Electoral Effects of Spatial Proximity to Protests on Voting in Russia

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This paper examines how localized protests in authoritarian regimes impact electoral outcomes, focusing on Russia between the 2016 and 2021 Parliamentary elections. Using data from anti-corruption protests led by Alexei Navalny in 2017 and pension reform protests in 2018, I analyze the effects of protest proximity on the ruling party's vote share, voter turnout, and opposition support. The findings indicate that protests reduce both the ruling party's vote share and turnout in affected areas but do not significantly increase support for opposition parties. The ruling party's losses are primarily driven by lower turnout among its supporters, with protests in the ruling party strongholds and larger protests showing more pronounced effects. Demonstrations met with crackdown on protesters appear to have somewhat larger negative effect on both turnout and the United Russia vote share compared to protests without repressions, although the difference is not statistically significant. These findings align with prior research while extending its scope to a harsher authoritarian context and different protest agendas. Moreover, the analysis suggests that protests influence attitudes not necessarily by revealing new information but by signaling declining regime support.

Introduction

Ensuring mass compliance has always been a critical task for authoritarian leaders. While rulers of the past relied on the power of tradition and brute military force to keep the population at bay, dictators of the twentieth century weaponized the sprawling machinery of the modern state to build all-penetrating repressive apparatus and establish strict control over mass media. In the age of information, however, dictators that rule through fear and totalitarian propaganda give their way to a different kind of autocrats: those cultivating an image of competence and even democratic legitimacy though populism and manipulation of information (Guriev and Treisman 2020, 2022). For such new dictators, the main goal becomes mimicking democratic leaders that are able to provide economic prosperity, hence a limited use of overt violence. Among other things, this democratic masquerade necessitates a different approach to managing mass dissent (Robertson 2010). Protests activity is not prohibited officially, and demonstrations of public dissent can happen significantly more often and with significantly fewer risks for the participants than in traditional dictatorships of fear. At the same time, allowing even a very limited degree of freedom of assembly may have significant ramifications for dictators that build their legitimacy though manipulation of information. Even in democratic contexts, costly political actions, such as protests, can act as mechanisms for information aggregation and affect the beliefs of non-participants regarding the state of the world and the policies of the government (Lohmann 1994a); in information environment typical for authoritarian regimes, public displays of dissent may have an amplified effect and even set off a cascade effect undermining regime support, eventually leading the the complete collapse of the regime (Kuran 1991; Lohmann 1994b). But even if futile from the perspective of the regime change – as, for example, recent protests in Hong Kong, Venezuela, Belarus, and Iran – localized dissent can signal an overall growing discontent, policy incompetency, and, depending on the regime's response, authoritarian nature of the ruler, and therefore affect voters' behavior.

In this paper, I investigate how local protests in an authoritarian regime, specifically Russia, affect the ruling party's vote share, voter turnout, and support for opposition parties. Specifically, I use data on protest activity in Russia between the 2016 and the 2021 Parliamentary elections – namely anti-corruption protests orginized by Alexei Navalny in 2017 and protests against the increase on the retirement age in 2018 – to investigate whether localities near protest sites register lower shares of votes for the regime party than those farther away, as well as the effect on the turnout and vote shares of opposition parties. Additionally, I conduct a set of heterogeneity tests to see if the effects in question change conditional on (1) the ruling party pre-treatment support in the region, (2) the magnitude of a protest, and (3) the authorities' use of repression against the protesters. The results suggest that protests negatively affect both the ruling party's total vote share and voter turnout. However, they do not appear to increase support for opposition parties, as no effect is observed on their combined total vote share. The analysis indicates that the ruling party's decreased vote share can be mainly attributed to decreased turnout in affected areas. In other words, local protests may deter regime supporters more than they mobilize potential opposition supporters. The effects are significantly more pronounced in the United Russia strongholds, and for protests with a larger number of participants and for those met with arrests by authorities, though the latter difference is not statistically significant.

Theoretically, the paper contributes to the literature on attitudinal consequences of protests, and political behavior and attitudes in authoritarian regimes in general. The result lend further support to the findings by Tertytchnaya (2020). Similarly to this study, I document how anti-governmental protests demobilize the regime supportrs more than they mobilize pro-opposition voters. Importantly, while Tertytchnaya's study focused on the short-term effects of the 2011 Russian protests on reported vote choice in the 2012 Presidential election, the results presented in the current paper suggest that such effects can manifest even several years after the protests, within a significantly harsher authoritarian environment, and for protests with agendas markedly different from those of 2011–2012. Thus, I find that both the anti-corruption protests organized by Navalny and the protests against the increase in the retirement age, primarily led by the Communist Party of the Russian Federation (CPRF), produced essentially the same outcomes. Importantly, the pension protests revealed little about the content of the government's policy, as the issue had been of nationwide salience since the reform was announced by then Prime Minister Medvedev. Therefore, the findings provide tentative evidence that the mechanism through which protests influence bystanders' attitudes is not necessarily linked to their ability to reveal substantively new information about the regime or its leaders. Instead, it may to be related to their role in signaling declining regime support and solidifying voters' views (Pop-Eleches, Robertson, and Rosenfeld 2022). In addition to that, I find that protest have a significantly more pronounced effect in regions where United Russia is traditionally strong.

The second contribution is empirical. To my knowledge, it is the first study that analyzes electoral outcomes in Russia on the lowest possible level of aggregation, that is one the level of electoral precincts and their territories. Using the data from Open Streets Maps, I was able to link more than 3,500,000 buildings to polling stations and reconstruct geographical borders of more than 30,000 electoral precincts in 403 largest settlements in Russia. This allowed me to calculate several key variables, such as median real estate price within the territory of the precinct and the mean night light emission. Coupled with doubly robust difference-in-differences estimator proposed by Sant'Anna and Zhao (2020), this allowed me to plausibly account for potential violation of the parallel trends assumption.

This research builds on these insights by examining the dynamics of protests in authoritarian settings, where repression is more common, and the government propaganda often frames protests as organized from abroad and aimed at the destruction of the country, which could dampen any long-lasting effect on people's attitudes (Arnon, Edwards, and Li 2023). By focusing on protests that do not result in sweeping change, my study contributes to understanding indirect ways in which protest activity can influence political dynamics in authoritarian regimes, with implications for the long-term stability and adaptability of these systems.

The remainder of the paper is organized as follows. In the next two sections, I briefly engage with the literature on protests and information in authoritarian regimes. The third section introduces to the empirical context, while the fourth discusses the research design and data. The section five present main results, and the sixth concludes.

Political protests, regime stability and information in autocracies

Anti-government demonstrations in authoritarian regimes are often studied for their effects on large-scale political outcomes. Research on the macro-level consequences of protests typically examines why some protests succeed in overthrowing regimes while others fail (Chenoweth and Belgioioso 2019), why some lead to concessions while others are met with repression (Klein and Regan 2018; Mueller 2024; Yuen and Cheng 2017), which types of protests are most effective for democratization (Chenoweth and Schock 2015; Kim and Kroeger 2019), or under what conditions they can trigger coups (Apolte 2022; Gerling 2017). But what happens when protests fail to trigger an immediate change? As recent protests in Hong Kong, Venezuela, Belarus, and Iran remind us, this outcome is significantly more probable than the others, as in most cases political protests result in neither regime change nor even concessions (Carothers and Youngs 2015). In fact, opposition leaders rarely proclaim regime change as their aim when calling people out on the streets, yet they organize protests on a relatively frequent basis regardless.

Even if unsuccessful in achieving macro-level outcomes, anti-regime demonstrations can have less direct effects. For instance, Frye and Borisova (2019) show that post-electoral protests in Russia in 2011 increased trust in government. Using variation in survey interview timing, they found respondents interviewed after the protests reported higher trust in institutions. This counterintuitive result stemmed from the regime's unexpectedly measured response: protests were peaceful, participants were unharmed, and demands appeared to be acknowledged. However, such favorable surprises are rare in authoritarian regimes, where harassment of dissidents is the norm. All else being equal, it is reasonable to expect protests to exacerbate negative attitudes toward the regime. In African countries, for example, protests generally seem to lead to a decrease in trust in government and leaders, and even more so when they are harshly repressed (Sangnier and Zylberberg 2017). As Tertytchnaya and Lankina (2020) show, the protests of 2011-2012 in Russia increased support for the protesters' demands among wider segments of society: in the first weeks of the anti-regime demonstrations, respondents expressed more sympathy for the protesters' demands. In a separate paper, Tertytchnaya (2020) examines how anti-regime protests influence voter behavior. Focusing on Russia's 2011-2012 protest wave, she finds that while opposition protests reduce support for the ruling regime and decrease political engagement among its supporters, they do not increase support for opposition parties.

