


LETTER

Educating for Democracy? Going to College Increases Political Participation

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Abstract

It is a long-standing view that educational institutions sustain democracy by building an engaged citizenry. However, recent scholarship has seriously questioned whether going to college increases political participation. While these studies have been ingenious in using natural experiments to credibly estimate the causal effect of college, most have produced estimates with high statistical uncertainty. I contend that college matters: I argue that, together, prior effect estimates are just as compatible with a positive effect as a null effect. Furthermore, analyzing two-panel datasets of $n \approx 10,000$ young US voters, using a well-powered difference-in-differences design, I find that attending college leads to a substantive increase in voter turnout. Importantly, these findings are consistent with the statistically uncertain but positive estimates in previous studies. This calls for updating our view of the education-participation relationship, suggesting that statistical uncertainty in prior studies may have concealed that college education has substantive civic returns.

Keywords: education; voter turnout; political participation; college; statistical power

Introduction

Whether citizens actively participate in politics is crucial for the quality and legitimacy of democracy (Dahl 1989; Lijphart 1997). Scholars have argued that if democracies fail to maintain a high level of participation, they will suffer from decreased institutional performance and responsiveness (Putnam, Leonardi and Nanetti 1993). Furthermore, unequal participation between societal groups may lead to bias in democratic representation (Lijphart 1997). A long-standing view in democratic thought states that education can serve to keep these negative effects at bay by creating and maintaining an engaged citizenry (Aristotle ca. 350 B.C.E./2013; Lipset 1959; Wolfinger and Rosenstone 1980). But while the link between education and participation seems solid for pre-adult education, a number of studies have seriously questioned the existence of a causal effect of college education on political participation (Berinsky and Lenz 2011; Burden et al. 2020; Dinesen et al. 2016; Henderson 2018; Kam and Palmer 2008; Lindgren, Oskarsson, and Persson 2019; Milligan, Moretti, and Oreopoulos 2004; Persson 2014; Sondheimer and Green 2010; Tenn 2007). If accurate, this is an important limiting condition on education's ability to sustain democracy, which challenges the idea that increased enrolment in college will address the democratic challenges of unequal turnout and declining youth participation (Holbein and Hillygus 2020; Lijphart 1997).

While the literature has made impressive progress by increasing internal validity, this study argues that existing work does not, however, enable us to rule out that attending college has a sizeable effect on political participation. A recent group of studies credibly rid their effect estimates of selection bias, i.e. by investigating admission grade cutoffs in an RDD (Apfeld et al. 2022) and as-if random variation in college enrolment due to the Vietnam draft (Berinsky and Lenz 2011). However, these strong causal research designs also came at a cost. In particular, because they rely on a small amount of the total variation in educational attainment and because they focus on special populations (for example, draftees or twins), they are only able to estimate the effect of college attendance with large statistical uncertainty (Dee 2004; Dinesen et al. 2016; Persson 2014). This means that when these studies arrive at statistically insignificant estimates, we still cannot rule out rather large effects of college. Specifically, even if attending college increased turnout by 24 percentage points, only two of the studies surveyed here were sufficiently powered to detect it (see below). Consequently, what we need are causally credible studies that also achieve the statistical power to detect reasonable effect sizes. Furthermore, we need to compare such new evidence with the well-identified previous studies.

In this article, I advance this debate by providing a well-powered test of the effect of college on voter turnout by putting forward the first difference-in-differences analysis of this question. I utilize a nationally representative cohort panel survey of more than 10,000 US high school graduates, coupled with fine-grained administrative data on college enrolment over time. The panellists both graduated high school and became eligible to vote in 2004, and the survey spanned both the 2004 and 2008 presidential elections. This cohort timing allowed me to compare the change in turnout rates from 2004 to 2008 between respondents who went to college in this period (“treated group”) and respondents who did not (“untreated group”). This difference-in-differences approach thereby avoids bias from time-invariant effects of both observed and unobserved confounders that are fixed over time. I also address the remaining challenge from dynamic (time-variant) effects of fixed confounders as well as time-varying confounders later in the manuscript (Sant’Anna and Zhao 2020). Furthermore, this design relatively improves external validity since it allows me to estimate the average treatment effect for all college attendees instead of local complier effects (Apfeld et al. 2022; Berinsky and Lenz 2011; Dee 2004).

