Ballots instead of Bullets?

The effect of the Voting Rights Act on political violence

Jean Lacroix

The extension of voting rights epitomizes the construction of modern democracies. This paper empirically investigates the effect of such an enfranchisement on political violence in the context of the US Voting Rights Act (VRA) of 1965, which forbade discrimination in voting. The formula the VRA used to determine the counties it applied to generated both geographic and temporal local discontinuities in enfranchisement. This paper's empirical strategy takes advantage of these features by comparing the evolution of political violence in geographically close covered and non-covered counties. Difference-in-differences estimates indicate that VRA coverage halved the incidence and the onset of political violence. Additional empirical evidence implies that voting became the new institutionalized way to state political preferences. Indeed, VRA coverage mostly decreased electoral and small-scale strategic violence. This result is not explained by disaggrievement. Extensions suggest that new strategies of political action may explain a decrease in violence after enfranchisement.

Keywords political violence, enfranchisement, Civil Rights Movement

JEL Classifications D74, N42, H89

CEBRIG Working Paper N°20-007

June 2022

Université Libre de Bruxelles - Solvay Brussels School of Economics and Management
Centre Emile Bernheim de Recherche Interdisciplinaire en Gestion
ULB CP114/03 50, avenue F.D. Roosevelt 1050 Brussels BELGIUM
cebrig@ulb.be - Tel : +32 (0)2/650.48.64

Ballots instead of Bullets? The effect of the Voting Rights Act on political violence*

Jean Lacroix †

Abstract

The extension of voting rights epitomizes the construction of modern democracies. This paper empirically investigates the effect of such an enfranchisement on political violence in the context of the US Voting Rights Act (VRA) of 1965, which forbade discrimination in voting. The formula the VRA used to determine the counties it applied to generated both geographic and temporal local discontinuities in enfranchisement. This paper's empirical strategy takes advantage of these features by comparing the evolution of political violence in geographically close covered and non-covered counties. Difference-in-differences estimates indicate that VRA coverage halved the incidence and the onset of political violence. Additional empirical evidence implies that voting became the new institutionalized way to state political preferences. Indeed, VRA coverage mostly decreased electoral and small-scale strategic violence. This result is not explained by disaggrievement. Extensions suggest that new strategies of political action may explain a decrease in violence after enfranchisement.

Keywords: political violence, enfranchisement, Civil Rights Movement.

JEL Codes: D74, N42, H89

*I would like to thank Toke Aidt, Alan de Bromhead, Vanessa Boese, Carles Boix, Micael Castanheira, Mathieu Couttenier, Quentin David, Sergio Espuelas, Martin Gassebner, Lennart Kaplan, Patrick Kuhn, Claire Lelarge, Pierre-Guillaume Méon, Kim Oosterlinck, Arianna Ornaghi, Sophie Panel, Mathieu Parenti, Vincent Pons, Khalid Sekkat and Ebonya Washington. I would also like to thank participants in the Economics and Politics Workshop in Lille, in the 2021 EPCS conference, in the PEDD conference (Münster), in the EPSA 2019 annual conference, in the 2019 Beyond Basic Questions workshop, the ADRES 2020 Conference and participants in seminars at the Paris School of Economics (RUES Seminar), DIAL-Paris Dauphine, Université Paris-Saclay, Université de Picardie, Université Panthéon-Assas, ECARES - Université libre de Bruxelles, CEB- Université libre de Bruxelles, Universitat de Barcelona, Ecole Normale Supérieure de Lyon, University College Dublin and Queen's University Belfast for useful comments and suggestions. I acknowledge financial support from the Fonds National de la Recherche Scientifique (FNRS). All errors are my own.

†jean.lacroix@universite-paris-saclay.fr, RITM, Université Paris-Saclay, 54 boulevard Desgranges, 92330 Sceaux.

1 Introduction

In his "The Ballot or the Bullet" speech, Malcolm X stated: "If we don't cast a ballot, it's going to end up in a situation where we're going to have to cast a bullet. It's either a ballot or a bullet."[1] A few years before this speech, Martin Luther King, who is known for his nonviolent activism, declared: "Give us the ballot, and we will transform the salient misdeeds of bloodthirsty mobs into the calculated good deeds of orderly citizens."[2] In the absence of equal voting rights, violence might erupt – as a way to intimidate disenfranchised citizens, as a way for these citizens to express their opinions and as a way to counter this expression. In this context, President Lyndon B. Johnson signed the Voting Rights Act (VRA) on August 6, 1965, to end discrimination in voting. At the end of the 1960s, political violence, however, remained a major issue in the US.[3] This casts doubt upon the effect of the VRA on political violence and raises the following questions: Does enfranchisement temper political violence? If enfranchisement does do so, what explains this relationship?

The effect of enfranchisement on political violence remains to be understood (Blattman and Miguel, 2010; Schwarzmantel, 2010). On the one hand, enfranchisement suppresses some motives to use political violence. It directly widens the representation of the public in the voting process, thereby addressing the demands of the newly enfranchised population (Acemoglu and Robinson, 2001). Enfranchisement also translates into redistributive and growth-enhancing policies (Meltzer and Richard, 1981; Lizzeri and Persico, 2004; Cascio and Washington, 2013). As a result, the opportunity cost of political violence is higher, grievances of the newly enfranchised population are lower, and political violence decreases (Collier and Rohner, 2008).

On the other hand, the post-enfranchisement political equilibrium may increase elites' incentives to compensate for their loss of power with political violence (Acemoglu and Robinson, 2008; Larcinese, 2017). The newly enfranchised may also turn violent if they do not reap the benefits they expected from enfranchisement (Finkel et al., 2015). These different theories emphasize the incentives to spur or temper violence as a result of changing policies due to enfranchisement (Reynal-Querol, 2002). Considering the different mechanisms they present, it remains unclear if enfranchisement reduces or, in fact, increases political violence.

On top of these indirect effects, enfranchisement also directly changes "opportunity structures" (Gleditsch and Ruggeri, 2010; Chacon et al., 2011). In other words, it might shift the equilibrium from a state in which violence is used to intimidate potential voters and to

[1]Quote from Malcolm X speech, "The Ballot or the Bullet," Cleveland, April 3, 1964.

[2]Speech before the Lincoln Memorial at the March on Washington, May 17, 1957.

[3]Examples include the Long Hot Summer of 1967, the protests of 1968 and the Martin Luther King assassination riots of 1968.

voice preferences to a state in which voting rights are guaranteed, and voting is used to voice preferences. Previous works have developed the concept of such a self-enforcing democracy theoretically but have provided little empirical evidence (Fearon, 2011; Przeworski, 2015). Two reasons may clarify this lack of empirical evidence. First, enfranchisement is an endogenous process likely caused by violence or by structural changes related to violence (Acemoglu and Robinson, 2001; Lizzeri and Persico, 2004; Aidt and Franck, 2015). Second, enfranchisement often parallels the many reforms inherent to democratic transitions. The effect of enfranchisement has never been isolated from any of these related changes (Reynal-Querol, 2002; Sunde and Cervellati, 2013). As a result, both a possible causal link running from enfranchisement to a decrease in political violence and the mechanisms behind this association remain unknown.

This paper devotes particular attention to these two elements. First, it takes advantage of both geographic and time local variation in the re-enfranchisement created by the VRA. Therefore, it isolates the effect of this re-enfranchisement on violence from any other parallel phenomenon.[4] Second, it uses information on instances of political violence to document a direct effect of enfranchisement on violence. Documenting this mechanism empirically complements previous studies focusing on redistribution, welfare or electoral results as transmission channels of the effect of enfranchisement on political violence (Besley et al., 2010; Rohner and Saia, 2020). In comparison, this study does not infer the motivations and dynamics of political violence from local economic conditions but directly uses this information from an event-level dataset.

The VRA forbade discrimination in voting, but all jurisdictions were not equally covered. Covered jurisdictions had to suspend any device limiting registration (Section 4.a of the VRA)[5], needed federal preclearance before any change in voting practices (Section 5 of the VRA) and could have federal officials register voters in their county (Section 6 of the VRA). Section 4.b of the VRA set up a detailed coverage formula: Political subdivisions with turnout/registration rates below 50 percent and maintaining a device limiting registration were covered by these special provisions. These criteria created a quasi-random assignment of VRA coverage at the local level.[6] Using this local variation, it is possible to neatly assess

[4]Black citizens were formally enfranchised by the 15$^{\text{th}}$ Amendment at the end of 19$^{\text{th}}$ century. Access to voting was later restricted. The VRA forbade these restrictions. For simplicity, I will refer to this re-enfranchisement or *de facto* enfranchisement as enfranchisement in the rest of the paper.

[5]The 1970 amendment of the VRA expanded this provision to the whole country.

[6]As a result, some counties having a device limiting voters' registration but registration rates above 50 percent were not covered (e.g. 60 counties in North Carolina). This offers randomness in the assignment of the treatment. For example, in 1970, the list of covered jurisdictions was updated according to 1968 registration rates, resulting in the partial coverage of California, Connecticut, Maine, Massachusetts and Wyoming. The quasi-random treatment in 1965 emerges as the turnout criterion varies on two levels. First, only counties experiencing a turnout above 50 percent and belonging to a state with turnout above 50 percent remained

the impact of enfranchisement on political violence. To do so, this study compares the evolution of political violence following the VRA in counties lying on either side of the border between covered counties (the treated group) and non-covered counties (the control group) in a difference-in-differences setting.

Focusing on the VRA has several merits. First, it avoids multiple identification issues by isolating the effect of enfranchisement on political violence from any parallel phenomena. Counties from one side or the other of the border between covered and non-covered counties experienced the same level and the same trend in political violence before the VRA passed. Moreover, federal officials were sent to covered areas, thereby insulating enfranchisement from intimidation and voter suppression. Second, this paper does not identify enfranchisement through registration rates or political participation. This relaxes the assumption that legal enfranchisement results in higher political participation or has a functional relationship with turnout. This, then, complements papers assessing enfranchisement that use measures of its success, such as the number of new voters (Berlinski and Dewan, 2011; De Bromhead et al., 2020), or that focus on post-enfranchisement specific processes such as elections (Cederman et al., 2013; Fetzer et al., 2018; Rohner and Saia, 2020).[7] Third, the VRA took place in the midst of a violent era in the US, offering enough variation to observe its effect on violence. Fourth, the VRA presents an interesting historical case challenging usual mechanisms explaining how enfranchisement influences violence. The first presidential election after the reform benefited the Republican candidate (Richard Nixon) and a pro-segregation candidate (George Wallace). Previous research suggests that this fact is not anecdotal and that the VRA did not shift the median voter towards more redistributive policies (Fresh, 2018; Ang, 2019). Following the VRA, a dealignment process decoupled policy preferences from income-related issues towards race-related issues (Kuziemko and Washington, 2018). This paper investigates how this dealignment process might have decreased violence as citizens no longer turned to violence to express their views on race, relying instead on the ballot.

According to baseline results, the VRA halved both the number of instances of political violence and the likelihood of the onset of new waves of political violence in covered counties. These results are robust to using alternative estimation methods. Additional results using event-level data suggest that enfranchisement may directly decrease violence. After enfranchisement, the correlation between political violence and peaceful protests is stronger, suggesting that enfranchisement decreased the strategic use of violence but did not decrease

uncovered. Consequently, some counties with high turnout rates were covered because they belonged to a state with turnout below 50 percent. Second, when county-level data were missing, coverage was defined using the registration rate, resulting in some counties being covered and others not depending on data availability.

[7] If former elites use violence to suppress newly enfranchised voting, the number of new voters might be endogenous to violence. Similarly, estimates focusing on elections period provide information on specific mechanisms but not on the overall effect of enfranchisement.

the escalation of violence during peaceful protests. It decreased both violence claiming desegregation and violence from supporters of segregation policies. This evidence suggests that enfranchisement decreased the returns from using violence to interfere with policymaking. As voting becomes competitive and accessible, citizens divest from violence and invest in voting.

This paper contributes to three strands of literature. First, it takes part in the debate on the consequences of the VRA (Besley et al., 2010; Cascio and Washington, 2013; Schuit and Rogowski, 2017; Fresh, 2018; Kuziemko and Washington, 2018; Bernini et al., 2018; Ang, 2019; Facchini et al., 2020). These previous studies mainly focused on the effect of the VRA on voting behavior and representation in the long run. The present study considers the interplay between access to voting and political violence in the shorter run. Second, it uses new methods to provide evidence of the impact of electoral institutions on violence. While previous studies mainly provide evidence of the impact electoral institutions have on violence at a macro level (e.g., studies that look at democratic institutions as a whole, as in: Hegre 2001; Collier and Rohner 2008; Sunde and Cervellati 2013; Hegre 2014), the empirical setting of this study isolates the effect of enfranchisement on political violence. Third, this paper directly contributes to the growing literature on the aftermath of enfranchisement (Aidt et al., 2010; Berlinski and Dewan, 2011; Bernini et al., 2018; Larcinese, 2017; Corvalan et al., 2020; Aidt et al., 2020; Rohner and Saia, 2020). Previous studies mainly focused on the political outcomes of enfranchisement (e.g., representation or electoral results) or on the long-term economic implications of enfranchisement. This study does not assume that the effect of enfranchisement on political violence is transmitted via its effect on electoral outcomes or on growth. Rather, it identifies how enfranchisement changes the strategic use of political violence as opposed to voting because of changing opportunity structures. Documenting this causal chain adds on to previous studies that either do not focus on political violence or that link the effect of enfranchisement on violence to its effect on economic activity.

2 How does enfranchisement influence the use of political violence?

This section distinguishes two sets of mechanisms explaining how enfranchisement may decrease political violence: the indirect effects, which captures the effect of policy changes and the direct effects, which captures changes in the strategic use of violence.

2.1 How enfranchisement influences violence: Indirect effects

Policies results from the mitigation of all enfranchised persons' preferred policies (Besley and Coate, 1997). After enfranchisement, a new group becomes a part of this mitigation process, and the policy maker must take this group's preferences into account (Fearon, 2011). Enfranchisement, therefore, increases the representation of newly enfranchised minorities (Pande, 2003). Documenting this effect, Bernini et al. (2018), Marschall et al. (2010) and Shah et al. (2013) show that the VRA increased the representation of Black voters. Should the preferences of the newly enfranchised be at odds with those of former elites, enfranchisement changes policies. This would induce changes in a wide array of policies such as redistribution (Meltzer and Richard, 1981; Husted and Kenny, 1997) and investment in primary education (Gallego, 2010). Such policy shifts decrease violence in two ways. First, the formerly disenfranchised face policies closer to their preferred ones. Passarelli and Tabellini (2017) show that as policies become closer to what citizens consider fair, they tend to no longer have specific policy grievances and abandon violence. Second, enfranchisement increases income and public spending, thereby increasing the opportunity cost of conflict (Collier and Rohner, 2008; Collier et al., 2009; Aneja and Avenancio-Leon, 2019; Rohner and Saia, 2020). In a booming economy, the economic returns from conflict are limited. As a consequence, political representation suppresses rent-seeking motives to use political violence (Reynal-Querol, 2002; Saideman et al., 2002; Besley and Persson, 2011). This reduces the cost of voting relative to political violence, resulting in a new way to settle on policies (Fergusson and Vargas, 2013). By contrast, enfranchisement potentially fuels violence should former elites resort to violence to compensate for their loss in de jure power (Acemoglu and Robinson, 2001; Larcinese, 2017; Acemoglu and Robinson, 2006, 2008) or groups resort to old divides to reap the benefits of the reform (Amodio and Chiovelli, 2018).

Elites may also non-violently block reforms and political changes after the extension of voting rights (Carvalho and Dippel, 2020). This mechanism may explain why the effect of enfranchisement on political outcomes does not always materialize (Berlinski and Dewan, 2011; De Bromhead et al., 2020). Consequently, the effect of enfranchisement on policies and redistribution also has to be questioned (Aidt et al., 2010, 2020; Corvalan et al., 2020).

2.2 How enfranchisement influences violence: Direct effects

Nevertheless, strategic considerations alone may impact violence after enfranchisement. When voting is not accessible, political violence may be a substitute to influence policies (Lipsky, 1968; Cederman et al., 2013; Little et al., 2015; Battaglini, 2017; Leventoğlu and Metternich, 2018). Enfranchisement changes the strategic motivations to use violence for both the previously enfranchised and the newly enfranchised. When one group is discriminated against in the voting process, that group is discouraged from investing its resources in voting, as is the rest of the population. The privileged win the voting contest with minimal investment, whereas the discriminated group does not invest in voting because it is sure of losing the ballot. This is a manifestation of the "discouragement effect" (Harris and Vickers, 1985, 1987; Konrad, 2012; Acemoglu and Robinson, 2017). In parallel, both groups have incentives to invest in fairer contests to influence policies, such as violence. After enfranchisement, voting becomes fairer, and both groups divert their resources from violence to voting. Voting ultimately becomes the institutionalized way for all to influence policies when voting becomes fair (Przeworski, 2015).

