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Voting Costs and Voter Turnout in Competitive Elections

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ABSTRACT

In the United States, competitive elections are often concentrated in particular places. These places attract disproportionate attention from news media and election campaigns. Yet many voting studies only test stimuli in uncompetitive environments, or only test for average effects, and simply assume the results are relevant to competitive contexts. This article questions that assumption by utilizing Election Day inclement weather as an exogenous and random cost imposed on voters. We test how voters in competitive and uncompetitive environments respond to this random cost and find that while rain decreases turnout on average, it does not do so in competitive elections. If voters in different electoral contexts do not react the same way even to rain, then serious doubt should meet claims that voters will react the same way to campaign appeals, economic factors, or other treatments tested in the literature. Careful consideration of effects that are heterogeneous with respect to electoral context can make the difference between a result that calls democracy into question and one that is politically irrelevant.

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In an uncompetitive region of an uncompetitive state, a shark attack evidently caused some citizens to vote against the incumbent President in 1916 (Achen and Bartels, 2004). If generalizable, then elections can be determined by events far outside a leader's control. Or, is this merely an example of unsought-after votes shifting in an inconsequential manner?[1] In dozens of field experiments in which get-out-the-vote mailers were sent to non-targeted random samples of citizens in mostly uncompetitive contests, turnout increased hardly at all (see Gerber *et al.*, 2008, Table 4). If generalizable, then campaigns have but a tiny influence on elections. Or, do such field experiments not resemble voter mobilization in close contests? A rainstorm can depress turnout (Gomez *et al.*, 2007); the loss of a football game can reduce incumbent support among fans (Healy *et al.*, 2010). If generalizable, elections can be decided by entirely idiosyncratic events. Or, are such effects limited to jurisdictions in which the election outcome is not in doubt?

Answers to these questions hinge on the assumption of homogeneous treatment effects across types of elections. If, as compared to voters in uncompetitive contests, voters in competitive elections are treated differently by politicians, or respond to different stimuli, or have a different voting calculus, then many claims about why people vote the way they do may not be applicable in exactly the circumstances that are of most interest to politicians and political scientists. As is well known, but not universally accounted for in the literature on voting, effects that are heterogeneous with respect to electoral context can make the difference between a result that calls democracy into question and one that is politically irrelevant.

In this paper, we ask whether voters residing in competitive electoral contexts respond differently than other voters when a randomly-assigned cost is imposed on them. We treat Election Day rain showers as an exogenous cost that makes voting more difficult and we examine whether this cost has a similar effect on voter turnout in competitive electoral contexts as in uncompetitive contexts. While we briefly speculate about why voters in competitive environments may react differently to a random imposition of a cost like bad weather, the primary objective of this essay is simply to test the comparability of competitive and uncompetitive contexts. If across competitive and uncompetitive environments, voters do not react the same way even to rain, the problem of external validity in studies that fail to take competitive context into account is graver than previously thought.

Our evidence shows that rain storms depress turnout *on average*, but not in close elections. This contrast may be the result of heightened campaign activity, heightened voter interest, or some other reason entirely, but whatever the reason elections in competitive contexts appear to be very different than elections in uncompetitive contexts. Recent work has suggested that a "competitive environment is not terribly important" in that turnout is not substantially higher in such environments (Holbrook and McClurg 2005, see also Gerber *et al.*, 2009). By estimating the effect of competition on voting conditional on the imposition of an exogenous cost, we show that competition does have an important impact on voter turnout after all. More generally, the result suggests that

[1] The election of 1916 was quite competitive, but not in President Wilson's home state of New Jersey. The Democratic campaign focused on the West, where progressive policies were more popular. The Northeast was beyond the Democrats' reach because of party infighting and Wilson's low popularity with Catholics and immigrants (Lovell, 1980).

any number of influences that might affect a citizen in an uncompetitive race may have no effect on a citizen in a close election.

EXTERNAL VALIDITY AND COMPETITIVE CONTEXT

Heterogeneous effects appear everywhere in studies of political behavior. A get-out-the-vote treatment can affect voters with high latent propensities to vote more than those with low latent propensities (Arceneaux and Nickerson, 2009); elites can influence the stated opinions of politically unaware citizens more than those of politically aware ones (Zaller, 1992); a campaign commercial can affect less informed partisans but not independents (Ansolabehere and Iyengar, 1995); and so forth. These nonlinearities matter because the magnitude of effects may be different across types of individuals in substantively important ways. Namely, if the effect of a stimulus is concentrated on a subset of the population that has no meaningful impact on politics, then the relevance of the overall effect is negligible. For instance, a treatment that dramatically changes the candidate preference of non-voters is considerably less interesting than a treatment that affects the preferences of actual voters.

The level of political competition in a jurisdiction has the potential to produce important heterogeneous outcomes; however, competitive context is a moderator that is as difficult to study as it is important. The difficulty in studying competitive context is partially due to measurement (i.e., it is not obvious how best to estimate the degree of electoral competition in a jurisdiction) and partially due to ethical and practical boundaries in conducting surveys and experiments in competitive environments. Experimentalists are not able to conduct field studies in which the treatment could potentially alter real-world political outcomes, and therefore testing a potent treatment in a ultra-competitive context is unlikely to garner approval from an institutional review board. The practical boundaries include the familiar problems of causal identification in observational studies and data limitations in estimating interaction effects with small representative samples. Yet, if ever there was an interaction effect that makes a substantive difference in the study of electoral politics it is competitive context. A treatment that moves voter turnout two points or moves support for one party two points matters immensely in a competitive environment and matters not at all in an uncompetitive one.

