

The Effect of Independent Online Media in an Autocracy

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Abstract

How do independent media affect regime support in an autocracy? We carry out two field experiments in Russia by promoting the country's only independent online TV channel to a randomized sample of cities and individuals before the 2016 parliamentary elections. In both experiments, we find that independent media foster polarization, increasing turnout and progovernment votes among regime supporters, and reducing it for nonsupporters. The effect, however, holds only for voters who rely on news from social media; among consumers of traditional media, our treatment uniformly decreases regime support. Our results highlight how social media can mediate the effect of independent media in autocracies.

Keywords: independent media, polarization, persuasion, elections, Russia, social media, field experiment

JEL: C93, D72, O33, P16

So long as an opinion is strongly rooted in the feelings, it gains rather than loses in stability by having a preponderating weight of argument against it.

John Stuart Mill, 1869

1 Introduction

How does the expansion of independent media affect support for an autocratic regime? It is widely believed that the persuasive power of independent media will undermine public support for an autocrat ([Lawson, 2002](#)), yet the empirical evidence on this topic remains surprisingly scarce, and the results are mixed at best. As domestic independent media are usually suppressed by autocratic regimes, scholars have resorted to studying the effects of foreign media on political attitudes in autocracies. They have found either insignificant ([Kern, 2011](#); [Gagliarducci et al., 2020](#)) or even positive effects of independent foreign media on support for the regime ([Kern and Hainmueller, 2009](#)), thus challenging conventional wisdom about media effects in autocracies. It also seems that modern “informational” autocrats who rely mostly on information manipulation rather than on repression ([Guriev and Treisman, 2019, 2020, 2022](#)) tolerate some limited form of domestic independent media. How this limited independent media may affect public support for the regime and why informational autocrats seem to tolerate it, is still an open question.

We try to shed light on this question by providing experimental evidence on the effect of domestic independent media on electoral support for the regime in an autocracy. We focus on the 2016 parliamentary elections in Russia and conduct a field experiment in which we give a randomized sample of cities and individuals free access to the only independent online TV channel in Russia three weeks before the elections. Our experimental design permits us to study the effect of exposure to independent media on support for the progovernmental party, and to examine how this effect depends on preexisting electoral support for the regime.

The existing literature suggests that independent media can persuade viewers to update their beliefs with new information and, consequently, to vote against the incumbent party. This effect is often observed for traditional independent media in young or emerging democracies, such as for example pre-Putin Russia ([Enikolopov et al., 2011](#)), Ukraine ([Peisakhin](#)

and Rozenas, 2018), and Mexico (Larreguy et al., 2020).

However, there is also growing evidence that media can foster an increase in polarization, rather than a uniform shift in opinion (Levendusky, 2013; Prior, 2013; Martin and Yurukoglu, 2017). Such a polarization effect is driven by biased processing of information that can emerge as a result of “motivated beliefs” or “motivated reasoning,” when individuals are likely to trust the information congruent with their existing beliefs, while discounting any evidence opposed to their beliefs. As a result, they might become more extreme in their position over time, a process that has been described as “attitude polarization” (Lord et al., 1979; Taber and Lodge, 2006). Biased processing of information and attitude polarization have been confirmed by numerous studies spanning several disciplines, including psychology (Lord et al., 1979; Greitemeyer, 2014; Suhay and Erisen, 2018), political science (Nyhan and Reifler, 2010; Baekgaard and Serritzlew, 2016; Little, 2019) and economics (Adena et al., 2015; Bénabou and Tirole, 2016; Golman et al., 2016). More recent studies find that attitude polarization is especially prevalent among consumers of nonmainstream media generally (Lau et al., 2017) and social media in particular (Bail et al., 2018). Thus, polarization effects might be especially relevant for autocracies, where the state exhibits tight control over traditional media and independent media is usually limited to online media outlets and social media.

A good context to test the effect of independent media in an autocracy was the Russian Federation in the 2010s, as it featured two characteristics that enabled our research design: regular national elections and independent media that were only available in a limited, online environment. First, Russian elections, although riddled with electoral fraud (Klimek et al., 2012; Enikolopov et al., 2013), still provided a relevant measure of electoral support for the regime. Second, as a result of the process of encroachment on press freedom that started after President Putin came to power, by 2016 Russia had only one remaining independent television channel that offered news coverage, TV Rain. The channel provided more coverage of corruption cases, political scandals, and international news than the federal TV channels controlled by the government. It also adopted a more balanced and sometimes critical tone on the topics that were covered by state-controlled TV. Since about 2011, TV Rain has been under constant political pressure, ranging from vilification by state propaganda to regular attacks by social media bots (Popescu and Secieru, 2018; Sanovich et al., 2018). In

January 2014, TV Rain was cut off from cable television, effectively restricting its range to approximately 50,000 subscribing viewers on the internet.

The fact that TV Rain was normally only accessible behind a paywall allowed us to carry out two field experiments shortly before Russia’s national parliamentary elections in 2016, by randomly allocating free access to the channel. We implemented the first experiment at the city level. From a sample of 42 midsize Russian cities, we randomly selected 15 cities that received the option of a one-month free-trial subscription to TV Rain for all internet users of these cities. In addition, we advertised the free-subscription option on Russia’s biggest social network, VKontakte.

The results of the city-level experiment indicate that, on average, our treatment led to a positive but insignificant effect on voting for the ruling party, United Russia. However, there was noticeable heterogeneity with respect to preexisting support for the regime. Our treatment had a positive effect on turnout and voting for United Russia in districts with higher support for the ruling party in the 2011 parliamentary elections. In districts with low previous regime support, on the contrary, votes for the ruling party fell slightly as a result of the treatment and turnout remained statistically indistinguishable from turnout in the control group.

A limitation of the city-level experiment in terms of external validity is that the treatment primarily targeted social media users, since the advertising campaign was run through VKontakte. To provide more granular analysis, address the ecological inference issue, and study the potential heterogeneity of effects based on personal characteristics, we conducted a second experiment at the individual level, in the form of a two-wave telephone survey, shortly before and after the elections. Surveys were administered to a random set of 1,211 respondents from 12 cities in the control group of the city-level experiment.¹

The treatment consisted of randomly distributing subscription codes that granted free access to TV Rain for one month. Respondents were treated at the end of the first wave of the survey after they provided information about their age, education, income, employment, media outlets for sourcing news, and, most importantly, their intention to participate in the

¹Keeping a small number of respondents per city ensures that our second experiment did not interfere with the city-level experiment.

elections and their party of choice. After the elections, we asked the respondents in the second wave of the survey about their actual voting behavior.

The results of the individual-level experiment demonstrate that the effect of having access to independent news was very different for people who rely on news from social media, compared to those who primarily rely on news from more traditional media. For people who use social media as one of their main sources of information, our treatment had a polarizing effect. After receiving the subscription offer, respondents who initially planned to vote for the ruling party were more likely to turn out at the election and vote according to their intentions. The opposite effect was observed for respondents who initially were not planning to support the ruling party. The two effects canceled each other out, so that on average, the treatment did not have a significant effect on either turnout or support for the ruling party. Respondents who get their news from more traditional media sources, on the other hand, responded to the treatment by voting less for the ruling party, regardless of their initial intentions, with no effect on electoral turnout. These results hold if we use a propensity score approach to control for self-selection into using social media as the source of news, which suggests that the difference in effects is likely to be driven by the causal effect of social media. Our results indicate that one of the mechanisms behind the effect of social media on political polarization is through its effect on processing information from other media sources.

Our paper contributes to the literature on the effects of independent media on political outcomes in autocracies (Egorov et al., 2009; Enikolopov et al., 2011; Adena et al., 2015) as well as the literature on the effects of social media on political outcomes (Allcot et al., 2020; Enikolopov et al., 2020; Zhuravskaya et al., 2020) and, in particular, the role of social media in authoritarian regimes (Pearce and Kendzior, 2012; King et al., 2013; Reuter and Szakonyi, 2015; Rød and Weidmann, 2015; King et al., 2017; Sobbrío, 2018; Tucker et al., 2017; Gainous et al., 2018; Chen and Yang, 2019; Keremoglu and Weidmann, 2020; Zhuravskaya et al., 2020). The paper is also related to research on political polarization that has documented the detrimental effect of political polarization on democratic institutions (Graham and Svobik, 2020; Arbatli and Rosenberg, 2021), by showing that polarization can also increase the stability of autocracies. These results could also explain the strategy of many authoritarian

regimes to allow for independent media, but only in the form of online outlets. In this way, autocratic rulers could solve the information dilemma faced by authoritarian regimes by maintaining independent sources of information about what is going on in the country, without these sources of information leading to a decline in support for the regime.

The rest of the paper is organized as follows. Section 2 explores the theoretical considerations in the existing literature. Section 3 provides background information on media and elections in Russia. Section 4 describes the design and results of the the city-level experiment, while Section 5 presents the individual-level experiment and its results. Section 6 concludes with a discussion of the findings and their potential implications.

2 Media Effects

The literature on the influence of media on political behavior suggests that media outlets can cause both a shift in a particular direction or an increase in divergence of opinions. On the one hand, by providing information that is biased in a particular direction, it can persuade voters to shift their support in favor of (or against) a particular party. For example, [Enikolopov et al. \(2011\)](#) show how access to the independent TV channel NTV during the 1999 parliamentary elections in Russia increased the combined vote share for the opposition by 6.3 percentage points while decreasing the vote share for the progovernment party by 8.9 percentage points. [Barone et al. \(2015\)](#) find an effect of similar size for pro-Berlusconi votes once independent digital TV was introduced in Italy, where most TV channels had been controlled by Berlusconi. As the magnitude of the media effects measured in Russia and Italy were significantly higher than those found by [DellaVigna and Kaplan \(2007\)](#) in a similar study for the United States, [Enikolopov and Petrova \(2015\)](#) argue that media effects may be generally stronger in captured environments and imperfect democracies, which is consistent with theoretical predictions by [Besley and Prat \(2006\)](#).

