**THE OREGON HEALTH INSURANCE EXPERIMENT:
EVIDENCE FROM EMERGENCY DEPARTMENT DATA**

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Analysis Plan

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Introduction

The goal of the analysis described here is to use the Oregon Health Insurance Experiment and data collected from 12 Portland-area emergency departments to estimate the effects of expanding Medicaid availability to a population of low-income adults. This analysis examines the effects on amount and type of emergency department utilization.

This analysis plan aims to pre-specify the analysis before comparing outcomes for treatment and control groups. By creating this analysis plan, which serves as a record of our ex ante planned analysis, we hope to minimize issues of data mining and specification searching. We do use the control distributions for all the outcomes and perform treatment-control comparisons that explore the validity of our analysis (such as balance on pre-randomization characteristics and uptake of insurance). This plan was constructed after viewing the findings from a mail survey and administrative data collected approximately one year after the lottery (Finkelstein et al. 2012) and in-person interview data collected approximately two years after the lottery (Baicker et al. *forthcoming*). The methods proposed here follow those undertaken in those analyses very closely; the outcome measures, however, are new.

Methods

Randomization and intervention

After opening a waiting list for a previously closed Medicaid program in early 2008, Oregon conducted eight lottery drawings from the waiting list between March and September 2008. Selected individuals won the opportunity – for themselves and any household member – to apply for health insurance benefits through Oregon Health Plan Standard (OHP Standard). OHP Standard provides benefits to low-income adults who are not categorically eligible for Oregon's traditional Medicaid program. To be eligible, individuals must be: ages 19-64; not otherwise eligible for Medicaid or other public insurance; Oregon residents; U.S. citizens or legal immigrants; without health insurance for six months; with income below the federal poverty level and assets below $2,000. Among the randomly selected individuals, those who completed the application process and met these eligibility criteria were enrolled in OHP Standard. OHP Standard provides relatively comprehensive medical benefits (including prescription drug coverage) with no consumer cost sharing and low monthly premiums (between $0 and $20, based on income), provided mostly through managed care organizations. The lottery process and OHP Standard have been described in more detail elsewhere (Finkelstein et al. 2012).

Emergency Department Data

We obtained standard individual-level emergency department visit data for twelve hospitals in the Portland-metro area from January 2007 through December 2010. We probabilistically matched these data to the Oregon Health Insurance Experiment Study population based on information provided at the time of lottery sign-up. The data include a hospital identifier, date of visit, detail on diagnoses, procedures, and charges, and, for those admitted to the hospital, dates of admission and discharge. Normal childbirth hospital admissions are not considered admitted through the emergency department and thus are not included in the emergency department data. We exclude the small number (N=836 in our control sample) of pregnancy- and childbirth-related visits, mostly involving pregnancy complications, that do appear in the emergency department data. Detail on variable definitions is given in the appendix; Appendix Table A2 shows the distributions of our analytic variables and Appendix Table A3 shows the distribution of visits in various decompositions.

Administrative data

We obtained pre-randomization demographic information that the participants provided at the time of lottery sign-up. We use these data primarily to construct eight "lottery list variables"[1] that we use to examine treatment and control balance. In addition, the state provided us with detailed data on Medicaid enrollment for every individual on the list (starting prior to the lottery and continuing through the study period). We use this to construct our measures of insurance coverage during the study period.

Analytic sample

We limited our analytic sample to the individuals residing in areas that primary rely on one of the twelve hospitals in our data for emergency department care. To identify these areas, we used hospital discharge data for the entire state of Oregon (described in more detail in Finkelstein et al, 2012). For each zip code of residence in Oregon, we considered all hospital admissions (to any Oregon hospital) originating in the emergency department; this analysis was not limited to our lottery list sample. We calculated the percent of these hospital admissions that was at one of our twelve hospitals. We identified zip codes where this percent was 98% or higher.[2] Figure 1 shows a map of the included zip codes. Our full analytic sample was thus all individuals in the Oregon Health Insurance Experiment who were residing in one of those zip codes at the time of lottery sign-up. This strategy is designed to alleviate concerns that our analytical sample may consider going to emergency departments outside of the 12 we observe, and that insurance could affect this selection.

[1] Specifically, we use: year of birth, sex, whether English is the preferred language for receiving materials; whether the individuals signed themselves up for the lottery or were signed up by a household member; whether they provided a phone number on sign-up; whether the individuals 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 2000 census from their ZIP code.

[2] We excluded eleven zip codes identified by this process which had fewer than 20 admissions through the emergency department.

Time frame of the study

For our primary analysis, we define the study period from March 10, 2008 (the first day that anyone was notified of being selected in the lottery) to September 30, 2009. This is the end date used in our previous analysis of hospital discharge data and other outcomes (Finkelstein et al. 2012). This 18-month observation period represents, on average, 15.6 months (standard deviation = 2.0 months) after individuals were notified of their selection in the lottery and, on average, 13.5 months (standard deviation = 2.6 months) after insurance coverage was approved for those selected by the lottery that successfully enrolled in OHP Standard.

We measure all pre-randomization versions of our outcomes from January 1, 2007 to March 9, 2008.

Statistical analysis

We estimate intent-to-treat models comparing outcomes among those selected in the lottery (the treatment group) to those who were on the list but not selected (the controls). We estimate linear probability models for outcomes. All analyses adjust for the number of household members on the lottery list because selection was random conditional on the number of listed household members; specifically, treatment assignment was done at the household level but the lottery drew individual names, so that households with more individuals on the list were more likely to be selected. All standard errors are clustered by household to account for intra-household correlation. All regressions include the pre-randomization version of the outcome (measured from January 1, 2007 to March 9, 2008) as an additional control. This is not required to avoid bias, but, by explaining some of the variance in the outcome, may improve the precision of the estimates.[3] As a sensitivity check, we test both excluding these pre-randomization outcomes and including demographic characteristics (measured prior to randomization) as covariates. We test the sensitivity of our model specification by estimating average marginal effects from logistic regressions.

This intent-to-treat analysis estimates the effect of being selected in the lottery (and therefore being able to apply for Medicaid), but the effect of insurance coverage per se may also be of interest. We therefore also present local average treatment effects, which estimate the effect of Medicaid for those covered because of the lottery. We estimated this by fitting a two-stage least squares model using selection in the lottery as an instrumental variable for ever being covered by Medicaid during the study, with the same adjustments and weights as in the intent-to-treat model (see Appendix). Imperfect take-up of Medicaid among those selected in the lottery reduces the statistical power of the study, but does not introduce bias into the estimates: because the lottery is random, it can be used to isolate the unbiased causal effect of insurance coverage on outcomes even if take-up is non-random and less than 100% (Angrist, Imbens and Rubin 1996).

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[3] To determine whether to include these pre-randomization versions of the outcome, we estimated how much variance they explained in the control sample. The partial r-squares ranged from 0.04 to 0.38 depending on the specific outcome.

Results

Preliminaries and initial analysis

The study population

Figure 2 shows the evolution of the study population from submitting names to inclusion in the emergency department analysis. Table 1 shows the characteristics of those included in the emergency department analysis. Because the analysis is restricted to the areas where emergency department use is almost exclusively at one of the 12 hospitals in our sample, we include only 33% of the full Oregon Health Insurance Experiment study population. As expected, there is no difference in inclusion based on treatment status (-0.142 percentage points; SE .395). There are no significant differences between treatment and control groups on the characteristics measured at the time of lottery sign-up (F-statistic 1.498; P= 0.152), on the pre-randomization versions of our outcomes (F-statistic 0.917; P= 0.631), or the combination of both (F-statistic 1.004; P= 0.467).

Insurance coverage

Table 2 reports the control means and effects of lottery selection for various definitions of insurance coverage. Being selected in the lottery is associated with an increase of 24.7 percentage points (SE 0.006) in the probability of having Medicaid coverage during our study period; we use this increase in insurance coverage due to the lottery to estimate local average treatment effects.[4]

There are two distinct Oregon Medicaid programs: the program for the traditional Medicaid population (OHP Plus) and the program for the expansion population (OHP Standard). We define someone as ever on "Medicaid" if they are on either Medicaid program, including both Plus and Standard. Since the lottery was for the OHP Standard program, that is where we would expect to find increases in coverage, and this is borne out in the data. In fact, the increase in OHP Standard is slightly greater than the increase in any Medicaid (25.2 percentage points compared to 24.7), suggesting that some of the increase in OHP Standard may have come from individuals who would have been on another Medicaid program at some point during the study period.

The effect of the lottery on Medicaid coverage attenuates over time: using "current" enrollment (measured on September 30, 2009) reduces the lottery effect on insurance coverage from 24.7 (row 1) to 14.3 (row 4). There are two reasons for this. First, those who successfully enroll in OHP (through the lottery or other means) are required to recertify eligibility every six

[4] These numbers do not correspond exactly to those reported in Finkelstein et al, 2012 which uses a slightly different definition of the study period based on individual notification dates (which vary across the 8 lottery draws from March to October). For the purposes of this paper, we define the study period as beginning on March 10, 2008, which is the first date that anyone was notified of being selected in the lottery.

months, leading to attrition in coverage. Additionally over time, those not selected in the lottery may obtain Medicaid coverage through the OHP Plus program.

Because the initial take-up of Medicaid was relatively low, lottery selection is associated with an average increase of 3.246 months on Medicaid (row 3) – both because only a subset of those selected in the lottery obtained coverage and because those who obtained coverage were not necessarily covered for the entire study period. For those who did obtain coverage through the lottery, there is an increase of 13.2 months on Medicaid (0.16). This is less than the 18 months in the study period for several reasons: lottery selection occurred in 8 draws between March and October 2008, initial enrollment in OHP took 1-2 months after lottery selection, and some of those enrolled in Medicaid through the lottery lost coverage by failing to recertify.

Emergency department utilization

The impact of Medicaid coverage on emergency department utilization is ambiguous *a priori*. By covering the cost of emergency department care, Medicaid may increase utilization, as we have found with other types of care (Finkelstein et al. 2012). Others have hypothesized that by increasing access to primary care and/or improving health, expanded insurance coverage could reduce emergency department utilization, and perhaps even total utilization.

