Empirical Research

The Effect of Medicaid Expansion on the Nature of New Enrollees' Emergency Department Use

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Abstract

We examine changes in emergency department (ED) visit acuity and care intensity for uninsured patients who gained Medicaid insurance in 2014 under the Patient Protection and Affordable Care Act. We use 2013-2015 longitudinal patient visit-level data from 30 EDs across 7 states from an emergency medicine group. We examine changes in ED use by previously uninsured Medicaid patients and patients remaining uninsured who were repeat ED users (≥1 visit before and after expansion) using a propensity-score weighted approach with statistical machine learning to estimate the weights. Compared with those remaining uninsured in nonexpansion states, newly covered Medicaid patients in expansion states showed a 29% relative increase in hospital admissions and 32% increase in admissions for nonambulatory care sensitive conditions with no increases in care intensity. Obtaining Medicaid insurance increased the relative proportion of ED visits requiring hospital admission suggesting increased outpatient access for low-acuity conditions previously addressed with ED care.

Keywords

Medicaid expansion, Affordable Care Act (ACA), emergency department, insurance access

Introduction

With nearly one in five of the country's nonelderly population having no health insurance in 2010, a major focus of the Patient Protection and Affordable Care Act of 2010 (ACA) was to expand health insurance coverage (Gruber, 2011). One of the ACA's key provisions expanded eligibility for Medicaid insurance, the public insurance program for low-income Americans. The original ACA required all states to expand Medicaid eligibility in 2014 to all able-bodied adults with ≤138% income relative to the federal poverty level ($16,245 for an individual in 2015 dollars) (Kaiser Family Foundation, 2012). Subsequently, a Supreme Court ruling left Medicaid expansion to the discretion of states (Kaiser Family Foundation, 2015a, 2015b). As of February 2019, 36 states and the District of Columbia have adopted Medicaid expansion, while 14 states have not, mostly concentrated in the Southeast and Central United States.

Considerable political divisions exist on the perceived value and impact of Medicaid expansion on health care utilization and there are active ongoing policy changes. Medicaid expansion was on the ballot in a number of states in the 2018 midterm elections, where voters in Idaho, Nebraska, and Utah approved ballot referendums for expansion, while voters in Montana rejected a proposal to make Medicaid expansion permanent (Galewitz, 2018). Maine's newly elected governor implemented Medicaid expansion in January 2019, which was overwhelmingly approved by the state's voters through a ballot initiative in 2017 (Meyer, 2019). Finally, a recent federal court ruling in Texas struck down as invalid all provisions of the ACA, including Medicaid expansion (Goodnough & Pear, 2018). These legislative and judicial efforts and the number of people affected by the legislation highlight the importance of empirical findings on the impacts of Medicaid expansion, particularly in settings like emergency departments (ED), which are perceived to be high cost.

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One concern regarding Medicaid expansion is its potential to increase ED use among the newly insured, as had occurred in two prior state-level expansions (Taubmam et al., 2014; Smulowitz, O'Malley, Yang, & Landon, 2014; Nikpay et al., 2017). Similarly, in Anderson, Dobkin, and Gross (2014), the authors study young adults who lose their health insurance after turning 23, and found the transition led to a decrease in ED visits. These results were hypothesized to occur due to the lower cost to the patient of always accessible ED care with insurance relative to being uninsured. Moreover, barriers to outpatient care often remain for those with Medicaid insurance as providers may not accept Medicaid patients, making the ED an easier or the only option for care (Centers for Disease Control and Prevention, 2015; Decker, 2011). On the other hand, these outpatient barriers are certainly lower for Medicaid beneficiaries than for the uninsured, as outpatient providers are not required to provide care to the uninsured and typically do not. In Antwi, Moriya, Simon, and Sommers (2015), the authors study young adults who remain as dependents on their parents' private health plans until age 26 years under the ACA's dependent coverage provision, and found a statistically significant yet modest decrease in ED use compared with a slightly older comparison group. This study suggests that some use of EDs could decrease, in particular ED use that was the result of insufficient access to outpatient care.

In a study of ED visits to over 500 hospitals, Pines et al. (2016) dispelled concerns that newly insured Medicaid patients would flood into EDs. Using data on early results from the 2014 natural experiment that some states did and did not expand Medicaid under the ACA, aggregate ED use was found not to have increased in Medicaid expansion states compared with nonexpansion states. Two factors were hypothesized to underlie this finding. First, in 2014, there was "payment parity" where primary care providers were paid Medicare rates, and new Medicaid patients may therefore have had better than usual access to outpatient care relative to earlier state-level expansions. Second, the newly insured covered under the 2014 Medicaid expansion may have been different than other Medicaid patients, specifically they had relatively higher incomes and thus may have patterns of health care use that rely less on EDs regardless of insurance status.

In a recent study by Xu et al. (2018), the authors study one of the states that expanded Medicaid, Maryland, and compare changes in ED utilization between matched uninsured and insured adult Maryland residents who visited an ED in the preexpansion period. Relative to those with any kind of health insurance at baseline, those who start out uninsured were found to increase their ED use, with most of the increase for high-acuity visits, meeting the rates for the insured. The finding that this increase was driven primarily by higher acuity visits and those leading to admissions is broadly consistent with the hypotheses proposed by Pines et al. (2016) that payment parity for Medicaid services might have limited increases in ED visits relative to earlier state-level Medicaid expansions. The Xu et al. (2018) study, however, assesses changes at the time of Medicaid expansion along with other ACA-related policies on all ED visiting uninsured, those who gained insurance (Medicaid/commercial) or remained uninsured in the post–insurance expansion period, and not just on those who gained Medicaid coverage under the ACA. Their comparison group also comprised patients insured at baseline in the same state—a very different demographic from those who were uninsured. Apart from focusing on just one state (which expanded Medicaid), the study does not assess the effect of gaining Medicaid insurance for those who gain it, relative to their remaining uninsured.

