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Out of the Woodwork: Enrollment Spillovers in the Oregon Health Insurance Experiment†

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We study the impact of expanded adult Medicaid eligibility on the enrollment of already-eligible children. We analyze the 2008 Oregon Medicaid lottery, in which some low-income uninsured adults were randomly selected to be allowed to apply for Medicaid. Children in these households were eligible for Medicaid irrespective of the lottery outcome. We estimate statistically significant but transitory impacts of adult lottery selection on child Medicaid enrollment: at three months after the lottery, for every nine adults who enrolled in Medicaid due to winning the lottery, one additional child also enrolled. Our results shed light on the existence, magnitude, and nature of so-called "woodwork effects." (*JEL* G22, H75, I13, I18, I38, J13)

With the 2010 passage of the Affordable Care Act (ACA) the United States has moved closer to universal health insurance eligibility, but universal health insurance enrollment remains more elusive. Incomplete enrollment is particularly pronounced in the Medicaid population, where about 14 percent of eligible adults and 7 percent of eligible children remain uninsured despite access to free or heavily subsidized coverage (Blumberg et al. 2018). To shed light on barriers to enrollment, we examine the impact of expanded Medicaid eligibility for adults on the Medicaid enrollment of their already-eligible children. Estimation of this so-called "woodwork" or "welcome-mat" effect also has implications for the total costs and benefits of expanded Medicaid eligibility. States cited potential woodwork effects to explain their reluctance to expand Medicaid under the ACA despite substantially enhanced federal subsidies; the enhanced subsidies did not apply to the previously eligible (Sommers and Epstein 2011).

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Credibly estimating woodwork effects, or any spillover effect, is challenging. Where the researcher may see a spillover effect from a policy change for group A on the behavior of group B, the skeptical seminar participant or referee may see a failed placebo test. Moreover, spillovers may be too small to detect reliably since in many contexts they are likely to be substantially smaller than direct effects. For good reason, therefore, the empirical bar for credibly identifying spillovers or the lack thereof is high.

The 2008 Oregon Health Insurance Experiment provides an excellent opportunity to surmount these challenges and estimate enrollment spillovers. A lottery randomly gave some low-income adults but not others the ability to apply for Medicaid. Children of these low-income adults were very likely already eligible for Medicaid; their eligibility did not depend on whether their parents won the lottery. The lottery only determined eligibility for adults.

We link existing Oregon Health Insurance Experiment data to newly obtained data on Medicaid enrollment for all Oregon Medicaid beneficiaries. Prior work found that in the year after random assignment, adults selected by the lottery to be able to apply for Medicaid were 25 percentage points more likely to enroll in Medicaid than adults who signed up for the lottery but were not selected (Finkelstein et al. 2012). Here, we use the lottery to study the impact of this expanded adult eligibility on the enrollment of their previously eligible children.

Figures 1 and 2 summarize our key finding: expanded adult Medicaid eligibility had a statistically significant impact on child Medicaid enrollment, with a spillover effect that is about an order of magnitude smaller than the direct effect. At three months after the lottery, we estimate that for every nine adults who enroll in Medicaid due to winning the lottery, one additional child also enrolls. The cost to the state of covering each child who enrolls due to woodwork effects is about one-fourth that of an adult covered through the lottery. The number of children who enroll due to woodwork effects is about 6 percent of the maximum possible woodwork effect, which we calculate based on the average number of Medicaid-eligible children not enrolled in control households.

Both the direct effect of winning the lottery on adult enrollment and the indirect effect on child enrollment attenuate over time, as some households not selected in the lottery gradually enroll in Medicaid through other mechanisms and some selected households that did enroll following the lottery do not re-enroll. As a result, one year after the lottery the impact of a household winning the lottery on their children's enrollment has declined from the initial, three-month, statistically significant increase of 0.024 children (compared to 0.22 adults) to a statistically insignificant increase of 0.008 children (compared to 0.14 adults).

These results suggest that woodwork effects may be quantitatively less important than previously conjectured. Claims of potentially large woodwork effects—in excess of half of the direct effects—were prominent in discussions of the likely impact of expanding adult Medicaid eligibility under the ACA (Murray 2009; Norman and Ferguson 2009). The existing literature on these impacts is primarily based on difference-in-difference analyses of state Medicaid expansions in the 1990s and 2000s and of the ACA Medicaid expansions of the 2010s (Aizer and Grogger 2003; Dubay and Kenney 2003; Frean, Gruber, and Sommers 2017; Hamersma, Kim, and Timpe 2019; Hudson and Moriya 2017; Sommers et al. 2016; Sonier, Boudreaux, and Blewett 2013). Studies of pre-ACA adult Medicaid expansions have tended to find fairly large child enrollment spillovers; for example, Dubay and Kenney (2003) find that Massachusetts's adult Medicaid expansion raised child coverage rates by 15 percentage points. However, analyses of the ACA Medicaid expansions have tended to find more modest effects, with child Medicaid coverage rates rising by roughly 3 percentage points due to expanded parental eligibility (Hudson and Moriya 2017; Sommers et al. 2016); this is roughly comparable to our estimate.[1] Of course, spillover effects may differ across contexts, particularly between the large-scale expansions studied by most of the prior literature and a small-scale expansion such as the one we study in Oregon.

Our findings contribute to the growing empirical literature on the pervasive phenomenon of incomplete take-up of social safety net programs. Commonly hypothesized barriers to take-up include lack of information about eligibility, transaction costs associated with enrollment, and stigma from program participation (Currie 2006). In the specific context of Medicaid, the ability of eligible individuals to wait and enroll when needed—so-called "conditional coverage"—may also contribute to incomplete formal enrollment at any given point in time (Cutler and Gruber 1996). Both information and transaction costs have been found to reduce take-up of Medicaid (Aizer 2003; Wright et al. 2017), the Supplemental Nutrition Assistance Program (Finkelstein and Notowidigdo 2019; Homonoff and Somerville 2020), the Earned Income Tax Credit (Bhargava and Manoli 2015), and disability insurance (Deshpande and Li 2019). Our empirical finding of a contemporaneous increase in adult and child enrollment due to winning the lottery for adult Medicaid is consistent with both limited information on eligibility and the transaction costs of enrolling creating barriers to children's Medicaid take-up. Further work disentangling the relative contributions of these two channels would be valuable, especially since they may have different implications for the welfare consequences of any woodwork effects (Anders and Rafkin 2021).

Our findings also contribute to the literature using the random assignment of adult Medicaid eligibility from the Oregon Health Insurance Experiment to study the impact of expanding Medicaid eligibility. Prior work has examined effects on adult health care use, health, financial outcomes, and voter participation (Baicker et al. 2014, 2013; Baicker and Finkelstein 2019; Finkelstein et al. 2012, 2016; Taubman et al. 2014). It found that in the first two years, Medicaid increased health care use across a wide range of settings; reduced out-of-pocket medical spending and unpaid medical debt; reduced depression and improved self-reported health; had no detectable impact on employment, earnings, or several measures of physical health; and led to a short-lived increase in voter turnout.