I find that attending college substantively increases voter turnout – especially among those without previous experience with political participation. As my approach relies on non-random variation in college attendance, I pay special attention to scrutinizing the identifying assumption of parallel trends in several ways and I analyze the sensitivity of the findings to various violations of it (Rambachan and Roth 2023; Sant’Anna and Zhao 2020). Importantly, using the subgroup of panellists who only attended college after 2008, I performed a placebo test, which indicates that turnout trends from 2004 to 2008 were parallel for future college attendees and future non-attendees. I also replicate the analyses using a second independent panel sample. While the design is not perfect, the very similar results across different tests with different assumptions – and across samples – support a causal interpretation of the results.

When comparing my findings to the existing literature, it becomes clear that the well-identified previous studies are, in fact, compatible with a substantial positive effect of college. Figure 1 makes this comparison of estimates and confidence intervals between the current study and the literature surveyed above. Notably, all previous estimates are positive, and they centre quite clearly around the effect size found here. This study’s estimate is within four of the five confidence intervals of previous studies. The figure also presents a meta-analytic average of the effect sizes, which is substantially positive and statistically significant also when we include only previous studies – and generally across meta-analytic specifications and selection criteria (see below). This is exactly what we would expect if college did have an effect, but this effect had been concealed by statistical uncertainty in previous studies. These findings help explain and reconcile the unsettled debate about whether college education causes voter turnout or not (for a review, see Aarøe et al. 2021; Persson 2015; Willeck and Mendelberg 2022). Thereby, this study provides a new perspective on

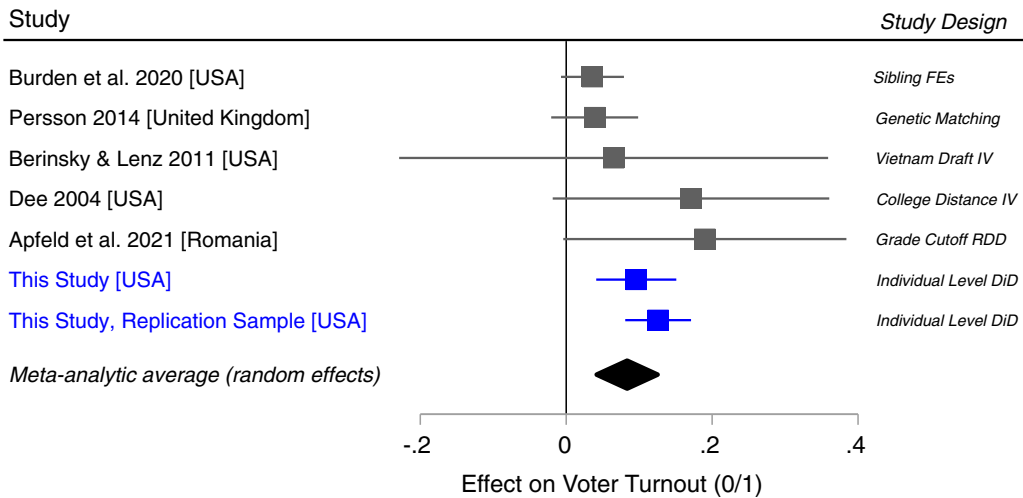


Figure 1. Effects of College on Voter Turnout.

Note: Estimated effects of college education on voter turnout in the literature. See Appendix C2 for details on the studies. Appendix C3 compares their statistical power. The bottom two rows show meta-analytic averages: The first uses all seven studies while the second uses only previous studies. Both are estimated using random effects (RE) weighting studies by inverse variance (see Appendix C4 for different meta-analytic specifications and inclusion criteria). The width of the diamond indicates the 95% confidence interval. Appendix C1 displays the similar mixed pattern among studies having an index of participatory acts as outcome rather than turnout.

the literature, suggesting that the mixed picture of existing evidence is attributable to a lack of statistical power that has arguably concealed these civic returns to college education.

