Political Cycles of Media Repression

Nikita Zakharov[*♠]

Günther G. Schulze[♠,♣]

University of Freiburg

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Abstract

We analyze media repression in Putin's Russia (2004-2019), a smart dictatorship that mimics democratic institutions, notably free elections and a relatively free press, but disallows them to work as an effective accountability mechanism. Using unique granular data on journalist harassment and the preset and staggered timing of local elections, we show strong political cycles of media repression. Employing monthly data on media tonality, we find that media repression before elections leads to more favorable news reporting. By disciplining the press predominantly during election times, smart dictators can uphold the image of competence and democratic rule while securing their power.

JEL codes: D72, H10, P43

Key words: authoritarian government, smart dictatorships, media repression, political election cycles, media tonality

[*] Corresponding author: Nikita.Zakharov@vwl.uni-freiburg.de

[♠] University of Freiburg, Institute of Economics, Rempartstr. 10, 79085 Freiburg, Germany

[♣] CESifo

1 Introduction

The last few decades have seen the advent of a new type of 'smart' dictators, who rely on creating and maintaining an image of competence and problem-solving skills for securing their rule instead of brute force and open repression. Rather than instilling fear or propagating an ideology that justifies their rule, they seek to convince the general public that they are competent leaders and a good choice. These new autocrats do not suppress opposition like old school dictators such as Josef Stalin, Adolf Hitler, Idi Amin, Kim Yong-Un, or Mao Zedong did; they prevent effective opposition from arising through carefully manipulated information and through mimicking democratic institutions such as general elections and fairly free media, at least in the eyes of the general public. This is the essence of Guriev and Treisman's theory of 'informational autocrats' (Guriev and Treisman 2019, 2020, 2022). Examples of such popular informational autocrats are Hugo Chavez, Alberto Fujimori, Victor Orban – and Vladimir Putin prior to the Russian-Ukrainian war. As open repression would be seen as an admission of incompetence and weakness, the challenge for the smart dictator is to effectively manipulate the media, but to keep this largely from the public eye, to hold elections, but to secure a favorable outcome. How do they do this? This micro-politics of media manipulation in informational autocracies are not very well understood. It is the concern of this paper.

We study targeted repression against journalists in pre-war Russia, a prime example of informational autocracies (Frye 2021). We hypothesize that media repression is heavily concentrated on pre-election periods in order to discipline journalists in the period that matters for electoral accountability. In off-election periods media are expected to be relatively free in order to provide credibility to the media, including to their messages in election periods, and to give legitimacy to the political system. We expect media harassment to be more prevalent prior to important elections, particularly prior to gubernatorial elections, and less prevalent before less important elections, such as regional parliament or city council elections. Media repression can take different forms (violence, detention, criminal prosecution, censorship). We expect those forms to dominate that show the least degree of traceable government involvement in order to maintain the image of a legitimate rule. Lastly, we expect media repression to be effective: an attack on the journalist would encourage reporting by the media to be more favorable for the incumbents. In short, we expect political cycles of media repression to exist and to translate into political cycles in the tonality of messages from the media.

We focus on subnational elections and employ a novel and detailed dataset on the incidents of media harassment at the city level over the last 16 years with monthly frequency. Our estimation strategy takes advantage of a staggered and idiosyncratically predetermined electoral calendar for local elections. The findings confirm our hypotheses: media repression is significantly more prevalent in the three months before local elections. This pattern is observed for various types of media repression, including violent harassment, censorship, and detention by the police, but not for legal actions against the journalists, supporting the hypothesis that electoral autocrats prefer types of repression that do not implicate them directly (Guriev and Treisman 2019). We also find high-stake elections - elections for the executive position at the region (governor) and a city level (mayor) - to produce cycles at a higher rate than elections for regional legislative bodies. Local elections of minor importance – elections to the city council – do not cause a statistically significant increase in media repression.

Furthermore, we demonstrate the disciplining effect of media repression on critical press coverage using a novel monthly media tonality index of regional governors based on the news coverage of over 67 thousand traditional and online news media. We find that an episode of media repression in the

months before the governor's election significantly increases his/her positive news coverage. This finding suggests that the repression of a single media outlet or journalist can send a powerful signal to the journalists' community, coercing them to provide more favorable coverage of an incumbent before the election.

Our paper speaks to different strands of the literature. Foremost, it is related to the business cycle literature (PBC, Nordhaus 1975; Rogoff 1990, Shi and Svensson 2006). Yet, while the PBC literature focuses on incumbents seeking to influence voters' perceptions about their competence by manipulating the real economy, we focus on the manipulation of the media to influence perceptions. While the motivation of incumbents may be related, the institutional setup is very different: such media manipulation is particularly characteristic of autocratic regimes that rule by skillful propaganda and censorship.[1]

Second, our findings advance the literature on the political economy of non-democracies, particularly the theory on informational autocracies by Guriev and Treisman (2019, 2020, 2022) by providing novel empirical evidence of information control – the targeted repression against the press – and by identifying its cyclical nature. Our paper thus details an essential governance technique in informational autocracies.

We also contribute to the literature on determinants of violence and harassment against journalists. Previous literature has focused on several prime determinants of violence against journalists such as corruption (Bjørnskov and Freytag 2016; Hughes and Márquez-Ramírez 2018), regime duration (Solis 2020), and major internal conflicts (VonDoepp and Young 2013). Carey and Gohdes (2021) show that there are more journalist killings in democracies, especially in those that devolve substantial powers to local governments.[2] We focus on subnational elections as a major determinant of violence against the media and are able to identify the cyclical nature of this violence due to the asynchronous and exogenous timing of local elections. Thanks to our unique data set, we can disaggregate our analysis by type of election and type of harassment.

Finally, our results on the disciplining effect of media repression on news coverage add to the literature that explores the various state manipulations of news reporting, such as state-sponsored advertising (Di Tella and Franceschelli 2011), defamation legislation (Stanig 2015), and physical violence against the journalist (Salazar 2019).

The paper is organized as follows. Section 2 describes theoretical considerations and derives testable hypotheses. Section 3 introduces the data on media repression and local elections in Russia. Section 4 presents the main results for political cycles. Section 5 investigates the disciplining effect of targeted media repression on news coverage. Section 6 concludes.

[1] The availability of this instrument may explain why so little evidence of traditional PBC is found in autocracies (Grier and Grier 2000).

