

# The Political Legacy of Violence: The Long-Term Impact of Stalin's Repression in Ukraine

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Political scientists have long been interested in how indiscriminate violence affects the behavior of its victims, yet most research has focused on short term military consequences rather than long-term political effects. We argue that large scale violence can have an intergenerational impact on political preferences. Communities more exposed to indiscriminate violence in the past will – in the future – oppose political forces they associate with the perpetrators of that violence. We document evidence for this claim with archival data on Soviet state violence in western Ukraine, where Stalin’s security services suppressed a nationalist insurgency by deporting over 250,000 people to Siberia. Using two causal identification strategies, we show that communities subjected to a greater intensity of deportation in the 1940’s are now significantly less likely to vote for ‘pro-Russian’ parties. These findings show that indiscriminate violence systematically reduces long-term political support for the perpetrator.

**Keywords:** indiscriminate violence, elections, civil war, Ukraine, causal inference<sup>1</sup>

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Can exposure to large scale violence shape the the political behavior of affected communities generations later? Previous research has highlighted the formative role of violence in political and economic development (Balcells, 2012; Blattman, 2009; Lupu and Peisakhin, 2015). Yet we know little about how violence affects *long-term* political preferences and attitudes, and whether this effect – previously studied at the individual level – also extends to communities.

We argue that indiscriminate violence reduces long-term political support for the perpetrator. Communities that experienced indiscriminate violence in the past tend to vote – generations later – against political forces they hold responsible for that violence. We empirically evaluate this claim with archival data on the Soviet campaign against nationalist rebels in western Ukraine (1943-1950) – a ‘hard test’ for our theoretical expectations. During this conflict, Stalin’s security services deported over 250,000 local residents to Siberia, defeating the insurgency by physically removing its local support base. If even such militarily ‘successful’ uses of indiscriminate violence reduce political support in the long run, we can expect similar patterns to hold in cases where perpetrators were unable to militarily defeat their opponents at the time.

We employ two independent research designs to estimate the persistent effect of Soviet violence in western Ukraine on election results from 2004 to 2014. First, we use access to Soviet railroads as an instrumental variable, to exploit exogenous variation in deportations due to logistics. Second, we employ a fuzzy regression discontinuity design, exploiting idiosyncratic variation in repression levels due to the discretion of local secret police officials and Communist party leaders. The results are similar across the two designs: contemporary electoral support for ‘pro-Russian’ parties is substantially weaker in communities that the Soviet government repressed more heavily.

Our findings contribute to several strands of research. First is the vast literature on the dynamics of civil conflict, which has overwhelmingly focused on the short-term military effects of violence (Kalyvas, 2006; Lyall, 2009). By demonstrating that violence can resonate across generations, and potentially influence election results decades later, we show

that extant research has been overlooking a significant part of the story. Second, our findings inform a wider debate on whether experiences of violence consolidate or undermine the political identities of affected communities (Balcells, 2012; Campbell, 1998; Lupu and Peisakhin, 2015). Our results support the somewhat provocative thesis that national identity can form in response to harsh repression (Kotkin, 2014), in line with earlier studies on cohesion and nationalism (Gellner, 1983). Finally, our study answers a lingering ‘so what’ question in political science research on historical episodes of violence. As we show, violent events that happened many decades ago are important for political science today because they persistently shape political preferences and identities.

## Past violence, future politics

Our study speaks to two related, but previously disconnected areas of inquiry. The first is international relations literature on violence in civil conflict, which has examined the effects of indiscriminate violence on the battlefield, but not on the ballot box. The second is comparative politics literature on violence and political behavior, which has mainly examined the short-term effects of violence, at an individual, rather than communal level.

Research on armed conflict has long confronted the puzzle of why political actors use indiscriminate violence<sup>1</sup>, what effect it has on its intended targets and the dynamics of conflict (see for example Kalyvas, 2006; Kocher, Pepinsky and Kalyvas, 2011; Lyall, 2009; Metelits, 2010; Weinstein, 2007). Some see indiscriminate violence as an effective military instrument to suppress rebellion – in the short term and at the local level, at least – because it raises the costs of continued resistance (Douhet, 1921) and depletes an opponent’s pool of recruits and resources (Byman, 2016; Lyall, 2009; Zhukov, 2015). Others argue that indiscriminate violence is either ineffective or counter-productive (Arreguín-Toft, 2001; Condra and Shapiro, 2012; Kalyvas, 2006; Kocher, Pepinsky and Kalyvas, 2011; Schutte, 2016). Since indiscriminate violence targets victims based on collective markers rather than their individual

behaviors (e.g., where one lives, not what one has done), such violence cannot incentivize people to act in line with the punisher’s wishes because they expect to be punished irrespective of what they do (Schelling, 1966). Moreover, indiscriminate violence can encourage people to act *against* the punisher precisely to avoid exposure to violence (Kalyvas, 2006; Kocher, Pepinsky and Kalyvas, 2011).

Largely missing from this debate are the far reaching political implications of *community-level* experiences of violence. The logic of arguments on both sides is closely tied to wartime conditions, when the imminent threat of violence colors every political decision. We do not currently know whether short-term military gains from indiscriminate violence can translate into long-term political success, or whether casualties among bystanders make it more difficult to win civilian loyalties in the long run.

Instead of focusing on long-term community-level effects, existing scholarship on the historical legacy of violence has mainly studied *individuals* who have directly experienced violence, and compares them to individuals without such experiences. For instance, Blattman (2009) finds that ex-combatants in Uganda are more likely to vote in elections, and Balcells (2012) argues that individual experiences of violence in the Spanish Civil War affected the ideological preferences of survivors. Lupu and Peisakhin (2015) show that Crimean Tatars’ family experience of Soviet violence affects their political attitudes three generations later.

Beyond the individual level, several recent studies have found that exposure to wartime violence may increase broader patterns of political participation and potentially affect electoral outcomes (De Luca, Verpoorten et al., 2015; Grosjean, 2014). Research on Israeli and Turkish politics has shown that terrorist attacks tend to increase support for parties that take a more hawkish stance toward the attackers (Berrebi and Klor, 2008; Getmansky and Zeitzoff, 2014). Like research on civil conflict, most of these studies have focused on shorter time scales. What little empirical evidence exists on long-term effects suggests that the political impact of violence decays over time (Costalli and Ruggeri, 2016).

## Theoretical expectations

We expect indiscriminate violence to reduce long-term political support for the perpetrator. While agnostic to the simultaneously coercive and alienating effects of indiscriminate violence during conflict, we argue that only the alienating effects exceed a conflict’s duration and solidify into community-level narratives of past victimization.

The starting point of our argument is that it is not necessary for a given individual to experience violence directly to be impacted by it. Violence against some members of the community might impact the beliefs and behaviors of others who were not direct victims. This distinction is particularly important in the case of indiscriminate violence, where perpetrators select targets based on collective criteria (‘where one lives’ not ‘what one does’) and every member of a community is a potential target. If the state deports, arrests or kills 20 percent of a community’s residents, it would not be unreasonable to think that the remaining 80 percent who did not directly experience this violence were nonetheless affected by it in a politically meaningful way. Such experience of common victimization can facilitate group cohesion and increase national identification (see [Gellner, 1983](#); [Stein, 1976](#)).

On a generational timescale, one of the consequences of community-level exposure to indiscriminate violence is a shared belief in a common enemy or threat. Because collective experiences drive collective identity, and collective identity drives political behavior ([Eyerman, 2001](#); [Shamir and Arian, 1999](#)), this backlash is likely to have a more lasting effect on political preferences than coercive threats from a long-gone perpetrator. While the coercive effects of indiscriminate violence operate only during wartime, these inflammatory effects may last considerably longer.

- *Hypothesis*: Past exposure to indiscriminate violence by actor  $i$  reduces future local support for actor  $i$ .

