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The Drowning-out Effect: Voter Turnout, Uncertainty, and Protests

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Abstract

Conventional wisdom suggests that high turnout in a free and fair election is desirable; it might signify proper representation, help the government identify and address social discontent, and hence reduce the likelihood of protests. In this paper, I demonstrate that this intuition does not necessarily hold. By extending a bargaining theory, I hypothesize that high turnout "drowns out" the voices of dissenters, creates uncertainties over the social discontent, and thus causes post-election protests. I test this hypothesis using election-day rainfall deviation as an instrumental variable for turnout and apply it to a new constituency-level dataset of Indian elections. I also extend a new design-based method, called near-far matching, that makes the causal comparison more powerful, robust, and explicit. The result shows that higher turnout increases the occurrence of protests. This finding implies that electoral democracy can be inherently imperfect as a conflict resolution mechanism.

Keywords: Election, Turnout, Conflict, Protest, Instrumental variable
JEL classification: D72, D74

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On 31 March 2006, when the government of West Bengal, led by the Left Front, announced a deal with an Indonesian conglomerate, Salim Group, to set up a chemical hub spread over 10,000 acres in the town of Nandigram, it raised tensions between the government and the residents who would be displaced from their land. Two weeks after the agreement, the West Bengal State Assembly Election appeared to confirm broad-based support for the government. *Over 82 percent* of eligible voters turned out in the election, in which the Left Front obtained 50 percent of the votes and 78 percent of the seats. With the landslide victory, which was "beyond all our expectations" (Outlook India 2006) even for the party leader, the government announced the expansion of the project on 18 May, now promising further 10,000 acres of the land in the nearby town of Singur to Tata Motors. The announcement, however, triggered a protest by more than 3,000 people on 1 June. The protest escalated into a series of demonstrations that continued for years. Retrospectively, a witness states that "the poll outcome was *wrongly interpreted* as a popular support in favour of the path followed by the LF [Left Front] for industrialisation" (Dinda 2013, 28).

What are the effects of electoral participation on protests? Although to my best knowledge no studies have analyzed the effects of turnout on protests, our conventional wisdom suggests that high turnout in a free and fair election would be laudable; it might mean better representation of people's opinions, which could allow a government to identify and address social discontents and hence mitigate protests. The case of West Bengal, however, casts doubt on this intuition, implying an alternative, or even opposite, possibility; high turnout may make a government misinterpret its popularity, and hence make it even more difficult to resolve the conflict efficiently. Does high turnout really help to resolve social conflict without invoking protests? If not, what is the underlying logic?

In this paper, by extending a bargaining theory of conflict to elections, I argue that high turnout indeed *increases* the likelihood of post-election protest. Even though high turnout may make an election more indicative of the *average* citizen's preferences, it does not necessarily reflect the intensity of a minority's dissatisfaction with a government's policies. Indeed, in the contemporary world, a majority of people are not strongly interested in politics or do not participate in protests, often prioritizing their private lives (World Values Survey 2016). When those people happen to turn out, the vote shares are less representative of those who are motivated enough to protest. The resulting uncertainty can result in the bargaining failure and protest. Thus, rather counterintuitively, high turnout is predicted to increase the likelihood of protests.

Testing this hypothesis, however, raises empirical challenges. As an overwhelming number of electoral studies indicate, voter turnout is endogenous to various electoral strategies, including campaigns, policy stances, clientelism, and pre-election protest and violence. Drawing on American voter scholarship (Hansford and Gomez 2010), I address the problems by using election-day rainfall deviation as an instrumental variable (IV or instrument) for turnout and applying it to a new constituency-level dataset of Indian state elections. I also extend a new near-far matching approach to the IV analysis (Baiocchi et al. 2010; Keele and Morgan 2016), which can address weak-instrument bias and, perhaps more importantly, make the causal comparison more explicit and less dependent on parametric assumptions. Consistent with the theoretical expectation, the analysis shows that electoral participation raises the likelihood of protests after the elections.

Election and Conflict

A common explanation about the effect of elections on conflicts can be found in so-called sore-loser effects, which posit that competitive elections create "sore" losers and drive them to pursue options outside the political system, including violent protests and armed conflicts (Collier 2011).

This explanation however does not account for why a winner of the election does not accommodate or make concessions to the sore losers. In fact, from the perspective of bargaining theories (Fearon 1995), because having a protest is potentially costly (Pierskalla 2010; Little, Tucker, and LaGatta 2015),[1] the incumbent is better off by offering peaceful conflict resolution and hence avoiding the unnecessary risks of facing protestors. Thus, even if elections create sore losers, it does not eliminate the possibility of an efficient conflict settlement.

One possible explanation for the bargaining failure is informational uncertainties (Fearon 1995).[2] In the presence of asymmetric information, a government and opposition can have conflicting views about the strength of their support bases. The government can underestimate the popularity of the opposition and thus propose a conflict resolution that is unacceptable to its opponents. The opposition rejects such an offer and initiates protests to compel their preferred policies (Londregan and Vindigni 2006; Cheibub and Hays 2017) or, more realistically, to signal the strength of their support base (Little, Tucker, and LaGatta 2015).

From this perspective, an election is considered as an institutional medium through which people express their discontent. It is possible that free and fair elections would reveal the public support for each party, prevent strategic miscalculations, and thus lessen the need for violent or non-violent protests (Przeworski 1991; Londregan and Vindigni 2006; Little, Tucker, and LaGatta

[1] The costs include a lower likelihood of victory in the next election (Madestam et al. 2013), potential escalation to armed conflict (Little, Tucker, and LaGatta 2015), and lower stock market evaluation on the firms associated with the incumbent (Acemoglu, Hassan, and Tahoun 2018).