However, despite these few notable exceptions, there is comparatively little research on the local effects of protests on the political attitudes and behavior of ordinary citizens in authoritarian regimes. This gap is particularly surprising given the influential literature suggesting that protests can lead to the regime collapse by changing the attitudes of bystanders. Thus, the literature on the diffusion of protests emphasizes the importance of anti-government demonstrations for encouraging larger numbers of citizens to openly oppose the regime. According to Kuran (1991), in autocracies, people often falsify their preferences to align with socially acceptable norms due to the costs of expressing dissent. However, individuals vary in their tolerance for the psychological costs of suppression. When these costs surpass a certain threshold, they reveal their true preferences. Signs of discontent lead people to reassess the regime's support and the acceptability of dissent, altering the payoff of preference falsification. This triggers a chain reaction where others follow suit, initiating a "revolutionary bandwagon" that can culminate in a regime's collapse. As Lohmann (1994a) argues, costly political actions - signing petitions, demonstrations, riots, etc. - can serve as a vehicle for disseminating information, whether about the regime's popularity or the broader conditions affecting individuals' preferences and policy decisions. Such methods can prove to be especially effective in a society where information flows are strictly controlled by the government, as happened in the GDR in 1989 (Lohmann 1994b). Thus, mass exodus from GDR to FRG and anti-regime demonstrations in Leipzig revealed to broader segments of the population the extent of the general discontent with the communist government, which led to the cascade of other demonstrations and eventually translated into mass protest and regime change.

Both Kuran's and Lohmann's models offer valuable insights into the dynamics of mass dissent in authoritarian regimes and serve as key conceptual foundations for this paper. All the more important are the differences between the two accounts. In Kuran's model, signs of dissent prompt people to reveal their true preferences by altering their beliefs about others' opinions and their perceptions of repression risks; the preferences themselves are largely not affected, only the decision to reveal (or conceal) them. In contrast, Lohmann's model emphasizes how protests can reveal new information that leads bystanders to update their beliefs about various aspects of the world, such as overall regime support and the state of the economy; while repression reduces the amount of information revealed by suppressing the number of participants and occurrences, in itself it plays a secondary role in Lohmann's model. In short, Kuran focuses on fear and social conformity, while Lohmann puts more emphasis on cues and learning.

The main distinction with regards to empirical expectations pertains to the effect of repression: if we live in Kuran's world, protests met with arrests and detentions signal higher costs of aligning with opposition, potentially leading to more preference falsification; In Lohmann's, such response could expose the malign nature of the regime, potentially making people more likely to defect from it. In addition to that, the very occurrence of protests in a more repressive environment sends a stronger signal about the level of discontent, which aligns with a more rigorous model proposed by Kricheli, Livne, and Magaloni (2011). According to its main propositions, although "more repressive autocratic regimes are in principle more stable, in part because they are better able to deter civil opposition," … "protest that takes place in a more repressive autocratic regime reaches its maximum information-revealing potential and hence is more likely to cascade into a successful uprising" (Kricheli, Livne, and Magaloni 2011). Similarly, in a working paper dedicated to electoral consequences of protests in Chile and Bolivia, Castro and Retamal (2022) find that in districts where the protesters were repressed, the incumbent parties received lower shares of votes.

As for the other key factors that can moderate the effect of protests on political behavior and attitudes, the models developed by Kuran and Lohmann generate largely the same expectations. Thus, both models predict that:

- (1) More frequent and more attended protests should have a stronger effect.
- (2) Protests can have a larger impact when they are least expected, particularly when the perceived support for the regime is initially high.

It is worth noting that the mechanisms outline in both models are not mutually exclusive. In reality, different individuals may respond to protests in ways that align with either model. For example, some individuals might reveal their true preferences only when they perceive a shift in the risks of dissent or the social acceptability of opposing the regime. On the other hand, others might be influenced less by the perceived risks of repression and more by the new knowledge they gain from observing the protests. In practice, both mechanisms may operate simultaneously within the same society, with some people acting based on fear and conformity, while others respond to cues and learning. Together, these dynamics can interact to amplify the effects of protests, making them both a signal of discontent and a catalyst for belief updating. Thus, according to Tucker (2007), post-election protests in Serbia, Ukraine, and Georgia in the 2000s disseminated information about electoral fraud, as well as lowered the perceived costs of participating in anti-regime activities.

Drawing on Kuran's and Lohmann's theoretical models, and supported by Tertytchnaya's work on Russia, the literature suggests that protests in authoritarian regimes can influence public attitudes and behavior even in the absence of immediate regime change. Kuran's approach emphasizes how visible dissent reduces the perceived costs of expressing opposition, while Lohmann's model views protests as informational events that prompt individuals to update their beliefs about regime strength. Tertytchnaya's findings demonstrate that such shifts can alter support for the regime and the opposition, ultimately affecting electoral outcomes.

These insights imply that protest frequency, size, timing, and the intensity of repression can shape the extent to which protest signals translate into changes in citizens' political views and actions. This study will test these expectations by examining data on protests, elections, vote shares, and turnout in Russia, providing a clearer understanding of how protests influence political landscapes under authoritarian conditions.

In the next sections, I will discuss the context and outline the empirical design used to test these propositions.

Background

Protests in Russia

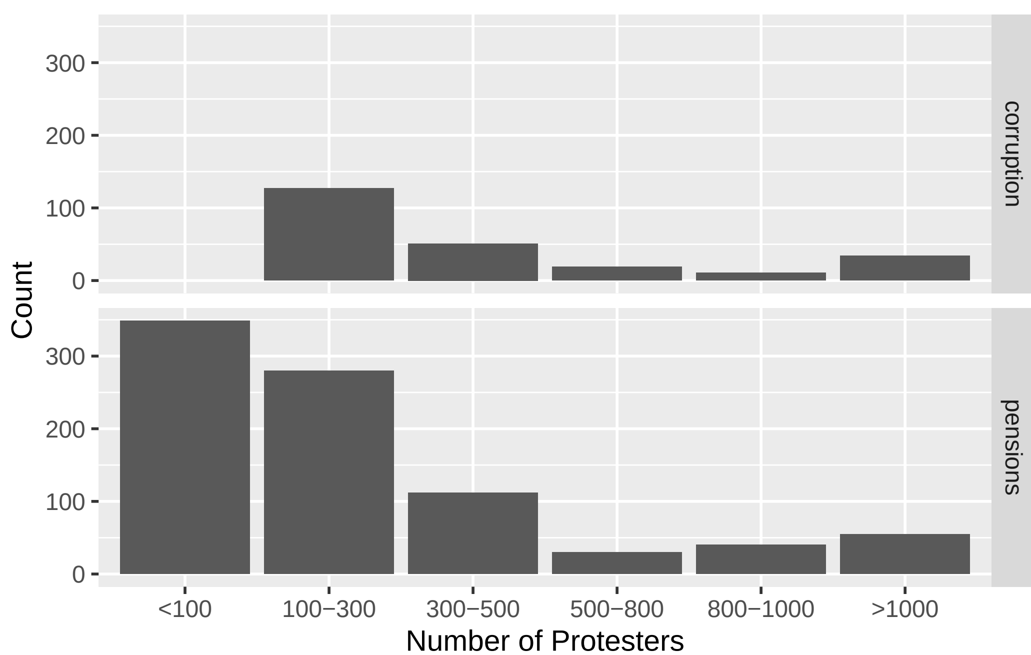
Before discussing the data, a few words about the protests in Russia are in order. Since Vladimir Putin was elected in 2000, there were four waves of protests. The first wave occurred in 2004 and was triggered by the monetization of social benefits. The government replaced fare-free public transit, medicines, and other services for disabled individuals, military personnel, and pensioners with disproportionately lower monetary compensations. This policy sparked widespread discontent, resulting in anti-government demonstrations, primarily in Moscow and Saint Petersburg. The second wave began in December 2011 following massive electoral fraud during the State Duma elections. These protests marked the first large-scale, explicitly political demonstrations since the 1990s, with up to 120,000 participants rallying in Moscow. Protesters demanded re-election, the release of political prisoners, and the resignation of the head of the Central Election Committee. Although the demonstrations continued into the spring of 2012, they were eventually suppressed by the regime.