We test this hypothesis by examining the effect of Soviet-era repression on contemporary voting in western Ukraine. Beyond its historical significance as the deadliest conflict in

Europe since the Second World War, the Soviet government's campaign against western Ukrainian nationalists in 1943-1955 has an attractive empirical property: it is a hard test for an adverse relationship between indiscriminate violence and future political behavior. Conflict scholars often cite western Ukraine as a historical example where indiscriminate violence 'worked', at least in the narrow military sense of reducing rebel attacks ([Byman, 2016](#); [Ucko, 2016](#); [Zhukov, 2015](#)). The history of the conflict also reveals several plausibly exogenous sources of variation in violence, which allow us to see if these military gains came at the expense of long-term political loyalty.

## Soviet violence in western Ukraine

In late 1942, western Ukraine was in a state of nature (see [Magosci, 1996](#), 625-637): neither Germans, who had just seized the territory from the Red Army, nor the Soviets who occupied it briefly prior to the German invasion, could credibly claim to control it. In this expanding security vacuum, the sole agents of the Soviet government were red partisan units whose primary objective was to disrupt German communications and logistics in the rear, kill local German collaborators and, eventually, to re-establish Soviet rule ([Gogun, 2012](#); [Statiev, 2014](#)). The partisans' primary local competitor was the Organization of Ukrainian Nationalists (OUN), which originally formed as an activist group seeking an independent, mono-ethnic Ukrainian state. In direct response to the advancements of Soviet partisans, OUN established its own armed wing, the Ukrainian Insurgent Army (UPA).

The Red Army regained formal control over western Ukraine in 1944. Due to the presence of nationalist insurgents, however, Soviet authorities could not establish viable political control in the countryside, and especially in those places where red partisans did not establish an early foothold. What followed was the most protracted and deadly domestic conflict the Soviet Union ever faced. During its combat phase from 1943 to 1955, as conservative estimates suggest, 127,454 persons were killed and another 266,206 were forcibly resettled to

other parts of the USSR.<sup>2</sup>

On paper, Moscow's objective was simple: 'all identified supporters [of OUN-UPA] in Ukraine are to be arrested with confiscation of property and sent to Chernogorsky special camp.'<sup>3</sup> In reality, the Soviets faced an *information problem*: identifying nationalist supporters was difficult and the OUN-UPA went to great lengths to deter the local population from cooperating with Soviet authorities. UPA supreme commander Roman Shukhevych summarized the organization's policy: '[we] should destroy all those who recognize Soviet authority. Not intimidate but destroy' (Statiev, 2010, p. 131). Given the OUN-UPA's uncompromising approach to security and experience in underground operations, in many places, the Soviets lacked basic information on residents' loyalties.

Unable to identify the true 'subversives,' Soviet authorities applied the principle of 'collective responsibility' and began deporting large segments of the local population to Siberia and the Far East. Unlike earlier deportations that forcibly relocated entire ethnic groups (e.g. Chechens, Crimean Tatars), Soviet deportations in Ukraine formally targeted only the families of those suspected of association with nationalists or whose absence could not be accounted for (Statiev, 2010, p. 173). Interpretations of what constituted a 'nationalist supporter' varied widely, often implicating people with no actual involvement or ties to the insurgency. In line with Stalin's 'five percent rule' – as long as five percent of victims are guilty, indiscriminate violence is justified (Gregory, 2009) – the NKVD sometimes deported entire villages. When failing to find the families identified as subversives, the NKVD would sometimes deport another family from the village as a replacement (Statiev, 2010, p. 175).

Moscow saw deportation not as a 'gentler' substitute for lethal violence, but as an escalation. Archival data reveal that deportation occurred in especially violent areas where the NKVD had difficulty identifying OUN-UPA supporters, and where other forms of violence proved insufficient (Zhukov, 2015). Deportation was also an escalation over more general forms of displacement (Balcells and Steele, 2016; Greenhill, 2011). Rather than simply creating an environment in which civilians faced strong incentives to flee, the Soviets went



further by forcibly removing civilians from their homes. In explicitly targeting families and children rather than combatants, deportation was arguably the most indiscriminate form of repression available to the state.

Deportation had a significant negative effect on nationalist violence. All else equal, the average district saw a 38.9 percent decline in OUN-UPA attacks immediately after cases of deportation, compared to a 7 percent increase after conventional counterinsurgency (Zhukov, 2015). This negative effect has reinforced popular perceptions of this conflict as one where indiscriminate violence ‘worked’ (Byman, 2016; Ucko, 2016).

A challenge in estimating the *long-term* effect of deportation on contemporary political preferences is the endogeneity of Soviet violence to the perceived scale of local resistance. Soviet authorities used these methods in what they suspected were nationalist strongholds, and these traditionally ‘anti-Russian’ areas may still have ended up voting ‘anti-Russian’ today, even without Soviet repression. As we show in the next section, however, the historical context reveals two plausibly exogenous sources of variation in Soviet violence: military logistics and the discretion of local leadership.

The Soviets’ deportation capacity was locally limited by their logistical ability to forcibly remove thousands of people from their homes and transport them to far-away regions of the USSR. Large-scale deportations were least costly in areas with direct access to Soviet railroad networks (Zhukov, 2016). The Soviets deported, on average, 102 more people from districts located within 2 miles of the railroad than from districts located farther away.<sup>4</sup> These railroads had a far greater impact on Soviet deportations than on rebel operations. Unlike red partisans in WWII, the OUN-UPA focused a relatively small share of their military activity on railroad sabotage, and – as a result – nationalist violence was not significantly higher in rail-accessible districts.<sup>5</sup>

The topology of this rail network predated the conflict by several decades – the western Ukrainian section of the Soviet rail system was a joint creation of Austria-Hungary, imperial Russia and interwar Poland. This was not a system originally developed to facilitate mass

resettlement to Siberia. An expansion of the network began in 1953, after the conflict largely subsided. Given this history, we can reasonably expect access to mid-century Soviet railroads to be both predictive of deportation’s scale, and orthogonal to contemporary politics (we document this in more detail in Appendix 6).

A second, more idiosyncratic source of variation was the zeal and ambition of local Soviet bosses, which varied discontinuously across the borders of administrative districts. This variation was a consequence of the deliberately localized nature of the anti-OUN campaign. To alleviate the information problem, Nikita Khrushchev – then First Secretary of the Communist Party of Ukraine (CP(b)U) – relied on local party branches and local NKVD officials to oversee the implementation of repression. A resolution by the Lviv regional party committee summarized this delegation of authority:

It is the personal responsibility of secretaries of district committees of the CP(b)U, NKVD and NKGB chiefs, chiefs of garrisons to make extensive use of the forms and methods recommended by the CP(b)U and personally by N. Khrushchev before March 1, to completely eliminate the remnants of the gangs of Ukrainian-German nationalists and ‘OUN underground.’<sup>6</sup>

While central authorities in Moscow dictated the general flavor of repression, its implementation was the responsibility of local political bosses and security services at the district (*rayon*) level – the local nucleus of party and administrative organization in the Soviet political system.<sup>7</sup> The heads of district-level party committees (*raykom*) and district-level secret police branches (NKVD and NKGB) ultimately decided whom to arrest, kill, and deport. As a consequence, repression varied significantly from district to district – in part due to the idiosyncratic preferences and beliefs of local bosses. In the mostly rural Rivne oblast, for instance, the Soviets resettled 611 individuals from Volodymyrets’ district, but only 32 from neighboring Morochne (Zarichne) district – a territory of comparable size and population. Similarly, the Soviets resettled 810 people from Horodok district in Lviv oblast, but ‘only’ 116 from neighboring Yavoriv district. Our empirical strategy exploits this local variation,

to more rigorously take stock of whether and how indiscriminate Soviet violence still affects the politics of this region today.

