Access to Health Insurance and the Use of Inpatient Medical Care: Evidence from the Affordable Care Act Young Adult Mandate

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Abstract

The Affordable Care Act of 2010 expanded health insurance coverage to young adults by allowing them to remain as dependents on their parent's private health insurance until they turn 26 years old. While there is evidence that this law has meaningfully affected the insurance status of young adults, we know very little of how this extended coverage has affected the use of medical care. We study the implications of the expansion for the use of an especially expensive form of medical care, inpatient hospital visits, using the Nationwide Inpatient Sample. We find evidence that compared to those who were excluded from the mandate but close in age, treated young adults 23-25 years of age increased their inpatient visits by 3.5 percent after the policy implementation. Both emergency and non-emergency sources of admissions contributed to this surge. Visits for mental illness increased by 5.5 percent. As a result of the reform, the prevalence of uninsurance among hospitalized young adults decreased by 10.8 percent.

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I. Introduction

In the United States, the transition from adolescence to young adulthood is associated with the loss of health insurance coverage (DeNavas-Walt, Proctor, and Smith, 2010). Prior to the Affordable Care Act (ACA), uninsurance among the non-elderly peaked at around ages 21 to 23 at close to 40 percent.[2] Young adults lack health insurance coverage because they age out of their parents' health insurance coverage when they graduate from either high school or college, and because public health insurance eligibility rules phase out coverage starting at age 19 (Nicholson et al., 2009; Levine, McKnight, and Heep, 2011; and Anderson, Dobkin, and Gross, 2012). In addition, young adults lack adequate experience and skills to obtain jobs that provide employer-sponsored health insurance (ESI) and are less likely to take up health insurance coverage even when offered (DeNavas-Walt, Proctor, and Smith, 2010; and Monheit et al., 2011).

The precarious health insurance status of young adults motivated the early ACA provision that starting in September 2010 has allowed young adults to remain as dependents on their parents' private health insurance plans until they turn 26 years old. Evidence of the impact of this expansion shows that a large number of uninsured young adults have benefitted from the mandate by becoming insured as dependents on parental policies, although magnitudes vary among studies (Cantor et al., 2012; Sommers and Kronick, 2012; Sommers et al., 2013; and Akosa Antwi, Moriya, and Simon, 2013). There is, however, limited evidence on the effect of this health insurance expansion on young adults' use of hospital care.

[2] Author's calculation using 2008 Current Population Survey data.

The ACA is expected to be the largest health insurance expansion since the establishment of Medicare in 1965. Young adults are a key population targeted under the law even beyond the specific mandate we study. On the one hand, because young adults are comparatively healthy, high young-adult enrollment is seen as an important goal for the success of health insurance exchanges (Weaver and Radnofsky, 2013). On the other hand, in an era of mental health parity in coverage, this population may tend to be costly to insurers because of their relatively high consumption of mental health care (Grant and Potenza, 2010). Estimating the effect of this ACA young adult insurance expansion on health care use is crucial for understanding the law fully, as well as for anticipating the effects of later expansions on this population. Empirical evidence of the impact of the young adult dependent coverage expansion is of high interest to policymakers, who will likely continue to fine-tune the details of the ACA for some time; it also contributes to the growing academic literature on the effect of health insurance on medical-care use.

We use the Nationwide Inpatient Sample (NIS), a nationally-representative database of inpatient admissions, from 2007 to 2011 to evaluate the effect of the young adult ACA insurance expansion on the use of inpatient medical care, treatment intensity, and insurance status. Inpatient visits are rare but expensive medical events and are a vital component of any effort to "bend the health care cost curve". For instance, for 23-to-25-year-olds, who are the focal age group in our paper, inpatient visits represented about 44 percent of total health care expenditures

even though only 6.9 percent of individuals in this age group had an inpatient visit in 2008.[3] We identify the effects of policy on the targeted age group using a differences-in-differences (DD) method that compares the treatment group of 23-to-25-year-olds to 27-to-29-year-olds, the latter being a comparison group that is close to but excluded from the expansion. We also conduct extensive robustness checks regarding the assumptions underlying this identification strategy.

We examine the impact of the ACA young adult mandate on inpatient care first on the total number of hospitalizations for the targeted young adult population. The transition from adolescence to adulthood often involves many life changes that can trigger mental distress, making services related to mental illness an important component of young adult medical care (Patel et al., 2007; and Yu et al., 2008). Thus, the second aspect we consider is the effect of the mandate on mental illness admissions. For both types of utilization, we analyze the impact of the mandate separately on admissions that originate from the emergency room (ER) and those that are direct admissions to inpatient care. To shed light on the changes hospitals likely face in reimbursement for young adult care as a result of reform, we estimate the change in insurance that occurred among the population hospitalized. We also evaluate the impact of the law on treatment intensity as measured by length of stay, number of procedures, and hospital charges.

We find that compared to slightly older young adults, those targeted by the law increased their overall (non-pregnancy-related) inpatient visits by 3.5 percent. Both ER and non-ER sources of admission are responsible for the overall increase in visits. We also find a 5.5 percent increase in mental-illness-related visits. Corresponding to these changes in service utilization, we

[3] Author calculations from 2008 Medical Expenditure Panel Survey.

find that the fraction of hospitalized young adults without insurance decreased by 10.8 percent compared to other adults who did not benefit from the expansion. The implied estimate of the impact of this health insurance coverage on the probability of an inpatient visit is 85.4 percent. This is larger than, but within the 95 percent confidence interval of, the 77 percent estimated by Anderson, Dobkin, and Gross (2013) in their study of the impact of losing health insurance coverage at age 23 on inpatient visits. In examining whether treatment intensity of hospital usage is affected, we find no robust evidence of changes in the impact of the law on length of stay, number of procedures, and total charges. Our findings on the use of inpatient care lead us to reject the null hypothesis that access to health insurance coverage has no impact on medical-care use.

II. Prior Literature: The Effect of Health Insurance Expansion on Utilization

A large body of empirical research studies the effect of health insurance expansions on the use of inpatient care. Most of these studies use a quasi-experimental research design to estimate the impact of health insurance expansions on the medical-care use of specific populations. Studies on Medicaid and Medicare expansions, for instance, find that they lead to an increase in the consumption of inpatient medical care (Dafny and Gruber, 2005; Card, Dobkin, and Maestas, 2008; and Finkelstein et al., 2012). Evidence on the impact of a near-universal health insurance coverage expansion in Massachusetts finds a decrease in the use of emergency departments but no change in hospitalizations (Kolstad and Kowalski, 2012; and Miller, 2012).

Research especially relevant for our work studies the impact of health insurance on medical-care use by young adults. Anderson, Dobkin, and Gross (2012 and 2013) estimate the effect of health insurance coverage on emergency department and inpatient visits by exploiting the sharp change in insurance coverage rates that result from young adults "aging out" of their parent's health insurance plans at ages 19 and 23. Using a regression discontinuity (RD) method, they find significant reductions in ER and inpatient visits at both age cutoffs. The effect of health insurance on inpatient care is larger at age 23 than at age 19. Losing health insurance at 23 decreases the probability of an inpatient visit by 77 percent. The same statistic at age 19 is 61 percent.

We are aware of two existing studies on the effect of the ACA young adult mandate on access and use. Using a nationally representative survey of young adults and an age and time period DD study design, Sommers et al. (2013) find evidence that the mandate increased self-reported access to care but had no statistically significant effect on self-reported usual source of care. Mulcahy et al. (2013) use a large convenience sample of claims on non-discretionary visits to the ER to evaluate the impact of the ACA young adult mandate on insurance status of ER users in a DD study design. They find that the expansion led to a 3.1-percentage-point increase in non-discretionary emergency care paid by private insurance among young adults.

Taken together, prior literature on the impact of health insurance expansions to Medicaid recipients, Medicare recipients, and residents of Massachusetts on inpatient care use is rather mixed, with some studies finding sizeable effects (Finkelstein et al., 2012; Dafny and Gruber,

2005; Card, Dobkin, and Maestas, 2008) and others finding no effects (Kolstad and Kowalski, 2012). For young adults, empirical evidence suggests a decrease in the use of medical care following the "aging out" of their parents' health insurance plans (Anderson, Dobkin, and Gross, 2012 and 2013) and an increase in access following the ACA young adult mandate (Sommers et al., 2013).

Our research makes several distinct contributions. We are the first to provide evidence on the impact of the ACA dependent coverage expansion on inpatient use, the most expensive form of medical care. Second, we contribute to the literature on the effect of insurance on use of care among young adults by evaluating the effect of gaining rather than losing health insurance on medical use. Evidence provided by Anderson, Dobkin, and Gross (2012 and 2013) uses a RD approach to measure the effect of anticipated loss of health insurance on medical-care use. The effect of gaining and losing health insurance may not be symmetric. Third, we examine the effect of the law on prevalence of insurance coverage among young adults using inpatient services; prior literature has studied the impact of the law on insurance in the general population as well as in the population of non-discretionary ER users, but there is reason to expect that results could be different for inpatient care users.

III: Mechanisms

The mechanisms by which the availability of health insurance is expected to affect medical-care use have been well covered in prior literature (Finkelstein et al., 2012; Kolstad and Kowalski, 2012; and Miller, 2012), thus our discussion of the hypotheses remains brief.

Access to health insurance reduces the cost of medical care for those who are newly insured. The moral hazard of health insurance suggests that newly insured young adults would increase their consumption of medical care. The effect of health insurance on how newly insured young adults enter the medical system is not clear. If inpatient and outpatient care are substitutes for some conditions, then having health insurance could reduce hospital ER admissions through better outpatient care. A countervailing point to this is that newly insured individuals tend to increase their use of the ER, suggesting it could be a complement rather than a substitute (Ginde, Lowe, and Wiler, 2012).

There are competing hypotheses about the likely effect of the mandate on treatment intensity. The law is expected to change the health insurance composition of hospitalized young adults by reducing the proportion of uninsured visits and increasing the proportion of hospital visits covered by private insurance. If health insurance status affects the treatment decisions of health care providers, then we would expect an increase in treatment intensity since privately insured individuals tend to receive more treatment than their uninsured counterparts (Doyle, 2005). Intensity of care may also decrease, as marginal young adults seeking care after the expansion could be healthier than others to the extent that young adults whose parents have ESI tend to be healthier. Thus, the direction of predicted change in intensity is ambiguous.

