Online Appendix for
Does Digital Advertising Affect Vote Choice? Evidence from a Randomized Field Experiment

Alexander Coppock, Donald P. Green, and Ethan Porter

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Contents

A Alternative outcome measures

In the main text, we report the effects of our treatments on vote share; here we report the effects on vote margin and turnout. As in the main analysis, we find small, statistically insignificant effects of our advertisements on both alternative outcome measures.

Table A.1: Effects on vote margin

	Model 1	Model 2	Model 3	Model 4
Any Treatment Video	47.15	6.54		
	(56.74)	(18.70)		
Treatment Video 1			18.07	−14.52
			(53.37)	(21.94)
Treatment Video 2			77.45	26.70
			(84.91)	(22.81)
Two Party Vote Margin (2016)		0.73*		0.73*
		(0.04)		(0.04)
Missingness Indicator (2016)		−66.23		−60.47
		(70.20)		(65.58)
Two Party Vote Margin (2014)		0.07		0.07
		(0.04)		(0.04)
Missingness Indicator (2014)		10.77		15.15
		(20.48)		(20.69)
Two Party Vote Margin (2012)		0.03		0.02
		(0.05)		(0.06)
Missingness Indicator (2012)		33.90		29.43
		(17.93)		(18.37)
Intercept	−72.16	88.28*	−72.16	89.62*
	(37.75)	(17.70)	(37.75)	(17.51)
R^2	0.00	0.86	0.01	0.86
Num. obs.	857	857	857	857
N Clusters	154	154	154	154

$^*p < 0.05$. CR2 Cluster-robust standard errors are in parentheses.

Table A.2: Effects on turnout

	Model 1	Model 2	Model 3	Model 4
Any Treatment Video	-23.59	10.19		
	(103.35)	(23.68)		
Treatment Video 1			-115.52	15.63
			(140.93)	(29.01)
Treatment Video 2			72.22	4.98
			(112.06)	(28.28)
Two Party Vote Total (2016)		0.87^{*}		0.87^{*}
		(0.02)		(0.02)
Missingness Indicator (2016)		858.20^{*}		857.18^{*}
		(124.46)		(123.83)
Two Party Vote Total (2014)		0.02		0.02
		(0.02)		(0.02)
Missingness Indicator (2014)		-8.21		-9.20
		(36.72)		(36.16)
Two Party Vote Total (2012)		-0.00		-0.00
		(0.04)		(0.04)
Missingness Indicator (2012)		-66.11		-65.34
		(49.82)		(48.82)
Intercept	1348.33^{*}	42.55	1348.33^{*}	41.89
	(69.36)	(48.37)	(69.36)	(47.81)
R^2	0.00	0.91	0.00	0.91
Num. obs.	857	857	857	857
N Clusters	154	154	154	154

$^{*}p < 0.05$. CR2 Cluster-robust standard errors are in parentheses.

B Alternative regression specification

At the request of a reviewer, we include here precinct-level regressions that include fixed effects for congressional district.

Table B.3: Effects on vote share (CD fixed effects)

	Model 1	Model 2	Model 3	Model 4
Any Treatment Video	51.94	3.62		
	(55.15)	(18.95)		
Treatment Video 1			31.94	−21.37
			(53.25)	(22.62)
Treatment Video 2			71.72	26.63
			(80.34)	(22.79)
Two Party Vote Margin (2016)		0.74*		0.74*
		(0.04)		(0.04)
Missingness Indicator (2016)		−76.80		−73.27
		(68.68)		(64.05)
Two Party Vote Margin (2014)		0.05		0.05
		(0.04)		(0.04)
Missingness Indicator (2014)		10.24		15.22
		(18.08)		(18.50)
Two Party Vote Margin (2012)		0.03		0.02
		(0.05)		(0.05)
Missingness Indicator (2012)		30.59		26.69
		(19.24)		(19.45)
Intercept	−126.10	62.96*	−127.86	61.80*
	(64.62)	(19.54)	(64.25)	(19.75)
R^2	0.06	0.86	0.06	0.86
Num. obs.	857	857	857	857
N Clusters	154	154	154	154

$^*p < 0.05$. CR2 SEs are in parentheses. All models include fixed effects for congressional district.

