A Field Experimental Test of Vote Swapping

Alexander Coppock*

March 30, 2018

Abstract

Vote swapping platforms encourage third party supporters in battleground states to swap votes with major party supporters in nonbattleground states. In the 2016 US Presidential Elections, battleground state supporters of Gary Johnson, Evan McMullin, and Jill Stein who nevertheless preferred Hillary Clinton to Donald Trump were matched with Clinton supporters in nonbattleground states. These (legal) exchanges are puzzling because they should have small effects on vote choice: third party supporters willing to vote for Clinton in the presence of a swap should be likely to vote for Clinton in the absence of a swap. This study provides the first field experimental test of vote swapping. Third party supporters seeking swap partners on TrumpTraders.org were randomly assigned to be matched or not matched. In the control group, 25% report voting for Clinton compared with 57% in the treatment group, for an Average Treatment Effect estimate of 32 percentage points.

---

*Alexander Coppock is Assistant Professor of Political Science, Yale University. This study was approved by the Yale University IRB, Protocol Number 2000021864. This study was conducted in collaboration with TrumpTraders.org; the author received no compensation for this report, nor were its contents subject to approval by TrumpTraders.org. This study was registered at EGAP.org prior to the collection of outcome data.

This study received IRB approval from [university withheld] and was conducted in collaboration with TrumpTraders.org. The author received no compensation for this report, nor were its contents subject to approval by TrumpTraders.org. The design and analysis strategies were registered at EGAP.org prior to the collection of outcome data. The preanalysis plan and the outcome survey are included in the online appendix to this paper.

Vote swapping has its modern origins in the 2000 U.S. Presidential Election. Ralph Nader, a third party candidate on the left with virtually no chance to win the election, drew support from voters who ostensibly would have preferred Al Gore, the Democrat, to George W. Bush, the Republican. Because the electoral college follows a logic of geographic representation, the extent to which a vote for Nader "spoils" the election for Gore varies state-to-state. In battleground states like Florida, if even a small fraction of Nader supporters had instead voted for Gore, the election outcome would have been different; in safe states like California or Texas, Nader supporters were free to "vote their conscience" with no effect on the election outcome. Nader supporters in battleground states therefore had an incentive to "swap votes" with Gore supporters in nonbattleground states. Encouraged by an article published in Slate by Jamie Raskin (then a Professor of Law at American University, now a U.S. Representative from Maryland), websites like voteswap2000.com, nadertrader.com, PresidentGore.com, tradevotes.com, votetrader.org, and VotExchange2000.com appeared in short order, only to shut down a few weeks later in the face of legal challenges (Raskin, 2000, 2004).

This article considers the causal effect of vote swapping on the vote choice of third party supporters in battleground states. On the one hand, the effect of vote swapping would appear to be obviously and trivially large. Because the vote swap is (admittedly nonbinding) agreement in which the Nader supporter promises to vote for Gore on the condition that the Gore supporter votes for Nader, the swap should have a nearly 100 percentage point effect on the Nader supporter's probability of voting for Gore. On the other hand, we must also entertain the possibility that vote-swapping Nader supporters would have voted for Gore anyway.

Third party supporters who are willing to participate in swaps are one of two types, which I will label "expressive" or "strategic." Strategic voters (Downs, 1957; Blais and Nadeau, 1996) are those who, in the absence of a swap, would vote for the "lesser of two evils" among the major party candidates. Expressive voters (Brennan and Buchanan, 1984; Schuessler, 2000) are those who, in the absence of a swap, would vote for a candidate with no chance to win. The presence of a swap does not change the vote choice of strategic voters but does make them better off in the sense that their utility is increasing in the vote total of their most preferred candidate. By contrast, a

successful swap does change the vote of the expressive voters. In the presence of a swap, expressive voters are able to vote strategically while reaping the expressive gains of a vote for the third party candidate.

These third party voters who vote *expressively* if swaps were not available but who vote *strategically* if swaps were available are a puzzling group. Their vote choice appears to be on a knife's edge in the sense that the electorally irrelevant vote of a stranger in a nonbattleground state makes the difference. If we grant that the utility derived from the electoral behavior of others is small, especially relative to the large difference in utility would-be vote-swappers perceive depending on whether Bush or Gore wins, we would expect this puzzling group to be small. Put differently, we would expect the share of expressive voters among those willing to swap to be much smaller than the share of strategic voters.

