



“How Colleges Can Increase Civic Engagement”

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Abstract

Young people are much less likely to participate in politics. Universities are expected to help remedy this problem. How can they do so? The answer remains unclear, as studies typically test one or two small interventions at a time, or the interventions have a voluntary, opt-in component. Yet universities have the capacity to use potentially more effective, bundled and opt-out “nudges”. In this study we leverage universities’ bureaucratic power to implement such interventions. We test the effects of a bundle of cognitive behavioral nudges embedded in a simulated university administrative check-in required of all students. We find that this intervention meaningfully increases turnout. It also has beneficial spillovers to other political attitudes and actions. The role of higher education in a robust democracy is not limited to teaching or service learning. It extends to the institutional power of universities to structure the choice environment to facilitate the actions people wish to take but often find difficult.

Keywords Voting · Voter registration · Turnout · Education · Political engagement · College students

Introduction

In the United States, older citizens turn out about twice as often as their younger counterparts (Holbein and Hillygus 2020). The skewed age composition of the electorate may distort representative government, shaping who gets elected and the poli-

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cies that get implemented (Griffin and Newman 2005; Leighley and Nagler 2013). Young Americans differ from their elders in demographics and political preferences (Munger and Phillips 2022; Pew Research Center 2020). They are more diverse in their ethnic, racial, and sexual identities, more likely to have an immigrant parent, have higher levels of education, and are more politically liberal.¹ In fact, some scholars argue that today, the generations differ more “than at any time in living memory” (Fisher 2022).

Colleges and universities are among the institutions that could help narrow the age gap in voting. Educational institutions are expected to foster civic engagement, because they are uniquely positioned to develop active citizens before they reach full adulthood (Hillygus 2005). What happens during the formative years tends to have long lasting effects (Holbein and Hillygus 2020; Prior 2018). Higher education has long been held responsible for nurturing politically informed and engaged young adults (Willeck and Mendelberg 2022).

This responsibility is a legal obligation. A 1998 amendment to the Higher Education Act requires universities to distribute voter registration forms to their students prior to the registration deadline. This reform has a large potential impact, because over 40% of young adults are enrolled in college per year (National Center for Education Statistics 2020). However, while universities have a legal responsibility to improve student voter registration and turnout, they may not be implementing this duty as effectively as they might. As suggestive evidence, relative youth turnout remained constant after the amendment passed in 1998 (Online Appendix 1 Fig. 4).

This legal mandate, and the weight of expectations from democratic theories, both raise the following question: How can universities increase civic engagement? This question is not only central to democratic citizenship, but also relevant to a core question in the study of political behavior: does education have a causal effect on political participation? The answer is decidedly mixed (Willeck and Mendelberg 2022). To be sure, education and participation are strongly correlated (Nie, Junn and Stehlik-Barry, 1996; Schlozman et al. 2012). But whether this is due to the causal effect of education remains contested (e.g., Berinsky and Lenz 2011; Dee 2003; Kam and Palmer 2008). The conflicting results are difficult to adjudicate partly because these studies have not often focused on *how* institutions of higher education matter.²

The few causally identified studies of *mechanisms* show that universities can indeed increase student registration and turnout rates. These studies offer a useful methodological advance in using random assignment. They also make a neglected but important theoretical point: while the conventionally understood participatory mission of universities is to promote civic participation by teaching civics, universities can also fulfill this mission through their institutional capacity to directly facilitate student participation. For instance, randomizing professors or student volunteers to hand out registration materials in the classroom moderately increases voter registration and turnout (Bennion and Nickerson 2016). Likewise, successfully randomizing classrooms to receive informational and social GOTV content can increase turnout

¹ <https://www.pewresearch.org/social-trends/2020/05/14/on-the-cusp-of-adulthood-and-facing-an-uncertain-future-what-we-know-about-gen-z-so-far-2/>.

² Some *observational* studies do examine mechanisms (Hillygus 2005).

among those already registered to vote (Bergan et al. 2022). In addition, college administrators who send randomly selected students emails with links to online registration materials and election reminders sometimes slightly increases registration and turnout (Bennion and Nickerson 2011; Bennion and Nickerson 2022; Kennedy and Mayorga 2009). However, while these studies make important methodological and theoretical contributions, at the end of the day, they typically find that the effects of these administrative interventions on turnout are quite modest.³

We build on this literature to argue that universities can in fact have a causal effect on political engagement. Furthermore, unlike most existing studies, we find that this effect need not be small. To have this effect, universities must more fully leverage their institutional power. Specifically, universities can encourage voter registration and turnout using a set of nudges embedded within their required student administrative processes. Such processes might include academic enrollment, course registration, or any bureaucratic process that every student must undergo.

We ground our argument in cognitive behavioral theories about the efficacy of ‘nudges’, self-persuasion, and salience. These theories offer specific interventions to make a desired behavior easier. To test these interventions, we designed a simulated academic year sign-in process closely modeled on an actual sign-in. The simulated sign-in was embedded in a survey experiment fielded among students in universities across the United States. Students were randomly assigned to a treatment or a placebo-control condition. The treatment invited students to engage in a series of political behaviors: to register to vote, to sign-up for voting reminders, and to share voter registration resources with friends and family. Following the GOTV literature, the placebo-control used a nearly identical format and asked students to engage in equivalent but non-political actions: calculate their plastic use, sign up for recycling reminders, and share these recycling resources with friends and family. Importantly, in both conditions, students had to take steps to opt out of initiating these actions. In that sense, this procedure mimics an institutional ‘nudge’ where every eligible individual is encouraged to participate by default, and the cost of participating drops while the cost of opting out increases.

This study addresses several limitations in studies of voting. First, existing studies have investigated the effects of piecemeal interventions, such as text messages but not online registration (Malhotra et al. 2011), or registering students but not texting them reminders to vote (Nickerson 2014). This approach is useful for identifying the precise effect of a specific intervention, but it typically finds modest effects, and may under-estimate the fuller effects of the more robust interventions that institutions are positioned to undertake. No study we know of has tested a bundled “grand treatment” of these interventions.