Enfranchisement changes the complementarity between violence and voting by changing the "political opportunity structure": decreasing the prospects of influencing policies using violence relative to voting (Gleditsch and Ruggeri, 2010; Chacon et al., 2011). Given this mechanism, enfranchisement decreases violence the most when its effects on voting are large; typically during electoral periods (Harish and Little, 2017). The VRA ensured the registration of numerous voters, thereby narrowing complementarities between voting and violence. First, it limited the prospects of targeted electoral violence. With a low number of enfranchised Black citizens, such attacks could be sufficient to suppress voting. As the number of newly enfranchised increases, these intimidation practices are less efficient. Second, the VRA limited the possibility to interfere with the registration process as the federal state directly intervened to guarantee the registration of voters.

To conclude, the "indirect" mechanism relates to disaggrievement. Following this view, enfranchisement decreases violence only for some groups – aligned with post-enfranchisment policies. This mechanism explains a decrease in violence over time as the effects of new policies materialize. Disaggrievement following policy changes would also affect peaceful protests and large protests turning violent. The "direct" mechanism, by contrast, would mostly decrease violence in electoral times, as enfranchisement mainly impacts complementarities between violence and voting around elections. This mechanism would not only affect some groups pleased by policy changes but all citizens. Likewise, should the "direct" mechanism hold, violence would decrease but complements of voting – such as peaceful protests, would not.

3 Historical background

3.1 Political violence in the US

Most historical accounts report the intertwined nature of violence and voting in the 1950s and 1960s US. Political violence is deeply rooted in US history. The 1961 US Commission for Civil Rights (USCCR) report on voting notes that already “the elections in 1878 and 1884 were marked by rioting and violence” (USCCR, 1961, p.40). Violence was used by white supremacists to ensure a political and social status quo. As stated in the 1959 report of the USCCR, “What fraud could not do, violence accomplished” (USCCR, 1959, p.46). Violence prevented Black citizens from registering and voting. It was also used to protest desegregation policies. The 1961 USCCR report on voting states “mob violence has erupted several times” in response to the recognition of civil rights (USCCR, 1961, p.25). The 1959 report also indicates the racial tensions following the integration of Black citizens in formerly segregated neighbourhoods (USCCR, 1959, p.536). This violence was sometimes strategically encouraged by organizations such as the Ku Klux Klan, although the USCCR also reports spontaneous outbursts of racial violence. Finally, the 1990 USCCR report on “Intimidation and Violence: Racial and religious bigotry in America” depicts the various forms such violence took throughout US history: “cross burnings, defacement, destruction, desecrament of religious property, infliction of personal injuries, and in some cases, the death of human beings” (USCCR, 1990, p.1).

Violence also emerged from claims for desegregation and to fight the political and social status quo. Debates on the role of violence in the struggle for civil rights is well illustrated by the positions of Martin Luther King and Malcolm X.[8] Even if Martin Luther King rejected violence as a political strategy, he mentioned the denial of voting rights as a possible explanation of violence. Finally, the interactions of pro-segregation and anti-segregation movements led to an escalation of violence with no clearly identifiable instigators. Such escalation could be a motivation by itself, as emphasized by Malcolm X in his “Communication and Reality” speech in 1964: “I am not against using violence in self-defense. I don’t call it violence when it’s self-defense, I call it intelligence.”[9] Most of these events did not lead to massive property destruction or to casualties although some acts of violence were quite destructive (Collins and Margo, 2007). The political dimension of this type of violence has notably been emphasized by Button (1978, p.157), who qualifies some of the violence experienced in the 1960’s as “politically purposeful”. Button (1978, p.173) also reports a poll showing that violence was seen as necessary to observe changes in society. These intuitions have been confirmed by

[8] See citations in introduction of this article.

[9] Speech to Peace Corps Workers, December 12, 1964

more recent studies documenting the political consequences of protests and riots in the Civil Rights era (Mazumder, 2018; Wasow, 2020).

Segration was not the only factor explaining violence. Additional causes of violence include notably the following: spatial factors and demographics (Spilerman, 1970; Mazur, 1972), ethnic competition for resources and geographic diffusion (Myers, 1997) and economic deprivation mixed with ethnic competition (Olzak et al., 1996). The Dynamics of Collective Action dataset (McAdam et al., 2003) reports additional motives to use violence (even if less frequent in the geographic area on which the present study focuses). This includes social movements, antiwar protests that sometimes turned violent and a broader recognition of civil rights (such as gay rights). Section 4.1 describes in more details what the dataset of this study records as political violence in the sample.

3.2 Voting Rights Act

At the end of the 19$^{\text{th}}$ century, the US Congress declared the equality of all citizens before the law, including electoral laws. As a consequence, the US implemented universal male suffrage without any racial distinction. To circumvent federal laws, some southern states passed Jim Crow laws, which were formally compliant with federal laws but de facto enforced racial segregation (Davidson and Grofman, 1994). For example, they included the requirement of literacy tests, voting taxes or the publication of registration lists. Public registration lists made coercion and threats against Black voters easier, while the implementation of tests and taxes provided local politicians with a discretionary power to systematically disenfranchise Black individuals. The coverage formula of the VRA imperfectly attempted to find and address these loopholes.

The final form of the VRA, its timing and coverage remained unknown until a few days before its signing. President Lyndon B. Johnson simply mentioned "the elimination of barriers to the right to vote" in his 1965 State of the Union Address. His staff was nevertheless skeptical about legislating on voting rights (May, 2013, Chapter 4). In the House Chamber, the Republican Congressman from Ohio, William McCulloch, presented an alternative piece of legislation that would considerable diminish the enfranchisement of Black voters. This alternative was voted out only on July 9, 1965. The VRA was eventually adopted on August 4, 1965 and signed by President Johnson on August 6, 1965. Its Section 2 states its global objective: "No voting qualification or prerequisite to voting, or standard, practice, or procedure shall be imposed or applied by any State or political subdivision to deny or abridge the right of any citizen of the United States to vote on account of race or color."

Immediately after the VRA was signed, US Attorney General Nicholas Katzenbach filed a lawsuit against Mississippi to abolish state polling taxes, and federal examiners were sent to covered states (May, 2013, Chapter 7). The registration campaign was a success as many Black voters went en masse to registration centers. In one of its reports, the USCCR stated that in October 1965, federal examiners already registered 56.789 Black voters in places covered by the VRA. To this number should be added the number of Black voters registered thanks to the VRA by local civil servants (USCCR, 1965, p.2). The same report mentions that just three months after the VRA passed, "in many areas of the South, there is full compliance with the Act." Previous research has shown the big push in registration and turnout generated by the VRA (Fresh, 2018; Ang, 2019). Figure 1 illustrates this overall success of the VRA and shows the distribution of turnout rates in VRA counties (left panel) and in other counties (right panel) in the 1964 (in red) and 1968 (in blue) presidential elections. While the right panel does not show any major evolution of turnout in non-VRA counties, the left panel shows that the distribution of turnout shifted to the right in VRA counties. Consequently, the difference in turnout between VRA counties and others has been divided by two between the two elections (Table 1).

Figure 1: Distribution of turnout rates (x-axis) in VRA and other counties (1964 & 1968 US presidential elections)

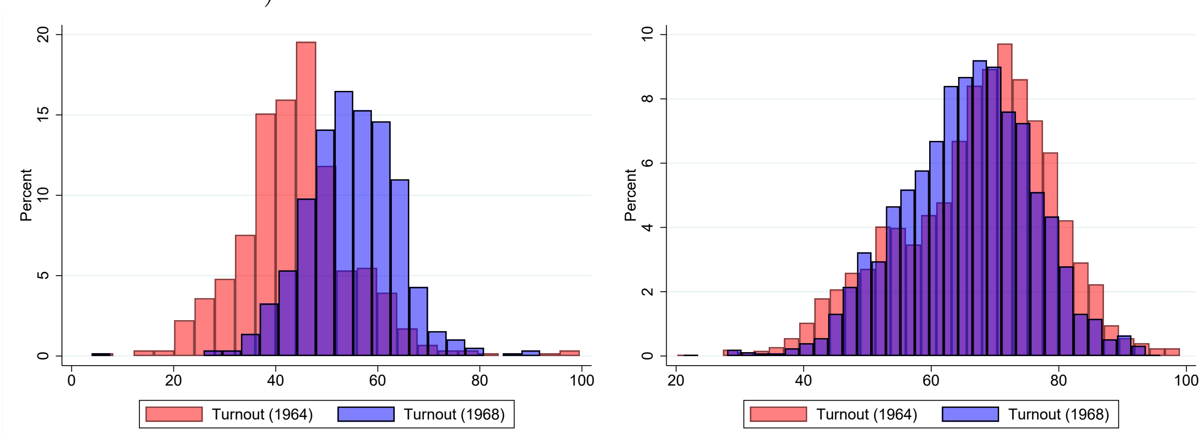


Table 1: Turnout rates in VRA and other counties (1964 &1968 US presidential elections)

Turnout rate	VRA	Others	Diff
1964	44.21	66.99	-22.78***
1968	54.83	65.69	-10.85***

To counter the inevitable enfranchisement of Black voters, politicians also encouraged the registration of white non-registered voters. Davidson and Grofman (1994, p.39), indeed, report the effort of Alabama Governor George Wallace to run a campaign to register white voters to counter the mobilization of Black voters. Focusing on Alabama, they observed that because of this endeavor “in absolute numbers the new white voters actually outstripped new Black voters: 276.622 registered between 1964 and 1967.” Along the same lines, Fresh (2018) estimates that VRA coverage increased white voters’ registration by 10 to 13 percentage points. All in all, VRA coverage did increase the registration of Black voters despite some efforts to block this process. In response to this unavoidable enfranchisement, white voters also started to register (Timpone, 1995; Valelly, 2009; Fresh, 2018; Ang, 2019). The fact that VRA coverage increased registration and voting among white voters suggests a shift in the way politics was performed after the VRA. Voting became a central political strategy – and not just for the newly enfranchised.

4 Data and method

The dataset uses geo-localized information on violence from the Dynamics of Collective Action dataset (McAdam et al., 2003). Each observation in the dataset contains information at the county-month level. The dataset records observations from August 1960 to August 1970 (as the coverage of the VRA changed in that year). The pre-treatment period and post-treatment period are then of the exact same length (60 months). A county belongs to the sample used in the baseline estimation if its centroid is less than 100 kilometers from the border between areas covered by the VRA and areas not covered by the VRA.[10]

4.1 Political Violence

The Dynamics of Collective Action dataset (McAdam et al., 2003) contains reports of public collective actions mentioned in the daily edition of the *New York Times*.[11] Because of the specificity of this source, the dataset likely underreports the number of actual collective actions.[12] It still has several merits: (1) the dataset captures events resonating at least at

[10] Appendix A.1 illustrates how the restriction of the sample to counties near the treatment border increases the comparability of treated and control counties.

[11] The McAdam et al. (2003) dataset records events if (1) more than one person engage in the activity, (2) the event is public, (3) the event is a protest event, and (4) participants have a grievance or claim to change society. Importantly for the context of this study, ethnic and racial conflicts are coded in the dataset even if they do not fulfill all the criteria mentioned above. The only criterion applying to the coding of ethnic and racial conflict is the presence of evidence that the actions was motivated by prejudice (McAdam et al., 2003).

[12] Such a bias in reporting likely did not negatively correlate with VRA coverage. Hence, it likely does not explain baseline results. It most likely generates an attenuation bias of the results.

the national level; (2) information from the *New York Times* is less prone to manipulation from local politicians and activists than information from local newspapers, and (3) using an unique source limits the source of variation in reporting both in space and time. From the McAdam et al. (2003) dataset, I define instances of political violence as events either (1) tagged as violent in the dataset or (2) referred to as violent in the *New York Times* and resulting in casualties. As a result of these definitions, this study considers events sharing three common characteristics: they are collective, have a political motivation and are violent. [13]

Figure 2 distinguishes large-scale events, such as riots and rallies, accounting for almost half of the events in the dataset (in various shades of grey) and lower-scale events.[14] Figure 2 presents information on the motivations to use violence. In the main sample of the analysis, 318 instances of political violence were recorded, of which 277 were related to segregation.[15] Among these 277 related to segregation, anti-segregation motives are as frequent as pro-segregation motives.

Each observation records the number of instances of political violence at the county-month level. Instances of violence (the number of events recorded as political violence) and onset of violence (the start of a new wave of political violence) are alternately used as the dependent variable. The variable $Onset_{i,t}$ is equal to one if violence occurred in county i at time t and no violence occurred in county i at time $t-1$ (see Hegre and Sambanis, 2006). Using this variable as the dependent variable ensures that baseline results do not emerge because of serial correlation and intuitively tests the effect of enfranchisement on the extensive margin of conflict. In the sample, 102 counties experienced at least one instance of political violence.

[13] Violence is here defined as physical violence against others or the violent destruction of properties and involves groups of individuals affiliated to some "social movement organizations" or not. As a consequence, violence from state officials is not included in the dataset.

[14] The distinction between smaller scale and larger scale events matches the information on the number of instigators for each event. 109 events involved fewer than 10 people, 40 involved groups of 10 to 49 people, 31 involved groups of 50 to 99 people, 110 involved hundreds of people and 28 involved thousands of people. The *New York Times* mentions an organization for 104 events.

[15] An event is defined as related to segregation in the dataset if the *New York Times* article directly refers to segregation, civil rights organizations or white supremacist groups. See Table 3 for estimations using this distinction.

Figure 2: Political violence - Event types

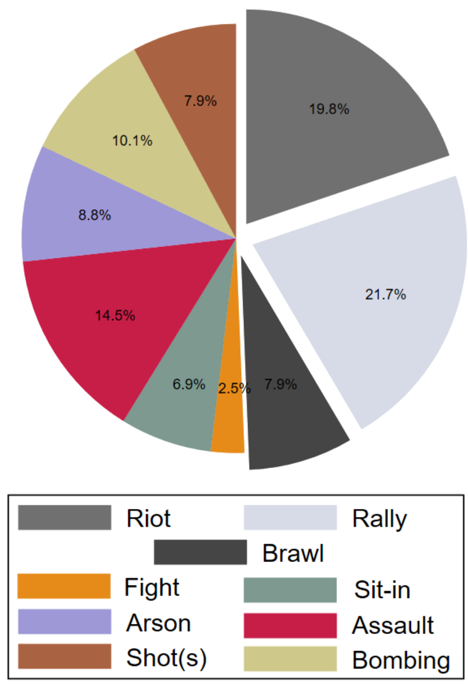
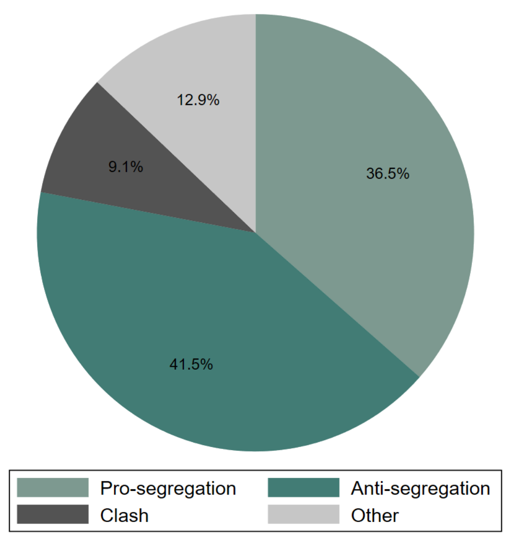


Figure 3: Political violence - Motives



4.2 VRA's coverage formula: Identification strategy

The identification strategy of this paper relies on the coverage formula of the 1965 VRA, Section 4(b). The formula defined two necessary criteria for a county to be covered. First, it had to use a test or device limiting registration. Second, its turnout/registration rate had to be below 50 percent in the November 1964 presidential elections. The second criterion was also defined at the state level: If a jurisdiction pertained to a state with a turnout/registration rate below 50 percent, it also fulfilled the second criterion. Because of this rule, state borders in most cases defined treatment regardless of county characteristics across these borders. For example, some counties with a turnout rate above 50 percent were covered because they belonged to a state maintaining a mechanism limiting registration and having a turnout rate below 50 percent. By contrast, some counties with mechanisms limiting registration were not covered if they and their states had a turnout/registration rate above 50 percent.