On the other hand, biased processing of information may lead to a situation in which exposure to the same media sources leads to different reactions among different people, depending on their preexisting beliefs, which may lead to an increase in polarization. A literature in political psychology (see, e.g., [Lord et al., 1979](#); [Garramone et al., 1990](#); [Ditto and Lopez, 1992](#); [Taber and Lodge, 2006](#)) and political economy (see, e.g., [Gentzkow and](#)

Shapiro, 2006; Gehlbach and Sonin, 2014) argues that if media bias is easily noticeable, media consumers may discount the message if it contradicts strong existing prior beliefs (priors). Messages targeting consumers who have strong opposite priors may even backfire by reinforcing existing priors rather than reversing them (Lord et al., 1979; Ditto and Lopez, 1992). For example, Adena et al. (2015) find that Nazi anti-Semitic propaganda during the 1930s was effective in places where anti-Semitism was historically high but had a negative effect on the support for Nazi messages in places with historically low anti-Semitism. Baysan (2019) identifies a similar result for a canvassing campaign before the April 2017 constitutional referendum in Turkey, with pro-opposition canvassing reducing the vote share for the government in pro-opposition neighborhoods, but increasing it in progovernment neighborhoods.

There is also a growing literature on the effect of social media on political polarization, summarized by Tucker et al. (2018) and Kubin and von Sikorski (2021). A number of recent studies show how social media usage is increasing political polarization (Gruzd and Roy, 2014; Hong and Kim, 2016; Allcot et al., 2020; Mosquera et al., 2020). The evidence on the exact effects of exposure to pro- or counterattitudinal information, however, remains less straightforward. While Kim (2015) and Knobloch-Westerwick et al. (2015) find that selective exposure to proattitudinal online media always seems to increase ideological and affective polarization, Jo (2020) finds that exposure to like-minded partisan media in South Korea can decrease polarization, because users might be better able to discern the underlying truth with media sources they are familiar with.

The literature on the effect of exposure to counterattitudinal information remains also somewhat contradictory. On the one hand, there is evidence that exposing individuals in the United States to counterattitudinal content on Twitter leads to a backlash that increases polarization (Bail et al., 2018). At the same time, Levy (2021) finds that exposure to counterattitudinal US news outlets decreases negative attitudes toward the opposing political party, while Kim (2019) also uses US data and finds mixed evidence. We add empirical evidence to this debate by looking at the effect of social media on polarization in an informational autocracy, the Russian Federation.

3 Background Information

3.1 Media Environment

In the 1990s, Russia enjoyed relatively free and competitive media ([Koltsova, 2006](#); [Skillen, 2017](#)). This started to change with the election of Vladimir Putin as the Russian president in March 2000, after which independent TV channels were one by one brought under the control of the government ([Becker, 2004](#); [Baker and Glasser, 2005](#); [Koltsova, 2006](#); [Lipman, 2009](#)). The increase in state control over the media was likely driven by the understanding that the media can play a crucial role in shaping public opinion and winning elections.² One example that might have contributed to the formation of this view was the privately owned TV channel NTV, which had openly campaigned against the pro-Putin party “Unity” before the 1999 Duma elections. As shown by [Enikolopov et al. \(2011\)](#), NTV had a strong impact on the elections, with the vote share for Unity being on average 8.9% lower in cities where NTV was available. When the channel continued to criticize Putin after his election victory in 2000, it was taken over by the state monopoly Gazprom and has since become a reliable source of progovernment news ([Belin, 2002](#); [Koltsova, 2006](#)). Establishing control over TV channels was of particular importance, since in Russia television still serves as the main source of political information, with 85% of the Russian population indicating in 2015 that their knowledge about Russia and the world comes mainly from TV, and 60% reporting that they watch TV every day.³

Government control over the media further increased after Putin’s reelection as president in May 2012. Following a wave of mass protests triggered by allegations of electoral fraud during the parliamentary elections in December 2011, the level of pro-Kremlin coverage perceptibly increased on Russia’s state-controlled media outlets and TV channels ([Pomerantsev,](#)

²In an interview with the director of the radio station Echo Moscow, Putin once famously remarked: “Contrary to a common perception, mass media is an instrument, rather than an institution.” ([Enikolopov et al., 2011](#), p. 3253). [White and Oates \(2003\)](#), [White et al. \(2005\)](#), and [Enikolopov et al. \(2011\)](#) all provide evidence that the government’s concern about the role of television in Russian politics may not have been unfounded.

³According to a survey by the independent Levada Center; <https://www.levada.ru/>. While 85% of the Russian population indicated TV as their primary source of news in 2015, the global average in that year was only 53% (<https://www.statista.com/chart/4089/preferred-news-sources/>).

2015; Oates, 2016; Spaiser et al., 2017). At the same time, the editorial teams of a number of leading online news outlets were forced to leave.⁴ In 2014, TV Rain was no longer allowed to broadcast on cable TV. When in the spring of 2016 the editorial team of RBC, a media organization including a business TV channel, a business newspaper, and a popular political news website, was replaced, Russia lost its last fully independent cable TV channel.⁵ Virtually all the remaining independent or semi-independent media, including TV Rain, were shut down in early March 2022, after the start of the war in Ukraine.

3.2 TV Rain

TV Rain was established in 2010 by the journalist Natalya Sindeyeva, with the aim of providing uncensored news on politics, economics, and cultural matters, as well as investigative journalism.⁶ In late 2011 it became widely known for covering the protests against electoral fraud during the 2011 parliamentary elections (Smyth and Oates, 2015; Spaiser et al., 2017). By the end of 2013, TV Rain had become a well-established and relatively influential media outlet with an average audience of almost 10 million viewers.

In early 2014, a controversy about a survey on the siege of Leningrad was used as a pretext by the government to exert pressure on Russia’s cable network providers, which were forced to take TV Rain from their networks. Shortly afterward, a ban on commercial advertising on cable and satellite TV stations was passed by the parliament, widely believed to be specifically designed to stem the flow of independent revenues TV Rain was relying on.⁷ Finally, in October 2014, the channel was forced to leave its studios in central Moscow on short notice; it had to continue broadcasting from a private apartment. As a result, the channel’s viewership fell from several million in early 2014 to about 60,000 at the end of the year. TV Rain nevertheless continued to function and broadcast as a paid online channel

⁴Noteworthy examples include the editors of Gazeta.ru in 2012 and Lenta.ru in 2014.

⁵Masha Lipman, “The Demise of RBC and Investigative Reporting in Russia,” *The New Yorker*, May 18, 2016.

⁶These are offered through a variety of formats, including traditional news broadcasts, interviews, talk shows, debates, and documentaries.

⁷Karoun Demirjian, “Russian advertising ban on paid cable and satellite channels threatens independents,” *Washington Post*, November 6, 2014. The advertising ban had the side effect of undercutting the financial resources of Russia’s last remaining independent regional TV channels.

and has remained an independent voice with an average audience of about 40,000 viewers, limited mostly to Moscow and St. Petersburg.

In 2016, when we carried out our experiments, the content of TV Rain continued to differ notably from that of the main state-controlled TV channels. It critically covered events inside Russia, including government involvement in cases of corruption, whereas news on state-controlled TV channels was almost exclusively progovernment.

Table A1 in Appendix A provides an example of the principal news items during the main evening news broadcast on Russia’s two main TV channels, Channel One and NTV on September 15, 2016, three days before the Duma elections. We number the items to show how the almost perfect overlap of the content in the two main state-controlled news broadcasts is obfuscated by permuting the order of the items. In covering a government conference on infrastructure development, alleged doping by U.S. athletes, ceasefires by troops allied with Russia in Syria and Ukraine, and the allegedly dirty electoral tactics of the U.S. election campaign, both channels closely follow the progovernmental line, while coverage of events actually occurring in Russia is absent. In comparison, TV Rain covers two corruption scandals in Russia, news about the head of Russia’s Investigative Committee, Alexander Bastrykin, losing the privilege of traveling in a convoy of cars, as well as some international news.

TV Rain was especially suitable for a field experiment on the effect of exposure to independent TV. The fact that the channel was available only on the internet and has a paywall for most of its content made it easy to distribute free access to a randomized sample of the population. At the same time, high levels of internet penetration implied a large potential audience for the channel. In 2016, about 63% of the Russian population used the internet more than once a week, and 44 % used it daily.⁸

3.3 The 2016 Parliamentary Elections

The elections for the 7th convocation of the State Duma of the Russian Federation were held on September 18, 2016. While 14 parties were listed on the ballot, observers anticipated that only a small number of proregime parties would enter the new parliament. The government

⁸<https://www.levada.ru/2019/12/05/dinamika-polzovaniya-internetom/>

party, United Russia, was expected to receive the majority of seats (Kynev, 2017), with the rest going to the so-called “systemic” opposition loyal to the regime.⁹ The electoral campaign, as well as the elections themselves, were characterized by low political competition and high political apathy. This lack of dynamism resulted from high levels of media control, the failure of the opposition to coordinate and communicate a coherent political message, and the absence of an active electoral campaign by the government party, as well as a change in the timing of the elections from the traditional date in December to a date in mid-September, when most people were just returning from their summer holidays. As a consequence, electoral participation was the lowest in modern Russian history, with a turnout rate of 48% (Kynev, 2017; McAllister and White, 2017).

4 City-Level Experiment

4.1 Experimental Design

Sample selection. Our first experiment provided a randomized subset of 42 mid-size cities in the European part of Russia with free access to TV Rain, in combination with a social media campaign advertising this free access.¹⁰ We focus on cities with a population between 100,000 and 500,000 inhabitants, as they are fairly representative of Russia, with about one-fifth of the country’s population living in cities of that size. They are also big enough to have the necessary internet infrastructure to access online television. The per capita audience of TV Rain in these cities, on the other hand, was five times smaller than in Moscow and St. Petersburg, with on average of 3,600 online visitors to the TV Rain website daily per city, or 1.1% of the population.¹¹ This provides us with a good setting to introduce a new source of information to an audience that previously had no or only very limited exposure to our treatment.

⁹The “systemic” opposition comprised the parties normally represented in the parliament: the Communist Party of the Russian Federation (CPRF), the Liberal Democratic Party of Russia (LDPR), and A Just Russia.

¹⁰The European part of Russia includes the Central, Southern, Northwestern, and Volga federal districts. We exclude the city of Sochi due to its special status as the host of the 2014 Olympics.

