

# Don't Make War, Make Elections

## *Franchise Extension and Violence in XIXth-Century Colombia\**

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### Abstract

This paper studies the effect of strengthening democracy, as captured by an increase in voting rights, on violent civil conflict in nineteenth-century Colombia. Empirically studying the relationship between democracy and conflict presents an enormous challenge. First, there are a number of conceptual problems in defining and measuring democracy. Second, democracy and violent conflict in society are jointly determined. We take advantage of an experiment of history to examine the impact of one simple, measurable dimension of democracy (the size of the franchise) on conflict, while at the same time attempting to overcome the identification problem. In 1853, Colombia established universal male suffrage. Using a simple difference-in-difference specification at the municipal-level, we find that municipalities where more voters were enfranchised relative to their population experienced a sharper decrease in the likelihood of violent battles. The results are robust to including a number of additional controls and, to investigate what forces plausibly drive these results, we perform a number of additional exercises. We look at which components of the proportion of new voters in 1853 explain the results, and we examine if results are stronger in places with more political competition and state capacity. We interpret our findings as suggesting that violence in nineteenth-century Colombia was a technology for political elites to compete for the rents from power, and that democracy constituted an alternative way to compete which substituted violence.

**Keywords:** Democracy, Civil Conflict, Colombia.

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# 1 Introduction

This paper studies the effect of strengthening democracy, as captured by an increase in voting rights, on violent civil conflict. The relationship between democracy and conflict (or peace) is at the center of many theories of democracy, democratization, and conflict. One line of research suggests that democratization is a way to avert conflict. In the theory put forward by Acemoglu and Robinson (2006), for example, politically excluded groups in society exercise violence (*de facto* power) to obtain policy concessions, unless they are given some *de jure* or “institutional” power. Democratization constitutes a credible concession of such power, and therefore avoids conflict. Another possibility is that democracy is a substitute for violence, but not because it offers institutional power to the excluded majority, but because it constitutes a technology to allocate power between political elites. This technology to allocate power replaces the use of force.

For inter-state conflicts, on the other hand, the famous and long-standing *democratic peace* hypothesis states that democracies are unlikely to go to war with each other. Many arguments have been put forward to defend this idea, including the fact that democracies face institutional constraints (in a democracy, the rulers are accountable to the population and the population at large wants to avoid the calamities of war and such costs like conscription), as well as the idea that democracies develop a set of “democratic norms” that run against war. These types of arguments can be (and have been) also applied to intra-state war, and there is also some evidence of a relationship between indicators of democracy or political freedom and some measures of internal violence, like state repression, terrorism, and human rights abuses<sup>1</sup>.

At the same time, democracy may exacerbate conflict through several channels. One alternative is that democracy, or at least democratic elections, exacerbates political (and other) identities, firing up violence. Eifert, Miguel, and Posner (2010) provide one suggestive study. These authors find that ethnic identities in Africa are strengthened by exposure to political competition: for every month closer their country is to a *competitive* presidential election, survey respondents are 1.8 percentage points more likely to identify in ethnic terms. More generally, democracy, by creating winners and losers (in elections), may increase incentives for violence, that could otherwise be avoided via, for example, power-sharing agreements. A key question is what are the conditions under which losers will peacefully relinquish power (Chacón, Robinson, & Torvik, 2011). If these conditions are not met, then more democracy may well imply more violence.

Whatever the theoretical relationship, empirically studying the relationship between democracy and conflict presents an enormous challenge. On the one hand, there are a

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<sup>1</sup>For instance, Abadie (2004) finds that political freedom explains terrorism, but it does so in a non-monotonic way, with countries in some intermediate range of political freedom being most prone to terrorism; Davenport (1997) finds that elections reduce the use of censorship and political restrictions, as well as human rights violations (see also Davenport (2007)).

number of conceptual problems in defining and measuring democracy. On the other hand, even if aptly measured, the levels of democracy and violent conflict in society are jointly determined. Hence, we face the usual hurdles when interpreting any correlation between measures of democracy and civil conflict as causal.

We take advantage of an experiment of history to examine the impact of one simple, measurable dimension of democracy (the size of the franchise) on conflict, while at the same time attempting to overcome the identification problem. We focus on nineteenth-century Colombia. In 1853, Colombia enacted a Constitution that abolished slavery as well as literacy and wealth requirements for voting, effectively establishing universal male suffrage. The Constitution also introduced, for the first time, direct presidential elections.

This natural experiment provides an ideal framework to study the impact of extending voting rights on levels of civil conflict. First, the increase in voting rights was very significant, since the preexisting wealth and literacy requirements together with slavery effectively disenfranchised the majority of voters. Second, like many other Latin American during these early years of independence from colonial powers, Colombia faced a number of civil conflicts during the nineteenth century. Crucially, and as detailed in the data section, we have a good measure at the municipality level of whether a battle was fought in any given year. Finally, and also key for our empirical strategy, in 1851 Colombia had one of its first comprehensive censuses of the population. The census data allow us to build a proxy for the number of “new voters” in each municipality following the national-level reform of 1853. Given preexisting differences in their demographic composition, some municipalities experienced a larger increase in their voting population than others, providing an ideal experiment to assess whether such places experienced more or less conflict following the reform.

Our basic strategy and main results are illustrated in Figure 1. The figure divides municipalities between those with a high proportion of new voters (where our proxy for new voters as percent of total population in 1853 is above the median) and those with a low proportion of new voters (which are below the median). We next compute, for each group and each year, the average value of a dummy variable that equals one if a battle was fought in a given municipality and year. The circles in the figure represent this average (hence the probability of a battle) for the municipalities with a high proportion of new voters. The triangles, on the other hand, are the corresponding averages for municipalities with a low proportion of new voters. Finally, the smoothed lines correspond to a locally weighted regression of the average battle dummy on year (the solid line is for the high-voters municipalities, and the dashed line is for the low-voters one). Vertical lines are shown for the year 1853 (dashed, when the Constitution extending the franchise was enacted) and for 1856 (solid, when the first presidential election with universal male suffrage and direct voting took place).

A number of clear and important messages emerge from Figure 1. First, prior to

1853, the municipalities with a high proportion of new voters do not differ, in terms of the probability of battles, to those with a low proportion of new voters. This validates our identification assumption that the preexisting differences in demographic structures are not correlated with differential trends in terms of violence. Second, starting around 1853, both groups of municipalities begin to diverge. In particular, in line with the idea that more democracy (here, voting rights for a larger share of the population) is a factor contributing to a fall in conflict, we observe that the probability of a battle in the low-voters municipalities is higher than in high-voters municipalities. Third, the difference seems to be maximized around 1856, when the presidential elections under the new rule took place. Also interestingly, the gap between the two types of municipalities starts to close in 1863. This is a meaningful year, as Colombia adopted an ultra-federal constitution, with individual states adopting new rules in terms of voting rights. Several of them pulled back some of the extension of voting rights that the 1853 Constitution had enacted. In short, Figure 1 paints a clear picture showing that, while in effect, the extension of voting rights of the 1853 Constitution decreased violence in places that benefitted more with this extension relative to places that benefitted less.

In the rest of the paper, we verify that the message from Figure 1 survives statistical scrutiny and is robust to a number of specification checks. We run a simple difference-in-difference specification that tests our hypothesis. In particular, we estimate a municipal-level regression where the dependent variable is a dummy variable that equals one if a battle was fought in a given year and the main independent variables are: our proxy for the new voters, an indicator variable that equals one after the 1851 reform, and their interaction. We find that the coefficient of the interaction term is negative and significant, implying that municipalities where the franchise extension of the 1853 Constitution enfranchised more voters relative to their population experienced a sharper decrease in the likelihood of violent battles than those where the share of enfranchised population was smaller.