The Pines et al. (2016) study was limited in that it only examined aggregate visit volumes and did not assess changes in the nature of ED use among those individuals newly gaining Medicaid coverage. If greater access to outpatient care was the mechanism for the lack of observed aggregate increase in ED visits, we would expect the newly insured to use EDs for relatively more serious conditions, while relying on primary care providers for lower acuity health care needs. If the lack of aggregate increase in ED use under the ACA was due to those newly eligible for Medicaid having different health care use patterns that rely less on ED care, regardless of insurance coverage, we would expect to see similarly infrequent use of the ED for low-acuity care both before and after they gained Medicaid coverage.

New Contributions

In this study, we use longitudinal patient-/visit-level data to examine the acuity and intensity of ED visits of previously uninsured people in Medicaid expansions states who gained Medicaid insurance in 2014 compared with similar patients who remained uninsured in states that did not expand Medicaid coverage. The target population is a specific subgroup of patients: Those who visited the ED multiple times in the study period, at least once while uninsured before expansion and at least once after expansion and under Medicaid. We acknowledge that this is a selective group and also a group of considerable policy interest among those concerned about overuse of the ED (Althaus et al., 2011; LaCalle & Rabin, 2010). Our analysis is focused on evaluating changes in the nature of these patients' ED use (and not their number of ED visits). In our study, we were able to link individual patients' ED visits to the same facility over time enabling us to track changes in individual patient's behavior as they switch from uninsured to Medicaid or remain uninsured. The ability to track individual patients who newly gained Medicaid coverage under the expansion over time distinguishes this study by enabling us to utilize each patient's preexpansion uninsured ED use as an internal control for their postexpansion ED use, and to compare changes in use over time for a comparable uninsured population from nonexpansion states.

Study Data and Methods

The main data source for this study was patient visit–level data from a national emergency medicine group which staffs 101 hospital-based EDs across 16 U.S. states.[1] The data included unique patient identifiers allowing us to link visits for the same patient over time to the same ED. Thus, we were able to track visits by a patient to the same facility over time. We analyzed visits from April 1, 2013 to September 30, 2015 by patients aged 18 to 64 years to 30 facilities in 7 states—Illinois, Nevada, Ohio, Rhode Island, West Virginia, North Carolina, and Oklahoma.[2] We selected these seven states on the basis of continuity of longitudinal data availability (before and after January 1, 2014 when Medicaid expansion went into effect) and having similar preexpansion Medicaid eligibility income limits. We detail the criteria for selecting these states in Table A.1 in the appendix (Supplemental Material available online). We defined "treatment" facilities as those in the states which expanded Medicaid on January 1, 2014. In our sample, these were 19 EDs in Illinois, Nevada, Ohio, Rhode Island, and West Virginia. "Control" facilities were those in states that did not expand Medicaid any time before the end of 2015. These were 11 EDs in North Carolina and Oklahoma. The study was approved by the institutional review board at Carnegie Mellon University.

We studied patients who visited the ED facilities in the 7 selected states at least once in the "preexpansion period" (April 1, 2013 to December 31, 2013) and at least once in the "postexpansion period" (January 1, 2014 to September 30, 2015). This inclusion criterion enabled us observe changes in patients' insurance status over time. "Treatment" group patients were those in expansion states who visited the ED while uninsured in the preexpansion period and who visited the ED with Medicaid insurance in the postexpansion period. "Control" group patients were those in nonexpansion states who visited the ED while uninsured in both the preexpansion and the postexpansion periods.

For each visit, we used data on patient demographics—age, gender, and zip code; and on visit characteristics—diagnosis codes (ICD-9), relative value units (RVUs)—a marker of visit intensity, disposition, payments, charges, and primary insurance type. While we did not have access to specific patient income data (the key criteria for Medicaid eligibility), we were able to link zip-code-level household median income and percentage of uninsured among the age 18- to 64-year-old population, from the 2009 to 2013 5-Year American Community Survey (ACS) file,[3] with patient zip code of residence. To address external validity, we compared the EDs in our data with nationally representative data. For this purpose, we made use of Emergency Department summary tables from the National Hospital Ambulatory Medical Care Survey (NHAMCS)[4]—an annual nationally representative sample survey of visits to EDs.

Outcome Variables

Our unit of analysis was the patient-time period, where the time period was defined as being before or after Medicaid expansion. The outcome variables were proxies for high acuity and intensity of ED visits: proportion of a patient's visits in the postexpansion period which led to hospital admissions, proportion of a patient's postexpansion visits which led to admissions and were for nonambulatory care sensitive conditions, average probabilities of postexpansion visits being "emergent and unavoidable," and average RVUs per visit (where averages are over multiple visits for the same patient in the postexpansion year when a patient has more than one visit) (Proctor, 2012). We used ED visit ICD-9 diagnosis codes to assess if ED visits were for "ambulatory care sensitive conditions" (ACSCs). The Agency for Healthcare Research and Quality has defined a list of ACSCs, which are 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."[5,6] To evaluate the "emergent" nature of an ED visit, we used the New York University Emergency Department visit severity algorithm (Billings, Parikh, & Mijanovich, 2000). The algorithm uses the primary ICD-9 diagnosis code to assign each visit a probability of falling into one of four categories: nonemergent; emergent/primary care treatable; emergent (ED care needed) but preventable/avoidable; and emergent (ED care needed), not preventable/avoidable. Figure A.1 in the appendix provides a visual depiction of the classification process. RVUs are an administrative measure of visit intensity and complexity.