IV. Data

The main data we use for our analysis is the Nationwide Inpatient Sample (NIS) compiled by the Healthcare Cost and Utilization Project (HCUP), at the Agency for Healthcare Research and Quality. The NIS data contains discharges at a 20% stratified sample of community hospitals in the United States. Every year, a hospital included in the sample contributes the universe of its discharges. On average the NIS has information on about eight million hospital stays a year from about 40 states in the U.S. Data from Alabama, Delaware, the District of Columbia, and Idaho are not in our sample because they do not contribute data to the NIS in any year of our sample. We exclude data from California, Maine, and Texas because precise information on age, which is crucial for our study design, is not available and thus prevents us from separating the control and treatment groups. However, we note that results are qualitatively the same when we include these states and estimate models with less than ideal control and treatment group age definitions.

We use the NIS data from 2007 to 2011 (the latest year for which data is available) to allow a sufficient look-back period to test for differences in trends between treatment and control groups. Each observation in our data is a patient discharge abstract, which includes detailed clinical information, such as primary and secondary diagnoses, and demographic information, such as age, gender, and race/ethnicity. We apply the HCUP Comorbidity Software to these

diagnoses to obtain comorbidity measures, which we in turn use as risk-adjustment variables in our analysis of treatment intensity.

We follow prior literature by using non-pregnancy-related visits as our main sample, excluding Major Diagnostic Categories (MDC) 14: Pregnancy, Childbirth, and the Puerperium. Our final sample is composed of 794,392 hospital visits for our treatment and comparison groups. We also perform secondary analysis on visits associated with mental illness (restricting our sample to MDC 19: Mental Diseases and Disorders; 128,310 visits). Our data also includes information on health insurance status such as private insurance, Medicaid, Medicare, other insurance, and no insurance. We cannot separately identify treated young adults who are dependents on their parent's health insurance from those who have their own ESI or non-group insurance.

To calculate our measure of number of inpatient visits, we aggregate the number of hospital discharges by young adults by quarter for each hospital. We distinguish between ER and non-ER admissions with the variable, admission source. Each hospital abstract has information on length of stay and ICD-9 codes for each procedure, up to six at the minimum. We sum these up to calculate our number of procedures variable.

V. Method

To estimate the effect of the young adult coverage expansion on the use of medical care and treatment intensity, one must isolate the policy's impact from contemporaneous changes. We

use a difference-in-difference (DD) method that compares a subset of targeted young adults, 23-25-year-olds and a control group of 27-29-year-olds, who are as similar as possible in age but excluded from the insurance expansion. Previous federal government policy allowed young adults who were full-time students to remain on their parent's insurance, and recent evidence shows that the largest reductions in insurance coverage prior to the ACA occurred at age 23 (Anderson, Dobkin, and Gross, 2013). We exclude 26-year-olds since we cannot accurately classify them as part of either group. Our empirical strategy rests on the strong assumption that the control group will account for other time-varying factors that would have led the treatment group to experience different rates of medical care access and use after reform. We test the validity of the DD estimator in Section VIII, where we discuss relevant robustness properties including trend tests and placebo tests using data prior to the ACA.

Our first analysis examines how the reform affected the use of inpatient medical care and how young adults entered the health care system. Using aggregated hospital level quarterly data on total number of non-pregnancy-related inpatient visits, non-pregnancy-related inpatient visits that originate from the ER, and non-pregnancy-related inpatient visits that did not originate from the ER, we estimate a model similar to that of Kolstad and Kolwalski (2012), who use the NIS data to evaluate health reform in Massachusetts. Our estimating equation is:

$$[1]\ Y_{hsgt} = \alpha + \gamma\, Treat_g + \delta\, Implement_t + \vartheta\, Enact_t + \eta\left(Treat_g * Implement_t\right) + \sigma\left(Treat_g * Enact_t\right) + X_{st}\beta + \tau_t + \zeta_{hs} + \varepsilon_{hsgt},$$

where Y_{hsgt} represents our outcome variables of interest for age range g in hospital h, state s, and quarter t. $Implement_t$ represents a dummy for the period after the law was implemented in September 2010. Since our analysis is at the quarterly level, our implementation phase starts from the fourth quarter of 2010 and runs through the latest period of data available, the fourth quarter of 2011. The variable $Treat_g$ is a binary variable for membership in the 23-25 age range (relative to the 27-29 range). The interaction of $Implement_t$ and $Treat_g$ captures the average impact after the law was implemented in September 2010 by comparing non-pregnancy-related hospital visits during this period to visits before the enactment of the law among the treatment group relative to the control group.

To examine possible anticipatory changes, we add a dummy variable, $Enact_t$, to capture the period from April 2010 to September 2010, the period between enactment and implementation of the law, and its interaction with the treatment dummy variable. The X_{st} vector represents control variables at the quarter-state level and includes the quarterly state unemployment rate, an interaction of the treatment dummy variable and the unemployment rate, and quarterly linear state-specific time trends. We also include dummy variables for year and quarter in τ_t, to control for seasonality and time. We include hospital fixed effects in ζ_{hs} to account for time-invariant hospital characteristics. We use ordinary least squares to estimate all of our continuous outcomes and the linear probability models for our binary outcomes. To account for correlation of hospital-level errors over time due to common forces, we cluster standard errors by hospital.

VI. Results

Summary Statistics

We present sample means of our treatment and control groups in Table 1. For most demographic and clinical dimensions the two groups appear similar. Females have slightly more inpatient visits than males, even though we use data on non-pregnancy-related admissions. For 23-25-year-olds, 49.3 percent of visits are by males, while the same statistic is 47.5% among 27-29-year-olds. Both groups are comprised of a similar proportion of Hispanics (10.3% vs. 10.2%), Whites (60.8% vs. 61.2%), and African Americans (22.1% vs. 22%). Targeted young adults have a higher likelihood of a mental health admission (17.1%) compared to slightly older adults (15.4%). In addition, our treatment group has a slightly higher likelihood of entering inpatient care through the ER (65.3% vs. 63%) or on the weekend (23.6% vs. 22.2%). The two groups have similar characteristics with respect to length of stay, number of procedures, and total charges.

When we consider health insurance status as our outcome, we see some level differences between the two groups. Conditional on seeking inpatient care, older adults are slightly more likely to have private health insurance than targeted young adults (35.8% vs. 35.2%). Targeted young adults are more likely to have Medicaid than older adults (28.6% vs. 27.7%) and less likely to have Medicare (6.7% vs. 9.7%). The rate of uninsurance is 22% for targeted young adults and 19.8% for older adults.

Impact of the ACA Young Adult Mandate on Use of Medical Care and Source of Admission

Table 2 contains regression results from Equation [1], where the dependent variable is the number of admissions at the quarter, hospital, and group (control vs. treatment) level. The results indicate that the expansion had a statistically significant impact on overall inpatient visits for non-pregnancy-related conditions. This change started during the enactment phase and continued after the implementation of the law. Relative to a mean average quarterly visits per hospital of 23.7 prior to the passage of the law, the coefficient of 0.061 in column 1 of Table 2 indicates that the overall number of visits increased by 2.6 percent between the law's enactment and implementation. After implementation, visits increased by 3.5 percent. The increases in visits during the law's enactment and implementation phases are not statistically different from each other.

In considering the possible substitution away from ER care to outpatient care, we investigate the effect of the health insurance expansion on the source of inpatient admission by estimating the impact of the law on non-pregnancy-related ER and non-ER admissions. In columns 2 and 3 of Table 2, we find that both ER and non-ER admissions increased after the law was implemented for young adults who benefitted from the mandate compared to those who did not. The number of ER admissions increased by 2.9 percent and the number of non-ER admissions increased by 4.6 percent. We also find a 4.5-percent increase in the number of non-ER admissions during the enactment periods. Direct hospital admissions tend to be scheduled

visits and are likely more sensitive to health insurance access, hence the larger percent increase is not surprising. Our results on the impact of the ACA dependent coverage expansion on overall non-pregnancy visits and by source of admission are consistent in direction with Anderson, Dobkin, and Gross (2012), who find that young adults decrease their ER and non-ER visits for non-pregnancy admissions when they age out of insurance.

Impact of the ACA Young Adult Mandate on Mental Illness Visits

We study mental health admissions separately because they may be especially impacted by the ACA young adult mandate given the particular health needs of this population, especially after the passage of the Mental Health Parity and Addiction Equity Act in 2008 (Garcia, 2010). We present our analysis of the impact of the mandate on the total number of mental health admissions in the last three columns of Table 2. We find a statistically significant 5.5-percent increase in the number of visits. Parsing these results by the source of admission, we find that visits originating from the ER comprise a significant portion of our overall mental health results. Admissions through the ER increased by 7.1 percent after the law was implemented. Our estimate for non-ER admissions after the law, while positive, is not statistically significant at conventional levels.

Impact of the ACA Young Adult Mandate on the Health Insurance Status of Inpatient Visits

In Table 3, we evaluate the impact of the mandate on the health insurance composition of young adults who sought inpatient care after the reform using the same sample and regression framework we describe above. The dependent variable here is the fraction of the hospital-quarter-level inpatient population with private insurance, no insurance, Medicaid, Medicare, and other insurance. These health insurance categories are mutually exclusive, so the horizontal sum of all the coefficients presented in Table 3 is zero. Our DD estimate presented in the first column of the top panel of Table 3 shows that the proportion of young adults in our treatment group with private insurance increased by 1.7 and 4 percentage points, respectively. Relative to the mean quarterly fraction of privately insured visits, these represent 4.6- and 10.9-percent increases in private coverage of non-pregnancy-related visits among our treatment group. Our next set of results shows that the fraction of young adults in our treatment group without health insurance decreased by a statistically significant 2.5 percentage points as a result of the law's implementation. This estimate implies that compared to the period before the passage of the ACA and relative to a control group, the fraction of young adult non-pregnancy-related inpatient care that was uninsured decreased by 10.8 percent. There is a marginally statistically significant negative coefficient associated with reform and Medicaid coverage. We find insignificant effects on the fraction of young adult care paid through Medicare or other insurance not elsewhere classified.