The current paper expands the scope of the analysis of the Oregon Health Insurance Experiment to consider potential indirect effects on individuals not directly subject to the experiment—namely, the children of participating adults. The Medicaid enrollment of the children of adults participating in the Oregon lottery has also been

[1] We estimate an increase of 0.024 children enrolled per winning household relative to the average 0.85 children living in each household (according to survey data), or about a 3 percentage point increase in child enrollment.

the subject of a prior study (DeVoe et al. 2015a), which found somewhat larger and longer-lived woodwork effects than we do. As we discuss in more detail in online Appendix A, this may be because the way the study constructed its analysis sample potentially introduced a source of bias.

The rest of the paper proceeds as follows. Section I describes our institutional setting as well as possible mechanisms by which winning the lottery for adult Medicaid eligibility might affect already-eligible children's Medicaid enrollment. Section II describes the empirical framework and data. Section III presents the results. The final section concludes.

I. Setting

A. *Medicaid in Oregon*

Oregon's Medicaid program is called the Oregon Health Plan (OHP) and consists of two distinct programs: OHP Plus and OHP Standard. The Oregon Health Insurance Experiment was a lottery for adults for coverage through OHP Standard; children of lottery participants were eligible for Medicaid coverage through OHP Plus and remained eligible regardless of whether their parents participated in or won the lottery.

At the time of the Oregon experiment, OHP Plus served the categorically eligible Medicaid population including older adults, adults with disabilities, pregnant women, people eligible for TANF, and foster children, with coverage in each category available up to certain income limits. Children aged 0–5 years below 133 percent of the federal poverty line and children aged 6–18 years below 100 percent of the poverty line were eligible for OHP Plus; children between these limits and 185 percent of the federal poverty line were eligible for health coverage through the Children's Health Insurance Program, or CHIP (Kaiser Family Foundation 2019; National Academy for State Health Policy, n.d.).

OHP Standard, the program subject to the 2008 lottery, covered uninsured adults under the federal poverty line aged 19–64 years who did not otherwise qualify for OHP Plus. By construction, therefore, the child of any adult eligible via lottery for OHP Standard (i.e., below 100 percent of the federal poverty line) would be eligible for OHP Plus. OHP Standard and OHP Plus both provided comprehensive insurance benefit packages without cost sharing, though OHP Plus's package was broader and had no premiums for children while OHP Standard charged a premium of up to \$20 per month (Berkobien 2008; Oregon Department of Human Services 2008a).

B. *The OHP Standard Lottery*

Enrollment in OHP Plus was continuously open and children of adults eligible for OHP Standard were continuously eligible to enroll in OHP Plus. However, due to limited state budgets, new enrollment in OHP Standard had not been permitted since 2004. In 2008, the state had the budget sufficient to cover an estimated 10,000 additional adults but anticipated significant excess demand if enrollment were reopened without restriction. It therefore applied for and received permission from federal regulators to conduct a lottery.

For a five-week period in January and February 2008, the state allowed anyone to sign up for a list from which lottery draws would be taken. This list was known as the reservation list. When individuals signed up for the lottery, they were told to list members of their household ages 19 and older whom they wanted on the reservation list. Extensive measures were taken to encourage sign-up: individuals could enroll by multiple means (telephone, fax, in person, postal mail, and online) and the enrollment form was limited to only one page. In all, 89,824 adults joined the reservation list. The state did not initiate any contact with these individuals unless they won the lottery. It is unlikely that the adult lottery sign-up process had any direct impact on their children's enrollment; the brief sign-up form did not communicate information about child eligibility for Medicaid or ask anything about children in the household (see online Appendix Figure A1).

Following the sign-up period, the state began conducting lottery draws from the reservation list. It conducted eight draws in total, roughly one a month, from the first draw in March 2008 to the last draw in October 2008. Although individual names were selected in the drawings, the state considered all adults in the individual's household to have won the lottery. Ultimately, 35,169 individuals were selected in order to enroll 10,000 additional people in OHP Standard.

Our main analysis focuses on the impact of the lottery over the first year, i.e., through October 2009. The state did not contact any of the lottery losers during this time period. This is because unselected individuals did not lose the lottery on a specific date. Indeed, the individuals who lost the lottery during our study period eventually became lottery winners when the state conducted further lottery drawings starting in late 2009, after our main analysis period (see Finkelstein et al. 2016).

Adults selected in the lottery could apply for OHP Standard. The state mailed households an OHP enrollment form when they were selected. From the date of mailing, the household had 45 days to apply and submit the relevant documentation. The state encouraged selected households to submit their forms by mailing them a reminder and calling them to offer assistance (Oregon Department of Human Services 2008b). The state reviewed applications when they were received and, if it verified eligibility for an OHP plan, enrolled the participant with coverage retroactive to the weekday after the enrollment form was mailed. We call this date the "adult eligibility date."

Among those selected, about 60 percent applied for coverage. Selected adults may not have applied due to lack of awareness or lack of attention to the paperwork that was mailed, the burden of filling out the paperwork and providing the required supporting documentation, or a realization that they were unlikely to be eligible for coverage after reviewing the materials the state had sent. Indeed, even among those who applied, only about one-half were deemed eligible and successfully enrolled in Medicaid. The main reason for a rejected application was failure to meet the income requirement, which required the last quarter's income to correspond to an annual income below the poverty line.[2] For more details on the lottery and application process, see Finkelstein et al. (2012) and Finkelstein et al. (2010).

[2] As noted, income limits for children extended higher—to 185 percent of the federal poverty line—mostly due to CHIP eligibility. Thus, spillovers could occur even for lottery-list adults who were or would have been rejected

C. *Enrolling in OHP Plus*

At the time of the lottery, the state was continuously accepting new applications for OHP Plus. To initiate an application, anyone could make a request online, by phone, by mail, or in person. Those applying for OHP—whether they were selected from the reservation list or requested an application from the state—were sent the same 46-page packet, 19 pages of which contained fill-in prompts. At a minimum, applicants were required to fill out a four-page section that requested information about themselves and their household, including information about any children in the household. All applicants were also required to provide proof of address, citizenship (if they were US citizens), and income. Depending on the household's circumstances vis-à-vis eligibility, an applicant could be required to fill out any of an additional nine sections in the packet, typically one to two pages each.

All children of adults eligible for OHP Standard were eligible for OHP Plus regardless of whether adults in their household participated in (or won) the lottery. Nonetheless, a parent's winning the lottery might increase the chance of their children enrolling in OHP Plus by increasing awareness of their children's eligibility or reducing the transaction costs of enrolling them. The OHP application form asked the applicant to "list yourself and everyone living with you" and included a checkbox next to each name to request benefits for that person (see online Appendix Figure A2). The form therefore gave parents a nudge and an opportunity to request benefits for their children even if they were not aware of the eligibility rules. In addition, the staff who processed OHP Standard applications were instructed to "check to see if the applicant qualifie[d] for any other medical programs" (Oregon Department of Human Services 2008b). Staff may have interpreted this directive as encouraging them to check on the eligibility of children in the same households as applicants. Finally, when participants applied in person, case workers may have encouraged them to check the box on the application to enroll their children in coverage.