Statistical Analysis

We first examined trends in total monthly ED visits, by payer type, separately for treatment and control EDs in our analytic sample. This served to demonstrate the extent to which the "treatment" of expanding Medicaid was taken up by those using the ED and to compare the general pattern of ED utilization in this study to that in previously cited literature. We then compared summary statistics of all visits with the EDs in our analysis sample in the "pre" period with those of a nationally representative sample of patients with ED visits using the 2013 ED summary tables from the NHAMCS. We also compared visits in the "post" period with those of a sample from the 2014 ED summary tables from the NHAMCS to assess if national trends after expansion are in line with trends at the facilities in our sample. These comparisons address the external validity of our analytic sample. We then focus our primary analysis at the patient level.

Propensity-Score Weighting Using Boosted Regression Trees. We defined control patients as those uninsured in both the pre- and postexpansion periods who visited EDs in states that did not expand Medicaid. This group provides an estimate of what the time trend in ED usage would have been for those gaining Medicaid, in states that did expand Medicaid, if they had instead remained uninsured. As shown in Appendix Figure A.2, the demographics and medical diagnoses of the uninsured treatment and control patients in the preexpansion period were generally similar while there were some differences in the distributions of the acuity of ED visits.

To address this, we used a machine learning approach to reduce potential bias in the treatment effect estimates by weighting the control patients to obtain a close approximation to the joint distribution of covariates of our treatment patients (Haviland, Eisenberg, Mehrotra, Huckfeldt, & Sood, 2016). Because the goal of the procedure was to balance the full joint distribution, if successful, it removed reliance on the particular specification of covariates in the outcome model. The propensity score weights stood in for the set of potentially complex nonlinear interactions required to obtain balance in the joint distribution of covariates for the patients.

To construct the propensity score weights, we used the statistical machine learning methodology generalized boosted regression (implemented in a streamlined version of the R package TWANG). Using generalized boosted regression in this context can be preferable to the more commonly estimated logit model for two reasons. First, generalized boosted regression fits highly flexible models incorporating potentially complex interactions of the covariates, leading to weights that produce better balance on the full joint distribution rather than just the marginal for each variable individually (McCaffrey, Ridgeway, & Morral, 2004). Second, this method produces a distribution of weights that is less extreme and hence less able to cause variance inflation, a well-known problem with the logit (Lee, Lessler, & Stuart, 2011).

We balanced treatment and control groups on covariates most likely to determine their Medicaid eligibility (treatment) and type of ED use (outcomes): patient demographics (age, gender) and detailed characteristics of ED visits preexpansion (number of visits, average RVU of visits, proportion of visits which led to admissions/discharges/transfers, Multilevel Clinical Classification Software codes associated with the visits, proportion of emergent/nonemergent visits, proportion of visits which led to non-ACSC admissions, and proportion of visits which were unreimbursed).

Empirical Model

We sought to identify the average effect of enrollment in Medicaid after the ACA expansion went into effect on the nature of previously uninsured ED visiting patients' subsequent ED use. We used the following patient-level propensity-score weighted lagged dependent variable (LDV) outcome model to estimate the average effect of Medicaid expansion on the new enrollees. Specifically, we estimate the differential changes in ED usage behavior between treatment and control patients:

$$\begin{aligned} Y_{post,i} = \beta_0 + \beta_1 Expansion_i + \beta_2 Y_{pre,i} + \beta_3 Age_i + \\ \beta_4 Female_i + \beta_5 Inc_i + \beta_6 PctUn_i + \epsilon_i \end{aligned} \tag{1}$$

Here, $Y_{post,i}$ was the outcome variable of interest for patient i in the postexpansion period. $Expansion_i$ was the indicator for treatment state—it equals 1 if the patient obtained Medicaid coverage in the postperiod and visits a facility in a state which expanded Medicaid on January 1, 2014, and 0 otherwise. The treatment effect was captured by β_1. $Y_{pre,i}$ was the value of the outcome variable of interest in the preexpansion period. Age_i was the age of the patient averaged over all their visits in the sample and was a continuous variable. $Female_i$ was a 0/1 indicator. Inc_i was the zip-code-level median income based on patient residence. $PctUn_i$ was the zip-code-level percentage of 18- to 64-year olds who are uninsured. We clustered the standard errors at the facility level. We run the outcome models in STATA using the clustered sandwich estimator, where the standard errors allow for intragroup correlation within facility, relaxing the usual requirement that the observations be independent. We cluster the errors at the facility level instead of the state level to account for correlation of care practices and coding practices at the ED level and patient correlation related to living in proximity to the ED.

In a simulation study, LDV model has been demonstrated to produce the most efficient and least biased estimates when the unconditional parallel trends assumption is violated (O'Neill, Kreif, Grieve, Sutton, & Sekhon, 2016). As we are not able to track preexpansion ED use before April 2013, this model serves as an attractive estimation approach in our setting. Furthermore, it has been proven in early propensity score literature that including pretreatment covariates in the outcome model in addition to weighting to obtain balance on the same covariates provides "double robustness," whereby the outcome regression model results are unbiased if either the outcome model or the propensity score model are correctly specified (Bang & Robins, 2005; Ho et al., 2007; Hullsiek & Louis, 2002; Stuart, 2010). As a robustness check, we also estimate the effects using a weighted difference-in-differences model.

