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Abstract: In 2008, Oregon initiated a limited expansion of a Medicaid program for uninsured, low-income adults, drawing names from a waiting list by lottery. This lottery created a rare opportunity to study the effects of Medicaid coverage using a randomized controlled design. Using the randomization provided by the lottery and emergency department records from Portland-area hospitals, we study the emergency department use of about 25,000 lottery participants over approximately 18 months after the lottery. We find that Medicaid coverage significantly increases overall emergency use by 0.41 visits per person, or 40 percent relative to an average of 1.02 visits per person in the control group. We find increases in emergency department visits across a broad range of types of visits, conditions, and subgroups, including increases in visits for conditions that may be most readily treatable in primary care settings.

One Sentence Summary: Using a randomized controlled design, we find that extending Medicaid coverage to uninsured low-income adults increases emergency department use.

Main text:

In describing the merits of expanding Medicaid to the uninsured, federal and state policymakers often argue that expanding Medicaid will reduce inefficient and expensive use of the emergency department (*1-4*). Expanded Medicaid coverage could, however, either increase or decrease emergency department use. On the one hand, by reducing the cost to the patient of emergency department care, expanding Medicaid could increase use and total health care costs. On the other hand, if Medicaid increases primary care access and use, or improves health, expanding Medicaid could reduce emergency department use, and perhaps even total health care costs. Despite the many claims made in public discourse, existing evidence on this topic is relatively sparse, and the results are mixed. Analyses of the 2006 health insurance expansion in Massachusetts found either unchanged (*5*) or reduced (*6*) use of emergency departments. Quasi-experimental analysis of expanded Medicaid eligibility for children found no statistically significant change in emergency department use (*7*). However, quasi-experimental evidence from young adults' changes in insurance coverage found that coverage increased emergency department use (*8, 9*). Likewise, the RAND Health Insurance Experiment from the 1970s, which randomized the level of consumer cost-sharing among insured individuals, found that more comprehensive coverage increased emergency department use (*10*).

In 2008, Oregon initiated a limited expansion of its Medicaid program for low-income adults, drawing approximately 30,000 names by lottery from a waiting list of almost 90,000 individuals. Those selected were enrolled in Medicaid if they completed the application and met eligibility requirements. This lottery presents a rare opportunity to study the effects of Medicaid coverage for the uninsured on emergency department use with a randomized controlled design. Using Oregon's Medicaid lottery and administrative data from the emergency departments of hospitals in the Portland area, we examine the impact of Medicaid coverage on emergency department use overall and for specific types of visits, conditions, and groups. The lottery allows us to isolate the causal effect of insurance on emergency department visits and care; random assignment through the lottery can be used to study the impact of insurance without the problem of confounding factors that might otherwise differ between insured and uninsured populations.

The Oregon Health Insurance Experiment. The lottery studied here was for Oregon Health Plan (OHP) Standard, a Medicaid expansion program that provides benefits to low-income adults who are not categorically eligible for Oregon's traditional Medicaid program. To be eligible, individuals must be aged 19-64, Oregon residents, U.S. citizens or legal immigrants, without health insurance for six months, and not otherwise eligible for Medicaid or other public insurance. They must have income below the federal poverty level (which was $10,400 for an individual and $21,200 for a family of 4 in 2008) and have less than $2,000 in assets. 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.

Oregon conducted eight lottery drawings from a waiting list for this Medicaid program between March and September 2008. Among the individuals randomly selected by lottery, those who completed the application process and met the eligibility criteria were enrolled (see Fig. S1). The lottery process and the insurance program are described in more detail elsewhere (*11*). Multiple institutional review boards have approved the Oregon Health Insurance Experiment research.

Our prior work on the Oregon Health Insurance Experiment used the random assignment of the lottery to study the impacts of the first two years of Medicaid coverage (*11-13*). We found that Medicaid improved self-reported general health and reduced depression; we did not find statistically significant effects on measured physical health, specifically blood pressure, cholesterol, or glycated hemoglobin levels. We also found that Medicaid decreased financial strain, but did not have statistically significant effects on employment or earnings. Perhaps most directly relevant to the current analysis, we found that Medicaid increased health care use. In particular, we found that Medicaid coverage increased self-reported access to and use of primary care, as well as self-reported use of prescription drugs and preventive care. Interestingly, we found no statistically significant effect of Medicaid on self-reported use of the hospital or the emergency department; however we did find that Medicaid increased hospital use as measured in hospital administrative data. We return to this disparity between estimates from self-reported and administrative data below.

Data. We obtained visit-level data for all emergency department visits to twelve hospitals in the Portland area from 2007 through 2009. Individuals residing in Portland and neighboring suburbs almost exclusively use these twelve hospitals (see Fig. S2). These hospitals also are responsible for nearly half of all inpatient hospital admissions in Oregon (*14*). We briefly describe the data here; additional details are given in the supplementary materials (*15*). The data are similar to those included in the National Emergency Department Sample (*16*) and include a hospital identifier, date and time of visit, detail on diagnoses, and whether the visit resulted in the patient being admitted to the hospital. We probabilistically matched these data to the Oregon Health Insurance Experiment study population based on name, date of birth and gender. We use these data to count emergency department visits and to characterize the nature of each visit, including the reason for the visit and whether it was an outpatient visit or resulted in a hospital admission.

The state provided us with detailed data on Medicaid enrollment for everyone on the lottery list. We use this to construct our measures of Medicaid coverage. We also obtained pre-randomization demographic information that people provided when they signed up for the lottery. We use these data (*17*), together with pre-randomization measures of our outcome variables, in our examination of treatment and control balance.

We collected survey data from individuals on the lottery list, including Oregon-wide mail surveys about 1 year after the lottery and Portland-area in-person interviews about 2 years after the lottery. We use these data, described in more detail elsewhere (*11*, *12*), to compare previously reported findings on self-reports of overall emergency department use to the results in the administrative data.

Our study period includes March 10, 2008 (the first day that anyone was notified of being selected in the lottery) through September 30, 2009 (the end date used in our previous analysis of administrative and mail survey data (*11*)). 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. Our pre-randomization period includes January 1, 2007 (the earliest date in the data) through March 9, 2008 (just before the first notification of lottery selection).

Statistical analysis. The analyses reported here were pre-specified and publicly archived (*18*). Pre-specification was done to minimize issues of data and specification mining and to provide a record of the full set of planned analyses.

We compare outcomes between the "treatment group" (those randomly selected in the lottery) and the "control group" (those not randomly selected). Those randomly selected could enroll in the lotteried Medicaid program (OHP Standard) if they completed the application and met eligibility requirements; those not selected could not enroll in OHP Standard. Our intent-to-treat analysis, comparing the outcomes in the treatment and control groups, provides an estimate of the causal effect of winning the lottery (and being permitted to apply for OHP Standard).

Of greater interest may be the effect of Medicaid coverage itself. Not everyone selected by the lottery enrolled in Medicaid; some did not apply and some who applied were not eligible for coverage (*19*). To estimate the causal effect of Medicaid coverage, we use a standard instrumental-variable approach with lottery selection as an instrument for Medicaid coverage. This analysis uses the lottery's random assignment to isolate the causal effect of Medicaid coverage (*20*). Specifically, it estimates a local average treatment effect capturing the causal effect of Medicaid for those who were covered because of the lottery, under the assumption that winning the lottery only impacts the outcomes studied through Medicaid coverage. In earlier work, we explored potential threats to this assumption and, where we could investigate them, did not find cause for concern (*11*). Imperfect (and non-random) take-up of Medicaid among those selected in the lottery reduces statistical power, but does not confound the causal interpretation of the effect of Medicaid.

In the main tables and text, we present local-average-treatment-effect estimates of the effect of Medicaid coverage. In Tables S2-S5, we also present intent-to-treat estimates of the effect of lottery selection (i.e. of winning permission to apply for OHP Standard). Both the intent-to-treat and local-average-treatment-effect estimates are driven by the variation created by the lottery, and the p-values are the same for both sets of estimates. The intent-to-treat estimate may be a relevant parameter for gauging the effect of the ability to apply for Medicaid; the local-average-treatment-effect estimate is the relevant parameter for evaluating the causal effect of Medicaid for those actually covered.

The supplementary materials provide more detail on our analytic specifications (*15*). We analyze outcomes at the level of the individual. Because the state randomly selected individuals from the lottery list, but then allowed all of the selected individuals' household members to apply for insurance, an individual's treatment probability (i.e. the probability of random selection in the lottery) varies by the number of the individual's household members on the list. To account for this, all analyses control for indicators for the individual's number of household members on the list (who were linked through a common identifier used by the state) and all standard errors are clustered according to household. Except where we stratify on pre-randomization use of the emergency department, outcome analyses also control for the pre-randomization version of the outcome (such as the presence of an emergency department visit in the pre-March 2008 period when examining the outcome of having an emergency department visit in the post-March 2008 study period). This is not required to estimate the causal effect of Medicaid, but, by explaining some of the variance in the outcome, may improve the precision of the estimates. Our results are not sensitive either to excluding these pre-randomization versions of the outcomes or to additionally including demographic characteristics (measured prior to randomization) as covariates (see Table S15). We fit linear models all outcomes; our results are not sensitive to instead estimating the average marginal effects from logistic regressions for binary outcomes or negative binomial regressions for continuous outcomes (see Table S16).

Emergency department analysis sample. We restrict our analysis to individuals who at the time of the lottery lived in a zip code where residents almost exclusively use one of the twelve hospitals in our data (*15*). Fig. S1 shows the evolution of the study population from submitting names for the lottery to inclusion in the emergency department analysis sample. Because of the zip code restriction, our analysis sample includes about one-third of the full Oregon Health Insurance Experiment study population. Table 1 shows the characteristics of the included sample. As expected, there is no difference in probability of inclusion in our analytic sub-sample between those selected in the lottery ("treatments") and those not selected ("controls") (-0.1 percentage points; SE 0.4). There are also no statistically significant differences between the groups in demographic characteristics measured at the time of lottery sign-up (F-statistic 1.498; P= 0.152), in measures of emergency department use in the pre-randomization period (F-statistic 0.909; P= 0.622), or the combination of both (F-statistic 1.013; P= 0.448).

Insurance coverage. In our analysis, we define Medicaid coverage as being covered at any point during the study period (March 10, 2008 to September 30, 2009) by any Medicaid program. This includes both the lotteried Medicaid program (OHP Standard) and the other non-lotteried Medicaid programs. The non-lotteried Medicaid programs are available to any low-income individual falling into particular eligibility categories, such as being pregnant or disabled; some individuals in both our treatment and control groups became covered through one of these alternative channels.

Being selected in the lottery increases the probability of having Medicaid coverage at any point during our study period by 24.7 percentage points (SE = 0.6). As shown in Table S7, the lottery affects coverage through increasing enrollment in the lotteried Medicaid program. Previous estimates from survey data suggest that there is no "crowd-out" of private insurance; the lottery does not affect self-reports of private insurance coverage (*11, 12*). For those who obtained Medicaid coverage through the lottery, there is an increase of 13.2 months of Medicaid coverage (SE = 0.2). This is less than the 18 months of the study period for several reasons: lottery selection occurred in 8 draws between March and October 2008, initial enrollment in Medicaid took 1-2 months after lottery selection, and some of those enrolled in Medicaid through the lottery lost coverage by failing to recertify as required every 6 months.

Emergency department use. As shown in Table 2, Panel A, Medicaid increases emergency department use. In the control group, 34.5 percent of individuals have an emergency department visit during our 18-

month study period. Medicaid increases the probability of having a visit by 7.0 percentage points (SE=2.4; P=0.003). Medicaid increases the number of emergency department visits by 0.41 visits (SE=0.12; P<0.001), a 41 percent increase relative to the control mean of 1.02 visits.

Table 2, Panel B, shows the effects of Medicaid on emergency department use separately for those with no visits, one visit, two or more visits, and five or more visits in the period prior to randomization. We also look at those with two or more outpatient visits (visits that did not result in a hospital admission) prior to randomization. In all groups, Medicaid increases use (although results are not statistically significant in most of the smaller sub-samples).

We also examine how the effects of Medicaid on emergency department use differ in various other subgroups (see Table S14 for estimates). Across the numerous sub-populations we consider, we do not find any in which Medicaid causes a statistically significant decline in emergency department use; indeed, with one exception, all of the point estimates are positive. The increase in emergency department use is larger for men than for women; there is some evidence of larger increases for younger individuals than for older individuals and of larger increases for those in poorer health.

Types of emergency department visits. We separate visits by whether they resulted in a hospital admission and by what time of day they occurred (Table 3). About 90 percent of emergency department visits in the control sample are outpatient visits. The increase in emergency department use from Medicaid is solely in outpatient visits; we find no statistically significant effect of Medicaid on emergency department visits that result in an inpatient admission to the hospital.

We next separate visits into those occurring during "on-hours" (7am – 8pm Monday through Friday) and those occurring during "off-hours" (nights or weekends). Just over half of the visits in our control sample occur during on-hours. Both on- and off-hours use increases with Medicaid coverage.