The bottom panel of Table 3 shows the impact of the law on the health insurance status of hospitalized young adults with a mental illness diagnosis. In the first column, we estimate a

4.1-percentage-point (14.6 percent) increase in the fraction of young adult mental-health-related inpatient visits paid through private health insurance. There is no statistically significant decrease in the prevalence of uninsurance among mental-illness-related inpatient care. Compared to the comparison group and to the period prior to the law's enactment, the fraction of mental health admissions for young adults paid by Medicaid decreased by 2.6 percentage points (7.6 percent). We find no meaningful impact of the law on Medicare or on other forms of insurance payment (last two columns).

With population estimates of the increase in hospital visits and of the decrease in the rate of uninsurance, we can provide an estimate of the impact of health insurance on the probability of an inpatient visit following the logic used in Anderson, Dobkin, and Gross (2013). We use data from the Survey of Income and Program Participation to get the population estimate of the law's impact on the rate of uninsurance among 23-to-25-year-olds. Using the same methods as those used in Akosa Antwi, Moriya, and Simon (2013), we find this estimate to be a 4.1-percentage-point reduction. Dividing our 3.5-percent increase in use of inpatient care by this number implies that gaining insurance increases the probability of an inpatient visit by 85.4 percent. Our point estimate is larger than but within the 95-percent confidence interval of the 77 percent estimated by Anderson, Dobkin, and Gross (2013).

Impact of the ACA Young Adult Mandate on the Treatment Intensity of Inpatient Visits

We study treatment intensity using a DD regression model that is similar to Equation [1] except that the unit of observation for this model is at the individual level rather than the hospital-quarter level. In the Massachusetts context, Kolstad and Kowlaski (2012) argue that if health insurance expansion alters observable characteristics of patients who seek care, then including these characteristics in a regression framework would blunt any estimated impact of the reform. As a result they estimate models with and without controlling for patient demographics and clinical characteristics and consider the model without patient characteristics as the preferred specification. We do the same in our evaluation of the impact of the mandate on treatment intensity. For models in which we include patient characteristics, we include a new vector Z_{ighst} not present in Equation [1]; this vector contains patient demographics and clinical indicators such as age, gender, race/ethnicity, whether the patient was admitted on the weekend, Charlson Index, the number of diagnosis codes (up to nine codes), an indicator for each of 27 comorbidity measures calculated using the HCUP Comorbidity Software, and indicator variables for Major Diagnostic Categories (MDCs). We estimate our continuous outcomes with ordinary least squares and our binary outcomes with a linear probability model.

As with our study of the number of hospitalizations, we consider here a sample of non-pregnancy-related inpatient visits (Table 4) and visits related to mental illness (Table 5) for studying the intensive margin. Starting in columns 1 and 2 of Table 4, we find that for our preferred specification there is no statistically significant effect of the law on the levels and log transformation of length of stay. We do find a statistically significant effect of the law for levels

and log transformation with the inclusion of patient demographics and characteristics. Those coefficients translate to 1.7-percent and 0.9-percent decreases in our levels and log specifications, respectively.

We also find that the law did not have a statistically significant impact on the number of procedures performed on average on young adults after implementation for our preferred specification. We do find suggestive evidence of an increase in the number of procedures during the enactment phase. This estimate is significant at the 5-percent level for our levels specification and at the 10-percent level for our log specification. Our results for total charges are sensitive to the inclusion of patient characteristics and functional form assumption. For our preferred specification, our estimate using levels is significant at the 5-percent level, while the log specification is significant at the 10-percent level for the enactment period. During the period between passage and implementation, we find that total charges increased by 2.3 percent for our levels regression and by 1.3 percent for our log specification. Our levels and log specifications with and without risk-adjustment variables all show a decrease in hospital charges following the law's implementation. All of these estimates except for log specification without risk-adjustment variables are, however, not significant at conventional levels.

In Table 5, we present the impact of the ACA mandate on intensity of mental-health-related inpatient visits. We estimate negative coefficients for the enactment and implementation phases of the law for both our levels and log specifications with and without patient characteristics for length of stay. Our preferred specification is statistically significant at the 10-

percent level for log of length of stay during the implementation period. This result is not robust but suggests a decrease in length of stay for young adults. For all our other variables, we do not find results consistent with any impact of the law on young adults relative to older adults. Overall we find a small and mainly statistically insignificant effect of the young adult mandate on the treatment intensity of young adults. These estimates, however, are sensitive to our function form assumption and therefore not robust.

VII. Heterogeneous Effect of the Mandate

A number of states had already extended private health insurance to young adults before the ACA enactment. Even though these expansions had additional eligibility rules based on residency and on student and marital status, some young adults gained access to private health insurance before the ACA became law (Levine, McKnight, and Heep, 2012). Thus, our main estimation, which does not account for this earlier eligibility, might underestimate the impact of the mandate, since we are classifying some young adults who already had access to health insurance as part of our treatment group. To investigate this possibility, we estimate an alternative model in which we account for states that had already passed young adult health insurance extension laws. We include a dummy variable for the passage of a state law as well as an interaction of our treatment group dummy variable and the passage of a state law to isolate the state and federal law effects. We present our analysis in Table 6 for our non-pregnancy diagnosis and mental illness samples. We find estimates of our enactment and implementation effects that

are similar to our specification that does not account for state laws (Table 2). Our coefficient that measures the effect of the state law is not significant for any of our outcomes of interest except for number of admission through the ER, in which we find a negative and statistically significant effect of state laws. We also find similar estimates for the two models when we consider our sample of young adult hospitalizations for mental illness care. Here, we find that state laws also increased the rate of hospitalizations covered by private health insurance among young adults who seek care for a mental illness.

We expect that availability of parental insurance affects insurance coverage and inpatient care use for only those whose parents have access to private health insurance. Since access to parental ESI is associated with higher-income households, we explore the impact of the mandate by median zip code income quartiles of young adults on the assumption that even if young adult children of higher-income households no longer live in the same household as their parents, they are more likely to live in higher-income zip codes than other young adults.[4] We thus expect a larger impact of the mandate on young adults who reside in the higher median income quartiles. We estimate the impact of the law on non-pregnancy and mental illness admissions on separate subsamples of young adults who reside in the top three median income zip codes and those who reside in the bottom median income zip code. We report our results in Table 7.

For our main sample, we find a larger point estimate of the impact of the law for those who reside in zip codes in the top three median income quartiles compared to the estimate for

[4] The median income quartiles are defined using these categories: $1-$38,999, $39,000-$47,999, $48,000-62,999, and $63,000 or more.

those who reside in zip codes in the bottom median income quartile. The point estimate is statistically significant only among those who reside in the higher income group. We find that there is no significant change in the number of admissions that originated from the ER, however direct hospital admissions increased by 4.3 percent for young adults who reside in high median income zip codes.

Although we choose treatment and control groups that are fairly close in age (23-25-year-olds vs. 27-29-year-olds), an even closer comparison would be to evaluate the impact of the mandate on adults just below and above the age cutoff of 26. Specifically, we evaluate the impact of the mandate by comparing 25-year-olds to 27-year-olds. These two age groups are likely the most similar on observable characteristics as they are separated by the age cutoff of 26. In Table 8, we find estimates that are consistent with our main results in Table 2. Compared to 27-year-olds and relative to the period before the law, among 25-year-olds the number of admissions increased by 5.3 percent; admissions through the ER increased by 4.2 percent, and admissions that did not originate from the ER increased by 7.3 percent.

VIII. Validity of Study Design

Estimating the impact of policies by comparisons between treatment and controls groups involve strong assumptions regarding the validity of the study design. We identify the impact of the ACA young adult mandate on hospitalizations by comparing the count of admissions by our treatment and control groups before and after the law. An implicit assumption we make for this

type of comparison is that the relative cohort size of the two groups does not change during our sample period. However, an alternative explanation for the 3.5-percent increase in the number of non-pregnancy-related visits we estimate could be that during our sample period, the population of 23-to-25-year-olds increased faster than the population of 25-to-29-year-olds did. Such an increase could mean a higher number of hospital visits by our treatment group compared to our treatment group. We verify that this is not the case with Census Bureau data on the population of our treatment and control groups from 2007 to 2011. Our treatment and comparison cohorts increased by 1.9 percent and 5.2 percent, respectively, during our sample period. This suggests a higher likelihood of a hospital visit by our comparison group rather than by our control group. Thus, the population growth biases our results toward zero.

One approach to increasing confidence in our ability to use those older than 26 to capture trends in hospitalizations that would have affected younger adults had the policy not occurred would be to examine the trends of our outcomes of interest for our treatment and control groups prior to the law's implementation. If we were to find that hospitalization rates increased for the treatment group even prior to the policy, this would suggest that the impact we estimate could be due to the difference in existing trends between our two groups. We first present visual evidence of pre-trends of main analysis in Figures 1, 2, 3, and 4. In Figure 1, we show trends for number of visits for our treatment and control groups. The vertical lines represent the passage of the law: the implementation of the law in September 2010 and the start of 2011, when most new health insurance plans start. We plot graphs for overall visits, visits through an ER, and visits not

through an ER for our main sample and sample with mental illness diagnosis. The plots are quarterly unconditional means of the aforementioned variables. For all non-pregnancy-related visits and visits that originated from the ER or did not originate from the ER (top row), we observe differences between the treatment and control groups; however none of the figures show strong pre-trends that could explain the impact we estimate. The bottom row of graphs for mental illness admissions shows a sharp increase in the number of admissions through an ER for targeted young adults after the law's implementation. In the other two plots we do not see diverging trends after implementation.

In Figure 2, we examine trends by health insurance status. In the first row of plots, visual inspection of the first graph shows that the two groups follow a similar trend before the law, with a sharp increase in the share of visits paid by private insurance after the implementation. The second plot shows the trend for the uninsured. Again, the two groups follow a similar trend before the law, with a sharp post-law decrease in the share of visits accounted for by uninsured young adults who are targeted by the law. In Figures 3 and 4, we show trends for our treatment intensity variables. These plots show no strong pre-trends and no sharp changes post-law.

