

Automatic Voter Re-registration as a Housewarming Gift: Quantifying Causal Effects on Turnout Using Movers

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June 17, 2022

Abstract

How much can automatic voter registration (AVR) increase turnout? Unlike many democracies, most American voters face the additional cost of registration, resulting in potential disenfranchisement. AVR is naturally expected to promote turnout, but its causal effects have rarely been quantified due to violations of crucial assumptions. I show that a variation of AVR that targets *existing* registrants as opposed to eligible non-registrants—termed automatic re-registration (ARR)—increases turnout by 5.8 percentage points. I exploit a natural experiment in a novel administrative dataset; election officials in Orange County, California, notified existing registrants who moved within-county that their residential addresses were automatically updated. The treatment alleviated registrants of re-registration burdens, but only for those who moved before the legal cutoff date, enabling a quasi-random treatment assignment. Contrary to the popular narrative, ARR had no significant effect on the turnout of registered Democrats, but Republicans’ and nonpartisans’ turnout increased by 8.1 and 7.4 percentage points.

How much can automatic voter registration (AVR) increase turnout? In the U.S., voter registration is not automatic, unlike in most democracies. Having to register to vote has been cited as a significant cost of voting that potentially disenfranchises the electorate (Rosenstone and Wolfinger, 1978; Highton, 2004; Ansolabehere and Konisky, 2006; Braconnier et al., 2017). Per theories of political participation (Riker and Ordeshook, 1968), higher costs would lower voter turnout. Naturally, AVR—defined here as an opt-out registration program—is expected to rid voters of such costs and promote voting access, leading to higher participation rates. This is an educated guess based on the successful adoption of opt-in “motor-voter” registration programs from The

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National Voter Registration Act (NVRA, 1993) (Knack, 1995; Highton and Wolfinger, 1998). Now, many states are adopting statewide AVR (Merivaki and Smith, 2020).

Despite its importance in theories of voting costs and voter enfranchisement—culminating in the For the People Act of 2021—a robust causal estimate of AVR’s impact on turnout in the U.S. is surprisingly lacking. This gap is because common forms of AVR are always implemented at the state or county level, the selection effect of which makes it difficult to estimate purely causal effects. It has been linked to increased turnout (Griffin et al., 2017), but the non-randomness of interventions jeopardizes counterfactual analyses, whether in cross-sectional or panel data.¹

I show that a variation of AVR that targets *existing* registrants as opposed to eligible non-registrants—termed automatic re-registration (ARR)—increases turnout by 5.8 percentage points. I exploit a natural experiment in a novel administrative dataset; election officials in Orange County, California, notified existing registrants who moved within-county that their residential addresses were automatically updated. This treatment alleviated the burden of having to re-register with new residential addresses, but only for those who moved before the legal cut-off date, enabling a quasi-random assignment of the treatment. I find that ARR significantly boosted the 2018 general turnout. Contrary to the popular narrative, it had no significant effect on Democrats’ turnout, while Republicans’ and nonpartisans’ turnout increased respectively by 8.1 and 7.4 percentage points.

Though ARR does not apply to non-registrants as AVR is typically understood, this paper makes important contributions to the literature. First, I provide robust causal estimates of lowering registration burdens on turnout, a valuable observation for theories of voting costs. I use individual-level data and not jurisdiction-level aggregated data. Moreover, the empirical strategy relies on a natural experiment, where the treatment is subject to an exogenous temporal discontinuity, circumventing selection biases. This informs the debate on whether registration indeed constitutes a significant barrier to political participation, where the empirical evidence has been mixed (Martinez and Hill, 1999).

Second, I quantify the effect of ARR on voter turnout for the first time, a policy implemented for decades yet never analyzed. In particular, ARR is designed to break down barriers for movers. Moving is a common life event that affects, on average, 10% of all American residents every year (Census Bureau, 2020).² Because changes in residence require re-registration to ensure that voters are assigned to correct political districts, movers face a unique burden of having to register again to maintain their voting status (Highton, 2000) as well as disruptions to their habit-inducing environment (Aldrich et al., 2011). The results show that ARR is strikingly effective in re-enfranchising such registrants, hinting that relief from new registration burdens is a significant factor even for existing registrants. The results also have implications for comparative research.

