Registration Effects of Automatic Voter Registration in the United States

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In recent years, a number of states have passed some version of automated voter registration (AVR). Implementation varies, but the core idea is to more aggressively promote voter registration as an option at the Department of Motor Vehicles. Evaluation of the impact of AVR has been limited thus far, mostly because AVR implementation has itself been limited. But post-reform data are now available for a number of states, and others that are considering adopting the reform are wondering what the effects of the laws have been in these states. What would registration and turnout have looked like in AVR states had the reform not been implemented? In this paper we take advantage of the first election cycle with significant post-AVR data across a range of states to explore the registration effects of AVR. We employ both difference-in-differences and synthetic control approaches to identify causality. Registration effects so far appear to have been solid overall and larger for Latinos. Evidence for Asian Americans and young people is more ambiguous, as is evidence for effects in individual states. We conclude with thoughts on future directions to help develop better estimates in these areas.

Automatic voter registration (AVR) laws take advantage of transactions at government agencies where applicant information can be captured and repurposed to register citizens to vote. Implementation varies, but the core idea is always to make eligible residents actively decline registration if they do not want it, rather than passively as before. The reform movement is still quite young, so there is little information about how effective AVR has been in the U.S. context. Oregon was the first to adopt a law generally agreed to be AVR, and of the 19 states that have adopted by the time of this writing, only 12 have actually implemented the reform and 10 of those did so during the 2018 election cycle. Thus, systematic evidence of the effects of these laws has been limited.

In this paper we begin to address this gap in the literature by analyzing the effect of AVR on registration rates. We limit ourselves to registration rates because higher registration is arguably the most basic intended consequence of the AVR reform. Our data and methodological approach also permit us to examine key underrepresented groups that many voter advocacy organizations have hoped AVR would draw onto the registration rolls: Latinos, Asian Americans, and young people.

Our results suggest the effects of AVR on registration are solid but not large so far, amounting on average to about a two percentage point increase in registration. We also find larger effects in some states and among Latinos in most states. Given the historical stability of registration rates, the limited implementation period in most states, and the fact that only a subset of voters can logically be considered in the treatment group thus far, we consider these results worth noting and a promising sign for the future of the reform.

Background

If there is an objective common to all AVR reforms, it is that residents should not have to actively seek out voter registration; instead, voter registration should occur as a matter of course unless it is actively declined. When citizens provide the government with information necessary to be registered–even if for some unrelated reason–the government should use it to register them unless expressly told not to.

The government touch point could be any agency, but the most typical one is the Department of Motor Vehicles (DMV). Virtually everyone uses the DMV at some point, and many DMV transactions require customers to provide personal details that can be used for voter registration as well. The DMV has in fact been required to offer voter registration in most states since passage of the National Voter Registration Act (NVRA) in 1993. But states have varied considerably in the ease and visibility of this "motor voter" option (Naifeh 2014; Highton and Wolfinger 1998). DMV customers often have to fill out the same information twice, the registration forms can be buried in piles of papers, and at times the DMV employees might forget to raise the issue at all. While such inconveniences might not deter more determined residents, part of the point of DMV registration is to encourage those who might find registration confusing or intimidating, or might not be acculturated into self-identifying as a voter, to sign up to vote anyway.

In fact, there are reasons to think that even when voter registration is offered at the DMV in exactly the way the NVRA envisions, the number of people taking advantage of it might still be limited. Research shows that even small changes to the way options are presented can significantly affect decisions. Independent of their preferences, people will often accept the status quo and avoid even simple behaviors that would be to their own long-term benefit (Thaler and Sunstein 2008). A customer who is at the DMV for a driver's license might skip over the questions about voter registration to save time, or because the issue seems unrelated to the original reason they came to the DMV. Thus, a customer's relationship to the voter registration questions matters. Will they be required to answer those questions? If they do not answer them, will they be registered anyway?