A study that tests a treatment, such as a campaign message or news media story, on voters in an uncompetitive context implicitly assumes that the effect identified is applicable to competitive contexts. The task here is to question that assumption. If uncompetitive environments are the typical laboratories for studying political effects, are the treatments in these laboratories, whether artificial or observed in the real world, similar to treatments in competitive environments? Are the recipients of treatments, whether experimental subjects or observed citizens, similar to recipients of treatments in competitive environments? Is the context in which the treatment is administered, whether manufactured or real, similar to the context of a competitive environment?[2]

[2] These questions parallel those asked by Davenport *et al.* (2010) when assessing whether field experiments are externally valid.

Particularly in Presidential contests, we have reason to question the assumptions of external validity related to competitive context. Because of the institution of the Electoral College, there are stark differences across state boundaries in Presidential elections between competitive and uncompetitive environments such that the application of findings from uncompetitive states to competitive states may be suspect. In Presidential contests, the 50 U.S. states are divided by political campaigns, and responsively by the news media, into safe states that do not merit attention and swing states that are winnable by either campaign. In their strategic allocation of time and resources, campaigns must ignore many places and dedicate their money and staff support to just a few places. States that the campaigns consider safe receive negligible amounts of TV ads, campaign phone calls, mailers, and visits by the candidates (Shaw, 2006; Hillygus and Monson, 2009). As one stark example of this, none of the Presidential and Vice-Presidential candidates set foot in about half of the states during the 2000 and 2004 general election campaigns. But in these two campaigns, the major candidates visited the battleground state of Florida a combined 131 times (Shaw, 2006).

Some evidence also points to voters *themselves* being quite different in competitive environments than uncompetitive ones. Voters in closely contested elections accumulate more information about the candidates and the race (Gimpel *et al.*, 2007; Hill and McKee, 2005). Media focuses on the campaigns as well, such that voters have the opportunity to develop an interest in the contest. Campaigns raise interest by mobilizing voters, reminding them to turn out and persuading them to vote for their respective candidates (Bergan *et al.*, 2005).

Whether voters in competitive environments act in similar ways to voters in uncompetitive ones is, however, a wide open question. Apart from the "minimal effects" presumption of yore that campaigns do not have meaningful influences (for a review, see Brady *et al.*, 2006), more recent work by Holbrook and McClurg (2005) and Gerber *et al.* (2009) suggests that competition and state-level disparities in campaign attention have very limited impact on voter turnout. If true, then researchers engaged in studies of uncompetitive environments may proceed to study causes of voter turnout and apply their findings to competitive contexts. If, however, voters respond differently to stimuli in competitive environments, then studies of uncompetitive environments are not useful for learning about phenomena pertinent to conditions when democratic outcomes are at stake.

RAIN AS A RANDOMLY ASSIGNED VOTING COST

To test how voters in different electoral contexts react to a stimulus, we utilize Election Day rain and snow storms as an exogenous cost that may be imposed on voters in swing states and safe states in identical ways. Inclement weather is a minor nuisance that is expected to provide enough added cost to participation so as to keep some voters at home. This is consistent with general predictions that when costs rise, such as when onerous registration requirements are imposed, participation rates decline (Wolfinger and Rosenstone, 1980; Aldrich, 1993). Voters are sensitive to changes in costs since voting is neither a very costly activity nor a very beneficial one — each person's decision to vote or abstain is, in his or her own mind, of minuscule consequence (Aldrich, 1993). Because the stakes are so low, small changes to the cost–benefit calculation can have significant effects on turnout rates.

After controlling for typical precipitation patterns in a jurisdiction, whether or not it actually rains on Election Day is random. While we cannot randomly assign voting jurisdictions to a competitive or uncompetitive electoral environment, we can treat bad weather as a randomly assigned cost to participation and observe how turnout is affected in competitive places versus uncompetitive places. If citizens in competitive environments are more resilient to voting costs than citizens in uncompetitive environments, we should see differential responses to rain showers across electoral contexts.

Gomez *et al.* (2007) estimate that, on average, an inch of rainfall can reduce turnout in a county by almost one percentage point. Gomez and his colleagues used new tools to estimate weather's effect, including spatial interpolation to measure county-level precipitation rates based on reports from over 20,000 weather stations. The result is a much more plausible measure of the effect of weather than previous research provided.[3] Utilizing Gomez *et al.*'s (2007) refined measures, we test how voter turnout varies depending on precipitation and the competitive environment. The natural random assignment of rain on Election Day provides the kind of traction necessary to estimate the effects of the competitive environment on turnout with observational data.

The actual mechanism that may cause voters in competitive environments to be more resilient to bad weather is beyond the scope of this research. However, we can speculate. First, as a result of the attention lavished upon voters in competitive environments, voter engagement is piqued and voters may be more enthusiastic and determined to vote than in uncompetitive environments in the face of obstacles like inclement weather. In addition, during the weeks and months preceding an election in a competitive jurisdiction, campaigns build a mobilization infrastructure. They help citizens register to vote, inform registrants about the date of election and location of the polling station, offer rides to the polls, and study voter registration records to identify the infrequent voters who will need the most attention.