The results are robust to including a number of additional controls. First, to control for national-level trends in violence, we include a full set of year fixed effects.

Second, to make sure that our results are not driven by changes in violence across municipalities after the franchise extension that also happen to be correlated with the size of our proxy of new voters, we also run specifications including differential trends parametrized as functions of observable municipality characteristics. The observable characteristics include geographic controls (historical road density, river density, and altitude), a measure of the average partisanship leaning of each municipality (the share of votes for Ospina, the conservative candidate, in the 1856 presidential elections), and historical institutional controls (an index of colonial institutions and the number of colonial functionaries, as compiled by García-Jimeno (2005) from Durán y Díaz (1794)).

Third, both to test for the presence of potential differential trends between munic-

palties with high and low proportion of new voters that are uncorrelated with the 1853 reform, and to deal with the fact that Colombia experienced a number of Constitutional reforms (many of which affected voting rights) we verify the validity of the results to the inclusion of differential trends following other major changes in voting rights. Specifically, we include time dummies and their interaction with new voters for the periods under which the 1832, 1843 and 1863 Constitutions, and the 1876 reform, prevailed. Among these changes, those of 1863 are especially interesting for us. Under the federal *Rionegro* Constitution, each of the states adopted its own decision regarding suffrage. We take advantage of this to examine whether municipalities in states that further increased voting rights experienced decreases in violence while states that reintroduced suffrage restrictions observed more violence. We find a higher probability of battles in municipalities of states that reduced voting rights after such reforms took effect. This is additional suggestive evidence in line with the idea that limiting voting rights created violence. However, we are more cautious about interpreting this correlation as causal, as the decision to extend or limit the suffrage requirements in each state is clearly endogenous and could be related with expected levels of violence. This contrasts our main result using the 1853 reform, in which we take advantage of a national-level reform and exploit the extent to which it differentially affected municipalities.

Our main result is robust these specification tests, providing convincing evidence of the pacifying effect of franchise extension in Colombia. To investigate what forces plausibly drive these results, we perform a number of additional exercises. First, we look at which components of the proportion of new voters in 1853 explain the results. We find that most of the effect is accounted for by young single men and married men, regardless of age. Instead, neither slaves nor old single men explain the effect of new voters on violence. Old and single men are a small proportion of the population in most municipalities, so this lack of an effect is perhaps not surprising. The fact that the proportion of slaves plays a small role is both reassuring about the validity of the identification assumption and falls in line with Colombian historiography on the causes of the nineteenth-century civil wars. It is reassuring because places with many slaves tended to be very different to places with few slaves. If all the effect was coming from the comparison between these two types of places, we would be worried that other omitted variable connected with the slave economy could explain the differences between municipalities. It falls in line with the historical record, too, as the civil wars of the nineteenth century have mostly been depicted as fights between political elites, not as violence around emancipation or other economic vindications by excluded slaves. Also, that the bulk of free men (young and single and married men regardless of age) drives the results is in line with the idea that this segment of the population plausibly includes the people who were most politically active at the time.

In an additional exercise, we look at whether results differ depending two key munic-

ipal characteristics: the degree of political competition and the extent of state capacity. We find that the impact of new voters is stronger in places with stronger state institutions and with more political competition. We interpret both of these results as suggesting that violence in nineteenth-century Colombia was essentially a technology for political elites to compete for the rents from power.

## 2 Data

The main variables in our analysis are the measure of violence and proportion of new voters at the municipality level. The proportion of new voters in each municipality comes from the 1851 Colombian Census. The Census categories do not exactly match the categories of enfranchised population, but they come very close. In particular, the 1853 Constitution gave the right to vote to all male citizens over the age of 21, including former slaves (now free men). Men under the age of 21 could also vote if they were already married. Hence, using the 1851 Census, we can approximate the voting population in each municipality as the sum of the number of single males over 16 (who would be old enough to vote by the time of the 1856 elections), the number of married slaves, and the number of married free men.

Therefore, the only category of voters that we cannot count is the number of single slaves over the age of 21. However, there are various reasons for which this is not a major concern. First, while we do not know the age composition of slaves, a very large percentage (50%) of free men were under age. If the age composition of slaves resembled to some extent that of free men, then many of the single slaves were unable to vote. Second, the census figures indicate that 40% of slaves were married, a sizable proportion. Third, just 0.7% of Colombians were slaves in 1951 (Tovar, 2007). Of course, this hides some important regional variation, but on average excluding single slaves implies excluding a very small share of the potential population or voters ( $0.007 * 0.60$ ). Finally, under the *Ley de Ventres* already in place in 1951, offsprings of slaves were free once they reached 18 years of age, so most of those classified as single slaves in the 1953 Census are likely to be too young to vote.

Undoubtedly, in nineteenth-century Colombia, a national-level census was a challenging task given the limited capacity of the state. Gómez (1969) mentions some of the hurdles: widowers were often classified as single, it is uncertain how unmarried couples were classified, some isolated communities (especially indigenous) were not counted, some people feared that the census would imply forthcoming taxation and tried to avoid it, and the quality of the data varied by regions. In spite of all these difficulties, he chooses “the 1851 Census as the base census, since [out of all nineteenth-century censuses] it was the one with less problems” (p. 91).

Moving to our second key variable, we rely on Riascos Grueso (1950) to count the

number of battles in each municipality and year in Colombia during the nineteenth century. In his *Geografía Guerrera de Colombia* (War Geography of Colombia), Riascos Grueso recorded, municipality by municipality, the history of battles taking place in each location since independence and into the *War of a Thousand Days* of the turn of the century. It is important to note that these battles include national-level civil wars as well as armed local disputes between political factions.

To illustrate the coding of these data, consider the following two examples of entries in Riascos Grueso's database. Under the entry for Puerto Colombia, municipality in the northern department of Atlntico, it reads:

The troops of Luis Bryón, Rafael Tono, Jerónimo Carbono, Pedro Celestino Guillén y Gutierrez and Juan Illingrot, won in the years (..) 20 and 21. Under Urdaneta's dictatorship, his side combatted with the rebels, led, in turn, by generals Policarpo Martinez and Ignacio Luque. In the year 40, rebels and government forces again fought, as part of the revolution that exploded in the atlantic coast in october that year<sup>2</sup>.

Taking into account that Urdaneta's dictatorship took place in 1830, our entry for the number of battles in Puerto Colombia equals one in 1820, 1821, 1830, and 1840, and zero otherwise. Notice that all battles in this municipality took place before 1853. The next example shows the case of a municipality, Rionegro in the department of Antioquia, with preponderance of battles after 1853:

On September 7 of 51 regular forces met in combat with the rebels, led by generals Tomás Herrera and Eusebio Borrero, defeated. Nine years later, centralists and federalists faced each other. In the year 64, January 4, doctor Pedro Justo Berrío defeated the militias of doctor Pascual Bravo, president of the state of Antioquia, who died in fighting (...) action which (...) permitted the victorious chief to take over Medellín and assume power of the *antiqueño* state. In the year 85 general Cándido Tolosa fought, twice, the government forces of captain Alejandro Ardila, who repelled the attacks.<sup>3</sup>

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<sup>2</sup>The translation is our own. The original entry reads: "Triunfaron la tropas de Luis Bryón, Rafael Tono, Jerónimo Carbono, Pedro Celestino Guillén y Gutierrez y Juan Illingrot, en los años (..) 20 y 21. Cuando la dictadura de Urdaneta, combatieron los defensores de éste con los rebeldes, dirigidos, en su orden, por los generales Policarpo Martinez e Ignacio Luque. El año 40, se batieron nuevamente insurgentes y gobiernistas, como consecuencia de la revolución que estalló en la costa atlántica en octubre de dicho año."

