Draft title: The short-run impact of extending public health insurance to low income adults: evidence from the first year of The Oregon Medicaid Experiment[+]

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Analysis Plan
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Section I: Introduction to the analysis plan

This document describes the analysis plan for the above-titled paper (or papers). The goal is that by pre-specifying a analysis plan, we avoid (or at least minimize) issues of data mining and specification searching. While we may well perform additional analyses motivated by our results, this analysis plan serves as a record of all of our ex ante planned analyses of these data.

At a very broad level, the goal of the analysis is to use the Oregon Medicaid Experiment together with mail survey data and administrative data (from credit reports, hospital discharges and death records) to estimate the short-run (approximately one year) causal effects of expanding public health insurance availability for low income adults.[1]

We organize the main analysis in this paper around the potential costs and benefits of health insurance. We therefore conduct three primary analyses: On the cost side we examine the impact of health insurance on (1) increased utilization. On the benefit side we examine the impact of health insurance on (2) health and on (3) financial security.

A key strength of our analysis is that rather than looking at only a narrow set of outcomes (such as e.g. hospital utilization), we are able to assess a wide range of potential costs and benefits, including analysis of the impact of health insurance on financial security for which there is relatively little analysis (and no experimental evidence).

Another strength of our analysis is the use of both administrative and survey data. Table 1 summarizes the types of data used in each of the primary analyses. The key advantage of administrative data is that they contain the universe (or a random sample thereof as described in more detail below) of the study population and therefore are not subject to potential biases from non response.[2] The main disadvantage of the administrative data is the limited range of outcomes we can measure in such data; this can be seen in Table 1. We therefore also draw on mail surveys we administered; in addition to measuring many of the same outcomes as in the administrative data, these surveys enable us to capture additional outcomes, in particular the health of the population and health care utilization outside an inpatient hospital setting. A primary concern with the survey data is potential bias introduced by non response; we investigate this in detail below. As a result of these tradeoffs, we view the two main types of data as highly complementary.

In addition to our primary analysis, we also undertake several supporting and exploratory analyses. Specifically: (1) a more detailed examination of the impact of health insurance on utilization, quality of care, and hospital sorting using primarily the hospital discharge data but also some survey data; (2) a more detailed analysis of the impact of health insurance on financial well being using the credit report data and survey data; (3) an exploration of potential *mechanisms* behind any impact of health insurance on improved health (including any impact which may accrue beyond our one year study horizon); these mechanisms include access to care,

[1] We hope to add more administrative data (specifically for outpatient surgical care, emergency department use, and income data) if they become available.

[2] Ashenfelter and Plant (1990) provide a well-known example of how non response (or attrition) bias can contaminate the analysis of a randomized experiment.

quality of care, preventive care, and health behaviors); and (4) exploratory analysis of the potential impact of health insurance on labor force participation and self reported happiness in the survey data. We note that we do not consider the more detailed analyses of our 3 primary domains (utilization, financial well being, and health) to be of less interest or importance than the analyses labeled "primary"; rather, we simply tried in the "primary" analysis to make an (albeit imperfect) attempt to summarize with a few numbers the bottom line impacts as might be used in a cost benefit analysis.

The rest of the analysis plan proceeds as follows. Section II provides an overview of the lottery design and our data sources. Section III presents our basic estimating equations and analytical framework. Section IV presents some initial information useful for the subsequent analysis. Specifically we examine balance of treatment and control in our various samples, present some descriptive characteristics of our population, and report the first stage results. Section V lays out the planned primary analyses on the impact of health insurance on utilization, financial strain, and health. Section VI lays out the supporting analysis of the more detailed look at the impact of health insurance on utilization; Section VII lays out the supporting analysis of the more detailed look at the impact of health insurance on financial well being. Section VIII lays out the supporting analysis of the impact of health insurance on potential mechanisms for health improvement. Section IX lays out the exploratory analysis of the impact of health insurance on labor force participation and happiness using the survey data. Section X discusses some potential interpretations and caveats.

A note on the data examined to date: In developing the analysis plan we generally restricted our investigation of the data to only the control data (which we examined in depth to get a sense of the distribution of outcomes in our control sample, which in turn guided many of our analysis choices). All summary statistics in this document refer to the control sample only, unless explicitly noted otherwise.

We made the following specific exceptions where we also looked at the treatment data: (1) Examination of balance of treatment and control (Table I2 and Appendix Tables A14-A16); examination of the first stage (Tables I4 through I6) and Figure I2; and examination of a potential supply side response of credit to increased health insurance (Table S2). Our logic behind these analyses is these were fundamental decision points whereby based on the results we might specify the rest of the analysis plan differently (e.g. if no first stage, etc). We also examined the length of time that treatment individuals had insurance (Table I1 and Figure I1), as well as the distribution of response times by treatment and control individuals (see Table I1).

The data we worked with in developing the analysis plan will not be the very "final" data. For example, we do not yet have the mortality data we will analyze (which we will also use to exclude individuals who died prior to the lottery), and some data received from the state may be revised. We try to note where data are likely to change. More generally many of the statistics included here to inform our thinking were created using not-yet-final versions of the data, therefore specific summary statistics may well change slightly; there are also likely some inconsistencies across tables (or between table and text) in the data since the data were in a state of (relatively minor) flux as we prepared this analysis plan.

Section II: Overview: lottery design and data sources.

IIA. Lottery design

The Oregon Health Plan (OHP) – created by one of the first federal waivers of traditional Medicaid rules – currently consists of two distinct programs: *OHP Standard* and *OHP Plus*; the lottery we study is for eligibility into OHP Standard. OHP Plus serves the categorically eligible Medicaid population.[3] By contrast, OHP Standard (the subject of our study) covers those who are financially but not categorically eligible for OHP Plus.

Specifically, OHP Standard provides coverage for adults (ages 19 - 64) who are Oregon residents, are U.S. citizens or legal immigrants, have been without health insurance for six months, have income below the federal poverty line, and have assets below $2,000.OHP Standard provides relatively comprehensive benefits with no consumer cost sharing. Physician services, prescription drugs, hospice care, mental health and chemical dependency services are covered, and some durable medical equipment, but dental and vision are not. All major hospital benefits are also covered, but there are some limitations (Office for Health Policy and Research, 2009).[4] Monthly premiums range from $0 to $20 depending on income (with those below 10 percent of the FPL paying $0). Using state Medicaid data from 2001-2004, Wallace et al (2008) estimate that average annual Medicaid expenditures for an individual on OHP Standard was about $3,000. Appendix 0 provides more detail on the OHP Standard benefit package and the steps taken to verify eligibility. OHP Standard is funded through provider tax revenue from large urban hospitals and Medicaid managed care organizations; this tax revenue is augmented by federal matching funds (Office for Health Policy and Research, 2009).

In early 2002, enrollment in OHP Standard peaked at about 110,000. Overall enrollment in OHP[5] also peaked at this time, at about 475,000. (Office for Health Policy Research, 2009) Due to budgetary shortfalls OHP Standard was closed to new enrollment after June 30, 2004. By early 2008, attrition had reduced the average monthly enrollment in OHP Standard to about 19,000, but the two-year budget period ending in June 2009 allowed for an average monthly enrollment of 24,000. The state therefore determined it had the budget to enroll an additional 10,000 adults. Therefore, in January, 2008, the Oregon Department of Human Services (DHS) announced that it would re-open the OHP Standard program. Because DHS (correctly) anticipated that the demand for the program would far exceed the 10,000 available new enrollment slots, DHS requested and received permission from the Centers for Medicare and Medicaid Services (CMS) to conduct a random drawing to add the new members.[6]

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[3] The categorically eligible are: pregnant women and children 0 to 18 up to 185% of the federal poverty level (FPL), people with disabilities up to the SSI income level or 300% of the SSI income level if meeting long-term care needs, and families enrolled in Temporary Assistance to Needy Families or with foster children up to 100% FPL.

[4] Emergency dental services only are covered. Mental health and chemical dependency coverage includes outpatient coverage. Covered medical equipment and supplies includes diabetic supplies, respiratory and oxygen equipment, ventilators, suction pumps, and tracheostomy, urology, and ostomy supplies. The OHP Standard hospital benefit is approximately 85 percent of the actuarial value of the full (OHP Plus) hospital benefit. The benefit includes evaluation, lab, ex-ray and other diagnostic tests, treatment of all emergency services, and "urgent conditions for which prompt treatment will prevent life threatening health deterioration." (Office for Health Policy and Research, 2009).

[5] OHP Overall enrollment includes individuals enrolled in OHP Standard, OHP Plus and OHP Exempt programs (citizen/alien waived emergency medical, breast and cervical cancer, and qualified Medicare beneficiaries).

[6] The department chose a random selection process because it gives everyone an equal opportunity to have their name drawn from the list. Adding people to OHP Standard based on health status was not allowed by Federal law,

That same month, DHS launched an extensive public awareness campaign about the opportunity to be considered for eligibility into OHP Standard. This campaign included releasing weekly press releases and radio public service announcements, sending letters to current participants in all DHS programs for low income Oregonians (e.g., WIC, food stamps, etc), and distributing educational materials to more than 1700 community partners including advocacy groups, health care providers, health plans, and state and local service agencies. During the 5 week period from January 28 through February 29, 2008, interested individuals could add themselves and/or others on the lottery list by telephone, fax, in person, by mail, or online. By the closing date of the lottery list, February 29, 2008, **88,648** individuals were on the list.[7] To sign up for the lottery the individual needed to provide information on name, birthday, gender, address, telephone number and preferred language of communication (sign up forms were available in English or in Spanish). They were also asked to list the name, gender and date of birth of anyone 19 or older in the household who the individual wished to add to the lottery list.[8] From this information, we were able to determine that some of the individuals on the list were not in fact eligible for OHP Standard (for example, not the correct age or not residents of Oregon) or (by looking at subsequent enrollment data) that they were unlikely to be successfully enrolled (for example because they had been signed up by an unrelated third-party). We used these pre-randomization criteria to make exclusions from our analytical study population which are described in detail in Appendix 1. After exclusions, **72,700** individuals were eligible to be included in our study. All subsequent numbers on lottery winners and study population are drawn from this **72,700** maximum potential study population.

Eight random lottery drawings from the list were conducted by Oregon's Department of Human Services' Division of Medical Assistance Programs (DMAP) from March 2008 – October 2008 (approximately once per month); as described in more detail in Appendix 0, we verified by computer simulation that the results of the lottery are consistent with the random drawing procedure the state described to us.

Importantly, the state considered the entire *household* of any winning name drawn to have won. This has two implications: first, the treatment occurs at the level of the household. Second, the nature of the selection process disproportionately favored individuals who listed more individuals in their household on the lottery sign up sheet; as a result, winning (treatment) individuals are disproportionately from larger households than the control (non winning) individuals. In all of our analyses we therefore include indicator variables for household size. We also cluster our standard errors on the household since the treatment occurs at the level of the household. [9]

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which prevents states from determining eligibility for federal programs based on health care conditions. DHS also considered selecting names on a 'first-come, first-served' basis, but rejected that option because it puts people without ready access to the information or the means to quickly get on the list at a disadvantage (Oregon Department of Human Services, 2008).

[7] The original lottery list we received from the state included duplicate records, test records, deactivated records and a handful of sign ups completed after the original draw. We removed these records from our list. Details are included in Appendix 1.

[8] Individuals were asked very little information when signing up for the lottery, as barriers to entry were intentionally kept low. Individuals could sign up unrelated third parties. No attempt was made during sign up to verify information or eligibility, or to get additional (third party) contact information. This will be important in understanding the relatively low take up among lottery winners, which we discuss in more detail below.

[9] Throughout this paper we define "household" and "household size" based on the number of individuals in the household listed on the lottery sign-up sheet. Our "household size" variable is of course distinct from the actual household size. Moreover we note that individuals in a winning household whose names were not included on the

In total, **29,411** individuals - representing **24,963** unique households - were selected by lottery. Winning households were eligible to apply for OHP Standard coverage; if they submitted the appropriate paperwork within the 45 day eligibility period after selection and demonstrate that they meet the eligibility requirements, they were enrolled in OHP Standard. Once enrolled in OHP Standard, they could remain enrolled indefinitely, provided that they re-certify their eligibility status every six months.

We obtained data from the state on the application and enrollment status of each individual selected from the lottery list. Overall take-up of insurance by those selected in the lottery was quite low, less than **a third**. There were two sources of slippage in take-up: only about 60 percent of those selected sent back applications, and only about half of those who sent back applications were deemed eligible for insurance; the primary source of ineligibility was due to failing to meet the income eligibility requirement.[10] Allen et al. (2010) provide more detail and discussion of take-up.[11]

II.B. Overview of Data Sources

We have two main types of data: administrative data and survey data. The survey and administrative data are highly complementary. The primary advantage of administrative data is that they cover the universe (or a random sample thereof) of lottery participants and are therefore not subject to potential non-response bias as in survey data. Another advantage of administrative data is that they are based on "objective" measures so are not subject to potential self-reporting biases (which may be influenced by health insurance). The primary advantage of the survey data is that they allow us to observe important outcomes that are not capturable with administrative data, in particular health (besides mortality), health care utilization outside of an inpatient hospital setting, the direct financial strain of medical expenses, and a broader measure of insurance coverage. Table 1 summarized which data elements are available for which types of substantive analyses.

Appendix 1 provides considerably more detail on our various data sources, matching and sampling strategies, as well as the overlap of sample across the various data sources. Here we briefly summarize the key features of each data type.

Administrative data

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lottery sign up form were also considered to have won; however such individuals are not on our lottery list and not contained in our data. The proportion of individuals in household size 1, 2 and 3 respectively was **76.6 %, 23.2 % and .2 %**. However, among the winning individuals, the household size proportions were **66.7 %, 32.8 % and .5 %**.

[10] This takeup number was lower than we (or the state) initially expected. It may partly reflect the low barriers to sign up for the lottery (relative to applying for insurance and qualifying for it if you won the lottery). However it should not, in principle, reflect a common explanation for low Medicaid take-up, namely the notion of "conditional coverage" among those who are eligible but do not take up Medicaid (Cutler and Gruber 1996). If selected individuals did not successfully enroll within the limited eligibility period after being selected, they could not apply later for coverage (although whether they understood that is not clear).

[11] The Allen et al paper uses the data provided by the state on the status of applications for insurance. For this analysis we instead use the state's enrollment data which we received at regular intervals. These data sources are supplied by different state agencies and do not perfectly agree. Specifically, just under 2% of the treatments are coded as approved for insurance in the application data, but never appear as enrolled in the enrollment data.

The administrative data consist of: (1) baseline demographic characteristics reported by individuals at the time of sign up for the lottery (January and February 08), which we refer to throughout as "lottery list variables" (2) state Medicaid data on weekly enrollment of study participants (which we use to estimate our first stage) (3) hospital discharge data from January 08 through September 09, (4) credit report data in snapshots from (primarily) February 08 and September 09 and (5) Mortality data. Note that both the hospital discharge data and the credit report data also provide some baseline (pre randomization) data that we can use in our analysis.

Lottery list variables

We use the lottery list to measure demographic information (reported at the time of sign up for the lottery, and hence pre randomization) for our entire sample. Specifically we construct the following "lottery list" variables: year of birth; sex; whether English is their preferred language for receiving materials; whether the individual signed themselves up for the lottery or was signed up by a household member, the number of household members included when signing up for the list (which we refer to as "household size" throughout this paper), whether the individual gave their address as a PO box, whether they signed up the first day the lottery list was open, the median household income in the zip code they gave, whether the zip code they gave is within a census-defined MSA, and whether they provided a phone number on sign up. Appendix Figure A0 shows the actual lottery sign up form.

Medicaid application data

We have data from the state on the status and disposition of any Medicaid application submitted by individuals selected in the lottery. We use this information primarily to help measure the length of time people in our treatment group are insured. It also provides information on the reasons for ineligibility, some of which are discussed in Allen et al. (2010).

Medicaid enrollment data

We have yearly summaries for enrollment during each year starting in 2002 and continuing through 2009. These summaries include the dates for any periods of enrollment OHP Standard, OHP Plus, and a variety of much smaller targeted programs. We also receive weekly snapshots of enrollment until the yearly files become available. There is a concern with these data, discussed in more detail in Appendix 1 which may lead to a slight overestimate in our first stage.

Creating matched lottery draws and notification dates for control individuals

In the hospital and credit report administrative data (described next) we measure the outcomes starting at the earliest possible date of a treatment effect. We define this date at the *"notification date"* which is the date at which individuals in that lottery draw were notified that they had been selected by the lottery; these dates represent the first indication that individuals had of their treatment status, although it predates enrollment on average by **10 weeks**. Notification date varies by **7 months** from the first to the last (eighth) lottery draw. The time frame over which an outcome is measured therefore varies by lottery draw.[12]

[12] A primary reason for doing this (rather than just measuring all outcomes from the earliest notification date for any lottery draw) is to increase the availability of "pre randomization" hospital data for analysis. If we analyzed outcomes from the earliest notification date we would have less than 3 months of pre-randomization data; this approach gives us up to 9 months for some lottery drawing and on average 5 months.

Since notification date varies by lottery draw, we needed to assign control individuals to lottery draws. For control individuals, we randomly assigned a *lottery draw* at the household level, stratified on household size, to match the distribution of lottery draws in the treatments. This resulted in an assignment such that the probability of treatment is constant across lottery draws conditional on household size. We did this so that within household size, treatment and control outcomes (measured since the relevant lottery draw's notification date) are measured over the same average time window. See Appendix 1 for more details.

Hospital discharge data (HDD)

Although inpatient admissions are relatively rare (for example, only about **6%** of our control sample has an inpatient admission over a 12 month period), they are quite expensive and account for a large share of medical expenditures. Therefore understanding the impact of health insurance on hospitalizations may be quite important for the overall impact of health insurance on health spending.

We obtained hospital discharge data for the entire state of Oregon from January 2008 through September 2009.[13] We ended the data period when we did for two related reasons: to match the timing of our mail survey (average response date was at the end of September 2009), and to study the approximate 1 year effect of insurance. Working the Office of Oregon Health Policy and Research (OHPR), we were able to probabilistically match these data to the lottery list, thereby identifying hospital admissions for our sample. These data are similar to the Hospital Cost and Utilization Project (HCUP) inpatient datasets; in fact, the data we obtained are the raw data used by OHPR to prepare the Oregon HCUP data. 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 there data with several hospital-level data sources (such as American Hospital Association data and the Center for Medicare and Medicaid Services' Hospital Compare data) to obtain additional detail on the hospitals.

We use the data to examine hospital utilization and quality of care for admissions occurring from the notification date through August 31, 2009. On average, this represents **15 months** after the notification of lottery winning and **13 months** after successful enrollment. We also construct pre-randomization measures of all our outcome variables using admissions from January 1, 2008 through the notification date; on average we have **5 months** of pre-randomization data.

Credit report data

We obtained the complete credit records for a subset of our lottery list from one of the three national credit reporting companies. Credit bureaus collect rich and detailed information on

[13] Unfortunately, prior to 2008, the state's hospital discharge data did not have enough individual identifying information to match to our lottery sample. Specifically, it lacked information on patient name.

virtually all formal consumer borrowing.[14] Yet the analysis of such data is still relatively rare in the economics literature.[15] Appendix 1 provides more detail on the data.

We use these data to study the impact of the lottery on financial well-being. Specifically, we study the impact of the lottery on the frequency and magnitude of adverse financial events (i.e. unpaid bills) and on access to credit. Adverse financial events include bankruptcies, judgments, collections, and late payments on credit cards. Access to credit measures include credit scores and available credit limits. A central point to note is that our low income population has extremely limited access to credit; this guides some of our choices of analysis variables, as well as some interpretation points, as we discuss in more detail below.

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, gender and date of birth as the individuals reported it in signing up for the lottery. Crucially, we use only pre-randomization data in matching our individuals to (pre-randomization) credit report data. This process generated a **66%** match rate with the February 2008 credit bureau data. Non matches arise either due to insufficient information for a definitive match (the credit bureau errs on the side of false negatives rather than false positives) and because some low income individuals will not have a credit file. We only analyze credit data for individuals who were matched to this February 2008 data.

Our primary outcomes analysis is from the September 2009 credit file which contains data through September 30, 2009. (We chose this time period for the same reasons enumerated above for the time period of the hospital discharge data.) The credit bureau was able to track over **97%** of the study participants found in February 2008 to the September 2009 file. A prime advantage of the credit data (besides their rich measures) is that they should not be subject to the types of non response bias concerns that plague survey data collected *post*-randomization; effectively we have a 97 percent response rate. Consistent with this, we demonstrate in Section IV (Table I2) and Appendix 4 (Tables A12-A14) below that match rates in the September 2009 file, pre-randomization lottery list variables for those who matched, and February 2008 outcome measures for those who matched are all balanced across treatment and control groups.

On average, as of September 30, 2009, we observe **16** months after the notification of lottery winning and **14** months after successful enrollment among winners. We also construct pre-randomization measures of all outcomes which we define in the February 2008 file using a "look back" period that is the same as the lottery-draw specific time elapsed from notification date through September 30, 2009.[16]

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[14] Avery, Calem and Canner (2003) provide an excellent,detailed discussion of credit bureau data; much of our discussion of the data and our choice of analysis variables is guided by their work.

[15] To our knowledge, credit bureau data have never been used to study the impact of health insurance. For recent examples of other papers that use credit bureau data, see e.g. Gross and Souleles (2002) who use individual level data to study the impact of access to credit and Mian and Sufi (2009) who use use zip-code level credit bureau data to study the impact of access to subprime mortgages.

[16] Specifically, since our analysis in the September 2009 data covers the period from "notification date" for a given lottery draw through September 2009, in our pre lottery data from February 2008 we define "pseudo notification

Mortality data

We will use mortality data for two purposes. First, as previously noted, we will exclude anyone from our sample that died prior to their notification date. Second, we will analyze mortality from the notification date through September 30, 2010. Ultimately our mortality data will come from the National Death Index; given the time lag involved, however, we will start with mortality data from Oregon's Center of Health Statistics (which will not include deaths outside Oregon). For now we are (temporarily) working with the Social Security master death index which may not be a complete record of deaths and therefore will not be used in the final analysis.

Survey data

The primary mail survey that we use in this analysis was conducted at about a little over a year post insurance coverage; we refer to this informally as our "12 month survey"[17] In addition, an initial survey was fielded between June 2008 and January 2009**;** for all but a few individuals it is not pre-randomization. Therefore we primarily use the initial survey to get a snapshot of the characteristics of our (control) sample at around the time of sign up.

The 12 month survey sample consists of **57,553** individuals from the **72,700** on the original sign up list and eligible for our study. Specifically, it includes virtually all (**29,172)** of the individuals selected by the lottery and **28,381** selected controls. The 12-month survey sample was split into 7 survey waves, with surveys mailed at staggered dates. The survey waves, which are described in more detail in Appendix 1, were the result of our attempts to draw our control sample concurrent with the original lottery drawings. Each time the state drew a group of lottery winners, we drew a set of control winners. 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 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) l. As a result, we will include *survey wave* dummies in all of our survey analysis (and also survey wave x household size dummies for similar reasons).[18]

The 12 month survey consisted of a basic protocol of three mail survey attempts. In addition, we designed an intensive protocol (conducted on approximately 30 percent of the non-respondents), which included additional tracking efforts and mailing and attempts at phone contacts. The response rate to the basic protocol was **36** percent; following the intensive protocol, the overall weighted response rate to the 12 month survey was **50** percent, where individuals who responded to the intensive follow up are weighted by the inverse probability of their being included in the intensive subsample. Some of the non-respondents were people we were unable to reach,

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dates" for each lottery draw so that the length of time from each lottery draw's "pseudo notification date" through February 2008 is the same as the length of time from the notification date through September 2009.

[17] In addition, a smaller survey of approximately 1/5 the sample size was conducted at approximately **six months** post insurance coverage. The current analysis plan does not include these data. More detail on this survey is available in Appendix 1.

[18] Note that these survey waves are not the same as lottery draw for treatment individuals or for our "matched lottery draw" for control individuals. For the administrative data, we did not have the staggered data collection of the mail surveys, so we do not need to control for survey wave in the analysis.

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 difference between treatment and control responders; we investigate this in detail in Section IVA below.

The average response date to the twelve-month survey was **September 23, 2009**. Treatment responders responded an average of **15 months** after they were notified; enrolled treatment responders responded to the twelve-month survey an average of **13 months** after their applications for insurance coverage was approved. There is considerable variation in the timing of the twelve-month survey, however, so that some enrolled treatment responders replied as early as **6 months** after they were first enrolled and some as late as **23 months** after.

Finally, we note that outcomes in the survey data are generally reported using either a 6 month look back period (e.g. number of doctor visits in the last 6 months), or about "current" conditions (e.g. number of prescription drugs you are currently taking). As a result, outcomes in survey and administrative data are not directly comparable (even among the sub-sample of responders) since, as noted previously, in the administrative data (hospital and credit report) we measure outcomes since the notification date through August or September of 2009.

Time Period of Study

Table I1 summarizes the average time period of our study population for different ways of considering when the treatment began. One starting point for the study would be the notification date of being selected in the lottery, which would seem to be the earliest possible date by which there could, in principle, be a treatment effect. Another starting point would be the date when individuals first were covered by insurance. The insurance obtained through the lottery actually applied retroactively back to only a few days after an individual's application was mailed. Selected individuals may have been unlikely to change their behavior while their applications were being processed (indeed this is our casual impression from focus group interviews with selected individuals), but the retroactive insurance coverage may have affected the financial burden associated with health care utilization during that period. The latest starting point we consider is the date of application approval for selected individuals. This would seem the most natural starting point for potential treatment effects. These dates are on average two months apart (reflecting the time it takes to fill out, send back, and process an application). On average individuals have been notified for about **16** months, covered (including retroactive coverage) for about **15** and approved (i.e. "real time" coverage) for about **14** months by the end of our study period on September 30, 2009. There is considerable variation in these time periods by lottery draw, so we present the lottery draw-specific numbers as well. In general we consider the time period of the study to be 14 to 16 months or "about one year."

For the survey data, these averages mask a great deal of heterogeneity in the time for insurance starting to survey response, as shown in Figure I1. This heterogeneity reflects the 8-month span of lottery draws, the 9-month span of survey fielding, and individual variation on survey response time. (See Appendix 2, Table A5 for more detail).

Section III. Estimating equations

III.A. Basic estimating equations: Reduced Form, First Stage, and 2SLS

Let y_{ij} represent outcome $j \in J$ for individual *i*, where *J* represents a "domain" of related outcomes. For example y_{ij} might be the self-reported health of individual *j,* which is one of the health measures in the health "domain" J. We define (sign) each outcome within a domain so that higher values all have the same interpretation within a domain (e.g. more health care use, more financial strain, worse health etc).

We begin by estimating the reduced form effect of the lottery which give the average difference in means between the treatment group (the lottery winners) and the control group (those not selected by the lottery). Based on the lottery design and sampling structure described above, our basic reduced form estimating equation is:

$$y_{ihj} = \beta_0 + \beta_1 LOTTERY_h + X_{ih}\beta_2 + V_{ih}\beta_3 + \varepsilon_{ij} = W\theta_j + \varepsilon_{ihj} \qquad (1)$$

where *i* denotes an individual, *h* denotes a household and *j* denotes a "domain". LOTTERY is an indicator variable for whether or not household *h* was selected by the lottery. The coefficient on LOTTERY (β_1) is the main coefficient of interest, and gives the reduced form effect of winning the lottery on the outcome studied; this is also referred to as the "intent to treat" (ITT) estimate..

The reduced form (or ITT) estimates from equation (1) provide an estimate of the causal effect of winning the lottery (i.e. winning eligibility to apply for OHP Standard). This gets at a potentially policy-relevant question of: what is the net impact of allowing interested individuals to apply for access to public health insurance? Since many policy proposals involve voluntary enrollment, this is a potentially interesting variable.

However, it is also of considerable interest to ask: what is the impact of *enrolling* interested individuals in the public health insurance program? This estimate will differ from the reduced form estimates from equation (1) for several reasons: not all individuals who win the lottery will enroll in OHP Standard (some will not send back applications and some who do will not be eligible);not all those who enroll will remain enrolled during our study period; some of those who enroll may do so at the same time that they drop private insurance (crowd out); and some of our control individuals may find other sources of insurance coverage.

The causal effect of insurance on outcomes of interest is modeled as follows:

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

We estimate equation (2) by two stage least squares (2SLS), using the following first stage equation[19]:

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

in which the excluded instrument is the variable "LOTTERY" with the first stage coefficient of δ_1. We discuss and explore in Section IV alternative possible definitions for our first stage variable "INSURANCE." Our baseline measure of INSURANCE will be whether or not the individual was (ever) on Medicaid (i.e., OHP Standard or OHP Plus) during the study period, as measured by the universe of the state's administrative data on Medicaid enrollment. Because the model is just identified, the 2SLS estimate of π_1 is given by the ratio of the reduced form and first stage coefficients ($\beta_1/\ \delta_1$).

The identifying assumption behind the 2SLS estimator is that winning the lottery only had an effect on the outcomes through obtaining insurance. This seems to us a reasonable assumption. Of course we cannot exclude potential "winning" effects on outcomes (perhaps e.g. on state of mind) that could operate without any impact on insurance, although it seems unlikely to us that such effects both exist and would persist for a year after the lottery. We also note that in much of the public health insurance literature there is a notion of a potential "option value" of public health insurance among those who are eligible but not covered since they may choose to take up that coverage if and when it becomes needed (e.g. they get sick). For this reason, Cutler and Gruber (1996) refer to such Medicaid-eligible but uncovered individuals as "conditionally covered". However in our context this is not relevant since individuals who won the lottery were only eligible for Medicaid coverage if they successfully submitted an application within 45 days of receiving it.