¹For example, Keele and Minozzi (2013) provide an overview of the potential pitfalls of using states as counterfactuals with instrumental variables when analyzing Election Day registration’s effect on turnout.

²Since movers tend to vote at lower rates, this proportion is likely smaller among registrants.

For example, [Braconnier et al. \(2016\)](#) reports that in France, where registration is similarly self-initiated, movers are often misregistered and do not turn out.

Finally, this paper is a counterexample to the popular idea that lowering registration costs only benefits Democrats, providing implications for current policy debates; as recently as in 2008, only 48% of registered voters supported AVR ([Alvarez et al., 2011](#)) though more recent studies show highly polarized support between Democrats and Republicans ([Mann et al., 2020](#)). While ARR is not equal to AVR, the root of the partisan debate is the same.

1 NCOA (National Change-of-Address) Processing and Automatic Re-registration

The NVRA, while mainly about facilitating registration for eligible non-registrants, also requires states to maintain an accurate, up-to-date voter database. For accurate list maintenance, states can use the permanent change-of-address (COA) requests submitted to the U.S. Postal Service (USPS). Individuals submit such requests so that the USPS can forward their mail to the new address while they make necessary adjustments. The USPS maintains the last 48 months' requests, called *NCOA data*, which approximates 160 million COA records with accurate old and new residences and when the individual moved/made the request. This data (1) determines the ARR treatment, and (2) is the key source of this paper.

California's law (CA Elec Code § 2225) requires the Secretary of State to match the statewide voter file to NCOA data (called *NCOA processing*). If existing registrants have changed addresses within the state, the Secretary will then notify relevant counties. For voters' privacy, COA request dates are coarsened to the month of the move instead of the exact date. The Secretary's office disseminates the data monthly, and each county makes necessary updates to its voter file.

If a county finds that a voter moved within-state, the voter file is *automatically updated*, and a confirmation postcard is sent to the new address. If this update was a mistake, the voter could inform the Registrar using pre-paid postage or phone.³ The NVRA requires that no list maintenance is performed 90 days before Election Day, setting an exogenous cutoff.

This is an automatic re-registration for registrants who move. It clearly conveys to the affected electorate that they have been subject to ARR, ridding movers of the costs of having to re-registering to vote. [Figure 1](#) shows the mailing sent out to the movers in its original form.

Most importantly, if the voter does not actively refute having moved, the voter file will reflect the new address. This is true even if voters do not return an acknowledgment of residential changes—without a counteraction, the USPS information is treated as a true move. If the voter is a permanent absentee, mail ballots will be sent to the new address. If the voter has crossed

³See Online Appendix B for relevant laws.

Dear Voter:

According to information we have received, the address where you live OR where you receive mail has changed to the address printed on the attached card.

If your new address is in Orange County, we will update your registration and future election materials will be sent to your address. If you no longer reside in Orange County, your voter registration has been placed in the inactive file. You must reregister in the county in which you now reside. To receive an affidavit call 1(800)345-VOTE.

Within 15 days, return the Business Reply portion of this card notifying us that your change of address is correct or is not a change of permanent residence.

If the information on this card is incorrect and you fail to notify our office, you may not receive your voting materials for future elections and your registration may be permanently canceled.

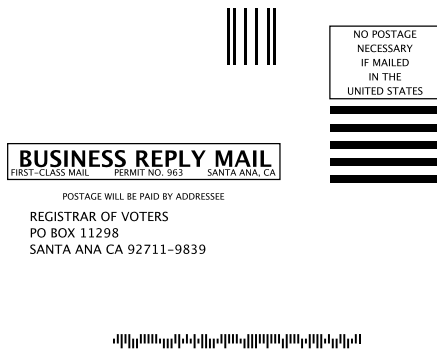
*If you need assistance in Chinese, Korean, Spanish or Vietnamese, please call (714) 567-7600.