## Data

To estimate the effect of Soviet violence on Ukrainian voting, we rely on declassified archival data on political violence and polling-station level contemporary election results. The administrative unit at which the NKVD organized its operations was the district, or *rayon*.<sup>8</sup> Because rayon boundaries changed over time, we obtained historical data on administrative boundaries from contemporaneous official Soviet directories and military maps (Main Topographic Directorate of the USSR General Staff; [Presidium of Supreme Soviet of USSR, Information-Statistical Division 1941/1946/1954](#)).

To measure the type and intensity of Soviet violence, we used archival event data first analyzed by [Zhukov \(2015\)](#). These data draw on a combination of declassified incident reports from central, regional and local organs of NKVD, Communist Party of Ukraine (KP(b)U),<sup>9</sup> and collections of OUN-UPA documents ([Sokhan' and Potichnyj, 2002/2003](#)). We used historical administrative boundaries to create rayon level measures of the intensity of Soviet repression (number of people deported) from 1943 to 1955 and indicators of red partisan control in 1942. We also calculated rayon level event counts for OUN-UPA violence, which we use as a covariate in our analyses.

We measure the scale of Soviet indiscriminate violence as the *absolute* number of people deported per rayon. We use absolute numbers rather than proportions for several reasons. First, the deportation quotas Soviet authorities sent to local officials – and the reports local officials sent back – used absolute rather than proportional numbers ([Gregory, 2009](#)). Second, more importantly, Ukraine's contemporary public debate on Soviet-era victimization privileges absolute numbers, even in cases – like the Crimean Tatars – where the proportion was 100 percent.<sup>10</sup> Lastly, we do not have reliable data on the denominator – neither the pre-

WWII 1931 Polish census nor the 1959 Soviet census offer reliable local population estimates for the mid-1940s.<sup>11</sup>

To measure contemporary pro-Russian political preferences (our dependent variable), we construct the variable *pro-Russian vote margin*, which we define as the difference between vote shares received by ‘pro-Russian’ and ‘pro-Western/pro-nationalist’ parties or candidates. To construct this variable, we use polling station-level election data from the Ukrainian Central Electoral Commission (UCEC) from 2004 to 2014. Using their geographic coordinates, we match the polling stations with their respective historical rayons (which do not always align with contemporary administrative boundaries) and then create a rayon-level average margin of pro-Russian votes.<sup>12</sup>

The labels ‘pro-Russian’ and ‘pro-Western/pro-nationalist’ are meant to capture, however imperfectly, two related cleavages in Ukrainian politics: geopolitical orientation and national identity. Although these two cleavages have shaped the Ukrainian party system since its inception (Birch, 2000), they became most vivid after the Orange Revolution in 2004 (Kuzio, 2010). The Party of Regions, its successor Opposition Block, and the Communist Party of Ukraine (successor of the Soviet Communist party) have consistently advocated closer economic and security relations with Russia, recognition of Russian as Ukraine’s second official language, and downplayed the importance of Ukrainian national identity. On the other side, parties like the Petro Poroshenko Bloc, Batkivshchina, Our Ukraine, and Yulia Tymoshenko Bloc have favored closer economic, cultural and military association with the EU and NATO, and – together with nationalist parties like Svoboda and Right Sector – have appealed to Ukrainian national identity in their electoral campaigns.<sup>13</sup>

To ensure that the compared units are geographically proximate and similar on many dimensions, we restrict our sample to the western oblasts of Ukraine.<sup>14</sup> Ukraine is a notoriously polarized country, with very distinct regional patterns of voting, often related to differences in imperial legacies (Peisakhin, 2012). Voters in western Ukraine, which was historically part of the Austro-Hungarian empire and was incorporated into the Soviet Union only after 1939,

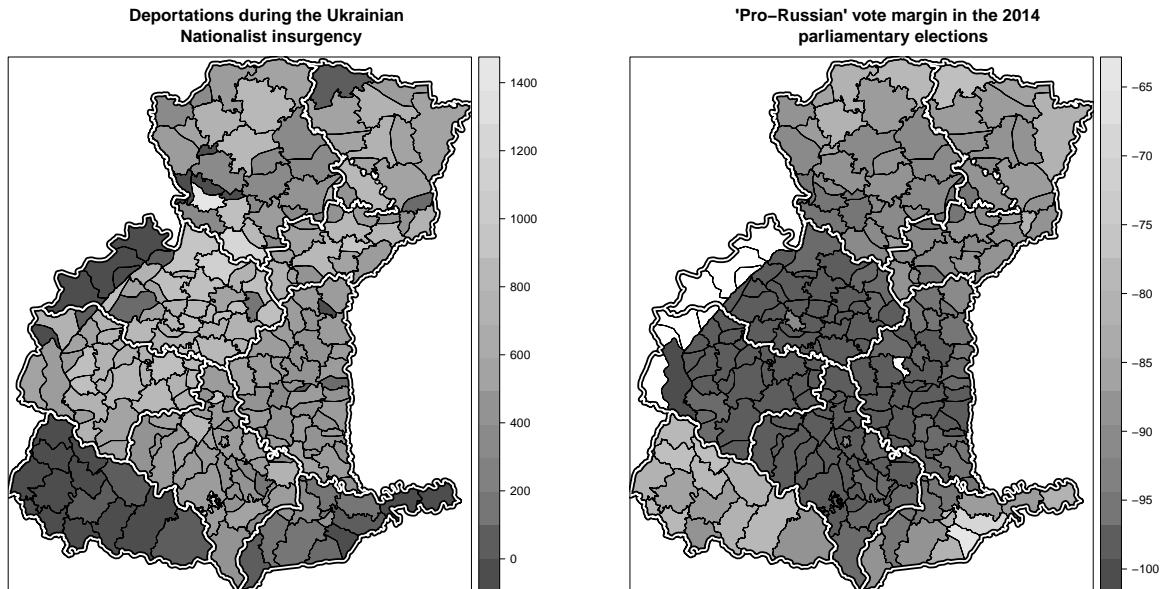


Figure 1: Historical violence and contemporary voting in western Ukraine. The figure on the left shows the counts of deported individuals. The right pane shows the ‘pro-Russian’ vote margin in the 2014 parliamentary elections. The westernmost rayons in white have no election data because the USSR returned them to Poland in 1945. Historical boundaries of *oblasts* appear in white. Please refer to Appendix 2 for residualized maps that account for systematic regional differences.

consistently vote more for Western-oriented and nationalist political candidates and parties. Conversely, voters in eastern Ukraine, incorporated into the Soviet Union by 1922, tend to vote for more ‘pro-Russian’ political forces. By restricting our sample to western Ukraine, we ensure that any variation in current voting patterns cannot be attributed to the historical legacies that make eastern and western Ukraine so different. We account for remaining intra-regional differences within western Ukraine – such as those between the historical regions of Volhynia, Galicia and Transcarpathia – through province (*oblast*)-level fixed effects.

