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Educational Attainment and Social Norms of Voting

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Abstract

Why does the likelihood of voting increase with education in the U.S.? Prominent theories attribute education's effect to human capital, which affords individuals resources needed to participate, but neglect social motivations. We test a theory of internalized social norms as another contributing factor, providing evidence in three studies. First, we show that highly educated people are more likely to view voting as a civic duty, and that civic duty partially mediates the effect of education on voting. Second, we show education is associated with a higher likelihood of overreporting voting in the 2016 election. Third, we show that educated respondents are more likely to withstand stimuli incentivizing them to report they will not vote in an upcoming election. The results imply that voting norms vary by education, and invite more attention to social explanations for socioeconomic disparities in turnout.

Keywords: Voting, Education, Civic Duty, Norms

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Why are highly educated Americans more likely to vote? The positive association between educational attainment and voting is well established (Campbell et al. 1960; Leighley and Nagler 2014; Verba et al. 1995; Wolfinger and Rosenstone 1980), and more recent work has provided some evidence of a direct causal effect of education on voting (e.g. Milligan et al. 2004; Sondheimer and Green 2010). Nonetheless, the mechanisms underlying the relationship are not fully understood. Prominent explanations attribute the effect of education to the accrual of certain types of human capital, holding that education allows individuals to obtain the political information and develop the skills needed to vote (e.g. Condon 2015; Delli Carpini and Keeter 1996; Hillygus 2005; Rosenstone and Hansen 1993; Verba et al. 1995). Yet, empirical evidence on voter turnout sometimes contradicts predictions generated from these theories. Based on elements of human capital theories, for example, we would expect that policy interventions intended to make voting easier would close the turnout gap, since the information and skills accrued through education would be less necessary for participation (e.g. Gallego 2010; Highton 1997; Wolfinger and Rosenstone 1980). While voting has generally become more accessible in the U.S. since the 1960s through a number of federal and state election reforms, middling turnout rates and unequal participation along socioeconomic lines have persisted (Hanmer 2009; Springer 2014). In fact, the turnout gap by education (and income, for that matter) in presidential elections has not noticeably decreased since the 1970s (Leighley and Nagler 2014). Human capital theories are integral to understanding the link between education and individual voting behavior, but they do not tell the entire story.

We turn our attention instead to the social norms surrounding voting. Building on previous articulations of the theory (Wolfinger and Rosenstone 1980), we argue that a norm of voting is more prevalent among the more educated, thereby providing them greater social benefits for adherence. There are a number of reasons to suspect why this might be the case. Such norms are repeatedly taught and reinforced over the course of an education. Furthermore, because educational attainment affects not only one's training but also one's

social environment, norms surrounding voting remain continually reinforced within social networks long after individuals leave school. Factors accompanying self-selection into higher educational attainment, like childhood socioeconomic status or certain personality traits, might also help explain differences in norms.

Though we do not adjudicate which of these mechanisms best explains differences in norms across levels of educational attainment, we provide new evidence in three studies that differences in norm internalization partially account for the relationship between educational attainment and voting. First, we analyze data from the 2016 American National Election Study.¹ We find that educational attainment is positively associated with seeing voting as a civic duty. Causal mediation analysis provides evidence that viewing voting normatively as a civic duty mediates the relationship between educational attainment and validated voting behavior. Second, we compare overreporting behavior among individuals with varying levels of education. If more educated individuals feel more internalized pressure to vote, we should find that more educated individuals are more likely to report having voted when, in reality, they did not. Analysis of two national surveys conducted after the 2016 general election confirms that the likelihood of overreporting increases with education. Third, we analyze data from an original survey experiment conducted via Mechanical Turk. The experiment subjects respondents to different hypothetical voting scenarios to determine how their behavior varies by education. We find that educated respondents are more likely to withstand stimuli incentivizing them to report they will not vote in an upcoming election.

The results across three studies provide evidence that voting disparities by educational attainment are due, in part, to differences in internalized norms of voting. Because the effect sizes we uncover are generally small, we do not claim that disparities in social norms are the only or even the primary force driving the turnout gap (see Jackson 1995). However, our findings bolster the idea that differences in norms can help explain long-standing educational disparities in voting behavior. Because educational attainment in the U.S. is a strong marker

¹Replication data and code is available at <https://doi.org/10.7910/DVN/LJN4B0>.

of social class, our findings have notable implications for research on socioeconomic inequality in political participation.

Social Norms and Voting Behavior

Americans commonly explain their motivation to vote by citing their sense of civic duty, an idea with deep roots in American political culture (e.g., de Tocqueville [1835] 1988). Classic economic models of voting include civic duty in their equations (e.g. Riker and Ordeshook 1968). Empirical work provides additional evidence that feelings of civic duty truly do motivate turnout, both in the U.S. (Blais and Achen 2019; Campbell et al. 1960) and in other advanced democracies (Blais 2000; Galais and Blais 2016).

Civic duty is one way to refer to a norm of voting—a sense that one *should* vote or that it is *good* to vote. Social norms guide behavior by conveying group standards about expected practices (McDonald and Crandall 2015). People engage in activities not simply because it addresses an immediate need, but because they are motivated to comply with behaviors that others would approve of—what psychologists call referent groups and individuals. The more people need referents’ approval, the more likely they are to engage in the behaviors that referents prefer (Montano and Kasprzyk 2015).

In explaining the influence of social norms, social psychologists make a distinction between descriptive and injunctive norms (Cialdini and Goldstein 2004). Descriptive norms represent what types of behavior are typical. Injunctive norms prescribe certain types of behavior—they tell people what they ought to do. Each type fulfills a different social function (Cialdini et al. 1990; Kallgren et al. 2000). Descriptive norms help individuals make optimal choices as people observe and imitate their peers’ behavior, void of any explicit social pressure. Complying with descriptive norms is a relatively automatic, low-effort process that guides an individual to a socially safe practice (Morris et al. 2015). Injunctive norms, by contrast, motivate people to engage in behaviors they might not otherwise engage in through

social pressure and the threat of social sanctions. Complying with injunctive norms is more effortful, deliberate, and strategic. It signals that an individual belongs to whatever group is granting social approval.

Relative to descriptive norms, injunctive voting norms more effectively motivate voting behavior. Consistent voting is still an irregular practice in the United States (taking numerous annual federal, state, and local elections into account), which means conveying the descriptive norm could *depress* voting. People who engage in normative behaviors at disproportionate rates have been found to bring their behavior in line with prevailing practices when exposed to a descriptive norm (Schultz et al. 2007). More generally, psychologists have found evidence of a boomerang effect, whereby learning that a behavior is both common and bad—like failing to show up on Election Day—makes people more likely to engage in it (Miller and Prentice 2016). Injunctive norms, which promise social approval through engaging in effortful practices, present no such risk.

The empirical literature in political science bears out the prediction that injunctive norms are more effective at motivating voting behavior. Numerous get-out-the-vote (GOTV) field experiments have found that reminding citizens of their civic duty, promising to report their voting behavior to neighbors, or inducing feelings of pride or shame increase the likelihood they will vote (Gerber et al. 2008, 2010; Panagopoulos 2013). By contrast, exposure to descriptive norms (for example, telling a person that lots of people are voting) seems to make subjects in similar experiments more likely to state their intention to vote (Gerber and Rogers 2009), but not to cast a ballot in reality (Panagopoulos et al. 2014).

Injunctive norms shape behavior through the threat of social sanction and the reward of social approval. Psychologists have documented that norms can also influence behavior by becoming internalized, which refers to social standards becoming personal standards (Thøgersen 2006). When a norm is internalized, social surveillance is not necessary for norms to be enforced. Instead, people comply with internalized norms in order to avoid feelings of guilt or to express their values (Morris et al. 2015).

Given uneven turnout rates, it is clear that not all citizens adhere to voting norms in the U.S. One explanation is that individuals vary in how much they internalize norms and behave in line with them. Another explanation is that different norms prevail in different subsets of the population. Relatively few studies have examined whether and how voting norms vary across subgroups in the population. The research that does so tends to focus on geographic or contextual variation—how the composition and characteristics of communities shape norms and ultimately voting behavior (Campbell 2006; Doherty et al. 2017). But norms also vary within subgroups of the population (Anoll 2018), and those subgroup boundaries may not match geographic or community boundaries. In this paper, we explore groups defined by educational attainment as one such subgroup that shapes political behavior.

Educational Attainment and Voting Norms

There are a number of reasons that individuals attaining higher levels of education should internalize norms of voting more than individuals with fewer years of formal education. One set of reasons involves activities that occur in educational institutions, while another set involves characteristics of the highly educated that make them both more likely to earn higher degrees and more likely to internalize voting norms.

Educational institutions work deliberately to instill voting norms. Schools encourage political participation and foster civic-mindedness. This begins in primary and secondary education, both informally through positive example from parents or teachers and formally through curricular instruction (Campbell 2006; Nie et al. 1996). Some local educational institutions can be more effective in instilling participatory attitudes in students than others (Litt 1963), and the attitudes that individuals form early in life can follow them in the long term (Campbell 2006).

Disparities in internalized norms of voting, though, should emerge more notably along the lines of educational attainment as students continue or stop their education in late

adolescence. A clear cutoff point emerges as students sort themselves into attending college after high school or not. College students are exposed to norms of voting in ways that young people outside of colleges do not experience. College curricula, especially in the social sciences, encourage participation (Hillygus 2005). Further, college students encounter voter registration drives and GOTV campaigns aimed at increasing youth turnout, often implemented by their peers, professors, and administrators. Any effects that the college environment has on participation are likely not uniform across students, since participation among college students varies by peer networks (Klofstad 2011) and campus civic cultures (Thomas and Brower 2017). Nonetheless, the typical social and civic environments at U.S. postsecondary institutions are more conducive than non-college environments for instilling voting norms. Crucially, social enforcement of voting norms in college occurs just at the time when students are old enough to begin voting legally.