We also classify visits using an algorithm developed by Billings et al (*21*) that is based on the primary diagnosis code for the visit. Fig. S3 provides more detail on this algorithm and the most common conditions contributing to each classification. Those visits that require immediate care in the emergency department and that could not have been prevented are referred to as "emergent, not preventable" (21% of control sample visits). Visits that require immediate care in the emergency department, but could have been prevented through timely ambulatory care are referred to as "emergent, preventable" (7%). Those visits that require immediate care, but that could be treated in an outpatient setting, are referred to as "primary care treatable" (34%). Visits that do not require immediate care are classified as "non-emergent" (19%) (*22*). Table 4 shows that Medicaid statistically significantly increases visits in all classifications except for the "emergent, non-preventable" category. The increases are most pronounced in those classified as "primary care treatable" (0.18 visits; SE=0.05; P<0.001) and "non-emergent" (0.12 visits; SE=0.04; P=0.001). We also examine the impact of Medicaid on visits for a variety of different conditions (Table S11) – although even the most prevalent individual conditions represent a relatively small share of emergency department visits (see Table S10). We do not find that Medicaid causes a statistically significant decrease in emergency department use for any of the conditions we consider; indeed, once again the vast majority of point estimates are positive. We find statistically significant increases in emergency department use for several specific conditions, including injuries, headaches, and chronic conditions.

Comparison to results from self-reports. Table 5 compares the results of this analysis of administrative records to previously reported results from our mail survey data (*11*) and our in-person interview data (*12*). Panel A summarizes the previously reported effects of Medicaid on overall emergency department use (the only outcome measured in the self-reported data) in each of the three data sources. In contrast to the results from administrative records, neither set of self-reports produced statistically significant changes in emergency department use. In prior work, we similarly found statistically significant effects of Medicaid on hospital use as measured in administrative data but not as measured in self-reports (*11*). This suggests there may be some systematic reasons that changes in use are detectable in administrative data but not in self-reported data.

The results from the administrative data may differ from results from the self-reported data for a variety of reasons. We briefly summarize them here and provide more detail in the supplementary materials (*15*). First, the timeframe of analysis is different; in particular, we are able to study outcomes over longer look-back periods in the administrative data. Second, the study populations are different; in particular, the self-reported data are by necessity limited to individuals who respond to the surveys or complete the interviews. Third, self-reports may differ from the administrative record even for the same individual over the same timeframe (because of incorrect recollections, for example, or mistakes about the site of care).

Panels B and C attempt to disentangle these factors by limiting the analysis to the same set of individuals and capturing use over the same timeframe. In Panel B, for respondents to the mail survey who are also in the administrative data sample, we compare results from self-reported use in the surveys to results from the administrative data for the same 6-month look-back period as the survey. We do the same in Panel C for the in-person interviews: for respondents to the in-person interview who are also in the administrative data sample, we compare results from self-reported use to results from the administrative data for the same 12-month look-back period as the interview.

For the same individuals and timeframes, our estimates are more precise in the administrative data than in the self-reports (Panels B and C). We mostly do not estimate statistically significant increases in emergency department use even in the administrative data (second rows of Panels B and C), but the estimates are broadly consistent with those in the full emergency department administrative data.

These results highlight important advantages of administrative data. Even for outcomes that can be self-reported, the emergency department administrative data are able to capture a longer look-back period and may have less misclassification, allowing for more precise estimates. An additional advantage of administrative data, of course, is that all of the analyses performed elsewhere in the paper on timing of visits and the detailed classification of visit type are only realistically possible with administrative records.

Discussion. Neither theory nor existing evidence provides a definitive answer to the important policy question of whether we should expect increases or decreases in emergency department use when Medicaid expands. All else equal, basic economic theory suggests that by reducing the out-of-pocket cost of a visit that an uninsured person would face, Medicaid coverage should increase use of the emergency department. It is also possible that Medicaid coverage may increase real or perceived access to emergency department care. There are, however, several potential offsetting channels by which Medicaid coverage could decrease emergency department use. Uninsured patients may seek treatment in the emergency department because of the legal requirement that hospitals provide care for emergent conditions regardless of insurance status (*23*). By increasing access to primary care, Medicaid coverage might allow patients to receive some care in physician offices rather than in the emergency department. Additionally, Medicaid coverage might lead to improved health and thus reduced need for emergency department care.

It is difficult to isolate the impact of Medicaid on emergency department use in observational data, since the uninsured and Medicaid enrollees may differ on many characteristics (including health and income) that are correlated with use of the emergency department. Indeed, we show in Table S17 that observational estimates that do not account for such confounding factors suggest much larger increases in emergency department use associated with Medicaid coverage than the results from our randomized controlled setting.

Using the random assignment of the Oregon lottery, we can isolate the causal effect of Medicaid coverage on emergency department use among low-income, uninsured adults. We find that Medicaid increases emergency department use. We estimate an average increase of 0.41 visits per covered person over an 18-month period, or about a 40 percent increase relative to the control average of 1.02 visits. A back-of-the-envelope calculation, using $435 as the average cost of an emergency department visit (*24*), suggests that Medicaid increases annual spending in the emergency department by about $120 per covered individual.

We also examine the impact of Medicaid on types of visits, conditions, and populations where we might expect the offsetting effects to be the strongest. In none of these do we detect a decline in emergency department use. Emergency department use increases even in classes of visits that might be most substitutable for other outpatient care, such as those during standard hours (on-hours) and those for "non-emergent" and "primary care treatable" conditions. This is in contrast to prior, quasi-experimental work finding that health insurance decreased this type of emergency department visit (*6*). We also find that Medicaid increases "emergent, preventable" visits, or visits for conditions likely preventable by timely outpatient care. By contrast, there is no statistically significant change in "emergent, non-preventable" visits. Relying on eventual diagnosis (as we do in our decomposition of visits types) can be problematic and may not accurately differentiate necessary and unnecessary emergency department use (*25*, *26*). However, the overall picture is similar using different classification systems (such as on-hour visits relative to off-hour visits, or outpatient emergency department visits relative to inpatient emergency department visits).

One interpretation of these findings is that Medicaid did not decrease emergency department use because it did not improve health or increase access to and use of primary care. The prior findings of the Oregon Health Insurance Experiment address this conjecture. They indicate that the increase in emergency department use occurred despite Medicaid increasing access to other types and sites of care, even within the first year. Medicaid increased self-reported primary care use, including outpatient physician visits, prescriptions, and recommended preventive care. Medicaid also improved self-reported access to and quality of care, such as getting all of the care needed, receiving high quality care, and having a usual place of care that was not an emergency department. The evidence on health is more mixed; Medicaid improved self-reported health and decreased depression in this population, but it did not produce statistically significant improvements in several different measures of physical health (*11*, *12*).

Our estimates of the impact of Medicaid on emergency department use apply to able-bodied, uninsured adults with income below the federal poverty level who express interest in insurance coverage. This population is of considerable policy interest given states' opportunity to expand Medicaid to all adults up to 138 percent of the federal poverty level under the Affordable Care Act. There are, however, important limits to the generalizability of our findings. Our sample population differs on several dimensions from those who will be covered by other Medicaid expansions (*11*, *19*). For example, ours is disproportionately white and urban-dwelling. It is also a population who voluntarily signed up for coverage; effects may differ in a population covered by an insurance mandate. In addition, we examine changes in emergency department use for people gaining an average of 13 months of coverage; longer-run effects may differ. Finally, the newly insured in our study comprise a very small share of the uninsured or total population in Oregon, limiting the system-level effects that insuring a larger share of the population might generate (*27*).

These limitations to generalizability notwithstanding, our study is able to make use of a randomized design that is rarely available in the evaluation of social insurance programs to estimate the causal effects of Medicaid on emergency department care. We find that expanding Medicaid coverage increases emergency department use across a broad range of visit types, including visits that may be most readily treatable in other outpatient settings. These findings speak to one cost of expanding Medicaid, as well as its net effect on the efficiency of care delivered, and may thus be a useful input for informed decision-making balancing the costs and benefits of expanding Medicaid.

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39. For emergency department visits coded as transferred to another hospital (1.4% of all visits and 12.4% 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.
40. This analysis was not pre-specified.

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Replication code and a modified version of the data are available on the Oregon Health Insurance Experiment website (http://www.nber.org/oregon/data).

Table 1. Treatment-control balance. We report the control mean (with standard deviation for continuous variables in parentheses) and the estimated difference between treatments and controls (with standard errors in parentheses) for the outcome shown in the left hand column. The final rows report the pooled F-statistics and p-values from testing treatment-control balance on sets of variables jointly. These sets include the lottery list variables in Panel B, the pre-randomization versions of our outcome variables (see Table S6), and the combination. Panel A sample consists of individuals in the full Oregon Health Insurance Experiment (OHIE) sample (N=74,922); Panel B sample consists of individuals in Portland-area zip codes (N=24,646), also referred to as the emergency department (ED) analysis sample.

	Control mean	Treatment-control difference*
Panel A: Percent of full OHIE sample included in ED analysis sample		
Included in ED analysis sample (%)	33.3	-0.1
		(0.4)
Panel B: Lottery list characteristics, conditional on being in ED analysis sample		
Year of birth	1968.3	0.1
	(12.1)	(0.2)
Female (%)	55.4	-1.0
		(0.6)
English as preferred language (%)	87.5	0.9
		(0.5)
Signed up self for lottery (%)	92.9	0.1
		(0.0)
Signed up first day of lottery (%)	9.1	0.6
		(0.4)
Gave phone number (%)	86.6	0.3
		(0.5)
Address a PO Box (%)	2.6	0.1
		(0.2)
Zip code median household income ($)	43027	182
	(9406)	(136)
F-statistic for lottery list variables		1.498
p-value		0.152
F-statistic for pre-randomization versions of the outcome variables		0.909
p-value		0.622
F-statistic for lottery list and pre-randomization variables		1.013
p-value		0.448

*For variables that are percentages, the treatment-control differences are shown as percentage points.

Table 2. Emergency department use. We report the estimated effect of Medicaid on emergency department use over our study period (March 10 2008 – September 30 2009) in the entire sample and in subpopulations based on pre-randomization emergency department use. For each subpopulation, we report the sample size, the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses), the estimated effect of Medicaid coverage (with standard error in parentheses), and the p-value of the estimated effect. Sample consists of individuals in Portland-area zip codes (N=24,646) or specified subpopulation (N in table).

		Percent with any visits*			Number of visits†		
	N	Mean Value in Control Group	Effect of Medicaid Coverage	p-value	Mean Value in Control Group	Effect of Medicaid Coverage	p-value
Panel A: Overall							
All Visits	24646	34.5	7.0 (2.4)	0.003	1.022 (2.632)	0.408 (0.116)	<0.001
Panel B: By emergency department use in the pre-randomization period							
No visits	16930	22.5	6.7 (2.9)	0.019	0.418 (1.103)	0.261 (0.084)	0.002
One visit	3881	47.2	9.2 (6.0)	0.127	1.115 (1.898)	0.652 (0.254)	0.010
Two+ visits	3835	72.2	7.1 (5.6)	0.206	3.484 (5.171)	0.380 (0.648)	0.557
Five+ visits	957	89.4	0.7 (8.3)	0.932	6.948 (7.635)	2.486 (2.079)	0.232
Two+ outpatient visits	3402	73.2	9.6 (6.0)	0.111	3.658 (5.375)	0.560 (0.742)	0.450

*For the percent-with-any-visits measures, the estimated effects of Medicaid coverage are shown as percentage points.
†The number-of-visits measures are unconditional, including those with no visits.

Table 3. Emergency department use by hospital admission and timing. We report the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses), the estimated effect of Medicaid coverage (with standard error in parentheses), and the p-value of the estimated effect. Visits are on-hours if occurring 7am – 8pm Monday through Friday and off-hours otherwise. Sample consists of individuals in Portland-area zip codes (N=24,646).

	Percent with any visits*			Number of visits†		
	Mean Value in Control Group	Effect of Medicaid Coverage	p-value	Mean Value in Control Group	Effect of Medicaid Coverage	p-value
By hospital admission:						
Inpatient visits	7.5	-1.2	0.385	0.126	-0.023	0.396
		(1.3)		(0.602)	(0.028)	
Outpatient visits	32.0	8.2	<0.001	0.897	0.425	<0.001
		(2.4)		(2.362)	(0.107)	
By timing of visit:						
On-hours visits	25.7	5.7	0.010	0.574	0.232	0.001
		(2.2)		(1.555)	(0.072)	
Off-hours visits	21.9	6.1	0.005	0.456	0.208	0.002
		(2.2)		(1.394)	(0.068)	

*For the percent-with-any-visits measures, the estimated effects of Medicaid coverage are shown as percentage points.
†The number-of-visits measures are unconditional, including those with no visits.

Table 4. Emergency department use by type of visit. We report the control mean of the dependent variable (with standard deviation in parentheses), the estimated effect of Medicaid coverage (with standard error in parentheses), and the p-value of the estimated effect. We use the Billings et al *(21)* algorithm to assign probabilities of a visit being each type, and therefore analyze only the number of visits (not the percent with any visits) as obtained by summing the probabilities across all visits for an individual. We use the abbreviation ED for emergency department. Sample consists of individuals in Portland-area zip codes (N=24,646).

