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Essays on the Political Economy of Development



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# Essays on the Political Economy of Development

A dissertation submitted in partial satisfaction of the
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by

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2022

The dissertation of Anastasiia Faikina is approved,
and it is acceptable in quality and form for publication
on microfilm and electronically.

University of California San Diego

2022

DEDICATION

To the brave people of Ukraine and all the fearless people around the world who fight for freedom and democracy.

EPIGRAPH

War is a product of despotism. Without despotism, there could be no war;
there could be fights, but not war. Despotism produces war, and war sustains despotism.
Those who want to fight war must fight only against despotism.

*Leo Tolstoy*

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ABSTRACT OF THE DISSERTATION

Essays on the Political Economy of Development

by

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Doctor of Philosophy in Economics

University of California San Diego, 2022

Professor Eli Berman, Chair

Economic development relies on political institutions and policies set by governments. This dissertation studies policies aimed at improving the functioning of three core state institutions: elections, bureaucracy, and local governments.

Chapter 1 studies the impacts of broadcast election video monitoring on voting in Russia. I use a regression discontinuity design based on the size of polling stations in the presidential election to estimate causal effects. I find that video monitoring reduced reported voter turnout and votes for the incumbent, suggesting a decrease in fraud. However, that decrease was partially offset by increased votes for the incumbent in neighboring unmonitored polling stations. Using a survey experiment, I find that higher awareness about video monitoring increased citizens' trust

in elections and their willingness to vote. All in all, these results suggest that video monitoring might improve citizens' attitudes toward the government at a low cost in terms of net lost votes.

Chapter 2 reports evidence from a randomized evaluation of a reform intended to improve the Afghan government's capacity to identify their employees and pay them for their work. This reform transitioned employees from being paid by trusted agents and banks to mobile money payments. We find that registration for a mobile money wallet with biometric identification helped eliminate 'ghost' employees from the payroll, albeit imperfectly. Mobile payments improved employees' satisfaction with how they are paid and promoted a pathway toward their financial inclusion, especially in the secure urban areas previously serviced by banks. Overall, we find that state modernization is possible even in the shadow of war.

Chapter 3 evaluates the effects of fiscal and administrative decentralization of local governments on economic activity in Ukraine. I exploit variation in the timing of decentralization across communities to estimate causal effects on firms and self-employed individuals using difference-in-differences methods. I find that decentralization reduced the number of active self-employed individuals, mainly in rural areas. There were, however, no significant changes in the number of active firms. These results suggest that successfully applied governance reforms might have unintended economic consequences.

# Chapter 1

# Illusive Transparency? Evidence on Election Video Monitoring

**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 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.

## 1.1 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 1.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,

[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.

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 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]

[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.

[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.

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 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

[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.

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. 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

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

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

[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.

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 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 1.2 sets out the institutional background, and Section 1.3 outlines the theoretical predictions. Section 1.4 describes the data sources and introduces the primary empirical strategy. In Section 1.5, I estimate the direct effects of video monitoring on election outcomes and discuss the mechanisms. Section 1.6 evaluates the indirect impacts of video monitoring on the displacement of votes and public goods spending. In Section 1.7, I present evidence from my original survey experiment and use this to adjudicate among the theories of why autocrats invest in video monitoring. Section 1.8 concludes and makes policy recommendations.

## 1.2 Background

### 1.2.1 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, 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).

[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.

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.

### 1.2.2 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%

[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.

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 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.

## 1.3 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

[12] Appendix Figure 1.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.

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 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

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

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.

## 1.4 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.

### 1.4.1 Data

**Location of Video Monitoring.** Estimating the effects of video monitoring requires knowledge on which polling stations had webcams. I collected this information from 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 1.A.2 shows that studied regions are spread geographically across the entire country. Appendix Table 1.A.1 shows that studied regions have slightly lower average reported turnout and incumbent's vote share than the non-studied regions.

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

[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.

**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

[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.

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.

**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 1.7.1 describes the survey experiment design in detail.

### 1.4.2 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 1.1 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

[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.

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:

$$m_s = \alpha_1 + \mathbb{1}\{v_s \geq c\}\left[f_r(v_s - c) + \delta\right] + \mathbb{1}\{v_s < c\} f_l(c - v_s) + \varepsilon_{1s} \tag{1.1}$$

$$y_s = \alpha_2 + \tau m_s + \mathbb{1}\{v_s \geq c\} g_r(v_s - c) + \mathbb{1}\{v_s < c\} g_l(c - v_s) + \varepsilon_{2s} \tag{1.2}$$

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 $\delta$ estimates a jump in the probability of video monitoring at the threshold of 1,000 registered voters. The second-stage estimate $\tau$ 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 1.2 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 1.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 1.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.

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

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 1.51.5.2) 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.

## 1.5 Direct Effects

### 1.5.1 Implementation

The placement of video monitoring at the polling stations with more than 1,000 registered voters affects the scale of its implementation. Figure 1.1 and Table 1.1 show that 44% 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 1.3 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 1.2 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)

[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.

indicate that implementation was less strict in rural areas, where slightly fewer eligible polling stations received video monitoring.

### 1.5.2 Effects on Voting

Does video monitoring affect voting? We now turn to the first set of results. Panels B–D of Figure 1.3 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 1.2 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 1.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 1.A.3

[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).

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 1.7 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 1.2 test whether video monitoring has larger effects in rural areas. Column (4) shows that video monitoring leads to a significant 4.4 percentage points (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]

I provide evidence from a series of placebo tests to check identification assumptions.

[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.

First, Figure 1.A.6 and Table 1.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 1.A.9 shows that there are no placebo effects using alternative thresholds of 850 and 1,150 registered voters.

I also show robustness of the results to alternative specifications. Table 1.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 1.A.11 and 1.A.12 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.

### 1.5.3 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 1.3 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 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 1.4 shows that there are, at most, small effects on voter registration and its possible

[25] Voters can request early voting when they cannot visit an assigned polling station on election day because of vacation, business trip, work, studies, public duty, or poor health condition. Similarly, voters can request mobile voting at their residence because of poor health condition, disability, or house arrest.

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 1.4 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 1.5 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

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

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 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 1.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 1.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 1.A.5 and Table 1.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.

## 1.6 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.

### 1.6.1 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 1.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 1.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 1.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 1.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 1.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:

$$\bar{m}_s^{ngh} = \alpha_1 + \mathbb{1}\{\bar{v}_s^{ngh} \geq c\}\left[f_r(\bar{v}_s^{ngh} - c) + \delta\right] + \mathbb{1}\{\bar{v}_s^{ngh} < c\} f_l(c - \bar{v}_s^{ngh}) + \varepsilon_{1s} \tag{1.3}$$

$$y_s^{unm} = \alpha_2 + \tau \bar{m}_s^{ngh} + \mathbb{1}\{\bar{v}_s^{ngh} \geq c\} g_r(\bar{v}_s^{ngh} - c) + \mathbb{1}\{\bar{v}_s^{ngh} < c\} g_l(c - \bar{v}_s^{ngh}) + \varepsilon_{2s} \tag{1.4}$$

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 $\delta$ estimates a jump in the mean probability of video monitoring among neighboring

[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.

polling stations at the threshold of 1,000 registered voters. The second-stage estimate $\tau$ estimates the displacement effect from monitored neighbors to the unmonitored polling station $s$.

Figure 1.5 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 1.6 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 1.A.13 shows the robustness of these results to pooling all neighboring polling stations instead of taking the means. Appendix Table 1.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

[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).

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 the direct impact of video monitoring shown in Table 1.2.[29]

### 1.6.2 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 1.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:

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

$$\text{Share Monitored}_{dr} = \alpha + \delta \cdot \text{Share Above Cutoff BW}_{dr} + \eta \cdot \text{Share BW}_{dr} + \theta_r + \varepsilon_{dr} \quad (1.5)$$

$$\Delta\text{Spending}_{dr} = \beta + \tau \cdot \text{Share Monitored}_{dr} + \gamma \cdot \text{Share BW}_{dr} + \mu_r + \varepsilon_{dr} \quad (1.6)$$

where Share $\text{Monitored}_{dr}$ is a share of polling stations equipped with video monitoring in the district $d$ of the region $r$. Share Above Cutoff $\text{BW}_{dr}$ is a share of polling stations with more than 1,000 registered voters in the bandwidth, an instrument for Share $\text{Monitored}_{dr}$. Share $\text{BW}_{dr}$ is a share of polling stations in the bandwidth that controls for a non-random distribution of polling station sizes. The second-stage estimate $\tau$ 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 $\text{BW}_{dr}$, in both stages.

Table 1.7 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 1.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.

## 1.7 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.

### 1.7.1 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 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

[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.

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)

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 |

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**Results.** I pool the list experiment groups together for all outcomes except voter intimidation. Equation (1.7) summarizes the empirical strategy:

$$\mathrm{Y}_{ir} = \alpha + \tau\,\mathrm{Treat}_{ir} + \varepsilon_{ir} \tag{1.7}$$

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 1.8 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 1.A.16 shows the balance of covariates across both the priming and listing treatments.

The last part of Table 1.8 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 1.6 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 1.9 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 1.9 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

[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.

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

Appendix Figure 1.A.9 and Table 1.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 1.A.10 and 1.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.

### 1.7.2 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 1.6 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 1.5, 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.

Cross-randomization allows me to estimate the overall level of voter intimidation and how it changes with a reminder about video monitoring. Equation (1.8) shows the empirical strategy:

$$\#\,\text{Statements}_{ir} = \beta_0 + \beta_1\,\text{Treat}_{ir} + \beta_2\,\text{Sens}_{ir} + \beta_3\,\text{Treat}_{ir} \times \text{Sens}_{ir} + \varepsilon_{ir} \tag{1.8}$$

where $\text{Treat}_{ir}$ is equal to one if respondent $i$ from region $r$ received the priming treatment about video monitoring, $\text{Sens}_{ir}$ is equal to one if the respondent received a sensitive statement in the list experiment, and $\beta_3$ captures the effect of priming on 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/.

**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 1.10 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 1.10. 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 1.5.2, 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."

## 1.8 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.

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

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.

## 1.9 Acknowledgments

Chapter 1, in part, is currently being prepared for submission for publication of the material. Faikina, Anastasiia. "Illusive Transparency? Evidence on Election Video Monitoring." The dissertation author was the sole author of this material.

## 1.10 Figures and Tables

Panel A

Panel B

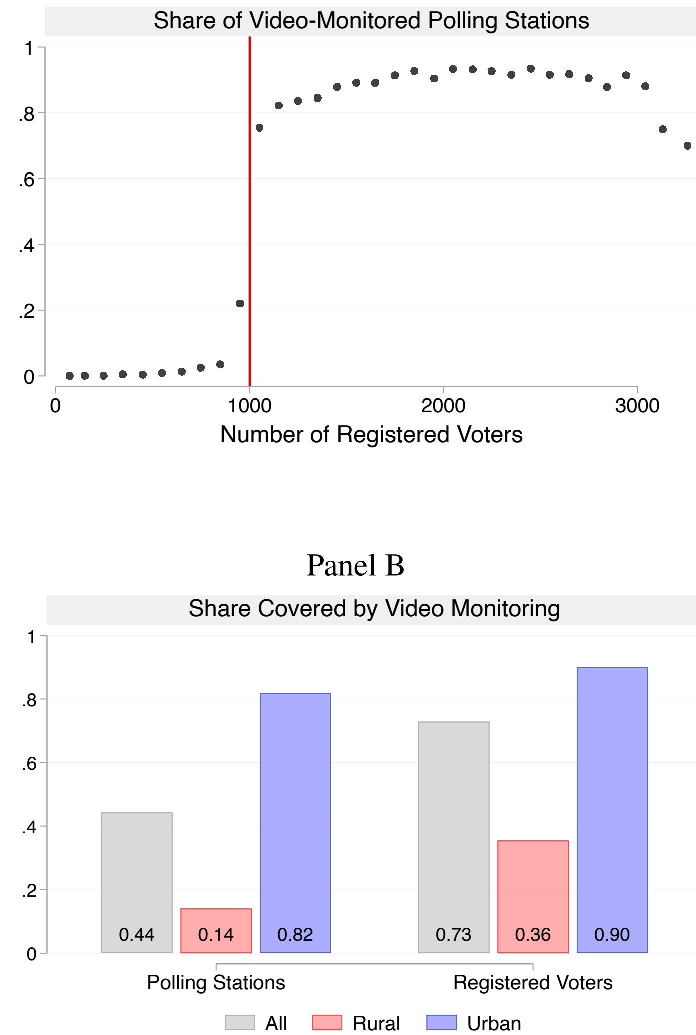


**Figure 1.1:** Video Monitoring Implementation

*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.

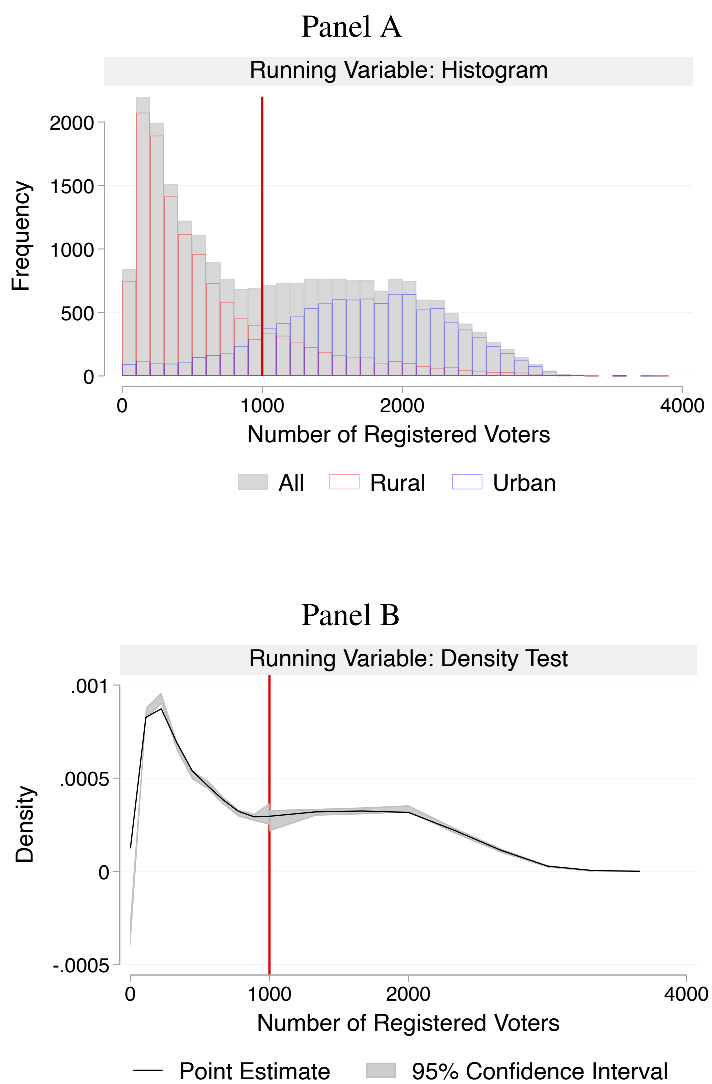


**Figure 1.2:** Manipulation Tests

*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 $= 0.42$).

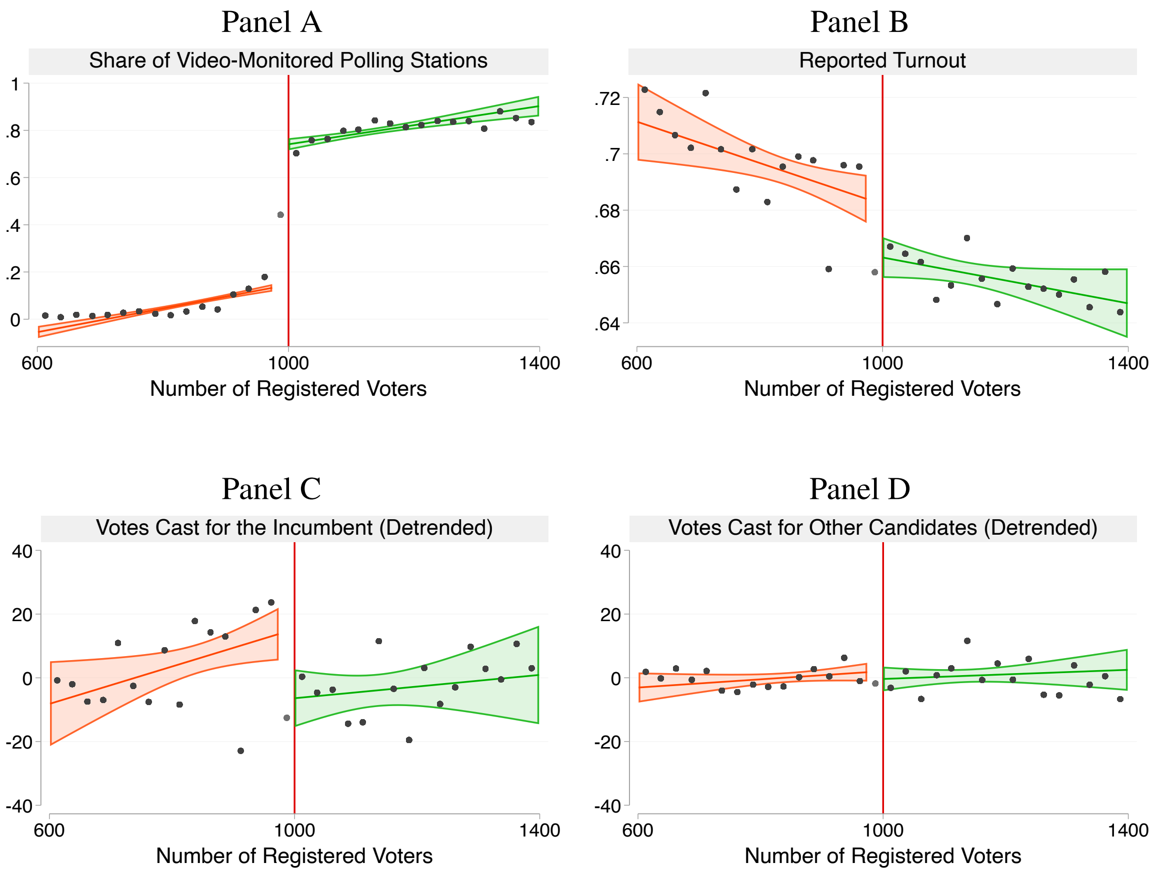


**Figure 1.3:** Effects of Video Monitoring on Voting

*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.

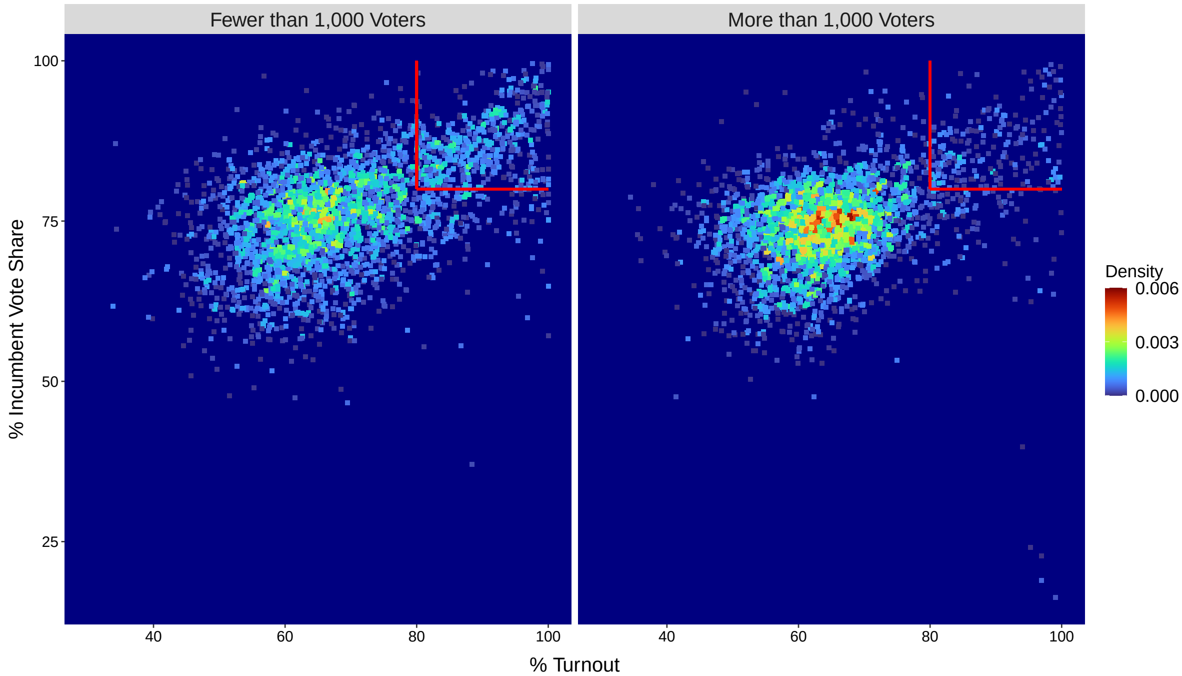


**Figure 1.4:** Effects of Video Monitoring on Fraud Indicators

*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).

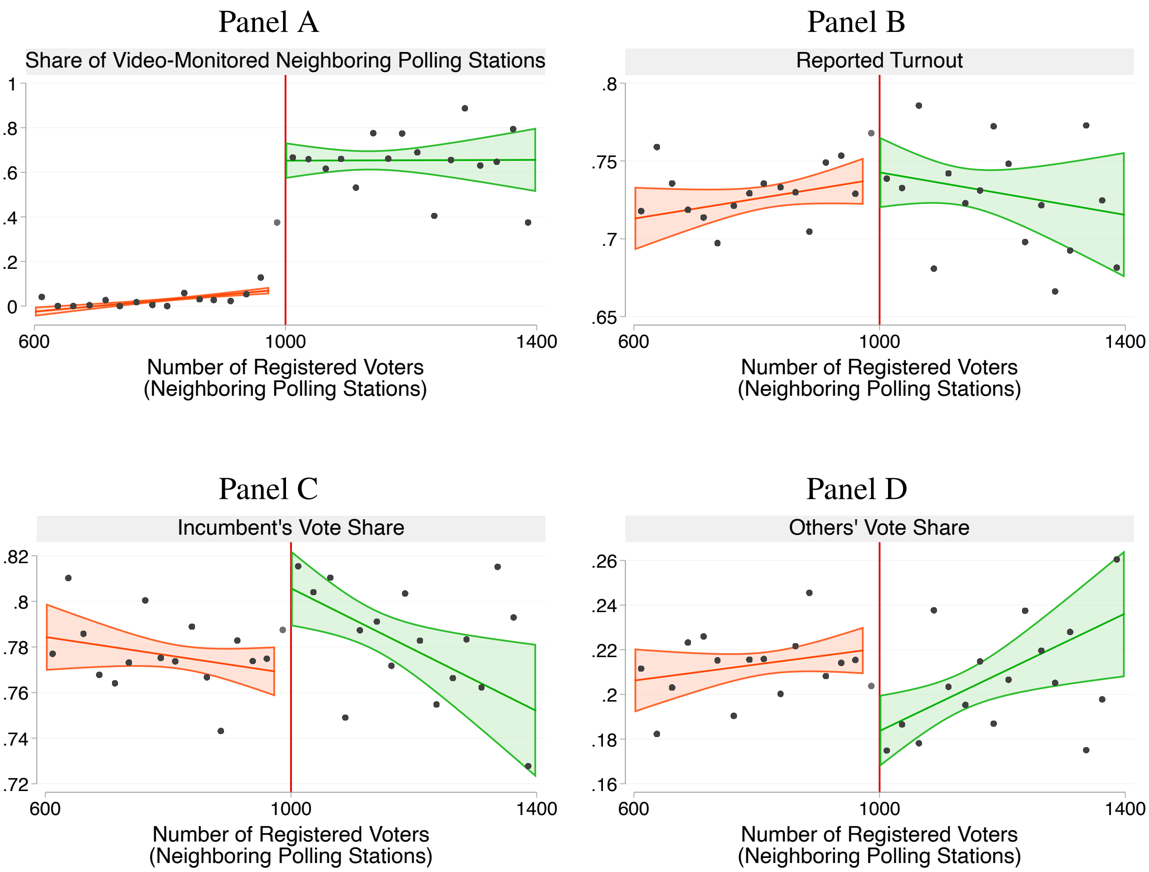


**Figure 1.5:** Vote Displacement

*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.

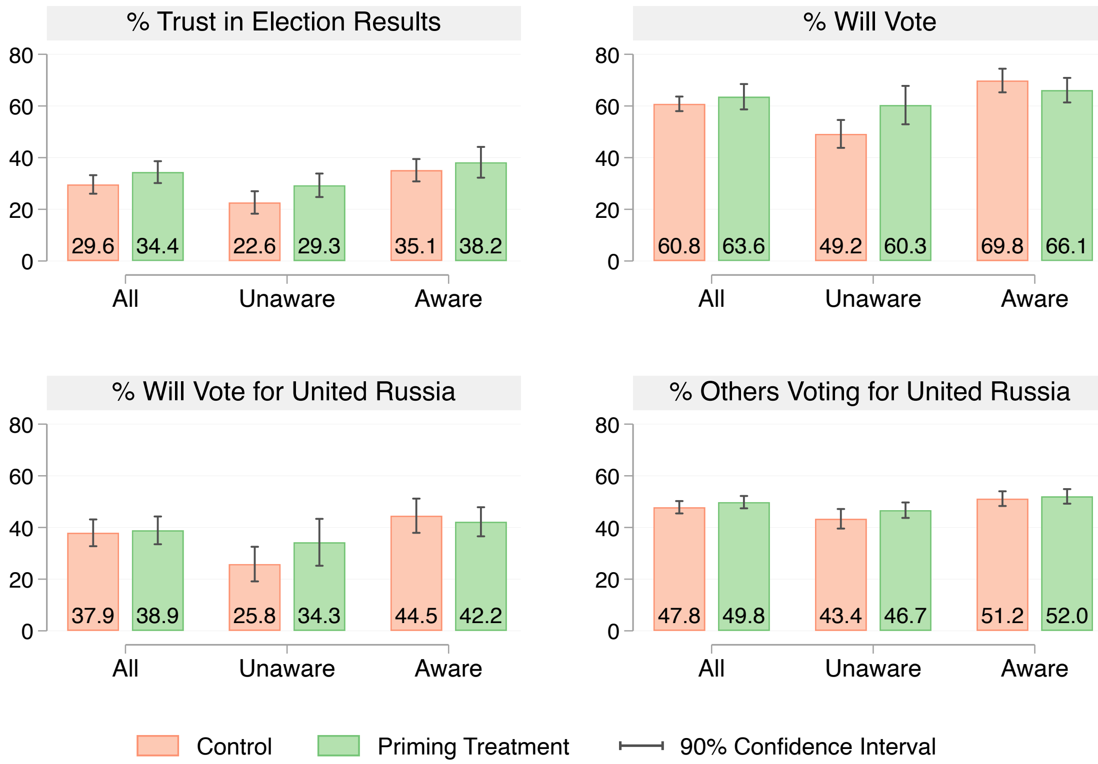


**Figure 1.6:** Survey Experiment: Effects of Priming on Voting and Attitudes

*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 1.1:** 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 1.2:** 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 1.3:** 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 1.4:** 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 1.5:** 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 1.6:** 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 1.7:** 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 1.8:** 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 1.9:** 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 1.10:** Survey Experiment: Effects of Priming on Voter Intimidation

| Outcome: # Statements Agreed | Sample (1) Full | (2) Unaware | (3) Aware |
|---|---|---|---|
| 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.

## 1.A Appendix: Additional Figures and Tables

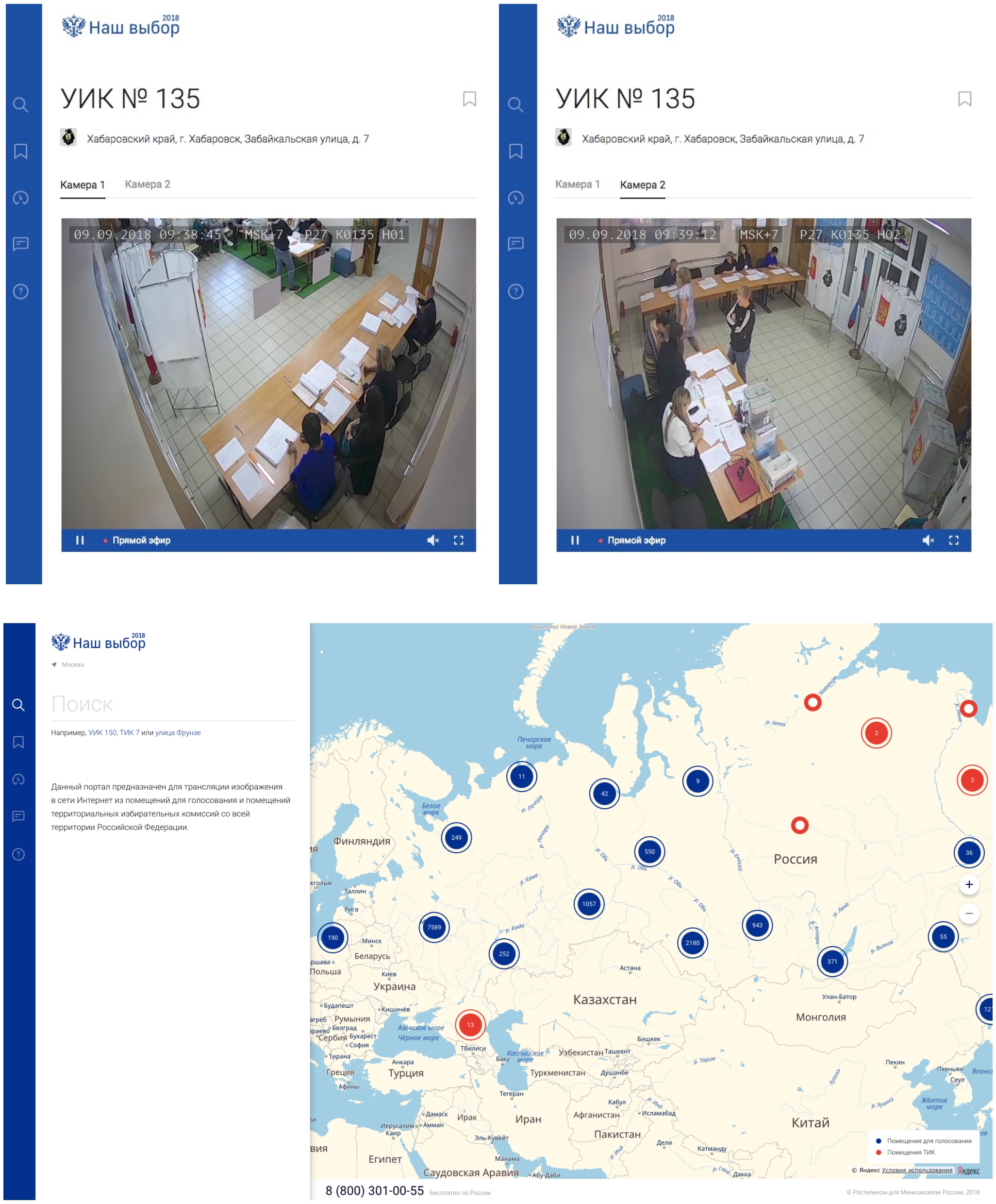


**Figure 1.A.1:** Screenshots of the Live-Stream Platform www.nashvybor2018.ru

*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.

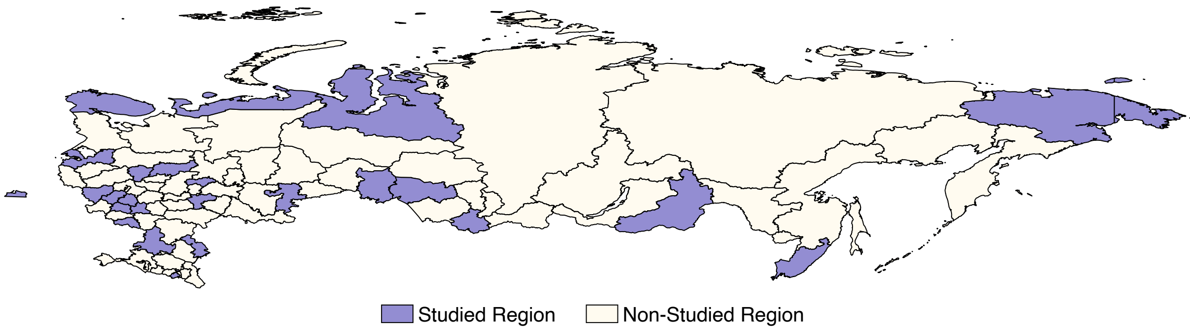


**Figure 1.A.2:** Map of Studied Regions of Russia

*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.

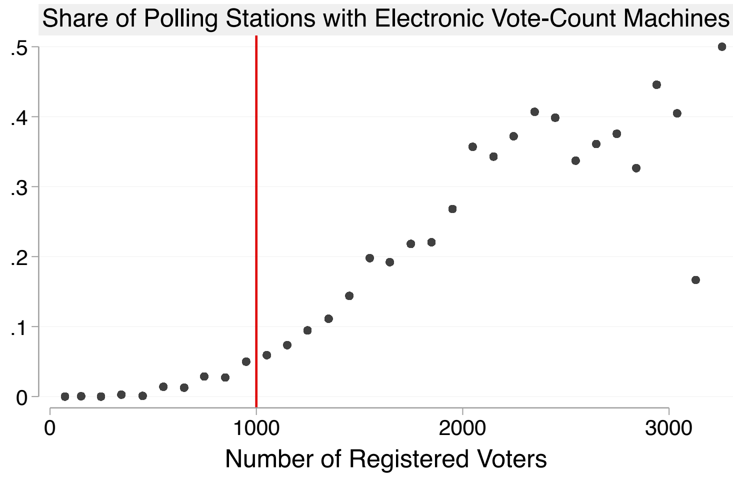


**Figure 1.A.3:** Electronic Vote-Count Machines

*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.

