On-Line Appendix To:

The Impact of Medicaid on Labor Force Activity and Program Participation: Evidence from the Oregon Health Insurance Experiment

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Appendix

Note: Virtually all the analyses reported here were pre-specified and publicly archived on January 22, 2013 at hypotheses@povertyactionlab.org. The results of a few additional post hoc analyses are marked with the symbol ^ when presented.

Data

SSA Data on Outcomes

We use SSA data to measure annual employment, earnings, and benefit receipt primarily from 2007 – 2009 (in some supplemental analyses described below, we explore 2006 data as well). 2007 is prior to the lottery; 2008 is the year the lottery was conducted and 2009 is post-lottery for everyone.

Data on annual earnings comes from the Master Earnings File which contains W-2 annual wage information for each job the individual held during the year as well as self employment income from schedule SE. We use this to code up individual annual earnings, and to break out earnings into wage and self-employment earnings. Data on annual SSDI benefits comes from the Master Beneficiary Record which is the master file for the Title II program. Data on annual SSI benefits comes from the Supplemental Security Records file, which is the master file for the title XVI program. These data on earnings and benefit receipt have been used in other recent studies of employment, earnings and benefit receipt (see e.g. Song and Manchester 2007, von Wachter, Song and Manchester 2011).

Additional Data

We obtained pre-randomization demographic information that the participants provided at the time of lottery sign-up. We use these data to match our study participants to the SSA records (see above) and also to construct nine ''lottery list variables'' that we use to examine treatment and control balance on pre-randomization demographics.[1] We obtained state administrative records on the complete Medicaid enrollment history of lottery list participants from prior to the lottery through the end of 2009. We use these data as our primary measure of the first-stage outcome (i.e., insurance coverage). In addition, we obtained state administrative records on the Food Stamp (SNAP) and TANF benefit history of lottery list participants from prior to the lottery through the end of 2009. We use these records on benefit receipt and amount granted to the household to generate a more complete definition of income and benefits. Additional information on each of these data sources is provided in the appendix of Finkelstein et al (2012).

Matching the Oregon Sample to SSA Records

We attempted to match the full Oregon Health Insurance Experiment analytic sample (N=74,922) to the Social Security Administration (SSA) records. We used full name and date of

[1] The nine "lottery list variables" provided at the time of sign up for the lottery are: age, gender, whether English is the preferred language for receiving materials; whether the individuals signed themselves up for the lottery or were signed up by someone else; whether they provided a phone number on sign-up; whether the individuals gave their address as a PO box; where the address they list is in an MSA; whether they signed up the first day the lottery list was open; and the median household income in the 2000 census from the zip code they gave.

birth (provided at time of lottery sign-up) to match them to SSA's masterfile of all SSNs ever issued; in addition to SSN, these data set includes last name, first name, middle initial, date of birth, sex, place of birth and date of death (if any). It is updated to account for new information (such as a name change or a birth date correction). We linked to the data as of the end of 2007 (i.e. right before the lottery). We then used SSN to link individuals to outcomes data on employment, benefits and earnings.

Due to large file sizes, we conducted the match in three steps.

Step 1: We searched for identical matches on first name, last name, and date of birth using SAS. 80.8% of our Oregon Health Insurance Experiment analytic sample (controls) was matched uniquely to a Social Security Number using this method.

Step 2: We then limited the remaining SSA records to ones that were sufficiently similar to one of the remaining unmatched Oregon records using SAS. Two records were considered "sufficiently similar" if they have identical date of birth and (1) they have similar first names[2] or (2) they have similar last names or (3) their first names and last names are both similar but the orders are reversed. We then probabilistically matched this subset of SSA records to the remaining unmatched Oregon records in Linkplus using first name, last name, and date of birth. We manually reviewed the pairs matched in Linkplus and picked those that we were "almost certain" (as a subjective matter) were true matches (we picked a conservative threshold in this step because most of the unmatched records in this step could still be matched in Step 3). Another 0.6% of the Oregon Health Insurance Experiment analytic sample (controls) was matched this way.

