Illusive Transparency? Evidence on Election Video Monitoring in Russia*

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Abstract

Authoritarian regimes, like democracies, hold elections and often equip them with transparency-enhancing technologies. Why would autocrats want to hold or appear to hold transparent elections? This paper examines the impacts of broadcast election video monitoring in Russia. I exploit a discontinuity in the assignment of webcams to polling stations during the 2018 presidential election to estimate the causal effects on voting. Video monitoring reduces reported voter turnout by 5.2% and votes for the incumbent (autocrat) by 8.3%, suggesting a decrease in fraud. However, that decrease is partially offset by increased votes for the incumbent in neighboring unmonitored polling stations, indicating a displacement of fraud. To explore why autocrats implement video monitoring, I conducted a nationwide survey experiment before the 2019 local elections. Treated respondents were informed of video monitoring, which significantly increased the trust in elections and willingness to vote among those not previously aware of transparency technologies. Overall, these results suggest that video monitoring allows autocrats to improve citizens' attitudes by creating an illusion of transparency at a low cost in terms of net lost votes.

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I. Introduction

The last decade saw the backsliding of democracy worldwide, with a decline in the number of democracies and steady decreases in democratic scores across countries (Lührmann and Lindberg, 2019).[1] Nowadays, 43% of the world's population and 87% of the poor live in hybrid and authoritarian regimes.[2] These modern non-democratic regimes look different from those of the previous century. They maintain control less through the overt use of violence, repression, and indoctrination. Instead, they imitate core democratic institutions and hold elections at different levels of government (Guriev and Treisman, 2019*a*). More than 80% of non-democracies elect legislatures on a regular basis (Cruz, Keefer and Scartascini, 2021). Most of them invest in electoral transparency by inviting international observers, who monitor up to 84% of national non-democratic elections (Hyde and Marinov, 2012), and equipping polling places with transparency-enhancing technologies. Some examples include video monitoring, electronic vote-count and voting machines, and transparent ballot boxes. This raises the question: why would autocrats want to hold or appear to hold transparent elections?

This paper explores the effects of election video monitoring in Russia, a well-known authoritarian regime. This technology involves the placement of webcams inside polling stations, which then stream voting online (as depicted in Appendix Figure A.1). Studying election video monitoring is interesting for three reasons. First, economists model elections as an accountability mechanism that helps align the incentives of citizens and those of their government (Besley, 2006). However, these models cannot explain the need for transparent elections in autocracies, which do not always maximize social welfare. Second, elections represent critical junctures that can trigger the mobilization of the masses and democratization (Acemoglu and Robinson, 2012). Electoral irregularities facilitated revolutions in several post-communist countries, such as Georgia, Kyrgyzstan, Ukraine, and Serbia (Tucker, 2007; Bunce and Wolchik, 2011). Third, various states have increasingly adopted video monitoring, including Albania, Armenia, Azerbaijan, Georgia, India, Israel, and Ukraine.[3] Many counties in the United States also install webcams in vote-counting centers.

The effects of video monitoring are ambiguous, especially in a non-democracy. On the one hand, it reduces the incentives of local authorities to commit election fraud inside

[1]For example, Freedom House reports that the number of countries with aggregate *Freedom in the World* score declines outnumbered those with score gains every year for the last 15 years, and this gap has been increasing over time. Source: "Freedom in the World 2021: Democracy under Siege," freedomhouse.org

[2]Hybrid and authoritarian (non-democratic) regimes are states with Polity IV score of less than 6; *poor* are residents of low-income countries defined by the World Bank classification.

[3]In some countries, video monitoring raised concerns of ballot secrecy violation and voter intimidation. For example, Arab citizens claimed that the Israeli government used cameras against them as a voter-intimidation tactic in the 2019 legislative election. Source: "Israel Voting Cameras Lowered Arab Turnout, Netanyahu Backers Claim," published on April 10, 2019, by the *New York Times*: www.nytimes.com.

polling stations since citizens can detect irregularities online. In turn, the reduced scope for fraud can make local officials more accountable to citizens for spending on public goods. On the other hand, autocrats can use video monitoring to the regime's advantage. They can displace fraud to unmonitored polling stations or substitute it with forms of manipulation that are hard to observe on video. In such circumstances, video monitoring can mislead citizens and form biased beliefs about the fairness of elections, potentially leading to better attitudes toward the government. In the most extreme case, autocrats can use it to intimidate voters and coerce votes for the incumbent. This paper attempts to resolve the ambiguity in the effects of video monitoring using quasi-experimental evidence from the administrative data and experimental evidence from an original survey experiment.

I estimate the causal effects of video monitoring on election outcomes by exploiting a discontinuity in the assignment of webcams to polling stations in the 2018 presidential election in Russia. The Central Election Commission suggested installing webcams only inside polling stations with more than 1,000 registered voters. I employ a fuzzy regression discontinuity design around this cutoff and combine it with election data from 23,000 polling stations covering 25 million registered voters (23.3% of all voters). Specifically, I compare election outcomes at polling stations whose voter coverage is immediately above the cutoff for video monitoring to those at polling places just below the cutoff level.[4]

I present four main results from the regression discontinuity analysis. First, video monitoring reduces the reported voter turnout by 5.2%. It also leads to 8.3% fewer votes in favor of the incumbent without a corresponding change in the votes cast for other candidates.[5] I show that these effects are consistent with a reduction in election fraud rather than a decrease in voters' willingness to vote. Using a standard elections forensic tool, I find that a large share of unmonitored polling stations experience abnormally high turnout and an abnormal increase in the incumbent's vote share; this pattern is commonly observed in countries with fraudulent elections. In contrast, video monitoring significantly reduces this share such that turnout and incumbent vote share resemble those for fair elections in democracies.

Second, I show that the direct effects of video monitoring are more prominent in rural areas known for higher levels of election fraud. Rural polling stations experience an 8.2% drop in votes cast for the incumbent. The estimate for urban areas is less precise, amounting to only a 5.7% reduction in votes. Nevertheless, only 14% of rural polling stations have webcams installed. This might indicate a decision not to install

[4]Not all eligible polling stations implemented video monitoring due to financial and legal constraints. Treatment-on-the-treated estimates adjust the differences for a 60-percentage-point gap in the implementation of video monitoring between the two groups.

[5]The results are robust to a series of robustness checks, including using different functional forms and bandwidths, and placebo tests using different elections and alternative thresholds.

video monitoring in rural areas so as to mitigate its effects on fraud overall. A back-of-the-envelope calculation suggests that the universal adoption of video monitoring would result in a 55% higher loss in votes compared to the results under the actual system of implementation.

Third, I find that video monitoring displaces votes to neighboring unmonitored polling stations. Using a similar empirical strategy to that already described, I show that unmonitored polling stations located in close geographical proximity to monitored stations experience a spike in the share of votes cast for the incumbent. A rough estimate suggests that this spike offsets more than half of the direct effect of video monitoring on the incumbent's votes.

Fourth, I explore whether the reduced scope for election fraud disciplines local officials to provide more public goods. I instrument the intensity of video monitoring in each district with the share of polling stations above the cutoff in the bandwidth. I do not find evidence of differential changes in public spending or its re-allocation across sectors in districts with a higher intensity of video monitoring before the election. Hence, there are no observable accountability effects of monitoring on public goods spending.

I then explore *why* a non-democratic regime might introduce election video monitoring in the first place. I use an original survey experiment conducted before the 2019 local elections for a nationally representative sample of 1,097 prospective voters to adjudicate among the following theories.[6] First, autocrats might concede some power to voters and invest in democratic institutions to prevent discontent (Acemoglu and Robinson, 2005). In particular, they might introduce video monitoring of elections to build the trust of citizens in electoral institutions and improve attitudes towards the regime (Fearon, 2011; Little, 2012). Second, autocrats might invest in fraud-reducing technologies to convince citizens of the regime's popularity and deter opponents (Egorov and Sonin, 2021). Third, autocrats might invest in video monitoring to intimidate voters and coerce support for the regime (Frye, Reuter and Szakonyi, 2014, 2019*a*).

The history of election video monitoring's introduction in Russia aligns with the first theory. In December 2011, Russia saw the largest protests since the fall of the USSR, triggered by numerous allegations of election fraud in the parliamentary elections. To convince voters of electoral integrity, the prime minister ordered the installation of cameras for future elections. The survey experiment further supports this theory.

The experiment provided a reminder about video monitoring of the upcoming local elections to a random half of respondents. I then inquired about respondents' voting intentions, perceptions about elections, and general views of democracy. I find that 60% of respondents had already been aware of webcams in the upcoming elections, 65% thought it was an effective policy, and 15% expressed their willingness to monitor elections online.

[6] Prospective voters are individuals who answered that their locality was going to have elections on the 2019 Single Voting Day.

Additionally, respondents who received a reminder about video monitoring were 16% more likely to trust the election results fully. This effect is mainly concentrated among respondents previously unaware of electoral transparency policies; this was proxied by the respondent's awareness of transparent ballot boxes. Those respondents who were unaware of transparency policies were 30% more likely to believe in the fairness of the results. They were 23% more likely to express an intention to turn out to vote and 33% more likely to vote for the incumbent (autocrat).[7] Overall, information about video monitoring improved citizens' attitudes toward the regime.

The survey data does not provide strong support for the second theory, that video monitoring convinces citizens of the regime's popularity. The reminder about video monitoring has only a small and statistically insignificant effect on beliefs about others' support of the ruling party. While respondents in the control group, on average, believed that 47.8% of other voters support candidates from the ruling party, this number only increased to 49.8% in the treatment group.

The data also contradicts the third theory of voter intimidation. First, the findings of lower turnout and fewer votes cast for the incumbent are inconsistent with voter intimidation. One would expect to see the opposite pattern given the coercion of voters in non-democratic regimes. There are also no effects observed on the votes cast for other candidates, indicating no intimidation of opposition supporters. Second, according to the list experiment I conducted as part of the survey to analyze voter intimidation, 26% of respondents had fears that their employer or local authorities would know whether they voted or not.[8] However, the level of voter intimidation does not significantly vary with a reminder about video monitoring.

To summarize, this paper makes several contributions to the existing literature. First, my findings contribute to the growing literature on the political economy of non-democracies (see Egorov and Sonin (2020) for a review of that literature). In particular, my findings add to the political science literature on elections held by autocrats, reviewed in Gandhi and Lust-Okar (2009) and Gehlbach, Sonin and Svolik (2016). My results improve our understanding of why autocrats might want to appear to hold transparent elections. While the direct effects of video monitoring are consistent with improved electoral accountability, these results are deceptive given its implementation only at large polling stations and the subsequent displacement of votes. As a result, voters form overly optimistic beliefs about the effectiveness of video monitoring and increase their trust in electoral institutions. These conclusions are similar to those of Herron (2010) and

[7]The latter is not statistically significant due to the smaller size of the sample of respondents willing to vote.

[8]The list experiment estimates sensitive beliefs without directly asking about them. All the respondents receive a list of statements related to the elections, and for a random half, an additional item about voter intimidation is included. They declare only the total number of statements with which they agree. The difference between the two treatment groups reveals the level of voter intimidation.

Sjoberg (2014), who study video monitoring of around 500 polling stations in Azerbaijan. I extend and deepen those insights by analyzing, for the first time, a nationwide video monitoring policy. The additional data, and the survey experiment, allow me to study vote displacement effects and distinguish between different theories.

My results contribute to other theories of why autocrats might want to hold transparent elections. They might use elections to collect information on the ruling party's support and performance of local officials (Cox, 2009; Malesky and Schuler, 2011; Miller, 2015; Martinez-Bravo et al., 2017). Autocrats may also want to appease the international community to gain legitimacy and receive foreign aid (Wright, 2009; Hyde and Marinov, 2014). However, as I discuss below, video monitoring is unlikely to be used for these reasons in Russia. Finally, my findings indicate potential trade-offs between reducing election fraud to improve the attitudes of the population at large and using fraud to deter opponents and project strength to allies (Simpser, 2013; Gehlbach and Simpser, 2015; Rozenas, 2016).

My conclusions are in line with the theory and cross-country empirical evidence of Guriev and Treisman (2019*a*,*b*), who show that modern autocrats manipulate information to signal the competence and benevolence of their regime. As my results indicate, autocrats use information about video monitoring to signal the fairness of elections, which improves attitudes toward the regime. This evidence contributes to the growing empirical literature on how autocrats manipulate citizens' beliefs. For example, Adena et al. (2015), King, Pan and Roberts (2017), and Chen and Yang (2019) show the effects of media tools, such as Nazi radio in Germany and Internet censorship in China, and Cantoni et al. (2017) show the effects of ideology promoted via a school curriculum.

Finally, my findings contribute to the literature on the impacts of transparency technologies on accountability and service delivery (Duflo, Hanna and Ryan, 2012; Lewis-Faupel et al., 2016; Muralidharan, Niehaus and Sukhtankar, 2016; Bossuroy, Delavallade and Pons, 2019; Banerjee et al., 2020; Muralidharan et al., 2021). Specifically, my findings are relevant to evaluations of technologies designed to improve electoral integrity (Callen and Long, 2015; Fujiwara, 2015). The direct effects of video monitoring suggest that this technology can improve electoral accountability when it is carefully implemented with this goal in mind. It thus offers a viable alternative to in-person election observation, which is proven to be effective (Hyde, 2007; Ichino and Schündeln, 2012; Casas, Diaz and Trindade, 2017; Enikolopov et al., 2013) but is often limited due to high costs. However, the indirect effects of video monitoring indicate that its use leaves room for manipulation and the displacement of votes from monitored to unmonitored polling stations. This result is consistent with the findings of Ichino and Schündeln (2012) and Asunka et al. (2017), who show similar displacement effects in the presence of traditional election observers in Ghana. It also accords with the result of Banerjee, Duflo and Glennerster (2008), who show that the local health administration adapted to the time-stamping machines monitoring nurse absenteeism in public health clinics in India. These results, taken together, indicate the need for more research on designing accountability-improving technologies that cannot be easily manipulated by authorities.

The remainder of the paper proceeds as follows. Section II sets out the institutional background, and Section III outlines the theoretical predictions. Section IV describes the data sources and introduces the primary empirical strategy. In Section V, I estimate the direct effects of video monitoring on election outcomes and discuss the mechanisms. Section VI evaluates the indirect impacts of video monitoring on the displacement of votes and public goods spending. In Section VII, I present evidence from my original survey experiment and use this to adjudicate among the theories of why autocrats invest in video monitoring. Section VIII concludes and makes policy recommendations.

II. Background

A. Elections in Russia

After growing democratic tendencies in the early 2000s, Russia has reverted to an autocratic state in the last decade.[9] Nowadays, it classifies as an electoral authoritarian regime. These regimes imitate democratic institutions, including elections. Russia holds elections at all government levels, including federal, regional, and local elections. However, they are not entirely free and fair.

The electoral system of Russia consists of multiple levels. The first level is the Central Election Commission, an independent body responsible for conducting federal elections and overseeing regional and local elections. It consists of 15 members. The president, State Duma, and Federation Council of Russia each appoint five members. The second level consists of 85 regional election commissions, one per region. They help conduct federal elections and organize regional elections. The third level consists of the territorial election commissions formed in the territories of cities and districts (*rayons*) by the corresponding regional election commissions. The fourth, the lowest level, consists of polling stations responsible for the conduct of all elections. The territorial election commissions form them for a term of five years. Each polling station has 3–16 employees, depending on the number of registered voters.

The Criminal Code of Russia defines the following categories of electoral malfeasance: hindering exercise of voting rights or work of election commissions, falsification of election documents, illegal issue and receipt of ballot papers, and falsification of election results. The most severe punishment is imprisonment for a period of one to four years. In practice,

[9] The Polity IV score reached a value of +6 in the early 2000s. It reverted to +4 in 2007 and stayed at that level. This score belongs to the bottom 40% of countries in the democratic range and is similar to Guinea, Zimbabwe, Algeria, and Ethiopia.

prosecutions of electoral malfeasance are rare and usually penalized with monetary fines or community sentences. As a consequence, election fraud is widespread in Russia and well-documented by Myagkov, Ordeshook and Shakin (2009), Enikolopov et al. (2013), and Rundlett and Svolik (2016).

This paper studies the 2018 presidential and 2019 local elections. The presidential election has been contested by eight candidates, including the incumbent president (autocrat). The incumbent won with 76.7% of votes and voter turnout of 67.5%. The major international observation mission by the OSCE concluded that the election was held in an overly controlled environment, marked by continued pressure on critical voices and a lack of real competition. However, it also noted that the Central Election Commission administered the process efficiently and openly.[10] The 2019 local elections selected representatives of different government levels, including parliamentary, gubernatorial (regional), and municipal. Candidates from the incumbent United Russia party won elections in most regions and municipalities.

B. Election Video Monitoring

In the last decade, the Central Election Commission of Russia invested substantial resources in technologies designed to improve the transparency and accountability of elections. It equipped polling stations with transparent ballot boxes, electronic vote counting and voting machines, video monitoring, and recently introduced electronic voting. The technology of video monitoring consists of placing webcams inside polling stations that stream the election day online. Russia is the largest implementer of video monitoring in the world, doing so on a national scale. Albania, Armenia, Azerbaijan, Georgia, India, Israel, and Ukraine have also implemented video monitoring in different years at a smaller scale. Many U.S. counties live-stream the vote-count process.

Russia first introduced video monitoring in the 2012 presidential election. The prime minister at the time ordered the installation of webcams in all polling stations in response to the large protests in December 2011, triggered by allegations of fraud in the 2011 parliamentary election.[11] As a result, 95% of all polling stations were equipped with webcams, around 90% of which streamed the election day online. Since then, regional and local election commissions have implemented video monitoring during lower-level elections at their discretion.

The 2018 presidential election brought back video monitoring on a national scale. The Central Election Commission implemented it at all territorial election commissions and

[10] The OSCE Office for Democratic Institutions and Human Rights: "Russian presidential election well administered, but characterized by restrictions on fundamental freedoms, lack of genuine competition, international observers say," www.osce.org

[11] A VCIOM survey revealed that 45% of the respondents considered the 2011 parliamentary election as not free and fair, and 31% of the respondents did not trust the results. The Levada Center found that 46% of the respondents did not trust the results to some extent.

polling stations with more than 1,000 registered voters. Each monitored polling station had two webcams, a computer, and an Internet connection. The first webcam broadcasted the place of issuance of ballots to voters. The second webcam broadcasted the ballot boxes and voting booths.

Each camera streamed live video footage, which citizens could freely access on the website www.nashvybor2018.ru.[12,13] The live stream covered the night before the election day (before 8:00 a.m.), voting (8:00 a.m.– 8:00 p.m.), and count of votes (after 8:00 p.m.). Voters could request access to a video recording from their polling station for three months after the election day. Presidential candidates could request access to any video recording during the same period. However, it was not easy to obtain such access in practice.

III. Theoretical Framework

Video monitoring can affect elections in two ways. First, it can change the behavior of local authorities, election administrators and politicians, by reducing the scope of election fraud. Second, it can impact voters who might change their voting behavior.

Similar to in-person observation, video monitoring reduces the incentives of local authorities to commit election fraud (see Hyde (2007) and Enikolopov et al. (2013) for the effectiveness of international and domestic monitors). Video monitoring can primarily prevent fraud easily observable on the video recording, such as ballot-box stuffing. It consists of casting ballots in place of registered voters who did not turn out to vote or bringing the same voters to multiple polling stations. Nevertheless, local officials can substitute observable fraud with other types, including voter registration and vote-count fraud, which are harder to identify on the video. They can also displace fraud to unmonitored polling stations. For example, Ichino and Schündeln (2012) find evidence of vote displacement in the presence of traditional election observers in Ghana.

If video monitoring successfully reduces election fraud, it might also trigger accountability effects. The classical principal-agent models consider elections as the primary accountability mechanism that helps align incentives of citizens and their government (Besley, 2006). Video monitoring might create additional incentives for politicians to improve the provision of public goods and services before the election. This would allow them to secure citizens' support of the incumbent and demonstrate loyalty to the regime.

Turning to voters, they receive information about video monitoring via media sources (TV, social media), signs at the polling stations, and a live-stream website. Since voters do not know about the monitoring status of their polling place before the election, they cannot change their registration for another polling station. However, they might change

[12] Appendix Figure A.1 presents screenshots of the website made on the election day.

[13] The Central Election Commission restricted access to the live video footage in the 2021 State Duma election. It is now available only at the special venues organized in the regional capitals.

their decision to vote or candidate choice on the election day if they receive information that their polling place is monitored. Moreover, they might change their voting behavior regardless of the monitoring status of their polling station if they react to the general information presented by the media.

On the one hand, video monitoring can trigger voter intimidation, which might have different effects on supporters and opponents of the regime in a non-democratic state. It can increase the turnout of government employees if they believe that their employer might see whether they voted or not. Frye, Reuter and Szakonyi (2014, 2019*a*,*b*) report widespread workplace mobilization in Russia and other non-democratic states. Video monitoring can also suppress the turnout of regime opponents if they believe it violates their vote secrecy. For example, Arab citizens claimed that the Israeli government used cameras against them as a voter-intimidation tactic in the 2019 legislative election.[14]

On the other hand, video monitoring can increase voters' trust in elections if they believe in its effectiveness in reducing election fraud. A representative poll of Russian citizens showed that 63% of respondents approved the introduction of video monitoring in 2012. The majority of respondents noted that it would reduce election fraud, promote fair and transparent elections, and allow to observe and document violations.[15] Because of higher trust in election results, voters might be more likely to turn out to vote and even vote in favor of the regime. These effects should be larger among those not previously aware of other transparency initiatives.