[2] The other possible avenue is the logic of commitment problems (Fearon 1995), which has been applied to the cases of post-war elections (Brancati and Snyder 2011; 2013).

2015; Cheibub and Hays 2017). Contrariwise, as a number of recent studies show (Daxecker 2014; Hafner-Burton, Hyde, and Jablonski 2016; Wig and Rød 2016), fraud and rigging can make the election a biased signal of popular opinion, which can create a risk of misunderstandings and hence incentivizes dissidents to take extra-institutional means. The unconsolidated democratic culture, including parochial or ethnicized politics (Varshney 2003; Wilkinson 2006), the lack of democratic experience (Salehyan and Linebarger 2015), and post-war instabilities (Brancati and Snyder 2013), can also undermine respect for electoral outcomes and result in conflicts and protests.

What is missing in these studies, however, is a possibility that elections can be imperfect as a signaling mechanism; because elections by themselves can only provide information about *aggregated* vote counts, they may not perfectly reflect the distribution of preference intensity. For example, Little et.al (2015) and Cheibub and Hays (2017) incorporate this possibility into their formal models, in which elections provide varying qualities of information about popular opinion and thus result in different likelihoods of protests or violence. These studies, however, treat the quality of the revealed information as exogenous parameters and thus do not explain why some elections provide precise information while others do not.

One potential answer to the question lies in *electoral participation*. Intuitively, one might argue that elections could accurately represent public opinion only when a large number of, and if possible all, citizens cast votes. For instance, Fearon (2011), who examines the roles of elections in collective actions against a ruler, assumes that *all* people cast either Yes or No votes for an incumbent. With this assumption, the elections are expected to reveal people's opinions, allow the citizens to coordinate their collective actions, and hence incentivize the ruler to appease the citizens. Similarly, Londregan and Vindigni (2006) also assume that voting is costless, and thus that *every* individual (at least weakly) prefers voting to abstention. This central feature of the model ensures that the elections provide precise information about parties' support bases, reduce the risks of strategic miscalculation, and hence allow negotiated conflict resolution.

Although the unanimous-turnout assumption in these studies is useful for their own purposes, it does not deny the importance of analyzing the strategic consequences of electoral participation. In fact, a large fraction of people in contemporary democracies are not strongly interested in politics, elections, or protest. According to the World Value Survey (2016),[3] 52.5% of the 69,553 respondents say they are not interested in politics, 47.6% of them have never and will never join any political activities, and 14.7% answered that they have never voted either national or local elections. Even among those who have cast votes, 45.3% said they would never participate in political actions. That is, for ordinary citizens, political issues have only secondary importance, and even those who turn out in elections may not be interested in protests or other political activities. If these less motivated citizens turn out in elections, does it help or hinder the information revelation mechanism?

The Drowning-out Effect

Based on the bargaining framework, I explain how high turnout can "drown out" the voices of dissenters, create uncertainties about the size of social dissents, and hence result in bargaining failure and protests.[4] Throughout this section, I assume turnout would be exogenous to players. Although this assumption may not be realistic, the simplification is "useful" (Clarke and Primo 2007) unless turnout would be *completely* determined by endogenous factors. Additionally, consistent with the literature (Little, Tucker, and LaGatta 2015; Fearon 2011) and the case of India,

[3] Non-responses are dropped.

[4] Due to the page limit, I leave the formal model and its proof to Supporting Information 1 and 2.

I consider a majoritarian system with mostly free and fair elections. I also focus on a case in which a loser candidate and its supporters can protest, though the argument can be extended to a case in which supporters of a winning candidate can protest after an election (e.g., protests against a winner's reneging on promises).

Drowning-out effect

Let me consider a standard bargaining setup after an election (Londregan and Vindigni 2006; Cheibub and Hays 2017). Suppose a winner of an election, who can make an offer to settle a dispute with a loser candidate or dissidents (e.g., land appropriation in the case of Singur Conflict). The opposition, by contrast, can either accept the offer or refuse it and initiate a protest. Having a protest with a large number of participants is costly for the winner (Pierskalla 2010; Little, Tucker, and LaGatta 2015),[5] whereas a small-scale protest will not have such negative consequences. Thus, ideally, the winner would like to make a concession only if a large number of people would join a protest; then she can avoid making an unnecessary concession to a weak opposition, or making an unsatisfactory offer to a strong opposition. A problem, however, is that the winner does not know the exact number of protestors a priori; as Kuran (1991) argues, protests are subject to unforeseeable expansion with a sudden increase of participants. Thus, the winner needs to use available information, guess the number of protestors, and hence make a decision that is optimal in her expectation.

In this regard, electoral outcomes, including turnout and vote shares, provide useful information (Londregan and Vindigni 2006; Fearon 2011; Cheibub and Hays 2017). Importantly, however, the information provided by the election does not *completely* reveal the number of

[5] See footnote 1 for details about the incumbent's costs of protests.

protestors. Because the costs for voting are usually smaller than the costs for joining protests, a fraction of people turn out and cast votes for the opposition even if they are unwilling to join protests. This means that the loser's absolute vote share is comprised of protestors and non-protestors, and thus it does not signal the exact number of protestors.

I argue that the uncertainty remains high especially when the turnout of an election is *high*. When voting costs are large for exogenous reasons such as election-day weather (thus turnout is low), the citizens turn out only if they are so politically motivated that they do not mind incurring such large costs for voting. Among those voters, only strong opponents of the winner will cast votes for the opposition candidates. Those voters are, however, precisely the people who are likely to join protests; they are the citizens who are the most hostile to the winner. Indeed, in an extreme case where voting would be as costly as joining protests, only protestors could incur such large costs and hence vote for the opposition. As a result, the loser's absolute vote share would be exactly the same as the fraction of protestors in the society. This allows the winner to obtain precise information about the number of protestors. In a sense, an election of low turnout *sifts* protestors from non-protestors and hence provides reliable information about the size of protestors.

