THE OREGON HEALTH INSURANCE EXPERIMENT: EVIDENCE FROM THE FIRST YEAR
APPENDICES

Appendix 1: Analytic sample, data sources and outcome variables

This appendix first describes how we constructed our sample universe from the original lottery list, including the process by which treatment and control groups were defined, and then gives details of our administrative and mail survey data sources. See the main text for a description of our overall analytic strategy, estimating equations, and main results.

1.1 Construction of our analytical sample

The original lottery list that we received from the state included 100,600 records. We excluded 9,780 records that had been "deactivated" by DHS and were therefore not eligible to be selected in the original lottery or in our initial selection of survey controls. (Most of these deactivations occurred when the state updated the information on a person – most often the identification of other household members – making a new record and de-activating the old record.) We dropped an additional 4 "test" records. We received monthly updated lists from DHS. We excluded 324 records from our sample that did not appear on the original list but did on later lists; none of these "newcomers" had household members who were already on the list

The state did not always consistently de-activate a record when making a new copy of it, and in addition some people could legitimately appear on the list multiple times (e.g. if they signed themselves up multiple times or if they signed themselves up and someone else signed them up as well). We reviewed the list for duplicates using the CDC's LinkPlus software (publically available at http://www.cdc.gov/cancer/npcr/tools/registryplus/lp.htm). Using the software we looked for records that matched based on first name, last name, date of birth, and an internal processing identification number. Two research assistants separately reviewed all potential duplicates identified by the software. Our goal was to create a list in which each individual appeared only once. We considered two records to be duplicates if the research assistants both classified them as duplicates. This process identified 659 records, which reflected 656 people who appeared multiple times on the list. In the process of fielding about

20,000 in-person surveys over 2009 and 2010, we manually identified an additional 9 duplicate records (7 people). We removed duplicate copies of these individuals from our list. This data cleaning left us with 89,824 unique individuals on the original lottery list.

The lottery list information of some individuals made clear that those individuals were not in fact eligible for OHP Standard. Based on these pre-randomization characteristics, we imposed several additional exclusions to limit our sample universe. We excluded 36 individuals who gave an address outside of Oregon. We also excluded 3,258 individuals with birthyears in 1944 or before (corresponding to being older than 64 by the end of 2009 and therefore presumably on Medicare by the end of our study period) or birthyears in 1989 or later (corresponding to being younger than 19 at the beginning of 2008 and therefore not eligible for OHP Standard). We further excluded 5,161 individuals who had given a group or institutional address when signing up for the lottery list and 5,708 individuals who had been signed up for the list by an unrelated thirdparty (such as a hospital billing office); our concern with these individuals is that they were unlikely to be effectively notified even if selected in the lottery and indeed, our analysis of the first stage suggested a low first stage for these individuals.[1] We excluded 134 individuals who died prior to the notification date.

Finally, although as described earlier we purged duplicate observations so that each individual appeared only once on our list, this left individuals on the list who had had multiple active observations during lottery selection and our data collection. We excluded from our analysis any individual who had appeared with multiple (active) copies on the original list; this excluded an additional 605 individuals. Individuals with multiple (active) copies have a higher probability of selection (and in principle could vary in the outcomes studied). While we could simply have controlled for the number of copies of the individual, given the small number we opted instead to exclude them. We note that we cannot, of course,

[1] We considered excluding the roughly 5% of the sample which was enrolled on OHP in the period immediately preceding the lottery (January 1, 2008 to March 9, 2008) since these individuals would not benefit from being selected. There was, however, a slight but statistically significant imbalance between treatments and controls (difference of 0.005 percentage points, se = 0.002). We believe this is the result of how the state obtained the enrollment data which we discuss in the section titled “Medicaid enrollment data” below.

be sure that we have identified and removed all individuals who had multiple (active) copies since our process for identifying duplicate copies required some judgment (see above). In other words, there is inevitably some measurement error in identifying multiple copies. However, the fact that in attempting to conduct about 20,000 in-person interviews, our intensive field work revealed only 7 people whom our process had not previously identified as having multiple active copies makes us relatively sanguine that in practice the measurement error is likely to be substantively unimportant. Following exclusions we were left with a total of 74,922 individuals to study. Of these individuals, 29,834 were selected as treatments.

Not all data sources were available for this entire sample universe, as detailed for each source below. Figure A1 shows the relationships between the original lottery list, our sample universe and the sample used for analysis of specific data sources. Table A1 shows the differences in lottery list characteristics for those samples.

1.2 Defining "lottery draw" for controls

The state conducted eight separate lottery drawings between March and September 2008; Table A2 details the size and timing of these draws, as well as when those who were selected were notified that they had been drawn (the earliest date at which selection might have an effect). Because of this variation in the timing of treatment, we decided to measure outcomes in the administrative data from the lottery-draw-specific notification date. This was done primarily to increase the availability of pre-randomization hospital discharge data. We have hospital discharge data starting January 1, 2008 which gives us less than 3 months of data prior to selection for those selected in the March 2008 drawing, but almost 9 months for data for those selected in the October 2008 drawing.

To have an appropriate comparison group, we assigned a matched "lottery draw" to all controls. This assignment was done randomly, at the household level and stratified on household size. For each household size, the assignment distributed the controls across lottery draws in proportion to the distribution of treatments of that household size across lottery draws. This resulted in an assignment such that the probability of treatment is constant across draws conditional on household size. There are slight variations in these probabilities for households of size 3, but there are so few of these households that the differences are not statistically significant; moreover, all of our main analysis controls for lottery draw.

1.3 State administrative data

Lottery reservation list data

Oregon's Department of Human Services' Division of Medical Assistance Programs (DMAP) provided us with a complete list of all individuals who signed up for the lottery. This list includes a unique personal identifier, a household identifier, whether the individual was selected in the drawing and the date selected if selected. It also includes self-reported information that individuals provided when they signed up for the lottery in January and February 2008; Figure A2 shows the lottery request form individuals completed in when signing up for the list. We use this self reported information to construct the nine "lottery list" variables defined in the text. We also use the sign-up list to construct our "household size" variable, defined as the number of individuals in the household listed on the lottery sign-up form.

Medicaid enrollment data

Oregon's Department of Human Services' Division of Medical Assistance Programs (DMAP) provided us with yearly enrollment summaries (starting in 2002) in the division's programs for each individual on the reservation list. These summaries include the dates for any periods of enrollment in OHP Plus (the Medicaid program for categorically eligible populations), OHP Standard (the Medicaid program those selected in the lottery could apply for), and several other small medical assistance programs. These are the primary data that we use to measure the first stage.

Our primary first stage measure is whether individuals were ever on public insurance during our study period. There was, however, considerable variation in enrollment rates over time. Figure A3 details the time pattern of enrollment of both treatment and controls in both OHP Standard and any Medicaid from notification date on. As expected for treatments, enrollment increases dramatically in the months following notification. Enrollment then decreases over time, especially following the 6-month recertification process. For controls, enrollment slowly increases over time, especially for any Medicaid.

The enrollment data are kept by the state under a different system than the reservation list and with a different individual identification number. As part of the random selections, for each individual selected, DMAP performed an automated search to see if that person was already in the enrollment system (i.e. had ever previously been enrolled in a DMAP program). If not, they then performed a manual search, and if that was unsuccessful as well, they assigned a new identification number in the enrollment system for the individual. In order to provide us with comparable data on the controls, they performed the automated search. They did not, however, perform the manual search or assign new identification numbers for the controls. To the extent to which the manual search was successful in matching individuals to enrollment records, we may be underestimating enrollment among our controls and thus overestimating our first stage. We suspect that in practice this effect is small. We have identification numbers for over 99% of the treatments and 89% of the controls. The data indicate that around 14% of the controls were enrolled in Medicaid during our study. Assuming the enrollment rate for those with missing identification numbers is the same as in the rest would increase that to 16% (.14/.89) and reduce our first stage by 2 percentage points; we should note that the rate in those missing identification numbers should in fact be much lower since any control without any enrollment in a state benefit program would legitimately have no record.

Food stamp enrollment and benefit data

Oregon's Department of Human Services' Children, Adults and Families Division (CAF) matched the lottery list to their database on individual food stamp benefits and provided us with monthly data starting in 2007 on food stamp receipt and food stamp benefits for the filing group for each individual on the lottery list. This process used the same identification number used to link individuals to their Medicaid enrollment data. As described above, the state's attempt to link individuals on the lottery list to these identification numbers was done more intensively for treatment than control individuals so that we may slightly over-estimate food stamp receipt for treatments relative to controls.

Medicaid application data

Oregon's Office of Health Policy and Research (OHPR), with the assistance of Oregon's Department of Human Services, Children, Adults and Families Division (CAF) provided us with detailed data on the status and disposition any application submitted by individuals selected in the lottery. We received these data in January 2009 after CAF had finished processing the applications received in response to the lottery. These data include the household identifier, whether primary member of the household, the Medicaid personal identifier, date application was received, status of application, program enrolled in (if enrolled – i.e. OHP Standard or OHP Plus), reasons for pending, transfer or denied status, date of decision, and additional information if case was transferred. We use these data primarily to ascertain the dates of application decision (see Table A1), the rate of application return, and the reasons for application denial.

1.4 Mortality data

We use mortality data from Oregon's Center of Health Statistics; these do not include deaths outside Oregon. We have data for all deaths occurring in Oregon from January 1, 2008 through December 31, 2009. We probabilistically matched our sample to the mortality data from the state using LinkPlus software.[2] This was done using date of birth, first and last name, middle initial, and zip code. We study mortality from the notification date through September 30, 2009. As noted earlier, we exclude from the sample population people who died prior to their notification date.

1.5 Hospital discharge data

Data source and matching process

We obtained hospital discharge data for the entire state of Oregon for discharges occurring in 2008 and the first three quarters of 2009. The data are collected by the Oregon Association of Hospitals and Health Systems (OAHHS) and maintained by the Office for Oregon for Health Policy and Research

[2] In the probabilistic matching we aimed (to the extent we could control this) to balance false positives and false negatives.

(OHPR). These data include records for all discharges from inpatient hospitals in Oregon. They are similar to the Hospital Cost and Utilization Project (HCUP) inpatient datasets. All 58 general hospitals in Oregon are included, but not federally-administered Veterans' Administration hospitals or specialty hospitals. Using American Hospital Association data we calculated that the included hospitals represent 93 percent of the hospital beds in Oregon.[3] The record for each admission includes a hospital identifier, dates of admission and discharge, detail on diagnoses and procedures, payor, source of admission and discharge destination. We combined the discharge data with several hospital-level data sources (such as the American Hospital Association data) to obtain additional detail on the hospitals.

We probabilistically matched our sample to the hospital discharge data using LinkPlus software. This was done using date of birth, first and last name, middle initial, gender and zip code.[4] Prior to the match, we conducted training exercises using only this subset of matching variables compared with a richer set of variables in a data set where we had more complete information. This allowed us to calibrate our assessment of potential matches (meaning agreement on any of the matching variables).[5]

Due to the sensitive and protected nature of the data, the match was conducted on-site at OHPR in conjunction with OHPR personnel, who then provided the study team with data including the matched study identifier but excluding the personally-identifying matching variables.

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[3] The five Oregon hospitals not in our data include 2 Veterans' Administration hospitals, 1 children's hospital, 2 state psychiatric hospitals and 1 alcohol and substance abuse treatment center. Of these, only the alcohol and substance abuse treatment center (Serenity Lane) reports any Medicaid admissions in the American Hospital Association data. That center reports approximately 30% of its admissions are Medicaid suggesting it may be used by our population. It is, however, quite small with only 55 beds and less than 1% of all inpatient admissions in Oregon. So any bias due to its not being included should be small.

[4] Although we have the full address of individuals on the lottery list, only zip code is available on the hospital discharge files. Unfortunately, prior to 2008, the state's hospital discharge data did not contain patient name and therefore could not be matched to the lottery list.

[5] We tried, to the extent possible, to give equal penalty to potential false positives and false negatives in deciding on what to call a match in order to maximize power. According to our calculations, the match probability threshold that maximizes power is a function of the number of matches: $\sqrt{n(n+1)}-n$. If n is 10,000 (approximately the number of admissions we expected in our sample), the threshold is approximately 0.5. We then ran test matches between two versions of the lottery list in order to try to calibrate our subjective assessment of the probability of a true match to the actual probability.

All of our analysis occurs at the person-level rather than the admission-level and excludes admissions for childbirth (coded as major diagnostic category 14), since many people in our sample would become categorically eligible for OHP Plus for childbirth, regardless of their lottery status. Specifically, pregnant women up to 185 percent of the federal poverty level can be covered by OHP Plus (Office for Oregon Health Policy and Research, 2009)..

The data we received included all hospital discharges from January 1, 2008 to September 30, 2009. We limit all of our analysis to hospital admissions occurring between January 1, 2008 and August 31, 2009. Our concern was that the discharges observed at the very beginning of the data period would be skewed to longer hospital stays and those observed at the end of the data period would be skewed to shorter hospital stays. Starting with admissions occurring on or after January 1, 2008 solves the first problem. Ending on August 31, 2009 limits the last problem as over 99 percent of hospital stays in the data are less than 30 days. For each individual in our study, we created utilization measures both pre- and post-lottery, with the lottery date based on the individual's lottery draw notification date (as described in the text). Outcomes for our study period are thus defined based on admissions from the individual's lottery notification date through August 31, 2009. Pre-randomization outcomes are defined based on admissions from January 1, 2008 through the notification date. As shown in Table A3, conditional on any admission, the median number of admissions for controls in our study period is 1 and the mean is 1.6.

Outcome measures

Utilization

We construct three measures of utilization commonly used in the literature (see e.g. Card et al 2009): (1) number of hospital days, (2) total list charges, and (3) number of procedures performed; these measures sum across multiple admissions for a given patient during the time window. Table A3 shows the conditional distribution of these variables; all three are quite right skewed.

List charges are standard accounting charges for room and procedures; they do not reflect the charges that are actually billed for; they also exclude physician services. While some argue that they are reasonable approximations of the cost of care (e.g. Doyle,2005), they may also be viewed as simply a price-weighted summary of treatment (Card et al 2009), albeit at artificial prices. Importantly, list charges are uniform across payer types within a hospital, and therefore are not mechanically affected by insurance coverage (Doyle, 2005). Of course, if the relationship between these "sticker prices" and actual utilization varies across hospitals, any effect of insurance on hospital sorting could potentially contaminate the analysis of list charges; as described in more detail in the text, we do not find any evidence of an effect of insurance on sorting across hospitals, although this may simply reflect low power.

Selected conditions

We identified seven selected conditions which were of particular interest based on their prevalence in our population. These are (mutually exclusive) conditions coded on the basis of primary diagnosis. We used the Hospital Cost and Utilization Project's Clinical Classification System to group diagnoses coded by ICD-9 codes into clinically relevant categories (HCUP CCS). Table A4 shows the top 10 diagnoses by classification in our sample of controls. The top seven diagnoses among our control sample were mood disorders (10% of admissions), skin and subcutaneous tissue infection (4%), diabetes mellitus with complications (3%) and alcohol-related disorders (3%), schizophrenia (3%), spondylosis and other back problems (3%) and pneumonia (3%). We decided to combine mood disorders and schizophrenia by expanding to the more general category of mental diseases or disorders (major diagnostic category 19) which will include both as well as other disorders. Because substance-related disorders was ninth, we also expanded to the more general category of alcohol and drug use (major diagnostic category 20).We combined diabetes with complications and diabetes without complications for completeness. We also created a composite heart disease category including myocardial infarction, angina and arrhythmia. Table A5 gives detail on the specific conditions which make up each of these categories and their prevalence in our sample. [6]

[6] Our coding is somewhat ad hoc since it involves creating composite conditions from underlying diagnosis codes, and there might well be other composite conditions that would also be prevalent. We created the list based on

Comparison across populations

Table A6 compares some summary measures on utilization in the hospital discharge data for our lottery list sample relative to other populations in Oregon (specifically all uninsured adults aged 19-64, all adults aged 19-64, and all admissions). Unlike all of the other analysis, this is done at the admission (rather than person) level. Compared to the overall Oregon population (column 2) our study population (column 8) has a disproportionate share of admissions through the ED and a disproportionate share of admissions for mental disorders.

We also tried to (at least roughly) compare the probability of admission for our sample to that of a general adult population. As seen in Table 4a, our control sample a 6.7 percent change of a hospital admission (for reason other than childbirth) during our study period, which corresponds to an annualized admission probability of about 5 percent. Using the pooled nationwide 2004-2009 National Health Interview survey, we estimate that the 12-month probability of an inpatient hospital admission (for any reason, including childbirth) is 8.2 percent in the general adult population.

1.6 Credit report data

Data source and matching process

We obtained the complete credit records for a subset of our lottery list from TransUnion's Consumer Credit Database; TransUnion is one of the three national credit reporting companies. Credit bureaus collect vast data that aims to cover virtually all U.S. consumer borrowing; the primary purpose of these data is for use by prospective creditors in assessing the credit-worthiness of current or potential consumers. Avery, Calem and Canner (2003) provide an excellent, detailed discussion of credit bureau data; most of our discussion of the data is based on their work.

Credit reports contain data gathered from three main sources: (1) public records (2) collection agencies, and (3) trade lines. Public records data – which are virtually complete – consists of information

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eyeballing the underlying codes and our priors on what might be interesting and prevalent in our population. An advantage of our having pre-specified this list is that the ad hoc nature need not particularly concern us.

on events such as bankruptcies, liens, and judgments. Collection records contain information on accounts in collection through collection agencies, most of which are not related to revolving credit accounts, such as collections for unpaid medical bills or unpaid utility bills. Collection records will not be a complete record of all accounts that have gone to collections since some creditors try to collect themselves rather than use collection agencies and not all collection agencies report to credit bureaus. The third source of data – and the vast majority of records that the credit bureau obtains – is information on credit provided by banks, finance companies and credit unions, and other institutions. Known as "trade lines", these data contain a wealth of information including the account opening date, outstanding balances, credit limit, and payment (or non payment) history on the account. Trade line data include information on revolving credit (such as credit cards, bank cards, retail store cards etc), mortgages, and installment loans. While these trade lines data are considered a near-comprehensive set of information on the credit available to the general population, they may be a less complete depiction of credit and credit history for our very low income population. Low income populations with poor access to traditional credit may rely more heavily on non-traditional forms of credit such as borrowing from relatives and friends, rent-to-own "purchases", pawn shops, etc. that would not be reported to credit bureaus.[7] In addition to the collected data – public records, collections and trade lines – the credit bureau also supplied us with their calculated credit score for each individual based on its proprietary scoring algorithm.