We interpret the 2SLS estimates as a local average treatment effect, or LATE, (Imbens and Angrist, 1994). In other words, the 2SLS estimate of π_1 identifies the causal impact of insurance among the subset of individuals who would obtain insurance on winning the lottery and would not obtain insurance without winning the lottery. In practice, we suspect that our LATE estimate is quite similar to the average treatment effect (ATE) (otherwise known as the treatment on the treated) since, we suspect that we have very few "always takers" (in the language of Angrist, Imbens and Rubin 1996) and therefore almost all of our treated individuals are in fact compliers. We discuss this in more detail in Section IVC ("First stage") below when we discuss who the compliers are in our data. However, because, as we discuss in more detail there, there are undoubtedly a small number of "always takers" among our lottery winners with insurance, we hesitate to claim that our 2SLS estimates give the treatment on the treated estimates. (In a different context, see Kling et al.2007 for an example of where this claim appears warranted).

Covariates:

[19] We briefly explored whether interacting LOTTERY with lottery list covariates would be a fruitful way of increasing power. Our explorations did not yield an obvious silver bullet of pre-randomization characteristics of lottery participants that produced substantially different first stage estimates. We hold out hope that if we are able to successfully complete the administrative processes needed to match our sample to administrative earnings records, that interacting with pre-lottery earnings may significantly enhance the power of our first stage.

In equations (1) through (3), we distinguish between two sets of covariates in the analysis. We denote by X_{ih} the set of covariates that are correlated with treatment probability (and potentially with the outcome y) and are therefore needed to ensure that the reduced form (respectively, 2SLS) estimates of β_1 (respectively, $\pi_{1)}$ gives an unbiased estimate of the relationship between winning the lottery (respectively, insurance) and the outcome.

Note that the definition of X_{ih} will vary based on the data source. Specifically, in all of analyses, these include indicator variables for whether the household is of size 2 or of size 3 (the omitted category is size 1). For outcome measures from the survey data, we will also include indicator variables for *survey wave* (and the interaction of these indicator variables with household size) since, as noted above, survey wave is correlated with treatment probability and may be correlated with the error term in the estimating equation.

We denote by V_{ih} a second set of covariates which can be included to potentially improve power by accounting for chance differences in variables between treatment and control group but which are (in principle; see discussion below on non response bias in the survey data) not needed for β_1 (respectively, $\pi_{1)}$ to give an unbiased estimate of the relationship between winning the lottery (respectively, insurance) and the outcome. There are three sources of such variables: (1) the set of demographic characteristics available from the pre-randomization lottery sign up list [20]; (2) pre-randomization y's as measurable in the administrative data and (3) the *lottery draw* that the individual is from. Note that unlike survey wave, the lottery draw is not needed as a control to produce an unbiased estimate since, by construction, treatment probability is uncorrelated with lottery draw within household size; however since the lottery draw affects the time window over which an outcome is measured – and therefore the mean outcomes will vary with lottery draw – controlling for it can improve power.

Our baseline survey analysis will control for none of these V_{ih} covariates; we will report sensitivity to adding controls for lottery list characteristics and pre-randomization outcomes in the administrative data as part of our investigation of non response bias in the survey data.

To improve power, except where explicitly noted[21], our baseline analysis of administrative outcomes in the hospital, credit report, and mortality data will include *lottery draw* indicators as well as lagged (pre randomization) measures for each outcome in the hospital and credit report data. We suspect that the point estimates will not be sensitive to such controls but will explore omitting these additional covariates and report on sensitivity.

Thus, in summary all analyses include household size dummies; the survey analysis also includes survey wave dummies and their interaction with household size dummies; the analysis of credit report, hospital discharge and mortality data also includes lottery draw dummies and (for the

[20] Specifically, it consists of variables based on lottery list information for year of birth, female dummy, preferred language English dummy, signed self up, whether address is a PO box (only for 12 month since sampled on in six months), whether signed up on the first day the list was open, median hh income in your zip code, whether have phone (only for 12 month since sampled on in six month), and whether in an MSA (according to your zip code and the census definition of an MSA).

credit report and hospital discharge data) the pre-randomization measure of the outcome analyzed.

Weighting and adjustment of standard errors:

In all of our analyses we cluster the standard errors on the household identifier since (as explained in the lottery design) the treatment is at the household level. (We discuss below in "outcomes and multiple inference" further adjustments for multiple inference). Analyses of administrative data (credit report, hospital discharge, and mortality data) are unweighted. The analyses of the survey data are weighted to account for the sampling design of the survey. Individuals who did not respond to the basic protocol are weighted by the inverse of the probability of being selected for the intensive subsample (See Appendix 1 for more detail)**.**

Quantile analysis:

In addition to the analysis of means depicted in equations 1 through 3, for some outcomes we will be interested in quantile analysis of the impact of health insurance on the right tail of the outcome's distribution. This is particularly the case for outcomes related to financial strain where the welfare gains will come disproportionately from removing extreme right tail debt or expenditures. For some outcomes, we will therefore estimate quantile treatment effects for each quantile.

Note that the quantile treatment effects do not identify the impact for a given individual (e.g. the median quantile treatment estimate is not the impact of the lottery for someone at the median of the non-lottery distribution) because of potential rank reversals. Rather, the quantile treatment effects give the differences in the treated and control *distributions* (rather than the treatment effects for identifiable individuals in either distribution). For our purposes this is not a concern; we are interested precisely in the question of whether the distribution (specifically the right tail) of some measure of financial strain changes with insurance.

III B. Dealing with many outcomes: domains, standardized treatment effects, and adjustments for multiple inference[22]

We group individual outcomes into domains based on the idea that they are measuring related constructs – for example, multiple measures of health – even though we do not assume (or imply by the grouping) that the different measures are all alternative proxies for a common underlying factor. Our three primary domains are: health care use, health, and financial well being.

The primary purpose of grouping into domains is to allow us to perform omnibus tests on whether there is any overall effect of health insurance on that domain. To improve the power of these tests, we sign the outcomes within each domain to improve power to detect effects that go in the same direction within a domain; our statistical test is of the standardized treatment effect within that domain. An additional advantage of this aggregation is that it reduces the number of statistical tests performed.

[22] This section draws heavily on Kling and Liebman (2004) and Kling Liebman and Katz (2007) in its approach

We rely on the standardized treatment effects for our "bottom line" summary estimate of the impact of health insurance on a domain. Although our preferred estimate pools measures across administrative and survey data, in recognition of the important differences between them – in data modality (self report vs administrative), look-back period, and potential response bias issues in the survey data – we also report the standardized treatment effects separately for the survey data and the administrative data within each domain.

We caution however that while the desire to summarize our results parsimoniously leads us to estimate and report standardized treatment effects, that they are far from a panacea as a "bottom line" estimate. A particular concern with them is that they implicitly "weight" each component of the index (i.e. each outcome within a domain) equally. These are presumably not the "correct" weights; for example, on the health domain, presumably one should not weight a positive depression screen and mortality equally![23] Nonetheless there is no obviously superior weighting scheme and we therefore report (unweighted) standardized treatment effects despite this problem.[24] Individual outcomes are reported separately in the main analysis, so that individual outcomes of interest can be examined separately. We believe the standardized treatment effects represent a complement to rather than substitute for these individual outcome analyses..

For a given domain (such as health care utilization), we will present two main sets of results: the estimates for each of the individual outcomes within that domain and the standardized treatment effect for all outcomes in that domain. The individual outcomes are reported due to their ease of interpretation and to give a sense of what is driving the "grouped" outcome (i.e. the standardized treatment effect); the standardized treatment effect is reported to increase power, to have one (albeit imperfect) summary measure for each domain of interest, and relatedly, to reduce the multiple inference problem.

For each individual outcome that contributes to a standardized treatment effect we report both the per-comparison p-value and the family-wise p-value adjusted for the multiple outcomes that contribute to that standardized treatment effect. This adjusted p-value is more appropriate for considering the test of the specific outcome as part of a set of tests on all the outcomes in the domain of that standardized treatment effect. Since the standardized treatment effects group outcomes that we consider conceptually similar, it seemed appropriate to us to do this multiple inference adjustment across outcomes that contribute to the same standardized treatment effect. We do not multiple inference adjust across what we view as conceptually distinct analyses, such as the impact of health insurance on health and the impact of health insurance on financial well being.

III.B.1 Standardized treatment effects:

[23] For the utilization domain where a natural weighting scheme suggests itself – namely the average cost of each type of use – we also report the standardized treatment effect weighting each outcome by our estimate of its cost to arrive at an estimated "spending" effect

[24] One suggestion was a principal components analysis, but we do not want to interpret each of the items within a group as proxies for the same, single underlying measure. Also a principle components analysis makes more sense for analysis of standardized outcomes (since the PCA gives weights to different outcomes) than for standardized treatment effects.

We define and compute the average standardized treatment effect in domain J as follows:

$$\sum_{j \in J} \frac{1}{J} \frac{\xi_{1j}}{\sigma_j} \tag{4}$$

where σ_j is the standard deviation of y_j in the control group and ξ_{1j} is the coefficient of interest for outcome j. (Specifically, for the reduced form, the ξ_{1j}'s correspond to the β_{1j}'s in equation (1) and for the 2SLS the ξ_{1j}'s correspond to the π_{1j}'s in equation (2)).

In order to account for covariance in the estimates of $\frac{\xi_{1j}}{\sigma_j}$ we estimate pooled OLS for all outcomes $j \in J$.

Specifically, we compute the average standardized treatment effect for the reduced form as follows. We estimate via pooled OLS:

$$Y = (I_j \otimes W)\theta + \varepsilon \tag{5}$$

where W is defined as in (1), I_j is a JxJ identity matrix and $Y = (y_1', \ldots . y_J')$ compute the average standardized treatment effect by simply averaging of the β_{1j}'s estimated in equation (5) and normalized by σ_j (see equation 4). We use the weights as specified above and again cluster on the household level. Note that the sample size, weights, and covariates included in W will vary depending on the data source as described in the text above.[25]

In the same manner, we can estimate the average standardized treatment effect for the 2SLS estimates by estimating, by pooled IV (using LOTTERY as an instrument for INSURANCE):

$$Y = (I_j \otimes G)\Pi + \nu \tag{6}$$

where G is defined as in (2), I_j is a JxJ identity matrix and $Y = (y_1', \ldots . y_J')$ and then once again computing the standardized treatment effects based on the coefficients from equation (6) (equivalently equation 2) and the formula given in equation (4).

Finally, we note that both the reduced version (equation 5) and the 2SLS version (equation 6) of the standardized treatment effects estimators treat the standard deviation of the outcome for the control group (i.e. σ_j) as known. We discuss in Appendix 3 how we could investigate the sensitivity of our results to this assumption, which we will do (and if they are sensitive presumably the baseline should account for the fact that sigma is not known).

[25] The point estimates obtained from equation (5) are identical to those obtained from the single-equation estimate (1). (In fact, we use this as a programming check!) The standard error and p-values for this standardized treatment effect is based on equation (5), and calculated using the "lincom" command in Stata for equation (4).

III. B.2. Adjusting for Multiple inference within a domain

To account for the multiple inference problem within a domain we will compute and report the family-wise p values within each domain. Since we are looking at multiple outcome measures within a domain, the per-comparison p-values will be lower than when each outcome is viewed as part of a "family of hypotheses" that health insurance has no effect on this domain. We will therefore also calculate and report the family-wise error rate adjusted p values. This p value corresponds to the probability of rejecting the null hypothesis of no effect on a given outcome under the null family of hypotheses of no effect on any outcomes in this domain. We will calculate these family-wise error rate adjusted p values based on 10,000 iterations of the free step-down resampling method of Westfall and Young (1993). This is more powerful than a standard Bonferroni correction because it does not assume independence across the outcomes within a domain and sequentially removes hypotheses from the family after they are rejected; see Kling and Liebman (2004) or Anderson (2008) for more detailed discussions as well as applications.

Section IV. Preliminaries / Initial analysis: study population; verification of randomization and first stage

IV.A The study population

Drawing on several data sources, we can form a picture of our study population of individuals who signed up for the lottery. Table I2, column 1 provides some basic, pre-randomization demographic information from the lottery list. (We defer a discussion of the remaining columns until Section IV.B). Table I2 indicates that our study population is 55 percent female and has an average age of about **42** at the end of our study period; the average age at sign up was about **40. 93** percent have a preferred language of English.

Table I3 provides additional selected descriptive statistics on some characteristics of interest for our study population from the initial survey conducted approximately concurrently with the lottery draws (see Appendix 1 for more detail). Since these outcomes were not collected pre-randomization we report them only for the control group. Moreover, since the outcomes are available only for those who responded to our survey.

As shown in more detail in Appendix Table A2, compared to the full sample, our responders are disproportionally female (**58** percent female), and disproportionately older (by about **2 years** on average), so the characteristics of control responders shown in Table I3 may also be somewhat non representative of the entire study population. Still, in a broad sense they provide a useful picture of our study population. They indicate that the population is **4%** Black, and **10%** Hispanic. Almost **one fifth** has less than a high school education, and another **half** have only a high school diploma or GED. At the time of the lottery **half** reported not currently working, and another **10 percent** worked less than 20 hours per week. Most strikingly, they are in quite poor health: **12 percent** report having ever been diagnosed with diabetes, **17 percent** with asthma, **29**

percent with high blood pressure, and **44 percent** with depression.[26] The last two panels show the distribution of household income (relative to the federal poverty line) and initial insurance coverage. Both are important for our first stage. About **44 percent** are below 50 percent of the federal poverty line (FPL) and about **71 percent** are below 100 percent of the federal poverty line; **10 percent** report being over 150 percent of the federal poverty line. This suggests that income eligibility requirements may disqualify some selected individuals, as indeed analysis of application data found to be the case (Allen et al., 2010). Finally, about one quarter of the controls report being on insurance (despite the fact that the list was supposed to be for uninsured individuals only); **9 percent** report having private insurance.

IV.B. Verification of randomization

As previously discussed (and see Appendix 1) we verified via computer simulation that the state did indeed randomly select households in the manner described. Another way to verify the randomization is to compare treatment and control differences in pre randomization characteristics. Table I2 reports the results. Specifically, we examine the balance between treatment and controls in the pre-randomization variables taken from the lottery list (collected in January and February 2008); these were the variables we originally used in Spring and Summer of 2008 to verify that the state had indeed randomly selected households (on the full lottery sample, before we subsequently made exclusions). Appendix 1 provides more detail on these variables.

Table I2 reports balance results for treatment vs. control individuals for 3 different samples. Column (2) reports results for the sample universe, which is the sample we analyze in the hospital discharge data and the mortality data; Column (3) reports the results for our credit subsample. Column (4) shows the results for the subsample of survey respondents. Panel A looks at differences in match / response rates for treatment vs controls for the credit subsample and the survey subsample. Panel B looks at differences in covariates between treatment and control within each subsample.[27]

A priori we were most concerned about the potential for imbalance in the subsample of survey respondents, given the high non response rate. We were not particularly concerned about likely imbalance in the full sample or the credit report sample (with an effective response rate of about 97 percent).

[26] These numbers probably understate the level of poor health in this population since under-diagnosis may be a real problem in a low income population with limited access to care.

[27] Table I2 examines the "balance of covariates" for the common set of lottery list covariates. In addition we examined the balance of treatment and controls on the dependent variables (i.e. "outcomes") we could observe prior to randomization. Specifically, in the hospital discharge sample and in the credit report data we can construct pre-randomization measures of the key dependent variables; for the survey data we used the hospital and credit report data to construct a few pre-randomization measures that are similar to some of the constructs measured in the survey data. These results are shown in Appendix 4 (Appendix Tables A14 through A16). We also examined balance (using the covariate list in Table I2) on the subsample that we drew to survey ("survey subsample") as a check on our own random drawing; the F-stat was **1.07** with p-value **.38.**

Column 2 shows that covariates are (not surprisingly) balanced in the full sample; presumably any differences here would be due to chance differences across treatment and control, unless our sample exclusion criteria described above inadvertently introduced differences. The overall F-stat has a p-value of **0.08**.

Column 3 shows the results for the credit sample. The overall match rate of our sample to the credit data (matching in September 2009) is **0.66** and is balanced across treatment and controls (panel A); the difference in match rate is a statistically and economically insignificant **0.004** percentage points (standard error = **0.004**). This is not surprising given that the match to the credit data was done using pre-randomization information from the lottery list (on name, address and date of birth) and matching to pre-randomization credit data (February 2008). Conditional on being matched to the pre-lottery credit report data, we followed 97 percent of the sample through to the post-lottery credit report data. Therefore we also did not expect any differences in pre-randomization characteristics between treatment and control in the credit report subsample whom we have outcome data on. This is confirmed in Panel B, where the F-Statistic for the credit report sample is **0.68**.

The analysis of the survey respondents is given in column (4). Given the 50 percent response rate, this is the sample where a priori we are most concerned about differential capturing of treatments and controls. By far the most important threat to the validity of our survey analysis is potential non response bias resulting from potential differences between treatment responders and control responders. The key concern is the validity of the identifying assumption that absent the lottery, treatment responders compared to control responders would have experienced similar outcomes in our study period. By construction, this assumption holds for the universe of treatment individuals compared to the universe of control individuals; the concern is whether it holds for the **50%** subsample who responded.

To examine response rate differences between treatment and control, we estimate equation (1) using on the left hand side a dummy for "did you respond to this survey". These results are shown in Panel A, column (4). Preliminary results suggest a **1.6** percentage point (standard error = **0.007**) difference in response rates.[28] This is comparable to, for example, the 1 to 3 percentage point difference in response rates between treatment and control that Angrist et al (2002) find off a similar response rate base (54%).[29] The RAND Health Insurance experiment experienced larger differences in response rates by treatment status (i.e. type of insurance plan).

[28] We also compared differences in mean response time between treatments and controls. The mean response time in the control group was **53 days (standard deviation: 58 days)**. We did the comparison by estimating equation (1) with response time as the dependent variable, including household size fixed effects, survey wave fixed effects and the interaction of the two, using survey weights and clustering standard errors on household. The coefficient on LOTTERY was **1.54 (standard error: 1.10**), suggesting no difference between treatment and controls.

[29] Note that if we are willing to assume a monotonic effect of treatment on response rates, no significant difference between treatment and control response rates would be sufficient to rule out non response bias (Lee 2002; Angrist 1997). Presumably with our relatively small difference in response rates, under this assumption we could develop reasonably tight bounds on the response rate bias. However it does not strike as us obvious that the "monotone treatment response" assumption is a reasonable assumption in our setting. We could potentially investigate the likely validity of this assumption a bit by looking at whether the effect of treatment on response rates is the same sign for every X but it's not clear this type of approach is very convincing. Therefore for now at least we have opted not to pursue it.

Rates of refusal and rates of attrition were both higher for those randomized into less generous plans. Rates of refusal ranged from 8% to 25% across treatment options; rates of attrition over the course of the study ranged from 5% to 15%. Thus total "response rates" varied by almost 24 percentage points, from 87 percent for those randomized into the most generous plans to 63.4 percent for those randomized into the least generous plans (Newhouse et al, 1993 p. 18-19)

Panel B, column (4) shows the difference in lottery list covariates between treatment and control responders. The overall F-stat (bottom row) has a p-value of **0.84.** Thus the results indicate that – to the extent we can examine it with our albeit somewhat limited set of pre-randomization covariates – the treatment and control respondents look similar. This is reassuring from the perspective of potential non-response bias in our survey data arising from difference between treatment responders and control responders.

Table I2 considered whether there is balance between treatment and control individuals in the full sample, the credit sub-sample and the sub-sample of survey respondents. This balance is necessary for the comparisons done in those data to be internally valid. In Appendix Table A2, we examine whether the credit sub-sample and sub-sample of survey respondents differ from the full sample. We note that survey respondents (i.e. treatment and control combined) overall differ substantially (and in unsurprising ways) on observables from survey non respondents; for example, responders tend to be considerably older (**4** years older on average) and disproportionately female (**59** percent female compared to **53** percent for non-responders). Similarly, those who matched to the credit data differ from those who did not match. This, of course, speaks to the generalizability of our findings in light of potential heterogeneous treatment effects, an issue we explore in more detail in table P4.

IV.C. First stage

Our first stage analysis is based on estimating equation (3). For our first stage, we must decide how to measure insurance ("INSURANCE") which is the first stage dependent variable in equation (3) and the endogenous right hand side variable in equation (2).

There are two main conceptual issues: (1) the definition (scope) of "insurance" and (2) the time period over which it is measured. We discuss and explore each in turn. We note at the outset that many of the choices are not obvious and involve judgment calls. To the extent that we mismeasure the first stage, we will produce 2SLS estimates with misleading magnitudes (but not signs) and this will affect interpretation. As a result, in the first stage tables below we present (and discuss) a range of plausible estimates; interested readers can rescale the reduced form estimates using their preferred first stage estimates to produce their preferred 2SLS estimates.

Table I4 shows the results of our first stage estimates. For all of this analysis we report results for three different samples: the entire list (our HDD and Mortality sample), the credit report random subsample, and the set of survey respondents. We expect the first stage to be the same (or very similar) for the HDD and credit report samples, unless having a credit report (or being found in the credit report data) is somehow correlated with insurance take up. We expect the first stage to potentially differ for respondents relative to the overall population since, as just noted, respondents and non respondents differ in many ways.

Baseline first stage estimates

The first row of Table I4 shows our preferred (and baseline going forward) measure of insurance, which is whether the individual was ever on Medicaid during our study period. We measure whether the individual is on Medicaid using the state's Medicaid enrollment files; Medicaid includes both OHP Standard and OHP Plus. We define our "study period" as follows: For the HDD and credit report samples (columns 1 through 4) we define the start of the time period as the notification date *n* (which varies by lottery draw). In the survey data (columns 5 and 6) we define the start of the study period as the first notification date (i.e. March 10, 2008).[30] For all samples, we define the end of the time period as September 30, 2009.[31] This is the end date for our hospital discharge data and credit report data, and close to the average survey response date which is September 23, 2009.[32]

The results in the first row of Table I4 indicate a first stage of **0.25** for both the full sample (column 2) and the credit report subsample (column 4), and a first-stage of **0.27** for survey respondents (Column 6); all of these first stages have F-statistics above **400**. As expected, the first stage is similar in the full sample (column 2) and the credit report sample (column 4). However, the estimated first stage is considerably higher for survey respondents than non respondents (**0.21 (**standard error = **0.0009),** not shown). This is not surprising as survey respondents tend to be more "together" on a number of dimensions.

We note that our first stage is considerably less the one. This reflects the low take-up of OHP Standard among those selected by the lottery. As we will show in Table I5 below, another potential contributing factor to a low first stage– crowd out of private insurance coverage among winning individuals – turns out in practice to not be substantively important, which is consistent with the eligibility requirement that individuals be uninsured for six months.

Alternative first stage estimates

The remaining rows of Table I4 explore the first stage under three alternative definitions of insurance. These vary the time period over which we define insurance, and which types of insurance we include.

The first alternative definition is the "# of months on Medicaid" during the study period. The results suggest an average difference between treatment and control of **2.7** months of Medicaid coverage over the study period. For some outcomes it might be reasonable to think of the impact

[30] We use a common start date across survey waves since within survey wave the notification date can vary among the ultimately treated.

[31] We received Medicaid administrative data files monthly through August 2009 and then weekly. For our insurance measurement, we use the first Medicaid file after the start date through the first Medicaid file after September 30 2009– as well as all intervening files.

[32] Some survey responses occurred well after this average response date, in large part because of the intensive follow-up timing. Specifically about **30** percent of our weighted responses are after the end of September 2009. Defining the study period differently for individuals based on their response date makes no difference for our estimated first stage estimates (see Tables I4 and I5 and discussion below).

of health insurance as linear in "number of months", however if there is pent-up demand in an uninsured population the effect might not be linear.

The second alternative definition is whether the individual is on Medicaid at the end of our study period; this can be thought of as providing a lower bound on the first stage (and hence an upper bound on the treatment effect), while our preferred measure ("ever on Medicaid" at the end of our study period) provides an upper bound on the first stage (and hence a lower bound on the treatment effect). The results suggest an average difference in insurance coverage at the end of our study period of **0.13**, compared to **0.25** when insurance is measured at any point over our study period. Over time churning eroded the difference in insurance between treatment and control individuals as enrolled individuals had to recertify their eligibility every 6 months, and control individuals could also find other insurance. This can be seen in more detail in Figure I2 which shows the time path (relative to the notification date) of whether the individual is on Medicaid (and whether the individual is on OHP Standard) separately for treatment and control individuals.

The third alternative definition is whether the individual is ever on OHP Standard during the study period. The results suggest that the estimated first stage for OHP Standard (last row) and Any Medicaid (first row) are indistinguishable, which is to be expected given that the program created eligibility for OHP Standard.

Other sources of insurance

Table I5 explores alternative definitions of "insurance" in the self-reported survey data, and compares the self-reported survey data to the state Medicaid enrollment data.[33] Specifically, we look at measures of OHP Standard, any Medicaid (OHP Standard or Plus), private insurance and any insurance. The main finding is that , among the survey respondents – for whom we can measure insurance more broadly than just Medicaid – there is little difference between the first stage estimated using self reported Medicaid (**0.20)** and self reported "any insurance (0.**18**). This is consistent with the low (**9%**) private insurance coverage in the initial survey (see Table I3) and no evidence of crowd out of private insurance in the first stage estimates in Table I5 (point estimate = **-0.009**). It also suggests that our baseline first stage measure of the impact of the lottery on Medicaid coverage — which has the advantage that it can be measured for all individuals and not just survey respondents –gives an accurate picture of its impact on any insurance coverage. Finally, a comparison of our estimated first stage defined based on whether "on Medicaid currently"(row 1, Table I5) and "on Medicaid as of end of September 2009" (third row of Table I4), indicates that (as noted in the earlier footnote) how we define the end of our study period for survey responders does not matter for the estimated first stage (the first stages are 0.178 and 0.173 respectively).

Who are the compliers?

Our 2SLS estimates will identify the impact of insurance coverage for complier individuals. While in general compliers are not identifiable in the data, the particular features of our setting

[33] To enhance comparability across the state Medicaid files and the survey self reports of insurance, we identify for each survey respondent, we identify the administrative data file immediately following the survey response date. We measure "current" insurance in that file, and we measure "# of months with Any Medicaid (last six months) in the six months prior to that file.

suggest that we may be reasonably able to approximate the "compliers" relative to the "non compliers" (always takers and never takers) in our treated (winning) population. In particular, individuals who are selected by the lottery and are on OHP Standard are very likely to be compliers since there is (in principle) almost no way to get OHP Standard without winning the lottery (and hence there should be no "always takers" on OHP Standard).[34] Moreover we know from conversations with the state that any individual selected by the lottery who applied for OHP Standard was first screened for eligibility into the more generous OHP Plus; therefore selected individuals who are on OHP Standard would not be on OHP Plus if they had not been selected. We therefore define our "approximate compliers" as those who were selected and on OHP Standard at any point over our study period, and our "approximate non compliers" as those who were selected and are not on OHP Standard at any point over our study period. This is approximate both because some of those selected who are on OHP Plus may in fact be compliers (since the state reviewed each application for selected individuals for potential OHP Plus eligibility before examining potential OHP Standard eligibility) and because some of those on OHP Standard may in fact be "always takers" (since a small number of controls obtained OHP Standard).[35] These results are shown in Table I6.[36]

Section V. Primary analyses: Health care utilization, financial well-being and health

We lay out our planned primary analyses. Appendix 2 provides more detail on variable and sample definitions. It also provides the underlying distribution of categorical variables (and some continuous ones) which informed various dichotimization choices.

The primary analysis tables all follow the same structure. As noted, we report standardized treatment effects within each domain. Our "bottom line" is shown on the bottom line, and pools the survey and administrative data within domain. However we also report standardized treatment effects separately for survey and administrative data, given the important differences between these two data sources in what constructs they measure, their sample universe, their look back period, and their measurement mechanism (i.e. administrative records vs. self-reports).

V.A. Impact on Health Care Utilization.

[34] We are able to investigate this assumption by looking at the fraction of our controls that are ever on OHP Standard during our study period; it is about **2%**. There are some limited ways to obtain OHP Standard without winning the lottery—for example, pregnant women who are on OHP Plus can sometimes stay on OHP Standard after giving birth—but this may also reflect some measurement error in the administrative data. (We tried to purge individuals on OHP in January 2008 using Medicaid enrollment data but measurement error there – or subsequently – could account for a tiny fraction being on OHP Standard in our study period).

[35] Absent these two sources of (presumably small) slippage, if we defined "INSURANCE" in equations (2) and (3) by "ever on OHP Standard" the 2SLS estimates of equation (2) would identify the "treatment on the treated" (or the average treatment effect) rather than just the local average treatment effect on compliers. Table I4 shows that the estimated first stage (equation 3) is in practice quite similar whether we use "Ever on OHP Standard" or "ever on Medicaid" to define INSURANCE.

[36] There is a more general method for estimating the characteristics of the compliers in settings where they are not identified in the data (Abadie, 2002). We will explore using this estimation to see how well it matches our estimation based on the "approximate" compliers.

