Aggregate Effects of Large-Scale Campaigns on Voter Turnout

Ryan D. Enos and Anthony Fowler[1]

(Revised and Resubmitted to the *Quarterly Journal of Political Science*)

Abstract

To what extent do political campaigns mobilize voters? Despite the central role of campaigns in American politics and despite many experiments on campaigning, we know little about the aggregate effects of an entire campaign on voter participation. Drawing upon inside information from presidential campaigns and utilizing a geographic research design that exploits media markets spanning state boundaries, we estimate the aggregate effects of a large-scale campaign. We estimate that the 2012 presidential campaigns increased turnout in highly targeted states by 7-8 percentage points, on average, indicating that modern campaigns can significantly alter the size and composition of the voting population. Further evidence suggests that the predominant mechanism behind this effect is traditional ground campaigning, which has dramatically increased in scale in the last few presidential elections. Additionally, we find no evidence of diminishing marginal returns to ground campaigning, meaning that voter contacts, each likely exhibiting small individual effects, may aggregate to large effects over the course of a campaign.

[1] Both authors contributed equally. Ryan D. Enos (renos@gov.harvard.edu) is an Assistant Professor in the Department of Government at Harvard University. Anthony Fowler (anthony.fowler@uchicago.edu) is an Assistant Professor in the Harris School of Public Policy Studies at the University of Chicago. We thank Scott Ashworth, Rich Beeson, Chris Berry, David Broockman, Ethan Bueno de Mesquita, Peter Enns, Susan Fiske, Rayid Ghani, Trey Grayson, Don Green, Andy Hall, Hahrie Han, Eitan Hersh, Greg Huber, Scott Jennings, Bob Kubichek, Mary McGrath, Liz McKenna, Ryan Meerstein, Zac Moffat, Ethan Roeder, Gaurav Shirole, John Sides, Aaron Strauss, Will Howell, and conference participants at SPSA and Oxford for helpful comments and insights into the 2012 presidential campaigns.

What are the consequences of political campaigns for mass participation? Despite extensive research on individual campaign tactics and cumulative campaign spending totaling billions of dollars, we do not yet understand the aggregate effects of an entire campaign on voter participation. In this paper, we take advantage of geographic discontinuities in campaigning and use new data on voters and campaigns to show that, in the aggregate, campaign efforts have large effects on voter turnout, causing the participation of millions of voters who would otherwise not have participated, and significantly altering the size and shape of the voting population.

A modern presidential campaign spends over one billion dollars to run hundreds of thousands of television advertisements and attempt hundreds of millions of individual voter contacts.[2] But there is reason to believe this massive effort has negligible or small effects on voter participation. Television advertising appears to have no substantively significant effect on turnout (e.g., Krasno and Green 2008). Isolated campaign efforts can increase voter participation, but the substantive size of these effects is usually small. Green, McGrath, and Aronow (2013) pool evidence from more than 200 get-out-the-vote (GOTV) experiments—published and unpublished—to show that, on average, door-to-door canvassing increases turnout by 1.0 percentage point, direct mail increases turnout by 0.7 percentage points, and phone calls increase turnout by 0.4 percentage points.[3] Previous research also suggests that individual variation in turnout is largely explained by non-campaign factors, such as individual utility (Riker and Ordeshook 1968), social pressure (Gerber, Green, and Larimer 2008), resources (Verba, Schlozman, and Brady 1995), or personality (Gerber et al. 2011), suggesting that the effects of large campaigns should be minimal.

[2] The number of television advertisements is taken from data made publicly available by *The Washington Post* and originally collected by Kanter Media/CMAG. Individual voter contact numbers are compiled from documents we obtained from the campaigns.

[3] These are estimates of the average intent-to-treat effects, obtained through private correspondence with Green, McGrath, and Aronow (2013).

With this research in mind, one might conclude that campaigns have a negligible effect on aggregate voter turnout. However, the tactics of campaigns have recently changed, with modern campaigns increasingly using scientifically tested, individual-level targeting (Hersh 2015, Isenberg 2013). In particular, the sheer scale of individual voter contact has increased dramatically over the past few presidential elections. These changes in tactics and resource allocation may have increased the ability of campaigns to mobilize voters.[4]

The GOTV experiments cited above do little to inform us about the aggregate effects of a modern campaign using these new methods, where targeted voters are potentially contacted dozens of times. In nearly all of these studies, a single targeting intervention is compared to no intervention. Few experiments have assigned subjects to receive multiple interventions (e.g., Cardy 2005; Gerber and Green 2000; Michelson, Bedolla, and Green 2007; Ramirez 2005), and none have come close to matching the level of a full-scale campaign. Given the current evidence, we have virtually no way of knowing what the effects of a large-scale campaign may be. At one extreme, the effects of many voter contacts could be counterproductive by fatiguing potential voters and turning them away. At the other extreme, the effects of many mobilization efforts could be synergistic and multiplicative. Somewhere in between, multiple interventions could have additive effects or the returns could be positive but diminishing. Simply put, despite extensive research on campaigning, we still know little about the aggregate effects of campaigns on political participation.

In this paper, we exploit the 2012 presidential campaign to assess the aggregate effects of a large-scale campaign on the size and composition of the voting population. We take advantage of variation in campaigning across state boundaries, validated turnout data for essentially every eligible voter in the United States, and perhaps most importantly, extensive information on campaign tactics

[4] These points apply only to the effect of campaigns on voter turnout. Campaigns have other purposes such as persuasion and information provision, and this paper says nothing about their effectiveness on those dimensions.

including data from the Romney campaign on the number and type of voter contacts attempted in each state. Our results suggest that the aggregate mobilizing effect of a presidential campaign is quite large. We estimate that the 2012 campaign increased aggregate turnout by 7-8 percentage points in the most heavily targeted states, and this effect is greater than 10 percentage points for targeted subgroups. In total, our estimates imply that 2.6 million individuals voted who would have otherwise not participated in the absence of campaigning, suggesting that modern campaigns play a significant role in mass participation.

Having identified a large, aggregate effect, we then explore several potential explanations and mechanisms. First, we discuss and rule out the possibility of biased estimates arising from differences between targeted and non-targeted states. Placebo tests show no differences between battleground and non-battleground residents residing in the same media market on demographic covariates or turnout in 2010. Furthermore, our results are unaffected by the inclusion or exclusion of individual-level covariates such as race and gender and state-level controls such as the coincidence of a senatorial or gubernatorial election and voting laws.

Second, we largely rule out the possibility that battleground voters are more motivated to vote even in the absence of campaigning. Placebo tests using survey data show no differences between battleground and non-battleground residents in terms of their pre-campaign levels of political interest or desire to vote. Our estimated effects of campaigning are larger for the kinds of citizens who were likely targeted, suggesting that statewide differences in voter motivation cannot explain our results.. Previous experimental evidence suggests that considerations of pivotality have little effect on turnout. We also show that there was little difference in participation between battleground and non-battleground states for previous elections before 2008, when presumably, the individual incentive to vote associated with living in a battleground state was just as great.

Third, we examine the effects of television and internet advertising, and consistent with previous research, we find that these effects are small. On the whole, the evidence suggests that most of our estimated effect is explained by ground campaigning, i.e., individual voter contact through door-to-door canvassing, phone calls, and direct mail. Furthermore, non-parametric analyses and back-of-the-envelope calculations suggest that there is little diminishing marginal return to ground campaigning. Our results are consistent with the possibility that individual voter contacts have small, positive effects, as identified in experiments, by many voter contacts add up to meaningful effects in large-scale campaigns.

Data

Our data on voters and participation comes from Catalist, a for-profit data vendor that maintains records of every eligible voter in the United States. We obtained detailed tabulations which allow us to reconstruct an anonymous version of their database. Therefore, for virtually every eligible voter in the U.S., we know their gender, predicted race, predicted income range, state, media market, and validated voter turnout.[5] We analyze this data in conjunction with information that we obtained through interviews and private correspondence with numerous high-level operatives and strategists from the Obama and Romney campaigns. Through these interviews, we obtained specific information regarding the campaigns' overall strategies, the extent of voter mobilization efforts, the places that were targeted, and the types of individuals that were targeted. Importantly, for the Romney campaign, we obtained data on the number and type of voter contacts attempted in each

[5] We do not know the voters' names, addresses, or any other identifying information. Catalist generates and maintains their database through state voter files and consumer records. There are surely some eligible but unregistered voters who are missed by Catalist and some ineligible individuals who are mistakenly included in their database. However, the numbers in their database closely match estimates of the eligible voting population in each state, suggesting that these concerns are minimal. For more information on Catalist data and examples of political science research employing this data source, see Ansolabehere and Hersh (2012) and Hersh (2013).

state, and for the Obama campaign, we obtained an internal prioritization of states. We also obtained data from the Obama campaign on all internet-based advertising, including the geographic location, type of advertisement, cost, volume, and advertising provider.

Empirical Strategy: Comparisons within Media Markets but across State Boundaries

Our basic empirical strategy relies on the discontinuous changes in presidential campaigning across state boundaries. Because of the Electoral College, the returns to campaigning vary across states. For example, the presidential campaigns made virtually no effort to mobilize voters in Wyoming and Massachusetts, where Romney and Obama were essentially guaranteed to win, respectively, regardless of campaign effort. However, both teams campaigned heavily in Colorado and New Hampshire, where the outcome of the race was uncertain and these states could potentially tip the nationwide election result. In this sense, we follow previous studies that have exploited this variation across battleground and non-battleground territory in studying campaigns or voting behavior (e.g., Ashworth and Clinton 2007; Gerber et al. 2009; Huber and Arceneaux 2007; Kim, Petrocik, and Enokson 1975; Krasno and Green 2008).

Of course, battleground states differ from non-battleground states in many ways other than the extent of voter mobilization. Most notably, voters in battleground territory receive more television advertising and more intense news coverage of the election. In order to separate the effects of mass media and campaign activity, we take advantage of media markets that span state boundaries and conduct all of our comparisons within media market, so that we can be sure that the "treated" and "control" groups received the same level of television ads and news coverage. This has the added benefit of focusing our comparisons on relatively small geographic areas where, presumably, individuals are more similar than those living far apart in different metropolitan areas. For example, we compare voters within the Denver media market that live in either Colorado or

Wyoming, and we compare voters within the Boston media market that live in either New Hampshire or Massachusetts. In some specifications, we also condition on individual characteristics—gender, race, and predicted income—to improve precision and account for further differences between citizens in different states. In additional specifications, we control for whether there is a competitive Senate or gubernatorial election in a state, the particular election laws and voting methods in a state, and the level of internet-based advertising in each state. The results are nearly identical across each specification, indicating that individuals in battleground states are, on average, comparable to those in the same media market but in a non-battleground state. Our basic strategy has similar elements to those of Huber and Arceneaux (2007) and Krasno and Green (2008) who compare residents of battleground and non-battleground media markets within the same non-battleground state to assess the impact of television advertising. However, our design is essentially the inverse; we compare residents of battleground and non-battleground states within the same media market, allowing us to hold media exposure constant.[6]

This strategy provides unbiased estimates of the aggregate effects of campaigning under the assumptions that, on average, conditional on observable covariates, voters in the same media market in battleground and non-battleground states 1) have the same underlying propensity to vote, 2) receive the same boost in turnout from mass media and 3) are subject to the same boost in turnout from forces other than campaigning such as psychological and social pressures not induced by the campaign. Assumption 1 is *a priori* plausible, because the factors determining which states are targeted are likely unrelated to the underlying interest of citizens in voting. Assumption 2 is defensible because voters in the same media market see the same television, because we are able to control for internet advertising, and because few television ads contain state-specific content. We

[6] In the Appendix, we replicate the basic design of Krasno and Green (2008) to estimate the effect of television advertising on turnout. Consistent with evidence from previous elections, we find that television ads had no detectable effect on turnout in 2012.

further justify all these assumptions, including Assumption 3, by referencing previous research, conditioning on observable covariates, running placebo tests, and showing that our estimates vary in predictable ways. We discuss each of these assumptions and their potential violations in more detail later in the paper.

Our design requires us to categorize the intensity of campaign activity across states, and we take three different approaches. Importantly, each of these approaches yields similar results, so our findings are not based on any particular measurement strategy. First, we code a binary variable, *Battleground*, indicating whether intense campaigning occurred in a particular state. This variable takes a value of 1 in Colorado, Florida, Iowa, Nevada, New Hampshire, North Carolina, Ohio, Virginia, and Wisconsin. These states received the vast majority of media attention, television ads, and campaign field offices (Sides and Vavreck 2013). These were also the states identified by news organizations and political consultants as the key swing states.

For our second approach, we divide the states into four categories according to an internal categorization used by the Obama campaign.[7] The first tier, receiving the greatest degree of campaign effort, includes Colorado, Iowa, Nevada, New Hampshire, Ohio, and Virginia. The second tier, also receiving significant campaign effort but less than the first tier, includes Pennsylvania, Florida, and North Carolina. The third tier, receiving some campaigning but less than the first two tiers, includes Wisconsin, Minnesota, Washington, Oregon, Michigan, and New Mexico. The remaining states in none of the three tiers received virtually no attention.

