Happy Birthday: You Get to Vote![1]

Paper Presented at the 2019 Annual Meeting of the
American Political Science Association, Washington, DC

Ellen Seljan
Lewis & Clark College

Paul Gronke
Early Voting Information Center at Reed College

Matthew Yancheff, '19
Reed College

Abstract

Automatic Voter Registration (AVR) systems register to vote all eligible individuals who transact with proscribed government agencies, most commonly the Department of Motor Vehicles (DMVs). Many individuals interact with the DMV due to the need to renew their drivers' licenses. Because licences expire on birthdays, an individual's birth date can be used as an exogenous reason why some individuals are registered to vote in time for an election, whereas others are not. Our analysis compares registration and voting rates for individuals with birth dates prior and subsequent to the voter registration deadline. After calculating a causal effect of AVR on turnout at the individual level, we extrapolate this effect to the overall effect of AVR on total voter turnout by state.

[1] The authors would like to acknowledge the support of the MIT Election Data and Science Lab (MEDSL), which awarded us a New Initiatives in Election Science grant (https://electionlab.mit.edu/engage/grants/past-recipients); the Alta S. Corbett Fund at Reed College, which funded Matthew Yancheff this summer; and Professor Jacob Grumbach of the University of California, Berkeley and Sean McIlwee of Data for Progress, both of whom provided us invaluable advice and feedback on this research. All conclusions remain the responsibility of the authors.

Introduction

Automatic Voter Registration (AVR) systems register to vote all eligible individuals who transact with proscribed government agencies, most commonly the Department of Motor Vehicles (DMVs). Sixteen states plus the District of Columbia have authorized some form of this policy since 2015, with several additional states considering adoption[2]. Notably, this is more than the number of states adopting "active" motor voter laws in the 1980s and early 1990s, a trend that sparked national legislation--the National Voter Registration Act (NVRA)--that passed in 1993 (Knack 1995).[3]

Straightforward analysis of the effect of AVR on voter turnout is hindered by various causal inference, endogeneity and omitted variable problems. AVR is implemented in a state all at once, making it difficult to construct a reasonable counterfactual. The base turnout rate of AVR registered voters is biased upwards because of the presence of voters in this pool who would have registered through traditional means and voted regardless of AVR. Temporal comparisons will be potentially biased either upwards or downwards, due to omitted variables particular to each election cycle, e.g. comparing turnout in a presidential election to turnout in an off year election. Likewise, geographic cross-sectional analysis within a state is potentially biased because the areas with high AVR take up rates are systematically different than areas with low AVR usage in a myriad of ways that cannot readily be controlled for, and which are correlated with turnout.[4] Like temporal analysis, these omitted variables could bias estimation of the effect of AVR on turnout either upwards or downwards.

Our insight in this paper is to take advantage of an *exogenous* reason why an individual is registered to vote through AVR, and use this exogenous reason as causal leverage on the impact

---

[2] National Conference of State Legislators. "Automatic Voter Registration" http://www.ncsl.org/research/elections-and-campaigns/automatic-voter-registration.aspx (accessed November 15, 2018).

[3] Knack (1995) defines "active" motor voter laws as those laws that provide all driver' s license applicants with the opportunity to register to vote without requiring a witness or notarization of mailed registration forms.

[4] A study of AVR in Oregon shows that census blocks that have higher proportions of automatically registered voters have higher proportions of younger, less well-educated, and lower income residents. See Griffin et al. (2017).

of AVR on turnout. This exogenous reason is the voter's birthdate. In a number of states, driver's licenses expire on birthdays, causing individuals to be more likely to visit the DMV, and hence be touched by AVR, prior to their birthday. This is important because only those individuals registered prior to a set registration deadline are eligible to vote in the November general election.

Our paper takes advantage of this exogenous variation, comparing the turnout rates of individuals born prior to the registration deadline to individuals born after the registration deadline. We use this point of causal leverage to construct precise estimates of the effect of AVR on voter turnout by state. In addition, we use this same point of comparison to construct causal estimates of AVR on the probability of registration. As the results below show, this research shows a significant and positive impact of AVR on turnout, comparable in magnitude to estimates from other research.

Literature Review

Scholars, advocates, and the election administration community recognize that registration requirements act as a barrier to participation. All states save North Dakota require voter registration, and these records are used by election administrators for election planning, such as assembling voter lists, distributing ballots, and allocating voting equipment. Ever since HAVA required states to maintain centralized voter registration databases, these lists have become an essential part of voter contact and mobilization (Hersh 2015).

While there are varying views on the need for voter registration, most agree, and scholars have shown for decades, that registration acts as a barrier to participation. In response, many states, and eventually the Federal government, took action to reduce the burdens created by registration requirements. AVR is, in this respect, only the most recent version of an extension of the motor voter laws that became common place in the American states in the 1980s and 1990s, culminating in the passage of the National Voting Rights Act in 1993.

These laws made DMVs a clearinghouse for voter registration, though always in a voluntary, opt-in manner on behalf of the registrant. These sort of voter laws have been extensively studied by academic researchers. Most clearly, analysis shows that the introduction of motor voter laws add voters to the rolls (Wolfinger and Hoffman 2001). In addition to raw numbers, both Hill (2003) and Rugley and Jackson (2009) found that motor voter legislation produces more equitable voter rolls in terms of income and age. Wolfinger and Hoffman (2001) note that the latter effect is particularly pronounced when the legislation is implemented in state agencies beyond the DMV, namely public assistance offices.

The effect of motor voter laws on turnout, however, is less clear. The strongest effects were found in states voluntarily implementing their own motor voter programs prior to NVRA. These results hold cross-sectionally (Franklin and Grier 1997) as well as in fixed effect models using time-series cross-sectional data (Rhine 1995, Knack 1995). Single-state analyses are mixed, with Highton and Wolfinger (1998) finding positive effects in the state of Colorado and Hammer (2009) on turnout for two of their four states analyzed (Michigan and North Carolina). Work by Piven and Cloward (2000) highlights the variance in implementation among states adopting these laws prior to the national legislation, potentially explaining these mixed effects. Regardless of policy or implementation, however, the endogeneity of motor voter laws must be considered, even in fixed effects analysis. In particular, the conditions that lead states to adopt motor voter laws might be the very conditions that foster stronger voter turnout, regardless of the policy change. Thus even positive and significant results cannot necessarily be attributed causally to the passage of motor voter legislation.

The adoption of the NVRA, of course, is not endogenous to state-specific characteristics. However an overall effect of the national legislation is dubious, perhaps in part to the extremely varied commitment to its implementation (Highton and Wolfinger 1998). Knack (1996) points out that the first presidential election following the implementation of NVRA had the lowest voter turnout rate since 1924 in addition to the lowest aggregate decline since 1920. Hammer

(2009) found positive and significant results to turnout for the subset of states that actively implemented NVRA, but Brown and Wedeking (2006) found no lasting changes. This research faces some of the same causal inference problems that we wrestle with here: variation in implementation by state means that the potential problem of state-level endogeneity again arises, calling into question any significant findings.

There is good reason to believe that AVR might be more successful than previous motor voter laws at increasing voter turnout. Social science research has confirmed the substantial effects of moving from opt-in to opt-out systems (Sunstein and Thaler 2008), which means the net cast by AVR will be much wider than traditional motor voter laws.[5] Indeed, non-rigorous analysis of turnout is encouraging: Oregon's voter turnout as a percentage of the Voting Eligible Population increased 4.1% from 2012 to 2016, the highest of any state during that time period, while California's 2018 midterm turnout increased by a whopping 18.9% from 2014 levels.[6] Unfortunately, such straightforward analyses are likely to give flawed results. Comparing a state's subsequent voter turnout to previous years or other states could either overestimate or underestimate AVR's effect size; one simply cannot disentangle AVR from other factors uniquely influencing state voter turnout, such as voter interest. This would be true even with more sophisticated comparative methods, such as the use of synthetic controls (McGhee et al 2017).