In the current paper, I focus of the third wave of the protest that started on March 26, 2017, with anti-corruption protests organized by Alexei Navalny and his team across Russia. Following the 2018 presidential election, protests against the announced increase in the retirement age added to this wave. While Navalny's team played a significant role, the Communist Party of the Russian Federation (CPRF) organized the majority of these anti-reform demonstrations. In total, I have the data on 1108 protest events, including their precise location within a city, the number of participants and whether or not any of the participants was detained.

The only nationwide protests not included to the sample are the January 2021 protests that took place after Alexei Navalny's detention upon his return to Russia, and the April 2021 protests organized by Navalny's team in response to his imprisonment conditions. While these protests attracted thousands of participants, their exclusion is unlikely to significantly affect the results. First, their spatial distribution likely mirrors the 2017 protests, which were also organized by the Anti-Corruption Foundation (AFC) and were similarly unsanctioned by authorities. Second, if protests occurred in previously unaccounted locations, this would contaminate the treatment group and bias the results toward zero. Consequently, the analysis may underestimate the true effect, providing a conservative lower-bound estimate.

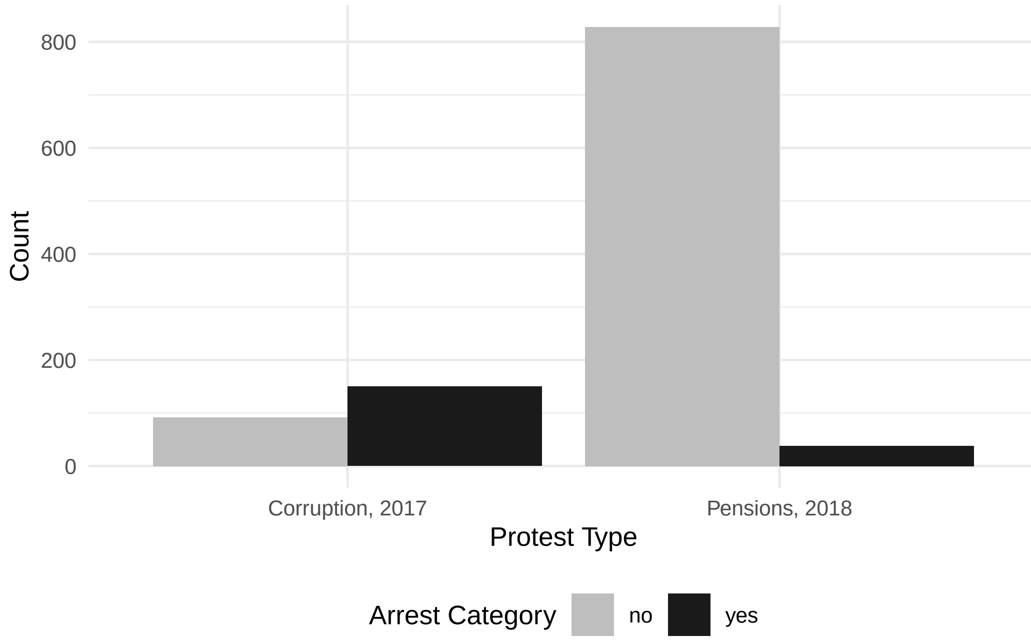
Several things are worth noting regarding the protests of 2017-2018. First, the majority of the 1108 protest events in the sample were organized in response to the announcement of the Pension Reform. Thus, out of 1108 protest event, 242 focused on corruption and took place in 2017, while 866 were held to oppose the pension reform in 2018. In both cases, there were substantial variations in the number of protesters. As shown in Figure 1, the Pension Reform protests attracted a significantly larger number of participants overall. Notably, protests with fewer than 100 participants accounted for over one-third of all Pension Reform protests.

Figure 1: The number of protesters across Anti-corruption and Pension reform protests



In addition, as reported in Figure 2 the authorities adopted a mixed approach: some protests were officially permitted or occurred without arrests, while others faced crackdowns. For example, out of 242 anti-corruption protests in the sample, 150 involved arrests of participants, while 92 did not. In contrast, protests against the increase in the retirement age saw significantly fewer crackdowns, with detentions reported in only 38 out of 866 cases. Importantly, the number of protesters is not correlated with whether authorities resorted to detentions or not.

Figure 2: Protests by Type and Authorities' Use of Repression



Second, when the authorities granted official permission for protests, the designated locations were often in hard-to-reach areas far from city centers. This extract from the regional newspaper's website illustrates the overall strategy of the authorities well:

> The protest rally in Tver on July 28 was organized by the local Communist Party. They planned to hold a public event on Saturday at Slava Square [the central square - I. F.]. For the rally to be sanctioned, it had to be approved by the city administration, which the organizers duly requested. In response, they received a "proposal letter," which did not explicitly ban the rally but relocated it from Slava Square to... the Cheminstitute [the district 9 km away from the city center - I. F.], citing that all significant public spaces in the city center and nearby areas would be occupied that day for the "Youth

> Matters” event. <…> Communist and deputy of the Tver Regional Legislative Assembly, Artyom Goncharov, posted on Facebook, stating that the rally in Tver would take place regardless:
>
> “The administration of Tver decided to suppress the protest against raising the retirement age by scheduling children’s events at all approved venues for public gatherings, except for the Cheminstitute. Well, we’ve decided to hold the event anyway, even if it has to be there.”

Because of this approach, the protests were not concentrated exclusively in central, potentially more affluent districts. Instead, they were distributed across localities with varying levels of wealth and centrality, creating a more balanced geographic spread than might be expected in a democratic context. While this likely reduced the number of participants and the potential city-wide impact of these protests, it also helps mitigate concerns about selection into treatment. In this case, selection into treatment could threaten the parallel trends assumption if protests were disproportionately held in areas with specific socioeconomic characteristics, such as higher wealth or greater political engagement, as these factors might lead to divergent pre-treatment trends in the outcome variable. By distributing protests more evenly across districts, the likelihood of such pre-existing differences driving the results is reduced, increasing confidence that any observed effects can be attributed to the protests rather than violations of the parallel trends assumption.

Third, the protests differed in their main organizers, agendas and the kind of information they could reveal. The anti-corruption protests were organized by the Anti-Corruption Foundation (ACF), led by Alexei Navalny. They began shortly after the release of a journalistic investigation by the ACF, in which Navalny and his team accused then-Prime Minister Dmitry Medvedev of controlling a network of ostensibly charitable foundations used to conceal ownership of luxurious villas, yachts, and other assets. In this regard, these protests could be seen as potential transmitters of information previously unknown to the general population. The 2018 pension reform protests, in their turn, began after Dmitry Medvedev, who was still a Prime Minister, announced the reform. Organized primarily by the Communist Party of the Russian Federation (CPRF) and other left-wing groups, these protests revealed little new information beyond the general public discontent with the reform itself.

Geographic boundaries of electoral precincts

A particular challenge in analyzing the local effects of protests is the absence of readily available data on the boundaries of electoral precinct territories. In Russia, the only readily available information is the location of polling stations. Individuals can vote only at their designated polling station, which is assigned based on their residential address[1]. While this data makes it possible to identify the buildings where votes are cast, it does not indicate which residential buildings are associated with each polling station. Since electoral territories vary in size and shape, understanding which protests might influence voting outcomes at a specific polling station requires knowing whether voters assigned to that station lived near the protest location. Without this information, it becomes impossible to determine the proximity of voters to protest events and, consequently, to analyze spatial effects of protests.