For our third approach, we utilize data on the number of voter contacts attempted by the Romney campaign in each state. Specifically, we obtained an internal document from the campaign summarizing the number of attempted phone calls, direct mail deliveries, and door-to-door

[7] Internal Obama campaign document from May 2012, "Developing State Priorities," obtained through personal communication with Obama campaign staff.

canvassing efforts in each state.[8] Using these numbers, we construct a continuous scale indicating the average effort level of the Romney campaign in each state. However, not all types of contacts indicate equal effort. In determining the relative weights that we should give to each type of voter contact, we relied upon estimates of the cost of each mobilization method. Green and Gerber (2008) estimate that the cost of phone calls, direct mail, and door knocks are approximately 30 cents, 75 cents, and 3 dollars, respectively, per attempt. These estimates allow us to approximate the amount of effort per state in terms of the hypothetical cost of those campaign efforts per eligible voter. Twenty-eight states plus Washington, D.C. received no effort whatsoever, while many other states received significant effort. Nevada received the highest level of effort with approximately 5 phone calls, 0.7 pieces of direct mail, and 0.5 door knocks attempted for every eligible voter in the state. This amounts to an imputed cost of $3.40 per person in Nevada. New Hampshire, Virginia, Iowa, Ohio, and Colorado were next, with costs exceeding 2 dollars per person. Wisconsin, Florida, and North Carolina followed with costs exceeding 1 dollar per person. We rescale this variable to range from 0 to 1 and call it *RomneyEffort*.[9] Comparing the emphasis put on different states by the Romney campaign to the internal categorization used by the Obama campaign, the campaigns appear to have prioritized the states in roughly the same way.[10] The state-level data from the Romney campaign, which we believe represents a significant improvement over previous attempts to measure campaign activity, is presented in the Appendix.

Figure 1 presents a map of *RomneyEffort* across states and media markets. Media markets spanning states with different levels of *RomneyEffort* are surrounded by thick black lines. Portions of

[8] Internal Romney campaign document, "Romney Voter Contact Summary," obtained through personal communication with Romney campaign staff.

[9] We combine the mobilization tactics into a single variable because the number of phone calls, direct mailings, and door knocks are, of course, highly correlated across states, making it unwise to use each as a separate variable in the regressions below.

[10] The 6 states in Obama's top tier are also the top 6 states for Romney. The next three states in Obama's second tier are all in Romney's top ten. The only discrepancy is Wisconsin, which appears to have been prioritized more highly by Romney than Obama.

each media market are shaded by the level of Romney effort in that state. So, for example, the Reno media market is much darker in Nevada than California, representing the variation in *RomneyEffort* across those states. States with no effort or only minimal effort are the lightest shade. The media markets that do not span state lines with varying levels of *RomneyEffort* are indicated by diagonal lines. The voters in these areas do not enter our sample. This map highlights the sample of voters and sources of variation that we utilize in the regressions below to estimate the effects of campaigning. In the Appendix, we show similar figures for our other variables measuring campaign activity.

We specify three approaches to estimating the average effect of campaigning on voter turnout, using each of the measures of campaign effort described above. Employing OLS, we estimate the following three regressions:

(1) $\text{Turnout}_{ijk} = \beta_0 \text{Battleground}_j + \gamma_k + \delta X_i + \varepsilon_{ijk}$.

(2) $\text{Turnout}_{ijk} = \beta_1 \text{Tier1}_j + \beta_2 \text{Tier2}_j + \beta_3 \text{Tier3}_j + \gamma_k + \delta X_i + \varepsilon_{ijk}$.

(3) $\text{Turnout}_{ijk} = \beta_4 \text{RomneyEffort}_j + \gamma_k + \delta X_i + \varepsilon_{ijk}$.

$Turnout_{ijk}$ is an indicator for voter turnout for individual i in state j and in media market k. As described previously, $Battleground_j$, $Tier1_j$, $Tier2_j$, and $Tier3_j$ are binary variables indicating whether state j falls into that particular category of campaign effort, and $RomneyEffort_j$ is a continuous variable ranging from 0 to 1 indicating the extent of voter mobilization effort by the Romney campaign in state j. γ_k represents media market fixed effects which account for the fact that individuals in different media markets received different levels of news coverage and television advertising. These fixed effects allow all of our inferences to be made within media markets but across state boundaries. X_i represents a vector of individual characteristics—race, income, and gender—that we include to account for the potential that even within media markets, there may be some systematic demographic differences across state boundaries.

β_0 in Equation 1 represents the average difference in turnout between battleground individuals and demographically similar non-battleground voters within the same media market. Under our assumptions, this quantity represents the average effect of the campaigns' mobilization efforts for all citizens in battleground states.[11] Similarly, β_1, β_2, and β_3 from Equation 2 represent the average effect of being in the first, second, or third tier of campaign effort, respectively, relative to receiving virtually no campaign effort. β_4 from Equation 3 represents the average effect of going from a state with virtually no mobilization effort to the state with the greatest voter mobilization effort. To be clear, all three specifications are intended to measure the aggregate effects of all GOTV campaigning by Obama, Romney, and independent groups. While we use information on mobilization efforts from the Obama and Romney campaigns in separate regressions, both campaigns prioritized states in virtually the same way, so all regressions inform us about the average effects of all campaigning. These differences between different regression results tell us nothing about the relative effectiveness of Obama and Romney. Later, we separately examine different subgroups of individuals in order to compare the effectiveness of the two campaigns.

Placebo Tests

Before proceeding to the results, we discuss placebo tests designed to assess the validity of our assumptions and empirical design. In each case, we implement our designs described above in Equations 1-3, using different placebo outcomes. One concern is that, for whatever reason, battleground residents differ from non-battleground residents in their underlying propensity to vote. To address these concerns, we conduct placebo tests using voter turnout in 2010 and demographic

[11] Specifically, our estimates are local to those individuals who live in a targeted state and who live in a media market that spans a non-targeted state. If the effect of campaigning is different for individuals in other places, we cannot say anything about the effect of campaigning on those places.

covariates (results shown in the Appendix).[12] We find minimal differences between battleground and non-battleground voters on these dimensions, suggesting that battleground voters are comparable to non-battleground voters in their underlying levels of participation and political interest.

Another concern is that battleground voters are more motivated to participate for reasons unrelated to campaigning, perhaps because perceive their vote to be more pivotal. We discuss this issue and present additional evidence later in the paper, but for now, we can partially address this this concern by conducting placebo tests using survey data from the Cooperative Campaign Analysis Project (CCAP) in the first 16 weeks of 2012, before the general campaign began. Specifically, we use survey measures of political interest and intention to vote as placebo outcomes, and find that battleground voters are no more likely to report that they are interested in politics or that they plan on voting before the campaign began, suggesting that residence in a battleground state and considerations of pivotally have little direct effect on participation independent of campaigning. The results of these placebo tests, discussed and presented in more detail in the Appendix, help to validate our design and suggest that our subsequent results can be attributed to campaigning as opposed to spurious factors and independent effects of battleground environments.

Aggregate Campaign Effects in 2012

Table 1 presents our main results using 2012 turnout as the dependent variable. These results measure the aggregate effect of all campaigning in 2012. Columns 1-3 show results for Equation 1, using battleground states as the key independent variable, Columns 4-6 show results for Equation 2, using state tiers as the key independent variable, and Columns 7-9 show results for Equation 3, using *RomneyEffort* as the key independent variable. Columns 1, 4, and 7 include no

[12] We cannot use turnout in 2008 as a placebo outcome, because presidential campaigning across states was very similar for both 2008 and 2012. Therefore, an analysis with 2008 turnout as the outcome variable would essentially be another test of the effectiveness of modern presidential campaigns using older, less accurate data.

controls other than media market fixed effects. Columns 2, 5, and 8 also include individual controls for gender, race, and income. Columns 3, 6, and 9 include an additional control for turnout in 2010. The inclusion of control variables has virtually no impact on our estimates, because these covariates are well-balanced across state lines. These results are also unchanged if we include a control variable indicating which states had a competitive gubernatorial or senatorial race in 2012.[13] Additional regressions controlling for different election laws and voting methods across states also produce nearly identical results.[14] The sample sizes vary across the three different estimation strategies because we only include media markets for which there is variation in the treatment variable. For example, the regressions associated with Equation 1 (Columns 1-3) only include media markets that span both a battleground and non-battleground state. This includes approximately 50 million individuals living in 38 media markets spanning 34 states. For Equation 2, we include all individuals living in media markets which span states in different tiers (88 million individuals in 49 media markets spanning 44 states). For Equation 3, we include all individuals living in media markets which span states receiving different levels of effort from the Romney campaign (119 million individuals in 74 media markets spanning 48 states—Washington, DC is included as a state).

Conditional on the media market and other factors, voters in battleground states were 4 percentage points more likely to turn out, suggesting that ground campaigning increased participation in battleground states by 4 percentage points, on average. Comparing the top tier of campaign priority to non-targeted states, we see that turnout was 6-7 percentage points higher in the more targeted regions. This difference is 2-4 percentage points for the second tier of states and 1-4

[13] We code competitive Senate or gubernatorial races as those classified beforehand as "toss up" races by Real Clear Politics. These races took place in Indiana, Massachusetts, Montana, Nevada, North Dakota, Virginia, Wisconsin, and Washington.

[14] Data on voting methods was collected from the National Council on State Legislatures. We code dummy variables corresponding to their categorizations (no-excuse absentee voting, early voting, early voting and no-excuse absentee voting, all-mail voting, and no early voting and excuse required for absentee).

percentage points for the third tier. As expected, for each specification, the estimated effect of Tier 1 residence is greater than the estimated effect of Tier 2 residence, which is greater than the estimated effect of Tier 3 residence, which is greater than zero. Similarly, in the last three columns, going from states with no campaign effort to those with maximal campaign effort corresponds to a 7-8 percentage point increase in turnout. With the exception of the Tier 3 estimates, all point estimates are substantively meaningful and statistically significant ($p < .01$ with standard errors clustered by state). Our aggregate results are consistent with large, powerful mobilizing effects of the presidential campaigns that increase with the extent to which a state was targeted. Voter mobilization appears to have significantly increased turnout in the most targeted states.

Figure 2 offers an approximate visualization of the effects of campaigning, based on the regression result in Column 7 of Table 1. Each data point represents a pair of states that share a media market. For example, the intersection of Massachusetts and New Hampshire in the Boston media market represents one data point in the figure. The difference in voter turnout between the two regions is plotted on the vertical axis, with higher values indicating higher turnout among the region that received more campaign contact. The difference in campaign effort between these two regions, as indicated by the *RomneyEffort* variable, is plotted on the horizontal axis. Consistent with the regression result, we see that, on average, the turnout differential increases significantly as the campaign effort differential increases. The solid line represents a linear fit, and the dashed curve represents a nonparametric kernel regression. In both cases, each observation is weighted by the population of the smaller region (represented visually by the size of the circle). This weighting approach appropriately mitigates the influence of the data points with very few observations.[15] The

[15] For example, the extreme observation with a turnout differential of −20 percentage points represents the intersection of Michigan and Minnesota in the Duluth-Superior media market, where the Michigan region has only 17,000 residents. Some regions are tiny—e.g., Nebraska's overlap with the Rapid City market has only 780 residents, while others are huge—e.g., the Philadelphia market covers 4.7 million people in Pennsylvania and 2.1 million people in New Jersey.

slope of the line, .071, is nearly identical to the regression results in Columns 7-9 of Table 1. Moreover, the relationship between the effort differentials and the turnout differentials are approximately linear, suggesting that the marginal returns to additional campaigning do not diminish over the range of contacts made by the campaigns. If these effects did diminish, then we would see the turnout differentials level-off at a certain point. However, the effects of campaigning appear to increase linearly in proportion to its intensity, helping to explain the large cumulative effects that we detect.

Large-scale campaigns using modern GOTV tactics appear to significantly increase electoral participation. Our results suggest that voter turnout would have been 7-8 percentage points lower in heavily targeted states like Nevada, New Hampshire, and Ohio had the presidential campaigns and interest groups not deployed their mobilizing activities. Even though a single campaign contact typically has a negligible effect on overall rates of participation, the widespread deployment of many such interventions appears to add up to a significant change in participation. The regression results in Table 1 allow us to approximate the total mobilizing effect of the campaign in 2012. If we multiply the coefficient in Column 7 by the level of *RomneyEffort* in a state and the voting-eligible population in a state, we obtain an approximation of the number of people mobilized. Repeating this for every state and computing the sum, we estimate that approximately 2.6 million individuals turned out to vote in 2012 who would have otherwise abstained in the absence of campaigning.

Variation across Individual Partisanship

Our data allows us to explore the extent to which our aggregate estimates of campaign effects vary across different kinds of voters. Specifically, because partisan campaigns explicitly target partisans who are likely to support their candidate if they vote, we would expect to obtain greater effects among partisans—or those predicted to be partisans based on their observable covariates—

than among moderates. Our interviews with operatives in both campaigns confirmed that targeting partisans was, in fact, their strategy. The most targeted individuals were strong partisans or those who look that way to the campaign based on their public records and demographics. For example, the Obama campaign generated an "Obama Support Score" using demographic and consumer data (similar to our data) in addition to their own contacts and focused their GOTV efforts on individuals with high scores (Nickerson and Rogers 2014).

