Civic Engagement in the Americas

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Abstract

This paper estimates the effect of voting eligibility on civic engagement measured along three dimensions: political motivation, political activities, and political knowledge. These outcomes originate in the AmericasBarometer 2004-2016 surveys of eligible voters. To identify the effects the paper exploits variation in field survey dates relative to election dates, given country-specific voting age laws. It is found that voter enfranchisement increases self-reported interest in politics, political socialization, and attendance of political meetings; however, consumption of political news is not statistically different between eligible and non-eligible citizens. Some evidence indicates that the political activities voters engage in translate into increased political knowledge, in contrast with the view that voters are rationally ignorant. The effects are larger in countries with enforced mandatory voting.

1 Introduction

The success of public policies depends critically on the level of citizen engagement in the policymaking process, e.g., through expressing preferences, or through selecting and monitoring policymakers. The research based on random assignment of information to voters in developing countries finds that the governance of public programs can be improved when voters are well-informed and actively participate in the policymaking process (Pande, 2011).

To what extent do citizens *choose* to exert effort to engage in the policymaking process? And, what factors can stimulate civic engagement? The theoretical literature on whether voters have incentives to acquire information, or whether instead they choose to remain "rationally ignorant" (Downs, 1957), has a long tradition in political economy. The empirical literature addressing this question has disproportionately focused on information consumption through the media. Less is known about the extent of citizen engagement through other modalities, such as talking about politics with family and friends, or attending meetings of political parties. Similarly, while the literature has focused on whether turning out to vote is associated with increased engagement, less attention has been given to whether enfranchisement itself provides a source of motivation for citizens to become politically engaged.[1]

In this paper we provide evidence on the level of civic engagement in three dimensions: political motivation, political activities, and political knowledge. The main empirical challenge to estimating the civic engagement of eligible voters is finding an adequate couterfactual that captures how these individuals would have behaved if ineligible to vote. To test how enfranchisement motivates voters to engage in political activities, and whether this translates into political knowledege, we focus on newly enfranchised voters surveyed after general elections. Some were eligible to vote in the most recent election, while others were not. We employ data from the AmericasBarometer surveys covering 34 countries biannually between 2004 and 2016. We focus on the 62,005 young voters in the surveys, ages 16-25, and use countries' minimum voting age requirements and election dates to determine who was eligible to vote in the most recent presidential election. Minimum voting age requirements provide a credibly exogenous source of variation that allows us to identify the effects of voter enfranchisement on a variety of outcomes. The civic outcomes include political interest, self-reported understanding of political issues, talking about politics, news consumption,

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[1]It is not clear that citizens who abstain from voting are always less engaged politically, as some may become alienated once they understand that none of the candidates are representing their preferences.

attendance at political meetings, and basic knowledge of the political process.

We employ three empirical designs to estimate civic engagement. First, we estimate regressions where individual-level civic outcomes depend on voting eligibility status, controlling for age at interview, income, and country and year fixed effects. Second, we restrict the sample to the youngest eligible and the oldest non-eligible, i.e., the age groups closest to the age eligibility cutoff; we call this subsample the cutoff sample. Third, we approximate age on election day using age at interview and the time between the election date and the interview date. This is an approximation because the surveys only elicit age in years, so actual birthdays are not reported. Then we use this approximate age at election as the running variable in a regression discontinuity design.

The results show that voter enfranchisement increases individuals' self-reported interest in politics, an effect that appears in all three empirical strategies and holds up to a battery of robustness tests. Additionally, we find consistent evidence that newly enfranchised voters also report talking about politics more frequently and attending political meetings more regularly than their non-eligible peers, while there is also some evidence that this translates into increased political knowledge. As the surveys were conducted at varying lengths of time after a general election, we exploit this feature of the data to study the dynamic element of becoming eligibile. As expected, the gap in civic engagement between eligible and non-eligible is greatest at the time of the most recent election. The gap shrinks as the previously ineligible respondents become eligible, and closes about four-five years on average after the election has passed. This convergence is due to the previously ineligible group increasing their engagement levels to match the eligible group by the time the next election takes place.

In an ideal democracy, a citizen's right to vote motivates political interest, which leads the citizen to acquire information, which is translated into knowledge and used in making decisions at the ballot box. Yet it is not clear why citizens take this responsibility seriously, given the seemingly low payoff to being a good citizen, and the resulting collective action problem. Exploiting the dual voluntary/compulsory voting rules in Brazil's electoral system where voting eligibility starts at age 16 but voting becomes mandatory at age 18, Lopez de Leon and Rizzi (2014) find that compulsory voting laws increase turnout among the eligible. However, those who vote have the same political knowledge as those who do not. The same is true for the intention-to-treat estimate, which measures the effect of mandatory voting on those whose decision to turn out is changed by the mandatory voting law. Using a similar

empirical strategy but different outcomes, Bruce and Costa Lima (2017) show that Brazil's mandatory voting rules make citizens on average 13.6 percentage points more likely to watch the country's main television newscast. This effect, which is particularly strong among low-income individuals, implies that mandatory voting encourages people to acquire information on issues that could be relevant to their voting decisions, though the study is unable to evaluate whether they ultimately become more knowledgeable.

Our paper complements the aforementioned work that exploits compulsory voting laws by focusing on the effect of voter enfranchisement rather than the effect of actually voting. While we find no evidence supporting the finding that enfranchisement increases news consumption, our results do point to a number of other potential outlets for increased political interest, and they provide some of the first evidence that enfranchisement may lead to a difference in political knowledge between eligible and non-eligible citizens. This stands in contrast to a study in Japan, which used birthdates and an online survey taken shortly after the 2016 House of Councillors election, that found no difference between the attitudes of respondents just below and just above the voting age cutoff of 18 in terms of their political interest and trust in government (Horiuchi, Katsumata and Woodard, 2017). Our results also suggest that while voter enfranchisement in voluntary voting systems has a motivating effect on voters, stronger effects may be seen in countries with enforced mandatory voting laws. This provides support for the Lijphart (1997) hypothesis that compulsory voting improves a country's civic culture.

In the next section, we discuss our data and empirical strategy. In Section 3, we present our main results. Section 4 shows the results of a series of robustness tests, and Section 5 explores the mechanisms behind the main effects, using interactions between eligibility and country-level variables such as mandatory voting laws and freedom of the press. We conclude in Section 6.

2   Data and Empirical Strategy

The main data for this study comes from the AmericasBarometer, a survey administered by the Latin American Public Opinion Project (LAPOP) at Vanderbilt University in countries across North, Central, and South America, and much of the Caribbean. The survey has been conducted in waves every two years from 2004 to 2016, and coverage has grown from 11

countries in 2004 to 29 in 2016; in total, the survey has been applied in 34 countries over this period. Table 1 shows the country and wave coverage of our data. For each wave, the sample is designed to be representative of each country and uses a multi-stage probability design, stratifying by region within the country, sub-stratifying each region by municipality size and urban/rural areas within the municipality, and selecting households in clusters. LAPOP then uses frequency matching to obtain a sample with similar age and gender distributions to the national census, interviewing only respondents who are of legal age to vote. With few exceptions, respondents are interviewed in their homes and data are collected with handheld electronic devices.

Restricting our sample to respondents between 16 and 25 years old, our final dataset contains 62,005 observations from 34 countries, organized as a repeated cross-section of a group of countries that grows with each subsequent wave. For each respondent, the data include the date of interview, as well as socioeconomic information such as years of schooling, ethnicity, sex, age, marital status, and information on the area where they live. Most LAPOP questions focus on public opinion topics, such as trust in institutions, perceptions of crime and the economy, and political participation. For our project we use questions related to political motivation, political activities, and political knowledge. Table 1 contains a description of the variables in our dataset.