To address this issue, I reconstructed the boundaries of electoral precinct territories using data from OpenStreetMap and polling station-building associations for 2022, which were scraped from the Central Election Committee website and provided by the Anti-Corruption Foundation. Specifically, I linked over 3.5 million buildings to their respective polling stations and reconstructed the geographical boundaries of 30,000 electoral precincts. These boundaries reflect those used during the 2021 State Duma election across 403 of Russia's largest settlements.

The second empirical problem for this research is the difficulty in constructing a stable unit of observation, which is essential for methods like Difference-in-Differences. DiD relies on tracking the same observational units over time to isolate the causal effects of an intervention or event. Without stable units, changes in the structure or boundaries of the units themselves can confound the results, making it impossible to attribute observed changes to the intervention rather than to shifts in the units of analysis. Unfortunately, polling station IDs are not stable and can change from one election to another. For example, in cases of partial redistricting, a polling station №100 can be split into №100 and №101, causing all subsequent polling station IDs to shift. Thus, a polling station №101 in 2016 might become №102 in 2021. Polling station locations are also not fixed, making it difficult to identify units based on their coordinates or addresses, as polling stations

[1] It is possible to obtain a voting absentee certificate and register to vote at a different polling station, though this option is not commonly used and does not allow one to vote for single-member district candidates.

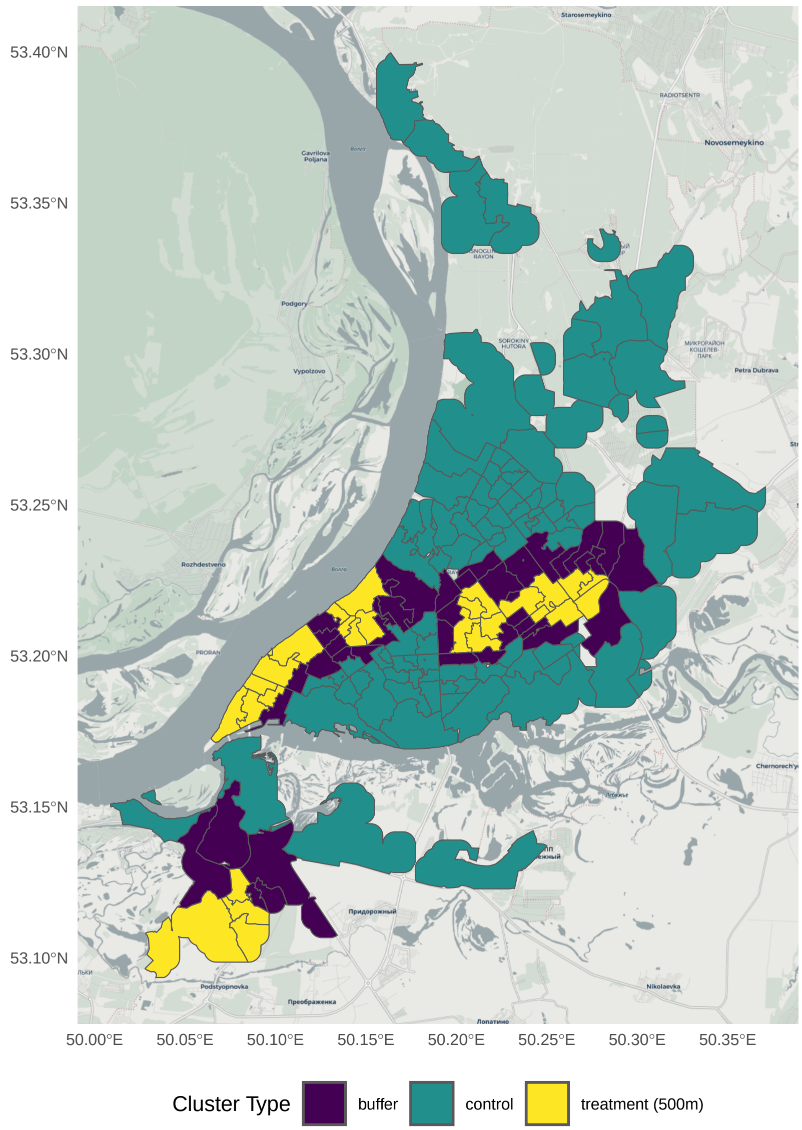
can relocate between elections. Finally, the boundaries of electoral precincts are also subject to change and can be modified between elections. This instability, combined with changes in precinct boundaries, necessitates a method that accounts for such variations.

To mitigate these issues, I cluster electoral precincts by grouping polling stations that are closely connected — either by being in the same building or through geographical proximity. As long as changes in polling station locations or precinct boundaries occur within the same cluster, the units remain stable for analysis. Thus, electoral precincts were clustered using the following criteria: (1) polling stations located in the same building were assigned to the same cluster, and (2) whenever possible, each cluster included at least two polling stations. This approach resulted in an average cluster size of 2.85 polling stations and provided a robust framework for mitigating the effects of changes in polling station IDs, locations, and boundaries.

The primary reason for clustering is to ensure that I can accurately determine voting patterns in specific territories, as my analysis relies on the geographic location of electoral precincts. Since the location of precincts can shift slightly between elections, even if the underlying territory remains the same, clustering precincts helps protect against these fluctuations. By grouping precincts into clusters, I also minimize the risk of distortions in my results caused by changes in precinct boundaries, as long as these changes occur within the same cluster. This method allows me to plausibly associate polling stations from 2016 with their counterparts from 2021 and ensures that the geographic integrity of the data is preserved. As a result, I am left with 10,333 polling station clusters in each election period, which serve as the core units of my analysis.

In all specifications, I compare the change in the outcome — total opposition vote share, United Russia vote share, or turnout — in the units within a specified threshold distance form the protest location against units farther away. Importantly, to mitigate potential spillover effects, I exclude poling station clusters that border treated unites. Otherwise, I risk contaminating the control group with observations that could be affected by protests. However, as reported in Appendix Section , the analysis remains robust when buffer clusters are included in the control group. The Figure 3 illustrates the resulting setup.

Figure 3: Treated, Untreated, and Buffer Clusters: Samara



Finally, I excluded clusters if they met any of the following criteria: (1) the number of polling stations within the cluster differed between the two election periods, (2) the polling station IDs did not match across the periods, or (3) the total number of voters assigned to the polling stations in the cluster varied by more than 20% between the elections. While only 50% of the cluster met this criterion, this allowed for the construction of the stable units of observations. This approach is justified because no decisions or outcomes are determined at the level of individual polling stations, eliminating concerns about potential strategic redistricting or manipulation. By focusing on clusters that meet the criteria, we ensure the construction of stable units of observation while minimizing the impact of administrative boundary changes. This method prioritizes data consistency across election periods without introducing noise related to precinct-level variations. As reported in Appendix Section , stable and unstable clusters are generally balanced with respect to the covariates used in the analysis.

Empirical strategy

Estimation

Using these data, along with official election results, I employ doubly robust difference-in-differences estimator proposed by (Sant'Anna and Zhao 2020) to compare the change in turnout, total opposition vote share, and the ruling party vote share between localities within a specified threshold and outside of it. I use the DR estimator because no data on the location of polling stations are available before 2014. This makes it impossible to interrogate the parallel trends assumption in a standard way, either by running placebo tests or by visualizing the trends. Instead, the DR estimator relies on the conditional parallel trends assumption, which requires that trends are parallel only after conditioning on observed covariates. This provides a more flexible and robust framework for causal inference.

The key advantage of this estimator lies in being "doubly robust", meaning it provides unbiased estimates of treatment effects if either the outcome model or the propensity score model is correctly specified (but not necessarily both). The general formula is:

$$\hat{\tau} = \frac{1}{n}\sum_{i=1}^{n}\left[\frac{Z_iY_i}{e(X_i)} - \frac{(1-Z_i)Y_i}{1-e(X_i)} + (1-Z_i)\hat{\mu}_1(X_i) + Z_i\hat{\mu}_0(X_i)\right],$$

where Z_i is the treatment indicator, Y_i is the outcome, $e(X_i)$ is the propensity score, and $\hat{mu}_1(X_i)$ and $\hat{mu}_0(X_i)$ are the predicted outcomes for the treated and control groups.

