Putting the Party Back into Politics: An Experiment Testing Whether Election Day Festivals Increase Voter Turnout

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A century and a half ago, casting a vote in the United States was an engaging social experience, as voters at the polls talked with friends, threw down shots of free whiskey, listened to lively entertainment, and generally had a good time (McGerr 1986). According to Altschuler and Blumin (2000, 75)

> [M]en went to the elections to talk, to conduct some business or another, or simply to take a break, knowing that others would be doing so as well. . . . [E]lections were not the silent and single-purpose events that they would become in the era of secret ballots and voting machines, and voters did not attend them just to vote and depart for work or home. They came, in Pomeroy's words, 'to meet each other and to vote,' and in many cases to remain for hours in the first of these pursuits, availing themselves or not of the parties' inducements to cast a particular ballot.

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Bensel's (2004, chap. 2) detailed description of how nineteenth-century Americans voted is replete with anecdotes about raucous polling place activity that surrounded the "inducements" to which Altschuler refers. But Bensel makes the further point that polling places were deliberately located in social gathering spots, very often saloons, where a festive atmosphere was both natural and expected.[1]

Americans have lost touch with the festive milieu surrounding nineteenth-century elections. By comparison to the elections of the nineteenth century, contemporary polling places are, in Bensel's (2004, 297) words, characterized by a "funereal placidity." Our polling places have been drained of their celebratory elements, and the 85% rates of voter turnout that once accompanied them have disappeared from our collective consciousness.

Inspired by these historical narratives, we set out to explore the feasibility of creating a more celebratory and community-focused atmosphere at the polls. Can the festive, social environment surrounding old-fashioned elections be recreated in ways that increase voter turnout? What would it mean to change the polling environment? Might it change the way that people regard participation in the electoral process?

This essay describes a series of pilot studies designed to shed light on these questions and to pave the way for a larger-scale investigation of the topic. During 2005 and 2006, we conducted randomized experiments in 14 geographic areas. These locations, summarized in Table 1, encompass a broad array of socioeconomic and ethnic profiles. Some settings were in affluent suburbs while others were in inner-cities. The communities ranged from predominantly White to predominantly minority. The elections, too, varied from municipal races in which not a single candidate ran opposed to contested midterm federal elections. Although these sites are by no means a random cross-section of polling locations, the diversity of settings provides some reassurance that the experimental results we obtain have reasonable external validity. After describing the theory behind Election Day festivals, our experimental design, and the character of the festivals we studied, we return to the question of external validity and what our results suggest about the causes of turnout decline after the nineteenth century.

Social Incentives and Voter Turnout

Over the past three decades, policies aimed at increasing voter turnout have largely focused on reducing the costs of voting. Federal "Motor Voter" legislation made voter registration easier and encouraged some states to adopt policies that allow voters to register to vote on Election Day. Early voting periods enabled citizens to cast ballots in person over several days. Several states adopted policies making it easier for voters to obtain mail-in ballots, enabling them to vote from home. In each case, policy has shifted in the direction of demanding less time and effort from voters.

These policies appear to have had some positive effects on voter turnout, but their overall impact has been rather disappointing (Berinsky 2005). The Motor Voter law seems to have had minimal effects on voter turnout (Highton 2004). Studies assessing the effects of various policy reforms on voter turnout have concluded that Election Day registration policies seem to raise turnout, but the effects are on the order of 3 percentage points during presidential election

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years (Fitzgerald 2005, 856; Knack 2001). Early voting periods and "no-fault" absentee ballots seem to have little effect, despite the convenience that they offer to voters (Fitzgerald 2005; Karp and Banducci 2000). These findings suggest that while the costs of voting are not irrelevant, the kinds of costs that voters nowadays confront are not large enough to provide much policy leverage.