	Number of visits*		
	Mean Value in Control Group	Effect of Medicaid Coverage	p-value
Requires Immediate Care			
Emergent, Not Preventable (Requires ED care, could not have been prevented)	0.213 (0.685)	0.049 (0.033)	0.138
Emergent, Preventable (Requires ED care, could have been prevented)	0.074 (0.342)	0.038 (0.018)	0.032
Primary Care Treatable (Does not require ED care)	0.343 (0.948)	0.180 (0.046)	<0.001
Does Not Require Immediate Care			
Non-emergent	0.201 (0.688)	0.118 (0.035)	0.001
Unclassified	0.196 (0.734)	0.059 (0.037)	0.107

*The number-of-visits measures are unconditional, including those with no visits.

Table 5. Comparing results from administrative data and self-reports. We report the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses), the estimated effect of Medicaid coverage (with standard error in parentheses), and the p-value of the estimated effect. In Panel A, we report the estimates from Table V in Finkelstein et al *(11)*, from Table 5 in Baicker et al *(12)*, and from Table 2. Table 5 in Baicker et al *(12)* reports only the number-of-visit measure; here we also present the percent-with-any-visits measure analyzed using the same methodology. In Panels B and C, we limit the previously published analyses to individuals also in the emergency department data, and compare the self-reported answers to the survey questions to the answers to the same survey questions constructed from administrative data.

		Percent with any visits*			Number of visits†		
	N	Mean Value in Control Group	Effect of Medicaid Coverage	p-value	Mean Value in Control Group	Effect of Medicaid Coverage	p-value
Panel A: Baseline Estimates							
Mail survey 6 months before response	23741	26.1	2.2 (2.3)	0.335	0.470 (1.037)	0.026 (0.056)	0.645
In-person interview 12 months before interview	12229	40.2	5.4 (4.1)	0.189	0.997 (1.999)	0.094 (0.166)	0.572
Emergency department data 18-month study period	24646	34.5	6.97 (2.4)	0.003	1.022 (2.632)	0.408 (0.116)	<0.001
Panel B: Limited to overlap sample between mail survey and emergency department data							
Self-report of use 6 months before response	7239	25.6	-0.01 (4.2)	0.997	0.482 (1.090)	-0.046 (0.107)	0.666
Administrative record of use 6 months before response	7239	16.2	4.6 (3.6)	0.197	0.296 (0.933)	0.052 (0.085)	0.538
Panel C: Limited to overlap sample between in-person and emergency department data							
Self-report of use 12 months before interview	10178	40.2	6.0 (4.5)	0.179	0.980 (1.959)	0.150 (0.177)	0.396
Administrative record of use 12 months before interview	10178	26.8	6.8 (4.0)	0.089	0.635 (1.828)	0.351 (0.168)	0.037

*For the percent-with-any-visits measures, the estimated effects of Medicaid coverage are shown as percentage points.

†The number-of-visits measures are unconditional, including those with no visits.

Supplementary Materials:
Materials and Methods
Supplementary Text
Figures S1-S3
Tables S1-S17
References (28-40)

Materials and Methods

Emergency department data

The state of Oregon does not routinely collected emergency department data. We therefore worked with the Oregon Association of Hospitals and Health Systems to obtain 2007-2009 emergency department data for all visits from twelve hospitals in the Portland metro area. The data include emergency department records, and for patients admitted to that same hospital, inpatient records. For patients transferred to another hospital, the data indicate if the individual was admitted to a hospital, but do not include detailed inpatient records. Hospital admissions for normal childbirth are not considered as originating in the emergency department and are not included in emergency department data. We restricted the sample to exclude a small number of visits for complications of pregnancy and childbirth that do appear.

We probabilistically matched the Oregon Health Insurance Experiment population to the emergency department data using name, date of birth and gender provided at the time of lottery sign-up. We used LinkPlus software which is based on the theoretical framework developed by Fellegi and Sunter (*28*). Although the performance of the software will vary based on the setting and the data sources, in controlled tests this software has been shown to produce matches with positive predictive value and sensitivity both over 94 percent (*29*). Due to the protected nature of the data, the match was conducted by members of our study team on-site at the Office for Oregon Health Policy and Research (OHPR) under the auspices of OHPR personnel, who then provided the study team with data that included the matched study identifier but excluded the personally-identifying matching variables.

Emergency department analysis sample

Fig. S1 shows the evolution of the study population from submitting names for the lottery to inclusion in the emergency department analysis ("emergency department analysis 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. This strategy is designed to alleviate concerns that individuals may consider going to emergency departments outside of the twelve we observe, and that Medicaid coverage could affect this selection.

To identify areas that primarily rely on one of the twelve hospitals in our data for emergency department care, we use 2008 and 2009 hospital discharge data for all fifty-eight hospitals in the entire state of Oregon. (We have hospital discharge data for the whole state, but emergency department data only for twelve Portland-area hospitals). To try to better proxy for patterns of emergency department use in hospital discharge data, we consider only hospital admissions that originated in the emergency department. For each zip code of residence in Oregon, we calculated the percent of these hospital admissions (originating in the emergency department) that was at one of our twelve hospitals. We restrict our analysis to zip codes where this percent very close to 100%. Specifically, we limit our "emergency department analysis sample" to individuals who at the time of lottery sign up were residing in a zip code where this percent was 98% or higher. The resulting sample includes 70 zip codes (Fig. S2 shows a map of the included zip codes) and 24,646 individuals, about one-third of the full analysis sample for the Oregon Health Insurance Experiment (*30*).

Table S1 compares our emergency department analysis sample to the full Oregon Health Insurance Experiment sample. The emergency department analysis sample includes only urban zip codes. The included individuals are more likely to have requested lottery materials in a language other than English and are less likely to have PO Box address, but otherwise are not very different from the full sample.

Analytic specifications

All of our regression specifications leverage the random assignment from the lottery to make comparisons between the treatment and control group. The lottery randomly assigned permission to apply for the lotteried Medicaid program, OHP Standard. We can estimate the effect of lottery selection by fitting ordinary least squares regressions and comparing the average outcome for all individuals selected in the lottery to the average outcome for all control individuals. This is an intent-to-treat estimate. We can estimate the effect of insurance by fitting two-stage least squares regressions (with lottery selection as an instrument for insurance coverage) and estimating the local average treatment effect of Medicaid coverage. Both approaches use the randomization of the lottery to estimate causal effects. The statistical inferences (p-values) are the same for both approaches as well, although the point estimates and standard errors are scaled differently. In our main tables (Tables 2-5), we report the estimates of the local-average-treatment-effect estimates of the effect of Medicaid coverage. Tables S2-S5 show both these estimates of the effect of Medicaid coverage and the intent-to-treat estimates of the effect of lottery selection.

Effect of Lottery Selection (Intent to Treat)

We estimate the intent-to-treat effect of lottery selection (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 the lotteried Medicaid program, OHP Standard.

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; 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 OHP Standard. 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 a causal estimate of the effect of lottery selection, as they are not related to treatment status, but they may improve the precision of the estimates. Our primary analysis adds as an additional covariate only the pre-randomization version of the outcome (i.e. the analogous outcome measured between January 1, 2007 and March 9, 2008). We use a missing indicator to handle the small number of missing pre-randomization observations. As a secondary analysis, we show results are not sensitive to other choices for the V_{ih} covariates (see Table S15).

In all of our intent-to-treat 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 pose no concerns in the absence of covariates or in fully saturated models (*31, 32*). 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 probabilities. We therefore are reassured that our results are not sensitive to an alternate specification using logistic regression and calculating average marginal effects for all binary outcomes (see Table S16). Our results for continuous outcomes are also not sensitive an alternate specification using negative binomial regression and calculating average marginal effects (see Table S16).

In all of our analyses, we cluster the standard errors on the household identifier since the treatment is at the household level; this allows for an arbitrary variance-covariance matrix for individuals within the same household.

The intent-to-treat analysis yields the casual effect of being able to apply for OHP Standard (the lotteried Medicaid program) under two key assumptions: assignment of the ability to apply for OHP Standard was in fact randomized in the way described, and treatment and control individuals in the sub-samples we use to analyze outcomes are not differentially selected from the full sample. The randomization procedure and verification of it was previously described (*11*). Differences in the inclusion rate of treatment and controls in our emergency department analysis sample, or in the pre-randomization characteristics of the treatment and control analysis samples would raise concerns about the second key assumption. The balance of treatment and controls on these dimensions shown in Table 1 and discussed further below is consistent with the identifying assumption.

Effect of Medicaid Coverage

The intent-to-treat estimates from equation (1) provide an estimate of the causal effect of winning the lottery (i.e. winning permission 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:

$$y_{ih} = \pi_0 + \pi_1 MEDICAID_{ih} + X_{ih}\pi_2 + V_{ih}\pi_3 + \nu_{ih} \tag{2}$$

where MEDICAID is defined as ever being on OHP Standard or any other Medicaid program at any point during the study period. All other variables are as defined in equation (1). We estimate equation (2) by instrumental variable regression, using the following first stage equation:

$$MEDICAID_{ih} = \delta_0 + \delta_1 LOTTERY_h + X_{ih}\delta_2 + V_{ih}\delta_3 + \mu_{ih} \tag{3}$$

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

We interpret the coefficient on MEDICAID from the instrumental variable estimation of equation (2) as the local average treatment effect of Medicaid (*20*). In other words, our estimate of π_1 identifies the causal impact of Medicaid among the subset of individuals who obtain Medicaid upon winning the lottery but who would not obtain Medicaid without winning the lottery (i.e. the compliers).

The estimated local average treatment effect is the causal effect of Medicaid for those who were covered because of the lottery. This 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 Medicaid coverage. We believe this is a reasonable approximation; in earlier work we discussed potential violations, and, where we could explore them, we did not find cause for concern (*11*).

Treatment-control balance

Table 1 shows the treatment and control balance on inclusion in our analytic sample and on demographic characteristics measured prior to randomization ("lottery list characteristics") in the emergency department analysis sample. It also reports F-statistics of treatment-control balance on pre-randomization versions of all our outcome variables in the emergency department analysis sample. Table S6 includes the detail on balance for each individual pre-randomization versions of all of our outcome variables.

Impact of the lottery on insurance coverage

Table S7 reports the control means and effects of lottery selection for various definitions of insurance coverage. The first row shows results for the measure ("on Medicaid at any point in the study period") which is used in all the analyses presented in the paper for estimating the effect of Medicaid. The results in the first row show that being selected in the lottery is associated with an increase of 24.7 percentage points (SE=0.6) in the probability of having Medicaid coverage during our study period. We define "Medicaid" to include any Medicaid programs, including OHP Standard and all of Oregon's other Medicaid programs; we define someone as "on Medicaid at any point in the study period" if they were covered for at any time between March 10, 2008 and July 30, 2009. Using one of the alternate definitions described below would change the magnitude of our estimated effects of Medicaid coverage, but not the associated p-values or statistical inference.

Since the lottery was for the OHP Standard program specifically, that is where we would expect to find increases in coverage due to the lottery. The second row of Table S7 indicates that this is the case. 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. Previous estimates from interview data suggest that the increase in Medicaid coverage does not come at the expense of private insurance coverage; there is no "crowd-out" of private insurance (*11, 12*).

The effect of the lottery on Medicaid coverage attenuates over time: if Medicaid coverage is defined by coverage at the end of the study period (on September 30, 2009) instead of "ever on Medicaid during our study period", the effect of the lottery on Medicaid coverage falls from 24.7 percentage points (row 1) to 14.3 (row 4). There are two reasons for this. First, those who successfully enroll in Medicaid (through the lottery or other means) are required to recertify eligibility every six months, leading to attrition in coverage. Additionally over time, those not selected in the lottery may obtain coverage through other non-lotteried Medicaid programs.

Because the initial take-up of Medicaid was relatively low, lottery selection is associated with an average increase of 3.25 months on Medicaid (row 3). For those who did obtain coverage through the lottery, there is an increase of 13.2 months on Medicaid (standard error = 0.2), estimated using our standard instrumental variable regression specification with the number of months on Medicaid as the dependent variable. 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.

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. We present two tables of summary statistics on the outcome variables. Table S8 provides detail on the distribution of the outcome variables for our control sample. These are defined at the level of the individual. A given individual may have more than one emergency department visit during our study period.

Table S9 shows the frequency and percent of visits of different types to the twelve emergency departments for both our control sample and for other populations in these data. Here the unit of observation is a visit (rather than an individual) because we do not have different visits for a given individual linked together except in our study population. In addition to our control sample, we report visit-level statistics to the 12 emergency departments for all patients, for visits by adults aged 19-64, and separately visits by insured adults and uninsured adults; in all of these cases, we limit the analysis to patients from the set of zip codes in our analysis sample, as shown in Figure S2.

Except where noted, we analyze both a binary indicator for any visit of that type and a continuous measure of the number of visits for that type. For all number-of-visit variables, we truncated at $2*99^{th}$ percentile (conditional on being non-zero) but leave the binary indicator for any visits unchanged.

For each outcome measure, we also define a corresponding pre-randomization version of the same outcome for the period January 1, 2007 to March 9, 2008.

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.

Hospital admission

An emergency department visit was classified as resulting in an **inpatient visit** if the patient was either admitted as an inpatient at that hospital 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.

Timing

Emergency department visits were classified according to time of day. **On-hours visits** capture visits that occurred between 7AM and 8PM on Monday through Friday. **Off-hours visits** capture visits that occurred either on the weekend or at night. This definition of on-hours follows previous work (*6*), but the results are very similar using an alternate (not pre-specified) definition of 9AM to 5PM.