We formalize our trend tests by estimating regressions with our outcomes of interest as left-hand-side variables. The right-hand-side variables for this regression include the same control variables as our main model, described above, except that the key variable of interest is an interaction between the linear time trend and the treatment group dummy instead of the usual difference-in-difference variables. We are interested in testing whether the treatment and control

groups have different pre-trends. The results of this estimation are presented in Appendix Tables 1 through 3. In Appendix Table 1, which covers non-pregnancy-related admissions and mental illness admissions, we show that trends are not statistically significantly different in the period prior to the ACA, except for a marginally statistically significant result on number of non-pregnancy-related admissions not through the ER. In Appendix Tables 2 and 3, we report our pre-reform trend test for insurance results (both all and mental illness care) and intensity of treatment for both types of care. For all of our outcomes, our treatment and control groups follow similar pre-trends; there is just one case in which a marginally statistically significant result occurs. Thus, these pre-trend tests indicate that it is unlikely that pre-reform trends alone could explain our results.

We estimate several additional models in which we assume falsely that the reform took place in quarters prior to March 2010, using data from the period before the ACA enactment. For each of the 11 quarters between January 2007 and March 2010, we re-estimate Equation [1] assuming a placebo date for the ACA law and create a distribution of the results from the replications. We perform this test for both non-pregnancy-related and mental illness admissions, as well as for our subsample analysis of young adults who reside in high-income zip codes and for the analysis of 25- vs. 27-year-olds.

We first examine the mean and standard deviation of the estimates reported in Appendix Table 4 relative to the values obtained in Tables 2, and 3 for our sample of non-pregnancy and mental-health-related admissions, for quantity of care as well as its insurance composition. For

our sample of non-pregnancy admissions, four specifications out of eight have two or more placebo results that are statistically significant at the 10-percent-or-smaller level. The largest number of placebo results that are statistically significant at the 1-percent level occurs for the number of admissions not through ER. We note that the coefficients of placebo laws are smaller than those we report in our actual analyses in Tables 2 and 3. Among the mental health illness categories, only two out of 88 possible estimates are statistically significantly different from zero: one at the 5-percent level and the other at the 10-percent level. Since we would expect that randomized placebo laws will be significant at the 5-percent level roughly 5 out of 100 times, this is not surprising.

In Appendix Tables 5, we conduct our placebo tests using a sample of young adults from high-median-income zip codes as well as a sample of 25- and 27-year-olds. We find that the placebo results are hardly ever statistically significant. In Appendix Tables 6 and 7, we show that the effects of placebo laws are mostly insignificant in the analysis of treatment intensity except for the effect on log of total charges, which is significant four out of eleven times when risk-adjusted variables are included in our sample of non-pregnancy-related admissions. In summary, the validity tests in this section confirm that our study design is reasonable and that our estimates likely capture the effects of the mandate rather than the difference in trends between the treatment and control groups.

IX. Discussion and Conclusion

We present the first estimates of the impact of the ACA health insurance expansion to young adults on access and use of medical care using a near nationally representative database of inpatient hospital visits. We examine the population of 23-25-year-olds, for whom access to parental insurance coverage was especially poor prior to the ACA. Compared to another group of individuals who are close in age but excluded from the law (27-29-year-olds), young adults who benefitted from the mandate increased their overall number of non-pregnancy-related inpatient visits by 3.5 percent. In our exploration of the impact of the mandate on mental health visits, an important component of inpatient care use by young adults, we find that young adults increased their visits by 5.5 percent. We also find strong evidence that the law achieved its intended purpose of decreasing uninsurance among young adults needing care. The fraction of young adult hospitalizations that is uninsured decreased by 10.8 percent due to the law.

Health insurance coverage could provide young adults with a regular source of medical care through outpatient visits that could potentially limit their use for the ER. Our analysis of the effect of the law on how young adults enter the health care system shows that young adults increase their admissions originating both from the ER and away from the ER. They were also much more likely than older adults to use the ER for mental health admissions. Our analysis of the impact of the mandate on the intensity of medical care shows no consistent or significant change in how long young adults stay in a hospital, the number of procedures that are performed on them, or on hospital charges.

Our findings contribute to the literature on the effect of health insurance coverage on access and medical care utilization. The general consensus in the literature is that health insurance increases the use of medical services. Our findings are consistent with this hypothesis as we find that young adults increased their overall use of inpatient services in response to gaining access to health insurance. We note that the health insurance expansion we study here is much smaller than other large-scale efforts such as the near-universal health insurance expansion in Massachusetts and the Medicaid expansion in Oregon. In addition, we study the short-run effect of the ACA health insurance expansion on young adults, leaving for future work the examination of the long-run effects. Also, since we use hospital discharge data that do not contain individual identifiers, we are unable to distinguish whether the increase in inpatient visits comes from increased frequency of visits by the same patients or an increase in visits by new patients. Future work using household- or individual-level surveys will help us better understand the mechanism behind the increase in inpatient care use found in this study, as well as to understand the effects on the use of care in other settings.

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Table 1: Summary Statistics of Treatment and Control Groups

	All observations	Age, 23-25	Age, 27-29
Demographic characteristics			
Age	26.2	24.0	28.0
Indicator: male	0.484	0.493	0.475
Indicator: white	0.611	0.608	0.612
Indicator: African-American	0.22	0.221	0.22
Indicator: Hispanic	0.102	0.103	0.102
Clinical Characteristics			
Indicator: mental illness	0.162	0.171	0.154
Number of diagnosis codes	5.19	5.05	5.31
Indicator: admitted through ER	0.641	0.653	0.630
Indicator: admitted on weekend	0.228	0.236	0.222
Utilization measures			
Length of stay (LOS)	4.39	4.43	4.36
Log of LOS	1.40	1.40	1.40
Number of procedure codes	1.14	1.11	1.16
Log of number of procedure codes	0.561	0.548	0.572
Total charges	26,065	26,064	26,065
Log of total charges	9.62	9.61	9.64
Health insurance status			
Indicator: covered by private insurance	0.355	0.352	0.358
Indicator: uninsured	0.208	0.220	0.198
Indicator: covered by Medicaid	0.281	0.286	0.277
Indicator: covered by Medicare	0.083	0.067	0.097
Indicator: covered by other insurance	0.067	0.070	0.064
Number of observations	794,392	371,080	423,312

Note: Sample estimates from the NIS data, 2007-2011, using data of non-pregnancy related admissions of young adults aged from 23-29, except for the removal of 26 year olds who are in neither control nor treatment. The data from California, Maine and Texas are excluded because precise information on age was not available. Observations in which length of stay exceeds 90 days are excluded. The mean for the race categories is calculated using observations in which race\ethnicity variable is available in the data. Not all states provide information on race\ethnicity for all years in the NIS dataset.

Table 2: Effect of Mandate on the Number and Sources of Admissions

	Non-pregnancy Related			Mental Illness		
	Number of all admissions	Number of admissions through ER	Number of admissions not through ER	Number of admissions	Number of admissions through ER	Number of admissions not through ER
ACA Enactment Effect	0.610 **	0.241	0.370 **	0.012	-0.175	0.187
(2010 Q2-Q3)	(0.266)	(0.200)	(0.157)	(0.282)	(0.219)	(0.172)
ACA Implementation Effect	0.820 ***	0.444 **	0.376 **	0.629 **	0.486 **	0.143
(2010 Q4-)	(0.270)	(0.197)	(0.150)	(0.287)	(0.203)	(0.178)
Dependent Variable Means						
Treatment, before ACA enactment	23.7	15.5	8.2	11.51	6.87	4.63
Control, before	27.2	17.1	10.1	11.90	7.03	4.87
Treatment, after ACA implementation	25.0	16.5	8.6	12.89	7.68	5.21
Control, after	28.1	17.9	10.2	12.82	7.39	5.43

Notes: (1) Number of observations is 30,640 in the first three columns and 10,588 in the last three columns. (2) Quarterly hospital-group (control vs. treatment group) level variables are calculated using the non-pregnancy related admissions (for columns 1-3) and mental illness admissions (for columns 4-6) of young adults aged from 23-29, except for the removal of 26 year olds who are in neither control nor treatment. (3) Cells of the table contain: coefficients and standard errors in parentheses. Coefficients in the first row are from the interaction of a dummy variable for treatment group (23-25 years old) and a dummy variable for the period after ACA enactment but before implementation (the second and third quarters of 2010); coefficients in the second row are from the interaction of a dummy variable for treatment group and a dummy variable for the period after ACA implementation (the fourth quarter of 2010 and onwards). (4) Standard errors are clustered at the hospital level. (5) Data: The NIS data from 2007 to 2011. The data from California, Maine and Texas are excluded because precise information on age was not available. Observations in which length of stay exceeds 90 days are excluded. (6) Dependent variables— column 1: number of all non-pregnancy related admissions; column 2: number of non-pregnancy related admissions originated from the ER; column 3: number of non-pregnancy-related admissions that did not originate from the ER; column 4: number of total mental illness admissions; column 5: number of mental illness admissions originated from the ER; and column 6: number of mental illness admissions that did not originate from the ER. (7) Other regressors are an indicator for treatment group, year fixed effects, quarterly fixed effects, state-specific linear time trend, hospital-specific fixed effects, quarterly state-level unemployment rate, interaction of unemployment and an indicator for treatment group. (8) Means of dependent variables are obtained for treatment and control groups before ACA enactment (2007 Q1- 2010 Q1) and after ACA implementation (2010 Q4 and onward). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 3: Fraction of Admissions Insured: Non-Pregnancy Related Admissions and Mental Illness Admissions