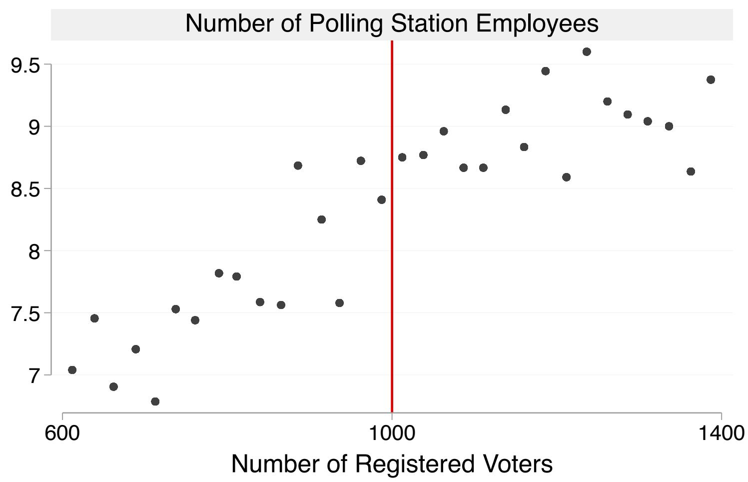


**Figure 1.A.4:** Polling Station Employees

*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.

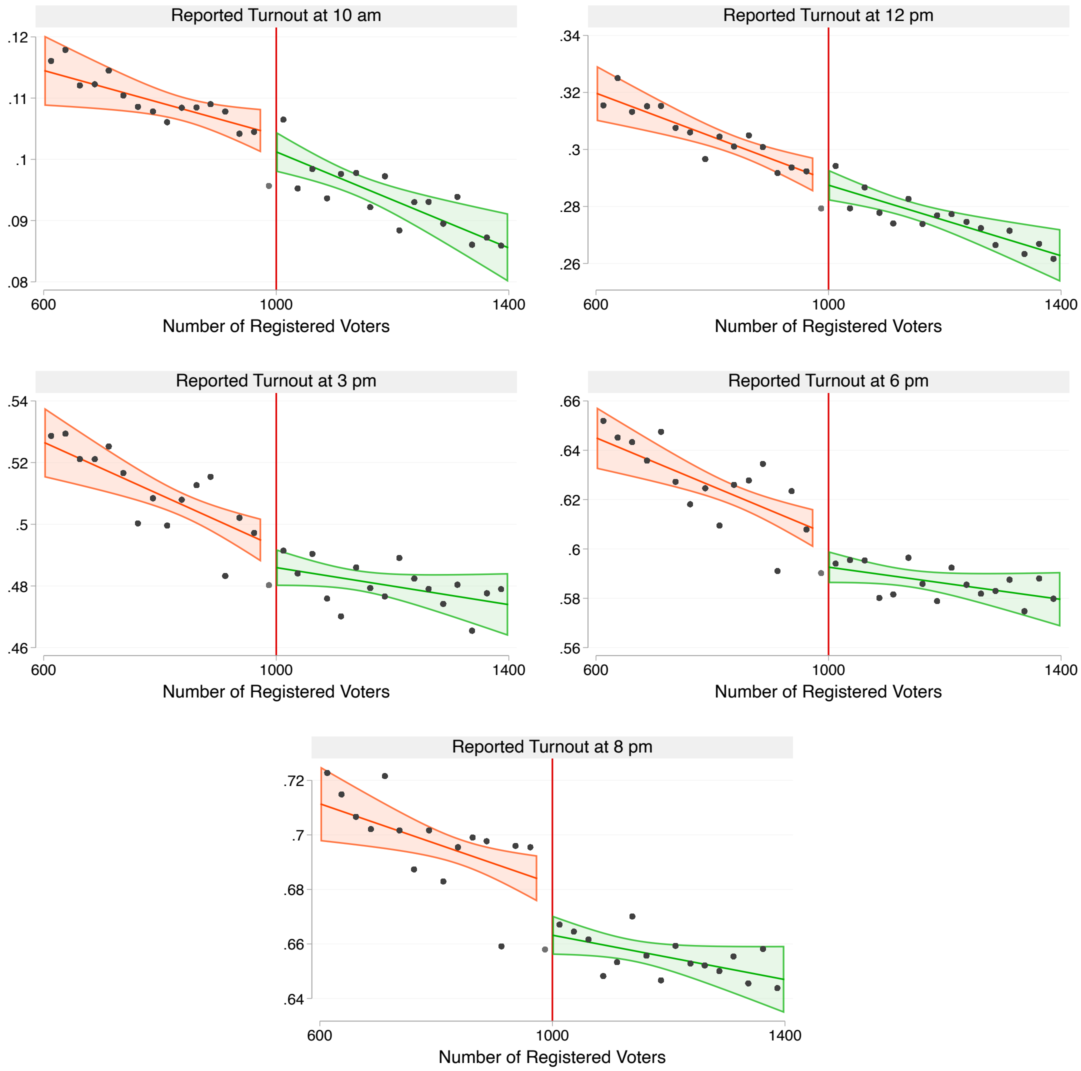


**Figure 1.A.5:** Effects of Video Monitoring on Reported Turnout over Time

*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.

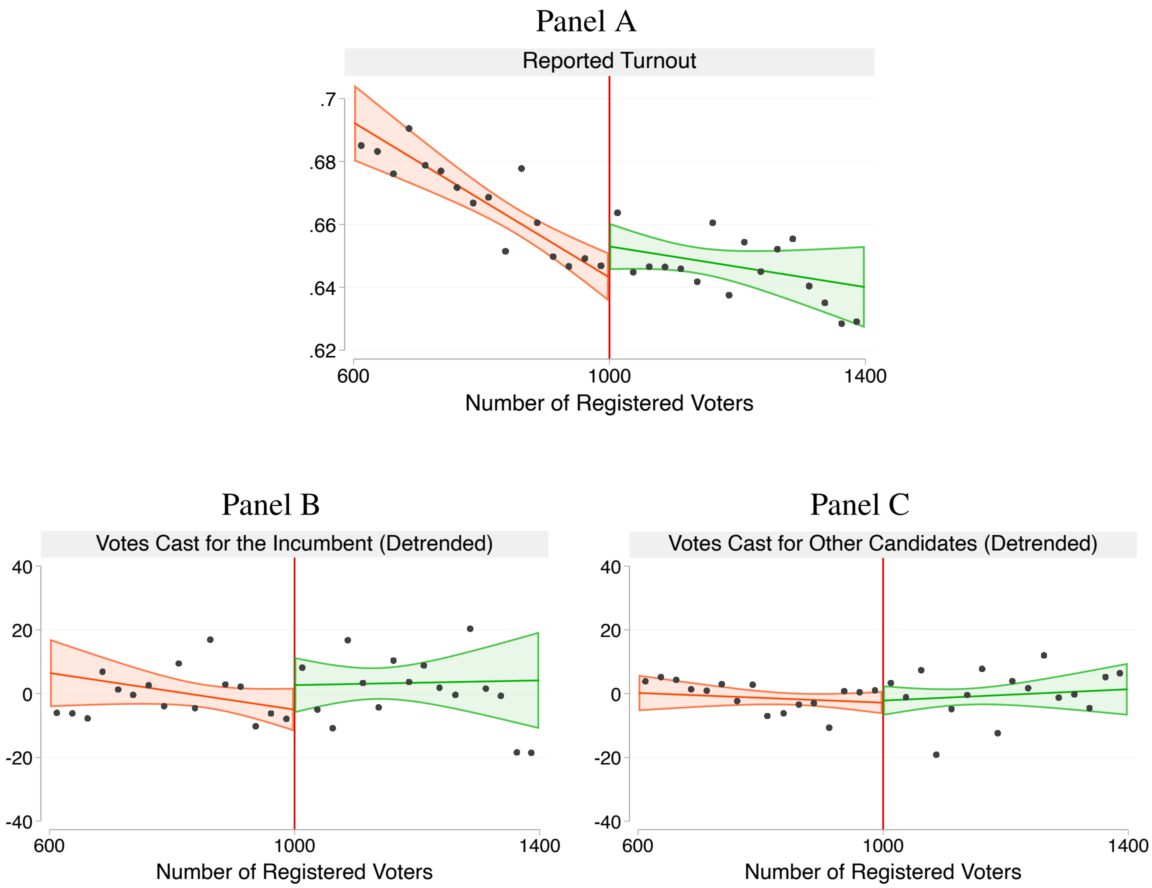


**Figure 1.A.6:** Placebo Effects on Voting: 2012 Presidential Election

*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.

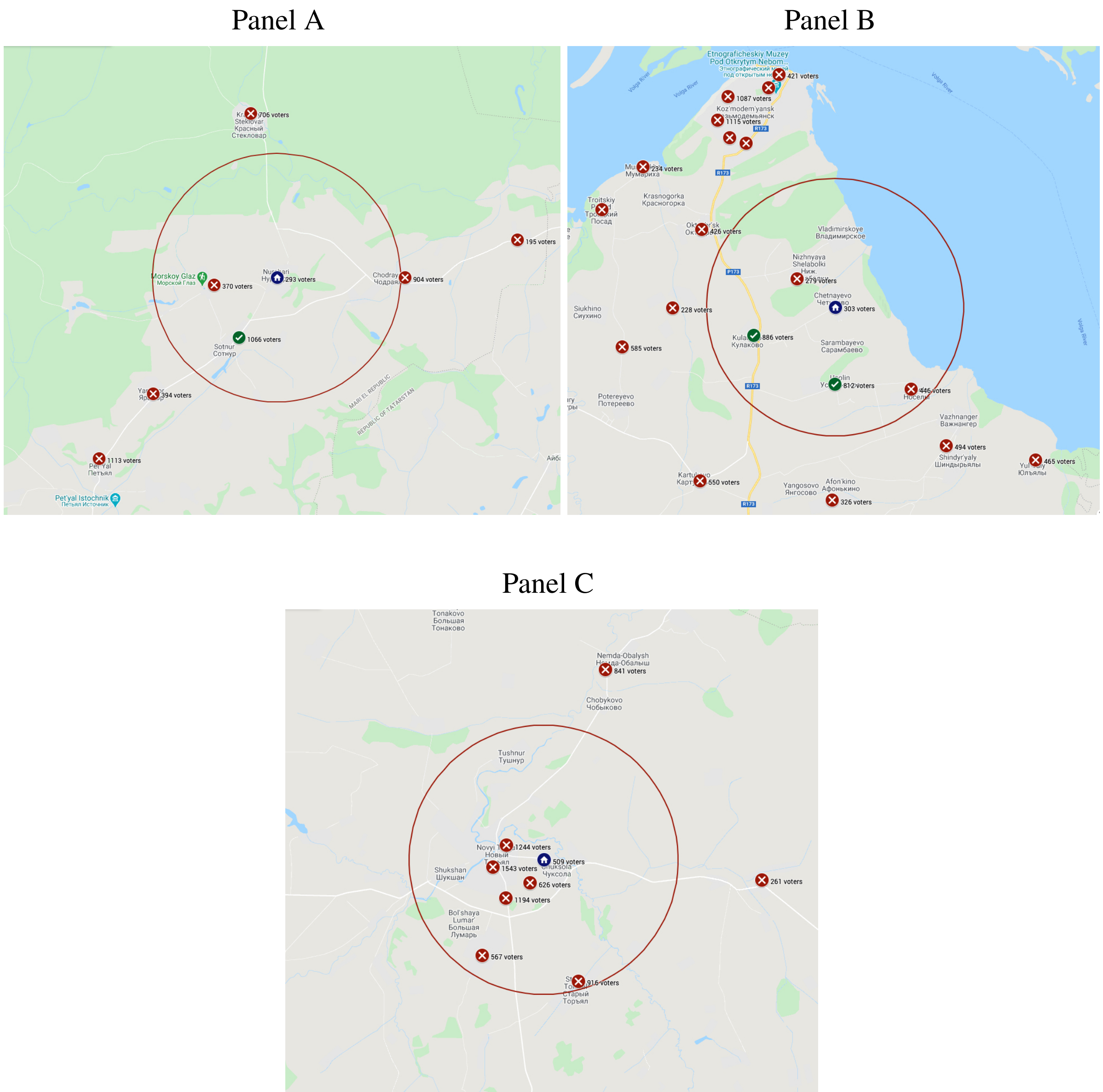


**Figure 1.A.7:** Estimating Vote Displacement: Selection of Polling Stations

*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.

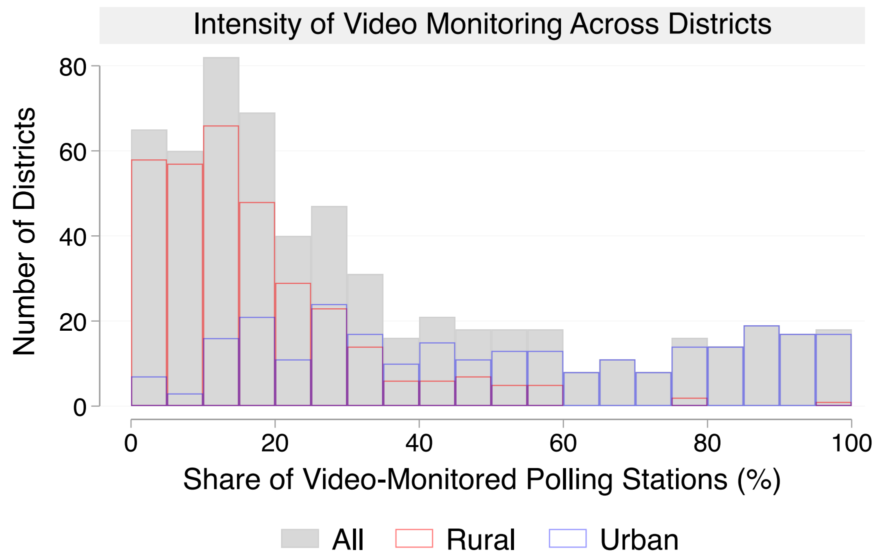


**Figure 1.A.8:** Intensity of Video Monitoring across Districts

*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.

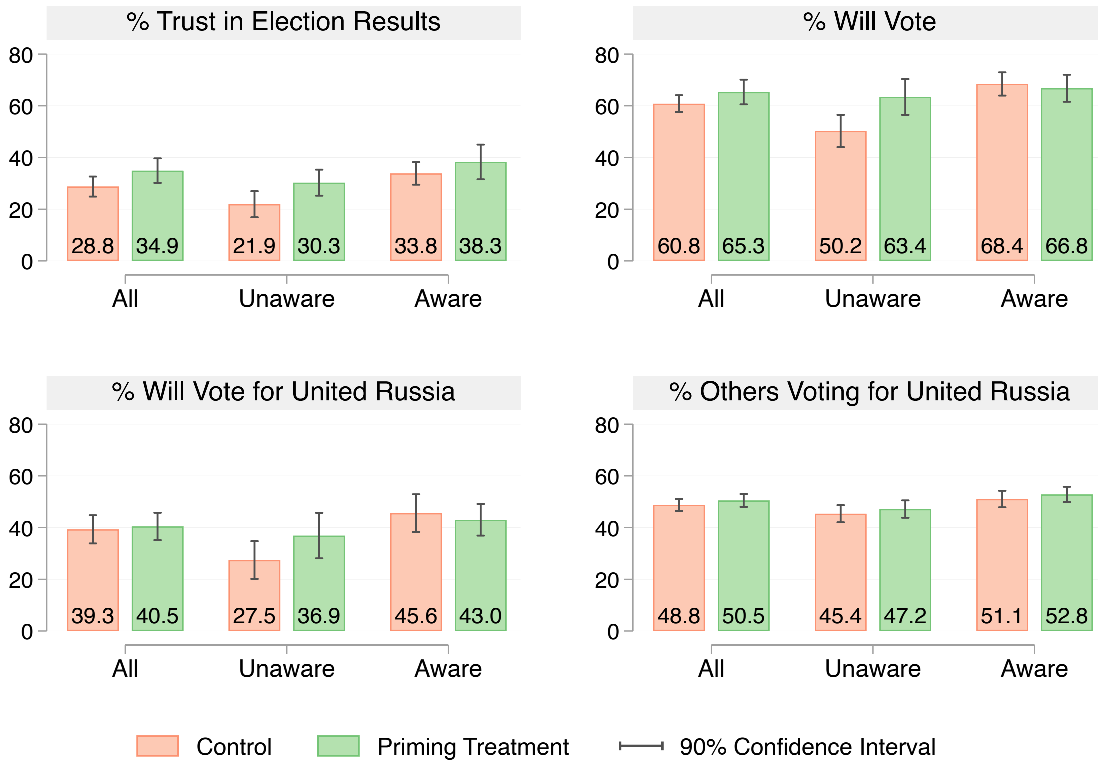


**Figure 1.A.9:** Survey Experiment: Effects of Priming Excluding Moscow

*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.


**Figure 1.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 1.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.

**Table 1.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.

**Table 1.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 1.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 1.A.4:** 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 1.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.

**Table 1.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 1.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.

**Table 1.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 1.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.

**Table 1.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 1.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 1.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.

**Table 1.A.13:** Vote Displacement: Robustness to Pooled 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 1.A.14:** 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.

**Table 1.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.

**Table 1.A.16:** Survey Experiment: Summary Statistics and Full Balance Test

| Statistic: | Mean Full | Mean Control | | Mean Priming Treatment | | P-value | # Obs. |
|---|---|---|---|---|---|---|---|
| | | No Sens. Item | Sens. Item | No Sens. Item | Sens. Item | | |
| | (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.

**Table 1.A.17:** Survey Experiment: Effects of Priming on Voting and Attitudes Excluding Moscow

| 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 | 0.05 | 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).

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# Chapter 2

# Modernizing the State During War: Experimental Evidence from Afghanistan

**Abstract.**

Modern states have the capacity to identify their employees and pay them for their work. This paper reports evidence from a randomized evaluation of a major reform intended to improve the Afghan government's ability to perform these essential functions, which involved over 30,000 employees of the Ministry of Education between 2018 and 2020. The first element of the program, designed to eliminate 'ghost' workers, required employees to register for a mobile money wallet with biometric identification. This policy change helped eliminate thousands of 'ghosts' from the payroll (2.8%-6% of all employees) and estimate that up to 17.6% of remaining employees are 'stand-ins' who do not actually work. The second element transitioned employees from receiving their salary in cash to receiving it via direct mobile money transfers. It led to a 26 percentage point increase in support for the reform to be scaled nationally and caused employees to dramatically increase activity on the mobile money network, demonstrating a potential pathway toward promoting financial inclusion. Because the experiment spanned both secure and contested regions, we can examine whether state control complemented the reform. Our results highlight the importance of long-term horizons in state-building efforts and the importance of physical security as a prerequisite for bureaucratic modernization. We find that progress is possible, even in the shadow of war and while the broader state is under threat.

## 2.1 Introduction

In fragile countries, where the World Bank forecasts two-thirds of the world's extreme poor may reside by 2030 (Corral et al., 2020), building state capacity is challenging but fundamental for political stability, economic development, and advancing human welfare. Correspondingly, rich countries have spent trillions of dollars in the 21st century alone aiming to catalyze institutional development in fragile states.[1] A core element of these efforts is modernizing the civil service. State employees are required for most essential functions of the state – from the maintenance of order and tax collection to the provision of services like health and education (Weber, 1919; Tilly, 1985; Bates, 2001; Besley and Persson, 2009).

Current trends make it clear that ending global poverty means ending it in the world's most fragile states (Pande and Page, 2018; Commission on State Fragility, Growth, and Development, 2018). This raises the question of whether it is possible to build institutions while control of the state remains contested. It is reasonable, and deeply important, to understand whether establishing a monopoly on violence must precede the development of a bureaucracy capable of delivering services. This paper reports on an experiment in Afghanistan – the highest-profile recent example of an international effort to build basic institutions – conducted across secure and contested regions. Afghanistan's varied landscape set the stage for a near-constant policy debate about whether state-building efforts should focus on easier gains in areas under control or target more contested areas. Afghanistan, therefore, is an ideal setting to study whether state control complements efforts to modernize the civil service.

Our analysis centers around a randomized evaluation of a reform implemented by the Afghan government that was designed to improve state capacity to serve two essential bureaucratic functions: to identify government employees and to pay them. The reform we evaluate was implemented by Afghanistan's Ministry of Education in 2017-2018 and included an experimental

[1] With its focus on formal institutions, state-building is distinct from 'nation-building', which emphasizes the development of national identity. While the highest-profile cases of U.S.-supported state-building efforts include Germany and Japan after World War II and Afghanistan and Iraq in the post-9/11 era, Lake (2010) identifies more than a dozen other relevant cases since 1900.

sample of just over 30,000 public employees (primarily teachers) working in roughly 1,500 schools across three large provinces.[2] We focus on education, which employs roughly 80% of the country's civilian public servants, because of its importance to the national economic development, and because the status quo payroll system was plagued by inefficiencies.[3] For instance, in a series of unannounced school visits we conducted prior to the MSP reform, almost half of the recorded teachers were not present despite having been paid in recent months. Issues with teacher salary payments were similarly severe: at baseline, only 28% of employees in our control group reported being very satisfied with their payment system and 57% experienced payment delays.

The reform itself involved two main components. First, employees were required to register for a mobile money wallet and provide their fingerprints for biometric identification. Second, employees began to receive their salaries via direct mobile money transfers instead of the status quo system that relied heavily on a network of 'trusted agents' to deliver cash payments. We refer to these two steps jointly as the Mobile Salary Payment (MSP) reform.

To evaluate the effectiveness of the MSP reform, we developed a two-stage randomized controlled trial to study the two components of the reform. We divided the 1,500 schools into 401 registration zones of about 80 employees each and randomly assigned zones into three treatment arms that varied in terms of the planned timing of implementation: (i) Early Registration, Early Payment, hereon EE; (ii) Early Registration, Late Payment, hereon EL; and (iii) Late Registration, Late Payment, hereon LL.[4] Through surveys conducted before, during, and after each stage of

[2]The MSP reform was led by Afghanistan's Office of the President, the Ministry of Communications and Information Technology, the Ministry of Finance, and the Ministry of Education, with financial and technical support from USAID and the World Bank.

[3]At the time of our study, the Ministry of Education was the third-largest ministry (behind defense and interior) in terms of expenditure, with an annual budget of 35 billion AFN (450 million USD). As with many fragile countries, Afghanistan spends a large share of its budget on public employee salaries. In 2018, it spent 71% of its total annual 3.4 billion USD recurrent budget on salaries, with roughly two-thirds funded by international donors. Source: World Bank (2019).

[4]EE constituted 137 zones where payroll verification began in May 2018, and MSP payments started in October 2018; EL constituted 129 zones where payroll verification also began in May 2018, but MSP payments were scheduled to be delayed by six months with respect to the EE group; LL constituted 135 zones where payroll verification and MSP payments were expected to be delayed respectively by four (September 2018) and six (April 2019) months.

the reform, we document four main results.

First, the registration stage of the reform eliminated several thousand 'ghost' workers and 'stand-ins' from the payroll. Specifically, 6% of employees did not register when they were required to do so – even though they were informed that this was a condition for future salary payments. Of those 6%, a slight majority (53%) eventually registered, while the remainder never completed registration. Of the 94% of registered employees, we estimate that between 5.6% and 17.6% are potentially stand-ins who submitted biometric data but likely never worked as teachers.[5] Adding potential stand-ins to the employees who never registered, we estimate that up to 20.4% of all employees could be ghosts. If all of these were removed from the payroll, the government would save AFN 26 million (USD 0.34 million) per month in just the early (EE and EL) registration zones in the three study provinces.[6]

Second, the MSP reform improved the salary experience of employees – but the effects were not immediate. One year into the program, employees randomly assigned to receive mobile money payments reported longer delays than those in the status quo system, and overall satisfaction with the salary experience was 26 percentage points lower. After two years, however, satisfaction, delays, and travel times equalized across all three groups, although those in the MSPs groups still reported greater transaction costs to receive their salary. More broadly, the share of employees who would vote to implement the reform for the entire ministry – a key indicator emphasized in our pre-analysis plan, was consistently higher among employees receiving mobile payments. Apart from these differences between treatment arms, we note a marked improvement in salary experience even among the control group. Over the two-year study period, the share of satisfied employees in the control (LL) group rose by 43 percentage points from 30% (s.e. 0.03) to 73% (s.e. 0.03), and the share of employees experiencing delays dropped 22 percentage points from 54% (s.e. 0.04) to only 32% (s.e. 0.03). We suspect is that part of this general improvement

[5]We identify these cases by administering a 'litmus test' survey which asks teachers to answer questions that any actual employee should readily be able to answer, such as the location of the school and the name of the principal.

[6]By contrast, one year after the beginning of the reform, only 43.1% of unregistered employees had been removed from the payroll following a formal adjudication process overseen by the Ministry of Education.

was driven by the increased attention given to the payment process by several ministries within the government.

Third, we find clear heterogeneity in the effectiveness of the reform. Many involved in this program, and comparable efforts over the last twenty years, articulated a view that salary payment modernization should begin in rural, contested areas. These arguments, made mainly by the Finance Ministry and leaders of Afghan banks, centered around the idea that state employees in the rich and relatively secure provincial capital were already served by banks and that salary issues and corruption were more severe in rural areas. A much smaller constituency – mainly those working for Afghan telecoms – argued the opposite, based on the idea that MSPs require a mobile money ecosystem, the density to support an agent network, and, most importantly, the security for mobile money agents to work as 'de facto' agents of the state who create a registry of employees and pay them. Several regularities from our data help make progress on these arguments. The reform yielded the most benefits and succeeded more quickly in secure, urban areas. But, progress was still made after two years, even in rural areas.

Fourth, we find evidence that MSPs increased the usage of mobile financial services. On average, employees who received mobile payments for one year sent AFN 520 more in mobile money transfers and AFN 250 in airtime compared to those who just started receiving mobile salaries. Treated employees also deposit 620 AFN more into their accounts and hold AFN 680 more in their mobile money wallets.

Taken together, these results suggest that the MSP initiative enabled modest improvements in state capacity: it helped clean up the payroll, improved employee satisfaction after two years, and encouraged the development of a digital financial economy. Several results emerging from this effort speak to the potential for state-building more generally.

First, the reform only bore initial fruit after two intensive years, particularly in rural areas. H.R. McMaster, whose involvement in the Afghan war culminated in serving as U.S. National Security Advisor, famously argued that Afghanistan was not a 20-year war but rather a one-year war fought 20 times over. His comment highlights the idea that the U.S. strategy shifted on about an annual basis, reflecting personnel rotations, American political cycles, and the need to find agendas that could show almost instant progress. This resonates with our results: had the reform been called off after only one year, it would have appeared broadly as a failure. Second, contrary to calls to begin the program in insecure and rural areas to address the higher degree of perceived corruption in these areas, the program worked much better in secure, urban areas and areas where the status quo ante payment system involved banks. This is consistent with the idea that state control complements (but is not necessarily a fundamental prerequisite for) efforts to modernize the bureaucracy. Third, the reform achieved important policy goals like modernizing salary payments and expanding digital financial inclusion.[7]

The complementarities between state control and efforts to build a broader state capable of delivering services are the topics of a broad literature (cf. Besley and Persson, 2011; Acemoglu and Robinson, 2012; Commission on State Fragility, Growth, and Development, 2018; U.S. Army, 2006). However, in this literature, there is little rigorous evidence on how fragile states can rebuild, particularly in the context of international intervention (cf. Fearon and Laitin, 2004; Weinstein, 2005). This paper partly addresses that gap while contributing to debates on the potential for technology to improve how governments process payments (Muralidharan, Niehaus and Sukhtankar, 2016; Banerjee et al., 2020) and accountability in service delivery more generally (Duflo, Hanna and Ryan, 2012; Callen and Long, 2015; Callen et al., 2016; Dhaliwal and Hanna, 2017; Bossuroy, Delavallade and Pons, 2019; Muralidharan, Niehaus and Sukhtankar, 2020). Indeed, our paper is especially close to Muralidharan, Niehaus and Sukhtankar (2016) in that it explores the potential to use technology to build core state capacities (in the case of that paper, building rosters of and paying citizens, and in ours, building lists of and paying government employees). Our paper is distinct in three important regards. First, we focus on an earlier stage in state modernization: organizing and incentivizing workers. Second and correspondingly, the Afghan government was at a more basic stage of development than the

[7] For previous research on mobile salary payments in Afghanistan's private sector, see Blumenstock et al. (2015); Blumenstock, Callen and Ghani (2018); Blumenstock et al. (Forthcoming).

Indian state of Andhra Pradesh.[8] Third, our experiment takes place during war in a setting where the monopoly on the use of violence is contested. In addition, our paper relates to the literature on measuring the degree of waste in service provision, such as absence and leakage (Chaudhury et al., 2006; Reinnika and Svensson, 2004; Olken, 2007). We also relate to the literature on experiments at scale (Muralidharan, Niehaus and Sukhtankar, 2016; Muralidharan and Niehaus, 2017) and the literature on how mobile money might expand financial inclusion (Aker et al., 2016; Blumenstock, Eagle and Fafchamps, 2016; Jack and Suri, 2014; Suri and Jack, 2016).

The rest of the paper proceeds as follows. Section 2.2 describes the MSP reform, our research design, and administrative and survey data sources. Section 2.3 presents results on biometric registration and ghost employees. Section 2.4 presents results on the effects of MSPs on the salary payment experience, financial inclusion, and education quality. Section 2.5 concludes.

## 2.2 Institutional Details, Research Design, and Data

The primary goals of the reform are: (1) removing ghost workers from the payroll; (2) reducing the extent of leakage in salary payments; (3) improving the salary payment experience; and (4) promoting financial inclusion through the creation of a mobile money ecosystem.

The research design aims to measure the extent to which the reform achieved these goals. We begin by providing details of the reform. We then describe the sample of MoE employees affected by the reform, treatment assignment, and treatment compliance. Finally, we describe the survey and administrative data used to evaluate the reform.

### 2.2.1 Details of the MSP Reform

The MSP reform involves three stages: (i) registering employees for mobile wallets and thereby auditing the payroll (registration); (ii) having the MoE determine who should remain on

[8] Pande and Page (2018) point out that the world's poor are split between fragile states, which cannot secure property rights and lack the monopoly of violence (like Afghanistan), and rapidly growing and more stable "high poverty middle income countries" (like India). Correspondingly, they argue that efforts to address poverty in fragile states should prioritize stability, and those in HPMICs should instead focus on redistribution, which corresponds to the difference in the objects of study between our paper and that in Muralidharan, Niehaus and Sukhtankar (2016).

the payroll after registration (adjudication); and (iii) the processing of payments using mobile money (payment). The MoE implemented the reform using a randomized and staggered rollout. Below are details regarding each of the three stages:

**Stage 1 - Registration:** A Mobile Network Operator (MNO) contracted by the government attempted to provide a mobile wallet to every teacher on the payroll during a pre-scheduled and pre-announced visit to one school (registration center) in each registration zone. During the visit, field agents collected biometric measurements of ten fingers and the national ID number of all present teachers. The purpose was to provide mobile wallets (in the form of mobile-money-enabled sim cards) and identify whether the teachers currently on the payroll could be found in the field. Importantly, before registration teams visited schools, teachers were informed by the MoE that their ability to continue receiving a salary depended on appearing for registration.

**Stage 2 - Adjudication:** The government reform team compared the registration lists against the payroll records and transmitted both lists and their discrepancies to a committee at the Ministry of Education. The committee was meant to determine who should continue getting paid based on these data. While a comparison of registration and payroll data could identify potential ghosts, it was up to the MoE to ultimately remove them.

**Stage 3 - Payment:** The list of verified employees created during the adjudication step was transmitted to the Mobile Network Operator (MNO). The MNO activated the wallet-enabled SIM cards for teachers and required biometric authentication when employees exchanged mobile money for cash at mobile money agents.

### 2.2.2 Creating the Experimental Sample

The population for this study is all MoE employees working in Kandahar, Nangarhar, and Parwan provinces. We identify this population based on official government payroll records from March 2017 to January 2018, containing information about 34,666 employees working in 1,829 schools.[9] We focus on March 2017 to January 2018 since implementation began in March

[9] The payroll records 1,921 schools, but 92 schools had no employees paid at all between March 2017 to January 2018. We also exclude a sample of 29 schools in Nangarhar that were part of an initial pilot of registration activities.

2018.

The experimental sample is determined as follows. First, we divided 1,829 schools into 501 distinct registration zones using the information on schools' location, number of employees, and security. We removed 100 'buffer' registration zones from the experimental sample in the next step. These zones are, by design, less secure, do not have typical schools (e.g., schools are madrassas, vocational training institutes, or other non-traditional schools managed by MoE), or have ten or fewer employees.[10] Note that these criteria were not rigidly and uniformly applied in creating the buffer. Our experimental sample still contains insecure regions, some non-traditional schools, and some small schools. We created buffer zones so that the implementing partner could iteratively work out and adapt both the process of biometrically registering employees and the logistics of managing agents to ensure that teachers could withdraw their salary on payday.

