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ORIGINAL PAPER

Promoting the Youth Vote: The Role of Informational Cues and Social Pressure

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Abstract
Young voters, including college students, turnout less than older citizens—particularly in non-presidential elections. We examine two promising intervention strategies in the 2018 midterm elections: information cues and social pressure. Additionally, we consider whether voting information and social pressure to vote spread to others through social ties. Using a large-scale field experiment involving sections of a university-wide first-year writing seminar, we examine whether informational and social pressure presentations are effective strategies for increasing college student voter turnout. Furthermore, by linking each student in our study to their roommates, we assess whether there were spillover effects from the interventions. Though the treatments did not alone affect turnout, we find positive effects from classroom treatments among first-year students who were registered to vote prior to the presentations. Additionally, we find positive peer spillover effects for turnout from the social pressure treatment when the roommate of the treated student was previously registered to vote.

Keywords Voting behavior · Youth turnout · Elections · Experiments · GOTV

Introduction

Young eligible voters, including college students, have the potential to cast decisive votes in American elections and to reshape the demographic composition of the American electorate (Medenica 2018). By 2018, millennials were the largest share

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of the voting-eligible population. By 2020, members of Generation Z (birth years 1997–2012) are projected to comprise 10 percent of all eligible voters (Cillufo and Fry 2020). Yet voter turnout rates for young people are much lower than for older citizens. The gap in turnout between voters aged 18–29 and those over 60 averages 28 percent in presidential elections and is wider still in midterm and off-year elections (e.g. Holbein and Hillygus 2016,2020; Fraga and Holbein 2020). These patterns are troubling because young voters' policy preferences—which differ systematically from older citizens—are under-represented in electoral politics (Maniam and Smith 2017). Furthermore, voting is habitual, often established at a young age. Thus, the best way to increase overall voter participation in the U.S. is to focus on younger people (Hart and Youniss 2018; Coppock and Green 2016; Franklin 1945; Plutzer 2002).

Low participation rates among young voters are often explained by two common assumptions: (1) young people are not interested in politics, and (2) young people are less informed than older people about how the government works. Yet both assumptions tend to mischaracterize the problem. First, public opinion surveys consistently show that the vast majority of young people *are* interested in politics, with interest levels mirroring those of older generations (Holbein and Hillygus 2020). Second, while traditional civic education can produce gains in political knowledge for young people, it has little effect on youth voter turnout (Hart and Youniss 2018; Gibson and Levine 2003). However, young people do exhibit "a persistent gap between turnout intentions and turnout behavior;" in other words, young people are much less likely to follow through on their intent to vote than older citizens (Holbein and Hillygus 2020). If young people are already interested in politics and receiving some form of civic education, but many are not following through on their intent to vote, what strategies would effectively improve voter participation and spillover to their peers? More specifically, can targeted educational efforts increase voter turnout among college students?

A college campus is well-suited for fielding a study to help answer these questions. In what follows, we report the results of an experimental intervention to mobilize college students at a large Midwestern university during the 2018 midterm elections. Previous research has found that schools are important venues for political socialization (Aggeborn et al 2020; Neundorf and Smets 2017), and that classroom interventions offer a promising route to mobilizing turnout (Bennion and Nickerson 2016). Although college students are not a representative population of young people, the structured interactions that occur on college campuses, particularly in classrooms and dorm rooms, enable a more controlled study. In college, young people experience political socialization away from their families through peer interactions inside and outside the classroom (Campbell 2008; Beck and Jennings 1991), and peer interactions in college can have lasting impacts on young people's civic participation (Klofstad 2007,2015). There have been surprisingly few efforts to conduct theory-driven experiments that manipulate the mobilization strategies presented in a classroom setting and follow the spillover effects of these mobilization efforts to peers.

We fill this gap and examine two promising intervention strategies for increasing college student and first-time voter turnout: providing actionable voting information

to students and positive social pressure. These interventions are designed to avoid self-selection bias by treating students in a classroom setting for a required first-year class. Our civic training manipulation provides a brief in-person classroom presentation with actionable voting information and builds on prior research showing that classrooms can be an effective setting to deliver such training. The social pressure manipulation is designed to examine whether the effects of civic training on voting could be enhanced through positive social pressure by providing students with the opportunity to pledge to vote on a magnet for their dorm room (with the magnet serving as a reminder of the pledge), encouraging them to share this pledge with a peer (their roommate), and offering a magnet to share with the roommate. We assessed the impact of these manipulations on voter turnout by randomly assigning sections of required first-year writing courses to one of three groups: (1) voting information presentation, (2) voting information plus social pressure manipulation, or (3) control. We also assess whether classroom interventions had spillover effects on voter behavior through communication via social ties. We examine the results of our treatments by matching subjects to voter files, using a third party vendor.

Among first-year students who were registered to vote prior to the start of the presentations, we find positive effects on turnout from the two classroom treatments, though our treatments did not alone increase turnout. Additionally, we find that the treatment combining voting information with a social pressure manipulation has positive spillover to the roommates of treated students when the roommate of the treated student was previously registered to vote. These findings suggest the need for increased attention to voting interventions that support students in following through on their intentions to vote and demonstrate the potential power of classroom interventions paired with efforts to spark peer discussion of politics.