One source of potential bias in our analysis was the possibility that among those who are uninsured but newly eligible for Medicaid, sicker patients preferentially obtained Medicaid insurance. Thus, even while we weighted the control group patients to match the treatment group patients on the joint distribution of their preexpansion clinical and demographic characteristics, if our treatment group patients enrolled in Medicaid because they were sicker in ways that are not observable from prior ED visits, we may observe an upward bias in our estimates. To address this possibility, we performed a robustness analysis using the same regression model as in Equation (1) to estimate the effect of expansion on patients potentially eligible for Medicaid expansion, by redefining our treatment and control groups. In this analysis, "Treatment" group patients were those in expansion states

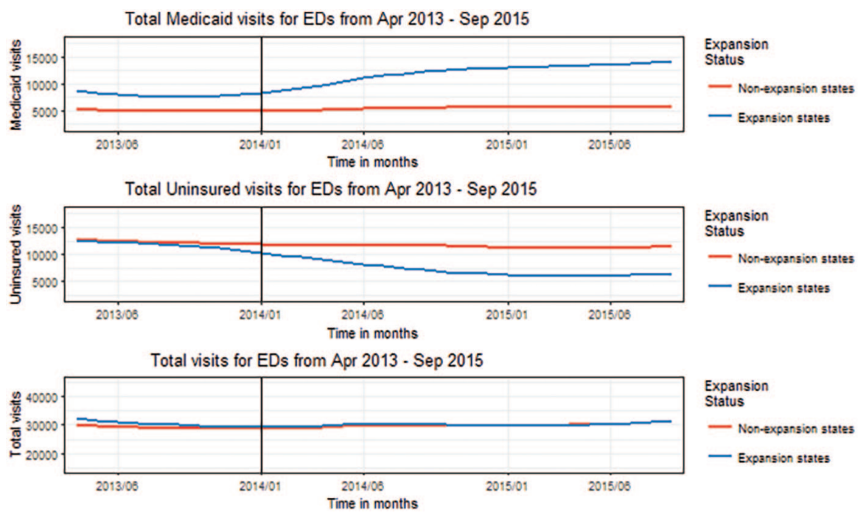

Figure 1. Facility-level emergency department (ED) visit trends during Medicaid expansion.
Note. Authors' analysis of monthly visits using data from 30 ED facilities across 7 U.S. states, April 2013 through September 2015.

who were uninsured in the preexpansion period and were uninsured or had Medicaid insurance in the postexpansion period. "Control" group patients were those in nonexpansion states who were uninsured in the preexpansion period and were uninsured or had Medicaid insurance in the postexpansion period. As before, we weighted our redefined "control" group to match the joint distribution of the preexpansion characteristics of the "treatment" group. Thus, if this selection bias was present, our redefined "treatment" group now included individuals who would have been eligible for Medicaid under the ACA but chose not to enroll as they were not "sick enough," along with the ones not eligible under the ACA expansion. This in turn would suggest that rates of high-acuity ED use for the presumed healthier nonswitching patients added to the original "treatment" group should be similar to or lower than for the "controls" (who were a mix of healthy and unhealthy). Under this scenario, the estimates of the average effect on the potentially eligible patients (AEP) are a weighted average of the positive estimate of the average effect on the switching patients (AES) and at most a zero estimate for the nonswitching patients resulting in a maximum result of the AES * take-up rate (proportion of switchers). We compared our AEP estimate with this expected result.

Another concern was the possibility that the overall pool of patients visiting facilities in expansion states might be systematically different than the pool visiting those in nonexpansion states. To address this, we ran a falsification test using a similar empirical model as in Equation (1) but on patients enrolled in Medicare, whose eligibility requirements did not change during the study period. Here the "treatment" and "control" group patients were Medicare patients who visited at least once before and after expansion in expansion and nonexpansion states, respectively.

We also note that we cover a limited number of states in our sample. As discussed earlier, this was largely because we restricted our analysis to states which had similar preexpansion eligibility limits for Medicaid. We had access to data from an additional 18 ED facilities which are in five states which expanded Medicaid in January 2014 (namely Arizona, California, Connecticut, Hawaii, New York), but which we excluded in our primary analysis as some of them already had some income-based eligibility limits for nonelderly able-bodied adults (Table A.1 in the appendix details criteria) and one other (CA) which expanded Medicaid prior to January 2014 in some of their counties. We performed robustness checks by using a similar empirical model as in Equation (1) and included these 18 facilities in the 5 states mentioned above in this sample.

Results

Facility-Level Trends

Figure 1 depicts the trends in insurance status of all patients visiting the included EDs through the period of Medicaid expansion, April 2013 to September 2015. We observe a sharp increase in Medicaid covered visits and a decline in uninsured visits after expansion went into effect that occurs only in expansion states. These trends are consistent with findings from Pines at al. (2016) showing strong take-up of Medicaid by ED users where Medicaid expansion occurred. EDs in this study were notably not the same ones used for the Pines et al. (2016) study. Our findings also span an additional 9 months into 2015. Also consistent with the prior study, no substantial change in the total number of ED visits in either treatment or control facilities is observed.