Table B.4: Effects on vote margin (CD fixed effects)

	Model 1	Model 2	Model 3	Model 4
Any Treatment Video	51.94	3.62		
	(55.15)	(18.95)		
Treatment Video 1			31.94	−21.37
			(53.25)	(22.62)
Treatment Video 2			71.72	26.63
			(80.34)	(22.79)
Two Party Vote Margin (2016)		0.74*		0.74*
		(0.04)		(0.04)
Missingness Indicator (2016)		−76.80		−73.27
		(68.68)		(64.05)
Two Party Vote Margin (2014)		0.05		0.05
		(0.04)		(0.04)
Missingness Indicator (2014)		10.24		15.22
		(18.08)		(18.50)
Two Party Vote Margin (2012)		0.03		0.02
		(0.05)		(0.05)
Missingness Indicator (2012)		30.59		26.69
		(19.24)		(19.45)
Intercept	−126.10	62.96*	−127.86	61.80*
	(64.62)	(19.54)	(64.25)	(19.75)
R^2	0.06	0.86	0.06	0.86
Num. obs.	857	857	857	857
N Clusters	154	154	154	154

$^*p < 0.05$. CR2 SEs are in parentheses. All models include fixed effects for congressional district.

Table B.5: Effects on turnout (CD fixed effects)

	Model 1	Model 2	Model 3	Model 4
Any Treatment Video	−27.54	9.18		
	(95.65)	(23.16)		
Treatment Video 1			−159.45	3.50
			(142.82)	(30.62)
Treatment Video 2			102.95	14.44
			(90.25)	(26.76)
Turnout (2016)		0.87*		0.87*
		(0.02)		(0.02)
Missingness Indicator (2016)		781.56*		781.60*
		(137.19)		(137.28)
Turnout (2014)		0.01		0.01
		(0.03)		(0.03)
Missingness Indicator (2014)		−1.74		−0.66
		(41.36)		(41.43)
Turnout (2012)		−0.00		−0.00
		(0.04)		(0.04)
Missingness Indicator (2012)		−26.88		−27.40
		(49.84)		(49.19)
Intercept	1580.42*	45.07	1568.81*	45.31
	(90.56)	(56.87)	(91.60)	(56.78)
R^2	0.08	0.91	0.09	0.91
Num. obs.	857	857	857	857
N Clusters	154	154	154	154

$^*p < 0.05$. CR2 SEs are in parentheses. All models include fixed effects for congressional district.

C Equivalence tests

At the request of a reviewer, we include here equivalence tests for the equivalence of our two treatments. We include here tests for equivalence using tolerances of 0.2 standard units and 0.1 standard units. The difference-in-means estimates of the difference between the two treatment groups are quite imprecise, with the result that none of the p-values for the difference-in-means estimates are smaller than 0.05. Using the covariate-adjusted models (marked as "OLS"), we can affirm equivalence at $p < 0.05$ for all three outcome variables using a 0.2 standard deviation equivalence tolerance. When we turn to the more restrictive tolerance of 0.1 SDs, only the OLS estimate of the difference on vote total can be affirmed equivalent.