<sup>3</sup>"El 7 de septiembre del 51 combatieron las fuerzas regulares con las insurgentes, dirigidas por los generales Tomás Herera y Eusebio Borrero, vencido. Nueve años después, lidiaron centralistas y federalistas. El año 64, enero 4, triunfó el doctor Pedro Justo Berrío sobre las milicias del doctor Pascual Bravo, presidente del estado de Antioquia, quien pereció en la refriega (...), acción ésta (...), cuya victoria permitió al jefe vencedor tomar a Medellín y asumir el mando del estado antioqueño. El añ 85 combatió el general Cándido Tolosa, dos veces, las fuerzas gobiernistas del capitán Alejandro Ardila, quien rechazó los ataques de aquél."

Rionegro thus exhibits one battle in 1851, 1860, 1864, two battles in 1885, and zero in the remaining years. Since there is no formal definition in Riascos Grueso’s account on what exactly a “battle” means, we rely in our main analysis on a simple dummy variable that equals one if the municipality experienced positive battles in a given year and zero otherwise. However, our results are almost identical when relying on the number of battles.

We also constructed a set of additional historical controls for robustness checks and to explore the mechanisms that may explain our results. These variables and their sources are listed in Appendix Table A-1. From the 1851 Census, we use some of the components of our “new voters” variable to explore which of them are most important for the differences in violence between municipalities. Specifically, we compute the shares of young single males (ages 16-50), old single males (ages 50 and over), married free men, and married slaves.

We rely on Bushnell (1970), who presents municipal-level data on the 1856 presidential elections, to construct a measure of political competition at the municipality level. Our measure of political competition equals zero when the winning candidate in municipality  $m$  received all votes among the top two candidates, and equals one if votes were equally split among the top two contestants. That is, we measure political competition as

$$1 - (votes\ first - votes\ second)/(votes\ first + votes\ second),$$

where *votes first* is the number of votes obtained by the top candidate of municipality  $m$  and *votes second* those obtained by the runner-up of municipality  $m$ . From the same source, we also compute the share of votes in each municipality for Ospina, the conservative candidate, as a control for average partisanship in each location.

To examine mechanisms, we rely on two simple measures for the extent of state capacity at the municipality level. First, with foundation dates from Bernard and Zambrano (1993) we calculate the age of each municipality in our sample. This variable serves as a coarse but useful proxy for state capacity, with older municipalities having on average stronger state institutions than newer ones. Second, we measure the strength of colonial institutions in each municipality with the index proposed by García-Jimeno (2005). With information from Durán y Díaz (1794), García-Jimeno (2005) builds an index of state capacity, ranging from 0 to 4, which counts whether a municipality had a tobacco *estanco*, *aguardiente estanco*, mail service and *alcabala* (*estancos* were state monopolies and *alcabalas* consumption taxes).

Also, in 1863 under the federal *Rionegro* Constitution, each of the states adopted its own decision regarding suffrage<sup>4</sup>. We take advantage of this to code an additional pair of

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<sup>4</sup>Suffrage rights in 1863 were set as follows: (i) Antioquia: men with at least 21 years of age, and married men regardless of age; (ii) Bolívar: men with at least 21 years of age, married men, and emancipated slaves; (iii) Casanare (Boyacá): literate men with 21 years of age or older; (iv) Cauca: men with



dummy variables which specify whether voting rights were increased or decreased in 1863. Since several states took steps in both directions at the same time (for example, Santander and Cundinamarca reduced the age to 18, but reinstated the literacy requirement) we followed a conservative approach and only assigned a value of one to municipalities in states where voting rights unambiguously increased or decreased. The only state with unambiguous increase in voting rights is Magdalena, with a decrease in voting age to 18 years and no return to other restrictions, while the states with unambiguous decrease in voting rights are Tolima, incorporating either literacy or wealth requirements, and Boyacá, incorporating literacy. Other states, like Antioquia, Bolívar, and Cauca, simply continued granting voting rights to males over the age of 21 and married men regardless of age.

Finally, our regressions include a number of controls, including geographic controls (historical road density, river density, altitude), a measure of the average partisanship leaning of each municipality (whether Ospina, the conservative candidate, obtained the majority of the votes in the 1856 presidential elections), and historical institutional controls (the index of colonial institutions and the number of colonial functionaries, as compiled by García-Jimeno (2005) from Durán y Díaz (1794)). Table 1 presents summary statistics for the variables in our analysis.

### 3 Empirical Strategy

Our empirical strategy exploits the introduction of universal male suffrage in Colombia’s 1853 Constitution. We take advantage of the fact that this extension of the franchise affected some municipalities more than others because of their preexisting demographic structure. Our most basic specification is the next simple difference-in-difference regression for municipality  $m$  at time  $t$ :

$$battle_{m,t} = \beta_1 + \beta_2 (new.voters_m \times d_T) + \beta_3 new.voters_m + \beta_4 \mathbf{X}_m + \delta_t + \varepsilon_{m,t}. \quad (1)$$

In (1),  $battle_{m,t}$  is a dummy variables that equals 1 if municipality  $m$  experienced one or more battles in year  $t$ , and zero otherwise<sup>5</sup>. The key proxy of the extent to which the introduction of universal male suffrage affected municipality  $m$  is  $new.voters_m$ . This proxy is built using the 1851 Census numbers, as detailed in the preceding section. The

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at least 21 years of age, and married men regardless of age; (v) Cundinamarca: literate men with 18 years of age or older; (vi) Magdalena: men with at least 18 years of age, and married men regardless of age; (vii) Santander: literate men with 18 years of age or older; (viii) Tolima: men with at least 21 years of age, and married men regardless of age, that either are literate or have wealth exceeding 200 pesos. We do not include Panamá, also a state of Colombia at the time, because we have no information on violence for its territory

<sup>5</sup>Results are very similar if, instead of using a dummy variable for battles as the dependent variable, we rely on the number of battles.

main coefficient of interest,  $\beta_2$ , captures the interaction of  $new.voters_m$  with  $d_T$ , a dummy variable that equals 1 for the time interval  $T$  under which the franchise extension was in effect. Given the abundance of constitutional changes in nineteenth-century Colombia, we explore various alternative time ranges in when defining  $d_T$ .

If  $\beta_2 < 0$ , then municipalities where the franchise extension of the 1853 Constitution enfranchised more voters relative to their population experienced a sharper decrease in the likelihood of violent battles than those where the share of enfranchised population was smaller. A negative value of  $\beta_2$  therefore indicates that the extension of voting rights causes less violence, while a positive coefficient would suggest that it exacerbates violence.

To verify the robustness of our results, we include a number of additional controls. In particular, to control for national-level trends in violence, we include a full set of year fixed effects,  $\delta_t$ . While during the XIXth Century several battles were regional, a number of national-level confrontations also took place. The year fixed effects flexibly control for all these national trends, or any other year-specific characteristics which may have influence the overall level of violence. A number of violent confrontations were more regional in nature.

In  $\mathbf{X}_m$  we also incorporate our set of geographic controls (road density, river density, altitude), electoral controls (the support for Ospina, the conservative candidate, in the 1856 presidential elections), and historical institutional controls (the number of colonial functionaries, as compiled by García-Jimeno (2005) based Durán y Díaz (1794)). Finally, we also allow for differential trends for periods following other major changes in the rules of the game regarding the size of the franchise (specifically, the 1832, 1843 and 1863 Constitutions, and the 1876 reform).

