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JOB MARKET PAPER

Effects of Losing Public Health Insurance on Healthcare Access, Utilization and Health Outcomes: Evidence from the TennCare Disenrollment

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Abstract

An extensive literature in economics has studied the effects of gaining public health insurance eligibility on health outcomes. In contrast, not much is known of the effects of losing public health insurance eligibility on health. This paper is the first to comprehensively study the effects of one of the largest public health insurance disenrollments in the U.S. on access to care, utilization of medical care and preventive care, and self-assessed health. The disenrollment was part of a 2005 reform to Tennessee's Medicaid program (TennCare) in which 170,000 residents – mainly non-elderly childless adults – lost public health insurance eligibility due to budget cuts. Using data from the 2000-2010 Behavior Risk Factor Surveillance System (BRFSS) and restricted-use versions of the 2000-2010 National Health Interview Survey with state identifiers, I compare differences in outcomes between childless adults and other adults in Tennessee with the associated differential for these two groups across other Southern states, before and after the reform. I confirm that the 2005 TennCare disenrollment significantly decreased overall health insurance coverage, and I provide the first evidence that the disenrollment significantly increased the likelihood of reporting forgone and delayed medical care due to cost and decreased the number of visits to a primary care physician. I also document increases in the number of days with bad health. Finally, I provide evidence of changes to patients' place of care and increases in Emergency Department visits. I do not find consistent evidence of effects for preventive care, although I do find suggestive evidence of increases in healthy behaviors. Overall the effects of the reform are concentrated among less educated childless non-elderly adults. These findings have potentially important implications for recent state public insurance expansions that are part of federal health care reform.

Keywords: Public Health Insurance, Medicaid, TennCare, Disenrollment, Access to Care

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1. Introduction

There is an extensive literature in health economics that explores the effects of public health insurance eligibility on outcomes of health and access to health care (Buchmueller et al. 2015; Finkelstein et al 2014; Carrie and Gruber, 1999; Kolstad and Kowalksi, 2012). However, most of what we know of the relationship between health insurance and health comes from empirical investigations of people gaining public health insurance. There has been relatively less research done on the effects of *losing* public health insurance on health, mostly due to lack of exogenous events that make cause people lose public health insurance eligibility. This paper is the first comprehensive study of the effects of losing public health insurance on population health outcomes using a quasi-experimental design. Specifically, I consider the effects of one of the largest public health insurance disenrollments in the U.S.: the 2005 Tennessee disenrollment in which approximately 170,000 residents were dropped from the state's Medicaid program, TennCare. This reform targeted non-elderly childless adults, an understudied population in the health insurance literature. This population is of particular interest since most of the recent Affordable Care Act (ACA) expansions target childless adults.[1]

Theoretically, predictions regarding the effects of losing health insurance on health are not necessarily symmetric to the predictions regarding the effects of gaining health insurance. The main difference relies on the accumulation of health capital: individuals who have had

[1] I define childless adults as adults who report having no children under 18 years-old living in their household. Using family relationships within household I am also able to identify within household adults with dependents and adults without dependents.

health insurance for an extended period of time could have a greater level of health capital than a person who has not had health insurance. For instance, consider a diabetic woman who has had health insurance for an extended period of time. During this time she has been able to learn that she has a chronic condition, the degree of the problem, and how to handle it. She has received information about the importance of an adequate diet and she may have had access to prescription drugs. Once this person loses health insurance, even though her health care access is reduced, she does not lose the information she has on her health condition. In contrast, consider the same woman who starts out without health insurance. In that case, it is likely that she would not have been able to obtain as much information on her health condition during her uninsured spell. If she gains health insurance, not only her health care access will increase but she may also experience large and immediate information gains. These and other examples illustrate the possibility of asymmetries in the effects of losing and gaining health insurance.