In contrast, when the costs for voting are relatively small (thus turnout is high), people who only marginally prefer the opposition over the winner can turn out and cast votes. Those citizens are, however, are not so politically motivated and hence are not willing to join protests. As a result, the loser's absolute vote share is comprised of a relatively large number of non-protestors, and thus the election does not provide precise information about the size of a protest. Indeed, if the winner would simply count the number of loser's votes, it would substantially overstate the number of protestors. Thus, with high turnout, the voices of non-protestors *drown-out* the voices of protestors, and hence the uncertainty remains high.

The uncertainty can result in bargaining failure and protests as suggested by the bargaining literature (Londregan and Vindigni 2006; Cheibub and Hays 2017). With high turnout, the election does not provide precise information about the size of protestors. The remaining uncertainty can make her miscalculate the number of protestors; she may underestimate the number of protestors, believe that a protest, if any, would be small and unsuccessful, and hence make little or no concession to the dissenters. The opposition, however, refuses such an unsatisfactory offer and call for a protest. The opposition tries to either impose their preferred outcome by large-scale protests, or to credibly signal their power via a protest. By contrast, when the turnout is low, the winner can obtain more precise information about the number of protestors, and hence it is less likely that she makes such a miscalculation. Low turnout therefore *helps* the winner properly adjust its strategic choices, strike a bargain, and hence achieve a negotiated settlement.

Hypotheses

Even though it is impossible to test all of the logical steps of the theory, we can test its observable implications. The first and critical hypothesis is that *high* turnout causally relates to the *higher* likelihood of protests after an election. This hypothesis is rather counterintuitive; high electoral participation, which we usually think is desirable, can result in protests. The hypothesis is also not expected by the sore-looser explanation and hence constitutes a critical hypothesis.[6]

The second and auxiliary hypothesis is that loser's *high* absolute vote share causally relates to the *higher* likelihood of protests after an election. Importantly, this hypothesis is *not* based on the logic that the loser's high absolute vote share means a larger number of protestors and hence results in a protest (sore-loser effect); instead, the reason is that with the high loser's absolute vote

[6] To my best knowledge, no study applies the sore-loser explanation to the effect of turnout.

share, the loser's votes are comprised of a relatively large number of non-protestors and hence there exists remaining uncertainty (drowning-out effect). Because this hypothesis itself cannot differentiate the sore-loser and drowning-out effects, I consider this hypothesis as auxiliary.[7]

An important note is that the hypotheses pertain to the effects of *exogenous* variation of turnout, and that the theory is not applicable to turnout's variation *endogenous* to the winner's and opposition's choices, such as mobilization and turnout of motivated voters (Nichter 2008), candidates' policy positions (Mayer 2007), clientelism and vote buying (Bratton 2008; Gans-Morse, Mazzuca, and Nichter 2014), voter intimidation (Klopp and Zuern 2007; Robinson and Torvik 2009; Kibris 2011), and pre-election violence and protests (Blattman 2009; Dunning 2011; Koch and Nicholson 2016; Harish and Little 2017). Although this certainly limits the scope of the theory, it also allows us to theoretically isolate the effect of turnout from the endogenous relationships. In fact, electoral studies emphasize how exogenous changes in turnout can potentially result in large differences in vote shares (Hansford and Gomez 2010). An empirical challenge, however, is that I need to find an exogenous variation in turnout. The causal identification is important not only for empirical rigor but also for ensuring the consistency between the theory and empirical analysis.

[7] Note that the effects of *relative* vote shares are less relevant to my theory. In addition, because relative vote shares are not affected by election-day weather, I cannot estimate the causal effect of relative voter shares with the instrumental variable approach. As I later mention, however, the effects of turnout *on* relative vote shares are relevant.

Research Design

Given the concerns with endogeneity, a critical question is how we can make the test closer to an ideal experiment. One way is to find an as-if randomly assigned variable, called an instrumental variable, that affects turnout and absolute vote shares. By restricting the variation of the explanatory variables to such exogenous variation, I can isolate the causal effect from any endogenous relationship. As an instrumental variable for turnout, American voting scholars use election-day rainfall deviation, measured as the amount of rainfall on an election day minus the average rainfall on the same day but in different years (Hansford and Gomez 2010). As widely recognized both in conflict and electoral studies, rainfall deviation can be considered as-if random and hence provides an opportunity for a natural experiment (Miguel, Satyanath, and Sergenti 2004; Afzal 2007; Ritter and Conrad 2016; Eynde 2018). Importantly, because the instrument is election-day rainfall *deviation*, its variation cannot be explained by climatic, geographical, or seasonal conditions (Hansford and Gomez 2010).[8] Moreover, because the IV affects turnout, it has an effect on *absolute* vote shares as well.

The instrumental variable analysis, however, does not yield the estimates of the causal effect without additional assumptions. One possibility is that rainfall deviation would affect the onset of protest except for its effect via turnout or absolute vote shares, and thus that we could not

[8] Because the rainfall deviation is a function of normal rainfall, one might think that the instrument is confounded by normal rainfall. Although this might be true for a particular observation, the expected value of rainfall deviation is independent of normal rainfall, as $\mathrm{E}\left[rain_{i,t} - \overline{rain}_{i,t}\right] = 0$ for any i. Indeed, in the following analysis, the average rainfall deviation is near zero (0.016 mm/h), and the correlation between the rainfall deviation and normal rainfall is 0.006 ($p = 0.50$).

easily isolate the effect of turnout from the circumventing effects. In fact, rainfall deviations are shown to affect a variety of phenomena, including economic production (Miguel, Satyanath, and Sergenti 2004; Afzal 2007; Eynde 2018) and political violence (Ritter and Conrad 2016). Given these findings, it might be hard to assume that rainfall deviation would have no circumventing effects (the assumption called exclusion restriction).