To make sure that our results are not driven by changes in violence across regions after the franchise extension that also happen to be correlated with the size of  $new.voters$ , we perform a number of robustness checks. On the one hand, with the inclusion of an interaction of the number of new voters with other meaningful institutional periods, particularly some of the years preceding the 1853 Constitution, we verify the assumption that municipalities with high and low number of new voters have “parallel trends.” Second, we show the robustness of our results to the inclusion of differential trends that are parametrized as functions of various municipality characteristics. In particular, we run regressions of the following form:

$$battle_{m,t} = \beta_1 + \beta_2 (new.voters_m \times d_T) + \sum_{x \in \mathcal{X}} \beta_x x_m d_T + \delta_m + \delta_t + \varepsilon_{m,t}, \quad (2)$$

where  $x_m$  are observable municipality characteristics (each one in our set of control variables  $\mathbf{X}_m$ , denoted with  $\mathcal{X}$ ). The rest of the variables are defined as before. Hence, equation (2) allows for differential trends for municipalities with different characteristics  $x_m$ . If these characteristics, rather than the presence of more new voters, explain

the different violence behavior of municipalities, then  $\beta_2$  should no longer be significant. Instead, finding  $\beta_2 < 0$  in these regressions is reassuring about the validity of our identification assumptions. Notice also that in (2) we include a full set of municipality fixed effects  $\delta_m$ , flexibly controlling for any fixed characteristics that may influence the average level of violence in municipality  $m$ .

In our estimation, we focus on the period 1821-1885. While the *Nueva Granada* first declared its independence in 1810, it seems reasonable to exclude the years of the *Patria Boba* (1810-1816), marked by intense internal discussion over the organization of the New Republic, as well as the succeeding years of conflict with the Spaniards to consolidate the independence. By 1821, with the Cúcuta Constitution, independence had been consolidated. In 1886, on the other hand, the new Constitution marked by an effort to centralize state authority and the Thousands Day War (1899-1902) that followed inaugurated a new period in Colombia's history, with the 1886 Constitution surviving in its basic form until 1991.

## 4 Results

### 4.1 Baseline results

Table (2) presents our basic difference-in-differences estimates of the effect of the 1853 democratization on political conflict in Colombia. We estimate model (1) by OLS.<sup>6</sup> The key message from this table is that our main coefficient of interest, the interaction of this dummy with the proportion of new voters, is negative and significant across specifications.

Column 1 presents the simplest version of our regression equation, with no additional controls  $X_m$  and where  $d_T$ , the post-period dummy, equals one after 1856 and zero otherwise. In this regression we also omit the full set of year fixed effects, as a useful reference point to examine the coefficient on  $d_T$ . Notice that the coefficient associated with the time dummy equals one after the 1856 elections is positive and significant, capturing an increasing trend in the overall level of violence.

In columns 2 and 3 we address some potential concerns related to the timing of the expected effects. In particular, while the reform was passed in 1853, the first elections for national-level office with universal male suffrage took place in 1856. This motivated our choice of 1856 as the key year. It could be argued, however, that the mere passing of the regulation relieved political tension and decreased violence, especially where many new potential voters had just been enfranchised. To test this, column 2 runs the same regression as in column 1, but defines the post-dummy  $d_T$  differently, assigning a value

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<sup>6</sup>We focus on the OLS estimates since linear models provide the best estimate of the true conditional expectation function. In contrast, non-linear models are very sensible to model specifications (Angrist & Pischke, 2009) and suffer from the incidental parameters problem in the presence of fixed effects.

of one for all years following 1853. A complementary exercise is done in column 3, where the interim years (between the reform and the presidential election) are simply dropped from the regression. In both cases, our estimate of  $\beta_2$  remains negative and significant. Moreover, the coefficient increases in magnitude (which, in column 2, indicates that indeed some effects could have kicked in with the mere passage of the reform). Since the exact definition of the post dummy as starting in 1853 or 1856 does not seem critical to our main conclusion, we henceforth focus on the post-1856 period (to stack the cards against us, as this is the smaller coefficient in absolute value).

A noteworthy result in columns 1 to 3 is that the coefficient associated with our estimate of the proportion of new voters is negative, suggesting that on average the incidence of violence was lower in places with a higher proportion of new voters. This could be a source of concern to the extent that it suggests that places with more new voters are fundamentally different to places with less new voters. However, once we control for observables in column 4, notice that these differences are no longer statistically significant. Hence, controlling for observables, it is only after the franchise extension that we observe municipalities with more new voters experiencing less violence. This gives more credence to our main result. In all remaining specification we will not report the coefficients on the post dummy or the share of new voters, as our specifications will include a full set municipality and year fixed effects, as in column 5 of Table 2.

## 4.2 Robustness

Though by granting universal male suffrage the 1853 Constitution was the most radical in terms of franchise extension, it was not the only major institutional reform during the nineteenth century in Colombia.<sup>7</sup> After the first constitution of independent Colombia (1821), subsequent constitutions were declared (in addition to the 1853 Charter) in 1832, 1843, 1858, 1863 and 1886.<sup>8</sup> Moreover, a major constitutional reform took place 1876. Most constitutions changed the franchise in one way or the other. Indirect presidential elections were the rule throughout the period (with the exception of the inter-constitutional period between 1853 and 1863), and the franchise was typically restricted to literate males over 21 (underaged could vote only if they were married), often with additional income or wealth requirements. Also, the 1863 Constitution consolidated a federal system and allowed each province to write their own Charter with different franchise rule.<sup>9</sup> This year, most states reversed the universal male suffrage achieved in

<sup>7</sup>As we noted, the 1853 Constitution implemented direct elections and abolished both literacy and property/rents requirements. It also abolished slavery. All male over 21 could vote and even younger men could do so as long as they were married.

<sup>8</sup>The 1886 Constitution was not changed until 1991, but it suffered tens of substantive reforms during the course of the twentieth Century.

<sup>9</sup>The country's name change then from *Confederación Granadina* to *Estados Unidos de Colombia* (United States of Colombia).

1853.

These institutional changes are likely to have an effect on the incidence of battles both before and after the universal male suffrage was set in 1853. With the inclusion of year fixed effects we are already taking into account any overall changes in violence associated with these different periods. This type of overall trends cannot, therefore, be contaminating our results. For these other constitutional reforms to affect our main results, it would have to be the case that they differentially affected places with different proportions of the new voters enfranchised in the 1853 Constitution. While this is a more intricate and perhaps less plausible story, we can directly test it. We do so on Table 3.

The simplest approach is to run, as in column 1, our basic specification (as in column 5 of Table 2) but where we restrict our post-dummy  $d_T$  to the period 1857-1862. This verifies whether our initial results are contaminated by the franchise backlash that took place in most of the country (except for the state of Magdalena) with the 1863 federal Constitution, and subsequent changes. Reassuringly, our estimate of  $\beta_2$  in this specification is not just negative and significant as in the baseline regression, but is much larger in absolute value ( $-0.0029$ ). Hence, if anything, we were underestimating the effect of the franchise extension on violence<sup>10</sup>.