As with other voting costs, once a campaign has a staff, an army of volunteers, and a get-out-the-vote (GOTV) plan in place in a state, it can respond to the hassle of Election Day storms. Campaign workers triage just these sorts of voting impediments. In fact, some campaigns go so far as to plan for bad weather well in advance of Election Day. To get a sense of campaign strategy in this domain, we spoke with a paid staffer from Hillary Clinton's 2008 Presidential primary campaign, who was involved in the GOTV operation in the crucial state of Iowa. Weeks in advance of the Iowa caucuses, the Clinton

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3 Prior to this 2007 study, political scientists found inconclusive evidence of a negative impact of weather on turnout. Merging county-level weather reports with individual-level data from the National Election Studies, Knack (1994) finds no effect on turnout and no partisan impact from rain. Matsusaka and Palda (1999) investigate the impact of extreme temperatures in Canadian elections and also find no effect. Merrifield (1993) and Schachar and Nalebuff (1999), on the other hand, do find that rain is negatively related to turnout in U.S. elections, but their results must be discounted due to their very blunt and unreliable measure of precipitation: rainfall in each state's largest city.

campaign bought shovels for every Iowa precinct — a pricey investment. Campaign volunteers were instructed to make sure the sidewalks were clean of snow and to offer to shovel driveways for supporters. The campaign also provided rides to citizens who did not want to drive in bad weather and babysitters for those who had children at home.

When asked why the campaign made this extraordinary effort before even knowing whether it would snow on the night of the caucuses, the staffer replied, "Because it's January. And it's Iowa." In a closely fought election where poor weather can be a serious factor, the campaigns build into their budget and strategy a plan to counteract weather. The high-stakes and frigid Iowa caucus may be an extreme case, but even in competitive states where the weather is less predictable than Iowa in January, campaigns have field offices and scores of volunteers who can intensify their GOTV drive to counteract the effects of a sudden rain or snow storm. As for the Clinton campaign's response to bad weather in other states, the staffer recalled that there was terrible rain and snow during the Virginia primary, but there the campaign did not make any special effort to compensate for the weather. This decision is not surprising, given that Senator Obama was leading Senator Clinton by about 20 points in the Virginia polls.

Little of the mobilization, engagement and response to Election Day costs that come along with swing state campaigns applies to the safe states. Campaigns are unlikely to expend any kind of effort if rain or snow falls in a safe state. They will not respond to minor voter registration or administrative nuisances either. Indeed, campaigns have no infrastructure in these states to make such an effort. In the absence of heightened mobilization and voter engagement, turnout will be depressed by small costs like poor weather. Because a change in turnout of even a few percentage points will not be nearly enough to swing the election in safe states, neither voters nor campaigns will be animated to compensate for the loss of votes.

The random assignment of Election Day rainstorms helps to gauge whether close elections and unclose elections are particularly comparable. If there is something about competitive elections — whether campaign activity or voter engagement, a combination of the two, or something else entirely — that makes voters capable of responding to a random cost in one environment but not the other, then this calls into question an array of estimated effects thought to be applicable to elections of varying degrees of competitiveness. If voters in different electoral contexts fail to react the same way to the rain, then serious doubt should meet claims that voters will react the same way to campaign appeals, economic factors, or other treatments tested in the literature. This point is especially important so that political scientists can separate the silly correlates of turnout and vote choice from the more serious and plausible causes.

ESTIMATION

Our estimation approach builds off of the methodology and data used by Gomez *et al.* (2007) (see also Hansford and Gomez (2010)), adding measures of electoral closeness in order to focus on how the randomly assigned cost (rain) has a different impact depending on the electoral environment. The dependent variable is the number of votes for President cast in a county divided by the voting age population of that county. The data include county-level returns in Presidential elections 1948–2000, leaving out Hawaii and Alaska because these states were not part of the union prior to 1959 and leaving out Oregon in 2000, since it switched to a widely utilized vote-by-mail system.

Gomez, Hansford, and Krause provide two measures of county rainfall and snowfall: the number of inches of rain or snow in each county on Election Day, and the number of inches of rain or snow in deviation from normal conditions in that county on the particular date of the election. Their analysis prefers the latter measure, while we use the former, for two reasons. First, in nearly every county deviation from normal rain is negative if actual rainfall is 0, making substantive interpretation of rainfall more complicated. Second, although Gomez, Hansford, and Krause assert that citizens in jurisdictions with more rain may be less deterred from voting due to the weather, we do not want to set up our model with this assertion established beforehand. Instead, we use the average amount of rain received by a jurisdiction as a control variable in our model.[4]

The bivariate relationship between rain and turnout indicates that rain serves as a cost, depressing turnout a small amount on average. Figure 1 fits a regression line with a simple linear model using turnout as the dependent variable and rainfall as the sole independent variable. The downward slope demonstrates that, at the county level, an increase in rainfall leads to a decrease in participation, even without accounting for the myriad of other factors that contribute to the voting calculus.[5]