[2] Only Mazzaro (2020) looks at elections as a motivation for violence against media – he finds a positive correlation between the intensity of electoral competition and violence against the media in local elections in Venezuela. Yet, as elections are synchronous his analysis is correlational, not causal.

2 Theoretical Considerations and Hypotheses

Smart dictators base their rule on popularity created by an image of their competence and by pseudo-democratic institutions, which mimic real democracies and thereby increase approval – notably a (relatively) free press and general elections. Any open use of repression would undermine this image and, thereby, their rule as such. The same would result if institutions were revealed to be phony and manipulated. While maintaining the image of competence and relative freedom, dictators need to effectively suppress opposition and secure electoral victory.

Key to their rule is to maintain two democratic institutions, at least in principle, but to temporally decouple them so that they cannot work as an effective accountability mechanism. The media are given some degree of freedom during off-election periods, which provides them with some credibility with the general public, but they are disciplined during election periods to send the right signals supporting the autocrat (which are more credible than if media were suppressed throughout). The media are disciplined through selected harassment of journalists, which sends a clear message to the community.[3] This gives us our first hypothesis:

H1: Journalist harassment is cyclical and concentrated in the months prior to the election.

Since violence against journalists needs to be used sparingly in order to uphold the image of freedom and competence of the leader, journalist harassment is concentrated on the important local elections, i.e. those that select the municipal or regional CEOs. Consequently, our second hypothesis is as follows:

H2: Cycles in journalist harassment are much more pronounced for mayoral and gubernatorial elections than for elections for province parliaments or city councils.

Journalist harassment can take a variety of forms. Smart dictators will choose those forms that do not implicate them. For instance, violence can be administered by thugs for hire, and censorship can be effected through firing journalists, removing them from editorial responsibilities etc. by media owners, which does not directly implicate the political leadership. In contrast, detention by the police and especially criminal prosecution directly involve the authorities, even if charges are unrelated to journalists' professional activities. The third hypothesis is:

H3: Cycles in harassment are more pronounced for violence and censorship and less so for criminal prosecutions.

Of course, harassment needs to yield the intended effect to make sense. It should improve the tonality of the media reports on the incumbent and boost his/her popularity. As we have data on popularity only for governors, we formulate the last testable hypothesis as:

H4: The tonality of news coverage about the incumbent improves after an incidence of journalist harassment.

[3] This argument solves the seeming puzzle of independent media being much more common in electoral autocracies than in other types of autocratic regimes (Stier, 2015): holding autocratic elections shifts the political activity of the opposition and public to election times (Harish and Little, 2017; Knutsen et al., 2017) making the critical reporting between elections by an independent media relatively harmless for the regime.

3 Data

Russia provides an ideal setting for testing the four hypotheses. First, local elections are staggered and exogenously predetermined, which allows for identifying electoral cycles. Second, Russian monthly data allow us to look at media repression in high frequency and to detect even short-lived cycles.[4] Third, we are able to use a unique dataset of journalist harassment that categorizes harassment by type. Finally, a unique measurement of the tonality of news coverage of regional governors enables us to test whether media repression has an effect on how favorable the media report on the incumbent and whether this translates into higher popularity.

3.1 Media repression

To measure media repression, we leverage a unique database covering cases of harassment of journalists in Russia.[5] It is assembled by two major non-governmental organizations (NGO) dedicated to protecting the professional rights of the journalist and promoting press freedom: the oldest Russian human-rights NGOs – Glasnost Defense Foundation (GDF), and the Russian Union of Journalists (RJU), one of the largest trade unions for journalists in Europe with over 100,000 members. The large regional networks of GDF and RJU and the active involvement of the journalists' community permit a continuous and complete collection of information about the incidents of media repression all over the country.[6] The data are available starting from 2004, which coincides with the time of the transition of Russia to electoral autocracy (e.g., Gill, 2006; Silitski, 2009, McFall 2021). Our analysis, thus, covers 16 years from 2004 until 2019, excluding the coronavirus pandemic years as potential outliers.[7]

The primary units of the database are short stories describing incidents of violation of the professional or human rights of journalists or news media outlets. Importantly, the stories contain the exact date of the incident and the location – mainly the city's name.[8] Our data show that media repression in Russia is widespread, with 4,801 stories reported in the period under investigation, or about 300 events per year. We exclude the incidents that happened in Moscow and Saint-Petersburg, as it is impossible to disentangle local and national politics for these outliers. We focus specifically on the large provincial cities – the regions' capitals and the non-capital cities with over 100,000 population.[9] Our sample includes 167 cities, almost all of which (92%) had a least one incident of media repression.[10]

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[4] High-frequency data are necessary since the campaign period is so short (two months). Using high frequency data Akhmedov and Zhuravskaya (2004) find sizable, but very short-lived political budget cycles in Russian regions when the country was still a transitional democracy.

[5] The database is published online at http://www.mediaconflicts.org/.

[6] Journalists are likely to be motivated to report their cases since they can obtain crucial legal help and professional assistance for their case from GDF and RJU.

[7] For example, Kofanov et al. (2021) show that the development of coronavirus pandemic was tightly connected to regional politics.

[8] In a small number of cases the location was identified more generally as a region or was missing. We do not include these cases in our analysis.

[9] Cities of smaller size have very few incidents of media repression, probably, due to scarcity of local news media in the first place.

[10] As is standard for subnational studies on Russia we exclude the city of Grozny in the Chechnya Republic (e.g., Schulze et al., 2016).

We aggregate the observations in each city by month, producing a balanced monthly panel dataset (*N=32,064*). We construct the main variable for media repression as a dummy that equals one if at least one incident has occurred in a given city and month. There are 1,828 non-zero observations, equivalent to 6% of all observations. The dummy variable is our preferred choice since the severity of media repression across different episodes cannot be compared reliably. Moreover, most non-zero city-month observations (82%) contain only one incident, while only 4% of observations contain more than two incidents.

We improve on the previous research on media repression by differentiating between types of media repression. The database classifies all the incidents into 11 groups: 1) journalists' deaths (*N of incidents=27*); 2) physical attacks on journalists (*N=519*); 3) attacks on the office of media outlets (*N=7*); 4) physical threats (*N=256*); 5) censorship (*N=389*); 6) seizure of newspaper circulation (*N=108*); 7) lay-offs (*N=108*); 8) blocking of the website (*N=21*); 9) DDoS attacks (*N=40*); 10) detention by police (*N=444*); 11) criminal prosecution (*N=353*);. For our analysis, we group these into four broader categories: First, we identify the violent incidents, summing together physical attacks, threats, and deaths (groups 1-4). This category – "*Violence*" – is the largest group of incidents with 723 non-zero month-city observations, suggesting that violence against journalists is the primary tactic of press intimidation.[11] This type of media repression should be particularly effective in signaling a higher risk of critical news coverage for the journalists' community.