In column 2, we further allow for differential trends parametrized as functions of the observable municipal characteristics, as in equation (2). The coefficient is still stable in magnitude, and significant at the 90% confidence level. In columns 3 to 7, we run similar specifications but where, instead of a post-1856 dummy or a period dummy for the duration of the 1853 constitution, we interact *new.voters* with period dummies for each one of the main time intervals under which other institutional arrangements prevailed. Hence, for instance, in column 3  $d_T$  equals one for the years 1832-1842 and zero otherwise. This period, corresponding to the *Nueva Granada* Constitution, had indirect elections with franchise restricted to age (21) (or else be married) and literacy and possession of rents or assets. In column 4, in turn,  $d_T$  captures the 1843 Constitution period, also a time with indirect elections and suffrage restrictions based on literacy and possession of rents or assets, but where the voting age was 21 years, regardless of marriage status. It is important to note that, in 1858, the *Confederación Granadina* Constitution was adopted. Yet, it did not change universal male suffrage, and so we consider its period as a continuation of the 1853 innovation of the franchise. Columns 5 and 6, instead, take into account the introduction of the federal *Estados Unidos de Colombia* Constitution

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<sup>10</sup>To gauge the size of these effects recall that  $\beta_2 = \left[ \frac{\partial \text{battle}_{m,t}}{\partial \text{new.voters}_m} \right]_{d_T=1} - \left[ \frac{\partial \text{battle}_{m,t}}{\partial \text{new.voters}_m} \right]_{d_T=0}$ . Using our most conservative estimate from Table 2, this means that 3.39% more new voters than the average municipality (a one-standard deviation increase), means about 0.3 percentage points ( $-0.000808 * 3.39$ ) lower probability of a battle after 1853 (from an average probability of 1.9% over such period). The effects when focusing merely on the 1857-62 period are larger. The same increase in new voters means about a 1 percentage point ( $-0.003 * 3.39$ ) lower probability of a battle after the franchise extension (from an average probability of 3% over such period)

of 1863. Since in 1876 there was a major reform of this constitution, we run separate regressions where  $d_T$  captures the periods 1863-1875 and 1877-1885 (in columns 5 and 6, respectively). While the 1876 reform did not establish a franchise (then a competence of individual states), this allows for a more flexible test of potential differential trends. Also, the 1876 Constitution changed electoral rules in that, to avoid continuous elections throughout the country, it homogenized electoral calendars across states so that elections for states' presidents were held at the same time.

Overall, the message from columns 3-6 is that the negative coefficient in our baseline specification is not likely to capture other more general negative differential trends between municipalities with high versus low proportion of new voters. Indeed, the interaction of *new.voters* with these other time periods is only significant in columns 3 and 5. Moreover, in these cases the interaction term is *positive*. Also, it bolsters our main result that in the period immediately preceding the 1856 elections (see column 4) the interaction coefficient is not significant and very small in magnitude. Thus, it seems there are no prior differential trends and only during the prevalence of universal male suffrage did the abundance of new voters help reduce violence. This confirms our identification assumption that municipalities with a high proportion of new voters would not have had different levels of violence than those with few new voters, had it not been for the introduction of universal male suffrage in 1853. This is perhaps most clear in column 7, where we include each of the time intervals of columns 2-6 and their interactions with the proportion new voters. Only the interaction with the 1857-1862 retains statistical significance.

In short, the evidence that extending voting rights served to reduce violence is very robust to controlling for potential omitted variables or differential trends. Before proceeding to examine which mechanisms could explain this result, notice that the 1863 changes are especially interesting for us. Since each of the states adopted its own decision regarding suffrage, we can examine whether municipalities in states that further increased voting rights experienced decreases in violence while states that reintroduced suffrage restrictions observed more violence. Hence, column 8 of Table 3 investigates this by adding two additional variables: 'Post 1863 up', which equals one after 1863 and for municipalities in states where voting rights unambiguously increased further in 1863 (Magdalena); and 'Post 1863 down', which equals one after 1863 and for municipalities in states where voting rights unambiguously decreased further in 1863 (Tolima and Boyacá). The coefficient on 'Post 1863 down' is positive and significant, showing a higher probability of battles in municipalities of states that reduced voting rights. This is additional suggestive evidence in line with the idea that limiting voting rights created violence. The coefficient on 'Post 1863 up' is negative, also in line with this idea, but it is not statistically significant and small. While suggestive and in line with our main result, we are more cautious about interpreting this correlation as causal, as the decision to extend or

limit the suffrage requirements in each state is endogenous and could be related with expected levels of violence.

### 4.3 Mechanisms

We have established that the extension of voting rights in nineteenth century Colombia reduced the level of violence. Municipalities where more voters were enfranchised with the introduction of universal male suffrage observed less violence than those in which there were less new voters.

At some level, this result is somewhat surprising given the perceptions about Colombia's democracy and violence. Indeed, one reading of Colombia's political history and its extraordinarily persistent political parties seems to suggest that political elites in Colombia were constantly trying to avoid war, but had a hard time achieving this. More importantly, the solutions that "worked" in the twentieth century could be regarded anti-democratic. A prominent example is the incomplete vote and power-sharing institutions of the early twentieth century, which Mazzuca and Robinson (2009) show were key in establishing order after the tumultuous nineteenth century. Perhaps more clearly, the *Frente Nacional* (National Front) established since the mid-twentieth century helped control the period of intense inter-partisan violence known as *La Violencia*. But, again, it did so via a power-sharing agreement under which the presidency was determined by an alternating Conservative and Liberal president every 4 years, with the two parties having parity in all other elective offices<sup>11</sup>. These arrangements, of course, excluded other political parties and reduced competition.

Also in line with the idea that "too much democracy" led to violence in Colombia, one common claim is that elections in Colombia polarized politics, and were often proximate determinants of rebellions (Posada-Carbó, 1995). Bushnell (1993) writes that "elections, though regularly held, were almost always marked by scattered incidents of violence in the back country and by charges of fraud the were often well founded." In 1879, in the *Diario de Cundinamarca*, the following description could be found: "elections in Colombia are real confrontations, and terrible confrontations of press, agitation, intrigue, letters, bribes, weapons, vengeance incentives, politics, cholera, menace" (Guerra, 1922, p.608).

These notions about the connection between democracy and violence in Colombia are probably also fueled by the clear indication that, in the Latin American context, Colombia is both extraordinarily democratic and extraordinarily violent. As Deas (1993) puts it "the periods of authoritarianism or militarism have been very scarce and very short in the one hundred and forty years of Colombia's existence as independent state. There have been numerous constitutional experiments, and this republic has had more elections, under more systems, central and federal, direct and indirect, hegemonic and

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<sup>11</sup>The National Front has been extensively studied. See, for example, Hartlyn (1988).

proportional, and with more consequences than any American or European country that could attempt to dispute the title.” The PolityIV democracy index, as depicted in Figure 2, confirms that Colombia has been relatively democratic in the Latin American context. Needless to say, Colombia has also been relatively violent in the region. This is confirmed by the years in conflict according to the UCDP/PRIO Civil War dataset (see Table 4).

Discussing twentieth-century violence, authors like Pecaut (1984) have argued that this dual condition is no coincidence. He asks: “Should we consider as pure coincidence this dual continuity-civil democracy, on the one hand, and social violence and guerrilla warfare, on the other? [My hypothesis] is that it is not merely coincidental. Violence is the flip side of Colombian democracy.” However, when looking into the reasons for this relation, it becomes apparent that it is the lack of representativeness of Colombia’s democracy, not the fact that Colombia is *too* democratic, what is hypothesized to be driving the connection. Pecaut (1984) thinks that Colombia remains “halfway in the establishment of political democracy. It certainly recognizes universal suffrage [but] it has been a manifestation of the immutable division between the two identities -Liberal and Conservative-”. Moreover this rivalry, strengthened after bipartisan conflict, “had nothing to do with the social stakes. Under these conditions, the social conflicts were doomed to unfold on another stage.”