Table 3 reports our results for overall emergency utilization; we consider both the probability of using the emergency department and the number of visits. About one-third of our sample has an ED visit over our 18-month study period. Conditional on having a visit, on average an individual in our sample has 3 visits over this period. We also consider whether an individual had more than 6 visits over our 18-month study period, in order to capture the impact of insurance on very frequent use of the emergency department.

We also decompose visits into inpatient visits (resulting in a hospital admission) and outpatient visits. On average, 12 percent of ED visits in our control sample result in an admission to the hospital. (All statistics on the proportion of visits of different types for the control sample can be found in Table A3).

Finally, as a measure of intensity, we include the sum of list charges across all visits during our study period for each individual. We report separately both the list charges specifically in the emergency department and the total list charges (which also includes inpatient charges for those ED visits that resulted in a hospital admission at the same hospital). List charges are accounting charges for rooms and procedures and do not reflect transacted prices. They are perhaps best viewed as a price-weighted summary of treatment, albeit at somewhat artificial prices (Card, Dobkin and Maestas 2009), and that is how we interpreted them in prior work using list charges for inpatient hospitalizations (Finkelstein et al. 2012). They have a large variance, as can be seen in Table 3, so we expect our estimates of the differences will be imprecise.

Composition of visit types

Table 4 reports our results for different types of emergency department visits. We use several different ways to classify the emergency department visits to distinguish between urgent, non-deferrable type of visits and more preventable or deferrable visits. These decompositions of visit types allow us to consider two hypothesized impacts of insurance. One hypothesis is that through expanded access to primary and preventive care, insurance will prevent negative health outcomes and thus emergency department and hospital use. If this were true, we would expect any reductions in emergency department use to be most pronounced in conditions that can be prevented by primary care. Another hypothesis is that through improved access to primary care, the insured will be able to substitute doctor office visits for emergency department care, and thus "inappropriate" use of the ED will decrease. We consider both.

Following Miller (2012) we first separate visits based on when they occur. We consider visits occurring during "on-hours" (8am – 8pm Monday through Friday) and those occurring during off-hours (weekends or overnight). Just under half of visits in our control sample occur during "on-hours" and just over half during "off hours" (i.e. weekends or overnight.). To the extent that insurance reduces emergency department use by increasing access to non-emergency department sources of care (e.g., standard office visits), we would expect on-hours visits to decrease relative to off-hours visits: access to primary care is less relevant for treatment choices when doctors' offices are closed. We test formally whether the effect of insurance is the same of each of the "off-hours" groupings relative to the "on-hours" category.

We then use the algorithm developed by Billings et al (2000) to decompose visits based on the primary diagnosis code for the visit into "emergent, not primary care preventable," "emergent, primary care preventable," "emergent, primary care treatable" and "non-emergent." Because the algorithm is probabilistic (each visit is assigned a probability for being each type), we present only the total margin, combining the probabilities across all visits during the study period. Roughly 19% of visits in our control sample are classified as "non-emergent," with 34%, 7% and 21% being classified as primary care treatable, primary care preventable and not primary care preventable respectively. The algorithm cannot classify the remaining 19% of visits in our control sample.

All four categories of care may see an increase in utilization in response to Medicaid because of reduced prices or a decrease in utilization as the result of improved health. The primary hypothesis we are examining is that the second through fourth categories will decrease relative to the first ("emergent, not primary care preventable"). The "emergent, primary care preventable" visits may decrease through improved primary and preventive care in the newly insured. The "emergent, primary care treatable" and "non-urgent" visits may decrease if the newly insured substitute away from "inappropriate" emergency department use towards "appropriate" office visit use. Because overall utilization may increase or decrease, we will consider relative changes in these various types of visits and our hypothesis (mentioned above) is that the last three categories should see a decrease in use relative to the first. We formally test whether the estimated effect of insurance is the same for each of these three categories, relative to the "emergent, not primary care preventable" category.

Finally, we identify visits for ambulatory care sensitive conditions using criteria included in the AHRQ Prevention Quality Indicators (Agency for Healthcare Research and Quality). These conditions have been identified as preventable with adequate primary care, but do not necessarily capture all visits that could have been prevented. Nearly 7% of visits in our control sample are considered ambulatory care sensitive. Like the "emergent, primary care treatable" visits above, these may decrease, relative to visits in general, through improved primary and preventive care.

An important caveat to these analyses is that the algorithms for identifying "inappropriate" use do not show widely differential use of the emergency department by the insured and the uninsured observationally. If we look either at the entire set of visits to all 12 Portland emergency departments, not limited to our study sample, or at ED visits nationally, the proportion of visits in the different categories is roughly the same for insured and uninsured adults (see Appendix Table A3 for the Portland analysis and Appendix Table A7 for the national analysis). For example, in our Portland EDs, 47% of ED visits for insured adults are on-hours, compared to 48% for uninsured adults. Nationally, 23% of emergency department visits are ex post identified as "emergent, non-preventable", which may be considered clearly "appropriate" use. This proportion does not vary greatly by insurance status (24.01% for insured adults compared to 21.35% for uninsured adults). Another analysis of Oregon emergency department use by Lowe et al. (Lowe and Fu 2008) notes some concerns about the performance of the algorithm developed by Billings et al. In particular, they note that, because of the limitations of administrative data, the algorithm uses *ex post* diagnosis for categorization rather then *ex ante* symptoms. This may cause inaccurate classification, as seeking care for alarming symptoms (i.e. chest pain) may be a completely appropriate use of emergency care, but may often result in diagnoses that are not, with hindsight, considered to be emergencies (i.e. heart burn).

It is possible that on the margin these algorithms are useful in distinguishing the type of utilization that changes in response to insurance, and indeed they have been interpreted and used in this fashion in prior research which has found differential responses to insurance along these dimensions (e.g. Miller 2012). However, it is also possible that these algorithms are too coarse to distinguish patterns of use, or it may be that, on average, the patterns of emergency care use are not as different across populations as is commonly believed.

Hospital type

All but one of the hospitals in our data are private, so we are not able to assess differential changes by hospital ownership. Instead we separate hospitals based on the percent of emergency department visits that were without insurance in the pre-period. We classify those above the median of 25.6 percent as "high uninsured volume" and those below as "low uninsured volume." Table 5 reports results for emergency department utilization at both types of hospitals.

We are interested in whether Medicaid shifts relative utilization away from high uninsured volume hospitals, as these hospitals may differ from others in quality or other aspects.

We also do an agnostic examination of whether insurance is associated with any change in the distribution of visits across emergency departments. We estimate a (likely low-powered) non-directional F-test of any sorting. To do this, we estimate the intent-to-treat effect separately for each of the 12 emergency departments (the outcome being "did the individual have a visit to that emergency department") and report the F-statistic and p-value on the null hypothesis that all the effect estimates are the same.

Use for specific conditions

In addition to general emergency department use, we consider use for several specific conditions of interest (Table 6). (Health Care Utilization Project 2011).

In Panel A, we group visits into clinical conditions, using the HCUP Clinical Classification Software (Health Care Utilization Project 2012), and identify eight conditions that are prevalent in our population (each accounting for more than 3% of visits in our control sample): injuries, mood disorders, , substance or alcohol related, skin infections, chest pain and heart conditions, back problems, headache, and abdominal pain. Together these conditions account for 48.70% of all visits in our control sample, and capture the nine most prevalent reasons for emergency department use except for disorders of the teeth and jaw. We specifically excluded this because dental care is not covered by Oregon Health Plan Standard (the lotteried program). The clinical conditions are mutually exclusive. Some of them are based on a single clinical condition; others, such as injuries, are groupings of multiple related conditions. Details on the selected conditions and their prevalence are included in Appendix Table A6. In addition to the eight conditions, we include a combination of mental-health, alcohol and substance related visits as these conditions tend to be highly comorbid. We do not have specific hypotheses about the impact of insurance on emergency department use for these conditions relative to general use. The selection and groupings of these conditions was ad hoc and intended to capture interesting and prevalent reasons for emergency department use in our population.

In Panel B, we also identify visits for chronic conditions using criteria developed by AHRQ (Healthcare Utilization Project 2011). These criteria are designed to identify hospital visits which are related to a chronic condition, not to imply that other visits are necessarily acute. The chronic condition indicator overlaps with the other conditions in the table. It is possible that visits for chronic conditions decrease relative to general visits in response to insurance as those chronic conditions may particularly benefit from access to primary care.

Heterogeneity of results

There is substantial variation in the frequency of emergency department use in our population, with a large fraction never or rarely using the emergency department, but some using it frequently. Frequent use may indicate either poor health or use of the emergency department as a source of primary care (or both); in either case, the effect of insurance may be different in frequent users than in the rest of the population. Frequent users may stand to benefit the most from increased access to primary care and improved health, leading to relative declines in emergency department use in this group. Alternately, frequent users may have ingrained patterns of emergency department utilization, making their use less responsive to insurance status. We classify individuals based on their usage of the emergency department prior to randomization (between January 1, 2007 and March 9, 2008). We create three subgroups: those with no pre-period emergency department utilization (roughly 2/3 of our sample), those with one pre-period visit (roughly 1/6 of our sample) and those with two or more pre-period visits (roughly 1/6 of our sample). We supplement these 3 sub-groups with another 2 intended to capture even more precisely frequent users of the emergency department. One group is individuals with two or more pre-period outpatient visits (an attempt to limit to those whose frequent use is not driven by severe disease). The other group is individuals with 5 or more visits in the pre-period. This corresponds to the top 12% of control group emergency department users in the pre-lottery period, and this group accounts for 42% of all pre-lottery period emergency department visits in our control population. Table 7 reports the results for our main outcomes broken into these subgroups.

In addition to these subgroups based on prior utilization, for those outcomes where we have substantively or statistically significant estimates, we plan to explore potential heterogeneity in the estimated effects of insurance along the following additional dimensions: gender, age (19-49 and 50-64), race (white and any non-white), pre-randomization access to credit (yes or no), education (more than high school and high school or less), smoking status (ever smoker and never smoker), and signing up for the lottery on the first possible day. This analysis follows Finkelstein et al. (2012) and is explained in more detail there (Finkelstein et al. 2011). The measures of race, access to credit, education, and smoking status use data sources not discussed here but described fully in Finkelstein et al. (2012).