Second, existing studies of education-based interventions typically do not measure both actual registration and turnout from voter files and psychological mechanisms

³ Bergan et al. (2022) find large but conditional turnout effects. They estimated the effect of successfully administered treatments.

measured in surveys (for a partial exception, see Bergan et al. 2022).⁴ This leaves unknown whether interventions affect actual behavior and why they do so.

Third, existing studies typically do not examine a broad range of outcomes beyond registration and turnout, such as signing a petition or encouraging others to register. Again, this may understate the full effect of institutional interventions.

Fourth, existing studies typically lack measures of the *behavioral uptake* of voting encouragements, such as data on whether the individual actually clicked through a provided link to register to vote and then actually voted. No study we know of specifically leverages administrative data on such measures.

Finally, existing studies typically do not randomize the administrative procedures colleges can use to increase voting. Those that randomize an intervention within a school often rely on instructor cooperation, which limits their reach (e.g., Bergan et al. 2022).⁵

Our preregistered design includes each of these features. We investigate the effect of a grand treatment closely modelled on a strong institutional intervention by universities, consisting of a fuller array of behavioral encouragements to register and to vote, on a rich set of outcomes (including political efficacy, self-reported intentions to act, actual behaviors measured in real time, and downstream actual registration and turnout), mediated by measures of students' actual uptake of the intervention from administrative records. The contents of the intervention draw on findings from the GOTV literature about effective psychological mechanisms such as self-persuasion and cognitive salience, as we elaborate below. In addition, for one university (School A), we measure compliance using *administrative records for the entire undergraduate student body*. This data allows us to compare our uptake rate with that of the school's population, a rare feature of vote intervention evaluations.

We find that this simulated intervention meaningfully increases an array of outcomes by approximately 4 to 6 percentage points, including political efficacy, political interest, signing a petition, and actual turnout. The turnout ITT estimate is 4.4 percentage points. Compared to a recent overview of GOTV effects, that is more than double the highest typical ITT effect, and more than 3 times the highest typical ITT effect in high-salience elections (Mann and Haenschen 2024, Table 2). To be sure, the effect is not uniformly positive for all subgroups; some students who are unsure if they are already registered and who also live in places where registration is difficult become less likely to register, though not less likely to vote. With that caveat, our overall findings suggest that universities can play a significant role in creating a civically engaged student body.

Rather than focusing exclusively on what they teach, universities can also exercise their bureaucratic power. This implies a very different model of higher education. By building an opt-out voter registration process into the administrative procedures

⁴ Bergan et al. (2022) relied on voter records for outcome measures. While it also fielded a survey, the survey data plays a much more peripheral role. That survey was fielded after the election, and thus, after the target behavior. It also did not measure attitudinal outcomes. In addition, it was opt-in, and had a 15% response rate. The present study builds on it and fills these gaps.

⁵ Bergan et al. (2022)'s intervention required cooperation from individual instructors. Most students assigned to treatment did not receive it for that reason (Bergan et al. 2022; Fig. 1). In the present study, the intervention can be delivered without instructors.

required of students, universities can respect the individual autonomy of students yet foster active citizens. In that sense, administrative interventions can function in addition to other factors that may help students become more engaged with politics, such as peer effects, broad socialization processes, or instructors' encouragement (Bergan et al. 2022; Kloffstad 2015; Shulman and Levine 2012).

Past Research

We draw on three cognitive behavioral literatures to derive hypotheses about an effective institutional intervention to promote political participation: nudge, self-persuasion, and cognitive salience. They are grounded in theories that center on the common human motivation to preserve cognitive effort and to rely on cognitive shortcuts and heuristics. This in turn produces a high degree of responsiveness to a choice environment that reduces effort (Leighley 2013).

Among the most significant barriers to voting for young people is the difficulty they face in registering to vote (Holbein and Hillygus 2020). College students face unique barriers that make it more challenging to register to vote. First, students attending college outside of their home state are able to register to vote in either their home state or the state where they attend college. Second, college students often move each year and need to update their registration each time they move.

One helpful approach to encouraging difficult action comes from “nudge” theories. “Nudge” is a well-developed concept in behavioral science. We refer specifically to small interventions in the choice environment that can lead to substantial changes in behavior (Thaler and Sunstein 2009). Importantly, nudges do not limit the choices available to an individual. Rather, they change the structure of the choice environment. Such nudges are increasingly used to promote beneficial behavior such as healthy habits or increasing savings.

Thaler and Sunstein (2009) use the example of organ donations. In some countries, individuals are automatically signed up to be organ donors, and they can opt-out of being a donor if they choose. This makes being an organ donor the default action. The United States uses an opt-in organ donation policy. The default option is that individuals are not organ donors, and an individual can opt-in to be an organ donor. Johnson and Goldstein (2003) conducted a survey experiment to test the default options of being an organ donor or not being an organ donor. The scholars showed participants opt-in, opt-out, or neutral (no default) donor consent forms and measured the number of people who decided to be organ donors. In the opt-in condition, only 42% of respondents were donors, and in the opt-out condition, 82% were donors. In the no-default condition, 79% of respondents were donors. This shows that the default option matters and changing the default option can provide a nudge for individuals to take actions they are inclined to take but find difficult. Similar nudge findings come from other domains, such as retirement savings (Douglas et al. 2015).⁶

⁶ See Maier et al. 2022 and Mertens et al. 2022 for recent discussion of this literature. Maier et al. (2022) argue that correcting for publication bias eliminates the average nudge effect. Mertens et al. (2022) disagree with this conclusion. In our view, Maier et al.'s findings do not mean that well-designed nudges

Here, we apply nudge theory to voting and civic engagement. Young people are often first-time voters, and have had less experience with registering to vote and voting. They lack the concrete knowledge and have yet to develop the skills necessary to navigate the bureaucracy of registering and voting. They also do not have the psychological benefit of habit, unlike older, long time voters. As a consequence, young people are more likely to have a psychological default of not voting. This is a type of “status quo bias” (Thaler and Sunstein 2009). This bias leads individuals to choose the least costly default action even if the non-default action is beneficial for them. A nudge intervention, then, is one that attempts to change the default option. Instead of a policy requiring people to undertake a psychologically effortful set of unfamiliar steps, opt-out policies make action easy by increasing the cost of not taking that action and reducing the effort necessary for the desired actions (Thaler and Sunstein 2009).