As an illustration, the USCCR mentioned in its report that the Attorney General set a list of 21 states maintaining devices and tests limiting voter registration (USCCR, 1965). Because of the turnout criterion, only 7 of these states were at least partially covered by the VRA. The treatment variable equals one if a county fell under VRA coverage from August 1965. As defined in Section 4(b) of the Act, it applies to counties in Alabama, Georgia, Louisiana, Mississippi, South Carolina and Virginia, as well as to 40 counties in North Carolina. Figure 4 displays the geographic distribution of political violence both before and after the VRA was adopted. Figure 5 focuses on the covered region: areas experiencing numerous episodes of political violence existed on both sides of the border between counties covered by the VRA in 1965 and those not covered.

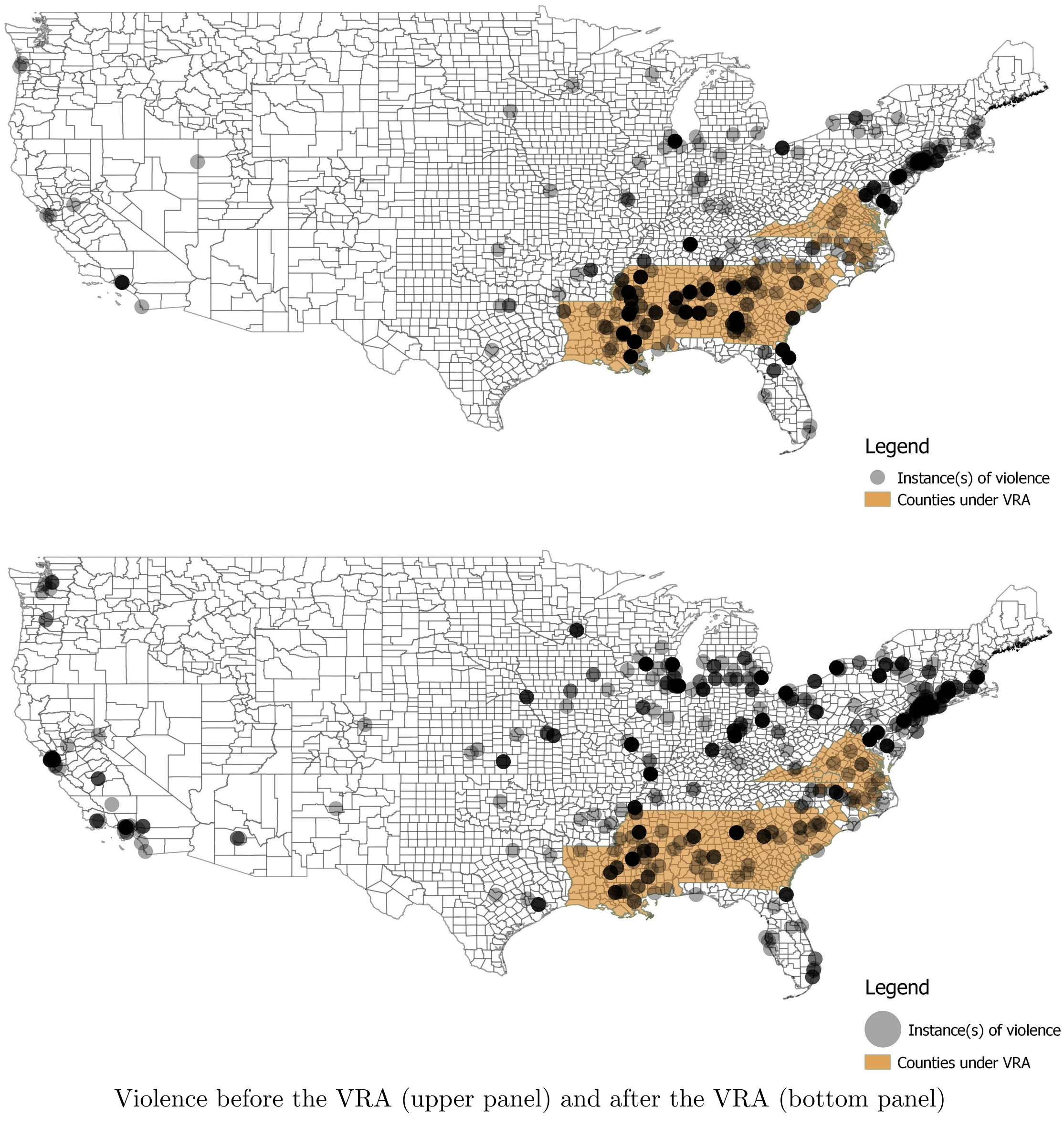
4.3 Estimation

Estimating Equation 1 for counties having the same *ex ante* probability of experiencing political violence provides an assessment of the average treatment effect of VRA coverage on political violence. Because the number of instances of political violence is a count variable, Equation 1 is estimated using a conditional fixed effects Poisson model. Equation 1 reads:

$$Pr(Viol_{i,t} = Y \mid Viol_{i,t-1}, VRA_{i,t}, \phi_t + \eta_i) = f(\alpha, \beta_1 Viol_{i,t-1}, \beta_2 VRA_{i,t}, \phi_t, \eta_i, \varepsilon_{i,t}) \tag{1}$$

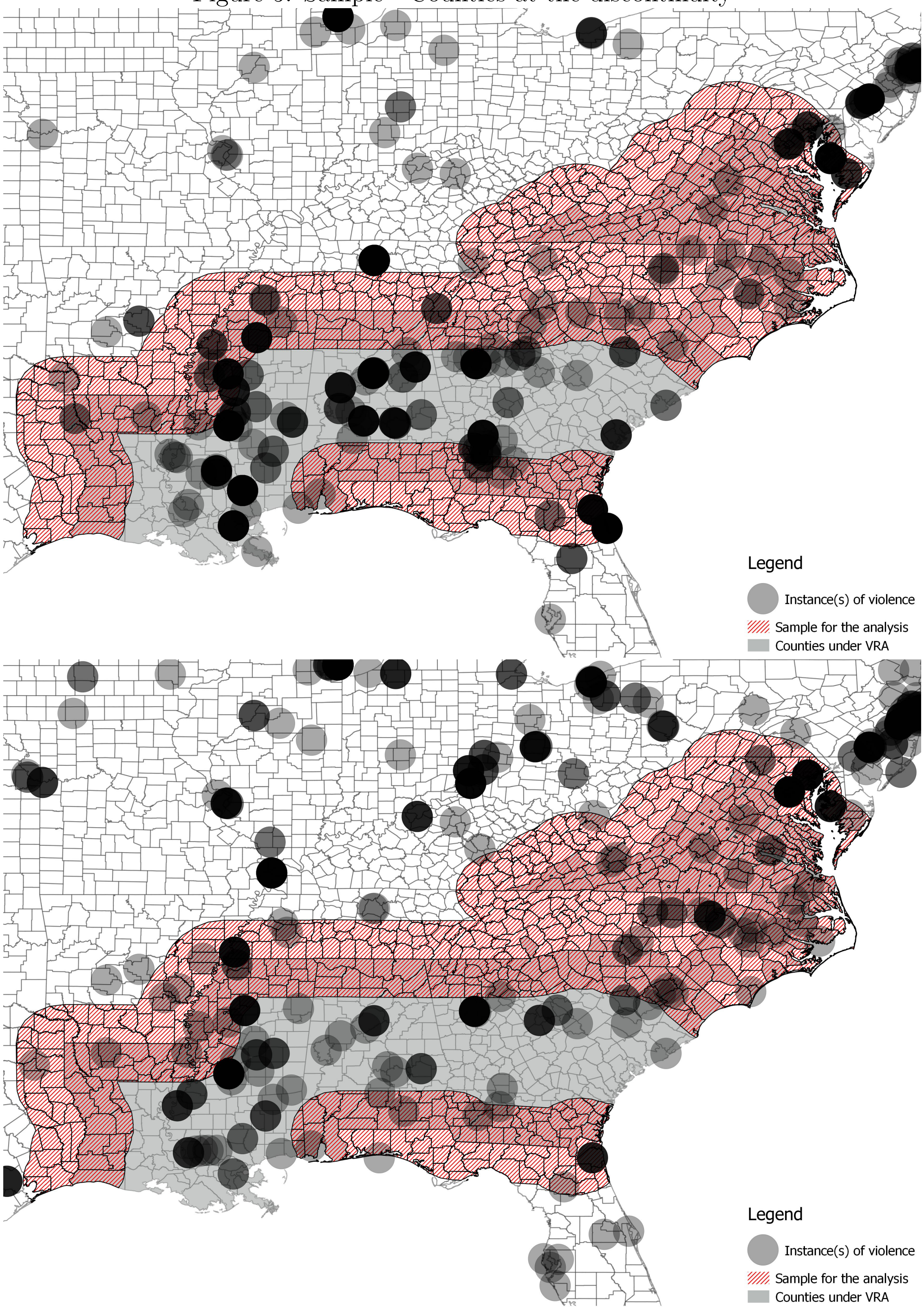
This equation sets up a difference-in-differences setting where: $VRA_{i,t}$ is a dummy variable

Figure 4: Political violence and the VRA in the US



Violence before the VRA (upper panel) and after the VRA (bottom panel)

Figure 5: Sample - Counties at the discontinuity

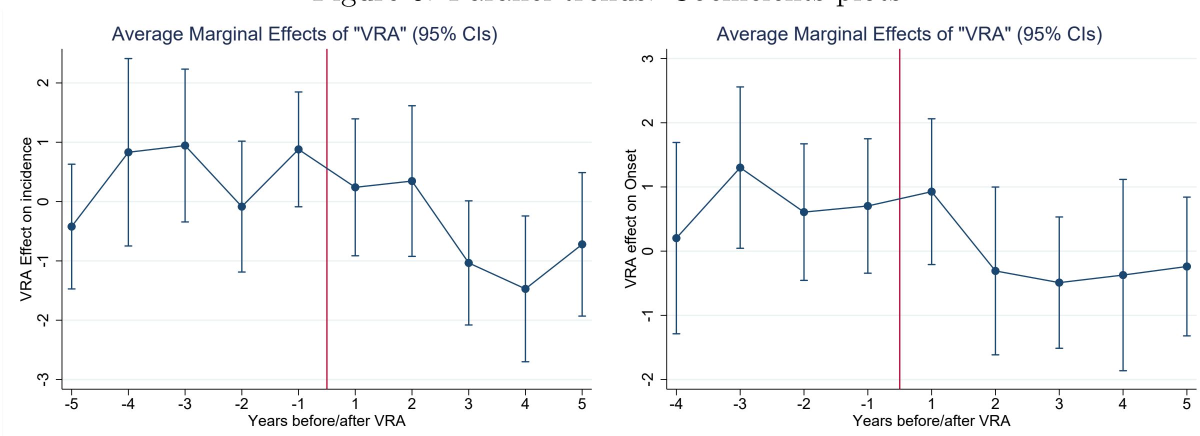


Violence before the VRA (upper panel) and after the VRA (bottom panel). Dark pink: area covered by the VRA belonging to the sample of the empirical analysis (treatment group). Light pink: area not covered by the VRA belonging to the sample of the empirical analysis (control group).

equaling one for counties covered by the VRA from August 1965. $Viol_{i,t}$ is the number of instances of violent episodes in county i at time t or the onset of a new wave of violence in county i at time t . Lagged values of violence ($Viol_{i,t-1}$) control for any persistence in the incidence of violence. In specifications using onsets of violence as the dependent variable, observations recording positive numbers of violent events in the previous month are dropped, since by definition these observations do not experience any onset of violence. η_i are county fixed effects. These fixed effects control for the time-invariant focus of the *New York Times* on some counties as well as for all time-invariant determinants of violence. All county-specific characteristics not changing within the time period of the study are captured by these fixed effects.[16] ϕ_t are month fixed effects. These fixed effects control for the time-varying conditions explaining violence in the whole sample. α, β_1, and β_2 are coefficients; and $\varepsilon_{i,t}$ is the error term. Coefficient β_2 captures the effect of VRA coverage. Standard errors are clustered at the county level.

The difference-in-differences estimators assess the average treatment effect of the VRA under the parallel trend assumption. Figure 6 presents evidence of these parallel trends between the treated and the control group in the sample. Pre-treatment coefficients do not vary in a systematic manner and belong to a similar range. The parallel trend hypothesis is also supported by a formal test of equivalence in trends and a placebo test using alternative treatment dates (Appendix A).

Figure 6: Parallel trends: Coefficients plots



Left panel: Yearly correlation coefficients between an identifier for counties that will be (before 08/1965) or are (after 08/1965) under special provisions of the VRA and the incidence of conflict. Right panel: Yearly correlation coefficients between an identifier for counties that will be (before 08/1965) or are (after 08/1965) under special provisions of the VRA and the onset of conflict.

[16]The time-variant focus of the *New York Times* likely correlates with VRA coverage, inducing an attenuation bias of this paper's estimates.

5 Empirical results: Enfranchisement reduces political violence

5.1 Baseline results

Table 2 presents baseline results. The left panel introduces estimates of Equation 1 when using the number of instances of political violence as the dependent variable, whereas the right panel presents estimates when using the onset of political violence as the dependent variable. In each case, I estimate Equation 1 for the whole US and then for a sample of counties within a 100km, 150km, or 200km buffer around the border between covered and non-covered counties. The lagged value of violence bears a positive sign when the number of instances of political violence is used as the dependent variable.

In the left panel (Columns 2.1 to 2.4), the VRA dummy variable always bears a negative coefficient, which is significant at least at the 5-percent level. When the control group consists of all non-covered counties in the US, results show a decrease in political violence in covered counties. The magnitude of the estimates decreases as bandwidths become smaller, and treated counties and control counties become more comparable. Using a 100 kilometer buffer around the discontinuity, VRA coverage more than halved the incidence of political violence per month: The incidence rate ratio of the VRA dummy variable is equal to 0.45 (Column 2.4).[17]

VRA coverage had a similar impact on the onset of violence (Columns 2.5 to 2.8). The VRA dummy variable always bears a negative coefficient significant at least at the 5-percent level. The incidence rate ratio for the VRA dummy variable varies between 0.24 and 0.41 depending on the buffer around the discontinuity. Considering the estimators with a buffer of 100 kilometers around the border, covered counties experienced four new waves of political violence (onset), while non-covered counties experienced ten after the VRA. VRA coverage, hence, reduced both the number of instances of political violence and the number of new waves of political violence. These results do not depend on the measure of political violence or on the definition of the buffer around the treatment border. Appendix B.1 to B.3 presents similar results when using alternative estimators such as OLS, conditional Logit, estimators integrating spatial lags or estimators not subject to the bias appearing in dynamic panel data.[18] It also shows estimates collapsing observations at the year level. In all these

[17]The incidence rate ratio is the ratio of the incidence of political violence in covered counties compared to counties not covered by the VRA after controlling for time and county fixed effects.

[18]These estimators take the Nickell bias into account (see B.2). This bias is likely to be limited here since T is large (120) and only materializes in estimates using Incidence as the dependent variable as estimations using Onsets by definition do not integrate the lagged dependent variable. Both estimators without the lagged dependent variable (as in Berger et al. (2013) - see subsection B.2.1) and estimators correcting for the

Table 2: Diff-in-diff: Poisson estimates

	(2.1)	(2.2)	(2.3)	(2.4)	(2.5)	(2.6)	(2.7)	(2.8)
Dependent variable	Incidence	Incidence	Incidence	Incidence	Onset	Onset	Onset	Onset
Sample	Whole US	Within 200km	Within 150km	Within 100km	Whole US	Within 200km	Within 150km	Within 100km
Incidence$_{-1}$	0.135***	0.287***	0.389***	0.438***				
	(3.370)	(4.880)	(7.234)	(5.412)				
VRA	-1.771***	-0.993***	-0.831**	-0.800**	-1.436***	-0.930***	-0.948***	-0.889**
	(-7.676)	(-2.990)	(-2.329)	(-2.149)	(-7.704)	(-3.263)	(-3.014)	(-2.365)
IRR	0.17	0.37	0.44	0.45	0.24	0.39	0.39	0.41
Control Incidence$_{-1}$	Yes	Yes	Yes	Yes	No	No	No	No
Drop if Incidence$_{-1>0}$	No	No	No	No	Yes	Yes	Yes	Yes
Observations	39,032	19,873	17,017	12,138	38,041	19,478	16,692	11,911
Number of counties	328	167	143	102	328	167	143	102

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is either the number of events considered as political violence per county-month (Incidence) or a dummy variable equal to one when a new wave of violence starts (Onset). Equation 1 is also estimated on different samples from the whole US down to counties falling into the 100km buffer around the border between covered and non-covered counties. When incidence is the dependent variable, the lagged dependent variable is added as a control. When onset is the dependent variable, observations with violence in the previous month are dropped because they, by construction, cannot record an onset of violence. All regressions include county fixed effects and month fixed effects. IRR stands for incidence rate ratio and is the ratio of the dependent variable in the treated sample compared to the control sample.

estimations, the VRA treatment variable is significant at the 10-percent level or below.

