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Evidence from a General Election

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Abstract

We exploit new experimental and quasi-experimental data to investigate voters' intrinsic motivation to engage politically. Does having the right to vote increase engagement or, given significant incentives to free ride, do eligible voters remain rationally unengaged? Does knowledge that one's group is pivotal reduce free riding? And are the politically engaged influenced by election-relevant policy information in the run-up to a major election? To address these questions, we fielded an original survey of 5,400 Mexican high school seniors just prior to the historic 2018 general election. Age-based regression discontinuity results show that the just-eligible score higher on measures of low-cost political engagement compared to the just-ineligible. A first survey experiment reveals that information that the youth vote will be pivotal increases the eligible respondents' interest in the presidential debate and in the election result. In the second experiment, information about current policy outcomes affects future policy priorities in ways consistent with the incentives of eligible respondents to collect relevant information on salient policy issues.

JEL classifications: D73, H83

Keywords: Political engagement, Free riding, Pivotal voters, Policy information

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1 Introduction

Are voters intrinsically motivated to engage in the political process or, given their significant incentives to free ride, do they remain rationally unengaged? The question is of long-standing importance in political economy, at least since Downs (1957), with most research focused on mature democracies. We investigate the effects of voting eligibility on the political engagement of thousands of Mexican high school seniors using an original in-class survey conducted shortly before the 2018 Mexican general election, which featured the country's sixth consecutive competitive presidential election following a period of 60 years of non-competitive elections. We exploit experimental evidence to examine two further key issues related to political engagement: to what extent does the free-rider problem suppress engagement? And under what conditions do the politically engaged exert effort to better inform their electoral choices?

The voting age requirement allows us to leverage a regression discontinuity design to capture eligibility effects on a wide range of engagement measures. The sample of high school seniors includes some just eligible to vote and some just ineligible. Engagement takes many possible forms that vary significantly with respect to salience and cost. The two groups of students exhibit significant differences with respect to the most salient and lowest cost forms of engagement: those who are just eligible to vote are significantly more interested in the results of the Mexican presidential election and in watching the third and last debate between the presidential candidates.[1]

Voters have different perceptions of whether their vote matters — the value of their vote. One survey experiment explores this issue by providing treated respondents with information that young people's votes may be pivotal in the next election. Treated respondents expressed greater interest in the election, an effect that was greatest among respondents who were eligible to vote.

Moreover, incentives to be engaged, for example to seek out and process information relevant for electoral decision making, change over the electoral cycle. The second survey experiment explores the incentives of politically engaged individuals to collect and process electorally relevant information in the weeks before the election. Treated respondents were given information about crime and education outcomes in Mexico. They are, on average, more likely to indicate that the next president should give a higher priority to these two

[1] A follow-up email survey taken after the final debate confirms that students who had earlier expressed intentions to watch the last presidential debate were significantly more likely to have actually watched it.

issues. However, the effect is *smaller* among eligible than ineligible respondents. This is consistent with the greater incentives of politically engaged respondents — those eligible to vote — to have already collected relevant information on incumbent policy performance in the weeks prior to the survey.

The survey respondents were 12th-grade high school students who took the survey online while in their classrooms. A total of 30 campuses of a high school network participated and they were located throughout Mexico. The survey was given four to six weeks before the general election of 2018. About 60 percent of students surveyed would be eligible to vote for the first time. The survey timing was highly salient: two of three presidential debates had already been televised at the time of the survey. We use respondents' exact date of birth as a running variable for voting eligibility to support a sharp regression discontinuity design.

Prior research examining whether eligibility increases political engagement has focused on mature democracies. Horiuchi, Katsumata, and Woodard (2021) use Japan's age restriction for automatic voter registration in a sharp regression discontinuity framework and find that just-eligible respondents are somewhat more likely to seek election-relevant information, but the estimates cannot identify any impact on specific activities undertaken to acquire information.[2] Holbein and Rangel (2020) find no effects of eligibility on political engagement for American respondents. Rosenqvist (2017) exploits an RDD design in Sweden and, focusing on general political knowledge, identifies no difference across just eligible and ineligible voters.[3]

We depart from prior work by studying the electoral setting of a developing country while making several research design innovations. First, our survey is taken prior to the election, thus avoiding post-rationalization biases that might emerge if reported political engagement is influenced by whether eligible respondents actually voted, or by learning the election result. Second, we explore different types political engagement varying in salience and the costs they impose. The two lowest cost and most salient activities, neither examined in earlier research, are interest in the election result and in viewing the final presidential debate. Both are significantly greater among just-eligible respondents.

Third, our survey accommodates two randomized experiments that probe the mechanisms behind the political engagement differences between the eligible and non-eligible. For

[2]Stiers, Hooghe, and Dassonneville (2021) similarly find that just-eligible voters in Belgium report paying more attention to politics but exhibit no broader evidence of greater political engagement. Their population is particularly young, however, since the eligibility age they examine is 16.

[3]Khoban (2019) examines the effect of voter eligibility on political engagement unrelated to elections and finds that it boosts non-electoral political participation.

example, we demonstrate that information that the youth vote is pivotal has a significant impact on political engagement. The belief that their vote is not pivotal is a potential explanation for the more modest effects on engagement uncovered in prior research in more mature democracies.

Other research either examines the impact of eligibility with alternative methodologies, or exploits an RDD framework to explore related issues, but not the impact of eligibility on political engagement. In the former category, Scartascini and Vlaicu (2018) use survey data from multiple Latin American countries and a difference-in-differences design. They find that voter eligibility increases self-reported interest in politics, political socialization, attendance at political meetings and consumption of political news.

In the latter category, several studies focus on the effects of obligatory voting in Brazil on political engagement (Lopez de Leon and Rizzi 2014, Singh and Roy 2018, and Bruce and Costa Lima 2019). Perhaps because compulsory voting has theoretically ambiguous effects on intrinsic motivation to be politically engaged, these studies yield ambiguous results on the effects of voting on political engagement. However, measures of engagement matter as well. Bruce and Costa Lima (2019) focus on one that is particularly salient and low-cost. Using an RDD design, they find that students who are just old enough to be obliged to vote are also more likely to watch Brazil's main television newscast.

Relative to prior research, the Mexican electoral context offers a more straightforward setting to identify the intrinsic motivations of eligible voters to engage politically. In Japan, for example, as Horiuchi, Katsumata, and Woodard (2021) explain, upon becoming eligible to vote Japanese citizens are also automatically *registered* to vote and informed through a government letter about the logistics of voting. This lowers the costs of political engagement but may also stymie self-motivation, which is one of the main arguments the authors use to explain their limited findings. In Brazil, individuals who turn 18 are not only eligible to vote, but obligated to do so; compulsory voting has a theoretically ambiguous effect on engagement, however. In contrast, in Mexico eligibility to vote is independent of registration to vote and voting is voluntary.

Finally, we were able to field a survey that, because of in-classroom administration by teachers and enumerators, exhibits a nearly 100 percent response rate. Prior studies rely on data from surveys with low response rates, raising the possibility that only the most engaged respond, suppressing variation and making eligibility effects harder to identify. For example, response rates were 8.5 and 15 percent in the surveys used by Horiuchi, et al (2021) and Holbein and Rangel (2020); our own follow-up email survey also had about an 8.5 percent response rate.

The survey experiment on pivotal voters contributes to a literature dating back to Downs (1957). He observed that voting, like other forms of political engagement, is subject to free-riding and rational abstention. Incentives to free-ride, however, fall when the private value of an individual's vote rises - for example, when the vote is more likely to be pivotal. Duffy and Tavits (2008) demonstrate in a laboratory setting that subjects who believe that they are pivotal are more likely to vote. The analysis here complements this research by showing that pivotality encourages other forms of engagement and does so even when pivotal groups are large — for example, 12 million individuals, as in the case of young Mexican voters — and the value of their vote is still much smaller than in a laboratory setting.[4]

The second survey experiment, providing treated respondents with information on policy performance, advances a large literature arguing that voter incentives to acquire and process new information are shaped by whether they expect additional cognitive investment to shift their beliefs. Gelman and King (1993) and Le Pennec and Pons (2019) find that the more voters have learned about candidates, the stronger should be their prior beliefs about them. Like DellaVigna and Gentzkow (2010), they conclude that the stronger are individuals' priors, the less their beliefs about candidates are affected by new information. Anticipating this, voters should invest less in acquiring and processing new information. Broockman and Kalla (2020) exploit data from a large survey experiment of American respondents to show that new information about presidential candidates with whom they are more familiar has a smaller effect on their preferences than new information about a candidate with whom they are less familiar. The effects documented in Bruce and Costa Lima (2019), that upon being obliged to vote young people in Brazil are more likely to watch the main television newscast, are strongest for those who have neutral evaluations of the incumbent president - those who have the weakest prior beliefs about which candidate is stronger.

We complement this work by exploring an earlier step in voters' cognitive processes. Before they decide which candidate they prefer, voters must settle on the criteria they will use to evaluate candidates. We examine the effects of information on policy outcomes on

[4]These results are relevant to theoretical analyses of the swing voter. Feddersen and Pesendorfer (1996) point to a swing voter's curse among the uninformed: indifferent voters, who are more likely to be pivotal, are less likely to vote if they are uninformed (see also Degan 2006). Consistent with this, Larcinese (2007) finds that exogenous exposure to mass media increases the probability of voting. Laboratory experiments by Battaglini, Morton, and Palfrey (2010) document more precisely that voters are aware of the swing voter's curse and adjust their behavior as Feddersen and Pesendorfer (1996) predict. We find that just-eligible students who are informed that young voters will be pivotal express more interest in politics and a stronger intention to acquire political information compared to the just-eligible who were not assigned to the pivotality treatment.

respondents' views regarding the policies that matter most to them rather than, as in past work, on respondents' candidate preferences.[5] We find that eligible respondents, who are more engaged and more likely to have established those criteria, are more resistant to using new information to update their criteria for presidential candidates. However, that resistance is greater for more salient issues, which eligible respondents are both more likely to have been informed about and more likely to have established as one of their voting criteria.

The next section reviews the survey and associated experiments and summarizes the data we collected. We then present estimates of the effects of eligibility on engagement, both non-parametric and from RDD specifications. The results of the two experiments are then presented and discussed, concluding with a discussion of the implications of the results for policy and future research.

2 Data

We collected information about political engagement over the period May 14-28, 2018, 4-6 weeks prior to the Mexican general election of July 1, 2018 (see the timeline in Figure 1). Like Leon and Rizzi (2014), we used classroom surveys of high school students to ensure high response rates. A network of private schools, Prepa UVM, allowed us to undertake surveys in 30 of their high schools. These were located around the country, from the northern border with the United States to the southern border with Guatemala (see Figure 2 and Table 1 for the distribution of respondents across the 30 campuses).

The main survey was computer-based, supervised by a teacher and an enumerator from Data OPM, a Mexico-based survey firm, and not incentivized. It consisted of 38 questions. 5,400 students completed the survey, similar to the sample size in Leon and Rizzi (2014); in-classroom administration ensured a response rate of nearly 100 percent.

To establish whether they had actually watched the final presidential debate, we also emailed a follow-up survey to all respondents who had voluntarily supplied their email addresses. The email went out on June 29, 2018, just after the last debate but before the election. To raise the response rate, this non-incentivized survey consisted of only two questions aimed at finding out if respondents had watched the last presidential debate. The 442 responses to this questionnaire yield a response rate similar to that of Horiuchi, et al. (2021).

[5] Voters may resist new information on candidates because they are unwilling to depart from their partisan identities (Fowler 2020); partisan identity should have less effect on their willingness to use information to establish the criteria that matter to them.

Ideal measures of political engagement unambiguously capture respondent efforts to discern which candidates most align with their interests and to support their favored candidates. However, it is difficult to anticipate which modalities of political engagement respondents will undertake: the benefits that voters perceive from different modalities and the costs to voters of undertaking them are unobservable.