Type of visit

We use the algorithm developed by Billings et al (*21*) to classify emergency department visits using the 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 much more extensive information than available in typical discharge data like ours, each physician classified each record into one of four categories. For each primary diagnosis, the probabilities assigned by the algorithm are based on averaging all the physicians' codings across all visits with that diagnosis.

Because the algorithm assigns each visit a probability of falling into each of the categories, we do not analyze percent-with-any-visits measures. We construct the number-of-visits measures by summing the assigned probabilities across all visits within the individual.

The categories are: **non-emergent** cases where care was not required within 12 hours (e.g. a toothache), **primary care treatable** cases where care was needed within 12 hours but could be provided in a primary care setting (e.g. a lumbar sprain), **emergent, preventable** cases that the doctors judge could have been avoided with proper primary or ambulatory care (e.g. an asthma attack), or **emergent, 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 (*33*).

Fig. S3 provides an illustration of the categories and lists the three most common primary diagnosis conditions for visits in each group in our control sample. The algorithm assigns probabilities to each visit on the basis of primary diagnosis, so common conditions such as strains and sprains and skin infections are among the most frequent reason for visits across multiple categories. This reliance on probabilities derived from *ex post* diagnoses rather than *ex ante* symptoms is one of the major limitations of this measure, as has been noted elsewhere (*25, 26*).

Comparison to results from self-reports

In Tables 5 and S5, we compare our results in the administrative emergency department records to results in self-reports from our mail survey data (*11*) and our in-person interview data (*12*). We do this using the previously published results, as well as new analysis of both the self-reported and administrative data in the same set of individuals and capturing the same timeframe of use. As discussed in the main text, the three data sources (administrative emergency department records, the mail survey and the in-person interviews) differ in a number of ways.

First, the data capture different periods of emergency department use. The emergency department data covers a full 18-month period from March 10, 2008 to September 30, 2009. The mail survey covers the six months before response; the average response date is September 4, 2009. The in-person interview covers the twelve months before the interview; the average interview date is April 11, 2010.

Second, the samples include different sets of individuals. The emergency department analysis is limited geographically to the Portland area, but includes all individuals in the specified zip codes. The mail survey analysis covers individuals across the state, but includes only those study participants who responded to the survey. The in-person interview analysis is limited to the Portland-area individuals (using slightly different criteria than the emergency department zip code restriction) and includes only those study participants who completed an interview. The analysis of the mail surveys and in-person interviews are also weighted to account for sampling and data collection procedures, and, in the case of the in-person interviews, for a new lottery beginning in the fall of 2009. These weights reduce the precision of the estimates.

Third, the self-reports of emergency department use in the mail surveys and in-person interviews may differ from the administrative record even for the same individual over the same timeframe (because of incorrect recollections, for example, or mistakes about the site of care).

In Panel B, we define **self-report of use, six months before response** based on response to the mail survey question, "In the last 6 months, how many times did you go to an emergency room to get care for yourself?" We define **administrative record of use, six months before response** based on administrative records of visits for an individual in the six months prior to that individual's survey response date. In essence, we attempt to answer the survey question for the individual using the administrative data. In Panel C, we do the same for the self-reports from the in-person interviews. We define **self-report of use, twelve months before interview** based on response to the interview question, "In the last 12 months, about how many times have you gone to an emergency room or urgent care clinic?" We define **administrative record of use, twelve months prior to interview** based on administrative records of visits for an individual in the twelve months prior to that individual's interview date.

The administrative records of use do not perfectly match the self-reports, and, in general, the self-reports indicate about fifty percent more use of the emergency department than the administrative records. This can be seen by comparing the control means from the self-reports and administrative records within Panel B or within Panel C. These discrepancies may reflect incorrect recollections on the self-reports (recalling visits that occurred outside of the relevant timeframe, for example, or visits to urgent care clinics rather then hospital-based emergency departments), reducing the precision of estimates when using self-reported data.

We also explored why we estimate statistically significant effects of Medicaid coverage on use in the administrative records but not in the self-reports (see Panel A). It is not solely due to the larger sample size of analysis in the administrative data. When we limit our main analysis of any emergency department use in the study period (March 10, 2008 to September 30, 2009) to the overlapping samples, the increase in number of emergency department visits remains significant (not shown in tables). For respondents to the mail survey who are also in the administrative data sample, we estimate the effect of Medicaid on the number of visits in the administrative data is 0.537 (SE=0.209; P=0.010, N=7239). For respondents to the in-person interviews who are also in the administrative data sample, we estimate the effect of Medicaid on the number of visits in the administrative data is 0.720 (SE=0.221, P=0.001, N=10178). Thus, the fact that we can find significant effects of Medicaid in the administrative records but not the self-reports presumably reflects some combination of the ability to use a longer timeframe over which to measure use and the reduced measurement error in the outcome variable.

Supplementary Text

All of the analyses we report in the main text, and most of the analyses we report here were pre-specified and publicly archived (*18*). Pre-specification was done to minimize issues of data and specification mining and to provide a record of the full set of planned analyses. When we do present an analysis that was not pre-specified in this supplementary material, we note it in the text and mark it with the symbol ^ in the tables.

Analysis of additional outcomes

Selected conditions

We examined the impact of Medicaid on emergency department visits for a variety of conditions. We used established algorithms to identify ambulatory-care-sensitive conditions and chronic conditions and injuries; we also formed our own groupings of some prevalent conditions. Table S10 shows the diagnoses included in each category and the frequencies in our control sample. For Table S10, we group the primary diagnosis ICD-9 codes using the AHRQ Clinical Classification Software (*34*).

To identify **ambulatory care sensitive** conditions, we adapted the AHRQ's Prevention Quality Indicators algorithm. These indicators were originally developed for use in inpatient hospital data to identify, using diagnosis and procedure codes, "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" (*35*). In our control sample, nearly 7 percent of the visits are coded as ambulatory-care sensitive conditions; the most common conditions coded this way are urinary tract infections, asthma and complications of diabetes. There is substantial overlap between this categorization and the "emergent, preventable" category presented in the main text: nearly 90% of the ambulatory care sensitive visits also contribute (i.e. have non-zero probabilities assigned) to the "emergent, preventable" category.

We classified visits as **chronic** or not using the AHRQ's Chronic Condition Indicator (*36*). 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." About 20 percent of the control visits are for chronic conditions; the most common conditions coded this way are mood disorders, alcohol-related disorders, and asthma.

We used the algorithm developed by Billings et al (*21*) to identify visits for **injuries**. Just over 20 percent of control visits are for injuries; the most common conditions coded this way are sprains and strains, contusions, and open wounds of extremities. We also defined the following seven groups of diagnoses for analysis: **skin conditions, abdominal pain, back conditions, chest pain and heart problems, headache, mood disorders, and substance abuse and mental health.** The seven condition groupings are pre-specified but arbitrary; together with injuries, they capture all of the top ten most prevalent conditions, but also include other less-prevalent closely related conditions. The condition groupings are mutually exclusive, with the exception of "substance abuse and mental health" which includes mood disorders.

Table S11 presents the results for these selected conditions. In Panel A, we present the two conditions based on established algorithms, and in Panel B, we present our groupings of prevalent conditions. As discussed in the main text, there are no conditions for which Medicaid causes a statistically significant decrease in emergency department use. For many of the conditions, the estimates are imprecisely estimated, but we do find statistically significant increases in emergency department use for injuries, headaches, and chronic conditions.

List charges

We examined whether Medicaid changed the list charges for emergency department visits. 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 (*37*). We have two list charge variables for each emergency department visit: emergency department facility charges and total charges. For each individual, all charges incurred in visits during the study period are summed. **Emergency department facility charges** are only the emergency department charges (*38*). **Total charges** is the full list charge associated with the visits, including not only emergency department charges but also all inpatient charges if the patient was admitted to that hospital (*39*). We truncate both measures at 2*99th percentile (conditional on being non-zero).

The results are shown in Table S12. We see increases in both emergency department and total list charges, but both estimates are quite noisy and consistent with a wide range of changes.

Hospital sorting and type

We examined whether Medicaid changed the pattern of use across hospitals. We calculated the fraction of visits to each emergency department that were for uninsured individuals. To do this we used data from all visits to our twelve hospitals, not just those by individuals in our analysis sample, during the pre-randomization period (January 1, 2007 to March 9, 2008). 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 S13 shows the results; the emergency department use increases are similar at both types of hospitals. Table S13 also includes a global sorting test, which tests the equivalence of the proportional increases from Medicaid at each of the twelve emergency departments in our sample. We see no evidence that Medicaid increased use disproportionately at some hospitals.

Heterogeneity of results

Table S14 presents our estimates of heterogeneity by age, gender, smoking history, pre-lottery diagnoses (*40*), race, education, credit access prior to the lottery (a measure of financial security), or whether an individual signed up for the lottery on the first day (a measure of eagerness to obtain health insurance). We use data from our in-person interviews, described in detail elsewhere (*12*), to define smoking history, pre-lottery diagnoses for specific conditions, race, and education. Smoking, pre-lottery diagnoses, and education are potentially endogenous either because treatment status could change the underlying status (causing individuals to start smoking, for example) or because treatment status could change reported status (causing individuals to report earlier diagnoses, for example); we find no evidence of imbalance on any of these measures (results not shown). Analyses of the smoking, diagnoses, race and education cuts are by necessity limited to individuals who are in the emergency department analysis sample and completed an in-person interview; this is roughly 41% of our emergency department analysis sample. We use credit report data, described in detail elsewhere (*11*), to define credit access prior to the lottery; we limit that analysis to individuals included in both the credit report data and the emergency department analysis sample; this is roughly 66% of our emergency department analysis sample.

For each subgroup, we show the number included, the first stage estimate of the increase in Medicaid coverage, the control means, and the estimates of the effect of Medicaid coverage. We test whether the estimated effects differ by group, and find statistically significant differences by gender, with larger effects for men. We also find suggestive evidence of larger increases in emergency department use for those in poorer health (as indicated by smoking history and pre-lottery diagnoses). The effects by race are statistically indistinguishable, but the point estimates suggest larger increases in emergency department use in those of non-white race. Similarly, the effects by age are statistically indistinguishable, but the point estimates suggest order-of-magnitude larger effects in the younger adults. We find no statistically significant differences in effects by education, prior access to credit, or first-day lottery sign-up.

Sensitivity of results

Tables S15 and S16 investigate the sensitive of our results to, respectively, the inclusion of different covariates and the use of non-linear models. For both, we present the intent-to-treat estimates of the effect of lottery selection. In Table S15 we show estimates for our baseline specification (which includes the pre-randomization version of the outcome as a covariate), a specification without the pre-randomization version of the outcome, and a specification adding to the baseline specification additional controls for demographic characteristics taken from the original lottery list at the time of lottery sign-up (*17*). The choice of covariates does not meaningfully impact our estimates.

Table S16 re-analyzes the impact of lottery selection using logistic regression for binary outcomes and a negative binomial model for continuous outcomes. For comparison we present the baseline linear intent-to-treat estimates as well. We present the original linear estimates, and the average marginal effects from the logistic and negative binomial models. The results from the logistic and negative binomial specifications are very similar to those from the corresponding linear specification.

Comparison to observational estimates in the same setting

Table S17 compares our experimental estimates on emergency department use to what we might have estimated using observational data (*40*). The first column presents our estimates of the effect of Medicaid coverage as presented in Tables 2-5. We then compare outcomes for the insured to those for the uninsured in our sample observationally. The next columns present various "as treated" comparisons of people with and without insurance. Column 2 compares the insured to the uninsured within our entire emergency department analysis sample. Column 3 compares the insured to the uninsured in our control group, a comparison that does not include variation in insurance due to the lottery. Column 4 compares, insured to the uninsured in our treatment group, a comparison that isolates the endogenous take-up of Medicaid upon winning the lottery.

These results highlight the importance of random assignment in identifying the impact of insurance coverage. In general, the observational approaches generate larger estimates of the impact of Medicaid on emergency department use. These differences suggest that, at least within this population, individuals selecting into health insurance coverage use more medical care than those who are uninsured, as standard adverse selection theory would predict.

Publicly available data

When possible, we make available data from the Oregon Health Insurance Experiment on our website (http://www.nber.org/oregon/data). With the data, we also provide our Stata code and additional documentation. These public use data include information from the lottery list (including treatment assignment), our measures of Medicaid coverage, and survey data we collected. For the administrative emergency department data, we provide the variables capturing whether an individual had any visits of a given type (as used for the analysis in the main text and supplementary materials). For the number-of-visits variables, we censor (top-code) the publicly available variables where necessary to ensure that no value has a frequency less than ten. This means that for many of the number-of-visit outcomes, the publicly available data will not directly replicate the results presented in the main text and supplementary materials, although our findings are robust to the censoring we imposed in the public use data.