Our research question focuses on whether citizens become more engaged in politics when they become eligible to vote. To determine voting eligibility, we need to know not just how old respondents are at the time of interview, but also how old they were on the date of the most recent election. LAPOP does not include respondents' dates of birth, requiring a strategy to infer a respondent's age at the last election. Using dates from presidential or general elections (for parliamentary systems) from the Institute for Democracy and Electoral Assistance (IDEA) and the Inter-Parliamentary Union, we created a database on elections between 1999-2016 in our sample of countries, merged this with the LAPOP data, and determined the most recent election at the time of each interview. Then we generated a new variable called $Score_{ict}$ as follows:

$$Score_{ict} = InterviewAge_{ict} - Gap_{ict} - MinAge_c \qquad (1)$$

where $InterviewAge_{ict}$ is the age at interview, for respondent i, living in country c, inter-

viewed in year t, Gap_{ict} is the number of years (days divided by 365) elapsed between the last election and the interview, and $MinAge_c$ is the minimum voting age in country c. We then use an assignment rule to determine voting eligibility:

$$Eligible_{ict} = \begin{cases} 1 \text{ if } Score_{ict} \geq 0 \\ 0 \text{ if } Score_{ict} \leq -1 \end{cases} \tag{2}$$

To see the intuition, take a simple example. Mexico held a presidential election on July 2, 2006. Hypothetical respondents $i = 1$ and $i = 2$ both live in Mexico and were interviewed on July 2, 2008, in Mexico's 2008 survey wave; the first respondent was 20 years old at the time of interview, and the second was 19. Exactly two years have elapsed since the election, so the interview gap is 2. The minimum voting age in Mexico is 18. In this case, $i = 1$ has an eligibility score of exactly 0 (i.e., they were 18 on the day of the election), while $i = 2$ has a score of -1 (i.e., they were 17 on the day of the election), and the examples represent a unique case where we can determine with certainty the respondents' ages at the time of the election. However, hypothetical respondent $i = 3$, also living in Mexico and 20 years old at the time of interview, was interviewed three weeks earlier, on June 11, 2008. In this case, 710 days have elapsed since the election, or approximately 1.945 years. This respondent's eligibility score will be 0.055. In other words, while this respondent was at least 18 at the time of the election, it is possible their birthday falls between June 11 and July 2 and that they were 19; in either case, we know with certainty that they were eligible to vote. Respondent $i = 4$, also 20 years old at the time of interview, was interviewed three weeks after July 2, on July 23, 2008. Because this falls 2.06 years after the election, the eligibility score is -0.06; assuming a uniform distribution of birthdates, there is about a 94% chance the respondent was 18 on the date of the election, and a 6% chance they were 17. Note, however, that this creates a subset of respondents for whom $-1 < Score_{ict} < 0$. We cannot know with certainty whether these respondents were eligible or ineligible to vote, and assign them missing values for $Eligible_{ict}$.

Table 2 presents descriptive statistics for the variables of interest in our sample. Our six outcome variables can be broadly organized into three categories - Motivation ($Interest$, $Understanding$), Activities ($Socialization$, $News$, $Meetings$), and Knowledge ($Knowledge$). Many of these outcomes are measured on ordinal scales. For example, the $Socialization$ question item asks respondents how frequently they talk about politics with others, and takes

values 1 (Never), 2 (Rarely), 3 (A few times per month), 4 (A few times per week), and 5 (Daily). One limitation of the LAPOP data is that not all question items are asked in every country in every year, resulting in observation counts that vary depending on the outcome.

Table 2 provides some insights into our sample. Of the observations for whom voting eligibility status can be determined, approximately 76.7% were eligible to vote in the last election and 23.3% were ineligible. Approximately 11.5% of the observations in the sample show ambiguous eligibility - their voting eligibility status in the most recent election cannot be determined with certainty, because $-1 < Score_{ict} < 0$. The respondents had an average age of 21.35 years at the time of the interview, with an average gap between most recent election and interview of 2.18 years. Age has a minimum value of 16 and a maximum of 25, reflecting two important characteristics of our data: i) only voting-age respondents are surveyed, and a small number of countries set the minimum voting age at 16; and ii) we exclude observations above 25 years old because our focus is on first-time voters. Income percentile is a variable created from a LAPOP question asking for the range of the respondent's monthly family income; because the income ranges are not standardized across waves or countries, we create percentiles describing the respondent's income in comparison to other respondents from the same country and survey wave.

To identify the effect of voting eligibility on our outcomes of interest, we employ three empirical strategies. These include a full-sample specification, a cutoff-sample specification, and a modified regression discontinuity design. The strategies differ in the way they control for age differences among respondents.

For our baseline specification, we estimate:

$$Y_{ict} = \beta Eligible_{ict} + \boldsymbol{\delta}'\mathbf{X}_{ict} + \gamma_c + \tau_t + \varepsilon_{ict} \tag{3}$$

where Y_{ict} is the outcome of interest for respondent i, living in country c, interviewed in year t, $\mathbf{X}_{ict}$ is a vector of individual-level controls including age at interview and income percentile, and γ_c and τ_t are fixed effects for country and year, respectively. ε_{ict} is an error term allowing for arbitrary within-survey correlation, i.e., clustered at the country-wave level.

For our first empirical strategy, equation (3) is estimated by OLS for the full sample, referring to all observations from 16 to 25 years old at the time of interview, excluding only the ambiguous eligibility observations. In our second strategy, we estimate equation (3) for

the sample at the cutoff, to include only the oldest ineligible (i.e., $-2 \leq Score_{ict} \leq -1$) and the youngest eligible observations (i.e., $0 \leq Score_{ict} \leq 1$) at the time of the last election. For both strategies, the coefficient of interest β identifies the effect of being eligible to vote on the three levels of citizenship under the assumption that voting eligibility in the last election is orthogonal to any confounding variables that also determine our outcomes of interest, once controls are included. We argue that this assumption should hold because, while the individual decision of whether or not to vote is clearly endogenous, voting eligibility is determined based on age restrictions defined by each country that generate a reasonably credible source of exogenous variation.

For our third strategy, we employ a modified regression discontinuity design. We use $Score_{ict}$ as our running variable and restrict the sample to observations where $-3 \leq Score_{ict} \leq$ 3, defining the eligibility cutoff rule as before. While there is uncertainty in most cases as to the exact age of the respondent at the time of the election, this uncertainty can be quantified; in the case of hypothetical Mexican respondent $i = 3$ mentioned earlier, there is a 94.5% chance they were 18 and a 5.5% chance they were 19 at the time of the election, assuming birthdates are uniformly distributed throughout the year. While there is uncertainty as to the exact age of any one respondent, as more observations are added, observations with lower eligibility scores would have been younger at the time of the election, on average, than those with high eligibility scores. We use a linear specification:

$$Y_{ict} = \beta Eligible_{ict} + \alpha Score_{ict} + \boldsymbol{\delta}' \mathbf{X}_{ict} + \gamma_c + \tau_t + \varepsilon_{ict} \tag{4}$$

The AmericasBarometer surveys are carried out independent of the electoral cycle in each country, and in many cases respondents are interviewed two years or more after the most recent election. Thus, for the three strategies described, β identifies the effect at some average time point following the election, which might be significantly diminished from the effect measured at the time of the election. To capture this dynamic element, we also estimate a modified version of these previous equations, including an interaction term between eligibility and interview gap:

$$Y_{ict} = \beta_1 Eligible_{ict} + \beta_2 Gap_{ct} \times Eligible_{ict} + \beta_3 Gap_{ct} + \boldsymbol{\delta}' \mathbf{X}_{ict} + \gamma_c + \tau_t + \varepsilon_{ict} \tag{5}$$

where Gap_{ct} is a measure of the time elapsed between the most recent election and the

end of the end date of the survey interviews in a given country-wave, thus with variation at the level of country-wave (i.e., a survey conducted in country c in year t). However, as Gap_{ct} increases, we also get closer to the next election. In this specification, β_1 measures the mean difference in civic engagament between eligible and non-eligible, as of the date of the election. Thus, the expected sign is positive. The coefficient β_2 is the change in the civic engagement differential one year after the election. If the differential shrinks as the previously ineligible become eligible, the sign should be negative. If political engagement increases among previously ineligible citizens as the next election approaches, β_3 should be positive. Thus, while we expect a difference between our treatment and control groups near the first election, where Gap_{ct} is close to zero, we expect β_2 to be such that it cancels out β_1 after approximately four or five years, at which point new elections are held, previously ineligible voters become eligible to vote for the first time, and our treatment and control groups would be expected to show similar levels of civic engagement.

In Table 3 we present the results of covariate balance tests showing the balance between eligible and non-eligible respondents on key demographic characteristics. We do this by showing the mean for each trait, along with the estimated coefficient from the regression on voting eligibility status with country and year fixed effects, controlling for age at interview and income percentile. This is done for the full sample, as well as for the sample who were at the cutoff at the last election. As shown, the eligible and non-eligible groups are well-balanced in terms of sex, race, religion, political ideology, employment status, and the characteristics of the areas in which they live. However, we also see that the eligible group has slightly more education and is slightly more likely to be single (never married).

Overall, we believe the results from Table 3 support the argument that our eligibility definition splits the sample into comparable treatment and control groups, and validates some of the assumptions of our identification strategy. The coefficient on voting eligibility tends to be close to zero and insignificant. While arbitrary voting age thresholds are credibly exogenous, there could be some concern that uncertainty regarding the eligibility status in the ambiguous-eligibility group might constitute an impediment to creating comparable treatment and control groups; however, Table 3 shows that once we control for age at interview, these groups appear similar in observable characteristics.

3 Results

This section presents the main results, based on the three empirical specifications outlined above.

3.1 Full Sample Specifications

Table 5 shows the results for the full sample, presenting estimates of equation (3) for our baseline specification in odd numbered columns. As shown, young people who were eligible to vote in the most recent election show a statistically significant higher interest in politics, as well as higher levels of knowledge, measured as correctly answering a question on the length of the president's term of office. The coefficients on age and income tend to be positive and significant, reflecting increased political engagement among older and wealthier voters. It is interesting that the only outcome for which income is not positive and significant is attendance at political meetings, perhaps reflecting a greater degree of grassroots activism and community organizing among less affluent citizens.

