Stalin's Terror and the Long-Term Political Effects of Mass Repression

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Abstract

Repression has a long-term negative effect on political participation. Using millions of arrest records from archival documents, and polling-station level election results, we examine how past exposure to repression during the Stalin era has affected voter turnout in Putin's Russia. To identify the effect of repression on voting, we use an instrumental variable design, exploiting exogenous variation in repression due to the structure of mid-century Soviet railroads, and travel distances to Gulag camps. We find that communities more heavily repressed under Stalin are consistently less likely to vote today. The electoral legacy of Stalin's terror – decades after the Soviet collapse, and across multiple election cycles (2003-2012) – is systematically lower turnout. To show that our result is not unique to the Putin regime, we replicate our analysis in neighboring Ukraine (2004-2014), and find similar patterns. These results challenge emerging findings that exposure to violence increases political participation.

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Reflecting on his years in correctional labor camps, Soviet writer and dissident Varlam Shalamov said, "He who has been there will never forget" (Hosking, 1991). During Josef Stalin's three decades in office, the Soviet Union convicted 3.8 million people for "counter-revolutionary" crimes (GARF, 1954). The Gulag – an acronym for "Main Directorate of Corrective Labor Camps and Labor Settlements" – was among the defining institutions of the USSR. Millions experienced the camps first hand, but many more felt their impact indirectly – through disappearances of friends, neighbors, and the transformation of their communities. What is the long-term legacy of Stalin's terror? Has exposure to political repression in the past made these communities less or more politically active today?

In this article, we empirically examine the effect of Stalin's terror on political participation in contemporary Russia. Using archival arrest records collected by the human rights organization Memorial, we estimate each Russian locality's exposure to repression during the Stalin era, and the effect of this repression on local voting patterns between 2003 and 2012. We find that communities more heavily repressed under Stalin are significantly less likely to vote in Russia's national elections, compared to nearby communities less exposed to Soviet terror.

By itself, a negative correlation does not demonstrate that the terror effect is causal. It is possible that Soviet authorities repressed heavily in areas that already stood in opposition to the federal government, and these initially restive communities continue to mistrust Moscow today. To address this concern, we use an instrumental variable design, exploiting the structure of mid-century Soviet railroads and travel distance to Gulag camps. The Soviet repressive apparatus depended heavily on railroads, which transported prisoners and connected populated areas to Gulags. However, the historical structure of the rail network – most of which predated Stalin's terror – has little direct impact on contemporary voting, apart from its influence on Soviet repression. To the extent that railroads also facilitated migration and economic development, we show that these alternative path-

ways should bias against finding a significant repression effect. Our results confirm that exposure to repression had a long-term negative effect on participation, equivalent to an 8.5 percent drop in local turnout in 2012. We consider the role of electoral fraud in this process, and show – with an analysis of data on Ukraine – that the effect is not unique to Putin's Russia.

We attribute this local decline in turnout to a deterrence of political activity. By punishing individuals for "counter-revolutionary" crimes – real or imagined – and in some cases extending this punishment to family members, the Soviet state raised the expected costs of even seemingly benign political participation. Citizens who lived in communities with a similar level of secret police vigilance developed converging expectations of how likely dissent will be detected (or invented), and how severely it will be punished. Where these costs have historically been high, local norms have come to favor an avoidance of political participation.

These findings make several novel contributions to research on political violence (Kalyvas, 2006; Lyall, 2009), repression (Mason and Krane, 1989; Davenport, 2007), and voting (Colton and Hale, 2009; Treisman, 2011). First, our results reinforce recent findings on the negative consequences of repression for political behavior (Balcells, 2012; Bautista, 2015), and challenge the emerging view that exposure to violence increases participation (Bellows and Miguel, 2009; Blattman, 2009; Bateson, 2012). Second, while past research has emphasized the short-term effects of repression over several months or years (Almeida, 2003; Boswell and Dixon, 1990; Gurr and Moore, 1997), we show that these effects may be durable over generations, sowing long-term distrust of political institutions. Third, unlike recent research on the legacy of Soviet mass deportations (Lupu and Peisakhin, 2016; Rozenas, Schutte and Zhukov, 2017), we show that repression need not be collective or indiscriminate to have community-level effects.

Repression and political participation

The question of 'who participates' in politics matters greatly for public policy and democratic development, because it shapes the set of preferences and opinions to which the government responds.[1] Even in non-democratic states, regimes often look to the electoral process as a source of legitimacy and corrective feedback (Brownlee, 2007; Magaloni, 2006; Schedler, 2006). To the extent that state repression might shape the makeup of an electorate – determining who votes and who stays at home – the electoral legacy of violence is of great importance for the theory and practice of government.[2]

The political effect of repression has been a matter of some debate. Several recent studies have found that exposure to violence increases political engagement (Bellows and Miguel, 2009; Blattman, 2009; Bratton and Masunungure, 2007; García-Ponce and Pasquale, 2015). Explanations for this effect have included backlash mobilization, where communities react to violence by aligning with the perpetrator's opponent (Francisco, 2004), and substitution effects, where victims channel resistance into non-violent forms of contestation (Lichbach, 1987). More recent studies have emphasized 'post-traumatic growth,' where exposure to violence yields psychological effects that increase social cohesion, altruism and collective coping (Bauer et al., 2016; Blattman, 2009; Gilligan, Pasquale and Samii, 2014), and 'expressive participation,' where voting is a means for victims' empowerment (Schuessler, 2000; Bateson, 2012).

An important shortcoming of this literature is its empirical focus on wartime violence by non-state actors and weak states – who are generally

[1] We define *participation* as "actions aimed at influencing the selection of government personnel and/or the actions they take" (Verba and Nie, 1972, 2). While this definition potentially includes protest activity and insurrection, we focus more narrowly on activities "within the system," like participation in the electoral process and voting in particular.

[2] We define *repression* as the use of "physical sanctions against an individual or organization, within the territorial jurisdiction of the state, for the purpose of imposing a cost on the target as well as deterring specific activities and/or beliefs perceived to be challenging to government personnel, practices or institutions" (Davenport, 2007).

unable to conduct violence on a massive scale, and sustain it for long periods of time.[3] The few studies that examined the legacy of repression in the Soviet Union (Rozenas, Schutte and Zhukov, 2017; Lupu and Peisakhin, 2016) have focused on one relatively idiosyncratic form of violence: mass deportation of geographically-concentrated minorities. Both of these contexts are likely to amplify the 'backlash effect' – either due to the perceived weakness of the perpetrator, or the indiscriminate nature of the violence. We do not yet know if such community-level effects exist where repression is more sustained, selective and diffuse, targeting individuals across the country rather than collectively punishing members of a cohesive group.

Another body of research argues that exposure to violence reduces participation in politics (Booth and Richard, 1996; García, 2010; Grosjean, 2014). The most common explanation here is preference falsification (Kuran, 1997), where recently-repressed individuals hide their true political preferences due to fears of renewed violence, and instead publicly express preferences at odds with their own (Bautista, 2015; García-Ponce and Pasquale, 2015). Other mechanisms have included a loss of trust in national institutions (Grosjean, 2014), and apathy (Wood, 2006).

