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THE EFFECT OF MEDICAID ON ADULT HOSPITALIZATIONS: EVIDENCE FROM TENNESSEE'S MEDICAID CONTRACTION

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ABSTRACT

The 2010 Affordable Care Act (ACA) Medicaid expansions aimed to improve access to care and health status among low-income non-elderly adults. Previous work has established a link between Medicaid coverage expansion and reduced mortality (Sommers, Baicker and Epstein, 2012), but the mechanism of this reduction is not clearly understood. Prior to the ACA, one of the largest policy changes in non-elderly adult Medicaid access was a 2005 contraction through which nearly 170,000 enrollees lost Medicaid coverage in Tennessee. We exploit this change in Medicaid coverage to estimate its causal impact on inpatient hospitalizations. We find evidence that the contraction decreased the share of hospitalizations covered by Medicaid by 21 percent and increased the share uninsured by nearly 61 percent, relative to the pre-reform levels and to other states. We also find that 75 percent of the increase in uninsured hospitalizations originated from emergency department visits, a pattern consistent with losing access to medical homes. However, uninsured hospitalizations increased for both avoidable and unavoidable conditions at the same rate, which does not suggest a lack of preventive care. Although there may be limited symmetry in response to Medicaid expansion and contraction, these findings are also consistent with the substantial decrease in uncompensated care costs in the states that have thus far expanded Medicaid under the ACA. These results also help shed light on the mechanisms by which Medicaid might affect mortality for non-elderly adults.

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1. Introduction

Previous research by Sommers, Baicker and Epstein (2012) has shown that state adult Medicaid expansions led to reductions in mortality among non-elderly (under age 65) adults in expansion states relative to non-expansion states. Although their study estimates that the pre-2010 Medicaid expansions reduced mortality by nearly 6 percent, the mechanism by which this reduction occurs is not clearly understood. Because health insurance is an important determinant of access to care, Medicaid may have a protective effect on health through utilization of medical services. Medicaid may also reduce mortality through other means such as its beneficial effect on financial stress related to affordability and access to health care (Finkelstein et al., 2012; Baicker et al., 2013).

Most of the literature examining the effects of Medicaid has analyzed policies that extend health insurance coverage to low-income children and pregnant women, populations whose experience may not generalize to those targeted by the Medicaid provisions of the Affordable Care Act (ACA) of 2010, specifically non-elderly adults without dependent children. While several papers have examined the impact of Medicaid coverage among non-elderly adults by analyzing the changes in Oregon, Wisconsin and Tennessee, these studies tend to focus on insurance coverage and labor market effects. Little research has investigated how health insurance status affects health care utilization among newly eligible populations, and the few studies that have been conducted do not provide consensus on these outcomes (Finkelstein et al., 2012; DeLeire et al., 2013).

One of the primary ways the ACA aims to reduce uninsurance is by expanding Medicaid to previously ineligible non-elderly adults. However, as several states have chosen not to implement these expansions, the reduction in uninsurance among the non-elderly has been considerably weaker in non-expansion states (DeLeire, Joynt and McDonald, 2014). The absence of insurance coverage left hospitals shouldering the burden of almost 60 percent of all uncompensated 2013 health care costs nationwide (Coughlin et al., 2014). Expanded Medicaid coverage can lower hospital uncompensated care costs through reductions in uninsured visits or by decreasing hospital admissions through improved access to preventive or outpatient care. Hospital costs may further decrease if health insurance reduces the role of emergency departments (EDs) as the primary source of care and redirects healthcare consumption towards preventive ambulatory care. Thus, estimates of Medicaid's effects on the payer composition of hospitalizations and the corresponding implications for hospital finances are empirically relevant and of broader interest in current health care reform discussions.

To analyze these implications empirically, we consider a 2005 policy change in Tennessee, which led to over 170,000 Medicaid beneficiaries losing coverage. We estimate the impact of this Medicaid contraction by comparing inpatient utilization in Tennessee to that in states that did not contract or expand Medicaid, before and after Tennessee's policy change. This identification strategy follows prior work on the labor market outcomes of Tennessee's Medicaid contraction (Garthwaite, Gross and Notowidigdo, 2014) as well as studies on the impact of health insurance expansions on hospitalization (Kolstad and Kowalski, 2012). Because Tennessee's Medicaid contraction primarily affected non-elderly non-pregnant adults while leaving other population subgroups – children, the elderly and pregnant women – relatively unaffected, we are also able to use a triple-difference study design taking advantage of this variation in exposure.

We begin by studying the impact of Tennessee's 2005 Medicaid contraction on the level of Medicaid and uninsured inpatient admissions among non-elderly adults. We expect to see an unambiguous decrease in Medicaid admissions. To the extent that those losing Medicaid were able to find private or other insurance coverage, we expect little increase in uninsured hospitalizations; if, on the other hand, many of those losing coverage were unable to find coverage, or if those who were at risk of hospitalization were especially likely to remain uninsured, we expect to see substantial increases in uninsured hospitalizations. Under the premise that the uninsured seek less care than the insured (Decker et al., 2013), we also expect a decrease in the aggregate volume of hospitalizations. Additionally, we examine whether the policy influenced the entry point for uninsured hospitalizations (i.e., through the ED or directly to the inpatient unit) and whether the hospitalizations were for preventable conditions. Evidence that hospitalizations were more likely to originate in the ED and that hospitalizations increased for preventable conditions would be consistent with reduced access to primary care or to a regular source of care (medical home) following the policy change.

Restricting Medicaid eligibility in Tennessee led to a 21 percent decrease in Medicaid coverage and a 61 percent increase in uninsurance among non-elderly adult hospitalizations, relative to the baseline and to changes in other states. Such a change has implications for hospital revenue streams, because most uninsured visits result in unpaid bills (Chappel, Glied and Kronick, 2011). The results from our preferred specification suggest that the volume of Medicaid inpatient hospitalizations decreased by 24 percent, and uninsured inpatient admissions increased by 55 percent, relative to the baseline. These results are consistent with the prior studies on the effect of Tennessee's Medicaid policy change on insurance coverage and hospital uncompensated care costs (Garthwaite, Gross and Notowidigdo, 2014, 2015).

We find that increases in admissions originating in the ED explain 75 percent of the overall increase in uninsured inpatient stays; this is not surprising because those who lose Medicaid coverage and become uninsured face a more difficult process in being admitted directly to inpatient care. Prior evidence showed that losing Medicaid coverage is associated with a higher incidence of preventable hospitalizations (Bindman, Chattopadhyay and Auerback, 2008). However, in our data uninsured hospital visits increased for both preventable and non-preventable conditions by the same magnitude (more than 50 percent, compared to pre-disenrollment levels). While this is consistent with a shift in the expected payment source of hospitalizations from Medicaid to uninsured after TennCare contraction, it does not suggest a loss in access to preventive care per se. When we examine how Medicaid contraction affected the total volume of inpatient hospitalizations among non-elderly adults in Tennessee, we find fairly consistent evidence of a decrease, although it is not statistically significant in all specifications. In our baseline specification using Southern states as control, the result is a negative coefficient that is not statistically significantly different from zero. In additional specifications this negative effect becomes statistically significant. This suggests that changes in hospital-based care may have played a role in explaining the connection between prior state Medicaid expansions and reduced mortality.

This study contributes to the literature by providing the first empirical evidence on the hospital care utilization impact of one of the largest contractions in state Medicaid policy. In doing so, it adds to work on the economic impact of the TennCare disenrollment (Garthwaite, Gross and Notowidigdo, 2014; 2015) and to the broader literature on the effect of Medicaid on medical care use among non-elderly adults, an important population for current health policy. Furthermore, contraction of Medicaid coverage has received far less attention in the empirical literature than Medicaid expansion. Our study addresses this relatively understudied phenomenon and explores its consequences for access to care and use of uncompensated care. The findings of this paper provide context for the results obtained by Sommers, Baicker and Epstein (2012) showing that Medicaid reduced mortality among non-elderly adults. Finally, our results indicate the ACA Medicaid expansions can reduce uninsured hospitalizations thereby decreasing hospital uncompensated care costs.

This paper is organized as follows. In Section 2, we describe the institutional details of Tennessee's Medicaid policy changes. Section 3 provides a review of the literature related to the causal effect of health insurance coverage on health care utilization. Section 4 explains the data, identification strategy and empirical framework. In Sections 5 we present robustness checks for our empirical specification. Finally, Section 6 concludes and discusses implications for the ACA.

2. Institutional Background

From its inception in 1965, Medicaid typically provided coverage only to low-income populations that the federal government mandated it serve, such as children, pregnant women, parents and disabled individuals. Prior to the ACA, the federal government did not routinely share the costs of enrollees ineligible for traditional Medicaid (mostly low-income, childless adults), referred to as "optional" or "expansion" populations. Accordingly, most states denied Medicaid coverage to these populations.

One of the ways in which states could extend Medicaid coverage to non-mandatory populations prior to the ACA was through section 1115 demonstration waivers of the Social Security Act. To do so, states had to obtain authorization from the Health Care Financing Administration (HCFA).[1] Tennessee obtained approval from the HCFA for its statewide Medicaid demonstration project, TennCare, in November 1993. TennCare was created with the objective of reducing uninsurance in Tennessee and reining in healthcare costs. In January 1994, Tennessee placed all of its Medicaid enrollees in managed care organization (MCO) contracted plans, aiming to control costs and use the savings generated to provide subsidized Medicaid coverage to optional populations. Uninsured individuals who qualified for TennCare coverage included those who did

[1] States could use section 1115 waivers to expand their Medicaid programs subject to budget-neutrality, such that a demonstration project would not cost the federal government more than the existing Medicaid program. This could be achieved by using existing Medicaid funds or savings/revenue from other state programs and restricting the benefit packages of new enrollees and streamlining service delivery options to limit costs (Holahan et al, 1995).

not have employer-sponsored insurance but whose annual income was too high to make them eligible for public insurance.[2] The demonstration project resulted in Tennessee achieving the highest Medicaid coverage rate of any state in the country, with 23 percent of its population enrolled in TennCare in 2004 (Farrar et al., 2007).

