ELECTION LAW JOURNAL
Volume 00, Number 0, 2020

DOI: 10.1089/elj.2019.0589

Promises and Perils of Mobile Voting

Anthony Fowler

ABSTRACT

Voter turnout is often low and unequal, but the opportunity to vote on a mobile device could drastically lower the cost of democratic participation. In 2018, West Virginia became the first U.S. state to utilize mobile voting in a federal election, allowing it for overseas voters from 24 of its counties. I utilize this trial to assess the likely effects of mobile voting on the size and composition of the voting population. Implementing a differences-in-differences design with individual-level administrative data, I estimate that the ability to vote with a mobile device increased turnout by three to five percentage points, and I find little evidence that the effects vary across age, party, or military status. At the same time, new survey data shows that many Americans are understandably wary of online voting.

Keywords: online voting, mobile voting, trust in elections, electoral reform, election administration, voter turnout

INTRODUCTION

Many citizens abstain from voting, and those who vote are often unrepresentative of the eligible population (e.g., Verba, Scholzman, and Brady 1995; Lijphart 1997). Furthermore, these inequalities in participation can have significant electoral and policy consequences (e.g., Bechtel, Hangartner, and Schmid 2015; Fowler 2013, 2015). Practitioners have proposed many reforms for increasing turnout and reducing inequalities in participation including early voting, vote-by-mail, less restrictive absentee voting, election-day registration, youth preregistration, and motor-voter registration. While these reforms have increased participation, the substantive magnitudes of the effects are often underwhelming (see Berinsky 2005 for a review).[1] Even get-out-the-vote interventions which reliably increase turnout (Green and Gerber 2015) can exacerbate the differences between voters and the eligible population (Enos, Fowler, and Vavreck 2014). Aside from drastic reforms like compulsory voting, it appears difficult to meaningfully increase the participation of underrepresented groups and reduce inequalities in participation.

One understudied reform that has the potential to meaningfully affect participation is mobile voting. If voters could cast their ballots online using only their cell phones or mobile devices, that could dramatically reduce the costs of voting, and it could have significant effects on the size and composition of the voting population. Although mobile voting might seem unrealistic to many Americans, several

---

Anthony Fowler is an associate professor in the Harris School of Public Policy at the University of Chicago in Chicago, Illinois, USA. Professor Fowler thanks Tusk Philanthropies and the Chicago Harris Cyber Policy Initiative for funding and I thank Jennifer Benz, Chris Berry, Jake Braun, Monica Bustinza, Hannah Fingerhut, Justin Grimmer, Mary Hanley, Will Howell, Deak Kersey, Aileen Kim, Marc Meredith, Sheila Nix, Tomas Okal, Nimit Sawhney, David Sterrett, Emily Swanson, Trevor Tompson, Analiese Wagner, Brittany Westfall, Adam Zelizer, and conference participants at the Election Sciences, Reform, and Administration conference at the University of Pennsylvania for help and feedback.

[1] Since Berinsky's influential review, the literature has continued to produce mixed and often underwhelming estimates of the effectiveness of electoral reforms. See, for example, Kousser and Mullin (2007), Burden et al. (2014), and Fowler (2017).

countries have utilized online voting in local and federal elections, and West Virginia recently became the first U.S. state to utilize mobile voting in a federal election, allowing it only for overseas residents from some of its counties. Denver, Colorado and King County, Washington recently utilized mobile voting in local elections, and we are likely to see more trials soon, so this is a good time to study the consequences of this reform.

We currently have mixed evidence on the effects of online voting on participation around the world. Several European countries abandoned internet voting after seeing that the increases in turnout were not as large as expected (see Goodman and Stokes 2018 for discussion), but we might worry that internet voting was implemented when turnout was otherwise declining. Utilizing more compelling differences-in-differences designs, Goodman and Stokes (2018) find that online voting in local elections in Ontario increased participation by 3.5 percentage points, and Germann and Serdült (2017) detect little effect of internet voting relative to mail voting in Swiss referendums. We have virtually no evidence on the effects of internet voting in the U.S. because until recently, internet voting had never been attempted in a major U.S. election.

Other studies have investigated the security concerns associated with online voting around the world. For example, Springhall et al. (2014) studied the system used by Estonia, reproduced it, and then—for research purposes—demonstrated how attackers could alter election results or undermine the legitimacy of the system. Schryen and Rich (2009) investigated several elections conducted online in Estonia, the Netherlands, and Switzerland and similarly concluded that there were significant concerns about security, verifiability, and transparency. And more recently, Specter, Koppel, and Weitzner (2020) studied the system used by West Virginia and concluded that it, too, is vulnerable to attacks. Although this article is not explicitly about the security of online or mobile voting, these concerns are relevant for understanding the effects of mobile voting on participation and trust in elections.

In this article, I first present new survey data on U.S. public opinion about voting technology. Americans report that they are less confident that votes cast online will be counted correctly and that they are less willing to utilize online voting compared to other voting technologies. I also find that informing people that online voting will be secured by blockchain technology has a counterintuitive negative effect on their willingness to vote online, perhaps because this leads people to think more about security concerns. Overall, the survey evidence suggests that Americans are quite wary of internet voting.

To see what happens when Americans have the opportunity to vote online, I study West Virginia's trial with mobile voting in 2018. I utilize administrative data and take advantage of the fact that mobile voting was only available for overseas residents from some of West Virginia's counties, allowing me to implement a differences-in-differences design. Among people likely living overseas (more details on this below), I find that having mobile voting as an option increases requests for ballots by six to nine percentage points, and it subsequently increases voter turnout by three to five percentage points. A back-of-the-envelope calculation suggests that approximately half the people casting a ballot with the mobile app would not have voted if mobile voting was not an option. Mobile voting can increase turnout, at least among people living overseas, and these estimated effects appear greater than the effects of many other electoral reforms. At the same time, public wariness of online voting should be carefully considered before rolling out mobile voting more broadly.

PUBLIC OPINION ABOUT VOTING TECHNOLOGY

In September of 2018, I partnered with the Associated Press and the NORC Center for Public Affairs Research to survey voting-age Americans about their views on voting technology. The survey utilized AmeriSpeak®, the probability-based panel of NORC at the University of Chicago. NORC devotes significant effort and resources to obtain a representative sample of voting-age Americans and maximize response rates. See Dennis (2019) for more details. Online and telephone interviews using landlines and cell phones were conducted on the topic of voting technology with 1,059 voting-age Americans.