REGISTRAR OF VOTERS
PO BOX 11298
SANTA ANA, CA 92711-1298
Phone No. (714) 567-7600



NON-PROFIT ORG.
U.S. POSTAGE
PAID
Santa Ana, Ca
Permit No. 77
Presorted

FORWARDING SERVICE REQUESTED



Dear Voter:
WE HAVE BEEN NOTIFIED THAT YOU HAVE MOVED.
Check and sign the correct box below.
Send back the completed card within 15 days.

<input type="checkbox"/> I live in Orange County and the address(es) above are correct.
Signature: _____ Date: _____
<input type="checkbox"/> I don't live in Orange County. Remove my name from the voter file.
Signature: _____ Date: _____
<input type="checkbox"/> The information above is incorrect.
I live at: _____ My mail is delivered to: _____
Signature: _____ Date: _____

NCOA

Figure 1: NCOA Mailing of Orange County, California, Front and Back

precinct boundaries, the new polling place's roster will have her name printed and not the old one.

ARR is an extraordinarily unique and proactive measure to help movers maintain their registration, but its efficacy has never been measured. Note that [Huber et al. \(2021\)](#) have shown that in Wisconsin, falsely flagging voters as movers depressed turnout, especially for racial minorities. However, the situation is dramatically different in California, where (1) same-day registration and voting are allowed, (2) in-county registrants who were flagged as movers were not removed but kept as active voters, and (3) because these were in-county movers, portable registration still applied ([McDonald, 2008](#)), ensuring provisional voting opportunities.

2 Data and Method

NCOA-based list maintenance also allows the detection of movers within the state.⁴ The data is built on 156 daily "snapshots" of more than 1.5 million unique voters in Orange County, California, from April 26th to December 31st, 2018 ([Kim et al., 2020](#)). Carefully documenting the changing data enables the detection of movers since registration records matched to USPS data will be automatically updated. Matching the voter file to USPS records allows for complete

⁴The detection applies to in-state movers across counties as well, but this data was inaccessible.

verification of a move that is not a correction of wrongly entered data. In total, there were more than 100,000 verified in-county movers between the 2016 and 2018 general elections.⁵ The dependent variable is the 2018 general turnout.

In Orange County, an NCOA matching and ARR were performed on July 26th, 2018, up to the movers who moved *before June 15th, 2018*, as the Secretary of State’s office disseminates NCOA data in the middle of the month. This discontinuity, stemming from the 90-day requirement, creates an interesting quasi-experimental opportunity, as those who have moved in the *latter half of June* and beyond were not subject to ARR, as opposed to those who moved in the *early half of June*, were subject to ARR, and were notified as such.⁶ This is a natural experiment with ARR as the treatment, and I estimate a regression model specified as

$$\text{Turnout}_i = \alpha + \beta \text{ARR}_i + \gamma \mathbf{X} + \epsilon_i \quad (1)$$

where i indexes individual registrants, \mathbf{X} contains covariates for conditional ignorability, ARR is the binary treatment variable, and the average treatment effect is β .

The final data consists of 5,585 movers who were registered at a different address by 2016 general but filed a change of address with the USPS in June 2018. The treatment group (3,294 voters) is movers from June 1st to 14th who have not voluntarily updated their registration records until late July. If the voter has voluntarily notified the Registrar so that the voter roll’s address is already up-to-date, the mailing is not sent. Therefore all others, including those who moved in early June and possibly disclosed it to the Registrar before July, are put to the control group (2,291 voters).⁷ If anything, this will estimate a lower bound of the treatment, as those who voluntarily inform the Registrar are more likely to vote.