Step 3: We then allowed for the possibility that there could be data entry errors in the date of birth field in either the Oregon reservation list or the SSA records. From SSA records that had not been matched in the previous two steps, we used SAS to extract only the ones that

1. Had both a similar first name and a similar last name as one of the remaining unmatched Oregon reservation records or had a similar name with first and last name reversed as one of the remaining unmatched Oregon reservation records ("similar" defined as above)

AND

2. Had a similar date of birth[3] as that same Oregon reservation record

In Linkplus, we probabilistically matched SSA records that fit these criteria to the remaining unmatched records from the Oregon reservation list on first name, last name, and date of birth. Once again we manually reviewed the pairs matched in Linkplus and identified pairs that we thought were likely to be true matches (using our subjective assessment of 80% or more

[2] "Similar" names were defined using the "soundex" algorithm in SAS. The goal of the algorithm is for homophones to be encoded to the same representation so that they can be matched despite minor differences in spelling. For example, the function considers Katherine, Catherine, and Kate the same name.

[3] "Similar" dates of birth were defined as follows. Two dates of birth, DMY1 and DMY2 (D = day, M = month, Y = year) were considered "similar" if: M1 = M2, Y1=Y2 (D1 may differ from D2 arbitrarily) OR D1 = D2, Y1 = Y2 (M1 may differ from M2 arbitrarily) OR D1 = D2, M1 = M2, Y1 differs from Y2 by 1 digit or transposition OR D1 = M2, M1 = D2, Y1 = Y2.

likely to be a match as our definitely of "likely true match"). We were able to match another 3.5% of the Oregon analysis sample (controls).

In total, we successfully matched 84.9% of the Oregon Health Insurance Experiment analytic sample (controls) to a unique Social Security Number. We use this matched sample for all the analyses in this paper, including the analyses of TANF and SNAP that do not require SSA data. Our analyses of outcomes in the main text use 2009 data. Table A1 reports the control mean, standard deviation and % positive for each continuous analytic variable; it also reports moments of the distribution control on positive value.

Weights

In the fall of 2009, the state of Oregon began conducting a new lottery for OHP Standard. For the first draw, the state mailed postcards to those on the original list who were not selected (our controls). Those who returned the postcard were added to the new waiting list and an initial draw was done just from that group. After the drawing, we probabilistically matched (using LinkPlus software) the new waiting list to our study population to identify individuals who were eligible for selection by the state (called "opt-ins") and those who were actually selected (called "selected opt-ins"). As with the original lottery, the draw was done on individuals, but the opportunity to apply for OHP (treatment) was extended to the whole household.

We exclude from our analytic sample individuals selected in this new lottery drawing. We adjust for this using weights constructed on the following principle: within any (even non-random) subset of the original sample base, a randomly selected group can be weighted to stand in for the non-selected remainder based on the probability of that random selection without introducing bias. We can thus construct a weight that corrects for the initial new lottery drawing done in the fall of 2009. The weights are designed to insure our analytic sample is balanced on treatment status.

We use inverse probability weights. Individuals in households that did not sign-up for the new lottery are assigned a weight of one. Individuals in households that signed-up for and were selected in the new lottery are assigned a weight of zero (dropped from the analysis). Individuals in households that signed-up for and were not selected in the new lottery are assigned a weight which is the inverse of the probability of remaining unselected conditional on signing-up for the new lottery. The selection probabilities varied by the number of household members on the list, so in all cases, we estimated the selection probability separately by strata of "tickets" (number household members on the new waiting list at the time of the drawing).

Table A2 summarizes the distribution of the weights. Since only one selection for the new lottery occurred before the end of 2009, the impact of the new lottery is very minimal for our main analysis on SSA outcomes.

Defining Indicator of Total Individual Earnings above FPL

In our analysis of the effect of insurance on labor force activity in Table 1, we look at an indicator for whether the study participant's total individual earnings (from both wages and self-employment) are above the Federal Poverty Level (FPL).