Responding to this uncertainty, the survey asks about a variety of forms of political engagement. Since political engagement is naturally triggered by political events, such as the presidential election, we expect those measures that describe election-related behavior to be more sensitive to eligibility than more general measures of engagement. Forms of engagement that are more costly, such as attendance at rallies, should be less sensitive. Figure 3 shows that all eight measures are greater among eligible respondents; five of them are statistically larger, with magnitudes between 0.10-0.20 standard deviations.

Two questions about political engagement are common in the literature and included in our survey. The first is "In general, how interested are you in politics?" On a four point scale, from not at all interested to very interested, respondents said they were little to somewhat interested in politics (2.78). The second commonly asked question is how often respondents discuss politics. On a four point scale, from never to frequently, respondents discussed politics often (3.47). Prior research also frequently asks about a more costly form of political engagement, which eligibility may therefore be insufficient to influence, "In the last year, how many campaign events have you participated in? (0, 1, 2, 3 or more)." Reflecting the costs of participation in rallies, only 15 percent of respondents had participated in even one event. Eligible respondents participated only slightly more than ineligible respondents.

Other questions are more salient because they ask respondents whether they are specifically engaged with the presidential election. The first was "Have you watched any of the presidential debates?"; 83 percent had watched at least one of the two TV debates that had already taken place at the time of the survey. Eligible respondents were somewhat but not significantly more likely to have watched earlier debates.

Two additional questions were tied specifically to the presidential election and exhibit significant differences between eligible and non-eligible respondents, "How much do you care about the result of the presidential election? (A Lot, Some, Little, None);" and "In total there will be three debates between the presidential candidates. Do you plan to watch the last one in about a month's time? (Yes, Probably, No)." Eligible respondents were significantly more engaged on both dimensions, nearly 0.20 standard deviations more likely to express interest watching the final debate and more than 0.15 standard deviation more likely to express interest in the results of the election.

Interest in the third debate is particularly revealing of respondent willingness to engage politically. Debates are not only a source of information for the politically engaged, but also of entertainment for both the engaged and disengaged. The third debate, however, is likely to have had less entertainment value than the first two. Those who express interest in the third debate are therefore more likely to be particularly interested in the presidential election and either watching to make sure that their preferred candidate performs adequately or to seek additional relevant electoral knowledge.[6]

The survey also contained a political quiz with ten questions that assess respondent knowledge about politics and political institutions. A large literature examines the proposition that informed voters are essential for elected officials to be properly accountable for the effects of their actions on voter well-being.[7] However, it is challenging to identify *a priori* the information voters believe they need to make electoral decisions and studies generally find ambiguous or weak effects of voting eligibility on general political knowledge. In their examination of compulsory voting in Brazil, Lopez de Leon and Rizzi (2014) find no differences between respondents subject to compulsory and those subject to voluntary voting requirements with respect to political knowledge of candidate party affiliation, policies implemented by the candidates, or comprehension of the ideological political spectrum.[8]

Four questions in the survey asked for basic facts about the political and electoral system in Mexico; they have no partisan or issue content. Since they have no obvious bearing on the selection of a preferred candidate for the upcoming election, there is no reason to expect the more politically engaged - those eligible to vote - to know more about them. Respondents were asked, how long is the president's term in office (94% answered correctly); how many votes are needed to elect the president (77% answered correctly, more votes than any other candidate, but another nine percent answered "more than half"); how long is a deputy's term (51% answered correctly); and how many deputies are there in the lower house (35% answered

[6]Bidwell, Casey and Glennerster (2020) present evidence of significant effects of candidate debates on voter and party decision-making.

[7]It may be necessary for only a small fraction of voters to be informed in order to protect the welfare of all voters (Martinelli 2007). Ashworth and Bueno de Mequita (2014) observe that politicians take voter information into account and present theoretical cases in which greater information can yield behavior that reduces voter welfare; a substantial empirical literature reveals disproportionately positive or null effects, however.

[8]On the other hand, a growing literature estimates significant effects of voter information on elections and politician accountability (see, e.g., Ferraz and Finan 2008, Fujiwara and Wantchekon 2013 and Cruz, Keefer, and Labonne 2021). In these settings, however, the information is not self-acquired but provided by researchers and directly or indirectly allows voters to evaluate incumbent performance.

correctly). Two questions were largely institutional, but with partisan content that could be electorally relevant: who is the current president of Mexico (99% answered correctly); and the party with the largest number of seats in Congress (83% answered correctly). One question has no partisan content, but asks for institutional knowledge about how decisions are made surrounding a specific and prominent concern of public policy in Mexico: if the President wanted to increase public spending to combat drug trafficking, which powers of government would have to approve it (33% answered correctly).

Three questions are specifically relevant for the election. To the extent that the politically engaged find them useful for identifying preferred candidates, they should be answered more correctly by eligible than ineligible respondents. One of these has only partisan content, but no relevance to respondent efforts to discern candidate differences on issues that concern them: who is the PRI's candidate for president in the next elections (91% answered correctly).

The other two have both partisan and issue content and are more relevant for respondent efforts to detect differences among candidates with respect to their public policy stances. A search of newspaper websites and party programs for issue-specific promises by the candidates yielded many references to promises to reduce crime or increase jobs that were common across candidates. There were few specifics that distinguished candidates, however. In the end, only discussions of old age pensions and public transport access yielded information that differed across candidates and could have been known by respondents. We asked respondents which of the major candidates advocated increasing old-age pensions and which increased access to public transport. A large fraction of respondents correctly identified the candidate who advocated an increase in old age pensions (70%) and a smaller fraction the candidate who advocated increased access to public transport (24%).

We divide the political knowledge questions into two categories and average responses within each group. The first category consists of those that ask about Mexican political institutions and have no issue or partisan content that might inform eligible students' electoral decision. Figure 3 indicates that eligible voters score more highly, but not significantly. The second category contains questions that have some partisan or issue content that might be relevant to their decision. The difference between eligible and non-eligible voters is statistically significant.[9]

[9] The first index, for "General questions", averages the responses to questions about the presidential term length, the number of votes to win the election, the congressional term length and the number of deputies. The second index, for "Election-relevant questions", averages the responses to questions about the current president's name and the party with most seats (at the time of the election), the incumbent party's candidate,

3 Regression Discontinuity Analysis

We take advantage of the voting age requirement that a Mexican citizen must be at least 18 years old on the day of the election in order to be eligible to vote. With age as the running variable, there is a sharp discontinuity in voting eligibility at age 18 where the probability of being eligible jumps from zero to one.

3.1 Non-Parametric Plot Analysis

We first examine the raw data to detect any discontinuity in political engagement outcomes at the age threshold, without any specific assumption about the functional relationship between political engagement and age. Panels (a) to (h) of Figure 4 present visual evidence of the discontinuity at the eligibility cutoff in the conditional mean function of various measures of political engagement. Just-eligible respondents express significantly more interest in watching the third and final presidential debate (panel (a)), more interest in the result (panel (b)), and in politics (panel (c)). Just-eligible respondents are also more likely to discuss politics (panel (d)), though the discontinuity is less pronounced, and to attend rallies (panel (e)). The just-eligible are not more likely to have watched the first two presidential debates, nor are their knowledge scores significantly different (panels (f), (f), and (h)). However, the initial two debates may have had more entertainment value – greater novelty factor, for example – than the third debate, even for ineligible students.[10]

3.2 Regression Discontinuity Specifications

We estimate the impact of eligibility on political engagement using a regression discontinuity design using exact dates of birth to identify a sharp discontinuity between eligible and ineligible respondents.[11]

the budget approval process, the name of the candidate that proposes to increase pension benefits, and the name of the candidate that proposes to increase access to public transit.

[10] An examination of the political quiz questions one-by-one reveals that eligible voters were significantly more likely to know the name of the PRI candidate for president ($p = .07$), and very significantly (five percentage points, $p < .001$) more likely to answer correctly the question about budget decisions regarding efforts to combat narcotics trafficking.

[11] The survey instrument contained the date-of-birth question both at the beginning of the survey and at the very end, to detect any misreporting. In case of mismatch in answers between the first and second question, the respondent is given another chance to report their date of birth. Mismatches are rare and mostly take the form of typing mistakes.

The relationship between political engagement and voting eligibility is given by:

$$Y_{ijt} = \alpha + \beta_k Elig_{ijt} + f\left(Age^T_{ijt} - k\right) + \lambda' \mathbf{X}_{ij} + \mu_{jt} + \epsilon_{ijt} \tag{1}$$

where i = student, j = campus, t = survey date, T = election date, k = age cutoff, Age^T_{ijt} = age on election day, $Elig_{ijt} = \mathbb{I}\{Age^T_{ijt} \geq k\}$, and ϵ_{ijt} = error term. Some specifications control for covariates $\mathbf{X}_{ij}$, summarized in Appendix Table A.1. The local average treatment effect is then given by:

$$\beta_k = \lim_{Age^T_{ijt} \to k^+} \mathbb{E}\left(Y_{ijt} \mid Age^T_{ijt}\right) - \lim_{Age^T_{ijt} \to k^-} \mathbb{E}\left(Y_{ijt} \mid Age^T_{ijt}\right) \tag{2}$$

where $k = 18$ and $T =$01jul2018.

The local average treatment effect of voting eligibility is well-identified if three assumptions hold. First, the cutoff rule for the running variable must not be manipulated. This assumption would be violated if voting eligibility status could be altered by misreporting age to electoral authorities. The rules governing elections in Mexico indicate that the assumption is unlikely to be violated, however, since the law requires that eligible individuals present a photo ID issued by the National Elections Institute (INE) as a condition of voting.[12]

Figure 5 reports results from the Cattaneo, et al. (2020) and McCrary (2008) density tests. Both reject the null hypothesis that individuals are systematically more likely to misreport their ages either above or below the eligibility cutoff. The second assumption, that potential outcomes are continuous in age at the cutoff, is strictly speaking untestable as potential outcomes are only partially observed.

The third assumption is that there must be no other treatments that affect political engagement at the cutoff. This assumption would not hold if the cutoff age of 18 also exposed young adults in Mexico to other significant legal and social changes that might influence their political engagement. Two such changes in other countries, automatic registration to vote and obligatory voting, are not present in Mexico. Other changes exist, but are minor. The legal right to consume alcohol begins at age 18 but is hardly likely to affect political engagement one to 1.5 months before the election. Eighteen is also the age of military conscription in Mexico, but is widely disregarded; there are no penalties associated with failure to register for conscription.

A central issue in RDD estimations is that the true functional form of the relationship

[12] See https://listanominal.ine.mx/scpln/.

between the running variable and the outcome is typically unknown, but an incorrect specification can introduce bias that yields spurious evidence of impact. To attenuate this problem, researchers narrow the bandwidths around the cutpoint, excluding observations outside of the bandwidths, sacrificing precision to reduce bias. They emphasize estimates that rely on optimal bandwidths derived from algorithms such as that proposed by Calonico, Cattaneo, and Titiunik (2014) (CCT).[13] In the discussion below, we present plots of the effects of eligibility at CCT optimal bandwidths and display the sensitivity of the eligibility effects to the choice of bandwidth.

Prior research on the engagement effects of voting eligibility is agnostic about the theoretical relevance of the tradeoffs between precision and bias in the context of the political engagement of first-time voters. However, at any time prior to the election, all first-time eligible voters have similar incentives to engage politically, regardless of whether they have been eligible for many months or just a few. Hence, there is no obvious theoretical relationship between the duration of their first-time eligibility — whether six months or two years before the election at which they will be first eligible to vote — and their incentive to be engaged in the weeks prior to the election. In theory, therefore, the loss of precision from adopting narrow bandwidths, excluding older first-time eligible voters, is likely to be large relative to the reduction in bias. We therefore also report estimates with substantially wider bandwidths than those based on the CCT algorithm.[14]

3.3 Regression Discontinuity Results

Figure 7 summarizes the effects of eligibility to vote on the eight measures of engagement, utilizing CCT bandwidths. The optimal bandwidths range from 130 to 140 days (for rallies, discussing politics, and general political knowledge), to 160 days (watching the first two debates) and approximately 180 days (interest in watching the final debate) to 205 to

[13]Horiuchi et al. (2021) use optimal bandwidths and their multiples (one-half and twice the optimal bandwidths). The optimal bandwidths are small, fewer than 12 weeks. Holbein and Rangel (2020) adopt a similar approach, centered on optimal bandwidths and their multiples, from 20 to 360 days. Lopez de Leon and Rizzi (2014) use three bandwidths, 6, 9, 12 months. Bruce and Costa Lima (2019), who examine only one measure of political engagement, use six bandwidths, from 4 to 24 months, in addition to an optimal bandwidth.