Factors Driving Youth Turnout

Many factors contribute to low turnout levels among young eligible voters. As Plutzer (2002) explains, typical voting costs are magnified for the youngest voters. These include (1) navigating the process of registration for the first time; (2) identifying the location of polling places; (3) learning about parties and candidates; and 4) relying on a peer group that includes a large share of similarly inexperienced young voters. All of these factors can play a role in the lower levels of turnout we routinely observe among young voters. College voters, in particular, often face an additional geographic challenge: their permanent home address—and voting location—may differ from their college address, making registration and voting more difficult and costly (Grumbach and Hill 2019). For young voters, these institutional and geographic hurdles to voting can be particularly challenging, since inexperienced voters must follow through on a task that can involve considerable uncertainty and effort (Holbein and Hillygus 2020).

Bennion and Nickerson (2016) propose that colleges are an excellent site for experimental interventions, both because the classroom setting offers an opportunity for communicating messages to student audiences, and because cooperation with university administration can enable access to particularized data, making it

possible to match students with the voter files. However, previous research has found that many get-out-the-vote (GOTV) interventions are ineffective among college students and young voters. Interventions such as celebrity promotions, online videos, and door-to-door canvassing, have all had minimal to null effects at increasing student voter turnout in experimental studies (Bergan 2011; Usry and Cobb 2013; Hill and Lachelier 2014).

One of the more encouraging areas, however, is when a professor or student volunteer presents about the voting process in class. Bennion and Nickerson (2016) conducted an experiment on college campuses in which students and faculty were recruited to give short voter registration outreach presentations to students. There, students were assigned to one of three groups: a control, a voter registration session administered by a professor, or a voter registration session administered by a student. The results showed that both professor-led and student-led presentations increased registration rates, and voter turnout by 2–3 percentage points.

We build on this study by developing a controlled experimental intervention designed to test theory-driven treatments in randomly selected first-year writing classrooms. We conceptualize two dimensions that shape the likelihood of voting among first-time voters. First, having never voted before, young voters often lack the practical knowledge required to navigate registration and voting processes (Holbein and Hillygus 2020) and are less confident in their political knowledge than older cohorts (Kaid et al. 2007). Second, social benefits are important as a source of behavioral motivation for this group. Weak voting patterns among peers have been shown to negatively affect college student voters compared to young adult voters who live at home (Bhatti and Hansen 2012). Conversely, college students who believe their friends think voting is important are more likely to vote themselves (Glynn et al. 2009). These two dimensions inform our theoretical expectations, outlined below.

Mobilization Through Practical Voting Information

Our first treatment focuses on providing practical information on how to register and vote. Such information is of particular value to college student voters, since they are often first-time voters and may be unfamiliar with voting regulations in their state (Holbein and Hillygus 2020). In one study of information provision to college students, researchers found that providing students with voting information both through voter registration drives and through brochures had a small but significant positive impact on turnout (Ulbig and Waggener 2011). Another study in Chicago that aimed to mobilize young voters by mailing a voter guide to registrants aged 18 to 30 found a very modest positive effect of 0.9 percent on the treatment group that received the information treatment (Miller et al. 2017).

More targeted civic training efforts show greater promise for increasing participation, especially when focused on practical skills. For example, Addonizio (2011) assesses the First-Time Voter Program, an interactive classroom presentation piloted in high schools in six states through a randomized field experiment that included simulations of registration and how to cast a ballot, and finds the presentations had

a 5.7 percent increase in voter turnout among the treatment group. Summarizing the research on civic training, Holbein and Hillygus write: "applied political learning shows much more promise than the status quo civics approach of memorization of facts about government and politics" (2020, 172). Thus, while information provision can promote turnout, the effect sizes tend to be quite small.

Social Pressure

Providing practical information about how to vote may subsidize the information costs of voting, but it does little to enhance motivations to vote (Green and Gerber 2019). To address the motivational dimension, our second treatment emphasizes the social benefits of voting. We draw on the social pressure literature to theorize that college student turnout will increase when students (1) are reminded of the social benefits of voting, (2) make a publicly visible commitment to vote, (3) are provided with a cue to remind them of that commitment, and (4) are encouraged to share that commitment with a roommate. Social pressure interventions in the voting behavior literature rely on theory and empirical research on social compliance in social psychology, which shows that individual behavior is powerfully motivated by a desire for social affiliation and perceived pressures to conform (Cialdini and Goldstein 2004).

For young voters in particular, social norms and peer communication have been shown to play an important role in shaping vote intentions. Students' perceptions of peer social norms can positively influence voting intentions. Students who perceive their friends or family are likely to vote and believe their peers value voting are themselves likely to report a higher intention of voting, above and beyond political interest and voting knowledge (Glynn et al. 2009). Normative perceptions about voting behavior are spread through peer communication among college students. The strength of political norms at a given university is related to the level of political activity exhibited by individual students at that campus (Shulman and Levine 2012). Motivating this body of work is the intuition that citizens' decisions to participate in politics are not formed in a vacuum, but rather depend on their social surroundings (Lazarsfeld et al. 1944).

Social pressure interventions have been shown to increase youth voter turnout. Encouraging students to make a pledge to vote that is visible to others is related to higher levels of turnout (Costa et al. 2018). The effects of pledging to vote are particularly strong among first-time voters. These findings are explained with reference to self-perception theory, which proposes that people develop attitudes by interpreting their own behavior, rather than the reverse. The theory predicts that an individual's behavior in a given situation (i.e. pledging to vote) will later be used as evidence in determining an attitude toward a behavior (Bem 1972). Such self-observations are more powerful under conditions where a person has little past experience with the behavior. Additionally, pledge to vote treatments are often paired with a cue that reminds individuals of their pledge closer to election day, reinforcing the attitudinal commitment made by individuals in the study. In prior studies of pledge to vote

treatments, pledge cards were mailed back to individuals to serve as a reminder of their pledge to vote (Costa et al. 2018; Burgess et al. 2000).