II. Empirical Framework and Data

A. *Empirical Framework*

Our analytic framework closely follows the standard approach used in prior analyses of the Oregon Health Insurance Experiment (see, e.g., Finkelstein et al. 2012). However, unlike prior studies, our unit of analysis is the household rather than the individual. We compare Medicaid enrollment for households selected by the lottery (the treatment group) to households who signed up for the lottery but were not selected (the control group). We look separately at adult Medicaid enrollment (which in prior work was considered the "first stage" of the experiment) and child Medicaid enrollment, which is the focus of our current analysis. These analyses were not prespecified.

due to high income. In practice, as discussed in the next footnote, we found no evidence of spillovers onto CHIP enrollment.

Our basic estimating equation is

$$(1) \qquad y_h = \beta_0 + \beta_1 LOTTERY_h + X_h \beta_2 + V_h \beta_3 + \varepsilon_h,$$

where the outcomes (y_h) are various measures of household h's Medicaid enrollment. We examine Medicaid enrollment for children and adults separately and at various time periods after the lottery. Our main analysis focuses on outcomes 90 days after the adult eligibility date—i.e., the weekday after the enrollment form was sent to winners of that lottery draw. Our main outcome is the number of children (or adults) enrolled. We also examine indicator variables for whether any children (or any adults) in the household were enrolled, as well as the number of child (or adult) member-months enrolled over the 90-day (3-month) period.

The indicator variable $LOTTERY_h$ takes the value of one if the household was selected by the lottery and zero if the household was on the reservation list but not selected by the lottery. The key coefficient of interest is β_1, which measures the impact of the household's lottery selection on enrollment.

We denote by X_h the set of covariates that are correlated with treatment probability (i.e., probability of winning the lottery). These covariates must be included for β_1 to be an unbiased estimate of the impact of winning the lottery. Treatment probability varies with the number of adults in the household that were 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. We therefore include indicator variables for the household size; 87 percent of households listed one member on the reservation list, 13 percent had two members, and less than 0.1 percent had three members. Lottery selection was random conditional on household size.[3]

We denote by V_h a second set of covariates that can be included to potentially raise statistical power because they are predictive of outcomes. These covariates are not needed for β_1 to give an unbiased causal estimate of the effect of lottery selection as they are independent of treatment status due to randomization, but they may improve the precision of the estimates. In our baseline analyses, we include indicators for the lottery draw as well as for four prelottery Medicaid enrollment measures (from January 15, 2008): number of reservation-list adults enrolled, any reservation-list adult enrolled, number of children enrolled, and any child enrolled. We show in robustness analyses below that results are similar but, as expected, less precise when prelottery enrollment measures are omitted.

To assign control households to lottery draws, we randomly allocated each control household to a lottery draw, stratified by household size; specifically, for each household size, lottery draws were randomly assigned to controls in proportion to the distribution of treatment households of that household size across the draws. This approach follows that in Finkelstein et al. (2012) and is motivated by the fact

[3] Finkelstein et al. (2012) provide more detail on how the lottery was conducted and verify that randomization was conducted as described.

that, as noted in Section IB, unselected adults on the lottery list did not lose the lottery on a specific draw. By randomly assigning lottery draws to control households, we can measure outcomes for both treatment and control households relative to each household's adult eligibility date (which varies by lottery draw) and include indicator controls for "lottery draw."

B. *Data Sources and Variable Construction*

We analyze two primary datasets provided by the State of Oregon: the reservation list and Medicaid enrollment data (Oregon DMAP 2008, 2016). The reservation list contains the information each individual provided at sign-up as well as whether they were selected by the lottery, and if so, in which lottery draw. The self-reported sign-up information consists of the name, address, sex, and date of birth of the individual signing up as well as anyone else in the household 19 years or older whom the individual wanted to add to the reservation list. All individuals on the reservation list are aged 19–64; there are no children on the list.

We have data on Medicaid enrollment for all Oregon Medicaid enrollees for three years, 2008 through 2010. These are spell-level data, which include the beginning and end date (if any) of the spell and the enrollee's name, date of birth and sex; the data also include address information with start and end dates for each location during the enrollment spell. We use these data to construct our outcome variables, which measure Medicaid enrollment over particular periods of time. Our main analyses focus on enrollment within the first year post lottery; in supplemental analyses, we show outcomes up to two years post lottery, the longest time period we can study before further lottery drawings starting in late 2009 ultimately treated the entire control group (see Finkelstein et al. 2016). The data contain both Medicaid and CHIP enrollment records. For our analysis, we count CHIP enrollment as a form of Medicaid enrollment.[4]

In order to measure the number of children and adults in each household who were enrolled in Medicaid, we use address information to match the reservation list to the Medicaid enrollment data. Online Appendix B provides more detail on this matching exercise. Briefly, we use ArcGIS to geocode addresses in both datasets, which returns a latitude–longitude coordinate pair for each address (accurate to 1.1 meters). We are able to geocode 80 percent of all addresses on the reservation list (or 91 percent, once we removed the 12 percent of addresses that listed a PO Box and therefore could not be geocoded) and 87 percent of the addresses in the Medicaid

[4] Because everyone who was eligible for the OHP Standard expansion had family income below 100 percent of the federal poverty line, we expect reservation-list children of "complier" adults (who gain or would gain coverage due to winning the lottery) to be eligible for traditional Medicaid and not CHIP. Consistent with this view, we only detected effects of the lottery on enrollment for income categories under 100 percent of the federal poverty line. Point estimates for higher income categories, including CHIP, were statistically and practically insignificant (online Appendix Figure A3). Still, we included CHIP in the analysis because the state did not verify eligibility of reservation-list households unless they won the lottery and applied for coverage. Thus, higher-income households could have entered and won the lottery; households may also have experienced income shocks between entering the lottery and winning. These households would not be able to enroll adults in OHP Standard but could end up having children covered under OHP Plus or CHIP.

enrollment data. We also extract and standardize apartment and unit numbers when available. We then match the geocoded addresses in the two datasets.

For each reservation-list household, we define the number of children enrolled in Medicaid as the number of children enrolled at the address the household provided on the reservation list. We define children as individuals under 19 years old on October 8, 2009, which is one year after the adult eligibility date for the last lottery draw. This ensures that they are children under Medicaid rules for the entirety of the main analysis period. We define the number of adults enrolled in Medicaid as the number of reservation-list members in the household who were enrolled at the address; to count as a match, the adult record must have the same date of birth and sex in both datasets.

Addresses on the lottery list were self-reported by households at the time of lottery sign-up, while addresses in the Medicaid data reflect the most recent address that Medicaid has on file. These addresses may differ. A potential threat to our research design would arise if the addresses of previously enrolled children were updated as a result of their parents winning the lottery, enrolling in Medicaid, and updating the addresses on file for the entire family. This scenario could spuriously lead us to find more children enrolled in Medicaid among lottery winners than lottery losers, even in the absence of any woodwork effect. To alleviate this concern, we use the first address on file in the enrollment data starting from January 1, 2008 to match the reservation-list households to Medicaid enrollment even if a Medicaid enrollee has a different subsequent address. We later show in robustness analyses that our findings are similar if we instead use contemporaneous addresses.

