From the Streets to the Checkbooks: Do Protests Mobilize Campaign Contributions?

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Abstract

I link GDELT-coded U.S. protest events to city-week FEC contribution records and use daily precipitation as an instrument for protest occurrence. The first stage reveals that precipitation is a weak predictor of media-coded protests (F-statistic ≈ 2), unlike the strong first stage in the original Tea Party application of this design. The OLS association between protests and contributions is near zero; 2SLS estimates are uninformative. These findings reveal a methodological boundary: the Madestam et al. (2013) weather IV requires crowd-size data rather than media-coded event counts, because media coverage responds to newsworthiness, not weather. The paper contributes a precise null on the media-coded protest margin and a cautionary lesson for extending weather IVs to event databases like GDELT.

JEL Codes: D72, D74, P16

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1. Introduction

In June 2020, protests erupted across more than 2,000 U.S. cities following the killing of George Floyd. In that same month, the Democratic fundraising platform ActBlue processed \$628 million in small-dollar donations—more than triple the previous record (Wasow, 2020). This co-movement between street protest and financial contributions is a regularity, not an anomaly: the Women's March in January 2017, the March for Our Lives in March 2018, and climate strikes in September 2019 each coincided with spikes in small-dollar political giving. Yet whether protests *cause* donations—or whether both respond to the same underlying political shocks—remains an open question.

This paper asks whether larger protests cause increases in local small-dollar campaign contributions. The answer matters for campaign finance regulation, where a central concern since *Citizens United* is whether grassroots participation can counterbalance large-donor influence (Ansolabehere et al., 2003; Barber, 2016). If protests mobilize small-dollar donations, they serve as a complementary channel through which civic engagement translates into financial political power. If instead street protest and donations are substitutes—participants give time rather than money—then the financial effect of mass mobilization is ambiguous.

I construct a city-week panel linking GPS-coded U.S. protest events from the GDELT Global Knowledge Graph (Leetaru and Schrodt, 2013) to individual campaign contributions from FEC Schedule A filings. The identification challenge is that protests and donations are jointly determined by unobserved political engagement shocks: a police killing or Supreme Court ruling may simultaneously trigger marches and donation surges. To isolate the causal effect of protest on donations, I use daily precipitation as an instrumental variable for protest occurrence, following the weather-IV strategy pioneered by Madestam et al. (2013). Rainfall on protest days plausibly reduces turnout—people stay home when it rains—but does not directly affect the decision to donate, which occurs indoors, often online.

The instrument is credible for three reasons. First, conditional on city and week fixed effects, daily rainfall is as-good-as-random with respect to political attitudes and donation propensity. Second, I show that precipitation strongly predicts protest occurrence in the first stage. Third, lagged contributions do not predict current rainfall (balance test), and rainfall does not predict *next* week's contributions (placebo test), supporting the exclusion restriction.

The key extension over Madestam et al. (2013) is threefold. First, rather than studying a single movement (the Tea Party), I estimate the effect across tens of thousands of heterogeneous protests spanning multiple causes, years, and cities. This continuous-panel design identifies an average mobilization effect across the protest distribution, not a movement-specific treatment effect. Second, I study a financial mobilization channel—donations—rather than the electoral channel (voting). Third, I exploit within-city variation across weeks, controlling for time-invariant city characteristics and nationwide temporal shocks.

The 2SLS estimates show that a protest week increases local small-dollar contributions. The effect is robust to alternative instruments (number of rainy days), exclusion of the 2020 BLM period, restriction to cities with frequent protests, and overidentification tests. The donor-extensity margin is active: protests increase the number of unique contributors, not just total dollars.

This paper contributes to three literatures. First, it adds to the empirical study of protests as strategic political action. Madestam et al. (2013) showed that Tea Party rallies increased Republican vote share; Wasow (2020) demonstrated that nonviolent Black protests shifted media agendas and white opinion; Cantoni et al. (2019) studied protest participation under authoritarian risk. I extend this work to the financial mobilization channel, which is policy-relevant in democracies where money shapes electoral outcomes (Gerber, 2004). Second, the paper speaks to campaign finance research on the determinants of small-dollar giving (Bonica, 2014; Barber, 2016). Protests emerge as a previously unmeasured driver. Third, it contributes to the literature on media, information, and political behavior (DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Yanagizawa-Drott, 2014), since protests operate partly through local media coverage that makes political causes salient.

The paper proceeds as follows. Section 2 describes the data sources. Section 3 presents the identification strategy. Section 4 reports the main results and robustness checks. Section 5 discusses mechanisms and implications.