We have credit report data from February 2007, February 2008, February 2009 and September 2009. Our primary analysis is based on data on outcomes from September 2009. In these data we can observe some outcomes currently (e.g. credit limit or credit score) and some outcomes since the notification date

[7] One high-profile form of non-traditional credit is payday loans. Payday lenders have their own credit bureau. However, such loans may not be an important source of credit in our population for several reasons. First, payday lending requires that one be employed and have a pay check, but only about one third of our sample reported working more than 20 hours per week in our survey data. Second, payday loans are generally small (on the order of about $100 to $300) and in Oregon in particular, payday lending has been quite restricted since a binding 2007 cap on payday lending there (Zinman, 2007).

(e.g. whether you have had a collection since the notification date). The February 2007 and February 2008 data are both measured prior to randomization.

The credit bureau matched the list of lottery participants to their credit report from February 2008 (i.e. right after the January – February 2008 lottery sign-up but *before* any lottery drawings began in March) on the basis of their full name, address, and date of birth as they reported when signing up for the lottery.[8] This process generated a 68.5% match rate with the February 2008 credit bureau data. There are two potential reasons why we would be unable to match a given lottery participant to a credit report. First, match rates are likely to be lower without social security number to match on.[9] Informal conversations with credit bureau staff suggested that with accurate current address (which we hoped to accomplish by matching to the February 2008 file contemporaneous with the lottery sign-up), match rates in the general population might be expected to be about 75 to 85 percent. However, with a weak current address that match rate might fall as low as 50 percent.[10] Second, in a very low income population, some individuals may not have a credit file.[11] Based on the expected match probabilities we suspect that roughly 10 to 20 percent of our population had no credit file.

The credit bureau followed any individual who appeared in the February 2008 data forward and backwards to the other archives (February 2007, February 2009 and September 2009) using their internal personal identifier variables. They were able to match 97 percent of individuals found in the February 2008 archive to the September 2009 archive (analogous to a 97 percent "response rate" since the initial sample is defined based on a pre-randomization (February 2008) match).

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[8] A large number of additional Oregonians who did not sign up for the lottery list were also included in the match request, to preserve the anonymity of who had signed up from the credit bureau. We subsequently removed these individuals from our analysis.
[9] Although individuals had the option to provide their social security number on the lottery sign-up form (see Figure A1), we did not have permission to use it.
[10] Note that in declaring a match the credit bureau errs strongly on the side of avoiding false positives rather than false negatives.
[11] Note that an individual need not have access to traditional credit to have a credit file; they will have a credit file even with no access to credit if they have ever had a public record (e.g. bankruptcy, lien, judgment) or a collection.

Outcome measures

All of the credit report outcomes we analyze are defined over the study period from (lottery draw specific) notification date through September 30, 2009 unless they are specific to "current" outcomes, in which case they are measured as of the end of September 2009. We also construct analogous measures of outcomes in the pre-randomization data (February 2008) with the same look-back period; specifically, for each lottery draw we defined an equal length pre-lottery look back window so that the look back length for each lottery draw is the same in the February 2008 data and the September 2009 data. These February 2008 measures are used both as controls in the main analyses and for examination of pre-randomization balance; to be analogous to our outcomes analysis, when we examine pre-randomization outcome balance in the February 2008 data we define (and control for) analogous measures in the February 2007 data, where we once again defined lottery draw specific look back periods that were the same length as in the September 2009 and February 2008 data.

Of the three primary sources of credit data (public records, collection agencies and trade lines – each described in more detail below), we focus primarily on the public records and collections, since "trade lines" (which reflect credit provided by banks, finance companies etc) are relatively uncommon in our low income population which has relatively low access to credit. that are more prevalent in our very low income population.[12] Our main analysis focuses on five measures of financial strain: whether the individual has had a bankruptcy, a lien, a judgment, a collection, or any credit account with a payment that is 30 days or more late ("a delinquency"). The first three measures come from the public records, the fourth from the collection data, and the fifth from trade line data. We further decompose collections into

[12] Another reason for this decision is that – a priori – we were concerned that health insurance might, by reducing the rate of bad medical debt, improve one's access to credit. In this case, there could be a mechanical (and perverse) effect of insurance on delinquencies and outstanding obligations in trade lines arising from an expansion of the risk set. In practice, as we document below, we did not detect any impact on access to credit.

medical and non-medical collections.[13] Table A7 shows the distribution of these data for our study population.

Data from public records

Credit report data contain virtually complete records on bankruptcies, liens and civil judgments.[14] About 85 percent of the bankruptcies in our population are Chapter 7 bankruptcies, the rest are Chapter 13. Liens refer to tax liens; they are generally taken out against individuals by governments for unpaid taxes.[15] We include both paid and unpaid liens. Approximately 60% of liens appear to be "ever paid".[16] Judgments are sought by a variety of parties including medical providers, governments, utility companies, collection agencies, and creditors (Avery et al. 2003). We include both paid and unpaid judgments; approximately one quarter of judgments are "ever paid".[17]

As the underlying incidence rates in Table A7 indicate, all three of these represent extreme right-tail negative events (although they are substantially more common in our lottery population than in a general population). They are also likely to occur with a lag after an initial adverse financial shock; therefore even if health insurance ultimately reduces the incidence of these events, we may not pick this up in our one year window.

Note that while public records data are generally complete, they will represent only a selected subset of unpaid bills. Given the monetary and time costs involved in bringing (and winning) legal proceedings

[13] There is some overlap in the liabilities captured by the different measures. For example, some collections will ultimately be sent to judgment (although not all collections are sent to judgments and not all judgments started as collection attempts). In addition, while bankruptcies, judgments, liens and collections may (and for the most part do) reflect non-credit related bills (e.g. medical bills, utilities, rent etc), credit-related late payments that ultimately get sent to collection or judgment will also show up in delinquencies. Delinquencies on credit accounts may be on revolving credit or on non-revolving credit (e.g. mortgages or installments); delinquencies are mechanically zero for the approximately one quarter of our sample that has no open credit over our study period.

[14] In addition, credit bureaus also collect public records on lawsuits and foreclosures. However the lawsuit data is highly incomplete (Avery et al. 2003) and foreclosures are extremely rare in our population, so we therefore choose not to examine them.

[15] Avery et al (2003) report that less than 1 percent of liens are taken out by non government entities.

[16] Since it is difficult to estimate payment rates using recent liens due to censoring, for this calculation we look at liens taken out between 2005 and 2007 and look at what fraction are paid by September 2009.

[17] Again to handle censoring we look at judgments taken out between 2005 and 2007, and what fraction of them are paid by the end of September 2009.

against an individual and then trying to serve and collect against a successful judgment, it is presumably only worthwhile to seek a judgment when the amount of money owed is large (relative to the fixed cost of seeking the judgment and collecting against it), and the person is deemed to have resources against which to collect. Consistent with this, we find that median judgment amounts owed are $1800 and mean judgment amounts owed are $3800. Thus these measures should be thought of as proxying – with a lag – for particularly large and unpaid bills.

One potential concern with interpreting changes in these measures is that health insurance itself could increase the probability (or the perception) that an individual has resources available to collect against, and thus increase the probability of a collection action conditional on an unpaid debt. In practice, as we demonstrate in Appendix A6, we do not detect any impact of insurance in our population on the market's assessment of credit worthiness (as measured by access to credit); this may be because there is no effect (health insurance is not directly observable by plaintiffs or on credit reports so the impact would have to be indirect e.g. by decreasing the rate of medical collections) or because our one year time horizon is too short for such affects to operate.

A similar concern with judgments is that a non trivial fraction of them are sought for delinquent payments on revolving credit creditors (approximately one fifth according to Avery et al (2003)). Therefore to the extent that health insurance eases access to such credit and therefore increases the "risk set" of potential judgment seekers, one could get perverse results whereby health insurance is associated with more judgments. Again, this is an issue of interpretation and one that we shed light on through our direct examination of whether health insurance affects credit access. This issue does not arise with liens, the vast majority of which are sought by governments.

Data from collection agencies

Collection data consist of unpaid bills (mostly not related to revolving credit) that have been sent to collection.[18] Collections offer two main advantages over public records: they are more common (and therefore capture financial strain at a less extreme point in the distribution), and they are likely to be occur with less of a lag (in general it takes only about 4 months for an unpaid bill to show up as a collection if it is sent to a collection agency that reports). There are, however, two concerns with collection data. First, there is incomplete coverage of unpaid bills. Not all unpaid bills are sent to collection; in general, entities with scale (such as hospitals and utility companies) are more likely to send things to collection agencies that relatively small operators such as small landlords or small business. Moreover, collection records will not be a complete record of all accounts that have gone to collections since some parties collect themselves rather than use collection agencies and not all collection agencies report to credit bureaus.

Second, the fact that not all providers report collection attempts to the credit bureau raises concerns about non-randomness of provider reporting by insurance status – both in terms of sorting of individuals across providers based on collection practices and in terms of variation in collection practices within providers based on insurance status. This seems a priori less a concern with non-medical collections (do you even get to choose your utility company?) but potentially a concern for the medical collections measure. We called a number of collection agencies in Oregon and a number of hospitals in Oregon to try to get a better sense of practices with respect to reporting of medical collections. Different collection agencies follow different reporting practices and we cannot rule out the possibility that there could be a correlation (of either sign) between reporting practices of the collection agency and the insurance characteristics of their creditor population. For example, it is possible that the uninsured (who are more likely to have unpaid medical bills) are more likely to sort into medical providers who do not send to collection agencies that report to the credit bureau, so that one could in theory spuriously find that insurance increases medical collections. Complicating such a selection story is that in practice it appears

[18] Avery et al (2003) report that in a general population, about 5 percent of collections are from revolving creditors.

from our conversations that at many hospitals the practice is not uniform even within the hospital; e.g. the hospital facility bill goes to a collection agency that does not report to the credit bureau, while the ER physician bill is sent to a different collection agency that does, and the non-ER physicians have yet their own standard. This makes such “shopping” by patients based on insurance status less likely, although certainly does not eliminate the concern. On the other hand, it is possible that providers with a lot of uninsured patients may be more likely to use collection agencies that report, as a threat mechanism, so that one could spuriously find that insurance decreases medical collections (or they could be less likely to try to collect because they are less optimistic about succeeding). We did not, however, find any evidence that insurance affects the sorting of patients across medical providers (See Appendix 3 and Table A18).

There is also the possibility that within hospital the decision to seek to collect (or, conditional on trying to collect, the decision to send to a collection agency that reports) could vary with an individual’s insurance status. Several discussions with Oregon hospitals did not turn up any indication of differential collection practices by insurance status, but this is not something we can definitely rule out.

Finally, we note that to the extent we are worried about insurance being correlated with collection practice, while this raises a potential concern with interpreting changes in medical collections in credit bureau data as evidence of changes in financial strain, changes in medical collections are still a real measure of something that affects credit and therefore of interest, albeit with a different interpretation.

We observe the date of collections and the amount currently owed (i.e. not yet paid) on each collection.[19] In practice, very few collections are ultimately paid. Only about 3 percent of collections are paid – 4 percent of non-medical collections and 1 percent of medical collections.[20] Collection amounts owed are very right-skewed. Conditional on having a positive collection balance, the average collection

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[19] Note that this may include collections reported prior to notification date and will exclude any collections that are paid or closed for some other reason (e.g. repossession) and the collection agency has therefore stopped trying to collect

[20] To handle the potential censoring problem (i.e. collections may be paid with a lag), we computed these statistics by looking at collections incurred between 2005 and 2007 and their status (paid or not) by the end of September 2009. The fraction paid is naturally lower if we looked at collections incurred since the notification date through September 2009.

balance in our sample is about \$7,300, with the 10^{th} percentile about \$330, the median about \$3,200, the 75^{th} percentile about \$8,000, and the 90^{th} percentile about \$17,300.

While we only analyze the amount owed in collections, we also observe the amount of money currently owed for liens, judgments, and late credit payments (delinquencies), although we cannot separate out medical from non-medical for these other measures. We are hesitant to look at these other measures since there is unavoidable double-counting (e.g. some collections eventually result in judgments) which could spuriously inflate our estimates of the impact of treatment on amounts owed. We therefore limit the analysis of amounts to collections, which are the most common of these adverse events and which have the added appeal that medical and non-medical collections can be distinguished.

Data from trade lines

From the trade line (credit) data we obtain measures for whether the individual has had any delinquency on any credit account since the lottery notification date. We look at any trade lines (credit) that the individual has open since the notification date, including not only revolving credit but also installment loans and mortgages. About three quarters of our sample has an open trade line by this measure (and about half have a revolving trade line). For the one quarter of our sample without open credit since the notification date these variables are mechanically zero; in this case a zero reflects not being at risk for a delinquency rather than having had a good payment pattern; this is a problem for interpretation only if health insurance increases the chance one has any credit over our time period; as discussed, we show below that it does not.

We measure delinquencies as any trade account that is 30 days or more past due. According to Avery et al (2003), delinquencies – and particularly major delinquencies, defined as 120 days or more past due – are important in consumer credit evaluations.[21] We do not focus on major delinquencies because we were

[21] Avery et al. (2003) page 61 note “in general an individual with a major derogatory will find quality for new credit diffcult, may face high interest rates for the credit received, or may be limited in further borrowing on existing open accounts.”

concerned about right-censoring given a study period of about 16 months. However, a downside to using non-major delinquencies is that not all creditors systematically report them (Avery et al (2003), page 62).

Comparison to other populations

Table A8 provides summary statistics on each of the outcomes for our lottery list control sample and for all Oregonians. For purposes of this table we define the outcomes "over the last 12 months" (rather than "since notification date" as we do for our analysis variables; a 12-month look-back period is slightly shorter than our average study period look-back of 16 months.[22]

Our lottery population is much lower income than the general population and therefore expected to look worse in terms of adverse financial events and access to credit.[23] This appears to be the case; for example, almost half of the lottery population has had a collection in the last 12 months compared to only 13 percent of the general Oregon population (for medical collections these numbers are 25 percent and 5 percent respectively). The average credit limit on revolving credit is about $10,000 for our lottery population compared to about $23,000 for the general population. Conditional on having any positive credit limit, these numbers are about $16,000 and about $40,000 respectively.[24]

1.7 Mail survey

Main mail survey

Our main mail survey sample consists of 58,405 individuals, including 29,589 treatments and 28,816 controls. We selected this subset of the controls when we conducted an initial series of mail surveys in waves roughly concurrent to the state's lottery drawings. The state provided us with information on those selected in each month's lottery drawing shortly after it had been completed. We then drew from the

[22] To identify specific time periods other than "last 12 months" requires access to more detailed (and hence expensive) data; we purchased this more granular data only for our study population.

[23] Note that our lottery sample excluded individuals aged 65+ while our "all of Oregon" sample includes all ages (since age is not readily available as a covariate to condition on).

[24] The one exception is that about 80 percent of our sample has a credit score, compared to about 63 percent in the general Oregon population. However note that an absence of a credit score is not the same thing as a bad credit score, rather it reflects insufficient information on the person. One way to generate a (bad) credit score is to have public records or collections on record, which our sample disproportionately does.

remaining risk population a stratified random sample of controls; we stratified on household size to try to match the household size distribution in the treatment sample which – as noted above – had a selection method that favored larger households. In addition, we oversampled controls relative to treatments in early survey waves because of the expectation that some controls would get selected by the state in later lottery draws.[25] We confirmed that we drew our control sample correctly by verifying that there was no substantive or statistical difference across treatment and control groups in the individual characteristics observed on the lottery list (see Balance section in Appendix 2). The mail surveys were sent in seven survey waves over a six week period in July and August 2009; extended follow-up lasted until March 2010.

There are two key implications of this sampling strategy. First, because we ultimately "ran out" of larger households to use as controls (and because the controls who subsequently got treated were disproportionately from larger households) our final sample is not balanced on household size between treatment and controls. Therefore we will include household size dummies in all our analysis. Second, because take-up was lower than we (or the state) expected, our attempts to oversample controls in early survey waves (to end up with an equal number of controls and treatment groups by survey wave) were insufficient. As a result, treatment probability varies in our sample by survey wave (it is higher than 50% in earlier survey waves and lower than 50% in later survey waves) and within household size. Since people surveyed earlier on average respond earlier, and since there may be seasonal or time trends in outcomes, in all the analysis of survey respondents we include indicators for survey wave and for the interaction of survey wave with household size. This survey wave is not the same as the matched lottery draw used for analysis of the administrative data, described above. Table A9 contains more detail on the timing of the different survey waves and the proportion of treatments within each wave.

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[25] We did this control selection on the original lottery list as received from the state (prior to removing duplicates and making exclusions). This most closely mimics the state's procedure. We then removed duplicates and made exclusions across both the treatment and control sample.

The basic survey protocol consisted of a screener postcard and 3 survey mailings. The third survey mailing included the URL of a website to complete the survey if preferred. If the screener postcard or any subsequent mailing was returned as undeliverable, attempts were made to find an updated address from the post office, the LexisNexis people search, and the Cascade Direct change of address database. If these attempts were unsuccessful and there was a phone number provided on the lottery list, we attempted to receive an updated address over the phone. The first of the survey mailings included a $5 cash incentive; in addition, responders were entered into a lottery to receive an additional $200.

Following the basic survey protocol, we had received 20,833 responses corresponding to a response rate of 36 percent. Of the 37,572 non-respondents to the basic protocol, we selected a subsample of 30 percent (11,413 individuals) for a more intensive follow-up protocol. We generated weights to account for this more complex sampling procedure. For those receiving the additional follow-up, the weights were proportional to the inverse of the probability of receiving additional follow-up.