We expect measurement error in our outcome variables—counts of children and adults enrolled in Medicaid at each reservation-list household—arising from imperfect matching of Medicaid enrollees to households on the reservation list. This measurement error may include both false positives (the reservation-list household matches enrollment of other households) and false negatives (the reservation-list household has some members enrolled in Medicaid that we fail to match). Under the null hypothesis that winning the lottery has no spillover effect on child enrollment, false positives and false negatives are both expected to be balanced between randomly assigned treatment and control households. However, under the alternative hypothesis—that Medicaid eligibility for adults does have (positive) spillover effects on the enrollment of children—false negatives will disproportionately occur in treatment households because some of the children who enroll due to the spillover will not be matched. We thus expect attenuation bias in our estimated impact of lottery selection on our primary outcome, the number of children enrolled in Medicaid in the household.[5] Below, we use an alternative and arguably more precise measure of adult enrollment to estimate the extent of measurement error in our adult enrollment measures; under the assumption that the extent of mismeasurement is the same

[5] We study two other enrollment measures. The first is member-months of enrollment, where we expect attenuation bias under woodwork effects for the same reason as described above. The other is a binary indicator for any child enrollment; with this nonlinear transformation of the enrollment count, the bias in the estimated treatment effect is of indeterminate sign.

for child and adult enrollment, we show that adjusting for measurement error has little quantitative impact.

C. *Sample Definition and Summary Statistics*

Our study sample consists of households on the reservation list. Following Finkelstein et al. (2012), we exclude individuals and households who were not eligible for OHP Standard because they gave an address outside of Oregon, were not in the right age range, died prior to the lottery, had institutional addresses, were signed up by third parties, would have been eligible for Medicare by the end of our study period, or were inadvertently included on the original list multiple times by the state. This leaves us with the 74,922 individuals that formed the analysis sample of Finkelstein et al. (2012). These individuals represent 66,210 households, our unit of analysis.

We further restrict our analysis to the 53,147 (80.3 percent) of these households that have reservation-list addresses that we successfully geocoded. We exclude 274 of these households because they are above the ninety-ninth percentile of prelottery number of children enrolled in Medicaid and therefore are likely measured with substantial error.[6] We explore robustness to our handling of outliers below. The final analysis sample consists of 52,873 households.

Table 1 shows descriptive statistics for variables measured prerandomization. We show statistics for control-group households and also report estimates of treatment–control differences. Panel A shows variables derived from the self-reported information provided on the reservation list and panel B shows four measures of prelottery Medicaid enrollment (specifically, as of January 15, 2008). The average age of the household member who signed up for the reservation list was 40, 58 percent were women, and 93 percent listed English as their preferred language; the median income in the household's ZIP code was, on average, \$39,774. Prior to randomization, 22 percent of households had at least one child enrolled in Medicaid and, conditional on enrollment, 1.9 children were enrolled. Consistent with prior work (Finkelstein et al. 2012), only a small fraction (3 percent) of households had a reservation-list adult enrolled before randomization. Columns 2 and 3 look at the treatment–control balance of these variables. Only one of the 11 measures—sex—is imbalanced between treatment and control (as it was in the sample analyzed in Finkelstein et al. 2012). Prelottery Medicaid enrollment is statistically indistinguishable between treatment and control (panel B), which suggests that children gaining coverage did not receive it retroactive to before the date on which we measure baseline enrollment. This is consistent with documentation from the state that coverage for adults was retroactive to a later date—the weekday after the enrollment form was sent to the household, which we have called the adult eligibility

[6] The prelottery measure of enrolled children is taken on January 15, 2008. Among households with a successfully geocoded address, the ninety-ninth percentile of the measure is five children enrolled. The exclusion above the ninety-ninth percentile is designed to reduce the chance that we inadvertently matched a reservation-list household to a large number of children outside that household; for example, a household in an apartment complex that failed to provide a unit number on the reservation list would match to all children in the building without a unit number in their Medicaid addresses.

Table 1—Treatment–Control Balance

Variable	Control mean (1)	Treatment–control difference (2)	p-value (3)
Panel A. Lottery-list variables			
Year of birth	1,968.4	0.132 (0.112)	0.236
Female	0.577	−0.011 (0.004)	0.017
English as preferred language	0.927	0.001 (0.002)	0.599
Signed up first day of lottery	0.093	0.001 (0.003)	0.661
Gave phone number	0.863	−0.005 (0.003)	0.094
In MSA	0.821	−0.002 (0.004)	0.524
Zip code median household income	39,774.1	8.825 (77.785)	0.910
Panel B. Baseline-enrollment variables			
Number children enrolled	0.416	0.007 (0.009)	0.439
Any children enrolled	0.218	0.003 (0.004)	0.399
Number reservation-list adults enrolled	0.027	0.001 (0.002)	0.491
Any reservation-list adults enrolled	0.026	0.001 (0.002)	0.498

Notes: The number of operations is 52,873. This table presents balance tests for two sets of variables. Specifically, it reports estimates of β_1 (the coefficient on an indicator for the household winning the lottery) from equation (1). The regressions control for household-size indicators but do not control for lottery-draw indicators or the measures of baseline Medicaid enrollment (which appear in Block B). Robust standard errors are in parentheses. Column 1 reports the average control group outcome. Column 2 presents the estimated regression coefficient and its standard error, which is the treatment–control difference. Column 3 reports the p-value from the test that the regression coefficient equals zero. Block A, which reports the lottery-list variables, contains demographics of individuals who signed up for the lottery, which were provided by participants or could be derived from this information. Block B, which reports the baseline-enrollment variables, contains the four measures of child and adult enrollment on January 15, 2008 at the household level derived from our linkage to Medicaid enrollment data. See Appendix Table A1 for balance tests for additional variables and comparisons to balance in prior work.

date in this manuscript—and supports our use of these covariates to raise statistical power, although we will also show robustness to omitting them.[7]

Finally, to estimate the number of children "at risk" of gaining coverage through the woodwork effect, we draw on additional data from a mail survey administered around the time of the lottery drawings to a random 75 percent of our analysis subsample of 52,873 households (The Oregon Health Study Group 2010); Section VC of Finkelstein et al. (2012) provides more detail on this survey. In our analysis subsample, the survey had an effective response rate of 46 percent. Among respondents, the average number of children per household was 0.85. We fail to reject the hypothesis of treatment–control balance in survey response rates ($P = 0.09$) and in children per household among respondents ($P = 0.20$). Control-group households that

[7] The sample analyzed here differs from the one analyzed in Finkelstein et al. (2012) in two respects. First, it is limited to households with addresses we could geocode; this meant, in particular, that we omitted the 12 percent of households on the reservation list that provided PO boxes for their addresses because they could not be geocoded. Second, we analyze outcomes at the household level rather than the individual level. For completeness, online Appendix Table A1 shows all of the variables in Table 1—as well as previously used prerandomization measures of hospital-utilization derived from a linkage to hospital-discharge data (see Finkelstein et al. 2012 for more details)—for our household-level analysis sample (column 1), the full household-level analysis sample based on the analysis sample in Finkelstein et al. (2012) (column 2), and the individual-level analysis sample analyzed in Finkelstein et al. (2012) (column 3). Online Appendix Table A2 then shows balance tests for each of these three samples and for each of the three sets of variables (where feasible) as well as omnibus tests of balance across all the available sets of variables. We are unable to reject the null hypothesis that the covariates are balanced across treatment and control groups for all ten of these tests.

responded to the survey averaged 0.47 children enrolled in Medicaid in the enrollment data. While these numbers come from different sources (survey responses among the subsample of responders versus matched administrative data for them)[8] and cover slightly different time periods, together they allow us to form a rough estimate of the size of the risk set: with 0.85 children per household less 0.47 children enrolled, we estimate that about 0.4 children could have potentially gained coverage per lottery household.

