When War Divides: Unequal Sacrifice and Wartime Support

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Abstract

Interstate war is said to unite a nation, but it can reveal who bears its costs. I argue that unequal sacrifice erodes wartime support when it is attributed to state action. When losses fall on marginalized groups, voluntary recruitment obscures state responsibility by framing losses as individual choices. Compulsory call-ups make that responsibility visible and signal continued losses. Burdened groups compare their sacrifice with the dominant group's and turn against the war and the leadership. I test this with event-study and pre-post designs around Russia's 2022 mobilization decree, using fatality, protest, survey, and Telegram data. After the decree, regions where minorities had borne disproportionate losses saw more anti-war protest and online content, and minority respondents reported greater anger and lower leadership approval, while ethnic Russians did not. These findings imply that wartime cohesion depends on how losses are distributed across groups and whether the state is held responsible for them.

Keywords: interstate conflict; wartime support; casualties; mobilization decree; ethnic inequality; distributional costs of war; Russia–Ukraine war

Word count: 11,856

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1 Introduction

Why do some interstate wars unite domestic society while others divide it? A long line of scholarship holds that collective war fighting and shared suffering can expand state capacity, rally publics behind incumbents, and forge national solidarity (e.g., Mueller 1973; Sambanis and Lee 2026; Sambanis, Skaperdas and Wohlforth 2015; Tilly 1985; Wimmer 2018). Yet wars do not ask all citizens to sacrifice equally. Military service, combat exposure, and casualties are rarely distributed evenly across regions, classes, or social groups (e.g., Huff, Min and Schub 2025; Huff and Schub 2021; Lyall 2020; Rozenas, Talibova and Zhukov 2024). Existing research shows that local exposure to casualties can shape support for war and incumbents (e.g., Althaus, Bramlett and Gimpel 2012; Davenport 2015; Gartner 2008; Green-Riley and Leber 2023; Kriner and Shen 2010), but it focuses primarily on the level of local loss. It therefore remains unclear whether, and under what conditions, unequal human costs across politically salient groups erode war's unifying effects or turn them into opposition.

I argue that the unifying effects of interstate war depend on how its burdens are distributed. When war's costs fall disproportionately on politically marginalized groups relative to the dominant group and that inequality becomes attributable to the state, burdened groups can turn against the leadership and the war itself. These groups evaluate wartime losses not only in absolute terms but also comparatively, with the dominant group serving as their reference group. This logic draws on relative deprivation theory, which emphasizes that grievance emerges through comparison to relevant others (e.g., Gurr 1970; Smith et al. 2012; Walker and Pettigrew 1984). Yet burdened groups may not direct this grievance at the state when military service is understood as individually chosen. Compulsory call-ups bring these disparities into political view by making accumulated sacrifice more salient, undermining the narrative of voluntary service, and tying responsibility for future losses directly to the central government. Unequal sacrifice then becomes politically consequential, eroding burdened groups' support for the leadership and its war.

I test these expectations in the context of Russia's full-scale invasion of Ukraine, focusing on the partial mobilization decree announced in September 2022. The war has produced large and sustained casualties that have fallen unevenly across Russia's regions and ethnic groups, with several predominantly non-Russian republics bearing losses far out of proportion to their population shares (Bessudnov 2023; Mediazona 2025). The mobilization announcement provides a discrete, centrally imposed shock that made the state's role in extracting further sacrifice explicit. Combining regional fatality records compiled by Mediazona (2025) with a name-based ethnicity classifier (Bessudnov et al. 2023), I construct a regional measure of war burden disparity between ethnic Russians and non-ethnic Russian minorities. I then link this measure to anti-war protest events, individual-level survey responses, and posts on public Telegram channels across Russian federal subjects, and implement event-study and pre-post designs.

Empirical results across all three data sources are consistent with the argument. Regions where ethnic minorities had borne disproportionate fatalities prior to the decree became more likely to witness anti-war protest after the announcement. This association is absent in the weeks before the decree and appears immediately after the shock. Gallup survey evidence shows a parallel pattern at the individual level. Following the announcement, ethnic minorities in higher-disparity regions report greater anger and lower approval of the national leadership. By contrast, ethnic Russians, the politically dominant group, show no statistically significant shift. Telegram data extend the pattern to daily communication. As with protest, anti-war content in high-disparity regions rises sharply at the announcement and subsides within weeks, a period in which prosecutions accelerated and public expression narrowed. Convergence across three sources with different coverage and biases makes the pattern unlikely to be an artifact of any one.

The paper makes three contributions. First, it specifies a condition under which interstate war can fracture domestic society. Existing scholarship documents both the unifying effects of external war (e.g., Mueller 1973; Sambanis and Lee 2026; Tilly 1985; 1994) and its capacity

to corrode social and political order (e.g., Bauer et al. 2016; Fiedler 2023). I show how the distribution of war's human costs helps explain these divergent outcomes. When sacrifice falls disproportionately on politically marginalized groups and they hold the central government responsible, war can weaken their ties to the state. Concentrating sacrifice on peripheral groups may initially insulate leaders from domestic opposition, but this insulation can weaken as state responsibility becomes visible. The argument thus connects wartime cohesion to the broader question of how states sustain the extraction and political support needed to fight wars abroad (Levi 1997; Rosen 1996; Tilly 1985).

Second, the paper extends research on the political consequences of wartime casualties. Existing work shows that casualties shape support for incumbents and war, particularly when exposure is geographically or socially concentrated (e.g., Althaus, Bramlett and Gimpel 2012; Davenport 2015; Gartner 2008; Gelpi, Feaver and Reifler 2009; Green-Riley and Leber 2023; Kriner and Shen 2010). I examine how these responses depend on disparities in losses across groups within the same locality. Using a regional measure of minority-to-majority fatality risk, the analysis shows that unequal sacrifice becomes politically consequential after a mobilization decree makes the state's responsibility more visible. This connects research on unequal military service and wartime death (Huff, Min and Schub 2025; Huff and Schub 2021; Lyall 2020; Rozenas, Talibova and Zhukov 2024) to research on wartime political support.

Third, the paper identifies unequal military sacrifice as a form of group-relative inequality that interstate war can generate. Research on horizontal inequality examines disparities in economic, political, and social standing across groups as causes of internal conflict (e.g., Cederman, Wimmer and Min 2010; Cederman, Gleditsch and Buhaug 2013; Cederman, Weidmann and Gleditsch 2011; Stewart 2008). War against a foreign enemy can itself produce disparities by distributing military risk and death unevenly across domestic groups. Unequal sacrifice therefore provides one mechanism through which international conflict reshapes domestic group relations.

2 War, National Unity, and Unequal Sacrifice

A long tradition holds that interstate war binds citizens to the state. In the bellicist account of state-building, interstate conflict may strengthen state authority by expanding fiscal capacity, conscription, and administrative reach, drawing populations more closely into central state institutions (Centeno 2002; Herbst 2000; Hui 2005; Tilly 1985; 1994). War-making also encouraged cultural standardization. Institutions such as mass education and military service spread common languages, civic narratives, and national identities that made large-scale mobilization for war possible (e.g., Darden and Mylonas 2016; Krebs 2006; Lee, Zhang and Herchenröder 2024; Paglayan 2022; Posen 1993; Sambanis, Skaperdas and Wohlforth 2015; Wimmer 2018). Sociological and psychological accounts point in the same direction. Conflict with an external enemy can redirect internal antagonism toward a shared national identity (Coser 2012; Simmel 1955), while salient common identities can reduce intergroup bias under favorable conditions (Gaertner and Dovidio 2000). Although these traditions emphasize different processes, they share the expectation that interstate war can strengthen political integration and national solidarity.

However, the unifying effects of war are conditional. Rally-round-the-flag studies document increases in executive approval after international crises, but these effects are often short-lived and diminish as costs mount and political contestation returns (e.g., Brody 1991; Mueller 1973). External threat can likewise consolidate or divide depending on the domestic political environment into which it enters (Myrick 2021). Evidence from conflict-exposed settings sharpens this point. Exposure to violence often strengthens cooperation within immediate groups while leaving attachment to the broader nation or state weak, null, or negative (e.g., Bauer et al. 2016; Bellows and Miguel 2006; Cassar, Grosjean and Whitt 2013). War can therefore produce heterogeneous, group-specific responses instead of uniform national solidarity (Besley and Reynal-Querol 2014; Hadzic, Carlson and Tavits 2020). These findings establish that war does not always unite, but they leave open why its effects differ across groups within the same society.

One source of this variation is the distribution of war's costs within domestic society. How a government distributes a burden shapes who turns against it. Governments under economic sanctions preserve core support by displacing costs onto more vulnerable groups (Allen 2008). Those on whom burdens fall, in turn, make claims about fairness. In mass warfare, the fact that ordinary men bore the risk of dying generated demands that the wealthy pay a corresponding price through taxation (Scheve and Stasavage 2012).

The human costs of war are distributed as unevenly as the fiscal ones. Combat exposure and death fall disproportionately on particular places and social groups (e.g., Huff, Min and Schub 2025; Huff and Schub 2021; Rozenas, Talibova and Zhukov 2024). The unevenness is partly strategic, since regimes organize their armed forces under constraints of loyalty and coup prevention, often concentrating military participation and combat exposure among particular groups (e.g., Harkness 2017; McMahon and Slantchev 2015; Paine 2022; Talmadge 2015). Governments also make efforts to limit and obscure the costs of the wars they fight (e.g., Kreps 2018; Valentino, Huth and Croco 2010), treating those costs as politically consequential. What makes them consequential is how publics respond to the resulting deaths.

The casualty sensitivity literature offers a well-developed account of how wartime deaths generate political costs for the leaders. At the national level, mounting fatalities erode support for both wars and incumbent governments (e.g., Gartner 2008; Gelpi, Feaver and Reifler 2009; Kriner and Shen 2010). At the local level, these effects are especially pronounced where losses are concentrated, as communities that bury their own dead are more likely to punish political leaders at the polls (e.g., Althaus, Bramlett and Gimpel 2012; Davenport 2015; Green-Riley and Leber 2023; Kriner and Shen 2012). Existing work thus shows that the political fallout of wartime casualties hinges on both total volume and local concentration.

Yet, existing research primarily examines how many casualties a community has suffered, rather than how its losses compare with those borne by other groups. One important exception is Kriner and Shen (2014), who show that Americans become less tolerant of

casualties when informed that deaths fall disproportionately on poor communities. Their study demonstrates that the distribution of casualties can shape public opinion, but it examines reactions among the public at large rather than the response of the group bearing the disproportionate burden. Therefore, existing research leaves unexplored how disproportionately burdened groups respond to such inequalities, and whether war's unifying effects survive when its most severe costs fall unequally across politically salient groups.

Addressing this gap is critical because military service and combat risk historically fall unevenly across social hierarchies. From conscription in Napoleonic France (Forrest 1989) and the Habsburg Monarchy (Judson 2016) to the recruitment of Punjabi Sikhs (Singh and Shani 2022) and Israeli Druze (Krebs 2006), states have long drawn soldiers disproportionately from particular groups. Race and class have likewise shaped who dies, as the disproportionate losses among African Americans and the working class in the Vietnam War illustrate (Appy 1993). Across these varied contexts, military burdens have repeatedly followed existing social and political divisions.

3 A Theory of War Burden Disparity

Foreign war does not impose a uniform social shock. In politically and socially heterogeneous states, wartime costs may fall unevenly across communities divided along ethnic, racial, religious, or class lines (e.g., Birnir et al. 2018; Cederman, Weidmann and Gleditsch 2011). I define *war burden disparity* as a relational condition in which a politically marginalized in-group bears a disproportionate share of wartime costs relative to that borne by a politically dominant out-group. These groups are socially bounded communities whose members share a collective identity (e.g., Tajfel and Turner 2000) and occupy different positions in relation to central political power.

These disparities are not exogenous to social structure. They form before the war through recruitment, occupational, and deployment patterns, and through deliberate choices over

whom to arm and where to send the soldiers (e.g., Harkness 2017; Huff and Schub 2021; Lyall 2020; Petersen 1989). Whether engineered or inherited, the disparities persist, since arrangements that keep losses away from politically central constituencies give governments little reason to intervene (e.g., Bueno de Mesquita et al. 2003; Valentino, Huth and Croco 2010). Yet a standing disparity need not be politically consequential. Its effects depend on how unequal sacrifice is interpreted. When military service appears voluntary or contractual, unequal losses can be understood as the cumulative result of individual decisions rather than as a distributional choice by the state. This interpretation survives only while the state can meet its manpower needs from the existing base.

I argue that a compulsory call-up disrupts this interpretation. It converts a disparity previously associated with voluntary service into an expectation of state-imposed unequal risk. By removing the appearance of individual discretion, it makes the state visibly responsible for who will be sent to fight. The expectation of the risk rests on what citizens can observe. Because conscripts require training before deployment, the call-up does not immediately alter the distribution of fatalities, and citizens do not yet know how the new summonses will be allocated. The existing pattern of wartime deaths is almost only available guide to which groups will bear the new compulsory burden. The call-up is also publicly and simultaneously announced, so each person can infer that others have received the same information and drawn the same conclusion about who will be taken (e.g., Beissinger 2002; Kuran 1991). Hence, once exposure becomes attributable to the state, the accumulated disparity gives the marginalized group reason to withdraw support from the leadership responsible for the war.

Citizens do not evaluate wartime losses only in absolute terms. They also compare their group's sacrifice with that borne by other groups. Members of a community that has suffered visibly greater losses than the politically dominant group may infer that the state treats their group as more expendable, giving rise to group-based relative deprivation (Gurr 1970; Smith et al. 2012; Walker and Pettigrew 1984). Citizens comply more readily with

wartime extraction when burdens appear fairly shared (Levi 1997), and a group bearing a disproportionate share has reason to view that condition as violated.

The target of this grievance is the central government, and the group's political position explains why. For a marginalized group, the dominant group against which it measures its sacrifice also enjoys greater access to central political power. When responsibility for unequal sacrifice is concentrated in a visible state decision, grievance is therefore directed upward toward the leadership that imposed the burden. The immediate affective response should be amplified anger. Politically, it should appear as lower approval of the national leadership and greater opposition to the war.

Because the mechanism depends on comparison between groups, resistance should be strongest among those who bear the heavier burden. This group may be defined by ethnicity, region, religion, or class, provided it is socially distinct and politically subordinate. Since these identities often overlap, the same grievance may be framed in several ways. Members of the dominant group may also oppose conscription, but because they do not experience a disproportionate burden, their opposition should not vary with the inequality.

The argument above yields the following hypothesis:

H1. Following a compulsory call-up, greater pre-existing disparity in regional war burdens reduces support for the war and the national leadership among disproportionately burdened groups, but not among the dominant group.