[14]In contrast, test scores are often a running variable in education studies, where scores above a certain cutpoint trigger eligibility for educational opportunities that are foreclosed to those with scores below the cutpoint (see, e.g., the impact of financial aid on college acceptance, Van der Klaauw 2002). Test scores, though, tend to be systematically related to outcomes of interest. In electoral studies, vote margins are often the running variable in studies comparing politician behavior just above and below the winning margin (for example, the impact of incumbency on re-election, Lee 2008). Again, in this case, politician behavior tends to be related to vote margins.

220 days (interest in the result, election-relevant knowledge, and interest in politics). The linear estimates include covariates and campus-by-date fixed effects, and standard errors are clustered at the campus level.

At optimal bandwidths, interest in the last presidential debate, in the result of the presidential election and in politics, and participation in political discussions differ notably between respondents who are just eligible and just ineligible to vote. Participation in political rallies is the most costly of the engagement measures and is unaffected by eligibility. Even ineligible respondents were likely to have found at least one of the first two debates to have high entertainment value, independent of their engagement with politics. Eligibility to vote should therefore have little impact on whether respondents viewed the first two debates. Neither measure of political knowledge (general and more electorally salient) is affected by eligibility to vote, reflecting the difficulty of specifying the precise types of knowledge that politically engaged individuals are likely to seek out, an issue addressed by the second experiment.

For the four measures of engagement where more significant differences between eligible and ineligible respondents emerge, three questions arise. Does the impact of eligibility change depending on whether relevant covariates are taken into account to increase the precision of the estimates? Are estimated differences robust to alternative functional forms aimed at capturing a possible relationship between age and political engagement? And are results robust to a wide range of bandwidths?

Tables 2 and 3 present RDD estimates for each of the four engagement measures, with and without covariates, for linear and quadratic specifications. Estimates are provided for the CCT optimal bandwidth, which varies across measures of engagement, and two constant bandwidths, one narrow and one broad, 180 days (approx. 6 months) and 480 days (approx. 16 months).

None of the covariates should vary systematically with eligibility, but their inclusion may improve precision (see Appendix Table A.1 for summary statistics). One is the response to a question about parents' income, "During the last year, your family's income was more than enough; just enough; at times not enough; never enough." A second covariate controls for whether neither parent, only one, or both parents attended college. The estimates also control for religious observance, "How frequently do you attend religious services, weekly, a few times a year, rarely, or never." Controls also include respondents' self-reported academic performance, "Your current scores average 90-100%, 75-90%, 60-75%, 40-60%, 40%"? The estimates control for other factors, including campus-date fixed effects.

The eligibility coefficient in each table indicates the difference in political engagement between those just eligible and those just ineligible to vote. The estimates are measured in standard deviations and thus are comparable across outcomes. In the linear specification in Table 2, it is nearly always large and significant for all four measures across all bandwidths. Two of these are the most salient, least costly types of political engagement, interest in the final presidential debate and interest in the result of the election. The covariates have little effect on the coefficients. The quadratic specifications in Table 3 also point to a significant effect of eligibility on interest in the final debate. Moreover, in the quadratic specifications, even those eligibility coefficients estimated at optimal bandwidths that do not achieve statistical significance exhibit nearly the same magnitudes.[15]

Tables A.2 and A.3 in the Appendix report linear and quadratic results for the other four measures of engagement. Consistent with the earlier discussion, eligibility does not influence these outcomes. They are either less salient (measures of political knowledge that may be entirely unrelated to what students believe they need to know in order to choose a presidential candidate); more costly (attendance at political events); or interesting for both eligible and ineligible voters (watching the first two presidential debates).

Expressions of political interest may not match behavior. We collected ex-post evidence after the third presidential debate took place to verify that intentions are correlated with behavior. In the main survey, we asked the respondents if they would like to share their email addresses with us so that we could share the results of the survey and 3,731 did so (69% of the sample). We sent all of these respondents a follow-up questionnaire that simply asked if they had watched the final presidential debate to which 442 replied. Among this subset of respondents, Figure 6 demonstrates that those who indicated in the main survey that they intended to watch the debate were much more likely to respond that they actually did watch it. Of those who expressed no intention to watch the debate in the main survey, only 20 percent reported actually watching it in the follow-up survey; among those who expressed intentions to watch the final debate, 80 percent said they actually watched it.

The results reported in the tables and in Figure 7 are not sensitive to the particular choice of bandwidth. Figure 8 presents RDD eligibility effects across all bandwidths using the linear specification. Each figure presents the 95% confidence intervals around the estimates. The dotted vertical line indicates the CCT-optimal bandwidth. At nearly all bandwidths,

[15]Two types of engagement, interest in the final debate and in the election result, were asked after respondents participated in the first survey experiment, concerning the pivotal youth vote. Hence, for those two sets of regressions, we also include the treatment status of respondents in that experiment.

eligibility has a strong effect on interest in the last debate and in the election result, and a nearly significant effect at nearly all bandwidths on interest in politics and participation in political discussions.

Prior research does not examine measures of political engagement that are both as salient and as low cost as interest in presidential debates or in the result of a presidential election. Their inclusion in the analysis therefore gives us a greater ability to detect more subtle shifts in political engagement. Eligibility has a large and robustly significant effect on the most salient and cheapest forms of engagement. It has a less robust effect on less salient, but also cheap forms of engagement (political interest, political discussions with others).

Results from prior research are also consistent with the hierarchy of relevance that emerges in these findings. Prior work finds that just-eligible respondents exhibit more general political interest and engage in more political discussions with others and that the differences are moderately robust (Horiuchi, Katsumata, and Woodard 2021, Scartascini and Vlaicu 2018). At the other end of the salience spectrum, however, prior research yields little systematic evidence of eligibility effects on political knowledge.[16]

The estimates reported here are local effects, precisely estimating the effects of the right to vote on 18 year olds. However, younger voters are generally known to be less politically engaged. Hence, the estimates are likely to be a lower bound on the average effect of the right to vote for the entire population of eligible voters.

4 Randomized Survey Experiments

Two experiments embedded in the second half of the survey explore mechanisms behind the eligibility effects on engagement. One examines whether individuals' knowledge that their group's vote is likely to be pivotal, and therefore "worth more", inspires greater engagement, and whether those effects are stronger among the eligible. The other compares how respondents process new information about public safety and education in Mexico that is relevant for their preferences over candidates. We estimate treatment effects in both experiments using the following specification.[17]

[16] Although addressing a different question, the effect of mandatory voting on political engagement, Bruce and Costa Lima (2019) find a robust RDD effect on a specific and arguably low-cost outcome, namely watching the most popular television news program in Brazil.

[17] The randomization protocol for both experiments was simple random assignment: all respondents had an equal probability of being assigned to treatment. The assignments were computer-generated in the survey software Qualtrics.

$$Y_{ijt} = \alpha + \beta Treated_{ijt} + \lambda' \mathbf{X}_{ij} + \mu_{jt} + \epsilon_{ijt} \tag{3}$$

Some specifications include covariates $\mathbf{X}$: parents' college attendance; household income; respondent academic performance; religious observance; gender; date of birth; and interest in politics (asked of all respondents prior to the experiments). Estimates further control for campus-date fixed effects and standard errors are clustered by campus. Under random assignment, the average treatment effect is then given by:

$$\beta = \mathbb{E}\left(Y_{ijt} \mid Treat_{ijt} = 1\right) - \mathbb{E}\left(Y_{ijt} \mid Treat_{ijt} = 0\right) \tag{4}$$

A key question is whether there are heterogeneous effects across eligible and ineligible voters. The null hypothesis of no heterogeneous effect is $H_0 : \beta^{Elig} = \beta^{Nonelig}$.

The treatment effects are well-identified if potential outcomes are independent of treatment assignment; if assignment to the treatment group is excludable and has no effect on unobserved outcomes that might also affect measured outcomes; and if non-interference holds (the Stable Unit Treatment Value Assumption) – subjects are not affected by the treatment status of other subjects. The survey design ensures the validity of these assumptions. Subjects were randomly assigned to treatment or control groups by computer algorithm that was unrelated to their political engagement, ensuring independence. The outcome variables were collected immediately after the information treatments were applied, ensuring excludability; and students were not allowed to communicate during the survey, ensuring non-interference.

Across both experiments, treated and control respondents are nearly identical on observables. A table for covariate balance (Table 4) demonstrates that in both experiments, treated and control groups did not differ significantly with respect to parents' education, religious attendance, income, academic performance, age, and gender. There is also balance with respect to campus location. Those measures of political engagement derived from questions respondents answered prior to the survey experiments also exhibit substantial balance. Interest in politics and whether they discussed politics or watched debates did not differ significantly between the treatment and control groups. Imbalance only emerges with respect to rally attendance, the least common form of engagement, and then only in the second, policy experiment.

4.1 Results: Pivotal Voters Treatment

The first experiment treated randomly-assigned respondents with information indicating that young adults were likely to be pivotal voters in the upcoming election.

Treated respondents received the following information.

> *On Sunday July 1 of this year, Mexico will elect a new president. For a record number of young Mexicans - 12 million, including all those who will turn 18 on or before July 1 - this presidential election will be the first for which they have the right to vote. Several analysts predict that the youth vote will influence the outcome of the elections.*

This information tells respondents that the value of young people's vote is potentially high. Since only eligible respondents can vote, this information is more salient to them and they should therefore react more strongly to the intervention. To measure the impact of this information on respondent political engagement, we then asked all respondents two of the engagement questions discussed earlier: "In total, there will be three debates between the presidential candidates. Do you plan to watch the last one in about a month's time? [Yes, Probably, No]." Then they were asked, "How much do you care about the result of the presidential election? [A lot, Some, Little, None]."

Table 5 presents estimates of the pivotal voter treatment effects using two specifications for each of the two engagement indicators. One has no controls, the other includes the controls listed above. In all cases, treated respondents, having received information that the youth vote might be pivotal, expressed significantly more interest in both the third debate and the result of the election. The pivotal voter treatment increased by .10 standard deviations both interest in the final debate and interest in the final result. The magnitude of the estimates is approximately the same (just slightly smaller) as that of the eligibility coefficients estimated in Tables 2 and 3. The introduction of covariates has almost no effect on these average treatment effects.

Figure 9 shows that the average treatment effect is driven almost entirely by its impact on eligible respondents. They react more strongly to the pivotal voters treatment, consistent with their higher level of political engagement. Table 6 confirms this. The coefficients on the interaction terms measure the treatment effects within the eligible and non-eligible groups. Compared to the untreated non-eligible, the treated eligible respondents express between two and three times more interest in the final presidential debate and the result of the election than the treated non-eligible. The F-test reported in the row "Coeff Equality" rejects the equality of the two coefficients in the case of interest in the result. The coefficient on untreated eligible voters reaffirms the importance of eligibility itself as a source of intrinsic motivation to engage politically: they exhibit more interest in the debate and the result than the untreated non-eligible; the difference is statistically significant in specifications without covariates.

This experiment demonstrates that information about a group's pivotal role in an election effectively raises the value that individuals place on their vote and significantly increases expressions of political engagement, interest in presidential debates and in the results of the presidential election. This result has two possible and complementary interpretations.

One is that, informed that the value of their vote is higher, individuals are willing to engage more. Another is behavioral: informed that they and their peers will be influential, respondents feel social pressure to engage more. However, neither the text of the treatment nor supplemental evidence that we collected support this second interpretation.