Yet, a number of studies have posed a serious challenge to the idea that schooling itself is responsible for greater participation among individuals with more education (Berinsky and Lenz 2011; Kam and Palmer 2008; Tenn 2007). It could be the case that the types of individuals who select into higher educational attainment are also the types who had internalized norms of voting before stepping foot on a college campus.

Perhaps the most prominent difference between college-educated Americans and their non-college counterparts is the socioeconomic background of their families (Kam and Palmer 2008). Parental education, for instance, carries notable consequences for individual political behavior later in life. Status transmission theories hold that children whose parents hold higher levels of economic resources and education are more likely to become politically active, likely because high-SES parents are equipped to socialize their children into the political sphere by modeling participatory behaviors (Gidengil et al. 2016; Verba et al. 2003, 2005). Consequently, any difference in norms along the lines of educational attainment could be attributed to childhood socialization rather than civic education.

Individual characteristics, like personality traits or skills, may also account for an asso-

ciation among education, civic duty, and voting. For example, a study of British youths shows that cognitive ability, presumably a factor that should be associated with higher educational attainment, independently predicts voting behavior before taking education into account (Denny and Doyle 2008). Pertinently for the present study, personality traits such as conscientiousness or agreeableness could predict both adherence to norms of civic duty and compliance with cultural expectations of completing a higher education.

Finally, social networks defined by level of education can play a role in maintaining norms, regardless of the independent effects of educational environment, childhood SES, or personality on instilling norms in the first place. As noted above, norms are enforced and internalized when a person is motivated to comply with behaviors approved of by referent groups and individuals—those granting desired social approval, especially those a person feels close to. However, the norms that people learn early in life (within schools or elsewhere) can continue to be enforced by individuals in their families and social networks (Campbell 2006). Americans’ social networks are notably segregated along the lines of education (McPherson et al. 2001), and some evidence suggests that social distance between people of differing education levels has increased in recent decades (Mare 1991; Smith et al. 2014). If individuals are less likely to have close relationships with people of differing education levels, peer-to-peer enforcement of voting norms is more likely to remain within segments of the population where voting is already viewed as expected behavior.

To summarize, individuals who have attained more education should be more likely to view voting as a civic duty. Whether caused by schooling, family background, personality traits, or adult social networks (or a combination of these factors), internalization of voting norms is likely to be more pronounced among people with higher levels of educational attainment. Civic participation, especially voting, is an expected behavior among the highly educated in the U.S., and consistent reminders of this expectation should eventually become self-enforcing. Voting regularly simply becomes taken for granted, even in the absence of active social pressure to do so (Morris et al. 2015). In turn, the highly educated should feel

motivated to vote regularly in elections, without needing to be reminded of the injunctive norm in the lead-up to Election Day.

Ours is not the first or only explanation of how differing norms along lines of educational attainment drive voting behavior. Our argument bears a good deal of resemblance to the point briefly made by Wolfinger and Rosenstone (1980) that, “American schools provide a good deal of explicit instruction and exhortation on citizenship that emphasizes the obligation to vote and thus might be thought to nurture a sense of civic duty” (18). Campbell (2006) also provides evidence that schools inculcate civic norms in students. But whereas Campbell’s work focuses on how the cross-sectional variation in the civic culture of subjects’ high schools drives their voting behavior later in life, the present work focuses on differences in levels of educational attainment contributing to one’s sense of civic duty and, subsequently, voting behavior. Finally, the sorting theory (Nie et al. 1996) holds that greater educational attainment gives individuals relatively higher social status and social network positioning that is more proximate to political power, thereby increasing the social benefits of voting. Our theory also considers social signaling as a motivational factor in voting, but sees voting as an act of adherence to localized norms rather than a zero-sum competition for status.

The present work also differs from recent research that has further confirmed the role of social pressure in spurring turnout (Doherty et al. 2017; Gerber et al. 2008, 2010, 2016; Panagopoulos et al. 2014). Social pressure is the mechanism through which norms are enforced. However, these studies tend to rely upon experimental methods that show the causal effect of applying social pressure to respondents. By contrast, our argument is that social pressure (as embodied in internalized norms) is already present in the decision to vote, sometimes unrecognized by individuals and affecting behavior without an explicit prompt from others (see also McDonald and Crandall 2015). For individuals who have internalized norms of voting, exposure to additional social pressure should have no effect; they will vote regardless.

In what follows, we test the norm internalization hypothesis in three studies. We first test

two expectations that directly derive from it: that educational attainment is associated with viewing voting as a civic duty, and that this view partially mediates the effect of education on voting. We then test two additional implications of our theory. If social norms drive differences in voting behavior, we should find that educated individuals face pressure to report having voted even when they did not. And if civic duty norms make the educated particularly motivated, they should be more likely to withstand discouragements to vote.

Study 1: Mediation Analysis

We first test our expectation that more educated citizens are more likely to understand voting as a civic duty. Then we test whether the association between education and voting norms helps account for education-based voting disparities. The American National Election Studies' 2016 Time Series Study is well-suited to test these expectations, since it both asked respondents whether they thought voting was a civic duty and validated their voting behavior in the 2016 general election.

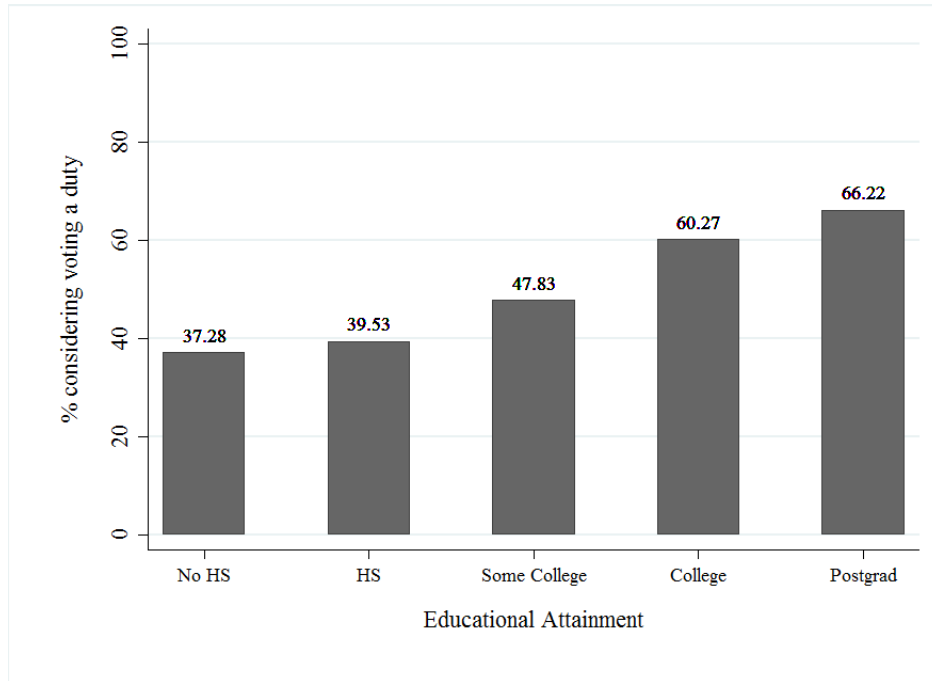
To assess respondents' endorsement of the voting norm, we rely on the following question:

Different people feel differently about voting. For some, voting is a duty—they feel they should vote in every election no matter how they feel about the candidates and parties. For others voting is a choice—they feel free to vote or not to vote, depending on how they feel about the candidates and parties. For you personally, is voting mainly a duty, mainly a choice, or neither a duty nor a choice?

Asking whether voting is a duty provides a good measure of whether individuals subscribe to the normative view of voting. Moreover, asking the question in a forced choice format reduces the likelihood of respondents simply agreeing that civic responsibility is important.² In the ANES sample, 51.2% of respondents felt that voting is mainly a duty, 37.9% of

²Compare, for instance, to a 2018 Pew Research Center finding that 91% of respondents very much agreed or somewhat agreed with the statement that voting is important to being a good citizen. Pew Research Center. "The Public, the Political System and American Democracy." Accessed online 1/29/19 at <http://www.people-press.org/2018/04/26/the-public-the-political-system-and-american-democracy/>.

Figure 1: Education and Civic Duty



Source: 2016 American National Election Study.

respondents felt that voting is mainly a choice, and the remaining 10.8% felt that voting is neither a duty nor a choice.

To assess civic duty's bivariate relationship with educational attainment, we created an ordinal variable *Education* such that a value of 0 indicates the respondent did not graduate high school, 1 indicates the respondent graduated high school only, 2 indicates the respondent completed some college credit, 3 indicates the respondent completed a 4-year bachelor's degree, and 4 indicates the respondent completed a graduate or professional degree. If education instills and reinforces a personal norm of voting, we should find that higher levels of education predict greater agreement that voting is mainly a duty.

Figure 1 displays the prevalence of believing that voting is a civic duty across levels of education. The differences in a sense of duty across groups are stark. While a little more than a third of respondents without a high school diploma reported feeling voting is a duty, almost two thirds of respondents with postgraduate degrees felt the same.