The current study comes with important shortcomings. Specifically, it relies on self-reported turnout and using administrative voting records, therefore it constitutes an important avenue for future research. Moreover, the applied research design tests a rather short-term effect of attending college. Thereby, the study merely scratches the surface of theoretically relevant heterogeneities in the civic returns to college. Future studies may fruitfully continue by empirically investigating whether college effects diminish with age and how individual-level effects have aggregate consequences for democracy depending on national contexts (Nie, Junn, and Stehlik-Barry 1996).

More broadly, the results support the general idea that educational institutions sustain democratic institutions by increasing students' political participation (Lipset 1959). Moreover, I find evidence that college compensates for lack of prior experience with political participation (Neundorf, Niemi, and Smets 2016). This is compatible with prior research on the expansion of earlier stages of education (Lindgren et al. 2019): Expanding college uptake to those who are less likely to vote has the potential to reduce inequalities in political participation.

The Connection between College and Voting

There are strong theoretical reasons to expect college education to impact political participation. Particularly, college may provide students with resources that are relevant for participation (Brady, Verba and Schlozman 1995). In the short run, these include relevant knowledge as well as psycho-social skills and social relations that enable citizens to follow through on their intention to vote (Hill 2020; Holbein 2017; Holbein and Hillygus 2020). Furthermore, social relations that entail a norm of civic duty may increase the social benefits of voting (Fieldhouse and Cutts 2020; Wolfinger and Rosenstone 1980). A coexisting strand of theorization posits that college attainment is *not* a cause of political participation: it states that we may see an empirical link, but pre-adult differences in socialization and genetics may be the real causes of both increased

educational attainment and participation (Jennings and Niemi 1974; Kam and Palmer 2008). From this perspective, educational attainment is “a proxy for preadult experiences and influences, not a cause of political participation” (Kam and Palmer 2008, 612).

The current study compares education effects from different contexts, election types, and research designs, and presents a difference-in-differences analysis of US college-goers and their turnout in presidential elections. Existing literature contains important insights on how we should expect effect sizes to vary between such settings. First, all elections are not created equal. Specifically, the literature suggests that the effect of education should be largest in elections where participatory norms are less salient and information more scarce (Ahlskog 2021). This suggests that studying presidential elections plausibly constitutes a hard test for education effects. Second, and contrastingly, the so-called law of dispersion posits that high-turnout elections leave the least room for stratification in turnout (Tingsten 1937; Persson et al. 2013). This means that education effects should be highest in low-turnout contexts, such as the US case. Third, we should distinguish between the long- and short-term effects of education: in Plutzer’s (2002) inertia framework, habitual voting is a behaviour that is more stable over time than non-voting. Thus, the causal effects of education on voter turnout likely decay with ageing, as less educated non-voters may eventually catch up and become habitual voters. Fourth, and finally, theories of relative education state that participation is a positional good that individuals may obtain by increasing their education, but this occurs at the cost of turnout for other individuals (Nie, Junn, and Stehlik-Barry 1996). If education effects are completely relative, then college merely redistributes voter turnout rather than boost overall participation rates. Empirical investigations suggest that education effects are both absolute and relative (Campbell 2009; Tenn 2005; Persson 2013; Helliwell and Putnam 2007), which means that college expansion likely affects participatory inequality both through increasing and redistributing voter turnout.

Existing Evidence, Statistical Power and Meta-Analysis

The empirical evidence on the connection between college and turnout is mixed. While several recent studies are based on causally credible research designs, I argue that most null findings in the existing literature are not statistically powered to be taken as evidence against an effect of college. I theorize that this may explain the unsettled state of the literature where most studies do not detect a statistically significant effect (Apfeld et al. 2022; Berinsky and Lenz 2011; Burden et al. 2020; Dee 2004; Dinesen et al. 2016; Kam and Palmer 2008; Persson 2014; although see Mayer 2011; Henderson and Chatfield 2011). Moreover, I argue that viewing prior studies in conjunction quite unambiguously indicates a positive effect of college.