Figure S1: Study Population and Emergency Department Analysis Sample

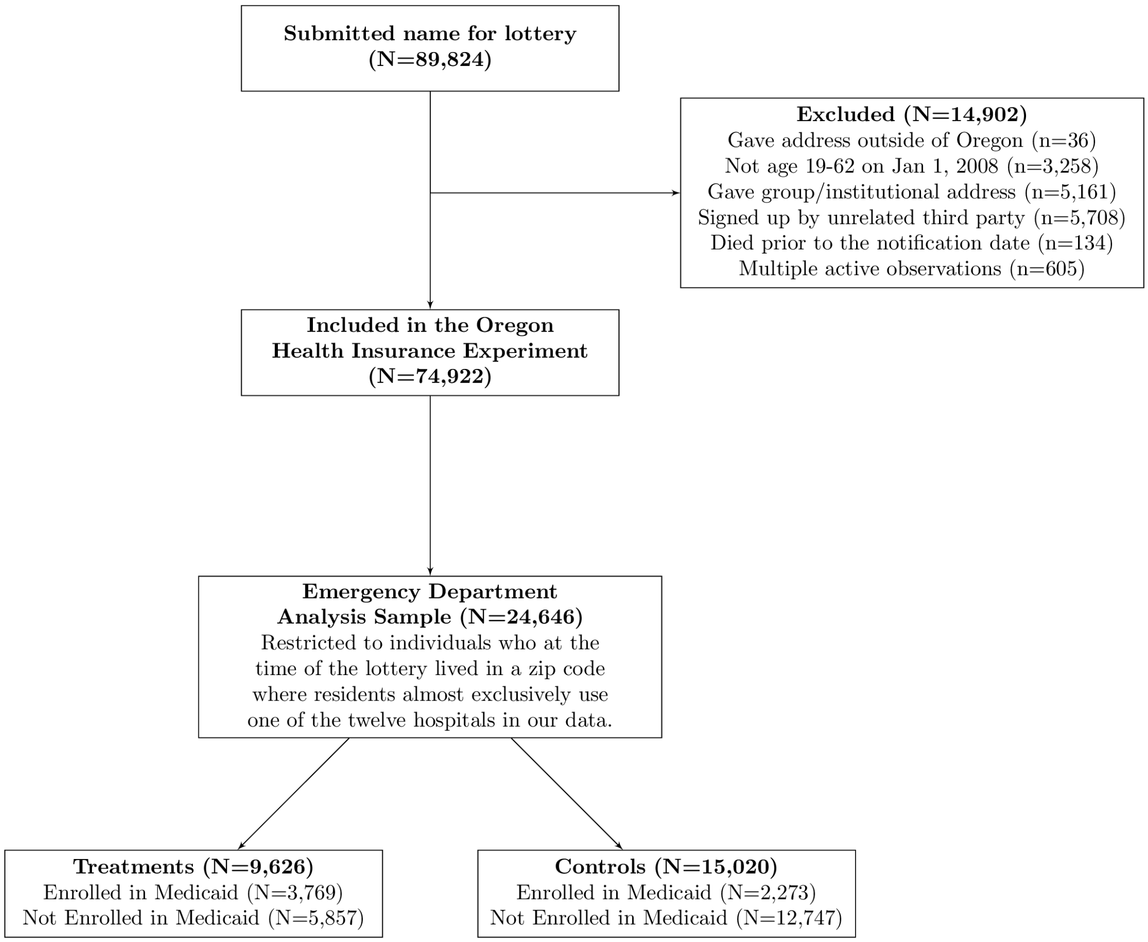
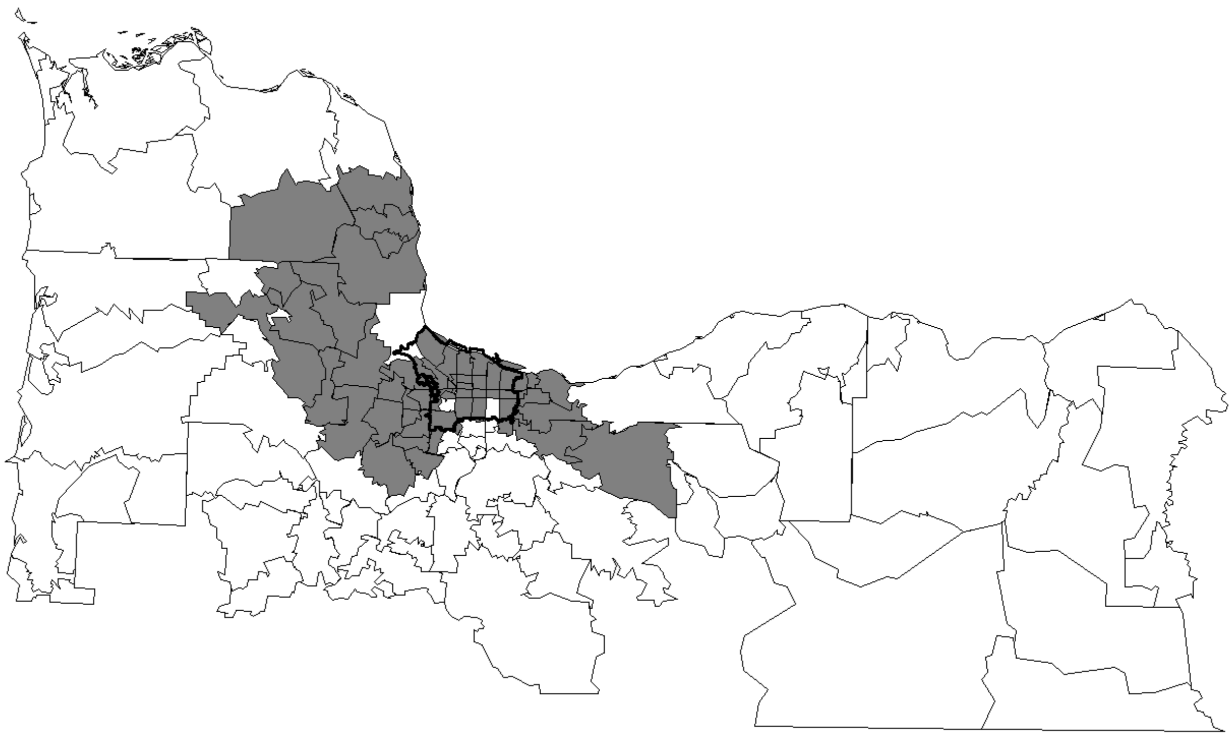
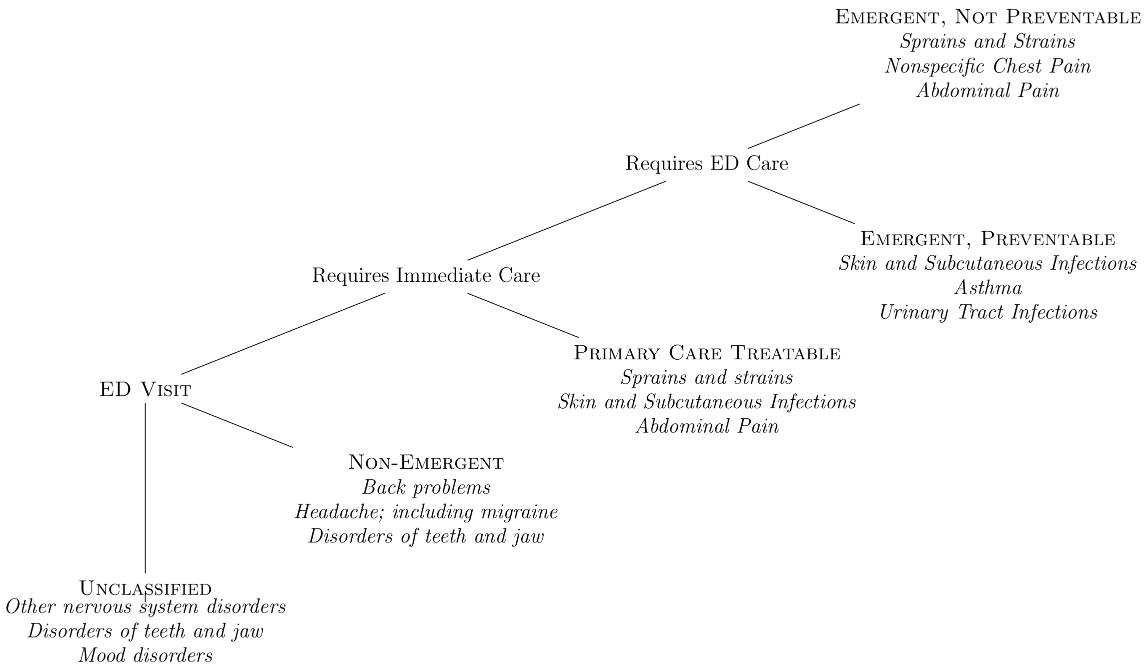


Figure S2: Map of Included Zip Codes



Notes: The map shows the city of Portland, Oregon and surrounding areas; Portland city boundaries (as of 2007) are outlined in black. Individuals residing in the shaded zip codes at the time of lottery sign-up are included in the emergency department analysis. The map was created using shapefiles from the Oregon Spatial Data Library. The map omits fifteen zip codes which are included in the analysis; thirteen within the Portland boundaries are too small geographically to display and two close to Portland that are not in the shapefile data.

Figure S3: Classification System for Type of Visit



Notes: The figure shows the classification into types of visits using the algorithm developed by Billings et al *(20)*. Emergency department visits are assigned, using primary diagnosis codes, probabilities of being "non-emergent," "primary care treatable," "emergent, preventable," and "emergent, not preventable." The conditions listed in *italics* below each category are the three most common conditions in each category. This is calculated by summing the probabilities assigned to each category, across all visits, within each condition. Thus, common conditions like "sprains and strains" and "skin and subcutaneous infections" contribute heavily to multiple categories.

Table S1: Differences in Lottery List Characteristics Across Samples

	Full Sample (1)	ED Sample (2)
Year of Birth	1968.0 (12.3)	1968.3 (12.1)
Female (%)	55.7	55.4
English as preferred language (%)	92.2	87.5
Signed up self for lottery (%)	91.8	92.9
Signed up first day of lottery (%)	9.3	9.1
Gave phone number (%)	86.2	86.6
Address a PO Box (%)	11.7	2.7
Zip code in metropolitian statistical area (%)	77.3	100.0
Zip code median household income (%)	39265 (8464)	43027 (9406)
N	74922	24646

Notes: Table shows the control means (with the standard deviations for continuous variables in parentheses) of the lottery list variables for the samples indicated in each column. The first column is for the full Oregon Health Insurance Experiment analytic sample; the second column is for the individuals in Portland-area zip codes (the emergency department or ED analysis sample).

Sample consists of individuals in specified group (N in table).

Table S2: Emergency Department Use

	N	First Stage	Percent with any visits				Number of visits			
			Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Overall										
All Visits	24646	24.6	34.5	1.7	7.0	0.003	1.022	0.101	0.408	<0.001
				(0.6)	(2.4)		(2.632)	(0.029)	(0.116)	

Notes: Column 1 reports the sample size for each analysis. Column 2 reports the coefficient (with standard error in parentheses) on LOTTERY from estimating the first-stage equation (2) in the specified sample. Columns 3 and 7 report the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses). Columns 4 and 8 report the estimated effect of lottery selection, specifically the coefficient (with standard error in parentheses) on LOTTERY from estimating equation (1) by OLS. Columns 5 and 9 report the estimated effect of Medicaid coverage, specifically the coefficient (with standard error in parentheses) on MEDICAID from estimating equation (3) by IV, as also reported in Table 2. Columns 6 and 10 report the p-value of the estimated effects. All regressions include indicators for the number of household members on the list and adjust standard errors for household clusters. The regressions for Panel A include controls for the pre-randomization version on the variable; the regressions for Panel B stratify on pre-randomization use (as indicated by the rows) and therefore do not include controls for pre-randomization use. The number-of-visits measures are unconditional, including those with no visits.

Sample consists of individuals in specified group (N in table).

Table S2, Continued

	N	First Stage	Percent with any visits: Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value	Number of visits: Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel B: By emergency department use in the pre-randomization period										
No visits	16930	23.5	22.5	1.6	6.7	0.019	0.418	0.061	0.261	0.002
				(0.7)	(2.9)		(1.103)	(0.020)	(0.084)	
One visit	3881	27.4	47.2	2.5	9.2	0.127	1.115	0.179	0.652	0.010
				(1.7)	(6.0)		(1.898)	(0.070)	(0.254)	
Two+ visits	3835	26.8	72.2	1.9	7.1	0.206	3.484	0.102	0.380	0.557
				(1.5)	(5.6)		(5.171)	(0.174)	(0.648)	
Five+ visits	957	25.4	89.4	0.2	0.7	0.932	6.948	0.619	2.486	0.232
				(2.1)	(8.3)		(7.635)	(0.525)	(2.079)	
Two+ outpatient visits	3402	25.9	73.2	2.5	9.6	0.111	3.658	0.144	0.560	0.450
				(1.6)	(6.0)		(5.375)	(0.193)	(0.742)	

Table S3: Emergency Department Use by Hospital Admission and Timing

	Percent with any visits				Number of visits			
	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	8
By hospital admission:								
Inpatient visits	7.5	-0.3	-1.2	0.385	0.126	-0.006	-0.023	0.396
		(0.3)	(1.3)		(0.602)	(0.007)	(0.028)	
Outpatient visits	32.0	2.0	8.2	<0.001	0.897	0.105	0.425	<0.001
		(0.6)	(2.4)		(2.362)	(0.026)	(0.107)	
By timing of visit:								
On-hours visits	25.7	1.4	5.7	0.010	0.574	0.057	0.232	0.001
		(0.5)	(2.2)		(1.555)	(0.018)	(0.072)	
Off-hours visits	21.9	1.5	6.1	0.005	0.456	0.051	0.208	0.002
		(0.5)	(2.2)		(1.394)	(0.017)	(0.068)	

Notes: Columns 1 and 5 report the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses). Columns 2 and 6 report the estimated intent-to-treat effect of lottery selection, specifically the coefficient (with standard error in parentheses) on LOTTERY from estimating equation (1) by OLS. Columns 3 and 7 report the estimated local-average-treatment effect of Medicaid coverage, specifically the coefficient (with standard error in parentheses) on MEDICAID from estimating equation (3) by IV, as also reported in Table 3. Columns 4 and 8 report the p-value of the estimated effects. The coefficient on LOTTERY from estimating the first-stage equation (2) is 24.7 percentage points. All regressions include indicators for the number of household members on the lottery list, control for the pre-randomization version of the outcome, and adjust standard errors for household clusters. The number-of-visits measures are unconditional, including those with no visits.