Automatic (default) registration is a common use of opt-out policies. These policies show promise for increasing the number of new registrants (McGhee and Romero 2020). There is suggestive evidence that automatic opt-out voter registration at Departments of Motor Vehicles increases youth voter registration and turnout (McGhee and Romero 2020; Morris and Dunphy 2019; Rakich 2019).

This is in line with general findings that reforms designed to make voter registration easier sometimes have a substantial impact on youth. For example, preregistering young adults before they become eligible to vote increases their turnout by up to 6 percentage points (Bertocchi et al. 2020; Holbein and Hillygus 2020). Allowing for same-day registration may increase youth turnout by 3–7 percentage points (Grumbach and Hill 2022). Thus, reducing the costs of voting and making it easier for young adults can increase the number of youths who get registered and vote.

In sum, then, we draw on the nudge literature to form our hypothesis that embedding participatory encouragements and facilitating participation within the university online administrative process will boost participatory attitudes and behaviors.

In addition to the nudge literature, the *self-persuasion* literature also offers useful insight for formulating hypotheses about effective interventions. This literature also draws on cognitive behavioral theories. Specifically, when a person tries to engage someone else in an action, this in itself can increase one’s own engagement. Interventions that have involved young people inviting others to engage in a specific act, such as practicing good study habits (Eskreis-Winkler et al. 2019) and persuading others about a stance in a debate (Schwardmann, Tripodi, and Weele 2019), increase their own likelihood of undertaking that action. These motivations may be especially strong among friends and family (Dahlgaard 2018; Nickerson 2008).

Self-persuasion evidence also exists in the GOTV literature. In the domain of voting, programs in schools that have encouraged young people to register their eligible friends and family (as a part of other school programs) have shown positive returns (Gill et al. 2018). Likewise, Bergan et al. (2022, p 2029) find effects from a bundled classroom delivery of GOTV materials including voting pledges, which students are

are ineffective, or that no nudge is effective. In fact, even Maier et al. find considerable heterogeneity in publication-bias-corrected estimates. Some nudges do succeed. Our intervention belongs in the category of structure nudges, which even in Maier et al’s results show a meaningful effect.

asked to share with their roommates. Building on this self-persuasion literature, we hypothesize that encouraging students to share voter registration resources and vote reminders with their friends and family will increase the student's own likelihood of taking political action.

A third cognitive behavioral literature useful for designing an intervention focuses on the role of *cognitive salience*. Even when people want to take action, they may not do so if they are busy or distracted. Reminders are a commonly used and effective behavioral approach. They curb forgetfulness by making a certain action or choice salient. Research has shown that reminders are effective at changing behavior (Hummel and Maedche 2019).

Again, evidence for this cognitive salience mechanism also comes from the GOTV literature. While reminders of an upcoming election are ineffective at increasing voter turnout overall (Green and Gerber 2015), they are effective for individuals who have already registered to vote (Dale and Strauss 2009) or have made a commitment to vote (Costa, Schaffner, and Prevost 2018). For example, reminders sent after individuals made a pledge to vote increased voter turnout by 4–6 percentage points (Costa, Schaffner, and Prevost 2018), and election reminders sent to already registered individuals increased voter turnout by 3 percentage points. We build on this literature to hypothesize that embedding voting reminder sign-ups within the academic sign-in process will increase political engagement and participation.

Universities have the ability to implement opt-out practices that reduce the cognitive costs and barriers students face to registering and voting. We hypothesize that adopting these practices will increase students' political participation and engagement. We designed an intervention whose contents draw on psychological mechanisms from the literature on nudges and from the GOTV literature. Specifically, the GOTV literature informs our use of self-persuasion and of cognitive salience in designing the contents of an opt-out intervention.

Data and Methods

The ideal research design to test our hypothesis would be to embed our intervention in the administrative procedures at a random sample of colleges and randomize students in each university to receive it or a placebo. Given logistical constraints and universities' reluctance to randomize interventions, we undertook the next best approach. Specifically, we embedded our intervention in an academic year sign-in simulation within a survey we sent to college students. We will describe the design below.

We utilized two sampling methods. We sent an email invitation to the survey to all School A undergraduates in August 2020, just before the university's academic year sign-in. To recruit students attending other colleges we used a snowball convenience sampling method: we contacted professors teaching large undergraduate political science courses in the fall of 2020 and asked them to send our survey invitation to their students. All participating students were entered into lotteries to win \$100. The payment lottery mimics the incentive to complete the academic sign-in including

completing the opt-out treatment (or placebo).⁷ We report response and completion rates in Appendix 2. All references to an Appendix are to the online supplemental appendix.

The sampled students come from 14 universities. The universities are located in various regions, including the Northeast, South, and Mountain West. They have student bodies ranging from liberal to conservative. They are predominantly selective private schools. They thus have somewhat higher percentages of white and Asian students, but also more Pell Grant recipients, than the national populations of both private and public schools. They are roughly representative of the national population of both private and public schools on gender. Appendix 2 reports these details. The number of students surveyed at each university in our sample varies (see Appendix 2).⁸ Appendix 2 reports response and completion rates where we have that data. In the conclusion, we discuss the sample's generalizability.

Our intervention consists of a single treatment and a single placebo-control group. To increase precision, we used non-adaptive sequential block-randomization by self-reported parental education (a strong predictor of young adults' political engagement) and by self-reported pre-treatment registration status. Within each block of parental education and pre-treatment registration status, students were randomly assigned to the treatment or control group as they started taking the survey. Complete randomization happened sequentially within each block.