Figure 1 summarizes this argument by illustrating the estimated effects of college on voter turnout in the literature, and their associated confidence intervals and research designs. In Appendices C2–C3, I provide a detailed review and comparison of these contributions to the literature.¹ Specifically regarding statistical power, I find that three of these previous studies would only reliably detect it if attending college increased turnout by at least 24 percentage points (70% power, $\alpha = 5\%$). The studies by Burden et al. (2020) and Persson (2014) could detect minimum effect sizes of 5.0 and 7.5 percentage points, respectively. Importantly, Figure 1 illustrates that prior studies generally yielded positive but statistically insignificant effect estimates. Moreover, the two bottom rows of Figure 1 present meta-analytic averages of the effect sizes – including all seven studies or only previous studies, respectively. The average is substantially positive and statistically significant across meta-analytic specifications and various selection criteria (see Appendix C4).

¹I discuss a set of additional relevant studies in Appendix C1 and C2 (Gidengil et al. 2019; Tenn 2005; Tenn 2007), including studies that use a survey index of other participatory acts as the dependent variable instead of voter turnout (Dinesen et al. 2016; Henderson and Chatfield 2011; Kam and Palmer 2008; Mayer 2011).

While all the included studies estimate the effect of attending college on voter turnout, different factors may limit the comparability between specific studies as outlined above. First, the Romanian and UK contexts may differ on important scope conditions, but leaving both non-US studies out does not alter the meta-analytic conclusions. Second, the studies use different identification strategies and estimates are “local” to different subgroups of college-goers. Ultimately, even if we rely solely on the three most conservative prior estimates in Figure 1, the meta-analysis still yields a statistically significant effect of 4 percentage points (see Appendix C4). In sum, prior studies are compatible with a positive effect, but we lack studies with statistical power to detect or rule out plausible effect sizes.

Data: A Panel with a Particular Timing

To study the effect of college, I take advantage of the Education Longitudinal Study of 2002 (ELS:2002) that recruited a nationally representative sample of the cohort of US high school students who would graduate in 2004.² For this cohort, there was a presidential election in the year they graduated high school and became eligible to vote – which allows me to measure turnout both before and after college. Specifically, I compare the change in voter turnout from 2004 to 2008 between respondents who went to college in this period and respondents who did not. The longitudinal survey includes self-reports of voting in both elections. Specifically, respondents reported turnout for the 2004 and 2008 elections in the 2006 and 2012 survey waves, respectively. While relying on self-reported turnout entails important limitations, the current design mitigates important parts of this concern.³ First, the difference-in-differences design takes into account education-based differences in over-reporting that do not change over time. Second, survey attrition and non-response are more prevalent among those who do not attend college, which exerts a countervailing influence on the estimated effects by increasing our estimate of turnout growth among non-college-goers (Dahlgaard et al. 2019; Lahtinen et al. 2019). Nevertheless, relying on self-reported turnout does constitute a limitation of this study, which future studies could alleviate by seeking out administrative data on voter turnout.

The key independent variable is college enrolment, and to measure it, I leverage that the ELS:2002 survey data were coupled with administrative data on college enrolment. I operationalize college enrolment as whether a respondent was enrolled at a more than two-year post-secondary institution (treated) or not (untreated) in the specific time period between the two elections. Out of the 9,387 cohort members with complete information, 1,187 (12.6%) did not go to college, while 8,200 (87.4%) attended college in the period.⁴ To distinguish the effects of college from the effects of completing high school, I restrict the sample to those who completed high school before the 2004 election and were eligible to vote. Finally, I replicated the main analyses in a second independent cohort panel sample (National Education Longitudinal Study of 1988). This addresses potential concerns about the 2008 campaign where Barack Obama was first elected president being an exceptional case; for example, in terms of incentives for over-reporting turnout among the college-educated (see also Appendix H).

Analytical Strategy and Estimation

This article aims to study the causal effect of attending college on voter turnout. I define this quantity of interest for the college-going group as the average difference between turnout in the election after they attended college (2008) and turnout in a counterfactual situation where – in the

²For details and descriptive statistics, see Appendix F.

³Appendix H discusses the consequences of recall, self-reporting and non-response in detail.