The argument applies most clearly under three conditions. First, citizens must be able to see which groups are dying at higher rates. They do not need exact casualty counts, but they need some way to tell that their group is bearing more of the losses. When the state tightly controls wartime information (e.g., Gibilisco and Steinberg 2023), the state can present unequal losses as shared heroism, making it harder for burdened groups to blame the government for the disparity. Second, past deaths must be a credible signal of who will be called up next. Burdened groups react to mobilization because they expect the existing

pattern of losses to continue. Third, the war's aims must be contested. A defensive war against an existential threat may produce enough national solidarity to override the group comparisons at the center of the argument (e.g., Van Evera 1994).

4 The Russian Home Front During the 2022 Invasion of Ukraine

I test the hypothesis on Russia's home front during its war against Ukraine. The conflict began in 2014 with Russia's annexation of Crimea and its support for separatist forces in eastern Ukraine. The invasion of February 24, 2022, turned it into a full-scale interstate war and sharply intensified the fighting (Davies, Pettersson and Öberg 2023). The war drew soldiers from communities across Russia and imposed heavy human costs on them.

The case offers particular leverage to evaluate the theory because a nationwide shift in military recruitment occurred against substantial pre-existing variation in wartime burden. On September 21, 2022, the Kremlin announced a partial mobilization decree intended to recruit approximately 300,000 additional personnel. Although officially presented as a limited mobilization, the policy was implemented through regional quotas that encouraged broad and sometimes indiscriminate call-ups (Cancian 2022). Its designation as "partial" also provided little assurance that the scope of compulsory service would remain limited. As human rights advocate Sergei Krivenko warned, what was partial "can become universal at a moment's notice" (Sherwin 2022). The decree therefore made the possibility of state-imposed conscription salient beyond those immediately mobilized. Whereas prior exposure could still be attributed partly to voluntary enlistment and economic incentives, mobilization revealed the government's capacity to compel participation and thereby changed how citizens could interpret both future risk and the disparities in sacrifice that had already emerged.

These disparities were especially politically meaningful because of Russia's multiethnic federal structure. Ethnic Russians constitute the dominant political core, while many

minority groups are territorially concentrated in republics and regions with historically and politically salient boundaries (Kivelson and Suny 2016). Recruitment during the war drew disproportionately from peripheral and minority regions (Driscoll, Sichinava and Berglund 2025), generating substantial regional variation in military losses before and after mobilization. The combination of a common national shock and pre-existing disparities therefore makes it possible to examine whether political responses changed more sharply after the decree in regions where minority communities had already borne greater relative war burdens.

Although the Russian government does not publish casualty figures disaggregated by ethnicity or region and has repeatedly minimized or obscured its losses, local deaths remained visible through funerals, personal networks, and regional online groups. Social media has also circulated politically sensitive information beyond state control in Russia (Enikolopov, Makarin and Petrova 2020). Citizens could therefore form impressions of the losses borne by their own communities even without access to comprehensive national casualty statistics.

Anecdotal accounts indicate that members of marginalized communities were already aware of unequal wartime burdens before the mobilization decree. By May 2022, Dagestan and Buryatia had the largest numbers of confirmed deaths, and local media and Telegram channels in Dagestan carried images of grieving relatives receiving official condolences (AFP 2022). Free Buryatia, founded in March 2022 as the first anti-war organization created by members of a Russian ethnic minority, cited the disproportionately high Buryat death toll as a reason for its founding. Its activists reconstructed the toll from social media posts, official statements, and reports of funerals, counting about 2.8 percent of recorded fatalities against a population share of 0.3 percent (Shcherbakova 2022). In Astrakhan, a Kazakh resident asked of the local dead in May 2022: "[Kazakhs] are clearly not the absolute majority [of Astrakhan soldiers]. Why do we hear almost only about their deaths?" (Radio Free Europe/Radio Liberty 2022). Such accounts are illustrative and may not represent the typical member of each group, but they suggest that minority communities could perceive

their relative burden before mobilization.

After the decree, the comparison became more explicit. In an October 2022 interview, Victoria Maladaeva of the Free Buryatia Foundation described Buryats and Tuvans as facing substantially greater risks of wartime death than ethnic Russians and portrayed mobilization as extending this disparity (Petkova 2022). Her account shows that wartime deaths were publicly framed as an unequal burden across regional and ethnic groups once mobilization made the state's role visible.

The key explanatory variable is the disparity in wartime fatalities between the politically dominant ethnic group and marginalized minority groups. I measure this concept using combat fatality records compiled by *Mediazona* in collaboration with BBC News Russian and volunteer contributors (Mediazona 2025).[1] These records link the fallen to their places of origin using verified local news reports, official notices, and relatives' social media posts. Official Russian statistics do not report casualties by ethnicity, and the state has strong incentives to obscure aggregate losses (Gibilisco and Steinberg 2023). These open-source, name-linked records are therefore the most systematic available evidence of who is bearing the costs of the war.

A natural concern is that these records undercount deaths. This concern is limited for the period I use to construct the measure. *Mediazona* and *Meduza* also produce an independent estimate of total losses based on excess inheritance cases opened for men in the National Probate Registry (Meduza 2025). For deaths between 2022 and mid-2023, the name-linked records closely track this probate-based estimate. Because the pre-decree window ended long before the data were compiled, nearly all deaths from this period have since been identified and added to the lists. This high coverage also limits how much regional differences in reporting can drive the measure. To the extent that some deaths from marginalized, rural, and minority communities remain unreported, the documented disparity would understate true inequality in sacrifice.

[1] *Mediazona* is an independent Russian outlet operating in exile that has been designated a "foreign agent" by the Russian government.

Ethnicity is inferred using the name-based classifier developed by Bessudnov et al. (2023), which is trained on *VKontakte* profiles containing self-reported language and location information.[2] I assign each individual to the aggregated category with the highest predicted probability.[3] Following Driscoll, Sichinava and Berglund (2025), I classify individuals labeled "Belarusian-Russian-Ukrainian" as East Slavic (ES) and all others as non-East Slavic ethnicities (NESE).[4] Following the Ethnic Power Relations data set (Vogt et al. 2015), I treat ES as the politically dominant group and NESE as the politically marginalized group.

I operationalize war burden disparity at the region level using the logged death risk ratio (DRR) between the NESE and ES populations.[5] The measure compares the per-capita death rate of the NESE population with that of the ES population, so it accounts for the demographic composition of each federal subject. Taking logs makes deviations from parity symmetric, so that equivalent disparities in opposite directions have equal magnitudes and opposite signs. It also limits the influence of a few extreme regional values.

Let $d_{i,s}^{NESE}$ and $d_{i,s}^{ES}$ denote combat deaths on day s in region i for the two groups, and let N_i^{NESE} and N_i^{ES} denote their population counts from the 2010 Russian Census.[6] Group g's cumulative combat deaths in region i, from the onset of the war on February 24, 2022 (T) through September 20, 2022, the day before the mobilization decree (M), are $D_i^g = \sum_{s=T}^{M-1} d_{i,s}^g$. The DRR for region i is the log ratio of the two groups' per-capita death rates:

[2]Appendix A.1 provides further details on the classification procedure.

[3]As a robustness check, I replicate all analyses using fractional category counts, summing each individual's predicted probabilities instead of assigning the individual exclusively to one category.

[4]Name-based methods cannot reliably distinguish subgroups within the ES category because of overlapping naming conventions.

[5]Throughout, "region" refers to federal subjects (*subyekty federatsii*).

[6]I rely on the 2010 Census instead of the 2021 Census, whose results have been widely questioned for irregularities in ethnic enumeration (e.g., Coalson 2023). Although the 2010 figures may introduce some measurement error, such error would most likely attenuate the estimated effects.

$$DRR_i = \log\left(\frac{D_i^{NESE}/N_i^{NESE}}{D_i^{ES}/N_i^{ES}}\right). \tag{1}$$

Positive values indicate that the non-East Slavic population bore more deaths relative to its size than the East Slavic population. The disparity measure is z-standardized across regions.

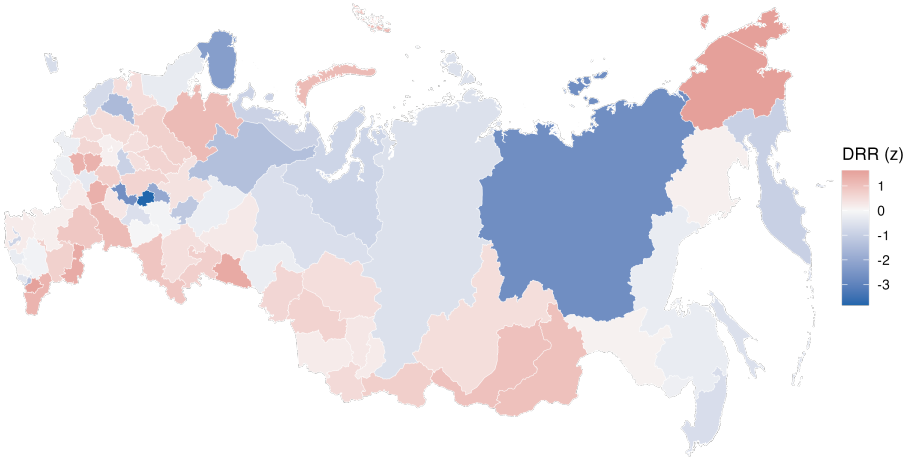


Figure 1: Pre-mobilization Decree Death Risk Ratio

Notes: The figure shows the standardized logged death risk ratio between non-East Slavic (NESE) and East Slavic (ES) groups. Red indicates a higher burden borne by NESE populations, and blue indicates a higher burden borne by ES populations.

Figure 1 maps the disparity across Russian federal subjects before the mobilization announcement. War burden disparity was far from evenly distributed. High values appear in several peripheral and minority regions, including Chechnya, Dagestan, Buryatia, Chukotka, and Astrakhan, while low values appear in regions such as Chuvashia, Sakha, Mordovia, Mari El, and Murmansk. The analysis below uses this pre-existing variation to examine whether responses to the mobilization decree differed with the burden minorities had already borne.

Before turning to the main analyses, I examine standardized bivariate associations between the pre-mobilization death-risk ratio and observable regional characteristics. The ratio is most strongly associated with the ethnic Russian population share and prior

cumulative death rate, while its associations with regional product, prior repression, and distance from Ukraine are comparatively weak. These patterns provide little descriptive support for accounts based primarily on economic disadvantage, coercion, or proximity to the war, although they cannot rule out confounding by unobserved factors. See Appendix A.2 for details.

5 Quantitative Evidence

This section evaluates the theoretical expectations using the pre-decree death-risk ratio and three observational data sources, namely protest events, survey responses, and posts on regional Telegram channels. Reliance on existing data is both an ethical and a practical choice. Conducting original surveys or interviews in Russia, particularly with ethnic minorities about an ongoing war and military mobilization, could expose respondents to substantial risks.

Russia's authoritarian context complicates the interpretation of each source. Surveillance, censorship, and coercion raised the costs of criticizing the war, while the state tightly controlled public narratives. Authorities mandated the euphemism "special military operation" (*spetsial'naya voennaya operatsiya*) in official reporting and criminalized statements contradicting the government's account of the conflict.[7] Protest participation, survey responses, and critical online expression therefore likely understate opposition to the war and mobilization.

The three sources capture complementary dimensions of political response. Protest events measure publicly observable collective action despite its high costs. Surveys compare the attitudes of marginalized and dominant groups within the same regions. Regional Telegram channels capture shifts in everyday political communication. Because each source is subject

[7]Federal Law No. 32-FZ of 4 March 2022 amended the Criminal Code to penalize the dissemination of knowingly "false information" about the armed forces (Article 207.3) and public actions "discrediting" their use (Article 280.3), with penalties of up to 15 years' imprisonment.

to different forms of selection and self-censorship, consistent patterns across all three provide stronger evidence than any single measure alone.

The following analyses examine whether a fixed, pre-existing war burden disparity predicts political responses once the mobilization decree makes the state's role in allocating wartime risk more visible. Because the disparity measure is constructed entirely from pre-decree fatalities, later political responses cannot have shaped it. A local discontinuity-in-time analysis finds no detectable break in the cumulative death risk ratio at the announcement (Table B.1), consistent with the decree changing the interpretation of an existing disparity rather than the disparity itself.

5.1 Behavioral Evidence: Anti-war Protests

The first outcome is anti-war protest, a high-threshold act that openly challenges both the state and the war. Given Russia's extensive surveillance and coercive capacity, participation entails substantial personal risk, making observed protest a conservative indicator of political dissent (e.g., Enikolopov, Makarin and Petrova 2020; Naunov 2025; Robertson 2010; Rosenfeld 2017). Anti-war protests are manually identified from the Armed Conflict Location and Event Data (ACLED) project (Armed Conflict Location & Event Data 2025). Russia offers an unusual measurement advantage for studying protest under authoritarianism. Event-data coverage generally depends on the density of the reporting environment and is often limited where governments restrict information (Baum and Zhukov 2015; Weidmann 2016). Yet Russia's geopolitical importance attracts sustained attention from international media and monitoring organizations, resulting in comparatively extensive documentation of contentious events despite domestic information controls.

Models

I estimate an event-study model in which the unit of analysis is a federal subject i observed over two-week event-time periods k relative to the mobilization announcement. I use two-

week periods because protest events are sparse. At the daily or weekly level, most region-periods contain no anti-war protest, producing many zeros and making estimates sensitive to small changes in event timing. The final pre-decree period, $k = -1$, serves as the reference period. I examine periods $k \in \{-4, \ldots, 1\}$, covering approximately eight weeks before mobilization announcement and four weeks after.

Specifically, I estimate the following linear probability model:

$$Y_{r,t} = \sum_{k \neq -1} \beta_k \left(\mathbb{1}\{K_t = k\} \times DRR_r^z \right) + \boldsymbol{\gamma}^\top \left(Post_t \times \mathbf{Z}_r \right) + \alpha_r + \delta_t + \varepsilon_{r,t}, \tag{2}$$

in which $Y_{r,t}$ is an indicator variable equal to one if region r experienced any anti-war protest during two-week period t, K_t is event time in two-week periods relative to the mobilization decree, and DRR_r^z denotes the standardized pre-decree log death risk ratio between NESE and ES fatalities. $Post_t$ indicates periods after the decree. α_r and δ_t denote region and period fixed effects, respectively. The coefficients β_k trace how the association between pre-decree war burden disparity and anti-war protest changes across event time relative to the final pre-decree period. The vector $\mathbf{Z}_r$ includes the regional share of ethnic Russian residents, the pre-decree fatality rate, and pre-decree anti-war protest activity. Russian population share and the prior fatality rate exhibit the largest observed associations with the pre-decree death risk ratio (Appendix Figure A.3), while prior protest captures pre-existing differences in regional opposition. I therefore interact these covariates with the post-decree indicator to account for the possibility that regions differing on these characteristics experienced systematically different changes in protest following the decree. The time-invariant main effects are absorbed by the region fixed effects. Standard errors are clustered by region in all specifications.

