes	no	no	no	yes	no	no	no
Initial poverty × year dummies	no	yes	yes	yes	no	yes	yes	yes
Time invariant controls × year dummies	no	no	yes	yes	no	no	yes	yes
Department dummies × year dummies	no	no	no	yes	no	no	no	yes

Notes: Robust standard errors in parentheses, clustered at the municipality level. All models include municipality fixed-effects and (ln) total municipal population. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 5
Robustness Tests. Armed Groups Presence

<i>Dependent variable</i>	Homicides of Public Officials, Politicians and Candidates							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln (per-capita CG transfers $_{t-1}$)	0.122** (0.057)	0.119** (0.058)	1.002** (0.387)	1.115** (0.491)	0.058*** (0.018)	0.063*** (0.023)	0.501** (0.200)	0.553** (0.254)
Ln (per-capita CG transfers $_{t-1}$) × Ln (state agencies $_{1995}$)	-0.040* (0.021)	-0.040* (0.021)	-0.280** (0.129)	-0.315** (0.158)				
Ln (per-capita CG transfer $_{t-1}$) × Ln (municipality employees $_{1995}$)					-0.017** (0.007)	-0.020** (0.008)	-0.101* (0.058)	-0.117* (0.070)
Armed Group Presence: (guerrillas & paramilitaries)	Low	Low	High	High	Low	Low	High	High
Observations	6,552	6,426	3,542	3,528	6,676	6,550	3,612	3,584
Municipalities	468	459	253	252	477	468	258	256
R^2 (within)	0.017	0.123	0.069	0.221	0.016	0.124	0.061	0.213
Initial poverty × year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Time invariant controls × year dummies	no	yes	no	yes	no	yes	no	yes
Department dummies × year dummies	no	yes	no	yes	no	yes	no	yes

Notes: Robust standard errors in parentheses, clustered at the municipality level. All models include municipality fixed-effects and (ln) total municipal population. Low (high) armed group presence samples defined by the municipalities having a mean presence below (above) the group-specific mean for both paramilitaries and guerrillas. Presence indicators from CERAC. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 6
Robustness Tests. Different War Events

<i>Dependent variable</i>	Violence against Civilians		Military Confrontation			Terrorist Attacks	
	Homicides	Massacres	Military Casualties	Combats	Ambush	Explosives	Against Infrastructure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log (per-capita CG transfer) _{<i>t-1</i>}	0.668 (0.588)	0.039 (0.099)	0.575 (0.416)	-0.180** (0.087)	0.054*** (0.019)	-0.052 (0.129)	0.571* (0.303)
Ln (per-capita CG transfer _{<i>t-1</i>}) × Ln (municipality employees ₁₉₉₅)	-0.314 (0.227)	-0.035 (0.040)	-0.222 (0.165)	0.059** (0.028)	-0.013** (0.006)	0.021 (0.048)	-0.207* (0.114)
Observations	13,914	13,914	13,914	13,914	13,914	13,914	13,914
No. Municipalities	994	994	994	994	994	994	994
R^2 (within)	0.230	0.084	0.110	0.134	0.084	0.469	0.495

Notes: Robust standard errors in parentheses, clustered at the municipality level. All models include municipality fixed-effects, the 1990 poverty rate interacted with time effects, time invariant controls interacted with time effects, and department-specific time effects. All models control for (ln) total municipal population. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

ONLINE APPENDIX
(Not for publication)

Table 1A. Colonial measures, LD models

<i>Dependent variable</i>	Δ (Homicides of Public Officials, Politicians and Candidates)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Ln (per-capita CG transfers $_{t-1}$)	0.126*** (0.031)	0.120*** (0.027)	0.089*** (0.028)	0.074* (0.043)	0.102*** (0.028)	0.096*** (0.028)	0.080*** (0.031)	0.062 (0.047)
Δ Ln (per-capita CG transfers $_{t-1}$) × Colonial state agencies	-0.073* (0.043)	-0.071* (0.041)	-0.060 (0.039)	-0.054 (0.039)				
Δ Ln (per-capita CG transfer $_{t-1}$) × Ln (colonial officials)					-0.200** (0.089)	-0.199** (0.087)	-0.181** (0.084)	-0.117* (0.071)
Municipalities	999	999	988	988	999	999	988	988
R^2	0.035	0.034	0.041	0.161	0.067	0.067	0.069	0.167
Δ poverty	yes	no	no	no	yes	no	no	no
Initial poverty	no	yes	yes	yes	no	yes	yes	yes
Time invariant controls	no	no	yes	yes	no	no	yes	yes
Department dummies	no	no	no	yes	no	no	no	yes

Notes: Robust standard errors in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2A. Robustness Tests. Different War Events, State Capacity Measured by Municipal Public Employees

<i>Dependent variable</i>	Violence against Civilians		Military Confrontation			Terrorists Attacks	
	Homicides	Massacres	Military Casualties	Combats	Ambush	Explosives	Against Infrastructure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log (per-capita CG transfer) _{<i>t-1</i>}	0.668 (0.588)	0.039 (0.099)	0.575 (0.416)	-0.180** (0.087)	0.054*** (0.019)	-0.052 (0.129)	0.571* (0.303)
Ln (per-capita CG transfer _{<i>t-1</i>}) × Ln (municipality employees ₁₉₉₅)	-0.314 (0.227)	-0.035 (0.040)	-0.222 (0.165)	0.059** (0.028)	-0.013** (0.006)	0.021 (0.048)	-0.207* (0.114)
Observations	13,914	13,914	13,914	13,914	13,914	13,914	13,914
No. Municipalities	994	994	994	994	994	994	994
R^2 (within)	0.230	0.084	0.110	0.134	0.084	0.469	0.495

Notes: Robust standard errors in parentheses, clustered at the municipality level. All models include municipality fixed-effects, the 1990 poverty rate interacted with time effects, time invariant controls interacted with time effects, and department-specific time effects. All models control for (ln) total municipal population. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.