Fighting for Tyranny:
State Repression and Combat Motivation

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We utilize over 100 million declassified Red Army personnel records from World War II to study how state repression shapes combat motivation. Our empirical strategy exploits plausibly exogenous variation in the scale of Stalin's repression prior to war due to explicit random targeting, logistical costs, and local administrative discretion. Soldiers from places exposed to higher repression were more likely to fight until death and less likely to flee, but also received fewer decorations for individual bravery. Repression appears to have induced obedience at the expense of initiative and increased battlefield casualties.

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1. Introduction

The development and survival of states often hinges on their ability to extract resources for war-making from their populations (Tilly, 1985). In modern mass warfare, one such key resource is the effort that ordinary citizens exert on the battlefield. What drives individuals to risk their lives, resist the temptation to flee, and take personal initiative when fighting for their country? Since the participation and conduct of individuals in war has far-reaching consequences for growth (Collier, 1999), labor markets (Acemoglu, Autor and Lyle, 2004), wages (Angrist, 1990), and inequality (Scheidel, 2018), a deeper understanding of these micro-level processes is essential.

Existing research has examined the effects of pecuniary incentives (Grossman, 1991), personal stakes (Hall, Huff and Kuriwaki, 2019), group loyalties (Shils and Janowitz, 1948; Costa and Kahn, 2003), ideology (Bartov, 1992; Barber IV and Miller, 2019), and fear of punishment (Chen, 2017) on combat motivation. We propose a complementary perspective, which underscores the role of soldiers' prior interactions with the state outside their line of duty. If we accept the premise that military institutions do not evolve in isolation from broader social and political structures, as military theorists have traditionally argued (Clausewitz, 1832/1984, 592-593), then we must also acknowledge the importance of lived experiences vis-a-vis the state that soldiers had prior to their service. Soldiers for whom these experiences were mostly positive may approach their duties differently than those who have come to see the state for which they are fighting as unjust or tyrannical.

The military effort of the Soviet Union during World War II, which we study in this paper, is arguably the most paradigmatic case for the question at hand. Within the span of a few years, the Soviet state headed by Joseph Stalin went from inflicting unprecedented mass terror against its people to having to rally those same people to fight in its name, in what became the world's deadliest-ever conflict. The Great Patriotic War — the eastern front of World War II — accounted for 93% of all European casualties and 18 of the 25 costliest battles on record (Overy, 1998, xvi). The Soviet Union lost over 11.2 million military personnel and 17.9 million civilians (Surinov and Oksenoyt, 2015). Almost 40% of the battlefield losses comprised soldiers who were captured, surrendered, deserted, or went missing (Krivosheev, 1997). Historians have puzzled over these numbers and debated whether Stalin's prewar coercion alienated the population to the point where many did not want to defend their homeland (Edele, 2017; Thurston, 2000). Incentives to avoid fighting were indeed compelling. In the battle of Stalingrad, average life expectancy was 24 hours for enlisted Soviet soldiers and three days for officers (Merridale, 2006). Given these odds, it is remarkable that the Red Army managed to keep millions of its troops in line fighting while others fled (Reese, 2011).

We conduct a quantitative study of how prewar mass violence by the state impacts the combat motivation of soldiers during war. We employ new detailed data on the Red Army in World War II, compiled from over 100 million declassified personnel records, to reconstruct the wartime trajectories of over 12 million soldiers. We also use micro-level data from over 2 million secret police case files on mass arrests prior to the war. By linking these records, we evaluate whether exposure to prewar repression could in part explain why some soldiers fought to death while others surrendered, deserted or went missing, and why some received decorations for valor and initiative in battle and others did not.

Our empirical approach exploits several features of Stalin's repression. The mass terror was locally arbitrary in that it targeted people not on the basis of individual behavior, but on the basis of high-order group-level characteristics like ethnicity, class, or geographic region. This permits us to estimate the effect of repression by comparing geographically proximate locations adjusting for the observables the regime used to select victims. In addition, we exploit variation in repression induced by access through railways, which was the key logistical constraint in reaching and transporting repression victims to camps. Finally, motivated by the fact that local administrators had enormous discretion in the implementation of repression, we exploit discontinuous changes in arrest levels across administrative borders. Sensitivity analyses and falsification tests suggest that the implied assumptions behind these empirical strategies are plausible.

Across all three empirical approaches, we find consistent evidence that Red Army soldiers from places with high levels of prewar repression had systematically different battlefield outcomes compared to others. First, they were more likely to be killed or wounded in action. Second, they were less likely to flee the battlefield, go missing, or defy orders for which they would face punishment. Third, these soldiers showed fewer acts of bravery as far as we can judge from their award records. It appears that repression made soldiers more compliant, but also less willing to take initiative.

We conduct additional tests to rule out the possibility that our results reflect wartime discrimination against soldiers from heavily-repressed areas, their selective assignment to more dangerous parts of the front or to units engaged in deadlier tasks. The same systematic differences emerge when we compare soldiers serving concurrently in the same units, who were exposed to similar battlefield conditions, leadership, and group cohesion while deployed. We also show that repression affects soldiers' behavior through both direct personal exposure and the exposure of one's peers from the same unit.

We rationalize these empirical results using a stylized model of soldiers' decision-making process, which focuses on trade-offs between intrinsic and extrinsic motivations (Bernheim, 1994; Benabou and Tirole, 2003). Individuals will perform a task either because they are intrinsically motivated to do so, or because the right extrinsic incentives (i.e. punishments, rewards) are in place. Two individuals may assign different intrinsic value to the same task, and may respond differently to the same performance incentives — depending, in part, on their past experiences. The interplay of these different motivational structures can explain how individuals behave and interact in battle.

In line with this reasoning, we argue that repression induces "perfunctory" rather than "consummate" compliance (Brehm and Gates, 1999, 17). People who learn first hand about the repressive nature of their state will come to expect that they — or even their families, as in the Soviet case — may be punished for the slightest hint of disloyalty. These individuals are more likely to comply with formal orders and collective norms, even at their own peril, but they do so not to consummate victory, but to avoid punishment.

The link between repression and war-making is by no means unique to the Soviet case. Similar tensions between pre-war state violence and wartime combat mobilization have surfaced in the Iran-Iraq War (Pollack, 2004, 182), the Second Congo War (Lyall, 2020, 332), and the civil war in Syria (Heydemann, 2013), among others. Yet several properties make the Soviet case especially suitable for empirical analysis. Highly bureaucratized Soviet military administration generated enormous amounts of granular data, permitting quantitative study of the largest-ever military effort at the level of individual soldiers. Furthermore, due to a nearly universal draft of the adult male population, we can avoid problems of self-selection into the military. Finally, the Soviet case allows us to partial out two factors central to earlier literature on combat motivation: unit cohesion and pecuniary incentives. Personnel turnover was too fast — due to conscription and combat losses — to secure the types of inter-personal bonds documented in other armies (Merridale, 2006); the Red Army offered no material inducements for combat performance.[1]

Most directly, this article contributes to economics and political science literature on the micro-foundations of military effort (Hirshleifer, 1989; Skaperdas, 1996; Costa and Kahn, 2003; Berman, Shapiro and Felter, 2011). Chen (2017) shows that randomly imposed death penalties in the British army during World War I largely failed to deter desertions. Our focus here is not on coercive incentives during war, but on repression prior to war. Our results suggest that soldiers who had experienced this violence more intimately may be more responsive to coercive measures imposed by the state on the battlefield. This paper also relates to research on intrinsic motivations to fight for material or ideological reasons (Barber IV and Miller, 2019; Grossman, 1991; Hall, Huff and Kuri-

[1]Soviet veterans received temporary benefits to assist with reintegration into civilian society during the mass demobilization of 1945 (e.g. easier access to higher education). Other veterans' benefits (e.g. interest-free loans for housing construction, travel discounts) came into effect three decades later (Edele, 2006).

waki, 2019). Our findings suggest that past experiences of state repression can undermine intrinsic motivations, resulting in lower exerted effort by highly-motivated types.

More broadly, this article extends research on how states' exploitative, coercive, and violent practices impact later economic and human development (Dell, 2010), social structure (Acemoglu, Hassan and Robinson, 2011), trust (Nunn and Wantchekon, 2011; Grosjean, 2014), voting behavior (Rozenas and Zhukov, 2019), and identity (Blaydes, 2018). Our paper also adds to the economic history literature on World War II and the Soviet Union (Harrison, 2000; Gregory and Harrison, 2005), using micro-level archival and administrative data to better understand the behavioral and institutional consequences of Stalin's rule. We show how political repression can shape states' ability to provide the most basic public good — national security. This underscores a previously overlooked negative externality of repression: whether or not it helps rulers remain in office, repression may impede the state's ability to defend itself effectively from external threats.

The rest of the paper proceeds as follows. We start with a stylized model of combat motivation to show how the shadow of state repression prior to war might incentivize conformity on the battlefield. We then present the data and outline our empirical strategies to identify the causal effects of repression on soldiers' battlefield fortunes. We then present our empirical estimates, and discuss their robustness and limitations. In the final section, we discuss a range of possible alternative interpretations of our findings and present auxiliary empirical tests to evaluate their plausibility.

2. Repression and Combat Motivation — Theoretical Expectations

To generate structured predictions of how prior experiences of repression may affect battlefield behavior, we use a stylized model of soldiers' choice. The model explores the conditions under which individuals with heterogeneous preferences will conform to a homogeneous standard of behavior (Bernheim, 1994; Kreps, 1997), like obeying orders from commanding officers. We assume that individuals fight for reasons both intrinsic (e.g. honor, patriotism, duty) and extrinsic (e.g. fear of punishment). Repression can affect individual choices through two channels — alienation (reducing intrinsic motivations) and deterrence (increasing extrinsic ones). We show how these countervailing forces increase obedience among lower-motivated soldiers, while reducing the willingness of higher-motivated soldiers to take initiative beyond the stipulated mandate.