We use a "grand treatment," which bundles three interventions from the nudge and GOTV literatures that have been shown to have some effect on relevant outcomes. This intervention aims to change the default registration process from opt in to opt out, and the content is designed to increase voting salience and utilize self-persuasion.

Our procedure consists of the following steps (Appendix 13). First, respondents are shown TurboVote.org embedded in the survey for at least 60 seconds and are asked to sign-up for Turbovote, which includes election reminders. Respondents are then walked through the process of registering to vote. Turbovote allows individuals to fill out the voter registration form for their state online. Respondents thus see how to complete and submit that form and are encouraged to do so. In the large majority of states registration is online. To those lacking ID from those states, Turbovote mails a voter registration form to their address in a pre-stamped envelope.

⁷ Students were offered payment lotteries (the email subject line said "Entry in 10 lotteries for \$100 for completing a quick academic survey.") The survey-based nudge consists of having to undergo a voter registration and voting process in order to receive a survey payment, embedded within a simulated academic sign-in. For realism, we drew on the actual School A academic check-in visuals and text. This mimics a nudge in an actual academic sign-in where students have to undergo a voter registration and voting reminder sign up in order to become students in good standing at the university. To be sure, the survey-based intervention is not the same as an actual academic year sign-in. Nevertheless, in important ways the survey did mimic a 'nudge' embedded in an actual academic sign-in. In both the survey simulation and actual academic sign-in process, students have to complete a bureaucratic process in order to accomplish their goal, and the motivating goal is not political in itself. They can opt out, but if they do so they cannot achieve the goal (of getting checked in for school, or receiving payment as a survey respondent). The simulation is a weaker version of the administrative process, both in the number and range of students exposed to it and in the effect on students.

⁸ Nearly 93% of our sample attend private schools.

The second step encourages respondents to contact their friends or family members. This involved a brief encouragement to share the Turbovote.org link on social platforms by directly messaging or tagging their friends and family. The platforms included Facebook, Twitter, WhatsApp, and email. Respondents were able to click on links in order to share (a message of “Join me in registering to vote” with a link to Turbovote auto populates on the platform). Screenshots of this step are in Appendix 12.

The placebo-control condition mirrors the process of the treatment group, but uses recycling instead of voting (Appendix 14). Placebo respondents were asked to calculate their plastic use on carbonfootprint.com and to sign up for recycling reminders and pledge to reduce and recycle plastics on recyclingpartnership.org. Respondents had to remain on each of these pages for at least 30 seconds. They were then asked to share the recycling pledge link on social platforms by directly messaging or tagging friends and family. Afterwards, respondents were encouraged to form a personal recycling intention plan. The placebo condition is identical to the treatment but without mentioning or promoting political engagement or behavior. (To the extent it does prompt political engagement, treatment effects will be under-estimated, setting up a difficult test of our intervention. See Appendix 15.)

We measure four types of outcomes. First, we measure a set of attitudinal mechanisms that may be increased by the treatment and in turn affect political participation. These mechanisms are political efficacy, interest in politics, intention to vote, and the belief that registering to vote is easier than it may seem. Providing young people with the opportunity to practice difficult tasks may make those tasks seem easier and lead them to believe they are able to complete them and interested in doing so. These are all self-reported survey measures.

Second, we include a set of behavioral measures of civic engagement in the survey. Respondents were asked if they were willing to sign a petition encouraging elected officials to make voter registration automatic, for example, through school administrative procedures. To those willing to sign, we provided a link to the petition on change.org. We measured whether a respondent was willing to sign the petition, clicked on the link to sign, and actually signed. These measures go beyond attitudinal mechanisms and bypass the pitfalls of self-reports, to provide a validated behavioral measure of civic engagement.

Third, we measure actual voter registration and voter turnout. We matched students with L2 voter files, using their self-reported first name, last name, ZIP code, and street address from our survey.⁹ We used a deterministic matching approach since our data was fairly clean, and deterministic matching performed well, better than the probabilistic approach (Appendix 3).¹⁰ We conduct each match at the state level.

⁹ We obtained voter files from L2, Inc., a national non-partisan firm.

¹⁰ Differences between the matching approaches are not correlated with treatment assignment, and the two approaches yield similar effects (Appendix Table 6). Within each state, we use four iterations of merging using first name, last name, zip, and street address as our merging fields. We do an exact merge of three fields and a fuzzy match on the fourth field. We use a reverse Jaro-Winkler string distance of 0.8 or greater for our fuzzy match (Enamorado, Fifield, and Imai 2019). For example, a student would be matched to the voter files if they had an exact match on first name, last name, and zip code and a fuzzy match on street address. This allowed us to match street address that had slight variations in spelling.

Since students can register in either their home state or university state, we perform a match for each student twice, once for each of these two states. The voter file measures offer individual-level behavioral outcomes not susceptible to error from self-reports (Fraga and Holbein 2020).

Finally, for School A, we have individual-level administrative records of Turbovote use for the entire population of students. This unusual data allows us to compare the uptake rate of our treated group to the uptake rate of the entire student body. We can thus assess whether our intervention prompts more students to use Turbovote than the school's other efforts do. (We will detail this analysis in the results section). To be clear, except for this TurboVote compliance analysis, which only use School A students, our analyses use all survey respondents from all our schools.

Our research design and main hypotheses were pre-registered.¹¹ We include de-identified data and code for replication in Dataverse.¹²

A total of 1,925 respondents began the survey. Of these, 467 dropped out of the study before they were exposed to any treatment materials. We set them aside from all analyses, as doing so cannot bias our treatment estimates. Another 532 stopped the survey post-treatment, after being exposed to the voter registration or recycling information. We retain these observations in our analysis of voter records, to avoid post-treatment bias. Appendix 2 reports rates of survey response, treatment exposure, and survey completion.¹³ We found no meaningful selective attrition (Appendix 4 reports attrition checks).¹⁴

Of the 1,458 students exposed to the treatment or placebo materials, we match 71.7% to the L2 voter files. Overall turnout in this sample was 56%.¹⁵ For those matched to the voter records, turnout was 78.2%.¹⁶

Results

Because not all students finished the survey, we estimated intent-to-treat effects (effects among survey completers are in Appendix 4). Our analyses include students from all schools, except for the compliance results (to be shown in Figure).¹⁷ All models control for parental education and previous registration status, since these were used for block randomization. They also control for gender, race and ethnicity,

¹¹ <https://doi.org/10.17605/OSF.IO/8E7UP>.