III. Results

A. *Spillover Estimates*

Figures 1 and 2 illustrate the time path of effects of winning the lottery on child enrollment and on adult enrollment. Both graphs plot treatment effects on the number of children or adults enrolled at varying times relative to the date of adult eligibility—the date that coverage would begin for adults who enrolled due to the lottery draw. The adult eligibility date is denoted with a dashed vertical line. We plot the estimated effects every 30 days from 30 days prior to adult eligibility to 360 days after, which corresponds to our analysis period of one year post adult eligibility.

Figure 1 shows the impact of lottery selection on the enrollment of the household's children. As expected, effects prior to the adult eligibility date are substantively and statistically insignificant. Child enrollment exhibits a large, concentrated increase immediately after adult eligibility begins. Figure 2 shows that the timing of the increase in child enrollment mirrors the timing of the increase in adult enrollment; this is consistent with children and adults applying for OHP together and the state enrolling them with roughly the same start dates. Both child and adult enrollment effects peak around 90 days and decline after that.

Table 2 presents point estimates of the coverage effects measured at 90 days after adult eligibility. Winning the lottery increases the expected number of children enrolled by 0.024. This represents about one child for every 41 winning households, or about a 3 percentage point increase relative to the 0.85 children per household that we estimated. We find a significant effect on the extensive margin of any child enrollment: winning the lottery increases the probability that a household enrolls at least one child by 1.3 percentage points. We also find effects on member-months, i.e., the total months of enrollment for all children in the household during the 90 days (3 months) following adult eligibility. Winning the lottery raises child member-months by 0.07. All of these effects are statistically significant at the <0.01 percent level.

Our baseline estimate of the spillover effect on the number of children enrolled is only about 6 percent of the 0.4 children that could have potentially gained coverage in the average household when adults applied. Thus the woodwork effect we estimate, while statistically significant, is only a fraction of its potential size. It is also substantially smaller than the direct effect of the lottery on adult enrollment.

[8] We suspect nonrespondents have similar average numbers of children because we estimate the average number of children enrolled in Medicaid to be 0.47 for control households that responded to the survey and 0.50 for control households that failed to respond.

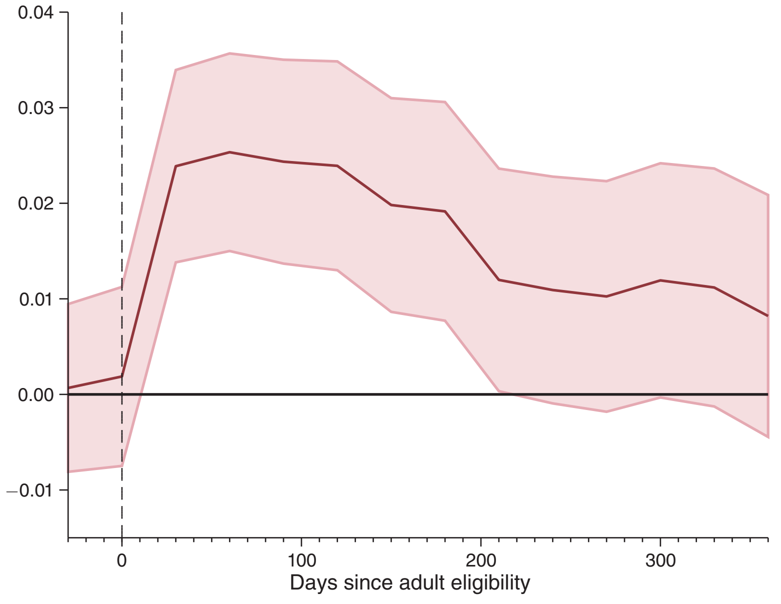

FIGURE 1. EFFECT OF WINNING THE LOTTERY ON NUMBER OF CHILDREN ENROLLED

Notes: This figure presents estimates of the effect of a household winning the lottery on the number of children in the household enrolled in Medicaid. Specifically, it plots estimates of β_1 (the coefficient on an indicator for the household winning the lottery) from equation (1); the outcome variables are the number of children enrolled at different 30-day durations (from −30 to 360) relative to the adult eligibility date. All regressions also control for household-size indicators, lottery-draw indicators, and the measures of baseline Medicaid enrollment. The shaded area indicates the 95 percent confidence interval for the effect estimates based on robust standard errors.

Winning the lottery increased adult enrollment by 0.22 (Table 2), indicating that for every nine adults who enrolled in Medicaid due to the lottery, one child also enrolled in Medicaid.

The magnitude of the spillover effect relative to the direct effect is likely even smaller when considered in terms of expenditures rather than enrollment. We do not directly observe Medicaid spending in our data. To approximate expenditures per child enrolled, we therefore estimate spillover effects by child Medicaid eligibility category and age, two child characteristics we do observe, and approximate costs using state per capita Medicaid cost projections by eligibility category groups and age (PriceWaterhouseCoopers 2006). As might be expected, essentially all children drawn into Medicaid due to the woodwork effect were eligible through the "below 100% FPL household" category, the same criterion that allowed their parents to enroll upon winning the lottery (online Appendix Figure A3). There was no detected effect on enrollment through any other eligibility category grouping. In particular, we did not detect an effect on disability-related categories for which the health care spending per enrollee is likely much higher. Estimates of spillover effects by child age suggest positive point estimates at most ages but show no obvious pattern and are quite noisy (online Appendix Figure A4). The state estimates suggest that the average cost per child for a child who enrolled due to the spillover effect was about \$150 per month. This is about one-fourth the average per capita cost of covering

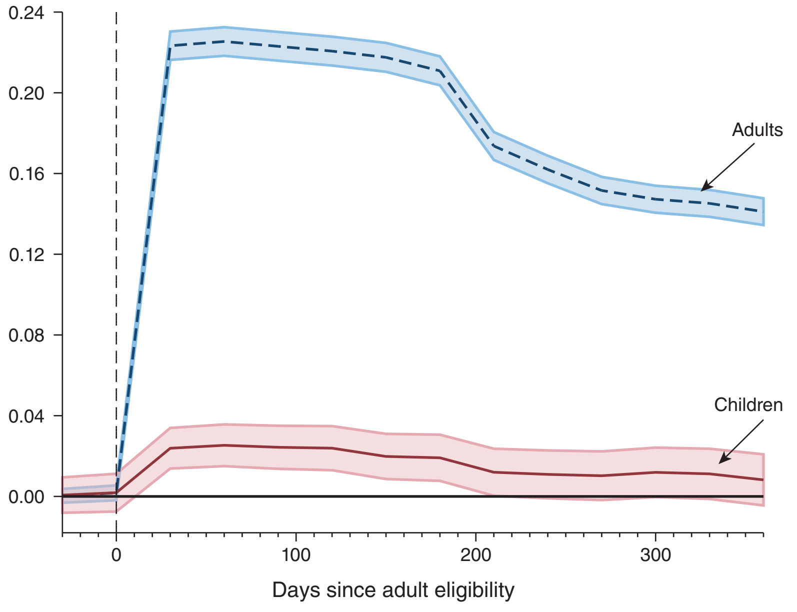

Figure 2. Effect of Winning the Lottery on Number of Adults and Children Enrolled

Notes: This figure presents estimates of the effect of a household winning the lottery on the number of reservation-list adults in the household enrolled in Medicaid (dashed blue line) and the number of children in the household enrolled in Medicaid (solid maroon line). Specifically, it plots estimates of β_1 (the coefficient on an indicator for the household winning the lottery) from equation (1); the outcome variables are the number of children (or number of adults) enrolled at different 30-day durations (from −30 to 360) relative to the adult eligibility date. All regressions also control for household-size indicators, lottery-draw indicators, and the measures of baseline Medicaid enrollment. The shaded areas indicate the 95 percent confidence interval for the effect estimates based on robust standard errors.