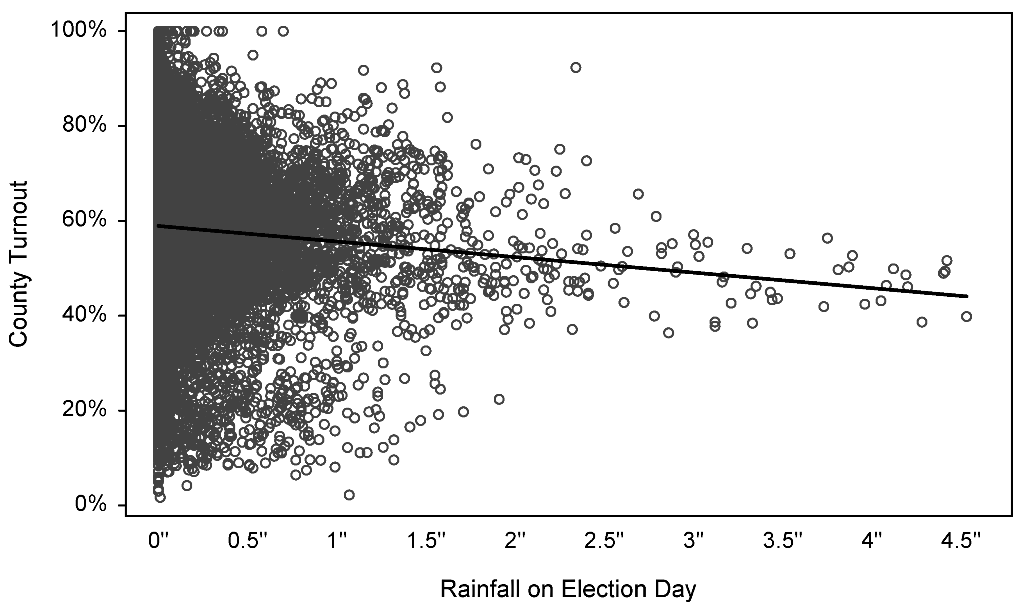

Figure 1. Bivariate relationship between rain and turnout.

4 Models run using the Rain Deviation measure have the same substantive result.

5 One may assume that rain had had a greater impact on voters in earlier time periods due to less effective means of transportation, poorer drainage systems, or other reasons. However, if we exclude all results prior to the 1964 election (about a third of our data), we see similar effects to those reported throughout this paper.

While Figure 1 would seem to suggest inclement weather deters voters, much variation is left unaccounted for. In our full regression models, we include a set of control variables with the goal of eliminating election-specific variation in turnout not attributable to the weather. These include county-level measures of average income, percent high school graduates, percent black, number of farms per capita,[6] and indicators for whether the county in a particular year required a poll tax, a literacy test, or a property requirement. Following Gomez, Hansford and Krause, we include a measure of how far ahead of the election citizens are required to register, whether a state has a motor-voter law in each year, indicators for the presence of a gubernatorial or senatorial race on the ballot, and the turnout level in the previous Presidential contest.[7]

Measuring Electoral Closeness

There are two common ways to measure electoral closeness: *ex post* estimates and *ex ante* estimates. An *ex post* measure relies on post-election data such as the actual margin of victory in each state. About 70% of political science articles that study closeness utilize this kind of measure (Geys, 2006; see, for example, Caldeira and Patterson, 1982; Cox and Munger, 1989; Settle and Abrams, 1976). An *ex ante* measure relies on pre-election opinion polls, previous election outcomes in the state, the normal vote, or subjective methods such as reports from campaign strategists about the states each campaign actively contested (see, for example, Hill and McKee, 2005; Lipsitz, 2008; Shaw, 1999, 2006; Wolak, 2006).[8]

For the present study, there are advantages to using each kind of measure, and so we show results from models using both, demonstrating that the substantive conclusion is the same in either case. For an *ex ante* measure, we use state-by-state predictions from Campbell *et al.*'s (2005) Presidential forecasting model, which is an updated version of Campbell's (1992) earlier forecasting work. The model provides a prediction of the two-party Democratic vote share in each state for each election. We transform the predicted Democratic vote share into a variable that ranges from 0 (least

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6 It can be argued that this variable does not belong in the model. Because farms are likely to be located in favorable climates and because climate is causally prior to farm locations, this variable corrupts the model by introducing post-treatment bias. Because running our statistical tests with and without this control does not change the substantive result, we have opted to leave the farms per capita variable in our model to be consistent with previous scholarship.

7 Without the aforementioned county-level control variables, our lagged turnout measure accounts for a great proportion of the variation we witness in turnout. After including other controls, the magnitude of variation explained by lagged turnout is somewhat reduced, but still significant, and therefore is included in the full model. A full model run without this variable produces the same substantive result.

8 Because we measure competitiveness at the state level on account of Electoral College incentives, it is worth noting that Maine (since 1972) and Nebraska (since 1996) both give a portion of their electoral votes to the popular vote winner in each congressional district. However, neither state split their electoral votes over the time period we examine, and so we treat these states just like all the others.

competitive) to 1 (most competitive) by performing the following operation: $Competitiveness = 1 - |(predicted\ Dem\ share - 0.5) * 2|$, with a measure of 0 equivalent to 100% of the vote going to the winning candidate and 0% to the losing candidate. A measure of 0.96 is equivalent to a 4% margin of victory, or the two-party vote share splitting 52% to 48%.[9] For an *ex post* measure, we perform the same operation on the actual Democratic share of the two-party vote in each state for each election.[10,11] We perform the same transformation as with the *ex ante* competitiveness measure. The correlation between the two measures is 0.81.

While the *ex ante* measure has the advantage of being estimated ahead of each election, predicted competitiveness has disadvantages. First, forecasting models have a particularly difficult time predicting how states will vote in turbulent periods when entire regions of the country experience party realignments (Campbell *et al.*, 2005). There are enough far-off predictions in the Campbell *et al.* (2005) forecasts (as well as in other forecasting models) to affect the results in some of our statistical analyses. Second, the *ex ante* measure does not take into account any campaign activity by design (see Gelman and King, 1993), meaning that any attempt by campaigns to make races more contested than economic or demographic conditions would suggest is not incorporated into the model. While we show estimates from models using both competitiveness measures, it is our sense that the actual election result in the state provides a better proxy for how competitive the election actually was than the pre-election predictions. Thus we will focus our attention on the models that utilize this *ex post* measure.