Suppose that each soldier must choose an action $a \in \mathbb{R}$, which measures one's observed level of battlefield resolve — defined as a willingness to continue fighting despite temptations to back down. More resolve implies a higher risk of death or injury. Let $\overline{a} \in \mathbb{R}$ denote the action ordered by commanders (e.g., "charge!"). The soldier could obey the order by choosing $a = \overline{a}$, or he could shirk by, for example, hiding in the trenches ($a < \overline{a}$). For concreteness, let $\underline{a}$ denote a cutoff such that if $a < \underline{a}$, the soldier shows especially low resolve by surrendering or deserting. Alternatively, the soldier could take initiative by going beyond what one's orders require ($a > \overline{a}$), such as personally capturing an enemy officer or continuing to carry out one's mission after being wounded.

Existing research suggests that exposure to state violence reduces trust in state institutions (Nunn and Wantchekon, 2011; Grosjean, 2014) and, more generally, incites negative sentiments towards the perpetrator of violence (Dell and Querubin, 2018). In our setting, this means that individuals who experienced state violence more intimately — personally, or indirectly through their family or community — should be intrinsically less motivated to fight for the state. This is the "alienation effect" of repression.

Formally, let $\omega \in \mathbb{R}$ represent a soldier's baseline intrinsic motivation. In the population of soldiers, ω is drawn from a distribution F, which we assume to be continuous with full support and increasing everywhere. Prior experiences of repression reduce the soldier's baseline intrinsic motivation to $\omega - \alpha r$, where $r \geq 0$ denotes repression and $\alpha > 0$ is the alienating effect. Action a results in an intrinsic loss of $(a - (\omega - \alpha r))^2$. In the absence of other considerations, the soldier would choose $a = \omega - \alpha r$.

Like any other organization, the military relies on a range of extrinsic incentives. A soldier who shirks by retreating or hiding when ordered to charge can be demoted, tried by a military tribunal (Chen, 2017), or shot by a blocking detachment (Statiev, 2010). In the Soviet case, desertion or even surrender may further result in the punishment of the soldier's family members. To the extent that repression creates "internalized expectations about [how] authority will respond punitively to challenging acts" (Beissinger, 2002, 326), a soldier exposed to state repression in the civilian domain will see the state's threat of punishment on the battlefield as more credible. This is the "deterrent effect" of repression.

To capture the deterrent effect, suppose that a soldier who shows *less* resolve than required by commanders suffers an extrinsic loss equal to $\mathbb{E}(\pi|r)(\overline{a}-a)^2$, where $\mathbb{E}(\pi|r)$ is the penalty that a soldier expects to receive by showing less resolve than asked.We assume that $\mathbb{E}(\pi|r) = \delta r$, where $\delta \geq 0$ is the "deterrence" parameter: a soldier who experienced more repression will infer that the incumbent's willingness and capacity to punish is higher and will be more reluctant to shirk ($\underline{a} < a < \overline{a}$), let alone defect ($a < \underline{a}$).[2]

We are interested in how the alienating and deterrent effects of repression concurrently influence the distribution of battlefield resolve in a population of soldiers. Each soldier chooses an optimal action that minimizes the sum of his intrinsic and extrinsic losses:

$$a^* \in \arg\min_{a\in\mathbb{R}} (a-(\omega-\alpha r))^2 + \delta r(a-\overline{a})^2 \mathbb{1}\{a<\overline{a}\}, \tag{1}$$

which solves to

$$a^*(\omega,r) = \begin{cases} \dfrac{\omega + r(\delta\overline{a}-\alpha)}{1+\delta r} & \text{if } \omega \leq \overline{a}+\alpha r; \\ \omega - \alpha r & \text{otherwise.} \end{cases} \tag{2}$$

The optimal action a^* is increasing in repression r for soldiers with low baseline motivation, $\omega \leq \overline{a} - \alpha/\delta$, and it is decreasing for soldiers with high baseline motivation $\omega > \overline{a} - \alpha/\delta$. The deterrent effect of repression dominates its alienating effect for soldiers

[2] The soldier's inference about the state's willingness and capacity to punish deviations from commanders' orders could be separately micro-founded using a signaling logic.

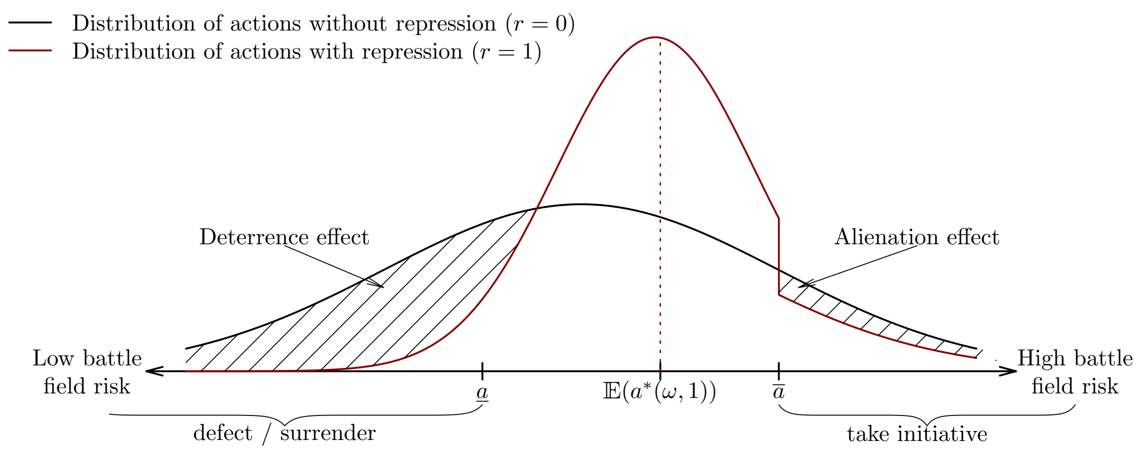


Figure 1: REPRESSION AND CONFORMITY ON THE BATTLEFIELD

with low intrinsic motivation (pushing a^* up), whereas for soldiers with high intrinsic motivation, the alienating effect dominates the deterrent one (pulling a^* down).

The following proposition stipulates that, as long as commanders order soldiers to show a sufficiently high level of resolve $\overline{a}$, increasing repression leads to more uniform combat effort (see Appendix A1 for the proof).

Proposition 1. *For each $\alpha > 0$ and $\delta > 0$, there is $\tilde{a}(\alpha, \delta)$ such that, if $\overline{a} \geq \tilde{a}(\alpha, \delta)$, then $\mathbb{E}(a^*(\omega, r))$ is increasing everywhere in r whereas $\Pr(a^*(\omega, r) < \underline{a})$ and $\Pr(a^*(\omega, r) > \overline{a})$ are decreasing everywhere in r.*

Figure 1 illustrates the mechanics behind the proposition. First, the average level of resolve that soldiers display on the battlefield, $\mathbb{E}(a^*(\omega, r))$, shifts to the right as a result of repression. To the extent that higher resolve induces more physical danger, repression should lead to higher average fatalities and injuries. Second, in addition to shifting the mean of the distribution of soldiers' resolve, repression also reduces its variance. Soldiers with low baseline motivation exhibit higher resolve, resulting in fewer cases of $a^* < \underline{a}$ (deterrent effect), while soldiers with high initial motivation exhibit lower effort than they would otherwise, with fewer cases of $a^* > \overline{a}$ (alienation effect).[3]

[3]While based on different micro-foundations, the latter result is similar to the well-known finding that extrinsic incentives can undermine intrinsic motivation (Kreps, 1997; Benabou and Tirole, 2003).

In sum, repression produces conformity: it compels soldiers to obey their orders and continue fighting even at a higher risk of death, but it saps the intrinsic motivation to take initiative above and beyond the formal mandate. Insofar as documented battle-field outcomes capture the relevant dimensions of combat motivation (which we show they do; see Section 4), we can draw the following testable predictions from this argument: as soldiers' exposure to repression increases, they become (1) more likely to be killed or wounded in action, (2) less likely to surrender, defect or go missing, but also (3) less likely to receive a medal for sacrifices beyond the call of duty.

3. The Context – Soviet Repression and War Effort

The Soviet legal code established a broad class of "counterrevolutionary" crimes, including treason, insurrection, espionage, contacts with foreign states, propaganda, agitation, and a failure to report any of the above. Between 1927 and 1953, around 3.8 million people were convicted of such crimes, most of them in the mid-late 1930's (GARF, 1954).

State repression sought to eliminate "anti-Soviet elements," but the regime had few means to identify those who engaged in "counterrevolutionary" activities or held "anti-Soviet" views. Stalin ordered security officials to target broadly-defined segments of the population, like residents of particular provinces, minorities and peasants, without discriminating between individuals inside those demographic categories. Stalin directed his subordinates to cast a wide net: "because it is not easy to recognize the enemy, the goal is achieved even if only five percent of those killed are truly enemies" (Gregory, 2009, 196). Eventually, people from all demographic and professional groups, including members of the Communist Party, the military, and security agencies, "everybody from the Politburo member down to the street cleaner," became victims of collective targeting (Ulam, 1973).

Central authorities provided little concrete guidance as to who should be repressed. Moscow issued numerical quotas of persons to be executed or sent to camps in each region and "everything else depended on the ingenuity of Security operations personnel"

(Solzhenitsyn, 1973, 69-70). Local executives often engaged in "exceptional competition" to exceed their quotas and signal administrative or ideological zeal (Chukhin, 1999, 76). The hard constraints on this competition were largely circumstantial: the need to cover transportation costs for those condemned to the camps, and to find "a place [to] bury the corpses" for the rest (Jansen and Petrov, 2002, 86, 88).

The blanket targeting of a wide cross-section of societal groups and arbitrary victimization of individuals within those groups created a perception that repression was largely random. Asked about how authorities decide whom to incarcerate or release, one NKVD officer explained, "Chance. People are always trying to explain things by fixed laws. When you've looked behind the scenes as I have you know that blind chance rules a man's life in this country of ours" (Conquest, 2008, 434).