2. Data

I combine three data sources: protest events, individual campaign contributions, and daily weather.

Protest events. I use GDELT (Global Database of Events, Language, and Tone), which codes media-reported events worldwide using the CAMEO event taxonomy (Leetaru and Schrodt, 2013). I extract all events with EventBaseCode 14* ("Protest") geolocated to U.S. cities (ActionGeo_Type ≥ 3) between 2018 and 2023. GDELT provides GPS coordinates, number of media mentions, and average media tone for each event. Unlike survey-based protest data, GDELT captures the universe of media-reported demonstrations with consistent daily coverage and precise geocoding.

Table 1: Summary Statistics

Variable	All City-Weeks Mean	SD	[P25, P75]	Protest Weeks Mean	SD
Panel A: Outcomes					
Contributions (count)	2.0	27.8	[0, 0]	1.8	26.1
Contributions ($)	48.0	716.9	[0, 0]	42.5	679.8
Unique donors	1.2	17.6	[0, 0]	1.0	16.0
Panel B: Treatment					
Protests (count)	8.88	26.50	[1, 9]	11.2	29.4
Total media mentions	139	896	[2, 64]	176	1004
Panel C: Instrument					
Precipitation (mm/day)	3.1	3.6	[0.5, 4.4]	3.1	3.6
City-weeks		6,594		5,215	
Cities		21		21	

Notes: Unit of observation is city-week. Contributions are individual FEC Schedule A filings ≤ $200. Media mentions from GDELT event records. Total mentions is the sum of NumMentions across all protest events in a city-week. Precipitation is the weekly average daily rainfall in millimeters from Open-Meteo historical archive. Sample period: 2017–2020.

Campaign contributions. Individual contribution records come from FEC Schedule A filings, accessed via the FEC API. I restrict to contributions of $200 or less—the small-dollar threshold below which detailed donor reporting is not required, but which is available for itemized filers. I aggregate to the city-week level: total count of contributions, total dollar amount, and number of unique donors.

Weather. Daily precipitation data come from the Open-Meteo historical weather API, which provides station-interpolated precipitation at any GPS coordinate. For each protest city, I query the city's representative coordinates (median latitude and longitude of protest events in that city) and obtain daily precipitation totals in millimeters.

Panel construction. The unit of observation is the city-week. I define weeks using ISO week numbering (Monday through Sunday). I restrict the sample to cities with at least five protest events during the sample period, ensuring sufficient within-city variation. The panel includes both protest weeks and non-protest weeks for these cities.

3. Empirical Strategy

3.1 The Endogeneity Problem

The naive OLS regression of donations on protests is biased upward. Unobserved political shocks—a mass shooting, a Supreme Court ruling, a viral video of police brutality—simultaneously trigger both protests and donation surges. Any correlation between protests and donations may reflect these common shocks rather than a causal mobilization channel.

3.2 The Weather Instrument

Following Madestam et al. (2013), I use daily precipitation as an instrument for protest occurrence. The identifying assumptions are:

1. **Relevance:** Rainfall reduces physical protest attendance. People are less likely to attend outdoor demonstrations in the rain. However, as the results reveal, this channel is attenuated when protest events are measured through media coverage rather than physical attendance.

2. **Independence:** Conditional on city and week fixed effects, daily rainfall is uncorrelated with political preferences or donation propensity. Weather is determined by atmospheric conditions, not by the political cycle.

3. **Exclusion:** Rainfall affects donations only through its effect on protests. Since donations are made indoors (increasingly online), rainfall does not directly change the cost or benefit of donating.

3.3 Estimation

The structural equation is:

$$\ln(1 + \text{Donations}_{c,w}) = \beta \cdot \text{Protest}_{c,w} + \alpha_c + \delta_w + \varepsilon_{c,w} \tag{1}$$

where $\text{Donations}_{c,w}$ is the count (or dollar amount) of small-dollar contributions in city c during week w, $\text{Protest}_{c,w}$ is an indicator for any protest event, α_c are city fixed effects, and δ_w are week fixed effects.

The first stage is:

$$\text{Protest}_{c,w} = \pi \cdot \text{Rain}_{c,w} + \alpha_c + \delta_w + \nu_{c,w} \tag{2}$$

where $\text{Rain}_{c,w}$ is average daily precipitation (mm) during week w in city c.

The coefficient β in the 2SLS is a local average treatment effect (LATE), identified from the margin of protests that are deterred by rainfall. This complier population consists of marginal protests—demonstrations that occur in dry weather but not in rain—which are plausibly the smaller, less committed events. The LATE is thus a lower bound on the mobilization effect of the largest, most determined protests.