In sum, prior literature suggests that practical voting information can have positive and significant effects on turnout, but effect sizes are often larger for social pressure treatments. Moreover, combining a social pressure treatment with an informational treatment can increase the effect on turnout, but this possibility has not yet been tested in the context of young voters (Davenport 2010). We therefore hypothesize that the information treatment will increase turnout, but turnout increases will be greater for students who receive the treatment combining information and social pressure.[1]

(H1) Students in the two treatment groups will turn out to vote at higher rates than those in the control group.

(H2) Students in the social pressure group will turn out to vote at a higher rate than the control group.

(H3) Students in the social pressure group will turn out to vote at a higher rate than the informational treatment group.

Spillover Effects

Our research design, while targeted at first-year college students in a required writing class, creates an opportunity to test a second set of hypotheses. We are able to leverage information about roommate assignments among first-year students to consider whether the effects of our treatments spillover to extend beyond the initially treated individual.[2] Research on random assignment of college roommates provides evidence that college students' social interactions with their roommates impact their political behavior (Klofstad 2011; 2015), such as of peer-to-peer effects on voting, contacting elected officials, and participation in partisan groups (Klofstad 2015; Gomez and Pietryka 2018). Higher levels of political discussions among college students are linked to higher levels of political behavior (Shulman and Levine 2012). Spillover and peer effects have previously been identified in experiments where experimentally treated voters influence their housemates, such that turnout increased among both the canvassed individual and the other registered voter in the household (see Nickerson 2008).

The explicitly social nature of the social pressure portion of the treatment, which offers students the opportunity to make a visible pledge to vote and share that pledge with a roommate, guides our expectation that the spillover effect will be larger for the roommates of students who receive the combined information and social

[1] Our hypotheses for this study were pre-registered with Evidence in Governance and Politics (EGAP), ID 20181022AA. Hypotheses H4-H6 specify "on-campus" roommates. While there were some students in the sample with local off-campus addresses, none of them had roommates in the classes randomly assigned to treatment condition, and they were therefore not eligible to be included in the models testing roommate effects.

[2] Discussions with a director of residential life at the university revealed that first-year roommates are not randomly assigned.

pressure treatment. Drawing from the literature on social spillovers, we expect the pledge to offer a stronger mechanism for voting communication and socialization between roommates.

(H4) The on-campus roommates of those in the control group will turn out at lower rates than the roommates of those in the social pressure treatment group.

(H5) The on-campus roommates of those in the informational treatment group will turn out at lower rates than the roommates of those in the social pressure treatment group.

(H6) The on-campus roommates of all treated students will turn out at higher rates than roommates of students in the control condition.

Heterogeneous Effects

There has been little research to explore whether college voter intervention effects are conditional on characteristics of participants. While peer effects can be highly influential for political behavior, these effects may not be uniform across individuals. One important category that mediates social influence in politics is gender. A 2017 report on college student voting found that women are more likely to vote than men (Thomas et al. 2017), and a study of social influence on voting among randomly assigned college roommates found evidence of peer to peer influence largely among female roommates (Gomez and Pietryka 2018).) In a study comparing the influence of political knowledge within men's and women's political social networks, "exposure to knowledgeable discussants is a consistent facilitator of women's participation, but not men's" (Djupe et al. 2016). Reviews of research on gender differences in social influence have found that stereotyping and gender role socialization typically explain higher levels of social influence among women (Eagly et al. 1995). Given this brief review, we propose the following hypothesis:

(H7) The female on campus roommates of those females in the treatment groups will turn out at higher rates than those on campus roommates of males in the treatment groups.

In addition, we consider whether the effect of our treatments on voter turnout will depend on the registration status of the treated participant. Analyzing this contingent effect was not part of our pre-registered hypotheses (much of GOTV literature focuses on registered voters but our study is looking at first time voters). The effects of the intervention may depend on prior voter registration for two reasons. First, the effort to register those who have not yet registered adds to the cost of voting, weakening the effect of the intervention on turnout. In the state where our study took place, the voter registration deadline was in early October, increasing the effort that would be required for students who were not already registered at the start of Fall semester. Second, prior work on turnout suggests that the effect of GOTV interventions varies by election salience as well as the voters' propensity to turn out (Arceneaux and Nickerson 2009). For low salience elections, interventions are much more effective on high propensity voters than low-propensity voters. For a non-presidential election, the effects of an intervention may be larger for those who have signaled their propensity to vote by registering prior to the intervention (although the salience

of a high-stakes midterm election may fall somewhere in the middle of the salience range explored in previous studies, which include low-turnout local elections and high-turnout presidential elections). As such, we consider the role of prior registration in the following research question: Will treatment effects depend on the registration status of treated students?

Data and Methods

We randomly assigned sections of a required first-year writing course at a major Midwestern university to one of three conditions prior to the 2018 midterm election: a control group receiving no treatment, an informational treatment group where students heard a short presentation about the voting process and major statewide issues, or a social pressure treatment group.[3] The social pressure group received the informational treatment plus information about the positive benefits of committing publicly to voting and talking with peers about voting. In the combined informational and social pressure treatment group, participants also received information about the positive effects of committing to vote and peer influence as well as a magnet on which they could publicly display their commitment to voting. These students were also given an additional magnet to provide to a roommate. The presentations took place in classrooms from September–October 2018. After the November 2018 election, we matched all first-year college students to voter file data provided by a private national vendor to assess whether the experimental treatments affected turnout for treated students and their roommates.

