Published in final edited form as:
Science. 2014 January 17; 343(6168): 263–268. doi:10.1126/science.1246183.

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

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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 by using a randomized controlled design. By using the randomization provided by the lottery and emergency-department records from Portland-area hospitals, we studied the emergency department use of about 25,000 lottery participants over about 18 months after the lottery. We found that Medicaid coverage significantly increases overall emergency use by 0.41 visits per person, or 40% relative to an average of 1.02 visits per person in the control group. We found 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 policy-makers often argue that expanding Medicaid will reduce inefficient and expensive use of the emergency department (1–4). However, expanded Medicaid coverage could 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. A quasi-experimental analysis of expanded Medicaid eligibility for

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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 about 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. By using Oregon's Medicaid lottery and administrative data from the emergency departments of hospitals in the Portland area, we examined the impact of Medicaid coverage on emergency-department use overall and for specific types of visits, conditions, and groups. The lottery allowed 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 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 to 64, Oregon residents, U.S. citizens or legal immigrants, without health insurance for 6 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 four in 2008) and have less than $2000 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 (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 2 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. Additionally, 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 returned to this disparity between estimates from self-reported and administrative data below.

Data

We obtained visit-level data for all emergency-department visits to 12 hospitals in the Portland area from 2007 through 2009. Individuals residing in Portland and neighboring suburbs almost exclusively use these 12 hospitals (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 on the basis of name, date of birth, and gender. We used 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 used this to construct our measures of Medicaid coverage. We also obtained prerandomization demographic information that people provided when they signed up for the lottery. We used these data (17), together with prerandomization 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 used 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 included 10 March 2008 (the first day that anyone was notified of being selected in the lottery) through 30 September 2009 [the end date used in our previous analysis of administrative and mail survey data (11)]. This 18-month observation period represented, on average, 15.6 months (standard deviation = 2.0 months) after individuals were notified of their selection in the lottery. Our pre-randomization period included 1 January 2007 (the earliest date in the data) through 9 March 2008 (just before the first notification of lottery selection).

Statistical Analysis

The analyses reported here were prespecified and publicly archived (18). Prespecification was done to minimize issues of data and specification mining and to provide a record of the full set of planned analyses.

We compared 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 used a standard instrumental-variable approach with lottery selection as an instrument for Medicaid coverage. This analysis used the lottery's random assignment to isolate the causal effect of

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Medicaid coverage (20). Specifically, it estimated 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 affects 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 nonrandom) take-up of Medicaid among those selected in the lottery reduced statistical power but did 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 to 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 analyzed 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) varied by the number of the individual's household members on the list. To account for this, all analyses controlled 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 were clustered according to household. Except where we stratified on prerandomization use of the emergency department, outcome analyses also controlled for the prerandomization 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 prerandomization versions of the outcomes or to additionally including demographic characteristics (measured before randomization) as covariates (table S15). We fit linear models for 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 (table S16).

Emergency-Department Analysis Sample

We restricted our analysis to individuals who, at the time of the lottery, lived in a five-digit postal code where residents almost exclusively used 1 of the 12 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 postal code restriction, our analysis sample included about one-third of the full Oregon Health Insurance Experiment study population. Table 1 shows the characteristics of the included sample. As expected, there was no difference in probability of inclusion in our analytic subsample between those selected in the lottery (treatments) and those not selected (controls) (−0.1 percentage points; SE = 0.4). There were 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 prerandomization 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 defined Medicaid coverage as being covered at any point during the study period (10 March 2008 to 30 September 2009) by any Medicaid program. This included both the lotteried Medicaid program (OHP Standard) and the other nonlotteried Medicaid programs. The nonlotteried 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 increased the probability of having Medicaid coverage at any point during our study period by 24.7 percentage points (SE = 0.6). The lottery affected coverage through increasing enrollment in the lotteried Medicaid program (table S7). Previous estimates from survey data suggested that there was no “crowd-out” of private insurance; the lottery did not affect self-reports of private insurance coverage (11, 12). For those who obtained Medicaid coverage through the lottery, there was 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 eight draws between March and October 2008, initial enrollment in Medicaid took 1 to 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, top, Medicaid increased emergency-department use. In the control group, 34.5% of individuals had an emergency-department visit during our 18-month study period. Medicaid increased the probability of having a visit by 7.0 percentage points (SE = 2.4; $P = 0.003$). Medicaid increased the number of emergency-department visits by 0.41 visits (SE = 0.12; $P < 0.001$), a 41% increase relative to the control mean of 1.02 visits.

