

Electoral Incentives, Partisan Identity, and Local Police Responses to the 2020 Black Lives Matter Protests in U.S. Cities

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This paper assesses the impact of local partisan control on BLM protest incidence and police responses. Using georeferenced event data and a local elections database, we implement a close-elections RDD on a range of protest outcomes. We find evidence that Republican mayoral incumbency causally reduced average citywide BLM protest incidence by 49.7% conditional on demographics. An exploration of channels suggests that this deterrence operated in an ex ante way: Republican mayors did not arrest protesters any more frequently; instead, protesters in Republican cities turned out for fewer total days and persisted for shorter durations. Finally, by exploiting the staggered incidence of mayoral elections, we also assess whether and how the imminence of reelection interacted with party identity to modulate police responses. We find no marginal effect of party identity but a negative main effect of 2020 mayoral reelection on protest incidence among the full sample of cities.

I. Introduction and Context

Following the murder of George Floyd in May 2020, the United States experienced one of the largest waves of demonstrations in its recent history. The Black Lives Matter (BLM) movement mobilized millions in demonstrations against police brutality and racial injustice across hundreds of cities. These protests posed a critical question for local policymakers: How should political incentives govern the optimal police response to these mass protests? This paper focuses on whether elected mayors — who often oversee police departments — prioritized different law enforcement tactics depending on their political party and the intensity of reelection pressures. We leverage a regression discontinuity design (RDD) around election thresholds to identify causal effects of incumbent party identity on protest policing, and we secondarily implement a natural experiment that exploits staggered local election timing (some mayors were up for re-election in 2020, others were not) to apprehend the role of electoral incentives.

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Several media reports have highlighted divergent police responses to BLM protests throughout the country. However, there is a dearth of high-quality, causal evidence on the political economy of these responses. This is the first analysis to unpack the causal relationships between local political arrangements and citywide responses to these protests.

The BLM movement is not only important to understand for its own sake, but it also provides an opportune forum for examining the workings of mass mobilization in fractious, but democratic, contexts. The striking uniformity of the Black Lives Matter movement (in tactic and motive) throughout the continental U.S. provides a neat empirical landscape that allows us to cut past at least some of the unobserved mechanisms that inevitably govern the dynamic game of mobilization and repression more broadly. Participation in BLM was broad, and protester behavior largely non-violent, making it relatively more tenable to chalk observed government responses up to partial equilibrium behavior and use reduced-form methods.

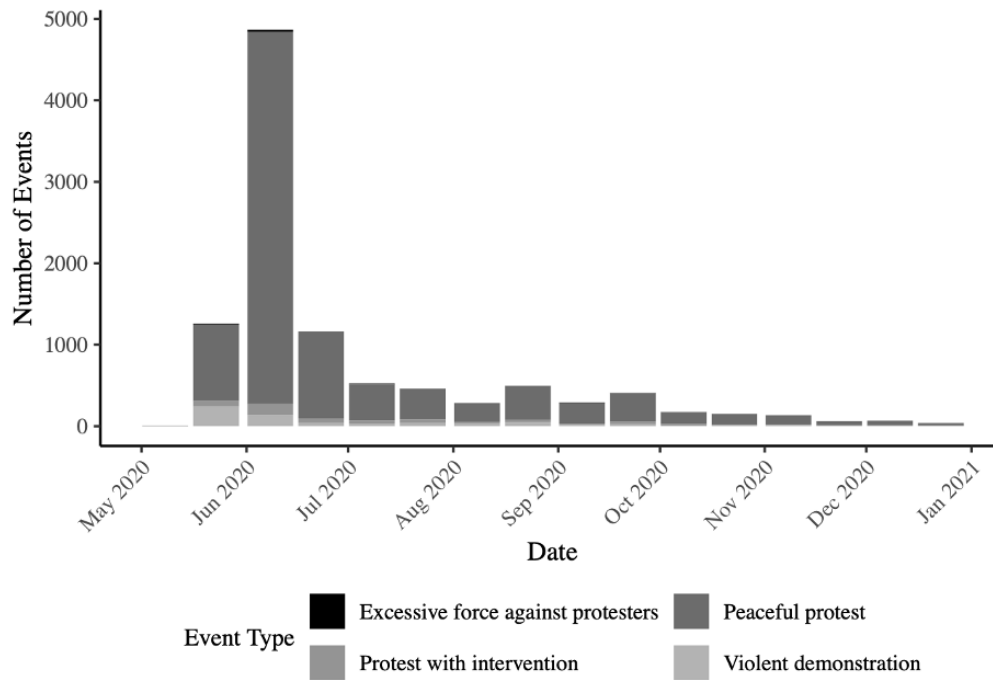


FIGURE 1. HISTOGRAM OF BLM PROTESTS BY TYPE OVER TIME—SOURCE: ACLED

Not only is BLM germane to the broad class of protests in democracies, but the local variations in leadership, demography, and protest dynamics also are ripe for exploring non-obvious questions about local political economy. For example, it is

not clear whether or through what channels local executive and legislative officials are held accountable for the actions of their police departments. Nor is it clear that protesters in a national movement even respond strategically to the identity and behavior of their local officials. We aim to delve into this rich research area using readily accessible datasets and empirical methods.

This paper proceeds as follows. Section II reviews existing literature. Section III outlines the data sources and sample. Sections IV—V examine key results. Section VI concludes.

II. Literature

This paper contributes to several strands of existing work. First, recent economics research emphasizes the role of modern communication technology and networks in facilitating protest mobilization. For instance, Cantoni et al. [2023] provide evidence that technology diffusion significantly increases protest incidence via reduced coordination costs. Additionally, Di Tella and Schargrodsky [2021] document that emotionally charged events can spur widespread mobilization, which governments then struggle to contain.

On the state repression side, the NAACP’s Thurgood Marshall Institute highlights significant disparities in U.S. police responses to racial justice demonstrations compared to other protests, documenting systematic bias in repression efforts [Fund and Institute, 2020]. Such repressive actions have been linked to subsequent changes in protest strategies, illustrating a dynamic protest–repression relationship [Carey, 2006].