After the German invasion on June 22, 1941, the Soviet Union drafted all military-age males – over 30 million civilians throughout the war – to support its 4.5 million-strong standing Red Army. Thus, the backbone of the Soviet defense against the Germans were ordinary citizens. The war became an "acid test" for Stalinism (Thurston, 2000): would the people risk their lives for a regime that only recently had terrorized them?

In some respects, Stalinism passed the test: millions of soldiers fought for the Soviet state, very often to death. Early in the war, the Red Army stumbled spectacularly due to prewar officer purges, politicized decision-making, and chronic mismanagement (Glantz, 1998). Ultimately, the Soviet Union won the war, and it did so largely by managing to keep its troops fighting *despite* organizational malaise and devastating human costs (Reese, 2011). At the same time, many soldiers voted against Stalinism with their feet. Half of all personnel losses in the first year of the war comprised soldiers missing in action or captured. Thousands were detained for desertion, espionage, sabotage, or treason. While there were many reasons to flee the battlefield, widespread distaste for how the Soviet state treated its citizens clearly did not help (Edele, 2017).

Moscow took draconian measures to hold its troops in line. On August 16, 1941, Stalin issued Order 270, stipulating that those "who surrender to the enemy shall be considered malicious deserters, whose families could be arrested" (Zolotarev, 1997, 58-60). Commanders were to prepare bi-weekly reports for the General Staff, listing captured soldiers and their families' addresses (Kachuk, 2013). Among the first victims of this order was Stalin's own son, Yakov, whose wife was sent to a labor camp after his capture by the Germans.

Stalin issued another disciplinary measure, Order 227, on July 28, 1942. It required every front to organize "penal units" staffed by men accused of disciplinary problems, and send them to the most dangerous sectors to "atone for their crimes against the Motherland with their blood" (Statiev, 2010, 726). The order also mandated the creation of "blocking units" under NKVD command, authorized to detain or execute retreating personnel.

Due to these coercive measures, soldiers who fought instead of fleeing might have done so because they were intrinsically committed to the cause or, alternatively, because they feared that they or their families would be punished. Soldiers who witnessed state repression prior to war may have been especially sensitive to these incentives. Yet they may also have been less motivated to defend the regime in the first place. To assess how these countervailing pressures affected battlefield choices, we must consider not only whether soldiers fought, but also *how* they fought.

4. Data and Measures

We draw on data from military personnel records, NKVD arrest records, and contextual data from geo-referenced historical atlases and other sources.

4.1. Military records

Our source of information on battlefield outcomes is the Russian Ministry of Defense's "People's Memory" (*Pamyat' Naroda*) database, which contains over 106 million declassified Red Army personnel records. The database includes 21 million records on irrecoverable losses and discharges, 23 million records from military transit points, 10 million registration cards, 1.3 million POW records, 5 million burial and exhumation records, 27 million decoration records, and 425,000 combat logs and staff documents. Aside from basic biographical information, these data record combat unit details (recruiting station, enlistment date, unit, rank), decorations, and the reason and date of soldier's discharge.

Due to illegible handwriting, errors in optical character recognition, abbreviations, misspellings, incomplete or missing fields and other errors that are inevitable in archival data, these records required significant preprocessing. This included, among other things, homogenizing names of military ranks and thousands of military units, assigning tactical units to parent divisions, corps and armies, and standardizing geographic references.

The most challenging aspect of data preprocessing was record classification. The same soldier can appear in multiple databases and there are no fields to match soldiers across them. This is an unsupervised classification problem, where each of the 106 million records must be assigned to one of an unknown number of soldiers. We approached this problem with a probabilistic record linkage method (Enamorado, Fifield and Imai, 2019), which we tailored to be operable with our data. Appendix A2 details our record classification procedure and the evidence validating its output.

Since we measure soldiers' exposure to repression through birth locations (see below), we excluded soldiers whose birthplaces were missing or could not be geocoded to the municipality level or lower. We also excluded soldiers born outside the territory of Soviet Russia (RSFSR), since some of these areas were not part of the Soviet Union prior to the war. Our final dataset contains 26,922,385 records for 11,680,930 soldiers.

4.2. Measures of Combat Motivation

We construct several proxy measures of combat motivation based on how soldiers were discharged from the army and the decorations they received. Before the war ended, 46% of soldiers were discharged because they were killed or wounded in action (K/WIA), missing in action (MIA), became prisoner of war (POW), deserted, defected or committed treason (DDT), or were punished for misconduct (PUN). 25% had received at least one medal, including 17% who received decorations specifically for personal valor in combat.

We use K/WIA as a measure of soldiers' resolve to fight (a^* in the language of the model). The idea here is that soldiers who followed orders and fought instead of fleeing faced a mechanically greater risk of death or injury. In combat, performing one's duties means engaging in actions that can kill or physically harm others. The cumulative probability of receiving a severe traumatic injury — due to hostile action or accident — rises as one remains on the battlefield for longer periods of time.

The second outcome of interest is whether a soldier displayed low combat motivation by fleeing ($a^* < \underline{a}$). Soldiers who defected, deserted, or committed treason (DDT) fall into this category, as do those who were punished (PUN) for insubordination. Although less clear-cut, another indicator of flight is whether a soldier became a POW. Not all POWs had made an individual choice to surrender; some did so on the orders of commanders. In the Soviet system, however, orders to surrender were illegal and soldiers were instructed to disobey them, even if they lacked the physical means to resist detainment. Stalin's Order 270, which equated surrender with treason, stipulated that "every soldier is obliged ... to demand that their superiors, if part of their unit is surrounded, fight to the end."

To avoid being held personally responsible, Red Army officers were reluctant to report unaccounted-for soldiers as DDTs or POWs. The common wartime practice was to report them as MIA, as an official from Russia's Ministry of Defense recently acknowledged:

> By official reports, out of our five million-plus missing in action just 100 thousand were reported as prisoners of war. In reality, there were 4.5 million. So the majority of those missing in action were prisoners of war. Everyone knew this. I'm certain that even Stalin knew.[4]

Accordingly, we treat MIA as another indicator of flight. This is a noisy measure, since some cases of MIA must have been KIA. However, this kind of measurement error is

[4]https://www.newsru.com/russia/04feb2011/stalin.html

more likely to attenuate than inflate our estimates, and is unlikely to be numerically large — the quoted estimates suggest that $\Pr(\text{POW}|\text{MIA}) = 4.5\text{M}/5\text{M} = 0.9$.[5]

Finally, to capture cases of initiative above and beyond one's formal duty ($a^* > \underline{a}$), we consider whether a soldier received at least one decoration for acts of valor in combat. These could include — in order of prestige — the medals For Courage, For Battle Merit, Order of Glory, and Hero of the Soviet Union. In contrast to other Soviet medals, which were awarded en masse, these decorations recognized individual performance in situations involving a risk to life, and had to be justified with detailed descriptions of individual acts.[6] Appendix A3 provides examples.

To assess how well these measures map onto the theoretical concept of combat motivation, we examine their correlation with the Red Army's effectiveness across battles. Using official descriptions of 225 major battles from the Russian MOD's "People's Memory" database, we classified each military operation as resulting in a territorial gain, loss or no change for Soviet forces. For each unit participating in a battle, we calculated monthly proportions of soldiers who were K/WIA, DDT, PUN, POW, MIA, or received one of the four valor decorations (Medal).[7] Using these unit-month level data, we estimated a regression equation where the dependent variable is a dummy equal to one if the battle resulted in territorial gain and the covariates are unit-month proportions of K/WIA, DDT, PUN, POW, MIA, and Medal. The regression also includes fixed effects for units, years, and months, to partial out unit-level factors as well as temporal and seasonal trends.

The results in Table 1 suggest that the aggregate success of army units correlated positively with higher casualty rates and higher rates of medals, and negatively with all measures of flight — MIA, POW, DDT, and PUN (although the latter coefficient is not significant). To the extent that combat motivation contributes to higher operational effec-

[5] According to Krivosheev (1997)'s more conservative numbers, of 4.6M designated MIAs, 0.5M were "true" MIA's, 1M returned to the front, and 3.1M were POWs, implying $\Pr(\text{POW}|\text{MIA}) = 0.67$.

[6] We also exclude career service awards and hybrid medals like the Order of the Patriotic War, which was awarded on both an individual basis (e.g. for a high number of enemy kills) as well as collectively to units, towns, factories and entire categories of veterans.

[7] Appendix A5 explains the procedure we used to classify battles and match them to army units.

	Dependent variable: territorial gain
Proportion KIA	$0.05\ (0.02)^{*}$
Proportion MIA	$-0.16\ (0.04)^{**}$
Proportion POW	$-0.25\ (0.08)^{**}$
Proportion DDT	$-0.50\ (0.16)^{**}$
Proportion PUN	$-0.06\ (0.06)$
Proportion Medal	$0.05\ (0.02)^{*}$

OLS coefficients and standard errors (in parentheses) clustered by unit and battle. Unit of analysis is military unit by month (N = 46,240). Fixed effects for units, years, and months are included. Significance levels: $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.1$

Table 1: SOLDIERS' BATTLEFIELD OUTCOMES AND ARMY UNIT PERFORMANCE

tiveness, Table 1 suggests that our proxy measures do capture the relevant latent quantity.

Figure 2 shows the geographic distribution of soldiers' birthplaces, with lighter colors representing higher proportions of soldiers who fought (K/WIA), and darker colors representing higher proportions that fled (MIA/POW/DDT/PUN). There is no macro-level spatial trend in the combat motivation as far as we can discern from these observables. Soldiers born near the western border — where much of the fighting took place — were no more likely to fight or flee than those born in Siberia. There is, however, substantial *local* variation in relative rates of fight and flight, which requires micro-level explanation.