Identification rests on a parallel-trends assumption that, in the absence of the mobilization decree, the relationship between pre-decree war burden disparity and anti-war protest would have evolved similarly across regions. I assess this assumption by inspecting the estimated coefficients for the pre-decree periods and testing whether these coefficients

are jointly distinguishable from zero.

Results

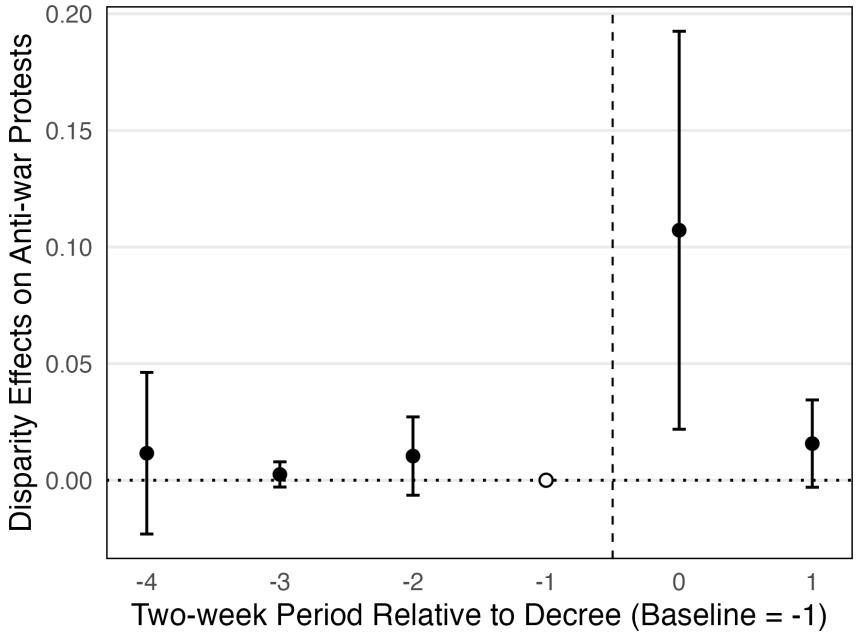


Figure 2: Differential Protest Responses to Mobilization Decree by Pre-Decree Death Risk Ratio

Figure 2 presents the event-study estimates. The coefficients for the pre-decree periods are small and centered near zero. The joint Wald test for the pre-decree coefficients fails to reject the null of no differential pre-trends ($p = 0.59$). This pattern suggests that high-disparity regions were not already on a distinct protest trajectory before the decree.

The pattern changes sharply after the mobilization announcement. In the first two-week period after the mobilization decree, a one-standard-deviation increase in the pre-decree death risk ratio is associated with a 10.42 percentage-point increase in the probability of observing any anti-war protest in a region-period. The coefficient in the following two-week period remains positive but is smaller and less precisely estimated. Averaging across post-decree periods, a one-standard-deviation increase in war burden disparity is associated with a 5.24 percentage point increase in the probability of anti-war protest (Table B.2). These

estimates are substantively meaningful given the rarity of open protest in an authoritarian setting.

The protest results are robust to alternative specifications and measurement choices. Adding logged gross regional product, pre-decree ES and NESE fatality rates, distance to Ukraine, and pre-decree repression leaves the post-decree event-study estimate positive and statistically significant. Controlling for ES and NESE fatality rates makes it less likely that the estimate reflects only the volume of majority or minority losses. The pre-decree coefficients remain close to zero, and the joint pre-trend test again fails to reject the null ($p = 0.588$). The results also hold when I construct the death-risk ratio using probability-weighted category counts rather than assigning each casualty exclusively to its most likely ethnic category. Appendix B.2 provides full results.

5.2 Survey Evidence: Anger and Approval of the Leadership

The protest analysis establishes a behavioral pattern at the regional level. I next examine whether the same pattern appears in individual attitudes and emotions. To do so, I draw on the 2022 Russia wave of the Gallup World Poll.[8] The survey was fielded between August 13 and November 2, 2022, which places the September 21 partial mobilization announcement inside the field period.

Models

I categorize respondents who self-identify as ethnic Russians or Belarusians as *Ethnic Russians* while those who self-identify as other ethnic groups in Russia are coded as *Minority*, consistent with the regional analyses.[9] After this coding, the analysis sample includes 1,897

[8] https://www.gallup.com/analytics/318875/global-research.aspx (accessed September 4, 2025).

[9] I drop respondents without Russian citizenship and ethnic Ukrainians from the analysis. The exclusion of ethnic Ukrainians reflects the distinct position of a group whose kin state is the target of Russia's war. Although the mobilization decree and wartime burden disparity could in principle affect how respondents report politically sensitive identities, this concern is

respondents, of whom 184 are coded as minorities,[10] from 59 federal subjects.

I designate two primary survey outcomes. The first is anger, measured by Gallup's question asking whether the respondent experienced anger during much of the previous day.[11] The second is leadership approval, measuring whether one approves of the country's leadership.[12] I also explore other variables, including broader negative affect and confidence in the national government and military, in the Appendix B.3.1.

I estimate the following linear model:

$$\begin{aligned} Y_{i,t} \; &= \beta_1 \left(DRR^{z}_{r(i)} \times Post_t \times Minority_i \right) + \beta_2 \left(DRR^{z}_{r(i)} \times Post_t \right) \\ &\quad + \beta_3 \left(DRR^{z}_{r(i)} \times Minority_i \right) + \beta_4 \left(Post_t \times Minority_i \right) + \beta_5 Minority_i + \boldsymbol{\gamma}^{\top} \mathbf{X}_i \\ &\quad + \alpha_{r(i)} + \delta_t + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

where $Y_{i,t}$ denotes the outcome for respondent i interviewed on day t. $DRR^{z}_{r(i)}$ is the standardized pre-decree log death risk ratio in the respondent's region, $Post_t$ indicates interviews conducted after the mobilization decree, and $Minority_i$ identifies ethnic-minority respondents. Because ethnic Russians constitute the reference group, β_2 gives the post-decree association between the death risk ratio and the outcome among ethnic Russians. The triple-interaction coefficient, β_1, gives the difference in this association between ethnic minorities and ethnic Russians. The corresponding association among ethnic minorities is therefore $\beta_1 + \beta_2$. The vector $\mathbf{X}_i$ includes university education, gender, age, and household income. Region fixed effects, $\alpha_{r(i)}$, absorb time-invariant regional differences, including the

limited here. The Gallup survey does not allow nonresponse on the citizenship and ethnicity items, and I find no evidence of differential post-decree changes in reported citizenship or ethnicity across higher- and lower-disparity regions.

[10]The minority respondents are evenly distributed around the decree cutoff, with 92 interviewed before and 92 after the announcement. Due to the sample size constraints, I cannot estimate event-study models.

[11]The item asks, "Did you experience the following feelings during a lot of the day yesterday? How about anger?" Response options are "Yes" and "No."

[12]The item asks, "Do you approve or disapprove of the job performance of the leadership of this country?" Response options are "Yes" and "No."

main effect of the death risk ratio, while interview-date fixed effects, δ_t, absorb shocks common to respondents interviewed on the same day, including the main effect of $Post_t$. Because minority and ethnic-Russian respondents are observed within the same regions and interview dates, the triple interaction compares their differential post-decree responses to regional burden disparity net of common regional and daily conditions.

Identification relies on the assumption that, absent the mobilization decree, the relationship between the pre-decree death risk ratio and the outcomes would not have changed differentially across the announcement date, either across regions or between minority and ethnic Russian respondents. Because the minority sample is too small to support a sufficiently precise event-study analysis, this assumption cannot be directly tested. Standard errors are clustered by federal subject, and Gallup survey weights are applied.

The quantities of interest are the post-decree changes in the death-risk-ratio association among minority and ethnic Russian respondents and the difference between them. Because the measure is signed, higher values indicate that minorities bore a greater relative share of fatalities, whereas lower values indicate that ethnic Russians bore a greater relative share. The theory therefore predicts that mobilization will produce stronger negative responses among minority respondents as the pre-existing death-risk ratio increases, but no comparable response among ethnic Russians, who were relatively insulated from the burden in those regions.

Results

Figure 3 reports the results using quantities derived from the regression estimates. Consistent with the theory, the post-decree response to pre-existing disparity is concentrated among minority respondents. Among these respondents, a higher pre-decree death risk ratio is associated with a larger increase in anger after the mobilization announcement. A one-standard-deviation increase in the death risk ratio is associated with roughly a 10-percentage-point increase in the probability of reporting anger. Among ethnic Russians living in the

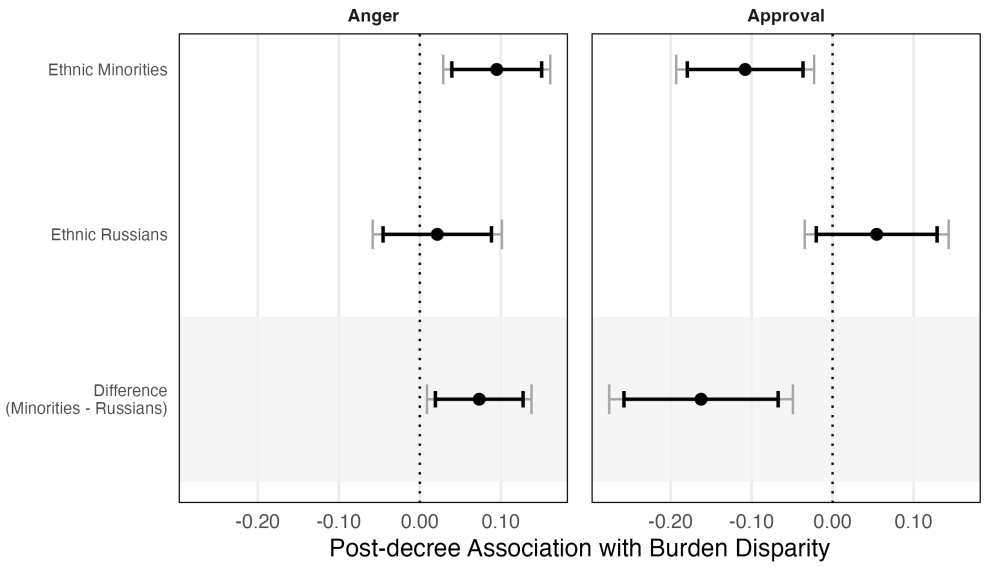


Figure 3: Post-Decree Changes in the Associations of Death-Risk Disparity with Leadership Approval and Anger

Notes: Points show post-decree associations for ethnic minorities and ethnic Russians and their difference. Shading marks the difference estimates; black and gray bars show 90 and 95 percent confidence intervals.

same regions, the corresponding association is statistically indistinguishable from zero. The minority–Russian difference is also statistically significant at the 5% level. This pattern suggests that the mobilization order did not simply generate a uniform regional reaction. Instead, it made pre-existing burden disparity salient for the group positioned to interpret that disparity as group-relative injustice.

The pattern is similar for leadership approval. Among minority respondents, higher pre-decree burden disparity is associated with lower approval of the national leadership after the announcement. A one-standard-deviation increase in the death risk ratio predicts a decline of about 0.12 points in leadership approval. Among ethnic Russians, the corresponding association is close to zero and statistically indistinguishable from zero. The minority–Russian difference is statistically significant at the 1%-level.

These results are consistent with the theoretical expectation. The response appears among ethnic minorities, for whom minority-skewed sacrifice falls on their own community, and in the outcomes closest to the mechanism: anger and leadership approval. Sadness and

Gallup's Negative Experience Index move tightly with anger (Figure B.1). Confidence in the government and military does not shift, though non-response on these regime-evaluation items rises modestly among minorities in higher-burden regions (Figure B.2), suggesting a marginal diffuse effect on state institutions broadly. Importantly, the minority–Russian differences in anger and leadership approval remain statistically significant after adjustment for multiple testing across the eleven Gallup outcomes reported in the main text and appendix (Table B.6).

Across the outcomes, the response is limited to ethnic minorities. By contrast, among ethnic Russians, the associations are indistinguishable from zero, and the minority–Russian difference is itself significant on both primary outcomes. Had unequal sacrifice generated grievance simply through a group's own accumulated losses, the association should not differ by group. Disproportionate loss becomes politically consequential when it falls on a group positioned to read it as unequal treatment by the state, which distinguishes a group-relative account from a casualty-exposure account.

Findings from the Gallup data analysis are robust to alternative specifications and measurement choices. Adding post-decree interactions with the regional ethnic Russian population share, Russian death rate, and minority death rate leaves the estimated responses among minority respondents substantively unchanged. The results also hold when the death-risk ratio is constructed using probability-weighted category counts rather than assigning each casualty exclusively to its most likely ethnic category. See Appendix B.3 for details.

5.3 Social Media Evidence: Anti-war Content in Public Telegram Channels

The protest and survey analyses capture visible contention and individual attitudes. Telegram evidence adds daily observation, which locates the response in time more precisely than protest events measured in two-week periods or survey responses split into pre- and post-decree periods. It also captures political expression among people who neither protest

nor answer surveys, which is most of the population under conditions where both carry risk. Its users skew younger, which overlaps with the population exposed to the call-up, though the same skew under-represents the rural communities where casualties were concentrated.

I construct a corpus of public Telegram channels oriented toward specific Russian federal subjects, identified through searches on federal-subject, city, town, and district names. Channels enter the corpus if they are publicly accessible, have nontrivial audiences, show a stable local or regional orientation, and post on both sides of the announcement. I retain channels devoted to local news, incidents, public safety, community affairs, local politics, public services, and other matters of shared concern to residents, and exclude government, politician-centered, military, commercial, and diaspora-oriented channels, along with channels whose local public status cannot be determined from channel-level information. Appendix C.1 reports the full inclusion criteria. The procedure yields 467,936 posts and replies from 456 channels across 77 regions during the two-month window around the decree.

Anti-war content is identified in two stages. First, I apply a ChatGPT 5.4 Nano model (OpenAI 2026) to screen all posts and replies. I define anti-war content as opposition to the war or the mobilization decree, criticism of the government's war policy, endorsement of anti-war protest or draft resistance, and oblique expressions of the same stance, including grief framed as a loss caused by the war and disavowal of the war as belonging to the leadership. Coded content ranges from direct statements, such as calling the mobilization a crime against one's own people, to indirect ones, such as mourning a neighbor who was buried after being sent to fight in a war the writer describes as belonging to someone else.[13] This first-stage screen identifies 1,315 posts as potentially anti-war. Second, I verify by hand only the posts flagged in the first stage, confirming 586 as anti-war. I adopt this procedure because the screening model produces many false positives but comparatively few false negatives. It therefore serves as a permissive first-stage filter, and hand verification removes the false positives it admits. Because the screen also misses some anti-war content

[13]Because expressing these views was prosecutable under Russian law during the period studied, I do not reproduce posts verbatim or identify the channels in which they appeared.

that cannot be recovered, the measure captures a conservative subset of anti-war expression, which attenuates estimated associations toward zero. Appendix C.2 reports the prompt, the validation results, and the coding protocol.