Overall, the effects of video monitoring are ambiguous and depend on the relative magnitudes of these counterbalancing mechanisms. The following sections test outlined theoretical predictions using multiple data sources, empirical strategies, and an original survey experiment.

IV. Data and Empirical Strategy

This section starts with a description of each data source, including an original survey experiment conducted before the 2019 local elections. It proceeds with a description of the primary empirical strategy used to estimate the effects of video monitoring on voting. I describe the auxiliary empirical methods in the corresponding sections of results.

A. Data

Location of Video Monitoring. Estimating the effects of video monitoring requires knowledge on which polling stations had webcams. I collected this information from

[14]Source: "Israel Voting Cameras Lowered Arab Turnout, Netanyahu Backers Claim," published on April 10, 2019, by the *New York Times*: www.nytimes.com

[15]Source: Results of a representative survey on socio-economic and political topics conducted by Public Opinion Foundation (FOM) in January of 2012.

the official documents (decrees) issued by regional election commissions. This data was available on the websites of 26 out of 85 regions in Russia.[16] These regions span more than 23,000 polling stations and cover more than 25 million (23.3%) registered voters. Appendix Figure A.2 shows that studied regions are spread geographically across the entire country. Appendix Table A.1 shows that studied regions have slightly lower average reported turnout and incumbent's vote share than the non-studied regions.

Election Outcomes. The primary administrative data source is the official results of the 2012 and 2018 presidential elections. I web-scraped this data from the website of the Central Election Commission of Russia in April–June 2018.[17] This data contains the following information at the polling station level: the number of votes cast for each candidate, the number of registered voters, the number of voters who moved in and out of each polling station during the officially allowed period of 45 days before the election, and detailed information on ballots. The last includes the number of valid and invalid ballots and the number of ballots received by each polling station before the election day, cast inside and outside of each polling station, cast in stationary and carry boxes, and cast before the official voting day.

Polling Station Characteristics. I collected two sets of characteristics of the polling stations. First, I obtained the addresses of all the polling stations using the documents (decrees) and information on the websites of the regional election commissions. I geocoded the addresses of the polling stations using Google and Yandex maps. I then categorized the locations of all the polling stations into urban or rural areas using a text search algorithm.[18] Second, I obtained the numbers and names of the polling station employees who worked in the 2018 presidential election for two regions in my sample.

Public Goods Spending. I collected data on local public procurement contracts to analyze the accountability effects of video monitoring on public goods spending. This data comes from the open registry of all the public procurement contracts in the country, available at www.zakupki.gov.ru. I obtained details on all the contracts signed four months before the 2018 presidential election (December 2017–March 2018) and the comparable period a year earlier (December 2016–March 2017). Each contract contains the following information: the code and name of each item, its value per unit, the number of units, the start and completion dates, the purchasing agency, and the funding source. I computed the total number and value of all the contracts per district (*rayon*), which include multiple territorial election commissions.

[16]The lack of this data for other regions can be due to poor website support, the negligence of bureaucrats, or to its intended unavailability.

[17]Different regions and outcomes were web-scraped on different dates.

[18]Russian addresses usually contain prefixes that distinguish settlement types. I define urban areas using addresses that include the following prefixes: "city," "urban settlement," and "urban-type settlement." Rural areas contain all other prefixes.

Survey Experiment. I also conducted an original survey experiment before the 2019 local elections to evaluate the effects of video monitoring on voter beliefs and behavior. It was carried out as part of the Omnibus survey conducted by the independent survey company Levada Market Research.[19,20] The survey consisted of face-to-face interviews with 1,608 adults from 50 regions of Russia. The sample was stratified by gender, age, education, location, and municipality size using Russian statistical agency data. The resulting sample is representative of the Russian population with a margin of error of 3.4%. The survey included a block of questions about demographics, social status, and government approval, followed by a survey experiment to estimate the effects of video monitoring. The experiment was conducted for a subsample of 1,097 respondents who indicated that their locality was going to have elections on Single Voting Day (September 8, 2019). Section VII.A. describes the survey experiment design in detail.

B. Empirical Strategy

To estimate the causal effects of video monitoring, one ideally needs a random assignment of webcams to polling stations. In the absence of experimental variation, I exploit a quasi-experimental discontinuity in the implementation of video monitoring. In the 2018 presidential election, the Central Election Commission instructed regional election commissions to use a rule of 1,000 registered voters to define eligibility for video monitoring. Figure 1 shows a graphical representation of a discontinuity in the assignment of webcams resulting from the use of this rule. In total, 44% of the polling stations had video monitoring: 89% with more than 1,000 registered voters and 2% below this threshold.

I employ a fuzzy regression discontinuity design around the cutoff of 1,000 registered voters to estimate the causal effects. In particular, I compare the election outcomes at the polling stations right above the cutoff of 1,000 registered voters, which were eligible for video monitoring, to the results at the polling stations right below the cutoff, which did not qualify for it. The following equations summarize the empirical strategy:

$$m_s = \alpha_1 + \mathbb{1}\{v_s \geq c\}\left[f_r(v_s - c) + \delta\right] + \mathbb{1}\{v_s < c\}f_l(c - v_s) + \varepsilon_{1s} \quad (1)$$

$$y_s = \alpha_2 + \tau m_s + \mathbb{1}\{v_s \geq c\}g_r(v_s - c) + \mathbb{1}\{v_s < c\}g_l(c - v_s) + \varepsilon_{2s} \quad (2)$$

where m_s is a dummy variable equal to one if a polling station s was equipped with video monitoring, v_s is the number of registered voters, c is a cutoff of 1,000 registered voters, y_s is an election outcome, and f and g are unknown functions. The first-stage estimate δ estimates a jump in the probability of video monitoring at the threshold of 1,000

[19] Omnibus surveys allow administering nationally representative surveys at a reasonable cost in geographically spread-out countries like Russia by pooling questions from multiple researchers who share the costs.

[20] Levada Market Research is known as the Levada Center in Russia.

registered voters. The second-stage estimate τ estimates the impact of video monitoring on the election outcome y.

The primary identifying assumption is no manipulation of the running variable, which determines eligibility for video monitoring. I choose as a running variable the number of registered voters 45 days before the election, i.e., before they could officially request to change their polling station. The regional election commissions decided where to install webcams before this date. Figure 2 plots the frequency of the running variable (Panel A) and McCrary density test (Panel B). There is no evidence of manipulation of voter registration at the time of webcam assignment (test statistic = -0.8, p-value = 0.42).

Another identifying assumption is no discontinuities in other polling station characteristics at the threshold of 1,000 registered voters that could have affected election outcomes. Appendix Figure A.3 shows that there is no discontinuity in the assignment of electronic vote-counting machines, another technology designed to improve electoral accountability. Appendix Figure A.4 shows for a subsample of two regions that there is no discontinuity in the number of polling station employees.[21] Thus, there are no other discontinuities in the observable characteristics.

Another assumption required for a consistent estimation of the second stage is that video monitoring is the only channel causing differences in election outcomes. A potential concern is that the number of registered voters crossing the threshold can affect election outcomes for reasons unrelated to video monitoring. For example, voters at slightly bigger polling stations can have different preferences. To address this concern, I will report below (in Section V.B.) placebo tests using alternative thresholds and the preceding 2012 presidential election, which had cameras at all polling stations.

Two-stage least squares regression also requires a monotonicity assumption. The rule of 1,000 registered voters should not make any polling station less likely to install video monitoring. I do not directly observe what types of eligible polling stations did not have video monitoring. However, the regional election commissions made eligibility decisions based on the official guidelines issued by the Central Election Commission. The officials could not install cameras at the polling stations established in hospitals and other health organizations that have inpatient departments, detention centers, other places of temporary stay, military units, and polar stations. Most eligible polling stations that did not have video monitoring should be located in these restricted places.

V. Direct Effects

A. Implementation

The placement of video monitoring at the polling stations with more than 1,000 registered voters affects the scale of its implementation. Figure 1 and Table 1 show that 44%

[21]This data is difficult to obtain for all regions.

of all polling stations are equipped with webcams in 26 studied regions. They cover 73% of registered voters and are predominantly located in urban areas. Around 94% of eligible urban polling stations with more than 1,000 registered voters have video monitoring. The remaining 6% of eligible urban polling places are located in hospitals, detention centers, and other places restricted by law. At the same time, only 69% of eligible rural polling stations have webcams, reflecting either a lack of technical infrastructure or its intentional absence. Overall, video monitoring is present at 82% of urban and 14% of rural polling stations.

Panel A of Figure 3 shows a graphical representation of the first stage using the bandwidth of 400 registered voters and separate linear trends on each side of the cutoff. Polling stations with more than 1,000 registered voters have an approximately 60 percentage points higher probability of video monitoring than polling stations with fewer than 1,000 registered voters.

One can also notice that a few (84 or 1.4%) polling stations right below the cutoff experience a jump to an intermediate point of around 45 percentage points. There are strong reasons that this jump does not reflect manipulation. First, these polling stations receive video monitoring even though they are not eligible for it. Video monitoring is not a desirable outcome because it makes election fraud more difficult. Second, these polling stations constitute only 1.4% of all polling stations in the bandwidth and are spread across one-third of all territorial election commissions in 23 regions. If there is an intentional manipulation, one would expect these polling stations to be concentrated in a few geographical areas. Hence, this intermediate jump is likely a result of the non-strict implementation rather than intended manipulation. The regional election commissions may have also used a different period to determine eligibility for video monitoring compared to my analysis.[22] Overall, the intermediate jump does not threaten the identification. However, it makes estimates noisier and biases them downward. To estimate a precise effect of video monitoring, hereafter, I remove polling stations with 975–1,000 registered voters that experience a jump from the primary analysis.

Panel A of Table 2 quantifies the first-stage estimates. Columns (1)–(3) show that polling stations with more than 1,000 registered voters have a 60 percentage points higher probability of video monitoring (significant at the 1% level, std. error = 2.8). The magnitude does not change with regional and territorial election commission fixed effects, which signals a similar implementation of video monitoring across different geographical units. Columns (4)–(5) indicate that implementation was less strict in rural areas, where slightly fewer eligible polling stations received video monitoring.

[22] The data on the number of registered voters is publicly available for two dates: the first day of the re-registration period (45 days before the election day) and the election day. I use the former as my running variable and a difference between the two dates as an outcome that quantifies re-registration.

B. Effects on Voting

Does video monitoring affect voting? We now turn to the first set of results. Panels B–D of Figure 3 show graphical representations of the reduced form estimates of the impact of video monitoring on election outcomes. Panel B shows an effect on reported turnout, defined as a ratio of the total cast votes over the number of registered voters. Polling stations with more than 1,000 registered voters eligible for video monitoring have an approximately two percentage points lower reported turnout. Hence, fewer votes were cast at the eligible polling stations. Panel C shows that the difference in reported turnout leads to approximately 22 fewer votes cast for the incumbent. At the same time, Panel D shows that there is no difference in the total votes cast for other candidates.

Panels B–D of Table 2 provide reduced form and second-stage estimates with and without region and election commission fixed effects. Column (3) of Panel B shows that polling stations equipped with video monitoring have 3.5 percentage points lower reported turnout (significant at the 1% level, std. error = 0.9). This effect corresponds to an approximately 5.2% decrease, given a mean turnout of 67.7%. Column (3) of Panels C and D shows that video monitoring leads to a reduction in the number of votes cast for the incumbent and no changes in the total votes cast for other candidates. Video-monitored polling stations have around 42 fewer votes cast for the incumbent compared to unmonitored polling stations (significant at 1% level, std. error = 9.4). It is equivalent to an approximately 8.3% decrease, given an average of 503 votes.

Appendix Table A.2 shows that these effects lead to an approximately one percentage point reduction in the incumbent's vote share and margin of victory.[23] Appendix Table A.3 shows no significant effects of video monitoring on the number of votes cast for each non-incumbent candidate.

These effects are not consistent with the hypothesis of voter intimidation. There is no effect on votes cast for the non-incumbent candidates who are more likely to be supported by opponents of the ruling party. Moreover, there is a negative effect on the votes cast for the incumbent. This result is inconsistent with the findings of Frye, Reuter and Szakonyi (2014) and Frye, Reuter and Szakonyi (2019*a*), who show that in non-democratic countries like Russia, local authorities and public employers induce citizens to vote in favor of the incumbent. Section VII. of this paper provides additional evidence from a survey experiment that information about video monitoring does not affect voter intimidation measured in a list experiment.

In contrast, these effects are consistent with a reduction in election fraud, which is shown to be more prevalent in rural areas of Russia (Myagkov and Ordeshook, 2008). Columns (4)–(5) of Table 2 test whether video monitoring has larger effects in rural areas. Column (4) shows that video monitoring leads to a significant 4.4 percentage points

[23] Detecting impacts on these variables is challenging because video monitoring reduces the total votes (the denominator) and the votes cast for the incumbent (the numerator).

(std. error = 1.3) or 6.3% decrease in reported turnout at the rural polling stations. This effect corresponds to an 8.2% decrease in the number of votes cast for the incumbent. At the same time, video monitoring leads to an insignificant two percentage points (std. error = 1.3) or 3% decrease in reported turnout at the urban polling stations. This corresponds to a 5.7% decrease in the votes cast for the incumbent. Hence, the effects of video monitoring are larger in rural areas, more susceptible to election fraud, where it covers only 14% of rural polling stations (36% of registered voters). The latter might signal a strategic decision not to install video monitoring in rural areas to reduce its effectiveness in preventing fraud. A back-of-the-envelope calculation indicates that the universal adoption of video monitoring would result in a 55% higher loss in votes compared to the status quo implementation.[24]

Appendix Section E. provides evidence from a series of placebo tests to check identification assumptions. First, Figure A.6 and Table A.8 show that there are no discontinuities in election outcomes at the threshold of 1,000 registered voters using data from the 2012 presidential election. That year, video monitoring covered almost all polling stations, irrespective of their sizes. Second, Table A.9 shows that there are no placebo effects using alternative thresholds of 850 and 1,150 registered voters.

Appendix Section F. shows robustness of the results to alternative specifications. Table A.10 shows that the estimates are only slightly smaller when not excluding intermediate points in the interval of 975–1,000 registered voters. Tables A.12 and A.11 show that the estimates are similar when using alternative functional forms (quadratic, cubic, and local linear) and bandwidths (300, 500, and optimal according to CCT), respectively.

C. Mechanisms

The previous section indicated that the effects of video monitoring on voting are consistent with a reduction in election fraud, which is more prominent in rural areas. This section tests further testable implications of that mechanism.

First, I use detailed records from the election day to understand whether the effects on votes come from regular, early, and mobile voting or voter registration. Table 3 shows that all polling stations receive approximately the same number of ballots before the election day (point estimate = −4.6, std. error = 9.3). However, monitored polling stations use 39 fewer ballots (std. error = 9.1), which is statistically equivalent to a reduction in votes cast for the incumbent. This difference comes from fewer ballots cast into a stationary ballot box located inside polling stations on election day. There are no significant differences in

[24] This calculation assumes an 8.2% decrease in votes at all rural polling stations and a 5.7% decrease in votes at all urban polling stations, irrespective of their size. This results in an additional 291 thousand votes reduction compared to the current loss of 533 thousand votes.

votes cast during early and mobile voting, collected in a carry box outside of the view of the webcam.[25] There are also no differences in the number of invalid and lost ballots.

Table 4 shows that there are, at most, small effects on voter registration and its possible manipulation.[26] The direction of the effects is consistent with monitored polling stations removing more existing voters and adding fewer new voters to the records. However, these effects are imprecise and small compared to the reduction in votes.

Second, I use election forensics tools that quantify election fraud by detecting abnormalities in the distribution of election results. I follow Klimek et al. (2012), Kobak, Shpilkin and Pshenichnikov (2016), and Lacasa and Fernández-Gracia (2019), who show that election fraud can be detected by a strong positive relationship between reported turnout and winner's vote share and a large mass of polling stations with abnormally high values of both variables.

Figure 4 shows a graphical representation of this tool for polling stations in the bandwidth of 400 registered voters. Polling places with fewer than 1,000 registered voters, ineligible for video monitoring, have a strong positive relationship between the reported turnout and incumbent's vote share. Moreover, there is a large mass of polling stations with abnormally high values (greater than 80%) of both variables. However, these abnormalities are much smaller at polling places with more than 1,000 registered voters, eligible for video monitoring.

Table 5 quantifies the effects of video monitoring on the probability of abnormally high (greater than 80%) values of turnout and incumbent's vote share. It shows that 17% of polling stations have high turnout, 27% have high incumbent's vote share, and 12.5% have high values of both variables. Video monitoring significantly reduces the probability of these abnormalities. For example, it decreases the likelihood of both abnormalities by 8.8 percentage points (significant at 1% level, std. error = 3.1). This effect is equivalent to a 70% reduction in the occurrence of abnormally high results. Moreover, columns (4) and (5) show that these abnormalities are four times more likely to happen at rural polling stations (18.7% versus 4.9% probability). Video monitoring eliminates them in urban areas and reduces them by half in rural areas.

These results are consistent with the hypothesis that video monitoring reduces election fraud, which is more prevalent in rural areas. In particular, they are compatible with a reduction in ballot-box stuffing (i.e., fraudulently casting ballots for the incumbent using those of registered voters who did not turn out to vote). In contrast, video monitoring does not affect another type of manipulation known as vote-count fraud (i.e., changing

[25] Voters can request early voting when they cannot visit an assigned polling station on election day because of vacations, business trips, work, study, public duties, or poor health conditions. Similarly, voters can request mobile voting at their residence because of poor health conditions, disabilities, or house arrest.

[26] Voters can officially change an automatically assigned polling station based on their residence to another polling station 45 days before the election.

the results during the vote count process). Election forensics literature measures this fraud by the probability of rounded results (i.e., results within a 0.05 margin of an integer number). Appendix Table A.5 shows that around 11% of polling stations experience this abnormality with no effects from video monitoring. Overall, these results indicate that video monitoring reduces ballot-box stuffing but does not eliminate all types of fraud.

Third, I show that the effects of video monitoring are stronger in election commissions with a higher winner's vote share in the 2012 and 2008 presidential elections. Based on the above election forensics literature, fraud is more likely in areas with higher turnout and winner's vote share. Appendix Table A.7 shows that video monitoring reduces the turnout by 8% (6.4%) and incumbent's votes by 11.2% (10.6%) in election commissions with the above-median winner's vote share in the 2018 (2012) presidential elections. In contrast, it decreases the turnout by only 1.6% (3.8%) and incumbent's votes by 4.8% (5.6%) in election commissions with the below-median winner's vote share. Thus, video monitoring is more effective in areas with a historically higher possibility of election fraud.

Finally, I provide additional evidence using the distribution of reported turnout on election day. If all voters have a similar distribution of times at which they vote, we would expect to see a gradual widening of the gap in turnout between monitored and unmonitored polling stations. However, Appendix Figure A.5 and Table A.6 indicate a different pattern. A one percentage point gap in reported turnout emerges at 10 a.m., when only 10% of the voters cast their votes. Then it does not significantly change for several hours and grows again after 3:00 p.m., when most citizens have already cast their votes. This pattern is consistent with a reduction in ballot box stuffing rather than changes in voter behavior. Election fraud is more likely to happen at the beginning and end of the election day when polling stations are not crowded. Additionally, election administrators have better information about how many people did not turn out to vote at the end of the day, whose ballots can be used for stuffing.

Overall, multiple forms of evidence strongly indicate that video monitoring reduced election fraud, specifically ballot box stuffing. In contrast, the results weigh against the voter intimidation hypothesis. The following sections explore how local authorities and citizens react to the presence of video monitoring.

VI. Indirect Effects

This section explores whether the negative effects of video monitoring on voting trigger changes in the behavior of local authorities, election administrators and politicians. First, I examine whether they displace fraud to neighboring unmonitored polling stations. Second, I estimate whether the reduced scope for election fraud makes local officials more accountable to citizens in public goods spending.

A. Displacement Effects

Only 44% of polling stations were equipped with video monitoring in the 2018 presidential election. Since it is difficult to commit fraud inside monitored polling stations, local authorities might displace it to unmonitored polling places. For example, Ichino and Schündeln (2012) show evidence of the displacement of fraud to unmonitored polling stations in the presence of traditional election observers in Ghana. Rundlett and Svolik (2016) show that in Russia and other non-democracies, election fraud is conducted not centrally by incumbents but rather locally by election commissions. Thus, the displacement of votes is more likely to happen inside the existing geographical boundaries.

To identify the displacement effects, one ideally wants to compare election results at unmonitored polling stations whose neighbors were randomly assigned video monitoring to voting outcomes at unmonitored polling places whose neighbors were randomly assigned to not be monitored. In the absence of random assignment, I develop an auxiliary empirical strategy based on quasi-random variation in the implementation of video monitoring. First, I take all unmonitored polling stations and their neighbors inside a geographical radius of three, five, or seven kilometers (depicted inside the red circle in Appendix Figure A.7). Second, I exclude unmonitored polling stations that have neighbors with more than 1,400 registered voters, the upper bandwidth boundary (shown in Panel C of Appendix Figure A.7). These neighbors were eligible for video monitoring, and hence, there could have been a displacement of fraud from them. However, they are outside of my identification window.[27] Third, I exclude neighbors with fewer than 600 registered voters, the lower bandwidth boundary (shown in red inside the red circle in Appendix Figure A.7). They were not eligible for video monitoring, and they are also outside of my identification window. Finally, I restrict the sample to unmonitored polling stations with the neighbors on only one side of the cutoff of 1,000 registered voters (i.e., all neighbors should have the same eligibility status). While these assumptions are quite restrictive, they allow me to causally estimate displacement effects.