We do not know precisely which citizens were targeted by the presidential campaigns. And even if we knew which individuals in battleground states were contacted, we wouldn't know which individuals in non-battleground states would have been contacted had they lived in a battleground states. Therefore, we generate our own measure of predicted partisanship, using our data from public and consumer records. Because vote choices are not public, the only direct indication of partisan attitudes is party registration, which is only available in some states. Moreover, we do not want to use party registration directly in our analysis, because campaigns work to register supporters in targeted states, making party registrants no longer comparable across state boundaries. And, of course, party registration is not present in all states. Therefore, we predict party registration using race, gender, and income, using data from the non-battleground states where party registration is present. We code a party registration variable which takes a value of 1 for registered Democrats, 0 for registered Republicans, and .5 for all other individuals. Then, we calculate the average level of this variable for every possible combination of race, gender, and income in non-battleground states with party registration. This score represents our predicted levels of partisan support (with higher values indicating more Democratic and less Republican support), which we impute for the entire sample. The variable ranges from .09 to .84, with a mean of .51 and a standard deviation of .13.

In order to assess the variation in our estimated effects across partisanship, we divide our predicted partisanship variable into 6 categories and re-estimate the regression from Column 7 of

Table 1 for each category. These estimates represent the effect of campaigning on turnout for voters at different levels of partisanship. Figure 3 presents the point estimates and 95% confidence intervals graphically. As expected, we see large estimates of mobilization for those voters predicted to be strongly partisan, and we obtain significantly smaller estimates for less partisan subgroups. Our estimated effects of campaign mobilization are 11.3 and 10.8 percentage points for our most Republican and Democratic subgroups, respectively the left- and right-most categories in Figure 3, while our estimates of mobilization are only 5.6 and 6.9 percentage points for the two least-partisan categories in the middle of Figure 3. We see the largest effects of campaigning among the most heavily targeted subgroups. This finding provides additional credibility to our results and partially informs the mechanisms behind the aggregate effects of campaigning, which we discuss later in the paper.

The estimates of aggregate voter mobilization from the previous section are averages across all eligible voters, and many individuals in targeted states likely received little direct contact from campaigns. In Figure 3, when we focus on smaller subsets of individuals that were more likely to have been contacted, we obtain larger estimates. Among predicted partisans, the large-scale interventions of the presidential campaigns appear to have increased turnout by more than 10 percentage points. Moreover, recall that our predictions of partisanship are imperfect. Presumably, if we had better information about the types of individuals targeted, we would estimate even greater mobilization effects among the most targeted groups, and we would expect starker differences in mobilization effects across subgroups.

One notable implication of these results is that campaigns can significantly change the composition of the voting population. By targeting partisan supporters, campaigns appear to produce a more partisan, polarized set of voters. Not only does this potentially distort the incentives of Presidential candidates to appeal to more partisan voters, but this also changes the set of

individuals participating in down-ballot races, potentially affecting Congressional and legislative polarization. Consistent with previous research (Enos, Fowler, and Vavreck 2014), these results suggest that voter mobilization and campaigning does not necessarily lead to a more representative set of voters. Furthermore, as mobilization techniques are more widely adopted,[16] we might expect the polarization of the voting population to increase further.

As a side note, this analysis allows us to roughly compare the effectiveness of the Obama and Romney campaigns in mobilizing their respective supporters. Despite the purported technological sophistication of the Obama campaign and its intense use of data-driven, evidence-based campaign tactics (e.g., Issenberg 2013), we see similar mobilization effects on both sides of Figure 3. The two campaigns were roughly comparable in their ability to turn out supporters. Moreover, this result is not an artifact of using the *RomneyEffort* variable; we obtain similar results with the other measures of campaign effort. Of course, this analysis does not preclude the possibility that the Obama campaign exceeded Romney in other areas that are more difficult to measure like persuasion or targeting. However, on the dimension of mobilizing people who look demographically like supporters, both campaigns appear to have been comparably effective.[17] One interpretation of the Romney campaign's slight advantage with their own partisans is that Democrats are simply more difficult to mobilize than Republicans. Indeed, previous research suggests that, on average, GOTV interventions are more effective for conservative and high-socioeconomic-status citizens (Enos, Fowler, Vavreck 2014). The amount of effort and resources needed to mobilize Democratic supporters may be greater than that needed to mobilize Republican supporters. With this in mind,

[16] See, for example, Steve Friess, "Ahead of the Midterms, GOP Operatives Are Obsessively Studying a Book about the Obama Campaign", *New York Magazine*, September 12, 2013.

[17] We take this as one data point suggesting that the accounts of the Obama campaign's effectiveness and the big data revolution in campaigning may be overstated. One potential explanation for this overstatement is that political reporters are guilty of a attribution error, allowing the result of the election—influenced by many external factors—to shape their assessments of the campaigns.

even if the Obama campaign was more advanced than the Romney campaign, this difference was not great enough to overcome this structural disadvantage.

Has Campaign Effectiveness Changed over Time?

Previous studies have identified little difference in turnout between battleground and non-battleground states (e.g., Ashworth and Clinton 2007; Gerber et al. 2009), yet we detect large differences for 2012. What might explain the discrepancy between our results and previous studies? One possibility is campaign effects have grown in recent years as a result of changes in technology, campaign strategy, or some other factor. Recent trends in campaign effects may also help to identify the most likely explanations for our aggregate results. However, we cannot use our same analytic strategy to investigate this possibility because our data is less accurate in past elections and we have less information on the campaign strategies for previous elections. As an alternative, we compare potentially pivotal and non-pivotal states in several recent presidential elections to ascertain whether campaign effectiveness appears to have changed over time.

For every presidential election between 1980 and 2012,[18] we determine which 5 states were most likely to be pivotal. We sort the states according to the their two-party vote share, and determine which state would have tipped the electoral college had one candidate won every state below it and had the other candidate won every state above it. We then select the two additional states on each side of this pivotal state to obtain our 5 pivotal states where much of the campaign effort was likely concentrated.[19] The top panel of Figure 4 plots the average difference in voter turnout between these pivotal states and all other states for each election, along with the

[18] Reliable estimates of the voting-eligible population (VEP) in each state are only available since 1980, so we focus on this recent period. Using voting-age population (VAP) as the denominator, we can extend the period of analysis, and the estimated trends are unchanged.

[19] Alternate codings, such as identifying the states categorized as "swing states" by news organizations, produce similar results.

corresponding 95% confidence intervals. Consistent with previous findings (e.g., Ashworth and Clinton 2007; Gerber et al. 2009), the differences in turnout between battleground and non-battleground states were small before 2008—hovering between 1.7 and 5.8 percentage points. Then, this difference shot up to 9.1 percentage points in 2008 and remained at 6.9 percentage points in 2012.

The bottom panel of Figure 4 presents a similar and arguably more credible analysis that draws inferences within states instead of between states. We run a differences-in-differences regression, taking advantage of states that switch in and out of the pivotal category, and estimate the extent to which within-state changes in battleground status correspond to within-state changes in turnout for each year.[20] These within state estimates allow us to approximate the effect of being a battleground state, and by proxy, the effect of campaigning on voter turnout in each election. Here, these effects are indistinguishable from zero between 1980 and 2000 but increase monotonically in each of the next three elections, with an estimated effect of 6.1 percentage points in 2012. Both the between- and within-state approaches suggest that the effect of presidential campaigning on voter turnout has increased significantly in recent elections.

What explains the apparent increase in the effectiveness of campaign mobilization? Other interpretations are possible, but in our view, the most likely explanation is that the ground game has become a much larger and more important part of presidential campaigns. The rebirth of GOTV field experiments in the 21st century (e.g., Green and Gerber 2008) presented hard evidence that individual voter contact such as door-to-door canvassing, phone calls, and direct mail can increase turnout. Then, several major policy and technological changes increased the availability of individual-level data, enabling campaigns to more easily identify, contact, and keep track of

[20] Specifically, we regress turnout on year dummy variables, interactions between the year dummies and the indicator for a battleground state, and state fixed effects: $\text{Turnout}_{it} = \beta_1\text{Pivotal1980} + \beta_2\text{Pivotal1984} + \ldots + \delta_t$ (year fixed effects) $+ \gamma_i$ (state fixed effects) $+ \varepsilon_{it}$. Figure 4 shows the interactive coefficients.

supporters (Hersh 2015, pp. 58-65). With these revolutions in both evidence and data availability, the sheer scale of ground campaigning increased dramatically in the last few presidential elections (Issenberg 2012, 2013, Enos and Hersh 2015). Because personal voter contact is known to influence participation, and because voter contact increased dramatically at the same time as our estimates of campaign effectiveness increased, we suspect that ground campaigning is the primary mechanism behind our aggregate effects. We return to a more detailed discussion of mechanisms in a subsequent section.

Can these Effects be Explained by Non-Campaign Factors?

We find that battleground residents are more likely to turn out than non-battleground residents of the same media market. We have already attempted to rule out the possibility that spurious factors correlated with battleground states bias our results. Placebo tests show that battleground and non-battleground residents of the same media market are comparable in terms of demographics, turnout in a non-presidential year, and their underlying interest in politics or voting. We also show that the results are unchanged by the inclusion of individual- and state-level covariates including the presence of other salient elections in the state and different registration and voting laws.

We have also discussed the possibility that residence in a battleground state influences turnout for reasons unrelated to campaigning. On one hand, an individual's probability of casting a pivotal vote is higher in swing states, which could increase the direct returns to voting and make one more likely to turn out (Riker and Ordeshook 1968). On the other hand, the probability of casting a pivotal vote is miniscule in both battleground and non-battleground states (Gelman, King, and Boscardin 1998). As Schwartz (1987) aptly points out, "Saying that closeness increases the

probability of being pivotal . . . is like saying that tall men are more likely than short men to bump their heads on the moon."

Despite the tiny probability of a pivotal vote in a presidential election, there are other reasons to expect higher turnout in battleground states even in the absence of campaigns. A combination of uncertainty and altruism could provide a rational basis for turnout and could explain higher participation in battleground states (Myatt 2012). Perhaps individuals in battleground states overestimate their probability of casting a pivotal vote, are more interested in the election for psychological reasons, or exert more social pressure on others for reasons independent of the campaign. We must acknowledge these possibilities, but we believe them to be minimal if not negligible for four reasons. (1) Our placebo tests show that battleground voters were no more likely to be interested in politics or express an intention to vote before the presidential campaign began. (2) The most recent evidence on the effects of pivotality, coming from experiments, suggests that considerations of pivotality have minimal effects on turnout (Enos and Fowler 2014; Hoffman, Morgan, and Raymond 2013). (3) If non-campaign factors explain our results, we would not necessarily expect to see the greatest effects among those individuals who were more likely to have been targeted, as in Figure 3. (4) Similarly, we would also not expect to see our estimated effect of campaigning increase dramatically in recent elections, as in Figure 4. Presumably, considerations of pivotally and other non-campaign factors were just as prevalent in previous elections, yet we only see these effects emerge in recent elections as campaign behavior has changed significantly.

How Do Campaigns Mobilize Voters?

For the reasons discussed in the previous section, we believe that we can reasonably attribute our estimated effects to campaigning as opposed to other spurious factors and the non-campaign incentives to vote. But what specific campaign-related activities can explain our aggregate estimates?

Here, we explore several potential explanations and present suggestive evidence that the most important factor is ground campaigning—i.e., door-to-door canvassing, phone calls, and direct mail. Of course, identifying mechanisms is difficult and often requires strong assumptions (Bullock, Green, and Ha 2010), so we should be cautious in our claims about particular channels. However, the particular mechanisms at work in this context are substantively important, and therefore worth exploring.

Part of our estimated campaign effects could come from the mobilizing effects of television and mass media. Our design rules out the channel by which more news coverage and television advertising, in general, increases turnout, because all of our comparisons are within media markets where all residents receive the same television news coverage and campaign advertisements. This leaves ground campaign activity, the activity that varied across states, within media markets, as the most likely source of variation in turnout across states. Nonetheless, we might worry that the effects of television advertisements and news coverage are somehow greater in battleground states. Perhaps television ads or news coverage only mobilize voters who know they are in a competitive state. We believe these possibilities hold negligible implications for our estimates for several reasons. First, previous evidence from Krasno and Green (2008) suggests that television advertising and battleground media coverage have little effect on turnout, and in the Appendix, we replicate this finding for 2012.[21] Second, in the Appendix, we present an additional research design that allows us to estimate the effect of television advertising specifically in battleground states, and again, we find no evidence of an effect. For both of these analyses, we combine our individual-level data with data on the extent of television advertising in each media market, and we exploit the idiosyncratic boundaries of media markets in order to obtain plausibly exogenous variation in advertising. Third,

[21] The finding that television advertising has little effect on average turnout does not preclude the possibility that television ads persuade voters. In fact, the explicit goal of television ads among campaigns and political consultants is often persuasion rather than mobilization.

only a small fraction of Presidential advertisements—about 5 percent—contained state specific content.[22] Fourth, we see little difference between battleground and non-battleground states in previous elections when television advertising was even more prevalent in political campaigns and when television news viewership was even greater. Therefore, there is little reason to believe that advertising would systematically be more salient to viewers in battleground states. One remaining possibility is that television advertising and mass media interact with other aspects of campaigning, e.g., voter mobilization efforts, thereby exacerbating the aggregate effects of other campaign activities. We cannot rule out this possibility, but our inability to detect an average effect of television on participation suggests that these interactive effects are likely small.