While the few investigations of the effects of losing health insurance have focused on one particular health related outcome, this paper studies a broad range of health outcomes. First I study people's decisions to go to the doctor and their rates of preventive care utilization.[2] I consider this to be the primary mechanism through which losing health insurance may affect health. Second, I study how losing health insurance affects self-rated health status and the number of reported sick days. I also consider the effects on where people choose to

[2] To this point there are two studies that examined the effect of the TennCare reform on a health related outcome: Hearvin et al. (2011) and Ghosh and Simon (2015). Heavrin et al. (2011) evaluate evaluate the effects of Tennessee's disenrollment on Emergency Department visits. Ghosh and Simon (2015) evaluates the effects of the disenrollment on hospitalizations. There have also been reports by the Robert Wood Johnson Foundation (Farrar et al., 2007) that describe through anecdotal evidence and interviews the effects of the disenrollment on individual's heath status.

obtain medical care and their total demand for care. Finally, I am also able to study changes in risky and non-risky health behaviors to identify presence of moral hazard.

In order to answer these questions, I use an exogenous reform that caused people to lose public health insurance. In 2005, Tennessee underwent a major Medicaid cutback, in which approximately 170,000 residents lost public health insurance eligibility. Recent research has examined the effects of this reform on labor supply (Garthwaite et al. 2014), hospital uncompensated care (Garthwaite et al. 2015) and inpatient hospitalizations (Ghosh and Simon, 2015).[3] The cutbacks were made on the 1994 TennCare Reform, which had expanded eligibility for public health insurance to non-traditional Medicaid beneficiaries. This expansion group was mostly composed of non-elderly childless adults and people who were considered "uninsurable."[4]

In doing so, the 2005 reform targeted a particular subpopulation that has been understudied in the health insurance literature: childless adults. At least half of the uninsured adult population in the United States is composed of childless adults. These individuals are 19 to 64-year-olds who are commonly lower income, less educated, and either work for an employer that does not provide health insurance or do not work enough hours to qualify for benefits (ASPE, 2005). This population constitutes a large portion of the population that would be affected by numerous Medicaid expansions under the Affordable Care Act (ACA) that aim to close the health coverage gap between individuals who are not poor enough to

[3] An inpatient is a patient that had a doctor recommend to stay at least one night in the hospital.
[4] This term refers to people who have been previously denied private health insurance.

qualify for Medicaid but not wealthy enough to purchase private health insurance.[5] Therefore, if any future cutbacks target the most recent expansions, childless adults may be the first group to lose coverage.[6]

My empirical strategy uses the sharp state-specific timing of the disenrollment combined with the fact that it mostly targeted childless adults to obtain inference on the effects of losing public health insurance eligibility on health care access, utilization of care and health outcomes. The first approach is a straightforward Difference-in-Differences (DD) model that compares residents of Tennessee to those of other southern states before and after the disenrollment. The second approach uses a triple difference (DDD) model to take advantage of the fact that the vast majority of individuals who lost eligibility during the reform were childless adults. Garthwaite et al. (2014) estimate that 91% of those affected by the disenrollment were adults without dependents under the age of 18. I compare the differential in outcomes of adults with and without children in Tennessee to the associated difference for the same groups in other southern states before and after the reform. In addition, given the single-state nature of my treatment, to account for state specific shocks I use synthetic control methods to corroborate my findings (Abadie et al. 2010).

I estimate that the TennCare disenrollment significantly decreased the likelihood of having health insurance between 2 and 5 percent. I provide evidence of decreases in health care access; specifically, I estimate an increase in the likelihood of forgone or delay medical

[5] Estimates range from 15 to 20 million of individuals covered by the ACA Medicaid Expansions. (Kenney et al., 2012)
[6] In 2012, the U.S. Supreme Court overturned the provision of the law requiring Medicaid expansions, leaving the decision up to each state. Since then, a considerable number of states have decided not to use federal money to expand Medicaid programs. As of March 2013, 17 states opposed Medicaid Expansion (Kaiser Report, 2013).

care due to cost of at least 10 percent and a decrease in the likelihood of seeing a general doctor of 4 percent. This serves as a mechanism to understand the decreases in health status. I estimate that the reported number of days with bad health over 12 months increased by 0.6 days, out of a mean of 5 days, and the number of days incapacitated increased by 0.84 days, over a mean of 4.7 days.