To summarize, I compare the outcomes of unmonitored polling stations, which have all their neighbors in the bandwidth eligible for video monitoring (Panel A of Appendix Figure A.7), to the outcomes of unmonitored polling places, which have all their neighbors in the bandwidth ineligible for video monitoring (Panel B of Appendix Figure A.7). When several neighbors are on the same side of the cutoff, I take the average number of voters and the probability of video monitoring. The following equations summarize the empirical strategy:

$$\bar{m}_s^{ngh} = \alpha_1 + \mathbb{1}\{\bar{v}_s^{ngh} \geq c\}\left[f_r(\bar{v}_s^{ngh} - c) + \delta\right] + \mathbb{1}\{\bar{v}_s^{ngh} < c\}f_l(c - \bar{v}_s^{ngh}) + \varepsilon_{1s} \quad (3)$$

$$y_s^{unm} = \alpha_2 + \tau\bar{m}_s^{ngh} + \mathbb{1}\{\bar{v}_s^{ngh} \geq c\}g_r(\bar{v}_s^{ngh} - c) + \mathbb{1}\{\bar{v}_s^{ngh} < c\}g_l(c - \bar{v}_s^{ngh}) + \varepsilon_{2s} \quad (4)$$

[27] The neighboring polling stations were as good as random to receive video monitoring only if the number of registered voters was close to the eligibility cutoff of 1,000.

where y_s^{unm} is election outcome at the unmonitored polling station s, $\bar{m}_s^{ngh}$ is the mean probability of video monitoring of its neighbors, $\bar{v}_s^{ngh}$ is the mean number of registered voters of the neighbors, c is a cutoff of 1,000 registered voters, and f and g are unknown functions. The first-stage estimate δ estimates a jump in the mean probability of video monitoring among neighboring polling stations at the threshold of 1,000 registered voters. The second-stage estimate τ estimates the displacement effect from monitored neighbors to the unmonitored polling station s.

Figure 5 plots first-stage and reduced-form estimates of displacement effects in the radius of five kilometers. Panel A shows that neighboring polling stations with more than 1,000 registered voters have an approximately 60 percentage points higher probability of video monitoring. This estimate is consistent with the first-stage estimates in the main results. Panel B shows that unmonitored polling stations, which have neighbors with more than 1,000 registered voters, have approximately the same turnout. However, Panel C shows that they have a 3.7 percentage points higher incumbent's vote share.[28] Symmetrically, Panel D shows that they have a 3.7 percentage points lower share of votes cast for other candidates.

Table 6 provides the reduced and second-stage estimates for a radius of three, five, and seven kilometers. Panel B confirms that there is a slightly positive but not statistically significant effect on turnout (point estimate = 1.1, std. error = 2.8). Panel C shows that unmonitored polling stations with monitored neighbors have an approximately 6.7 percentage points higher incumbent's vote share compared to unmonitored places with unmonitored neighbors (significant at the 1% level, std. error = 2.3). This corresponds to an approximately 8.6% higher vote share, given a mean of 77.9%. Panel D shows that this increase leads to a symmetric decrease in the percentage of votes cast for other candidates. The displacement effects reach the highest value in the radius of five kilometers and decrease for longer radii. This is consistent with the idea that displacement effects are bounded by the existing geographical limits.

Appendix Table A.13 shows the robustness of these results to pooling all neighboring polling stations instead of taking the means. Appendix Table A.14 shows the robustness to restricting the sample to unmonitored polling stations that have a single neighbor in the bandwidth of 600–1,400 voters.

Overall, these effects are consistent with the displacement of fraud from monitored to neighboring unmonitored polling stations. The likely mechanism is vote-count fraud, given no significant effects on turnout. A back-of-the-envelope calculation suggests that it corresponds to approximately 25 votes for an average unmonitored polling station. In total, fraud displacement compensates for around 330 thousand votes, more than half of

[28] It is not possible to estimate displacement effects in levels because there is no clear relationship between votes cast at the unmonitored polling station and the size of its neighbors (the running variable).

the direct impact of video monitoring shown in Table 2.[29]

B. Accountability Effects

Fair elections are the primary accountability mechanism that helps align the incentives of citizens and those of their government (Besley, 2006). Thus, the net benefits of video monitoring depend on whether it makes authorities more accountable to citizens. Since video monitoring reduces the scope for election fraud, it can motivate politicians to provide more public goods before the election and increase incumbent's support.

To test this hypothesis, I collected data on all public procurement contracts signed at the local level. I aggregated the number and value of all the procured projects at the district level and matched them to the election records. To estimate the effects of video monitoring on public goods spending, I exploit the variation in the intensity of video monitoring across districts (*rayons*), shown in Figure A.8. Specifically, I examine whether districts with a higher share of video-monitored polling stations spend more on public goods before the election. Since the information about video monitoring is available only around four months before the election, I use a difference in total per capita spending between a period of four months before the 2018 presidential election (December 2017 – March 2018) and a corresponding period a year earlier (December 2016 – March 2017), which allows to absorb the time-invariant differences in spending across districts. To address the potential endogeneity of the intensity of video monitoring, I instrument it with the share of polling stations with more than 1,000 registered voters in the bandwidth. The following equations summarize the empirical strategy:

$$\text{Share Monitored}_{dr} = \alpha + \delta \cdot \text{Share Above Cutoff BW}_{dr} + \eta \cdot \text{Share BW}_{dr} + \theta_r + \epsilon_{dr} \quad (5)$$

$$\Delta\text{Spending}_{dr} = \beta + \tau \cdot \text{Share Monitored}_{dr} + \gamma \cdot \text{Share BW}_{dr} + \mu_r + \varepsilon_{dr} \quad (6)$$

where Share Monitored$_{dr}$ is a share of polling stations equipped with video monitoring in the district d of the region r. Share Above Cutoff BW$_{dr}$ is a share of polling stations with more than 1,000 registered voters in the bandwidth, an instrument for Share Monitored$_{dr}$. Share BW$_{dr}$ is a share of polling stations in the bandwidth that controls for a non-random distribution of polling station sizes. The second-stage estimate τ estimates the impact of a 1% increase in the share of monitored polling stations on changes in district public spending.

The exclusion restriction requires that the instrument affects changes in public spending before the election only through the effect of video monitoring. While the assignment of webcams is quasi-experimental, the number of polling stations with 600–1,400 registered voters in a particular district may not be a random event. To address this concern, I control for the share of polling stations in the bandwidth, Share BW$_{dr}$, in both stages.

[29] This calculation assumes a uniform displacement effect on the incumbent's vote share of 6.7 percentage points across all unmonitored polling stations.

Table 7 presents the results. Panel A shows that the instrument strongly predicts the total share of monitored polling stations. A 10 percentage points increase in the share of polling stations with more than 1,000 registered voters in the bandwidth of 400 registered voters leads to a 4.7 percentage points increase in the share of monitored polling stations (significant at the 1% level, std. error = 0.3).

Panels B and C show that, on average, districts start fewer projects and spend around RUB 300 (approximately $4) less per capita before the election. However, they finish roughly the same number of projects with the same value by March. There are no significant effects of the intensity of video monitoring on changes in the number of projects and their value. The point estimates are -0.1 (std. error = 0.1) for the number of projects and -5.7 (std. error = 6.5) for the total value of projects in RUB. These estimates are close to zero and not statistically significant (confidence intervals include zero). Both rural and urban districts experience similar patterns.

Additionally, Appendix Table A.15 shows the heterogeneous effects by major spending sectors. On average, districts do not substantially change spending across sectors. There are also no effects of the intensity of video monitoring on spending changes. Thus, video monitoring does not lead to substitution effects across sectors.

Overall, the data show that video monitoring has no significant impact on public goods spending. Most likely, this is because it does not have large effects on the election results and leaves room for the displacement of votes to unmonitored polling stations.

VII. Why Video Monitoring?

The regression discontinuity analysis indicates that video monitoring reduces election fraud committed in favor of the incumbent. However, local authorities partially offset these effects by displacing fraud to neighboring unmonitored polling places. This raises the question: why would autocrats want to implement video monitoring? This section reports the results of an original survey experiment used to adjudicate between competing theories.

A. Improving Attitudes Toward the Regime

"Opposition will always indicate that the election was unfair. Always! ... To minimize the possibility of indicating that these [2012 presidential] or future elections will be dishonest or maybe dishonest, to knock out those who want to delegitimize the power in the country ... I propose and ask the Central Election Commission to set up webcams in all polling stations in the country ... so that the country sees what happens at a particular [ballot] box. To completely remove all falsifications on this matter."

Prime Minister of Russia
Annual Call-In Show
December 15, 2011

The Russian prime minister (autocrat) gave this speech just a few days after the largest protests since the fall of the USSR, which were sparked by numerous allegations of election fraud in the 2011 legislative elections.[30] His statement indicates that the government introduced video monitoring to stop the spread of beliefs about the unfairness of elections that de-legitimized the regime and triggered protests.

Consistent with the fears of the Russian government, Tucker (2007) and Bunce and Wolchik (2011) show that electoral irregularities initiated revolutions in several post-communist countries, including Georgia, Kyrgyzstan, Ukraine, and Serbia. A more recent example comes from Bolivia, where the incumbent president had to step down after losing control over violent protests sparked by allegations of election fraud.[31]

Acemoglu and Robinson (2005) model autocrats who concede to voters and invest in democratic institutions to prevent a revolution and regime change. Fearon (2011) theoretically shows the importance of investments in electoral institutions to prevent discontent, such as holding elections on a regular schedule and monitoring their conduct. Little (2012) takes it further by showing that manipulations will always occur in the equilibrium under a broad set of assumptions. However, citizens will know about the existence of fraud and discount the result, rendering fraud ineffective. As a result, incumbents will invest in the monitoring of elections to reduce visible, inefficient fraud.

Survey Experiment Design. To test these theories, I conducted a survey experiment for a nationally representative sample of 1,097 respondents approximately two weeks before the 2019 local elections.[32] I first asked all respondents whether they knew about transparent ballot boxes, a proxy for awareness of transparency initiatives. I then introduced the priming treatment comprised of a reminder to a random half of respondents that many polling stations in the upcoming elections would have video monitoring. It also provided information that the government-run website was going to stream the election day online. The control group did not receive any information about video monitoring. After introducing the treatment, I inquired about respondents' voting intentions and beliefs, trust in elections, and general views on democracy. To estimate the effects on voter intimidation, I cross-randomized a list experiment, which included a veiled response treatment group indirectly asked about it. The table below summarizes the survey design:

Video Monitoring Priming Treatment

Do you know that many polling stations in the upcoming election will have webcams that will stream voting online? (Yes/No)

[30] For instance, see the article "Russian election: Biggest protests since fall of USSR," published by BBC News on December 11, 2018: https://www.bbc.com/news/.

[31] For example, see the article "Bolivia's president resigns amid election-fraud allegations," published on November 10, 2019, by AP News: https://apnews.com/.

[32] The survey was conducted on August 22–28; Single Voting Day was held on September 8.

The website nashvybor2019.ru will stream the election day online from all polling stations in the country equipped with webcams. You can visit nashvybor2019.ru on the election day and observe the voting at any polling station with video monitoring. Will you observe the election on nashvybor2019.ru? (Yes/No/Hard to tell)

	Video Monitoring Priming	
List Experiment	Control	Treatment
Direct Response	271	273
Veiled Response	288	265
Total	559	538

Results. I pool the list experiment groups together for all outcomes except voter intimidation. Equation (7) summarizes the empirical strategy:

$$\mathrm{Y}_{ir} = \alpha + \tau \,\mathrm{Treat}_{ir} + \varepsilon_{ir} \tag{7}$$

where Y_{ir} is the outcome for respondent i from region r, and $Treat_{ir}$ is an indicator equal to one if the respondent received the priming treatment about video monitoring. Table 8 shows the balance of covariates between the treatment and control groups. In line with expectations, only 1 out of 21 outcomes is significant at the 5% significance level. Appendix Table A.16 shows the balance of covariates across both the priming and listing treatments.

The last part of Table 8 shows that 60% of treated respondents acknowledged that they were already aware of video monitoring in the upcoming local elections. Hence, the priming treatment provides new information only for 40% of respondents and reinforces existing knowledge for the remaining 60%. Moreover, 15% of respondents expressed their willingness to observe the elections online using video monitoring.

Figure 6 shows the effects of the priming treatment on the primary outcomes: trust in election results, voting intentions, and beliefs about others' voting behavior. First, a reminder about video monitoring increases respondents' trust in election results from 29.6% in the control group to 34.4% in the treatment group. This effect corresponds to a 4.8 percentage points or 16% increase in trust (significant at the 10% level, std. error = 2.7). It primarily happens in the subsample of respondents unaware of another transparency tool, transparent ballot boxes. The latter has a moderate correlation of 0.45 with awareness about video monitoring and thus serves as a proxy for general knowledge about electoral transparency policies. Only 22.6% of respondents unaware of transparent ballot boxes believe in fair election results in the control group. The priming treatment increases this share by 6.7 percentage points, a 30% increase (significant at the 10% level, std. error = 3.6). In contrast, respondents who are already aware of transparent ballot boxes have a higher baseline level of trust in election results of 35.1%. The priming treatment slightly increases this share by 3.1 percentage points, an insignificant 9% increase (std. error = 3.3).

Second, a reminder about video monitoring also increases respondents' willingness to vote. In the entire sample, the share of respondents willing to vote increases from 60.8% to 63.6%. The effect is again mainly concentrated among unaware respondents. Around 49.2% of unaware respondents intend to vote in the control group. This share increases by 11.1 percentage points or 23% in the treatment group (significant at the 5% level, std. error = 4.5). In contrast, aware respondents have a higher baseline willingness to vote of 69.8%, which does not significantly change with the priming treatment (point estimate = −3.7, std. error = 4.2).

Finally, greater awareness of video monitoring leads to a higher willingness to vote for the incumbent United Russia party. About 25.8% of unaware respondents intend to vote for United Russia in the control group. This share increases by 8.5 percentage points or 33% in the treatment group (std. error = 5.9). On the other hand, aware respondents already had a higher baseline willingness to vote for United Russia of 44.5%, which is not significantly affected by the treatment (point estimate = −2.3, std. error = 4.9).

Table 9 shows the results for all pre-specified outcomes and includes p-values from Fisher's exact test and p-values adjusted for the multiple-hypotheses testing. The treatment effect on trust in election results is significant at the 10% level in both the full sample and the unaware subsample.[33] However, it does not survive the multiple-hypotheses testing because there is no effect on the second pre-specified outcome, beliefs that elections will lead to improvements. The treatment effect on the willingness to vote is significant at the 5% level and survives the multiple-hypotheses testing for the subsample of unaware respondents. The treatment effect on the willingness to vote for United Russia is borderline significant at the 10% level given the smaller number of respondents who answered this question. The total effect on the voting intentions index is 0.19 standard deviations, and it is significant at the 1% level for the unaware subsample.

In addition, Table 9 shows that there are no effects on the overall index of democratic values. However, the priming treatment leads to stronger beliefs that democracy has not yet been established in Russia. While 83% of respondents in the control group believe that Russia is at least partially a democracy, five percentage points fewer respondents believe so in the treatment group. These negative effects are present among both unaware and aware respondents. A possible explanation is that respondents might perceive some features of video monitoring as non-democratic. An alternative interpretation is that video

[33] The survey experiment was not powered to identify the effects at the 5% significance level. The existing capacities of the survey firm, their sampling of regions and settlements, and varying election cycles across geographical areas restricted the sample size to 1,097 respondents.

monitoring might remind voters that elections would remain unfair and non-democratic in its absence.

Appendix Figure A.9 and Table A.17 show that the priming treatment effects have larger magnitudes and higher precision in the subsample that excludes respondents residing in Moscow whose beliefs are harder to change. First, they have higher awareness of video monitoring because it has been present at all polling stations in most elections since 2012. Second, a larger share of residents have well-established views opposing the government.

Appendix Figures A.10 and A.11 show heterogeneous treatment effects on the primary outcomes using individual characteristics. Female, young, less educated, unemployed, and higher-income respondents have larger priming treatment effects on trust in the election results. However, these heterogeneous effects do not translate into a higher willingness to vote except for less educated respondents. In contrast, respondents who approve of the government, do not watch TV daily, and do not use the Internet have higher priming treatment effects on both the trust and willingness to vote.

Overall, these findings support the theory of autocrats using video monitoring to increase trust in elections and improve attitudes toward the regime. These effects are particularly strong among those who were unaware of other electoral transparency policies.

B. Alternative Theories

Convincing Voters of Government Popularity. Alternative theories rely on the premise that autocrats can invest in the conduct of elections to convince voters and elites of their popularity (Egorov and Sonin, 2021). They can use video monitoring to assure citizens that others genuinely support the regime by producing a picture of clean elections. I test this theory by measuring respondents' beliefs about what percent of the other respondents support candidates from the ruling United Russia party.

The last panel of Figure 6 shows that an average respondent in the control group believes that 47.8% of other respondents will vote for the candidates from the incumbent United Russia party. This share is ten percentage points higher than the actual number of respondents willing to vote for United Russia. Hence, respondents hold overly optimistic beliefs about the government's popularity. The priming treatment increases these beliefs by two percentage points (std. error = 1.4); this difference is small and not statistically significant.

Similarly, unaware respondents believe that 43.4% of others support the regime. The priming treatment increases this share by 3.3 percentage points (std. error = 2.2) or 7.6%. Aware respondents believe that 51.2% of others will vote for United Russia candidates. The priming treatment does not significantly change their beliefs (point estimate = 0.9, std. error = 2.0).

Overall, information about video monitoring slightly increases beliefs about the regime's popularity. However, these effects are not statistically significant and smaller in magnitude than the corresponding effects on trust in elections and respondents' own willingness to vote.

Intimidating Voters. Autocrats can also implement video monitoring to intimidate voters and coerce voting in favor of the regime. Frye, Reuter and Szakonyi (2014, 2019*a*,*b*) document that voter intimidation is a common practice in many non-democratic countries, including Russia. Even though webcams do not violate ballot secrecy, they capture whether people turn out to vote. As a result, citizens might feel intimidated that their employer or local authorities will know whether they voted. They might also have incorrect beliefs that webcams can capture their choice of candidates.

Several policy reports and media articles support these concerns. The Venice Commission and the OSCE Office jointly advised the government of Georgia to remove video monitoring. They wrote the following: "The use of recording devices in the polling station, even if it does not infringe on the secrecy of the ballot, may appear to do so and can also intimidate some voters. As such, this provision may have a chilling effect on suffrage rights, potentially leading to intimidation, fear, and coercion."[34] Similarly, media outlets reported that cameras inside polling stations intimidated Arab voters and lowered their turnout in the 2019 Israeli elections.[35]

As I discussed in Section V., the direct effects of video monitoring on election outcomes are inconsistent with voter intimidation. First, video monitoring has a negative impact on the incumbent's votes. It contradicts the hypothesis of voter intimidation, which usually takes form of coercing votes in favor of the incumbent in non-democratic regimes. Second, video monitoring does not affect the votes cast for the opposition candidates. Hence, there is no suppression of opposition supporters.

I provide additional evidence against voter intimidation by interacting the priming treatment with the cross-randomized list experiment, a survey method used to mitigate respondents' social desirability bias when eliciting information about sensitive topics. The list experiment randomly split respondents into two groups and presented them with a list of statements. The direct response group received three statements about the election. The veiled response group received the same items plus an additional sensitive statement on voter intimidation (shown below). Afterward, all respondents were asked to indicate only the total number of statements with which they agreed. A difference between average responses in these two groups indicates the level of voter intimidation.

[34]Source: Council of Europe, Venice Commission. Opinion No. 571/2010: Joint Opinion on the Election Code of Georgia. Strasbourg/Warsaw, June 9, 2010. Page 17: https://www.venice.coe.int/.

[35]For example, see the article "Minority Arabs in Israel object to cameras at polling centers," published by Reuters on April 9, 2019: https://www.reuters.com/.

Cross-randomization allows me to estimate the overall level of voter intimidation and how it changes with a reminder about video monitoring. Equation (8) shows the empirical strategy:

$$\#\ \text{Statements}_{ir} = \beta_0 + \beta_1\ \text{Treat}_{ir} + \beta_2\ \text{Sens}_{ir} + \beta_3\ \text{Treat}_{ir} \times \text{Sens}_{ir} + \epsilon_{ir} \tag{8}$$

where Treat_{ir} is equal to one if respondent i from region r received the priming treatment about video monitoring, Sens_{ir} is equal to one if the respondent received a sensitive statement in the list experiment, and β_3 captures the effect of priming on voter intimidation.

List Experiment

You will now see several statements. You do not need to state whether you agree with each one or not. Instead, please indicate the total number of items you agree with after reading all of them.

1. I saw a campaign ad on TV or heard one on the radio.
2. My polling station is within walking distance of my house.
3. The electoral campaigns annoy me.

4 (Sensitive). My employer or local authorities might know whether I voted or not.

The first row of Table 10 shows the level of voter intimidation among respondents who did not receive a reminder about video monitoring. Around 26% of them feel intimidated by the perception that the local authorities or their employer might know whether they voted or not. This estimate is in line with the findings of Frye, Reuter and Szakonyi (2014, 2019*a*,*b*), who show that up to 17% of employees experience coercion in Russia. Moreover, while 30% of unaware respondents experience voter intimidation, only 22% of aware respondents experience it. This is a first piece of evidence against the theory of voter intimidation because awareness about transparent ballot boxes is predictive of knowledge about video monitoring.

The second piece of evidence comes from the interaction effect displayed in the third row of Table 10. The interaction effect is positive, but it is not statistically significant in the full sample (point estimate = 0.06, std. error = 0.11). This means that a reminder about video monitoring does not have a significant effect on voter intimidation. Moreover, there is a precisely zero effect for the respondents who were unaware of other transparency initiatives (point estimate = −0.00, std. error = 0.14).