Despite these efforts, TennCare was unable to sustain its cost-control objective, and the state of Tennessee submitted a waiver amendment proposal to the Centers for Medicare and Medicaid Services (CMS, formerly HCFA) in September 2004. In November 2004, Governor Phil Bredesen announced that TennCare would stop covering the optional population (Chang and Steinberg, 2014). CMS approved the proposal in March 2005, which authorized disenrollment of TennCare beneficiaries over age 19 who were not eligible for the open Medicaid categories.

The disenrollment took place within a span of 3 months beginning late July 2005. In 2004, administrative records showed that there were 1,340,824 beneficiaries of Tennessee Medicaid, of whom 1,079,975 were in mandatory categories and 260,849 were in optional categories. Nearly 160,000 adults belonging to the optional population had been disenrolled from TennCare by the fourth quarter of 2005, which represented a 12 percent reduction in Medicaid enrollment in the state. By 2006, the total number of adult TennCare beneficiaries disenrolled reached approximately 170,000. Comprising non-elderly adults, this disenrolled population from Tennessee was similar to those gaining coverage under the expanded Medicaid categorical eligibility provisions of the ACA; both groups were predominantly composed of adults without dependent children in the household (Garthwaite, Gross and Notowidigdo, 2014).

Because the TennCare disenrollees and those gaining Medicaid eligibility following the ACA expansions share key characteristics, our results can potentially imply connections between Medicaid eligibility and health care utilization among the newly eligible population. However, because the health status of the Tennessee disenrollees could have been worse than those gaining coverage under the ACA Medicaid expansions, our estimates regarding the disenrollment must be

[2] Eligibility in the optional categories of TennCare for the non-elderly required beneficiaries' annual income to be less than 400 percent of the federal poverty level (FPL) and included sliding scale premiums for beneficiaries with incomes above 100 percent FPL. These optional/expansion populations included non-elderly adults who either (1) were "uninsured" on March 1, 1993 and had continued to be without health insurance since or (2) belonged to the "uninsurable" category – individuals who were denied health insurance due to pre-existing health conditions (Government Accountability Office, 1995; Moreno and Hoag, 2001).

interpreted with caution. On the other hand, the disenrollees could be healthier because of the healthcare they received while insured.[3] If the ACA Medicaid expansions induce unhealthier adults to opt out of private health insurance and take up Medicaid (Clemens, 2015), it is possible that our estimates will have greater external validity for extrapolating to the non-elderly adult population gaining coverage as a result of the ACA.

3. Previous Literature

3.1. Literature on the Impact of Health Insurance on Hospital Care Utilization

Our paper is closely related to several quasi-experimental studies that examine the effect of health insurance on inpatient hospitalizations. Most of the prior research finds that extending health insurance coverage leads to increases in hospital utilization among children, young adults and the elderly (Dafny and Gruber, 2005; Anderson, Dobkin and Gross 2012, 2014; Antwi, Moriya and Simon, 2015; Card, Dobkin and Maestas, 2008). However, low-income non-elderly adults differ from these other populations in their prevalence of health conditions and patterns of use (Decker et al., 2013), and thus health insurance may have distinct effects their use of hospital care.

Recent studies on Medicaid expansions in Oregon and Wisconsin provide evidence on Medicaid's hospital utilization effects among low-income adults. Using administrative data on hospital visits in Oregon, Finkelstein et al. (2012) find that Medicaid coverage gained through random assignment led to a 30 percent increase in the probability of hospitalization among previously uninsured low-income adults who were categorically ineligible for traditional Medicaid. On the other hand, using administrative data from Wisconsin, DeLeire et al. (2013) find that inpatient hospitalizations *decreased* by 59 percent when previously uninsured low-income childless adults automatically gained Medicaid (BadgerCare) coverage. Massachusetts reform that extends private as well as public coverage, however, does not appear to increase hospitalizations (Kolstad and Kowalski, 2012). Hence, the evidence from these studies is inconclusive with respect to Medicaid's impact on inpatient hospital use among non-elderly adults.

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[3] Based on interviews with providers and current and former TennCare beneficiaries in the first year following the disenrollment, Farrar et al. (2007) report that 67,000 of the 170,000 Medicaid disenrollees were uninsurable as they did not qualify for any other insurance coverage due to poor health status; in addition, a large fraction of the disenrollees had multiple chronic conditions.

3.2. Literature on TennCare Contraction

A small body of research has examined the TennCare contraction using quasi-experimental methods, and shows sizable net increases in uninsurance and uncompensated care despite increases in private coverage. Garthwaite, Gross and Notowidigdo (2014) examine the impact of the Medicaid contraction in Tennessee on insurance and labor market outcomes using within- and across-state variation in the Current Population Survey (CPS). They find that Medicaid coverage among non-elderly adults declined by 4.6 percentage points and that private insurance coverage increased by 1.6 percentage points in the two years following TennCare contraction. In a subsequent article, Garthwaite, Gross and Notowidigdo (2015) examine data from the American Hospital Association and Joint Annual Reports from Tennessee Department of Health and find that uncompensated costs increased in Tennessee by 18 percent after the disenrollment compared to other states, and that the effects were concentrated in hospitals with EDs. The increase in prevalence of uninsurance after the Medicaid policy change in 2005 and the effect on hospital uncompensated care costs, suggests that healthcare utilization in Tennessee may have been affected as well.

Two observational studies report on ED health care use in Tennessee after the Medicaid contraction. Using a census of all Tennessee ED visits between 2004 and 2006 (inclusive), Heavrin et al. (2011) describe a 22 percent decrease in adult Medicaid visits and a 39.5 percent increase in uninsured adult visits in Tennessee after the contraction. They also note a 2 percent increase in the fraction of uninsured ED visits that result in inpatient hospitalization. Emerson et al. (2012) use the census of ED discharges for one Tennessee county (Davidson, which includes the city of Nashville) for 2003-2007 and report increases in both the number of ED visits for ambulatory-sensitive conditions and hospital uncompensated care costs after the disenrollment. Although their data also contained inpatient hospitalizations, the paper focuses only on ED visits; they do, however, note that the number of uninsured inpatient admissions among non-elderly adults increased by 42 percent, but there was only a very minor decline of 0.6 percent in Medicaid hospital admissions. Because these studies only use Tennessee data, it is unclear that we can draw causal lessons from them because some changes may reflect national trends. Additionally, unlike scheduled direct inpatient hospitalizations that are price sensitive, ED visits are less responsive to insurance status. Therefore, while these studies based on data from ED visits are informative, it is also important to examine utilization of hospital-based care, both scheduled and otherwise, using a quasi-experimental study design.

Our study goes beyond previous work as it is the first non-observational study to examine the healthcare-use impacts of Tennessee's Medicaid contraction, and the first to focus specifically on inpatient hospitalization. Our research contrasts with prior quasi-experimental studies from other states on adult Medicaid policy because we examine the impact of a loss of Medicaid coverage on healthcare utilization, whereas Finkelstein et al. (2012) and DeLeire et al. (2013) analyze expansions of Medicaid coverage. Healthcare consumption may respond asymmetrically to Medicaid coverage gain or loss. For example, loss of Medicaid coverage may have less impact on utilization as patients are already familiar with the healthcare system, whereas transitioning from uninsurance to Medicaid may increase use of care but only after a lag due to difficulties in navigating the new and complex healthcare environment. We also extend the methods used in these earlier two studies: we use a cross-state identification strategy as well as within-state controls, whereas the findings from Oregon and Wisconsin were based on within-state control groups. Our empirical method comparing one state to several others is closest to that employed by Kolstad and Kowalski (2012) in studying the effect of the Massachusetts health care reform on inpatient hospitalizations, and to that of Garthwaite, Gross and Notowidigdo (2014) in analyzing the effect of Tennessee's Medicaid contraction on labor market outcomes. We also include within-state control groups to estimate a triple difference specification, similar to Garthwaite, Gross and Notowidigdo (2014), who compare outcomes among those under 65 to those over 65.

4. Method

4.1. Data

Our empirical analysis uses the Nationwide Inpatient Sample (NIS) 2001-2009, which is part of the Healthcare Cost and Utilization Project (HCUP) of the Agency for Healthcare Research and Quality and contains patient-level data on all inpatient stays from a 20-percent national sample of community hospitals.[4] Each year of the data contains patient-level information on age, gender,

[4] The American Hospital Association (AHA) defines community hospitals as "all non-Federal, short-term, general and other specialty hospitals, excluding hospital units of institutions." The NIS sample includes among community hospitals various specialty hospitals such as obstetrics-gynecology, orthopedic, ear-nose-throat and pediatric institutions as well as academic medical centers and public hospitals and long-term acute care facilities, all since 2005. Short-term rehabilitation hospitals, long-term non-acute care hospitals, psychiatric hospitals and alcoholism/chemical

race, source of admission, payer (including Medicaid, Medicare, private insurance, self-pay or no charge, Tricare, CHAMPUS, etc.), diagnosis and procedures performed for all admissions in a sampled hospital. The NIS includes state identifiers and, for some states, county identifiers for hospitals in the sample.

Administrative hospital data has a number of advantages over survey data. First, administrative data is superior with regard to accuracy of information on payer source due to a higher potential for measurement error in self-reports. Second, these data allow us to use four full years from pre- and post-treatment periods. Unlike the Oregon and the Wisconsin studies which use only one year of post-treatment data, this broader time span will help us observe changes in the utilization of medical services that may take longer to manifest. One of the limitations of the NIS is the lack of longitudinal patient identifiers, which prevents us from examining hospitalization patterns specifically among formerly Medicaid-insured patients. Furthermore, we are unable to observe utilization of primary care or outpatient care. Although alternative data sets (such as the Medical Expenditure and Panel Survey) contain individual-level information on health insurance coverage as well as socio-demographic characteristics and medical care utilization before and after the policy changes at the state level, the size of these datasets preclude the study of single-state policies for comparatively rare medical events like hospitalizations. Thus, following prior studies on hospitalization responses to health insurance policy, we utilize cross-sectional administrative data.

4.2. Empirical Strategy

Difference-in-difference framework

To isolate the causal effect of TennCare contraction, we use several complementary identification strategies. Our main approach is a simple difference-in-difference framework similar to that used by Garthwaite, Gross and Notowidigdo (2014) when studying labor-market outcomes. We compare hospitalizations among non-elderly adults in Tennessee with those in other Southern states (first difference) before and after Medicaid policy changes (second difference). [5] Next, we

dependency treatment facilities are excluded from the NIS sample. NIS increased the number of states represented each year, from 33 in 2001 to 44 in 2009.

