BRENNAN CENTER FOR JUSTICE

AVR Impact on State Voter Registration

New Brennan Center Report Finds Significant Gains in Voter Rolls

by Kevin Morris and Peter Dunphy

Executive Summary

Over the past five years, a significant reform of voter registration has been enacted and implemented across the country. Automatic voter registration or AVR offers the chance to modernize our election infrastructure so that many more citizens are accurately registered to vote.[1]

AVR features two seemingly small but transformative changes to how people register to vote:

1. Citizens who interact with government agencies like the Department of Motor Vehicles are registered to vote, unless they decline. In other words, a person is registered unless they opt out, instead of being required to opt in.

2. The information citizens provide as part of their application for government services is electronically transmitted to elections officials, who verify their eligibility to vote. This process is seamless and secure.

In the past five years, 15 states and the District of Columbia have adopted AVR.[2] (Three states — Connecticut, Utah, and New Mexico — have adopted something very close to automatic registration.)[3]

How has automatic registration worked? Has it, in fact, increased registration rates as its proponents had hoped? This report is the first comprehensive analysis of the impact of AVR on voter registration rates. In the past, individual states have reported increases in voter registration since the adoption of automatic voter registration. But that could be due to many factors, such as compelling candidates or demographic change. Previous analyses have not spoken as to cause and effect or examined the impact of different approaches to AVR.

Is it possible to isolate the impact of automatic registration itself? This multistate analysis leverages low-level voter file data from around the country and cutting-edge statistical tools to present estimates of automatic voter registration's impact on registration numbers.

This report finds:

- AVR markedly increases the number of voters being registered — increases in the number of registrants ranging from 9 to 94 percent.

- These registration increases are found in big and small states, as well as states with different partisan makeups.

These gains are found across different versions of the reform. For example, voters must be given the opportunity to opt out (among other things, to protect ineligible people from accidentally being registered). Nearly all of the states with AVR give that option at the point of contact with govern-

ment agencies; two ask for opt-outs later in the process. The increase in registration rates is similarly high whichever version of the policy is adopted.

How did we do this study? We were able to isolate the effect of AVR using a common political science method known as "matching." We ran an algorithm to match areas that implemented AVR with demographically similar jurisdictions that did not. Matching similar jurisdictions allowed us to build a baseline figure of what a state's registration rate would have looked like had it not implemented AVR. By aggregating and comparing baseline jurisdictions to AVR jurisdictions, we demonstrated that AVR significantly boosted the number of people being registered everywhere it was implemented.

Our nation is stronger when more people participate in the political process. This report shows that AVR is a highly effective way to bring more people into our democracy.

Jurisdiction*	% Increase in Registrations
Oregon	15.9%
Georgia	93.7%
Vermont	60.2%
Colorado	16.0%
Alaska	33.7%
California	26.8%
Rhode Island	47.4%
Washington, DC	9.4%

*In order of implementation date

Introduction

Automatic voter registration (AVR) is an innovative policy that streamlines the way Americans register to vote through two simple tweaks to the traditional method of registering voters:

1. Eligible citizens are automatically registered to vote when they interact with designated government agencies, unless those individuals affirmatively decline. This switch to an "opt-out" system is a subtle but impactful change from the status quo "opt-in" method, which requires eligible citizens to take an affirmative step to register to vote.

2. These government agencies will electronically transfer voter registration information to election officials, avoiding paper registration forms. This saves paper costs and ensures that voter rolls are kept up-to-date.

As of March 2019, 15 states and the District of Columbia have enacted AVR. This is remarkable given that the first state to adopt AVR, Oregon, passed the reform just four years ago, in March 2015.[4]

Previous research has found that states that implemented AVR have seen registration rates rise. However, this research has often failed to establish a causal relationship — that AVR, absent other factors, was responsible for the rise in registrations.[5]

This new report by the Brennan Center for Justice seeks to prove just that. This study examines the seven AVR states (and Washington, DC) that have been operating the program long enough for meaningful results to be available. By using a common political science method known as "matching," we can quantify both the impact and statistical significance of the implementation of AVR in a state. The report concludes that in every jurisdiction that implemented AVR, the policy boosted the number of registrations by a statistically significant degree.

In the following pages, we explain some of the key variations of state AVR policies, detail state factors that could affect the size of the impact of AVR on registrations, lay out our methodology, then provide a state-by-state profile that quantifies and visualizes that impact of AVR. The technical appendix that follows provides a more detailed explanation of the methodology and econometric results.

Variations in AVR

No two AVR systems are exactly the same. Factors including a state's primary system, criminal disenfranchisement law, and technological environment are relevant to the state's AVR design.

For instance: Sixteen states have either closed or partially closed primaries, which makes party registration an important part of the voter registration process.[6] In AVR systems that register voters unless they decline via a mailer (also known as a "back-end" opt-out), voters must return a postcard to indicate the party with which they wish to register. This extra step is often not taken by voters. In Oregon, for example, only 14.5 percent of people registered through AVR in 2018 returned the mailer to select a party. As a result, close to 85 percent of new voters registered through AVR were automatically marked as nonaffiliated, an outcome that would matter greatly in some states and hardly at all in others.[7]

As observable from the chart below, AVR *usually*: is adopted legislatively, is implemented only at the state Department of Motor Vehicles (DMV), and places the opportunity to opt out during the transaction (sometimes called a "point-of-service" or "front-end" opt-out). However, variation exists. For example, Alaska links AVR to the annual check that gets mailed to more than 90 percent of residents who register for the state's Permanent Fund Dividend derived from oil revenues.[8] Georgia and Colorado adopted AVR administratively, meaning it was done without implementing legislation.[9] Oregon provides the opt-out opportunity through the mail — anyone who doesn't respond to a mailing within 21 days gets registered (sometimes called a "back-end" opt-out).[10] Six of the states that have passed AVR either extend automatic registration beyond the DMV or give secretaries of state the power to do so if they believe another agency has the resource capabilities to implement AVR.[11]

There are a few factors that influence the extent to which the introduction of AVR affects the rate of voter registration:

1. **Pre-AVR Rate of Registration.** AVR will likely have a greater impact when introduced in a state in which a smaller proportion of eligible citizens are already registered to vote, as compared with a state in which a higher proportion are already registered. Even in states with high registration rates, AVR is still a valuable reform because

AVR Policy by Jurisdiction

State	Approval Date	Implementation Status	Covered Agencies	Declination Type
Alaska	November 2016: Ballot Measure 1 approved by voters	Implemented March 1, 2017	Permanent Fund Dividend Division	Back-end (post-transaction mailer)
California	October 2015: AB 1461 signed into law	Implemented April 23, 2018	DMV	Front-end (point-of-service)
Colorado	2017: Approved administratively	Tested at certain locations February 2017, subsequently implemented statewide	DMV	Front-end (point-of-service)
DC	December 2016: B21-0194 signed into law	Implemented June 26, 2018	DMV	Front-end (point-of-service)
Georgia	2016: AVR approved administratively	Implemented September 1, 2016	DMV	Front-end (point-of-service)
Illinois	August 2017: SB 1933 signed into law	Statutory implementation deadline of July 2018	DMV, plus social service agencies that the State Board of Elections determines to have reliable personal information for voter registration	Front-end (point-of-service)
Maryland	April 2018: SB 1048 enacted without governor's signature	Statutory implementation deadline of July 2019	DMV, Maryland Health Benefit Exchange, local departments of social services, and the Mobility Certification Office	Front-end (point-of-service)

State (cont'd)	Approval Date	Implementation Status	Covered Agencies	Declination Type
Massachusetts	August 2018: H 4671 signed into law	Statutory implementation deadline of January 2020	DMV and MassHealth, plus social service agencies verified by the secretary of state to collect the information necessary to determine eligibility for voter registration	Back-end (post-transaction mailer)
Michigan	November 2018: Proposal 3 approved by voters	Implementing legislation has not yet been passed	Implementing legislation has not yet been passed	Implementing legislation has not yet been passed
Nevada	November 2018: Ballot Question 5 approved by voters	No specific statutory deadline set	DMV	Front-end (point-of-service)
New Jersey	April 2018: AB 2014 signed into law	Implemented November 2018	DMV, plus social service agencies verified by the secretary of state to collect the information necessary to determine eligibility for voter registration	Front-end (point-of-service)
Oregon	March 2015: HB 2177 signed into law	Implemented January 1, 2016	DMV	Back-end (post-transaction mailer)
Rhode Island	July 2017: HB 5702 signed into law	Implemented June 11, 2018	DMV, plus social service agencies verified by the secretary of state to collect the information necessary to determine eligibility for voter registration	Front-end (point-of-service)
Vermont	April 2016: HB 458 signed into law	Implemented January 1, 2017	DMV	Front-end (point-of-service)
Washington	March 2018: HB 2595 signed into law	Statutory implementation deadline of July 2019	DMV, plus social service agencies verified by the secretary of state to collect the information necessary to determine eligibility for voter registration	Front-end (point-of-service)
West Virginia	April 2016: HB 4013 signed into law	Statutory implementation deadline of July 2019	DMV	Front-end (point-of-service)

it makes election administration more effective and helps capture much of the remaining unregistered population.[12]

2. **Rate of Registration at Implementing Agency Prior to AVR.** A state where most eligible persons visiting the AVR agency have already opted in to registration will see fewer additional people registered via AVR than a state with more "slippage," i.e., persons who are eligible to register but leave the agency without having registered. In the same vein, a state that exempts some portion of its agency transactions from AVR is expected to yield fewer registrants than a state that utilizes AVR in most transactions.

3. **Percentage of State Driver's License Holders**. Except for Alaska, all the states included in this study have implemented AVR at the DMV.[13] In the future, some states plan to extend AVR to other public agencies beyond the motor vehicle agency.[14] States with low car ownership rates, and consequently fewer driver's license holders, should expect to register fewer individuals with AVR if solely implemented at the DMV. Said states have strong incentives, therefore, to implement AVR at agencies beyond the DMV to expand the potential impact of the program.

4. **Noncitizen Population.** Every state in the country allows noncitizens to get driver's licenses.[15] Twelve states and the District of Columbia even grant legal permission to persons who are in the country without documentation to obtain driver's licenses,[16] but only citizens can lawfully participate in federal elections. Noncitizens who register to vote, even if they are lawfully present in the United States and even if they do so accidentally, can face serious legal consequences. As such, we want noncitizens to opt out. Accordingly, states with higher rates of noncitizens obtaining driver's licenses may expect a higher opt-out rate than states with few noncitizens. Each state should design

its AVR process to minimize the risk that noncitizens inadvertently register to vote.

There are other factors that influence the number of people who will be registered through AVR. For instance, 34 states disenfranchise citizens living in the community with felony convictions.[17] Although these disenfranchised individuals can get driver's licenses, they are prohibited from registering to vote and therefore should opt out of AVR. Similarly, domestic violence survivors often opt out of registering to vote because voter rolls are publicly available throughout the country.[18] Note, however, that although the presence of disenfranchised citizens and citizens with concerns about their information being publicly available will influence the number of people opting out of registrations, these populations are likely too small to have a statistically meaningful impact on estimates of AVR's effect.

Statewide Results and Methodology

In the following pages, we assess the impact of automatic voter registration on a state-by-state basis. The information for each state includes a profile of the demographic makeup of the state, a brief discussion of the methodology and any data limitations, and the reported results.

The analysis in this report rests on matching census tracts in states that implemented AVR to tracts in those that did not. We then compare the difference in registration counts between these two groups to estimate the impact of AVR. This is commonly referred to in statistics as a "matched difference-in-differences" model. Here's how these two processes work:

Matching

Myriad factors affect the rise and fall of registration rates in states over time. The purpose of this report is to isolate a single factor in this mix: the implementation of AVR. The abundance of factors impacting registration rates poses significant methodological challenges because we cannot know exactly what would have happened in the states that implemented AVR had they not done so. Accordingly, we must devise a statistical model to estimate how many individuals would have been registered in a state if the state had not implemented AVR. We compare how many voters were actually registered with this estimation of what would have happened without AVR to determine the impact of the policy.

Here's a basic rundown of how our matching works. We started by calculating the number of weekly registrations in every census tract in each state whose voter file we had access to. This includes every state that implemented AVR prior to the 2018 midterms as well as nine others.[19] For each of these census tracts, we also find various demographic information that is related to the number of people registering to vote.[20] Some of these criteria include: voting-age population; growth rate of voting-age population; education; nonwhite and noncitizen population; median income and unemployment; and number of registrations in 2013.[21]

Every "treated" census tract (census tracts in states where AVR was implemented) was then matched to the three[22] census tracts most similar to it among our pool of "untreated" census tracts (tracts in states where AVR has not yet been implemented). To determine which census tracts were most similar to one another, we used the genetic match developed by political scientist Jasjeet Sekhon.[23] Sekhon's matching algorithm is a common and widely accepted methodology for assessing policy impact. In the past decade, many studies in peer-reviewed academic journals have based their methodology on this matching technique.[24]

We then compared the growth in registrations in AVR census tracts and the control census tracts to determine whether the number of voters being registered increased more in places where AVR was introduced.

Modeling

To determine whether registration rates in treated tracts exceeded rates in control tracts, we run a simple difference-in-differences model. The periods of analysis are state-specific and based on when a state implemented AVR. In every case, we compare the growth in registrations from the pre-period (before each state's AVR implementation date) to the post-period (after the implementation date) in the control tracts with the growth in the treated tracts. If the average number of weekly registrations grew by five in the control tracts and by seven in the treated tracts, for instance, we would attribute the difference — two registrations per week — to automatic voter registration.