Following (**abadie2023should?**), I compute bootstrapped standard errors clustered on the level of the treatment assignment. In other words, all adjacent electoral precincts within the specified threshold (discussed below) are assigned the same cluster ID, which is then used to calculate the standard errors.

Outcome variables

The analysis focuses on three key outcome variables:

1. **Turnout**:
    - Measured as the percentage of registered voters who cast their ballots in a given cluster.
2. **Combined Total Vote Share of Opposition Parties**:
    - Calculated as the total vote share of all opposition parties combined, expressed as a percentage of the total number of registered voters in the cluster.
3. **Total Vote Share of United Russia**:
    - The vote share of United Russia, relative to the total number of registered voters in the cluster.

Measuring vote share relative to the total number of registered voters in the district, rather than relative to valid votes cast, provides a clearer and more comprehensive measure of a party's overall support. This approach is particularly advantageous in contexts where turnout may vary significantly across clusters or be influenced by the treatment. If vote shares were calculated only relative to valid votes, changes in turnout could confound the results by altering the denominator, making it difficult to disentangle whether observed effects are due to shifts in voter preferences, turnout dynamics, or both.

By using the total number of registered voters as the denominator, this measure captures changes in both the absolute level of support for a party and any turnout-related dynamics. For instance, a decline in United Russia's vote share relative to total registered voters could reflect either a reduction in their core support or an increase in abstention among their voters, both of which are relevant outcomes. Similarly, it ensures that the combined vote share of opposition parties is not artificially inflated or deflated by variations in turnout, providing a more stable and interpretable metric. This method thus allows for isolating the treatment effect on electoral outcomes without conflating it with turnout-related factors or shifts in the behavior of other parties' supporters.

These outcome variables provide insights into the effects on voter participation, support for opposition parties, and support for the ruling party.

Treatment variables

The treatment variables are defined based on proximity to protest sites, using three distinct formulations:

- 500-Meter Radius: Clusters located within 500 meters of a protest site are considered treated.
- 1000-Meter Radius: Clusters within 1000 meters of a protest site are classified as treated.
- Magnitude-Based Threshold: The treatment threshold is dynamically determined by the magnitude of the protest. For protests with up to 400 participants, the threshold increases linearly with the number of protesters. Beyond 400 participants, the threshold distance decays logarithmically. This approach results in a treatment threshold ranging between 1 and 600 meters.

For each distance threshold, clusters within the specified distance are assigned to the treatment group, neighboring clusters are placed in a buffer zone excluded from the analysis, and all remaining clusters are designated as the control group. This approach enables the analysis of the effects of proximity to protests on the defined outcome variables at varying spatial scales, while minimizing the risk of underestimating the effect due to spillovers between treated and neighboring districts.

Covariates

A key methodological concern when comparing places in the vicinity of protests and those farther away is the potential systematic differences between treated and untreated units, which may exhibit divergent trends in their support for the regime and the opposition. Protests, for instance, could be more likely to occur in central, wealthier areas, where patterns of support differ, and disillusionment with the regime tends to rise more rapidly compared to peripheral regions, where support for the regime remains more resilient. To address potential violations of the assumption of unconditional parallel trends, I include a comprehensive set of covariates that capture the economic, demographic, and geographic characteristics of the studied clusters, such as real estate prices, voter density, nightlight intensity, and geographic coordinates

The primary data source consists of over 1.8 million geo-referenced real estate advertisements obtained from Avito, a leading online classified advertisements platform. From this dataset, I extract two key metrics: the median price per square meter at the cluster level and the ratio of this median price to the city-wide average. The price ratio is particularly valuable as it highlights whether a cluster's real estate values exceed or fall below the city average, serving as a proxy for local economic conditions and desirability. In addition to real estate data, I incorporate nightlight intensity metrics derived from BlackMarble satellite imagery, which act as a proxy for economic activity and urban development, capturing fine-grained variations in urbanization and vibrancy across clusters.

To further account for demographic and spatial factors, I include voter density, calculated as the number of registered voters per unit area, to measure population concentration. Geographic coordinates, specifically latitude and longitude, are also incorporated to control for spatial patterns and regional variations that might influence outcomes. Together, these covariates allow for a detailed analysis of cluster-level differences, helping to mitigate potential violations of the parallel trends assumption and ensuring the robustness of the results.

Results

Effect of protests on turnout and support for political parties

Table 1 reports the main results for the effect on the turnout. Here, as well as in the following tables, coefficient estimate represent the difference in the change in the outcome of interest for treated and untreated units. In all specifications, the combined effects of the protests of both types are statistically significant, indicating a turnout decrease of 1.3 to 1.65 percentage points in the affected localities. When estimating the effects of the two types of protests separately, a substantial difference emerges: across various specifications of the treatment, Anti-Corruption protests lead to a turnout decrease that is, on average, twice as large as that associated with Pension Reform protests.

Table 1: Estimated difference between treated and untreated units in the turnout change (2021 vs. 2016 elections)

	Treatment					
	500m		1000m		Relative	
All Protests						
Estimate	-1.289***	-1.122**	-1.655***	-1.645***	-1.505***	-1.44**
SE	(0.495)	(0.561)	(0.406)	(0.483)	(0.567)	(0.656)
Obs.	8358	8264	8564	8472	8382	8280
Pension Reform						
Estimate	-0.84*	-0.575	-1.417***	-1.342***	-0.917	-0.724
SE	(0.509)	(0.554)	(0.401)	(0.473)	(0.592)	(0.597)
Obs.	8176	8082	8382	8290	8200	8098
Anti-Corruption						
Estimate	-1.675**	-1.76**	-1.885***	-1.989***	-2.569***	-3.01***
SE	(0.68)	(0.714)	(0.479)	(0.522)	(0.844)	(0.98)
Obs.	7796	7702	8002	7910	7820	7718
Covariates	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

However, as Figure 4 show, the 83% confidence intervals for the effects intersect in all but one case, providing weak evidence in favor of statistically significant differences.

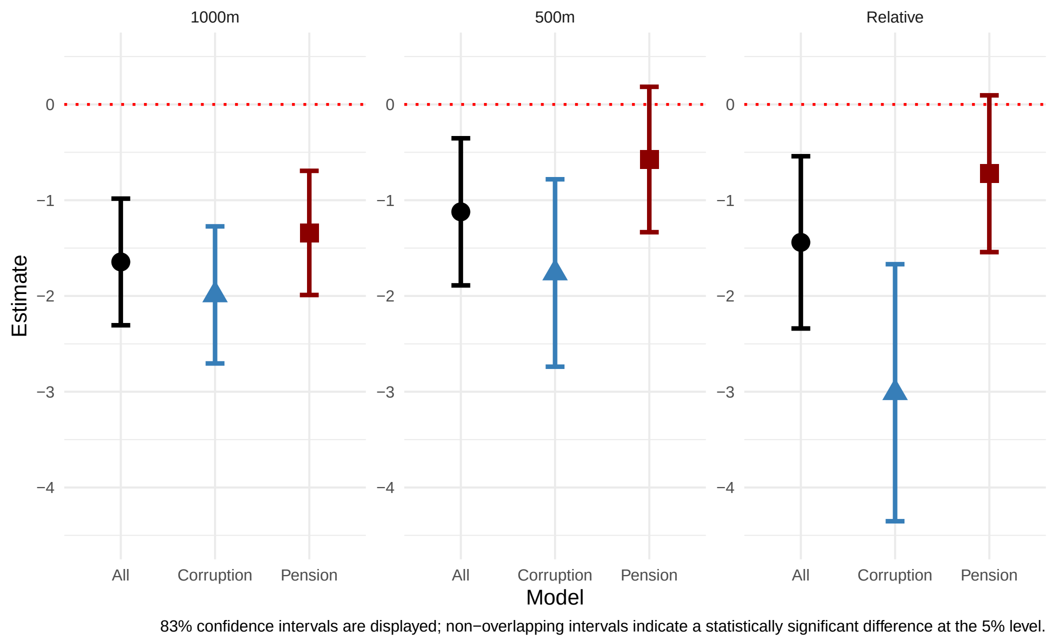


Figure 4: Estimated difference between treated and untreated units in the turnout change (2021 vs. 2016 elections)

The next question is whether the turnout decline impacted pro-regime voters, pro-opposition voters, or both. As Table 2 demonstrates, there is a consistent negative effect on United Russia's total vote share across all specifications when considering both types of protests combined. Notably, in all specifications the effect on United Russia's vote share closely mirrors the turnout decline. As before, Anti-Corruption protests have a more pronounced impact on voter behavior compared to Pension Reform protests.