This dynamic game between citizens and governments has made it necessary for economists and political scientists to think carefully about observed and unobserved equilibrium behaviors by both citizens and state officials.

Theoretically, protest mobilization aligns with strategic coordination models emphasizing collective action dynamics and threshold behaviors [Barrett and Chen, 2021]. Ritter and Conrad [2016] highlight endogeneity concerns in repression studies, underscoring the necessity of careful identification strategies.

Clever empirical designs have highlighted how communication technology significantly impacts the protesters’ side of the game. Manacorda and Tesei [2020] use a lightning strike IV design to demonstrate that the staggered rollout of mobile phone networks in Africa causally increased protest frequency — especially during economic downturns — by reducing the coordination costs of collective action. Enikolopov et al. [2020] use a creative social network founders’ design to show that access to the Russian social platform *Vkontakte* causally increased anti-government mobilization.

Meanwhile, political incentives also shape government responses to protests. Levitt’s seminal work on electoral cycles highlights how election proximity alters policy outcomes [Aytaç et al., 2017]. Similarly, leadership transitions impact repression severity, suggesting that newly elected or vulnerable incumbents adjust their repressive tactics strategically [Licht and Allen, 2018].

The 2020 BLM protests influenced policy outcomes, as documented by Klein Teeselink and Melios [2025], who found protests boosted Democratic vote shares in the 2020 presidential election through voter mobilization and persuasion. The protests also triggered economic policy responses including discussions around police budgets and reform, though these varied significantly based on local political dynamics [Ebbinghaus, 2024].

In light of insights from the international literature on citizen-government protest dynamics, our paper centers the U.S. context to see whether we can deepen understandings of political incentives and mass mobilization at the micro level, given the profound cultural and policy effects that we already know the BLM movement inspired. In doing so, we also contribute to the already vast body of work that implements close-elections regression discontinuity designs for causal inference.

III. Data and Empirical Strategy

A. Data Sources

Our empirical analysis integrates data from three primary sources. First, we rely on protest event data from the Armed Conflict Location & Event Data Project (ACLED), a widely used dataset documenting political violence and demonstrations in real-time across multiple countries, including detailed geo-referenced events in the United States [ACLED Project]. ACLED’s systematic data collection (drawing on news, social media, and partner organizations) provides a high-resolution, real-time dataset of protest occurrence and police response. Its reliability and granularity far exceed official government protest data (which are scant), making it a cornerstone of political science work. One data quality caveat often noted is media reporting bias — events in large cities or involving violence get more coverage — but ACLED attempts to mitigate this through varied sourcing and validation.

Second, we derive demographic control variables (and a placebo covariate — land area) from the U.S. Census Bureau’s American Community Survey (ACS), which provides comprehensive population characteristics at the city level, including population size, racial composition, education, and median household income.

Third, we obtain election information from the new American Local Government Elections Database, which compiles detailed data on mayoral election dates, electoral margins, and incumbent party affiliations across U.S. cities [de Benedictis-Kessner et al., 2023]. This comprehensive database includes approximately 57,500 mayoral, city council, and county elections from the past 30 years across most medium and large U.S. cities (of populations above 50,000). It includes detailed information on 78,000 candidates (incumbency, party affiliation, race/gender, vote totals) and election timing for multiple local offices. Crucially for research on staggered election timing, the dataset identifies the election dates and cycles for each city’s leadership. Many U.S. municipalities hold off-cycle elections (e.g., in odd years or in the spring of presidential years), and this database allows us to

exploit this variation. For example, one can compare cities with elections imminent in 2020 to those where mayors were not facing voters as a quasi-exogenous source of differences in political incentives (which this paper carries out partially). The dataset was manually assembled from state and county records and is now publicly available.

B. Sample Selection and Merging Procedure

Starting from the full ACLED dataset covering all U.S. protest events in 2020, we isolate events specifically related to Black Lives Matter (BLM) protests by restricting observations to events where the primary actor field explicitly references BLM-related codes (actor codes containing terms such as “Black Lives Matter” or “BLM”). We further limit our analysis temporally to protests occurring between May 25, 2020 (the date of George Floyd’s death) and November 3, 2020 (the date of the general elections).

The resulting protest-level data is then merged with the elections dataset by matching events to their respective city names. After merging, we collapse the protest-level data to the city level. This procedure generates a city-level dataset, ensuring each city constitutes a single observation, which allows for straightforward analysis without the need for clustered standard errors.

C. Outcome Definitions

We define six primary outcomes capturing various dimensions of protest intensity and policing response at the city level:

- 1) **Arrest Days:** Total number of protest days involving arrests, identified through keyword searches (“arrest”, “detain”, “taken into custody”) within the event notes.
- 2) **Protest Days:** Number of unique days during the study period when at least one protest event occurred.
- 3) **Arrest Ratio:** Ratio of protest days involving arrests to the total number of protest days in the city.
- 4) **Violent Protest Days:** Number of protest days categorized explicitly as “Violent demonstration” by ACLED event type classifications.
- 5) **Violent Arrest Ratio:** Ratio of violent protest days involving arrests (again, keyword-identified) to the total number of violent protest days.
- 6) **Nonviolent Arrest Ratio:** Ratio of nonviolent protest days involving arrests to the total number of nonviolent protest days.

D. Empirical Specifications

Our empirical analysis consists of two primary specifications, each designed to examine distinct hypotheses.

In the first specification, we implement a linear parametric regression discontinuity design (RDD) leveraging the electoral margin as a running variable to examine the causal impact of narrowly won Republican elections (margins around the 0.5 threshold) on protest outcomes:

$$(1) \quad Y_i = \alpha_0 + \alpha_1 \text{Margin}_i + \alpha_2 D_i + \alpha_3 (\text{Margin}_i \times D_i) + \mathbf{X}_i' \delta + \mu_i$$

where:

- Y_i is the protest outcome for city i .
- Margin_i is the vote share for the Republican candidate in city i , with 0.5 representing the exact cutoff.
- D_i is a binary treatment indicator, equal to 1 if the Republican won reelection ($\text{margin} \geq 0$), and 0 otherwise.
- $(\text{Margin}_i \times D_i)$ is an interaction term that allows for distinct slopes of the outcome variable on either side of the electoral cutoff.
- \mathbf{X}_i represents city-level demographic controls (e.g., population size, racial composition, education, median household income).
- μ_i is the robust error term.

