

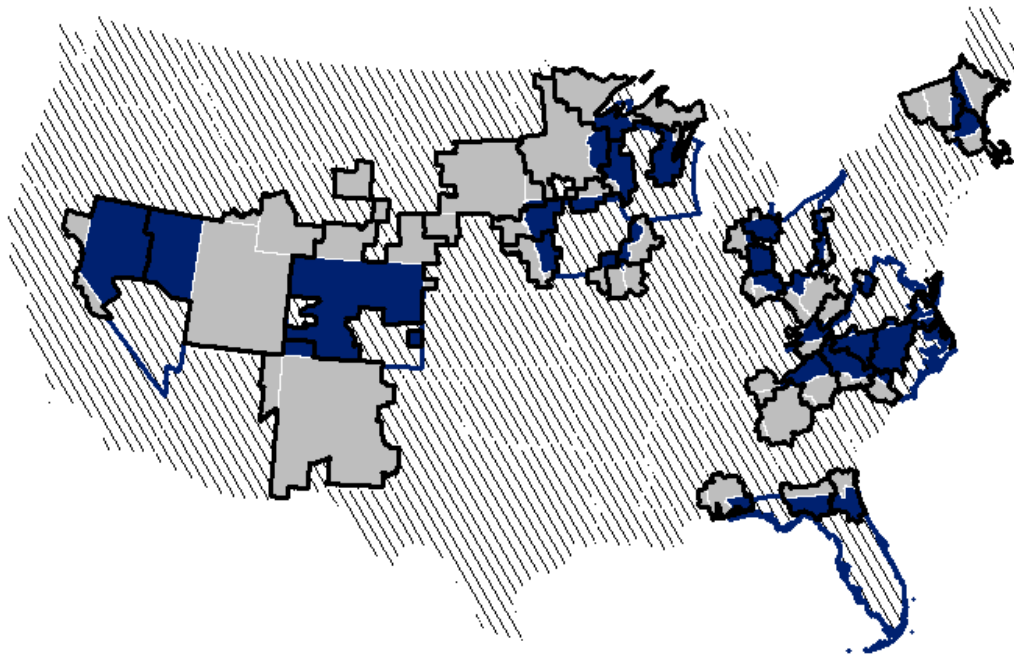
**Appendix**  
 Aggregate Effects of Large-Scale Campaigns on Voter Turnout  
 Ryan D. Enos and Anthony Fowler

**Table A1. Data on Romney Contacts by State**

State	Attempted Contacts by			Voting-Eligible Pop.	Cost Per Person
	Phone	Mail	Door		
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

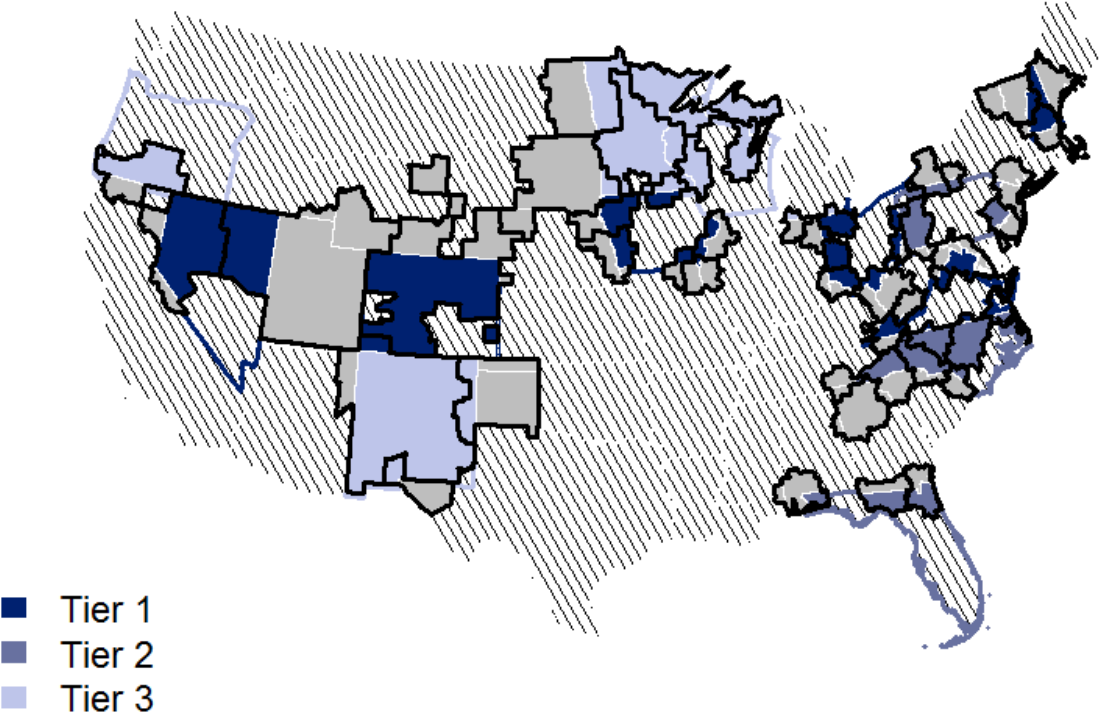
## Maps of Variation in *Battleground* and *Obama Tier* Variables

Figure A2: *Battleground 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.<sup>1</sup> 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.<sup>2</sup> 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

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<sup>1</sup> 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.