The corpus provides evidence about regional online information environments rather than representative public opinion, and it does not isolate speech by affected minority individuals. It is also not representative of all channels active in 2022, since the audience threshold is measured at collection and the balanced-panel requirement conditions inclusion on channels remaining active and accessible. The resulting corpus reflects public discourse among surviving regional channels with nontrivial audiences.

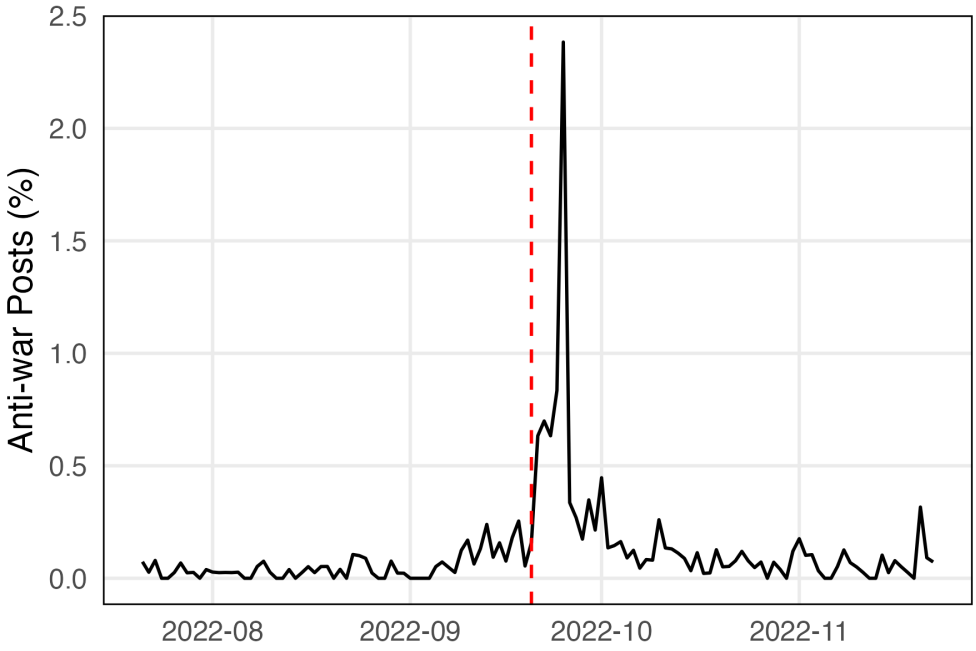


Figure 4: Anti-war Content in Public Local Telegram Sphere

Notes: The figure plots the daily share of posts and replies classified as anti-war in the Telegram corpus. The vertical line marks the September 21, 2022 mobilization announcement.

Figure 4 displays the daily share of anti-war content in the corpus. Anti-war content rises sharply after the announcement and then declines over the following weeks. The decree produced a broad discursive shock across Russian Telegram channels. The event-study design that follows examines whether this increase was greater in regions where minorities had borne

a disproportionate share of the burden before the announcement.

Models

I estimate a message-level event-study model to examine whether pre-decree disparity predicts the appearance of anti-war content. The outcome, $Y_{m,c,t}$, is an indicator equal to one if message m (a post or reply) in channel c on date t is classified as anti-war. The key predictor is the standardized pre-decree log death risk ratio, DRR_r^z, measured at the regional level. Event time, K_t, is measured in weeks relative to the mobilization decree, with the week immediately before the decree, $k = -1$, serving as the reference period. The analysis covers weeks $k \in \{-9, \ldots, 8\}$.

Specifically, I estimate the following model:

$$\begin{aligned} Y_{m,c,t} = &\sum_{k \neq -1} \beta_k \left(\mathbb{1}\{K_t = k\} \times DRR_{r(c)}^z \right) + \theta Reply_m \\ &+ \pi \left(Post_t \times RussianProp_{r(c)} \right) + \alpha_c + \delta_t + \varepsilon_{m,c,t}, \end{aligned} \tag{4}$$

where channel fixed effects (α_c) absorb time-invariant differences across Telegram channels, including regional characteristics such as the main effect of the death risk ratio, and date fixed effects (δ_t) absorb daily shocks common to all channels, including the overall national surge in war-related discussion around the decree. I include an indicator for whether the message is a reply because replies may differ systematically from channel posts in tone, emotion, or oppositional content. $RussianProp_{r(c)}$ is the share of ethnic Russian residents in the channel's region from the census, and its interaction with the post-decree indicator accounts for regions with different ethnic composition responding differently after the decree. Standard errors are clustered by region.

The coefficients β_k estimate whether anti-war content changes more in high-disparity regions than in low-disparity regions in each week relative to the final pre-decree week. If mobilization activates latent burden disparity, the pre-decree coefficients should be close to

zero, while post-decree coefficients should become positive.

Results

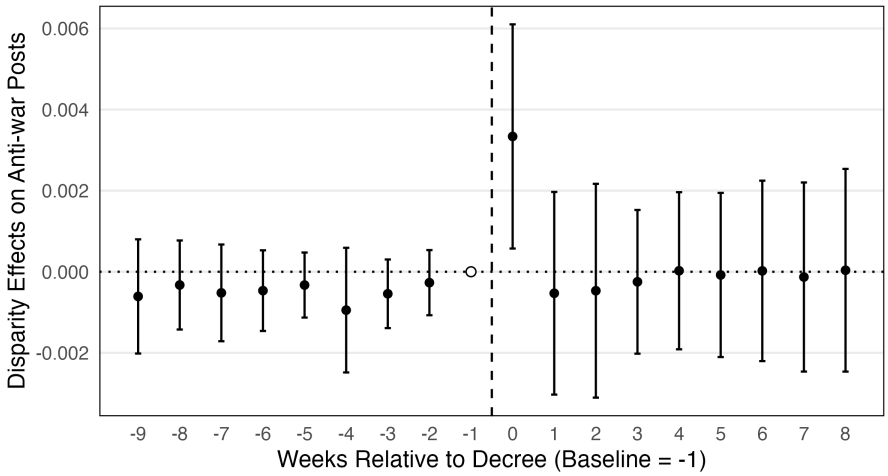


Figure 5: War Burden Disparity and Anti-war Telegram Content Around Mobilization Decree

Notes: Points show event-study estimates of the association between pre-decree burden disparity and anti-war content by week relative to the mobilization announcement. The reference period is the week before the decree. Vertical bars denote 95 percent confidence intervals. Standard errors are clustered by region.

Figure 5 reports the event-study estimates. The pre-decree coefficients are small and statistically indistinguishable from zero. A joint test of the eight pre-decree coefficients fails to reject the null of no differential pre-trends ($p = 0.46$).

In the week of the mobilization decree, the coefficient increases sharply to 0.0033 ($p = 0.019$). A one-standard-deviation increase in pre-decree war burden disparity is therefore associated with a 0.33-percentage-point larger increase in the probability that a post or reply is anti-war, relative to the week immediately preceding the decree. The coefficient returns close to zero in the following week and remains statistically indistinguishable from zero thereafter. The results indicate a sharp but short-lived increase in anti-war content in higher-disparity regions following mobilization.

The short duration of the increase does not necessarily imply that the underlying grievance dissipated equally quickly. The mobilization decree created an immediate information and attribution shock, but public expression of that response carried substantial risk. During the month following the decree, Russian authorities detained individuals for anti-war social-media posts and opened criminal cases over online statements criticizing mobilization (OVD-Info 2022; 2023). More broadly, wartime restrictions on online speech contributed to an environment of self-censorship (Freedom House 2023).

Against this background, both users and channel administrators had incentives to limit the visibility of anti-war discussion after the initial reaction. Users may have deleted posts, moved to private conversations, or adopted less explicit language, while administrators may have removed comments or restricted replies to reduce legal exposure. These specific responses are not observed directly in the data and should therefore be treated as possible mechanisms rather than established explanations. The decline in observable content may reflect both fading attention and a contraction in the willingness or opportunity to express opposition publicly. The evidence is therefore strongest for an immediate activation of visible anti-war discourse, not for the rapid disappearance of the underlying grievance.

The Telegram results are robust to alternative specifications, measurement choices, and levels of aggregation. Adding time-varying regional controls leaves the estimated post-decree increase in anti-war content substantively unchanged. The results also hold when I construct the death-risk ratio using probability-weighted category counts, estimate a pre-post specification around the announcement, and aggregate the data to the channel-week level using the share of anti-war posts as the outcome. Across these specifications, channels in regions with greater pre-existing death-risk disparities exhibit larger increases in anti-war content after the decree. See Appendix B.4 for details.

The Telegram evidence complements the protest and survey results. Protest activity captures a short-term high-risk behavioral response. Survey responses show greater anger and lower leadership approval among ethnic minorities in high-disparity regions. The

Telegram data show that this anti-war signal also appears in local public communication. Because the Telegram data do not identify speakers' ethnicity and are limited to surviving public channels, I treat this evidence as supplementary.

Alternative Explanations

Two other explanations could produce similar patterns. The first is that the decree mattered because it revealed military setbacks, not because it made the state responsible for unequal losses. The decree came shortly after Ukraine's Kharkiv counteroffensive of September 6 to 12, 2022 (Khurshudyan et al. 2022), and the announcement itself may have informed citizens that the war was going badly. Because the event studies show no anticipation effects, the Kharkiv setback alone is unlikely to explain responses that vary with disparity. The announcement may still have conveyed both kinds of information, and the observational data cannot fully separate them.

The second explanation is that high pre-mobilization disparity represents regions where mobilization was more intense or more targeted at minorities. In such scenario, the post-decree responses would reflect who was mobilized, not how earlier losses were interpreted. Official data on mobilization by region and ethnicity are not published, so I use fatalities identified as mobilized soldiers as an indirect measure and regress two region-level outcomes on the pre-decree DRR. The first is the share of post-decree fatalities identified as mobilized. The second is the gap in post-decree DRR between mobilized and non-mobilized fatalities. Both relationships would be positive if the alternative explanation were correct. The first is indistinguishable from zero and the second is negative (Table **??** in Appendix), so neither indicates more intense or minority-targeted mobilization in high-disparity regions. Mobilized fatalities are an imperfect proxy for mobilization itself, but they do not show the predicted pattern.

6 Discussion and Conclusion

When do interstate wars unite domestic society, and when do they divide it? Evidence from Russia's home front following the 2022 invasion of Ukraine shows that the answer depends partly on how wartime deaths are distributed across politically salient groups. After the September mobilization announcement, regions with greater pre-existing disparities in wartime death experienced larger increases in anti-war protest and anti-war content on Telegram. Survey respondents from ethnic minorities in these regions also reported greater anger and lower approval of the national leadership, while ethnic Russians living in the same regions showed little comparable response. These patterns remain after accounting for the overall regional death rate and the group-specific death rates of ethnic Russians and non-Russian minorities. Together, the findings are consistent with the argument that state-imposed mobilization made pre-existing disparities in wartime mortality politically salient, leading disproportionately burdened groups to evaluate their sacrifice against that of ethnic Russians and weakening their support for the leadership and the war.

These findings qualify a long tradition that treats war as a nation- and state-building device. In this account, mass mobilization, conscription, and shared sacrifice expand state capacity and forge national identity (e.g., Sambanis and Lee 2026; Tilly 1985; 1994; Wimmer 2018), an argument that largely assumes the burdens of war are broadly shared. The evidence here suggests that the effect depends on how those burdens are distributed. Conscription may bind citizens to the state when sacrifice is widely shared, but may erode that bond when sacrifice is visibly concentrated on a marginalized group. Wartime social cohesion is therefore linked to the broader problem of how multiethnic states sustain the domestic extraction required to fight wars abroad (Levi 1997; Rosen 1996; Tilly 1985).

The findings also extend research on wartime casualties. Casualty sensitivity is commonly treated as a national or local response to accumulating losses (e.g., Althaus, Bramlett and Gimpel 2012; Gartner 2008; Gelpi, Feaver and Reifler 2009; Kriner and Shen 2010; 2012; Mueller 1973). The evidence here suggests that responses to casualties are also relational,

because losses can enter politics through comparison across groups. This helps explain why the response is concentrated among minorities rather than ethnic Russians living in the same regions, a pattern consistent with group-referent relative deprivation (e.g., Gurr 1970; Smith et al. 2012; Walker and Pettigrew 1984). Work on horizontal inequality treats group disparities as a cause of civil conflict (e.g., Cederman, Gleditsch and Buhaug 2013; Cederman, Weidmann and Gleditsch 2011; Stewart 2008). The findings here suggest that war against a foreign enemy can generate those disparities, by distributing death unevenly across groups.

The findings further speak to the leader's side of this calculation. Concentrating losses outside the political core may initially reduce the domestic costs of war for national leaders. Such an advantage weakens when a mobilization decree makes the state visibly responsible for allocating future risk, leaving leaders to choose between broadening extraction to include core constituencies and bearing the political costs of continued reliance on marginalized groups (e.g., Allen 2008; Valentino, Huth and Croco 2010). The fiscal side of war shows the same pattern, since governments can finance conflict through debt rather than taxation to keep its costs from being felt (Kreps 2018). Neither arrangement removes the cost. When manpower needs outrun what recruitment can supply at an acceptable price, the leadership must compel service, and compulsion destroys the appearance of individual choice that payment preserved.

Russia's conduct since 2022 illustrates this trade-off. As battlefield losses have continued, the state has relied heavily on financial incentives instead of another general call-up, offering increasingly large enlistment bonuses that matter most in poorer and more peripheral regions. Some of the highest payments have come from impoverished ethnic republics, including Mari El, which has offered several times the federal minimum (Kluge 2025; The Moscow Times 2026). Paid recruitment is not only a fiscal strategy, but it can also preserve an interpretation that compulsory mobilization disrupted. A recruit who accepts a substantial bonus appears to have chosen the risks of combat, even when economic necessity forced them to do so.

This strategy is costly and may face diminishing returns. Recruitment reportedly fell to

roughly 800 contract signings per day in early 2026 despite high financial incentives (Kluge 2026). The leadership therefore faces a choice between two costs. Renewed mobilization could ease manpower pressures, but it would again make the state visibly responsible for deciding which regions and social groups bear the risks of death. This paper does not show that concern about these political costs caused the leadership to defer another call-up. However, it shows why conscription may carry political costs that paid recruitment can partly obscure, especially where economic disadvantage and political marginalization overlap.

Several limitations remain. The war burden measure relies on a name-based classifier (Bessudnov et al. 2023) applied to fatality records compiled by an independent media outlet (Mediazona 2025). Deaths from marginalized communities may be underreported, which would make the observed disparity a lower bound. Several large minority populations, including Chuvash, Mordvin, and Udmurt communities, also use naming conventions that are largely indistinguishable from East Slavic ones. Their casualties are therefore assigned to the dominant group, which works against finding the asymmetry reported here. The measure also divides the population into ethnic Russians and non-Russians, setting aside differences among minority groups that occupy distinct positions in relation to the central state. Future research should examine whether unequal wartime burdens generate different political responses across minority communities, drawing where possible on sources that record ethnicity directly rather than inferring it from names.