As a final note, academics and pundits focus on *how* competitive a state is, but it is plausible to think voters and campaigns are more concerned with whether or not their state is considered "battleground," aside from the magnitude of competitiveness. Shaw (2006) constructs an indicator for whether at least one Presidential campaign considered a state to be a battleground state as a measure of competitiveness. Linking Shaw's measure of battleground status to *ex post* competitiveness, the average margin of victory in a battleground state is 5.72%, with a lower quartile of 8%. A non-battleground state averages a 15.59% margin of victory, with an interquartile range of 21–7.89%. Given that many factors produce closer margins of victory than expected by campaigns (but not necessarily by voters), we can be reasonably confident that our measures of competitiveness are compatible with subjective understandings of how contested a state

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9 Three state elections cause problems for this *ex ante* variable. For Alabama in 1948 and Alabama in 1964, no national Democrat was on the Presidential ballot. In the 1960 Mississippi presidential election, a plurality of voters selected "Unpledged," rather than selecting a candidate. Because the forecasting model requires the state election result from the previous three elections in order to calculate a prediction, six other state-year combinations are missing as well. For these six elections (Alabama in 1952, 1956, 1968, and 1972; Mississippi in 1964 and 1968), we impute a value based on the actual Democratic vote share garnered in those elections. For all other state-years, we use the transformation of the prediction.

10 Because the effect of rain and snow on vote share is on the magnitude of no more than a couple percentage points in the most extreme weather scenario, there is no great concern that weather conditions make states substantially more or less competitive as measured by an *ex post* variable.

11 State-by-state vote share data was accessed through David Leip, "David Leip's Atlas of U.S. Presidential Elections," http://www.uselectionatlas.org, 2009.

is in a given election. As the Shaw variable only exists for the 1988–2000 subset of the data, we opt not to use this coarser measure in our models.

Figures 2 and 3 split the county-level dataset into uncompetitive and competitive states, here defined as those states where the margin of victory is 30% or greater for

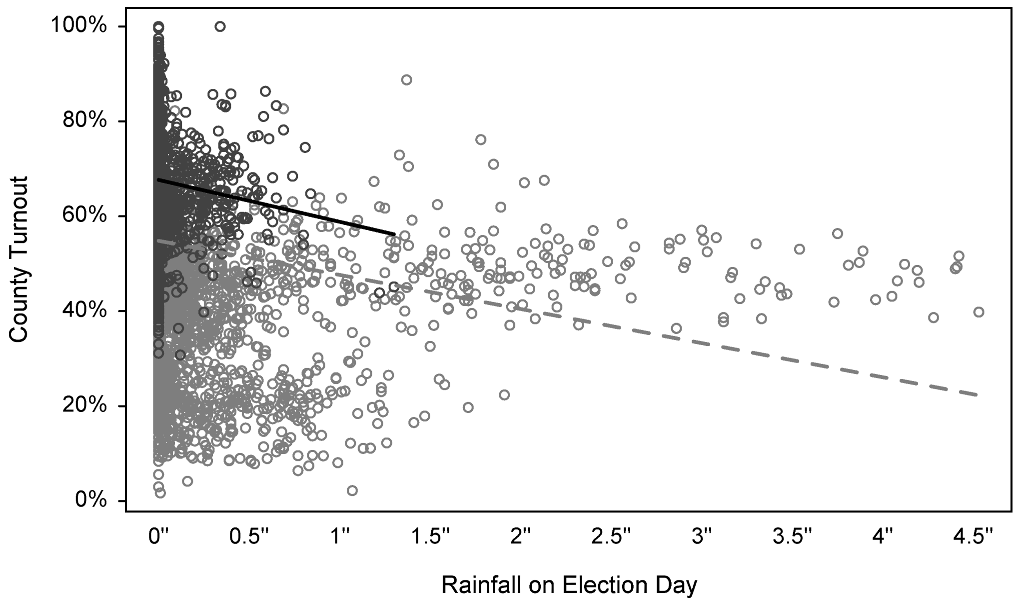

Figure 2. Bivariate relationship between rain and turnout, uncompetitive states.[12]

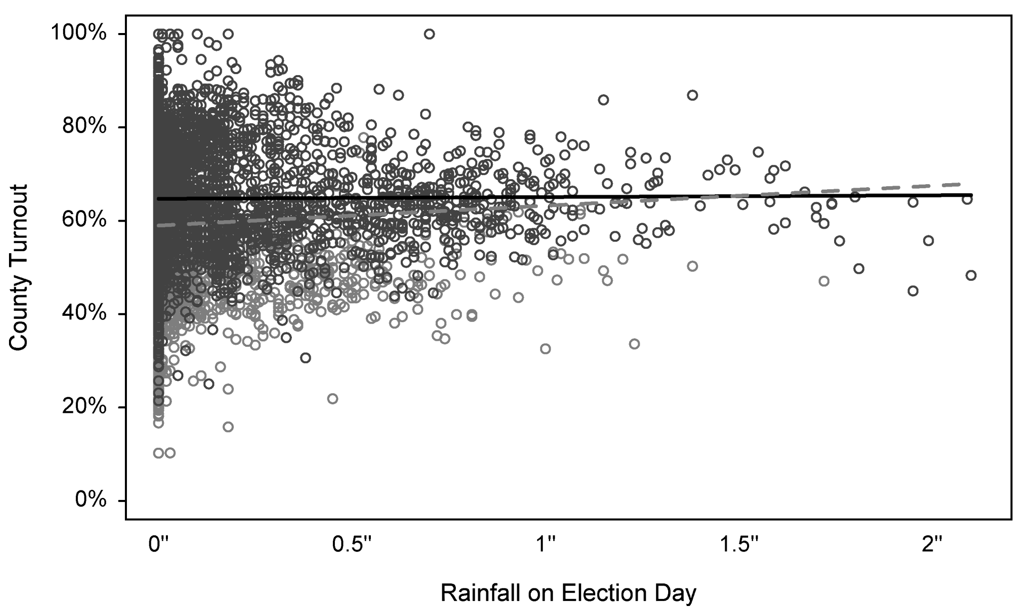

Figure 3. Bivariate relationship between rain and turnout, competitive states.[12]

[12] Southern counties are shown in light gray; non-Southern counties are shown in dark gray.