Another potential driver of our estimated campaign effects is digital and internet advertising, which can be targeted at fine levels of geography. *A priori*, we expect the relative effectiveness of digital campaigning vs. other efforts such as traditional voter contact to be small for several reasons. First, the sheer scale of internet advertising was relatively small. We communicated with the digital teams of the Obama and Romney campaigns and each team spent less than 10 million dollars on registration and GOTV.[23] This amount pales in comparison to more traditional forms of mobilization, where the efforts of each campaign exceeded 100 million dollars. Second, because television advertisements do not significantly increase turnout (e.g., Krasno and Green 2008), we might expect similarly small effects for internet advertising which is similar to television advertising but smaller in scale.[24] Third, the few experiments related to internet campaigning show small effect sizes. E-mails have zero (Nickerson 2007a, 2007b) or negligible (Malhotra, Michelson, and Valenzuela 2012) effects on registration and turnout. An extremely salient treatment utilizing social

[22] We collected data on all of Obama and Romney's televised ads from Stanford's Political Communication Lab, and we searched for state-specific references. Only 19 of 360 ads contained references to a specific state.

[23] Personal communications with Romney and Obama staff, January-February 2014.

[24] Both campaigns relied on video advertisements for their internet advertising, which is a medium very similar to that of television.

pressure conducted through Facebook on Election Day in 2010 increased turnout by only 0.3 percentage points (Bond et al. 2012). The only experiment, to our knowledge, directly assessing political internet advertisements finds no effect on candidate evaluations (Broockman and Green 2014).

Furthermore, our data allow us to partially separate the effects internet advertising and other campaign activity, and we estimate that the effect of internet advertising is minimal. We obtained data from the Obama campaign on their digital campaigning activities in each state, and we can include this measure of digital campaign effort in the same regressions shown in Table 1. In these regressions, discussed and presented in more detail in the Appendix, the coefficients on overall campaign effort are virtually unchanged while the coefficients on digital campaigning are actually slightly negative. Therefore, we conclude that our estimated effect of campaigning cannot be meaningfully attributed to digital or internet advertising.

By ruling out the most plausible alternatives, we conclude that the most prevalent mechanism explaining our estimates is traditional ground campaigning such as door-to-door canvassing, phone calls, and direct mail. The scale of this kind of voter contact increased dramatically at the same time that the differences between turnout in battleground and non-battleground states increased. The effectiveness of this form of campaign activity has received the strongest empirical support from previous research (see Green and Gerber 2008), and its deployment and reach is significantly greater than newer forms of campaign activity such as digital advertising. We conclude that aggregate estimates of campaign effects likely arise primarily through the personal forms of voter communication that have received experimental support and have recently become a significant component of large-scale political campaigns.

What are the Marginal Returns to Mobilization Efforts?

A natural question, after seeing many estimates of small effects of individual voter contacts is whether many voter contacts aggregate up to something meaningful in a large-scale election. For example, a typical get-out-the-vote experiment compares the effect of a single voter contact (e.g., an attempted phone call) to nothing. As mentioned previously, the average effect of a single voter contact is typically small. Across more than 200 experiments analyzed by Green, McGrath, and Aronow (2013), the average effect of a single door knock, phone call, or piece of mail is 1.0, 0.7, or 0.4 percentage points, respectively. In isolation, these effects appear small, but do they add up to something meaningful in a large-scale campaign deploying many voter contacts at once?

Previous evidence provides little guidance on the aggregate returns to many voter contacts. Few experimental studies have randomly assigned subjects to receive multiple voter contacts (but see Cardy 2005; Gerber and Green 2000; Michelson, Bedolla, and Green 2007; Ramirez 2005), and these studies are often underpowered and still fail to come anywhere close to the level of voter contact in a large-scale campaign. If the returns to campaign activity diminish quickly, then the effects of a large-scale campaign might be no bigger than the small GOTV effects identified in experiments. However, if multiple campaign efforts are additive or even synergistic, then the effects of a large-scale campaign could be massive. On one hand, there must be a point at which the returns to campaign activity diminish, because turnout among a targeted population can never exceed 100 percent. However, up to that ceiling, we have little theory or evidence suggesting that the effects of additional campaigning should diminish quickly, slowly, or not at all.

Our study provides several opportunities to assess the marginal returns to campaign activity. First, our large aggregate estimates suggest that multiple voter contacts do add up to something meaningful and do not diminish too quickly. Second, Figure 2 shows that the effect of campaign activity on turnout increases approximately linearly with the difference in campaign effort. In other

words, we find no evidence that the effect of campaign effort diminishes with increasing effort. Furthermore, if we are willing to assume that all of our estimated effects are explained by ground campaigning, an admittedly strong assumption, then we can conduct several back-of-the envelope calculations to further assess the aggregate effects of multiple contacts.

According to our data, the Romney campaign placed 35.8 million phone calls, sent 3.4 million pieces of mail, and knocked on 2.8 million doors in the state of Ohio alone. The voting-eligible population of Ohio was approximately 8.6 million, so this means that the Romney campaign placed 4.2 phone calls, sent 0.4 pieces of mail, and knocked on 0.3 doors per person. These figures are similar for other heavily targeted states. Nevada—the state with the greatest level of effort—saw 5 phone calls, 0.7 pieces of mail, and 0.5 door knocks from the Romney campaign for every eligible voter. Qualitative reports suggest that the Obama campaign exerted similar mobilization efforts, focusing more on door knocks and less on phone calls.[25] Therefore, we approximate that the average eligible voter in a heavily targeted state received 7-10 phone calls, 1-2 pieces of mail, and 1-2 door knocks from the presidential campaigns. Assuming that our estimated effects are driven solely by ground campaigning, we would conclude that a treatment including approximately 7-10 phone calls, 1-2 pieces of mail, and 1-2 door knocks increases turnout, on average, by 7-8 percentage points. This is remarkably consistent with the hypothesis that treatment effects are additive. Using the average estimates from Green, McGrath, and Aronow (2013),[26] our point estimates are close to the sum of the average intent-to-treat effects of 10 phone calls, 2 pieces of mail, and 2 door knocks (100.4 + 20.7 + 21.0 = 7.4). If the returns to ground campaigning diminish over the range of observed contacts, we would have expected much lower aggregate estimates than those that we do obtain. This result may be surprising to those who expect diminishing returns to multiple voter contacts, but

[25] Personal communication with high-level Obama campaign staff, May to July 2013.

[26] We believe these figures, obtained from over 200 field experiments, represent the best available estimates of these average effects.

there are compelling reasons to expect additive or even multiplicative effects, at least up to a point. Only a small fraction of citizens will answer the door, pick up the phone, or read their mail, and each new campaign effort may reach a new subset of citizens. Moreover, some citizens that are unresponsive to single treatments may be eventually mobilized by multiple treatments.

Another calculation yields a similar conclusion. We previously estimated that presidential campaigning mobilized 2.6 million voters who would not have turned out in the absence of campaigning. Our data from the Romney campaign indicates that they attempted 225 million voter contacts. If we assume that the Obama campaign attempted a similar number, then we could say that 550 million voter contacts produced approximately 2.6 million votes. Again, assuming that all of our estimated effects arise through voter contact, we would conclude that the average effect of a single voter contact on turnout was 0.6 percentage points ($2.6/550 \approx .0058$), a number that is strikingly consistent with the average intent-to-treat effect identified through experiments. Although we cannot be sure that our estimated effects are not explained by something other than individual voter contact, these analyses suggest that multiple voter contacts can have large aggregate effects, and we find no evidence that the returns to campaigning diminish quickly.

If we are willing to assume that our estimated effects are explained by ground campaigning and that the effects of voter contacts are approximately additive, then we can rescale our previously estimated regression coefficients in order to interpret the marginal returns to dollars spent on the ground camapaign. The estimated coefficient of .078 from Column 7 of Table 1 suggests that the effect of going from no campaign effort to the campaign effort in the most heavily targeted state increases turnout in that state by 7.8 percentage points. In the most heavily targeted state—Nevada, the Romney campaign spent the equivalent of $3.40 per person in their ground mobilization efforts. If we assume that the Obama campaign exerted similar effort, then we can divide this coefficient by 6.8 and conclude that, on average, an extra dollar of campaign effort per capita increases turnout by

1.1 percentage points. Alternatively, this estimate implies that the average cost of generating a single vote is about 87 dollars.

Conclusion

Campaigns can influence the size and shape of the voting population, and the advent of the modern, scientifically-driven campaign may increase the depth and scope of this influence. However, rigorous evidence has been lacking. Experiments show that individual voter contacts increase turnout, but the substantive size of these effects is small. To our knowledge, the 2012 presidential campaign offers the best available opportunity to assess the effects of modern, large-scale campaigns, because these methods were deployed at a larger scale than ever before, because these efforts varied idiosyncratically across states, and because we have unprecedented information about the activities of the presidential campaigns. According to our estimates, the 2012 presidential campaign increased average levels of turnout by approximately 7 percentage points in the most heavily targeted states, mobilizing 2.6 million individuals who would have otherwise not turned out. In short, large-scale campaigns can significantly increase political participation. Furthermore, our graphical analysis in Figure 2 and back-of-the-envelope calculations suggest that the effects of many mobilization efforts may be approximately additive.

In this paper, we have offered the first systematic assessment of the cumulative mobilization effect of a large-scale campaign. Contrary to some expectations, large-scale campaigns can significantly increase the size and composition of the voting population, rather than simply mobilizing a small fraction of voters on the margin. This phenomenon, in conjunction with recent increases in the use of ground campaigning, marks a significant change in the American electoral landscape, with millions of otherwise nonparticipating voters going to the polls. These findings may

also lend insights for increasing participation in general, as the returns to multiple campaign efforts may be greater than previously expected.

References

Ansolabehere, Stephen and Eitan Hersh. 2012. Validation: What Big Data Reveal about Survey Misreporting and the Real Electorate. *Political Analysis* 20(4):437-459.

Ashworth, Scott and Joshua D. Clinton. 2007. Does Advertising Exposure Affect Turnout? *Quarterly Journal of Political Science* 2(1):27-41.

Bond, Robert M., Christopher J. Fariss, Jason J. Jones, Adam D. I. Kramer, Cameron Marlow, Jaime E. Settle, and James H. Fowler. 2012. A 61-Million-Person Experiment in Social Influence and Political Mobilization. *Nature* 489:295-298.

Broockman, David E. and Donald P. Green. 2014. Do Online Advertisements Increase Political Candidates' Name Recognition or Favorability? Evidence from Randomized Field Experiments. *Political Behavior* 36(2):263-289.

Bullock, John G., Donald P. Green, and Shang E. Ha. 2010. Yes, But What's the Mechanism? (Don't Expect an Easy Answer). *Journal of Personality and Social Psychology* 98(4):550-558.

Enos, Ryan D. and Anthony Fowler. 2014. Pivotality and Turnout: Evidence from a Field Experiment in the Aftermath of a Tied Election. *Political Science Research and Methods* 2(2):309-319.

Enos, Ryan D., Anthony Fowler, and Lynn Vavreck. 2014. Increasing Inequality: The Effect of GOTV Mobilization on the Composition of the Electorate. *Journal of Politics* 76(1):273-288.

Enos, Ryan D. and Eitan Hersh. 2015. Party Activists as Campaign Advertisers: The Ground Campaign as a Principal-Agent Problem. *American Political Science Review*, forthcoming.

Cardy, Emily Arthur. 2005. An Experimental Field Study of the GOTV and Persuasion Effects of Partisan Direct Mail and Phone Calls. *The Annals of the American Academy of Political and Social Science* 601:28-40.

Gelman, Andrew, Gary King, and W. John Boscardin. 1998. Estimating the Probability of Events That Have Never Occurred: When Is Your Vote Decisive? *Journal of the American Statistical Association* 93(441):1-9.

Gerber, Alan S., Donald P. Green, and Christopher W. Larimer. 2008. Social Pressure and Voter Turnout: Evidence from a Large-Scale Field Experiment. *American Political Science Review* 102(1):33-48.

Gerber, Alan S., Gregory A. Huber, David Doherty, Conor M. Dowling, Connor Raso, and Shang E. Ha. 2011. Personality Traits and Participation in Political Processes. *Journal of Politics* 73(3):692-706.

Gerber, Alan S., Gregory A. Huber, Conor M. Dowling, David Doherty, and Nicole Schwartzberg. 2009. Using Battleground States as a Natural Experiment to Test Theories of Voting. APSA 2009 Toronto Meeting Paper.

Green, Donald P. and Alan S. Gerber. 2008. *Get Out the Vote: How to Increase Voter Turnout*. Brookings Press.

Green, Donald P., Mary C. McGrath, and Peter M. Aronow. 2013. Field Experiments and the Study of Voter Turnout. *Journal of Elections, Public Opinion, and Parties* 23(1):27-48.

Hersh, Eitan. 2013. Long-term Effect of September 11 on the Political Behavior of Victims' Families and Neighbors. *Proceedings of the National Academy of Sciences* 110(52):20959-20963.

Hersh, Eitan. 2015. *Hacking the Electorate: How Campaigns Perceive Voters*. Cambridge University Press.

Hoffman, Mitchell, John Morgan, and Collin Raymond. 2013. One in a Million: A Field Experiment on Belief Formation and Pivotal Voting. Working paper.