We hypothesize that health insurance increases health care utilization, but note that this is ex ante not obvious (if e.g. the offset effect exists and is strong enough, or health improves substantially as a result of health insurance).

Table P1 reports the results for the impact of health insurance on health care utilization. The left hand most column describes the outcome of analysis. Column 1 reports the control mean, column 2 the reduced form estimate of equation (1), column 3 the 2SLS estimate of equation (2). For individual outcomes we report both the per comparison p value and the family-wise p value across the individual outcomes within domain.

In the hospital discharge data we are able to 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,[37] and (3) number of procedures performed; these measures sum across multiple admissions for a given patient during the time window.

In the survey we asked about four different types of health care utilization: (1) the number of drugs you currently take, (2) the number of outpatient visits over the last six months, (3) the number of ED visits over the last six months and (4) the number of overnight hospital stays over the last six months (excluding those for childbirth).

We report two standardized treatment effects for the survey data. One weights each of these measures equally. The other attempts to estimate the impact on total spending by weighting each type of total health care use by an estimate of the average cost of each use.[38] While "spending" is the most interesting summary measure it also involves the most assumptions since it requires that we weight each type of care by average costs per use; these weights may change based on health insurance (since health insurance may affect intensity of use) and our analysis would not capture any such changes in the weights.

[37] We note that 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 not mechanically affected by insurance coverage (Doyle, 2005). However, a concern is that the relationship between these "sticker prices" and actual utilization may vary across hospitals, so that any effect of insurance on hospital sorting could potentially contaminate the analysis. The tables as presented here do not adjust for hospital fixed effects. If there were evidence of sorting across hospitals (tested by a global f-test), we would also consider the sensitivity of these results to including hospital fixed effects.

[38] We operationalize the weights as follows: we select all noneldery (19-64 year old) adults below 100% of poverty who are publicly insured in the 2002-2007 Medical Expenditure Panel Survey. This gives us a total sample of over 7,500 individuals. We use their expenditures (all inflated with the CPI-U to 2007 dollars) to calculate average expenditures per outpatient visit, average expenditures per ED visit, average expenditures per inpatient day (for visits not related to childbirth), and average semi-annual (six month) spending per prescription drug. All spending is total expenditures (i.e. not just insured) expenditures. Setting the average spending of an outpatient visit to 1, the resultant relative average costs are: 2.9 for ED visits, 50.3 per inpatient visit, and 1.05 for six-month drug expenditure per drug. The underlying costs are $150 per outpatient visit, $435 per ED visit, $7,523 per inpatient visit, and $156 per six month expenditure per current prescription drug. (The relative average costs are very similar for the uninsured population (not shown)).

The bottom row reports the "bottom line" standardized treatment effect pooled across the administrative and survey data. We wanted to average across one measure of each type of care but have four different measures of hospital use (one in the survey and three in the discharge data). For the standardized treatment effect we use the "number of hospital days" as measured in the discharge data.

V.B. Impact on Financial Well-Being

We hypothesize that health insurance should reduce financial strain and hence the occurrence and magnitude of adverse financial events. However, given the very low income and assets of our study population, it is possible that access to charity and uncompensated care among the uninsured substantially mutes the impact of health insurance on financial well-being in this population. Table P2 reports the results; the format of the table is analogous to P1.

Credit report measures

The top panel analyzes the impact of health insurance on financial strain in the credit report data. We examine five measures of the occurrence of an "adverse financial event" between the notification date and September 2009, which represents an average time horizon of **16 months**. All are events that are likely have a major negative impact on one's access to credit, at least in a general population (Avery et al 2003).[39] The five measures are: (1) any bankruptcy, (2) any lien, (3) any judgment, (4) any collection, (5) and any delinquency on a credit account (i.e. any credit account with payment at least 30 days late), (We discuss in Section VII below another potential measure of financial strain based on the balances owed on revolving credit accounts).

Broadly speaking, all of these measures are measures of unpaid bills (outstanding obligations). Measures like bankruptcies, judgments, liens and collections are likely to be relatively large unpaid bills, given the fixed costs of attempting to collect, or of seeking a judgment or a lien. Delinquencies on credit accounts may be on revolving credit or on non revolving credit (i.e. mortgages or installments); these variables are mechanically zero for the approximately **one quarter** of our sample who have no open trade lines of any kind over our study period.[40] As can be seen in the first column of table P2, the relative frequency of these adverse financial events varies greatly in our population; over the **16 month** analysis window, about **2 percent** of the control group had a bankruptcy or lien, and about **6 percent** had a judgment, while about **40 percent** had a collection and about **one third** had a delinquency on a credit account.

[39] Avery et al (2003) note that "Perhaps the most important factors considered in credit evaluation are a consumer's history of repaying loans and any evidence of money-related public actions or non-credit-related collections" (page 60-61).

[40] There will be some overlap in the unpaid bills the different measures count. 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). Naturally, major delinquencies are a subset of any delinquency. 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 the delinquency measures. However the individual measures of conceptually distinct and independently of interest which is why we tolerate the overlap (in some cases there is also no way to undo it). The multiple-inference adjustment accounts for the correlation structure of the outcomes.

For collections (but not other unpaid bills) we can distinguish between medical and non medical collections (and analyze them separately in the supporting analysis in Section VII below). In our population (and in general populations),medical collections are the single most common form of collection; using data from general populations with more detail, Avery et al (2003) note that after medical collections, the next most common form of collections are utilities. Over our sample period, about **29 percent** have a medical collection and about **39 percent** have a non medical collection.

A few specific caveats and comments are in order about the credit report data:

Selective reporting of collections: A potential concern with collections data is that – unlike the bankruptcies, judgments and liens – not all collections are reported to the credit bureaus. In Appendix 2 we discuss in more detail the potential concern this raises with non random sorting of individuals by insurance status across collection agencies; in general we consider this only plausible for medical collections (not non medical collections) and potentially difficult even for medical collections. We describe in the appendix some tests we will do to examine this issue, primarily by examining whether in fact health insurance is associated with changes in the distribution of hospitals used. Depending on the results, we might potentially want to exclude collections from the main analysis.

Incidence: We note that the incidence of a decline in any unpaid bills may be on providers of the underlying goods and services, rather than on the individual consumer. For example, in our population, only about **3 percent** of collections are paid.[41] About one quarter of judgments are eventually paid. This suggests that the providers (e.g. hospitals, utilities companies etc). would be direct beneficiaries of any decreases in collections. Of course the consumer may benefit too, even if the collection or judgment would have been unpaid, since these can negatively impact one's credit score.

Time horizon: While our analysis of the "one year" effects of the lottery is based on analyzing data through September 2009, we note that many of the outcomes we are looking at reflect financial strain with a lag. For example, discussions with hospitals in Oregon suggest that it will take about 3 to 4 months before an unpaid hospital bill is sent to a collection agency. Similarly it will take time to decide to seek a judgment and then to win a judgment. (Other variables, like late payments on trade lines may show up sooner, i.e. within 30 days). Therefore we may not fully capture the financial strain over the entire time period.

Survey measures

The bottom panel of Table P2 analyzes the impact of health insurance on financial strain in the survey data. We analyze six summary measures of financial strain. Four measures are binary (1) whether you have any out of pocket medical expenditures in the last six months, (2) whether you currently owe money for medical expenses, (3) whether you have had to borrow money (or skip

[41] 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.

paying other bills) to pay medical expenses in the last six months, and (4) whether you have been refused medical treatment because of medical debt in the last six months. The other two measures are the *amount* of out of pocket medical expenses in the last six months and the *amount* of medical debt currently owed.

Differences between credit data vs. survey data

The credit report data (like the hospital discharge data) differ from the survey data in their look back horizon, the sample universe (random subsample vs. self-selected responders), and in their modality of data collection (administrative vs self report). In general, all of these differences point to a preference for the administrative data, if it can capture the same construct as the survey data (e.g. hospital days). However, the credit report data have an important limitation in our context, which may increase the relative value of the survey data, even when they are attempting to measure similar things (e.g. medical collections in the credit report data and "currently owe money for medical expenses" in the survey data). Specifically, while the credit bureau data are extremely comprehensive in measuring most formal credit, for our low income population they may do a fair amount of borrowing from non traditional sources – such as pawn shops and family members - -that would not show up on credit reports. The measures are therefore not as broad as our survey measures.

Finally, a general limitation of all the analysis of the impact of health insurance on financial well being in table P2 is it focuses on mean outcomes of amounts owed (or the admittedly arbitrary cutoff of "owe anything"). Particularly for financial well being, the tails of the distribution may be much more important than the means. In our supporting analysis of financial strain in Section VII below we examine the impact of health insurance on other moments of the debt distribution.

V.C. Impact on Health Insurance on Health

We hypothesize that health insurance may improve health. Table P3 summarizes the results. We have one measure of health from administrative data: mortality, which we measure from the notification date through September 30th 2009.

We construct eight different measures of health status in the survey data. The first two use the question about self report health (fair, poor, good, very good, or excellent) to construct two binary measures: (1) self reported health fair or poor (45 percent of the population) and, to examine "tail" behavior, (2) self reported health poor (14 percent of the population). The other measures are: (3) whether you health status has gotten worse over last six months (vs stayed the same or gotten worse), (4) the number of days impaired by physical or mental health in the last month (0-30), (5) the number of days not in good physical health in last month (0-30), (6) the number of days not in good mental health in the last month (0-30), and (7) whether you screened positive for depression Many of these measures capture both physical and mental health; the last two however capture only mental health.

VD. Heterogeneous treatment effects

Our results are obviously very specific to our specific population. In thinking about potentially extrapolating to other settings, it is useful to examine whether treatment effects vary across

observable characteristics. We explore potential heterogeneity in treatment effects for each of our three primary domains. Table P4 reports the results. We report results for standardized treatment effects only. Since the first stage may differ across groups, we focus on the 2SLS estimates of equation 2 (rather than the reduced form estimates of equation 1). For each group, we report the first stage, and the pooled standardized treatment effect (i.e. the bottom line of Tables P1 through P3) for different groups. The first row replicates the results for the full sample.

We examine results by various demographics: gender, age, race and urbanicity. Specifically we compare 2SLS estimates for individuals aged 19-49 to those aged 50-63 (based on age at the end of 2008); the latter represent **about one third** of our sample. For those in the survey respondent subsample, we also compare the 2SLS estimates for individuals reporting white race (**83**% of our sample) to those reporting any non-white race. We also compare the results for individuals living in urban or suburban zip codes compared to those in rural settings (defined using Census Metropolitan Statistical Areas).

We also examine results by measures of SES. We would like to examine heterogeneity in treatment effects by initial income (although of course everyone in our study population is quite poor so income variation is presumably limited). Unfortunately, we have not yet been able to link to administrative data that would give us a measure of pre-randomization income. However, we have two other measures of SES. First, using the credit report data from February 2008 (prior to randomization) we can distinguish between those with some vs. effectively no access to mainstream credit; specifically we divide the credit report subsample into the approximately **55%** of the sample who has some revolving credit in February 2008 (the "have prior credit" subsample) and the approximately **45%** who do not ("do not have prior credit"). Second, using the survey respondent subsample, we split the sample by self-reported education. Specifically, we compare the approximately **70%** who have a high school education or less with the approximately **30%** who report more than a high school education. We suspect that education is (relatively) immutable and not responsive to insurance in our population but we will examine this directly and remove the analysis if that appears to not be the case.

We would like to examine heterogeneity of treatment effects by initial health status, but this requires measures of pre randomization health. We can however analyze the effects in the survey respondent subsample by whether they report "ever smoking" (approximately **two thirds)** or not. Smoking is both a direct contributor to poor health and correlated with measures of poor health. Of course it is possible that insurance coverage may affect whether or not you "ever smoked"; we consider this unlikely but will examine it directly and remove the analysis if that appears to be the case. Age may be a proxy for health (since older people are in worse health) although of course it captures other things.

Finally, we are interested in examining potential heterogeneity in our estimates by the time since insurance coverage. We are particularly interested in whether the short run impact on utilization is larger than longer run impacts (suggesting pent up demand), as well as whether the impact on health and financial well being increases over time. However the time frame of the current data does not seem of great interest. Average time since approval ranges only from 10 to 17 months by survey draw; while there is a greater potential time range in the hospital utilization data we worry about power to detect differences in effects in this data over the available time range (and

hospitalization is not exactly where the pent up demand effect seems most likely). For now we have deferred any specific planned analysis here, but if we appear to have reasonable power we may well revisit this issue, perhaps by making use of the earlier, substantially smaller survey conducted at approximately 6 months after the lottery (see Appendix 1 for details)..

VE. Sensitivity analysis

V.E.1 Sensitivity to covariates

Our baseline specification (which is what we have shown thus far) does not include the (pre-randomization) lottery list covariates (defined in e.g. Table I2) as regressors. However, for the administrative data, it does include controls for pre-period y and lottery draw. Table S1 investigates the sensitivity of our standardized treatment effects in our three primary domains to these choices. Specifically, we report three sets of results: our baseline specification (columns 1 and 4 for the reduced form and 2SLS respectively), a specification without controlling for pre-period y and lottery draw in the administrative data (columns 2 and 5 respectively), and our baseline specification augmented to control for the lottery list covariates (columns 3 and 6, respectively).

A priori, we were more concerned about sensitivity of our survey results given the potential for non response bias. However for completeness, we report the sensitivity of all our primary standardized treatment effects, including those in just the survey data, just the administrative data, and the "pooled" bottom line estimates.

Note: If results are sensitive to inclusion of controls we will change protocol to make inclusion of covariates the baseline specification (vs current plan) We may also want to think about exactly how to include the covariates (e.g. propensity scores etc).

V.E.2. Contamination of credit report data via access to credit margin

If health insurance improves access to credit, we have to exercise caution in interpreting a decrease in adverse financial events as evidence of decreased financial strain in the credit report data; there could be perverse results, for example, whereby an improvement in the market's assessment of an individual's credit worthiness encourages plaintiff's 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" in the credit report data (since the possibility of the adverse financial events increases). Any such effect would have to be indirect, since whether one has health insurance is not a matter of public record, not is it information that credit bureaus collect or that enters algorithms for credit scores. We therefore considered it a priori not very likely, but still very important to examine since it could contaminate our interpretation of the credit report data. [42]

[42] 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 negatively impact one's credit score (and hence

Therefore, after specifying the analysis plan but before "breaking the code" on the main results, we investigated this issue to decide whether or not to include the credit report data in the primary analysis. Specifically, we examined whether health insurance was associated with increased access to credit in the credit report data.

We have three measures of access to credit: (1) Do you have a credit score?[43] (2) Do you currently have a thick file (defined as having two or more open trade lines of any kind, including revolving credit or installment loans)[44], and (3) total current credit limit across all 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). 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 particularly concerned about potential dynamic patterns in credit access. For example, health insurance might initially improve one's access to credit, which could in turn generate an accumulation of unpaid debt and hence ultimately 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" activities (like pawn shops or family members) to "on the books" borrowing; any delinquency then would be "on the books" and could therefore worsen one's perceived credit worthiness. For the substantive purpose of understanding the impact of health insurance on access to credit in the first year, we measure access to credit as of the end of our study period (September 2009); this analysis is discussed in the supporting analysis of the impact of health insurance on financial well being in Section VII. However for interpreting our estimates of our primary analysis of the impact of health insurance on adverse financial events in the credit report data we want to know if health insurance *ever* improved access to credit over our study period; measuring credit access only at the end point could miss the "over-shooting" dynamic whereby access to credit first rose and then fell. Therefore we here we analyze access to credit defined by the "maximum access to credit" over our study period (i.e. notification date through September 2009). To measure "maximum access

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access to credit). 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 "off the books" loans (e.g. pawn brokers, relatives etc) to "on the books" formal credit loans.

[43] About **80 percent** of our sample has a credit score. We cannot analyze credit scores for this population because we do not know how to treat those without credit scores. Those without credit scores do not necessarily have a worse "latent" credit score 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.

[44] 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).

to credit" we compute the maximum of each credit measure over the February 2009 and September 2009 archives.

The results, summarized in Table S2, suggest that health insurance is not in fact associated with an increase in access to credit for this population over our time horizon.

Finally, we note that if adverse financial events decline in a substantively important way (e.g. results in P2 suggest substantial declines in adverse financial events), this should ultimately translate into improved access to credit. However this may not show up during our time horizon.

Section VI: Supporting analyses: Impact of health insurance on utilization

In this section we explore the impact of health insurance on health care utilization in some more detail. This exploration should enhance our primary analysis of the impact of health insurance on utilization in table P1. We conduct two main types of supporting analyses. First, we examine responsiveness on the extensive margin (any use). We do this both because it is interesting to try to decompose any utilization response between the extensive and intensive margin (e.g. going to the doctor more vs. treated more intensively once you are there) and because there may be less measurement error on this margin (e.g. are you currently taking any drugs vs. how many different drugs are you currently taking?) so that we may have power to detect responses on the extensive margin but not on the "total" margin even if there is no offsetting decline in intensity. Second, we use the richness of the hospital discharge data to conduct more in-depth analysis of the impact of health insurance on various dimensions including the type of admission, the intensity of treatment conditional on being admitted, the quality of care (both in and out of the hospital), and the type of hospital used.

VI.A Impact of health insurance on utilization: Extensive margin.

Table U1 shows the results for use on the extensive margin; it has the same format at the primary analysis tables. The top panel shows the results for "any hospital admission" over our study period in the hospital discharge data. The bottom panel shows the results in the survey data for whether you are currently taking prescription drugs, used any outpatient care in the last six months, had an ED visit in the last six months, or any hospital visit in the last six months. Once again we report standardized treatment effects separately for each data as well as the "pooled" standardized treatment effect which uses the survey data on drug use, outpatient care use, and ED visit and using the hospital discharge data measure for any hospital use.

VI.B: More detailed examination of utilization patterns in the hospital discharge data

The hospital discharge data allow us to examine the impact of health insurance on inpatient hospital utilization as well as the impact of health insurance on the quality and type of care received, which may be an input into any health benefits from health insurance. Once again, more detail on variable definitions can be found in Appendix 2.

Our analysis is similar in spirit to several recent non-experimental analyses of the impact of insurance using hospital discharge data, particularly Card et al. (2008, 2009) who use a

regression-discontinuity framework to study the impact of health insurance associated with Medicare coverage at age 65, Anderson et al. (2010) who use a regression-discontinuity framework to study the impact of health insurance associated with losing parental coverage at age 19, and Kowalski and Kolstad (2010) who use a difference-in-differences framework to study the impact of the Massachusetts universal coverage expansion.

As previously noted, although hospitalizations are relatively rare, they are quite expensive. Therefore on the "cost side" of the potential impact of health insurance, studying the impact on hospital care utilization may be of first order importance. We note up front that the sign of any utilization effect is ambiguous; we might expect utilization to go up with insurance (a price effect) or down (due to improved outpatient care and/or improved health); [45] in general the literature has tended to find increases in utilization (see e.g. Card et al 2008, Anderson et al 2010).

Type of admission and treatment conditional on admission
Table U2 examines the impact of insurance on hospital utilization by type of admission. We consider three types of admission: any, not through the emergency department, and through the emergency department.[46]

Approximately **7%** of our sample has any admission during our study period; note that on average our study period (which lasts from the notification date – which varies across lottery draws – through August 31 2009) is **15 months**, so this corresponds to an annual admission rate of **6%** for our sample. Of individuals with any admission, approximately **60%** have an admission through the ED and approximately **40%** have an admission not through the ED; note that these need not sum to 100 since individuals may have more than one admission over the study period; of the **7%** of individuals who have at least 1 admission, **29%** have more than 1 admission; the mean number of admissions conditional on having more than one is **3.**

There are two motivations for analyzing outcomes separately for these different types of hospitalizations. First, we hypothesize that admissions through the ED are more severe and less discretionary, therefore they may be less price sensitive (i.e. less responsive to insurance).[47] Focusing on non-ED admissions may therefore increase our power to detect any impact of insurance on utilization. Second, we are interested in not only how overall utilization increases but also in the question of whether insurance affects the intensity or nature of treatment (e.g. number of days in hospital, procedures performed etc). In a sample in which there is a utilization

[45] Note that it is also possible that admissions through the ED may decline if insurance allows individuals to access hospitals directly, rather than through the emergency room.
[46] We considered added an analysis of "non discretionary" ED admissions classified using Card et al's (2009) methodology based on the ratio of weekend to weekday admissions. We were concerned, however, about our ability to identify "non discretionary" admissions given that some conditions that may allow for discretion on admission may not be uniformly distributed through the week, and could thereby spuriously appear non discretionary., In addition, our preliminary investigation of classifying non discretionary conditions in a population ages 19-64 identified about 10 percent of our sample, raising concerns about power. Nonetheless this approach is appealing as it potentially allows us to isolate the impact of insurance on the intensive margin among a set of individuals whose hospitalization is not affected by insurance. We may pursue alternative ways of doing this. We are grateful to Carlos Dobkin for his help providing us with code and discussing the algorithm used in Card et al (2009)..
[47] This is consistent with the findings in the literature suggesting a greater responsiveness of the non ED admission rate to insurance coverage (see e.g. Card et al. 2008, Anderson et al. 2010).

response on the extensive margin, any impact on treatment intensity conflates compositional changes in the sample that is in the hospital with any changes in treatment conditional on intensity. The hope is that if we can isolate a subset of admissions where there is no response on the extensive margin (e.g. admissions through the ED) we can study the impact of insurance on treatment intensity directly. Of course, even without a response on the extensive margin, we must be careful in interpreting any change in treatment as a pure direct insurance effect; if health insurance affects underlying health then the nature of the people being treated with and without health insurance may differ. Here, we are at least re-assured that our average admission is occurring after only **6 months** of insurance and the maximum time on insurance for someone in our sample is **18 months**.
Panel A reports the results on the extensive margin by type of admission. We expect more of a response on the extensive margin for "not through ED" than "through ED." It is possible that admissions through the ED might potentially go down with insurance coverage if insurance coverage reduces ED utilization.

Panel B reports the results for total hospital use, by type of admission. We have three measures of use: (1) number of hospital days, (2) total list charges,[48] and (3) number of procedures performed; these measures sum across multiple admissions for a given patient during the time window. These three different measures all reflect in different ways the "intensity" of treatment within the hospital. Therefore, we present a standardized treatment effect across the three measures and, for the same reason, we report family-wise p values adjusted across these three measures. In addition, given that all three of these outcomes are very right skewed (see Appendix 2) we also report (see column 4) reduced form results from a proportional regression, with is the practice of other reserachers see e.g. Anderson et al. (2010), Card et al. (2008, 2009)). Specifically, we report results based on quasi-maximum likelihood poisson (QMLE poisson)[49] At this point we are not planning to report the 2SLS analogs of these models since we view this as merely exploratory (do they give very different results?). However if the results do look substantively different, this is something we may well want to do.

The results in Panel B for "all admissions" replicate the primary analysis in Table P1. Of particular interest is the impact on utilization for types of admission for which there is no extensive margin response, so that we should be able to isolate the intensive margin (modulo potential differences in underlying health as discussed above).

[48] We note that 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 not mechanically affected by insurance coverage (Doyle, 2005). However, a concern is that the relationship between these "sticker prices" and actual utilization may vary across hospitals, so that any effect of insurance on hospital sorting could potentially contaminate the analysis. The tables as presented here do not adjust for hospital fixed effects. If there were evidence of sorting across hospitals (tested by a global f-test), we would also consider the sensitivity of these results to including hospital fixed effects.

[49] A natural alternative would be a log model but the large proportion of zeros makes this innapropriate. 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..

Utilization for selected conditions

We also examined the impact of insurance on utilization (extensive margin and total) for 7 diagnoses we thought were of interest and also of high prevalence in our population: heart disease, diabetes, skin infections, mental disorders, alcohol or substance abuse, back problems and pneumonia. These conditions are mostly groupings of multiple diagnosis codes (see Appendix 2 for details), but include the six most common clinical conditions (mood disorders, skin infection, diabetes, alcohol-related disorders, spondylosis or other back problems, pneumonia, and schizophrenia or psychoses,) which account for **28%** of all admissions.[50] The five conditions are mutually exclusive, and are coded based on primary diagnosis.

The results are summarized in Table U3. We report two utilization measures: any admission and the standardized treatment effect across the three measures of total utilization (number of days, list charges, and number of procedures). We examine these outcomes for all hospitalizations of a particular diagnosis; however, to aid in interpretation we show the proportion of each diagnosis that is "through ED" but we do not cut the diagnoses by this category. We also note that the interpretation of this analysis is not clear; once again, utilization could go down because health improves, or up because care is now less expensive.

Quality of care and hospital type

The hospital discharge data also allow us to examine the impact of insurance on quality of care received. We examine the impact of insurance on both the quality of care the individual receives and the type of hospital the individual goes to (which may relate to the quality of care received).

Table U4 shows the analysis for our four measures of quality of care the individual receives. These 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. Panel A reports our one measure for outpatient care: admissions for ambulatory-care sensitive condition. Panel B reports our three measures for inpatient and subsequent care: not having a patient-safety event in the hospital, not being readmitted within 30 days of discharge, and the average hospital quality of all admissions. As each of these three is intended to capture some aspect of inpatient quality of care, we combine them into a single domain and calculate the standardized treatment effect across all three. We also report both the per comparison and the family wise p value. We note that an important caveat with all three inpatient quality of care measures is that they are measured conditional on have a hospital admission.

The outcome "any hospital admission for an ambulatory sensitive care condition" is defined using the AHRQ Prevention Quality Indicators criteria to identify admissions suggest poor quality in outpatient care. About **13** percent of people in our sample admitted to the hospital have an admission for an ambulatory sensitive care condition, the most common ones being

[50] Our coding of "common conditions" 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 eyeballing the underlying codes and our priors on what might be interesting and prevalent in our population. An advantage of our pre-specifying this list is that the ad-hoc nature need not particularly concern us.

complications from diabetes, pneumonia and asthma. We use this as a way of inferring the quality of outpatient care. We note however that the interpretation is not obvious. In particular, if admissions for these "ambulatory sensitive care" conditions goes down, this suggests improvements in outpatient quality of care. But if we see no change (or an increase in utilization) it could 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 those with insurance.

The outcome "not having a patient safety event" is defined using the AHRQ Patient Safety Indicators criteria. These criteria are intended to identify potentially preventable adverse events or complications, such as foreign bodies being left behind during procedures, infections due to medical care, deaths in low-mortality conditions, and certain postoperative complications. About **2** percent of admitted individuals have a patient safety event. We analyze this outcome conditional on admission since one is not ``at risk'' for a patient safety event if one is not admitted; there is an obvious selection problem here but it is not solved by analyzing the unconditional outcome. One needs to exercise some caution in interpreting the analysis since insurance may affect the probability (and nature) of being in the risk set (i.e. hospital admissions). Because our analysis is conditional on admission, we do not control for the individual's outcome in the pre-randomization period.

The outcome "not readmitted in 30 days?" is also analyzed conditional on an admission (for the same reasons, and with the same caveats), and (also for the same reasons) we therefore do not control for the individual's outcome in the pre-randomization period. Approximately **12** percent of individuals admitted to the hospital have a re-admission within 30 days. 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 used and interpreted as such.

The outcome "average hospital quality" is defined using a standardized average of the Hospital Quality Alliance process-of-care measures. For individuals with multiple hospital stays we take the length-of-stay-weighted average of the hospitals. We analyze this outcome conditional on admission, since the quality of the hospital is undefined for those not admitted, and do not control for the pre-randomization period outcome. As with "have a patient safety event" and "re-admission in 30 days," this suggests caution in interpreting the analysis.

Finally, Table U5 shows our analysis of the impact of insurance on the type of hospital the individual attends. One way that health insurance may affect health is by affecting the type of hospital that one attends. We begin with a (probably low powered) agnostic examination of whether insurance is associated with any change in the distribution of hospital admissions across hosppitals. We do this by estimating a non-directional F test of any sorting. To do this we repeat the reduced form analysis in equation (1) separately for each of the 58 hospitals (and the outcome variable "did the individual have an admission to that hospital") and report the F-statistic on the null hypothesis that all the coefficients are the same. We then examine the impact of the lottery separately on utilization of public and private (for profit and non profit) hospitals,

and test whether the coefficients are equal; approximately **12%** of our admissions are to public hospitals.

For all our analysis of hospital type we estimate 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.

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). We expect that an impact of insurance on the type of hospital is more likely (and more interesting to study) when there is genuine hospital choice, compared to say in rural areas where close substitutes are not available. Hospital choice is defined for this analysis on the relevant margin: For any hospitalization, patients living in zip codes where less than **60%** of admissions are to a single of hospital are considered to have choice; this preserves approximately **70%** of the admissions in our sample. For public vs private hospitalization, patients living in zip codes in which more than **10%** and less than **90%** of admissions were to a public hospital are considered to have choice over hospital type; this preserves approximately **40%** of our sample.

We also perform analyses limiting the "with choice" subsample to to non-ED admissions. We expect that, even if there is a choice of hospitals, patients are more likely to be able to choose on more discretionary admissions. We report all of these analyses for "all admissions", "admissions with choice" and "Non-ED admissions with choice."

Note 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 over the period we are looking at). Therefore the means of the sub categories may not match the totals.