Of course, the personal characteristics of individuals with and without advanced degrees differ quite a bit. We estimated two multiple regression models to control for potential confounding factors. We relied on the same civic duty item described above as the dependent variable for both. When fully branched, responses to this question become a 7-point civic duty scale, where higher values indicate greater endorsement of the voting norm. The first model uses the ordinal measure of educational attainment described above; the second uses the binary variable *College*, with values of one indicating the respondent holds at least a four-year bachelor’s degree. In both models we control for age, sex, race, ethnicity, nation of origin, religious attendance, strength of partisanship, and political interest.³ Table A1 of the online appendix reports the full results of both regression models. In line with expectations, the coefficient estimate for the education variable is positive and statistically significant in both models after controlling for these factors.⁴

We also tested whether our civic duty item, which pits civic duty against the idea of voting as a choice, might evoke a value of autonomy. Respondents who value both autonomy and civic duty might, as a result, underreport a sense of civic duty, an outcome that would be problematic for our tests if autonomy were negatively associated with educational attainment. Under this scenario, the negative relationship between autonomy and educational attainment would be driving the relationship seen in Figure 1, and the relationship between

³*Age* is a respondent’s self-reported age in years, controlling for shifting norms of political involvement over time in the U.S. The squared term for age is also included in the model. *Female* is a binary variable indicating a respondent’s sex. *White*, *Black*, and *Hispanic* are binary variables indicating the race/ethnicity by which a respondent self-identifies (with all others serving as the reference group). *Foreign Born* is a binary variable with a value of 1 indicating the respondent was born outside the U.S., controlling for differences in political norms across cultures. *Religious Attendance* is an ordinal variable measuring the frequency of a respondent’s participation in religious services, controlling for norms of social obligation instilled through religious institutions rather than schools. *PID Strength* is a folded 7-point party identification scale. A value of 0 indicates true independents, 1 indicates party-leaning independents, 2 indicates weak partisans, and 3 indicates strong partisans. *Interest* measures the respondent’s self-reported interest in politics, ranging from a value of 0 (not at all interested) to 3 (very interested).

⁴A remaining concern is that citizens who attend college differ fundamentally from those who do not in ways that are not captured by the controls in the previous footnote. If these underlying characteristics also increase the likelihood of seeing voting as a civic duty, they represent another potential confound. In Table A1 of the online appendix, we replicate the models with additional controls for five personality traits that might drive both college attendance and the civic duty norm: dependability, extraversion, openness to new experiences, carelessness (reverse-coded), and being conventional. The effects for education in both models are unaffected with these additional controls.

civic duty and educational attainment would be spurious. We found an alternative measure of *Autonomy* in the item, “Our country needs free thinkers who will have the courage to defy traditional ways, even if this upsets many people.” Interestingly, higher values of *Autonomy* were weakly but positively correlated with higher education levels ($r = 0.12$), meaning that, if anything, the civic duty norm endorsement among the highly educated was underreported. However, regression models presented in Table A2 in the appendix do not provide any evidence that valuing autonomy is related to our civic duty measure or otherwise impacts the results we observe in Figure 1 or Table A1.

We next test whether civic duty mediates the effect of education on voting behavior. The dependent variable we analyze is a binary measure of respondents’ voting behavior, which is coded as 1 if the respondent voted in the 2016 presidential election and 0 if she did not. We use respondents’ validated voting behavior, rather than relying on self-reports. We continue to use the measures of education and the civic duty norm described above.

Traditional mediation analyses using observational data seek to measure the indirect effects of some variable X on a dependent variable Y through a mediating variable M (e.g. Baron and Kenny 1986). However, traditional mediation analysis of observational data using OLS estimators has been criticized for producing biased estimates (Bullock and Ha 2011; Bullock et al. 2010). In short, OLS produces unbiased estimates of indirect effects only if no other mediating variable Z affects both M and Y (i.e. there are not multiple mediators)—an assumption that social science researchers can rarely justify. In the context of our research, it requires an assumption that no other variable simultaneously affects both civic duty and turnout.

In an ideal research design, indirect effects could be estimated if both the treatment variable X and the mediating variable M were randomly assigned. Of course in the real world, neither educational attainment nor feelings of civic duty meet this criterion. We take a step beyond traditional mediation analysis while working within the constraints of the available observational data by employing a matching approach in concert with causal mediation

Table 1: Average Causal Mediation Effects

	Variable of Interest	Placebo
Civic Duty (ACME)	0.023 [0.015, 0.030]	
Science Spending (ACME)		0.005 [0.000, 0.010]
College (ADE)	0.146 [0.115, 0.180]	0.165 [0.130, 0.200]

Note: Estimated effect size for each variable, with 95% confidence intervals in brackets below. The estimates decompose the total effect of college education on voting into the indirect effect through the mediating variable (ACME) and the direct effect of college education on voting (ADE).

analysis (Imai et al. 2011). This technique allows researchers to estimate an average causal mediation effect (ACME) when the treatment variable X is randomized but the mediating variable M is not. Instead, post-estimation sensitivity analysis allows the researcher to estimate the threat to inference from unobserved variables confounding the mediating effect. We use matching to preprocess the data so that our treatment X approximates as-if random assignment (Ho et al. 2007).⁵

For this analysis, we use *College*, a binary measure of educational attainment with values of one indicating the respondent holds at least a four-year bachelor’s degree. We rely on this cutoff because graduating from college is a significant educational achievement that meaningfully distinguishes graduates and non-graduates in a range of outcomes, including, we argue, adherence to voting norms. Educational attainment is not randomly assigned, and so we match on a variety of observed pre-treatment covariates using the genetic matching technique developed by Sekhon and Diamond (2013). The list of covariates and an analysis of balance is presented in Figure A1 of the online appendix.

Next, we estimate the average causal mediation effect (ACME) of a college degree on voting through civic duty. The effects are estimated using linear regression for the mediator

⁵We note that we match only on observables, leaving open the possibility of confounding through unobserved variables.

model and probit for the outcome model. The results are presented in Table 1.

The results indicate a small but significant indirect effect of a college degree on validated voting behavior through civic duty. The estimated ACME for the mediator civic duty is roughly 0.023, while the estimated average direct effect (ADE) of a college degree is estimated to be 0.146. The results indicate roughly 14% of the total effect of a college degree on voting is mediated by a sense of civic duty.⁶

We note that this finding rests on the sequential ignorability assumption, which holds that no unobserved confounding variables affect both the mediator and the outcome. Sensitivity analysis allows us to quantify what proportion of the variance (R^2) in the mediating and outcome variables would need to be explained by a confounding covariate for the sign of the ACME estimate to change from positive to negative. The analysis indicates that the true ACME changes signs if the product of these two R^2 values is greater than 0.0357. That is, the positive estimate is robust if unobserved confounders explain less than about 19% of the variance in both the mediator and outcome models ($\sqrt{0.0357} = 0.1889$). Plots of the sensitivity analysis for both the sensitivity parameter ρ and the model R^2 values are located in Figure A2 of the online appendix.

Because of the high sensitivity of our results to the assumptions of the specification, we compare the mediating effect of civic duty to a placebo variable. If the placebo variable indicates a comparable mediating effect to civic duty, then we should be more suspicious of the results obtained for civic duty. For comparison, the placebo should be a variable affected by a person's education level but that should not affect a person's likelihood of voting.

For this purpose, we chose a survey item measuring respondents' preferences for federal government spending on scientific research on a three-point scale (*Science Spending*). More educated people tend to be more supportive of government spending on science and technology, perhaps due to greater literacy and interest in science, greater trust in governing institutions to spend money wisely, or even self-interest in benefiting at the personal or

⁶The 14% is calculated from dividing the ACME by the total effect, which is the sum of the ACME and the ADE.

community level from government largesse (Miller 1983; Sanz-Menendez et al. 2013). However, we have little reason to suspect that attitudes toward science spending motivate voting behavior. Federal science spending is neither known to mobilize a large and passionate constituency to go to the polls (even among the well-educated), nor do campaigns often attempt to mobilize supporters on the issue. (Neither Donald Trump nor Hillary Clinton made science spending a key issue in 2016 as respondents replied to ANES researchers.)

The estimated ACME for the placebo is listed in the second column of Table 1. In line with expectations, the estimate is smaller (0.005), indicating less than 3% of the effect of education on voting is mediated by support for science spending. The confidence interval for the ACME strictly speaking does not cross zero. However, sensitivity analysis (located in Figure A3 of the online appendix) indicates that the true ACME changes signs if the product of the R^2 values for confounding variables in the mediator and outcome models is greater than 0—indicating that, for all intents and purposes, the mediation effect for this variable is null.

Overall, the results indicate an indirect effect of educational attainment on voting through civic duty. We urge caution in the interpretation of this finding; the results above do not constitute unimpeachable evidence of a causal mediation effect. Unobserved confounders in the matching process may threaten inference. Readers may also think of variables shaped by educational attainment that affect both a sense of civic duty and voting behavior, in violation of the assumption of sequential ignorability. An example of a potential confounder would be interest in politics, if a college education increased individual interest that led to norm internalization.⁷ Furthermore, this mediation analysis cannot allow us to conclude that norms arise from a college education rather than the set of life circumstances associated with the completion of a college education. However, we do think the method employed here represents an improvement over traditional mediation analysis, particularly given the constraints of the available observational data. Resting on the assumptions of the model,

⁷However, Prior (2010) suggests that interest forms in childhood and adolescence and demonstrates that it is more or less stable over one’s adulthood.