Table 1. Comparison of 2013 Visits in the Data Set With the 2013 National Hospital Ambulatory Medical Care Survey (NHAMCS).

	% Distribution of visits, sample	% Distribution of visits, NHAMCS
A: Comparison on selected patient characteristics		
Age (years)		
<15	11.2	18.2
15-24	15.4	15.1
25-44	32.6	27.5
45-64	24.7	23.3
≥65	16.1	15.9
Sex		
Female	56.8	56.0
Payment source		
Medicaid	24.6	34.1
Medicare	21.5	19.6
Commercial	22.3	36.0
Self-pay	28.9	15.1
Others	2.6	5.6
B: Comparison on primary diagnosis codes		
Infectious and parasitic diseases	2.2	2.8
Neoplasms	0.1	0.1
Endocrine, nutritional, metabolic diseases, and immunity disorders	2.0	1.5
Mental disorders	2.4	3.6
Diseases of the nervous system and sense organs	4.0	5.0
Diseases of the circulatory system	4.2	3.4
Diseases of the respiratory system	9.5	10.9
Diseases of the digestive system	5.9	6.3
Diseases of the genitourinary system	6.3	5.2
Diseases of the skin and subcutaneous tissue	4.3	3.7
Diseases of the musculoskeletal system and connective tissue	7.7	7.2
Symptoms, signs, and ill-defined conditions	23.8	22.6
Injury and poisoning	23.3	21.4
Supplementary classification	1.2	2.5
All others	3.2	3.0

Note. The 2013 sample comprises 1.249 million visits to 30 facilities across the 7 states in our sample: Illinois, North Carolina, Nevada, Ohio, Oklahoma, Rhode Island, and West Virginia. Data for the 2013 NHAMCS survey is available at https://www.cdc.gov/nchs/data/ahcd/nhamcs_emergency/2013_ed_web_tables.pdf

Descriptive Statistics on Patient Visits

Table 1 presents descriptive statistics for all visits to the EDs in the analysis sample in the year 2013 compared with data from the nationally representative 2013 NHAMCS. In general, the diagnoses and demographics of those visiting the EDs in our analysis sample are similar to those seen in ED visits nationwide. One exception is the difference in payment source. The percent of uninsured visits to the ED facilities in our sample exceeds that in the national survey data by almost 13% and the percentage of commercial visits in the sample is correspondingly smaller. As our focus is on the pre-ACA uninsured population, having a larger number of them in the EDs in our data relative to EDs nationally is an advantage to evaluate a treatment effect in our study. In Table A.2 in the appendix, we perform a similar comparison using 2014 data from the NHAMCS ED Summary Tables and find similar trends.

Table 2 compares control and treatment group patients in the preexpansion period on demographic and visit-level characteristics before and after the control group is propensity score weighted. Here, the effective sample size (ESS) of the weighted control group is approximately the number of observations from a simple random sample that yields an estimate with sampling variation equal to the sampling variation obtained with the weighted comparison observation (Ridgeway, McCaffrey, Morral, Burgette, & Griffin, 2014). It gives an estimate of the number of control group patients that are comparable to the treatment group after weighting. On average, in the preperiod, treatment group patients are somewhat more likely to be female, older, visit the same ED fewer number of times, have higher visit intensity (based on average RVUs/visit), have higher rates of visits which led to admissions, and have higher rates of “emergent” and unavoidable preexpansion visits. The propensity score

Table 2. Comparison Between Treatment and Control Group[a] Patients Before Medicaid Expansion.

		Control group	
	Treatment group	After weighting	Before weighting
Number of patients	7,822	12,826.71[b]	20,873
Average age	38.62	38.27	36.12
Proportion of patients female	0.55	0.55	0.50
Average no. of visits	1.60	1.62	1.90
Average RVU of visits	3.58	3.54	3.47
Proportion of visits which led to hospital admission	0.15	0.14	0.07
Proportion of visits which led to non-ACSC admissions	0.12	0.12	0.06
Proportion of emergent and unavoidable visits	0.14	0.14	0.12

Note. RVU = relative value unit; ACSC = ambulatory care sensitive conditions. Authors' analysis of treatment and control group patients using data from 30 emergency department (ED) facilities across 7 U.S. states, April 2013 through December 2013.
[a]Treatment group patients were those in expansion states who visited the ED while uninsured in the preexpansion period and who visited the ED with Medicaid insurance in the postexpansion period. Control group patients were those in nonexpansion states who visited the ED while uninsured in both the preexpansion and the postexpansion periods. [b]This is the effective sample size of the control group, after weighting. It gives an estimate of the number of control group patients that are comparable to the treatment group after weighting.

weighting was successful in making the control group comparable to the treatment group on all preexpansion characteristics. We present the detailed weighting criteria and show the full unadjusted (preweighting) and adjusted (postweighting) covariate balance in Table A.3 in the appendix. We perform graphical diagnoses as shown in Figure A.2 in the appendix using cobalt (Greifer, 2018) in R (R Core Team, 2018) to assess the balance and do not use hypothesis tests that incorporate information on the sample size (e.g., *t*-tests) as measures of balance, as recommended in Stuart (2010), as they can be misleading as measures of balance because they often conflate changes in balance with changes in statistical power.