Section VII: Supporting analyses: Impact of health insurance on financial well-being

We use the credit report and the survey data to pursue three additional types of analyses of the impact of health insurance on financial well-being. The first two use exclusively the credit data: within the credit report data we decompose (where possible) the analysis into the impact of health insurance on medical and non medical debt, and we also undertake an examination of some additional outcomes. Finally, we explore (in both the survey and credit report data) the impact of health insurance on the distribution – particularly the right tail – of medical expenditures and of debt. This distributional analysis is arguably the most important for assessing the impact of health insurance on financial security. Once again, Appendix 2 provides more detail on variable definitions.

VII.A: Medical vs non medical debt (collections)

Where possible we distinguish between medical and non medical adverse financial events; we presume health insurance is most likely to reduce the rate of adverse medical financial events.

We can distinguish medical from non medical collections. In our population (and in general populations), medical collections are the single most common form of collection; using data from general populations with more detail, Avery et al (2003) note that after medical collections, the next most common form of collections are utilities. Over our sample period, about **29 percent** have a medical collection and about **39 percent** have a non medical collection. On average an individual in our sample owes about **$2,000** in medical collections and **$2,800** in non medical collections.

Table F1 reports the analysis of the impact of health insurance on medical vs. non medical collections (Panel A and Panel B, respectively). Of course, one caveat to this analysis is that while presumably anything identified as "medical collection" is indeed medical debt, some non medical collections may reflect medical debt (for example, if one charges one's medical bills to a credit card and then doesn't pay the credit card bill and the credit card company attempts to collect against you).

VII.B Additional outcomes in the credit data.

Other measures of financial strain

Finally, Table F1 panel C reports our analysis of the impact of health insurance on total current balances owed on all open revolving credit, which is another potential measure of financial strain. We view 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 individuals without revolving credit (i.e. the denominator is not defined if the individual has no revolving credit).[51]

Impact of health insurance on access to credit

The impact of insurance on access to credit is of direct interest, particularly in our low income and severely credit constrained population. A priori, we suspect an effect is unlikely, but still think it worth investigating. The sign of the impact of health insurance on access to credit is a priori ambiguous. One the one hand, if health insurance reduces the rate of medical collections it could improve the market's perception of one's credit worthiness and hence access to credit.[52] On the other hand, health insurance it is possible that there could be "over-shooting" whereby with increased access to credit comes an accumulation of unpaid debt and hence ultimately 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" activities (like pawn shops or family members) to "on the books" borrowing; any delinquency

[51] Only about **55** percent of our sample even 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.

[52] Since health insurance information is not collected by creditors or credit bureaus, it does not directly factor into credit access. If health insurance increases access to credit it is most likely via reducing unpaid medical bills (e.g. medical bills sent to collection) and therefore reducing a major derogatory that negatively impacts credit score.

then would be "on the books" 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 measure's of one's true credit worthiness (since there may be substitution that leads to more recording ("on the books") of behavior holding behavior constant) than of the market's assessment of one's credit worthiness; the latter is a real and interesting outcome.

For the substantive purpose of understanding the impact of health insurance on access to credit in the first year, we measure access to credit as of the end of our study period (September 2009). We examine three outcomes: whether you have a credit score, whether you have a thick file, and the total credit limit on all open revolving credit. These variables were defined in Section VE.

The results are shown in Table F2. The top panel shows results for the full sample. The bottom panel shows results for the "prior credit" subsample analyzed in P4. 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 "do you have a credit score" 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.

We note that the top panel is very similar to the analysis in table S2 except there the measure was "maximum access to credit" (to get at whether access to credit ever went up over our study period) while here the analysis is on access to credit at the end of our study period. [53] Once again we 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. The credit score (i.e. the market's assessment of your delinquency probability) is probably closest to a "pure" supply side measure, conditional on having one (not having one primarily reflects a lack of information on the person).

We note that if adverse financial events decline in a substantively important way, this should ultimately translate into improved access to credit. However this may not show up during our time horizon.

VII.C.Distribution of medical debt and medical out of pocket expenditures

Because the large potential welfare gains from health insurance improving financial security come from it reducing the extreme right tail of out of pocket medical expenditures and debt, we

[53] Note that the "maximum access" variable means for the controls (column 1 Table S2) look quite similar from the "current access" variable means for the controls for the analogous variables (column 1, panel A, Table F2). This is consistent with these individuals not experiencing large changes in access to credit over the approximately 12 month study period. The one exception is for the "total credit limit on revolving credit" which arises because the February 2009 data counts more credit in computing this statistic so it is simply a data / mechanical effect (and should not differentially affect treatment and controls). See Appendix 2 for more detail.

also examine the impact of health insurance on the entire distribution of out of pocket medical expenditures and debt. This may be more important than its impact on mean expenditures or debt; relatedly the analysis of the "any" margin for health expenditures or debt is admittedly an ad hoc cutoff and not necessarily a margin of interest. Finally, the right skewness of the measures – for example, conditional on having any collection the mean is more than double the median, the 90th percentile is more than double the mean etc – we did not want to limit ourselves to analysis of mean amounts owed. The empirical results from Finkelstein and McKnight (2008) suggest that there can be important effects of health insurance in the tails of this distribution, as standard insurance theory would suggest.

Using the survey data, we report quantile treatment estimates for the sum of out of pocket medical expenses in last six months and the amount of money owed for medical expenses. We prefer to combine these two measures because they are just different ways of financing the same thing. However, the time frame is different for the two questions and therefore we also show results separately for each of the two components. FiguresF1-F3 reports the results; they graph the point estimates (and 95 percent confidence intervals) of the quantile treatment estimates.

Using the credit report data, we also report quantile analysis of the impact of treatment on the quantiles of the distribution of the amount currently owed in collection accoonts, overall and separately for medical and non medical. [54] [55] In analogous fashion to Figures F1 through F3, Figures F4- F6 report the results for (respectively) all collections, medical collections and non medical collections.

In all of this quantile analysis, we are particularly interested in the right tail – i.e. the 75th to 99th percentile, and even more so the 90th to 99th percentile. We focus on this right tail for several related reasons: this is what insurance is designed to reduce, it is where reduction in spending or debt will have the largest welfare benefits, and it is where Finkelstein and McKnight (2008) found evidence of a decline in out of pocket expenditures associated with the introduction of Medicare.

Section VIII: Supporting analyses: Impact of health insurance on health: some potential mechanisms

The data allow us to investigate six potential mechanisms by which health insurance may affect health. Two potential mechanisms were already examined in our main analysis: (1) financial strain (which may be a substitute for health or a complement) and (2) health care use. We also look at four other potential mechanisms using the survey data: (3) whether health insurance

[54] We also observe the amount of money currently owed for liens, judgments, and late credit payments (delinquencies). We are hesitant to look at these measures since there is unavoidable double counting (e.g. some collections are sent to judgments) which could spuriously inflate our estimates of the impact of treatment on amounts owed. We therefore limit the analysis 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.

[55] Note that people can have positive collection balances even if they have not incurred a collection since the notification date since it can be owed on a prior collection.

improves *access* to care, (4) whether health insurance improves *quality* of care, (5) whether health care increases recommended preventive care utilization, and (6) whether health insurance affects *health behavior*. Health insurance may potentially improve some or all of these domains. We note that it is possible that some of these mechanisms will have longer run effects on health than detectable during the time period of our analysis.

The results are summarized in table M1 which follow the same basic structure as the primary analysis tables.

Health care access: We examine five measures of access: (1) whether you have a usual place of clinic- or office-based care, (2) whether you have a personal doctor, (3) whether you got all the medical care you needed in the last six months, (4) whether you got all the drugs you needed in the last six months, (5) and whether you did not use the ED for non ED care in the last six months. We note that the two "need" measures are somewhat difficult to interpret as one's subjective assessment of one's "need" may be endogenous to insurance (or health care utilization that in turn is affected by insurance).

Quality of care: We look at self-reported quality of care (good, very good or excellent relative to fair or poor); we note that this is conditional on receiving care which is unavoidable but raises possible selection issues. We also note that one's subjective assessment of "quality" may be directly affected by gaining insurance, even if objective quality is unaffected.

Recommended preventive care: We look at whether you have had a pap test within the last year (women only), whether you have had a mammogram in the last year (women aged 40+ only), whether you have ever had your blood cholesterol checked and whether you have had a test for high blood sugar for diabetes. We note that for the first two we look at the relevant population (limiting mammograms to the age group recommended at the time of our study) and look within the last year since the recommendation is for annual mammograms. For blood cholesterol and diabetes checks we look at whether one has "ever" had them because the recommendation is to do it every 3 to 5 years (and about 50% of our sample has been insured for more than a year).[56] We note that receiving recommended preventive care presumably reflects both care utilization and the quality of that care utilization.

Health behavior: We look at two health behaviors, whether you currently smoke and whether you are less physically active compared to others your age.

Section IX: Exploratory analyses: impact of health insurance on labor force participation and happiness

We examine the impact of health insurance on self-reported happiness and on labor force participation in our survey data. We do not have a strong prior as to whether we would find an effect (or how we would interpret it). There is evidence from Gallup polls (Kahneman 2010) of a strong, positive cross-sectional relationship between insurance coverage and subjective well being. The impact of public health insurance eligibility on labor force participation is ex ante

[56] Our survey only distinguishes between preventive care received within the last year and that received more than a year ago. See Appendix 2 for detail.

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 such participation may be to get access to private health insurance.

We measure happiness by comparing those who report themselves "not too happy" with those who report themselves as "pretty happy" or "very happy". We look at three measures of labor force participation: whether currently employed, whether currently work 20+ hours per week (which is a natural dividing line beyond which employers are more likely to offer health insurance on the job) and gross (pre-tax) household income. (Because of the censoring of income for the approximately **1.5** percent of people who report income in the highest bin ("above $50,000") we also check and report the sensitivity of our estimates to a Tobit.[57]

Table E1 reports the results. The table follows the same basic structure as the primary analysis tables.

Section X. Interpretations and caveats

X.A: Comparison of our estimates to existing evidence

We can (and will) compare specific results (where possible) to existing estimates in the experimental literature (i.e. Rand) and quasi-experimental literature.

In addition, we are interested in the difference between the experimental estimates and the OLS. To compare our results to evidence from an observational setting we therefore perform some additional analyses. There is no direct observational analogue of our research question which can be estimated in our data, so we take several complementary approaches.

We first compare, in our full study population, individuals who differ in their insurance coverage. Table C1 shows these results. The first column replicates our 2SLS results for comparison. The second column compares all those with any Medicaid at any point in the study period to all those without Medicaid (regardless of lottery status). This represents the "as treated" analysis sometimes done in clinical trials. This is an observational comparison of those with insurance to those without, but it is not necessarily analogous to what would be observed in the absence of the lottery. Much of the variation in insurance in this population is driven by the lottery; of the **25 percent** of our overall population with insurance at any point in the study period, **47 percent** are treatment individuals with OHP Standard. This will push our estimate towards the experimental estimate. To avoid having much of the variation in our insurance variable coming from the lottery, the third column performs the same analysis in the control group only. Limiting to the control group, we compare those with any Medicaid at any point in the study period to those without. This is a cleaner observational comparison of those with insurance to those without

[57] Note that (as detailed in Appendix 2) 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.

than column 2, because it does not include the experimental variation. Most of the insurance in the control group, however, is OHP Plus, Oregon's traditional Medicaid program. This program has difference insurance coverage, and more importantly, is available to a different population than OHP Standard which is the main insurance program we are studying. Thus, to the extent this estimate is different from our main estimates, it will not be clear how much this is due to using observation data (and the corresponding potential for bias) and how much is due to heterogeneous treatment effects.

As a final comparison in our study population, we limit to the treatment group and compare those who received OHP Standard with those who had no Medicaid. This focuses then on the same insurance program, and same population, as our main estimates. It is perhaps the most direct observational analogue for our randomized analysis for the effects of OHP Standard. To the extent that these estimates differ from our main results, it will be due to the endogenous take-up of OHP Standard among those offered a chance to apply. As discussed elsewhere, the incomplete take-up of insurance reflects both that some selected individuals are not in fact eligible and that some eligible individuals did not apply for insurance.

All of the above analysis represents various non experimental comparisons within our study population. To provide observational estimates of the impact of insurance coverage from outside of our data, we turn to the Behavioral Risk Factors Surveillance Study (BRFSS). Using BRFSS data from the years 2004-2009, we limit the sample to individuals aged 19 to 64 who are below 100 percent of the federal poverty line (N= 144,829). In this sample, we compare those with any insurance to those without **(**about **56** percent of the BRFSS sample has insurance)[58]. We analyze five outcomes which either roughly or exactly match questions in the mail survey. These are refused treatment because of medical debt, number of days of impaired physical or mental health during past 30 days, number of days of physical health not good during past 30 days, number of days of mental health not good during past 30 days, and screened positive for depression in the last two weeks.[59] We therefore have some measures of financial strain and some of health. We are looking into adding some comparable measures of utilization from the National Health Survey.

For each outcome we report three results. Table C2 shows the results. In column 1 we report the 2SLS from our lottery population. In column 2 and 3, for comparison, we report OLS estimates of the outcome on insurance coverage in the BRFSS. In column 2 we report the simple bivariate regression. In column 3 we add controls for age, an indicator variables for being white, an indicator variable for being male, an indicator variable for having a high school degree or less, an indicator variable for being married, number of children living in the household, an indicator variable for being currently employed, and indicator variables for annual income below $10,000 or between $10,000 and $25,000.

[58] The BRFSS asks whether the individual has "any kind of health care coverage, including health insurance, prepaid plans such as HMOs, or government plans such as Medicare?".

[59] For all but the first variable, the BRFSS questions and our mail survey questions are identical. For the first question the mail survey asks if you were "refused treatment because of medical debt (last 6 months)" while the BRFSS asks "was there a time in the past 12 months when you needed to see a doctor but could not because of cost." Also the depression screen is only available in the BRFSS for two years (2006 and 2008).

X.B. Caveats and interpretations

Interpretations: We plan on discussing the magnitude of our estimates (both point estimates and the 95 percent confidence interval to see what we can reject) and comparing them to both the existing literature and our OLS estimates (see above).

Caveats on interpretation
It is important to be clear about some of the important caveats to interpreting our results.

First, our results capture only the short run (approximately one year) effects of health insurance; longer run follow ups are needed (if a first stage persists) to capture longer run effects.

Second, our results are specific to the population and insurance that we study. They speak most directly to the effect of offering public health insurance (Medicaid) to a low income uninsured population that has expressed interest in obtaining health insurance (signed up for the lottery). Caution must be exercised in extrapolating to mandatory insurance coverage (since our effects are limited to those who both expressed interest in health insurance and chose to take up the insurance if selected), to insurance coverage for a wealthier population, or to a different kind of insurance coverage. The analysis of heterogeneous treatment effects (Table P4) is designed to help guide out of sample extrapolations.

To get more of a sense of who our population is that the results apply to, Table C3 compares our lottery participants (both the whole list, and those who respond to our survey) to the general low income population in Oregon and the rest of the U.S.. See also Allen et al (2010) for more discussion. In addition, in the Appendix we report on and how our lottery participants compare in the hospital discharge data to the general Oregon population (Table A7), and how our lottery participants compare in credit report data to the general Oregon population outcomes (see Table A9)..

Our results are also specific to the health care setting in Oregon and the Oregon Health Plan in particular. The Oregon lottery represented a voluntary expansion to adults ages 19-64 who are below the federal poverty level, but do not qualify for traditional Medicaid. Currently, there is substantial variation in what states offer to this population. In 2009, fewer than half of the 50 states offered any coverage for this population. Of the states that did offer coverage, some offered full Medicaid benefits, some offered a more limited benefit program (like OHP Standard), and some offered premium assistance for purchasing private insurance (often with work-related eligibility requirements) (Kaiser, 2010a). As part of the Affordable Care Act, starting in 2014, all states will be required to provide Medicaid coverage to adults ages 19-64 up to 133% of the federal poverty level (Kaiser, 2010b).

Third, by their nature, our findings speak to the partial equilibrium effects of covering a small number of people, holding constant the rest of the health care system. They are therefore difficult to extrapolate to the likely effects of major health care reform, in which there may well be supply side responses from the health care sector (see Finkelstein, 2007).

Finally, in our survey data, self-reported measures, particularly self-reported health measures, may be subject to important biases due to insurance; for example, health insurance may induce health care use which may make one report being in better or worse health, even without an underlying change in health; our in-person study (which is being fielded over the September 2009 through December 2010 period) will attempt to address this by collecting physical, objective health measures.

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Figures and Tables

Table 1: Data used in Primary Analyses

	Administrative data	Mail survey data (self reports)
Cost I: Utilization	Hospital discharge data measures of inpatient use.	Hospital use, emergency department, outpatient care of all types, drugs
Benefit I: Reduction in Financial strain	Credit bureau data with measures of medical and non medical late bill payment; measures of extreme strain (judgments, bankruptcy and liens); market's assessment of credit worthiness	Alternative measures of financial strain; may capture things not captured by credit report (e.g. borrowing from non traditional sources). Also directly asks about out of pocket medical expenses.
Benefit II: Improved health	Mortality.	Variety of self-reported measures

Table I1: Time From Start of Insurance (various definitions) through September 30, 2009

	Time from lottery notification date (months)	**Time from retroactive insurance coverage (months)**	**Time from insurance application approval (months)**
All	16	15	14
SE	*2*	*2*	*3*
Lottery draw 1	19	19	17
Lottery draw 2	18	18	16
Lottery draw 3	18	17	15
Lottery draw 4	17	16	14
Lottery draw 5	16	15	13
Lottery draw 6	15	14	12
Lottery draw 7	14	13	11
Lottery draw 8	13	12	10

Notes: Table reports the mean time (in months) from start of insurance (by various definitions) through September 30, 2009 for the overall sample and for each lottery draw. We report the mean for each draw and the mean and standard deviation for the overall sample. The time of insurance starting is defined in three ways. The first column measures from the lottery notification date. This is the earliest those selected knew of their selection status. The second column measures from the retroactive insurance coverage date. For those receiving insurance as a result of the lottery, this is the date insurance began (even if the application processing did not finish until after this date). The third column measures from the date of insurance application approval (for approved treatment individuals only). Sample size is **74851** for overall sample and 8852 for approved treatment individuals only.

Table I2: Balance of treatment and controls

	Mean (Std dev) for controls (full sample)	Different between treatment and control		
		Sample Universe (HDD and Mortality Sample)	Credit Report Subsample	Survey Respondent Subsample
	(1)	(2)	(3)	(4)
Panel A: Match / response rates:				
Matched in Sept 09 credit data?	0.658	.N/A	-0.004	.N/A
SE	*0.474*	*.*	*0.004*	*.*
p-value	*.*	*.*	*0.289*	*.*
Responded to survey?	0.502	N/A	N/A	-0.016
	0.5			*0.007*
	.			*0.016*
Panel B: Lottery list characteristics				
Year of birth?	1967.98	0.2	0.19	-0.093
	12.269	*0.099*	*0.119*	*0.19*
	.	*0.044*	*0.11*	*0.624*
Female?	0.558	-0.008	-0.005	-0.005
	0.497	*0.003*	*0.004*	*0.007*
	.	*0.013*	*0.219*	*0.473*
English as preferred language?	0.923	0.001	0.003	-0.001
	0.267	*0.003*	*0.003*	*0.005*
	.	*0.774*	*0.389*	*0.916*
Signed self up?	0.916	*0*	0	-0.002
	0.277	*0*	*0.001*	*0.003*
	.	*0.128*	*0.655*	*0.507*
Signed up first day of list?	0.095	*-0.001*	-0.002	0.006
	0.293	*0.002*	*0.003*	*0.005*
	.	*0.68*	*0.497*	*0.244*
Gave phone number?	0.861	*-0.003*	0	0.005
	0.346	*0.003*	*0.003*	*0.004*
	.	*0.367*	*0.976*	*0.143*
Address a PO Box?	0.117	*0*	0.002	-0.002
	0.321	*0.003*	*0.003*	*0.005*
	.	*0.915*	*0.477*	*0.7*
In MSA?	0.773	-0.002	-0.002	0.001
	0.419	*0.004*	*0.004*	*0.007*
	.	*0.495*	*0.654*	*0.903*
Median hh income of zip code	39250.5	59.246	24.039	0.824
	8457.894	*72.714*	*89.67*	*134.867*
	.	*0.415*	*0.789*	*0.995*
Pooled F – stat	.	1.70645	0.73158	0.55191
p-value	*.*	*0.081582*	*0.680313*	*0.837135*
N	*.*	*74851*	*49552*	*24048*

Notes: All variables are pre-randomization "demographics" taken from the lottery list (from January and February 2008). "English as preferred language" indicates whether you did not check a box requesting materials in a language

other than English. “Signed up self?” is an indicator for whether you signed yourself up (as opposed to a household member including your name when they signed up). “Signed upon first day of list?” is an indicator variable for whether you signed up the first day the list was open. “Gave phone number” is an indicator variable for whether you provided a phone number when you signed up. The first column reports the mean and standard deviation of these variables for the control sample. Columns (2) through (4) report estimated differences between treatments and controls for the outcome shown in the left hand column and the subsample indicated in the column heading (except in Panel A where the whole sample is used). Specifically it reports the coefficient on LOTTERY based on estimating equation (1); the dependent variable is given in the left hand column. All regressions include household fixed effects. In addition, in column (4) we include survey wave fixed effects and the interaction of survey wave fixed effects and household fixed effects; the regressions in column (4) also use the survey weights, while those in columns 2 and 3 are unweighted. All standard errors are clustered on household. We report the coefficient, standard error, and per comparison p-value. The last row of the table reports the pooled F-stat from estimating the analogous variant of equation (5) on all the variables shown above.

Table I3: Additional initial characteristics of control survey responders

	Mean	(standard deviation)
Race		
% White	0.827	*0.378*
% Black	0.036	*0.186*
Ethnicity		
% Spanish / Hispanic / Latino	0.104	*0.306*
Education		
% Less than High School	0.179	*0.383*
% High school diploma or GED	0.515	*0.5*
% Vocational Training or 2-year degree	0.206	*0.404*
% 4-year college degree or more	0.101	*0.302*
Employment		
% don't currently work	0.53	*0.499*
% work < 20 hrs per week	0.094	*0.291*
% work 20-29 hrs per week	0.108	*0.311*
% work 20+ hrs per week	0.268	*0.443*
Health status		
Ever diagnosed w diabetes	0.114	*0.318*
Ever diagnosed w asthma	0.166	*0.372*
Ever diagnosed w high blood pressure	0.284	*0.451*
Ever diagnosed w Emphysema or Chronic Bronchitis	0.077	*0.266*
Ever diagnosed with Depression	0.443	*0.497*
Income relative to FPL		
% below 50% of FPL	0.446	*0.497*
% 50-75% of FPL	0.131	*0.337*
% 75-100% of FPL	0.151	*0.358*
%100 – 150% of FPL	0.168	*0.374*
% above 150% FPL	0.105	*0.306*
Insurance coverage		
Any insurance?	0.243	*0.429*
OHP / Medicaid insurance?	0.075	*0.263*
Private insurance?	0.093	*0.29*
Other?	0.09	*0.286*
# of months of last 6 with insurance	0.934	*2.019*

Note: We report survey responses for control individuals who responded to the initial survey (N = **13268**). Reponses to the initial survey (mostly) occurred shortly after randomization. In particular, the survey was fielded starting in June 2008. The average response date was August 29, 2008 (inter-quartile range; July 31 to September 22). The total survey size of controls was or which we got responses; response rates vary slightly by question (see appendix 2 for more detail). For the insurance questions, we code as "yes" if the respondent checked that insurance type box; since the survey allows you to check multiple boxes for types of insurance, the subgroups (OHP/Mediciad, private, and other) won't necessarily add up to "any". Private insurance includes employer and privately paid insurance; "Other" insurance includes "Medicare and other." We treat responses for insurance as missing if the responder checked "I don't know" or left all check boxes blank.

Table I4: First stage estimates

	Entire List (HDD and Mortality Sample)		Credit Report Sub-Sample		Survey Respondent Sub-sample	
	Control mean (1)	Estimated First Stage (2)	Control Mean (3)	Estimated First Stage (4)	Control mean (5)	Estimated First Stage (6)
Baseline Measure: On Medicaid, ever	0.147	0.264	0.141	0.263	0.141	0.302
SE	.	*0.004*	.	*0.004*	.	*0.007*
F	.	*5597.33*	.	*3763.916*	.	*2047.04*
Alternative definitions:						
# of months on Medicaid	1.473	3.458	1.407	3.472	1.576	4.093
	.	*0.045*	.	*0.055*	.	*0.09*
	.	*5814.782*	.	*3944.236*	.	*2069.935*
On Medicaid, end of time period	0.11	0.152	0.104	0.156	0.108	0.197
	.	*0.003*	.	*0.004*	.	*0.006*
	.	*2356.257*	.	*1677.263*	.	*1045.805*
On OHP Standard, ever	0.028	0.274	0.03	0.273	0.027	0.314
	.	*0.003*	.	*0.004*	.	*0.005*
	.	*8562.328*	.	*5648.552*	.	*3266.905*
Sample Size	.	74851	.	49552	.	22867

Note: Even numbered columns reports coefficient on "LOTTERY" variable from estimating the first stage relationship shown in equation (3), along with the standard error (right below) and the F stat (below that) for the "LOTTERY" variable. Odd numbered columns report the control mean on the "INSURANCE" outcome. The first row shows our preferred first stage measure. The remaining rows show alternative measures. All regressions include dummies for household size. Columns (2) and (4) also include dummies for lottery draw. Column (5) also include dummies for survey wave and survey wave interacted with household size dummies. Regressions on the sample of survey respondents (column 5) use the 12 month survey weights. In all regressions, standard errors are adjusted for household clusters. All insurance variables are measured from the state Medicaid enrollment files. The time period for the estimates in columns 1 through 4 starts with the first Medicaid file after the notification date (which varies by lottery draw) and goes through the first Medicaid file after September 30th, 2009 (October 5, 2009); in columns 5 and 6 the time period is defined from the first notification date of any lottery draw (i.e. March 10, 2008) through the average response date (by survey wave).

Table I5: First stage estimates: other sources of insurance

	Control Mean	Estimated first stage
On Medicaid "currently" (closest administrative record to survey response date)	0.097	0.183
SE	.	*0.006*
F	.	*1018.266*
On Medicaid "currently" (*self-report*)	0.117	0.197
SE	.	*0.006*
F	.	*1008.061*
Have Private Insurance "currently" (*self report*)	0.128	-0.008
SE	.	*0.005*
F	.	*2.44*
Have any insurance "currently" (*self report*)	0.324	0.18
SE	.	*0.008*
F	.	*557.115*
# of months (0-6) insured in last 6 months (*self report*)	1.733	1.156
SE	.	*0.043*
F	.	*715.591*
# of months with Any Medicaid (six months ending with survey response)	0.601	*1.323*
SE	.	*0.034*
F	.	*1478.718*
N	.	*22867*

Note: Table reports coefficient on "LOTTERY" variable from estimating the first stage relationship shown in equation (3), along with standard error (in parentheses) and the F-statistic [in square brackets] for the "LOTTERY" variable. All regressions are done on the sample of survey respondents. 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. Table compares estimates using the state Medicaid enrollment files to those using self-reported survey data. To enhance comparability, we chose the administrative eligibility file that represents the date which is immediately after the survey response date. We measure "current" insurance in that file, and number of months of the last six in the six months ending with that file. In the survey, respondents could report various types of insurance; we define "private insurance" as employer or private insurance and "any insurance" as Medicaid, Medicare, employer, private or other insurance.