Multiple attempts were made to reach individuals in the intensive follow-up subsample by phone if they had not responded to the basic protocol. When reached by phone, individuals were asked to confirm their contact information and to complete the survey over the phone. Intensive follow-up subsample individuals also received two additional mailings. The first was a postcard providing information for accessing the survey online, an email address and 800-number for updating contact info, and a detachable pre-paid postcard for updating contact info. It offered a $5 incentive for contacting the survey team in one of those ways. The second additional mailing was a letter with the same information as the postcard (minus the detachable address update card) and offering a $10 incentive. Furthermore, if basic tracking had failed to yield a usable address or phone number, substantially more extensive tracking attempts were made. This additional tracking used the following tools: online searches on Google, whitepages.com, social networking sites (such as MySpace and Facebook); searches of commercial databases (in particular CLEAR); searches of public documents such as court documents, marriage licenses, etc. All our surveys asked for information on third-party locators (friends and family), and we contacted these individuals to ask for updated address and phone information for the study participant.

In November 2009, while we were still fielding the twelve-month survey, the state opened a new reservation list for OHP Standard and began conducting new lottery draws from this list. This meant that some of our control sample could potentially be given the opportunity to apply to OHP Standard before responding to the survey. We were concerned about our ability to correctly interpret these responses given that the short-run effects of being given this opportunity could well differ for the longer-run effects of health insurance that the 12-month survey was intended to measure. The state provided us with the entire new list identifying those selected in each of the new lottery draws conducted during our fielding, which we used to ensure that data collected were not contaminated by these effects. First, we excluded data from surveys returned by newly selected individuals after they were notified of their selection. This resulted in collected data being excluded for 36 survey respondents. Second, we took advantage of the fact that, although the set of individuals in our sample who signed up for the new lottery list was not a random subset (meaning that we could not simply exclude from our sample anyone who signed up for the new lottery), selection by the state within that group was indeed random. We weighted data collected after each new draw from those on the new list but not selected to stand in for the data that was excluded from those who were selected from the new list. The weights were assigned to be proportional to the inverse of the probability of not being selected conditional on being on the new list. These were calculated conditional on the number of times an individual or someone in their household appeared on the new list, to reflect the state's procedure. This can be thought of as analogous to choosing random subsamples of non-responders on fixed dates to receive additional follow-up.

The response rate to the basic protocol for the 12 month survey was 36 percent; following the intensive protocol, the overall weighted (weights based on probability of inclusion in intensive follow-up subsample) response rate was 50 percent. Some of the non-respondents were people we were unable to reach because they were deceased or incarcerated. For others, the address provided on the lottery list was no longer active by the time of the 12-month survey and we were not able to locate an updated address. Excluding all individuals with these characteristics leads to an adjusted weighted response rate of 54 percent**.** This is a good response rate for a mixed-mode mail and phone survey of a low-income population in the United States (for some comparisons see e.g. Beebe et al 2005, Brown et al. 1999, Carlson et al 2006, Fowler et al. 1999, Gallagher et al. 2005, Hartz et al. 2000, and AHQR 2001), although it of course leaves substantial scope for non response bias arising from differences between treatment and control responders; we investigate this in detail in Section 4 and in Appendix 2**.**

Two earlier surveys: initial mail survey and 6 month mail survey

We conducted two earlier versions of the main mail survey. These are analyzed briefly in the main text (see Table 11). An initial mail survey was fielded between June 2008 and November 2008 on the same sample that was subsequently used in our main survey described above. The survey protocol included a screener postcard, 2 survey mailings plus phone follow-up for non-responders. If the screener postcard or any subsequent mailing were returned as undeliverable, attempts were made to find an updated address from the post office, the LexisNexis people search and the Cascade Direct change of address database. If these attempts were unsuccessful and there was a phone number provided on the lottery list, we attempted to receive an updated address over the phone. The first of the survey mailings included a $5 cash incentive; in addition, responders were entered into a lottery to receive an additional $200. We received responses from 26,423 individuals, a response rate of 45 percent. The average response date of the initial survey was August 29, 2008.[26]

A “6 month” mail survey was conducted on a limited subsample (n=11,756) of the initial survey. We oversampled respondents to the initial mail survey in the six month survey sample. For analysis of the six month survey, we use survey weights which are proportional to the probability of being sampled. This

[26] We estimate a 1.4 percentage point (standard error = 0.005) lower response rate for treatment individuals than control individuals in this initial survey, which is similar to the response rate differential we found in the main survey (see Table 2). Pre-randomization characteristics that we can observe all appear balanced across treatment and controls for responders to this initial survey; the p-values on the F-tests of differences between treatment and control characteristics (shown in Table 2 column 4 for the main survey) are each bigger than 0.34 for the initial survey.

survey was fielded between January 2009 and May 2009. The survey protocol were the same as for the initial survey. We received responses from 5,411 individuals, with a weighted response rate of 42 percent. The average response date was February 23, 2009. [27]

The survey instruments for the two earlier surveys differed in a few ways from the main survey (with refinements based on fielding). The initial survey did not include questions on happiness, depression, medications for specific conditions, smoking, work impairment or preventive care. The six-month survey did not include questions on preventive care. On the questions asked in all three surveys, some questions were reordered and there were some wording changes, especially to the questions about out-of-pocket expenses.

Outcome measures

The survey instrument was designed by the study team. Where possible, and as described below, we adapted modules from existing surveys. Each version of the survey was pilot tested on individuals on the reservation list but not in our survey sample, and revised to improve clarity and flow prior to the main distribution. Figure A4 shows the survey instrument, which provides the exact wording of each question. In the descriptions below the relevant question number from the survey is referenced for each outcome. For each outcome analyzed, Table A10 provides information on what survey elements were used to construct it and the percent of responders for whom we have data for that question. We analyze many of the variables as dichotomous transformations of continuous or categorical variables. Table A11 provides detail on the distribution of the underlying variables as well as where we censored any of the continuous measures.

[27] We estimate a 3.5 percentage point (standard error = 0.011) higher response rate for treatment individuals than control individuals in this initial survey, which is similar to the response rate differential we found in the main survey (see Table 2). Pre-randomization characteristics that we can observe all appear balanced across treatment and controls for responders to this six month survey; the p-values on the F-tests of differences between treatment and control characteristics (shown in Table 2 column 4 for the main survey) are each bigger than 0.73 for the six month survey.

Health care use

Our measures of health care use were loosely based on the 2003 survey instrument for the Center for Studying Health System Change's Community Tracking Study (Center for Studying Health System Change, 2005). Participants self-reported the number of prescription medications they were taking (Question 12). We asked separately about outpatient doctor visits (Q15), emergency room use (Q16) and hospital stays (Q18). For each of these we examined both whether there was any use (extensive margin) and the number of prescriptions, doctor's visits, emergency room visits, and hospital stays (total use margin). All of the total measures were truncated at twice the 99th percentile, since reports above that were implausible and were likely errors (for example, a subject reporting currently taking 1027 medications). Only a small number of observations were affected by this truncation (see Table A11).

Financial strain for health care costs

We were not able to find a module on out-of-pocket spending in a national survey that seemed well-suited to our purposes. Most surveys which collect detailed medical expenditure data go into far more detail than was feasible on a mail survey (detailing each medical encounter, for example). We worked with survey experts to design questions asking about total out-of-pocket medical expenses in the last 6 months (Q20) and then breaking them down into several large categories for specific types of care (Q21a-d) designed to aid in recall. If a participant responded no to the overall question, but then indicated positive out-of-pocket medical expenses for a specific type of care, we considered the respondent to have positive spending. Participants also self-reported whether they owed money for medical expenses (Q22), had borrowed money or skipped paying other bills to pay for medical expenses (Q23), or been refused treatment because of money owed (Q24). For the quantile analysis on total out-of-pocket expenses (sum of Q21a-d) and the total amount owed (Q22), we treated missing amounts as zeroes. Table A11 provides more detail on the distributions of each component and the totals.

Self-reported health

We used several different measures of health. We used the CDC's "Healthy Days Measures" (Q26, Q28-30) designed to measure health-related quality of life (Hennessy et al, 1994). These questions have been used in the Behavioral Risk Factors Surveillance Survey since 1993 (CDC, 1993-2008). We considered the four questions from this measure separately. These four questions were: whether the participant reported being in fair or poor health as compared to excellent, very good or good health; the number of days (of the last 30) the participant reported having not good physical health; the number of days having not good mental health; and the number of days where poor health interfered with usual activities.

As an additional measure of general health, we asked "How has your health changed in the last 6 months?" (Q27). This is very similar to a question used in the National Health and Nutrition Examination Survey (CDC, 2005-2006). We examined whether the participant reported having worse health compared to health that was better or the same six months ago.

We assessed depression using the two-question version Patient Health Questionnaire (Kroenke et al, 2003). The questions (Q33 and Q34) ask about the primary symptoms of depression: dysphoric mood (feeling "down, depressed or hopeless") and anhedonia (being bothered by "little interest or pleasure in doing things") in the last 2 weeks. Each of the two questions was scored 0 – 3 (based answers ranging from "not at all" to "nearly every day") and the scores were summed. Those with a score of 3 or above were considered to have screened positive for depression. The PHQ-2 screen with a cut-point of 3 has a sensitivity of 82.9 and a specificity of 90.0 for major depressive disorder (Kroenke et al, 2003).

Access to care

Our measures of access to care were taken from the 2003 survey instrument for the Center for Studying Health System Change's Community Tracking Study (Center for Studying Health System Change, 2005). We made changes to the wording of various questions to simplify and to improve the survey flow based on our cognitive testing of our initial survey instrument. In addition, we made some slight changes to make the information gathered more specific to our setting.

We asked whether participants had a usual place of medical care (Q3) and where that usual place of care was (Q4). We considered participants to have a usual place of office- or clinic-based care if they indicated they did have a usual place of care and it was a private doctor's office or clinic, a public health clinic, community health center, tribal clinic or a hospital-based clinic. We did not consider participants to have a usual place of office- or clinic-based care if they indicated their usual place of care was a hospital emergency room or urgent care clinic. We also asked whether participants had a personal doctor or health care provider (Q5).

To assess whether participants received all needed medical care, we asked first if the participant needed medical care (Q6) and if so, whether they received all needed medical care (Q7). Participants who reported not needing medical care were considered to have received all needed medical care. Whether participants received all needed prescription medications was assessed in the same way (Q9 and Q10).

To further assess access to outpatient care, we examined whether participants used the emergency room for non-emergency care. Participants reported emergency room use (Q16) and reasons for that use (Q17). We considered a participant to have used the emergency room for non-emergency care if the participant reported having used the emergency room and did not indicate "I needed emergency care" as a reason.

Quality of care

Participants reported on the quality of the medical care received (Q19). We examined whether the care received was excellent, very good, or good versus fair or poor. This is not defined if the participant reported not having received medical care.

Preventive care

For preventive care, we based our questions on those used in the Behavioral Risk Factors Surveillance Survey (CDC, 1993-2008). We asked all participants about testing for cholesterol (Q37) and diabetes (Q38); we asked female participants about mammograms (Q39) and pap smears (Q40). We limit the analysis of use of pap smears to women and limit mammograms to women over age 40 in order to match the recommendations for appropriate care in place at the time (U.S. Preventive Services Task Force, 2002). For each of the preventive care measures, we examined whether the participant reported ever having had the test compared to never. We expected that most of the effect would be on care within the last year. Because some of the treatment sample responded to the twelve-month survey more than a year after receiving insurance, however, we were concerned that we would miss an early boost in the use of preventive care if we only looked in the last year.

Health behaviors

We inquired about smoking behavior using a set of three questions taken from the National Health and Nutrition Examination Survey (CDC, 2005-2006). Smoking was measured as reporting current cigarette use on some or all days (Q42). Those reporting never smoking (Q41) were not considered to be current smokers.

We asked about physical activity relative to other people of the same age (Q32) using a question from the National Health Interview Survey. This measure of perceived level of physical activity has been shown to correlate moderately with more detailed measures of self-reported physical activity (Weiss, 1990).

Other outcomes

Happiness was assessed using a question from the General Social Survey (National Opinion Research Center, 2008). Participants reported overall feeling very happy, pretty happy or not too happy (Q25). We compared those reporting feeling not too happy to those reporting feeling pretty or very happy.

For self-reported income, we assigned individuals the mid-point of the bin they reported. For the approximately 1.5 percent of the sample in the top bin ("above $50,000") we simply censored income at $50,000. We constructed income relative the federal poverty level using the self-reported income (the mid-point of the bin), self-reported number of total household members and the federal guideline.

Appendix 2: The randomization procedure and additional balance results.

This appendix provides supporting evidence for our analytical strategy based on random assignment. We first describe the randomization process, and then give evidence that the treatment and control groups are well-balanced.

2.1 Randomization process

The lottery's random selection process was performed by Oregon's Department of Human Services (DHS) on their mainframe computer; IBM DB2 software was used to perform the random selection (Oregon DHS, 2009). DHS provided us with a written description of their randomization procedure and the key pieces of the computer code they used to select individuals from the lottery list. We verified through independent computer simulations that we could replicate the results of their described procedure (to within sampling error). Specifically, we wrote our own program to implement the procedure they described to us, and ran it 500 times. The results are shown in Table A12. On all the characteristics of individuals on the lottery list that we can observe, the mean characteristics in the actual selected were well within the distribution of sample means from our 500 simulations (within two standard deviations of the mean of the means in the simulations). We are reporting this comparison for the entire original list, prior to the removal of duplicates and imposition of exclusions described in Appendix 1 (since this is the sample the state drew from).

2.2 Balance results

The above analysis was designed to verify whether or not the state in fact randomized as described to us. The results are consistent with the randomization process as described. Another important concern, however, is bias due to differential success in matching to administrative data or in response to our survey by treatment versus control. A priori we were most concerned about the survey data because of its 50 percent response rate. (As explained above, the effective match rate in the credit report data is 97 percent).

To investigate this concern, we examined treatment-control balance on pre-randomization characteristics for each of our three samples: the sample universe (analyzed in the hospital discharge and credit report data), the credit report subsample, and the survey respondent subsample. This allows us to look for differential matching or differential response rates (selection) between treatment and control groups based on pre-randomization characteristics.

We tried to select the pre-randomization outcomes that parallel the post-randomization outcomes actually analyzed; we can do this "exactly" in the hospital discharge and credit report data (since we have the same measures pre- and post-randomization); we tried to find reasonable approximations in the administrative data to proxy for pre-randomization measures of some of the survey outcomes. While the exact set of pre-randomization outcomes we analyze is of course somewhat arbitrary, they were all pre-specified in the analysis plan.[28] For the full sample used to analyze the hospital discharge data, we examined balance on 12 pre-randomization hospital outcomes. We can measure these outcomes from January 1, 2008 through the lottery notification date; on average we observe 5 months of pre-randomization hospital discharge data. For the credit report subsample, we examined balance on 10 pre-randomization credit outcomes measured in the February 2008 credit report file. To mimic our analysis of outcomes in the September 2009 credit report file (where we measure outcomes from the lottery-draw-specific notification date through September 30, 2009), we constructed look back periods for each lottery draw that were the same length in the February 2008 file as in the September 2009 file, so that the look-back period in the pre-randomization data for each lottery draw was the same as in the post-randomization data used in the main analysis.

As noted, we do not directly observe any pre-randomization outcomes in survey data. For the analysis of balance on pre-randomization outcomes for the survey respondent subsample, we instead use the administrative data to construct pre-randomization variables that reasonably closely approximate the

[28] The analysis plan was archived on December 3, 2010 at http://www.nber.org/sap/20101203/ and at hypotheses@povertyactionlab.org.

outcomes we measure in the survey data. Specifically, in the hospital discharge data we measure "any hospital admission for non-childbirth" and "number of hospital visits for non-childbirth" from January 1 2008 through the notification date (on average 5 months). In the credit report data we measure "any non-medical collection" and "any medical collection" in the February 2008 credit report data; these are designed to approximate the survey questions "did you borrow money or skip paying other bills to pay your medical bills" and "do you currently owe money for medical bills", respectively.

Table A13 lists the coefficients describing the balance for each of the individual outcomes. Panel A shows lottery list characteristics, while Panel B shows pre-randomization outcomes measured in the hospital discharge and credit report administrative data. The main interest is in the pooled-F statistics that are shown in Table 2 in the main text. In each case we are unable to reject the null of balance.[29] We also examined balance between treatment and control in the lottery list characteristics for the subsample that we drew to survey ("survey subsample") as a check on our own random drawing; the F-stat was 0.995 with p-value 0.441 (results not shown).

While this analysis is quite reassuring, it is naturally subject to several limitations. While we were able to create a comparable time period for pre-randomization credit report outcomes,[30] our pre-randomization period in the hospital discharge data is substantially shorter (5 months versus the 16 months available post-randomization, with no earlier period available as a control in the balance analysis). The main limitation with the survey data balance is that our pre-randomization measures come from administrative rather than survey data (and are thus measured differently).

2.3 Sensitivity of results to covariate adjustment

Consistent with these balance results, we find that the estimates are not sensitive to which covariates we control for. These results are shown in Table A14. Specifically, we investigate the sensitivity of our

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[29] The only coefficients that are individually significant at the five percent level are female (in the full sample in column 2) and any judgment in the credit report sample in column 3.

[30] We were able to match 94 percent of our September 2009 main credit report analysis sample back to the February 2007 archive. We included a dummy for outcomes missing in the February 2007 archive.

key standardized treatment effect estimates, including: total hospital utilization for all admissions in the hospital discharge data (Table 4b, Panel A); overall financial strain in the credit report data (Table 7, Panel A); and total health care utilization, overall financial strain and self-reported health in the survey data (respectively, Table 5 right hand panel, Table 8, and Table 9, Panel B). We report three sets of results: our baseline specification (columns 1 and 4 for the reduced form and 2SLS respectively, for comparison), a specification without controlling for pre-period y and lottery draw in the administrative data (columns 2 and 5 respectively), and our baseline specification adding controls for the lottery list covariates (columns 3 and 6, respectively). Overall the results are quite stable, as would be expected.

Appendix 3: Additional results

This section reports the findings from additional analyses pre-specified in the analysis plan but not reported directly in the main text.[31] More detail on baseline or preferred specifications is provided in the main text.

3.1 Hospital discharge data

Poisson estimates for total hospital utilization

Table A15 reports the results for quasi-maximum likelihood poisson estimates of the reduced form equation (1) for the three measures of total hospital utilization in the administrative data (days, list charges, and procedures). We estimated a proportional model for these outcomes – in addition to the linear estimates reported in Table 4b – because of the skewed nature of these outcomes (see Appendix 1, Table A3).[32] Since the estimates reflect proportional changes, instead of reporting a standardized treatment effect we report the simple average of the individual estimates. The results from the poisson model are qualitatively similar to the linear estimates in suggesting increases in all three measures, and the implied proportional effects of the linear reduced form are roughly similar in magnitude to the poisson estimates. However the Poisson estimates tend to be even less precisely estimated than the OLS estimates.