Table 2—Effects on Child and Adult Medicaid Enrollment at 90 Days

Outcome	Control mean (children) (1)	Treatment effect (children) (2)	Treatment effect (adults) (3)	Effect ratio child:adult (4)
Number enrolled	0.457	0.024	0.223	0.110
		(0.005)	(0.004)	
Any enrolled	0.234	0.013	0.205	0.062
		(0.003)	(0.003)	
Member-months	1.372	0.074	0.667	0.110
		(0.015)	(0.011)	

Notes: The number of observations is 52,873. This table presents estimates of the effect of a household winning the lottery on child and reservation-list adult Medicaid enrollment outcomes 90 days after the adult eligibility date. Specifically, it reports estimates of β_1 (the coefficient on an indicator for the household winning the lottery) from equation (1); all regressions also control for household-size indicators, lottery-draw indicators, and the measures of baseline Medicaid enrollment. Robust standard errors are in parentheses. Column 1 reports the average control-group child-enrollment outcome. Columns 2 and 3 present treatment-effect estimates on child and adult enrollment, respectively. Column 4 reports the ratio of child to adult treatment effects. The rows report results from three different dependent variables. "Number enrolled" is the count of members enrolled in Medicaid at 90 days after adult eligibility. "Any enrolled" is an indicator for the number enrolled being greater than zero. "Member-months" is the total months of enrollment at the household during the 90-day period following adult eligibility.

Table 3—Effects on Medicaid Enrollment at Varying Durations

Outcome: Number enrolled	Control mean (children) (1)	Treatment effect (children) (2)	Treatment effect (adults) (3)	Effect ratio child:adult (4)
30 days after adult eligibility	0.455	0.023	0.224	0.103
		(0.005)	(0.004)	
90 days after adult eligibility	0.457	0.024	0.223	0.110
		(0.005)	(0.004)	
180 days after adult eligibility	0.462	0.020	0.211	0.093
		(0.006)	(0.004)	
270 days after adult eligibility	0.472	0.010	0.152	0.068
		(0.006)	(0.003)	
365 days after adult eligibility	0.484	0.008	0.141	0.059
		(0.006)	(0.003)	

Notes: The number of observations is 52,873. This table presents estimates of the effect of a household winning the lottery on child and reservation-list adult Medicaid enrollment outcomes at varying durations after the adult eligibility date. Specifically, it reports estimates of β_1 (the coefficient on an indicator for the household winning the lottery) from equation (1); all regressions also control for household-size indicators, lottery-draw indicators, and the measures of baseline Medicaid enrollment. Robust standard errors are in parentheses. Outcomes are the number of children or adults enrolled in Medicaid at the specified number of days after the adult eligibility date. Column 1 reports the average control-group child-enrollment outcome. Columns 2 and 3 present treatment-effect estimates on child and adult enrollment, respectively. Column 4 reports the ratio of child to adult treatment effects.

an OHP Standard adult, the group the state sought to expand directly through the lottery.

Finally, we explore the time pattern of enrollment effects. The initial Medicaid coverage period for children (or adults) was the six months after enrollment began, excluding the first calendar month. To retain coverage beyond this point, the state required both adults and children to reapply and demonstrate that they were still eligible (Oregon Department of Human Services 2008c). Figure 1 shows that a decline in the treatment effects on the number of children covered occurs roughly 180 to 210 days after adult eligibility. The timing suggests that some of the children who gained coverage through woodwork effects did not recertify their eligibility.

Table 3 quantifies how the woodwork and direct effects decline over time. One year after the lottery, woodwork effects are one-third the magnitude of the 90-day estimate and are no longer statistically significant. Effects for adults also decline, but at a somewhat slower rate. As a result, whereas at 90 days 9 adults gain coverage for every child, at one year the ratio rises to 17 covered adults per covered child.

To better understand the sources of attenuating treatment effects, Figure 3 plots the average number of children enrolled in the treatment and control groups at 30-day intervals from the adult eligibility date.[9] For comparison, Figure 4 plots the analogous estimates for adult enrollment. For both groups, the figures show that

[9] Comparing raw averages for the treatment and control groups does not generally yield the lottery effect because winning was a function of household size. To account for this issue and to ensure that the averages align with the treatment effects depicted in Table 3, we calculate adjusted averages based on the regression estimates. Specifically, after running the regression we use the coefficient estimates to predict the enrollment, first assuming that all households were treated and then assuming all households were not.

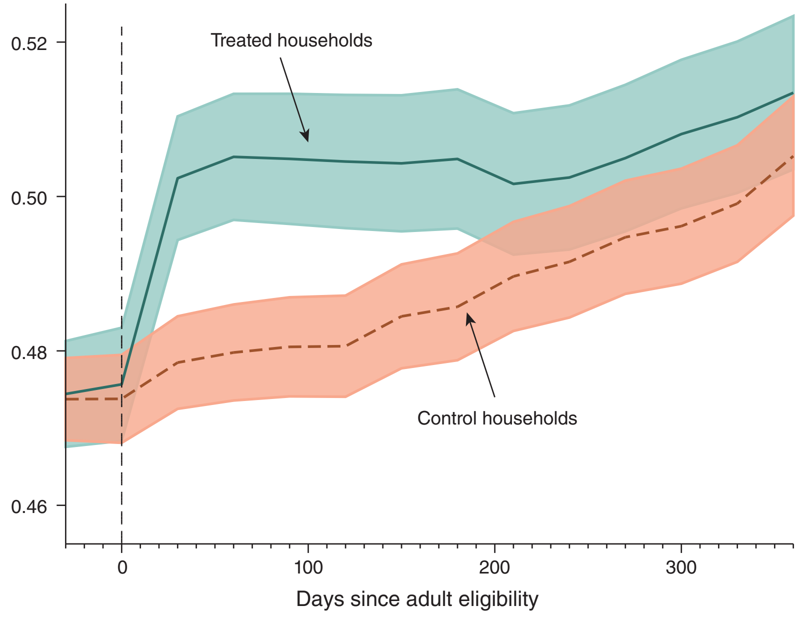

Figure 3. Average Number of Children Enrolled in Treated and Control Households

Notes: This figure presents the average number of children in treatment and control households enrolled in Medicaid at different 30-day durations (from −30 to 360) relative to the adult eligibility date. The averages are adjusted estimates derived from the regression given by equation (1). Specifically, after running the regression, we use the coefficient estimates to predict the enrollment assuming all households were treated and then assuming all households were not. The "treated households" estimate can therefore be interpreted as the expected average level of enrollment if all households in the analysis sample were treated, while the "control households" estimate is the expected average if all households were not treated. The shaded areas indicate 95 percent confidence intervals for the adjusted averages.

two factors contribute to the attenuation of the treatment effects: a drop-off in the enrollment of the treatment group when recertification is required (180–210 days from adult eligibility), and a secular increase in enrollment in the control group. For children (Figure 3), the latter effect appears quantitatively much more important, suggesting that the woodwork effect often acts to hasten the enrollment of eligible children who would otherwise have gained coverage within the year. For adults (Figure 4), the decline in treatment-group enrollment around the recertification period appears to be the main driver of attenuation; the only way control-group adults (who lost the lottery) could enroll in Medicaid is if they became categorically eligible for OHP Plus.