First, the text of the treatment does not suggest any problems or express any concerns surrounding the political engagement of young people, but merely observes that at *prevailing* levels of political engagement by young people, their votes are likely to be pivotal. Second, there is no theoretical reason to expect susceptibility to social desirability bias to differ across eligible and ineligible respondents. Even if it does, such a difference is consistent with greater motivation to be politically engaged among eligible voters. In this case, however, the motivation is external and not intrinsic. Third, our follow-up survey indicates that those who expressed interest in watching the third debate also actually watched it. It is unlikely that social desirability bias accounts for actual viewing behavior. If it did, it would have had to be strong enough not only to lead respondents to misreport their interest on a survey that they were asked to complete in their classroom, but also to misreport their actual behavior on a survey outside of the classroom that they were under no compulsion to complete.

4.2 Results: Policy Information Treatment

The second experiment explores whether individuals use new information about policy outcomes in Mexico to update their policy priorities for the next president. These priorities are salient since they enter into the criteria respondents use to form their preferences over candidates. We investigate whether eligible and ineligible respondents differ in how they use information about policy outcomes shortly before the election.

The cognitive task that confronts voters is to identify which candidate characteristics and issue stances they care most about, collect information about those characteristics and stances, and determine which candidates align most closely with respondents' own preferences.

Politically engaged individuals should value this effort more than the less engaged, and all should value this effort more highly for issues that are more salient. As the survey was fielded weeks before the election, eligible respondents were likely to have *already* made cognitive investments in identifying their preferred candidates. Sinclair and Plott (2012) find that polls are more accurate in the weeks before an election, when engaged individuals have already made the cognitive investment required to identify their preferred candidate. For individuals who have already made the cognitive investment in processing information, as we discuss below, the marginal returns to additional investment are lower. Prior research reviewed earlier also concludes that the marginal value of exerting effort in collecting and processing new information declines with previous effort.[18]

For two reasons, therefore, eligible respondents, who have already made the effort to acquire and process electorally relevant information prior to participating in our survey, are likely to be less responsive to the treatment. First, they are more likely to have already seen the information. Second, they should be less willing to undertake additional cognitive effort to process new information, since prior research suggests they anticipate that such effort is less likely to lead them to change the candidate preferences that they have already formed. In fact, results from the second experiment indicate that eligible voters are *less* sensitive to information about policy outcomes.

In the second experiment, randomly selected respondents received the following information treatment:

> *Many know that last year, crime and violence levels in Mexico were higher than in any of the last ten years. It is less well known that for many years, according to international rankings, Mexico has lagged behind similar countries, such as Argentina, Brazil, and Chile, in the quality of university education.*

All respondents were then asked,"In your opinion, how much priority should the next President give to the following problems? Crime and violence, College education quality, Low incomes, Corruption, and Environmental pollution." The order of problems was randomized across respondents. For each problem, respondents assigned a priority, Highest,

[18]This latter conclusion emerges, for example, from Steiner, Stewart and Matějka (2017). They demonstrate that when information is costly, agents' beliefs are sticky over time and linked in one period to beliefs in earlier periods.

High, Medium, Low, Lowest, without forcing a ranking among alternatives. We expect that, on average, treated respondents will assign a higher weight to crime and education than control respondents. However, to the extent that eligible respondents have already undertaken the cognitive effort of choosing the policy criteria for identifying preferred candidates, and have actually formulated a candidate preference on the basis of those criteria, the marginal value of the information we provide to treated respondents may be lower for them than for ineligible respondents.

The treatment provides information on two problems, crime and university education. We therefore compare the priority given by respondents to these two issues compared to the priority they give to the other issues. Since we specifically care about the ordinal ranking that they give to crime and education, we construct a relative score by subtracting the priority given to other issues from the priority given to crime and education.[19]

Specifically, we calculate the average priority the respondents assign to crime and education (yielding, for example, three if they give crime the highest priority of five and education the lowest priority of one), and subtract the average priority they attach to the other three issues, corruption, the environment, and poverty (again, summing priorities across the three issues and dividing by three). We then examine the robustness of the result, comparing the average priority given to crime and education relative only to corruption; only to pollution; and only to poverty.

Table 7 presents evidence of the average treatment effect. Regressions with covariates include the same covariates as those in the previous table reporting the results of the pivotal voters experiment.[20] The results reported in Table 7 confirm that information on Mexican performance on crime and university education has a significant average effect on policy priorities, across all four measures of the relative priority attached to crime and education. Treatment impacts are nearly invariant to controls for covariates. The relative priority assigned by treated respondents to crime and education was about .12 standard deviations higher than the relative priority assigned by untreated respondents.

[19] Respondents could react to the treatment by reducing the priorities they assign to pollution, corruption and poverty, or by increasing the priorities they assign to crime and education. Hence, the priorities assigned to crime and education *relative* to other issues is most relevant for identifying a potential treatment effect.

[20] Estimates also control for treatment status in the pivotal voter experiment, which preceded the information experiment, recalling that all respondents participated in both experiments. Since respondents were re-randomized prior to the second experiment, treatment status in the first experiment is balanced across treatment and control groups in the second. We therefore expect no effect of treatment status in the first experiment on treatment effects in the second. Consistent with this, treatment status in the pivotal experiment is small and statistically insignificant in estimates of information treatment effects in the the second experiment.

Figure 10 reveals substantial heterogeneity in treatment impacts across eligible and non-eligible respondents. The eligible are significantly less likely to change their policy priorities in reaction to the treatment than non-eligible respondents. Table 8 reports interaction coefficients that estimate the treatment effects separately within the eligible and non-eligible groups. The impact of the information treatment on eligible respondents is less than half as large as on ineligible respondents. Only for poverty is there no difference in treatment impact across eligible and ineligible voters. The F-tests reported in the "Coeff Equality" row indicate that the difference is nearly always statistically significant and consistent with the conjecture that eligible respondents place a lower marginal value on additional information, having already invested cognitive effort in identifying their preferred candidates.

Covariate estimates, although non-experimental, are revealing in their own right and reported in the Appendix Table A.4. Respondents for whom the cognitive burden of processing the information is lower — those with higher average academic marks — assigned a higher priority to crime and education; those reporting more comfortable economic circumstances and parents with university education assigned a lower priority to crime and education. The differences associated with gender are the largest, twice as large as any other: male respondents assigned a much lower priority to crime and education than female respondents.[21]

We further explore mechanisms by exploiting the difference in salience between crime and education and separately examining the relative priorities attached to crime and education by treated and control respondents. Theory points to two offsetting effects of salience. On the one hand, respondents should be more responsive to information on more salient issues, increasing the treatment effect on the priority attached to the more relative to less salient issue. On the other hand, respondents, particularly eligible respondents, are more likely to have been previously exposed to information on the more salient issue, reducing the treatment effect on the priority attached to the more salient issue.

Crime is significantly more salient in Mexico than education, even for the young. Among young respondents (15 to 25 years old) in the 2021 *Latinobarómetro* survey, 11.4% indicated that crime was the principal problem of the country; only 4.7% pointed to education. This difference is also reflected in media coverage, which is greater for crime.[22] Although we do not have crime-only and education-only treatment groups, we can compare the effects of

[21]Male and female respondents are equally responsive to the information treatment, however.

[22]A search for articles about "crime policy" (*política crimen* or *política delincuencia*) in one major Mexican newspaper, *El Universal*, yields 748 and 522 articles, respectively, in 2018. A search for *política universitaria* yields only 155. For *política delincuencia* the search was `https://www.eluniversal.com.mx/resultados-busqueda/politica20delincuencia`, February 8, 2022.

the joint provision of crime and education information on the relative priorities attached to crime and education individually.

Two Appendix tables focus separately on the effects of the treatment on the relative priority attached to crime and to education. Results are consistent with both greater respondent knowledge of the more salient issue, crime, and a willingness to incorporate new information about the less salient policy area, education, into their electoral calculations. Table A.5 reports the effects of eligibility and treatment on the priority given to crime relative to other policies and Table A.6 does the same for education. Consistent with the incentives of eligible respondents to collect information on more salient issues prior to the survey, the interaction terms in the two tables indicate that compared to the non-eligible, the eligible were significantly *less* affected by the information treatment, and more so in the priority they attached to crime (Table A.5) than in the priority they attached to education (Table A.6). The main treatment ("Policy Treat") coefficients in the crime table indicate the effect of the policy information on non-eligible respondents. The coefficients are large and highly significant in the education table, but less so in the crime table, indicating that non-eligible respondents were also more likely to have been exposed to information on the more salient issue prior to the survey.

In sum, the two tables indicate that for both crime and education, the eligible respondents are less responsive to the treatment, consistent with the greater investments that eligible respondents had already made in determining their electoral preferences. The difference is greatest for more salient policies (crime): eligible respondents had either already been exposed to this information on the more salient crime outcomes, or they had already made the cognitive effort of deciding whether the more salient issue of crime was a priority for the next president. In contrast, the treatment *did* shift the priorities of eligible voters with respect to education, consistent with them having expended less effort exploring this less salient issue prior to the election. The relative priority assigned to education by treated eligible respondents is 0.10 standard deviations greater than the relative priority assigned by untreated eligible respondents, significant at less than one percent. The effect on eligible respondents was still lower in magnitude than the treatment effect on ineligible respondents, consistent with eligible respondents´reluctance to update. However, given new information about a policy issue that they had not previously prioritized, eligible respondents were willing to significantly update their policy priorities even three weeks before the election, adding a new dimension to the cognitive processes of voters that extends beyond the analysis in, for example, Steiner, Stewart and Matějka (2017).

5 Conclusion

Evidence from an original survey of Mexican high school seniors sheds light on key issues in the study of voter behavior. One is the effect of voter eligibility on political engagement. Evidence from mature democracies reveals weak effects. We find, in contrast, that eligibility increases the intrinsic motivation to politically engage of individuals in younger democracies. Our empirical setting offers an unusual opportunity to identify effects of eligibility on engagement. Apart from a sample of respondents that allows us to credibly satisfy the conditions for a sharp RDD, the analysis takes advantage of the timing of a consequential general election and of data that were collected in precisely the period during which differences between eligible and ineligible respondents should be most evident, in the few weeks before the election. In addition, we examine new types of political engagement that vary in their electoral salience and their cost to voters. The most robust differences emerge for the two that are most salient and least costly, but which prior research has not directly investigated: interest in the current election and in the final debate of the presidential candidates.

Another key issue is the extent to which the free-rider problem reduces intrinsic motivation to engage politically. Evidence on the magnitude of the free-rider effect is scarce. Our experiment reveals that respondents are highly sensitive to the free rider problem and react significantly to information that their peer group could be pivotal in the elections. This effect is driven by eligible voters, precisely as one would expect to be among the more motivated individuals for whom the value of the vote is more salient.

Finally, a key aspect of political engagement is the willingness to exert effort to collect information to inform electoral choices. Previous research has relied on knowledge tests such as those that we report in our own analysis and, as in our analysis, found little effect of eligibility on political knowledge. However, these tests presume that the researchers know which issues are most salient to prospective voters, as well as when these voters are most motivated to exert effort to collect and process this information. Unfortunately, researchers are rarely knowledgeable about either of these dimensions.

We advance understanding of this issue by providing respondents with information about policy outcomes and observe whether this leads them to update policy priorities in the period before elections. Ideally, all individuals would respond to new information about policy outcomes. However, eligible respondents appear to have largely made the cognitive investment in identifying the issues which will guide their electoral choices. They are *less* responsive to this treatment than ineligible respondents. Suggestive evidence supports two possible explanations for this. Eligible respondents may have already informed themselves about salient facts, and they may be more reluctant to change their policy priorities after having already invested in identifying their preferred candidate. However, when presented with new information about a policy issue they had not previously considered, even eligible respondents appear to be willing to update their policy priorities, if not by as much as ineligible respondents.

It is this last mechanism to which future research should turn. Substantial debate concerns the nature of information markets and the willingness of individuals to seek out or even pay attention to information that conflicts with their prior beliefs. We find significantly less willingness of eligible students to adjust their policy priorities in response to information. However, preliminary evidence points to a fruitful target for future research, to disentangle what variations in novelty and salience can trigger greater adjustments, and how these interact with the timing of information acquisition and processing by intrinsically motivated voters.