¹² <https://doi.org/10.7910/DVN/MJC96A>.

¹³ In School A, where we can calculate the response rate, it is about 21%. Pooling schools, the percent of students exposed to the assigned content is 76% of those who began the survey; 926 completed the survey (completion rate of 48%). See Appendix 2.

¹⁴ Preregistered balance checks are in Appendix 5.

¹⁵ Compared to the national rate calculated by the National Study of Learning, Voting, and Engagement (NSLVE) for college students for the 2020 presidential election (Thomas et al. 2020), our sample rate is lower: NSLVE estimated a 83% voter registration rate and 66% voter turnout rate for the national rate; we estimate 72–74% and 56–60% respectively for our sample.

¹⁶ We cannot determine if registration happened pre-treatment or post-treatment, but this does not affect our treatment estimates since the control group captures the baseline pre-treatment.

¹⁷ Descriptive statistics are in Appendix 7.

Pell Grant recipient, age, and indicators for schools with the most students (Schools A and B), to improve efficiency. All covariate and blocking variables are self-reported.

Figure 1 reports the main Intent to Treat (ITT) estimates with 95% confidence intervals (using robust standard errors). The corresponding regression tables are in Appendix 8.¹⁸ As Fig. 1 shows, treatment assignment increased participatory attitudes and behaviors relative to assignment to the placebo. The effect is smaller for attitudes and intentions, and larger for actual behaviors. Specifically, treated students report greater efficacy (3.5 percentage points, $p = 0.03$), interest in politics (3.9 points, $p = 0.03$), ease of voting (2.9 points, $p = 0.04$), and intention to vote (2.2 points, $p = 0.05$). In addition, while these students are not more likely to *say* they will sign a petition to make voter registration automatic, they *are* more likely to click on the peti-

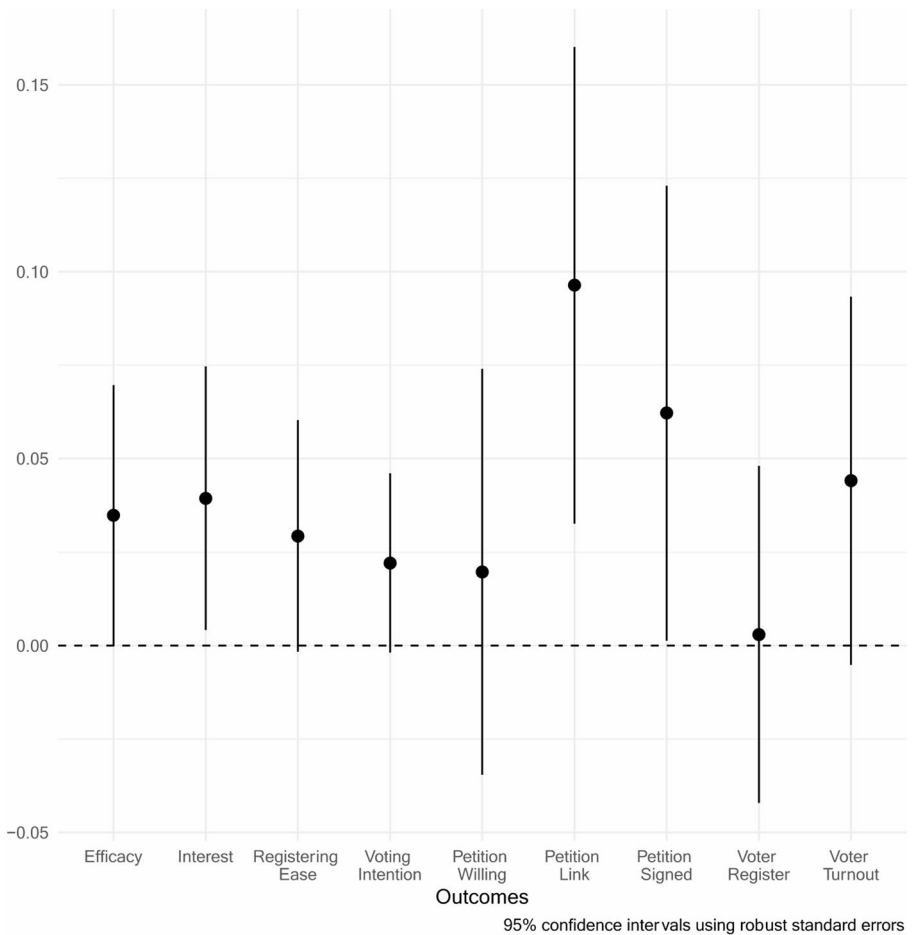


Fig. 1 Treatment effects on political attitudes and behaviors (ITT)

¹⁸ Models have an effective N between 842 and 1045.

tion link (9.6 points, $p = 0.003$) and to actually sign (6.2 points, $p = 0.02$).¹⁹ Finally, while treated students were only slightly more likely to register (and this effect is not statistically significant), they *were* more likely to vote (4.4 points, $p = 0.04$). P-values are one-tailed because we expect positive effects.²⁰ This effect on turnout is among the larger ITT effects in GOTV experiments with college students when pooling registered and unregistered students.

As expected, the treatment did not affect irrelevant outcomes (Appendix Table 16). Being encouraged to take political action should not increase the perceived ease of recycling or the intent to recycle. These placebo outcome measures test the possibility that students in the treatment group are merely willing to engage in any socially desirable behavior. The treatment did not increase attitudes unrelated to the voting intervention. Nudging students to vote increases their voting but not their intentions to recycle.