For the five states that implemented AVR in 2016 and 2017, we generally limit our analysis to the first 35 weeks of 2013 and 2017. In other words, we compare the growth in registrations in treated tracts from the first 35 weeks of 2013 and the first 35 weeks of 2017 with the growth in the same period in the control tracts. We compare 2013 (our pre-period) to 2017 (our post-period) because they are at the same position within the four-year presidential election cycle. We choose the odd years to decrease the interference from election-year registration spikes that could bias our results. Although we do not include 2015 in our econometric estimates, we show the control and treated tracts in 2015 in the charts in the pages that follow. We include these to demonstrate that the growth rate in registrations in treated and untreated census tracts was roughly the same from 2013 to 2015 (just as we would expect, because AVR had not yet gone into effect) and that AVR census tracts began to grow more quickly only after AVR was implemented.

We limit our period of analysis to the first 35 weeks of each year because some of the control tracts had local elections in

the fall of 2017. As these elections approached, get-out-the-vote drives may have registered many people. Registration surges from these drives have nothing to do with AVR. Therefore, we did not include periods in which registration drives were likely to impact registration rates in either treated or control tracts.

Similarly, registration surges prior to the 2018 midterm elections have the potential to distort our results in states that implemented AVR in 2018. To avoid this potential problem, we end our 2018 analyses in August 2018. In each of these models, we use nine months of data (December 2017 to August 2018), and compare the pre-implementation portion of the period with the post-implementation portion of the period in the control and treated census tracts.

For a more in-depth discussion of our matching and econometric results, please see the Technical Appendix.

Oregon

Growth in registration rates due to AVR: **15.9%**

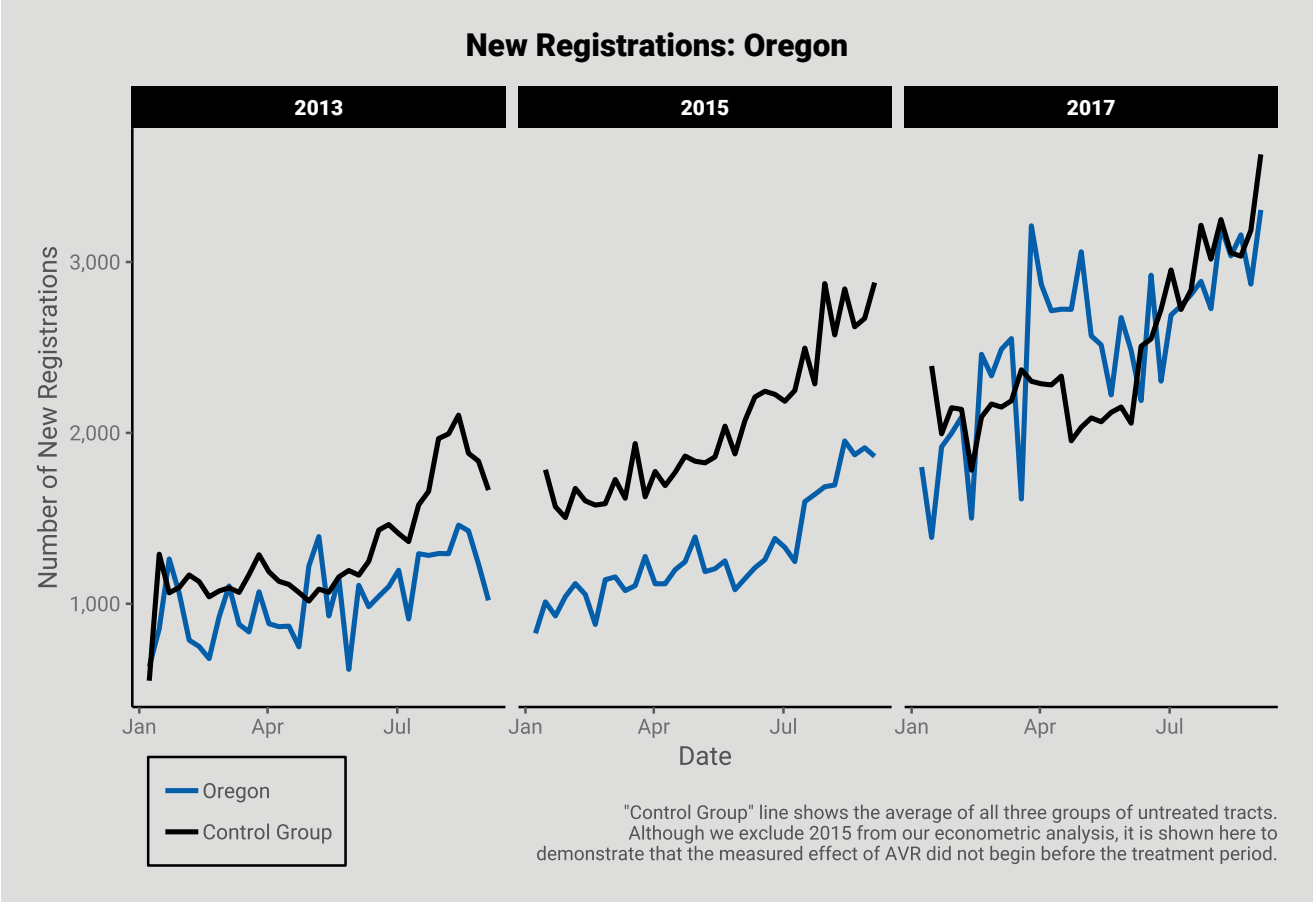
Oregon became the first state to pass AVR (in March 2015) and to implement it (in January 2016).[25] To analyze its impact, we used the state's voter file and, with the help of the secretary of state's office, added the original date of registration to the file of each voter in the state.

There were two parts to Oregon's AVR program: the registration of citizens who went into the DMV during the studied period, and the "look-back." By look-back, we mean that when Oregon implemented AVR, the DMV had reliable information on the citizenship status of individuals who had visited the DMV in 2014 and 2015.[26] Using this information, the DMV automatically registered (and sent mailers to) the eligible Oregonians who had visited it over that period. This was tremendously successful and resulted in over 122,000 Oregonians being registered.[27] However, because the look-back did not impact the number of new people being registered at the DMV each day following implementation, we have excluded the impact of the look-back from our analysis of the state.

Our model suggests that the implementation of automatic voter registration increased the statewide rate of new registrations by 15.9 percent (again, this is of people who went to the DMV after implementation). As noted, Oregon is unique among the states for a number of reasons, including that it has placed the opt-out opportunity at the back end. Perhaps surprising to some, Oregon's use of a back-end opt-out system does not produce higher registration rates than states that chose a front-end opt-out model. The results from Oregon indicate that the decision to switch from an opt-in system to an opt-out system (and, of course, the ability to implement the "look-back") was far more important than the decision about where to place the opportunity to decline registration.

State Profile:

- Passage type: legislative
- Implementation date: January 1, 2016
- Method of opt-out: back-end (post-transaction mailer)
- Registration rate pre-AVR: 76.83%
- % noncitizen population: 6.3%
- Car ownership rate: 92.4%



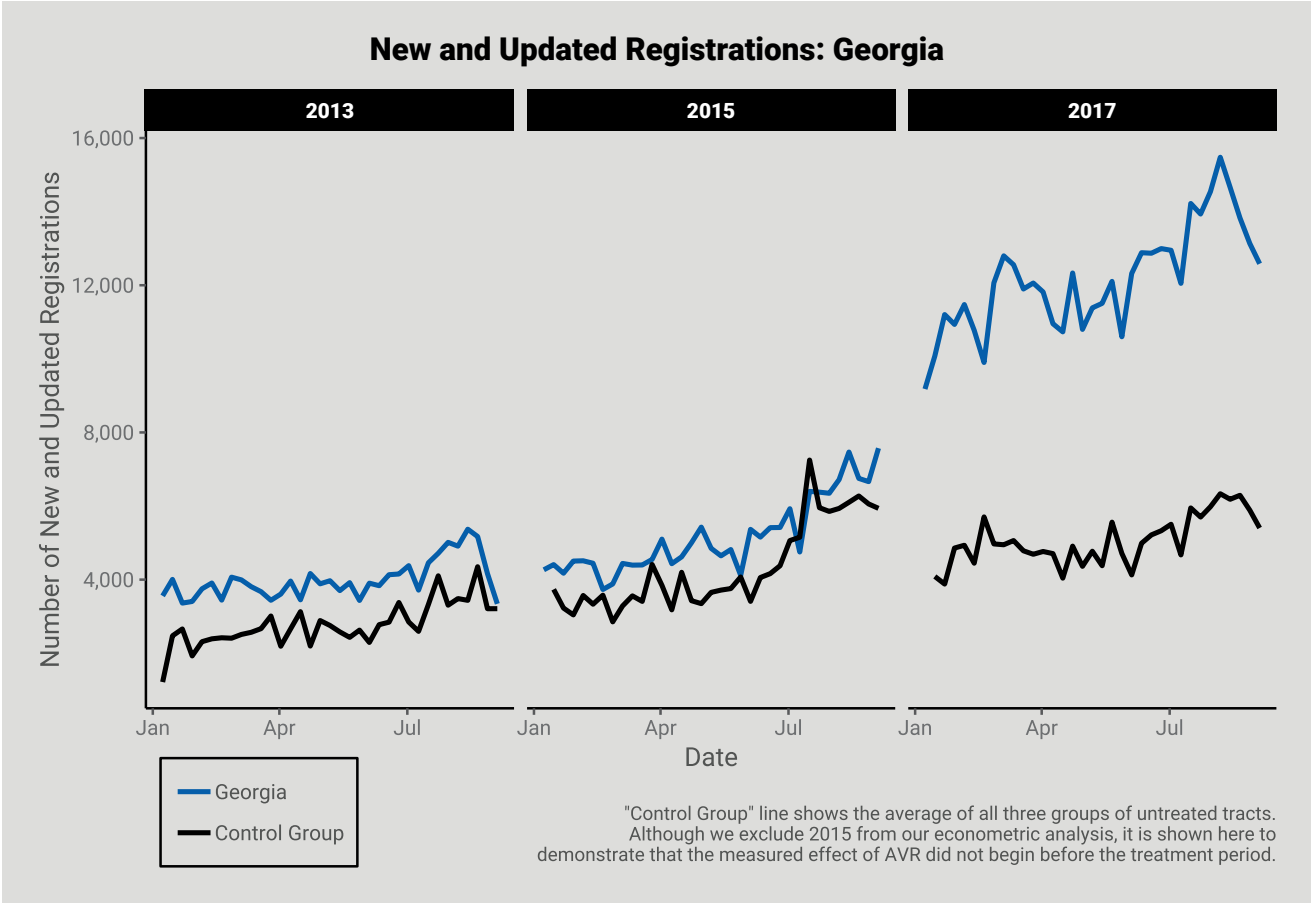
Georgia

Growth in registration rates due to AVR: **93.7%**

We used the Georgia voter file to compare new or materially updated registrations over time. The control tracts estimate that, without AVR, Georgia would have registered just over 6,279 voters each week in this period in 2017. Georgia actually registered an average of just over 12,160 each week — a 93.7 percent increase. This is, of course, a very large increase. The precise reasons for the increase are outside the scope of this report, but may be attributable to Georgia's voter list maintenance practices. Georgia officials reported instead that the increase could be attributed to the active role that Georgia DDS employees take in encouraging drivers' license applicants to register, among other things.[2829]

State Profile:

- Passage type: administrative
- Implementation date: September 1, 2016
- Method of opt-out: front-end (point-of-service)
- Registration rate pre-AVR: 74.94%
- % noncitizen population: 7.2%
- Car ownership rate: 93.3%