¹¹Paid subscribers are even less common, with the average city in our sample having about 80 subscribers in 2016 (about 0.03% of the population, versus 0.19% in Moscow).

The main concern in selecting the city sample was the prevalence of electoral fraud, which had been continuously increasing since the early 2000s (Shpilkin, 2011; Enikolopov et al., 2013; Buzin et al., 2016; Kobak et al., 2016) and had reached a new high during the 2016 elections (Kalinin and Mebane, 2017). Since electoral fraud was not uniformly distributed across localities, we excluded from our sample cities with exceptionally high levels of fraud in the previous elections. In particular, we first excluded cities from regions with the status of ethnic republics, as these are well known for widespread electoral manipulation (Myagkov and Ordeshook, 2008; Goodnow et al., 2014).¹² We then also excluded cities where United Russia received more than 60% of the vote in the 2011 Duma elections, which is about two standard deviations above the average vote share in regions that are not ethnic republics, and hence a strong indicator for high levels of electoral fraud.

Experimental treatment and randomization. The primary objective of the experimental treatment was to increase the audience of TV Rain in the treated cities.

The treatment consisted of two elements. The first element was the temporary cancellation of the monthly subscription cost of about \$7.60 (480 rubles). This cost represented a strong barrier for viewers outside of Russia’s main cities, as it corresponded to about 2% of the average monthly income in Russia’s regions at the time. We eliminated the subscription cost by offering a one-month free-trial option to users from 20 randomly selected cities from our sample.

For every user, the city of origin was identified with the help of a location-specific IP address. A user from a treated city received a pop-up window with a free trial offer every time when opening the TV Rain website.¹³ The free-trial option was introduced for all treatment cities on August 26, 2016, 24 days before the election.

The second element of our treatment was an online advertising campaign on Russia’s most popular social media site, VKontakte, to attract attention to the free-trial offer. This campaign was carried out in 15 cities that were randomly selected from the 20 cities that

¹²The exclusion of ethnic republics also allows for greater homogeneity in terms of local political regime and culture (Dininio and Orttung, 2005).

¹³This geotargeting method is widely used for commercial purposes and is fairly accurate in identifying geographical locations (Shavitt and Zilberman, 2011).

received the free-trial offer. The remaining five cities were used to test whether the free-trial option by itself without an advertising campaign affected viewership and political behavior.

The advertising campaign included a banner ad next to the user’s personal newsfeed, as well as the same banner ad shown as a post in the main newsfeed of city-specific VKontakte groups. The banner consisted of an image of one of the two main news anchors of TV Rain, as well as the message: “One month subscription for free: Only now watch TV Rain for free for the whole month (tvrain.ru).” Banners next to personal newsfeeds were displayed on average 1.5 million times per city, or six times per registered VKontakte user. The banner campaign started on September 1, 2016, 18 days before the elections.

In addition, the same banner was also shown as a post on the wall of a public VKontakte group in each treated city, between six and nine days before the elections. These VKontakte groups normally attract subscribers by offering them access to local news, while generating income through paid advertisements. For each city, we selected the group with the biggest number of subscribers - ranging from 30,000 to 130,000 subscribers - to place a post. In 12 cities, the owners of the VKontakte group accepted and posted the advertisement, while in three cities they deemed the advertisement too political and declined to post it. In these three cases, we resorted to the second largest group in the city, where the advertisement was accepted and posted.

Table B1 in Appendix B presents the results of the randomization, suggesting that the 15 fully treated cities, as well as the five cities that received free access but not the advertising campaign, are similar to the control group in terms of city size, socioeconomic characteristics, and electoral results in the previous parliamentary election.

Effect of the treatment on TV Rain viewership. To assess whether our treatment increased the consumption of TV Rain, we look at the change in TV Rain viewership and the number of posts containing links to the TV Rain website, as posted by VKontakte users on their personal walls and in public VKontakte groups. We measure viewership at the city level as the log number of pages viewed daily per thousand people. Data were collected from Yandex Metrics, the Russian equivalent of Google Analytics. We calculate viewership separately for the 18-day period before the election, when our full treatment was applied,

and for a period 30 days before the beginning of our treatment, to estimate the difference-in-difference effect. The log number of posts with links to the TV Rain website, per million of inhabitants, are similarly calculated at the city level for the period of the main treatment, as well as for the month before.

Table 1 shows that TV Rain viewership increased by 11% as a result of the combination of the free trial and the advertising campaign (column 1). The free-trial offer alone did not change the number of pages viewed. As a placebo test, we substitute our dependent variable - TV Rain viewership in 2016 - with TV Rain viewership before the Duma elections in 2011, for the same length as that of our treatment in 2016. Column 2 shows that the main treatment - free access and the advertising campaign - is not correlated with viewership of TV Rain during the 18 days before the 2011 election. There is also no significant effect on viewership of the incomplete treatment of free access without advertising, suggesting that the effect we measure in column 1 is indeed driven by our treatment that combined free access to TV Rain with an advertising campaign.

In addition, we look at whether our treatment motivated people to share TV Rain links on social media. Column 3 reports a 13% increase in posts and reports containing TV Rain links as a result of our treatment, an effect very similar in size to the growth in viewership, albeit significant only at the 10% level. This result suggests potential spillover effects of our treatment to friends and contacts of directly treated individuals.¹⁴

Finally, in addition to the direct effect of watching TV Rain, the population exposed to the promotion campaign could also have been affected by the advertisements themselves if they had some prior knowledge about TV Rain or through reading TV Rain-related comments under posts in public VKontakte groups.

Measuring electoral outcomes. Our main outcome variables are turnout and votes for the ruling party, United Russia, at the polling station level. We obtain electoral data from the Central Electoral Commission (CEC) of the Russian Federation.¹⁵ The turnout measure

¹⁴Due to a lack of data on online posts in 2011, we are not able to perform a placebo test as we did for the case of TV Rain viewership.

¹⁵Raw data are available online at the CEC website: <http://www.cikrf.ru/>.

Table 1: The Effect of the Treatment on TV Rain Consumption

	(1)	(2)	(3)
Dependent variable:	Pages viewed		Vkontakte posts
Election:	Duma 2016	Duma 2011	Duma 2016
Treatment	0.10*** (0.03)	-0.11 (0.13)	0.12* (0.07)
Free access only	-0.02 (0.03)	0.05 (0.17)	-0.01 (0.05)
Pages viewed, pretreatment	0.95*** (0.02)	0.83*** (0.09)	
Vkontakte posts, pretreatment			1.02*** (0.07)
Observations	42	42	42
R2	0.99	0.82	0.98

Notes: Jackknife corrected robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

is calculated as the number of ballots cast by 100 registered voters. To measure electoral support for the ruling party, we use the number of votes cast for United Russia, divided per hundred registered voters. We follow existing research on Russian elections (see, e.g., [Klimek et al. 2012](#) or [Enikolopov et al. 2013](#)) and exclude polling stations with fewer than 100 voters, as these are usually special cases such as hospitals or prisons, with potentially high levels of coercion to vote for the ruling party.¹⁶ We use a difference-in-differences approach to estimate the effect of our treatment on votes in 2016, by controlling for voting results in the previous Duma election, in 2011. Because the borders of polling stations were redrawn between both elections, we control for the election results in 2011 at the electoral district level, which is the second lowest level of data aggregation: a city from our sample has on average two districts, and each district includes on average 58 polling stations.

We control for potential electoral manipulation at the polling-station level by using the number of absentee votes in the 2016 election per 100 registered voters minus the number of absentee votes in the 2011 election at the district level. As the previous literature suggests ([Harvey, 2016](#); [Rundlett and Svolik, 2016](#); [Frye et al., 2019](#)), the number of absentee votes is particularly indicative of electoral fraud in locations where results are manipulated by ballot stuffing or voter coercion.

¹⁶This leads to the exclusion of 265 polling stations, or about 5% of the polling stations in our sample.

Estimation strategy. Our preferred estimation strategy is ordinary least squares with jackknife-corrected errors clustered at the city level, to reduce any potential bias stemming from the relatively small number of treated cities (Long and Ervin, 2000; Cameron et al., 2008). We use the following linear model to estimate the average effect of our two treatments:

$$\begin{aligned} Electoral\ outcomes_i = & \alpha + \beta_1 Treatment_i + \beta_2 UR_{i\ 2011} + \beta_3 Turnout_{i\ 2011} + \\ & + \beta_4 Free\ access\ only_i + \beta_5 \Delta Absentee\ votes_{i\ 2011-16} + \epsilon_i \end{aligned} \quad (1)$$

where *Electoral outcomes_i* is the vote or the turnout of United Russia in the 2016 election at the polling station *i*; *United Russia 2011_i* and *Turnout 2011_i* are the turnout and votes for United Russia in the previous Duma election at the district level; *Treatment_i* is the variable for the full treatment that equals one if the polling station is in a city that received both the free-trial access and the advertising; *Free access only_i* equals one if the city had only free-trial access but no advertising; $\Delta Absentee\ votes_{i\ 2011-16}$ is a proxy for the potential change in electoral manipulation between the 2011 and 2016 elections; and ϵ_i is the error term. A linear negative effect of our treatment on turnout and votes would mean that the persuasion effect of being exposed to independent media dominates.

The nonlinear estimation is expected to capture the effect of polarization among regime supporters, using the interaction of the treatment with previous voting results for United Russia, at the electoral district level. We estimate the following equation:

$$\begin{aligned} Electoral\ outcomes_i = & \alpha + \beta_1 Treatment_i + \beta_2 Treatment_i \times UR_{i\ 2011} + \\ & + \beta_3 Free\ access\ only_i + \beta_4 Free\ access\ only_i \times UR_{i\ 2011} + \beta_5 UR_{i\ 2011} + \\ & + \beta_6 Turnout_{i\ 2011} + \beta_7 \Delta Absentee\ votes_{i\ 2011-16} + \epsilon_i \end{aligned} \quad (2)$$

In the case of a polarization effect, the interaction of our treatment with previous voting results for United Russia (β_2) would be positive and the direct effect of the treatment (β_1) would be negative. Polarization should trigger core supporters of the regime to attend the election, and, therefore, we expect an increase in turnout and votes for the ruling party, whereas people who do not support the regime would be even less likely to vote for the ruling party.