Because we do not have a way to identify household members in the SSA records, we use individual earnings. We compare this to a constructed federal poverty level for the individual's household. We take the number of household members on the lottery list, and adjust it based on survey responses about household size.[4] Where there is a single household member on the list, we use an adjusted household size of 2.63; for two household members on the list, we use 3.71 and for three household members on the list, we use 4.25. We calculate the 2009 FPL for households of each size using the published guidelines. For example, the FPL for an adjusted household size of 2.63 is $10830+$3740*(2.63-1) = $16926.

This variable is thus measured with several sources of error, and but we still view it as a potentially interesting threshold aimed at capturing if an individual is earning enough to lift the household out of poverty.

Results

Balance of Treatment and Control

In previous work (Finkelstein et al. 2012) we discuss the random assignment of treatment and control groups. Here we examine treatment and control differences in the subset of individuals who matched the SSA records. Differences would threaten the validity of our inferences, which relies on the assumption that the treatment and control individuals in our analysis sub-sample are not differentially selected. Reassuringly, our analysis finds no evidence of differential match rates between treatment and control, or differences in pre-randomization characteristics between treatment and control.

Table A3, Panel A, shows that the match rates to the SSA data are balanced between treatments and controls among the Oregon Health Insurance Experiment analytic sample.[5] The table also shows, among the 85% of the sample that matched to the SSA data, various pre-randomization demographics and outcomes are balanced between (matched) treatments and (matched) controls. The table reports balance on a large number of individual, pre-randomization

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[4] We use data from our 12-month mail survey, described in detail in Finkelstein et al. 2012. We use the adjusted household sizes for all individuals in the analysis, not just the subset who responded to the survey.

[5] Table A3 shows a total sample that we tried to match to the SSA data of 73,253, which is slightly smaller than the full Oregon Health Insurance Experiment analysis sample that we tried to match to the SSA data (N=74,922). The difference reflects the small fraction of the sample that has a zero weight when analyzing 2009 data (see Table A2 and discussion therein). Match rates for treatment vs. control and characteristics of matched treatments vs. controls are also balanced on the unweighted sample (not shown).

covariates. Panel B shows balance on the demographics provided at sign-up on the lottery list. Panel C shows balance in the main outcomes we planned to analyze for 2009, as measured in 2007. We report summary F-statistics and p-values on the treatment-control balance of groups of variables: the lottery list demographics, the pre-randomization (2007) outcomes, and on both sets of variables combined. In all cases, we are unable to reject the null of treatment-control balance.

Although the treatment and control individuals who matched to SSA thus appear balanced on a broad array of pre-randomization characteristics, not surprisingly the set of individuals who match to SSA differ in some systematic ways from those in the analytic sample who did not match. Table A4 shows the results for the controls. Most notably, nearly 30% of unmatched individuals list a language other than English as their preferred language for applications, compared to only about 5% of matched individuals. In addition, unmatched individually are also about a year older and slightly less likely to list a PO Box as an address, to have signed up on the first day, or to have responded to the 12 month mail survey. They are also more likely to live in an MSA and in zip codes with slightly higher median household income.

First Stage

Table A5 reports our first stage estimates based on equation (3). Our measure of insurance is whether the individual was every on Medicaid from the beginning of the lottery (March 10, 2008) through the end of 2009. The first row reports our first stage estimate of 0.265 (standard error = 0.004) for the sample we matched to SSA records, which is the analysis sample in this paper. The F-statistic for this first stage is over 4,500. The second row shows the first stage estimate for the 15 percent of the sample that we did not match to the SSA data. The first stage for that sample is noticeably lower (0.19 (standard error = 0.009)).[6]

Breakdown of Earnings Results

Table A6 shows additional details of the earnings results shown in the main text (Table 1). The first three rows repeat the results from Table 1. The last two rows break out the analysis of annual earnings (row 2) separately into wage earnings and self employment earnings. Most earnings come from wages. We find no evidence of an impact of Medicaid on either of these components of earnings.