Figure 2 and 4% or less in Figure 3. As southern states are often noted to have distinct participatory patterns, particularly before the fall of Jim Crow, these plots show a separate regression with the South included as a dashed gray line, and Southern counties shown in lighter gray.[13] Here we begin to see evidence for a differential impact of costs in competitive and uncompetitive environments, with voters in uncompetitive states more susceptible to rain-induced low turnout than those in competitive states. Again, however, a full model is necessary to interpret whether this effect is simply an artifact of unaccounted-for variation.

Results from the Multivariate Turnout Model

The data consist of 14 time units, with each time period including approximately 3,115 county cross-sectional units.[14] We use a least-squares mixed-effects approach to generate estimates from this dataset, which includes random effects for each county and fixed effects for each election year. These effects are included in an attempt to mitigate systematic between-county and between-election variation.[15] We add our measures of competitiveness both as a main effect and in interaction terms with the rain and snow variables. This enables us to gauge the impact of the random cost on turnout in close versus landslide contests. As a final step towards limiting unaccounted for variation, we apply Coarsened Exact Matching (CEM, see Iacus *et al.*, 2009) to ensure our treatment group (rained) and control group (did not rain) are balanced on the average amount of rain in a county.[16] After matching, our treatment group consists of 16,381 observations, while our control group has 26,737 observations. The mean level of turnout in the treatment group is 58.5%, slightly lower (as expected) than the control group, at 58.7%. The mean value of the average rain variable is 0.091 in the treatment group, and 0.089 in the control group, again conforming to our expectations but demonstrating that the groups are sufficiently comparable.[17]

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[13] It is common to exclude data from southern states when analyzing predictors of turnout (see Hansford and Gomez, 2010). However, we include southern states in our analyses (where data is comparable to the non-south) as we include a set of controls that attempts to account for major institutional impediments to turnout. Running regression models without southern states included produces similar results to the full dataset.

[14] Counties in Alabama in 1948 and 1964 and Mississippi in 1960 are not included, as the national democratic party candidate was not on the ballot in these states.

[15] Models using fixed effects for both counties and election years can alternatively be employed. A Hausman test reveals that the fixed effects and random effects models are significantly different, but not in regard to our coefficients of interest, thus not changing the substantive results we present here. For consistency with past work (i.e., Gomez *et al.*, 2007), we show mixed effects results in Table 1. However, the appendix table shows all models with fixed effects specifications.

[16] While Election Day rainfall is a cost exogenous to the political system, the distribution of rainfall across the nation follows predictable patterns based on climate. We control for average rain in the statistical model, but as an added precaution we match observations on average rainfall as well. The matching process produces six treated observations without comparable untreated observations, all from three counties in the Olympic Peninsula region of Washington. These observations are trimmed from the final dataset. As one of the few regions of the nation with a temperate rain forest, it is not especially surprising that these observations do not have viable pairings.

In Table 1, we estimate the effect of inclement weather on county-level turnout in the two electoral environments.[18] We show five variations of the result, all of which utilize the rain and snow variables that are described above. Model 1 is a basic model, examining the effect of rainfall on average without including measures of electoral closeness. In Models 2 and 3, we use the *ex post* competitiveness variable.[19] Finally, in Models 4 and 5, we use the *ex ante* competitiveness variable. We show results for our models including competitiveness both with and without other county-level control variables.

In Model 1, which includes just our reduced set of controls and excludes measures of competitiveness entirely, we see that both rain and snow have a significant, negative effect on turnout. This effect is similar to that found in Gomez *et al.* (2007), validating our basic strategy and demonstrating that, on average, rain deters voters a slight amount.

However, looking across Models 2–5 in Table 1, each estimate tells the same story regarding the heterogeneity of rain's effect on turnout depending on the level of competitiveness. The positive coefficient on the competitiveness variable indicates that turnout is higher in counties situated in competitive states, a finding consistent with the literature on electoral closeness. The negative coefficients on rain and snow indicate that in an uncompetitive environment, inclement weather decreases turnout. Most important, the large positive coefficients on the interaction terms suggest that the demobilizing effect of rain is mitigated by a competitive electoral environment.[20]

To get a sense of how strongly our theory is supported by the data, we have constructed Figure 4, which simulates how county-level turnout changes with increasing rain in two different electoral environments: a competitive environment where the spread between the parties is 4 percentage points and an uncompetitive environment where the spread is 30 points. All variables apart from those related to competitiveness and rainfall are held at their means. With average precipitation in a county, turnout is only slightly higher in the competitive environment than in the uncompetitive environment. However, under rainy conditions, turnout drops in the safe state, but actually slightly increases in the competitive state.

While we would not want to make too much of the slight increase in turnout under rainy conditions in competitive states, this pattern does suggest that campaigns might be overcompensating in close elections to ensure that voters are not deterred by inclement weather. Campaigns might do so by increasing the number of GOTV phone calls they

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17 Matching, in this instance, does not change the substantive result.