Table C.6: Equivalence Tests

Outcome	SD	Estimator	Estimate	SE	p (0.2SD)	p (0.1SD)
Vote Share	0.165	DIM	-0.046	0.031	0.663	0.828
Vote Share	0.165	OLS	-0.002	0.011	0.004	0.106
Vote Margin	433.178	DIM	-59.386	84.907	0.374	0.575
Vote Margin	433.178	OLS	-41.226	25.443	0.037	0.467
Vote Total	982.813	DIM	-187.748	150.990	0.477	0.723
Vote Total	982.813	OLS	10.654	32.177	0.000	0.003

D Map

At the request of a reviewer, we include here (Figure D.1) a map of Florida that overlays ZIP codes on top of voting precinct boundaries. The main point that this map communicates is that precincts are much smaller than ZIP codes and the boundaries of the two sorts of geographic division do not nest within each other nicely. We caution here that we do not base our inferences on the precinct-to-ZIP code mapping from the geographic data that produce this map. The map only represents the 148 ZIP codes that have geographic boundaries, not the full set of 210 ZIP codes in our experiment, some of which can not be represented on a map. As described by the US Census, "USPS ZIP Codes are not areal features but a collection of mail delivery routes." The interested reader can refer to this website for more on the counter-intuitive definition of ZIP codes: https://www.census.gov/programs-surveys/geography/guidance/geo-areas/zctas.html