Research on Oregon's AVR, however, has some additional preliminary, positive research findings. Two studies, published by the think tanks Demos and The Center for American Progress, attempt to triangulate the effect of Oregon's AVR on voter turnout by identifying those individuals who registered and voted in 2016 but were unlikely to do so in the absence of AVR.

---

[5] Within the election administration field, AVR systems can be either "front-end" (sometimes also called "automated" voter registration) and "back-end". Under the front end system, a citizen is given an opportunity to register to vote (or have their registration records updated due to an address change) during a DMV transaction, and the citizen needs to positively assent to the change. Under the back-end system, eligible citizens are automatically registered, and have an opportunity to opt-out later in response to a postcard mailer. Neither system is the same kind of "opt-in" system that is used in non-AVR states, where a citizen can fill out a paper (or in some cases, electronic) voter registration form after completing their DMV transaction.

[6] Pillsbury, George with Julian Johannesen. "America Goes to the Polls 2016." Nonprofit Vote and The U.S. Elections Project. Available at https://www.nonprofitvote.org/documents/2017/03/america-goes-polls-2016.pdf/(accessed November 15, 2018).

McElwee et al. (2017) found that 89,000 new voters registered by AVR in 2016, and, in total, there were 53,000 more new voters in 2016 than 2012. Using the latter estimate in their calculation, they suggest that AVR may have increased voter turnout by 2%. Similarly, Griffin et al. (2017) estimate that 40,000 individuals voted because AVR in the 2016 election that otherwise wouldn't have. This would account for 1.9% of voters in 2016. This estimate was derived by counting the number of individuals who voted and registered via AVR who fit the following criteria: 1) were not registered during the 2008, 2010, 2012, or 2014 elections, 2) were old enough that they could have been registered and voted since 2008, and 3) did not return their registration postcard to indicate partisanship.

Unfortunately, triangulating new voters in this way still does not fully address the potential endogeneity concerns. Both of these approaches may underestimate or overestimate the true effect on voter turnout. Leveraging differences in turnout over time makes the estimate subject to differences beyond the adoption of AVR. For one, variation in the candidates running for office in 2012 and 2016 may have resulted in differences in voter interest, causing the increase in new voters. Alternatively, the increase in new voters may simply have been caused by population increases. Indeed, the Oregon DMV reported a 48% percent increase in surrendered licenses from out of state during that time interval, an increase of over 30,000 individuals. Failure to take into account additional voters from out of state may also unduly inflate the triangulated estimates provided in McElwee et al. 2017 and Griffin et al. 2017, as these individuals would have no Oregon voter history but may nonetheless been likely voters with or without AVR.[7] On the other hand, these authors may have underestimated the effect of AVR on voter turnout by ignoring the effect on the youth vote or other individuals that simply did not fit their specific criteria.

Overall, existing analyses of motor voter laws and AVR are mixed or tenuous, which is much like the broader literature on the effect of all electoral reforms on voter turnout (see for example Kousser and Mullin 2007, Gronke et al. 2008, Grimmer et al. 2018; Mycoff, Wagner and Wilson 2009, Burden et al. 2014, Neiheisel and Burden 2012 but also Gerber, Huber and Hill 2013;

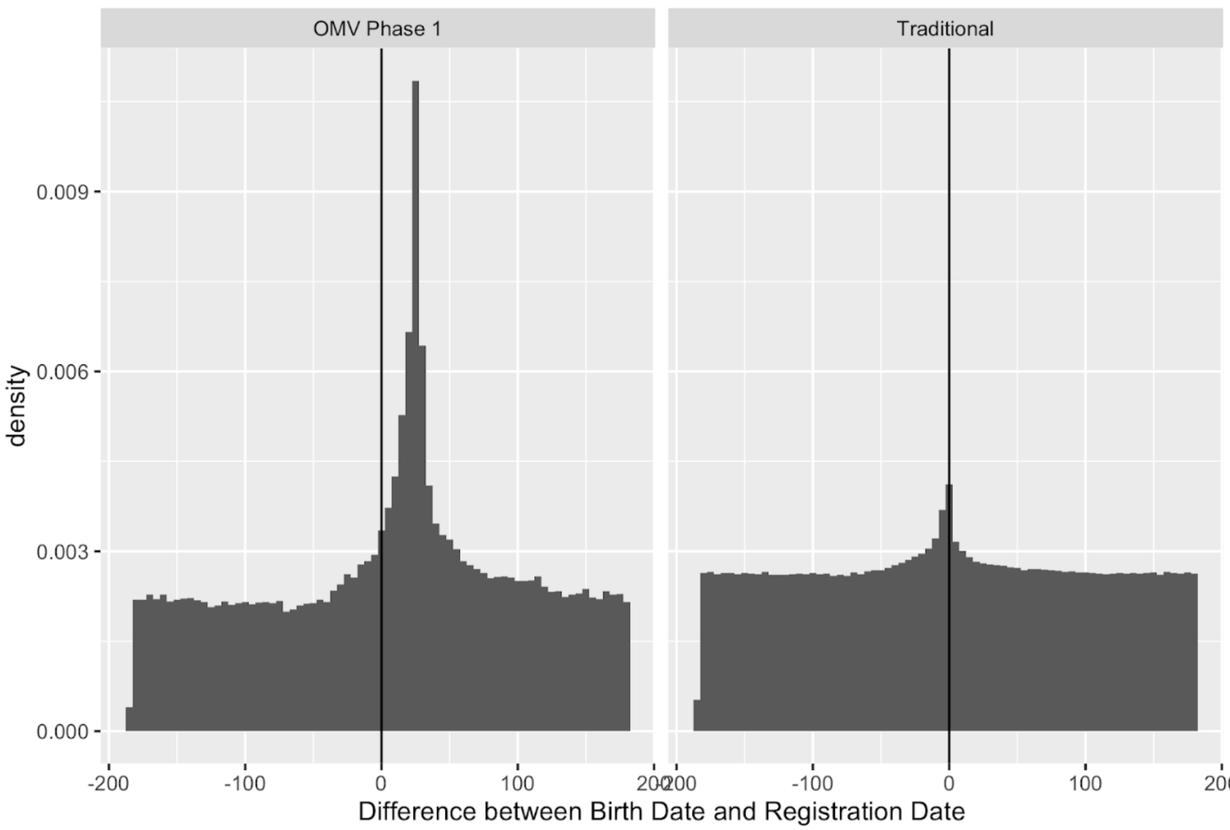
[7] New voters in the Robert et al. analysis increased by 62%.

Holbein and Hillygus 2016; Leighley and Nagler 2013). In particular, researchers note that there is a trade-off between lowering the costs of voting, and making it more difficult for parties to mobilize voters (Hanmer 2009). In this way, lowering registration and voting costs does not necessarily guarantee increases in voter turnout. Rigorous analysis is necessary to confirm the effects of any policy change.