AVR treats these issues as important and redesigns the DMV (or other agency) transaction accordingly. The way it does so differs considerably across states. The main distinctions are between *back end* and *front end* AVR (Root 2019), and between *default registration* and a *forced choice*. Back end AVR states no longer bring up voter registration at the government agency. Instead, the state establishes whether a resident is eligible to vote based on information provided when the person signs up for the government program, and then the state contacts that person after the fact to offer the chance to opt out. By contrast, front end AVR raises the voter registration option at the government agency and allows the customer to opt out at that point as a part of the overall transaction.

Separate from the front end / back end distinction, states have also varied how they make residents engage with the voter registration questions. Some alter the default option: in the absence of a choice, eligible residents are placed into the program and must actively decide to opt out. Currently all back end AVR states use default registration: residents are registered first and must actively opt out after the fact if they do not want to be.[1] But some front end states also have a default option: there is a box on the agency form that offers the option to decline voter registration, and if the box is *not* checked the resident will be registered. Other front end states work primarily through a forced choice or "hard stop": customers must answer the voter registration questions or they cannot complete their transaction. The default may or may not be to place a voter into registration, but because they must answer one way or the other the default is not very relevant.[2]

Of these possibilities, changing the default option should encourage more voter registration than forcing a choice, because the former makes it more complicated to avoid registration. For similar reasons, a default option should produce more registrants when implemented through the back end than the front end, if only because the back end requires registered voters to notice a letter in the mail and respond. Of the states that have implemented so far, front end AVR with default registration is somewhat more common (Georgia, Illinois, Rhode Island, Vermont, and Washington, DC.) than front end with a forced choice

[1] A back end state would not have default registration if it required residents to respond to the post-transaction contact if they *wanted* to be registered, rather than to *avoid* registration as is presently the case.

[2] California is planning to implement a middle ground between these possibilities by forcing a choice but pre-filling the "yes" box on the voter registration question. However, California still requires customers to affirmatively attest to eligibility prior to answering the voter registration question, and this eligibility question is a forced choice. Thus, the process is still a forced choice and not a default registration approach.

(California, Colorado, Connecticut, Delaware, and Utah), and both dominate the back end approach (Alaska and Oregon).

AVR is typically promoted as a means to one or more of three goals. First, it is seen as a more effective and efficient means of maintaining the registration rolls. Residents who move often forget to re-register, which complicates or even prevents voting in the next election and also sows doubt among election administrators as to which records are permanently inactive and which could be active again if properly updated. By handling the process of re-registration when a voter is already engaged with government for a different purpose, AVR ensures that the rolls contain the most up-to-date information. This purely administrative benefit is important but we will not address it further in this study.

The second goal is to increase registration, and through that to boost turnout. The hope is that many people who are eligible to register and vote but have not done so will assent to being registered if the process is made as simple as possible. With AVR, there is no longer a need to figure out how to register or to overcome natural human inertia. Almost every aspect of the process is now handled by the state.

One might question whether registration per se should be of any interest. Indeed, the political science literature has rightly treated registration as little more than a mediator for turnout. Wolfinger and Rosenstone (1980) famously highlighted registration as a key obstacle to voting, but it was the voting that really mattered to them and not the registration. Work on registration reforms has taken a similar tack. Analysis of same-day or Election-Day registration almost exclusively uses turnout as the dependent variable, finding effects anywhere from a small decrease up to a more substantial increase, with little discussion of the overall changes in registration rates that accompany these effects (Brians and Grofman 2001; Hanmer 2009; Keele and Minozzi 2013; Knee and Green 2011). Ansolabehere and Konisky (2006) approach the question from the other direction by estimating the effect of introducing registration in the 1970s in counties where it had not existed before. They find a comparable decline in eligible turnout of about 4 percent, but offer no estimate of a registration effect (indeed, it is not clear how such an effect could even be estimated when registration itself did not previously exist). One study did estimate that 3-4 million eligible Americans in 2012 missed out on registration in the 2012 presidential election due to registration deadlines (Street et al. 2015), and there have also been some field experiments where the immediate treatment concerns registration (Mann and Bryant 2019). But such studies are the exception rather than the rule.

There is nothing inherently wrong with focusing on turnout. Turnout is an exercise of political power, while registration is just a means to that end. Nor does registration lead inevitably to turnout. Perhaps the most successful American registration reform of the last century was the Voting Rights Act, but when it was first implemented many African Americans who were added to the rolls did not vote in high numbers (Colby 1986). It is reasonable to see registration as entirely secondary.