At this point, Hansford and Gomez (2010)'s approach is distinguished from other applications of rainfall instruments and perhaps offers something new to conflict studies. Instead of using annual or monthly rainfall deviation, they propose *election-day* rainfall deviation as an instrumental variable. While rainfall deviation in general has broad effects, the effects of rainfall deviation on a very particular day should be fairly limited and thus less likely to violate the exclusion restriction. For instance, while excessive annual rainfall can substantially affect agricultural production, which can, in turn, affect protest probabilities, rainy weather on an election day cannot have such a large impact. In fact, there is no excessive election-day rainfall (i.e., rainfall disasters) for the period of analysis.

Although election-day weather might directly affect protests on the polling day (Ritter and Conrad 2016), election-day protests are prohibited and hence extremely rare in India, and, more importantly, this study analyzes protests after an election *excluding those on the election day*. Thus, unless election-day rainfall would directly affect protests several days or months after the election, the exclusion restriction holds. In the later robustness checks, I also examine the possibility that election-day rainfall would affect election-day protests, which in turn could relate to later protests, and the possibility that election-day rainfall could correlate with later rainfall, which in turn would affect post-election protests.

The other possibility is that election-day rainfall has no tangible effect on turnout or absolute vote shares. If this were the case, the instrumental variable would be irrelevant and could tell us nothing about the causal effect. Furthermore, even when an instrument has a statistically significant effect but the predictive power is weak, the weak instrument still produces large bias and makes the conventional estimator (two-stage least square: TSLS) extremely sensitive to small errors.[9] A powerful instrument, by contrast, is robust to minor violations of random assignments (Angrist, Imbens, and Rubin 1996; Stock, Wright, and Yogo 2002).

The weak instrument, however, can be a real problem for the election-day rainfall instrument. From one perspective, election-day rainfall appears to increase the physical costs of voting and hence depress turnout rates (Hansford and Gomez 2010). The shift in voting costs however may be negligible (Persson, Sundell, and Öhrvall 2014). On the other hand, election-day rainfall can also decrease the opportunity costs of voting; people may have fewer things to do on a rainy day, or they may leave work early. People can spend the extra hours voting (Lind 2014; 2015; Kang 2015). In fact, recent findings differ across countries (Artés 2014; Arnold and Freier 2016; Meier, Schmid, and Stutzer 2016). Although this paper is not intended to settle this dispute in electoral studies, these studies provide theoretical reasons to suspect the weak-instrument bias.

[9] The other required assumptions include the stable unit treatment value assumption (SUTVA) and the monotonicity (Angrist, Imbens, and Rubin 1996). The monotonicity assumption can potentially be violated in this study, but the assumption can be relaxed (de Chaisemartin Clément 2017). Regarding the SUTVA, see the later subsection *Instrumental Variable*.

Near-far Matching for Instrumental Variable Analysis

A recent refinement of an instrumental variable design, proposed by Baiocchi et al. (2010; 2012) in statistics and more recently Keele and Morgan (2016) in political science, provides a design-based solution to the weak instrument problems. Their insight is that we can explicitly incorporate the strength of an instrumental variable into the framework of matching and hence to optimize both the instrument strength and covariate balance. Because the method is based on matching, it not only enhances the predictive power of an instrument but also makes the causal comparison more explicit and less reliant on functional form assumptions (Baiocchi et al. 2010; 2012; Keele and Morgan 2016). Furthermore, because the matching method is built upon a non-bipartite matching algorithm (Lu et al. 2011), it can fully accommodate a continuous instrumental variable and thus can be used in a greater variety of applications.

The intuition behind the method is that instead of comparing all units at once, it is better to compare units that are similar in covariate values but *different* in the values of an instrumental variable. Because they are different in the values of the instrument, their turnout rates are also expected to be different as well, indicating a more powerful comparison. The near-far matching creates those pairs by "penalizing" units that have similar values in the instrument. In particular, the matching is done with a penalized distance metric;

$$w_{ij}^{*} = \begin{cases} w_{ij} + ce^{-|d_{ij}|} & \text{if} \quad |d_{ij}| \leq \tau \\ w_{ij} & \text{otherwise} \end{cases},$$

where i, j denotes two units in a sample, w_{ij} is the rank-based Mahalanobis distance, which is robust to outliers (Rosenbaum 2009; Keele and Morgan 2016), and d_{ij} is the difference in the values of an instrumental variable. The size of penalty c is usually set to a large integer (Rosenbaum 2009; $c = 1000$ in this study), and the fraction of the penalized units τ is selected by a grid-search (in this study, $\tau = 0.3$).[10] Using the penalized distance metric w_{ij}^{*}, a non-bipartite matching algorithm (Lu et al. 2011) creates pairs that minimize the distances.[11] Once the matching is done, we assign a "treatment" status $T_i = 1$ if its rainfall deviation is larger than its counterpart in a pair, and otherwise give a "control" status $T_i = 0$.[12]

Because the matching creates a dichotomous instrumental variable (T_i), we can use a variety of estimators that are more powerful and robust, and require fewer assumptions than the TSLS and its cousins.[13] Following the literature (Andrews and Marmer 2008; Baiocchi et al. 2010; Keele and Morgan 2016), I use a Hodge-Lehmann (HL) non-parametric estimator, which is more powerful than the Anderson-Rubin semi-parametric estimator (1949) commonly used in economics (Andrews and Marmer 2008). Intuitively, the HL estimator is derived from a series of hypothesis tests regarding a causal quantity λ:

$$\left(Y_{i:T_i=1} - Y_{i:T_i=0}\right) = \lambda\left(D_{i:T_i=1} - D_{i:T_i=0}\right),$$

where $Y_{i:T_i=1}$ and $Y_{i:T_i=0}$ are the outcome variables (the onset of protest) when unit i is treated and not treated respectively, and $D_{i:T_i=1}$ and $D_{i:T_i=0}$ are the corresponding explanatory variables. The

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[10] For the detail of the grid-search, see Supporting Information 7. Note that as far as the penalty is heavy, nearfar matching is robust to the specifications of the penalty function (Rosenbaum 2009).