We asked respondents how confident they were that votes cast using different methods would be counted accurately, allowing them to report that they were not at all, not very, somewhat, very, or

TABLE 1. CONFIDENCE THAT VOTES WOULD BE COUNTED ACCURATELY

	Not at all	*Not very*	*Somewhat*	*Very*	*Extremely*	*Avg*	*Diff*	P-*val*
Baseline	.078	.145	.374	.245	.158	.565		
Electronic w/o paper receipt	.116	.211	.459	.152	.062	.458	−.107	.000
Electronic w/ paper receipt	.045	.071	.388	.341	.155	.623	.058	.000
Paper ballots, scanned	.044	.115	.433	.286	.123	.583	.018	.124
Paper ballots, hand counted	.112	.222	.370	.200	.096	.486	−.078	.000
Paper ballots, mailed in	.116	.213	.410	.175	.086	.476	−.089	.000
Votes cast online	.171	.263	.382	.133	.051	.407	−.158	.000

The top row indicates the survey-weighted proportion of respondents giving each of five possible answers to a question about their confidence that votes will be counted correctly in 2018. *Avg* is the average of an index measuring intention to vote, with *not at all confident* corresponding to 0, *extremely confident* corresponding to 1, and the other responses equally spaced in between. The subsequent rows report the comparable numbers when respondents were asked about their confidence that votes would be counted correctly under various voting technologies. *Diff* reports the difference in the confidence index between each technology and the baseline question. *P-val* is the two-sided *p*-value arising from testing the null hypothesis that average confidence is the same with this technology and in the baseline question.

extremely confident. The various voting methods were electronic ballots with and without a paper receipt, paper ballots that are scanned, paper ballots that are hand counted, ballots cast via mail, and ballots cast online. We also asked a baseline question regarding respondents' confidence that votes would be counted accurately in the 2018 midterm elections.[2] Table 1 shows the survey-weighted proportion of respondents selecting each response. The "avg" column reports the average intention to vote for each technology, where the confidence for each respondent is scaled such that "not at all" is 0, "extremely" is 1, and the other options are evenly spaced in between. The "diff" column reports the difference in the average index for each voting technology and the baseline level of intention to vote. And the "p-val" column reports the two-sided *p*-value arising from testing the null hypothesis of no difference.

We see the highest confidence under electronic ballots with paper receipts, and paper ballots that are scanned are a close second. Perhaps reassuringly, this is consistent with the recommendations of cybersecurity experts who believe that a verifiable paper trail is the best way to protect the integrity of elections and guard against hackers who would like to manipulate election results (e.g., Blaze et al. 2018). For the other technologies, respondents are notably less confident that votes would be counted accurately. We see the lowest confidence that votes will be counted accurately under online voting.

We also asked respondents how likely they were to vote in the upcoming 2018 midterm elections, giving them the option to report that they will certainly, probably, probably not, or certainly not vote, along with the intermediate option of reporting that they are not sure if they will or will not vote. We then asked them to similarly report their intention to vote in the hypothetical scenario in which different voting technologies were their only option available. The options were the same as those discussed above.

Table 2 reports the survey-weighted proportion of respondents selecting each possible level of vote intention for each possible voting technology. As before, the "avg" column reports the average intention to vote for each technology, where the vote intentions for each person are scaled such that "certainly not" is 0, "certainly" is 1, and the other options are evenly spaced in between. The "diff" column reports the difference in the average index for each voting technology and the baseline level of intention to vote, and the "p-val" column reports the two-sided *p*-value arising from testing the null hypothesis of no difference.

The results for vote intention closely mirror those for confidence that votes are cast accurately. We see the highest reported intention to vote under electronic ballots with paper receipts, and paper ballots that are scanned come in second place. For the other technologies, respondents are notably less likely to

---

[2] Unfortunately, the baseline question about the 2018 midterms was asked in a slightly different way than the other questions about various voting methods. Instead of reporting that they were not at all, not very, somewhat, very, or extremely confident, respondents reported whether they were not at all, only a little, a moderate amount, quite a bit, or a great deal confident. Therefore, the differences between each voting technology and the baseline condition could be plagued by this difference in question wording, but the differences between various voting technologies are not.

Table 2. Reported Intention to Vote with Different Technologies

	Certainly not	*Prob not*	*Not sure*	*Prob*	*Certainly*	*Avg*	*Diff*	P-*val*
Baseline	.076	.061	.135	.161	.567	.770		
Electronic w/o paper receipt	.090	.076	.182	.201	.450	.711	–.059	.000
Electronic w/ paper receipt	.071	.042	.136	.229	.522	.772	.002	.839
Paper ballots, scanned	.074	.053	.163	.211	.499	.753	–.018	.059
Paper ballots, hand counted	.099	.069	.196	.182	.455	.706	–.064	.000
Paper ballots, mailed in	.103	.115	.174	.162	.447	.683	–.087	.000
Votes cast online	.114	.141	.191	.148	.407	.648	–.122	.000

The top row indicates the survey-weighted proportion of respondents giving each of five possible answers to their likelihood of voting in 2018. *Avg* is the average of an index measuring intention to vote, with *certainly not* corresponding to 0, *certainly* corresponding to 1, and the other responses equally spaced in between. The subsequent rows report the comparable numbers when respondents were asked if they would vote if a particular voting technology was the only option available to them. *Diff* reports the difference in the vote intention index between each technology and the baseline question. *P-val* is the two-sided *p*-value arising from testing the null hypothesis that average vote intention is the same with this technology and in the baseline question.

report that they would vote compared to the baseline vote intention question, and the differences are highly statistically significant. The lowest reported intention to vote is under online voting, with respondents 16 percentage points less likely to report that they will certainly vote under online voting than under their existing voting technology and with the weighted index being .122 points lower. Mail ballots are also unpopular, with the weighted index .087 points lower than the baseline. Some studies of mail ballots in real elections show quite positive results (e.g., Gerber, Huber, and Hill 2013), and this finding is somewhat surprising in light of that evidence. It's possible that mail ballots have been successful in increasing turnout in places like Washington, but they might have negative effects if introduced nationwide. Or it's possible that respondents underestimate their utilization of mail ballots if that were their only option. In any case, potential voters report that they are less confident that votes would be counted accurately and that they are less likely to participate under less standard technologies like online voting.

