

# Universities & Political Participation: Measuring Impacts of the 1998 Reauthorization of the Higher Education Act of 1965

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## **Abstract**

I study the impacts of university-led signals on political participation. The 1998 reauthorization of the Higher Education Act mandated that universities in states without same-day voter registration make attempts to register students to vote by distributing voter registration forms via mail. Using a difference-in-differences design and a fixed effects model, I compare college-age populations in states whose universities had new mandates to register students to vote to college-age populations in states without mandates. I record a modest 3.6 percentage point increase in voter registration but a 3.8 percentage point decrease in voter turnout. I end with a discussion about shortcomings of my model as well as implications for government policy.

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# 1 Introduction

In the post-2010s United States, there has been serious conversation surrounding whether young people should vote, what the voting age should be, and whether university students who move from one state, say California, to another “swing” state, say Wisconsin, should be able to vote in Wisconsin. Most recently in the 2024 presidential primaries, Hanover and Dartmouth received national attention because although former President Trump won the state of New Hampshire, Nikki Haley won Hanover handily (Tabet [2024](#)). On Twitter and elsewhere, pundits posited that college students and other college-educated individuals at Dartmouth who would ordinarily vote Democrat voted Republican instead to diminish Trump’s margins. This setting provides fertile ground for understanding how the presence of universities facilitates the voting process. Since I cannot analyze 2024 voting data due to voter privacy and data collection concerns, I now turn to the first major act mandating universities to play an active role in the electoral process: the 1998 Reauthorization of the Higher Education Act of 1965.

The 1998 reauthorization of the Higher Education Act of 1965 included a mandate that universities in states without same-day voter registration make an effort to register students to vote by mailing them ballots. This mandate was enforced by a \$25,000 fine for every violation. In 1998, only six states had same-day voter registration: Idaho, Maine, Minnesota, New Hampshire, Wisconsin, and Wyoming. One state, North Dakota, had no voter registration. Every other state did not have same-day voter registration. Because this mandate targeted universities in states with specific criteria, it generated a quasi-experiment. I use a difference-in-differences research design to study the impacts of this mandate on voter registration and turnout among college-age individuals. My treatment group is my population of interest in states with the mandate. My comparison group is my population of interest in states without the mandate. My pre-period comprises elections from 1988 to 1996, and my post-period includes elections from 2000 to 2008. I focus on presidential elections because turnout is typically depressed during midterm elections.

Applying a robust fixed-effects model to data from the Current Population Survey’s Voting and Registration Supplement (CPS VRS) provided by the Integrated Public Use Microdata Series (IPUMS), my estimates provide mixed findings for treatment states in the post-treatment period. Specifically, I record a 3.6 percentage point increase in voter registration and a 3.2 percentage point decrease in voter turnout in states of interest after the 1998 reauthorization. My estimates of voter registration are only mildly significant, while my voter turnout estimate is significant at the 99% level. I discuss these findings as well as implications for future policy in my results section. The rest of the paper is organized thus: section 2 gives context and a review of literature surrounding voting. Section 3 is a discussion of my research methodology. I discuss my data in section 4, followed by my findings in section 5. Section 6 is my conclusion. Relevant figures and tables are in section 7, and I conclude with an appendix.

## 2 Background and Literature Review

### 2.1 General Political Participation Literature

The literature on political participation spans sociology, economics, behavioral science, and political science, and studies how voting is constructed, what causes candidates to run, how elections are conducted, and, most pertinent to this paper, what gets people to register to vote and actually show up. In the United States, despite having an aging population, young people between the ages of 18 to 29 comprise one of the largest age groups (Goldstein 2023). But data from Statista summarizing voter turnout shows inconsistent turnout across young people: in the past thirty years, it has ranged from a low of 39.6% in 1996 to a high of 55% in 2020 (*Youth voter turnout in presidential elections in the U.S. 1972-2020* 2023). Notably, turnout among older people (50+ years) has for long resided in the high 60%, eclipsing every other age range.