Overall, the list experiment designed to measure voter intimidation shows no evidence of it being affected by video monitoring. These results are consistent with the conclusions presented earlier in section V..B., which shows no effect on the votes cast for opponents.

Collecting Local Information. Autocrats can also invest in electoral transparency to gather information on the regime's true support and the performance of local authorities, resolving the so-called Dictator's Dilemma (Wintrobe, 2000). Miller (2015) provides cross-country evidence that negative electoral shocks help autocrats to adjust their public spending. Malesky and Schuler (2011) and Martinez-Bravo et al. (2017) show that elections help autocrats to monitor local officials.

However, it is unlikely that autocrats introduce video monitoring to collect local information. First, this theory provides a reasonable explanation for investing in the transparency of local elections. However, it does not thoroughly explain the need for video-monitoring the presidential election. As Egorov and Sonin (2021) note, a nationally representative poll would be a much cheaper and less risky way to gather the information.

Second, video monitoring does not entirely eliminate election fraud, which persists at unmonitored polling stations. If the government was interested in collecting accurate information, it would install video monitoring at all polling stations or use a technology that local authorities could not manipulate with other types of fraud.

Third, video monitoring does not discipline local politicians to be more accountable to citizens before the election. Hence, the central government cannot use it to identify and remove disloyal or poor-performing local officials.

Appeasing the International Community. Alternatively, non-democracies might invest in electoral transparency to appease the international community. First, international donors can put pressure on non-democratic regimes by making foreign aid conditional on fair elections (Wright, 2009). Second, international observers can serve the role of referees by revealing information on the fairness of elections and supporting or refuting the opposition's claims about election fraud (Hyde and Marinov, 2014).

However, Russia is unlikely to invest in the video monitoring of elections to attract foreign aid. It has one of the lowest debt-to-GDP ratios globally, equal to 17.8% in December 2020. Russia is also a foreign aid donor rather than its recipient. The Organization for Economic Co-operation and Development (OECD) reports that Russia provided USD 1.2 billion in official development assistance in 2019.[36]

Similarly, international election observers do not consider video monitoring to resolve all electoral irregularities in Russia. The primary election observation mission organized by the OSCE Office for Democratic Institutions and Human Rights (OSCE/ODIHR) acknowledges the efforts of the Russian Central Election Commission to increase the transparency by installing web cameras in the 2012 presidential elections. However, it also highlights that "there are inherent limitations as to what web cameras can and cannot capture, and therefore, from an outset, they cannot be regarded as an ultimate safeguard against any possible manipulations."

[36]Source: Organization for Economic Co-operation and Development (OECD), Development Co-Operation Profiles: www.oecd-ilibrary.org.

VIII. Conclusion

The last decade saw the backsliding of democracy worldwide. Modern authoritarian regimes mimic core democratic institutions. They regularly conduct elections at many levels of government and invest significant resources in improving their transparency. This paper studies why autocrats want to hold or appear to hold transparent elections by evaluating the effects of broadcast video monitoring of polling stations in Russia.

I estimate the impacts of this technology on voting by exploiting a discontinuity in the assignment of webcams to polling stations in the 2018 presidential election. I find that video monitoring reduces reported voter turnout by 5.2% and votes for the incumbent (autocrat) by 8.3%. Additional analysis suggests that these effects are consistent with a reduction in election fraud rather than an increase in voter intimidation. However, the decrease in the incumbent's votes is partially offset by increased election fraud in neighboring unmonitored polling stations. As a result, video monitoring does not lead to significant accountability effects on public goods spending.

To understand why autocrats implement video monitoring, I conducted a nationwide survey experiment before the 2019 Russian local elections. Treated respondents were informed of video monitoring, which significantly increased the trust in elections and willingness to vote among those not previously aware of transparency technologies. At the same time, information about video monitoring did not affect voter intimidation or beliefs about the support of the ruling party by others. Taken together, these results suggest that video monitoring allows autocrats to improve citizens' attitudes by creating an illusion of transparency at a low cost in terms of net lost votes.

These findings provide policy implications for international donors and civil society organizations that actively invest in the transparency of elections, including USAID, the Carter Center, the National Democratic Institute, and the ODIHR. My results show that authoritarian governments might use accountability-enhancing technologies to create a facade of transparency and influence citizens' beliefs and behavior. International organizations should carefully evaluate whether their activities, including election observation, help strengthen non-democratic regimes rather than promote democracy.

Similarly, international donors should assess the possible indirect effects of technologies designed to improve the transparency of other public institutions before sponsoring them to hybrid and non-democratic regimes. A mere introduction of digital technologies might not reach the intended goals in states with weak institutions. Such technologies might leave room for the manipulation of their use without proper sanctions.

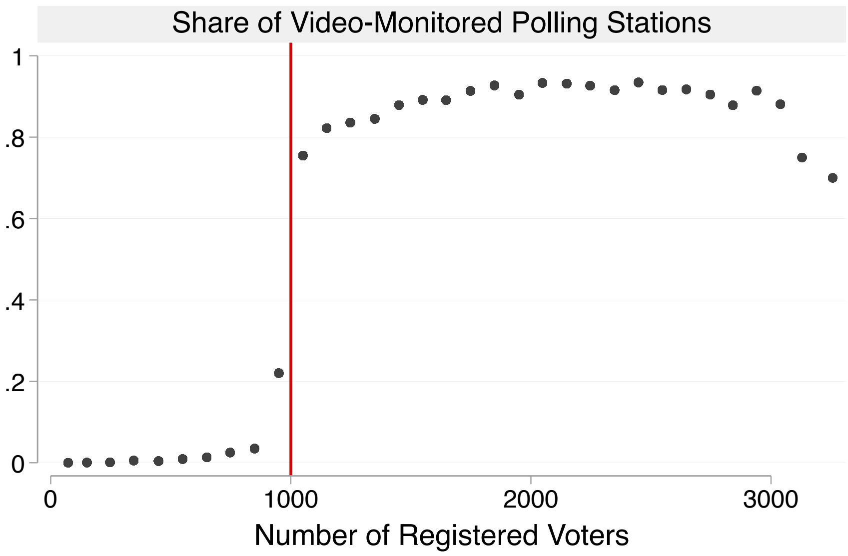
Overall, these conclusions support the theoretical literature (Besley and Persson, 2011; Acemoglu and Robinson, 2012) and emerging policy views (World Bank, 2016) that states and international donors should first invest in building cohesive democratic institutions to reap the dividends of digital technologies for the general population.

AcknowledgmentsAcknowledgments

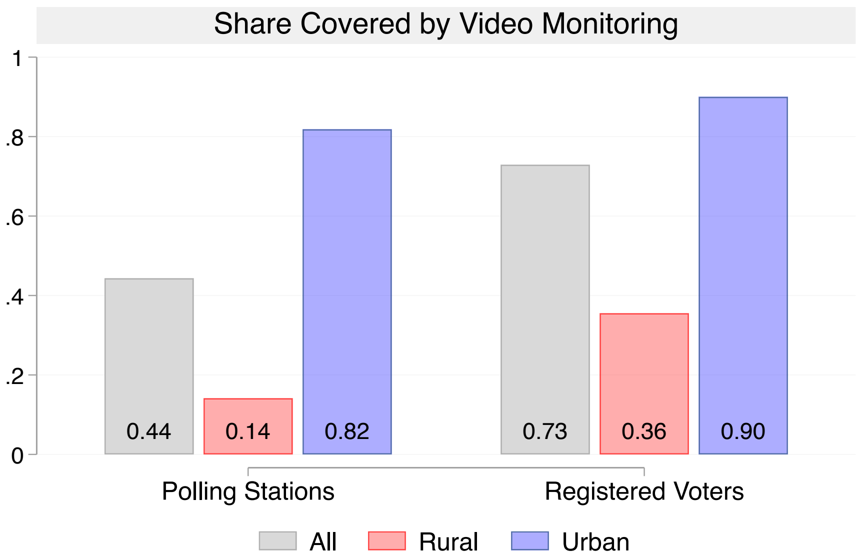
Figures and Tables

Figure 1: Video Monitoring Implementation

Panel A:



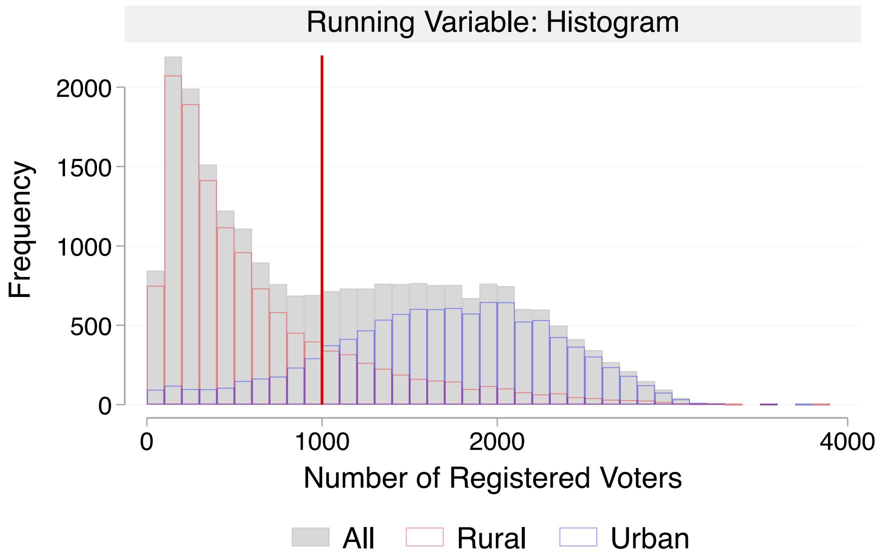
Panel B:



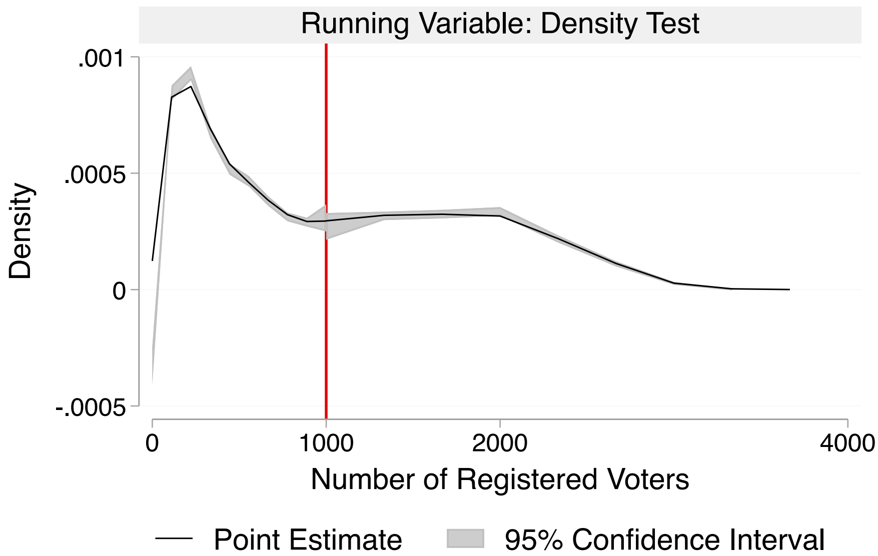
Notes: This figure shows the implementation of video monitoring in the 2018 presidential election. Panel A plots the share of video-monitored polling stations over their sizes defined by the number of registered voters. Each point is an average of observations in bins of 100 registered voters. Five outlying polling stations with more than 3,300 registered voters are excluded for presentation purposes. The red vertical line depicts the cutoff of 1,000 registered voters, which defined eligibility for video monitoring. Panel B plots the share of polling stations and registered voters covered by video monitoring.

Figure 2: Manipulation Tests

Panel A:

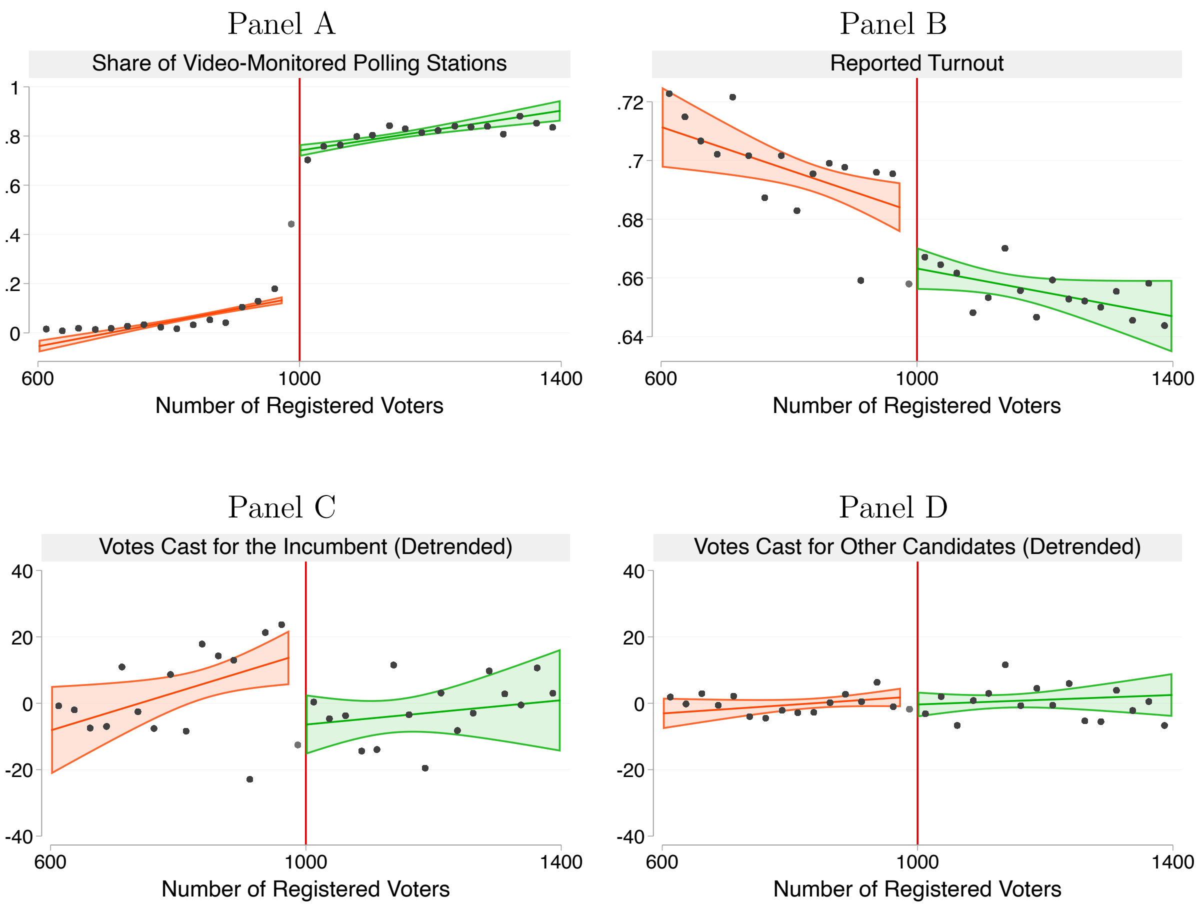


Panel B:



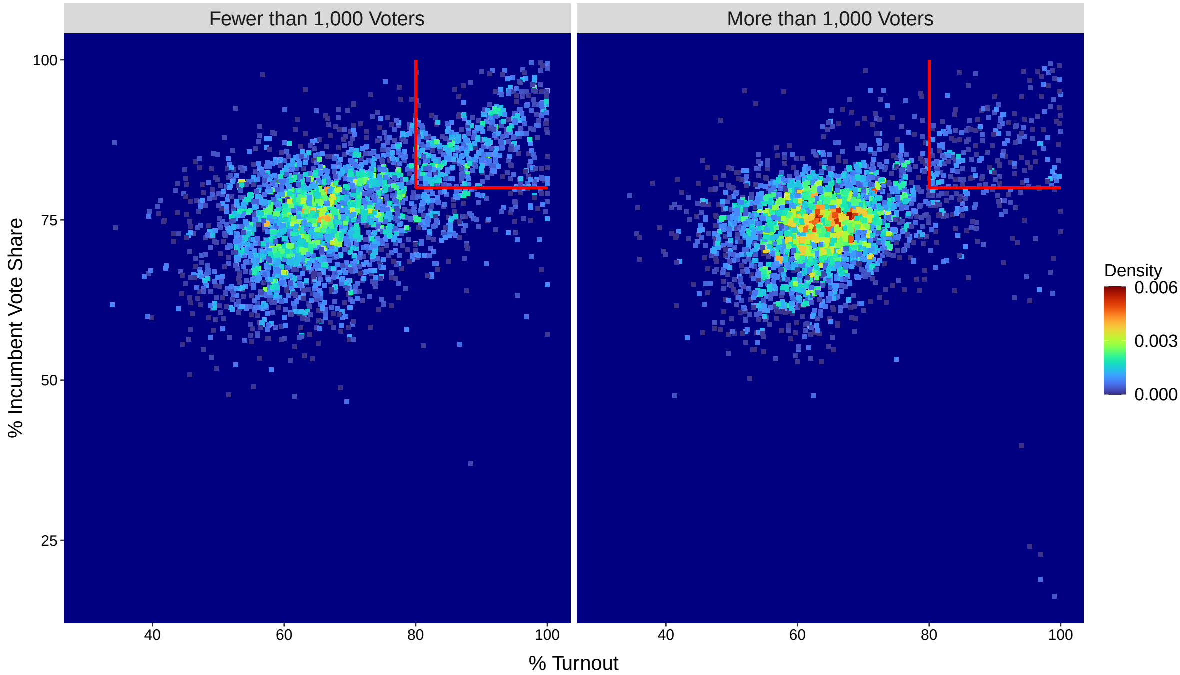
Notes: This figure shows manipulation tests of the running variable. Panel A plots a histogram of the number of registered voters in bins of 100 for the full sample and urban and rural subsamples. Panel B plots a non-parametric density function of the number of registered voters on both sides of the cutoff. The manipulation density test based on McCrary (2008) fails to reject the null hypothesis of no discontinuity around the cutoff of 1,000 registered voters (test statistic = -0.8, p-value = 0.42).

Figure 3: Effects of Video Monitoring on Voting



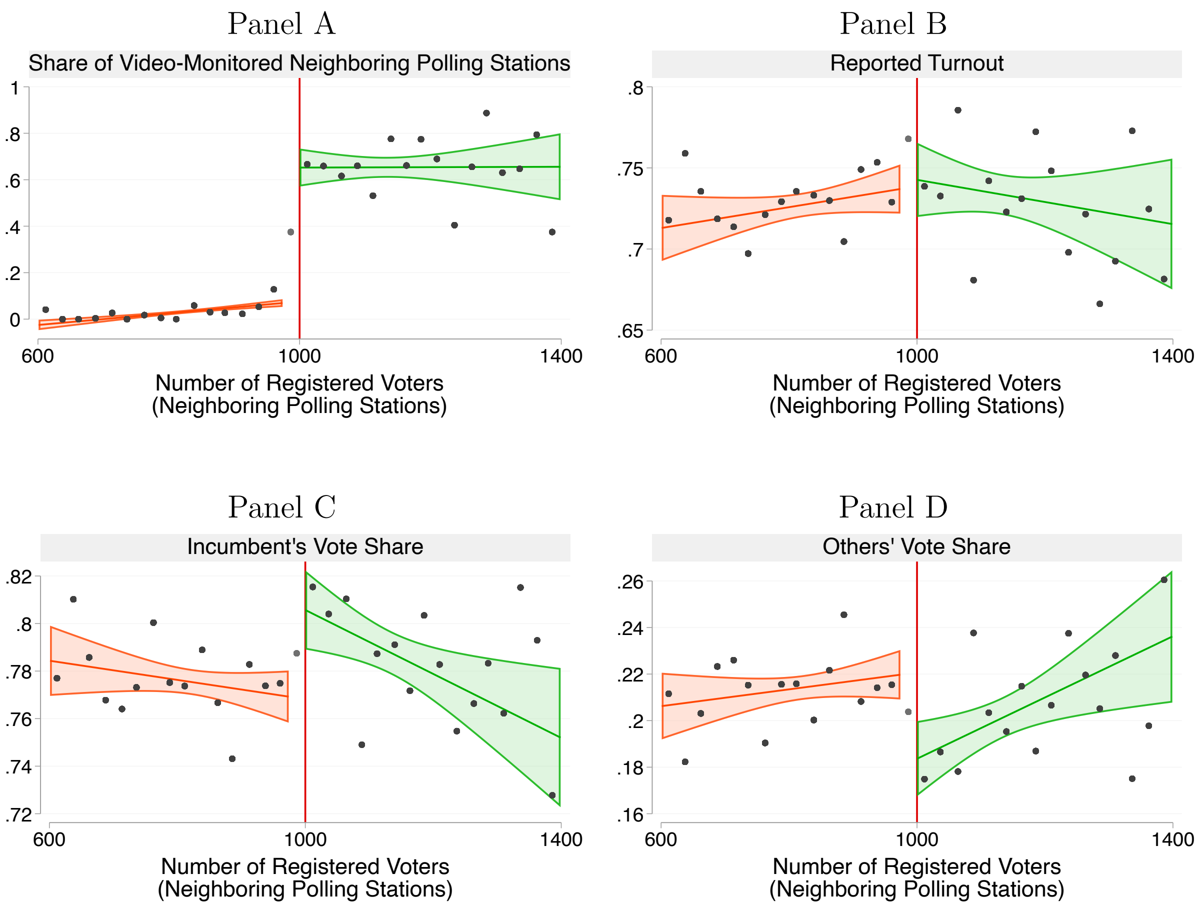
Notes: This figure plots first-stage and reduced-form estimates of the impact of video monitoring on voting. Each point plots means of observations in bins of 25 registered voters. The solid lines plot predicted values and 95% confidence intervals of a linear regression estimated separately on each side of the cutoff within the bandwidth of 400 registered voters. For presentation purposes, detrended graphs difference out trends associated with a positive relationship between the number of votes and registered voters.