⁴Appendix F2 analyzes representativeness and sample attrition.

same election – they had not attended college. Without access to randomized variation in college attendance, the panel data provides some advantages when seeking to identify this quantity. First, it allows me to take into account pre-existing differences in turnout levels by comparing the *changes* (rather than levels) in turnout between the treated and untreated respondents. This within-person approach strengthens causal inference since it avoids a specific kind of bias caused by unobserved time-invariant (fixed) confounders; namely, whether these confounders affect the levels but not the growth in turnout (Sant’Anna and Zhao 2020). Since fixed confounders may have an effect on both levels and growth in turnout – that is, time-invariant and dynamic effects, respectively – I also address the latter kind of confounding later in the manuscript.

Formally, I consider each respondent $i \in \{1, \dots, N\}$ at two time points $t \in \{2004, 2008\}$. The years 2004 and 2008 are the pre-treatment and post-treatment election years, respectively. D_i is the binary treatment variable, indicating whether the respondent went to college between the two elections or not. The binary outcome variable (Y_{it}) is whether a respondent (i) voted at a given election (t). For estimation, I rely on the following linear two-way fixed-effects model, which – in this two-wave case – is equivalent to a difference-in-differences estimator (Imai and Kim 2021, 412):

$$Y_{it} = \alpha_i + \beta_t + \delta[D_i \times PostPeriod_t] + \gamma X_{it} + \epsilon_{it} \quad (1)$$

Here, α_i denotes an individual-level unit fixed effect. β_t is a year fixed effect that controls for any general time trends in voter turnout. In some specifications below, I modify the year fixed effects to model counterfactual time trends that are specific to groups defined by pre-college characteristics (that is, covariate-by-year fixed effects). Formally, this replaces β_t with $[\beta_t \times Z_i]$, where Z_i is a vector of pre-college covariates. $PostPeriod_t$ is a binary variable that equals 1 in the post-treatment year. Finally, X_{it} is a vector of time-varying covariates.⁵ ϵ is the error term. Standard errors are clustered at the individual level.

The coefficient of interest is δ , which is the difference-in-differences when we compare the development in turnout from 2004 to 2008 between respondents who went to college and those who did not.

Plausibility of Parallel Trends

Without a randomly assigned shock to college enrolment, we rely heavily on the assumption of parallel trends in order for δ to identify the average treatment effect on the treated. This assumption requires that college attendees would – had they not gone to college – have experienced the same development over time in voter turnout as non-attendees. For instance, a possible violation of this assumption would be if the pre-college differences between college attendees and non-attendees, such as parental resources, led them to differential rates of growth in turnout. Given its importance, I closely scrutinize this assumption below.⁶

First, to indirectly test the parallel trends assumption, I use variation in future college attendance within the untreated group. I find that among those who eventually became college-goers (that is, after 2008), turnout did not develop differently from 2004 to 2008 than among those who never went to college (see Appendix A2).⁷ However, this placebo analysis has limited statistical power, which means that while we fail to reject that turnout trends were parallel for future college attendees and future non-attendees in this test, we cannot confidently confirm it either (Rambachan and Roth 2023).⁸ Therefore, second, I present a version of the main analysis

⁵Adding time-varying covariates assumes that the effects of covariates are homogenous between treatment groups (Callaway and Sant’Anna 2021; Goodman-Bacon 2021).

⁶“Appendix I” provides details on the five approaches outlined below.

⁷The estimated placebo difference-in-differences was 1.9 pp ($p = 0.59$).