Note that the primary specification of interest throughout our analysis is the robust nonparametric RD, but we include the parametric version as a heuristic.

The second specification assesses whether cities with mayors facing imminent electoral challenge (within the 2020 electoral cycle) respond differently to BLM protests compared to cities without immediate electoral pressure. Specifically, we estimate the following regression model:

$$(2) \quad Y_i = \beta_0 + \beta_1 2020_i + \beta_2 \text{PRepub}_i + \beta_3 (2020_i \times \text{PRepub}_i) + \mathbf{X}_i' \gamma + (2020_i \times \mathbf{X}_i') \delta + \varepsilon_i$$

where:

- Y_i is the outcome measure in city i .
- 2020_i is a binary indicator equal to 1 if the city's mayor was up for reelection in 2020 and 0 otherwise.

- PRepub_i is a continuous variable between 0 and 1, representing the probability (based on Ballotpedia, news, etc.) that the mayor of city i identifies as Republican.
- $(2020_i \times \text{PRepub}_i)$ captures the heterogeneous effect of electoral accountability conditional on the partisan alignment of the mayor.
- \mathbf{X}_i is a vector of city-level demographic control variables, including population size, racial composition, education, and median household income.
- $(2020_i \times \mathbf{X}_i)$ are interaction terms that allow the effect of mayoral reelection timing to differ depending on city demographics.
- ε_i is the robust error term.

Both specifications incorporate heteroskedasticity robust standard errors and Benjamini–Yekutieli (BY) adjusted p-values to account for multiple hypothesis testing [Benjamini and Yekutieli, 2001].

E. Identifying Assumptions

Regression Discontinuity (RD) Identifying Assumptions:

- 1) **Continuity at Threshold:** Potential outcomes are continuous functions of the margin of victory at the cutoff.

$$\lim_{\text{Margin}_i \downarrow 0} E[Y_i(0)|\text{Margin}_i] = \lim_{\text{Margin}_i \uparrow 0} E[Y_i(0)|\text{Margin}_i]$$

- 2) **No Manipulation (As-if Randomization):** Agents cannot precisely manipulate the assignment variable (Republican vote share) around the cutoff.
- 3) **Local Randomization:** Close elections (around the cutoff) mimic randomized assignment of treatment (Republican victory).
- 4) **Excludability:** Other determinants of protest outcomes vary smoothly around the electoral margin cutoff.
- 5) **Stable Unit Treatment Value Assumption (SUTVA):** The treatment status and outcomes of one city do not affect outcomes in another city (i.e., no spillovers).

OLS Specification Identifying Assumptions:

- 1) **Conditional Independence:**

$$E[\varepsilon_i | 2020_i, \mathbf{X}_i] = 0$$

The treatment (whether the mayor is up for reelection in 2020) is exogenous, conditional on observed and unobserved covariates.

- 2) **No Omitted Variable Bias:** There are no unobserved city-level confounders correlated with both election timing and protest outcomes.
- 3) **Correct Functional Form:** The relationship between covariates and outcomes is correctly specified.
- 4) **Stable Unit Treatment Value Assumption (SUTVA):** Outcomes for each city are independent of the treatment assignment or outcomes in other cities (i.e., no spillovers).

F. Descriptive Statistics

Table 1 shows population moments for our six primary outcomes (and three additional protest episode outcomes to be defined later) and the sample size.

TABLE 1—CITY-LEVEL DESCRIPTIVE STATISTICS FOR PROTEST EPISODE AND RD OUTCOMES

Variable	mean	sd	min	max	median	n
num_episodes	6.3505618	5.9580577	1	35.0000000	4.0000000	445
avg_duration	1.3962620	0.4967083	1	4.0000000	1.2500000	445
max_duration	3.1640449	3.3855232	1	23.0000000	2.0000000	445
arrest_count	1.4112360	3.0549560	0	27.0000000	0.0000000	445
num_days	10.0943820	11.8321630	1	80.0000000	5.0000000	445
arrest_rate	0.0895931	0.1478199	0	1.0000000	0.0000000	445
violent_rate	0.0540915	0.1033232	0	0.5714286	0.0000000	445
violent_arrest_rate	0.6196235	0.4031038	0	1.0000000	0.6818182	147
nonviolent_arrest_rate	0.0597679	0.1321546	0	1.0000000	0.0000000	445

IV. Partisan Effects

Table 2 reports the regression discontinuity results where treatment is the Republican mayoral candidate exceeding the 0.5 vote share. Under the identifying assumptions, Republican incumbency significantly reduces the number of arrest days and significantly reduces the number of protest days. The local linear estimates must be interpreted with caution as they do not duly prioritize observations near the cutoff, biasing treatment effects.

The null results in Panels C–F are also informative — we do not have any evidence that Republican cities have harsher police departments conditional on either violent or non-violent protests. Instead, the decrease in protests in cities

under Republican control does not appear to respond to any apparent increase in the *ex post* cost to participation.

The nonparametric RD results are thus consistent with a Republican deterrent effect, although these estimates alone do not provide a clear picture.

An important caveat is that the lack of micro data prevents a granular understanding of dynamic selection into — and out of — participation. We do not know, for example, if the perceived threat of a Republican crackdown deterred the very subgroup of protesters that would otherwise have borne the brunt of police reactions. However, an intuitive selection story is that those facing the highest costs to participation (the “hardcore”) were less likely to be deterred by any perceived risk of arrest. In that case, *ex ante* deterrence would have made the pool of protesters more hardcore on average. And, if those unfazed by *ex ante* threats were also unfazed by *ex post* crackdowns, we would expect to see a positive treatment effect on arrests per day, either at the violent or nonviolent levels. But we are unable to detect either such an effect.