We can also ask, descriptively, which kinds of people are more willing to participate under various voting technologies? In Table 3, I regress each respondent's vote intention (using the 0–1 scale described above) on binary indicators for age categories, gender, white racial identity, and college completion, along with the natural logarithm of reported income. Each respondent is weighted according to the survey weights with the goal of

Table 3. Who Is More Willing to Vote with Various Technologies?

	Electronic, no receipt	*Electronic, receipt*	*Paper, scanned*	*Paper, hand counted*	*Mail*	*Online*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Age 30–39	.009	.000	.003	–.006	.035	–.052
	(.035)	(.029)	(.031)	(.033)	(.035)	(.039)
Age 40–49	.022	.010	.034	.034	.010	.022
	(.028)	(.025)	(.024)	(.029)	(.026)	(.031)
Age 50–69	–.018	–.004	–.036	.021	–.017	–.061*
	(.025)	(.021)	(.021)	(.024)	(.026)	(.029)
Age >70	–.053	–.005	–.024	–.032	.015	–.134**
	(.027)	(.020)	(.021)	(.024)	(.032)	(.041)
Female	–.032	.016	–.010	–.028	–.010	–.018
	(.018)	(.016)	(.016)	(.018)	(.021)	(.023)
White	.037	.004	.007	.010	.004	.010
	(.021)	(.017)	(.019)	(.020)	(.023)	(.026)
College	.036	.002	.020	.059**	.071**	.106**
	(.018)	(.016)	(.015)	(.017)	(.020)	(.024)
log(Income)	.018	.021*	.015	.030**	.050**	.049**
	(.010)	(.010)	(.009)	(.011)	(.014)	(.014)
Survey weights	X	X	X	X	X	X
Vote intention FEs	X	X	X	X	X	X
Observations	1,059	1,059	1,059	1,059	1,059	1,059

Robust standard errors in parentheses; $**p<0.01$, $*p<0.05$; FEs, fixed effects.

estimating the average relationships of interest for a nationally representative sample. I also include fixed effects for each possible vote intention response in the baseline question. For many of the voting technologies, most of the estimated coefficients are close to zero, meaning that the kinds of people willing to vote under this technology are not meaningfully different from the kinds of people who are generally willing to vote in the baseline question. Wealthier and college-educated people are more willing to vote with paper ballots that are hand counted, mail ballots, and online ballots. And people over the age of 50 are much less likely to report a willingness to vote online. So combining the results from Tables 2 and 3, we see that online voting is the least popular voting technology and this is especially true among older people, lower-income people, and those without a college degree.

I also embedded a randomized experiment into the questions about online voting in order to study the conditions under which people might be more or less willing to cast their ballots online. Specifically, a randomly selected half of respondents were asked about "votes cast online," and the other half were asked about "votes cast online using blockchain, the technology behind Bitcoin and other virtual currencies." Perhaps the biggest concern with online voting is the security of the vote totals, and advocates of online voting argue that these concerns can be mitigated through blockchain technology, so I wanted to test whether respondents are reassured by this argument or not.

To estimate the effect of this information, I regress confidence that votes cast online would be counted correctly or willingness to vote online (both measured by their 0–1 indices) on an indicator for whether online voting was presented with or without the blockchain information. As before, I utilize survey weights, and I show the results with and without fixed effects for the baseline responses. These fixed effects are not necessary to obtain unbiased results, but they improve precision. Table 4 shows that telling respondents that online voting will be secured by blockchain technology has little effect on their confidence that online votes will be counted accurately, and it paradoxically reduces their willingness to vote online. The estimated effects on vote intention are substantively large—.097 and .075 points on the 0–1 scale—and highly statistically significant. A plausible explanation for this counterintuitive result is that many people are not reassured by the information about blockchain technology, and furthermore, this treatment reminds them that online voting comes with increased security risks.

Table 4. Does Blockchain Framing Increase Trust in Online Voting?

	Confidence online votes counted correctly		*Willingness to vote online*	
	(1)	*(2)*	*(3)*	*(3)*
Blockchain treatment	−.005	−.004	−.097**	−.075**
	(.021)	(.021)	(.028)	(.024)
Constant	.410**		.698**	
	(.015)		(.019)	
Survey weights	X	X	X	X
Baseline confidence fixed effects		X		
Baseline vote intention fixed effects				X
Observations	1,059	1,059	1,059	1,059

Robust standard errors in parentheses; ** $p < 0.01$, * $p < 0.05$.

The survey data gives us reasons to be concerned about the effectiveness of online voting in improving participation. On average, people report less willingness to cast ballots online relative to other methods and lower confidence that votes cast online would be counted correctly. Furthermore, the negative reactions to online voting are only worsened when we remind them of security concerns by discussing blockchain technology. Now, we turn to West Virginia to estimate the effects of mobile voting on participation in a real election.

MOBILE VOTING IN WEST VIRGINIA IN 2018

In 2018, West Virginia partnered with Voatz Inc. to offer mobile voting as an option for some of its overseas voters. They started with a "test pilot" in two counties (Harrison and Monongalia) during the May primary election. The test pilot was not advertised widely, and only 13 votes were cast using a mobile device, but state officials decided to expand mobile voting for the November general election. West Virginia did not want to excessively burden county election officials with this new technology, so they allowed counties to opt in or out of the project, and ultimately 24 of the state's 55