Huber, Gregory A. and Kevin Arceneaux. 2007. Identifying the Persuasive Effects of Presidential Advertising. *American Journal of Political Science* 51(4):957-977.

Issenberg, Sasha. 2013. A More Perfect Union: How President Obama's Campaign Used Big Data to Rally Individual Voters. *MIT Technology Review* 116(1):41-51.

Kim, Jae-On, John Petrocik, and Stephen Enokson. 1975. Voter Turnout among the American States: Systematic and Individual Components. *American Political Science Review 69*(1):107-123.

Krasno, Jonathan S. and Donald P. Green. 2008. Do Televised Presidential Ads Increase Voter Turnout? Evidence from a Natural Experiment. *Journal of Politics* 70(1):245-261.

Malhotra, Neil, Melissa R. Michelson, and Ali Adam Valenzuela. 2012. Emails from Official Sources Can Increase Turnout. *Quarterly Journal of Political Science* 7(3):321-332.

Michelson, Melissa R., Lisa Garcia Bedolla, and Donald P. Green. 2007. New Experiments in Minority Voter Mobilization. Report for The James Irvine Foundation.

Myatt, David P. 2012. A Rational Choice Theory of Voter Turnout. Working paper.

Nickerson, David W. 2007a. Does Email Boost Turnout? *Quarterly Journal of Political Science* 2(4):369-379.

Nickerson, David W. 2007b. The Ineffectiveness of E-Vites to Democracy: Field Experiments Testing the Role of E-mail on Voter Turnout. *Social Science Computer Review* 25(4):494-503.

Nickerson, David W. and Todd Rogers. 2014. Political Campaigns and Big Data. *Journal of Economic Perspectives* 28(2):51-74.

Ramirez, Ricardo. 2005. Giving Voice to Latino Voters: A Field Experiment on the Effectiveness of a National Nonpartisan Mobilization Effort. *The Annals of the American Academy of Political and Social Science* 601:66-84.

Riker, William H. and Peter C. Ordeshook. 1968. A Theory of the Calculus of Voting. *American Political Science Review* 62(1):25-42.

Schwartz, Thomas. 1987. Your Vote Counts on Account of the Way It Is Counted: An Institutional Solution to the Paradox of Voting. *Public Choice* 54(2):101-121.

Sides, John and Lynn Vavreck. 2013. *The Gamble: Choice and Chance in the 2012 Presidential Election*. Princeton University Press.

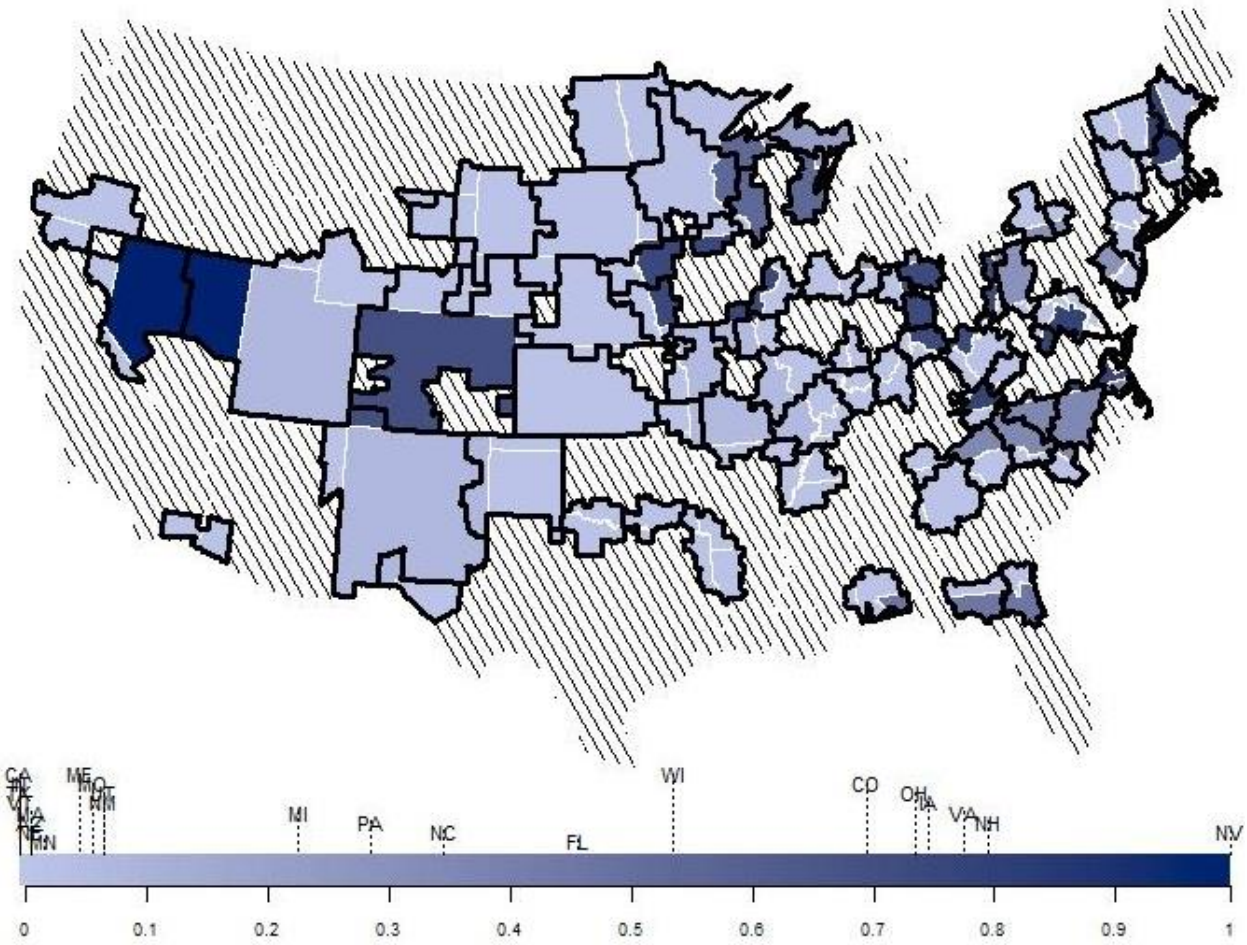
Verba, Sidney, Kay Lehman Schlozman, and Henry E. Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics*. Harvard University Press.

Table 1. Average Effects of Ground Campaigning on 2012 Voter Turnout

	DV = 2012 Voter Turnout								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Battleground	.042	.041	.041						
	(.011)	(.010)	(.010)						
Tier 1				.067	.063	.056			
				(.006)	(.006)	(.012)			
Tier 2				.039	.043	.023			
				(.006)	(.005)	(.007)			
Tier 3				.035	.025	.011			
				(.016)	(.017)	(.010)			
Romney Effort							.078	.078	.068
							(.012)	(.012)	(.014)
Media Market Fixed Effects	X	X	X	X	X	X	X	X	X
Individual Controls		X	X		X	X		X	X
Control for 2010 Turnout			X			X			X
N individuals		49,549,516			87,650,509			118,645,707	
N media markets		38			49			74	
N states		34			44			48	
R-squared	.020	.101	.624	.014	.102	.621	.019	.104	.624

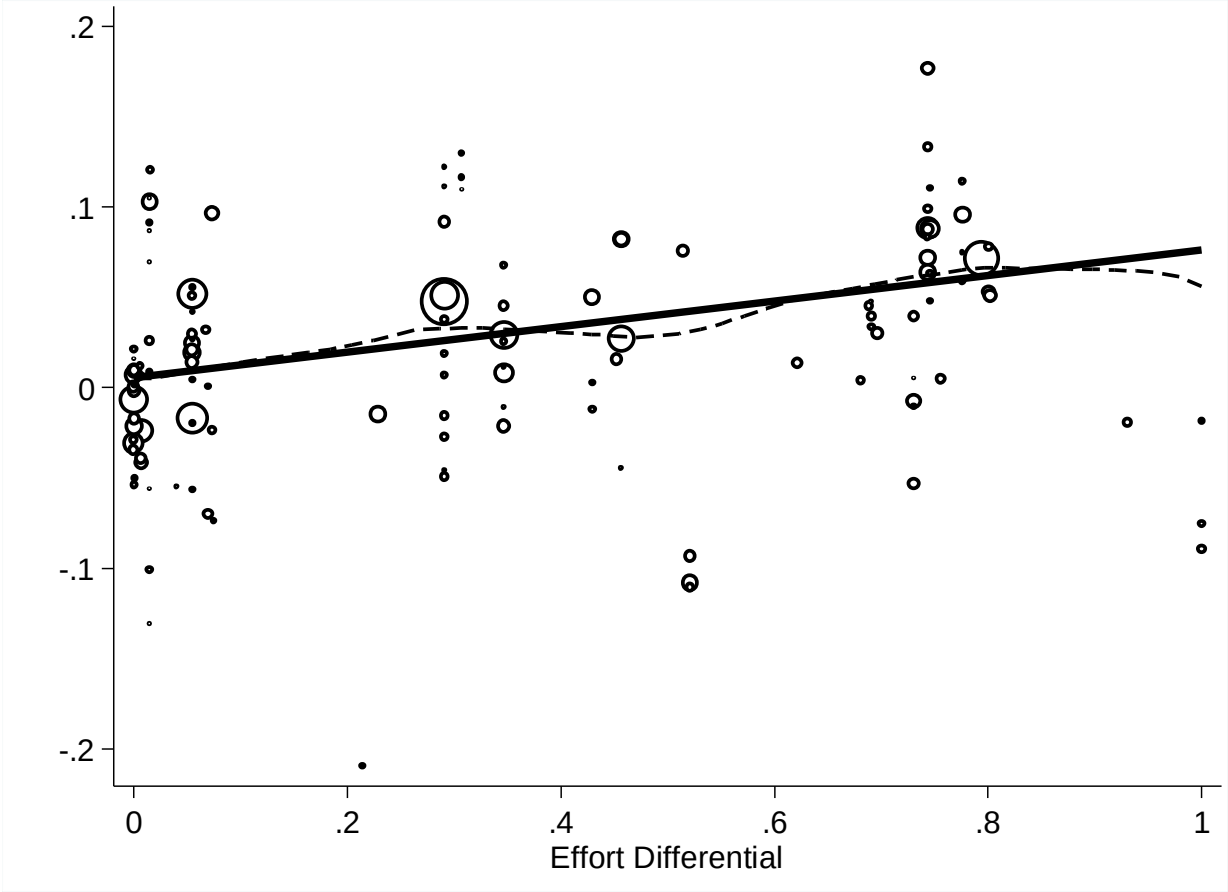
Battleground is an indicator variable for battleground territory where the campaigns made significant efforts to mobilize voters. Tier 1, Tier 2, and Tier 3 are indicator variables for the three categories of states that received different levels of campaign priority from the Obama campaign. Romney Effort is a continuous variable ranging from 0 to 1 indicating the level of mobilization efforts employed by the Romney campaign in each state. Media Market Fixed effects allow for all of the inferences to be drawn within media markets but across state or district boundaries. Specifications with individual controls include dummies for gender, race, and predicted income category. The first two regressions include all individuals living in media markets that span both battleground and non-battleground territory. The next two regressions include all individuals living in media markets that span territories at different tiers. The final two regressions include all individuals living in media markets that span territories receiving differential mobilization effort from the Romney campaign. Conditional on media market and other factors, individuals living in battleground territory were 4 percentage points more likely to turn out to vote. Those living in the first tier of states were 6 to 7 percentage points more likely to turn out, relative to those in non-targeted states. In the second tier, this difference is 2 to 4 percentage points, and in the third tier this difference ranges from 1 to 4 percentage points. Those in the states where the Romney campaign exerted the greatest effort were 7 to 8 percentage points more likely to vote than those where Romney exerted no effort. All of these results are consistent with powerful mobilizing effects of campaign efforts. Standard errors, clustered by state, are in parentheses.