In the online Appendix, we extend the analysis of the treatment effects out to 720 days for both children and adults (online Appendix Figures A5 and A6). The estimates become somewhat noisier as they extend past the one-year mark because we must increasingly up-weight a portion of the study population to adjust for a new lottery for OHP Standard that the state conducted beginning in fall 2009 (see Baicker et al. 2013 and Finkelstein et al. 2016 for more detail). Our finding of economically small and statistically insignificant woodwork effects at one year continues to hold over this longer horizon.

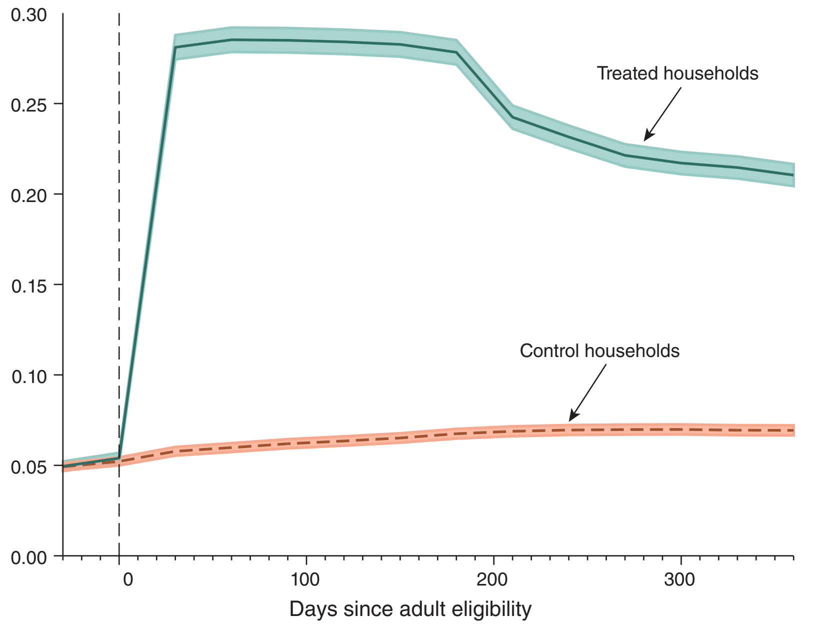

FIGURE 4. AVERAGE NUMBER OF ADULTS ENROLLED IN TREATED AND CONTROL HOUSEHOLDS

Notes: This figure presents the average number of adults on the reservation list in treatment and control households enrolled in Medicaid at different 30-day durations (from −30 to 360) relative to the adult eligibility date. The averages are adjusted estimates derived from the regression given by equation (1). Specifically, after running the regression, we use the coefficient estimates to predict the enrollment assuming all households were treated and then assuming all households were not. The "treated households" estimate can therefore be interpreted as the expected average level of enrollment if all households in the analysis sample were treated, while the "control households" estimate is the expected average if all households were not treated. The shaded areas indicate 95 percent confidence intervals for the adjusted averages.

B. *Heterogeneity in Spillover Effects*

We explore potential heterogeneity in spillover effects along several dimensions. First, we consider the coverage gains of previously unenrolled children relative to the retention of coverage by previously enrolled children. To do so, we separately analyze spillover effects counting only children who were not enrolled in Medicaid prior to randomization and counting only children who were enrolled previously. Effects are statistically significant on both outcomes (Table 4, panel A), but the gains are concentrated in previously unenrolled children, where the point estimate amounts to about three-fourths of the total enrollment effect. This result suggests that woodwork effects primarily enroll previously unenrolled children, with smaller effects on the retention of the previously enrolled.

We also explore whether the woodwork effect is concentrated in the three quarters of households that did not already have a child enrolled in Medicaid, compared to the quarter that had some ex ante child enrollment (Table 4, panel B). For households without prior enrollment, effects are similar in magnitude to the full sample and highly statistically significant. Effects for households with prior enrollment are also similar in magnitude but are measured more imprecisely, at least partly

Table 4—Heterogeneity in Effects on Child Enrollment

	Panel A. Outcome measure only counts children				*Panel B. Sample restricted to households with*			
	Not enrolled ex ante		Enrolled ex ante		No child enrolled ex ante		≥1 child enrolled ex ante	
Outcome	Control mean (1)	Treatment effect (2)	Control mean (3)	Treatment effect (4)	Control mean (5)	Treatment effect (6)	Control mean (7)	Treatment effect (8)
Number enrolled	0.111	0.018 (0.004)	0.346	0.007 (0.003)	0.096	0.023 (0.005)	1.750	0.032 (0.017)
Any enrolled	0.073	0.012 (0.002)	0.185	0.002 (0.002)	0.059	0.014 (0.003)	0.859	0.010 (0.006)
Member-months	0.303	0.057 (0.012)	1.069	0.016 (0.008)	0.261	0.074 (0.014)	5.352	0.076 (0.044)
Observations	52,873		52,873		40,856		12,017	

Notes: This table presents estimates of the effect of a household winning the lottery on child Medicaid enrollment outcomes 90 days after the adult eligibility date. Specifically, it reports estimates of β_1 (the coefficient on an indicator for the household winning the lottery) from equation (1); all regressions also control for household-size indicators, lottery-draw indicators, and the measures of baseline Medicaid enrollment. Robust standard errors are in parentheses. The rows report results from three different dependent variables. "Number enrolled" is the count of members enrolled in Medicaid at 90 days after adult eligibility. "Any enrolled" is an indicator for the number enrolled being greater than zero. "Member-months" is the total months of enrollment at the household during the 90-day period following adult eligibility. In panel A, the outcome measures are defined as in Table 2 but only count children who were not enrolled ex ante (on January 15, 2008) on the left side and only count children who were enrolled ex ante on the right side. In panel B, the outcome measures are defined identically to those in Table 2, but the sample is split into households with no ex ante child enrollment (on January 15, 2008) on the left side and households with ex ante child enrollment on the right side.

reflecting the smaller sample. These findings suggest that effects may be similar for both household types.

Finally, we limit the analysis to the sample of households that reported having children in survey data (Table 5). Spillover effects are similar in the baseline sample and the subsample that answered the survey (panels A and B). As expected, treatment effects are larger for survey-respondent households that reported having children (panel C). Enrollment rose by 0.04 children per household due to the lottery, a 52 percent larger treatment effect than that for all survey respondents. Since the average household in this sample reported two children in the survey and average enrollment in the control arm was one child, this result suggests that the woodwork effect represents about 4 percent of the children not enrolled in Medicaid prior to the lottery, similar to the 6 percent share we estimated for the full analysis sample in the previous section.