Table 2: Estimated difference between treated and untreated units in the change of United Russia vote share (2021 vs. 2016 elections)

	Treatment					
	500m		1000m		Relative	
All Protests						
Estimate	-1.304***	-1.133**	-1.41***	-1.342***	-1.402***	-1.347**
SE	(0.475)	(0.501)	(0.372)	(0.414)	(0.511)	(0.556)
Obs.	8358	8264	8564	8472	8382	8280
Pension Reform						
Estimate	-0.899*	-0.663	-1.18***	-1.067**	-0.839	-0.698
SE	(0.467)	(0.455)	(0.367)	(0.429)	(0.564)	(0.56)
Obs.	8176	8082	8382	8290	8200	8098
Anti-Corruption						
Estimate	-1.79***	-1.748***	-1.635***	-1.632***	-2.466***	-2.679***
SE	(0.523)	(0.595)	(0.394)	(0.42)	(0.768)	(0.82)
Obs.	7796	7702	8002	7910	7820	7718
Covariates	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

As for the effect on the total opposition vote share, as Table 3 shows, we essentially observe no statistically significant differences between treated and untreated units. This goes in line with previous research, where opposition protests were shown to disengage pro-regime voters while neither making them more likely to switch to opposition, nor leading to increased mobilization on the sided of pro-opposition voters.

Table 3: Estimated difference between treated and untreated units in the change of total opposition vote share (2021 vs. 2016 elections)

	Treatment					
	500m		1000m		Relative	
All Protests						
Estimate	0.016	0.012	-0.244	-0.302	-0.103	-0.093
SE	(0.198)	(0.231)	(0.171)	(0.194)	(0.227)	(0.257)
Obs.	8358	8264	8564	8472	8382	8280
Pension Reform						
Estimate	0.059	0.088	-0.237	-0.275	-0.078	-0.026
SE	(0.221)	(0.227)	(0.17)	(0.211)	(0.236)	(0.255)
Obs.	8176	8082	8382	8290	8200	8098
Anti-Corruption						
Estimate	0.115	-0.012	-0.25	-0.357*	-0.103	-0.331
SE	(0.248)	(0.274)	(0.189)	(0.217)	(0.328)	(0.363)
Obs.	7796	7702	8002	7910	7820	7718
Covariates	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Hetergeneity

Electoral vulnerability

While exposure to protests appears to influence voting behavior in Russia, particularly by disengaging regime supporters, the questions remains: why? One explanation could be that protests fail to mobilize people because election outcomes are largely predetermined, and disillusioned voters may see little point in shifting their support to the opposition. In that case, we may expect observing a more pronounced effect on the opposition vote share in places where people expect the ruling party to perform poorly. At the same time, the opposite may be true for the effect on turnout: in places where the ruling party is traditionally perceived to be dominant, protests may have a larger effect on turnout, but still fail to increase opposition vote share. In the latter case, the effect on turnout can simply be explained mathematically: if protests are informative primary for regime supporters, the larger the pool of such supporters is, the more of them will disengage. In addition to that, protests can have a larger information revelation potential in places where most people believe the party to be extremely popular.

To investigate the role of the regime party's electoral position, I split the sample based on the regional UR share in the pre-treated period (2016). Specifically, I treat regions in which the UR vote share in the 2016 election was higher than the county average of 48% as UR strongholds (approximately one third of the sample), and compare the effect in such regions to the effect in regions were UR performed poorly. The complete list of regions in both groups is reported in the Appendix Section . My first expectation is that the effect on turnout will be larger in the UR strongholds than everywhere else.

The results reported in Table 4 indeed support this intuition. In fact, the overall negative effect on turnout appears be be driven almost exclusively by the observations from regions with above-average United Russia vote share: while the effect in the regions with low UR vote share hovers around 0.5 and does not reach statistical significance in most specification, in the UR strongholds, localities in the vicinity of protests register 4-7% lower turnout compared to the control group. As shown in the Figure 5, this difference is statistically significant.

Table 4: Estimated difference between treated and untreated units in the turnout change in regions with above- and below-average national UR vote share

	Treatment					
	500m		1000m		Relative	
UR Stronghold	Yes	No	Yes	No	Yes	No
All Protests						
Estimate	-4.821***	-0.509	-5.228***	-1.039***	-5.813***	-0.288
SE	(1.669)	(0.356)	(1.264)	(0.332)	(1.742)	(0.409)
Obs.	2446	5818	2470	6002	2456	5824
Pension Reform						
Estimate	-3.724**	-0.532	-4.496***	-1.076***	-4.384**	-0.3
SE	(1.686)	(0.394)	(1.366)	(0.333)	(1.761)	(0.408)
Obs.	2394	5688	2418	5872	2404	5694
Anti-Corruption						
Estimate	-5.109***	-0.689	-5.387***	-1.216***	-7.594***	-0.516
SE	(1.926)	(0.477)	(1.447)	(0.37)	(2.207)	(0.547)
Obs.	2278	5424	2302	5608	2288	5430
Covariates	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1.

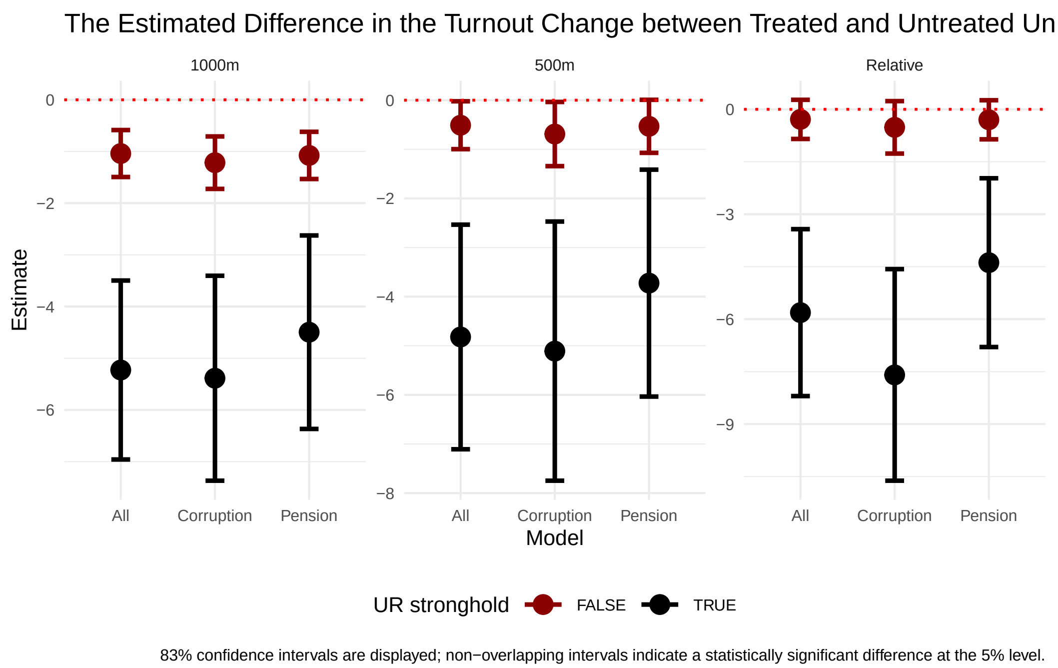


Figure 5: Estimated difference between treated and untreated units in the turnout change in regions with above- and below-average national UR vote share

And yet, the second part of the proposed mechanism does not work: regardless of the baseline support for the United Russia, protests do not seem to increase opposition vote share, as reported in Table 5. Just as before, regardless of the formulation of the treatment variable or the type of the protest, we observe no effect on the total opposition vote share.