Figure 2.A.1 provides an aerial view of a typical urban (Panel A) and rural (Panel B) registration zone. The urban zones are typically much smaller and comprised of a smaller number of schools, as there are more students per school in those areas.

Our final experimental sample consists of the 32,398 MoE employees paid at least once between March 2017 and June 2019 in one of the 1,516 schools belonging to the 401 experimental registration zones.[11] For each registration zone, we also randomly selected one school for full listing exercises of all the employees present at the school during an unannounced audit visit and extensive surveys of a sub-sample of present employees (up to 3 in each school).

### 2.2.3 Assignment to Treatment

The upper portion of Figure 2.2 depicts the timing of the reform. The basic design for the evaluation randomizes the timing of the payroll verification process and the implementation of MSPs payments at the registration zone level. Here, we describe the design as conceptualized at the beginning of the reform. After the buffer zones were removed, the remaining 401 zones were assigned to one of the three treatment arms:

[10] We include the final condition because of the high fixed cost of surveying a school with only ten teachers.

[11] Our pre-analysis plan mentions 1,535 schools, but there were no paid employees in 19 of them.

1. Early registration, early payment (hereafter EE): 137 zones where payroll verification was scheduled to begin in May 2018 and MSP payments were scheduled to begin in October 2018;
2. Early registration, late payment (hereafter EL): 129 zones where payroll verification was also scheduled in May 2018, but MSP payments were delayed by six months with respect to the EE group (April 2019);
3. Late registration, late payment (hereafter LL): 135 zones where payroll verification and MSP payments were delayed respectively by four (September 2018) and six months (April 2019) with respect to the EE group.

Figure 2.1 provides a map of the registration zones by status across our three provinces. Because our implementing partners were under immense pressure to show progress on increasing the number of government workers paid via mobile money, we could not negotiate the delay of the initiation of payment in the second and third treatment arms beyond April 2019.

Treatment assignment is stratified at the district level. The three studied provinces contain 49 districts, and our experimental sample includes 42 of them.[12]

### 2.2.4 Treatment Compliance

The central and lower portion of Figure 2.2 display the evolution through time of the number of employees who registered for a mobile wallet and those who started being paid via MSP by treatment assignment.

As is evident from the central portion of Figure 2.2, the majority of employees in the EE and EL group were registered between May 2018 and July 2018. Most of the registration in the LL group happened between November 2018 and December 2018. Due to capacity constraints of the MNO, registration activities in this group continued at a slow pace in 2019.

[12]The number of zones per treatment is not equal due to the stratification: not all districts have a number of registration zones that is a multiple of three. The different sizes are due to the random assignment of the one or two "remainder" zones in such districts.

As displayed in the lower portion of Figure 2.2, employees in the EE group started to be paid by MSP in October 2018 in accordance with the schedule. However, due to insufficient number of agents, the transition of employees into MSP payment was gradual, and many of the employees in the EE group received their first MSP payment either in November or December 2018. During these months, in disagreement with the initial plans, about half of the employees in the EL group were also transitioned to MSP payments to show progress on increasing the number of employees paid via mobile money. Only a small minority of employees in the LL group were paid via MSP during the study period, starting mostly in February and April 2019. Finally, even after April 2019 the number of employees paid via MSP did not increase in the EL and LL group, reflecting the persisting capacity constraints of the MNO. In April 2019, 72% of employees in the EE group, 44% in the EL group, and 8% in the LL group reported being paid by mobile money. In May 2020, there numbers increased to 81%, 49%, and 33%, respectively.

While the compliance with the original schedule was far from perfect, the randomization generated variation in the number of employees involved in the reform across treatment groups, as displayed in Figure 2.2 and Appendix Table 2.A.3.

### 2.2.5 Survey Instruments

We have four primary survey instruments:

**Listing Survey:** In each school, we administer a short survey of all available teachers. The exercise begins with an unannounced visit where an enumerator lists all employees present at the school. The teachers are then asked basic questions regarding their identity, job, and salary experience (with a special focus on the timing of salary payments and leakage). The enumerator arrives at the school with a pre-compiled list (built from the payroll records) of employees recently paid for work at that school. During the visit, the principal is asked for an explanation for absent employees. The enumerator also takes pictures of key administrative documents, including attendance sheets. The listing survey was conducted three times: at baseline in May 2018 (before the registration activities started in the EE and EL groups), at midline in November

2018, and at endline 1 in April 2019 (before the MSP payments were scheduled to start in the EL and LL groups).

We randomly selected 401 schools, one per registration zone, for the listing surveys. However, the survey company could not visit all the schools due to security issues. They visited only 375 schools at baseline, 369 at midline, and 362 at endline 1. The total number of employees listed on the payroll and present during the audit visits is 6,264 at baseline, 5,793 at midline, and 5,214 at endline 1, respectively.

**Full Survey:** After administering the listing exercise at baseline and endline 1, we also randomly selected up to three teachers in each school for full surveys. They were asked about their salary payment experience, employment history, teaching, and mobile usage. In total, we interviewed 1,005 teachers at baseline and 974 teachers at endline 1.

Additionally, we administered an extra round of the endline survey (endline 2) in May–June 2020 to analyze the long-term effects of MSPs. We attempted to contact via mobile phone 945 employees whom we interviewed in-person during the first endline survey and for whom we obtained a valid phone number. We successfully interviewed 739 of them with a non-response rate of 21.8% using a short version of the endline 1 survey instrument.

**'Litmus Test' Survey:** We anticipated that some employees who appear at registration might be 'stand-ins,' who are not legitimate teachers registering, so that ghost salaries are not eliminated. In February–March 2019, we conducted a 'litmus test' phone survey to distinguish real employees from 'stand-ins.' We sampled 2,663 employees, including those who did not register during the first registration wave and, for comparison, who registered with delay. They were asked for basic details about their workplace and job, such as the school's geographic location, the principal's identity, and the rank of their position. Real employees should be able to answer these questions easily, while 'stand-ins' might find some of them difficult.

**Learning Assessment:** In May 2019, we conducted a learning assessment of 1,001 children aged 6–10 who live in the proximity area of 367 accessible schools selected for the listing exercise. We administered a short household survey with an adult to collect basic information about the children within each randomly selected household. Afterward, one child in each household was randomly chosen to take the learning assessment. The instruments were adopted with the permission of the New York University's Assessment of Learning Outcomes and Social Effects of Community-based Education (ALSE) in Afghanistan. The learning assessment, modeled off the Early Grade Reading Assessment (EGRA) and Early Grade Math Assessment (EGMA), tested literacy and numeracy skills such as counting, number and letter recognition, addition, subtraction, reading, and reading comprehension.

### 2.2.6 Administrative Data

We also collected administrative data from the following two sources:

**Payroll data (M-41):** These data comprise the official payroll records for the universe of employees generated when the Ministry of Education submits monthly requests for salaries to the Ministry of Finance. These records contain employee-level information, including names, basic demographics, and monthly salary payments.

**Mobile Money Transactions data:** These data record each payment issued via mobile money as a separate entry. These also contain information about salary availability, disbursement, and mobile money usage, including deposits, remaining balances, peer-to-peer transfers, merchant payments, and airtime top-ups.

## 2.3 Biometric Registration and Ghost Employees

### 2.3.1 Identifying Ghost Employees

Before the reform, the government lacked a reliable list of employees in the MoE.[13] Moreover, numerous reports indicated a high share of ghost workers, an impression that is supported by the widespread absenteeism of teachers in schools.

[13] Specifically, the MoE maintained a human resources roster and a payroll, which differed substantially from each another.

Several features of the registration process help to verify employees. First, the MoE provided clear directions to employees that they must appear for registration to continue receiving their salaries. This gave an incentive for legitimate employees to register. However, it also incentivized corrupt actors to generate 'stand-ins.' In addition, biometric identification required employees to provide all ten fingerprints.Cashing out salary payments after that required biometric authentication. Therefore, adding a 'stand-in' to the payroll required that this person appeared every month to cash out their salary.

Our data allow us to directly identify both employees who did not physically register and those who were eventually removed from the payroll. This provides a direct measure of the number of employees removed from the payroll as a result of registration and the associated cost savings.

Figure 2.3 summarizes registration outcomes in early registration zones (EE and EL treatments).[14] The first panel displays the share of employees on the payroll who: (i) registered during the in-person registration wave ended on July 18, 2018; (ii) registered after July 18; or (iii) never registered. Among employees paid for six months before registration, 94% registered on time, 3.2% registered with some delay, and 2.8% never registered. Hence, a conservative, lower bound estimate for the number of ghosts is 2.8% of all employees. Their salaries amount to about AFN 3.4 million (USD 44,000 using the 2018/19 exchange rate) or 2.7% of the total monthly wage bill in these schools.

### 2.3.2 Removing Ghost Employees from the Payroll

After the primary registration wave, the MoE formed an adjudication committee tasked with removing ghost employees from the payroll. The committee compared payroll disbursement records to registration records and determined the identity of employees who did not register or whose records did not match. If an employee could not be confirmed, the committee was responsible for removing them from the payroll.

[14]To generate conservative estimates, we exclude from this analysis 23 early registration zones in Nangarhar, in which less than 50% of employees registered due to ongoing violent conflict.

Using the payroll records, we can track whether MoE continued to request salaries for unregistered employees. The second panel of Figure 2.3 shows the share of unregistered employees who continued receiving their salaries after the registration process. 89.6% of them remain on the payroll one month after the start of the reform. This share decreases to 56.9% within twelve months of the registration. However, 53.6% of unregistered employees were still receiving their salary even twenty months after the registration. As a result, the incomplete removal of unregistered employees limited savings to only 1.2% (AFN 1.5 million or USD 20,000) of the total monthly wage bill in the early registration zones.

### 2.3.3 Validating Ghost Employees

Between February and March 2019, we conducted a short 'litmus test' phone survey with MoE employees. The purpose of the survey was to validate whether unregistered employees were indeed ghosts and explore the extent to which registered employees might have been 'stand-ins' registering so that ghost salaries are not eliminated. We sampled a total of 2,663 employees who had a phone number listed in the payroll records (see Appendix Section 2.B for more details on the sampling procedure).

**Litmus Test Design.** We asked employees seven simple questions about their job: (1) the name of the school in which they work; (2) the district in which their school is located; (3) the employee's rank and (4) position; (5) the principal's and (6) headmaster's names; and (7) the total number of employees working in the school. We designed and piloted our litmus test in collaboration with the MoE to identify a set of questions that legitimate employees should be able to answer easily. The purpose is to identify uninformed stand-ins who may have more difficulty answering these questions.

Figure 2.4 displays the results. The first panel shows the share of employees who answered the phone and were available to take the test. 79% of registered on time and 77% of employees who registered with a delay were available to take the test (the difference is not statistically significant at the 5% level). However, this share is only 64% for unregistered employees (the difference is statistically significant at the 1% level). This is the first indicator that some unregistered employees might not be real people.

The second panel shows scores conditional on taking the litmus test (Appendix Figure 2.A.2 reports scores separately for each question). Employees who registered on time have the highest average score of 5.05 out of seven questions. Employees registered with delay have a slightly lower average score of 4.76 (the difference is statistically significant at the 5% level). Finally, unregistered employees have the lowest average score of 4.25 (the difference is statistically significant at the 1% level). Lower scores imply that a considerable share of registered with delay and unregistered employees are likely 'stand-ins' – illegitimate employees who registered for a mobile wallet without knowing simple details about their job.

**How many 'stand-ins' registered?** An ideal litmus test would perfectly discriminate between true employees and stand-ins. Such a test requires a set of questions that: i) would be easy enough for all true employees to answer correctly (even though some of them might have low literacy rates or other characteristics that would make answering the test hard), ii) would be non-trivial, so that stand-ins could not answer correctly, and iii) could be graded using official records.

While our litmus test is imperfect (because real employees might not know some answers and stand-ins might answer the simplest questions correctly, and official records might have errors), it is nevertheless informative. Recognizing these limitations, Appendix Section 2.B uses litmus scores to estimate bounds on the share of stand-ins. The idea is simple: the lower the scores of registered employees than the scores of known real employees, the higher the number of stand-ins must be.[15]

We find that estimates of stand-ins range from 5.8% to 18.1% of registered employees (5.6% to 17.6% of all employees), depending on how strictly we mark the litmus test. Adding

[15]Our exercise requires two assumptions. First, stand-ins would score zero on the litmus test. This implies that our estimates are lower bounds for the actual number of stand-ins: we attribute low scores among registered employees to a few stand-ins with zero scores when they could be due to a higher number of stand-ins with low but positive scores. Second, the average score of all true employees is the same as the average score of true employees who could be verified through the registration process and unannounced visits to schools.

these potential stand-ins to the 2.8% of unregistered, we estimate that up to 20.4% of all employees might be ghosts. If they were removed from the payroll, the government would save AFN 26 mln (USD 0.34 mln) per month only in early registration zones in three provinces.

## 2.4 Mobile Salary Payments Effects

We next evaluate the other two main goals of the MSP reform: improving salary payment experience and increasing employees' financial inclusion. We also estimate whether the changes associated with these goals affected the quality of education schools provided.

### 2.4.1 Estimating Strategy

We start analyzing the effects of Mobile Salary Payments by estimating Intention-to-Treat (ITT) effects using the following specification:

$$Y_{izd} = \alpha + \beta_{EE} EE_z + \beta_{EL} EL_z + \mu_d + \varepsilon_{izd} \tag{2.1}$$

where $Y_{izd}$ is the outcome for unit $i$ (at the school/teacher/student level) from registration zone $z$ located in district $d$, $EE_z$ and $EL_z$ are indicators for Early Registration, Early Payment and Early Registration, Late Payment treatment zones, respectively, and $\mu_d$ are district fixed effects. Standard errors are clustered at the registration zone (treatment unit) level.

Table 2.A.2 shows the balance of outcomes at baseline conducted at the start of registration activities in May of 2018. Since we conducted randomization based on the administrative data before the baseline survey, there is a minor imbalance for one outcome, share experienced delays, in the EE treatment zones. In the Appendix Table 2.A.5, we address this issue by showing the robustness of the results controlling for baseline outcomes.[16]

Given treatment non-compliance in Early Registration, Late Payment (EL) and Late Registration, Late Payment zones, we also estimate Treatment-on-the-Treated (ToT) effects using

[16]Baseline outcomes are averaged at the school level because different employees were sampled across survey waves due to the unannounced nature of visits and random assignment for the full teacher survey.

the following 2SLS specification:

$$\begin{aligned} Y_{izd} &= \gamma + \beta_{MSP} MSP_{izd} + \mu_d + \varepsilon_{izd} \\ MSP_{izd} &= \theta + \varphi_{EE} EE_z + \varphi_{EL} EL_z + \eta_d + \varepsilon_{izd} \end{aligned} \tag{2.2}$$

where we instrument $MSP_{izd}$, the indicator for receiving salaries via mobile money, with the treatment assignment indicators $EE_z$ and $EL_z$ (see Appendix Table 2.A.3 for the first stage results).

To keep the discussion focused and because the estimates on all coefficients are generally fully aligned, in the text below, we will refer primarily to the treatment effects associated with the EE treatment – i.e., the arm designed to evaluate the full extent of the reform and for which compliance with the original research design was higher – and to ToT estimates.[17] For completeness, Figures and Tables also report the estimates for the EL treatment.

### 2.4.2 Effects on Payment Experience

The MSP reform created a remarkable shift in how funds flowed from the government to its employees. Prior to the reform, all salaries, even those delivered by Motameds, were deposited by the government in individual private bank accounts. This provided a valuable source of transaction fees and funds for fractional reserve lending to private banks. As such, advocates of traditional banking had strong incentives to resist the reform. To countervail this pressure, the reform needed to improve teachers' salary payment experience enough that they would argue for it to continue. In this section, we review results related to this. Against this background, we place particular weight on a survey question that asks teachers whether they would vote in favor of scaling the reform ministry-wide. The question was not incentivized, but it is reasonable for employees involved in the experiment to assume that this information would weigh heavily in the government's decision over whether to scale the program.

[17] Appendix Table 2.A.8 provides additional evidence that the effects described below are driven by mobile payments rather than biometric registration by simultaneously instrumenting registration and payments with the treatment group assignment.

The reform created marked changes to teachers' payment experience. Figure 2.5 depicts the simple means of survey measures of teachers' payment experience in the three treatment groups at baseline, after about one year (endline 1), and after about two years (endline 2) since the start of the reform. Table 2.1 reports treatment effects on the same outcomes in regression format, following specifications (1) and (2).

First, we find suggestive evidence that the reform potentially created system-wide improvements in teachers' payment experience. Focusing on the control group, only 30% (s.e. 0.03) of employees were satisfied with their payment system at baseline. The satisfaction rate increased to 63% (s.e. 0.04) by the end of year 1 and 73% (s.e. 0.03) of year 2. Similarly, the share of employees experiencing delays in the control group decreased from 54% (s.e. 0.04) to 41% (s.e. 0.04) and 32% (s.e. 0.03), respectively. There were, however, no system-wide changes in travel time and costs, which cannot be easily improved without substantial investments in infrastructure for the status-quo payment systems. While these effects should be treated with caution due to the non-randomized nature of this aspect of the reform, they suggest that increased attention to the payment process can substantially improve employee satisfaction.[18]

Second, mobile salary payments provided a better payment experience, revealed by the higher support for the reform in the treatment groups. Here we focus on the share of employees voting in favor of implementing MSPs for the entire ministry. This outcome is designed to measure comprehensive sentiment toward the reform and employees' revealed preference for the payment system. More than 90% of employees supported the reform across all treatment groups before its start. One year into implementation, this share dropped to 42% (s.e. 0.04) in the control (LL) group. This is not surprising given the relative improvements in the status quo systems described above and the implementation challenges of MSP described below. However, it is worth noticing that even at this point, support was still 11 percentage points (s.e. 0.04) higher in the early treatment (EE) group (ToT p.e. 0.18, s.e. 0.07).

[18] Appendix Section 2.C provides additional evidence that these effects represent system-wide changes rather than spatial treatment externalities.

After the implementation challenges were resolved during the second year, the share of employees supporting the reform started to revert to its baseline values. It reached 65% (s.e. 0.04) in the control (LL) group and remained 12 percentage points (s.e. 0.04) higher in the early treatment (EE) group (ToT p.e. 0.26, s.e. 0.08).

These results show that poor implementation can quickly undermine support, which is critical for the reform to be politically viable. They also indicate that employees might be willing to incur initial costs to gain future benefits of mobile payments.

Third, mobile salary payments experienced teething problems during the first year, followed by approximate convergence to the status quo payment systems by the end of the second year. While 63% of employees in the LL control group were very satisfied with their status quo payment system in year 1, this share went down by 25 percentage points in the EE treatment group. However, the satisfaction rate went up to 73% in the LL group and equalized across all groups (ToT p.e. -0.00, s.e. 0.08) in year 2.

The initial reduction in satisfaction was mainly brought it in by implementation challenges experienced by the mobile network operator, which were reflected in increased delays and transaction costs. During the first year, the share of employees experiencing delays was 41% in the LL control group and 25 percentage points (s.e. 0.05) higher in the EE treatment group (ToT p.e. 0.40, s.e. 0.07). Moreover, an average employee spent 31 minutes cashing out his salary in the LL control group, while this time was 27.2 minutes (s.e. 5.9 ) higher in the EE treatment group (ToT p.e. 43.2, s.e. 9.5). Similarly, an average employee spent AFN 45.7 in travel costs to cash out his salary in the LL control group, while this amount was AFN 80.1 minutes (s.e. 15.6) higher in the EE treatment group (ToT p.e. 125.8, s.e. 23.0).

By the end of the second year, delays and travel time equalized across all groups, while monetary travel costs remained higher in the treatment groups. Only 32% of employees experienced delays in the LL group, with no significant differences in the treatment groups (ToT p.e. -0.06, s.e. 0.08). An average employee also spent 36.6 minutes to cash out his salary with similar travel times in the treatment groups (ToT p.e. 3.7, s.e. 5.9). However, employees still incurred considerable monetary travel costs of AFN 53.3 (s.e. 5.5) in the LL control group and AFN 28.5 more (s.e. 9.2) in the EE treatment group (ToT p.e. 53.6, s.e. 17.0).

Appendix Figure 2.A.3 provides suggestive evidence that the initial implementation challenges could potentially be due to insufficient experience of mobile money agents disbursing salaries. The low number of agents did not drive the issues: by the end of the first year, the number of active agents was above 60 and remained relatively stable in the following year.[19] Instead, the problems stemmed from agents' experience: there was a high turnover of agents during the first year of the reform so that the average agent's tenure was below 20 days.[20] Once the mobile money operator established a stable workforce of trained mobile money agents by negotiating better contracts, it could organize timely and more convenient salary disbursement.

Fourth, we find that leakage of salary payments was not a serious issue in our sample from the Ministry of Education, and mobile payments brought it to zero. We measure leakage by employees' self-reported payments to someone to receive their salary. On average, employees paid less than AFN 25 (USD 30 cents, less than 1% of the average monthly salary) to receive their salary even at baseline. 87% of employees reporting positive payments gave money to trusted agents to cover transportation costs, 18% gave money to bank agents to cover fees, and fewer than 2% paid to other MoE staff (possibly indicating the magnitude of actual leakage). This result contrasts with the expectations of many policymakers who believed that leakage was a severe problem, especially among teachers previously paid by trusted agents.

MSPs significantly reduced self-reported payments because salaries were transferred directly to employees' mobile wallets (eliminating payments to trusted agents), and withdrawal fees were subsidized by the government (eliminating payments to bank agents). During the first year, average payments amounted to AFN 22.3 in the control LL group and were only slightly lower in the treatment groups (ToT p.e. -5.0, s.e. 5.0). By the end of the second year, payments

[19] We identify the number of active agents by unique mobile numbers of merchants who conducted cash withdrawals ("customer to merchant withdrawal – cashout" in the transaction data) in a given month.

[20] We define agent tenure as the number of days between the first and the last merchant's transaction.

further declined to AFN 12.5 in the LL group and were largely eliminated in the treatment groups (ToT p.e. -17.6, s.e. 3.9).

### 2.4.3 Heterogeneous Effects on Payment Experience

Beginning with the earliest initiatives to pay government salaries using mobile money in 2009, several government officials, especially those working at the Ministry of Finance, argued that such reforms should begin in rural areas. The logic is that these are the areas not already served by banks, and so the reform might create the largest increases in financial inclusion and the traceability of payments if it started in rural areas. However, rural areas pose major challenges for MNOs: few mobile money agents, not enough mobile money business to sustain agents, gaps in phone coverage, and insecurity making agents vulnerable to theft.

Given this debate, two natural dimensions of interest for heterogeneity analysis are the location of schools (rural vs. urban) and the pre-reform payment system (banks vs. trusted agents).

**Rural vs. Urban Districts.** To analyze differences in treatment effects by the location of schools, we estimate heterogeneous Intention-to-Treat effects by interacting treatment zone indicators with indicators for urban and rural schools using the following specification:

$$Y_{iszd} = \alpha + \beta_{EE} EE_z + \beta_{EL} EL_z + \eta_{EE} EE_z \times Urban_{szd} + \eta_{EL} EL_z \times Urban_{szd} + \\ + \theta Urban_{szd} + \mu_d + \varepsilon_{iszd} \quad (2.3)$$

where $Urban_{szd}$ is a dummy equal to one if school $s$ from registration zone $z$ located in urban district $d$ (the omitted category are rural districts).[21] Then $\beta_{EE}$ and $\beta_{EL}$ estimate the effects of MSPs for schools in rural areas while ($\beta_{EE}$ + $\eta_{EE}$) and ($\beta_{EL}$ + $\eta_{EL}$) estimate the effects for schools in urban areas.

[21] We define districts as urban based on the population density of more than 300 inhabitants per squared kilometer.

Similarly, we estimate heterogeneous Treatment-on-the-Treated effects using the following specification:

$$
\begin{aligned}
Y_{iszd} &= \gamma + \beta_{MSP} MSP_{iszd} + \eta_{MSP} MSP_{izd} \times Urban_{szd} + \theta Urban_{szd} + \mu_d + \varepsilon_{izd} \\
MSP_{iszd} &= \phi + \delta_{EE} EE_z + \delta_{EL} EL_z + \kappa_{EE} EE_z \times Urban_{szd} + \kappa_{EL} EL_z \times Urban_{szd} + \\
&\quad + \theta Urban_{szd} + \eta_d + \varepsilon_{iszd} \\
MSP_{iszd} \times Urban_{szd} &= \psi + \rho_{EE} EE_z + \rho_{EL} EL_z + \tau_{EE} EE_z \times Urban_{szd} + \tau_{EL} EL_z \times Urban_{szd} + \\
&\quad + \theta Urban_{szd} + \pi_d + \varsigma_{iszd}
\end{aligned}
\tag{2.4}
$$

where $\beta_{MSP}$ estimates the effects of MSPs for schools in rural areas, while ($\beta_{MSP}$ + $\eta_{MSP}$) estimate the effects for schools in urban areas.

Table 2.2 presents the results. In the first year, it is evident that employees were made worse-off by MSPs both in urban and rural areas. ToT effects show a 27 percentage point decrease in the share of very satisfied with the payment system, a 24 percentage points increase in the share experiencing delays, and a 34-minute increase in travel time in urban areas. Importantly, however, employees paid by MSP in urban areas were nonetheless more likely to support the reform than those paid with the status quo method. Support for the reform was instead not different between the control and treatment groups in rural areas, where the teething problems of MSPs were more severe. Indeed, ToT effects indicate a 58 percentage point decrease in satisfaction, a 64 percentage points increase in delays, and a 56-minute increase in travel time in rural areas.

By the end of the second year, when most of the initial issues with MSPs were resolved, employees paid by MSPs were significantly more likely to support the reform both in urban and rural areas. This is mainly due to a reduction in travel time to the pre-reform level and a significant decrease in delays, particularly in urban areas.

In urban areas, improvements in the salary payment experience were substantial. By endline 2, employees paid by MSPs were 24 percentage points more likely to be satisfied and

26 percentage points less likely to experience delays than their other colleagues paid by trusted agents or banks. In rural areas instead, even though things improved substantially with respect to the previous year, employees paid by MSP were still 20 percentage points more likely to experience delays and 32 percentage points less likely to be satisfied than colleagues who remained in the status-quo systems.

**Banks vs. Trusted Agents as a Status Quo.** In addition to checking for heterogeneity between rural and urban schools, we also estimate heterogeneous Intention-to-Treat and Treatment-on-the-Treated effects by interacting treatment zone indicators with an indicator for banking payments at baseline using a similar specification.

Figure 2.6 and Appendix Table 2.3 present the results. First, we note that, contrary to expectations, trusted agents were perceived by employees as a better payment system than banks. In the LL group at endline 1, employees paid by trusted agents were more satisfied with the payment system and reported fewer delays in payments and lower travel time than employees paid by banks. As a result, being relatively more satisfied with the status quo, they were also less likely to support the MSP reform.

Second, estimates indicate that employees paid by MSP were worse-off with respect to employees paid by trusted agents in the first year. ToT estimates show a 69 percentage point decrease in the share of very satisfied with the payment system, a 65 percentage points increase in the share experiencing delays, and a 66 minutes increase in travel time. Importantly, however, employees paid by MSP were not less likely to support the reform than those paid by trusted agents.

Instead, employees paid by MSP were not worse off than employees paid by banks. Even in the first year of the reform, when the MNO was still building capacity, we found no statistically significant differences in satisfaction, delays, or travel time between MSP and banks. On the contrary, ToT estimates indicate that employees paid by MSP rather than banks were 28 percentage points more likely to support the reform.

Put differently, the initial issues in the first year of MSPs were concentrated in areas where trusted agents used to disburse salaries. These are the areas where the status quo payment system was better to begin with and where it was more challenging for the MNO to operate (more remote areas where banks are incapable of operating). Implementing the reform only in these more remote areas would have given an overly negative first impression of MSP.

By endline 2, when most of the initial issues with MSP were evened out, employees paid by MSP were 22 percentage points more likely to support the reform than those paid by trusted agents. Indeed, differences in satisfaction and delays almost disappeared, and travel time and cost differences substantially diminished (to 14 minutes and about 80 AFNs).

At the same time, even among employees initially paid by banks, support for the reform remained higher for employees who experienced MSP. At endline 2, these were also 30 percentage points more likely to report being very satisfied with the payment system.

### 2.4.4 Effects on Quality of Education

The reform generated several substantial changes within the MoE. Conceivably, this could affect the quality of education that schools provide.

We measure the quality of education using two metrics. First, we follow the literature on teacher absenteeism in developing countries (Chaudhury et al., 2006) and look at the effects on teacher attendance. In particular, we measure the share of employees who were present during unannounced visits to schools. The unannounced visits conducted at Midline allow us to separately estimate the effects of registration since mobile payments were only starting in the treatment zones at that time. Panel A of Table 2.4 shows that 52% of employees paid at Baseline were present during the Midline unannounced visits to control schools located in the Late Registration (LL) zones.[22] The attendance rates were slightly lower but not significantly different in treatment schools located in the Early Registration (EE and EL) zones. Hence, biometric registration did not have significant effects on employee attendance.

[22]Note that these shares can underestimate the actual teacher presence rates in the visited schools. The listing exercise sample included both single-shift and multiple-shift schools, and unannounced visits sometimes spanned only part of the business day. There is also a natural turnover of employees across survey waves.

Similarly, the unannounced visits to schools at Endline 1 allow us to measure the total effects of mobile salary payments on teacher attendance one year after the start of the reform. 46% of employees paid at Baseline were present during the Endline 1 unannounced visits to control schools located in the Late Registration, Late Payments (LL) zones. The attendance rates were slightly lower but not statistically significant in treatment schools located in the Early Registration (EE and EL) zones, in which significantly more employees switched to mobile salary payments.

Second, even though teachers were equally likely to show up to schools, they could have changed their efforts or teaching methods due to changes in their salary payment experience. We measure the effects on students' knowledge by conducting an independent learning assessment of 1,001 children aged 6–10 who live nearby 367 experimental schools. Following Burde, Middleton and Samii (2017) who performed a similar survey in Afghanistan, we construct math, reading, and total test scores and standardize them relative to the scores of children living nearby control schools in the Late Registration, Late Payments (LL) zones.

Panel B of Table 2.4 presents the results for students' learning outcomes. The effects of mobile salary payments on students' test scores are small and not statistically significant. Children living in the catchment area of treated schools in EE (EL) zones have a 0.05 (0.06) standard deviations higher test score than the control schools in LL zones. This difference reflects slightly positive but insignificant effects on math and reading test scores.

Collectively, we do not find evidence that biometric registration or mobile salary payments impacted the quality of education during the first year of the reform. Importantly, the teething problems of the reform in the first year do not impact teachers' attendance or learning outcomes. It is plausible that improved payment experience during the second year could have positively affected the quality of education in the longer term. Unfortunately, it was not feasible to conduct unannounced visits to schools or learning assessments at the time of Endline 2 due to COVID-19.

### 2.4.5 Effects on Financial Inclusion

The reform also sought to increase financial opportunities for employees. Doing so could potentially create a critical mass of users and kickstart the development of a mobile financial infrastructure for the country.

Before the reform, few employees used formal financial institutions. Even though 67% of employees report having a bank account at baseline, most used it solely to withdraw their salaries.[23] Hence, mobile money could serve as the first formal account enabling financial inclusion.

We estimate the effects on financial inclusion by measuring mobile money use observed in transaction records covering around 16,000 employees from October 2018 to February 2020. These data record transactions for employees with a mobile wallet. Therefore, we cannot observe outcomes for unregistered teachers, who comprise much of our pure control LL group throughout the study. We take a dosage response approach to estimate the causal effect of time spent receiving MSPs on other types of mobile transactions. Specifically, we estimate:

$$Y_{izd} = \alpha + \beta \; Months_{izd} + \mu_d + \varepsilon_{izd} \tag{2.5}$$

where $Months_{izd}$ is the number of months an employee $i$ from the registration zone $z$ in district $d$ received MoE payments to his mobile money account at the time of the last transaction. $\beta$ measures a change in outcome $Y$ with an additional month of receiving salaries via mobile money.

To identify the causal impact of time spent receiving MSPs, we estimate the following 2SLS specification:

$$\begin{aligned} Y_{izd} &= \gamma + \beta \; Months_{izd} + \mu_d + \varepsilon_{izd} \\ \#Months_{izd} &= \theta + \varphi_{EE} EE_z + \varphi_{EL} EL_z + \eta_d + \varepsilon_{izd} \end{aligned} \tag{2.6}$$

[23] Only 12% of employees send money via banks, 5% save in their bank accounts, and 3% have an outstanding bank loan.

where we instrument the number of months, $Months_{izd}$, with treatment zone indicators, $EE_z$ and $EL_z$. By design, employees in EL and LL zones started receiving payments via mobile money in later months compared to employees in EE zones.

Table 2.5 presents the results. First, employees send more money to others using their mobile wallets over time. They send around AFN 41 (s.e. 27) more transfers to other mobile money users with an additional month of MSP payments. Moreover, they send around AFN 7 (s.e. 1.6) more to their mobile phone plan and around AFN 19 (s.e 4.9) more to others' phone plans (known as airtime top-ups). These effects amount to AFN 520 more in mobile money transfers and AFN 250 in airtime top-ups to others' phone plans over an average of 12.9 months of MSP payments.

Second, beyond having their salary directly deposited by the MOE on their account, employees also deposit more money into their accounts and leave larger balances over time. An additional month of MSP payments increases deposits – money added to the mobile money wallet via agents or banks – by around AFN 48 (s.e. 14). This is equivalent to AFN 620 over 12.9 months. Similarly, an extra month of MSP payments leads to AFN 53 (s.e. 27) higher pre-pay balances – remaining balance before the last MSP payment was deposited – which correspond to AFN 680 over 12.9 months. Overall, these effects might signal increased trust in the mobile money system and potentially higher savings over time.