Patient-Level Regression Results

Table 3 summarizes the effects of gaining Medicaid coverage on measures of high-acuity ED use. Those patients who gained Medicaid coverage had increases of 4.3 percentage points ($p < .05$) in the proportion of visits resulting in hospital admissions relative to control patients. Relative to the base rate of 15% of ED visits resulting in admission to the hospital in the preperiod, this represents a nearly 29% increase. Compared with the control group, those who gained Medicaid coverage also had increases of 3.8 percentage points ($p < .05$) in the proportion of visits which led to admissions for "non-ACSCs." Relative to the base rate of 12% of ED visits resulting in admission to the hospital for a health condition that was not ambulatory care sensitive in the preperiod, this represents a nearly 32% increase. For our other proxy measures of high-intensity ED use: proportion of unavoidable "emergent" visits and the average RVUs/visit, the point estimates for the coefficients of interest were positive, but not statistically significant. We observe negative, but statistically insignificant, differences in measures of low-acuity visits, the proportion of nonemergent visits and emergent but primary care treatable visits. These results along with changes in individual Multi-level Clinical Classification Software codes are detailed in Tables A.4 and A.5 in the appendix. Due to the modest number of clusters in our analysis sample (30 EDs), effect sizes needed to be fairly large for us to have the power to detect them. Table A.6 in the appendix details the results of the traditional difference-in-differences model specification. We note that our estimates are of a similar magnitude and significance as that of the weighted LDV model. One concern is the number of clusters (facilities) and their unbalanced nature which might lead to over rejection of the null (Cameron & Miller, 2015). To mitigate that concern, we run our models using the pairs bootstrap clustering method. Because of complications involved with weighting, we compare our unweighted estimates from the sandwich cluster method to that from bootstrap clustering, and found the results to have similar statistical significance. Results are shown in Table A.7 in the appendix.

Table 4 summarizes the AEP, when we compare the outcome measures of the redefined groups. We observe that for the outcomes with statistically significant AES findings, our coefficients of interest are in the same direction as in the original outcome model and reduced substantially less than what the take-up rate in the "treatment" group would suggest (where we would expect them to be reduced more if the potential bias were present). The AEP estimates are marginally statistically significant with an increase of 3.1 percentage points ($p < .1$) in the proportion of visits resulting in hospital admissions, and 2.7 percentage points ($p < .1$) in the proportion of visits which led to admissions for "non-ACSCs." We note the increases in the number of observations within clusters (EDs), but the same number of clusters results in effectively unchanged power for the AEP and AES analyses. These results are suggestive that our findings of an

Table 3. Average Effect on the Switching Patients: Results on Measures of High-Acuity and High-Intensity Use.

	Proportion of visits which led to hospital admission	Proportion of visits which led to non-ACSC admissions	Proportion of emergent and unavoidable visits	Average RVUs/visit
Newly covered by Medicaid (vs. those remaining uninsured)[a]	0.043** (0.018)	0.038** (0.016)	0.010 (0.007)	0.055 (0.067)
No. of patients	28,223	28,223	28,223	28,222
Average treatment group values before Medicaid	0.15	0.12	0.14	3.58
% Change[b]	28.7	31.7	7.1	1.5
95% CI for % change	[4.7, 52.7]	[4.2, 60]	[−2.9, 17.1]	[−2.4, 5.4]

Note. RVU = relative value unit; ACSC = ambulatory care sensitive conditions. Authors' analysis of patient-level emergency department (ED) use using data from 30 ED facilities across 7 U.S. states, April 2013 through September 2015. Standard errors in parentheses are clustered at the facility level $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$. These are propensity score weighted ordinary least squares models where the dependent variable is patient-level outcome variable in the postexpansion period. Controls: age, sex, zip-code-level median income, zip-code-level percentage uninsured, value of outcome variable of interest in preexpansion period.

[a]This is the β_1 coefficient associated with the $Expansion_i$ indicator in Equation (1). Those newly covered by Medicaid were in expansion states, while the comparison group remained uninsured and visited EDs in nonexpansion states. Median income and percentage uninsured in zip code were sourced from the 2009 to 2013 American Community Survey 5-Year Estimates. [b]Percentage change and the 95% confidence intervals are calculated using average treatment values before Medicaid expansion as the denominator.

Table 4. Average Effect on the Potentially Eligible Patients: Results on Measures of High-Acuity and High-Intensity Use.

	Proportion of visits which led to hospital admission	Proportion of visits which led to non-ACSC admissions	Proportion of emergent and unavoidable visits	Average RVUs/visit
Patients in expansion states (vs. those in nonexpansion)[a]	0.031* (0.016)	0.027* (0.015)	0.006 (0.006)	0.005 (0.062)
No. of patients	40,975	40,975	40,975	40,972
Average "treatment group" values before Medicaid	0.13	0.11	0.14	3.53
% Change[b]	23.9	24.6	4.3	1.4
95% CI for % change	[−1.5, 50]	[−3.6, 52.7]	[−5, 13.6]	[3.4, 3.7]

Note. RVU = relative value unit; ACSC = ambulatory care sensitive conditions. Authors' analysis of patient-level emergency department (ED) use using data from 30 ED facilities across seven U.S. states, April 2013 through September 2015. Standard errors in parentheses are clustered at the facility level $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$. These are propensity score weighted ordinary least squares models where the dependent variable is patient-level outcome variable in the postexpansion period. Controls: age, sex, zip-code-level median income, zip-code-level percentage uninsured, value of outcome variable of interest in preexpansion period.