Figure 4: Effects of Video Monitoring on Fraud Indicators



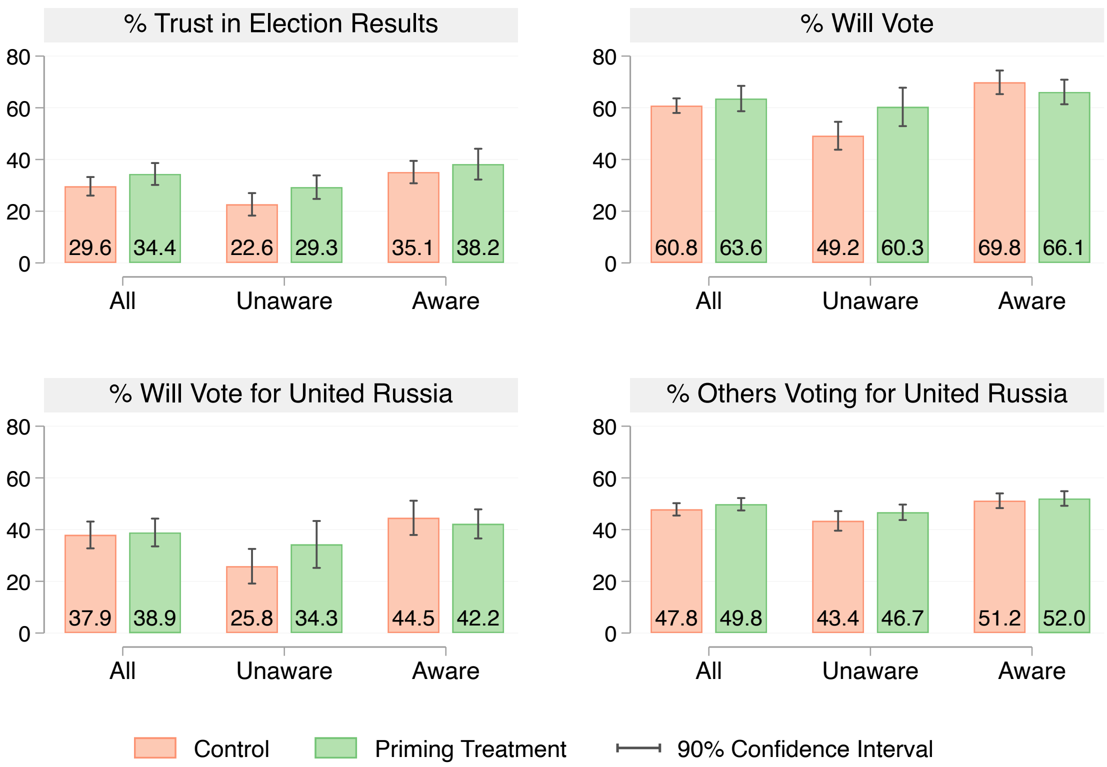
Notes: This figure shows the association between eligibility for video monitoring and the share of polling stations with abnormally high values ($\geq$ 80%) of reported turnout and incumbent vote share, indicators of fraud based on election forensics literature (Klimek et al., 2012). Each point corresponds to a joint density of the reported turnout and incumbent vote share in bins of one percentage point. The sample includes polling stations in the bandwidth of 400 registered voters on both sides of the cutoff. Red lines depict the turnout and vote share of 80%. The Pearson's correlation between two variables equals 0.55 for polling stations with fewer than 1,000 registered voters (ineligible for video monitoring) and 0.41 for polling places above this cutoff (eligible for video monitoring).

Figure 5: Vote Displacement



Notes: This figure shows the displacement of votes to unmonitored polling stations. Panel A plots the share of unmonitored neighboring polling stations as a function of their number of registered voters. Panels B-D plot election outcomes at unmonitored polling stations as a function of the number of registered voters at neighboring polling places. This estimation (i) excludes unmonitored stations, which have neighbors with more than 1,400 voters (upper bandwidth boundary); (ii) excludes neighbors with fewer than 600 registered voters (lower bandwidth boundary); (iii) excludes neighbors outside of the 5 km radius from unmonitored polling stations; (iv) restricts the sample to unmonitored polling places, which have all neighbors on one side of the cutoff; (v) takes the average number of voters if there are several neighbors. Each point plots means of observations in bins of 25 registered voters. The solid lines plot predicted values and 95% confidence intervals of a linear regression estimated separately on each side of the cutoff within the bandwidth of 400 registered voters.

Figure 6: Survey Experiment: Effects of Priming on Voting and Attitudes



Notes: This figure plots the effects of the priming treatment, which informed respondents about video monitoring during the survey experiment. The sample consists of 1,097 respondents who answered that there were going to be elections in their locality. Each subfigure plots the effects for the full sample (All), the subsample of respondents who were not aware of transparent ballot boxes (Unaware), which serves as a proxy for awareness of video monitoring (correlation of 0.45), and the subsample of respondents who were aware of it (Aware). Brackets denote 90% confidence intervals using standard errors clustered by region.

Table 1: Summary Statistics: 2018 Presidential Election

Statistic:	Mean	Std.Dev.	Median	Min	Max	# Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Urban	0.45	0.50	0.00	0.00	1.00	23301
Registered Voters	1088.26	794.71	955.00	16.00	4172.00	23301
Reported Turnout	0.69	0.13	0.66	0.27	1.00	23299
Incumbent's Vote Share	0.76	0.08	0.76	0.06	1.00	23299
Incumbent's Vote Margin	0.63	0.14	0.64	-0.38	1.00	23299
Share of Votes Cast Inside	0.90	0.10	0.93	0.00	1.40	23299
Share of Votes Cast Outside	0.10	0.10	0.07	0.00	1.00	23299
Share of Early Votes	0.00	0.03	0.00	0.00	0.98	23299
Share of Invalid Votes	0.01	0.01	0.01	0.00	0.90	23299
Video-Monitored	0.44	0.50	0.00	0.00	1.00	23301
Video-Monitored (Urban)	0.82	0.39	1.00	0.00	1.00	10397
Video-Monitored (Urban, $\geq$ 1,000 Voters)	0.94	0.24	1.00	0.00	1.00	8865
Video-Monitored (Urban, $<$ 1,000 Voters)	0.11	0.31	0.00	0.00	1.00	1532
Video-Monitored (Rural)	0.14	0.35	0.00	0.00	1.00	12904
Video-Monitored (Rural, $\geq$ 1,000 Voters)	0.69	0.46	1.00	0.00	1.00	2530
Video-Monitored (Rural, $<$ 1,000 Voters)	0.01	0.08	0.00	0.00	1.00	10374

Notes: This table reports the summary statistics for the 2018 presidential election. The sample includes a universe of 23,301 inland polling stations in 26 studied regions, excluding 402 polling stations located on the vessels in navigation on the voting day. Election results are not available for two monitored polling stations, which canceled voting because of election fraud.

Table 2: Effects of Video Monitoring on Voting

Sample:	Full	Full	Full	Rural	Urban
	(1)	(2)	(3)	(4)	(5)
Panel A: Share of Video-Monitored Polling Stations					
First Stage	0.595***	0.598***	0.595***	0.549***	0.623***
	[0.028]	[0.028]	[0.028]	[0.036]	[0.049]
Outcome Mean	0.436	0.436	0.436	0.251	0.668
	(0.496)	(0.496)	(0.496)	(0.434)	(0.471)
Panel B: Effect on Reported Turnout					
Reduced Form (ITT)	-0.019**	-0.021***	-0.021***	-0.024***	-0.012
	[0.009]	[0.007]	[0.005]	[0.007]	[0.008]
Second Stage (2SLS)	-0.032**	-0.035***	-0.035***	-0.044***	-0.020
	[0.015]	[0.011]	[0.009]	[0.013]	[0.013]
Outcome Mean	0.677	0.677	0.677	0.704	0.644
	(0.124)	(0.124)	(0.124)	(0.127)	(0.110)
Panel C: Effect on Incumbent's Votes					
Reduced Form (ITT)	-21.711**	-23.611***	-24.717***	-21.753***	-18.393**
	[10.182]	[7.191]	[5.666]	[7.598]	[9.151]
Second Stage (2SLS)	-36.476**	-39.491***	-41.572***	-39.656***	-29.516**
	[17.069]	[11.861]	[9.357]	[13.670]	[14.654]
Outcome Mean	502.540	502.540	502.540	488.609	520.949
	(165.244)	(165.244)	(165.244)	(166.916)	(161.368)
Panel D: Effect on Others' Votes					
Reduced Form (ITT)	-2.453	-2.345	-1.465	-4.654	-0.889
	[3.681]	[3.084]	[2.847]	[3.214]	[5.869]
Second Stage (2SLS)	-4.121	-3.923	-2.465	-8.484	-1.427
	[6.167]	[5.163]	[4.794]	[5.888]	[9.433]
Outcome Mean	152.477	152.477	152.477	131.774	178.102
	(66.269)	(66.269)	(66.269)	(59.167)	(65.887)
Region FEs	No	Yes	No	No	No
Commission FEs	No	No	Yes	Yes	Yes
# Observations	5721	5721	5721	3142	2515

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports first-stage estimates and effects of video monitoring on voting in the 2018 presidential election. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table 3: Mechanisms: Effects on Different Categories of Ballots

Outcome:	Number of Ballots						
	Received	Unused	Inside	Outside	Early	Invalid	Lost
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Second Stage (ToT)	-4.597	39.254***	-39.334***	-4.687	0.167	0.091	0.003
	[9.337]	[9.047]	[12.231]	[5.445]	[1.767]	[0.946]	[0.003]
Outcome Mean	933.093	270.173	601.534	59.820	1.564	7.629	0.003
	(235.671)	(148.486)	(190.300)	(53.167)	(22.556)	(6.919)	(0.058)
Region FEs	No	No	No	No	No	No	No
Commission FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	5721	5721	5721	5721	5721	5721	5721

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of video monitoring on the number of ballots of different categories in the 2018 presidential election. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table 4: Mechanisms: Effects on Voter Registration

Outcome:	Number of Registered Voters			
	Included	Asinh Included	Excluded	Asinh Excluded
	(1)	(2)	(3)	(4)
Second Stage (ToT)	-6.596	-0.021	2.775	0.104**
	[8.745]	[0.076]	[2.140]	[0.052]
Outcome Mean	47.195	4.093	53.604	4.548
	(68.772)	(0.907)	(28.026)	(0.561)
Region FEs	No	No	No	No
Commission FEs	Yes	Yes	Yes	Yes
# Observations	5721	5721	5721	5721

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of video monitoring on the number of voters who changed their registration status for another polling place before the 2018 presidential election. Included (excluded) voters measure the number of voters added to (excluded from) voter registration records due to changes in the registration status. *Asinh* applies the inverse hyperbolic sine transformation to reduce the skewness of variables, which have a large share of zero values. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table 5: Mechanisms: Effects on Fraud Indicators

Outcome:	Share of Polling Stations with Abnormally High ($\geq$ 80%) Values of				
	Turnout	Inc. Vote Share	Turnout and Inc. Vote Share		
Sample:	Full	Full	Full	Rural	Urban
	(1)	(2)	(3)	(4)	(5)
Second Stage (ToT)	-0.098***	-0.111**	-0.088***	-0.099**	-0.061*
	[0.030]	[0.043]	[0.031]	[0.048]	[0.036]
Outcome Mean	0.172	0.269	0.125	0.187	0.049
	(0.377)	(0.443)	(0.331)	(0.390)	(0.215)
Region FEs	No	No	No	No	No
Commission FEs	Yes	Yes	Yes	Yes	Yes
# Observations	5721	5721	5721	3142	2515

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of video monitoring on the share of polling stations with abnormally high values ($\geq$ 80%) of reported turnout and incumbent vote share, fraud indicators in election forensics literature (Klimek et al., 2012). Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table 6: Effects on Vote Displacement

Radius:	3 km		5 km		7 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Share of Video-Monitored Neighboring Polling Stations						
First Stage	0.501***	0.527***	0.577***	0.582***	0.613***	0.617***
	[0.077]	[0.075]	[0.071]	[0.070]	[0.059]	[0.059]
Outcome Mean	0.203	0.203	0.185	0.185	0.166	0.166
	(0.384)	(0.384)	(0.374)	(0.374)	(0.360)	(0.360)
Panel B: Effect on Reported Turnout						
Reduced Form (ITT)	0.006	-0.001	0.004	0.006	0.013	0.008
	[0.026]	[0.019]	[0.023]	[0.016]	[0.021]	[0.015]
Second Stage (2SLS)	0.012	-0.003	0.007	0.011	0.021	0.013
	[0.052]	[0.036]	[0.039]	[0.028]	[0.033]	[0.025]
Outcome Mean	0.712	0.712	0.727	0.727	0.739	0.739
	(0.134)	(0.134)	(0.131)	(0.131)	(0.132)	(0.132)
Panel C: Effect on Incumbent's Vote Share						
Reduced Form (ITT)	0.029	0.023*	0.037**	0.039***	0.038**	0.036***
	[0.022]	[0.013]	[0.019]	[0.013]	[0.016]	[0.011]
Second Stage (2SLS)	0.058	0.043*	0.065*	0.067***	0.063**	0.058***
	[0.045]	[0.025]	[0.034]	[0.023]	[0.026]	[0.019]
Outcome Mean	0.764	0.764	0.779	0.779	0.788	0.788
	(0.095)	(0.095)	(0.095)	(0.095)	(0.093)	(0.093)
Panel D: Effect on Others' Vote Share						
Reduced Form (ITT)	-0.029	-0.022*	-0.037**	-0.038***	-0.038**	-0.035***
	[0.021]	[0.013]	[0.018]	[0.012]	[0.015]	[0.011]
Second Stage (2SLS)	-0.058	-0.043*	-0.064**	-0.065***	-0.062**	-0.057***
	[0.043]	[0.025]	[0.032]	[0.023]	[0.025]	[0.019]
Outcome Mean	0.225	0.225	0.210	0.210	0.202	0.202
	(0.092)	(0.092)	(0.092)	(0.092)	(0.090)	(0.090)
Region FEs	No	Yes	No	Yes	No	Yes
Commission FEs	No	No	No	No	No	No
# Observations	1095	1095	1538	1538	2061	2061

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports vote displacement effects to unmonitored polling stations in the 2018 presidential election. This estimation (i) excludes unmonitored stations, which have neighbors with more than 1,400 voters (upper bandwidth boundary); (ii) excludes neighbors with fewer than 600 registered voters (lower bandwidth boundary); (iii) excludes neighbors outside of the specified radius from unmonitored polling stations; (iv) restricts the sample to unmonitored polling places, which have neighbors on one side of the cutoff; (v) takes the average number of voters if there are several neighbors. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table 7: Effects on Changes in Public Goods Spending

Projects:	Started by March			Started and Finished by March		
Sample:	Full	Rural	Urban	Full	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Share of Video-Monitored Polling Stations (%)						
First Stage	0.471***	0.143***	0.555***	0.471***	0.143***	0.555***
	[0.033]	[0.021]	[0.047]	[0.033]	[0.021]	[0.047]
Outcome Mean	32.493	16.526	51.614	32.493	16.526	51.614
	(28.283)	(13.720)	(29.132)	(28.283)	(13.720)	(29.132)
Panel B: Change in the Number of Projects						
Second Stage (ToT)	-0.103	0.011	-0.218	-0.027	-0.142	0.001
	[0.124]	[0.469]	[0.196]	[0.034]	[0.143]	[0.050]
Outcome Mean	-10.691	-7.228	-15.378	-0.992	-0.170	-2.064
	(40.093)	(30.002)	(49.485)	(10.860)	(9.180)	(12.563)
Panel C: Change in the Total Value of Projects per Capita						
Second Stage (ToT)	-5.681	0.673	-11.180	-1.814	-8.499	-0.378
	[6.477]	[28.861]	[8.911]	[1.295]	[6.579]	[1.567]
Outcome Mean	-321.638	-309.724	-359.049	0.756	13.414	-24.203
	(2086.590)	(1930.417)	(2254.078)	(406.898)	(405.370)	(401.114)
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-Stat	207.629	45.687	139.529	207.629	45.687	139.529
# Observations	596	324	267	596	324	267

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of video monitoring on changes in district public goods spending before the 2018 presidential election. Changes in the number of projects and their total value are defined by the difference between December 2017 – March 2018 (four months before the election) and December 2016 – March 2017 (a corresponding period a year earlier), with winsorized values in the bottom- and top-2%. Specifications use data at the district level, instrument the share of video-monitored polling stations with the percentage of polling places above the cutoff in the bandwidth of 400 registered voters, and control for the share of polling stations in the bandwidth. Standard errors clustered by region are reported in brackets. Standard deviations are reported in parentheses.

Table 8: Survey Experiment: Summary Statistics and Balance Test

Statistic:	Mean Full	Mean Control	Mean Treat	Difference Treat-Control	P-value	# Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Geographical Area						
Urban (=1)	0.78	0.78	0.77	-0.01	0.68	1097
	(0.42)	[0.04]	[0.04]	[0.02]		
Moscow (=1)	0.13	0.13	0.12	-0.01	0.32	1097
	(0.33)	[0.12]	[0.11]	[0.01]		
St. Petersburg (=1)	0.05	0.04	0.06	0.02	0.29	1097
	(0.22)	[0.04]	[0.06]	[0.02]		
Rural (=1)	0.22	0.22	0.23	0.01	0.68	1097
	(0.42)	[0.04]	[0.04]	[0.02]		
Individual Characteristics						
Female (=1)	0.56	0.57	0.55	-0.02	0.63	1097
	(0.50)	[0.02]	[0.02]	[0.03]		
Age	46.21	45.56	46.88	1.33	0.19	1097
	(16.50)	[0.68]	[0.73]	[1.00]		
Incomplete Secondary School (=1)	0.05	0.04	0.05	0.01	0.67	1097
	(0.21)	[0.01]	[0.01]	[0.01]		
Secondary or Vocational Education (=1)	0.64	0.65	0.64	-0.01	0.71	1097
	(0.48)	[0.03]	[0.02]	[0.03]		
Higher Education (=1)	0.31	0.31	0.31	0.00	0.84	1097
	(0.46)	[0.04]	[0.03]	[0.02]		
Employed (=1)	0.60	0.59	0.60	0.02	0.60	1097
	(0.49)	[0.03]	[0.02]	[0.03]		
Retired (=1)	0.26	0.25	0.26	0.01	0.73	1097
	(0.44)	[0.02]	[0.02]	[0.03]		
Income Level	3.18	3.17	3.20	0.04	0.58	1096
	(1.02)	[0.07]	[0.06]	[0.06]		
Media Consumption						
Daily Internet User (=1)	0.60	0.62	0.58	-0.04	0.16	1097
	(0.49)	[0.03]	[0.03]	[0.03]		
Internet Non-User (=1)	0.22	0.21	0.22	0.00	0.85	1097
	(0.41)	[0.02]	[0.03]	[0.02]		
Daily TV User (=1)	0.67	0.66	0.68	0.03	0.41	1097
	(0.47)	[0.03]	[0.02]	[0.03]		
TV Non-User (=1)	0.08	0.09	0.08	0.00	0.82	1097
	(0.28)	[0.02]	[0.01]	[0.02]		
Government Approval						
Approves President's Work (=1)	0.66	0.64	0.69	0.06	0.04	1080
	(0.47)	[0.03]	[0.02]	[0.03]		
Approves State Duma's Work (=1)	0.38	0.37	0.39	0.02	0.55	1066
	(0.49)	[0.03]	[0.03]	[0.04]		
Approves Government's Work (=1)	0.45	0.45	0.46	0.01	0.75	1074
	(0.50)	[0.03]	[0.03]	[0.03]		
Approves Any (=1)	0.71	0.70	0.71	0.01	0.70	1094
	(0.46)	[0.03]	[0.02]	[0.03]		
Transparency Technologies						
Aware of Transparent Ballot Boxes (=1)	0.56	0.56	0.56	0.00	0.89	1097
	(0.50)	[0.03]	[0.02]	[0.03]		
Aware of Video Monitoring (=1)	0.60	-	0.60	-	-	538
	(0.49)		[0.02]			
Will Observe Election Online (=1)	0.15	-	0.15	-	-	523
	(0.36)		[0.02]			

Notes: This table reports summary statistics and balance test of random assignment of 1,097 respondents to priming treatment in the survey experiment. Column (1) reports the mean of each variable, with standard deviations in parentheses, for the full sample. Columns (2) and (3) report the mean of each variable, with standard errors clustered by region in brackets, for each experimental condition. Column (5) documents the difference in means between treatment and control groups, with standard errors clustered by region in brackets. Column (6) reports the p-value of a t-test of equality of means.