These findings are also supported by the financial inclusion survey conducted at the end of the first year. Appendix Table 2.A.10 shows that peer-to-peer transfers index is 0.39 standard deviations higher in the EE group. In particular, this effect is driven by an around 18 percentage points increase in the share of employees who sent or received a transfer using their mobile phone in the last 12 months. This corresponds to a 300% increase relative to a 6 percent mobile transfer rate in the control group. Similarly, 2% of employees from EE and EL groups report saving in their mobile money accounts at the end of the first year compared to 0% in the control group. This is a sizable growth given that only 18% of employees report savings using other means.

Table 2.6 and Appendix Tables 2.A.11 and 2.A.12 further show that the effects on mobile money use (except deposits) are larger among employees previously paid by banks, in urban, and secure areas.

Overall, these results indicate that mobile salary payments facilitate the expansion of the mobile money ecosystem and financial inclusion. Employees increase their mobile money usage by holding larger balances and sending more transfers, which are critical for further growth.

## 2.5 Conclusion

Over the last two decades, the war in Afghanistan carried tremendous financial and human costs. The U.S. spent 2.3 trillion dollars, and around 176,000 people, mostly Afghans, were killed (Brown Watson Institute, 2022). In addition, at the time of writing, more than half of the country's population – an estimated 22.8 million people – face life-threatening food insecurity as the economy crumbles (UN WFP, FAO, 2021), and many of the advances in human rights achieved during the last 20 years are being quickly reversed. An unprecedented international effort to modernize Afghan institutions seems to have ended very near to where it started: with Afghanistan under the control of a brutal Taliban theocracy.

It is hard to imagine a scenario that could more starkly call ideas about whether and how to build states into question. Indeed, it makes a case that any such effort, no matter how it is executed, may not be worth the phenomenal financial and human costs. The record of international efforts to transform Afghanistan by Americans, Soviets, the British, and even Alexander the Great further suggests that the failure of external intervention is inevitable.

The experiment reported in this paper reveals some small glimmers of hope regarding possibilities and a corresponding set of lessons related to the broad questions of whether and how to build fragile states. The first lesson regards the broad question of whether state control must necessarily precede the development of a bureaucracy capable of delivering services. Our results indicate that stability and state control certainly make professionalizing the bureaucracy much easier, but that it remains possible (although very challenging) to develop service delivery functions in a contested space.

The remaining lessons are more practical and regard how to execute a reform like MSPs. Early signs of success took years, not months. The approach contrasts with the many short-term international efforts that broadly worked around and not with the state (Pande and Page, 2018; Commission on State Fragility, Growth, and Development, 2018). Moreover, Afghanistan is a country of rich variety, with stark differences between (at the time of the experiment) relatively safe and prosperous provincial capitals, unstable areas with active conflict, and quieter rural areas where the Taliban exercised de facto control of all state functions. Correspondingly, the possibilities for taking even the basic first steps toward creating a state that our experiment deals with should be expected to vary. Still, a reform that was an object of intense focus by senior Afghan policymakers did eventually bear initial fruit on proximate measures related to payroll execution, though service delivery improvements still lagged. In a closely related domain, government salary payments appear to have spurred a digital financial economy. Employees began to use their mobile wallets to save and purchase airtime for transfer. This suggests that such a reform, at scale, could catalyze a new digital financial ecosystem in Afghanistan.

The experiment also points out many flaws in the design of the MSPs reform and opportunities to improve them. First, registration should include additional layers of screening, beyond biometrics and along the lines of our litmus test, to filter out stand-ins. Second, relevant government authorities should take more decisive action when ghosts are identified. Third, agent networks need to densify, perhaps with the government's support, to improve service quality and fully realize the financial inclusion aspects of the reform. Fourth, the early policy perspective that MSPs should start in rural and insecure areas needs to be revised. The traditional trusted agent payment system worked better than banks, and rural areas lacked the security and the density needed to catalyze mobile money agent networks. And last, such reforms require the government and mobile network operators to develop a set of 'back end' capacities to deliver on their promise. However, we find suggestive evidence that focusing on these capabilities can yield a better payment process for all government employees, including those paid by traditional means.

While certainly incomplete and constrained by the broader challenges in Afghanistan, the progress we document on reforming government salaries is noteworthy, reflecting years of efforts. Given our findings, pessimistic narratives that dismiss fragile states as hopelessly corrupt, inept, or mired in historical complexity should be revisited. Instead, the feasibility of state-building in fragile contexts will likely depend directly on the prioritization, patience, and context-sensitivity of institutional reforms. For external actors seeking to promote development and stability in such settings, the challenge is working through and not around the state, given the temptation to pursue quick impacts at the cost of long-term sustainability.

## 2.6 Acknowledgments

Chapter 2, in part, is currently being prepared for submission for publication of the material. Blumenstock, Joshua E.; Callen, Michael; Faikina, Anastasiia; Fiorin, Stefano; Ghani, Tarek. “Modernizing the State During War: Experimental Evidence from Afghanistan.” The dissertation author was a primary investigator and author of this material. I gratefully acknowledge my co-authors for their wisdom and support.

# 2.7 Figures and Tables

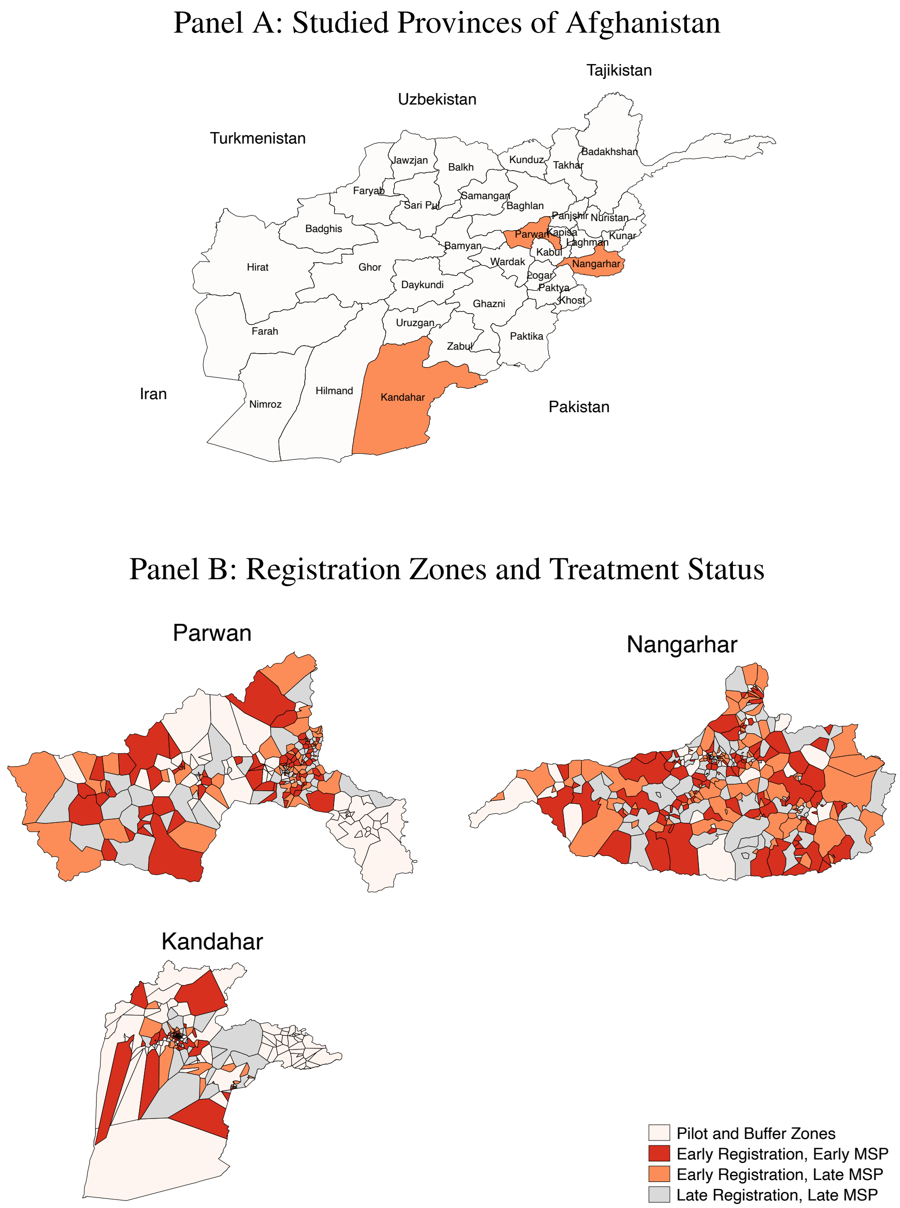


**Figure 2.1:** Map of Provinces and Registration Zones

*Notes:* Panel A plots the map of Afghanistan, with three studied provinces highlighted in red. Panel B plots registration zones colored according to the treatment status in studied provinces. For presentation purposes, boundaries of registration zones were created using jittered polygons around schools belonging to the same zone.

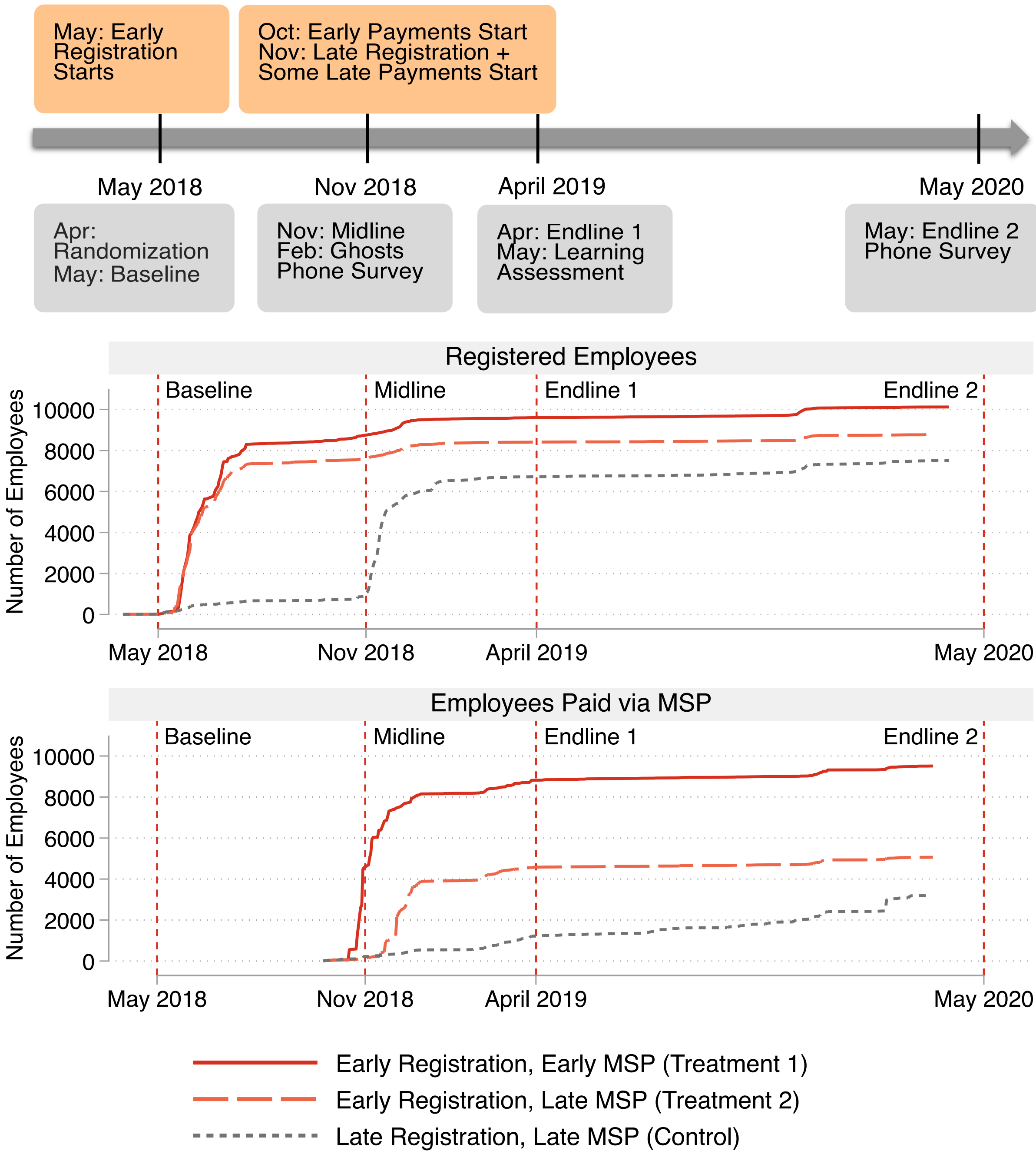


**Figure 2.2:** Project Implementation Timeline and Treatment Compliance

*Notes:* This figure plots the timeline of project implementation and treatment compliance. The top panel shows the dates of the main milestones: the start of registration and payments in different treatment groups and administered surveys. The center panel presents the cumulative number of employees registered using registration data. The bottom panel presents the cumulative number of employees paid via mobile salary payments using transaction data.

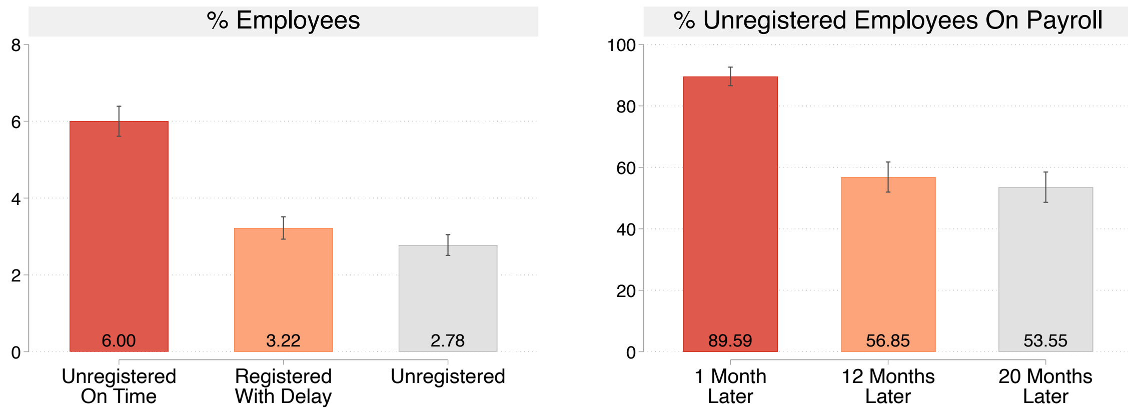


**Figure 2.3:** Registration Outcomes

*Notes*: This figure presents registration outcomes. The sample consists of 14,184 employees from 243 Early Registration zones who were paid for six months before the reform (excluding 23 registration zones with fewer than 20% registered employees). The employee is "Registered on Time" if he registered for a mobile money wallet during the main registration wave (before July 18, 2018). Alternatively, the employee is "Registered With Delay" if he registered after the main registration wave (after July 18, 2018).

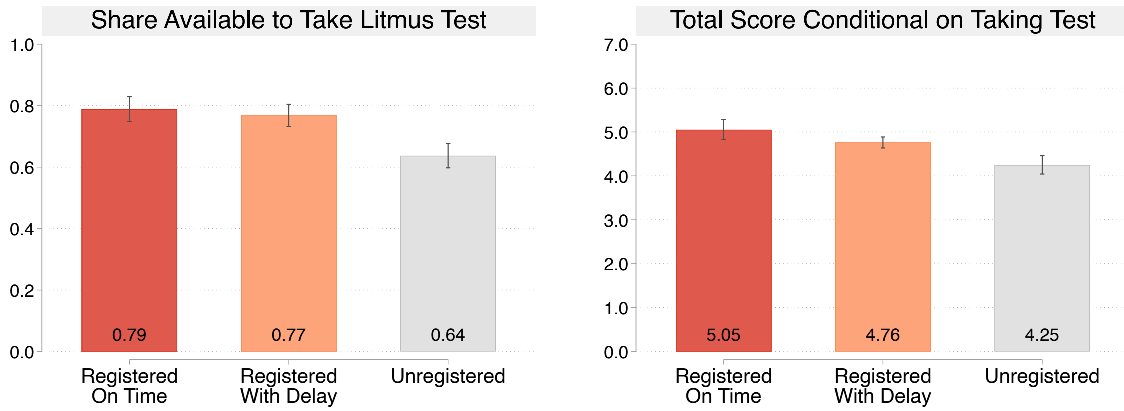


**Figure 2.4:** Litmus Test Score by Registration Status

*Notes*: This figure presents an average litmus test score by employees' registration status (weighted by the sampling probability). The sample consists of 2,545 employees from Early Registration zones, selected for the phone survey of ghost employees in February of 2019. The litmus test score is defined as the total number of correct answers to the following seven questions: school district and name, employee's rank and position, principal's and headmaster's names, and the total number of employees. The employee is "Registered on Time" if he registered for a mobile money wallet during the main registration wave (before July 18, 2018). Alternatively, the employee is "Registered with Delay" if he registered after the main registration wave (after July 18, 2018).

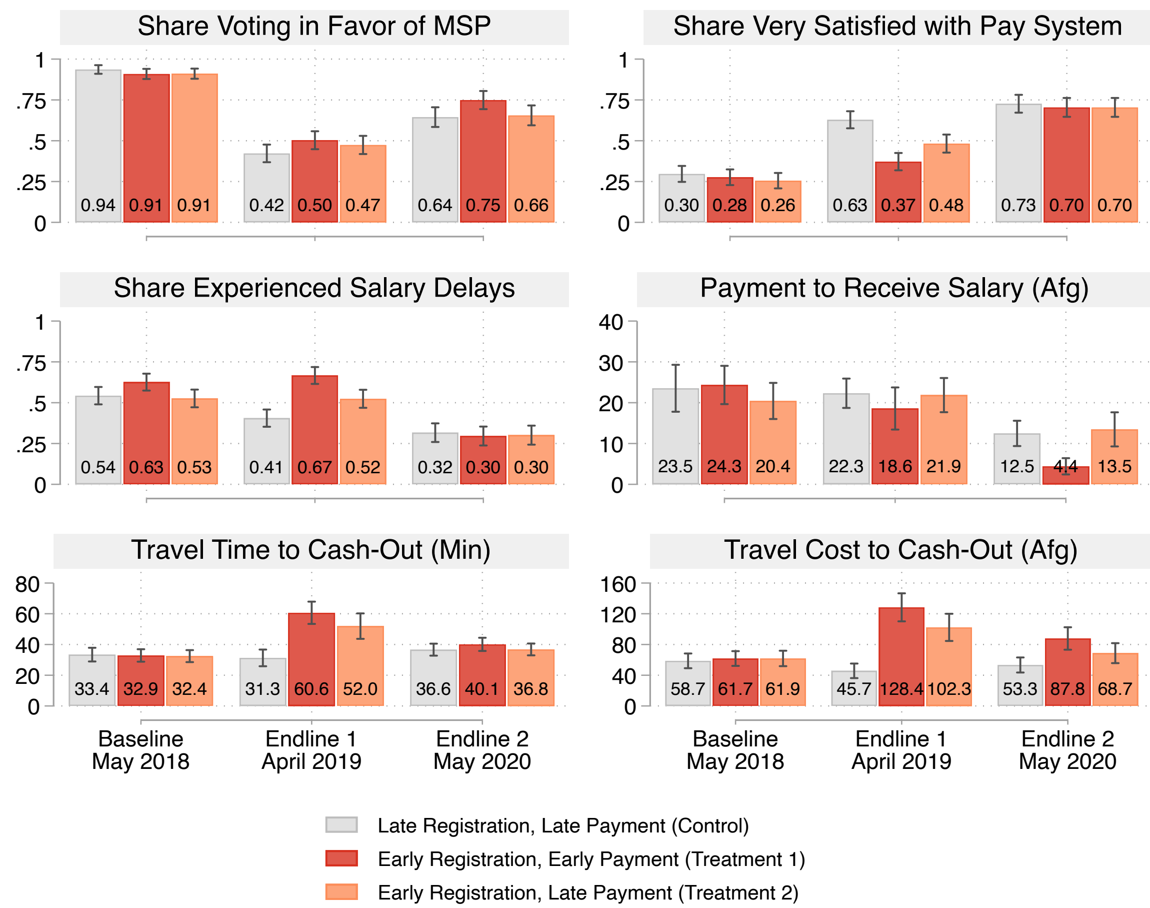


**Figure 2.5:** Effects on Salary Experience

*Notes:* This figure presents simple means of outcomes measuring payment experience at baseline (columns 1–3), endline 1 (columns 4–6), and endline 2 (columns 7–9). The sample consists of MoE employees who participated in the full baseline, endline 1, and endline 2 surveys, respectively, and responded to all questions. Appendix Table 2.A.4 shows the corresponding treatment effects without district (strata) fixed effects.

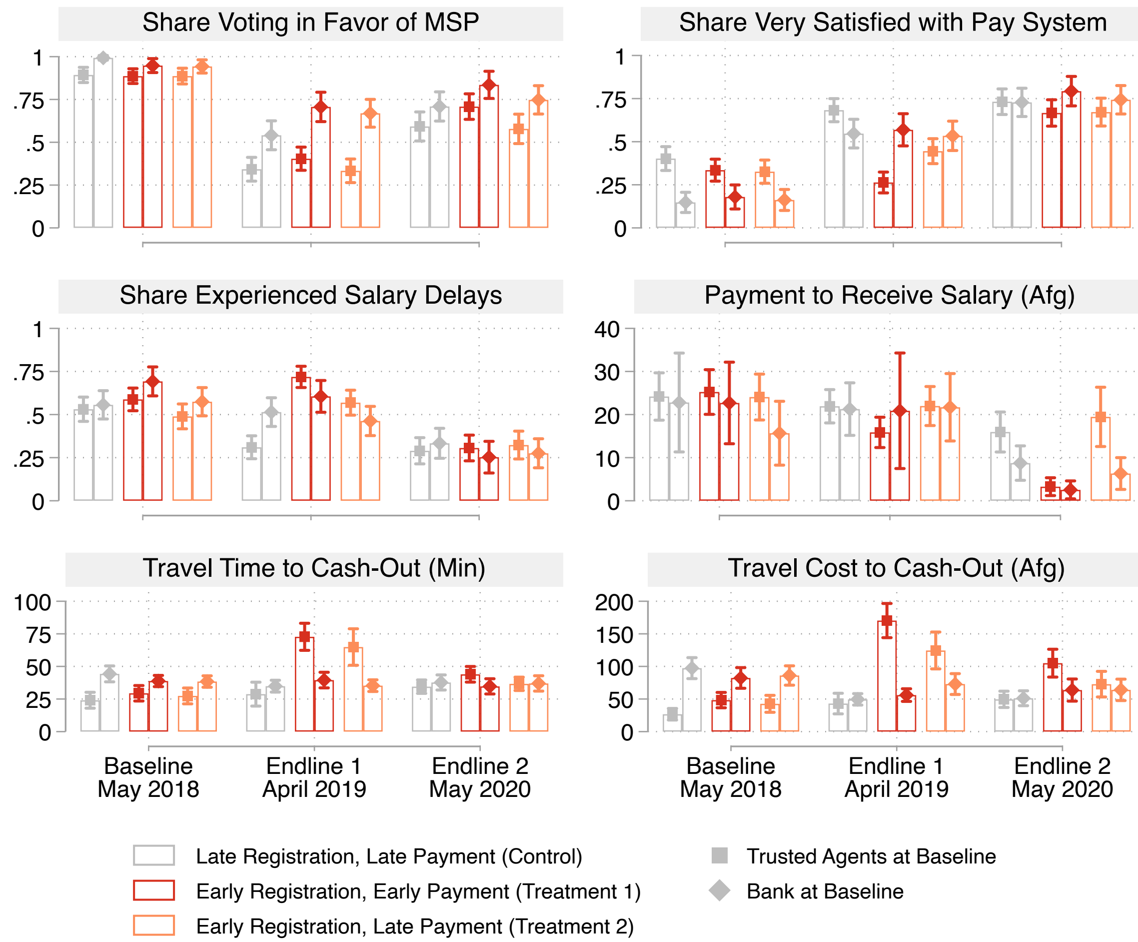


**Figure 2.6:** Heterogeneous Effects on Salary Experience by Baseline Payment System

*Notes:* This figure presents a balance of main outcomes at baseline in May of 2018 (columns 1–3) and treatment effects at endline 1 in April of 2019 (columns 4–6) and endline 2 in May of 2020 (columns 7–9), separately for each baseline payment system. The payment system is defined at the school level by the majority of answers at baseline. The sample consists of MoE employees who participated in the full baseline, endline 1, and endline 2 surveys, respectively, and responded to all questions.

**Table 2.1:** Effects on Salary Payment Experience

| Outcome | LL Mean (1) | EE Effect (2) | EL Effect (3) | ToT Effect (4) | N Obs. (5) |
|---|---|---|---|---|---|
| *Panel A: Endline 1 – April 2019* | | | | | |
| Share Voting in Favor of MSP | 0.42 | 0.11** | 0.08* | 0.18*** | 950 |
| | [0.04] | [0.04] | [0.04] | [0.07] | |
| Share Very Satisfied with Pay System | 0.63 | -0.25*** | -0.14*** | -0.40*** | 966 |
| | [0.04] | [0.05] | [0.05] | [0.08] | |
| Share Experienced Salary Delays | 0.41 | 0.25*** | 0.15*** | 0.40*** | 969 |
| | [0.04] | [0.05] | [0.05] | [0.07] | |
| Payment to Receive Salary (Afg) | 22.31 | -3.21 | -1.64 | -4.97 | 959 |
| | [2.63] | [3.40] | [3.00] | [5.00] | |
| Travel Time to Cash-Out (Min) | 31.28 | 27.19*** | 21.82*** | 43.21*** | 922 |
| | [3.24] | [5.88] | [6.05] | [8.50] | |
| Travel Cost to Cash-Out (Afg) | 45.72 | 80.06*** | 58.80*** | 125.78*** | 932 |
| | [6.78] | [15.59] | [13.55] | [23.00] | |
| *Panel B: Endline 2 – May 2020* | | | | | |
| Share Voting in Favor of MSP | 0.65 | 0.12*** | 0.01 | 0.26*** | 712 |
| | [0.04] | [0.04] | [0.04] | [0.08] | |
| Share Very Satisfied with Pay System | 0.73 | -0.00 | -0.01 | -0.00 | 736 |
| | [0.03] | [0.04] | [0.04] | [0.08] | |
| Share Experienced Salary Delays | 0.32 | -0.03 | -0.02 | -0.06 | 735 |
| | [0.03] | [0.04] | [0.04] | [0.08] | |
| Payment to Receive Salary (Afg) | 12.48 | -7.79*** | 1.25 | -17.56*** | 738 |
| | [1.85] | [2.03] | [2.55] | [3.85] | |
| Travel Time to Cash-Out (Min) | 36.63 | 1.66 | -0.24 | 3.68 | 725 |
| | [2.19] | [3.19] | [3.00] | [5.94] | |
| Travel Cost to Cash-Out (Afg) | 53.32 | 28.54*** | 13.39 | 53.55*** | 635 |
| | [5.49] | [9.22] | [8.20] | [17.00] | |

*Notes:* This table reports treatment effects on salary payment experience, controlling for the district (strata) fixed effects. The Endline 1 (2) sample consists of 970 (739) MoE employees who participated in the full survey and self-reported their payment system. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. ToT is a treatment-on-the-treated effect obtained by instrumenting self-reported MSP payments with the treatment group assignment. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.2:** Heterogeneous Effects on Salary Payment Experience by Urban-Rural District Status

| Outcome | Urban District | | | | | Rural District | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | LL Mean (1) | EE Effect (2) | EL Effect (3) | ToT Effect (4) | N Obs. (5) | LL Mean (6) | EE Effect (7) | EL Effect (8) | ToT Effect (9) | N Obs. (10) |
| *Panel A: Endline 1 – April 2019* | | | | | | | | | | |
| Share Voting in Favor of MSP | 0.45 | 0.20*** | 0.14** | 0.31*** | 537 | 0.39 | -0.01 | -0.02 | -0.01 | 413 |
| | [0.05] | [0.06] | [0.06] | [0.09] | | [0.06] | [0.06] | [0.07] | [0.10] | |
| Share Very Satisfied with Pay System | 0.60 | -0.18*** | -0.11* | -0.27*** | 543 | 0.66 | -0.35*** | -0.19*** | -0.58*** | 423 |
| | [0.05] | [0.07] | [0.07] | [0.10] | | [0.06] | [0.07] | [0.07] | [0.12] | |
| Share Experienced Salary Delays | 0.48 | 0.16** | 0.08 | 0.24** | 545 | 0.31 | 0.38*** | 0.24*** | 0.64*** | 424 |
| | [0.05] | [0.07] | [0.07] | [0.10] | | [0.05] | [0.06] | [0.06] | [0.11] | |
| Payment to Receive Salary (Afg) | 17.98 | -4.34 | -4.25 | -6.86 | 536 | 27.86 | -1.67 | 2.13 | -2.26 | 423 |
| | [3.50] | [5.45] | [4.26] | [7.82] | | [3.83] | [3.35] | [3.99] | [5.37] | |
| Travel Time to Cash-Out (Min) | 26.34 | 24.12*** | 10.23 | 33.79*** | 500 | 37.13 | 31.15*** | 37.46*** | 55.69*** | 422 |
| | [2.68] | [5.68] | [6.61] | [8.19] | | [6.33] | [10.90] | [10.84] | [17.10] | |
| *Panel B: Endline 2 – May 2020* | | | | | | | | | | |
| Share Voting in Favor of MSP | 0.72 | 0.10* | -0.01 | 0.24** | 405 | 0.55 | 0.15** | 0.04 | 0.30** | 307 |
| | [0.04] | [0.06] | [0.05] | [0.12] | | [0.05] | [0.06] | [0.07] | [0.12] | |
| Share Very Satisfied with Pay System | 0.69 | 0.12** | 0.05 | 0.24** | 419 | 0.77 | -0.18*** | -0.12* | -0.32** | 317 |
| | [0.04] | [0.05] | [0.05] | [0.10] | | [0.04] | [0.06] | [0.06] | [0.12] | |
| Share Experienced Salary Delays | 0.38 | -0.14** | -0.10* | -0.26** | 419 | 0.24 | 0.12** | 0.10 | 0.20* | 316 |
| | [0.04] | [0.06] | [0.05] | [0.12] | | [0.04] | [0.06] | [0.06] | [0.11] | |
| Payment to Receive Salary (Afg) | 5.78 | -3.75* | 3.37 | -9.84** | 422 | 21.02 | -13.76*** | -1.93 | -27.85*** | 316 |
| | [1.70] | [2.16] | [2.09] | [4.28] | | [3.09] | [3.78] | [5.59] | [7.12] | |
| Travel Time to Cash-Out (Min) | 37.04 | -0.60 | -2.46 | -0.14 | 414 | 36.11 | 5.07 | 3.16 | 9.09 | 311 |
| | [3.05] | [3.70] | [3.45] | [7.13] | | [3.10] | [5.70] | [5.42] | [10.73] | |

*Notes:* This table reports heterogeneous treatment effects on salary payment experience by urban-rural district status, controlling for the district (strata) fixed effects. The sample consists of 970 (739) MoE employees who participated in the full endline 1 (2) survey. The district status is defined as urban (rural) if it has a population density of more (less) than 300 inhabitants per squared kilometer. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. ToT is the treatment-on-the-treated effect obtained by instrumenting self-reported MSP payments with the treatment group assignment. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.3:** Heterogeneous Effects on Salary Payment Experience by Baseline Payment System

| Outcome | Bank at Baseline | | | | | Trusted Agents at Baseline | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | LL Mean | EE Effect | EL Effect | ToT Effect | N Obs. | LL Mean | EE Effect | EL Effect | ToT Effect | N Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Panel A: Endline 1 – April 2019* | | | | | | | | | | |
| Share Voting in Favor of MSP | 0.54 | 0.17** | 0.12* | 0.28*** | 374 | 0.34 | 0.08 | 0.04 | 0.12 | 564 |
| | [0.06] | [0.07] | [0.06] | [0.11] | | [0.05] | [0.06] | [0.06] | [0.09] | |
| Share Very Satisfied with Pay System | 0.55 | 0.08 | 0.02 | 0.10 | 381 | 0.68 | -0.47*** | -0.27*** | -0.69*** | 573 |
| | [0.06] | [0.08] | [0.08] | [0.13] | | [0.05] | [0.06] | [0.06] | [0.10] | |
| Share Experienced Salary Delays | 0.51 | 0.04 | -0.06 | 0.04 | 383 | 0.31 | 0.43*** | 0.32*** | 0.65*** | 574 |
| | [0.06] | [0.08] | [0.08] | [0.13] | | [0.05] | [0.06] | [0.06] | [0.10] | |
| Payment to Receive Salary (Afg) | 21.27 | 0.39 | -2.35 | -0.48 | 376 | 21.93 | -4.71* | -0.05 | -6.80* | 571 |
| | [4.22] | [7.32] | [5.94] | [10.86] | | [3.02] | [2.45] | [2.58] | [3.58] | |
| Travel Time to Cash-Out (Min) | 34.85 | 4.25 | -2.68 | 6.58 | 372 | 28.75 | 44.51*** | 41.77*** | 66.23*** | 541 |
| | [3.18] | [5.11] | [4.41] | [8.02] | | [5.28] | [8.99] | [10.34] | [12.72] | |
| Travel Cost to Cash-Out (Afg) | 49.50 | 7.34 | 12.66 | 19.56 | 376 | 43.04 | 132.94*** | 97.13*** | 194.99*** | 546 |
| | [6.19] | [10.16] | [12.21] | [16.97] | | [11.34] | [24.68] | [22.19] | [35.78] | |
| *Panel B: Endline 2 – May 2020* | | | | | | | | | | |
| Share Voting in Favor of MSP | 0.71 | 0.11* | 0.05 | 0.34** | 302 | 0.59 | 0.12** | -0.02 | 0.22** | 402 |
| | [0.05] | [0.06] | [0.05] | [0.17] | | [0.05] | [0.06] | [0.06] | [0.09] | |
| Share Very Satisfied with Pay System | 0.73 | 0.10 | 0.05 | 0.30 | 310 | 0.73 | -0.07 | -0.08 | -0.10 | 416 |
| | [0.05] | [0.06] | [0.06] | [0.19] | | [0.04] | [0.06] | [0.06] | [0.09] | |
| Share Experienced Salary Delays | 0.33 | -0.12* | -0.10 | -0.33 | 310 | 0.29 | 0.03 | 0.06 | 0.03 | 415 |
| | [0.05] | [0.07] | [0.06] | [0.23] | | [0.04] | [0.05] | [0.05] | [0.08] | |
| Payment to Receive Salary (Afg) | 8.74 | -5.52** | -2.24 | -17.46*** | 312 | 15.96 | -11.53*** | 3.91 | -21.27*** | 416 |
| | [2.27] | [2.26] | [2.32] | [6.55] | | [2.76] | [2.83] | [4.19] | [4.57] | |
| Travel Time to Cash-Out (Min) | 37.69 | -5.21 | -3.67 | -14.83 | 307 | 34.48 | 8.99** | 4.34 | 14.15** | 408 |
| | [3.42] | [4.62] | [4.56] | [13.76] | | [2.74] | [4.38] | [4.16] | [6.72] | |
| Travel Cost to Cash-Out (Afg) | 51.16 | 7.33 | 1.78 | 21.26 | 271 | 49.54 | 53.11*** | 28.95** | 79.66*** | 354 |
| | [6.06] | [9.24] | [9.03] | [25.69] | | [6.67] | [13.45] | [12.02] | [21.26] | |