The following covariates are controlled: number of times moved in 24 months (once, twice, or three times), distance from home to polls (Dyck and Gimpel, 2005; McNulty et al., 2009), permanent absentee voting status (Southwell and Burchett, 2000), age, inferred gender (Blevins and Mullen, 2015), inferred race (Imai and Khanna, 2016), partisan affiliation, 2016 general turnout history, census block-level median household income of both old and new residences, whether the voter was born abroad (e.g., a naturalized citizen), the Congressional district of the new home, and increasing degree of informational cost depending on how voters’ political districts changed.⁸ Congressional districts were controlled in the model to account for some hotly con-

⁵Specifically, it is possible to detect (1) all voters who had voluntarily reported their change of address to the Registrar *before* any NCOA processing, and (2) all voters who did not voluntarily report but filed a change of address with the USPS—the latter category of voters had their residence changes detected through NCOA processing, and ultimately had their address updated within the Registrar’s database.

⁶Although I cannot determine the exact date of the residential move (allowing for no other windows than two weeks before and after the treatment), whether the voter moved later than the cutoff for the month of June 2018 is clear.

⁷If the disclosure is voluntary, whether the voter moved in early or late June cannot be parsed.

⁸There are five categories: whether the voter moved within the same street address, precinct, state-level political districts, Congressional districts, or moved across Congressional districts.

tested House races, potentially driving higher turnout.

I present linear probability model results with a binary treatment specification and robust standard errors for clarity of coefficient interpretations. Logistic regression results' average marginal effects are identical to the LPM estimates. Almost all Kolmogorov-Smirnov tests of pre-treatment variables pass, showing a well-balanced dataset—see Table 1.⁹

	Treatment Group Mean	Control Group Std.	Mean	Std.	Statistic	p-value
Demographics						
— Age	43.68	16.14	44.20	16.65	0.0227	0.4909
— Male	0.47	0.50	0.47	0.50	0.0056	1.0000
— White	0.59	0.49	0.59	0.49	0.0026	1.0000
— Hispanic	0.16	0.37	0.14	0.34	0.0223	0.5111
— Asian	0.10	0.30	0.11	0.32	0.0155	0.9029
— Imputed Household Income (Previous Residence)	91.95	25.80	89.67	24.93	0.0442	0.0103
— Imputed Household Income (New Residence)	93.15	25.62	91.99	25.91	0.0271	0.2757
— Born Abroad	0.23	0.42	0.27	0.44	0.0317	0.1318
Political Variables						
— Democrat	0.32	0.47	0.33	0.47	0.0054	1.0000
— Republican	0.34	0.47	0.32	0.46	0.0209	0.5953
— 39th Congressional District	0.12	0.32	0.11	0.32	0.0060	1.0000
— 45th Congressional District	0.34	0.47	0.35	0.48	0.0066	1.0000
— 46th Congressional District	0.12	0.33	0.12	0.32	0.0068	1.0000
— 47th Congressional District	0.05	0.21	0.05	0.21	0.0002	1.0000
— 49th Congressional District	0.09	0.29	0.09	0.28	0.0064	1.0000
Turnout History						
— Gen. 2016 Turnout	0.73	0.44	0.72	0.45	0.0166	0.8522
— Pri. 2016 Turnout	0.33	0.47	0.33	0.47	0.0008	1.0000
— Gen. 2014 Turnout	0.24	0.43	0.25	0.43	0.0055	1.0000
— Pri. 2014 Turnout	0.10	0.30	0.11	0.32	0.0157	0.8932
— Gen. 2012 Turnout	0.47	0.50	0.45	0.50	0.0188	0.7235
— Pri. 2012 Turnout	0.12	0.33	0.14	0.35	0.0169	0.8371
Other Variables						
— Permanent Absentee Voter	0.61	0.49	0.72	0.45	0.1010	0.0000
— Times Moved	1.19	0.40	1.24	0.46	0.0335	0.0961
— Distance to the Polls	0.45	0.44	0.48	0.44	0.0573	0.0003

Table 1: Covariate Summary Statistics and Kolmogorov-Smirnoff Tests

3 ARR Increases Turnout

Figure 2 shows the regression result in the first column.¹⁰ ARR increases turnout for movers by 5.8 percentage points (95% CI: [0.0329, 0.0823]). To put this in comparison with non-movers, 70% of registrants who did not move in the last 24 months voted, while 64.0% of the treatment group voted, up 5.9% from 58.1% of the control group.

