The Impact of Medicaid Expansion on Voter Participation: Evidence from the Oregon Health Insurance Experiment*

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Abstract: In 2008, a group of uninsured low-income adults in Oregon was selected by lottery for the chance to apply for Medicaid. Using this randomized design and state administrative data on voter behavior, we analyze how a Medicaid expansion affected voter turnout and registration. We find that Medicaid increased voter turnout in the November 2008 Presidential election by about 7 percent overall, with the effects concentrated in men (18 percent increase) and in residents of democratic counties (10 percent increase); there is suggestive evidence that the increase in voting reflected new voter registrations, rather than increased turnout among pre-existing registrants. There is no evidence of an increase in voter turnout in subsequent elections, up to and including the November 2010 midterm election.

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1. Introduction

The interaction between government policy and political participation is central to questions in political science, political economy, and public economics. We focus on the impact on voter participation of a specific, means-tested government policy: Medicaid. Medicaid is operated as a partnership between the state and federal governments to provide public health insurance to low income individuals. It is the largest means-tested program in the US; at over $550 billion in expenditures in 2016, it dwarfs the next largest means-tested programs (food stamps (SNAP) and the Earned Income Tax Credit, each of which were only about $70 billion in 2016).[1]

We examine how expanding Medicaid to previously uninsured low-income adults affected voter turnout and registration. Credibly identifying the causal impact of a policy on political behavior is challenging (Campbell, 2012). The expansion of Medicaid is no exception: Medicaid recipients differ from the uninsured in many ways – such as, for example, socio-economic status and health – that may directly affect voter participation. The confounding factors make it difficult to make inferences about the causal impact of Medicaid from observational comparisons of voting behavior of Medicaid enrollees compared to "similar" uninsured individuals.

In 2008, Oregon used a lottery to allocate a limited number of slots in its Medicaid program, Oregon Health Plan (OHP) Standard, for low-income, previously uninsured adults (ages 19-64). The state drew names at random from a waiting list of approximately 90,000 for 10,000 available slots. Those selected were able to apply for OHP Standard and, if found eligible, to enroll. Oregon's use of a lottery offers the opportunity to assess the effect of Medicaid coverage using a randomized evaluation design that is not contaminated by confounding factors.

Prior work on the Oregon Health Insurance Experiment used the lottery as an instrument for Medicaid coverage and examined the impact of Medicaid on health care use, health outcomes, and financial outcomes over the first two years (Finkelstein et al., 2012, Baicker et al., 2013; Baicker et al., 2014; Taubman et al., 2014; Finkelstein et al., 2016). It found that Medicaid increased health care use across a wide range of settings – including hospital admissions, emergency room visits, prescription drugs, primary care, and preventive care; where it was possible to analyze time patterns, these effects appear persistent over the first two years of coverage. The prior work on the Oregon Experment also found that Medicaid improved financial security – reducing unpaid medical bills and out of pocket medical spending and virtually eliminating the risk of catastrophic out of pocket medical spending – but had no impact on employment or earnings. Finally, it found that Medicaid reduced depression and improved self-reported health, but had no detectable impacts on several measures of physical health.

[1] See Center for Medicare & Medicaid Services (2018), Department of Agriculture (2017), and Internal Revenue Service (2016).

We now use the random selection in the lottery to study, for the first time, impacts on voter participation. To do so, we link administrative data on lottery participants, whether they won the lottery, and their Medicaid enrollment, to Oregon's statewide voter lists. These allow us to analyze voter turnout in elections from November 2006 through November 2010, and voter registration as of June 2010. Prior to looking at the data on outcomes for the treatment group, all of the analyses presented in the main text were pre-specified and publicly archived in a detailed analysis plan.[2]

We analyze the impact of Medicaid on voter participation, using random selection by the lottery (which occurred from March through September 2008) as an instrument for Medicaid coverage. The results indicate that Medicaid increased voter turnout in the November 2008 presidential election. Overall, Medicaid increased voter turnout by 2.5 percentage points (standard error = 0.014), or about 7 percent relative to the 34 percent mean turnout in the control group. The impact was particularly pronounced for men, for whom Medicaid increased turnout by 5.4 percentage points (standard error = 1.86), or about 18 percent, and for residents in democratic counties, for whom Medicaid increased turnout by 3.6 percentage points (standard error = 1.8), or about 10 percent.

We also find some evidence that Medicaid increased voter registration, although the results were statistically insignificant. Intriguingly, however, the statistically insignificant increases in registration were roughly the same magnitude as the increase in turnout, and similarly concentrated among males and among residents of democratic counties. We interpret this as suggestive evidence that the increase in voter turnout may reflect the behavior of newly-registered voters. We find no evidence of increased turnout in subsequent state-wide and local elections after November 2008 and before November 2010, or in the statewide general election in November 2010. We are unable to study the impacts of the 2008 lottery on voting beyond the 2010 elections because after that point, individuals who had been in the control group were given the opportunity to sign up for a new lottery, effectively ending the experiment.

We note two important limitations or nuances to our findings. First, our results do not necessarily generalize to other actual or potential Medicaid expansions. As discussed in more detail in Finkelstein et al. (2012), our results should be interpreted in light of the particular characteristics of the study population and the specific nature and timing of the Oregon Medicaid expansion.

Second, following prior work we use lottery selection as an instrument for Medicaid coverage. These instrumental variable (IV) estimates are based on the exclusion restriction that the only channel through which winning the lottery affected voting was via its impact on receiving Medicaid coverage. However, the exclusion restriction is potentially violated when analyzing voting behavior. It is possible, for example, that "winning" something from the government affects voting behavior directly (see e.g. Mettler 2005). In addition, applying for Medicaid could

[2] The analysis plan, posted in September 2015, is available at http://www.nber.org/oregon/documents/analysis-plan/analysis-plan-voting-2015-09-23.pdf

increase voting – whether or not the applicant ultimately received Medicaid – since, under the 1993 National Voter Registration Act, public assistance offices are, in principle (although compliance varies), required to offer clients voter registration forms and assistance (Michener 2016). In this case, the results should be interpreted as the impact of expanding Medicaid *eligibility*, rather than the impact of expanding Medicaid *coverage*. We therefore also present intent-to-treat estimates of the impact of winning the lottery (i.e. the ability to apply for Medicaid); by construction, these estimates are one-fourth the size of the IV estimates of the impact of Medicaid coverage (since winning the lottery increased the probability of Medicaid by about one-fourth). Arguably, the impact of expanded eligibility is as, or more, interesting than the impact of coverage per se, since eligibility, unlike receipt, is a policy lever that the government can directly control (see e.g. Gruber 1997).

The rest of paper proceeds as follows. Section two briefly reviews potential channels by which Medicaid may affect political participation and some of the existing evidence. Section three describes the intervention and data, section four presents the empirical framework, and section five presents the results. A final section concludes.

2. Medicaid and Political Participation: Potential Channels and Existing Evidence

A priori, the sign as well as the magnitude of any impacts of means-tested programs on political participation are ambiguous, and may vary depending on their specific design. Broadly speaking, the literature focuses on two potential mechanisms for policy feedback – i.e. ways in which today's policies can influence future political participation. These are: "resource effects" – redistributing resources in a way that can affect political behavior – and "interpretive effects" – changing the ways individuals perceive political institutions as well as their relationship with those institutions (Pierson 1993; Campbell 2012).