5.2 Robustness checks - Beyond state-level borders

As most of the variation used in baseline estimates occurs at the state level, I complement these first results with additional results relying on a different variation. Table 3 presents both estimates comparing counties with similar turnout ratios before treatment and estimates within North Carolina, as in Fresh (2018). As such, these estimators change the sample used to ensure that the treated and the control groups are comparable across two other characteristics: pre-treatment political participation and state policies (both pre-treatment and post-treatment). Panel A selects different samples based on counties' turnout in the 1964 election to compare some counties under VRA with other similar counties regarding the second criterion for coverage. The results are in line with baseline results. All coefficients attached to the VRA treatment variable are significant at least at the 5-percent level. According to the incidence rate ratios, VRA coverage halved the incidence of violence, in line with baseline estimates. In Panel B, the sample is North Carolina. Columns 3.B.1 and 3.B.2 estimate Equation 1 on the set of all counties in North Carolina. Columns 3.B.3 to 3.B.6 run the same estimation on a subset of counties in North Carolina having similar turnout ratios in the 1964 presidential election. In all these estimates, the VRA dummy variable is significant at least at the 10-percent level, and the incidence rate ratios are either lower or comparable to baseline estimates. Given the low number of violent events recorded in North Carolina, these results should be interpreted cautiously. Also relying on variation within North Carolina, Appendix C.1 adds state-month fixed effects to the estimation to get rid of any state-specific time varying factors and obtains similar results.

5.3 Robustness checks - Alternative treatments

Our baseline estimates consider treatment a binary variable equal to one if a county was covered by the VRA as of August 1965 without considering the aftermath of enfranchisement in those counties. As a complement, Table 4 presents triple-difference estimates. Columns 4.1 and 4.2 interact the treatment variable with the percent of Black population in 1960 to identify counties in which the reform likely had the highest impact (as in Bernini et al., 2018). Columns 4.3 and 4.4 interact the treatment variable with the ratio of turnout in the 1968 presidential elections over turnout in the 1964 presidential elections, to identify

dynamic panel data bias (see Breitung et al. (2021) - subsection B.2.2).

Table 3: Alternative identification strategies: Selection on turnout and within-state variations

Panel A: Turnout to define the control group

	(3.A.1)	(3.A.2)	(3.A.3)	(3.A.4)	(3.A.5)	(3.A.6)
Dependent variable	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$
Sample	T<55%	T<55%	40% <T<60%	40% <T<60%	45% <T<55%	45% <T<55%
VRA	-1.393***	-0.867***	-1.114***	-0.961***	-1.131**	-0.695**
	(-4.491)	(-3.102)	(-3.093)	(-3.549)	(-2.332)	(-1.990)
IRR	0.25	0.42	0.33	0.38	0.32	0.50
Control $\text{Inc}_{c,t}$	Yes	No	Yes	No	Yes	No
Drop if $\text{Inc}_{c,t-1}$>0	No	Yes	No	Yes	No	Yes
Observations	17,024	14,858	12,614	11,591	6,048	4,941

Panel B: Within North Carolina estimates

	(3.B.1)	(3.B.2)	(3.B.3)	(3.B.4)	(3.B.5)	(3.B.6)
Dependent variable	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$
Sample	Whole NC	Whole NC	30%<T<70%	30% <T<70%	T>45%	T>45%
VRA	-2.260**	-1.823*	-2.951**	-2.532**	-2.350**	-1.912*
	(-2.354)	(-1.940)	(-2.434)	(-2.131)	(-2.323)	(-1.859)
IRR	0.10	0.16	0.05	0.08	0.10	0.15
Control $\text{Inc}_{c,t-1}$	Yes	No	Yes	No	Yes	No
Drop if $\text{Inc}_{c,t-1}$>0	No	Yes	No	Yes	No	Yes
Observations	460	437	418	395	225	208

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Panel A solely uses Turnout to select the sample. Panel B uses estimation within North Carolina. The dependent variable is the number of events considered as political violence per county-month ($\text{Incidence}_{c,t}$) or a dummy variable equal to one when a new wave of violence starts ($\text{Onset}_{c,t}$). Sample: T<55% stands for turnout below 55%, 40% <T<60% for turnout between 40 and 60%, and 45% <T<55% for turnout between 45 and 55%. Whole NC stands for whole North Carolina. 30% <T<70% stands for turnout between 30 and 70 %, and T>45% stands for Turnout above 45%. All regressions include county fixed effects and month fixed effects. IRR stands for incidence rate ratio and is the ratio of the dependent variable in the treated sample compared to the control sample.

counties where voters mobilized the most via the ballot. Columns 4.5 and 4.6 interact the treatment variable with the vote share of Wallace compared to Nixon to identify places in which the dealignment process described in Kuziemko and Washington (2018) took place. All these estimators are negative and significant at least at the 10-percent level. When adding an additional variation capturing the consequences of VRA coverage on US politics, results remain similar to baseline results.

Table 4: Alternative treatment - Reform extensive margin and decrease in violence

Dep Variable	(4.1) Incidence$_{c,t}$	(4.2) Onset$_{c,t}$	(4.3) Incidence$_{c,t}$	(4.4) Onset$_{c,t}$	(4.5) Incidence$_{c,t}$	(4.6) Onset$_{c,t}$
VRA $\times$ % Black Population$_{1960}$	-0.0172**	-0.0157**				
	(-2.291)	(-2.058)				
VRA $\times$ $\frac{TurnoutPres_{1968}}{TurnoutPres_{1964}}$			-0.454*	-0.423*		
			(-1.924)	(-1.828)		
VRA $\times$ $\frac{Wallace_{1968}}{Wallace_{1968}+Nixon_{1968}}$					-1.111**	-1.190**
					(-2.244)	(-2.509)
IRR	0.98	0.98	0.63	0.65	0.33	0.30
Control DV$_{c,t-1}$	Yes	No	Yes	No	Yes	No
Drop if Inc$_{c,t-1}$>0	No	Yes	No	Yes	No	Yes
Observations	12,138	11,911	12,019	11,817	12,138	11,911
Nb of counties	102	102	101	101	102	102

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Equation 1 is estimated on a sample of counties falling into the 100km buffer around the border between covered and non-covered counties. The dependent variable is the number of events considered as political violence per county-month (Incidence$_{c,t}$ - Columns 4.1, 4.3 and 4.5) or a dummy variable equal to one when a new wave of violence starts (Onset$_{c,t}$ - Columns 4.2, 4.4 and 4.6). All regressions include county fixed effects and month fixed effects. IRR stands for incidence rate ratio and is the ratio of the dependent variable in the treated sample compared to the control sample.

5.4 Identification - Discussion

The identification strategy hypothesizes that without differences in VRA coverage, violence would have evolved similarly in covered counties and others after 1965. The identification relies on this time $\times$ space discontinuity. County fixed effects capture all remaining county-specific time-invariant characteristics and month fixed effects capture all remaining time-specific space-invariant characteristics. Hence this estimation nets out long-run differences between counties (economic, demographic, and social) and the regional time-varying prevalence of violence. The estimator may only correlate with factors evolving at the time of the reform and following the exact same border. It should, first, be noted that no other federal reforms aimed at desegregation used this exact same border. The focus on counties close to the treatment border increases the comparability of counties in the treated and in the control group and reduces the probability of differences in time $\times$ space shocks, as mentioned in other papers using a similar strategy (Aneja and Avenancio-Leon, 2019; Ang, 2019; Bernini et al., 2018; Facchini et al., 2020). By focusing on the border, I assume that differences in VRA coverage created a discrete change in time and space at the treatment border when the VRA reform took place. Other cultural, economic and political changes, however, likely did not change so abruptly around the treatment border at the time of the VRA reform. The identification of the paper still relies on two specific assumptions. First, covered counties did not suffer from selection into treatment. They should not be selected on characteristics that could explain a decrease in violence in the post-treatment period. Second, there are no other changes both in space and in time that could correlate with the coverage of the VRA and political violence. It is impossible to entirely ensure the validity of these two assumptions. Table 5 nevertheless sums up different tests and results suggesting that these assumptions hold.

Table 5: Summary - Identification

Identification - Assumptions:	Test	Reference
Parallel trends	Test for differences in trends	Appendix A.2
	Placebo test - Alternative timing	Appendix A.3
No parallel reforms affecting state borders	Adding state-month fixed effects to the estimation	Appendix C.1
	Geographic matching and RDD estimates at the border.	Appendix C.4
No selection of low turnout counties	Test with placebo treatment (Turnout: <50% at the county-level)	Appendix C.2
No manipulation around the treatment threshold	Distribution of turnout rate - No bunching around the threshold	Appendix C.3

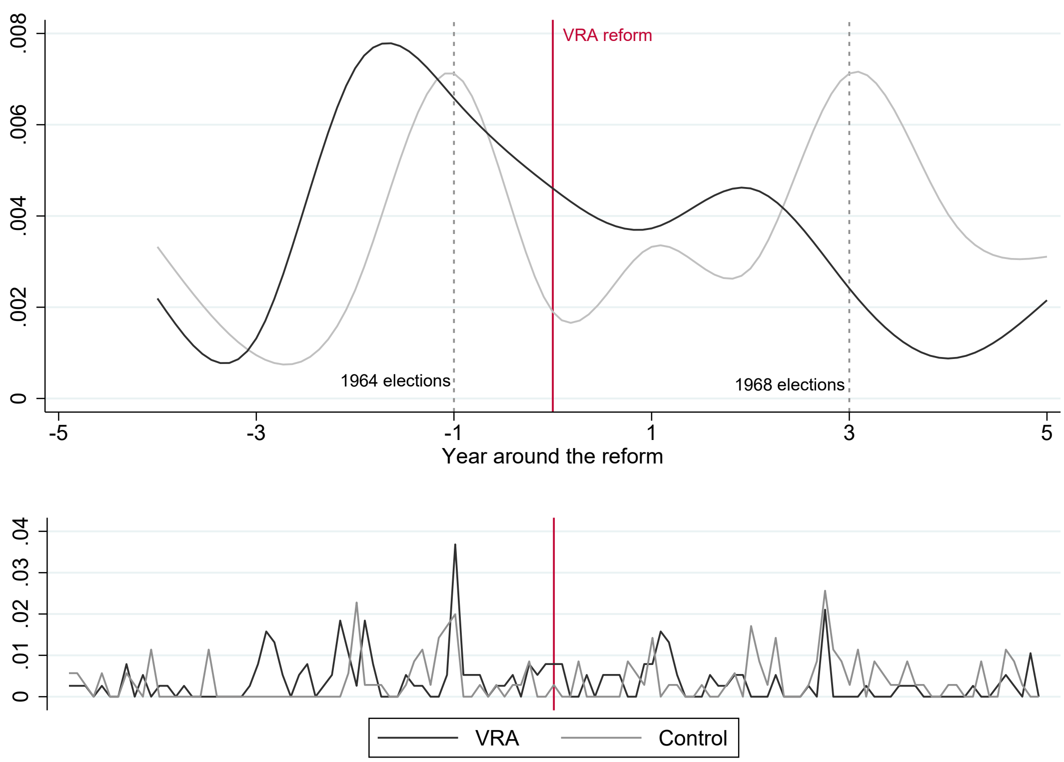
6 The effect of enfranchisement on political violence: Transmission channels

Section 6 presents evidence of a "direct" transmission channel from enfranchisement to political violence. This section goes beyond typical investigations of the transmission channels linking enfranchisement to violence that use data on local conditions to infer motives to use violence. The event-level data used in this paper documents in details the logic of perpetrators and shows how the dynamics of violence unfolded following enfranchisement. This data is therefore better suited to detect a "direct" transmission channel. This approach does not simply hypothesize that some local conditions lead to some specific behavior but in fact directly observes this behavior. This investigation of mechanisms is driven by the theoretical predictions of Section 2: a "direct" transmission channel would affect small-scale violence during electoral periods both by newly-enfranchised and others. This would not hold for the "indirect" transmission channel. Section 6, thereby, complements other pieces of research on the long-run effects of the VRA that may result from "indirect" transmission channels (see Appendix E). Section 6 is also relevant to the literature on other determinants of political violence such as the cost of collective action (Lohmann, 1993).

6.1 How enfranchisement influences violence: Timing and perpetrators

As a first attempt to understand the effect of the VRA on political violence, this section focuses on electoral periods. Figure 7 shows that before presidential elections in the sample (in 1964 and 1968), there is a surge in violence in the control group. This surge also occurs in the treated group before the 1964 elections but not before the 1968 elections. This timing suggests that, after enfranchisement, citizens may invest in elections and, therefore, divest from violence. Additional estimations confirm this evolution of the effect of VRA coverage

Figure 7: Electoral violence and enfranchisement



Upper panel: The graph presents the average number of events tagged as political violence by year for (future) VRA counties and the control group. To produce the graph, yearly incidence of violence has been smoothed. The red line indicates the implementation of the VRA, and the two dashed lines represent the 1964 and 1968 presidential elections. Bottom panel: The graph presents the raw data used to construct the upper panel: incidence rates of violence at the county/month level for treated counties and counties in the control group.

over time and around elections. Table 6 presents results of the estimation of the following equation:

$$Pr(Viol_{i,t} = Y \mid Viol_{i,t-1}, VRA_{i,t}, \phi_t + \eta_i) = f(\alpha, \beta_1 Viol_{i,t-1}, \Gamma_2 VRA_{i,t} \times Timing_t, \phi_t, \eta_i, \varepsilon_{i,t}) \tag{2}$$

Γ_2 is a vector of three coefficients capturing the effect of VRA coverage in a first phase (VRA Base: August 1965 to November 1967), in the pre-presidential election period (VRA Pre-elect: November 1967 to November 1968), and in the post-presidential election period (VRA Post-elect: post November 1968). It further distinguishes different violent events according to the claims of their perpetrators to better understand how enfranchisement changed different groups' motives to use violence over time. Columns 6.1 and 6.2 consider all types of violence. These results suggest that VRA counties experienced most of the decrease in violence around the 1968 elections.[19]

Later columns document the origins of this specific timing. Columns 6.3 and 6.4 show that segregation-related violence was similarly timed in VRA counties. VRA coverage mainly decreased segregation-related violence during electoral seasons. Such violence potentially results from both disenfranchised groups using violence to interfere with the electoral process and of enfranchised groups using violence to mobilize their own electorate and maintain their electoral advantage. To investigate the timing of the effect of VRA coverage on violence from these two groups, Columns 6.5 to 6.8 analyse the effect of VRA coverage on pro-segregation and anti-segregation violence for which disenfranchised and enfranchised interests are easy to identify. From the predictions in Section 2, these columns investigate a possible direct mechanism in two ways. First, the direct mechanism would impact both pro-segregation violence and anti-segregation violence, whereas the indirect mechanism would only decrease anti-segregation violence. Second, the direct mechanism would imply a decrease in violence from the newly-enfranchised already before the elections (as they mobilize using voting and not violence). According to the indirect mechanism, this decrease would be more prominent after the elections. Columns 6.5 and 6.6 suggest that pro-segregation violence decreased after the 1968 elections. Even if not significant, the treatment variable for the pre-election period

[19] The timing of the election matches with reactions to the Martin Luther King assassination. This variation in grievance in time is captured by month fixed effects. In this study, treated counties and the control group share similar characteristics and dynamics of violence before the VRA. Hence, the assassination of Martin Luther King alone does not explain the difference in violence observed between these two neighbouring areas. At most, VRA coverage might have explained a different reaction to the assassination between the two areas: Citizens in treated counties may have invested their resources in the upcoming elections, while those in the control may not have in line with the mechanisms presented in the article.