In the even-numbered columns of Table 5 we present results from the dynamic specification in equation (5). The coefficients on voting eligibility go in the expected direction and generally increase in magnitude over the baseline specification. Although the coeficient for the effect of eligibility on political knowledge is no longer significant, the coefficient for political interest increases in magnitude, while the coefficients for having political conversations (socialization) and attendance at political meetings increase measurably and are both statistically significant. Regarding the dynamic element, the interaction between eligibility and interview gap is negative and often significant, reflecting the over-time convergence of civic engagement levels between the eligible group and the non-eligible group. The size of this coefficient is such that it would cancel out the effect of eligibility on political interest and meetings after about four years, and cancel out the effect on socialization after about three years; this roughly corresponds to the electoral cycle and provides evidence in support of the hypothesis that the two groups become similarly engaged closer to a new round of elections where the previously ineligible group become newly enfranchised voters. The coefficient on Gap_{ict} is generally positive, showing that the shrinking difference between eligible and non-eligible over the electoral cycle is driven by the increase in engagement of the previously non-eligible, rather than a decline in engagement of the eligible.

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3.2   Cutoff Sample Specifications

Table 6 shows results for the sample at the cutoff, which are qualitatively similar to the results for the full sample. For the baseline specification, the estimates for political interest and knowledge are somewhat smaller than in those for the full sample, though both retain their significance. In the dynamic specification, the estimates for the cutoff sample are often nearly twice as large as those for the full sample. Statistical significance is retained for both political interest and meetings, although the estimate for socialization is no longer significant at standard levels. While the estimate on the dynamic interaction continues to go in the expected direction, it is less precisely estimated compared with the full sample results.

Comparing the estimates for our control variables between the full and cutoff samples, we again note that the income coefficient remains positive and significant for all outcomes except political meetings. On the other hand, the coefficient estimate for age is less consistent across samples and at times is negative and statistically significant; this is perhaps to be expected for political meetings, but less so for political interest.

3.3   Regression Discontinuity

Estimates for the regression in equation (4) are shown in Table 7, for both the static and dynamic specifications. Recall that these regressions use the sample where $-3 \leq Score_{ict} \leq 3$, again excluding observations with ambiguous eligibility status. We continue to observe positive, statistically significant estimates for the effect of eligibility on political interest, socialization, and attendance at political meetings, seen in both the static and dynamic models. Particularly for the static model, the estimates also tend to be quite a bit larger than the estimates in the full sample specification. We also note that the estimates on the dynamic interaction continue to go in the expected direction, and show some statistical significance. The magnitude of the dynamic term for political interest and socialization is such that the civic engagement differential would decline to zero after approximately five years.

In general, we believe the results from our three specifications provide evidence in support of an effect for voter eligibility on political engagement, particularly as measured by self-described political interest, frequency of talking about politics, and attendance at political meetings. This stands in contrast to the null results found in the related work for Brazil

(Lopez de Leon and Rizzi, 2014) and Japan (Horiuchi, Katsumata and Woodard, 2017). The results suggest a story where political interest increases in newly enfranchised voters compared to their ineligible peers, perhaps because for the first time they have both a stake and a voice in national politics. This increased interest may translate into a higher degree of socialization, as newly enfranchised voters share their increased interest and excitement about politics with those around them in politically-tinged conversations. This interest may also lead them to attend meetings of political movements or parties for the first time as they attempt to learn more about the political process and gain a greater voice. Importantly, the results also suggest that the effect is strongest as of the time of the last election. As the next election approaches, previously ineligible voters become engaged, closing the gap with eligible voters.

4 Robustness

In this section we present a series of checks to show the robustness of our results to different sample compositions, control variables, model specifications, eligibility definitions, and eligibility cutoff thresholds.

4.1 Country Composition

Recall that our sample includes 34 countries, spanning a diverse group that includes large Latin American countries like Mexico, Brazil, and Colombia, smaller countries in Central America, English-speaking Caribbean islands, the United States, and Canada. Countries were also added progressively, with the original 2004 wave of the survey including Mexico, Central America and some of South America, while most South American countries were included in 2006, and the United States, Canada, and Caribbean islands were included progressively starting in 2006. To show that our results are not driven by any one specific country and robustness to different samples, we provide evidence from two robustness tests. First, we run our baseline regressions separately for each country and plot the coefficients to show that outliers are not driving the results. Second, we exclude the United States, Canada, and English-speaking Caribbean from the sample to show robustness to a different composition of countries.

Figure 1 shows the coefficient distributions from country-by-country regressions of the

baseline specification with the full sample. As shown, no one country appears to be driving the results. The distribution for political interest is centered slightly to the right of zero, as expected from the positive coefficient seen in the baseline regressions. To the extent that extreme outliers are seen, these tend to fall on the negative end of the distribution and represent small Caribbean islands that appear in only one wave (2016) and have relatively few observations.

Table 8 presents the results for the restricted sample regressions. Odd-numbered columns present the results using the full sample, but excluding the United States and Canada, while even-numbered columns show the results excluding the United States, Canada, and English-speaking Caribbean countries. Few observations are lost from the baseline when we exclude the USA and Canada, reflecting the fact that while these are large countries, LAPOP does not interview more of their citizens than it does for other countries, and neither country was included in the the original 2004 wave. A greater proportion of observations is dropped when we also exclude the English-speaking Caribbean, resulting in a 10-20% reduction in observations from the full sample specification, depending on the outcome variable. Results do not vary significantly between specifications and are similar to the dynamic results in Table 5; while the estimate is somewhat smaller for political interest in the restricted sample, the estimate for socialization is marginally greater in magnitude, and the estimate for knowledge increases slightly and becomes marginally significant in the second restriction. The interaction between eligibility and interview gap continues to go in the expected direction and is little changed in terms of magnitude or significance.

4.2 Control Variables

Table 9 shows the results for the full sample dynamic regressions, with variations on the set of included control variables. In odd-numbered columns, we include three additional controls - a dummy for sex, equal to one if the respondent is male, years of schooling,[2] and a dummy equal to one if the respondent has never been married and is not cohabitating with a domestic partner. While the estimates for eligibility decline slightly compared to the baseline regressions, they remain comparable in magnitude and retain significance. Estimates for the dynamic elements are also qualitatively similar to those in our baseline regressions. With

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[2]Education was excluded from our baseline regressions because of concerns about endogeneity, i.e., education may respond to becoming eligible to vote.

respect to the new control variables, we observe a positive, statistically significant correlation between males and all of our outcomes of interest, with the exception of political knowledge. Similarly, respondents with more education, as expected, appear more politically engaged, interested in politics, and knowledgeable, while the relationship between being single and the outcomes of interest is less consistent.

In the even-numbered columns, we present results analagous to the original full sample dynamic regressions, where income percentile has been replaced by an alternative measure of financial wellbeing. The measure is derived from a question asking respondents about their family's economic situation, and takes four possible values ranging from "Not enough income, and having a hard time" (1) to "Good enough and can save" (4). In general, the results are qualitatively similar to those in the odd-numbered columns of the table and to those in the full sample dynamic regressions in Table 5, with the most obvious difference being the marginally significant positive result for the effect of eligibility on political knowledge seen in Column 12. On the whole, we believe the estimates in Table 9 confirm the robustness of our main results and assuage concerns that the exclusion of education in our main set of control variables impacts the estimates on our coefficients of interest.

4.3 Naive Eligibility Definition

As discussed in the Data and Empirical Strategy section, the nature of the data creates some ambiguity as to a respondent's true age at the time of the last election, resulting in a group of observations for whom eligibility status at the last election cannot be determined with certainty, constituting about 11.5% of the sample. Here, we present two alternative, "naive" eligibility definitions, which allows us to show the robustness of our main results to the inclusion of this group of observations.

For our first naive strategy, we define a new eligibility variable as follows:

$$Eligibility'_{ict} = \begin{cases} 1 \; if \; InterviewAge_{ict} - GapYear_{ict} \geq MinAge_c \\ 0 \; if \; InterviewAge_{ict} - GapYear_{ict} < MinAge_c \end{cases} \tag{6}$$

where $GapYear_{ict} = InterviewYear_{ict} - ElectionYear_{ct}$. To return to our hypothetical Mexican respondent examples from the Data and Empirical Strategy section, respondents $i = 1, 2, 3$ would continue to be classified as before. However, respondent $i = 4$, previously assigned to the ambiguous-eligibility group and assigned a missing value for eligibility, is

now assigned an eligibility value. Their age at interview, 20, is subtracted by 2 (2008 minus 2006), which is equal to the minimum voting age of 18; this naive strategy assigns them to the eligibile group.