The second category, "*Censorship,*" aggregates media repression episodes that aim to silence journalists by non-violent means. It includes groups 5-9. There are 594 non-zero observations of censorship in our sample.

Finally, we are interested in media repression executed by public authorities – particularly the police and judicial system. We assemble two categories, "*Detention*" and "*Criminal Prosecution*," which incorporate groups (8) and (9), respectively. Both types of media repression have a very similar frequency in our sample, with 376 and 339 non-zero observations for "*Detention*" and "*Criminal Prosecution,*" respectively. The central distinction between the two categories is the involvement of the state power: criminal prosecution is a public process that engages the whole legal system, including police, prosecutors, and judges, and, therefore, is more visible, while detention by the police is a minor and, thus, a less noticeable operation involving only a few police officers. According to the theoretical prediction by Guriev and Treisman (2019), autocrats prefer to disguise their involvement in targeted repression, and, therefore, we expect to see no political cycle in criminal prosecutions against journalists (cf. Hypothesis 3). Similar to our primary measure, we construct a dummy variable for each subcategory that equals one only if the city-month observation contains an incident of the respective category.

3.2 Local elections

We collect data on all regular local elections from 2004 until 2019 from the Central Election Commission of the Russian Federation. There are four types of regular local elections.[12] At the regional level, there are elections of the governor and the regional parliament, and at the city level, there are elections of the mayor and the city council. The elections for city councils and regional parliaments occurred regularly every 4-5 years, depending on the local electoral legislation. Regional governor elections were replaced by a presidential appointment system in 2005, resumed in 2012, and have

[11] It is also worth mentioning that police in Russia often mischaracterizes violence against journalist as general street crime and is notoriously incapable of solving these violent incidents (CPJ, 2009).

[12] We do not include local referenda as they are rare and issue-oriented.

been held every five years since then. Mayor elections were held regularly every 4-5 years in almost every city at the start of the period under investigation, but they were gradually replaced by an appointment system leaving only about ten cities with direct mayor elections in 2019. The gradual elimination of the mayor's elections was potentially an endogenous decision, and recent research by Reuter et al. (2016) shows that the mayor elections were continued only in the cities with strong political machines of the local elites – hence less contested elections. Since the highly contested elections that often require political manipulations were canceled, our estimates are likely to understate the actual persistence of the media repression cycle for this type of election. Additionally, the availability of political appointments of governors and mayors in our timeframe allows us to use them as a placebo treatment since critical reporting is unlikely to sabotage the appointment. We present the frequency of local elections by year in Appendix A, Figure A.1.

Our estimation strategy takes advantage of the exogenous timing of local elections in Russia due to the predetermined and staggered electoral calendar. This calendar is a product of an administrative reform introduced in the early post-Soviet period that enabled sub-national units to introduce local elections on an ad hoc basis. In the case of governor elections, the calendar endured the introduction of the appointment system and the subsequent re-introduction of elections: appointments took over the timing of the elections (Sidorkin and Vorobyev 2018), and when elections resumed after 2012, they continued the original, idiosyncratic schedule. Additionally, the exact timing of the election within the year is predetermined by a single-voting day, a special date for holding local elections for all the subnational units where elections are due.[13] Single-voting days eliminate concerns about local elites' strategic maneuvering of electoral timing.

Finally, Russian local elections have a relatively short pre-electoral period: the announcement of the election and registration of the candidates begin not earlier than 100-90 days before the voting day.[14] Thus, we expect the media repression cycle to manifest within the three months-interval.

3.3 Tonality of the news coverage

We employ a tonality index of press reporting about regional governors assembled and published by Medialogia, a leading Russian news monitoring agency, to test whether media repression enhances the favorability of news reporting before the governor's election. The index is a monthly measure based on the daily monitoring of over 67,000 sources of registered mass media sources, including national and local TV channels, newspapers, websites, radio, and blogs. The monitoring automatically identifies each news piece mentioning the name of each regional governor, weights it by the size of the potential audience (e.g., newspaper readership, TV viewership, or approximate views for the website or a blog), and categories it as a positive or a negative depending on the tonality. As a result, each news piece is assigned a value from -1000 to +1000, where the higher value indicates higher favorability of the news piece. The tonality index is a sum of the values of all individual news pieces

[13] The single voting day concept was legislatively introduced in 2005, appointing two days for each year to be an election day for all due elections: the second Sunday of March and October. Since 2012, the single voting days have been transformed into one single day taking place on the second Sunday of September. In 2016, it was moved to the third Sunday of September.

[14] Whether it is 100 or 90 days before the election depends on the local electoral regulations.

over one month that we divide by 1000 for convenience.[15] The index is available for all regional governors for the years 2017-2019.

Alongside the index, Medialogia also publishes the overall number of news mentions, allowing us to test whether media repression also changes the number of articles mentioning the governor.

4 Empirical Approach and Results

4.1 Main results

Our estimation strategy follows the approach commonly used in the political business cycle (PBC) literature, including Akhmedov and Zhuravskaya (2004), the only other study on PBC in Russia.[16]

We estimate the change in the risk of the incidence of media repression using a modified Poisson regression with high-dimensional fixed effects and standard errors clustered at the region level (Correia et al., 2020).[17] The estimation equation is the following:

$$Y_{it} = \sum_{j\in\{-1\ ;12\}} a_j\, Election_{jit} + \beta\,(L)Y_{it-1} + \tau_t + \lambda_{is} + \varepsilon_{it}\,, \qquad (1)$$

where *i* and *t* identify cities and time in months, respectively; Y_{it} is the occurrence of media repression in a city-month; $Election_{jit}$ is a dummy that equals one if *t* is *j* months away from elections (*j* equals one in the month of the election, takes negative values before the elections, and positive after); τ_t is a full set of time fixed effects, one for each month *t*; λ_{is} is a dummy for each of the twelve calendar months in each city that accounts for city-specific seasonality and city-specific fixed effects. We include the lag polynomial of the dependent variable, $\beta\,(L)Y_{it-1}$, to account for autocorrelation (where *L=5*).[18]

We present our main result in Figure 1 by plotting the coefficients for the election month and twelve months before and after the election. We find a large and statistically significant electoral cycle before local elections: media repression becomes more than one and a half times more likely in the month of elections and one month before. The magnitude is remarkable: an average risk of a journalist or a media outlet being harassed in any single month is about 5.7%, but it increases to 9% before the elections. Our results thus support our first hypothesis.