4.3. Data on repression

Our data on repression come from the "Victims of Political Terror" archive, maintained by the Russian human rights organization Memorial (Memorial, 2014). This archive contains individual records for those arrested under Article 58 ("counterrevolutionary activity") of the Soviet penal code. It draws on declassified Ministry of Interior documents from federal, ministerial and regional archives, prosecutor's offices, and the Commission for the Rehabilitation of Victims of Political Repression, with supplementary information from newspapers, regional NGOs, "Memory Books," and survivors' families.

These data are limited in scope to individual prosecutions for alleged political dissent.

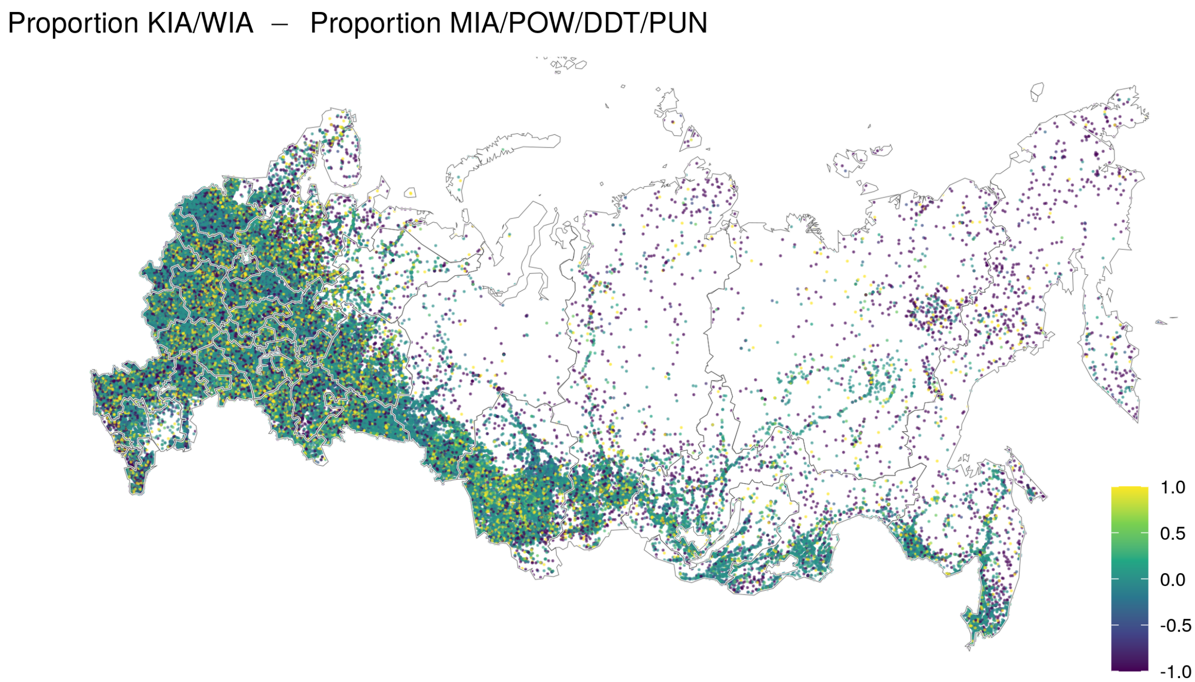


Figure 2: BATTLEFIELD OUTCOMES OF RED ARMY SOLDIERS, BY BIRTH LOCATION

This includes 70% of the 3.8 million convictions under Article 58, but excludes millions of victims of famines, deportations, and counterinsurgency operations. Using the same approach as with military data, we found geographic coordinates for 2.15 million pre-WWII arrests (74%), using victims' residential addresses (where available) or birthplaces.

We measure exposure to repression by counting the number of arrests in the vicinity of a soldier's birth location. Specifically, the variable we use in the analysis is:

$$\text{Repression} = \log(1 + \text{Arrests within 10 km of birth location}),$$

where the logarithmic transformation is used to reduce skewness.[8] Our geographic measure of repression rests on the idea that people are more aware of the repression in their home communities than in more distant locations. With the exception of elite show trials, political repression against ordinary citizens was not publicized, and people learned

[8]Analyses with alternative bandwidths (1-20km) do not produce major differences (Appendix A7.5).

about the actions of the state mostly through family, neighbors, friends, or co-workers.

One concern in using birth locations is that some soldiers may have moved away before repression occurred. We can take stock of this issue by examining the distribution of travel distances between birth locations and the 1,869 military commissariats where soldiers were drafted. The median soldier was born 88km from his draft location; 20% were born within 1km, and 80% within 335km. If most soldiers remained in their areas of birth long enough to be drafted, they were likely also around for the terror of the 1930s.

We use absolute rather than per capita numbers of arrests because this is how narratives about state violence are typically framed and memorialized. 60 arrests (our sample median) from a town of 1,000 are unlikely to be perceived as two times more "repressive" than from a town of 2,000. Indeed, Soviet state security records, historical and autobiographical narratives measure the scale of repression exclusively in absolute numbers.

At the same time, we do need to ensure that our measures of repression are not conflated with local population density. The Soviet censuses of 1926, 1937, 1939 do not provide information on population below the district level.[9] However, we adjust for several proxies of local population density (distance to administrative center, road density, collective farms). In addition, we implement a matched cluster sampling design that selects pairs of locations that are as similar to each other as possible on observables, including the number of soldiers drafted as a proxy for local population size (Appendix A7.1).

Figure 3 shows the geographic distribution of Soviet repression. Although not as widespread as the distribution of soldiers' birth locations, arrests affected every region of the country. Many arrests were concentrated around the main railroad network (black lines), although remote northern regions also did not fully escape the NKVD's reach.

[9] District-level geographic precision allows us to estimate population counts for small areas (e.g. grid cells, see footnote 12), but not point estimates for specific birth locations.

Dots: arrests before WWII. Lines: railroads in 1937. Polygons: oblasts in 1937.

Figure 3: GEOGRAPHY OF PRE-WWII SOVIET REPRESSION

4.4. Additional data

We collected additional data on local political economy, logistics and ethnicity. We measure state capacity using the distance in 1935 from each birthplace to the nearest district administrative center, where local NKVD branches were based (TsIK, 1935). We geo-referenced maps of economic activity from the 1937 *Large Soviet Atlas of the World* (Gorkin et al., 1937, 155), which provided us with information on several important variables. To distinguish between urban and rural areas, we calculated hectares of cropland within 10km of each birthplace. To account for the targeting of peasants due to collectivization, we counted the number of state farms within 10km of each birthplace in 1937 (Gorkin et al., 1937, 161). We also augmented these maps with information from Afonina (1996) to build a geo-referenced dataset of railway junctions, stations, and tracks, which we use to construct an instrument for repression (see details in next section).

With few exceptions, the military records do not include information on soldiers' ethnicity. This is problematic, because ethnic minorities were more likely to face repression. To address this issue, we build a nationality classifier for soldiers' surnames. Using the Memorial archive, which contains nationality information for 916,675 arrestees with 163,284 unique surnames, we trained a Support Vector Machine (SVM) classifier to identify whether a surname represents Russian nationality. The algorithm achieved 96.5% out-of-sample predictive accuracy. We then assigned to each military personnel record a dummy variable equal to one if the surname is predicted to be of Russian nationality.[10]

5. Empirical Approach

We employ three empirical strategies: small area fixed effects, instrumental variables and regression discontinuity.

5.1. Ordinary Least Squares with Grid Cell Fixed Effects

Our first empirical strategy is motivated by the observation that, at the local level, Stalin's terror was notoriously arbitrary in its selection of targets. Since the regime used group-level targeting, we can treat exposure to repression as plausibly exogenous given the observables that the regime itself used in selecting victims. One such observable was ethnicity: Soviet authorities often viewed national minorities as politically disloyal and subjected them to greater coercion. Another was socio-economic: the regime saw *kulaks* ("rich" peasants) as an obstacle to collectivization, but defined "kulak" so loosely that most rural residents faced a heightened risk of coercion. A third was geographic: regions in the western borderlands, the Far East, and areas with a history of peasant uprisings against collectivization faced higher arrest quotas (Getty and Naumov, 1999).

Let y_i denote a battlefield outcome for soldier i and let $\text{Repression}_{j[i]}$ denote repression

[10]For surnames that do not appear in the training data, we assigned the predicted nationality of the surname that is closest in Jarro-Winkler string distance. We compared oblast-level proportions of Russians against census data from 1939. Wilcoxian rank-sum tests suggest that our SVM-classified oblast-level proportions were drawn from the same distribution as oblast-level census proportions (Appendix A4).

around the birth location j of soldier i. We fit the following OLS regression:

$$y_i = \gamma \cdot \text{Repression}_{j[i]} + \boldsymbol{\beta}'\boldsymbol{X}_{ij} + s\big(\text{lon}_{j[i]}, \text{lat}_{j[i]}\big) + \text{Cell}_{k[i]} + \epsilon_i. \tag{3}$$

The vector $\boldsymbol{X}_{ij}$ contains individual-level covariates (ethnicity and year of birth) as well as location-level covariates, including hectares of cropland and the number of state farms within 10 km of soldier's birth location (to account for higher repression of peasants), distances to the nearest administrative district center and nearest road junction as proxies for local state capacity and population density. The term $s(lon, lat)$ represents a two-dimensional spatial spline, which we include to capture local geographic trends.

To account for higher targeting of certain administrative regions (*oblasts*), it would suffice to include fixed regional effects. But even within-regional comparisons would involve locations that potentially differ on unobserved background characteristics. To ensure more balanced comparisons, we partition 1937 Soviet Russia's administrative regions into a regular 25×25km grid, and include a fixed effect for the grid cell k in which soldier i was born. Because geographically proximate locations tend to share background characteristics like population density, ethnic and socio-economic composition, these small area fixed effects should balance the unmeasured confounders. They also ensure that our inferences are drawn by comparing birthplaces no more than $\sqrt{25^2 + 25^2} \approx 35$km apart.[11]

The underlying assumption behind this design is that exposure to repression is *locally* exogenous. We evaluate this assumption by testing whether the geographic distribution of arrest locations within grid cells was spatially random. Within each 25×25km cell, we tested the null hypothesis that arrest locations are the realization of a uniform Poisson point process. We were unable to reject this hypothesis in 87-96% of cells, depending on the test procedure (Appendix A6.1). Although arrest density varies between cells and regions, the *local* spatial distribution of repression appears quite arbitrary.