Figure 1 shows the rayon-level distribution of Soviet-era deportations and ‘pro-Russian’ vote-margin in the 2014 parliamentary elections. Table 1 shows descriptive statistics for the main variables. Several patterns are worth noting. First, while a negative correlation between the maps is clearly visible, it is partially driven by historical legacy. Levels of repression and voting partially follow the 1918 partition of western Ukraine between Poland, Czechoslovakia, Hungary, and the USSR. For this reason, we use in all our estimations

Variable	Mean	Std. Dev.	Min.	Max.	N
<i>Explanatory variables</i>					
Deported individuals	521.85	289.13	0	1379	226
Soviet partisan control	0.18	0.39	0	1	226
Soviet partisan operations	0.12	0.32	0	1	226
<i>Outcome variables</i>					
‘Pro-Russian’ margin (parl. 2014)	-93.41	5.26	-98.41	-74.20	226
‘Pro-Russian’ margin (pres. 2014)	-92.98	6.44	-99.26	-65.21	226
‘Pro-Russian’ margin (parl. 2012)	-23.18	17.43	-48.26	45.33	226
‘Pro-Russian’ margin (pres. 2010)	-69.98	23.90	-95.83	41.32	226
‘Pro-Russian’ margin (parl. 2007)	-75.65	17.69	-94.68	-16.14	226
‘Pro-Russian’ margin (parl. 2006)	-62.52	16.75	-87.64	-1.51	226
‘Pro-Russian’ margin (pres. 2004)	-86.78	17.75	-99.42	-10.87	226
<i>Instrumental variables</i>					
Distance to railway (km)	7.04	11.17	0	70	226
Fraction forested	0.37	0.48	0	1	226
<i>Covariates</i>					
Intensity of rebel violence	25.93	25.16	0	199	226
Percent arable land	0.07	0.50	0	5	226
Days under German occupation	1083.90	80.69	953	1244	226
Urbanization	0.47	0.50	0	1	226
Russian-speaking in 1931 (%)	0.36	0.58	0	2.35	210

Table 1: Descriptive statistics.

fixed effects at the level of the oblast (white borders). Second, there is significant local variation *within* oblasts, with some spatially contiguous rayons seeing markedly different levels of repression and voting. This fact – clearly visible in residualized maps that adjust for regional differences (see Appendix 2) – helps us identify the impact of violence at the district level.

## Results

To estimate the effect of WWII-era violence on present-day elections in Ukraine, we employ two identification strategies, at two levels of analysis. First, we analyze election outcomes over historical rayons using instrumental variables. Second, we analyze election outcomes at

the polling-station level with a fuzzy regression discontinuity design.

### *Instrumental Variable Analysis*

Our instrumental variable analysis builds on the following reduced-form equation:

$$y_{ij} = \alpha + \beta x_{ij} + \theta M_{ij} + \gamma L_j + \mathbf{v}_{ij} + \epsilon_{ij} \quad (1)$$

The dependent variable in Equation (1) is the ‘pro-Russian’ vote margin in rayon  $i$ , oblast  $j$  ( $y_{ij}$ ).<sup>15</sup> The explanatory variable of interest is the legacy of violence ( $x_{ij}$ ), as measured by the scale of Soviet deportation.  $M_i$  is a matrix of pre-treatment control variables, including the intensity of nationalist violence, the proportion of a rayon’s territory that the Soviets designated as arable agricultural land, the number of days the rayon spent under German occupation, pre-WWII urbanization (dummy variable indicating the presence of at least one city or ‘urban-type settlement’), and the share of the population that was Russian-speaking prior to the war, based on the 1931 Polish census.<sup>16</sup>  $L_j$  is a vector of oblast-level fixed effects, and  $\epsilon_{ij}$  are iid errors. To account for spatial dependence, we include Moran eigenvectors  $\mathbf{v}_{ij} = v_{ij1}, \dots, v_{ijm}$  as synthetic covariates capturing residual autocorrelation (Dray, Legendre and Peres-Neto, 2006).<sup>17</sup>

The difficulty in identifying the effect of past violence on present-day voting is that contemporary political preferences may have deep historical roots, preceding even the Second World War. If prewar political loyalties drove some of the violence during and after WWII, then the  $\beta$  coefficient in Equation (1) may simply be capturing the effect of previous anti-Russian sentiment on today’s anti-Russian sentiment.

To account for this possibility, we looked for an exogenous source of variation in Soviet deportations, which is likely to affect contemporary voting only through its influence on violent behavior. To this end, we used distance from each district to the railroad network as an instrumental variable for deportation. Because the structure of the rail network in

Ukraine was fixed and predated the bulk of the violence, this variable is causally prior to the Soviet counterinsurgency. To the extent that rail networks affect elections 70 years later, we can reasonably attribute that relationship to their effect on military activity in this period.

The railroad instrument would violate the exclusion restriction if – besides affecting military operations – rail networks drove Soviet-era economic development and industrialization and, in turn, economic policy preferences generations later. However, such patterns are more likely to attenuate than inflate the negative effect of indiscriminate violence on the ‘pro-Russian’ vote margin. Communities that saw more deportation due to their accessibility by rail (i.e. near built-up areas, cities, factories), should also have benefited more from subsequent Soviet economic development, which tended to favor urban development and industrialization at the expense of the countryside.

To facilitate the instrumental variable approach, we decompose the violence-voting relationship from Equation (1) into two stages. In the first stage, we use local railroad access as an instrument for violence:

$$x_{ij} = \mu + \zeta Z_{ij} + \phi M_{ij} + \kappa L_j + \mathbf{v}_{ij} + \mathbf{u}_{ij} + v_{ij} \quad (2)$$

where  $Z_{ij}$  is the instrumental variable, and  $\mathbf{u}$  and  $\mathbf{v}$  are sets of Moran eigenvectors for the first stage and second stage, respectively. The second-stage equation, which estimates the effect of violence on voting, is:

$$y_{ij} = \alpha + \beta^{(IV)} \hat{x}_{ij} + \theta M_{ij} + \gamma L_j + \mathbf{v}_{ij} + \epsilon_{ij} \quad (3)$$

where  $\hat{x}_i$  are the fitted values of  $x_i$  from Equation (2), and the  $\beta^{IV}$  estimate is a local average treatment effect, representing the effect of deportation on voting in districts where deportation was more intense due the proximity of railroads.

We estimated Equations (2-3) for every Presidential and Parliamentary election between 2004 and 2014 for western Ukraine. Table 2 and Figure 2 report standardized coefficient



estimates for  $\hat{\beta}^{(IV)}$ .<sup>18</sup> Appendix 3 reports the full set of parameter estimates.

Our first-stage estimates confirm that deportation was less intense in rayons less accessible by railroads. On average, a one-standard deviation increase in distance to the nearest railroad ( $\uparrow$  6.2 km) reduced the number of local number of deportees by .11 standard deviations ( $\downarrow$  31 deportees per district).<sup>19</sup>

<i>Dependent variable: ‘pro-Russian’ vote margin</i>							
	2014 Parl.	2014 Pres.	2012 Parl.	2010 Pres.	2007 Parl.	2006 Parl.	2004 Pres.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Second stage</i>							
Deportations ( $\hat{\beta}^{(IV)}$ )	-0.175** (0.085)	-0.098 (0.105)	-0.342*** (0.109)	-0.301*** (0.093)	-0.260*** (0.071)	-0.246*** (0.089)	-0.030 (0.093)
OUN-UPA violence ( $\hat{\theta}$ )	0.052 (0.046)	0.004 (0.053)	0.074 (0.058)	0.094* (0.049)	0.068* (0.039)	0.093** (0.047)	-0.004 (0.047)
<i>First stage</i>							
Distance to rail ( $\hat{\zeta}$ )	-0.106** (0.050)	-0.093* (0.047)	-0.102** (0.049)	-0.097** (0.048)	-0.107** (0.049)	-0.101** (0.048)	-0.091* (0.046)
Covariates	Y	Y	Y	Y	Y	Y	Y
Oblast FE	Y	Y	Y	Y	Y	Y	Y
Moran eigenvectors	Y	Y	Y	Y	Y	Y	Y
Observations	217	217	207	217	215	216	218
Adjusted R <sup>2</sup>	0.842	0.812	0.783	0.823	0.875	0.837	0.854
Weak instrument	5.082**	4.495**	4.926**	5.676**	10.485**	7.221**	5.749**
Wu-Hausman test	1.419	0.003	5.169*	4.397*	5.323*	3.475	0.95
Sargan test	15.473	10.918	10.604	5.455	13.899*	8.568	8.423
Moran’s I resid.	-2.728	-1.89	-2.698	-2.976	-3.347	-2.572	-1.709
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01						

Table 2: Instrumental variable regression results. Standardized coefficients, with standard errors in parentheses. Intercept and control variables other than OUN-UPA violence not shown.