Past research in the second category, with some exceptions (Grosjean, 2014), has considered mainly the short-term impact of repression – in the years and months before the threat of resumed violence has subsided, and before shared community-level experiences have had a chance to coalesce. Because both of these forces are likely to suppress political participation in the short run, we do not yet know how durable these negative effects are.

Theoretical expectations

We expect state repression to reduce long-term participation in politics. While almost all repression aims to deter future political dissent by raising

[3] A recent exception is Bauer et al. (2016), who find strong evidence of cooperative behavior following a wide range of community-level violent experiences. However, their meta-analysis is limited to war-related violence, mostly in Sub-Saharan Africa.

its expected costs, there are several reasons why achieving this deterrent effect can be difficult. Repression is most likely to deter if it is anticipated and avoidable (Schelling, 2008, 2): if individuals expect that the state will detect and punish dissent, but that one can also avoid this punishment by abstaining from the activity in question. These conditions require that the state is strong enough to carry out its coercive threats, and that it does so in a sufficiently selective and predictable manner. Repression is unlikely to deter if its agents are relatively weak political actors with a limited capacity for sustained violence (because 'bad' behavior will go unpunished), or if a strong actor indiscriminately punishes an entire community or ethnic group (because 'good' behavior does not prevent punishment).

In practice, repression is rarely 100 percent anticipated and avoidable. Yet some forms of violence come closer to this ideal than others. Individual arrests, for instance, are more likely to "seem avoidable" than mass deportation, since an individual's alleged political actions – rather than group affiliation or place of residence – is the official basis for repression. Even if this basis is false, bystanders can still learn from the arrest, and try to avoid actions that they expect authorities to misperceive as dissent.

The type of political behavior that falls into this category will vary from place to place, depending on the vigilance of local authorities, and how broadly they are willing to interpret and apply a given statute. Through repeated exposure to local repressive institutions over time, members of a community will converge in their expectations of how costly dissent – or even benign political action – is likely to be. Where these costs have been historically high, local norms of political participation will favor "keeping one's head down" and staying out of politics.

We evaluate the empirical validity of this claim by examining the effect of Stalin-era repression on contemporary voting in Russia. While Stalin's repression took many forms, our focus is on a subset that most closely aligns with the scope of our argument: the imprisonment of individuals under Article 58 of the Soviet penal code ("counter-revolutionary activity").

History of Soviet repression

What made Soviet state terror distinctive was its scale. Based just on the size of its population, the Soviet Gulag system – comprising regular and special prisons, filtration camps, POW camps, corrective labor colonies, special settlements, and scientific prisons – was about twenty-five times larger than its counterpart in Nazi Germany.[4] This population included both political and criminal inmates, the former of whom were typically convicted under Article 58 of the Soviet penal code.

In force between 1927 and 1961, Article 58 established a broad class of "counter-revolutionary" crimes, including treason, insurrection, terrorism, espionage, industrial sabotage, contacts with foreign states, propaganda, agitation, and a failure to report any of the above. Most of these crimes carried mandatory minimum sentences, from six months to ten years. Some offenses, like espionage and treason, were potentially punishable by death.

The range of activities that fell under Article 58 was so wide that even mildly critical or heterodox political statements could become cause for arrest – or concern among others that they could be arrested for failing to report. By creating strong incentives to inform, Soviet authorities drew local communities directly into the repression process. As many inmates landed in the camps following accusations from neighbors, co-workers and family members, the space for public and even private expression of political preferences gradually shrank.

Between 1921 and 1953, the Soviet state convicted 3.8 million people under Article 58 (GARF, 1954). A typical case began with a person's detention, interrogation and (forced) confession, often followed by an expedited trial and conviction by a "special troika" – comprising an NKVD officer, party secretary and prosecutor – and transfer to a labor camp. According to one report, of 1.5 million individuals the NKVD arrested in 1937-1938,

[4] The number of inmates in Soviet concentration camps reached over a million in 1938; German concentration camps held 20,000 German citizens at the time (Snyder, 2012).

troikas convicted 85.4 percent (Getty, Rittersporn and Zemskov, 1993).

Beyond their punitive function, Gulags served an economic purpose, as a source of cheap labor that the state regularly mobilized for large construction works, gold, metal and coal mining, logging and other engineering projects. At its peak, the Gulag accounted for two percent of all laborers in the Soviet Union (Gregory and Lazarev, 2013). These 12-14 hour daily heavy labor shifts, combined with harsh climate and malnutrition, contributed to a very high mortality rate. In 1937-1938, average life expectancy in the Gulag was between two and five years, despite an average length of sentence of 10-25 years (Brunet, 1993).

Those fortunate enough to survive the Gulag returned to a life of permanent political disenfranchisement and social alienation. Some of these long-term costs also extended to family members, especially if the latter did not originally report the crime. The wives, children and siblings of those convicted as 'traitors of the Motherland' were subject to prosecution and imprisonment under Article 58. Children of the repressed lost voting rights, paid higher taxes, and had difficulty obtaining university education and professional advancement in most industries.

For the disenfranchised, rehabilitation was a long and uncertain process. It involved multiple redemptive steps, including engaging in "socially useful labor" and demonstrating loyalty to the regime. Even then, rehabilitation was neither automatic nor irreversible. Some were disenfranchised and reinstated multiple times, and even those wrongly deprived of rights had to formally appeal. Some forms of collective punishment of kin (e.g. exile of Kulaks' families) concluded in the late 1930s. Other policies, like internment of children in special settlements, continued until 1954.

In some cases, prisoners received amnesty, in the form of a commuted sentence and partial restoration of rights. Amnesty initially extended to only special categories of prisoners, like women with children, and those convicted of more minor offenses. Later, the practice extended to other political prisoners, like those convicted of collaboration with occupying

troops during World War II. By 1960, amnesty commissions rehabilitated over 715,120 victims, many of whom were no longer alive (Dobson, 2009).

After Stalin's death in 1953, the new General Secretary of the Central Committee Nikita Khrushchev condemned his predecessor's "cult of personality," dismantled the Gulag labor camp system, and renamed cities and landmarks bearing Stalin's name. While repression later re-emerged in a more limited form under Leonid Brezhnev, this new wave generally favored milder sentences or exile as punishment for dissent.

The political legacy of Stalin's repression in contemporary Russia is ambiguous. The post-Soviet period witnessed some coming to terms with the terror. In the early 1990s, the KGB opened its archives to the public, and Russia's Supreme Soviet established a Presidential Commission for the Rehabilitation of Victims of Political Repression. Monuments to political prisoners appeared across Russia, with state support.

Since Vladimir Putin's ascent to power in 1999, Russia has seen a gradual restoration of Soviet symbols. In 2001, the Duma voted to restore the Soviet national anthem, with new lyrics, and adopted a modified Soviet banner as the official flag of the armed forces. In 2007 and 2008, a new teachers' manual called Stalin an "effective manager," and an updated school history textbook depicted the Great Terror as a rational economic necessity. In June 2015, the Moscow City Legislature voted to restore a statue of Feliks Dzerzhinsky, founding director of the Soviet secret police, to its former location in Lubyanka Square. Similar initiatives have proliferated at the local level, with regional and municipal officials eager to signal their patriotism through Soviet nostalgia.