Table 9: Survey Experiment: Effects of Priming on Voting and Attitudes

Outcome:	All					Unaware					Aware				
	Control Mean	Treatment Effect	P-value	Fisher's P-value	MHT Q-value	Control Mean	Treatment Effect	P-value	Fisher's P-value	MHT Q-value	Control Mean	Treatment Effect	P-value	Fisher's P-value	MHT Q-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Voting/Supporting United Russia															
Will Vote (=1)	0.61	0.03	0.40	0.35	.93	0.49	0.11**	0.02	0.02	.05	0.70	-0.04	0.37	0.34	1
	(0.49)	[0.03]				(0.50)	[0.04]				(0.46)	[0.04]			
Will Vote for United Russia (=1)	0.38	0.01	0.80	0.80	1	0.26	0.08	0.16	0.14	.12	0.45	-0.02	0.64	0.63	1
	(0.49)	[0.04]				(0.44)	[0.06]				(0.50)	[0.05]			
% Others Voting for United Russia	47.83	1.98	0.16	0.23	.93	43.36	3.33	0.14	0.18	.12	51.17	0.86	0.67	0.70	1
	(24.77)	[1.39]				(22.70)	[2.21]				(25.76)	[2.03]			
Trust in Election															
Trust in Election Results (=1)	0.30	0.05*	0.09	0.10	.21	0.23	0.07*	0.07	0.11	.18	0.35	0.03	0.36	0.45	.56
	(0.46)	[0.03]				(0.42)	[0.04]				(0.48)	[0.03]			
Election Will Lead to Improvements (=1)	0.37	-0.02	0.33	0.41	.21	0.31	-0.02	0.62	0.71	.45	0.42	-0.03	0.36	0.43	.56
	(0.48)	[0.03]				(0.46)	[0.03]				(0.49)	[0.04]			
Democratic Values															
Russia is a Democracy (=1)	0.83	-0.05**	0.03	0.03	.08	0.81	-0.05	0.15	0.18	.37	0.84	-0.05	0.10	0.11	.46
	(0.38)	[0.02]				(0.39)	[0.04]				(0.37)	[0.03]			
# Democratic Values Agreed	2.35	-0.04	0.65	0.70	.76	2.43	-0.18	0.18	0.23	.37	2.30	0.07	0.48	0.55	.93
	(1.57)	[0.08]				(1.66)	[0.13]				(1.50)	[0.10]			
Tax Compliance is Important (=1)	0.65	0.02	0.45	0.54	.76	0.64	0.04	0.30	0.41	.37	0.66	0.00	0.93	0.95	1
	(0.48)	[0.02]				(0.48)	[0.04]				(0.47)	[0.04]			
Indices															
Voting/Supporting United Russia Index	0.00	0.05	0.12	0.14	.32	0.00	0.19***	0.00	0.00	0	0.00	-0.03	0.55	0.51	.83
	(0.59)	[0.03]				(0.56)	[0.04]				(0.59)	[0.05]			
Trust in Election Index	0.00	0.03	0.53	0.58	.53	0.00	0.06	0.28	0.39	.41	0.00	0.00	0.98	0.99	.99
	(0.79)	[0.04]				(0.77)	[0.06]				(0.80)	[0.06]			
Democratic Values Index	0.00	-0.04	0.14	0.21	.32	0.00	-0.06	0.24	0.26	.41	0.00	-0.03	0.46	0.52	.83
	(0.56)	[0.03]				(0.53)	[0.05]				(0.59)	[0.04]			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the results of the survey experiment based on the pre-analysis plan. Columns (1)–(5) report the results for the full sample of 1,097 respondents. Columns (6)–(10) and (11)–(15) report the results separately for subsamples split by awareness of transparent ballot boxes, which serves as a proxy for awareness of video monitoring (correlation of 0.45). Columns (1), (6), and (11) report the mean level of outcomes for the control group, with standard deviations in parentheses. Columns (2), (7), and (12) report the priming treatment effects of a reminder about video monitoring, with standard errors clustered by region in brackets. Columns (3), (8), and (13) report sampling-based p-values from a standard t-test. Columns (4), (9), and (14) report randomization-based Fisher's exact p-values for the sharp null hypothesis of no effect from permutation tests with 10,000 repetitions. Columns (5), (10), and (15) report q-values, which adjust the sampling-based p-values for the false discovery rate (FDR) within each hypothesis group following Anderson (2008) and Benjamini, Krieger, and Yekutieli (2006) and the family-wise error rate (FWER) between indices following Anderson (2008) and Westfall and Young (1993).

Table 10: Survey Experiment: Effects of Priming on Voter Intimidation

Outcome:	Sample		
# Statements Agreed	Full	Unaware	Aware
	(1)	(2)	(3)
Sensitive Statement (=1)	0.26***	0.30***	0.22**
	[0.07]	[0.10]	[0.09]
Priming Treatment (=1)	0.02	0.07	-0.01
	[0.06]	[0.10]	[0.09]
Sensitive x Priming (=1)	0.06	-0.00	0.10
	[0.11]	[0.14]	[0.14]
No Sens., Control Mean	1.51	1.37	1.62
	(0.79)	(0.78)	(0.79)
# Observations	1062	464	598

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of the priming treatment on voter intimidation using a listing experiment. The first row estimates the level of voter intimidation in the priming control group (the impact of a sensitive item on the number of statements respondents agree with). The second row estimates the effect of the priming treatment on the number of statements in the listing control group. The third row estimates the priming treatment effect on voter intimidation (the interaction effect of two treatments). Column (1) shows estimates for the full sample. Columns (2)–(3) document estimates for subsamples split by awareness about transparent ballot boxes, a proxy for awareness about video monitoring (correlation = 0.45). Standard errors clustered by region are reported in brackets. Standard deviations are reported in parentheses.

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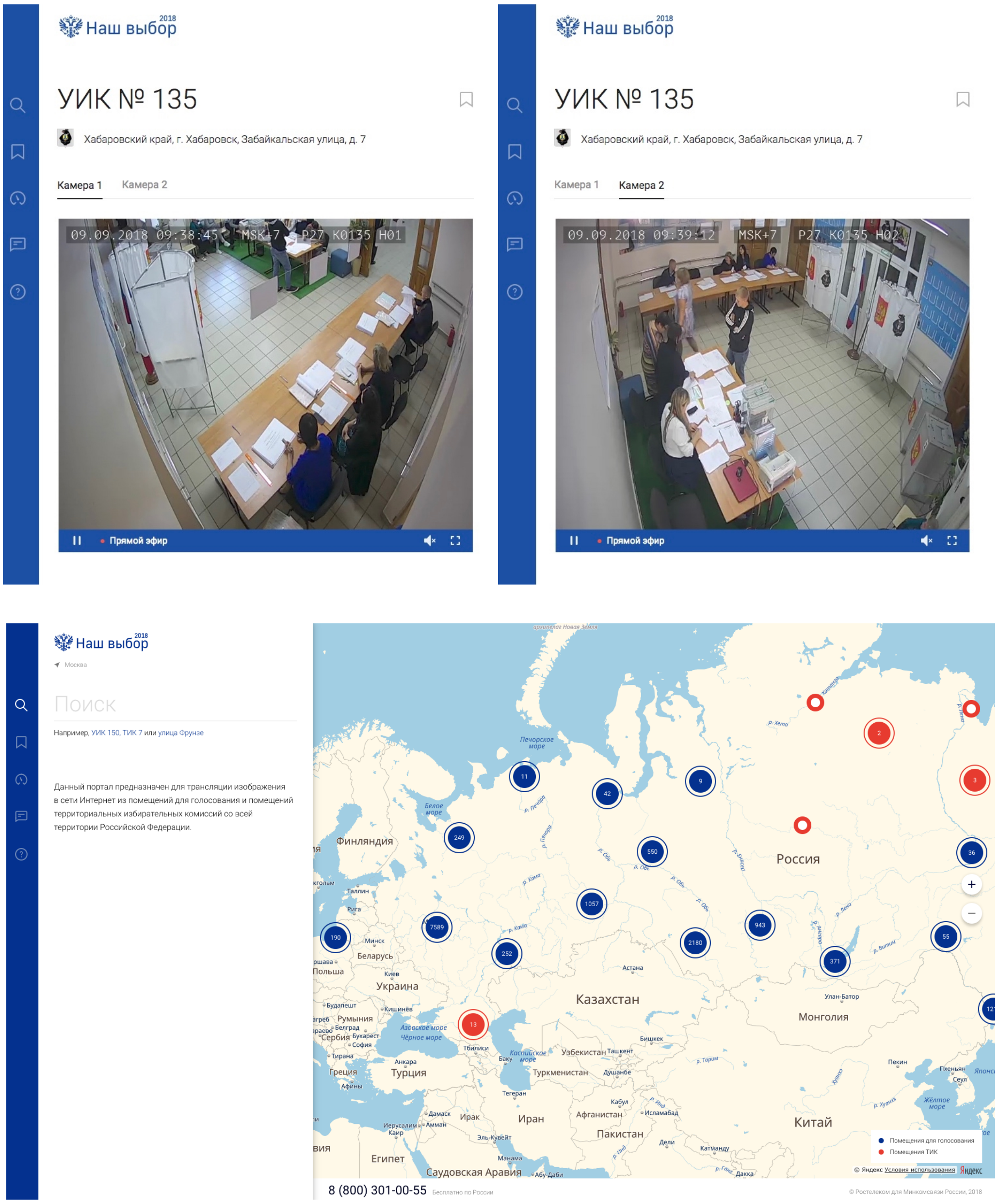
Appendix

A. Background

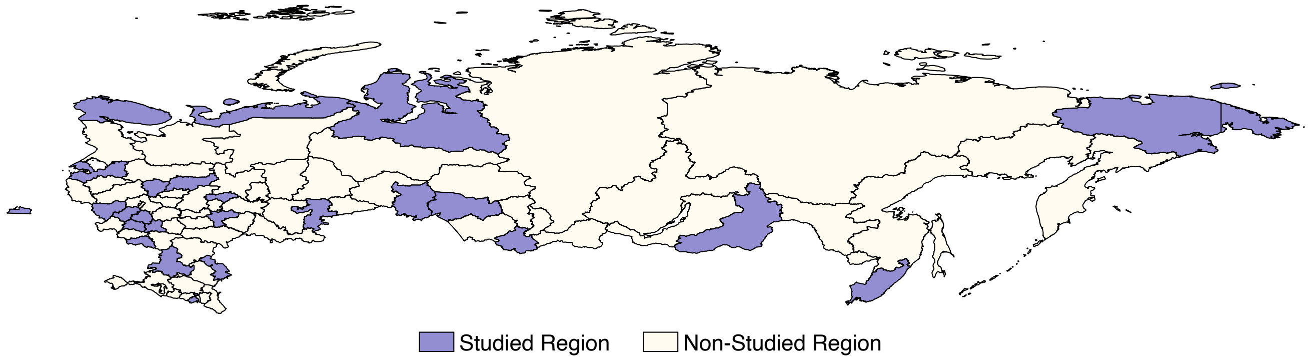
Figure A.1: Screenshots of the Live-Stream Platform, www.nashvybor2018.ru



Notes: This figure shows screenshots of the live-stream platform, made during the 2018 local elections. The upper left picture captures a place of issuance of ballots to voters, while the upper right picture captures ballot boxes at the same polling station (Khabarovski Krai, #135). The bottom picture illustrates how monitoring was crowdsourced, showing how a webcam feed could be accessed over the internet for any monitored station.

B. Election Records Data

Figure A.2: Map of Studied Regions of Russia



Notes: The blue color highlights the twenty-six studied regions, which provided information on the location of video monitoring on the websites of regional election commissions.

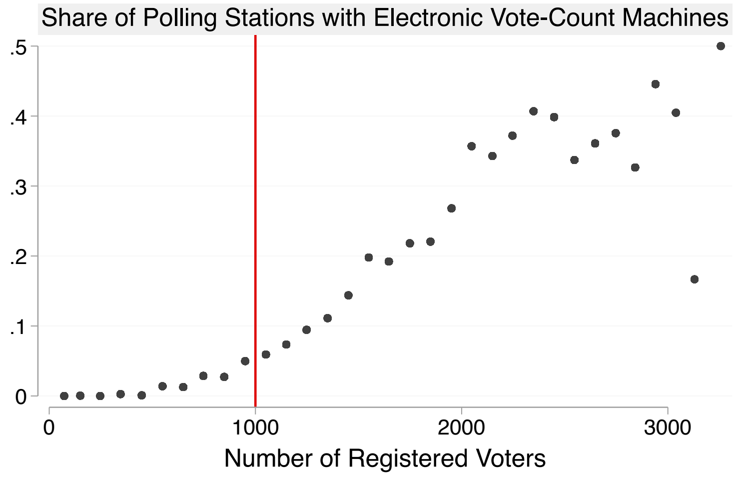
Table A.1: Regional Summary Statistics

Regions:	All	Studied	Non-Studied	Difference	P-value
	(1)	(2)	(3)	(4)	(5)
Panel A: 2018 Presidential Election					
Reported Turnout	0.69	0.68	0.69	-0.01	0.49
	(0.09)	[0.02]	[0.01]	[0.02]	
Incumbent's Vote Share	0.77	0.76	0.77	-0.01	0.53
	(0.07)	[0.01]	[0.01]	[0.01]	
# Observations	85	26	59	85	
Panel B: 2018 Presidential Election (Excluding Crimea and Sevastopol)					
Reported Turnout	0.69	0.68	0.69	-0.01	0.47
	(0.09)	[0.02]	[0.01]	[0.02]	
Incumbent's Vote Share	0.76	0.75	0.77	-0.01	0.36
	(0.06)	[0.01]	[0.01]	[0.01]	
# Observations	83	25	58	83	
Panel C: 2012 Presidential Election					
Reported Turnout	0.67	0.66	0.67	-0.01	0.83
	(0.10)	[0.02]	[0.01]	[0.02]	
Incumbent's Vote Share	0.64	0.62	0.66	-0.04	0.05
	(0.10)	[0.01]	[0.01]	[0.02]	
# Observations	83	25	58	83	

Notes: This table reports regional summary stistics for studied and non-studied regions. Robust standard errors are reported in brackets. Standard deviations are reported in parentheses.

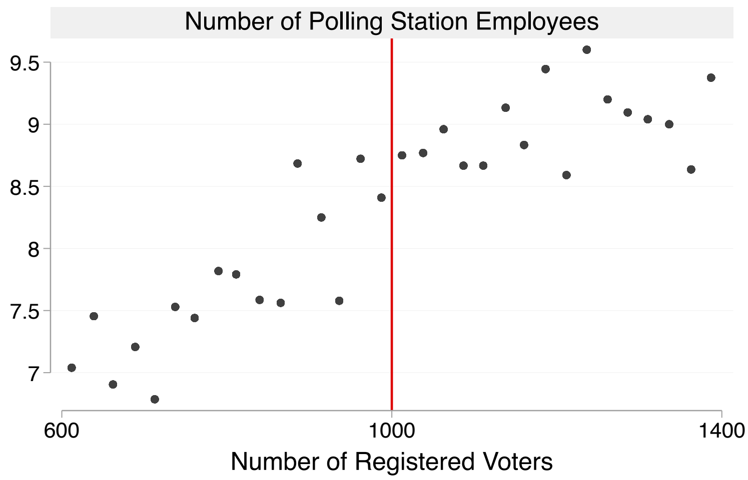
C. Threats to Identification: Discontinuities in Polling Station Characteristics

Figure A.3: Electronic Vote-Count Machines



Notes: This figure plots the share of polling stations with electronic vote-counting machines in bins of 100 registered voters. The red vertical line depicts the cutoff of 1,000 registered voters, which defined eligibility for video monitoring.

Figure A.4: Polling Station Employees



Notes: This figure plots the average number of polling station employees in bins of 25 registered voters. The sample includes polling stations in the bandwidth of 400 registered voters in two studied regions of Russia. The red vertical line depicts the cutoff of 1,000 registered voters, which defined eligibility for video monitoring.

D. Impact on Election Outcomes: Additional Results

Table A.2: Effects on Incumbent's Vote Share and Margin of Victory

Outcome:	Incumbent's Vote Share			Incumbent's Vote Margin		
Sample:	Full	Rural	Urban	Full	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Second Stage (2SLS)	-0.009*	-0.001	-0.009	-0.012	0.003	-0.012
	[0.006]	[0.009]	[0.008]	[0.009]	[0.015]	[0.013]
Outcome Mean	0.755	0.773	0.732	0.621	0.648	0.589
Region FEs	No	No	No	No	No	No
Commission FEs	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	5721	3142	2515	5721	3142	2515

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of video monitoring on the incumbent's vote share and margin of victory in the 2018 presidential election. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table A.3: Effects on Votes Cast for Other Candidates

Outcome:	Votes Cast for						
	Cand 1	Cand 2	Cand 3	Cand 4	Cand 5	Cand 6	Cand 7
Second Stage (ToT)	-0.202	-1.967	-0.623	0.085	0.363	-0.180	0.059
	[0.302]	[3.288]	[1.542]	[0.624]	[0.317]	[0.279]	[0.442]
Outcome Mean	4.063	85.192	43.339	7.473	4.479	3.860	4.071
	(3.001)	(45.289)	(20.834)	(7.460)	(4.303)	(4.276)	(5.043)
Region FEs	No	No	No	No	No	No	No
Commission FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	5721	5721	5721	5721	5721	5721	5721

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of video monitoring on the number of votes cast for other candidates in the 2018 presidential election. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table A.4: Mechanisms: Placebo Effects on Fraud Indicators, 2012 Presidential Election

Outcome:	Share of Polling Stations with Abnormally High ($\geq 80\%$)		
	Turnout	Inc. Vote Share	Turnout and Inc. Vote Share
	(1)	(2)	(3)
Reduced Form (ITT)	0.008	0.009	0.008
	[0.018]	[0.016]	[0.013]
Outcome Mean	0.121	0.112	0.046
	(0.326)	(0.316)	(0.210)
Region FEs	No	No	No
Commission FEs	Yes	Yes	Yes
# Observations	5364	5364	5364

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the placebo effects of video monitoring on the share of polling stations with abnormally high values ($\geq 80\%$) of reported turnout and incumbent vote share, fraud indicators in election forensics literature (Klimek et al., 2012), in the 2012 presidential election. Specifications use data at the polling station level, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

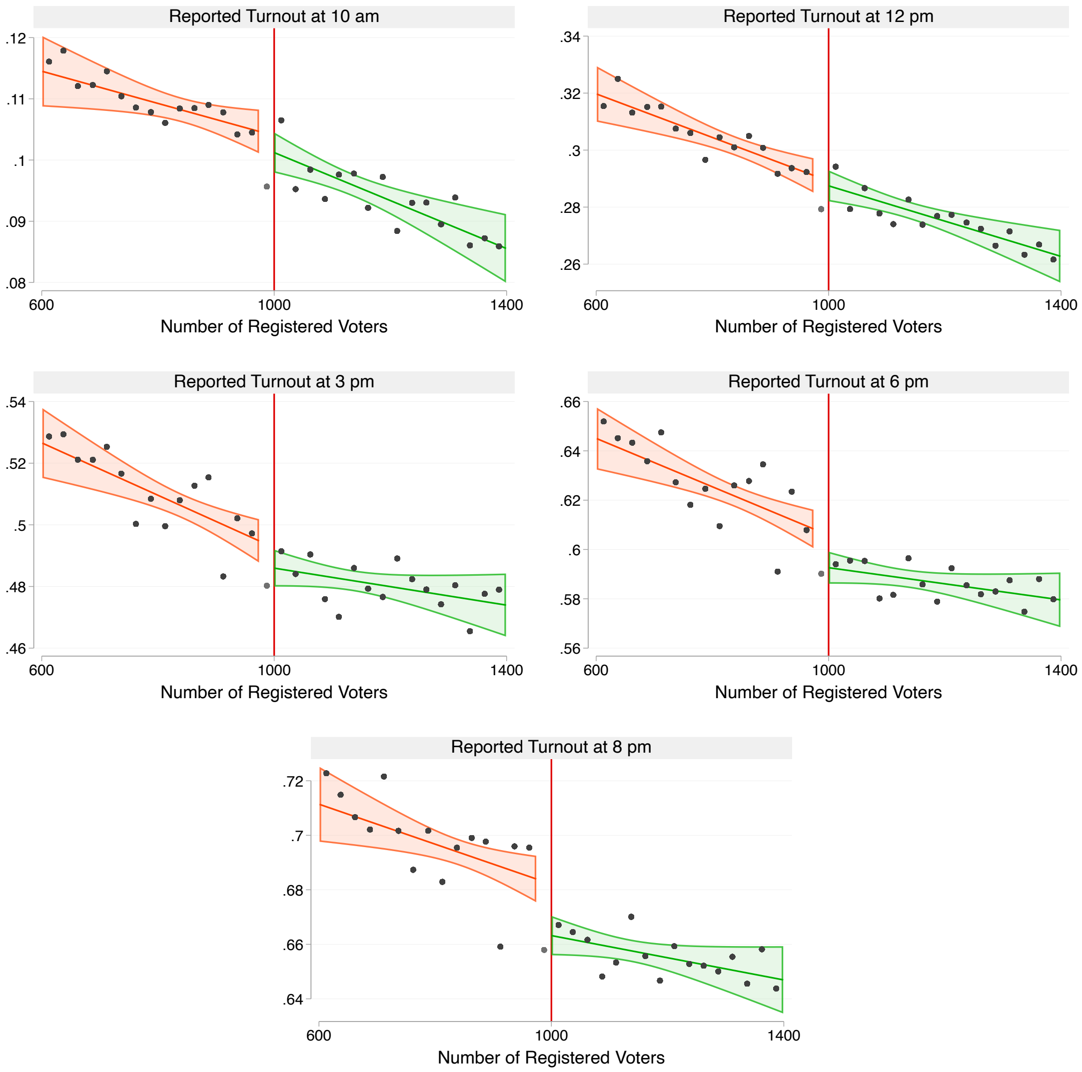
Table A.5: Mechanisms: Effects on Vote-Count Fraud Indicators

Outcome:	Share of Polling Stations with Rounded Values of					
	Turnout			Inc. Vote Share		
Sample:	Full	Rural	Urban	Full	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Second Stage (ToT)	0.010	0.039	-0.010	-0.035	-0.002	-0.060
	[0.037]	[0.056]	[0.052]	[0.032]	[0.047]	[0.048]
Outcome Mean	0.113	0.117	0.109	0.107	0.113	0.101
	(0.317)	(0.322)	(0.311)	(0.310)	(0.317)	(0.301)
Region FEs	No	No	No	No	No	No
Commission FEs	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	5721	3142	2515	5721	3142	2515

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of video monitoring on the share of polling stations with rounded values of reported turnout and incumbent vote share, indicators of vote-count fraud in election forensics literature (Klimek et al., 2012), in the 2018 presidential election. Rounded values are decimal numbers within a 0.05 margin of the nearesest integer number. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Figure A.5: Effects of Video Monitoring on Reported Turnout over Time



Notes: This figure plots reduced-form estimates of the impact of video monitoring on the reported turnout over time. Each point plots means of observations in bins of 25 registered voters. The solid lines plot predicted values and 95% confidence intervals of a linear regression estimated separately on either side of the threshold within the bandwidth of 400 registered voters.