[a]This is the β_1 coefficient associated with the $Expansion_i$ indicator in Equation (1). The modified "treatment group" patients were in expansion states and were uninsured in the preexpansion period and remained uninsured or switched to Medicaid insurance postexpansion, while the "control group" patients were similar patients in nonexpansion states. Median income and % uninsured in zip code were sourced from the 2009 to 2013 American Community Survey 5-Year Estimates. [b]Percentage change and the 95% confidence intervals are calculated using average treatment values before Medicaid expansion as the denominator.

increase in high-acuity visits in expansion compared with nonexpansion states are robust to the potential source of bias of unobservably sicker patients enrolling in Medicaid. Detailed results are shown in Table A.8 in the appendix.

In our falsification analysis on patients continually enrolled in Medicare, we do not observe any statistically significant differences between our "treatment" and "control" group patients on the outcome measures described above. This mitigates potential concerns about the pool of patients visiting facilities in expansion states being systematically different than the pool visiting those in nonexpansion states. Results are shown in Table A.9 in the appendix.

For our analysis where we included visits to 18 additional facilities in the five states (Arizona, California, Connecticut, Hawaii, New York) we previously excluded from our analysis, we observe that the point estimates of our coefficients of interest are in the same direction as in the original outcome model. The estimates are marginally statistically significant with an increase of 3.1 percentage points ($p < .1$) in the proportion of visits resulting in hospital admissions, 2.8 percentage points ($p < .1$) in the proportion of visits which led to admissions for "non-ACSCs," and 1 percentage point ($p < .1$) in the proportion of unavoidable "emergent" visits. The drop in effect size is expected as now our "treatment" group also includes relatively more well-off individuals (as these new states already had some degree of coverage for the poorest able-bodied adults), whose inclusion potentially dilutes the expansion effect. Detailed results are in Table A.10 in the appendix.

Discussion

In the complex health care landscape of the United States, EDs occupy a unique position and have been the focus of a number of interventions and studies to assess their use, particularly for low-acuity conditions that could potentially be treatable elsewhere (Ragin et al., 2005; Trueger et al., 2017). Our study finds that gaining Medicaid coverage under the ACA shifts previously uninsured patients toward using the ED for conditions that were more likely to result in hospital admission and in admissions for nonambulatory sensitive conditions than the same patients had been using the ED for previously, compared to trends for those who remained uninsured in nonexpansion states.

This study is the first to our knowledge to demonstrate that individual patients gaining Medicaid coverage under the ACA shifted their ED use toward visits for higher acuity conditions. Moreover, this is the first study that follows individual patients gaining Medicaid insurance through the 2014 Affordable Care Act Medicaid expansion and compares their changes in ED use to patients who are similar in their baseline ED use and remain uninsured in states not expanding Medicaid. Our results are broadly consistent with Xu et al. (2018) despite their estimation of a different parameter, use of an insured rather than uninsured comparison group with different baseline ED use, and focus on one state. The effect size in our study was large—specifically ED encounters were nearly 29% more likely to result in hospital admission in expansion states, compared with those visiting the ED who remained uninsured in nonexpansion states. We also find an increase of similar magnitude (nearly 32%) in admissions for non-ACSCs. This is a particularly important finding for the repeat ED user population studied here, for whom low-acuity ED use is especially of concern.

An explanation for these results is that newly insured Medicaid patients' access to outpatient care may have been relatively improved. Hence, they may seek lower acuity care elsewhere, more often using the ED for truly "sick" care. This is consistent with the findings of a previous study regarding reduction in low-acuity ED use among young adults gaining private insurance coverage by being just under versus just above the age cutoff for obtaining health insurance coverage through their parents' plans under the ACA (Antwi et al., 2015). This is also consistent with findings in Roberts and Gaskin (2015) that adults with Medicaid coverage have (on average) higher visits per year to primary care providers than low-income adults without Medicaid. Our study also confirmed prior studies showing that despite clear take-up of Medicaid in expansion states, aggregate ED use did not disproportionately increase in expansion relative to nonexpansion states (Pines et al., 2016). Our confirmatory finding extends to the first 9 months of 2015 and in a different sample of facilities.

We note two other potential explanations of these findings. First is that admission and related decisions in the ED may be influenced by patient insurance status changing from uninsured to Medicaid (Kindermann, Mutter, Houchens, Barrett, & Pines, 2015). However, federal law—Emergency Medical Treatment and Labor Act—requires all EDs and emergency physicians working in EDs to treat and stabilize patients to the capability of the facility regardless of insurance status (Centers for Medicare & Medicaid Services, 2014). This law is likely to limit the extent to which care choices in the EDs are affected by insurance status. Second, there is a concern that Medicaid-managed care network arrangements could influence their Medicaid enrollees to use EDs at different hospitals than those in our data set. Medicaid is required to fully cover ED costs at any hospital if it is determined that the need for care was an emergency. If it is determined that the need for care was not an emergency, there can be nominal cost sharing, which can vary by state (American College of Emergency Physicians, 2018; Siddiqui, Roberts, & Pollack, 2015). There is no evidence, however, that this cost sharing differs for EDs in different hospitals (e.g., by network status), thus helping mitigate this concern.

One possible reason for the different results here than in Oregon, where those gaining Medicaid coverage increased ED use by 40% for both high- and low-acuity conditions, is the Medicaid Parity Demonstration Program (Finkelstein et al., 2012; Taubman et al., 2014). This program increased the amount Medicaid paid for primary care visits to Medicare payment levels for just the first year of Medicaid expansion. This may have resulted in greater than usual gains in access to primary care for those newly obtaining Medicaid coverage under the ACA relative to Oregon's lottery-based expansion. It is also possible that there were psychological effects of winning access to Medicaid coverage via a lottery versus obtaining it through federal legislation that affected subsequent health care use patterns (Haisley, Mostafa, & Loewenstein, 2008).