[11] The algorithm drops several outlier observations that can potentially worsen the quality of covariate balance. The fraction of the dropped observations (ρ) is another tuning parameter that requires a grid-search (in this study, $\rho = 0.1$). See Supporting Information 7.

[12] Although dichotomizing an *instrumented* variable (turnout) can introduce biases (Marshall 2016), such biases do not arise when we coarsen an *instrumenting* variable (rainfall deviation).

[13] In the later robustness check, I also report the TSLS estimates.

parameter λ denotes the causal effect. Under the set of the assumptions which I described above, the causal quantity is estimated by $\hat{\lambda}$;

$$\left(Y_{i:T_i=1} - Y_{j:T_j=0}\right) = \hat{\lambda}\left(D_{i:T_i=1} - D_{j:T_j=0}\right),$$

where i, j are treated and control units that are paired by the matching. The point estimate $\hat{\lambda}$ and its confidence interval are obtained by conducting a series of non-parametric tests for $\hat{\lambda} = \lambda_0$ (Wilcoxon signed-rank sum tests) and retaining the value of λ_0 that is not rejected with the highest *p*-value or those not rejected at a 5% significance level. The non-parametric estimator is particularly advantageous for this study as the outcome is binary and hence linear models are "worrisome" if not misleading (Baiocchi et al. 2010, 1293). Note also that the HL estimate is concerned with pairwise differences, which are analogous to having $\frac{n}{2}$ pair-specific fixed effects in a linear regression model.

Case and Measurement

I apply the near-far matching IV method to the case of Indian State Assembly elections. India is one of the quintessential examples in which long-standing democracy and violent and non-violent protests coexist. Given the availability of data about the protest, rainfall, and covariates, the sample includes all assembly elections between 2000 and 2015.[14] Although I could conduct a similar analysis for the National Parliamentary elections, the area size of the parliamentary constituency is relatively large, and thus the sample size is fairly limited (around 500 units). Previous studies also emphasize the importance of State Assembly elections in Indian politics (Ziegfeld 2016;

[14] The geocoded census data are available only after 2000, and the latest data for the other variables are available until 2015.

Tudor and Ziegfeld 2019). The following analysis therefore consists of over 4,300 constituencies[15] in the 94 State Assembly elections between February 2000 and November 2015. A majority of the states have two or three elections in an interval of five years.[16] The overall sample includes 11,416 election-constituency observations (before matching).[17]

In the period of 2000–2015, the Indian economy experienced rapid growth, and the political situation in the central government had been relatively stable. The elections in this period are generally free and fair backed by the long history of democracy. In fact, the Election Committee of India is believed to be one of the most competent public institutions in India (Banerjee 2007). The State Assembly elections are held under the first-past-the-post rule. The Election Committee of India decides polling dates one to three months ahead of an election. Depending on the size of

[15] 3,915 and 3,772 constituencies before and after the 2008 delimitation respectively.

[16] Because the interval between elections is long and the dataset is a wide but short panel, I consider the dataset is as-if cross-sectional. Clustering the standard errors for each constituency does not change the results. See Supporting Information 9.

[17] Because the sample size is large and thus it is computationally too expensive to apply the near-far matching algorithm to the entire sample, following Rosenbaum (2009), I subset the sample to groups of elections that were held at the same period, apply the matching within each group, and pool the matched observations. I apply the matching to each election group instead of each election, as some states, including Tripura, Mizoram, Meghalaya, Sikkim, and Union Territories, are too small for the matching. Matching within each state results in weaker power of the instrument, but the main results still hold (the results are available upon request). Table SI.6.1 is the list of the election groups.

a state, they create one to seven groups of constituencies and assign a different polling date to each group. All polls are usually done within a month. Due to heavy rain in the monsoon season (June – September), a majority of the elections are held in dryer months. Note that although these selection processes may indicate a non-random assignment of rainfall amounts, this study uses election-day rainfall *deviation* as the instrument, which is beyond the control of the election committee.[18] In fact, the IV design is particularly useful for the case of India, in which clientelism and parochialism are persistent and hence electoral results are hardly exogenous.

Outcome Variable

The outcome variable is the onset of protests after an election, which is derived from the Integrated Crisis Early Warning System (ICEWS; Boschee et al. 2015). The ICEWS dataset is a machine-coded event data based on over 38 million multilingual (including Hindi and other languages in India) news stories all over the world. The ICEWS dataset is even accredited as "the current gold standard for event data" (Metternich et al. 2013, 901).[19] Moreover, the ICEWS dataset has a finer classification of protest types, including demonstration, hunger strikes, strikes, obstruction, and riots, which allows me to test underlying causal mechanisms (see the later section about causal mechanisms). My outcome variable takes a value of 1 if there is at least one event of a protest within one-year after a polling day (excluding the polling day).[20] Because ongoing protest and its

[18] The postal votes are limited to service and other special voters (less than 1%) and repolling is also extremely rare.

[19] For details of the ICEWS dataset, refer to O'Brien (2010).