Figures and Tables

1. Figures

Figure 1: Data collection timeline

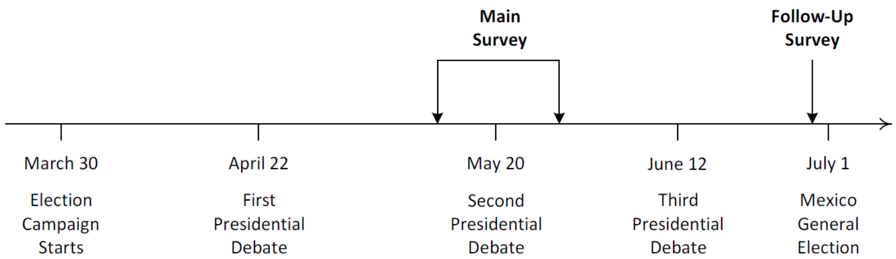
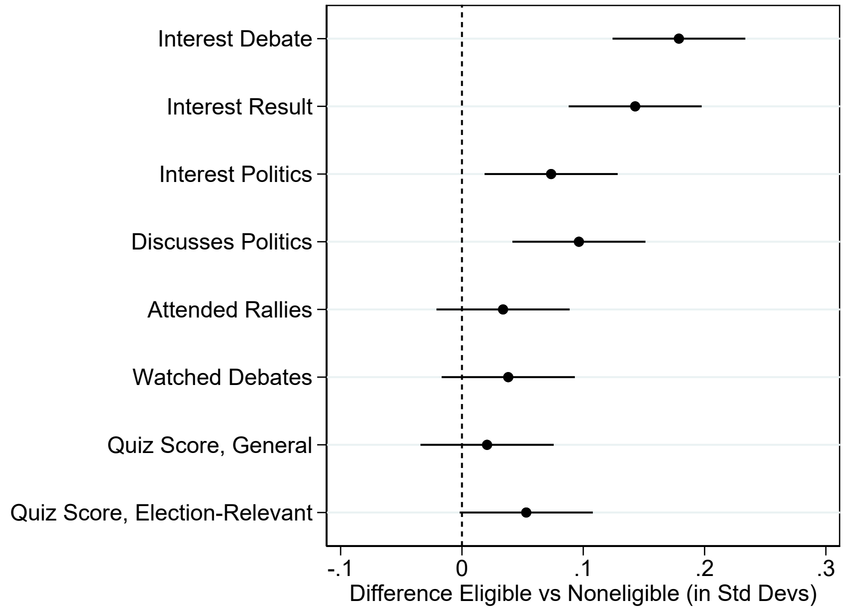


Figure 2: Spatial distribution of the participating campuses

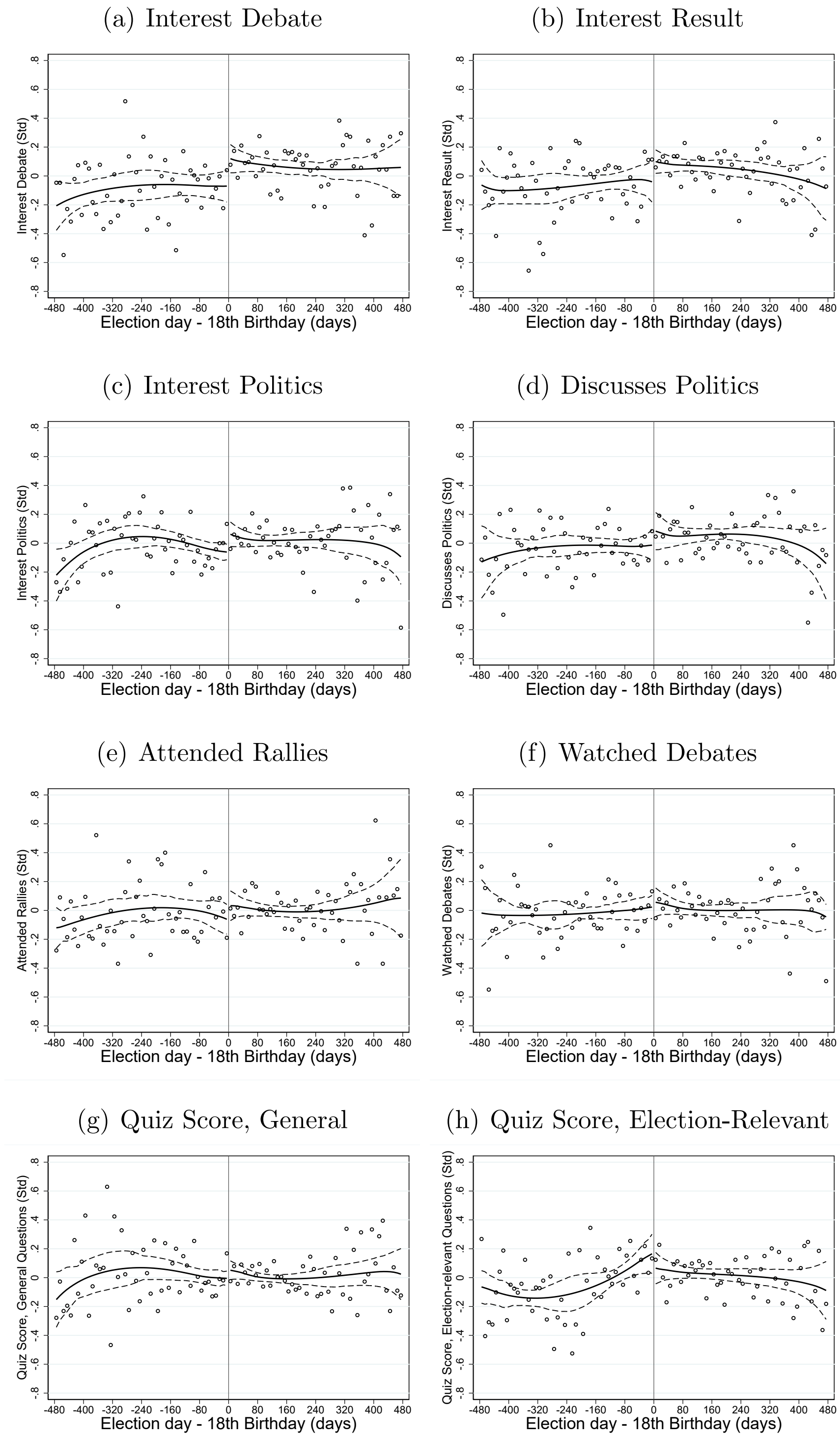
Notes: This figure presents the geographical distribution of the 30 Prepa UVM campuses that participated in the surveys and experiments. Table 1 presents the number of students per campus in the main survey.

Figure 3: Mean differences between eligible and non-eligible students



Notes: This figure plots the difference in means between eligible and non-eligible participants for eight outcomes of political engagement, all standardized. We report the point estimate and 95% Confidence Intervals.

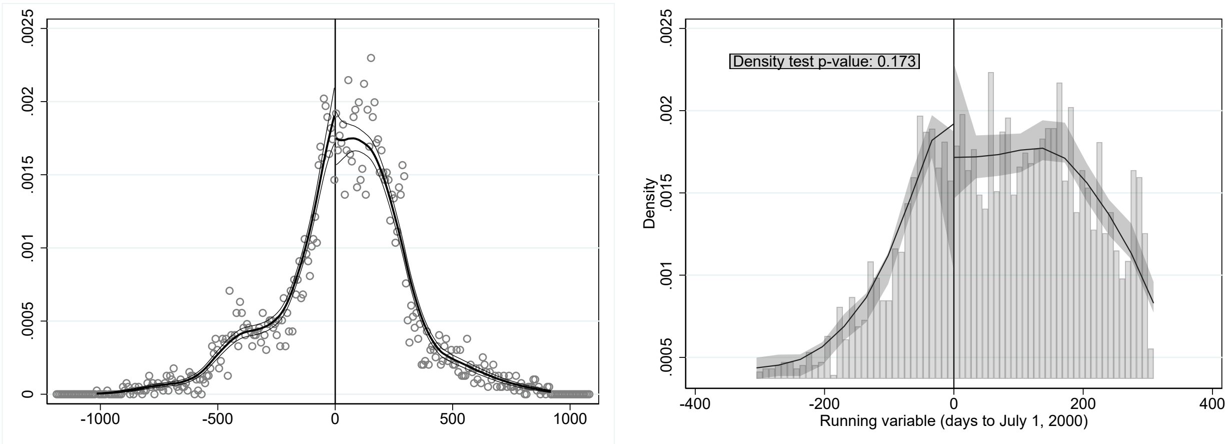
Figure 4: Non-parametric plots - political engagement and voting age



Notes: These figures present the RD non-parametric plots for variables on political engagement. The dots represent the average of each dependent variable on bins for the number of days to the 18th birthday from the date of the election, whereas the solid black lines are the smoothed values of Kernel-weighted local polynomial regressions of the binned scatter plots data. The dashed lines are the 95% confidence intervals of the smoothed values. All dependent variables are standardized.

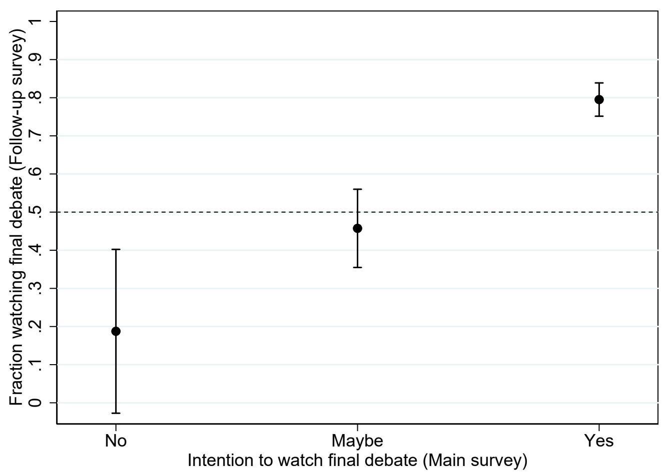
Figure 5: Threshold manipulation tests

(a) McCrary (2008) test (b) Cattaneo, Jansson & Ma (2020) test



Notes: These figures present the graphical results of two threshold manipulation tests for the running variable, which is the difference between the date of birth and July 1, 2000. Panel (a) presents the McCrary (2008) test, which results in an estimate of the discontinuity of -0.954 (and a standard error of 0.7734). Panel (b) plots the Cattaneo, Jansson, and Ma (2020) test where we assume a local quadratic approximation. We also report the p-value for the bias-corrected density test.

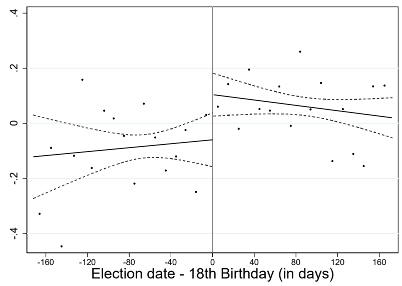
Figure 6: Intentions vs actual watching of final debate



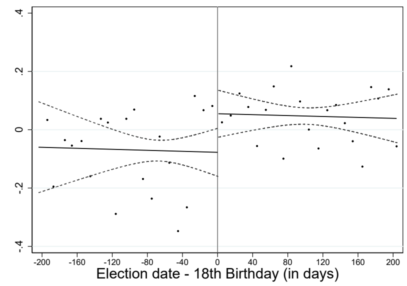
Notes: This figure presents the fraction of respondents that reported watching the third presidential debate in the follow-up survey, by intention of watching it in the main survey. The main survey was collected between May 14 and 28, 2018. The third presidential debate happened on 12 June. The follow-up survey went out on 29 June, and was completed by 442 students. We report point estimates and 95% confidence intervals.