The observed empirical patterns may also be understated because self-censorship can suppress survey answers and online expression in authoritarian settings (e.g., Kuran 1991). Evidence from list experiments suggests that stated support for Putin is largely genuine and cannot be attributed entirely to concealment (e.g., Chapkovski and Schaub 2022; Frye et al. 2017). However, these studies do not directly address group-specific grievance under the legal restrictions imposed after February 2022. The analysis therefore draws on several forms of evidence, including protest behavior, online communication, and survey measures

of anger and leadership approval, each of which is subject to different forms of selection and concealment.

The theory also assumes that members of the burdened group had some informal sense of their community's relative burden before mobilization made it a matter of open political attribution. I do not observe this awareness directly. No survey item measures whether respondents knew of the disparity before the decree, and the evidence for prior awareness is anecdotal. It comes from a small number of visible cases, and those who speak publicly or organize around ethnicity may differ from the typical member of their group. Future research would benefit from survey items that measure perceived, not just actual, relative burden, and from more complete casualty data, including systematic historical records that only states are able to collect.

The observational design cannot fully separate responses to unequal sacrifice from sensitivity to losses within one's own group. The results remain after controlling for the population-standardized death rates of ethnic Russians and minorities, which makes a purely own-group-loss explanation less likely. Still, marginalized minorities may respond strongly to deaths within their communities without explicitly comparing their losses with those of ethnic Russians. Future work should distinguish these mechanisms experimentally, varying casualty distributions and state responsibility independently.

How far the mechanism travels depends on the kind of burden a war imposes, the kind of war being fought, and how tightly the state controls what citizens learn about the losses. This study examines military fatalities, but civilian victimization, displacement, and destruction follow different distributional logics and may generate different attribution patterns. The Soviet experience in the Great Patriotic War illustrates how the other two conditions interact. Unequal sacrifice across republics and deported minorities did not prevent the war from becoming a foundation of Soviet integration (Rozenas, Talibova and Zhukov 2024; Weiner 2011), in a case where the state framed the war as existential and defensive while monopolizing the information environment. Wars are said to make nations. Whether they do

depends on whose sacrifices become visible, against whom they are compared, and whether the state is held responsible for their unequal distribution.

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Online Appendix

When War Divides: Unequal Sacrifice and Wartime Support in Russia

Contents

A Construction of War Burden Disparity **1**
- A.1 Application of Classifier by Bessudnov et al. (2023) . . . 1
- A.2 Regional Correlates of the Death Risk Ratio . . . 3

B Empirical Analyses **4**
- B.1 Discontinuity in War Burden Disparity at the Mobilization Decree . . . 4
- B.2 Mobilization Intensity and Pre-Decree Disparity . . . 4
- B.3 Anti-war Protests . . . 5
  - B.3.1 Pre-post Model . . . 5
  - B.3.2 Models Using the Probability-Weighted Death Risk Ratio . . . 7
  - B.3.3 Regression Table (Figure 2) . . . 11
- B.4 Gallup Poll . . . 13
  - B.4.1 Additional Outcomes . . . 13
  - B.4.2 Models Using the Probability-Weighted Death Risk Ratio . . . 17
  - B.4.3 Regression Table (Figure 3) . . . 18
- B.5 Telegram Posts . . . 20
  - B.5.1 Week-Channel Specification . . . 21
  - B.5.2 Models Using the Probability-Weighted Death Risk Ratio . . . 21
  - B.5.3 Regression Table (Figure 5) . . . 25

C Telegram Corpus Construction and Coding **27**
- C.1 Channel Selection . . . 27
- C.2 Two-Stage Coding of Anti-war Content . . . 27

A Construction of War Burden Disparity

A.1 Application of Classifier by Bessudnov et al. (2023)

To construct the regional measure of war burden disparity, I apply the pre-trained name-based classifier developed by Bessudnov et al. (2023) to the first and last names in the fatality records. The classifier predicts perceived ethnicity from personal names using a machine-learning model trained on *VKontakte* data. I use the authors' aggregated classification, which groups names into 15 categories and achieves an overall test-set accuracy of 0.92. Because the classifier predicts perceived ethnicity, not ethnic self-identification, the resulting classification should be understood as a proxy for ethnic origin as signaled by names.

For each fatality, I select the category with the highest predicted probability. I then collapse the predicted categories into two groups. The first is the East Slavic category used by the original classifier, which combines Russian, Belarusian, and Ukrainian names. The second includes all remaining non-East Slavic categories: Armenians, Azerbaijanis, Bashkirs and Tatars, Buryats, Chechens, Dagestanis and Ingush, Georgians, Jews, Kabardians, Adyghe, Karachays, Balkars and Ossetians, Kalmyks, Kazakhs and Kyrgyz, Moldovans, Tajiks and Uzbeks, Tuvans, and Yakuts. According to Bessudnov et al. (2023), the aggregated classifier performs particularly well for the combined East Slavic category, with reported precision of 0.94 and recall of 0.98. I refer to these two categories as Russian and non-Russian, respectively, for ease of presentation.

I aggregate classified fatalities by region and use deaths recorded between the beginning of the invasion and September 20, 2022, to construct the pre-decree death risk ratio described in the main text.

As a robustness check, I reestimate the analyses using a probability-weighted measure of recorded deaths in Appendix B.3.2, B.4.2, and B.5.2. Rather than assigning each fatality entirely to its most likely category, this measure assigns its raw predicted East Slavic probability to the Russian count and the remaining probability to the non-Russian count. I aggregate these expected counts by region and construct the death risk ratio in the same manner as the main measure. This approach retains uncertainty in the classifications.

The measure has several limitations. Some ethnic groups, including Chuvash, Mordvin, Udmurt, Mari, and Komi, are difficult to distinguish from ethnic Russians using names alone and are therefore not separately identified by the classifier. The authors also caution that name-based classification cannot recover individual ethnic self-identification and is better suited for estimating group distributions in large samples than for classifying particular individuals (Bessudnov et al. 2023). The regional disparity measure should therefore be interpreted as an aggregate proxy for the distribution of wartime fatalities rather than as a definitive classification of each individual fatality.

From the beginning of the invasion through June 30, 2023, the data contain 35,363 fatality records, of which 29,495 are classified as Russian and 5,868 as non-Russian. Thus, 16.6% of recorded fatalities are classified as non-Russian. Figure A.1 presents the monthly distribution of these fatalities from February 2022 through June 2023.

Figure A.2 compares the pre-mobilization logged death risk ratio, constructed cumulatively through September 20, 2022, with the regional non-Russian population share. The relationship is modestly negative ($r = -0.21$, $p = 0.053$). Regions with larger

non-Russian populations do not mechanically exhibit greater measured burden disparity. If anything, the estimated relationship runs in the opposite direction.

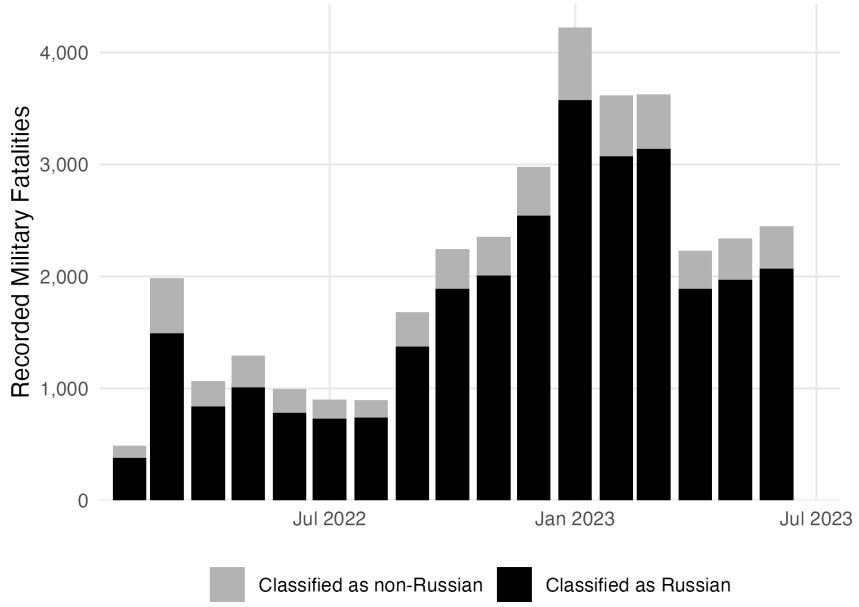


Figure A.1: Monthly Recorded Military Fatalities by Predicted Ethnic Category, February 2022–June 2023

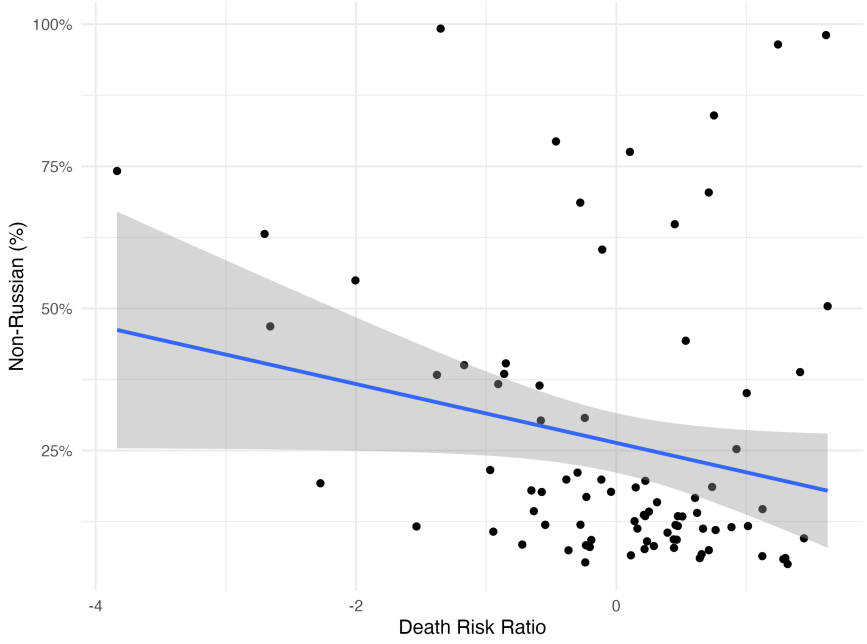


Figure A.2: Pre-Decree Death Risk Ratio and Non-Russian Population Share

A.2 Regional Correlates of the Death Risk Ratio

I also examine whether the pre-decree death risk ratio is associated with observable regional characteristics that could independently shape political responses to mobilization. Figure A.3 reports standardized bivariate associations between the death risk ratio and five regional characteristics, estimated using separate OLS regressions. Both variables are standardized within the complete-case sample for each comparison, making the coefficients equivalent to pairwise correlations. Following Ho et al. (2007), I focus on the magnitude of these associations rather than treating statistical significance as a criterion for balance. Horizontal bars report 95% confidence intervals obtained from 2,000 region-level bootstrap samples.

The Russian population share and prior cumulative death rate show the largest positive point estimates, whereas gross regional product, prior repression, and distance to Ukraine are more weakly associated with the measure. The first relationship may reflect the demographic composition underlying the construction of relative fatality rates, while the second suggests that measured disparity is somewhat greater in regions with a greater number of recorded losses overall. I therefore include Russian population share and prior cumulative fatalities as regional controls in the main controlled specifications. By contrast, I include gross regional product, prior repression, and distance to Ukraine only in extended robustness checks or in specifications where they are especially relevant to the outcome under study. These comparisons are descriptive and do not rule out confounding by unobserved regional characteristics.

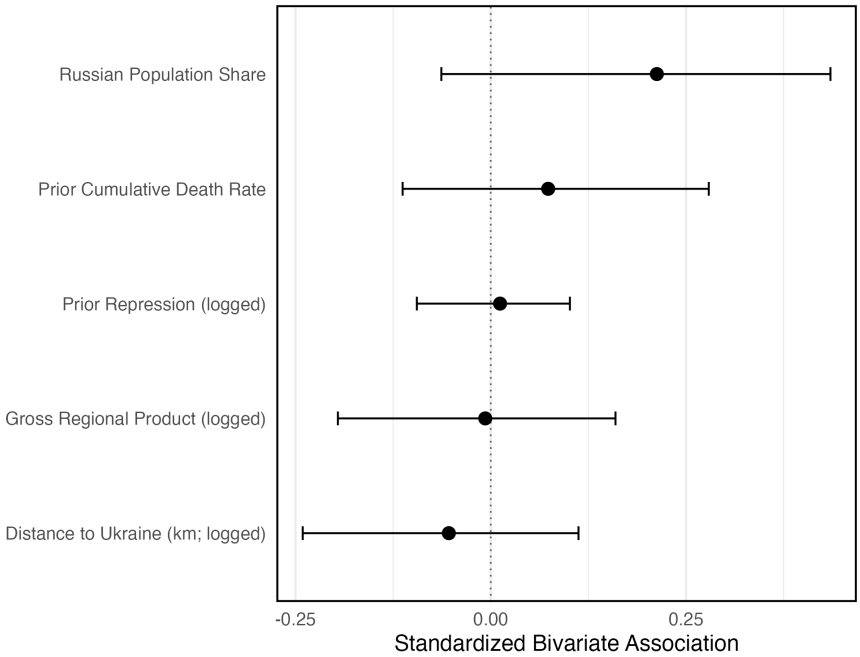


Figure A.3: Standardized Associations Between Regional Characteristics and the Pre-Decree Death Risk Ratio

B Empirical Analyses

B.1 Discontinuity in War Burden Disparity at the Mobilization Decree

The main analyses treat the September 21, 2022 mobilization decree as an attribution shock that made the state's role in allocating wartime risk more visible. This interpretation assumes that the announcement did not coincide with an immediate change in the regional distribution of military fatalities. I assess this possibility with a local discontinuity-in-time analysis centered on the mobilization announcement, using the method of Calonico, Cattaneo and Titiunik (2014).

For this exercise, I examine daily observations from the beginning of the war (February 24, 2022) through June 30, 2023. The outcome is the regional death risk ratio calculated separately for each day, and the running variable is the number of days relative to September 21. The models estimate local linear regressions on either side of the cutoff and report robust bias-corrected inference. I present results using triangular, Epanechnikov, and uniform kernels to assess sensitivity to alternative weighting schemes.

Table B.1 shows no detectable discontinuity in daily war burden disparity at the mobilization announcement. The estimated break ranges from 0.047 to 0.051 across kernels, with robust p-values between 0.785 and 0.797. The confidence intervals include zero in every specification.