[20] I also use different time windows. See the later section about robustness checks.

cessation are conceptually different from the onset of new protests,[21] following the standard definition of "onset" (Sundberg and Melander 2013), I exclude the observations of pre-election protests (1,100 observations; about 8.71% of the sample before matching). The sample contains 573 onsets of protests (5.02% of the sample; before matching).[22]

It is worthwhile to note that alternative datasets of protests are limited. Currently, Varshney-Wilkinson dataset about Hindu-Muslim riots (Varshney 2003; Wilkinson 2006) is available only up to 2000 at the level of districts. For this period, Indian democracy is not always stable, and the rainfall data are unavailable. Furthermore, given the potentially large costs of participation in riots, including the risks of arrest and detention, riots are unlikely to be suitable for testing my hypothesis (as the scope of the hypothesis is limited to protest with relatively small costs; see the later section about causal mechanisms). Similarly, the Uppsala Conflict Data Program Georeferenced Event Dataset (Sundberg and Melander 2013) has data about violent events but not non-violent protests. The Armed Conflict Location and Event Data (Raleigh et al. 2010) has records about protests and riots in India but only after 2016, for which period the rainfall and covariates are not available. They also do not distinguish violent and non-violent protests.

[21] Although my theory assumes the existence of *conflict of interests* before an election, it considers the situation in which the conflict is not yet materialized to protest before the election.

[22] The data summary and their map are in Supporting Information 5. There are 8,682 protest events one year after elections. We dichotomize them and exclude the observations of pre-election protest.

Finally, the Social Conflict Analysis Database (Salehyan et al. 2012) or Nonviolent and Violent Campaigns and Outcomes (Chenoweth and Lewis 2013) dataset has no data for India.[23]

Another issue is the potential for reporting biases (Weidmann 2016). Because the outcome variable (ICEWS) rests on media reports and hence is susceptible to reporting biases, the *instrumental* (not explanatory) variable must be free from such errors. Otherwise, if *both outcome and instrumental variables* would be contaminated by reporting biases, the causal estimate could also be biased.[24] The satellite-based data of rainfall, which are obtained regardless of media coverage, provides a way to guard against such a possibility. In a later robustness check, I also conduct a placebo test to assure that the main findings are not explained by reporting biases.

Explanatory Variables

The explanatory variables are the turnout rate and loser's absolute vote share. The data are scraped from the *Statistical Report on General Election* published by the Election Committee of India. The average turnout rate is high (0.69) in India. The dataset also contains other fine-grained information, including polling dates, valid votes, gender ratios, and the number of polling stations.

Instrumental Variable

The instrumental variable is the polling-day rainfall deviation, which comes from the Climate Prediction Center Morphing technique (CMORPH) satellite images. The images are available every thirty minutes from 1 January 2000 to 2015 at a spatial resolution of eight-by-eight kilometers. The CMORPH products are created from sensors of multiple satellites (Xie et al. 2011;

[23] The Electoral Contestation and Violence dataset (Daxecker, Amicarelli, and Jung 2019) is available only for national elections.

[24] See Supporting Information 12 for detailed discussion.

Joyce et al. 2004).[25] The normal rainfall is estimated by the average rainfall amounts five days around the date of a polling day but in different years (1998-2015).[26] The polling-day rainfall deviation is the difference between the observed and normal total rainfall for voting hours. The average rainfall deviation is, as expected, near-zero (0.016 mm/h). To be sure, there is no observation of extreme rain that would cause floods or other natural disasters. Moreover, because the rainfall data are based on satellite images, they are not affected by the problems relating to weather station data (Schultz and Mankin 2019).

A possible issue for the rainfall deviation measure is the spatial correlation (Hansford and Gomez 2010). This study guards against such a possibility both by design and method. Because the polling dates are different even within a single State Assembly election in India, rainfall deviations should be less correlated even within a state. Furthermore, the near-far matching and the HL estimator exploit the variation *within* matched pairs. Because the matched pairs are expected to have different values in the rainfall deviation, it is unlikely that they are neighboring constituencies, or that a unit is affected by the treatment status of the other unit in its matched pair. Thus, the near-far matching not only strengthens the instrument but also provides a design-based approach to spatial dependency.

Covariates

An important control variable is the past-one-month average of rainfall deviations. Because deviant rainfall in past can induce rainfall in an election day, the past rainfall needs to be controlled. The other covariates, which are not necessary for making a valid inference but are useful for

[25] For details of the rainfall measurement and other data sources, see Supporting Information 3.

[26] See Supporting Information 3 for details.

guarding against possible violations of the IV assumptions, are selected by using Keele and Morgan (2016) as a baseline and considering unique characteristics of India.[27] They include logged population, the proportion of Muslims, scheduled tribes and castes, urban population, and farmers[28] recorded in the 2001 Census of India.[29]

Results

The following table (Table 1) shows the estimates of the effect of turnout rates on the onset of protest and its 95% confidence intervals. Consistent with the drowning-out hypothesis, the result shows that turnout indeed increases the likelihood of protest in the subsequent period. The point estimate 1.447 indicates that if a turnout rate increases by 1 percentage point, it increases the probability of subsequent protest by 1.447%. Given the rarity of protests (only 5.019% of the sample), the effect size is not small; compared to the sample average, a unit has 1.29 times higher probability of protests if its turnout rate is 1 percentage point higher.

Table 1. The Effect of Turnout on the Onset of Protest

	protest
Point estimate	1.447
95% CI	[0.339, 3.043]

$n = 10{,}260$.

As reported in Supporting Information 8 (Table SI.8.1), the near-far matching improves both the covariate balance and the instrument's strength, giving further credence to the above finding. While the covariates are somewhat imbalanced without the near-far matching, probably

[27] The summary of the data is provided in Figure SI.5.1.

[28] I include the indicator of farmers to account for rural non-farmers (e.g. livestock raiser).