It has been well-documented that wealthy people are more likely to vote than the poor (Highton and Wolfinger 2001; Leighley and Nagler 2014), although the causal impact of resources on voting – as opposed to other correlates such as education or church involvement – is unclear (Wolfinger and Rosenstone 1980; Brady et al. 1995; Mettler and Stonecash 2008). Evidence from the Oregon experiment (Finkelstein et al., 2012; Baicker et al., 2013) – as well as quasi-experimental studies of recent Medicaid expansions (Hu et al., 2016, Mazumder and Miller 216) indicates that Medicaid lowers out-of-pocket medical spending and medical debt. Thus Medicaid may increase political participation by increasing economic resources.

Another potential resource effect may be via an impact of Medicaid on improving recipients' health. Poor health may discourage political participation by directing attention to personal matters and away from political ones, and by inhibiting the cognitive abilities and civic skills required for participation (Pacheco and Fletcher 2015; Blais 2000). Evidence from the Oregon experiment on health impacts is mixed, suggesting that Medicaid improved self-reported health and reduced depression, but has no detectable impact on measures of physical health (Finkelstein

et al., 2012; Baicker et al., 2013; Baicker and Finkelstein 2013). Reductions in depression may potentially increase individuals' sense of political efficacy, and hence their political participation (Ojeda 2015).

Medicaid may also have interpretative effects, but the sign of such interpretative effects is a priori ambiguous. Universal programs may convey messages of inclusion and empowerment, which then create positive psychological effects and encouraging political participation (Wilson 1987; Skocpol 1991). Means-tested programs such as Medicaid, however, may undermine political participation by conveying negative lessons about the quality and nature of government as well as encouraging feelings of powerlessness (Schneider and Ingram 1993; Soss 1999; Soss 2002; Campbell 2012). On the other hand, the interpretative effects of means tested programs could be positive: endorsement of welfare programs by major parties and politicians could dampen or even reserve the negative stigmatizing effects (Clinton and Sances 2018), as could expanded Medicaid eligibility.

Empirical evidence from randomized trials and regression discontinuity designs in developing countries suggests that means-tested government benefits increase voter turnout (e.g. De La O, 2013, Manacorda et al., 2011, Labonne, 2013, Pop-Eleches and Pop-Eleches, 2012). However, in the US, where there are relatively fewer causal estimates of the impact of means-tested benefit receipt on voting, the existing studies point to *decreases* in turnout. Gay (2012) looks at the impact of the Moving to Opportunity randomized experiment of housing mobility on voting, finding that receipt of public housing assistance reduced voter turnout; she provides some suggestive evidence that this was due to the disruption of social networks associated with moves to better neighborhoods. Dave et al. (2016) use the quasi-experimental variation in the timing of different state welfare reforms in the early 1990s, and find that welfare reform – which aimed to reduce dependence on cash welfare – decreased voter turnout among low-income women, with effects confined to presidential elections.

In our specific context of Medicaid, several prior papers have tried to examine the impact of Medicaid on voter participation (e.g. Michener 2017), but the challenges to identifying causal effects have been substantial. To try to surmount these challenges, two recent papers have analyzed the relationship between geographic changes in Medicaid coverage arising from the 2014 Medicaid expansions under the ACA and geographic changes in voter participation. Haselswerdt (2017) estimates that increases in Medicaid enrollment increased voter turnout in the 2014 Congressional elections, while Clinton and Sances (2018) estimate that Medicaid increased voter registration in 2014 (with effects persisting through 2016), and increased voter turnout in 2014 but not in 2016.

3. Intervention and Data

3.1 Randomization and Intervention.

Oregon opened a waiting list for a previously closed Medicaid program in early 2008 and then conducted eight lottery drawings from the waiting list between March and September 2008. Selected individuals won the opportunity – for themselves and any household member – to apply for health insurance benefits through Oregon Health Plan Standard (OHP Standard). OHP Standard provides benefits to low-income adults who are not categorically eligible for Oregon's traditional Medicaid program. To be eligible, individuals must be: ages 19-64; not otherwise eligible for Medicaid or other public insurance; Oregon residents; U.S. citizens or legal immigrants; without health insurance for six months; with income below the federal poverty level and assets below $2,000. Among the randomly selected individuals, those who completed the application process and met these eligibility criteria were enrolled in OHP Standard. OHP Standard provides relatively comprehensive medical benefits (including prescription drug coverage) with no consumer cost sharing and low monthly premiums (between $0 and $20, based on income), provided mostly through managed care organizations. The lottery process and OHP Standard have been described in more detail elsewhere (Finkelstein et al., 2012).

Starting in the fall of 2009, the state conducted a new lottery for OHP Standard. As part of this new lottery, the state mailed postcards to those on the original list that were not selected (our controls) asking if they would like to be included in this second lottery. Those who returned the postcard were added to the new waiting list and an initial draw was done just from that group. By the end of 2010, all of the controls had been given the opportunity to sign up for this new lottery. Our analysis therefore does not extend beyond 2010.

3.2 Data sources

Lottery list and Medicaid enrollment

The state provided us with a list of everyone who signed up for the lottery – including their name and basic self-reported demographics – and whether and when they were selected. The lottery list provides the basic demographics that we use in our heterogeneity analysis, including gender, age, whether the primary language is English, and whether the zip code of residence is in a democratic county, defined as a county in which the majority voted for Obama in 2008; we refer to these as "lottery list variables". The state also provided detailed data on Medicaid enrollment for every individual on the list. We use this to construct our primary measure of insurance coverage during the study period. Both data sets are analyzed and described in detail elsewhere (see Finkelstein et al., 2012).

Voting Data

The novel data used in this analysis are the voting data. Statewide voter lists are maintained by Oregon's Office of the Secretary of State, Elections. The data contain individual-level information on whether the individual is registered to vote as of the data's date (and if so the current political party registration, if any), and whether the individual voted in various prior

elections. Elections in the data include both statewide elections (such as the November 2008 general election) and local elections in which certain districts vote on particular measures or elect local politicians (e.g. school board members).

The data also contain the full name, date of birth, and gender of each individual on the list, which we used to probabilistically match to the lottery study population, using standard techniques used previously with this population (e.g. Finkelstein et al., 2012; Baicker et al., 2014; Taubman et al., 2014; Finkelstein et al., 2016). Those on the lottery list who did not appear in the voter registration records were assumed not registered (as well as have not voted).

We use two main data sets in our analysis: one that we obtained in June 2010 (hereafter "2010 data") and one that we obtained in July 2013 (hereafter "2013 data"). There are two main differences between the two data pulls. First, they provide information on current registration as of different dates. Second, the 2010 data contain voting information on elections from May 2008 through May 2010 while the 2013 file contain voting data on elections from May 2006 through May 2012, but omit some smaller local elections included in the 2010 data. As a result, we are able to control for pre-lottery voting behavior using the 2013 file, but not the 2010 file. We supplement both files with a "cancelled voter" file that we obtained in June 2015 and used to replace a small number of missing voting records for registered individuals in the two main files. Appendix A provides more detail on the data construction.