There is a rich history of political scientists and economists attempting to understand what gets people to go out and vote and how government policies affect voting. In

their landmark work *Who Votes* published in 1980, Wolfinger and Rosenstone identified key markers of difference between voters and non-voters, finding that educated people voted more, as did married people and older people, and that income did not play as significant a role in predicting voting behavior as the aforementioned three characteristics. They also found that lowering barriers to voter registration was associated with higher turnout levels. But their study, conducted in the 1970s, suffered from limitations in methodological advancement and was more descriptive than explanatory. Following their work, other scholars have looked into different motivators of or hindrances to turnout and the ways that other pieces of government legislation have affected voting. For example, Cascio and Washington (2014) leveraged the passage of the Voting Rights Act of 1965 to study whether increased black enfranchisement led to higher black turnout and increased state transfers. While they used a research design (triple difference-in-differences) similar to this paper’s, they focused on a different population. Alvarez, Bailey, and Katz (2008) found that harsher voter identification laws lead to lower turnout, especially among minoritized populations.

Further still, work on voter registration by scholars such as Brians and Groffman (2001), Leighley and Nagler (2014), Nickerson 2015, Ansolabehere and Konisky 2006 all find a strong positive relationship between the decrease of barriers to registering to vote and an increase in both registration and turnout rates. These studies leveraged a variety of research designs ranging from random assignment (Nickerson, 2015) to a before-and-after comparison (Brians and Groffman, 2001). The case of random assignment featured a limited sample without wide applicability, while Brians and Groffman’s estimates, although accounting for state fixed effects, did not follow the more widely accepted difference-in-differences approach (which was doubly crucial given that the states they studied have specific similarities—rural, cold, etc.). Ansolabehere and Konisky looked within states for their estimates. My study, on the other hand, uses a broader, more rigorous research design, as well as treatment and comparison groups that span geographies.

## 2.2 Political Participation by Young People

The subfield of political participation research focused on turnout among young people is small but growing. Holbein and Hillygus (2016) found that preregistration—where young voters register before 18 and get automatically added to voter rolls—led to increases in voter turnout. They further exploited a discontinuity in birth dates to estimate the effects of varying exposure to preregistration in Florida on turnout. Similarly, Bertocchi et al. (2020) validated Holbein and Hillygus’s results and also found that this emergence of a newly voting population was associated with increases in government spending on items of interest to the new electorate. Grumbach and Hill (2022) used a difference-in-differences approach to find that same-day registration’s increases of voter turnout were pronounced among younger people (ages 18-24). Their age group under consideration is similar to mine but our questions are fundamentally different because the university functions as my locus.

On the subject of university-led efforts, Bogard et al. (2008) found that a university-led voter registration and sensitization campaign led to significant increases in voter registration rates and voter turnout. However, their study was limited to Hofstra University students, raising questions about external validity. Similarly, Bennion and Nickerson (2016) employed a field experiment on a large sample size of students across different college campuses to study the impacts of exposure to pro-political participation presentations on student registration and turnout. Their study found a positive causal relationship. However, their study differs from mine because they looked at researcher-led interventions, whereas I am studying nationwide mandates. Their follow up study (2021) focused on college students but studied online registration systems instead. Bergan et al. (2021) used random assignment to study the roles of information cues and social pressure on driving turnout, finding a positive relationship. However, their study was limited to a small population sample and focused on classroom-targeted effects.

The two studies most crucial to mine are Bertocchi et al.’s paper and Grumbach

and Hill’s paper. Bertocchi et al. use a difference-in-differences approach to study pre-registration, where younger voters can register to vote before they reach the age of eligibility. They extend their methodology using a triple differences framework with older voters as an additional comparison group in order to prevent state-specific shocks from biasing their estimates. Their staggered approach to the difference-in-differences design is helpful in isolating differential timing of treatment. In handling CPS VRS data, they correct the discrepancies in the voter registration and voter turnout rates by coding every voter as registered to vote. Their other strategies—restricting the sample to observations between the ages of 18 and 90, and the population of interest to observations between the ages of 18 and 24—will be helpful in my analysis. The second study, Grumbach and Hill’s, also focuses on the age range of 18-24 and employs a difference-in-differences approach that looks at changes *within* states across time in order to protect for “time-invariant characteristics of states that may affect both SDR and turnout” (Grumbach and Hill 2022). From both studies’ strategies, I adopt state and year fixed effects to control for state- and year- specific changes that might affect my estimates.