<sup>2</sup> 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.<sup>3</sup>

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,

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<sup>3</sup> 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.<sup>4</sup>

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.<sup>5</sup> 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 ( $.05 \times .07 = .0035$ ), 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.

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<sup>4</sup> 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.

<sup>5</sup> 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 (.012)	.001 (.011)	.001 (.007)						
Tier 1				.014 (.016)	.010 (.015)	.011 (.007)			
Tier 2				.024 (.013)	.027 (.012)	.004 (.008)			
Tier 3				.029 (.017)	.018 (.019)	.009 (.018)			
Romney Effort							.014 (.020)	.013 (.019)	.008 (.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 (.014)	-.004 (.013)	-.012 (.013)						
Tier 1				-.003 (.018)	.000 (.020)	-.006 (.019)			
Tier 2				-.005 (.017)	-.011 (.014)	-.019 (.012)			
Tier 3				.046 (.030)	.031 (.024)	.020 (.025)			
Romney Effort							-.008 (.023)	-.008 (.022)	-.021 (.022)
Early Primary State			.025 (.015)			.021 (.014)			.025 (.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 (.008)	.007 (.008)	.011 (.009)						
Tier 1				.005 (.008)	.002 (.008)	.005 (.007)			
Tier 2				.019 (.010)	.022 (.009)	.026 (.008)			
Tier 3				-.015 (.009)	-.018 (.010)	-.012 (.011)			
Romney Effort							.010 (.010)	.008 (.011)	.014 (.012)
Early Primary State			-.012 (.008)			-.011 (.008)			-.011 (.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.<sup>6</sup> 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.<sup>7</sup> Of course, the strong

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<sup>6</sup> Multiple files obtained from Obama staff, March 18, 2014. Data aggregated by the authors.

<sup>7</sup> 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.

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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 (.014)	.091 (.013)	.080 (.016)
Digital Effort	-.041 (.014)	-.044 (.015)	-.042 (.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.<sup>8</sup> 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

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<sup>8</sup> 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 (.006)	.002 (.005)	.005 (.003)						
Total Ads				.013 (.016)	.002 (.013)	.008 (.009)			
Advertising Dollars per Capita							.004 (.012)	-.008 (.010)	.001 (.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 (.031)	.041 (.030)	.013 (.019)			
Advertising Dollars per Capita				.042 (.057)	.076 (.056)	.023 (.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.*

## OLS and Logit Yield Nearly Identical Estimates

In Table A11, we show both OLS and logit estimates for several of our specifications from Table 1. Throughout the paper, we utilize OLS because of its computational efficiency, substantive interpretability, and its advantageous statistical properties—i.e., “best linear approximation to the conditional expectation function” (see *Mostly Harmless Econometrics*, Ch. 3)—even in the presence of a dichotomous dependent variable. Nonetheless, we demonstrate here that Logit yields nearly identical estimates. Specifically, Table A11 reports the results from Columns 1, 2, 7, and 8 of Table 1, respectively, using both OLS and logit. The first column of Table A11 reports OLS coefficients and the second column reports logit marginal effects. The marginal effects are computed by estimating a logit model—otherwise identical to the OLS equation, calculating the predicted values for each observation with the key explanatory variables (Battleground and Romney Effort) set at their extremes (1 and 0) with all other independent variables left at their actual values, and then calculating the average difference between these predicted values across all observations. We see in the table that the OLS coefficients are nearly identical to the logit marginal effects in every case.

The third and fourth columns of Table A11 also report the t-statistic and z-statistic associated with the key independent variable resulting from the OLS and logit estimation, respectively. In each case, standard errors are clustered by state. In addition to the point estimates being nearly identical across OLS and logit, we see that the statistical significance is nearly identical as well.

**Table A11. OLS vs. Logit Estimates**

Specification	OLS coefficient	Logit marginal effect	OLS t-stat	Logit z-stat
Battleground (no controls)	.0420	.0420	3.73	3.77
Battleground (w/ controls)	.0414	.0409	4.08	4.06
Romney Effort (no controls)	.0783	.0781	6.32	6.38
Romney Effort (w/ controls)	.0776	.0765	6.61	6.56