Figure 7: Linear RD plots at optimal bandwidth

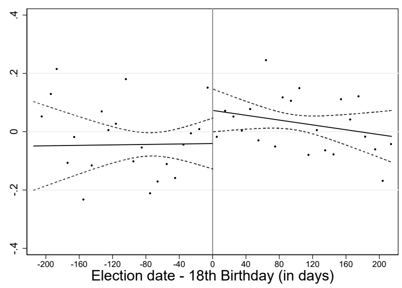
(a) Interest Debate



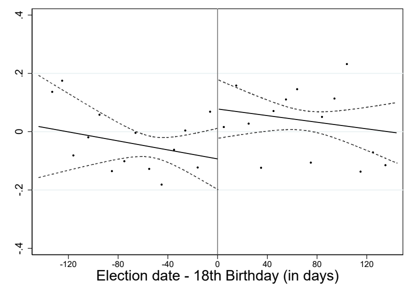
(b) Interest Result



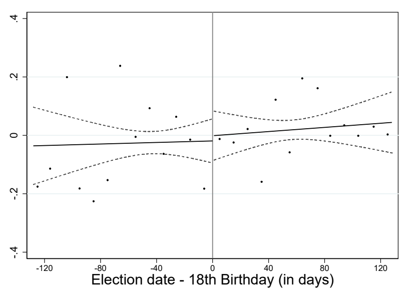
(c) Interest Politics



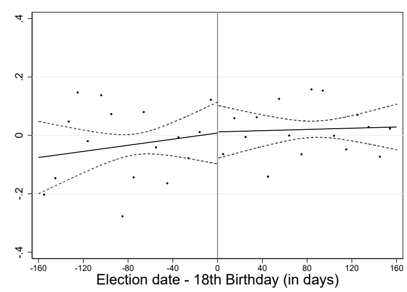
(d) Discusses Politics



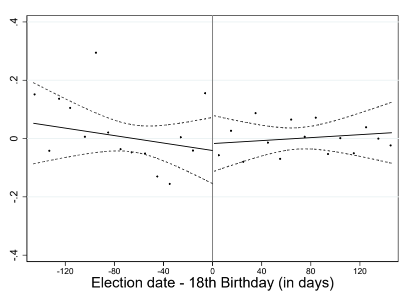
(e) Attended Rallies



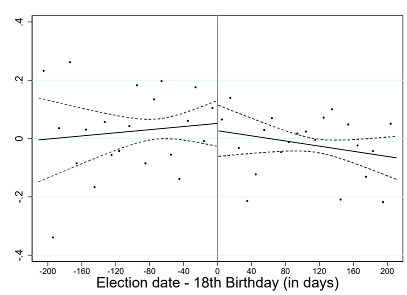
(f) Watched Debates



(g) Quiz Score, General



(h) Quiz Score, Election-Relevant

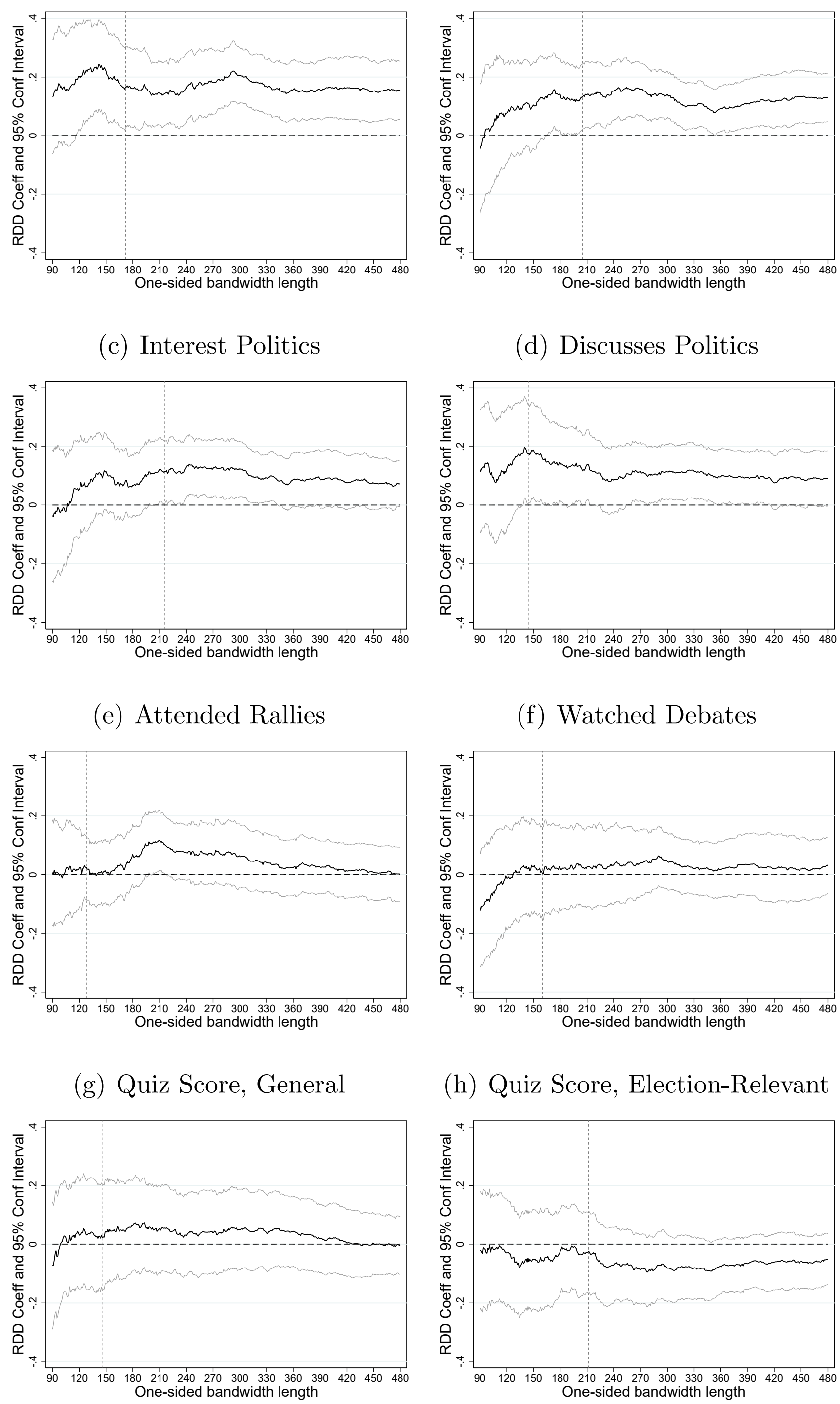


Notes: These figures present the RD plots of the effect of being eligible to vote on political engagement variables within the Calonico, Cattaneo, and Titiunik (CCT) optimal bandwidths. The dots represent the averages of the dependent variable in 10-day-length bins of the running variable, whereas the solid black lines are the predicted values of a 1st degree RD model ($y_i = \alpha + \beta_1 Eligible + \beta_2 RunningVar + \beta_3 RunningVar \times Eligible + \varepsilon_i$). The dash lines are the 95% confidence intervals of the predicted values. The running variable is the number of days to the 18th birthday. All dependent variables are standardized.

Figure 8: RD estimates: sensitivity to bandwidth length

(a) Interest Debate

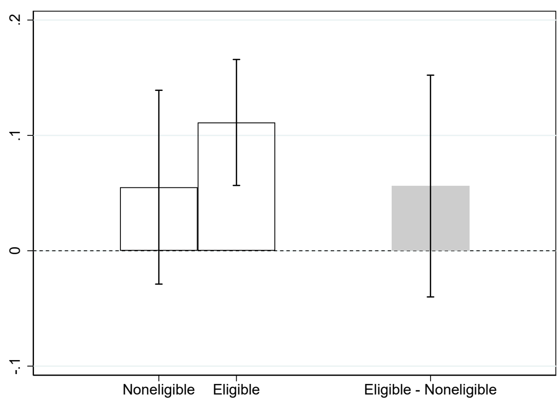
(b) Interest Result



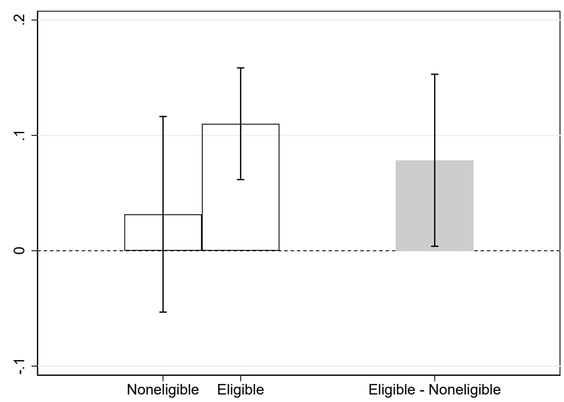
Notes: These figures present the RD coefficients of being eligible to vote on the outcomes of interest, to different one-side bandwidths from the cutoff (days to birthday). The solid black lines denote the coefficients, whereas the solid grey lines are their 95% confidence intervals. The dotted grey lines represent the CCT optimal bandwidths. All dependent variables are standardized.

Figure 9: Heterogeneous treatment effects in the pivotal voters experiment

(a) Interest Debate



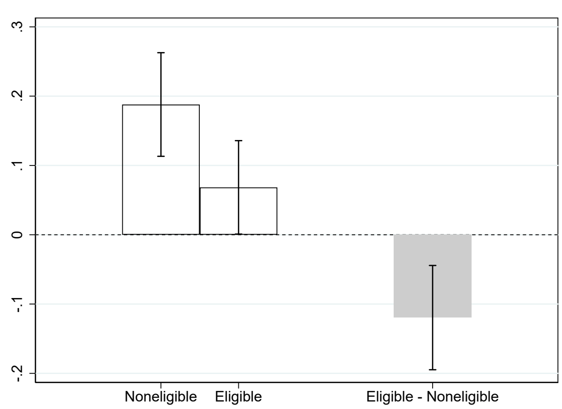
(b) Interest Result



Notes: These figures present the effect of being treated in the pivotal voters experiment on the two outcomes of interest by voting eligibility status. The white bar plots represent the coefficient of a treatment dummy when regressing the outcomes separately for non-eligible and eligible to vote participants, and the grey bar is the difference between the coefficient for eligible and the coefficients for non-ineligible. The spiked lines plot the 95% confidence intervals for the estimates. All regressions control for interest in politics, sex, parents education, religious attendance and academic performance. All dependent variables are standardized.

Figure 10: Heterogeneous treatment effects in the policy information experiment

(a) Crime and education relative to all



Notes: This figure presents the estimate of the effect of being treated in the policy information experiment on the principal outcome of interest: $(Crime + Educ)/2 - (Pollution + Corruption + Poverty)/3$. The white bar plots represent the coefficient of a treatment dummy when regressing the outcome separately for non-eligible and eligible to vote participants, and the grey bar is the difference between the coefficient for eligible and the coefficients for non-ineligible. The spiked lines plot the 95% confidence intervals for the estimates. All regressions control for interest in politics, sex, parents education, religious attendance and academic performance. The dependent variable is standardized.