That said, there are reasons to think that registration may be the most appropriate outcome variable to examine at this stage of AVR implementation, when the reform is still young in most states. AVR is different than many previous registration reforms. Election-Day registration, for example, is about removing barriers for citizens who are already motivated to vote. Those who take the time to show up to register at a polling place on Election Day will almost certainly cast a ballot. (It would be strange if they did not.) By contrast, AVR presses registration on some who might not have considered it and who may not think much about their registration afterward. This makes AVR less about removing barriers (though it has that element as well) and more about *mobilization*: about urging eligible residents to get registered when it was not part of their plans. Much like the Voting Rights Act–and maybe more important, the intense registration drives around it (Fraga 2018)–those who get registered through AVR might not have high turnout at first. They will only gradually become more regular voters through further mobilization after the registration step is far behind them.

Thus, though we believe that the turnout effects of AVR are ultimately more important than the registration effects, at this point the registration effects may be a better early indicator of the reform's performance. The registration effects always give a sense of the potential that the reform has created. If the registration effects are large, then weak turnout effects are not a failure of the reform per se but a sign that more work must be done to mobilize the newly registered voters. If the registration effects themselves are small, no amount of mobilization can make up for that fact without obviating the point of the reform itself.

The final goal of AVR is to improve the turnout gap. While it is valuable to increase registration and turnout overall, the U.S. electorate is not representative of the population as a whole and addressing this problem is often seen as at least as important as raising participation per se (Fraga 2018). Some scholars have urged turning away from reform efforts altogether because the participation increases are modest and come at the cost of exacerbating inequity (Berinsky 2005). Thus, it is not just the total registration rate that matters, but the rate in key underrepresented groups. Do they register at higher rates as well? Is the effect for these groups larger or smaller than for the population as a whole? An overall registration increase that also made the electorate less representative–that risked increasing the turnout gap–would be a cause for concern.

Data and Methods

For the purposes of this study, then, our outcome variable is registration. We examine changes in total registration rates, but to address the question of representativeness and equity we also explore the same for Latinos, Asian Americans, and young people ages 18 to 24–all groups who register at rates far below their overall population numbers. We test these registration effects with two data sources: survey data on registration from the U.S. Census over a long period of time, and a more limited set of registration totals from the last two election cycles. Each has some strengths and weaknesses that we will discuss in turn.

Our primary data come from the November Voting and Registration Supplement of the U.S. Census's Current Population Survey (CPS). Though we have CPS data from 1980 through 2018, for the best comparisons to the present day we never use data earlier than 1994, and for most purposes our earliest year is 2002.[3] The CPS includes a range of demographic estimates which we marry to data on the competitiveness of statewide partisan elections for Governor and U.S. Senate and President. With these data, we calculate registration as a share of the total eligible population, which in the CPS is determined by age and citizenship. Item non-response bias has become a more serious problem with the CPS over time, and many of the same underrepresented groups that are a focus of this study have become more likely to skip the Voting and Registration Supplement questions (Hur and Achen 2013; McDonald 2012). To address this problem we impute missing data with the Amelia package for R (King et al. 2001; Honaker, King, and Blackwell 2009).

With these CPS data we must construct a plausible counterfactual for what might have happened in AVR states in the absence of the reform. Our first approach leverages the large sample size available for these surveys and runs a difference-in-differences (DID) model for all respondents from 2002 through 2018 (Ashenfelter and Card 1985). DID models seeks to account for any unmeasured differences between AVR states and other states that do not vary over time, and for any shifts in registration from one year to the next that occurred uniformly across all states. This is accomplished with a full set of dummy variables for states and years. In addition to the dummy variables typical of DID models, we also control for a range of demographic covariates such as age, education, mobility, and employment status that vary across both states and years. The causal effect of AVR is identified off changes in registration not accounted for with these other systematic factors.