Figure 1: *RomneyEffort* by State and Media Market



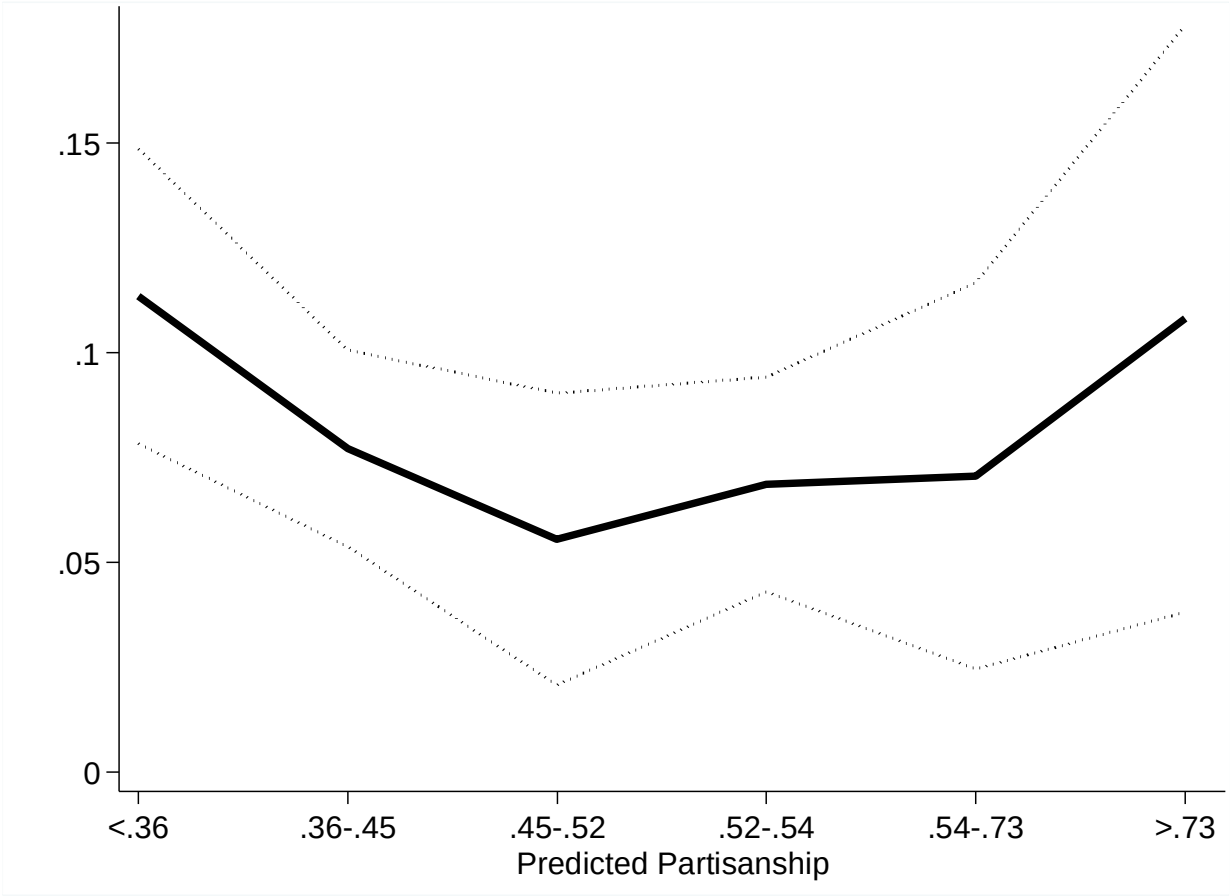
Media markets spanning states with different levels of RomneyEffort are surrounded by thick black lines. Portions of each media market are shaded by the level of Romney effort in that state. So, for example, the Reno media market is much darker in Nevada than in California, representing the variation in RomneyEffort across those states. States with no effort or only minimal effort are the lightest shade. These are states, for example Utah, where the campaign made some contact but nowhere near the levels in battleground states. The media markets that do not span states with varying RomneyEffort (not included in our analysis) are indicated by diagonal lines. The states are also aligned horizontally along the bottom of the figure by level of RomneyEffort (vertical alignments are varied for visual ease).

Figure 2. Average Effects of Voter Mobilization in 2012



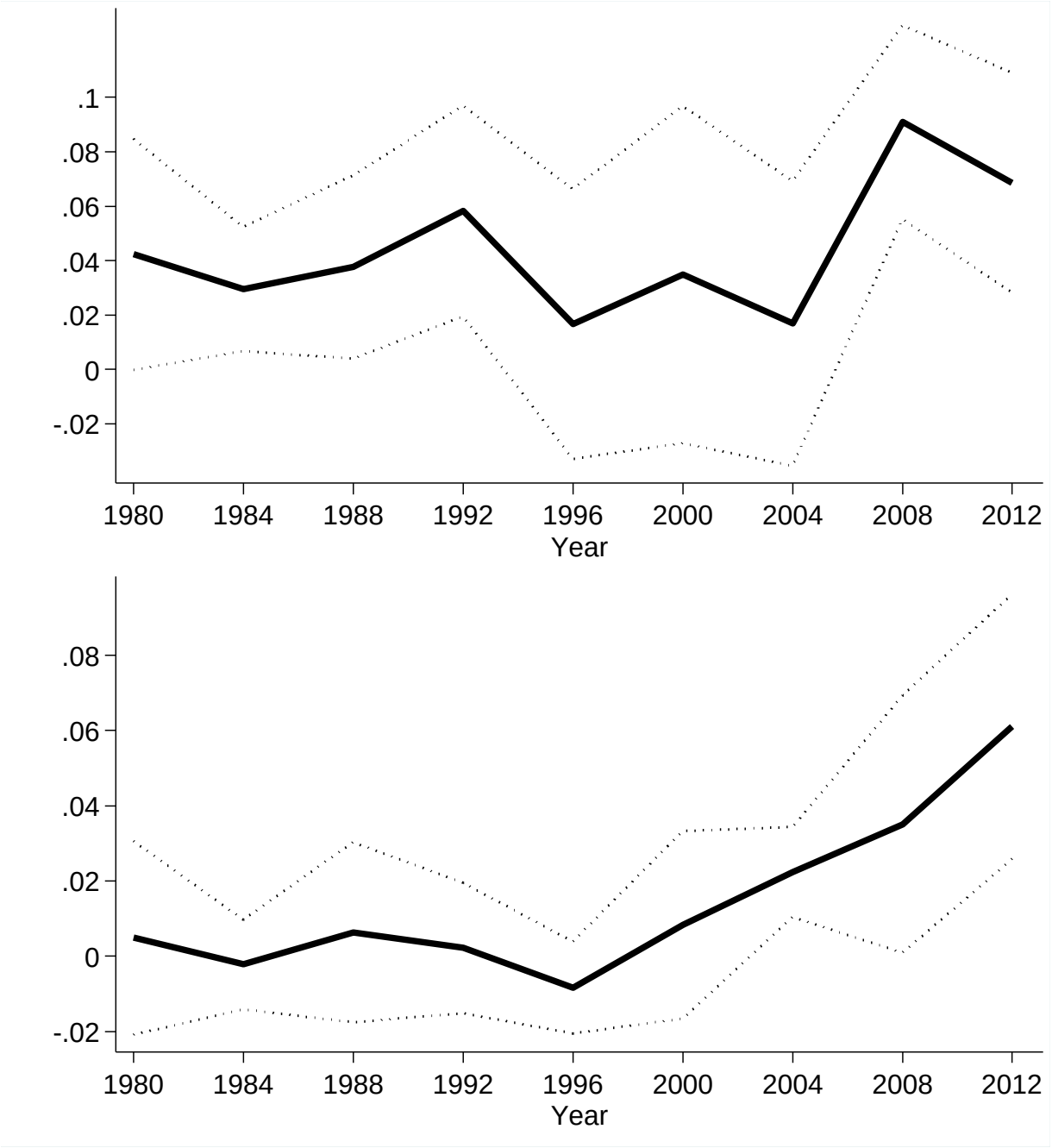
Each circle represents the intersection of two states within a single media market (e.g., MA and NH residents living in the Boston media market). The size of each circle is proportional to the population (that of the smaller region). The vertical axis plots the difference in 2012 turnout between the two regions (with higher values representing higher turnout in the more heavily targeted state) and the horizontal axis plots the difference in campaign effort (as measured by the RomneyEffort variable) between the two states. The solid line represents a linear fit and the dashed curve represents a kernel regression. In both cases, each observation is weighted by population (that of the smaller region). Consistent with the regression results in Table 1, the turnout differential increases strongly as the effort differential increases. Moreover, the relationship is approximately linear suggesting approximately additive effects of multiple GOTV contacts.

Figure 3. Variation of Campaign Effects across Predicted Partisanship



The figure plots the results for six different regressions, replicating the analysis from Column 7 in Table 1 for different subsets of individuals with different levels of predicted partisanship. Going from left to right, we analyze the 10 percent of individuals with the lowest levels of predicted partisanship (most Republican), then the 10-25th percentiles, 25-50th percentiles, 50-75th percentiles, 75-90th percentiles, and the top 10 percent (most Democratic). The solid line represents the coefficients and the dotted lines indicate 95% confidence intervals (generated from state-clustered standard errors). We see the greatest estimates (11 percentage points) for the most partisan subgroups, and much smaller effects for the least partisan groups which were less likely to be contacted by the campaigns.

Figure 4. Increasing Campaign Effects in Recent Presidential Elections



The figure presents two sets of analysis consistent with the notion that campaign effects have increased in recent elections. We define the 5 most pivotal states in every presidential election from 1980 to 2012 and compare the turnout rates between pivotal and non-pivotal states in each year. The top panel shows the average turnout differential between pivotal and non-pivotal states in each year. The bottom panel shows regression coefficients which explore within-state changes in turnout as a state switches from non-pivotal to pivotal in a particular year. In both cases, the turnout differential between the pivotal and non-pivotal categories has increased in recent elections. Dotted lines indicate 95% confidence intervals (derived from state-clustered standard errors).

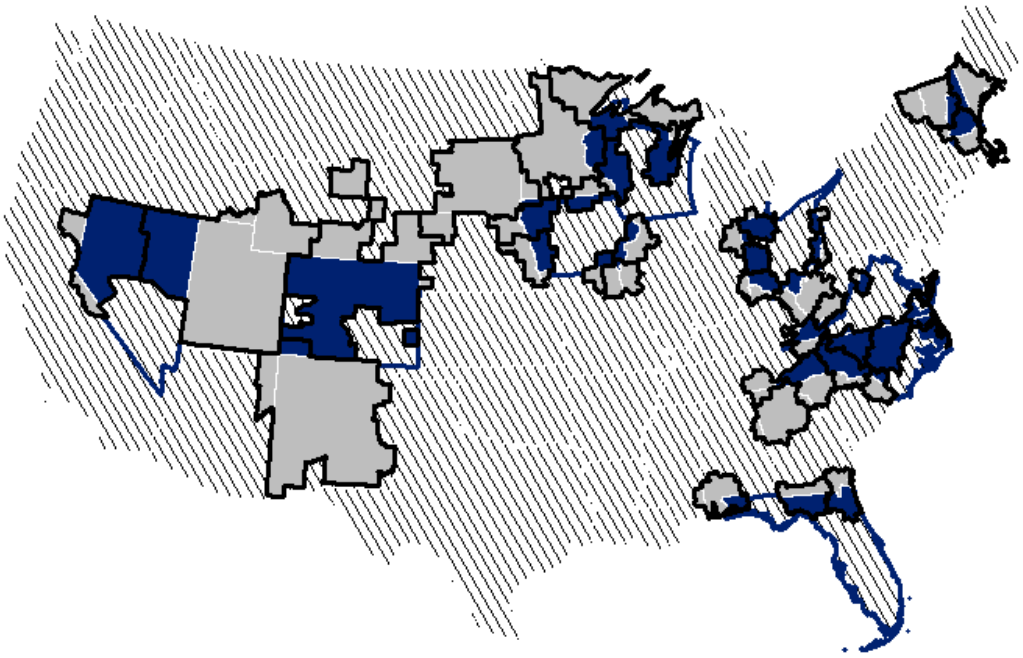
Appendix

Table A1. Data on Romney Contacts by State

	Attempted Contacts by				
State	Phone	Mail	Door	Voting-Eligible Pop.	Cost Per Person
Nevada	8,816,024	1,247,837	818,031	1,776,403	$3.40
New Hampshire	4,322,647	632,810	329,843	1,013,741	$2.72
Virginia	24,082,528	3,376,377	1,851,916	5,808,768	$2.64
Iowa	11,196,485	1,110,231	513,207	2,263,375	$2.53
Ohio	35,809,370	3,390,991	2,849,782	8,644,958	$2.53
Colorado	15,471,683	1,573,946	938,777	3,654,045	$2.36
Wisconsin	14,367,117	1,023,387	878,122	4,233,992	$1.82
Florida	35,738,414	4,056,126	2,314,147	13,348,802	$1.55
North Carolina	12,547,020	1,304,697	1,153,806	6,970,868	$1.18
Pennsylvania	16,433,988	277,997	1,474,486	9,674,379	$0.99
Michigan	7,681,325	357,254	1,039,975	7,317,247	$0.78
New Mexico	1,015,489		19,955	1,432,375	$0.25
Utah	1,447,579			1,835,666	$0.24
Missouri	1,437,169		131,016	4,410,813	$0.19
Maine	400,402		14,172	1,047,901	$0.16
Minnesota	669,269			3,876,752	$0.05
Nebraska	137,090		8,535	1,321,948	$0.05
Arizona	368,962			4,360,076	$0.03
Massachusetts	401,338			4,781,421	$0.03
Vermont	5,981			495,866	$0.00
Texas	180,098			16,100,196	$0.00
Indiana	22,047			4,759,100	$0.00
California	101,080			23,620,070	$0.00