	Non-pregnancy related Admissions				
	Private Insurance	Uninsured	Medicaid	Medicare	Other Insurance
ACA Enactment Effect	0.017 **	-0.006	-0.001	-0.004	-0.006
(2010 Q2-Q3)	(0.008)	(0.007)	(0.008)	(0.005)	(0.004)
ACA Implementation Effect	0.040 ***	-0.025 ***	-0.012 *	-0.002	-0.004
(2010 Q4-)	(0.007)	(0.006)	(0.007)	(0.004)	(0.003)
Dependent Variable Means					
Treatment, before ACA enactment	0.366	0.231	0.269	0.060	0.067
Control, before	0.395	0.196	0.255	0.087	0.061
Treatment, after ACA implementation	0.360	0.218	0.286	0.063	0.064
Control, after	0.339	0.207	0.289	0.095	0.062
	Mental Illness Admissions				
	Private Insurance	Uninsured	Medicaid	Medicare	Other Insurance
ACA Enactment Effect	0.004	-0.002	-0.007	0.004	0.002
(2010 Q2-Q3)	(0.013)	(0.011)	(0.012)	(0.010)	(0.006)
ACA Implementation Effect	0.041 ***	-0.011	-0.026 **	0.002	-0.006
(2010 Q4-)	(0.011)	(0.010)	(0.011)	(0.009)	(0.006)
Dependent Variable Means					
Treatment, before ACA enactment	0.28	0.19	0.34	0.11	0.07
Control, before	0.28	0.17	0.31	0.17	0.06
Treatment, after ACA implementation	0.29	0.19	0.34	0.10	0.07
Control, after	0.23	0.19	0.35	0.15	0.07

Notes: (1) Number of observations is 28,478 in the first set of rows and 9,509 in the second set of rows. (2) Dependent variables—column 1: ratio of admissions covered by private health insurance; column 2: ratio of uninsured admissions; column 3: ratio of admissions covered by Medicaid; column 4: ratio of admissions covered by Medicare; and column 5: ratio of admissions covered by other insurance. (3) See Notes (2)-(5), (7) and (8) under Table 2. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 4: Effect of Mandate on Intensity of Treatment for Non-pregnancy Related Admissions

	LOS	Log of LOS	Number of procedures	Log of number of procedures	Total charges	Log of total charges
Risk-adjustment variables (including individuals' demographic characteristics) are excluded.						
ACA Enactment Effect (2010 Q2-Q3)	0.0211	0.0014	0.0255 **	0.0081 *	845.9 **	0.0125 *
	(0.0469)	(0.0058)	(0.0115)	(0.0043)	(402.7)	(0.0068)
ACA Implementation Effect (2010 Q4-)	-0.0568	-0.0063	-0.0026	-0.0022	-425.7	-0.0096 *
	(0.0387)	(0.0046)	(0.0092)	(0.0038)	(296.6)	(0.0057)
Risk-adjustment variables are included.						
ACA Enactment Effect (2010 Q2-Q3)	-0.0015	-0.0006	0.0115	0.0032	554.2	0.0064
	(0.0411)	(0.0049)	(0.0096)	(0.0037)	(346.2)	(0.0056)
ACA Implementation Effect (2010 Q4-)	-0.0772 **	-0.0087 **	0.0023	0.0002	-351.3	-0.0067
	(0.0322)	(0.0038)	(0.0079)	(0.0032)	(261.1)	(0.0048)
Dependent Variable Means						
Treatment, before ACA enactment	4.43	1.40	1.09	0.543	24,402	9.56
Control, before	4.33	1.40	1.15	0.569	24,333	9.58
Treatment, after ACA implementation	4.36	1.39	1.10	0.544	29,163	9.72
Control, after	4.35	1.40	1.15	0.569	29,648	9.75

Notes: (1) Number of observations is 794,392. (2) Observations are non-pregnancy-related admissions of young adults aged from 23-29, except for the removal of 26 year olds who are in neither control nor treatment. Observations in which length of stay exceeds 90 days are excluded. (3) Dependent variables—column 1: length of stay; column 2: log of length of stay; column 3: number of procedure codes; column 4: log of number of procedure codes; column 5: total charges; and column 6: log of total charges. (4) Other regressors - an indicator for the period after ACA enactment but before implementation, an indicator for the period after ACA implementation, an indicator for treatment group, year-specific fixed effects, quarter-specific fixed effects, quarterly state-specific linear time trends, hospital-specific fixed effects, quarterly state-level unemployment rate, and interaction of unemployment rate and an indicator for treatment group are included in the both sets of regressions. Risk-adjusted variables that are included in the second set of the regressions are an indicator for each year of age, gender, race/ethnicity, an indicator for admission occurred on the weekend, Charlson Index, the number of diagnosis codes (up to nine codes), an indicator for each of 27 comorbidity measures obtained by the HCUP Comorbidity Software, and indicator variables for major diagnostic categories. (5) See Notes (3)-(5) and (8) under Table 2. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 5: Effect of Mandate on Intensity of Treatment for Mental Illness Admissions

	LOS	Log of LOS	Number of procedures	Log of number of procedures	Total charges	Log of total charges
Risk-adjustment variables (including individuals' demographic characteristics) are excluded.						
ACA Enactment Effect (2010 Q2-Q3)	-0.0447	-0.0067	0.0027	-0.0014	-82.1	-0.0045
	(0.132)	(0.0128)	(0.0085)	(0.0042)	(275.1)	(0.0128)
ACA Implementation Effect (2010 Q4-)	-0.151	-0.0162 *	0.0038	0.0021	-234.7	-0.0066
	(0.112)	(0.0098)	(0.0064)	(0.0033)	(220.9)	(0.0108)
Risk-adjustment variables are included.						
ACA Enactment Effect (2010 Q2-Q3)	-0.0504	-0.0071	0.0026	-0.0016	-84.5	-0.0047
	(0.125)	(0.0119)	(0.0083)	(0.0040)	(266.5)	(0.0118)
ACA Implementation Effect (2010 Q4-)	-0.1820 *	-0.0193 **	0.0036	0.0022	-295.3	-0.0097
	(0.110)	(0.0095)	(0.0062)	(0.0032)	(218.6)	(0.0104)
Dependent Variable Means						
Treatment, before ACA enactment	6.69	1.75	0.25	0.14	13,510	9.09
Control, before	6.63	1.76	0.27	0.15	13,399	9.10
Treatment, after ACA implementation	6.55	1.74	0.28	0.15	15,547	9.25
Control, after	6.64	1.76	0.27	0.15	15,568	9.26

Notes: (1) Number of observations is 128,310. (2) Observations are mental illness admissions of young adults aged from 23-29, except for the removal of 26 year olds who are in neither control nor treatment. Observations in which length of stay exceeds 90 days are excluded. (3) Other regressors are the same as those listed in Note (4) under Table 4 except that we include an indicator variable for DRGs instead of an indicator variable for major diagnostic categories. (4) See Notes (3)-(5) and (8) under Table 2 and Note (3) under Table 4. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 6: Effect of the State Laws on the Number, Sources of Admissions and Fraction of Admissions Insured

	Non-pregnancy Related Admissions								
	Number of admissions	Number of admissions through ER	Number of admissions not through ER	Rate of Private Insurance	Rate of Uninsured	Rate of Medicaid	Rate of Medicare	Rate of Other Insurance
State Law Effect	-0.340	-0.352 **	0.012	0.012 *	-0.005	-0.005	-0.001	-0.001
	(0.266)	(0.178)	(0.151)	(0.007)	(0.005)	(0.006)	(0.004)	(0.003)
ACA Enactment Effect	0.676 **	0.308	0.367 **	0.014 *	-0.005	0.000	-0.004	-0.006
(2010 Q2-Q3)	(0.267)	(0.202)	(0.154)	(0.009)	(0.008)	(0.008)	(0.005)	(0.004)
ACA Implementation Effect	0.904 ***	0.531 ***	0.373 **	0.037 ***	-0.024 ***	-0.011	-0.001	-0.004
(2010 Q4-)	(0.275)	(0.202)	(0.151)	(0.007)	(0.006)	(0.007)	(0.004)	(0.003)
	Mental Illness Admissions							
	Number of admissions	Number of admissions through ER	Number of admissions not through ER	Rate of Private Insurance	Rate of Uninsured	Rate of Medicaid	Rate of Medicare	Rate of Other Insurance
State Law Effect	0.021	0.150	-0.130	0.005	0.002	-0.005	0.004	-0.006
	(0.228)	(0.156)	(0.133)	(0.010)	(0.008)	(0.011)	(0.009)	(0.006)
ACA Enactment Effect	0.004	-0.210	0.214	0.003	-0.002	-0.006	0.003	0.003
(2010 Q2-Q3)	(0.288)	(0.221)	(0.176)	(0.013)	(0.011)	(0.013)	(0.010)	(0.006)
ACA Implementation Effect	0.621 **	0.442 **	0.178	0.039 ***	-0.011	-0.024 **	0.001	-0.004
(2010 Q4-)	(0.299)	(0.208)	(0.184)	(0.012)	(0.010)	(0.012)	(0.009)	(0.006)

Notes: (1) Number of observations is 30,640 in the first set of rows and 10,588 in the second set of rows. (2) Cells of the table contain: coefficients, and standard errors in parentheses. Coefficients in the first row are from the interaction of a dummy variable for treatment group (23-25 years old) and a dummy variable for state law implementation; coefficients in the second row are from the interaction of a dummy variable for treatment group and the period after ACA enactment but before implementation (the second and third quarters of 2010); coefficients in the third row are from the interaction of a dummy variable for treatment group and a dummy variable for the period after ACA implementation (the fourth quarter of 2010 and onwards). (3) Dependent variables— column 1: number of total admissions; column 2: number of admissions originated from the ER; column 3: number of admissions that did not originate from the ER; column 4: rate of admissions covered by private health insurance; column 5: rate of uninsured admissions; column 6: rate of admissions covered by Medicaid; column 7: rate of admissions covered by Medicare; and column 8: rate of admissions covered by other insurance. (4) Other regressors are a dummy variable for state law implementation in addition to the regressors listed in Note (7) under Table 2. (5) See Notes (2), (4), (5) under Table 2. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 7: Effect of the Mandate on the Number and Sources of Non-pregnancy Related Admissions by Zip-Code Median Household Income