What about the benefits of voting? Although political scientists have written extensively on feelings of satisfaction that result from fulfilling a civic obligation (Riker and Ordeshook 1968; Verba and Nie 1972), social pressures to adhere to norms of civic participation (Huckfeldt and Sprague 1992; Lake and Huckfeldt 1998), and beliefs about the importance of the upcoming election (Campbell et al. 1960), they have paid little attention to the positive attractions of going to the polls. This omission doubtless reflects the fact that, in the contemporary United States at least, the selective benefits of going to the polls are few. Ben and Jerry's Ice Cream has been known to provide free cones to those who *register* to vote, but in federal elections, material inducements to cast ballots are prohibited.[2]

Why might one suppose that festivals would increase turnout? One reason, as mentioned earlier, is the historical correlation between festival activity and voter turnout. Another is the powerful role of personal influence evident in recent experimental research. Dozens of field experiments conducted since 1998 suggest that impersonal modes of communication—direct mail, automatic phone calls, routinized calls made by telemarketing firms, electronic mail—have a negligible effect on turnout (Green and Gerber 2004). Much more effective are personal appeals, such as those delivered face-to-face by political canvassers. If festivals can draw people to the polling location, face-to-face encouragement can be expected to impel party-goers to cast ballots.

An obvious difference between contemporary festivals and the social context of nineteenth-century elections is that the regulation of elections is nowadays more stringent, and most state and federal laws prohibit not only vote-buying but any kind of quid pro quo inducement to vote. However, we were informed by various local and state officials that if properly advertised (i.e., described in a way that draws no connection between voting and gaining a benefit) and provided without regard for whether the recipient voted or was likely to vote, food and entertainment would not violate these statutes. Thus, in conformance to contemporary norms and laws, our experimental festivals offered food, fun, and music but lacked the free-wheeling attributes of their nineteenth-century counterparts. And unlike the social activities surrounding elections in the nineteenth century, which of course were men-only affairs, our parties were meant for general audiences, including children. Therefore, the empirical question we address is whether these family-friendly, alcohol-free, nonpartisan events raise turnout. If so, both reformers and social scientists should give greater attention to the benefits side of the voting equation.

Experimental Design

The research design employed here is what Boruch et al. (2004) have termed "place-based" field experimentation. The unit of analysis is the voting precinct. In each site, festival organizers identified the list of precincts that were deemed suitable for hosting events.

The criteria for what was considered suitable varied from site to site; often the dominant factor in selecting target precincts was proximity to the neighborhoods in which a community organization was active, so that festivals could be arranged quickly and inexpensively. In order to prevent the selection of target precincts from biasing our assessment of the festival's influence on turnout, the 38 target precincts were randomly assigned to treatment and control conditions. The treatment precincts received a festival; the control precincts did not. Because randomization of precincts occurred within each of the 14 locations, we must take care to include fixed effects for each location when analyzing the results. In effect, we are pooling the results from 14 distinct experiments.

The basic statistical model that emerges from our experimental design represents turnout as a function of three quantities: whether a given precinct received the treatment, the location within which random assignment occurred, and the past turnout of each precinct. Thanks to random assignment, past turnout is not required for unbiased inference, but by reducing the amount of unexplained variability in turnout, it greatly improves the precision with which we estimate the effects of our experimental treatment.

One final nuance concerns the scaling of the dependent variable. Because turnout in some of the elections we study is very low—sometimes below 10%—we transform the percentage of voting among registered voters into the log-odds of turnout in order to make the data more suitable for linear regression. Our regression model for precinct i at time t is

$$
\begin{aligned}
LN\frac{Turnout_{t,i}}{100-Turnout_{t,i}} &= \beta_0 + \beta_1 Treatment_i \\
&\quad + \beta_2 San\ Francisco_i \\
&\quad + \beta_3 Portland_i + \beta_4 Lewiston_i \\
&\quad + \beta_5 Austin_i + \beta_6 Pittsburgh_i \\
&\quad + \beta_7 Hartford_i + \beta_8 Stockton_i \\
&\quad + \beta_9 Green\ Bay_i + \beta_{10} St.\ Paul_i \\
&\quad + \beta_{11} Oakland_i + \beta_{12} Tallahassee_i \\
&\quad + \beta_{13} New\ Haven\ Municipal_i \\
&\quad + \beta_{14} New\ Hampshire_i \\
&\quad + \beta_{15}\left(LN\frac{Turnout_{t-1,i}}{100-Turnout_{t-1,i}}\right) \\
&\quad + \varepsilon_{t,i}.
\end{aligned}
$$

The key parameter of interest is β_1, which represents the average treatment effect of the festivals.