The estimated treatment effect is very large. To put this in context, take the landmark results of get-out-the-vote (GOTV) mailing analysis in Gerber et al. (2008). Showing households that

⁹See Online Appendix F for further discussions.

¹⁰The full regression results can be found in Online Appendix E.



Figure 2: The Effect of Automatic Re-registration and Placebo Tests

Note: average turnout: treated group (64.0%), control group (58.1%), and stayers (70.0%).

voting is public information and their own records resulted in a 4.9 percentage point increase in turnout. Threatening to expose voting records to neighbors resulted in an 8.1 percentage point increase. Many GOTV devices are insignificant or low in effect sizes.

For robustness, I perform placebo tests for cases that should theoretically yield null results. An intuitive test for sample self-selection is using turnout of previous elections. Because residential changes occurred after the 2016 general election, ARR should not affect the turnout of previous elections. The placebo tests all pass for 2016.¹¹

So why is the effect so large? First, ARR was an official, pre-paid postage mailing from election administrators and not a GOTV mailing from third-party civic organizations. [Mann and Bryant \(2020\)](#) show that even a simple postcard from official election administrators can encourage voter registration and turnout (2 percentage points) without any legal or administrative process changes—an official ‘nudge.’ [Malhotra et al. \(2012\)](#) also show that while third-party emails made no difference in turnout, emails from official sources increased turnout.

In addition, Orange County in 2018 was highly contentious. One district which had a Republican representative for thirty-five years now elected a Democrat. Voters may have been more responsive to stimuli related to the general election. However, note that Congressional districts of new residences are already controlled for in the model.

One could say that movers are, in general, a more peripheral set of voters, albeit temporarily. Many papers such as [Highton \(2000\)](#) and [Hansen \(2016\)](#) have demonstrated that disruptions of

¹¹Note that 2018 primary was not used because its date overlaps with voters’ moving period.

social environments can depress turnout for movers, and [Highton and Wolfinger \(2001\)](#) shows that residential stability slowly increases turnout. But also note that [Highton \(2000\)](#) argues that the major reason that movers have lower turnout is because of the re-registration burden. Therefore, from a policy perspective, this is an ideal setting to test the effect of resolving registration burdens. Considering that the sample is limited to in-county movers, likely less impacted by moving than out-county or out-state movers, the estimated effect is extraordinary.

Finally, one could also note that because information costs of learning new political contexts are lower for in-county movers, they may show a differentially large treatment effect. If further subset by disruptions to political environments, while the effect is null for those with minimal disruptions (i.e., within-precinct movers), the effect is also null for those with significant disruptions such as changes in congressional districts (see Online Appendix G). While this rules out the possibility that ARR is entirely driven by nudging, the effect may be smaller for out-county or out-state movers.

It is true that, unlike motor-voter type AVR, the ARR is not on non-registrants but rather existing registrants who have been disrupted. For non-registrants, automatic registration after interacting with the USPS may not boost turnout as much—these are a more peripheral population who have so far not been convinced by other registration drives such as interactions with the DMV. But as aforementioned, the effect is underestimated by including voluntary disclosers in the control group. Hence, the true effect size might be even larger, so that even for non-registrants, we may find some effects.

4 Subgroup Analyses By Party

Here I discuss subgroup analyses by party affiliation (Figure 3). Although the treatment is not blocked by subgroups, as convenience voting measures are often quoted as benefiting registered Democrats, it is still worth performing analyses by party registration.

In particular, national AVR reform is a major part of For the People Act of 2021. In this sweeping bill, automatic registration is defined as a “system that registers an individual to vote in elections for Federal office in a State, if eligible, by electronically transferring the information necessary for registration from government agencies to election officials of the State so that, unless the individual affirmatively declines to be registered, the individual will be registered to vote in such elections.” Although not explicitly specified, ARR is also applicable by this definition since USPS is a government agency.