Evidence for the relationship between voter registration and birthdays

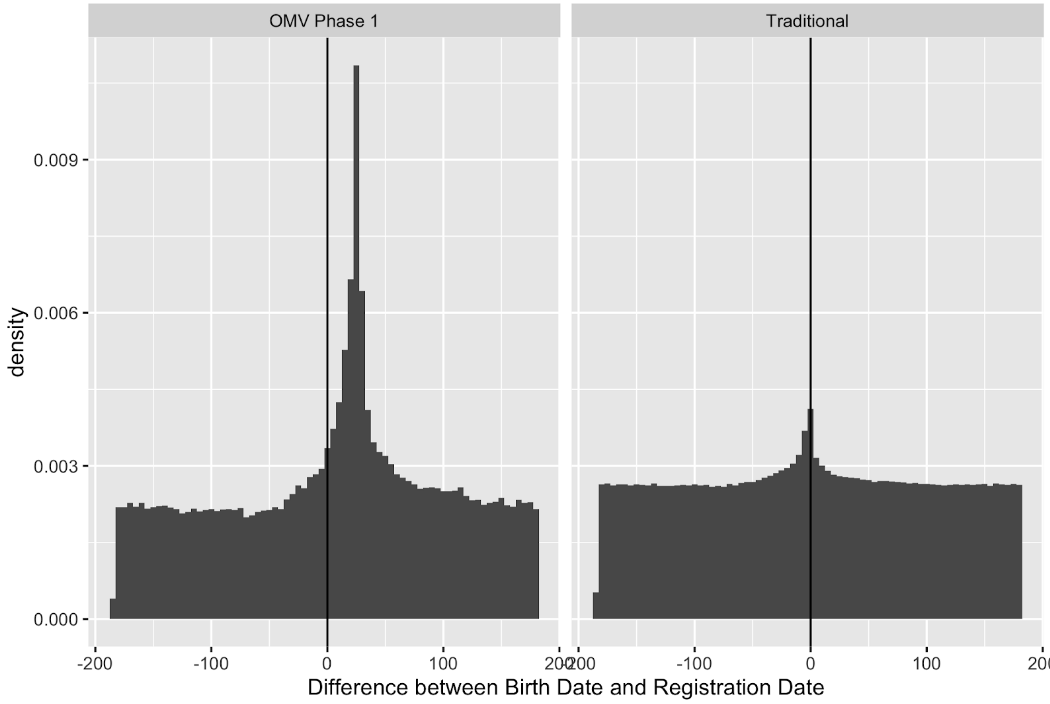
We expect to see a systematic relationship between effective voter registration dates and birthdays due to DMV policy that all driver's licenses expire on birth dates. This effect will be strong for AVR registered voters and small but present for traditionally registered voters due to pre-existing motor voter policy and age-induced voter eligibility. Figure 1 displays the distribution of all voters in Oregon (2016) and California (2018) voter files based on the difference between their birthdate and their effective voter registration date for AVR and non-AVR registrants. The x-axes display the difference in calendar days between these two events, with a maximal difference of 182 days prior and subsequent to one's birthdate.

The graphs show that the relationship between birth date and registration date is stronger for AVR than non-AVR registered voters. In Oregon, effective registration dates most commonly fall in the month following one's birthday, peaking precisely 25 post-birthday. Much of his delay is due to AVR policy that potential voters have 21 days to opt-out of registration or choose a political party, with the remainder presumably due to administrative processing time. In California, the modal AVR registrant has an effective registration date that falls 1 day prior to their birthdate.

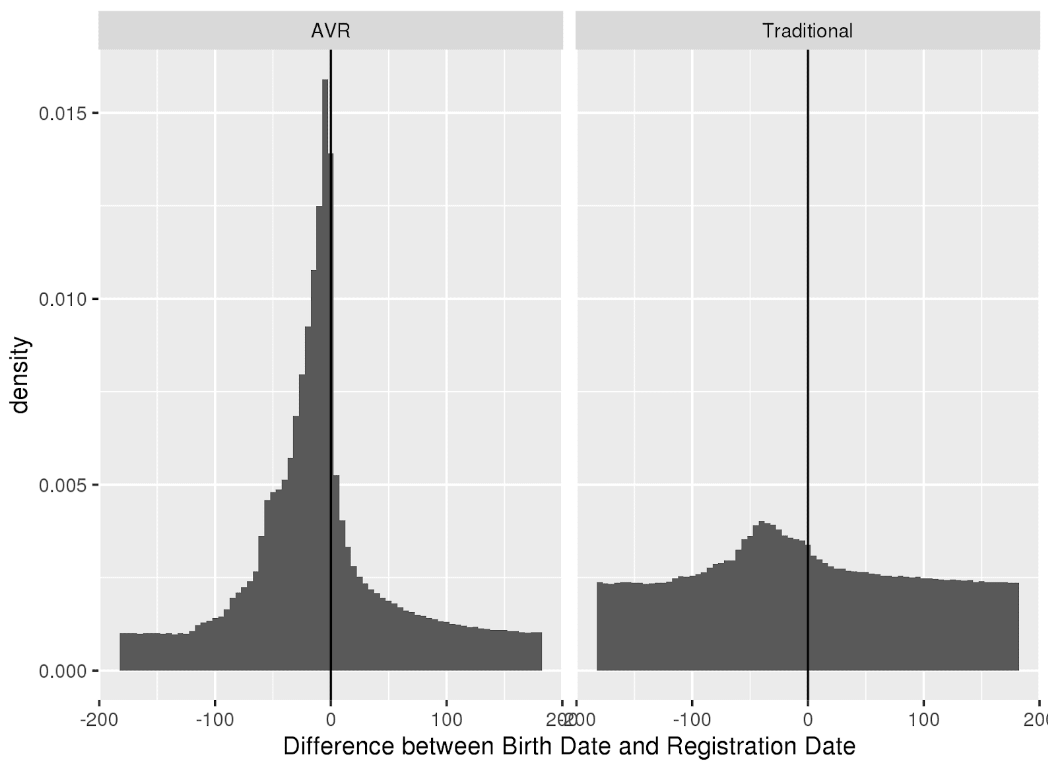


OMV Phase 1
Traditional
density
0.009
0.006
0.003
0.000
-200
-100
0
100
200
Difference between Birth Date and Registration Date

Figure 1
Birthdates, Registration, and Oregon Motor Voter



2018 California Registrants



Statistical analysis confirms that AVR registered voters are more likely than traditionally registered voters to have effective registration dates in proximity to their birthday. Here we will define "proximity" broadly as 30 days prior or subsequent to one's birthday. Using each states full voter file, we estimated a logistic regression with the dependent variable coded as 1 if the voter's effective registration date is within 30 days (prior or subsequent) to their birthday. The independent variables are dichotomous variables indicating whether the voter was registered traditionally or by AVR. The marginal effects from this estimation suggest that registration through AVR increases your probability of having effective registration dates in proximity to your birthday by 7.9% in the State of Oregon (2016) and 24% in California (2018), each estimate relative to traditionally registered voters.[8]

Table 1: AVR Registrants Are More Likely to Register Close To Birth Date

	Oregon 2016	California 2018
AVR Registration	7.86%	24.53%

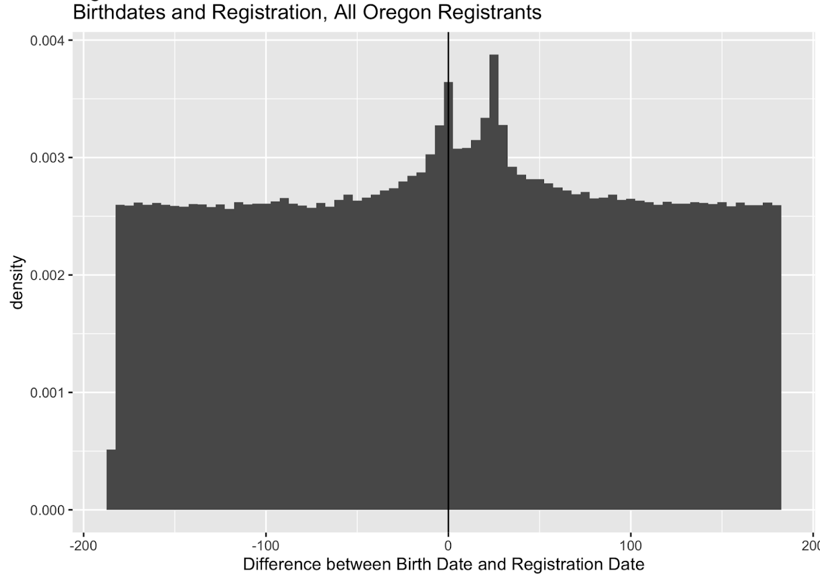
Notes: Table entries are marginal effects from a logistic regression predicting effective date of registration by AVR registration status.

It is important to note that AVR is not the only driver of a relationship between birth dates and registration dates. For one, age induced voter eligibility leads to voter registration subsequent to one's birthday, a relationship heightened in proximity for states with pre-registration laws. The second additional driver is pre existing motor voter policies. As previously noted, a myriad of state and federal laws and administrative policies have made the DMV a source for voter registration for some time. In addition to opt-in voter registration, some states, including Oregon, use DMV change of address data to automatically update their voting rolls.