Overall, these results suggest two main implications. First, protests can have an amplified effect in situations when their occurrence is least expected. In our case, even accounting for the fact that the United Russia's electoral strongholds had a larger pool of voters that could potentially be affected by the protests, the effect on turnout and, therefore, United Russia's vote share, is around 10 times larger than in regions where the ruling party's dominance was contested more successfully. Second, the failure of protests to mobilize disengaged United Russia supporters or attract additional pro-opposition voters, even in areas where United Russia appears to be more vulnerable, suggests that the conditions required for such mobilization are likely much more demanding than those needed for individuals to simply defect from the ruling party. It could be, for example, that exposure to protests, while making people more skeptical of the ruling party, do not affect their beliefs about the opposition. Ultimately, the protests in question were organized by various groups that rarely collaborated with one another. For instance, the Pension protests did not result in any kind of political alliance between A Just Russia and the Communist Party, the two largest left-wing parties, nor did it elevate any new force to a prominent position on the political stage. All in all, it can be that exposing the current government's lacks of support or incompetence is not enough. Equally important is presenting a credible and viable alternative.

Table 5: Estimated opposition vote share change between treated and untreated units in the turnout change in regions with above- and below-average national UR vote share

	Treatment					
	500m		1000m		Relative	
UR Stronghold	Yes	No	Yes	No	Yes	No
All Protests						
Estimate	-0.406	-0.016	-0.687	-0.247	-0.513	-0.023
SE	(0.523)	(0.239)	(0.507)	(0.206)	(0.555)	(0.244)
Obs.	2446	5818	2470	6002	2456	5824
Pension Reform						
Estimate	-0.297	-0.051	-0.681	-0.276	-0.448	-0.089
SE	(0.515)	(0.231)	(0.479)	(0.203)	(0.573)	(0.264)
Obs.	2394	5688	2418	5872	2404	5694
Anti-Corruption						
Estimate	0.064	-0.1	-0.485	-0.315	0.199	-0.273
SE	(0.573)	(0.291)	(0.51)	(0.23)	(0.783)	(0.342)
Obs.	2278	5424	2302	5608	2288	5430
Covariates	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1.

Protest magnitude

Additionally, the relatively moderate size of most protest events under examination could partly account for the observed effects. While there is little reason to believe that the size of the protests alone would alter the key pattern – namely, their limited mobilization potential – it may still play a role in understanding the mechanisms through which protests influence public behavior. In general, the literature suggests that large-scale demonstrations should have a larger information revelation potential. At the same time, poorly attended protests may provide little information about "the state of the world", or might even signal the weakness of dissenters, thereby reinforcing the stability of the regime. Whether this is the case is ultimately an empirical question, which this subsection aims to address.

Specifically, I estimate the effect separately for protests with participant numbers above and below the median threshold of 500. In both cases, I compare the change in the outcome of interest between elections for the restricted treatment group and the full control group. The results for turnout are presented in Table 6. As expected, protests with a larger number of participants demonstrate a significantly greater effect compared to those below the median.

Table 6: Estimated change in turnout between treated and untreated units in regions with above- and below-median protest magnitude

	Treatment					
	500m		1000m		Relative	
Above Median N.	Yes	No	Yes	No	Yes	No
All Protests						
Estimate	-1.74**	-0.119	-1.987***	-1.152**	-3.023***	-0.004
SE	(0.752)	(0.608)	(0.541)	(0.492)	(0.975)	(0.727)
Obs.	7710	7696	7918	7902	7726	7710
Pension Reform						
Estimate	-0.921	-0.073	-1.605***	-1.166**	-1.687*	0.099
SE	(0.731)	(0.6)	(0.553)	(0.495)	(0.919)	(0.834)
Obs.	7626	7612	7834	7818	7642	7626
Anti-Corruption						
Estimate	-1.951**	-0.4	-2.06***	-1.42**	-4.059***	-0.989
SE	(0.774)	(0.726)	(0.551)	(0.573)	(1.342)	(1.164)
Obs.	7550	7344	7758	7550	7566	7358
Covariates	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

As for the effect for the total opposition vote share, once again, there is no evidence that larger protests lead to increased mobilization. If anything, the effect is negative in some cases, although it is not consistent across specification.

Table 7: Estimated change in opposition vote share between treated and untreated units in regions with above- and below-median protest magnitude

	Treatment					
	500m		1000m		Relative	
Above Median N.	Yes	No	Yes	No	Yes	No
All Protests						
Estimate	-0.036	0.351	-0.375	-0.178	-0.394	0.331
SE	(0.283)	(0.242)	(0.23)	(0.218)	(0.372)	(0.313)
Obs.	7710	7696	7918	7902	7726	7710
Pension Reform						
Estimate	0.12	0.323	-0.317	-0.212	-0.192	0.252
SE	(0.275)	(0.261)	(0.223)	(0.21)	(0.369)	(0.372)
Obs.	7626	7612	7834	7818	7642	7626
Anti-Corruption						
Estimate	-0.035	0.382	-0.386*	-0.261	-0.577	0.255
SE	(0.312)	(0.302)	(0.22)	(0.218)	(0.465)	(0.552)
Obs.	7550	7344	7758	7550	7566	7358
Covariates	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Repression

Finally, I compare the effect separately for protests that were met with arrests and for those that concluded peacefully. As discussed before, repression could expose the regime's malign nature, potentially increasing defections from it, or signal the potential repercussions of dissent for the opposition. Therefore, the aim of this comparison is to determine whether repression deters opposition by raising the costs of dissent or, alternatively, amplifies dissatisfaction with the regime by highlighting its oppressive nature.

Due to the limited variation in this variable, the results should be interpreted as suggestive and treated with caution when drawing broader conclusions. Since only a small fraction of pension protests resulted in arrests (38 out of 866), my analysis focuses on the 2017 anti-corruption protests, where repression was both more prevalent and balanced. In this case, 150 out of 242 protests involved the detention of protesters, while 92 did not. Table 8 report the results. Although, as previously noted, the results are only indicative, there appears to be little difference between protests that faced a crackdown and those that concluded peacefully.

Table 8: Estimated change in opposition vote share between treated and untreated units in regions with above- and below-median protest magnitude

	Treatment					
	500m		1000m		Relative	
Above Median N.	Yes	No	Yes	No	Yes	No
Turnout						
Estimate	-1.174	-0.909	-1.7***	-1.605***	-2.925**	-2.373**
SE	(0.809)	(0.672)	(0.557)	(0.502)	(1.488)	(1.013)
Obs.	7422	7412	7628	7618	7436	7426
ER share						
Estimate	-1.379**	-1.087*	-1.393***	-1.282***	-1.403	-0.728
SE	(0.676)	(0.628)	(0.459)	(0.417)	(0.959)	(0.946)
Obs.	7422	7412	7628	7618	7336	7348
Opposition share						
Estimate	0.205	0.178	-0.307	-0.323	-0.18	-0.337
SE	(0.314)	(0.279)	(0.234)	(0.227)	(0.506)	(0.458)
Obs.	7422	7412	7628	7618	7436	7426
Covariates	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Discussion

This study examines the impact of localized protest activity on electoral outcomes in Russia between the 2016 and 2021 parliamentary elections. The analysis reveals that exposure to protests leads to a significant decrease in voter turnout and the ruling party's vote share, particularly in areas near protest sites. Importantly, while support for United Russia declines, there is no corresponding increase in the opposition parties' vote share. This suggests that protests in authoritarian regimes primarily demobilize regime supporters without mobilizing opposition voters.

These findings align with theoretical frameworks that emphasize the role of protests as information-revealing events in authoritarian contexts. According to Kuran's theory of preference falsification, individuals may conceal their true preferences out of fear but can be influenced to reveal them upon perceiving growing dissent (Kuran 1991). Similarly, protests act as mechanisms for information aggregation, affecting the beliefs and behaviors of both participants and observers (Lohmann 1994b). In controlled information environments, even local signs of dissent can signal growing dissatisfaction with the regime, prompting supporters to abstain from voting.