Social science research on the political effects of Soviet repression is limited. Several macro-level studies have found that communist rule – though not repression specifically – can have lasting effects on political preferences and behavior (Pacek, Pop-Eleches and Tucker, 2009; Pop-Eleches and Tucker, 2011). A handful of papers have more directly studied the impact of repression on support for communism, finding mixed results (Kapelko,

Markevich and Zhuravskaya, 2010; Levkin, 2014; Adler, 2010). Two recent studies on Ukraine have found reduced support for pro-Russian parties among families and communities exposed to mass deportation (Lupu and Peisakhin, 2016; Rozenas, Schutte and Zhukov, 2017). Despite this preliminary evidence of long-term political backlash, the community-level effect of Stalin-era arrests on political participation remains largely unknown.

Data

To investigate the relationship between Stalin's terror and political participation in contemporary Russia, we draw on several types of data, including declassified archival materials, Soviet administrative documents, and polling station-level statistics on voter turnout. Our data on the local intensity of Soviet repression draw on Memorial (2014)'s 'Victims of Political Terror' archive, the most comprehensive open source of information currently available on victims of Stalin-era repression. The Memorial archive includes over 2.65 million records of individuals arrested and convicted of political crimes by the secret police between 1921-1959.

The original sources of these records – which we enumerate in Online Appendix A.1 – are Russian Interior Ministry documents, declassified and released by federal, ministerial and regional archives, prosecutor's offices, and the Commission for the Rehabilitation of Victims of Political Repression. Other sources include newspapers, regional human-rights NGOs, "Memory Books," and materials from survivors' families.

Although it is the largest existing database of its kind, Memorial's archive accounts for only a fraction of Soviet repression victims.[5] Because these data comprise mainly individual arrests under Article 58, they exclude millions of victims of Soviet famine and mass deportation, individuals killed during and after the Russian Civil War, or during Soviet counterinsur-

[5] Memorial (2014) includes about 70 percent of the 3.8 million convicted under Article 58 – which excludes victims of famine, civil war and other government-caused deaths.

gency operations, and political dissidents from the post-Stalin period. Also under-represented are former Soviet republics other than Russia.[6] Our empirical scope is therefore limited to a particular form of state repression: the persecution of individuals for alleged political dissent.

We found approximate geographic coordinates for 2.3 million records (87 percent), using victims' pre-arrest residential addresses, or birthplaces.[7] For each record, we also recorded basic biographical information (e.g. education, nationality, profession), and arrest details (e.g. charge, sentence).

We combined these data with polling-station level voting results from Russia's Central Election Commission, including vote shares and turnout statistics for the 2003, 2007 and 2011 Duma (parliament) elections, and the 2004, 2008 and 2012 presidential elections.[8] The challenge of matching arrest locations to polling stations is that Russia's contemporary administrative boundaries do not always align with earlier, Soviet ones, and many locations have changed jurisdiction. We addressed this issue in two ways. First, we created synthetic geographic units, based on a uniform 25×25 km^2 grid.[9] Second, we replicated all analyses with units based on contemporary Russian district (*rayon*) borders.[10]

For each geographical unit, we calculated the total number of individuals Soviet authorities arrested and sent to the Gulag (normalized by area of territory). We also tallied the proportion of local eligible voters who cast a ballot in the six national elections, and vote shares received by each candidate and party. The maps in Figure 1 show the geographic distribution of (a) Stalin-era arrests and (b) voter turnout in 2012.

To measure the logistical costs of repression, we use data on the to-

[6] 90 percent of Memorial (2014)'s cases occurred on the territory of present-day Russia, the remainder in Kazakhstan (100,000), Belarus (80,000), Ukraine (40,000), Kyrgyzstan (12,000) and Uzbekistan (8,000).

[7] We geocoded street- and municipal-level addresses using Application Programming Interfaces (APIs) from Google Maps and Yandex.

[8] We geocoded the physical address of each polling station, using the same APIs.

[9] The advantage of grid cells is that unit boundaries are exogenous and time-invariant.

[10] These results, omitted here for space, were substantively the same.

pography of mid-century Soviet railroads, the locations of 618 major railroad junctions, travel distances between them (Military-Topographical Directorate of the General Staff of the Red Army, 1945; Afonina, 1996), and the locations of Gulag camps (Smirnov, 1998). We also collected local data on other factors affecting voting, including pre-repression population, urbanization and ethnicity (from the 1926 Soviet Census and other sources), terrain, and contemporary economic performance. Table 1 reports summary statistics for these variables.

Before we proceed, there are several potential sources of error to consider. The first stems from our use of birthplaces for geocoding, where pre-arrest residential addresses were missing. Birth locations are not necessarily same as arrest sites and, given the vast Soviet landscape, distances between them can be great. Although a problem for geocoding accuracy, these more tenuous links between birth and arrest locations should bias our results toward zero – since repression's impact on birth communities should in theory be more indirect.

A second source of error is Memorial's occasionally imprecise and inconsistent recording of place names. While, in general, the archive lists addresses at the village, district, and province levels, in some cases, one or two of these may be missing. In addition, the territorial-administrative division of the Russian Federation has changed from that of its Soviet predecessor, as have the names of many municipalities. To identify and correct systematic geocoding errors, we iteratively drew random subsets of arrests, manually compared geocoded locations to original records, and cross-checked them against a list of name changes from Soviet administrative directories (Presidium of Supreme Soviet of USSR, Information-Statistical Division, 1941/1946/1954).

A third complicating factor is migration. Many decades separate contemporary voting from Stalin-era repression, and the people who now live in these communities are not necessarily descendants of those repressed under Stalin. While there is little we can do to empirically address it, mi-

gration is not as critical a barrier to inference as one may initially assume. Population movements in Russia have historically been heavily regulated, particularly after the institution of internal passports and residency permits (*propiski*) in 1932.[11] To the extent that significant migration did occur, the direction of this bias is likely toward zero. The intuition is straightforward: if the people currently residing in these communities have fewer personal or family connections to those who witnessed Soviet repression first hand, repression's effect on local political participation should be weaker.

We now take a closer look at these potential biases, and develop an empirical strategy to estimate the long-term effect of Stalin's terror.

Empirical strategy

Empirically identifying the effect of repression on voting is challenging. It is possible that Soviet authorities were more likely to repress in areas already mistrustful of the state, and these areas continue to mistrust the Kremlin today. To obtain an unbiased and consistent estimate of the effect of mass terror, we follow past studies (Rozenas, Schutte and Zhukov, 2017) and exploit exogenous variation in repression due to railroads and travel distances to Gulag. As we argue below, Soviet repression depended heavily on railroads, but Stalin-era railroads have little direct influence on contemporary voting, apart from how they shaped the geography of terror.

Railroads and the logistics of repression

Figure 2 shows the structure of the Soviet railroad network in 1945, along with arrest locations (points), rail stations (squares), and Gulag camps (triangles). From these data, we constructed three instrumental variables for

[11] These permits, which local police issued on a limited basis, tied Soviet citizens to "permanent places of residence." Soviet law proscribed individuals from seeking housing, employment and education where they had no such permit, under penalty of a fine and up to two years in prison. The propiska system remains in force in Russia's large cities.

repression: travel distance from each locality to (1) the nearest rail station, (2) the nearest point on the railway line, and (3) the nearest Gulag camp.[12] These instruments have several attractive properties: railroads are strongly predictive of, but causally prior to repression, and there are few pathways, other than repression, by which they can suppress turnout 70 years later.