Table 6: Timing of the effect: Enfranchisement and electoral violence

Dependent Variable	(6.1) Incidence$_{c,t}$ All	(6.2) Onset$_{c,t}$ All	(6.3) Incidence$_{c,t}$ Segr	(6.4) Onset$_{c,t}$ Segr	(6.5) Incidence$_{c,t}$ Pro-Segr	(6.6) Onset$_{c,t}$ Pro-Segr	(6.7) Incidence$_{c,t}$ Anti-Segr	(6.8) Onset$_{c,t}$ Anti-Segr
VRA Base	-0.333	-0.282	-0.133	-0.254	0.198	-0.0350	-0.161	-0.304
	(-0.766)	(-0.643)	(-0.304)	(-0.526)	(0.326)	(-0.0596)	(-0.291)	(-0.488)
VRA Pre-Elect	-1.378***	-1.272**	-1.672***	-1.669***	-1.947	-1.951	-1.455**	-1.491**
	(-2.716)	(-2.332)	(-2.779)	(-2.657)	(-1.612)	(-1.645)	(-2.132)	(-2.147)
VRA Post-Elect	-1.237**	-1.555***	-1.032*	-1.261**	-1.837**	-2.120**	-0.753	-0.946
	(-2.497)	(-3.110)	(-1.917)	(-2.372)	(-2.037)	(-2.516)	(-1.354)	(-1.641)
IRR VRA Base	0.72	0.75	0.88	0.78	1.21	0.97	0.85	0.74
IRR VRA Pre-Elect	0.25	0.28	0.19	0.19	0.14	0.14	0.23	0.23
IRR VRA Post-Elect	0.29	0.21	0.36	0.28	0.16	0.12	0.47	0.39
Control for Inc$_{c,t-1}$	Yes	No	Yes	No	Yes	No	Yes	No
Drop if Inc$_{c,t-1}$>0	No	Yes	No	Yes	No	Yes	No	Yes
Obs	12,138	11,911	11,186	10,989	7,735	7,618	7,735	7,610
Number of counties	102	102	94	94	65	65	65	65

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the number of violent events (Column 6.1), a dummy variable equal to one for the onset of violence (Column 6.2), the number of segregation-related violent events (Column 6.3), a dummy variable for the onset of segregation-related violence (Column 6.4), the number of pro-segregation violent events (Column 6.5) a dummy variable for the onset of pro-segregation violent events (Column 6.6), the number of anti-segregation violent events (Column 6.7) and a dummy variable for the onset of anti-segregation violent events (Column 6.8).VRA Base is a dummy variable equal to one for counties under the VRA from August 1965 to November 1967. VRA Pre-Elect is a dummy variable equal to one for counties under the VRA from November 1967 to November 1968. VRA Post-Elect is a dummy variable equal to one for counties under the VRA from November 1968 to the end of the sample (August 1970). All regressions include county fixed effects and month fixed effects. IRR stands for incidence rate ratio. The dependent variable is defined at the county-month level.

displays a low incidence rate ratio as well, suggesting that some of the effect of VRA coverage on pro-segregation violence might have materialized before the elections. In Columns 6.7 and 6.8, only the coefficients attached to the pre-electoral period are significant and negative. The incidence rate ratios for this period are lower than for any other period: 0.23. The incidence rate ratios remain relatively low after the elections suggesting that this effect may have persisted over time. According to these results, anti-segregation violence exhibits the pattern suggested by a direct mechanism. It mainly decreased the year before the 1968 elections. This timing suggests that the electoral campaign might have been different in VRA counties compared to others.

Table 7 further rationalizes these results by investigating the electoral consequences of VRA coverage. At the aggregate level, the 1968 elections do not match the indirect channel story as they marked the ascent of a pro-segregation candidate, Alabama Governor George Wallace and ultimately an electoral turnover with the victory of President Richard Nixon. Table 7 investigates if VRA coverage in our sample led to similar effects to better explain the specific timing observed in Table 6. Panel A focuses on electoral participation and Panel B on electoral results. These results show an increase in turnout in VRA counties compared to others (Panel A). This increase however did not benefit democrats as the VRA variable only explains differences in the vote shares for Nixon (negative) and the vote share for Wallace (positive) (Panel B). Taken together, these results do not suggest an “indirect” transmission channel but rather a “direct” one. Following the VRA, even the most extremist political views turned to the ballot and did not resort to violence. In the short-run, this did not impact the Republican-Democrat balance, but it benefited a pro-segregation candidate.

Table 7: Voting Rights Act and electoral consequences

Panel A: Electoral results

	(7.A.1) ΔTurnout $Pres_{68-64}$	(7.A.2) ΔTurnout $Pres_{68-64}$	(7.A.3) ΔTurnout $Pres_{68-64}$	(7.A.4) Turnout Cong $_{66}$	(7.A.5) Turnout Cong $_{68}$	(7.A.6) Turnout Cong $_{70}$
VRA	3.355***	3.535***	3.512***	3.985***	4.453***	-0.474
	(5.013)	(5.292)	(5.811)	(3.906)	(4.185)	(-0.490)
Observations	726	726	726	675	643	651
Adjusted R-squared	0.522	0.424	0.522	0.439	0.470	0.423

Panel B: Turnout

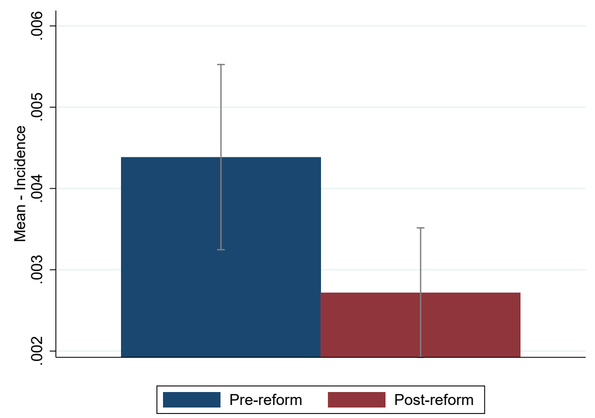
	(7.B.1) Δ(Dem-Rep)$_{66}$	(7.B.2) Δ(Dem-Rep)$_{68}$	(7.B.3) Δ(Dem-Rep)$_{70}$	(7.B.4) %Humphrey$_{68}$	(7.B.5) %Wallace$_{68}$	(7.B.6) %Nixon$_{68}$
	-4.293	3.673	-3.114	0.113	6.765***	-6.451***
	(-1.449)	(1.193)	(-1.037)	(0.154)	(4.595)	(-4.813)
Observations	688	688	664	731	730	731
Adjusted R-squared	0.552	0.465	0.422	0.518	0.292	0.257

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Cross-county regressions comparing post-VRA outcomes. Estimated via OLS. Sample= 100km around the border between VRA counties and others. Panel A - Electoral results: Control variables: (%Vote share Democrats - %Vote Share Republicans) in the 1964 Congress Elections, Increase in the difference between the %Vote shares of Democrats and of Republicans between 1960 and 1964 (Columns A.1 to A.3), Vote Share Johnson in the 1964 presidential elections, Vote Share Goldwater in the 1964 presidential elections (Columns A.4 to A.6). Dependent variables: Difference in %vote share between Democrats and Republicans in the 1966 congress elections (A.1) in the 1968 congress elections (A.2) in the 1970 congress elections (AC.3). %Vote share in the presidential elections of 1968 of Humphrey (A.4), Wallace (A.5) and Nixon (A.6). Panel B - Turnout: Control variables: %Turnout: 1964 presidential elections - B.1 and B.3, 1960 presidential elections - B.2 and B.3, 1960 Congress elections, 1962 Congress elections, 1964 Congress elections: B.4 to B.6. Dependent variables: Δ%Turnout between 1964 and 1968 presidential elections (B.1 to B.3), %Turnout in the 1966 Congress Elections (B.4), in the 1968 Congress Elections (B.5), in the 1970 Congress Elections (B.6).

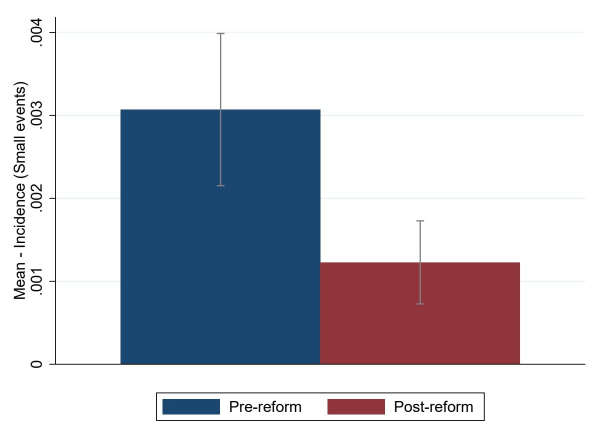
6.2 How enfranchisement influences violence: Type of violence

Section 6.2 expands this result by investigating the type of events affected by VRA coverage. I hypothesize that the size of an event reveals the type of mobilization it followed from. In particular, small events are likely strategic and disruptive and involve the most convinced activists. By contrast, large events likely result from mass mobilization reflecting aggrievement in the population. Figure 8 presents how the different types of violent events evolved in VRA counties after the Act passed. The overall number of violent events decreased after the VRA (Panel A), and this decrease mainly resulted from a decrease in small-scale events (involving fewer than 100 participants - Panel B). By contrast the number of large scale events (100 or greater participants) remained stable after 1965 (Panel C). Table 8 more closely examines this decrease in the number of small events. Focusing on small events could document a

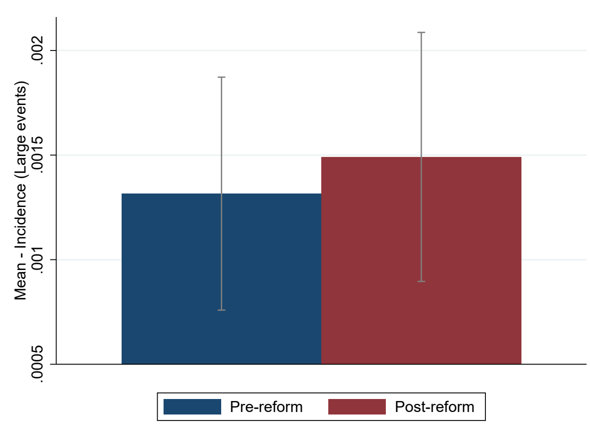
Figure 8: Evolution of violence in VRA counties



Panel A: Incidence (All events)



Panel B: Incidence (Small-scale events)



Panel C: Incidence (Large-scale events)

potential direct channel that implies a decrease in strategic violence, but not a decrease in violence from aggrievement. Columns 8.1 to 8.6 estimate Equation 1 using small-scale events with violence as the dependent variable; Columns 8.7 to 8.8 estimate the baseline model in a sample of counties having experienced no more than one event involving numerous participants (>99) in the pre-treatment period; and Columns 8.9 to 8.10 estimate the baseline model in a sample of counties having experienced fewer than 5 events involving numerous participants (>99) between 1960 and 1970. This last set of estimates ensures that baseline estimates do not emerge because nation-wide movements gathering large crowds developed differently in the control and treatment groups.

Coefficients attached to the VRA variable are all negative and statistically significant even when controlling for lagged large events (Columns 8.1 and 8.2) and the interaction of the VRA variable and lagged large events (Columns 8.3 to 8.6). Interestingly, the interaction term between the VRA variable and lagged large events is never significant, meaning that the Act did not change the functional relationship between small- and large-scale events. When excluding counties experiencing mass mobilization from the sample (Columns 8.7 to 8.10), the VRA variable remains negative and significant at the 5- or 10-percent level. The incidence rate ratios are comparable to baseline results, suggesting that baseline results are not driven by the mobilization of large crowds in large cities. These results are important since in this data some events may originate from perpetrators that do not live in the county in which they exert violence. If we consider that enfranchisement changes the strategic motives to use violence in some places regardless of the living conditions of this county, enfranchisement would still impact violence from these outside perpetrators.[20] These additional results, however, indicate that most acts of violence are perpetrated by local citizens since small-scale events are less likely to be instigated by citizens travelling from afar. In addition, these results show that small, disruptive events decreased in places where no massive mobilization took place. Accordingly, the effect of the VRA on violence goes beyond a potential effect on grievance that would affect large-scale events. These results align better with a direct mechanism whereby activists change the way they influence policies than with an indirect mechanism through which massive groups are disaggrieved following enfranchisement.

[20]It should be noted that all results still assess the role of enfranchisement as a geographical determinant to use violence. Should the motivation to use violence be totally disconnected from where violence takes place in, then there would not be any observable correlation between VRA coverage and violence.

Table 8: Voting Rights Act and the composition of violence

	(8.1)	(8.2)	(8.3)	(8.4)	(8.5)	(8.6)	(8.7)	(8.8)	(8.9)	(8.10)
Dep Variable	Incidence $Small_{c,t}$	Onset $Small_{c,t}$	Incidence $Small_{c,t}$	Onset $Small_{c,t}$	Incidence $Small_{c,t}$	Onset $Small_{c,t}$	Incidence $All_{c,t}$	Onset $All_{c,t}$	Incidence $All_{c,t}$	Onset $All_{c,t}$
Sample	100km	100km	100km	100km	100km	100km	$Large<2_{60-65}$	$Large<2_{60-65}$	$Large<5_{60-70}$	$Large<5_{60-70}$
Small $events_{c,t-1}$	0.501***		0.574***		0.556***					
	(4.902)		(6.401)		(4.678)					
Large $events_{c,t-1}$	0.569**									
	(2.332)									
$Incidence_{c,t-1}$							0.721***		0.553***	
							(4.493)		(5.078)	
VRA	-0.891**	-0.803*	-1.014**	-0.856**	-1.116**	-0.930**	-1.004**	-0.926**	-0.966*	-0.848*
	(-2.067)	(-1.806)	(-2.392)	(-2.083)	(-2.367)	(-2.099)	(-2.219)	(-2.103)	(-1.950)	(-1.793)
$Large_{t-6,t-1}$			0.206	0.404***						
			(1.355)	(3.440)						
VRA × $Large_{t-6,t-1}$			0.346	0.101						
			(1.203)	(0.330)						
$Large_{t-12,t-1}$					0.0749	0.176				
					(0.444)	(1.224)				
VRA × $Large_{t-12,t-1}$					0.0833	-0.0423				
					(0.291)	(-0.134)				
Constant	-2.630***	-3.020***	-2.613***	-3.100***	-2.457***	-2.950***	-2.784***	-3.203***	-2.881***	-3.283***
	(-40.98)	(-35.10)	(-35.82)	(-37.18)	(-23.72)	(-28.48)	(-23.31)	(-28.21)	(-32.70)	(-28.61)
IRR VRA	0.41	0.45	0.36	0.43	0.33	0.39	0.37	0.40	0.38	0.43
IRR VRA × $Large_{t-6,t-1}$			1.41	1.11						
IRR VRA × $Large_{t-12,t-1}$					1.09	0.96				
Control for $DV_{c,t-1}$>0	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Drop if $Inc_{c,t-1}$>0	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4,899	4,674	4,624	4,405	3,968	3,766	6,390	6,034	7,315	6,719

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Estimator: Poisson. Sample: Within the 100km buffer. Incidence $Small_{c,t}$ is the number of events involving less than 100 participants in county c at time t. Onset $Small_{c,t}$ is equal to 1 when at least one small event took place in county c at time t and no small event took place in county t at time t-1. Incidence $All_{c,t}$ is the number of events in county c at time t. Onset $Small_{c,t}$ is equal to 1 when at least one event took place in county c at time t and no event took place in county t at time t-1. $Large<2_{60-65}$ stands for all counties within the 100km buffer having experienced 0 or 1 large events in the five years before the VRA passed. $Large<5_{60-70}$ stands for all counties within the 100km buffer having experienced less than 5 large events in the study period. Incidence Small is the number of events tagged as political violence involving less than 100 participants. Onset Small is the onset of new wave of violence involving less than 100 participants. Large $events_{c,t-1}$ is the number of large events (>99participants) occuring in the past month in county c. $Large_{t-12,t-1}$ is the number of large events (>99participants) occuring in the past twelve months in county c. $Large_{t-6,t-1}$ is the number of large events (>99participants) occuring in the past six months in county c. All regressions integrate a month fixed effects and county fixed effects. IRR stands for incidence rate ratio.

6.3 Testing indirect effects: The role of peaceful protests

Section 6.3 distinguishes the potential "direct" effect of enfranchisement from the potential "indirect" effect. To do so, it investigates the evolution of peaceful protests in the sample and provides more information on the violence-generating mechanisms before and after the VRA.