*Notes:* This table reports heterogeneous treatment effects on salary payment experience by the baseline payment system, controlling for the district (strata) fixed effects. The sample consists of 958 (729) MoE employees who participated in the full endline 1 (2) survey. The payment system (trusted agents or banks) is defined by the majority answer in each school at baseline (the school is dropped if answers are split equally between the two systems). LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. ToT is the treatment-on-the-treated effect obtained by instrumenting self-reported MSP payments with the treatment group assignment. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.4:** Effects on Quality of Education at Endline 1

| Outcome | LL Mean (1) | EE Effect (2) | EL Effect (3) | ToT Effect (4) | N Obs. |
|---|---|---|---|---|---|
| *Panel A: Employees' Attendance* | | | | | |
| Share Present at Midline | 0.52 | -0.02 | -0.03 | | 10906 |
| | [0.03] | [0.03] | [0.03] | | |
| Share Present at Endline | 0.46 | -0.04 | -0.02 | -0.04 | 10731 |
| | [0.03] | [0.03] | [0.03] | [0.03] | |
| *Panel B: Students' Learning Assessment* | | | | | |
| Total Score (std.dev.) | 0.00 | 0.05 | 0.06 | 0.07 | 1101 |
| | [0.06] | [0.08] | [0.08] | [0.11] | |
| Math Score (std.dev.) | 0.00 | 0.05 | 0.09 | 0.07 | 1101 |
| | [0.06] | [0.08] | [0.09] | [0.11] | |
| Reading Score (std.dev.) | 0.00 | 0.04 | 0.04 | 0.06 | 1101 |
| | [0.06] | [0.08] | [0.08] | [0.11] | |

*Notes:* This table reports treatment effects for the outcomes measuring the quality of education during the first year of the reform, controlling for the district (strata) fixed effects. Panel A presents the results for attendance rates of MoE employees at the Midline (November 2018) and Endline 1 (April 2019). The sample consists of MoE employees working in the visited schools who received positive salary payments at baseline according to the administrative payroll data (M41 forms). The employee is present if the survey team found him at the school during the unannounced visit. Endline ToT is the treatment-on-the-treated effect obtained by instrumenting the payment via MSP with the treatment group assignment. Panel B presents the results for students' learning outcomes based on the independent learning assessment. The sample consists of 1,001 children aged 6–10 who live in the catchment area of 367 experimental schools. The test scores are standardized relative to the outcomes in the control group. Test score ToTs are the treatment-on-the-treated effects obtained by instrumenting the share of employees paid via MSP with the treatment group assignment. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.5:** Effects on Mobile Money Wallet Use

| Outcome | 1 Month Mean (1) | # Months OLS (2) | # Months 2SLS (3) | N Obs. (4) |
|---|---|---|---|---|
| Deposit (AFN) | 28.56 | 40.20*** | 47.68*** | 15951 |
| | [21.85] | [8.33] | [14.05] | |
| Transfer to Customer Wallet (AFN) | 19.52 | 30.66** | 40.57 | 15951 |
| | [19.81] | [14.05] | [27.15] | |
| Airtime Top-up to Own Number (AFN) | 1.23 | 6.64*** | 7.01*** | 15951 |
| | [0.39] | [0.87] | [1.61] | |
| Airtime Top-up to Another Number (AFN) | 5.43 | 16.81*** | 19.18*** | 15951 |
| | [1.62] | [2.87] | [4.88] | |
| Pre-Pay Balance (AFN) | 1319.43 | 85.85*** | 52.50* | 15951 |
| | [181.93] | [12.29] | [26.98] | |

*Notes:* This table reports the effects of an additional month of MSP payments on mobile money wallet use, controlling for the district (strata) fixed effects. The sample consists of 15,951 employees who received at least one payment via MSP. 2SLS instruments the number of months paid via MSP with the treatment group assignment (F-statistic in the first stage is 125.4). On average, employees received MSP payments for 12.9 months (14.7 in EE, 13.4 in EL, and 5.9 months in LL groups). The following outcomes are measured as the total value of transactions conducted in October 2018 – December 2020: deposits are money added to the mobile money wallet via agents or bank transfers; transfers to customer wallet are peer-to-peer transfers to another mobile money user; airtime top-ups are money added to the pre-paid mobile phone call plan. Pre-pay balance is the remaining balance at the time of the last MoE payment for each employee. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.6:** Heterogeneous Effects on Mobile Money Wallet Use by Baseline Payment System

| Outcome | Bank at Baseline | | | | Trusted Agents at Baseline | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 Month Mean | # Months OLS | # Months 2SLS | N Obs. | 1 Month Mean | # Months OLS | # Months 2SLS | N Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Deposit (AFN) | 10.55 | 43.98*** | 39.11** | 8142 | 76.95 | 30.85*** | 86.62*** | 7809 |
| | [9.41] | [10.60] | [15.30] | | [76.71] | [11.95] | [28.73] | |
| Transfer to Customer Wallet (AFN) | 0.00 | 30.98 | 45.41 | 8142 | 71.99 | 29.86** | 11.43 | 7809 |
| | [0.00] | [18.86] | [31.85] | | [72.79] | [14.15] | [21.99] | |
| Airtime Top-up to Own Number (AFN) | 1.25 | 7.30*** | 7.58*** | 8142 | 1.18 | 5.00*** | 4.94 | 7809 |
| | [0.33] | [1.14] | [1.80] | | [1.12] | [1.06] | [3.53] | |
| Airtime Top-up to Another Number (AFN) | 6.78 | 20.31*** | 21.53*** | 8142 | 1.82 | 8.12*** | 6.33 | 7809 |
| | [2.16] | [3.80] | [5.62] | | [1.29] | [1.88] | [6.00] | |
| Pre-Pay Balance (AFN) | 1429.85 | 86.91*** | 43.48 | 8142 | 867.38 | 92.75*** | 26.52 | 7809 |
| | [200.37] | [14.93] | [29.45] | | [332.28] | [22.41] | [71.59] | |

*Notes:* This table reports the heterogeneous effects of an additional month of MSP payments on mobile money wallet use by the baseline payment system, controlling for the district (strata) fixed effects. The sample consists of 15,951 employees who received at least one payment via MSP. 2SLS instruments the number of months paid via MSP with the treatment group assignment (F-statistic in the first stage is 39.2 for trusted agents and 113.5 for banks). On average, employees previously paid by trusted agents received MSP payments for 13.9 months (15 in EE, 13.7 in EL, and 7.1 months in LL groups), while employees previously paid by banks received MSP payments for 11.9 months (14.2 in EE, 13.1 in EL, and 5.5 months in LL groups). The following outcomes are measured as the total value of transactions conducted in October 2018 – December 2020: deposits are money added to the mobile money wallet via agents or bank transfers; transfers to customer wallet are peer-to-peer transfers to another mobile money user; airtime top-ups are money added to the pre-paid mobile phone call plan. Pre-pay balance is the remaining balance at the time of the last MoE payment for each employee. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

# 2.A Appendix: Additional Figures and Tables

Panel A: Urban Registration Zone Panel B: Rural Registration Zone

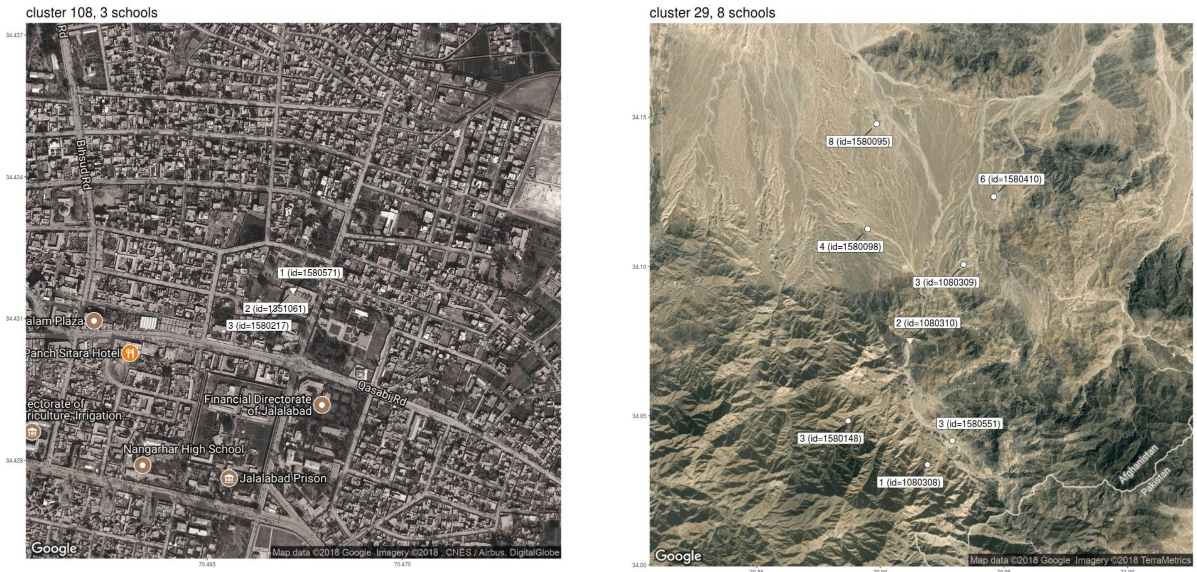


**Figure 2.A.1:** Examples of Two Registration Zones in Nangarhar

*Notes:* This figure shows examples of two registration zones located in urban and rural areas of Nangarhar Province. White windows display the location and identifiers of the schools located in these zones.


**Figure 2.A.2:** Litmus Test Scores by Registration Status

*Notes*: This figure presents litmus test scores for each question by employees' registration status (weighted by the sampling probability). The sample consists of 2,545 employees from Early Registration zones, selected for the phone survey of ghost employees in February of 2019. The litmus test score is defined as the total number of correct answers to the following seven questions: school district and name, employee's rank and position, principal's and headmaster's names, and the total number of employees. The employee is "Registered on Time" if he registered for a mobile money wallet during the main registration wave (before July 18, 2018). Alternatively, the employee is "Registered with Delay" if he registered after the main registration wave (after July 18, 2018).

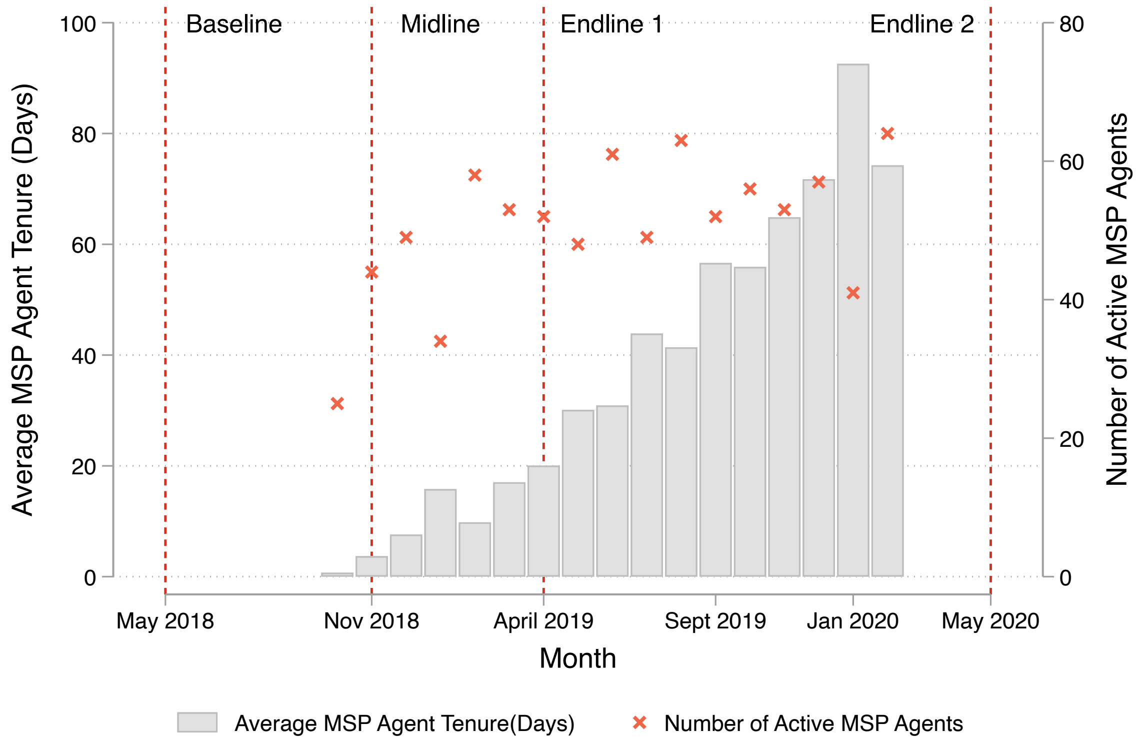


**Figure 2.A.3:** Tenure of MSP Agents

*Notes:* This figure plots the average MSP tenure and the total number of active mobile money agents across months of program implementation. Tenure is measured by the number of days an agent was active in prior months.

**Table 2.A.1:** Balance of Nonresponse Rates for Main Outcomes

| Outcome | Sample Mean (1) | LL Mean (2) | EE Mean (3) | EL Mean (4) | P-value LL=EE=EL (5) | N Obs. (6) |
|---|---|---|---|---|---|---|
| *Panel A: Baseline – May 2018* | | | | | | |
| Share Voting in Favor of MSP | 0.02 | 0.02 | 0.02 | 0.01 | 0.82 | 1005 |
| | (0.14) | [0.01] | [0.01] | [0.01] | | |
| Share Very Satisfied with Pay System | 0.01 | 0.00 | 0.01 | 0.00 | 0.50 | 1005 |
| | (0.08) | [0.00] | [0.01] | [0.00] | | |
| Share Experienced Salary Delays | 0.00 | 0.00 | 0.00 | 0.01 | 0.24 | 1005 |
| | (0.06) | [0.00] | [0.00] | [0.01] | | |
| Payment to Receive Salary (Afg) | 0.01 | 0.02 | 0.01 | 0.01 | 0.88 | 1005 |
| | (0.12) | [0.01] | [0.01] | [0.01] | | |
| Travel Time to Cash-Out (Min) | 0.08 | 0.08 | 0.08 | 0.07 | 0.86 | 1005 |
| | (0.27) | [0.03] | [0.02] | [0.02] | | |
| *Panel B: Endline 1 – April 2019* | | | | | | |
| Share Voting in Favor of MSP | 0.02 | 0.03 | 0.01 | 0.02 | 0.31 | 970 |
| | (0.14) | [0.01] | [0.01] | [0.01] | | |
| Share Very Satisfied with Pay System | 0.00 | 0.01 | 0.00 | 0.00 | 0.83 | 970 |
| | (0.06) | [0.00] | [0.00] | [0.00] | | |
| Share Experienced Salary Delays | 0.00 | 0.00 | 0.00 | 0.00 | 0.32 | 970 |
| | (0.03) | [0.00] | [0.00] | [0.00] | | |
| Payment to Receive Salary (Afg) | 0.01 | 0.01 | 0.02 | 0.01 | 0.44 | 970 |
| | (0.11) | [0.00] | [0.01] | [0.01] | | |
| Travel Time to Cash-Out (Min) | 0.05 | 0.05 | 0.04 | 0.06 | 0.64 | 970 |
| | (0.22) | [0.02] | [0.01] | [0.02] | | |
| *Panel C: Endline 2 – May 2020* | | | | | | |
| Share Voting in Favor of MSP | 0.04 | 0.06 | 0.02 | 0.03 | 0.14 | 739 |
| | (0.19) | [0.02] | [0.01] | [0.01] | | |
| Share Very Satisfied with Pay System | 0.00 | 0.00 | 0.00 | 0.01 | 0.22 | 739 |
| | (0.06) | [0.00] | [0.00] | [0.01] | | |
| Share Experienced Salary Delays | 0.01 | 0.00 | 0.00 | 0.01 | 0.13 | 739 |
| | (0.07) | [0.00] | [0.00] | [0.01] | | |
| Payment to Receive Salary (Afg) | 0.00 | 0.00 | 0.00 | 0.00 | 0.32 | 739 |
| | (0.04) | [0.00] | [0.00] | [0.00] | | |
| Travel Time to Cash-Out (Min) | 0.02 | 0.02 | 0.01 | 0.03 | 0.48 | 739 |
| | (0.14) | [0.01] | [0.01] | [0.01] | | |

*Notes:* This table reports balance of nonresponse rates for the main outcomes of the study. The sample consists of MoE employees who participated in the full survey at the baseline, endline 1 and endline 2, respectively. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. Standard errors in parentheses and robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.2:** Balance of Outcomes at Baseline

| Outcome | Sample Mean (1) | LL Mean (2) | EE Effect (3) | EL Effect (4) | P-value LL=EE=EL (5) | N Obs. (6) |
|---|---|---|---|---|---|---|
| *Panel A: Full Survey* | | | | | | |
| Share Voting in Favor of MSP | 0.92 | 0.94 | -0.03 | -0.02 | 0.55 | 985 |
| | (0.27) | [0.02] | [0.03] | [0.02] | | |
| Share Very Satisfied with Pay System | 0.28 | 0.30 | -0.02 | -0.04 | 0.68 | 999 |
| | (0.45) | [0.03] | [0.04] | [0.04] | | |
| Share Experienced Salary Delays | 0.57 | 0.54 | 0.09** | 0.01 | 0.13 | 1001 |
| | (0.50) | [0.04] | [0.04] | [0.04] | | |
| Payment to Receive Salary (Afg) | 22.78 | 23.53 | 1.09 | -3.96 | 0.66 | 990 |
| | (46.26) | [4.17] | [3.74] | [4.22] | | |
| Travel Time to Cash-Out (Min) | 32.91 | 33.41 | -2.38 | -1.84 | 0.97 | 928 |
| | (37.19) | [3.19] | [3.65] | [3.35] | | |
| Travel Cost to Cash-Out (Afg) | 60.77 | 58.72 | -1.43 | 0.08 | 0.95 | 935 |
| | (87.70) | [7.83] | [6.05] | [5.98] | | |
| *Panel B: Listing Exercise* | | | | | | |
| Share Employees Present | 0.55 | 0.56 | 0.00 | -0.01 | 0.65 | 10986 |
| | (0.50) | [0.04] | [0.04] | [0.04] | | |

*Notes:* This table shows the balance of primary outcomes at baseline, controlling for the district (strata) fixed effects. The full survey sample is 1,005 MoE employees who participated in the baseline survey. The listing exercise sample consists of MoE employees who worked in schools visited during baseline and received salary payments in the same month according to the administrative payroll data (M41 forms). The employee is present if the survey team found him at school during the unannounced visit. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. Standard errors in parentheses and robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.3:** MSP Effects: First Stage Estimates

| Outcome | LL Mean (1) | EE Effect (2) | EL Effect (3) | EE - EL Difference (4) | N Obs. (5) |
|---|---|---|---|---|---|
| *Panel A: Endline 1 – April 2019* | | | | | |
| Share Self-Reported MSP | 0.08 | 0.63*** | 0.37*** | 0.27*** | 970 |
| | [0.02] | [0.04] | [0.04] | [0.05] | |
| *Panel B: Endline 2 – May 2020* | | | | | |
| Share Self-Reported MSP | 0.33 | 0.49*** | 0.15*** | 0.32*** | 739 |
| | [0.04] | [0.04] | [0.04] | [0.04] | |

*Notes:* This table reports the first stage estimates, controlling for the district (strata) fixed effects. The Endline 1 (2) sample consists of 970 (739) MoE employees who participated in the full survey and self-reported the payment system. Share self-reported MSP is the share of employees that self-reported receiving their last salary payment via Mobile Salary Payments at Endline 1 and 2, respectively. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.4:** Effects on Salary Payment Experience (without Strata Fixed Effects)

| Outcome | LL Mean (1) | EE Effect (2) | EL Effect (3) | ToT Effect (4) | N Obs. (5) |
|---|---|---|---|---|---|
| *Panel A: Endline 1 – April 2019* | | | | | |
| Share Voting in Favor of MSP | 0.42 | 0.08 | 0.05 | 0.13 | 950 |
| | [0.04] | [0.06] | [0.06] | [0.09] | |
| Share Very Satisfied with Pay System | 0.63 | -0.26*** | -0.15*** | -0.40*** | 966 |
| | [0.04] | [0.05] | [0.06] | [0.09] | |
| Share Experienced Salary Delays | 0.41 | 0.26*** | 0.12** | 0.40*** | 969 |
| | [0.04] | [0.05] | [0.06] | [0.08] | |
| Payment to Receive Salary (Afg) | 22.31 | -3.73 | -0.45 | -5.59 | 959 |
| | [2.63] | [4.11] | [3.99] | [6.24] | |
| Travel Time to Cash-Out (Min) | 31.28 | 29.36*** | 20.68*** | 46.08*** | 922 |
| | [3.24] | [6.30] | [7.22] | [9.42] | |
| Travel Cost to Cash-Out (Afg) | 45.72 | 82.64*** | 56.56*** | 129.27*** | 932 |
| | [6.78] | [16.32] | [15.23] | [24.97] | |
| *Panel B: Endline 2 – May 2020* | | | | | |
| Share Voting in Favor of MSP | 0.65 | 0.10** | 0.01 | 0.23*** | 712 |
| | [0.04] | [0.05] | [0.05] | [0.09] | |
| Share Very Satisfied with Pay System | 0.73 | -0.02 | -0.02 | -0.04 | 736 |
| | [0.03] | [0.04] | [0.04] | [0.09] | |
| Share Experienced Salary Delays | 0.32 | -0.02 | -0.01 | -0.04 | 735 |
| | [0.03] | [0.04] | [0.04] | [0.09] | |
| Payment to Receive Salary (Afg) | 12.48 | -8.06*** | 0.99 | -18.40*** | 738 |
| | [1.85] | [2.27] | [3.28] | [3.87] | |
| Travel Time to Cash-Out (Min) | 36.63 | 3.47 | 0.16 | 7.54 | 725 |
| | [2.19] | [3.26] | [2.98] | [6.63] | |
| Travel Cost to Cash-Out (Afg) | 53.32 | 34.50*** | 15.40* | 68.32*** | 635 |
| | [5.49] | [10.96] | [8.44] | [24.24] | |

*Notes:* This table reports treatment effects on primary outcomes. The Endline 1 (2) sample consists of 970 (739) MoE employees who participated in the full survey and reported their payment system. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. ToT is a treatment-on-the-treated effect obtained by instrumenting a self-reported MSP payment with the treatment group assignment. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.5:** Effects on Salary Payment Experience (with Baseline Controls)

| Outcome | LL Mean (1) | EE Effect (2) | EL Effect (3) | ToT Effect (4) | N Obs. (5) |
|---|---|---|---|---|---|
| *Panel A: Endline 1 – April 2019* | | | | | |
| Share Voting in Favor of MSP | 0.42 | 0.11** | 0.08* | 0.17** | 938 |
| | [0.04] | [0.04] | [0.05] | [0.07] | |
| Share Very Satisfied with Pay System | 0.63 | -0.25*** | -0.14*** | -0.39*** | 954 |
| | [0.04] | [0.05] | [0.05] | [0.08] | |
| Share Experienced Salary Delays | 0.41 | 0.27*** | 0.16*** | 0.42*** | 956 |
| | [0.04] | [0.05] | [0.05] | [0.07] | |
| Payment to Receive Salary (Afg) | 22.31 | -2.86 | -1.25 | -4.32 | 946 |
| | [2.63] | [3.37] | [2.94] | [4.85] | |
| Travel Time to Cash-Out (Min) | 31.28 | 25.52*** | 23.01*** | 41.26*** | 863 |
| | [3.24] | [6.08] | [6.54] | [8.78] | |
| *Panel B: Endline 2 – May 2020* | | | | | |
| Share Voting in Favor of MSP | 0.65 | 0.12*** | 0.01 | 0.26*** | 707 |
| | [0.04] | [0.04] | [0.04] | [0.08] | |
| Share Very Satisfied with Pay System | 0.73 | 0.01 | -0.01 | 0.02 | 728 |
| | [0.03] | [0.04] | [0.04] | [0.08] | |
| Share Experienced Salary Delays | 0.32 | -0.04 | -0.02 | -0.07 | 727 |
| | [0.03] | [0.04] | [0.04] | [0.08] | |
| Payment to Receive Salary (Afg) | 12.48 | -7.78*** | 1.46 | -18.05*** | 730 |
| | [1.85] | [2.01] | [2.58] | [3.90] | |
| Travel Time to Cash-Out (Min) | 36.63 | -0.03 | -0.19 | 0.01 | 672 |
| | [2.19] | [3.27] | [3.23] | [6.31] | |

*Notes:* This table reports treatment effects on the salary payment experience, controlling for the district (strata) fixed effects. The Endline 1 (2) sample consists of 957 (731) MoE employees who participated in the full survey and self-reported the payment system, and whose school had the full baseline survey. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. ToT is the treatment-on-the-treated effect obtained by instrumenting the self-reported payment via MSP with the treatment group assignment. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.6:** Spatial Treatment Externalities on Salary Payment Experience (within 2 km)

| Outcome | LL Mean (1) | EE Effect (2) | EL Effect (3) | # EE Neighb Schools 2 km (4) | # EL Neighb Schools 2 km (5) | # Total Neighb Schools 2 km (6) | N Obs. (7) |
|---|---|---|---|---|---|---|---|
| *Panel A: Endline 1 – April 2019* | | | | | | | |
| Share Voting in Favor of MSP | 0.42 | 0.11** | 0.09** | 0.00 | -0.02 | 0.00 | 950 |
| | [0.04] | [0.04] | [0.04] | [0.01] | [0.02] | [0.01] | |
| Share Very Satisfied with Pay System | 0.63 | -0.25*** | -0.14*** | 0.00 | -0.00 | -0.00 | 966 |
| | [0.04] | [0.05] | [0.05] | [0.02] | [0.02] | [0.01] | |
| Share Experienced Salary Delays | 0.41 | 0.25*** | 0.15*** | 0.00 | -0.01 | 0.01 | 969 |
| | [0.04] | [0.05] | [0.05] | [0.02] | [0.02] | [0.01] | |
| Payment to Receive Salary (Afg) | 22.31 | -4.05 | -1.04 | 0.64 | -0.73 | -0.09 | 959 |
| | [2.63] | [3.02] | [3.00] | [1.20] | [0.95] | [0.55] | |
| Travel Time to Cash-Out (Min) | 31.28 | 28.10*** | 22.17*** | -0.32 | 0.19 | -0.77 | 922 |
| | [3.24] | [6.31] | [6.52] | [1.56] | [1.69] | [0.79] | |
| *Panel B: Endline 2 – May 2020* | | | | | | | |
| Share Voting in Favor of MSP | 0.65 | 0.11*** | 0.01 | 0.01 | 0.00 | -0.00 | 712 |
| | [0.04] | [0.04] | [0.04] | [0.01] | [0.02] | [0.01] | |
| Share Very Satisfied with Pay System | 0.73 | -0.01 | -0.02 | 0.01 | 0.01 | -0.00 | 736 |
| | [0.03] | [0.04] | [0.04] | [0.01] | [0.01] | [0.01] | |
| Share Experienced Salary Delays | 0.32 | -0.03 | -0.03 | -0.00 | 0.01 | -0.01 | 735 |
| | [0.03] | [0.04] | [0.04] | [0.01] | [0.01] | [0.01] | |
| Payment to Receive Salary (Afg) | 12.48 | -8.72*** | 1.54 | 0.83 | -0.49 | -0.11 | 738 |
| | [1.85] | [2.25] | [2.59] | [0.85] | [0.90] | [0.39] | |
| Travel Time to Cash-Out (Min) | 36.63 | 3.35 | -0.24 | -1.35 | 0.42 | -0.03 | 725 |
| | [2.19] | [3.27] | [3.05] | [1.10] | [1.00] | [0.61] | |

*Notes:* This table reports spatial treatment externalities for the main outcomes of the study within a two-kilometer radius, controlling for the district (strata) fixed effects. The Endline 1 (2) sample consists of 970 (739) MoE employees who participated in the full survey and self-reported the payment system. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. # EE (EL) Neighb Schools 2 km is the number of neighboring schools within a two-kilometer radius located in EE (EL) treatment zone, # Total Neighb Schools 2 km is the total number of neighboring schools within the same radius. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.7:** Effects of Registration and Payments: First Stage Estimates

| Outcome | LL Mean (1) | EE Effect (2) | EL Effect (3) | EE - EL Difference (4) | F-stat (5) | N Obs. (6) |
|---|---|---|---|---|---|---|
| *Panel A: Endline 1 – April 2019* | | | | | | |
| Share Registered | 0.66 | 0.26*** | 0.25*** | 0.01 | 24.74 | 970 |
| | [0.04] | [0.04] | [0.04] | [0.02] | | |
| Share Self-Reported MSP | 0.08 | 0.63*** | 0.37*** | 0.27*** | 117.37 | 970 |
| | [0.02] | [0.04] | [0.04] | [0.05] | | |
| *Panel B: Endline 2 – May 2020* | | | | | | |
| Share Registered | 0.65 | 0.29*** | 0.25*** | 0.02 | 25.13 | 739 |
| | [0.05] | [0.04] | [0.04] | [0.02] | | |
| Share Self-Reported MSP | 0.33 | 0.49*** | 0.15*** | 0.32*** | 68.09 | 739 |
| | [0.04] | [0.04] | [0.04] | [0.04] | | |

*Notes:* This table reports the first stage estimates, controlling for the district (strata) fixed effects. The Endline 1 (2) sample consists of 970 (739) MoE employees who participated in the full survey and self-reported the payment system. Share registered is the share of emoloyees who registered for a mobile money wallet before Endline 1 and 2, respectively. Share self-reported MSP is the share of employees that self-reported receiving their last salary payment via Mobile Salary Payments at Endline 1 and 2, respectively. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.8:** Effects of Registration and Payments on Salary Experience

| Outcome | LL Mean (1) | EE ITT Effect (2) | EL ITT Effect (3) | Registration ToT Effect (4) | Payment ToT Effect (5) | N Obs. (6) |
|---|---|---|---|---|---|---|
| *Panel A: Endline 1 – April 2019* | | | | | | |
| Share Voting in Favor of MSP | 0.42 | 0.11** | 0.08* | 0.14 | 0.12 | 950 |
| | [0.04] | [0.04] | [0.04] | [0.42] | [0.19] | |
| Share Satisfied with Payment System | 0.85 | -0.26*** | -0.13*** | 0.23 | -0.51*** | 966 |
| | [0.03] | [0.04] | [0.04] | [0.39] | [0.18] | |
| Share Experienced Salary Delays | 0.41 | 0.25*** | 0.15*** | -0.02 | 0.41** | 969 |
| | [0.04] | [0.05] | [0.05] | [0.41] | [0.18] | |
| Payment to Receive Salary (Afg) | 22.31 | -3.21 | -1.64 | 2.09 | -5.86 | 959 |
| | [2.63] | [3.40] | [3.00] | [30.12] | [14.79] | |
| Travel Time to Cash-Out (Min) | 31.28 | 27.19*** | 21.82*** | 69.50 | 14.70 | 922 |
| | [3.24] | [5.88] | [6.05] | [70.41] | [30.49] | |
| Travel Cost to Cash-Out (Afg) | 45.72 | 80.06*** | 58.80*** | 140.45 | 68.45 | 932 |
| | [6.78] | [15.59] | [13.55] | [128.01] | [59.20] | |
| *Panel B: Endline 2 – May 2020* | | | | | | |
| Share Voting in Favor of MSP | 0.65 | 0.12*** | 0.01 | -0.15 | 0.34*** | 712 |
| | [0.04] | [0.04] | [0.04] | [0.20] | [0.13] | |
| Share Satisfied with Payment System | 0.99 | -0.03* | -0.03* | -0.13 | 0.02 | 736 |
| | [0.01] | [0.01] | [0.01] | [0.09] | [0.06] | |
| Share Experienced Salary Delays | 0.32 | -0.03 | -0.02 | -0.05 | -0.04 | 735 |
| | [0.03] | [0.04] | [0.04] | [0.21] | [0.13] | |
| Payment to Receive Salary (Afg) | 12.48 | -7.79*** | 1.25 | 21.68 | -28.53*** | 738 |
| | [1.85] | [2.03] | [2.55] | [14.39] | [8.46] | |
| Travel Time to Cash-Out (Min) | 36.63 | 1.66 | -0.24 | -4.61 | 5.94 | 725 |
| | [2.19] | [3.19] | [3.00] | [15.35] | [8.89] | |
| Travel Cost to Cash-Out (Afg) | 53.32 | 28.54*** | 13.39 | 33.20 | 36.99 | 635 |
| | [5.49] | [9.22] | [8.20] | [38.02] | [25.81] | |