⁸Based on the placebo estimates, Appendix A2 analyzes the sensitivity of the main findings to various magnitudes of post-treatment differences in trends using the logic suggested in Rambachan and Roth (2023). For instance, if we assume that the true violation of the parallel trends assumption is 4 percentage points, then all point estimates of the college effect would still

where I restrict the untreated group to include only the above-mentioned “future college attendees”. In this way, I only compare people who attend college at different points in time. This restricted comparison group model assumes that the *timing* of college rather than *college per se* is not correlated with counterfactual turnout trends. In Appendix E, I show that the results are not sensitive to varying the timing of college in the comparison group. Third, I present results with covariate-specific time trends, which means that time fixed effects are interacted with fixed pre-college covariates – specifically cognitive skills, gender, race, and parental SES (see Appendix I2) – to address potential bias from dynamic effects of fixed covariates; that is, predispositions to differential growth in turnout (see for example, Hall and Yoder 2022; Schafer et al. 2022; Hainmueller and Hangartner 2013). These specifications compute college-goers’ counterfactual trends using only respondents with, for example, similar pre-college cognitive ability. To control for the habit-forming effect of voting, I interact pre-college voting with time fixed effects, resulting in time trends that are specific to whether a respondent voted in the pre-treatment election or not (Coppock and Green 2016; Plutzer 2002).⁹ Fourth, I present results based on a matching difference-in-differences estimator where I match the two groups on pre-treatment characteristics (Sant’Anna and Zhao 2020).¹⁰ Fifth, I include a rich set (see note in Table 1) of time-varying control variables to rule out that changes in turnout are due to alternative life events that are potentially correlated with attending college (Burden et al. 2017; Highton and Wolfinger 2001; Sønderskov et al. 2022). Due to possible post-treatment bias, I present the results both with and without post-treatment covariates (Elwert and Winship 2014).

Taken together, these analyses help address plausible concerns about parallel trends. I find similar results across these strategies with different assumptions, which supports a causal interpretation of the estimates.

Results

I first estimate the average effect of college attendance on turnout among college-goers. Table 1 shows these difference-in-differences estimates.¹¹ Here, I compare college-goers to three different comparison groups: all untreated respondents (models 1–3), future college-goers (models 4–6, “restricted”), and the matched untreated group (models 7–8). Across the different models, I find similar results. Compared to models 1–3, the reduction in effect size in models 4–8 indicates that these restrictive models – especially the restricted untreated group in models 4–6 – do improve internal validity and, therefore, should be our preferred models. In these, I estimate that attending college leads to an increase in voter turnout of between 8.9 and 10.6 percentage points ($p \leq 0.004$).¹² A post-hoc power analysis of the models yields minimum detectable effect sizes between 3.7 and 7.7 percentage points (70% power, 95% confidence level) – a remarkable improvement to earlier studies (cf. Appendix C3). Replicating the analyses in an independent sample of 1992 high school graduates yields a similarly sized positive average effect of college, reported in Appendix B.

We can now compare these findings with the effect estimates of previous studies by looking at Figure 1. Notably, the main study’s estimate is within the confidence intervals of four of the five previous studies as well as the meta-analytical average across studies. This is exactly what we

be positive between 4.9 and 9.7 percentage points and only the most conservative and statistically uncertain model would become statistically insignificant. This constitutes a hard test because not all models are sensitive to the same kinds of selection bias (see Appendix A2).

⁹Additionally, there is a methodological reason to allow trends in turnout to vary with pre-college voting, which relates to the ceiling and floor effects that are inherent to binary outcomes. I elaborate and formally prove this proposition in Appendix D.

¹⁰Matching was performed using genetic matching with replacement based on parental education and income, gender, race, and previous turnout (Appendix I3).

¹¹Entries are OLS-estimates of the coefficient δ in equation (1). See Appendix K on estimation.

¹²Additionally, in Appendix L, I estimate a positive effect on local election turnout.

Table 1. Effect of College Education on Voter Turnout

	Full Sample			Restricted Untr. Grp.			Matched	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attended College × Post Period	0.137*** (0.014)	0.130*** (0.016)	0.107*** (0.016)	0.096*** (0.028)	0.106*** (0.031)	0.089** (0.031)	0.116*** (0.026)	0.101*** (0.027)
Time FEs & Individual FEs	✓	✓	✓	✓	✓	✓	✓	✓
Pre-college Voting × Time FEs	✓	✓	✓	✓	✓	✓	✓	✓
Time-varying Controls		✓	✓		✓	✓		✓
Additional Pre-college Covariates × Time FEs			✓			✓		
<i>Units</i>	10,426	9,652	9,554	9,360	8,717	8,644	9,289	8,582
<i>Observations</i>	19,813	18,298	18,118	17,842	16,593	16,454	18,578	17,068