Vermont

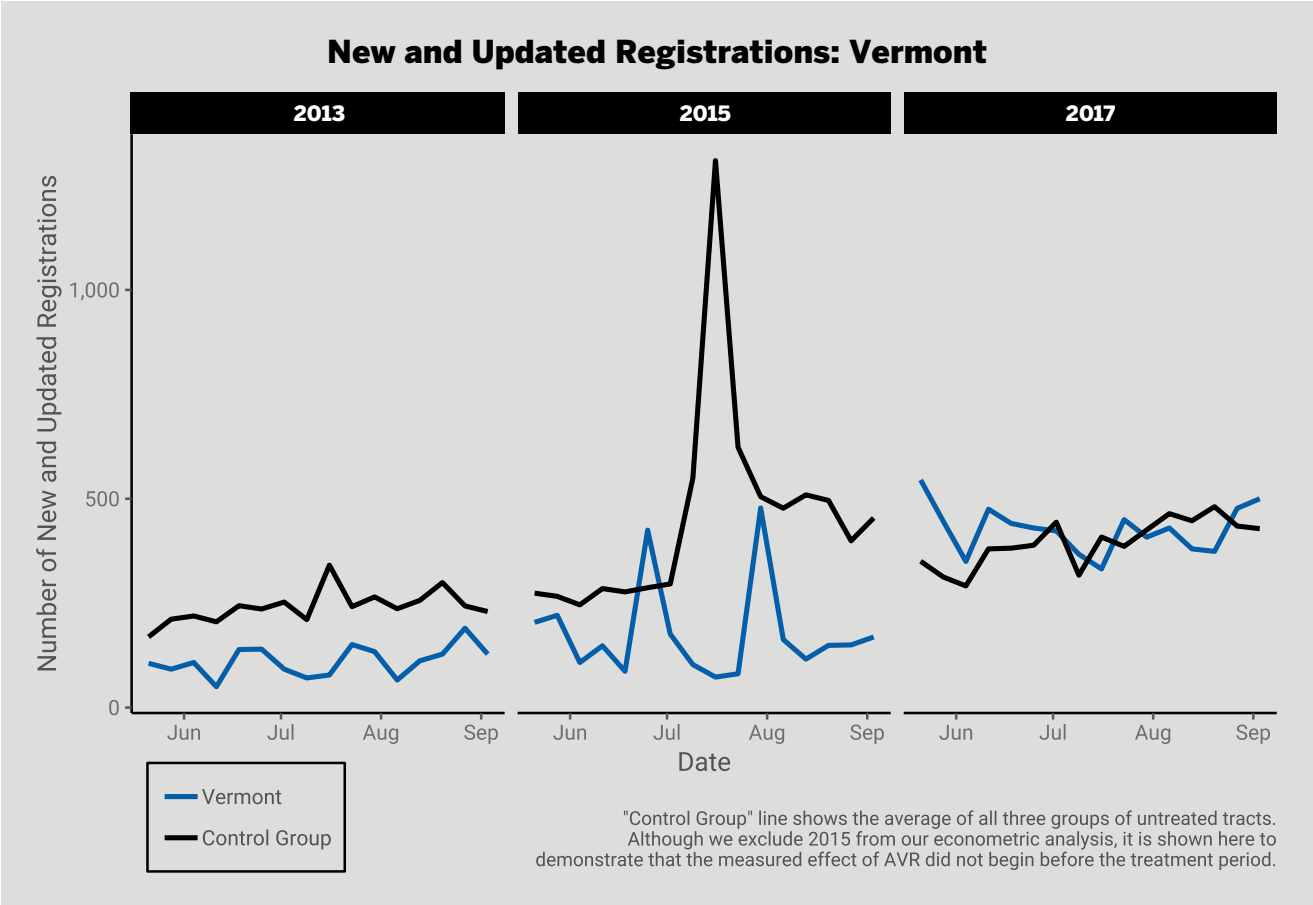
Growth in registration rates due to AVR: **60.2%**

In early 2016, Vermont implemented a new policy that required a state tax filer to include a driver's license number or state ID number.[30] The data suggest that this policy encouraged many to go to the DMV to renew their licenses. This surge of DMV visitors led to many new registrations — a surge that had nothing to do with AVR but was nonetheless a positive outcome. This new policy meant that registrations in the first 20 weeks of 2017 were far higher than the first 20 weeks of 2013. Because it is impossible to know what proportion of this increase was due to the new tax-filing policy and what proportion was due to AVR, we exclude these first 20 weeks from our analysis.

Our model estimates that, without AVR, Vermont would have registered 266 voters each week in 2017. Vermont actually registered an average of 427 voters each week — a 60.2 percent increase.

State Profile:

- Passage type: legislative
- Implementation date: January 1, 2017
- Method of opt-out: front-end (point-of-service)
- Registration rate pre-AVR: 89.22%
- % noncitizen population: 2.2%
- Car ownership rate: 93.2%



Colorado

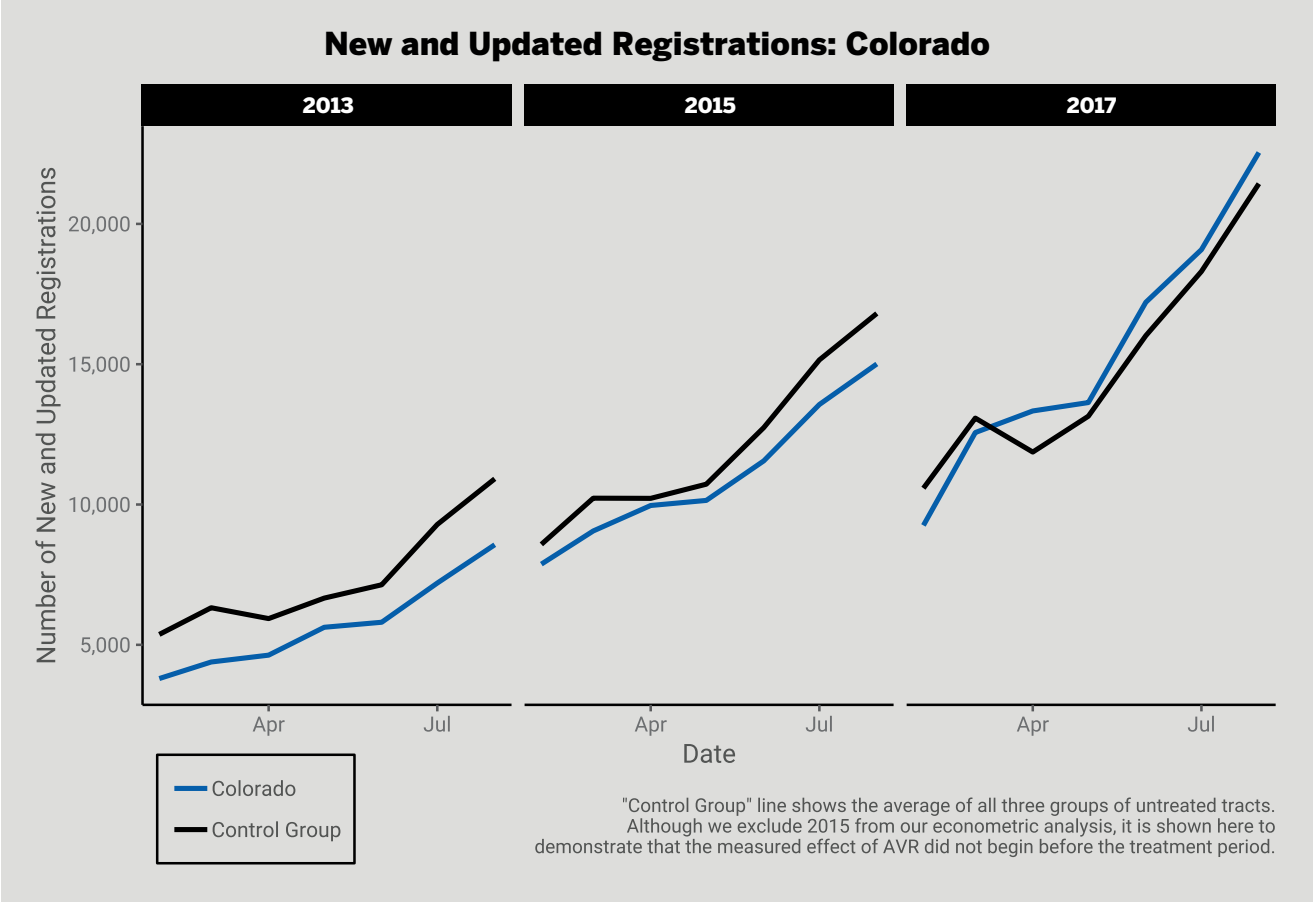
Growth in registration rates due to AVR: **16.0%**

At the end of 2016, Colorado changed the way its voter file data are reported.[31] For this reason, we cannot compare weekly registration numbers in the state from 2013 to 2017 as we do in others. While we can still match Colorado with other states, we must measure the number of *monthly* registrations per tract to account for this data limitation. Because Colorado did not implement AVR until February 2017, we run our model from February through August 2017. These may be somewhat conservative estimates, because Colorado did not immediately implement AVR statewide.[32]

Our model estimates that, without AVR, Colorado would have registered an average of 13,258 voters each month. But Colorado actually registered an average of 15,374 voters per month — a 16.0 percent increase.

State Profile:

- Passage type: administrative
- Implementation date: tested at certain locations February 2017, subsequently implemented statewide
- Method of opt-out: front-end (point-of-service)
- Registration rate pre-AVR: 87.25%
- % noncitizen population: 7.0%
- Car ownership rate: 94.56%



Alaska

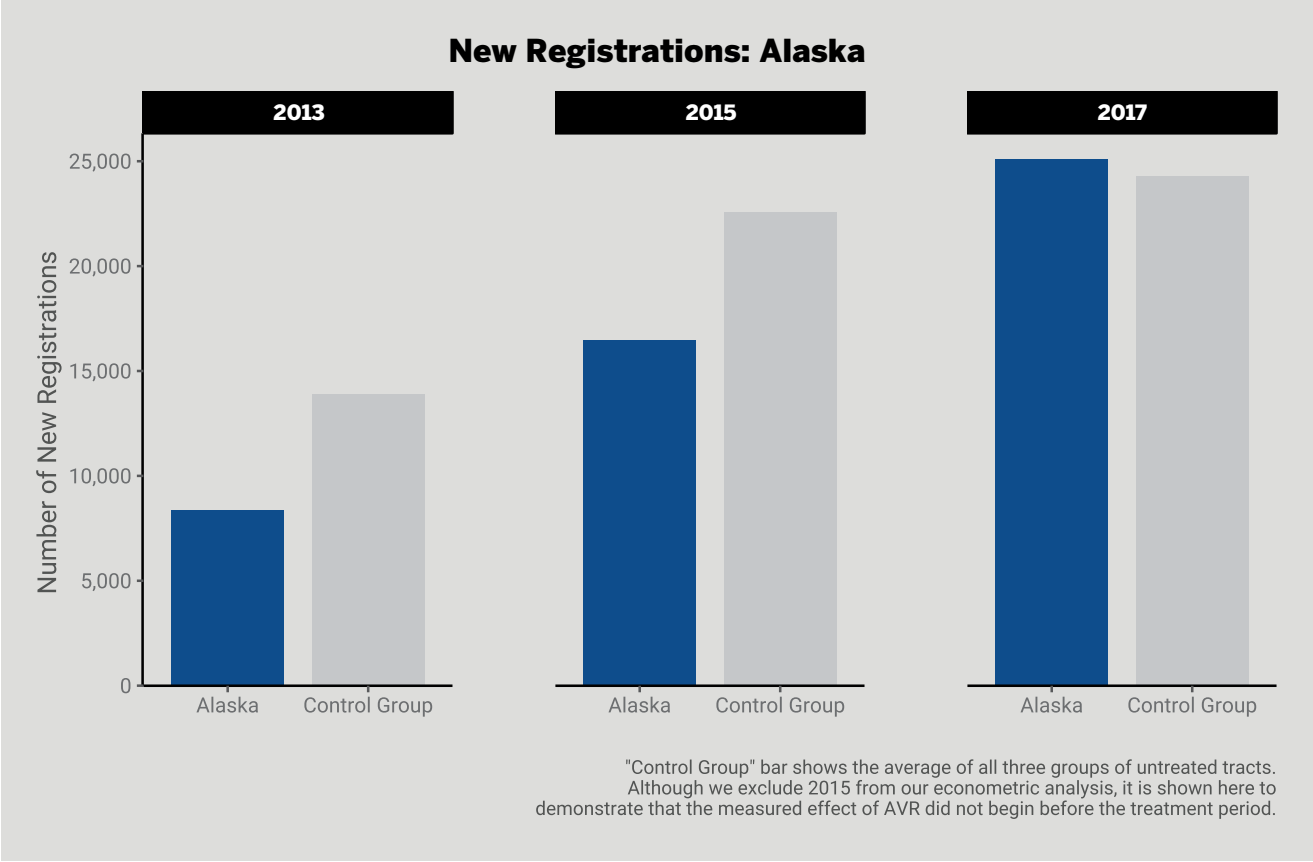
Growth in registration rates due to AVR: **33.7%**

Alaska implemented AVR as of March 1, 2017, but rather than operating primarily through the DMV, Alaska registers citizens through its Permanent Fund Dividend (PFD).[33] The PFD annually distributes money from the profit of the state's oil production to all Alaskans who sign up for the program.[34] Since Alaska sends out PFD mailers only once a year,[35] our model must use data at the *yearly* level.

Our model estimates that, without AVR, Alaska would have registered just over 18,750 voters in 2017. But Alaska actually registered 25,077 — a 33.7 percent increase.