Sample consists of individuals in Portland-area zip codes (N=24,646).

Table S4: Emergency Department Use by Type of Visit

	Number of visits			
	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)
Requires Immediate Care				
Emergent, Not Preventable	0.213	0.012	0.049	0.138
(Requires ED care, could not have been prevented)	(0.685)	(0.008)	(0.033)	
Emergent, Preventable	0.074	0.009	0.038	0.032
(Requires ED care, could have been prevented)	(0.342)	(0.004)	(0.018)	
Primary Care Treatable	0.343	0.044	0.180	<0.001
(Does not require ED care)	(0.948)	(0.011)	(0.046)	
Does Not Require Immediate Care				
Non-emergent	0.201	0.029	0.118	0.001
	(0.688)	(0.009)	(0.035)	
Unclassified	0.196	0.015	0.059	0.107
	(0.734)	(0.009)	(0.037)	

Notes: Column 1 reports the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses). Column 2 reports the estimated intent-to-treat effect of lottery selection, specifically the coefficient (with standard error in parentheses) on LOTTERY from estimating equation (1) by OLS. Column 3 reports the estimated local-average-treatment effect of Medicaid coverage, specifically the coefficient (with standard error in parentheses) on MEDICAID from estimating equation (3) by IV, as also reported in Table 4. Column 4 reports the p-value of the estimated effects. The coefficient on LOTTERY from estimating the first-stage equation (2) is 24.7 percentage points. All regressions include indicators for the number of household members on the lottery list, control for the pre-randomization version of the outcome, and adjust standard errors for household clusters We use the Billings et al *(21)* algorithm to assign probabilities of a visit being each type, and therefore analyze only the number of visits (not the percent with any visits) as obtained by summing the probabilities across all visits for an individual. The number-of-visits measures are unconditional, including those with no visits. We use the abbreviation ED for emergency department.

Sample consists of individuals in Portland-area zip codes (N=24,646).

Table S5: Comparing Adminstrative Data and Survey Data Results

			Percent with any visits				Number of visits			
	N	First Stage	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Baseline Estimates										
Mail survey	23741	29.0	26.1	0.7	2.2	0.335	0.470	0.0074	0.026	0.645
6 months before response				(0.7)	(2.3)		(1.037)	(0.016)	(0.056)	
In-person interview	12229	24.1	40.2	1.3	5.4	0.189	0.997	0.023	0.094	0.572
12 months before interview				(1)	(4.1)		(1.999)	(0.04)	(0.166)	
Emergency department data	24646	24.6	34.5	1.7	7.0	0.003	1.022	0.101	0.408	<0.001
18-month study period				(0.6)	(2.4)		(2.632)	(0.029)	(0.116)	

Notes: Column 1 reports the sample size for each analysis. Column 2 reports the coefficient (with standard error in parentheses) on LOTTERY from estimating the first-stage equation (2) in the specified sample. Columns 3 and 7 report the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses). Columns 4 and 8 report the estimated effect of lottery selection, specifically the coefficient (with standard error in parentheses) on LOTTERY from estimating equation (1) by OLS. Columns 5 and 9 report the estimated effect of Medicaid coverage, specifically the coefficient (with standard error in parentheses) on MEDICAID from estimating equation (3) by IV, as also reported in Table 5. Columns 6 and 10 report the p-value of the estimated effects. All regressions include indicators for the number of household members on the list and adjust standard errors for household clusters. The first set of estimates in Panel A and all the estimates in Panel B are weighted using mail survey weights and include indicators for survey wave and interactions between survey-wave indicators and number-of-household-member indicators. The second set of estimates in Panel A and all the estimates in Panel C are weighted using in-person weights. In Panel A, we report the estimates from Table V in Finkelstein et al*(11)*, from Table 5 in Baicker et al*(12)*and from Table 2. Table 5 in Baicker et al (12) reports only the number-of-visit measure; here we also present the percent-with-any-visits measure analyzed using the same methodology. In Panels B and C, we report estimates for the individuals in the emergency department analysis sample who were respondents to the mail survey (Panel B) or the in-person interviews (Panel C). We report estimates using both self-reported responses and responses to the same question coded in the administrative data.

Sample consists of individuals in specified group (N in table).

Table S5, Continued

	N	First Stage	Percent with any visits: Mean Value in Control Group	Percent with any visits: Effect of Lottery Selection	Percent with any visits: Effect of Medicaid Coverage	Percent with any visits: p-value	Number of visits: Mean Value in Control Group	Number of visits: Effect of Lottery Selection	Number of visits: Effect of Medicaid Coverage	Number of visits: p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel B: Limited to overlap sample between mail survey and emergency department data										
Self-report of use	7239	28.8	25.6	-0.004	-0.01	0.997	0.482	-0.013	-0.046	0.666
6 months before response				(1.2)	(4.2)		(1.090)	(0.031)	(0.107)	
Administrative record of use	7239	28.8	16.2	1.3	4.6	0.197	0.296	0.015	0.052	0.538
6 months before response				(1.0)	(3.6)		(0.933)	(0.024)	(0.085)	
Panel C: Limited to overlap sample between in-person and emergency department data										
Self-report of use	10178	24.0	40.2	1.4	6.0	0.179	0.980	0.036	0.150	0.396
12 months before interview				(1.1)	(4.5)		(1.959)	(0.043)	(0.177)	
Administrative record of use	10178	24.0	26.8	1.6	6.8	0.089	0.635	0.084	0.351	0.037
12 months before interview				(1.0)	(4.0)		(1.828)	(0.041)	(0.168)	

Table S6: Treatment-Control Balance (Pre-randomization variables)

	Control mean (1)	Treatment-control difference (2)
Any visits	32.0	0.4 (0.6)
Number of visits	0.815	0.002 (0.027)
Any inpatient visits	6.6	-0.2 (0.3)
Number of inpatient visits	0.096	-0.007 (0.006)
Any outpatient visits	29.7	0.6 (0.6)
Number of outpatient visits	0.721	0.005 (0.025)
Any on-hours visits	23.5	-0.1 (0.6)
Number of on-hours visits	0.480	-0.009 (0.017)
Any off-hours visits	19.3	0.9 (0.5)
Number of off-hours visits	0.339	0.003 (0.013)
Number of emergent, not preventable visits	0.164	0.004 (0.007)
Number of emergent, preventable visits	0.064	-0.002 (0.004)
Number of primary care treatable visits	0.274	0.010 (0.010)
Number of non-emergent visits	0.168	-0.004 (0.008)
Number of unclassified visits	0.147	-0.007 (0.007)

Notes: Column 1 reports the control mean of the variable (with standard deviation for continuous outcomes in parentheses). Column 2 reports estimated differences between treatments and controls for the dependent variable (shown in the left hand column), specifically the coefficient (with standard error in parentheses) on LOTTERY based on estimating equation (1). All regressions include indicators for the number of household members on the list and adjust standard errors for household clusters. The any-visits measures are presented as percentages with the estimated difference in percentage points. The table reports this analysis of treatment-control balance for the pre-randomization versions of the outcome variables; Table 1 reports the same analysis for a set of characteristics reported on the lottery list.

Sample consists of individuals in Portland-area zip codes (N=24,646).

Table S6, Continued

Any ambulatory care sensitive visits	3.9	-0.1 (0.3)
Number of ambulatory care sensitive visits	0.055	-0.00239 (0.004)
Any chronic conditions visits	8.7	-0.5 (0.4)
Number of chronic conditions visits	0.159	-0.019 (0.009)
Any visits for injury	12.3	0.3 (0.4)
Number of visits for injury	0.174	0.016 (0.008)
Any visits for skin conditions	3.1	0.1 (0.2)
Number of visits for skin conditions	0.050	0.000 (0.004)
Any visits for abdominal pain	2.8	0.0 (0.2)
Number of visits for abdominal pain	0.041	-0.001 (0.004)
Any visits for back conditions	2.3	0.2 (0.2)
Number of visits for back conditions	0.033	0.006 (0.004)
Any visits for chest pain/heart problems	2.0	-0.01 (0.18)
Number of visits for chest pain/heart problems	0.026	0.000 (0.003)
Any visits for headache	1.8	-0.1 (0.2)
Number of visits for headache	0.033	-0.004 (0.006)
Any visits for mood disorders	1.7	-0.04 (0.2)
Number of visits for mood disorder	0.027	-0.002 (0.003)
Any visits for substance abuse/mental health	3.7	-0.2 (0.2)
Number of visits for substance abuse/mental health	0.068	-0.006 (0.006)

Table S7: Insurance Coverage (First Stage Estimates)

	Control mean (1)	Estimated FS (2)
On Medicaid at any point in the study period	15.1	24.7 (0.6)
On OHP Standard at any point in the study period	2.4	25.2 (0.5)
# of Months on Medicaid in the study period	1.7	3.2 (0.1)
On Medicaid at the end of study period	11.1	14.3 (0.5)

Notes: Column 1 reports the control mean for alternate definitions of "MEDICAID." Column 2 reports the coefficient (with standard error in parentheses) on LOTTERY from estimating the first-stage equation (2) using the specified definition of "MEDICAID." All regressions include indicators for the number of household members on the lottery list and adjust standard errors for household clusters. The study period starts on March 10, 2008 and ends on September 30, 2009. In all our analyses of the local-average-treatment effect of Medicaid in the paper, we use the definition in the first row: "On Medicaid at any point in the study period."

Sample consists of individuals in Portland-area zip codes (N=24,646).

Table S8: Summary of Analytic Variables (control sample only)

	Percent with any visits	Conditional on having any visits: Mean	SD	Median	75th %tile	95th %tile	Truncation cutpoint	Number of truncations
All visits	34.5	2.97	3.79	2	3	9	44	10
By hospital admission:								
Inpatient visits	7.5	1.67	1.49	1	2	4	18	2
Outpatient visits	32.0	2.80	3.48	2	3	9	40	10
By timing of visit:								
On-hours visit	25.7	2.24	2.39	1	3	6	28	8
Off-hours visit	21.9	2.09	2.34	1	2	6	28	5
By type of visit:								
Emergent, Not Preventable		0.89	1.17	0.62	1.00	2.50	14	7
Emergent, Preventable		0.66	0.81	0.34	0.84	1.98	9	2
Primary Care Treatable		1.18	1.45	0.72	1.34	3.72	15	10
Non-emergent		1.23	1.28	0.80	1.46	3.47	13	4
Unclassified		1.69	1.45	1.00	2.00	5.00	16	3
For matching to self-reports:								
6 months before survey response	16.3	1.82	1.62	1.00	2.00	5.00	22	3
12 months before in-person interview	26.8	2.44	3.01	1.00	3.00	7.00	34	5

Notes: Table details the distribution the number of emergency department visits of different types. The mean, standard deviation, median, 75th and 95th percentiles reflect non-zero observations only. We use the Billings et al *(21)* algorithm to assign probabilities of a visit being each type, and therefore analyze only the number of visits (not the percent with any visits) as obtained by summing the probabilities across all visits for an individual. For those variables, "percent with any visits" is not defined; the summary statistics reflect non-zero observations as with all other variables. We truncate our analysis variables at 2*99th percentile of the distribution, conditional on being non-zero. We report the variable-specific cut-point for truncation and the number of truncated observations.

Sample consists of control group individuals in Portland-area zip codes (N=15,020).