We also preregistered a secondary hypothesis: that the treatment may work differently among students who were not already registered to vote (e.g., Bergan et al. 2022). To test this hypothesis, before treatment assignment we asked respondents if they were already registered (see Appendix 7 for wording). We define students as sure of their registration status if they report “I know I am registered” (68%); we define all other students as unsure.²¹

Figure 2 displays intent-to-treat effects from the same model described above, separately by this measure of prior registration status. As Fig. 2 shows, the treatment increased participatory attitudes especially among students unsure if they were already registered pre-treatment.²² Among these unsure students, treatment increased engagement on most attitudinal outcomes, especially political interest (9.3% points).

However, the treatment has a more consistent and precisely estimated *behavioral* effect among students who indicated they knew they had registered to vote pre-treatment. As shown in the righthand panel in Fig. 2, for these sure students, the treatment had large effects on all behavioral outcomes, including registration (4.9 points, $p = 0.029$) and turnout (6.3 points, $p = 0.02$).²³

Why might this be? We address this question in two ways. First, why would the treatment get someone to do something they had already done, that is, get students to register after they were already registered? Notably, not all students who said they were already registered to vote were actually registered. Specifically, we did not

¹⁹ The petition variables measure the difference between the placebo petition about recycling and the treatment petition about voting.

²⁰ We employed a one-tailed test because our primary hypothesis predicts a positive effect. We base that hypothesis on the fact that the GOTV literature finds positive or null effects but almost never a meaningful negative effect, and on theories of nudges, which also expect positive or null but not negative effects. The unexpected negative effect among a subset of students in our study does not change the fact that our original expectation justifies a one-tailed test. We did not pre-register one- or two-tailed tests in our pre-registration plan, but expected a positive effect which corresponds to a one-tailed test. We report two-sided CIs in figures.

²¹ In our analytic sample, 982 students (631 who completed survey) knew they were registered prior to the treatment, and 476 students (295 who completed survey) were uncertain.

²² This was pre-registered as an exploratory analysis.

²³ P-values are one-tailed, calculated using permutation tests.

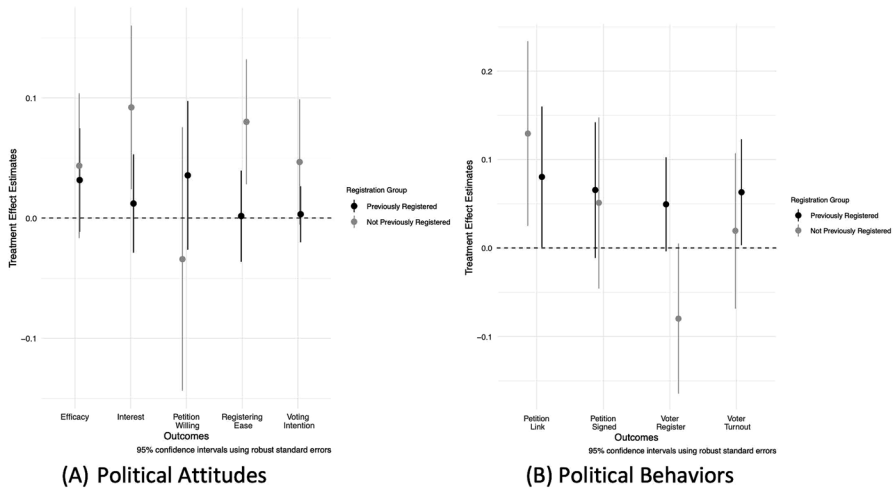


Fig. 2 Effects on political attitudes and behaviors, by Pre-Treatment Registration (ITT). **(A)** Political attitudes. **(B)** Political behaviors

match 36% of these students to the official records of actual voter registration.²⁴ To be sure, not all unmatched students are unregistered; some of them may simply be difficult to match. That said, it is likely that many of them had not in fact been registered. There is social desirability pressure toward saying one is registered to vote. In addition, some students who had been registered may have decided to register again in a new address. Students are a highly residentially mobile population, and moving requires re-registration (Grumbach and Hill 2022).

Second, why would those who say they were registered be more affected by the treatment? A likely possibility is that this is a proxy for an interest in voting or a sense of duty to vote. The treatment may tip these students over the behavioral threshold of action and cause them to do something they already wish to do (Bergan et al. 2022).

Moreover, as shown in Fig. 2, we found an unexpected large negative treatment effect on voter registration among students unsure of their prior registration status (−8 percentage points, $p = 0.08$, two-tailed). This finding is surprising given the *positive* effect in the same subgroup on voter turnout and on related attitudes (as we showed in Fig. 2).

A possible reason for this negative effect on registration among the unsure is related to students’ challenges with registering to vote in states that make registration difficult. These challenges would affect students who were not already registered to vote, as they may be less experienced with registering and possibly less politically motivated to overcome challenges with registration. In the state suggestion logic we used for our survey, we had identified states as being difficult if they did not allow online registration or if they required a separate form of ID to vote absentee. After discovering the unexpected negative effect on registration among the unsure, we used that state suggestion logic to create a measure that takes a value of 1 if a student has a

²⁴ This is less than the overall percentage of student matched to voter files (74%).

state ID from a difficult state or doesn't have a state ID, and 0 if they have a state ID from an easy state. We then contrast the effect for those with a state ID from a difficult compared to an easy state. Our analysis here is restricted to students unsure of their registration status pretreatment. We find that the negative effect on registration is driven by students from states that make registration difficult: The effect among these students is -16 percentage points ($SE = 0.08$, $N = 140$), compared to -5 percentage points ($SE = 0.054$, $N = 336$) among students from states with easy registration. That is, the negative effect is statistically indistinguishable from zero in easy registration states, and large and precise in difficult states. Students unsure of their registration status appear to be deterred from registering if their ID is from states where registering is difficult. We replicate this finding using another definition for hard versus easy registration states (Appendix 9). We caution, however, that our sample of these unsure students is small. The possible negative effect on registration on those who have not registered in the past should be examined in the future with better-powered unregistered samples.