These results provide no evidence that the mobilization decree immediately altered the daily distribution of fatalities across ethnic groups. They are therefore consistent with the interpretation that mobilization announcement increased the political salience and state attribution of an existing disparity, not immediately creating a new one. The exercise does not establish that the distribution of fatalities remained unchanged over the subsequent war period. Its narrower purpose is to show that the announcement did not coincide with a detectable immediate break in the daily disparity measure.

Table B.1: Discontinuity in War Burden Disparity at Mobilization Decree

Statistic	Triangular	Epanechnikov	Uniform
RD estimate	0.047	0.050	0.051
Robust SE	(0.183)	(0.184)	(0.187)
95% CI	[-0.312, 0.406]	[-0.311, 0.410]	[-0.315, 0.417]
Robust p-value	0.797	0.786	0.785
Bandwidth	118.71	107.34	85.62
Effective observations	19,238	17,901	14,246

B.2 Mobilization Intensity and Pre-Decree Disparity

One concern is that regions with higher pre-decree disparity may also have experienced more intense mobilization, or mobilization that fell more heavily on minorities. If so, the post-

decree response could reflect who was mobilized rather than how earlier wartime losses were interpreted. Because official data on mobilization by region and ethnicity are not available, I use fatalities identified as mobilized soldiers during the 180 days after the decree as an indirect measure. Mobilized status comes from Mediazona (2025), which records whether each identified fatality was a mobilized soldier.

I estimate two region-level regressions using standardized pre-decree DRR as the predictor. The first outcome is the share of post-decree fatalities identified as mobilized soldiers. The second is the difference in post-decree DRR between mobilized and non-mobilized fatalities. If responses in high-disparity regions were mainly driven by especially intense or minority-concentrated mobilization, both relationships should be positive. Table B.2 reports the results.

Table B.2: Pre-decree disparity and the composition of post-decree fatalities

	Mobilized share	DRR gap: mobilized − non-mobilized
Pre-decree disparity (z)	0.003	-0.209**
	(0.009)	(0.096)
Observations	82	83
R^2	0.00132	0.05437

*Heteroskedasticity-robust standard-errors in parentheses. ***: 0.01, **: 0.05, *: 0.1*

The coefficient for the mobilized share is close to zero and precisely estimated. High-disparity regions therefore do not have a larger share of post-decree fatalities coming from mobilized soldiers. The coefficient for the DRR gap is negative and statistically significant at the 5% level. In regions with higher pre-decree disparity, disparity among mobilized fatalities is smaller, not larger, than disparity among non-mobilized fatalities. These patterns do not support a simple account in which the results are driven by especially intense or minority-concentrated mobilization in regions that already had high disparity.

This exercise is admittedly indirect. Mobilized fatalities are not the same as the number of people mobilized. They capture only mobilized soldiers who later died and whose status could be identified, and differences in deployment and combat exposure may affect who appears in the fatality data. Misclassification of mobilized status may also add noise. The results therefore cannot show that mobilization intensity was unrelated to pre-decree disparity. They do, however, show that the post-decree fatality data do not display the pattern expected if high-disparity regions were subsequently exposed to especially intense or minority-concentrated mobilization.

B.3 Anti-war Protests

B.3.1 Pre-post Model

For a parsimonious summary, I also estimate a single pre-post specification for the protest analysis presented in Figure 2. I estimate the following model:

$$Y_{i,k} = \delta\left(\mathbb{1}\{k \geq 0\} \times DRR_i\right) + \boldsymbol{\gamma}^\top\left(\mathbb{1}\{k \geq 0\} \times \mathbf{Z}_i\right) + \alpha_i + \lambda_k + \varepsilon_{i,k}, \tag{1}$$

where the coefficient δ summarizes the average post-decree change in the association between war burden disparity and anti-war protest, and $\mathbf{Z}_i$ denotes a set of pre-decree covariates. I cluster standard errors by federal subjects.

Table B.3 reports the findings. Across both models, higher pre-mobilization decree disparity is a significant predictor of increased protest activity in the post-decree period.

Table B.3: War Burden Disparity and Anti-War Protest Around Decree (Pre-post)

	Pre-post Model	Full Controls	Absolute Rates
Post × Death Risk Ratio	0.052*	0.052*	0.058**
	(0.022)	(0.020)	(0.020)
Post × Russian Population Share	−0.264*	−0.290**	−0.444**
	(0.103)	(0.094)	(0.146)
Post × Prior Anti-War Protest	0.090**	0.065*	0.073*
	(0.033)	(0.026)	(0.029)
Post × Death Rate	5.569	9.842*	
	(4.477)	(4.781)	
Post × Minority Death Rate			−0.105
			(0.159)
Post × Russian Death Rate			0.852
			(0.980)
Post × Gross Regional Product		0.057*	0.043
		(0.028)	(0.027)
Post × Distance to Ukraine		−0.013	−0.005
		(0.025)	(0.025)
Post × Prior Repression		−0.100	−0.087
		(0.152)	(0.155)
Observations	498	492	492
R^2	0.451	0.457	0.455
Region FEs	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes
SEs Clustered by Region	Yes	Yes	Yes

Dynamic specifications report two-week periods from $t = -4$ to $t = 1$, with $t = -1$ as the omitted reference period. $+\ p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

B.3.2 Models Using the Probability-Weighted Death Risk Ratio

As a robustness check, I reestimate the pre-post and dynamic models using the probability-weighted death risk ratio in Tables B.4 and B.5. The results leave the substantive conclusion unchanged. The pre-post association remains positive, while the dynamic estimates show little evidence of differential trends before the decree and a pronounced increase immediately afterward. Joint tests fail to reject the null that the three pre-decree coefficients are zero in both the parsimonious specification ($p = 0.390$) and the extended specification ($p = 0.388$). The relationship between preexisting war burden disparity and anti-war protest therefore does not depend on assigning each fatality entirely to a single predicted ethnic category.

Table B.4: War Burden Disparity and Anti-War Protest Around Decree (Pre-post; (Probability Weighted Measure)

	Pre-post Model	Full Controls	Absolute Rates
Post × Death Risk Ratio	0.063*	0.062**	0.068**
	(0.024)	(0.022)	(0.023)
Post × Russian Population Share	−0.362**	−0.386***	−0.508***
	(0.114)	(0.103)	(0.147)
Post × Prior Anti-War Protest	0.092**	0.067*	0.073*
	(0.033)	(0.026)	(0.029)
Post × Death Rate	4.766	8.939+	
	(4.551)	(4.869)	
Post × Minority Death Rate			−0.093
			(0.160)
Post × Russian Death Rate			0.629
			(1.080)
Post × Gross Regional Product		0.057*	0.041
		(0.027)	(0.027)
Post × Distance to Ukraine		−0.010	−0.003
		(0.025)	(0.025)
Post × Prior Repression		−0.096	−0.082
		(0.155)	(0.158)
Observations	498	492	492
R^2	0.451	0.457	0.455
Region FEs	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes
SEs Clustered by Region	Yes	Yes	Yes

Dynamic specifications report two-week periods from $t = -4$ to $t = 1$, with $t = -1$ as the omitted reference period. $+ \; p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.5: War Burden Disparity and Anti-War Protest Around Decree (Dynamic; Probability Weighted Measure)

	Model 1	Model 2	Model 3
$t = -4 \times$ Death Risk Ratio	0.020	0.020	0.020
	(0.017)	(0.017)	(0.017)
$t = -3 \times$ Death Risk Ratio	0.004	0.004	0.004
	(0.004)	(0.004)	(0.004)
$t = -2 \times$ Death Risk Ratio	0.016	0.016	0.016
	(0.013)	(0.013)	(0.013)
$t = 0 \times$ Death Risk Ratio	0.104*	0.105*	0.111*
	(0.044)	(0.041)	(0.043)
$t = 1 \times$ Death Risk Ratio	0.040*	0.039*	0.045*
	(0.017)	(0.017)	(0.018)
Post $\times$ Russian Population Share	−0.362**	−0.386***	−0.508***
	(0.115)	(0.103)	(0.147)
Post $\times$ Prior Anti-War Protest	0.092**	0.067*	0.073*
	(0.034)	(0.027)	(0.029)
Post $\times$ Death Rate	4.766	8.939+	
	(4.570)	(4.890)	
Post $\times$ Minority Death Rate			−0.093
			(0.161)
Post $\times$ Russian Death Rate			0.629
			(1.085)
Post $\times$ Gross Regional Product		0.057*	0.041
		(0.027)	(0.027)
Post $\times$ Distance to Ukraine		−0.010	−0.003
		(0.025)	(0.025)
Post $\times$ Prior Repression		−0.096	−0.082
		(0.156)	(0.159)
Observations	498	492	492
R^2	0.456	0.462	0.459
Joint pre-trend test	1.01 (p = 0.390)	1.01 (p = 0.388)	1.01 (p = 0.389)
Region FEs	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes

Continued on next page

Table B.5: War Burden Disparity and Anti-War Protest Around Decree (Dynamic; Probability Weighted Measure) (Continued)

SEs Clustered by Region	Yes	Yes	Yes

Dynamic specifications report two-week periods from $t = -4$ to $t = 1$, with $t = -1$ as the omitted reference period. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

B.3.3 Regression Table (Figure 2)

Table B.6: War Burden Disparity and Anti-War Protest Around Decree (Dynamic)

	Model 1	Model 2	Model 3
$t = -4 \times$ Death Risk Ratio	0.012	0.012	0.012
	(0.018)	(0.018)	(0.018)
$t = -3 \times$ Death Risk Ratio	0.002	0.003	0.003
	(0.003)	(0.003)	(0.003)
$t = -2 \times$ Death Risk Ratio	0.010	0.010	0.010
	(0.009)	(0.009)	(0.009)
$t = 0 \times$ Death Risk Ratio	0.104*	0.104*	0.110*
	(0.044)	(0.042)	(0.042)
$t = 1 \times$ Death Risk Ratio	0.013	0.012	0.018
	(0.011)	(0.012)	(0.011)
Post $\times$ Russian Population Share	−0.264*	−0.290**	−0.444**
	(0.104)	(0.094)	(0.147)
Post $\times$ Prior Anti-War Protest	0.090**	0.065*	0.073*
	(0.033)	(0.027)	(0.029)
Post $\times$ Death Rate	5.569	9.842*	
	(4.496)	(4.801)	
Post $\times$ Minority Death Rate			−0.105
			(0.160)
Post $\times$ Russian Death Rate			0.852
			(0.984)
Post $\times$ Gross Regional Product		0.057*	0.043
		(0.028)	(0.027)
Post $\times$ Distance to Ukraine		−0.013	−0.005
		(0.025)	(0.025)
Post $\times$ Prior Repression		−0.100	−0.087
		(0.153)	(0.156)
Observations	498	492	492
R^2	0.459	0.466	0.463
Joint pre-trend test	0.64 (p = 0.590)	0.64 (p = 0.593)	0.63 (p = 0.594)

Continued on next page

Table B.6: War Burden Disparity and Anti-War Protest Around Decree (Dynamic) (Continued)

Region FEs	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes
SEs Clustered by Region	Yes	Yes	Yes

Dynamic specifications report two-week periods from $t = -4$ to $t = 1$, with $t = -1$ as the omitted reference period. $+ \ p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B.4 Gallup Poll

B.4.1 Additional Outcomes

This section extends the Gallup analysis to additional measures of negative affect, support for the political leadership, and confidence in state institutions. I estimate the same weighted triple-interaction specification used in the main text, including region and interview-date fixed effects, individual demographic covariates, and post-decree interactions with the regional ethnic Russian population share and logged cumulative fatalities recorded before the decree. Standard errors are clustered by region. For each outcome, the figures report the post-decree association between the standardized pre-decree death risk ratio and the outcome separately for ethnic minorities and ethnic Russians, together with the difference between the two group-specific estimates.

Figure B.1 reports results for sadness, stress, and a standardized negative-affect index. The pattern closely parallels the anger results presented in the main text. Among ethnic minorities, greater pre-decree burden disparity is associated after the mobilization decree with higher sadness, greater stress, and more negative affect overall. The corresponding associations among ethnic Russians are substantially smaller. The estimated minority–Russian differences are positive across all three outcomes and remain statistically distinguishable from zero after adjustment for multiple testing. This pattern indicates that the association between burden disparity and negative affect is concentrated among the group bearing the disproportionate losses.

Figure B.2 examines confidence in the national government and the military, as well as indicators for item nonresponse on these questions. The group-specific estimates for reported confidence are less precise than those for negative affect. Nevertheless, the signs are consistent with lower institutional confidence among minorities in regions with greater burden disparity: the minority association is negative for both government and military confidence, while the corresponding Russian estimates are close to zero or positive. The minority–Russian difference is particularly pronounced for military confidence, although it does not remain statistically distinguishable from zero after adjustment for multiple testing.

The item-nonresponse outcomes reveal a clearer group difference. Among ethnic minorities, higher pre-decree burden disparity is associated after mobilization with a greater probability of not answering the government- and military-confidence questions. No comparable association appears among ethnic Russians. The minority–Russian difference for military-confidence nonresponse remains statistically distinguishable from zero after the FDR adjustment, whereas the corresponding difference for government-confidence nonresponse does not. These results should be interpreted cautiously because nonresponse may reflect several processes, including uncertainty, reluctance to express a politically sensitive view, or survey disengagement. Even so, they suggest that analyses limited to respondents providing substantive answers may understate the extent to which burden disparity shapes minorities' responses to questions about state institutions.

Figure B.3 presents alternative outcomes related to evaluations of the political leadership. The first outcome indicates nonresponse to the leader-approval question, while the second directly measures leader disapproval. Among ethnic minorities, greater pre-decree burden disparity is associated after mobilization order with both a higher

probability of not reporting an approval response and greater leader disapproval. The corresponding associations among ethnic Russians are close to zero or negative. The minority–Russian differences are statistically distinguishable from zero at the 10 percent level before adjustment but not after the FDR correction. These results therefore provide directionally consistent evidence that mobilization decree reduced political support among minorities in regions where they had borne a disproportionate share of wartime fatalities.

Finally, because the analysis examines multiple outcomes, Table B.7 reports both unadjusted and false-discovery-rate (FDR) adjusted p-values (Benjamini and Hochberg 1995) for the minority–Russian differences across all eleven outcomes presented in the main text and appendix. The differences for anger, approval, sadness, stress, the negative-affect index, and military-confidence nonresponse remain statistically significant at the 5% level after adjustment. The differences for leader-approval nonresponse, leader disapproval, government-confidence nonresponse, and military confidence remain significant at the 10% level, while the difference for government confidence does not.

