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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% overall, with the effects concentrated in men (18% increase) and in residents of Democratic counties (10% 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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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 United States; 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 (aged 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 (Baicker *et al.*, 2013, 2014; Finkelstein *et al.*, 2012, 2016; Taubman *et al.*, 2014). 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 Experiment 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.

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

Finally, it found that Medicaid reduced depression and improved self-reported health, but had no detectable impacts on several measures of physical health.

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% relative to the 34% 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%, and for residents in Democratic counties, for whom Medicaid increased turnout by 3.6 percentage points (standard error = 1.8), or about 10%.

We also find some evidence that Medicaid increased voter registration, although the results were statistically insignificant. Intriguingly, however, the statistically insignificant increases in registration are 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 prior work on the Oregon Health Insurance Experiment, 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 (Finkelstein *et al.*, 2012).

[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.

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 increase voting — whether or not the applicant ultimately received Medicaid — given that, 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 (because 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, because eligibility, unlike receipt, is a policy lever that the government can directly control (see, e.g., Gruber, 1997).

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 (Campbell, 2012; Pierson, 1993).

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 (Brady *et al.*, 1995; Mettler and Stonecash, 2008; Wolfinger and Rosenstone, 1980). Evidence from the Oregon experiment (e.g., Baicker *et al.*, 2013; Finkelstein *et al.*, 2012) as well as quasi-experimental studies of recent Medicaid expansions (Hu *et al.*, 2016; Mazumder and Miller, 2016) 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 (Blais, 2000; Pacheco and Fletcher, 2015). Evidence from the Oregon experiment on health impacts is mixed; it indicates that Medicaid improved self-reported health and reduced depression, but had no detectable impact on measures of physical health (Baicker *et al.*, 2013; Baicker and Finkelstein, 2013; Finkelstein *et al.*, 2012). 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 encourage political participation (Skocpol, 1991; Wilson, 1987). 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 (Campbell, 2012; Schneider and Ingram, 1993; Soss, 1999, 2002). 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 reverse 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., Labonne, 2013; Manacorda *et al.*, 2011; O, Ana L. De La, 2013; Pop-Eleches and Pop-Eleches, 2012). However, in the United States, where relatively fewer causal estimates of the impact of means-tested benefit receipt on voting are available, the existing studies are mixed. Gay (2012) looks at the impact of the Moving to Opportunity randomized experiment on housing mobility on voting, and finds 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 — increased 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. 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.

Intervention and Data

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: aged 19–64; not otherwise eligible for Medicaid or other public insurance; Oregon residents; US 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.

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., Baicker *et al.*, 2014; Finkelstein *et al.*, 2016; Finkelstein *et al.*, 2012; Taubman *et al.*, 2014). 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*). Two main differences are present 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; by contrast, the 2013 file contains voting data on elections from May 2006 through May 2012, but omits 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 (pp. 2–7) provides more detail on the data construction.

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.

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:

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

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 because 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, given that 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.

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:

$$y_{ih} = \pi_0 + \pi_1 \text{INSURANCE}_{ih} + X_{ih}\pi_2 + V_{ih}\pi_3 + \nu_{ih} \quad (2)$$

where INSURANCE is a measure of insurance coverage and all other variables are as defined in Equation (1). Specifically, following prior work on the Oregon Health Insurance Experiment, we define INSURANCE as an indicator variable for Medicaid coverage at any point from the first lottery notification through the latest outcome analyzed. The prior work on the Oregon Health Insurance Experiment found that the lottery had no impact on non-Medicaid sources of insurance coverage, such as private insurance (Finkelstein *et al.*, 2012).

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

$$\text{INSURANCE}_{ih} = \delta_0 + \delta_1 \text{LOTTERY}_h + X_{ih}\delta_2 + V_{ih}\delta_3 + \mu_{ih} \quad (3)$$

in which the excluded instrument is the variable LOTTERY. Prior work on the Oregon Health Insurance Experiment discussed this first-stage relationship in detail. The first stage (i.e., impact of winning the lottery on probability of being enrolled in Medicaid during the one- or two-year study period) is about 0.25. This is primarily due to incomplete takeup among lottery winners: only about 60% 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 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.

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 on the Oregon Health Insurance Experiment (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 (pp. 8–10) describes these weights in more detail.

Balance

A central question in any analysis of a state-conducted randomization is whether the state actually randomized as they described. This was explored extensively in our prior work (see especially Finkelstein *et al.*, 2012) on the Oregon Health Insurance Experiment, which showed both via simulations and pre-lottery balance tests that the state appears to have randomized as indicated.[3]

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).

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 2.1–2.5 percentage points, depending on the specification. This represents a 6–7% increase off the 33–34% 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%) than men (30%), but the impact of Medicaid on voting appears to be entirely concentrated among men. Medicaid

[3]For completeness, Appendix Table A1 (p. 12) 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.

Table 1: November 2008 voter turnout.