*Notes:* This table reports treatment effects for the main outcomes of the study, controlling for the district (strata) fixed effects. The Endline 1 (2) sample consists of 970 (739) MoE employees who participated in the full survey and self-reported the payment system. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. Registration and Payment ToT effects are the treatment-on-the-treated effects obtained by jointly instrumenting the registration and self-reported MSP payment with the treatment group assignment. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.9:** Heterogeneous Effects on Salary Payment Experience by Territory Control

| Outcome | Government Control | | | | | Insurgent Activity | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | LL Mean | EE Effect | EL Effect | ToT Effect | N Obs. | LL Mean | EE Effect | EL Effect | ToT Effect | N Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Panel A: Endline 1 – April 2019* | | | | | | | | | | |
| Share Voting in Favor of MSP | 0.45 | 0.13*** | 0.13*** | 0.22*** | 797 | 0.28 | 0.00 | -0.25*** | 0.07 | 153 |
| | [0.04] | [0.05] | [0.05] | [0.07] | | [0.07] | [0.09] | [0.09] | [0.17] | |
| Share Very Satisfied with Pay System | 0.65 | -0.27*** | -0.17*** | -0.42*** | 812 | 0.55 | -0.15 | -0.01 | -0.29 | 154 |
| | [0.04] | [0.06] | [0.05] | [0.08] | | [0.10] | [0.11] | [0.10] | [0.19] | |
| Share Experienced Salary Delays | 0.42 | 0.26*** | 0.15*** | 0.39*** | 814 | 0.32 | 0.24*** | 0.12 | 0.45*** | 155 |
| | [0.04] | [0.06] | [0.06] | [0.08] | | [0.09] | [0.09] | [0.08] | [0.17] | |
| Payment to Receive Salary (Afg) | 21.22 | -4.86 | -1.97 | -7.10 | 804 | 27.68 | 4.92 | -0.61 | 9.73 | 155 |
| | [2.83] | [3.96] | [3.36] | [5.67] | | [6.77] | [5.11] | [5.91] | [9.91] | |
| Travel Time to Cash-Out (Min) | 29.34 | 27.18*** | 24.06*** | 42.38*** | 767 | 40.23 | 27.41 | 9.16 | 51.21* | 155 |
| | [2.78] | [5.96] | [6.80] | [8.66] | | [13.12] | [18.37] | [10.87] | [29.79] | |
| Travel Cost to Cash-Out (Afg) | 50.32 | 72.55*** | 60.68*** | 111.74*** | 778 | 24.00 | 116.25** | 43.63 | 217.55** | 154 |
| | [7.74] | [15.11] | [14.88] | [22.44] | | [11.26] | [53.15] | [29.11] | [84.88] | |
| *Panel B: Endline 2 – May 2020* | | | | | | | | | | |
| Share Voting in Favor of MSP | 0.67 | 0.12*** | 0.01 | 0.26*** | 609 | 0.51 | 0.11 | 0.05 | 0.30 | 103 |
| | [0.04] | [0.05] | [0.04] | [0.09] | | [0.08] | [0.10] | [0.10] | [0.29] | |
| Share Very Satisfied with Pay System | 0.72 | 0.03 | 0.02 | 0.05 | 627 | 0.74 | -0.17 | -0.21** | -0.39 | 109 |
| | [0.04] | [0.04] | [0.04] | [0.08] | | [0.05] | [0.11] | [0.09] | [0.29] | |
| Share Experienced Salary Delays | 0.33 | -0.06 | -0.06 | -0.10 | 626 | 0.23 | 0.13 | 0.23** | 0.28 | 109 |
| | [0.03] | [0.05] | [0.04] | [0.09] | | [0.06] | [0.10] | [0.09] | [0.27] | |
| Payment to Receive Salary (Afg) | 10.22 | -7.94*** | 1.91 | -17.49*** | 628 | 23.72 | -6.94 | -3.19 | -18.77 | 110 |
| | [1.79] | [2.06] | [2.67] | [3.83] | | [5.67] | [6.77] | [8.17] | [17.75] | |
| Travel Time to Cash-Out (Min) | 36.85 | -0.01 | -1.45 | 0.76 | 617 | 35.50 | 10.95 | 7.05 | 27.84 | 108 |
| | [2.50] | [3.39] | [3.28] | [6.18] | | [3.96] | [8.63] | [7.28] | [19.70] | |
| Travel Cost to Cash-Out (Afg) | 53.90 | 20.06** | 12.18 | 34.05** | 540 | 50.29 | 77.39** | 19.48 | 235.77*** | 95 |
| | [6.33] | [9.00] | [8.43] | [16.36] | | [8.55] | [33.06] | [25.72] | [71.40] | |

*Notes:* This table reports heterogeneous treatment effects on salary payment experience by district territory control based on the 2018 SIGAR Records, controlling for the district (strata) fixed effects. The sample consists of 970 (739) MoE employees who participated in the full endline 1 (2) survey. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. ToT is the treatment-on-the-treated effect obtained by instrumenting self-reported MSP payments with the treatment group assignment. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.10:** Effects on Financial Inclusion at Endline 1

| Outcome | LL Mean (1) | EE Effect (2) | EL Effect (3) | ToT Effect (4) | N Obs. (5) |
|---|---|---|---|---|---|
| P2P Transactions Index | 0.00 | 0.39 [0.005] | 0.35 [0.006] | 0.63 [0.002] | 965 |
| Money Transfer via Mobile Phone (=1) | 0.06 | 0.18 (0.001) | 0.17 (0.001) | 0.29 (0.001) | 965 |
| Money Transfer via Other Means (=1) | 0.24 | 0.02 (0.742) | 0.01 (0.756) | 0.03 (0.471) | 965 |
| | | | | | |
| Savings Index | 0.00 | 0.08 [0.673] | 0.07 [0.409] | 0.13 [0.595] | 960 |
| Saves Typically (=1) | 0.36 | 0.02 (0.688) | 0.07 (0.306) | 0.05 (0.739) | 960 |
| Saves: Mobile Money Account (=1) | 0.00 | 0.02 (0.244) | 0.02 (0.244) | 0.04 (0.065) | 960 |
| Saves: Other than Mobile Money (=1) | 0.18 | 0.00 (0.871) | 0.01 (0.719) | 0.00 (1.000) | 960 |
| Savings: Mobile Money Account (AFG) | 0 | 106 (0.414) | 47 (0.306) | 164 (0.504) | 960 |
| Savings: Other than Mobile Money (AFG) | 3547 | 864 (0.799) | -1109 (0.688) | 1030 (1.000) | 960 |
| | | | | | |
| Financial Empowerment Index | 0.00 | -0.09 [0.794] | -0.13 [0.440] | -0.16 [0.712] | 952 |
| Empowered in Small Purchases (=1) | 0.92 | -0.03 (0.567) | -0.05 (0.330) | -0.05 (0.566) | 952 |
| Empowered in Expensive Purchases (=1) | 0.97 | -0.01 (0.567) | -0.01 (0.567) | -0.02 (0.566) | 952 |
| | | | | | |
| Consumption Index | 0.00 | -0.01 [0.874] | 0.02 [0.956] | -0.01 [0.893] | 959 |
| No Hunger | 0.88 | 0.04 (0.524) | 0.06 (0.201) | 0.07 (0.648) | 959 |
| No Unpaid School Fees | 0.82 | -0.01 (0.912) | -0.01 (0.912) | -0.02 (0.822) | 959 |
| No Default | 0.64 | 0.00 (0.912) | 0.02 (0.912) | 0.00 (0.957) | 959 |
| No Unpaid Medical Emergency | 0.74 | -0.05 (0.647) | -0.05 (0.524) | -0.08 (0.648) | 959 |
| | | | | | |
| Understands Salary Calculation (=1) | 0.80 | -0.03 [0.827] | 0.01 [0.943] | -0.04 [0.875] | 963 |
| | | | | | |
| Borrowing Index | 0.00 | 0.05 [0.827] | 0.02 [0.956] | 0.07 [0.875] | 948 |
| Outstanding Loan (=1) | 0.58 | 0.07 (1.000) | 0.03 (1.000) | 0.11 (1.000) | 948 |
| Overdue Loan (=1) | 0.52 | 0.04 (1.000) | 0.00 (1.000) | 0.05 (1.000) | 948 |
| Overdue Loan Amount (AFG) | 25662 | -1033 (1.000) | -128 (1.000) | -1517 (1.000) | 948 |
| Principal Loan Amount (AFG) | 30773 | -1971 (1.000) | 1636 (1.000) | -2478 (1.000) | 948 |

*Notes:* This table reports treatment effects for the financial inclusion outcomes at the endline 1 (April 2019), controlling for the district (strata) fixed effects. The sample consists of 970 MoE employees who participated in the full survey and self-reported the payment system in the Endline 1 survey. LL is the Late Registration, Late Payment (Control) group; EE is the Early Registration, Early Payment (Treatment 1) group; and EL is the Early Registration, Late Payment (Treatment 2) group. ToT is the treatment-on-the-treated effect obtained by instrumenting the self-reported payment via MSP with the treatment group assignment. Romano-Wolf p-values adjusting for FWER between indexes in square brackets. Sharpened q-values adjusting for FDR within indexes in brackets. Standard errors are clustered at the registration zone (treatment unit) level.

**Table 2.A.11:** Heterogeneous Effects on Mobile Money Wallet Use by Urban-Rural District Status

| Outcome | Urban District | | | | Rural District | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 Month Mean | # Months OLS | # Months 2SLS | N Obs. | 1 Month Mean | # Months OLS | # Months 2SLS | N Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Deposit (AFN) | 10.48 | 43.15*** | 41.01*** | 10306 | 78.34 | 30.89** | 80.37** | 5645 |
| | [9.35] | [9.98] | [15.05] | | [78.18] | [14.12] | [32.41] | |
| Transfer to Customer Wallet (AFN) | 0.00 | 30.88* | 45.16 | 10306 | 73.28 | 29.95* | 9.18 | 5645 |
| | [0.00] | [17.74] | [31.26] | | [74.20] | [16.46] | [25.85] | |
| Airtime Topup to Own Number (AFN) | 1.24 | 6.89*** | 7.40*** | 10306 | 1.20 | 5.84*** | 5.20 | 5645 |
| | [0.33] | [1.08] | [1.75] | | [1.15] | [1.23] | [4.14] | |
| Airtime Topup to Another Number (AFN) | 6.74 | 19.06*** | 21.06*** | 10306 | 1.85 | 9.68*** | 7.43 | 5645 |
| | [2.15] | [3.62] | [5.51] | | [1.32] | [2.17] | [7.08] | |
| Pre-Pay Balance (AFN) | 1427.00 | 79.64*** | 48.60* | 10306 | 610.36 | 124.72*** | 64.62 | 5645 |
| | [199.28] | [14.00] | [28.46] | | [217.52] | [24.75] | [72.29] | |

*Notes:* This table reports the heterogeneous effects of an additional month of MSP payments on mobile money wallet use by the urban-rural district status, controlling for the district (strata) fixed effects. The sample consists of 15,951 employees who received at least one payment via MSP. 2SLS instruments the number of months paid via MSP with the treatment group assignment (F-statistic in the first stage is 119.6 in urban districts and 27.9 in rural districts). On average, employees in urban districts received MSP payments for 12.6 months (14.7 in EE, 13.3 in EL, and 5.6 months in LL groups), while employees in rural districts received MSP payments for 13.3 months (14.6 in EE, 13.5 in EL, and 6.9 months in LL groups). The following outcomes are measured as the total value of transactions conducted in October 2018 – December 2020: deposits are money added to the mobile money wallet via agents or bank transfers; transfers to customer wallet are peer-to-peer transfers to another mobile money user; airtime top-ups are money added to the pre-paid mobile phone call plan. Pre-pay balance is the remaining balance at the time of the last MoE payment for each employee. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

**Table 2.A.12:** Heterogeneous Effects on Mobile Money Wallet Use by Territory Control

| Outcome | Government Control | | | | Insurgent Actitivity | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 Month Mean | # Months OLS | # Months 2SLS | N Obs. | 1 Month Mean | # Months OLS | # Months 2SLS | N Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Deposit (AFN) | 29.23 | 43.37*** | 47.18*** | 14521 | 0.00 | -22.05 | 154.58*** | 1430 |
| | [22.38] | [8.51] | [14.17] | | [0.00] | [33.68] | [53.26] | |
| Transfer to Customer Wallet (AFN) | 19.98 | 32.29** | 44.21 | 14521 | 0.00 | -1.38 | -106.40 | 1430 |
| | [20.29] | [14.73] | [27.24] | | [0.00] | [5.50] | [119.37] | |
| Airtime Topup to Own Number (AFN) | 1.26 | 6.83*** | 7.27*** | 14521 | 0.00 | 2.90*** | -5.72 | 1430 |
| | [0.40] | [0.91] | [1.63] | | [0.00] | [1.00] | [6.96] | |
| Airtime Topup to Another Number (AFN) | 5.56 | 17.45*** | 19.41*** | 14521 | 0.00 | 4.26* | 0.45 | 1430 |
| | [1.66] | [2.98] | [4.93] | | [0.00] | [2.46] | [0.46] | |
| Pre-Pay Balance (AFN) | 1311.87 | 87.33*** | 51.87* | 14521 | 810.88 | 128.99** | 258.90 | 1430 |
| | [184.81] | [12.94] | [27.29] | | [773.62] | [63.50] | [168.45] | |

*Notes:* This table reports the heterogeneous effects of an additional month of MSP payments on mobile money wallet use by the 2018 territory control, controlling for the district (strata) fixed effects. The sample consists of 15,951 employees who received at least one payment via MSP. 2SLS instruments the number of months paid via MSP with the treatment group assignment (F-statistic in the first stage is 124.7 for government-controlled districts and 15.9 for insurgent districts). On average, employees in government-controlled districts received MSP payments for 12.7 months (14.6 in EE, 13.3 in EL, and 5.8 months in LL groups), while employees in insurgent districts received MSP payments for 14.4 months (14.9 in EE, 13.6 in EL, and 11.5 months in LL groups). The following outcomes are measured as the total value of transactions conducted in October 2018 – December 2020: deposits are money added to the mobile money wallet via agents or bank transfers; transfers to customer wallet are peer-to-peer transfers to another mobile money user; airtime top-ups are money added to the pre-paid mobile phone call plan. Pre-pay balance is the remaining balance at the time of the last MoE payment for each employee. Robust standard errors clustered at the registration zone (treatment unit) level in squared brackets.

## 2.B Appendix: Validating Ghost Employees

### 2.B.1 Survey Sampling

We sampled a total of 2,663 employees who had a phone number listed in the payroll records, with a breakdown as follows:[24]

1. 753 employees who did not register by the time of the phone survey. We included in the sample all of the employees who had not registered, even if they should have. This is the group that we expect to have the highest proportion of ghost workers.

2. 987 employees who did not register by July 19, 2018, but did register before the phone survey took place. These employees are potentially suspicious because they failed to appear during the registration drive but eventually got registered. These could be either 'stand-ins,' who are not employees in any real sense, or simply employees who genuinely could not make registration. We sampled all employees fitting this description.

3. Three additional categories of employees, registered by July 19, 2018, and belonging to the same schools as the samples 1 and 2. These provide useful comparisons to the two suspicious cases above since they are similar employees working at the same school, who were given the same opportunity to register, but *did* appear for registration. These three categories are:

    a. 175 employees who registered on time, working in the 182 schools visited for the unannounced baseline visit, and present during this audit visit. We sampled one employee in each school, but seven schools had no employees satisfying these criteria. This is the sample of employees who are most likely to be genuine employees: they were present at school at baseline and registered on time. For this reason, we consider them our main comparison group.

[24] Almost all employees have a (possibly outdated) phone number listed in their payroll records. For employees who appear for registration, we also observe the new phone number given to them to open the mobile wallet account. Moreover, for employees who participated in our baseline survey, we also have the phone numbers that they reported as currently using. Using these phone numbers, when available, could have improved our chance of reaching those employees. However, to ensure that all employees had an equal chance of being contacted, we only called numbers listed in the payroll records before the experiment started.

b. 153 employees who registered on time and worked in the 182 schools visited for the baseline audit but were absent during the audit. We sampled one employee in each school, but 29 schools had no employee satisfying these criteria. These might be real employees who happened to be absent at the time of the audit or 'stand-ins' who registered during the first wave of registration.

c. 356 employees who registered on time and worked in the 356 schools that were not selected for the baseline audit. We sample one employee in this category from each school.

4. 239 employees registered on time and worked in the 239 schools with no ghost workers (we sample one employee per school). These are of interest because they work in extremely well-run schools, or the perfect registration record reflects a successful attempt to provide stand-ins for each slot on the payroll.

### 2.B.2 Estimates of 'Stand-ins'

We introduce some notation for clarifying how we use the results of the litmus test to bound the number of stand-ins who registered. Let $T$ be a dummy variable equal to 1 if a respondent is a true employee, $Z$ be a dummy variable equal to 1 if the respondent is a zombie or stand-in, and $R$ be a dummy for employees who registered, with $R = T + Z$.

Ideally, we would have liked to design a litmus test that could discriminate perfectly between true employees and zombies. Let $L$ be a dummy variable equal to 1 if the respondent passed the litmus test (for example, because he answered all seven questions correctly, but the threshold could be even a different one): then we would like the probability of success $\mu$ for true employees to be $\mu_T = P(L = 1|T = 1) = 1$ and $\mu_Z = P(L = 1|Z = 1) = 0$, that is all true employee will pass the test, and all zombies will fail it. The success rate among registered employees $\mu_R$ can be decomposed into $\mu_R = \mu_T \times P(T = 1) + \mu_Z \times P(Z = 1)$, and with $\mu_T = 1$ and $\mu_Z = 0$, $1 - \mu_R = P(Z = 1)$ identifies the proportion of stand-ins among registered employees.

Designing such a litmus test, however, proved challenging: indeed, we need to ask a set of questions which: i) would be easy enough for all true employees to answer correctly (even though some of them might have low literacy rates or other characteristics which would make answering the test hard), ii) would be non-trivial so that stand-ins could answer correctly, and iii) could be graded by us using information available (for example in the existing payroll records). While we ultimately failed to design a test that could discriminate perfectly true employees from false ones, we were able to create a test that was nevertheless informative, as Figure 2.4 indicates: employees who were more likely to be true employees (having registered in time and being present during our baseline audit visit) had on average higher scores than those less likely to be current employees (having not registered and been absent during our baseline audit).

This implies that, while we don't have $\mu_T = 1$ and $\mu_Z = 0$, we can use the phone survey responses to design litmus test $L$ such that $\mu_T > \mu_Z$. Moreover, below we show that by adding some assumptions to the exercise, it is still possible to recover information about the proportion $P(Z = 1)$ of stand-ins in the population of registered employees starting from the share of $\mu_R$ of employees who pass the test among those who register.

**Assumption 1.** First, we note that $\mu_R = \mu_T \times P(T = 1) + \mu_Z \times P(Z = 1)$ can be rewritten as $P(Z = 1) = \frac{\mu_R - \mu_T}{\mu_Z - \mu_T}$, so that assuming $\mu_Z = 0$ allows us to calculate a lower bound for $P(Z = 1)$.

Intuitively, the score among registered employees can be low because of zombies either i) because a lot of zombies get registered, or ii) because zombies have extremely low scores: so, assuming that zombies have a score of zero bounds from below the possible size of the zombie population. In this sense, assumption 1 is relatively unproblematic.

However, while $\mu_R$ can be estimated through the proportion of respondents who pass the litmus test in the data, $\mu_T$ is unobserved, so that even the lower bound $P(Z = 1)_{\mu_Z = 0} = 1 - \frac{\mu_R}{\mu_T}$ cannot be computed. Nevertheless, while we don't observe the success rate of all true employees because we cannot, in general, know who is a true employee and who is not, we observe it for a subsample of them. Indeed, we can reliably consider employees who registered early and were present during the baseline audit visit as true employees.

**Assumption 2.** We assume that there exists a test $L$ such that the probability of success of all true employees $\mu_T$ is equal to the probability of success of the subset of true employees who registered early and who were present during the baseline audit visit $\mu_V$ ($V$ for 'verified employees').

We consider this second assumption as more demanding: indeed, it could be the case that verified employees know more about their school with respect to true employees who are often absent from work. Using the threshold of a score of 7 in the litmus test would then be problematic, because verified employees would outperform absent true employees. The problem here is that in calculating $P(Z=1)_{\mu_Z=0} = 1 - \frac{\mu_R}{\mu_V}$ we would attribute differences between the performance of all registered employees $\mu_R$ and the performance of verified employees $\mu_V$ entirely to the presence of zombies scoring zero, while it is actually in part attributable to the lower performance of true but absent employees ($\mu_V > \mu_T$). This problem can be alleviated by lowering the threshold for the litmus test. For example, using a threshold of 4 rather than 7 should make it more likely that the proportion of true employees scoring at least 4 would be well approximated by the proportion of verified employees scoring at least 4 (the fact that the true employees score relatively more 4 and 5, and the verified employees score more 6 and 7 would not matter). The issue with lowering the threshold too much is that the first assumption that $\mu_Z = 0$, which is likely justifiable for a threshold of 7, might become less reasonable for a threshold of 4 if relatively many stand-ins could be getting such a score, leading to a lower bound too far from the true $P(Z=1)$.

Using these assumptions and the phone survey results, we find that a score of 7 results in a rate of stand-ins equal to at least 18.1% of registered employees, a score of 4 in a lower bound of 8.8%, and simply answering the phone in a bound of 5.8%. Adjusting these numbers for the share of registered employees (97.2%), the bounds range from 5.6% to 17.6% of all employees.

## 2.C Appendix: Spatial Treatment Externalities

The MSP reform could have created spatial externalities given the scale of its implementation and the amount of attention it received from policymakers. First, with the start of the reform, policymakers and ministries involved in its implementation could have paid more attention to improving the payment experience in all schools, regardless of their treatment status. This is partly evident from the changes in payment experience outcomes in the control group. Second, schools located near those in the treatment group could have felt pressure to improve the payment experience of their employees. Conversely, they could have also experienced deterioration in the payment experience if the reform had caused local disruptions in the payment process.

While it is not feasible to formally test for the program-wide externalities due to data limitations, we test for spatial externalities across studied schools. Following Miguel and Kremer (2004), we estimate it using the following specification:

$$\begin{aligned} Y_{iszd} = \alpha + \beta_{EE} EE_z + \beta_{EL} EL_z + \eta_{EE}\,\#EE\ Neighbors^{2km}_{szd} + \eta_{EL}\,\#EL\ Neighbors^{2km}_{szd} + \\ + \gamma\,\#Total\ Neighbors^{2km}_{szd} + \mu_d + \varepsilon_{iszd} \quad (2.7) \end{aligned}$$

where $Y_{iszd}$ is the payment experience outcome for employee $i$ from school $s$ located in registration zone $z$ and district $d$, $\#EE\ Neighbors^{2km}_{szd}$ and $\#EL\ Neighbors^{2km}_{szd}$ are the number of neighboring schools within a two-kilometer radius from school $s$ located in EE and EL treatment zones, $\#Total\ Neighbors^{2km}_{szd}$ is the total number of neighboring schools within the same radius. The main identifying assumption is that, conditional on the total number of neighboring schools within a fixed radius, the number of treated neighboring schools is random.

Appendix Table 2.A.6 presents the results. The estimates measuring treatment effects on payment experience in columns (2) and (3) remain similar to those reported in Table 2.1. Moreover, the estimates measuring externalities in columns (4) and (5) are close to zero and not statistically significant. This implies that there is no evidence of spatial externalities across schools located within a two-kilometer radius.

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# Chapter 3

# Community Governance and Local Economic Activity

**Abstract.**

Decentralization of local governments is one of the most popular governance reforms, actively promoted by major international donors. This reform aims to align government incentives through better information and accountability at the local level. This paper evaluates the effects of fiscal and administrative decentralization of local governments on economic activity in Ukraine. I exploit quasi-experimental variation in the timing of decentralization across communities to estimate causal effects on the universe of firms and self-employed individuals using difference-in-differences methods. I find that decentralization caused a steady reduction in the number of active self-employed individuals by up to 6%. These effects were concentrated in rural areas, for which decentralization was designed to catalyze economic development. There were, however, no changes in the total number of private firms, with only nominal growth in newly established enterprises. These results suggest that successfully applied governance reforms might have unintended economic consequences.

## 3.1 Introduction

Developed countries are significantly more decentralized than developing countries, having twice higher shares of subnational revenues and taxes in their budgets (Gadenne and

Singhal, 2014). International donors actively promote the decentralization of developing states, citing its benefits for the provision of public goods and services and the economy (World Bank, 2004; IMF, 2009; United Nations, 2009). However, the theoretical literature gives ambiguous predictions of its impacts. One strand of studies predicts positive effects of decentralization due to better knowledge of local preferences and needs by local governments (Tiebout, 1956), easier monitoring of local officials by citizens (Seabright, 1996), and competition between communities (Besley and Case, 1995). At the same time, other studies highlight possible adverse impacts due to lost economies of scale (Oates, 1972; Boffa, Piolatto and Ponzetto, 2016), local elite capture (Bardhan and Mookherjee, 2000), and rent-seeking (Cai and Treisman, 2004; Sonin, 2010).

This paper studies the effects of a major decentralization reform on local economic activity in Ukraine. It is a compelling setting for three reasons. First, it is one of the largest countries in Europe with a population of more than 42 million people, which remains relatively poor, with a GDP per capita of USD 3,725. The centralized structure was believed to be one of the core impediments to economic development, especially in remote areas. Second, it is an exemplary decentralization reform, the successful implementation of which was recognized by foreign governments and international organizations. The reform created new local units – territorial communities, which received substantial administrative, legislative, and fiscal powers. Finally, a staggered implementation of the reform allows estimation of its causal effects. In contrast, similar reforms in other countries often happen at once on the whole territory, which complicates the evaluation of their impacts.

To estimate the effects of decentralization, I assembled a dataset detailing the progress of the reform across Ukrainian settlements from 2015 to 2020. I also obtained registration records for the universe of self-employed individuals and firms from the official government registries. This data includes detailed information on 4.2 million self-employed individuals and 1.1 million firms that were active from 2011–2019. I combined the two datasets by matching physical addresses to settlements. I then computed the number of active individuals and firms in each settlement per year, my primary unit of analysis.

I exploit quasi-experimental variation in the timing of decentralization across the country to estimate its causal effects using difference-in-differences methods. I compare economic outcomes in settlements that went through decentralization in earlier years to those that went through it later. I first estimate dynamic effects using a two-way fixed effects model with settlement and year fixed effects. This model shows that both groups of settlements had parallel trends in the number of active self-employed individuals and firms before the reform. I also estimate average effects using a standard difference-in-differences model.

The empirical analysis produces five main results. First, decentralization led to a steady reduction in the number of active self-employed individuals, declining by almost 6% by the fifth year. On average, each settlement had 20.2 active self-employed individuals per 1,000 inhabitants, which went down by 0.5 individuals as a result of decentralization. I show that these effects are robust to a more demanding specification with region-specific linear time trends. They are also robust to Callaway and Sant'Anna's estimator, which provides unbiased estimates in case of heterogeneous effects across years or cohorts of settlements decentralized in different years.

Second, there were no significant changes in the number of active firms. On average, each settlement had 6.2 active firms per 1,000 inhabitants, and decentralization reduced this number by 0.05 firms. There was only slight growth in the number of active newly established enterprises. Moreover, this growth was at least 200 times smaller than the corresponding reduction in self-employment. Hence, the effects on self-employed individuals were not driven by changes in the organizational form of their businesses.

Third, the negative effects on self-employment were concentrated in rural areas, for which decentralization was originally designed to catalyze economic development. On average, rural settlements had 18.9 self-employed individuals, which went down by around 1.7% as a result of the reform. There were no significant effects in urban areas, perhaps because many urban centers obtained more powers before the reform.

Fourth, the impacts of decentralization were higher in regions with better economic indicators, such as regional employment rate and gross regional product. Settlements in areas above the median gross regional product experienced a significant 2.8% reduction in self-employment compared to an insignificant 0.5% decrease in other regions. This result might indicate that the reform potentially had worse consequences for better-performing individuals and businesses.

Fifth, I provide suggestive evidence that a reduction in self-employment was not driven by the clean-up of inactive registrations, changes in the organizational form, and higher tax rates. I also do not find significant differences in the number of active firm branches, a proxy for their economic growth. Instead, I see a higher reduction in self-employment in regions with higher perceived corruption and informality rates, possibly indicating decentralized corruption. Yet, assessing the precise impacts on economic indicators of firms and corruption levels remains an important area for future research.

This paper contributes to the growing literature studying decentralization of local governments (reviewed in Gadenne and Singhal, 2014 and Mookherjee, 2015). Despite the vast theoretical literature, there is little rigorous empirical evidence. Since decentralization reforms often happen at once in the whole country, prior studies have mostly relied on cross-country comparisons (Fisman and Gatti, 2002; Treisman, 2002; Fan, Lin and Treisman, 2009). My paper contributes to a few recent studies that provide causal micro estimates of the impacts of horizontal decentralization, which changes the size and number of local units, on public goods provision (Dahis and Szerman, 2021; Weaver, 2021) and environmental externalities (Burgess et al., 2012; Lipscomb and Mobarak, 2016). In contrast, I study a full-scale vertical decentralization, which creates new local units and gives them administrative, legislative, and fiscal powers. In particular, my findings provide new insights on whether decentralization promotes local economic activity.[1]

My results also contribute to the literature studying the possible adverse effects of

[1]Dahis and Szerman (2021) find that district splitting in Brazil did not affect the number of private enterprises and jobs.

decentralization. First, they contribute to the studies highlighting heterogeneous effects of decentralization, which primarily benefits prosperous communities and leaves behind the poor who need it the most (Galiani, Gertler and Schargrodsky, 2008; Bardhan and Mookherjee, 2006). Similarly, I find that the negative effects on economic activity are concentrated in rural areas with lower economic activity and development levels. Second, my paper relates to the studies looking at the relationship between decentralization and economic activity, which found evidence of protectionism and rent-seeking in Russia (Slinko, Yakovlev and Zhuravskaya, 2005; Ponomareva and Zhuravskaya, 2004) and China (Young, 2000; Poncet, 2005). I show that the detrimental impacts are larger in regions with higher levels of corruption and informal activity, possibly indicating similar mechanisms.

The remainder of the paper proceeds as follows. Section 3.2 provides background on the Ukrainian economy and the decentralization reform. Section 3.3 describes data sources and introduces the primary empirical strategy. Section 3.4 presents results, and Section 3.5 discusses mechanisms. Section 3.6 concludes.

## 3.2 Background

This section provides an overview of the Ukrainian economy, describes the main changes brought by decentralization reform, and discusses possible effects on economic activity.

### 3.2.1 Ukrainian Economy

Ukraine is one of the largest countries in Europe, with a population of 42 million people. Yet, it is also one of the poorest countries on the continent, with a GDP per capita of $3,725 in 2020. The Ukrainian economy is characterized by high unemployment and informality rates, which are in part driven by high levels of corruption. Only 16 million (65%) of 24.4 million working-age Ukrainians are officially employed, and at least 3.2 million work informally. Around 2.5 million, 16% of officially employed, are self-employed and individual entrepreneurs.

The Ukrainian economy has suffered a significant dip as a result of the Euromaidan Revolution, the annexation of Crimea, and the start of the war in the Donbas region in 2014–15.

Ukraine lost 6.6% of its GDP in 2014 and 9.8% more in 2015. However, a series of successful reforms, including decentralization, reversed this trend, and the economy grew again at the rate of 2.4–3.5% in 2016–2019.

### 3.2.2 Decentralization Reform

As reminiscent of the Soviet Union, Ukraine remained highly centralized before the 2015 decentralization reform. Local governments had only nominal powers of collecting taxes and proposing their budgets. However, all decision-making happened at the upper levels of the government, which approved local budgets and distributed transfers to local governments. This centralized structure was considered one of the main impediments to economic development, especially in remote areas.

Appendix Figure 3.A.1 summarizes main changes brought by the reform. Before the reform, regions (oblasts) were split into districts (rayons), which were further divided into municipalities (radas). Municipalities served the function of local governments by collecting taxes, proposing budgets, and executing projects, but they did not have any executive powers. Districts were responsible for local services, but they were also controlled by and dependent on regional and state resources.