## 2.3 Contributions

My study contributes to the literature in a few key ways. To my knowledge, while there have been papers looking at political involvement among young people, no studies have focused on the role universities play at a larger scale, especially when viewed through the context of the 1998 reauthorization of the Higher Education Act. While my difference-in-differences design is extremely common across election studies, my paper also addresses the questions of validity across a wider range of areas by looking at a federal law. These research design strategies enable my results to have broader applicabilities. Finally, my findings have rich policy implications: if the United States wants to bolster political participation by young people, it is crucial to understand the significance, if any, of universities in fostering political participation.



### 3 Methods

I employ a difference-in-differences research design to study the impacts of university-led voter registration efforts on two specific outcomes—voter registration and voter turnout—among university-age individuals who are eligible to vote.

#### 3.1 Simple Difference in Differences

My focus is on presidential elections from 1988 to 2008. My cut off is at 2008 because that year marked a shift from compulsory physical mailing of voter registration forms to an option that allowed universities to choose between physically mailing forms or sending them via email. I focus on presidential elections because turnout is typically depressed during midterms. My pre-period encompasses all years before the reauthorization year, 1998, while my post-period encompasses years after 1998. I exploit the differences in voter registration policy across states as at the time of passage to isolate the impacts of the 1998 Reauthorization.

My population of interest is voting-eligible populations (U. S. citizens, dependent on incarceration status for some states) currently enrolled in a 2- or 4-year college or university. My simple difference-in-differences estimation equation is as follows:

$$Y_{ist} = \beta_1 \text{afterRA}_t \cdot \text{hadMandate}_s + \beta_2 \text{afterRA}_t + \beta_3 \text{hadMandate}_s + \beta_4 X_{ist} + \varepsilon_{ist} \quad (1)$$

#### 3.2 Fixed Effects

I leverage a fixed effects model for my second set of estimations. I include year fixed effects to control for year-specific shocks, and I include state fixed effects to control for unobservable characteristics or changes in states that may bias my estimates. Both my post-period and treatment dummies are dropped from this model because they are collinear with time and state fixed effects, respectively.

My fixed effects estimation equation is

$$Y_{ist} = \beta_1 \text{afterRA}_t \cdot \text{hadMandate}_s + \beta_2 \text{stateFE}_s + \beta_3 \text{yearFE}_t + \beta_4 X_{ist} + \varepsilon_{ist} \quad (2)$$

### 3.3 Model Interpretation

Across both models, my dependent variable  $Y_{ist}$  is a dummy assigned to 1 if individual  $i$  in state  $s$  voted in year  $t$  and 0 otherwise (replace ‘voted’ with ‘registered to vote’ for registration rates).  $\text{afterRA}$  is a dummy that equals 1 in an election year after the 1998 re-authorization of the Higher Education Act and 0 in years before the re-authorization.  $\text{hadMandate}$  is a dummy that takes on a value of 1 in states subject to the federal government’s mandate, i.e. states with voter registration requirements but not same day voter registration, and 0 otherwise.  $\beta_3$  is the coefficient of the interaction term, capturing the outcomes in treatment states in the post-treatment period. This interaction of my key variables of interest will provide results of the estimation.  $X_{ist}$  is a vector for controls likely to influence voter participation such as income, race, gender, and age.  $\varepsilon_{ist}$  is the error term, which is clustered at the state level.

### 3.4 Validity of Estimates

Under a few assumptions, my estimates of  $\beta_3$  may be considered causal. The first assumption is that without the treatment (the mandate applied differentially based on statewide electoral policy), trends in registration and turnout would have been parallel. I present summary statistics for the pre-period and post-period across treatment and comparison groups in my data section. A second necessary, but not sufficient, assumption is that targets supposed to be treated actually complied with the treatment, i.e. that higher education institutions actually mailed out voter registration forms. Because of a \$25,000 fine for each violation of the mandate, it is reasonable to assume that colleges and universities were compelled to comply.