Our second modeling approach is the synthetic control method (Abadie, Diamond, and Hainmueller 2010, 2015). This involves identifying a "synthetic control group" that consists of a weighted combination of all other states (the "donor pool"). The weights are determined by each state's similarity to a given AVR state using a set of variables specified by the analyst.

[3]Data from too long before the treatment can risk creating a comparison set that is no longer relevant to the time when the treatment was applied.

The key is that these weights are calculated based on data prior to the policy intervention, and then the trajectories of the AVR state and the synthetic control are compared after the intervention. The more these trajectories diverge, the more the treatment is presumed to have had an effect.

More formally, the synthetic control method seeks a set of weights $\boldsymbol{W^*}$ for the donor pool which solves the following constrained minimization problem:

$$\boldsymbol{W^*} = min_W(\boldsymbol{X_1} - \boldsymbol{X_0W})'\boldsymbol{V}(\boldsymbol{X_1} - \boldsymbol{X_0W})$$

where $\boldsymbol{X_1}$ is a $(kX1)$ vector of k preintervention characteristics for the treated state, $\boldsymbol{X_0}$ is a (kXJ) matrix of the same variables for the J states in the donor pool, $\boldsymbol{W}$ is a $(JX1)$ matrix of weights for the control states, and $\boldsymbol{V}$ is a set of coefficients indicating the relative predictive power of each of the variables in the model.

The synthetic control method is similar to the DID in that it strives to control for unobserved confounds that can undermine estimates of causal effect. However, the DID presumes that these unobserved confounds are constant in time, and so drop out through the differencing process. The synthetic control, by contrast, allows these confounds to vary in time, permitting the trajectories of the treated and the comparison states to differ prior to the policy intervention. Abadie, et al. (2010) demonstrate that if the synthetic control model is run on a sufficiently large number of pretreatment time periods, the method can fit the observed covariates specified in the model only if it also accounts for any time-varying unobserved confounds.

While it is possible to set V based on a priori substantive beliefs about the relative importance of each of the variables, it is more typical to estimate V from the data such that the pretreatment outcomes in the treated and synthetic control states most closely resemble each other. If $\boldsymbol{Y_1}$ is the pretreatment turnout series for the AVR state and $\boldsymbol{Y_0}$ the same for the untreated states, then this process identifies $\boldsymbol{V^*}$ such that

$$\boldsymbol{V}^* = min_V(\boldsymbol{Y_1} - \boldsymbol{Y_0}\boldsymbol{W}^*(\boldsymbol{V}))'(\boldsymbol{Y_1} - \boldsymbol{Y_0}\boldsymbol{W}^*(\boldsymbol{V}))$$

Though this is a sensible way of approaching the question, it does not entirely identify the appropriate value of $\boldsymbol{V}$. There remains the question of model specification; in other words, which of the values of $\boldsymbol{V}$ should be set to zero prior to estimation? One of the challenges of the synthetic control method as applied to AVR is the small number of post-treatment periods. This makes the post-treatment effect estimates more susceptible to random shocks. The magnitude of the effect estimate might therefore be more vulnerable to model specification, and our confidence in any given model estimate more uncertain. Adding to the challenge, the model building process cannot avoid the fundamental bias-variance trade-off that is a part of all such work. A model that perfectly fits the pretreatment turnout series for a particular AVR state might nonetheless poorly predict out-of-sample observations, adding noise to the treatment estimate and undermining our confidence in the reform effect (James et al. 2013; McClelland and Gault 2017). As detailed below, we address this problem with a cross-validation approach that seeks to identify the models that strike the best balance between these competing considerations, and so we offer estimates based on a range of models rather than just one.

The second choice in constructing a synthetic model is the range of time over which the chosen variables are averaged, and so the period over which the model seeks to optimize the fit. If the relationship between the covariates and the dependent variable is different in the early years of the time series, it is better to favor later years for the sake of estimating the model (McClelland and Gault 2017). The third choice is the total range of time one wants to fit. For this analysis, we always used the same time period for averaging as we did for optimizing. As we will see, this range of choices sometimes produces a comparable range of possible treatment effects.