In our second strategy, we employ the $Score_{ict}$ defined previously. Recall that this variable takes an individual's age at interview and compares it to the minimum voting age, such that a $Score$ of zero means the individual was exactly the voting age, while values above (below) zero refer to years above (below) the minimum voting age at the time of the last election. Recall also that this variable can take decimal values, reflecting the degree of uncertainty about the exact age of the respondent at the last election, and that the ambiguous group always falls between $-1 < Score_{ict} < 0$ (i.e., there is some degree of uncertainty whether they were exactly the cutoff age for voting or one year below the cutoff). Hypothetical Mexican respondent $i = 4$ had a $Score_{ict} = -0.06$, meaning there is a 6% chance they were 17 at the last election (i.e., ineligible) and a 94% chance they were 18 (i.e., eligible), assuming a uniform distribution of birthdays throughout the year. Thus, we define this alternative eligibilty measure as:

$$Eligibility''_{ict} = \begin{cases} 1 \ if \ Score_{ict} \geq -0.5 \\ 0 \ if \ Score_{ict} < -0.5 \end{cases} \tag{7}$$

Results are presented in Table 10, with odd- and even-numbered columns showing results from the first and second definitions, respectively. The results are qualitatively similar to those from the full sample dynamic regressions in Table 5, though somewhat smaller in magnitude. This suggests an attenuation bias due to measurement error, which would be expected due to the fact that these strategies erroneously assign some non-eligible observations to the eligible group, and vice versa.

4.4   Ordered Probit

As described previously, our outcome variables are measured on ordinal scales, with the exception of political knowledge, which is a dummy. Here we present the results of ordered probit regressions, showing the robustness of the OLS results to a logistic regression model. Results are shown in Table 11 for the baseline and dynamic specifications with the full sample. The strategy confirms the results from Table 5, and we note the addition of a positive, statistically significant result for the effect on knowledge in the dynamic version of the ordered probit regression.

4.5   Eligibility Threshold Falsification Test

We conduct a falsification by manipulating the minimum voting age threshold. Specifically, we re-run the dynamic regressions for the full sample, defining eligibility at two years below the true cutoff, one year below, at the true cutoff, one year above the true cutoff, and two years above. Thus, at the true cutoff the regression results would be the same as those presented in the dynamic regressions in Table 5, while the manipulated cutoffs provide placebo treatment and control groups. If the estimates for eligibility truly reflect the effect of being eligible to vote on political engagement, we would expect to find two patterns: i) the effect of eligibility should only be significant at the true cutoff, and ii) plotting the estimated coefficients on a graph where the x-axis ranges from two years below to two years above the true cutoff and the y-axis measures the estimated coefficient, we should see an inverted U shape, representing a greater effect at the true cutoff and declining estimated effects as the manipulated cutoff gets further from the true cutoff.

The results from this test are plotted in Figure 2. Overall, the results suggest the use of the correct cutoff. For political interest, the plot shows the predicted inverted U shape, though the estimates are only significant at the 95% confidence interval both one year above and below the true cutoff. For attendance at political meetings, the effect is only significant at the 95% confidence interval for the estimate at the true cutoff and the plot somewhat resembles an inverted U, though several of the placebo estimates are significant at the 90% confidence interval. However, socialization performs somewhat poorly on the falsification test, with a shape that does not resemble an inverted U and estimates significant at the 95% confidence interval for placebo treatments two years below and one year below the true cutoff.

An important limitation to these tests is that the treatment-control split becomes even more heavily skewed when we define the cutoff below its true value. This is because LAPOP only interviews voting-age respondents. As the voting age is artificially set below the true threshold, more 16-25 year-old respondents at the time of survey end up in the eligible group for the most recent election; in the case of political interest, for example, 95.3% of the sample is eligible when we define the threshold two years below the true cutoff, and 88.7% is eligible when the threshold is set one year below the true cutoff, compared to 77.6% when the threshold is set at the true cutoff. This contributes to the wider confidence intervals on the left-hand side of the true threshold in Figure 2, and may explain in some measure the

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poorer performance of the falsification tests on this side of the cutoff.

4.6 Regression Discontinuity with Varying Slopes

Next, we modify the regression discontinuity in equation (4) to allow the slope on the $Score_{ict}$ term to vary on either end of the eligibility cutoff threshold. The modified equation is as follows:

$$Y_{ict} = \beta_0 + \beta_1 Eligible_{ict} + \beta_2 Score_{ict} + \beta_3 Eligible_{ict} \times Score_{ict} + \boldsymbol{\delta}' \mathbf{X}_{ict} + \gamma_c + \tau_t + \varepsilon_{ict} \quad (8)$$

Results for both static and dynamic versions of this specification are presented in Table 12. The interaction between $Score$ and eligibility, determining the difference in slope between the eligible and ineligible groups, tends to be close to zero and is consistently insignificant, with the exception of a marginally significant, negative difference in the dynamic version of the regression for knowledge. Overall, this suggests little difference between the slope of the RD running variable for the two groups. For the estimates on eligibility, results are similar to the regression discontinuity results in Table 7, with a positive, significant result for political interest and socialization in the static version, as well as for interest, socialization, and meetings in the dynamic version. However, the marginally significant result for meetings in the static version of the original RD specification is no longer significant in the varying-slope version, though the estimate is moderately larger.

5   Channels

In this section we use interactions with country-level characteristics/institutions to test hypotheses regarding the channels through which acquiring the right to vote may lead to increased political engagement. First, we examine whether voters become increasingly engaged in politics because they want to make an informed, optimal decision with their vote. If this is the case, the effect should be stronger in countries with mandatory voting laws, where newly enfranchised voters not only have the opportunity to vote for the first time, but also face some form of obligation to exercise their voting rights. Second, we expect that the presence of a free and independent press provides additional incentives for newly enfranchised voters to become informed by offering them information of higher quality; we

examine this by interacting a measure of freedom of the press with voting eligibility.

5.1 Mandatory Voting

Although a number of countries in Latin America and the Caribbean have mandatory voting laws, these laws are only enforced and punished through some form of sanction in Ecuador, Peru, Uruguay, Brazil, and Argentina, as well as in Chile until 2012. Such policies have been found to be effective at increasing turnout in elections, particularly among citizens with lower levels of education (Jaitman, 2013). It is possible that, by inducing registered voters to get to the polls, the effect of voter enfranchisement on political engagement will be stronger in these countries. This would provide evidence that it is not simply being eligible to vote that results in increased engagement, but rather that engagement is an active part of voters taking their voting obligations seriously, and preparing to vote by informing themselves on candidates and policy issues. On the other hand, increased engagement could result from the act of voting, as voters feel a greater sense of civic duty after casting their vote and pay closer attention to how politicians they voted for (or against) are performing (Lijphart, 1997).

To test this, we take data on the enforcement of mandatory voting laws for the countries in our sample from IDEA. We define a dummy variable called $Voluntary$, equal to one if no mandatory law is enforced, and equal to zero otherwise (i.e., the aforementioned group of six countries)[3]. We then interact eligibility with the $Voluntary$ dummy, expecting a negative coefficient reflecting a more moderate effect for eligibility in countries without enforced mandatory voting laws.

The results from the static model are shown in Table 13 in even and odd-numbered columns for the full and cutoff samples, respectively. The coefficient for the effect of voting eligibility on political interest is substantially larger than in the baseline specification, suggesting the effect is being driven by newly enfranchised voters in countries with enforced mandatory voting. Consequently, the interaction term consistently shows a negative coefficient, suggesting an attenuated effect in countries without enforced mandatory voting; in the cases of political interest and socialization, the magnitude of this effect is such that it essentially cancels out the effect of voting eligibility. We also note that the coefficient for the

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[3]Note, however, that while all of these countries have enforced mandatory voting, in Ecuador, Brazil, and Argentina it is mandatory starting at age 18, but not at the minimum voting age in those countries (16).

effect on knowledge remains positive and significant, as in the baseline regressions, and that the estimate for the effect on understanding is positive and now marginally significant. While the estimates for the effect of $Voluntary$ are inconsistent in direction, it is worth pointing out that Chile is the only country with variation in its mandatory voting laws over the period of study; this is also why the coefficient cannot be estimated in the political socialization regression, as the 2006-2010 period for which this item was included in the survey does not cover Chile's 2012 transition to a voluntary voting regime.

5.2   Free Press

Free, independent, quality journalism is necessary in a well-functioning democracy. Voters rely on the media to highlight important policy issues, frame the political debate, and provide basic information on when events like elections are taking place and identify candidates to watch. The presence of local Spanish-language news programming has been found to increase voter turnout among Latinos in the United States, suggesting the mere presence of relevant media in a format viewers can understand increases engagement and participation in politics (Oberholzer-Gee and Waldfogel, 2009). In a randomized experiment, Gerber, Karlan and Bergan (2009) find that exposure to media influences voters' support for particular candidates, though they find no effect on knowledge of political events, political opinions, or voter turnout.