[5] Of the 17 states that the Census defines as the South region, the NIS does not include 4 of them (Alabama, Delaware, Mississippi and Washington, D.C.) during the years 2001-2009.

exclude all birth-related hospitalizations for this specification, because they are arguably less likely to be affected by the disenrollment.[6] Our reduced-form estimating equation, similar to that of Kolstad and Kowalski (2012), uses hospital data aggregated at the *hospital-quarter* level and is of the following form:

$$(1)\quad Y_{ht} = \alpha + \gamma Post_t + \mu Treat_h + \delta Treat_h x Post_t + X_{ht}\beta + \theta_h + \tau_t + \varepsilon_{ht},$$

In equation (1) Y_{ht} denotes our outcome variable of interest for hospital *h* and time *t.* The regressor of principal interest here is *TreatxPost*, where (a) $Treat$ is a binary indicator that takes the value of 1 for Tennessee and 0 for other states; (b) $Post$ is a binary indicator taking the value of 1 for year 2006 and after, 0 otherwise and (c) the parameter δ is the difference-in-difference estimate of the impact of the Medicaid contraction in Tennessee. The vector X_{ht} includes patient-level demographic and clinical characteristics aggregated at the cell level.

The model includes year- and quarter-fixed effects to capture aggregate time trends that are common to both the treatment and the comparison states. We include the unemployment rate, and an interaction between the treatment indicator and the unemployment rate, to control for the effect of business cycles or other macroeconomic factors.[7] We also include hospital-fixed effects to account for unmeasured time-invariant hospital-specific factors that could affect utilization outcomes. Following Kolstad and Kowalski (2012), we cluster standard errors at the state level to account for arbitrary correlations in error terms at the state level over time (Bertrand, Duflo and Mullainathan, 2004). We exclude data from 2005, the year in which the contraction took place.[8] Based on prior studies on the effect of health insurance expansions on inpatient care utilization

[6] Non-birth admissions indicate inpatient stays in the sample with a major diagnostic code (MDC) other than 14. An MDC code of 14 indicates that the principal diagnosis for the date of discharge was pregnancy, childbirth or puerperium.

[7] We merge in unemployment rate data from the Bureau of Labor Statistics' Local Area Unemployment Statistics at the county-by-year level for the NIS states in which a county is identified; in other states, we merge in the statewide average annual unemployment rate. We also merge in county and state population estimates from the Census Bureau in a similar manner, for use in later specifications where outcomes are measured per capita. Because the population of Tennessee grew by roughly 9.7 percent between 2001 and 2009 (authors' calculations based on Census estimates), the use of per-capita measures separates the effect of secular trends in population size from the impact of the disenrollment on the extensive margin.

[8] For cleaner identification, in our main specification we have dropped observations from the year 2005, as disenrollment was announced in November 2004, began in July 2005 and continued through the last quarter of 2005. Following earlier work by Garthwaite, Gross and Notowidigdo (2014), we have defined the post-period as the year 2006 and later. As there may have been anticipatory effects following the announcement of the policy change in the fourth quarter of 2004. We explored an alternative specification by dropping both years 2004 and 2005 from the analysis sample; we found our results to be similar (results available upon request).

(Kolstad and Kowalski, 2012; Miller, 2012; Antwi, Moriya and Simon, 2015), we use ordinary least squares to estimate the equation for ease of interpretation.

Our difference-in-difference identification strategy uses plausibly exogenous variation in insurance coverage due to the disenrollment. This approach rests on an assumption that the control states serve as an appropriate counterfactual for Tennessee, absent Medicaid reform. This assumption is more likely to hold if pre-treatment trends between Tennessee and all other Southern states are similar; our empirical strategy tests this condition. We check the sensitivity of our DD results by augmenting this basic specification using two different approaches. First, we identify appropriate comparison states by utilizing (a) all states in the NIS as a comparison group, or (b) a group of NIS states identified through a synthetic control-matching procedure (Abadie, Diamond and Hainmueller, 2010). Second, we employ a triple-difference framework.

Triple-difference framework

We use a difference-in-difference-in-difference identification strategy to identify the causal effect of Medicaid on inpatient hospitalizations by using within-state comparison groups that were not directly affected by TennCare disenrollment. We exploit the fact that utilization among those under age 19, the elderly and pregnancy-related hospitalizations among the non-elderly is not likely to be directly affected by TennCare disenrollment.[9] Garthwaite, Gross and Notowidigdo (2014) also use a DDD strategy by comparing labor market outcomes of the non-elderly to the within-state group over age 65.

In equation (2), we compare changes in insurance coverage rates by payer type and utilization among non-elderly adults in Tennessee relative to our three possible within-state control groups, relative to other states before and after the policy change in 2005.

$$(2)\ Y_{ght} = \alpha + \beta_1 Treat_h + \beta_2 Post_t + \beta_3 WSA_g + \gamma_1 Treat_h x Post_t + \gamma_2 Treat_h x WSA_g + \gamma_3 Post_t x WSA_g + \delta Treat_h x Post_t x WSA_g + X_{ght}\emptyset + \theta_h + \tau_t + \varepsilon_{ght}$$

[9] We use the elderly (age 66 and older) as a control group only to study total hospitalization volume outcomes. We do not use this control group strategy to study insurance outcomes because of the dominant role of Medicare. We identify the pregnancy group as non-elderly inpatient stays with primary diagnosis recorded as birth-related conditions (where the MDC code takes the value of 14).

The variable Y_{ght} is the outcome of interest for inpatient admissions in age group g for hospital h and time t. The indicator variable WSA (within-state affected groups) takes the value of 1 for the targeted individuals between ages 20 and 64 (inclusive); it takes the value of 0 for each of the three alternative control groups in three separate specifications. This specification includes all covariates that were included in (1). Even though each of the control groups is not directly affected by disenrollment, spillover effects may exist. To address this possibility, in the DDD framework we estimate whether TennCare disenrollment led to proportionally higher changes in the outcomes of interest among the affected group relative to those in the controls, in addition to comparing outcomes in Tennessee to those in other states before and after 2006. The coefficient of interest here again is δ.

Compared to the DD estimation strategy, the DDD method allows us to control for confounding shocks to non-elderly hospitalizations that differ between Tennessee and other states at the time of the TennCare policy change. At a national level, one concern with incorporating data on those over age 65 is that the implementation of Medicare part D in 2006 could undermine the use of elderly hospitalizations in the DDD because of possible spillover effects from drug use to hospitalizations (Kaestner, Long and Alexander, 2014). As there is no reason to expect these spillover effects to differ across states, we also estimate a DDD using those over 65 and find similar results as the DD. A potential drawback to using pregnant women and children as within-state controls is that in 2005, CMS authorized TennCare to restrict pharmaceutical benefits for continuing non-pregnant Medicaid beneficiaries. However, these changes appear minor, and it is unclear whether they were binding and actually instituted.

4.3. Hypotheses

Following the Medicaid contraction, we expect to find that fewer hospitalizations in Tennessee were paid through Medicaid, and, to the extent that those losing Medicaid were unable to obtain other coverage, we expect to find that the share of uninsured hospitalizations increased. [10]

[10] Our measure of uninsured admissions includes inpatient stays categorized as self-pay (the patient was billed directly by the hospital) and no charge (neither patient nor insurer was billed; likely attributed to charity care). HCUP documentation reports that self-pay categories may not reflect full payment of outstanding charges, and that in the event of non-payment hospitals bear the burden of unpaid costs as uncompensated care (bad debt). On average, uninsured families with incomes less than 200 percent of the federal poverty level have enough assets to pay in full for only 4 percent of their hospitalizations (Chappel, Glied and Kronick, 2011).

Given that lack of health insurance increases the cost of obtaining medical care, we expect to find that the total volume of hospitalizations among non-elderly adults decreased after the Medicaid contraction as a result of a price effect. Garthwaite, Gross and Notowidigdo (2014) provide evidence that the Tennessee disenrollment increased private insurance coverage through an employment increase, thus partially offsetting the decrease in public coverage. However, those who are at risk of hospitalization are likely over-represented among those who were not able to find employment following the disenrollment. To the extent that disenrollees seeking hospital-based care obtained private insurance, we expect to also find an increase in private insurance coverage for hospitalizations.

The uninsured tend to preferentially use the ED, as opposed to office-based care (Anderson, Dobkin and Gross, 2014), due to legislative provisions such as the Emergency Medical Treatment and Labor Act (EMTALA) requiring hospitals to provide stabilizing care to all patients presenting at emergency rooms regardless of their ability to pay. In the NIS, we are able to observe whether an inpatient admission originated in the ED, although we are unable to observe outpatient ED visits in our data. We decompose uninsured inpatient hospitalizations by source of admission, specifying ED or otherwise. We expect to find a higher number of uninsured hospitalizations resulting from ED following the TennCare contraction.

A higher volume of uninsured hospitalizations could also reflect an adverse effect of coverage loss on access to primary or office-based care due to uninsurance, as the financial disincentives associated with lack of health insurance may induce the uninsured to forego ambulatory care, leading to more hospitalizations among the uninsured for preventable medical conditions. Therefore, we expect to find greater increases in hospitalizations among the uninsured for preventable relative to unpreventable conditions.

5. Results

5.1. Descriptive Statistics

Table 1 presents sample statistics of inpatient admissions for non-birth-related conditions among those aged 20 to 64 from NIS 2001-2009 for Tennessee and the other Southern states that serve as the comparison group.[11] There are broad similarities between Tennessee and the comparison states in the age, gender and racial composition of their inpatient stays before TennCare contraction. In addition, the means for clinical characteristics are not very different across the two groups. Unsurprisingly, given the broad scope of Tennessee's Medicaid program prior to 2005, inpatient admissions in Tennessee are substantially more likely to be Medicaid insured and less likely to be uninsured relative to the comparison states in the pre-contraction period. After the TennCare disenrollment in 2005, a smaller fraction of hospitalizations are now Medicaid insured and a greater fraction are uninsured in Tennessee compared to before 2005, reaching the same levels as in the comparison states (a little over 16 percent of hospitalizations are Medicaid insured and 13 percent are uninsured). By comparison, private insurance and Medicare move fairly similarly as sources of coverage among hospitalization in Tennessee and in control states over this time period. In the last panel, we present sample means for the population-adjusted volume of hospitalizations. The changes in volume outcomes across insurance types correspond directly with the changes in health insurance composition of the hospitalizations. We observe a decline in Medicaid hospitalizations from 5.6 per 1,000 population in the state to 2.5 per 1,000, and increase in uninsured hospitalizations from 1.3 per 1,000 to 2.0 per 1,000, in Tennessee. Reflecting the larger decrease in Medicaid volume than the increase in uninsured volume, total volume of hospitalizations in Tennessee appears to have decreased in the post-contraction period. The volume of hospitalizations in total and by insurance type remained relatively stable in the comparison states.