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[6] As noted above, the un-matched sample disproportionately listed languages other then English at sign-up as their preferred language for applications. It may be that some unmatched individuals are recent immigrants, documented or undocumented. Such individuals would not be matched to SSA records and would be less likely to be eligible for Medicaid. Although only U.S. citizens and a small number of qualified non-citizens can receive Medicaid, there was no screening during the application process to prevent them from signing up for the lottery.

Timing of Food Stamp Results

We (post-hoc) examined the timing of the increase in food stamps receipt with respect to winning the lottery. As an indirect way of probing the extent to which the is the direct effect of winning the lottery (rather than an effect of Medicaid as in the IV interpretation). Any direct effect of winning the lottery on SNAP would likely show up relatively quickly probably within the first three months,[7] but effects that occur with sufficient lags may be more plausibly due to Medicaid coverage.

Table A7 investigates this. Specifically, we analyze the reduced form (ITT) effect of winning the lottery on outcomes defined as "start receiving SNAP" (for the first time since January 2007) over a given 3-month interval relative to one's notification date of winning the lottery.[8] The table shows a statistically significant increase in the probability of starting to receive SNAP during the first 1 to 3 months after winning the lottery, and a similarly sized, statistically significant increase in the probability of starting to receive SNAP during the 4 to 6 months after winning the lottery; this latter effect seems less likely due to a direct effect of winning the lottery than to the effect of Medicaid coverage. The increase in first receipt of SNAP continues in the subsequent 3-month periods out to the end of our study period (13-15 months from winning the lottery) although these effects are not individually statistically significant. Taken together, we view the time pattern as suggestive of there being some "application effect" as well as some of the increase in SNAP not coming from the direct effect of winning the lottery and applying for Medicaid.

Applications to SSDI and SSI

Our primary analysis of receipt of SSDI and SSI (Table 2 of main text) uses data on benefit receipt, which is recorded retrospectively based on the date the individual became eligible for the program following a successful application. Another way to analyze the impact of Medicaid on SSDI and SSI is to examine how it affects *applications* to these programs and approvals of these applications. Post-hoc we decided to do this as well, using the Social Security Administration's 831 files, which contain application dates for SSDI and SSI, as well as the ultimate disposition of the application (e.g. whether they were approved or not).

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[7] As the Appendix to Finkelstein et al. (2012) details (see especially Table A2 there), applications for Medicaid were due within about two and a half months of being notified of winning the lottery. If an individual applied for Medicaid in Oregon in person (rather than by mail), case workers are instructed to offer assistance to interested applicants in applying for TANF and SNAP as well (which are handled by the same case workers). Conversations with state officials in Oregon indicate receipt of SNAP benefits is retroactive to the application date, so a boost directly related to winning the lottery and applying for Medicaid would show up within three months.

[8] There were eight separate lottery drawings, each with their own notification date (for details again see Table A2 of Finkelstein et al. 2012). For control individuals, we randomly assigned a lottery draw at the household level, stratified on the number of household members on the list, to match the distribution of lottery draws among the treatments so that, by construction, treatment probability is uncorrelated with lottery draw within strata. These notifications dates range from March to September 2008; by contrast, all other analyses in this paper measure outcomes from the start of the lottery (i.e. the first notification date) of March 10 2008

Table A8 shows the results of this additional analysis. The first two columns report the results of balance tests on these application and approval outcomes, measured in 2007. Columns 3 and 4 report the analysis of these outcomes in 2009. There is evidence of imbalance on the 2007 (pre-randomization) variables "applied for SSI" and "applied for SSDI"; both indicate that subsequent lottery winners were statistically significantly less likely to have applied for SSI or SSDI in 2007 (on the order of 0.5 percentage points). The analysis of 2009 outcomes (which controls for the 2007 level of the dependent variable), shows that Medicaid coverage is associated with a statistically significant, but economically small increase in the probability of applying for SSDI of about 1.3 percentage points, and a borderline statistically significant increase in approvals for SSDI of about half that magnitude. There is a borderline statistically significant increase the probability of applying for SSI of similar magnitude, although no evidence of an increase in approvals. Combined with the evidence on benefit receipt in Table 2, our overall reading of the evidence is that Medicaid coverage does not have a quantitatively large impact on SSDI or SSI receipt, although there is some suggestive evidence that it may have a statistically significant effect on approved SSDI applications.