Utilization for specific conditions

We examined the impact of insurance on utilization for seven (mutually exclusive) conditions that are both of medical interest and of reasonably high prevalence in our population: heart disease, diabetes, skin infections, mental disorders, alcohol or substance abuse, back problems, and pneumonia; together these

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[31] The analysis plan was archived on December 3, 2010 at http://www.nber.org/sap/20101203/ and at hypotheses@povertyactionlab.org.

[32] A natural alternative would be a log model but the large proportion of zeros makes this inappropriate. The QMLE-Poisson model requires only that the conditional mean be correctly specified for the estimates to be consistent; see e.g. Wooldridge (2002, Chapter 19) for more discussion.

conditions account for about 35 percent of (non-childbirth) admissions.[33] Table A16 summarizes the results both for the extensive margin and for the standardized treatment effect across the three measures of total utilization. The results show a statistically significant increase in utilization (both extensive and total) for heart disease, but no evidence of increases in utilization for any of the other six conditions.[34]

Quality of care

We examined the impact of insurance on measures of quality of care in the hospital discharge data. Table A17 shows the results. Our measures of quality of care are based on the Agency for Healthcare Research and Quality (AHRQ) Quality Indicators (AHRQ 2006a). These are measures of health care quality that can be coded in hospital discharge data; AHRQ makes software to code these freely available on the web (AHRQ downloads). Our measures capture quality of different aspects of care, although each has important limitations in interpreting them this way, which we note below. We divide our quality measures into measures on outpatient and inpatient care.

For quality of outpatient care, we measure admissions for ambulatory-care sensitive conditions. We coded admissions as ambulatory-care sensitive using the AHRQ Prevention Quality Indicators criteria. These criteria are intended to identify admissions that could potentially be prevented with better quality outpatient care. They include admissions for complications of diabetes, bacterial pneumonia and asthma. We examined whether an individual was admitted for an ambulatory-care sensitive condition. Conditional on admission, about 13 percent of people in our sample admitted to the hospital had an

[33] The conditions are mostly groupings of multiple diagnosis codes (see Appendix 1.5 for details), but include the seven most common clinical conditions (mood disorders, skin infection, diabetes, alcohol-related disorders, schizophrenia or psychoses, spondylosis or other back problems, and pneumonia). Although our selection of conditions is somewhat ad hoc, it has the virtue of having been pre-specified in the analysis plan.

[34] The increase in total utilization for heart disease is in turn driven by statistically significant increases in each of the three components of the index (hospital days, number of procedures, and list charges). The heart disease category is itself a composite of several heart conditions, including acute myocardial infarction and congestive heart failure, which may require both acute and scheduled care. Although the results in Table A16 do not adjust for multiple inference, it is easy to see that even the low-powered Bonferroni adjustment (in which the p-value for any given outcome is multiplied by the number of outcomes shown in the Table) would still leave the results for heart disease statistically significant.

admission for an ambulatory sensitive care condition, the most common ones being complications from diabetes, pneumonia and asthma (together accounting for 60% of these ambulatory-case-sensitive conditions). We use this as a way of inferring the quality of outpatient care.

The results are shown in Panel A of Table A17. The 2SLS estimates indicate a statistically insignificant increase of 0.2 percentage points (standard error = 0.3) in admissions for ambulatory care sensitive conditions; the point estimate suggests an approximately 22 percent increase in admissions for ambulatory care sensitive conditions (off of a baseline – unconditional on admission – of 0.9 percent). With 95 percent confidence we can reject declines in admissions for ambulatory care sensitive conditions of more than 0.2 percentage points (or one fifth). The interpretation is unclear. It may be that outpatient quality did not improve. It may be that it improved but we do not have the power to detect an effect. It may be that insurance does improve outpatient quality of care for these conditions but that this is masked by an offsetting price effect which increases admissions for these conditions among the insured.

We also examined three measures of the quality of inpatient and subsequent care: not having a patient-safety event in the hospital (such as post-operative infections, bedsores, etc.), not being readmitted within 30 days of discharge, and the average quality of the hospital to which patients are admitted. As each of these three is intended to capture some aspect of inpatient quality of care, we combined them into a single domain and calculated the standardized treatment effect across all three. We note that an important caveat with all three inpatient quality of care measures is that they are each measured conditional on having a hospital admission. We did so because one is only "at risk" for patient safety events and re-admission if one is admitted, and because average hospital quality is not defined for individuals who are not admitted. However, if health insurance changes the composition of admissions this may well complicate interpretation of the results.

We coded admissions as *including a patient safety event* using the AHRQ Patient Safety Indicators (AHRQ 2006b) criteria. These criteria are intended to identify admissions with potentially preventable adverse events or complications. There are 25 such conditions total, of which 3 are obstetric-specific and therefore excluded. The conditions include, for example, foreign bodies being left behind during procedures, infections due to medical care, deaths in low-mortality conditions, and certain postoperative complications. Rates of these complications have been found to vary across hospitals, but do not necessarily correlate with other measures of hospital quality (Romano 2003; Isaac 2008). Conditional on admission, less than 0.2 percent of our sample has a patient safety event.

We coded an admission as leading to a *readmission* if the same individual had a separate admission beginning within 30 days of the discharge date for the index admission. We limit this variable to those with an index admission occurring by June 30, 2009 in order to be able to observe the full 30-day window (we need to allow enough time for the full index admission, up to 30 days, and then the full secondary admission). We note that care must be taken in interpreting re-admission as a measure of quality of care received (in the hospital or post-discharge) since presumably re-admission rates may also reflect underlying health status at time of admission, which may also vary across treatment and control; therefore re-admission is not a pure measure of quality of care, although it is often interpreted as such. Conditional on admission, about 12 percent of our sample is re-admitted within 30 days.

We used data from the Hospital Quality Alliance process-of-care measures to assess the *quality* of the hospitals to which our sample was admitted. These data are made publicly available from the Center for Medicare and Medicaid Services' Hospital Compare website (http://www.hospitalcompare.hhs.gov/).[35] The process-of-care measures show how often patients at a given hospital receive recommended treatments for specific conditions. The measures include, for example, the percent of heart attack patients given aspirin at arrival, the percent of pneumonia patients given influenza vaccination, and the percent of surgery patients who were given an antibiotic within one hour before surgery. There are seven measures related to heart attack care, four related to heart failure care, six related to pneumonia care, and eight related to surgical care. Higher composite scores for each condition-specific set of measures have been

[35] These data were not available for 5 of our 58 hospitals – representing less than 2% of the admissions – because the sample sizes were too small.

associated with better outcomes for those conditions (Jha, 2007; Stulberg, 2010). We standardize each measure (i.e. subtract the mean and divide by the standard deviation) because some are more dispersed than others, and take an average of the standardized measures across all conditions as a summary of quality. Note that this hospital quality variable is defined based on the treatment of all patients at the hospital, not simply those in our sample. For individuals in our sample with hospital admissions at different hospitals, we define their average hospital quality as the length-of-stay-weighted average of hospital quality for all admissions.

Once again, the results in Table A17 are difficult to interpret. The 2SLS estimate of the standardized treatment effect across the three measures indicates that insurance is associated with a 0.026 standard deviation (standard error = 0.061) increase in these quality of inpatient care measures. None of the individual effects is remotely close to statistically significant. Whether there is a real improvement that we lack the power to detect or whether there is no improvement in inpatient quality is difficult to determine.

Sorting across hospital types

We examined whether insurance affects the type of hospital to which individuals are admitted. The results are shown in Table A18. We used the American Hospital Association 2008 Annual Survey data to identify the ownership of the 58 hospitals in our data. Most of the hospitals are not-for-profit (43 of the 58 hospitals) and only a few are for-profit (2 of the 58). The remaining 13 are public. Because there are so few for-profit hospitals, we separate hospitals into public and private (where private includes both for-profit and not-for-profit).

For all our analysis of hospital type we estimated logit (proportional) models since one would naturally expect any increase in hospitalization associated with the treatment to be larger (in level terms) at larger hospitals; our question is whether insurance changes the distribution (proportion) of patients across different hospitals, specifically the mix between public and private hospitals. Of course, any analysis of the impact of insurance may conflate substitution across hospital types with compositional changes (insurance may affect the type of patient that goes to a hospital and different types of patients may use different types of hospitals). Insurance is more likely to affect the type of hospital patients go to in areas of the state where there is genuine choice among hospitals, as opposed to areas (presumably rural) where there is only one (type of) hospital readily available. Whether an individual had hospital "choice" was defined at the zip code level (based on the entire Oregon hospital discharge data set, not just our lottery sample). Specifically, we defined an individual in our sample as having "choice" over whether to go to a public vs. private hospital if they lived in a zip code in which more than 10% and less than 90% of total admissions for all patients in that zip code were to a public hospital; this preserves approximately 40% of our sample. We also performed analyses limiting the "with choice" subsample to admissions that did not originate in the emergency department. We expect that, even if there is a choice of hospitals, patients are more likely to be able to choose in non-emergency situations.

The results are shown in Table A18 and do not indicate any evidence of insurance affecting sorting by hospital type.

3.2 Credit report data

Access to credit

We examined whether health insurance improves access to credit in our low income and severely credit-constrained population. A priori, we suspected such an access effect was unlikely. Any effect would have to be indirect, since whether one has health insurance is not a matter of public record, nor is it information that credit bureaus collect or that enters algorithms for credit scores. The most likely channel by which health insurance may improve access to credit is by reducing the rate of medical collections – which are major negative financial events that adversely impact one's credit score (and hence future access to credit). However, it is unclear exactly how important collections are to credit scores – the credit scoring algorithms are proprietary and our data use agreement prohibits our attempting to "reverse engineer" them – and how long a lag would be needed before a decline in collections would translate into an improved credit score. It is also possible that health insurance could decrease access to credit in the long run. For example, if health insurance initially increased access to credit and one therefore accumulated more debt, it could ultimately translate into a worse assessment of one's credit-worthiness. A related mechanism by which health insurance could first increase and then reduce access to credit in our severely credit-constrained population is that increased access to credit might cause individuals to shift borrowing from "off-the-books" mechanisms (like pawn shops or family members) to "on-the-books" mechanisms; any delinquency would then show up in one's credit file and could therefore worsen one's perceived credit-worthiness. We note that such a substitution story does still constitute a "real" outcome if interpreted correctly; in other words, our measures of credit access should be interpreted less as measures of true credit-worthiness (since there may be substitution that leads to more formal on-the-books borrowing holding total borrowing constant) than of the market's assessment of credit worthiness (since on-the-books borrowing affects credit scores that affect future access to credit); the latter is a real and interesting outcome.

We analyze three measures of access to credit: (1) having a credit score[36] (2) having a "thick file" (defined as having two or more open trade lines of any kind, including revolving credit or installment

[36] Credit bureaus use the data in credit reports to generate a "credit score" for the individual. This provides a measure of the market's assessment of the individual's credit worthiness, and is relied on heavily by lenders in determining whether and at what terms to lend to an individual. Specifically, it is based on the probability of being seriously delinquent (i.e. 90 days or more delinquent on a payment) on a credit account in the next two years; note that while collections and public records are not captured directly in this outcome measure, they figure importantly in the algorithm by which this outcome is predicted (i.e. in the generation of the credit score) and therefore can have substantial affects on one's ability to obtain credit. We analyze the "VantageScore" credit risk score provided to us by the credit bureau. It can range from a low of 501 (the worst) up to a high of 990 (the best); scores have a letter grade attached to them ranging in 100 point increments from "A" (901-990) to "E" (501-600) (see e.g.; http://www.mortgagefit.com/credit-rating/vantagescore.html). About 80 percent of our sample has a credit score. It is not clear how to treat those without credit scores, as they do not necessarily have worse "latent" credit scores than those with credit scores; rather they have insufficient credit history or recent activity to form a credit score. Having a credit score is therefore a measure of credit activity. Moreover, those with credit scores in our population tend to have extremely low scores. About 40 percent have a score that puts them in the "high risk" category (i.e. grade of E), which means that they are likely to be turned down by lenders, and another thirty percent have a score in the "non prime" category (i.e. grade of D), which means that they can get access but on less favorable terms than typical; only about 30 percent of those with scores (or about 16 percent of the whole population) have scores that would qualify them for credit on reasonably favorable terms.

loans),[37] and (3) total current credit limit across all open revolving credit (this is mechanically zero for the approximately half of our full sample that has no open revolving credit at the end of our study period).[38] Note that although we call these measures of "access to credit" they are not pure supply side measures. All of them reflect a combination of access to credit and demand for credit; i.e. we do not observe "latent access to credit," only credit that was applied for and granted.

We are interested in improvements in access to credits for two related but distinct reasons. First, at a substantive level, we are interested in whether individuals experience increased access to credit. Second, if health insurance improves access to credit, we have to exercise caution in interpreting a decrease in adverse financial events in the credit report data as evidence of decreased financial strain; as described above, there could be perverse results, for example, whereby an improvement in the market's assessment of an individual's credit worthiness encourages plaintiffs' attempts to collect against unpaid bills (since the individual is viewed as having "deeper pockets"), or provides new credit which provides opportunities to be late on paying. Thus we believe that if health insurance increases access to credit we are biased against finding that health insurance reduces financial strain as measured by our "adverse financial events" (since the potential for adverse financial events increases).[39]

These two purposes suggest two different time frames over which to measure "access to credit". For our substantive analysis, we examine access to credit at the end of our study period (September 2009).

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[37]Having a thick file is a measure of credit activity used by some credit bureaus. It is a more stringent measure than having a credit score; only about forty percent of the sample has a thick file (and everyone with thick files has a credit score).

[38] We construct this measure – following the approach of the credit bureau – by summing across the credit limit on each open revolving trade line (if reported) and if not reported using the maximum prior balance on record for that trade line to proxy for the credit limit. In practice, we only need to use the highest prior balance on less than 10 percent of our open revolving trade lines.

[39] Moreover, we believe that any bias resulting from an impact of insurance on access to credit likely contaminates different measures to different degrees. In particular, the route seems more indirect for bankruptcies, liens, judgments, and collections (operating via a perceived effect on ability to collect on (largely non-credit) unpaid bills) than for the credit measures of late payments (where there could be a literal expansion in the "risk set" of late payments through an expansion of credit limits). The concern with the late payment on credit accounts measures as measures of financial strain is perhaps particularly severe in our low income population where increased access to credit could encourage substitution of formal for informal borrowing. This – together with the relatively limited use of credit by our population – motivated our primary focus on measures of financial strain from public records and collections data.

However for purposes of interpreting the adverse financial events measures we also examine the "maximum access to credit" over the study period (notification date through September 2009). For this we use data from February 2009 in addition to September 2009 to look at the maximum. Our rationale here is that it is possible that health insurance might first increase access to credit but then delinquencies could cause that access to contract; our goal was to assess whether access to credit ever went up over our time period.

The results are shown in Table A19. The first four columns show results for "current access to credit" while the last four show results for "maximum access to credit". We report results both for the full credit report sample (Panel A) and for the approximately 55 percent of our credit report sample who had some revolving credit in February 2008 (Panel B). A primary reason for analyzing this subsample is that perhaps the best measure of access to credit – i.e. credit score – is only defined among those with prior credit. Therefore for this subsample instead of analyzing whether you have a credit score at all, we analyze your actual credit score; over 98 percent of this subsample has a credit score in September 09. The credit score is the market's assessment of the individual's credit worthiness, with higher numbers reflecting better perceived credit worthiness (and hence access to credit). We set the credit score to missing for the small fraction of the prior credit sample who do not have a credit score. The results show no statistically or economically significant impact on access to credit during our study period.

Balances owed on revolving credit

We also examined the impact of health insurance on balances owed on all open revolving credit, which is another potential measure of financial strain. We viewed this analysis as exploratory for two primary reasons. First, it is difficult to know what to expect – or how to interpret – a change in total balances. On the one hand, if one is less financially strained one may carry lower balances. On the other hand, it is possible (although presumably unlikely in our population) that an *increase* in this measure could reflect decreased financial strain (if health insurance reduces the need for precautionary savings).

Second, a preferred measure might be one's balances relative to one's credit limit. We do not analyze this variable, however, because it is not defined for the almost half of our sample without revolving credit.[40]

Parallel to our "credit limit across all open revolving credit", we define a total balance variable that gives balances on all open revolving credit (with those without any balances coded as zero). Note that both the credit and the balance variable include delinquent accounts but not closed accounts (since information is often not updated after an account is closed; in addition, one presumably no longer has "access" to the credit limit on a closed account).

Table A20 reports our analysis of the impact of health insurance on balances owed. We find nothing.

3.3 Survey data

Labor force participation

We performed exploratory analysis of labor force participation. The impact of public health insurance eligibility on labor force participation is ex ante ambiguous. On the one hand, by potentially improving health and/or the efficiency of care delivery, health insurance may make it easier to participate in the labor force. On the other hand, public health insurance eligibility may discourage labor force participation because of its income eligibility ceiling and/or because one of the incentives for the uninsured to gain employment may be to get access to health insurance.

We looked at three measures of labor force participation: whether currently employed, whether currently working 20+ hours per week (which is a natural dividing line above which employers are more likely to offer health insurance), and gross (pre-tax, but post cash transfer) household income.[41] The results shown in Table A21 lack precision, but the point estimates are suggestive of an increase in labor force participation associated with insurance. The 2SLS estimate of the standardized treatment effect

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[40] Only about 55 percent of our sample has revolving credit. Moreover, even conditioning on having revolving credit prior to randomization (February 08), only 85% of our sample has revolving credit in September 09.

[41] Note that (as detailed in Appendix 1) income is reported in bins and we use the midpoint of each bin for our income. This means that any movement in income "within a bin" due to health insurance will not be captured by our estimates. In addition, income is censored at the top-coded value of $50,000 for about 1.5 percent of the population.

indicates a 0.05 standard deviation increase labor force participation (standard error = 0.04) associated with insurance; this reflects (statistically insignificant) increases in all three of the individual measures.