[11]The median (maximum) distance between two birth locations in a grid cell is 8.9 km (16.9 km).

5.2. *Railway Access as Instrument*

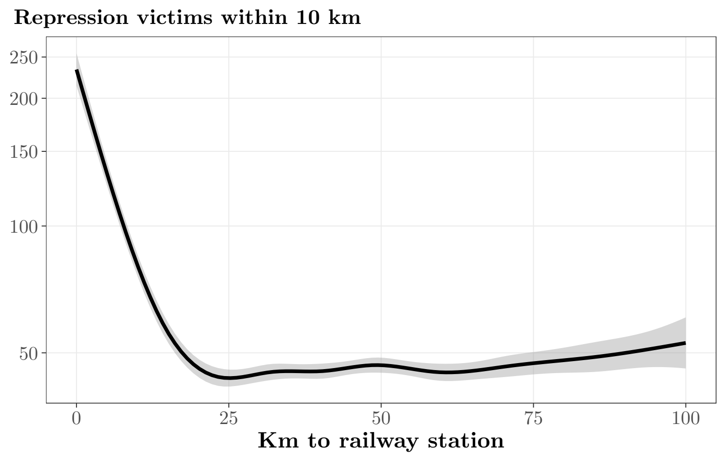
Even if repression is exogenous on a small geographic scale, OLS estimates may suffer from attenuation bias due to errors in the measurement of repression through archival sources. To correct for such bias, we use two-stage least squares (2SLS). This approach exploits the fact that Stalin's repression was an industrial-level operation. Arrestees had to be transported to execution sites, prisons, and distant labor camps in large numbers and on short deadlines. The primary means for implementing these operations were railways (Kokurin and Petrov 2000, 525). A third of the budget assigned to the Great Terror campaign was earmarked for rail transport fees (Getty and Naumov, 1999, 478).

Motivated by these facts, we use access to railways, measured as the distance from a birth location to the nearest railway station, as an instrument for repression. The idea here is that otherwise similar locations may be exposed to varying levels of repression due to differing costs of accessing and transporting arrestees. One concern with this instrument is that may be capturing economic development and population density. All our 2SLS estimations include distance to the nearest administrative center and nearest road junction, which approximate local development and density more directly than railways. Indeed, the Soviet railway system was built not to help foster local economic development or connect population centers, but to help access resource-rich areas (Hopper, 1930).

To test whether birthplaces with better railway access saw more repression, all else equal, we fit the following semi-parametric regression:

$$\text{Repression}_j = f(\text{Raildist}_j) + \boldsymbol{\beta}'\boldsymbol{X}_j + \text{Cell}_{k[j]} + s(\text{lon}_j, \text{lat}_j) + \epsilon_j, \tag{4}$$

where j indexes birth locations, $Raildist_j$ is distance from location j to the nearest railway station, and f is a smooth function approximated by cubic regression splines. As before, we add grid cell fixed effects, location level covariates, and a spatial spline. To ensure greater homogeneity, the 2SLS analyses use only locations within 100 km of rail stations.



The estimated function $\hat{f}$ with 95% confidence bounds relating railway access to repression, adjusted for geographic covariates and grid cell fixed effects. Vertical axis is on logarithmic scale.

Figure 4: RAILWAY ACCESS AND REPRESSION

Figure 4 shows a graph of the estimated function $\hat{f}$. The expected number of repression victims declines precipitously with distance to rail stations, even after accounting for local density of roads, distance to major cities, and other covariates. A 10km higher proximity to a railway station increases the number of victims by about a factor of two.

Note that we estimate function f at the level of birth location, not individual soldier, because this is the level at which the relationship between railway access and repression operates. Specification (4) helps us find an optimal transformation f of $Raildist_j$ that yields the strongest *linear* first stage relationship. In the 2SLS regression specified at the level of a soldier, we use the variable $\hat{f}(\text{Raildist}_{j[i]})$ as the instrument. The first stage model is

$$\text{Repression}_{j[i]} = \alpha \cdot \hat{f}\left(\text{Raildist}_{j[i]}\right) + \boldsymbol{\beta}'\boldsymbol{X}_{ij} + \text{Cell}_{k[j]} + s(\text{lon}_j, \text{lat}_j) + \epsilon_i, \tag{5}$$

where $\boldsymbol{X}_{ij}$ includes both location-level and soldier-level covariates. In the second stage, we regress wartime individual outcomes on the predicted values of repression from (5).

The exclusion restriction behind our 2SLS strategy is that railway access impacted the future behavior of soldiers only through repression, and not some other channel outside the included covariates. One reason to doubt this assumption is that railways played a key role in the Soviet military effort: the front stretched thousands of kilometers from the Baltic to the Caspian Sea and motorized vehicles could only support operations up to 300-400 km (Davie, 2017). However, only a small fraction of RSFSR's railroad network fell inside areas of active military operations or behind German lines: 3.7% in an average month, and 16% cumulatively at any point in the war. The railway structure also changed significantly in 1941-1945, as Soviet authorities built 6,700 km of new railroad lines (Zickel, 1989, 552), which are not part of the instrument.

Another potential violation of the exclusion restriction has to do with railroads' use in military mobilization. While almost all military-age males were drafted, it is possible that someone living near railways faced different battlefield conditions by virtue of being drafted in the chaotic early months of the war, when incentives to flee and the odds of being killed were highest. However, the proximity of one's birthplace to railroads does not relate systematically to the timing of conscription (see Appendix A6.2). Furthermore, we also show that our results hold even if we compare soldiers who served concurrently in the same military unit, and who therefore faced similar battlefield conditions.

5.3. Geographic discontinuities

Our last empirical strategy utilizes the fact that regional state security and party officials practiced enormous discretion when implementing central orders. A town located in a region with a highly zealous NKVD chief could face significantly more repression than a nearby town from a different region with less ambitious or less cruel security officials. These idiosyncratic qualities of local officials cannot be measured directly, but we can infer which Soviet administrative regions (*oblasti*) had lower or higher than expected numbers of victims. We first identify administrative regions where the number of victims fell below or above what one would expect conditional on local population size and urbanization levels using the following linear regression:

$$\text{Repression}_{rk} = \alpha + \beta_1 \cdot \ln\big(\text{Population}_{rk}\big) + \beta_2 \cdot \text{Urbanization}_{rk} + \epsilon_{rk}, \tag{6}$$

where r indexes region and k indexes grid cells.[12] After estimating the above regression, we calculate the average residual $\bar{\epsilon}_r$ for each region r. When $\bar{\epsilon}_r$ is positive (negative), repression in region r is above (below) what its background characteristics would predict.

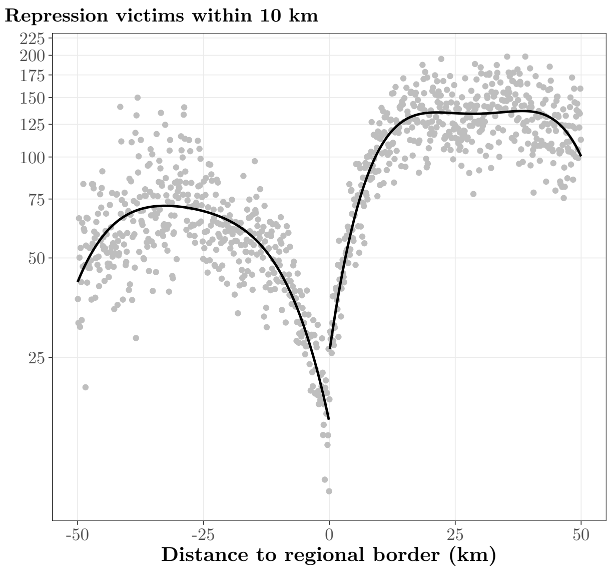
We then select pairs of adjacent regions (r, r') such that $\text{sign}(\bar{\epsilon}_r) \neq \text{sign}(\bar{\epsilon}_{r'})$, that is, where one region has higher than expected and another has lower than expected levels of repression. Let d_{jr} denote the distance from birth location j in region r to the border of the nearest region. The forcing variable δ_{jr} is constructed as follows:

$$\delta_{jr} = \begin{cases} d_{jr} & \bar{\epsilon}_r > 0 \text{ and } \bar{\epsilon}_{r'} < 0, \\ -d_{jr} & \bar{\epsilon}_r < 0 \text{ and } \bar{\epsilon}_{r'} > 0. \end{cases}$$

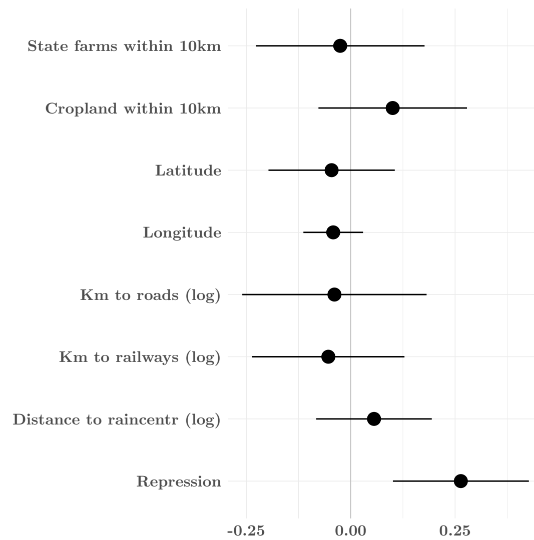
For example, if $\delta_{jr} = -2$, then birthplace j is inside a low-repression region two kilometers away from a high-repression region. Had the administrative border between regions r and r' curved slightly differently to include birthplace j in r' instead of r, the level of repression in j would have been higher, in expectation. This is a plausible counterfactual: Soviet regions underwent a series of territorial reforms, in which authorities subdivided large regions into smaller, more "manageable" units (*razkrupnenie*). The first phase of these reforms concluded in 1936, just prior to the Great Purge (Shiryaev, 2011).