2. Tables

Table 1: Distribution of students across campuses

	Students	Percentage
Aguascalientes	103	1.93
Chihuahua	154	2.89
Ciudad Victoria	97	1.82
Coyoacán	364	6.84
Cuernavaca	125	2.35
Guadalajara Norte	267	5.02
Guadalajara Sur	299	5.62
Hermosillo	114	2.14
Lago de Guadalupe	95	1.78
Lomas Verdes	183	3.44
Mexicali	199	3.74
Monterrey Cumbres	225	4.23
Monterrey Norte	233	4.38
Mérida	171	3.21
Nogales	65	1.22
Puebla	113	2.12
Querétaro	251	4.71
Reynosa	81	1.52
Roma	214	4.02
Saltillo	116	2.18
San Luis Potosí	68	1.28
Tampico	125	2.35
Texcoco	89	1.67
Tlalpan	219	4.11
Toluca	212	3.98
Torreón	87	1.63
Tuxtla	160	3.01
Veracruz	215	4.04
Villahermosa	169	3.17
Zapopan	511	9.60
Total	5324	100.00

Table 2: Regression Discontinuity Estimates - Linear Fit

	Interest Debate			Interest Result			Interest Politics			Discusses Politics		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Baseline												
Eligible	0.138**	0.155**	0.152***	0.115	0.134**	0.135***	0.077	0.073	0.074*	0.129*	0.140**	0.101**
	(0.055)	(0.066)	(0.052)	(0.073)	(0.061)	(0.043)	(0.058)	(0.051)	(0.039)	(0.065)	(0.064)	(0.047)
Observations	3,280	2,880	4,846	2,556	2,874	4,838	2,669	2,883	4,850	2,942	2,883	4,850
Bandwidth (days)	215	180	480	156	180	480	163	180	480	183	180	480
Panel B: Baseline + Covariates												
Eligible	0.166**	0.160**	0.152***	0.133**	0.141**	0.131***	0.114**	0.068	0.074*	0.176**	0.144**	0.091*
	(0.069)	(0.067)	(0.051)	(0.057)	(0.062)	(0.042)	(0.055)	(0.053)	(0.040)	(0.086)	(0.065)	(0.048)
Observations	2,784	2,879	4,844	3,166	2,873	4,836	3,279	2,879	4,844	2,383	2,879	4,844
Bandwidth (days)	172	180	480	205	180	480	216	180	480	145	180	480

Notes: This table presents the estimates for the effect of being eligible to vote on the main political engagement outcomes (β in Equation 1), when $f(Age - k)$ is a first-degree polynomial. Six regressions are run for each dependent variable, which vary in bandwidth and inclusion of covariates. Columns (1), (4), (7), and (10) are models estimated with Calonico, Cattaneo, and Titiunik (2014) optimal bandwith. Regressions in Columns (2), (5), (8), and (11) are estimated in respondents younger or older than the voting age by no more than 180 days, and those in Columns (3), (6), (9), and (12) for differences of 480 days or less. Panel (A) models only control for campus and date fixed effects, whereas Panel (B) estimates additionally control for sex, parents education, religious attendance and academic performance. Interest Debate and Interest Result models also control for treatment status in the pivotal voters experiment. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** is significant at the 5% level, and *** is significant at the 1% level.

Table 3: Regression Discontinuity Estimates - Quadratic Fit

	Interest Debate			Interest Result			Interest Politics			Discusses Politics		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Baseline												
Eligible	0.140	0.218*	0.182***	0.133	-0.039	0.085	-0.032	-0.003	0.103	0.113	0.098	0.107*
	(0.087)	(0.108)	(0.060)	(0.083)	(0.123)	(0.055)	(0.107)	(0.117)	(0.061)	(0.084)	(0.121)	(0.056)
Observations	3,894	2,880	4,846	4,125	2,874	4,838	3,064	2,883	4,850	4,176	2,883	4,850
Bandwidth (days)	280	180	480	308	180	480	196	180	480	316	180	480
Panel B: Baseline + Covariates												
Eligible	0.137	0.227**	0.189***	0.018	-0.026	0.093	0.010	0.011	0.110*	0.122	0.113	0.113*
	(0.085)	(0.108)	(0.059)	(0.097)	(0.122)	(0.055)	(0.119)	(0.120)	(0.062)	(0.099)	(0.124)	(0.058)
Observations	3,939	2,879	4,844	3,282	2,873	4,836	2,938	2,879	4,844	3,778	2,879	4,844
Bandwidth (days)	285	180	480	216	180	480	184	180	480	269	180	480

Notes: This table presents the estimates for the effect of being eligible to vote on the main political engagement outcomes (β in Equation 1), when $f(Age - k)$ is a second-degree polynomial. Six regressions are run for each dependent variable, which vary in bandwidth and inclusion of covariates. Columns (1), (4), (7), and (10) are models estimated with Calonico, Cattaneo, and Titiunik (2014) optimal bandwith. Regressions in Columns (2), (5), (8), and (11) are estimated in respondents younger or older than the voting age by no more than 180 days, and those in Columns (3), (6), (9), and (12) for differences of 480 days or less. Panel (A) models only control for campus and date fixed effects, whereas Panel (B) estimates additionally control for sex, parents education, religious attendance and academic performance. Interest Debate and Interest Result models also control for treatment status in the pivotal voters experiment. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** is significant at the 5% level, and *** is significant at the 1% level.

Table 4: Balance tests for randomized experiments

Variable	Overall Mean	Pivotal voters Control	Pivotal voters Treatment	Diff. p-value Voting	Policy information Control	Policy information Treatment	Diff. p-value Policy
Parents Educ	2.376	2.363	2.388	0.220	2.363	2.389	0.182
	(0.010)	(0.014)	(0.014)		(0.014)	(0.014)	
Religious Attendance	2.496	2.485	2.506	0.471	2.517	2.474	0.153
	(0.015)	(0.021)	(0.021)		(0.021)	(0.021)	
Income	3.169	3.164	3.174	0.596	3.157	3.181	0.215
	(0.010)	(0.014)	(0.014)		(0.014)	(0.014)	
Academic Perf	4.429	4.432	4.426	0.704	4.418	4.440	0.185
	(0.008)	(0.012)	(0.012)		(0.012)	(0.011)	
Age	17.483	17.477	17.488	0.628	17.487	17.479	0.718
	(0.011)	(0.016)	(0.016)		(0.016)	(0.016)	
Male	0.476	0.487	0.466	0.129	0.468	0.484	0.238
	(0.007)	(0.010)	(0.010)		(0.010)	(0.010)	
Campus longitude	-100.735	-100.781	-100.695	0.519	-100.630	-100.849	0.100
	(0.067)	(0.095)	(0.093)		(0.094)	(0.095)	
Campus latitude	21.828	21.813	21.847	0.736	21.780	21.883	0.314
	(0.051)	(0.073)	(0.071)		(0.071)	(0.074)	
Interest Politics	2.780	2.773	2.788	0.511	2.765	2.797	0.157
	(0.011)	(0.016)	(0.016)		(0.016)	(0.016)	
Watched Debates	1.831	1.832	1.830	0.886	1.830	1.832	0.886
	(0.005)	(0.007)	(0.007)		(0.007)	(0.007)	
Discusses Politics	3.472	3.460	3.484	0.212	3.461	3.484	0.259
	(0.010)	(0.014)	(0.014)		(0.014)	(0.014)	
Attended Rallies	1.223	1.214	1.232	0.289	1.238	1.208	0.064
	(0.008)	(0.011)	(0.012)		(0.012)	(0.011)	
Observations	5,324	2,613	2,708		2,681	2,639	

Table 5: Average treatment effect in pivotal voters experiment

	Interest Debate		Interest Result	
	(1)	(2)	(3)	(4)
Voting Treat	0.099***	0.091***	0.091***	0.080***
	(0.028)	(0.024)	(0.029)	(0.028)
Observations	5,320	5,317	5,312	5,309
Fixed Effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: This table presents the estimates for the effect of being treated in the pivotal voters experiment on the two main outcomes of political engagement (β in Equation 3). Columns 2 and 4 additionally control for voting eligibility status, interest in politics, sex, parents education, religious attendance and academic performance. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 6: Heterogeneous treatment effects in pivotal voters experiment

	Interest Debate		Interest Result	
	(1)	(2)	(3)	(4)
Voting Treat × Eligible	0.117***	0.110***	0.124***	0.112***
	(0.031)	(0.026)	(0.026)	(0.024)
Voting Treat × Noneligible	0.074	0.062	0.043	0.031
	(0.046)	(0.043)	(0.045)	(0.041)
Eligible	0.137**	0.097	0.081**	0.054
	(0.052)	(0.059)	(0.034)	(0.044)
Coeff Equality (p-value)	.417	.332	.0734	.0393
Observations	5,320	5,317	5,312	5,309
Fixed Effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: This table presents the estimates of the conditional average treatment effect of the pivotal voters experiment for two groups: the eligible and the non-eligible to vote. $Eligible - Noneligible$ represents the estimate of difference between the impacts of both groups. $Eligible - Noneligible$ (p-value) is the p-value of a test where the null hypothesis is that this difference is equal to 0. Columns 2 and 4 additionally control for interest in politics, sex, parents education, religious attendance and academic performance. Both dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 7: Average treatment effect in policy information experiment

	Crime & Educ Rel All		C&E Rel Corruption		C&E Rel Pollution		C&E Rel Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy Treat	0.121***	0.115***	0.118***	0.120***	0.056*	0.048	0.087**	0.082**
	(0.030)	(0.030)	(0.025)	(0.025)	(0.030)	(0.030)	(0.032)	(0.032)
Voting Treat	-0.051	-0.051	-0.023	-0.023	-0.017	-0.016	-0.068*	-0.067*
	(0.034)	(0.034)	(0.027)	(0.027)	(0.028)	(0.027)	(0.034)	(0.035)
Eligible	-0.037	-0.036	-0.038	-0.014	-0.044*	-0.027	0.004	-0.033
	(0.023)	(0.045)	(0.029)	(0.048)	(0.025)	(0.039)	(0.025)	(0.040)
Observations	5,317	5,317	5,317	5,317	5,317	5,317	5,317	5,317
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table presents the estimates for the effect of being treated in the policy information experiment on outcomes that represent the relative concern for corruption and education (those affected by the experiment) relative to other factors. The outcomes are created from these computations (respectively): $(Crime+Educ)/2-(Poverty+Corruption+Pollution)/3$, $(Crime+Educ)/2-Corruption$, $(Crime+Educ)/2-Pollution$, and $(Crime+Educ)/2-Poverty)$. All regressions control for voting eligibility status and for being treated in the first experiment, and columns 2, 4, and 6 additionally control for voting eligibility status, interest in politics, sex, parents education, religious attendance and academic performance. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 8: Heterogeneous treatment effects in policy information experiment

	Crime & Educ Rel All		C&E Rel Corruption		C&E Rel Pollution		C&E Rel Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy Treat × Eligible	0.045**	0.041**	0.059**	0.060**	0.014	0.006	0.063*	0.058*
	(0.019)	(0.019)	(0.026)	(0.026)	(0.027)	(0.026)	(0.032)	(0.030)
Policy Treat × Noneligible	0.106***	0.104***	0.127***	0.127***	0.102**	0.098**	0.089**	0.086**
	(0.020)	(0.021)	(0.019)	(0.019)	(0.039)	(0.039)	(0.038)	(0.039)
Eligible	0.010	0.011	0.006	0.024	0.006	0.022	0.017	-0.013
	(0.020)	(0.025)	(0.030)	(0.038)	(0.025)	(0.036)	(0.028)	(0.033)
Coeff Equality (p-value)	.0104	.00981	.0438	.0478	.0339	.0307	.553	.532
Observations	5,317	5,317	5,317	5,317	5,317	5,317	5,317	5,317
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table presents the estimates of the conditional average treatment effect of the policy information experiment for two groups: the eligible and the non-eligible to vote. $Eligible - Noneligible$ represents the estimate of difference between the impacts of both groups. $Eligible - Noneligible$ (p-value) is the p-value of a test where the null hypothesis is that this difference is equal to 0. The outcomes are created from these computations (respectively): $(Crime + Educ)/2 - (Poverty+Corruption+Pollution)/3$, $(Crime+Educ)/2-Corruption$, $(Crime+Educ)/2-Pollution$, and $(Crime+Educ)/2-Poverty)$. All regressions control for voting eligibility status and for being treated in the first experiment, and columns 2, 4, and 6 additionally control for voting eligibility status, interest in politics, sex, parents education, religious attendance and academic performance. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** at the 5% level, and *** at the 1% level.