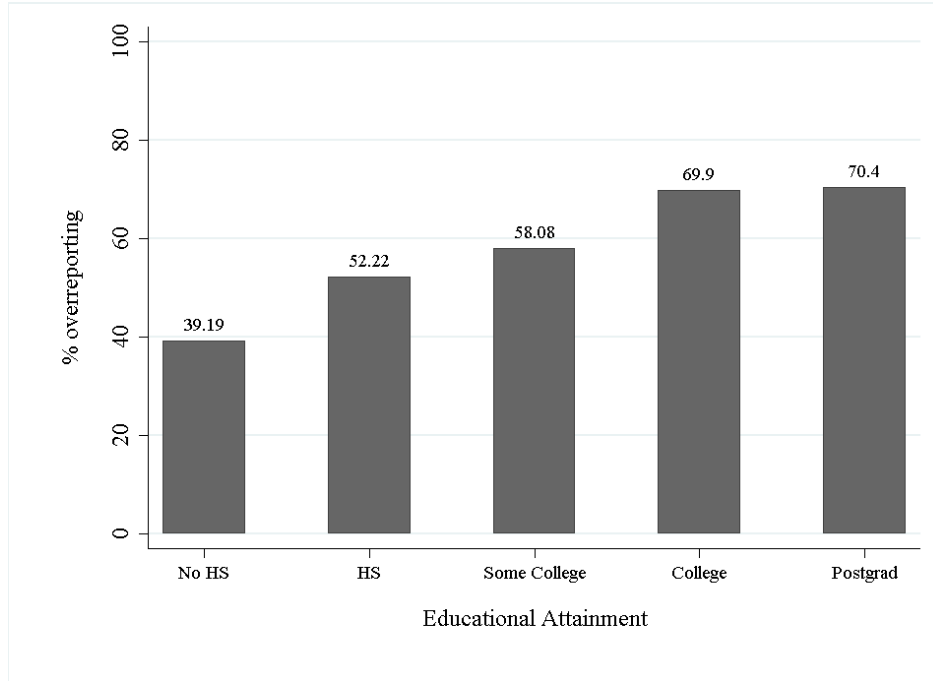
the results support our prediction that a sense of civic duty mediates the effect of education on voting.

Study 2: Overreporting

Study 1 provides evidence that education makes citizens more likely to perceive voting as a civic duty, which, in turn, prompts them to vote. Next we turn to a second implication of our theory. If, as we claim, recognizing the normative importance of voting drives the more educated to vote, we should find that they reported having voted even when they did not—also referred to as “overreporting.” Because their behavior is shaped by sustaining prevailing norms, the more educated should be more reluctant to admit when their actions deviate from social expectations, even when revealing their true behavior has no obvious negative repercussions in a survey environment. Compared to overreporting of news exposure, which can be attributed to imperfect memory and flawed methods of estimation among respondents (Prior 2009), overreporting of voting is better attributed to concerns of social desirability, particularly among groups in the population that feel greater pressure to vote (Belli et al. 2001; Bernstein et al. 2001). As such, the educated should be more likely to overreport voting.

Previous studies have demonstrated the relationship between educational attainment and overreporting using data from ANES waves dating to 1964 and 1990 (Belli et al. 2001; Bernstein et al. 2001) and the 2008 Cooperative Congressional Election Study (Ansolabehere and Hersh 2012). We replicate these earlier efforts with more contemporary data. We turn to the 2016 ANES as our primary data source for this study, since it includes respondents’ self-reports about their voting behavior as well as validation of whether they actually did so. We can use the discrepancy between the two to test whether education predicts erroneous claims that a respondent voted. According to Silver et al. (1986), researchers should measure overreporting by observing self-reports only among validated nonvoters. Measuring

Figure 2: Overreporting among Validated Nonvoters by Level of Education



Source: 2016 American National Election Study.

overreporting by observing the veracity of self-reports among all respondents or among all respondents who claimed they voted includes populations not at risk for overreporting. Any estimates of variables that contribute to the individual propensity to overreport using these two samples will be sensitive to the marginal distribution of true voters and true nonvoters. In subsequent analyses, we only include ANES respondents who were validated not to have voted in the 2016 general election.⁸

We first show the distribution of overreporters across levels of education in Figure 2. The majority of all validated nonvoters in this sample (59.46%) claimed to have voted, but overreporting varies systematically by education. While roughly 70% of both four-year college degree holders and postgraduate degree holders overreported, the prevalence of overreporting declines as educational attainment decreases. A bare majority of respondents holding only a high school diploma overreported (52.22%), while a minority of respondents

⁸Specifically, we use votes that were validated through clerical review.

without a high school diploma overreported (39.19%).

Next, we test the association of education with overreporting through a logistic regression model that features the same set of demographic and political controls as the models in Study 1. The results are reported in Table 2. Even with the inclusion of controls, education remains positively and significantly associated with overreporting. Substantively, a one-unit increase in education is associated with a five percentage point increase in the probability of overreporting, holding other variables in the model at their means and medians.⁹ Among the controls, only strength of partisan identity and interest in politics have a significant, positive relationship with overreporting.

As an additional test of the hypothesis, we replicate the ANES results using data from the 2016 CCES, which also validates respondents' voting behavior. Findings are presented in Figure A4 and Table A3 of the online appendix. The results also show a positive and significant association between education and overreporting among validated nonvoters. Similarly to the ANES results, strength of partisan identity and interest in politics are also the only control variables to be positively associated with overreporting in both CCES models.

We take these results to mean that as educational attainment increases, individuals are more likely to believe that they *should* report having voted, even if they did not. Even with no explicit social pressure placed on them, highly educated respondents usually reported having voted, regardless of whether they voted in reality. These results match earlier findings (Ansolabehere and Hersh 2012; Belli et al. 2001; Bernstein et al. 2001), and they contribute evidence that the relationship has persisted over time, particularly as mass levels of educational attainment have continued to increase.¹⁰ This evidence is consistent with the idea that internalization of voting norms is more prevalent among highly educated citizens.

⁹The results of logistic regression models using a dummy variable for college education, also reported in Table 2, point to a similar conclusion.

¹⁰The association between normative feelings about voting and educational attainment may help to explain trouble that pollsters have reaching low-education voters. See, for instance, Pew Research Center, "Why 2016 Election Polls Missed Their Mark." Accessed online 6/21/19 at <https://www.pewresearch.org/fact-tank/2016/11/09/why-2016-election-polls-missed-their-mark/>.

Table 2: Overreporting among Validated Nonvoters

	(1)	(2)
Education	0.20* (0.08)	
College		0.45* (0.19)
Age	0.01 (0.03)	0.01 (0.03)
Age ²	0.00 (0.00)	0.00 (0.00)
Female	-0.14 (0.17)	-0.12 (0.17)
White	0.13 (0.33)	0.12 (0.33)
Black	0.52 (0.41)	0.50 (0.41)
Hispanic	-0.26 (0.38)	-0.30 (0.38)
Foreign Born	-0.27 (0.32)	-0.27 (0.31)
Religious Attendance	0.09 (0.06)	0.09 (0.06)
PID Strength	0.29* (0.08)	0.30* (0.08)
Interest	0.45* (0.10)	0.46* (0.10)
Constant	-1.92* (0.68)	-1.70* (0.68)
<i>N</i>	961	961
BIC	1317.99	1320.51

Note: * $p < 0.05$. Data from the 2016 ANES. Survey-weighted standard errors in parentheses. Significance tests are two-tailed.

Study 3: Survey Experiment

So far, we have relied on observational data to demonstrate the connection between education and voting through social norms. Next, we draw on an original survey experiment to

assess whether the educated respond differently when presented with a hypothetical financial incentive not to vote.

Observing individuals' stated intentions to vote presents another opportunity to study voting norms. Intentions can reveal what individuals wish their behavior to be and often reflect what individuals perceive to be prevailing social norms. For example, many people intend to donate to charity or wish they donated more than they already do. That intention reflects both a broader injunctive norm that it is good for individuals to donate, as well as a personal desire for the individual to conform with that norm.

Pertinent to this study, many national surveys ask respondents whether they intend to vote in a given upcoming election. The results are often striking; typically, the vast majority of respondents report that they intend to vote. For example, in the 2016 ANES, 94% of respondents reported intending to vote for president while 79% of respondents reported they intended to vote for a member of Congress. The high rate of self-reported intention to vote reflects a widely recognized norm surrounding voting in the U.S. It is clear that most individuals feel like they *should* report that they intend to vote. However, observing intention to vote alone is a poor indicator of norm internalization—it is costless for respondents to parrot expected answers.¹¹

Norm internalization would be better revealed if respondents were asked to choose between conflicting alternatives. In a survey experiment, we present respondents with a choice between adhering to a norm of voting by stating their intention to vote, and violating the norm when presented with a hypothetical financial incentive to abstain from voting. In line with our internalized norm hypothesis, we expect that the highly educated will be less likely to violate the norm, since they have more fully internalized it. Based on that expectation, we designed a survey experiment that tests the likelihood of norm violations, similar to a design created by White et al. (2014).

¹¹An intention to vote could be revealed by an individual overreporting as well. However, in contrast with overreporting, respondents reporting their intentions have no true past behavior on which to base their response.