Throwing a Modern Day Festival

The model for the festivals in our study was developed in the small middle-class town of Hooksett, New Hampshire, the site of our first festival in spring of 2005. The festival was preceded by a week of publicity and local organizing. A week before Hooksett's municipal election, we met with the town administrator, election officials, and community leaders. We asked them to publicize our poll party and to hand out flyers advertising our event at town meetings. We also displayed posters announcing the event at local stores and meeting spots. On the Saturday before Election Day, the regional newspaper, the *Union Leader*, contained a flyer advertising an "Election Day Poll Party," giving the location and time. The local paper, the *Hooksett Neighborhood News*, also advertised the event. On the Sunday before Election Day, a story describing the party appeared in the *Union Leader*. At the same time, three dozen lawn signs advertising the event were planted on busy streets in town. Finally, two prerecorded 30-second phone calls were directed to 3,000 Hooksett households. The first call was made on Saturday and the second on Election Day. Both extended an invitation to the party and gave details about its hours (3:00 to 7:00 p.m.) and location. More than 89% of the calls were successfully completed, with approximately two-thirds of the

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completed calls reaching answering machines. Because automated calls have been repeatedly shown to have negligible effects on voter turnout (Green and Gerber 2004), we do not believe that these calls, by reminding people of the upcoming election, explain the apparent effects of the festival. Whether newspaper publicity generates turnout, however, remains unknown.

On Election Day, the festival took place immediately outside the polling place, on the front lawn of the local middle school. A large tent was set up surrounded by signs encouraging people to enjoy free snacks, drinks, and raffles. A cotton candy machine attracted a steady stream of children, and a professional DJ played upbeat, family-friendly music. People of all ages milled about the party tent. Young children snacked and played catch. Elderly couples took advantage of the chairs around the tent to sit, listen to the music, and eat the free sandwiches we provided. The free food relieved some harried parents of dinner preparation that evening, and they mingled with their friends and neighbors. People at the party seemed aware of the event prior to coming to the polls to vote. They had read the flyer, received the calls, or heard about the various advertised activities from other residents.

This model was next transported to inner-city New Haven, in order to see whether an analogous festival could be staged in a neighborhood that has none of Hooksett's suburban attributes. Our pool of target precincts consisted of two adjacent wards, both close to 90% African American and economically depressed. The experimental intervention again consisted of a publicity campaign and a festival at the randomly chosen treatment precinct. A week before the municipal election, we met with community leaders and asked them to publicize the poll party and to distribute flyers at ward meetings and church gatherings. We also displayed posters and lawn signs announcing the event at local meeting spots and outside of residential homes. The principal from the elementary school where we held the poll party also distributed our flyers to parents, students, and teachers. Three pre-recorded phone calls were directed to Ward 21-registered voters' households. The first call was made on Friday before Election Day, the second on Sunday before Election Day, and the third on Election Day. The calls extended an invitation to the party and gave details about its hours (3:00 to 7:00 p.m.) and location.

On Election Day, the festival took place immediately outside the polling place, on the front lawn of a local elementary school. As in Hooksett, a large tent was set up surrounded by signs encouraging people to enjoy free hamburgers, hotdogs, and hot chocolate, and to participate in the raffle. Again, cotton candy and popcorn drew crowds of children. A popular local DJ played family-friendly music while he doubled as a clown. At points during the party, he trolled through the neighborhood with his wife, also dressed as a clown, calling for neighbors to join the party and to vote. The neighborhood has its share of violence, so we were asked by the school officials to have a police officer on duty during the hours of the poll party. Another police officer sat on watch inside the school building. Despite these concerns, there was no trouble of any kind at this site or at any of the sites in our study. As in Hooksett, people of all ages milled about the party tent. An impromptu dance contest broke out among the children, the prize being 14 one-dollar bills collected from the audience. Mothers and fathers mingled with their friends and neighbors. The party-goers devoured an enormous quantity of hot dogs and hamburgers. Again, as in Hooksett, people at the party seemed to be aware of the event prior to coming to the polls to vote. Many stated that they were lured to the party by the music and the smell of barbecue cooking throughout the neighborhood. Others said that they had seen the lawn signs, received the calls, or heard about the various advertised activities from other residents.