Hence, perhaps just as in the twentieth century the lack of representativeness of the democratic institutions led to protracted guerrilla warfare in Colombia, the lack of political rights in nineteenth-century Colombia exacerbated violence. Our results clearly suggest that this was the case. But, to better understand some of the channels at play, we next perform a set of additional exercises.

First, in Table 5, we look at which components of the proportion of new voters in 1853 explain the results. Specifically, instead of interacting *new.voters* with the 1857-1862 dummy, we interact independently each of its components (the municipal shares of married slaves, young and single free men, old and single free men, and married free men). We do so separately, in columns 1-4, and including all components with their interactions in column 5. These regressions include differential trends as functions of observable characteristics and a full set of municipality and year fixed effects.

The results show that the effect of *new.voters* is accounted for by young single men and married men, regardless of age. Instead, neither slaves nor old single men explain the effect of new voters on violence. Old and single men are a small proportion of the population in most municipalities, so this lack of an effect is perhaps not surprising. In turn, the fact that the proportion of slaves plays a small role is both reassuring about the validity of the identification assumption and falls in line with Colombian historiography on the causes of the nineteenth-century civil wars. It is reassuring because places with many slaves tended to be very different to places with few slaves. If all the effect was coming from the comparison between these two types of places, we would be worried that



other omitted variable connected with the slave economy could explain the differences between municipalities. And it falls in line with the historical record, because the civil wars of the nineteenth century have mostly been depicted as fights between political elites, not as violence around emancipation or other economic vindications by excluded slaves. Also, that the bulk of free men (young and single and married men regardless of age) drives the results is in line with the idea that this segment of the population plausibly includes the people who were most politically active at the time.

A second set of exercises is present in Table 6. This table splits the sample according to several characteristics of municipalities, in order to explore which kinds of municipalities experienced a greater payoff (in terms of reduced violence) when the franchise was extended. In particular, we look at two key municipal characteristics: the degree of political competition and the extent of state capacity. In column 1, we run the regression for the set of municipalities in which the extent of political competition is above the median (with competition captured by an inverse measure of the vote margin between the first and second most-voted candidate in the municipality during the 1856 presidential elections). Column 2, instead, runs the same regression for municipalities in the least-competitive half. Interestingly, we find that only in competitive municipalities the interaction of the 1857-1862 dummy and new voters is significant and negative. Moving to proxies for the extent of state capacity in each municipality, in columns 3 and 4 we split the sample according to the age of each municipality (the older half is included in column 3, and younger municipalities in column 4). Older municipalities are likely to have a more consolidated presence of the state, and so this is a crude but useful proxy for state strength at the local level. To measure institutions more directly, in columns 5 and 6 we split the sample using the index for colonial institutions explained in section 2. Column 5 includes the set of municipalities with some colonial institutions, and column 6 those municipalities without. Again, we associate the latter with less local state capacity. In both cases, the results are clear to point out that only in municipalities with greater state capacity (columns 3 and 5) the interaction of the 1857-1862 dummy and new voters is significant and negative.

Hence, the impact of new voters is stronger in places with stronger state institutions and with more political competition. We interpret both of these results as suggesting that violence in nineteenth-century Colombia was essentially a technology for political elites to compete for the rents from power. In Colombia, these elites were represented by the enduring Liberal and Conservative parties. There is considerable consensus that violence was largely between parties, but that differences between parties were small, except on ecclesiastic issues and federalism. As Bushnell (1984) asks, “if there were, then, no substantial differences on policy between the two parties, save in ecclesiastical matters, why were Colombian political contests so hard-fought, even violent?” (p. 27). He replies: “political struggles in Colombia revolved around competition for control of

the bureaucratic positions, that is, for the meager booty contained in the public treasury or simply for the social status that official positions conferred” (p. 27). If these are the causes of violence, we expect (as we find in our data) that if elections could serve to substitute for violence, the places where elections are most strengthened (because more people gain the right to vote) should have less violence especially if the contest for power is tight enough (political competition) and if the prize for winning the contest is attractive enough (state capacity).

## 5 Conclusion

As noted before, in spite of all the theoretical attention, empirically studying the relationship between democracy and conflict presents an enormous challenge. ‘Democracy’ is a complex, multi-dimensional concept, and the usual endogeneity concerns are particularly pressing in regressions of violence on measures of democracy.

This paper takes advantage of an experiment of history to examine the impact of one simple, measurable dimension of democracy (the size of the franchise) on conflict, while at the same time attempting to overcome the identification problem. In 1853, Colombia established universal male suffrage. Using a simple difference-in-difference specification at the municipal-level, we find that municipalities where more voters were enfranchised relative to their population experienced a sharper decrease in the likelihood of violent battles. The results are robust to including a number of additional controls, suggesting that, in nineteenth century Colombia, elections substituted violence.

Notice however that the statement stemming from our results is conditional and more subtle: *given* there are elections, we find that there is less violence if more people can participate in them. There are several possible and related reasons why this effect could arise. First, with more participation, elections are more legitimate, and democracy becomes a more widely accepted set of rules for allocating power, reducing violence. Second, and quite relevant for the Colombian case, electoral fraud (casting votes for those who were not allowed to vote) in elections is often a proximate cause of violence. This cause is eliminated or at least attenuated by permitting participation of more citizens. Indeed, a very common form of electoral fraud is stuffing ballots of those ineligible to vote. One may think that this channel operates mainly during elections, but it could also be relevant off-election periods. In particular, politically active people may be less willing to get involved in violence if they anticipate to take part in electoral politics and if they expect elections not to be fraudulent.

In the case of Colombia, violence in the nineteenth century was largely between parties, and revolved around competition for control of the bureaucratic positions and rents from power more than around ideological differences. If elections substituted violence in this context, the franchise extension should have a larger effect in those areas where politically

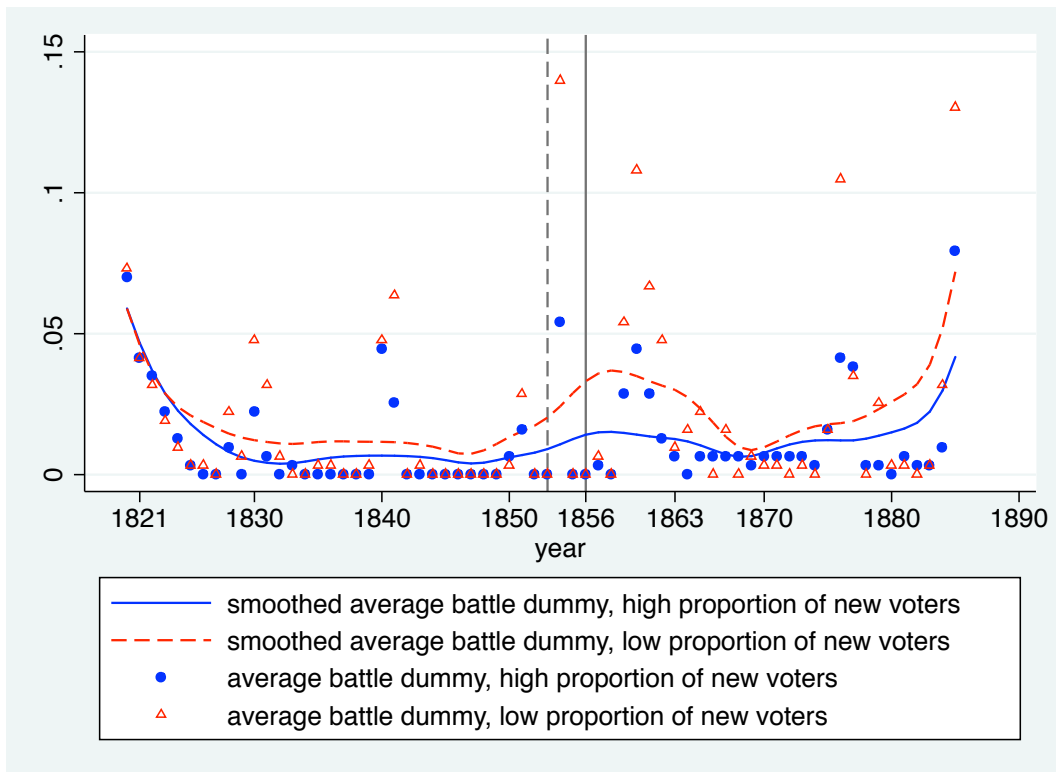
active citizens formed the majority of new voters, where competition between parties was stiffer, and where rents from government were larger. This is, indeed, what we find.