[15] The methodology of the index is available at Medialogia website:
https://www.mlg.ru/about/technologies/#mediaindex

[16] We improve upon the approach of Akhmedov and Zhuravskaya (2004), who also use monthly data, in several aspects. First, we focus on cities rather than on regions, and thus on more numerous, and more fine grained entities, and cover period twice as long. Second, we investigate the effect of all types of local elections and not only governor elections. Third, and most important, we can treat the timing of the elections as truly exogenous due to a fixed single-voting day that had not yet been introduced in the period that Akhmedov and Zhuravskaya (2004) investigated.

[17] Modified Poisson regression is a preferred estimation technique for correlated binary data with a large number of clusters and high-dimensional fixed effects (e.g., Zou and Donner, 2013). Alternatively, logistic regression and linear probability models produce very similar results.

[18] The optimal number of five lags is calculated using the Akaike criterion. We use this number of lags for all specifications for the sake of uniformity, but changing the number of lags to the optimal for other estimations separately does not affect the results.

4.2 Cycles by type of harassment

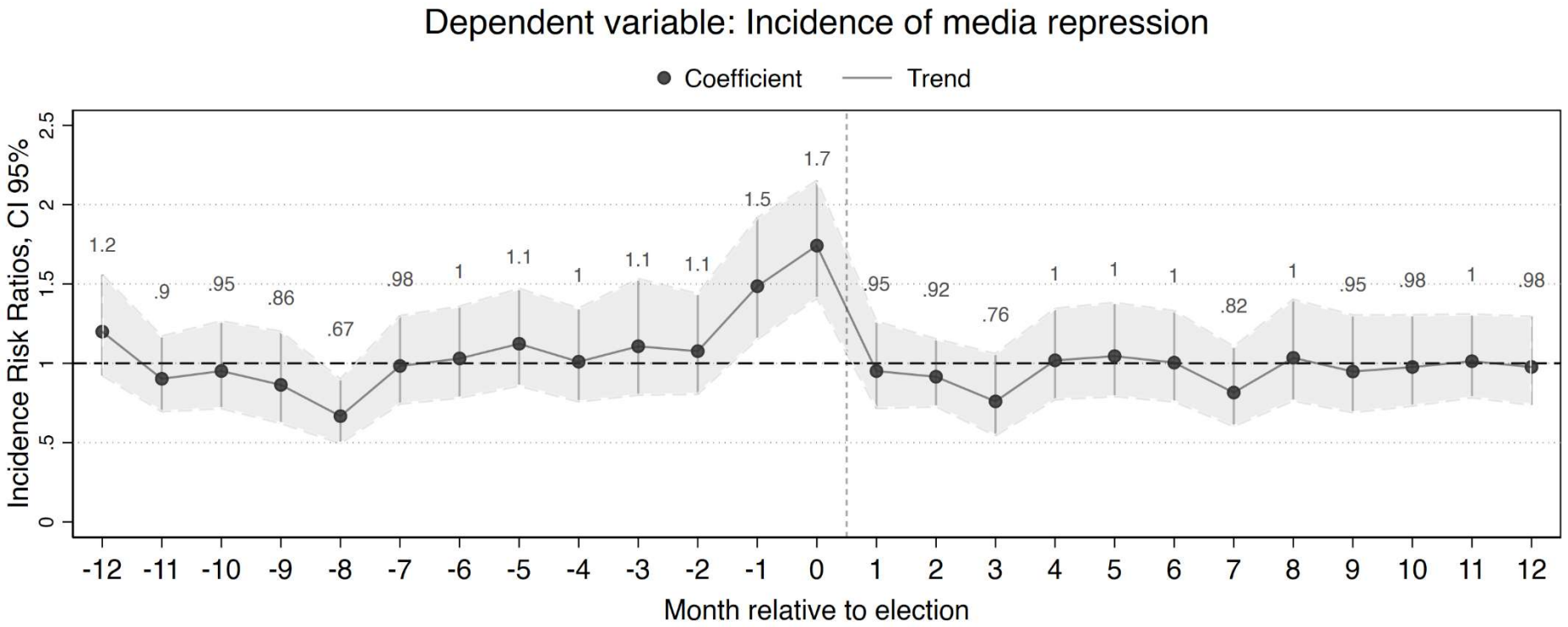
We disaggregate journalist harassment by type; results are presented in Figure 2. We observe sizable electoral cycles for the largest category of media repression, "*Violence*" (Panel A). A significant increase in the risk of violence against journalists is observed in each of the three months of the pre-electoral period, in line with our expected cycle length since the elections officially start around 80-100 days before the election day. Violence against journalists is already very frequent, with about a 2.3 percent risk of occurring in a given city in an average month, but this risk rises to almost 4 percent during the pre-election period.

"*Censorship*" is our second most common category of media repression, with an incidence risk of 1.9% in an average month. In Panel B, we see positive and significant coefficients for the election month and one month before the elections indicating an almost two-fold increase in the risk of journalists being actively silenced.

Media repression of both categories, "*Detention*" and "*Criminal Prosecution,*" require direct involvement of the law-enforcement authorities to a varying degree: detentions are carried out mainly by police, while legal actions against journalists also involve judges and prosecutors. Following Guriev and Treisman's (2019: 102) hypothesis that autocrats seek to camouflage state involvement in targeted repressions, we expect political cycles in both categories to be less prominent. The estimation results are presented in Panels C and D for "*Detention*" and "*Criminal Prosecution,*" respectively. We find a statistically significant but relatively short-lived electoral cycle for detentions of journalists by the police: the risk of being detained increases from 1.2% to 2.5% only in the election month. In contrast, the risk of criminal prosecution for journalists is not statistically different in the pre-electoral period compared to an average month.