The 2005 festivals in Hooksett and New Haven served as models for a dozen festivals in 2006. In collaboration with Working Assets, a long-distance phone company that funds progressive political causes, we designed a series of precinct-level experiments. Local community groups funded by Working Assets conducted the festivals. In order to assist the community groups, which were organizing a festival for the first time, we developed a festival preparation checklist. These included instructions about planning and advertising the festival, steps for setting up and holding the festival on Election Day, and requirements about follow-up after the festival so that researchers could gather qualitative information about each event. Table 1 lists some of the salient features of each festival. Space constraints prevent us from describing them in detail, but the core elements of the treatment were consistent across widely varying settings: pre-event publicity, a tent defining the space of the party, music, and free food. In each case, party-goers were informed that the festival was sponsored by the local coordinators and that attendance was open to everyone. Approximate attendance varied from a maximum of 250 in Tallahassee to a minimum of 10 in Lewiston, with an average of 75 overall.

Results

Table 2 reports the regression coefficients, both with and without controls for past turnout rates. Although the estimated treatment effect is similar in magnitude in both regressions, the regression that controls for past turnout generates much more precise estimates. We therefore focus our attention on the results from the regression that controls for past turnout. This regression model estimates β_1 to be .261, with a standard error of .091. The t-ratio of 2.85 is significant (one-tailed) at the .01 level, indicating that there is less than a 1% chance that one would see a positive effect this large due to chance. This t-ratio falls to 2.49 but remains well below conventional significance thresholds when we use bootstrapping to calculate the standard errors, so as to account for the extra variability that occurs when small numbers of precincts are divided into treatment and control groups (see Freedman, Pisani, and Purves 1998, A32–3). Regression diagnostics presented in the Appendix show that the residuals pass the standard tests of homoskedasticity and normality.

This estimated treatment effect, expressed in terms of percentage points, implies that a festival held in a context where the expected base rate of voting is 50% would produce a turnout rate of 56.5%—a 6.5 percentage-point increase. In a low-turnout (10%) context, an increase of 0.261 in log-odds implies a treatment effect of 2.6 percentage points.[3]

How do Election Day festivals compare to other get-out-the-vote tactics in terms of cost-effectiveness? Green and Gerber (2004, 94) suggest that direct-mail campaigns tend to produce votes at rates of at least $60 per vote. Phone-calling campaigns vary in terms of cost-effectiveness, with the most efficient commercial phone banks coming in at $35 per vote and the least efficient at more than $100 per vote. Door-to-door canvassing campaigns are, where feasible, more cost-effective at roughly $20 per vote. The 2005 and 2006 festivals cost a total of $26,630. Assuming that the festivals increased the log-odds of turnout by 0.261, a total of 960 additional votes were generated in the treatment precincts.[4] This ratio implies that festivals generate votes at an average rate of $28 per vote.

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Table 1
Descriptive Information about Experimental Sites