We examine further the changes indicated in Table 1 between pre- and post-periods by depicting the exact trends in insurance composition and volume of hospitalizations by insurance type between years 2001-2009 in Figures 1 and 2. The three vertical lines in Figures 1 and 2 denote the announcement of TennCare contraction in 2004Q4, its implementation in 2005Q3, and the beginning of the post-period in 2006Q1. Figure 1 shows that there is a sharp decline in Medicaid admissions and an uptick in uninsured hospital admissions in Tennessee immediately following the announcement in the fourth quarter of 2004; the trend continues through 2005 into the post-contraction period beginning in 2006. Figure 2 also shows the similarity in pre-policy trends in

[11] The states included here are those that the U.S. Census Bureau defines as the Southern states and are a part of the NIS sample, namely Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, West Virginia, Kentucky, Tennessee, Arkansas, Louisiana, Oklahoma and Texas.

volume of hospitalizations by payment source between treatment and control states, and the pronounced changes in volume of Medicaid and uninsured admissions 2004Q4 onwards in Tennessee compared to other states[12].

5.2. Effect of Medicaid Contraction on Insurance Coverage Among Inpatient Hospitalizations

The results in panel A of Table 2 come from the DD regression specification (1) and demonstrate the effect of TennCare contraction on the insurance composition of non-elderly adult patients in the inpatient sample. The proportion of inpatient admissions with Medicaid decreased by 6.3 percentage points. This represents a 21 percent decrease relative to the pre-treatment mean showing that 30.4 percent of hospitalizations in Tennessee were Medicaid insured. The proportion of uninsured inpatient admissions in Tennessee increased relative to the comparison states; the coefficient estimate of a 4.1 percentage point increase in column 5 of panel A implies an approximately 61 percent increase in the proportion of uninsured inpatient admissions following the policy change, relative to the pre-contraction mean of 6.7 percent. Taken together, these results on changes in insurance coverage composition suggest that the contraction led to a shift in the patient payment composition for hospitals from Medicaid to uncompensated care. Our estimate of the impact of the policy change on Medicaid coverage among inpatient hospitalizations is consistent with findings of general population level insurance changes in prior literature; Garthwaite, Gross and Notowidigdo (2014) find reduced Medicaid coverage of 5.1 percentage points and an increase in private coverage by 1.7 percentage points, among non-elderly adults in Tennessee. Our results suggest that estimates of increased private insurance in the general population do not generalize to this relatively unhealthy population seeking hospital-based care.

5.3. Effect on Volume of Admissions

Panel B of Table 2 shows the impact of Medicaid contraction on the volume of inpatient admissions by insurance status. The dependent variable in panel B is the population adjusted rate of hospitalizations at the hospital-quarter level (number of admissions divided by county population in 10,000s). The point estimates for Medicaid and uninsured hospitalization in panel B

[12] Figure 2 reveals a drop in hospitalization rates from 2001 to 2002 levels, across all insurance types. Even though this trend appears common to both the treatment and comparison states, we conducted additional analyses using NIS 2002-2009 data and found our results to be similar.

have the same sign as the corresponding results in panel A and are all statistically significant at the 1-percent level. The estimates indicate a decrease in Medicaid visits and an accompanying increase in uninsured visits. Relative to an average population adjusted Medicaid hospitalizations rate of 72.259 in Tennessee, the point estimate of -17.481 in column 1 implies a reduction of nearly 24 percent. The coefficient estimate of 11.066 on uninsured hospitalizations suggests that among the non-elderly hospitalized population there was a 55-percent increase in the volume of uninsured visits following TennCare disenrollment relative to the initial pre-treatment mean of 20.316 uninsured hospital admissions. These estimates are consistent with prior studies that find a decrease in Medicaid and an increase in uninsured inpatient hospitalizations following TennCare contraction (Heavrin et al., 2011; Emerson et al., 2012; & Garthwaite, Gross and Notowidigdo, 2015). The last column in panel B shows that there a negative but statistically insignificant effect on overall admissions after the disenrollment. In later specifications in section 5.7 using alternate control groups, this effect is statistically significant. This implies that some of those who lost Medicaid still incur hospitalizations but now with no source of insurance (replacing just over half of all Medicaid hospitalizations that would otherwise have occurred; 11.066 /17.481), while the remaining hospitalizations are less likely to occur at all.[13]

5.4. Effect on Source of Uninsured Admissions

To understand the nature of the change in inpatient admissions better, in Table 3 we now examine whether contraction affected the source of uninsured admissions. The first 2 columns indicate whether the admission originated in the emergency room. Of the total increase of 11.066 per-capita uninsured admissions in Tennessee (DD estimate from table 2, panel B, column 5), the estimates in columns 1 and 2 show that 8.391 of these took place through the ED, with the remaining 2.686 through non-ED sources. Both these estimates are statistically significant at the 1 percent level. This suggests that nearly 75 percent of the total increase in volume of uninsured admissions in the post-disenrollment period was driven by an increase in those that originated in the ED, which is much larger than the increase in non-ED uninsured admissions. In other words,

[13] We also estimate a triple-differences model using the over-age-65 inpatient admissions as the third within-state control group to obtain similar effects on total population adjusted hospital admissions, suggesting that the Medicaid contraction led to a decline in the aggregate volume of inpatient admissions (results are available upon request).

among uninsured inpatient admissions, rise in admissions through the emergency room outpaced rise in admissions through non-ED sources.

When compared to baseline means of per-capita uninsured visits, the implied treatment effect is 64 percent and 37 percent for ED and non-ED visits, respectively. This finding is consistent with a scenario in which cost-related barriers to care lead the uninsured to seek care through emergency rooms. This result is comparable in direction to Heavrin et al. (2011), who find an increase in uninsured ED visits resulting in inpatient admission after the Medicaid contraction in Tennessee. Likewise, Garthwaite, Gross and Notowidigdo (2015) find that TennCare disenrollment led to higher uncompensated care costs in Tennessee hospitals, and that the increase was more pronounced among hospitals with an ED.

5.5. Effect on Preventable Admissions Among the Uninsured

Inpatient hospitalizations due to ambulatory-care-sensitive conditions[14] (ACSC) are considered to be potentially preventable through timely and/or good-quality care provided in a less resource-intensive outpatient setting. Hence, the Agency for Healthcare Research and Quality (AHRQ) bases their prevention-quality indicators (PQIs) on inpatient ACSC hospitalizations to measure population-level access to good-quality preventive care in an outpatient or office-based setting. Given this inverse relationship between access to primary care and preventable hospitalizations, we expect the disenrollment to increase uninsured hospitalizations for preventable medical conditions.

Columns 3 and 4 of Table 3 report DD estimates of the impact of the Medicaid contraction on uninsured inpatient admissions, decomposed by whether the medical condition is unpreventable in nature. While we expect that as Medicaid contracts, former Medicaid patients may now appear as uninsured patients and thus increaseing uninsured hospitalizations, there may also be other implications for uninsured hospitalizations beyond this simple accounting effect. If Medicaid contraction reduces access to ambulatory care for the newly uninsured, they may now appear more often for preventable hospitalizations. The direction of these point estimates in columns 3 and 4

[14] The list of ACS conditions used in this study includes but is not limited to medical conditions such as COPD, hypertension, CHF, uncontrolled diabetes, angina without procedure and adult asthma as specified in AHRQ guidelines.

of Table 3 suggest that uninsured inpatient hospitalizations increased for preventable as well as non-preventable conditions after the changes in TennCare eligibility, and by slightly more than 50 percent relative to the baseline pre-disenrollment levels. Thus, although we expect that Medicaid contraction also reduced access to ambulatory care, the pattern of hospitalization change does not provide evidence consistent with a shift in composition towards more preventable hospitalizations by this measure of "preventability." These estimates must, however, be interpreted with caution, as they only provide implicit confirmation of our hypothesis given that our data does not provide information on utilization in other settings.

In prior work, Kolstad and Kowalski (2012) find corresponding declines in preventable admissions among the non-elderly population after the Massachusetts health care expansion. Our results are also similar in direction to Emerson et al., (2012) who find that TennCare contraction was associated with higher uninsured ACSC admissions. .

Further analysis

5.6. Effect on Intensity of Hospital Treatment

The results from Table 2 suggest that the relative changes in payer mix may have had an adverse impact on hospital finances due to shifts in the expected source of payment for inpatient admissions; changes in patient health mix, however, could potentially exacerbate such an effect. These second-order effects may capture changes in the characteristics of the patient pool. In particular, it can be argued that the disenrollment may have altered the health mix of the uninsured hospital admissions, as this group now includes individuals with poorer health status who do not qualify for health insurance from sources other than TennCare. An increase in post-2005 intensity of treatment among the uninsured, as measured by the number of procedures performed during an inpatient stay and the length of stay, would provide evidence of such an effect. We display these results in Table 4.[15]

In columns 1-6 of Table 4 we report findings on intensity of treatment among all admissions and in the sample of uninsured visits only. Columns 5 and 6 show that length of stay

[15] The number of procedure codes reported in the NIS varies by state. We record up to a total of 6 procedures, which is the minimum number reported by the states during the period 2001-2009.

increased among uninsured hospital visits, while the number of procedures appears statistically unaffected (in column 4). This increase in length of stay among the uninsured provides suggestive evidence that the disenrollment shifted the composition of uninsured patients towards the less healthy. However, the disenrollment did not appear to have any effect on any of the measures of treatment intensity when all admissions were considered together. These effects on volume and treatment intensity together suggest an upward shift in the volume of uncompensated hospitalizations, a likely decrease in total hospitalizations, especially of those that are reimbursed, as well as an increase in resource intensity for treatment of the uninsured, which potentially exacerbates the fiscal pressure on hospitals.