After the reform, regions were still split into districts, which were further divided into territorial communities (hromadas).[2] Newly created communities consist of voluntary-united settlements or municipalities. They often combine multiple municipalities, which provides the financial sustainability of local units by increasing their size.

Territorial communities are self-governed local bodies that form their budgets and keep a significant share of taxes. In particular, they keep 100% of property tax, 100% of single tax (primarily paid by self-employed individuals), 60% of personal income tax, 100% of income tax for community entities, 100% of payments for administrative services, 25% of environmental tax, and 5% of excise tax (levied on tobacco, alcohol, and petroleum products). Communities

[2]The next phase of the reform reduced the number of districts by amalgamating them into bigger units. This phase started in 2020 and is not being evaluated in this paper.

are now also responsible for local services, including education, medicine, sports, culture, social protection, and administrative services, previously held under the district authority.

### 3.2.3 Decentralization and Economic Activity

Decentralization has been considered one of the most successful reforms in Ukraine. It received praise externally among foreign governments and internally among citizens. As of 2021, 60% of citizens continued to believe that Ukraine needs decentralization of local governments (KIIS, 2021). Panel A of Figure 3.A.2 shows the high recognition of the reform in the resolution of the European parliament. Panel B shows examples of how newly-created communities help local entrepreneurs establish and grow their businesses. They organize co-working spaces and hubs where entrepreneurs can help each other and share ideas, help residents self-organize and establish cooperatives, and create favorable conditions for local businesses.

On the one hand, decentralization might indeed boost local economic activity. First, local governments might better know and fulfill the preferences and needs of the local population, including local businesses (Tiebout, 1956). Second, they might be more accountable to citizens because it is easier to monitor politicians at the local level (Seabright, 1996). Third, competition between local governments might incentivize them to provide favorable tax and investment conditions to attract local businesses (Besley and Case, 1995).

On the other hand, decentralization might also negatively affect economic activity. First, communities have fiscal incentives to increase tax rates and improve tax compliance since tax collection is the primary source of revenues used to finance local public goods (Gadenne and Singhal, 2014).[3] Second, decentralization might cause decentralized corruption, given the creation of additional government layers and the overall high level of corruption (Shleifer and Vishny, 1993).[4] Similarly, it might cause elite capture and rent-seeking, resulting in protectionism

[3] Communities have the authority to establish tax rates within the nationally set limits for the following taxes: single tax for self-employed individuals and small businesses, land tax, real estate tax, and tourism tax.

[4] Ukraine was ranked 142nd in 2014 (122nd in 2021) out of 180 countries in the Transparency International Corruption Perceptions Index.

and better conditions for a few (usually larger) businesses that support local politicians or their parties (Cai and Treisman, 2004, 2005; Sonin, 2010).

Overall, the net impacts of decentralization are ambiguous and depend on the relative magnitudes of these effects. The following sections empirically estimate the net effects of the decentralization reform on the self-employed and firms in Ukraine.

## 3.3 Data and Empirical Strategy

This section starts with a description of data sources. It then outlines the primary empirical strategy used to estimate the effects of decentralization on self-employed individuals and firms and discusses identification assumptions.

### 3.3.1 Data

**Decentralization Details.** The detailed information on decentralization was web-scraped in November 2020 from the website decentralization.gov.ua maintained by the Ukrainian government. This information contains names of the new communities (hromadas), dates when they were created, names and codes (koatuu) of settlements that were amalgamated into them, and the corresponding names of municipalities (radas) and districts (rayons) they were part of before. I matched this data to the official subnational administrative boundaries to obtain population counts and geographical coordinates.

**Registry of Self-Employed Individuals.** This registry was web-scraped in January 2021 from the website data.gov.ua maintained by the Ukrainian government. This data contains registration numbers and names of the universe of self-employed individuals (known as "individual entrepreneurs" in Ukraine), their physical address, contacts, current status, 6-digit industry codes and names, and registration and termination dates. The studied sample is restricted to self-employed individuals who were active from 2011–2020 and who were located in the regions fully controlled by the Ukrainian government.[5] All self-employed individuals were matched to

[5] I exclude from the analysis the following regions: Crimea, Sevastopol, Luhansk and Donetsk regions (oblasts).

the corresponding settlement and municipality using their physical addresses.[6] I also identified whether the individual is female or male based on the patronymic name.[7]

**Registry of Firms, Public Offices, and Non-Profit Organizations.** This registry was web-scraped in January 2021 from the website data.gov.ua maintained by the Ukrainian government. This data contains the following information for the universe of firms, public offices, and non-profit organizations: registration and identification numbers, name of the organization, physical address, contacts, legal form, current status, 6-digit industry codes and names, registration and termination dates, names of founders, managers, signers, and beneficiaries, initial capital, and similar information for all branches. Similar to the self-employed, I restrict the sample to firms active from 2011–2020 and located in the regions controlled by the Ukrainian government. I also match their location to the settlement and municipality using the same procedure.

**Regional Statistics.** These statistics were obtained from the website ukrstat.gov.ua of the State Statistics Service of Ukraine. They include the following outcomes: 2010 employment rate, 2010 gross regional product, and 2017 rate of informal activity. I also obtained 2011 regional corruption perceptions indices from the Kyiv International Institute of Sociology (KIIS).

### 3.3.2 Empirical Strategy

To estimate the causal effects of decentralization of local governments, I exploit staggered implementation of the reform across settlements. Appendix Figure 3.A.3 shows the reform progress in 2015–2019 before it was fully completed in 2020. This allows me to estimate the effects by comparing settlements that went through decentralization in earlier years and those that went through it later. Specifically, I estimate the following dynamic difference-in-differences model:

[6] I matched settlements using settlement, municipality, and district names for unique cases and using geocoordinates based on addresses for non-unique cases or cases with missing municipality or district names in the address field.

[7] In the Ukrainian language, most females' patronymic names end with a suffix "vna", while males' patronymic names end with a suffix "vich".

$$Y_{srt} = \alpha_s + \gamma_t + \sum_{\tau != -1} \beta_\tau Decentralized_{sr\tau} + \varepsilon_{srt} \tag{3.1}$$

where $Y_{srt}$ is the outcome variable in settlement $s$ from municipality $m$ in year $t$; $\alpha_s$ are settlement fixed effects, which control for fixed characteristics of settlements across years; $\gamma_t$ are calendar year fixed effects, which control for common shocks within each year; $Decentralized_{sr\tau}$ are dummy variables equal to one if the settlement $s$ from municipality $m$ got decentralized $\tau$ years ago relative to year $t$, where $\tau$ ranges from –6 to +4. Standard errors are clustered at the municipality level since settlements from the same municipality were frequently amalgamated into the same community.

To estimate average treatment effects across years, I use the following difference-in-differences specification:

$$Y_{smt} = \alpha_s + \gamma_t + \beta_t Decentralized_{smt} + \varepsilon_{smt} \tag{3.2}$$

This empirical strategy relies on the three identification assumptions. First, outcomes should follow parallel trends in the absence of decentralization reform. While this assumption cannot be tested directly, an indirect test checks for parallel trends in the outcomes before the reform. I test it by estimating the coefficients $\beta_\tau$ where $\tau < 0$ from specification (3.1).

Second, there should not be any anticipatory responses to the reform by municipalities, self-employed individuals, or firms. Before decentralization, municipalities were severely constrained in their activity since budgets had to be approved by upper government levels. Hence, it is unlikely that they had the resources to prepare for decentralization before new communities started to operate. Similarly, when the reform began in 2015, it was hard for self-employed individuals and firms to predict which year their settlement would be decentralized. There were no clear implications of the reform for their economic activity since newly elected community officials could have chosen a policy that supports or hurts local businesses.

Third, there should be homogeneous treatment effects across years and cohorts of settlements decentralized in different years (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). I addresses a possible violation of this assumption by employing Callaway and Sant'Anna's estimator, which is robust to the heterogeneity of treatment effects along these dimensions.

## 3.4 Results

This section presents the effects of decentralization on self-employed individuals and firms using difference-in-differences models. It also shows heterogeneous effects by urban-rural settlement status and regional economic indicators.

### 3.4.1 Effects on Self-Employed

Figure 3.1 presents the causal effects of decentralization on self-employed individuals based on the dynamic difference-in-differences model specified in Equation (3.1). Panel A shows the impact on the number of active self-employed individuals per 1,000 inhabitants. Before the reform, communities that decentralized in earlier years had similar outcome trends to those that decentralized later. The point estimates $\beta_\tau$ for $\tau < 0$ are close to zero and not statistically significant at the 5% level. This evidence provides support for the parallel trends assumption.

After the reform, decentralized communities had approximately 0.5 fewer self-employed individuals per 1,000 inhabitants in the first two years. This number further increases to around 1.7 fewer self-employed individuals by the fifth year. All point estimates $\beta_\tau$ for $\tau \geq 0$ are statistically significant at the 1% level.

Panel B shows the effects on the inverse hyperbolic sine (Asinh) transformation of the number of self-employed, which is approximately equivalent to a percent change. Before the reform, there were no statistically significant differences between communities that decentralized in earlier and later years. After the reform, decentralized communities experienced an approximately one percent decrease in the number of self-employed in the first two years. This number increases to almost six percent by the fifth year.

Table 3.1 displays the average treatment effects across years based on the traditional difference-in-differences model specified in Equation (2). Column (1) shows that decentralization led to 0.54 fewer self-employed (std. error = 0.15), given an average of 20.2 active individuals per 1,000 inhabitants. Column (2) shows that this effect is approximately equivalent to a 1.7% reduction (std. error = 0.5) over five years.

Overall, these results suggest that decentralization caused a steady reduction in the number of active self-employed individuals, which declined by almost six percent by the end of the fifth year.

### 3.4.2 Effects on Firms

Figure 3.2 shows the corresponding effects of decentralization on firms. Before the reform, there were similar trends in the number of active firms between communities that decentralized earlier and those that did it later. The point estimates $\beta_\tau$ for $\tau < 0$ are close to zero and not statistically significant at the 5% level.

After the reform, there are no statistically significant effects of decentralization on the number of firms per 1,000 inhabitants and the corresponding percent changes. The point estimates $\beta_\tau$ for $\tau \geq 0$ are slightly negative but not statistically significant at the 5% level. Similarly, Columns (3)–(4) of Table 3.1 show that the average effects on firms are close to zero and not statistically significant.

To summarize, decentralization did not lead to significant changes in the number of active firms. However, these results do not reveal whether existing firms experienced internal economic growth.

### 3.4.3 Robustness Checks

Appendix Figure 3.A.4 shows the robustness of the results to excluding one region at a time. The estimates for self-employed remain stable at the baseline level of 1.7% and significant at the 5% level. The estimates for firms are close to zero and not statistically significant. Hence, the effects are not driven by a single region.

Appendix Table 3.A.1 displays the robustness of the effects to a more demanding specification with region-specific linear time trends. The effects on self-employed have a slightly smaller magnitude but remain statistically significant at the 5% level. The impact on firms remains close to zero and not statistically significant at conventional confidence levels.

Appendix Figure 3.A.5 presents the effects on self-employed using Callaway and Sant'Anna's estimator, which provides unbiased estimates in case of heterogeneous effects across years and cohorts of settlements decentralized in different years. Panel A shows the robustness of dynamic effects to this estimator. Panel B shows that the impacts on self-employed are the largest for the first cohort of settlements decentralized in 2015.

Panel A of Appendix Figure 3.A.6 shows the robustness of the dynamic effects on firms to Callaway and Sant'Anna's estimator. Similar to self-employed, Panel B shows a slight decrease in active firms for the first cohort of settlements decentralized in 2015. Appendix Table 3.1 presents the corresponding average treatment effects.

### 3.4.4 Heterogeneous Effects

**Urban–Rural Status.** Figure 3.3 presents the heterogeneous effects of decentralization by urban–rural settlement status. Panel A shows that the negative effects on self-employment are concentrated in rural settlements. The effects in urban settlements are close to zero and not statistically significant at the 5% confidence level. Panel B shows no significant effects on the number of active firms in either rural or urban settlements.

Table 3.2 quantifies the average effects. Column (1) shows that decentralization led to an approximately 1.7% reduction (std. error = 0.5) in self-employment in rural settlements over five years. Column (2) shows a null effect of the number of active firms.

Overall, decentralization failed to increase the number of active self-employed individuals and firms in rural areas, for which it was designed to boost economic development. It also did not lead to changes in urban areas, possibly because local governments in big cities obtained more powers before the reform.

**Economic Indicators.** Figure 3.4 displays the heterogeneous effects of decentralization by the 2010 regional employment rate. Panel A shows that a decrease in self-employment mainly happened in regions above the median employment rate. The effects are close to zero and not statistically significant in regions below the median employment rate. Similarly, Panel B shows a slight decrease in active firms in regions with a higher employment rate.

Column (4) of Table 3.2 displays that decentralization led to an around 2.4% decrease (std. error = 0.6) in self-employment and 0.8% reduction (std. error = 0.3) in the activity of firms in regions with above the median employment rate. Similarly, Columns (5)–(6) show the effects of approximately the same magnitudes for above and below the median gross regional product.

All in all, decentralization caused a reduction in the number of active self-employed and firms in regions with better economic indicators. It might indicate that the reform had worse consequences for better-performing individuals and businesses. It might also signal that a part of this reduction translated into higher informal economic activity, given its high nationwide rate of more than 20%.[8]

**Economic Sectors.** Columns (1)–(6) of Table 3.A.3 show that decentralization reduced the economic activity of self-employed individuals in several popular sectors, including commerce, ICT, and transportation. Table 3.A.4 displays that the industrial sector also experienced a slight decline of 0.4% in the number of active firms. There were no significant changes in firms' activity in other popular sectors, including commerce, construction, professional activity, and agriculture. A decrease in industrial production was compensated by a corresponding increase in less popular sectors.

**Gender.** Columns (7)–(8) of Table 3.A.3 show that both female and male self-employed individuals experienced similar negative effects on their activity. Approximately 1.5% (std. error = 0.5) of male and 1.7% (std. error = 0.5) of female self-employed individuals left their activity as a result of decentralization.

[8] The rate of participation in informal activity reaches almost 50% in some regions of Ukraine.

## 3.5 Mechanisms

Section IV shows that decentralization led to a decrease in the number of active self-employed individuals and no significant changes in the number of active firms. This section provides suggestive evidence that a reduction in self-employment was not driven by the clean-up of registration records, higher tax rates, or changes in the organizational form. It also discusses alternative mechanisms, such as the internal growth of existing firms and decentralized corruption.

### 3.5.1 Clean-Up of Registration Records

Decentralization reform gave administrative powers over the management of registration records to the newly created territorial communities. They could have conducted the verification of records and a clean-up of inactive registrations, which resulted in fewer active self-employed individuals.

Panel A of Figure 3.5 tests this mechanism by splitting self-employed by their registration status. It shows a steady reduction in the number of existing self-employed individuals, which is inconsistent with a one-time clean-up of the records. It also displays a corresponding decrease in new registrations of around 2 percent per year, which indicates a structural change in the incentives for self-employed. Moreover, Table 3.3 shows a relatively higher average decrease of 2.3% (std. error = 0.5) of new registrations compared to a 1.4% (std. error = 0.5) reduction of existing registrations. All in all, these effects are inconsistent with the clean-up of registration records by newly-created local governments.

### 3.5.2 Changes in the Organizational Form

Self-employed individuals could have also changed their organizational form for a small firm without significantly altering their economic activity. It could have been due to more favorable conditions for firms, such as an easier registration process, lower taxes, or fewer restrictions on their activity.

Panel B of Figure 3.5 indicates no changes in the number of existing firms and a slight increase in new registrations of around 1 percent in years 1–3. However, these effects are relatively small compared to a reduction in self-employment. On average, settlements register approximately 0.25 new firms per 1,000 inhabitants each year, and decentralization increased this number by 0.6%. At the same time, each settlement has around 20.2 self-employed per 1,000 inhabitants, and decentralization reduced this number by 1.7%. Thus the effects of decentralization on new firm registrations are at least 200 times smaller than a corresponding decrease in self-employment.

Hence, the results cannot be explained by changes in the organizational form. Indeed, owners of small businesses have more favorable conditions if they operate as self-employed, including a simplified registration process, lower taxes, and relatively high limits on the annual income and number of employees.

### 3.5.3 Tax Rates

Higher tax rates could have also lowered incentives to engage in self-employment. The majority of self-employed qualify for the simplified tax procedure and pay a single tax, while the non-qualified individuals pay a uniform personal income tax. Decentralization empowered communities to set a single tax rate within the nationally set limits and keep 100% of the revenues in the local budget.

There are three main groups of self-employed based on the total income, industry, and the maximum number of employees.[9] Group 1 has the lowest income limit of around one mln hryvnias, cannot hire employees, and pays 0–10% of the living wage in taxes. Group 2 has a higher income bar of around five mln hryvnias, can hire up to 10 employees, and pays 0–20% of the minimum wage in taxes. Group 3 has an even higher income limit of 7.5 mln hryvnias, can hire an unlimited number of employees, and pays either 3% of income + VAT or 5% of income

[9] There is also a fourth group for farmers and agricultural produces who face fixed single tax rates based on the type of land.

in taxes. These tax rates are kept low to incentivize entrepreneurship and reduce unemployment rates.

The government started to publish the registry of single taxpayers in 2019. This registry includes information on tax rates for all self-employed individuals who use the simplified tax procedure. I obtained this data for December 2019 and matched it to the registry of all self-employed individuals. I estimate that around 85% of self-employed individuals pay a single tax, while the rest pay a standard flat income tax. I compute the single tax rate for each group in all settlements and estimate the difference between decentralized and non-decentralized communities using the following model:

$$\text{Tax Rate}_{smd}^{2019} = \alpha + \beta \text{Decentralized}_{smd} + \varepsilon_{smd} \tag{3.3}$$

Table 3.4 present the results. There are no significant differences in single tax rates for Groups 1 and 3 between decentralized and non-decentralized settlements. Settlements in decentralized communities have a 0.1 percentage point lower (std. error = 0.06) single tax rate for Group 2. This difference is small and favorable for self-employed individuals. The average tax rates also indicate that most communities set the maximum tax rates for all groups.

Overall, a reduction in self-employment is not driven by higher tax rates. Most self-employed individuals pay a simplified single tax, set to approximately the same level in all settlements and significantly lower than the standard income tax rate.

### 3.5.4 Alternative Mechanisms

**Growth of Existing Firms.** Even though there was no growth in the total number of active firms, decentralization could have caused internal growth of existing enterprises. It could have created more jobs, which were more attractive to self-employed individuals than their economic activity.

The official registry of firms does not provide information on the number of employees and economic indicators like revenues. However, it has detailed records on all active branches, which can be used as a proxy for their economic growth. When firms grow and become more profitable, they often expand their business by opening new branches in another part of the same community or even in a different community.

Appendix Table 3.A.5 shows that decentralization does not lead to a growth in the number of active branches. Column (1) shows a slightly negative but insignificant impact on the number of firms with at least one active branch. Columns (2)–(3) show null effects on the average number of branches per firm.

These results provide suggestive evidence that decentralization did not result in higher internal growth of firms. Yet, more research is needed to estimate the effects on employment and economic indicators.

**Decentralized Corruption.** Decentralization could have also led to decentralized corruption because it created an additional layer of local governance responsible for registration and taxation. If it increased the value of bribes self-employed and firms need to pay to continue their activity, some could have left the formal sector.

Appendix Table 3.A.6 provides suggestive evidence that self-employed were possibly switching to informal activity. Columns (1)–(2) display that a reduction in self-employment was concentrated in regions with above the median values of the 2010 regional corruption perceptions index. Regions with a higher corruption level had a 2.9% (std. error = 0.7) decrease in self-employment, while other regions had only an insignificant 0.7% (std. error = 0.6) reduction.

Similarly, Columns (3)–(4) show that the effects of decentralization on self-employed individuals were larger in regions with above the median 2017 regional level of informal activity.[10] While 2% of self-employed (std. error = 0.6) terminated their activity in regions with high informality levels, only 0.9% (std. error = 0.7) did it in regions with low informality levels.

Overall, these results suggest that a reduction in self-employment could have been driven

[10]This data is not available for earlier years.

by higher corruption levels or other administrative hurdles.[11] An open avenue for future research is to evaluate the impact of decentralization on corruption and informal economic activity.

## 3.6 Conclusion

Decentralization remains one of the most popular governance reforms in developing countries. While growing evidence suggests that decentralization improves the provision of public goods and services, there is little rigorous evidence of its effects on economic growth. This paper estimates the causal effects of decentralization of local governments on economic activity by exploiting a staggered implementation of the reform in Ukraine.

My findings indicate that decentralization caused a steady reduction in the number of active self-employed individuals by up to 6%. These effects were concentrated in rural areas, for which decentralization was designed to catalyze economic development. At the same time, there was no significant impact on firms, with only slight growth in the number of newly-established enterprises.

Additional evidence suggests that a reduction in self-employment was not driven by the clean-up of inactive registrations, changes in the organizational form, or higher tax rates for self-employed. I discuss that alternative mechanisms might include decentralized corruption or an internal growth of firms and their workforce. An important area for future research is determining a precise mechanism and developing a policy to mitigate possible detrimental effects and boost the economic growth of both self-employed and firms.

Another avenue for future research is determining what types of self-employed left their activity and whether they remained unemployed, switched to the informal market, or joined the corporate workforce. This would help determine the net economic effects of decentralization and develop policies to assist these individuals.

These findings provide important policy implications for foreign governments and international organizations, such as the IMF, World Bank, and United Nations, which actively

[11] This is also reflected in a recent survey of Ukrainian citizens, which suggests that their primary expectation from decentralization reform remains a reduction in corruption (KIIS, 2021).

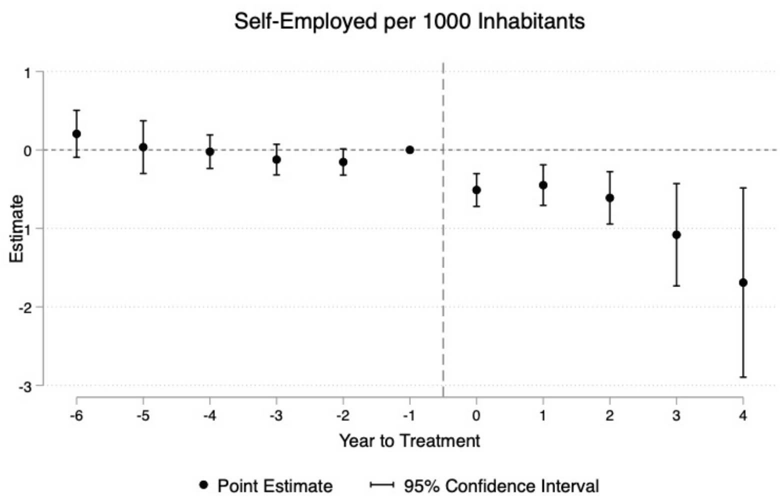
promote decentralization reforms worldwide. It is important to understand the underlying economic effects of these reforms since local officials often gain substantial powers to set local economic policies. It might also be helpful to train local officials on what policy tools they can use to promote local entrepreneurship and avoid unintended economic consequences.

## 3.7 Acknowledgments

Chapter 3, in part, is currently being prepared for submission for publication of the material. Faikina, Anastasiia. "Community Governance and Local Economic Activity." The dissertation author was the sole author of this material.

## 3.8 Figures and Tables

Panel A




Panel B

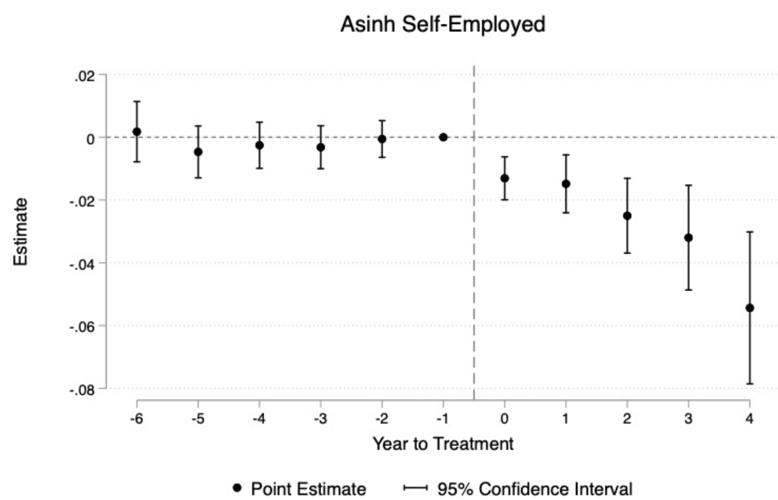
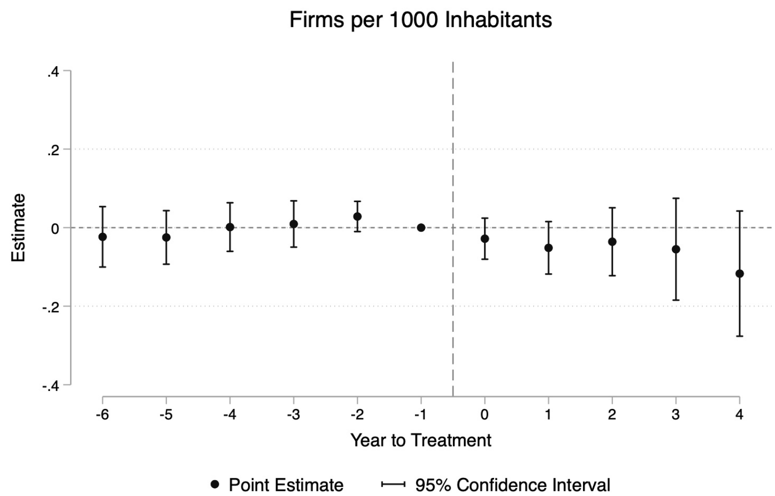



**Figure 3.1:** Effects on Self-Employed Individuals

*Notes:* This figure plots the effects of decentralization of local governments on the number of active self-employed individuals using the dynamic difference-in-differences model specified in equation (3.1). Panel A shows the effect on self-employed per 1,000 inhabitants, while Panel B shows the impact on the inverse hyperbolic sign (Asinh) of the outcome. The vertical dashed line corresponds to the decentralization year. Standard errors are clustered at the former municipality level.

Panel A




Panel B

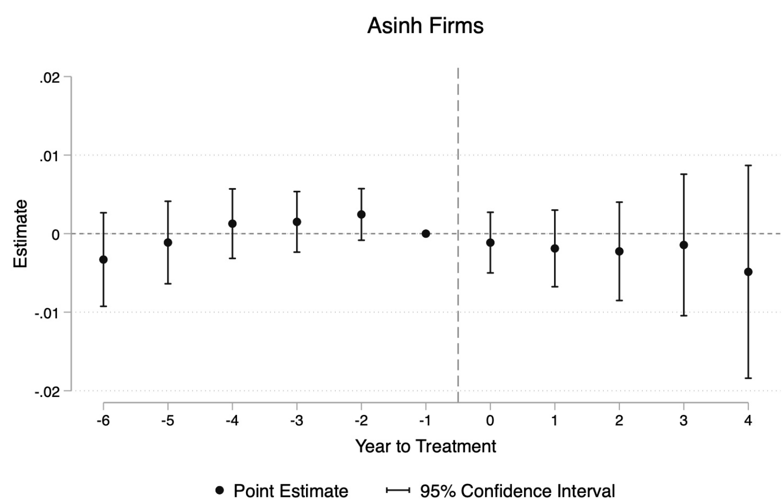
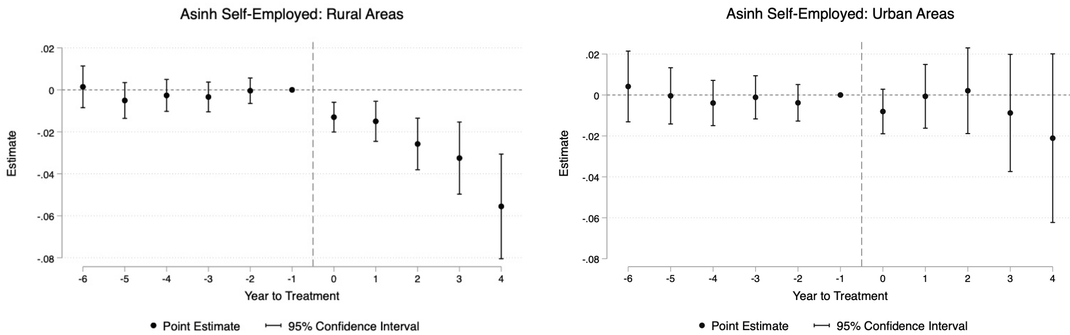



**Figure 3.2:** Effects on Firms

*Notes:* This figure plots the effects of decentralization of local governments on the number of active firms using the dynamic difference-in-differences model specified in equation (3.1). Panel A shows the effect on firms per 1,000 inhabitants, while Panel B shows the impact on the inverse hyperbolic sign (Asinh) of the outcome. The vertical dashed line corresponds to the decentralization year. Standard errors are clustered at the former municipality level.

Panel A: Self-Employed




Panel B: Firms

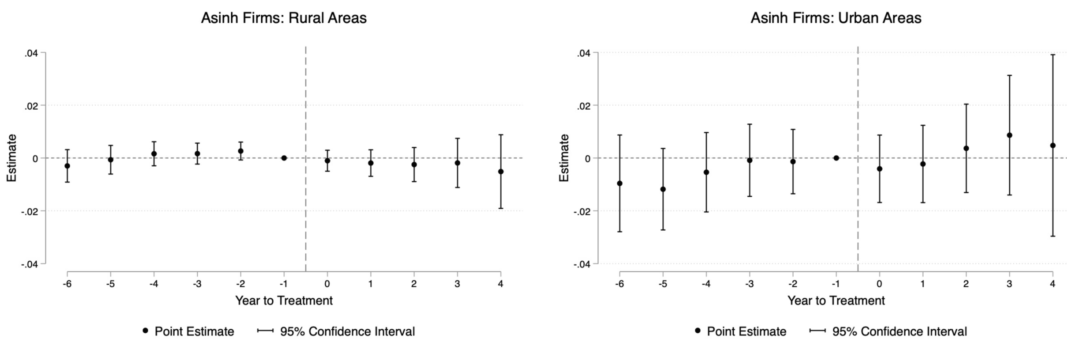
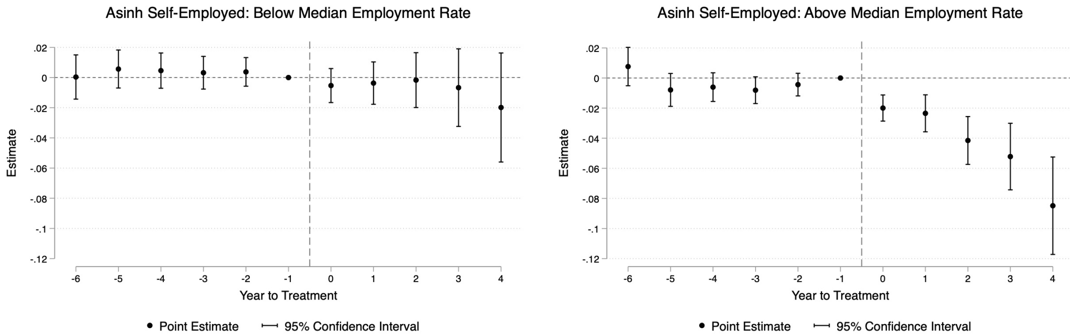



**Figure 3.3:** Heterogeneous Effects by Urban–Rural Settlement Status

*Notes:* This figure plots the heterogeneous effects of decentralization by urban–rural settlement status. The vertical dashed line corresponds to the decentralization year. Standard errors are clustered at the former municipality level.

Panel A: Self-Employed




Panel B: Firms

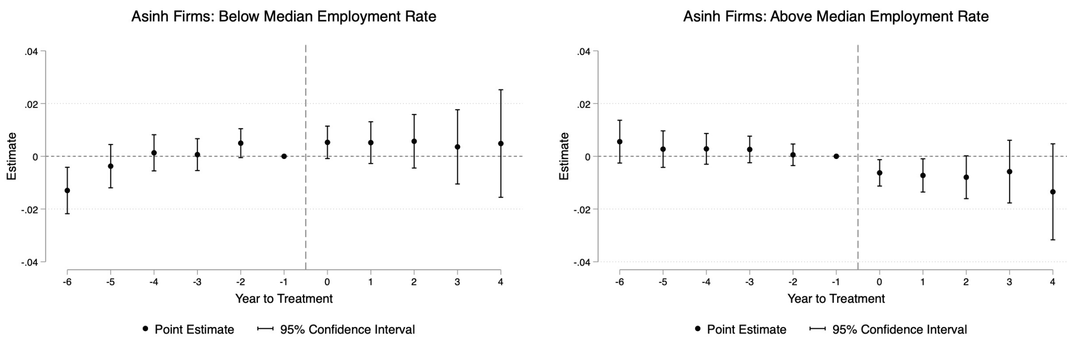
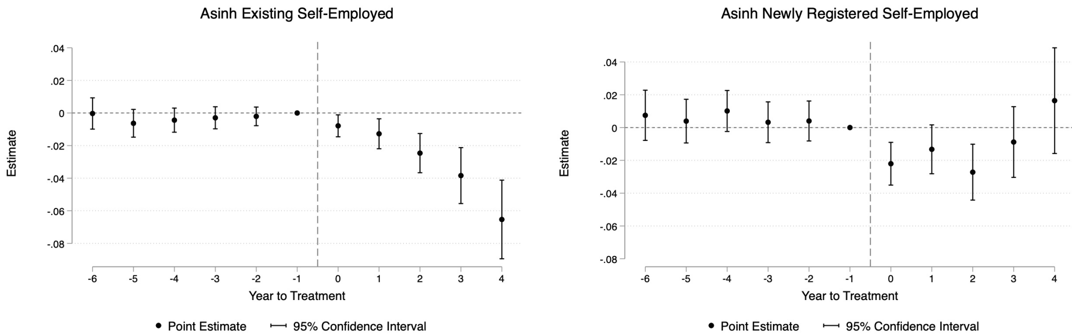



**Figure 3.4:** Heterogeneous Effects by Regional Employment Rate

*Notes:* This figure plots the heterogeneous effects of decentralization by the 2010 regional employment rate. The vertical dashed line corresponds to the decentralization year. Standard errors are clustered at the former municipality level.