4. Analytic Framework

Our analytical framework follows the standard approach we have used in our prior analyses of the Oregon Lottery. We briefly summarize it here.

4.1 Intent-to-Treat Effect of the Lottery (ITT)

Our treatment group is comprised of those selected in the lottery and our controls are those who were not. We estimate the intent-to-treat (ITT) effect of winning the lottery (i.e. the difference between treatment and controls) by fitting the following OLS equation:

$$y_{ih} = \beta_0 + \beta_1 LOTTERY_h + X_{ih}\beta_2 + V_{ih}\beta_3 + \varepsilon_{ih} \quad (1)$$

where *i* denotes an individual and *h* denotes a household.

LOTTERY is an indicator variable for whether or not household *h* was selected by the lottery. The coefficient on *LOTTERY* (β_1) is the main coefficient of interest, and gives the average difference in (adjusted) means between the treatment group (the lottery winners) and the control group (those not selected by the lottery); it is interpreted as the impact of being able to apply for OHP Standard through the Oregon lottery.

We denote by X_{ih} the set of covariates that are correlated with treatment probability (and potentially with the outcome) and therefore must be controlled for so that estimates of β_1 give an unbiased estimate of the relationship between winning the lottery and the outcome. In all of our analyses, X_{ih} includes indicator variables for the number of individuals in the household listed on the lottery sign-up form (hereafter "household size"); although the state randomly sampled from individuals on the list, the entire household of any selected individual was considered selected and eligible to apply for insurance. As a result, selected (treatment) individuals are disproportionately drawn from households of larger household size. In all of our analyses we cluster the standard errors on the household identifier since the treatment is at the household level.

We denote by V_{ih} a second set of covariates that can be included to potentially improve power by accounting for chance differences between treatment and control groups in variables that may be important determinants of outcomes. These covariates are not needed for β_1 to give an unbiased estimate of the relationship between winning the lottery and the outcome, as they are not related to treatment status, but may improve the precision of the estimates by explaining some of the variance in the outcome. Our primary analysis includes no such V_{ih} covariates, but we also show that our results are robust to including pre-lottery voting behavior as an additional, V_{ih} covariate.

4.2 Local Average Treatment Effect of Medicaid (LATE)

The intent-to-treat estimates from equation (1) provide an estimate of the causal effect of winning the lottery (i.e. winning the opportunity to apply for OHP Standard). This provides an estimate of the net impact of expanding eligibility for public health insurance. We are also interested in the impact of insurance *coverage* itself. We model this as follows:

$$y_{ih} = \pi_0 + \pi_1 INSURANCE_{ih} + X_{ih}\pi_2 + V_{ih}\pi_3 + \nu_{ih} \qquad (2)$$

where INSURANCE is a measure of insurance coverage and all other variables are as defined in equation (1). Specifically, we define INSURANCE as an indicator variable for Medicaid coverage at any point from the first lottery notification through the latest outcome analyzed. In previous work we found that the lottery had no impact on non-Medicaid sources of insurance coverage, such as private insurance (Finkelstein et al., 2012). In prior work, we found a first stage (i.e. impact of winning the lottery on probability of being enrolled in Medicaid during the one- or two-year study period) of about 0.25. This is primarily due to incomplete takeup among lottery winners: only about 60 percent of those who won sent back applications, and about half of those who sent back applications were deemed ineligible, primarily due to failure to meet the income eligibility threshold (Finkelstein et al., 2012).

We estimate equation (2) by two stage least squares (2SLS), using the following first stage equation:

$$INSURANCE_{ih} = \delta_0 + \delta_1 LOTTERY_h + X_{ih}\delta_2 + V_{ih}\delta_3 + \mu_{ih} \quad (3)$$

in which the excluded instrument is the variable *LOTTERY*. We interpret the coefficient on insurance from instrumental variable estimation of equation (2) as the local average treatment effect of insurance, or LATE (Imbens and Angrist, 1994). In other words, our estimate of π_1 identifies the causal impact of insurance among the subset of individuals who obtain insurance upon winning the lottery but who would not obtain insurance without winning the lottery (i.e. the compliers).

The LATE interpretation requires the additional identifying assumption that the only mechanism through which winning the lottery affected the outcomes studied was the lottery's impact on insurance coverage. As discussed in the introduction, this exclusion restriction may well be violated.

4.3 Analytic Weights

Our analysis takes place at several different points in time: November 2008 voting, June 2010 registration, voting through June 2010 (excluding November 2008), and November 2010 voting. As in previous work (Baicker et al., 2013), for analyses of outcomes in Fall 2009 or later, we use weights to adjust for a new lottery for OHP Standard which the state conducted beginning in the fall of 2009; Appendix B describes these weights in more detail.

4.4 Balance

A central question in any analysis of a state-conducted randomization is whether the state actually randomized as they described. We have explored this extensively in prior work (see especially Finkelstein et al. 2012) and showed both via simulations and pre-lottery balance tests that the state appears to have randomized as indicated.[3]

5. Results: Voting and Registration

Table 1 shows the results for voter turnout in the November 2008 general election, overall and by pre-specified categories. This and all subsequent tables follow the same format: we present the control mean in column 2, the impact of lottery selection on the outcome (i.e. intent-to-treat analysis) in column 3, the first stage impact of lottery selection on Medicaid coverage in column 4, the impact of Medicaid coverage on the outcome (i.e. IV analysis) in column 5, and the p-value (which is the same for the intent-to-treat and IV analyses) in column 6. We focus our discussion on the impact of Medicaid (column 5).

[3] For completeness, Appendix Table A1 shows treatment and control balance on pre-randomization demographic characteristics ("lottery list variables") as well as whether the individual voted prior to the lottery (i.e. in a 2006 or 2007 election), and confirms that, as expected, these characteristics are similar between the treatment and control prior to the lottery.

Panel A shows the results for voter turnout in November 2008, using three different measures of voting: as measured in the 2010 data, as measured in the 2013 data, and as measured in the 2013 data, controlling for whether the individual voted in any pre-lottery election (i.e. in 2006 or 2007). The results indicate that Medicaid increased the probability of voting, although the results range from statistically significant, to marginally significant, to marginally insignificant depending on the specification. The point estimates indicate that Medicaid increased the probability of voting in the November 2008 general election by 2.1 to 2.5 percentage points, depending on the specification. This represents a 6 to 7 percent increase off the 33-34 percent voting rate among the controls.

Panel B looks at results separately by pre-specified cuts of the data. The most striking pattern is by gender. In the control group, voting rates are somewhat higher among women (37 percent) than men (30 percent), but the impact of Medicaid on voting appears to be entirely concentrated among men. Medicaid increased the probability of voting for women by a statistically insignificant 0.4 percentage points (p-value = 0.83). By contrast, Medicaid increased the probability of voting for men by a statistically significant 5.4 percentage points (p-value = 0.004); this represents an 18 percent increase in the probability of men voting relative to the control group. There is also some evidence of larger effects in democratic counties – i.e. counties where the majority voted for Barack Obama in the 2008 presidential election. In such counties, Medicaid increased the probability of voting by 3.6 percentage points (p-value = 0.048) or about 10 percent; in non-Democratic counties, Medicaid increased the probability of voting by a statistically insignificant 0.8 percentage points (p-value = 0.74) or about 2 percent. Appendix Table A2 shows these results are robust to measuring voting in the 2013 data instead of in the 2010 data, and also to controlling for whether the individual voted in a pre-lottery election in 2006 or 2007.