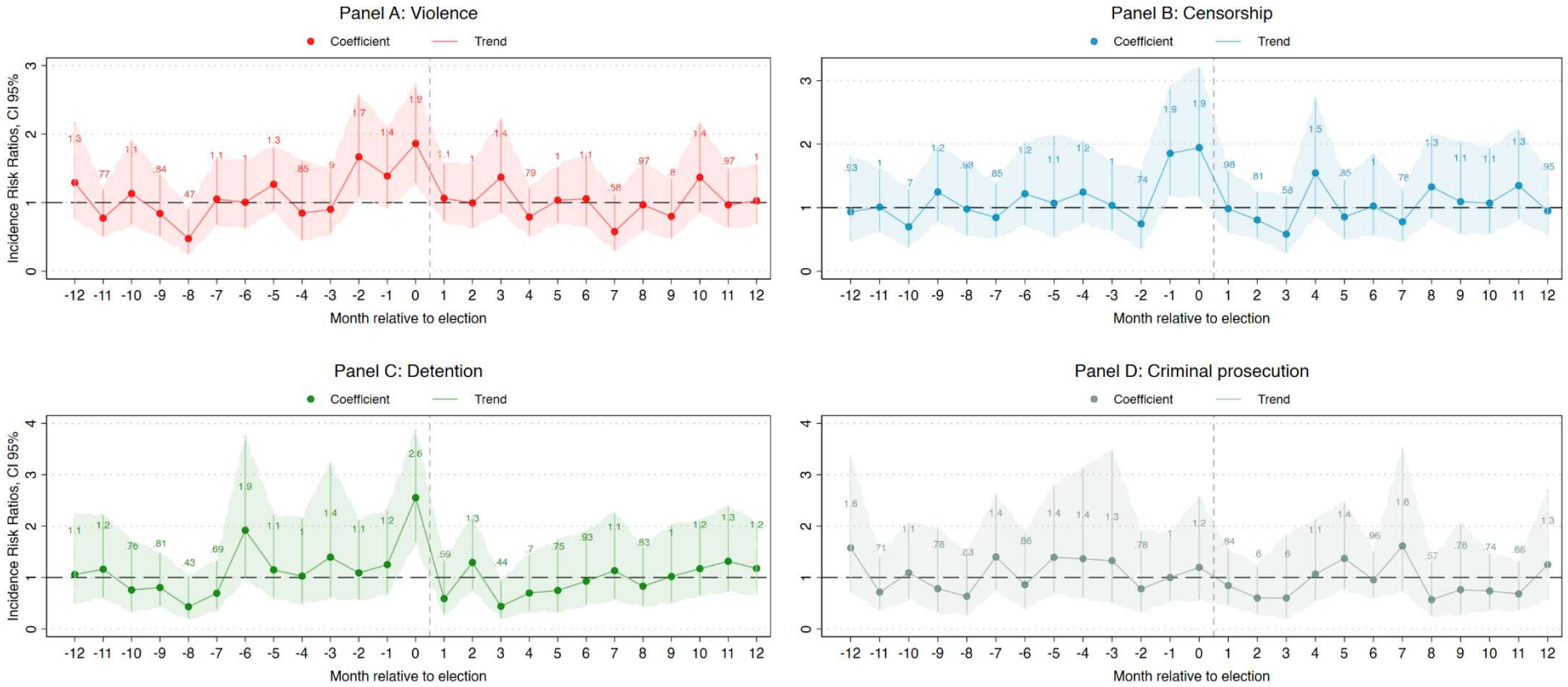
In sum, cycles are more pronounced for forms of repression with less discernible government involvement. This finding supports our third hypothesis.

Figure 1: Political cycles of media repression



Note: Coefficients represent incidence risk ratios with CI 95%. Modified Poisson regression with mitlidimensional fixed effects includes time FE and city-calendar month FE. Robust SE are clustered at the region level.

Figure 2: Political cycles by type of the repression



Note: Coefficients represent incidence risk ratios with CI 95%. All regressions include five lag-polinomial, full set of time dummies and fixed effects for each city-calendar month. Robust SE are clustered at the region level.

4.3 Cycles by type of elections

We further investigate whether the effect differs by the type of elections. We modify the estimation equation (1) by substituting the monthly dummies for the occurrence of any local election with the dummy variables for the pre-electoral period (3 months, including the election month) for all four types of local elections. We also add similar dummy variables for three months before the appointments of mayors and governors where applicable. The new equation is the following:

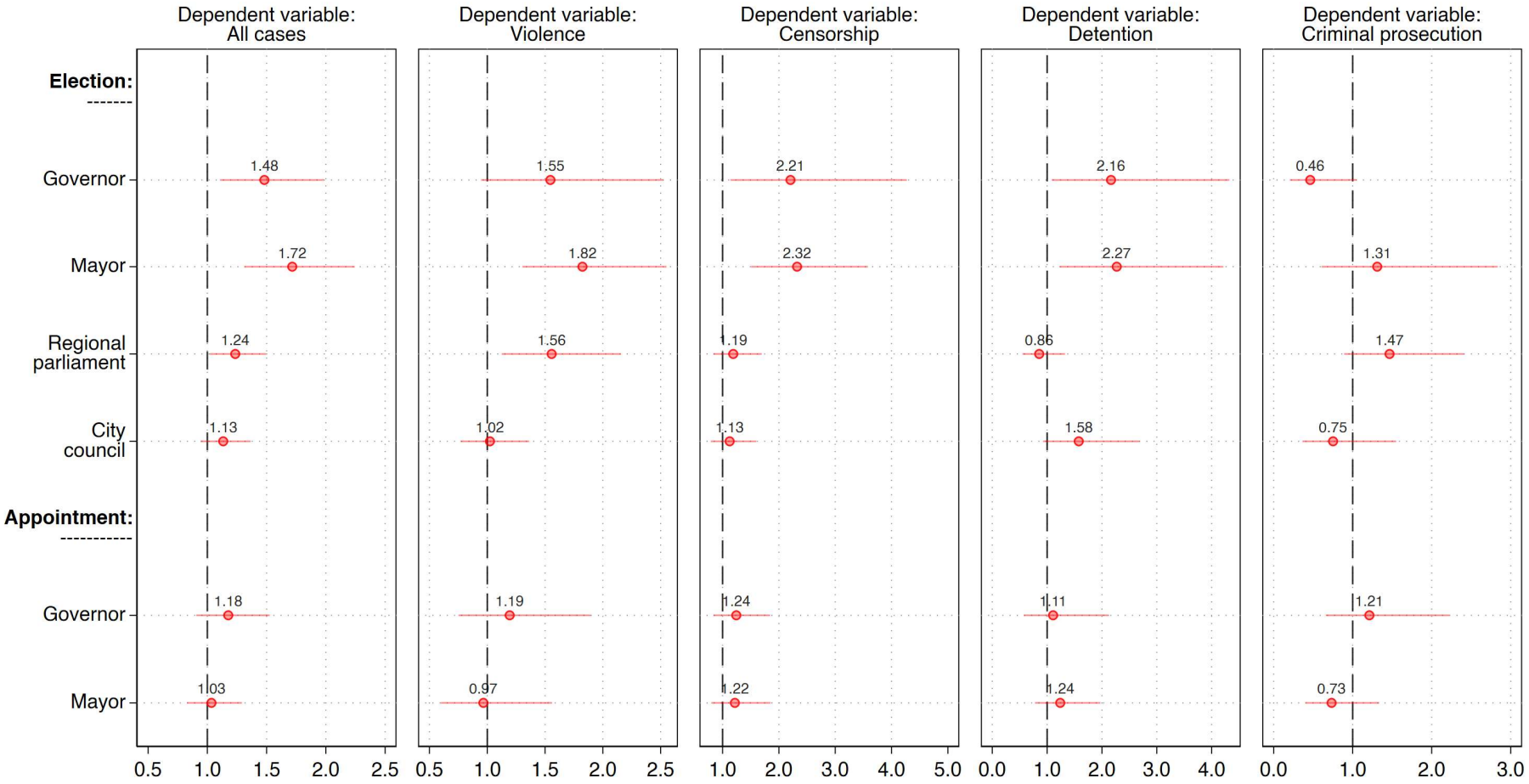
$$Y_{it} = \beta\ Governor\ election_{it} + \gamma\ Mayor\ election_{it} + \delta\ Regional\ parliament\ election_{it} + \zeta\ City\ council\ election_{it} + \eta\ Governor\ appointment_{it} + \mu\ Mayor\ appointment_{it} + \beta\ (L)Y_{it-1} + \tau_t + \lambda_{is} + \varepsilon_{it}\,. \quad (2)$$