The results also build on the work of Tertytchnaya, who found that anti-government protests demobilize regime supporters more than they mobilize opposition voters (Tertytchnaya 2020). While her study focused on immediate effects, this research demonstrates that such effects can persist over time and across different protest agendas, including both Anti-Corruption protests organized by Alexei Navalny and Pension Reform protests led by the Communist Party. This suggests that the mechanism influencing bystanders' attitudes may not be tied to revealing new information but rather to signaling declining regime support (Pop-Eleches, Robertson, and Rosenfeld 2022). However, the evidence presented here is only suggestive, and further research is required to uncover the precise mechanisms through which protests influence bystanders' attitudes and behavior.

Furthermore, the heterogeneity analysis indicates that the negative effect on turnout is significantly larger in regions where United Russia has traditionally been strong. This supports the notion that protests have an amplified impact where they are least expected, consistent with Kricheli et al.'s argument regarding the information-revealing potential of protests in more repressive autocracies (Kricheli, Livne, and Magaloni 2011). Larger protests also have a more substantial negative impact on turnout, aligning with Lohmann's idea that the magnitude of protests enhances their informational value (Lohmann 1994b). However, even significant protests fail to boost the opposition's vote share, indicating that exposure to dissent is insufficient to mobilize voters without a credible alternative. Regarding repression, the analysis finds little difference between protests that faced crackdowns and those that concluded peacefully.

Overall, this study contributes to understanding political behavior in authoritarian regimes by showing that protests can erode support for the ruling party by demobilizing its base, especially in areas where the regime appears strong. However, without a compelling opposition, this disengagement does not necessarily translate into increased support for opposition parties.

More broadly, the study sheds light on the logic behind authoritarian regimes' intolerance to public displays of dissent. Even relatively small protests can undermine the regime's perceived dominance by signaling cracks in its support base, discouraging participation among its nominal supporters. This sensitivity to public dissent explains why authoritarian governments often respond to protests with repression and censorship, seeking to prevent the spread of demobilization effects. By tightly controlling the public sphere, they aim to sustain the illusion of widespread approval and suppress signals that might encourage further disengagement. Ultimately, the findings underscore the precarious nature of manufactured legitimacy in authoritarian systems, where the appearance of stability can be eroded once dissent becomes visible.

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Appendix

Covariate table for preserved (stable) and excluded (unstable) clusters

		Units	
Variable	**Statistic**	**Unstable Unit**	**Stable Unit**
ER_share_2021	Mean	19.0435	19.53371
	SE	0.1492734	0.1475779
area_km2	Mean	2.114039	2.109732
	SE	0.02400726	0.02304287
mean_night_lights	Mean	42.80612	37.66142
	SE	0.3752388	0.3833603
median_price_m2	Mean	119673.3	86320.58
	SE	883.2674	531.2641
voters_density	Mean	6613.991	6380.13
	SE	218.7334	128.3555
within_city_price_m2	Mean	0.9650313	0.9528
	SE	0.00191748	0.001875912

Average change in the number of regsitered voters for preserved (stable) and excluded (unstable) clusters

		Units	
Variable	**Statistic**	**Unstable Unit**	**Stable Unit**
voters_difference	Mean	2681.44583	365.891569
	SE	43.03486	4.083685

Buffer clusters in the control group

Regions with above and below average UR share in 2016

Table A4: The List of Regions with UR Vote Share Below or Above Nation Average

Below Average	Above Average
Astrakhan Oblast	Bashkortostan

Sverdlovsk Oblast

Chelyabinsk Oblast

Irkutsk Oblast

Saint Petersburg

Leningrad Oblast

Krasnoyarsk Krai

Moscow Oblast

Moscow

Tver Oblast

Perm Krai

Yamalo-Nenets Autonomous Okrug

Pskov Oblast

Vologda Oblast

Primorsky Krai

Oryol Oblast

Karelia

Kaliningrad Oblast

Vladimir Oblast

Yaroslavl Oblast

Altai Krai

Orenburg Oblast

Murmansk Oblast

Amur Oblast

Khanty-Mansi Autonomous Okrug — Yugra

Belgorod Oblast

Mari El

Tomsk Oblast

Khabarovsk Krai

Kalmykia

Kemerovo Oblast

Tatarstan

Volgograd Oblast

Rostov Oblast

Nizhny Novgorod Oblast

Stavropol Krai

North Ossetia–Alania

Mordovia

Tula Oblast

Lipetsk Oblast

Saratov Oblast

Tuva Republic

Kabardino-Balkaria

Chuvashia

Dagestan

Tambov Oblast

Bryansk Oblast

Karachay-Cherkessia

Altai Republic

Kaluga Oblast

Jewish Autonomous Oblast

Sakhalin Oblast

Arkhangelsk Oblast

Kamchatka Krai

Republic of Khakassia

Smolensk Oblast

Kursk Oblast

Kirov Oblast

Samara Oblast

Penza Oblast

Kurgan Oblast

Republic of Sakha (Yakutia)

Republic of Komi

Ulyanovsk Oblast

Tyumen Oblast

Voronezh Oblast

Novgorod Oblast

Adygea

Udmurtia

Ivanovo Oblast

Kostroma Oblast

Novosibirsk Oblast

Zabaykalsky Krai

Ryazan Oblast

Table A1: Estimated Difference Between Treated and Untreated Units in the Turnout Change (2021 vs. 2016 Elections), with Buffer Clusters in the Control Group

	Treatment					
	500m		1000m		Relative	
All Protests						
Estimate	-1.019**	-0.76	-1.373***	-1.194***	-1.345**	-1.143*
SE	(0.511)	(0.516)	(0.429)	(0.426)	(0.58)	(0.613)
Obs.	10200	10086	10200	10086	10200	10086
Pension Reform						
Estimate	-0.571	-0.241	-1.136***	-0.906**	-0.758	-0.466
SE	(0.508)	(0.517)	(0.404)	(0.444)	(0.591)	(0.587)
Obs.	10018	9904	10018	9904	10018	9904
Anti-Corruption						
Estimate	-1.405**	-1.318*	-1.604***	-1.512***	-2.409***	-2.58***
SE	(0.609)	(0.688)	(0.445)	(0.495)	(0.883)	(0.895)
Obs.	9638	9524	9638	9524	9638	9524
Covariates	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: Estimated Difference Between Treated and Untreated Units in the UR Vote Share Change (2021 vs. 2016 Elections), with Buffer Clusters in the Control Group

	Treatment					
	500m		1000m		Relative	
All Protests						
Estimate	-1.099**	-0.946**	-1.209***	-1.049***	-1.233**	-1.113**
SE	(0.438)	(0.442)	(0.342)	(0.396)	(0.479)	(0.507)
Obs.	10200	10086	10200	10086	10200	10086
Pension Reform						
Estimate	-0.694	-0.489	-0.978***	-0.783**	-0.669	-0.484
SE	(0.424)	(0.438)	(0.342)	(0.398)	(0.544)	(0.508)
Obs.	10018	9904	10018	9904	10018	9904
Anti-Corruption						
Estimate	-1.585***	-1.521***	-1.433***	-1.317***	-2.296***	-2.373***
SE	(0.543)	(0.563)	(0.386)	(0.437)	(0.746)	(0.794)
Obs.	9638	9524	9638	9524	9638	9524
Covariates	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: Estimated Difference Between Treated and Untreated Units in the Total Opposition Vote Share Change (2021 vs. 2016 Elections), with Buffer Clusters in the Control Group

	Treatment					
	500m		1000m		Relative	
All Protests						
Estimate	0.08	0.186	-0.165	-0.145	-0.113	-0.03
SE	(0.203)	(0.209)	(0.174)	(0.177)	(0.232)	(0.233)
Obs.	10200	10086	10200	10086	10200	10086
Pension Reform						
Estimate	0.124	0.248	-0.157	-0.123	-0.088	0.019
SE	(0.201)	(0.193)	(0.168)	(0.178)	(0.243)	(0.246)
Obs.	10018	9904	10018	9904	10018	9904
Anti-Corruption						
Estimate	0.179	0.203	-0.171	-0.194	-0.113	-0.207
SE	(0.236)	(0.252)	(0.2)	(0.211)	(0.301)	(0.347)
Obs.	9638	9524	9638	9524	9638	9524
Covariates	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**

Note:
Standard errors are clustered on the level of the treatment assignment and reported in parentheses.
Calculated using bootstrap method with 1000 iterations
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.