## 4 Data

My data comes from the Current Population Survey (CPS) Voting and Registration Supplement (VRS) compiled by the Integrated Public Use Microdata Series (IPUMS). The IPUMS CPS VRS is collected biennially when the November monthly survey is conducted. Its variables—in addition to typical CPS data set characteristics such as age, education, and labor force participation—include whether respondents registered to vote or not, whether they voted or not, and how. My data spans the elections from 1988 to 2008. The relevant universe for the data set includes American citizens who are eligible to vote. The data do not specify whether respondents voted for one office or down ballot, or what the respondent’s party affiliation was. Every observation in the data set is a person.

I construct key dummy variables in my larger data set in order to more easily create subsamples. I generate *iscitizen*, which equals 1 if an observation is a U.S. citizen and 0 otherwise. Other key dummy variables are *incollege*, *whiteshr*, *ishispan*, *ismarried*, *iswoman*, *isemployed*. These indicate whether an individual is in college full or part time, whether they are white or not, whether they are hispanic or not, their marital status, their sex, and their labor force participation status. My key outcome variables (dependent) are dummies for voter registration and voter turnout: *didvotereg*, *didvote*. These dummies are binaries that capture whether individuals registered to vote and whether they voted. Finally, for my difference-in-differences, I construct two dummy variables: a dummy *hadmandate* equal to 1 if a state had to follow the HEA reauthorization’s mandate (i.e. had no same day voter registration) and 0 otherwise; and a dummy *afterRA* equal to 1 for years after the 1998 reauthorization and 0 otherwise. Because I am interested in looking at presidential elections, my dummy *ispres* equals 1 in presidential election years and 0 otherwise.

My dependent variables and outcomes of interest are dummies for voter registration and voter turnout. Some shortcomings in my data set are: 1) There was no citizenship question before 1994. My remedy for this is assuming that the CPS’s labelling of voting

status assigns “not in universe” to people unable to vote, including non-citizens in years prior to 1994. 2) The variable *schcoll* does not accurately present all folks enrolled in college or university. First, the CPS does not survey college dorms as residential spaces. Next, the survey question asks respondents whether they attended classes the week before. They state: “College or high school students who were currently on holiday or seasonal vacation were to answer yes, but those not taking classes during summer vacation were to answer no.” Since this question cannot accurately reflect enrollment, I construct a variable *isage* (a binary variable) that identifies individuals aged between 18 and 25. Further, I construct a variable *targpop* which looks within my target age range and identifies individuals with at least a high school diploma but no undergraduate or graduate degree. This target population is my strongest identification of the population likely to be influenced by the 1998 reauthorization’s mandate.

I present two tables of means. Table 1 includes all observations in the voting-eligible universe. Table 2 includes individuals in my target population, which I am using as a proxy for attending college. Standard errors are in parentheses. I have weighted observations using the voter supplement weights provided by the CPS. In both tables, columns 1 and 2 show state-level means in the pre-treatment period for states without the mandate and states that would be under the mandate. Columns 3 and 4 show means in the post-treatment period. Column 5 shows ordinary-least-squares difference-in-differences for each mean. I cluster at the state level. In Table 1, the difference-in-differences coefficient for hispanics is significant. However, this is not the case in my target population. Table 2 suggests that the parallel trends assumption holds across all means, which should provide a suitable precondition for difference-in-differences estimates focusing on my group of interest. Regardless of this, I utilize all the averaged variables as controls in my regressions to see whether their effects on turnout and registration match existing electoral studies. Finally, figures illustrating my parallel trends (Figure 5 and Figure 6) are in the appendix. These and all other figures and graphs are calculated for the target population and applied VRS weights.

## 5 Results

### 5.1 Findings

My results are presented in the Table 3. Standard errors are clustered at the state level and calculated robust to heteroskedasticity. I apply weights provided by the Voting and Registration Supplement. My estimates are calculated for the target population, that is individuals between the ages of 18-25 who have a high school diploma but neither a college nor a graduate degree.