In terms of demand for medical care, I provide evidence that the likelihood of people to change their place of care due to health insurance reasons increases by almost half out of a mean of 3 percent. This effect is larger for low educated individuals, who experience a 115 percent increase. Relatedly, I find that, after the reform, this group is less likely to report the doctor's office or HMO provider as their source of usual care and is more likely to report an Emergency Department (ED), hospital outpatient department or a clinic as their source of usual care. In terms of health care utilization, I show that the likelihood of going to an Emergency Department increases by 7 percent along the intensive and extensive margins. I also find a 20 percent decrease in the number of surgeries and the likelihood of having a surgery.

In terms of inpatient stays, using survey data I find a 10 percent decrease in the number of times a patient has stayed overnight in a hospital. Using administrative data I find a 40 percent decrease in the number of discharges per hospital quarter for the non-elderly. I also I find a significant 20 percent reduction in the payments coming from Medicaid and a 30 percent increase in the payments coming from the patient. These results are larger for individuals with a high school degree or less and they are robust to the choice of alternative control groups as well as inference adjustment that accounts for the single state nature of the reform. I also find suggestive evidence of the presence of moral hazard: I estimate an 8 percent increase in the likelihood of getting a flu shot and engagement in healthier behaviors.

My paper contributes to the literature in the following ways: First, I provide the literature's first comprehensive evidence on the population health effects of losing public health insurance eligibility using a quasi-experimental design. Second, I investigate possible mechanisms of how changes in health insurance status can affect health, and in doing so I provide evidence of how people's decisions regarding health care and health behaviors changed after the disenrollment. Third, part of the mixed evidence of public health insurance eligibility effects on healthcare utilization comes from analyzing different types of data: survey data versus administrative data. In my paper, I use both types of data. I provide evidence from two population representative surveys and one administrative dataset on inpatient hospitalizations. Furthermore, having numerous datasets allows me to study the reform in a comprehensive way by investigating not only health care access but also changes in preventive care, health behaviors, health care utilization and health status.

In addition to these contributions, this paper is important for policy-makers since it provides evidence on a particular population of interest: childless adults. This population is the target of the recent ACA Medicaid expansions which have recently met significant opposition, and their future is highly contingent upon political and economic environments. Especially since a considerable number of states have opted to depend on state funding rather than federal funding to comply with the ACA. Even if most of the ACA mandates are not repealed, it is not unreasonable to expect that budget deficits could drive states to enact public health insurance cutbacks similar to the 2005 disenrollment in Tennessee.

The rest of the paper proceeds in the following manner. Section 2 describes the existing literature on the effects of changing public health insurance eligibility on health. Section 3 provides institutional background on the 2005 TennCare reform. Section 4 explains the empirical strategy. Section 5 describes the data. Section 6 presents the results, and Section 7 offers a discussion and conclusions.

2. Literature Review

In this section I review the literature on the effects of policy-induced changes in health insurance on health outcomes, with a focus on studies examining public health insurance eligibility.

2.1. Studies on the effects of gaining insurance coverage on health

A large literature in economics has examined the effects of obtaining public health insurance eligibility on health outcomes. I focus here on papers with populations similar to the one I study: namely, non-elderly childless adults.[7]

Recently, two health care reforms have received a significant amount of attention: the 2008 Oregon Medicaid Lottery and the 2006 Massachusetts health insurance reform. Both of these reforms mostly affected non-elderly adults. In fact, it is estimated that around 56 percent of the people affected by Oregon Lottery were childless adults while 50 percent of people affected by the MA health reform were childless adults (Garthwaite et al., 2014).