[29] For detail of the covariates, see Supporting Information 4.

due to finite-sample errors, the near-far matching properly adjusts the remaining imbalances. Furthermore, even though the power of the instrument measured by the first-stage F statistic is 2.1 before matching, the near-far matching raises it to 29.0, which is far above the conventional criterion of 10 (Stock, Wright, and Yogo 2002). The covariate balance and stronger instrument mean that the above finding is robust to subtle violations of the random-assignment assumption.

On average, an upward deviation in election-day rainfall has an effect to increase turnout rates.[30] The treated observations have a 0.8 percentage points higher average turnout rate than the control observations (with a corresponding confidence interval $[0.2, 1.3]$). While this finding is contradictory to the explanations based on physical costs (Hansford and Gomez 2010), it is consistent with those based on opportunity costs (Kang 2015; Lind 2015; 2014). That says, this paper is not intended to evaluate these explanations in electoral studies. Because the existing findings are different across countries, future studies may need to look at multiple countries and explore institutional and cultural conditions.

Auxiliary Hypothesis: Absolute Vote Share

The following table (Table 2) shows the effect of the loser's absolute vote share on the onset of protests. Because election-day rainfall simultaneously affects both turnout and absolute vote shares, the instrumental variable design is readily applicable to the absolute vote share as well (in fact, the first stage F statistic is 19.5 after the near-far matching). The analysis indeed shows that the higher the loser's absolute vote share is, the more likely the post-election protests are. On average, if the loser's absolute vote share increases by 1 percentage point, it raises the probability of a subsequent

[30] On average, the treated observations have 0.094 mm/h more precipitation than their normal amounts, while the control units have 0.063 mm/h less precipitation than usual.

protest by 2.177 percentage points. The large effect size is not surprising given the fact that the loser's vote share is a proximate cause of protests.

Table 2. The Effect of the Loser's Absolute Vote Share on the Onset of Protest

	protest
Point estimate	2.177
95% CI	[0.506, 4.991]

$n = 10{,}260$.

Causal Mechanisms and Robustness Checks

Although rigorously testing every step of the bargaining theory is difficult and, generally speaking, testing a theoretical model is not possible (Clarke and Primo 2007), it is still important to examine underlying causal mechanisms. To this end, I theoretically consider possible alternative explanations and test their observable implications. Given the limitation of the measurement and research designs, I do not intend to definitely exclude the alternative explanations; the following analyses should be considered as suggestive evidence.

Causal Mechanisms I: Habituation

An alternative explanation is habituation; with electoral participation, people might be habituated to political participation and hence more willing to join electoral and non-electoral activities, including protests. I test this explanation by analyzing the effect of turnout on electoral participation in the next election (especially, I use the first difference of turnout in order to remove autocorrelation; for detail, refer to Imbens and Wooldridge (2009)). The theoretical underpinning is that if turnout would have an effect on the habituation to non-electoral participation, it should have a similar or even stronger effect on electoral participation. The following table (Table 3), however, indicates that turnout does not significantly increase turnout in the next election. In fact, not only the standard error is large (which to some extent can be explained by missingness), but also the point estimate is close to zero.

Table 3. The Effect of Turnout on Turnout in the Next Election

	Δturnout
Point estimate	0.029
95% CI	[-0.574, 3.189]

Because $turnout_{t+1}$ is missing for the last elections, and also because a number of constituencies are merged or eliminated in 2008, there are a large number of missing values; $n = 2{,}969$.

Causal Mechanisms II: Mobilization and Enthusiasm

In an analysis about the effect of turnout on the recurrence of civil war, Letsa (2016) argues that active electoral participation results in an intense atmosphere, elites' mobilization, and uncontrollable momentum of escalation to violent conflict (to be sure, this mechanism posits that high turnout could *cause* mobilization, *not the other way around*. The *effects of* mobilization are well accounted for by the IV design and hence do not constitute an alternative explanation). Although I believe that the context is different for non-violent protests, the outcome variable in this paper actually includes riots, which may raise a concern that the results might be more consistent with Letsa's argument. I address this concern by comparing the effects of turnout on riots and other non-violent protests (demonstration, strikes, hunger strikes, and obstruction). I also analyze the effect on the mobilization of armed forces (such as the call for violent uprisings and the deployment of rebel forces), which is available in the ICEWS dataset. As seen in Table 4, the effects on riots and force mobilization are not statistically significant and substantively very small. The point estimate for riots is less than one-tenth of the main estimate, and even the upper bound is less than half of the main estimate. The effect on force mobilization is zero. By contrast, the estimate about the effect on non-violent protests is very similar to the main estimate.

Table 4. The Effects of Turnout on the Onsets of the Different Types of Protest

	non-violent protest	riot	force mobilization
Point estimate	1.525	0.129	0.000
95% CI	[0.453, 3.106]	[-0.323, 0.626]	[-0.230, 0.235]

$n = 10{,}260$.

Causal Mechanisms III: Party Victories

Yet another explanation can be electoral victories. That is, because different parties have different policies, if turnout would affect party victories, it might also change government policies and thus influence the likelihoods of post-election protests. As seen in Table 5, however, I do not find any evidence that high turnout affects the winning probabilities of the two major parties' or other minor parties' victories in India. Although this does not indicate that turnout has "no" effect on winning probabilities, the party victories do not explain the main results of this paper.

Table 5. The Effects of Turnout on Party Victories

	INC victory	BJP victory	Other's victory
Point estimate	-0.853	-0.207	1.060
95% CI	[-3.238, 1.218]	[-2.056, 1.694]	[-1.012, 3.394]

INC: Indian National Congress. BJP: Bharatiya Janata Party. $n = 10{,}260$.