This brief sketch of the incentives faced by vote-swappers leads to two predictions. Vote swapping is a political phenomenon that (1) should occur in elections with plausible third party "spoilers" and geographic differences in the value of a vote and (2) should have small average effects on the vote choice of those who participate.

The first prediction – vote swapping should occur when the conditions are right – is partially demonstrated by the existence of vote swapping websites in a number of countries (votepair.ca in Canada, voteswap.org in the UK). The second prediction – small effects of vote swapping on those who would swap if given the chance – is difficult to demonstrate with observational data. While it may be that Nader supporters who did and did not participate in swaps behaved differently at the ballot box, we cannot be sure if Nader supporters who *select into* swapping were just more willing to vote for Gore than those supporters who chose not to participate in swaps. In order to estimate the effect of vote swapping on the swappers, we need a source of random variation in access to swaps.

The bulk of the extant scholarship on vote swapping considers its legality, constitutionality, or morality (Randazza, 2001; Sisgold, 2001; Rushing, 2002; Sanders, 2004). A small group of papers develops the formal properties of vote swapping (Hartvigsen, 2006; Bervoets and Merlin, 2012; Bervoets et al., 2015; Bervoets and Merlin, 2016). A section of (Reeher et al., 2009) is devoted to

an in-depth account of vote swapping websites in the 2000 election. No previous research attempts to empirically assess the effect of vote swapping on the vote choice of those who participate in swaps.

In a field experiment conducted with a vote swapping website (TrumpTraders.org) during the 2016 Presidential Election, third party voters who signed up to be matched with nonbattleground state voters were randomly assigned to one of two groups. The treatment group continued with the matching process as usual. If an eligible nonbattleground state voter could be located, then the voters chatted via Facebook to make arrangements and assurances. The TrumpTraders.org website provided no further enforcement or accountability mechanisms, so vote swappers had to head to the polls trusting that their partners would hold up their sides of the bargain. In the control group, the matching process was suppressed at the moment the voter signed up to be matched. From the perspective of the user, the matching process appeared to fail for logistical reasons (a nonbattleground state voter could not be found).

To preview the results, I find conclusive evidence that contradicts the prediction of small effects of swaps on vote choice. Compared to those in the control group, voters assigned to the vote swap treatment report voting for Hillary Clinton more by 32 percentage points.

Theory

In this section, I provide a brief formalization of vote swapping, similar to the one presented in Hartvigsen (2006). Without loss of generality, I will set up the problem using candidate names from the 2016 election, the setting of the field experiment reported here.

A vote swap requires two individuals. Individual i is the voter in the battleground state who prefers a third party candidate (Gary Johnson). Individual j is the voter in the nonbattleground state who prefers the major party candidate that i would prefer among the major party candidates if forced to choose; i.e., both i and j prefer Hillary Clinton to Donald Trump. Individuals i and j can vote for one of three candidates or abstain. Individual i's utility function is some function of the winner of the election (W), i's vote choice (V_i), and j's vote choice (V_j).

I assume the following properties of individual i's utility function:

- $U_i(W = Johnson) > U_i(W = Clinton) > U_i(W = Trump)$, holding V_i and V_j constant.

- $U_i(V_i = Johnson) > U_i(V_i = (Clinton, Abstain)) > U_i(V_i = Trump)$, holding W and V_j constant. Regardless of who wins or what j does, i would prefer to vote for Johnson than Clinton or Trump for expressive reasons. The relative ranking of $U_i(V_i = Clinton)$ and $U_i(V_i = Abstain)$ is unclear ex ante. Individual i may experience psychological disutility from not voting at all but also disutility from voting for a trully disliked candidate; it is unclear which disutility dominates.

- $U_i(V_j = Johnson) > U_i(V_j = (Clinton)) > U_i(V_j = (Abstain)) > U_i(V_j = Trump)$, holding W and V_i constant. Presumably, individual i would like it most if j voted for Johnson and marginally prefers if j votes rather than stays home, because i does not bear j's costs of voting. This preference may stem from a hope that j's vote for Johnson would put him over a threshold for funding or debate participation.

Individual i only faces a dilemma if she perceives V_i to be consequential, i.e., she must believe that her vote matters, either in the sense of pivotality or in the sense that she anticipates a large regret if Trump wins and she did not vote for Clinton (Ferejohn and Fiorina, 1974). A slight elaboration of the model would allow i to be uncertain about how consequential her vote is. The less consequential she perceives her vote to be, the more likely she is to vote for Johnson (and not participate in a swap). In the absence of a swap, she has to compare $U_i(W = Trump, V_i = Johnson, V_j = Clinton)$ to $U_i(W = Clinton, V_i = Clinton, V_j = Clinton)$. Since i prefers Clinton to Trump, the only circumstance under which she should vote for Johnson is if the positive difference in expressive utility of voting for Johnson over Clinton exceeds the negative difference in utility of a Trump versus a Clinton presidency. This comparison distinguishes the strategic from the expressive voters among third party supporters in the absence of a swap.