[7] I do not review a large literature that has studied policy induced changes in public health insurance eligibility for different target populations such as: Medicaid expansions for pregnant women (Currie and Gruber, 1996b) and infants (Currie and Gruber, 1996a; Dafny and Gruber, 2005), or obtaining coverage through Medicare for the elderly (Card, Dobkin and Maestas, 2004; 2009; Finkelstein and McKnight, 2008). Buchmueller et al (2015) summarizes the main findings from the extensive literature of the effects of the Medicaid program on a variety of economic and health outcomes.

There are three main papers that estimate the effects of the Oregon Medicaid Lottery on health outcomes: Finkelstein et al. (2012), Baicker et al. (2013) and Taubman et al. (2014). These studies provide evidence from survey data and administrative data on the effects of the Oregon Medicaid Lottery in which some individuals were randomly selected to gain Medicaid eligibility. From survey data the studies found that outpatient visits increased by 35 percent and the likelihood of having a prescription filled increased by 15 percent. They also document increases in preventive care: namely cholesterol tests, blood tests for diabetes, mammograms, and Pap tests. Nevertheless, they did not find changes in diagnoses for any of the conditions that were associated with the changes in preventive care. They also find increases in self-assessed measures of health but did not find evidence of changes in ER utilization or inpatient stays.[8] In contrast, using administrative data to study the intervention showed that inpatient admissions increased by 30 percent while ER visits increased by 40 percent over an 18 month period.[9]

The impact of the Massachusetts health reform of 2006 on adult health has been extensively studied. This reform expanded Medicaid while at the same time creating incentives to obtain private health insurance. Most of these papers use a Difference-in-Difference strategy to compare outcomes in Massachusetts before and after the reform with the associated changes in outcomes for individuals in other states. They find evidence that the Massachusetts reform increased health coverage by about 6 percent (Kolstad and Kowalski, 2012; Long et al., 2009), which consequently increased access to care (Long et al., 2014), breast and cervical cancer

[8] The authors conjecture that the increases in self-reported ratings of health can be mostly explained by the reductions in financial distress.
[9] They report that the increase in inpatient stays is mostly not originating from the ED.

screenings three years after the implementation (Sabik et al., 2015) and self-assessed ratings of health (Courtemanche et al., 2014). Miller (2012) and Long et al. (2012) found a reduction in ED utilization between 5 and 8 percent. Finally, Kolstad and Kowalsi (2012) found no evidence of changes in inpatient admissions but they do document a decline in inpatient admissions originating from the ED.

There are other less studied Medicaid expansions from Wisconsin, New York, Maine and Arizona, each with different target populations and unique aspects of the expansions. DeLeire et al. (2013) and Burns et al. (2014) study the Wisconsin Medicaid expansion that occurred in 2003 and allowed approximately 9,000 residents to gain health insurance. This expansion was targeted at low-income, uninsured and non-elderly adults with chronic health conditions. Both studies used administrative claims data from 2008-2009. Burns et al. focus on the population of rural adults while DeLeire et al. (2013) focus on adults from all areas.[10] DeLeire et al. (2013) found that outpatient visits increased by 29 percent, emergency department visits increased by 46 percent, inpatient hospitalization decreased by 59 percent and preventable hospitalizations decreased by 48 percent. Burns et al. (2014) found that obtaining public health insurance eligibility increased the likelihood of outpatient visits by 39 percent, preventative services by 93 percent (i.e. physical check-ups, health education, and smoking cessation), and inpatient visits by 124 percent.[11] The expansions from New York,

[10] DeLeire et al. (2013) use an individual fixed effect model to identify changes in outcomes within individuals over time, while Burns et al. use a regression discontinuity method to compare individuals who enrolled in the public health insurance program right before and after the date of last enrollment, which was an unforeseen date since the enrollment was supposed to continue after that date.