4.2 Results

Table 2 presents the results of the city-level experiment. The linear estimations for turnout and votes for United Russia (columns 1 and 5, respectively) produce positive but statistically insignificant coefficients for the combination of free trial access and the advertising campaign. Columns 2 and 6 show that these results are robust to the inclusion of the proxy for electoral manipulation. Interestingly, the difference in absentee votes between the elections has a positive and significant effect on turnout and votes for the ruling party, indicating the presence of electoral fraud in our sample. Overall, the positive but insignificant effects of our main treatment in the linear estimation suggest that providing free access to independent media did not result in a reduction of votes for the progovernment party. Our results are thus different from those found by [Enikolopov et al. \(2011\)](#) for pre-Putin Russia, where access to opposition media caused a decrease in turnout and votes for the progovernment party.

Table 2: The Effect of TV Rain on Voting Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Turnout 2016				United Russia 2016			
Treatment	2.75 (2.18)	3.16 (2.30)	-11.27 (8.03)	-14.82** (7.11)	1.94 (1.92)	2.17 (2.11)	-12.27** (5.83)	-14.29** (6.49)
Treatment $\times$ UR ₂₀₁₁			0.75* (0.39)	0.96*** (0.33)			0.76** (0.30)	0.88** (0.36)
Free access only	0.95 (2.19)	1.09 (2.21)	-0.44 (15.50)	-0.01 (15.97)	1.02 (2.18)	1.10 (2.08)	6.59 (27.24)	6.83 (27.26)
Free access only $\times$ UR ₂₀₁₁			0.07 (1.01)	0.05 (1.05)			-0.35 (1.81)	-0.36 (1.83)
UR ₂₀₁₁	0.03 (0.30)	0.13 (0.28)	-0.04 (0.31)	0.04 (0.28)	0.48* (0.26)	0.53** (0.25)	0.42 (0.26)	0.47* (0.26)
Turnout ₂₀₁₁	0.35 (0.25)	0.28 (0.23)	0.37 (0.26)	0.31 (0.23)	-0.17 (0.20)	-0.20 (0.20)	-0.16 (0.20)	-0.19 (0.20)
Δ Absentee votes _{2011–16}		1.68*** (0.33)		1.73*** (0.32)		0.93*** (0.23)		0.99*** (0.20)
Observations	4624	4624	4624	4624	4624	4624	4624	4624
R ²	0.07	0.21	0.08	0.24	0.09	0.17	0.12	0.21

Notes: Jackknife corrected standard errors clustered at the city level are in parentheses. * $p < 0.1$,

** $p < 0.05$, *** $p < 0.01$.

The nonlinear estimation reveals a more complex relationship. Column 3 of Table 2 shows that our main treatment increased turnout only in cities where the ruling party received

more than a certain level of votes in the previous election. This effect becomes even more significant when we control for potential electoral manipulation. The left graph in Figure 1 illustrates this nonlinear relationship by plotting the marginal effects of column 4.¹⁷ We see that the effect of our main treatment on voter turnout is significant and positive in those electoral districts where more than 20% of registered voters voted for United Russia in 2011.

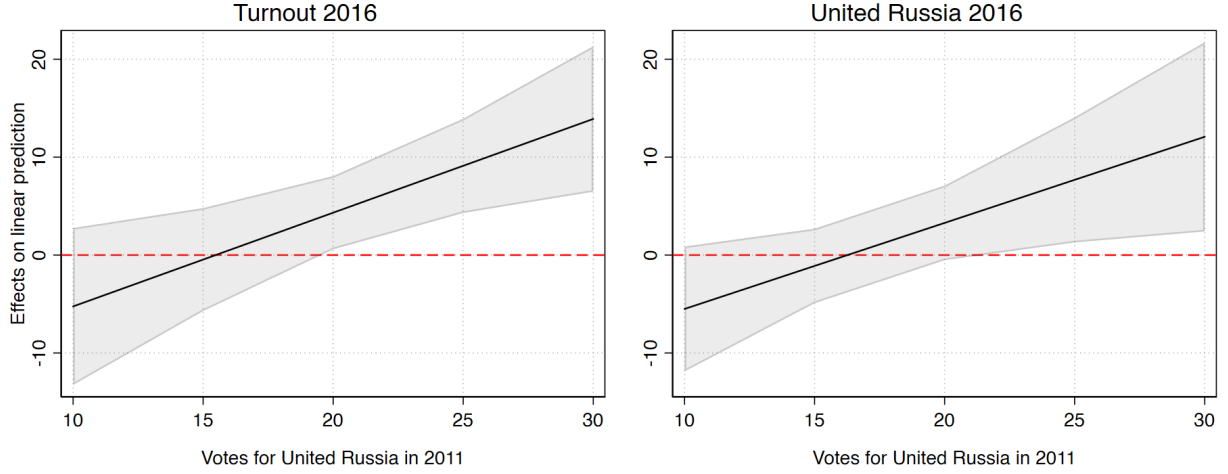
Similar results are observed for the vote share of the progovernment party. The direct effect of our treatment on voting is significant and negative, while its interaction with previous support for United Russia is significant and positive (columns 7 and 8 of Table 2). The second graph in Figure 1 demonstrates that the effect of TV Rain on the electoral support for United Russia was positive in cities with high levels of support for United Russia in the previous elections and becomes statistically significant for levels of support higher than 20% in 2011. The effect is negative for cities with low levels of support for United Russia in the previous elections, becoming statistically significant if the level of support in 2011 was lower than 11%.

Table 1 showed that our incomplete treatment of free access without advertising did not increase the viewership of TV Rain. Consistent with this finding, the results in Table 2 indicate that the provision of free access to TV without advertising did not affect votes or turnout in any of the specifications. Note also that both our proxies for electoral manipulation are positive and strongly significant, suggesting that ballot stuffing and voter coercion indeed played a role during the elections.

To confirm that our results are not caused by spurious correlation or pretreatment trends, we perform a placebo test by substituting our dependent variable with election results from the 2007 parliamentary elections, at the polling-station level. The results are reported in Table 3. Both linear (columns 1 and 5) and nonlinear estimations (columns 3 and 7) indicate no significant effect of the free-trial access and the advertising campaign (columns 1 and 3), as well as of free-trial access alone, on turnout or electoral outcomes. Adding proxies for the change in electoral manipulation between 2007 and 2011 in columns 2, 4, 6, and 8 also does not influence our results.

¹⁷The horizontal axis depicts the range of votes for United Russia during the 2011 Duma election.

Figure 1: Nonlinear Effect of TV Rain Promotion on Turnout and Votes for United Russia



Note: Conditional marginal effects with 95% CIs.

Overall, the results of the city-level experiment provide evidence for a polarizing effect of independent media on turnout and voting for the progovernment party. In cities with high levels of preexisting government support, it led to an increase in turnout and an increase in the share of votes for the progovernment party, whereas in cities with low levels of preexisting government support, the effect was the opposite.

5 Individual-Level Experiment

5.1 Experimental Design

To provide more comprehensive analysis of the heterogeneity of effects with respect to individual characteristics and to address the issue of ecological inference, we complemented the city-level experiment with an individual-level experiment. The experiment was implemented in the context of a two-wave survey before and after the 2016 elections. In the first wave, we asked respondents about their voting intentions and then treated a randomized subgroup of respondents with a free-subscription offer to TV Rain. In the second wave, respondents were asked about their actual voting behavior during the elections.

In particular, this experiment allowed us to test whether the effect of our treatment depends on whether subjects use social media as a source of information. Our advertising

Table 3: Placebo Regression Using Results From The 2007 Duma Election

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Turnout 2007				United Russia 2007		
Treatment	-0.79 (2.32)	-0.59 (1.88)	6.69 (10.76)	4.91 (8.81)	1.84 (1.72)	1.98 (1.55)	5.94 (11.06)	4.67 (9.68)
Treatment $\times$ UR ₂₀₁₁			-0.40 (0.61)	-0.30 (0.49)			-0.22 (0.65)	-0.15 (0.57)
Free access only	-2.13 (2.50)	-1.55 (2.18)	-13.81 (26.99)	-12.66 (25.39)	0.12 (1.94)	0.53 (1.84)	-12.08 (11.03)	-11.26 (10.05)
Free access only $\times$ UR ₂₀₁₁			0.72 (1.83)	0.68 (1.72)			0.75 (0.72)	0.72 (0.65)
UR ₂₀₁₁	-0.38 (0.25)	-0.28 (0.24)	-0.38 (0.30)	-0.29 (0.29)	0.24 (0.24)	0.31 (0.23)	0.21 (0.27)	0.28 (0.26)
Turnout ₂₀₁₁	0.57*** (0.16)	0.50*** (0.15)	0.58*** (0.17)	0.51*** (0.15)	-0.01 (0.18)	-0.06 (0.18)	0.01 (0.19)	-0.04 (0.19)
Δ Absentee votes _{2011–2007}		1.10*** (0.13)		1.09*** (0.13)		0.78*** (0.16)		0.77*** (0.15)
Observations	4584	4584	4584	4584	4584	4584	4584	4584
R2	0.07	0.29	0.08	0.30	0.05	0.22	0.07	0.23

Notes: Jackknife corrected standard errors clustered at the city level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

campaign in the first experiment was conducted exclusively via the Russian social media platform VKontakte, which means that the effect was driven primarily by users of social media. Since social media is known to have a polarizing effect (e.g., [Allcot et al. \(2020\)](#)), interactions in social media could have been a plausible mechanism behind the results we find in Section 4. In the city-level experiment, we found some anecdotal evidence in support of this hypothesis: by looking at individual posts below the advertisements in city-level groups, we were able to identify 381 posts that explicitly reacted to our advertisements. Of these 381 posts, we manually classified 81 (or 21%) as strictly negative reactions, with a higher share of negative posts in cities with higher levels of voting for United Russia in 2011. These 81 posts initiated further, often highly polarized discussions.