Sensitivity of results

As our primary specification we use linear probability models even for rates of binary outcomes. We also will use an alternate specification of logistic models and estimated marginal effects for all binary outcomes. We will also investigate the sensitivity of results to adjustment for covariates. We will report our primary specification that includes adjustment for the pre-period version of the outcome, as well as a specification without this adjustment and one adding controls for a more complete set of pre-randomization characteristics.

Combining with other data sources

Estimating spending (combining emergency department and hospital visits)

We can combine the emergency department data used here with hospital discharge data described and analyzed in Finkelstein et al 2012, to estimate the change in annual spending due to changes in emergency department and hospital use.

In Table 8, we make a back-of-the-envelope calculation of the change in annual spending associated with insurance by weighting each type of use by its average cost among low-income publicly insured adults in the Medical Expenditure Panel Survey (MEPS). We show results separately for outpatient ED visits, and for all hospitalizations, regardless of whether or not they originated in the ED. The hospitalization results are all taken from the hospital discharge data previously analyzed in Finkelstein et al. 2012, but limited to the 12 hospitals for which we already have ED data and to individuals in the emergency department sample.

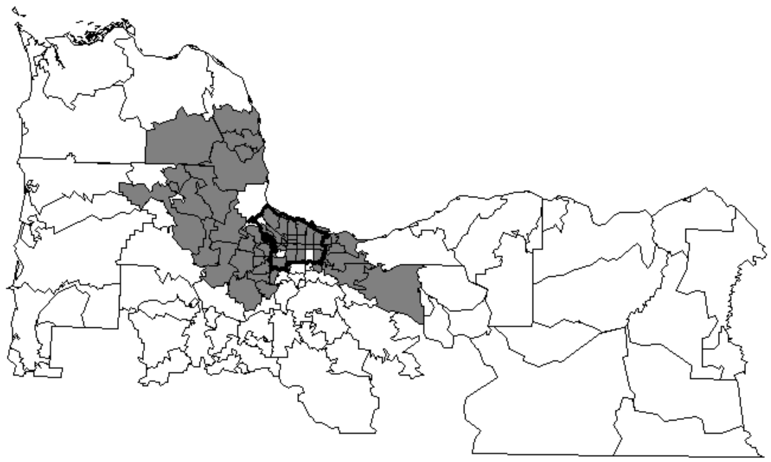
Timing analysis

In addition to our primary analysis, we consider the time path of the effects of health insurance coverage over a longer time period through July 15, 2010. The effect of expanded health insurance coverage on emergency department and hospital utilization may vary as time passes. Initially, there may be pent-up demand for services that leads to relative increases in utilization by the newly insured. Other mechanisms that may lead to relative decreases in utilization, such as improved health or medical management of chronic disease, may only appear later. Furthermore, changes in patterns of use may occur slowly. Thus, we will examine how utilization is effected by insurance over different time horizons in both the emergency department data and the hospital discharge data.

Comparison for ED analysis to previous results

We have previously reported on the effects of emergency department use in both our twelve-month mail survey data (Finkelstein et al. 2012) and our in-person interview data (Baicker et al. *forthcoming*). The results presented here may differ from those for a variety of reasons: the samples are different in each analysis, the time period is different, and self-reported data may differ from administrative data. Here we mimic the survey results using administrative data. For the twelve-month mail survey, we limit to the overlap sample of survey respondents in the emergency department analytic sample (N=13, 452). We estimate the effect of health insurance on emergency department utilization from the survey data in this sample. Then, for each twelve-month mail survey respondent in the overlap sample, we construct a measure of any and number of emergency department visits in the six months prior to that individual's survey response date. This can then be compared to the survey responses to a question about use in the last six months. Similarly, for the in-person survey, we limit to the overlap sample of respondents in the emergency department analytic sample (N=9,501). We estimate the effect of health insurance on emergency department utilization from the in-person data in this sample. Then, for each in-person interviewee, we construct a measure of any and number of emergency department visits in the year prior to that individual's interview date and compare it to the interview response about use in the last year. Table 10 presents a comparison of our results as measured in the survey and interview and our results as measured in the emergency department data.

Figure 1: Map of Included Zip Codes



Note: Shaded ZIP codes are included in the ED Analysis. Portland City Boundaries (as of 2007) are outlined in black. Thirteen small ZIP codes within the Portland boundaries that are included in the analysis are not pictured due to their small size. Two additional analysis ZIP codes to the southwest of Portland are also not pictured. Maps were created using shapefiles from the Oregon Spatial Data Library.

Figure 2: Study Population and Analytic Sample

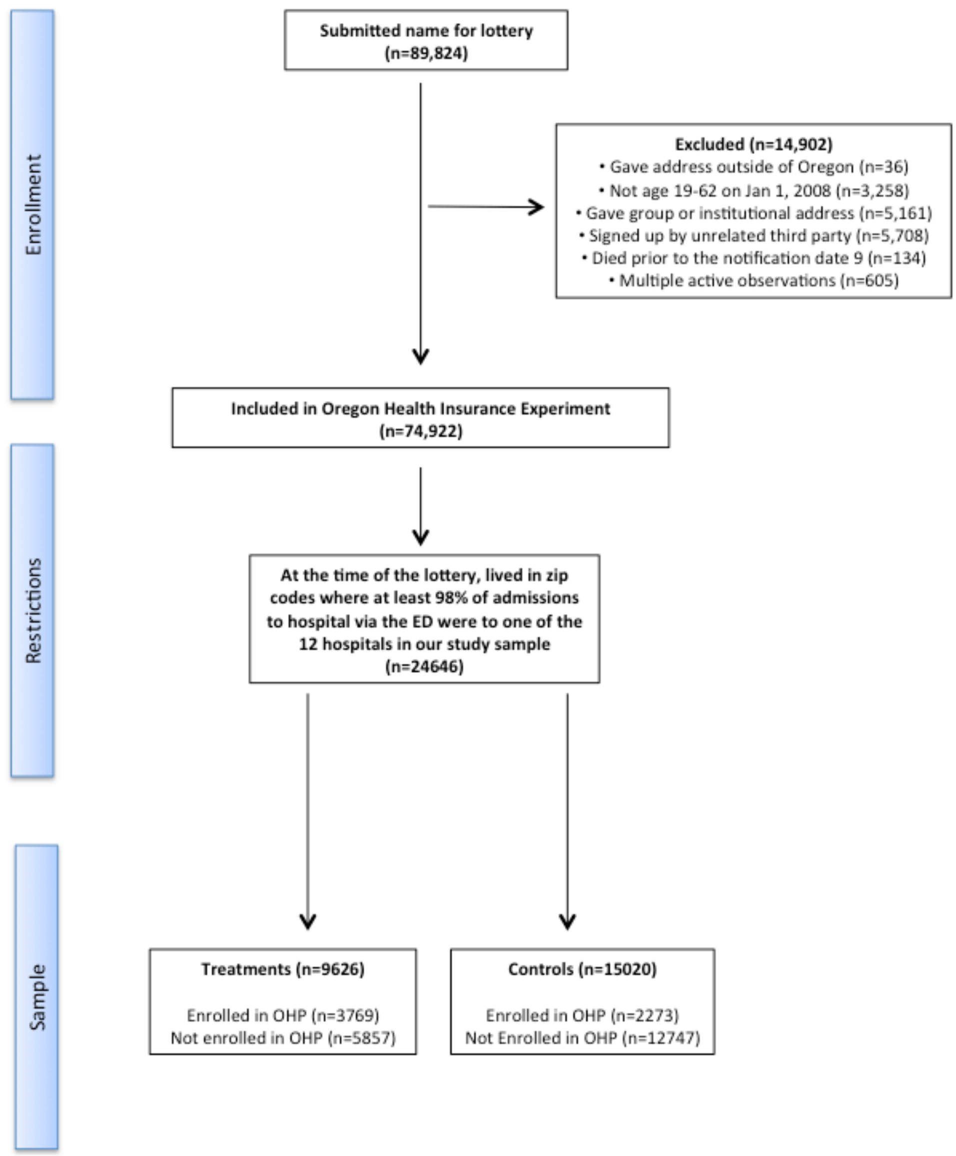


Table 1: Treatment-Control Balance

	Control mean (1)	Treatment-control difference (2)
Panel A: Included in ED analysis sample		
Included in ED analysis sample	33.313	-0.142 (0.395)
Panel B: Lottery list characteristics, conditional on being in ED analysis sample		
Year of birth	1968.335	0.098 (0.170)
Female	0.554	-0.010 (0.006)
English as preferred language	0.875	0.009 (0.005)
Signed up self	0.929	0.001 (0.000)
Signed up first day of lottery	0.091	0.006 (0.004)
Gave phone number	0.866	0.003 (0.005)
Address a PO Box	0.026	0.001 (0.002)
Zip code median household income	43027.246	182.410 (136.175)
F-statistic for lottery list variables		1.498
p-value		0.152

Continued on the next page

(Standard errors in parentheses.)

Notes: The first column reports the mean for the control respondents. The second column reports the difference between the average outcome for all individuals selected in the lottery and the average outcome for all control individuals, as calculated by ordinary least squares regression; the dependent variable is given in the left hand column. All regressions include indicators for each household size and adjust standard errors for household clusters.

Panel A: Sample consists of individuals in the full analysis sample (N=74922).

Panels B and C: Sample consists of individuals in Portland-area zip codes (N=24646).