Columns 1 and 2 are ordinary least-squares regressions for voter turnout and voter registration, respectively, without any controls. Columns 3 and 4 are OLS regressions for turnout and registration, respectively, with controls clustered on county. Columns 5 and 6 represent my robust fixed effects model. In column 5, the coefficient of the interaction term for voter turnout, *treat* x *post* suggests that the 1998 reauthorization was associated with a decrease in voter turnout by 3.2 percentage points in states under the mandate. Column 6 records the coefficient of the interaction term for voter registration as 0.0361. This suggests that the 1998 reauthorization was associated with a 3.61 percentage point increase in voter registration in states under the mandate. Figures 1 and 2 show trends in turnout and registration, respectively, over time.

My coefficient for turnout is statistically significant at the 99% level, while my coefficient for registration is only significant at the 90% level. The coefficients on my controls—collapsed dummies for female, white share, employment status, hispanic identity—support other electoral studies which have found that women vote at higher rates than men, whites vote at higher rates compared to non-whites, and employed individuals vote more than unemployed individuals. With fixed effects, my R-squared statistics improve for both turnout and registration. However, the low R-stat (0.028 for turnout, 0.019 for registration) suggests that the predictive power of my model is low even with fixed effects.

## 5.2 Event Study Plot

Figures 3 and 4 show difference-in-differences estimates of the interaction between election year and states under the mandate. I included the same controls as before: female, white share, employment status, and hispanic share; as well as year and state fixed effects.

The event study plot is consistent with the regression results which show a decrease in voter turnout in states under the mandate in the post-treatment period as well as an increase in registration rates in the same context. Note the different axes of the figures when reading results.

## 5.3 Potential Shortcomings and Future Directions

One of the weaknesses of my identification strategy is that I cannot thoroughly narrow down who is a college student and who is not. Specifically, it is possible that my sample—18-25 years old who have a high school diploma but neither an undergraduate nor a graduate degree—includes individuals not enrolled in college or university. College enrollment in the United States has been in the decline, with figures as low as 44% in recent years.

To further narrow my contrast of treatment to comparison state, I identify neighboring states for each comparison state: Montana, Vermont, Iowa, Illinois, and South Dakota. My estimation finds similar decreases in voter turnout (-5.2 percentage points) and increases in registration (4.1 percentage points) both significant at the 90% level. I run additional regressions for my non-target population. Estimates are not significant.

## 6 Conclusion

My estimation of the impacts of the 1998 reauthorization of the Higher Education Act of 1965 shows that there is an associated increase in voter registration by approximately 3.6 percentage points and decreases in turnout by 3.8 percentage points. Although the

predictive power of my model is low, my study provides some evidence that mandating universities to facilitate voter registration may have yielded increases in registration rates (in tune with other studies focusing on more direct effects) but may not have been sufficient enough to record actual impacts in turnout. Existing literature suggests that the most impactful way of bolstering turnout remains reducing barriers to voting, including requiring registration.

## 7 Figures and Tables

Variable	Before the 1998 reauthorization		After the 1998 reauthorization		Difference in differences
	States without mandate	States under mandate	States without mandate	States under mandate	
<b>Age</b>	43.4289 (18.018)	43.7438 (18.1394)	46.162 (17.5201)	46.263 (17.6318)	-0.214 (0.170)
<b>Target population</b>	0.1571 (.3639)	0.157 (.3638)	0.1415 (.3485)	0.1426 (.3497)	-0.00170 (0.00370)
<b>White</b>	0.9636 (.1873)	0.8502 (.3568)	0.9425 (.2327)	0.8237 (.381)	-0.00546 (0.00677)
<b>Hispanic</b>	0.0119 (.1084)	0.0593 (.2362)	0.0236 (.1518)	0.0875 (.2826)	0.0165 (0.00757)
<b>Married</b>	0.6023 (.4894)	0.5738 (.4945)	0.6001 (.4899)	0.5616 (.4962)	-0.0101 (0.00837)
<b>Employed</b>	0.716 (.451)	0.663 (.4727)	0.7277 (.4452)	0.6728 (.4692)	-0.00188 (0.00465)
<b>Female</b>	0.5104 (.4999)	0.525 (.4994)	0.5119 (.4999)	0.5231 (.4995)	-0.00348 (0.00251)
<b>In college (according to CPS)</b>	0.033 (.1787)	0.0293 (.1686)	0.0472 (.212)	0.0479 (.2135)	0.00443 (0.00342)
<b>Number of observations</b>	25511	265209	31872	239368	561,516