Repression on an industrial scale is not possible without the means to transport prisoners. The average arrest in our dataset occurred 437 km from the nearest Gulag. Railroads were the primary means by which the NKVD moved people to these camps (Kokurin and Petrov 2000, 525, Conquest 2008, 311, Snyder 2012, 137). By reducing the costs of transporting prisoners over long distances, railroads gave some localities a comparative advantage as providers of forced labor. They also eliminated the need to locally incarcerate dissidents, increasing the number of cases that local organs could process without straining their correctional resources. In rail-accessible localities, the NKVD could repress more people, at lower cost.

The data indeed suggest that rail-accessible localities produced significantly more Gulag inmates than localities of similar size positioned further away. Holding constant urbanization, population size and other potential confounders, a lack of rail access decreased the local supply of Gulag inmates by .7 standard deviations (95% confidence interval: -.87,-.52), on average.[13] In addition to increasing local capacity for arrests, railroads affected the sentences some people received (see Online Appendix B.1). All else equal, the proportion of political arrestees sentenced to the Gulag – as opposed to execution – was .07 standard deviations higher (95% CI: -.12,-.02) in rail-accessible areas. Unless the incidence of capital crimes like treason was genuinely higher in logistically isolated areas, the data sug-

[12] Since most arrest sites and Gulags were not directly adjacent to the railroad, we calculated distances to Gulags as the sum of the Euclidean distance from arrest site to the nearest rail line, the railroad travel distance from there to the point on the network closest to the Gulag, and Euclidean distance from this second point to the Gulag.

[13] This estimate is a standardized regression coefficient, from the first-stage instrumental variable regression of logged repression on railroad proximity (Model 2, Table 2).

gest that, where transporting prisoners was relatively inexpensive, those political prisoners were more likely to serve time in the Gulag.

An underlying assumption behind our first two instrumental variables is that the proximity of railroads influenced the local supply of Gulag prisoners, but the supply of prisoners did not influence railroad access. On a historical level, this assumption is not unreasonable: 77 percent of the Russian railroad system was in service before the Soviets came to power, and transporting political prisoners was never one of its primary purposes.[14] Rail construction continued in Soviet times, but our data suggest that the locations of Gulags – existing, new, or planned – had no discernible impact on network expansion (see Online Appendix B.2).

Another assumption, which motivates our use of distance to Gulags as an instrumental variable, is that prisoners' destination locations influenced their locations of origin, but not the other way around. If Soviet authorities purposefully built the camps in close proximity to population centers, the instrumental variable would not be valid. This scenario, however, is at odds with historical data. Between 1924 and 1953, the Soviets tended to build Gulags mostly in rural areas, separated from the nearest major city by hundreds of kilometers (see Online Appendix B.3). Rather than disperse them across the country, Soviet authorities built camps in clusters – with new facilities typically appearing in close proximity to existing ones, where requisite infrastructure was already in place. Railroads were an essential piece of this infrastructure – all else equal, a standard deviation increase in distance from the railroad reduced the probability of new Gulag construction by 80 percent (95% CI: -97,-64).

A potentially important concern is that railroads have many effects on a country's political economic development, beyond facilitating repression.

[14] Of the 74,325 kilometers of track in place by Stalin's death in 1953, 57,007 (or 77 percent) predated the Russian Revolution of 1917 (Afonina, 1996). Tsarist authorities saw railroad construction primarily as a means to facilitate military mobilization, transport bulk commodities (e.g. grain to markets, coal to factories), and facilitate eastward migration. These priorities remained largely consistent in Soviet times.

The reduced form relationship between our instruments and turnout is relatively weak: pairwise Pearson coefficients range between .06 and .1. Yet if alternative channels exist between Soviet transportation infrastructure and voting today, the exclusion restriction may not be valid. Although it is difficult to imagine that railroads did not affect voting in other ways, there are at least three reasons why – in the current case – these alternative pathways are more likely to attenuate the terror effect than to inflate it.

First, Soviet-era railroads are unlikely to have suppressed post-Soviet turnout by shaping local economic performance. Railroads were arteries of economic development in the USSR, and towns strategically positioned were among the beneficiaries of Soviet industrialization. Empirical studies of post-Soviet elections in Russia have shown economic prosperity to be a reliable predictor of participation and regime support (Colton and Hale, 2009; Treisman, 2011). If rail-accessible areas saw disproportionate decline after the Soviet collapse – when many factory towns lost state subsidies – then turnout there may be low not due to repression, but economics.

Our data suggest the opposite: the local economic benefits of railroads carried over to the post-Soviet period. All else equal, localities with railroad access in the 1940's have continued to see lower unemployment and higher economic performance in 2000-2012, compared to the remote countryside (see Online Appendix B.4). If railroad towns are indeed more prosperous today, the effect of railroads on economic development is unlikely to be the reason for lower turnout there.

Second, railroads are unlikely to have affected elections by facilitating internal migration. The Soviet state heavily regulated migration, but, on the margins, railroads at least made it less costly to leave. It is therefore possible that lower turnout in these areas simply reflects the displacement of more politically-active citizens to less-heavily repressed localities. Such a dynamic is not inconsistent with our expectation of lower participation after repression, but it would nevertheless be problematic for inference.

Migration data from the 1989 Soviet census tell a different story. All

else equal, Soviet-era migration tended to flow from the countryside to the cities, and from localities with railroad access to ones that were more remote (see Online Appendix B.5). Due to these opportunities for population movement, the same logistical convenience that exposed some communities to repression should also have made these communities less likely to remain intact – making long-term community effects harder to detect. Voter turnout in migrant-receiving communities also tends to be lower, suggesting that higher political activism among those who moved away is unlikely to be the reason for lower turnout in repressed areas.

Third, even if there are potential alternative pathways linking railroad access to contemporary politics, there are very few pathways other than repression linking voting to the proximity of Gulags. Gulags were generally not located in densely populated urban areas, and Soviet citizens rarely traveled on these paths, except while heading to or from prison.

Model specification

Our first- and second-stage model specifications, respectively, are

$$\ln(\text{Repression}_i) = Z_i\zeta + X_i\delta + R_i\eta + u_i \tag{1}$$

$$\text{Turnout}_i = \ln(\text{Repression}_i)\theta + X_i\beta + R_i\gamma + \epsilon_i \tag{2}$$

where the second-stage dependent variable, Turnout_i, is the proportion of registered voters in locality i who voted in national elections. The main explanatory variable, $\ln(\text{Repression}_i)$, is the natural log of individuals arrested and resettled from locality i to Gulag camps under Stalin. Because the $25\times25\text{km}^2$ cells are of equal size, this measure automatically normalizes repression levels by geographic area. Our primary quantity of interest is θ, the 2SLS coefficient on repression.

The instrumental variable, Z_i, can be either (1) Euclidean distance from i to the nearest rail station, or (2) to the nearest rail line, and (3) distance

from i to the nearest Gulag camp, including Euclidean distance to and from the railroad, and travel distance on the railroad network itself. We estimate the model separately for each of these measures.

Also on the right hand side are a vector of dummy variables, R_i, that indicate the administrative unit (*oblast*) to which locality i belongs, and represent fixed regional differences in voter turnout.[15] X_i is a vector of local covariates, including ruggedness of terrain,[16] which we expect to increase the logistical costs of repression, and pre-treatment urbanization (1926 census),[17] which we include because targets for repression were more plentiful and turnout is typically lower in cities. In separate models (Online Appendix C.1), we include exposure to World War II-era violence as a post-treatment adjustment, measured as as distance to German-occupied territories, where wartime displacement and postwar repression were high.