Table 2 looks at the impact of Medicaid on voter registration, which we measure as of June 2010 in the 2010 data. None of the results are statistically significant but the patterns are of interest, especially when viewed alongside the turnout results from Table 1. In particular, the groups that experienced larger increases in voter turnout – men and individuals in democratic counties – also experienced larger increases in the probability of being registered to vote. The magnitudes are also roughly similar. For example, we estimate that Medicaid increased the probability of being registered in a democratic county by 3.7 percentage points and the probability of voting in a democratic county by 3.6 percentage points. Likewise, we estimate that Medicaid increased the probability of a man voting in the November 2008 election by 5.4 percentage points, and of a man being registered to vote by 3.6 percentage points. We interpret this as suggestive evidence that the increase in voting may primarily reflect new voter registrations, rather than increased turnout among pre-existing registrants.

Table 3 looks at voter turnout in other post-lottery elections. Panel A analyzes voter turnout in the statewide November 2010 election. Once again, we present three different specifications (as in Table 1) with broadly similar results. Turnout is lower in the 2010 midterm election (about

one-quarter rather than one-third for the 2008 election). There is no evidence that Medicaid increased turnout in the November 2010 election; indeed, the point estimates are suggestive of a possible decline in voting. In Panel B we examine voting in any post-lottery election through June 2010 except for the November 2008 election; these consist of local elections, primaries, or state-wide special elections that ran from late May 2008 through June 2010 (see Table A3 for details). Once again there is no evidence of an increase in voter turnout due to Medicaid. The point estimates are suggestive of a statistically insignificant 2.4 percentage point (over 10 percent) increase in voting (p-value = 0.11).

Overall the results in Table 3 suggest that the impact of Medicaid on voting in the November 2008 election does not persist over subsequent elections. This finding admits two – very different – possible interpretations. One is that the impact of Medicaid on voting is confined to presidential elections, or other high turnout elections. For our study population, voter turnout is at least 50 percent higher in the November 2008 election than in either of the other two elections we studied; Medicaid may only affect the marginal voter in such high-turnout elections. Another possibility is that the impacts of Medicaid coverage dissipate over time. Given the complementary findings from Clinton and Sances (2018) that the 2014 ACA Medicaid expansion increased voter turnout in the 2014 election but not the 2016 (presidential) election, we read the evidence as overall suggestive of an immediate, but temporary impact of Medicaid.

6. Conclusion

Decisions to expand or contract public benefit programs in general and public health insurance plans in particular are often politically fraught, not least because of the implications for resource allocation across groups. These issues are compounded by the differential voting patterns of affected groups, and further complicated by the effect that program coverage might have on voting behavior.

Despite the first-order importance of these questions, evidence of the causal effect of public benefit programs on voter behavior in the US is limited. This paper examines the impact of a major public program in the US, Medicaid, on voter registration and voter turnout. To do so, we take advantage of a 2008 policy in Oregon that randomly assigned access to Medicaid to assess the causal effect of Medicaid on turnout and registration. We find significant impacts on voter turnout – particularly among men and in Democratic counties – that show up immediately after the Medicaid expansion but do not persist two years later.

Our finding of a temporary impact of the 2008 Oregon Medicaid expansion on voter turnout is similar to what Clinton and Sances (2018) found in the context of the 2014 ACA Medicaid expansions. This is striking since the political discourse surrounding the two Medicaid expansions was very different. The Oregon Medicaid expansion stemmed from a state agency's decision about a fair way to allocate a limited number of available slots, and was not particularly

partisan or politicized. By contrast, the ACA Medicaid expansions were highly politicized and partisan.

A "fade out" effect on voter turnout in these two very different climates suggests that the temporary nature of Medicaid's effect on turnout may be a more general result. This in turn raises an intriguing puzzle, as neither resource effects nor interpretive effects are obviously transitory in nature. Moreover, evidence that voting is habit forming (e.g. Plutzer 2002, Gerber et al. 2003) would further suggest a permanent impact of Medicaid on voter turnout. The finding from these two different studies of a temporary impact of Medicaid on voting presents a critical puzzle for future work. More broadly, our results contribute important insights about the relationship between the social safety net and democratic governance.

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Table 1: November 2008 Voter Turnout

	N	Control Group Mean	Effect of Lottery Selection	First Stage	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall						
Measured in 2010 data	74922	33.8	0.69	0.27	2.5	0.073
			(0.38)	(0.003)	(1.4)	
Measured in 2013 data	74922	32.8	0.58	0.27	2.1	0.130
			(0.38)	(0.003)	(1.4)	
Measured in 2013 data, controlling for pre-period voting	74922	32.8	0.61	0.27	2.2	0.046
			(0.30)	(0.003)	(1.1)	
Panel B: By Category						
Gender						
Female	41249	37.2	0.11	0.26	0.4	0.831
			(0.50)	(0.004)	(1.9)	
Male	33673	29.6	1.51	0.28	5.4	0.004
			(0.52)	(0.004)	(1.9)	
Age						
Ages 19-49	54814	30.4	0.56	0.26	2.1	0.197
			(0.44)	(0.004)	(1.7)	
Ages 50-64	20108	43.1	1.12	0.29	3.8	0.142
			(0.76)	(0.01)	(2.6)	
English-language lottery materials						
No	6440	7.0	0.30	0.19	1.6	0.674
			(0.71)	(0.01)	(3.7)	
Yes	68482	36.1	0.64	0.28	2.3	0.113
			(0.41)	(0.003)	(1.5)	
Zip in a Democratic county (2008)						
No	26723	32.4	0.22	0.28	0.8	0.733
			(0.65)	(0.01)	(2.3)	
Yes	48199	34.6	0.95	0.27	3.6	0.048
			(0.48)	(0.004)	(1.8)	

Notes: The first stage variable is an indicator for Medicaid coverage at any point from the first lottery notification through the November 2008 election. In Panel A, different rows use different data pulls (as indicated) and the third row additionally includes an indicator variable for whether the individual voted in a pre-lottery election (defined as having voted in at least one of the 2006 or 2007 elections shown in Table A3). Results in Panel B all use the 2010 data pull. Column (3) shows the intent-to-treat estimates from equation (1); column (4) shows the first stage estimates from equation (3); column (5) shows the IV estimates of the impact of Medicaid coverage using the lottery as an instrument for Medicaid from equation (2). All analyses are unweighted, include controls for household size, and adjust the standard errors for household clusters.