Table 1: Pre-treatment and post-treatment means for states with mandates and states without mandates, all populations



Variable	Before the 1998 reauthorization		After the 1998 reauthorization		Difference in differences
	States without mandate	States under mandate	States without mandate	States under mandate	
Age	21.4797 (2.162)	21.5032 (2.2187)	21.342 (2.1672)	21.379 (2.1746)	0.0135 (0.166)
White	0.958 (.2007)	0.8215 (.3829)	0.915 (.2789)	0.7865 (.4098)	0.00783 (0.00926)
Hispanic	0.011 (.1042)	0.0744 (.2624)	0.0364 (.1874)	0.1271 (.3331)	0.0273 (0.0173)
Married	0.1751 (.3801)	0.1905 (.3927)	0.1336 (.3402)	0.1371 (.344)	-0.0118 (0.0177)
Employed	0.799 (.4009)	0.7485 (.4339)	0.8161 (.3875)	0.725 (.4465)	-0.0407 (0.0221)
Female	0.5068 (.5001)	0.5197 (.4996)	0.4889 (.5)	0.5033 (.5)	0.00147 (0.00790)
In college (according to CPS)	0.303 (.4596)	0.2775 (.4478)	0.3999 (.4899)	0.4182 (.4933)	0.0438 (0.0312)
Number of observations	2459	23994	3222	22831	52,410

Table 2: Pre-treatment and post-treatment means for states with mandates and states without mandates, target population

	(1)	(2)	(3)	(4)	(5)	(6)
	Voter turnout	Voter registration	Voter turnout	Voter registration	Voter turnout	Voter registration
treat x post	-0.0360** (0.0179)	0.0397** (0.0165)	-0.0327** (0.0129)	0.0423** (0.0200)	-0.0381*** (0.0118)	0.0361* (0.0209)
Female			0.0465*** (0.00661)	0.0414*** (0.00539)	0.0465*** (0.00622)	0.0411*** (0.00521)
White			0.0169 (0.0106)	0.0253** (0.0107)	0.0146 (0.0107)	0.0249** (0.00994)
Employed			0.0228*** (0.00466)	0.0188*** (0.00525)	0.0266*** (0.00470)	0.0194*** (0.00500)
Hispanic			-0.0956*** (0.0130)	-0.0778*** (0.0109)	-0.0924*** (0.00863)	-0.0776*** (0.00994)
Had Mandate	-0.0807*** (0.0135)	-0.119*** (0.0120)	-0.0718*** (0.0248)	-0.110*** (0.0314)		
After 1998	0.0278 (0.0172)	-0.0631*** (0.0157)	0.0314*** (0.00925)	-0.0596*** (0.0179)		
Constant	0.535*** (0.0130)	0.717*** (0.0115)	0.478*** (0.0255)	0.657*** (0.0323)	0.486*** (0.0146)	0.523*** (0.0225)
Year FE?	No	No	No	No	Yes	Yes
County FE?	No	No	No	No	Yes	Yes
Clustering?	No	No	Yes	Yes	Yes	Yes
Observations	52,410	52,410	52,410	52,410	52,410	52,410
R-squared	0.002	0.003	0.008	0.007	0.028	0.019

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 3: OLS and fixed effects regression estimates