In sum, the intervention increased participatory attitudes among students unsure if they were already registered, and increased political behaviors (petitioning, registering, and voting) among students who said they were already registered. It nudges students who say they are registered but may not have been registered at their current address to actually do so, and to vote. However, the treatment caused a decrease in registration among unsure students, despite increasing participatory attitudes in this same subgroup. This negative effect is driven by students with IDs from states that make registration difficult. Essentially, administrative procedures that encourage and enable students to sign a petition, to register, to vote, and to encourage others to vote can be highly effective for most students, but further research is needed on students who face registration barriers.

Another source of possible heterogeneity is race and class. We preregistered exploratory tests of heterogeneous effects by race and SES, as universities may have different effects on students from relatively disadvantaged backgrounds (Mendelberg et al. 2020). Accordingly, we test for an interactive effect of the treatment and, respectively, race, parental education, and Pell Grant status, in separate models. None of these models yield heterogeneous effects (see Appendix 10).²⁵ The intervention is equally effective for students from relatively advantaged and disadvantaged backgrounds.

Lastly, we report compliance rates. We have administrative individual-level records of Turbovote use for the entire population of students in School A. Thus, we can measure each of those students' actual compliance within the Turbovote website. These compliance measures are: whether they signed up for Turbovote at all, whether they signed up for Turbovote specifically through our survey (with our link), and whether they signed up for Turbovote without our link. Figure 3 reports these raw compliance measures for School A. As Fig. 3 shows, 88.4% of students assigned to treatment signed up for Turbovote at all, while only 39.2% of students assigned to the placebo did so ($p < 0.000$). That is, the treatment group's sign-up rate is more than twice the placebo group's rate. This large difference is remarkable given that School

²⁵ A pilot study with students at School A is broadly consistent with the paper (See appendix).

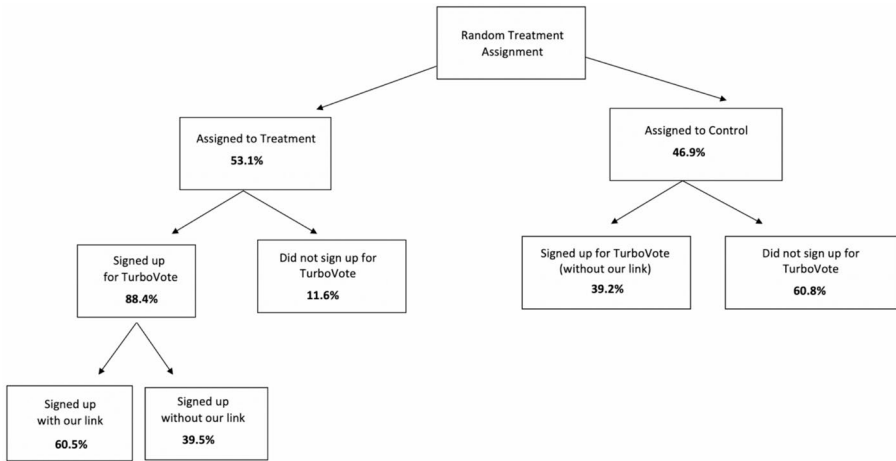


Fig. 3 Compliance: Turbovote Sign-up (School A)

A had a large campus-wide effort to register students to vote using Turbovote (this is why as many as 39% of students in our placebo condition signed up for Turbovote).²⁶

Of these 88.4% who were assigned to treatment and signed up for Turbovote, 60.5% signed up for Turbovote specifically using the link embedded in our survey (the remaining 39.5% did so using links offered by School A through other means). This is a high level of compliance compared to GOTV efforts using phone-banking and canvassing, which have contact rates of around 25–35% (Bennion and Nickerson 2019; Gerber and Green 2000; Ha and Karlan, 2009).²⁷

Furthermore, 20% of students who signed up for Turbovote using our link then actually registered to vote through our Turbovote link (not shown in the figure). This is roughly twice the rate of students who registered to vote after signing up for Turbovote *without* our link ($p = 0.003$).^{28,29}

In other words, a large majority of students assigned to our treatment actually engaged with Turbovote, and did so at more than twice the rate of the control group, and once on Turbovote, treated students were twice as likely to register to vote. The overall ITT for School A on voter registration is 7.3 percentage points. This suggests our procedure was much more effective both in getting students onto Turbovote and

²⁶ School A attempted to disseminate TurboVote information to all students through student-led efforts and included a TurboVote link in its actual academic year sign-in (but did not *require* students to click the link, engage with TurboVote, or register). This distribution began on August 24 th, halfway through our fielding (we fielded August 21– August 27). That is, even the control group were exposed to TurboVote, through means other than our survey. Thus, our treatment effects on registration and turnout may be depressed.

²⁷ As expected, no control student signed up using our link.

²⁸ The difference from students who registered after signing up for Turbovote *without* our link is similar for the treatment and placebo groups: 11% in the treatment and 9% in the control condition; $p = 0.003$ for each.

²⁹ In an experiment where students were emailed a link to download a voter registration form, only 4% downloaded the form (Bennion and Nickerson 2011).

at promoting follow-through once on Turbovote, compared to alternative means of getting students onto the same platform.

The foregoing compliance data comes only from School A. To assess compliance in the other schools, we collected other compliance measures for students in all schools, using self-reports (see Appendix 6, including Appendix Table 9). Those results show that compliance was high in the sample as a whole. For example, 85% of respondents assigned to the treatment and who completed the survey reported registering to vote during the treatment.

Discussion

Our results suggest that embedding voter registration and vote reminder resources into universities' administrative procedures may help universities create a more politically engaged student body. We find that encouraging students to register to vote, to sign-up for election reminders, and to share registration and vote reminder resources with friends and family in an academic year sign-in simulation increases students' participatory attitudes and behavior. The effects on behaviors are stronger for students who say they have registered. Importantly, when matching these students to voter files, we found a nontrivial number who had not actually registered. Providing registration materials in administrative procedures can be highly beneficial even for students who say they are already registered.