5.7. Sensitivity Checks

We test the validity of our DD identification strategy by employing a series of sensitivity checks. Although Figures 1 and 2 show that hospitalization trends in Tennessee seemed to match other Southern states in the period prior to TennCare contraction, here we formally test the equality of pre-treatment trends. We regress each of our outcome variables on the Tennessee indicator interacted with a linear time-trend (measured in quarters) using NIS data for 2001-2004. The results from this test are displayed in Appendix Table A1, panels A and B. A statistically significant coefficient on the regressor *TrendxTreat* would indicate that Tennessee and the other Southern states experienced different trends in that outcome prior to Medicaid contraction, and that a DD estimator might pick up the continuation of this divergence. The key coefficients in panel A, where the outcomes are fractions of admissions by insurance type, are not statistically significant, and support our identification strategy. Among the volume outcomes of interest in panel B, the uninsured visits outcome has statistically significantly different pre-trends as indicated by the interaction terms, although the magnitude is substantially smaller than the corresponding DD estimate.[16] For example, the uninsured column in panel B of Table A1 shows a coefficient of 0.620 (p-value<0.01) while the DD coefficient corresponding to this model, in panel B of table 2, shows a magnitude of 11.066 (p-value<0.01). Nevertheless, this raises the concern that our estimates may be biased if we do not account for these pre-existing trends, and so we include state linear time trends in a sensitivity analysis. In Table A2 panel D, we find that this inclusion does

[16] Appendix Table A8 reports estimates from a similar pre-trend analysis where we use all other states in the NIS 2001-2004 as controls and obtain similar results.

not change our estimates in a substantial way. The largest difference in magnitudes of the point estimates between Table 2 panel A and Table A2 Panel D is in the Medicaid column; the coefficient in Table 2 is -0.063 and in Table A2 Panel D is it -0.047 (with the state time trend included), and both are statistically significant at the 1 percent level. We also present results using quadratic and cubic state time trends (Table A2 and A4); they too show consistent results.

Our main analysis compared Tennessee to all other Southern states in our data, following the approach in Garthwaite, Gross and Notowidigdo (2014). We estimate the sensitivity of the choice of control groups by using all states in the NIS sample as comparison states and present the coefficient estimates for the proportion and volume outcomes in panel A of Tables A2 and A4, respectively[17]. These results confirm our findings from the baseline model; the coefficients on our main outcomes of interest – Medicaid and uninsured visits – are similar in magnitude and precision. We explore the choice of control states further by using a synthetic control matching technique as outlined by Abadie, Diamond and Hainmueller (2010). We use both levels and trends of Medicaid hospitalizations and the control variables in the pre-treatment period as criteria to obtain the appropriate subset of control states after aggregating our patient-level data to state-year cells.[18] As clustering at the state level tends to result in unreliable standard errors when the number of states is less than 11 (Angrist and Pischke, 2008), we cluster the standard errors in the synthetically matched DD specification at the state-year level, similar to Courtemanche and Zapata's (2014) analysis of the effect of the healthcare reform in Massachusetts using control states picked through a synthetic match. In Tables A2 and A4 (for payment composition and volume of hospitalizations, respectively), panel B presents the DD estimates where control states were matched on levels, while for the estimates in panel C the control group was matched on trends, using pre-2005 data on Medicaid admissions. The coefficients on Medicaid and uninsured

[17] Note that both Missouri and Massachusetts experienced considerable changes in health insurance coverage during the implementation and post-disenrollment periods in Tennessee. Because such contemporaneous changes can bias estimation, these two states are not entered as control states in any specification. In particular, Missouri introduced substantial cutbacks in its Medicaid program in 2005, resulting in more than 100,000 beneficiaries losing coverage. Massachusetts, meanwhile, adopted legislation in 2006 with the goal of attaining near-universal health insurance coverage in the state, which included a large-scale Medicaid expansion.

[18] The synthetic control group chosen by this algorithm when matched on per-capita Medicaid admissions (levels) is 36.7% KY, 24.1% NY and 39.3% TX. By matching on the rate of change in per-capita Medicaid admissions (trends) the resulting group of control states is composed of 13.1% CO, 30.6% MD, 42.2% NJ, 11.5% NY and 2.7% UT. Control variables include age, gender, race, unemployment rate, poverty rate and median income averaged over the 2001 to 2004 sample period. In both cases, we reweight the sample using the synthetic weights to obtain the control group.

admissions remain statistically significant across specifications and are largely similar when compared to the estimates in Table 2, providing further evidence that our DD estimates are not sensitive to the exact choice of control states. For all these tests described in Tables A2 and A4, we have also included linear, quadratic and cubic time trends (panels D, E and F) and find that, as with our main specification, results are largely insensitive to this addition.

We next present results of our DDD specification (equation 2) using (1) the Southern states and (2) all NIS states as comparison states. Tables A3 and A5 present results in which we use two alternative within-state control groups: those aged 0-19, and non-elderly hospitalizations that are pregnancy related. Our results from this specification point to similar conclusions as those from the DD, although the DDD estimates are generally slightly larger. The largest difference in coefficients is for the volume of Medicaid hospitalizations, where the point estimate in Table 2 panel B (DD) is -17.481 and the DDD (table A5 panel B) is -24.222; both are statistically significant at the 1 percent level.

To check the robustness of our results in Table 3 examining source of admission for uninsured hospitalizations and whether visits are for conditions considered preventable, we present in Tables A6 and A7 results of each of the specifications conducted for our main analysis. This check includes testing the sensitivity of control group choice, of including state time trends, and of estimating the DDD specifications. Overall, our conclusions remain unchanged, as evident in the similarity of the point estimates as well as statistical precision across specifications. These results confirm the earlier finding that the impact of the Medicaid contraction was disproportionately higher for uninsured admissions taking place through the ED and that both preventable and non-preventable uninsured admissions increased.[19]

Finally, in table A9, we examine the impact on total volume of hospitalizations using different control groups. In table 2, we found a statistically insignificant effect (negative coefficient). Using all the NIS states as the control group, we now find that the Medicaid contraction led to a decline in total volume of hospitalizations (statistically significant at the 5

[19] In addition, we estimated equation 2 by using those over age 65 as a within-state comparison group. The DDD estimates for per-capita uninsured admissions and each of the four outcomes in Table 4 are reassuringly similar to our estimates from the baseline specification in terms of direction, magnitude and precision (results are available upon request).

percent level, and similar in terms of magnitude). In the DDD specifications this negative effect remains statistically significant, suggesting that neither non-elderly nor Tennessee-specific factors are confounding these results. Therefore, we interpret these results for total volume of hospitalizations as evidence of a decrease in overall inpatient utilization in response to the Medicaid contraction.

5.8. Discussion of Results

Our estimates of the impact of Medicaid coverage changes on the utilization outcomes from Table 2 panel A and B are qualitatively comparable to findings from earlier literature on the impact of health insurance on inpatient hospitalizations among adults. In particular, using a similar difference-in-difference model and the same data source as our study, Kolstad and Kowalski (2012) find that the near-universal health insurance coverage expansion in Massachusetts resulted in a 2.31 percentage-point drop in uninsurance. Compared to the pre-reform uninsurance rate of 6.43 percent, this drop represents a decline of 36 percent. Furthermore, Antwi, Moriya and Simon (2015) find the dependent coverage mandate of the ACA to have decreased uninsured hospitalizations among young adults between ages 19 to 29 in the NIS sample by 12.7 percent. Our results are also consistent with evidence from a randomized controlled trial conducted in Oregon that found a 30 percent increase in inpatient hospitalizations among those provided Medicaid (Finkelstein et al., 2012). However, our estimates differ from results in Wisconsin (DeLeire et al., 2013), where insurance expansion is associated with a decrease in inpatient utilization. While, the direction of the impact of the Medicaid contraction is similar to the effect of Medicaid expansions studied in prior literature, the effect on magnitude may not be symmetric. Medicaid expansions can potentially increase access and utilization of both inpatient and outpatient care that may be complements or substitutes in consumption. Consequently, the net effect on utilization will depend on the relative strengths of these opposing forces.

Unlike our results for Medicaid and uninsurance, the coefficient estimates for Medicare, private and other insurance are generally less consistent across specifications in direction and precision, and are therefore not as informative. Garthwaite, Gross and Notowidigdo (2014) present evidence that as public insurance coverage dropped, private insurance coverage increased and a crowd-out rate of 34.6 percent resulted among childless adults in the Current Population Survey data in response to TennCare disenrollment. They argue that the disenrollment led to higher labor supply among childless adults, suggesting that the disenrollees who valued health insurance obtained jobs with health insurance. This may have contributed to higher private insurance coverage rates. In contrast, we find no systematic evidence that private insurance increased as a share of all hospitalizations in our data. This discrepancy may reflect differences in the incidence and morbidity of health conditions between the populations covered in CPS and the NIS.

Given that the underlying health conditions of the population of interest in Garthwaite, Gross and Notowidigdo (2014) are unknown, it is plausible that that their findings are not generalizable to a hospital-care-seeking population that is likely negatively selected in terms of health status. In particular, their estimate of crowd-out for the sub-sample reporting poor health was less than one-third of the size of the estimate for those reporting good health. This finding suggests that the comparatively unhealthy individuals seeking hospital-based care were unlikely to have obtained private insurance coverage after being disenrolled from Medicaid. Notably, in their subsequent analysis using aggregate hospital data, Garthwaite, Gross and Notowidigdo (2015) report a decline in the number of privately insured hospitalizations in 2006. Although their study is limited to one year of post-disenrollment data in contrast to our four years, this result provides indirect evidence in support of our finding that there was no substantial increase in private insurance coverage in the sample of hospital admissions.