The second-stage results show that, in virtually every election cycle since 2004, the ‘pro-Russian’ vote margin was lower in rayons with a high level of post-WWII Soviet deportation. The effect is slightly stronger for parliamentary elections than for presidential ones, but the results are generally consistent in direction and magnitude.

In the parliamentary elections of 2014, the overall vote margin for the Opposition Bloc

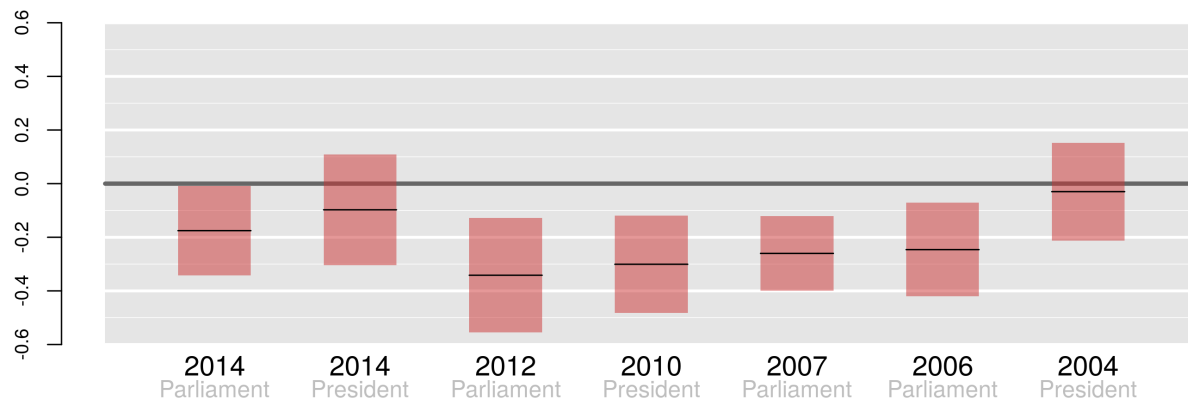


Figure 2: Standardized  $\beta^{(IV)}$  coefficients: deportations. Quantities reported are standard deviation change in the ‘pro-Russian’ vote margin following a one-standard deviation increase in Soviet indiscriminate violence (deportations).

(successor to Party of Regions) was .18 standard deviation lower in areas of high Soviet deportation. These effects were even greater during the 2012 parliamentary elections – which the ‘pro-Russian’ parties won nationally. In those elections, the ‘deportation effect’ was a decline of .34 standard deviations in the vote margin, equivalent to a loss of 6 percent.

If we disaggregate the ‘pro-Russian’ bloc into individual parties, we see that the direction and size of the effects are consistent for mainstream ‘pro-Russian’ parties (e.g. Party of Regions, Opposition Bloc) but are even stronger for parties on the fringe. One such example is the Communist Party of Ukraine (CPU), successor to Ukraine’s Soviet-era ruling party. Dissolved in 2015, following an investigation by the General Prosecutor for ‘financing terrorism,’ CPU had never been popular in western Ukraine. Yet there are places in this region where Communists under-performed their already low expectations – the same places the Soviets heavily repressed 70 years earlier. A standard deviation increase in Soviet deportation reduces CPU’s vote share, on average, by .28 standard deviations, suggesting that Stalin’s repression had a strongly negative effect on the electoral fortunes of the Soviets’ ideological successors.

Past OUN-UPA violence, by comparison, appears to have had a weakly positive impact on the ‘pro-Russian’ vote. In most election cycles, however, areas hard-hit by OUN-UPA

violence were neither more nor less likely to vote for ‘pro-Russian’ parties.

### *Regression Discontinuity Design*

Stalin’s repressive apparatus produced significant local variation, in part due to the autonomy it awarded to state and party agents implementing the deportations ([Hagenloh, 2009](#), p. 9). As we discussed in the historical section, Moscow dictated the general policy line on western Ukraine, but the ultimate judgement on who was to be repressed depended on the idiosyncrasies of the local district-level heads of the NKVD and district-level committees of the Communist party (*raykom*). Consequently, some citizens were exposed to a larger risk of state violence simply by virtue of living in districts where local NKVD and Communist party officials were more willing to repress indiscriminately, more zealous to signal their loyalty to the central authorities through excessive repression, or simply more capable to repress.

As we show in [Appendix 5.1](#), deportation levels varied drastically across spatially contiguous districts. For example, districts that experienced zero deportations were contiguous to other districts, which experienced anywhere from 0 to 1,400 deportations (with mean of 294). After adjusting for regional effects, spatial auto-correlation between districts explains only ten percent of variation in deportation levels. This numerical evidence suggests that idiosyncratic district-level factors were very important in determining patterns of Soviet repression.

Since the Soviets created district boundaries in western Ukraine only a few years prior to post-1943 deportations, we cannot attribute this cross-district variation to some underlying cross-district heterogeneity. Given the historical evidence, these differences are more likely driven by the idiosyncrasies of the security and party apparatus.

To explore the possibility that administrative boundaries drove geographic patterns of repression – and to provide further evidence of the long-term impact of Soviet deportations – we implement a fuzzy regression discontinuity design ([Imbens and Lemieux, 2008](#)). In this analysis, we measure the outcome variable (‘pro-Russian’ vote margin) at the precinct level,

not at the district level as in the IV analyses. The idea here is that a precinct located in a historical district with more repressive local security service personnel and a more vigilant party apparatus experienced a greater risk of deportation than a nearby precinct located in a historically less repressive district. Accordingly, if ‘pro-Russian’ electoral support changes discontinuously as one moves across the border from a historically less repressive to a more repressive district, then we can plausibly attribute this discontinuous effect to the precinct’s historical exposure to repression.

Let  $d_{ij}$  denote a forcing variable – the shortest-path distance (in kilometers) between polling station  $i$  in historical district  $j$  and the border to the nearest other historical district.  $d_{ij}$  takes negative values if district  $j$  had fewer deportations than the district in which precinct  $i$  is located. If district  $j$  had more deportations than  $i$ ’s district, then  $d_{ij}$  takes positive values. For example, the value  $d_{ij} = -3$  means that precinct  $i$  (in district  $j$ ) is located three kilometers away from the border of a historical district that had more deportations than district  $j$ ; thus precinct  $i$  is located in an area with greater risk of deportations.<sup>20</sup>

For the estimates to be meaningful, we compare precincts located across the borders of districts with *contrasting* levels of historical deportations. In our main analysis, we only include those districts in which the levels of deportations were one standard deviation below or above the sample mean. In Appendix 5.3, we show that our results are robust to alternative contrast cut-off rules. In addition, to make sure we are comparing spatially proximate units, we only include polling stations located within ten kilometers of district borders. These restrictions result in a relatively small sample of 935 polling stations across all elections. To gain more statistical power, we pool data from all elections into a single analysis.