	Top Three Zip-Code Income Quartiles			Bottom Zip-Code Income Quartile		
	Number of admissions	Number of admissions through ER	Number of admissions not through ER	Number of admissions	Number of admissions through ER	Number of admissions not through ER
ACA Enactment Effect	0.392	0.174	0.217	0.173	0.004	0.168
(2010 Q2-Q3)	(0.243)	(0.177)	(0.146)	(0.167)	(0.131)	(0.103)
ACA Implementation Effect	0.501 **	0.229	0.272 **	0.296	0.200	0.096
(2010 Q4-)	(0.223)	(0.167)	(0.126)	(0.181)	(0.126)	(0.102)
Dependent Variable Means						
Treatment, before ACA enactment	18.04	11.67	6.37	9.47	6.27	3.21
Control, before	20.45	12.69	7.76	11.08	7.04	4.04
Treatment, after ACA implementation	19.25	12.47	6.78	10.32	6.93	3.38
Control, after	21.39	13.51	7.88	11.88	7.66	4.22

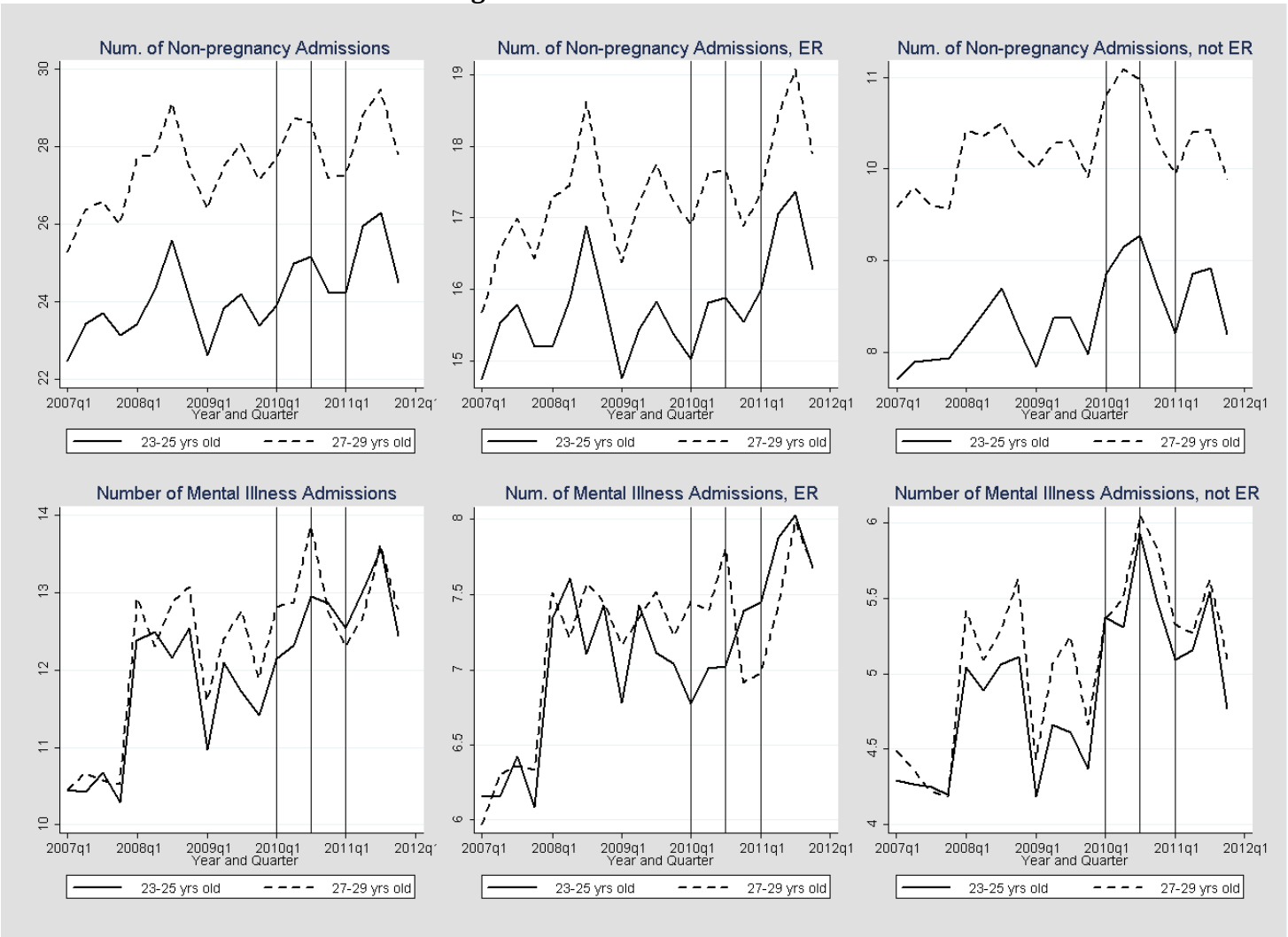
Notes: (1) Number of observations is 26,238 in the first three columns and 24,126 in the last three columns. (2) Quarterly hospital-group (control vs treatment group) level variables are calculated using the non-pregnancy related admissions of young adults (aged from 23-29, except for the removal of 26 year olds who are in neither control nor treatment) who reside in the zip codes that are in the top three quartiles of median household income (for the first three columns) or in bottom quartile of median household income (for the last three columns). (3) Dependent variables— columns 1 and 4: number of all non-pregnancy related admissions; columns 2 and 5: number of non-pregnancy related admissions originated from the ER; and columns 3 and 6: number of non-pregnancy-related admissions that did not originate from the ER. (4) See Notes (3)-(5) and (7)-(8) under Table 2. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 8: Effect of the Mandate on the Number and Sources of Non-pregnancy Related Admissions among 25 and 27 years old

	Number of admissions		Number of admissions through ER		Number of admissions not through ER	
ACA Enactment Effect	0.215		0.071		0.144	
(2010 Q2-Q3)	(0.152)		(0.112)		(0.094)	
ACA Implementation Effect	0.488	***	0.252	**	0.236	***
(2010 Q4-)	(0.126)		(0.101)		(0.068)	
Dependent Variable Means						
Treatment, before ACA enactment	9.29		6.05		3.24	
Control, before	10.04		6.36		3.68	
Treatment, after ACA implementation	9.74		6.39		3.35	
Control, after	10.07		6.48		3.58	

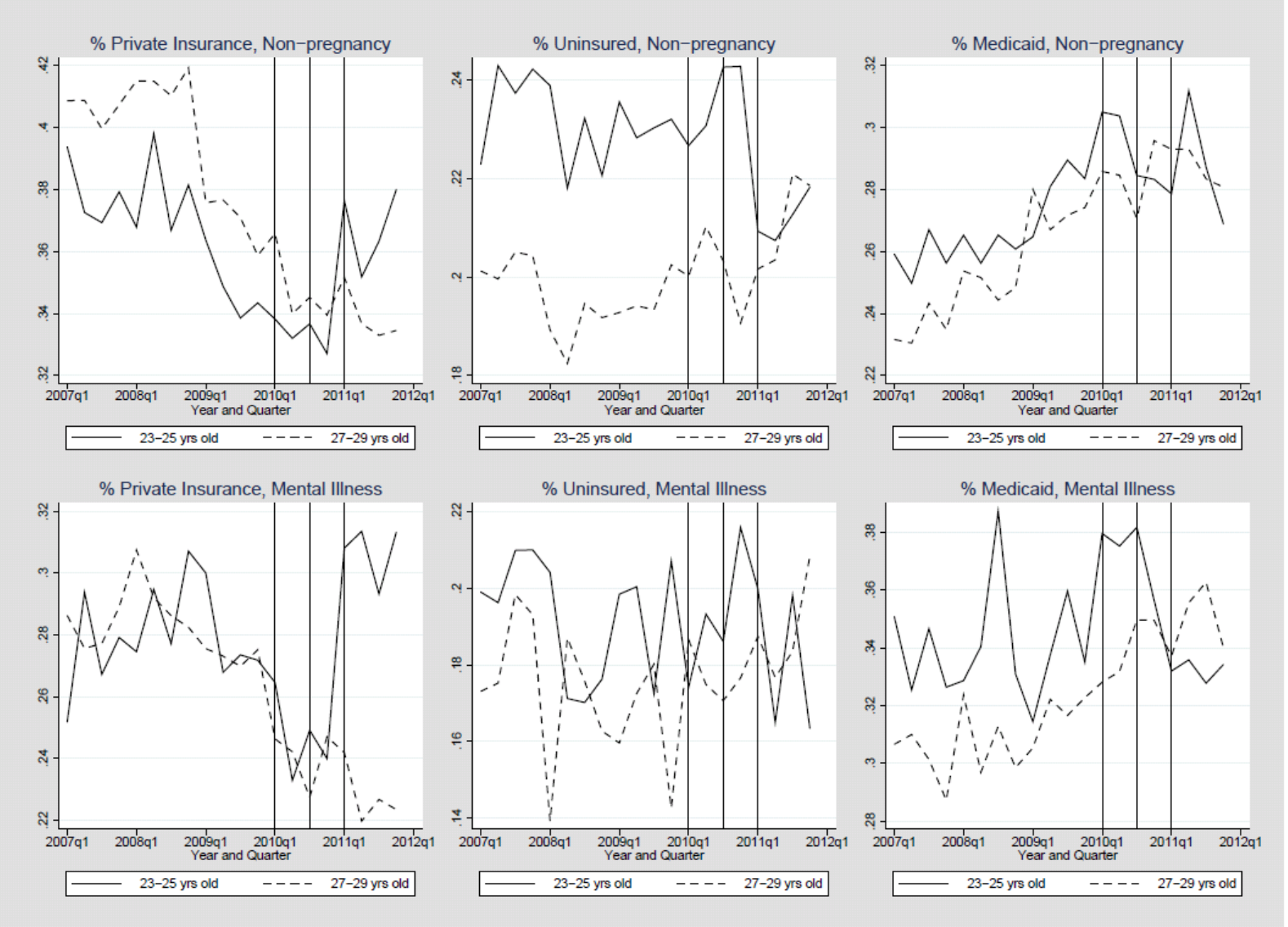
Notes: (1) Number of observations is 26,862. (2) Quarterly hospital-group (control vs treatment group) level variables are calculated using the non-pregnancy related admissions of young adults aged 25 and 27. (2) Dependent variables— column 1: number of all non-pregnancy related admissions; column 2: number of non-pregnancy related admissions originated from the ER; and column 3: number of non-pregnancy-related admissions that did not originate from the ER. (3) See Notes (3)-(5) and (7)-(8) under Table 2. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Figure 1: Number of Admissions