Panel A: Self-Employed




Panel B: Firms

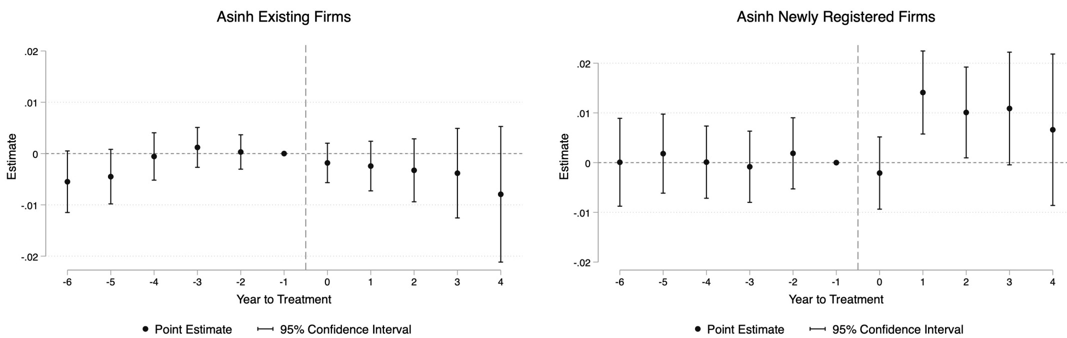



**Figure 3.5:** Mechanisms: Registration Status

*Notes:* This figure plots the heterogeneous effects of decentralization by registration status. Panel A shows the effects on self-employed, while Panel B shows the impacts on firms. The vertical dashed line corresponds to the decentralization year. Standard errors are clustered at the former municipality level.

**Table 3.1:** Average Effects on Self-Employed and Firms

| Outcome | Self-Employed | | Firms | |
|---|---|---|---|---|
| | Active per 1,000 | Asinh Active | Active per 1,000 | Asinh Active |
| | (1) | (2) | (3) | (4) |
| Decentralized (=1) | -0.536*** | -0.017*** | -0.045 | -0.002 |
| | [0.151] | [0.005] | [0.035] | [0.003] |
| Outcome Mean | 20.171 | 2.306 | 6.211 | 1.311 |
| | (33.753) | (1.765) | (11.331) | (1.408) |
| Year FEs | Yes | Yes | Yes | Yes |
| Settlement FEs | Yes | Yes | Yes | Yes |
| Observations | 237195 | 237195 | 237195 | 237195 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* This table shows the average effects of decentralization on the number of active self-employed individuals and firms at the settlement level. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

**Table 3.2:** Average Effects by Settlement Status and Economic Indicators

| Outcome | Settlement Status | | Regional Employment Rate | | Gross Regional Product | |
|---|---|---|---|---|---|---|
| | Rural | Urban | Low | High | Low | High |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Asinh Active Self-Employed* | | | | | | |
| Decentralized (=1) | -0.017*** | -0.003 | -0.008 | -0.024*** | -0.005 | -0.028*** |
| | [0.005] | [0.008] | [0.007] | [0.006] | [0.006] | [0.007] |
| Outcome Mean | 2.156 | 6.358 | 2.411 | 2.207 | 2.286 | 2.325 |
| | (1.575) | (1.804) | (1.800) | (1.726) | (1.731) | (1.796) |
| *Panel B: Asinh Active Firms* | | | | | | |
| Decentralized (=1) | -0.003 | 0.002 | 0.005 | -0.008** | 0.002 | -0.004 |
| | [0.003] | [0.006] | [0.004] | [0.003] | [0.004] | [0.004] |
| Outcome Mean | 1.184 | 4.764 | 1.293 | 1.329 | 1.213 | 1.404 |
| | (1.216) | (1.795) | (1.384) | (1.430) | (1.325) | (1.476) |
| Year FEs | Yes | Yes | Yes | Yes | Yes | Yes |
| Settlement FEs | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 228735 | 8460 | 115515 | 121680 | 115173 | 122022 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* This table shows the average effects of decentralization on the number of active self-employed individuals and firms by settlement status and economic indicators. The urban–rural status is based on the official administrative classification, where "misto" and "selyshche miskogo typu" are urban and "selo" and "selyshche" are rural. The low–high values correspond to the below–above median levels of the 2010 regional employment rate and gross regional product. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

**Table 3.3:** Mechanisms: Average Effects by Registration Status

| Outcome | Asinh Self-Employed | | Asinh Firms | |
|---|---|---|---|---|
| | Existing | Newly Registered | Existing | Newly Registered |
| | (1) | (2) | (3) | (4) |
| Decentralized (=1) | -0.014*** | -0.023*** | -0.002 | 0.006** |
| | [0.005] | [0.005] | [0.003] | [0.003] |
| Outcome Mean | 2.213 | 0.766 | 1.287 | 0.158 |
| | (1.746) | (1.118) | (1.396) | (0.536) |
| Year FEs | Yes | Yes | Yes | Yes |
| Settlement FEs | Yes | Yes | Yes | Yes |
| Observations | 237195 | 237195 | 237195 | 237195 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* This table shows the average effects of decentralization on the number of active self-employed individuals and firms by their registration status. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

**Table 3.4:** Mechanisms: Single Tax Rates for Self-Employed Individuals

| Outcome | Single Tax Rate | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Group 1 0–10% living wage | | | Group 2 0–20% minimum wage | | | Group 3 3% revenue+VAT or 5% revenue | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Decentralized (=1) | -0.024 | -0.001 | -0.002 | -0.509*** | -0.152** | -0.100* | -0.008 | 0.000 | -0.002 |
| | [0.015] | [0.016] | [0.015] | [0.067] | [0.065] | [0.059] | [0.007] | [0.005] | [0.006] |
| Outcome Mean | 9.855 | 9.855 | 9.855 | 18.303 | 18.303 | 18.303 | 4.969 | 4.969 | 4.969 |
| | (0.680) | (0.680) | (0.680) | (3.265) | (3.265) | (3.265) | (0.446) | (0.446) | (0.446) |
| Observations | 10194 | 10193 | 10076 | 16714 | 16713 | 16598 | 14657 | 14656 | 14541 |
| Region FEs | No | Yes | No | No | Yes | No | No | Yes | No |
| District FEs | No | No | Yes | No | No | Yes | No | No | Yes |

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

*Notes:* This table shows the relationship between decentralization and 2019 single tax rates at the settlement level. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

## 3.A Appendix: Additional Figures and Tables

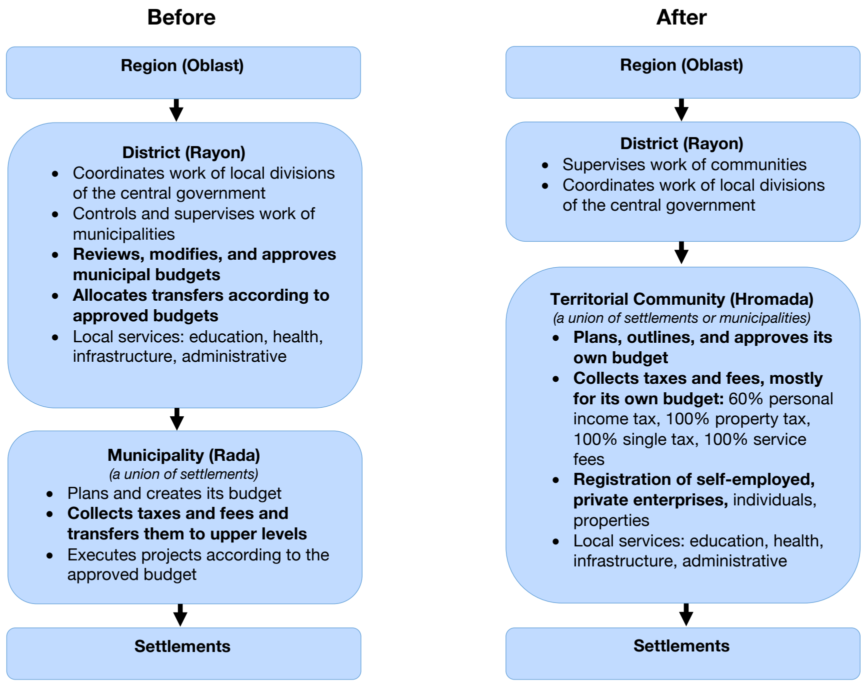



**Figure 3.A.1:** Decentralization Reform: Main Changes

*Notes:* This figure outlines changes in the administrative units and their responsibilities as a result of decentralization reform.

Panel A


Panel B

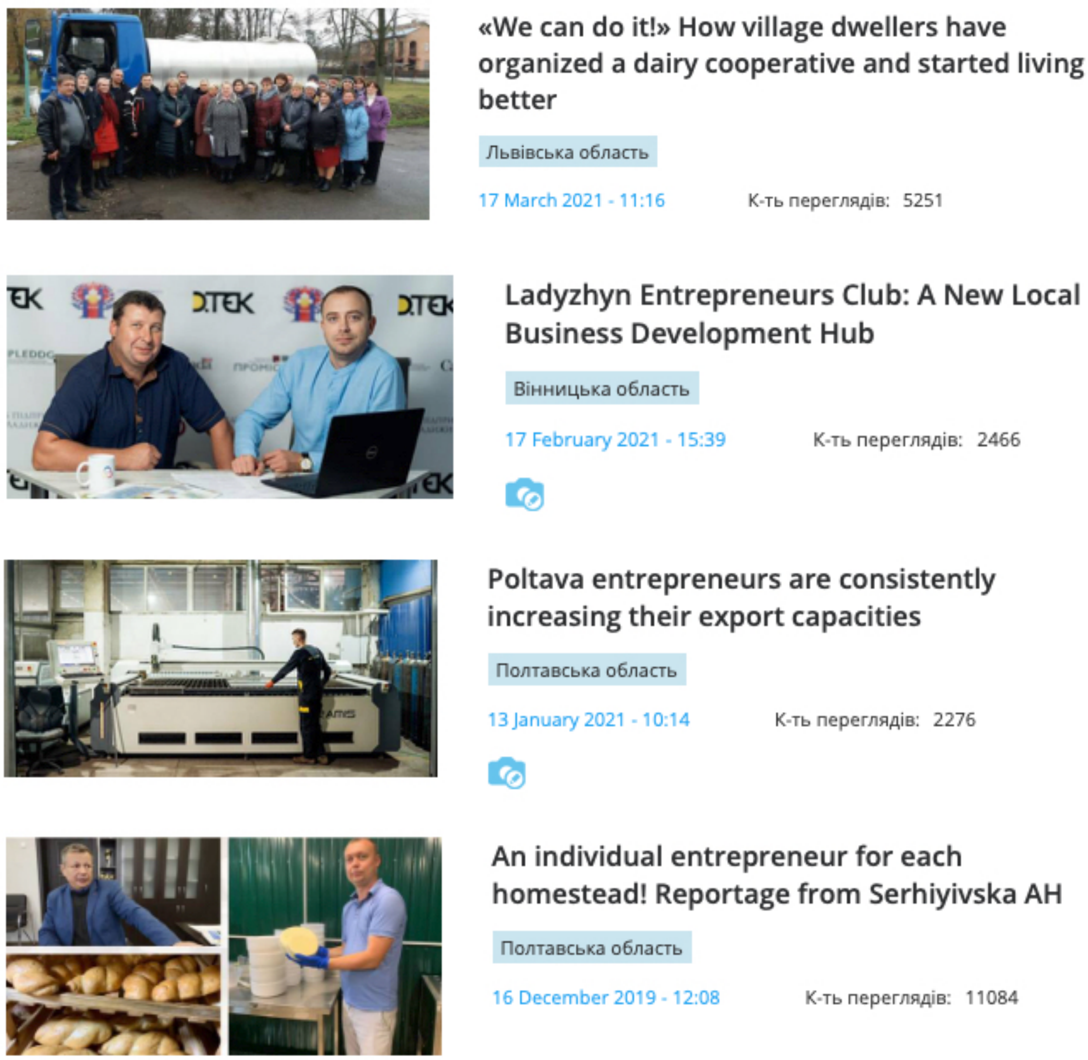



**Figure 3.A.2:** Decentralization Reform: Success Stories

*Notes:* This figure shows that decentralization is presented as one of the most successful reforms in Ukraine. Panel A displays its recognition by the European Parliament's resolution. The picture was taken from a Ukrainian media source ukrinform.net. Panel B presents success stories of local entrepreneurs taken from the official decentralization website decentralization.gov.ua.

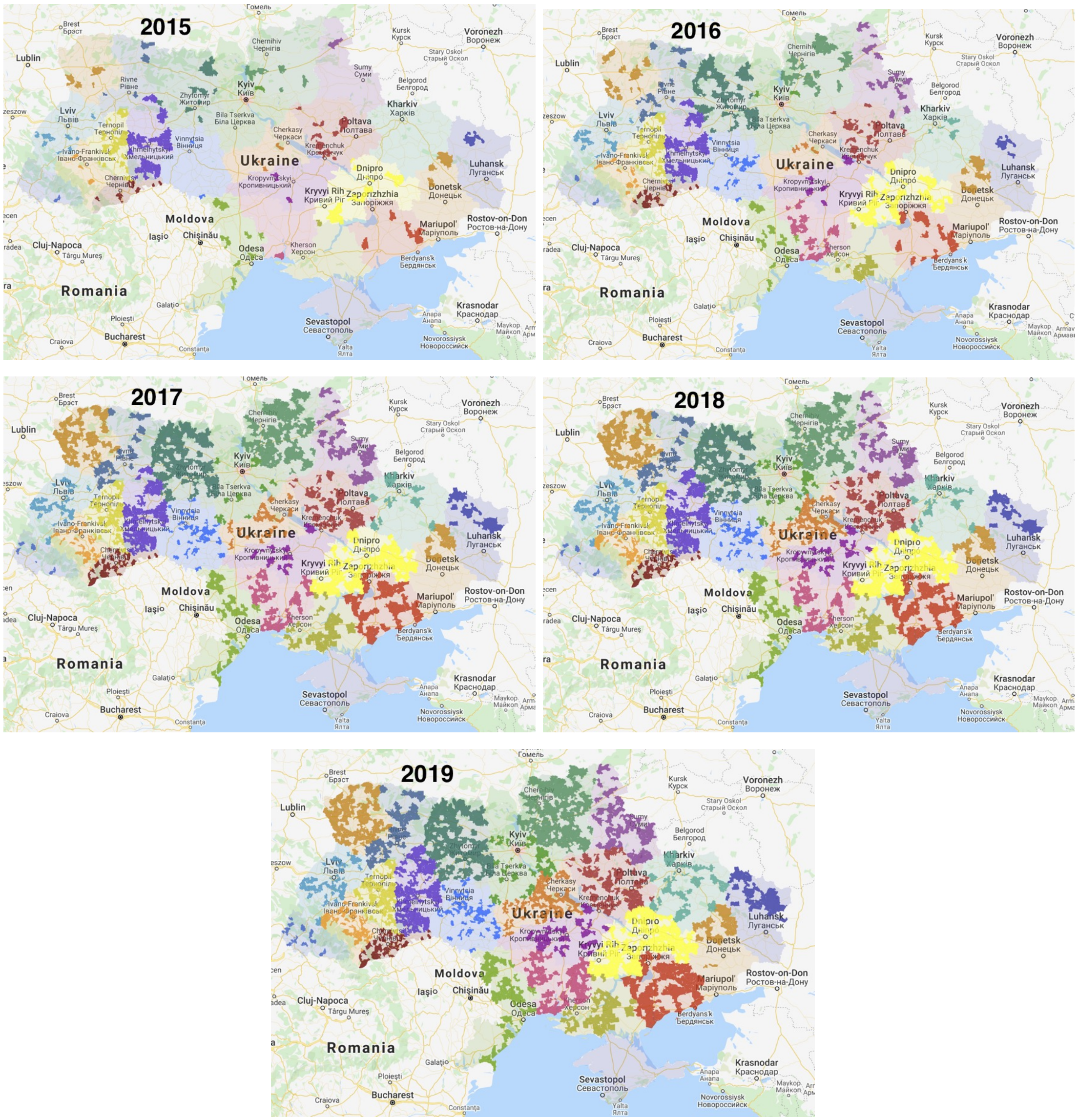
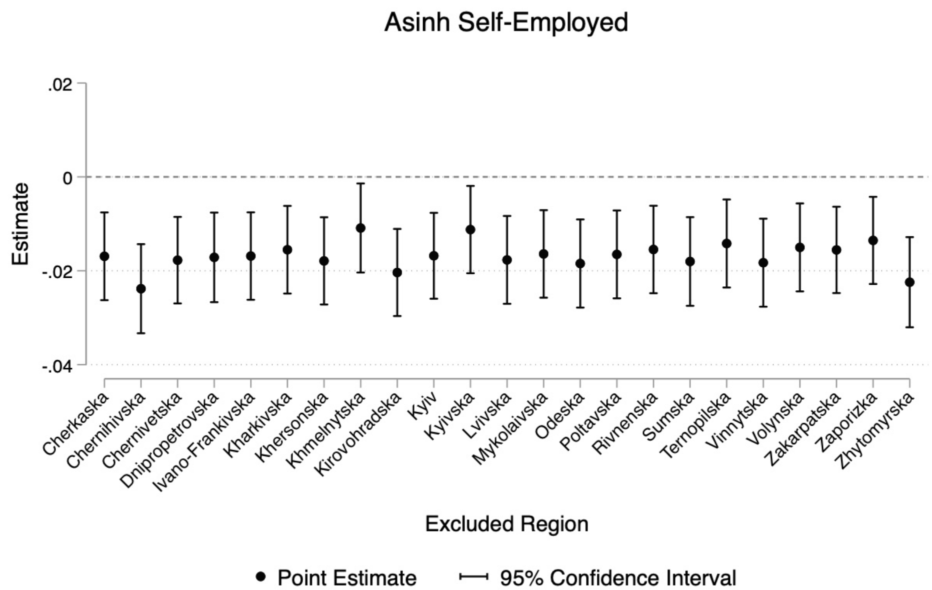



**Figure 3.A.3:** Decentralization Reform: Progress

*Notes:* This figure shows the progress of decentralization reform in 2015–2019 before being fully completed in 2020. Different colors denote regions of Ukraine, and darker colors highlight decentralized communities. The maps were taken from the official decentralization website decentralization.gov.ua in November 2020.

Panel A: Self-Employed




Panel B: Firms

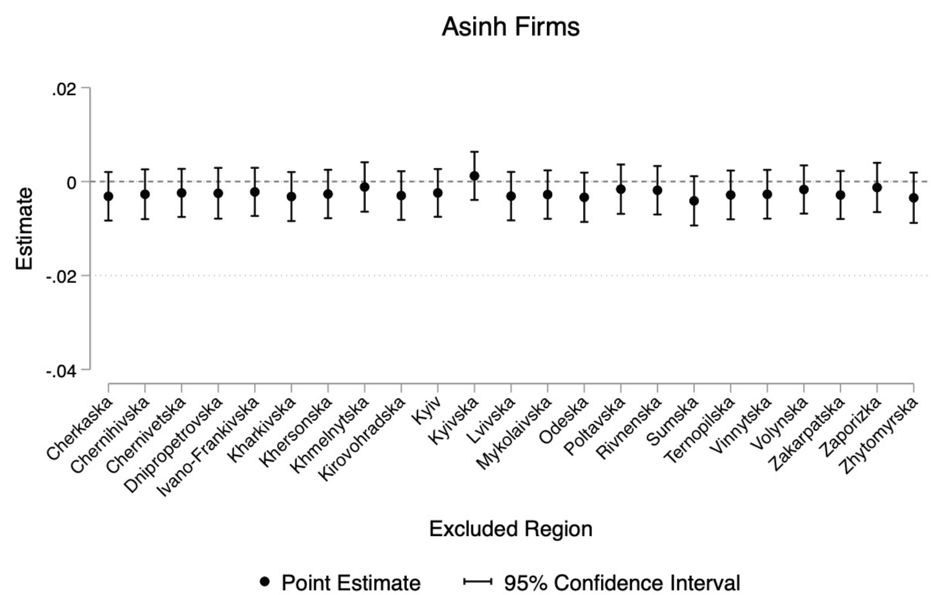
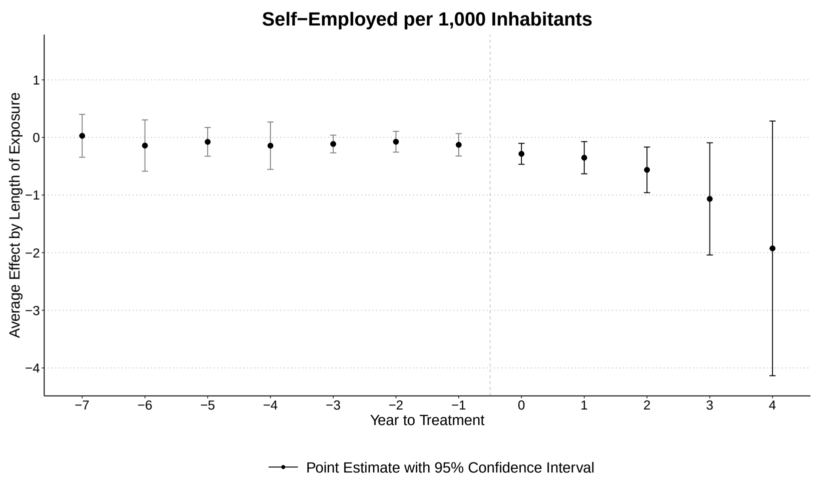



**Figure 3.A.4:** Average Effects Excluding One Region at a Time

*Notes:* This figure plots the average effects of decentralization, excluding one region at a time. Standard errors are clustered at the former municipality level.

Panel A: Dynamic Effects




Panel B: Group Effects

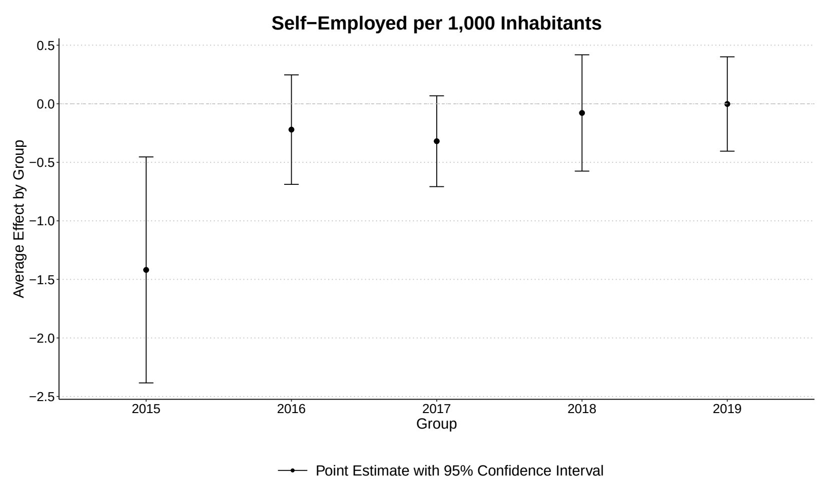
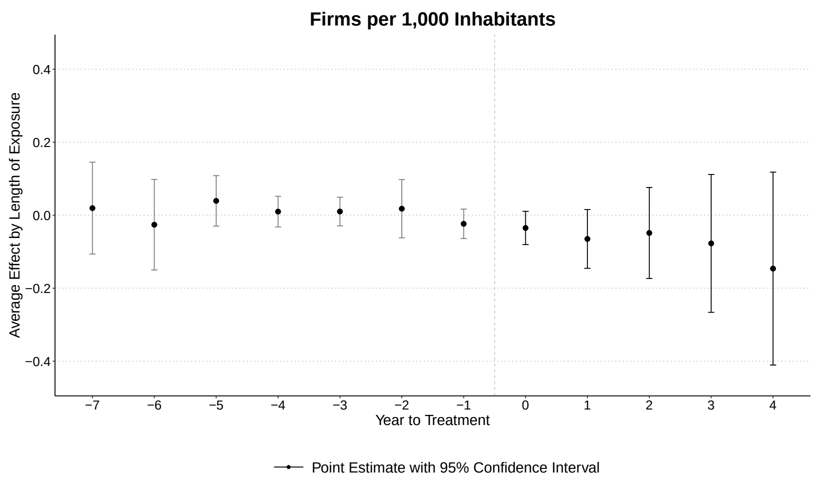



**Figure 3.A.5:** Effects on Self-Employed Individuals: Callaway and Sant'Anna's Estimator

*Notes:* This figure plots the effects of decentralization of local governments on the number of active self-employed individuals per 1,000 inhabitants using Callaway and Sant'Anna's estimator. Panel A shows the average treatment effects by the length of exposure to treatment, while Panel B shows the average group effects for each cohort of decentralized settlements. The vertical dashed line corresponds to the decentralization year. Standard errors are clustered at the former municipality level.

Panel A: Dynamic Effects




Panel B: Group Effects

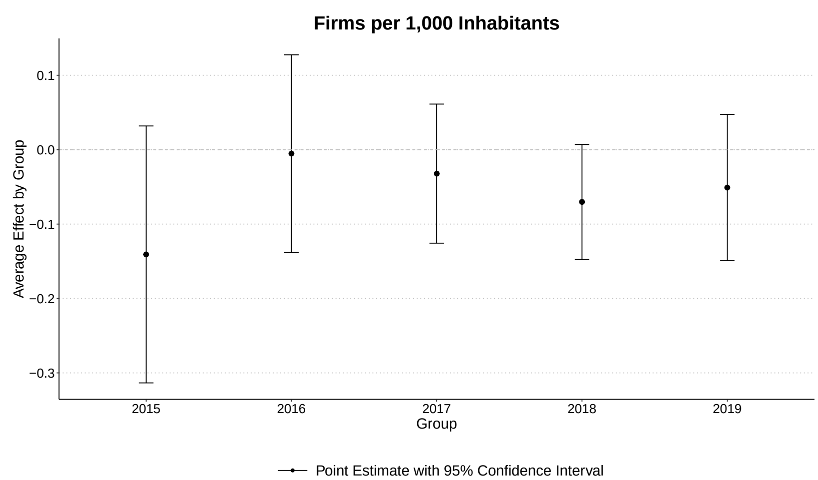



**Figure 3.A.6:** Effects on Firms: Callaway and Sant'Anna's Estimator

*Notes:* This figure plots the effects of decentralization of local governments on the number of active firms per 1,000 inhabitants using Callaway and Sant'Anna's estimator. Panel A shows the average treatment effects by the length of exposure to treatment, while Panel B shows the average group effects for each cohort of decentralized settlements. The vertical dashed line corresponds to the decentralization year. Standard errors are clustered at the former municipality level.

**Table 3.A.1:** Average Effects on Self-Employed and Firms with Region–Specific Linear Time Trends

| Outcome | Self-Employed | | Firms | |
|---|---|---|---|---|
| | Active per 1,000 | Asinh Active | Active per 1,000 | Asinh Active |
| | (1) | (2) | (3) | (4) |
| Decentralized (=1) | -0.413*** | -0.010** | 0.018 | 0.002 |
| | [0.143] | [0.005] | [0.033] | [0.003] |
| Outcome Mean | 20.171 | 2.306 | 6.211 | 1.311 |
| | (33.753) | (1.765) | (11.331) | (1.408) |
| Year FEs | Yes | Yes | Yes | Yes |
| Settlement FEs | Yes | Yes | Yes | Yes |
| Observations | 237195 | 237195 | 237195 | 237195 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* This table shows the average effects of decentralization on the number of active self-employed individuals and firms with region-specific linear time trends. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

**Table 3.A.2:** Average Effects on Self-Employed and Firms: Callaway and Sant'Anna's Estimator

| Outcome | Self-Employed | | Firms | |
|---|---|---|---|---|
| | Active per 1,000 | Asinh Active | Active per 1,000 | Asinh Active |
| | (1) | (2) | (3) | (4) |
| Average Dynamic Effect | -0.838*** | -0.032*** | -0.074 | -0.004 |
| | [0.257] | [0.006] | [0.041] | [0.003] |
| Average Group Effect | -0.406*** | -0.017*** | -0.052 | -0.003 |
| | [0.114] | [0.004] | [0.028] | [0.002] |
| Outcome Mean | 20.171 | 2.306 | 6.211 | 1.311 |
| | (33.753) | (1.765) | (11.331) | (1.408) |
| Year FEs | Yes | Yes | Yes | Yes |
| Settlement FEs | Yes | Yes | Yes | Yes |
| Observations | 237195 | 237195 | 237195 | 237195 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* This table shows the average dynamic and group effects of decentralization on the number of active self-employed individuals and firms at the settlement level using Callaway and Sant'Anna's estimator. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

**Table 3.A.3:** Average Effects on Self-Employed by Sector and Gender

| Outcome | Asinh Self-Employed By Sector | | | | | | By Gender | |
|---|---|---|---|---|---|---|---|---|
| | Commerce | ICT | Transport | Professionals | Industry | Other | Male | Female |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Decentralized (=1) | -0.010** | -0.008** | -0.011** | -0.006 | 0.002 | -0.010** | -0.015*** | -0.017*** |
| | [0.005] | [0.004] | [0.005] | [0.004] | [0.004] | [0.005] | [0.005] | [0.005] |
| Outcome Mean | 1.854 | 0.291 | 0.686 | 0.358 | 0.591 | 1.101 | 1.879 | 1.616 |
| | (1.643) | (0.762) | (1.098) | (0.845) | (1.016) | (1.327) | (1.632) | (1.562) |
| Year FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Settlement FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 237195 | 237195 | 237195 | 237195 | 237195 | 237195 | 237195 | 237195 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* This table shows the heterogeneous average effects of decentralization on the number of active self-employed individuals by economic sector and gender. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

**Table 3.A.4:** Average Effects on Firms by Sector

| Outcome | Asinh Firms By Sector | | | | | |
|---|---|---|---|---|---|---|
| | Commerce | Industry | Construction | Professionals | Agriculture | Other |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Decentralized (=1) | -0.002 | -0.004** | 0.000 | 0.000 | -0.001 | 0.004** |
| | [0.002] | [0.002] | [0.002] | [0.001] | [0.002] | [0.002] |
| Outcome Mean | 0.347 | 0.344 | 0.189 | 0.118 | 0.934 | 0.276 |
| | (0.867) | (0.823) | (0.658) | (0.532) | (1.082) | (0.811) |
| Year FEs | Yes | Yes | Yes | Yes | Yes | Yes |
| Settlement FEs | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 237195 | 237195 | 237195 | 237195 | 237195 | 237195 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* This table shows the heterogeneous average effects of decentralization on the number of active firms by economic sector. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

**Table 3.A.5:** Average Effects on Firm Branches

| Outcome | Asinh Firms w/ Active Branches | Asinh Branches All Firms | Asinh Branches Firms w/ Active Branches |
|---|---|---|---|
| | (1) | (2) | (3) |
| Decentralized (=1) | -0.001 | 0.000 | 0.000 |
| | [0.001] | [0.000] | [0.001] |
| Outcome Mean | 0.032 | 0.002 | 0.030 |
| | (0.254) | (0.032) | (0.208) |
| Year FEs | Yes | Yes | Yes |
| Settlement FEs | Yes | Yes | Yes |
| Observations | 237195 | 237195 | 237195 |

*Notes:* This table shows the average effects of decentralization on the number of active firm branches at the settlement level. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

**Table 3.A.6:** Mechanisms: Average Effects by Corruption Perceptions and Informal Activity

| Outcome | Corruption Perceptions | | Informal Activity | |
|---|---|---|---|---|
| | Low | High | Low | High |
| | (1) | (2) | (3) | (4) |
| *Panel A: Asinh Self-Employed* | | | | |
| Decentralized (=1) | -0.007 | -0.029*** | -0.009 | -0.020*** |
| | [0.006] | [0.007] | [0.007] | [0.006] |
| Outcome Mean | 2.233 | 2.390 | 2.142 | 2.462 |
| | (1.732) | (1.799) | (1.795) | (1.723) |
| *Panel B: Asinh Firms* | | | | |
| Decentralized (=1) | -0.001 | -0.005 | -0.006* | 0.002 |
| | [0.003] | [0.004] | [0.004] | [0.004] |
| Outcome Mean | 1.294 | 1.331 | 1.307 | 1.315 |
| | (1.379) | (1.439) | (1.470) | (1.346) |
| Year FEs | Yes | Yes | Yes | Yes |
| Settlement FEs | Yes | Yes | Yes | Yes |
| Observations | 126603 | 110592 | 115416 | 121779 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* This table shows the average effects of decentralization on the number of active self-employed individuals and firms by the level of perceived corruption and informal activity. The low-high values correspond to the below-above median levels of the 2011 corruption perceptions index and the 2017 rate of informal activity. Standard errors clustered at the former municipality level are shown in square brackets. Standard deviations are reported in parentheses.

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