The CPS has a number of strengths. Since respondents self-report their race and ethnicity, these characteristics are as accurately measured as they are likely to be. Moreover, the long time series in the CPS makes it amenable to more robust causal identification. The synthetic control method would not be possible without at least a few years worth of pre-treatment data, and these pre-treatment outcomes play a key role in accounting for unobserved confounds.

At the same time, the CPS also has limitations for this study. First is error in self-reported registration. It has long been noted that many respondents will say they are registered when they are not, but for our purposes the more serious problem may be the reverse: in an AVR state, many respondents may only be dimly aware that they *are* registered (especially in back end AVR states) and so will say they are not. It is difficult to know how much bias this introduces into the estimates but there is almost certainly some.

The second problem is a loss of precision due to sampling. Ordinarily sampling error would not be a serious problem for a survey as large as the CPS, but within each state only those residents who go to a relevant government agency are truly in the treatment group. The problem only becomes more acute when we limit our analysis to the underrepresented groups. Sample size and sampling error will attenuate our estimates and make it difficult to confidently identify a treatment effect even if one may in fact exist. The challenge is most serious with the synthetic control analysis, which uses data aggregated at the state level and so loses a large amount of information relative to the individual-level CPS analysis.

To address some of these problems, we also turn to aggregate totals of state registration files from the data vendor Catalist. These data avoid the problems with registration self-report because they reflect actual registration. Because they are based in the total registration file, they also avoid the sampling problem and so have much better precision for identifying effects on the subset of people actually touched by the reform. However, the registration data cover only 2016 and 2018, preventing the use of the synthetic control group design. As such, we use a very basic DID design instead. The registration data also require estimating race and ethnicity with auxiliary information, rather than with the more straightforward approach of asking the registrants themselves. (Age, however, is quite well measured in these data.)

Because of these limitations, we use the registration data as a check on the CPS results. Where they agree we can have more confidence in the conclusions. We also caution against firm conclusions for individual states using the synthetic control because of the limited degrees of freedom. The synthetic control will offer a glimpse of possible treatment effects but only just.

It is important to be clear what we are testing here. It is common to describe the effect of AVR in terms of the number of people who register at the DMV or other government agency. This is a perfectly acceptable approach to evaluating the reform's impact: it measures the willingness of DMV customers to register through the AVR system, and so the attractiveness of the system in a general sense. But those who take advantage of AVR might have registered a different way if AVR had not been available. Thus, we are exploring a more expansive question: did AVR encourage a substantial number of people to register who would not have done so otherwise? A large number of people might register to vote under AVR even if the system does not encourage any *new* people to register. Likewise, we might see an increase in registration coincident with the new system, but if this increase was not substantially larger than in states where AVR was not implemented, we would not be able to conclude that the AVR reform was responsible for the change. This is a more exacting test of the system, especially at this early stage.

Analysis

Table 1 contains the DID estimates from the CPS. The effect of AVR in this model is reasonably large, increasing registration 2.1 percentage points. The effects for Latinos and young people look to be even larger, reaching 7.1 and 5.1 percentage points, respectively. In the context of relatively stable registration rates an increase of 2.1 percentage points is fairly substantial, and an increase over 5 percentage points for just one or two election cycles is quite notable. The effect for Asian Americans, however, is estimated to be negative and very noisy, leaving little confidence that there is any effect at all for that group.

Table 1: Registration effects of AVR: CPS difference-in-differences models, 2002-2018