We start by estimating the following reduced-form equation:

$$\text{Pro-Russian Margin}_{ij} = \alpha_0 + \alpha_1 \mathbb{1}\{d_{ij} > 0\} + f(d_{ij}) + \epsilon_{ij}, \quad (4)$$

where  $\mathbb{1}\{d_{ij} > 0\}$  is equal to one if the polling station is located in a historical district with

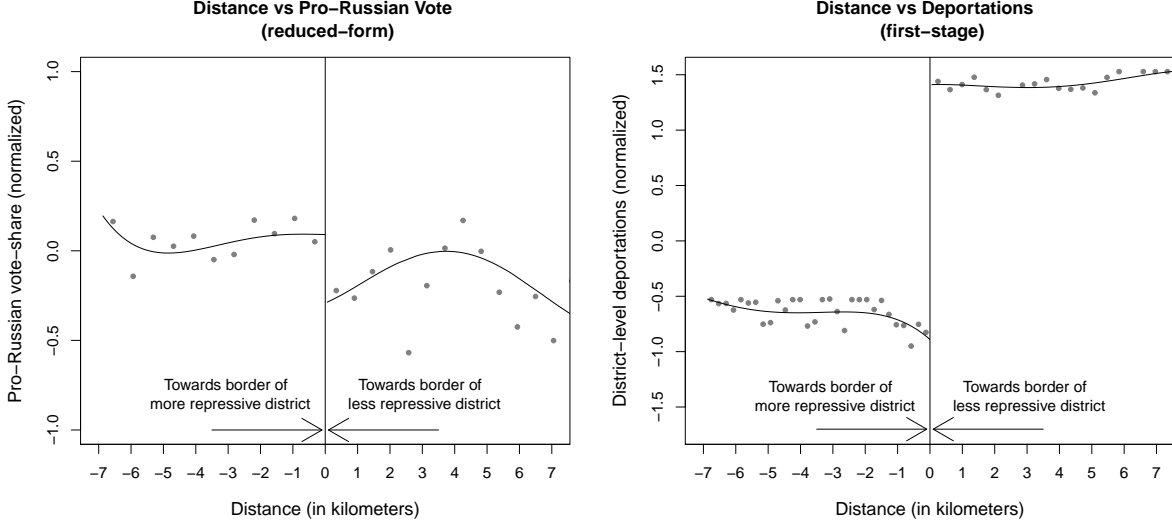


Figure 3: Reduced-form relationships between the instrument (distance from the contiguous district with more repression), deportations, and ‘pro-Russian’ vote.

a high level of repression and zero otherwise.  $f(d_{ij})$  is a smooth function of distance to the nearest district border, which we estimate non-parametrically using the approach in [Calonico, Cattaneo and Titiunik \(2014\)](#). The parameter  $\alpha_1$  captures the spatial discontinuity effect of polling station  $i$  being located in a high-repression historical district on the contemporary ‘pro-Russian’ vote margin.

The estimated reduced-form relationship between the forcing variable and ‘pro-Russian’ support appears in the left panel of Figure 3, which uses the data-driven bin selection method ([Calonico, Cattaneo and Titiunik, 2015](#)). There is a clear discontinuous drop in ‘pro-Russian’ electoral support in more repressive districts. The estimated value of  $\alpha_1$  is equal to  $-0.39$  (S.E. = 0.17, p-value < 0.03), meaning that ‘pro-Russian’ support drops by about 39 percent of a standard deviation as we move from precincts located in historically low-deportation districts to high-deportation districts.

The right panel of Figure 3 depicts the first-stage relationship between the forcing variable  $d_{ij}$  and standardized district-level deportations:

$$\text{Deportations}_j = a_0 + a_1 \mathbb{1}\{d_{ij} > 0\} + f(d_{ij}) + \epsilon_{ij}. \quad (5)$$

	Estimate	S.E.	p-value
1. Bias-corrected	-0.183	0.081	0.024
2. Bias-corrected with robust errors	-0.183	0.094	0.051
3. TSLS without variance weights	-0.171	0.081	0.036
4. TSLS with variance weights	-0.186	0.088	0.036

Table 3: Standardized Fuzzy RD estimates of the deportation effect on current pro-Russian vote. Standard errors in 3 and 4 are clustered at the district level.

There is a very evident discontinuity between the forcing variable  $d_{ij}$  and deportation levels, which is not surprising given that we constructed the forcing variable to have this feature. Given the discontinuities in both reduced-form equations above, we can now estimate the second-stage regression:

$$\text{Pro-Russian Vote}_{ij} = \beta_0 + \beta_1 \text{Deportations}_j + g(d_{ij}) + e_{ij}, \quad (6)$$

where  $g$  is an unknown smooth function and  $\beta_1$  represents the quantity of interest – the fuzzy RD effect of deportations on current ‘pro-Russian’ vote. Table 3 shows the estimated values of  $\beta_1$  using four different estimation methods. For rows 1 and 2, we use bias-corrected local-polynomial estimation with conventional and robust standard errors, respectively, computed using the approach in [Calonico, Cattaneo and Titiunik \(2015\)](#). Row 3 shows the two-stage least squares (2SLS) estimate of  $\beta_1$  using quartic polynomial approximation to the functions  $f$  and  $g$  in Equations (5-6), following the procedure in [Imbens and Lemieux \(2008\)](#). Row 4 shows the weighted 2SLS estimate, which assigns greater weights to precincts located at the borders of districts with more contrasting levels of deportations.<sup>21</sup> Since our measure of deportations varies only at the district level, we cluster the standard errors by districts.

The fuzzy RD estimates are very similar across the four estimation methods: a one standard deviation increase in post-WWII deportations reduced support for ‘pro-Russian’ candidates from 17 to 19 percent of a standard deviation, depending on the estimation method. In three specifications (rows 1, 3, and 4), the estimated effect is significant at the 95 percent confidence level and in one specification (row 2) it is significant at the 90

percent confidence level. Importantly, the estimated effect of deportations is very similar in magnitude to the earlier IV estimates. The numeric similarity of the estimated effects across two independent designs (and using outcomes measured at different levels) is a strong indication these effects are not spurious.

### *Caveats*

As in any non-experimental study, one should interpret the above results with a degree of caution, for several reasons.

First, the IV analyses invoked the exclusion restriction assumption, which requires that the instrument (distance to railways) affects the outcome (‘pro-Russian’ support) only through deportations and not some other channel unaccounted by the covariates ([Angrist and Pischke, 2008](#)). While we cannot entirely eliminate concerns related to the exclusion restriction, we conducted a placebo test for the railways instrument to see how well the exclusion restriction is likely to hold. In the test, we exploited the fact that Zakarpats’ka oblast in the south-western corner of Ukraine experienced very few deportations because the USSR annexed it only in 1946, after the main early waves of deportations. For this reason, we can treat Zakarpats’ka oblast as a placebo case: if the exclusion restriction is violated in a way that biases the  $\hat{\beta}^{(IV)}$  coefficient in the negative direction, then distance to railroads should have a positive reduced-form effect on ‘pro-Russian’ support in precincts located in this placebo region. Our reasoning is that – since distance to railways was unlikely to drive deportations in Zakarpatiia – the reduced-form effect would indicate that distance to railways affected present political loyalties through some other channel than deportations. The results of this placebo test (Appendix 6) support the exclusion restriction assumption.

A second concern is that the railroad instrument may affect rebel violence as well as state violence (e.g. sabotage of government communications), and – because rebel violence may also affect the ‘pro-Russian’ vote – variation in OUN-UPA violence due to rail access may present an alternative pathway from railroads to political preferences.<sup>22</sup> To account for this

possibility, we reran our analyses with OUN-UPA violence as the instrumented variable, and railroad access as the instrument. As we report in Appendix 4, there is little evidence that railroads drove variation in rebel attacks, or that rebel attacks drive voting today.

The fuzzy RD analysis also carries several important caveats. First, the settlements located across borders of districts with contrasting repression levels might not be similar in terms of their pre-treatment characteristics. In Appendix 5.2 we show that our design is well-balanced with respect to settlement-level religious and ethnic characteristics as measured by the 1921 Polish census.<sup>23</sup> It remains possible that the design is not well-balanced with respect to other characteristics for which we do not have settlement-level measures. Strong imbalance that would invalidate the design seems unlikely, because the Soviets created western Ukraine’s administrative structure only mostly right before Germany invasion. It is doubtful that in such a short period of time – most of it under German occupation – spatially proximate settlements became very different from each other by virtue of belonging to different districts.