Table S9: Comparison of Emergency Department 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	376972	100	270918	100	102514	100	16016	100
By gender:										
Male	271822	46.0	171469	45.5	114979	42.4	55104	53.8	7199	45.0
Female	318838	54.0	205493	54.5	155931	57.6	47408	46.3	8817	55.0
By age:										
19-49	281034	47.6	281034	74.6	190727	70.4	87608	85.5	12110	75.6
50-64	95938	16.2	95938	25.5	80191	29.6	14906	14.5	3892	24.3
By hospital admission:										
Inpatient Visit	87450	14.8	45075	12.0	35704	13.2	9321	9.1	1932	12.1
Outpatient Visit	503229	85.2	331897	88.0	235214	86.8	93193	90.9	14084	87.9
By timing of visit:										
On-hours	307965	52.1	198742	52.7	142081	52.4	54869	53.5	8935	55.8
Off-hours	282714	47.9	178230	47.3	128837	47.6	47645	46.5	7081	44.2
By type of visit:										
Emergent, Not Preventable	135806	23.0	87813	23.3	65040	24.0	21888	21.4	3372	21.0
Emergent, Preventable	40126	6.8	23114	6.1	15333	5.7	7576	7.4	1150	7.2
Primary Care Treatable	192177	32.5	125169	33.2	88931	32.8	34962	34.1	5392	33.7
Non-emergent	104605	17.7	69952	18.6	48144	17.8	21117	20.6	3087	19.3
Unclassified	117965	20.0	70924	18.8	53470	19.7	16971	16.5	3016	18.8

Notes: The unit of analysis for this table is emergency department visits rather than individuals. All analyses are based on the emergency department data for the 12 Portland area hospitals from March 10, 2008 through September 30, 2009. Columns 9 and 10 are visits by individuals in our control sample; the other columns include all visits to the 12 hospitals by individuals with Portland area zip codes. Emergency department visits with missing primary payer information were counted neither as insured or uninsured (this represents 0.6% of the full sample).

Sample consists of emergency department visits for specified group (N in table).

Table S10: Select Conditions (control sample only)

	N	Percent of Category	Percent of all Control Visits
	(1)	(2)	(3)
Panel A			
Ambulatory Care Sensitive Condition	1038	100.0	6.5
Urinary tract infections	283	27.3	1.8
Asthma	223	21.5	1.4
Diabetes Mellitus w. complications	135	13.0	0.8
Pneumonia (except caused by TB or STD)	130	12.5	0.8
Chronic obstructive pulmonary disease	100	9.6	0.6
Essential hypertension	54	5.2	0.3
Fluid and electrolite disorders	43	4.1	0.3
Congestive heart failure; non hypertensive	38	3.7	0.2
Chronic Condition	3149	100.0	19.7
Mood disorders	491	15.6	3.1
Alcohol-related disorders	291	9.2	1.8
Asthma	240	7.6	1.5
Anxiety disorders	236	7.5	1.5
Headache; including migraine	204	6.5	1.3
Other nervous system disorders	186	5.9	1.2
Schizophrenia/other psychotic disorders	149	4.7	0.9
Diabetes mellitus with complications	135	4.3	0.8
Substance-related disorders	128	4.1	0.8
Chronic obstructive pulmonary disrease	111	3.5	0.7
Epilepsy; convulsions	88	2.8	0.6
Screening and history of mental health	59	1.9	0.4
Essential hypertension	54	1.7	0.3
Menstrual disorders	48	1.5	0.3
Diverticulosis and diverticulitis	45	1.4	0.3
Esophageal disorders	41	1.3	0.3
Congestive heart failure; nonhypertensive	39	1.2	0.2
Spondyloiosis; invertebral disc disorder	38	1.2	0.2
Cardiac arythmia	34	1.1	0.2
Other	532	16.9	3.3

Notes: The unit of analysis is emergency department visits rather than individuals. All analyses are based on the emergency department visits by individuals in the control group from March 10, 2008 through September 30, 2009. Conditions in Panel A (ambulatory care sensitive conditions and chronic conditions) can overlap with each other and with conditions in Panel B. All conditions in Panel B are mutually exclusive, with the exception of "substance abuse and mental health" which includes “mood disorders” (also classified separately).

Sample consists of control group emergency department visits (N=17,498 visits).

Table S10, Continued

Panel B			
Injury	3503	100.0	21.9
Sprains and strains	1390	39.7	8.7
Superficial Injury; contusion	575	16.4	3.6
Open wounds of extremities	300	8.6	1.9
Other Injuries due to external causes	179	5.1	1.1
Open wound (head, neck, trunk)	159	4.5	1.0
Fracture of Upper Limb	154	4.4	1.0
Fracture of Lower Limb	104	3.0	0.7
Intracranial Injury	96	2.7	0.6
Poisoning by other Medications/Drugs	62	1.8	0.4
Substance-related disorders	62	1.8	0.4
Joint disorders and dislocations; trauma related	57	1.6	0.4
Burns	54	1.5	0.3
Poisoning by psychotropic agents	53	1.5	0.3
Complications of surgical procedures or medical care	52	1.5	0.3
Other fractures	47	1.3	0.3
Complication of device; implant or graft	36	1.0	0.2
Skull and face fractures	36	1.0	0.2
Other	87	2.5	#############
Skin conditions (skin and subcutaneous tissue infections)	884	100.0	5.5
Abdominal Pain	782	100.0	4.9
Back Conditions (spondylosis, other back problems)	683	100.0	4.3
Chest Pain and Heart Problems	591	100.0	3.7
Nonspecific Chest Pain	550	93.1	3.4
Acute myocardial infarction	21	3.6	0.1
Coronary atherosclerosis and other heart disease	20	3.4	0.1
Headache (headache, including migraine)	501	100.0	3.1
Mood disorders	491	100.0	3.1
Substance Abuse and mental health issues	1346	100.0	8.4
Mood disorders	491	36.5	3.1
Alcohol-related disorders	291	21.6	1.8
Anxiety Disorders	236	17.5	1.5
Schizophrenia and other psychotic disorders	149	11.1	0.9
Substance-related disorders	132	9.8	0.8
Adjustment disorders	15	1.1	0.1
Suicide/intentional self-inflicted injury	15	1.1	0.1
Other	17	1.3	0.1

Table S11: Emergency Department Use for Selected Conditions

	Percent with any visits				Number of visits			
	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
Ambulatory-care-sensitive condition	4.6	0.5 (0.3)	2.1 (1.1)	0.060	0.067 (0.396)	0.009 (0.005)	0.036 (0.021)	0.0883
Chronic condition	10.1	0.8 (0.4)	3.2 (1.5)	0.035	0.203 (0.896)	0.022 (0.011)	0.090 (0.045)	0.044
Panel B								
Injury	14.5	1.2 (0.5)	4.9 (1.9)	0.008	0.324 (0.988)	0.035 (0.013)	0.144 (0.051)	0.005
Skin conditions	3.7	0.04 (0.2)	0.2 (1.0)	0.876	0.057 (0.372)	0.005 (0.005)	0.021 (0.020)	0.292
Abdominal pain	3.4	-0.1 (0.2)	-0.3 (0.9)	0.753	0.052 (0.385)	0.001 (0.005)	0.003 (0.020)	0.872
Back conditions	3.0	0.07 (0.2)	0.3 (0.9)	0.738	0.045 (0.333)	0.002 (0.004)	0.007 (0.017)	0.697
Chest pain and heart problems	2.6	0.06 (0.2)	0.2 (0.9)	0.781	0.034 (0.254)	0.003 (0.003)	0.012 (0.013)	0.365
Headache	1.9	0.5 (0.2)	1.9 (0.7)	0.013	0.033 (0.407)	0.009 (0.005)	0.037 (0.019)	0.056
Mood disorders	1.7	0.1 (0.2)	0.3 (0.7)	0.655	0.033 (0.338)	-0.004 (0.003)	-0.017 (0.013)	0.213
Substance abuse and mental health	4.0	0.2 (0.2)	0.8 (1.0)	0.412	0.087 (0.634)	0.006 (0.008)	0.023 (0.032)	0.483

Notes: Columns 1 and 5 report the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses). Columns 2 and 6 report the estimated intent-to-treat effect of lottery selection, specifically the coefficient (with standard error in parentheses) on LOTTERY from estimating equation (1) by OLS. Columns 3 and 7 report the estimated local-average-treatment effect of Medicaid coverage, specifically the coefficient (with standard error in parentheses) on MEDICAID from estimating equation (3) by IV. Columns 4 and 8 report the p-value of the estimated effects. The coefficient on LOTTERY from estimating the first-stage equation (2) is 24.7 percentage points. All regressions include indicators for the number of household members on the lottery list, control for the pre-randomization version of the outcome, and adjust standard errors for household clusters. The number-of-visits measures are unconditional, including those with no visits.

Sample consists of individuals in Portland-area zip codes (N=24,646).

Table S12: List Charges

	Total charges for all visits			
	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)
ED Facility Charges	1445	72	294	0.128
	(4215)	(48)	(193)	
Total Charges	3639	197	798	0.299
	(14886)	(190)	(769)	

Notes: Column 1 reports the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses). Column 2 reports the estimated intent-to-treat effect of lottery selection, specifically the coefficient (with standard error in parentheses) on LOTTERY from estimating equation (1) by OLS. Column 3 reports the estimated local-average-treatment effect of Medicaid coverage, specifically the coefficient (with standard error in parentheses) on MEDICAID from estimating equation (3) by IV. Column 4 reports the p-value of the estimated effects. The coefficient on LOTTERY from estimating the first-stage equation (2) is 24.7 percentage points. All regressions include indicators for the number of household members on the lottery list, control for the pre-randomization version of the outcome, and adjust standard errors for household clusters. We use the abbreviation ED for emergency department.

Sample consists of individuals in Portland-area zip codes (N=24,646).

Table S13: Emergency Department Use by Hospital Type

	Percent with any visits				Number of visits			
	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value	Mean Value in Control Group	Effect of Lottery Selection	Effect of Medicaid Coverage	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
By uninsured volume								
High uninsured volume	23.3	1.3	5.5	0.010	0.595	0.057	0.230	0.005
		(0.5)	(2.1)		(1.770)	(0.020)	(0.082)	
Low uninsured volume	18.4	1.0	4.0	0.042	0.436	0.051	0.207	0.004
		(0.5)	(2.0)		(1.499)	(0.018)	(0.072)	
Global test of sorting				0.56				0.26

Notes: Columns 1 and 5 report the control mean of the dependent variable (with standard deviation for continuous outcomes in parentheses). Columns 2 and 6 report the estimated intent-to-treat effect of lottery selection, specifically the coefficient (with standard error in parentheses) on LOTTERY from estimating equation (1) by OLS. Columns 3 and 7 report the estimated local-average-treatment effect of Medicaid coverage, specifically the coefficient (with standard error in parentheses) on MEDICAID from estimating equation (3) by IV. Columns 4 and 8 report the p-value of the estimated effects. The coefficient on LOTTERY from estimating the first-stage equation (2) is 24.7 percentage points. All regressions include indicators for the number of household members on the lottery list, control for the pre-randomization version of the outcome, and adjust standard errors for household clusters. The number-of-visits measures are unconditional, including those with no visits.

The global test for sorting on the percent with any visits is calculated by estimating the intent-to-treat estimates of lottery selection, specifically equation (1), for each of the 12 emergency departments using logistic regression, and then doing an F-test of the null that all the estimated effects are equal. The logistic regressions include indicators for the number of household members on the lottery list, control for an indicator of pre-randomization emergency department use overall (at any of the 12 emergency departments), and adjust standard errors for household clusters. The global test for sorting on the number of visits is calculated by estimating intent-to-treat effects of lottery selection, specifically equation (1), for each of the 12 emergency departments with a negative binomial model, and then doing an F-test of the null that all the estimated effects are equal. The negative binomial regressions include indicators for the number of household members on the lottery list, control for the number of pre-randomization emergency department visits overall (at any of the 12 emergency departments), and adjust standard errors for household clusters. The p-values reported in columns 4 and 8 for the global tests are for the F-tests.

Sample consists of individuals in Portland-area zip codes (N=24,646).

Table S14: Heterogeneity

	N	First Stage	Percent with any visits: Mean Value in Control Group	Percent with any visits: Effect of Medicaid Coverage	Percent with any visits: p-value	Number of visits: Mean Value in Control Group	Number of visits: Effect of Medicaid Coverage	Number of visits: p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full Sample	24646	24.7	34.5	7.0	0.004	1.022	0.408	<0.001
				(2.4)		(2.632)	(0.116)	
Gender								
Men	11172	26.4	33.4	12.3	<0.001	0.992	0.484	0.003
				(3.2)		(2.636)	(0.162)	
Women	13474	23.3	35.4	1.8	0.600	1.046	0.331	0.042
				(3.4)	[0.02]	(2.629)	(0.163)	[0.5]
Age								
Older (age 50-64)	6205	27.8	34.2	0.7	0.864	0.908	0.175	0.333
				(4.3)		(2.303)	(0.181)	
Younger (age 19-49)	18441	23.6	34.6	9.5	0.001	1.061	0.502	0.001
				(2.9)	[0.09]	(2.735)	(0.145)	[0.16]
Ever smoker (measured in in-person interviews)								
Ever smoker	4373	30.6	45.6	15.3	0.002	1.404	0.975	<0.001
				(5.0)		(2.952)	(0.263)	
Never smoker	5801	22.5	28.5	-1.5	0.782	0.664	0.265	0.191
				(5.6)	[0.02]	(1.855)	(0.203)	[0.03]

^This analysis was not pre-specified.