Estimation results are presented in Figure 3. Elections for the governor and mayor executive positions show the most pronounced political cycles: the likelihood of media repression rises by 48 and 72 percent in the three months before the governor and mayor elections, respectively. These political cycles occur in violent incidents, censorship, and detentions by the police, but not in criminal prosecutions (just as for the aggregate figures). When we look at the effect of Mayors' and governors' appointments as a placebo test, we find no such pattern as the lower panel of Figure 3 demonstrates.

Elections to the regional parliaments also produce statistically significant political cycles in media repression, yet of a smaller size and only for violent repression. These elections are still important for the ruling elite, and winning the elections provides significant monetary and political benefits, but these benefits have to be shared among a larger set of participants which should explain the relatively smaller size of the cycle.

The coefficients for city council elections are slightly above one but are never statistically significant. These elections are of minor importance since the political power in the city council is not only shared among the council members but is also relatively marginal. Therefore, it makes sense that we do not find political cycles in such cases. Overall, political harassment cycles are more pronounced for CEO elections (governors and mayors) than for parliament elections, and they are stronger for more important positions. This finding supports our Hypothesis 2.

Figure 3: Political cycles by type of local elections



Note: Coefficients represent incidence risk ratios with CI 95%. Independent variables are dummies that equal one in the election month and two preceding months.
All regressions include five lags, full set of time dummies and fixed effects for each city-calendar month. Robust SE are clustered at region level.

5 Disciplining effect of media repression during the election period

Does media repression positively affect how the local press reports about the politicians during the election period? Obviously, increased harassment during election times would not make sense if it were ineffective. Its persistence therefore suggests that media harassment is effective. Yet, we are able to test this directly by employing a novel monthly dataset on the media popularity of the regional governors. The data are available for the years 2017-2019, during which 51 out of a total of 79 regions held governor elections, and 17 (33%) of those elections were preceded by at least one episode of media repression in the three pre-electoral months. Thus, we can analyze how media repression in the pre-electoral period influences news reporting by the press. The timeframe also includes 24 regional parliamentary elections, 7 (23%) of which were preceded by media repression during the three months before the election day. We include them in our analysis as a placebo test since media repression associated exclusively with parliamentary elections should not affect the tonality of news reporting about the governor.

We estimate tonality and media reporting using OLS with monthly regional data and robust clustered errors. Our estimation includes, as before, a full set of time dummies, city-calendar month fixed effects to account for city-specific seasonality, and a five lag-polynomial to account for autocorrelation. The estimation equation is:

$$Y_{it} = \beta\, Governor\ election_{it} + \sigma\, Regional\ parliament\ election_{\ it} + \mu\, Media\ repression_{it} + \eta\, Governor\ election_{it} \times Media\ repression_{it} + \zeta\, Regional\ parliament\ election_{it} \times Media\ repression_{it} + \beta\,(L) Y_{it-1} + \tau_t + \lambda_{is} + \varepsilon_{it}\,, \quad (3)$$

where Y_{it} is either the tonality index or the number of mentions of the governor in the news (see Section 3.3) in each region *i* and month *t*; $Governor\ election_{it}$ and $Regional\ parliament\ election_{\ it}$ are dummies that equal one in the respective election month and two months before that. The interaction of media repression with a period before governor elections is our variable of interest as its positive coefficient would confirm the existence of the disciplining effect of media repression on reporting. At the same time, the interaction of media repression with the pre-electoral period for regional parliament elections is a placebo treatment because the spillovers of media repression prior to these elections should not affect press reporting about the governor.

The estimation results are plotted in Figure 4. Unsurprisingly, we find that the tonality of press reporting about the governor significantly increases in the three months before the governor's election day by 9.4 points or about 29% of its mean value (left panel of Figure 4). The elections to regional parliaments slightly decrease the tonality index; however, the effect is statistically significant only at the 10% level. Media repression is, in normal times, negatively associated with the tonality of the press reporting about the governor, potentially suggesting an unfavorable reporting itself to be a factor that drives repression. In the pre-electoral period media repression coincides with a large increase in how favorably the media report about the governor. The magnitude is almost the same as the increase due to upcoming elections; in other words, repression doubles the positive effect of the campaign period on the tonality of press reporting. Finally, repression before the parliament elections has no effect on the tonality index in the news for the governor, as we have expected.