Table 2, bottom, 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 before randomization. We also looked at those with two or more outpatient visits (visits that did not result in a hospital admission) before randomization. In all groups, Medicaid increased use (although results are not statistically significant in most of the smaller subsamples).

We also examined how the effects of Medicaid on emergency-department use differ in various other subgroups (see table S14 for estimates). Across the numerous subpopulations we considered, we did not find any in which Medicaid caused 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 separated visits by whether they resulted in a hospital admission and by what time of day they occurred (Table 3). About 90% of emergency-department visits in the control sample are outpatient visits. The increase in emergency-department use from Medicaid was solely in outpatient visits; we found no statistically significant effect of Medicaid on emergency-department visits that result in an inpatient admission to the hospital.

We next separated visits into those occurring during on hours (7 a.m. to 8 p.m. Monday through Friday) and those occurring during off hours (nights or weekends). Just over half of the visits in our control sample occurred during on hours. Both on- and off-hours use increased with Medicaid coverage.

We also classified visits by 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 required immediate care in the emergency department and that could not have been prevented were referred to as emergent, not preventable (21% of control sample visits). Visits that required immediate care in the emergency department but could have been prevented through timely ambulatory care were referred to as emergent, preventable (7%). Those visits that required immediate care but that could have been treated in an outpatient setting are referred to as primary care treatable (34%). Visits that did not require immediate care were classified as nonemergent (19%) (22). Medicaid statistically significantly increased visits in all classifications except for the emergent, nonpreventable category (Table 4). The increases were most pronounced in those classified as primary care treatable (0.18 visits; SE = 0.05; $P < 0.001$) and nonemergent (0.12 visits; SE = 0.04; $P = 0.001$). We also examined the impact of Medicaid on visits for a variety of different conditions (table S11), although even the most prevalent individual conditions represented a relatively small share of emergency-department visits (table S10). We did not find that Medicaid caused a statistically significant decrease in emergency-department use for any of the conditions we considered; indeed, once again the vast majority of point estimates are positive. We found 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). The top section 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 that 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 time frame of analysis is different; in particular, we were able to study outcomes over longer periods in the administrative data. Second, the study populations were different; in particular, the self-reported data were 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 time frame (because of incorrect recollections, for example, or mistakes about the site of care).

The rest of Table 5 attempts to disentangle these factors by limiting the analysis to the same set of individuals and capturing use over the same time frame. In the second section, for respondents to the mail survey who are also in the administrative data sample, we compared results from self-reported use in the surveys to results from the administrative data for the same 6-month period as the survey. We did the same in the third section for the in-person interviews: For respondents to the in-person interview who are also in the administrative data sample, we compared results from self-reported use to results from the administrative data for the same 12-month period as the interview. For the same individuals and time frames, our point estimates were larger and our standard errors were smaller in the administrative data compared with the self-reports.

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 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. 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), but uninsured patients can be charged for this legally required care. 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. 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, because 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 (table S17) that observational estimates that did not account for such confounding factors suggested much larger increases in emergency-department use associated with Medicaid coverage than the results from our randomized controlled setting.

By using the random assignment of the Oregon lottery, we could isolate the causal effect of Medicaid coverage on emergency-department use among low-income, uninsured adults. We found that Medicaid increases emergency-department use and estimated an average increase of 0.41 visits per covered person over an 18-month period, or about a 40% 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 examined the impact of Medicaid on types of visits, conditions, and populations in which we might expect the offsetting effects to be the strongest. In none of these did we detect a decline in emergency-department use. Emergency-department use increased 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 nonemergent 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 found 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, nonpreventable 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 with use of 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% of the federal poverty level under the Affordable Care Act. However, there are 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 examined changes in emergency-department use for people gaining an average of 13 months of coverage; longer-run effects may differ. Last, 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 was 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 found that expanding Medicaid coverage increased 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 that balances the costs and benefits of expanding Medicaid.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