How the quality and indepence of news media might impact political engagement is less obvious. In a number of countries in Latin America, including Mexico and Brazil, journalists face violence by criminal groups (Comittee to Protect Journalists, 2016), resulting in a difficult media environment and likely leading to self-censorship for fear of reprisal. In such cases, voters may become disengaged with politics as the media focuses on softer stories, and motivated voters become frustrated by the lack of quality information and their inability to identify the perpetrators of media opression. In other countries, such as Venezuela, increasing invervention from the government into the media market, coupled with obstruction from security forces and prosecutions for defamation have led to the deterioration of the freedom of the press in recent years (Freedom House, 2017); yet the resulting political polarization has arguably led to increased political engagement as citizens mobilize in support or opposition to the government's actions.

To test whether voter enfranchisement has an increased impact on political engagement

in countries with a freer press, we employ data from the Freedom House Freedom of the Press Index (FPI). We take the median FPI for each year, and define a *CapturedPress* dummy variable equal to one if the respondent lived in a country equal to or above the median (i.e., less free) and equal to zero if the country had an FPI value below the median (i.e., more free) at the time of the interview. If living in a country where the media faces more hostility provides disincentives to becoming politically engaged, because information is lower-quality and less useful for making informed voting decisions, we would expect the coefficient on the interaction term to be negative. As in the case of mandatory voting laws, we estimate the regression for the full and cutoff samples.

Results are presented in Table 14 in even and odd-numbered columns for the full and cutoff samples, respectively. The estimates for eligibility are similar to the results from our baseline specification presented in Tables 5 and 6. The direction of the estimate for the interaction between eligibility and captured press is not always consistent and shows little statistical significance. On the other hand, the estimate for the effect of living in a more oppressive media environment (*CapturedPress* = 1) is positive and marginally significant for the effect on political interest and socialization in the cutoff sample regressions. Perhaps unexpectedly, the estimate is positive and marginally significant for news consumption in both samples ($p = .101$ for the cutoff sample). *CapturedPress* appears to have no impact on political knowledge or attendance at political meetings, and may have a negative effect on self-reported understanding of political issues. This suggests not a correlation between oppression of the media and self-reported political interest, but rather an environment where it becomes more difficult to convert that interest into action. Consequently, while the average citizen might be more interested in politics in a more oppressive media environment, no disproportionate increase in interest occurs in such countries when citizens become newly enfranchised voters, perhaps again because they feel it is difficult to convert interest into actionable change.

6   Conclusions

This paper provides evidence on the level of civic engagement of young voters in the Americas using variation in eligibility provided by minimum voting age rules. Civic engagement is measured along three dimensions: political motivation, political activities, and political

knowledge. These measures are based on surveys taken between elections. The three empirical strategies employed have similar implications: enfranchisement is associated with more political interest; this manifests itself in more political socialization and more frequent attendance of political meetings; these activities translate into better political knowledge, although this latter effect is somewhat small. Noticeably, increased political interest is not accompanied by more consumption of political news. We also find that non-eligible voters close the engagement gap by the time they become eligible in the next election. Finally, we find some evidence that civic engagement is stronger under mandatory voting laws.

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Appendix

Figure 1: Distribution of Coefficients from Country-By-Country OLS Regressions
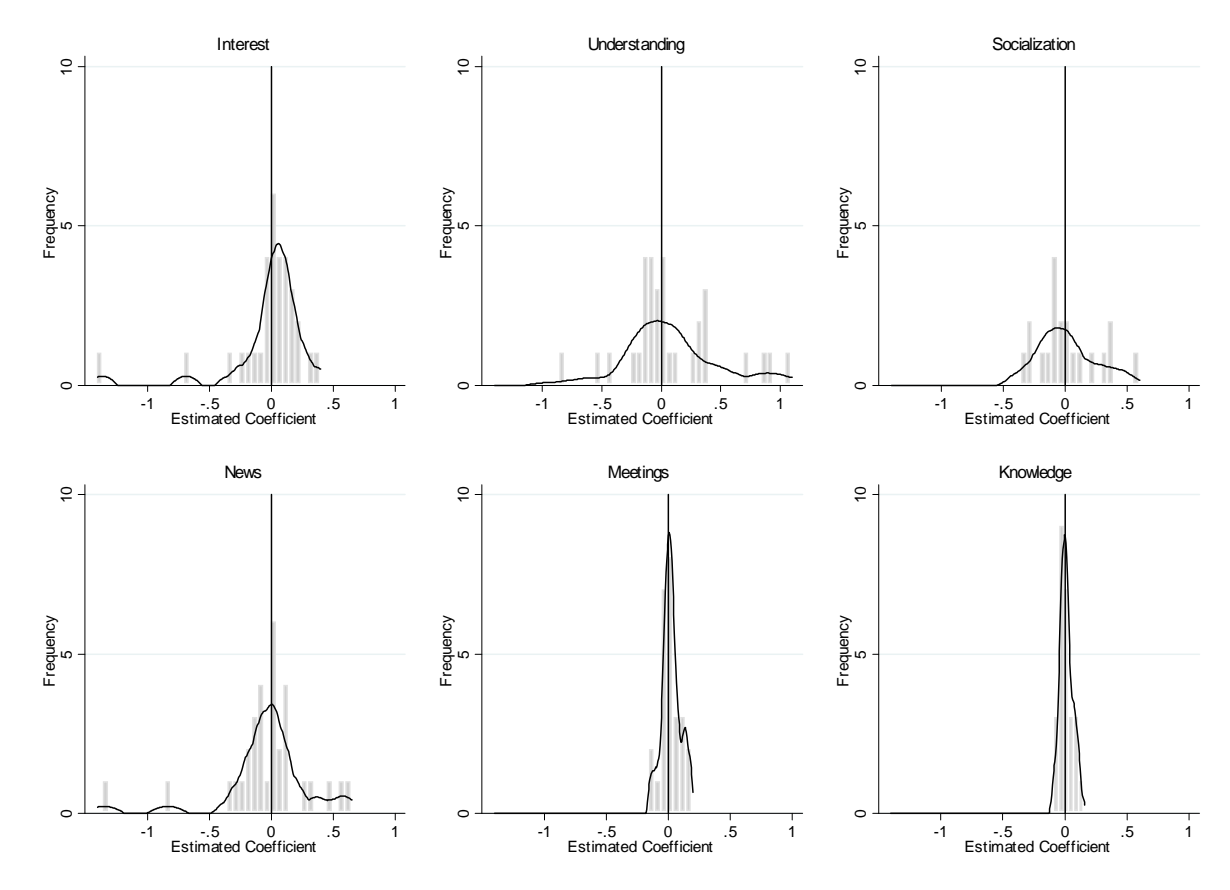
Notes: Coefficients from the country-by-country regressions of the dependent variable on voting eligibility, controlling for age, income, and year FEs, using the full sample. Plots the distribution of the the estimated effect of eligibility.

Figure 2: Falsification Tests
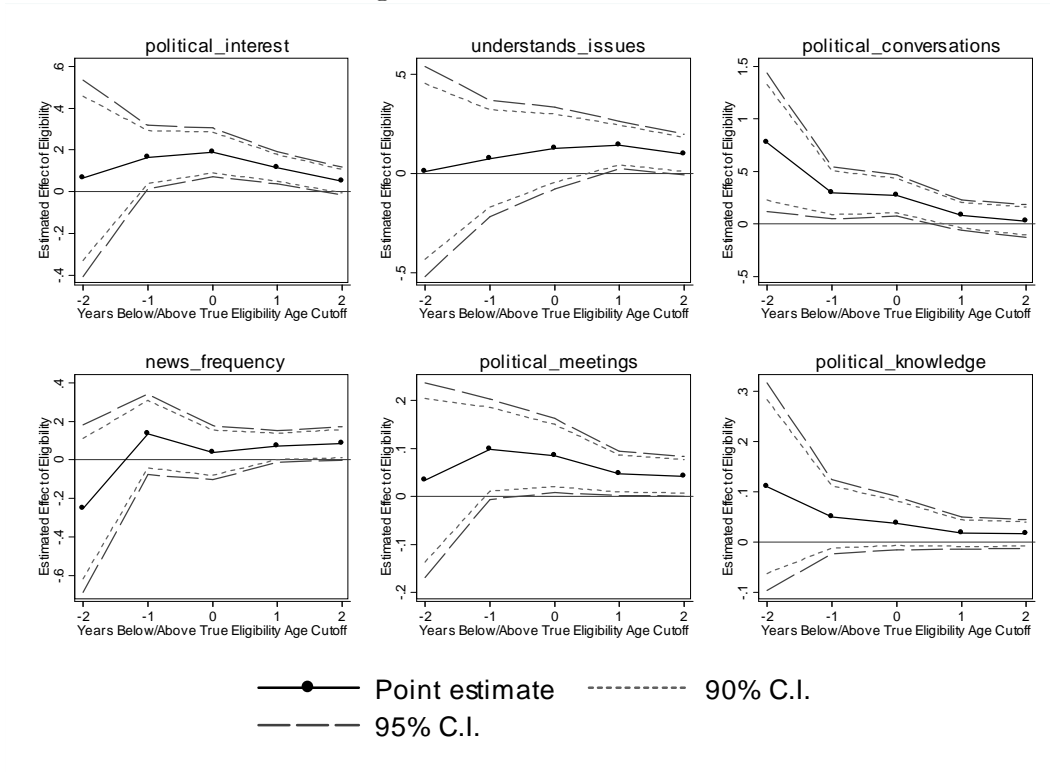
Notes: Plots the estimated coefficient for eligibility from dynamic regressions with country and year FE, controlling for age and income., using the full sample. Regressions manipulate the minimum voting age threshold by the amount on the x-axis. Standard errors clustered at the country-wave level.