Table A.6: Mechanisms: Effects on Reported Turnout Over Time

Outcome:	Reported Turnout				
	10 am	12 pm	3pm	6 pm	8 pm
	(1)	(2)	(3)	(4)	(5)
Second Stage (ToT)	-0.011***	-0.011	-0.018**	-0.027***	-0.035***
	[0.004]	[0.007]	[0.008]	[0.009]	[0.009]
Outcome Mean	0.102	0.291	0.496	0.607	0.677
	(0.053)	(0.090)	(0.104)	(0.114)	(0.124)
Region FEs	No	No	No	No	No
Commission FEs	Yes	Yes	Yes	Yes	Yes
# Observations	5721	5721	5721	5721	5721

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of video monitoring on the reported turnout over time. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table A.7: Mechanisms: Heterogeneous Effects by Winner's Vote Share in the 2012 and 2008 Presidential Elections

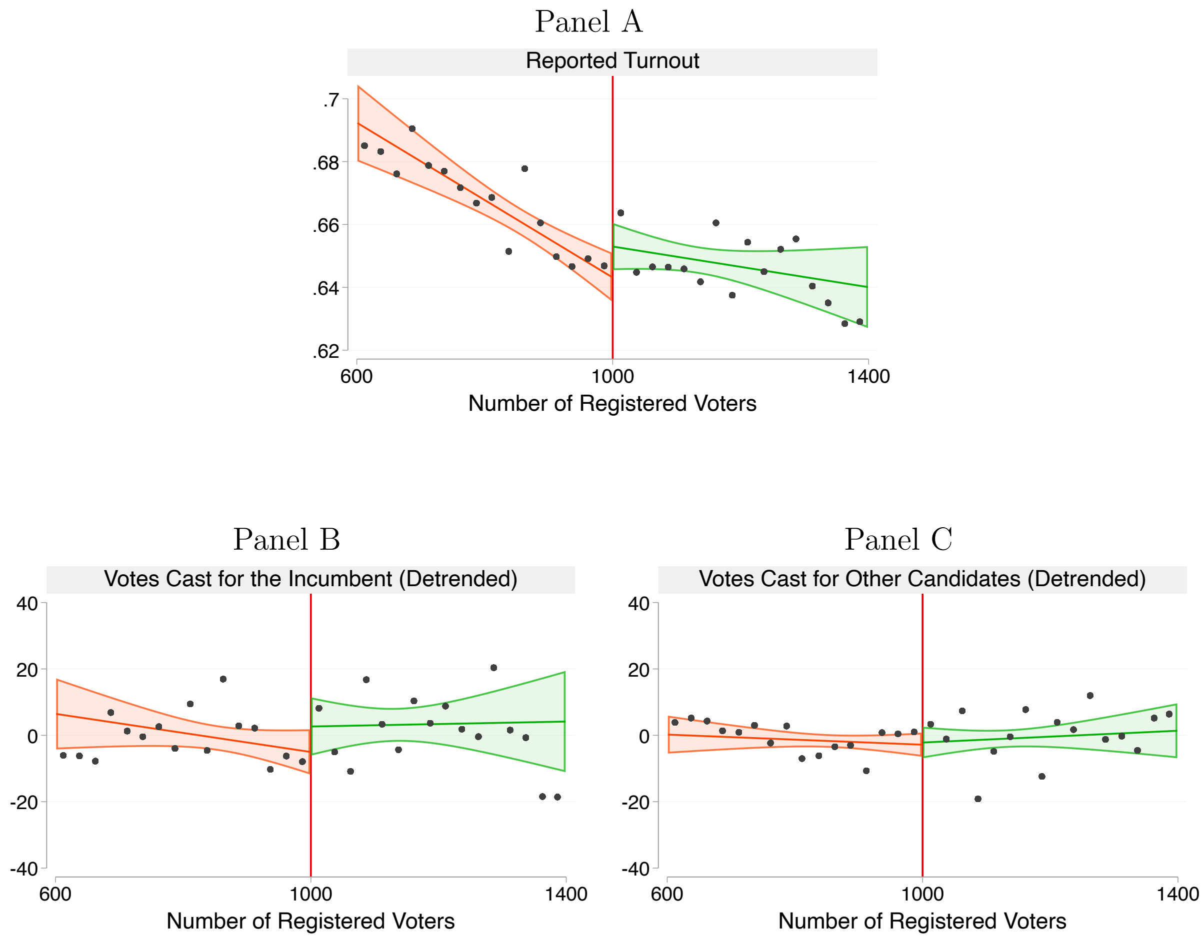
Sample:	Full	2012 Winner's Vote Share		2008 Winner's Vote Share	
		High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)
Panel A: Share of Video-Monitored Polling Stations					
First Stage	0.595***	0.579***	0.609***	0.540***	0.669***
	[0.028]	[0.037]	[0.044]	[0.037]	[0.041]
Outcome Mean	0.436	0.391	0.492	0.379	0.510
	(0.496)	(0.488)	(0.500)	(0.485)	(0.500)
Panel B: Effect on Reported Turnout					
Second Stage (ToT)	-0.035***	-0.057***	-0.010	-0.046***	-0.024*
	[0.009]	[0.012]	[0.012]	[0.013]	[0.012]
Outcome Mean	0.677	0.705	0.643	0.714	0.630
	(0.124)	(0.133)	(0.102)	(0.130)	(0.098)
Panel C: Effect on Incumbent's Votes					
Second Stage (ToT)	-41.572***	-57.726***	-23.303*	-56.691***	-25.887**
	[9.357]	[13.213]	[12.910]	[14.323]	[11.826]
Outcome Mean	502.540	517.484	481.305	532.810	460.845
	(165.244)	(175.722)	(146.273)	(175.665)	(137.897)
Panel D: Effect on Others' Votes					
Second Stage (ToT)	-2.465	-8.288	3.773	-4.563	-0.354
	[4.794]	[6.039]	[7.767]	[6.076]	[7.657]
Outcome Mean	152.477	139.789	169.160	139.531	170.229
	(66.269)	(66.557)	(61.851)	(66.350)	(61.669)
Region FEs	No	No	No	No	No
Commission FEs	Yes	Yes	Yes	Yes	Yes
# Observations	5721	3123	2561	3186	2498

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the heterogeneous effects of video monitoring on voting by the winner's vote share in the 2012 and 2008 presidential elections. The high (low) winner's vote share is defined by the above (below) median value at the election commission level. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

E. Impact on Voting: Placebo Effects

Figure A.6: Placebo Effects on Voting, 2012 Presidential Election



Notes: This figure plots reduced-form estimates of the impact of video monitoring on voting in the 2012 presidential election. Each point plots means of observations in bins of 25 registered voters. The solid lines plot predicted values and 95% confidence intervals of a linear regression estimated separately on either side of the threshold within the bandwidth of 400 registered voters. For presentation purposes, detrended graphs difference out trends associated with a positive relationship between the number of votes and registered voters.

Table A.8: Placebo Effects on Voting, 2012 Presidential Election

	(1)	(2)	(3)
Panel A: Effect on Reported Turnout			
Reduced Form (ITT)	0.010	0.006	0.008
	[0.007]	[0.006]	[0.005]
Outcome Mean	0.658	0.658	0.658
	(0.115)	(0.115)	(0.115)
Panel B: Effect on Incumbent's Votes			
Reduced Form (ITT)	7.762	5.260	7.470
	[7.659]	[6.274]	[5.080]
Outcome Mean	412.566	412.566	412.566
	(141.323)	(141.323)	(141.323)
Panel C: Effect on Others' Votes			
Reduced Form (ITT)	0.581	-0.311	-0.793
	[3.759]	[3.158]	[2.796]
Outcome Mean	223.596	223.596	223.596
	(87.819)	(87.819)	(87.819)
Region FEs	No	Yes	No
Commission FEs	No	No	Yes
# Observations	5364	5364	5364

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the placebo effects of video monitoring on voting in the 2012 presidential election. Specifications use data at the polling station level, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table A.9: Placebo Effects on Voting, Alternative Cutoffs

Cutoff:	1,000	850	1,150
	(1)	(2)	(3)
Panel A: Share of Video-Monitored Polling Stations			
First Stage	0.595***	-0.074***	-0.136***
	[0.028]	[0.017]	[0.022]
Outcome Mean	0.436	0.256	0.616
	(0.496)	(0.436)	(0.486)
Panel B: Effect on Turnout			
Reduced Form (ITT)	-0.021***	-0.001	0.002
	[0.005]	[0.005]	[0.005]
Second Stage (ToT)	-0.035***	0.013	-0.018
	[0.009]	[0.070]	[0.037]
Outcome Mean	0.677	0.693	0.662
	(0.124)	(0.130)	(0.117)
Panel C: Effect on Incumbent's Votes			
Reduced Form (ITT)	-24.717***	1.025	-1.361
	[5.666]	[4.727]	[6.249]
Second Stage (ToT)	-41.572***	-13.935	9.976
	[9.357]	[63.915]	[46.082]
Outcome Mean	502.540	424.082	571.070
	(165.244)	(155.865)	(172.590)
Panel D: Effect on Others' Votes			
Reduced Form (ITT)	-1.465	1.754	-4.608
	[2.847]	[2.348]	[2.993]
Second Stage (ToT)	-2.465	-23.859	33.779
	[4.794]	[31.945]	[22.483]
Outcome Mean	152.477	123.131	179.947
	(66.269)	(61.029)	(68.706)
Region FEs	No	No	No
Commission FEs	Yes	Yes	Yes
# Observations	5721	6515	5764

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the placebo effects of video monitoring on voting using alternative cutoffs of 850 and 1,150 registered voters. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters for the cutoff of 1,000 registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

F. Impact on Election Outcomes: Robustness of Results

Table A.10: Robustness of Effects on Voting to Inclusion of Intermediate Data Points (975–1,000 Registered Voters)

Sample:	Full	Full	Full	Rural	Urban
	(1)	(2)	(3)	(4)	(5)
Panel A: Share of Video-Monitored Polling Stations					
First Stage	0.488***	0.489***	0.484***	0.460***	0.481***
	[0.027]	[0.027]	[0.027]	[0.036]	[0.045]
Outcome Mean	0.437	0.437	0.437	0.256	0.663
	(0.496)	(0.496)	(0.496)	(0.436)	(0.473)
Panel B: Effect on Reported Turnout					
Reduced Form (ITT)	-0.010	-0.012**	-0.015***	-0.021***	-0.004
	[0.008]	[0.006]	[0.005]	[0.006]	[0.006]
Second Stage (2SLS)	-0.021	-0.024**	-0.030***	-0.045***	-0.009
	[0.016]	[0.012]	[0.009]	[0.014]	[0.013]
Outcome Mean	0.677	0.677	0.677	0.704	0.644
	(0.124)	(0.124)	(0.124)	(0.127)	(0.110)
Panel C: Effect on Incumbent's Votes					
Reduced Form (ITT)	-12.344	-13.318**	-17.111***	-17.355**	-10.432
	[8.726]	[6.183]	[4.887]	[6.935]	[7.069]
Second Stage (2SLS)	-25.279	-27.247**	-35.362***	-37.702**	-21.686
	[17.748]	[12.407]	[9.905]	[14.838]	[14.497]
Outcome Mean	502.010	502.010	502.010	489.247	518.772
	(164.383)	(164.383)	(164.383)	(166.148)	(160.609)
Panel D: Effect on Others' Votes					
Reduced Form (ITT)	-1.046	-1.285	-1.041	-4.525	0.560
	[3.144]	[2.666]	[2.400]	[3.093]	[4.273]
Second Stage (2SLS)	-2.142	-2.630	-2.152	-9.830	1.164
	[6.424]	[5.458]	[4.958]	[6.761]	[8.875]
Outcome Mean	152.364	152.364	152.364	132.122	177.354
	(65.669)	(65.669)	(65.669)	(58.838)	(65.260)
Region FEs	No	Yes	No	No	No
Commission FEs	No	No	Yes	Yes	Yes
# Observations	5914	5914	5914	3244	2609

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the robustness of effects on voting to the inclusion of intermediate data points with 975–1,000 registered voters. Specifications use data at the polling station level, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table A.11: Robustness of Effects on Voting to Alternative Functional Forms

Functional Form:	Linear	Quadratic	Qubic	Local Linear
	(1)	(2)	(3)	(4)
Panel A: Share of Video-Monitored Polling Stations				
First Stage	0.595***	0.481***	0.399***	0.598***
	[0.028]	[0.045]	[0.069]	[0.030]
Outcome Mean	0.436	0.436	0.436	0.436
	(0.496)	(0.496)	(0.496)	(0.496)
Panel B: Effect on Reported Turnout				
Reduced Form (ITT)	-0.021***	-0.026***	-0.038***	-0.021**
	[0.005]	[0.009]	[0.014]	[0.009]
Second Stage (ToT)	-0.035***	-0.054***	-0.097***	-0.035**
	[0.009]	[0.018]	[0.037]	[0.014]
Outcome Mean	0.677	0.677	0.677	0.677
	(0.124)	(0.124)	(0.124)	(0.124)
Panel C: Effect on Incumbent's Votes				
Reduced Form (ITT)	-24.717***	-22.947**	-27.601*	-23.611***
	[5.666]	[9.399]	[14.590]	[8.915]
Second Stage (ToT)	-41.572***	-47.657**	-69.258*	-39.477***
	[9.357]	[19.318]	[37.488]	[14.620]
Outcome Mean	502.540	502.540	502.540	502.540
	(165.244)	(165.244)	(165.244)	(165.244)
Panel D: Effect on Others' Votes				
Reduced Form (ITT)	-1.465	-8.669*	-10.038	-2.352
	[2.847]	[4.755]	[7.331]	[3.390]
Second Stage (ToT)	-2.465	-18.005*	-25.188	-3.933
	[4.794]	[10.074]	[19.109]	[5.681]
Outcome Mean	152.477	152.477	152.477	152.477
	(66.269)	(66.269)	(66.269)	(66.269)
Region FEs	No	No	No	Yes
Commission FEs	Yes	Yes	Yes	No
# Observations	5721	5721	5721	5721

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the robustness of effects on voting to alternative functional forms. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate functions on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table A.12: Robustness of Effects on Voting to Alternative Bandwidths

Bandwidth:	400	300	500	CCT	CCT	CCT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Share of Video-Monitored Polling Stations						
First Stage	0.595***	0.550***	0.622***	0.498***	0.565***	0.475***
	[0.028]	[0.034]	[0.024]	[0.043]	[0.032]	[0.049]
Outcome Mean	0.436	0.453	0.419	0.464	0.446	0.471
	(0.496)	(0.498)	(0.493)	(0.499)	(0.497)	(0.499)
Panel B: Effect on Reported Turnout						
Reduced Form (ITT)	-0.021***	-0.023***	-0.021***	-0.025***	-0.022***	-0.027***
	[0.005]	[0.007]	[0.005]	[0.008]	[0.006]	[0.009]
Second Stage (ToT)	-0.035***	-0.042***	-0.034***	-0.050***		
	[0.009]	[0.012]	[0.007]	[0.017]		
Outcome Mean	0.677	0.675	0.679	0.674		
	(0.124)	(0.125)	(0.125)	(0.124)		
Panel C: Effect on Incumbent's Votes						
Reduced Form (ITT)	-24.717***	-24.161***	-25.820***	-23.555**	-23.839***	-24.439**
	[5.666]	[7.036]	[4.947]	[9.190]	[6.535]	[10.597]
Second Stage (ToT)	-41.572***	-43.936***	-41.513***		-42.157***	
	[9.357]	[12.540]	[7.850]		[11.331]	
Outcome Mean	502.540	506.617	492.012		504.551	
	(165.244)	(152.813)	(181.102)		(156.120)	
Panel D: Effect on Others' Votes						
Reduced Form (ITT)	-1.465	-4.234	-0.195	-7.029	-3.302	-7.393
	[2.847]	[3.578]	[2.477]	[4.751]	[3.310]	[5.622]
Second Stage (ToT)	-2.465	-7.699	-0.313			-15.567
	[4.794]	[6.538]	[3.983]			[11.975]
Outcome Mean	152.477	153.747	149.535			154.815
	(66.269)	(63.114)	(71.697)			(59.095)
Region FEs	No	No	No	No	No	No
Commission FEs	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	5721	4040	7599	2812	4522	2352

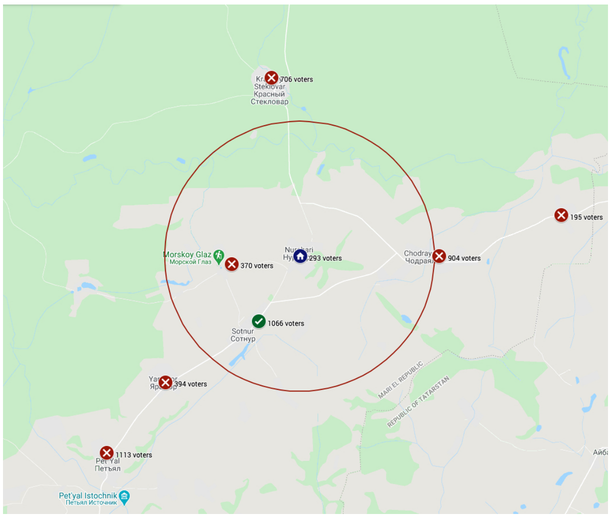
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the robustness of effects on voting to alternative bandwidths. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, exclude election commission clusters with a single observation, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is specified in each column; CCT corresponds to the optimal bandwidth, computed separately for each outcome using the procedures by Calonico, Cattaneo and Titiunik (2014). Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

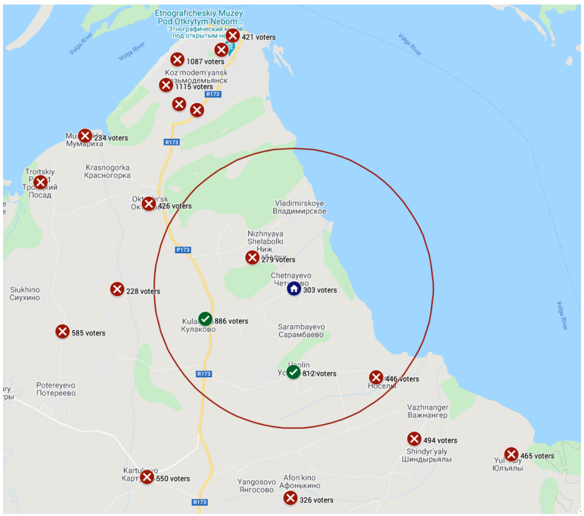
G. Displacement Effects

Figure A.7: Estimating Vote Displacement, Selection of Polling Stations

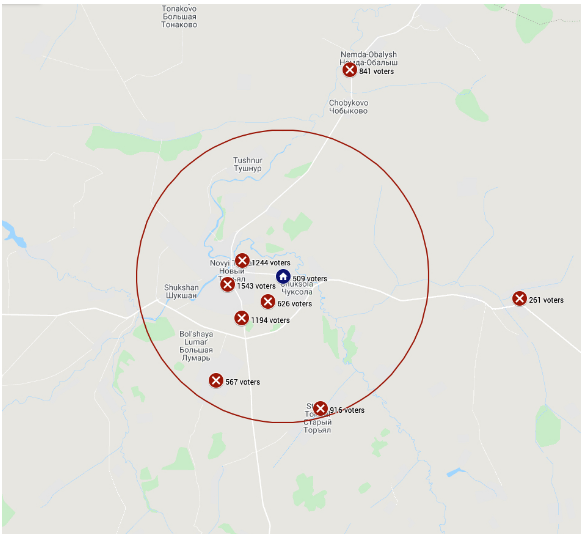
Panel A



Panel B



Panel C



Notes: This figure shows examples of polling stations included and excluded from the analysis of displacement effects in the radius of 5 kilometers. Panel A shows an example of the included unmonitored polling station, which has one neighbor with 1,000–1,400 voters. Panel B shows an example of the included unmonitored polling station, which has two neighbors with 600-1,000 registered voters. Panel C shows an example of excluded unmonitored polling station because it has a neighbor with more than 1,400 voters.