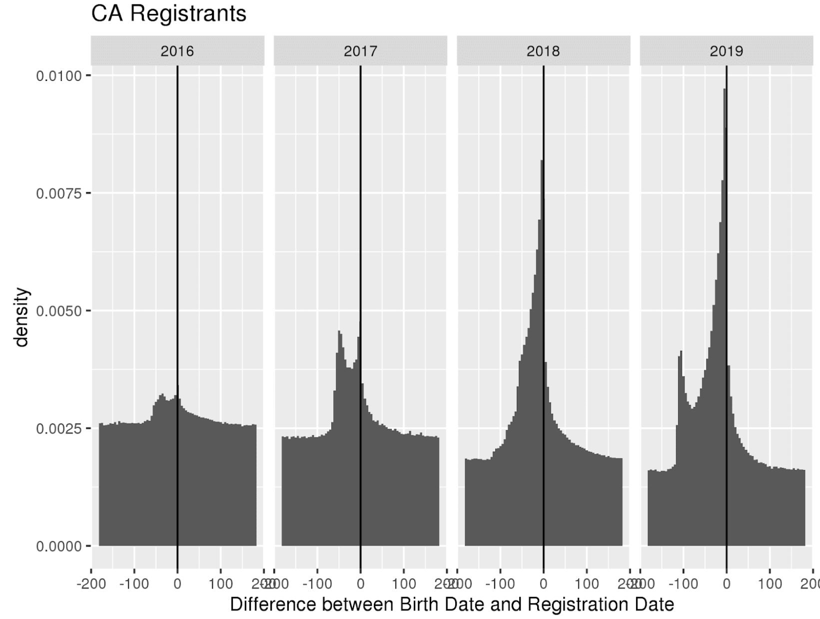
[8] In the state of Oregon, voters registered by AVR Phase 2 are omitted from the analysis. Phase 2 was a retrospective effort to register individuals who made a qualifying transaction under AVR at the DMV in 2014 and 2015. These potential voters were registered enmass in July 2016, and as such there is no relationship between their birthdate and effective registration date.

Figure 2 displays the relationship between birthdate and registration date for 2016 registrants in the state of Oregon, and for 2016-2019 for California. The bimodal peaks represent the dual effects of traditional motor voter policy and AVR. The ability to differentiate these policies based on timing is unique to Oregon due to its policy that gives potential voters 21 days to opt out. In all other states AVR and traditional motor voter policies cannot be disentangled except perhaps with respect to temporal changes.

Figure 2
Birthdates and Registration, All Oregon Registrants



CA Registrants



Methods: Instrumental Variable Analysis

Now that we have established the relationship between birth dates and voter registration, we turn to the methodology we employ: instrumental variable analysis. This technique is used when correlation between the explanatory variables and the error term is suspected. In the case of AVR, we are concerned that registration with AVR may be correlated with other factors that affect voter turnout, biasing estimation. An instrumental variable (IV), sometimes called an "instrument", is a third variable that is correlated with your explanatory variable, but not with omitted variables of concern (the error term).

A dichotomous variable coding birthdays just prior and subsequent to a cutoff date is a valid instrument in the case of AVR. As previously illustrated, birthdays are correlated with AVR registration date due to driver's license renewal policy. Those registered to vote prior registration deadlines will be eligible to vote in a given election, whereas those who are registered subsequently will not. However, the timing of one's birthday, at least within a subset of the calendar year as we will discuss in greater depth later, is not correlated with any known factors that affect voter turnout, meaning it is exogenous.

Instrumental variable analysis will provide us with an estimate of the Local Average Treatment Effect (LATE). The LATE is the causal estimate of the treatment for the subset of individuals who receive the treatment *only* through the causal pathway of the instrument. In the language of Angrist et al. (1996), it is the average treatment effect for the "compliers". Compliers receive the treatment if and only if the instrument is switched on. In this study, the LATE is the treatment effect for those who are registered to vote in time for the general election only as a result of the timing of their birthday, and who would otherwise not have been registered.

It is theoretically useful to breakdown the type of voters that will drive the LATE estimate. Imagine there are five types of potential voters, a spectrum of individuals who vary based on vote likelihood and method of registration. For shorthand, we will call them "already voters", "would-be voters", "cost-conscience voters", "uninterested voters", and "anti-voters". Summaries of our predictions for each voter type appear in Table 2.

Table 2: Theoretical Predictions for the Impact of AVR on Turnout					
Voter Type	**Vote Likelihood**	**Registration Likelihood if Z=0**	**Registration Likelihood if Z=1**	**AVR Likelihood**	**Angrist IV Typology**
Already	High	High	High	Low	Always Takers
Would-Be	High	High	High	Medium	Always Takers
Cost-Conscious	Medium	Low	High	High	Compliers
Uninterested	Low	Low	High	High	Compliers
Anti-voter	Zero	Zero	Zero	Zero	Never Takers

Already voters are those individuals who pre-exist in the voter rolls prior to AVR implementation. They may be interested in politics and vote with regularity. Would-be voters are those individuals who are not yet registered to vote, perhaps due to a recent move or otherwise change in eligibility, but would achieve registration in time for the voter registration deadline regardless of AVR. These individuals may or may not register via AVR, depending on whether they happen to go to the DMV in advance of the voter registration deadline. These voters would likely cast votes at rates similar to the already voters.

In contrast, cost-conscience and uninterested voters, would only register to vote in the face of AVR policy. In particular, these are the individuals who we expect to be influenced by the instrument; If their birthday falls prior to voter registration deadlines they will register to vote in time for the election, whereas if it falls afterwards they will not.[9] We expect each of these voter

[9] Note that birthdays determine only the timing of registration, not whether or not registration occurs at all.

types to exclusively register by AVR, though cost-conscience voters will vote at higher rates. Combined, this is the population that will drive the LATE estimate.

Finally, anti-voters are individuals opposed to voter registration and voting, opting out of AVR if registration is proposed. By nature of their absence in the voter file, these individuals do not appear in our analysis. We view this omission as non-consequential since both their voting outcome and registration status are unaffected by the instrument

This typology of voters highlights how descriptive statistics of AVR registrant turnout overemphasize the effect of AVR. AVR registrants will be a combination of Would-Be Voters, Cost-Conscience Voters, and Uninterested Voters. However, because Would-Be Voters would find other means of registration in the absence of AVR, their presence in the pool of AVR registrants overstates the causal impact of the policy. In contrast, this instrumental variable analysis will provide a causal effect of the effect on Cost-Conscience and Uninterested Voters, the group that would be unlikely to register to vote at all in the absence of AVR. Following the language of Angrist (1990), these groups of individuals constitute the “compliers” and their treatment effect constitutes the LATE.

Data

This analysis utilizes data from voter registration files in Oregon (2016) and California (2018). We limit the data in several ways. Most significantly, we limit each data file to voters who updated or initiated their registration while AVR was in place. This is the pertinent subset of the data for our purposes, as inference is drawn based on the timing of one’s registration transaction, in particular for those individuals whose birthdays affect their registration timing. Individuals who did not change their registration status in a given calendar year are extraneous because by definition they were not affected by AVR. Further, their presence in the data weakens the strength of our instrument, since the relationship between birthdays and registration timing is stronger in the time interval when AVR is in place.

We also limit our analysis based on the registrant's particular birthday. This follows guidance from critiques of previous research exploiting birthdays as an exogenous variable for instrumental variable analysis (Angrist 1990, Angrist and Krueger 1992). In particular, Buckles and Hungerman (2010) bring to light the limits of the exogeneity of birthdates. Using data on maternal characteristics, they show that mothers who give birth to children in the winter are more likely than mothers who birth children in the summer to exhibit characteristics associated with low socioeconomic status, namely they are more likely to be teenagers, who are unmarried, and who lack a high school degree (Buckles and Hungerman 2010) .