Table 1: Country and Wave Coverage

Country	Waves
Antigua and Barbuda	2016
Argentina	2008-2016
Bahamas	2014
Barbados	2014
Belize	2008-2014
Bolivia	2004-2016
Brazil	2006-2016
Canada	2006-2016
Chile	2006-2016
Colombia	2004-2012, 2016
Costa Rica	2004-2008, 2012-2016
Dominica	2016
Dominican Republic	2004-2016
Ecuador	2004-2016
El Salvador	2004-2016
Grenada	2016
Guatemala	2004-2016
Guyana	2006-2016
Haiti	2006-2016
Honduras	2004-2016
Jamaica	2006-2016
Mexico	2004-2016
Nicaragua	2004-2016
Panama	2004-2016
Paraguay	2006-2016
Peru	2006-2016
Saint Kitts and Nevis	2016
Saint Lucia	2016
Saint Vincent and the Grenadines	2016
Suriname	2012-2014
Trinidad and Tobago	2010-2014
United States	2006, 2010-2016
Uruguay	2006-2016
Venezuela	2006-2016

Table 2: Variable Descriptions

Variable	Source	Description	Scale	Waves
Interest	LAPOP	How much interest do you have in politics?	Ordinal (1=None,4=A lot)	2006-2016
Understanding	LAPOP	You understand the most important political issues of this country. Do you agree with this statement?	Ordinal (1=Strongly Disagree,7=Strongly Agree)	2008-2016
Socialization	LAPOP	How often do you discuss politics with other people?	Ordinal (1=Never,5=Daily)	2006-2010[1]
News	LAPOP	How often do you pay attention to the news, whether on TV, radio, newspapers or internet?	Ordinal (1=Never,5=Daily)	2010-2016
Meetings	LAPOP	How often do you attend meetings of a political party or political organization?	Ordinal (1=Never,5=Once a week)	2004-2016
Knowledge	LAPOP	How many years is the presidential term of office in (country)?	Dummy (0=Incorrect,1=Correct)	2004-2016[2]
Age	LAPOP	How old are you? (Age at interview)	Ordinal (16-25)	2004-2016
Income Percentile	LAPOP	Created from question on range of household income. Percentile by country-wave.	Continuous (0.01-1.0)	2004-2016
Gap	LAPOP/IPU/ IDEA	Time between interview and election, created using date of interview from LAPOP and last election from IPU and IDEA.	Continuous (Days divided by 365)	2004-2016
Eligibility	LAPOP/IPU/ IDEA	Eligible to vote in last election.	Dummy (0=Ineligible,1=Eligible)	2004-2016
Ambiguous Eligibility	LAPOP/IPU/ IDEA	Voting eligibility status in last election cannot be determined with certainty.	Dummy (0=Unambiguous,1=Ambiguous)	2004-2016
Score	LAPOP/IPU/ IDEA	Age at interview - Gap - Minimum Voting Age	Continuous	2004-2016
Voluntary Voting	IDEA	No enforced mandatory voting law.	Dummy (0=Cumpolsory Voting,1=Voluntary)	2004-2016
Captured Press	FreedomHouse	Freedom of Press Index from 1(Most Free)-100(Least Free). Captured Press if FPI equal to or above that year's median.	Dummy (0=Free Press,1=Captured Press)	2004-2016

Notes: [1]Low coverage in 2010; [2]Low coverage in 2016

Table 3: Summary Statistics

Variable	Mean	S.D.	Min	Max	Countries	Obs
Interest	2.057	0.942	1	4	34	55,800
Understanding	3.833	1.709	1	7	34	46,106
Socialization	2.196	1.111	1	5	23	17,714
News	4.183	1.078	1	5	34	38,389
Meetings	1.221	0.602	1	4	26	55,916
Knowledge	0.834	0.372	0	1	27	43,817
Eligibility	0.767	0.423	0	1	34	54,844
Ambiguous Eligibility	0.115	0.320	0	1	34	62,005
Age	21.35	2.396	16	25	34	62,005
Income Percentile	0.441	0.295	0.01	1	34	49,443
Score	1.549	2.844	-5.995	8.767	34	62,005
Gap (years)	2.184	1.380	0.0219	5.995	34	62,005
Voluntary Voting	0.792	0.406	0	1	34	62,005
Captured Press	0.548	0.498	0	1	34	62,005

Notes: Number of observations varies across variables due to country coverage or question coverage in bi-annual surveys. Variable descriptions and data sources are in Table 1.

Table 4: Covariate Balance

	Full Sample		Cutoff Sample	
	Mean	Coefficient	Mean	Coefficient
Male	.4982	.0012	.5027	.0157
		(.009)		(.0103)
Single (Never married)	.708	.0206*	.7995	.0353***
		(.0128)		(.0121)
White	.2022	.0027	.1941	.0059
		(.0088)		(.0112)
Indigenous	.0645	.002	.0634	.0047
		(.0049)		(.0065)
Christian	.8105	-.0026	.8025	.0045
		(.0073)		(.0099)
Conservatism (1=Least,10=Most)	5.4424	.0893	5.459	.0801
		(.0663)		(.08)
Works	.4067	.0086	.3377	.0016
		(.0107)		(.0142)
Education (Years)	10.579	.4336***	10.469	.2373***
		(.0751)		(.0773)
Urban	.658	.0045	.657	-.0103
		(.0104)		(.0123)
Municipality Size	3.1496	-.0233	3.1373	.0156
		(.0429)		(.0501)

Notes: Estimates show the coefficient for voter eligibility from model with country and year fixed effects, controlling for age and income percentile. Standard errors clustered at the country-wave level. *** p<0.01,**p<0.05,*p<0.1.

Table 5: Full Sample Results

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.061**	0.188***	0.070	0.128	-0.045	0.270**	-0.001	0.037	0.018	0.085**	0.046***	0.038
	(0.024)	(0.06)	(0.043)	(0.105)	(0.054)	(0.100)	(0.034)	(0.072)	(0.018)	(0.039)	(0.012)	(0.027)
Eligibility × Gap		-0.046***		-0.033		-0.094***		-0.021		-0.022*		-0.011
		(0.017)		(0.029)		(0.030)		(0.019)		(0.013)		(0.007)
Gap		0.031		0.004		0.105***		0.003		0.018		-0.012
		(0.019)		(0.028)		(0.031)		(0.019)		(0.013)		(0.009)
Age	-0.009**	-0.007**	-0.008	-0.004	0.021***	-0.019**	0.024***	0.027***	0.004*	0.005**	0.001	0.005***
	(0.004)	(0.004)	(0.007)	(0.006)	(0.008)	(0.008)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)	(0.001)
Income Percentile	0.266***	0.266***	0.538***	0.538***	0.598***	0.597***	0.342***	0.343***	-0.012	-0.013	0.077***	0.077***
	(0.023)	(0.023)	(0.039)	(0.039)	(0.047)	(0.047)	(0.030)	(0.030)	(0.013)	(0.013)	(0.010)	(0.010)
Observations	40136	40136	33059	33059	13281	13281	26995	26995	40069	40069	30714	30714
Eligible (%)	77.6	77.6	76.9	76.9	77.8	77.8	77.4	77.4	77.0	77.0	78.4	78.4
Countries	34	34	34	34	23	23	34	34	26	26	27	27
Survey Period	2006-2016	2006-2016	2008-2016	2008-2016	2006-2010	2006-2010	2010-2016	2010-2016	2004-2016	2004-2016	2004-2016	2004-2016

Note: Regressions with country and year fixed effects. Standard errors clustered at the country-wave level. *** p<0.01,**p<0.05,*p<0.1.