Second, there is the issue of compound treatment effects: if district bosses had discretion to repress based on their personal capabilities and political vigilance, they could have also done many other things differently (e.g., seize properties, distribute resources, mobilize local population). If so, we cannot ascribe the estimated fuzzy RD effect entirely to deportations. In the absence of reliable data on what the local NKVD and party bosses did in addition to deporting local populations, we cannot eliminate this concern systematically. However, we believe that, even if plausible, the bias created by the compound effect problem is not likely to be large: defeating the nationalist insurgency was by far the most important policy goal for both state security services and the Communist party apparatus during the time, and deportation was Moscow’s main policy tool in this battle.

Finally, because they each rely on some assumptions that we cannot possibly test, the reported IV and fuzzy RDD estimates could potentially be more biased than a simple OLS estimate. Pooling data across all elections and adjusting for regional and election-level fixed effects, the OLS effect of deportations is equal to  $-0.061$  (p-value  $< 0.01$ ). In Appendix



7, we conduct the [Caetano \(2015\)](#) exogeneity test, which suggests that this OLS coefficient most likely *underestimates* the true effect. This is consistent with the fact that fixed effects OLS estimates are substantially smaller than IV and fuzzy RDD estimates.

## Alternative Explanations

The results just presented indicate a strong and consistent relationship between the legacy of Soviet-era violence and voting patterns in contemporary western Ukraine. However, these empirical patterns are consistent with several explanations.

The explanation that we proposed, based on existing scholarship and our knowledge of the Ukrainian case, is that common experiences and memories of violence shape people’s identities, and these identities shape the political preferences of communities impacted by violence ([Eyerman, 2001](#); [Shamir and Arian, 1999](#); ?). Communities exposed to indiscriminate violence shared an experience in which every household was potentially at risk of repression, irrespective of their loyalty toward the Soviet regime. Such communities came to see the Soviet regime as targeting them less for their behavior, than for their identity as Galicians, Volhynians, or simply Ukrainians – an identity which local nationalists defined in opposition to Muscovite, Russian rule. The identity-forming effect of violence was further reinforced by the structural consequences of Soviet indiscriminate violence, which left a relatively high proportion of nationalist ‘identity entrepreneurs’ in affected communities.

Our proposed explanation rests on the claim that the indiscriminate nature of violence matters: because many Ukrainians fell prey to Soviet repression due to *who* they were, rather than their loyalty or disloyalty toward the Soviet state, deportations increased long-term anti-Russian sentiment. However, it is certainly plausible to argue that the nature of violence did not matter and that its long-term impact would have remained the same even if Soviet repression was highly discriminate.

While we cannot rule out this alternative explanation directly, we can assess its plau-

sibility indirectly by comparing the effect of more indiscriminate Soviet deportations with the long-term political effect of more selective violence perpetrated by pro-Soviet partisans during World War II. Red partisans were state-sponsored guerrilla groups that formed and operated in Ukraine during Nazi occupation. In places where red partisans established an early foothold, they forged deeper links to local populations, and cultivated local informants. Here, the NKVD could later rely on existing networks of loyalists, who formed special ‘extermination battalions’ and local self-defense forces (Burds, 1997; Tkachenko, 2000), and used violence on a more discriminate basis (Statiev, 2014, p. 1545). Where the red partisans failed to establish an early foothold, the Soviets had little or no local loyalist network to fall back on after WWII, and compensated for a lack of local information with violence.<sup>24</sup> These patterns are consistent with other conflicts: violence is typically more extreme and indiscriminate where combatants lack territorial control, and are unable to distinguish opponents from bystanders (DeMeritt, 2015; Kalyvas, 2006).

If the nature of violence is indeed irrelevant, then territorial control by red partisans – which serves as a proxy measure for Soviet *capacity* for selective violence – should have a similar effect on current voting patterns as do deportations. We test this possibility by replicating the earlier IV analysis, with WWII-era partisan control as the treatment variable, and forest cover as an instrumental variable. Forest cover allowed the partisans to establish base camps behind German lines, by concealing their activities from surveillance. The forestation instrument is appropriately orthogonal to both the establishment OUN-UPA base camps (due to the nationalists’ superior ability to hide in plain sight among the local population) and the extent of Soviet deportations.<sup>25</sup>

Figure 4 reports the results of this analysis (see Appendix 4 for full table of coefficients). The evidence strongly contradicts the notion that the nature of violence does not determine its long-term political impact. Rayons where red partisans established strongholds – and where Soviet authorities subsequently had better information – see significantly more ‘pro-Russian’ voting today. The normalized magnitude of this effect is one tenth of a standard

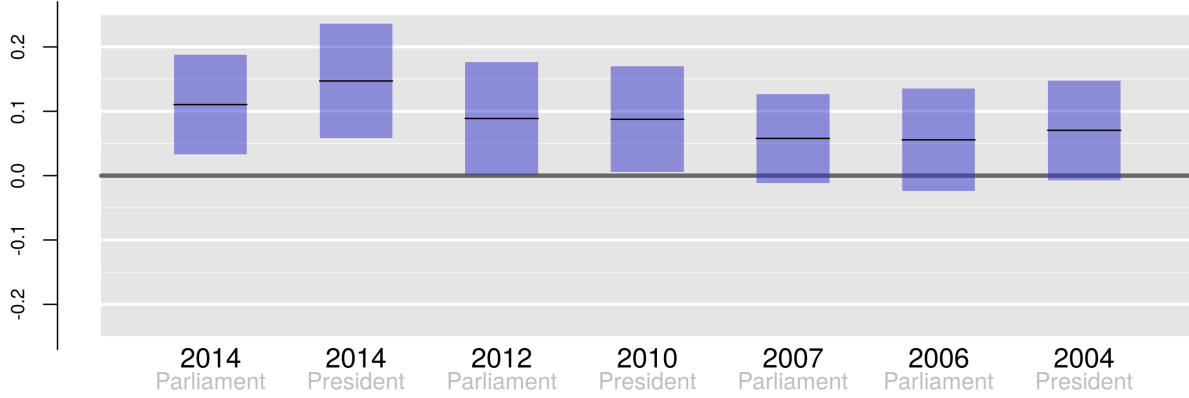


Figure 4: Standardized  $\beta^{(IV)}$  coefficients: partisans. Quantities reported are standard deviation change in the ‘pro-Russian’ vote margin following a one-standard deviation increase in Soviet capacity for selective violence (partisan control in WWII).

deviation in the parliamentary elections of 2010-2014, and .15 standard deviations in the 2014 presidential race. While indirect, this test indicates that more selective tactics may have *improved* long-term support for Moscow.

The second alternative explanation is that the Soviet violence has significantly impacted ethno-linguistic composition. Hence, we see less support for ‘pro-Russian’ parties in more historically violent areas, because those areas simply have a lower proportion of Russian speakers. New settlers from eastern Ukraine and Russia may have viewed formerly repressed areas as hostile to Russian speakers, and instead settled in places they found more welcoming, like former partisan strongholds.

To test the plausibility of this alternative explanation, we implement two types of tests. First, we check whether Soviet violence had an impact on modern ethno-linguistic composition. Second, we check whether Soviet violence continues to affect voting after we adjust for modern ethno-linguistic composition.<sup>26</sup> If the effect attenuates significantly after this post-treatment adjustment, the ‘ethnic composition mechanism’ may be a plausible explanation of our results. To measure ethno-linguistic composition, we use rayon-level differences in the percentage of Russian to Ukrainian speakers, as measured in the 2001 Ukrainian census. The results, reported in Appendix 9, show that Soviet violence did not have a discernible

impact on modern linguistic composition. Moreover, the estimated effect of Soviet violence on contemporary voting continues to be very similar to our earlier estimates, after controlling for post-treatment linguistic composition and other potential confounders.<sup>27</sup>

While the above analyses indicate that some alternative explanations of our findings are not plausible given the evidence, we certainly cannot rule out other possibilities, including the effect of repression on economic development, family structure, and social networks, among other things. Given the long-term political significance of trauma in post-conflict societies, understanding the intermediate effects of violence on the structure, identity and decision-making calculus of affected communities should be a priority area for future research.