Because the maternal characteristics associated with winter birthdays may also be associated with voting patterns, we choose to confine our sample to registrants whose birthdays occur during select period of time, namely those born in the interval of time following the voter registration deadline and the election, compared to mirror interval proceeding that block of time. This subset has several additional properties convenient to our particular data analysis needs. First, it omits individuals that were ineligible to vote in the general election due to age requirements. Previous research has hypothesized that those born before and after the election are differentially affected by voter mobilization and, perhaps, persistent enthusiasm or lack thereof (Holbein and Hillygus 2016; Nyhan et al. 2017). Second, our time interval is far enough away from AVR implementation dates to provide for a cleaner treatment effect. However, individuals in this birthday window do exhibit a higher relationship between birthdate and registration date than the general population, simply because registration surges at this point in the election cycle. This surge is apparent both for traditional and AVR registration. We mitigate this concern with tests for differential registration bias between the birthday intervals analyzed.

Our instrumented variable is a dichotomous variable coded as one if the voter is registered to vote by the general election voter registration deadline, zero otherwise[10]. Our primary instrument

[10] There is an important reason why we do not code this as registration specifically via AVR prior to the deadline. "Always takers" will sometimes register through AVR, and sometimes register through traditional means. The option they choose depends on their birthdate. If their birthdate precedes the election, they will register using AVR whereas if their birthday follows the election they will register traditionally. It is important that there is no

is a dichotomous indicator for birthdays that occur prior to a cutoff date, the voter registration deadline in California and 21 days prior to the voter registration deadline in Oregon[11]. In Oregon, the cutoff date varies from voter registration deadline due to the fact that AVR registrants have 21 days to opt-out of registration. Twenty-one days prior to the voter registration deadline is thus the latest date by which visits to the DMV would result in AVR registration without affirmative action by the individual. In California, the voter registration deadline is the analogous date.

Table 3 presents difference of proportion and difference of means tests for covariates based on our instrument. We do not expect any of these variables to be affected by birthdates. As is evident in the table, we largely achieve statistical balance on all covariates for the smaller birthday window, but not birthdays in the full calendar year. However, the statistical discrepancy is in part the result of lower power for the birthday window. This is most notably true for the differences in proportions by the birthday intervals for whites and those registered as democrats. Regardless, the differences present within both the birthday window and the full calendar year are substantively small in size, as we will see much smaller than what we find for voter turnout. The results constitute important evidence for the independence of our instrument and bolster confidence in our decision to rely on a smaller subset of voters based on their birthdate.

We additionally test for differential voter registration bias. As noted by Nyhan et al, differential rates of voter registration can cause bias in turnout estimates (Nyhan et al. 2017). Registration might vary based on birthdays for two reasons. First, and non consequentially, birth patterns vary over the course of a year, which would naturally lead to variation by registration levels. Second, political campaigns may differentially mobilize voters based on their birthdays, particularly those whose birthdays fall prior or subsequent to the election. To test for differential mobilization within our intervals, we use federal CDC natality data as a baseline for population by birthdays.[12]

correlation between the instrument and instrumented variable for the "always takers". This is only achieved if the instrumented variable is *any* form of registration.

[11] Due to differences in registration dates, the birthday windows for California are different sizes. Oregon's birthday window is 84 days, September 6 through November 6, while California's is 30 days, October 8 through November 6.

[12] It would be better to use state data, as birth trends are affected by weather patterns. Unfortunately, day-by-day natality data is not available at the state level.

We conduct a t-test for the number of registered voters as a proportion of births based on our registration cutoff dates. We find statistically significant registration differences using the full calendar year, but not our narrower birthday window.

Table 3: Testing for Covariate Bias, Full Registration File vs. Birthday Window				
	OR window	**OR Full**	**CA Window**	**CA Full**
Democrat	.00376, p=.354	.00171, p=.0003	.002, p=.005	.003, p=.000
Republican	.0012, p=.437	.00245, p=.005	.002, p=.265	.0017, p=.000
Age	-.0518, p=.442	.0907, p=.0162	.069, p=.211	1.3295, p=.000
White	-.0032, p=.023	-.0029, p=.0002	.0005, p=.739	.004, p=.000
Registration	.0007, p=.796	-.0015, p=.002	.06, p=.378	.180, p=.000
Notes: Cell entries are the differences in proportions or means between birthday intervals before and after the registration deadline for each group (e.g. Democrat, non-Democrat), and the calculated p-value, comparing the full registration file with the file restricted by proximity to the registration deadline.				

Results

Following Angrist (1990), it is useful to start an instrumental variable analysis with a calculation of Wald estimates. Wald estimates are the ratio of the difference of proportion of the outcome Y (voter turnout) for the group Z = 1 (Pre registration cutoff birthdays) and the group Z = 0 (Post registration cutoff birthdays) to the difference in proportions of the variable X (registration by the election deadline) for the group Z = 1 and the group Z = 0. These estimates are helpful to present due to their transparency of calculation. Table 3 presents the construction of Wald estimates for the relevant state elections. Again, In order to improve the strength of our instrument, we limit the data to individuals who registered while AVR was in place.

$$\text{Wald Estimate} = \frac{E[y|z=1] - E[y|z=0]}{E[x|z=1] - E[x|z=0]}.$$

Table 4: Wald Test for Instrumental Variable, Test (AVR) Group		
	Oregon 2016	California 2018
First Stage: Pre/Post Registration by Deadline Difference	.02***	.0871***
Second Stage: Pre/Post Voting Rate Difference	.0048***	.0089***
Wald Estimate	.284***	.103***
*** p<0.01, ** p<0.05, * p<0.1		

A Wald estimate is equivalent to the coefficient of a two stage least squares instrumental variable regression with no covariates and a dichotomous instrument. As with the coefficients from instrumental variable regressions, it should be interpreted as the LATE, or treatment effect on the compliers. Here we see that voter registration, implicitly through AVR since this is the type of registration with the strongest association with birthdays, increases the likelihood of voting by 28% in Oregon 2016 and 12% in California 2018. Voters use AVR both for new registration and to update their registration, so this statistic is the combined effect for both groups. In Oregon, 95% of those registered via AVR in 2016 and voted were new voters (meaning they had no record going back to 2008),[13] whereas in California, only 25% of AVR registrants are new registrants, as identified by that state's voter file. This stark difference is important to keep in

[13] Source: Sean McIlwee, Brian Schaffner, and Jessie Rhodes. June 20, 2017. "Automatic Voter Registration in Oregon." Demos Policy Brief.
https://www.demos.org/policy-briefs/oregon-automatic-voter-registration#footnote4_2z3wymy

mind when interpreting the effect of the policy, as it drives much of the difference between these two estimates.

As previously stated, this is the causal effect for the compliers, the subgroup of population that would register if and only if their birthday happens to fall before the AVR voter registration cutoff date. As we will address in the next section, calculating the number of compliers is important in understanding the magnitude of the effect of AVR for the state overall. Finally, this causal effect may encapsulate multiple policies in place. In particular, this estimation strategy cannot disentangle previous motor voter policies from automatic voter registration. As such, this effect must be interpreted as the aggregate affect of all motor voter policies.

To bolster the confidence in our results we conduct placebo tests for years prior to AVR implementation. This is a helpful exercise as it can help us gauge the extent to which previous motor voter activities affect our results. Using the Oregon 2016 and California 2018 voter files, we limit the files to voters who updated or originated their registration four years prior. We further limit the data to age-eligible individuals in those years. In Oregon, we repeat this exercise both for what would have been the AVR registration cutoff date, and for the voter registration deadline. We move the interval of birthdays considered to reflect this difference. These analyses find some significant results for registration, but not voting. Further, the registration effects are smaller than what is evident under AVR. This mostly allies with our expectation that traditional motor voter legislation was less effective at AVR, smaller in terms of registration and statistically null in terms of voter turnout.

We now add covariates to this estimation using a two-stage least squares instrumental variable regression. In the first stage, instruments predict the explanatory variable of interest. In the second stage, the model estimated values in stage 1 are used to predict the dependent variable. As before, our instruments are birthday indicators, the instrumented explanatory variable is registration prior to the voter registration deadline, and the dependent variable is voter turnout.