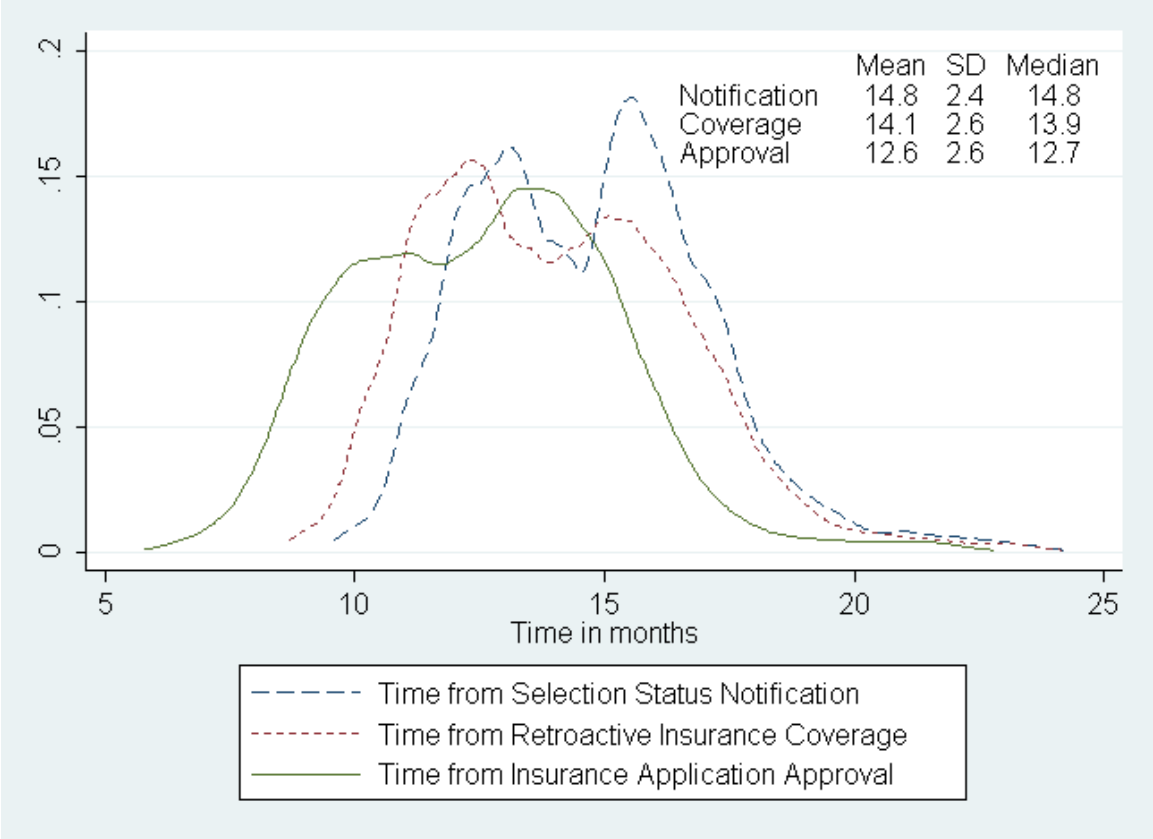
Table I6: "Compliers" vs. "Non-Compliers"

	Mean (standard deviation) for controls	Compliers vs non compliers		
		Approximate compliers (among treated)	Approximate non compliers (among treated)	Difference
	(1)	(2)	(3)	(4)
Year of birth?	1967.98	1967.09	1968.69	-1.598
SE	*12.269*	*11.749*	*12.285*	*0.15*
p-value	*.*	*.*	*.*	*0*
Female?	0.558	0.542	0.539	0.003
	0.497	*0.498*	*0.498*	*0.006*
	.	*.*	*.*	*0.674*
English as preferred language?	0.923	0.954	0.88	0.074
	0.267	*0.21*	*0.325*	*0.003*
	.	*.*	*.*	*0*
Signed self up?	0.916	0.885	0.811	0.074
	0.277	*0.32*	*0.392*	*0.004*
	.	*.*	*.*	*0*
Signed up first day of list?	0.095	0.117	0.086	0.031
	0.293	*0.321*	*0.28*	*0.004*
	.	*.*	*.*	*0*
Gave Phone Number?	0.861	0.883	0.861	0.022
	0.346	*0.321*	*0.346*	*0.004*
	.	*.*	*.*	*0*
Address a PO Box?	0.117	0.132	0.111	0.02
	0.321	*0.338*	*0.315*	*0.004*
	.	*.*	*.*	*0*
In MSA?	0.773	0.751	0.77	-0.019
	0.419	*0.432*	*0.421*	*0.005*
	.	*.*	*.*	*0.001*
Median household income of zip code	39250.5	39014.1	39455	-440.93
	8457.894	*8465.871*	*8445.012*	*108.732*
	.	*.*	*.*	*0*
Pooled F – stat	.	.	.	122.588
p-value	.	.	.	*0*
N	.	.	.	*35172*

Note: Column reports pre-randomization variables taken from the lottery list (from January and February 2008). The first column reports the mean and standard deviation of these variables for the controls. "English as preferred language" indicates whether you did not check a box requesting materials in a language other than English. "Signed up self?" is an indicator for whether you signed yourself up (as opposed to a household member including your name when they signed up). "Signed upon first day of list?" is an indicator variable for whether you signed up the first day the list was open. "Gave phone number" is an indicator variable for whether you provided a phone number when you signed up. Columns 2 and 3 report the mean and standard deviation of these variables for our "approximate compliers" (defined as treatment individuals who are on OHP Standard at any point over our study period) and "approximate non compliers" (defined as treatment individuals who are not on OHP Standard at any point over our study period). Column 4 reports the estimated difference in each variable between our approximate compliers and approximate non compliers based on estimating a variant of equation (1) on the treatment sample and with the only right hand side variable an indicator variable for "complier"; we report the coefficient, standard error, and per comparison p-value. The last row of the table reports the pooled F-stat from estimating the analogous variant of equation (5) on all the variables shown above. All regressions are unweighted; there are no covariates and no

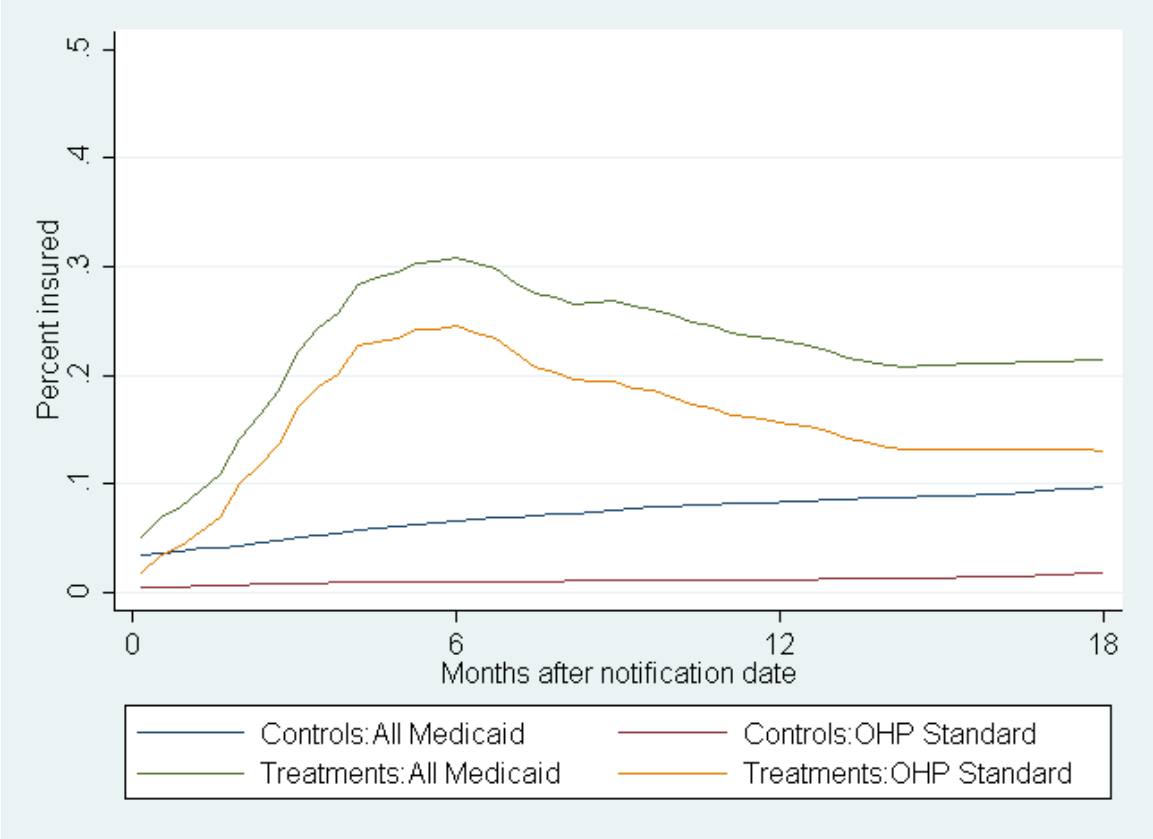
clustering of the standard errors. Note that we do not need household size dummies or survey wave dummies since our analysis is only among treated individuals.

Figure I1. Distribution of Time from Insurance Starting to Survey Response



Notes: Figure shows the distribution of time (in months) from insurance starting to survey response for survey responders only. The time of insurance starting is defined three ways; the lottery notification date, the retroactive insurance coverage date and the date of insurance application approval (for approved treatment individuals only). Sample consists of responders to the 12-month survey (**N=23,337**).

Figure I2 Time path of Medicaid and OHP Standard enrollment



Notes: Figure shows the percent with public insurance coverage over time. Time is measured in months relative to notification date. Percent with insurance is shown separately for treatments and controls, and both all Medicaid coverage and OHP Standard coverage percentages are given. Sample consistent of full sample universe (N=**72,700**).

Table P1: Impact of Health Insurance on Health Care Utilization

	Control mean	Reduced form	2SLS
	(1)	(2)	(3)
Hospital Discharge Data:			
# of days (since notification date)	-0.006		
SE	*0.004*		
p	*0.112*		
adj-p			
List charges (since notification date)	2667.72		
	19995.48		
	.		
	.		
# of Procedures (since notification date)	0.158		
	1.104		
	.		
	.		
Standardized Treatment Effect (Discharge data)	.		
SE	.		
p	.		
Survey Data:			
Number of prescription drugs currently taking	2.311		
	2.874		
	.		
	.		
Number of outpatient visits in last six months	1.916		
	3.097		
	.		
	.		
Number of ED visits in last six months	0.469		
	1.035		
	.		
	.		
Number of hospital visit in last six months	0.098		
	0.4		
	.		
	.		
Standardized treatment effect (Survey data)	.		
SE	.		
p	.		
Estimated spending effect (Survey data)	.		
	.		
	.		
Standardized treatment effect using both data sources:			
SE			
p	.		

Note: Table reports the coefficient on LOTTERY from estimating equation (1) by OLS (column 2), the coefficient on INSURANCE from estimating equation (2) by IV (column 3). For each outcome we report the estimate, standard error, per-comparison p-value and the family-wise p-value across all of the individual outcomes analyzed in the table. The top panel reports analysis using the hospital discharge data (N=**74851)**; here outcomes are measured since the notification date through September 30th, 2009. Means in the hospital data therefore reflect an average look back

period of **15 months**. The bottom panel reports the results for the survey data **(N=22867)**; here outcomes are measured either at time of survey response or "over last 6 months" as indicated. All regressions include household size fixed effects and standard errors are clustered on household. Regressions that use hospital discharge data also include lottery draw fixed effects and the analogous outcome measure for the time period from January 1, 2008 until the notification date. Regressions that use survey data also include survey wave fixed effects, and the interaction of survey wave and household size. Regressions that use survey data are weighted using the survey weights. Standardized treatment effects report results based on estimating equation (5) for the reduced form (column 2) or IV estimates of equation (6) for the 2SLS (column 3) and then calculating standardized treatment effects based on equation (4). For each standardized treatment effect we report the estimate, standard error, and per comparison p-value. The "estimated spending effect" is estimated by estimating equation (5) for the reduced form or 2SLS estimates of equation (6) (column 3) using the survey utilization measures each weighted by an estimate of the cost per use (see text for more details). The bottom line "standardized treatment effect using both data sources" uses the measures of prescription drug, outpatient, and ED use from the survey and the "number of hospital days" measure from the hospital discharge data.

Table P2: Impact of health insurance on financial well being:

	Control mean	Reduced form	2SLS
	(1)	(2)	(3)
Credit Report Data:			
Any bankruptcy (since notification date) *SE* *p-value* *adj-p-value*	0.013 *0.115* . .		
Any lien (since notification date)	0.021 *0.142* . .		
Any judgment (since notification date)	0.061 *0.239* . .		
Any collection (since notification date)	0.476 *0.499* . .		
Any delinquency on credit account (since notific. date)	0.368 *0.482* . .		
Standardized treatment effect (Credit Report Data)	. . .		
Survey data:			
Any out of pocket medical expenses in last six months?	0.554 *0.497* . .		
Currently owe money for medical expenses?	0.595 *0.491* . .		
Borrowed money or skipped bills for medical bills (last 6 months)?	0.364 *0.481* . .		
Refused treatment bc of medical debt (last 6 months?	0.081 *0.273* . .		
Standardized Treatment Effect (Survey data)	. . .		
Standardized treatment effect using both data sources	. . .		

Note: Table reports the coefficient on LOTTERY from estimating equation (1) by OLS (column 2), the coefficient on INSURANCE from estimating equation (2) by IV (column 3). For each outcome we report the estimate, standard error, per-comparison p-value and the family-wise p-value across all the individual outcomes analyzed in the table. The top panel reports analysis using the credit report data (N=**49552)**; here outcomes are measured since the notification date through September 30th, 2009; the credit report data therefore reflects an average look-back time of **16 months.** The bottom panel reports the results for the survey data **(N=22867)**; here outcomes are measured either at time of survey response or "over last 6 months" as indicated. All regressions include household size fixed effects and standard errors are clustered on household. Regressions that use credit report data also include lottery draw fixed effects and the analogous outcome measure from the February 2008 credit report data. Regressions that use survey data also include survey wave fixed effects, and the interaction of survey wave and household size. Regressions that use survey data are weighted using the survey weights. Standardized treatment effects report results based on estimating equation (5) for the reduced form (column 2) or 2SLS estimates of equation (6) (column 3) and then calculating standardized treatment effects based on equation (4). For each standardized treatment effect we report the estimate, standard error, and per comparison p-value. The bottom line "standardized treatment effect using both data sources" pools across all the survey and credit report measures.

Table P3: Impact of Health Insurance on Health:

	Control mean	Reduced form	2SLS
	(1)	(2)	(3)
Mortality data:			
Died (since notification date)?	0.005 *0.074* .		
Standardized Treatment Effect (mortality data)	. . .		
Survey data:			
Self reported health fair or poor?	0.452 *0.498* . .		
Self reported health poor?	0.14 *0.347* . .		
Health gotten worse over last six months?	0.286 *0.452* . .		
Number of days impaired by physical or mental health during past 30 days	8.133 *10.384* . .		
Number of days of physical health not good during past 30 days	9.664 *10.942* . .		
Number of days mental health not good during the past 30 days	11.259 *11.451* . .		
Screened positive for depression in last two weeks?	0.329 *0.47* . .		
Standardized treatment effect (survey data)	. . .		
Standardized treatment effect using both data sources	. . .		

Note: Table reports the coefficient on LOTTERY from estimating equation (1) by OLS (column 2), the coefficient on INSURANCE from estimating equation (2) by IV (column 3). For each outcome we report the estimate, standard

error, per-comparison p-value and the family-wise p-value across all of the individual outcomes analyzed in the table. The top panel reports analysis using the mortality data for the sample universe (N=**74,851)**; here outcomes are measured since the notification date through September 30th, 2009; the mortality data therefore reflects an average look-back time of **16 months.** The bottom panel reports the results for the survey data **(N=22867)**; here outcomes are measured over various time horizons as indicated in the table. All regressions include household size fixed effects and standard errors are clustered on household. Regressions that use mortality data also include lottery draw fixed effects. Regressions that use survey data also include survey wave fixed effects, and the interaction of survey wave and household size. Regressions that use survey data are weighted using the survey weights. Standardized treatment effects report results based on estimating equation (5) for the reduced form (column 2) or 2SLS estimates of equation (6) 2SLS (column 3) and then calculating standardized treatment effects based on equation (4). For each standardized treatment effect we report the estimate, standard error, and per comparison p-value. The bottom line "standardized treatment effect using both data sources" pools across all the survey and mortality measures.

Table P4: Analyzing heterogeneity in impact of health insurance (2SLS)

	N	First stage	Health Care Use	Financial well-being	Health
Full sample					
Gender:					
Male	33647	0.278			
Female	41203	0.253			
p-value of difference	.	.			
Age:					
50-63	19747	0.284			
19-49	55104	0.256			
p-value of difference	.	.			
Urban/rural:					
Urban/suburban (MSA)	57605	0.259			
Rural (non-MSA)	17246	0.277			
p-value of difference	.	.			
Prior financial status (Credit Report Subsample)					
Have prior credit	27808	0.234			
Do not have prior credit	21744	0.299			
p-value of difference					
Education (Survey Respondents)					
High school or less	7733	0.296			
More than high school	15545	0.317			
p-value of difference	.	.			
Ever Smoke (Survey Respondents)					
Ever Smoke	15026	0.339			
Never Smoke	8528	0.265			
p-value of difference	.	.			

Note: Table reports first stage estimates and standardized treatment effects in subgroups of the sample. We use the group-specific first stage (shown in column 2), reporting the coefficient on the "LOTTERY" variable from estimating the first stage relationship shown in equation (3). Those regressions include dummies for household size and lottery draw, and the standard errors are clustered by household. The time period for the insurance variable starts with the notification date and runs through September 30, 2009. Columns 3-5 report the standardized treatment effects based on IV estimates of equation (6) and then calculating standardized treatment effects based on equation (4). For each standardized treatment effect we report the estimate, standard error, and per comparison p-value. The top row shows the results for the full sample; these standardized treatment effects, which replicate the "bottom rows" of Tables P1 through P3, pool survey and administrative data; see notes to those tables and text for more detail on the components of these standardized treatment effects. The subsequent rows show results for various sub-groups. We calculate the group-specific effects by fully interacting group with the regression predictors. We use the standard deviations from the full control sample in calculating the standardized effect. P value of difference is calculated by testing the whether the linear combination of the INSURANCE by group interactions, standardized using the standard deviations from the full control sample, is equal to zero.

Table S1: Sensitivity of Standardized Treatment Effects to Covariates

	Reduced Form			2SLS		
	baseline	No V's	Add lottery list V's	baseline	No V's	Add lottery list V's
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Utilization						
Discharge data						
Survey data						
Survey data (spending effect)						
Pooled						
Panel B: Financial well being						
Credit report data						
Survey data						
Pooled						
Panel C: Health						
Mortality data						
Survey data						
Pooled						

Note: Table reports standardized treatment effects based on estimating equation (5) (for the reduced form) or equation (6) by IV for the 2SLS and then calculating standardized treatment effects based on equation (4). For each standardized treatment effect we report the estimate, standard error, and per comparison p-value. Columns (1) and (4) show the baseline specification for the reduced form and 2SLS, respectively. These are taken from Tables P1 through P3 and their construction is described in those table notes and accompanying text. Columns 2 and 5 show the sensitivity of the results to removing lottery list draw dummies and pre-period y from the baseline specification of the administrative data. Columns 3 and 6 show the results from adding the lottery list covariates (see e.g. Table I2) to the baseline specification in both the survey and the administrative data.

Table S2: Impact of Health Insurance on "Maximum" Access to Credit

	Control mean	Reduced Form
Full sample		
Have a credit score?	0.826	0.002
SE	*0.379*	*0.002*
p-value	.	*0.407*
Have a thick file?	0.422	0.001
	0.494	*0.003*
	.	*0.774*
Credit limit on all open revolving credit	11415.9	-5.732
	31558.07	*130.463*
	.	*0.965*
Standardized treatment effect	.	0.002
	.	*0.003*
	.	*0.53*
N	49552	49552

Notes: "Maximum" access to credit is defined over the February 2009 and September 2009 credit report archive. All 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 reports the coefficient on LOTTERY from estimating equation (1) by OLS (column 2).All regressions include household size fixed effects and standard errors are clustered on household; they also include lottery draw fixed effects and the pre period outcome as additional controls. For each outcome we report the estimate, standard error, per-comparison p-value and the family-wise p-value across all the individual outcomes shown in the table. The bottom row reports the standardized treatment effects report results based on estimating equation (5) and then calculating standardized treatment effects based on equation (4). For the standardized treatment effect we report the estimate, standard error, and per comparison p-value. A "thick file" is defined as 2 or more open trade lines.

Table U1: Impact of Health Insurance on Utilization: Extensive Margin

	Control mean	Reduced Form	2SLS
	(1)	(2)	(3)
Hospital Discharge Data:			
Any Hospital Admission (since notification date)	0.068		
se	(0.252)		
p			
adj-p			
Standardized treatment (Discharge Data)			
se			
p			
Survey Data:			
Currently taking any prescription drugs?	0.636		
se	(0.481)		
p			
adj-p			
Any outpatient care visit in last six months?	0.575		
se	(0.494)		
p			
adj-p			
Any ED visit in last six months?	0.261		
se	(0.439)		
p			
adj-p			
Any Hospital Visit in last six months	0.072		
se	(0.258)		
p			
adj-p			
Standardized treatment effect (Survey data)			
se			
p			
Standardized treatment effect (pooled)			
se			
p			

Note: Table reports the coefficient on LOTTERY from estimating equation (1) by OLS (column 2), the coefficient on INSURANCE from estimating equation (2) by IV (column 3). For each outcome we report the estimate, standard error, per-comparison p-value and the family-wise p-value across all the individual outcomes shown in the table. The top panel reports analysis using the hospital discharge data (N=**74851)**; here outcomes are measured since the notification date through September 30th, 2009 and therefore reflect an average look back period of **15 months**. The bottom panel reports the results for the survey data **(N=22867)**; here outcomes are measured either at time of survey response or "over last 6 months" as indicated. All regressions include household size fixed effects and standard errors are clustered on household. Regressions that use hospital discharge data also include lottery draw fixed effects and the analogous outcome measure for the time period from January 1, 2008 until the notification date. Regressions that use survey data also include survey wave fixed effects, and the interaction of survey wave and household size. Regressions that use survey data are weighted using the survey weights. Standardized treatment

effects report results based on estimating equation (5) for the reduced form (column 2) or 2SLS estimates of equation (6) (column 3) and then calculating standardized treatment effects based on equation (4). For each standardized treatment effect we report the estimate, standard error, and per comparison p-value. The bottom line "standardized treatment effect using both data sources" uses the measures of prescription drug, outpatient, and ED use from the survey and the "any hospital admission" measure from the hospital discharge data.

Table U2: Impact of Health Insurance On Different Types of Admissions

	Control mean	Reduced Form (Linear)	2SLS (Linear)	Reduced Form (alternate: Poisson)
	(1)	(2)	(3)	(4)
Panel A: Any hospital use (by type of care)				
Any hospital admission?	0.068			X
	(0.252)			
Any admission	0.029			X
not through ED?	(0.168)			
Any admission	0.049			X
through ED?	(0.216)			
Panel B: Total hospital use (by type of care)				
All admissions				
# of days	0.507			
	(3.822)			
List charges	2667.717			
	(19995.483)			
procedures	0.158			
	(1.104)			
Standardized treatment effect				
Admissions not through ED				
# of days	0.306			
	(2.387)			
List charges	1541.54			
	(12928.374)			

procedures	0.082			
	(0.723)			
Standardized treatment effect				
Admissions through ED				
# of days	0.056			
	(1.38)			
List charges	340.583			
	(8790.589)			
procedures	0.02			
	(0.345)			
Standardized treatment effect				

Note: Table investigates non-childbirth-related hospital admissions during the time period from notification date to August 31, 2009. Table reports the mean of each outcome in the control group (column 1); note that the average time period over which these statistics are computed is **15 months**. Table reports the coefficient on LOTTERY from estimating equation (1) by OLS (column 2), the coefficient on INSURANCE from estimating equation (2) by IV (column 3), and the coefficient on LOTTERY from estimating equation (1) using QMLE Poisson (Panel B) in column (4). For each outcome in Panel A we report the estimate, standard error, and per-comparison p-value; in Panel B we report the estimate, standard error, per-comparison p value and the family-wise p-value across the three individual outcomes used to create a standardized treatment effect within each hospitalization type. Standardized treatment effect reports results based on estimating equation (5) for the reduced form (column 3) or 2SLS estimates of equation (6) (column 4) and then calculating standardized treatment effects based on equation (4). For the standardized treatment effects we report the estimate, standard error, and per comparison p-value. 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 = **74851**).

Table U3: Impact of health insurance on hospital utilization for selected conditions

			Any admission		Reduced Form standardized treatment effect for utilization (days, procedures, charges) (5)
	Share of admissions (1)	Fraction of admissions through ED (2)	Control mean (3)	Reduced Form (4)	
Heart disease	5.2			.	
Diabetes	3.4			.	
Skin infection	4.1			.	
Mental disorders	13.4			.	
Alcohol or substance use	4.1			.	
Back problems	2.6			.	
Pneumonia	2.5			.	

Note: 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), the coefficient on LOTTERY from estimating equation (1) by OLS on the dependent variable "any admission of that type" (column 4) and the standardized treatment effect (column 5) estimated by equation (5) based on three outcomes (for that diagnosis): number of days, number of procedures, and list charges For each outcome we report the estimate, standard error,and per-comparison p-value. 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 = **74851**).

Table U4: Impact of Health Insurance on quality of care, as measured in hospital data

	Control mean	Reduced form	2SLS
	(1)	(2)	(3)
Panel A: Outpatient quality of care			
Ambulatory-care-sensitive condition?	0.009		
	(0.095)		
Panel B: Inpatient quality of care (conditional on any admission)			
No patient safety event			
conditional on any admission?			
Not re-admitted in 30 days			
conditional on any admission?			
Average hospital quality	0.157		
conditional on any admission?	(0.24)		
Standardized treatment effect			

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 (2) 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 effect reports results based on estimating equation (5) for the reduced form (column 2) or 2SLS estimates of equation (6) (column 3) and then calculating standardized treatment effects based on equation (4). For the standardized treatment effects we report the estimate, standard error, and per comparison p-value. 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 = **74851**) 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=**5079**). For re-admission in 30 days, the sample is limited to those admitted between the notification date and June 30, 2009 (N=**4557**). 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 U5: Impact of Health Insurance on Hospital Type

	Sample Size	Control mean	Reduced form (Logit)	p-value of test for equality
	(1)	(2)	(3)	(4)
All admissions				
Global test		X	X	
Public?		0.01		
Private?		0.06		
With choice				
Global test		X	X	
Public?		0.02		
Private?		0.057		
Non-ED with choice				
Global test		X	X	
Public?		0.007		
Private?		0.021		

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." Whether an individual has hospital "choice" is defined at the zip code level (based on the entire Oregon hospital discharge data set, not just our lottery sample) and is specific to the outcome. For the global test, it is defined as any zip code where fewer than 60% of all admissions were to the primary hospital. For the public/private comparison, it is defined as any zip code where more than 10% and fewer than 90% of admissions were to a public hospital. Table reports the sample size for each analysis (column 1), the mean in the control group (column 2) and the coefficient on LOTTERY from estimating equation (1) by Logit (column 3). The global test for hospital sorting is calculated by estimating equation (1) by Logit with admission to each of the 58 hospitals as the outcome, then testing whether the 58 coefficient on LOTTERY are equal. The p-value reported in

column 4 for the global tests is for that F-test. The p-value reported in column 4 for the public/private and high/low quality comparisons is from a t-test of the equality of the coefficients 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.

Table F1: Impact of Health Insurance on Financial Well Being: Additional Analyses

	Control mean	Reduced Form	2SLS
	(1)	(2)	(3)
Panel A: Medical Debt			
Any Medical Collection	0.272		
SE	*0.445*		
	.		
	.		
Amount owed in medical collection	2020.71		
	6789.571		
	.		
	.		
Standardized treatment effect	.		
	.		
	.		
Panel B: Non-Medical Debt			
Any non-medical collection	0.372		
	0.483		
	.		
	.		
Amount owed in non medical collection	2765.35		
	9539.82		
	.		
	.		
Standardized treatment effect	.		
	.		
	.		
Panel C: Other measures			
Current balances on all open revolving credit	2287.95		
	12268.46		
	.		

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 measured from notification date through September 2009 except for "amount owed in collection" which gives the current balance of collections as of September 30th 2009. For each outcome shown in the left hand column 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 (i.e. medical debt, non medical debt). Standardized treatment effects report results based on estimating equation (5) for the reduced form (column 2) or 2SLS estimates of equation (6) (column 3) and then calculating standardized treatment effects based on equation (4). 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. N = **49,552**.

Table F2: Impact on current access to credit

	Control mean	Reduced Form	2SLS
	(1)	(2)	(3)
Full sample			
Currently have a credit score?	0.806		
	(0.395)		
Currently have a thick file?	0.391		
	(0.488)		
Total current credit limit on all open revolving credit	7300.654		
	(25276.554)		
standardized treatment effect			
Prior credit subsample			
Credit score			
Currently have a thick file?	0.634		
	(0.482)		
Total current credit limit on all open revolving credit	12998.16		
	(32750.687)		
standardized treatment effect			

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= 49,552**; "prior credit" subsample is defined by the **55%** of the full sample that had at least one open revolving credit account prior to randomization (i.e.

in February 2008); **N= 27,808.** 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, per-comparison p-value and the family-wise p-value across the individual outcomes that contribute to the standardized treatment effect within a given sample (i.e. full sample or prior credit subsample).. Standardized treatment effects report results based on estimating equation (5) for the reduced form (column 2) or 2SLS estimates of equation (6) (column 3) and then calculating standardized treatment effects based on equation (4). 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.

Figures F1-F3: quantile analysis in survey data:
We show quantile treatment estimates for the sum of out of pocket expenditures plus debt as well as each component individually.

Figures F4-F6
Quantile treatment estimates for current balance of all collections, medical collections and non medical collections.

Table M1: Potential Mechanisms for Health Effects: Survey data

	Control mean	Reduced Form	2SLS
	(1)	(2)	(3)
Domain: Access			
Have usual place of clinic-based care?	0.499		
SE	*0.5*		
	.		
	.		
Have personal doctor?	0.489		
	0.5		
	.		
	.		
Got all needed medical care?	0.685		
	0.465		
	.		
	.		
Got all needed drugs?	0.765		
	0.424		
	.		
	.		
Didn't use ED for non ED care in last 6 months?	0.915		
	0.278		
	.		
	.		
Standardized treatment effect	.		
	.		
	.		
Domain: Quality of care			
Overall quality of care received in last six months is good/vg/exc	0.708		
	0.455		
	.		
	.		
Domain: Recommended Preventive Care			
Blood cholesterol check (ever)	0.624		
	0.484		
	.		
	.		
Blood test for high blood sugar (ever)	0.603		
	0.489		
	.		
	.		