Figure 5a plots the relationship between forcing variable δ_{jr} and predicted levels of repression. To preclude comparisons of wildly different locations, we restrict the analysis to birthplaces within ±50 km of regional borders. The figure shows a clear discontinuous

[12]Data on local population size and urbanization are from the 1926 Soviet Census. To disaggregate these district-level data to smaller grid cells, we used dasymetric mapping, a spatial interpolation technique that employs ancillary data to obtain filtered area-weighted local estimates (Mennis, 2003). We used historical land cover maps (Gorkin et al., 1937) to exclude uninhabitable areas (water, deserts, glaciers) and to distinguish between built-up and rural areas.



(a) Local means with local polynomial fit.



(b) Discontinuity effects with 95% CI's (normalized outcomes).

Figure 5: DISCONTINUITY OF REPRESSION AT REGIONAL BORDERS

jump across regional borders. The bias-corrected local-polynomial estimate of the discontinuity effect (Calonico, Cattaneo and Titiunik, 2015) is 0.52 (S.E. clustered by grid cells is 0.14) on the logarithmic scale, or about 10 victims on the natural scale.

To check if observables other than repression also change discontinuously across borders, we conduct balance tests. Figure 5b displays point estimates and 95% confidence intervals of discontinuity effects for eight covariates, normalized to have a standard deviation of one for comparability. Only repression shows a discontinuous jump, suggesting that border discontinuities are a plausibly exogenous source of variation in repression.

We exploit the discontinuities in repression across administrative borders using a fuzzy regression discontinuity design (FRDD):

$$\text{Repression}_{j[i]} = \alpha \cdot \mathbb{1}\{\delta_{jr[i]} > 0\} + g_1\big(\delta_{jr[i]}\big) + \boldsymbol{\beta}'\boldsymbol{X}_{ij} + s(\text{lon}_j, \text{lat}_j) + \epsilon_{1i}, \tag{7}$$

$$y_i = \gamma \cdot \text{Repression}_{j[i]} + g_2\big(\delta_{jr[i]}\big) + \boldsymbol{\beta}'\boldsymbol{X}_{ij} + s(\text{lon}_j, \text{lat}_j) + \epsilon_{2i},$$

where g_1 and g_2 are smooth functions of forcing variable δ_{jr} estimated using regression splines, and indicator $\mathbb{1}\{\delta_{jr} > 0\}$ is the instrument. Both stages include covariates and spatial splines, but exclude grid cell fixed effects because, by construction (cells are nested within regions), the instrument cannot vary within cells.

5.4. Clustering and Weights

The outcome variables in our study are measured the level of individuals, but we observe exposure to repression at the level of birth locations. Due to the potential correlation of errors across individuals from the same location (cluster), the effective sample size is bound to be smaller than the number of individual soldiers in the data. To account for this correlation of errors, we cluster standard errors by birth location, which is the level at which the treatment varies. We also cluster standard errors by grid cells to account for spatial autocorrelation. Finally, to incorporate the uncertainty inherent in our procedure of classifying military records, we weigh soldiers by the geometric mean of pairwise matching propensities of records assigned to them (see Appendix A2).

6. RESULTS

We first analyze how repression changed soldiers' resolve to fight, as measured by their propensity to be killed or wounded. Table 2 reports coefficient estimates from OLS with fixed effects, 2SLS with a railway instrument, and FRDD with a spatial instrument.

Estimates from all three designs suggest that soldiers from areas with more repression were more likely to die in battle. In the OLS specification, increasing repression in the soldier's birthplace from zero to 32 people (first quartile in sample) meant a $[\ln(32+1) - \ln(0+1)] \times 0.4 \approx 3.5 \times 0.4 = 1.4$ percentage point higher chance of death or injury. In the 2SLS and FRDD specifications, the changes are 11.6 and 3.5 percentage points.

Next, we analyze how repression shaped soldiers' proclivity to flee the battlefield. Table 3 shows coefficients for a range of observables that capture this outcome. In the first column, the outcome is an index *Flee*, indicating whether a soldier was reported as either

	OLS	2SLS	FRDD
Coefficient	0.4 (0.1)**	3.3 (0.6)**	1 (0.3)**
Mean Y	21.2	20.5	19
First Stage $\mathcal{F}$		146.6	25.3
Gridcells	12,143	5,654	1,582
Birthplaces	182,225	147,803	44,154
Soldiers	11,427,895	9,725,085	2,316,908

Outcome = killed or wounded in action (KIA/WIA), measured on percentage scale (0 to 100). Standard errors in parentheses, clustered by birth location and grid cell. All models include grid cell fixed effects, individual and birth location-level covariates. Observations weighted by record clustering probability. 2SLS analyses exclude birth locations > 100km from railroad. FRDD analyses exclude locations in non-matched regions and > 50km from regional borders. Significance levels (two-tailed): $^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.

Table 2: REPRESSION AND RESOLVE TO FIGHT

missing, surrendered, deserted, defected, committed treason, or punished for misconduct. Outcomes in the remaining columns are the index's four constitutive measures.

Coefficients in the first column are consistently negative and statistically significant at 95% confidence across all designs. Substantively, increasing repression from zero to the first quartile (32) reduced a soldier's chances of flight by $3.5 \times -0.2 = -0.7$ to $3.5 \times -2.3 = -8.1$ percentage points, depending on the specification. Coefficients for MIA, the most frequent indicator of flight, are also consistently negative and significant.

Other indicators offer more mixed results, from negative in the case of PUN to negative, null and even positive for POW and DDT. Some of this variability may reflect the idiosyncrasies of Red Army reporting. Most POW's, as we noted, were officially misreported as MIA's and cases of DDT were very rare, which raises the possibility that these outcomes emerged under qualitatively different circumstances or reflect underlying heterogeneities in repression's effect. We also find that positive coefficients on POW and DDT do not survive robustness tests, whereas coefficients for MIA and PUN remain negative and mostly significant (Appendix A7.4).

Table 4 reports the estimated effects of repression on battlefield initiative, measured through a soldier's receiving at least one decoration for acts of valor. The estimated coef-

	Flee	MIA	POW	DDT	PUN
Model	OLS				
Coefficient	-0.2 (0.1)**	-0.2 (0.04)**	-0.03 (0.04)	0.003 (0.002)	-0.01 (0.005)**
Mean Y	24	17.4	5.8	0.2	0.8
Model	2SLS (First-stage $\mathcal{F} = 146.6$)				
Coefficient	-2.3 (0.4)**	-2 (0.3)**	-0.3 (0.2)	0.03 (0.01)**	-0.1 (0.02)**
Mean Y	24.4	17.6	5.9	0.2	0.8
Model	FRDD (First-stage $\mathcal{F} = 25.3$)				
Coefficient	-0.6 (0.2)*	-1 (0.2)**	0.5 (0.1)**	0.02 (0.01)*	-0.03 (0.02)$^{\prime}$
Mean Y	23.9	17.6	5.3	0.2	0.9

Outcomes on percentage scale (0 to 100): missing in action (MIA), becoming prisoner of war (POW), defecting, deserting, committing treason (DDT), being punished for battlefield misconduct (PUN), or any of the above (Flee). See the note under Table 2 for the number of soldiers, birthplaces, grid cells, and other details. Significance levels (two-tailed): $^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.

Table 3: Repression and Flight from the Battlefield

ficients are negative and significant in all models. Using the most conservative estimate (OLS), a soldier from a place with 32 repression victims was $3.5 \times -0.2 = -0.7$ percentage points less likely to receive a medal than a soldier from a place with no repression.

One potential concern with these results is that 2SLS estimates and, in some cases, FRDD estimates are substantively larger than the OLS estimates. The differences could be due to sample selection, since we restricted 2SLS analyses to locations within 100 km of railroads and FRDD to $\pm$ 50 km of regional borders. However, sensitivity analyses (Appendix A7.6) show that OLS coefficients are nearly identical when we restrict the sample to locations near railroads and borders.

The relatively large magnitude of the 2SLS results may also indicate possible violations of the exclusion restriction. In Appendix A7.7, we conduct sensitivity analyses to assess how large the violations of the exclusion restriction must be to invalidate the above results (Conley, Hansen and Rossi, 2012). We find that, for example, to overturn the positive 2SLS effect of repression on KIA/WIA, moving one's birth location 38 kilometers closer to a railway station (i.e. from the median distance to zero) would need to increase

	OLS	2SLS	FRDD
Coefficient	-0.2 (0.1)**	-2.1 (0.4)**	-1.2 (0.2)**
Mean Y	17.9	18	18.1
First Stage $\mathcal{F}$		146.6	25.3

Outcome = receiving at least one valor decoration (*For Battle Merit*, *For Courage*, *Order of Glory*, *Hero of Soviet Union*), measured on percentage scale (0 to 100). See the note under Table 2 for the number of soldiers, birthplaces, grid cells, and other details. Significance levels (two-tailed): $^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.

Table 4: REPRESSION AND BATTLEFIELD INITIATIVE

the risk of K/WIA by at least 3% through a channel other than repression. The reported estimates appear to be robust to non-trivial violations of the exclusion restriction.

Two additional explanations of the differences between OLS and two-stage estimates are possible. OLS may suffer from attenuation bias due to errors in the measurement of repression. Since instruments should alleviate this attenuation bias, the larger magnitude of 2SLS and FRDD point estimates makes sense. Furthermore, the two-stage estimates represent the local effect of repression induced by railway access and proximity to regional borders. It is possible that repression induced by exogenous factors appeared more arbitrary, and as such, induced a stronger effect on combat behavior.