Health behaviors

We also examined the impact of health insurance on two “health behaviors”: smoking and a measure of physical activity. The results are shown in Table A22. Their interpretation is not obvious. We find no evidence of a decline in the probability of smoking. We find a substantial and statistically significant increase in the probability of reporting that one is more physically active than others one’s age. While this can be viewed as a potential measure of exercise effort (following Weiss, 1990), it could also – particularly in the context of other health questions (see Figure A4) – be interpreted as another measure of self-reported health rather than a health behavior.

3.4 Heterogeneous treatment effects

We explored potential heterogeneity in treatment effects along a number of pre-specified dimensions including demographics, socio-economic status, and a proxy for health. In general, we lacked the power to draw any sharp conclusions about differential impacts.

Table A23 reports the results. Since the first stage may differ across groups, we report the 2SLS estimates of equation 2 (rather than the reduced form estimates of equation 1). The first row replicates the baseline results. We examined results by various demographics, including gender, age (50-63 vs. 19-49 at the end of 2008), race (white vs. non-white as reported in the main survey), and urbanicity (MSA vs. non-MSA). We also examine results by measures of SES. While everyone in the sample is quite poor, we can subdivide them along two dimensions. First, using the credit report data from February 2008 (prior to randomization), we distinguish between those with some vs. effectively no access to mainstream credit (i.e. whether they have any revolving credit). Second, using the survey respondent subsample, we split the sample by self-reported education (high school education or less vs. more than high school).[42] Finally, as a proxy for initial health status, we split the survey respondents into those who report "ever smoking" and those who do not. [43]

In general, we lack power to make precise statements. In the survey data, there is some weak evidence that older individuals may have a greater increase in health care utilization, and that higher SES individuals (within this relatively low SES group) may have a smaller increase in health care utilization.

The first stage estimates for different sub-populations also provides information on the characteristics of the complier population relative to the overall study population. The relatively likelihood a complier compared to a randomly drawn person in the sample has a given characteristic is indicated by the ratio of the first stage for people of that characteristic relative to the overall first stage (see Angrist and Pishke (2009, page 171). As indicated by column 2 of Table A23, compliers are, among other things, disproportionately white, disproportionately smokers, and disproportionately constrained financially as measured by whether or not they had access to credit prior to the lottery.

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[42] We suspected a priori that education was (relatively) immutable and not responsive to insurance in our population. In practice, estimation of equation (1) with "high school or less" as the binary dependent variable (control mean = 0.67) yields a substantively and statistically insignificant coefficient of -0.0009 (standard error =0.007).

[43] Smoking is both a direct contributor to poor health and correlated with measures of poor health. We considered it a priori unlikely that insurance coverage would affect whether you ever smoked and indeed estimation of equation (1) with "ever smoked" as the binary dependent variable (control mean =0.64) yields a substantively and statistically insignificant coefficient of -0.004 (standard error = 0.007). Age may also be a proxy for health (since older people are in worse health) although of course it captures other things.

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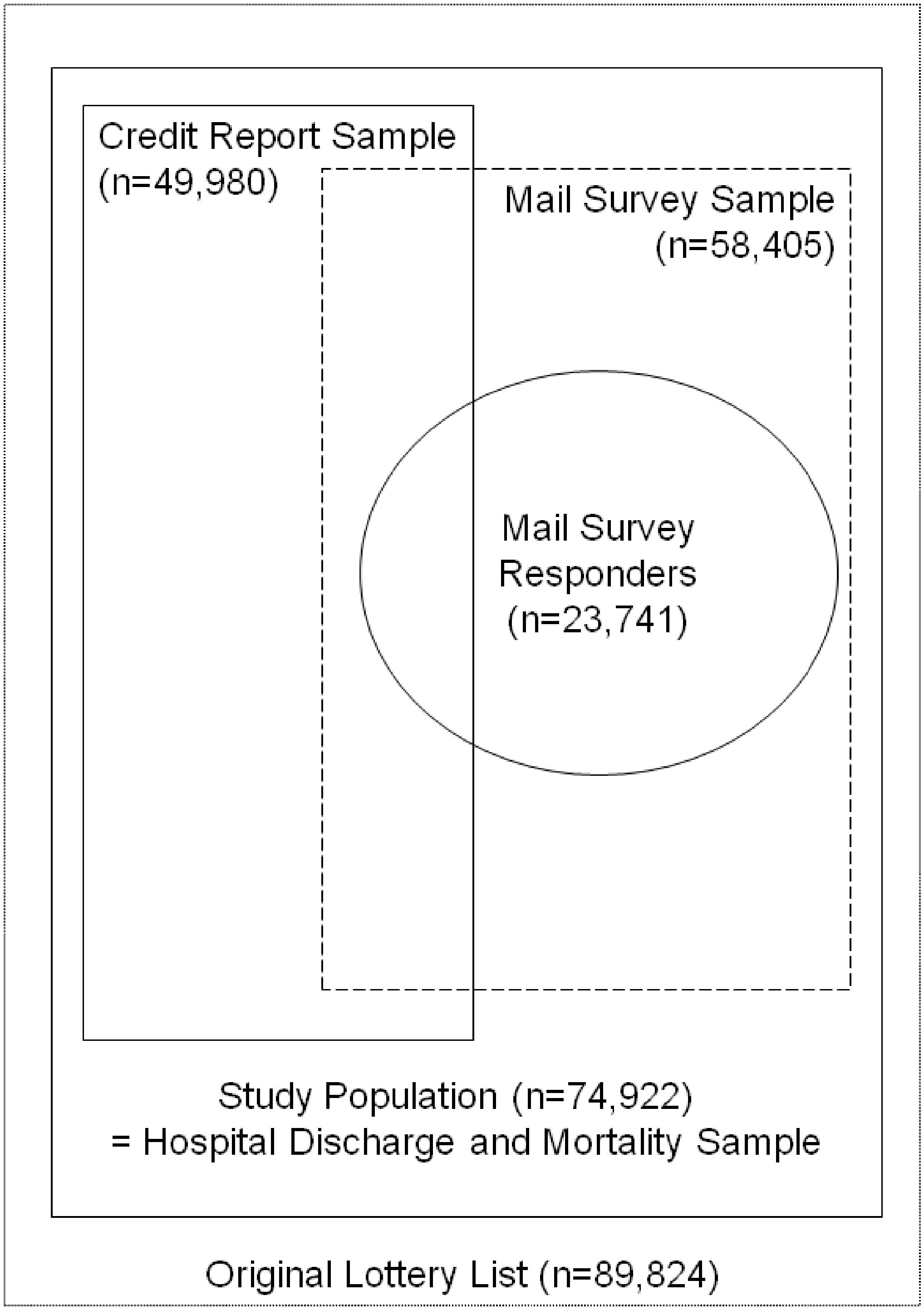
Appendix Figure A1: Overlapping sample



Appendix Figure A2: Lottery Request Form

OHP Standard reservation list request

You can give us your reservation request in any of the following ways:

- **Electronically –** Use the link on www.oregon.gov/DHS/open to give us your information.
- **Mail –** Mail this form to OHP Standard, PO Box 14520, Salem, OR 97309-5044.
- **Fax –** Fax this form to: 503-373-7866 or 503-378-6295.
- **In person –** Drop this form off at any DHS field office (call 800-699-9075 for locations).
- **Phone –** Call 800-699-9075 or 503-378-7800 (TTY), Mon-Fri, 7a.m. - 7p.m. PST. The call will take 10-20 minutes.

① Your name (Last, First, M.I.) | Maiden or other names used

Phone Number ( ) | Message Number ( )

Home Address | City | State | ZIP

Mailing Address (if different) | City | State | ZIP

② List anyone 19 or older in your household you want to add to the reservation list.

Name (Last, First, M.I.)	Relation to you	Gender	Date of Birth	*(voluntary)* * Social Security Number
	Self	☐ M ☐ F		
		☐ M ☐ F		

*Providing a Social Security Number (SSN) is voluntary for the OHP Standard Reservation List request. DHS is allowed to ask for SSNs by OAR 461-135-1125(5) to help identify people to prevent duplicate reservations. DHS will not deny a request to be placed on the OHP Standard Reservation List if you do not provide an SSN.

③ If you need materials in a language other than English, check the appropriate box.

☐ Spanish ☐ Russian ☐ Vietnamese ☐ Other: ____________________

④ If you want written materials in a different format, check the box that applies:

☐ Braille – information is printed in Braille.

☐ Audio tape – information is recorded on an audiocassette tape.

☐ Large print – materials are printed in this size.

☐ Computer disk – information is saved as "plain text" on a 3.5-inch floppy disk.

☐ Spoken – information is read by a DHS employee in person or over the telephone.

I understand that this request is not an application for medical assistance.

Signature | Date

OHP 3203 (10/25/07)

OHP Standard reservation list request

The Oregon Health Plan (OHP) is a medical assistance program for low-income Oregonians. OHP offers two primary benefit packages – OHP Plus and OHP Standard.

The OHP Standard benefit package offers medical assistance to low-income people who are 19 or older and not pregnant. OHP Standard has been closed to new enrollment for the past three years because of funding restrictions.

We are getting ready to open the OHP Standard program to a limited number of people. To be fair to everyone, we have created an OHP Standard reservation list. Anyone can ask to be put on the reservation list – the reservation list is not an application.

The OHP Standard reservation list will be open from January 28 - February 29, 2008. During this time frame you can add your name to the list. The front of this form shows the ways you can request an OHP Standard reservation.

Once your name has been added to the reservation list, we will send you a confirmation postcard with a reservation number. After the list is closed, we will randomly pull a limited number of names and mail OHP Standard Reservation List applications to those individuals.

Remember

1. We can add your name to the reservation list only between January 28 - February 29, 2008. We will not accept reservation requests that are received after February 29, 2008, even if they are postmarked with an earlier date. If you are making your request close to the end of February 2008 you should make your request by phone, fax or electronically.

2. Anyone can add your name to the list. You can have someone else call or send in a request form for you. You can have only one reservation.

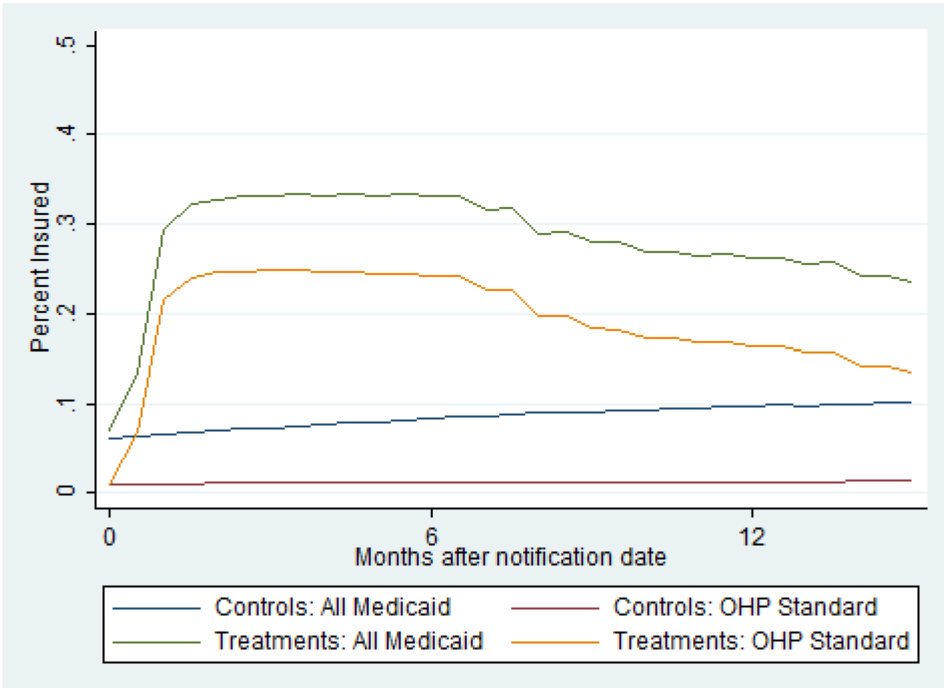
3. This information applies only to OHP Standard. OHP Plus and other benefit packages have different eligibility requirements. You already may qualify for coverage through one of these packages. To see if you or anyone in your household is eligible, you must complete an OHP Application. To request an OHP Application, call 1-800-359-9517 or pick one up at your local DHS branch office.

4. Adding yourself to the reservation list does not mean you have applied for or qualified for any kind of OHP coverage. At any time you can complete an OHP Application to see if you already qualify for OHP coverage, even if your name is on the reservation list.

5. You can have only one reservation. We will not add your name to the reservation list more than once.

OHP 3203 (10/25/07)

Appendix Figure A3: Time path of first stage.



Appendix Figure A4: Twelve-month survey instrument

Your Health Matters

OREGON

Health Study

OREGON HEALTH STUDY
12-MONTH FOLLOW-UP SURVEY

About a year ago, we sent you the first survey in the ongoing Oregon Health Study. Now, please help the study continue by telling us about your health and health care experiences in the last six months. Your experiences will help Oregon leaders improve access to health care in the future. Whether you were able to respond to the last survey or not, it is extremely important for us to hear from you on this survey.

You may choose to answer this survey or not. If you do, all information that would let someone identify you or your family will be kept private. Your personal information will not be shared with anyone without your OK. Choosing not to answer this survey will not affect any health benefits you may be receiving.

If you return this survey, you will be entered into a drawing to win $200.

You may notice a number on this survey. This number is used only to let us know if you returned your survey so we don't keep sending reminders, and to enter you into the $200 drawing.

Before you fill out this survey, please **read the included letter** explaining the study. If you have questions, want to know more about the study, or want to leave the study, please call 1-877-215-0686, or visit our website at **www.OregonHealthStudy.org**.

OHS

Survey Instructions

1. Answer all the questions by checking the box to the left of the answer.
2. You are sometimes told to skip over questions in this survey. When this happens, you will see an arrow with a note that tells you what question to answer next, like this:

   ☑ Yes ➡ **(Go to Question 1)**
   ☐ No

START HERE ⬇

Your Health Coverage

1. Do you **currently** have health insurance through any of the following? *Mark all that apply.*
   - ❑ Oregon Health Plan (OHP)/Medicaid
   - ❑ Medicare
   - ❑ Employer or family member's employer
   - ❑ A private plan I pay for myself
   - ❑ Other coverage: ________________________
   - ❑ I don't have any insurance now
   - ❑ I don't know

2. For how many of the **last 6 months** did you have some kind of health insurance?
   - ❑ No insurance during last 6 months
   - ❑ 1 Month
   - ❑ 2 Months
   - ❑ 3 Months
   - ❑ 4 Months
   - ❑ 5 Months
   - ❑ Insured for all of the last 6 months

Your Health Care

3. Is there a place you **usually** go to receive medical care?
   - ❑ Yes
   - ❑ No ➡ **(Go to Question 5)**

4. Where do you usually go to receive medical care? *Mark only one.*
   - ❑ A private doctor's office or clinic
   - ❑ A public health clinic, community health center, or tribal clinic
   - ❑ A hospital-based clinic
   - ❑ A hospital emergency room
   - ❑ An urgent care clinic
   - ❑ Some other place not listed here
     ↳ *Where?* ___________________________
   - ❑ I don't have a usual place
   - ❑ I don't know

5. Do you have one person you think of as your personal doctor or health care provider?
   - ❑ Yes
   - ❑ No

6. Was there a time in the **last 6 months** when you needed medical care?
   - ❑ Yes
   - ❑ No ➡ **(Go to Question 9)**

Continue

7. If you needed medical care in the **last 6 months**, did you get **all** the care you needed?
   - ❑ Yes ➡ **(Go to Question 9)**
   - ❑ No
   - ❑ I didn't need care in the last 6 months

8. The **most recent time** you went **without** needed medical care, what were the main reasons? *Mark all that apply.*
   - ❑ It cost too much
   - ❑ I didn't have insurance
   - ❑ The doctor wouldn't take my insurance
   - ❑ I owed money to the care provider
   - ❑ I couldn't get an appointment quickly enough
   - ❑ The office wasn't open when I could get there
   - ❑ I didn't have a doctor
   - ❑ Some other reason: ______________________
   - ❑ I don't know

9. Was there a time in the **last 6 months** when you needed **prescription medication**?
   - ❑ Yes
   - ❑ No ➡ **(Go to Question 13)**

10. If you needed prescription medications in the **last 6 months**, did you get **all** the medications you needed?
    - ❑ Yes ➡ **(Go to Question 12)**
    - ❑ No
    - ❑ I didn't need medications in the last 6 months

11. The **most recent time** you went **without** prescription medications you needed, what were the main reasons? *Mark all that apply.*
    - ❑ They cost too much
    - ❑ I didn't have insurance
    - ❑ I didn't have a doctor
    - ❑ I couldn't get a prescription
    - ❑ I couldn't get to the pharmacy
    - ❑ Some other reason: ______________________
    - ❑ I don't know

12. How many different prescription medications are you currently taking?
    ______ *prescription medications*

13. Was there a time in the **last 6 months** when you needed **dental care**?
    - ❑ Yes
    - ❑ No ➡ **(Go to Question 15)**

14. If you needed dental care in the **last 6 months**, did you get **all** the care you needed?
    - ❑ Yes
    - ❑ No
    - ❑ I didn't need dental care in the last six months

15. In the **last 6 months**, how many times did you go to a doctor's office, clinic, or other health care provider to get care for yourself? *Don't include hospital and emergency room visits or dental care. Your best estimate is fine.*
    - ❑ None
    - ❑ 1 time
    - ❑ 2 times
    - ❑ 3 or more times
      How many? ______

16. In the **last 6 months**, how many times did you go to an emergency room to get care for yourself? *Your best estimate is fine.*
    - ❑ None ➡ **(Go to Question 18)**
    - ❑ 1 time
    - ❑ 2 times
    - ❑ 3 or more times
      How many? ______

17. The **most recent time** you went to the emergency room, what was the reason you went there instead of somewhere else for health care? *Mark all that apply.*
    - ❑ I needed emergency care
    - ❑ I didn't have insurance
    - ❑ Doctors' offices/clinics were closed
    - ❑ I couldn't get an appointment to see a regular doctor soon enough
    - ❑ I didn't have a personal doctor
    - ❑ I couldn't afford the copay to see a doctor
    - ❑ I needed a prescription drug
    - ❑ I didn't know where else to go
    - ❑ Some other reason: ______________________
    - ❑ I don't know
    - ❑ I haven't gone to the emergency room in the last 6 months