Sample

Incoming students must live on campus and fulfill a writing requirement in their first year, with the majority of students taking the general university writing class. These writing classes enroll between 15 and 30 students per section. The university registrar shared the full list of sections as well as student rosters for each, which included 3,299 students. However, given our interest in observing the effect of the experimental interventions on voter turnout, we include only students who were eligible to vote in the 2018 midterm election. To restrict our sample, we omitted all students who had a non-U.S. permanent address or were identified as international students by the university registrar ($N = 497$) and students who were under the age of 18 on Election Day ($N = 1$). This resulted in a sample of 2,801 students.

[3] We do not employ a full factorial design in this instance. While such a design would allow us to infer the main effect of social pressure absent information, such a condition would be devoid of content and render the social pressure treatment meaningless, since in this context the social pressure treatment would not have made much sense without the information treatment. As such, we remain agnostic about whether social pressure may affect and thereby enhance the strength of the treatment, or whether it may carry an independent effect.

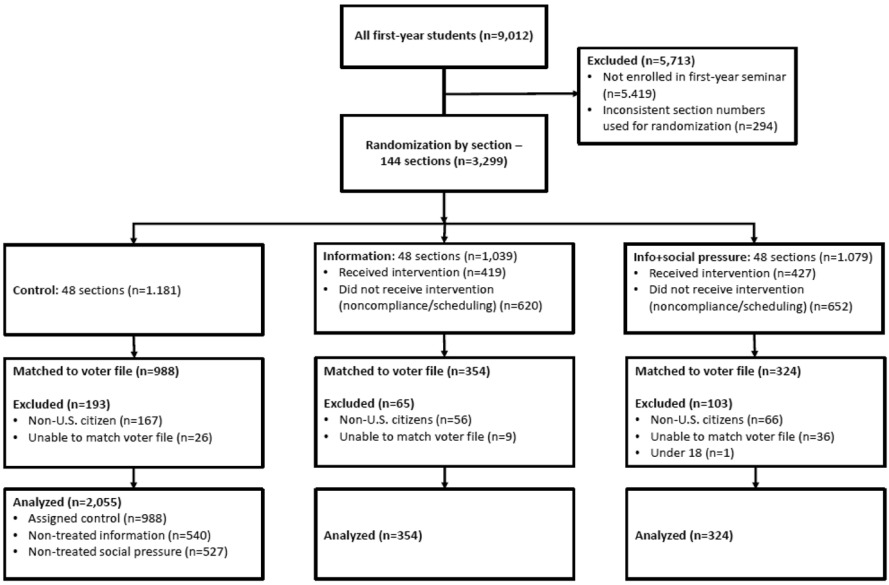

Fig. 1 Experimental flow chart

Experimental Interventions

Sections of the first-year writing seminar were randomly assigned into one of three conditions. Figure 1 displays a see flow chart of the full experimental design. The control condition did not receive an in-class presentation. In both treatment conditions, a trained student presenter visited the class to give a 10-min nonpartisan presentation on the upcoming midterm election. For the information treatment, the presentation provided two types of information: basic information about the voting process (such as eligibility, how to register, how to submit a ballot, and important dates and deadlines) and information about the specific issues and candidates on the ballot. On the latter type, focus in the presentation was limited to federal (U.S. Senate, U.S. House) and select statewide races (including three highly publicized statewide ballot questions). The presentation concluded with links to additional resources where students could learn more. Example slides from the information treatment are included in Figure S1 in the online supplemental file.

The second treatment condition included this information and combined it with additional content about the value of positive peer influence on the voting process. Specifically, students were informed that committing to vote and sharing this pledge with others has been shown to be a strong influencer on the voting behavior of others. To facilitate this process, students in this treatment condition also received magnets with registration and election dates, on-campus polling locations, and–most notably–a space on the magnet where they could sign their names to pledge to vote. The presentation concluded by encouraging students to sign the pledge, talk to their roommates about voting, and pass out extra magnets for their roommates to sign

and display. Example slides and an image of the magnet design (with identifying information blocked out) for this treatment are provided in Figures S2 and S3 of the online supplemental file.

All presentations, whether information only or information + social pressure treatments, were delivered by students at the university, whom we recruited on campus. To maximize consistency across presentations, all presentations followed the same script and utilized the same slides created by the research team. Presenters attended multiple training sessions before entering classrooms. In addition, we instructed the student presenters to speak and dress in a nonpartisan manner (i.e. no campaign clothing or stickers, no advocacy group paraphernalia) to ensure consistency across the treatments.[4]

To gain entry to classrooms, we partnered with the director of the first-year writing program. With their permission, and after randomly assigning sections to experimental conditions, we emailed the instructors for each treatment condition informing them of the day a student would visit their class for the presentation. To take up as little class time as possible, student presenters arrived early prior to the start of class to set up the PowerPoint presentation. All presentations took place at the beginning of class. Even with support from the director of the writing program and university administration, the refusal rate on the part of instructors of individual sections was high. We completed 32 classroom treatments; there were 61 sections assigned to treatment conditions that we were unable to complete. Of these, the failed treatments arose from instructor refusals or instructor rescheduling requests that could not be accommodated. Despite this, randomization checks suggest that treatment success rate did not result in significant imbalances between treatment and control groups; the only exception was that students in honors sections of the first-year writing class were less likely to belong to the successfully treated information condition than to the control group (see Table S1 in the online supplemental file for full randomization check and Table S2 for descriptive statistics about the sample).