The number of news mentioning the governor, however, does not change following repression, neither for gubernatorial nor for parliament elections nor outside election times (right panel of Figure 4). (The governor is more reported on during election times, as one would expect.)

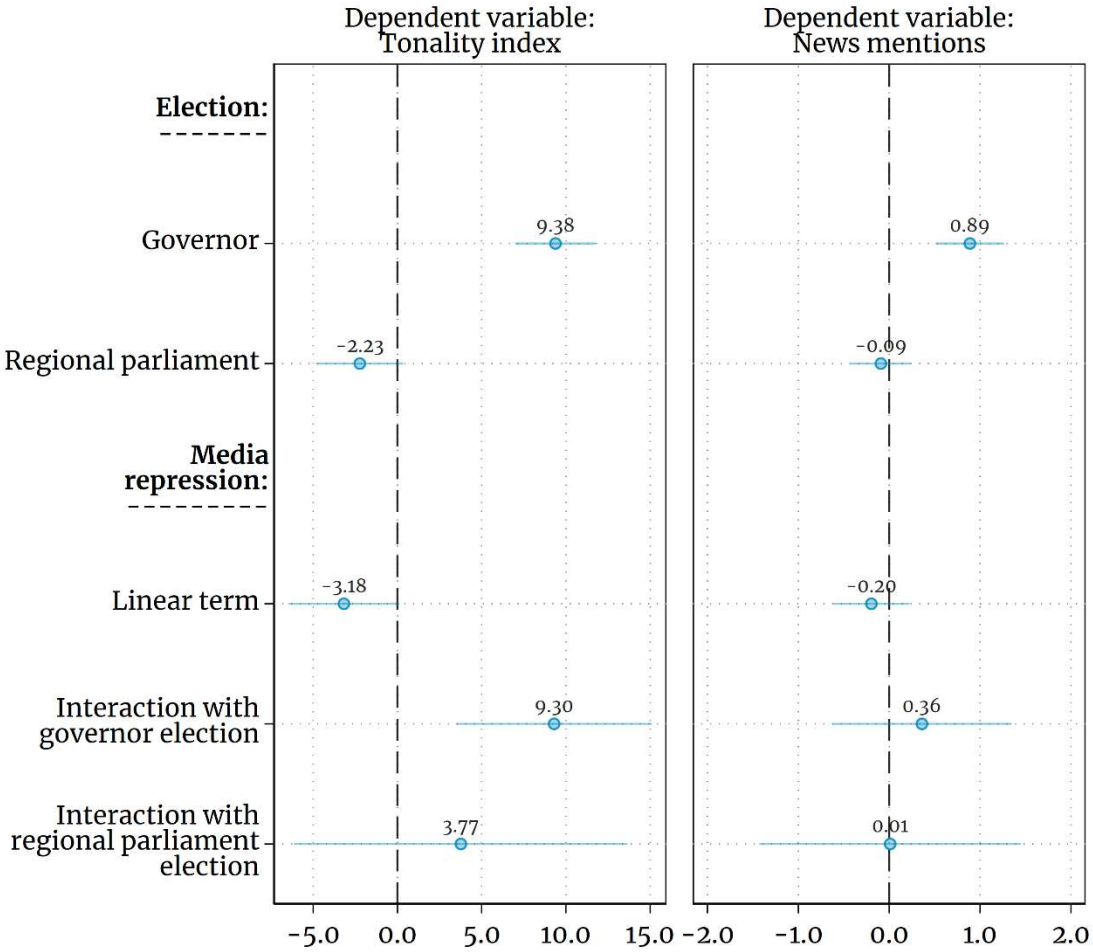
Our findings, thus, confirm the fourth hypothesis: media repression of journalists in the pre-election period improves the tonality (not the quantity) of media reports.

This disciplining effect is crucial for an incumbent as it directly translates into higher public and electoral support during the election. In Figure 5, Panel A, we plot the pre-election increase in the tonality index (measured as its average value in the election month and two months before it and divided by the average value over the whole period, in percentage) against the winning margin between the first and second place in the election for each of the 51 governors' elections. The plot demonstrates a strong positive relationship between the favorability of press coverage before the election and the electoral results. Additionally, since Russian electoral results often suffer from electoral fraud (Myagkov et al., 2005; Enikolopov et al., 2013; Harvey, 2016; Skovoroda and Lankina, 2017), we also assemble a regional estimate of governors' approval using national representative opinion surveys carried out monthly by an established independent Russian polling organization, Levada-Center. We construct the pre-election approval rating using the share of positive answers to the question "do you approve the performance of the governor of your region?" by all respondents in the region surveyed in the election month and two months before.[19] We plot the approval rating against the increase in the tonality measure before the governors' elections and again observe a strong positive relationship.

We conclude that, in short, journalist harassment is effective as it coerces the press into favorable reporting, which is associated with better electoral results and higher public approval.

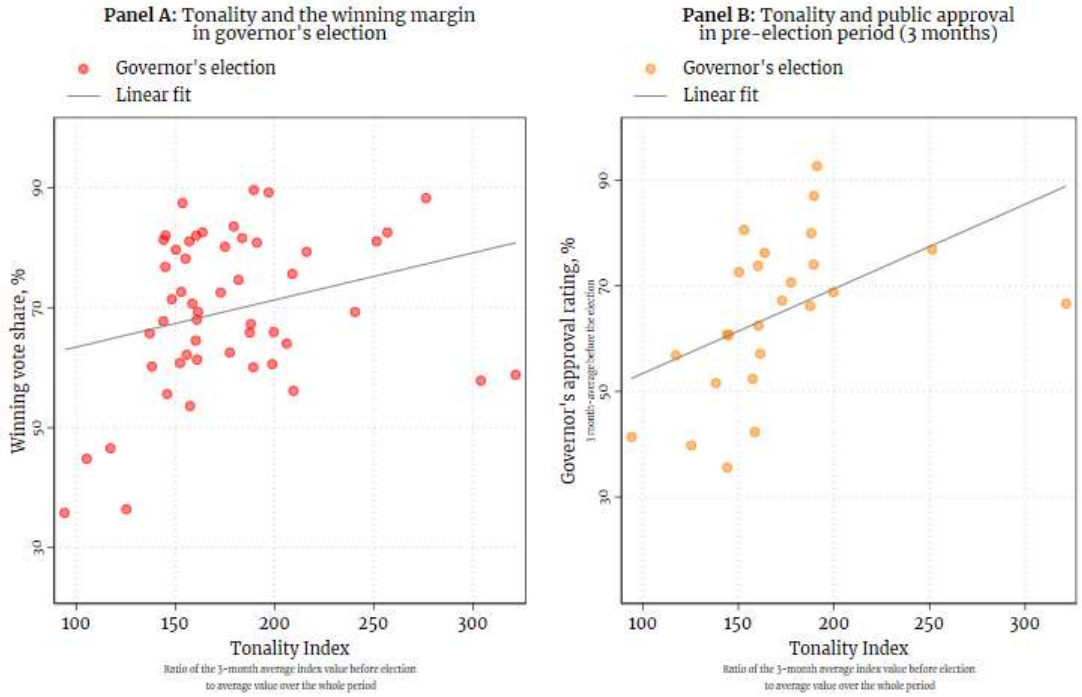
[19] The survey data is available only for 25 governor elections as smaller regions are not covered.