C. *Sensitivity Analysis*

Mismeasurement of addresses will create false negatives in our matching of reservation-list households to their enrollment data and, in the presence of woodwork effects, can attenuate our estimates (see Section IIB). To gauge the potential magnitude of this attenuation bias, we make use of an alternative—and arguably more accurate—measure of adult Medicaid enrollment, which was produced by the state Division of Medical Assistance Programs (DMAP) and used in prior Oregon study

Table 5—Effects on Child Enrollment for Households with Children

	Panel A. Baseline specification		*Panel B. Survey subsample*		*Panel C. Survey subsample with children*	
Outcome	Control mean (1)	Treatment effect (2)	Control mean (5)	Treatment effect (6)	Control mean (7)	Treatment effect (8)
Number enrolled	0.457	0.024	0.469	0.029	1.008	0.044
		(0.005)		(0.009)		(0.018)
Any enrolled	0.234	0.013	0.246	0.015	0.518	0.025
		(0.003)		(0.004)		(0.008)
Member-months	1.372	0.074	1.396	0.095	2.999	0.159
		(0.015)		(0.024)		(0.050)
Observations	52,873		17,126		7,317	

Notes: This table presents estimates of the effect of a household winning the lottery on child Medicaid enrollment outcomes 90 days after the adult eligibility date. Specifically, it reports estimates of β_1 (the coefficient on an indicator for the household winning the lottery) from equation (1); all regressions also control for household-size indicators, lottery-draw indicators, and the measures of baseline Medicaid enrollment. Robust standard errors are in parentheses. The rows report results from three different dependent variables. "Number enrolled" is the count of members enrolled in Medicaid at 90 days after adult eligibility. "Any enrolled" is an indicator for the number enrolled being greater than zero. "Member-months" is the total months of enrollment at the household during the 90-day period following adult eligibility. Panel A repeats estimates from the baseline specification (see Table 2). Panel B presents these estimates for the subset of households that responded to the initial Oregon study participant survey. Panel C presents the estimates for the subset of households that responded to the survey and stated that they had one or more family members under age 19 living in their home.

analyses.[10] We estimate the ratio of treatment effects on adult enrollment (i.e., β_1 from equation 1) from the address-based measure of enrollment to the DMAP-based measure. Online Appendix Table A3 presents the two enrollment estimates as well as the correction (i.e., their ratio), which ranges from about 0.71 to 0.73 depending on the time frame; in other words, the address-based matching yields estimated treatment effects for adult enrollment that are 27 to 29 percent lower than the DMAP-based matching approach. Under the assumption that the rate at which we fail to capture Medicaid enrollment for reservation-list adults is the same as for their children, we can then apply the same correction factor to the estimated treatment effects for children. This procedure increases the estimated impact on the number of children enrolled at 90 days from 0.024 to 0.034 (online Appendix Table A3). Of course, to the extent that even the DMAP-based matching has measurement error, these corrected estimates (for both adults and children) may still understate enrollment effects.

Online Appendix Table A4 explores additional robustness exercises. Column 1 replicates the baseline results from Table 2. Subsequent columns show sensitivity to specific alternatives, with results that are generally similar to baseline. Column 2 omits controls for prerandomization Medicaid enrollment: we control only for household size and lottery draw. As expected, given the use of these controls in the

[10] To examine the two different measures of adult Medicaid enrollment, we studied their agreement for the 52,873 reservation-list household heads in the analysis sample in December 2008. The results are consistent with a lower rate of false negatives for the DMAP measure. Specifically, both yielded the same enrollment status for the vast majority of adults (92 percent), but when they disagreed, it was largely because the DMAP measure detected enrollment when the address-based measure did not (7 percent) rather than vice versa (1 percent).

baseline specification to raise power, treatment effects are similar but measured more imprecisely when omitting them. Column 3 uses contemporaneous addresses rather than the first observed address to match reservation-list households to Medicaid enrollment data. Using contemporaneous addresses is appealing because it is possible that the initial addresses in the enrollment data could be out of date, leading to mismeasurement when we match the reservation list to enrollment. However, this approach could lead to upwardly biased estimates if, for example, the state updates children's addresses when their parents enroll in Medicaid. Compared to the baseline specification, effects are slightly larger using the contemporaneous-address approach.

Columns 4 and 5 explore alternative approaches to handling outliers. In column 4, we take a more draconian approach, further omitting households above the ninety-fifth percentile (more than three enrolled children) rather than our baseline approach of omitting households above the ninety-ninth percentile (more than five enrolled children); the estimates are quite similar, showing that lesser outliers do not drive our findings. In column 5 we make no outlier exclusion, adding back the 275 outlier households representing just 0.5 percent of the overall sample. This change shrinks estimates of the effect on the number of children enrolled by about 40 percent and more than doubles the standard error so that the woodwork effect is no longer statistically significant. We suspect that results including outliers are substantially contaminated by measurement error: the outlier households have a median prerandomization enrollment of 7 children and a mean of 11; some are (implausibly) matched to hundreds of enrolled children. Not surprisingly, the estimates of the woodwork effect on whether a household has any children enrolled are essentially unaffected by the treatment of outliers.

IV. Conclusion

We use the 2008 randomized expansion of adult Medicaid eligibility in Oregon to better understand the magnitude and duration of woodwork or spillover effects of Medicaid eligibility expansions on populations that were already Medicaid eligible. We find clear evidence of woodwork effects: for every nine adults who gained coverage from the expansion, so did one already-eligible child. While statistically significant, the increase in the number of eligible children who enrolled in Medicaid represents only about 5 percent of our estimated number of children of lottery-list adults who could have enrolled. Because the marginal enrolled child has about one quarter of the spending level of the typical adult in the low-income Medicaid pool the state intended to expand, the fiscal consequences of these spillover effects are even smaller than the enrollment numbers suggest.

Both the direct effect on adult enrollment and the spillover effect on child enrollment fade over the subsequent year. While the decline in direct effects is mostly driven by disenrollment of adults due to recertification rules, the decline in spillover effects is driven primarily by children in control households enrolling in Medicaid. This suggests that the spillover effect may primarily cause earlier enrollment of already-eligible children who would otherwise have enrolled soon thereafter.

In the last decade, the United States has moved closer to universal insurance eligibility by making both Medicaid and subsidized private health insurance available to a much broader population. Our findings, estimated from an earlier and smaller Medicaid eligibility expansion for a group similar to those covered by more recent Medicaid expansions, shed light on the determinants of incomplete take-up of Medicaid. The time pattern of the spillover effects—occurring contemporaneously with the direct enrollment effects—is consistent with both information frictions and application costs limiting take-up. That said, the magnitude of the effects we estimate casts some doubt on the potential for large spillovers from expanding Medicaid eligibility for adults on Medicaid enrollment of their already-eligible children. Taken together, the findings highlight the continuing challenges and opportunities that policy makers will face in translating increases in Medicaid eligibility into increases in Medicaid enrollment.

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