One of the advantages of using aggregate hospital data is that it provides a clearer understanding of the spillover effect of TennCare disenrollment on the local healthcare market through its effect on hospital finances. Based on the volume results, not only do we find a reduction in Medicaid admissions after the disenrollment, but we also find an increase in uninsured admissions. To the extent that the direction of this shift from Medicaid to uninsured represents a shift in expected source of payment towards uncompensated care, we conclude that, overall, TennCare disenrollment had a negative impact on hospital finances. Given that the ACA Medicaid expansions aim to reduce uninsurance, the direction of our estimates suggest that hospital uncompensated care costs should decrease in expansion states. This prediction is consistent with recent findings in the literature that the 2010 Medicaid expansion in Connecticut, under the ACA provisions, led to reductions in uncompensated care costs incurred by hospitals in the state (Nikpay, Buchmueller and Levy, 2015).

6. Conclusion

In this paper we present the first estimates of the impact of Tennessee's Medicaid contraction in 2005 on inpatient hospital care utilization among non-elderly adults. By comparing the insurance composition of inpatient admissions in Tennessee to other Southern states using administrative data on a nationwide sample of inpatient hospital stays, we find that the prevalence of uninsurance among hospital admissions increased by nearly 60 percent after the Medicaid contraction. As expected, the prevalence of Medicaid among hospitalizations decreased by about 20 percent. The volume of hospitalizations with Medicaid decreased by 25 percent and uninsured hospitalizations were 60 percent higher. We also find increases in uninsured inpatient admissions that originated in the ED which is not surprising given that the uninsured have difficulty scheduling direct admission to inpatient care. There is also evidence that the contraction reduced the overall volume of hospitalizations, although the effect is statistically insignificant when other Southern states are used as the comparison group. This result is consistent with recent studies that find health insurance increases use of inpatient medical care (Anderson, Dobkin and Gross, 2012, 2014; Antwi, Moriya and Simon, 2015; Card, Dobkin and Maestas, 2008; and Dafny and Gruber, 2005). This finding also supports the possibility that Medicaid may increase hospital care utilization as one mechanism that explains mortality results obtained by Sommers, Baicker and Epstein (2012).

While prior studies find that pre-ACA state Medicaid expansions reduced mortality among non-elderly adults, the mechanism driving this result was unclear. To date, evidence on whether Medicaid increases inpatient utilization among non-elderly adults is inconclusive. Given that the ACA state Medicaid expansions target non-elderly adults, it is valuable to understand how the newly eligible population utilizes medical care. The results from this study suggest that increased use of hospital-based care due to Medicaid coverage expansions may have been a plausible pathway leading to mortality reductions among the non-elderly population. This evidence also suggests that state Medicaid expansions following the ACA guidelines can potentially improve access and utilization in expansion states. Evidence already exists that early ACA provisions for young adults have reduced out of pocket costs for the uninsured (Busch, Golberstein and Meara, 2014), and that pre-ACA state Medicaid expansions decreased personal bankruptcies, plausibly through reductions in out-of-pocket medical expenditures (Gross and Notowidigdo, 2011). Correspondingly, the ACA-related Medicaid expansions may lower out-of-pocket spending, particularly for high-cost hospital care.

Our results also shed light on the potential negative spillover effect that Medicaid disenrollment may have on hospitals through increased uninsured visits. By focusing on a policy change that targeted non-elderly adults (who were neither disabled nor pregnant), our results suggest that Medicaid expansions will decrease uninsured hospitalizations, thereby reducing use of hospital uncompensated care. Back-of-the-envelope calculations suggest that a predicted 12-13 million increase in non-elderly Medicaid and CHIP enrollment due to the ACA (Congressional Budget Office, 2014) will reduce uninsured inpatient stays by 2.9 to 3.1 million each year in the expansion states. Under the assumption that the uninsured utilize inpatient care at the same rate as Medicaid beneficiaries, we divide 17.481, the point estimate of per-10,000 Medicaid inpatient visits, by the pre-treatment mean of 72.259, and multiply by the estimated Medicaid enrollment increase of 12-13 million. As of July 2015, 19 states have decided not to implement the ACA Medicaid expansions; if, following ACA guidelines, reductions in Medicaid and Medicare disproportionate-share hospital (DSH) payments outpace the decrease in uncompensated care costs at hospitals, then hospital finances may be adversely affected in non-expansion states. Given that our analysis is based on a single-state study of a Medicaid contraction, these estimates must be interpreted with caution with respect to the ACA.

The income-based-eligibility approach of Medicaid and Medicaid's re-enrollment policies may lead enrollees to involuntarily drop out of Medicaid over time due to fluctuations in income and employment, which can shift enrollees across income eligibility thresholds. Sommers (2009) estimates that nearly 43 percent Medicaid beneficiaries lose coverage within 12 months of enrollment due to transitions in employment, family structure or income, thereby facing uninsurance. Nonetheless, little is known about the implications of loss of Medicaid coverage for use of medical services or its effect on the healthcare system. We find evidence that the post-disenrollment increase in uninsured hospitalizations was primarily due to an increase in inpatient admissions originating in the ED. This finding provides suggestive evidence of the adverse effect of Medicaid disenrollment on access to medical care. Indeed, the increase in preventable hospitalizations among the uninsured lends further support to the adverse impact of loss of Medicaid coverage on access to care and to the potential negative spill-over effects on hospital finances. In light of these results, ACA Medicaid expansions that reduce uninsurance will likely decrease ED use by the uninsured as well as uninsured admissions for ambulatory-case sensitive conditions in expansion states, thereby reducing hospital uncompensated care costs.

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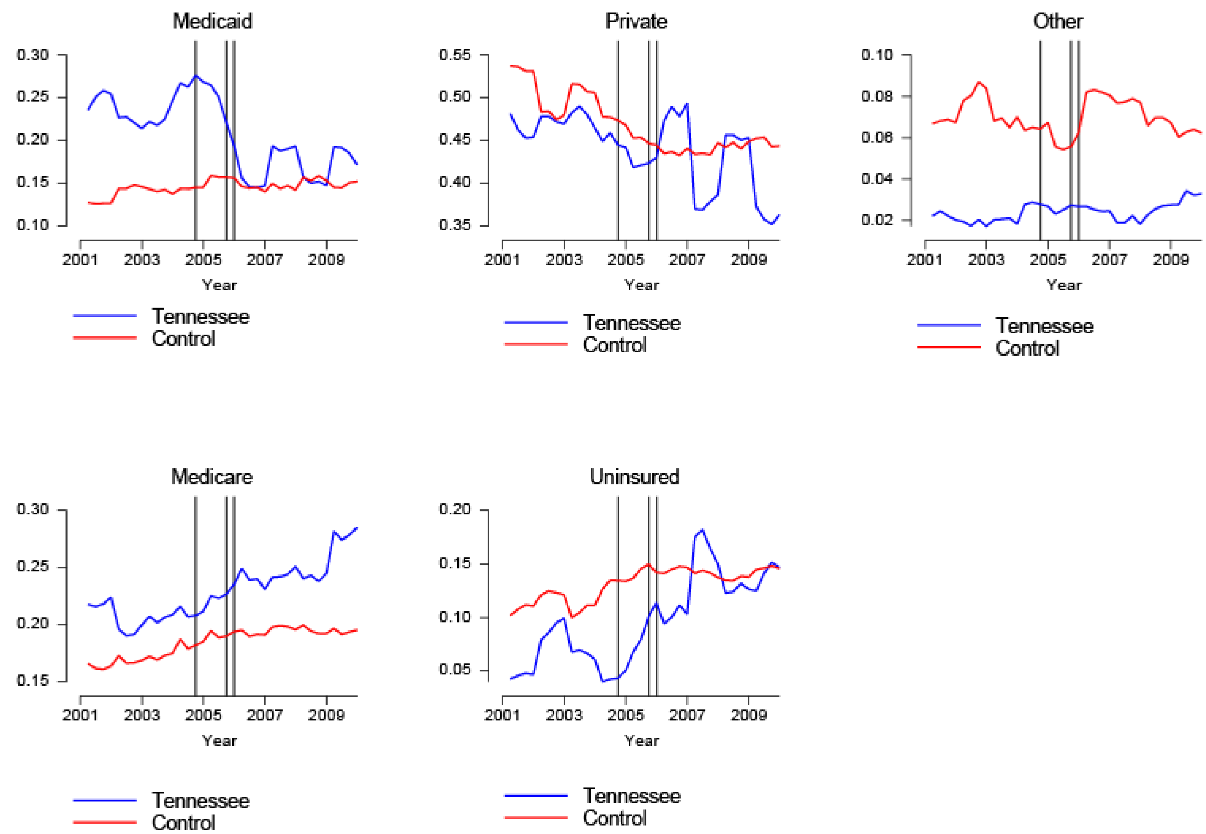
Table 1: Summary Statistics for the Treatment and Comparison States

	Tennessee		Southern States	
	Before	After	Before	After
Demographic characteristics				
Age	47.4	48.1	46.8	47.5
Female	52.7%	51.9%	53.1%	52.2%
White	75.4%	72.5%	48.1%	50.2%
African-American	20.2%	17.8%	16.9%	18.3%
Hispanic	0.6%	1.2%	8.5%	8.7%
Other	2.3%	1.1%	2.9%	3.5%
Clinical characteristics				
Number of diagnosis codes	5.50	7.74	5.45	7.36
Length of Stay (LOS)	4.75	4.81	4.63	4.72
Log(LOS)	2.22	2.23	2.16	2.18
Number of procedure codes	1.60	1.66	1.48	1.62
Health Insurance Status				
Medicaid	24.4%	16.6%	13.8%	15.0%
Uninsured	5.8%	13.0%	12.4%	14.9%
Private	46.6%	43.0%	50.3%	44.0%
Medicare	20.9%	24.9%	16.9%	19.1%
Other Insurance	2.2%	2.6%	6.6%	7.0%
Hospitalization rates by Insurance Type				
Medicaid	5.6	2.5	1.8	1.6
Uninsured	1.3	2.0	1.5	1.5
Private	10.7	6.6	6.6	4.6
Medicare	4.8	3.8	2.2	2.0
Other Insurance	0.5	0.4	0.9	0.8
Total	22.9	15.3	13.1	10.5
Number of Observations	458,664	321,255	5,131,400	4,406,199

Notes: Sample estimates obtained from NIS 2001-2009 for all adults between ages 20-64. We use non-birth admissions only. Demographic characteristics (excluding age) and health insurance status variables are measured as percentages. Hospitalization rates are measured at the state-year level per 1,000 population. Before-period includes years 2000-2004 and after-period represents years 2006 and later. Control states consist of the Southern states in the NIS except Tennessee. See section 5.1 for details.