Matching to Voter Files

To assess the treatments' effects on voter turnout, we relied upon a private vendor to match student enrollment data obtained via the university registrar with state voter databases, similar to Bennion and Nickerson (2011). To administer this process, student—including name, age, gender, race, ethnicity, and home address—was provided to the vendor, who then combed state voter databases for all 50 states to find likely matches. Matches were then cross-checked to ensure accuracy, specifically using the variables of age (discrepancy of more than one year between registrar and vendor data) and state of residence (failure to match, with neither matching the state of the university). This resulted in an additional 68 cases dropped from our sample,

[4] A total of six student presenters were used, and variation in treatment effectiveness was assessed by accounting for potential random effects of presenter on turnout. There was significant variation among the presenters, suggesting that some were more effective than others; these results are presented in Table A8 of the online supplemental file.

resulting in a final sample of 2733. Table S3 in the online supplemental file provides a breakdown of treatment assignment and treatment success for the sample.

Analysis

We conduct two main analyses to test our predictions. The first assesses the direct effects of the treatments on voter turnout. Here, we focus only on students enrolled in the first-year writing courses and assigned to receive either of the treatments or the control condition. Students not enrolled in these courses are omitted due to potential differences between students enrolled in the first-year writing seminar and those who were not; for example, students not enrolled in these seminars may have tested out of these courses due to higher rates of prior academic achievement, which could be associated with a host of other demographic and socioeconomic factors. We use treatment success as our key independent variable, though only approximately 40 percent of those sections assigned to a treatment condition were successfully treated. To account for potential issues surrounding our relatively low rate of treatment success, analyses were conducted with and without control variables—including race and ethnicity, gender, age, and course type (e.g. honors, remedial).

The second set of analyses focuses on the potential indirect effects of the treatments, assessing the potential for treatment effects to spill over to roommates. We observe students who had at least one roommate who was enrolled in the first-year writing seminar, whether in the treatment or in the control conditions ($N = 1{,}261$). Again, treatment success serves as our key independent variable, indicating whether students' roommates were treated or not, while the above control variables—with the exception of course type and the addition of the student's number of roommates—are also included.

For each set of analyses, two sets of models are estimated. The first focuses on the full sample of students, allowing us to observe the influence of the treatments on all voting age participants in our sample, whether registered to vote or not. Since the majority of treatments were administered prior to the state registration deadline for voting in the 2018 midterm election, presentations also encouraged voter registration. However, whether due to issues associated with the research design (e.g. time constraints; scheduling conflicts) or the costs associated with both registering and voting for non-registered students being too prohibitive, we do not detect such an effect on registration behavior for those not registered prior to the interventions, a finding consistent with prior research on registration drives (Bennion and Nickerson 2011, but see also Ulbig and Waggener 2011).

The second set of models explicitly considers RQ1 in assessing whether students' registration status influenced the effectiveness of treatments in promoting voter turnout, using subgroup analyses to investigate the effect of each intervention depending on whether or not students were registered prior to the start date of the in-class presentations. While analyses including only students who were already registered prior to the campaign are comparable with most prior work on turnout studying treatment effects for registered voters (Green and Gerber 2019), observing treatment effects

for registered and non-registered students separately offers greater insights into for whom GOTV interventions are likely to succeed in driving up voter turnout.

Linear probability models using OLS with robust clustered standard errors are estimated in all analyses (Angrist and Pischke 2008). Despite the use of a dichotomous outcome measure (whether students voted or not), prior work has used linear probability models to estimate the effects of GOTV interventions (e.g. Bennion and Nickerson 2011; Gerber et al. 2008) and recent work has suggested the appropriateness of using linear models for experimental designs with binary outcomes (e.g. Gomila 2020).[5] The direct effects models include clustering by course section to account for possible idiosyncrasies across the various sections of the writing seminar (e.g. instructor characteristics, course content). The indirect models account for clustering by local residence. Estimates of the coefficient for the treatment success variables indicate the average treatment effect on students' probability of voting.

Results

H1 predicts that students assigned to the two treatment groups will turn out to vote at higher rates than those in the control group. We turn first to the full sample of students in Table 1. There, the treatment variable, which aggregates the information and social pressure treatments into a single indicator, is not significant (Model 1, $p = 0.26$, one-tailed; Model 2, $p = 0.20$, one-tailed). However, as our research question suggests, registration status matters for treatment effectiveness. Models 5 and 6 in Table 2 provide no evidence that the treatments increased turnout among voters who were not registered prior to the classroom intervention. Conversely, the treatment had a significant and positive effect on turnout for voters who had registered prior to the administration of the treatments (Table 2, Model 10, $p = 0.03$, one-tailed). Substantively, the treatment increased the likelihood of voting among previously registered students by around seven percentage points compared to those in the control group.