Table 9: Enfranchisement: Peaceful protests ↔ political violence

	(9.1)	(9.2)	(9.3)	(9.4)	(9.5)	(9.6)	(9.7)	(9.8)	(9.9)	(9.10)
Dep variable	Peaceful	Peaceful	Viol	Viol	Viol	Viol	Viol	Viol	Viol	Viol
	$\text{Inc}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Inc}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Inc}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Inc}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Inc}_{c,t}$	$\text{Onset}_{c,t}$
Sample	100km	100km	100km	100km	100km	100km	100km	100km	P<2	P<2
VRA	-0.553*	-0.313	-0.742**	-0.726*	-0.943**	-0.986**	-0.830**	-0.794**	-1.429**	-1.159*
	(-1.908)	(-1.456)	(-2.165)	(-1.959)	(-2.507)	(-2.492)	(-2.398)	(-2.126)	(-2.250)	(-1.885)
$\text{Peaceful}_{c,t}$			0.357***	0.392***			0.367***	0.388***		
			(6.810)	(5.281)			(6.996)	(5.467)		
$\text{Peaceful}_{c,t-1}$	0.175***				0.0911*	0.0886**	-0.0583	-0.0278		
	(6.333)				(1.832)	(2.390)	(-1.094)	(-0.443)		
$\text{VRA}\times\text{Peaceful}_{c,t-1}$					0.398***	0.680**	0.425***	0.599***		
					(3.750)	(2.302)	(3.963)	(2.771)		
IRR VRA	0.58	0.73	0.48	0.48	0.39	0.37	0.44	0.45	0.24	0.31
IRR VRA×Peaceful					1.49	1.97	1.53	1.82		
Control for $\text{DV}_{c,t-1}$	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Drop if $\text{Inc}_{c,t-1}>0$	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	14,875	14,282	12,138	11,911	12,138	11,911	12,138	11,911	6,307	6,249
Number of counties	125	125	102	102	102	102	102	102	53	53

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Each regression estimates Equation 1 controlling for the lagged dependent variable when this variable is not the onset of a new wave of political violence or of peaceful protests. The dependent variable in each column is as follows: number of peaceful protests (9.1), onset of a new wave of peaceful protests (9.2), number of events tagged as political violence (9.3, 9.5, 9.7 and 9.9) and onset of a new wave of political violence (9.4, 9.6, 9.8 and 9.10). In the last two columns, the sample consists of counties that did not experience multiple peaceful protests. All regressions include county fixed effects and month fixed effects. IRR stands for incidence rate ratio.

Columns 9.1 and 9.2 estimate the effect of VRA coverage on peaceful protests. The coefficient for the VRA treatment variable is significant at the 10-percent level when the number of violent events is the dependent variable but is insignificant when using the onset of new waves of peaceful protests as the dependent variable. The impact of VRA coverage on peaceful protests was limited compared to its impact on violence. After controlling for the number of peaceful events in our baseline model using violent events as the dependent variable (Columns 9.3 and 9.4), the coefficient of the VRA dummy variable is significant at the 5- or 10-percent level. The incidence rate ratios are also in line with baseline estimates. Hence, VRA coverage affected violence beyond its effect on peaceful protests.

Columns 9.5 to 9.8 further document the interrelation between peaceful protests and political violence. Political violence may arise either as a strategy in itself (e.g. terrorism) or as the result of escalation from peaceful protests (e.g. a peaceful protest turning into a riot). To estimate how much VRA coverage impacted the correlation between lagged peaceful protests and political violence, the VRA dummy variable is interacted with the lagged number of peaceful protests (Columns 9.5 to 9.6). A negative coefficient would suggest indirect effects: before the VRA violence would be linked to peaceful protests whereas after the VRA decreasing peaceful protests would explain a decrease in the correlation between violence and peaceful protests. A positive coefficient would suggest direct effects: strategic violence would not be linked to violence before the reform but declining afterwards thereby increasing the correlation between peaceful protests and violence. In these columns, the VRA dummy variable is significant and negative at the 5-percent level, and the incidence rate ratios of 0.37 and 0.39 are in line with previous estimates. The interaction term of the VRA dummy variable with the lagged number of peaceful protests is positive and significant. After the VRA, the relative importance of lagged peaceful protests in explaining violence increases. This means that VRA coverage decreased the strategic use of violence more than violence that occurred as a result of escalation. Columns 9.9 to 9.10 further exclude counties having experienced more than one instance of peaceful collective action in the study period. The coefficients attached to the VRA variable remain significant at least at the 10-percent level, and incidence rate ratios are slightly lower than baseline results even after dropping around half of the sample. Accordingly, results in Table 9 suggest that a decrease in violence is possible even when no peaceful mobilization occurred. Enfranchisement may decrease strategic violence, and this decrease may not necessarily result from disaggrievement. This decrease net of the effect of enfranchisement on disaggrievement can be considered as a decrease in the strategic use of violence – the direct mechanism between enfranchisement and violence.

7 Conclusion

Enfranchisement reduces political violence. This paper presents empirical evidence of the causal negative effect the VRA had on political violence in the US in the 1960s. This piece of legislation de facto increased voter enfranchisement in the South by ending discrimination in voting. To assess the causal impact of this enfranchisement, I compare political violence dynamics between counties covered by the VRA and their non-covered neighbours. Covered counties experienced a decrease in political violence after the VRA was adopted. I interpret these results as evidence of the negative effect enfranchisement has on political violence.

This article also points to some mechanisms explaining how enfranchisement might reduce political violence. The electoral mobilization of all different voters mattered to lead to this de-escalation process. As a result of the reform, voters mobilized via the ballot and stopped using violence to interfere with the electoral process via intimidation or the mobilization of their own. Enfranchisement provides citizens with an incentive to use voting as an institutional way of expressing political preferences. It may decrease violence beyond its effect on policies, by changing political strategies. That way, enfranchisement decreases low-scale strategic violence, such as bombings, arsons and shootings aimed at disrupting the electoral process.

This research offers new insights into the democracy-violence nexus in two ways. First, it precisely identifies the causal effect of enfranchisement on political violence. It isolates the effect of enfranchisement and uses local discontinuities to do so. Second, it empirically documents a new mechanism explaining this effect. It shows that enfranchisement may decrease political violence beyond its effect on voting outcomes or on redistribution. The VRA was a specific reform enforced by a federal government in local jurisdictions. Hence, it limited the use of violence to curb the registration of new voters. This big push in access to voting rights reduced the returns from violence relative to voting. Conclusions from this research also have implications beyond the US in the 1960s. This paper's finding shed a new light on possible ways to curb violence in different contexts. For example, after the decision of the US Supreme Court to dismantle some provisions of the VRA (Shelby vs Holder, 2013), several states set up devices limiting voters' registration.[21] Under these circumstances, violence erupted while the Voting Rights Advancement Act failed to receive enough votes in the Senate on two occasions: November 2021 and January 2022.[22] Would restoring the VRA have been a solution to political unrest? These results suggest that it would have partly depended on how much people believed and invested in electoral and registration campaigns and on how much they believed that voicing their concerns via the ballot is the way to be heard. Similar conclusions hold when assessing political violence in different countries experiencing issues with enfranchisement, such as today in the Democratic Republic of the Congo or in India. Future research should investigate how the effect of enfranchisement on political violence varies across contexts to better understand the prospects of democratization or of any reform aiming to integrate populations previously excluded from politics. Such investigations could also broaden our knowledge of the consequences of enfranchisement in democracies less mature than the US in the 1960s.

[21] https://www.nytimes.com/2018/06/23/us/politics/voting-rights-alabama.html
https://www.independent.co.uk/news/world/americas/us-election/florida-felony-vote-republican-sb7066-amendment-4-voter-supression-trump-a9236721.html
https://www.theguardian.com/us-news/2020/jun/25/shelby-county-anniversary-voting-rights-act-consequences
https://time.com/5852837/voter-suppression-obstacles-just-america/

[22] The US House of Representatives passed this bill in December 2019.

References

Acemoglu, D. and J. A. Robinson (2001). A theory of political transitions. *American Economic Review 91*(4), 938–963.

Acemoglu, D. and J. A. Robinson (2006). Economic backwardness in political perspective. *American Political Science Review 100*(1), 115–131.

Acemoglu, D. and J. A. Robinson (2008). Persistence of power, elites, and institutions. *American Economic Review 98*(1), 267–93.

Acemoglu, D. and J. A. Robinson (2017). The emergence of weak, despotic and inclusive states. Technical report, National Bureau of Economic Research.

Aidt, T., S. L. Winer, and P. Zhang (2020). Franchise extension and fiscal structure in the united kingdom 1820-1913: A new test of the redistribution hypothesis. *CESifo Working Paper 8114*.

Aidt, T. S., M. Daunton, and J. Dutta (2010). The retrenchment hypothesis and the extension of the franchise in England and Wales. *Economic Journal 120*(547), 990–1020.

Aidt, T. S. and R. Franck (2015). Democratization under the threat of revolution: Evidence from the great reform act of 1832. *Econometrica 83*(2), 505–547.

Amodio, F. and G. Chiovelli (2018). Ethnicity and violence during democratic transitions: Evidence from south africa. *Journal of the European Economic Association 16*(4), 1234–1280.

Aneja, A. P. and C. F. Avenancio-Leon (2019). The effect of political power on labor market inequality: Evidence from the 1965 voting rights act. *Mimeo*.

Ang, D. (2019). Do 40-year-old facts still matter? Long-run effects of federal oversight under the voting rights act. *American Economic Journal: Applied Economics 11*(3), 1–53.

Battaglini, M. (2017). Public protests and policy making. *Quarterly Journal of Economics 132*(1), 485–549.

Berger, D., W. Easterly, N. Nunn, and S. Satyanath (2013). Commercial imperialism? political influence and trade during the cold war. *American Economic Review 103*(2), 863–96.

Berlinski, S. and T. Dewan (2011). The political consequences of franchise extension: Evidence from the second reform act. *Quarterly Journal of Political Science 6*(34), 329–376.

Bernini, A., G. Facchini, and C. Testa (2018). Race, representation and local governments in the US south: the effect of the Voting Rights Act. *CEPR Working paper*.

Besley, T. and S. Coate (1997). An economic model of representative democracy. *Quarterly Journal of Economics 112*(1), 85–114.

Besley, T. and T. Persson (2011). The logic of political violence. *Quarterly Journal of Economics 126*(3), 1411–1445.

Besley, T., T. Persson, and D. M. Sturm (2010). Political competition, policy and growth: theory and evidence from the US. *Review of Economic Studies 77*(4), 1329–1352.

Blattman, C. and E. Miguel (2010). Civil war. *Journal of Economic literature 48*(1), 3–57.

Breitung, J., S. Kripfganz, and K. Hayakawa (2021). Bias-corrected method of moments estimators for dynamic panel data models. *Econometrics and Statistics*.

Button, J. W. (1978). *Black violence*. Princeton University Press.

Carvalho, J.-P. and C. Dippel (2020). Elite identity and political accountability: a tale of ten islands. *The Economic Journal 130*(631), 1995–2029.

Cascio, E. U. and E. Washington (2013). Valuing the vote: The redistribution of voting rights and state funds following the voting rights act of 1965. *Quarterly Journal of Economics 129*(1), 379–433.

Cederman, L.-E., K. S. Gleditsch, and S. Hug (2013). Elections and ethnic civil war. *Comparative Political Studies 46*(3), 387–417.

Chacon, M., J. A. Robinson, and R. Torvik (2011). When is democracy an equilibrium? Theory and evidence from Colombia la Violencia. *Journal of Conflict Resolution 55*(3), 366–396.

Collier, P., A. Hoeffler, and D. Rohner (2009). Beyond greed and grievance: feasibility and civil war. *Oxford Economic papers 61*(1), 1–27.

Collier, P. and D. Rohner (2008). Democracy, development, and conflict. *Journal of the European Economic Association 6*(2-3), 531–540.

Collins, W. J. and R. A. Margo (2007). The economic aftermath of the 1960s riots in american cities: Evidence from property values. *Journal of Economic History 67*(4), 849–883.

Corvalan, A., P. Querubin, and S. Vicente (2020). The political class and redistributive policies. *Journal of the European Economic Association 18*(1), 1–48.

Davidson, C. and B. Grofman (1994). *Quiet revolution in the South: The impact of the Voting Rights Act, 1965-1990*. Princeton University Press.

De Bromhead, A., A. Fernihough, and E. Hargaden (2020). Representation of the people: Franchise extension and the "Sinn Féin election" in Ireland, 1918. *The Journal of Economic History 80*(3), 886–925.

Eubank, N. and A. Fresh (2020). Enfranchisement and incarceration after the 1965 voting rights act.

Facchini, G., B. G. Knight, and C. Testa (2020). The franchise, policing, and race: Evidence from arrests data and the voting rights act.

Fearon, J. D. (2011). Self-enforcing democracy. *Quarterly Journal of Economics 126*(4), 1661–1708.

Fergusson, L. and J. Vargas (2013). Don't make war, make elections-franchise extension and violence in xixth-century colombia.

Fetzer, T., S. Kyburz, et al. (2018). Cohesive institutions and political violence. Technical report, Empirical Studies of Conflict Project.

Finkel, E., S. Gehlbach, and T. D. Olsen (2015). Does reform prevent rebellion? Evidence from Russia's emancipation of the serfs. *Comparative Political Studies 48*(8), 984–1019.

Fresh, A. (2018). The effect of the Voting Rights Act on Enfranchisement: Evidence from North Carolina. *Journal of Politics 80*(2), 713–718.

Gallego, F. A. (2010). Historical origins of schooling: The role of democracy and political decentralization. *The Review of Economics and Statistics 92*(2), 228–243.

Gleditsch, K. and A. Ruggeri (2010). Political opportunity structures, democracy, and civil war. *Journal of Peace Research 47*(3), 299–310.

Harish, S. and A. T. Little (2017). The political violence cycle. *American Political Science Review 111*(2), 237–255.

Harris, C. and J. Vickers (1985). Perfect equilibrium in a model of a race. *Review of Economic Studies 52*(2), 193–209.

Harris, C. and J. Vickers (1987). Racing with uncertainty. *Review of Economic Studies 54*(1), 1–21.

Hegre, H. (2001). Toward a democratic civil peace? Democracy, political change and civil war, 1816-1992. *American Political Science Review*.

Hegre, H. (2014). Democracy and armed conflict. *Journal of Peace Research 51*(2), 159–172.

Hegre, H. and N. Sambanis (2006). Sensitivity analysis of empirical results on civil war onset. *Journal of conflict resolution 50*(4), 508–535.

Husted, T. A. and L. W. Kenny (1997). The effect of the expansion of the voting franchise on the size of government. *Journal of Political Economy 105*(1), 54–82.

Konrad, K. A. (2012). Dynamic contests and the discouragement effect. *Revue d'économie politique 122*(2), 233–256.

Kuziemko, I. and E. Washington (2018). Why did the democrats lose the south? Bringing new data to an old debate. *American Economic Review 108*(10), 2830–67.

Larcinese, V. (2017). Enfranchisement and representation: Evidence from the introduction of quasi-universal suffrage in italy. *LSE mimeo*.

Leventoğlu, B. and N. W. Metternich (2018). Born weak, growing strong: Anti-government protests as a signal of rebel strength in the context of civil wars. *American Journal of Political Science*.

Lipsky, M. (1968). Protest as a political resource. *American political science review 62*(4), 1144–1158.

Little, A. T., J. A. Tucker, and T. LaGatta (2015). Elections, protest, and alternation of power. *Journal of Politics 77*(4), 1142–1156.

Lizzeri, A. and N. Persico (2004). Why did the elites extend the suffrage? Democracy and the scope of government, with an application to Britain's "Age of Reform". *Quarterly Journal of Economics 119*(2), 707–765.

Lohmann, S. (1993). A signaling model of informative and manipulative political action. *American Political Science Review 87*(2), 319–333.

Marschall, M. J., A. V. Ruhil, and P. R. Shah (2010). The new racial calculus: Electoral institutions and black representation in local legislatures. *American Journal of Political Science 54*(1), 107–124.

May, G. (2013). *Bending toward justice: the Voting Rights Act and the transformation of American democracy.* Basic Books.

Mazumder, S. (2018). The persistent effect of us civil rights protests on political attitudes. *American Journal of Political Science 62*(4), 922–935.

Mazur, A. (1972). The causes of black riots. *American Sociological Review 37*(4), 490–493.

McAdam, D., J. McCarthy, S. Olzak, and S. Soule (2003). Dynamics of collective action dataset. *Stanford University*.

Meltzer, A. H. and S. F. Richard (1981). A rational theory of the size of government. *Journal of Political Economy 89*(5), 914–927.

Myers, D. J. (1997). Racial rioting in the 1960s: An event history analysis of local conditions. *American Sociological Review*, 94–112.

Nickell, S. (1981). Biases in dynamic models with fixed effects. *Econometrica*, 1417–1426.

Olzak, S., S. Shanahan, and E. H. McEneaney (1996). Poverty, segregation, and race riots: 1960 to 1993. *American Sociological Review*, 590–613.

Pande, R. (2003). Can mandated political representation increase policy influence for disadvantaged minorities? Theory and evidence from india. *American Economic Review 93*(4), 1132–1151.

Passarelli, F. and G. Tabellini (2017). Emotions and political unrest. *Journal of Political Economy 125*(3), 903–946.

Przeworski, A. (2015). Acquiring the habit of changing governments through elections. *Comparative Political Studies 48*(1), 101–129.