There is no two-stage least squares estimator for dichotomous outcomes and estimates are derived from linear probability models in both stages.[14]

Table 5: Wald Test for Instrumental Variable, Placebo Groups			
	Oregon 2012- AVR cutoff	Oregon 2012 - registration deadline	California 2014
Placebo First Stage: Pre/Post Registration by Deadline Difference	.0081*	.0168***	.0124***
Placebo Second Stage: Pre/Post Voting Rate Difference	.0011	.0047	.0052
Placebo Wald Estimate	Insig.	Insig.	Insig.
*** p<0.01, ** p<0.05, * p<0.1			

We controlled for several demographic variables. We code for residency in a populous county dichotomously, coded as one if you live in a county with over 150,000 residents, and zero otherwise. We also include control variables that measure whether or not the voter is registered as a Democrat, Republican, or with a third party, the omitted category being an unaffiliated voter. We control for race and gender using imputations from R's wru and gender packages.[15] Finally, we include a continuous variable that counts the age of the potential voter at the time of the election.

Our estimation appears in Table 6. Diagnostic tests confirm that our instrument has sufficient strength. Following rule of thumbs put forward by Stock and Watson (2007), the F-tests

[14] IV probit estimation, which allows for nonlinear estimation of a dichotomous instrument, but not outcome, failed to converge.

[15] Race is imputed using county-level census data in addition to surnames. Gender is imputed based on year of birth and given names. Due to missing values, gender is excluded from all but the gender subgroup analyses.

comparing the sum of squared residuals from first-stage models with and without our instrument is above 10, F=187 in Oregon 2016 and F=3,142 in California 2018.

The two-stage results conform to the Wald estimate, showing a 29% effect of AVR in Oregon and 10% in California 2018. Again, the previous caveats apply. We must interpret these results as the LATE, as a combined effect for new and updated registration for AVR, as well as a combined effect for AVR and previous motor voter policies. Indeed, as we will show shortly, much of the variation in these estimates has to do with the proportion of AVR registrants that are new voters. The control variables behave expected, with age, race identified as white, urban residency, and partisan political affiliation all positively associated with voting.

Table 6: Birthdates, AVR, and Turnout

	Oregon 2016	California 2018
Registered Prior to Deadline	0.287***	0.102***
	(0.085)	(0.016)
Populous County	0.039***	-0.015***
	(0.002)	(0.004)
Black, African American	0.110***	-0.026
	(0.030)	(0.031)
White	0.154***	0.034
	(0.029)	(0.031)
Asian	0.114***	-0.050
	(0.030)	(0.031)
Hispanic	0.098***	-0.077**
	(0.030)	(0.031)
Democrat	0.313***	0.176***
	(0.005)	(0.002)
Republican	0.309***	0.126***
	(0.003)	(0.002)
Third Party	0.222***	0.070***
	(0.003)	(0.003)
Age at Election	0.002***	0.005***
	(0.0001)	(0.00004)
Constant	-0.069	0.294***
	(0.069)	(0.033)
Observations	267,327	446,422
R^2	0.163	0.099
Adjusted R^2	0.163	0.099
Residual Std. Error	0.444 (df = 267316)	0.453 (df = 446411)

Notes:

Second stage of a two-stage least squares instrumental regression (linear probability model). Dependent variable is voter turnout. Omitted categories are race identified as Other and non-affiliated partisanship. *** p<0.01, ** p<0.05, * p<0.1

Subgroup Analysis

It is important to remember that the effects presented above are combination effects for all registrations. It is quite possible that there are heterogeneous treatment effects, notably for new and re-registrants, categories whose relative group sizes vary greatly by state. Fortunately, it is possible to rerun our estimation within subgroups to gauge such important variation. Table 7 presents the results of a series of two-stage least squares analyses, each run on a different subgroup population. As with our primary analysis, we are leveraging exogenous differences in birthdates to construct our estimates.

This exercise reveals several interesting variations in effect sizes. Notably, the difference in the effect size between California and Oregon is almost entirely explained by differential AVR compositions of new registrants and re-registrants. Whereas Oregon's AVR enrolls mostly new registrants, with registration updates occurring by different means, California's AVR predominantly results in re-registration. This action seemingly has a much smaller effect on voter turnout, boosting turnout by 0.054 for re-registrants compared to 0.289 for new registrations. This latter effect is statistically indistinguishable from the effect of AVR in Oregon.

We also see a very interesting effect of gender on voter turnout. In both California and Oregon, the effect of AVR on turnout is stronger for women than men. Specifically, in Oregon the effect size for women is 0.435, compared to only 0.21 in men, more than double. In California, the overall effect for women was 0.12 compared to only 0.07 for me. Explanations for this unexpected policy consequence are worth considering. A straightforward interpretation is that barriers to voter registration are more significant for women than men and that AVR corrects this disparity. Alternatively, however, it may simply be more a function of the recent political climate than the policy per-se, with women differentially energized.

Age is also an important determinant for AVR induced turnout. In both states, we clearly see a larger effect for younger age categories. In Oregon, the highest effect sizes are for age groups 24-30 (0.4) and 31-40 (0.42). In California, the largest effect size is for ages 18-23 (0.25). It is quite likely, however, that our method understates the effect for the youngest age category in Oregon. Recall that our instrument is most strongly associated with license renewals, not the procurement of new IDs or licenses, an action that occurs following instead of prior to birthdates and with less of a pronounced relationship. In California, licenses must be renewed every 5 years, and hence likely includes those aged 21. In Oregon, in contrast, licenses must be renewed every 8 years. Additionally, in Oregon, a provisional licenses (granted under the age of 18) must be renewed precisely 2 years after the date of issue - an event more likely to occur following one's birthdate. Lack of strength of our instrument may therefore be attributed to an attenuated effect calculation for this age group, particularly in Oregon.

Marginal Effects on Voter Turnout:

	California 2018	Oregon 2016
All Registrants	.102 p=.000	.287 p=.000
New Registrants	.289 p=.000	Not Estimated
Re-Registrants	.054 p=.001	Not Estimated
Men	.0702 p=.0026	.206 p=.059
Women	.123 p=.000	.435 p=.005
Aged 18-23	.252 p=.0167	.260 p=.095

Aged 24-30	.132 p=.035	.403 p=.021
Aged 31-40	.122 p=.0035	.417 p=.012
Aged 41-60	.107 p=.000	.237 p=.086
Aged 60+	.0587 p=.004	.260 p=.094
Populous County	.102 p=.000	.261 p=.010
White	.121 p=.000	.297 p=.007
Hispanic	.10997 p=.001	.022 p=.96
Black	.0413 p=.659	.403 p=.63
Asian	-.005 p=.931	.293 p=.96
Registered Democrats	.0917 p=.002	.273 p=.195
Registered Republicans	.0942 p=.0016	-.033 p=.93
Registered 3rd Parties	.244 p=.059	.306 p=.80
Nonaffiliated	.104 p=.000	.264 p=.010
Notes: Entries are the estimated effect on turnout of being registered prior to the deadline, from a series of instrumental variables regressions, using the AER package in R.		

Registration and Total Turnout Effect

The turnout rates presented above belie the relative magnitude of the effect of AVR in each state. As previously mentioned, the results constitute the increase in vote likelihood due to AVR for the compliers, those who registered in time to vote only by chance of the timing of their birthdate. It is possible for a state to have a large turnout effect, but few compliers, an outcome theoretically of lesser magnitude than a policy that produces a small turnout effect but has many compliers. Hence, to fully understand the effect of AVR, it is important to estimate the number of compliers.

There is a straightforward calculation to estimate the number of compliers in an instrumental variable analysis. The number of compliers is simply proportional to the first stage of an instrumental variable analysis. More precisely, following Angrist and Pichke (2008), the number of compliers is given by the first stage times the probability the instrument is switched on. For this calculation, and henceforth, we opt to use all birthdays in the calendar year, not just the smaller birthday window, so as to obtain total estimates for the entirety of AVR implementation. [16] We use this data because the birthday window right before the voter registration deadline is an outlier in terms of registration rates and as a result produces an unusually high estimate of compliers, which makes for inappropriate for extrapolation. We believe that birthday induced bias from using the entire calendar year is lower for the question of registration than turnout, particularly since registration under AVR does not require positive action. This theoretical reason, as well as the substantively low differences produced by our balance estimates help alleviate concerns about this choice.