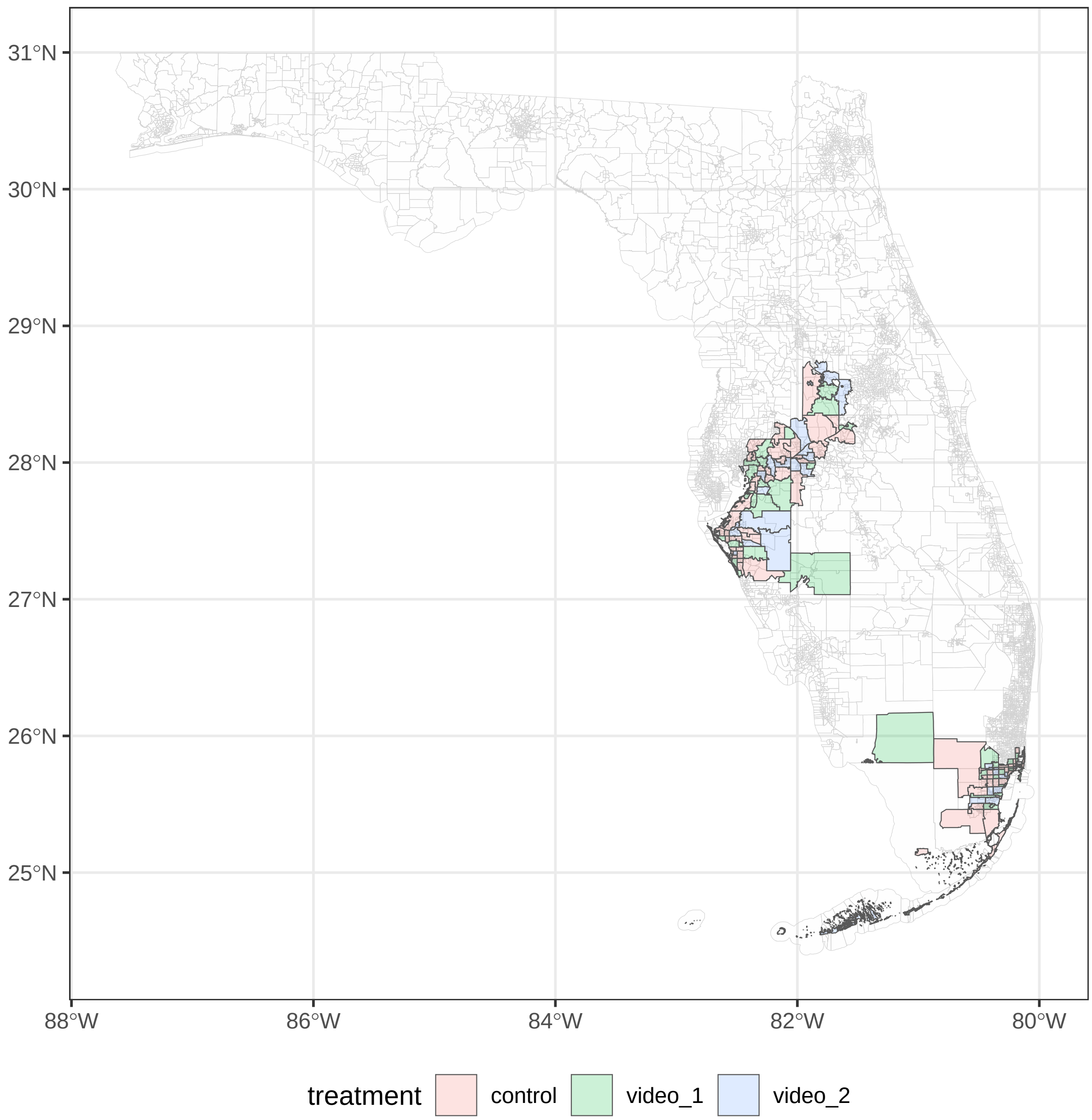


Figure D.1: Map of precincts and ZIP codes; see proceeding page for details on the limitations of this figure

E Balance

In this section, we show that the randomization generated balance on pre-treatment characteristics by regressing the treatment indicator on pre-treatment covariates. Each of the individual coefficients is nonsignificant, as is a joint test of significance ($p = 0.33$).

Table E.7: Experimental Balance

	Model 1
Intercept	0.553*
	(0.083)
Two Party Vote Share (2016)	0.000
	(0.000)
Missingness Indicator (2016)	0.083
	(0.158)
Two Party Vote Share (2014)	−0.000
	(0.000)
Missingness Indicator (2014)	0.096
	(0.080)
Two Party Vote Share (2012)	−0.000
	(0.000)
Missingness Indicator (2012)	−0.122
	(0.087)
R^2	0.035
Num. obs.	1096
N Clusters	169

$^*p < 0.05$. CR2 Cluster-robust standard errors are in parentheses.

F Aggregating to the ZIP-code level

As described in the main text, we block-randomized 210 ZIP-codes to receive treatment advertisements or not. In the main text, we restrict our analysis to those voting precincts that were wholly contained within treatment or control zip codes in order to sidestep the question of what to do with precincts that span multiple zip codes. The strategy in the main text estimates the causal effects of treatment on single zip code precincts without bias, but it does end up omitting some zip codes entirely.

In this section, we take an alternative approach to the problem of precincts that overlap zip codes. In particular, we determine what fraction of each precinct lies within each zip code using voter file data. We then apportion the number of Democratic and Republican votes earned in each zip code according to those fractions. For example, if 30% of a precinct lies within a zip code, then we multiply that precincts' Democratic and Republican vote totals by 0.3 and add that number to the zip code's total count.

F.1 ZIP-level balance

We first show that this approach generates experimental balance in Table F.8. A joint significance test returns a p-value of 0.328.

Table F.8: Experimental Balance (Zip code level)

	Model 1
Intercept	0.500^{*}
	(0.039)
Two Party Vote Share (2016)	0.000
	(0.000)
Two Party Vote Share (2014)	-0.000
	(0.000)
Two Party Vote Share (2012)	0.000
	(0.000)
R^2	0.009
Num. obs.	210

$^{*}p < 0.05$. HC2 Cluster-robust standard errors are in parentheses.

In Tables F.9, F.10, and F.11, we show that analyzing our experiment at the zip code level does not change our main conclusions. Whether we adjust for covariates or not, or whether we disaggregate by the two treatment videos or not, we find very small effects of treatment that cannot be distinguished from zero.