Continue

18. In the **last 6 months**, how many different times were you a patient in a hospital at least overnight? *Do not include hospital stays to deliver a baby.*
   - ❑ None
   - ❑ 1 time
   - ❑ 2 times
   - ❑ 3 or more times
     - *How many?* ______

19. Overall, how would you rate the **quality** of the medical care you've received in the **last 6 months**?
   - ❑ Excellent
   - ❑ Very Good
   - ❑ Good
   - ❑ Fair
   - ❑ Poor
   - ❑ I didn't receive medical care in the last 6 months

Your Health Care Costs

20. In the **last 6 months**, have you paid any out of pocket medical expenses for yourself? *(Out of pocket costs are costs you pay yourself. Do not include dental costs.)*
   - ❑ Yes
   - ❑ No ➡ **(Go to Question 22)**

21. In the **last 6 months**, about how much money did you spend out of pocket on each of the following types of medical care for yourself? *Do not include dental costs. Out of pocket costs are costs you have already paid yourself. Your best estimate is fine.*

   A. Visits to doctors' offices, clinics or health centers
   - ❑ $0 - no money out of pocket
   - ❑ More than $0
     - I spent about this much: $________

   B. Emergency rooms or overnight hospital care
   - ❑ $0 - no money out of pocket
   - ❑ More than $0
     - I spent about this much: $________

   C. Prescription medicines (don't include medicines you can buy without a prescription)
   - ❑ $0 - no money out of pocket
   - ❑ More than $0
     - I spent about this much: $________

   D. Other medical care not covered above
   - ❑ $0 - no money out of pocket
   - ❑ More than $0
     - I spent about this much: $________

22. Do you **currently** owe money to a health care provider, credit card company, or anyone else for medical expenses?
   - ❑ Yes ➡ *If yes, about how much do you owe?* $____________
   - ❑ No

23. In the **last 6 months**, have you had to borrow money, skip paying other bills, or pay other bills late in order to pay health care bills?
   - ❑ Yes
   - ❑ No

24. In the **last 6 months**, has a doctor, clinic, or medical service refused to treat you because you owed money to them for past treatment?
   - ❑ Yes
   - ❑ No
   - ❑ I don't know

Your Health

25. Taken all together, how would you say things are these days–would you say that you are very happy, pretty happy, or not too happy?
   - ❑ Very happy
   - ❑ Pretty happy
   - ❑ Not too happy

26. In general, would you say your health is:
   - ❑ Excellent
   - ❑ Very Good
   - ❑ Good
   - ❑ Fair
   - ❑ Poor

27. How has your health changed in the **last 6 months**?
   - ❑ My health has gotten better
   - ❑ My health is about the same
   - ❑ My health has gotten worse

28. Thinking about your physical health, which includes physical illness and injury, for how many days during the **past 30 days** was your physical health **NOT GOOD**?
   - Total number of days (0-30): __________

Continue

29. Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the **past 30 days** was your mental health **NOT GOOD**?

Total number of days (0-30): __________

30. During the **past 30 days**, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?

Total number of days (0-30): __________

31. Does a physical, mental, or emotional problem now limit your ability to work at a job or business?
- ❑ Yes
- ❑ No

32. Compared to most people your age, are you more physically active, less physically active, or about the same?
- ❑ More physically active
- ❑ About the same
- ❑ Less physically active

33. Over the **past 2 weeks**, how often have you been bothered by little interest or pleasure in doing things?
- ❑ Not at all
- ❑ Several days
- ❑ More than half the days
- ❑ Nearly every day

34. Over the **past 2 weeks**, how often have you been bothered by feeling down, depressed, or hopeless?
- ❑ Not at all
- ❑ Several days
- ❑ More than half the days
- ❑ Nearly every day

35. Have you ever been told by a doctor or other health professional that you have any of the following?

	Yes	No
Diabetes or Sugar Diabetes	❑	❑
Asthma	❑	❑
High Blood Pressure	❑	❑
Emphysema or Chronic Bronchitis (COPD)	❑	❑
Heart Disease, Angina, or Heart Attack	❑	❑
Congestive Heart Failure	❑	❑
Depression or Anxiety	❑	❑
High Cholesterol	❑	❑
Kidney Problems	❑	❑

36. In the **last 6 months**, have you **taken medication** for any of the following?

	Yes	No
Diabetes or Sugar Diabetes	❑	❑
Asthma	❑	❑
High Blood Pressure	❑	❑
Emphysema or Chronic Bronchitis (COPD)	❑	❑
Heart Disease, Angina, or Heart Attack	❑	❑
Congestive Heart Failure	❑	❑
Depression or Anxiety	❑	❑
High Cholesterol	❑	❑
Kidney Problems	❑	❑

37. Have you ever had your blood cholesterol checked?
- ❑ Yes, within the last year
- ❑ Yes, but it's been more than a year
- ❑ Never

38. Have you ever had a blood test for high blood sugar or diabetes?
- ❑ Yes, within the last year
- ❑ Yes, but it's been more than a year
- ❑ Never

The next two questions ask about health screenings recommended for women. **If you are male, please skip ahead to question 41.**

39. Have you ever had a mammogram?
- ❑ Yes, within the last year
- ❑ Yes, but it's been more than a year
- ❑ Never

40. Have you ever had a pap test or pap smear?
- ❑ Yes, within the last year
- ❑ Yes, but it's been more than a year
- ❑ Never

41. Have you smoked at least 100 cigarettes in your **entire life**?
- ❑ Yes
- ❑ No ➡ **(Go to Question 45)**

Continue

42. Do you **now** smoke cigarettes every day, some days, or not at all?

- ❑ Every day
- ❑ Some days
- ❑ Not at all ➡ **(Go to Question 45)**

43. On average, how many cigarettes do you now smoke **a day**?

↳ ______ *cigarettes per day*

44. In the **last 12 months**, have you been advised by a doctor or health professional to quit smoking?

- ❑ Yes
- ❑ No
- ❑ I haven't seen a doctor in the last 12 months

About You

45. Are you male or female?

❑ Male ❑ Female

46. What is the YEAR of your birth? 19_____

47. Are you currently employed or self employed?

- ❑ Yes, employed by someone else
- ❑ Yes, self-employed
- ❑ Not currently employed
- ❑ Retired

48. About how many hours per week, on average, do you work at your current job(s)?

- ❑ I don't currently work
- ❑ Less than 20 hours per week
- ❑ 20-29 hours per week
- ❑ 30 or more hours per week

49. What was your gross household income (before taxes and deductions are taken out) for last year (2008)? *Please include any cash assistance or unemployment you may have received. Your best estimate is fine.*

- ❑ $0
- ❑ $1 to $2,500
- ❑ $2,501 to $5,000
- ❑ $5,001 to $7,500
- ❑ $7,501 to $10,000
- ❑ $10,001 to $12,500
- ❑ $12,501 to $15,000
- ❑ $15,001 to $17,500
- ❑ $17,501 to $20,000
- ❑ $20,001 to $22,500
- ❑ $22,501 to $25,000
- ❑ $25,001 to $27,500
- ❑ $27,501 to $30,000
- ❑ $30,001 to $32,500
- ❑ $32,501 to $35,000
- ❑ $35,001 to $37,500
- ❑ $37,501 to $40,000
- ❑ $40,001 to $42,500
- ❑ $42,501 to $45,000
- ❑ $45,001 to $47,500
- ❑ $47,501 to $50,000
- ❑ $50,001 or more

50. Would you describe yourself as Spanish, Hispanic, or Latino?

- ❑ Yes
- ❑ No

51. How would you describe your race? *Mark all that apply.*

- ❑ White
- ❑ Black or African-American
- ❑ American Indian or Alaska Native
- ❑ Asian
- ❑ Native Hawaiian or Pacific Islander
- ❑ Other: ________________________________

52. What is the **highest** level of education you have completed? *(Mark only one)*

- ❑ Less than high school
- ❑ High school diploma or GED
- ❑ Vocational training or 2-year degree
- ❑ A 4-year college degree or more

53. What is your current living arrangement? *Mark all that apply.*

- ❑ Live alone
- ❑ Live with partner or spouse
- ❑ Live with parents
- ❑ Live with other relatives (including children)
- ❑ Live with friends or roommates
- ❑ Other: ________________________________

54. How many family members, including yourself, counting adults and children, are living in your home? *(For example, if you live alone, you should write "1".)*

↳ Size of Household:________________

55. Of the family members living in your house, how many are under age 19?

↳ Number under age 19:_____________

Continue

Contact Information

Thank you for participating! This study will continue for three years, and we would like to contact you again. It is important for us to have a way to reach you if you move during that time.

Please tell us two people who **do not** live with you and would know how to reach you if you moved. Good contacts are people like your mother, a sister or brother, or a good friend.

This information will NOT be shared, and will be used by us ONLY if we are unable to find you, and ONLY for the purpose of continuing this study.

Name:________________________________ Relationship: ________________________

Address: ____
Street Apartment #

__________________________________ Email address________________________:
City State Zip

Home Phone: ___________________________ Cell or Message Number: _______________

Name:________________________________ Relationship: ________________________

Address: ____
Street Apartment #

__________________________________ Email address________________________:
City State Zip

Home Phone: ___________________________ Cell or Message Number: _______________

Name:________________________________ Relationship: ________________________

Address: ____
Street Apartment #

__________________________________ Email address________________________:
City State Zip

Home Phone: ___________________________ Cell or Message Number: _______________

When you have finished your survey, please place it in the postage-paid envelope, and drop it in the mail. Thank you for your time!

Table A1: Differences in lottery list characteristics across different samples

	Full Sample	Credit report subsample	Survey subsample	Survey respondents
	(1)	(2)	(3)	(4)
Year of birth	1968.0	1967.2	1968.0	1966.2
	(12.255)	(12.07)	(12.119)	(12.149)
Female	0.56	0.57	0.55	0.59
English as preferred language	0.92	0.93	0.91	0.92
Signed up self	0.92	0.91	0.88	0.88
Signed up first day of lottery	0.09	0.10	0.09	0.10
Gave phone number	0.86	0.87	0.87	0.91
Address a PO Box	0.12	0.12	0.12	0.13
In MSA	0.77	0.78	0.77	0.75
Zip code median household income	39,265	39,535	39,326	39,225
	(8463.542)	(8518.825)	(8529.575)	(8442.09)
N	74,922	49,980	58,405	23,741

Notes: The columns show the means (and standard deviations in parentheses for the non-binary variables) of the lottery list variables given in the first column for the various samples indicated in the different columns.

Table A2: Details of timing of lottery

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lottery draw	1	2	3	4	5	6	7	8
Number of people selected	3,176	3,173	3,182	3,005	2,936	5,899	5,637	2826
Advance notification	not sent	not sent	4/16	5/9	6/11	7/14	8/12	9/11
Applications mailed	3/10	4/7	5/7	6/6	7/7	8/7	9/5	10/7
Retroactive insurance	3/11	4/8	5/8	6/9	7/8	8/8	9/8	10/8
Applications due	5/31	5/23	6/23	7/24	8/22	9/22	10/23	11/24
Average application decision	4/28	5/28	7/3	8/1	8/31	10/6	11/8	11/28
Months from notification thru 9/30/09	13	14	15	16	17	18	18	19

Notes: All dates are in 2008. Notification date is defined by the "Advance notification date" except when advance notification was not sent (i.e draws 1 and 2), in which case it is defined by the application mailed date. Across all lottery draws, average time from notification through September 30 2009 is 16 months (standard deviation = 2 months) and average time from application approval through September 30, 2009 is 14 months (standard deviation = 3 months). "Number of people selected" is based on our analysis sample of 74,922 individuals.

Table A3: Distributions of hospital utilization, conditional on any admission

	Mean	SD	25th %tile	Median	75th %tile	95th %tile	99th %tile
Number of separate hospital stays	1.59	1.38	1	1	2	4	7
Total length of stay (days)	7.44	12.79	2	3	8	27	66
Total number of procedures	2.32	3.52	0	1	3	9	16
Total list charges	39,017	67,233	11,143	19,983	40,216	132,295	292,148

Note: Table details the distribution of several measures of hospital utilization. This is limited to our control sample for the period from notification date to August 31, 2009. It is limited to the 7% of our controls with any hospital admission in that time period.

Table A4: Top 10 clinical conditions among the controls

	N	Frequency (%)
	(1)	(2)
Mood disorders	709	10.13
Skin and subcutaneous tissue infections	278	3.97
Diabetes mellitus with complications	228	3.26
Alcohol-related disorders	201	2.87
Schizophrenia and other psychotic disorders	195	2.79
Spondylosis; intervertebral disc disorders; other back problems	184	2.63
Pneumonia (except that caused by tuberculosis or sexually transmitted disease)	176	2.52
Pancreatic disorders (not diabetes)	149	2.13
Substance-related disorders	134	1.92
Biliary tract disease	127	1.82

Notes: Summary of non-childbirth admissions occurring between January 1, 2008 and August 31, 2009 for our control sample.

Table A5: Selected conditions in our control sample

	N (1)	Percent of category (2)	Percent of all admissions (3)
Mental	977	100	13.96
Mood disorders	709	72.57	10.13
Schizophrenia and other psychotic disorders	195	19.96	2.79
Adjustment disorders	26	2.66	0.37
Anxiety disorders	22	2.25	0.31
Miscellaneous disorders	9	0.92	0.13
Delirium, dementia, and amnestic and other cognitive disorders	5	0.51	0.07
Personality disorders	5	0.51	0.07
Suicide and intentional self-inflicted injury	4	0.41	0.06
Impulse control disorders, NEC	2	0.2	0.03
Substance	278	100	3.97
Alcohol-related disorders	195	70.14	2.79
Substance-related disorders	83	29.86	1.19
Heart	357	100	5.1
Coronary atherosclerosis and other heart disease	96	26.89	1.37
Acute myocardial infarction	93	26.05	1.33
Congestive heart failure; nonhypertensive	91	25.49	1.3
Cardiac dysrhythmias	67	18.77	0.96
Conduction disorders	6	1.68	0.09
Cardiac arrest and ventricular fibrillation	4	1.12	0.06
Diabetes	231	100	3.3
Diabetes mellitus with complications	228	98.7	3.26
Diabetes mellitus without complication	3	1.3	0.04
Skin	278	100	3.97
Skin and subcutaneous tissue infections	278	100	3.97
Back Problems	184	100	2.63
Spondylosis; intervertebral disc disorders; other back problems	184	100	2.63
Pneumonia	176	100	2.52
Pneumonia (except that caused by TB or STDs)	176	100	2.52

Notes: Summary of non-childbirth admissions occurring between January 1, 2008 and August 31, 2009 for our control sample.

Table A6: Comparison of hospital admissions (different samples)

	All		Adults aged 19-64		Uninsured adults aged 19-64		Control sample	
	N	%	N	%	N	%	N	%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
By gender:								
Male	217538	47	107485	48	17086	56	3300	47
Female	245323	53	116975	52	13372	44	3697	53
By type of admission:								
Non-ED	214499	46	108909	49	8612	28	2420	35
All ED	248362	54	115551	51	21846	72	4577	65
By length of stay:								
1-2 days	194270	42	103540	46	14852	49	2945	42
3-4 days	131149	28	59510	27	7872	26	1861	27
5 or more days	137442	30	61410	27	7734	25	2191	31
By number of procedures:								
None	173649	38	77101	34	12980	43	3268	47
One	109550	24	55507	25	7160	24	1471	21
Two or more	179662	39	91852	41	10318	34	2258	32
By list charges:								
Less than 5,000	34043	7	16083	7	2111	7	584	8
5,000 – 9,999	88717	19	42014	19	7064	23	1612	23
10,000 – 24,999	189809	41	94445	42	13795	45	2972	42
25,000 or more	150292	32	71918	32	7488	25	1829	26
By condition:								
Mental disorders	20960	5	16417	7	2051	7	977	14
Alcohol/substance	5451	1	4759	2	1122	4	278	4
Heart disease	47377	10	15408	7	2134	7	357	5
Diabetes	7213	2	4664	2	1069	4	231	3
Skin infection	8354	2	5250	2	1422	5	278	4
Back Problems	15871	3	10011	4	379	1	184	3
Pneumonia	17563	4	5186	2	848	3	176	3

Notes: All analyses are based on the hospital discharge data from January 1 2008 through September 30, 2009 but exclude childbirth and new births. In total, there were 84935 hospital stays for childbirth and 78162 new births. The childbirth stays included 80169 stays for adults ages 19-64, 1868 stays for uninsured adults aged 16-64 and 7036 stays for our control sample. Columns 7 and 8 are for our control sample; the other columns include a larger set of individuals in Oregon.

Table A7: Distributions of credit data variables

Panel A: Number of events (percent)

	0	1	2	3	4	5	6	7	8+
# of bankruptcies	98.93	1.06	0.02	0	0	0	0	0	0
# of liens	98.51	1.24	0.2	0.03	0.01	0	0	0	0
# of judgments	95.65	3.72	0.5	0.1	0.02	0.01	0	0	0
# of collections	67.53	10.67	6.85	4.66	3.12	2.00	1.39	1.00	2.78
# of medical collections	82.03	8.15	3.78	2.02	1.18	0.69	0.55	0.37	1.23
# of non medical collections	74.37	11.67	6.24	3.4	1.89	0.97	0.59	0.33	0.54

Notes: Table shows percent of each variable in each column. All variables measured in the September 2009 archive since notification date.

Panel B: Distribution of collection amounts

	% positive	Mean	SD	10th pctile	25th pctile	Median	75th pctile	90th pctile	95th pctile
All collections	63.45	7216.244	14392.8	309	1026	3114	7836	16891	26678
Medical collections	48.32	3992.573	9287.991	185	455	1328	3732	9461	16381
Non medical collections	53.97	4909.626	12468.76	199	609	1788	4710	11159	18237

Notes: All variables are measured in the September 2009 archive. Note that individuals with positive collection balances may have incurred them prior to the notification date.

Panel C: Distribution of delinquencies

	% with any open trade line	% with none	% with only minor	% with major
Full sample	74.15	50.38	19.56	39.11
Prior credit subsample	94.81	54.72	22.08	33.85

Notes: Columns 1-3 are conditional on having any open trade line. Minor delinquencies are those outstanding less then 120 days; major are those outstanding 120 days or more. Prior credit subsample defined as having a revolving credit account in February 2008. All data in table are measured from notification date through September 2009.