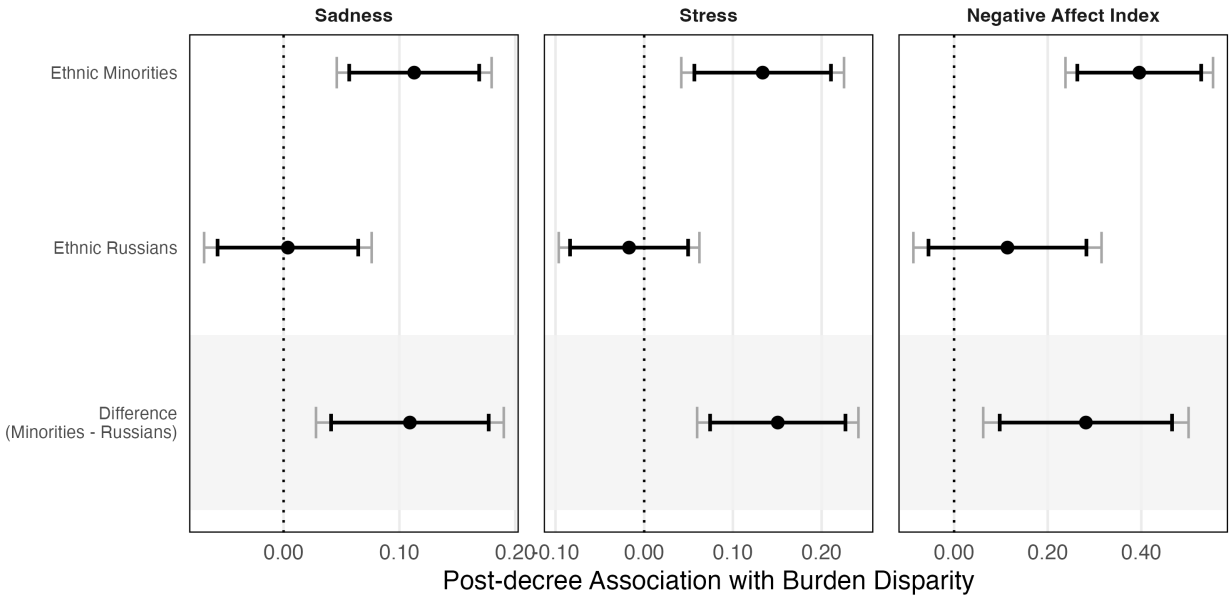


Figure B.1: War Burden Disparity and Additional Measures of Negative Affect

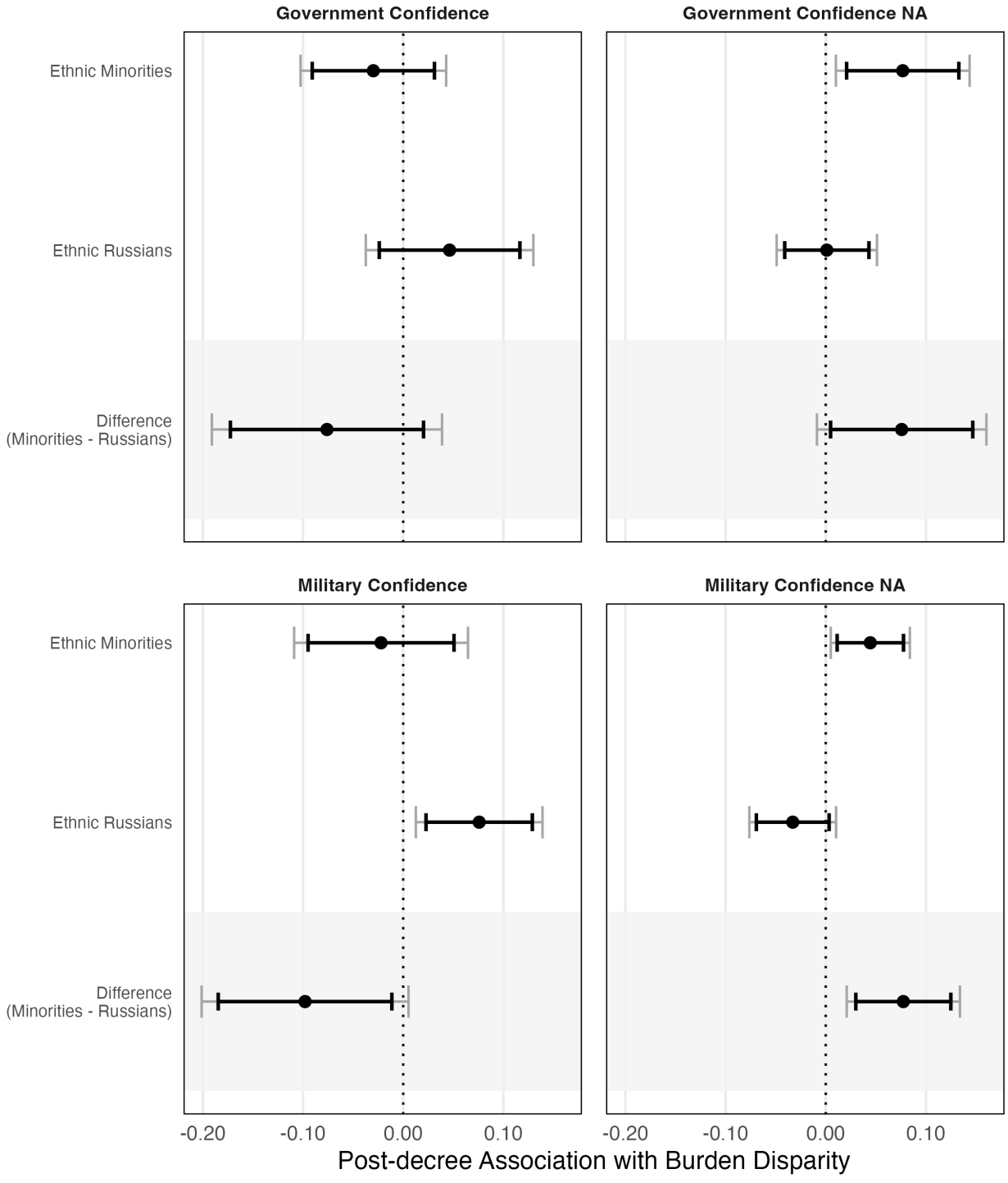


Figure B.2: War Burden Disparity and Confidence in Government and the Military

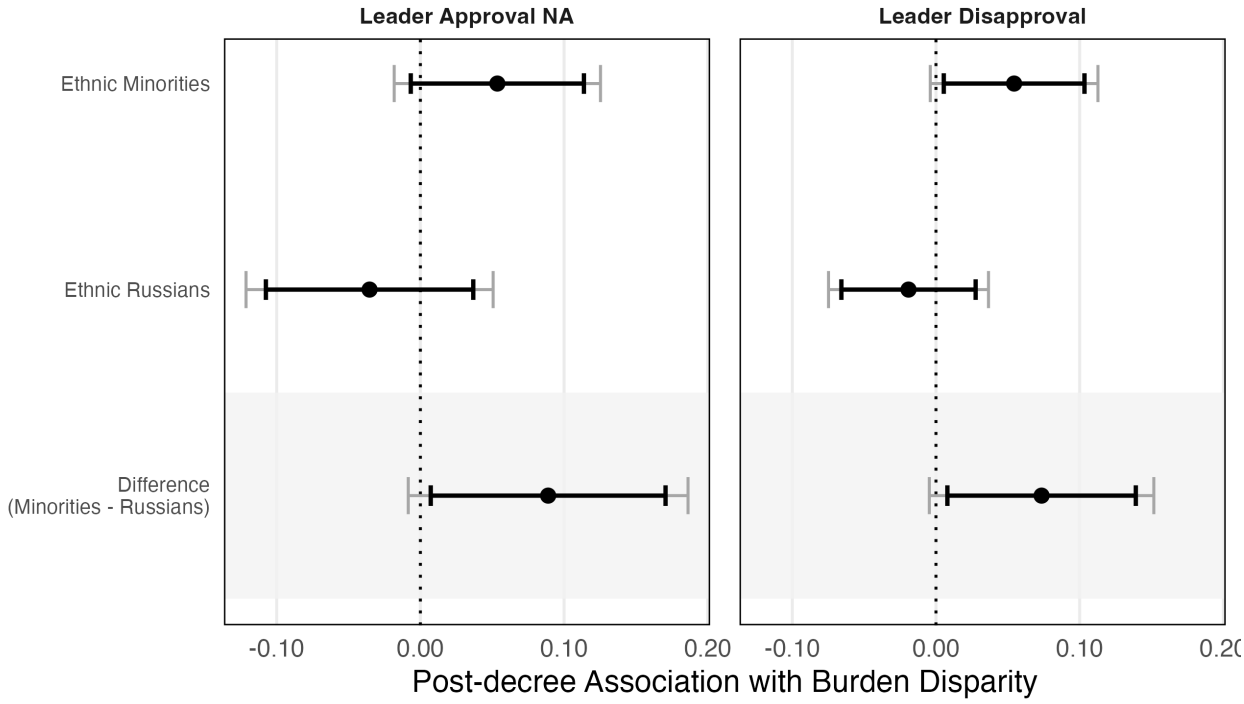


Figure B.3: Alternative Measures of Support for the Political Leadership

Table B.7: Differences Between Ethnic Minorities and Ethnic Russians Across Gallup Outcomes

Outcome	Minorities - Russians	Std. Error	Raw p-value	FDR-adjusted p-value
Anger	0.073	0.033	0.026	0.047
Approval	-0.162	0.058	0.005	0.023
Sadness	0.109	0.041	0.008	0.023
Stress	0.151	0.046	0.001	0.013
Negative Affect Index	0.282	0.112	0.012	0.026
Leader Approval NA	0.089	0.050	0.073	0.087
Leader Disapproval	0.073	0.040	0.065	0.087
Government Confidence	-0.076	0.059	0.194	0.194
Government Confidence NA	0.076	0.043	0.079	0.087
Military Confidence	-0.098	0.053	0.063	0.087
Military Confidence NA	0.077	0.029	0.007	0.023

B.4.2 Models Using the Probability-Weighted Death Risk Ratio

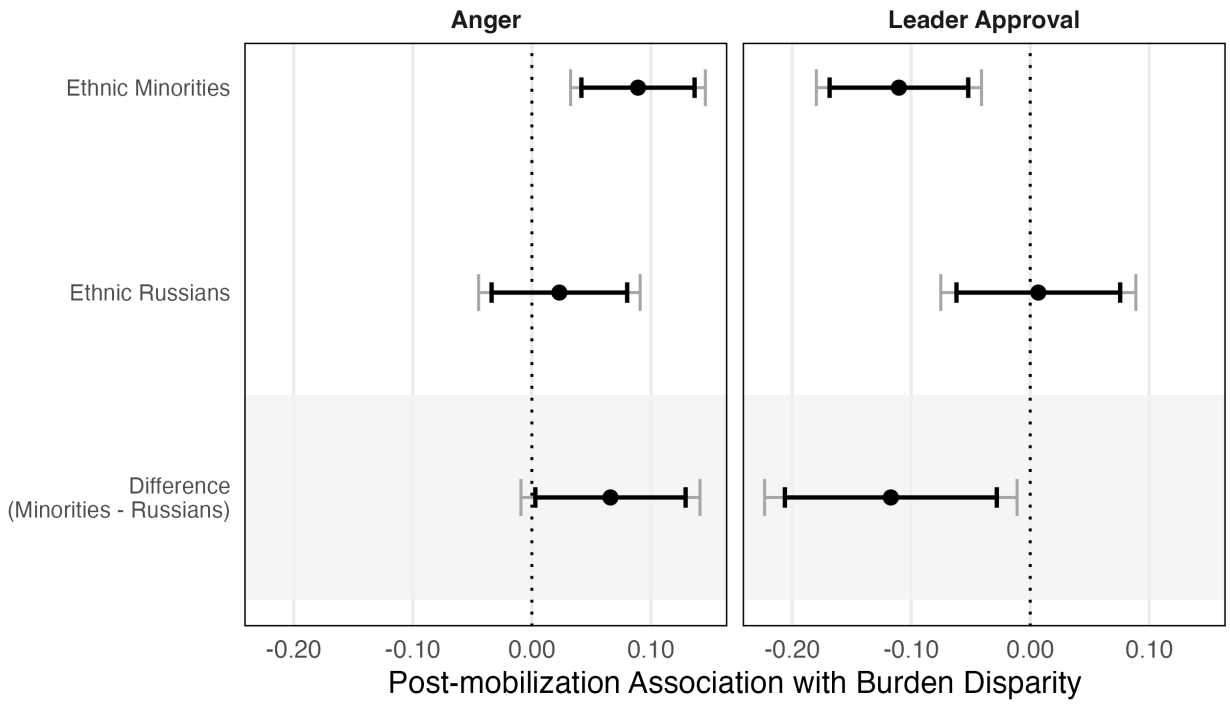


Figure B.4: Post-Mobilization Decree Association between Probability-Weighted Death Risk Ratio and Primary Survey Outcomes

B.4.3 Regression Table (Figure 3)

Table B.8: Post Decree Association between Death Risk Ratio and Primary Survey Outcomes (Figure 3)

	Anger	Leader Approval
Minority	−0.074+	0.060
	(0.042)	(0.101)
Burden disparity × Post-mobilization	0.022	0.054
	(0.041)	(0.045)
Burden disparity × Minority	−0.054*	0.173***
	(0.021)	(0.049)
Post-mobilization × Minority	0.085	−0.109
	(0.053)	(0.108)
Burden disparity × Post-mobilization × Minority	0.073*	−0.162**
	(0.033)	(0.058)
Observations	1888	1895
R^2	0.138	0.152
Adj. R^2	0.066	0.082
RMSE	0.332	0.453
Demographic Controls	Yes	Yes
Region FEs	Yes	Yes
Date FEs	Yes	Yes
Survey weights	Yes	Yes
SE Clustered by Region	Yes	Yes

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.9: Post Decree Association between Death Risk Ratio and Primary Survey Outcomes (Additional Region-level Controls)

	Anger	Leader Approval
Minority	−0.053	0.054
	(0.043)	(0.106)
Burden disparity × Post-mobilization	0.039	0.031
	(0.039)	(0.055)
Burden disparity × Minority	−0.046*	0.170**
	(0.019)	(0.051)
Post-mobilization × Minority	0.055	−0.108
	(0.060)	(0.122)
Burden disparity × Post-mobilization × Minority	0.063+	−0.159*
	(0.033)	(0.063)
Observations	1888	1895
R^2	0.139	0.153
Adj. R^2	0.066	0.081
RMSE	0.332	0.453
Demographic Controls	Yes	Yes
Region FEs	Yes	Yes
Date FEs	Yes	Yes
Regional Covs.	Yes	Yes
Survey weights	Yes	Yes
SE Clustered by Region	Yes	Yes