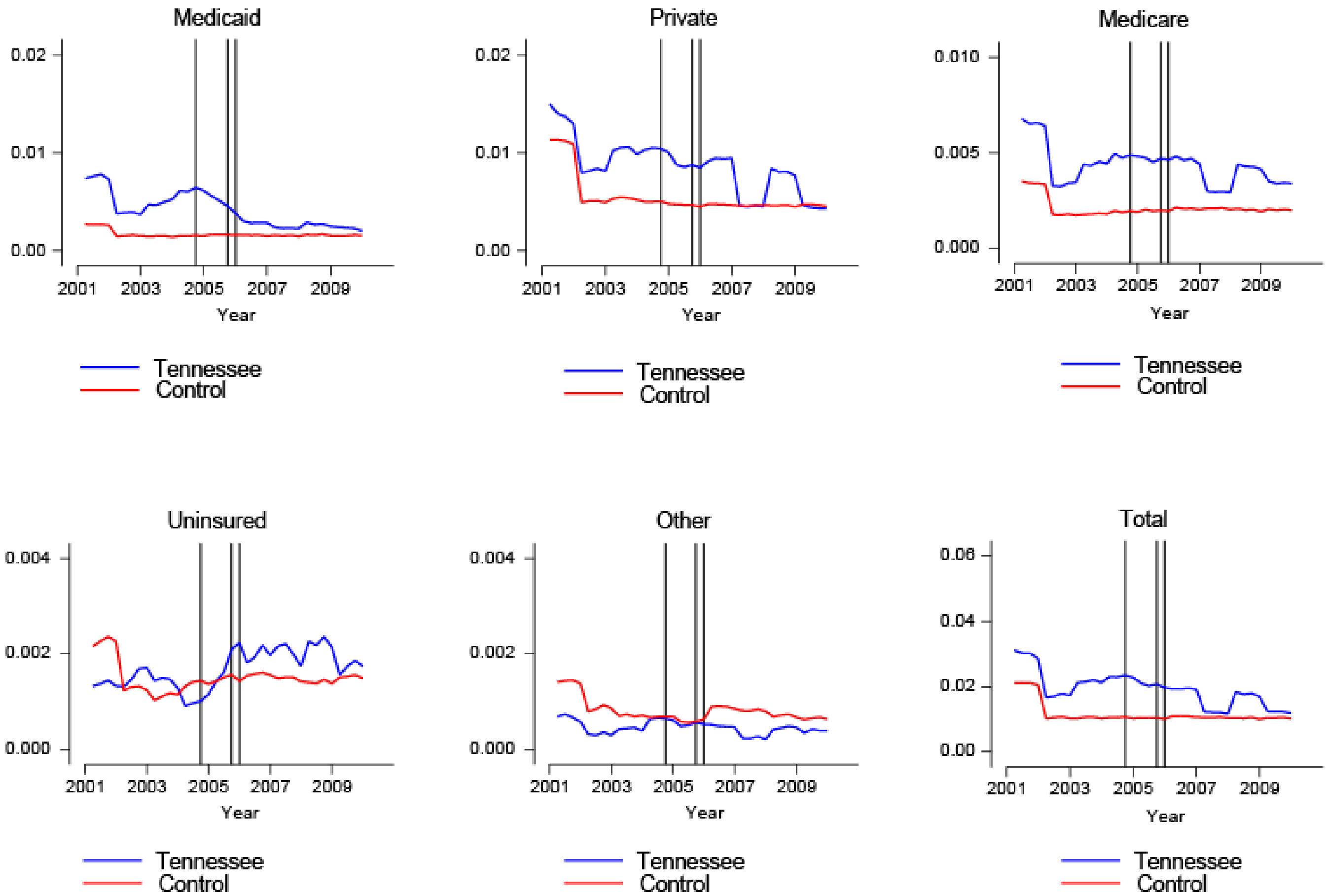
Figure 1: Insurance Status of Inpatient Admissions Among The Non-Elderly



Notes:

1. Source: NIS 2001-2009
2. The first vertical line represents 2004Q4 when TennCare disenrollment was announced, the second vertical line represents 2005Q3 when the disenrollment was implemented and the third vertical line represents 2006Q1 – the beginning of the post-period in our analysis.
3. Sample includes non-pregnancy-related inpatient admissions among 20-64-year-olds.
4. Control states consist of the Southern states in the NIS except Tennessee. See section 5.1 for details.

Figure 2: Hospitalization Rates Among The Non-Elderly (by Insurance Status)



Notes:

1. Source: NIS 2001-2009
2. The first vertical line represents 2004Q4 when TennCare disenrollment was announced, the second vertical line represents 2005Q3 when the disenrollment was implemented and the third vertical line represents 2006Q1 – the beginning of the post-period in our analysis.
3. Sample includes non-pregnancy-related inpatient admissions among 20-64-year-olds.
4. Control states consist of the Southern states in the NIS except Tennessee. See section 5.1 for details.

Table 2: Effect on Inpatient Admissions by Source of Coverage (DD Estimates using Southern States as Comparison Group)

	(1)	(2)	(3)	(4)	(5)
	A. Fraction of Admissions				
	Medicaid	Private	Other	Medicare	Uninsured
PostxTreat	-0.063***	0.007	0.004	0.011***	0.041***
	(0.005)	(0.004)	(0.003)	(0.002)	(0.004)
Dependent Variable Means					
Treatment, Before	0.304	0.349	0.016	0.265	0.067
Control, Before	0.157	0.418	0.065	0.234	0.126
Treatment, After	0.197	0.322	0.021	0.342	0.117
Control, After	0.151	0.373	0.052	0.307	0.118
Observations	11,479	11,479	11,479	11,479	11,479

	(1)	(2)	(3)	(4)	(5)	
	B. Hospitalization Rate					
	Medicaid	Private	Other	Medicare	Uninsured	Total
PostxTreat	-17.481***	-5.454	0.120	-5.012	11.066***	-15.585
	(3.146)	(3.497)	(2.166)	(8.159)	(1.237)	(13.172)
Dependent Variable Means						
Treatment, Before	72.259	139.259	6.911	71.628	20.316	313.692
Control, Before	46.416	145.397	23.148	63.242	34.674	314.018
Treatment, After	56.918	122.881	8.022	104.416	33.399	328.367
Control, After	59.908	125.561	20.079	94.101	35.329	335.777
Observations	11,481	11,481	11,481	11,481	11,481	11,481

Notes: Sample estimates obtained from NIS 2001-2009 for all adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and the Southern states as comparison group. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Table 3: Effect on Source and Type of Uninsured Admissions (DD Estimates using Southern States as Comparison Group)

	(1)	(2)	(3)	(4)
	Dependent Variable: Uninsured Hospitalization Rate			
	Uninsured Admissions through ED	Uninsured Admissions not through ED	Uninsured Admissions for Preventable Conditions	Uninsured Admissions for Non-Preventable Conditions
PostxTreat	8.391***	2.686***	1.993***	9.073***
	(1.066)	(0.357)	(0.242)	(1.027)
Dependent Variable Means				
Treatment, Before	13.030	7.267	3.547	16.769
Control, Before	23.873	10.684	5.788	28.886
Treatment, After	23.992	9.355	4.762	28.637
Control, After	24.512	10.664	5.108	30.222
Observations	11,481	11,481	11,481	11,481

Notes: Sample estimates obtained from NIS 2001-2009 for full sample of adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and the Southern states as comparison group. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Table 4: Effect on Intensity of Treatment

(DD Estimates using Southern States as Comparison Group)

	(1)	(2)	(3)	(4)	(5)	(6)
	Intensity (All Admissions)			Intensity (Uninsured Admissions Only)		
	Number of Procedures	Length of Stay (LOS)	Log of LOS	Number of Procedures	Length of Stay (LOS)	Log of LOS
PostxTreat	-0.034	-0.036	0.002	-0.009	0.100**	0.019***
	(0.029)	(0.033)	(0.004)	(0.013)	(0.033)	(0.003)
Dependent Variable Means						
Treatment, Before	0.944	3.999	1.385	0.870	3.380	1.260
Control, Before	1.073	4.932	1.449	0.897	3.841	1.309
Treatment, After	1.006	4.123	1.404	0.892	3.449	1.285
Control, After	1.154	5.813	1.518	0.917	3.906	1.305
Observations	11,481	11,481	11,481	9,930	9,930	9,930

Notes: Sample estimates obtained from NIS 2001-2009 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and the Southern states as comparison group. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Appendix

Table A1: Pre-Treatment Trend Test (Southern States as Comparison Group)

	(1)	(2)	(3)	(4)	(5)
	A. Dependent Variable: Fraction of Admissions				
	Medicaid	Private	Other	Medicare	Uninsured
TrendxTreat	0.000	0.000	0.001	0.001	-0.002
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)
Observations	5,250	5,250	5,250	5,250	5,250

	(1)	(2)	(3)	(4)	(5)
	B. Dependent Variable: Hospitalization Rate				
	Medicaid	Private	Other	Medicare	Uninsured
TrendxTreat	-0.347	3.236	1.097*	0.329	0.620***
	(0.454)	(2.191)	(0.499)	(0.474)	(0.175)
Observations	5,252	5,252	5,252	5,252	5,252

Notes: Sample estimates obtained from NIS 2001-2004 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and the Southern states as comparison group. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01.