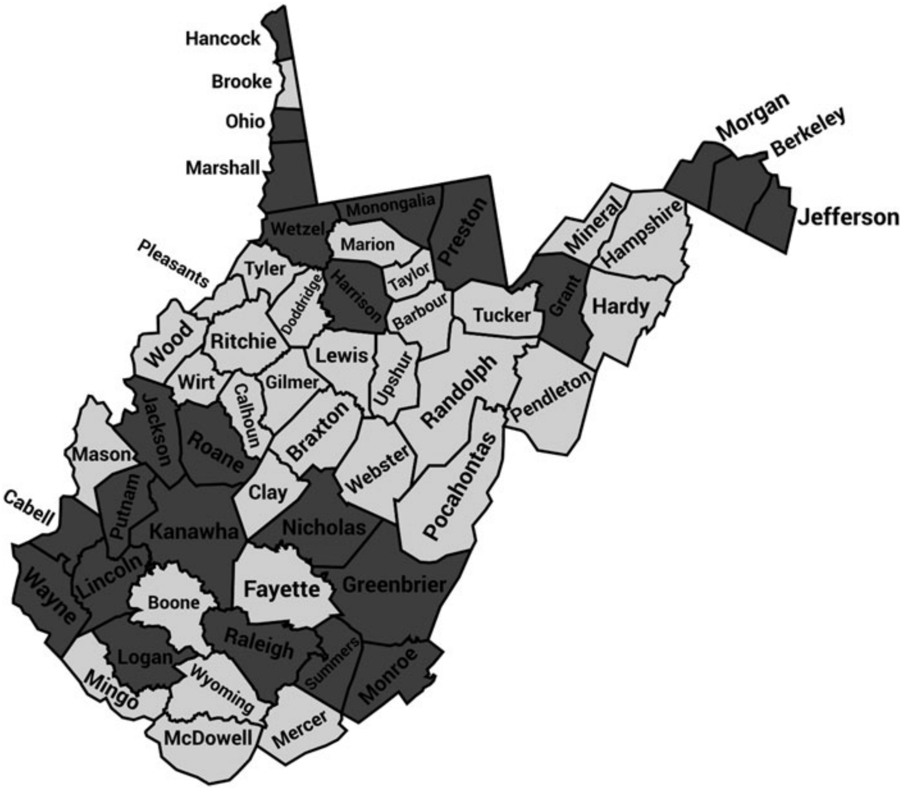

FIG. 1. West Virginia counties allowing mobile voting. Counties allowing mobile voting are *darkened*.

counties allowed mobile voting for overseas citizens. Figure 1 shows a map of West Virginia's counties, with those allowing mobile voting darkened. In total, 144 votes were cast with a mobile device in the November election.[3]

Mobile voting worked in the following way. As in every state, registered voters in West Virginia who were living outside the country were eligible to submit a Federal Post Card Application (FPCA), according to Uniformed and Overseas Citizens Absentee Voting Act (UOCAVA). Many of these citizens are active military personnel deployed overseas and their spouses, but other citizens living overseas are also eligible. If an FPCA is submitted and approved, the voter receives an absentee ballot along with instructions for submitting it. Overseas voters typically have the option of submitting their ballots via mail, fax, or scanning and e-mailing, and if the voters' permanent residence was in one of the 24 counties allowing mobile voting, they were also given the option to download a mobile app created by Voatz and submit their ballot online using the app.

The Voatz app uses the camera on the voter's mobile device to verify their identity, and it utilizes blockchain technology with the goal of protecting the anonymity and security of each individual's vote. While downloading a new app, verifying one's identity, and learning how to use a new technology is not costless, mobile voting is potentially more convenient and less time consuming than voting by mail or at a polling location. Among the 183 people who requested the app, 160 downloaded it, and 144 cast their votes with it.

Potentially limiting the effectiveness of mobile voting in this case is the fact that eligible voters first had to submit an FPCA in order to later vote using the mobile app. To submit an FPCA, West Virginians must fill out and sign a paper form and submit it via mail, fax, or by scanning and e-mailing. The people willing and able to submit an FPCA likely already have the interest and ability

---

[3] See "24 Counties to Offer Mobile Voting Option for Military Personnel Overseas," *Secretary of State Mac Warner*, September 20, 2018, <sos.wv.gov/news/Pages/09-20-2018-A.aspx>, and "Warner Pleased with Participation in Test Pilot for Mobile Voting," *Secretary of State Mac Warner*, November 16, 2018, <sos.wv.gov/news/Pages/11-16-2018-A.aspx> for more information.

to vote, so the effect of mobile voting may be lower among this subpopulation. The effects of mobile voting might be notably greater if registered voters could use the mobile app without first having to request it or if they could request it using only their mobile device.

Further limiting the relative effectiveness of mobile voting in this case is the fact that UOCAVA voters already had the option of casting their ballots via e-mail, and many did so. Of course, voting with the mobile app is still likely more convenient (for most) than filling out a paper ballot, scanning it, and e-mailing it. But to the extent that e-mail voting is already more convenient than more traditional forms of voting, we would expect that mobile voting would be even more effective if introduced for a population that didn't have the option of voting via e-mail.

To assess the effects of mobile voting on participation, I have obtained individual-level administrative data from the West Virginia Secretary of State's Office. This data contains information on every registered voter in West Virginia including their gender, age, party registration, county of residence, and turnout history. Ideally, we'd like to have data on all individuals who were eligible for UOCAVA status and test whether having mobile voting as an option made them more likely to vote. Unfortunately, there is no database of individuals eligible for UOCAVA status, and we only learn that a voter is eligible after they submit an FPCA. Furthermore, I don't want to focus only on the sample of individuals who submitted an FPCA since the option of mobile voting may have increased FPCAs—a hypothesis that I later test and confirm.

Therefore, to identify a sample of individuals that are most plausibly eligible for UOCAVA status and who are likely comparable between counties with and without mobile voting, I identify all registered voters who previously submitted an FPCA for the primary or general elections in 2014 or 2016 or for the primary election in 2018. In other words, if an individual submitted an FPCA in any of the previous five elections for federal office, they are included in the sample, regardless of whether they submitted an FPCA in the 2018 general election.[4] I also exclude any individuals who cast a vote in the 2018 general election without submitting an FPCA since they were clearly not UOCAVA eligible and since the availability of mobile voting for UOCAVA voters in their county likely had no effect on their decision to turn out. To be conservative, I drop the two counties that allowed mobile voting in the 2018 primary election since the pilot may have affected FPCA applications in those counties in the primary, although the subsequent results are not meaningfully changed if I include those two counties and/or if I do not use FPCAs from the 2018 primary when constructing the sample.