Table 2: Registered to Vote (as of June 22, 2010)

	N	Control Group Mean	Effect of Lottery Selection	First Stage	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall and by Party Affiliation						
Overall	74922	42.0	0.51	0.24	2.1	0.228
			(0.42)	(0.004)	(1.8)	
Registered as a Democrat	74922	19.2	0.22	0.24	0.9	0.499
			(0.33)	(0.004)	(1.4)	
Registered as a Republican	74922	7.8	0.11	0.24	0.5	0.635
			(0.24)	(0.004)	(1.0)	
Registered with another political party	74922	3.6	-0.04	0.24	-0.2	0.803
			(0.15)	(0.004)	(0.6)	
Registered as a non-affiliated voter	74922	11.5	0.21	0.24	0.9	0.429
			(0.27)	(0.004)	(1.1)	
Panel B: By Category						
Gender						
Female	41249	44.9	0.27	0.23	1.2	0.617
			(0.54)	(0.01)	(2.4)	
Male	33673	38.5	0.92	0.25	3.6	0.117
			(0.59)	(0.01)	(2.3)	
Age						
Ages 19-49	54814	39.4	0.31	0.23	1.3	0.525
			(0.49)	(0.004)	(2.1)	
Ages 50-64	20108	49.0	1.17	0.26	4.6	0.153
			(0.82)	(0.01)	(3.2)	
English-language lottery materials						
No	6440	10.3	0.45	0.16	2.8	0.618
			(0.91)	(0.01)	(5.6)	
Yes	68482	44.8	0.37	0.24	1.5	0.406
			(0.44)	(0.004)	(1.8)	
Zip in a Democratic county (2008)						
No	26723	41.7	-0.12	0.25	-0.5	0.868
			(0.71)	(0.006)	(2.9)	
Yes	48199	42.2	0.86	0.23	3.7	0.103
			(0.53)	(0.005)	(2.3)	

Notes: All data are from the 2010 data pull, and all analyses are weighted to account for a series of new Medicaid lottery draws that began in Fall 2009, using weights that account for lottery selection through June 1, 2010. First stage variable is an indicator for Medicaid coverage, defined as being on Medicaid at any point from the first lottery notification through June 1, 2010. Column (3) shows the intent-to-treat estimates from equation (1); column (4) shows the first stage estimates from equation (3); column (5) shows the IV estimates of the impact of Medicaid coverage using the lottery as an instrument for Medicaid from equation (2). All analyses include controls for household size, and adjust the standard errors for household clusters.

Table 3: Voter Turnout - Other Post-Lottery Elections

	N	Control Group Mean	Effect of Lottery Selection	First Stage	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: November 2010 Election						
Measured in 2013 data	74922	22.9	-2.6	0.23	-11.3	0.080
			(1.5)	(0.01)	(6.5)	
Measured in 2013 data, controlling for pre-period voting	74922	22.9	-1.2	0.23	-5.2	0.228
			(1.0)	(0.01)	(4.3)	
Panel B: Any Other Post-Lottery Election						
Measured in 2010 Data	74922	20.5	0.6	0.24	2.4	0.107
			(0.4)	(0.004)	(1.5)	

Notes: Panel A outcome is from the 2013 data pull; Panel B outcome is from the 2010 data pull. "Any other post-lottery election" is any election from May 2008 through June 2010 except the November 2008 election (see Table A3 for list). The analysis of the November 2010 election is weighted to account for a series of new Medicaid lottery draws that began in Fall 2009, using weights to account for lottery selection through November 2010; the first stage variable is defined as being on Medicaid at any point from the first lottery notification date through the November 2010 election. The "any other post-lottery" election uses weights through June 2010 and defines the first stage variable as being on Medicaid at any point from the first notification date through June 1, 2010. Column (3) shows the intent-to-treat estimates from equation (1); column (4) shows the first stage estimates from equation (3); column (5) shows the IV estimates of the impact of Medicaid coverage using the lottery as an instrument for Medicaid from equation (2). All analyses include controls for household size, and adjust the standard errors for household clusters.

Appendices (online only)

Appendix A: Voting Data

The statewide voter list is maintained by the Office of the Secretary of State, Elections Office, in Oregon. The state's data distinguish between three types of voters: active, inactive, and cancelled. An "active" voter is someone who has voted or re-registered within the last 5 years. In Oregon, people who do not vote for more than 5 years need to re-register to remain active. Only registered individuals may vote, and they must update their voter registration in the case of a move, a name change, or if they wish to register or change an association with a political party (OregonLaws, Chapter 247). An "inactive" voter is someone who has not voted or re-registered in five years; in addition, individuals may be moved from the active to the inactive file for various administrative reasons (such as being incarcerated or having a bounced-back ballot mailing or a signature challenge that the individual did not respond to). "Cancelled" voters are individuals who are "inactive" for five years, who die, we are found to be registered in another state.

We obtained three separate individual-level data sets from the Oregon Secretary of State's Election Division. The two main data sets we use were received in June 2010 (hereafter "2010 data") and July 2013 (hereafter "2013 data"). Each of these data sets contains a list of currently active voters, currently inactive voters, and a voting history over a series of preceding elections. There are two key differences between these files. First, they provide information on the list of "active" voters at two different points in time. Second, they contain voting histories for different elections. Specifically, the 2010 data contain voting information on elections from May 2008 through May 2010 while the 2013 file contain voting data on elections from May 2006 through May 2012, but omit some smaller local elections included in the 2010 data.

We received a third data set in July 2015 that contains a list of all names that were placed on the "cancelled" voter list from 2006 on (hereafter "cancelled voter" data); since voters may remain on the cancelled voter file indefinitely, names in this file may or may not appear in the 2010 and 2013 data.

We probabilistically matched each of these three data sets to the Oregon Health Insurance Experiment lottery list using LinkPlus software. The match was done based on full name, date of birth, and gender and followed the matching procedure done in earlier work with the lottery list (see e.g. Finkelstein et al. 2012). Due to the protected nature of the lottery data, matching of the lottery data to the voting data was done on a secure, non-networked computer, and all identifiers were removed before analysis.

Individuals on the lottery list could thus match to each of the voter files or not. For each election represented in each voter file (e.g. 2008 election as described in the 2010 voter data file), lottery list members who matched could be characterized as having voted, being registered for the election but not having voted, or not having a record for that particular election (e.g. having

registered to vote in 2009, so not having a voting record for 2008). For individuals on the lottery list who have missing voting records in each of 2010 and 2013 data pulls - either because they did not match to the voting file or matched but had no record for particular elections - we match them to voting information in the cancelled voter file. If matched, we replace these people's missing voting records in 2010 or 2013 data with records from the cancelled voter data.

We use these data to define two main outcomes of interest:

Registration: We measure registration – and the political party the individual is registered with – as of June 22, 2010 in the 2010 data pull. Specifically, we define any one on the active voter file as of that date as registered as of that date.

Voting: We measure whether the individual voted in various elections. Specifically we measure whether the individual voted in:

- November 2008 general election. Our baseline specification measures this in the 2010 data pull. We also measure this same outcome (whether the individual voted in the November 2008 general election) in the 2013 data.
- November 2010 statewide election – measured in the 2013 data pull
- Any election post-lottery through June 2010 except the November 2008 election. These were local elections, primaries, or statewide special elections that ran from 5/27/2008 through 6/1/2010. They are listed in Table A3.
- 2006 and 2007 elections. These data are only available in the 2013 data. We use them both to test for balance in voting behavior pre-lottery and as a control variable to increase power in studying the impact of Medicaid on post-lottery voting using the 2013 data. The 2010 data do not contain these pre-lottery elections.