Note: Difference-in-differences estimates of how college-goers changed their turnout between the 2004 and 2008 elections compared to non-attendees. In models 4–6, the untreated group is restricted to future college-goers. Models 7–8 use a matched comparison group. Time-varying controls include residential mobility, living with parents, getting married, crime victimization, becoming seriously ill or disabled, job loss, having children, parental divorce, parental job loss, serious illness in the family, a parent's death, and the death of a relative or friend. All models include time trends that are specific to whether a respondent voted in the pre-college election or not. Models 3 and 6 further interact time fixed effects with pre-college cognitive skills, gender, race, and parental education and income. OLS estimates with robust standard errors clustered by individual in parentheses. Appendix A3 reports all estimates.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

would expect if college did have an effect, but this had been concealed by statistical uncertainty in previous studies.

My data also speaks to another theoretical reason for the mixed results in previous studies. The compensation hypothesis states that the effect of education is larger among less privileged students as it compensates for lack of political socialization at home (Neundorf, Niemi, and Smets 2016; Lindgren et al. 2019; although see Mendelberg et al. 2021). Given the mass expansion of college education (Trow 2007; Stevens, Armstrong and Arum 2008), this suggests that the effect of college on college-goers may have increased over time.¹³ I test this argument by formally testing whether the college effect is larger among students without previous experience with political participation (see Appendix A1). I find that the effect is larger by around 7 percentage points ($p \leq 0.025$) among previous non-voters, suggesting that a college effect is more likely among those who are initially less likely to vote. I also find that the effect is larger among disadvantaged groups defined by race, whereas results are mixed for socio-economic status (see Appendix A1). I do not find support for the compensation hypothesis in the 1992 replication sample (Appendix B discusses these findings). However, across samples, I find a substantive positive effect in both the voter and non-voter subgroups. This suggests that we may expect college to have civic returns in both high-turnout and low-turnout populations.

Conclusion and Discussion

Does going to college increase political participation? I have provided unique panel evidence on this question and synthesized it with existing studies. I find that attending college does indeed lead to increased voter turnout. The findings suggest that statistical uncertainty in previous studies is likely to have concealed substantive civic returns to college education and that we should update our view of the education-participation relationship accordingly.

Therefore, this article helps explain and reconcile the debate about whether college education causes voter turnout or not. Future research may fruitfully seek new ways of testing this relationship with both statistical power and causal credibility. Specifically, using administrative data on voter turnout would alleviate an important limitation of the current study, which is the reliance on self-reported turnout.

My findings also contribute to our understanding of the political socialization of young adults. In line with studies of earlier stages of education (Lindgren et al. 2019), I find initial evidence that college helps less politically experienced students catch up, in terms of voting, to more privileged students. The current study testifies to a rather short-term effect of attending college. We still need future studies to ascertain whether the causal effect of college diminishes with age and whether the context of low-turnout US presidential elections limits the generalizability of the findings in this letter.

Finally, the results support the general idea that educational institutions – even higher education – sustain democratic institutions through creating and maintaining an engaged citizenry (Lipset 1959; Lindgren et al. 2019). While the current study finds individual-level turnout effects, the aggregate consequences of college expansion depend on the degree to which these effects are relative or absolute (Nie, Junn, and Stehlik-Barry 1996; Helliwell and Putnam 2007). Deciding whether college primarily redistributes turnout or boosts overall turnout – and disentangling the causal mechanisms – are challenges left unaddressed in this article. In both cases, the findings imply that expanding college uptake to those who are less likely to vote has the potential to reduce inequalities in political participation.

Supplementary material. The supplementary material for this article can be found at: <https://doi.org/10.1017/S0007123424000486>

¹³Appendix J elaborates this argument.

Data availability statement. Replication data for this article can be found in Harvard Dataverse at: <https://doi.org/10.7910/DVN/PZCPOB> (Jensen 2025).

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Competing interests. None.

Ethical standards. This work complies with APSA's Principles and Guidance on Human Subject Research. Appendix G contains further ethical considerations.

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