More repression, less turnout

Our results confirm that exposure to Stalin-era repression depresses voter turnout. Figure 3 reports standardized instrumental variable estimates of this effect. Table 2 reports parameter estimates and model diagnostics for the 2012 presidential elections.

First stage coefficients for all instruments are negative, suggesting that the scale of repression was decreasing in distance to both railroads and Gulags. The weak instrument test statistic is large and highly significant, suggesting that instruments are strongly correlated with repression. The Wu-Hausman test, which compares our specification to OLS, further suggests that instrumental variable estimates are consistent and OLS are not.

The second-stage estimates of the repression effect on voting are all neg-

[15] These fixed effects also account for unobserved sources of oblast-level variation in Memorial (2014)'s reporting of repression, such as differences in regional archival policies.

[16] We measure rugged terrain as standard deviation of elevation.

[17] We measure urbanization as percent of local population residing in urban areas.

ative and highly significant. In areas where logistics drove variation in state terror, a standard deviation increase in repression produced a .73 standard deviation decline in turnout – a reduction equivalent to 8.5 percent of the local electorate. As Figure 3 shows, this negative effect has appeared in every national election between 2003 and 2012.

While the relationship appears negative, it is possible that some unobserved or latent characteristic shared by neighboring localities is driving our result. Indeed, Figure 1 clearly shows that neighboring localities tend to vote in similar ways. The Moran's I statistics of residual spatial autocorrelation, reported at the bottom of Table 2, are also large and highly significant for Models 1-3, confirming that locations with high voter turnout tend to cluster around each other, and that our models overlook this fact.

To account for spatial dependence, we re-estimated Models 1-3, adding Moran Eigenvectors as synthetic covariates capturing residual autocorrelation (Dray, Legendre and Peres-Neto, 2006) (see Online Appendix C.2). Models 4-6 in Table 2 report the results for 2012 elections, while Online Appendix C.2 also includes a replication of Figure 3, with Moran eigenvectors.[18] As before, across all election cycles, political participation is consistently lower where Stalin-era repression was more intense.

Of course, Stalin's terror is neither the sole nor principal driver of voter turnout in contemporary Russia. In Online Appendix D, we consider several alternative explanations, including urban-rural differences, ethnic differences and economic performance. As we show, the repression effect remains strongly negative after we account for these confounders.

Electoral fraud and inflated turnout

An important caveat to our analysis is that vote tallies from Russia's Central Election Commission are, by any standard, deeply flawed measures

[18] Due to the computational costs of inverting a 29,279×29,279 matrix, we lowered the spatial resolution from 25×25 km^2 to 200×200 km^2. This reduction in statistical power, combined with the eigenvectors, should make it more difficult to detect a significant effect.

of political participation. Electoral fraud is widespread in Russia, and local officials often inflate turnout figures to signal loyalty to the Kremlin. In Chechnya, for instance, authorities reported 99.59 percent turnout in 2012, with 99.82 percent backing Putin. In one precinct (#451), Putin received 1,482 votes from 1,389 registered voters, placing official turnout at 107 percent. The extent of fraud varies at the local level, and this variation may bias our results in one of two ways. If inflated turnout is more common in historically-repressed areas – where authorities may be wary of a less-than-stellar result – then the bias should be toward zero, since Stalin's terror would appear to increase turnout. If, instead, fraud is more common in less-repressed areas, then it may actually inflate the repression effect.

To see how local electoral fraud affects our statistical estimates, we took a closer look at the 2012 presidential elections. For each polling station in 2012, we looked at two electoral forensics measures: the probability of "incremental" and "extreme" fraud, estimated with Mebane (2016)'s finite mixture model.[19] We aggregated these probabilities to the same spatial units we used before (25km grid cells), and ran two sets of analyses.

First, to establish the direction of bias, we looked at the relationship between Stalin-era repression and contemporary electoral fraud. To this end, we fit a simple quasi-binomial model, in which the probability of each type of fraud in 2012 depends on Stalin-era repression, a set of local covariates (e.g. pre-treatment urbanization, geographic terrain), and oblast-level fixed effects. The results, which we report fully in Online Appendix E, suggest that Stalin-era repression had no impact on the probability of incremental fraud, but is negatively correlated with the probability of extreme fraud. This type of fraud is also more common in rural localities, and in areas – like Chechnya – with rough terrain. This result is potentially concerning for our analysis, since the repression-fraud relationship is in the same direction as that between repression and raw turnout.

[19] Extreme fraud indicates near-100 percent turnout and near-total reallocation of votes to winner; incremental fraud indicates substantial reallocation of votes (Mebane, 2016, 2).

In our second set of analyses, we asked whether – given this bias – the effect of repression on turnout disappears if we restrict our sample to only those localities where fraud is highly unlikely. To this end, we replicated Models 1-3 in Table 2 on subsets of Russian localities, in which the probability of fraud did not exceed some threshold $p \in [0, 1]$. We report the results of this sensitivity analysis in Figure 4. While the probability of incremental fraud does not influence our estimates, extreme fraud probabilities do. As we would expect, the negative effect on turnout is smaller in localities where extreme fraud was highly unlikely. However, the negative effect never fully goes away, even as p approaches zero. Although fraud makes it more difficult to identify the effect of Stalin-era repression, the estimate remains negative and statistically significant even after we drop locations where evidence of ballot-box stuffing exists.

Generalizability

What does turnout actually represent in Putin's Russia? Since there is little uncertainty about the outcome of national elections, a decision to stay home may signify not only political disengagement, but latent opposition to Putin. The data seem to support this interpretation. As we report in Online Appendix F, opposition support is higher – and Putin's is lower – in historically repressed areas, across all elections. One may then wonder if the negative effect of repression on turnout is simply an artifact of Russia's domestic political environment – where Putin has consolidated power to the point where participation and regime support are synonymous – or whether it represents a more general pattern. To address this concern, we replicate our analysis with electoral data from Ukraine – a country with a similar legacy of repression, but very different post-Soviet politics.

Evidence from Ukrainian elections, 2004-2014

To show that our findings are not unique to Russia – and that the negative effect on participation still holds where Putin is not the incumbent – we replicated our analysis with electoral data from Ukraine. Under Stalin, Ukrainian NKVD agents reported up the same chain of command as their Russian counterparts, relied on the same railroad network and sent prisoners to the same camps (Rozenas, Schutte and Zhukov, 2017). After independence, the two countries' political paths diverged. As Russian politics took a decidedly authoritarian turn in the 2000's, Ukraine's electoral landscape remained divided, with power alternating between rival 'pro-Western' and 'pro-Russian' coalitions.[20] The relative competitiveness of Ukraine's elections – coupled with its shared Soviet past – offers an attractive opportunity to test the generalizability of our results.

To ensure that results are maximally comparable, we used the same data sources on Soviet-era repression, logistics and demographics, and matched them to polling-station level turnout figures from Ukraine's Central Election Commission. We aggregated these data to the same units of analysis (25km grid cells), and adopted the same identification strategy and model specification as in Eq. 1-2. We then replicated Models 1-3 from Table 2 for every national Ukrainian election between December 2004 and May 2014.