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Table 6: Cutoff Sample Results

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.056**	0.438***	0.086	0.243	-0.042	0.346	-0.046	-0.028	0.003	0.215***	0.027**	0.025
	(0.025)	(0.111)	(0.058)	(0.255)	(0.051)	(0.235)	(0.045)	(0.164)	(0.022)	(0.072)	(0.011)	(0.055)
Eligibility × Gap		-0.046**		-0.048		-0.032		0.000		-0.011		-0.010
		(0.020)		(0.033)		(0.030)		(0.029)		(0.015)		(0.008)
Gap		0.161***		0.047		0.170		0.009		0.096***		-0.007
		(0.052)		(0.112)		(0.114)		(0.082)		(0.033)		(0.024)
Age	-0.008	-0.136**	-0.026	-0.038	-0.081***	-0.075	0.027	0.018	0.015	-0.076**	-0.011**	0.005
	(0.010)	(0.052)	(0.025)	(0.116)	(0.019)	(0.125)	(0.018)	(0.080)	(0.009)	(0.034)	(0.005)	(0.024)
Income Percentile	0.232***	0.233***	0.555***	0.556***	0.475***	0.477***	0.315***	0.315***	-0.035*	-0.035*	0.074***	0.074***
	(0.041)	(0.041)	(0.073)	(0.073)	(0.089)	(0.089)	(0.050)	(0.050)	(0.020)	(0.020)	(0.016)	(0.016)
Observations	9294	9294	7835	7835	2939	2939	6372	6372	9301	9301	6959	6959
Eligible (%)	58.3	58.3	57.5	57.5	58.6	58.6	58.2	58.2	57.4	57.4	58.3	58.3
Countries	34	34	34	34	23	23	34	34	26	26	27	27
Survey Period	2006-2016	2006-2016	2008-2016	2008-2016	2006-2010	2006-2010	2010-2016	2010-2016	2004-2016	2004-2016	2004-2016	2004-2016

Note: Regressions with country and year fixed effects. Standard errors clustered at the country-wave level. *** p<0.01,**p<0.05,*p<0.1.

Table 7: Modified Regression Discontinuity Results

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.090***	0.226***	0.043	0.125	0.177***	0.327***	-0.018	0.045	0.036*	0.083*	-0.006	0.031
	(0.033)	(0.060)	(0.063)	(0.112)	(0.053)	(0.093)	(0.053)	(0.083)	(0.021)	(0.044)	(0.014)	(0.026)
Eligibility × Gap		-0.048***		-0.030		-0.060**		-0.026		-0.017		-0.014*
		(0.017)		(0.030)		(0.026)		(0.022)		(0.013)		(0.007)
Gap		0.169**		-0.058		-0.228		-0.399		0.010		-0.030
		(0.067)		(0.171)		(0.354)		(0.250)		(0.081)		(0.040)
Score	-0.013	0.117*	0.029	-0.052	-0.089***	-0.360	0.002	-0.415*	-0.009	-0.012	0.022***	-0.018
	(0.013)	(0.069)	(0.022)	(0.178)	(0.021)	(0.349)	(0.018)	(0.250)	(0.010)	(0.084)	(0.006)	(0.041)
Age	-0.004	-0.132*	-0.037**	0.046	0.067***	0.343	0.025*	0.446*	0.012	0.016	-0.014***	0.027
	(0.010)	(0.069)	(0.017)	(0.175)	(0.017)	(0.347)	(0.013)	(0.247)	(0.008)	(0.083)	(0.005)	(0.041)
Income Percentile	0.222***	0.222***	0.465***	0.465***	0.578***	0.581***	0.336***	0.336***	-0.020	-0.020	0.079***	0.079***
	(0.029)	(0.029)	(0.050)	(0.050)	(0.060)	(0.060)	(0.036)	(0.036)	(0.016)	(0.016)	(0.012)	(0.012)
Observations	23051	23051	19240	19240	7438	7438	15692	15692	22990	22990	17210	17210
Eligible (%)	69.5	69.5	68.9	68.9	68.5	68.5	69.9	69.9	68.5	68.5	70.1	70.1
Countries	34	34	34	34	23	23	34	34	26	26	27	27
Survey Period	2006-2016	2006-2016	2008-2016	2008-2016	2006-2010	2006-2010	2010-2016	2010-2016	2004-2016	2004-2016	2004-2016	2004-2016

Note: Regressions with country and year fixed effects. Standard errors clustered at the country-wave level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8: Restricted Sample Regressions

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.186***	0.147**	0.134	0.085	0.270**	0.277**	0.045	0.045	0.086**	0.075*	0.037	0.049*
	(0.060)	(0.065)	(0.106)	(0.120)	(0.100)	(0.105)	(0.073)	(0.080)	(0.040)	(0.042)	(0.027)	(0.029)
Eligibility × Gap	-0.047***	-0.034*	-0.036	-0.018	-0.094***	-0.093***	-0.023	-0.024	-0.022*	-0.018	-0.011	-0.014*
	(0.018)	(0.019)	(0.029)	(0.031)	(0.030)	(0.030)	(0.020)	(0.022)	(0.013)	(0.013)	(0.007)	(0.008)
Gap	0.030	0.026	0.004	-0.001	0.105***	0.111***	0.005	0.010	0.018	0.018	-0.012	-0.009
	(0.019)	(0.021)	(0.028)	(0.031)	(0.031)	(0.031)	(0.019)	(0.020)	(0.013)	(0.014)	(0.009)	(0.009)
Age	-0.007*	-0.008*	-0.004	-0.003	0.019**	0.017*	0.027***	0.027***	0.005**	0.004*	0.006***	0.005***
	(0.004)	(0.004)	(0.006)	(0.007)	(0.008)	(0.009)	(0.005)	(0.005)	(0.002)	(0.002)	(0.001)	(0.001)
Income Percentile	0.268***	0.281***	0.546***	0.591***	0.597***	0.642***	0.341***	0.346***	-0.012	-0.009	0.077***	0.083***
	(0.024)	(0.026)	(0.039)	(0.042)	(0.047)	(0.048)	(0.031)	(0.035)	(0.013)	(0.014)	(0.010)	(0.010)
Observations	39150	32992	32312	26752	13281	11824	26142	21184	39718	35607	30630	26403
Excluded	US,Can	English	US,Can	English	US,Can	English	US,Can	English	US,Can	English	US,Can	English

Note: Regressions with country and year fixed effects. Restriction 1 refers to the full sample, excluding USA and Canada; Restriction 2 excludes USA, Canada, and English-speaking Caribbean. Standard errors clustered at the country-wave level. *** p<0.01,**p<0.05,*p<0.1.

Table 9: Alternative Controls

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.174***	0.176***	0.109	0.136	0.224**	0.253**	0.031	0.035	0.086**	0.080**	0.034	0.048*
	(0.058)	(0.057)	(0.109)	(0.097)	(0.100)	(0.099)	(0.076)	(0.073)	(0.039)	(0.037)	(0.027)	(0.025)
Eligibility × Gap	-0.046***	-0.042**	-0.036	-0.040	-0.082***	-0.088***	-0.024	-0.016	-0.022*	-0.020*	-0.011	-0.014**
	(0.017)	(0.017)	(0.030)	(0.028)	(0.029)	(0.028)	(0.020)	(0.020)	(0.013)	(0.012)	(0.007)	(0.007)
Gap	0.031*	0.028	0.004	0.007	0.103***	0.099***	0.005	-0.002	0.019	0.013	-0.011	-0.008
	(0.018)	(0.018)	(0.028)	(0.026)	(0.029)	(0.026)	(0.020)	(0.021)	(0.013)	(0.012)	(0.008)	(0.008)
Age	-0.007*	-0.004	-0.001	0.005	0.020**	0.022**	0.019***	0.029***	0.005**	0.006**	0.006***	0.006***
	(0.004)	(0.004)	(0.006)	(0.006)	(0.008)	(0.009)	(0.005)	(0.005)	(0.002)	(0.002)	(0.001)	(0.001)
Income Percentile	0.131***		0.310***		0.347***		0.226***		-0.048***		0.025**	
	(0.022)		(0.039)		(0.042)		(0.030)		(0.013)		(0.010)	
Male	0.098***		0.218***		0.225***		0.042***		0.059***		0.003	
	(0.011)		(0.021)		(0.026)		(0.015)		(0.009)		(0.004)	
Education	0.034***		0.054***		0.051***		0.039***		0.007***		0.013***	
	(0.002)		(0.004)		(0.005)		(0.003)		(0.001)		(0.001)	
Single - Never married	0.012		0.042*		0.010		-0.120***		0.001		0.017***	
	(0.012)		(0.023)		(0.024)		(0.018)		(0.008)		(0.005)	
Alt. Income Measure		0.068***		0.162***		0.126***		0.093***		-0.001		0.020***
		(0.007)		(0.013)		(0.013)		(0.010)		(0.005)		(0.004)
Observations	38745	43718	31956	37562	13152	13104	25867	30802	39282	41207	30337	32303

Note: Regressions with country and year fixed effects. Standard errors clustered at the country-wave level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

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Table 10: Naive Eligibility Definitions