State Profile:

- Passage type: legislative
- Implementation date: March 1, 2017
- Method of opt-out: back-end (post-transaction mailer)
- Registration rate pre-AVR: 100.16%[36]
- % noncitizen population: 4.1%
- Car ownership rate: 90.5%



California

Growth in registration rates due to AVR: **26.8%**

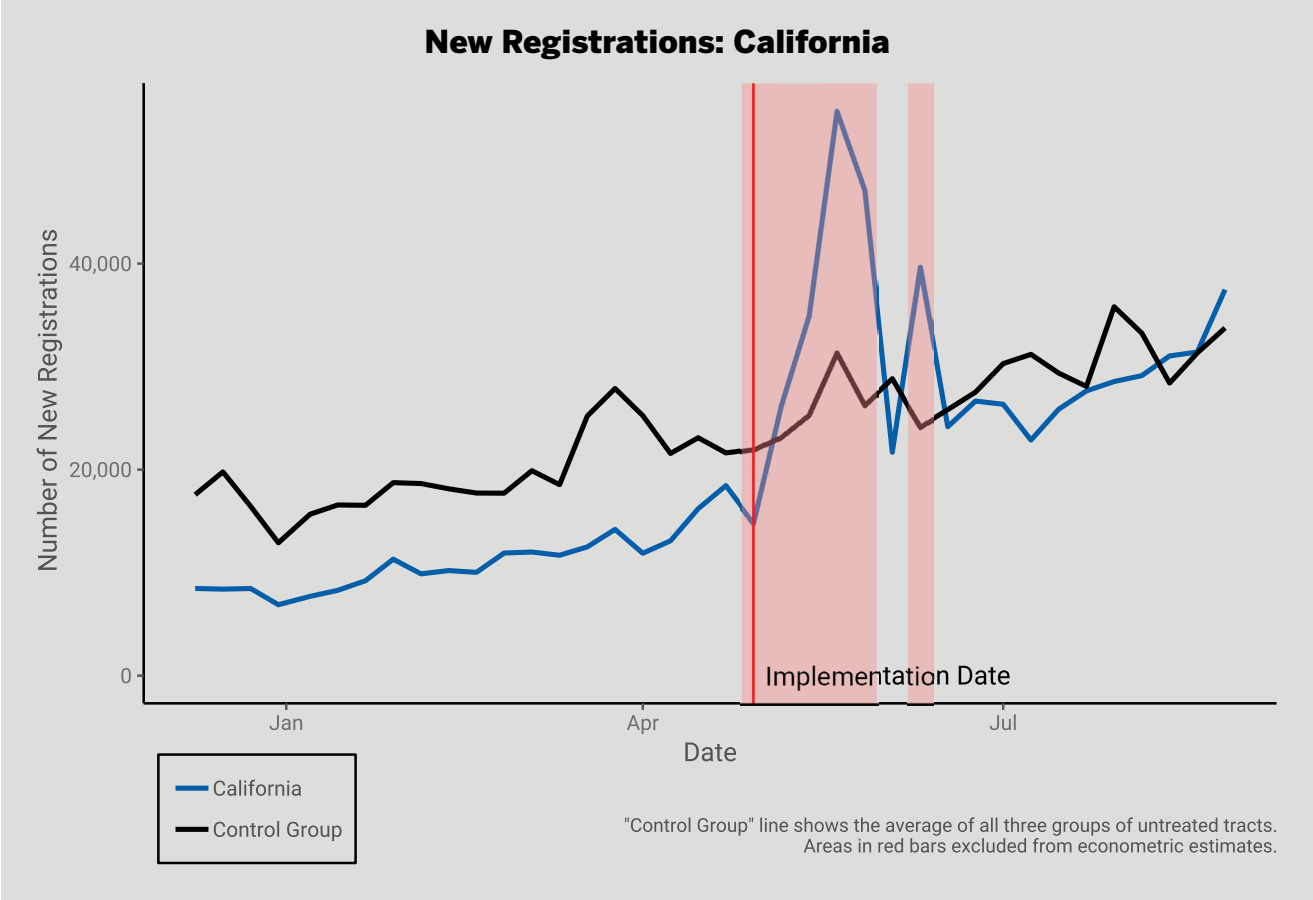
The state of California places certain restrictions on what users of their voter file data may publish or disclose. To comply with these restrictions, we did not geocode voters to their home census tracts, but instead used zip codes for both treatment and control groups.

For California, we created a model that compared registrations in California and control zip codes in the period immediately before and following the implementation of AVR. In California, we compare the 20 weeks before implementation in April 2018 with the 18 weeks following implementation. To avoid overestimating the impact of AVR, the weeks leading up to the registration deadline for California's primary and the week of the state's primary election day have been excluded.

Our model estimates that, without AVR, California would have registered 21,876 voters each week after implementation (excluding the weeks impacted by the primaries). But California actually registered an average of almost 28,000 voters per week during this period — a 26.8 percent increase.

State Profile:

- Passage type: legislative
- Implementation date: April 23, 2018
- Method of opt-out: front-end (point-of-service)
- Registration rate pre-AVR: 79.06%
- % noncitizen population: 16.4%
- Car ownership rate: 93.6%



Rhode Island

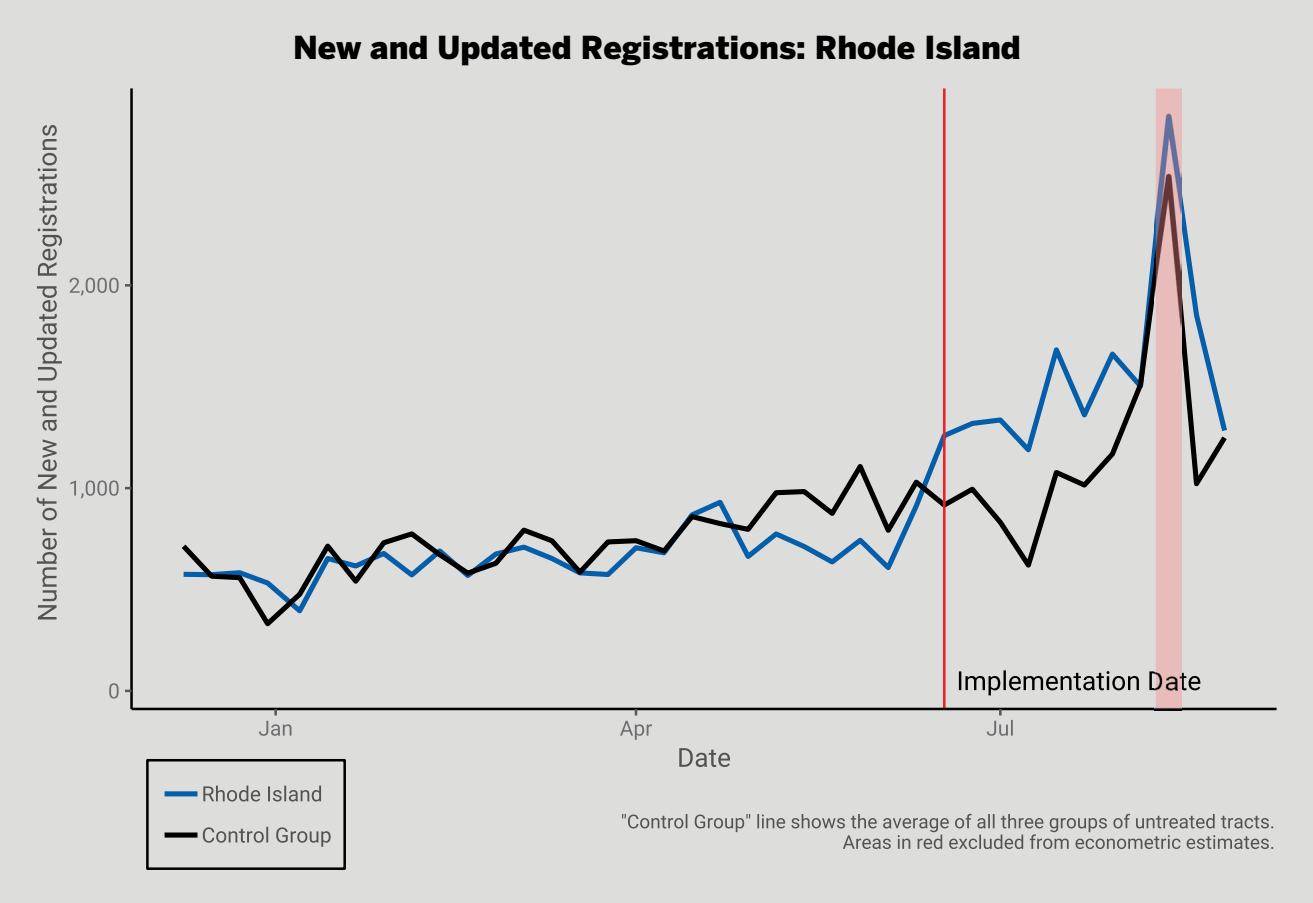
Growth in registration rates due to AVR: **47.4%**

For Rhode Island, we created a model that compared registrations in Rhode Island and control tracts in the period immediately before and following the implementation of AVR. In Rhode Island, we compare the 27 weeks before implementation in June 2018 with the 11 weeks following implementation. We exclude the week of Rhode Island's primary to avoid overestimating the impact of AVR.

Our model estimates that, without AVR, Rhode Island would have registered 1,071 voters each week after implementation (with the exception of the primary week). But Rhode Island actually registered an average of 1,578 voters per week during this period — a 47.4 percent increase.

State Profile:

- Passage type: legislative
- Implementation date: June 11, 2018
- Method of opt-out: front-end (point-of-service)
- Registration rate pre-AVR: 87.25%
- % noncitizen population: 7.1%
- Car ownership rate: 90.3%



Washington, DC

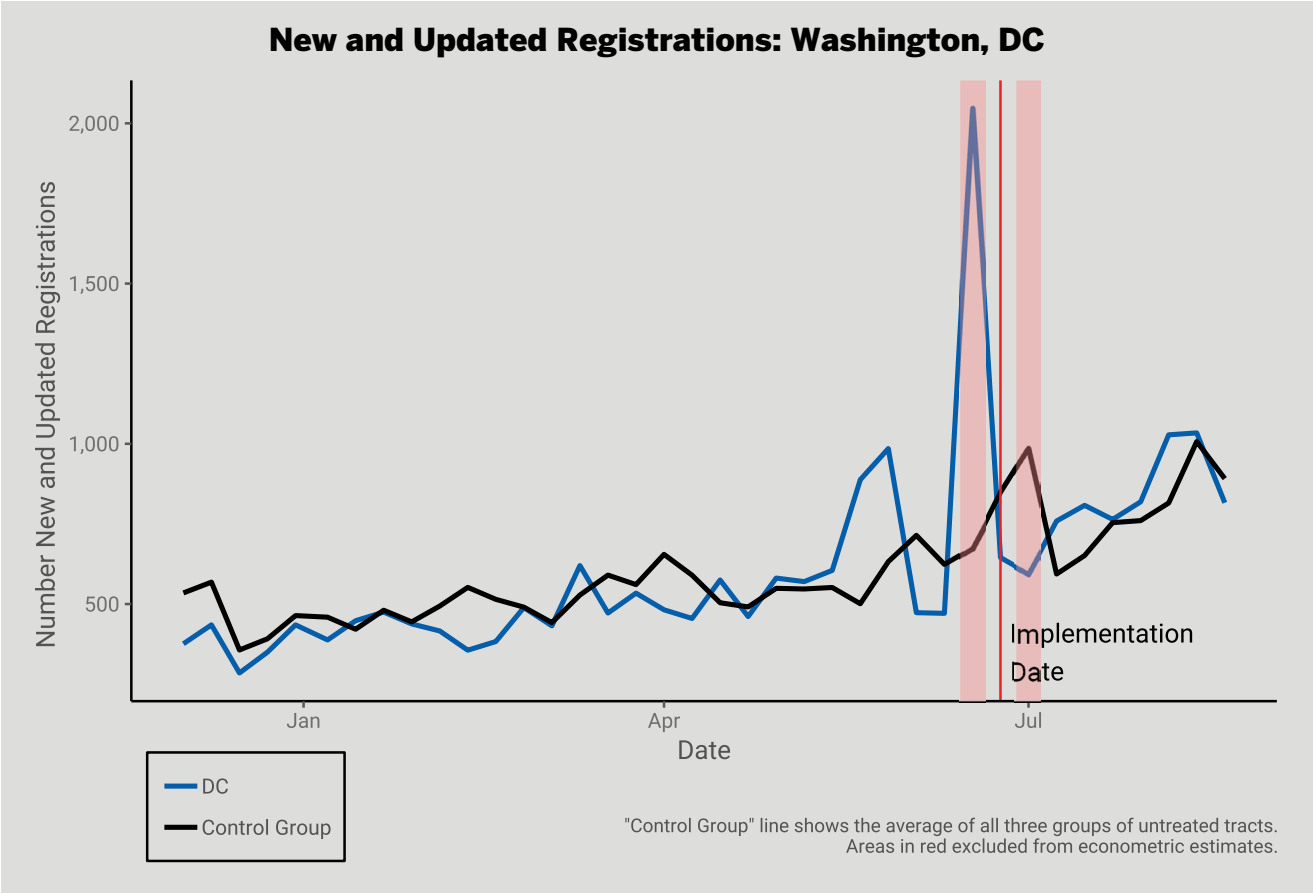
Growth in registration rates due to AVR: **9.4%**

For DC, we created a model that compared registrations in DC and control tracts in the period immediately before and following the implementation of AVR. In DC, we compare the 29 weeks before implementation in June 2018 with the 9 weeks following implementation. To avoid skewing the analysis, week 25 was excluded since it featured the District's primary election. Week 27 was also excluded, as many DC tracts match to Washington State tracts, where the primary election in week 27 distorts the analysis.

Our model estimates that without AVR, Washington, DC, would have registered 763 voters each week after implementation (with the exception of the excluded week). But Washington, DC, actually registered an average of 834 voters per week in each tract during this period — a 9.4 percent increase.

State Profile:

- Passage type: legislative
- Implementation date: June 26, 2018
- Method of opt-out: front-end (point-of-service)
- Registration rate pre-AVR: 99.84%
- % noncitizen population: 8.4%
- Car ownership rate: 64.3%



Moving Forward: The AVR of Tomorrow

The data from this report make clear that certain factors matter more than others in the success of an AVR system in a state. These key takeaways include:

There is little evidence that one particular version of AVR works uniformly better than others. We did not find that certain distinctions between AVR systems (such as method of opt-out) were particularly meaningful. For instance, states with back-end opt-out like Oregon and Alaska did not achieve categorically higher levels of registration increases compared with states with a front-end opt-out.

Automatic voter registrations can be a successful policy no matter the jurisdiction. We do not find that AVR is more effective in states that lean left (like Oregon) or right (like Georgia). Nor has AVR been more effective in large California than in small Rhode Island. The most striking result of this study is how well automatic voter registration works across the country, boosting registration rates in a wide variety of states.