The key result in Panel B is highly economically significant, marking a 63% decrease in protest days compared to the population mean. It is thus crucial to audit this finding for potential confounds.

Due to the important body of econometric literature on the importance of RDD falsification tests (particularly within close-election designs), we must implement the following checks in turn [de la Cuesta and Imai, 2016, Marshall, 2022]. We first use a density test per McCrary [2008] to falsify the smoothness (and lack of manipulation) around the treatment threshold. Appendix Table 1 and Figure 1 show the results of this test, which fails to reject smoothness.

Next, we implement the RD on key covariates to make sure they do not vary discontinuously across the threshold. Appendix Table 5 shows that when we restrict to close elections around the 0.5 margin, there is a robustly negative difference in the Black population share across the threshold, suggesting that cities in which a Republican mayor narrowly won differ systematically in their racial composition compared to cities where a Democrat narrowly won.

This evidence suggests a demographic discontinuity at $\text{margin}=0.5$ — i.e., the typical continuity assumption for a standard RD — might be violated. Voters and cities that end up just on one side of 0.5 appear systematically whiter (or less Black) than those on the other side. Practically, that means we must be cautious in interpreting the RD as ‘as-if random’ at 0.5; the data indicate that racial composition of the city is correlated with which side of the threshold the city happens to end up on.

The negative gap persists across multiple bandwidth choices (Appendix Table 8). In contrast, at “placebo” thresholds (0.3, 0.7) there is no stable pattern, implying that the demographic difference is unfortunately specific to the real treatment cutoff at 0.5. Despite the continuity of observables having been violated, we attempt to partial out this confounder in Appendix Table 6. Although our coefficient on number of arrest days (Panel A) disappears, our primary coef-

ficient of interest on number of protest days retains its statistical and economic significance with only slight attenuation.

Appendix Table 7 shows that this key result also survives a stringent p-value correction for the false discovery rate (FDR) associated with multiple hypothesis testing. Since our outcomes co-vary by construction (many are quotients of each other), it is most appropriate to implement Benjamini-Yekutieli sharpened p-values as opposed to more lenient methods [Benjamini and Yekutieli, 2001, Benjamini and Hochberg, 1995, Holm, 1979].

We next confirm in Appendix Table 2 that our nonparametric robust RD estimates on the number of protest days do not rely on a particular choice of RD bandwidth. Finally, Appendix Table 3 shows that, although our estimates fail the placebo cutoff at 0.3, this concerning result loses significance after adjusting for the FDR (Appendix Table 4).

It is important to examine thoroughly the initially discontinuous difference in Black population percentages across the threshold. To do so, we implement robustness checks on the violation of our identifying assumption, and we place the covariate (percent Black population) on the left-hand side. Tables 8 and 9 show that this discontinuity endures changes to the RD bandwidth and passes both placebo tests. However, given that explicitly adjusting for demographic covariates still leaves our point estimate on protest days statistically and economically significant (effect size of 49.7% in Appendix Table 7), we maintain that there is a significant treatment effect even conditioning on demographic confounders. For visual persuasion, Appendix Figure 2 shows the RD Plot of number of protest days, demeaned by demographic covariates. There is very clearly a sharp discontinuity at the election threshold.

Notwithstanding the relative econometric robustness of this result, caution must be taken in interpreting it. Close-elections RDs do not capture the general effect of party identity; instead, they capture the local average treatment effect (LATE) of party identity *within very close elections only*. Eggers et al. [2015] affirm the importance of implementing the above robustness checks to overcome potential sorting and manipulation around the threshold — both important internal validity threats. However, we cannot rule out heterogeneous treatment effects for non-close elections.

Furthermore, Marshall [2022] importantly highlights the importance of understanding elections as equilibrium outcomes when interpreting close-elections RD results. Estimates will always be subject to compensating differentials — if voters are biased against a particular characteristic, candidates possessing that characteristic may have to compensate by being stronger in other areas (for example, having higher competence, more resources, or better experience) to win close elections. As a result, when comparing narrowly winning candidates, the observed differences in intent-to-treat (ITT) effects reflect not only the effect of the characteristic of interest (party identity) but also the influence of these additional compensatory factors. Therefore, without additional assumptions or adjustments, the

RD estimate in such designs captures a compound effect rather than the isolated causal impact of the candidate characteristic. Due to limited data on candidate characteristics, we do not adjust for these differentials in our analysis and must interpret the point estimates with caution. However, given the gigantic size of the RD point estimate on protest days (even after adjusting for key covariates), it is reasonable to conclude that local party identity likely had an economically significant causal effect on protest participation during the BLM movement, even if our effect size is inflated.

Although we lack micro data, we still have the statistical resources to conduct some interesting exploratory analysis on mechanisms. We create three new city-level outcomes to better understand the causal effects of party identity on protest incidence. In particular, we define “episodes” of protesting as any consecutive chain of protest days within a city. We can then construct the number of protest episodes, the average duration of the episodes, and the longest duration of an episode for each city, and measure how the RD treatment (Republican victory) affects each dimension. Table 4 reports results. In close mayoral elections, Republican victory has large and significant negative effects on the number of BLM protest episodes, the average duration of episodes, and on the length of the longest episode for each city. On a percentage basis, the most sizable effect is that on the *number* of protest episodes. If we think of this as a crude decomposition of the absolute RD effect on protest days, we can tentatively conclude that *ex ante* deterrence is largely at the root of our original estimate. As a stylized description, however much *ex post* police deterrence might have made protesters resign as the days wore on (average episode duration), we see that deterrence had an even larger role in preventing people from mobilizing in the first place (number of episodes). This narrative — and the lack of causal effects on *rates* of arrest per day of protest — troubles any notion that local partisan differences actually caused observable differences in police responses. Instead, partisanship had an extremely tangible effect on mobilization, an effect that I argue is best explained by strategic deterrence. That said, with the data shown here, it is not possible to distinguish deterrence due to fear from deterrence due to negotiation, for example. It is just as consistent with these results that police departments in Republican cities were somehow more effective at mollifying potential protesters as it is that they were more effective at scaring them off the streets.