Mammogram w/in last 12 months (women >=40 only)	0.299		
	0.458		
	.		
	.		
Pap test w/in last 12 months (women only)	0.406		
	0.491		
	.		
	.		
Standardized treatment effect	.		
	.		
	.		
Domain: Health behaviors			
Currently smoke	0.415		
	0.493		
	.		
	.		
Less physical active compared to others your age?	0.396		
	0.489		
	.		
	.		
Standardized treatment effect	.		
	.		
	.		

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). 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 reports results based on estimating equation (5) for the reduced form (column 2) or 2SLS estimates of equation (6) (column 3) and then calculating standardized treatment effects based on equation (5). 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=22867**).

Table E1: Exploratory analysis of other outcomes: survey data

	Control mean	Reduced Form	2SLS
	(1)	(2)	(3)
Happiness			
Not too happy (vs pretty or very happy)	0.406		
SE	*0.491*		
	.		
	.		
Labor force participation			
Currently employed?	0.456		
	0.498		
	.		
	.		
Work 20+ hrs at current job)?	0.358		
	0.479		
	.		
	.		
Income	13028.5		
	11835.41		
	.		
	.		
Standardized treatment effect	.		
	.		
	.		

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). 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 reports results based on estimating equation (5) for the reduced form (column 2) or 2SLS estimates of equation (6) (column 3) and then calculating standardized treatment effects based on equation (4). 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=22867**).

Table C1. Observational estimates of the effect of insurance

	Estimation from the random assignment	Any Medicaid vs. No Medicaid	Any Medicaid vs. No Medicaid (controls only)	OHP Standard vs. No Medicaid (treatment only)
	(1)	(2)	(3)	(4)
Sample size	74851	74851	44518	26772
Percent Insured	27	27	17	34
Health care use				
Financial well-being				
Health				

Note: Table explores comparability of the randomized results to observational estimates. Column (1) reports the 2SLS standardized treatment effects from Tables P1-P3 respectively. For the remaining columns, we report standardized treatment effects, computed based on OLS estimates of equation (1) and then calculating standardized treatment effects based on equation (2), but substituting for the variable LOTTERY in equation (2). In Column (2) the estimation of equation (1) is done replacing LOTTERY with an indicator for "Any Medicaid." In Column (3), the estimation of equation (1) is done replacing LOTTERY with an indicator for "Any Medicaid" and the analysis is limited to the controls only. In Column (4), the estimation of equation (1) is done replacing LOTTERY with an indicator for "OHP Standard" and the analysis is limited to the treatments only, excluding treatments with other Medicaid coverage. For each standardized treatment effect we report the estimate, standard error, and per comparison p-value. Regressions using the hospital, credit and mortality data include household size fixed effects, lottery draw fixed effects, and the pre-period version of the variables (except in the case of mortality). Regressions using the survey data include household size fixed effects survey wave fixed effects, and the interaction of the two, and are weighted using the survey weights. All standard errors are clustered on the household.

Table C2. Observational estimates of the effect of insurance in BRFSS

	Estimation from Random Assignment - OHP	Any insurance vs. No insurance	Any insurance vs. No insurance (with adjustment)
	(1)	(2)	(3)
Refused treatment bc of medical debt (last 6 months)?		-0.293 (0.006) [0]	-0.31 (0.006) [0]
Self reported health fair or poor?		0.02 (0.006) [0.001]	-0.004 (0.006) [0.45]
Number of days impaired by physical or mental health during past 30 days		2.406 (0.219) [0]	0.931 (0.208) [0]
Number of days of physical health not good during past 30 days		2.265 (0.189) [0]	0.936 (0.175) [0]
Number of days of mental health not good during past 30 days		0.606 (0.19) [0.001]	-0.252 (0.184) [0.172]
Screened positive for depression in last two weeks?		0.002 (0.002) [0.305]	0 (0.002) [0.807]

Note: Table explores comparability of the randomized results to observational estimates. Column (1) reports the 2SLS estimates previously reported for specific outcomes that we observe in national data (see Tables P1-P3). Column (2) reports OLS, Column (3) reports OLS with adjustment. Sample Size for BRFSS data is **144,829**. BRFSS data is aggregated across 2004-2009. Table reports coefficient, standard error, and p-value.

Table C3 : Comparison of OHP Control Sample to Behavioral Risk Factor Surveillance System Data

	BRFSS				OHP	
	Uninsured		All		Controls	
	Oregon	Rest of US	Oregon	Rest of Us	All	12 Month Mail Survey Sample
Female	0.487	0.492	0.519	0.552	0.558	0.594
Age	34.095	35.39	35.012	36.435	43	42.79
English Questionnaire	0.673	0.647	0.792	0.739	0.923	0.935
Insured	0	0	0.507	0.551	.	0.324
Ethnicity - Hispanic	0.391	0.5	0.284	0.408	.	0.124
Race - White	0.775	0.594	0.798	0.617	.	0.819
Race - Black	0.012	0.168	0.015	0.188	.	0.039
Education: High School/GED or below	0.734	0.754	0.657	0.703	.	0.669
Education: More than High School	0.266	0.246	0.343	0.297	.	0.331
Currently Employed	0.516	0.525	0.461	0.46	.	0.456
Income: <$10k	0.317	0.355	0.356	0.381	.	0.498
Income: 10k-<25k	0.683	0.645	0.644	0.619	.	0.402
Income: 25k-<35k	0	0	0	0	.	0.1
Household Size	4.578	4.617	4.339	4.465	.	2.989
# of Children in Household	1.684	1.611	1.571	1.612	.	0.905
Has a Doctor	0.331	0.35	0.556	0.576	.	0.489
General Health is Fair/Poor	0.286	0.327	0.29	0.336	.	0.452
# of bad physical health days in last 30 days	12.445	11.331	13.334	12.713	.	9.664
# of bad mental health days in last 30 days	12.916	12.932	13.078	13.269	.	11.259
# of poor health days in last 30 days	12.786	11.875	13.854	13.387	.	8.133
Ever been diagnosed with Diabetes	0.063	0.063	0.075	0.091	.	0.175
Ever been diagnosed with Asthma	0.174	0.119	0.221	0.158	.	0.276
Ever had a Heart Attack	0.031	0.039	0.035	0.054	.	0.104
Ever had Cholesterol Checked	0.377	0.447	0.472	0.561	.	0.624
Had a Mammogram within past 1 year	0.229	0.438	0.449	0.551	.	0.373
Had a Pap Test within past 1 year	0.771	0.562	0.551	0.449	.	0.627
Currently a Smoker	0.279	0.444	0.39	0.525	.	0.324
Total N	0.721	0.556	0.61	0.475	.	0.676

Notes: Table explores comparability of our study population to the low income population in Oregon and the rest of the US. We report means for (1) low-income uninsured adults ages 19-64 in Oregon, (2) low-income uninsured adults ages 19-64 in the rest of the US, (3) all low-income adults ages 19-64 in Oregon, (4) all low-income adults ages 19-64 in the rest of the US, (5) our control sample, and (6) our control sample limited to 12-month survey responders. Columns (1)-(4) use data from the BRFSS (pooling 2004 to 2009) and restricted to those ages 19-64. Rest of US refers to other 47 continental US states, besides Oregon. Columns (5) and (6) use lottery list variables for age and gender and data from the 12-month survey for all other variables. Mammogram and Pap Test variables are applicable only to female respondents aged 40 or over. 2008 BRFSS data is missing cholesterol check variable.

Appendices

Comment: Some of the statistics in the appendices refer to control means only (unless explicitly noted as in Appendix Tables A14-A16). Also much / most of the appendices were assembled using earlier versions of the data so the exact numbers will all change, but are useful nonetheless for general ballpark feels.

Contents

Appendix 0: Oregon Health Plan and the lottery

In 1989, the Oregon legislature passed a package of health care bills creating the Oregon Health Plan with the goal to expand coverage to families and childless adults up to 100% of the federal poverty level while controlling costs with a managed care delivery system and a prioritized list of services. To achieve this goal, Oregon applied for one of the first federal waivers of traditional Medicaid rules under Section 1115 of the Social Security Act. The waiver was approved in 1993 and enrollment in Oregon Health Plan (OHP) began in 1994 (Conviser 1995; Oregon DHS, 2006).

Starting in 2003, OHP was split into two distinct programs: *OHP Standard* and *OHP Plus.* OHP Plus serves the categorically eligible Medicaid population. The eligible population for OHP Plus is defined as pregnant women and children 0 to 18 up to 185% of the federal poverty level (FPL), people with disabilities up to the SSI income level or 300% of the SSI income level if meeting long-term care needs, and families enrolled in Temporary Assistance to Needy Families or with foster children up to 100% FPL (Oregon Office of Health Policy and Research, 2007).

OHP Standard (the subject of this proposal) covers the Medicaid expansion population, those who are financially but not categorically eligible for OHP Plus. Specifically, it provides coverage for adults (ages 19 - 64) who are Oregon residents, are U.S. citizens or legal immigrants, have been without health insurance for six months, have income below the federal poverty line, and have assets below $2,000. Prior to the split of OHP Standard and OHP Plus, OHP covered over 100,000 adults in this expansion population. Following changes made in 2003, enrollment dropped substantially. (Oregon Office of Health Policy and Research, 2007)

Due to budgetary shortfalls OHP Standard was closed to new enrollment after June 30, 2004. Gradual attrition reduced the average monthly enrollment to about 19,000, but the two-year budget period ending in June 2009 allowed for an average monthly enrollment of 24,000. The state therefore determined it had the budget to enroll an additional 10,000 adults. Therefore, in January, 2008, the Oregon Department of Human Services (DHS) announced that it would re-open the OHP Standard program. Because DHS (correctly) anticipated that the demand for the program would far exceed the 10,000 available new enrollment slots, DHS requested and received permission from the Centers for Medicare and Medicaid Services (CMS) to conduct a random drawing to add the new members.[60]

That same month, DHS launched an extensive public awareness campaign that included releasing weekly press releases and radio public service announcements, sending letters to current participants in all DHS programs for low income Oregonians (e.g., WIC, food stamps, etc), and distributing educational materials to more than 1700 community partners including advocacy groups, health care providers, health plans, and state and local service agencies. During the 5 week period from January 28 through February 29, 2008, interested individuals could add

[60] The department chose a random selection process because it gives everyone an equal opportunity to have their name drawn from the list. Adding people to OHP Standard based on health status was not allowed by Federal law, which prevents states from determining eligibility for federal programs based on health care conditions. DHS also considered selecting names on a 'first-come, first-served' basis, but rejected that option because it puts people without ready access to the information or the means to quickly get on the list at a disadvantage (Oregon DHS, 2009).

themselves and/or others on the lottery list by telephone, in person, by mail, or online. (Oregon DHS, 2009)

Oregon's Department of Human Services' Division of Medical Assistance Programs (DMAP) conducted the random lottery drawings from the reservation list. The drawings were conducted from March 2008 – October 2008 (approximately once per month). The state randomly selected individuals from the list. If an individual was selected, however, the state considered the entire *household* of the individual drawn to have won. This has two implications: first, the treatment occurs at the level of the household. Second, the nature of the selection process disproportionately favored individuals who listed more individuals in their household on the lottery sign up sheet; as a result, winning (treatment) individuals are disproportionately from larger households than the control (non winning) individuals, and we will include indicator variables for household size as controls in all of our analyses.

Since random assignment of health insurance eligibility is a key feature of our study, we have taken great care in establishing and verifying the random assignment of lottery and control selection. The lottery's random selection process was performed on Oregon's Department of Human Services (DHS)'s mainframe computer, and 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. On all the characteristics of individuals on the lottery list that we can observe (i.e. age, gender, preferred language, geographic location, etc) the mean characteristics in the actual selected were well within two standard deviations of the sample means from our 500 simulations as reported in Table A1. We are reporting this comparison for the entire original list, but the simulations were initially performed draw by draw and doing the comparison draw by draw yields similar results.

Those selected in the lottery were given the opportunity to apply for OHP Standard. The application covers all household members applying for coverage. It inquires about Oregon residence, U.S. citizenship, insurance over the last six months, household income over the last two months, and assets (Oregon DHS, Form 7210). Documentation of identity and citizenship (in the form of passports, birth certificates, etc) and income (in the form of pay stubs, letters from employers, etc) is required (Oregon DHS, Form 7222). The state reviewed applications, enrolling eligible individuals in OHP Standard (or if applicable OHP Plus). Enrolled individuals are required to reapply every six months to remain enrolled in OHP.

OHP Standard provides relatively comprehensive benefits with no consumer cost sharing. Monthly premiums range from $0 to $20 depending on income. Most care is provided through managed care organizations. Physician services, prescription drugs, and mental health and chemical dependency services are covered, but dental and vision are not. Although there are some restrictions on hospital coverage, all major benefits are covered. The hospital benefit plan has an actuarial value that is approximately 85 percent of the value of the full hospital benefit package available to individuals on OHP Plus. (Office of Health Policy and Research, 2007).

Appendix 1: Detail on our sample and our data sources

A1.1. Sample definition

The original lottery list which we received from DHS included 100,600 records, but further examination revealed that some records were duplicate copies of the same people. We reviewed the list for duplicates using the CDC's LinkPlus software. Using the software we looked for records that matched based on first name, last name, date of birth, social security number and an internal processing identification number. Two research assistants separately reviewed all potential duplicates identified by the software. We considered two records to be duplicates of the same people if the research assistants both classified them as duplicates. This process identified **8,823** duplicate records.

We removed the duplicate records from our sample. We also removed **131** test records and **2,809** additional records that had been "deactivated" by DHS and thus were not eligible to be selected in the original lottery or in our initial control selection. We had received monthly updated lists from DHS, and there were a small number of records which did not appear on the original list, but did on later lists. We also removed these **189** records from our sample. This data cleaning left us with **88,648** unique individuals on the original lottery list.

In addition, 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 **34** individuals who gave an address outside of Oregon. We only included those with birthyears between 1944 and1989 (corresponding to ages 19-64 at the end of 2008) since those outside this age range were not eligible for OHP Standard. This excluded an additional **2,093** individuals. We additionally excluded **876** individuals born in 1944 (age 63 at the end of 2008), since within 1 year both treatments and controls would be eligible for Medicare and thus not differ in insurance status. We further excluded **4,986** individuals who had given a group or institutional address when signing up for the lottery list and **5,631** individuals who had been signed up for the list by an unrelated thirdparty (such as a hospital billing office).[61] Our concern with these individuals is that they were unlikely to be effectively notified even if selected in the lottery.

Following exclusions we were left with a total of **72,700** individuals to study. Of these individuals, **29,411** were selected as treatments. Figure A1 shows the relationships between the original lottery list, our sample universe and the sample used for analysis of specific data sources. Table A2 shows differences in pre-randomization characteristics for the samples used in the analysis of specific data sources.

A1.2. Data sources

A1.2A. Administrative Data

[61] 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 0.007 percentage points, se = 0.002). We believe this is the result of how the state obtained the enrollment data which we discuss below.

We rely on administrative data for the results of the lottery (who was selected, who applied for insurance coverage and who received coverage from the state) and for detailed measures of certain key outcomes, including health care utilization and financial strain.

Matching lottery draw for controls

Because the state conducted the lottery drawings over a period of eight months, it makes sense to separate treatments into lottery draws for some analysis. For example, 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 more than 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 significant. Table A3 summarizes the assignment of households into treatment by matched lottery draw and household size.

Lottery reservation list data

Oregon's Department of Human Services' Division of Medical Assistance Programs (DMAP) provided 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. We use this self reported information to construct the following "lottery list" variables: year of birth; sex; whether English is their preferred language for receiving materials; whether the individual is signed themselves up for the lottery or was signed up by a household member[62], the number of household members on the list, whether they gave their address as a PO box, whether they signed up the first day the lottery list was open, the median household income in the zip code they gave, whether the zip code they gave is within a census-defined MSA, and whether they provided a phone number on sign up. The actual sign-up sheet is shown in **Figure A0.**[63]

Enrollment data

Oregon's Department of Human Services' Division of Medical Assistance Programs (DMAP) provided us with enrollment in the division's programs for each individual on the reservation list. They provided yearly summaries for enrollment during each year starting in 2002 and continuing through 2009. These summaries include the dates for any periods of enrollment in DMAP program and which program. DMAP also continues to provide us with weekly snapshots of enrollment until the 2010 yearly summary become available.

[62] This is different than whether the person was added to the list by a third party. In addition to whether the individual signed themselves up or was signed up by a household member, the lottery list data we received from the state include the name of the person signing up the individual. When this name did not match any of the household members on the list, we considered that a "third party" sign-up and excluded such individuals from our analysis.

[63] Note that although SSN may be provided, we do not have permission to use it in our analysis.

The enrollment data is kept by the state under a different system than the reservation list and with a different 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. 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 as successful in matching individuals to enrollment records, we may be underestimating enrollment in our controls and those overestimating our first stage. We suspect that in practice this effect is small, as we have identification numbers for over **99**% of the treatments and **88**% of the controls and around **12%** of the controls were enrolled in Medicaid during our study. Assuming the rate in those with missing identification numbers is the same as in the rest would increase that to **14**% (.12/.88) 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.

Application data

Oregon's Office of Health Policy and Research (OHPR), with the assistance of Oregon's Department of Human Services, Children and Families (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), reasons for pending, transfer or denied status, date of decision, and additional information if case was transferred.

Hospital discharge data

We worked with OHPR to obtain hospital discharge data for the entire state of Oregon and to match these data to our sample. The data are collected by the Oregon Association of Hospitals and Health Systems (OAHHS) and maintained by OHPR. These data include records for all discharges from inpatient hospitals in Oregon. They 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.[64] 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 obtained data for the entire state of Oregon for

[64] 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.

discharges occurring in 2008 and the first three quarters of 2009. This was the most recent data available when we received the data in the spring of 2010.

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. Prior to doing the match, we conducted training exercises matching lists where we had more complete information using only these matching variables. This allowed us to calibrate our assessment of potential matches (meaning agreement on any of the matching variables). Our goal was to code a potential match as a match when our estimate of the probability of its being a true match exceeded 0.5.[65] We matched to the 2008 and 2009 data separately. Overall, 59,948 of 657,790 total records were identified as potential matches, and we coded 17,391 as matches. Due to the sensitive nature of the data, after we conducted the match on site, OHPR provided us with data including the matched study identifier but excluding the personally-identifying matching variables.

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 in 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 separated pre- and post-lottery hospital utilization. For treatments, the division between the pre- and post- periods was the date of notification for winning the lottery. For controls, as described above, we randomly assigned each control a matching lottery draw. We used the notification date associated with that lottery draw as the dividing line for the pre- and post-periods. This means that the pre- and post-periods are for differ in length by draw. The pre-period averages **5** months and ranges from **2** months for the first draw to **8** months for the last draw. The post-period averages **15** months and ranges from **12** to **18** months.

Credit report data

We obtained the complete credit records for a subset of our lottery list from 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.

[65] We calculated that setting this threshold at 0.5 maximized power. According to our calculations, the optimal threshold 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 calibrate our subjective assessment of the probability of a true match to the actual probability.

Credit reports contain data gathered from three main sources: (1) public records (2) collection agencies, and (3) trade lines. Public records data – which is virtually complete – consists of information on such events as bankruptcies, liens, and judgments. Collection records contain information on accounts in collection, most of which are not credit related, 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 parties 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 – are data 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 who, with poor access to traditional credit, may rely more on non-traditional forms of credit such as borrowing from relatives and friends, rent to own "purchases", pawn shops etc which would not be reported to credit bureaus.[66]

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 the individual's data at the end of the data archive and their proprietary scoring algorithm.

Data files: 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) and some since the notification date (e.g. have you had a collection since the notification date)? We also use analogous February 2008 data to control for pre randomization values of the dependent variable in the analysis. When we examine "maximum access to credit" we use February 2009 as well as September 2009 data to define the "maximum access to credit" over our sample period; we use February 2008 and February 2007 data to define the analogous "maximum access to credit" variables. Finally, when we check for balance of treatment and controls outcomes for those who matched to the credit report data we use the February 2008 data and control for February 2007 outcomes (to be parallel to the main analysis).

Matching process and results: 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, gender and date of birth, as they reported it in signing up for the lottery.[67]

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[66] One high-profile form of non traditional credit are pay day loans. Pay day lenders have their own credit bureau. However, such loans are not necessarily an important source of credit in our population for several reasons. First, pay day lending requires that one be employed and have a pay check, while 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).

[67] 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.

This process generated a **66%** 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, without social security number to match on, match rates were expected to be substantially lower than they would be with social security number. Informal conversations with credit bureau staff suggested that (in the general population) if the address is accurate and current (which we hoped to accomplish by matching to the February 2008 file which immediately follows the time of the lottery sign up), match rates 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. Second, in a very low income population, some individuals may not have a credit file.[68] Based on the expected match probabilities we suspect that roughly 10 to 20 percent of our population had no credit file. Any individuals who appeared in the February 2008 data we then followed forward in the credit bureau's data archives to retrieve additional credit report data from them from September 2009.[69] We measure all outcomes in the September 2009 archive; we use the February 2008 archives to measure the same outcomes pre randomization, and the February 2007 archive to measure the equivalent "pre period" outcomes when analyzing balance in the February 2008 archive. We also use the February 2009 archive when defining the "maximum" of various access to credit variables over the time period from notification date through September 2009.

We were able to follow **97%** of the individuals matched to the February 2008 file forward to the September 2009 file.

Table A2 compares characteristics of the lottery list who matched to the September 2009 to those who did not.

A1.2B Survey Data

We rely on survey data for a broad measure of insurance coverage, as well as key measures of health care use, health, and financial strain. We also use survey data to look at several potential mechanisms by which health insurance may improve health: health care access, health care quality and health behavior. We conducted 3 main mail surveys: one around the time of the initial lottery (referred to as "initial survey"), one in January – May 2009 (referred to as the "6 month survey") and one in June 2009 – March 2010 (referred to as the 12 month survey). Both the initial survey and the 12 month survey had a sample size of about 60,000 – consisting in equal parts of treatment and control individuals. The 6 month survey had a smaller sample size of around 13,000. The protocol for the initial survey involved two mail surveys (the first containing a $5 cash "thank you"), followed by attempted phone contacts. Given the limited success of the phone contacts, they were abandoned in the 6 month survey; in the 12 month survey we did enhanced tracking on a subsample and phone contacts on a subsample; this was designed to boost response rates in a subsample. More detail on the sample and protocols for each survey is provided below. The content of the three surveys was extremely similar. Appendix 2 provides detail on the content as well as the slight modifications and additions made.

Mail survey sample

[68] 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.
[69] We were able to follow over **97** percent of individuals whom we found in the February 2008 data into these additional files, using the credit bureau's internal personal identifier variables.

We conducted initial mail survey in waves concurrent to the state's lottery drawings. The state provided us with each month's lottery drawings shortly after it had been completed. We then drew from the 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. Table A4 reports the sample sizes within each stratum of household size and survey wave as well as the proportion of treatments originally selected as controls.[70]

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 control (although the difference is slight; see Table A4). Therefore we will include household size dummies in all our analysis. Second, because takeup 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). As a result, we will include survey wave dummies in all of our survey analysis (and also survey wave x household size dummies for similar reasons). This survey wave is not the same as the matched lottery draw used for analysis of the administrative data.

We confirmed that we drew our control sample correctly by verifying that there is no substantive or statistical difference across treatment and control groups in the characteristics of individuals that we can observe on the lottery list (i.e. birthyear, gender, preferred language (See Table A2)).

The mail survey sample consisted of **57,553** individuals, including **29,172** treatments and **28,318** controls. Figure A2 provides more detail on the relationships between the samples for each of the specific mail surveys.

Initial mail survey

The initial mail survey was fielded between June 2008 and November 2008. 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.

[70] 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. Table A4 and all subsequent discussion report on the sample after these changes.

We received responses from **26,065** individuals, a response rate of **45** percent. The average response date of the initial survey was **August 29, 2008**. Table A5 gives detailed timing on the survey fielding and responses compared to the lottery process.

True baseline subsample

For most of our responders, our initial survey does not represent a baseline (i.e. pre-randomization) survey. Due to timing constraints in getting the information on winning individuals from the state before these individuals were notified, as well as initial funding constraints, we were unable to survey most treated individuals prior to their learning of their lottery status, although the controls were surveyed while they still thought they had a chance of being selected. However, by chance, some of our controls returned surveys and then were treated (won). For these treated individuals only we have *pre-randomization (i.e. true baseline)* information. We note however that these are not a random sample (since they are drawn from people who return surveys fast) although within this they are random. We ended up with **1,260** true baseline treatments (out of **13,105** treatment responders to our initial survey). Using the randomly assigned notification date described above, we can identify an analogous sample for our controls. Of the **12,960** control responders, **1,659** responded before the notification date and are true baseline controls. We are not currently doing anything with the true baseline sample by itself. They do have a higher response rate, but are a selected sample. We could analyze them separately (and control for pre period outcomes if we wanted to).

Six month mail survey

The six month survey sample consisted of limited subsample (n=**11,619**) of the initial survey. We over-sampled the true baseline treatments and members of their households as well as a set of matched controls. For analysis of the six month survey, we use survey weights which are proportional to the probability of being sampled.

The six month survey was fielded between January 2009 and May 2009. The survey protocol included a screener postcard and 2 survey mailings. 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 **5,411** individuals, a weighted response rate of **42** percent. In this group, the average response date to the six-month survey was **February 27, 2009**.

Twelve month mail survey

The twelve month survey was fielded between June 2009 and November 2009 with extended follow-up through March 2010. The twelve-month survey sample included the same **57,630** individuals who were in the initial mail survey. The survey sample was divided into 2 sub-samples (tiers) corresponding to a basic and more intensive follow-up protocol.

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 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.

Following the basic survey protocol, we had received **20,555** responses corresponding to a response rate of **36** percent. Of the **36,998** non-respondents to the basic protocol, we selected a subsample of **30** percent (**11,276** 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.

Individuals in this intensive follow-up subsample received phone follow-up. They also received two additional mailings. The first was a postcard providing with information for accessing the survey online, an email address and 800-number for updating contact info, and a detachable pre-paid postcard also 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, 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.

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. To avoid contaminating the data, we excluded data for those selected starting on the day they were notified of their selection in the new lottery.

Although the set of individuals in our sample who signed up for the new lottery list were not a random subset, within that group, those selected by the state were a random subset. The state provided us with the entire new list identifying those selected in each of the new lottery draws conducted during our fielding. For each new lottery draw, we excluded data collected on those selected after notification. This resulted in collected data being excluded from **34** people. We then weighted data collected after that date from those eligible for selection, but not selected, to stand in for the data that was excluded from those selected. The weights were assigned to be proportional to the inverse of the probability of not being selected conditional on being eligible for selection. These were calculated conditional on household size to reflect the state's

procedure. This can be thought of as analogous to dropping to random subsamples of non-responders for additional follow-up on fixed dates.

We received responses from **23,447** individuals, a weighted response rate of **52** percent. The average response date to the twelve-month survey was **September 23, 2009**. Enrolled treatment responders replied to the twelve-month survey an average of **12.5 months** after they were enrolled. There is considerable variation in the timing of the twelve-month survey, however, so that some enrolled treatment responders replied as early as **6 months** after they were first enrolled and some as late as **23 months** after. Table A2 compares those who responded to the 12-month survey to those who were surveyed and did not respond. Table A5 provides more detail on the timing of the twelve-month survey.

Appendix 2: Detail on definitions of the variables

For the analysis, we combine outcomes from various data sources, including the administrative data and mail survey data. Table A6 provides an overview of which data sources contribute to which domains.

A2.A Hospital Discharge Data

For each of the outcome domains in the hospital discharge data, detailed descriptions of the variables used are given below. Table A7 provides a summary of the hospital discharge data comparing all of Oregon, adults aged 19-64, all uninsured adults aged 19-64 and our lottery list control sample. Where applicable Table A8 provides detail on the distribution of the underlying variables for our specific study population.

For all the analysis, we used data at the person-level rather than the admission level (as in Table A7). We considered admissions occurring between the individual's lottery notification date and August 31, 2009. We excluded all admissions for childbirth (coded as major diagnostic category equal to 14).

Health care use

As our primary measures of utilization, we examined the total number of hospital days, the total list charges and the total number of procedures. For hospital days, list charges, and number of procedures, these variables are coded cumulatively. If an individual had two hospital stays, one of 3 days with 2 procedures and one of 2 days with 1 procedure, the variables would be defined as 5 hospital days and 3 procedures. In addition to the total number of hospital days, the total list charges and the total number of procedures, we also considered whether there were any hospital admissions.

We then further classified admissions into those through the emergency department and those not through the emergency department. For both these types of admissions, we also considered whether there was any admission, total number of hospital days, total list charges and the total number of procedures.

Selected conditions

We identified five selected conditions which were of particular interest based on their prevalence in our population. 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 A8, Panel B shows the top 10 diagnoses by classification in our controls sample. The top six 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%**), spondylosis and other back problems (**3**%) and pneumonia (**3**%). Because schizophrenia and substance-related disorders were also in the top ten, we decided to expand to the more general categories of mental diseases or disorders (major diagnostic category 19) and alcohol and drug use (major diagnostic category 20). We combined diabetes with complications with diabetes without complications for completeness. We also created a composite heart disease category including myocardial infarction, angina and arrhythmia. Table

A8, Panel C gives detail on the specific conditions which make up each of these categories and their prevalence in our sample.

Quality of care

Our measures of quality of care are based on the Agency for Healthcare Research and Quality (AHRQ) Quality Indicators. 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).