We recruited 807 respondents through Amazon’s Mechanical Turk in the spring of 2018. Summary statistics describing the demographic and political composition of the sample are presented in Table A4 of the online appendix. Respondents were randomly assigned to one of three conditions. In the first condition (*Control*), respondents were simply asked, “How likely are you to vote in this year’s midterm elections in November?” Respondents answered on a 7-point Likert scale ranging from “Extremely Unlikely” (1) to “Extremely Likely” (7). Respondents assigned to the first condition served as the control group. Reflecting a widely recognized norm of voting, the majority of respondents reported they were at least somewhat likely to vote (mean response in the *Control* condition = 5.34).

In the second condition (*Incentive*), respondents were given the following prompt:

Suppose that on Election Day for the midterms this November, you find out you’ve been randomly chosen to win a \$500 cash prize from a drawing you entered. You must claim your prize in person by the end of the day. However, you haven’t voted yet. You have time either to claim your prize or to vote, but you can’t do both. How likely would you be to turn down the prize and vote?

The prompt encourages respondents to weigh whether they would be willing to violate norms of voting when given an incentive to do so. By design, the prompt grants respondents implicit permission to report that they would *not* vote. We consider this a feature of the design. Respondents without strong normative commitments to voting were given permission to deviate from an expected response that they would vote. At the same time, respondents with a strong normative view of voting could costlessly reaffirm their commitment to participation if they so chose. By pitting the importance of voting directly against a hypothetical incentive, we are testing whether respondents faithfully convey the voting norm when faced with an opposing pressure. This is a sign of the norm’s internalization, so withstanding the incentive to abstain indicates that respondents value voting quite highly.

We settled on \$500 as an amount large enough to attract the attention of a wide swath of respondents while not so large as to make abstaining an inevitable decision (Who wouldn’t

skip an election to claim a \$1 million prize?). We expect that assignment to the *Incentive* condition will significantly depress respondents' intention to vote. However, we expect the size of the decrease to be conditional on educational attainment. If voting norms vary across levels of education as predicted, the treatment should depress intention to vote less for high-education respondents than it does for low-education respondents.

We also included a third condition to observe how respondents navigated the tradeoffs between explicit social pressure and the disincentive. In the third condition (*Incentive + Local News*), respondents were presented with the same hypothetical scenario, but were also given the following additional information:

A local TV reporter will be on site to interview you if you accept the prize. The reporter plans to ask you on camera whether you voted.

Whereas respondents in the *Incentive* condition were presented the choice between an incentive and voting in a social vacuum, respondents in the *Incentive + Local News* condition were presented with a reminder of potential social consequences for taking the money. The prompt is intended to capture the threat of social pressure (i.e. norm enforcement) in influencing respondents' decisions. If we are correct that voting norms differ across levels of education, we should see that high-education respondents are more susceptible to the social pressure treatment. Empirically, we should see that the social pressure treatment increases the intention to vote among high-education respondents compared to the *Incentive* condition more than it does the intention to vote among low-education respondents.

We plot the results of these experiments in Figure 3. Additionally, coefficient estimates and standard errors from the OLS regression models for all subsequent analyses are presented in Table 3 below. The top panel of Figure 3 displays the means across conditions. In the *Control* condition, respondents were quite likely to report an intention to vote (mean = 5.34 on a 7-point scale). As expected, offering respondents an incentive to abstain significantly depressed the *Incentive* group's intention to vote relative to respondents in the *Control* condition (-2.43, $p = 0.00$). We do not see evidence of respondents defying the treatment.

However, the social pressure treatment in the third experimental condition seems to have been less effective. On average, respondents in the *Incentive + Local News* condition reported being slightly more likely to vote than respondents in the *Incentive* condition, but that difference was not statistically significant (0.28, $p = 0.11$).

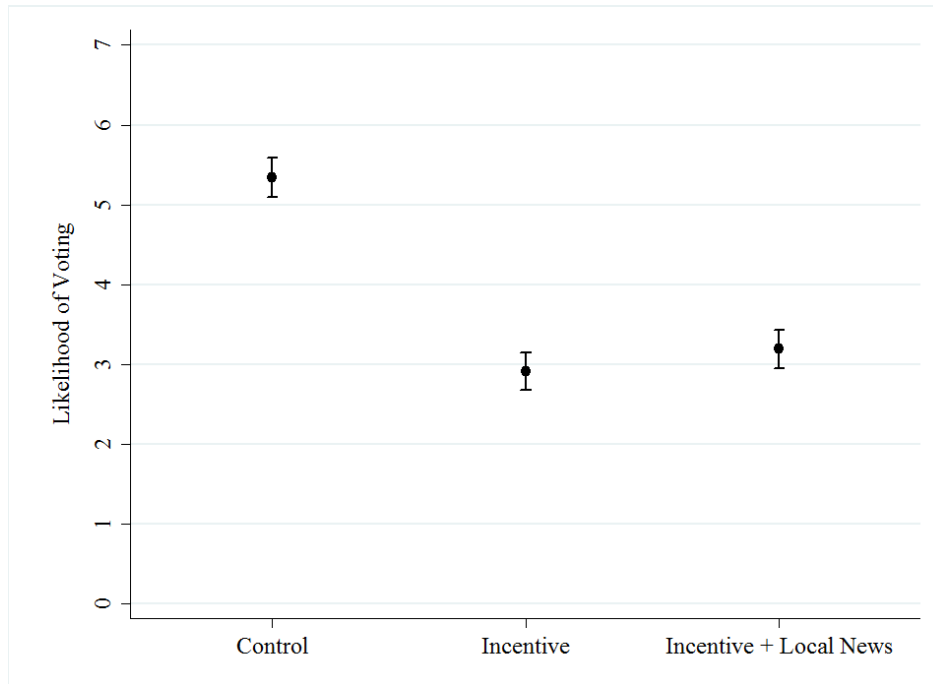
The principal test of our theoretical expectations comes from the results in Panel B, which presents conditional means for high-education and low-education respondents. If respondents with more education internalize voting norms more fully, we should see a difference in their likelihood of voting in the face of incentives not to vote. For ease of interpretation, we divide education into a binary measure based on whether or not the respondent holds at least a four-year college degree.¹²

In the *Control* condition, respondents with and without a college degree report being essentially equally likely to vote. Moving from the *Control* to the *Incentive* condition, the reported likelihood of voting among both college-educated and non-college-educated respondents drops off significantly. Non-college-educated respondents' likelihood of voting decreases by 3.03 ($p = 0.00$) on a 7-point scale. However, college-educated respondents' likelihood of voting only decreases by 1.98, a significant difference from non-college-educated respondents of 1.05 ($p = 0.00$). Clearly, college-educated respondents are not completely insulated from incentives to abstain. Yet, the disparity between education levels is itself noteworthy. This finding is consistent with the expectation that more educated respondents will be less willing to violate norms of voting even when given an incentive to do so.

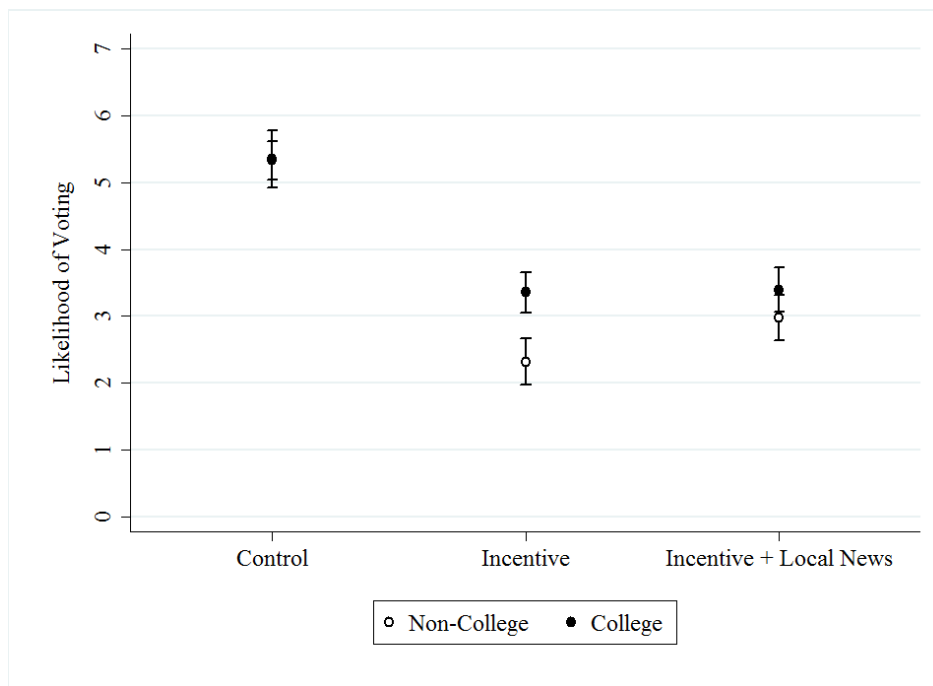
Next we determine whether social pressure is more effective in driving voting intentions for high-education respondents. Moving from the *Incentive* to *Incentive + Local News* condition, non-college-educated respondents report an increase of 0.66 in likelihood of voting on a 7-

¹²We investigate whether the experimental results are robust to using different measures of educational attainment. Results are presented in Table A5 in the appendix. The results hold when education is measured using an ordinal variable as in Figure 1 above, as indicated by the positive and significant coefficient estimate for the *Incentive X Education* variable in column 1. However, the results do not hold when the educational cutpoint is placed at Some College (column 2) or at Post-Graduate Degree (column 3). Together these tests suggest that while normative pressure increases with education, a four-year college degree seems to serve as a crucial threshold.

Figure 3: Experimental Results



Panel A: Treatment



Panel B: Education

Note: Authors' data. 95% confidence intervals reported.