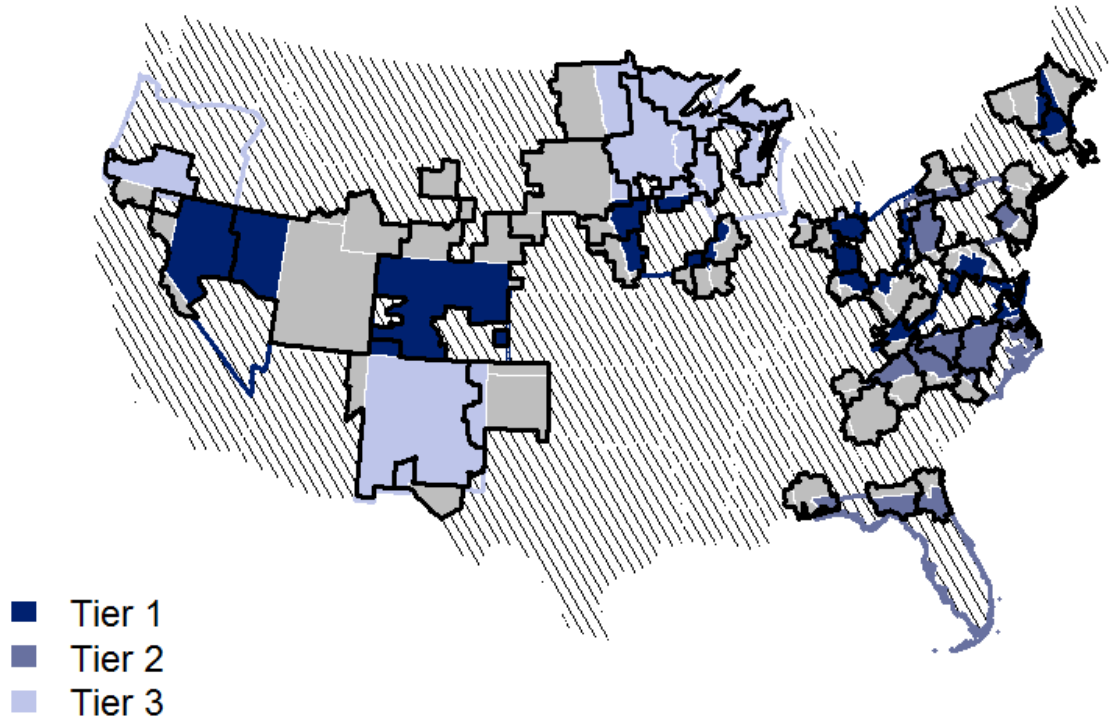
Maps of Variation in *Battleground* and *Obama Tier* Variables

Figure A2: *Battlground Status* by State and Media Market



Media markets spanning states with different Battleground status are surrounded by thick black lines. Dark purple states are battleground states, while gray states are not. So, for example, the Reno media market is dark purple in Nevada and gray in California, representing the variation in Battleground stats across those states. The media markets that do not span states with varying Battleground status (not included in our analysis) are indicated by diagonal lines.

Figure A3: Obama Tiers by State and Media Market



Media markets spanning states with different Obama Tiers are surrounded by thick black lines. The priority of the states in tiers are indicated by shades of purple, with the highest tier states being the darkest, while gray states are states that had no status in the Obama tiers. So, for example, the Reno media market is dark purple in Nevada and gray in California, representing that Nevada was in Tier 1, while California was in no tier. The media markets that do not span states with varying Obama Tiers (not included in our analysis) are indicated by diagonal lines.

Placebo Tests

Table A2 shows the results of our placebo tests using voter turnout in 2010. Columns 1-3 show results for Equation 1 where we compare battleground and non-battleground states. Columns 4-6 show the results for Equation 2 comparing the 4 different tiers of states. Columns 7-9 show results for Equation 3 examining turnout across *RomneyEffort*. All regressions include media market fixed effects, and columns 2, 5, and 8 include individual-level controls for gender, race, and income. Columns 3, 6, and 9 additionally include a control variable indicating whether a state had, according to Real Clear Politics, a toss-up senatorial or gubernatorial race in 2010. The sample sizes vary across the three different estimation strategies because we only include media markets for which there is variation in the treatment variable. For example, the regressions associated with Equation 1 (Columns 1-3) only include media markets that span both a battleground and non-battleground state. This includes approximately 50 million individuals living in 38 media markets spanning 34 states. For Equation 2, we include all individuals living in media markets which span states in different tiers (88 million individuals in 49 media markets spanning 44 states). For Equation 3, we include all individuals living in media markets which span states receiving different levels of effort from the Romney campaign (119 million individuals in 74 media markets spanning 48 states—Washington, DC is included as a state).

Examining our specifications with no individual-level control variables (Columns 1, 4, and 7), we see no meaningful differences between individuals in targeted and non-targeted states in their turnout levels in 2010. All point estimates are statistically indistinguishable from zero, suggesting that individuals within the same media market but spanning state boundaries appear to have similar underlying propensities to vote. When we add individual control variables—gender, race, and predicted family income—to these regressions (Columns 2, 5, and 8) or state-level controls for salient elections (Columns 3, 7, and 9), the point estimates are virtually unchanged because these

covariates are well balanced across targeted and non-targeted states.[27] Most of the point estimates are small relative to our main results using 2012 turnout, so these differences are unlikely to meaningfully bias our main results. Moreover, our main results are unchanged by the inclusion of 2010 voter turnout as a control variable, further bolstering confidence in our design and results.[28] These placebo tests support the plausibility of our empirical design. Voters within the same media market but spanning states that received differential levels of presidential campaigning are similar in their baseline levels of participation.

The results in Table A2 alleviate concerns that our comparison units are not fundamentally comparable in their underlying propensity to participate in elections. However, these results do not address additional concerns about factors specific to presidential elections that may lead voters in battleground states to participate more. For example, battleground voters may participate more, even in the absence of campaigning because they believe their vote is more likely to be pivotal. To address these issues, we present additional placebo tests using survey data from 2012 to measure differences between battleground and non-battleground voters in their underlying interests in voting. The 2012 CCAP surveyed approximately 1,000 different individuals in each week of 2012 before the November election. We focus on the surveys from the first 16 weeks—before the RNC declared Romney as the presumptive nominee on April 25th. During this period, there was little general

[27] All individual controls are included as dummy variables, so linearity assumptions are not a concern. For example, when we say that we include controls for race, we include binary indicators for white, black, Hispanic, and Asian individuals, leaving other races and missing race information as the omitted category. Predicted family incomes are divided into 9 categories (<5k, 5-12.5k, 12.5-20k, 20-30k, 30-40k, 40-60k, 60-100k, >100k, and missing). We also have some data on age, but we do not incorporate it into our analyses, because age data is missing for many individuals and the rate of missing data varies significantly across states.

[28] Only 1 of the 15 coefficients in Table A2 is statistically significant (the coefficient associated with Tier 2 when individual controls are included), and given the number of tests, at least one significant coefficient is expected to arise by chance. The statistical significance of Tier 2, rather than Tier 1, despite Tier 1 receiving greater campaign activity has no clear interpretation. This false positive disappears once we control for competitive senatorial or gubernatorial races, because two of the three states in the second tier (PA and FL) happened to have a competitive senatorial or gubernatorial race in 2010.

election campaign activity, so any differences between voters in battleground and non-battleground states are unlikely to be attributable to campaigning.[29]

The CCAP asked two questions each week that are useful for detecting any underlying differences that might plague the results of our study. First, they asked each respondent about their level of interest in politics/current events, allowing respondents to say that they are "very much interested," "somewhat interest," "not much interested," or "not sure." Just over half of the respondents report that they are very much interested, so we code a binary variable indicating whether each respondent chose that particular option. The CCAP also asked each respondent who they would support in the presidential election. Respondents could choose among the following choices: "The Democratic Party candidate," "The Republican Party candidate," "Other," "Not sure," or "I would not vote." We code a binary variable indicating whether a respondent reported that they would not vote. For an alternate approach (Table A5), we also code a binary variable indicating whether a respondent reported that they would vote for the Democratic or Republican candidate.

Tables A3, A4, and A5 below present a series of placebo regressions testing whether individuals in targeted states reported greater levels of interest in politics or greater intentions to vote before the general election campaigns got underway. If voters in targeted states are more likely to turn out even in the absence of campaigning, we would expect to find large differences in these surveys. However, if there are no meaningful differences before campaigning began, then we can more confidently attribute our main results to campaigning. Because we cannot match the CCAP respondents to low levels of geography, we do not include media market fixed effects in these placebo analyses. Also, because we pool survey responses from different weeks, we include week fixed effects to account for any changes over time in these attitudinal variables. In all other respects,

[29] Obama may have already engaged in general election campaigning at this point, but this activity would create a bias toward a failure of our placebo test.

these regressions mimic those in Table A2. For each outcome and for each measure of campaign effort across states, we present three separate regressions. The first has no controls. The second includes individual controls for age, race, and gender. The third also includes a control variable indicating whether the respondent's state held an early primary (before Super Tuesday). In total, across all three tables, we obtain 45 placebo estimates. Only 2 of these estimates are statistically significant (we would expect 2.25 by chance), and they go in the opposite direction that we would expect if battleground voters were more motivated to vote independent of campaigns.[30]

Some readers may notice that our placebo tests are imperfect, because most of the states targeted in 2012 were also targeted in 2008, and if 2008 campaigning was effective, we might expect to see differences in 2010 and early 2012 since voting is known to be habitual.[31] However, the extent of this bias is likely small. Meredith (2009) estimates that the effect of voting in one election on turnout in a subsequent election is about 5 percentage points. If we assume that campaigning in 2008 increased turnout by 7 percentage points (consistent with our point estimate in 2012), then the propagating effect of 2008 campaigning on subsequent turnout is 0.35 percentage points (.05*.07 = .0035), a negligible effect.

In Table A6, we also present placebo tests using demographic covariates from Catalist as our outcomes of interest. None of these placebo results are statistically or substantively significant. We also predict each individual's propensity to vote based on their gender, race, and income, and we find no differences in predicted vote propensity between battleground and non-battleground residents within the same media market.

[30] Individuals living in Tier 2 states were slightly more likely to report that they will not vote compared to other states (Columns 5 and 6 in Table A4)—a result that is most likely explained by chance.

[31] See, for example, Meredith, Marc. 2009. Persistence in Political Participation. *Quarterly Journal of Political Science* 4(3):186-208.

In short, we find no evidence that individuals in targeted states had different turnout behaviors, intentions to vote, or levels of political interest before presidential campaigning began. These results suggest that our identifying assumptions are sound and that our subsequent results can be attributed to campaigning as opposed to spurious factors and independent effects of battleground environments.

Table A2. Placebo Tests using 2010 Voter Turnout

	DV = 2010 Voter Turnout								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Battleground	.003	.001	.001						
	(.012)	(.011)	(.007)						
Tier 1				.014	.010	.011			
				(.016)	(.015)	(.007)			
Tier 2				.024	.027	.004			
				(.013)	(.012)	(.008)			
Tier 3				.029	.018	.009			
				(.017)	(.019)	(.018)			
Romney Effort							.014	.013	.008
							(.020)	(.019)	(.009)
Media Market Fixed Effects	X	X	X	X	X	X	X	X	X
Individual Controls		X	X		X	X		X	X
Control for Sen. or Gov. Race			X			X			X
N individuals		49,549,516			87,650,509			118,645,707	
N media markets		38			49			74	
N states		34			44			48	
R-squared	.013	.108	.108	.012	.099	.099	.014	.099	.099

All variables are defined as in Table 1. Because the dependent variable is voter turnout in 2010, a year where there was no presidential campaigning, these regressions constitute placebo tests, indicating whether there are underlying differences between targeted and non-targeted states. All of the point estimates are substantively small, and all but one are statistically indistinguishable from zero. The results are consistent with the underlying assumptions of our empirical strategies. Standard errors, clustered by state, are in parentheses.

Table A3. Placebo Tests Using Political Interest before the General Election Campaign

	DV = "very much interested" in politics and current events								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Battleground	−.005	−.004	−.012						
	(.014)	(.013)	(.013)						
Tier 1				−.003	.000	−.006			
				(.018)	(.020)	(.019)			
Tier 2				−.005	−.011	−.019			
				(.017)	(.014)	(.012)			
Tier 3				.046	.031	.020			
				(.030)	(.024)	(.025)			
Romney Effort							−.008	−.008	−.021
							(.023)	(.022)	(.022)
Early Primary State			.025			.021			.025
			(.015)			(.014)			(.014)
Individual Controls		X	X		X	X		X	X
Survey Week Fixed Effects	X	X	X	X	X	X	X	X	X
Observations					15,998				
R-squared	.001	.106	.106	.002	.106	.106	.001	.106	.106

All variables are defined as in Table 1. Data is from the 2012 CCAP. We analyze responses for the first 16 weeks of 2012, before Romney was the presumptive nominee and before general election campaigning got underway. We find no detectable differences in political interest between battleground and non-battleground voters before general election campaigning, providing empirical support for our assumptions that these individuals are comparable in terms of their underlying interests and intentions to vote. Standard errors, clustered by state, are in parentheses.

Table A4. Placebo Tests Using Vote Intention before the General Election Campaign

	DV = "will not vote" in 2012								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Battleground	.007	.007	.011						
	(.008)	(.008)	(.009)						
Tier 1				.005	.002	.005			
				(.008)	(.008)	(.007)			
Tier 2				.019	.022	.026			
				(.010)	(.009)	(.008)			
Tier 3				−.015	−.018	−.012			
				(.009)	(.010)	(.011)			
Romney Effort							.010	.008	.014
							(.010)	(.011)	(.012)
Early Primary State			−.012			−.011			−.011
			(.008)			(.008)			(.008)
Individual Controls		X	X		X	X		X	X
Survey Week Fixed Effects	X	X	X	X	X	X	X	X	X
Observations					15,998				
R-squared	.002	.033	.033	.003	.034	.034	.002	.033	.033

All variables are defined as in Table 1. Data is from the 2012 CCAP. We analyze responses for the first 16 weeks of 2012, before Romney was the presumptive nominee and before general election campaigning got underway. We find no detectable differences in vote intentions between battleground and non-battleground voters before general election campaigning, providing empirical support for our assumptions that these individuals are comparable in terms of their underlying interests and intentions to vote. For the two statistically significant coefficients, the point estimate is in the "wrong" direction (tier 2 voters are more likely to say that they will not vote), and the substantive size is small. These differences are most likely attributable to chance and multiple testing. Out of 60 placebo tests in Tables A2, A3, A4, and A5, we obtain 3 statistically significant coefficients, exactly what we would expect by chance. Standard errors, clustered by state, are in parentheses.