H2, which suggests that the social pressure treatment would promote larger student turnout relative to the control group, also received partial support. Again looking at the full sample (Table 1), the effect of the social pressure intervention on turnout did not significantly differ from zero (Model 3, $p = 0.48$, one-tailed; Model 4, $p = 0.49$, one-tailed). However, the story changes again when considering students' registration status (Table 2). For non-registered students, the social pressure treatment appears to have a significant *negative* effect, suggesting a lower propensity to vote amongst these students (Model 7, $p = 0.04$, one-tailed; Model 8, $p = 0.04$, one-tailed). On the other hand, the social pressure treatment corresponded to higher turnout for students registered to vote prior to the administration of treatments. In the model including control variables, students who received the social pressure treatment were around seven percentage

[5] All analyses were also estimated using logistic regression (see Table A7 in the online supplemental file). Results were consistent across estimation approach.

Table 1 Average treatment effects on voting behavior, full sample

	Model 1	Model 2	Model 3	Model 4
Treatment	.01 (.02)	.02 (.02)	–	–
Info	–	–	.03 (.03)	.03 (.03)
Pledge	–	–	.00 (.03)	−.00 (.02)
African American	–	-.10* (.02)	–	−.10* (.02)
Asian American	–	-.07* (.03)	–	−.07* (.03)
Latino	–	-.08* (.03)	–	−.08* (.03)
Female	–	.04* (.02)	–	.04* (.02)
Age	–	.02 (.01)	–	.01 (.01)
Class A	–	-.05 (.04)	–	−.05 (.04)
Class B	–	.16* (.03)	–	.16* (.03)
Constant	.24 (.01)	-.06 (.20)	.24 (.01)	−.05 (.20)
N R^2	2,733 .00	2,733 .02	2,733 .00	2,733 .02

OLS estimates displayed with robust standard errors clustered by first year writing section

*$p < .05$, one-tailed

points more likely to vote than those in the control group (Model 12, $p = 0.05$, one-tailed). However, when comparing the coefficients for the information only treatment and the social group treatment for registered voters, there is no evidence that the social group treatment had a stronger effect on turnout (Model 11: $F(1,125) = 0.05$, $p = 0.83$; Model 12: $F(1,125) = 0.00$, $p = 0.97$). Thus, the results do not support H3.

Moving to the potential for spillover effects on roommates' voting behavior, H4 predicts that the roommates of students in the social pressure condition would vote at higher rates compared to the roommates of students assigned to the control group. Results from analyses of the full sample (Table 3) indicate a small positive effect for the social pressure treatment on roommate turnout; however, this effect falls short of statistical significance (Model 15, $p = 0.15$, one-tailed; Model 16, $p = 0.11$, one-tailed). But as with results observing the direct effects of the interventions, registration status again matters for treatment effectiveness (see Table 4). The treatment had no significant indirect effect on turnout for roommates who were not registered to vote (Model 19, $p = 0.49$, one-tailed; Model 20, $p = 0.45$, one-tailed) but a positive, significant effect on turnout for roommates

Table 2 Average treatment effects on voting behavior by registration status

	Not registered prior to treatment				Registered prior to treatment			
	Model 5	Model 6	Model 7	Model 8	Model 9	Model 10	Model 11	Model 12
Treatment	−.01	−.01	–	–	.06	.07*	–	–
	(.02)	(.02)			(.04)	(.03)		
Info	–	–	.01	.01	–	-	.05	.07
			(.02)	(.02)			(.05)	(.05)
Pledge	–	–	−.04*	−.04*	–	-	.06	.07*
			(.02)	(.02)			(.05)	(.04)
African American	–	−.03	–	−.03	–	−.16*	–	−.16*
		(.02)		(.02)		(.04)		(.04)
Asian American	–	−.03	–	−.03	–	−.09*	–	−.09*
		(.03)		(.03)		(.05)		(.05)
Latino	–	−.00	–	−.00	–	−.14*	–	−.14*
		(.03)		(.03)		(.06)		(.06)
Female	–	.01	–	.01	–	.07*	–	.07*
		(.02)		(.02)		(.03)		(.03)
Age	–	.00	–	.00	–	.02	–	.02
		(.01)		(.01)		(.02)		(.02)
Class A	–	−.04	–	−.04	–	−.02	–	−.02
		(.03)		(.03)		(.10)		(.10)
Class B	–	.00	–	.01	–	.15*	–	.15*
		(.04)		(.04)		(.04)		(.04)
Constant	.09	.01	.09	.01	.41	.03	.41	.03
	(.01)	(.17)	(.01)	(.17)	(.02)	(.31)	(.02)	(.31)
N	1,454	1,454	1,454	1,454	1,279	1,279	1,279	1,279
R^2	.00	.00	.00	.01	.00	.03	.00	.03

OLS estimates displayed with robust standard errors clustered by first year writing section

*$p < .05$, one-tailed

who were registered prior to treatment (Model 23, $p = 0.04$, one-tailed; Model 24, $p = 0.03$, one-tailed). Substantively, the registered roommates of students who received the social pressure intervention treatment were between eight and nine percentage points more likely to vote in the 2018 election.