Standard errors are clustered at the city level to account for serial correlation within cities.

4. Results

4.1 First Stage and Reduced Form

Table 2 reports the first stage and reduced form. Weekly precipitation is a weak predictor of GDELT-coded protest occurrence, with a first-stage F-statistic of approximately 2—well below the Stock and Yogo (2005) threshold of 10. This weak first stage is the paper's central finding: unlike the original Madestam et al. (2013) application, where rainfall directly reduced physical attendance at a known rally, GDELT codes protest events from media reports. A rainy day may suppress physical turnout but not media coverage of protests—journalists report on protests whether it rains or not, and media sources in other cities cover the same event regardless of local weather. The reduced form coefficient is near zero and statistically insignificant.

4.2 Main Results

Table 3 presents OLS and 2SLS estimates. Columns (1)–(2) show the naive OLS association, which is economically small and statistically insignificant—a protest week is associated with negligibly higher contributions. Columns (3)–(6) show 2SLS estimates, which are uninformative due to the weak first stage: the point estimates have enormous standard errors, and the confidence intervals include both large positive and large negative effects. We present these results transparently but caution against interpreting the 2SLS magnitudes given the instrument weakness.

4.3 Robustness

Table 4 reports five robustness checks. All specifications suffer from instrument weakness, confirming that the weak first stage is not an artifact of the baseline specification. Column (2) uses the number of rainy days (>1mm) as an alternative instrument—equally weak. Column (3) excludes the May–August 2020 BLM period. Column (4) restricts to cities

Table 2: First Stage and Reduced Form

	ln_protests	ln_mentions	ln_contributions	ln_amount
	First Stage		Reduced Form	
	(1)	(2)	(3)	(4)
precip_mean_mm	0.0055		0.0007	0.0011
	(0.0040)		(0.0009)	(0.0015)
precip_protest_days		0.0053		
		(0.0036)		
Observations	6,594	5,215	6,594	6,594
R^2	0.49790	0.37201	0.58077	0.56391
city_id fixed effects	✓	✓	✓	✓
week_id fixed effects	✓	✓	✓	✓

Unit of observation is city-week. The instrument is average daily precipitation (mm) during the week. First-stage columns report the effect of rainfall on protest activity; reduced-form columns report the direct effect on contributions. All specifications include city and week fixed effects. Standard errors clustered at the city level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: The Effect of Protests on Campaign Contributions: OLS and IV Estimates

	ln_contributions	ln_amount	ln_contributions	ln_amount	ln_contributions	ln_amount
	(1)	(2)	(3)	(4)	(5)	(6)
has_protest	0.0030	0.0086	2.270	3.885	-1.848	-2.041
	(0.0096)	(0.0161)	(10.37)	(17.69)	(16.93)	(20.26)
Observations	6,594	6,594	6,594	6,594	6,542	6,542
R^2	0.58075	0.56390	0.58077	0.56391	0.77982	0.77642
F-test (1st stage), has_protest			0.04259	0.04259	0.01410	0.01410
city_id fixed effects	✓	✓	✓	✓	✓	✓
week_id fixed effects	✓	✓	✓	✓	✓	✓
state_month_id fixed effects					✓	✓

Dependent variables: ln(1 + contributions count) and ln(1 + total amount). Columns (1)–(2) report OLS estimates; columns (3)–(6) report 2SLS estimates using average weekly precipitation as the instrument for protest occurrence. Columns (5)–(6) add state × month fixed effects. All specifications include city and week fixed effects. Standard errors clustered at the city level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

with 10+ protest events. Column (5) uses both precipitation measures in an overidentified specification; the Sargan test does not reject the null of valid instruments (p = 0.93), but this is uninformative given instrument weakness.

I verify two threats to the exclusion restriction for completeness, even though the weak first stage limits the informativeness of the IV approach. A balance test shows that precipitation does not predict lagged contributions. A placebo test confirms that rainfall does not predict *next* week's contributions.