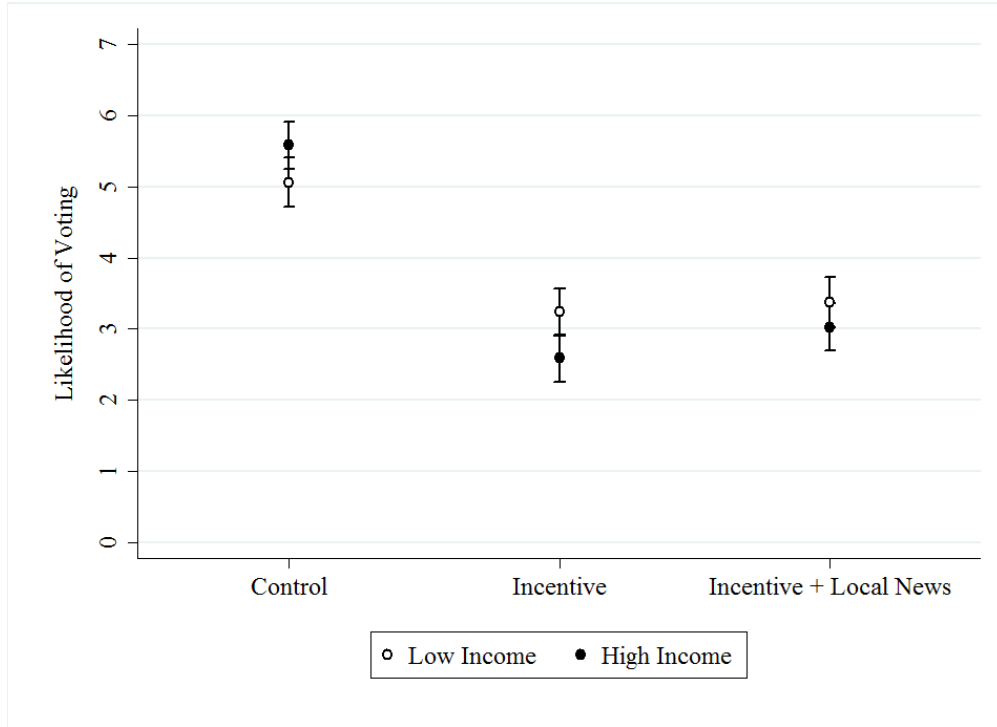
point scale ($p = 0.10$). College-educated respondents report a small and insignificant increase of 0.04 ($p = 0.87$). The difference in treatment effects across the two groups is -0.62 ($p = 0.08$). This finding is inconsistent with the expectation that more educated respondents will be more susceptible to social pressure. Surprisingly, if anything, explicit social pressure increased the likelihood of non-college-educated respondents to report voting. We discuss possible interpretations of this inconsistent finding below.

A potential criticism of our interpretation of the conditional treatment effects is that highly educated respondents are less likely to take the reward because they are in a better economic position to bear the opportunity costs if they chose to forgo the financial prize. Under that explanation, \$500 would be relatively more valuable to low-education respondents than to high-education respondents, perhaps threatening the norms-based interpretation. If the results are driven by the financial resources of the highly educated, we should see the same pattern if we compare high-income respondents' response to treatment with the response of low-income respondents. For ease of interpretation, we divided respondents into two income groups based on whether their household income fell above or below the sample median.¹³

The results appear in Figure 4. Moving from the *Control* to *Incentive* condition, low-income respondents' likelihood of voting decreases by 1.83 ($p = 0.00$). However, high-income respondents' likelihood of voting decreases by 3.00 ($p = 0.00$). The difference in treatment effects between these two groups is -1.17 ($p = 0.00$). Inconsistent with a resources explanation, high-income voters (those best able to bear the opportunity cost of forgoing the incentive) actually report being *more* likely to take the money instead of voting. Moving from the *Incentive* to *Incentive + Local News* condition reveals little noteworthy in the way of social pressure's effect. Both low-income (0.14, $p = 0.58$) and high-income (0.44, $p = 0.06$) respondents show small but non-significant increases in likelihood of voting, and the difference in treatment effects between the two groups is small and statistically insignificant

¹³Household income was measured categorically, and the median respondent fell in the range of \$40,000–\$49,000 a year. The median is admittedly an arbitrary cutpoint for distinguishing between high- and low-income respondents. In Table A6 of the online appendix, we show the results are robust to choices of different arbitrary cutpoints of income.

Figure 4: Experimental Results for High- and Low-Income Respondents



Note: Authors' data. 95% confidence intervals reported.

(0.30, $p = 0.30$).

As a final robustness check, we estimate the treatment effects for college-educated and non-college-educated respondents while controlling for income. The results are presented in the fourth column of Table 3. Compared to the second column, without the control, the coefficient estimates and standard errors remain virtually the same. Interestingly, the coefficient estimate for the income variable is signed negatively and not statistically significant, suggesting that income is not associated with a higher likelihood of voting once education is taken into account. Taken together, these results suggest that financial need is not driving the conditional effects of education documented above.¹⁴

The experimental results provide mixed support for our theory. More educated respon-

¹⁴Highly educated respondents may also disproportionately possess other resources like time or civic skills, not just income. While we do not have the data to rule out these other resources as driving the relationship observed above, the income finding weakens suspicions that the results are *necessarily* explained by resources.

Table 3: Experimental Results by Moderating Variable

	(1) Treatment	(2) Education	(3) Income	(4) Education w/ Income Control
Incentive	-2.43* (0.17)	-3.03* (0.28)	-1.83* (0.25)	-3.03* (0.28)
Incentive + Local News	-2.15* (0.18)	-2.38* (0.28)	-1.69* (0.25)	-2.37* (0.28)
College		-0.02 (0.26)		0.01 (0.27)
Incentive X College		1.05* (0.36)		1.04* (0.36)
Incentive + Local News X College		0.44 (0.36)		0.43 (0.36)
High-Income			0.52* (0.25)	-0.23 (0.14)
Incentive X High-Income			-1.17* (0.34)	
Incentive + Local News X High-Income			-0.87* (0.35)	
Constant	5.34* (0.12)	5.35* (0.22)	5.07* (0.18)	5.44* (0.23)
<i>N</i>	807	807	807	807
Adj. <i>R</i> ²	0.22	0.24	0.23	0.24

Note: * $p < 0.05$. Authors' data. Standard errors are presented in parentheses. Significance tests are two-tailed.

dents displayed more resistance to accepting a financial incentive not to vote (evidence suggesting internalized norms). Moreover, the fact that high-income respondents were more likely than low-income respondents to take the financial reward rather than vote casts doubt on the idea that \$500 was relatively more valuable to low-education voters. However, the results also showed that, in the presence of the incentive, additional social pressure in the form of having to be transparent about taking the money in a TV news interview increased low-education respondents' likelihood of voting more than high-education respondents' likelihood. This finding, which did not align with expectations, could have several reasonable explanations. The first possibility is that non-college-educated respondents are more re-

sponsive to explicit social pressure than college-educated respondents. A second possibility, compatible with the first, is that college-educated respondents were more likely to have internalized norms, such that additional social pressure to conform with norms did not move their responses across conditions. However, we cannot rule out any of the following additional possibilities: that unobserved differences between the groups explain the difference, that evoking a local news report to apply social pressure is a weak treatment, or that the null result would not replicate in future iterations of the experiment.

It is worth briefly noting here that this experimental design does not isolate the causal effect of education on norm internalization. Respondents were not randomly assigned to certain levels of educational attainment, meaning that any antecedent factors present in the self-selection process are not controlled for. Rather, the purpose here is to illustrate differences in response to the experimental treatment between groups with differing levels of educational attainment. The results are consistent with our theory, though they do not contribute dispositive causal evidence of it.

Conclusion

In three studies, we have provided evidence consistent with the idea that college-educated Americans are more likely to have internalized social norms surrounding voting. First, the educated express greater belief that voting is a civic duty, and this belief partially mediates the effect of educational attainment on validated voting behavior. Further, the college-educated are more likely to engage in two additional behaviors: falsely reporting having voted on a survey, and withstanding financial pressure to deviate from the voting norm. This evidence suggests that more educated individuals vote at higher rates, in part, because of social norms.

To be clear, the effects we uncover are modest. Civic norms help explain the tendency of educated citizens to vote at higher rates, but they neither rule out nor overshadow other

forces that drive this disparity. Moreover, these results do little to illustrate exactly *why* norms differ based on level of education. Given that many of them actively work to encourage voting behavior, educational institutions remain a likely suspect as a space where such norms are instilled. However, the evidence presented here does not rule out the possibility that antecedent factors are responsible for the relationship we find between education and norm internalization. For instance, it could be the case that individual need to comply with social expectations, perhaps affected by personality or early childhood socialization in the family, drives both norm adherence and educational attainment.

While the idea that internalization of civic norms varies with educational attainment is not new (Jackson 1995; Wolfinger and Rosenstone 1980), this link has receded from the academic and public conversation around voting. It is worth retesting after decades of expanding educational attainment in the American public. Because we do not test our explanation directly against other explanations in the analyses above, we cannot comment on the importance of norms relative to other potential mechanisms for education’s effect on voting, such as political knowledge or verbal ability. However, this work does provide evidence that at least some of education’s effect comes from the establishment and reinforcement of voting norms.