We do find a subset of students who may not be as positively affected. These students are uncertain of their registration status and have state IDs from states that make it difficult to register to vote. This is a small subset of our sample, and future research would benefit from seeing whether these findings replicate, and developing a better understanding of the barriers for this group of students.

The positive, main findings are consistent with the cognitive behavioral literature on nudges. By including these resources in administrative procedures as an opt-out process, students can be effectively encouraged to register, to vote, and to become more politically efficacious and active. The intervention affects not only narrow behaviors, but also related attitudes, promoting various forms of political engagement. This suggests it may change long term habits.

The findings here invite scholars to rethink the main channels by which higher education may matter. Universities can execute their civic mission not only through curricula or service learning, but through their administrative powers (Bennion and Nickerson 2022). Higher education institutions can do more than offer civic education. In fact, evidence is unclear about whether civics curricula have large causal effects on political efficacy or political action (Mulder 2023; Willeck and Mendelberg 2022). The results of our study suggest universities *can* have a causal impact on these important outcomes. They are not only teaching institutions; they are also bureaucratic institutions, and they can fulfill their broader civic mission — now legally mandated — through this neglected but powerful channel.

Including our intervention in administrative procedures would make for a simple, scalable policy reform, one that could substantively increase the number of new registrants and voters each year, helping to increase voter turnout among young people

and create a more representative democracy. That said, our finding on students uncertain of their registration status and face higher hurdles to registering implies this intervention may affect subsets of students differently. Further research is needed to understand whether this intervention could have a boomerang effect for some students, why that may happen, and what the implications of this are for broader policy implementation. Overall, however, the intervention we tested shows promise in raising students' political engagement and actual voting, and is plausible as an actual policy that universities can adopt. Some universities already include voter registration materials in their academic year sign-in.

For example, after we administered our surveys to School A undergraduates, the school included a voter registration link in its required academic year sign-in.³⁰ School A students then registered and turned out to vote at high rates: 86% of students registered, and 75% of students voted, compared to the national college student average that year of 83% and 66% respectively (Thomas et al. 2020). These high rates at School A were achieved partially through Turbovote, the platform we used in our study. We know this from our analysis of the university's administrative records of Turbovote uptake in 2020: 43% of students signed up for Turbovote, and 22.4% of those students actually registered to vote through Turbovote. Along with this observational data, the results of our study suggest that our proposed intervention can be effective at increasing voter registration and turnout as well as participatory attitudes like political efficacy.

There are important caveats about the external validity of our study. Our sample was not a representative sample of college students. For example, it consists mostly of full-time students at private universities. Such students may differ systematically from part-time students and from public university students, for example, in their socioeconomic status. In addition, private and public schools differ as institutions, not only in the demographics of their students. Relevant institutional characteristics may include the school's culture of voting, the number of students enrolled, the school's endowment, the cost of attendance, and the level of financial aid the school provides (Mendelberg et al. 2020). For example, in schools offering robust financial aid, students from lower-income backgrounds benefit from a norm of peer voting (Mendelberg et al. 2020). Perhaps, then, the extent to which a school serves the needs of all students and the generosity of its financial aid will affect whether our intervention succeeds.³¹ The intervention may work as well, less well, or better in a different type of school and different student population, and there are many potential sources of heterogeneity across students and across schools, including full- or part-time status, two- or four-year schools, and so on. In addition, the COVID pandemic may have affected the results of this study. Generalizability across elections and across populations of students and schools is a subject for future research.

In addition, one might object that this intervention does not tackle the still lower voting rate of young people who do not attend college. From a democratic perspective, it is certainly problematic that non-college young people have an even lower chance for political representation than their college-attending same-age peers. That

³⁰ School A undertook extra effort to register students in the 2020 election.

³¹ Our sample of private schools has somewhat higher percentages of Pell recipients.

said, it is nevertheless also important to address college student voting, and colleges are legally mandated to help them do so. While these young people may vote at higher rates than non-college peers, there is room to increase college students' civic engagement and voting, which falls far behind older ages (if not prior cohorts at the same age). And while many young people never attend college, most do. According to a recent Bureau of Labor Statistics survey, 62% of high school graduates were enrolled in college or university as of October 2023.³² This study suggests a potentially powerful positive role of institutional bureaucracy in increasing political participation among young citizens who attend college, who make up the large majority of young people. Certainly, we caution against generalizing the findings from our study to all young people. However, the generally positive findings call for increased attention to how institutional bureaucracy might leverage opt-out procedures in various settings and with various populations. Studies are already emerging with suggestive evidence that such procedures make a difference when implemented at DMVs, which serve a large cross-section of the young population (e.g., McGhee and Romero 2020; Morris and Dunphy 2019). The present study contributes to this literature new evidence from a randomized design with both administrative and survey data and both attitude and behavior measures.

The nudge approach lends itself well to the problem at hand. A large majority of Americans would like to vote but believe "elections are poorly designed in the United States to help people do what they think they should do" (Elliott 2017). A nudge-based policy is attractive because unlike mandates, bans, or economic incentives, it preserves liberty while cognitively incentivizing actions people already intend to do. Whether it works well for some and poorly for others should be the focus of future research.

Supplementary Information The online version contains supplementary material available at <https://doi.org/10.1007/s11109-025-10048-0>.

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Data Availability Data and code are at: <https://doi.org/10.7910/DVN/MJC96>.

Declarations

Conflict of interest The authors declare they have no financial or non-financial interests to declare.

Ethics Approval The study was approved by the Princeton University IRB (protocol # 0000008176, first approved March 28, 2017, approved for continuing review through August 13, 2025). It was performed in accordance with the ethical standards of the 1964 Declaration of Helsinki and its amendments or comparable ethical standards, including securing informed consent from participants.

³² <https://www.bls.gov/news.release/hsgec.nr0.htm>.

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