Site	Date/Time	# in T:C	Type of Election	Median HH Income in Treatment Precinct	Demographic Profile of Treatment Precincts	Party Organizers & Sponsors	Other Notes
San Francisco	6/6/06 3–7 p.m.	1:2	Primary, Gov. & State Offices	$45,112	40% Caucasian, 27% Af-Am, 7% Latino	Working Assets, League of Young Voters	R&B music (disc jockey), jazz band
Portland	6/13/06 4–8 p.m.	1:1	Primary, Minimally Competitive	$25,556	92% Caucasian, 2% Af-Am, 2% Latino	Working Assets, League of Young Voters	Hip hop music; attempts to attract a younger crowd
Lewiston	6/13/06 3–7 p.m.	1:1	Primary, Minimally Competitive	$18,555	100% Caucasian	Working Assets, League of Young Voters	Alternative music, poetry reading; attempts to attract a younger crowd
Austin	5/13/06 12–4 p.m.	3:3	Municipal, Minimally Competitive	T1: $21,147; T2: $52,469; T3: $47,809	T1: 49% Latino, 44% Af-Am; T2: 90% Caucasian; T3: 78% Caucasian, 13% Asian, 11% Latino	Working Assets, Buena Vista	Many children, moon-jump ride for kids, piñata and quesadillas
Pittsburgh	3/14/06 3–7 p.m.	1:1	Special Election, Municipal	$90,615	100% Caucasian	Working Assets, League of Young Voters	University of Pittsburgh, student-districts, pizza, cold weather
Hartford	8/8/06 3–7 p.m.	1:2	Primary, Governor	$22,140	44% Latino, 36% Af-Am	Working Assets, CT Citizen Action Group	Face painting for children, barbecue
Stockton	6/6/06 3–7 p.m.	1:3	Primary, Governor	$49,205	51% Asian, 17% Latino, 14% Caucasian, 10% Af-Am	Working Assets, League of Women Voters	Advertisements in Spanish and English, acoustic guitarist singing songs in Spanish, middle school group playing Taiko drums
Green Bay	9/12/06 3–7 p.m.	1:2	Primary, Open Congressional Seat	$38,820	86% Caucasian, 7% Latino, 4% Asian, 3% Native American, 1% Af-Am	Working Assets, Citizen Action of Wisconsin	Students received community service credit for volunteering at event, municipal politicians attended event to speak about voting, heavy rain
St. Paul	9/12/06 3–7 p.m.	1:1	Primary, Minimally Competitive	$38,774	64% Caucasian, 11% Af-Am, 10% Asian, 8% Latino	Working Assets, League of Young Voters	Sidewalk chalk games and face painting for kids, local vendors selling jewelry, paintings
Oakland	11/07/06 3–7 p.m.	1:1	General Election, Gov. State-wide offices, Congressional Seats	$48,232	36% Af-Am, 24% Caucasian, 22% Latino	Working Assets, Music For America	Pizza, barbecue, New Orleans-style brass band
Tallahassee	11/07/06 3–7 p.m.	1:1	General Election, Gov. State-wide offices, Congressional Seats	$52,776	58% Caucasian, 34% Af-Am, 4% Latino	Working Assets, Young People For the American Way	Florida A & M University, hamburgers & hotdogs, pool and bowling for students
New Hampshire	05/10/05 3–7 p.m.	1:1	Municipal Election, No contested races	$61,654	96% Caucasian	Authors, Yale ISPS	Disc Jockey, cotton candy, great weather, entire families enjoying event
New Haven Municipal	11/08/05 3–7 p.m.	1:1	Municipal; No contested races	$35,504	37% Af-Am, 36% Caucasian, 21% Latino	Authors, Yale ISPS	Clown, cotton candy, popcorn, many children, spontaneous dance contest
New Haven Senatorial Primary	08/08/06 3–7 p.m.	1:2	Primary, Gov., Congressional Seats	$35,504	37% Af-Am, 36% Caucasian, 21% Latino	Working Assets, Yale ISPS	Festival became site of televised Senatorial candidate's stump speech

This figure is certainly respectable and may have been more impressive had our sample of elections and precincts been configured differently. Many of our festivals took place in low-salience elections and low-turnout precincts. Because the baseline turnout rate was very low, a sizeable change in log-odds failed to produce a large change in the absolute number of votes. Had the baseline level of turnout been 50%, the .261 logit estimate implies that 2,339 votes would have been generated, at a rate of $11 per vote.

Conclusion

The 2005–2006 Election Day festivals experiments have broad-ranging implications. For campaigns, the pattern of statistically and substantively significant findings means that social gatherings represent a potentially valuable voter mobilization tactic. Just as voter mobilization experiments demonstrating the cost-effectiveness of door-to-door canvassing helped encourage political campaigns to invest more resources in this activity (Bai 2004), research showing the effectiveness of festivals may revive the kind of benefits-oriented approach to voter mobilization that prevailed a century and a half ago.

That said, the full range of practical implications has yet to be discovered. How best to throw a party remains an open research question. Our nonpartisan festivals, varied as they were, scarcely exhaust the possible variations on the festival theme. One could imagine festivals held on weekends in jurisdictions

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Table 2
Effects of Election Day Festivals on the Log-odds of Voter Turnout

	Regression Excluding Past Voter Turnout		Regression Including Past Voter Turnout	
	B	SE	B	SE
Treatment	0.203	0.166	0.261*	0.091
San Francisco	–1.413	0.406	–2.081	0.240
Portland	–2.372	0.454	–3.188	0.272
Lewiston	–2.214	0.454	–2.817	0.262
Austin	–2.655	0.353	–2.029	0.211
Pittsburgh	–3.008	0.454	–1.933	0.288
Hartford	–0.501	0.406	–0.954	0.230
Stockton	–1.655	0.380	–1.937	0.211
Green Bay	–1.195	0.406	–0.946	0.225
St. Paul	–1.966	0.454	–1.027	0.279
Oakland	–0.740	0.454	–0.970	0.251
Tallahassee	–0.079	0.454	–1.267	0.296
New Hampshire	–1.989	0.454	–1.902	0.249
New Haven Municipal	–1.050	0.454	–1.274	0.251
Log-odds of Turnout In Prior Election	N/A	N/A	0.679	0.092
Constant	–0.339	0.292	0.849	0.227
N	38		38	
Adjusted R-square	.754		.926	