$+ \ p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B.5 Telegram Posts

Table B.10: War Burden Disparity and Anti-War Posts Around Decree

	Model 1	Model 2	Model 3
Post × Death Risk Ratio	0.002**	0.002**	0.002**
	(0.001)	(0.001)	(0.001)
Post × Russian Population Share	−0.014**	−0.015***	−0.017**
	(0.004)	(0.004)	(0.006)
Post × Death Rate	0.859	16.389	
	(13.894)	(21.152)	
Post × Minority Death Rate			0.156
			(0.886)
Post × Russian Death Rate			2.945+
			(1.564)
Post × Prior Repression		0.000	0.000
		(0.001)	(0.001)
Post × Distance to Ukraine		0.000	0.000
		(0.000)	(0.000)
Post × Gross Regional Product		0.001	0.001+
		(0.001)	(0.001)
Reply		0.001	0.001
		(0.001)	(0.001)
Observations	467 936	455 026	455 026
R^2	0.091	0.091	0.091
Channel FEs	Yes	Yes	Yes
Date FEs	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes
SEs Clustered by Region	Yes	Yes	Yes

$+ p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

B.5.1 Week-Channel Specification

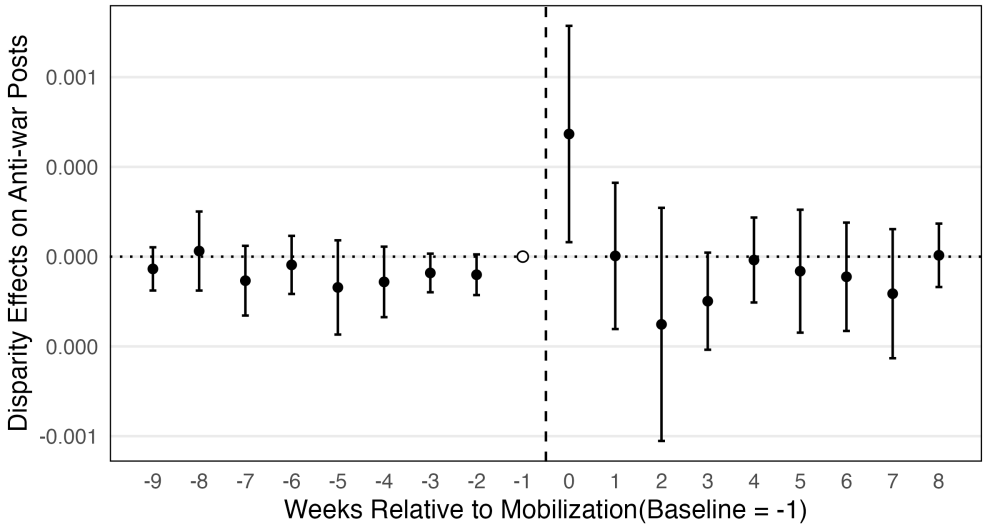


Figure B.5: War Burden Disparity and Antiwar Telegram Content Around Decree (Week-Channel)

B.5.2 Models Using the Probability-Weighted Death Risk Ratio

Table B.11: Dynamic Association Between War Burden Disparity and Anti-War Content Around Call-up Decree (Probability Weighted Measure)

	Model 1	Model 2	Model 3
$t = -9 \times$ Death Risk Ratio	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
$t = -8 \times$ Death Risk Ratio	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
$t = -7 \times$ Death Risk Ratio	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
$t = -6 \times$ Death Risk Ratio	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)
$t = -5 \times$ Death Risk Ratio	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)

Continued on next page

Table B.11: Dynamic Association Between War Burden Disparity and Anti-War Content Around Call-up Decree (Probability Weighted Measure) (Continued)

$t = -4 \times$ Death Risk Ratio	−0.001+	−0.001+	−0.001+
	(0.001)	(0.001)	(0.001)
$t = -3 \times$ Death Risk Ratio	−0.001+	−0.001+	−0.001+
	(0.000)	(0.000)	(0.000)
$t = -2 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
$t = 0 \times$ Death Risk Ratio	0.004**	0.004**	0.004**
	(0.001)	(0.001)	(0.001)
$t = 1 \times$ Death Risk Ratio	0.002*	0.001	0.001
	(0.001)	(0.001)	(0.001)
$t = 2 \times$ Death Risk Ratio	0.002*	0.001+	0.001
	(0.001)	(0.001)	(0.001)
$t = 3 \times$ Death Risk Ratio	0.002***	0.001**	0.001*
	(0.000)	(0.001)	(0.001)
$t = 4 \times$ Death Risk Ratio	0.002**	0.002**	0.002*
	(0.001)	(0.001)	(0.001)
$t = 5 \times$ Death Risk Ratio	0.002**	0.002*	0.002*
	(0.001)	(0.001)	(0.001)
$t = 6 \times$ Death Risk Ratio	0.002**	0.002*	0.002*
	(0.001)	(0.001)	(0.001)
$t = 7 \times$ Death Risk Ratio	0.002*	0.002*	0.002*
	(0.001)	(0.001)	(0.001)
$t = 8 \times$ Death Risk Ratio	0.003*	0.002*	0.002*
	(0.001)	(0.001)	(0.001)
Post × Russian Population Share	−0.017***	−0.018***	−0.019***
	(0.003)	(0.003)	(0.004)
Post × Death Rate	−6.744	5.849	
	(11.373)	(17.604)	
Post × Minority Death Rate			0.057
			(0.658)
Post × Russian Death Rate			1.053

Continued on next page

Table B.11: Dynamic Association Between War Burden Disparity and Anti-War Content Around Call-up Decree (Probability Weighted Measure) (Continued)

			(1.309)
Post × Prior Repression		0.000	0.000
		(0.001)	(0.001)
Post × Distance to Ukraine		0.000	0.000
		(0.000)	(0.000)
Post × Gross Regional Product		0.001	0.001
		(0.001)	(0.001)
Reply		0.001	0.001
		(0.001)	(0.001)
Observations	467 936	455 026	455 026
R^2	0.091	0.091	0.091
Pre-trend Wald statistic	1.07	1.06	1.07
Pre-trend p-value	0.380	0.386	0.381
Channel FEs	Yes	Yes	Yes
Date FEs	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes
SEs Clustered by Region	Yes	Yes	Yes

Dynamic specifications report weekly associations relative to mobilization, with $t = -1$ as the reference period. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B.12: War Burden Disparity and Anti-War Content Around Call-up Decree

	Model 1	Model 2	Model 3
Post × Death Risk Ratio	0.003***	0.003***	0.003***
	(0.001)	(0.001)	(0.001)
Post × Russian Population Share	−0.018***	−0.019***	−0.019***
	(0.004)	(0.004)	(0.005)
Post × Death Rate	−6.645	6.031	
	(11.507)	(17.877)	
Post × Minority Death Rate			0.070
			(0.671)
Post × Russian Death Rate			1.087
			(1.318)
Post × Prior Repression		0.000	0.000
		(0.001)	(0.001)
Post × Distance to Ukraine		0.000	0.000
		(0.000)	(0.000)
Post × Gross Regional Product		0.001	0.001
		(0.001)	(0.001)
Reply		0.001	0.001
		(0.001)	(0.001)
Observations	467 936	455 026	455 026
R^2	0.091	0.091	0.091
Channel FEs	Yes	Yes	Yes
Date FEs	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes
SEs Clustered by Region	Yes	Yes	Yes

$+ p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

B.5.3 Regression Table (Figure 5)

Table B.13: Dynamic Association Between War Burden Disparity and Anti-War Posts Ratio Around Decree

	Model 1	Model 2	Model 3
$t = -9 \times$ Death Risk Ratio	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)
$t = -8 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
$t = -7 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
$t = -6 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
$t = -5 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
$t = -4 \times$ Death Risk Ratio	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)
$t = -3 \times$ Death Risk Ratio	−0.001	−0.001	−0.001
	(0.000)	(0.000)	(0.000)
$t = -2 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
$t = 0 \times$ Death Risk Ratio	0.004*	0.004*	0.004*
	(0.002)	(0.001)	(0.001)
$t = 1 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
$t = 2 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
$t = 3 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
$t = 4 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
$t = 5 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
$t = 6 \times$ Death Risk Ratio	0.000	0.000	0.000

Continued on next page

Table B.13: Dynamic Association Between War Burden Disparity and Anti-War Posts Ratio Around Decree (Continued)

	(0.001)	(0.001)	(0.001)
$t = 7 \times$ Death Risk Ratio	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
$t = 8 \times$ Death Risk Ratio	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)
Post × Russian Population Share	−0.013***	−0.014***	−0.016**
	(0.004)	(0.004)	(0.005)
Post × Death Rate	0.722	15.889	
	(13.564)	(20.542)	
Post × Minority Death Rate			0.156
			(0.850)
Post × Russian Death Rate			2.739+
			(1.475)
Post × Prior Repression		0.000	0.000
		(0.001)	(0.001)
Post × Distance to Ukraine		0.000	0.000
		(0.000)	(0.000)
Post × Gross Regional Product		0.001	0.001+
		(0.001)	(0.001)
Reply		0.001	0.001
		(0.001)	(0.001)
Observations	467 936	455 026	455 026
R^2	0.091	0.092	0.092
Pre-trend Wald statistic	0.96	0.93	0.97
Pre-trend p-value	0.465	0.489	0.460
Channel FEs	Yes	Yes	Yes
Date FEs	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes
SEs Clustered by Region	Yes	Yes	Yes

Dynamic specifications report weekly associations relative to mobilization, with $t = -1$ as the reference period. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

C Telegram Corpus Construction and Coding

C.1 Channel Selection

Candidate channels were identified using federal-subject, city, town, and district names, as well as locally salient regional or ethnic terms. These searches generated a broad candidate pool, while inclusion in the analytic corpus was determined by each channel's content and geographic focus.

To enter the main sample, a channel must satisfy four criteria. First, it must be publicly accessible through a Telegram link at the time of collection. Second, it must have at least 100 subscribers or users during the fall 2025–spring 2026 collection window. Third, its title, handle, description, pinned material, or posting history must clearly indicate a stable local or regional orientation. Fourth, it must be active on both sides of the announcement, with at least five posts between January 1 and September 20, 2022 and at least five posts between September 21 and December 31, 2022.

I further restrict the sample to channels clearly identifiable as local or regional public forums. These include channels focused on local news, incidents, public safety, community affairs, local politics, public services, complaints, and other matters of shared concern to residents of a city, district, republic, oblast, krai, or federal subject. I exclude ambiguous channels from the main specification. This choice avoids relying on post-level judgments for channels whose local public status cannot be determined confidently from channel-level information.

I also exclude channels that fall outside the target corpus: official government and administrative channels; politician-centered channels; military, recruitment, veteran, or soldier-support channels; facility- or institution-specific channels; commercial, marketplace, jobs, real-estate, or advertising channels; and distant or diaspora-oriented channels. This procedure yields 467,936 posts from 456 channels across 77 regions.

C.2 Two-Stage Coding of Anti-war Content

Anti-war content is rare, and exhaustive hand-coding of 467,936 posts is infeasible. I therefore use a two-stage procedure: an automated screen applied to the full corpus, followed by hand coding of every flagged post. The screen serves as a permissive filter, and hand coding removes the false positives it admits.

First Stage: Automated Screen I classify Russian-language Telegram messages using OpenAI's `gpt-5.4-mini`, accessed through the `gabriel` package (Asirvatham, Mokski and Shleifer 2026) for Python on July 16, 2026, submitting each post or reply separately. The prompt appears in Listing 1.

Listing 1: Prompt Used to Classify Telegram Content

```
You are a political text classifier. The text is in Russian.
   Interpret it accordingly.

Classify the message using the definition below. Return only 0 or 1.
```

```
ANTI-WAR OR ANTI-MOBILIZATION EXPRESSION:

Code 1 only if the author or channel itself expresses, endorses, or
   favorably amplifies:
- opposition to Russia's war in Ukraine;
- opposition to the mobilization decree or conscription for the war;
- criticism of the government's war or mobilization policy from an
   oppositional perspective;
- support for anti-war protest, draft resistance, or refusal to
   participate.

Code 0 if the message:
- merely reports anti-war statements, protests, arrests, or draft
   resistance without endorsing them;
- condemns, mocks, or criticizes anti-war actors;
- criticizes the government for prosecuting the war too weakly or
   ineffectively;
- concerns unrelated protests, local grievances, public services,
   prices, corruption, or ordinary administrative disputes;
- reports hardships, casualties, or mobilization without expressing
   opposition;
- is ambiguous about the author's own position;
- concerns any other subject, including matters unrelated to the war
    or mobilization.

Text:
[POST]

OUTPUT:
0 or 1
```

The model returns a binary value for each message. Messages assigned 1 are treated as candidate anti-war content, and messages assigned 0 as not containing such content. The same model, prompt, and coding rule are applied uniformly to all posts and replies. The screen flags 1,315 posts.

I assess the screen against my own hand coding on a stratified sample of 500 posts, comprising 200 posts that the screen flagged and 300 it did not. Among flagged posts, 44.5 percent were confirmed as anti-war. Among unflagged posts, one of 300 contained anti-war content. The screen therefore flags a great deal that is not anti-war, and it also misses genuine cases, though with a single missed case in the sample the corpus-wide miss rate cannot be estimated precisely.

Second Stage: Manual Coding Given this error pattern, I hand-code all 1,315 flagged posts using a written protocol. A post is verified as anti-war when it conveys the author's or channel's opposition to, or grief about, the war or the mobilization. The stance must be attributable to the author or channel, although it may be expressed indirectly through

grief, irony, prayer, or lament. Positive anchors include explicit opposition, wishes that the war end, disavowal of the war as belonging to the leadership, denial of the war's religious legitimacy, support for anti-war protest or draft resistance, casualties framed as blameworthy losses, and irony directed at pro-war voices. Negative anchors include neutral reporting of protests or prosecutions, wartime logistics and assistance, pro-war or victory framing, legal or expert analysis, anti-regime content unrelated to the war, and general wishes for peace without a clear reference to the war. Boundary rules distinguish stance from topic, wishes that the war end from wishes that it be won, and neutral reporting from reporting accompanied by oppositional commentary. Of the 1,315 flagged posts, 586 were verified as anti-war, or 44.6 percent. The remainder enter the final measure as zeros, as do all posts the screen did not flag.

To assess the reliability of this coding, two independent coders classified a common set of 648 of the flagged posts under the same protocol, without access to my labels. They agreed with each other on 87.0 percent of cases (Cohen's $\kappa = 0.54$), and each reproduced my classification with high precision, at 0.97. The disagreement that remains concerns how much obliquely expressed opposition clears the threshold rather than what counts as anti-war content. Table C.1 reports accuracy, precision, recall, and F_1 for each coder against the coding used in the analysis.

Table C.1: Coder Agreement with the Coding Used in the Analysis

Metric	Coder 1	Coder 2
Accuracy	0.92	0.87
Precision	0.97	0.97
Recall	0.94	0.88
F_1	0.95	0.92

The measure therefore identifies anti-war content among posts the screen flagged. Posts the screen did not flag are not recovered, so the measure captures a conservative subset of anti-war expression, and missed posts enter the outcome as zeros. This under-detection attenuates estimated associations toward zero.

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