States should choose implementing agencies likely to reach many residents. Washington, DC, a city where just 64.3 percent of households own vehicles, has only implemented AVR at the DMV. This means that fewer residents are exposed to AVR, likely explaining why the impact of AVR in DC was small compared with other states in our study, all of which have car ownership rates that exceed 90 percent. This may be illuminating for other states considering AVR. In New York State, for instance, just 71 percent of households own cars, and this percentage is far lower in New York City. The state would do well to consider adding agencies beyond the DMV to ensure that AVR reaches a larger pool of potential voters. The addition of agencies beyond the DMV would be especially useful in ensuring a diverse electorate, as low-income residents are the least likely to own cars and interact with the DMV.[37]

There is also evidence that the frequency with which individuals visit a designated AVR agency can impact the effect of AVR. California, Rhode Island, and Vermont, for instance, all require their drivers to renew their licenses at least once every five years (most states require drivers to renew only every eight or more years). These states all saw impressive gains from AVR, indicating that individuals who visit AVR agencies more frequently (in this case, DMVs) may be more likely to accept the default option presented to them.

Technical Appendix

In an ideal world, we would know exactly how many registered and unregistered citizens visited each AVR agency each day. We would know what share of these eligible individuals registered to vote before AVR went into effect and how many were registered afterward. Most election administrators, however, do not track the data at this level. As such, we use the statewide voter file to build a model to assess the impact of AVR. For each of the AVR states included in this study, we geocoded voters to the census tracts in which they reside.[38] We then calculated how many voters were registered in each week in each census tract in the states that implemented AVR (at AVR and non-AVR agencies). These numbers form the bases for each analysis.

To account for election-cycle impacts and seasonality in the data, we do not always compare the period immediately before AVR was implemented with the period immediately after. Some states, for instance, implemented AVR in early 2017. Comparing the number of registrations in each census tract in 2017 with the same number from 2016 would underestimate the number of new registrations because far more individuals register to vote in federal election years. In the case of states that implemented in 2016 or 2017, we compare weekly registration counts in 2017 with weekly registration counts in 2013 — the same spot in the previous four-year election cycle.

Of course, we cannot simply attribute any growth in the number of weekly registrations from the period before implementation to the period after to AVR; it is likely that there are other influences causing the overall number of weekly registrations to increase or decrease. These influences would exist irrespective of whether AVR was implemented or not, and therefore need to be controlled for.

We do this through using a statistical technique called "matching."[39] The idea is simple: for every census tract where AVR was implemented, we look at other census tracts around the country to find census tracts where AVR was *not* implemented but which are otherwise similar. Because we do not have voter-file data from every state in the country, not every non-AVR tract is available for matching.[40] We match these census tracts based on multiple criteria that influence registration rates:[41] the growth in voting-age population between 2013 and 2017, racial and ethnic demographics, education levels, and others.[42] We match each treated census tract to the three[43] most similar untreated tracts.[44] These control tracts can come from any control state: a census tract in Georgia, for instance, might match to one tract in Florida, one in North Carolina, and one in New York. No state is singularly similar to Georgia; in aggregate, however, these matched census tracts create a group of control tracts that do look much like Georgia. We allow the same control tract to match with multiple treatment tracts (called "matching with replacement"), and our regressions weight observations based on the number of times they match.

After matching "treated" census tracts (tracts in states that have implemented AVR) to "untreated" census tracts (tracts in states that have not implemented AVR), we are able to build a strong control set (hereafter referred to as "control tracts"). Any growth in weekly registrations in treated tracts *above and beyond* the growth in registrations in the control tracts can be attributed to automatic voter registration. To determine this impact, we run a simple difference-in-differences model.[45]

Below, we present the demographics of each state,[46] the demographics of the control tracts to which the treated tracts were matched, and the difference-in-differences model. After presenting the results from these models, we discuss the potential of using time series analyses rather than matched difference-in-differences.

Overview for Oregon, Georgia, Vermont, Colorado, and Alaska

Each of these states implemented in either 2016 or at the beginning of 2017; therefore, the model for these five states is essentially the same.[47] To avoid state-specific impacts from the 2016 election that cannot be accounted for in the matching process, we exclude 2016 from the analysis. Because some of the untreated census tracts had local elections in the fall of 2018, we limit our difference-in-differences models to the first 35 weeks (roughly eight months) in 2013 and 2017.

The table below presents the results of matching on this set of states:

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
Tract-Level Variables	**Treated**	**Control**	**Treated**	**Control**	**Mean Diff**	**eQQ Med**	**eQQ Mean**	**eQQ Max**
Citizen Voting-Age Population	3,440.40	3,098.44	3,440.40	3,444.17	98.90	76.83	72.65	57.05
Citizen Voting-Age Population Change (2013–2017)	0.06	0.05	0.06	0.05	65.10	67.31	62.75	53.19
Number of Registrations in 2013	83.26	74.73	83.26	78.27	41.44	17.77	34.77	39.54
% Latino	0.12	0.14	0.12	0.12	64.01	47.51	44.02	23.68
% Non-Hispanic Black	0.16	0.15	0.16	0.15	18.19	41.65	48.45	48.44
% Non-Hispanic White	0.65	0.63	0.65	0.65	88.62	90.09	87.41	80.85
% Noncitizens	0.05	0.07	0.05	0.05	96.49	63.82	60.91	56.75
% Without a Car	0.07	0.13	0.07	0.07	95.73	88.42	86.16	74.19
% Moved in Past 12 Months	0.17	0.14	0.17	0.16	73.55	90.20	86.52	77.86
% With Some College Education	0.74	0.73	0.74	0.74	98.52	68.01	58.02	33.61
Median Income	60,768.82	62,189.42	60,768.82	60,519.29	82.43	56.93	44.69	24.49
% Unemployed	0.07	0.08	0.07	0.07	98.81	53.91	40.61	-19.63
Median Age	38.67	40.64	38.67	38.98	84.47	67.43	67.06	68.32
County-Level Variables								
% Registered in 2014	0.77	0.82	0.77	0.81	29.89	31.59	23.67	23.27
Presidential Republican Voteshare (2016)	0.46	0.43	0.46	0.46	94.52	59.07	54.54	15.98

Oregon

Variable	Treatment Group	Control Group
Tract-Level Variables		
Citizen Voting-Age Population	2,948,750	2,936,343
Citizen Voting-Age Population Change (2013–2017)	6.3%	6.2%
Number of Registrations in 2013	55,184	74,981
% Latino	12.7%	11.4%
% Non-Hispanic Black	1.8%	2.5%
% Non-Hispanic White	76.5%	74.8%
% Noncitizens	5.7%	5.5%
% Without a Car	7.3%	6.0%
% Moved in Past 12 Months	18.0%	17.0%
% With Some College Education	77.2%	77.7%
Median Income	$60,265	$66,595
% Unemployed	7.0%	6.6%
Median Age	40	40
County-Level Variables		
% Registered in 2014	76.9%	80.2%
Presidential Republican Voteshare (2016)	39.3%	40.4%

Regression output:

Dependent Variable: Registrations	
Oregon in 2017	0.422*** (0.052)
Oregon	-0.334*** (0.037)
2017	1.400*** (0.039)
Constant	1.587*** (0.028)
Observations	129,096
R^2	0.132
Adjusted R^2	0.132
Residual Std. Error	3.413 (df = 129092)
F Statistic	6,539.172*** (df = 3; 129092)
Note:	*p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered at census tract level. Data from first 35 weeks in 2013 and 2017.

As discussed above, we limit the analysis to the first 35 weeks of 2013 and 2017 to avoid the impact that local elections in the fall of 2013 and 2017 might have on our estimates. These local elections might have increased the number of individuals registering to vote — an increase unrelated to AVR.

The table at left shows that the average control tract had 1.40 more weekly registrations in 2017 than in 2013. This represents the expected increase in registrations in Oregon census tracts had the state not implemented AVR. However, the real Oregon census tracts increased by this amount *plus* an additional 0.42, for a total increase of 1.8 new registrations per week per tract.

The additional 0.42 registrations is the estimated impact of AVR in Oregon — an increase of 15.9 percent. This percentage is calculated by comparing the number of registrations our model predicts would have occurred in the absence of AVR with how many actually happened.

This increase is significant at the 99 percent level.

Georgia

Variable	Treatment Group	Control Group
Tract-Level Variables		
Citizen Voting-Age Population	7,148,450	7,099,101
Citizen Voting-Age Population Change (2013–2017)	6.9%	6.5%
Number of Registrations in 2013	210,173	147,051
% Latino	9.3%	10.5%
% Non-Hispanic Black	30.9%	28.2%
% Non-Hispanic White	53.6%	55.3%
% Noncitizens	5.9%	5.5%
% Without a Car	6.6%	7.1%
% Moved in Past 12 Months	15.7%	15.8%
% With Some College Education	71.5%	71.6%
Median Income	$58,442	$55,009
% Unemployed	7.7%	7.6%
Median Age	37.1	37.5
County-Level Variables		
% Registered in 2014	75.4%	83.0%
Presidential Republican Voteshare (2016)	50.8%	50.6%

Regression output:

Dependent Variable: Registrations	
Georgia in 2017	3.014*** (0.079)
Georgia	0.624*** (0.040)
2017	1.172*** (0.030)
Constant	1.423*** (0.026)
Observations	337,498
R^2	0.246
Adjusted R^2	0.246
Residual Std. Error	5.102 (df = 337494)
F Statistic	36,677.550*** (df = 3; 337494)
Note:	*p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered at census tract level. Data from first 35 weeks in 2013 and 2017.

As discussed above, we limit the analysis to the first 35 weeks of 2013 and 2017 to avoid the impact that local elections in the fall of 2013 and 2017 might have on our estimates. These local elections might have increased the number of individuals registering to vote — an increase unrelated to AVR.

The table at left shows that the average control tract had 1.17 more weekly registrations in 2017 than in 2013. This represents the expected increase in registrations in Georgia census tracts had the state not implemented AVR. However, the real Georgia census tracts increased by this amount *plus* an additional 3.01, for a total increase of 4.19 new registrations per week per tract.

The additional 3.01 registrations is the estimated impact of AVR in Georgia — an increase of 93.7 percent. This percentage is calculated by comparing the number of registrations our model predicts would have occurred in the absence of AVR with how many actually happened.

This increase is significant at the 99 percent level.

Vermont

Variable	Treatment Group	Control Group
Tract-Level Variables		
Citizen Voting-Age Population	493,455	495,973
Citizen Voting-Age Population Change (2013–2017)	1.1%	1.6%
Number of Registrations in 2013	8,000	11,422
% Latino	1.8%	3.3%
% Non-Hispanic Black	1.2%	2.1%
% Non-Hispanic White	93.2%	90.6%
% Noncitizens	2.0%	2.1%
% Without a Car	6.6%	5.8%
% Moved in Past 12 Months	13.3%	11.3%
% With Some College Education	75.4%	76.3%
Median Income	$59,764	$67,389
% Unemployed	4.4%	4.6%
Median Age	42.8	43.0
County-Level Variables		
% Registered in 2014	84.0%	81.9%
Presidential Republican Voteshare (2016)	30.6%	34.4%

In early 2016, Vermont implemented a new policy under which state tax filers were required to include their driver's license number or state ID number. It appears that this policy encouraged residents to go to the DMV to renew their driver's licenses and, subsequently, get registered to vote. This can be seen in the data: the increase from March 2013 to March 2017 is much higher than the increase in other months.

This policy, of course, has nothing to do with automatic voter registration. In order to isolate the impact of the new tax-filing policy from the impact of AVR, we exclude the first 20 weeks of the period. As discussed above, we exclude the period after week 35 in 2013 and 2017 to avoid the impact that local elections in the fall of 2013 and 2017 might have on our estimates. These local elections might have increased the number of individuals registering to vote — an increase unrelated to AVR.

We look, therefore, at the number of registrations in the 20th-35th weeks of 2013 and 2017 in Vermont and the matched untreated census tracts.

Regression output:

Dependent Variable: Registrations	
Vermont in 2017	0.876*** (0.102)
Vermont	-0.710*** (0.061)
2017	0.846*** (0.054)
Constant	1.319*** (0.047)
Observations	17,056
R^2	0.085
Adjusted R^2	0.084
Residual Std. Error	3.267 (df = 17052)
F Statistic	525.291*** (df = 3; 17052)
Note:	*p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered at census tract level. Data from weeks 20–35 in 2013 and 2017.

The table at left shows that the average control tract had 0.85 more weekly registrations in 2017 than in 2013. This represents the expected increase in registrations in Vermont census tracts had the state not implemented AVR. However, the real Vermont census tracts increased by this amount *plus* an additional 0.88, for a total increase of 1.72 new registrations per week per tract.

The additional 0.88 registrations is the estimated impact of AVR in Vermont — an increase of 60.2 percent. This percentage is calculated by comparing the number of registrations our model predicts would have occurred in the absence of AVR with how many actually happened.

This increase is significant at the 99 percent level.

Colorado

Variable	Treatment Group	Control Group
Tract-Level Variables		
Citizen Voting-Age Population	3,874,810	3,946,978
Citizen Voting-Age Population Change (2013–2017)	9.3%	8.3%
Number of Registrations in 2013	81,146	93,737
% Latino	21.3%	17.8%
% Non-Hispanic Black	3.8%	3.9%
% Non-Hispanic White	68.7%	68.1%
% Noncitizens	5.9%	6.5%
% Without a Car	5.0%	5.1%
% Moved in Past 12 Months	18.6%	17.2%
% With Some College Education	78.9%	77.9%
Median Income	$71,926	$70,672
% Unemployed	5.4%	5.6%
Median Age	37.7	38.2
County-Level Variables		
% Registered in 2014	78.5%	78.3%
Presidential Republican Voteshare (2016)	43.3%	43.4%

According to the Colorado Department of State, Colorado switched its National Change of Address (NCOA) list provider at the end of 2016. The NCOA is the list that is created when people tell the post office to send their mail to a different address. This switch changed the way in which updated registrations were indicated in the voter file. Prior to late 2016, NCOA updates were processed throughout the month, with a new registration date indicating the date on which the change was made. Beginning in late 2016, however, all NCOA address updates in any month were given the same new registration date.[48] Because of this change, we cannot compare weekly registration numbers in Colorado from 2013 with 2017.