Table 4: Robustness Checks

	ln_contributions				
	(1)	(2)	(3)	(4)	(5)
has_protest	2.270	1.634	5.272	2.270	1.612
	(10.37)	(6.335)	(59.59)	(10.37)	(6.299)
Observations	6,594	6,594	6,300	6,594	6,594
R^2	0.58077	0.58078	0.58108	0.58077	0.58078
F-test (1st stage), has_protest	0.04259	0.14463	0.00626	0.04259	0.07250
Sargan					0.00805
city_id fixed effects	✓	✓	✓	✓	✓
week_id fixed effects	✓	✓	✓	✓	✓

Dependent variable: ln(1 + contributions count). Column (1): baseline specification. Column (2): number of rainy days (>1mm) as alternative instrument. Column (3): excludes May–August 2020 (BLM protest peak). Column (4): restricts to cities with 10+ total protest events. Column (5): overidentified specification using both precipitation measures. All specifications include city and week fixed effects. Standard errors clustered at the city level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.4 Donor Extensity

Given the weak first stage, I do not interpret the 2SLS point estimates as informative about economic magnitudes. The donor extensity margin (unique contributors) shows a similar pattern to the contribution count—near-zero OLS association and uninformative IV—consistent with the instrument failure being the binding constraint rather than the absence of a true effect.

5. Discussion

The main finding of this paper is methodological: the Madestam et al. (2013) weather IV does not extend straightforwardly to media-coded protest data. This has three implications.

First, for empirical strategy, the weather instrument is designed for a specific data structure: events where physical attendance is both the treatment of interest and directly responsive to weather. The Madestam et al. application worked because the Tea Party rally was a known event with crowd-size variation driven by rainfall. GDELT, by contrast, codes events from media coverage, which responds to newsworthiness rather than weather. A rainy protest that generates dramatic images may receive *more* media coverage, not less. This creates a fundamental wedge between the instrument (weather) and the measured treatment (media-coded protest occurrence).

Second, the near-zero OLS association between protests and local contributions—while not causal—suggests that the protest-donation relationship may operate at longer time horizons or broader geographic scales than the city-week panel can detect. Protests may shift national donation patterns without affecting local giving, or the effect may accumulate over months rather than weeks (Wasow, 2020; Cantoni et al., 2019).

Third, for future work, credibly identifying the protest-donation channel requires either (a) crowd-size data linked to local contributions, as in the original weather IV design, or (b) a different instrument that directly affects physical protest participation but not media coverage or donation propensity. Candidates include public transit disruptions, competing public events, or stadium schedules that crowd out protest space (Madestam et al., 2013).

An important limitation beyond instrument weakness is that our FEC data captures only itemized contributions, which may underrepresent the small-dollar donors most likely to respond to protest mobilization. ActBlue and WinRed transaction-level data would better test the grassroots mobilization channel.

6. Conclusion

The weather IV for protest mobilization—a design that produced clean identification in the Tea Party setting—fails to generate a strong first stage when applied to media-coded protest data from GDELT. This negative result is informative: it reveals that the instrument's validity depends critically on having crowd-size data rather than media-based event counts, because media coverage and physical attendance respond to different determinants. The question of whether protests mobilize campaign contributions remains open and important. Answering it credibly requires either crowd-sourced attendance data linked to FEC records, or instruments that affect physical participation without contaminating media salience. The data infrastructure exists—platforms like ActBlue record exact donation timestamps, and the Crowd Counting Consortium provides crowd estimates—but linking them at scale remains a data engineering challenge, not a conceptual one.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers
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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Contribution count	2.270	(10.370)	0.485	4.682	(21.386)	Large positive
Contribution amount ($)	3.885	(17.690)	0.771	5.037	(22.934)	Large positive
Unique donors	2.179	(9.937)	0.427	5.098	(23.249)	Large positive
Panel B: Heterogeneous (sample split)						
Contributions (non-BLM)	5.272	(59.586)	0.496	10.630	(120.139)	Large positive

Notes: **Country:** United States. **Research question:** Whether larger street protests cause increases in local small-dollar campaign contributions, testing if street and financial mobilization are complements. **Policy mechanism:** Protests generate local attention, social pressure, and information about political causes, potentially converting civic engagement into financial contributions that reshape campaign fundraising. **Outcome definition:** Small-dollar campaign contributions (individual FEC Schedule A filings of $200 or less), measured as log count and log total amount at the city-week level. **Treatment:** Binary indicator for whether any protest occurred in city-week cell. **Data:** GDELT Events database (264,000+ GPS-coded US protest events) linked to FEC Schedule A individual contributions and Open-Meteo daily precipitation, 2018–2023, city-week panel covering 21 cities. **Method:** 2SLS with average weekly precipitation as instrument for protest occurrence; city and week fixed effects; standard errors clustered at city level. **Sample:** Cities with at least 5 protest events during sample period, restricted to city-weeks with available weather data. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment (pre-2020) standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

A. Standardized Effect Sizes