F.2 ZIP-level estimate

Table F.9: Effects on Vote Share (Zip code level)

	Model 1	Model 2	Model 3	Model 4
Any Treatment Video	0.0099	0.0015		
	(0.0145)	(0.0088)		
Treatment Video 1			0.0167	0.0055
			(0.0178)	(0.0086)
Treatment Video 2			0.0030	-0.0025
			(0.0181)	(0.0128)
Missingness Indicator (2016)		0.7451*		0.7449*
		(0.0593)		(0.0592)
Two Party Vote Share (2014)		0.0476*		0.0463
		(0.0235)		(0.0238)
Missingness Indicator (2014)		4.8184*		4.6876*
		(2.3429)		(2.3766)
Two Party Vote Share (2012)		-0.0031		-0.0006
		(0.0444)		(0.0453)
Missingness Indicator (2012)		-0.2794		-0.0337
		(4.4175)		(4.5047)
Intercept	0.4652*	0.1101*	0.4652*	0.1097*
	(0.0099)	(0.0210)	(0.0099)	(0.0209)
R^2	0.0022	0.6607	0.0044	0.6614
Num. obs.	210	210	210	210

$^*p < 0.05$. HC2 robust standard errors are in parentheses.

Table F.10: Effects on Vote Margin (Zip code level)

	Model 1	Model 2	Model 3	Model 4
Any Treatment Video	68.5920	−77.2672		
	(269.4097)	(156.3489)		
Treatment Video 1			9.3438	−132.2842
			(275.9061)	(177.3083)
Treatment Video 2			127.8401	−23.5587
			(384.5197)	(211.4121)
Missingness Indicator (2016)		0.5251*		0.5242*
		(0.0550)		(0.0554)
Two Party Vote Share (2014)		0.1689*		0.1703*
		(0.0567)		(0.0576)
Missingness Indicator (2014)		−255.9467*		−246.7978
		(113.4945)		(130.5084)
Two Party Vote Share (2012)		−0.2590*		−0.2587*
		(0.1175)		(0.1173)
Missingness Indicator (2012)		91.8170		112.0516
		(136.4758)		(139.7764)
Intercept	−408.7512*	203.0807	−408.7512*	201.2060
	(183.9857)	(136.3052)	(183.9857)	(136.3445)
R^2	0.0003	0.6543	0.0008	0.6547
Num. obs.	210	210	210	210

$^*p < 0.05$. HC2 robust standard errors are in parentheses.

Table F.11: Effects on Turnout (Zip code level)