The results are in Figure 3.¹² For Democrats, ARR shows no effect statistically different from zero (95% CI: [-0.0226, 0.0615]). The effects are strong for the rest. The point estimate is 8.1 percentage points for Republicans (95% CI: [0.0376, 0.1250]) and 7.4 percentage points for registrants with

¹²Again, see Online Appendix E for full regression results.

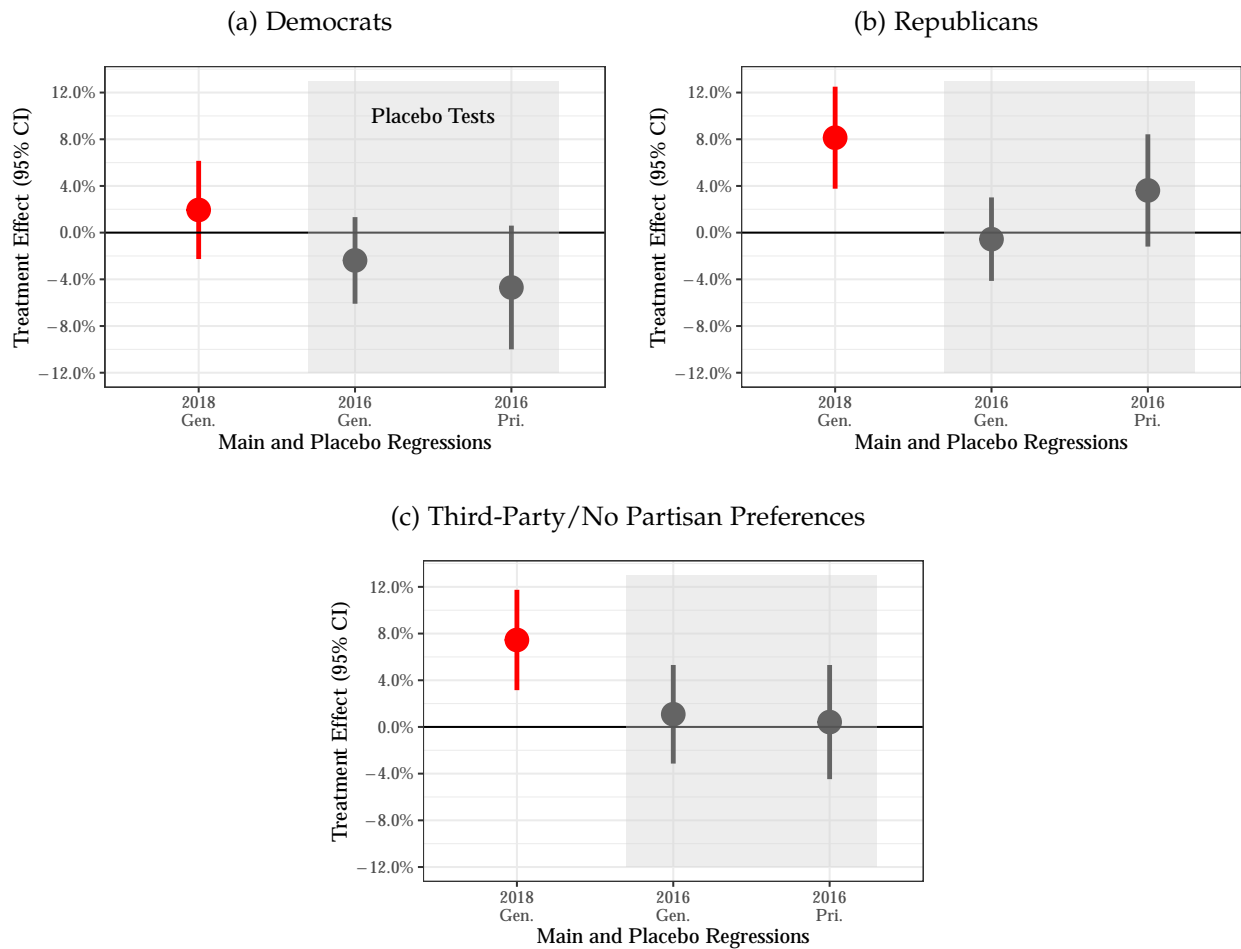


Figure 3: ARR Effects by Party

Note: average turnout: treated Democrats (68.8%), control-group Democrats (67.4%), stayer Democrats (72.4%), treated Republicans (66.8%), control-group Republicans (58.6%), stayer Republicans (77.4%), treated other voters (56.8%), control-group other voters (49.1%), stayer other voters (59.3%).

third-party or no partisan affiliations (95% CI: [0.0315, 0.1175]). The placebo tests all pass.

Although it remains to be seen whether we will see similar effects in counties other than this traditional conservative bastion, this is one piece of evidence that ARR, a close cousin of AVR, is not uniformly beneficial to the Democratic party. Indeed, given that the distribution of parties was almost uniform in the dataset—32.4% Democrats and 32.8% Republicans—why the treatment had no effect for Democrats is puzzling.

One possibility is that conditional on being a Democrat, voters choose more to be permanent absentees. In the test (full) dataset, 69.7% (67.0%) of Democrats and 64.2% (65.0%) of Republicans were permanent absentee. ARR does indeed have a greater effect on those who are not permanent absentees (8.9 percentage points) compared to those who are (4.1 percentage points), again passing all placebo tests (see Online Appendix E). But while the differential adoption rate of absentee voting is likely a factor, it is still not enough to fully explain the partisan gap.

Another possibility is that base mobilization for Democrats was much stronger—that they voted regardless of the treatment status, stimulated by the Trump presidency. This possibility is more difficult to test but seems unlikely since overall, Republicans turned out more (76.7%, conditional on registration) than Democrats (72.1%).

A final possibility is that many voters who registered as “No Partisan Preference” options are more Democratic-leaning. If only voters with no partisan preferences are considered, the effect estimate is 8.1% (95% CI: [0.0376, 0.1250]). So while including them could shift the treatment effect upwards, it still seems that Republicans benefited more from ARR in Orange County.