The findings presented here speak to recent field experimental research establishing that normative social pressure can cause people to vote (Doherty et al. 2017; Gerber et al. 2008, 2010, 2016; Panagopoulos et al. 2014). Much of this research focuses on treatment effects for the population while ignoring potentially important moderating or mediating factors. This can partly be explained by the fact that field experimental data is dependent on publicly available voter file data that do not include many relevant variables. Some evidence suggests that other variables, specifically age (Panagopoulos and Abrajano 2014) and partisanship (Panagopoulos and van der Linden 2016), moderate the effect of social pressure. Our findings suggest that educational attainment plays a moderating role as well.

Our findings also speak to public conversations around voter turnout. Widespread, pop-

ular explanations of voting disparities focus strongly on costs and barriers. Such a focus presupposes that people want to vote in the first place but are prevented from doing so by burdensome administrative requirements, difficulties with accessibility, or a lack of information. Though the policy changes have been noteworthy, the evidence is mixed on whether such efforts to reduce participation barriers have improved turnout (Berinsky 2005; Burden et al. 2014; Gronke et al. 2007). For instance, the historic surge in turnout in the 2018 midterm elections, particularly among young voters, was not prompted exclusively by the systematic removal of voting barriers. This suggests that a fuller account of voting behavior that takes account of norms and motivations is needed.

Our findings underscore the idea that habitual voting is a learned behavior, perhaps instilled by communities and reinforced in social networks (Gerber et al. 2003). Norms governing individual behavior vary across communities. Though a social norm of voting is widespread in the United States, voting should not be viewed as a default cultural expectation. The results we present suggest that socioeconomic disparities in voting will persist until reformers take steps to motivate greater participation in conjunction with their efforts to reduce the costs of voting.

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Online Appendix for “Educational Attainment and Social Norms of Voting”

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Table A1: Civic Duty and Educational Attainment

	(1)	(2)	(3)	(4)
Education	0.30* (0.042)		0.29* (0.043)	
College		0.74* (0.097)		0.72* (0.096)
Age	-0.026 (0.014)	-0.024 (0.014)	-0.026 (0.014)	-0.024 (0.014)
Age ²	0.00042* (0.00014)	0.00039* (0.00014)	0.00042* (0.00013)	0.00040* (0.00013)
Female	0.041 (0.095)	0.069 (0.095)	0.044 (0.097)	0.069 (0.097)
White	0.32 (0.17)	0.32 (0.17)	0.31 (0.16)	0.30 (0.16)
Black	0.80* (0.22)	0.78* (0.21)	0.80* (0.22)	0.78* (0.21)
Hispanic	0.62* (0.22)	0.59* (0.22)	0.59* (0.21)	0.56* (0.21)
Foreign Born	0.15 (0.20)	0.12 (0.20)	0.15 (0.20)	0.12 (0.20)
Religious Attendance	0.13* (0.031)	0.13* (0.031)	0.14* (0.033)	0.14* (0.032)
PID Strength	0.30* (0.049)	0.30* (0.049)	0.30* (0.049)	0.30* (0.049)
Interest	0.36* (0.059)	0.38* (0.058)	0.37* (0.061)	0.38* (0.060)
Dependable			0.023 (0.053)	0.027 (0.053)
Extraverted			-0.040 (0.028)	-0.044 (0.028)
Openness			0.026 (0.043)	0.032 (0.042)
Careless (Reverse-Coded)			-0.011 (0.034)	-0.012 (0.033)
Conventional			-0.009 (0.037)	-0.016 (0.036)
Constant	2.11* (0.37)	2.41* (0.37)	2.12* (0.50)	2.41* (0.50)
<i>N</i>	4092	4092	4053	4053
<i>R</i> ²	0.11	0.11	0.11	0.12

Note: *p<0.05. Survey-adjusted standard errors are presented in parentheses. Significance tests are two-tailed.

Table A2: Civic Duty, Value of Autonomy and Educational Attainment

	(1)	(2)	(3)	(4)
Autonomy	0.06 (0.04)	0.01 (0.04)	0.04 (0.06)	0.05 (0.05)
College		0.89* (0.10)	1.19* (0.40)	0.74* (0.10)
College X Autonomy			-0.078 (0.10)	
Age				-0.02 (0.01)
Age ²				0.00039* (0.00014)
Female				0.08 (0.09)
White				0.28 (0.16)
Black				0.75* (0.21)
Hispanic				0.54* (0.22)
Foreign Born				0.17 (0.20)
Religious Attendance				0.14* (0.03)
PID Strength				0.30* (0.05)
Interest				0.38* (0.06)
Constant	4.16* (0.17)	4.04* (0.17)	3.96* (0.22)	2.26* (0.40)
<i>N</i>	4248	4218	4218	4085
<i>R</i> ²	0.00	0.03	0.03	0.11

Note: * $p < 0.05$. Survey-adjusted standard errors are presented in parentheses. Significance tests are two-tailed.

Figure A1: Covariates and Balance in Data Matching in Study 1

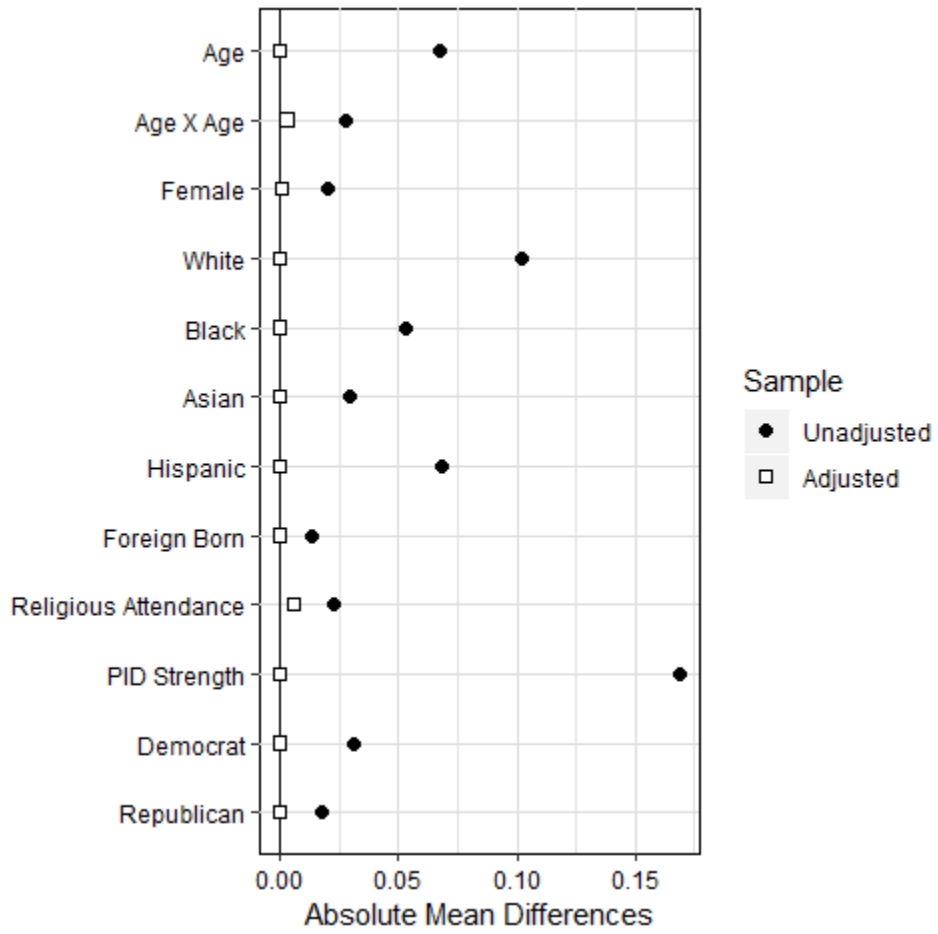
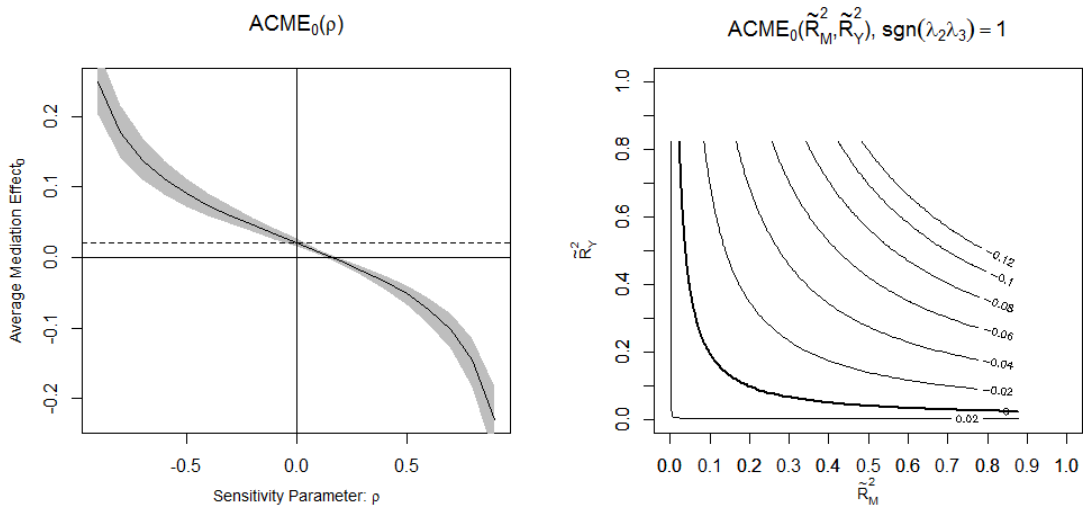
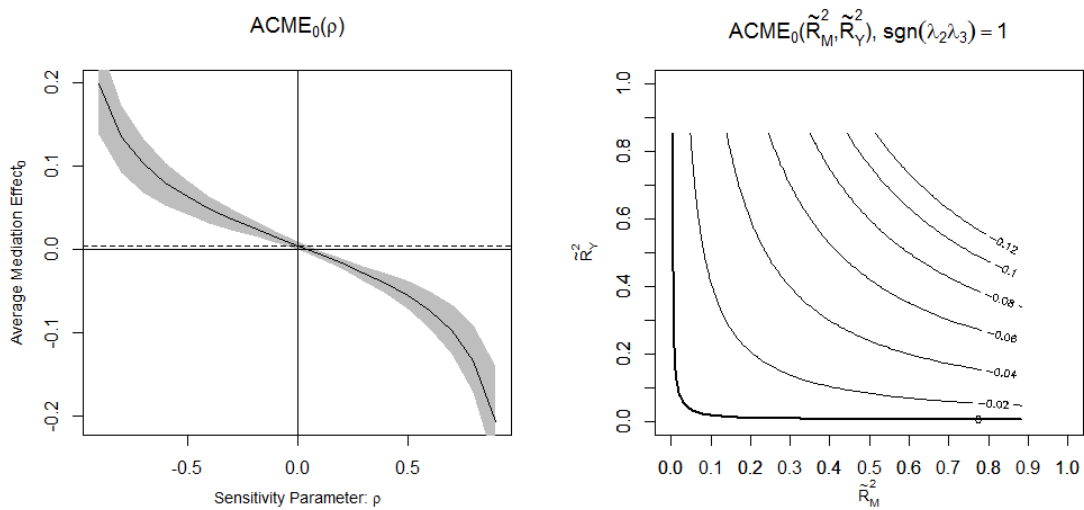