It is unclear if gains in access to outpatient care that may have led to reductions in low-acuity ED use will persist now that the Parity program has ended. In our sample, only Nevada continued with higher primary care fees in 2015 (Snyder, Paradise, & Rudowitz, 2014). Recent work on access to outpatient care during the first two years of the ACA Medicaid expansions continued to find increased access during the second year of implementation, but also with longer wait times for appointments, which suggests that some challenges in access to outpatient care reemerged (Miller & Wherry, 2017). Therefore, future work is needed to confirm that increased use of outpatient care for repeat ED users gaining Medicaid coverage is the reason for shifts in the nature of ED use and to understand how this evolves over time as parity payment levels change in some states.

For hospitals, our findings suggest that more hospital resources may be required to care for Medicaid-insured populations per visit after Medicaid expansion, particularly as care shifts to patients requiring more ED and hospital resources, including inpatient beds. Therefore, the changing nature of ED use after insurance expansions has implications for resource planning in these facilities. From a policy perspective, understanding the impact of the ACA's Medicaid expansion is important considering that there remains significant political controversy on the perceived value of Medicaid expansion. Our finding that repeat users are shifting their ED use to care for higher acuity conditions suggests that on this dimension the Medicaid expansion is doing what it was intended to do: move ED use to those who really need it and improve the efficiency of health care delivery. This shift in ED use may have been enabled by better outpatient management for acute and chronic conditions, lowering acute exacerbations of ambulatory sensitive conditions in this population.

Limitations

As study subjects were not randomized, our results could have been affected by selection bias on unobserved factors—the treatment and control group patients could be dissimilar in ways we cannot observe. In particular, the lack of patient-specific income data means we could not definitively ascertain if the uninsured control patients would have been eligible for Medicaid if they were in the treatment states. However, our robust weighting and regression control strategy mitigated this bias to the extent that the rich set of observables in our models—demographics, preexpansion ED visit clinical and payment information, and zip-code-level characteristics—proxy for patient income and other potential unobserved confounders. Future research should consider taking into account heterogeneity in Medicaid patient acceptance rates of primary care physicians in specific locations and studying how this affects local ED use. Furthermore, our findings may not be generalizable beyond the specific subgroup of patients studied: repeat ED users. Our analysis was enabled by our ability to track patient visits within the same facility but limited in that this tracking was not available across facilities. The Billings algorithm for classification of emergent and nonemergent visits has well-known limitations (Raven, Lowe, Maselli, & Hsia, 2013). In particular, the algorithm leaves a significant proportion of visits unclassified. To that end, we examine other measures of visit acuity: proportion of visits which led to admissions, proportion of visits which led to admissions for non-ACSCs, and visit intensity: RVUs/visit. Furthermore, while in Table A.5 in the appendix we see changes in the case-mix for some clinical categories for which patients present to the ED after gaining Medicaid access, future work could also examine why specific sets of conditions might have changed after expansion. Finally, this study had modest power to detect impacts due to the clustering of patients within a limited number of EDs. Because of this limitation, the hypotheses we failed to reject in this study deserve additional study with greater numbers of EDs.

Conclusion

Our study suggests that the impact of Medicaid expansion on previously uninsured repeat ED users' subsequent ED utilization was to increase the proportion of ED visits for higher acuity conditions. Specifically, individuals who acquired Medicaid insurance after being uninsured presented more often to the ED for conditions that required hospitalization after their ED visit and hospitalizations for nonambulatory care sensitive conditions. Our findings suggest that ending Medicaid expansion may increase low-acuity use of EDs for those who lose insurance and reduce the efficiency of EDs for their intended design: to take care of critically ill and injured patients that require acute services.

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Notes

1. Not every ED in the database is hospital based. Some may be free standing. There are 101 facilities across all years in the database. In any particular year, the number of EDs would be lower as facilities get added and subtracted as contracts change. The 16 states are: Arizona, California, Connecticut, Hawaii, Illinois, Kentucky, Michigan, North Carolina, New Hampshire, Nevada, New York, Ohio, Oklahoma, Pennsylvania, Rhode Island, and West Virginia.
2. We restricted our analysis to visits after April 1, 2013 as we were unable to link patient IDs before this date.
3. The ACS is an ongoing survey that provides data every year—giving communities the current information they need to plan investments and services. The ACS covers a broad range of topics about social, economic, demographic, and housing characteristics of the U.S. population (https://www.census.gov/programs-surveys/acs/technical-documentation/table-and-geography-changes/2013/5-year.html)
4. The NHAMCS is designed to collect data on the utilization and provision of ambulatory care services in

hospital emergency and outpatient departments and ambulatory surgery locations. Findings are based on a national sample of visits to the EDs, outpatient departments, and ambulatory surgery locations of noninstitutional general and short-stay hospitals (https://www.cdc.gov/nchs/data/nhamcs/web_tables/2014_ed_web_tables.pdf)

5. Agency for Healthcare Research and Quality. Prevention Quality Indicators Overview (http://qualityindicators.ahrq.gov/Modules/pqi_resources.aspx).
6. ACSC visits are determined using a mix of visit diagnosis and procedure codes. Since we do not have access to the procedures that admitted patients underwent, we only use the visit diagnosis codes for classification.

Supplemental Material

Supplemental material for this article is available online.

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