5 Discussion

This paper exploited a natural experiment to robustly test the causal effects of automatic voter re-registration (ARR). ARR, unlike AVR, is not for non-registrants. However, ARR still applies to an important group of existing registrants facing additional burdens of re-registration after changing residences.

I find that ARR increases turnout by 5.8 percentage points for in-county movers of Orange County, California, where some had the benefit of ARR and some did not. Moreover, the common rhetoric that convenience voting measures disproportionately benefit Democrats turned out to be false in this case, as Democrats had a small and statistically insignificant increase in turnout, while both Republicans and third-party/nonpartisans saw considerable boosts.

The results are promising. Although currently unique to California, ARR was designed for higher turnout and better voter list maintenance, and it is already part of the NVRA. The implementation is relatively simple, and it is not inherently favorable towards Democrats—both in nature and effect. From a public policy perspective, it is an encouraging confirmation that variations of AVR can help voters easily take part in the political process.

The results speak to important literature on political participation and voter disenfranchisement—ARR does increase turnout per predictions of the theory of voting costs. Moreover, the estimated effect size is large. In addition, in line with other literature that debunks common myths about partisan effects of voter participation (Shaw and Petrocik, 2020), I show that ARR does not always benefit Democrats more than Republicans.

If ARR is to be implemented nationwide, it will still be vital to systematically assess whether it merely increases the turnout of those already participating at higher rates. If the latter, more public discussions will be necessary about how election administration impacts representation.

The author declares the human subjects research in this article was deemed exempt from review by American University (IRB-2022-1). The author also declares no ethical issues or conflicts of interest in this research. This research was funded by the John Randolph Haynes and Dora Haynes Foundation. Research documentation and/or data that support the findings of this study are openly available at <https://github.com/sysilviakim/turnoutARR>. Limitations on the data availability are discussed in Online Appendix A.

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