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Medicaid Increases Emergency-Department Use: Evidence from Oregon's Health Insurance Experiment

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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 - epartment 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.

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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38. The twelve hospitals in our sample appear to have different practices for coding emergency department facility charges. For seven hospitals in the sample, these include all emergency department visit charges, so emergency department facility charges are equal to total charges for outpatient visits. For the remaining five hospitals, total charges are not equal to emergency department charges for outpatient visits; there can be additional charges for outpatient visits not included in the facility charge. Because of these different practices, our results for emergency department charges may be sensitive to changes in which hospitals are being visited. We therefore tested whether the proportion of emergency department visits occurring at the seven hospitals where emergency department charges capture all outpatient charges changes with insurance, which it did not. We also did a global test of whether Medicaid changes the distribution of visits across emergency departments and found no evidence that it does.
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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Supplementary Materials

www.sciencemag.org/cgi/content/full/science.1246183/DC1
Materials and Methods
Supplementary Text
Figs. S1 to S3
Tables S1 to S17

References (*28*–*40*)

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

*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.