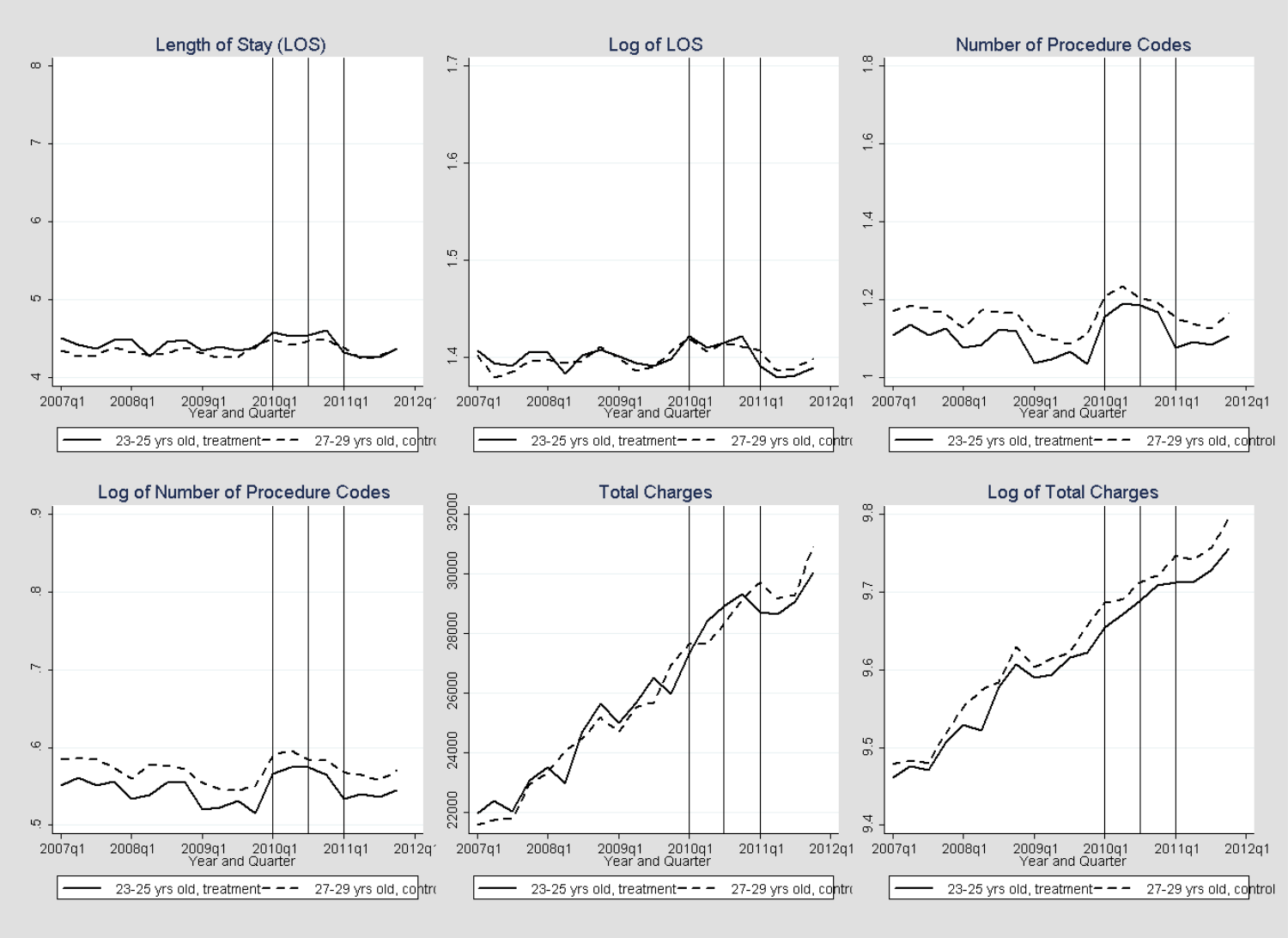
Note: Sample estimates from the NIS data, using data from 2007 to 2011. The first vertical line indicates the first quarter of 2010 when the ACA was passed, the second vertical line indicates the third quarter of 2010 when the dependent coverage mandate was implemented, and the third vertical line indicates the first quarter of 2011 when most new insurance plan years start after the implementation of the mandate.

Figure 2: Percent of Admissions Insured



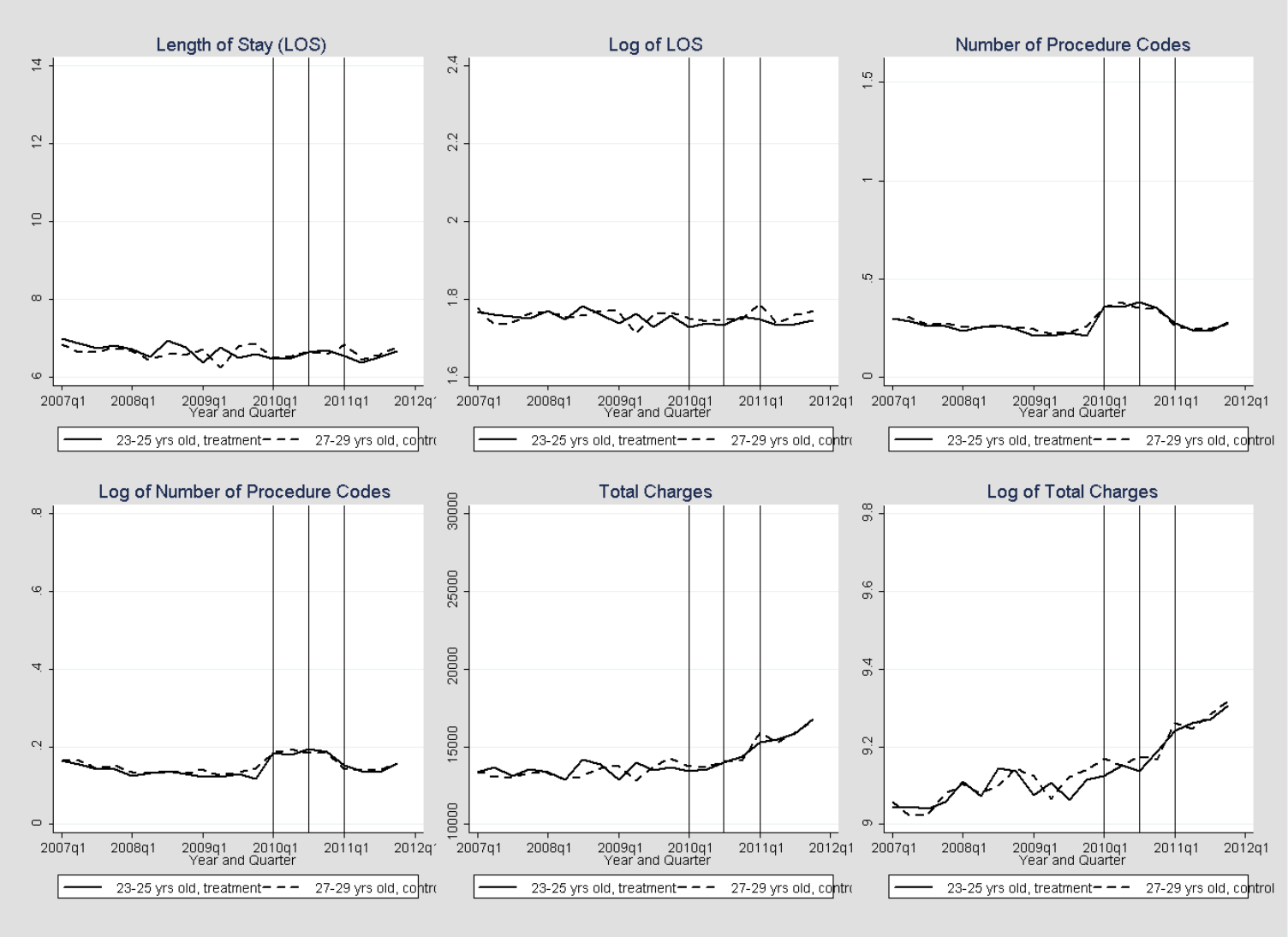
Note: See Note under Figure 1.

Figure 3: Treatment Intensity of Non-pregnancy Related Admissions



Note: See Note under Figure 1.

Figure 4: Treatment Intensity of Mental Illness Admissions



Note: See Note under Figure 1.

Appendix

Appendix Table 1: Test for Equality of Pre-Reform Trends for Number of Admissions

	Non-pregnancy Related Admissions			Mental Illness Admissions		
	Number of all admissions	Number of admissions through ER	Number of admissions not through ER	Number of all admissions	Number of admissions through ER	Number of admissions not through ER
Interaction of time trend and a dummy variable for treatment group	0.0977	0.0382	0.0594 *	0.0084	0.0389	-0.0305
	(0.060)	(0.041)	(0.034)	(0.053)	(0.039)	(0.030)

Note: (1) Number of observations is 18,740 for the first three columns and 6,372 for the last three columns. (2) Data: The NIS data for the period from the first quarter of 2007 to the fourth quarter of 2009, which is prior to the passage of the ACA in March 2010. The data from California, Maine and Texas are excluded because precise information on age was not available. Observations in which length of stay exceeds 90 days are excluded. (3) Cells of the table contain: coefficients, and standard errors in parentheses. Coefficients are from the interaction of a dummy variable for treatment group (23-25 years old) and a linear measure for time trend (number of quarters since the first quarter of 2007), which shows whether there was a different time trend for the control vs. the treatment group in the period prior to policy enactment. (4) Dependent variables— column 1: number of total non-pregnancy related admissions; column 2: number of non-pregnancy related admissions originated from the ER; column 3: number of non-pregnancy related admissions that did not originate from the ER; column 4: number of total mental illness admissions; column 5: number of mental illness admissions originated from the ER; and column 6: number of mental illness admissions that did not originate from the ER. (5) Other regressors are a linear time trend, a dummy variable for the treatment group, and all other explanatory variables included in our main specification. (6) See Notes (2) and (4) under Table 2.