V. Election Pressure Effects

We now implement a second quasi-experiment exploiting the staggered timing of mayoral elections in cities across the U.S. Even though our RD did not yield observable evidence of differential police behavior on partisan lines, this section is another opportunity to assess whether electoral *incentives* tangibly affected local *state* responses to protests beyond the perceptual protester-side effects of party identity.

Beneath the identifying assumption that a city’s assignment to mayoral election

in 2020 was random, we can use “up for reelection” as a treatment variable that proxies for election pressures. If incumbents adapt their behavior due to reelection pressure (as many citizen-candidate models suggest), then it is natural to assume that behavioral effects would grow as elections approach and as the public increasingly bases voting preferences to their material conditions.

Table 5 shows the distribution of candidates up for reelection by party, and Appendix Figure 1 shows a map of incumbents by party and 2020 reelection status. Balance on observables can reassure us that any measured effects of treatment (having a 2020 reelection) are not due to imbalance on unobservables. We thus assess demographic balance by reelection and party status in Tables 6–9 using paired t-tests. Unfortunately, Democratic cities that were up for reelection in 2020 are systematically richer and whiter than those that were not, and Republican cities that were *not* up for reelection are systematically whiter and less populous than Democratic cities that were *not* up for reelection. This is not unexpected given that election cycles are likely historically determined and correlate with relevant characteristics; however, this means that our results may not be interpreted as causal evidence of electoral incentives without additional controls.

Tables 10 and 11 show our six primary outcomes. Column (1) is unconditional, Column (2) includes demographic controls, Column (3) is unconditional with an interaction between reelection and party, and Column (4) includes both an interaction and demographic controls (which are also interacted with party).

The salient results are as follows: reelection pressures appear in Column (1) to reduce arrests, reduce protest incidence, and reduce protester violence. However, after conditioning on demographics, only the former two effects persist. On the party level, being Republican appears to further intensify the reduction in protest incidence, but after adding controls, we can no longer reject partisan equality in treatment effects. No other interacted effects may be recovered.

In a sense, this second analysis appears to replicate the patterns borne out by the RD — we only ever see robust effects at the level of arrests and protest days. However, given that treatment is now reelection rather than incumbent identity, it is important to interpret carefully. Conditioning on demographic factors, being up for reelection reduces protest days. It is unclear what mechanism would explain this effect, and, once again, several explanations match these results.

First, protesters may have experienced reduced perceived returns to protest participation if they felt they could soon voice their grievances at the ballot box. Alternatively, protesters might have been deterred due to the risk and uncertainty associated with an impending change in the balance of power. In other words, would-be protesters might have felt more comfortable turning out against the devil they knew. It still might have been that incumbents facing imminent reelection possessed different levels of leverage over local police departments (e.g., lame duck effects) and thus had a varied propensity to alter protest responses. Our dataset is not sufficient to disambiguate these competing explanations.

VI. Conclusion

This study provides robust evidence that mayoral party identity and electoral incentives critically influenced city-level responses to the 2020 Black Lives Matter protests. Our analysis demonstrates that Republican incumbency significantly deterred protest incidence without altering *ex post* police reactions, suggesting that protesters either internalized policing risks or that unobserved state behavior in Republican cities (e.g., negotiations or policy capitulations) satisfied protesters enough to reduce returns to participation. Furthermore, electoral accountability appears to temper protest activity, underscoring the complex interaction between democratic processes and civic unrest. These findings shed important light on how local political dynamics mediate the delicate choices of whether to repress — and whether to protest. Future analysis would do well to incorporate protest participation survey data (which I tried and failed to get access to) to isolate mechanisms, subject this result to a richer set of politician and geographical covariates, and model the strategic interaction structurally.

	Nonparametric	Local Linear
Panel A: Number of Arrest Days		
Coefficient	-0.904* (0.547)	-1.292*** (0.388)
Observations	281	496
Bandwidth	0.10	—
Panel B: Number of Protest Days		
Coefficient	-6.370*** (2.157)	-6.048*** (1.515)
Observations	281	496
Bandwidth	0.14	—
Panel C: Arrest Days Per Protest Day		
Coefficient	-0.003 (0.060)	0.002 (0.021)
Observations	257	445
Bandwidth	0.10	—
Panel D: Violent Protest Days Per Protest Day		
Coefficient	-0.029 (0.030)	-0.047*** (0.015)
Observations	257	445
Bandwidth	0.11	—
Panel E: Arrest Days Per Violent Protest Day		
Coefficient	0.116 (0.276)	0.059 (0.117)
Observations	114	147
Bandwidth	0.13	—
Panel F: Arrest Days Per Non-Violent Protest Day		
Coefficient	0.028 (0.058)	0.034* (0.019)
Observations	257	445
Bandwidth	0.09	—

Notes: Each panel compares two RD estimates at margin=0.5. Nonparam = rdrobust. Local Linear = parametric LM with treat and linear margin. We show coefficients with standard errors in parentheses and significance stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE 2—RDD ESTIMATES (PANELS A–F)

TABLE 3—RD ESTIMATES OF PROTEST EPISODE MEASURES (MARGIN = 0.5) WITH DEMOGRAPHIC CONTROLS AND BY-ADJUSTED p -VALUES

Outcome	Coefficient (SE)	BY-adjusted p -value
Number of Episodes	-3.234*** (0.955)	0.0038789
Average Duration (days)	-0.357** (0.130)	0.0170828
Max Duration (days)	-1.719** (0.733)	0.0347973

TABLE 4—RD ESTIMATES CONVERTED TO PERCENT CHANGES RELATIVE TO BASELINE MEANS

Outcome	Mean	RD Coefficient (SE)	% Change	BY-Adj p-value
num_episodes	6.350600	-3.324*** (0.955)	-52.3%	0.0038789
avg_duration	1.396262	-0.357** (0.130)	-25.6%	0.0170828
max_duration	3.316304	-1.719** (0.733)	-51.8%	0.0347973