Table A2: Alternative Difference-in-Differences Model Specifications

Dependent Variable: Fraction of Inpatient Admissions

		(1)	(2)	(3)	(4)	(5)	(6)
		Dependent Variable: Fraction of Inpatient Admissions					
	Interaction Term	Medicaid	Private	Other	Medicare	Uninsured	N
A. Difference-in-difference estimates using all states as control	*PostxTreat*	-0.071***	0.012***	0.004*	0.011***	0.044***	29,867
		(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	
B. Difference-in-difference estimates using synthetic control states (level)	*PostxTreat*	-0.046***	0.001	0.007	0.014**	0.025***	6,734
		(0.013)	(0.009)	(0.004)	(0.006)	(0.007)	
C. Difference-in-difference estimates using synthetic control states (growth rate)	*PostxTreat*	-0.051***	0.001	-0.002	0.016**	0.036***	4,687
		(0.018)	(0.009)	(0.004)	(0.008)	(0.010)	
D. Difference-in-difference using Southern states as control, includes state-specific linear time trends	*PostxTreat*	-0.049***	-0.005	0.007**	0.015***	0.031***	11,048
		(0.012)	(0.004)	(0.003)	(0.004)	(0.007)	
E. Difference-in-difference using Southern states as control, includes state-specific squared time trends	*PostxTreat*	-0.047***	-0.004	0.004	0.015***	0.032***	11,048
		(0.013)	(0.004)	(0.003)	(0.004)	(0.009)	
F. Difference-in-difference using Southern states as control, includes state-specific cubic time trends	*PostxTreat*	-0.050***	0.000	0.003	0.014***	0.034***	11,048
		(0.012)	(0.005)	(0.003)	(0.003)	(0.009)	

Notes: Sample estimates obtained from NIS 2001-2009 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and exclude year 2005 from sample. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Table A3: Alternative Triple-Differences Model Specifications

Dependent Variable: Fraction of Inpatient Admissions

		(1)	(2)	(3)	(4)	(5)	(6)
		Dependent Variable: Fraction of Inpatient Admissions					
	Interaction Term	Medicaid	Private	Other	Medicare	Uninsured	N
A. Triple-difference estimates using Southern states	*PostxTreatxOver20*	-0.075***	0.009*	0.005	0.012***	0.049***	21,903
		(0.007)	(0.005)	(0.004)	(0.003)	(0.004)	
B. Triple-difference estimates using Southern states	*PostxTreatxNon-birth*	-0.073***	0.010*	0.004	0.014***	0.045***	19,068
		(0.005)	(0.005)	(0.004)	(0.002)	(0.004)	
C. Triple-difference estimates using all states	*PostxTreatxOver20*	-0.084***	0.020***	0.005**	0.008***	0.050***	57,779
		(0.005)	(0.004)	(0.002)	(0.003)	(0.002)	
D. Triple-difference estimates using all states	*PostxTreatxNon-birth*	-0.080***	0.020***	0.004**	0.009***	0.046***	52,253
		(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	

Notes: Sample estimates obtained from NIS 2001-2009. Each coefficient estimate represents a separate regression. We exclude year 2005 from sample. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01.

Table A4: Alternative Difference-in-Differences Model Specifications

Dependent Variable: Hospitalization Rate

		(1)	(2)	(3)	(4)	(5)	(6)
		Dependent Variable: Hospitalization Rate					
	Interaction Term	Medicaid	Private	Other	Medicare	Uninsured	N
A. Difference-in-difference estimates using all states as control	*PostxTreat*	-17.677***	-8.055***	-0.022	0.612	11.470***	29,881
		(1.881)	(2.175)	(0.768)	(3.722)	(0.747)	
B. Difference-in-difference estimates using synthetic control states (level)	*PostxTreat*	-19.839***	-22.733***	2.016	-14.774**	8.127***	6,736
		(4.820)	(6.156)	(1.559)	(5.701)	(2.120)	
C. Difference-in-difference estimates using synthetic control states (growth rate)	*PostxTreat*	-18.270***	-20.032***	-0.933	-8.966**	7.773***	4,689
		(5.501)	(7.338)	(0.664)	(4.246)	(2.437)	
D. Difference-in-difference using Southern states as control, includes state-specific linear time trends	*PostxTreat*	-13.332*	-8.179	-4.470	3.105	11.194***	11,050
		(6.970)	(8.051)	(3.034)	(12.111)	(3.548)	
E. Difference-in-difference using Southern states as control, includes state-specific squared time trends	*PostxTreat*	-12.326*	-5.738	-4.358	7.647	10.710**	11,050
		(5.993)	(6.656)	(3.882)	(10.522)	(3.473)	
F. Difference-in-difference using Southern states as control, includes state-specific cubic time trends	*PostxTreat*	-12.493**	-3.875	-3.096	10.803	10.196***	11,050
		(4.374)	(5.555)	(3.510)	(8.293)	(2.775)	

Notes: Sample estimates obtained from NIS 2001-2009 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Table A5: Alternative Triple-Differences Model Specifications

Dependent Variable: Hospitalization Rate

		(1)	(2)	(3)	(4)	(5)	(6)
		Dependent Variable: Hospitalization Rate					
	Interaction Term	Medicaid	Private	Other	Medicare	Uninsured	N
A. Triple-difference estimates using Southern states	*PostxTreatxOver20*	-19.895***	-5.788	0.862	-6.055	11.556***	21,910
		(2.005)	(3.736)	(2.474)	(5.768)	(1.627)	
B. Triple-difference estimates using Southern states	*PostxTreatxNon-birth*	-24.222***	-6.630*	0.524	-4.855	11.476***	19,074
		(1.775)	(3.220)	(2.602)	(5.279)	(1.718)	
C. Triple-difference estimates using all states	*PostxTreatxOver20*	-19.684***	-9.735***	0.196	2.027	12.327***	57,779
		(1.005)	(2.654)	(0.915)	(3.442)	(0.867)	
D. Triple-difference estimates using all states	*PostxTreatxNon-birth*	-22.875***	-10.286***	-0.015	2.200	12.584***	52,276
		(0.946)	(2.646)	(0.994)	(3.382)	(0.916)	

Notes: Sample estimates obtained from NIS 2001-2009. Each coefficient estimate represents a separate regression. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01.

Table A6: Alternative Difference-in-Differences Model Specifications

Dependent Variable: Uninsured Hospitalization Rate

		(1)	(2)	(3)	(4)	(5)
		Dependent Variable: Uninsured Hospitalization Rate				
	Interaction Term	Uninsured ED	Uninsured non-ED	Uninsured ACSC	Uninsured non-ACSC	N
A. Difference-in-difference estimates using all states as control	*PostxTreat*	9.382*** (0.581)	2.127*** (0.288)	1.711*** (0.137)	9.759*** (0.652)	29,881
B. Difference-in-difference estimates using synthetic control states (level)	*PostxTreat*	7.192*** (1.913)	0.949*** (0.302)	1.033*** (0.370)	7.094*** (1.870)	6,736
C. Difference-in-difference estimates using synthetic control states (growth rate)	*PostxTreat*	6.620*** (2.126)	1.167*** (0.423)	0.760 (0.462)	7.013*** (2.067)	4,689
D. Difference-in-difference using Southern states as control, includes state-specific linear time trends	*PostxTreat*	9.221*** (2.693)	1.918* (0.964)	1.756** (0.618)	9.438*** (2.944)	11,050
E. Difference-in-difference using Southern states as control, includes state-specific squared time trends	*PostxTreat*	8.874*** (2.793)	1.781* (0.853)	1.712** (0.610)	8.998*** (2.885)	11,050
F. Difference-in-difference using Southern states as control, includes state-specific cubic time trends	*PostxTreat*	8.557*** (2.358)	1.596** (0.718)	1.725*** (0.512)	8.471*** (2.292)	11,050

Notes: Sample estimates obtained from NIS 2001-2009 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Table A7: Alternative Triple-Differences Model Specifications

Dependent Variable: Uninsured Hospitalization Rate

		(1)	(2)	(3)	(4)	(5)
		Dependent Variable: Uninsured Hospitalization Rate				
	Interaction Term	Uninsured ED	Uninsured non-ED	Uninsured ACSC	Uninsured non-ACSC	N
A. Triple-difference estimates using Southern states	*PostxTreatxOver20*	8.966***	2.590***	1.888***	9.667***	21,910
		(1.456)	(0.435)	(0.194)	(1.484)	
B. Triple-difference estimates using Southern states	*PostxTreatxNon-birth*	8.903***	2.588***	1.794***	9.682***	19,074
		(1.615)	(0.351)	(0.237)	(1.514)	
C. Triple-difference estimates using all states	*PostxTreatxOver20*	10.142***	2.169***	1.736***	10.591***	57,779
		(0.692)	(0.319)	(0.136)	(0.760)	
D. Triple-difference estimates using all states	*PostxTreatxNon-birth*	10.281***	2.300***	1.649***	10.935***	52,276
		(0.769)	(0.297)	(0.151)	(0.793)	

Notes: Sample estimates obtained from NIS 2001-2009. Each coefficient estimate represents a separate regression. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, **significant at 0.05, *** significant at 0.01.

Table A8: Pre-Treatment Trend Test (All States as Comparison Group)

	(1)	(2)	(3)	(4)	(5)
	A. Dependent Variable: Fraction of Admissions				
	Medicaid	Private	Other	Medicare	Uninsured
TrendxTreat	-0.000	0.001	0.000	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Observations	14,284	14,284	14,284	14,284	14,284

	(1)	(2)	(3)	(4)	(5)
	B. Dependent Variable: Hospitalization Rate				
	Medicaid	Private	Other	Medicare	Uninsured
TrendxTreat	-0.784***	1.312	0.551**	-0.443	0.484*
	(0.252)	(1.230)	(0.255)	(0.289)	(0.273)
Observations	14,286	14,286	14,286	14,286	14,286

Notes: Sample estimates obtained from NIS 2001-2004 for full sample of adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and all states as comparison group. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01. Massachusetts and Missouri have been excluded.

Table A9: Effect on Total Volume of Hospitalizations (DD estimates)

A. South States - Main specification

	Total (DD)		Total (Pre-Trend Test)
PostxTreat	-15.585	*TrendxTreat*	4.660
	(13.172)		(3.419)
Observations	11,481		5,252

B. All StatesA9

	Total (DD)		Total (Pre-Trend Test)
PostxTreat	-12.704**	*TrendxTreat*	0.808
	(5.473)		(2.021)
Observations	29,881	Observations	14,286

C. Triple-difference estimates

	Southern states as control		All states as control
PostxTreatxOver20	-18.704**	*PostxTreatxOver20*	-14.523***
	(8.245)		(4.505)
Observations	21,910	Observations	57,779
PostxTreatxNon-birth	-23.122**	*PostxTreatxNon-birth*	-18.096***
	(7.833)		(4.885)
Observations	19,074		52,276

Notes: Sample estimates obtained from NIS 2001-2009. Each coefficient estimate represents a separate regression. Total volume of hospitalizations is measured at the hospital-quarter level per 10,000 population. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01. Massachusetts and Missouri have been excluded.