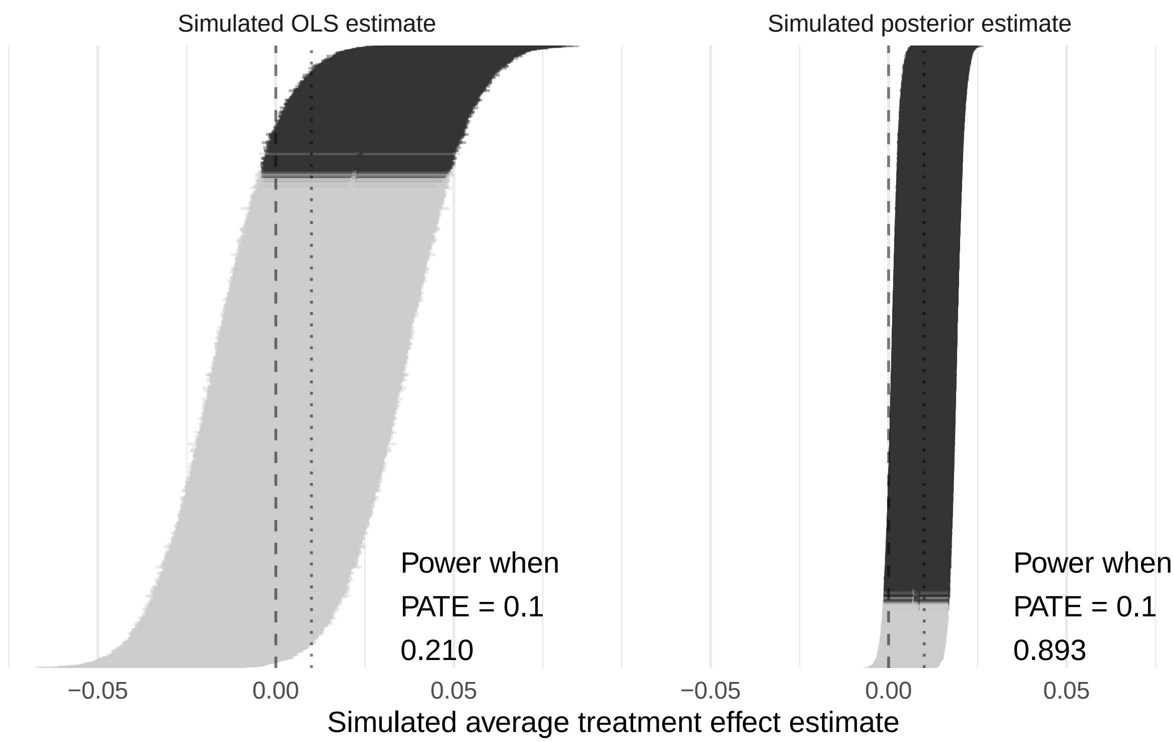
	Model 1	Model 2	Model 3	Model 4
Any Treatment Video	−89.8289	195.6585		
	(953.4329)	(526.4417)		
Treatment Video 1			−439.0957	1018.5754
			(1125.3363)	(685.7638)
Treatment Video 2			259.4380	−590.5218
			(1182.4407)	(631.3777)
Missingness Indicator (2016)		0.5533*		0.5594*
		(0.0735)		(0.0716)
Two Party Vote Share (2014)		0.3027*		0.3062*
		(0.0840)		(0.0817)
Missingness Indicator (2014)		−711.4928		−810.5134
		(393.1901)		(664.3380)
Two Party Vote Share (2012)		−0.4790*		−0.4852*
		(0.1361)		(0.1344)
Missingness Indicator (2012)		−1718.2381*		−1990.3628*
		(865.2703)		(924.3390)
Intercept	7811.4898*	2332.4258*	7811.4898*	2292.1032*
	(701.0554)	(1138.7188)	(701.0554)	(1112.1093)
R^2	0.0000	0.6781	0.0013	0.6848
Num. obs.	210	210	210	210

$^*p < 0.05$. HC2 robust standard errors are in parentheses.

G Design diagnosis

In this section, we describe a design diagnosis (Blair et al., 2019) that evaluates the statistical power of our study. Using the `DeclareDesign` package for R (Blair et al., 2018), we simulated our research design using the exact block randomization procedure described in the main text. We drew simulated treatment effects from the distribution implied by similar studies conducted to-date: a normal distribution centered at 0.01 with a standard deviation of 0.005. In each simulation, we calculated the OLS we would have obtained from the raw data and also the posterior we would have calculated when combining the prior and the data. Figure G.2 shows that own its own, the power of our study is low, at 21.0%. However, when we combine our estimate with previously-available information, power increases dramatically, to 89.3%. The full design declaration and diagnosis code is available in the replication archive.

Figure G.2: Design diagnosis of two estimators



H Pre-analysis Plan

An anonymized version of our preanalysis plan is appended to this document.

Florida Pre-Roll Ad Experiment: 2018 Midterm Elections

PRE-ANALYSIS PLAN

November 19, 2018

Introduction

Prior to the November 2018 general elections, a randomized controlled trial (RCT) was conducted to evaluate the effect of on-line pre-roll ads designed to encourage voting for Democratic candidates in four Florida congressional districts.

The current study contributes to the small literature on digital ads, which has focused on static Facebook ads (e.g., Broockman and Green 2012 https://link.springer.com/article/10.1007/S11109-013-9239-Z). To our knowledge, this is the first RCT to evaluate the effects of pre-roll video ads.

Hypotheses

The principal hypothesis is that precincts assigned to ads will show higher Democratic vote share than control precincts. Since the pro-Democratic ads do not mention any specific office and could conceivably affect all partisan races, our outcome measure will be the average vote share for House, Senate, and Governor. A one-tailed test will be used to assess this directional hypothesis (i.e., any of the ads outperforms control) at alpha = .05.