TABLE 5—MAYORS UP FOR 2020 REELECTION BY PARTY

Up for 2020 Reelection?	D	R	Sum
No	230	147	377
Yes	65	51	116
Sum	295	198	493

TABLE 6—COVARIATE BALANCE FOR DEMOCRATS BY REELECTION STATUS

Variable	Up for Reelection	Not Up for Reelection	p-value	Significance
total_pop	290397.08 (593673.78)	353504.07 (778869.05)	0.513	
pct_black	14.26 (13.82)	17.83 (15.82)	0.099	.
median_hh_income	85499.48 (24220.93)	78085.39 (24260.98)	0.044	*

TABLE 7—COVARIATE BALANCE FOR REPUBLICANS BY REELECTION STATUS

Variable	Up for Reelection	Not Up for Reelection	p-value	Significance
total_pop	220425.8 (191040.37)	195885.38 (145686.17)	0.444	
pct_black	11.3 (11.67)	10.54 (10.83)	0.713	
median_hh_income	85397.64 (28325.57)	81916.22 (20454.85)	0.458	

TABLE 8—COVARIATE BALANCE BY PARTY FOR CITIES UP FOR REELECTION

Variable	Democrat (Mean (SD))	Republican (Mean (SD))	p-value	Significance
total_pop	290397.08 (593673.78)	220425.8 (191040.37)	0.396	
pct_black	14.26 (13.82)	11.3 (11.67)	0.228	
median_hh_income	85499.48 (24220.93)	85397.64 (28325.57)	0.984	

TABLE 9—COVARIATE BALANCE BY PARTY FOR CITIES NOT UP FOR REELECTION

Variable	Democrat (Mean (SD))	Republican (Mean (SD))	p-value	Significance
total_pop	353504.07 (778869.05)	195885.38 (145686.17)	0.009	**
pct_black	17.83 (15.82)	10.54 (10.83)	0.000	***
median_hh_income	78085.39 (24260.98)	81916.22 (20454.85)	0.202	

	(1)	(2)	(3)	(4)
Panel A: Arrest Count				
(Intercept)	0.516*** (0.047)	0.797*** (0.051)	-0.626 (1.935)	-0.619 (1.911)
Up for Reelection	-0.453*** (0.099)	-0.345* (0.120)	-0.285** (0.100)	-0.185 (0.120)
Probability Repub.	—	—	-1.268*** (0.163)	-0.849*** (0.162)
Up for Reelection × Prob. Repub.	—	—	0.321 (0.276)	0.076 (0.269)
Joint p-value	—	—	0.032	0.058
Observations	397	369	397	369
Panel B: Protest Days				
(Intercept)	2.379*** (0.018)	2.613*** (0.020)	5.268*** (0.749)	4.843*** (0.745)
Up for Reelection	-0.361*** (0.038)	-0.245*** (0.046)	-0.213*** (0.038)	-0.102* (0.047)
Probability Repub.	—	—	-0.976*** (0.059)	-0.648*** (0.058)
Up for Reelection × Prob. Repub.	—	—	0.252* (0.099)	0.053 (0.097)
Joint p-value	—	—	0.045	0.078
Observations	397	369	397	369
Panel C: Arrest Ratio				
(Intercept)	-1.695*** (0.051)	-1.643*** (0.055)	-7.174*** (2.063)	-6.424** (2.093)
Up for Reelection	-0.108 (0.107)	-0.121 (0.128)	-0.068 (0.109)	-0.069 (0.130)
Probability Repub.	—	—	-0.307 (0.166)	-0.180 (0.168)
Up for Reelection × Prob. Repub.	—	—	0.075 (0.281)	-0.030 (0.281)
Joint p-value	—	—	0.112	0.201
Observations	359	356	359	356

TABLE 10—COMPARISON OF TREATMENT EFFECTS (PANELS A–C). NOTE: COLUMNS (1) = NO INTERACTION/NO CONTROLS; (2) = NO INTERACTION/WITH CONTROLS; (3) = INTERACTION/NO CONTROLS; (4) = INTERACTION/WITH CONTROLS. STANDARD ERRORS ARE IN PARENTHESES. SIGNIFICANCE STARS REFLECT BY-ADJUSTED P-VALUES (BENJAMINI–YEKUTIELI). *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, . $p < 0.1$. JOINT P-VALUE TESTS $H_0 : \beta_{\text{UPFORREELECT}} + \beta_{\text{UPFORREELECT} \times \text{PROBREP}} = 0$.

Variable	(1)	(2)	(3)	(4)
Panel D: Violence Count				
(Intercept)	-2.086*** (0.059)	-2.005*** (0.063)	-13.007*** (2.399)	-13.070*** (2.417)
Up for Reelection	-0.273* (0.131)	-0.193 (0.151)	-0.249 (0.132)	-0.156 (0.152)
Probability Repub.	—	—	-0.567 (0.209)	-0.428 (0.211)
Up for Reelection × Prob. Repub.	—	—	-0.040 (0.368)	-0.165 (0.368)
Joint p-value	—	—	0.089	0.143
Observations	359	356	359	356
Panel E: Violent Arrest Ratio				
(Intercept)	0.625*** (0.116)	0.673*** (0.124)	-5.942 (5.284)	-4.550 (5.360)
Up for Reelection	0.106 (0.265)	0.051 (0.303)	0.115 (0.267)	0.070 (0.304)
Probability Repub.	—	—	-0.395 (0.406)	-0.308 (0.410)
Up for Reelection × Prob. Repub.	—	—	0.433 (0.735)	0.348 (0.739)
Joint p-value	—	—	0.250	0.401
Observations	135	134	135	134
Panel F: Nonviolent Arrest Ratio				
(Intercept)	-2.272*** (0.067)	-2.257*** (0.074)	-2.027 (2.707)	-0.915 (2.769)
Up for Reelection	-0.029 (0.137)	-0.083 (0.169)	0.020 (0.139)	-0.021 (0.171)
Probability Repub.	—	—	-0.045 (0.202)	0.050 (0.205)
Up for Reelection × Prob. Repub.	—	—	0.019 (0.345)	-0.057 (0.344)
Joint p-value	—	—	0.672	0.889
Observations	359	356	359	356

TABLE 11—COMPARISON OF TREATMENT EFFECTS (PANELS D–F). NOTE: STANDARD ERRORS ARE IN PARENTHESES. SIGNIFICANCE STARS REFLECT BY-ADJUSTED P-VALUES (BENJAMINI–YEKUTIELI). *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, . $p < 0.1$. JOINT P-VALUE TESTS $H_0 : \beta_{\text{UPFORREELECT}} + \beta_{\text{UPFORREELECT} \times \text{PROBREP}} = 0$.