First survey-wave. The first wave of the survey was carried out from September 1 to September 9, 2016, ending eight days before the elections. Respondents were randomly selected from the population of 12 cities in the control group of the city-level experiment, with approximately 100 respondents per city.¹⁸ Overall, 1,211 respondents were interviewed

¹⁸Using a small number of respondents per city ensured that the outcomes of the city-level experiment were

in the first wave. The main requirement for the respondents to be included in the sample was being of voting age (18 years or older) and using the internet at least two to three times a week, with 86.4% of the respondents indicating that they use the internet daily.

To test for potential heterogeneous effects of our treatment with respect to social media consumption, we separated the sample into two subgroups: respondents who used social media as a source for obtaining news, and those who used more traditional media sources instead. To identify media consumption, respondents had to identify the media sources they were using most frequently, from a list with the following options: (1) federal TV channels; (2) cable TV channels; (3) TV Rain; (4) radio Echo MSK; (5) radio Mayak; (6) the Internet (excluding social media); (7) social media (Vkontakte, Facebook, Odnoklassniki, etc.); (8) newspapers and journals; and (9) other sources.¹⁹

One group of respondents (n=690) indicated social media as one of their primary sources of information, while the other group (n=521) said they rely on traditional media sources. Figure 2 presents the distribution of media sources for the whole sample, as well as for the two subgroups. State-controlled federal television remained one of the most popular news sources in Russia, with around 78% of respondents indicating it as a primary source of information. The internet and social media, where state control and censorship were relatively less stringent, are placed second and third. Next come a range of other government-controlled media, such as cable television, newspapers, and the popular Radio Mayak. 11% of respondents used the option to indicate “other sources,” with close social contacts such as relatives, friends, or colleagues being mentioned most frequently, together with entertainment, TV channels, other radio channels, or local press. The two media outlets with some independent content, Radio Echo Moscow and TV Rain, came in last, with 8% and 4%, respectively.

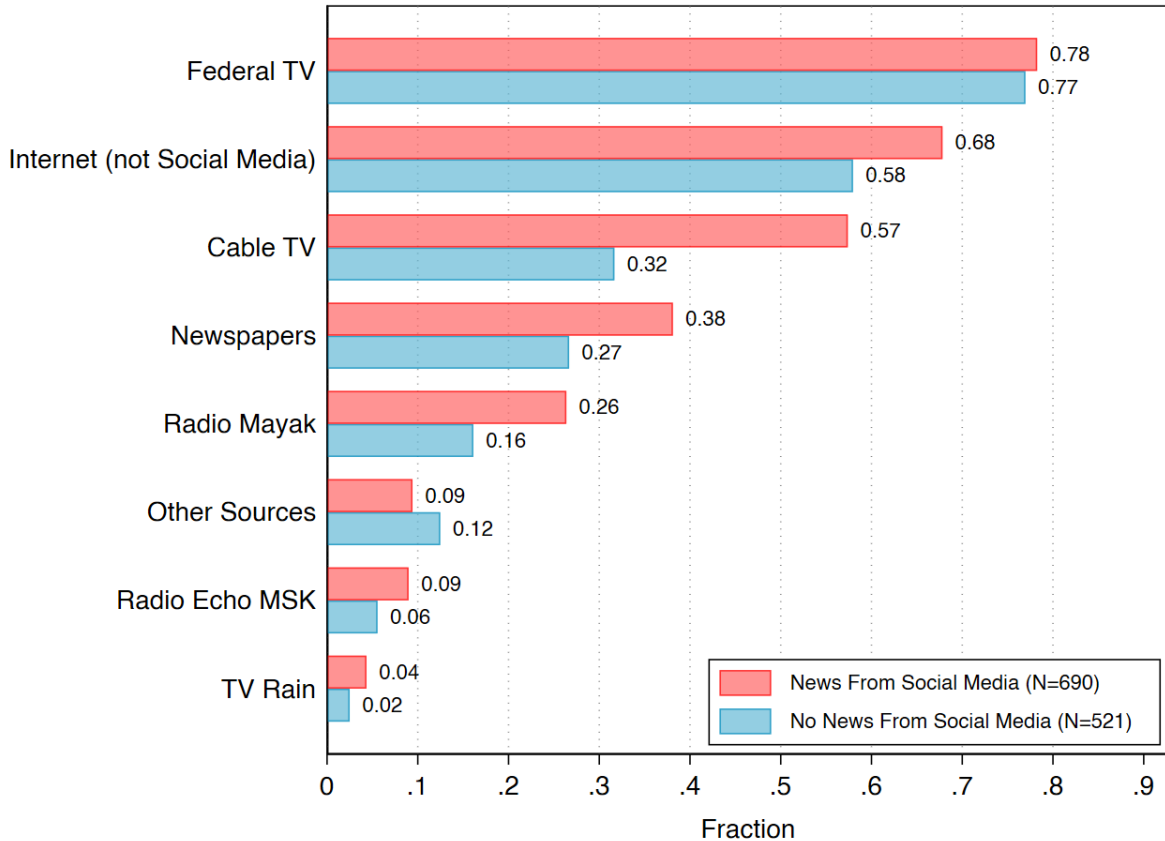
Figure 2 reveals an important pattern: while the relative popularity of news sources remains constant for both subgroups, the social media group features more diverse news consumption. On average, respondents from this group selected 3.8 sources, compared to 2.2 sources for the other group. Previous findings in the literature link more diverse news

not affected.

¹⁹The “other sources” category allowed for the possibility to mention an additional media source, should the respondent be willing to disclose it. There were no limits on the number of sources that respondents could identify.

consumption with higher levels of affective polarization (Lau et al., 2017). Similarly, Taber and Lodge (2006) find more sophisticated and politically knowledgeable individuals to have stronger prior attitudes and, therefore, to exhibit more polarization when presented with incongruent evidence. Since the increase in the diversity of media exposure in our sample is mainly driven by nonmainstream alternatives, our observation also relates to a study by Tsfati and Cappella (2003), who find that higher nonmainstream media consumption is associated with media skepticism. The difference in media exposure between the two subgroups may therefore be related to the potential heterogeneity in the reaction of social media users, compared to users of traditional media.

Figure 2: Popularity of News Sources Among the Respondents in the First Wave



We used the first wave of the survey to collect information on individual characteristics such as age, education, material well-being, employment, and voting intentions for the upcoming Duma elections. Table C2 in Appendix C provides a balance test for these char-

acteristics. We see that the social media group was five years younger on average, less likely to hold a university degree, and to have watched TV Rain previously, but more likely to use the internet daily. Notably, both groups indicated very similar intentions to attend the elections and to vote for United Russia.

Experimental treatment and estimation strategy. At the end of the interview, respondents were randomly provided with our experimental treatment, consisting of a free-subscription offer to TV Rain. The treatment was offered to two out of every three respondents and was framed as a small gift for taking part in the survey. The subscription offer included a 12-digit code sent via SMS to the respondent’s telephone, together with an online link to the TV Rain website, where the code could be redeemed. Overall, 806 subscription codes were distributed.²⁰ Table C3 in Appendix C provides a balance test for the treatment and control groups, showing that the randomization of the treatment worked successfully.

Our main independent variable, *Treatment*, equals one if the respondent was offered the subscription code, and zero otherwise. Similar to equation (1) in the city-level experiment, we use a difference-in-differences approach to estimate the linear effect of the subscription offer on the outcome variables in each subgroup in the second wave. In particular, we estimate the following linear probability model:

$$\begin{aligned} Outcome_i = \alpha + \beta_1 Treatment_i + \beta_2 UR_intent_i + \beta_3 Turnout_intent_i + \\ + \chi'_i \Psi_1 + \epsilon_i \end{aligned} \quad (3)$$

In the nonlinear estimation, we add an interaction term of the subscription offer with the intention to vote for United Russia from the first wave. This term is analogous to the interaction in the city-level experiment (equation 2, in Section 4.1), providing us with the following nonlinear estimation:

$$\begin{aligned} Outcome_i = \alpha + \beta_1 Treatment_i + \beta_2 Treatment_i \times UR_intent_i + \\ + \beta_3 UR_intent_i + \beta_4 Turnout_intent_i + \chi'_i \Psi_1 + \epsilon_i \end{aligned} \quad (4)$$

²⁰To encourage a higher uptake of our treatment among treated respondents, we sent an additional SMS notification after a couple of days, with a subscription code, the link, and a reminder that the link would be valid only within the next three days.

We estimate these effects in the subsamples of social media users and nonusers separately.

Second wave: attrition. The second survey wave was carried out between October 12 and 18, 2016 three weeks after the elections and one month after the first wave. Of the 1,211 original respondents, 483 agreed to be interviewed again, giving us a response rate of about 40%, which is standard in this type of literature (Gerber et al., 2009; Asher, 2016). Tables C2 and C4 show that sample attrition did not alter the composition of the two subgroups, nor did it affect the randomization of the treatment. To test whether attrition was associated with our treatment, we employ the same estimation strategy as in equations (3) and (4), with attrition as an outcome variable that is equal to one if a respondent dropped from the second wave, and zero otherwise. Table C1 in Appendix C reports the results for both subgroups. In all specifications, the coefficients for linear and nonlinear terms are statistically insignificant, suggesting that our treatment did not affect attrition.

Second wave: uptake of the treatment. In both waves, we used the same question to ask respondents whether they had watched TV Rain in the past. We use the difference in answers between the waves to estimate the effectiveness of our treatment on increasing the viewership of TV Rain. As an outcome variable, we construct a dummy that equals one if a respondent reported watching TV Rain in the second wave but not in the first wave.²¹

Table 4 shows that the subscription offer motivated about 14% of treated respondents to start watching TV Rain, at a rate similar for both subgroups.²² This magnitude is very close to the one we found in the city-level experiment (see Table 1). In the social media group, however, only respondents who intended to vote for United Russia significantly increased their likelihood of watching TV Rain when offered the subscription (columns 3 and 4).

²¹In 17 cases, respondents - somewhat inconsistently - reported in the first wave to have watched TV Rain in the past, while reporting in the second wave not to have watched TV Rain previously. Since we are interested only in an *increase* in watching, we assign a value of zero to these observations. Alternatively, omitting these data points does not affect the final results.