We conducted a battery of additional robustness tests. Instead of analyzing individual outcomes, we averaged them at the level of birthplace and then ran the same regressions on these aggregated outcomes. Our conclusions remain identical (see Appendix A7.1). We also considered the possibility that our results are biased due to incompletely observed records. We observe discharge records for 46% of soldiers, and our analyses assumed that soldiers without such records continued their service until the end of the war. In Appendix A7.2, we replicate our earlier analyses while excluding individuals whose discharge reasons are not observed. Again, our conclusions remain robust. Our results also hold when we consider a more selective subset of medals (Appendix A7.3).

Finally, a well known problem with clustered treatment designs is bias due to unequal cluster size. In our case, because higher-population areas may, mechanically, see higher absolute numbers of arrests, the treatment level is correlated with cluster size. To evaluate these biases, we adopt a matched cluster sample design (Imai et al., 2009), sampling pairs of birthplaces that are similar on observable pre-treatment covariates, are from the same grid cell and in the same quintile of cluster size. The procedure yields a matched sample of 43,118 clusters (23.5% of total), or 21,559 matched pairs. We ran our analyses on the matched sample, and found substantively consistent results (Appendix A7.1).

7. Interpretation

We now consider several potential interpretations of our findings, including conformity, as well as discrimination, selective assignment, battlefield conditions, and peer effects.

7.1. Conformity

Empirical patterns align closely with the logic of conformity in Proposition 1. Consistent with the comparative static that expected battlefield resolve $\mathbb{E}(a^*(\omega, r))$ is increasing in repression r, we find that soldiers exposed to higher levels of prewar repression were more likely to fight until death or injury. These soldiers were also less likely to shirk their duties to the point of fleeing the battlefield, consistent with the deterrent effect, where $\Pr(a^*(\omega, r) < \underline{a})$ is decreasing in r. Finally, the conformity logic holds that, due to the alienation effect — $\Pr(a^*(\omega, r) > \overline{a})$ is decreasing in r — repression should reduce incentives to exceed one's orders. Evidence that soldiers from places with more repression were less likely to receive valor decorations is consistent with this prediction.

The observation that prewar terror conditioned soldiers to signal greater resolve resonates with qualitative evidence from historical accounts. Merridale (2006, 46-47) tells the story of a soldier, Ilya, whose estranged father the NKVD arrested in 1937. After the arrest, Ilya changed his educational and professional plans "for fear of unwelcome inquiries." Yet in 1941, he volunteered to serve at the front, ultimately sustaining life-threatening injuries at Stalingrad. Those who survived or witnessed the terror were "bound together by shared awe, shared faith and shared dread... It was far easier, as even the doubters found, to join the collective and share the dream than to remain alone, condemned to isolation and the fear of death" (Merridale, 2006, 45-46). The same incentives that increased compliance also encouraged overly cautious decision-making. As Overy (1998, 32) writes, "the result [of Stalin's terror] was the triumph of military illiteracy over military science, of political conformity over military initiative."

While the prediction that repression induces conformity finds support in the data, there are alternative interpretations for some of these results, which we must evaluate.

7.2. Discrimination

We interpreted the negative relationship between repression and medals as indicating a lack of initiative. Another possibility, however, is that soldiers from heavily-repressed areas faced systematic discrimination. Unit commanders may have been more hesitant to recommend — and higher authorities less likely to approve — decorations for soldiers from "problematic" spots of the country. Archival evidence offers no indication that commanding officers were aware of aggregate levels of repression at subordinates' birth locations, which is necessary for the discrimination argument to explain our results.[13] Moreover, Table 3 shows that soldiers from high-repression locations were *less* likely to face punishment for real or presumed violations of the military code, which is the opposite of what we would expect to see if these soldiers were subject to higher scrutiny.

To assess more systematically this alternative explanation, we check whether a similarly negative relationship exists between repression and promotions. Rank advancement decisions followed a structurally similar bureaucratic process to medals, but were more weakly tied to individual performance in combat. Similar to medals, unit commanders were responsible for recommending individuals for promotion, with conferral authority residing with higher ministerial or party authorities (see Appendix A3 for details).

[13]Officers did have information about the family and "political background" of individual soldiers, but this is clearly not the same as information about the geographic distribution of aggregate repression levels.

Unlike medals — where specific combat actions were the main consideration — criteria for promotion were more varied, and included factors like length of service, the need to quickly fill higher-ranking billets, constraints due to ethnic or religious quotas, soldiers' disciplinary record, party membership and other indicators of political loyalty. As such, there were many more opportunities for discrimination to enter the promotion process than in the simpler conferral of merit-based awards.

If discrimination drove our results on medals, we should see a similar negative effect for promotions. As Table 5 reports, this is not the case. Under our baseline specifications with wartime promotion (i.e. at least one rank advancement) as the outcome, none of the three estimates are negative in sign, and the two estimates that reach statistical significance are positive. Unless rank advancement was insulated from political pressure while the conferral of decorations was not (which seems implausible), discrimination cannot explain these results. It is more likely that the army's promotion system favored conformity over initiative, as past historical studies suggest (Glantz and House, 2015, 10).

	OLS	2SLS	FRDD
Coefficient	0.04 (0.02)*	0.3 (0.1)**	0.04 (0.1)
Mean Y	7.2	7.2	7.3
First Stage $\mathcal{F}$		141.8	25.7
Gridcells	11,056	5,558	1,558
Birthplaces	161,817	132,134	39,528
Soldiers	7,226,470	6,161,836	1,467,297

Outcome = receiving at least one promotion to a higher rank over the course of the war, measured on percentage scale (0 to 100). Standard errors in parentheses, clustered by birth location and grid cell. All models include grid cell fixed effects, individual and birth location-level covariates. Observations weighted by record clustering probability. 2SLS analyses exclude birth locations > 100km from railroad. FRDD analyses exclude locations in non-matched regions and > 50km from regional borders. This analysis further excludes soldiers for whom rank information is unavailable (18% of cases). Significance levels (two-tailed): $^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.

Table 5: REPRESSION AND ADVANCEMENT TO HIGHER RANK

7.3. Selective Assignment

Did soldiers from repressed places die in larger numbers because they were selectively assigned to more dangerous parts of the front? We assess this possibility by checking whether exposure to prewar repression increases the likelihood of two outcomes that are strongly predictive of higher casualty rates. First is assignment to the infantry branch of the army, where direct exposure to enemy fire was higher than in other branches, like artillery and aviation. Second is assignment to so-called "penal units," which were routinely ordered to charge through minefields and machine-gun fire.

	Infantry	Penal
Model	OLS	
Coefficient	-0.02 (0.04)	0.01 (0.002)**
Mean Y	86.1	0.1
Gridcells	10,466	10,466
Birthplaces	150,707	150,707
Soldiers	5,724,988	5,724,988
Model	2SLS (First-stage $\mathcal{F} = 144.2$)	
Coefficient	-0.3 (0.2)	0.03 (0.01)**
Mean Y	85.8	0.1
Gridcells	5,538	5,538
Birthplaces	123,449	123,449
Soldiers	4,788,716	4,788,716
Model	FRDD (First-stage $\mathcal{F} = 26.1$)	
Coefficient	0.1 (0.1)	0.01 (0.01)
Mean Y	85.2	0.1
Gridcells	1,551	1,551
Birthplaces	36,764	36,764
Soldiers	1,083,299	1,083,299

Coefficients represent estimated effect of repression on soldiers' chances of assignment to an infantry unit or penal unit on percentage scale (0 to 100). Robust standard errors in parentheses, clustered by birth location and grid cell. All models include grid cell fixed effects, individual and birth location-level covariates. Because data on branch and unit assignments are available only for 50.3% of draftees, sample size is considerably smaller for these analyses. Significance levels (two-tailed): $^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.

Table 6: REPRESSION AND MILITARY UNIT ASSIGNMENT

The evidence here is mixed. Table 6 reports the estimated effects of repression on assignment to these units. Conscripts from high-repression areas were no more likely to serve in the infantry branch, but there is some evidence that prewar repression correlates with assignment to penal units. A soldier from a location with 32 arrests (first quartile) was $3.5 \times 0.01 = 0.04$ to $3.5 \times 0.03 = 0.1$ percentage points more likely to serve in a penal unit than a soldier from a location with no arrests. Given that just .08% of soldiers were assigned to penal units over the duration of the war, this accounts for only a tiny fraction of the estimated effect of repression on battlefield deaths and injuries.

7.4. Battlefield Conditions

A related concern is that battlefield conditions, rather than prewar experiences, are driving our results. Some soldiers may have faced different incentives by virtue of where and when they saw combat. Soldiers' incentives may have co-evolved with wartime improvements to the Red Army's strategy, tactics, force structure, organization, leadership, training, weaponry and logistical support. They may have also shifted with Soviet efforts to expose German cruelty and to improve morale through nationalist appeals.

To address this possibility, we expand out baseline models to include fixed effects for the unit in which each soldier served, and month of deployment. In the case of OLS:

$$\begin{aligned} y_i = \gamma \cdot \text{Repression}_{j[i]} + \boldsymbol{\beta}' \boldsymbol{X}_{ij} + s\big(\text{lon}_{j[i]}, \text{lat}_{j[i]}\big) + \text{Cell}_{k[i]} \\ + \text{Military unit}_{u[i]} + \text{Month of war}_{t[i]} + \epsilon_i. \end{aligned} \tag{8}$$

where u indexes military units and t indexes months from June 1941 to May 1945. We add the same set of fixed effects to our 2SLS and FRDD specifications. Because 26% of soldiers served in more than one unit during the war, we disaggregated soldiers' records by unit assignment for this analysis. The variable *Military unit* has almost 12,000 unique values, and identifies the smallest-echelon unit mentioned in each record.[14] Note that both unit

[14] Among records with non-missing unit information, we traced 53% to a specific division, 10% to a

assignment and month of service are "post-treatment" measures. If including these post-treatment measures washes away the effects of repression, then it would suggest that unit assignment and the evolution of the war drove these effects.