*The significance of the treatment effect, using conventional OLS standard errors, is $p < .01$. The standard error of the treatment effect is estimated to be .122 using bootstrapping, a procedure that accounts for the fact that random assignment occurred within a small set of precincts, implying a p-value of .022. When interpreting the dummy variables for site, the omitted category is the 2006 New Haven Senate primary election.

that allow for early or mail-in balloting, festivals that are designed to feature all-school musical performances or other big-draw events, or partisan festivals held in homes, workplaces, or organization headquarters. As our studies of festivals move forward, we welcome partners interested in exploring and evaluating the many variations on the festivals theme. Of course, university research funds can only be used to sponsor or promote nonpartisan events, but we nevertheless remain eager to investigate the enormous political potential of this approach to voter mobilization.

From a theoretical standpoint, the results presented here have two important implications. First, they shed new light on how institutional changes in the late-nineteenth century may have brought about a decline in voter turnout. Much has been said about the introduction of the secret ballot and the way in which secrecy disrupted vote-buying (Rusk 1974). Relatively little attention has been paid to other reforms, such as the requirement that candidates and campaign workers remain a certain distance (often 100 feet) from voting places, a rule that not only disrupts surveillance of voting activity but also dramatically alters the social ambiance surrounding the polling station. As Bensel (2004) points out, prior to these reforms the act of casting a ballot put the voter at the center of a public spectacle played out in front of an often raucous crowd. After reform, there was nothing to see and no one watching.

The second implication concerns the paradox of political participation. Scholars have long observed that elections create a collective action problem. From the standpoint of the rational self-interested voter, voting is time-consuming, and the chances of casting a pivotal vote are remote. One theoretical answer to this paradox has been to posit psychic benefits that voters receive when they vote (Riker and Ordeshook 1968), and one interpretation of the effect presented here is that festivals provide social approbation for and from those who perform their civic duty. Another theoretical answer is that the cost of voting is so small that even minor material inducements are sufficient to draw nonvoters to the polls (Palfrey and Rosenthal 1985, 73). The surge in turnout we observe is consistent with both hypotheses, and further experiments are needed to examine the relative power of social and gustatory benefits.

Finally, we wish to underscore the importance of exploring broad historical questions through randomized experimentation. Granted, one cannot easily recreate the atmosphere surrounding nineteenth-century elections, because even if one were to hand out whiskey and cigars, these blandishments are no longer part of a voting process that welcomes and emphasizes them. As Lieberson (1987) has argued, causal processes may work in an asymmetrical fashion; the decline in turnout that accompanies the disappearance of selective benefits may exceed the rise in turnout associated with their reintroduction. Nevertheless, until one performed experiments of the kind described in this essay, one would have had no idea whether festivals increase turnout by 0.5%, 5%, or 50%. Contemporary experiments cannot resolve historical puzzles, but they do bring important new facts to bear.

Some aspects of nineteenth-century elections—petty bribes and drunken brawls—are best left in the past. Nevertheless, contemporary America has something positive to learn from its distant past, and social scientists have an important role to play insofar as they draw behavioral hypotheses from history, propose interventions to test these hypotheses, and embed these interventions within a rigorous experimental research design.

Notes

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1. Today, other countries, such as India, view Election Day as a significant cultural ritual

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where the community gathers as a whole to socialize and affirm its commitment to democracy (Hauser and Singer 1986).

2. This prohibition has not prevented some firms from offering rewards to those who produce proof of having voted. For example, in 2002 a chair of ice cream stores in the Chicago area offered free ice cream to voters.

3. These figures are based on the following calculations. The log-odds of 50% is 0, and the log-odds of 56.5% is .261. Similarly, the log-odds of 12.6% is −1.94, which is approximately .261 logits greater than the log-odds of 10%, which is −2.2.

4. This figure is calculated by subtracting the estimated treatment effect from the log-odds of voting in each treatment precinct, in order to calculate the counterfactual voter turnout rate. Subtracting this rate from the observed rate and multiplying by the number of registered voters in the treatment precinct gives the estimated vote gain. Bear in mind that most of the treatment precincts in our sample were small. Whereas Hooksett had close to 8,200 registered voters and publicity was broadcasted to the entire town, the average treatment precinct other than Hooksett had 1,858 registered voters and publicity was targeted fairly narrowly. Raising turnout from 10% to 12.5% in a precinct with 1,858 voters means generating 46 votes.