Sometimes Florida releases ballot-image data on specific ballots, which would enable us to test whether our ads increased straight-ticket balloting for Democratic candidates. We will perform this test if these data become available.

Although we are not principally concerned about the superiority of one of the two ads, we will conduct a comparison using the regression model below (omitting the control group), conducting a two-sided test.

We do not expect the ads to increase turnout, but precinct-level vote totals will be used to check.

Sample

The unit of random assignment was the zip code, from a population of zip codes that were associated with four congressional districts. These 210 zip codes and districts are listed in the accompanying randomization script.

Random Assignment of Treatment

The accompanying randomization script shows how the block-random assignment was conducted. The blocking variable was the number of precincts per zip code; we blocked on the number of precincts because the unit of analysis is the precinct, and this is in effect a cluster-randomized trial with clusters of unequal size. Blocking on cluster size maintains the approximate unbiasedness of unweighted regression, and the randomization procedure broke ties randomly in cases where the number of observations within blocks was not divisible by 3 so that treatment probabilities are constant for all blocks. The table below reproduces the assignment to control, video 1, or video 2.

```
##    treatment
## Z   control video_1 video_2
##   0     104       0       0
##   1       0      53      53
```

Intervention

The two ads used in this study may be found in the attached supplementary materials. The ads were deployed at the same time (starting October 25), and each of the ad was run until its budget of $30,000 was exhausted. Viewers shown the ad could not get past the ad until they had watched at least three seconds (some left the webpage rather than wait three seconds); some voluntarily watched the entire ad, and some clicked on the ad. Information from the ad vendor indicates that the two ads had the following clicks, full views, and three-second views:

	Clicks	Entire views	3 Sec views
First Ad	24832	70266	611732
Second Ad	17185	47647	516746

Overall, the first ad was more effective at eliciting clicks, full viewing, and 3-second viewing.

Data and Outcome Measures

Our primary outcome measures will be obtained at the precinct level from the certified SOS results that are scheduled to be released on November 20th. This PAP will be filed beforehand.

Method for Estimating Average Treatment Effects

Since the experiment is blocked by the number of precincts per zip code, but the assignment probabilities do not vary across blocks, there is no need to control for block.

Our regression models of average vote share across three partisan offices on treatment assignment (scored 1 if a precinct received either ad) will include covariates from all past federal general elections since the last redistricting cycle, with indicator variables for missingness in the case of uncontested races.

We will report 95% confidence intervals for the average treatment effect, using a margin of error equal to the estimated standard error from the covariate-adjusted OLS regression multiplied by the appropriate critical value from the *t*-distribution. Hypothesis testing will be conducted using randomization inference.

To assess robustness, we will also report a simple regression with only a treatment indicator, omitting covariates. We expect these results to be similar but less precisely estimated due to the exclusion of prognostic covariates. When interpreting the results, we will rely primarily on the covariate-adjusted estimates.

Covariates in the Event of Boundary Changes

Consistent with the [ANONYMIZED] SOP, we plan to include the covariates mentioned above (lagged vote share) in our estimation in order to produce a more precise estimate of the treatment effect. In the event that experimental precincts are newly formed, and therefore voter turnout in previous elections is unavailable, we will use an average of the previous precincts' voting histories as covariates in the precinct-level analysis.

Default Procedures for Decisions Not Explicitly Specified

For any decisions not explicitly specified in this pre-analysis plan, we plan to follow the "standard operating procedure" document of [ANONYMIZED] which can be found on [ANONYMIZED].

References

Blair, Graeme, Jasper Cooper, Alexander Coppock, and Macartan Humphreys. 2019. "Declaring and Diagnosing Research Designs." *American Political Science Review* 113 (3): 838–859.

Blair, Graeme, Jasper Cooper, Alexander Coppock, Macartan Humphreys, and Neal Fultz. 2018. *DeclareDesign: Declare and Diagnose Research Designs.*