The results, shown in Figure 5, are consistent with what we found in Russia: turnout is consistently lower in Ukrainian localities more heavily-exposed to repression.[21] The overall direction of the relationship, across all election cycles, is negative. Whether the incumbent is a 'pro-Western' president like Viktor Yushchenko (2005-2010) or a 'pro-Russian' president like Viktor Yanukovych (2010-2014), localities where Stalin-era repression

[20] Ukrainian parties with a generally 'pro-Western' foreign and trade policy orientation include Our Ukraine, Yulia Tymoshenko Bloc, Batkivshchina, and Petro Poroshenko Bloc. 'Pro-Russian' parties include Party of Regions, Communist Party, and Opposition Bloc.

[21] Estimates are more uncertain than before for the Gulag distance instrument, which is not surprising, since Ukraine is a smaller territory with less variation on that variable.

was high see lower political participation today.

The similarity of the repression-turnout relationship in Ukraine is striking for several reasons. First, elections in Ukraine – while flawed in many respects (Myagkov, Ordeshook and Shakin, 2009) – are far more competitive than in Russia. Since power regularly changes hands across elections, it is more difficult to interpret political participation as tacit approval of the regime. Second, Ukrainian elections show that the emergence of a Putin-style ex-KGB strongman is not necessary to "activate" the repression effect. In Ukraine, this effect held even during the presidency of Viktor Yushchenko – who sought EU and NATO membership and attempted to rehabilitate veterans of the anti-Soviet Ukrainian Insurgent Army. The consistently lower turnout in Ukrainian elections hints at a more general decline in political participation among historically-repressed communities.

Discussion

The analysis of the preceding sections shows that Stalin-era repression strongly affects political behavior in contemporary Russia. Localities exposed to higher levels of state terror are significantly less likely to vote today, even after accounting for several important contextual factors and econometric concerns. What is less clear is why repression has this effect, and why our negative finding is at odds with recent evidence that violence increases political participation (Blattman, 2009; Bateson, 2012). Although a direct examination of mechanisms lies outside the scope of our article, we briefly consider one such possibility: repression deterred participation by increasing the expected costs of political activity.

The deterrence effect rests on several distinctive features of Soviet repression. First, because the USSR was a strong autocratic regime, which held on to power for 70 years before loosening its repressive grip, a potential resumption of violence became a persistent feature of everyday life. This fact distinguishes the USSR from empirical contexts in which previous scholars

detected a positive violence-participation link – principally non-state actors and security forces in weak or developing states.

Second, by creating incentives for citizens to inform on one another, Stalin's secret police ensured that potential dissidents would be hesitant to reveal their political preferences not only to government officials, but also to neighbors, co-workers and family members.

Third, by extending some punishments for political crimes to family members, the state ensured that these lessons would be passed on to future generations. By applying the principle of "guilt by association" to the rest of the household, even after a prisoner had served his or her term, Soviet authorities ensured that Stalin's terror would have an intergenerational effect on political participation. For other community members not directly victimized, but who potentially witnessed their repressed neighbors' plight, the message was clear: political participation is dangerous; expressing the "wrong" political preferences can ruin your life; if you oppose the regime, it is better to keep quiet.

The Soviet state collapsed in 1991, so why does the deterrence effect persist? One explanation is that, because Russia is becoming politically less free, historically repressed communities extrapolate Soviet-era lessons to the current regime.[22] This possibility, however, does not explain the similar effects we found in Ukraine. Another explanation is that turnout is low due to a more general mistrust in state institutions, many of which have not fully come to terms with their violent past. While laws on the rehabilitation of repression victims exist in both countries, not all classes of political prisoners have been subject to rehabilitation. A posthumous restoration of rights, moreover, does not negate decades of suspicion and fear that some communities experienced.

To investigate the plausibility of the deterrence mechanism, we briefly examine the impact of a much earlier intervention: Soviet-era amnesty. We compare the political consequences of repression to cases where individ-

[22] Freedom House currently classifies Russia as 'Not Free' (score of 6.0).

uals were arrested, convicted, and sentenced, but subsequently released and partially rehabilitated under an order of amnesty. Because amnesty reduced the lifetime costs of repression, a positive link between amnesty and turnout would be consistent with deterrence.

Figure 6 reports the results of regressions of voter turnout on the proportion of local arrestees who either served their term in the Gulag (red) or received amnesty (blue). Consistent with the deterrence story, turnout is higher where a greater proportion of repression victims received amnesty, and lower where amnesty was rare.

Of course, Soviet authorities did not grant amnesty at random: they did so mainly for lesser crimes (i.e. not treason or espionage), and may have only done so in cases where they had reason to expect political loyalty. This selection bias prevents us from interpreting the estimates in Figure 6 as anything other than a correlation. Our preliminary analysis, however, opens the possibility that amnesty of political prisoners not only dampens the negative repression effect, but may even reverse it.

Conclusion

Using data on contemporary voting in Russia and Ukraine, and archival records on Stalin-era repression, we found a robust negative relationship between the number of people sent to Gulag camps and future political participation. To address the potential endogeneity of repression, we exploited exogenous variation in repression due to the accessibility of railroads and the proximity of Gulags. Our results confirm that – where logistics drove repression – the effect of repression on turnout is consistently negative. We also considered a host of alternative explanations for the terror effect, and argued that these additional sources of error either bias our results toward zero, or do not significantly affect our estimates.

That said, our analysis has several important limitations, which we are not able to address. First is the problem of ecological inference. Due to the

technical impossibility of directly matching arrest records to votes, we used a geographic grid to make inferences about community-level behavior. Future efforts should confront information loss from aggregation, and correct potential biases it may cause. Second, data limitations prevent us from examining the intermediate effects of repression on social structures and interactions within affected communities, and how these changes translate into political behavior. Uncovering these and other mechanisms linking repression and voting should be a priority for future research.

Our findings are consistent with anecdotal accounts about the lasting political trauma of state repression. Even where repression is "effective" in the narrow sense of keeping a regime in power, such actions can result in long-term distrust of a country's political institutions. This distrust, our data suggest, has outlived both Stalin and the Soviet Union, and remains a political challenge in contemporary times.