The possibility of vote swaps may help resolve the dilemma. Individual i now has to compare $U_i(W = Trump, V_i = Johnson, V_j = Clinton)$ to $U_i(W = Clinton, V_i = Clinton, V_j = Johnson)$. The disutility that i receives from voting for Clinton is now offset by the utility of individual j voting for Johnson.

Now let us consider whether the possibility of vote swapping is likely to change i's vote choice. In the absence of vote swapping, some third party voters will "hold their noses" and vote for Clinton. Given the opportunity to swap, however, these voters will take it. As they were going to vote for Clinton anyway, it is all to the good if individual j increments Gary Johnson's vote total by one. The only voters for whom vote swapping makes a difference are therefore those who, in the absence of a swap, would resolve their dilemma by voting for Johnson and stomaching a Trump win, but if a swap were available, would vote for Clinton. For those people, the following inequalities must hold: $U_i(W = Clinton, V_i = Clinton, V_j = Johnson) > U_i(W = Trump, V_i = Johnson, V_j = Clinton) > U_i(W = Clinton, V_i = Clinton, V_j = Clinton)$.

In order for vote swapping to make any difference, it must therefore be true that the difference in utility that i receives from individual j voting for Johnson over Clinton *exceeds* the difference in utility of voting for Johnson and electing Trump versus voting for and electing Clinton. To the extent that we believe that very few people have utility functions with this property, we would expect vote swapping schemes to have small average effects on who third party supporters vote for.

This hypothesis – small effects of vote swapping on vote choice because those who select into swaps are already resigned to voting for Clinton – was the main hypothesis preregistered in advance of the experiment. Alternatives include the "faithless swapper" prediction according to which swaps would have no effect because in the absence of swaps, Johnson voters vote for Johnson for expressive reasons and in the presence of swaps, renege on their promise to vote for Clinton because $U_i(V_i = Johnson, V_j = Johnson) > U_i(V_i = Clinton, V_j = Johnson)$. Theories that generate predictions of a positive effect of swaps generally require that (a) in the absence of a swap, third party voters vote expressively for their preferred candidate but that (b) in the presence of a swap derive utility from faithfully participating in the swap.

Design

The subjects of the experiment were 4,500 third party supporters who registered to swap votes on TrumpTraders.org in the weeks preceding the 2016 election. Of these, 500 were assigned to

control and the remainder were assigned to treatment using complete random assignment.[1] Because subjects arrived at the site in a stream, blocking on pre-treatment covariates was not possible.

Subjects in the treatment group proceeded through the normal TrumpTraders.org process. If the site administrators found an eligible match, subjects were later connected via Facebook with a Clinton voter in a nonbattleground state. Subjects in the control group were simply not matched. This design allows us to identify the effect of being matched among those third party supporters in battleground states who are willing, in principle, to swap votes with a nonbattleground state voter. This group of voters is unlikely to be representative of third party supporters in general, as evidenced by their interest in swapping votes.

All subjects were invited via email to participate in an online survey after the election.[2] Of the 4,500 subjects in the experiment, 218 responded, for a 4.8% unconditional response rate. The survey software from which the email invitations were sent reports 923 bounced emails, suggesting a 6.1% response rate among those who actually received an email. Response rates in the mid single digits are not uncommon for survey invitations by email (e.g., Coppock et al., 2018). Under the assumption that subjects' probability of response does not depend on the treatment assignment, we can use the survey data to estimate the average treatment effect among those who would respond regardless of treatment assignment – so-called "Always-Reporters" (Gerber and Green, 2012, Chapter 7). Consistent with the Always-Reporters assumption, the difference in response rates across treatment groups is nonsignificant ($p = 0.66$)

The survey first assessed a series of background characteristics (7-point party identification, Race/Ethnicity, Age, Gender, 7-point ideology, and 7-point education). An omnibus randomization

---

[1]The preanalysis plan specified a total of 5,000 subjects, 4,500 to be assigned to treatment and 500 held out as controls, but due to an administrative miscommunication, only 4000 subjects were assigned to treatment.