²²We acknowledge that the estimated magnitudes might be somewhat downward biased if treated respondents were answering this question strategically in anticipation of another free subscription (Zhang et al., 2018). A relatively long lag between the initial subscription offer and the second wave of the survey might also have contributed to imprecise self-reporting.

Table 4: Treatment and Increase in Watching TV Rain

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	News from social media				No news from social media			
Dependent variable:	Increase in TV Rain watching							
Treatment	0.13*	0.14*	0.08	0.09	0.12*	0.14**	0.11*	0.12*
	(0.07)	(0.07)	(0.08)	(0.08)	(0.06)	(0.07)	(0.06)	(0.07)
			[0.27]	[0.27]			[0.07]	[0.09]
Treatment $\times$ UR_intent			0.27**	0.29**			0.03	0.11
			(0.11)	(0.13)			(0.10)	(0.11)
			[0.00]	[0.00]			[0.24]	[0.07]
UR_intent	0.08	0.07	-0.10*	-0.12	0.03	0.02	0.01	-0.04
	(0.07)	(0.08)	(0.05)	(0.08)	(0.05)	(0.05)	(0.07)	(0.08)
Turnout_intent	-0.01	-0.00	-0.00	0.00	-0.06	-0.04	-0.07	-0.04
	(0.06)	(0.07)	(0.06)	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	270	267	270	267	213	212	213	212
R2	0.09	0.25	0.10	0.27	0.12	0.35	0.12	0.35

Notes: Robust standard errors are in parentheses; p-values for the estimated marginal effects conditional on the intent to vote for United Russia are in brackets. Linear probability model. All estimations include fixed effects for the city and the day of the initial interview. Controls include age, dummies for employment, levels of education, categories for material well-being, news sources, internet usage, watching TV Rain. previously. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Second wave: outcome variables. Our main outcome variables for the individual-level experiment are turnout and voting for United Russia during the 2016 Duma elections. We construct the variable *Individual turnout* as a binary variable that equals one if a respondent answered in the second wave that they participated in the election, and zero otherwise. Similarly, the binary variable *Vote for United Russia* equals one if a respondent indicated in the second wave that they voted for United Russia during the election, and zero otherwise.²³

5.2 Results for the Subgroup of Social Media News Consumers

Table 5 presents the results of our experiment for those respondents who indicated that they use social media as one of their primary sources of news. In the linear estimations, we do not find an effect of our treatment on either turnout (columns 1 and 2) or voting for United Russia (columns 5 and 6). These results suggest that for the social media subgroup, the

²³Compared to the question on turnout, for which all respondents provided an answer, this second question was more sensitive, resulting in a non response rate of about 10% for each group. The non response was not correlated with our treatment. We assign missing values for the variable *Vote for United Russia* in case of a non-response.

average treatment effect was negligible.

However, the nonlinear estimations indicate that the treatment mobilized regime supporters to attend the election (columns 3 and 4) and to vote for the ruling party (columns 7 and 8). The effect we find is substantial: our treatment increased the likelihood of regime supporters attending the election and voting for United Russia by about one-third.

The treatment led to a reduction in turnout and support for the government party among voters who were not initially planning to vote for it. These effects, however, are not statistically significant, perhaps, because for this subgroup the treatment did not translate into an increase in watching the independent channel (see Table 4).

Overall, the results indicate that for active users of social media, getting access to independent media had a polarizing effect, which is consistent with the results of the city-level experiment.

Table 5: Results for the Group of Respondents Who Consume News From Social Media

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	News from social media							
Dependent variable:	Individual turnout				Vote for United Russia			
Treatment	-0.07 (0.09)	-0.03 (0.09)	-0.14 (0.10) [0.14]	-0.11 (0.10) [0.18]	-0.02 (0.07)	0.00 (0.07)	-0.07 (0.07) [0.30]	-0.03 (0.07) [0.48]
Treatment $\times$ UR_intent			0.43*** (0.15) [0.06]	0.48*** (0.16) [0.01]			0.44*** (0.16) [0.02]	0.36** (0.15) [0.02]
UR_intent	0.27*** (0.07)	0.27*** (0.08)	-0.01 (0.14)	-0.05 (0.14)	0.63*** (0.08)	0.67*** (0.07)	0.35** (0.15)	0.42*** (0.14)
Turnout_intent	0.53*** (0.05)	0.47*** (0.07)	0.54*** (0.05)	0.48*** (0.07)	0.22*** (0.04)	0.16*** (0.05)	0.22*** (0.04)	0.17*** (0.05)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	270	267	270	267	244	241	244	241
R2	0.25	0.39	0.27	0.41	0.40	0.50	0.42	0.52

Notes: Robust standard errors are in parentheses; p-values for the estimated marginal effects conditional on the intent to vote for United Russia are in brackets. Linear probability model. All estimations include fixed effects for the city and the day of the initial interview. Controls include age, dummies for employment, levels of education, categories for material well-being, news sources, internet usage, and watching TV Rain previously. * indicates $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Results for the Subgroup of Traditional Media News Consumers

The results of our experiment for those respondents who indicated that they use only traditional media as their sources of news are presented in Table 6. While we do not find a significant effect of the subscription offer on individual turnout in either the linear or the nonlinear specifications (columns 1 to 4), there is a strong negative effect on voting for United Russia for the whole subgroup, regardless of the prior voting intentions (columns 5 to 8). On average, respondents were 25 percentage points less likely to vote for the ruling party when offered free access to TV Rain (column 6).

Overall, our results suggest that while exposure to independent media can convince viewers to change their minds in a traditional media setting, in the more polarized environment of social media, the same message might instead mobilize regime supporters.

Table 6: Results for the Group of Respondents Who Do Not Consume News From Social Media

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	No news from social media							
Dependent variable:	Individual turnout				Vote for United Russia			
Treatment	-0.05 (0.09)	-0.11 (0.09)	-0.05 (0.10) [0.62]	-0.10 (0.10) [0.34]	-0.21*** (0.06)	-0.25*** (0.07)	-0.20*** (0.06) [0.00]	-0.24*** (0.08) [0.00]
Treatment $\times$ UR_intent			-0.03 (0.17) [0.64]	-0.08 (0.17) [0.28]			-0.04 (0.17) [0.11]	-0.06 (0.16) [0.07]
UR_intent	0.02 (0.09)	0.02 (0.09)	0.04 (0.13)	0.07 (0.13)	0.47*** (0.08)	0.48*** (0.09)	0.49*** (0.13)	0.51*** (0.13)
Turnout_intent	0.59*** (0.07)	0.52*** (0.08)	0.59*** (0.07)	0.52*** (0.08)	0.14*** (0.05)	0.07 (0.07)	0.14*** (0.05)	0.07 (0.07)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	213	212	213	212	191	190	191	190
R2	0.23	0.40	0.23	0.40	0.34	0.47	0.34	0.47

Notes: Robust standard errors are in parentheses; p-values for the estimated marginal effects conditional on the intent to vote for United Russia are in brackets. Linear probability model. All estimations include fixed effects for the city and the day of the initial interview. Controls include age, dummies for employment, levels of education, categories for material well-being, news sources, internet usage, and watching TV Rain previously.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.4 Social Media: Self-Selection or Causal Effect?

The results of the individual-level experiment demonstrate that exposure to independent media leads to an increase in polarization in the subsample of social media users. However, it is not clear whether this is driven by the characteristics of the users that self-select into using social media, or whether it reflects a causal effect of social media. To test this question, we employ a propensity-score approach to create a measure that predicts social media news consumption. We use the full set of control variables as in previous estimations and construct the variable *SM_score* that takes values ranging from 0 to 1, with higher values indicating a higher likelihood of consuming news from social media.²⁴ We employ the propensity score as a means to minimize the effect of self-selection into consuming news from social media in two separate tests: (1) using the inverse value of the propensity score as analytical weights ($1/SM_score$); (2) including *SM_score* and its interaction with the treatment variable as two additional controls. For convenience, as a baseline estimation, we use a triple-interaction model of variables for the treatment, social media, and intention to vote for United Russia, instead of splitting the sample into two subgroups as in Subsections 5.2 and 5.3. The triple-interaction model extends the previous equation 4 as follows::

$$\begin{aligned} Outcome_i = & \alpha + \beta_1 Treatment_i + \beta_2 Treatment_i \times UR_intent_i + \beta_3 Treatment_i \times SM_i + \\ & + \beta_4 UR_intent_i \times SM_i + \beta_5 Treatment_i \times UR_intent_i \times SM_i + \beta_6 SM_i + \quad (5) \\ & + \beta_7 UR_intent_i + \beta_8 Turnout_intent_i + \chi'_i \Psi_1 + \epsilon_i \end{aligned}$$

where *SM* equals one if the respondent consumes news from social media, and zero otherwise. As in the previous sections, we estimate a linear probability model.

Table 7 demonstrates that our baseline results in column 1 for turnout and column 4 for proregime voting are similar to the results from the estimation with split samples. We observe a polarization effect, as represented by the triple-interaction term of the treatment, social media news consumption, and intention to vote for United Russia, which has a positive and statistically significant coefficient for individual turnout (column 1) and voting for the

²⁴Regression results for the propensity-score matching are presented in Appendix C, Table C1.

ruling party (column 4) and a magnitude similar to our split-sample estimation in Table 5.

To establish that social media news consumption played a causal role in polarizing voters, we further minimize the influence of personal characteristics by assigning analytical weights inversely proportional to the propensity score SM_score . The regression results presented in columns (2) and (4) of Table 5 are similar to the baseline estimations, suggesting that polarization is driven by the consumption of social media news, and not the personal characteristics associated with this choice of media.

As an additional robustness check, we also control for the propensity score in equation 5. The results on polarization remain virtually the same for both the effects on turnout and on voting for United Russia (columns 3 and 6). The triple-interaction term between treatment, intention to vote for United Russia, and dummy for using social media remains unchanged in magnitude and statistical significance. These results provide additional evidence that polarization in response to our treatment among social media users is driven by a causal effect of social media, rather than by the self-selection of social media users.