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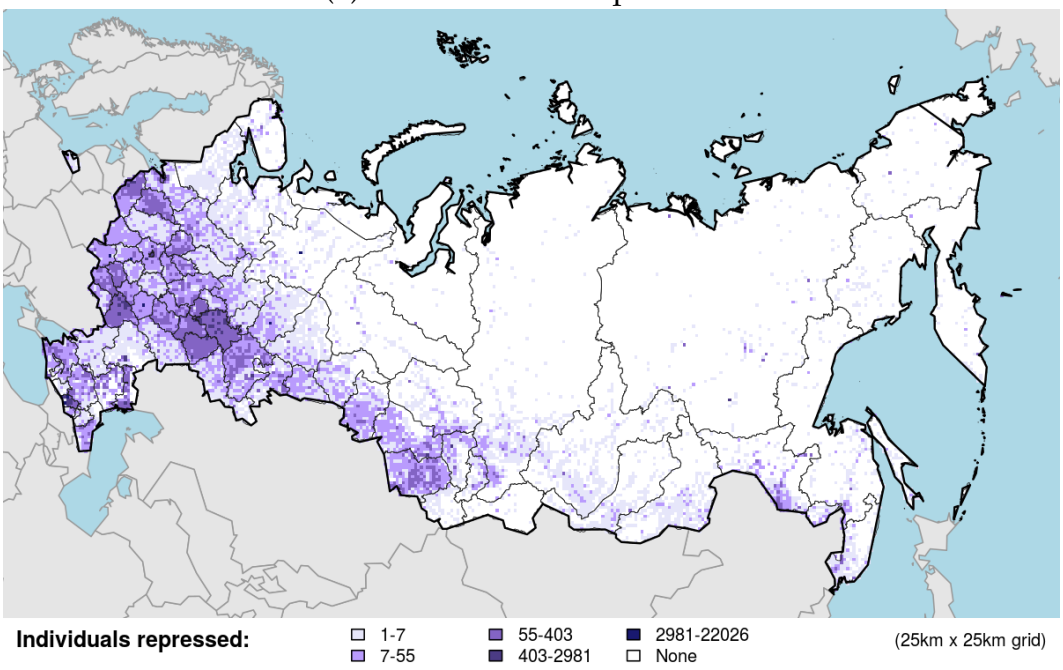
Table 1: SUMMARY STATISTICS (25×25 km^2 grid)

Name	Mean	Median	Std.Dev.	Range	N	Source
Stalin-era repression						
Repression (per cell)	4.00	34.16	235.41	[0, 13630]	8,336	Memorial
Nearest station (km)	436.63	765.08	790.64	[0, 3533.37]	29,279	GSRA
Nearest railroad (km)	432.10	753.52	799.98	[0, 3533.37]	29,279	GSRA
Distance to Gulag (km)	245.95	387.60	836.32	[4.87, 22562.83]	8,336	GSRA & Memorial
Contemporary voting						
Turnout (2003)	58.55	60.73	13.02	[6.67, 100]	7,416	CEC RF
Turnout (2004)	69.34	70.56	13.07	[33.45, 100]	7,418	CEC RF
Turnout (2007)	67.54	69.44	13.48	[31.03, 100]	7,984	CEC RF
Turnout (2008)	75.15	74.95	12.40	[34.8, 100]	7,447	CEC RF
Turnout (2011)	60.78	63.68	14.75	[20.89, 100]	8,054	CEC RF
Turnout (2012)	68.04	69.85	12.59	[30.12, 100]	8,076	CEC RF
Covariates						
Urbanization (1926)	0.25	0.25	0.17	[0, 1]	23,925	USSR1926
Dist. to WWII front (km)	1716.28	1587.49	911.58	[0, 3239.11]	29,279	DPE1985
Std.Dev. elevation (m)	25.40	48.75	67.63	[0, 1212.81]	29,279	ETOPO30
Num. ethnic groups (1964)	1.00	1.28	0.61	[0, 7]	29,279	GREG
Proportion Russian (1964)	0.00	0.38	0.43	[0, 1]	28,382	GREG
Pop. density (2000)	0.36	8.42	78.49	[0.01, 6147.65]	29,191	GPW
Unemployed (2003)	9.45	9.61	2.89	[1.29, 55.75]	25,796	Rosstat
GRP (2003)	114.90	139.53	133.03	[3.8, 960]	29,222	Rosstat
Unemployed (2004)	8.82	8.80	2.92	[1.64, 43.55]	25,796	Rosstat
GRP (2004)	133.10	169.73	168.04	[4.8, 1194.1]	29,222	Rosstat
Unemployed (2007)	7.60	7.21	2.99	[0.83, 52.55]	25,836	Rosstat
GRP (2007)	205.80	322.12	370.94	[8.6, 5145.9]	29,279	Rosstat
Unemployed (2008)	7.35	7.70	2.48	[0.93, 53.34]	25,836	Rosstat
GRP (2008)	246.50	392.17	412.08	[14.8, 6731.2]	28,660	Rosstat
Unemployed (2011)	6.89	7.44	2.34	[1.44, 48.15]	25,836	Rosstat
GRP (2011)	384.70	531.98	500.72	[21.5, 8401.9]	28,660	Rosstat
Unemployed (2012)	6.23	6.56	2.23	[0.81, 47.68]	25,836	Rosstat
GRP (2012)	483.00	635.38	603.81	[26.1, 10021.5]	28,660	Rosstat

Memorial: Memorial (2014); Smirnov (1998); GSRA: Main Military Communications Directorate of the Red Army (1943); Military-Topographical Directorate of the General Staff of the Red Army (1945); CEC RF: Central Election Commission of the Russian Federation (2003-2012); GTOPO30: U.S. Geological Survey (1996); GREG: Weidmann, Rød and Cederman (2010); GPW: CIESIN and Columbia University (2005); USSR1926: 1926 All-Union Census; Rosstat: *Russia in Figures: Statistical Handbook* (2004-2012).

Figure 1: Maps of Soviet repression and political participation.

(a) Scale of Soviet repression.



(b) Voter turnout.

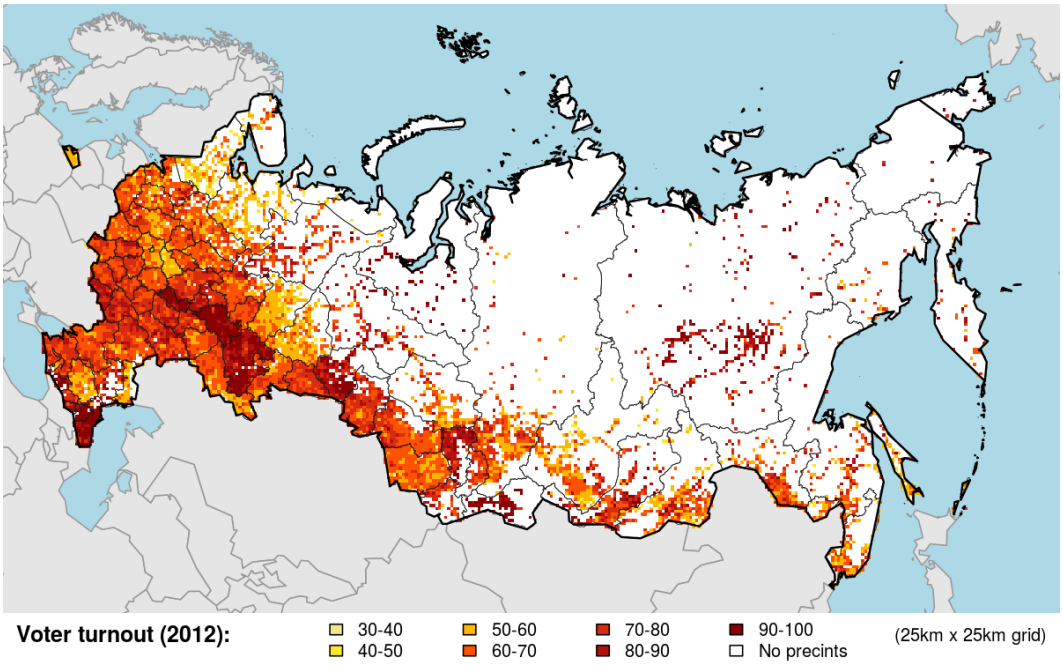


Figure 2: Soviet railroad network and Gulag system.