Table 1, continued

Panel C: Pre-randomization characteristics, conditional on being in ED analysis sample		
Any ED visits	0.320	0.005 (0.006)
Number of ED visits	0.815	0.002 (0.027)
More than five ED visits	0.041	-0.002 (0.003)
Any Inpatient ED visit	0.066	-0.003 (0.003)
Number of ED Inpatient visits	0.096	-0.007 (0.006)
Any Outpatient ED visit	0.297	0.006 (0.006)
Number of Outpatient ED visits	0.721	0.005 (0.025)
Any Weekday Daytime ED visit	0.222	-0.001 (0.006)
Number of Weekday Daytime ED visits	0.442	-0.010 (0.016)
Any Offhours ED visit	0.209	0.007 (0.005)
Number of Offhours ED visits	0.380	0.003 (0.014)
Any Weekend ED visit	0.143	0.010 (0.005)
Number of weekend ED visit	0.224	0.008 (0.009)
Any Overnight ED visit	0.144	0.000 (0.005)
Number of Overnight ED visits	0.224	-0.001 (0.010)
Number of non-emergent ED visits	0.168	-0.004 (0.008)
Number of Primary Care Treatable ED visits	0.274	0.010 (0.010)
Number of Emergent, Preventable ED visits	0.064	-0.002 (0.004)
Number of Emergent, Nonpreventable ED visits	0.164	0.004 (0.007)
Number of Unclassified ED visits	0.147	-.0068 (0.007)

Number of 'Avoidable' ED visits	0.507	-0.002 (0.018)
Any ACSC Visit	0.039	-0.001 (0.003)
Number of ACSC Visits	0.055	-0.0024 (0.004)
Any high uninsured volume ED visit	0.21	0.008 (0.005)
Number of visits to a high volume uninsured hospital	0.47	0.009 (0.018)
Any low uninsured volume ED visit	0.168	-0.005 (0.005)
Number of low uninsured volume ED visits	0.35	-0.0079 (0.016)
Any ED visit for injury	0.123	0.003 (0.004)
Number of ED visits for injury	0.174	0.016 (0.008)
Any mood disorder related ED visit	0.017	0.000 (0.002)
Number of mood disorder related ED visits	0.027	-0.002 (0.003)
Any skin condition related ED visit	0.031	0.001 (0.002)
Number of skin condition related ED visits	0.050	0.000 (0.004)
Any abdominal pain related ED visit	0.028	0.000 (0.004)
Number of abdominal pain related ED visits	0.041	-0.0013 (0.004)
Any back condition related ED visit	0.023	0.002 (0.002)
Number of back condition related ED visits	0.033	0.006 (0.004)
Any heart condition related ED visit	0.020	0.000 (0.002)
Number of heart condition related ED visit	0.026	0.000 (0.003)
Any headache related ED visit	0.018	-0.001 (0.002)
Number of headache related ED visits	0.033	-0.004 (0.006)

Any substance abuse/mental health related ED visit	0.037	-0.002 (0.002)
Number of substance abuse/mental health related ED visi	0.068	-0.007 (0.006)
Any chronic condition ED visit	0.087	-0.005 (0.004)
Number of chronic conditions ED visits	0.159	-0.019 (0.009)
F-statistic for pre-randomization outcomes		0.917
p-value		0.631
F-statistic for lottery list and pre-randomization		1.004
p-value		0.467

Table 2: First Stage Estimates

	Control mean	Estimated FS
	(1)	(2)
Ever on Medicaid	0.151	0.247 (0.006)
Ever on OHP Standard	0.024	0.252 (0.005)
# of Months on Medicaid	1.675	3.247 (0.083)
On Medicaid, end of study period	0.111	0.143 (0.005)

(Standard errors in parentheses.)

Notes: The first column reports the control mean for the measure of "INSURANCE" defined in the left-hand column. The second column reports the effect on insurance coverage, which compares the average of the insurance measure for all individuals selected in the lottery to the average of the insurance measure for all control individuals, as calculated by ordinary least squares regression. The study period is defined as starting on March 10, 2008 and ending on September 30, 2009. All regressions include dummies for household size and adjust standard errors for household clusters.

Sample consists of individuals in Portland-area zip codes (N=24646).

Table 3: Emergency Department Utilization

	Extensive Margin				Total Margin			
	Control Mean	ITT	LATE	p-values	Control Mean	ITT	LATE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All ED Visits	0.345	XX (XX)	XX (XX)	XX	1.022 (2.632)	XX (XX)	XX (XX)	XX
Six or more ED Visits	0.042							
By type of visit:								
Inpatient ED visits	0.075				0.126 (0.60)			
Outpatient ED visits	0.32				0.897 (2.36)			
By intensity:								
Total ED Charges					1445.497 (4215.26)			
Total Charges					3638.837 (14886.27)			

(Standard errors in parentheses)

Notes: Columns 1 and 5 report the control mean of the dependant variable and standard deviation for continuous outcomes. Columns 2 and 6 report intention-to-treat estimates, which compare the average outcome for all individuals selected in the lottery to the average outcome for all control individuals, as calculated by ordinary least squares regression. Columns 3 and 7 report the local-average-treatment-effect for insurance coverage as estimated by instrumental variable regression. Columns 4 and 8 report the per-comparison p value. All regressions include indicators for each household size, control for the pre-randomization outcome, and adjust standard errors for household clusters.

Sample consists of individuals in Portland-area zip codes (N=24646).

Table 4: Emergency Department Utilization by Timing and Urgency

	Extensive Margin				Total Margin			
	Control Mean	ITT	LATE	p-values	Control Mean	ITT	LATE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
By timing of visit:								
"On hours" visits	0.243	XX (XX)	XX (XX)	XX	0.522 (1.443)	XX (XX)	XX (XX)	XX
"Off-hours" visits	0.234	XX (XX)	XX (XX)	XX	0.503 (1.458)	XX (XX)	XX (XX)	XX
p-value (vs. "on hours")				XX				XX
Weekend visits	0.161				0.284 (.935)			
p-value (vs. "on hours")				XX				XX
Overnight visits	0.174				0.321 (1.051)			
p-value (vs. "on hours")				XX				XX
By urgency:								
Emergent, not preventable	N/A	XX (XX)	XX (XX)	XX	0.213 (.685)	XX (XX)	XX (XX)	XX
"Avoidable"	N/A	XX (XX)	XX (XX)	XX	0.615 (1.634)	XX (XX)	XX (XX)	XX
p-value (vs. "emergent, not preventable")				XX				XX
Emergent, preventable	N/A				0.074 (.342)			
p-value (vs. "emergent, not preventable")								XX
Primary care treatable	N/A				0.343 (.948)			
p-value (vs. "emergent, not preventable")								XX
Non-emergent	N/A				0.201 (0.688)	XX (XX)	XX (XX)	XX
p-value (vs. "emergent, not preventable")								XX
Unclassified	N/A				0.196 (.734)			
p-value (vs. "emergent, not preventable")								XX
By preventability:								
Ambulatory-care sensitive	0.046	XX (XX)	XX (XX)	XX	0.067 (.396)	XX (XX)	XX (XX)	XX

(Standard errors in parentheses)

Notes: Columns 1 and 5 report the mean of the dependent variable in the control sample and standard deviation for continuous outcomes. Columns 2 and 6 report intention-to-treat estimates, which compare the average outcome for all individuals selected in the lottery to the average outcome for all control individuals, as calculated by ordinary least squares regression. Columns 3 and 7 report the local-average-treatment-effect for insurance coverage as estimated by instrumental variable regression. Columns 4 and 8 report the per-comparison p value. All regressions include indicators for each household size and adjust standard errors for household clusters. For each outcome, we test whether the estimated intention-to-treat effect is the same as for the reference outcome (either on-hours visits or emergent, not preventable visits). We report the p-values in Columns 4 and 6, in the row directly below other results for the outcome.
Sample consists of individuals in Portland-area zip codes (N=24646).

Table 5: Emergency Department Utilization by Hospital Type

	Extensive Margin				Total Margin			
	Control Mean	ITT	LATE	p-values	Control Mean	ITT	LATE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
By Uninsured volume								
High Uninsured volume	0.233	XX	XX	XX	0.595	XX	XX	XX
		(XX)	(XX)		(1.770)	(XX)	(XX)	
Low Uninsured volume	0.184	XX	XX	XX	0.436	XX	XX	XX
		(XX)	(XX)		(1.499)	(XX)	(XX)	
Global test of sorting				XX				XX

(Standard errors in parentheses)

Notes: Columns 1 and 5 report the mean of the dependent variable in the control sample and standard deviation for continuous outcomes. Columns 2 and 6 report intention-to-treat estimates, which compare the average outcome for all individuals selected in the lottery to the average outcome for all control individuals, as calculated by ordinary least squares regression. Columns 3 and 7 report the local-average-treatment-effect for insurance coverage as estimated by instrumental variable regression. Columns 4 and 8 report the per-comparison p value. All regressions include indicators for each household size, control for the pre-randomization outcome, and adjust standard errors for household clusters. The global test for sorting is calculated by the intention-to-treat estimates for each of the 12 emergency department, then doing an F-test of the null that all the estimated effects are equal. The p-value reported in column 4 for the global test is for that F-test.

Sample consists of individuals in Portland-area zip codes (N=24646).

Table 6: Emergency Department Utilization by Selected Conditions

	Extensive Margin				Total Margin			
	Control Mean	ITT	LATE	p-values	Control Mean	ITT	LATE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
Injury	0.145	XX (XX)	XX (XX)	XX	0.324 (0.988)	XX (XX)	XX (XX)	XX
Skin conditions	0.037				0.057 (.372)			
Abdominal pain	0.034				0.052 (.385)			
Back conditions	0.030				0.045 (.333)			
Chest pain or heart	0.026				0.034 (.254)			
Headache	0.019				0.033 (.407)			
Mood disorders	0.017				0.033 (.338)			
Substance abuse and mental health issues	0.040				0.087 (.634)			
Panel B								
Chronic condition	0.101	XX (XX)	XX (XX)	XX	0.203 (.896)	XX (XX)	XX (XX)	XX

(Standard errors in parentheses)

Notes: Columns 1 and 5 report the mean of the dependent variable in the control sample and standard deviation for continuous outcomes. Columns 2 and 6 report intention-to-treat estimates, which compare the average outcome for all individuals selected in the lottery to the average outcome for all control individuals, as calculated by ordinary least squares regression. Columns 3 and 7 report the local-average-treatment-effect for insurance coverage as estimated by instrumental variable regression. Columns 4 and 8 report the per-comparison p value. All regressions include indicators for each household size, control for the pre-randomization outcome, and adjust standard errors for household clusters.

Sample consists of individuals in Portland-area zip codes (N=24646).