Panel D: Distribution of credit scores

	% with score	Mean	SD	10th pctile	25th pctile	Median	75th pctile	90th pctile	95th pctile
Full sample	81.29	653.4578	112.5803	523	565	631	722	832	871
Prior credit subsample	98.37	685.8004	115.1135	543	594	671	771	855	887

Notes: All variables are measured in the September 2009 archive. Prior credit subsample defined as having revolving credit

Panel E: Distribution of credit grades

	% with score	A	B	C	D	E
Full sample	81.29	39.26	30.94	16.09	11.25	2.46
Prior credit subsample	98.37	31.94	27.08	21.16	16.21	3.61

Notes: All variables are measured in the September 2009 archive. Prior credit subsample defined as having revolving credit account in February 2008.

Panel F: Distribution of credit limits

	% positive	Mean	SD	10th pctile	25th pctile	Median	75th pctile	90th pctile	95th pctile
Full sample	50.46	14487.02	31928.68	334	1000	4649	15000	35500	58700
Prior credit subsample	85.07	15261.72	32733.77	400	1200	5100	16016	37100	61100

Notes: All variables are measured in the September 2009 archive. Prior credit subsample defined as having revolving credit

Table A8: Credit bureau summary statistics for lottery population compared to all of Oregon

	Lottery list controls N = 29,900	All of Oregon N=4,464,555
Adverse financial events		
Any bankruptcy in last 12 months	0.01	0.01
Any lien in last 12 months	0.02	0.01
Any judgment in last 12 months	0.05	0.02
Any collection in last 12 months	0.47	0.13
Total current collection amount	4763.12	975.41
Any medical collection in last 12 months	0.25	0.05
Any non medical collection in last 12 months	0.36	0.1
Currently have any open credit (trade line)	0.67	0.59
Any delinquency in last 12 months	0.34	0.14
Any major delinquency in last 12 month	0.26	0.08
Measures of access to credit		
Currently have a credit score?	0.8	0.63
Current credit score (conditional on any)	651.26	765.12
Currently have a thick file	0.37	0.41
Currently have an open revolving credit account	0.43	0.49
Mean total current credit limit	9866.39	23487.193
Median total current credit limit	700	1096
Mean total current credit limit (conditional on positive)	16139.015	41112.986
Median total current credit limit (conditional on positive)	4966	18600

Notes: All data are from September 2009. Time period (look back) does not match our analysis variables which are defined relative to notification date. In addition some current variables will not match exactly (e.g. thick file, whether have an open revolving credit account) since they are defined to be analogous to how they can be defined for all of Oregon and this differs slightly from our analysis variable definitions. Credit limit variables also do not match our analysis variables in that they refer to credit limits on any revolving credit account (open or closed) verified in last 13 months. while our analysis looks just at open revolving credit. Thick file is defined as two or more open trade lines.

Table A9: Details of survey timing (2009)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Survey wave	1	2	3	4	5	6	7	All
N	6,567	6,526	6,522	8,145	8,207	12,351	10,087	58,405
% treatment	65	66	66	48	49	42	36	51
% treatment in hh size 1	62	63	62	44	46	44	36	48
% treatment in hh size 2	72	72	74	54	53	39	31	57
% treatment in hh size 3	95	81	85	.	.	.	.	86
Earliest survey mailing	6/25	7/9	7/20	8/3	8/6	8/11	8/14	6/25
Average response time (days)	55	50	49	50	52	56	56	53
Average response date	8/19	8/28	9/7	9/22	9/27	10/6	10/9	9/20
Average months from notification thru mailing	13.7	13.8	13.9	13.7	13.5	13.3	13.1	13.5
Average months from notification thru avg response	15.4	15.4	15.5	15.4	15.2	15.2	15	15.3
Average months from approval thru avg response	15	14.3	13.5	12.8	12.1	11.8	10.7	13.1

All dates are in 2009. "Notification" is defined as in Table A2.

Table A10: Summary of analytic variables from the mail survey data

	Time frame of question	Survey questions	Non-missing data (N)	Non-missing data (%)
Health Care Use: Extensive				
Any prescription drugs?	Current	Q12	18332	77
Any outpatient visit?	Last 6 months	Q15	23528	99
Any ER visit?	Last 6 months	Q16	23550	99
Any inpatient hospital visit?	Last 6 months	Q18	23609	99
Health Care Use: Total				
Number of prescription drugs	Current	Q12	18321	77
Number outpatient visits	Last 6 months	Q15	23477	99
Number ER visits	Last 6 months	Q16	23517	99
Number inpatient hospital visits	Last 6 months	Q18	23609	99
Financial Strain of health care costs				
Owe any out of pocket medical expenses?	Last 6 months	Q20	23462	99
Currently owe money?	Current	Q22	23487	99
Borrowed money for medical bills?	Last 6 months	Q23	23446	99
Refused treatment because of medical debt?	Last 6 months	Q24	22605	95
Health status				
Self-reported health % fair or poor	Current	Q26	23397	98
% health gotten worse	Last 6 months	Q27	23443	99
Number of days impaired by physical or mental health	Last 30 days	Q30	21915	92
Number of days of physical health not good	Last 30 days	Q28	21415	90
Number of days mental health not good	Last 30 days	Q29	21632	91
Screened Positive for Depression?	Last 2 weeks	Q34	23406	98
Access				
Have usual place of care?	Current	Q3	23387	98
Have a personal doctor?	Current	Q5	23537	99
Got all needed medical care?	Last 6 months	Q6, Q7	22940	96
Got all needed drugs?	Last 6 months	Q9, Q10	22860	96
Used ER for non ER care?	Last 6 months	Q16, Q17	23566	99
Quality				
Overall quality of care (condl on receipt)	Last 6 months	Q19	16336	69
Preventive care				
Blood cholesterol check	Last 12 months	Q37	23426	99
Blood test for high blood sugar	Last 12 months	Q38	23410	98
Mammogram (women only) *	Last 12 months	Q39	8108	99
Pap test (women only) *	Last 12 months	Q40	8084	99
Health behavior				
Smoke		Q41, Q42	23141	97
Happiness				
Overall happiness (not too happy vs. very or pretty happy)	Current	Q25	23450	99

*The count of non-missing observations is restricted when the question only applies to a particular subgroup (e.g., we would only expect responses for mammogram, pap test questions from women).

Table A11: Distribution of raw survey answers (limited to control sample)

Panel A: Health Care Use

	Percent reporting any	Mean	SD	Median	75th %tile	95th %tile	Cutpoint for truncation	% of data truncated
Prescription drugs (Q12)	64.3	3.6	2.8	3	5	10	24	0
Outpatient visits (Q15)	57.5	3.2	3.1	2	3	9	30	0.4
ER visits (Q16)	25.5	1.8	1.3	1	2	4	10	0.3
Inpatient hospital visit (Q18)	7	1	1	1	2	3	4	0.3

Note: In Panels A and B, the mean, standard deviation, median, 75th and 95th percentile values reflect non-zero observations only. Percent reporting any use/expenses, cutpoint for censoring and percent of data censored reflect all valid non-missing data, including observations with zero values.

Panel B: Financial Strain

	Percent reporting any	Mean	SD	Median	75th %tile	95th %tile	Cutpoint for truncation	% of data truncated
Out of pocket expenses								
Doctor visits (Q21a)	39.2	375.3	2163.1	150	300	1000	n/a	n/a
ER or hospital (Q21b)	8.3	1437.3	4169.8	400	1200	5000	n/a	n/a
Prescription drugs (Q21c)	42.7	212.9	615.9	90	200	700	n/a	n/a
Other medical (Q21d)	13.4	669	6013.5	150	350	1610	n/a	n/a
Total owed (Q22)	54.9	4466.9	10510.9	1000	3500	20000	100000	0.4
Total out of pocket or owed	66	3,452	8,695	745	2,552	15,300	100,000	0.4

Note: In Panels A and B, the mean, standard deviation, median, 75th and 95th percentile values reflect non-zero observations only. Percent reporting any use/expenses, cutpoint for censoring and percent of data censored reflect all valid non-missing data, including observations with zero values.

Panel C: Health Status

	N	%
General health (Q26)		
Excellent	541	4.6
Very Good	1838	15.6
Good	4005	34
Fair	3727	31.7
Poor	1652	14
Health changed (Q27)		
Better	1317	11.2
Same	7037	59.7
Worse	3434	29.1
Depressed? (Q34)		
Not at all	4402	37.4
Several days	4111	34.9
More than half the days	1434	12.2
Nearly every day	1837	15.6

Panel D: Mechanisms		
	N	%
Quality		
Overall quality of care (Q19)		
Excellent	1315	11.2
Very Good	1959	16.7
Good	2294	19.5
Fair	1618	13.8
Poor	738	6.3
Preventive care		
Blood cholesterol check (Q37)		
Yes, last year	3700	31.4
Yes, more than a year ago	3652	31
No	4442	37.7
Blood test for high blood sugar (Q38)		
Yes, last year	3554	30.2
Yes, more than a year ago	3508	29.8
No	4721	40.1
Mammogram (Q39)		
Yes, last year	1427	20
Yes, more than a year ago	2490	35
No	3203	45
Pap test (Q40)		
Yes, last year	2820	39.7
Yes, more than a year ago	3973	56
No	302	4.3
Health behavior		
Smoke (Q41,Q42)		
Every day	3793	32.6
Some days	1071	9.2
Not at all	6754	58.1

Table A12: Comparison of actual and simulated lottery selection

	Mean in those selected in lottery (1)	Mean of mean in simulations (2)	SD of mean in simulations (3)	# of SD difference (4)
Year of birth	1967	1967	0.079	0.748
Female	0.525	0.528	0.002	1.310
English as preferred language	0.931	0.930	0.001	0.591
Signed up self	0.843	0.843	0.002	0.158
Signed up first day of lottery	0.090	0.089	0.002	0.590
Gave phone number	0.847	0.848	0.002	0.294
Address a PO Box	0.131	0.130	0.002	0.338
Zip code median household income	38,885	38,840	53	0.849
In MSA	0.747	0.746	0.002	0.438

Notes: Column 1 reports the average lottery list characteristics of those selected by the lottery. Columns 2 and 3 report the results from our simulated lottery drawings (attempting to mimic the state's procedure). Column 4 shows the difference between in actual mean and the mean of the mean in the simulations in terms of the standard deviations of the means in the simulations. The analysis is done on the full lottery list that the state drew from.

Table A13: Lottery list characteristics and balance of treatment and controls

	Control Mean (std dev)	Difference between treatment and control		
		Full sample	Credit report subsample	Survey respondents subsample
	(1)	(2)	(3)	(4)
Panel A: Lottery list characteristics				
Year of birth	1968.0	0.162	0.136	-0.066
	(12.255)	(0.1)	(0.119)	(0.191)
Female	0.557	-0.007	-0.003	-0.004
	(0.497)	(0.003)	(0.004)	(0.007)
English as preferred language	0.922	0.002	0.004	-0.00033145
	(0.268)	(0.003)	(0.003)	(0.005)
Signed up self	0.918	0.00030426	0.001	-0.002
	(0.274)	(0.00027774)	(0.001)	(0.003)
Signed up first day of lottery	0.093	0.001	0.001	0.006
	(0.29)	(0.002)	(0.003)	(0.005)
Gave phone number	0.862	-0.003	0.00008793	0.006
	(0.345)	(0.003)	(0.003)	(0.004)
Address a PO Box	0.117	0.00043895	0.002	-0.002
	(0.321)	(0.003)	(0.003)	(0.005)
In MSA	0.773	-0.002	-0.002	0.001
	(0.419)	(0.004)	(0.004)	(0.007)
Zip code median household income	39,265.4	44.891	12.998	22.031
	(8463.542)	(72.887)	(89.653)	(135.815)
Panel B: Pre-randomization outcome measures				
Any hospital admission	0.035	-0.001		-0.002
	(0.184)	(0.001)		(0.002)
Any hospital admission not through ED	0.014	-0.00048952		
	(0.117)	(0.001)		
Any hospital admission through ED	0.025	-0.001		
	(0.156)	(0.001)		
Number of hospital days (all)	0.225	-0.005		
	(2.095)	(0.017)		
Number of hospital procedures (all)	0.066	-0.002		
	(0.636)	(0.005)		
List charges (all)	1075.539	-19.722		
	(10915.704)	(88.912)		
Number of hospital days (non ED admissions)	0.083	0.006		
	(1.238)	(0.011)		
Number of hospital proceudres (non ED admissions)	0.029	0.002		
	(0.371)	(0.003)		
List charges (non ED admissions)	426.628	33.968		
	(8006.786)	(68.44)		
Number of hospital days (ED admissions)	0.142	-0.011		
	(1.516)	(0.011)		
Number of hospital proceudres (ED admissions)	0.037	-0.004		
	(0.481)	(0.003)		
List charges (ED admissions)	648.91	-53.69		
	(6894.16)	(53.114)		
Any bankruptcy	0.011		0.00021864	
	(0.105)		(0.001)	
Any lien	0.02		0.001	
	(0.14)		(0.001)	
Any judgment	0.067		-0.006	
	(0.251)		(0.002)	
Any collection	0.487		-0.001	
	(0.5)		(0.004)	
Any delinquency	0.399		-0.001	
	(0.49)		(0.004)	
Any medical collection	0.255		-0.005	-0.012
	(0.436)		(0.004)	(0.007)
Any non-medical collection	0.388		0.002	0.008
	(0.487)		(0.004)	(0.007)
Currently have a credit score?	0.822		0.001	
	(0.383)		(0.002)	
Currently have a thick file?	0.426		-0.00007941	
	(0.495)		(0.003)	
Current credit limit (revolving credit)	8930.072		27.275	
	(28837.395)		(132.186)	
Current credit score	648.435		0.85	
	(113.385)		(0.638)	
Number of hospital visits in pre period	0.331			-0.002
	(0.471)			(0.004)
N		74,922	49,980	23,741

Columns 2 through 4 report the coefficient on "LOTTERY" from estimating equation (1) on the dependent variable shown in the left hand column. In Panel A, the dependent variables are all taken from the lottery list and are available for the full sample; the control means for the full sample are shown in column 1. In columns 2 and 3 the regressions include household size dummies. In column 4 the regressions include household size and survey wave dummies and their interactions; the regressions in column 4 use survey weights. In Panel B, the dependent variables are taken either from the hospital discharge data or the credit report data, depending on the variable. Variables from the hospital discharge data are measured from January 1, 2008 through notification date (on average, 5 months). Variables from the credit report data are measured in the February 2008 data archive with a look back period for each lottery draw as in the September 2009 data. The control means in column 1 are based on the full sample for the hospital discharge data variables and the credit report subsample for the credit report variables. The regressions in column 2 include household size and lottery draw. The regressions in column 3 include household size, lottery draw, and the prior measure of the outcome from the February 2007 archive (measured with the same look back period). The regressions in column 4 include dummies for household size, survey wave and their interaction, as well as lottery draw dummies (since the outcomes in Panel B are measured relative to notification dates) and for the credit outcomes we also control for the means in February 2007; the regressions in column 4 use survey weights. All standard errors are clustered on the household. See Appendix 1 for more details on variable definitions.

Table A14: Sensitivity of standardized treatment effects to covariates

	Reduced form			2SLS		
	Baseline	No pre-randomization outcome or lottery draw controls	Add lottery list variables	Baseline	No pre-randomization outcome or lottery draw controls	Add lottery list variables
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Administrative data						
Hospital utilization	0.012	0.012	0.013	0.047	0.048	0.050
	(0.007)	(0.007)	(0.007)	(0.026)	(0.027)	(0.026)
	[0.073]	[0.071]	[0.059]	[0.073]	[0.071]	[0.059]
Financial strain	0.002	0.001	0.002	0.009	0.005	0.006
	(0.005)	(0.005)	(0.005)	(0.019)	(0.021)	(0.019)
	[0.653]	[0.817]	[0.739]	[0.653]	[0.817]	[0.739]
Panel B: Survey data						
Total health care utilization	0.040	n/a	0.038	0.137	n/a	0.133
	(0.011)	n/a	(0.011)	(0.038)	n/a	(0.037)
	[0.00032061]	n/a	[0.00037779]	[0.00032061]	n/a	[0.00037779]
Financial strain	-0.089	n/a	-0.088	-0.305	n/a	-0.303
	(0.01)	n/a	(0.01)	(0.035)	n/a	(0.035)
	[4.101E-18]	n/a	[2.649E-18]	[4.101E-18]	n/a	[2.649E-18]
Self reported health	0.059	n/a	0.060	0.203	n/a	0.207
	(0.011)	n/a	(0.011)	(0.039)	n/a	(0.038)
	[0.0000001521]	n/a	[0.000000044]	).0000001521]	n/a	[0.000000044]

(Standard errors in parentheses)
[Per comparison p-values in square brackets]

Note: Table reports standardized treatment effects based on estimating equation (1) (for the reduced form) or equation (3) by IV, and then calculating standardized treamtnet effects based on equation (2). For each standardized trematnet effect we report the estimate, the standard error (in parentheses), and the per comparison p-value [in square brackets]. Columns (1) and (4) show the baseline specification for the reduced form and 2SLS, respectively. The baseline results for Panel A, "hospital utilization" can be found in Table 4b, Panel A (all hospital admissions); the baseline results for Panel A "financial strain" can be found in Table 7, Panel A. In Panel B, the baseline results for "total health care utilization" can be found in Table 5, right hand panel, for "financial strain" in Table 8, and for self-reported health in Table 9, Panel B. Columns 2 and 5 show the sensitivity of the results in the administrative data to removing lottery draw dummies and the pre-randomization measure of the outcome from the baseline specification. Columns 3 and 6 show the results from adding the lottery list covariates to the baseline specification in both the survey and the administrative data. All regressions include household size dummies and (in the survey data) for survey wave and the interaction of survey wave with household size; the analysis of survey data uses the survey weights. All standard errors are clustered on the household.