Table 2: Effect of Soviet repression on voter turnout. Quantities reported are standardized 2SLS coefficients and 95% confidence intervals.

Second stage results	*Dependent variable:*					
	Turnout (2012)					
	(1)	(2)	(3)	(4)	(5)	(6)
log(Repression)	−0.72***	−0.64***	−0.57**	−0.34***	−0.33***	−0.24**
	(−0.99, −0.45)	(−0.91, −0.36)	(−0.97, −0.17)	(−0.50, −0.17)	(−0.50, −0.16)	(−0.41, −0.08)
First stage results	*Dependent variable:*					
	log(Repression)					
Distance to station	−0.73***			−0.65***		
	(−0.91, −0.56)			(−0.84, −0.45)		
Distance to railroad		−0.69***			−0.64***	
		(−0.87, −0.52)			(−0.83, −0.44)	
Distance to Gulag			−0.12***			−0.11*
			(−0.17, −0.08)			(−0.20, −0.01)
Oblast FE	Y	Y	Y	Y	Y	Y
Moran eigenvectors	N	N	N	Y	Y	Y
Observations	7,026	7,026	7,026	391	391	391
R^2	0.24	0.31	0.35	0.70	0.70	0.70
Adjusted R^2	0.23	0.30	0.34	0.62	0.62	0.63
Residual Std. Error	0.82 (df = 6942)	0.79 (df = 6942)	0.76 (df = 6942)	0.59 (df = 312)	0.58 (df = 312)	0.58 (df = 312)
Weak instruments	67.51***	60.79***	26.18***	13.57***	13.06***	9.47***
Wu-Hausman	42.1***	29.15***	9.99**	4.74*	4.14*	0.46
Moran's I (resid)	32.27***	32.78***	33.18***	-3.19	-3.2	-3.25

'p<0.1; *p<0.05; **p<0.01; ***p<0.001

Figure 3: Effect of Soviet repression on voter turnout in Russia. Quantities are standardized coefficient estimates of $\hat{\theta}$ from equation 2. Blue bars are 95% confidence intervals.

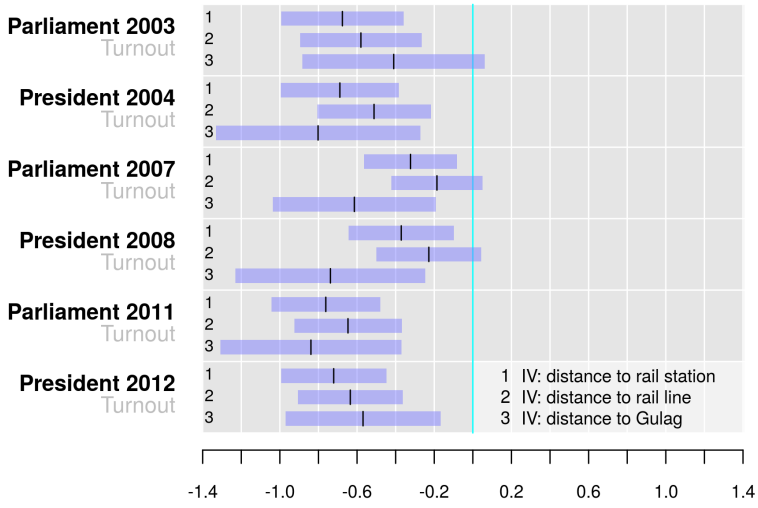


Figure 4: Effect of electoral fraud on the repression effect. Quantities represent standardized coefficient estimates of $\hat{\theta}$ from equation 2, for data subsets in which the probability of electoral fraud is at most p.

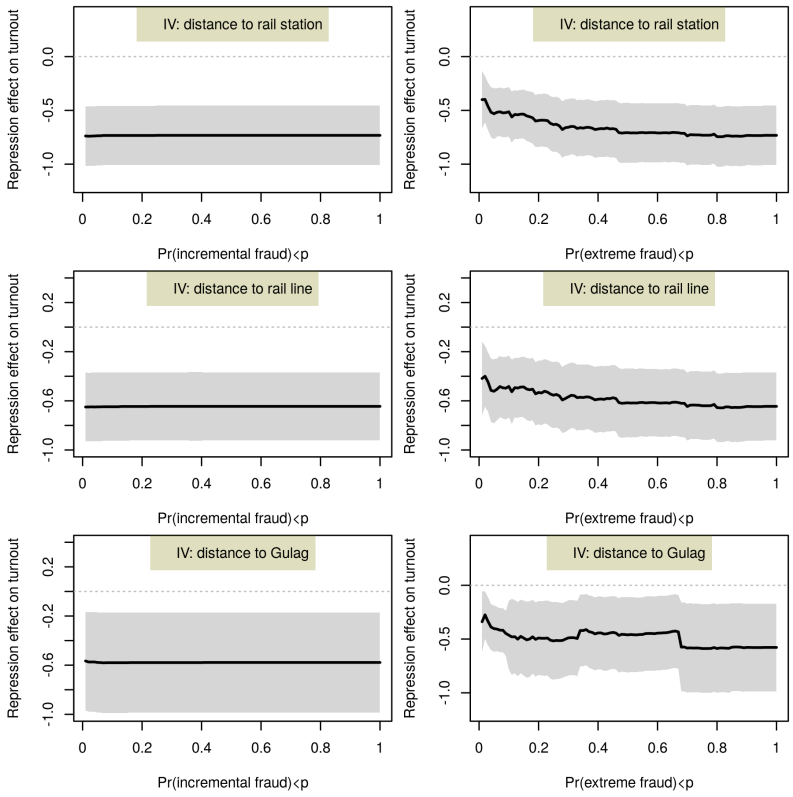


Figure 5: Effect of Soviet repression on voter turnout in Ukraine. Quantities are standardized coefficient estimates of $\hat{\theta}$ from equation 2.

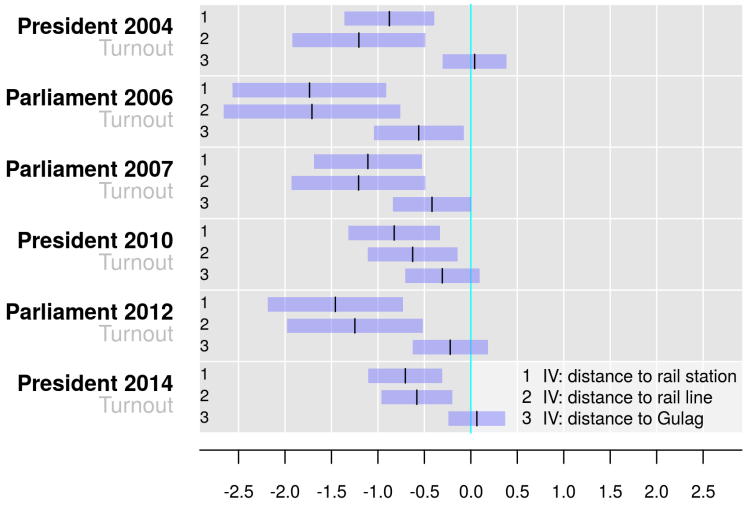


Figure 6: Effect of amnesty on turnout. Standardized coefficient estimates from regressions of voter turnout on the proportion of local arrestees who did (blue) or did not receive amnesty (red).