In total, this sample consists of 1,754 registered voters in West Virginia who were likely living overseas and eligible for UOCAVA status. To be sure, some of these individuals may have no longer been living overseas in November 2018, and there are others who were eligible for UOCAVA status who are excluded from this sample because they either hadn't submitted an FPCA before or they only recently moved overseas. Among the 1.1 million registered voters in West Virginia not in my sample (excluding the two pilot counties), only 293 (0.03%) submitted an FPCA for the 2018 general election. Conversely, among the 1,754 individuals in my sample, 397 (22.6%) did so.

Because my sample consists of people who are likely living overseas, they are clearly not representative of West Virginians in general. Active military members comprise 46 percent of the sample, and their spouses make up another 10 percent. Most notably, the sample skews young and male. Only 38 percent of the sample is female, relative to 49 percent among other registered voters from West Virginia. The average age is 40, compared to an average age of 50 among other registered voters from the same counties in West Virginia. The sample also skews Republican with 42 percent registered Republicans (vs. 33 outside the sample) and 30 percent registered Democrats (vs. 41). Although the estimates from this sample may not generalize to the entire state or country, this is still an interesting population to study since young people tend to be underrepresented at the polls and voting may be more burdensome for those living overseas.

To estimate the effect of having mobile voting as an option, I test whether an outcome of interest—

---

[4] If I restrict my attention to only those individuals who submitted an FPCA in the most recent two, three, or four elections, the subsequent results are virtually unchanged. See Table A1 in the Appendix. If I restrict my attention to only those who submitted a Federal Post Card Application (FPCA) in the most recent election (the 2018 primary), the sample size is too small to draw meaningful conclusions.

either submitting an FPCA or casting a vote—changed differentially in November 2018 for the counties offering mobile voting versus the other counties in West Virginia. Specifically, I regress the outcome of interest in the 2018 general election on an indicator for the availability of mobile voting and controls for the outcome of interest in previous elections. In some specifications, I include controls for party registration, gender, and age. Although my sample includes 1,753 registered voters, the treatment of interest is clustered at the county level, and there could be county-specific shocks to participation, so my standard errors are clustered at the county level.

Because I control for prior patterns of the outcome of interest, and because mobile voting was first introduced in this sample for the 2018 general election and only for some counties, this is implicitly a differences-in-differences design. We are comparing changes in an outcome of interest for voters who did and did not gain the option of mobile voting. When studying turnout, I also run specifications that control for the level of turnout in various elections for voters who are not likely UOCAVA eligible. These specifications are akin to a triple differences design whereby we're implementing differences-in-differences designs separately for those who are and aren't UOCAVA eligible and then testing for a difference between the two. This design accounts for the potential concern that the counties participating in the mobile voting trial were experiencing general upticks in turnout in the 2018 general election, and it arguably makes an even weaker identifying assumption. I also conduct placebo tests, explained below, in order to assess the plausibility of my identifying assumptions.

Estimates of the effect of mobile voting on FPCAs are shown in Table 5. In Column 1, I control for whether each individual submitted an FPCA in each of the five previous elections. In Column 2, I add in controls for party, gender, and age category. In Column 3, I include fixed effects for each possible combination of FPCA history across the five previous elections, effectively matching individuals on their exact histories. And in Column 4, I include fixed effects for each unique combination of FPCA history, party, gender, and age category, effectively conducting exact matching on those covariates.[5] Across all four specifications, the estimated effect of mobile voting on FPCAs is substantively large and statistically significant. Having mobile voting as an option in one's county makes a UOCAVA-eligible voter six to nine percentage points more likely to submit an FPCA. Approximately 14 percent of the individuals in my sample in counties without mobile voting submitted an FPCA for the 2018 general elections, so this estimated effect represents a large increase over that baseline. Apparently, the prospect of being able to vote with a

TABLE 5. EFFECT OF MOBILE VOTING OPTION ON FEDERAL POST CARD APPLICATIONS

	DV = FPCA G2018			
	(1)	*(2)*	*(3)*	*(4)*
Mobile voting available	.088**	.073**	.085**	.063**
	(.023)	(.021)	(.023)	(.023)
FPCA P2018	.740**	.730**		
	(.041)	(.041)		
FPCA G2016	.161**	.151**		
	(.032)	(.035)		
FPCA P2016	.125**	.126**		
	(.018)	(.018)		
FPCA G2014	.128**	.109**		
	(.033)	(.032)		
FPCA P2014	.016	−.005		
	(.030)	(.034)		
Registered Democrat		.041		
		(.021)		
Registered Republican		−.045		
		(.024)		
Female		.018		
		(.017)		
Age 30–39		.053**		
		(.015)		
Age 40–49		.059*		
		(.022)		
Age 50–69		.172**		
		(.025)		
Age ≥70		.111*		
		(.044)		
Constant	−.092**	−.130**		
	(.032)	(.037)		
FPCA history FEs			X	
Exact matching				X
Observations	1,754	1,754	1,754	1,754

County-clustered standard errors in parentheses; $^{**}p<0.01$, $^{*}p<0.05$. P stands for primary elections and G stands for general elections, so, as an example, FPCA G2016 is an indicator for whether an individual submitted an FPCA in the run-up to the 2016 general election. FEs, fixed effects; FPCA, federal post card applications.

---

[5] The only difference between this approach and other forms of exact matching is that this regression implicitly gives the matched groups with a more even mix of treated and untreated individuals more weight. Another approach, for example, might try to put equal weight on all treated individuals for which there is an exact match, although this approach would be less efficient and provide less precise estimates. For more on the connections between regression and matching, see Angrist and Pischke (2009, 69–77) and Angrist and Pischke (2015, 55–68).

mobile app piqued the interests of many and led to a significant increase in FPCAs.

Table 6 shows the estimated effects of mobile voting on turnout. Columns 1–2 and 4–5 are analogous to the specifications shown in Table 5, but with FPCAs replaced by turnout. In Columns 3 and 6, I also include controls for the county turnout rate among those not in the sample in each of the six elections. Not all of the estimates are statistically significant at conventional levels, although the estimated effect of mobile voting on turnout ranges from three to five percentage points. Substantively, this effect is larger than most get-out-the-vote interventions and most other electoral reforms including early voting and vote by mail.