	KIA/WIA	Flee	Medal
Model		OLS	
Coefficient	0.3 (0.1)**	-0.2 (0.04)**	-0.2 (0.1)**
Mean Y	59.3	22.7	18.8
Gridcells	9,647	9,647	9,647
Birthplaces	128,888	128,888	128,888
Soldiers	4,431,766	4,431,766	4,431,766
Model		2SLS (First-stage $\mathcal{F} = 136.9$)	
Coefficient	2.8 (0.4)**	-1.2 (0.2)**	-2.7 (0.4)**
Mean Y	58	23.3	19.3
Gridcells	5,444	5,444	5,444
Birthplaces	106,016	106,016	106,016
Soldiers	3,716,848	3,716,848	3,716,848
Model		FRDD (First-stage $\mathcal{F} = 33.7$)	
Coefficient	1 (0.2)**	-0.2 (0.1)$^{\prime}$	-0.9 (0.2)**
Mean Y	55.8	24	20.5
Gridcells	1,525	1,525	1,525
Birthplaces	31,326	31,326	31,326
Soldiers	840,783	840,783	840,783

Outcomes on percentage scale (0 to 100). Standard errors in parentheses, clustered by birth location and grid cell. All models include grid cell, unit and month fixed effects, individual and birth location-level covariates. Observations weighted by record linkage probability. Sample includes disaggregated personnel records, with non-missing unit assignment and date information. 2SLS analyses exclude birth locations > 100km from railroad. RDD analyses exclude locations in non-matched regions and > 50km from regional borders. Significance levels (two-tailed): $^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.

Table 7: ESTIMATES ADJUSTING FOR MILITARY UNIT AND MONTH.

The results of these analyses are in Table 7. Coefficient estimates remain similar to baseline specifications, although they slightly differ in magnitude. Depending on the estimator, increasing repression from zero to the first quartile (32 arrests) increased one's chances of death or injury by 1.1 to 9.8 percentage points, decreased flight by 0.7 to 4.2

brigade, 28% to a regiment, 2.4% to a battalion and 7.4% to a company.

points, and chances of receiving a medal by 0.7 to 9.5 points (as before, percentage change $\approx 3.5\hat{\gamma}$). Exposure to prewar repression continues to drive battlefield behavior, even when we compare soldiers serving concurrently within the same units, who thus fought in the same battles, under the same commanders, with the same comrades-in-arms.

7.5. Peer Effects

Soldiers do not make decisions independently, and their choices may reflect not only their own prewar experiences, but also the backgrounds and actions of others in their unit. If a majority of one's comrades flees the battlefield, a soldier may have few opportunities to deviate from this pattern. The scope of individual agency may be similarly limited if almost no one flees. Heroic acts by others may encourage one to take similar action, and so on. Due to this interdependence, repression may affect combat motivation not only through individual exposure, but also, indirectly, through its effect on one's peers.

To analyze peer effects, we follow the econometric approach in Carrell, Sacerdote and West (2013) and augment the regression equation in (8) with two additional terms:

$$
\begin{aligned}
y_{it} =& \gamma \cdot \text{Repression}_{j[i]} + \rho \cdot \overline{\text{y}}_{ut[-i]} + \zeta \cdot \overline{\text{Repression}}_{ut[-i]} + \boldsymbol{\beta}' \boldsymbol{X}_{ij} + s\big(\text{lon}_{j[i]}, \text{lat}_{j[i]}\big) \\
&+ \text{Cell}_{k[i]} + \text{Military unit}_{ut[i]} + \text{Month of war}_t + \epsilon_{it}
\end{aligned}
\tag{9}
$$

where $\overline{y}_{ut[-i]}$ and $\overline{Repression}_{ut[-i]}$ are, respectively, the average outcome and the average level of repression for the peers of soldier i in unit u during month t (calculated excluding soldier i). In the terminology of Manski (1993), ρ is the endogeneous peer effect and ζ is the exogenous peer effect. Since state repression was a taboo topic and long-term bonds between soldiers could not crystallize due to high turnover within units, it is unlikely that soldiers could form correct expectations about the repression experienced by their peers. Given this lack of an empirically plausible mechanism, we assume that exogenous peer effects did not play an important role in combat motivation ($\zeta = 0$). Provided that $\rho \neq 1$

and $\gamma \neq 0$, we can solve for the reduced form equation

$$y_{it} = \gamma \cdot \text{Repression}_{j[i]} + \psi \cdot \overline{\text{Repression}}_{ut[-i]} + \boldsymbol{\beta}' \boldsymbol{X}_{ij} + s\big(\text{lon}_{j[i]}, \text{lat}_{j[i]}\big) + \text{Cell}_{k[i]} + \text{Military unit}_{ut[i]} + \text{Month of war}_t + \epsilon_{it}, \quad (10)$$

where $\psi = \gamma\rho/(1-\rho)$ is the reduced form peer effect.

We estimate the above equation using OLS. The estimates are valid only if the assignment of soldiers to units with low versus high average levels of repression is exogenous. This assumption is plausible given the pressures of mass mobilization in the USSR. Soviet mobilization plans left little room for accommodating the individual preferences of 30 million military-age males (i.e. no self-selection) or organizing unit composition on a dimension as obscure as exposure to repression. Unit assignment had some systematic components — reservists (i.e. older individuals with prior training) were sent to the front more quickly than untrained conscripts, military commissariats responsible for implementing the draft were organized by regional military district (most covering tens of thousands of square kilometers), and specialized units existed for soldiers with both exceptional skills (e.g. special forces) and disciplinary problems (e.g. penal units). However, these specialized units represented a tiny share of the army, and we can address the correlation of individual abilities through unit fixed effects. We can similarly account for geographic sorting with fixed effects for the grid cell of a soldier's birth. Monthly fixed effects further account for common shocks due to seasonal variation and the changing dynamics of the war. In cases where the unit assignment was based on conscripts' observable characteristics (e.g. age, ethnicity, class), controlling for these variables should eliminate the potential upward bias in estimated group coefficients.

Table 8 reports the estimated reduced form parameters as well as the endogenous peer effects recovered from these estimates ($\hat{\rho} = \hat{\psi}/(\hat{\psi} + \hat{\gamma})$). The estimated coefficient on individual exposure to repression ($\hat{\gamma}$) remains consistent with our baseline estimates:

after controlling for the repression of a soldier's peers from the same unit (and other covariates), a one-quartile increase in repression (from 0 to 32 arrests) increased one's chances of death or injury by 0.7 percentage points, decreased flight by 0.7 points, and reduced the probability of a medal by 0.4 points. The individual level estimates in our baseline models do not seem to be confounded by peer effects.

	KIA/WIA	Flee	Medal
Direct individual effect ($\hat{\gamma}$)	0.2 (0.03)**	-0.2 (0.03)**	-0.1 (0.03)**
Reduced form peer effect ($\hat{\psi}$)	0.3 (0.02)**	-0.03 (0.02)**	-0.3 (0.02)**
Endogenous effect ($\hat{\rho}$)	0.6 (0.04)**	0.1 (0.1)*	0.8 (0.1)**
Mean Y	63.3	24.2	14.6
Gridcells	9,658	9,658	9,658
Birthplaces	128,354	128,354	128,354
Soldiers	4,369,898	4,369,898	4,369,898

Outcomes on percentage scale (0 to 100). Bootstrapped standard errors in parentheses. All models include grid cell, unit and month fixed effects, individual and birth location-level covariates. Observations weighted by record linkage probability. Sample includes disaggregated personnel records, with non-missing unit assignment and date information. Significance levels (two-tailed): $^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.

Table 8: ESTIMATES ADJUSTING FOR PEER EFFECTS.

For all three outcomes, the endogenous peer effect estimate ($\hat{\rho}$) is positive and significant at the 95 percent confidence level, confirming that soldiers' fortunes were positively correlated with those of others in their unit. If one's unit took exceptionally high losses in a given month, an individual's own chances of death or injury were considerably higher. A similar pattern held for the probabilities of fleeing or receiving a medal. Soldiers' behavior — for better or worse — varied with the behavior of their comrades-in-arms.

8. CONCLUSION

Our analysis of Red Army personnel records suggests that soldiers with greater exposure to Stalin's terror were more likely to fight to death or injury than to flee the battlefields of the Second World War. They were also less likely to show personal initiative in battle, as far as that can be inferred from military decorations.

We adopted multiple estimation strategies to ascertain whether these patterns reflect a spurious correlation or a genuine, potentially causal relationship. We exploited local randomness in the selection of repression victims, exogenous variation due to logistical costs, and geographic discontinuities due to local administrative discretion. All three designs indicate that soldiers from places with more prewar repression were more likely to fight until death and less likely to flee, but they also received fewer decorations for valor. We conducted a battery of robustness tests to consider inferential threats from clustered treatment assignment, measurement errors and the validity of the railway instrument. We also considered several alternative substantive explanations for our results, including discrimination, selective unit assignment, battlefield conditions and peer effects.

While we cannot exclude the possibility that other, unobserved factors are driving these statistical relationships, our analyses overwhelmingly suggest that the net effect of prewar repression was conformity. Soldiers from places with higher levels of repression obeyed orders and kept fighting not because repression turned them into zealous patriots willing to go beyond their call of duty, but because they were more cognizant of what the state might do if they did not comply. While past repression may have compelled less-motivated soldiers to more forcefully signal their resolve, our evidence also reveals that repression may have decreased effort by highly-motivated types. Together, the countervailing forces of deterrence and alienation may have helped resolve some principal-agent problems associated with fighting, but they did so more by inculcating "mindless obedience" (Murray and Millett, 2000, 26), than by incentivizing innovation or skill.

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