As noted above, in each data set, if the individual is missing a voting record, we tried to match them to the voting information in the cancelled voter file and if we found a match, we replaced the missing voting record with information from the cancelled voter file. In practice, this resulted in few additional voters. For example in the 2010 data, we added 130 voters (0.5 percent) to the November 2008 election, and in the 2013 data we added 927 additional voters to the November 2008 election.

Assessing data quality

Not everyone in the 2010 voter files appears in the 2013 voter files (and vice versa). Table A4 summarizes these results before we merge the cancelled voter data with 2010 or 2013 data pulls. For example, it shows that of 43,201 people in the 2010 voter files (active or inactive), only 37,310 are in the 2013 voter files. Likewise, of the 40,819 people in the 2013 voter files, only 37,310 are in the 2010 voter files. There are several potential reasons for this. First, there could be matching noise introduced by our probabilistic matching techniques. Second, there could be genuine entry into the data between 2010 and 2013, due to new registrations. Third, there could

be genuine exit from the data between 2010 and 2013, due to individuals being moved to the "cancelled" file because of death, incarceration, a move out of state, remaining inactive for 5 years, or other administrative reasons.

Reassuringly, we found that only 170 people (0.2 percent of the lottery list) enter the data between 2010 and 2013 *and* are recorded as having voted in the 2008 election; these presumably reflect errors in our probabilistic match. Likewise, of the 32,383 people who have a voting record (yes/no) in 2008 recorded in the 2010 and 2013 data, only 12 (<0.01 percent) have a different outcome recorded. These checks suggest only a small amount of noise in our measures.

Our primary concern, however, is not with noise (mis-measurement, mis-matching, attrition etc.) per se, but the potential for endogenous selection into the sample based on post-lottery behavior. For example, if the lottery affected voting behavior in 2008, and voting behavior in 2008 affected presence in the 2010 files (i.e. someone who might otherwise have been moved to "cancelled" is maintained), then using information in the 2010 file to infer the effect of the lottery on voting behavior in 2008 would be contaminated by differential selection of treatment and control groups into the sample. Likewise, any impact the lottery had on mortality or moves out of state could also affect our ability to measure 2008 or 2010 voting behavior. Reassuringly, our prior analysis shows no substantial effect of the lottery on mortality (Finkelstein et al., 2012), but the other avenues still have the potential to affect the sample we observe.

Fortunately, we were able to obtain the list of all names placed in the cancelled voter file since 2006, which should capture any exit from our data between 2010 and 2013 due to individuals being moved to the "cancelled" file. About 50 percent, or 2,987, of "exited voters" (those who matched in 2010 but not 2013 data) can be found in the cancelled voter data. Beyond that, we also find 4 percent, or 1,092, of lottery list individuals who did not appear in either 2010 or 2013 data in the cancelled voter file.

In Table A5 and A6 we cross tabulate the match status in cancelled and 2010 data files with respect to voting records of November 2008 and November 2010 elections in order to assess the quality of the cancelled voter file. In particular, we want to see how many missing voting records can be found in cancelled voter file, and how often there are conflicting voting records between the cancelled file and other two data sets. The results show that, cases of conflicting voting records are rare (<10 cases or <0.1 percent of exiting records in both elections), while the file only helps us fill in a marginal share of missing voting records among the lottery list individuals - 0.3 percent and 0.7 percent in November 2008 and November 2010 elections, respectively.

Since there are still 50 percent of unexplained exits between 2010 and 2013 data, we performed two additional tests for potential endogenous measurement as mentioned earlier. First, we looked at whether entry or exit between 2010 and 2013 was correlated with treatment status. Second, using the 2013 file, we analyzed whether pre-lottery (2006 or 2007) voting was correlated with

treatment status. Both tests use updated 2010 and/or 2013 data sets after the inclusion of voting records from the cancelled file. In each case we ran the following regression:

$$y_{ih} = \beta_0 + \beta_1 LOTTERY_h + X_{ih}\beta_2 + \epsilon_{ih} \quad (1)$$

where i indexes individuals and h indexes households, LOTTERY is an indicator for whether household h was selected in the lottery. X_{ih} includes controls for household size indicators. Standard errors are clustered on the household.

Table A7 shows the results, which are reassuring. The top panel shows that the probability of voting in the pre-lottery period is balanced between treatment and control, and the bottom panel shows that entry into and exit out of the data sets are also balanced.

Appendix B: Analytic Weights

For analyses of outcomes in fall 2009 or later, we use weights to adjust for a new lottery for OHP Standard which the state conducted beginning in the fall of 2009. These weights were previously used in Baicker et al. (2013).

At the start of the new lottery, the state mailed postcards to those on the original list that were not selected (our controls) asking if they would like to be included in this second lottery. Those who returned the postcard were added to the new waiting list and an initial draw was done just from that group. Following that initial draw, the state opened the new waiting list to the general public (including both our controls and our treatments as well people not on our original list); drawings from this list were conducted approximately monthly. Unlike the original 2008 waiting list, the new waiting list remained continuously open: individuals could sign up at any point. As with the original lottery, draws were done on individuals, but the opportunity to apply for OHP Standard (the treatment) was extended to the whole household. After each drawing, we probabilistically matched (using LinkPlus software) the new waiting list to our study population to identify individuals who were eligible for selection by the state (called "opt-ins") and those who were actually selected in a given drawing (called "selected opt-ins"). By December 6, 2010 the state had selected everyone in our original sample who signed up for the new lottery.

Given the difficulty in interpreting the "treatment" received by those who were drawn in the new lottery, we drop the selected opt-ins from our analytic sample and use weights to correct for this. For each lottery drawing, the set of opt-ins is not a random sample of our study population: signing up for the new list was optional, and thus subject to the influence of factors such as underlying health. However, the set of selected opt-ins *is* a random sample of the opt-ins. We therefore use weights to adjust for the individuals dropped because of the second lottery using the following principle: within any (even non-random) subset of the original study population, a randomly selected group can be weighted to stand in for the non-selected remainder based on the probability of that random selection.

The weights we use are roughly analogous to weighting done for censoring or attrition in longitudinal data (Cole and Hernán, 2008, Kalton, 1986). As in those settings, we weight each observation at each time point by the inverse probability of being in the sample, and we generate overall weights as the product of the weights across all time points. We do not need to model the probability of being selected in the new lottery as a function of covariates; we know the process was random and we can observe the selection proportions.

More specifically, let O_t be the set of opt-ins in our study population eligible for new lottery drawing on date t. Let S_t be the set of opt-ins selected in drawing on date t. We define the weight for individual i to be:

$$w_t(i) = \begin{cases} \frac{1}{1-p_t} \; if \; i \; in \; O_t \; and \; in \; S_t \\ 0 \; if \; i \; in \; S_t \\ 1 \; if \; i \; not \; in \; O_t \end{cases} \quad (2)$$

where p_t is the probability of an opt-in being selected.