[2]I also preregistered that I would estimate the effects of treatment on turnout measured by the voter file. Because I only had access to first name, last name, and state, the match to the voter file was abysmal, so I omit that analysis entirely. However, the survey results suggest that very large proportions of both experimental groups voted in the 2016 election, with no difference across treatment and control. This finding consistent with expectations of very high turnout among this very politically engaged subset of Americans.

check[3] finds that the distribution of covariates across treatment and control is consistent with random assignment ($p = 0.86$).

The main outcome, vote choice, asked "Who did you vote for? (If you participated in a successful vote swap, who did you personally vote for?) [Donald Trump, Hillary Clinton, Evan McMullin, Jill Stein, Gary Johnson, Other]" In accordance with the preanalysis plan, this question was recoded into three binary variables, corresponding to voting for Trump, Clinton, or a third party candidate. The very small number of subjects (8) who reported not voting in the 2016 election were coded as 0 on all three binary outcomes. A manipulation check question asked "Were you able to successfully swap votes? [Yes, No]." In the invitation to the survey and at three times throughout the survey, respondents were assured that their responses would be fully anonymized, at least partially mitigating the measurement error due to social desirability bias or demand effects.[4]

The survey also included some questions intended to probe the mindset of vote swappers. An open-ended question asked respondents what attracted them to try and swap votes and if they had any concerns. Many respondents took the opportunity to express their dissatisfaction with the major party candidates and articulated the basic logic of vote swapping. Some respondents expressed that they did not fully trust the person with whom they were swapping but thought the swap was nevertheless worth trying. After the open-ended question, subjects were asked, "Do you think that people on sites like TrumpTraders.org can be trusted to swap votes if they say they will? [Yes, No]" Approximately half (54%) of the sample answered yes to this question, and responses did not appear to depend on treatment assignment ($p = 0.69$).

[3]Using randomization inference, the observed F-statistic from a regression of the treatment indicator on the covariates is compared to the randomization distribution of F-statistic. Covariate-by-covariate balance is presented in the appendix, with all differences nonsignificant.

[4]See Mummolo and Peterson (2018) for some evidence that demand effects in survey contexts may be small.

Results

The main results are presented in Figure 1 (corresponding regression tables are presented in the appendix). Difference-in-means estimates are plotted as filled circles and covariate-adjusted estimates as triangles. I use the adjustment proposed in Lin (2013), which amounts to interacting treatment with all covariates then calculating the difference in predicted treatment and control means and has many desirable statistical properties.[5] I will focus on the adjusted estimates because they are slightly more precise than the unadjusted estimates. The substantive interpretation of results does not depend on this choice.

The vote swapping treatment increased self-reported vote choice for Hillary Clinton by 32 percentage points (SE: 8 points) and decreased voting for a third party candidate by 35 points (SE: 9 points). We observe no effect on voting for Donald Trump or on turnout.

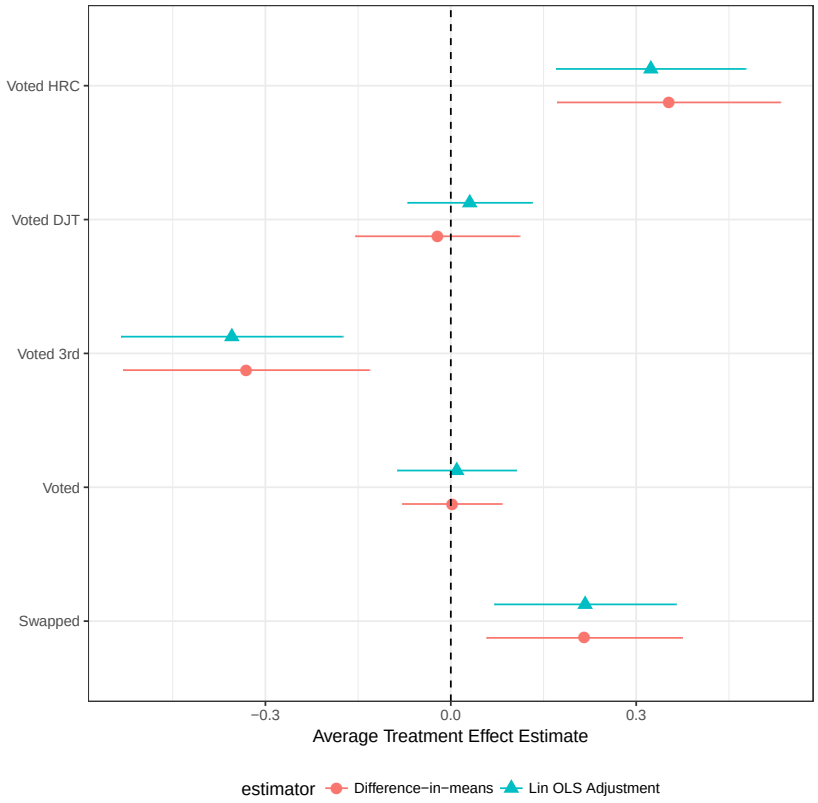
The experimental results paint a relatively clear picture. In the absence of a vote-swapping program, approximately one-quarter to one-third of this type of voter would have ended up voting for Clinton anyway. Contrary to expectations, approximately one-third of these voters changed their vote from a third party candidate to Clinton as a result of the swap.