The number of compliers estimated is 18,726 registrants in Oregon 2016 and 581,828 in California 2018. Unfortunately, this simplistic calculation cannot capture the full effect of AVR in practice. This estimate captures the number of people who register to vote in time for the

[16] Recall that the smaller birthday window coincides with an unusual level of registration, as mobilizations and voter interest are in full force in this time period.

election only because of their birthday -- a likely small proportion of number of people that registered by AVR who otherwise would fail to register, the practical estimate of interest. Most apparently, AVR reaches a much broader group of individuals than license renewers, the primary population whose actions are correlated with our instrument. Those who visit the DMV due to a change of address or a misplaced license make these transactions with no correlation to their birthdate, and thus would not be counted as compliers even if they would not register to vote in the absence of AVR. Even more, not even all those who renew their license will be counted as compliers using our method. In most states, individuals with expiring licenses have a year to renew their licence in advance. A lack of procrastination causes a contamination effect between our comparison groups. This behavior would likewise cause an underestimate of the number of individuals who would not have registered to vote in the absence of AVR policy.

We run a simulation to probe the degree downward bias in our estimate of compliers. In exercise, we begin with the assumption that all individuals will behave as compliers, e.g. register to vote in time for the election if and only if they visit the DMV prior to the registration deadline. We use empirical data on DMV visitation patterns to determine the likelihood that individuals will visit the DMV by this cutoff. In this simulated exercise, the difference in registration rates by our birth date intervals constitutes the maximal number of potential birthday compliers that our instrument would ever estimate. As a proportion of the population for which the instrument is switched on (which is the actual number of compliers in this simulated exercise), this value proxies as an estimate of complier bias and we use it to extrapolate a more universal effect of AVR.

Empirical DMV visitation patterns are obtained from the subset of individuals who registered using AVR specifically. This data was made available by request from the Oregon Secretary of State and appears in the California voter registration file.[17] By definition, this subset of individuals registered at the time of their visit to the DMV and, as such, their effective

[17] In Oregon, AVR registration is defined only as OMV Phase 1 registration. In California, AVR registration is defined as registration with registration methods of “DMV”, “DL44”, or “RBM”categories with constitute in person, electronic, or by mail transactions respectively.

registration date serves as a proxy for their DMV visit date.[18] From this data, we produce a probability distribution of the likelihood of visiting the DMV relative to one's birthdate. This distribution mirrors those expressed in figure 1 for each state..

In our simulation, this probability distribution is applied to every individual who registered to vote in Oregon 2016 and California 2018 following AVR implementation. For each voter, we calculate the cumulative probability of going to the DMV between the start of AVR implementation and the voter registration cutoff date. This value will vary based on the voters birthdate. For example, if you were born on the day that AVR took effect, your cumulative probability would be constituted only by the likelihood of going to the DMV *after* your birthday, for as many days that span till the voter registration deadline. In contrast, if your birthday fell 30 days after AVR implementation, your likelihood of visiting the DMV prior to the voter registration deadline would be the cumulative probability of going to the DMV 30 days prior to your birthday plus every subsequent day until the voter registration deadline. Again, because we are assuming in this simulation that every voter will behave as a complier, the cumulative probability of going to the DMV is the same as the probability of being registered to vote in time for the election.

We divide this data into birthday intervals based on our instrumental variable. Here, this necessarily means that those whose birthdays fall after the election are included and fall in the uninstrumented birthday interval. From this data, we repeat our first stage Wald estimation. The difference in proportional election-eligible registration rates between these two groups times the number of individuals in the pre-registration cutoff birthday interval is the number of potential birthdate compliers. This number will necessarily be higher than our original estimate of birthday compliers because it counts as compliers people who are in reality "always takers" -- people who would register in advance regardless of their birthdate. This number tracks the maximum number of people we would expect to see as birthday compliers if everyone behaved as a complier.

[18] For Oregon, this is technically the date visited the DMV plus 21 days for registration to mature and any additional administrative processing time. These probability distributions are visualized in the first panel of Figure 1.

Alone, the first-stage estimate constitutes a measure of downward bias of our initial complier estimate. It is 0.43 in California and 0.106 in Oregon. These numbers can be interpreted as the percentage of compliers captured by our instrument, given real-world DMV visitation patterns. Our method works much better in California rather than Oregon because birthdays better explain DMV visitation patterns in that state, likely due to shorter license expiration durations (5 years vs 8 years in Oregon)

By dividing by our estimate of complier bias, we can obtain unbiased registration and turnout statistics. Specifically, we calculate that, though a combination of preexisting motor voter legislation and the adoption of AVR, the State of Oregon gained 40,617 voters in a 6 month period of AVR implementation while California gained 139,558 voters in its 6 months of implementation in 2018. Equalizating the duration of implementation to 6 months, this produces a 0.8% turnout effect in Oregon and 0.5% effect in California as a percentage of VEP.

Table 8: Compliers and Extrapolated Turnout Effects

	California 2018	*Oregon 2016*
Estimated Birthday Compliers	581,828	18,726
Potential (Simulated) Birthday Compliers in Calendar Year	1,082,659	461,397
Complier Bias: Simulated Compliers as a Proportion of Z=1	0.43	0.106
Unbiased Turnout Estimate during AVR	139,558	40,617
Standardized Turnout: Voters in 6 month Period as a Percentage of VEP	0.00874	0.00582

Conclusion

This paper leverages an exogenous reason why citizens are affected by AVR, the expiration of their driver's license. Because license expiration occurs on one's birth date, we were able to leverage birthdate information to test for an effect of AVR on registration and turnout. Specifically, we compared turnout rates of registrants whose birthdays fall before and after the voter registration deadline. We found that having a birthday in the latter interval, which would allow you to be registered at the DMV in time for the registration deadline, resulted in a 29% increase in turnout likelihood for Oregonians and 10% increase for Californians. However, all of this discrepancy between states was shown to be a result of the fact that California's AVR system produces many updates to registrations, an act with a much smaller marginal effect on turnout.

Replication of our estimation method on subgroups produced several interesting insights. AVR appears to have a larger effect on women than men, as well as for younger voters. Partisans are also marginally more affected, particularly registrants for third parties. Analysis that estimated effects by race were not encouraging, with AVR having the most robust effects on whites, though issues concerning power must be considered when interpreting these findings.

Finally, this analysis used data on DMV visitation patterns to probe the overall turnout effects in Oregon and California. Adjusting for differences in the Voting Eligible Populations, we find that 6-months of AVR implementation led to a 0.8% turnout effect in Oregon and 0.5% effect in Oregon. Given that the effect sizes are expected to be rather constant over the short term, it appears that the effect of AVR will be substantial. Moving forward, we hope to repeat our analysis for additional states and years and better probe the potential heterogenous treatment effects for different qualifying transactions under AVR.

Works Cited

Barone, Guglielmo, and Guido de Blasio. 2013. "Electoral Rules and Voter Turnout." *International Review of Law and Economics* 36 (October): 25–35. https://doi.org/10.1016/j.irle.2013.04.001.

Brians, Craig Leonard. 1997. "Residential Mobility, Voter Registration, and Electoral Participation in Canada." *Political Research Quarterly* 50(1): 215–27. doi:10.1177/106591299705000111.

Brians, Craig Leonard, and Bernard Grofman. 2001. "Election Day Registration's Effect on U.S. Voter Turnout." *Social Science Quarterly* 82 (1): 170–83. https://doi.org/10.1111/0038-4941.00015.

Broockman, David E. 2014. "Do Female Politicians Empower Women to Vote or Run for Office? A Regression Discontinuity Approach." *Electoral Studies* 34 (June): 190–204. https://doi.org/10.1016/j.electstud.2013.10.002.