6 Conclusion

Our paper investigates the effect of independent media on electoral support for the regime in an autocracy. By carrying out two complementary field experiments, at the city level and the individual level, we show that for social media users, exposure to an independent online TV channel had a polarizing effect, causing supporters of the regime to mobilize and increase their support for the progovernment party, while the effect is the opposite for nonsupporters. For voters who rely primarily on traditional media as a source of news, the same intervention led to a uniform reduction of support for the progovernment party. Results from a propensity-score analysis suggest that this heterogeneity is driven by a causal effect of social media, rather than by self-selection of social media users.

Social media can affect the way information from independent media is processed by supporters of the regime in several potentially complementary ways. First, it allows people with strong progovernment priors to find information that contradicts information that they receive from independent media. Second, it allows them to discuss this information with

Table 7: Robustness Tests Using Triple Interactions for Actual Social Media News Consumption and Its Propensity Score

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Individual Turnout			Vote for United Russia		
Specification:	Baseline	Weights	Controls	Baseline	Weights	Controls
Treatment	-0.10 (0.10)	-0.08 (0.10)	-0.08 (0.17)	-0.31*** (0.09)	-0.27*** (0.09)	-0.34** (0.17)
Treatment $\times$ UR_intent	-0.15 (0.20)	-0.24 (0.20)	-0.19 (0.32)	-0.11 (0.18)	-0.10 (0.17)	0.17 (0.32)
Treatment $\times$ SM	-0.10 (0.16)	-0.11 (0.16)	-0.08 (0.17)	0.25* (0.13)	0.18 (0.13)	0.26* (0.15)
Treatment $\times$ UR_intent $\times$ SM	0.72** (0.28)	0.75*** (0.27)	0.72** (0.31)	0.44* (0.26)	0.44* (0.25)	0.57* (0.30)
UR_intent $\times$ SM	-0.20 (0.23)	-0.26 (0.21)	-0.15 (0.24)	-0.05 (0.23)	-0.07 (0.21)	-0.15 (0.26)
SM	0.44 (0.75)	0.16 (0.73)	0.44 (0.75)	-1.56** (0.70)	-1.85** (0.75)	-1.65** (0.70)
UR_intent	0.09 (0.14)	0.17 (0.14)	0.22 (0.24)	0.53*** (0.15)	0.53*** (0.14)	0.38 (0.27)
Turnout_intent	0.52*** (0.11)	0.53*** (0.11)	0.51*** (0.11)	0.09 (0.09)	0.11 (0.09)	0.10 (0.09)
Treatment $\times$ SM_score			-0.04 (0.32)			0.05 (0.33)
Treatment $\times$ UR_intent $\times$ SM_score			0.12 (0.70)			-0.69 (0.66)
UR_intent $\times$ SM_score			-0.35 (0.57)			0.48 (0.56)
SM_score			-0.14 (1.00)			1.23 (0.89)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	479	479	479	431	431	431
R2	0.60	0.64	0.60	0.67	0.72	0.67

Notes: Robust standard errors are in parentheses. Linear probability model. Columns (2) and (5) are regressions that use the inverse of the probability of consuming news from social media as analytical weights. All estimations include fixed effects for the city and the day of the initial interview and controls (age, dummies for employment, levels of education, categories for material well-being, news sources, internet usage, and watching TV Rain previously) and their interaction with social media news consumption. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

other people who have similar political positions, who in turn could provide arguments that counter the information they receive from independent media. Participation in such discussions can also strengthen beliefs of the supporters of the regime through self-persuasion (Schwardmann et al., 2022). Finally, the combination of receiving independent news and observing discussions of the opponents of the regime in social media may mobilize the supporters of the pro-governmental party by signaling potential vulnerability of their party in the elections. Unfortunately, our empirical setting does not allow us to separate these two mechanisms, but future work may shed more light on their relative importance.

By showing how the effect of independent media can differ depending on the media environment, we provide a novel explanation for why independent online media often have difficulties challenging authoritarian regimes. As long as online media increase political polarization, they can actually be instrumental to the stability of the regime. While polarization is often considered detrimental to the stability of existing democracies (Abramowitz and McCoy, 2019; Klein, 2020) the effect may be the opposite in autocracies. By providing voters with a simulacrum of political conflict and activity, regime supporters are mobilized to support the regime in authoritarian elections.

This may explain why authoritarian regimes such as Russia often opt to combine relatively free online media with much tighter state control over the traditional mass media. On the one hand, it can make sense for an authoritarian state to keep some media outlets free, as individuals who rely solely on biased information from state television will be less likely to read, watch, or listen to it, rendering excessive propaganda ineffective at the margin (Besley and Prat, 2006; Gehlbach and Sonin, 2014). In addition, the presence of some free media may be beneficial, enabling the autocrat to control and incentivize bureaucrats and thus improve the quality of governance (Egorov et al., 2009). It can also help monitoring dissatisfaction toward the government among citizens and push potential opposition to split, if there is sufficient preference heterogeneity among citizens (Chen and Xu, 2017). But keeping some media outlets completely free from government control is also inherently risky, as independent information on these outlets may disrupt the effectiveness of propaganda (Welch, 1992) created by full media control.²⁵ Our findings that independent news unambiguously reduces

²⁵See Libman and Rochlitz (2019), chapter 4, for a detailed discussion of this informational trade-off in the

government approval among respondents who get their political news mainly from traditional news outlets support the existence of this risk.

This risk, however, seems to be less acute on social media. In our study, we find that access to free online media increases both electoral turnout of regime supporters and votes for the ruling party in voting districts where support was sufficiently high. Given this polarization effect of access to news on free online media, authoritarian regimes may prefer a combination of tolerating relatively free online media while tightly controlling the traditional mass media. As long as the free online media reach a sufficiently small share of voters, and their signals do not reach the traditional mass media audience, this combination may well be an optimal choice for an authoritarian government. This trade-off, however, may change in a situation when access to independent news becomes too dangerous for the regime, which can explain why the remaining independent media outlets were closed in Russia earlier this year, just after the start of the war in Ukraine.

cases of the authoritarian governments in Russia and China.

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Appendix

Appendix A Comparing news coverage

Table A1: Principal News Items on Russian State TV and TV Rain, September 15th, 2016

Channel 1	NTV	TV Rain
1) Vladimir Putin calls on the Russian population to vote on September 18th	2) Meeting of the State Council about Infrastructure Development in Southern Russia	Anti-Corruption NGO FBK reports about Dmitry Medvedev's dacha, allegedly worth 30 billion rubles
2) Infrastructure Development in Southern Russia debated during a meeting of the State Council	3) American athletes were doped during the Rio Olympics	Search in a museum in Novorossya was linked to an explosion in St. Petersburg
3) Hacker group "Fancy Bear" publishes information about doping among US athletes	1) Vladimir Putin calls on the Russian population to vote on September 18th	A big inflatable moon was flying through the streets of the Chinese city Fuzhou
4) Ceasefire on the initiative of Lugansk and Donetsk in the Donbass region is observed	6) The US election campaign is a dirty fight for power, Hillary after health problems comes back	The former customs head of Vnukovo airport hides from the police after corruption charges
5) In Syria the government army is observing the conditions of the ceasefire	4) Ceasefire in the Donbass region	A piece of an airplane found near Tanzania is part of MH370
6) The candidates for the presidential election in the US file their medical history	5) The Russian Ministry of Defense shows live pictures from the siege of Aleppo	The head of Russia's Investigative Committee Alexander Bastrykin no longer has the right to travel in a convoy of cars

Appendix B City level experiment

Table B1: Randomization of the Treatments in the City-Level Experiment

	(1)	(2)		(3)	
	All cities	Full treatment		Free access only	
	Mean	Mean	P-value	Mean	P-value
City size, geography and status					
Population, thousands	249.84	248.99	0.97	242.09	0.88
Number of electoral districts	1.83	1.73	0.72	1.60	0.68
Regional capital, dummy	0.43	0.40	0.79	0.20	0.28
Distance to Moscow, km log	6.18	6.20	0.89	6.48	0.32
Social characteristics in 2011 and 2016					
Average monthly salary, log					
2016	10.34	10.34	0.93	10.40	0.54
2011	9.88	9.90	0.69	9.91	0.76
Unemployment, per 100 population					
2016	0.62	0.69	0.52	0.40	0.27
2011	0.71	0.64	0.46	0.44	0.19
Registered Vkontakte users, log per 1000 population					
2016	6.46	6.64	0.28	5.84	0.07*
2011	5.64	5.82	0.31	5.06	0.11
Electoral outcomes: 2011 Duma election					
Turnout, %	55.31	54.12	0.50	53.25	0.57
Votes for United Russia, %	36.37	34.65	0.41	33.72	0.53
Absentee votes, per 100 registered voters	1.19	1.18	0.96	0.87	0.28
TV Rain viewership prior to the treatment					
TV Rain website pages viewed daily, log per 1000 population					
August 2016	2.87	2.88	0.99	2.80	0.83
November-December 2011 (before 2011 Duma election)	1.90	1.83	0.65	1.92	0.96
N cities	42	15		5	

Note: * $p < 0.1$.

Appendix C Individual level experiment

Table C1: TV Rain Treatment and Attrition From the Second Wave of the survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	News from Social Media				No news from Social Media			
Dependent variable:	Attrition from the second wave							
Treatment	-0.01 (0.06)	-0.01 (0.06)	-0.00 (0.06)	0.01 (0.07)	-0.02 (0.07)	-0.00 (0.07)	-0.01 (0.07)	0.01 (0.08)
Treatment $\times$ UR_intent			-0.04 (0.12)	-0.11 (0.12)			-0.03 (0.12)	-0.08 (0.12)
UR_intent	-0.03 (0.06)	-0.02 (0.06)	-0.00 (0.10)	0.06 (0.10)	-0.06 (0.06)	-0.06 (0.06)	-0.04 (0.10)	-0.01 (0.10)
Turnout_intent	-0.03 (0.05)	-0.01 (0.06)	-0.03 (0.05)	-0.01 (0.06)	-0.03 (0.06)	0.01 (0.07)	-0.03 (0.06)	0.01 (0.07)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	690	685	690	685	521	516	521	516
R2	0.04	0.10	0.04	0.10	0.05	0.12	0.05	0.12

Notes: Robust standard errors are in parentheses, * indicates $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Linear probability model. All estimations include fixed effects for the city and the day of the initial interview. Controls include age, dummies for employment, levels of education, categories for material well-being, news sources, internet usage, watching TV Rain previously.