More surprising still is that the 32 percentage point average treatment effect estimate on Clinton vote choice may be an underestimate of the true effectiveness of the TrumpTrader program because of the high rates of noncompliance. Only 37% of the treatment group report having successfully swapped (compared with 15% of the control group, who may be misremembering, be misreporting, or have found an alternative method for swapping votes). Under the tenuous assumption that the treatment only affects vote choice through a successful match, the implied Complier Average Causal Effect on Clinton vote is $32/0.37 = 86.5$ percentage points. The true average treatment effect likely lies somewhere between the two values, but is conclusively not zero.

---

[5]Some analysts prefer to estimate logit or probit regressions when the outcome is binary. As is well-known (see Gerber and Green (2012, Chapter 2) for a textbook treatment), the difference-in-means is unbiased for the ATE regardless of the outcome space and OLS with covariate adjustment is consistent for the ATE. The substantive results are unchanged when these data are analyzed via logit or probit.

Figure 1: Average Treatment Effects of Vote Swapping Treatment



Discussion

This study presents the first empirical estimates of the effect of vote swapping on vote choice, finding that being assigned to swap votes increases voting for Clinton by 32 percentage points. Stated another way, this treatment caused a 128% increase in voting for Clinton over a base of 25% in the control group. By any measure, this treatment effect estimate is substantial, especially in comparison to the very small campaign effects documented in a recent meta-analysis (Kalla and Broockman, 2018). The large effect of vote swapping on vote choice is surprising, given the intuition that the group of people who select into signing up to swap votes were likely *already* resigned to voting for Clinton. Indeed, the positive rate of voting for Clinton in the control group indicates that this expectation applies to some, but conclusively not all, voters in this sample. These results suggest that vote swapping programs may be an effective campaign tactic to persuade third party voters to vote for major party candidates.

Given these surprising results, we are left to speculate about why vote swapping is so effective. It could be that indeed, these voters prefer a Trump presidency and voting for Johnson to a Clinton presidency and voting for Clinton. That explanation seems implausible, mostly because of the large difference in outcomes depending on who is president contrasted with the small gesture of voting for a candidate who will lose with certainty. Another explanation might be that voters know that they are not pivotal, they participate in swaps for the consumption value, and then feel a mild sense of obligation to their swap partner to vote for Clinton. Finally, it could be that swapping allows these voters to give themselves "permission" to violate a norm of sincere voting through an interstate moral offset. The open-ended survey responses provide some weak evidence in support of each of these explanations, though the precise set of mechanisms at play is likely different for each person.[6] Future experiments should probe each of these possible mechanisms through direct manipulation at the time of the swap.

[6]The full text open-ended responses are available in the replication archive for this study.

References

Bervoets, Sebastian and Vincent Merlin. 2012. "Gerrymander-proof Representative Democracies." *International Journal of Game Theory* 41(3):473–488.

Bervoets, Sebastian and Vincent Merlin. 2016. "On Avoiding Vote Swapping." *Social Choice and Welfare* 46(3):495–509.

Bervoets, Sebastian, Vincent Merlin and Gerhard J. Woeginger. 2015. "Vote Trading and Subset Sums." *Operations Research Letters* 43(1):99–102.

Blais, André and Richard Nadeau. 1996. "Measuring Strategic Voting: A Two-step Procedure." *Electoral Studies* 15(1):39 – 52.

Brennan, Geoffrey and James Buchanan. 1984. "Voter Choice: Evaluating Political Alternatives." *American Behavioral Scientist* 28(2):185–201.

Coppock, Alexander, Emily Ekins and David Kirby. 2018. "The Long-lasting Effects of Newspaper Op-Eds on Public Opinion." *Quarterly Journal of Political Science* .

Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York: Harper and Row.

Ferejohn, John A. and Morris P. Fiorina. 1974. "The Paradox of Not Voting: A Decision Theoretic Analysis." *American Political Science Review* 68(2):525–536.

Gerber, Alan S. and Donald P. Green. 2012. *Field Experiments: Design, Analysis, and Interpretation*. New York: W.W. Norton.

Hartvigsen, David. 2006. "Vote Trading in Public Elections." *Mathematical Social Sciences* 52(1):31–48.

Kalla, Joshua L. and David E. Broockman. 2018. "The Minimal Persuasive Effects of Campaign Contact in General Elections: Evidence from 49 Field Experiments." *American Political Science Review* 112(1):148–166.

Lin, Winston. 2013. "Agnostic Notes on Regression Adjustments to Experimental Data: Reexamining Freedman's Critique." *Annals of Applied Statistics* 7(1):295–318.

Mummolo, Jonathan and Erik Peterson. 2018. "Demand Effects in Survey Experiments: An Empirical Assessment." *Unpublished Manuscript* .
URL: *https://scholar.princeton.edu/jmummolo/publications/demand-effects-survey-experiments-empirical-assessment*

Randazza, Marc J. 2001. "The Constitutionality of Online Vote Swapping." *Loyola of Los Angeles Law Review* 34:1297.

Raskin, Jamin B. 2000. "Nader's Traders: How to Save Al Gore's Bacon by Swapping Votes on the Internet." *Slate Magazine* .

Raskin, Jamin B. 2004. *Overruling Democracy: the Supreme Court versus the American People.* Psychology Press.

Reeher, Grant, Steve Davis and Larry Elin. 2009. *Click on Democracy: the Internet's Power to Change Political Apathy into Civic Action.* Westview Press.

Rushing, John M. 2002. "Vote Swapping and Free Speech: Voice, Politics, and Choice." *Texas Forum on Civil Liberties & Civil Rights* 7:73.

Sanders, Anthony B. 2004. "In Defense of Vote Buying: How Nader Traders Can Defeat Rent Seeking." *Journal of Pubic Law and Policy* 26:43–72.

Schuessler, Alexander A. 2000. "Expressive Voting." *Rationality and Society* 12(1):87–119.

Sisgold, Jesse. 2001. "Vote-Swapping Over the Internet: Free Speech or Voter Corruption." *Hastings Communications and Entertainment Law Journal* 24:149–168.

Appendix: Regression Tables

Table 1: Balance Table

	7-pt Party ID	White	Age	Female	7-pt Ideology	7-pt Education	Responded To Survey
Swap Treatment	−0.37	−0.03	−0.12	−0.01	−0.24	0.24	−0.00
	(0.28)	(0.07)	(0.25)	(0.11)	(0.30)	(0.28)	(0.01)
Constant (Control)	3.69***	0.88***	3.73***	0.54***	3.42***	4.42***	0.05***
	(0.26)	(0.06)	(0.23)	(0.10)	(0.28)	(0.27)	(0.01)
Num. obs.	218	218	218	218	218	218	4500

HC2 robust standard errors are in parentheses.

Table 2: Unadjusted Average Treatment Effect Estimates

	Voted HRC	Voted DJT	Voted 3rd	Voted	Swapped
Swap Treatment	0.35***	−0.02	−0.33**	0.00	0.22**
	(0.09)	(0.07)	(0.10)	(0.04)	(0.08)
Constant (Control)	0.23**	0.12	0.65***	0.96***	0.15*
	(0.08)	(0.06)	(0.10)	(0.04)	(0.07)
Num. obs.	218	218	218	218	218

HC2 robust standard errors are in parentheses. Models do not adjust for pre-treatment covariates.

Table 3: Covariate-Adjusted Average Treatment Effect Estimates

	Voted HRC	Voted DJT	Voted 3rd	Voted	Swapped
Swap Treatment	0.32***	0.03	−0.35***	0.01	0.22**
	(0.08)	(0.05)	(0.09)	(0.05)	(0.07)
Constant (Control)	0.25***	0.07	0.68***	0.95***	0.15*
	(0.07)	(0.05)	(0.08)	(0.05)	(0.07)
Num. obs.	218	218	218	218	218

HC2 robust standard errors are in parentheses. Models adjust for 7-pt Party ID, White/nonwhite, Age, Female/nonfemale, 7-pt Ideology, and 7-pt Education using the adjustment described in Lin (2013).