Total Income and Economic Self-Sufficiency Results

Table A9 shows the results of analysis combining data on benefit receipt and earnings to examine total income and "economic self-sufficiency."

Total income is defined as the sum of wage income, self-employment earnings, SSI and SSDI benefits, plus the household amount of TANF benefits. Not surprisingly given the main earnings and benefits results in Tables 1 and 2, we find no statistically significant effect of Medicaid on whether the individual reports "any income" or on average annual income.

Our index of economic self-sufficiency follows the spirit of the one developed by Kling, Liebman, and Katz (Kling, Liebman and Katz 2007) but the underlying measures differ slightly. Our index of economic self-sufficiency is based on 4 measures: (1) employment, (2) earnings, (3) SNAP receipt, and (4) government income. Our measure of employment is an indicator for whether the adult had any wage or self-employment earnings during a specific year. Earnings is the amount of total individual wage or self-employment earnings during the year. SNAP is measured as receiving any SNAP benefit for that year. Government income is the total amount of individual SSI/SSDI income, SNAP benefits, and TANF benefits.

To generate our composite index, we follow Kling et al and average a normalized transformation of each outcome. The normalized transformation is generated by subtracting the mean of the control group and divide by the standard deviation of the control group. For any given outcome Y_k , the normalized, transformed outcome is therefore: $Y_k^*=(Y_k-\mu_k)/\sigma_k$ where the mean and standard deviation are based on the control group. The summary index across our four outcomes is $Y^*=\Sigma_k Y_k^*/K$. We reverse the sign for adverse measures of economic self-sufficiency (government income and food stamp receipt), so that a higher value of the normalized measure represents a more "beneficial" outcome.

We find that Medicaid decreases our economic self-sufficiency index, defined based on whether the individual was employed, the amount he earned, (the negative of) whether he received SNAP, and (the negative of) government income (defined as the sum of SSI, SSDI, SNAP and TANF benefits). Row 3 indicates that Medicaid decreases this measure of economic self-sufficiency by 0.08 standard deviations (standard error = 0.02). Given our results in Table 1 and Table 2, we investigated the extent to which this decline in our measure of economic self-sufficiency was driven by the role of SNAP in the formula. In the last row of Table A9, we exclude SNAP from the economic-self sufficiency index (both receipt of SNAP and its contribution to government income). The results indicate that the decline in economic self-sufficiency is entirely driven by the increase in SNAP.

Sensitivity Analyses

Tables A10 and A11 show results for various sensitivity analyses for our main analyses on employment and earnings (Table 1) and benefit receipt (Table 2). We consider sensitivity to the time period of analysis and to the choice of covariates. The results are quite similar across all alternative specifications.

Our main analyses use outcome variables from 2009 calendar year because 2009 is the only full post-treatment year for which we can isolate the effect of insurance on finance outcomes. Since the first lottery selection happened in March 10, 2008, it is possible that the 2008 income and benefit amount for some individuals selected in the earlier rounds were affected by the lottery. We therefore repeat our main analyses using 2008 calendar year outcome variables; we also repeat the analysis with the outcome variables defined over the combined 2008-2009 period.

Our main analyses also controls for the 2007 (pre-randomization) version of the dependent variable from 2007. We decided to include the 2007 measure of the dependent variable based on analysis (in the control sample) of the partial R-squared as a measure of the the predictive power of lagged dependent variables on our outcome variables (see Table A12).[9] We repeat our main analyses without controlling for pre-randomization version of the outcomes for robustness check. In addition, we repeat our main analyses including as controls not only the pre-randomization version of the outcome but also the nine pre-randomization “lottery list” variables included in Table A3 (Panel B).

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[9] Our investigations indicated that further including 2006 data did not substantially further improve the partial R-squared (not shown).

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Tables