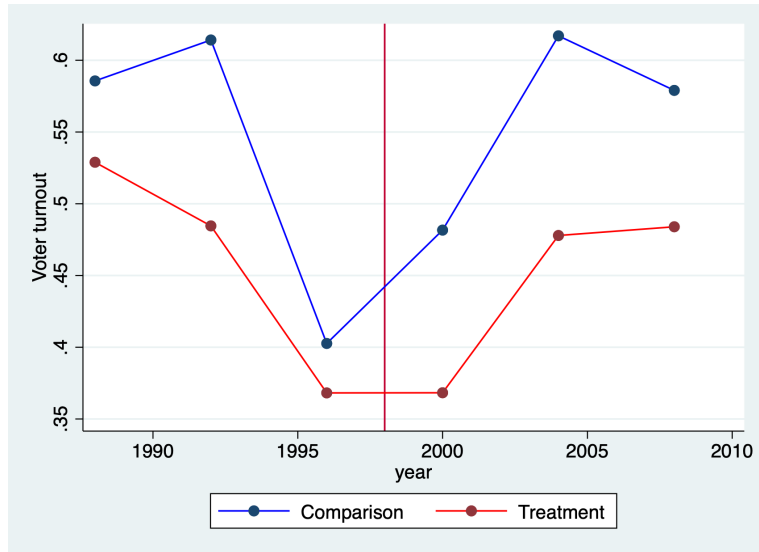


Figure 1: Voter turnout over time

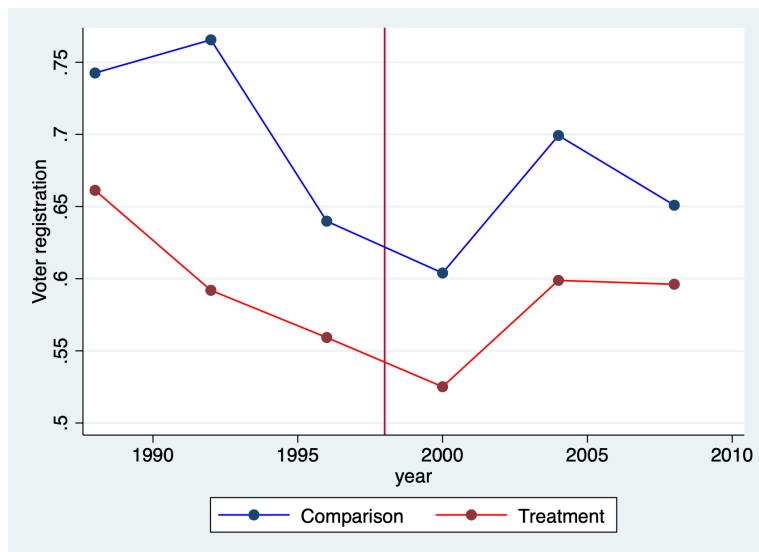


Figure 2: Voter registration over time

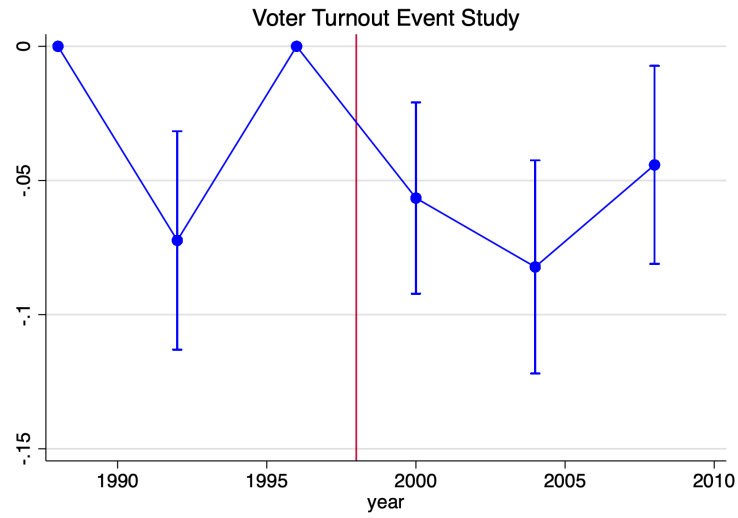


Figure 3: Event study estimates of voter turnout by year

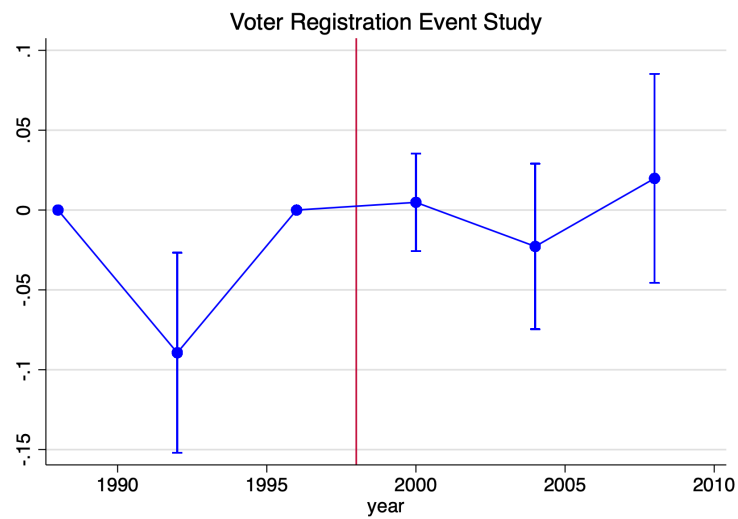


Figure 4: Event study estimates of voter registration by year