Brown, Robert D., and Justin Wedeking. 2006. "People Who Have Their Tickets But Do Not Use Them: 'Motor Voter,' Registration, and Turnout Revisited." *American Politics Research* 34 (4): 479–504. https://doi.org/10.1177/1532673X05281122.

Burden, Barry C., David T. Canon, Kenneth R. Mayer, and Donald P. Moynihan. 2014. "Election Laws, Mobilization, and Turnout: The Unanticipated Consequences of Election Reform." *American Journal of Political Science* 58(1): 95-109. https://doi.org/10.1111/ajps.12063.

Enamorado, Ted, Benjamin Fifield, and Kosuke Imai. ``fastLink: Fast Probabilistic Record Linkage." available through The Comprehensive R Archive Network.

Franklin, Daniel P., and Eric E. Grier. 1997. "Effects of Motor Voter Legislation: Voter Turnout, Registration, and Partisan Advantage in the 1992 Presidential Election." *American Politics Quarterly* 25 (1): 104–17. https://doi.org/10.1177/1532673X9702500106.

Gemmiti, Nathan V. 1998. "Porsche or Pinto: The Impact of the Motor Voter Registration Act on Black Political Participation." *Boston College Third World Law Journal*18: 71.

Gerber, Alan S., Gregory A. Huber, and Seth J. Hill. 2013. "Identifying the Effect of All-Mail Elections on Turnout: Staggered Reform in the Evergreen State." *Political Science Research and Methods* 1 (1): 91–116. https://doi.org/10.1017/psrm.2013.5.

Gerber, Alan S., Daniel P. Kessler, and Marc Meredith. 2011. "The Persuasive Effects of Direct Mail: A Regression Discontinuity Based Approach." *The Journal of Politics*73 (1): 140–55. https://doi.org/10.1017/S0022381610000927.

Green, Donald P., Terence Y. Leong, Holger L. Kern, Alan S. Gerber, and Christopher W. Larimer. 2009. "Testing the Accuracy of Regression Discontinuity Analysis Using Experimental Benchmarks." *Political Analysis* 17 (4): 400–417.

Griffin, Rob, Paul Gronke, Tova Wang, and Liz Kennedy. 2017. "Who Votes With Automatic Voter Registration?" *Center for American Progress*(blog). Accessed January 18, 2018. https://www.americanprogress.org/issues/democracy/reports/2017/06/07/433677/votes-automatic-voter-registration/.

Gronke, Paul, Eva Galanes-Rosenbaum, Peter A. Miller, and Daniel Toffey. 2008. "Convenience Voting." *Annual Review of Political Science* 11(1): 437-455.

Hanmer, Michael J. 2009. *Discount Voting: Voter Registration Reforms and Their Effects*. Cambridge University Press.

Hersh, Eitan D. 2015. *Hacking the Electorate: How Campaigns Perceive Voters*. Cambridge: Cambridge University Press.

Highton, Benjamin, and Raymond E. Wolfinger. 1998. "Estimating the Effects of the National Voter Registration Act of 1993." *Political Behavior* 20 (2): 79–104. https://doi.org/10.1023/A:1024851912336.

Hill, D. 2003. "A Two-Step Approach to Assessing Composition Effects of the National Voter Registration Act." *Electoral Studies* 22 (4): 703–20. https://doi.org/10.1016/S0261-3794(02)00019-7.

Holbein, John B., and D. Sunshine Hillygus. 2016. "Making Young Voters: The Impact of Preregistration on Youth Turnout." *American Journal of Political Science*60 (2): 364–82. https://doi.org/10.1111/ajps.12177.

Keele, Luke, Rocío Titiunik, and José R. Zubizarreta. 2015. "Enhancing a Geographic Regression Discontinuity Design through Matching to Estimate the Effect of Ballot Initiatives on Voter Turnout." *Journal of the Royal Statistical Society: Series A (Statistics in Society)*178 (1): 223–39. https://doi.org/10.1111/rssa.12056.

Knack, Stephen. 1995. "Does 'Motor Voter' Work? Evidence from State-Level Data." *The Journal of Politics*57 (3): 796–811. https://doi.org/10.2307/2960193.

———. 1999. "Drivers Wanted: Motor Voter and the Election of 1996" *PS: Political Science* 32 (2): 237–43. https://doi.org/10.2307/420558.

Knack, Stephen, and James White. 1998. "Did States' Motor Voter Programs Help the Democrats?" *American Politics Quarterly* 26 (3): 344–65. https://doi.org/10.1177/1532673X9802600304.

Kousser, Thad, and Megan Mullin. undefined/ed. "Does Voting by Mail Increase Participation? Using Matching to Analyze a Natural Experiment." *Political Analysis* 15 (4): 428–45. https://doi.org/10.1093/pan/mpm014.

Krasno, Jonathan S., and Donald P. Green. 2008. "Do Televised Presidential Ads Increase Voter Turnout? Evidence from a Natural Experiment." *The Journal of Politics* 70 (1): 245–61. https://doi.org/10.1017/S0022381607080176.

Leighley, Jan E., and Jonathan Nagler. 2013. *Who Votes Now?: Demographics, Issues, Inequality, and Turnout in the United States*. Princeton University Press.

Martinez, Michaek D., and David Hill. 1999. "Did Motor Voter Work?" *American Politics Quarterly* 27 (3): 296–315. https://doi.org/10.1177/1532673X99027003002.

McElwee, Sean, Brian Schaffner, and Jesse Rhodes. 2017. "Oregon Automatic Voter Registration." Demos. Accessed November 20, 2018. https://www.demos.org/publication/oregon-automatic-voter-registration.

Meredith, Marc. 2009. "Persistence in Political Participation." *Quarterly Journal of Political Science* 4 (3): 187–209. https://doi.org/10.1561/100.00009015.

Mycoff, Jason D., Michael W. Wagner, and David C. Wilson. 2009. "The Empirical Effects of Voter-ID Laws: Present or Absent?" *PS: Political Science & Politics* 42 (1): 121–26. https://doi.org/10.1017/S1049096509090301.

Neiheisel, Jacob R., and Barry C. Burden. 2012. "The Impact of Election Day Registration on Voter Turnout and Election Outcomes." *American Politics Research* 40(4): 636–64. doi:10.1177/1532673X11432470.

Parry, Janine A., and Todd G. Shields. 2001. "Sex, Age, and the Implementation of the Motor Voter Act: The 1996 Presidential Election." *Social Science Quarterly* 82 (3): 506–23. https://doi.org/10.1111/0038-4941.00039.

Pew Charitable Trust. 2014. "Measuring Motor Voter" Accessed January 18, 2018. http://www.pewtrusts.org/~/media/assets/2014/05/06/measuringmotorvoter.pdf.

Piven, Frances Fox and Richard A. Cloward. 2000. *Why Americans Still Don't Vote: And Why Politicians Want It That Way*. Boston, MA: Beacon Press.

Rhine, Staci L. 1995. “Registration Reform and Turnout Change in the American States.” *American Politics Quarterly* 23 (4): 409–26. https://doi.org/10.1177/1532673X9502300404.

Rugeley, Cynthia, and Robert A. Jackson. 2009. “Getting on the Rolls: Analyzing the Effects of Lowered Barriers on Voter Registration.” *State Politics & Policy Quarterly* 9 (1): 56–78. https://doi.org/10.1177/153244000900900103.

Skovron, Christopher, and Rocıo Titiunik. 2015. “A Practical Guide to Regression Discontinuity Designs in Political Science.” Unpublished paper.

Thistlethwaite, Donald L., and Donald T. Campbell. 1960. Regression-discontinuity analysis: An alternative to the ex post facto experiment. *Journal of Educational Psychology, 51*(6), 309-317. http://dx.doi.org/10.1037/h0044319

Wolfinger, Raymond E., and Jonathan Hoffman. 2001. “Registering and Voting with Motor Voter.” *PS: Political Science & Politics* 34 (1): 85–92. https://doi.org/10.1017/S1049096501000130.