Figure A2: Sensitivity Analysis for Causal Mediation Analysis in Study 1



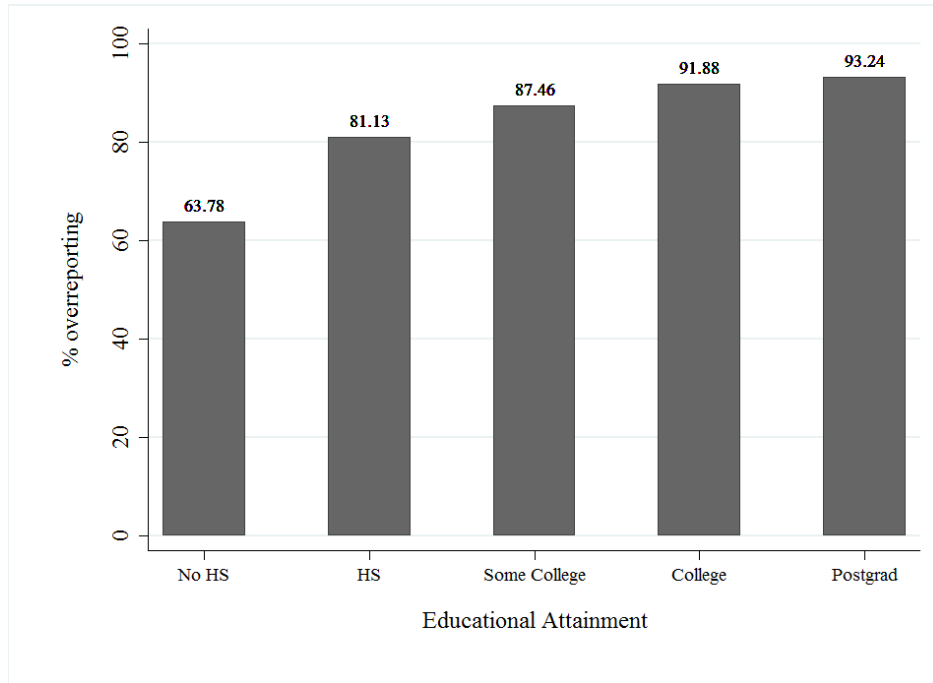
The left panel plots the values of the true ACME against values of the sensitivity parameter ρ . The dashed line represents the estimated ACME when $\rho = 0$ (under sequential ignorability). The estimated ACME for the model is 0.023. The right panel plots the amount of variation a confounding variable would need to explain in the mediating variable M and outcome variable Y to yield a true ACME with a value listed next to the curved line. For this model, the product of the R^2 values necessary for the true ACME to equal 0 is 0.0357.

Figure A3: Sensitivity Analysis for Placebo in Study 1



The left panel plots the values of the true ACME against values of the sensitivity parameter ρ . The dashed line represents the estimated ACME when $\rho = 0$ (under sequential ignorability). The estimated ACME for the model is 0.0005. The right panel plots the amount of variation a confounding variable would need to explain in the mediating variable M and outcome variable Y to yield a true ACME with a value listed next to the curved line. For this model, the product of the R^2 values necessary for the true ACME to equal 0 is 0.

Figure A4: Overreporting among Validated Non-Voters by Level of Education (CCES)



Source: 2016 Cooperative Congressional Election Study.

Table A3: Overreporting among Validated Non-Voters: Replication of Table 2 using CCES

	(1)	(2)
Education	0.61* (0.061)	
College		0.95* (0.092)
Age	-0.0065 (0.020)	0.0042 (0.019)
Age ²	0.0004* (0.0002)	0.0003 (0.0002)
Female	-0.19 (0.100)	-0.13 (0.10)
White	0.37* (0.18)	0.33 (0.20)
Black	-0.04 (0.22)	-0.12 (0.23)
Hispanic	0.57* (0.23)	0.45 (0.24)
Foreign Born	-0.28* (0.14)	-0.26 (0.14)
Religious Attendance	0.02 (0.03)	0.03 (0.03)
PID Strength	0.25* (0.04)	0.24* (0.05)
Interest	0.50* (0.06)	0.54* (0.06)
Constant	-1.65* (0.50)	-1.07* (0.50)
<i>N</i>	12,363	12,363
BIC	8263.62	8501.26

Note: *p<0.05. Data from the 2016 CCES. The results obtained from a survey-weighted logistic regression model with standard errors in parentheses. Significance tests are two-tailed.

Table A4: Summary Statistics for MTurk Sample

(1)	(2)
Education	
High School or less	12.39%
Some College	28.75%
Four-Year Degree	46.34%
Postgraduate Degree	12.52%
Male	60.47%
Age (mean)	35.12
Income (mean)	\$35,000 - \$39,999*
Race/Ethnicity	
White alone	70.76%
Asian alone	12.27%
Black alone	6.57%
Other	10.41%
Latino/Hispanic (of any race)	13.75%
Ideology	3.53**
Partisanship	
Democrats & Leaners	52.54%
Republicans & Leaners	33.33%
Independent/Other	14.12%

Notes: Author's data. *Mean of 8.65 using categorization scheme. **Ideology measured on 7-point scale, with 7 being most conservative.

Table A5: Experimental Results by Varying Education Cutpoints

	(1) Education (Ordinal)	(2) Some College	(3) College	(4) Post-Graduate Degree
Incentive	-3.63* (0.59)	-2.61* (0.58)	-3.03* (0.28)	-2.56* (0.18)
Incentive + Local News	-2.37* (0.58)	-2.15* (0.56)	-2.38* (0.28)	-2.12* (0.18)
Education	0.07 (0.16)			
Incentive X Education	0.47* (0.21)			
Incentive + Local News X Education	0.10 (0.21)			
Some College		0.07 (0.49)		
Incentive X Some College		0.21 (0.61)		
Incentive + Local News X Some College		0.01 (0.59)		
College			-0.02 (0.26)	
Incentive X College			1.05* (0.36)	
Incentive + Local News X College			0.44 (0.36)	
Post-Graduate				0.38 (0.38)
Incentive X Post-Graduate				0.75 (0.50)
Incentive + Local News X Post-Graduate				-0.17 (0.56)
Constant	5.15* (0.46)	5.28* (0.47)	5.35* (0.22)	5.29* (0.13)
<i>N</i>	807	807	807	807
Adj. <i>R</i> ²	0.24	0.22	0.24	0.23

Note: * $p < 0.05$. Authors' data. Standard errors are presented in parentheses. Significance tests are two-tailed.

Table A6: Experimental Results by Varying Income Cutpoints

	(1)	(2)	(3)
	Median	1 st Quartile	3 rd Quartile
Incentive	-1.83* (0.25)	-1.32* (0.37)	-2.19* (0.20)
Incentive + Local News	-1.69* (0.25)	-2.16* (0.39)	-2.02* (0.21)
High-Income	0.52* (0.25)	0.18 (0.30)	0.46 (0.27)
Incentive X High-Income	-1.17* (0.34)	-1.41* (0.42)	-0.83* (0.38)
Incentive + Local News X High-Income	-0.87* (0.35)	0.01 (0.43)	-0.41 (0.39)
Constant	5.07* (0.18)	5.20* (0.27)	5.20* (0.15)
<i>N</i>	807	807	807
Adj. <i>R</i> ²	0.23	0.24	0.22

Note: * $p < 0.05$. Authors' data. Standard errors are presented in parentheses. Significance tests are two-tailed. Table presents changes in experimental results when the arbitrary cutpoint between high-income and low-income is moved from the median to the first quartile and the third quartile. In all three cases, higher-income respondents are more likely to take the hypothetical financial incentive rather than vote, as indicated by the significant, negatively signed interaction term for Incentive X High Income in each model.