	All	Latinos	Asian-Americans	Youth (18-24)
Intercept	0.240^{***}	0.141^{*}	0.063	-0.158^{***}
	(0.011)	(0.064)	(0.092)	(0.044)
AVR	0.021^{***}	0.071^{**}	-0.015	0.051^{**}
	(0.005)	(0.025)	(0.038)	(0.018)
Age	0.004^{***}	0.005^{***}	0.003^{***}	0.014^{***}
	(0.000)	(0.000)	(0.000)	(0.001)
Female	0.022^{***}	0.024^{***}	0.015^{*}	0.021^{***}
	(0.001)	(0.005)	(0.006)	(0.004)
Some HS	0.056^{***}	0.043^{***}	$0.050^{\#}$	0.151^{***}
	(0.005)	(0.011)	(0.027)	(0.025)
HS graduate	0.188^{***}	0.156^{***}	0.125^{***}	0.265^{***}
	(0.004)	(0.009)	(0.020)	(0.024)
Some College	0.300^{***}	0.289^{***}	0.266^{***}	0.424^{***}
	(0.004)	(0.009)	(0.021)	(0.024)
College graduate	0.368^{***}	0.352^{***}	0.332^{***}	0.499^{***}
	(0.004)	(0.010)	(0.020)	(0.025)
Post-graduate	0.382^{***}	0.388^{***}	0.385^{***}	0.467^{***}
	(0.004)	(0.011)	(0.023)	(0.038)
Unemployed	-0.035^{***}	-0.015	0.000	-0.035^{***}
	(0.003)	(0.010)	(0.019)	(0.008)
Moved last year	0.035^{***}	0.019^{*}	0.021	0.022^{***}
	(0.003)	(0.008)	(0.014)	(0.006)
Moved 1-2 years ago	0.118^{***}	0.110^{***}	0.102^{***}	0.099^{***}
	(0.002)	(0.007)	(0.013)	(0.005)
Latino	-0.019^{***}	−−	−−	-0.033^{*}
	(0.005)			(0.013)
Non-hispanic white	0.031^{***}	−−	−−	0.034^{**}
	(0.005)			(0.012)
African American	0.072^{***}	−−	−−	0.082^{***}
	(0.005)			(0.014)
Asian American	-0.108^{***}	−−	−−	-0.081^{***}
	(0.006)			(0.017)
U.S. Senate race	-0.012^{*}	-0.040	0.026	-0.016
	(0.006)			(0.020)
U.S. Senate margin	-0.017^{*}	-0.051	0.019	-0.023
	(0.007)			(0.024)
Governor margin	-0.003	-0.040^{***}	-0.017	-0.010
	(0.002)			(0.007)

State fixed effects	X	X	X	X
Year fixed effects	X	X	X	X
AIC	144103	16310	9435	21163
Null AIC	164941	18164	10151	23438
N	837280	65949	35245	92527

Note: Cell entries are linear probability model coefficients and standard errors, calculated in R 3.6.0.

The findings are different when we turn to the synthetic control. To account for modeling variability, we ran a wide range of different models that accounted for demographics, political variables, and lagged registration numbers. A list of these models and the variables they included can be found in Table 2. We tested each of these models on pre-treatment outcomes, omitting one year of data in turn, predicting that year out of sample, and then averaging the squared prediction errors for this leave-one-out validation. This offered a mean squared prediction error (MSPE) for each of the models. When we then ran the models on the full data, we weighted the average of the treatment effects from these models by the inverse of this MSPE, thus upweighting models that would be expected to have greater out-of-sample predictive accuracy in the absence of the AVR policy intervention.

As recommended by Abadie, et al. (2010), we also ran a placebo test. For each of the models, we pretended as if each of the non-treated states was in fact treated with AVR and then recorded the treatment effect. We then calculated the share of treatment effects that were larger than the effect for the state that actually implemented AVR. The share of states with larger effects becomes the probability of seeing the treatment effect by chance– that is, due to factors unrelated to the application of the AVR treatment. We also averaged these probability numbers with the same weighting procedure that we used for the treatment effects.

The results are reported in Table 3 for all CPS respondents. Here we see more mixed evidence of an AVR treatment effect. The largest effects are in Oregon, Georgia, and Utah, and to a lesser extent, California. The rest of the AVR states show little to no effect.

Table 2: Models for the synthetic control analysis

	Model						
Variable	Political	Demogs.	All Lags	Lags + Demogs.	Lags + Political	Mixture	Full
Non-Hispanic white		X		X		X	X
Age		X		X			X
Married		X		X			X
Some college +		X		X			X
Moved last 2 years		X		X			X
U.S. Senate race	X				X	X	X
U.S. Senate margin	X				X	X	X
Governor margin	X				X	X	X
Lag 1			X	X	X	X	X
Lag 2			X				
Lag 3			X	X	X	X	X
Lag 4			X				
Lag 5			X	X	X	X	X
Lag 6			X				
Lag 7			X	X	X	X	X
Lag 8			X				

Moreover, no state's synthetic control estimate comes with a high degree of confidence; the closest is Oregon, with a null probability of 0.19, followed by Georgia with 0.23. Alaska, California, and Connecticut, at least fall below 0.4, but Connecticut's estimate suggests a registration decline rather than an increase.