Selection probabilities varied by the number of household members on the new list, so in all cases, we estimated the selection probability separately by strata of "tickets" (household members on the new waiting list at time t).

The final analytic weight W is simply the product all the weights w_t introduced up to the end date. This end date is chosen based on the date of the outcome analyzed. Analysis of different outcomes use different weights.

We refer to the set of weights by their end date (i.e. June 2010 weights use the product of weights up through June 2010). Analyses of November 2008 voting are unweighted (since this occurred prior to the lottery). Analyses of registration (as of June 2010), and of "any other election in the data" (which includes elections through June 1, 2010), use the June 2010 weights; analyses of November 2010 voting use the November 2010 weights.

Table A8 shows the distribution of the June 2010 and November 2010 weights. One can see that the November 2010 weights involve a much greater share of individuals with zero weights (and a higher upweighting of the remaining individuals), reflecting several large new lottery draws that occurred between those dates. The control group is far more impacted by the weights than the treatment group as they were more likely to sign up for the new lottery.

The voting in November 2008 precedes the new lottery so the analysis is unweighted. The June 2010 registration and voting through June 2010 uses weights to account for new lottery draws through June 2010. The November 2010 voting analysis uses weights to account for new lottery draws through November 2010. As the new lottery progressed, the weights become more extreme, which has a potential precision cost.

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Table A1: Treatment-Control Balance

	Unweighted			June 2010 Weights			November 2010 Weights		
	Control mean	Treatment-control difference	p-value	Control mean	Treatment-control difference	p-value	Control mean	Treatment-control difference	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lottery List Variables									
Year of Birth	1968.0	0.16	0.104	1968.0	0.18	0.091	1968.1	-0.07	0.826
		(0.10)			(0.11)			(0.33)	
Female	0.56	-0.01	0.039	0.56	-0.01	0.011	0.55	0.01	0.356
		(0.003)			(0.003)			(0.01)	
English as preferred language	0.92	0.002	0.346	0.92	0.004	0.198	0.93	-0.01	0.293
		(0.003)			(0.003)			(0.01)	
Signed up self	0.92	0.0003	0.273	0.92	0.0004	0.163	0.92	0.001	0.094
		(0.0003)			(0.0003)			(0.001)	
Signed up first day of lottery	0.09	0.001	0.627	0.09	0.001	0.647	0.11	-0.03	0.055
		(0.002)			(0.003)			(0.02)	
Gave Phone Number	0.86	-0.003	0.300	0.86	-0.002	0.420	0.87	-0.01	0.424
		(0.003)			(0.003)			(0.01)	
Address is a PO Box	0.12	0.0004	0.873	0.12	0.001	0.755	0.12	0.0001	0.991
		(0.003)			(0.003)			(0.01)	
Zip code median household income	39265.4	44.9	0.538	39273.5	14.2	0.855	39178.5	4.9	0.989
		(72.9)			(78.2)			(339.5)	
Ever voted in the pre-period	0.23	-0.0005	0.889	0.23	-0.001	0.787	0.24	-0.03	0.089
		(0.003)			(0.004)			(0.02)	
F statistic		F	p-value		F	p-value		F	p-value
lottery list variables		1.322	0.227		1.718	0.089		1.316	0.230
lottery list variables and pre-period voting		1.175	0.306		1.530	0.131		1.379	0.191

Notes: We report the coefficient on LOTTERY from estimating equation (1) on the dependent variable shown in the first column. All dependent variables are measured based on the lottery sign up, except for "every voted in the pre-period" which is defined as voting in a 2006 or 2007 election, as measured in the 2013 data pull and the cancelled voter file. All regressions include indicators for the number of household members on the lottery list, adjust standard errors for household clusters. Columns (1)-(3) are unweighted, (4)-(6) use weights through June 2010, and (7)-(9) use weights through November 2010. The final rows report the pooled F-statistics (and p-values) from testing treatment-control balance on sets of variables jointly.

Table A2: November 2008 Voter Turnout (Using 2013 Data)

	N	Control Group Mean	Effect of Lottery Selection	First Stage	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Overall, Measured in 2010 Data	74922	33.8	0.69	0.27	2.5	0.073
			(0.38)	(0.003)	(1.4)	
Panel A: Measured in 2013 Data						
Overall	74922	32.8	0.58	0.27	2.1	0.130
			(0.38)	(0.003)	(1.4)	
Gender						
Female	41249	35.7	-0.08	0.26	-0.3	0.864
			(0.49)	(0.004)	(1.9)	
Male	33673	29.2	1.47	0.28	5.2	0.005
			(0.52)	(0.004)	(1.9)	
Age						
Ages 19-49	54814	29.3	0.42	0.26	1.6	0.325
			(0.43)	(0.004)	(1.6)	
Ages 50-64	20108	42.1	1.08	0.29	3.7	0.157
			(0.76)	(0.01)	(2.6)	
English-language lottery materials						
No	6440	6.9	0.47	0.19	2.5	0.503
			(0.70)	(0.01)	(3.7)	
Yes	68482	35.0	0.51	0.28	1.8	0.210
			(0.40)	(0.003)	(1.4)	
Zip in a Democratic county (2008)						
No	26723	31.3	0.06	0.28	0.2	0.929
			(0.64)	(0.01)	(2.3)	
Yes	48199	33.6	0.86	0.27	3.2	0.069
			(0.48)	(0.004)	(1.8)	
Panel B: Measured in 2013 Data, Controlling for Pre-period Voting						
Overall	74922	32.8	0.61	0.27	2.2	0.046
			(0.30)	(0.003)	(1.1)	
Gender						
Female	41249	35.7	0.13	0.26	0.5	0.752
			(0.40)	(0.004)	(1.5)	
Male	33673	29.2	1.24	0.28	4.4	0.003
			(0.42)	(0.004)	(1.5)	

Age						
Ages 19-49	54814	29.3	0.57	0.26	2.2	0.109
			(0.36)	(0.004)	(1.4)	
Ages 50-64	20108	42.1	0.69	0.29	2.3	0.223
			(0.56)	(0.01)	(1.9)	
English-language lottery materials						
No	6440	6.9	0.19	0.19	1.0	0.750
			(0.59)	(0.01)	(3.1)	
Yes	68482	35.0	0.60	0.28	2.2	0.065
			(0.33)	(0.003)	(1.2)	
Zip in a Democratic county (2008)						
No	26723	31.3	0.13	0.28	0.5	0.796
			(0.50)	(0.01)	(1.8)	
Yes	48199	33.6	0.87	0.27	3.3	0.023
			(0.38)	(0.004)	(1.4)	

Notes: This table resembles Panel B of Table 1 except that results all use the 2013 data pull. The first stage variable is an indicator for Medicaid coverage at any point from the first lottery notification through the November 2008 election. Column (3) shows the intent-to-treat estimates from equation (1); column (4) shows the first stage estimates from equation (3); column (5) shows the IV estimates of the impact of Medicaid coverage using the lottery as an instrument for Medicaid from equation (2). All analyses are unweighted, include controls for household size, and adjust the standard errors for household clusters. Panel B additionally includes an indicator variable for whether the individual voted in a pre-lottery election (defined as having voted in at least one of the 2006 or 2007 elections shown in Table A3).