Table 7a: Heterogeneous Treatment Effects, Control Means

			Extensive Margin			Total Margin				
	N	First stage	All ED Visits	Inpatient ED Visits	Outpatient ED Visits	All ED Visits	Inpatient ED Visits	Outpatient ED Visits	Total ED Charges	Total Charges
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Full sample	24646	0.247	0.345	0.075	0.32	1.022	0.126	0.897	1445.497	3638.837
Pre-period Utilization										
No visits	16930	0.235	0.225	0.043	0.205	0.418	0.057	0.361	595.033	1663.977
One visit	3881	0.274	0.472	0.092	0.436	1.115	0.142	0.973	1573.494	4147.264
Two+ visits	3823	0.268	0.721	0.197	0.693	3.446	0.396	3.053	4843.654	11243.368
Two+ outpatient visits	3390	0.259	0.731	0.185	0.712	3.618	0.372	3.248	5010.928	11153.418
Five+ visits	945	0.254	0.892	0.315	0.866	6.824	0.796	6.03	9471.944	21565.427

Notes: Table 7 reports control means and local-average-treatment effect estimates for different subsamples defined based on prelottery ED usage (no ED visits, one ED visit, two or more ED visits (inpatient and outpatient), two or more ED visits (outpatient only), and five or more ED visits. Columns 1 and 2 report the sample size and first stage estimate. Table 7a shows the mean in the control sample of the variable indicated by the column heading, and Table 7b shows the local-average-treatment-effect estimate of the effect of insurance. For each subgroup, we test if the local-average-treatment-effect estimate is the same as that for the reference subgroup (of no visits) and report the p-value from that test. For the full sample results, also reported in Table 3, we include the local-average-treatment-effect estimate, the standard error (in parantheses) and the p-value [in square brackets].

Sample consists of individuals in Portland-area zip codes (N=24635).

Table 7b: Heterogeneous Treatment Effects, Effect of Insurance

			Extensive Margin			Total Margin				
	N	First stage	All ED Visits	Inpatient ED Visits	Outpatient ED Visits	All ED Visits	Inpatient ED Visits	Outpatient ED Visits	Total ED Charges	Total Charges
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Full sample	24646	0.247	XX	XX	XX	XX	XX			
			(xx)	(xx)	(xx)	(xx)	(xx)			
			[XX]	[XX]	[XX]	[XX]	[XX]			
Pre-period Utilization										
No visits	16930	0.235	XX							
One visit	3881	0.274	XX							
p-value (vs. "no visits")			(XX)							
Two+ visits	3823	0.268								
p-value (vs. "no visits")			(XX)							
Two+ outpatient visits	3390	0.259								
p-value (vs. "no visits")			(XX)							
Five+ visits	945	0.254								
p-value (vs. "no visits")			(XX)							

(Standard errors in parantheses)
[p-values in square brackets]

Table 8: Spending estimate

	Total Margin			
	Control Mean	ITT	LATE	p-values
	(1)	(2)	(3)	(4)
Outpatient ED visits	0.897	XX	XX	XX
	(2.362)	(XX)	(XX)	
Inpatient hospital visits	0.169	XX	XX	XX
	(.811)	(XX)	(XX)	
Annual spending ($)	1107.72			

(Standard errors in parentheses)

Notes: Column 1 reports the mean and standard deviation of the dependent variable in the control sample. Column 2 reports the intention-to-treat estimate, which compares the average outcome for all individuals selected in the lottery to the average outcome for all control individuals, as calculated by ordinary least squares regression. Column 3 reports the local-average-treatment-effect for insurance coverage as estimated by instrumental variable regression. Column 4 reports the per-comparison p value. All regressions include indicators for each household size, control for the pre-randomization outcome, and adjust standard errors for household clusters. Spending estimates associated with utilization effects are caluclated using the 2002–2007 (pooled) Medical Expenditure Panel Survey (MEPS). We use their expenditures (all inflated with the CPI-U to 2007 dollars) to calculate average expenditures per ER visit ($435) and average expenditures per inpatient visit (for visits not related to childbirth) ($7523). Since the study period runs from 10 March 2008- 30 September 2009, we divide estimates by 1.5 in order to calculate annual costs.

Sample consists of individuals in Portland-area zip codes (N=24646).

Table 9: Comparing adminstrative and survey data

	Extensive Margin				Total Margin			
	Control Mean	ITT	LATE	p-values	Control Mean	ITT	LATE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Use from mail survey ("last 6 months")	0.554	XX (XX)	XX (XX)	XX	1.100 (1.50)	XX (XX)	XX (XX)	XX
Use matched to mail survey ("last 6 months")	0.454				0.691 (0.99)			
Use from in-person survey ("last 12 months")	0.515	XX (XX)	XX (XX)	XX	1.463 (2.47)	XX (XX)	XX (XX)	XX
Use matched to in-person ("last 12 months")	0.319				0.562 (1.14)			

(Standard errors in parentheses)

Notes: Columns 1 and 5 report the control mean of the dependant variable and standard deviation for continuous outcomes. Columns 2 and 6 report intention-to-treat estimates, which compare the average outcome for all individuals selected in the lottery to the average outcome for all control individuals, as calculated by ordinary least squares regression. Columns 3 and 7 report the local-average-treatment-effect for insurance coverage as estimated by instrumental variable regression. Columns 4 and 8 report the per-comparison p value. All regressions include indicators for each household size, control for the pre-randomization outcome, and adjust standard errors for household clusters. Regressions for mail survey are weighted using mail survey weights and include indicators for survey wave and interactions between survey wave and household size. Regressions for in-person survey are weighted using in-person weights.

Sample for all above analysis consists of overlap between survey respondents and the emergency department sample (N=2497 for mail survey and N=6563 for in-person).

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Appendix

Appendix

Emergency department data

We obtained emergency department data for visits occurring between January 1, 2007 and December 31, 2010 from hospitals in the Portland metro area.[1] The Oregon Association of Hospitals and Health Systems (OAHHS) collected the data from the hospitals. The data include emergency department records, and for patients admitted to that same hospital, inpatient records. For patients transferred to another hospital, the data do not include inpatient records. We estimate this is about 1.41% of all visits and 12.41% of visits resulting in an admission in our control sample. Normal childbirth hospital admissions are not considered as originating in the emergency department and are not included in emergency department data. A small number of visits for complications of pregnancy and childbirth do appear however. We restricted the sample to exclude pregnancy- and childbirth-related complications.

We probabilistically matched the Oregon Health Insurance Experiment population to the emergency department data using LinkPlus software. This was done using name, date of birth and gender. Due to the protected nature of the data, the match was conducted on-site at the offices of Oregon Health Policy and Research (OHPR) in conjunction with OHPR personnel, who then provided the study team with data including the matched study identifier but excluding the personally-identifying matching variables.

Analytic sample

We limited our analytic sample to the individuals residing in areas that primarily rely on one of the twelve hospitals in our data for emergency department care. To identify these areas, we used hospital discharge data for the entire state of Oregon (described in more detail in Finkelstein et al, 2012). For each zip code of residence in Oregon, we considered all hospital admissions (to any Oregon hospital) originating in the emergency department; this analysis was not limited to our lottery list sample. We calculated the percent of these hospital admissions that were to one of our twelve hospitals. We then limited our sample to zip codes where this percent was 98% or higher.[2] We excluded eleven codes identified in this way which had fewer than 20 total admissions though the emergency department; all the remaining included zip codes had at

[1] The initial data obtained included thirteen hospitals. One hospital was missing data for 2007 and 2008, so we excluded this hospital (accounting for 63,432 emergency department visits or 2.93% of the initial data).

[2] The 98% cut-point was chosen based on the observed distribution of the data. This cut-point captures 33% of the full Oregon Health Insurance Experiment sample. Even lowering the cut-point to 90% only increases that share to 36%, but raising it to a 99% cut-point decreases that share to 21%.

least 70 such admissions. Our full analytic sample was thus all individuals in the Oregon Health Insurance Experiment who were residing in one of those zip codes at the time of lottery sign-up.

Appendix Table A1 compares this sample to the full Oregon Health Insurance Experiment sample. The sample we use for the emergency department analysis is completely urban and more likely to have requested lottery materials in a language other than English, but otherwise is not very different from the full sample.

Analytic specifications

Intent-to-Treat Effect of the Lottery (ITT)

Our treatment group are those selected in the lottery and our controls are those who were not. We estimate the intent-to-treat (ITT) effect of winning the lottery (i.e. the difference between treatment and controls) by fitting the following OLS equation:

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

where i denotes an individual and h denotes a household.

LOTTERY is an indicator variable for whether or not household h was selected by the lottery. The coefficient on *LOTTERY* (β_1) is the main coefficient of interest, and gives the average difference in (adjusted) means between the treatment group (the lottery winners) and the control group (those not selected by the lottery); it is interpreted as the impact of being able to apply for OHP Standard through the Oregon lottery.

We denote by X_{ih} the set of covariates that are correlated with treatment probability (and potentially with the outcome) and therefore must be controlled for so that estimates of β_1 give an unbiased estimate of the relationship between winning the lottery and the outcome. In all of our analyses, X_{ih} includes indicator variables for the number of individuals in the household listed on the lottery sign-up form (hereafter "household size"); although the state randomly sampled from individuals on the list, the entire household of any selected individual was considered selected and eligible to apply for insurance. As a result, selected (treatment) individuals are disproportionately drawn from households of larger household size.

We denote by V_{ih} a second set of covariates that can be included to potentially improve power by accounting for chance differences between treatment and control groups in variables that may be important determinants of outcomes. These covariates are not needed for β_1 to give an unbiased estimate of the relationship between winning the lottery and the outcome, however, as they are not related to treatment status. Our primary analysis adds only the pre-randomization version of the outcome (i.e. the analogous outcome measured between January 1, 2007 and March 9, 2008). As a secondary analysis, we will explore whether our results are sensitive to inclusion of V_{ih} covariates.

In all of our ITT estimates and in our subsequent instrumental variable estimates (see below), we estimate linear models even though a number of our outcomes are binary. Because we are interested in the difference in conditional means for the treatments and controls, linear probability models would pose no concerns in the absence of covariates or in fully saturated models (Angrist 2001, Angrist and Pischke 2009). Our models are not fully saturated, however, so it is possible that results could be affected by this functional form choice, especially for outcomes with very low or very high mean probability. We therefore explore the sensitivity of our results to an alternate specification using logistic regression and calculating average marginal effects for all binary outcomes.

In all of our analyses we cluster the standard errors on the household identifier since the treatment is at the household level.