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. The full AHRQ module allows comparisons of areas based on rates of these admissions for the population of the area. For our analysis, we used the base of our study sample, and examined the percent admitted for an ambulatory-care sensitive condition. Table A8, Panel D gives detail on the specific conditions that contribute to this category.

We coded admissions as including a patient safety event using the AHRQ Patient Safety Indicators 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 so we did not include. 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). The full AHRQ module allows for comparisons of rates of these events in the population at risk for such an event. As the rates to do necessarily correlate with other measures of quality, however, the patient safety indicators may be better used to find cases with potential problems (AHRQ Guide to Patient Safety Indicators). This is how we use them, looking at the percent of patients having 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 used data the Hospital Quality Alliance process-of -care measures to assess the quality of the hospitals in our sample. These data are made publicly available from the Center for Medicare and Medicaid Services' Hospital Compare website.[71] 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

[71] 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.

related to surgical care. Higher composite scores for each condition-specific set of measures have been associated with better outcomes for those conditions (Jha, 2007; Stulberg, 2010)

We standardize each measure because some are more dispersed than others, and take an average of the standardized measures across all conditions as a summary of quality. A scatterplot of this standardized hospital quality measure against hospital quality rank for each hospital is included in Figure 4. For individuals with multiple hospital admissions, we define their average hospital quality as the length-of-stay-weighted average of hospital quality for all admissions.

Sorting across hospitals

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 non-profit (**43** of the 58 hospitals) and only a few were 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.

A2.B Credit report Data

For each of the outcomes analyzed, we provide detailed descriptions of the variables. Table A9 provides summary statistics on these outcomes comparing all Oregonians to our lottery list control sample; although our analysis variables are defined "since notification date" for comparison purposes this table provides variables defined "over the last 12 months", which is slightly smaller than our average study period look back of **16 months**.[72] Where applicable, Table A10 provides detail on the distribution of the underlying analytical variable (for our study population)

Adverse financial events

All of these outcome measures are measured from the notification date through the end of the relevant archive. We count them if there was any occurrence since the notification date, even if it was paid in full prior to the end of our study period.

For the September 2009 archive, the notification date is the actual notification date (defined by lottery draw). For the analogous measures of outcomes in the pre randomization data (February 08) which define a pseudo "notification date" by lottery draw so that the number of days between the pseudo notification date and the end of February 2008 is the same as the number of days between the actual notification date and the end of September 2009. In thinking about the comparison to annualized measures, note that the average length of our study period is **16 months.**

We examine seven measures of adverse financial events: (1) any bankruptcy (2) any lien (3) any judgment (4) any collection (5) amount owed in collection (6) any delinquency on a credit account, defined as a payment that is at least 30 days late (7) any major delinquency on a credit account, defined as at least 120 days late.[73] We further break down collections and judgments

[72] 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.

[73] Beyond 120 days late other payment status types in this category include bills charged off, sent to collection, sent to civil judgment and so forth. We follow Avery et al. (2003) and distinguish between any late payment and any

into medical and non medical components. The first three measures (bankruptcy, liens and judgments) come from public records data; the collection measures come from collection data and the last two measures (delinquencies and major delinquencies) come from trade line (credit) data. At a broad level, all are measures for unpaid bills of various kinds.

Data from public records[74]

Bankruptucy

We measure whether the individual has had any bankruptcy since the notification date. About **1.5** percent of our sample has a bankruptcy; of these, about 85 percent are Chapter 7 bankruptcy and the rest are Chapter 13.

Liens

We measure whether the individual has a tax lien taken against them since their notification date. Liens are generally taken out by governments for unpaid taxes.[75] About **2 percent** of our sample has a lien against them since our notification date. We include both paid and unpaid liens. Approximately **60%** of liens appear to be "ever" paid.[76]

Civil judgments

We measure whether the individual has had a judgment against them since their notification date. About **6 percent** of our sample has had a judgment against them. 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. Less than **10 percent** of judgments taken out since notification date have been paid by the end of our sample period.[77]

Comments: Bankruptcies, liens and judgments represent extreme right tail negative events. 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 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),

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seriously late payment (what they call "any derogatory" and "any major derogatory"). Major derogatories represent substantially more serious delinquencies, in terms of the impact on qualifying for new credit. Moreover, Avery et al. (2003) note that not all creditors systematically report non major derogatories. However in practice the fraction having any delinquency (37 percent) and the fraction having a major delinquency (30 percent) are v similar therefore in P2 we ended up opting to just look at any delinquency.

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

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

[76] 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.

[77] Of course this reflects censoring. If we look at judgments taken out between 2005 and 2007, approximately **one quarter** have been paid by the end of September 2009.

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.

This has two implications. First, these measures should be viewed as proxying for particularly large unpaid bills; in other words, they are a selected subsample. Second, and potentially more concerning, we must consider the possibility that having health insurance may influence the probability that a judgment or lien or bankruptcy is sought against an individual, conditional on a given unpaid bill. Before deciding whether to seek a judgment potential plaintiffs are likely to assess whether the subject has the resources to make good on a served judgment. We will investigate the impact of our lottery on access to credit (i.e. perceived credit worthiness) and interpret the results on these measures in this context. The tight timing makes this relatively less likely to occur.

Another concern with judgments is that a non trivial fraction of them are sought by creditors (approximately one fifth according to Avery et al (2003)). Therefore to the extent that health insurance eases access to 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 can 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.

Finally we note that while we have data on the amount of liens and judgments we are not analyzing it. As explained in the main text, we were particularly concerned about double counting of money that is e.g owed in collections and against which a judgment is also taken out.

Data from collection agencies

We measure whether the individual has had any collection reported since their notification data. Approximately **half** our sample has had a collection reported since their notification date.

We also observe the current balance on all collections; this gives us the amount still owed in collections as of the extract date.[78] Our collection amount therefore measures the current amount in collection that has not been paid. In practice, very few collections are paid. Only about **3 percent** of collections are paid; this number is about **5 percent** for non medical collections and **2 percent** for medical collections.[79]

Conditional on having a positive collection balance, the average collection balance in our sample is about **$7,400.** The distribution is quite skewed. The 10th percentile of collection balances is about **$335,** the median is about **$3,000**, the 75th percentile is about **$8,000** and the 90th percentile is about **$17,000.**

[78] 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

[79] 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.

We further distinguish between medical and non medical collections; about **30** percent of our sample has had a medical collection reported since notification date and about **40** percent has had a non medical collection reported. Medical collections account for about **40 percent** of all collection balances. Among non medical collections, Avery et al (2003) report that utility collections are the most common type; credit related collections are quite rare.

Comments

Collection data consist of (mostly non-credit-related) unpaid bills that have been sent to collection.[80] 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 a small landlord 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.

The fact that not all providers report collection attempts to the credit bureau raises concerns about non randomness of provider reporting by insurance status. This seems a priori less a concern with non medical collections (e.g. do you even get to choose your utility company?) but potentially a concern for the medical collections measure. Despite this concern about selective reporting, 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 who reports).

Potential selective reporting of collections by insurance status:

Different collection agencies follow different practice in this regard 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 spuriously find that insurance increases medical collections. Complicating such a selection story is that in practice it appears from our conversations with several Oregon hospitals that at many hospitals the practice is not uniform within the hospital; e.g. the hospital 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" harder. Still it is a potential concern with these data. 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).

In general, we consider the concern about selective reporting of collections that could be correlated with insurance status less plausible for non medical collections than for medical collections. For medical collections we can investigate this issue by examining whether or not insurance affects the sorting of patients across medical providers (see Table U5); if there is no

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

change in sorting then there is little concern with spurious results driven by differences across providers in reporting practices. Briefly, we plan to examine both non directional sorting and sorting by ownership type, since it is possible (although we have not uncovered any evidence to this affect) that collection and/or reporting practices could differ by hospital type.

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.

Data from trade lines: delinquencies

From the trade line (credit) data we obtain measures for whether the individual has had any delinquency on any credit account since the notification date, and whether the individual has had any major delinquency on any credit account since the notification date. We look at any trade lines (credit) that the individual has open since date n, including not only revolving credit but also installment loans and mortgages. About **three quarters** of our sample has any open trade line since date n; of these **three-quarters** have a revolving trade line.

Of our sample period, about **37** percent have a delinquency on any trade and about **30 percent** have a major delinquency.[81] Of those with any open trade line (credit) since the notification date, these numbers are **50%** and **40 percent** respectively. We note that for those without any open credit since the notification date – **one quarter** of our population **–** these variables are mechanically zero but this reflects not being at risk for a derogatory rather than having had a good payment pattern; this is a problem only if health insurance increases the chance one has any credit over our time period.

We follow Avery et al (2003) and further distinguish between any delinquency and a "major delinquency". A delinquency is a payment on a bill that is at least 30 days late. A "major delinquency" is defined as a late payment that is 120 days or more late. Beyond 120 days accounts are often charged off, and may be sent to collection or judgment. According to Avery et al (2003) delinquencies – and particularly major delinquencies – are important in consumer

[81] Given this distribution we thought of analyzing only major delinquencies but were concerned about right censoring (since our sample period is only about 12 months).

credit evaluations[82]; not all creditors systematically report minor delinquencies (Avery et al (2003). page 62), which is another reason to look only at major delinquencies.

Access to credit

We measure access to credit over two time frames. Our main substantive analysis focuses on access to credit at the end of our study period (September 2009). 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..

We have three measures of access to credit: (1) credit score (2) Do you have at thick file? And (3) Total credit limit across all open revolving credit. For the measures for our end of study period these are taken as the current measures as of the end of September 2009.
Total credit limit across all open revolving credit is constructed – 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.

Credit scores: Credit bureaus use the data in credit reports to generate a "credit score" for the individual, which we also analyze.[83] This provides a measure of the market's assessment of the indivdiual'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, or worse) 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.

About **20%** of our sample does not have a credit score (indicating too little contact with formal credit markets to be able to generate a score)[84]. Moreover, among those who do have credit scores, their scores tend to be extremely low. **About 40 percent** have a score that puts them in the "high risk" category which means that they are likely to be turned down by lenders (i.e. grade of E), and another **thirty percent** have a score in the "non prime" category which means that they can get access but on less favorable terms than typical (i.e. grade of D); only about **one**

[82] 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."
[83] The score is for an individual, not a household.
[84] This number is likely even higher in the entire lottery population since one way to generate a match to our February 08 credit report data is to have enough contact with formal credit markets to have a credit score. Thus our sample presumably disproportionately has a credit score.

quarter of those with scores (or about **19%** of the whole population) have scores that would qualify them for credit on reasonably favorable terms.[85]

Thick file This is defined as having two or more open trade lines at the end of the study period; it 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 a credit score has a thick file).

(3) Total credit limit across all open revolving credit. About **half** of our full sample and **about 12 percent** of the prior credit subsample has no open revolving credit in our study period; their credit limit is set to zero.

Note that the total credit limit variable is the one variable that is defined differently in the February 2009 data archive. Specifically in that data archive we do observe the total credit limit across all revolving credit, whether opened or closed (as opposed to just open revolving credit). As a result, this variable is mechanically higher in February 2009 and therefore when used to define "maximum credit" over the February 2009 and September 2009 data archive we get a substantially larger "maximum credit" than "current credit" (compare column 1 of Tables S2 and F2). This however should not pose any problem for the analysis of maximum access as the data discrepancy is symmetric across treatments and controls.

Finally, in parallel to our "credit limit across all open revolving credit" we define a *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 / stale once an account is closed; in addition one presumably no longer as "access" to the credit limit on a closed account).

Comment: comparing our population to typical Oregonians (Table A9)

As noted, Table A9 provides a comparison of our main outcomes for our lottery list control sample and for all Oregonians. 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.[86] 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

[85] The credit score we use is called the "VantageScore". 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" to ("E") letter grade system is that an A is 901 – 990, a B 801 -900, etc down in 100 point intervals. (see e.g.; http://www.mortgagefit.com/credit-rating/vantagescore.html**.** Borrowers with grades of C (701-800 prime) through A (901-990 super prime) generally get access to credit at reasonably favorable terms. Those with grades of D (601-700; non prime) will encounter difficulty getting credit but may be able to do so at less favorable terms while those with scores of E ("high risk", 501-600) generally get turned down.

[86] 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).

$23,000 for the general population. Conditional on having any positive credit limit, these numbers are about $16,000 and about $40,000 respectively.[87]

A2.C Mail Survey Data

For each of the outcome domains in the survey data, detailed descriptions of the variables used are given below. Table A11 provides detail on each of the outcome variables including the reference timeframe of the question, the sample size for that question and the percent of twelve-month survey responders for whom we have data for that question. We analyze many of the variables as dichotomous transformations of continuous or categorical variables. Table A12 provides detail on the distribution of the underlying variables.

All measures were self-reported on the 12-month mail survey. In the descriptions below the relevant question number of the twelve-month survey is referenced for each outcome. The survey instrument itself is included on Figure A3 to provide the exact wording of each question. The survey instruments for the initial and six-month surveys were very similar, and Table A13 details the slight differences between the surveys. The survey instrument was designed by the study team. Each version was revised based on our experience with the previous one. All versions were pilot tested on individuals on the reservation list but not in our survey sample, and revised to improve clarity and flow.

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 (intensive margin). All of the intensive 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 A12).

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). In the absence of a question to apart, we worked with survey experts to design a question breaking medical expenditures into several large categories for specific types of care (Q21a-d).

[87] 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.

We asked whether participants had paid any out-of-pocket medical expenses in the past six months (Q20). If the participant responded no, but later indicated that they had paid out-of-pocket medical expenses for a specific type of care, we considered the response to be yes.

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 out-of-pocket expenses, we created a variable for the total out-of-pocket expenses constructed from the out-of-pocket expenses reported for various types of medical care (Q21a-d) and the total amount owed (Q22). We treated missing amounts as reports of zero paid or owed for the purpose of this sum. Table A12 provides more detail on the distributions of each component of this total as well as the total itself.

General health status

Our measure of general health status included 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. We examined whether the participant reported being in fair or poor health as compared to excellent, very good or good health. We also examined being in poor health as compared to all others. We also examined the number of days (of the last 30) the participant reported having not good physical health, having not good mental health and having poor health interfere 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.

Depression was assessed using the two-question version Patient Health Questionnaire (Kroenke et al, 2003). The questions 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") Each of the two questions was scored 0 – 3 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 the questions and improve the question 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 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).

As further assess access to outpatient care, we examined whether participants using 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 compared to fair or poor. This is not defined if the participant reported not having received medical care.

Preventative care

For preventative 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 preventative care measures, we examined whether the participant reported ever having had the test compared to never. We expect that most of the effect would be on care within the last year. Because some of the treatments responded to the twelve-month survey more than a year after receiving insurance, however, we are concerned that we will miss an early boost in the use of preventative care if we only look 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 assign each individual 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 censor income at $50,000.

Appendix 3: Detail on some econometrics

3.1Quantile treatment effects and quantile IV
To do.

.

3.2Standardized treatment effects

Note that in both the ITT version (equation 3) and the ToT version (equation 5) of the standardized treatment effects estimators treat the standard deviation of the outcome for the control group (i.e. σ_j) as known (see also discussion in Kling and Liebman (2004), especially equation 11).

To investigate the sensitivity of our results to this assumption, we could use the delta method to calculate the standard error of the standardized treatment effect given in equation (2) based on a pooled OLS system that "stacks" the estimated mean effects (i.e. equation 3 or equation 5) with a series of estimates of the standard deviation for the control group. Specifically let ε_j be the deviation from the mean of y_j for members of the control group (note therefore ε_j is only defined for members of the control group).
i.e. $\varepsilon_j = y_{ij} - \bar{y}_j$ where we look over i's in the control group only.

Therefore we estimated the pooled (stacked) ols in which the first stack is as in equation (3) or (5) and the remainder of the stack is:

$$\varepsilon^2 = (I_j)\theta + \varepsilon$$

Then the square root of the estimated coefficients from the second set of stacks gives you the estimate of σ_j_hat. Then can use delta method (see "nlcom" in stata) to test whether the average of the standardized coefficients is 0.

Appendix 4: Additional results

Comparison of balance of treatment and control.

We performed some additional balance checks by looking at the balance of treatment and controls on outcomes we can examine prior to randomization. In Table A14 we examine the balance of treatment and controls of the entire study population to pre-randomization hospital outcomes. In Table A15 we examine the balance of treatment and controls who matched to the credit report on their pre-randomization credit outcomes. In Table A16 we examine the balance of treatment and control survey respondents on selected pre-randomization credit report and hospital outcomes that reasonably closely approximate outcomes we measure in the survey data.

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Appendix Figures and Tables

Figure A0: OHP 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)

Figure A1: Overlapping samples

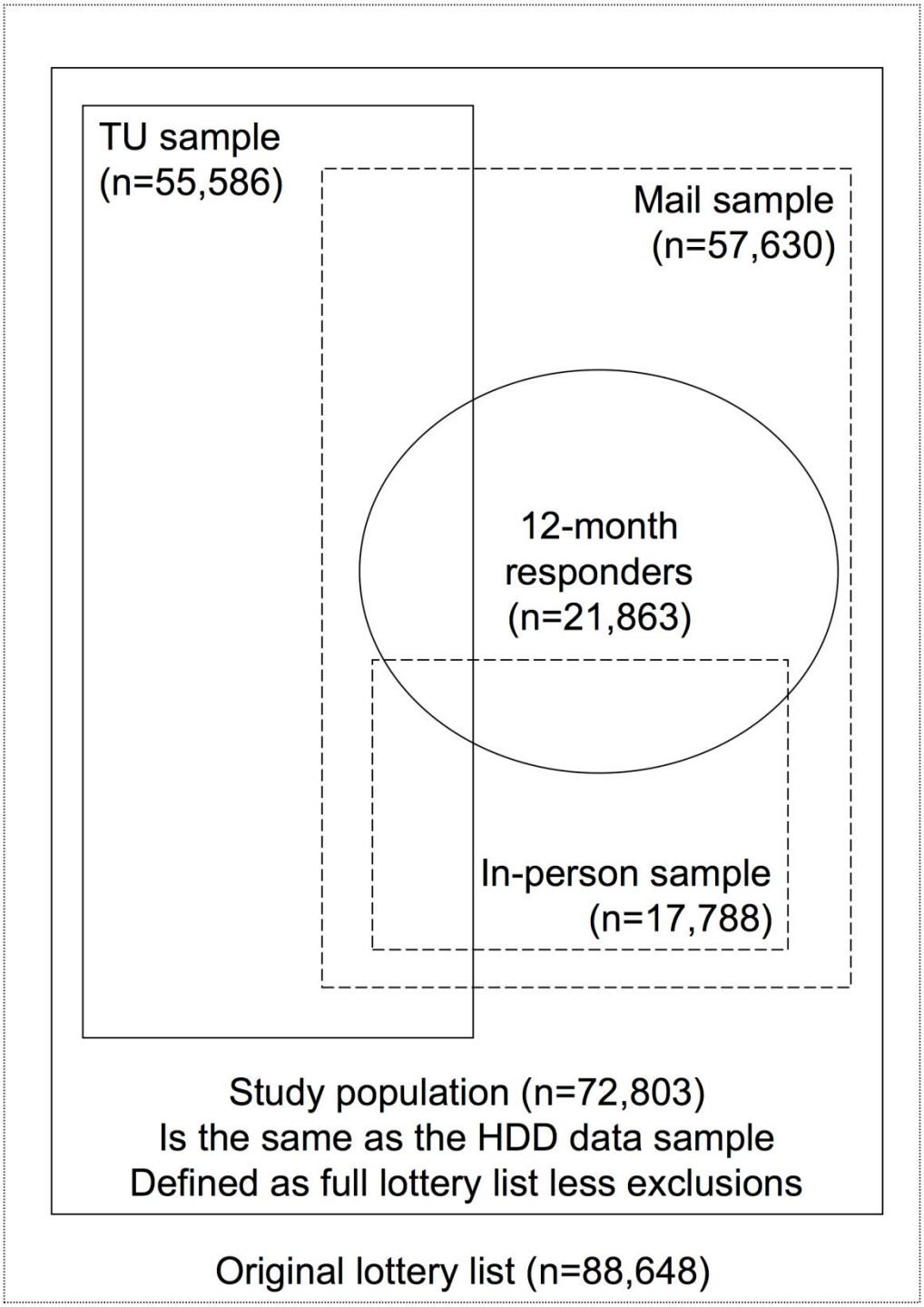

Figure A2: More detail on mail samples

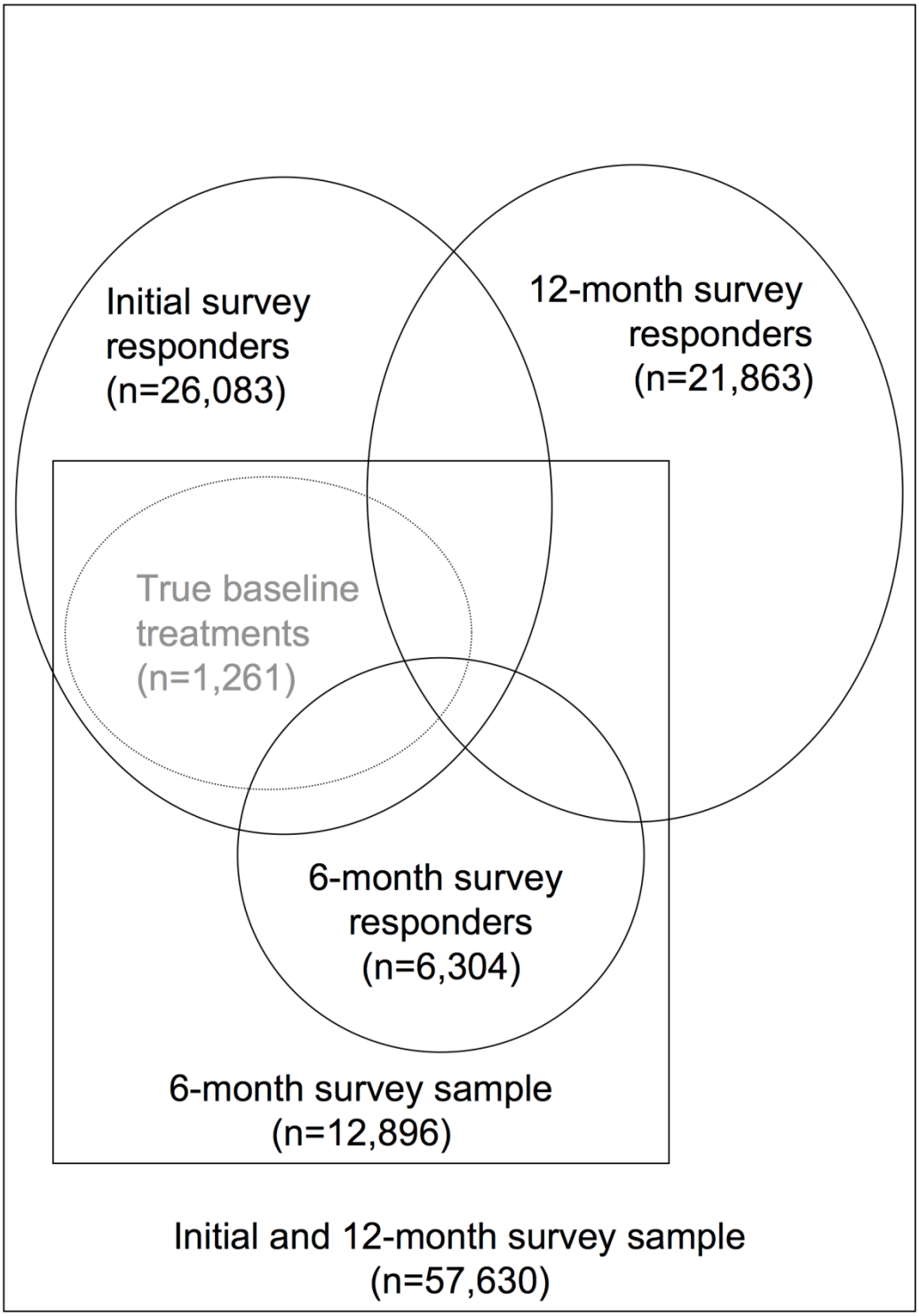

Figure A3: Twelve-month survey instrument

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**.

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)**

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): _____

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)**

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!

Figure 4: Scatterplot of Hospital Quality Rank against Hospital Quality

Table A1: 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)
Panel A: Lottery list variables (Jan / Feb 2008)				
Year of birth?	1967	1967	0.081	0.804
Female?	0.52	0.528	0.002	1.529
English as preferred language?	0.93	0.93	0.001	0.604
Signed self up?	0.84	0.843	0.001	0.118
Signed up first day of list?	0.09	0.089	0.002	0.45
Have phone?	0.85	0.848	0.002	0.312
Address a PO Box?	0.13	0.13	0.002	0.403
Median household income of zip code	38885	38839	0.345	0.903
In MSA?	0.75	0.746	0.003	0.438

Table A2: Differences in baseline (pre-randomization) characteristics between different samples

	Entire study population	Matched credit report subsample	Full survey sample	Survey responders	Survey non responders
	(1)	(2)	(3)	(4)	(5)
Year of birth?	1967.98	1967.21	1968.07	1966.29	1969.95
sd	*12.269*	*12.076*	*12.152*	*12.177*	*11.854*
Female?	0.558	0.572	0.552	0.59	0.524
	0.497	*0.495*	*0.497*	*0.492*	*0.499*
English as preferred language?	0.923	0.93	0.911	0.917	0.901
	0.267	*0.255*	*0.285*	*0.277*	*0.299*
Signed up self?	0.916	0.909	0.875	0.881	0.872
	0.277	*0.287*	*0.33*	*0.324*	*0.334*
Signed up first day of list?	0.095	0.102	0.092	0.101	0.08
	0.293	*0.302*	*0.289*	*0.301*	*0.271*
Have phone?	0.861	0.866	0.866	0.913	0.822
	0.346	*0.34*	*0.341*	*0.282*	*0.382*
Address a PO Box?	0.117	0.121	0.117	0.127	0.102
	0.321	*0.326*	*0.321*	*0.332*	*0.302*
In MSA?	0.773	0.78	0.769	0.752	0.789
	0.419	*0.414*	*0.422*	*0.432*	*0.408*
Median household income of zip code	39250.5	39522.5	39328.6	39234.1	39518.2
	8457.894	*8506.274*	*8538.418*	*8441.891*	*8836.248*
N	74851	49552	59260	22867	10342

Note: Table reports differences in baseline characteristics across various subpopulations. Each columns reports means (and standard deviations). Column (1) reports the full sample (used in the hospital discharge data analysis); column 2 reports the subsample that matched to the credit report data from September 2009 (which is the sample we analyze in the credit report data); column 3 reports the subsample that was surveyed; columns 4 and 5 respectively report results for survey responders and non responders. All the covariates shown in the left hand column are taken from the pre-randomization lottery list.

Table A3: Treatment and control sample sizes by matched lottery draw and household size

Matched Lottery Draw	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	All
In household size 1	5591	5625	5722	5440	5559	11022	10957	5759	55674
Number treated	1969	1981	2015	1916	1958	3882	3859	2028	36066
Percent treated	35	35	35	35	35	35	35	35	35
In household size 2	1938	1891	1901	1817	1660	3347	2952	1346	16852
Number treated	1110	1082	1088	1040	950	1916	1690	774	9650
Percent treated	57	57	57	57	57	57	57	57	57
In household size 3	62	59	20	6	0	15	9	3	174
Number treated	54	52	17	6	0	12	9	3	153
Percent treated	87	88	85	100	0	80	100	100	88
Total sample size	7591	7575	7643	7263	7219	14384	13918	7107	72700

Note: This table categories treatments by the lottery draw in which they were selected and controls by their household size and matched lottery draw.

Table A4: Treatment and control sample sizes by survey wave and household size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	All
Total treatment sample	4241	4230	4260	3894	3938	5075	2607	968	29213
In household size 1	2574 60.7	2561 60.5	2606 61.2	2423 62.2	2471 62.7	3533 69.6	2508 96.2	966 99.8	19642 67.2
In household size 2	1607 37.9	1611 38.1	1622 38.1	1471 37.8	1467 37.3	1542 30.4	99 3.8	2 0.2	9421 32.2
In household size 3	60 1.4	58 1.4	32 0.8	0 0	0 0	0 0	0 0	0 0	150 0.5
% sampled as controls	27	27	27	38	40	31	10	0	28
Total control sample	2268	2225	2173	4129	4180	7061	4161	2220	28417
In household size 1	1606 70.8	1611 72.4	1560 71.8	2886 69.9	2906 69.5	4645 65.8	3945 94.8	2218 99.9	21377 75.2
In household size 2	657 29	605 27.2	606 27.9	1243 30.1	1274 30.5	2416 34.2	216 5.2	2 0.1	7019 24.7
In household size 3	5 0.2	9 0.4	7 0.3	0 0	0 0	0 0	0 0	0 0	21 0.1
% of sample treated	65	66	66	49	49	42	39	30	51
Total sample size	6509	6455	6433	8023	8118	12136	6768	3188	57630

Note: This table categories treatment status based on eventual treatment status, not treatment status at time of survey wave. The row "sampled as controls" gives the number and percent of treatments in each wave that were initially sampled as controls.