Appendix Table 2: Test for Equality of Pre-Reform Trends for Number Admissions by Health Insurance

	Non-pregnancy Related Admissions				
	Private Insurance	Uninsured	Medicaid	Medicare	Other Insurance
Interaction of time trend and a dummy variable for treatment group	0.000 (0.002)	0.001 (0.002)	-0.003 (0.002)	0.000 (0.001)	0.002 * (0.001)
	Mental Illness Admissions				
	Private Insurance	Uninsured	Medicaid	Medicare	Other Insurance
Interaction of time trend and a dummy variable for treatment group	0.001 (0.003)	0.000 (0.002)	-0.001 (0.003)	0.000 (0.003)	-0.001 (0.002)

Note: (1) Number of observations is 18,740 for the first set of rows and 6,372 for the second set of rows. (2) Dependent variables— column 1: rate of admissions covered by private health insurance; column 2: rate of uninsured admissions; column 3: rate of admissions covered by Medicaid; column 4: rate of admissions covered by Medicare; and column 5: rate of admissions covered by other insurance. (3) See Notes (2) and (4) under Table 2, Note (2) under Table 3, and Notes (2), (3) and (5) under Appendix Table 1.

Appendix Table 3: Test for Equality of Pre-Reform Trends for Intensity of Treatment

	Non-pregnancy Related Admissions					
	LOS	Log of LOS	Number of procedures	Log of number of procedures	Total charges	Log of total charges
Risk-adjustment variables (including individuals' demographic characteristics) are excluded.						
Interaction of time trend and a dummy	-0.0082	-0.0012	0.0013	0.0007	20.4	0.0003
variable for treatment group	(0.0097)	(0.0011)	(0.0026)	(0.0010)	(69.0)	(0.0014)
Risk-adjustment variables are included.						
Interaction of time trend and a dummy	-0.0058	-0.0009	-0.0004	0.0000	10.2	-0.0005
variable for treatment group	(0.0091)	(0.0010)	(0.0022)	(0.0009)	(65.2)	(0.0012)
	Mental Illness Admissions					
	LOS	Log of LOS	Number of procedures	Log of number of procedures	Total charges	Log of total charges
Risk-adjustment variables (including individuals' demographic characteristics) are excluded.						
Interaction of time trend and a dummy	-0.0333	-0.0024	-0.0014	-0.0008	-30.8	-0.0011
variable for treatment group	(0.0300)	(0.0027)	(0.0021)	(0.0010)	(70.0)	(0.0029)
Risk-adjustment variables are included.						
Interaction of time trend and a dummy	-0.0215	-0.0013	-0.0017	-0.0010	-2.35	0.0003
variable for treatment group	(0.0289)	(0.0025)	(0.0021)	(0.0010)	(68.2)	(0.0027)

Notes: (1) Number of observations is 467,134 in the first set of rows and 74,174 in the second set of rows. (2) See Note (4) under Table 2, Notes (2) and (3) under Table 4, and Notes (2), (3) and (5) under Appendix Table 1.

Appendix Table 4: Effects of Placebo Laws on the Number of Admissions

	Distribution of the coefficients of the placebo laws		Number of coefficient estimates that are significant in the placebo law regressions (out of 11 estimates for each row)			Estimated effects in the main specification			
	Mean	Standard deviation	1% level	5% level	10% level	Enactment effect (2010 Q2-Q3)		Implementation effect (2010 Q4-)	
Non-pregnancy Related Admissions									
Number of total admissions	0.435	0.545	2	2	1	0.610	**	0.820	***
Number of admissions through ER	0.185	0.399	1	1	2	0.241		0.444	**
Number of admissions not through ER	0.250	0.161	4	0	1	0.370	**	0.376	**
Rate of private health insurance	0.000	0.011	0	0	0	0.017	**	0.040	***
Rate of uninsured	0.004	0.006	0	0	0	-0.006		-0.025	***
Rate of Medicaid	-0.010	0.007	2	0	0	-0.001		-0.012	*
Rate of Medicare	0.001	0.006	0	0	0	-0.004		-0.002	
Rate of other insurance	0.005	0.004	0	1	0	-0.006		-0.004	
Average num. of significant coefficients			1.13	0.50	0.50				
Mental Illness Admissions									
Number of total admissions	0.058	0.198	0	0	0	0.012		0.629	**
Number of admissions through ER	0.150	0.186	0	0	1	-0.175		0.486	**
Number of admissions not through ER	-0.092	0.119	0	0	0	0.187		0.143	***
Rate of private health insurance	0.005	0.016	0	0	0	0.004		0.041	***
Rate of uninsured	0.001	0.015	0	1	0	-0.002		-0.011	
Rate of Medicaid	-0.003	0.010	0	0	0	-0.007		-0.026	**
Rate of Medicare	-0.001	0.011	0	0	0	0.004		0.002	
Rate of other insurance	-0.003	0.005	0	0	0	0.002		-0.006	
Average num. of significant coefficients			0	0.13	0.13				

Note: (1) Data: The NIS data for the period from the first quarter of 2007 to the fourth quarter of 2009, which is prior to the passage of the ACA in March 2010. The data from California, Maine and Texas are excluded because precise information on age was not available. (2) We select each possible quarter between the second quarter of 2007 and the fourth quarter of 2009 one at a time. We then estimate the main model using each separate placebo date for defining the "Implement" variable. We show here the means and standard deviations of the coefficients we obtain. (3) The last two columns repeat estimates from Tables 2 and 3 for comparison.

Appendix Table 5: Effects of Placebo Laws on the Number of Non-Pregnancy Related Admissions among Patients from the Zip Codes in the Top Three Quartiles of Median Household Income and among 25 and 27 years old

	Distribution of the coefficients of the placebo laws		Number of coefficient estimates that are significant in the placebo law regressions (out of 11 estimates for each row)			Estimated effects in the main specification		
	Mean	Standard deviation	1% level	5% level	10% level	Enactment effect (2010 Q2-Q3)	Implementation effect (2010 Q4-)	
Patients from Top Three Zip-Code Income Quartiles								
Number of total admissions	-0.029	0.283	0	0	0	0.392	0.501	**
Number of admissions through ER	-0.011	0.254	0	0	0	0.174	0.229	
Number of admissions not through ER	-0.018	0.119	0	0	0	0.217	0.272	**
25 and 27 years old								
Number of total admissions	0.134	0.138	0	0	1	0.215	0.488	***
Number of admissions through ER	0.014	0.080	0	0	0	0.071	0.252	**
Number of admissions not through ER	0.119	0.072	0	1	1	0.144	0.236	***

Note: (1) See Tables (1) and (2) under Appendix Table 4. (2) The last two columns repeat estimates from Tables 7 and 8 for comparison.

Appendix Table 6: Effects of Placebo Laws on Treatment Intensity, Non-Pregnancy Related Admissions

	Distribution of the coefficients of the placebo laws		Number of coefficient estimates that are significant in the placebo law regressions (out of 11 estimates for each row)			Estimated effects in the main specification			
	Mean	Standard deviation	1% level	5% level	10% level	Enactment effect (2010 Q2-Q3)		Implementation effect (2010 Q4-)	
Risk-adjustment variables (including individuals' demographic characteristics) are excluded.									
Length of stay (LOS)	-0.026	0.036	0	0	0	0.021		-0.057	
Log of LOS	-0.003	0.005	0	0	0	0.001		-0.006	
Number of procedures	0.006	0.014	0	0	1	0.026	**	-0.003	
Log of number of procedures	0.002	0.005	0	0	0	0.008	*	-0.002	
Total charges	102.7	532.0	1	0	0	845.9	**	-425.7	
Log of total charges	0.003	0.011	1	0	0	0.013	*	-0.010	*
Risk-adjustment variables are included.									
Length of stay (LOS)	-0.017	0.044	0	0	0	-0.002		-0.077	**
Log of LOS	-0.002	0.006	0	0	1	-0.001		-0.009	**
Number of procedures	0.000	0.013	0	1	0	0.012		0.002	
Log of number of procedures	0.000	0.004	0	0	0	0.003		0.000	
Total charges	78.2	493.2	0	0	0	554.2		-351.3	
Log of total charges	0.000	0.011	1	3	0	0.006		-0.007	

Note: (1) See Tables (1) and (2) under Appendix Table 4. (2) The last two columns repeat estimates from Table 4 for comparison.

Appendix Table 7: Effects of Placebo Laws on Treatment Intensity, Mental Illness Admissions

	Distribution of the coefficients of the placebo laws		Number of coefficient estimates that are significant in the placebo law regressions (out of 11 estimates for each row)			Estimated effects in the main specification		
	Mean	Standard deviation	1% level	5% level	10% level	Enactment effect (2010 Q2-Q3)	Implementation effect (2010 Q4-)	
Risk-adjustment variables (including individuals' demographic characteristics) are excluded.								
Length of stay (LOS)	-0.124	0.137	0	0	1	-0.045	-0.151	
Log of LOS	-0.006	0.014	0	0	0	-0.007	-0.016	*
Number of procedures	-0.006	0.011	1	0	0	0.003	0.004	
Log of number of procedures	-0.003	0.006	1	0	0	-0.001	0.002	
Total charges	-91.3	339.4	0	0	0	-82.1	-234.7	
Log of total charges	-0.001	0.021	1	1	0	-0.004	-0.007	
Risk-adjustment variables are included.								
Length of stay (LOS)	-0.092	0.170	0	0	1	-0.050	-0.182	*
Log of LOS	-0.003	0.013	0	0	0	-0.007	-0.019	**
Number of procedures	-0.007	0.011	0	1	0	0.003	0.004	
Log of number of procedures	-0.004	0.006	1	0	0	-0.002	0.002	
Total charges	-13.2	341.3	0	0	0	-84.5	-295.3	
Log of total charges	0.002	0.020	0	1	0	-0.005	-0.010	

Note: (1) See Tables (1) and (2) under Appendix Table 4. (2) The last two columns repeat estimates from Table 5 for comparison.