Local Average Treatment Effect of Medicaid (LATE)

The intent-to-treat estimates from equation (1) provide an estimate of the causal effect of winning the lottery (i.e. winning the opportunity to apply for OHP Standard). This provides an estimate of the net impact of expanding *access* to public health insurance. We are also interested in the impact of insurance *coverage* itself. We model this as follows:

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

where INSURANCE is a measure of insurance coverage and all other variables are as defined in equation (1). We estimate equation (2) by two stage least squares (2SLS), using the following first stage equation:

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

in which the excluded instrument is the variable *LOTTERY.*

We interpret the coefficient on insurance from instrumental variable estimation of equation (2) as the local average treatment effect of insurance, or LATE (Imbens and Angrist 1994). In other words, our estimate of π_1 identifies the causal impact of insurance among the subset of individuals who obtain insurance upon winning the lottery but who would not obtain insurance without winning the lottery (i.e. the compliers).[3]

The LATE interpretation requires the additional identifying assumption that the only mechanism through which winning the lottery affects the outcomes studied is the lottery's impact on insurance coverage. We believe this is a reasonable approximation; in earlier work we discussed potential violations; where we could explore them we did not find cause for concern (Finkelstein et al. 2012).

[3] If insurance is defined as "ever on OHP Standard" we can probably be comfortable interpreting the IV estimates of equation (3) as the treatment-on-treated (ToT) rather than a LATE. In practice, there are two small violations of this interpretation. First, if there were no way to get OHP Standard without winning the lottery there would be no "always-takers" in the terminology of Angrist, Imbens and Rubens (1996), but about 2 percent of our controls got onto OHP standard through some limited alternative mechanisms —for example, pregnant women who are on OHP Plus can sometimes stay on OHP Standard after giving birth. Second, it is possible that some compliers were put on OHP Plus rather than Standard, since case workers are instructed to first check applicant eligibility for Plus; in practice this number is likely to be small since the estimated first stage is very similar for "ever on Medicaid" (which includes Plus and Standard) and "ever on OHP Standard" (see rows 1 and 2 of Table S4).

Outcome variables

The outcomes in this analysis are drawn from the emergency department records from twelve Portland-area emergency departments for visits occurring between March 10, 2008 and September 30, 2009. Table A2 provides detail on the distribution of the outcome variables. These are defined at the level of the individual. A given individual may have more than one ED visit during our study period. Table A3 shows the frequency and percent of visits of different types for our control sample and at the 12 emergency departments more generally; here the unit of observation is a visit.

As outcomes, for each type of emergency department visit, we analyze a binary indicator for any visits of that type and a continuous measure of the number for visits of that type. For all number-of-visit variables, we truncated at $2*99^{th}$ percentile, conditional on any, but leave the binary indicator for any visits unchanged.

Visits

Individuals are classified as having an **emergency department visit** if there is an encounter record at one of the twelve Portland-area emergency departments. Individuals having **more than 6 visits** in the study period are also identified. An emergency department visit was classified as resulting in an **inpatient visit** if the patient was either admitted as an inpatient or transferred to another hospital for inpatient care. An Emergency Department visit was classified as an **outpatient visit** if it did not result in hospitalization.

List charges

We have two list charge variables describing the costs billed for each emergency department visit: emergency department facility charges and total charges. **Total charges** is the full list charge associated with the visits, including all inpatient charges if the patient was admitted to that hospital. For emergency department visits resulting in an admission to another hospital (1.69% of all visits and 12.96% of all admissions), total charges do not include the inpatient charges. For admissions for mental health or substance abuse, this problem is particularly pronounced with only 60% of hospital admissions having an associated inpatient record. **Emergency department facility charges** are only the emergency department charges. For seven hospitals in the sample, these include all emergency department visit charges, so emergency department facility charges are equal to total charges for outpatient visits. For the remaining five hospitals, emergency department facility charges are, on average, between 62 and 72% of total charges for outpatient visits, so there are additional charges associated with emergency department use that are not captured. For each individual, all charges incurred in visits during the study period are summed. List charges are accounting charges for rooms and procedures and do not reflect transacted prices. They are perhaps best viewed as a price-weighted summary of treatment, albeit at somewhat artificial prices (Card, Dobkin and Maestas 2009).

Because the hospitals in our sample appear to have different practices in coding emergency department facility charges, our results for emergency department charges may be sensitive to changes in which hospitals are being visited. We specifically test whether the proportion of emergency department visits occurring at the seven hospitals where emergency department charges capture all outpatient charges changes with insurance. If Medicaid coverage changes which emergency departments people use, we might see changes in this variable even if treatment intensity did not change. Of course, even for total charges, if hospitals have different cost to charge ratios and Medicaid changes which emergency departments people go to, any effect on total charges may in part reflect this type of reporting difference; as a result we will also do a global test of whether Medicaid changes the distribution of people across emergency departments.

Time of visit

Emergency department visits were classified according to time of day. An emergency department visit was classified as a **weekend** visit if the emergency department admission date was recorded as Saturday or Sunday, and **overnight** if the admission hour was listed as between 8PM and 7AM (inclusive). The **off-hours** indicator captures visits that occurred either on the weekend or overnight. The **on-hours** indicator is the complement of off-hours admissions and captures visits that occurred during weekday, daytime hours.

Urgency

We use the algorithm developed by Billings et al (Billings, Parikh and Mijanovich 2000) to derive the urgency of each emergency department visit using that admission's primary ICD-9 diagnosis code. To construct this algorithm, a panel of emergency department and primary care physicians was given access to a sample of 6,000 full emergency department records. These full records contained detailed information about the patient including age, gender, vital signs, medical history, presenting symptoms and also information about the resources used on the patient in the emergency department, the diagnoses made and procedures performed. Based on this extensive information, the doctors classified each record into one of four categories based on severity. Individual conditions may have received different severity classifications based on the information in the emergency department record. For example, a complaint like "headache" or "abdominal pain" may be non-emergent or may indicate potential serious underlying conditions or episodes. To capture this, the ICD9 diagnosis codes were assigned a probability weight indicating the probability that a case with a given ICD9 code was classified in one of four severity categories.

The severity categories are: a **non-emergent** case where care was not required within 12 hours (e.g. a toothache), **emergent but primary care treatable** cases where care is needed within 12 hours but can be provided in a primary care setting (e.g. a lumbar sprain), **emergent but preventable** cases that the doctors judge could have been avoided with proper primary or ambulatory care (e.g. an asthma attack), or **emergent and non-preventable** cases that could not have been avoided with primary care (e.g. a heart attack).

An emergency department admission is marked as **unclassified** if the emergency department algorithm did not assign it a probability weight or if the primary diagnosis code was missing. Missing diagnosis codes represent a very small share of all admissions: primary diagnosis codes are missing for 82 of the control group's 17498 emergency department admissions. The large proportion of visits that are unclassified result from the algorithm not assigning probabilities to the primary diagnosis for that visit. Presumably these diagnoses are too infrequent to have been included in the dataset of visits coded by the panel of physicians who created the algorithm.

All admissions are classified regardless of whether or not they resulted in hospitalization. Miller (2012) classifies all inpatient admissions as emergent on the basis that they were severe enough to result in hospitalization: this methodology is not followed here. Because the algorithm assigns probabilistic weights, we do not define the extensive margin variables (any such visits) and construct the total margin variables (number of such visits) by summing the assigned probabilities across all visits.

Ambulatory-care-sensitive conditions

An alternate measure for potentially avoidable hospitalizations or ambulatory-care-sensitive conditions was adapted from AHRQ's Prevention Quality Indicators. These indicators were originally developed to identify "conditions for which good outpatient care can potentially prevent the need for hospitalization or for which early intervention can prevent complications or more severe disease" (Agency for Healthcare Research and Quality). A condition in the emergency department data was identified as **ambulatory care sensitive** by its diagnosis and procedure codes.

Hospital type

Using emergency department admissions from the pre-randomization period (January 1, 2007 to March 10, 2008), we generated an uninsured fraction of admissions (a ratio of uninsured adult emergency department visits to total adult emergency department visits). The hospitals were then split at the median (25.6 percent) and the six hospitals with the higher ratios were defined as **high uninsured volume hospitals** while the six hospitals with lower ratios of uninsured to total adult admissions were defined as **low uninsured volume hospitals.** The average uninsured fraction of admissions for the high volume group was 28.8 percent, and the average uninsured fraction of admissions for the low volume group was 19.3 percent. Table A4 shows the uninsured fraction of admissions for each of the twelve hospitals in our data.

Specific conditions

The first diagnosis (ICD-9 code) listed on the emergency department admission record was used to identify specific conditions.

The ICD-9 codes associated with the primary diagnoses for each admission were aggregated into a more manageable number of clinical classifications using the Clinical Classification Software (Health Care Utilization Project 2012). The top 10 clinical conditions in the control sample are shown in Table A5. Guided by this list of the most prevalent conditions, diagnoses were grouped as follows: **injuries, mood disorders, skin conditions, abdominal pain, back conditions, chest pain and heart problems, headache, and mental health and substance-related disorders**. The groupings are ad hoc, aimed at capturing the most prevalent conditions in useful groupings. Details of specific conditions included in these groups are provided in Table A4.

Visits were classified as **chronic** using the Chronic Condition Indicator (CCI), which was produced by the Healthcare Cost and Utilization Project (Health Care Utilization Project 2011). A chronic condition is defined as, "a condition that lasts 12 months or longer and meets one or both of the following tests: (a) it places limitations on self-care, independent living, and social interactions; (b) it results in the need for ongoing intervention with medical products, services, and special equipment." Examples of conditions classified as chronic by the CCI are hypertension (401.0), heart disease (429.9), dementia (290.0), and chronic bronchitis (491.21).

There is substantial overlap between the chronic conditions indicator and the other prevalent conditions. Almost all (more than 98%) of the mental-health and substance related visits are also coded as chronic, all mood disorders are coded as chronic, and 40% of the headache visits are also coded as chronic. For the other conditions, less than 10% of admissions are coded as chronic. Conversely, in our control sample, approximately half of the conditions coded as chronic are also coded as another condition.

Table A6 shows, for each of these eight specific categories we consider, the conditions included and their prevalence in our population.