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.139***	0.128***	0.076	0.052	0.149**	0.185**	0.035	-0.019	0.046*	0.073**	0.013	0.042**
	(0.037)	(0.044)	(0.068)	(0.075)	(0.072)	(0.078)	(0.043)	(0.053)	(0.024)	(0.031)	(0.016)	(0.018)
Eligibility × Gap	-0.038***	-0.040***	-0.018	-0.012	-0.058**	-0.072***	-0.018	-0.005	-0.013	-0.021*	-0.006	-0.013**
	(0.012)	(0.014)	(0.019)	(0.021)	(0.023)	(0.025)	(0.012)	(0.016)	(0.009)	(0.011)	(0.005)	(0.006)
Gap	0.020	0.022	-0.011	-0.016	0.071**	0.084***	0.000	-0.011	0.010	0.017	-0.018***	-0.011*
	(0.014)	(0.016)	(0.019)	(0.021)	(0.027)	(0.028)	(0.016)	(0.016)	(0.010)	(0.011)	(0.006)	(0.007)
Age	-0.006*	-0.002	-0.002	-0.000	0.017**	0.018***	0.027***	0.028***	0.005***	0.005***	0.006***	0.005***
	(0.003)	(0.003)	(0.006)	(0.006)	(0.007)	(0.007)	(0.005)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)
Income Percentile	0.270***	0.270***	0.530***	0.530***	0.600***	0.600***	0.353***	0.353***	-0.012	-0.012	0.076***	0.076***
	(0.023)	(0.023)	(0.038)	(0.038)	(0.045)	(0.045)	(0.031)	(0.031)	(0.013)	(0.013)	(0.010)	(0.010)
Observations	45339	45339	37245	37245	14991	14991	30481	30481	45310	45310	34636	34636
Strategy	1	2	1	2	1	2	1	2	1	2	1	2

Note: Regressions with country and year fixed effects. Strategy 1 defines eligibility by subtracting the difference between interview year and election year from age at interview and comparing to the voting age. Strategy 2 considers observations with Score greater than or equal to -0.5 as eligible. Standard errors clustered at the country-wave level. *** p<0.01,**p<0.05,*p<0.1.

Table 11: Ordered Probit

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.072***	0.230***	0.041	0.078	-0.023	0.309***	-0.011	0.034	0.075	0.245**	0.184***	0.206**
	(0.028)	(0.070)	(0.026)	(0.064)	(0.056)	(0.104)	(0.036)	(0.073)	(0.046)	(0.100)	(0.050)	(0.104)
Eligibility × Gap		-0.056***		-0.020		-0.098***		-0.023		-0.061*		-0.067**
		(0.020)		(0.018)		(0.029)		(0.019)		(0.031)		(0.028)
Gap		0.039*		0.003		0.112***		0.005		0.041		-0.037
		(0.022)		(0.017)		(0.028)		(0.020)		(0.031)		(0.031)
Age	-0.010**	-0.008**	-0.005	-0.002	0.021***	0.018**	0.030***	0.032***	0.010	0.013**	0.007	0.026***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)
Income Percentile	0.312***	0.312***	0.329***	0.329***	0.607***	0.607***	0.371***	0.371***	-0.042	-0.042	0.364***	0.369***
	(0.027)	(0.027)	(0.025)	(0.025)	(0.045)	(0.045)	(0.035)	(0.034)	(0.032)	(0.032)	(0.048)	(0.048)
Observations	40136	40136	33059	33059	13281	13281	26995	26995	40069	40069	30714	30714

Note: Regressions with country and year fixed effects, using the full sample. Standard errors clustered at the country-wave level. *** $p<0.01$,** $p<0.05$,* $p<0.1$

Table 12: RD Results With Varying Slopes

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.102**	0.211***	0.029	0.100	0.228**	0.324***	-0.031	0.019	0.045	0.081*	-0.021	0.013
	(0.049)	(0.062)	(0.089)	(0.119)	(0.086)	(0.104)	(0.082)	(0.093)	(0.030)	(0.045)	(0.021)	(0.028)
Eligibility × Gap		-0.051***		-0.034		-0.061**		-0.030		-0.018		-0.017**
		(0.018)		(0.030)		(0.026)		(0.023)		(0.014)		(0.007)
Gap		0.171**		-0.055		-0.227		-0.397		0.011		-0.027
		(0.067)		(0.171)		(0.357)		(0.251)		(0.081)		(0.040)
Score	-0.020	0.131*	0.037	-0.030	-0.119**	-0.357	0.010	-0.394	-0.015	-0.010	0.031**	-0.001
	(0.024)	(0.071)	(0.045)	(0.178)	(0.047)	(0.361)	(0.037)	(0.256)	(0.016)	(0.084)	(0.012)	(0.041)
Eligibility × Score	0.009	-0.017	-0.010	-0.027	0.037	-0.003	-0.009	-0.026	0.007	-0.003	-0.010	-0.020*
	(0.023)	(0.023)	(0.045)	(0.047)	(0.048)	(0.048)	(0.035)	(0.033)	(0.016)	(0.017)	(0.012)	(0.012)
Age	-0.004	-0.132*	-0.036**	0.047	0.066***	0.342	0.025*	0.448*	0.012	0.016	-0.014***	0.028
	(0.010)	(0.069)	(0.017)	(0.175)	(0.016)	(0.347)	(0.013)	(0.247)	(0.008)	(0.083)	(0.005)	(0.041)
Income Percentile	0.222***	0.222***	0.465***	0.465***	0.578***	0.581***	0.336***	0.336***	-0.020	-0.020	0.079***	0.079***
	(0.029)	(0.029)	(0.050)	(0.050)	(0.060)	(0.060)	(0.036)	(0.036)	(0.016)	(0.016)	(0.012)	(0.012)
Observations	23051	23051	19240	19240	7438	7438	15692	15692	22990	22990	17210	17210

Note: Regressions with country and year fixed effects. Standard errors clustered at the country-wave level. *** p<0.01,**p<0.05,*p<0.1

Table 13: Institutional Interactions - Mandatory Voting Laws

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.125**	0.172***	0.160***	0.129*	0.132***	-0.020	0.030	-0.018	0.001	0.005	0.046*	0.037*
	(0.054)	(0.046)	(0.056)	(0.070)	(0.044)	(0.048)	(0.042)	(0.065)	(0.026)	(0.023)	(0.023)	(0.021)
Eligibility × Voluntary	-0.074	-0.140***	-0.106*	-0.054	-0.201***	-0.027	-0.035	-0.030	0.019	-0.003	-0.000	-0.012
	(0.056)	(0.049)	(0.057)	(0.076)	(0.054)	(0.069)	(0.041)	(0.068)	(0.028)	(0.025)	(0.023)	(0.022)
Voluntary	0.119	0.232*	-0.068	-0.190	0.000	0.000	0.129**	0.283**	-0.103**	-0.069	0.076**	0.063
	(0.108)	(0.123)	(0.137)	(0.316)	(.)	(.)	(0.064)	(0.129)	(0.043)	(0.048)	(0.035)	(0.045)
Observations	40136	9294	33059	7835	13281	2939	26995	6372	40069	9301	30714	6959
Sample	Full	Cutoff	Full	Cutoff	Full	Cutoff	Full	Cutoff	Full	Cutoff	Full	Cutoff

Note: Regressions with country and year fixed effects, controlling for age at interview and income percentile. All specifications use the static model. Voluntary defined as the country not having an enforced mandatory voting law in place at the time of the most recent election. Standard errors clustered at the country-wave level. *** p<0.01,**p<0.05,*p<0.1.

Table 14: Institutional Interactions - Freedom of the Press

	Interest		Understanding		Socialization		News		Meetings		Knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligibility	0.066**	0.073*	0.057	0.085	-0.067	-0.087	0.020	-0.005	0.017	0.020	0.039***	0.032**
	(0.031)	(0.038)	(0.056)	(0.070)	(0.054)	(0.088)	(0.037)	(0.053)	(0.021)	(0.029)	(0.014)	(0.014)
Eligibility × Captured Press	-0.009	-0.029	0.021	0.001	0.034	0.042	-0.034	-0.076	0.002	-0.028	0.011	-0.006
	(0.034)	(0.043)	(0.060)	(0.080)	(0.070)	(0.094)	(0.041)	(0.063)	(0.023)	(0.028)	(0.016)	(0.016)
Captured Press	0.023	0.102*	-0.083	-0.145	0.015	0.145*	0.129*	0.139	-0.029	0.015	-0.019	-0.024
	(0.044)	(0.058)	(0.097)	(0.106)	(0.062)	(0.078)	(0.076)	(0.084)	(0.041)	(0.041)	(0.024)	(0.023)
Observations	40136	9294	33059	7835	13281	2939	26995	6372	40069	9301	30714	6959
Sample	Full	Cutoff	Full	Cutoff	Full	Cutoff	Full	Cutoff	Full	Cutoff	Full	Cutoff

Note: Regressions with country and year fixed effects, controlling for age at interview and income percentile. All specifications use the static model. Standard errors clustered at the country-wave level. *** p<0.01, **p<0.05, *p<0.1.