Table 3: Synthetic control estimates: total registration

	Average Treatment Effect	Pr(Treatment)
Alaska	0.004	0.374
California	0.014	0.327
Colorado	0.008	0.611
Connecticut	−0.020	0.274
Delaware	0.009	0.837
Georgia	0.024	0.235
Illinois	0.000	0.639
Oregon	0.032	0.185
Rhode Island	0.010	0.699
Utah	0.022	0.681
Vermont	−0.007	0.797
Washington	0.007	0.433

Note: Treatment estimates come from a weighted average of the synthetic control models as described in the text. Weighting is by the inverse of the mean squared prediction error from a leave-one-out cross-validation conducted on the pre-treatment time period. "Pr(Treatment)" is the average probability of a treatment at least as large when running the same model on placebo states.

Tables 4 through 6 show the results for each of our key subgroups. As suggested by the full DID model, larger effects can be found among Latinos and young people, though the estimates vary considerably across states and still fall short of traditional thresholds for strong statistical confidence. Some are also negative, which is not the effect we would expect from AVR. However, the effects are mostly positive: 10 out of 12 states for young people and 8 out of 12 states for Latinos. The results for Asian Americans are far more variable, however, with many negative effects that are substantively quite large. As noted above, it is especially challenging to use the CPS data to explore the effects for racial and ethnic minorities in the synthetic control. Many racial and ethnic groups are highly concentrated in a small number of states; in the case of Asian Americans, fully 60 percent of the nation's population falls in just 10 states. This makes it difficult to develop a solid donor pool with statistically stable estimates for the synthetic control analysis.

Our final approach to this question uses actual registration data from 2016 and 2018. The DID model estimates using these data can be found in Table 7. Because we have limited degrees of freedom for this calculation and because most demographic differences for two election cycles will be accounted for with the state fixed effects, we limit the other covariates in these models to competition-related variables alone.

These DID estimates are generally stronger than the comparable estimates from the CPS. The estimate for Latinos is about 9 percent and the one for Asian Americans over 13 percent. The overall effect here is remarkably close to the overall DID estimate with the CPS data: 2.0 percentage points versus 2.1. Only the youth estimate is far smaller, falling very close to zero.

Conclusions

Our analysis has focused on the effect of the raft of new automatic voter registration laws on total state registration and the registration rates among key underrepresented communities. We employ a range of data sources and methodologies to address this question. To the extent these methodologies agree, we can have greater confidence in the results.

Table 4: Synthetic control estimates: Latino registration

	Average Treatment Effect	Pr(Treatment)
Alaska	−0.023	0.586
California	0.041	0.173
Colorado	0.007	0.371
Connecticut	−0.031	0.503
Delaware	0.046	0.685
Georgia	−0.031	0.501
Illinois	0.020	0.208
Oregon	−0.006	0.735
Rhode Island	−0.054	0.507
Utah	0.031	0.510
Vermont	0.015	0.532
Washington	0.061	0.244

Note: Treatment estimates come from a weighted average of the synthetic control models as described in the text. Weighting is by the inverse of the mean squared prediction error from a leave-one-out cross-validation conducted on the pre-treatment time period. "Pr(Treatment)" is the average probability of a treatment at least as large when running the same model on placebo states.

Table 5: Synthetic control estimates: Asian American registration

	Average Treatment Effect	Pr(Treatment)
Alaska	0.096	0.512
California	0.018	0.359
Colorado	0.162	0.196
Connecticut	−0.065	0.714
Delaware	−0.149	0.689
Georgia	0.029	0.668
Illinois	−0.029	0.549
Oregon	0.010	0.363
Rhode Island	−0.046	0.309
Utah	−0.142	0.127
Vermont*	−−	−−
Washington	0.105	0.064

Note: Treatment estimates come from a weighted average of the synthetic control models as described in the text. Weighting is by the inverse of the mean squared prediction error from a leave-one-out cross-validation conducted on the pre-treatment time period. "Pr(Treatment)" is the average probability of a treatment at least as large when running the same model on placebo states. *Vermont did not have sufficient Asian American CPS respondents for estimation.