## 8 Appendix

### 8.1 Parallel Trends

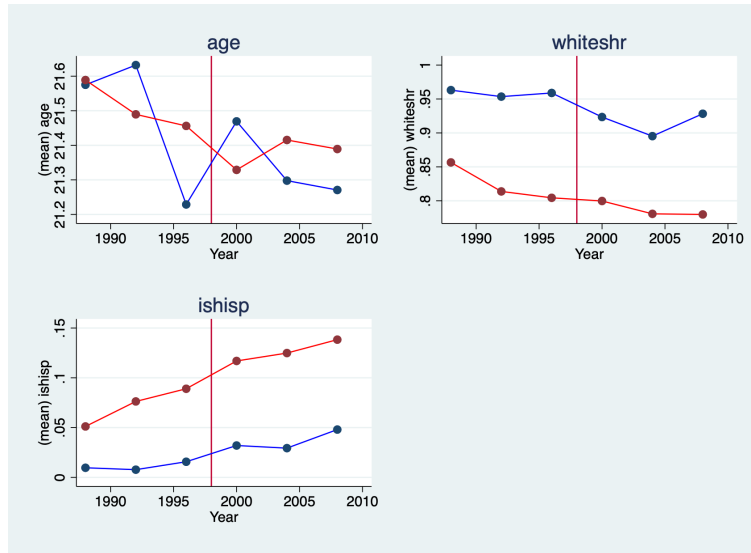


Figure 5: Parallel trends graphs for selected characteristics of the target population. CPS VRS weights applied.

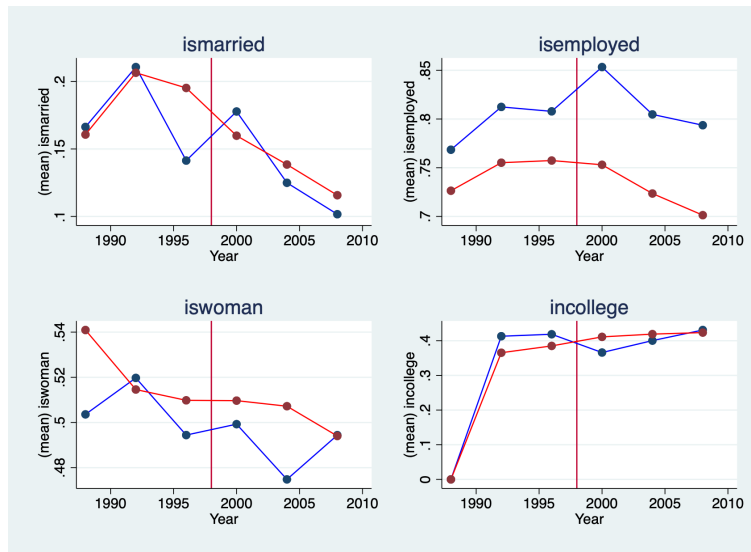


Figure 6: Parallel trends graphs for selected characteristics of the target population. CPS VRS weights applied.

## References

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