Although the matching procedure is the same for Colorado as for other states, our dependent variable in Colorado measures the number of *monthly* registrations in each census tract in 2013 and 2017. We began the analysis in February in each year (the month in 2017 in which Colorado implemented AVR) and ran it through August to avoid the interference of fall elections in untreated census tracts. Because Colorado did not initially implement the program statewide, these may be somewhat conservative estimates.

Regression output:

Dependent Variable: Registrations	
Colorado in 2017	1.715*** (0.264)
Colorado	-1.345*** (0.151)
2017	6.112*** (0.211)
Constant	5.978*** (0.120)
Observations	38,164
R^2	0.185
Adjusted R^2	0.185
Residual Std. Error	12.174 (df = 38160)
F Statistic	2,887.712*** (df = 3; 38160)
Note:	*p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered at census tract level. Data from February to August of 2013 and 2017.

The table at left shows that the average control tract had 6.11 more monthly registrations in 2017 than in 2013. This represents the expected increase in registrations in Colorado census tracts had the state not implemented AVR. However, the real Colorado census tracts increased by this amount *plus* an additional 1.72, for a total increase of 7.83 new registrations per month per tract.

The additional 1.72 registrations is the estimated impact of AVR in Colorado — an increase of 16.0 percent. This percentage is calculated by comparing the number of registrations our model predicts would have occurred in the absence of AVR with how many actually happened.

This increase is significant at the 99 percent level.

Alaska

Variable	Treatment Group	Control Group
Tract-Level Variables		
Citizen Voting-Age Population	527,810	531,303
Citizen Voting-Age Population Change (2013–2017)	4.6%	6.6%
Number of Registrations in 2013	8,362	13,896
% Latino	6.8%	10.2%
% Non-Hispanic Black	3.1%	5.2%
% Non-Hispanic White	61.5%	69.4%
% Noncitizens	3.3%	3.9%
% Without a Car	10.6%	7.2%
% Moved in Past 12 Months	18.9%	17.9%
% With Some College Education	75.6%	76.6%
Median Income	$78,706	$62,980
% Unemployed	8.2%	7.6%
Median Age	34.7	36.0
State-Level Variables		
% Registered in 2014	98.1%	78.6%
Presidential Republican Voteshare (2016)	51.4%	45.8%

Automatic voter registration works differently in Alaska than it does in the other states included in this study. In each of the other states we examine, AVR is implemented at the DMV, which means its effect can be examined on a daily or weekly basis. However, in Alaska, AVR is implemented through its Permanent Fund Dividend. The PFD automatically registers voters only once each year. This means that any effect from AVR must be calculated at the annual level.

Although the matching procedure is the same for Alaska as for the other states, our dependent variable in Alaska measures the number of *annual* registrations in each census tract in 2013 and 2017.

Regression output:

Dependent Variable: Registrations	
Alaska in 2017	38.309*** (7.393)
Alaska	-33.539*** (4.734)
2017	62.994*** (4.150)
Constant	84.218*** (4.097)
Observations	854
R^2	0.257
Adjusted R^2	0.254
Residual Std. Error	111.003 (df = 850)
F Statistic	98.052*** (df = 3; 850)
Note:	*p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered at census tract level. Data from 2013 and 2017.

The table at left shows that the average control tract had 63.0 more registrations in 2017 than in 2013. This represents the expected increase in registrations in Alaska census tracts had the state not implemented AVR. However, the real Alaska census tracts increased by this amount *plus* an additional 38.31, for a total increase of 101.31 new registrations per tract.

The additional 38.3 registrations is the estimated impact of AVR in Alaska — an increase of 33.7 percent. This percentage is calculated by comparing the number of registrations our model predicts would have occurred in the absence of AVR with how many actually happened.

This increase is significant at the 99 percent level.

California

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
Zip Code-Level Variables	**Treated**	**Control**	**Treated**	**Control**	**Mean Diff**	**eQQ Med**	**eQQ Mean**	**eQQ Max**
Voting-Age Population	18,404.90	10,582.12	18,404.90	17,530.88	88.83	84.21	79.62	72.55
Number of New Registrations in Pre-Period	136.33	167.88	136.33	240.54	-230.36	-239.04	-240.23	-206.31
% Latino	0.31	0.09	0.31	0.26	77.00	75.84	76.06	71.72
% Non-Hispanic Black	0.04	0.09	0.04	0.10	-38.83	-257.71	-208.28	-140.65
% Non-Hispanic White	0.51	0.77	0.51	0.54	89.02	91.08	89.40	85.85
% Noncitizens	0.11	0.04	0.11	0.10	95.33	92.02	91.85	89.70
% Without a Car	0.06	0.08	0.06	0.07	79.21	-34.87	-19.72	-38.71
% Moved in Past 12 Months	0.14	0.12	0.14	0.13	79.49	74.91	73.85	67.91
% With Some College Education	0.72	0.71	0.72	0.71	-85.03	74.74	68.40	59.20
Median Income	68,767.16	60,204.83	68,767.16	67,102.30	80.56	71.37	69.68	68.10
% Unemployed	0.08	0.07	0.08	0.08	74.64	81.46	79.23	70.01
Median Age	40.66	42.78	40.66	40.63	98.51	58.59	76.12	78.64
County-Level Variables								
% Registered in 2016	0.66	0.83	0.66	0.73	59.43	53.78	47.01	37.08
Presidential Republican Voteshare (2016)	0.36	0.52	0.36	0.44	49.55	65.63	55.62	33.77

In California, we do not have a full year of post-implementation data. Therefore, we construct a difference-in-differences model within the implementation year. Because of restrictions on geocoding addresses in the California voter file, we ran the same analyses but at the zip code level instead.[49] We look to see whether zip codes in California increased their registrations more after California implemented AVR than the control zip codes.

We continue to match on the number of registrations in the pre-period, which has changed from 2013 to the 20 weeks immediately before implementation (from December 4, 2017 to April 22, 2018).[50] We also match on the share of citizen voting-age population registered as of the 2016 election instead of the 2014 election.[51]

In California, we compare the 20 weeks before implementation in 2018 with the 18 weeks after implementation. We begin our pre-period in December 2017 to avoid any impact from local elections the month before. To avoid overestimating the impact of AVR, the weeks leading up to the registration deadline for California's primary and the week including the state's primary election day have been excluded.

Regression output:

Dependent Variable: Registrations	
California Post-Implementation	3.618*** (0.601)
California	-5.211*** (0.503)
Post-Implementation	6.685*** (0.490)
Constant	12.020*** (0.472)
Observations	107,328
R^2	0.080
Adjusted R^2	0.080
Residual Std. Error	26.697 (df = 107324)
F Statistic	3,112.430*** (df = 3; 107324)
Note:	*p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered at zip code level. Data from 12/4/2017 to 8/26/2018 Weeks 17–21 and 23 of 2018 excluded.

The second table on the previous page shows that the average control zip code had 6.69 more weekly registrations in the period after April 23 than in the period immediately preceding it. This represents the expected increase in registrations in California zip codes had the state not implemented AVR. However, the real California zip codes increased by this amount *plus* an additional 3.62, for a total increase of 10.3 new registrations per week per zip code.

The additional 3.62 registrations is the estimated impact of AVR in California — an increase of 26.8 percent. This percentage is calculated by comparing the number of registrations our model predicts would have occurred in the absence of AVR with how many actually happened.

This increase is significant at the 99 percent level.

Rhode Island

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
Tract-Level Variables	**Treated**	**Control**	**Treated**	**Control**	**Mean Diff**	**eQQ Med**	**eQQ Mean**	**eQQ Max**
Citizen Voting-Age Population	3,268.75	3,098.80	3,268.75	3,229.05	76.64	32.95	55.56	50.26
Number of New Registrations in Pre-Period	74.42	76.60	74.42	82.57	-273.66	-141.39	-38.10	26.16
% Latino	0.14	0.14	0.14	0.15	-506.26	-159.19	-105.85	-94.73
% Non-Hispanic Black	0.06	0.15	0.06	0.08	79.18	54.46	51.02	47.70
% Non-Hispanic White	0.73	0.63	0.73	0.71	79.71	76.45	61.07	34.38
% Noncitizens	0.06	0.07	0.06	0.06	40.43	-101.82	-26.30	13.14
% Without a Car	0.10	0.13	0.10	0.11	75.46	42.10	39.34	21.64
% Moved in Past 12 Months	0.14	0.14	0.14	0.13	-53.01	10.03	18.54	7.00
% With Some College Education	0.72	0.73	0.72	0.73	33.58	44.29	42.19	34.41
Median Income	63,071.82	62,010.49	63,071.82	65,391.58	-118.57	33.97	39.21	37.59
% Unemployed	0.07	0.08	0.07	0.07	18.48	-162.98	-166.67	-114.86
Median Age	40.66	40.68	40.66	40.89	-923.64	-16.23	17.67	48.85
County-Level Variables								
% Registered	0.92	0.96	0.92	0.81	-258.15	-241.73	-129.99	-74.28
Presidential Republican Voteshare (2016)	0.39	0.44	0.39	0.40	80.51	51.38	34.96	14.56

In Rhode Island, we do not have a full year of post-implementation data. Therefore, we construct a difference-in-differences model within the implementation year. We look to see whether census tracts in Rhode Island increased their registrations more in the weeks immediately after Rhode Island implemented AVR than the control census tracts.

We continue to match on the number of registrations in the pre-period, which has changed from 2013 to the 27-week period immediately before implementation (from December 4, 2017, to June 10, 2018).[52] We also match on the share of citizen voting-age population registered as of the 2016 election instead of the 2014 election.

Regression output:

Dependent Variable: Registrations	
Rhode Island Post-Implementation	2.112*** (0.162)
Rhode Island	-0.301*** (0.100)
Post-Implementation	1.705*** (0.097)
Constant	3.058*** (0.074)
Observations	22,274
R^2	0.150
Adjusted R^2	0.150
Residual Std. Error	4.850 (df = 22270)
F Statistic	1,314.778*** (df = 3; 22270)
Note:	*p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered at census tract level. Data from 12/4/2017 to 8/26/2018. Week 31 in 2018 excluded because of primary election distortion.

In Rhode Island, we compare the 27 weeks before implementation in 2018 with the 11 weeks after implementation. We begin our pre-period in December 2017 to avoid any impact from local elections in November 2017. To avoid overestimating the impact of AVR, the week of the deadline for registering for Rhode Island's primary election has been excluded.

The table at left shows that the average control tract had 1.71 more weekly registrations in the period after June 11 than in the period immediately preceding it. This represents the expected increase in registrations in Rhode Island census tracts had the state not implemented AVR. However, the real Rhode Island census tracts increased by this amount *plus* an additional 2.11, for a total increase of 3.82 new registrations per week per tract.

The additional 2.11 registrations is the estimated impact of AVR in Rhode Island — an increase of 47.4 percent. This percentage is calculated by comparing the number of registrations our model predicts would have occurred in the absence of AVR with how many actually happened.

This increase is significant at the 99 percent level.

Washington, DC

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
Tract-Level Variables	**Treated**	**Control**	**Treated**	**Control**	**Mean Diff**	**eQQ Med**	**eQQ Mean**	**eQQ Max**
Citizen Voting-Age Population	2,823.81	3,098.80	2,823.81	2,799.90	91.30	37.70	20.03	0.86
Number of New Registrations in Pre-Period	89.97	83.67	89.97	86.59	46.33	-135.60	-6.10	41.70
% Latino	0.10	0.14	0.10	0.10	98.64	75.90	72.82	59.54
% Non-Hispanic Black	0.50	0.15	0.50	0.44	82.01	77.89	74.79	60.60
% Non-Hispanic White	0.34	0.63	0.34	0.36	93.45	87.22	85.22	78.20
% Noncitizens	0.07	0.07	0.07	0.08	-47.12	60.83	23.75	-27.05
% Without a Car	0.35	0.13	0.35	0.30	78.04	75.74	75.65	71.21
% Moved in Past 12 Months	0.19	0.14	0.19	0.18	87.98	92.77	88.26	72.59
% With Some College Education	0.78	0.73	0.78	0.78	91.17	87.07	81.76	61.16
Median Income	82,936.30	62,010.49	82,936.30	68,515.49	31.09	27.13	26.39	37.72
% Unemployed	0.10	0.08	0.10	0.10	93.99	66.22	67.25	46.60
Median Age	34.94	40.68	34.94	35.34	92.98	33.86	68.64	70.70
District-Level Variables								
% Registered	1.00	0.96	1.00	1.01	78.73	-206.26	-53.19	3.47
Presidential Republican Voteshare (2016)	0.04	0.44	0.04	0.21	57.72	40.40	-30.60	0.00

In Washington, DC, we do not have a full year of post-implementation data. Therefore, we construct a difference-in-differences model within the implementation year. We look to see whether census tracts in the District increased registrations more after they implemented AVR than the control census tracts.