Although the estimated effect of mobile voting on turnout is substantively meaningful, it is notably smaller than the estimated effect on FPCAs. This is consistent with the survey findings that many individuals are wary of online voting. Many people

Table 6. Effect of Mobile Voting Option on Turnout

	DV = Voted G2018					
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Mobile voting available	.050*	.039	.031	.048*	.028	.034
	(.022)	(.020)	(.022)	(.022)	(.018)	(.021)
Voted P2018	.450**	.436**	.430**			
	(.043)	(.049)	(.049)			
Voted G2016	.094**	.078**	.074**			
	(.014)	(.013)	(.013)			
Voted P2016	.073**	.078**	.081**			
	(.022)	(.022)	(.021)			
Voted G2014	.117**	.099**	.097**			
	(.025)	(.025)	(.024)			
Voted P2014	−.051	−.060	−.042			
	(.055)	(.057)	(.056)			
Registered Democrat		.031	.036*			
		(.017)	(.017)			
Registered Republican		−.063**	−.062**			
		(.018)	(.018)			
Female		.004	.003			
		(.015)	(.014)			
Age 30–39		.075**	.076**			
		(.019)	(.018)			
Age 40–49		.096**	.098**			
		(.021)	(.022)			
Age 50–69		.159**	.157**			
		(.031)	(.031)			
Age ≥70		.089	.098			
		(.055)	(.053)			
County turnout G2014			.467			.572
			(.507)			(.491)
County turnout G2016			−.158			.217
			(.479)			(.606)
County turnout G2018			.498			−.151
			(.485)			(.616)
County turnout P2014			−.377			−.336
			(.262)			(.281)
County turnout P2016			−.113			−.183
			(.417)			(.453)
County turnout P2018			−.024			.180
			(.365)			(.401)
Constant	.012	−.023	−.214			
	(.016)	(.021)	(.117)			
Vote history FEs				X		
Exact matching					X	X
Observations	1,754	1,754	1,754	1,754	1,754	1,754

County-clustered standard errors in parentheses; **$p<0.01$, *$p<0.05$.

may have submitted an FPCA because they were curious about mobile voting but later chose not to follow through and cast a vote.

There are 1,148 individuals in the sample from counties that opted into mobile voting. If we suppose the effect of mobile voting on turnout was four percentage points (roughly averaging across estimates from Table 6), this suggests that 46 individuals voted who would have not otherwise voted if mobile voting wasn't an option. In the counties being studied (excluding Harrison and Monongalia), 107 individuals cast a ballot using the mobile app. This suggests that just over half the people using the mobile app would have voted anyway had the app not been available, but almost half the people using the app were induced to vote because of mobile voting.

Tables 7 and 8 show the results of the placebo tests in which I implement the same designs but use participation in previous elections as outcomes of interest. When implementing exact matching in the placebo tests, I only use turnout or FPCA information from previous elections. When studying FPCAs in previous elections, none of the estimates are statistically significant. Some of the estimates are positive but with large standard errors, and in every case, exact matching shrinks the estimate toward zero. None of the placebo estimates are as large as even the smallest estimate in Table 5, suggesting that the estimated effect of mobile voting on FPCA applications is not easily attributable to differential trends of the counties opting into the trial. When studying turnout, however, some of the estimates are large and statistically significant, suggesting that we should be more cautious in interpreting the estimated effects on turnout. Because larger counties were more likely to opt into mobile voting, differential trends in turnout are possible. However, I have attempted to account for these differences as best as possible by matching voters based on their turnout histories, party registration, gender, and ages, and also controlling for general trends in turnout across counties.

Table 9 tests whether the effects of mobile voting vary across different groups within the sample. Because the sample is not representative of all

TABLE 7. PLACEBO TESTS FOR FEDERAL POST CARD APPLICATIONS

	FPCA P2018			*FPCA G2016*			*FPCA P2016*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
Mobile voting	.054	.051	.028	.038	.032	.001	.013	.015	−.013
	(.035)	(.034)	(.024)	(.035)	(.032)	(.021)	(.028)	(.027)	(.027)
FPCA G2016	−.091	−.094							
	(.051)	(.051)							
FPCA P2016	.067*	.067*		.051	.048				
	(.028)	(.030)		(.047)	(.046)				
FPCA G2014	−.020	−.023		−.320**	−.324**		.047	.032	
	(.023)	(.024)		(.064)	(.060)		(.066)	(.065)	
FPCA P2014	.031	.028		−.198*	−.204*		.046	.037	
	(.068)	(.066)		(.098)	(.093)		(.091)	(.090)	
Reg Dem		.009			−.018			.116**	
		(.023)			(.022)			(.034)	
Reg Rep		−.019			−.015			.098**	
		(.021)			(.023)			(.030)	
Female		.009			.038*			−.039*	
		(.012)			(.014)			(.018)	
Age 30–39		.005			.044*			.045	
		(.018)			(.020)			(.031)	
Age 40–49		.011			.083**			.107**	
		(.015)			(.026)			(.032)	
Age 50–69		.017			.083**			.026	
		(.023)			(.026)			(.031)	
Age ≥70		−.004			−.157**			.001	
		(.037)			(.056)			(.051)	
Constant	.113	.113		.882**	.847**		.260**	.158**	
	(.058)	(.059)		(.032)	(.031)		(.029)	(.031)	
Exact matching			X			X			X
Observations	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754

County-clustered standard errors in parentheses; $^{**}p < 0.01$, $^{*}p < 0.05$.