Notes: Column 1 reports the sample size for each analysis. Column 2 reports the coefficient (with standard error in parentheses) on LOTTERY from estimating the first-stage equation (2) in the specified sample. Columns 3 and 6 report the control means of the dependent variable (with standard deviation for continuous outcomes in parentheses). Columns 4 and 7 report the estimated local-average-treatment effect of Medicaid coverage, specifically the coefficient (with standard error in parentheses) on MEDICAID from estimating equation (3) by IV, for each subgroup. Columns 5 and 8 report the p-values for each subgroup estimate and [in brackets] the comparison between the differences in the subgroup estimates where the reference subgroup is the first under each heading. All regressions include indicators for the number of household members on the lottery list, control for the pre-randomization version of the outcome, and adjust standard errors for household clusters. The number-of-visits measures are unconditional, including those with no visits. Where noted, the subgroups are defined using information from other data sources described in more detail in Finkelstein et al*(11)*(credit report data) and Baicker et al*(12)*(in-person interviews). The "pre-lottery diagnosis" variable indicates an individual had a diagnosis prior to the lottery of at least one of the following: diabetes, hypertension, high cholesterol, heart attack, congestive heart failure, cancer, emphysema, asthma, failing kidneys, depression/anxiety.

Sample consists of individuals in specified group (N in table).

Table S14, continued

	N	First Stage	Percent with any visits: Mean Value in Control Group	Percent with any visits: Effect of Medicaid Coverage	Percent with any visits: p-value	Number of visits: Mean Value in Control Group	Number of visits: Effect of Medicaid Coverage	Number of visits: p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
^Pre-lottery Diagnoses (measured in in-person interviews)								
Pre-lottery Diagnosis	5734	26.3	41.8	9.1	0.079	1.228	0.928	<0.001
				(5.2)		(2.810)	(0.261)	
No Pre-lottery Dx	4444	25.5	27.8	6.1	0.246	0.649	0.261	0.137
				(5.3)	[0.69]	(1.692)	(0.176)	[0.03]
Race (measured in in-person interviews)								
White	6778	28.4	38.4	5.9	0.174	1.049	0.527	0.007
				(4.3)		(2.461)	(0.196)	
Non-white	3366	21.1	30.8	9.7	0.196	0.848	0.937	0.004
				(7.5)	[0.66]	(2.321)	(0.324)	[0.28]
Education (measured in in-person interviews)								
More than high school	3490	25.3	32.3	7.8	0.230	0.776	0.926	0.001
				(6.5)		(1.922)	(0.279)	
High school or less	6677	26.3	37.6	7.6	0.097	1.085	0.519	0.012
				(4.6)	[0.99]	(2.622)	(0.206)	[0.24]
Prior financial status (measured in credit report data)								
Had prior credit	9000	22.8	25.7	3.8	0.343	0.577	0.273	0.036
				(4.0)		(1.652)	(0.130)	
No prior credit	7243	27.2	44.5	3.5	0.410	1.457	0.447	0.065
				(4.2)	[0.96]	(3.262)	(0.242)	[0.53]
Signed up first day								
First Day	2288	35.7	38.2	8.0	0.142	1.190	0.405	0.142
				(5.5)		(2.868)	(0.276)	
Not first day	22358	23.5	34.1	6.7	0.010	1.005	0.408	0.001
				(2.6)	[0.83]	(2.607)	(0.127)	[0.99]

Table S15: Sensitivity of Results to Choice of Covariates (Effect of Lottery Selection)

	Percent with Any Visits			Number of Visits		
	Baseline results	Without pre-randomization versions of outcome variables	With lottery list variables	Baseline results	Without pre-randomization versions of outcome variables	With lottery list variables
	(1)	(2)	(3)	(4)	(5)	(6)
All visits	1.7	1.9	1.7	0.101	0.084	0.098
	(0.6)	(0.6)	(0.6)	(0.029)	(0.035)	(0.029)
	[0.004]	[0.003]	[0.004]	[<0.001]	[0.016]	[0.001]
Inpatient	-0.3	-0.3	-0.3	-0.006	-0.009	-0.006
	(0.3)	(0.3)	(0.3)	(0.007)	(0.007)	(0.007)
	[0.385]	[0.303]	[0.339]	[0.395]	[0.205]	[0.394]
Outpatient	2.0	2.2	2.0	0.105	0.095	0.102
	(0.6)	(0.6)	(0.6)	(0.026)	(0.032)	(0.027)
	[0.001]	[<0.001]	[0.001]	[<0.001]	[0.003]	[<0.001]
On-hours	1.4	1.4	1.3	0.057	0.047	0.056
	(0.5)	(0.6)	(0.5)	(0.018)	(0.021)	(0.018)
	[0.01]	[0.018]	[0.015]	[0.001]	[0.026]	[0.002]
Off-hours	1.5	1.8	1.5	0.0512	0.056	0.050
	(0.5)	(0.6)	(0.5)	(0.017)	(0.019)	(0.017)
	[0.005]	[0.002]	[0.006]	[0.002]	[0.004]	[0.004]

Notes: All columns report the estimated intent-to-treat effect of lottery selection, specifically the coefficient (with standard error in parentheses) on LOTTERY from estimating equation (1) by OLS and associated p-value [in brackets]. All regressions include indicators for the number of household members on the lottery list, and adjust standard errors for household clusters. Column 1 and 4 regressions control for the pre-randomization version of the outcome; these results are also reported in Tables S2-S4. Column 2 and 5 regressions do not control for pre-randomization version of the outcome. Column 3 and 6 regressions control for pre-randomization version of the outcome and the lottery list variables (full list is in Panel B of Table 1). The number-of-visits measures are unconditional, including those with no visits.

Sample consists of individuals in Portland-area zip codes (N=24,646).

Table S15, continued

	Percent with Any Visits			Number of Visits		
	Baseline results	Without pre-randomization versions of outcome variables	With lottery list variables	Baseline results	Without pre-randomization versions of outcome variables	With lottery list variables
	(1)	(2)	(3)	(4)	(5)	(6)
Emergent, Not Preventable	N/A	N/A	N/A	0.012	0.015	0.011
				(0.008)	(0.009)	(0.008)
				[0.139]	[0.113]	[0.184]
Emergent, Preventable	N/A	N/A	N/A	0.009	0.008	0.009
				(0.004)	(0.005)	(0.004)
				[0.033]	[0.104]	[0.033]
Primary Care Treatable	N/A	N/A	N/A	0.044	0.046	0.042
				(0.011)	(0.013)	(0.011)
				[<0.001]	[<0.001]	[<0.001]
Non-emergent	N/A	N/A	N/A	0.029	0.026	0.028
				(0.009)	(0.010)	(0.009)
				[0.001]	[0.007]	[0.001]
Unclassified	N/A	N/A	N/A	0.015	0.013	0.015
				(0.009)	(0.010)	(0.009)
				[0.108]	[0.193]	[0.099]

Table S16: Sensitivity of Results to Functional Form (Effect of Lottery Selection)

	Percent with Any Visits		Number of Visits	
	Linear Probability Model	Logistic Model	Linear Model	Negative Binomial Model
	(1)	(2)	(3)	(4)
All visits	1.716	2.000	0.101	0.086
	(0.592)	(0.670)	(0.029)	(0.021)
	[0.004]	[0.004]	[<0.001]	[<0.001]
Inpatient	-0.286	-0.290	-0.0058	-0.0032
	(0.329)	(0.310)	(0.007)	(0.005)
	[0.385]	[0.35]	[0.395]	[0.087]
Outpatient	2.027	2.300	0.105	0.087
	(0.585)	(0.650)	(0.026)	(0.019)
	[0.001]	[<0.001]	[<0.001]	[<0.001]
On-hours	1.400	1.500	0.057	0.044
	(0.547)	(0.580)	(0.018)	(0.014)
	[0.01]	[0.01]	[0.001]	[0.001]
Off-hours	1.490	1.600	0.051	0.041
	(0.532)	(0.550)	(0.017)	(0.012)
	[0.005]	[0.005]	[0.002]	[0.001]

Notes: All columns report the estimated intent-to-treat effect of lottery selection, specifically the coefficient (with standard error in parentheses) on LOTTERY from estimating equation (1) and associated p-value [in brackets]. Column 1 and 3 results are estimated by OLS; these results are also reported in Tables S2-S4. Column 2 results are estimated by logistic regression and are reported as average marginal effects. Column 4 results are estimated by negative binomial regression and are reported as average marginal effects. All regressions include indicators for the number of household members on the lottery list, control for the pre-randomization version of the outcome, and adjust standard errors for household clusters. The number-of-visits measures are unconditional, including those with no visits.

Sample consists of individuals in Portland-area zip codes (N=24,646).

Table S16, continued

	Percent with Any Visits		Number of Visits	
	Linear Probability Model	Logistic Model	Linear Model	Negative Binomial Model
	(1)	(2)	(3)	(4)
Emergent, Not Preventable	N/A	N/A	0.012	0.008
			(0.008)	(0.007)
			[0.139]	[0.234]
Emergent, Preventable	N/A	N/A	0.009	0.009
			(0.004)	(0.004)
			[0.033]	[0.015]
Primary Care Treatable	N/A	N/A	0.044	0.034
			(0.011)	(0.009)
			[<0.001]	[<0.001]
Non-emergent	N/A	N/A	0.029	0.026
			(0.009)	(0.007)
			[0.001]	[<0.001]
Unclassified	N/A	N/A	0.015	0.01
			(0.009)	(0.007)
			[0.108]	[0.131]

^Table S17: Observational Estimates of Effect of Insurance in Study Population

	Random assignment	Any Medicaid vs. No Medicaid	Any medicaid vs. No Medicaid (controls only)	Any Medicaid vs. No Medicaid (treatment only)
	(1)	(2)	(3)	(4)
Panel A: Sample Size and Percent Insured				
Sample Size	24646	24646	15020	9626
% Insured	25	25	15	27
Panel B: Percent with Any Visits				
All visits	7.0	13.6	15.4	10.2
	(2.4)	(0.7)	(1.1)	(1.1)
	[0.003]	[<0.001]	[<0.001]	[<0.001]
Inpatient	-1.2	4.1	6.5	1.2
	(1.3)	(0.4)	(0.7)	(0.6)
	[0.385]	[<0.001]	[<0.001]	[0.05]
Outpatient	8.2	13.4	14.5	10.8
	(2.4)	(0.7)	(1.1)	(1.1)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]
On-hours	5.7	12.1	14.2	8.3
	(2.2)	(0.7)	(1.0)	(1.0)
	[0.01]	[<0.001]	[<0.001]	[<0.001]
Off-hours	6.1	10.0	10.6	7.5
	(2.2)	(0.6)	(1.0)	(1.0)
	[0.005]	[<0.001]	[<0.001]	[<0.001]

^This analysis was not pre-specified.

Notes: Panel A reports the sample size for each analysis and the percent insured by the definition used for that analysis. Panel B reports coefficients, standard errors (in parentheses), and associated p-values [in brackets]. Column 1 reports our experiment's estimates of the local-average-treatment effect of Medicaid coverage, specifically the estimate for MEDICAID from estimating equation (3) by IV; these results are also reported in Tables 2-4. All other columns are based on observational comparisons of insurance coverage in our population, specifically OLS estimation of equation (1), but with the variable LOTTERY substituted with an indicator for "Medicaid" defined as any Medicaid coverage at any point in the study period. All regressions include indicators for the number of household members on the lottery list, and all standard errors are clustered on the household. Column 2 compares all those with any Medicaid coverage during our study period to those without Medicaid (regardless of lottery status); this represents the "as treated" analysis sometimes done in clinical trials. To avoid having much of the variation in insurance coming from the lottery, the third column performs the same analysis within the control group; here, most of the insurance coverage is through the non-lotteried Medicaid programs that cover a different population than OHP Standard. The fourth column therefore performs the analysis within the treatment group, where most of the insurance coverage comes through OHP Standard (the lotteried program).

Sample consists of individuals in specified group (N in table).

Table S17, continued

Panel C: Number of visits				
All visits	0.408	0.469	0.452	0.399
	(0.116)	(0.037)	(0.060)	(0.056)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Inpatient	-0.023	0.081	0.137	0.017
	(0.028)	(0.009)	(0.018)	(0.012)
	[0.396]	[<0.001]	[<0.001]	[0.147]
Outpatient	0.425	0.413	0.355	0.389
	(0.107)	(0.034)	(0.054)	(0.052)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]
On-hours	0.232	0.303	0.309	0.239
	(0.072)	(0.023)	(0.037)	(0.035)
	[0.001]	[<0.001]	[<0.001]	[<0.001]
Off-hours	0.208	0.219	0.226	0.184
	(0.068)	(0.022)	(0.036)	(0.034)
	[0.002]	[<0.001]	[<0.001]	[<0.001]
Emergent, Not Preventable	0.049	0.097	0.104	0.094
	(0.033)	(0.010)	(0.016)	(0.016)
	[0.138]	[<0.001]	[<0.001]	[<0.001]
Emergent, Preventable	0.038	0.040	0.050	0.021
	(0.018)	(0.005)	(0.009)	(0.008)
	[0.032]	[<0.001]	[<0.001]	[0.011]
Primary Care Treatable	0.180	0.174	0.149	0.161
	(0.046)	(0.014)	(0.023)	(0.022)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Non-emergent	0.118	0.118	0.123	0.096
	(0.035)	(0.011)	(0.018)	(0.017)
	[0.001]	[<0.001]	[<0.001]	[<0.001]
Unclassified	0.059	0.136	0.163	0.088
	(0.037)	(0.013)	(0.022)	(0.019)
	[0.107]	[<0.001]	[<0.001]	[<0.001]