Annual spending estimation

We use data from the emergency department records to estimate the number of outpatient emergency department visits (and the effect of insurance) as above. We use data from the hospital discharge data described and analyzed in Finkelstein et al (2012) to estimate the number of inpatient hospital visits. We include all hospital admissions, regardless of whether or not they originated in the emergency department data. We limit the hospital records to the 12 hospitals for which we already have emergency department data and to individuals in the emergency department sample (living the selected Portland-area zip codes at the time of the lottery). The zip code restriction was designed to capture zip codes where at least 98% of hospital admissions through the emergency department were to one of our 12 hospitals. Hospital admissions not through the emergency department, which we also use here, follow slightly different patterns, but would not have identified a very different set of zip codes. Of the 70 zip codes included in our analysis, only 12 have fewer than 98% of all admissions to one of our 12 hospitals, and the share going to our 12 hospitals in those 12 zip codes is never less than 96%. Conversely, there are 17 zip codes not included in our emergency department analysis where at least 98% of all admissions were to one of our 12 hospitals; 8 of these barely missed the cutoff for inclusion, with at least 96% of all admissions through the emergency department at one of our 12 hospitals, while the remaining 9 were excluded because they had fewer than 20 hospital admissions via the ER

To calculate the implied annual spending effects associated with the estimated utilization effects we use data from the 2002-2007 (pooled) Medical Expenditure Panel Survey (MEPS) on expenditures of all nonelderly (19-64) adults below 100 percent of poverty who are publicly insured. 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 ED visit and average expenditures per inpatient visit (for visits not related to childbirth). All spending numbers are bases on total expenditures (i.e. not just expenditures in the insured or insurance expenditures). The costs are $435 per ED visit and $7,523 per inpatient visit. For each type of utilization we observe (outpatient ED visit and inpatient visit), we multiply the estimated change in number by the cost per visit estimated in the MEPS. We scale the number of visits by the number of months in the study period to produce the number on an annual basis.

Comparison to US emergency department visits

NHAMCS data

We use data from the emergency department component of the National Hospital Ambulatory Medical Care Survey (NHAMCS), a nationally representative survey of emergency department and hospital outpatient visits conducted annually by the Centers for Disease Control and Prevention (Centers for Disease Control and Prevention (CDC) 2000). The NHAMCS' emergency department component contains admissions-level data from non-institutional and short-stay hospitals' emergency departments. We combine survey years 2005-2009 (inclusive). The data was restricted to non-childbirth visits using the same procedure as the emergency department data.

The classification of the urgency of visits was implemented in the NHAMCS data in the same way as in our sample. The NHAMCS data necessitated a modified version of the "ambulatory care sensitive condition" classification as coded by AHRQ's Prevention Quality Indicators. Because the NHAMCS contains only outpatient records, there are no records of procedure codes done after hospital admission. As a result, it was not possible to classify the NHAMCS admissions into four of the prevention quality indicator categories: hypertension (7), heart failure (8), angina without procedure (13), and lower-extremity amputation due to diabetes (16) because these categories relied on the presence or absence of a particular procedure. In addition to these variables, the NHAMCS data contains a **triage variable** which records the immediacy with which a patient should be seen: immediately, 1-15 minutes, 15-60 minutes, greater than one hour but less than 2 hours, 2 hours to 24 hours.

Comparing the insured and uninsured

Table A7 shows the characteristics of emergency department visits for the full US population, the adult population, insured adults and uninsured adults. For comparison purposes, the final column of Table A7 shows our controls sample's admissions (with the ambulatory care sensitive conditions recoded to match the NHAMCS coding).

The uninsured are commonly believed to disproportionately use the emergency department for care that could be provided in a primary care setting or could have been avoided with appropriate primary care (Newton et al. 2008). Using the NYU Algorithm classifying visits into non-emergent, primary care treatable, primary care preventable and non-preventable, we do not see strong evidence for this. The uninsured do use the emergency department for care that is ex-post identified as non-emergent, primary care treatable or primary care preventable. Surprisingly, the insured also use the emergency department in these ways and at very similar rates. The same is true of the use of emergency department for ambulatory-care-sensitive conditions.

The same pattern is apparent in our data of all emergency department visits at Portland hospitals (Table A3). Although uninsured adults have a slightly higher share of visits that are non-emergent, primary care treatable and emergent preventable, all of these categories have non-trivial mass for the insured adult sample (e.g. 18 percent have non-emergent visits, and 33 percent have primary care treatable compared to 21% and 34%, respectively, for uninsured adults).

That the uninsured do not use the emergency department disproportionately overall or for avoidable and non-urgent care, as identified by these algorithms, suggests that either the commonly held view is not supported by the data or that the algorithms are not effectively capturing the true underlying phenomenon.

Looking at two other measures of the severity or emergency of visits, we do see large differences between the insured and uninsured. The insured are much more likely to be triaged to be seen in the next hour (42% vs 37%) and are much more likely to be admitted to the hospital for further care (12% vs. 8%). Again, the interpretation of this is not entirely clear. It may be that the insured are seeking care for more serious, more emergent illnesses needing immediate and extensive care. It is also possible, however, that insurance status directly impacts triage and admissions decisions and an insured patient is more likely to be seen quickly and admitted than a similarly presenting uninsured patient.

Table A1: Differences in Lottery List Characteristics across samples

	Full Sample (1)	ED Sample (2)
Year of Birth	1968.00	1968.34
	(12.255)	(12.084)
Female	0.56	0.55
English as preferred language	0.92	0.88
Signed up self	0.92	0.93
Signed up first day of lottery	0.09	0.09
Gave Phone Number	0.86	0.87
Address a PO Box	0.12	0.03
In MSA	0.77	1.00
Zip code median household income	39265	43027
	(8463.542)	(9405.867)
N	74922	24646

Notes: Table shows the means (with the standard deviations in parentheses for non-binary variables) of the lottery list variables given in the first column for the samples indicated in each subsequent column.

Table A2: Summary of analytic variables (Control sample)

	Percent reporting any	Conditional on any						
		Mean	SD	Median	75th %tile	95th %tile	Truncation cutpoint	Number of truncations
All ED Visits	0.34	2.97	3.79	2.00	3.00	9.00	44.00	10.00
By type of visit:								
Inpatient ED visit	0.08	1.67	1.49	1.00	2.00	4.00	18.00	2.00
Outpatient ED visit	0.32	2.80	3.48	2.00	3.00	9.00	40.00	10.00
By timing of visit:								
On-hours visit	0.24	2.15	2.26	1.00	2.00	6.00	26.00	7.00
Off-hours visit	0.23	2.15	2.36	1.00	2.00	6.00	28.00	9.00
Weekend visits	0.16	1.76	1.68	1.00	2.00	5.00	20.00	4.00
Overnight visits	0.17	1.84	1.88	1.00	2.00	5.00	22.00	6.00
By urgency:								
Non-emergent	0.08	0.60	1.08	0.00	0.80	2.46	6.00	4.00
Primary care treatable	0.14	0.74	1.28	0.33	0.91	2.86	6.00	10.00
Emergent, preventable	0.03	0.26	0.60	0.00	0.34	1.16	4.00	2.00
Emergent, not preventable	0.09	0.52	0.99	0.18	0.68	1.99	4.00	7.00
Unclassified	0.12	1.69	1.45	1.00	2.00	5.00	16.00	3.00
Ambulatory care sensitive	0.05	1.47	1.17	1.00	2.00	3.00	16.00	1.00
By Hospital Type:								
High uninsured volume	0.23	2.56	2.91	2.00	3.00	8.00	32.00	8.00
Low uninsured volume	0.18	2.38	2.76	1.00	3.00	7.00	32.00	3.00
Selected Conditions:								
Injury	0.14	1.87	1.84	1.00	2.00	5.00	24.00	4.00
Skin Conditions	0.04	1.57	1.19	1.00	2.00	4.00	14.00	1.00
Substance Abuse/Mental Hea	0.04	2.14	2.36	1.00	2.00	7.00	30.00	1.00
Abdominal Pain	0.03	1.52	1.44	1.00	1.00	4.00	18.00	0.00
Back Conditions	0.03	1.53	1.21	1.00	2.00	4.00	14.00	0.00
Chest Pain	0.03	1.28	0.92	1.00	1.00	3.00	12.00	1.00
Headache	0.02	1.78	2.39	1.00	1.00	5.00	32.00	0.00
Mood Disorders	0.02	1.92	1.76	1.00	2.00	5.00	20.00	0.00
Chronic conditions	0.10	2.02	2.08	1.00	2.00	6.00	24.00	3.00

Notes: Table details the distribution of several types of ED usage at 12 Portland-area EDs from 10 March 2008 to 30 September 2009. Summary statistics reflect non-zero observations only, after truncating at 2*99%. The classifications of urgency are missing for 17% of the sample.

Sample consists of control group individuals in Portland-area zip codes (N=15020).

Table A3: Comparison of ED visits in different populations

	All		Adults aged 19-64		Insured adults aged 19-64		Uninsured adults aged 19-64		Control sample	
	N	%	N	%	N	%	N	%	N	%
	(1)	(2)	(3)	(4)	(7)	(8)	(5)	(6)	(9)	(10)
All	590679	100.00	376972	100.00	270918	100.00	102514	100.00	17498	100.00
By gender:										
Male	271822	46.02	171469	45.49	114979	42.44	55104	53.75	7819	44.69
Female	318838	53.98	205493	54.51	155931	57.56	47408	46.25	9679	55.31
By age:										
19-49	281034	47.58	281034	74.55	190727	70.40	87608	85.46	13212	75.51
50-64	95938	16.24	95938	25.45	80191	29.60	14906	14.54	4112	23.50
By type of visit:										
Inpatient Visit	87450	14.80	45075	11.96	35704	13.18	9321	9.09	2117	12.10
Outpatient Visit	503229	85.20	331897	88.04	235214	86.82	93193	90.91	15381	87.90
By timing of visit:										
On-hours	273536	46.31	177538	47.10	126691	46.76	49286	48.08	8837	50.50
Off-hours	317143	53.69	199434	52.90	144227	53.24	53228	51.92	8661	49.50
Weekend	171859	29.10	107933	28.63	78397	28.94	28435	27.74	4787	27.36
Overnight	206871	35.02	130177	34.53	93668	34.57	35244	34.38	5483	31.34
By urgency:										
Non-emergent	104605	17.71	69952	18.56	48144	17.77	21117	20.60	3370	19.26
Primary care treatable	192177	32.53	125169	33.20	88931	32.83	34962	34.10	5889	33.66
Emergent, preventable	40126	6.79	23114	6.13	15333	5.66	7576	7.39	1257	7.19
Emergent, non-preventable	135806	22.99	87813	23.29	65040	24.01	21888	21.35	3710	21.20
Unclassified	117965	19.97	70924	18.81	53470	19.74	16971	16.55	3271	18.69
Ambulatory care sensitive	44850	7.59	23495	6.23	16845	6.22	6439	6.28	1150	6.57
Selected conditions										
Injury	145861	25.06	93593	25.27	70141	26.54	22590	22.04	3843	22.07
Mental Health/Sub. Abuse	26180	4.43	21962	5.83	15190	5.61	6607	6.44	1429	8.17
Abdominal Pain	25560	4.33	19236	5.1	13731	5.07	5280	5.15	863	4.93
Back Condions	14090	2.39	11800	3.13	8453	3.12	3229	3.15	728	4.16
Chest Pain/Heart Conditions	21979	3.72	15104	4.01	11646	4.3	3267	3.19	625	3.57
Skin Condions	18603	3.15	14496	3.85	8216	3.03	6133	5.98	959	5.48
Headache	15733	2.66	13778	3.65	10419	3.85	3205	3.13	539	3.08
Mood Disorders	8445	1.43	7196	1.91	5312	1.96	1824	1.78	515	2.94
Chronic condition	95929	16.48	63839	17.24	46570	17.62	16779	16.37	3406	19.56
By Hospital Type										
High uninsured volume	287074	48.6	181445	48.13	122388	45.18	57756	56.34	10068	57.54
Low uninsured volume	303605	51.4	195527	51.87	148530	54.82	44758	43.66	7430	42.46