TABLE 8. PLACEBO TESTS FOR TURNOUT

	Voted P2018				*Voted G2016*				*Voted P2016*			
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*	*(11)*	*(12)*
Mobile	.035*	.032*	.038*	.022	.067*	.063*	.056*	.061*	−.005	−.005	−.010	.018
	(.016)	(.015)	(.015)	(.016)	(.026)	(.025)	(.025)	(.028)	(.027)	(.026)	(.027)	(.027)
G2016	.027*	.024*										
	(.011)	(.011)										
P2016	.105**	.103**			.154**	.154**						
	(.019)	(.019)			(.025)	(.023)						
G2014	.036*	.030*			−.026	−.051			.118**	.107**		
	(.015)	(.014)			(.034)	(.035)			(.035)	(.036)		
P2014	.074*	.069*			−.122*	−.105			.243**	.227**		
	(.031)	(.031)			(.058)	(.058)			(.046)	(.047)		
Dem		.016				−.008				.095**		
		(.016)				(.028)				(.028)		
Rep		−.005				.013				.061		
		(.013)				(.032)				(.034)		
Female		.015				.111**				.017		
		(.014)				(.016)				(.013)		
Age 30–39		−.007				.106**				−.083**		
		(.017)				(.028)				(.029)		
Age 40–49		−.007				.177**				−.008		
		(.019)				(.028)				(.033)		
Age 50–69		.035				.152**				−.049		
		(.024)				(.028)				(.037)		
Age ≥70		.022				.117*				.027		
		(.036)				(.047)				(.058)		
Cty P18				.394								
				(.257)								
Cty G16				−.066				.296				
				(.235)				(.371)				
Cty P16				−.805**				−.032				.766*
				(.245)				(.527)				(.372)
Cty G14				.972**				.206				−.495
				(.252)				(.640)				(.489)
Cty P14				−.176				−.298				.092
				(.183)				(.261)				(.226)
Constant	−.009	−.013			.672**	.531**			.192**	.169**		
	(.014)	(.019)			(.023)	(.035)			(.023)	(.036)		
Exact match			X	X			X	X			X	X
Observations	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754

County-clustered standard errors in parentheses; **$p<0.01$, *$p<0.05$.

West Virginians and because we might be interested in assessing how mobile voting affects the representation of certain groups, we'd like to know which kinds of voters are particularly mobilized. On one hand, we might expect that lowering the cost of voting will especially benefit underrepresented groups, and we might think young voters—who are typically underrepresented—would be particularly receptive to mobile voting. On the other hand, we might expect wealthier and more educated people to have smartphones and information-technology skills and be more aware of government reforms (e.g., Alvarez and Nagler 2001).

Because of data limitations, I can only test for certain kinds of variation, and the estimates for certain subgroups are imprecise. Specifically, I test for variation across gender, age groups, party registration, and whether an individual is in the military or a military spouse. The effects of mobile voting on FPCAs and turnout appear to be larger for women, but otherwise, there is little substantively or statistically significant evidence that the effects of mobile voting vary meaningfully across age, party, or military affiliation. If anything, the estimated effects of mobile voting are actually greater among individuals over the age of 50, although these differences are not statistically significant. This is inconsistent with the finding from Table 3 that older individuals are less willing to cast a vote online. The survey evidence might overstate the

Table 9. Tests for Heterogeneous Effects

	FPCA G2018				*Voted G2018*			
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Mobile	.058*	.073**	.091*	.081*	.036	.037	.068*	.043
	(.024)	(.020)	(.035)	(.036)	(.024)	(.022)	(.031)	(.041)
Female	−.002				.003			
	(.019)				(.017)			
Age 30–49		.050**				.087**		
		(.016)				(.017)		
Age ≥50		.150**				.118*		
		(.039)				(.044)		
Democrat			.046				.047	
			(.026)				(.028)	
Republican			−.023				−.019	
			(.025)				(.025)	
Military				−.060*				−.009
				(.027)				(.032)
Spouse				−.040				.051
				(.052)				(.061)
Mobile*Female	.068*				.030			
	(.029)				(.028)			
Mobile*Age 30–49		−.001				−.005		
		(.028)				(.026)		
Mobile*Age ≥50		.028				.062		
		(.050)				(.052)		
Mobile*Democrat			.009				−.008	
			(.040)				(.037)	
Mobile*Republican			−.034				−.057	
			(.041)				(.034)	
Mobile*Military				−.011				.014
				(.032)				(.038)
Mobile*Spouse				.029				−.010
				(.067)				(.075)
FPCA history FEs	X	X	X	X				
Vote history FEs					X	X	X	X
Observations	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754

County-clustered standard errors in parentheses; $^{**}p < 0.01$, $^{*}p < 0.05$; FE, fixed effects.

skepticism of older voters, or conversely, it might understate the willingness of young people to embrace mobile voting. In any case, the positive effects of mobile voting do not appear to be driven by a sample that overrepresents men, young people, and military affiliates. The results also suggest that mobile voting may not be as effective in correcting the underrepresentation of young voters as one might have expected.

CONCLUSION

As discussed throughout, there are many limitations that should prevent one from drawing overly strong conclusions from this single study. For the survey results, we know that voters often misreport whether they voted in the past, let alone whether they will vote in the future or would vote under various hypothetical scenarios. Many survey respondents may now know whether they would vote under different scenarios, and even if they do, they may not answer the questions honestly. For the observational analysis of mobile voting in West Virginia, we might worry that the uniqueness of the setting limits the generalizability of the results. For instance, the novelty of having the opportunity to use mobile voting for the first time may have induced participation, but it may not continue in future elections. In fact, novelty effects—whereby turnout increases initially but then reverts toward typical levels in subsequent elections—have been found for other electoral reforms (e.g., Gerber, Huber, and Hill 2013; Gronke, Galanes-Rosenbaum, and Miller 2007) and even for internet voting (Germann and Serdült 2017). Furthermore, overseas voters may be highly unrepresentative of the general population of eligible voters in the

U.S. Perhaps military families are especially civic minded or especially trusting of government. Perhaps mobile voting is especially effective for those who cannot go to the polls and who are less exposed to social pressure to vote.

Furthermore, we cannot cleanly identify from public records who was and wasn't eligible for mobile voting, and we might worry that the counties that adopted mobile voting had overseas residents who were increasingly interested in voting. Despite these limitations, I have attempted to provide the best available evidence on the effects of mobile voting in the U.S., and I have attempted to study both the promises and potential perils of mobile voting in a thorough way.

Although many are understandably wary of online and mobile voting, the results of this study suggest that when people have the opportunity to cast a vote online, many of them take it up, and a meaningful share of eligible voters are induced to vote who would not have otherwise cast a ballot. Although West Virginia's trial was small, only affecting overseas residents from some counties, and requiring individuals to first submit a Federal Post Card Application before utilizing mobile voting, the results suggest that mobile voting may be more effective in increasing turnout than many other electoral reforms. Furthermore, if mobile voting could be implemented and advertised on a larger scale without first requiring individuals to submit an FPCA, the effects could be even greater. At the same time, mobile voting raises new security risks that should be closely considered before it is further adopted. In fact, a recent study of the very system used in West Virginia suggests that the public's concerns with online voting are justified (Specter, Koppel, and Weitzner 2020). As more states and localities experiment with mobile voting, researchers should pay close attention to its promises and perils.