Table A5: Timing of lottery selection, survey mailing and response

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Draw*	1	2	3	4	5	6	7	8	All
Lottery (2008)									
Advance Notification	not sent	not sent	4/16/08	5/9/08	6/11/08	7/14/08	8/12/08	9/11/08	n/a
Applications Mailed	3/10/08	4/7/08	5/7/08	6/6/08	7/7/08	8/7/08	9/5/08	10/7/08	n/a
Retroactive Insurance Date	3/11/08	4/8/08	5/8/08	6/9/08	7/8/08	8/8/08	9/8/08	10/8/10	n/a
Applications Due	5/31/08	5/23/08	6/23/08	7/24/08	8/22/08	9/22/08	10/23/08	11/24/08	n/a
Avg Application Decision	4/28/08	5/28/08	7/3/08	8/1/08	8/31/08	10/6/08	11/8/08	11/28/08	n/a
Initial Survey (2008)									
Earliest survey mailing	6/16/08	7/7/08	7/14/08	7/23/08	7/23/08	8/21/08	9/7/08	10/3/08	n/a
Avg survey response time (days)**	34.5	26.5	27.4	30.9	30.8	30.8	31.3	23.9	
Months between lottery and mailing***	3.2	3	2.9	2.5	1.4	1.3	0.9	0.7	
Months btw lottery and avg response***	4.4	3.9	3.8	3.5	2.4	2.3	1.9	1.5	
Months btw decision and response (avg)+	1.8	1.4	0.9	0	-0.6	-0.4	-0.6	-1.1	
Six Month Survey (2009)									
Earliest survey mailing	1/12/09	1/21/09	1/25/09	2/5/09	2/13/09	2/23/09	3/2/09	3/23/09	n/a
Avg survey response time (days)**	20.7	17.9	18.5	21.2	20.6	16	19.5	11.6	
Months between lottery and mailing***	10.1	9.5	9.3	8.9	8.1	7.4	6.6	6.3	
Months btw lottery and avg response***	10.8	10.1	9.9	9.6	8.8	7.9	7.3	6.7	
Months btw decision and response (avg)+	6.2	6.2	6.3	5.2	5.0	4.9	4.8	4.3	

(continued on next page)

Table A5: Timing of lottery selection, survey mailing and response (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Draw*	1	2	3	4	5	6	7	8	All
Lottery (2008)									
Advance Notification	not sent	not sent	4/16/08	5/9/08	6/11/08	7/14/08	8/12/08	9/11/08	n/a
Applications Mailed	3/10/08	4/7/08	5/7/08	6/6/08	7/7/08	8/7/08	9/5/08	10/7/08	n/a
Applications Due	5/31/08	5/23/08	6/23/08	7/24/08	8/22/08	9/22/08	10/23/08	11/24/08	n/a
Avg Application Decision	4/28/08	5/28/08	7/3/08	8/1/08	8/31/08	10/6/08	11/8/08	11/28/08	n/a
Twelve Month Survey (2009)									
Earliest survey mailing	6/25/09	7/9/09	7/23/09	8/3/09	8/6/09	8/11/09	8/14/09	8/14/09	n/a
Avg survey response time (days)**	41.2	35.3	32.8	35.1	37	39.1	37.2	42	
Months between lottery and mailing***	15.5	15.1	15.2	14.8	13.8	12.9	12.1	11.1	
Months btw lottery and avg response***	16.9	16.2	16.3	16	15.1	14.2	13.3	12.5	
Months btw decision and response (avg)+	14.4	13.9	13.4	12.4	11.9	11.5	10.8	9.8	

*Some in the treatment group were surveyed as controls before being selected in the lottery. They are then associated with two different draws, the survey draw in which they were surveyed initially and the lottery draw in which they were selected. Thus, although surveys were never mailed before lottery notification for the corresponding draw, some treatments were surveyed (and responded) before being notified of status.

** Average survey response time is calculated as the difference between the survey return date and the earliest survey mailing date. This difference is calculated individually, and then averaged for each draw.

***Months between lottery and survey mailing is calculated as the difference in days between the earliest mailing date and the advance notification date (both dates given in chart). Months between lottery and average response is calculated as the difference between the average survey response date and the advance notification date (given in chart). For both calculations, days are converted to months by dividing by 30.4 as the average number of days in a month. For draws 1 and 2 which did not include advance notifications, the date of the application mailing is used instead.

****Average months between decision and average response is calculated as the average of the difference in days between the individual's application decision date and the individual's response date, limited to those enrolled in any OHP insurance. As above, days are converted to months by dividing by 30.4.

Table A6: Data sources and data domains

	Lottery List Data	Application Data	Eligibility Data	Credit report Data	Hospital Discharge Data	Mail Survey Data
Pre-lottery characteristics	X			X	X	X*
Insurance status		X	X			X
Demographics	X	X				X
Health care use					X	X
Financial strain				X		X
Health status						X
Access						X
Quality					X	X
Preventive care						X
Health behavior						X
Other outcomes						X

*****The mail survey data only provided pre-lottery characteristics for true baseline treatments and matched controls.

Table A7: Comparison of hospital admissions (different samples)

	All admissions		Admissions for adults aged 19-64		Admissions for uninsured adults aged 19-64		Admissions for lottery sample controls	
	N	%	N	%	N	%	N	%
All	462861	100	224460	100	30458	100	6778	100
By gender:	.	.	.	.	.	.	.	.
Male	217538	47	107485	48	17086	56	3237	48
Female	245323	53	116975	52	13372	44	3541	52
By type of admission:	.	.	.	.	.	.	.	.
Non-ED	214499	46	108909	49	8612	28	2353	35
All ED	248362	54	115551	51	21846	72	4425	65
By length of stay:	.	.	.	.	.	.	.	.
1-2 days	194270	42	103540	46	14852	49	2856	42
3-4 days	131149	28	59510	27	7872	26	1799	27
5 or more days	137442	30	61410	27	7734	25	2123	31
By number of procedures:	.	.	.	.	.	.	.	.
None	173649	38	77101	34	12980	43	3136	46
One	109550	24	55507	25	7160	24	1420	21
Two or more	179662	39	91852	41	10318	34	2222	33
By list charges:	.	.	.	.	.	.	.	.
Less than 5,000	34043	7	16083	7	2111	7	557	8
5,000 – 9,999	88717	19	42014	19	7064	23	1555	23
10,000 – 24,999	189809	41	94445	42	13795	45	2885	43
25,000 or more	150292	32	71918	32	7488	25	1781	26
By condition:	.	.	.	.	.	.	.	.
Mental disorders	20960	5	16417	7	2051	7	906	13
Alcohol/substance	5451	1	4759	2	1122	4	278	4
Heart disease	47377	10	15408	7	2134	7	351	5
Diabetes	7213	2	4664	2	1069	4	229	3
Skin infection	8354	2	5250	2	1422	5	276	4
Back Problems	15871	3	10011	4	379	1	177	3
Pneumonia	17563	4	5186	2	848	3	170	3
Quality measures:	.	.	.	.	.	.	.	.
Ambulatory-care	56489	12	20031	9	3716	12	797	12
Patient safety event	7090	2	3136	1	259	1	73	1
Readmitted								

Average quality								
By hospital ownership:	.	.	.	.	.	.	.	.
Public	60312	13	30410	14	4096	13	924	14
Non-profit	389449	84	188471	84	25661	84	5691	84
For-profit	13100	3	5579	2	701	2	163	2

Notes: All analyses 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 742 stays for our control sample.

Table A8: Additional detail on hospital measures (our study population)

Panel A: Detail on distributions conditional on any hospital admission

	Mean	SD	25th %tile	Median	75th %tile	95th %tile	99th %tile
Number of separate hospital stays	1.57	1.37	1	1	2	4	7
Total length of stay (days)	7.31	12.64	2	3	7	26	66
Total number of procedures	2.32	3.63	0	1	3	9	16
Total list charges	39134.62	70927.11	11260.45	19915.85	39557.65	130648.3	303551.2

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.

Panel B: Selected conditions in our control sample

Condition	N	Percent of category	Percent of all admissions
Mental	906	100	13.4
Mood disorders	681	75.2	10
Schizophrenia and other psychotic disorders	156	17.2	2.3
Adjustment disorders	25	2.8	0.4
Anxiety disorders	21	2.3	0.3
Miscellaneous disorders	9	1	0.1
Personality disorders	5	0.6	0.1
Delirium, dementia, and amnestic and other cognitive disorders	4	0.4	0.1
Suicide and intentional self-inflicted injury	4	0.4	0.1
Impulse control disorders, NEC	1	0.1	<0.1
Substance	278	100	4.1
Alcohol-related disorders	195	70.1	2.9
Substance-related disorders	83	29.9	1.2
Heart	351	100	5.2
Coronary atherosclerosis and other heart disease	95	27.1	1.4
Congestive heart failure; nonhypertensive	90	25.6	1.3
Acute myocardial infarction	89	25.4	1.3
Cardiac dysrhythmias	65	18.5	1
Conduction disorders	6	1.7	0.1
Cardiac arrest and ventricular fibrillation	6	1.7	0.1
Diabetes	229	100	3.4
Diabetes mellitus with complications	226	98.7	3.3
Diabetes mellitus without complication	3	1.3	<0.1
Skin	276	100	4.1
Skin and subcutaneous tissue infections	276	100	4.1
Back Problems	177	100	2.6
Spondylosis; intervertebral disc disorders; other back problems	177	100	2.6

Pneumonia	170	100	2.5
Pneumonia (except that caused by TB or STDs)	170	100	2.5

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

Panel C: List (in order) of top 10 clinical conditions and their share.

	N	Frequency (%)
Mood disorders	681	10.05
Skin and subcutaneous tissue infections	276	4.07
Diabetes mellitus with complications	226	3.33
Alcohol-related disorders	201	2.97
Spondylosis; intervertebral disc disorders; other back problems	177	2.61
Pneumonia (except that caused by tuberculosis or sexually transmitted disease)	170	2.51
Schizophrenia and other psychotic disorders	156	2.3
Pancreatic disorders (not diabetes)	148	2.18
Substance-related disorders	131	1.93
Asthma	124	1.83

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

Panel D: What CCS diagnoses occur when an ambulatory sensitive event is flagged?

	N	Frequency (%)	Cumulative Frequency (%)
Diabetes mellitus with complications	221	27.73	27.73
Pneumonia (except that caused by tuberculosis or sexually transmitted disease)	138	17.31	45.04
Asthma	120	15.06	60.1
Chronic obstructive pulmonary disease and bronchiectasis	95	11.92	72.02
Congestive heart failure; nonhypertensive	78	9.79	81.81
Urinary tract infections	56	7.03	88.83
Fluid and electrolyte disorders	25	3.14	91.97
Essential hypertension	17	2.13	94.1
Appendicitis and other appendiceal conditions	16	2.01	96.11
Hypertension with complications and secondary hypertension	14	1.76	97.87
Coronary atherosclerosis and other heart disease	12	1.51	99.37
Other	5	0.63	100

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

Table A9: Credit bureau summary statistics for lottery population compared to all of Oregon.

	Lottery list (Control Sample)	All of Oregon
	N = 28,562	N=4,464,555
Adverse financial events		
Any bankruptcy in last 12 months	0.011	0.006
Any lien in last 12 months	0.017	0.006
Any judgment in last 12 months	0.052	0.017
Any collection in last 12 months	0.47	0.13
Total current collection amount	4808.748	975.4054
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	0.33	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	765
Currently have a thick file	0.37	0.4
Currently have an open revolving credit account	0.43	0.48
Mean total current credit limit	$9,939	$23,487
Median total current credit limit	700	1096
Mean total current credit limit (conditional on positive)	$16,228	$41,112
Median total current credit limit (conditional on positive)	5000	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 A10: Distribution of underlying analytical variables.

Panel A: Distribution of number of events (percent)

Variable	0	1	2	3	4	5	6	7	8	9	10	10+
# of bankruptcies	98.6	1.4	0.2									
# of liens	97.80	1.85	0.29	0.04	0.02	0.00						
# of judgments	93.62	5.52	0.73	0.10	0.03							
# of collections	49.51	16.12	10.65	7.30	4.91	3.13	2.22	1.60	0.97	0.88	0.64	2.09
# of medical collections	71.23	12.57	6.15	3.49	1.87	1.17	0.91	0.53	0.58	0.33	0.22	1.07
# of non medical collections	60.53	17.81	9.78	5.23	2.87	1.51	0.89	0.47	0.32	0.23	0.11	0.26

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, conditional on positive

	% positive	Mean	SD	10^{th} pctile	25^{th} pctile	Median	75^{th} pctile	90^{th} pctile	95^{th} pctile
All collections	65.20	7377.628	14371.05	335	1069	3196	8060	17410	27455
Medical collections	50.01	4066.241	9178.617	190	473	1368	3906	9679	16777
Non medical collections	55.43	5009.202	12492.39	204	634	1817	4779	11409	18789

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

	Fraction at risk – i.e. with any open credit (trade line) since notification date	Conditional on having any open trade line since notification date		
		Fraction with no delinquencies	Fraction with only minor delinquencies (<120 days late)	Fraction with major delinquencies (120 days + late)
Full sample	.7360829	.4991914	.191543	.4004471
Prior credit subsample	.9438792	.545698	.2164714	.3453632

Notes: 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

	Mean	SD	10^{th} pctile	25^{th} pctile	Median	75^{th} pctile	90^{th} pctile	95^{th} pctile
Conditional on having any credit score	651.2673	112.0916	522	563	629	719	830	869
Conditional on having any credit score in "prior credit" subsample	683.9377	114.9074	542	592	668	767	854	886

Panel E: Distribution of credit score grades

	A	B	C	D	E
Conditional on having any credit score	2.38%	10.87%	15.71%	31.10%	39.94%
Conditional on having any credit score in "prior credit" subsample	3.51%	15.78%	20.91%	32.10%	27.70%

Panel F: Distribution of credit limits

	% positive	Mean	SD	10th pctile	25th pctile	Median	75th pctile	90th pctile	95th pctile
Total credit limit on open revolving credit, conditional on positive	48.14	14449.42	34137.35	300	1000	4500	14900	35000	57800
Credit limit, conditional on positive in "prior credit" subsample	82.27	15150.26	34936.7	385	1153	5000	15865	36500	60100

Table A11: Summary of analytic variables from the mail survey data

	Time frame of question	Survey question	Non-missing data N	Non-missing data %
Health Care Use: Extensive				
Any prescription drugs?	Current	Q12	18067	77
Any outpatient visit?	Last 6 months	Q15	23216	99
Any ER visit?	Last 6 months	Q16	23236	99
Any inpatient hospital visit?	Last 6 months	Q18	23297	99
Health Care Use: Intensive			18057	77
Number of prescription drugs	Current	Q12	23167	99
Number outpatient visits	Last 6 months	Q15	23206	99
Number ER visits	Last 6 months	Q16	23297	99
Number inpatient hospital visits	Last 6 months	Q18	23146	99
Financial Strain of health care costs			23174	99
Owe any out of pocket medical expenses?	Last 6 months	Q20	23133	99
Currently owe money?	Current	Q22	22310	95
Borrowed money for medical bills?	Last 6 months	Q23	23077	98
Refused treatment because of medical debt?	Last 6 months	Q24	23122	99
Health status			21627	92
Self-reported health % Fair or Poor	Current	Q26	21121	90
How has health changed over last six months? % Gotten worse	Last 6 months	Q27	21345	91
Number of days impaired by physical or mental health	Last 30 days	Q30	23088	98
Number of days of physical health not good	Last 30 days	Q28	23075	98
Number of days mental health not good **(M)**	Last 30 days	Q29	23223	99
Screened Positive for Depression? (**M)**	Last 2 weeks	Q34	22635	96
Access:			22560	96
Have usual place of care?	Current	Q3	23256	99
Have a personal doctor?	Current	Q5	16064	68
Got all needed medical care?	Last 6 months	Q6, Q7	23115	99

Got all needed drugs?	Last 6 months	Q9, Q10	23098	98
Used ER for non ER care?	Last 6 months	Q16, Q17	7799	99
Quality			7775	99
Overall quality of care (condl on receipt)	Last 6 months	Q19	22828	97
Preventive care?			23137	99
Blood cholesterol check	Last 12 months	Q37	18067	77
Blood test for high blood sugar	Last 12 months	Q38	23216	99
Mammogram (women only)	Last 12 months	Q39	23236	99
Pap test (women only)	Last 12 months	Q40	23297	99
Health behavior			18057	77
Smoke		Q41, Q42	23167	99
			23206	99
Other			23297	99
Overall happiness (not too happy vs. very or pretty happy)	Current	Q25	23146	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 A12: Distribution of raw survey answers (limited to control sample)

Panel A: Health care use

	Percent reporting any use	Mean	SD	Median	75th %tile	95th %tile	Cutpoint for censoring	% of data censored
Prescription drugs (Q12)	64.1	3.6	2.8	3	5	9	24	0
Outpatient visits (Q15)	57.1	3.2	3.2	2	3	9	30	0.4
ER visits (Q16)	25.3	1.8	1.3	1	2	4	10	0.3
Inpatient hospital visit (Q18)	6.8	1.4	0.7	1	2	3	4	0.3

Panel B: Financial strain

	Percent reporting expenses	Mean	SD	Median	75th %tile	95th %tile	Cutpoint for censoring	% of data censored
Out of pocket expenses								
Doctor visits (Q21a)	39.4	375.8	2172.6	150	300	1000	n/a	n/a
ER or hospital (Q21b)	8.4	1463.6	4191.1	400	1200	5000	n/a	n/a
Prescription drugs (Q21c)	42.7	213.3	618.2	96	200	715	n/a	n/a
Other medical (Q21d)	13.3	681.7	6073.5	150	350	1700	n/a	n/a
Total owed (Q22)	55	4462	10449.1	1000	3500	20000	100000	0.4
Total out of pocket or owed	66.1	3452.1	8640.8	750	2580	15324	100000	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	% (of non-missing data)
General health (Q26)		
Excellent	536	4.6
Very Good	1813	15.7
Good	3927	33.9
Fair	3679	31.8
Poor	1628	14.1
Health changed (Q27)		
Better	1293	11.1
Same	6920	59.6
Worse	3393	29.2
Depressed? (Q34)		
Not at all	4336	37.4
Several days	4057	35
More than half the days	1413	12.2
Nearly every day	1801	15.5

Panel D: Potential Mechanisms

	N	% (of non-missing data)
Quality		
Overall quality of care (Q19)		
Excellent	1276	11
Very Good	1919	16.6
Good	2238	19.3
Fair	1597	13.8
Poor	735	6.4
Preventive care		
Blood cholesterol check (Q37)		
Yes, last year	3615	31.1
Yes, more than a year ago	3626	31.2
No	4377	37.7
Blood test for high blood sugar (Q38)		
Yes, last year	3469	29.9
Yes, more than a year ago	3452	29.7
No	4684	40.4
Mammogram (Q39)		
Yes, last year	1393	20
Yes, more than a year ago	2455	35.2
No	3127	44.8
Pap test (Q40)		
Yes, last year	2737	39.4

Yes, more than a year ago	3922	56.4
No	292	4.2
Health behavior		
Smoke (Q41,Q42)		
Every day	3745	32.7
Some days	1055	9.2
Not at all	6644	58.1

Table A13: Summary of differences between initial, six month and twelve month surveys

Question	Question number in initial survey	Question number in six month survey	Six-month survey notes	Question number in twelve month survey	Twelve-month survey notes
Your health coverage					
Received app?	1	x		x	
Sent app?	2	x		x	
Why didn't you send app?	3	x		x	
Accepted into OHP std?	4	x		x	
Main reason of denial?	5	x		x	
Currently have health ins?	6	1		1	
How many of last 6m did you have health ins?	7	2		2	
Your health care	1	x		x	
Is there a usual place you receive care?	8	3		3	
Where do you go to receive care?	9	4		4	
Need medical care in last 6m?	10	6		6	
Did you get all the care you needed in the last 6m?	11	7		7	
Why did you go without medical care most recently?	12	8		8	
Did you need rx in the past 6m?	13	9		9	
Did you get all the rx you needed in the last 6m?	14	10		10	
Main reason you went without rx most recently?	15	11		11	
How many rx are you taking?	16	12		12	
Need dental in last 6m?	17	13		13	
Receive all the dental care you needed in last 6m?	18	14		14	
How many times did you go to an office or clinic in last 6m?	19	15		15	
How many times did you go to an ER in last 6m?	20	16		16	
Why did you go to the ER most recently?	21	17	new option added	17	
How many times did you stay overnight in the hospital in last 6m?	22	18		18	
How many total days did you spend in hospital in last 6m?	23	19		x	removed from

					12m
Do you have one person you think of as personal doctor or health care provider?	X	5	new for 6m	5	
How would you rate quality of med care in last 6m?	x	20	new for 6m	19	
Your health care costs					
Average OOP costs for rx in last 6m?	24	x	consolidated into 21,22	x	
How much did you spend on medical care for yourself in last 6m?	25	x	no similar question	x	
Did you borrow money, skip bills, pay late because of health bills in last 6m?	26	24	question order changed	23	option order changed
Do you owe money for medical expenses?	27	23	question order changed	22	option order changed
Have you been refused treatment because you owe money?	28	25		24	
Have you paid ANY OOP in the last 6m?	x	21	new for 6m	20	
How much money did you spend on various health expenditures in last 6m?	x	22	new for 6m	21	slight change in layout
Your health					
How is your health in general?	29	27		26	
Physical health problems in last 30 days?	30	29		28	
Mental health problems in last 30 days?	31	30		29	
Health problems causing impairment of activies in last 30 days?	32	31		30	
How has your health changed in last 6m?	33	28	question order changed	27	
Have you been told you have any of these health conditions?	34	34	new options added	35	
Overall happiness?	X	26	new for 6m	25	
Depressed in last 30 days?	X	32	new for 6m	34	question order changed
Little interest in things the last 30 days?	X	33	new for 6m	33	question order changed

Have you taken medication for the following health issues?	X	35	new for 6m	36	
Have you smoked 100 ciggs?	X	36	new for 6m	41	
Rate of smoking?	X	37	new for 6m	42	
How many ciggs a day?	X	38	new for 6m	43	
Does health limit your ability to work?	X	x		31	new for 12m
How fit are you compared to age peer?	X	x		32	new for 12m
Have you had blood cholesterol checked?	X	x		37	new for 12m
Have you been checked for diabetes?	X	x		38	new for 12m
Had a mammogram?	X	x		39	new for 12m
Had a pap test or pap smear?	X	x		40	new for 12m
have you been advised to quit smoking in past 12m?	x	x		44	new for 12m
About you					
Gender?	35	39		45	
Year of birth?	36	40		46	
Employment?	37	41		47	
Gross household income?	38	43	question order changed	49	
Hispanic?	39	44		50	
Race?	40	45		51	
Work hours?	41	42	question order changed	48	
Education?	42	46		52	
Living arrangement?	43	47		53	
Household Size?	44	48		54	
Family members <19?	45	49		55	
Family members <19 insured?	46	50		x	removed from 12m

Table A14: Treatment vs Control Balance of Pre-randomization hospital outcomes of entire study population

	Control mean (and standard deviation)	Difference between treatment and controls
Any hospital admission	0.036	-0.001
SE	(0.187)	(0.001)
p-value		[0.408]
Any hospital admission not through ED	0.014	-0.001
	(0.119)	(0.001)
		[0.478]
Any admission through ED?	0.026	-0.001
	(0.159)	(0.001)
		[0.469]
All admissions		
# of days	0.238	-0.007
	(2.195)	(0.017)
		[0.674]
Procedures	0.07	-0.001
	(0.632)	(0.005)
		[0.791]
List charges	1130.171	-1.739
	(11400.411)	(91.95)
		[0.985]
Non-ED admissions		
# of days	0.089	0.005
	(1.356)	(0.012)
		[0.662]
Procedures	0.031	0.002
	(0.401)	(0.003)
		[0.592]
List charges	457.974	40.761
	(8612.159)	(71.944)
		[0.571]
ED admissions		
# of days	0.149	-0.012
	(1.512)	(0.011)
		[0.263]
Procedures	0.039	-0.003
	(0.449)	(0.003)
		[0.379]
List charges	672.197	-42.5
	(6795.238)	(53.041)
		[0.423]
Pooled F – stat	.	0.481252
[p value]	.	0.888283
(N)	.	74851

Pooled F – stat on all of above outcomes plus lottery list variables from Table I2	.	1.088895
[p value]	.	0.355809
(N)	.	74851

All outcomes are measured in the hospital discharge data from January 01 2008 through the notification date; on average we have 5 months of data on an individual. First column reports mean and standard deviation of each outcome for the controls. Second column reports the estimated difference in each variable between treatment and control based on estimating equation (1) with household size and lottery draw indicators; we report the coefficient, standard error (clustered on household id) and the per-comparison p-value. The last two rows of the table reports the pooled F-stat from estimating equation (5) (with the same set of covariates) on all the variables shown above it, and on all those variables plus the lottery list variables examined in Table I2. All regressions are unweighted.

Table A15: Treatment vs Control Balance of Pre-randomization Credit Report outcomes among those who matched to credit data

	Control mean	Difference between treatment and control
Any bankruptcy (February 08) *SE* *p-value*	0.011 (0.106)	0 (0.001) [0.985]
Any lien (February 08)	0.02 (0.141)	0.001 (0.001) [0.481]
Any judgment (February 08)	0.068 (0.251)	-0.005 (0.002) [0.025]
Any collection (February 08)	0.491 (0.5)	-0.001 (0.004) [0.735]
Any delinquency on credit account (February 08)	0.404 (0.491)	-0.002 (0.004) [0.66]
Any Medical Collection (February 08)	0.257 (0.437)	-0.005 (0.004) [0.206]
Any non-medical collection (February 08)	0.39 (0.488)	0.002 (0.004) [0.57]
Currently have a credit score? (February 08)	0.821 (0.383)	0.001 (0.002) [0.69]
Currently have a thick file? (February 08)	0.428 (0.495)	0.001 (0.003) [0.783]
Total current credit limit on all open revolving credit (February 08)	8983.654 (28960.476)	31.201 (132.341) [0.814]
Pooled F – stat *[p value]* *(N)*		0.731511 0.680379 49552
Pooled F – stat on all of above outcomes plus lottery list variables from Table I2 *[p value]* *(N)*		0.783678 0.729548 49552

Note: Table investigates potential biases stemming from non matches to the credit report data by looking at pre-randomization characteristics among those who matched to the September 2009 archive; characteristics are measured in the February 2008 credit report data and defined exactly analogously to how they were defined in the September 2009 data (i.e. from the pseudo notification date through date n unless defined as "current").

Column (1) reports the control means (and std dev). Column 2 reports the estimated coefficient on TREATMENT based on estimating equation (1). All regressions include controls for household size fixed effects and lottery draw; in addition the outcomes analyzed in the February 2008 data include controls for the same outcome measured in February 2007 (these are missing for the approximately **6 percent** of the September 2009 analysis sample whom we do not have data for in February 2007; we set the values to their mean and include a dummy for missing). We report the coefficient, standard error (clustered on household id) and per-comparison p-value.
The last two rows of the table reports the pooled F-stat from estimating equation (5) (with the same set of covariates) on all the variables shown above it, and on all those variables plus the lottery list variables examined in Table I2. All regressions are unweighted.

Table A16: Treatment vs Control Balance of Pre-randomization outcomes among survey respondents

	Control mean	Difference between treatment and control
Any hospitalization in pre period (HDD)?	0.029	0
SE	(0.168)	(0.003)
p-value		[0.988]
# of hospital visits in pre period (HDD)	0.037	0
	(0.248)	(0.004)
		[0.935]
Any medical collections in pre period (CR)	0.226	-0.011
	(0.418)	(0.007)
		[0.109]
Any non medical collections in pre period (CR)	0.332	0.007
	(0.471)	(0.007)
		[0.314]
Pooled F – stat		1.043981
[p value]		0.382749
(N)		22867
Pooled F – stat on all of above outcomes plus lottery list variables from Table I2		0.76866
[p value]		0.694521
(N)		22867

Note: Table investigates potential biases stemming from non-response to the survey by looking at pre-randomization characteristics among those who responded to the survey. Characteristics are taken either from outcomes in hospital discharge data for the time period between January 1, 2008 and notification date, or from outcomes in February 2008 credit report data back through the "pseudo notification" date n. These four measures were chosen in the administrative data because we can measure them pre randomization and because they reasonably closely approximate outcomes we measure in the survey. The two hospital utilization measures (both of which exclude child birth) very closely capture the corresponding survey measures: specifically, we measure (1) "any hospital admission for non childbirth" and (2) "number of hospital visits for non childbirth". We measure these for the full pre period of hospital discharge data, specifically from January 1 2008 through the notification date. In the credit report data, (3) "any medical collections" is used to approximate the "owe $ for medical bills" survey question and (4) "any non medical collections" is used to approximate the survey question about "did you borrow $ / skip paying other bills to pay your medical bills question"). The credit report measures are from February 2008 archive back to the pseudo notification date. Column (1) reports the control means and std dev. Column (2) reports the estimated coefficient on TREATMENT based on estimating equation (1). All regressions include household size fixed effects, survey draw fixed effects, and the interaction of the two. Regressions using the administrative outcomes also include lottery draw fixed effects, and regressions for the two credit outcomes include analogous outcomes from the February 2007 credit data and a dummy variable for missing February 2007 credit data. All regressions are weighted using the survey weights. All standard errors are clustered on the household. We report the coefficient, standard error, and per-comparison p-value. The last two rows of the table reports the pooled F-stat from estimating equation (5) (with the same set of covariates) on all the variables shown above it, and on all those variables plus the lottery list variables examined in Table I2.