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TABLE 1—SUMMARY OF MCCRARY DENSITY TEST AT CUTOFF $c = 0.5$

Statistic	Left of c	Right of c
Number of Observations	281	215
Effective Observations	164	139
Order of Estimation (p)	2	2
Order of Bias Correction (q)	3	3
Bandwidth (h)	0.17	0.17
Kernel	Triangular	
Bandwidth Method	Estimated	
Variance Estimation Method	Jackknife	
Robust Test Statistic (T)	-0.0961	
Robust Test p -value	0.9235	

Note: The test was performed using the `rddensity` package. A robust t-test is reported, and the null hypothesis of no discontinuity is not rejected.

FIGURE 1. MCCRARY DENSITY PLOT

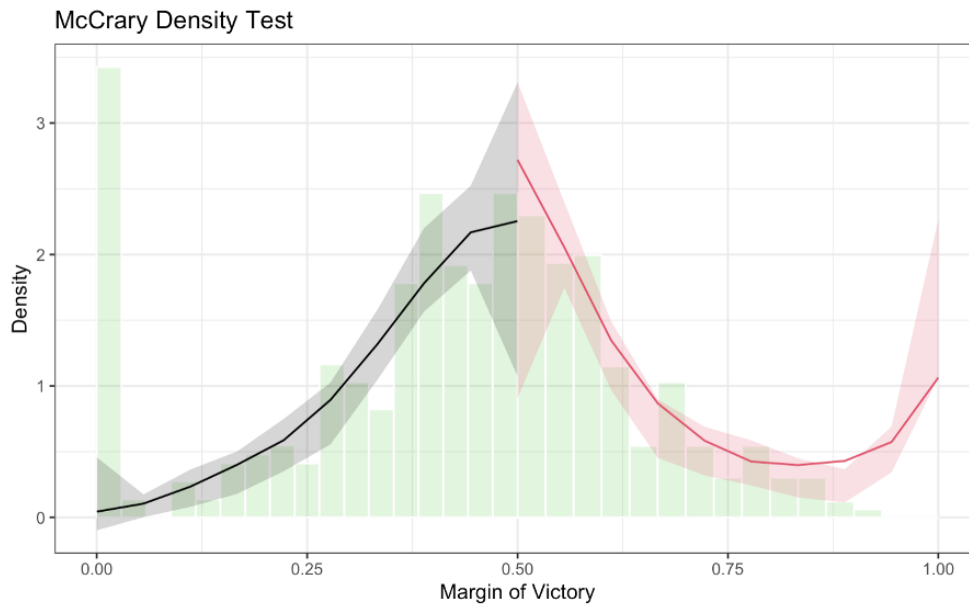


TABLE 2—BANDWIDTH SENSITIVITY CHECK FOR PRIMARY OUTCOMES (RD ESTIMATES ON OUTCOMES WITH COVARIATE ADJUSTMENT)

Outcome	Bandwidth	Result
arrest_count	0.05	-0.838 (0.659)
arrest_count	0.10	-0.650 (0.490)
arrest_count	0.15	-0.482 (0.429)
arrest_count	0.20	-0.617 (0.406)
num_days	0.05	-7.720*** (2.812)
num_days	0.10	-5.885*** (2.044)
num_days	0.15	-4.248** (1.741)
num_days	0.20	-4.436*** (1.634)
arrest_rate	0.05	0.007 (0.083)
arrest_rate	0.10	0.002 (0.058)
arrest_rate	0.15	0.014 (0.049)
arrest_rate	0.20	0.023 (0.043)
violent_rate	0.05	-0.036 (0.035)
violent_rate	0.10	-0.034 (0.029)
violent_rate	0.15	-0.040 (0.027)
violent_rate	0.20	-0.041* (0.025)
violent_arrest_rate	0.05	-0.566 (0.473)
violent_arrest_rate	0.10	-0.287 (0.264)
violent_arrest_rate	0.15	-0.003 (0.236)
violent_arrest_rate	0.20	0.025 (0.209)
nonviolent_arrest_rate	0.05	0.033 (0.077)
nonviolent_arrest_rate	0.10	0.052 (0.054)
nonviolent_arrest_rate	0.15	0.060 (0.048)
nonviolent_arrest_rate	0.20	0.065 (0.041)

TABLE 3—PLACEBO CUTOFF RD ESTIMATES FOR PRIMARY OUTCOMES (WITH COVARIATE ADJUSTMENT)

Outcome	Cutoff	Result
arrest_count	0.3	-0.388 (1.128)
arrest_count	0.7	0.501 (0.548)
num_days	0.3	-8.516* (4.424)
num_days	0.7	4.529 (3.526)
arrest_rate	0.3	0.074** (0.035)
arrest_rate	0.7	0.156 (0.098)
violent_rate	0.3	0.001 (0.027)
violent_rate	0.7	-0.024 (0.024)
violent_arrest_rate	0.3	0.064 (0.274)
violent_arrest_rate	0.7	-0.733 (0.602)
nonviolent_arrest_rate	0.3	0.064* (0.033)
nonviolent_arrest_rate	0.7	0.158 (0.099)

TABLE 4—PLACEBO CUTOFF RD ESTIMATES ON NUM_DAYS WITH COVARIATE ADJUSTMENT AND BY CORRECTION

Cutoff	Result
0.3	-8.516 (4.424)
0.7	4.529 (3.526)