Reynal-Querol, M. (2002). Ethnicity, political systems, and civil wars. *Journal of Conflict Resolution 46*(1), 29–54.

Rohner, D. and A. Saia (2020). Ballot or bullet: The impact of uk's representation of the people act on peace and prosperity.

Saideman, S. M., D. J. Lanoue, M. Campenni, and S. Stanton (2002). Democratization, political institutions, and ethnic conflict: A pooled time-series analysis, 1985-1998. *Comparative Political Studies 35*(1), 103–129.

Schuit, S. and J. C. Rogowski (2017). Race, representation, and the voting rights act. *American Journal of Political Science 61*(3), 513–526.

Schwarzmantel, J. (2010). Democracy and violence: A theoretical overview. *Democratization 17*(2), 217–234.

Shah, P. R., M. J. Marschall, and A. V. Ruhil (2013). Are we there yet? The voting rights act and black representation on city councils, 1981–2006. *Journal of Politics 75*(4), 993–1008.

Spilerman, S. (1970). The causes of racial disturbances: A comparison of alternative explanations. *American Sociological Review*, 627–649.

Sunde, U. and M. Cervellati (2013). Democratizing for peace? The effect of democratization on civil conflicts. *Oxford Economic Papers 66*(3), 774–797.

Thompson, J. A. (1986). The voting rights act in north carolina: An evaluation. *Publius: The Journal of Federalism 16*(4), 139–154.

Timpone, R. J. (1995). Mass mobilization or government intervention? the growth of black registration in the south. *Journal of Politics 57*(2), 425–442.

USCCR (1959). *Report of the United States Commission on Civil Rights*. Washington DC: Government Printing Office.

USCCR (1961). *1961 U.S. Commission on Civil Rights Report Book 1: Voting.* Washington DC: Government Printing Office.

USCCR (1965). *The Voting Rights Act...the first months*. Washington DC: Government Printing Office.

USCCR (1990). *Intimidation and violence: racial and religious bigotry in America : a restatement.* Washington DC: Government Printing Office.

Valelly, R. M. (2009). *The two reconstructions: The struggle for black enfranchisement.* University of Chicago Press.

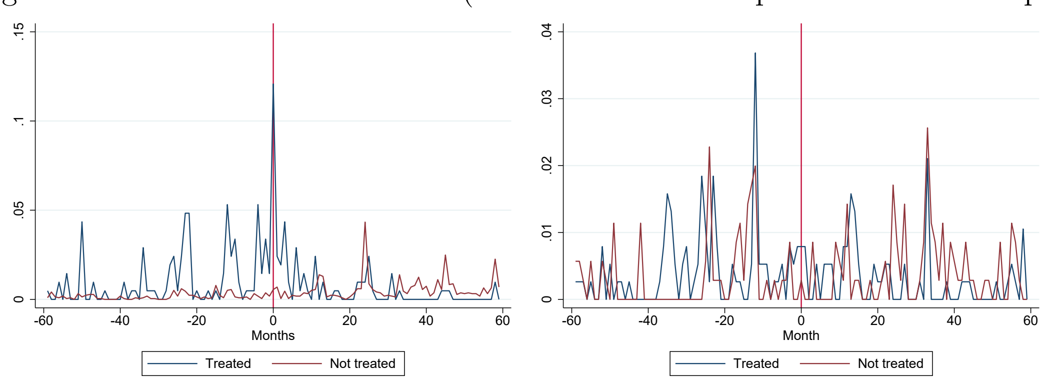
Wasow, O. (2020). Agenda seeding: How 1960s black protests moved elites, public opinion and voting. *American Political Science Review*, 1–22.

A Appendix - Parallel trends: Additional evidence

This section investigates the parallel trend assumption. Appendix A.1 presents two figures showing how much the selection of a sample close to the treatment border reduces the differences in the level and cycles of violence between the treated units and the control group. A formal test of parallel trends in Appendix A.2 confirms this first intuition. This test estimates the difference in trends between VRA counties and others during the pre-treatment period and controls for other counties' trends in different ways (by either adding a trend or month fixed effects). The trend specific to VRA counties never has a significant coefficient. This suggests that counties under VRA coverage did not experience different dynamics of violence than other counties within the 100km buffer before coverage. This formal test of the parallel trend is further backed with a placebo test using alternative timings to determine whether the actual treatment date explains baseline results or if it picks up pre-treatment events (Appendix A.3). The logic of this test is the following. Pre-1965 placebo treatments capture the actual treatment and some noise correlated with the evolution of violence in covered counties before the VRA passed. Should this noise explain baseline results, then the placebo treatment would be significant. Otherwise, the noise would make placebo treatments less significant than the actual treatment. Results in Appendix A.3 show this second direction. The treatment is significant only from 1965 onwards. Comparing the placebo treatment of 1964 with the one of 1965, we observe a drop in the incidence rate ratio and also an increase in statistical significance, indicating a discontinuity at this date.

A.1 Parallel trends

Figure A1: Parallel trends – Levels (difference whole sample – restricted sample)



Average number of instances of political violence before and after the VRA passed. Left-panel: Whole US / Right-panel: Sample limited to counties within a 100km buffer around the border between covered and non-covered counties.

A.2 Formal test of difference in trends before treatment

	(A.2.1)	(A.2.2)	(A.2.3)	(A.2.4)	(A.2.5)	(A.2.6)	(A.2.7)	(A.2.8)	(A.2.9)	(A.2.10)	(A.2.11)	(A.2.12)
Dependent variable	Log($\text{Inc}_{c,t}$+1)	Log($\text{Inc}_{c,t}$+1)	Log($\text{Inc}_{c,t}$+1)	$\text{Onset}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Incidence}_{c,t}$	$\text{Incidence}_{c,t}$	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Onset}_{c,t}$
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson
VRA X Time Trend	2.90e-05	2.24e-05	2.24e-05	2.78e-05	2.89e-05	2.89e-05	0.00549	0.00549	0.00842	0.00476	0.00497	0.00458
	(1.067)	(0.895)	(0.895)	(1.073)	(1.034)	(1.031)	(0.480)	(0.480)	(0.715)	(0.392)	(0.384)	(0.376)
Constant	0.00103**	0.000775*	0.00243	0.000996**	0.00100**	0.00404*						
	(2.497)	(1.940)	(1.461)	(2.509)	(2.296)	(1.731)						
Control	Trend	Trend	FE	Trend	Trend	FE	Trend	Trend	FE	Trend	Trend	FE
Control for $\text{Inc}_{c,t-1}$	No	Yes	Yes	No	No	No	No	Yes	Yes	No	No	No
Drop if $\text{Inc}_{c,t-1}$>0	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Observations	43,860	43,129	43,129	43,860	43,016	43,016	3,422	3,422	3,422	3,480	3,310	3,310
R-squared	0.000	0.025	0.028	0.000	0.000	0.003						
Number of counties	731	731	731	731	731	731	58	58	58	58	58	58

Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Dependent variables are the: log number of events tagged as political violence (variable equal to Log($\text{Inc}_{c,t}$+1) - Columns A.2.1, A.2.2 and A.2.3), the number of events tagged as political violence (Columns A.2.7, A.2.8, A.2.9) or a dummy variable coding the onset of a new wave of political violence (Columns A.2.4, A.2.5, A.2.6, A.2.10, A.2.11 and A.2.12). The equation is estimated either by OLS (Columns A.2.1 to A.2.6) or by Poisson (Columns A.2.7 to A.2.12). Controls include either a time trend (Columns A.2.1, A.2.2, A.2.4, A.2.5, A.2.7, A.2.8, A.2.10 and A.2.11) or month fixed-effects (A.2.3, A.2.6, A.2.9 and A.2.12). All estimations have county fixed effects. Columns A.2.2, A2.3, A2.8 and A2.9 add the lagged control variable as control.

A.3 No selection into treatment - Timing

Treatment in	(A.3.1) 08/62	(A.3.2) 08/63	(A.3.3) 08/64	(A.3.4) 08/65	(A.3.5) 08/66	(A.3.6) 08/67	(A3.7) 08/68
$\text{Inc}_{c,t-1}$	0.438***	0.416***	0.444***	0.438***	0.434***	0.420***	0.437***
	(5.468)	(5.298)	(5.592)	(5.412)	(5.242)	(4.884)	(5.567)
VRA	0.181	-0.606	-0.524	-0.800**	-0.900**	-1.326***	-1.165***
	(0.391)	(-1.443)	(-1.399)	(-2.149)	(-2.447)	(-3.610)	(-2.774)
IRR	1.20	0.55	0.59	0.45	0.41	0.27	0.31
Nb Counties	102	102	102	102	102	102	102
Obs	12,138	12,138	12,138	12,138	12,138	12,138	12,138

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the number of events considered as political violence per county-month (incidence). Each column defines a placebo date (month and year) for treatment from which the treatment variable is equal to 1 for the counties that were under VRA coverage. The lagged dependent variable is added as a control. All estimations include county fixed effects and time fixed effects. IRR stands for incidence rate ratio and is the ratio of the dependent variable in the treated sample compared to the control sample.

B Appendix - Robustness checks: Alternative estimators

Appendix B further shows that baseline results hold when using different estimators. Appendix B.1 uses OLS estimators and transforms the dependent variable two ways, a log transformation (Columns B.1.1 to B.1.4) and an inverse hyperbolic sine transformation (Columns B.1.5 to B.1.8). In each case, the estimator remains significant at least at the 10-percent level. Appendix B.2.2 applies the dynamic panel error correction model developed by Breitung et al. (2021) to correct for the bias in this type of model (Nickell, 1981). Estimations follow from models controlling for either one lag (Columns B.2.2.1 to B.2.2.3) or two lags of the dependent variable (Columns B.2.2.4 to B.2.2.6) and alternately use the log transformation of the incidence of violence or its inverse hyperbolic sine transformation as the dependent variable. In all cases, the estimators are significant at least at the 10-percent level. Appendix B.3 uses several other estimators. First, conditional logit is used to estimate models with Onset as the dependent variable (Columns B.3.1 to B.3.4). Next, Equation 1 is estimated on a dataset with observations collapsed at the county-year level (Columns B.3.5 and B.3.6). Columns B.3.7 and B.3.8 present the estimations of a spatial panel model. All these estimators are significant and of a magnitude similar to the baseline estimates.

B.1 Estimations using OLS

Table B.1: Estimations using OLS

	(B.1.1)	(B.1.2)	(B.1.3)	(B.1.4)	(B.1.5)	(B.1.6)	(B.1.7)	(B.1.8)
	Log($\text{Inc}_{c,t}$+1)	Log($\text{Inc}_{c,t}$+1)	Log($\text{Inc}_{c,t}$+1)	Log($\text{Inc}_{c,t}$+1)	IHS $\text{Inc}_{c,t}$	IHS $\text{Inc}_{c,t}$	IHS $\text{Inc}_{c,t}$	IHS $\text{Inc}_{c,t}$
Sample	Whole US	200km	150km	100km	Whole US	200km	150km	100km
VRA VRA	-0.00337***	-0.00221***	-0.00194**	-0.00156*	-0.00433***	-0.00283***	-0.00250**	-0.00201*
	(-6.257)	(-3.173)	(-2.467)	(-1.806)	(-6.249)	(-3.160)	(-2.462)	(-1.804)
Constant	0.00154**	0.00255*	0.00201	0.00255	0.00197**	0.00327*	0.00254	0.00323
	(2.113)	(1.809)	(1.520)	(1.522)	(2.117)	(1.802)	(1.514)	(1.516)
Control $\text{DV}_{c,t-1}$	YES	YES	YES	YES	YES	YES	YES	YES
Obs	369,852	134,946	111,146	86,989	369,852	134,946	111,146	86,989
Nb of counties	3,108	1,134	934	731	3,108	1,134	934	731

Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Estimator: OLS. Sample: Within the 100km buffer. Log($\text{Inc}_{c,t}$+1) stands for the logarithmic transformation of the Incidence variable. IHS $\text{Inc}_{c,t}$ stands for the Inverse Hyperbolic Sine transformation of the Incidence variable. The lagged dependent variable is added as a control. All estimations include county fixed effects and month fixed effects.

B.2 Nickell Bias

B.2.1 Model without lagged dependent variable

Table B.2.1: Non-dynamic panel model

	(B.2.1.1)	(B.2.1.2)	(B.2.1.3)	(B.2.1.4)
Dependent variable	Incidence	Incidence	Incidence	Incidence
Sample	Whole US	200km	150km	100km
VRA	-1.818***	-0.978***	-0.839**	-0.788*
	(-7.806)	(-2.753)	(-2.067)	(-1.794)
IRR	0.16	0.38	0.43	0.45
Control $\text{Incidence}_{c,t-1}$	No	No	No	No
Observations	39,600	20,160	17,280	12,360
Nb of counties	330	168	144	103

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the number of events considered as political violence per county-month (Incidence). Equation 1 is also estimated on different samples from the whole US down to counties falling into the 100km buffer around the border between covered and non-covered counties. All regressions include county fixed effects and month fixed effects. IRR stands for incidence rate ratio and is the ratio of the dependent variable in the treated sample compared to the control sample.

B.2.2 Dynamic panel error correction model

Table B.2.2: Dynamic panel error correction models

	(B.2.2.1)	(B.2.2.2)	(B.2.2.3)	(B.2.2.4)	(B.2.2.5)	(B.2.2.6)
Dep Variable	Log($Inc_{c,t}$+1)	IHS $Inc_{c,t}$	$Onset_{c,t}$	Log($Inc_{c,t}$+1)	IHS $Inc_{c,t}$	$Onset_{c,t}$
VRA	-0.00154*	-0.00200*	-0.00172**	-0.00153*	-0.00198*	-0.00174**
	(-1.811)	(-1.808)	(-2.170)	(-1.915)	(-1.911)	(-2.302)
Constant	0.00252	0.00320	0.00410*	0.00141	0.00179	0.00150
	(1.522)	(1.516)	(1.736)	(1.109)	(1.106)	(1.089)
Nb Lags	1	1	1	2	2	2
Obs	86,989	86,989	86,989	86,258	86,258	86,258

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Estimator: Breitung et al. (2021) bias-corrected estimator for dynamic panel data models. Sample: Within the 100km buffer.Log($Inc_{c,t}$+1) stands for the logarithmic transformation of the Incidence variable. IHS $Inc_{c,t}$ stands for the Inverse Hyperbolic Sine transformation of the Incidence variable. $Onset_{c,t}$ is a variable equal to one when a new wave of violence starts. All estimations include county fixed effects and month fixed effects.

B.3 Alternative estimators

Table B.3: Alternative estimators

	(B.3.1)	(B.3.2)	(B.3.3)	(B.3.4)	(B.3.5)	(B.3.6)	(B.3.7)	(B.3.8)
Dep variable	$Onset_{c,t}$	$Onset_{c,t}$	$Onset_{c,t}$	$Onset_{c,t}$	$Incidence_{c,t}$	$Onset_{c,t}$	Log($Inc_{c,t}$+1)	Onset
Sample	Whole US	200km	150km	100km	100km	100km	100km	100km
Dataset	Month	Month	Month	Month	Year	Year	Month	Month
Estimator	C. Logit	C. Logit	C. Logit	C. Logit	Poisson	Poisson	SAR	SAR
VRA	-1.601***	-0.988***	-1.002***	-0.934**	-0.912*	-0.923**	-0.00158*	-0.00168**
	(-8.203)	(-3.208)	(-2.901)	(-2.257)	(-1.833)	(-2.057)	(-1.830)	(-2.057)
Control $Incidence_{c,t-1}$	No	No	No	No	Yes	No	Yes	No
Drop if $Incidence_{c,t-1}$>0	Yes	Yes	Yes	Yes	No	Yes	No	Yes
Observations	38,041	19,478	16,692	11,911	864	718	86,989	86,989

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Samples: Whole US: not restricted; 200km: within a 200km buffer around the border between treated counties and others; 150km: within a 150km buffer around the border between treated counties and others; and 100km: within a 100km buffer around the border between treated counties and others. Estimators: C.Logit : Conditional logit / Poisson / SAR: Spatially autocorrelated errors. Dataset: Month: Dataset at the county-month level / Year: Dataset at the county-year level. $Onset_{c,t}$ is equal to one if a wave of violence starts in county c at time t. $Incidence_{c,t}$ is equal to the number of violent events in county c at time t. All estimations include county fixed effects and month fixed effects.