Figure 4: Disciplining effect of media repression on press reporting



Note: OLS regression, CI 95%. All regressions include five lags,full set of time dummies and fixed effects for each city-calendar month. Robust SE are clustered at region level.

Figure 5: Tonality of the press reporting and popularity of the governor in the elections



6 Conclusion

This paper has focused on a central mechanism that smart dictators – informational autocrats in the terminology of Guriev and Treisman – use to secure their rule. Seeking to maintain the image of competence and adherence to democratic principles, they allow two key elements of functioning democracies, a free press and general elections, to exist, but not at the same time. By temporally decoupling them, smart dictators disallow both elements to function as effective accountability mechanisms. While the press is relatively free during off-election times, thereby gaining reputation and credibility, they are closely and effectively disciplined during election times. Critical voices are silenced during critical times.

As a consequence, we observe political cycles of media repression, which we establish empirically using rich data on the repression of journalists in Russian cities and taking advantage of the predetermined, staggered electoral timing for identifying a causal effect of elections on media repression. Our results show a 1.5-3 fold increase in the risk of physical attacks on journalists, censorship, or detention by the police in the three months before the elections. This suggests that harassment against the press is not personal but a strategic instrument as it coerces other journalists to refrain from critical reporting. We confirm this hypothesis by providing empirical evidence that in the three months before the governor election, media repression is strongly associated with a more favorable tonality of the press reporting about the incumbent governor.

Our findings explain a recently observed seeming paradox of independent media being substantially more common in electoral autocracies (Stier 2015): during off-election times, relatively free media do not jeopardize the smart dictator's rule, on the contrary, they serve as a smokescreen to veil autocratic governance. When they might pose a threat during election times, critical media are effectively silenced.

Free press and free elections are essential ingredients of democracies, but they work effectively only in tandem.

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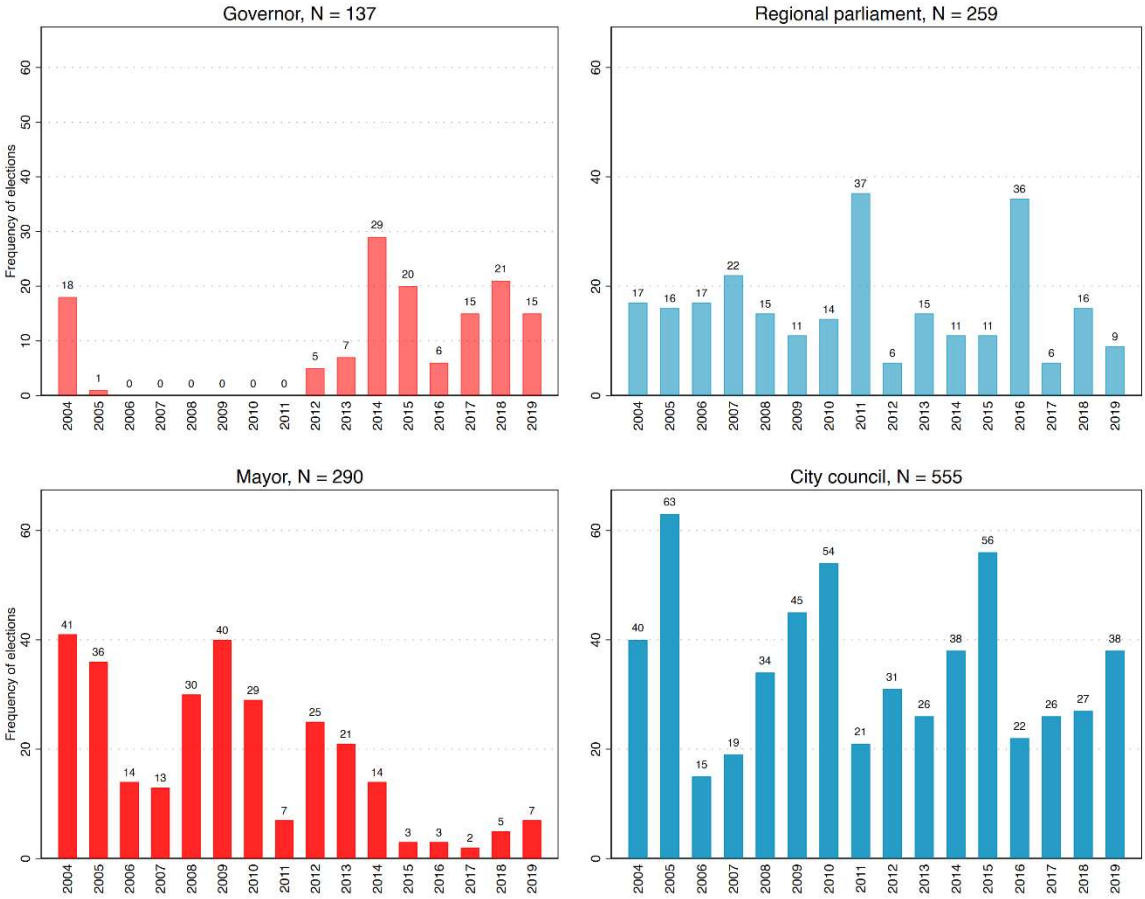
Appendix

Figure A.1: Frequency of local elections in Russia



Note: *The figure reports the frequency of the distinct elections occurring in a particular year in Russia. Hence, while the number of city-level elections coincides with the number of non-zero city-month observations for respective elections in the dataset, there are almost twice more non-zero city-month observations for regional elections.*