We continue to match on the number of registrations in the pre-period, which has changed from 2013 to the 29 weeks immediately before implementation (from December 5, 2017,to June 26, 2018).[53]

Regression output:

Dependent Variable: Registrations	
DC Post-Implementation	0.404** (0.182)
DC	-0.156 (0.198)
Post-Implementation	1.508*** (0.139)
Constant	2.956*** (0.142)
Observations	16,200
R^2	0.048
Adjusted R^2	0.048
Residual Std. Error	4.912 (df = 16196)
F Statistic	271.307*** (df = 3; 16196)
Note:	*p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered at census tract level. Data from 12/5/2017 to 8/27/2018. Weeks 25 and 27 in 2018 excluded because of primary election distortion.

In Washington, DC, we compare the 29 weeks before implementation in 2018 with the 9 weeks after implementation. We begin our pre-period in December 2017 to avoid any impact from local elections a month earlier. We exclude week 25 in 2018, the week of the primary election in Washington, DC, because of the distorting effect of Election Day registration. Similarly, we exclude week 27 in 2018 because many DC tracts match to Washington State tracts, where the registrations for the primary election in week 27 distort the analysis.

The table at left shows that the average control tract had 1.51 more weekly registrations in the period after June 26 than in the period immediately preceding it. This represents the expected increase in registrations in its census tracts had the District not implemented AVR. However, the real Washington, DC, census tracts increased by this amount *plus* an additional 0.40, for a total increase of 1.91 new registrations per week per tract.

The additional 0.40 registrations is the estimated impact of AVR in Washington, DC — an increase of 9.4 percent. This percentage is calculated by comparing the number of registrations our model predicts would have occurred in the absence of AVR with how many actually happened.

This increase is significant at the 95 percent level.

Findings Hold Even Using a Different Methodology

Statistical estimates of policy impacts are never perfect. While we, and our peer reviewers, believe that the afore-mentioned methodology provides the best estimate of AVR's impact, we built another model to test our hypothesis that AVR is generally helpful in increasing registration rates, and that the states with opt-out placements in one location will not outperform the states with an opt-out in another location. This second model was a time series analysis for the states that implemented AVR in 2016 or 2017.[54]

The two models in this report could be compared to estimating the effect of a drug in a clinical trial. A researcher might find two very similar individuals and could give one of the individuals a drug and the other a placebo. Comparing what happened with each of these two individuals would reveal the impact of the drug. This is similar to our matching method.

A medical researcher could also instead decide to use a patient's own history to investigate the impact of the drug. If a patient has woken up with a headache every day for the past year but takes a pill and tomorrow wakes up *without* a headache, one could surmise that the lack of headache is due to the pill. This would be more similar to a time series analysis.

To do the time series model, we use historical data from each of the states to estimate what would have happened in that state if it had not implemented AVR. This is compared with what actually happened. If the number of actual registrations significantly exceeds the number that the historical data forecast would have occurred without AVR, we attribute that difference to AVR.

As is typical for time series models, we include variables to account for seasonality and election-cycle patterns. We also account for underlying trends to control for natural population growth. In each model, we use statewide daily registration data from January 1, 2010, through December 31, 2017. Because our dependent variable measures daily registration counts, we fit them using a Poisson regression. We conservatively use robust standard errors to ensure the validity of our results.

Time Series Models in Non-AVR States

The first building block for this analysis requires that we run the time series model on non-AVR states. This is for comparison purposes: if AVR had an impact on the number of individuals being registered, we would expect to find a statistically significant effect in states that implemented AVR and insignificant results in states that did not implement.

As noted earlier, we used voter files from 17 states. We present two models for each state: one with a dummy variable that begins on January 1, 2016, and runs through the end of 2017, mirroring the period during which Oregon had AVR. In the second model for each state, we include a dummy for only 2017, roughly corresponding to the period in which AVR began in Georgia and Vermont. These variables measure whether the number of voters being registered in 2016 and 2017 in non-AVR states was higher than each state's history would lead us to expect.

Among our nine comparison states, three are significantly elevated over the entire 2016 and 2017 period. This indicates that, in the case of Oregon, time series analysis may be inappropriate: these states indicate that registration rates were elevated at this time even where AVR was not implemented. When we limit our dummy variable to just 2017, however, Washington State is no longer significantly elevated. The increase in Connecticut is substantially smaller. The effect in New Jersey becomes smaller as well.[55]

(see table page 29)

Time Series Regressions in Non-AVR States										
	CT (1)	**CT (2)**	**FL (3)**	**FL (4)**	**MI (5)**	**MI (6)**	**NV (7)**	**NV (8)**	**NJ (9)**	**NJ (10)**
2016–2017	0.430*** (0.103)		-0.204*** (0.051)		0.037 (0.037)		0.027 (0.075)		0.281** (0.086)	
2017		0.177* (0.086)		-0.239*** (0.047)		0.015 (0.037)		0.123 (0.073)		0.216* (0.084)
Trend	0.0004*** (0.0001)	0.001*** (0.0001)	0.0003*** (0.00002)	0.0002*** (0.00003)	0.0004*** (0.00001)	0.0004*** (0.00002)	0.0005*** (0.00003)	0.0004*** (0.0001)	0.0002*** (0.00003)	0.0003*** (0.0001)
Presidential Election Year	0.583*** (0.113)	0.789*** (0.116)	0.610*** (0.065)	0.486*** (0.064)	0.256*** (0.053)	0.272*** (0.053)	1.094*** (0.067)	1.150*** (0.070)	0.367*** (0.068)	0.522*** (0.067)
Midterm Election Year	-0.135 (0.134)	-0.082 (0.134)	-0.054 (0.070)	-0.094 (0.069)	-0.036 (0.057)	-0.034 (0.058)	0.271** (0.086)	0.301*** (0.081)	-0.397*** (0.091)	-0.346*** (0.085)
Saturday or Sunday	-3.010*** (0.151)	-3.008*** (0.151)	-2.830*** (0.074)	-2.830*** (0.074)	-2.469*** (0.031)	-2.469*** (0.031)	-1.257*** (0.146)	-1.257*** (0.146)	-1.586*** (0.051)	-1.585*** (0.051)
Constant	4.200*** (0.099)	3.954*** (0.102)	6.887*** (0.044)	6.969*** (0.050)	6.320*** (0.041)	6.305*** (0.047)	3.999*** (0.062)	3.977*** (0.071)	5.799*** (0.061)	5.668*** (0.081)
McFadden's Pseudo R^2	0.717	0.714	0.734	0.735	0.773	0.773	0.655	0.655	0.593	0.592
Observations	2,922	2,922	2,922	2,922	2,922	2,922	2,922	2,922	2,922	2,922
Note:	*p<0.05; **p<0.01; ***p<0.001 Data from 1/1/2010 to 12/31/2017. Month dummies (which are interacted with election year dummies) not shown. Robust standard errors in parentheses.									

Time Series Regressions in Non-AVR States (cont'd)								
	NY (11)	**NY (12)**	**NC (13)**	**NC (14)**	**OH (15)**	**OH (16)**	**WA (17)**	**WA (18)**
2016–2017	0.062 (0.085)		0.015 (0.076)		-0.204 (0.111)		0.107* (0.053)	
2017		-0.209* (0.083)		0.019 (0.067)		-0.485*** (0.108)		-0.004 (0.051)
Trend	0.0002*** (0.00003)	0.0003*** (0.0001)	0.0005*** (0.00004)	0.0005*** (0.00005)	0.001*** (0.0001)	0.001*** (0.0001)	0.0003*** (0.00002)	0.0003*** (0.00003)
Presidential Election Year	0.600*** (0.079)	0.554*** (0.079)	0.617*** (0.066)	0.628*** (0.067)	0.776*** (0.075)	0.545*** (0.080)	0.497*** (0.052)	0.527*** (0.052)
Midterm Election Year	-0.355*** (0.097)	-0.387*** (0.095)	-0.204* (0.087)	-0.199* (0.085)	0.061 (0.093)	-0.024 (0.097)	-0.065 (0.060)	-0.063 (0.060)
Saturday or Sunday	-2.097*** (0.113)	-2.097*** (0.113)	-2.095*** (0.145)	-2.095*** (0.145)	-1.690*** (0.055)	-1.689*** (0.055)	-0.979*** (0.071)	-0.979*** (0.071)
Constant	6.312*** (0.067)	6.282*** (0.083)	5.567*** (0.067)	5.559*** (0.071)	5.439*** (0.091)	5.513*** (0.100)	5.620*** (0.048)	5.571*** (0.056)
McFadden's Pseudo R^2	0.626	0.628	0.658	0.658	0.488	0.497	0.586	0.585
Observations	2,922	2,922	2,922	2,922	2,922	2,922	2,922	2,922
Note:	*p<0.05; **p<0.01; ***p<0.001 Data from 1/1/2010 to 12/31/2017. Month dummies (which are interacted with election year dummies) not shown. Robust standard errors in parentheses.							

Time Series Models in AVR States

The second building block of the time series analysis is to run the time series model on states that did implement AVR in 2016 or 2017 (Oregon, Georgia, and Vermont). Below is the regression output for that model:

Dependent Variable: Number of Registrations			
	Oregon (1)	**Georgia (2)**	**Vermont (3)**
Post AVR Implementation (OR)	0.351*** (0.074)		
Post AVR Implementation (GA)		0.525*** (0.064)	
Post AVR Implementation (VT)			0.413*** (0.110)
Trend	0.0001*** (0.00001)	0.0001*** (0.00001)	0.0001*** (0.00002)
Presidential Election Year	0.468*** (0.134)	0.579*** (0.087)	1.052*** (0.096)
Midterm Election Year	0.168 (0.146)	0.051 (0.087)	0.366*** (0.108)
Saturday or Sunday	-1.587*** (0.069)	-1.357*** (0.043)	-3.346*** (0.115)
Constant	4.796*** (0.095)	6.240*** (0.063)	3.013*** (0.102)
McFadden's Pseudo R^2	0.600	0.595	0.612
Observations	2,922	2,922	2,922
Note:	*p<0.05; **p<0.01; ***p<0.001 Data from 1/1/2010 to 12/31/2017. Month dummies (which are interacted with election year dummies) not shown. Weeks 25 and 27 in 2018 excluded because of primary election distortion.		

When we run these models on the states that implemented AVR in 2016 or 2017, the effects are larger than those in the control state and are statistically significant at the 99.9 percent level.

Comparing Results of Matched Difference-in-Differences vs. Time Series

Unremarkably, the estimated impact of AVR differs according to the methodology. The difference-in-differences methodology used data from other states to estimate what would have happened without AVR. The time series methodology created estimates based on a state's own history. Again, we believe the matched difference-in-differences methodology produces the better estimate because it incorporates more information relevant to a state's registration rate absent AVR.

To aid in the comparison of these numbers with the increases reported in the matched difference-in-differences section, we here convert these logged coefficients into percent increases and report them together with the results from our matched difference-in-differences models.

Both models show that impact of AVR is statistically significant and that it increased registration rates in exciting and impressive numbers.

The key takeaway here is that even under very different models, we can see that AVR was successful at registering Americans to vote — irrespective of where the opt-out was placed.

State	Percent Increase Time Series	Percent Increase Matched Difference-in-Differences
Oregon	42.0%	15.9%
Georgia	69.0%	93.7%
Vermont	51.2%	60.2%

Endnotes

1 The Brennan Center for Justice first developed automatic registration in 2008. Since then, we have proudly helped push for its adoption around the country. *See:* Wendy Weiser, Michael Waldman, and Renée Paradis, *Voter Registration Modernization* (New York: Brennan Center for Justice, 2009), http://www.brennancenter.org/sites/default/files/legacy/publications/VRM.Proposal.2008.pdf.

2 "History of AVR Implementation Dates," Brennan Center for Justice, accessed February 19, 2019, https://www.brennancenter.org/analysis/history-avr-implementation-dates.

3 Ibid.

4 Ibid.

5 For instance, *see:* Sean McElwee, Brian Schaffner, and Jesse Rhodes, *Oregon Automatic Voter Registration* (New York: Demos, 2017), https://www.demos.org/publication/oregon-automatic-voter-registration; Rob Griffin, Paul Gronke, Tova Wong, and Liz Kennedy, *Who Votes with Automatic Voting Registration?* (Washington, DC: Center for American Progress, 2017), https://www.americanprogress.org/issues/democracy/reports/2017/06/07/433677/votes-automatic-voter-registration/.

6 "State Primary Election Types," National Conference of State Legislatures, accessed February 5, 2019, http://www.ncsl.org/research/elections-and-campaigns/primary-types.aspx.

7 "2018 Oregon Motor Voter Registrations by County," Election Division, Oregon Secretary of State, accessed March 8, 2019, https://sos.oregon.gov/elections/Documents/OMV/omv-yearlyReports-2018.pdf.

8 Matthew Berman and Random Reamey, *Permanent Fund Dividends and Poverty in Alaska*, Institute of Social and Economic Research, University of Alaska Anchorage, November 2016, https://iseralaska.org/static/legacy_publication_links/2016_12-PFDandPoverty.pdf.

9 "Automatic Voter Registration," National Conference of State Legislatures, accessed February 6, 2019, http://www.ncsl.org/research/elections-and-campaigns/automatic-voter-registration.aspx.

10 Ibid.

11 "Policy Differences of Automatic Voter Registration," Brennan Center for Justice, accessed February 19, 2019, https://www.brennancenter.org/analysis/policy-differences-automatic-voter-registration.

12 For this report, we calculate the pre-AVR rate of registration by dividing the most recent active registration estimates for the state (provided by jurisdictions to the Election Assistance Commission for its biennial *Election Administration and Voting Survey*) by the citizen voting-age population for that year calculated by the U.S. Census Bureau. This allows us to standardize registration rates throughout states, as the format of state voter registration statistics vary from state to state.