18 We use the the `ls.mixed` command in the `Zelig` package of `R` to generate these estimates (see Bailey and Alimadhi (2007) and Imai *et al.* (2008)).

19 The inclusion of a quadratic term to account for a non-linear impact of competitiveness was not statistically significant, and was therefore excluded from our final results.

20 Although rain has consistent effects regardless of controls used, snow is far more sensitive to the type of competitiveness measure employed and generally produces higher standard errors. In 40% of observations where snow was measured, it also rained *in the same county during the same election year*, potentially leading to the less predictable effects we observe. To account for this, we generated a merged "precipitation" variable using a standard conversion factor for snow to rain (see Baxter *et al.*, 2005), and found the same results as the rain variable.

Table 1. Impact of competitiveness and inclement weather on turnout.

Dependent variable: county turnout:	Model 1 No competitive measure	Model 2 *ex post* competitive (reduced)	Model 3 *ex post* competitive (full)	Model 4 *ex ante* competitive (reduced)	Model 5 *ex ante* competitive (full)
Competitiveness		3.296	2.840	0.810	−0.308
		(0.239)	(0.248)	(0.286)	(0.299)
Rain × competitiveness		6.318	5.913	9.018	8.124
		(0.577)	(0.562)	(0.799)	(0.776)
Snow × competitiveness		0.144	1.574	−2.255	−0.200
		(0.863)	(0.839)	(1.281)	(1.243)
Rain	−0.672	−5.262	−5.226	−7.939	−7.489
	(0.112)	(0.441)	(0.430)	(0.655)	(0.636)
Snow	−0.260	−0.322	−1.708	1.772	−0.234
	(0.095)	(0.734)	(0.713)	(1.157)	(1.123)
% High school grads			0.529		0.477
			(0.046)		(0.046)
Income			0.230		0.285
			(0.093)		(0.093)
% African American			−0.024		−0.024
			(0.003)		(0.003)
Farms per capita			24.533		23.266
			(0.932)		(0.923)
Registration closing date			−0.029		−0.032
			(0.001)		(0.001)
Motor voter			−0.145		−0.052
			(0.110)		(0.110)
Property requirement			−3.162		−3.224
			(0.318)		(0.322)
Literacy test			−0.106		−0.086
			(0.105)		(0.105)
Poll tax			−5.865		−5.952
			(0.139)		(0.139)
Gubernatorial election			−0.123		−0.124
			(0.065)		(0.065)
Senatorial election			0.076		0.055
			(0.051)		(0.051)
Turnout_{t-1}	0.865	0.859	0.746	0.862	0.751
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)

(*Continued*)

Table 1. (*Continued*)

Dependent variable: county turnout:	Model 1 No competitive measure	Model 2 *ex post* competitive (reduced)	Model 3 *ex post* competitive (full)	Model 4 *ex ante* competitive (reduced)	Model 5 *ex ante* competitive (full)
Average rain	−3.725	−4.618	2.228	−4.090	2.592
	(0.495)	(0.496)	(0.586)	(0.501)	(0.584)
Average snow	6.446	6.524	6.821	6.417	6.648
	(0.379)	(0.378)	(0.429)	(0.379)	(0.427)
Constant	6.963	4.613	10.494	6.536	13.080
	(0.147)	(0.230)	(0.325)	(0.256)	(0.351)
Observations	43,118	43,118	43,118	43,118	43,118
Log-Likelihood	−131,732	−131,521	−130,331	−131,649	−130,438
Likelihood Ratio Test	88,259	88,681	91,061	88,426	90,847

Note: Standard errors are in parentheses. Year fixed effects are estimated but not presented.

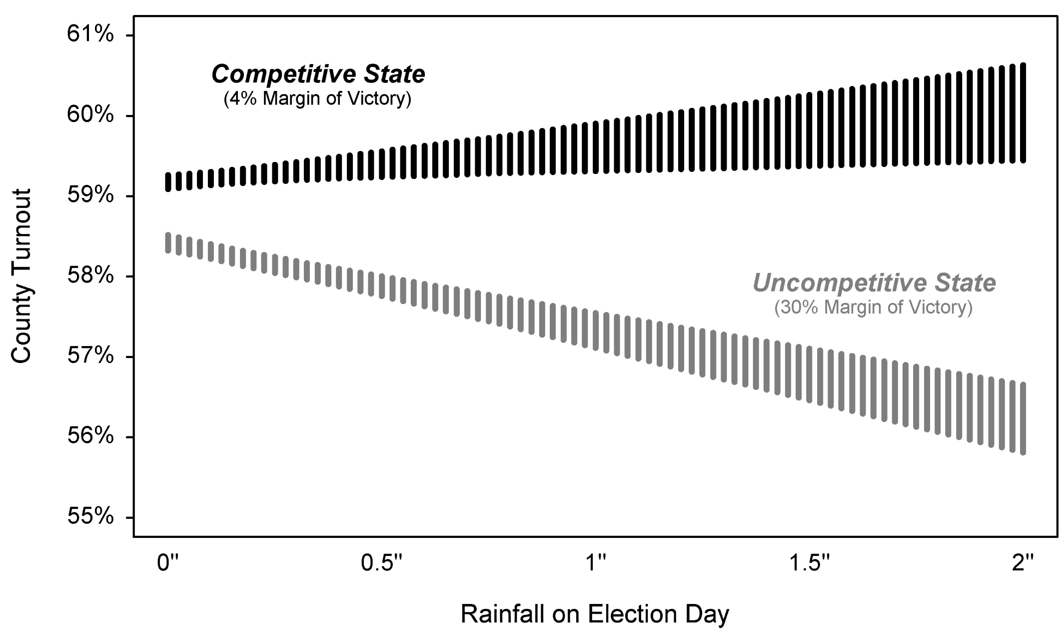

Figure 4. Simulated outcomes, competitive and uncompetitive environments.

make or by offering more rides to the polls, as was suggested by the Clinton staffer with whom we spoke.