	N (1)	Control group mean (2)	Effect of lottery selection (3)	First stage (4)	Effect of medicaid coverage (5)	*p*-Value (6)
Panel A: Overall						
Measured in 2010 data	74,922	33.814	0.691	0.271	2.549	0.073
			(0.385)	(0.003)	(1.420)	
Measured in 2013 data	74,922	32.789	0.577	0.271	2.129	0.130
			(0.382)	(0.003)	(1.408)	
Measured in 2013 data, controlling for pre-period voting	74,922	32.789	0.609	0.271	2.244	0.046
			(0.304)	(0.003)	(1.123)	
Panel B: By category						
Gender						
Female	41,249	37.170	0.106	0.264	0.402	0.831
			(0.498)	(0.004)	(1.886)	
Male	33,673	29.593	1.510	0.281	5.381	0.004
			(0.523)	(0.004)	(1.864)	
Age						
Ages 19–49	54,814	30.369	0.562	0.263	2.140	0.197
			(0.436)	(0.004)	(1.657)	
Ages 50–64	20,108	43.054	1.119	0.294	3.808	0.142
			(0.762)	(0.006)	(2.596)	
English-language lottery materials						
No	6,440	7.021	0.296	0.189	1.570	0.674
			(0.705)	(0.011)	(3.734)	
Yes	68,482	36.081	0.645	0.279	2.312	0.113
			(0.407)	(0.003)	(1.458)	
Zip in a Democratic county (2008)						
No	26,723	32.434	0.220	0.279	0.788	0.733
			(0.646)	(0.006)	(2.313)	
Yes	48,199	34.565	0.950	0.267	3.561	0.048
			(0.479)	(0.004)	(1.797)	

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) 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.

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% increase in the probability of men voting relative to the control group. Some evidence also points to 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%; 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%. Appendix Table A2 (pp. 13–14) 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%) increase in voting (p-value = 0.11).

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

	N (1)	Control group mean (2)	Effect of lottery selection (3)	First stage (4)	Effect of medicaid coverage (5)	*p*-Value (6)
Panel A: Overall and by party affiliation						
Overall	74,922	42.032	0.511	0.238	2.149	0.228
			(0.424)	(0.004)	(1.782)	
Registered as a Democrat	74,922	19.168	0.222	0.238	0.933	0.499
			(0.328)	(0.004)	(1.381)	
Registered as a Republican	74,922	7.757	0.115	0.238	0.481	0.635
			(0.241)	(0.004)	(1.015)	
Registered with another political party	74,922	3.603	−0.038	0.238	−0.160	0.803
			(0.152)	(0.004)	(0.640)	
Registered as a non-affiliated voter	74,922	11.504	0.213	0.238	0.894	0.429
			(0.269)	(0.004)	(1.129)	
Panel B: By category						
Gender						
Female	41,249	44.851	0.272	0.226	1.206	0.617
			(0.543)	(0.005)	(2.408)	
Male	33,673	38.464	0.922	0.254	3.623	0.117
			(0.589)	(0.005)	(2.314)	
Age						
Ages 19–49	54,814	39.441	0.309	0.231	1.338	0.525
			(0.487)	(0.004)	(2.107)	
Ages 50–64	20,108	48.972	1.170	0.256	4.568	0.153
			(0.817)	(0.007)	(3.194)	
English-language lottery materials						
No	6,440	10.261	0.455	0.164	2.778	0.618
			(0.911)	(0.014)	(5.570)	
Yes	68,482	44.751	0.367	0.245	1.498	0.406
			(0.441)	(0.004)	(1.802)	
Zip in a Democratic county (2008)						
No	26,723	41.688	−0.118	0.248	−0.478	0.868
			(0.713)	(0.006)	(2.880)	
Yes	48,199	42.219	0.858	0.232	3.693	0.103
			(0.526)	(0.005)	(2.262)	

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 (1)	Control group mean (2)	Effect of lottery selection (3)	First stage (4)	Effect of medicaid coverage (5)	*p*-Value (6)
Panel A: November 2010 election						
Measured in 2013 data	74,922	22.895	−2.590	0.229	−11.330	0.080
			(1.529)	(0.013)	(6.463)	
Measured in 2013 data, controlling for pre-period voting	74,922	22.895	−1.170	0.229	−5.156	0.228
			(0.984)	(0.013)	(4.273)	
Panel B: Any other post-lottery election						
Measured in 2010 data	74,922	20.528	0.577	0.238	2.428	0.107
			(0.358)	(0.004)	(1.505)	

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.

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% 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.

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 United States is limited. This paper examines the impact of a major public program in the United States, 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) find in the context of the 2014 ACA Medicaid expansions. This is striking because 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, because neither resource effects nor interpretive effects are obviously transitory in nature. Moreover, evidence that voting is habit forming (e.g., Gerber *et al.*, 2003; Plutzer, 2002) 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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