Table A.13: Effects on Vote Displacement, Robustness to Pooling Neighbors

Radius:	3 km		5 km		7 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Share of Video-Monitored Neighboring Polling Stations						
First Stage	0.586***	0.594***	0.646***	0.650***	0.636***	0.628***
	[0.077]	[0.064]	[0.072]	[0.058]	[0.064]	[0.058]
Outcome Mean	0.192	0.192	0.187	0.187	0.164	0.164
	(0.394)	(0.394)	(0.390)	(0.390)	(0.371)	(0.371)
Panel B: Effect on Reported Turnout						
Reduced Form (ITT)	0.042	0.013	0.018	0.011	0.025	0.022
	[0.028]	[0.016]	[0.026]	[0.014]	[0.023]	[0.015]
Second Stage (2SLS)	0.072	0.022	0.028	0.017	0.039	0.036
	[0.050]	[0.027]	[0.041]	[0.022]	[0.037]	[0.024]
Outcome Mean	0.687	0.687	0.698	0.698	0.712	0.712
	(0.126)	(0.126)	(0.126)	(0.126)	(0.130)	(0.130)
Panel C: Effect on Incumbent's Vote Share						
Reduced Form (ITT)	0.036	0.024**	0.027	0.030***	0.036**	0.035***
	[0.022]	[0.011]	[0.021]	[0.011]	[0.018]	[0.010]
Second Stage (2SLS)	0.061	0.040**	0.042	0.047***	0.057**	0.056***
	[0.038]	[0.018]	[0.033]	[0.018]	[0.028]	[0.016]
Outcome Mean	0.746	0.746	0.755	0.755	0.766	0.766
	(0.091)	(0.091)	(0.095)	(0.095)	(0.097)	(0.097)
Panel D: Effect on Others' Vote Share						
Reduced Form (ITT)	-0.035*	-0.023**	-0.026	-0.029***	-0.035**	-0.034***
	[0.021]	[0.011]	[0.021]	[0.011]	[0.017]	[0.010]
Second Stage (2SLS)	-0.060*	-0.039**	-0.040	-0.045**	-0.055**	-0.054***
	[0.036]	[0.018]	[0.032]	[0.017]	[0.027]	[0.016]
Outcome Mean	0.242	0.242	0.233	0.233	0.224	0.224
	(0.088)	(0.088)	(0.092)	(0.092)	(0.094)	(0.094)
Region FEs	No	Yes	No	Yes	No	Yes
Commission FEs	No	No	No	No	No	No
# Observations	2667	2667	3414	3414	4146	4146

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the robustness of vote displacement effects to pooling neighboring polling stations instead of taking means. This estimation (i) excludes unmonitored stations, which have neighbors with more than 1,400 voters (upper bandwidth boundary); (ii) excludes neighbors with fewer than 600 registered voters (lower bandwidth boundary); (iii) excludes neighbors outside of the specified radius from unmonitored polling stations; (iv) restricts the sample to unmonitored polling places, which have neighbors on one side of the cutoff; (v) pools all neighbors. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

Table A.14: Effects on Vote Displacement, Robustness to a Single Neighbor

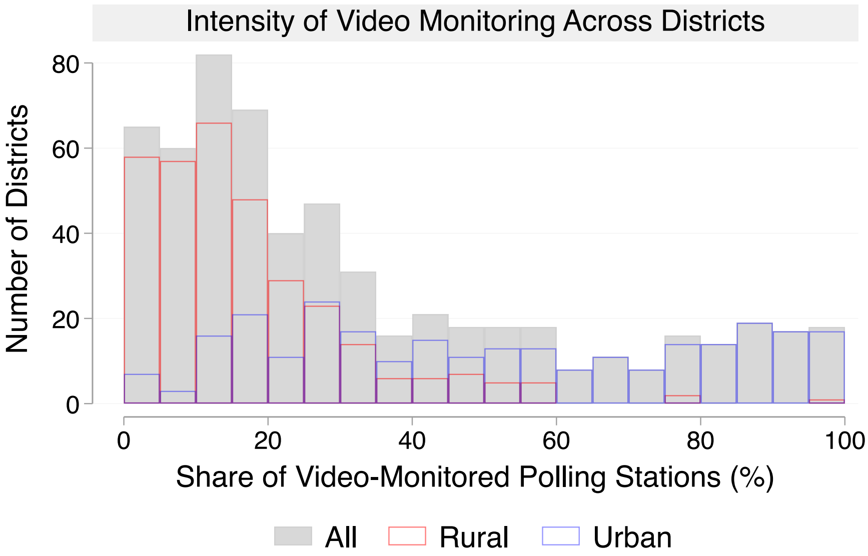
Radius:	3 km		5 km		7 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Share of Video-Monitored Neighboring Polling Stations						
First Stage	0.499***	0.518***	0.561***	0.575***	0.603***	0.609***
	[0.085]	[0.086]	[0.078]	[0.076]	[0.066]	[0.064]
Outcome Mean	0.194	0.194	0.173	0.173	0.167	0.167
	(0.396)	(0.396)	(0.379)	(0.379)	(0.373)	(0.373)
Panel B: Effect on Reported Turnout						
Reduced Form (ITT)	-0.001	0.005	-0.001	0.011	0.004	0.000
	[0.026]	[0.020]	[0.023]	[0.017]	[0.021]	[0.016]
Second Stage (2SLS)	-0.002	0.009	-0.002	0.019	0.006	0.000
	[0.053]	[0.038]	[0.041]	[0.030]	[0.035]	[0.026]
Outcome Mean	0.731	0.731	0.743	0.743	0.752	0.752
	(0.133)	(0.133)	(0.130)	(0.130)	(0.130)	(0.130)
Panel C: Effect on Incumbent's Votes Share						
Reduced Form (ITT)	0.022	0.020	0.019	0.027**	0.020	0.020**
	[0.020]	[0.013]	[0.015]	[0.012]	[0.013]	[0.010]
Second Stage (2SLS)	0.044	0.039	0.034	0.047**	0.034	0.033*
	[0.040]	[0.025]	[0.028]	[0.022]	[0.022]	[0.017]
Outcome Mean	0.782	0.782	0.796	0.796	0.800	0.800
	(0.091)	(0.091)	(0.088)	(0.088)	(0.086)	(0.086)
Panel D: Effect on Others' Vote Share						
Reduced Form (ITT)	-0.022	-0.020	-0.019	-0.026**	-0.021	-0.020**
	[0.019]	[0.013]	[0.015]	[0.012]	[0.013]	[0.010]
Second Stage (2SLS)	-0.044	-0.039	-0.034	-0.046**	-0.035	-0.034**
	[0.038]	[0.026]	[0.027]	[0.022]	[0.022]	[0.017]
Outcome Mean	0.206	0.206	0.194	0.194	0.191	0.191
	(0.087)	(0.087)	(0.085)	(0.085)	(0.084)	(0.084)
Region FEs	No	Yes	No	Yes	No	Yes
Commission FEs	No	No	No	No	No	No
# Observations	727	727	1063	1063	1433	1433

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the robustness of vote displacement effects to a single neighbor restriction. This estimation (i) excludes unmonitored stations, which have neighbors with more than 1,400 voters (upper bandwidth boundary); (ii) excludes neighbors with fewer than 600 registered voters (lower bandwidth boundary); (iii) excludes neighbors outside of the specified radius from unmonitored polling stations; (iv) restricts the sample to unmonitored polling places, which have a single neighbor on either side of the cutoff. Specifications use data at the polling station level, exclude intermediate data points in the interval of [975, 1000) registered voters, and include separate linear trends on each cutoff side with triangular weights. The bandwidth is 400 registered voters on both sides of the cutoff. Standard errors clustered by election commission are reported in brackets. Standard deviations are reported in parentheses.

H. Accountability Effects: Additional Results

Figure A.8: Intensity of Video Monitoring across Districts



Notes: This figure plots the histogram of the share of video-monitored polling stations in bins of 5 percentage points. The unit of observation is a district (rayon). The district is urban if more than half of its population lives in urban areas.

Table A.15: Effects on Changes in Public Goods Spending by Economic Sector

Sector:	All	Administrative	Infrastructure	Education	Social Services	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Change in the Number of Projects						
Second Stage (ToT)	-0.027	-0.006	0.020	-0.031	-0.025**	0.005*
	[0.034]	[0.017]	[0.012]	[0.019]	[0.011]	[0.003]
Outcome Mean	-0.992	0.237	0.180	-0.884	-0.493	0.087
	(10.860)	(5.582)	(3.826)	(6.198)	(3.530)	(0.803)
Panel B: Change in the Total Value of Projects per Capita						
Second Stage (ToT)	-1.814	-1.284	-0.019	-0.048	-0.098	-0.002
	[1.295]	[0.936]	[0.403]	[0.292]	[0.092]	[0.018]
Outcome Mean	0.756	-8.025	8.176	1.262	-2.478	0.828
	(406.898)	(295.031)	(127.530)	(93.338)	(29.013)	(5.755)
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	596	596	596	596	596	596

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the heterogeneous effects of video monitoring on changes in district public goods spending before the election by the economic sector. Changes in the number of projects and their total value are defined as the differences between December 2017 – March 2018 (four months before the election) and December 2016 – March 2017 (a corresponding period a year earlier), with winsorized values in the bottom- and top-2%. Specifications use data at the district level, instrument the share of video-monitored polling stations with the percentage of polling places above the cutoff in the bandwidth of 400 registered voters, and control for the share of polling stations in the bandwidth. Standard errors clustered by region are reported in brackets. Standard deviations are reported in parentheses.

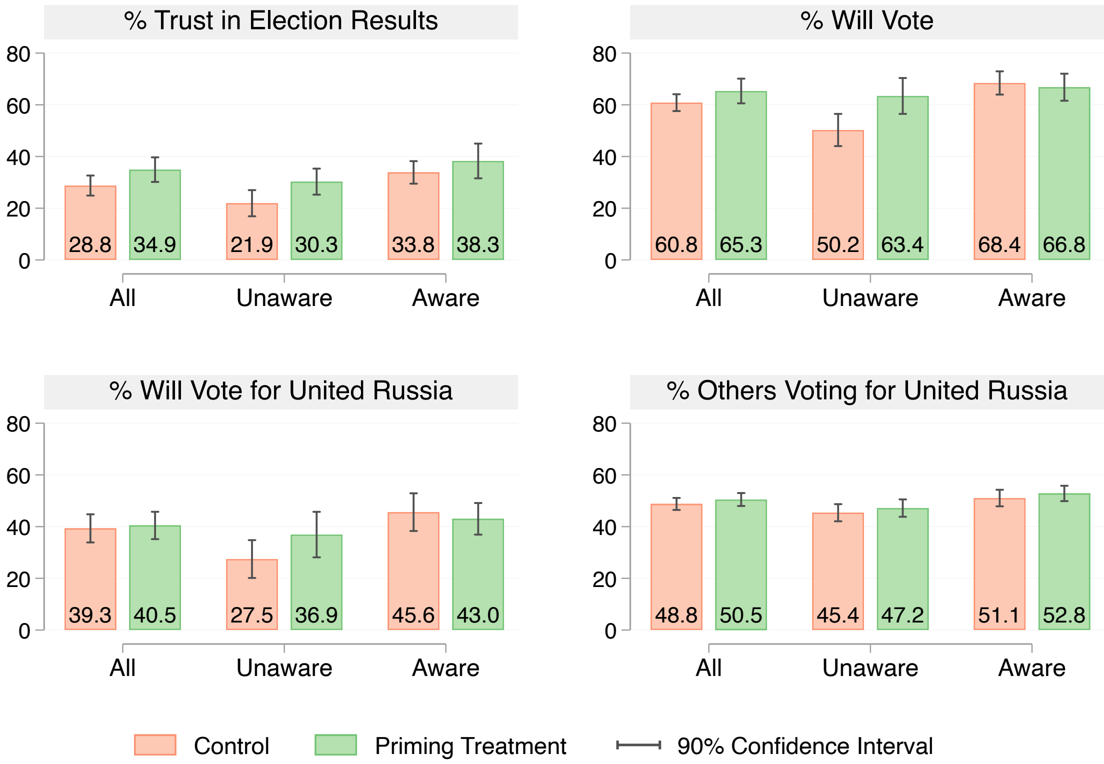
I. Survey Experiment: Additional Results

Table A.16: Survey Experiment: Summary Statistics and Full Balance Test

Statistic:	Mean Full	Mean Control No Sens. Item	Mean Control Sens. Item	Mean Priming Treatment No Sens. Item	Mean Priming Treatment Sens. Item	P-value	# Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Geographical Area							
Urban (=1)	0.78	0.79	0.77	0.76	0.78	0.86	1097
	(0.42)	[0.05]	[0.05]	[0.05]	[0.05]		
Moscow (=1)	0.13	0.13	0.14	0.10	0.15	0.66	1097
	(0.33)	[0.11]	[0.12]	[0.09]	[0.13]		
St. Petersburg (=1)	0.05	0.04	0.05	0.07	0.05	0.75	1097
	(0.22)	[0.04]	[0.04]	[0.07]	[0.05]		
Rural (=1)	0.22	0.21	0.23	0.24	0.22	0.86	1097
	(0.42)	[0.05]	[0.05]	[0.05]	[0.05]		
Individual Characteristics							
Female (=1)	0.56	0.56	0.57	0.58	0.52	0.48	1097
	(0.50)	[0.03]	[0.02]	[0.02]	[0.03]		
Age	46.21	44.65	46.41	47.94	45.79	0.16	1097
	(16.50)	[1.07]	[0.99]	[1.15]	[0.93]		
Incomplete Secondary School (=1)	0.05	0.06	0.03	0.04	0.05	0.46	1097
	(0.21)	[0.02]	[0.01]	[0.01]	[0.01]		
Secondary or Vocational Education (=1)	0.64	0.63	0.66	0.62	0.66	0.67	1097
	(0.48)	[0.04]	[0.04]	[0.03]	[0.03]		
Higher Education (=1)	0.31	0.31	0.31	0.34	0.29	0.66	1097
	(0.46)	[0.04]	[0.04]	[0.04]	[0.03]		
Employed (=1)	0.60	0.59	0.58	0.55	0.66	0.07	1097
	(0.49)	[0.04]	[0.03]	[0.04]	[0.03]		
Retired (=1)	0.26	0.24	0.26	0.32	0.20	0.01	1097
	(0.44)	[0.03]	[0.03]	[0.03]	[0.02]		
Income Level	3.18	3.19	3.15	3.19	3.21	0.92	1096
	(1.02)	[0.08]	[0.09]	[0.07]	[0.09]		
Media Consumption							
Daily Internet User (=1)	0.60	0.64	0.61	0.55	0.62	0.23	1097
	(0.49)	[0.04]	[0.03]	[0.03]	[0.05]		
Internet Non-User (=1)	0.22	0.19	0.24	0.23	0.20	0.48	1097
	(0.41)	[0.03]	[0.03]	[0.03]	[0.04]		
Daily TV User (=1)	0.67	0.67	0.65	0.67	0.70	0.80	1097
	(0.47)	[0.03]	[0.04]	[0.03]	[0.03]		
TV Non-User (=1)	0.08	0.08	0.09	0.09	0.07	0.88	1097
	(0.28)	[0.02]	[0.02]	[0.02]	[0.01]		
Government Approval							
Approves President's Work (=1)	0.66	0.66	0.62	0.68	0.71	0.11	1080
	(0.47)	[0.03]	[0.04]	[0.03]	[0.03]		
Approves State Duma's Work (=1)	0.38	0.38	0.36	0.36	0.42	0.45	1066
	(0.49)	[0.04]	[0.03]	[0.04]	[0.03]		
Approves Government's Work (=1)	0.45	0.46	0.44	0.45	0.46	0.94	1074
	(0.50)	[0.04]	[0.03]	[0.04]	[0.03]		
Approves Any (=1)	0.71	0.74	0.67	0.69	0.73	0.28	1094
	(0.46)	[0.04]	[0.03]	[0.03]	[0.03]		
Transparency Technologies							
Aware of Transparent Ballot Boxes (=1)	0.56	0.56	0.57	0.52	0.60	0.35	1097
	(0.50)	[0.03]	[0.04]	[0.03]	[0.03]		
Aware of Video Monitoring (=1)	0.60	-	-	0.58	0.62	-	538
	(0.49)			[0.03]	[0.04]		
Will Observe Election Online (=1)	0.15	-	-	0.17	0.14	-	523
	(0.36)			[0.03]	[0.02]		

Notes: This table reports summary statistics and balance test of random assignment of 1,097 respondents to priming and list experiment treatments in the survey experiment. Column (1) reports the mean of each variable, with standard deviations in parentheses, for the full sample. Columns (2)–(5) report the mean of each variable, with standard errors clustered by region in brackets, for each experimental condition. Column (6) reports the p-value of an F-test of joint equality of means between four experimental groups.

Figure A.9: Survey Experiment: Effects of Priming, Excluding Moscow



Notes: This figure plots the effects of the priming treatment, which provides information about video monitoring. The sample consists of 958 respondents who resided outside Moscow and answered that there would be elections in their locality. Each subfigure plots the effects for the full sample (All), the subsample of respondents who were not aware of another transparency tool, transparent ballot boxes (Unaware), and the subsample of respondents who were aware of transparent ballot boxes (Aware). Brackets denote 90% confidence intervals using standard errors clustered by region.

Table A.17: Survey Experiment: Effects of Priming on Voting and Attitudes, Excluding Moscow

Outcome:	All					Unaware					Aware				
	Control Mean (1)	Treatment Effect (2)	P-value (3)	Fisher's P-value (4)	MHT Q-value (5)	Control Mean (6)	Treatment Effect (7)	P-value (8)	Fisher's P-value (9)	MHT Q-value (10)	Control Mean (11)	Treatment Effect (12)	P-value (13)	Fisher's P-value (14)	MHT Q-value (15)
Voting/Supporting United Russia															
Will Vote (=1)	0.61	0.05	0.17	0.15	.78	0.50	0.13***	0.01	0.01	.03	0.68	-0.02	0.69	0.71	1
	(0.49)	[0.03]				(0.50)	[0.05]				(0.47)	[0.04]			
Will Vote for United Russia (=1)	0.39	0.01	0.79	0.77	.78	0.27	0.09	0.15	0.12	.18	0.46	-0.03	0.64	0.61	1
	(0.49)	[0.04]				(0.45)	[0.07]				(0.50)	[0.06]			
% Others Voting for United Russia	48.77	1.71	0.29	0.33	.78	45.38	1.79	0.43	0.50	.3	51.05	1.77	0.41	0.45	1
	(24.47)	[1.61]				(22.13)	[2.24]				(25.72)	[2.13]			
Trust in Election															
Trust in Election Results (=1)	0.29	0.06**	0.03	0.05	.07	0.22	0.08**	0.04	0.06	.09	0.34	0.04	0.20	0.30	.68
	(0.45)	[0.03]				(0.42)	[0.04]				(0.47)	[0.03]			
Election Will Lead to Improvements (=1)	0.38	-0.02	0.43	0.48	.28	0.33	-0.02	0.61	0.66	.44	0.42	-0.02	0.53	0.58	.68
	(0.49)	[0.03]				(0.47)	[0.04]				(0.49)	[0.04]			
Democratic Values															
Russia is a Democracy (=1)	0.83	-0.05*	0.07	0.07	.27	0.82	-0.06	0.15	0.14	.37	0.84	-0.04	0.25	0.29	1
	(0.38)	[0.03]				(0.38)	[0.04]				(0.37)	[0.03]			
# Democratic Values Agreed	2.27	-0.03	0.77	0.79	1	2.36	-0.20	0.18	0.19	.37	2.20	0.10	0.35	0.42	1
	(1.55)	[0.09]				(1.67)	[0.15]				(1.46)	[0.11]			
Tax Compliance is Important (=1)	0.66	0.01	0.80	0.85	1	0.64	0.03	0.46	0.53	.37	0.67	-0.01	0.75	0.77	1
	(0.48)	[0.03]				(0.48)	[0.04]				(0.47)	[0.04]			
Indices															
Voting/Supporting United Russia Index	0.00	0.06*	0.10	0.10	.25	0.00	0.18***	0.00	0.00	0	0.00	-0.01	0.91	0.90	.97
	(0.58)	[0.04]				(0.55)	[0.05]				(0.59)	[0.05]			
Trust in Election Index	0.00	0.04	0.34	0.39	.34	0.00	0.08	0.23	0.30	.3	0.00	0.02	0.71	0.75	.97
	(0.80)	[0.05]				(0.76)	[0.07]				(0.81)	[0.06]			
Democracy Index	0.00	-0.04	0.19	0.23	.34	0.00	-0.07	0.17	0.18	.3	0.00	-0.02	0.69	0.73	.97
	(0.58)	[0.03]				(0.56)	[0.05]				(0.59)	[0.04]			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the results of the survey experiment based on the pre-analysis plan. Columns (1)–(5) report results for the subsample of 958 respondents who reside outside of Moscow and answered that there were going to be elections in their locality. Columns (6)–(10) and (11)–(15) report results separately for subsamples split by awareness of transparent ballot boxes, which serves as a proxy for awareness of electoral transparency policies in general. Columns (1), (6), and (11) report the mean level of outcomes for the control group, with standard deviations in parentheses. Columns (2), (7), and (12) report the priming treatment effects of a reminder about video monitoring, with standard errors clustered at the regional level in brackets. Columns (3), (8), and (13) report sampling-based p-values from a standard t-test. Columns (4), (9), and (14) report randomization-based Fisher's exact p-values for the sharp null hypothesis of no effect from permutation tests with 10,000 repetitions. Columns (5), (10), and (15) report pre-specified q-values which adjust the sampling-based p-values for the false discovery rate (FDR) within each hypothesis group following Anderson (2008) and Benjamini, Krieger, and Yekutieli (2006) and the family-wise error rate (FWER) between indices following Anderson (2008) and Westfall and Young (1993).

Figure A.10: Survey Experiment: Heterogeneous Effects of Priming (Part 1)

Notes: This figure plots the heterogeneous effects of the priming treatment, which provides information about video monitoring. Brackets denote 90% confidence intervals using standard errors clustered by region.

Figure A.11: Survey Experiment: Heterogeneous Effects of Priming (Part 2)

Notes: This figure plots the heterogeneous effects of the priming treatment, which provides information about video monitoring. Brackets denote 90% confidence intervals using standard errors clustered by region.