TABLE 5—RD COVARIATE BALANCE FOR SELECTED CITY-LEVEL VARIABLES AT MARGIN = 0.5

Covariate	Coeff (Robust)	95% CI	p-value
log(Pop)	-0.110	[-0.496, 0.271]	0.565
log(Inc)	0.100	[-0.053, 0.274]	0.186
%Black	-9.399	[-18.494, -3.365]	0.005

	Nonparam	Local Linear
Panel A: Number of Arrest Days		
Coefficient	-0.554 (0.443)	-0.858** (0.357)
Observations	278	493
Bandwidth	0.12	—
Panel B: Number of Protest Days		
Coefficient	-5.015*** (1.830)	-3.693*** (1.297)
Observations	278	493
Bandwidth	0.12	—
Panel C: Arrest Days Per Protest Day		
Coefficient	0.002 (0.058)	0.013 (0.021)
Observations	254	442
Bandwidth	0.10	—
Panel D: Violent Protest Days Per Protest Day		
Coefficient	-0.036 (0.028)	-0.040*** (0.014)
Observations	254	442
Bandwidth	0.12	—
Panel E: Arrest Days Per Violent Protest Day		
Coefficient	-0.363 (0.275)	0.004 (0.117)
Observations	113	146
Bandwidth	0.09	—
Panel F: Arrest Days Per Non-Violent Protest Day		
Coefficient	0.051 (0.054)	0.044** (0.019)
Observations	254	442
Bandwidth	0.10	—
<p><i>Notes:</i> Each panel compares two RD estimates at margin=0.5. Nonparam = rdrobust with covariate adjustment; Local Linear = parametric LM with covariate adjustment (including pct_black, log_pop, and log_median_hh_income). Coefficients are reported with standard errors in parentheses and significance stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.</p>		

TABLE 6—RDD ESTIMATES W/ COVARIATE ADJUSTMENT (PANELS A–F)

TABLE 7—PRIMARY RD ESTIMATES AT MARGIN = 0.5 WITH COVARIATE ADJUSTMENT AND FDR CORRECTION. NONPARAM = RDROBUST; LOCAL LINEAR = PARAMETRIC LM. COEFFICIENTS ARE REPORTED WITH STANDARD ERRORS IN PARENTHESES; SIGNIFICANCE STARS ARE BASED ON BENJAMINI–YEKUTIELI ADJUSTED P-VALUES.

Outcome	RD_Result	LM_Result	RD_N	RD_BW	LM_N
Panel A: Number of Arrest Days	-0.554 (0.443), Adj p = 0.776	-1.292*** (0.388), Adj p = 0.006	278	0.1225256	496
Panel B: Number of Protest Days	-5.015* (1.830), Adj p = 0.090	-6.048*** (1.515), Adj p = 0.001	278	0.1239131	496
Panel C: Arrest Days Per Protest Day	0.002 (0.058), Adj p = 1.000	0.002 (0.021), Adj p = 1.000	254	0.1002837	445
Panel D: Violent Protest Days Per Protest Day	-0.036 (0.028), Adj p = 0.776	-0.047*** (0.015), Adj p = 0.006	254	0.1235567	445
Panel E: Arrest Days Per Violent Protest Day	-0.363 (0.275), Adj p = 0.776	0.059 (0.117), Adj p = 1.000	113	0.0931785	147
Panel F: Arrest Days Per Non-Violent Protest Day	0.051 (0.054), Adj p = 1.000	0.034 (0.019), Adj p = 0.250	254	0.0972014	445

TABLE 8—BANDWIDTH SENSITIVITY (RD ESTIMATES ON % BLACK WITH COVARIATE ADJUSTMENT)

Bandwidth	Result
0.05	-9.522 (5.303)
0.10	-8.425 (3.466)
0.15	-6.416 (2.858)
0.20	-4.371 (2.518)

TABLE 9—PLACEBO CUTOFF RD ESTIMATES ON % BLACK WITH COVARIATE ADJUSTMENT

Cutoff	Result
0.3	4.541 (5.161)
0.7	-13.937 (14.346)

TABLE 10—RD ESTIMATE ON PLACEBO COVARIATE (LAND AREA) WITH COVARIATE ADJUSTMENT

Outcome	Result
Land Area	58.492 (43.506)

FIGURE 2. RD PLOT OF PROTEST DAYS, DEMEANED BY COVARIATES

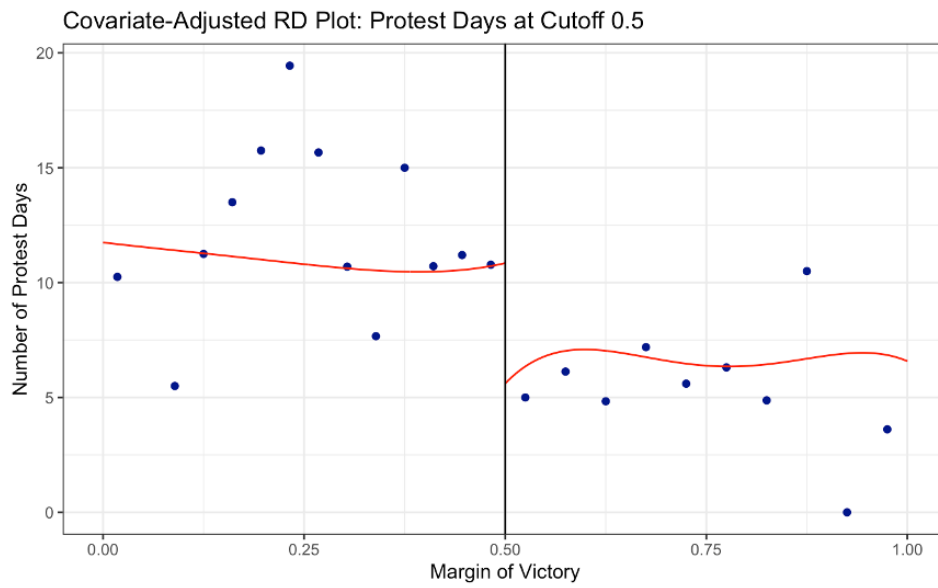


FIGURE 3. 2020 MAYORAL ELECTION MAP



Source: American Local Government Elections Database