Assumption Check: Relative Vote Share

In the theoretical argument, I focus on the turnout of less motivated citizens. As far as the turnout of those citizens is more sensitive to election-day weather, the IV analysis can provide the quantities of theoretical interest (the average effect local to the less motivated citizens). Nonetheless, one might argue that the turnout of politically motivated people might be even more sensitive to election-day weather. Although I am rather skeptical of this view and it is impossible to directly test the assumption, I can test the observable implication. If I can assume that election-day weather affects motivated citizens but the effects are similar regardless of their party supports, the changes in turnout should not alter the *relative* vote shares of the parties.[31] By contrast, if election-day weather only affects less motivated citizens and if their voting behaviors are less predictable, high turnout should increase the relative vote share of losers and thus shrink the winning margin.[32] I test these implications by changing the outcome variable to the loser's relative vote share. Table 6 indeed indicates that high turnout increases the loser's relative vote share (that is, smaller winning margin).

Table 6. The Effect of Turnout on the Loser's Relative Vote Share

	loser's relative vote share
Point estimate	0.469
95% CI	[0.125, 0.901]

$n = 10{,}260$.

Robustness Checks I: Exclusion Restriction

I also conduct a series of robustness checks. First, if an upward deviation of polling-day rainfall were to *increase* the likelihood of protest on the same day, and if the protest probabilities are positively autocorrelated over time, it would violate the exclusion restriction and thus invalidate

[31] Consider that 50 supporters of Party A and 20 supporters of Party B cast votes in an election (the relative vote share is therefore 10:4). If weather affects both parties in the same manner, we can multiply the turnout by a constant $\alpha > 0$. This, however, does not change the relative vote share, $50\alpha: 20\alpha = 10: 4$.

[32] Continuing the example, now consider additional 20 voters who are less motivated and hence randomly cast votes to Party A or B (this assumption is not necessary for deriving the main hypotheses. See Supporting Information 1 and 2). If those citizens also turn out, the expected value of the relative vote share becomes $(50 + 0.5 * 20): (20 + 0.5 * 20) = 10: 5$.

the causal inference. However, polling-day protest is prohibited in India and hence extremely rare (only 5 observations: <0.001% of the sample). In fact, a falsification test provides no discernible relationship between the IV and the onset of polling-day protests.[33]

Similarly, if rainfall deviations positively correlate over time, and if rainfall would *increase* the likelihood of protest after elections, it would also create a spurious relationship. However, the correlation between the election-day rainfall deviation and average rainfall deviation one month after the elections is very weak ($r = 0.065$).[34] To be sure, I also analyze the effects of turnout on protests in different post-election periods. Because a rainfall deviation in a single day is unlikely to predict a rainfall deviation several months later, I would be more confident if turnout has impacts on protests several months after polling. I find similar effects for the periods from the three to eight months after elections, and the effects are statistically indistinguishable from zero in the other periods. Theoretically, this result makes sense because it will take several months until a new government revises policy, and also because the information revealed by the elections will also become obsolete as time passes.[35]

Robustness Checks II: Reporting Bias

If *both* the protest and rainfall measures would be contaminated by reporting biases, it could bias the causal estimate. Although I do not find compelling reasons for reporting biases in the satellite-

[33] See Table SI.10.1.

[34] Controlling for the rainfall deviation one month *after* the elections can cause the biases due to the post-treatment control.

[35] See Figure SI.11.1. In Supporting Information 16, I also qualitatively examine whether three to eight months are reasonable estimates.

based rainfall data, I also conduct a falsification test by regressing the incidence of protests one-year *before* polling on election-day rainfall deviation. If anomalous rainfall would be associated with reporting biases, we should see a correlation between election-day rainfall and pre-election protests. The placebo test, however, shows no association.[36]

Robustness Checks III: Miscellaneous

The main finding is also robust to other changes, including the omission of each state,[37] omission of deviant observations,[38] alternative estimation techniques with and without matching,[39] use of protest counts as an outcome variable,[40] different hyperparameters of the near-far matching,[41] and the use of spatial regressions.[42]

Conclusion

In this paper, I argue that high turnout in free and fair elections has an unintended effect on conflict resolution. A simple extension of bargaining theory indicates that high turnout can drown out the voices of potential protestors, make it difficult for the government to precisely estimate the size of the dissenters, and thus create a positive probability of protest. Because turnout is considered to be endogenous to protest, I use election-day rainfall deviation as a source of exogenous variation and

[36] See Table SI.12.1.

[37] See Figure SI.13.1.

[38] See Figure SI.14.1. Also refer to Supporting Information 5.

[39] See Table SI.9.1 to SI.9.4.

[40] See Table SI.9.5.

[41] See the end of Supporting Information 7.

[42] See Table SI.15.1.

apply the near-far matching IV method to make a more explicit and robust causal comparison. The analysis shows that higher turnout indeed increases the likelihoods of protest after the elections.

Although the analyses about the causal mechanisms are rather limited and thus I cannot definitely exclude the alternative possibilities, the theory and empirics imply that high turnout can render elections to noisy signals of popular discontents. The remaining uncertainty can periodically cause protests. As previous studies point out (Little, Tucker, and LaGatta 2015), protest thus constitutes an informational channel through which dissenters non-electorally express their opinions; even when their voices are drowned out, they can still signal their dissents via protests. This means that protests, and, more broadly, the freedom of assembly, supplement the limitations of electoral conflict resolution.

A caveat is that the scope of the analysis is limited; we focus on a majoritarian electoral system, state elections in India, and, most importantly, exogenous variation in turnout. Although I believe this is a critical departure from existing theoretical models, which assume unanimous turnout, it is extremely important to incorporate electoral mobilization, vote-buying, and other dynamics of turnout. Moreover, given the lack of micro-level data and research designs, I cannot strongly exclude alternative explanations, such as habituation and mobilization mechanisms. It is therefore a task of future studies to extend the theories and empirics to endogenous turnout, and also to rigorously analyze the causal mechanisms at a micro level.

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