Additional analyses offer partial evidence that the observed spillover effect was unique to the social pressure treatment, in accordance with H5. As shown in Table 4, the information-only treatment at no point influenced roommate turnout regardless of registration status, and F-tests comparing the effect of this treatment to the social pressure treatment, while not statistically significant at conventional levels, suggest potentially differential effects of the two treatments among roommates who were previously registered to vote (Model 23, $F(1,893) = 3.23$; $p = 0.07$; Model 24, $F(1,893) = 3.21$; $p = 0.07$). Furthermore, the coefficient for the aggregate treatment variable indicates that the roommates of students who received *either* the information or social pressure treatments were no more likely to turn out than roommates

Table 3 Average treatment effects on roommates' voting behavior, full sample

	Model 13	Model 14	Model 15	Model 16
Treatment	.01 (.02)	.02 (.02)	–	–
Roommate info	–	–	−.01 (.03)	.00 (.03)
Roommate pledge	–	–	.03 (.03)	.03 (.03)
African American	–	−.10* (.03)	–	−.10* (.03)
Asian American	–	−.09* (.04)	–	−.09* (.04)
Latino	–	−.16* (.03)	–	−.16* (.03)
Female	–	.04* (.02)	–	.04* (.02)
Age	–	.00 (.02)	–	.00 (.02)
2 roommates	–	−.00 (.04)	–	−.00 (.04)
3 roommates	–	−.05* (.03)	–	−.05* (.03)
i Info treatment	−.01 (.04)	-.01 (.04)	−.01 (.04)	−.01 (.04)
i Pledge treatment	.03 (.04)	.04 (.04)	.03 (.04)	.04 (.04)
i Not in Experiment	.05* (.02)	.05* (.02)	.05* (.02)	.05* (.02)
Constant	.22 (.02)	.18 (.42)	.22 (.02)	.18 (.42)
N R^2	2,138 .00	2,138 .02	2,138 .00	2,138 .02

OLS estimates. Displayed with robust standard errors clustered by on-campus address

*$p<.05$, one-tailed. Sample includes people with at least one roommate in individual experiment (excluding *i*)

of students even among registered voters (Model 21, $p=0.21$, one-tailed; Model 22, $p=0.19$, one-tailed), failing to support H6.

Our final prediction suggested that any indirect effects of the treatments on roommates would be greater among female students than for male students (H7). Table 5 tests this possibility using interaction terms to assess the difference in the treatment effect for male and female students, looking exclusively at students who were registered prior to the start of the presentations.[6] The coefficient for

[6] The registrar data coded all students as either male or female gender. Students living on campus live in rooms consisting of either all male or all female students. Most off-campus students in the sample lived in single-gender rooms as well. Four students who had a mix of female and male students living in a room were excluded from the analyses.

Table 4 Average Treatment Effects on Roommates' Voting Behavior by Registration Status

	Not registered prior to treatment				Registered prior to treatment			
	Model 17	Model 18	Model 19	Model 20	Model 21	Model 22	Model 23	Model 24
Treatment	−.00 (.02)	−.00 (.02)	–	–	.03 (.03)	.03 (.03)	–	–
Roommate info	–	–	-.00 (.02)	.00 (.03)	–	–	−.02 (.04)	−.02 (.04)
Roommate pledge	–	–	.00 (.02)	.00 (.03)	–	–	.08* (.05)	.09* (.05)
African American	–	.00 (.03)	–	.00 (.03)	–	−.15* (.05)	–	−.15* (.05)
Asian American	–	−.03 (.03)	–	−.03 (.03)	–	−.10 (.07)	–	−.10 (.07)
Latino	–	−.02 (.03)	–	−.02 (.03)	–	−.24* (.05)	–	−.23* (.05)
Female	–	.02 (.02)	–	.02 (.02)	–	.05* (.03)	–	.06* (.03)
Age	–	−.01 (.02)	–	−.01 (.02)	–	−.01 (.03)	–	−.01 (.03)
2 roommates	–	.00 (03)	–	.00 (.03)	–	.04 (.06)	–	.03 (.06)
3 roommates	–	−.01 (.02)	–	−.01 (.02)	–	−.07 (.04)	–	−.07* (.04)
i Info treat	.03 (.04)	.03 (.04)	.03 (.04)	.03 (.04)	−.07 (.07)	−.07 (.07)	−.07 (.07)	−.07 (.07)
i Pledge treat	−.01 (.03)	−.00 (.03)	−.01 (.03)	.01 (.02)	.09 (.08)	.09 (.08)	.09 (.08)	.09 (.08)
i Not in Experiment	.01 (.02)	.01 (.02)	.01 (.02)	.01 (.02)	.05 (.03)	.05 (.03)	.05 (.03)	.04 (.03)
Constant	.07 (.01)	.22 (.34)	.07 (.01)	.22 (.35)	.39 (.03)	.67 (.65)	.39 (.03)	.67 (.65)
N	1,054	1,054	1,054	1,054	1,084	1,084	1,084	1,084
R^2	.00	.00	.00	.00	.01	.03	.01	.03

OLS estimates with robust standard errors clustered by on-campus room

*$p < .05$, one-tailed. Sample includes people with at least one roommate in individual experiment (excluding *i*)

All Female X Treatment indicates whether the treatments had differential effects on students living in a room with all females than for students living in a room with all males. Whether looking at the aggregate treatment measure or the effect of each treatment separately, there was no evidence that the treatment had a greater spillover effect for female students than for male students. Simply put, male and female students were similarly affected by their roommates, failing to support H7.