## Discussion

The main goal of this paper was to identify the long-term community-level political effect of historical violence. Using data on Soviet repression and contemporary voting in western Ukraine, we found that the Soviet policy of forced population resettlement had significant *negative* long-term effects on local support for ‘pro-Russian’ parties. We arrive at this result through two independent identification strategies – an instrumental variable design that exploits exogenous variation in violence due to railroads, and a regression discontinuity design that exploits variation due to local Soviet leadership. Both approaches yield the same finding: indiscriminate violence perpetrated by the Soviet state fomented strong anti-Soviet (and, consequently, anti-Russian) sentiment that persisted through generations.

Detecting such an effect should have been difficult in western Ukraine. Recent evidence suggests that Soviet authorities ‘won’ this conflict in part through their systematic use of deportation – physically uprooting the insurgency’s local base of support. These population movements, and the decades of internal migration that followed, should have attenuated ties between Soviet repression and the communities that live in these places today. If the political effect of violence is consistently negative in Ukraine, then this effect is likely stronger in cases

where the perpetrator initially ‘lost.’

These findings have important implications for our understanding of how large scale violence shapes political identities and behaviors in the long run. First, our results suggest that the impact and trauma of violence extend beyond the individual level, and persist at the level of communities, reaching individuals who did not experience violence directly. Second, while we do find that violence potentially creates strong political preferences in opposition to the perpetrators of violence, this effect is not uniform and is highly dependent on the nature of violence.

More broadly, our results suggest that even militarily ‘successful’ cases of indiscriminate violence – of which western Ukraine was certainly an example at the time – may not prevent larger political losses in the long-term. The fact that ‘anti-Russian’ political preferences are currently strongest in localities where Soviet violence was most extreme shows that future scholars should look beyond short-term battlefield effects, and take a deeper look at how such violence shapes post-war political behavior.

## Notes

<sup>1</sup>By ‘indiscriminate violence’ we do not mean only arbitrary or random violence, but also violence singling out particular groups, such as males of military age, members of a particular ethnic group, or inhabitants of localities that are under enemy control.

<sup>2</sup>GA SBU, F.13, D. 373, T. 103, L. 9-11.

<sup>3</sup>People’s Commissariat of Internal Affairs (NKVD) order of January 7, 1944.

<sup>4</sup>The average district was 2 miles (3.2 km) from the closest railroad in 1945.

<sup>5</sup>The KS test statistic for the difference in means (28-21 rebel attacks) is insignificant, with  $p = .19$ .

<sup>6</sup>Information of the Inspector of the Central Committee of the CPSU(b) N. Gusarov on the audit of Ukrainian party organization and the identification of a number of serious

shortcomings in its work,' July 13, 1946. Documents of 20th century <http://doc20vek.ru/node/2329>.

<sup>7</sup>Local discretion in the execution of violence is not unique to western Ukraine (Mitchell, 2004).

<sup>8</sup>Several *rayons* comprise an *oblast*, or province.

<sup>9</sup>Key archival sources include GARF, F. R-9478, Op. 1; GARF, F. R-9479, Op. 1; RGVA, F. 38650, Op. 1; TsDAGOU, F. 1, Op. 23.

<sup>10</sup>Consider, for example, the following headlines from Ukrainian media: 'How communists hastily killed more than 20,000 people in western Ukraine,' *TV Channel 24*, June 25, 2015; 'In one day more than 230,000 Crimean Tatars were deported,' *Uzhorod Inform*, May 27, 2016.

<sup>11</sup>One concern regarding the absolute measure is that population size may confound the relationship between deportation and political preferences. In Appendix 8, we show that this is unlikely.

<sup>12</sup>The UCEC provides the geographic coordinates for 2014 elections. For earlier elections, we geocoded the locations of polling stations using, in combination, the Yandex Maps service and GeoNames geographical database.

<sup>13</sup>Appendix 1 provides our exact coding scheme and classification of parties and candidates.

<sup>14</sup>These include Drohobych, Lviv, Rivne, Stanislaviv, Ternopil' and Volyn' oblasts (which the USSR annexed from Poland), as well as Chernivets'ka (from Romania) and Zakapats'ka oblasts (from Czechoslovakia). The resulting sample size comprises 226 administrative districts (rayons), based on the WWII-era Soviet administrative structure.

<sup>15</sup>The 'pro-Russian' vote margin is the percentage won by Party of Regions + Communist Party of Ukraine – all other parties.

<sup>16</sup>Because the Polish census does not include 16 rayons in Transcarpathia, we estimated models with the Russian language variable separately. Results are consistent with those reported below.

<sup>17</sup>The Moran eigenvector method diagonalizes the  $N \times N$  connectivity matrix  $\mathbf{C}$  (where  $c_{ij} = 1$  if districts  $i$  and  $j$  share a border) to select the set of  $m$  eigenvectors with the largest achievable Moran’s I coefficient of autocorrelation. To prevent multicollinearity, the algorithm extracts eigenfunctions of  $[\mathbf{I} - \mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}']\mathbf{C}[\mathbf{I} - \mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}']$ , where  $\mathbf{X} = [\iota_n \ X \ L]$  is the  $N \times (k + 1)$  matrix of covariates.

<sup>18</sup>To simplify comparisons across models, we report standardized coefficient estimates, which represent the number of standard deviations the outcome would change following a one-standard deviation increase in the explanatory variable.

<sup>19</sup> $\hat{\zeta}$  estimates vary slightly across elections due to different sets of Moran eigenvectors being included in first-stage regressions.

<sup>20</sup>Note that since we are using one-dimensional forcing variable, our design is not the standard *geographic* RDD in which a two-dimensional forcing variable is used (Keele and Titiunik, 2014). Using a two-dimensional forcing variable is complicated in our setting because we employ the fuzzy RD design. The main reason to use a two-dimensional forcing variable is to avoid comparing units that are distant from each other even though they are both close to a common border. This concern is quite negligible in our case because the average length of borders between districts is only 24.4 kilometers—units that are close to a border are also close to each other.

<sup>21</sup>Such weighting accounts for the fact that an RD design is most sensible in cases where neighboring districts have highly contrasting deportation levels.

<sup>22</sup>We are grateful to an anonymous reviewer for raising this point.

<sup>23</sup>For the RDD balance test, we need *settlement*-level demographic data, which we were able to obtain from 1921, but not from 1931 Polish census used in our IV analyses. As noted by Kopstein and Wittenberg (2011), who also use these census data, the 1921 census over-counts the Polish population. Since the variation in over-counting is unlikely to be correlated with *future* deportations, this systematic error in census data is unlikely to bias the results.

<sup>24</sup>For example, in the five districts of Stanislaviv oblast previously controlled by partisans, Soviet authorities conducted 40 percent fewer operations during the conflict than in other parts of the oblast. Similarly, they conducted 25 percent fewer operations in partisan-held districts of Rivne oblast.

<sup>25</sup>Districts with above-average nationalist activity were neither more nor less heavily forested than ones where they were less active. Districts the partisans controlled were 46 percent more heavily forested than the regional average.

<sup>26</sup>We use the same type of IV specification as above in equation 1, with modern ethnolinguistic composition as an additional covariate.

<sup>27</sup>In Appendix 8, we also discuss (and rule out) the possibility that our results are driven by the variation of pre-WWII and contemporary population/urbanization levels.

## Acknowledgements

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