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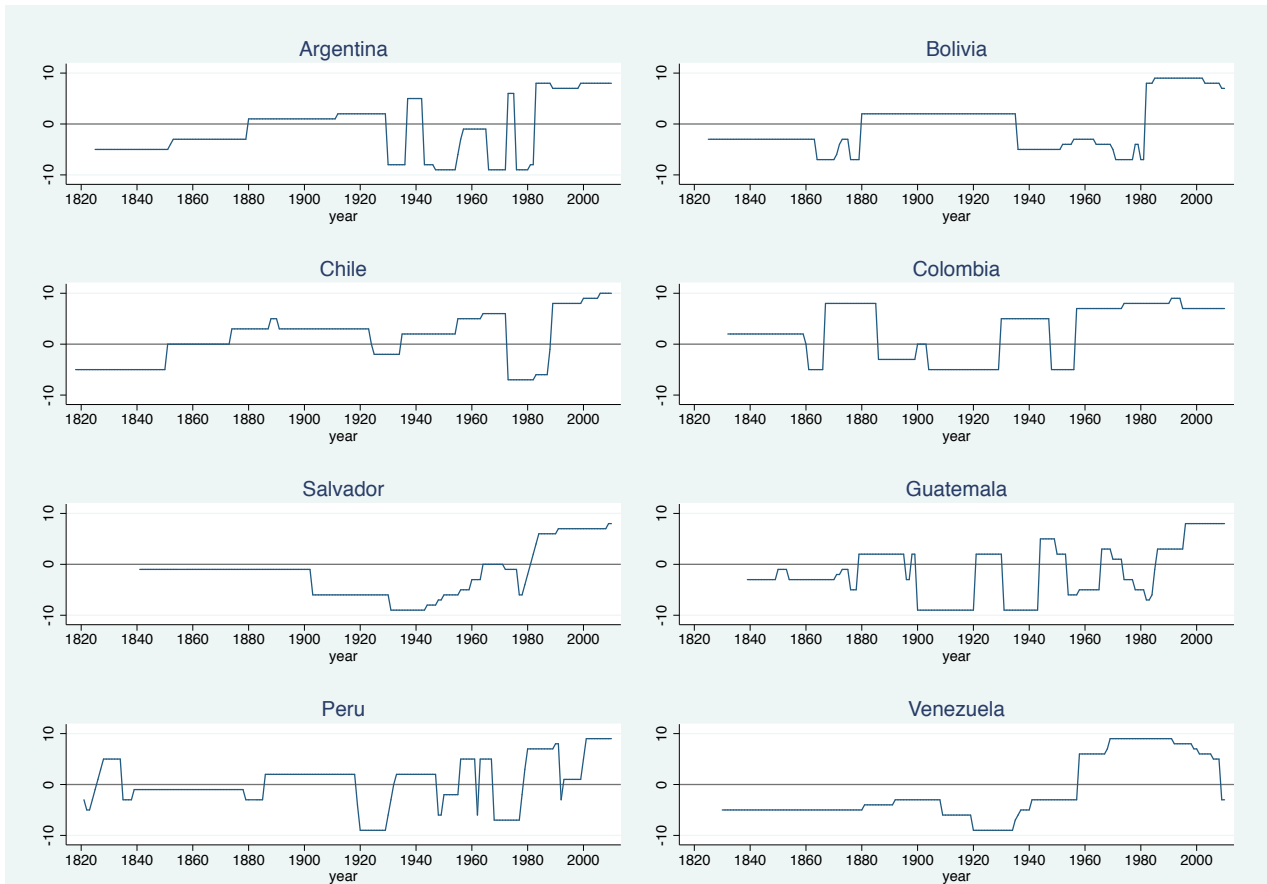
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**Figure 1: XIXth-century Battles in Colombia**  
 Yearly average of municipal battle dummy, by level of new voters



Note: Municipalities with a high proportion of voters are those for which the proxy for new voters (as % of population) in 1853 is above the median (and the rest are classified in the low proportion categories). The smoothed battles lines correspond to a locally weighted regression of the average battle dummy on year (with a 0.3 bandwidth). Vertical lines are shown for 1853 (dashed, when the Constitution extending the franchise was enacted) and 1856 (solid, when the first presidential election with universal male suffrage and direct voting took place).

**Figure 2: Polity IV Index since Independence for a sample of Latin American countries**



Note: Polity IV democracy index (polity2) for the set of displayed countries (-10=least democratic, 10=most democratic).

**Table 1: Summary statistics**

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min.</b>	<b>Max.</b>	<b>N</b>
Battles (dummy=1 if battle in municipality)	0.01	0.12	0	1	40170
Battles before 1856	0.01	0.11	0	1	22248
Battles after 1856	0.02	0.14	0	1	17922
Battles from 1857 to 1862	0.03	0.18	0	1	3708
Estimated share of new voters (in %)	24.62	3.39	14.61	44.8	40170
Minimum distance to Royal road (kms)	17.27	21.24	0.03	144.84	40170
Altitude, municipality center	1286.91	939.63	2	3087	40170
Primary rivers density (kms/Km2)	35.72	288.65	0	6467.87	40170
Secondary rivers density (kms/Km2)	19.87	55.32	0	865.63	40170
Tertiary rivers density (kms/Km2)	15.48	36.86	0	507.31	40170
Dummy =1 if Ospina (conservative) won in 1856	0.42	0.49	0	1	40170
Number of state functionaries in 1794	9.20	157.53	0	3844	40170
State presence index in 1794	0.86	0.97	0	4	40170

Note: See Appendix Table A-1 for variable definition and sources. Battles for 1821-1885.



**Table 2: Democratization and Violence**  
**New voters and battles after the 1856 reform**

<i>Dependent variable: Incidence of battles, 1821-1885</i>					
	(1)	(2)	(3)	(4)	(5)
Prop. new voters	-0.00119*** (0.000301)	-0.000841*** (0.000290)	-0.000841*** (0.000290)	-0.000383 (0.000328)	
Post 1856	0.0274*** (0.00988)			0.0274*** (0.00988)	
Post 1856 x Prop. new voters	-0.000808** (0.000386)			-0.000808** (0.000386)	-0.000808** (0.000389)
Post 1853		0.0463*** (0.0111)	0.0380*** (0.0106)		
Post 1853 x Prop. new voters		-0.00144*** (0.000429)	-0.00116*** (0.000414)		
Controls				<b>Yes</b>	<b>Yes</b>
Municipality & time FE					<b>Yes</b>
Observations	40,170	40,170	38,316	40,170	40,170
Municipalities	618	618	618	618	618
R-squared	0.003	0.004	0.003	0.017	0.092

Ordinary Least Squares regressions. Robust standard errors clustered at the municipal level in parentheses. \* is significance at the 10% level, \*\* is significance at the 5% level, \*\*\* is significance at the 1% level. Controls in columns 4-5 include geographic controls (distance to royal road, river density, altitude), electoral controls (support for Ospina, the conservative candidate, in the 1856 presidential elections), and historical institutional controls (the number of colonial functionaries and an index of presence of colonial institutions in each municipality). See Appendix Table A-1 for variable definition and sources.