Table A5. Placebo Tests Using Vote Intention before the General Election Campaign (Alternate Coding)

	DV = plan to vote for Dem. or Rep. in 2012								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Battleground	−.011	−.011	−.016						
	(.016)	(.016)	(.016)						
Tier 1				−.010	−.005	−.010			
				(.018)	(.019)	(.017)			
Tier 2				−.017	−.021	−.027			
				(.021)	(.020)	(.020)			
Tier 3				.003	.012	.005			
				(.018)	(.019)	(.020)			
Romney Effort							−.019	−.017	−.024
							(.022)	(.022)	(.022)
Early Primary State			.015			.014			.014
			(.016)			(.017)			(.015)
Individual Controls		X	X		X	X		X	X
Survey Week Fixed Effects	X	X	X	X	X	X	X	X	X
Observations					15,998				
R-squared	.001	.037	.037	.001	.037	.037	.001	.037	.037

All variables are defined as in Table 1. Data is from the 2012 CCAP. We analyze responses for the first 16 weeks of 2012, before Romney was the presumptive nominee and before general election campaigning got underway. We find no detectable differences in vote intentions between battleground and non-battleground voters before general election campaigning, providing empirical support for our assumptions that these individuals are comparable in terms of their underlying interests and intentions to vote. Standard errors, clustered by state, are in parentheses.

Table A6. Placebo Tests using Demographics

	Dependent Variable								
	Female	White	Black	Hispanic	Asian	Inc. > 20k	Inc. > 40k	Inc. > 100k	Vote Propensity
Romney Effort	−.008	.046	−.018	−.020	−.007	−.004	−.006	−.005	.003
	(.004)	(.035)	(.021)	(.012)	(.006)	(.012)	(.012)	(.016)	(.003)
Media Market Fixed Effects	X	X	X	X	X	X	X	X	X
N individuals						118,645,707			
N media markets						74			
N states						48			
R-squared	.001	.100	.088	.092	.010	.011	.013	.020	.016

Standard errors, clustered by state, are in parentheses.

Separating the Effects of Internet Advertising and the Ground Campaign

We test the relative importance of digital vs. ground campaigning by utilizing data from the Obama campaign on their digital campaigning activities in each state. Specifically, we acquired information on dollars spent on digital campaigning and advertisements run in each state.[32] As with ground campaign effort, we divide the dollars spent by the voting-eligible population in each state and rescale this variable to range from 0 to 1. Table A7 presents the total digital expenditures of the Obama campaign in each state. The campaign also spent 1.8 million dollars on nationwide digital advertising (not included in the table) that was not geographically targeted, explaining why residents of other states may have seen some digital ads. As expected, digital advertising varied significantly across states with virtually no such activity in 41 states or DC. In Iowa, the Obama campaign spent almost one dollar per eligible voter, and they also made significant digital efforts in Colorado, Nevada, Ohio, and New Hampshire. Also as expected, digital and GOTV campaign effort are highly correlated across states, but enough variation exists that we can include both in a regression to estimate the relative importance of each strategy. Specifically, we repeat the regressions from Columns 7-9 of Table 1 but also include the continuous variable indicating digital effort which also ranges from 0 to 1. In other words, we regress voter turnout on continuous measures of ground effort and digital effort along with media market fixed effects and several control variables. Table A8 presents the results.

Consistent with our previous claim that our results are largely explained by traditional ground campaigning, we see that the coefficients for ground effort are similar to those in Table 1, while the coefficients for digital campaign effort are actually negative.[33] Of course, the strong

[32] Multiple files obtained from Obama staff, March 18, 2014. Data aggregated by the authors.

[33] One explanation for this negative coefficient is that the Obama campaign attempted to strategically target these advertisements in areas where the turnout of supporters, based on early and absentee voting, was below their targets (personal communication with an Obama staffer on March 19, 2014). Unlike traditional ground campaigning, digital advertising is nimble and can be shifted

correlations between ground and digital effort mean that these results should not be interpreted too strongly. Nonetheless, to the extent that we detect effects of campaigning on turnout, it appears to be attributable to the traditional forms of GOTV campaigning that have consistently been shown to boost turnout in experimental studies.

quickly based on last-minute information (e.g., early voting returns), so these kinds of concerns about omitted variables or reverse causation are more prevalent for digital advertising than for other forms of campaigning. For these reasons, the negative coefficient should not be interpreted as evidence of a negative effect of digital campaigning.

Table A7. Data on Obama Digital Expenditures by State

State	Digital Expenditures	Voting-Eligible Population	Cost Per Person
Iowa	$2,152,923	2,263,375	$0.95
Colorado	$1,203,553	3,654,045	$0.33
Nevada	$341,847	1,776,403	$0.19
Ohio	$942,067	8,644,958	$0.11
New Hampshire	$107,297	1,013,741	$0.11
Wisconsin	$328,833	4,233,992	$0.08
Virginia	$353,110	5,808,768	$0.06
Florida	$803,499	13,348,802	$0.06
North Carolina	$331,661	6,970,868	$0.05

Table A8. Separating Effects of the Digital Campaign and Traditional GOTV

	DV = 2012 Voter Turnout		
	(1)	(2)	(3)
Ground Effort	.091	.091	.080
	(.014)	(.013)	(.016)
Digital Effort	−.041	−.044	−.042
	(.014)	(.015)	(.015)
Media Market Fixed Effects	X	X	X
Individual Controls		X	X
Control for 2010 Turnout			X
N individuals		118,645,707	
N media markets		74	
N states		48	
R-squared	.019	.104	.624

The table mirrors Columns 7-9 of Table 1 with the exception of one new variable. "Ground Effort" is the Romney Effort variable, indicating the level of ground campaigning in each state. "Digital Effort" is a continuous measure, ranging from 0 to 1, indicating the dollars per person that the Obama campaign spent on internet ads in each state. Virtually all of the previous estimates appear to be attributable to ground campaigning as opposed to digital campaigning. Standard errors, clustered by state, are in parentheses.

Effects of TV Advertising on Turnout in 2012

In order to assess the effects of television advertising, we conduct two sets of analyses, shown in Tables A9 and A10. First, we replicate the analysis of Krasno and Green (2008) using our individual-level data from 2012. This design allows us to generate precise and credible estimates of the effect of television advertising on turnout in non-battleground states. Second, to address the possibility that the effects of television advertising are greater in battleground states, perhaps because they interact with ground campaigning, we present a second research design exploiting plausibly exogenous variation in television advertising, coming from the fact that some media markets have only a small share of residents in a battleground state, meaning that advertising there is less efficient.

In our first approach, following Krasno and Green (2008), we exploit the fact that television advertising is intended for battleground states but spills over into the non-battleground states that share a media market with a battleground state. This spillover provides arguable exogenous variation in television advertising, allowing us to estimate the effect of television advertising on turnout in non-battleground states. We restrict our attention to voters in non-battleground states where some media markets overlap with a battleground state and others do not—there are 24 such states that fit these criteria. We compare the turnout behavior of voters in the media markets that share territory with a battleground state to that of voters in the non-battleground media markets in the same state. To do this, we regress 2012 turnout on an indicator for a "battleground media market" (a media market that shares territory with a battleground state) and state fixed effects. Columns 1-3 of Table A9 present the results. In column 2 we add controls for race, income, age, and gender, and in column 3 we include 2010 turnout as an additional control variable.

To provide a more precise test of the effect of television in non-battleground states, we collected data on presidential ads aired in each media market.[34] In addition to our binary indicator for battleground and non-battleground media markets, we code two additional explanatory variables which capture the extent of television advertising in each market. We code a variable indicating the total number of ads aired in each media market, which, within this sample, ranges from 0 to 57,466 and takes a mean value of 1,046 in non-battleground media markets and a mean value of 9,097 in battleground media markets. We rescale this variable to range from 0 to 1 and call it "Total Ads." Columns 4-6 of Table A9 replicates our analysis from Columns 1-3 using this variable as our primary explanatory variable. We also code a variable indicating the number of advertising dollars per capita spent in each media market, which, within this sample, ranges from 0 to 23.6 and takes an average value of 0.43 in the non-battleground media markets and an average value of 3.11 in battleground media markets. Again, we rescale this variable to range from 0 to 1 and call it "Advertising Dollars per Capita." Columns 7-9 present results using this variable as our primary explanatory variable.

Consistent with Krasno and Green (2008), we find no evidence that television advertising increases turnout in non-battleground states across all specifications. The point estimate for each specification is substantively tiny and precisely estimated. Moving from no advertising to the maximal level of advertising has no detectable effect, and the point estimates are less than a single percentage point in all but one specification.

Although television advertising appears to have no effect in non-battleground states, perhaps the effect is larger in battleground states, perhaps because television advertising interacts with ground campaigning. To address this possibility, we present a second research design in order to

[34] This data was collected by Kanter Media/CMAG and made public available by *The Washington Post* at http://www.washingtonpost.com/wp-srv/special/politics/track-presidential-campaign-ads-2012/.

estimate the effect of television advertising on turnout in battleground states. Here, we exploit the fact that some media markets are less efficient targets, because much of their territory covers non-battleground states. For example, all of the Las Vegas media market lies within Nevada, a key swing state, making it a prime target for advertising. As a result, the presidential campaigns aired 56,538 ads and spent $28.84 per capita in this market. Alternatively, the Salt Lake City media market spills into Nevada, but only 2 percent of the market's residents live in the battleground state, meaning that advertising in the Salt Lake City media market is highly inefficient. For this reason, the presidential campaigns spent no money in the Salt Lake City media market. Therefore, a comparison of demographically similar individuals within Nevada residing in either the Las Vegas or Salt Lake City media markets may provide credible estimates of the effect of television advertising in battleground states. Our subsequent design leverages situations like this for all battleground states, always restricting our comparisons within states.

We code a continuous variable indicating the proportion of residents in each media market who reside in a battleground state. Within battleground states, this variable is arguably exogenous to participation, yet it strongly predicts the extent of advertising in a media market, because spending advertising to an audience largely outside of a battleground state is highly inefficient. Therefore, we use this variable as an instrument for the extent of advertising in each media market, measured in each of the two ways described above (total ads or dollars per capita). Specifically, we estimate the effect of television advertising on voter turnout in battleground states through two-stage least squares regressions where the proportion of battleground residents in a media market is used as an instrument for advertising effort. We include state fixed effects in all specifications, confining all comparisons within states, and some specifications include demographic controls and a control for turnout in 2010.

Table A10 presents the results of these analyses. These estimates are less precise than those in Table 19, but we similarly detect no effect of television advertising on turnout in battleground states. The first-stage F-statistics are all large, suggesting that the proportion of battleground residents in a media market is indeed a strong predictor of television advertising. As in Table A9, the explanatory variables are recoded to range from 0 to 1, so the coefficients can be interpreted as the estimated effect of going from no advertising to the greatest observed level of advertising in any state. None of the point estimates are statistically significant, although some specifications are imprecise. Nonetheless, we fail to find evidence that television increases turnout in battleground or non-battleground states, and for some specifications, we can statistically reject any hypothesized effect of television advertising greater than one or two percentage points. Taken together, these analyses suggest that the air campaign has little effect on participation, and most likely, the large differences we observe between battleground and non-battleground states are most likely attributable to ground campaigning.

Table A9. Effects of TV Advertising in Non-Battleground States

	OLS, DV = 2012 Voter Turnout								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Battleground Media Market	.007	.002	.005						
	(.006)	(.005)	(.003)						
Total Ads				.013	.002	.008			
				(.016)	(.013)	(.009)			
Advertising Dollars per Capita							.004	−.008	.001
							(.012)	(.010)	(.008)
State Fixed Effects	X	X	X	X	X	X	X	X	X
Individual Controls		X	X		X	X		X	X
Control for 2010 Turnout			X			X			X
N individuals					141,886,753				
N media markets					139				
N states					24				
R-squared	.012	.301	.709	.012	.301	.709	.012	.301	.709

Standard errors, clustered by media market, are in parentheses.

Table A10. Effects of TV Advertising in Battleground States

	2SLS, DV = 2012 Voter Turnout					
	(1)	(2)	(3)	(4)	(5)	(6)
Total Ads	.023	.041	.013			
	(.031)	(.030)	(.019)			
Advertising Dollars per Capita				.042	.076	.023
				(.057)	(.056)	(.035)
State Fixed Effects	X	X	X	X	X	X
Individual Controls		X	X		X	X
Control for 2010 Turnout			X			X
N individuals			61,433,423			
N media markets			66			
N states			9			
First-Stage F-Statistic	9.9	10.2	10.2	7.0	7.3	7.3

Standard errors, clustered by media market, are in parentheses. Total Ads and Advertising Dollars per Capita are instrumented using the proportion of battleground state residents in the media market.