13 Brennan Center for Justice, "Policy Differences of Automatic Voter Registration."

14 In Massachusetts, this other agency is MassHealth, the state welfare agency. Maryland's AVR covers the Maryland Health Benefit Exchange (the state's health insurance exchange established through the Affordable Care Act), local departments of social services, and the Mobile Certification Office. *See*: Brennan Center for Justice, "Policy Differences of Automatic Voter Registration."

15 "Foreign Nationals Driving in the U.S.," USA.gov, accessed February 19, 2019, https://www.usa.gov/visitors-driving.

16 "States Offering Driver's Licenses to Immigrants," National Conference of State Legislatures, accessed February 19, 2019, http://www.ncsl.org/research/immigration/states-offering-driver-s-licenses-to-immigrants.aspx/.

17 "Criminal Disenfranchisement Laws Across the United States," Brennan Center for Justice, accessed February 19, 2019, https://www.brennancenter.org/criminal-disenfranchisement-laws-across-united-states.

18 Danielle Root, *Obstacles to Voting for Survivors of Intimate Partner Violence*, (Washington, DC: Center for American Progress, 2018), https://www.americanprogress.org/issues/democracy/reports/2018/11/01/460377/obstacles-voting-survivors-intimate-partner-violence/.

19 Because we do not have the registered voter file from every state (and therefore cannot calculate the number of voters registered in each census tract), not every non-AVR census tract in the country is available for our control set. The non-AVR states whose census tracts are available for matching in this study include Connecticut, Florida, Michigan, Nevada, New Jersey, New York, North Carolina, Ohio, and Washington State. For states

that implemented prior to 2018, we also allow treated census tracts to match to tracts in Rhode Island and Washington, DC, because AVR was not implemented in these tracts until 2018. California is excluded from this group since users of the voter file are prohibited from geocoding the data, and therefore we cannot break the state down to the census tract level. Although New Jersey implemented AVR in late 2018, it was not implemented until the end of the post-period in each state. As such, during our period of analysis, every New Jersey census tract was "untreated" with AVR.

20 Stanley Kelley, Richard E. Ayres, and William G. Bowen, "Registration and Voting: Putting First Things First," *The American Political Science Review* 61, no. 2 (1967): 359–79, doi:10.2307/1953251.

21 A full list of the data on which we match can be found in the Technical Appendix.

22 We perform a 1:3 match to strike a balance between the precision improvements possible from multiple matches and the bias that such multiple matches may introduce. *See:* Jeremy A. Rassen, Abhi A. Shelat, Jessica Myers, Robert J. Glynn, Kenneth J. Rothman, and Sebastian Schneeweiss, "One-to-Many Propensity Score Matching in Cohort Studies," *Pharmacoepidemiology and Drug Safety* 21 (2012): 69–80, doi:10.1002/pds.3263.

23 Jasjeet Sekhon, "Multivariate and Propensity Score Matching Software with Automated Balance Optimization: The Matching Package for R," *Journal of Statistical Software*, 42, no. 7, (2011): 1–52, doi:10.18637/jss.v042.i07.

24 Sekhon's matching algorithm has been used to investigate questions such as the correlation between level of education and voter turnout, the effect of poll consolidation, and how vote by mail impacts turnout. *See*, for instance, Alexander K. Mayer, "Does Education Increase Political Participation?" *The Journal of Politics* 73, no. 3 (2011): 633–45, doi: 10.1017/s002238161100034x; John E. McNulty, Conor M. Dowling, and Margaret H. Ariotti, "Driving Saints to Sin: How Increasing the Difficulty of Voting Dissuades Even the Most Motivated Voters," *Political Analysis* 17, no. 4 (2009): 435–55, doi:10.1093/pan/mpp014; Betsy Sinclair, Thad E. Hall, and R. Michael Alvarez, "Flooding the Vote: Hurricane Katrina and Voter Participation in New Orleans," *American Politics Research* 39, no. 5 (September 2011): 921–57, doi: 10.1177/1532673X10386709; Michael C. Herron and Jonathan Wand, "Assessing Partisan Bias in Voting Technology: The Case of the 2004 New Hampshire Recount," *Electoral Studies* 26, no. 1 (2007): 247–61, doi: 10.1016/j.electstud.2006.02.004. Other recent papers employing matching techniques include: Thad Kousser and Megan Mullin, "Does Voting by Mail Increase Participation? Using Matching to Analyze a Natural Experiment," *Political Analysis* 15, no. 4 (2007): 428–45, doi: 10.1093/pan/mpm014; Luke Keele and Rocío Titiunik, "Geographic Natural Experiments with Interference: The Effect of All-Mail Voting on Turnout in Colorado," *CESifo Economic Studies* 64, no. 2 (2018): 127–149, doi: 10.1093/cesifo/ify004; Kosuke Imai, "Do Get-Out-the-Vote Calls Reduce Turnout? The Importance of Statistical Methods for Field Experiments," *The American Political Science Review* 99, no. 2 (2005): 283–300, doi: 10.1017/S0003055405051658.

25 Oregon H.B. 2177, 78th Cong. (2015), https://olis.leg.state.or.us/liz/2015R1/Downloads/MeasureDocument/HB2177.

26 "VRM in the States: Oregon," Brennan Center for Justice, accessed March 1, 2018, https://www.brennancenter.org/analysis/vrm-states-oregon.

27 "Oregon Motor Voter Program Statistics," Election Division, Oregon Secretary of State, accessed March 8, 2019, https://sos.oregon.gov/elections/Documents/OMV/omv-statistics-2016.pdf. Note that no other AVR state has done a look-back.

28 Kevin Rayburn (Assistant Elections Director and Deputy General Counsel, Georgia Secretary of State), in phone discussion with authors, April 2019.

29 In April 2017, the Georgia Sixth Congressional District held a special election. Throughout this report, we focus on odd years to isolate the impact of AVR from election-specific factors. However, our statistical analysis showed that the leadup to the April special election did not materially influence statewide voter registration rates. As such, we do not exclude the period in the leadup to the election from our econometric model.

30 "Vermont Department of Taxes Implements New Measures to Prevent Fraud," Vermont Department of Taxes, released January 19, 2016, https://tax.vermont.gov/press-release/vermont-department-taxes-implements-new-measures-prevent-fraud.

31 Melissa Polk (legal and internal operations manager, Colorado Department of State), email message to Kevin Morris, February 20, 2019. (On file with author.)

32 "VRM in the States: Colorado," Brennan Center for Justice, accessed March 16, 2018, https://www.brennancenter.org/analysis/vrm-states-colorado.

33 National Conference of State Legislatures, "Automatic

Voter Registration," http://www.ncsl.org/research/elections-and-campaigns/automatic-voter-registration.aspx.

34 Eligibility for the PFD requires establishing residency in Alaska prior to January 1 of the year an individual seeks a dividend and attesting an intention to remain an Alaska resident for the indefinite future. "Permanent Fund Dividend Information & Instructions," Alaska Department of Revenue, accessed February 19, 2019, https://pfd.alaska.gov/LinkClick.aspx?fileticket=-ZonQe0MaHs=&tabid=425&portalid=6.

35 Gail Fenumiai (director, Division of Elections, Alaska Office of the Lieutenant Governor), email message to Kevin Morris, February 21, 2019. (On file with author.)

36 From time to time, jurisdictions may have registration rates of more than 100 percent. This may be due to errors in estimates of the citizen voting-age population from the Census, or due to ineligible voters not being removed from the rolls.

37 Some states that have already passed AVR serve as a good example of how this could work in practice. Massachusetts, the state with the second-lowest car ownership rate behind New York, will be implementing AVR at MassHealth, the state Medicaid program, as well as the DMV. The law also gives the secretary of state the authority to extend automatic registration to other agencies that they verify collect information necessary to determine eligibility for voter registration.

38 Due to aforementioned restrictions on data publication in California, we had to code data on the zip code level.

39 In recent years, the use of matching models has become increasingly popular in political science. It has been used to investigate questions such as the correlation between level of education and voter turnout, the effect of poll consolidation, and how vote by mail impacts turnout. *See*, for instance, Kousser and Mullin, "Does Voting by Mail Increase Participation? Using Matching to Analyze a Natural Experiment," 428-445; Mayer, "Does Education Increase Political Participation?," 633-645; McNulty, Dowling and Ariotti, "Driving Saints to Sin: How Increasing the Difficulty of Voting Dissuades Even the Most Motivated Voters," 435-455; Keele and Titiunik, "Geographic Natural Experiments with Interference: The Effect of All-Mail Voting on Turnout in Colorado," 127-149; Sinclair, Hall and Alvarez, "Flooding the Vote: Hurricane Katrina and Voter Participation in New Orleans," 921-957.

40 As explained in endnote 19, the untreated states whose census tracts are available for matching in this study include Connecticut, Florida, Michigan, Nevada, New Jersey, New York, North Carolina, Ohio, and Washington State. We also include Rhode Island and Washington, DC, as untreated tracts for states who implemented prior to 2018.

41 *See*: Kelley, Ayres, and Bowen, "Registration and Voting: Putting First Things First," 359-379.

42 The census-tract-level data on which we match come from the Census Bureau's 2017 American Communities Survey: voting-age population, change in voting-age population (2013–2017), percent Latino, percent non-Hispanic black, percent non-Hispanic white, percent noncitizens, percent of households without a car, percent of individuals who have moved in the past 12 months, percent of individuals with at least some post–high school education, median income, percent unemployed, median age, and the number of registrations in 2013 (from the voter file). We also assign census tracts two county-level characteristics (state-level characteristics in Alaska): percent of citizen voting-age population registered as of the 2014 election (Sources: US Election Assistance Commission; US Census Bureau) and presidential Republican vote share in 2016 (Source: MIT Election Data and Science Lab). Finally, we include the census tract's state's region, as defined by the Census Bureau.

43 We perform a 1:3 match to strike a balance between the precision improvements possible from multiple matches and the bias that such multiple matches may introduce. *See:* Rassen et al., "One-to-many Propensity Score Matching in Cohort Studies," 69-80.

44 The weight ascribed to each of the covariates on which we matched was estimated using a genetic matching procedure. *See:* Sekhon, "Multivariate and Propensity Score Matching Software with Automated Balance Optimization: The Matching Package for R," 1-52.

45 In three states — Oregon, Alaska, and California — the registered voter file updates the date of registration whenever a voter interacts with an agency that collects voter information. We are not interested in whether AVR increases the number of individuals who reaffirm their information, but rather how it changes the number of individuals who either register for the first time or update their information in such a way that, had they not done so, they would have been ineligible to vote. To account for this problem with the data, we use the original date of a voter's registration in these states. We assume that new registrations make up a constant share of new and materially updated registrations before and after AVR, and that AVR does not impact these two groups differently.

46 Because we weight the individual census tracts by population for the presentation of the statewide demographics to compare with the control group, they may differ slightly from statewide estimates from the Census Bureau.

47 As will be discussed below, Colorado's difference-in-differences model looks at *monthly* registrations in each census tract, while Alaska's looks at *annual* registrations. Because these are the dependent variables, this does not affect the matching procedure.

48 Melissa Polk (legal and internal operations manager, Colorado Department of State), email message to Kevin Morris, February 20, 2019. (On file with author.)

49 For county-level variables, zip codes that cross county lines were assigned the demographics of the county in which most voters were registered.

50 Although we are looking at a policy change that occurred in 2018, demographic data for each census tract are still from the 2017 ACS 5-Year Survey, the latest year for which census data are available.

51 In the previously discussed states, where we compared 2013 with 2017, one of our matching criteria was the growth in population from 2013 to 2017. Over a four-year period, population growth likely impacts the number of individuals registering to vote — where there is greater population growth, there is likely greater growth in registrations. In the states that implemented in 2018, however, we are not comparing periods four years apart but rather periods within a single year. Population growth is far less likely to influence registration growth within a single year. Furthermore, the Census Bureau estimates population on an annual level; comparing the population in December of 2017 with June of 2018 is not possible.

52 Although we are looking at a policy change that occurred in 2018, demographic data for each census tract are still from the 2017 ACS, the latest year for which census data are available.

53 Ibid.

54 As discussed earlier, we cannot examine the impact of AVR on daily registrations in Colorado or Alaska due to data and programmatic limitations. As such, we do not include them in this discussion.

55 New Jersey implemented AVR in late 2018. AVR did not impact the number of registrations occurring each day in 2016 or 2017. As such, it is included here (and in our matching) as a control state.

About the Brennan Center for Justice

The Brennan Center for Justice at NYU School of Law is a nonpartisan law and policy institute that seeks to improve our systems of democracy and justice. We work to hold our political institutions and laws accountable to the twin American ideals of democracy and equal justice for all. The Center's work ranges from voting rights to campaign finance reform, from ending mass incarceration to preserving Constitutional protection in the fight against terrorism. Part think tank, part advocacy group, part cutting-edge communications hub, we start with rigorous research. We craft innovative policies. And we fight for them — in Congress and the states, the courts, and in the court of public opinion.

About the Brennan Center's Democracy Program

The Brennan Center's Democracy Program works to repair the broken systems of American democracy. We encourage broad citizen participation by promoting voting and campaign finance reform. We work to secure fair courts and to advance a First Amendment jurisprudence that puts the rights of citizens — not special interests — at the center of our democracy. We collaborate with grassroots groups, advocacy organizations, and government officials to eliminate the obstacles to an effective democracy.

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