**Table 3: Robustness: Controlling for Other Constitutional Changes**

<i>Dependent variable: Incidence of battles, 1821-1885</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
N. voters x ...								
...1857-1862	-0.00289***	-0.00205**					-0.00187**	-0.00185**
	(0.000988)	(0.000941)					(0.000861)	(0.000861)
...1833-1842			0.000767**				0.000652	0.000657
			(0.000376)				(0.000544)	(0.000544)
...1844-1855				-8.33e-05			-6.39e-05	-5.84e-05
				(0.000360)			(0.000546)	(0.000546)
...1864-1875					0.00109**		0.000893	0.000852
					(0.000459)		(0.000585)	(0.000584)
...1877-1885						-0.000852	-0.000743	-0.000784
						(0.000542)	(0.000674)	(0.000672)
Post 1863 increase							-0.00315	
							(0.00657)	
Post 1863 decrease							0.00680***	
							(0.00261)	
Mun. & time FE	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>
Controls x 1857-1862		<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>
Observations	40,170	40,170	40,170	40,170	40,170	40,170	40,170	40,170
Municipalities	618	618	618	618	618	618	618	618
R-squared	0.093	0.095	0.094	0.094	0.094	0.094	0.095	0.095

Ordinary Least Squares regressions. Robust standard errors clustered at the municipal level in parentheses. \* is significance at the 10% level, \*\* is significance at the 5% level, \*\*\* is significance at the 1% level. Controls (interacted with the 1857-1862 dummy) as in Table 2, columns 4-5. Full set of municipality and year fixed effects are in all columns. 'Post 1863 up' equals one for municipalities in states were voting rights unambiguously increased further in 1863 (Magdalena) and 'Post 1863 down', equals one for municipalities in states were voting rights unambiguously decreased further in 1863 (Tolima and Boyacá). See Appendix Table A-1 for variable definition and sources.

**Table 4: Years in conflict, 1946-2005**

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Country:	Years in conflict
Argentina	7
Bolivia	3
Chile	1
Colombia	40
El Salvador	14
Guatemala	33
Peru	21
Venezuela, RB	2

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Note: Years in conflict over the 1946-2005 period according to the UCDP/PRIO Armed Conflict Dataset.

**Table 5: Mechanisms I: Components of ‘proportion of new voters’**

<i>Dependent variable: Incidence of battles, 1821-1885</i>					
	(1)	(2)	(3)	(4)	(5)
N. voters- <i>married slaves</i> x 1857-1862	0.00324* (0.00192)				0.00138 (0.00200)
N. voters- <i>young sing. free</i> x 1857-1862		-0.00181** (0.000866)			-0.00242** (0.000942)
N. voters- <i>old sing. free</i> x 1857-1862			0.00170 (0.00548)		0.00349 (0.00583)
N. voters- <i>married free</i> x 1857-1862				-0.00193** (0.000963)	-0.00230** (0.00102)
Mun. & time FE	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>
Controls x 1857-1862	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>
Observations	40,170	40,170	40,170	40,170	40,170
Municipalities	618	618	618	618	618
R-squared	0.095	0.095	0.094	0.094	0.095

Ordinary Least Squares regressions. Robust standard errors clustered at the municipal level in parentheses. Controls (interacted with the 1857-1862 dummy) are as in Table 2, columns 4-5.. See Appendix Table A-1 for variable definition and sources.

**Table 6: Mechanisms II: Political competition, Age of municipality, Colonial institutions**

<i>Dependent variable: Incidence of battles, 1821-1885</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
	Political Comp.		Age of municipality		Colonial institut.	
	High	Low	Old	New	Yes	No
N. voters x 1857-1862	-0.00364** (0.00141)	-0.00174 (0.00174)	-0.00526*** (0.00197)	-0.000635 (0.000797)	-0.00431*** (0.00121)	0.000918 (0.00144)
Mun. & time FE	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>
Controls x 1857-1862	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>
Observations	17,420	17,420	19,890	20,280	23,205	16,965
Municipalities	268	268	306	312	357	261
R-squared	0.109	0.081	0.113	0.076	0.109	0.071

Ordinary Least Squares regressions. Robust standard errors clustered at the municipal level in parentheses. \* is significance at the 10% level, \*\* is significance at the 5% level, \*\*\* is significance at the 1% level. High and low competition and old and new municipalities are defined in terms of the median in the sample. See Appendix Table A-1 for variable definition and sources.

**Table A-1: Variables and sources**

Variable	Description	Source
No. of Battles	No. of battles municipality in each and municipality and year, 1820-1885	Riascos Grueso (1950)
New voters	Proxy for the new franchise. Single men 16+ in 1851 (21 by 1856), married men, and married slaves (as a % of total municipal population)	1851 Census
Young single free men	Single men ages 16-50 in 1851 (in %)	1851 Census
Old single free men	Single men ages 50+ in 1851 (in %)	1851 Census
Married slaves	Married slaves in 1851 (in %)	1851 Census
Married free men	Married men in 1851 (in %)	1851 Census
Pol. competition	$1 - \frac{\text{votes}_{\text{first}} - \text{votes}_{\text{second}}}{\text{votes}_{\text{first}} + \text{votes}_{\text{second}}}$ , with votes from the 1856 presidential election	Bushnell (1970)
Votes for Ospina	Share of votes for Ospina, the conservative candidate, in each municipality in the 1856 presidential election	Bushnell (1970)
Age of municipality		
New and old municipality	$\text{age} = 1856 - \text{foundation date}$ , Old municipality is > 100 years	Bernard and Zambrano (1993)
Colonial Institutions		
	State Capacity Index (0-4): count of tobacco <i>estanco</i> , <i>aguardiente estanco</i> , mail service and <i>alcabala</i> .	García-Jimeno (2005) based on Durán y Díaz (1794)
Good and bad institutions 1863 increase & 1863 decrease	Good institutions if state capacity > 0 (median is 0) Dummy=1 if 1863 State Constitution increased (decreased) franchise in State <i>s</i> in 1863, and zero otherwise*	1863 State Constitutions
Distance to royal roads	Computed as the distance from the centroid of each municipality to the nearest royal road	M. A. Bautista based on IGAC (2002)
Altitude	Altitude above sea level, in meters, of the urban center of each municipality	CEDE, Universidad de los Andes
Rainfall	Mean annual rainfall level in each municipality	CEDE, Universidad de los Andes
Density of primary, secondary and tertiary rivers	Quotient between the longitude of each kind of river in meters and the surface area of each municipality in square kilometers	Colombian Geographical Institute (IGAC)
Number of colonial functionaries	Full account of the Colonial State bureaucracy, coding all the crown employees in each city, villa or town; match to the municipalities in our sample	García-Jimeno (2005) based on Durán y Díaz (1794)

\*States were Antioquia, Bolívar, Casanare (Boyacá) (1863 decrease=1), Cauca, Cundinamarca, Magdalena (1863 increase=1), Santander, and Tolima (1863 decrease=1).