ED Charges										
Less than $500.00	150789	25.53	92531	24.55	63965	23.61	27686	27.01	4617	26.39
$500.00-$999.00	154251	26.11	98754	26.2	70172	25.9	27533	26.86	4290	24.52
$1000.00-$1999.00	164801	27.9	104134	27.62	76520	28.24	26684	26.03	4721	26.98
$2000.00 or more	120760	20.44	81499	21.62	60222	22.23	20596	20.09	3869	22.11
Total Charges										
Less than $750.00	192665	32.62	120560	31.98	83442	30.8	36100	35.21	5920	33.83
$750.00-$1499.00	143145	24.23	93800	24.88	66684	24.61	25959	25.32	4055	23.17
$1500.00-$2999.00	118079	19.99	82779	21.96	59490	21.96	22330	21.78	3785	21.63
$3000.00 or more	136790	23.16	79833	21.18	61302	22.63	18125	17.68	3738	21.36

Notes: All analyses are based on the emergency department data for 12 Portland area hospitals from March 10, 2008 through September 30, 2009. Columns 9 and 10 are for our control sample; the other columns include a larger set of individuals in the Portland area. Columns 1-8 are restricted based on the patient's zip code that was recorded in the ED admission while columns 9 and 10 are restricted based on the pre-lottery zip of record for the controls. ED admissions with missing primary payer information were counted neither as insured or uninsured (this represents 0.6% of the sample). Charges bins include the full distribution of charges (including entries of zero).

Table A4: Percent of Visits Uninsured By Hospital

0.0556
0.1894
0.2025
0.2138
0.2317
0.2555
0.2630
0.2857
0.2871
0.2916
0.2959
0.2998

Note: Table shows the fraction of uninsured patients for each of the twelve hospitals in the ED study. Fraction of Uninsured Patients is defined as the ratio of uninsured ED visits to total adult ED visits in the period before March 10, 2008.

Table A5: Top 10 Clinical Conditions (Control sample)

	N (1)	% (2)
Sprains and strains	1516	8.66
Skin and Subcutaneous Tissue Infections	959	5.48
Abdominal Pain	863	4.93
Disorders of the Teeth and Jaw	781	4.46
Spondylosis; intervertebral disc disorders; other back problems	728	4.16
Superficial Injury; contusion	635	3.63
Nonspecific chest pain	580	3.31
Headache, including migrane	539	3.08
Mood disorders	515	2.94
Other Nervous System Disorders	437	2.50

Note: Sample includes all non-childbirth admissions for the control group from 10 March 2008-30 September 2009 (N=17498).

Table A6: Select Conditions (Control sample)

	N	Percent of Category	Percent of all Control Admissions
	(1)	(2)	(3)
Panel A			
Injury	3843	100	21.96
Sprains and strains	1516	39.45	8.66
Superficial Injury; contusion	635	16.52	3.63
Open wounds of extremities	333	8.67	1.9
Other Injuries due to external causes	197	5.13	1.13
Open wound (head, neck, trunk)	178	4.63	1.02
Fracture of Upper Limb	175	4.55	1.00
Fracture of Lower Limb	114	2.97	0.65
Intracranial Injury	106	2.76	0.61
Poisoning by other Medications/Drugs	68	1.77	0.39
Substance-related disorders	65	1.69	0.37
Joint disorders and dislocations; trauma related	59	1.54	0.34
Burns	57	1.48	0.33
Complications of surgical procedures or medical care	56	1.46	0.32
Poisoning by psychotropic agents	55	1.43	0.31
Other fractures	51	1.33	0.29
Skull and face fractures	41	1.07	0.23
Complication of device; implant or graft	39	1.01	0.22
Other	98	2.54	0.56
Skin conditions	959	100	5.48
Skin and Subcutaneous Tissue Infections	959	100	5.48
Abdominal Pain	863	100	4.93
Abdominal Pain	863	100	4.93
Back Conditions	728	100	4.16
Spondylosis; intervertebral dis disorders; other back prol	728	100	4.16
Chest Pain and Heart Problems	625	100	3.57
Nonspecific Chest Pain	580	92.8	3.31
Acute myocardial infarction	24	3.84	0.14
Coronary atherosclerosis and other heart disease	21	3.36	0.12
Headache	539	100	3.08
Headache, including migrane	539	100	3.08
Mood disorders	515	100	2.94
Mood disorders	515	100	2.94
Substance-related disorders	449	100	2.57
Alcohol-related disorders	310	69.04	1.77
Substance-related disorders	139	30.96	0.79

Substance Abuse and mental health issues	1429	100	8.17
Mood disorders	515	36.04	2.94
Alcohol-related disorders	310	21.69	1.77
Anxiety Disorders	250	17.49	1.43
Schizophrenia and other psychotic disorders	161	11.27	0.92
Substance-related disorders	139	9.73	0.79
Adjustment disorders	18	1.26	0.1
Suicide/intentional self-inflicted injury	15	1.05	0.09
Other	21	1.47	0.13

Panel B

Chronic Condition	3406	100.00	19.47
Mood disorders	515	15.12	2.94
Alcohol-related disorders	310	9.10	1.77
Asthma	255	7.49	1.46
Anxiety disorders	250	7.34	1.43
Headache; including migraine	214	6.28	1.22
Other nervous system disorders	201	5.90	1.15
Schizophrenia/other psychotic disorders	161	4.73	0.92
Diabetes mellitus with complications	142	4.17	0.81
Substance-related disorders	135	3.96	0.77
Chronic obstructive pulmonary disrease	117	3.44	0.67
Epilepsy; convulsions	92	2.70	0.53
Screening and history of mental health	62	1.82	0.35
Essential hypertension	60	1.76	0.34
Menstrual disorders	53	1.56	0.30
Congestive heart failure; nonhypertensive	52	1.53	0.30
Diverticulosis and diverticulitis	51	1.50	0.29
Esophageal disorders	45	1.32	0.26
Regional enteritis and ulcerative colitis	45	1.32	0.26
Spondyloiosis; invertebral disc disorder	42	1.23	0.24
Cardiac arythmia	41	1.20	0.23
Other	563	16.53	3.23

Notes: Sample is all of the non-childbirth admissions for the control group from 10 March 2008-30 September 2009 (N=17498). Note that Alcohol and Drug-related conditions appearing as "injury" reflect drug and alcohol-related poisonings. All conditions are mutally exclusive, with the exception of the chronic condition indicator which classified several of the major conditions in its "chronic" classification, and the "substance abuse and mental health" which includes both depression and substance abuse diagnoses (also classified separately).

Table A7: NHAMCS vs. ED Control Sample

	All	Adults aged 19-64	Insured adults aged 19-64	Uninsured adults aged 19-64	Control sample
	%	%	%	%	%
	(1)	(2)	(3)	(4)	(5)
Gender					
Male	46.38	45.08	41.44	52.07	44.69
Female	53.62	54.92	58.56	47.93	55.31
Age					
19-49	45.15	75.70	71.70	84.88	75.51
50-64	14.49	24.30	28.30	15.12	23.50
By Visit Type					
Inpatient	12.67	10.66	12.09	7.73	12.10
Outpatient	87.33	89.34	87.91	92.27	87.90
By Urgency:					
Non Emergent	19.60	20.56	20.68	21.36	19.26
Primary Care Treatable	34.19	33.74	33.36	34.22	33.66
Emergent, Preventable	7.25	6.49	6.74	6.43	7.19
Emergent, Non Preventable	22.19	22.66	22.91	21.61	21.20
Unclassified	16.76	16.55	16.32	16.39	18.69
ACSC (PQIs)	6.04	5.33	5.62	5.03	5.83
Triage Variable (NHAMCS Only)					
Immediate	3.63	3.42	3.46	3.28	.
Within 15 minutes	8.51	8.32	8.84	7.14	.
15-60 Minutes	28.56	28.89	29.84	26.98	.
1-2 hours	16.55	16.79	16.30	17.67	.
2-24 hours	8.18	8.66	7.96	10.32	.

Note: Columns 1-4 are based on the National Hospital Ambulatory Medical Care Survery (NHAMCS) years 2006-2009 (inclusive). Subpopulations are (with unweighted sample sizes in parentheses): full sample (170719), adults aged 19-64 (101930), adults aged 19-64 with a a primary expected source of payment of private or public medical insurance or "other" (70154), and adults with a primary expected source of payment of "self pay," "charity/no charge," or "unknown" aged 19-64 (27964). Column 5 is based off ED admissions of the ED control sample that occured between March 10, 2008 and September 30, 2009. The control sample is composed of individuals living in Portland-area zip codes where at least 98% of ED admissions went to one of the EDs in the study. Unlike in Table A2, this acsc classification conditions omits four categories (hypertension, heart failure without procedure, angina without a procedure and lower extremity amputation due to diabetes) that required procedure codes for classification, as these codes are not available for the NHAMCS data.

Appendix References

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