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Appendix

Regression Diagnostics

Figure A1 illustrates the multivariate regression results presented in Table 2. The regression line shows the effect of a one-unit change in the treatment variable, net of the effects of past voter turnout and site-specific dummy variables.

Breusch-Pagan/Cook-Weisberg test for homoskedasticity: This test assesses whether the variance of the disturbances is predicted by the fitted values of the regression. The test-statistic of 0.69 with 1 degree of freedom is non-significant, $p = .41$.

Shapiro-Wilk W test for normality, as applied to the residuals: The test-statistic of Z = .92 is non-significant, $p = .18$.

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Figure A1
Illustration of the Estimated Effect of Treatment on the Log-odds of Voter Turnout, Controlling for Covariates

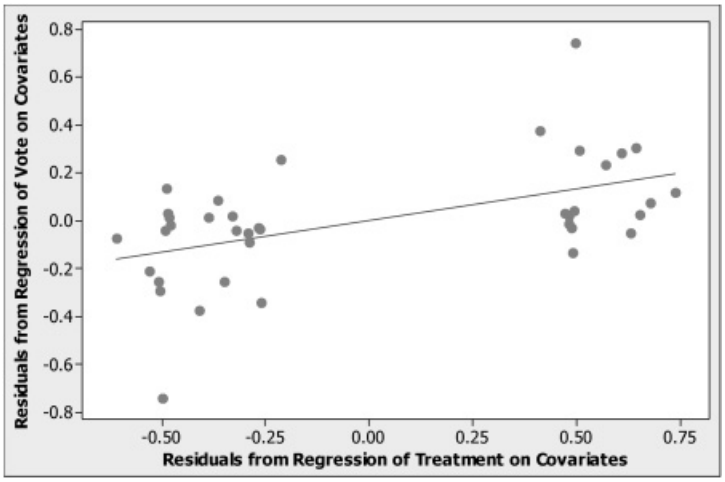

Table A1
Site-by-Site Treatment Effects

Site	Estimated Treatment Effect	Approximate Standard Error	Cumulative Estimated Treatment Effect	Cumulative Standard Error
Austin	–0.04	0.28	–0.04	0.28
Green Bay	0.12	0.40	0.02	0.23
Hartford	0.03	0.40	0.04	0.20
Hooksett	1.50	0.48	0.23	0.18
Lewiston	0.19	0.48	0.23	0.17
New Haven (2006)	0.13	0.40	0.21	0.16
New Haven (2005)	0.11	0.48	0.20	0.15
Oakland	–0.18	0.48	0.17	0.14
Pittsburgh	1.46	0.48	0.27	0.14
Portland	0.13	0.48	0.26	0.13
San Francisco	0.59	0.40	0.29	0.13
St. Paul	0.03	0.48	0.28	0.12
Stockton	0.21	0.34	0.27	0.11
Tallahassee	0.54	0.48	0.28	0.11
Overall	**0.28**	**0.11**	**0.28**	**0.11**

Table A1 shows the results from regressions in which change in turnout (in logits) is regressed on a dummy variable for treatment. Regression results are shown for each site in columns 1 and 2. Columns 3 and 4 show how these results cumulate into the overall regression result presented in the bottom row. The overall result is slightly different from what is presented in Table 2 because here the use of a differenced dependent variable (current turnout minus lagged turnout) in effect fixes the value of β_{15} at 1.0, whereas β_{15} is a free parameter in Table 2. We fix the value of β_{15} in order to estimate treatment effects for sites where N = 2. Standard errors were calculated by extrapolating from the estimated OLS standard error based on the total sample, assuming homoskedasticity. Note that each of the confidence intervals for the 14 sites encompasses the overall treatment estimate of .28, which justifies the simple pooling of the results, as shown here. A formal test of homogenous treatment effects across sites is non-significant (χ^2 with 13 degrees of freedom = 16.6, p = .22). The fact that two of the 14 sites show negative estimates is consistent with what we would expect given sampling error. In fact, a nonparametric one-tailed test of the null hypothesis of no positive treatment effect may be calculated based on the fact that the sign of the estimated treatment effect is positive in 12 of 14 instances, p = .006.

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