DISCUSSION

We have shown that estimating an *average* effect of precipitation on voter turnout without taking into account the electoral environment leads to faulty inferences about the importance of Election Day weather. Heavy rain and snow present a clear, tangible cost to voters on Election Day, but this cost will not be felt in the same way across electoral environments. If the election in a particular state matters (i.e., the election is close), inclement weather has no substantive impact. More to the point, we have shown that a random cost imposed on voters in different electoral environments does not have a uniform effect on turnout rates. Voters in competitive environments appear to be more resilient than voters in uncompetitive environments.

Bad weather is among the most basic and easy-to-understand costs that influence a citizen's decision to vote on Election Day. But even weather does not have the same influence over all voters. If the effect of weather is heterogeneous across electoral environments, it seems fair to ask what other treatments might have differential effects in competitive and uncompetitive places. Our finding, though simple and straightforward, emphasizes the difficult challenge that scholars face in making inferences about the way elections are decided from studies that do not account for heterogeneous contextual effects.

Campaigns have limited resources, and they focus these resources in places where they can make a difference. Given the incentive structure of the Electoral College, campaigns will ignore many states and inundate a few states with campaign activities. Campaign activities produce more informed voters who are better equipped to make informed vote decisions, and GOTV operations in swing states help voters overcome all sorts of obstacles on Election Day. Campaigns respond to complaints at individual precincts, call supporters multiple times throughout the day, and have a plan in place in the event of inclement weather. Elections are simply not given this kind of attention in uncompetitive states.

As a larger research agenda, our efforts here point to a more intensive study of heterogeneous effects in elections research. Just as precipitation has a different effect on turnout depending on the electoral context, other features of elections, such as campaign mobilization and voter reaction to economic hardship, may have similarly heterogeneous effects. We conceive this modest study as a test of the sensitivity of election research to electoral context. The result of this test is a compelling affirmation of the view that competitive elections and uncompetitive elections simply must be treated as substantively distinct phenomena. In competitive places, the recipients of campaign appeals and other political stimuli are different, and the elite actors providing those stimuli behave differently; therefore we ought to expect that effects in such places are different as well.

Appendix

Table A. Alternative to Table 1, with county and year fixed effects instead of county random effects and year fixed effects.

Dependent variable: county turnout:	Model 1 No competitive measure	Model 2 *ex post* competitive (reduced)	Model 3 *ex post* competitive (full)	Model 4 *ex ante* competitive (reduced)	Model 5 *ex ante* competitive (full)
Competitiveness		6.356	4.046	4.510	1.444
		(0.263)	(0.260)	(0.310)	(0.310)
Rain × competitiveness		5.613	5.968	8.886	8.069
		(0.589)	(0.562)	(0.812)	(0.774)
Snow × competitiveness		−0.244	0.183	−4.298	−2.251
		(0.878)	(0.835)	(1.307)	(1.240)
Rain	−1.013	−4.966	−5.460	−8.138	−7.679
	(0.115)	(0.451)	(0.430)	(0.666)	(0.635)
Snow	−0.641	−0.377	−0.724	3.184	1.399
	(0.098)	(0.746)	(0.710)	(1.180)	(1.120)
% High school grads			0.139		0.194
			(0.080)		(0.080)
Income			0.426		0.422
			(0.137)		(0.137)
% African American			−0.229		−0.224
			(0.010)		(0.010)
Farms per capita			18.473		18.402
			(1.949)		(1.958)
Registration closing date			−0.028		−0.033
			(0.002)		(0.002)
Motor voter			−0.428		−0.423
			(0.114)		(0.114)
Property requirement			−4.236		−4.074
			(0.330)		(0.334)
Literacy test			−1.342		−1.336
			(0.121)		(0.122)
Poll tax			−7.749		−7.936
			(0.147)		(0.147)
Gubernatorial election			−1.122		−1.188
			(0.091)		(0.091)
Senatorial election			0.133		0.137
			(0.050)		(0.050)

(*Continued*)

Table A. (*Continued*)

Dependent variable: county turnout:	Model 1 No competitive measure	Model 2 *ex post* competitive (reduced)	Model 3 *ex post* competitive (full)	Model 4 *ex ante* competitive (reduced)	Model 5 *ex ante* competitive (full)
Turnout_{t-1}	0.734	0.722	0.595	0.723	0.595
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Average rain	7.975	8.069	7.995	7.734	7.806
	(0.878)	(0.870)	(0.827)	(0.874)	(0.830)
Average snow	2.882	3.079	3.484	3.097	3.419
	(0.658)	(0.651)	(0.618)	(0.655)	(0.621)
Constant	12.415	7.711	20.453	9.281	22.833
	(0.207)	(0.281)	(0.421)	(0.304)	(0.440)
Observations	43,118	43,118	43,118	43,118	43,118
R^2	0.866	0.865	0.839	0.865	0.840

Note: Standard errors are in parentheses. Year and county fixed effects are estimated but not presented. This table demonstrates that the key result — the negative effect of rain on turnout is concentrated in uncompetitive electoral contexts — is robust to a fixed effects as well as a random effects specification.

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