Table A3: Elections in the Data

Date	Election	Data
Pre-Period Elections		
5/16/2006	Statewide primary (including: OR Governor, US Congress, OR supreme court judge, OR state legislature)	2013
11/7/2006	State general election (including: Governor, US Congress, OR supreme court judge, US state legislature)	2013
5/15/2007	Off-year primary election	2013
11/6/2007	Special election	2013
Post-Lottery Elections		
5/20/2008	Statewide primary election (including: President, U.S. Congress state legislature)	2013
5/27/2008	Local elections	2010
7/15/2008	Local elections	2010
9/16/2008	Local elections	2010
10/7/2008	Local elections	2010
11/4/2008	Statewide general election (including: President, U.S. Congress, OR state legislature)	2010, 2013
11/18/2008	Local elections	2010
3/10/2009	Local elections	2010
5/5/2009	Local elections	2010
5/19/2009	Primary election	2010, 2013
6/23/2009	Local elections	2010
8/11/2009	Local elections	2010
9/15/2009	Local elections	2010
9/29/2009	Local elections	2010
10/13/2009	Local elections	2010
10/27/2009	Local elections	2010
11/3/2009	Local elections	2010
11/4/2009	Local elections	2010, 2013
11/17/2009	Local elections	2010
12/8/2009	Local elections	2010
12/15/2009	Local elections	2010
12/29/2009	Local elections	2010
1/26/2010	Statewide special election (to vote on two tax measures)	2010, 2013
3/9/2010	Local elections	2010
5/18/2010	Statewide primary election (including US Congress; OR governor; OR state legislature)	2010, 2013
6/1/2010	Local elections	2010
11/2/2010	Statewide general election (including US Congress; OR governor; OR state legislature)	2013

Table A4: 2010 and 2013 Data Files

			2013 data				
			Not matched	**Matched**			**Total**
				Missing Nov 2008 Voting Data	Registered but Did Not vote in November 2008	Voted in November 2008	
2010 data	**Not matched**		28,212	3,245	94	170	31,721
	Matched	Missing Nov 2008 Voting Data	3,029	4,818	3	7	7,857
		Registered but Did Not Vote in Nov 2008	1,155	46	8,731	7	9,939
		Voted in November 2008	1,707	53	5	23,640	25,405
	Total		34,103	8,162	8,833	23,824	74,922

Notes: "Missing" from the 2008 November voting data means that the individual is matched to the voting data but we have no record of whether she voted in that election. That could be, for example, because she was registered to vote after that election (but before the data pull).

Table A5: November 2008 Voting Records in the Cancelled Voter File and in the 2010 Data

			Cancelled Voter Data				
				Matched			
			Not matched	Missing Nov 2008 Voting Data	Registered but Did Not Vote in Nov 2008	Voted in November 2008	Total
2010 Data	**Not Matched**		30,333	1,213	49	126	31,721
	Matched	Missing Nov 2008 Voting Data	5,443	2,410	0	4	7,857
		Registered but Did Not Vote in Nov 2008	8,353	83	1,498	5	9,939
		Voted in November 2008	23,280	183	4	1,938	25,405
	Total		67,409	3,889	1,551	2,073	74,922

Notes: "Missing" from the 2008 November voting data means that the individual was not registered at the time of the election. We replace voting records in the 2010 data that are "not matched" or "missing" with matched voting records from the cancelled voter file.

Table A6: November 2010 Voting Records in the Cancelled Voter File and in the 2013 Data

			Cancelled Voter Data				
				Matched			
			Not matched	Missing Nov 2010 Voting Data	Registered but Did Not Vote in Nov 2010	Voted in November 2010	Total
2013 Data	Not Matched		30,024	3,487	277	315	34,103
	Matched	Missing Nov 2010 Voting Data	8,214	2,267	10	10	10,501
		Registered but Did Not Vote in Nov 2010	13,774	163	490	1	14,428
		Voted in November 2010	15,397	95	2	396	15,890
	Total		67,409	6,012	779	722	74,922

Notes: "Missing" from the 2010 November voting data means that the individual was not registered at the time of the election. We replace voting records in the 2013 data that are "not matched" or "missing" with matched voting records from the cancelled voter file.

Table A7: Tests of Balance for Sample Selection

	Control mean	Treatment-control difference	p-value
	(1)	(2)	(3)
Voted in November 2006 election	0.17	0.004	0.147
		(0.003)	
Voted in November 2007 election	0.15	-0.002	0.587
		(0.003)	
Voted in any 2006 or 2007 election	0.23	-0.0005	0.889
		(0.003)	
Entry	0.04	-0.0002	0.898
		(0.002)	
Exit	0.04	0.00001	0.996
		(0.002)	

Notes: We report the coefficient on LOTTERY from estimating equation (1) on the dependent variable shown in the first column. All regressions include indicators for the number of household members on the lottery list, adjust standard errors for household clusters, and are unweighted. The first three rows (analyzing voting in pre-lottery elections) use data from the 2013 data pull and the cancelled voter data. "Voted in any 2006 or 2007 election" includes the November 2006 state elections and the November 2007 special election (including 2 ballot measures) in the previous rows, as well as the May 2006 and May 2007 primaries. "Entry" is an indicator for individuals who appear in the 2013 data pull but not in the 2010 data pull or the cancelled voter file. "Exit" is an indicator for individuals who appeared in the 2010 data pull but not in the 2013 data pull or the cancelled voter file.

Table A8: Distribution of the Weights

	Mean	Standard Deviation	Min	Median	75th%ile	95%ile	Max	N	Share with zero weight
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
June 2010 weights									
Full Sample	1.0	0.4	0	1	1	1.4	3.9	74922	0.09
Controls	1.0	0.4	0	1	1.3	1.5	3.9	45088	0.13
Treatments	1.0	0.2	0	1	1	1.3	2.7	29834	0.04
June 2010 weights (non-zero weights)									
Full Sample	1.1	0.2	1	1	1.1	1.4	3.9	67885	
Controls	1.2	0.2	1	1	1.4	1.5	3.9	39097	
Treatments	1.0	0.1	1	1	1	1.3	2.7	28788	
November 2010 weights									
Full Sample	1.0	3.2	0	1	1	1	190.0	74922	0.35
Controls	1.0	3.6	0	1	1	1.2	190.0	45088	0.44
Treatments	1.0	2.5	0	1	1	1	139.4	29834	0.21
November 2010 weights (non-zero weights)									
Full Sample	1.5	3.8	1	1	1	3.3	190.0	48767	
Controls	1.8	4.6	1	1	1	9.5	190.0	25217	
Treatments	1.3	2.7	1	1	1	1	139.4	23550	

Notes: Table shows the distribution of weights used to account for the new health insurance lottery that started in the fall of 2009. The top two panels (June 2010 weights) display the distribution of weights used to analyze registration and voting in elections (excluding the 2008 general election and the 2010 midterms), accounting for new lottery selection through June 1, 2010. The bottom two panels (November 2010 weights) report weights used to analyze voting in the 2010 Midterms, accounting for new lottery selection through November 4, 2010.