Table A15: Total Hospital Utilization, Poisson estimates

(Adminstrative data)	Control Mean	Poisson Reduced form
	(1)	(2)
Panel A: All hospital admissions		
Days	0.498	0.012
	(3.795)	(0.057)
		[0.837]
List Charges	2612.522	0.082
	(19941.992)	(0.056)
		[0.145]
Procedures	0.155	0.121
	(1.08)	(0.053)
		[0.021]
Average effect		0.072
		(0.05)
		[0.156]
Panel B: Admissions through ER		
Days	0.299	0.041
	(2.326)	(0.061)
		[0.502]
List Charges	1502.493	0.08
	(12749.194)	(0.065)
		[0.219]
Procedures	0.081	0.108
	(0.694)	(0.066)
		[0.101]
Average effect		0.076
		(0.058)
		[0.191]
Panel C: Admissions not through ER		
Days	0.199	-0.003
	(2.38)	(0.086)
		[0.968]
List Charges	1110.029	0.078
	(12422.468)	(0.08)
		[0.327]
Procedures	0.075	0.103
	(0.708)	(0.07)
		[0.14]
Average effect		0.059
		(0.072)

(Standard errors in parentheses)
[Per comparison p-values in square brackets]

Notes: Table investigates non-childbirth-related hospitalizations during the time period from notification date to August 31, 2009. All outcomes are measured unconditionally (i.e. are not conditional on admission). All estimates are done by quasi-maximum likelihood Poisson. Column 2 reports the coefficient and standard error on LOTTERY from estimating equation (1) by quasi-maximum likelihood Poisson (the OLS analog was shown in Table 4b, column 2). The "average effect" reports the average estimated coefficient on LOTTERY across the three outcomes shown in the panel. All regressions include household size fixed effect, lottery draw fixed effects and the analogous outcome measure for the time period from January 1, 2008 through the notification date. All standard errors are clustered on the household. Sample consists of entire sample universe (N = 74922).

Table A16: Hospital Utilization for Selected Conditions (Administrative Data)

	Share of admissions	Fraction through ER	Any admission		Total Utilization
			Control Mean	Reduced form	Reduced Form
	(1)	(2)	(3)	(4)	(5)
Heart Disease	0.044	0.676	0.004	0.002	0.027
			(0.061)	(0.001)	(0.008)
				).00032696]	[0.001]
Diabetes	0.028	0.891	0.002	0.001	0.007
			(0.045)	).00035306)	(0.006)
				[0.139]	[0.259]
Skin inefection	0.04	0.836	0.004	-2.42E-04	0.006
			(0.062)	).00045629)	(0.008)
				[0.596]	[0.458]
Mental Disorders	0.133	0.623	0.009	2.766E-05	0.002
			(0.092)	(0.001)	(0.007)
				[0.967]	[0.785]
Alcohol or substance use	0.042	0.642	0.003	-0.000267	-0.004
			(0.057)	).00041628)	(0.006)
				[0.521]	[0.444]
Back problems	0.028	0.164	0.003	-8.01E-05	-0.008
			(0.052)	).00039113)	(0.006)
				[0.838]	[0.174]
Pneumonia	0.025	0.862	0.003	8.585E-05	-0.001
			(0.051)	).00038324)	(0.007)
				[0.823]	[0.939]

(Standard errors in parentheses)
[Per comparison p-values in square brackets]

Notes: Table investigates non-childbirth-related hospital admissions and utilization for various diagnoses during the time period from notification date to August 31, 2009. Table reports, for the control group, the percent of all admissions which are of the specified diagnosis (Column 1) and what fraction of admissions of that diagnosis are through the emergency department (Column 2). Table reports the mean of "any admission" for each diagnosis in the control group (column 3). In column 4 we report the coefficient and standard error on LOTTERY from estimating equation (1) by OLS on the dependent variable "any admission of that type." In column 5 we report the standardized treatment effect (calculated based on equation 2) for OLS estimates of the impact of LOTTERY from estimating equation (1) for three different outcomes (for that diagnosis): number of days, number of procedures, and list charges. All regressions include household size fixed effect, lottery draw fixed effects and the analogous outcome measure for the time period from January 1, 2008 to notification date. All standard errors are clustered on the household. Sample consists of entire sample universe (N = 74922).

Table A17: Impact of Health Insurance on Quality of Care, as Measured in Hospital Data

	Control Mean (1)	Reduced Form (2)	2SLS (3)	p-values (4)
Panel A: Outpatient Quality of Care				
Ambulatory-care-sensitive condition	0.009	0.001	0.002	[0.469]
	(0.095)	(0.001)	(0.003)	
Panel B: Inpatient quality of care (conditional on any admission)				
No patient safety event	0.981	0.003	0.01	[0.431]
	(0.136)	(0.004)	(0.013)	{0.721}
Not re-admitted in 30 days	0.875	-0.004	-0.013	[0.722]
	(0.331)	(0.01)	(0.036)	{0.721}
Average hospital quality	0.156	0.003	0.01	[0.706]
	(0.241)	(0.007)	(0.025)	{0.721}
Standardized Treatment effect		0.007	0.026	[0.675]
		(0.018)	(0.061)	

(Standard errors in parentheses)
[Per comparison p-values in square brackets]
{Family wise p-values in curly brackets}

Note: Table investigates non-childbirth-related hospital admissions during the time period from notification date to August 31, 2009. Panel A considers outpatient quality of care; Panel B considers multiple measures of inpatient quality of care. Table reports the mean of each outcome in the control group (column 1); for patient safety events , readmissions and average hospital quality control means are reported conditional on admission. Table reports the coefficient on LOTTERY from estimating equation (1) by OLS (column 2), and the coefficient on INSURANCE from estimating equation (3) by IV (column 3). For each outcome we report the estimate, standard error, per-comparison p-value and the family-wise p-value across the individual outcomes shown in the table. Standardized treatment effects report results based on equation (2). The regressions for patient safety events, readmissions and average hospital quality are done conditional on having any hospital admission. All regressions include household size fixed effects and lottery draw fixed effects. The regressions for ambulatory-care-sensitive conditions include analogous outcome measure for the time period from January 1, 2008 to notification date. All standard errors are clustered on the household. Sample consists of entire sample universe (N = 74922) for ambulatory-care sensitive conditions, and the universe of individuals with any admission since the notification date for patient safety event and average hospital quality (N=5033). For re-admission in 30 days, the sample is further limited to those admitted between the notification date and June 30, 2009 (N=4485). This additional restriction is done to allow for the first hospital stay, plus up to 30 days before another admission, plus the second hospital stay to all be completed by the end of our data in September 30, 2009.

Table A18: Impact of Health Insurance on Hospital Type

	Control Mean	Reduced Form (Logit)	p-value for test of equality
	(1)	(2)	(3)
Panel A: All Admissions			
Public Hospital	0.01	0.123	0.604
	(0.1)	(0.076)	
		[0.104]	
Private Hospital	0.059	0.082	
	(0.236)	(0.033)	
		[0.013]	
Panel B: With Choice			
Public Hospital	0.019	0.126	0.576
	(0.137)	(0.099)	
		[0.202]	
Private Hospital	0.056	0.064	
	(0.23)	(0.061)	
		[0.299]	
Panel C: Non-ED With Choice			
Public Hospital	0.007	0.166	0.744
	(0.084)	(0.155)	
		[0.283]	
Private Hospital	0.021	0.224	
	(0.142)	(0.093)	
		[0.016]	

(Standard errors in parentheses)
[Per comparison p-values in square brackets]

Note: Table investigates non-childbirth-related hospital admissions during the time period from notification date to August 31, 2009. The results are presented separately for all admissions, all admissions limiting to the subsample of individuals with hospital "choice" and non-emergency-department admissions limiting to the subsample of individuals with hospital "choice" (see text for definition). Table reports the mean probability of admission to each hospital type in the control group (column 1) and the coefficient on LOTTERY from estimating equation (1) by Logit for the dependent variable measuring any admission of the type specified (column 2). Column 3 reports the p-value from a t-test of the equality of the coefficients for public admissions and private admissions reported in column 3. For all Logit coefficients we report the Log odds. All regressions include household size fixed effect, lottery draw fixed effects and the analogous outcome measure for the time period from January 1, 2008 to notification date. All standard errors are clustered on the household. Note that our analysis is at the level of the individual yet "type of hospital care" is at the level of an individual admission (and a given individual may have multiple admissions). Sample size is 74,922 for Panel A, and 23,861 for panels B and C.

Table A19: Access to Credit (admin)

	Control Mean	Reduced Form	2SLS	p-values	Control Mean	Reduced Form	2SLS	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Current Access to Credit				Maximum Access to Credit			
Panel A: Full Sample								
Currently have a credit score?	0.805	0.003	0.01	[0.273]	0.827	0.002	0.009	[0.304]
	(0.396)	(0.002)	(0.009)	{0.61}	(0.378)	(0.002)	(0.009)	{0.667}
Currently have a thick file?	0.386	0.001	0.005	[0.66]	0.423	0.00044507	0.002	[0.891]
	(0.487)	(0.003)	(0.012)	{0.659}	(0.494)	(0.003)	(0.013)	{0.953}
Total current credit limit on all open revolving credit	7109.001	-86.582	-340.161	[0.426]	11415.977	7.971	31.32	[0.952]
	(24741.895)	(108.702)	(426.98)	{0.659}	(31537.009)	(133.255)	(523.537)	{0.953}
Standardized treatment effect		0.002	0.008	[0.582]		0.002	0.009	[0.497]
		(0.004)	(0.014)			(0.003)	(0.014)	
Panel B: Prior Credit Subsample								
Credit Score	651.256	-0.348	-1.443	[0.616]				
	(112.034)	(0.694)	(2.877)	{0.657}				
Currently have a thick file?	0.386	0.001	0.005	[0.66]				
	(0.487)	(0.003)	(0.012)	{0.657}				
Credit limit on all open revolving credit	7109.001	-86.582	-340.161	[0.426]				
	(24741.895)	(108.702)	(426.98)	{0.657}				
Standardized treatment effect		-0.001	-0.005	[0.738]				
		(0.004)	(0.015)					

(Standard errors in parentheses)
[Per comparison p-values in square brackets]
{Family wise p-values in curly brackets}

Note: Table reports the coefficient on LOTTERY from estimating equation (1) by OLS (column 2), and the coefficient on INSURANCE from estimating equation (2) by IV (column 3). All outcomes are defined based on the current information in the September 2009 credit file. "Full sample" is N= 49980; "prior credit" subsample is defined by the 56% of the full sample that had at least one open revolving credit account prior to randomization (i.e. in February 2008); N=27895. A "thick file" is defined as 2 or more open trade lines. For each outcome shown in the left hand column we report the estimate, standard error and per-comparison p-value. Standardized treatment effects are calculated based on equation (2). For each standardized treatment effect we report the estimate, standard error, and per comparison p-value. All regressions include household size fixed effects, lottery draw dummies, and the analogous outcome measure from the February 2008 credit report data. All standard errors are clustered on the household.

"Maximum" access to credit is defined over the February 2009 and September 2009 credit report archive. All "maximum access to credit" outcomes are therefore measured as the maximum value of the current measures in these two archives. When controlling for the measure prior to randomization these are measured over the February 2008 and February 2007 archives. Note that the February 2009 measure of "credit limit" is not the same as the September 2009 measure. Specifically, in all archives but February 2009 we are able to examine the credit limit on open revolving credit accounts. In February 2009 however, the variable measures the credit limit on all revolving credit accounts verified within the last 13 months, even if currently closed. This affects the mean but should not affect the analysis since it should not differentially affect treatments compared to controls.

Table A20: Impact of Health Insurance on Financial Strain: Additional Analysis

	Control Mean	Reduced Form	2SLS	p-values
	(1)	(2)	(3)	(4)
Current Balances on all Open Revolving Credit	7109.001	-86.582	-340.161	[0.426]
	(24741.895)	(108.702)	(426.98)	

(Standard errors in parentheses)
[Per comparison p-values in square brackets]

<u>Notes:</u> All outcomes are measured since notification date through September 2009. Column 2 reports the coefficient and standard error on LOTTERY from estimating equation (1) by OLS. Column 2 reports the coefficient and standard error on INSURANCE from estimating equation (3) by IV. Column 4 reports the per-comparison p value and the family wise p-value across the different measures used to create the standardized treatment effect. Standardized treatment effect reports results based on equation (2). All regressions include household size fixed effects, lottery draw fixed effects, and the analogous outcome measure from the February 2008 credit report data. All standard errors are clustered on the household. Sample consists of all those matched to credit report data (N = 49980).

Table A21: Exploratory Analysis of labor force participation: survey data

	Control Mean	Reduced Form	2SLS	p-values
	(1)	(2)	(3)	(4)
Currently Employed?	0.456	0.011	0.039	[0.136]
	(0.498)	(0.008)	(0.026)	{0.292}
Work 20+ hrs at current job?	0.358	0.004	0.013	[0.6]
	(0.48)	(0.007)	(0.025)	{0.598}
Income	13053.031	176.396	602.798	[0.352]
	(11841.507)	(189.613)	(650.692)	{0.567}
Standardized treatment effect		0.015	0.052	[0.214]
		(0.012)	(0.042)	

(Standard errors in parentheses)
[Per comparison p-values in square brackets]
{Family wise p-values in curly brackets}

Note: Table reports the coefficient on LOTTERY from estimating equation (1) by OLS (column 2), and the coefficient on INSURANCE from estimating equation (3) by IV (column 3). For each outcome we report the estimate, standard error, per-comparison p-value and the family-wise p-value across the individual outcomes that contribute to a given standardized treatment effect. Standardized treatment effects are calculated based on equation (2). For the standardized treatment effects we report the estimate, standard error, and per comparison p-value. All regressions include household size fixed effects, survey wave fixed effects, and the interaction of the two. All regressions are weighted using the survey weights. All standard errors are clustered on the household. Sample consists of responders to the 12-month survey (N=23741).

Table A22: Health "behaviors", Survey data

	Control Mean	Reduced Form	2SLS	p-values
	(1)	(2)	(3)	(4)
Do not smoke currently	0.583	0.001	0.004	[0.891]
	(0.493)	(0.008)	(0.027)	{0.892}
More physically active than others your age	0.604	0.021	0.071	[0.005]
	(0.489)	(0.007)	(0.026)	{0.009}
Standardized treatment effect		0.022	0.077	[0.05]
		(0.011)	(0.04)	

(Standard errors in parentheses)
[Per comparison p-values in square brackets]
{Family wise p-values in curly brackets}

Notes: Column 2 reports the coefficient and standard error on LOTTERY from estimating equation (1) by OLS. Column 3 reports the coefficient and standard error on INSURANCE from estimating equation (3) by IV. Column 4 reports the per-comparison p value and the family wise p-value across the four different measures of financial strain used to create the standardized treatment effect. Standardized treatment effect reports results based on equation (2). All regressions include household size fixed effects, survey wave fixed effects, and the interaction between the two. All standard errors are clustered on the household and all regressions are weighted using survey weights. Sample consists of survey responders (N = 23741).

Table A23: Heterogeneous Impact of Health Insurance (2SLS)

	Administrative Data					Survey data				
	N	First stage	Hospital utilization	Financial strain	Survival	N	First stage	Total health care utilization	Financial strain	Self-reported health
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Full sample	74922	0.256	0.047	0.009	0.014	23741	0.29	0.137	-0.305	0.203
Gender										
Male	33673	0.27	0.079	-0.004	0.025	9690	0.3	0.154	-0.26	0.255
Female	41248	0.25	0.019	0.02	0.006	14050	0.28	0.126	-0.336	0.162
p-value (diff)			(0.248)	(0.452)	(0.752)			(0.704)	(0.252)	(0.215)
Age										
50-63	19724	0.27	0.036	0.031	0.035	7876	0.3	0.223	-0.305	0.27
19-49	55198	0.25	0.052	-0.001	0.002	15865	0.29	0.088	-0.306	0.176
p-value of difference			(0.826)	(0.427)	(0.683)			(0.087)	(0.992)	(0.256)
Race (Survey respondents)										
White	19556	0.31	0.123	-0.003	0.034	19527	0.3	0.137	-0.334	0.23
Non-white	4221	0.26	0.099	-0.083	-0.046	4214	0.24	0.138	-0.143	0.052
p-value of difference			(0.812)	(0.281)	(0.215)			(0.991)	(0.061)	(0.106)
Urbanicity										
MSA	57645	0.25	0.033	0.001	0.041	17755	0.29	0.146	-0.297	0.232
Non-MSA	17275	0.27	0.093	0.032	-0.069	5986	0.3	0.117	-0.322	0.125
p-value of difference			(0.295)	(0.5)	(0.114)			(0.732)	(0.754)	(0.232)
Prior financial status (credit report subsample)										
Have prior credit	27895	0.23	0.026	0.03	-0.015	10562	0.24	0.065	-0.313	0.17
Do not have prior credit	22085	0.29	0.011	-0.014	0.027	6829	0.34	0.217	-0.322	0.256
p-value of difference			(0.825)	(0.238)	(0.549)			(0.099)	(0.914)	(0.353)
Education (survey respondents)										
More than high school	7673	0.29	0.147	-0.041	0.026	7667	0.29	0.088	-0.28	0.167
High school or less	15341	0.31	0.111	-0.001	0.031	15311	0.29	0.171	-0.307	0.221
p-value of difference			(0.64)	(0.483)	(0.921)			(0.295)	(0.721)	(0.505)
Smoker (survey respondents)										
Ever smoke	14871	0.33	0.129	-0.007	0.005	14850	0.32	0.158	-0.33	0.175
Never smoke	8421	0.25	0.097	-0.056	0.074	8407	0.24	0.097	-0.26	0.233
p-value of difference			(0.649)	(0.391)	(0.118)			(0.452)	(0.377)	(0.484)

Note: Table reports estimates for different subsamples as shown in the left hand column. Columns 1 and 2 report the sample size and first stage estimate (based on equation 4) for the full for which the measure is available. Columns 6 and 7 report the analogous results for the subsample of survey responders. The table reports the point estimates for the IV standardized treamtent effects which are based on the coefficient on INSURANCE from estimating euqation (3) by IV and then calculating the standardized treamtent effect based on equation (2). "p-value of difference" refers to the p-value of the difference in the standardized treatment effects between the two groups. The baseline (row 1) results were previously reported as follows. Column 3: Table 4b, Panel A; Column 4: Table 7 Panel A; Column 5: Table 9, Panel A (converted to standardized units); Column 8: Table 5 right hand panel; Column 9: Table 8; Column 10: Table 9 panel B. For the sub-group analysis we use the standard deviation from the pooled group when standardizing.