Table 5 Gender and spillover effects

	Registered prior to treatment			
	Model 25 (no controls)	Model 26 (with controls)	Model 27 (no controls)	Model 28 (with controls)
Treatment	.04 (.05)	.04 (.05)	–	–
Roommate info	–	–	−.04 (.07)	−.04 (.06)
Roommate pledge	–	–	.12* (.07)	.11 (.07)
All Female Roommates	.05 (.04)	.06 (.04)	.05 (.04)	.05 (.04)
All Female × Roommate Treat	-.02 (.07)	-.01 (.07)	–	–
All Female × Roommate Info	–	–	.03 (.09)	.02 (.09)
All Female × Roommate Pledge	–	–	−.06 (.09)	−.04 (.09)
Constant	.36 (.04)	.60 (.65)	.36 (.04)	.57 (.66)
N	1,080	1,080	1,080	1,080
R^2	.01	.03	.01	.03

OLS estimates with robust standard errors clustered by on-campus room

*$p < .05$, one-tailed. Sample includes people with either all female or all male roommates, with at least one roommate in individual experiment (excluding i). Controlled models include variables for race/ethnicity (African American, Asian American, Latino), age, number of roommates, and whether the respondent received a treatment (full reporting in online supplemental file)

Discussion

Younger voters wield considerable—and increasing—power in shaping electoral outcomes in the United States, rapidly overtaking older demographics as the largest voting bloc in the American electorate. Nonetheless, barriers to voting disproportionately affect younger voters, resulting in their collective inability to fully realize the weight of their electoral power. This project observed the effectiveness of interventions in addressing two such barriers—lack of information and lack of opportunity for normative influence—among young voters, utilizing the classroom setting to observe the effects of these interventions on voter turnout among first-year college students during the 2018 midterm election.

We find that classroom interventions have substantively large and positive effects on college youth voter engagement, though these effects were largely observed among students who have already overcome perhaps the most difficult voting barrier of all: voter registration. Additionally, when the interventions encouraged engagement with others about the importance of voting, evidence of positive spillover effects did emerge among treated subjects' roommates, albeit again only if they were registered prior. Thus, among a population that

increasingly constitutes a large and non-negligible potential voting bloc, our study prescribes classroom interventions as effective means of reaching a subset of young voters and their peers.

One question raised by our findings regarding spillover effects involves the mechanism behind the influence of the social pressure intervention on students' roommates. Given that our social pressure treatment makes use of a number of elements (e.g., voting pledge, magnet, and encouragement to reach out to others), we cannot isolate the causal agent for our observed spillover effects. However, we can speculate as to the potential nature of this effect via an opt-in survey administered immediately following the November 2018 election. While sent to all first-year students, 410 responses were received from students enrolled in the first-year writing seminar (15% response rate; see Tables A11 through A13 of the online supplemental file for more detail on the survey and full reporting of results). Results suggest that the presentations made an impression on students (80% of those assigned to the information condition and 88% from the social pressure condition recalled the presentation versus 15% in the control group), while a large percentage of those receiving the social pressure treatment reported signing the pledge (69%) and displaying the provided magnet (56%). Students in the social pressure treatment also recalled conversing more frequently with their roommates about the election than in the other groups. While far from definitive, these results suggest that the social pressure treatment may have influenced roommate voting behavior in part by prompting conversations about politics.

Some limitations of the current study bear noting. First, the nature of the field experiment required cooperation from instructors to allow access to their classrooms to deliver presentations, yet we were confronted with some reluctance and difficulty in coordinating with instructors. Some instructors were unwilling to give up class time and were concerned about students' involvement in an experiment for which there was a control group. In other cases, we simply could not determine a date amenable to both presenters and instructors alike. These issues had two primary consequences for our study. First, as noted previously, this study had a relatively low rate of treatment success. Second, scheduling difficulties resulted in interventions being delivered either just prior to or shortly after registration deadlines, thereby challenging our ability to observe the impact of the treatment on registration behavior (see Bennion and Nickerson 2016). While the complex logistics of field experiments remain a constraint on the method, future efforts can learn from and anticipate these issues to achieve greater treatment success. Additionally, while our survey of students was helpful in beginning to understand what may have driven roommate turnout, our inability to isolate the mechanism underlying the spillover effects is due to the multifaceted nature of the social pressure treatment and remains an area ripe for future study. Finally, future research would be well-served to build on our study and examine the long-term effects of youth voter turnout and examine whether interventions such as these have effects that persist beyond the election at hand.

Nonetheless, together, our findings build upon prior experimental studies investigating the effectiveness of interventions to promote civic engagement and voting among college students (Bennion and Nickerson 2016; Ulbig and Waggener 2011). These studies suggest that there is untapped potential on college campuses to offer

more information and supportive messaging related to voting. Our work demonstrates the ability of school administrators and academics to unite in pursuit of civic education, made all the more possible since universities have both the resources and social settings to attempt more evidence-based interventions to promote voter turnout—with the potential to expand the pool of young voters in future elections.

Additionally, while Nickerson (2008) found evidence of spillover effects in GOTV efforts at the household level among older adults, the results of this study suggests the potential for a similar effect among new households that students are forming with their college roommates. Unlike Nickerson, however, we find spillover effects only when the intervention explicitly encouraged social pressure, indicating that more direct appeals are important to mobilize spillover effects for this population of young voters.

Another key takeaway from this research involves the importance of voter registration. Recent research suggests that reforms to registration deadlines, such as same day voter registration, disproportionately increases turnout for young voters, especially those aged 18–24 (Grumbach and Hill 2019). Building on Grumbach and Hill's (2019) argument, our study highlights the importance of voter registration and suggests that the effectiveness of GOTV interventions among young voters may depend on whether they are already registered. Recall that while we find no effect of the treatments on the full sample of treated students, we observe significant positive treatment effects among students already registered to vote. As such, easing registration requirements may result in greater success of interventions aimed at driving up youth turnout.

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