Table 6: Synthetic control estimates: youth registration (ages 18-24)

	Average Treatment Effect	Pr(Treatment)
Alaska	0.021	0.411
California	0.030	0.252
Colorado	0.057	0.141
Connecticut	−0.026	0.283
Delaware	0.006	0.545
Georgia	0.043	0.149
Illinois	0.017	0.477
Oregon	0.094	0.189
Rhode Island	−0.005	0.705
Utah	0.061	0.440
Vermont	0.025	0.599
Washington	0.026	0.530

Note: Treatment estimates come from a weighted average of the synthetic control models as described in the text. Weighting is by the inverse of the mean squared prediction error from a leave-one-out cross-validation conducted on the pre-treatment time period. "Pr(Treatment)" is the average probability of a treatment at least as large when running the same model on placebo states.

Table 7: Difference-in-differences with registration file data, 2016-18

	Total	Latinos	Asian Americans	Youth (18-24)
Intercept	0.889***	0.646***	0.719***	0.511***
	(0.029)	(0.120)	(0.170)	(0.065)
AVR	0.020*	0.089*	0.132**	0.010
	(0.011)	(0.045)	(0.064)	(0.024)
U.S. Senate race	−0.012	0.023	0.045	−0.009
	(0.019)	(0.077)	(0.109)	(0.042)
U.S. Senate margin	−0.019	−0.003	0.072	−0.017
	(0.024)	(0.097)	(0.137)	(0.052)
Governor margin	−0.006	−0.034	−0.065*	0.015
	(0.006)	(0.026)	(0.036)	(0.014)
State fixed effects	*X*	*X*	*X*	*X*
Year fixed effects	*X*	*X*	*X*	*X*
Observations	100	100	100	100
Adjusted R^2	0.971	0.783	0.659	0.942
Residual Std. Error	0.021	0.085	0.121	0.046

Note: *p<0.1; **p<0.05; ***p<0.01
Cell entries are ordinary least squares coefficients and standard errors, calculated in R 3.6.0. Congressional results are weighted by seat apportionment.

At the time of this writing, many of these estimates point in somewhat different directions. For example, our estimates with the Current Population Survey suggest weak or even negative effects for Asian Americans but modest effects for young people; our estimates with registration data suggest strong effects for Asian Americans and weak effects for young people. Our most consistent effects are for overall registration, and registration among Latinos. Overall effects from both the CPS and the registration data point toward an effect size of approximately two percentage points. Both sources also agree that the reform increased Latino registration rates, and to a substantial degree: 7.1 percent in the CPS and 8.9 percent in the registration data.

We also employed the synthetic control method with the CPS data to test the effects in individual states. These effects were more variable and imprecisely estimated, though in the broadest terms they followed the same basic pattern: modest overall effects, stronger effects for Latinos and young people, and weak effects for Asian Americans. However, it is challenging to develop robust estimates for these groups with CPS data because the sample size in some states limits the potential comparison group.

Overall, these findings are preliminary and deserve further analysis and scrutiny. The CPS data are useful for individual-level analysis but have limitations for the aggregate analysis of the synthetic control design. Conversely, the registration data can be useful for aggregate analysis, but the limited time series currently available to us makes the analysis far less robust than we would prefer. In future versions of this paper we hope to obtain a longer history of registration data, which we can then use to develop a more robust implementation of the synthetic control group design. That should help us pin down state-by-state estimates that can better illuminate the consequencdes of different AVR approaches; it will also help us develop a more robust design for our multi-state AVR estimate.

On balance, the results here are cautiously promising for AVR. At this early point, it appears to produce a modest increase in registration in most of the states that have adopted it, while perhaps encouraging even higher registration for some of the underrepresented groups that are a central focus of these reforms.

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