Table C2: Differences Between Groups With Social Media News Consumption and without

	First wave			Second wave		
	News from social media Mean	No news from social media Mean	P-value	News from social media Mean	No news from social media Mean	P-value
Voting intentions						
Turnout_intent	0.85	0.85	0.81	0.86	0.86	1.00
UR_intent	0.14	0.17	0.21	0.15	0.18	0.30
Internet use: 1 if daily, 0 if 2-3 timers per week	0.90	0.81	0.00***	0.90	0.82	0.01***
Age , in full years	35.24	40.83	0.00***	37.50	42.46	0.00***
Education level						
Primary	0.01	0.02	0.57	0.01	0.02	0.14
Secondary	0.10	0.06	0.02**	0.09	0.05	0.09*
Technical college	0.01	0.02	0.18	0.00	0.02	0.10
Vocational education	0.38	0.33	0.09*	0.38	0.28	0.02**
Unfinished university degree	0.05	0.04	0.50	0.05	0.05	0.81
University degree	0.45	0.53	0.01***	0.45	0.57	0.01***
Doctorate	0.01	0.01	0.82	0.01	0.01	0.85
Material well-being: we can afford to buy ...						
No food	0.03	0.04	0.45	0.04	0.02	0.29
Food, but not clothes,	0.12	0.12	0.95	0.12	0.10	0.49
Food, clothes, but not home appliances	0.33	0.29	0.13	0.34	0.35	0.86
Food, clothes, appliances, but not a car	0.27	0.26	0.66	0.26	0.27	0.77
Food, clothes, appliances, a car, but resources are not unlimited	0.14	0.20	0.01***	0.15	0.18	0.37
Everything	0.07	0.07	0.65	0.06	0.05	0.55
No answer	0.03	0.03	0.69	0.03	0.03	0.98
Employment status						
State-owned firm, public office	0.24	0.22	0.53	0.23	0.23	0.84
Private company	0.35	0.39	0.16	0.34	0.38	0.36
Non-profit organization	0.03	0.03	0.74	0.04	0.03	0.65
Self-employed/entrepreneur	0.10	0.14	0.05**	0.09	0.15	0.04**
Student	0.03	0.02	0.04**	0.03	0.01	0.18
Pensioner	0.08	0.10	0.20	0.11	0.12	0.83
Housekeeping	0.08	0.04	0.01***	0.09	0.04	0.06*
Unemployed, looking for job	0.06	0.04	0.12	0.05	0.02	0.08*
Unemployed, not looking for job	0.01	0.00	0.05*	0.01	0.00	0.07*
No answer	0.01	0.01	0.60	0.00	0.01	0.05*
News source						
Internet (except social media)	0.68	0.58	0.00***	0.69	0.62	0.11
Newspapers	0.38	0.27	0.00***	0.40	0.27	0.00***
Radio Echo MSK	0.09	0.06	0.03**	0.08	0.06	0.48
Radio Mayak	0.26	0.16	0.00***	0.24	0.17	0.04**
Federal TV	0.78	0.77	0.59	0.78	0.77	0.76
Cable TV	0.57	0.32	0.00***	0.55	0.29	0.00***
TV Rain	0.04	0.02	0.08*	0.06	0.03	0.24
Watched TV Rain before	0.17	0.24	0.00***	0.22	0.29	0.08*
Observations	1211	1211		483	483	

Note: * p < 0.1, ** p < 0.05, *** p < 0.01.

Table C3: Randomization in the Individual Experiment By Subgroups, 1st wave

	News from social media			No news from social media		
	Control Mean	Treatment Mean	P-value	Control Mean	Treatment Mean	P-value
Voting intentions						
Turnout_intent	0.87	0.84	0.43	0.85	0.86	0.74
UR_intent	0.14	0.14	0.80	0.17	0.16	0.69
Internet use: 1 if daily, 0 if 2-3 timers per week	0.92	0.89	0.34	0.83	0.80	0.43
Age, in full years	35.98	34.91	0.30	41.82	40.27	0.18
Education level						
Primary	0.01	0.01	0.71	0.03	0.01	0.12
Secondary	0.10	0.09	0.90	0.04	0.07	0.25
Technical college	0.01	0.01	0.88	0.03	0.01	0.12
Vocational education	0.39	0.38	0.76	0.31	0.34	0.48
Unfinished university degree	0.07	0.04	0.11	0.04	0.04	0.73
University degree	0.42	0.46	0.36	0.53	0.52	0.92
Doctorate	0.01	0.01	0.88	0.02	0.01	0.49
Material well-being: we can afford to buy ...						
No food	0.02	0.03	0.46	0.03	0.05	0.28
Food, but not clothes,	0.15	0.11	0.12	0.11	0.12	0.65
Food, clothes, but not home appliances	0.31	0.34	0.51	0.31	0.28	0.51
Food, clothes, appliances, but not a car	0.24	0.29	0.16	0.27	0.25	0.62
Food, clothes, appliances, a car, but resources are not unlimited	0.14	0.14	0.98	0.19	0.20	0.78
Everything	0.08	0.07	0.51	0.07	0.07	0.93
No answer	0.06	0.03	0.04**	0.03	0.03	0.66
Employment status						
State-owned firm, public office	0.27	0.22	0.23	0.19	0.24	0.28
Private company	0.35	0.35	0.96	0.39	0.39	0.91
Non-profit organization	0.01	0.03	0.14	0.03	0.03	0.66
Self-employed/entrepreneur	0.10	0.11	0.91	0.16	0.13	0.43
Student	0.03	0.04	0.83	0.01	0.02	0.50
Pensioner	0.10	0.07	0.17	0.11	0.09	0.46
Housekeeping	0.06	0.10	0.07*	0.04	0.05	0.86
Unemployed, looking for job	0.06	0.07	0.63	0.04	0.04	0.99
Unemployed, not looking for job	0.01	0.01	0.71	0.01	0.00	0.19
No answer	0.01	0.01	0.50	0.02	0.01	0.72
News source						
Internet (except social media)	0.69	0.67	0.58	0.62	0.56	0.21
Newspapers	0.40	0.37	0.49	0.27	0.26	0.79
Radio Echo MSK	0.07	0.10	0.22	0.05	0.06	0.82
Radio Mayak	0.26	0.27	0.89	0.14	0.17	0.37
Federal TV	0.78	0.79	0.80	0.78	0.76	0.70
Cable TV	0.60	0.56	0.44	0.29	0.33	0.31
TV Rain	0.04	0.05	0.59	0.03	0.02	0.46
Watched TV Rain before	0.18	0.16	0.58	0.28	0.22	0.09*
Observations	690	690		521	521	

Note: * $p < 0.1$, ** $p < 0.05$.

Table C4: Randomization in the Individual Experiment By Subgroups, 2nd Wave

	News from social media			No news from social media		
	Control Mean	Treatment Mean	P-value	Control Mean	Treatment Mean	P-value
Voting intentions						
Turnout_intent	0.86	0.86	0.89	0.88	0.84	0.44
UR_intent	0.15	0.15	0.98	0.18	0.18	0.96
Internet use: 1 if daily, 0 if 2-3 timers per week	0.92	0.89	0.53	0.85	0.79	0.31
Age , in full years	37.57	37.46	0.95	41.39	43.27	0.30
Education level						
Primary	0.01	0.01	0.66	0.03	0.02	0.44
Secondary	0.06	0.11	0.22	0.04	0.06	0.64
Technical college	0.00	0.01	0.46	0.03	0.01	0.20
Vocational education	0.43	0.35	0.21	0.24	0.31	0.28
Unfinished university degree	0.08	0.03	0.08*	0.07	0.03	0.27
University degree	0.40	0.48	0.21	0.58	0.57	0.93
Doctorate	0.01	0.01	0.95	0.01	0.01	0.85
Material well-being: we can afford to buy ...						
No food	0.03	0.05	0.58	0.00	0.04	0.05**
Food, but not clothes,	0.15	0.10	0.28	0.10	0.10	0.97
Food, clothes, but not home appliances	0.36	0.34	0.73	0.38	0.33	0.45
Food, clothes, appliances, but not a car	0.19	0.29	0.07*	0.30	0.24	0.29
Food, clothes, appliances, a car, but resources are not unlimited	0.16	0.14	0.74	0.17	0.18	0.88
Everything	0.06	0.06	0.84	0.03	0.06	0.39
No answer	0.05	0.02	0.19	0.01	0.05	0.12
Employment status						
State-owned firm/public office	0.26	0.22	0.40	0.17	0.26	0.12
Private company	0.36	0.34	0.73	0.42	0.36	0.31
Non-profit organization	0.01	0.06	0.06*	0.03	0.03	0.99
Self-employed/entrepreneur	0.05	0.11	0.12	0.15	0.15	0.95
Student	0.03	0.03	0.91	0.01	0.02	0.73
Pensioner	0.14	0.10	0.32	0.12	0.12	0.93
Housekeeping	0.07	0.09	0.62	0.07	0.02	0.15
Unemployed, looking for job	0.05	0.05	0.80	0.02	0.02	0.78
Unemployed, not looking for job	0.02	0.01	0.53	0.00	0.00	.
No answer	0.00	0.00	.	0.00	0.02	0.13
News source						
Internet (except social media)	0.72	0.67	0.48	0.67	0.58	0.16
Newspapers	0.44	0.38	0.35	0.26	0.28	0.75
Radio Echo MSK	0.08	0.07	0.77	0.08	0.05	0.43
Radio Mayak	0.22	0.26	0.51	0.18	0.16	0.59
Federal TV	0.79	0.78	0.82	0.82	0.74	0.17
Cable TV	0.55	0.55	0.91	0.27	0.31	0.59
TV Rain	0.07	0.05	0.34	0.03	0.03	0.99
Watched TV Rain before	0.22	0.21	0.86	0.32	0.27	0.44
Observations	270	270		213	213	

Note: * $p < 0.1$, ** $p < 0.05$.

Figure C1: Coefficient plot for the probit regression used to construct the propensity score for social media news consumption