Acknowledgments

We are grateful to A. Chandra, J. Levin, R. Levin, B. Olken, J. Shapiro, and H. Williams for helpful comments and advice; to Mira Bernstein for immeasurable contribution to the study; to I. Colaiacovo, N. Subramanian, A. Zhou, A. Barnett, and J. Dennett for expert research assistance; to M. Callan for invaluable expertise in collecting and processing the data; to the Oregon Association of Hospital and Health Systems and the hospitals who provided emergency-department data; to numerous Oregon state employees for help acquiring the necessary data and for answering our many questions about the administration of state programs; and to our generous funders. The Oregon Health Insurance Experiment study was funded by the Assistant Secretary for Planning and Evaluation in the U.S. Department of Health and Human Services, the California HealthCare Foundation, the John D. and Catherine T. MacArthur Foundation, the National Institute on Aging (grants P30AG012810, RC2AGO36631, and R01AG0345151), the Robert Wood Johnson Foundation, the Alfred P. Sloan Foundation, the Smith Richardson Foundation, the U.S. Social Security Administration (through grant 5 RRC 08098400-03-00 to NBER as part of the SSA Retirement Research Consortium), and the Centers for Medicare and Medicaid Services. The findings and conclusions expressed are solely those of the authors and do not represent the views of the funders. Replication code and a modified version of the data are available on the Oregon Health Insurance Experiment Web site (www.nber.org/oregon/data.html).

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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 the bottom section, the prerandomization versions of our outcome variables (table S6), and the combination. The top sample consists of individuals in the full Oregon Health Insurance Experiment (OHIE) sample (*N* = 74,922); the bottom sample consists of individuals in Portland-area postal codes (*N* = 24,646), also referred to as the emergency-department (ED) analysis sample. For variables that are percentages, the treatment-control differences are shown as percentage points.

	Control mean	Treatment-control difference
Percent of full OHIE sample included in ED analysis sample		
Included in ED analysis sample (%)	33.3	−0.1 (0.4)
Lottery list characteristics, conditional on being in ED analysis sample		
Year of birth	1968.3 (12.1)	0.1 (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 post office box (%)	2.6	0.1 (0.2)
Postal code median household income ($)	43,027 (9406)	182 (136)
F statistic for lottery list variables		1.498
P value		0.152
F statistic for prerandomization versions of the outcome variables		0.909
P value		0.622
F statistic for lottery list and prerandomization variables		1.013
P value		0.448

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Table 2

Emergency-department use

We report the estimated effect of Medicaid on emergency-department use over our study period (10 March 2008 to 30 September 2009) in the entire sample and in subpopulations based on prerandomization 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 postal codes (*N* = 24,646) or specified subpopulation (*N* in table). 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.

		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**
			Overall				
All visits	24,646	34.5	7.0 (2.4)	0.003	1.022 (2.632)	0.408 (0.116)	<0.001
			By emergency-department use in the prerandomization period				
No visits	16,930	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

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 were on hours if they occurred from 7 a.m. to 8 p.m. Monday through Friday and off hours otherwise. Sample consists of individuals in Portland-area postal codes (N = 24,646). 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.

	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 (1.3)	0.385	0.126 (0.602)	−0.023 (0.028)	0.396
Outpatient visits	32.0	8.2 (2.4)	<0.001	0.897 (2.362)	0.425 (0.107)	<0.001
By timing of visit						
On-hours visits	25.7	5.7 (2.2)	0.010	0.574 (1.555)	0.232 (0.072)	0.001
Off-hours visits	21.9	6.1 (2.2)	0.005	0.456 (1.394)	0.208 (0.068)	0.002

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 used the Billings *et al*. (21) algorithm to assign probabilities of a visit being each type and therefore analyzed only the number of visits (not the percent with any visits) as obtained by summing the probabilities across all visits for an individual. Sample consists of individuals in Portland-area postal codes (N = 24,646). The number-of-visits measures are unconditional, including those with no visits.

	Number of visits		
	Mean value in control group	**Effect of Medicaid coverage**	***P* value**
Required immediate care			
Emergent, not preventable (Required ED care, could not have been prevented)	0.213 (0.685)	0.049 (0.033)	0.138
Emergent, preventable (Required ED care, could have been prevented)	0.074 (0.342)	0.038 (0.018)	0.032
Primary care treatable (Did not require ED care)	0.343 (0.948)	0.180 (0.046)	<0.001
Did not require immediate care			
Nonemergent	0.201 (0.688)	0.118 (0.035)	0.001
Unclassified			
	0.196 (0.734)	0.059 (0.037)	0.107

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. At top, 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 by using the same methodology. In the next two sections, we limited the previously published analyses to individuals also in the emergency-department data and compared the self-reported answers to the survey questions to the answers to the same survey questions constructed from administrative data. 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.

		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**
Estimates in mail-survey, in-person, and emergency-department data							
Mail survey 6 months before response	23,741	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	12,229	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	24,646	34.5	7.0 (2.4)	0.003	1.022 (2.632)	0.408 (0.116)	<0.001
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
Limited to overlap sample between in-person and emergency-department data							
Self-report of use 12 months before interview	10,178	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	10,178	26.8	6.8 (4.0)	0.089	0.635 (1.828)	0.351 (0.168)	0.037