REFERENCES

"24 Counties to Offer Mobile Voting Option for Military Personnel Overseas." 2018. *Secretary of State Mac Warner*, September 20. <https://sos.wv.gov/news/Pages/09-20-2018-A.aspx>.

Alvarez, R. Michael and Jonathan Nagler. 2001. "The Likely Consequences of Internet Voting for Political Representation." *Loyola of Los Angeles Law Review* 34(3): 1115–1153.

Angrist, Joshua D. and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics*. Princeton, NJ: Princeton University Press.

Angrist, Joshua D. and Jörn-Steffen Pischke. 2015. *Matering 'Metrics*. Princeton, NJ: Princeton University Press.

Bechtel, Michael M., Dominik Hangartner, and Lukas Schmid. 2015. "Does Compulsory Voting Increase Support for Leftist Policy?" *American Journal of Political Science* 60(3): 752–767.

Berinsky, Adam J. 2005. "The Perverse Consequences of Electoral Reform in the United States." *American Politics Research* 33(4): 471–491.

Blaze, Matt, Jake Braun, Harri Hursti, David Jefferson, Margaret MacAlpine, and Jeff Moss. 2018. *Report on Cyber Vulnerabilities in U.S. Election Equipment, Databases, and Infrastructure*. Report of the DEF CON 26 Voting Village <bit.ly/2Fj3NUq>.

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.

Dennis, J. Michael. 2019. *Technical Overview of the Amerispeak® Panel, NORC'S Probability-Based Household Panel*. <bit.ly/2Ks32gf>.

Enos, Ryan D., Anthony Fowler, and Lynn Vavreck. 2014. "Increasing Inequality: The Effect of GOTV Mobilization on the Composition of the Electorate." *Journal of Politics* 76(1): 273–288.

Fowler, Anthony. 2013. "Electoral and Policy Consequences of Voter Turnout: Evidence from Compulsory Voting in Australia." *Quarterly Journal of Political Science* 8(2): 159–182.

Fowler, Anthony. 2015. "Regular Voters, Marginal Voters, and the Electoral Effects of Turnout." *Political Science Research and Methods* 3(2): 205–219.

Fowler, Anthony. 2017. "Does Voter Preregistration Increase Youth Participation?" *Election Law Journal* 16(4): 485–494.

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.

Germann, Micha and Uwe Serdült. 2017. "Internet Voting and Turnout: Evidence from Switzerland." *Electoral Studies* 47: 1–12.

Goodman, Nicole and Leah C. Stokes. 2018. "Reducing the Cost of Voting: An Evaluation of Internet Voting's Effect on Turnout." *British Journal of Political Science*. Advance online publication. <https://doi.org/10.1017/S0007123417000849>.

Green, Donald P. and Alan S. Gerber. 2015. *Get Out the Vote: How to Increase Voter Turnout*, 3rd edition. Washington, DC: Brookings Institution Press.

Gronke, Paul, Eva Galanes-Rosenbaum, and Peter A. Miller. 2007. "Early Voting and Turnout." *PS: Political Science & Politics* 40(4): 639–645.

Kousser, Thad and Megan Mullin. 2007. "Does Voting by Mail Increase Participation? Using Matching to Analyze a Natural Experiment." *Political Analysis* 15(4): 428–445.

Lijphart, Arend. 1997. “Unequal Participation: Democracy’s Unresolved Dilemma.” *American Political Science Review* 91(1): 1–14.

Schryen, Guido and Eliot Rich. 2009. “Security in Large-Scale Internet Elections: A Retrospective Analysis of Elections in Estonia, The Netherlands, and Switzerland.” *IEEE Transactions of Information Forensics and Security* 4(4): 729–744.

Specter, Michael A., James Koppel, and Daniel Weitzner. 2020. “The Ballot Is Busted Before the Blockchain: A Security Analysis of Voatz, the First Internet Voting Application Used in U.S. Federal Elections.” MIT Internet Policy Research Initiative working paper.

Springhall, Drew, Travis Finkenauer, Zakir Durumeric, Jason Kitcat, Harri Hursti, Margaret MacApline, and J. Alex Halderman. 2014. “Security Analysis of the Estonian Internet Voting System.” *Proceedings of the 21st ACM Conference on Computer Communications Security*.

Verba, Sidney, Kay Lehman Schlozman, and Henry E. Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics*. Harvard University Press.

“Warner Pleased with Participation in Test Pilot for Mobile Voting.” 2018. *Secretary of State Mac Warner*, November 16. <sos.wv.gov/news/Pages/11-16-2018-A.aspx>.

Address correspondence to:
Anthony Fowler
Harris School of Public Policy
1307 East 60th Street, #2031
University of Chicago
Chicago, IL 60637
USA

E-mail: anthony.fowler@uchicago.edu

Received for publication July 17, 2019; received in revised form February 17, 2020; accepted February 21, 2020; published online June 17, 2020.

APPENDIX TABLE A1. ROBUSTNESS TO NUMBER OF PREVIOUS ELECTIONS USED TO DETERMINE OVERSEAS STATUS

	DV = FPCA G2018				
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Mobile voting	.063**	.061**	.066**	.065**	.080
	(.023)	(.023)	(.023)	(.024)	(.207)
	DV = Voted G2018				
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Mobile voting	.028	.028	.029	.026	.086
	(.018)	(.018)	(.020)	(.020)	(.287)
Exact matching	X	X	X	X	X
# Recent elections	5	4	3	2	1
Observations	1,754	1,725	1,591	1,528	157

County-clustered standard errors in parentheses; $^{**}p<0.01$, $^{*}p<0.05$. The table shows that the estimates of the effect of mobile voting on Federal Post Card Application (FPCA) requests or turnout are largely unchanged if instead of focusing on those who submitted an FPCA application in one of the previous five elections, we focus only on individuals who submitted an FPCA in the last four, three, or two elections. If we only use the most recent election (the 2018 primary), our sample size shrinks dramatically and our standard errors accordingly increase, although if anything, the estimated effects increase as we restrict our sample to those who submitted an FPCA in more recent elections.