C Appendix - Identification - Supporting tests

Appendix C ultimately presents more evidence in support of the identification strategy. Appendix C.1 first adds month-state fixed effects to the baseline estimation to exploit some of the within-state variation in VRA coverage available in North Carolina. All estimates are significant at the 1 percent level, and incidence rate ratios are either of the same magnitude than baseline estimates or lower. Appendix C.2 further tests if the selection of counties with low turnout ratios into coverage explains baseline treatments. To do so, within the 100km buffer it considers a placebo treatment equal to one for counties with a turnout below 50 percent and zero otherwise (Columns C.2.1 and C.2.2). It then divides this placebo treatment between actually treated counties (Columns C.2.3 and C.2.4) and non-treated counties

(C.2.5 and C.2.6). The treatment turns negative and significant in the former case but not in the latter. This indicates that in the few cases of counties with low turnout that did not fall under the coverage of the VRA, violence increased. Accordingly, baseline estimates are not biased because of the selection of low turnout counties into treatment. Appendix C.3 further shows that the 50 percent threshold for treatment has not been manipulated since there is no bunching in the distribution of turnout in the sample. Finally, Appendix C.4 presents various estimates comparing more geographically close counties. Appendix C.4.1 uses geographic matching to match each treated unit with the closest county or counties. I then compare the difference in the level of violence between the treated group with this set of closest counties both before and after the treatment. No matter the number of counties in the comparison group, there is no statistically significant difference between the treated group and the control group before treatment. Differences nevertheless materialize after the treatment and counties under VRA experience less violence. Appendix C.4.2 uses the border between treated and non-treated counties to document a similar decrease in violence. These estimates do not indicate a discrete change in other county characteristics at the same border before the VRA (Appendix C.4.3).

C.1 State-month fixed effects

Table C.1: Poisson model with state-months fixed effects

	(C1.1)	(C1.2)	(C1.3)	(C1.4)	(C1.5)	(C1.6)	(C1.7)	(C1.8)
Dep Variable	$\text{Inc}_{c,t}$	$\text{Inc}_{c,t}$	$\text{Inc}_{c,t}$	$\text{Inc}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Onset}_{c,t}$
Sample	Whole US	200km	150km	100km	Whole US	200km	150km	100km
VRA	-1.816***	-1.368***	-1.294***	-1.418***	-1.428***	-1.023***	-1.007***	-1.337***
	(-8.674)	(-4.379)	(-3.567)	(-2.904)	(-7.111)	(-3.448)	(-2.837)	(-2.717)
Constant	-0.726***	-0.524***	-0.473***	-0.371***	-1.490***	-1.170***	-1.063***	-0.680***
	(-16.15)	(-7.199)	(-5.568)	(-2.803)	(-68.10)	(-16.26)	(-11.25)	(-5.774)
IRR	0.16	0.25	0.27	0.24	0.24	0.36	0.37	0.26
Control $\text{Incidence}_{c,t-1}$	Yes	Yes	Yes	Yes	No	No	No	No
Drop if $\text{Incidence}_{c,t-1}$>0	No	No	No	No	Yes	Yes	Yes	Yes
Observations	9,549	2,557	1,854	908	7,991	2,179	1,601	763

Robust z-statistics in parentheses:*** p<0.01, ** p<0.05, * p<0.1. Estimator: Poisson. Sample: Whole US: not restricted; 200km: within a 200km buffer around the border between treated counties and others; 150km: within a 150km buffer around the border between treated counties and others; and 100km: within a 100km buffer around the border between treated counties and others. $\text{Inc}_{c,t}$ is the number of violent events occuring in a county c at time t. $\text{Onset}_{c,t}$ is equal to one when a new wave of violence starts in county c at time t. All estimations include county and time fixed effects. IRR stands for Incidence Rate Ratio.

C.2 No selection into treatment - Turnout criterion

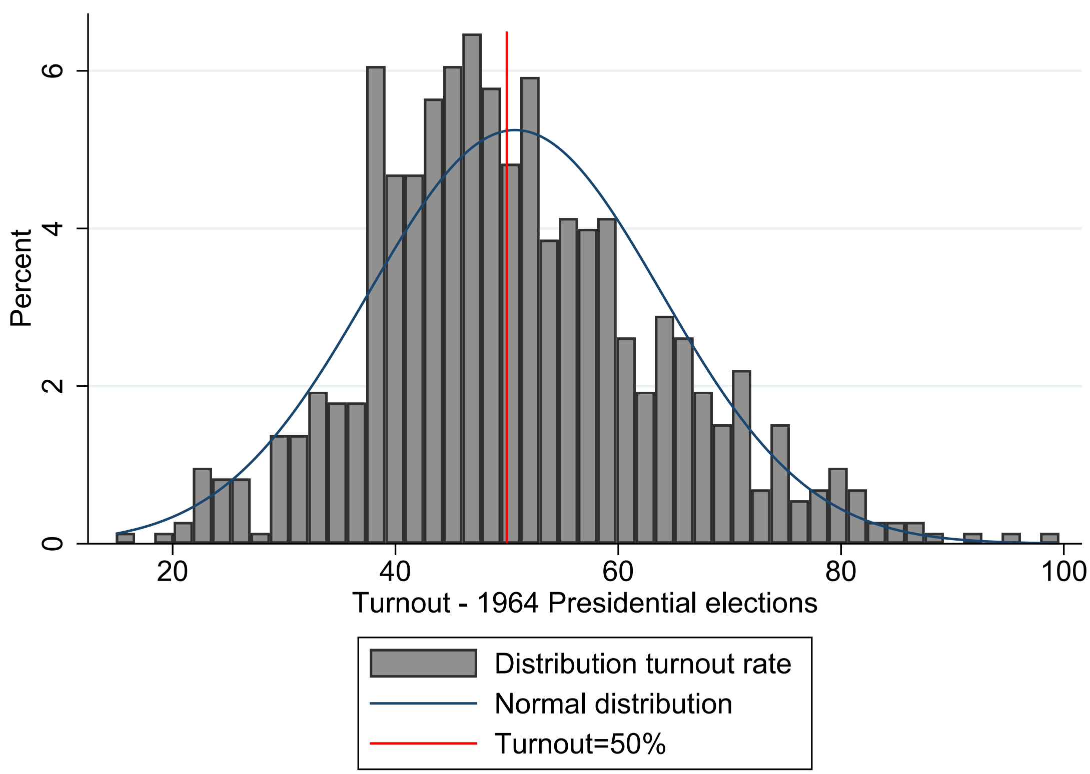
Table C.2: Testing selection on turnout

	(C.2.1)	(C.2.2)	(C.2.3)	(C.2.4)	(C.2.5)	(C.2.6)
Dependent Variable	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$	$\text{Incidence}_{c,t}$	$\text{Onset}_{c,t}$
Placebo treatment= Turnout_{1964}<50% X Considered as treated =1 at the treatment time						
Considered as treated	All	All	VRA	VRA	No VRA	No VRA
$\text{Incidence}_{c,t-1}$	0.498***		0.498***		0.478***	
	(5.468)		(5.672)		(5.072)	
Considered as treated	-0.542	-0.519	-0.655*	-0.639*	0.561	0.504
	(-1.458)	(-1.408)	(-1.826)	(-1.831)	(0.730)	(0.694)
IRR Treatment	0.58	0.60	0.52	0.53	1.75	1.66
Control $\text{Incidence}_{c,t-1}$	Yes	No	Yes	No	Yes	No
Drop if $\text{Incidence}_{c,t-1}>0$	No	Yes	No	Yes	No	Yes
Observations	12,019	11,817	12,019	11,817	12,019	11,817
Number of counties	101	101	101	101	101	101

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Each column estimates Equation 1 when defining a placebo treatment variable instead of the actual treatment. The dependent variable is the number of events tagged as political violence (Columns C.2.1, C.2.3 and C.2.5) or a dummy variable coding the onset of a new wave of political violence (Columns C.2.2, C.2.4 and C.2.6). All estimations include county fixed effects and month fixed effects.

C.3 No manipulation at the 50% threshold

Figure C.3: Distribution - Turnout rate at the 1964 presidential election



C.4 Estimates at the border

C.4.1 Geographic matching at the discontinuity

Table C.4.1: Geographic matching at the discontinuity

(Buffer=100km)

(C.4.1.1)	(C.4.1.2)	(C.4.1.3)	(C.4.1.4)	(C.4.1.5)	(C.4.1.6)
Before VRA	After VRA	Before VRA	After VRA	Before VRA	After VRA
1 Match		3 Matches		5 Matches	
-0.013	-0.122**	0.007	-0.065*	-0.006	-0.072**
(-0.40)	(-2.35)	(0.29)	(-1.91)	(-0.23)	(-2.31)
3655obs	3655obs	3655obs	3655obs	3655obs	3655obs

Robust z-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Each column presents the results of a matching exercise before and after the reform. Each observation at the county-year level is matched with its nearest neighbour using latitude, longitude and year to match observations. The pre-treatment period and the post-treatment period both contain 3,655 observations.

C.4.2 RDD estimates at the border

Dep Variable	(A.6.2.1) Incidence	(A.6.2.2) Incidence	(A.6.2.3) Incidence	(A.6.2.4) Incidence
Panel A: Before the VRA				
Robust RDD estimator	-0.00112	-0.00133	-0.00280	-0.00325
	(0.00490)	(0.00509)	(0.00565)	(0.00593)
Observations	42,360	41,654	42,360	41,654
Panel B: After the VRA				
Robust RDD estimator	-0.232***	-0.143***	-0.300***	-0.210***
	(0.0495)	(0.0352)	(0.0674)	(0.0529)
Observations	42,240	42,240	42,240	42,240
Control		All$_{-1}$		All$_{-1}$
Bandwidth	MSE	MSE	CER	CER

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the number of events tagged as political violence . Each Column presents two RDD estimates of the effect of the VRA on political violence. Panel A presents results when estimating the defined regression on the panel of observations before the VRA. Panel B presents results when estimating the defined regression on the panel of observations after the VRA. Estimations in Columns A.6.2.2 and A.6.2.4 add the lagged realization of violence as an extra control. The definition of the bandwith is as follows: MSE: Mean squared errors / CER: Coverage Error Rate. Kernel: Epanechnikov.

C.4.3 RDD estimates at the border - Placebo test

Variable	(A.6.3.1) Pop density	(A.6.3.2) Pop Growth	(A.6.3.3) Non-white Pop	(A.6.3.4) Pop >21y.o	(A.6.3.5) Vote Dem	(A.6.3.6) No Educ	(A.6.3.7) Migrants	(A.6.3.8) Unempl	(A.6.3.9) Farms	(A.6.3.10) Tenant farms	(A.6.3.11) Welfare
	-582.1	20.07	8.868	-0.444	-7.043	-0.561	2.818	-1.320	-9.801	0.960	-0.0156
	(-0.318)	(1.206)	(0.879)	(-0.270)	(-0.936)	(-0.180)	(0.528)	(-1.167)	(-0.738)	(0.132)	(-1.622)
Nb of Obs	701	701	701	701	700	701	701	701	669	668	669

Robust z-stat in parentheses: *** p<0.01, ** p<0.05, * p<0.1. RDD estimates are at the border between covered counties and others. Pop Density: Nb of inhabitants per squared km in 1960. Pop Growth: Growth of the population between 1950-1960. Non-white: Share of the population that is non-white in 1960. Pop >21y.o: Population aged 21 or more in 1960. Vote Dem: Vote share of Democrats in the 1960 Presidential elections. No Educ: Percentage of the population (above 25 years old) having received less than 4 years of schooling in 1960. Migrants: Percentage of the population coming from another county in 1960. Unemployment: Unemployment rate in 1960. Farms: Percentage of land being occupied by farms in 1960. Tenant Farms: Percentage of farms being tenant farms in 1960. Welfare: Welfare expenses per capita in 1962. Robust Selection of Bandwidth. Kernel: Epanechnikov.

D Appendix - Variables and descriptive statistics

Variables	Mean	s.d	Min	Max	Definitions and Sources
VRA	0.26	0.44	0	1	=1 if covered by special provisions of the VRA
All violence (Incidence)	0.004	0.079	0	5	Number of violent political events per county per month (Dynamics of collective action dataset - McAdam et al., 2003)
All violence (Onset)	0.002	0.047	0	1	Dummy variable equal to 1 if a new wave of violent political events starts (Dynamics of Collective Action dataset - McAdam et al., 2003)
Segregation-Incidence	0.003	0.074	0	5	Number of violent political events per county per month when the claim or the title of the *New York Times* article indicated concerns regarding segregation (Dynamics of Collective Action dataset - McAdam et al., 2003)
Segregation-Onset	0.002	0.044	0	1	Dummy variable equal to 1 if a new wave of violent political events related to political violence starts (Dynamics of Collective Action dataset- McAdam et al., 2003)
Peaceful protests	0.013	0.23	0	16	Number of peaceful protests per county per month (Dynamics of collective action dataset - McAdam et al., 2003)
Δ(Dem-Rep)$_{66}$	35.68	52.45	-94.84	100	Difference in Democrat vote share and Republican vote share in the 1966 Congress elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
Δ(Dem-Rep)$_{68}$	26.67	50.81	-99.79	99.79	Difference in Democrat vote share and Republican vote share in the 1968 Congress elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
Δ(Dem-Rep)$_{70}$	36.12	49.55	-75.8	100	Difference in Democrat vote share and Republican vote share in the 1970 Congress elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)

Variables	Mean	s.d	Min	Max	Definitions and Sources
% Nixon	31.99	17.76	0.39	99.9	% Votes for Nixon in the 1968 Presidential elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
% Humphrey	28.10	12.89	0.49	99.99	% Votes for Humphrey in the 1968 Presidential elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
% Wallace	38.08	20.49	0	91.6	% Votes for Wallace in the 1968 Presidential elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
$\frac{Wallace_{1968}}{Wallace_{1968}+Nixon_{1968}}$	0.53	0.25	0	0.99	Computed as % Wallace/(% Nixon+% Wallace)
$\frac{TurnoutPres1968}{TurnoutPres1964}$	1.17	0.272	0.56	2.75	Ratio of the Turnout in the 1968 presidential elections / Turnout in the 1964 presidential elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
ΔTurnout Pres$_{68-64}$	6.00	9.127	-43.9	44.6	Difference in turnout rates between the 1968 presidential elections and the 1964 presidential elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
Turnout in 1966	35.20	12.50	0	85.8	Turnout in the 1966 congressional elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
Turnout in 1968	47.22	13.23	3.8	84.1	Turnout in the 1968 congressional elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
Turnout in 1970	34.91	12.18	3.5	84.2	Turnout in the 1970 congressional elections (ICPSR. General Election Data for the United States, 1950-1990. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-11-22. https://doi.org/10.3886/ICPSR00013.v2)
Percent Black Population	22.97	19.53	0	79.1	% Black Population in 1960 (US Census Bureau - 1967)

E Appendix - Literature on the VRA

Appendix E: Literature on the Voting Rights Act

Paper	**Outcome**	**VRA**	**Period**	**Method**	**Result in covered areas**
Aneja and Avenancio-Leon (2019)	Black-White Earning gaps	1965 - 1975	1950-1980	County-pair estimates at the border	Increase in Black relative wage
Ang (2019)	Voter turnout	1975	1960-2016	Difference-in-differences at the state level and county-level Regression discontinuity using turnout cutoff	Increase in turnout Decrease in Democratic support
Bernini et al. (2018)	Change in Black Elected Officials	1965	1962-1980	Triple-difference	Increase in the share of elected black politicians
Besley et al. (2010)	Anti-growth policies	1965	1950-2001	Panel estimations using the 1965 VRA as instrument	Increase in political competition and Decrease in anti-growth policies
Cascio and Washington (2013)	Public spending	1965	1957-1982	Triple-difference	Increase in state transfers Increase in Black students education quality
Eubank and Fresh (2020)	Black State incarceration rates	1965	1946-1982	Difference-in-differences at the state-level	Increase in Black incarceration rates
Facchini et al. (2020)	Incarceration rates	1965	1960-1981	Triple-difference	Decrease in Black incarceration rates
Fresh (2018)	Registration rates, Turnout	1965	1960-1980	Difference-in-differences within North Carolina	Increase in Black registration rate, white registration rate and overall turnout
Husted and Kenny (1997)	Public expenditures	1965-1970	1950-1988	Pooled OLS	Increase in welfare spending but not in other spending
Schuit and Rogowski (2017)	Representatives' Civil Right Support	1965	1959-1999	Difference-in-Differences + Genetic Matching	Increase in Representatives' support to Civil Rights legislation
Shah et al. (2013)	Black Council Representation	1965	1981-2006	MLE model with bounded outcome variable	Increase in Black Council Representation
Thompson (1986)	Electoral Results + Registration	1965	1956-1976	Group Comparison + Matching within North Carolina	Increase in Black voter registration and representation
Timpone (1995)	Registration	1965	1940-1982	Time series analysis - Pulse function	Increase in Black voter registration following the VRA