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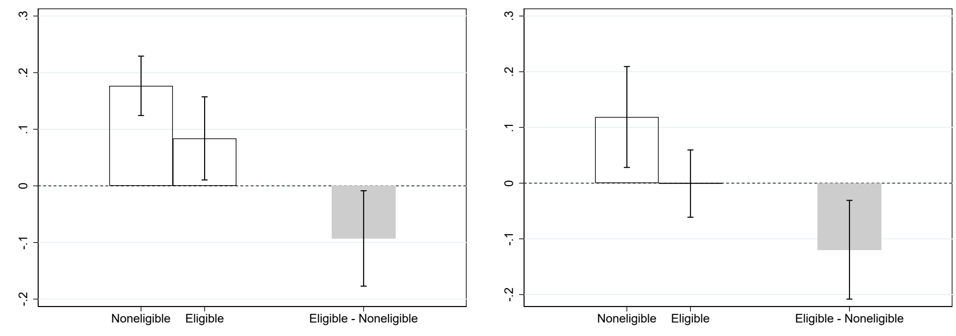
A Online Appendix (Not for Publication)

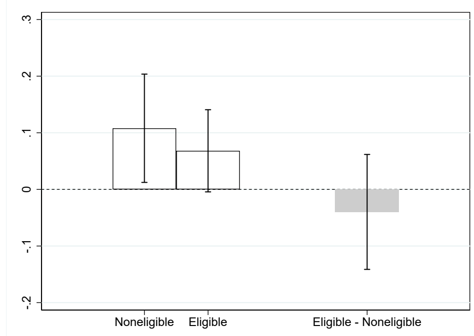
A1 Additional figures

Figure A.1: Heterogeneous treatment effects in policy information experiment

(a) Crime and educ relative to corruption (b) Crime and educ relative to pollution



(c) Crime and educ relative to poverty



Notes: These figures present the effect of being treated in the policy information experiment on the three additional outcomes of Table 8: $(Crime + Educ)/2 - Corruption$, $(Crime + Educ)/2 - Pollution$, and $(Crime + Educ)/2 - Poverty$). The white bar plots represent the coefficient of a treatment dummy when regressing the outcomes separately for non-eligible and eligible to vote participants, and the grey bar is the difference between the coefficient for eligible and the coefficients for non-ineligible. The spiked lines plot the 95% confidence intervals for the estimates. control for voting eligibility status, sex, parents education, religious attendance and academic performance. All dependent variables are standardized.

A2 Additional tables

Table A.1: Descriptive statistics for the full sample

	Obs	Mean	Std Dev	Min	Max
Interest Debate	5320	2.584	0.610	1	3
Interest Result	5312	3.524	0.739	1	4
Interest Politics	5324	2.780	0.831	1	4
Discusses Politics	5324	3.472	0.728	1	4
Attended Rallies	5324	1.223	0.605	1	4
Watched Debates	5324	1.831	0.375	1	2
Quiz Score, General	5324	0.645	0.221	0	1
Quiz Score, Election-Relevant	5321	0.607	0.144	0	.875
Parents Educ	5317	2.376	0.731	1	3
Religious Attendance	5317	2.496	1.082	1	4
Income	5317	3.169	0.722	1	4
Academic Perf	5317	4.429	0.605	1	5
Male	5324	0.476	0.499	0	1

Notes: This table presents the mean, standard deviation, minimum, maximum, and number of observations for the outcomes and individual-level covariates, in their original scales.

Table A.2: Regression Discontinuity Estimates for Other Outcomes - Linear Fit

	Attended Rallies			Watched Debates			Quiz Score, General			Quiz Score, Election-Relevant		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Baseline												
Eligible	0.100*	0.062	-0.015	0.018	0.022	0.028	0.031	0.070	0.003	-0.039	-0.028	-0.043
	(0.052)	(0.053)	(0.047)	(0.070)	(0.073)	(0.049)	(0.095)	(0.080)	(0.052)	(0.074)	(0.074)	(0.045)
Observations	3,227	2,883	4,850	3,322	2,883	4,850	2,321	2,883	4,850	3,162	2,881	4,847
Bandwidth (days)	210	180	480	219	180	480	139	180	480	204	180	480
Panel B: Baseline + Covariates												
Eligible	0.018	0.069	0.002	0.003	0.021	0.033	0.025	0.063	-0.002	-0.025	-0.024	-0.051
	(0.055)	(0.052)	(0.047)	(0.081)	(0.072)	(0.049)	(0.091)	(0.079)	(0.050)	(0.070)	(0.073)	(0.044)
Observations	2,170	2,879	4,844	2,624	2,879	4,844	2,403	2,879	4,844	3,241	2,879	4,844
Bandwidth (days)	128	180	480	160	180	480	146	180	480	212	180	480

Notes: This table presents the estimates for the effect of being eligible to vote on the rest of political engagement outcomes (β in Equation 1), when $f(Age - k)$ is a first-degree polynomial. Six regressions are run for each dependent variable, which vary in bandwidth and inclusion of covariates. Columns (1), (4), (7), and (10) are models estimated with Calonico, Cattaneo, and Titiunik (2014) optimal bandwith. Regressions in Columns (2), (5), (8), and (11) are estimated in respondents younger or older than the voting age by no more than 180 days, and those in Columns (3), (6), (9), and (12) for differences of 480 days or less. Panel (A) models only control for campus and date fixed effects, whereas Panel (B) estimates additionally control for sex, parents education, religious attendance and academic performance. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** is significant at the 5% level, and *** is significant at the 1% level.

Table A.3: Regression Discontinuity Estimates for Other Outcomes - Quadratic Fit

	Attended Rallies			Watched Debates			Quiz Score, General			Quiz Score, Election-Relevant		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Baseline												
Eligible	-0.039	-0.039	0.063	-0.040	-0.098	0.009	-0.020	-0.053	0.077	0.018	-0.083	-0.093
	(0.083)	(0.101)	(0.056)	(0.106)	(0.119)	(0.073)	(0.118)	(0.123)	(0.088)	(0.097)	(0.117)	(0.075)
Observations	3,076	2,883	4,850	3,195	2,883	4,850	3,064	2,883	4,850	3,723	2,881	4,847
Bandwidth (days)	197	180	480	207	180	480	196	180	480	263	180	480
Panel B: Baseline + Covariates												
Eligible	-0.004	-0.024	0.077	-0.043	-0.080	0.019	-0.036	-0.053	0.075	0.005	-0.077	-0.093
	(0.095)	(0.102)	(0.053)	(0.113)	(0.115)	(0.069)	(0.117)	(0.121)	(0.086)	(0.093)	(0.115)	(0.073)
Observations	2,928	2,879	4,844	3,051	2,879	4,844	2,992	2,879	4,844	3,734	2,879	4,844
Bandwidth (days)	183	180	480	194	180	480	189	180	480	264	180	480

Notes: This table presents the estimates for the effect of being eligible to vote on the rest of political engagement outcomes (β in Equation 1), when $f(Age-k)$ is a second-degree polynomial. Six regressions are run for each dependent variable, which vary in bandwidth and inclusion of covariates. Columns (1), (4), (7), and (10) are models estimated with Calonico, Cattaneo, and Titiunik (2014) optimal bandwith. Regressions in Columns (2), (5), (8), and (11) are estimated in respondents younger or older than the voting age by no more than 180 days, and those in Columns (3), (6), (9), and (12) for differences of 480 days or less. Panel (A) models only control for campus and date fixed effects, whereas Panel (B) estimates additionally control for sex, parents education, religious attendance and academic performance. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** is significant at the 5% level, and *** is significant at the 1% level.

Table A.4: Treatment effects in policy information experiment

	Crime & Educ Rel All		C&E Rel Corruption		C&E Rel Pollution		C&E Rel Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible	0.018	0.020	0.009	0.033	0.007	0.026	0.021	-0.015
	(0.035)	(0.044)	(0.041)	(0.053)	(0.029)	(0.041)	(0.033)	(0.039)
Policy Treat	0.187***	0.182***	0.175***	0.176***	0.118**	0.112**	0.108**	0.104**
	(0.036)	(0.037)	(0.026)	(0.027)	(0.045)	(0.045)	(0.046)	(0.047)
Policy Treat × Eligible	-0.109***	-0.111***	-0.095**	-0.094**	-0.102**	-0.106**	-0.034	-0.036
	(0.038)	(0.039)	(0.044)	(0.044)	(0.046)	(0.047)	(0.052)	(0.052)
Voting Treat	-0.052	-0.052	-0.024	-0.024	-0.018	-0.018	-0.068*	-0.067*
	(0.034)	(0.034)	(0.027)	(0.027)	(0.028)	(0.027)	(0.034)	(0.035)
Interest Politics		0.016		-0.038**		0.076***		-0.013
		(0.014)		(0.015)		(0.018)		(0.014)
Parents Educ		0.042**		0.021		0.039**		0.027
		(0.018)		(0.020)		(0.017)		(0.019)
Religious Attendance		-0.021		-0.000		-0.011		-0.032**
		(0.013)		(0.011)		(0.017)		(0.015)
Income		0.027		-0.021		0.039*		0.033*
		(0.019)		(0.022)		(0.020)		(0.017)
Academic Perf		0.066***		0.006		0.048**		0.080***
		(0.022)		(0.027)		(0.022)		(0.024)
Age		-0.003		-0.014		-0.023		0.031
		(0.037)		(0.031)		(0.031)		(0.030)
Male		0.062***		-0.068***		0.119***		0.061**
		(0.022)		(0.021)		(0.026)		(0.028)
Constant	-0.044	-0.498	-0.052*	0.300	-0.023	-0.296	-0.022	-0.968*
	(0.033)	(0.636)	(0.030)	(0.546)	(0.029)	(0.523)	(0.029)	(0.526)
Observations	5,317	5,317	5,317	5,317	5,317	5,317	5,317	5,317
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table presents the estimates for a heterogeneous treatment effect design applied to the policy information experiment. The outcomes are created from these computations (respectively): $(Crime + Educ)/2 - (Poverty + Corruption + Pollution)/3$, $(Crime + Educ)/2 - Corruption$, $(Crime + Educ)/2 - Pollution$, and $(Crime + Educ)/2 - Poverty$. All regressions control for voting eligibility status and for being treated in the first experiment, and columns 2, 4, and 6 additionally control for covariates presented in table 4. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.5: Treatment effects in policy information experiment - Crime comparisons

	Crime Rel All		Crime Rel Corruption		Crime Rel Pollution		Crime Rel Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible	-0.013	0.038	0.002	0.051	-0.013	0.038	-0.017	0.002
	(0.051)	(0.052)	(0.050)	(0.052)	(0.048)	(0.049)	(0.041)	(0.044)
Policy Treat	0.049	0.111***	0.072**	0.130***	0.009	0.070	0.039	0.062
	(0.034)	(0.035)	(0.034)	(0.033)	(0.029)	(0.043)	(0.033)	(0.042)
Policy Treat × Eligible		-0.103**		-0.097**		-0.101**		-0.038
		(0.043)		(0.048)		(0.049)		(0.049)
Observations	5,317	5,317	5,317	5,317	5,317	5,317	5,317	5,317
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the estimates of the main and heterogeneous treatment effects for the policy information experiment. The outcomes are created from these computations (respectively): $Crime - (Poverty + Corruption + Pollution)/3, Crime - Corruption, Crime - Pollution,$ and $Crime - Poverty$. All regressions control for voting eligibility status, for being treated in the first experiment, and for interest in politics, sex, parents education, religious attendance and academic performance. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.6: Treatment effects in policy information experiment - Education comparisons

	Educ Rel All		Educ Rel Corruption		Educ Rel Pollution		Educ Rel Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible	-0.042	-0.004	-0.023	0.010	-0.036	0.008	-0.041	-0.028
	(0.045)	(0.047)	(0.048)	(0.055)	(0.035)	(0.040)	(0.044)	(0.042)
Policy Treat	0.130***	0.176***	0.129***	0.170***	0.077**	0.130***	0.106***	0.121**
	(0.026)	(0.036)	(0.019)	(0.025)	(0.030)	(0.043)	(0.030)	(0.047)
Policy Treat × Eligible		-0.076		-0.067		-0.089*		-0.025
		(0.047)		(0.047)		(0.047)		(0.055)
Observations	5,317	5,317	5,317	5,317	5,317	5,317	5,317	5,317
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the estimates of the main and heterogeneous treatment effects for the policy information experiment. The outcomes are created from these computations (respectively): $Educ-(Poverty+Corruption+Pollution)/3, Educ-Corruption, Educ-Pollution,$ and $Educ-Poverty$. All regressions control for voting eligibility status, for being treated in the first experiment, and for interest in politics, sex, parents education, religious attendance and academic performance. All dependent variables are standardized. Standard errors are clustered at the campus level (30 clusters), and reported in parentheses. * is significant at the 10% level, ** at the 5% level, and *** at the 1% level.