[11] In both cases, their sample is not representative of the average uninsured person. In DeLeire et al. (2013), the authors do not have a control group made of individuals who did not gain coverage. This means that part of their estimated effect might be driven by reasons unrelated to changes in health insurance coverage.

Maine and Arizona were studied by Sommers et al. (2012). They compared the expansions in these states to neighboring states and found that Medicaid coverage increased by 2.2 percentage points and that the expansions were associated with a reduction in all-cause mortality for older, non-white, lower income individuals. They also find reduced rates of delayed care and increases in "excellent" and "very good" ratings of self-assessed health.

Another recent study examines the effects of an insurance expansion for childless adults, despite that it is not a public health insurance expansion per se. Barbaresco et al. (2014) use a provision from the ACA (in effect since September 2010) which extends the permissible age for individuals to remain under their parents' health insurance plan to age 26. They use a difference-in-difference approach in which the treatment group is composed of 23-25 year-olds (right below the age cutoff) and the control group is made up of 27-29 year-olds. The authors found that this mandate increased the likelihood of having health insurance, having a primary care doctor, and reporting excellent health. They also found that the provision decreased the likelihood of being unable to afford medical care and receiving a flu vaccine.

2.2. Studies on the effects of losing insurance coverage on health

In contrast to the numerous studies of gaining public insurance eligibility, I am aware of no published work in economics on the health effects of losing public health insurance eligibility.[12] In a recent working paper, Ghosh and Simon (2015) use the same TennCare

[12] Recently, Garthwaite et al. (2014) studied the effects of the 2005 TennCare disenrollment on employment and labor force participation. Using the Current Population Survey, they found that the reform was associated with a 4.6 percentage point increase in employment for childless adults. This effect was stronger for jobs providing employer health insurance and for individuals working more than 20 hours a week. Their results suggest that if individuals were able to obtain health insurance independently from their employers, some of them would leave their jobs, work less hours, or exit the labor force. In addition in Garthwaite et al. (2015), the authors used the Tennessee reform to study the effects on the

reform I study here and investigate its effects on inpatient hospitalizations. They find that that the disenrollment decreased the share of hospitalizations covered by Medicaid by 21 percent. They also find a 75 percent increase in the uninsured hospitalizations originating from emergency department visits. They report that uninsured hospitalizations increased for both avoidable and unavoidable conditions, which does not suggest lack of preventive care. They find suggestive evidence of decreases in inpatient stays. This research complements my findings on the effects of the disenrollment; I not only study the effects of the disenrollment on the sample of inpatient hospitalizations but also provide evidence of the effects for the overall population using two population-based representative datasets.

In the medical and health policy fields, there are a several additional studies on people losing health insurance. Hearvin et al. (2011) compared emergency department (ED) visits in Tennessee before and after the disenrollment controlling for state linear and non-linear trends. Using administrative data from hospitals, they found that the overall number of outpatient visits decreased while the share of uninsured individuals visiting EDs increased. In my paper, I find increases in the number of visits to the ED as opposed to decreases. Since they do not provide a control group to compare Tennessee's outcomes, it is possible that their estimated effect reflects both changes from the TennCare reform and the regional trend decline in ED visits that was occurring around the same time.

Lurie et al. (1984; 1986) explore the effects of a contraction of California's Medicaid expansion program in 1982. California cut public health insurance eligibility for 270,000

disenrollment on uncompensated care provided by hospitals. They found that the disenrollment caused an increase of $138 million dollars in uncompensated care.

medically indigent residents and transferred the funds to subsidize the medically indigent's cost of care in county health care facilities'. However, counties were not obliged to provide free care. Lurie et al. (1986) perform a survey of 215 individuals, of which 186 were affected by the disenrollment and rest were part of a control group. They found that the population affected by the disenrollment had higher levels of uncontrolled hypertension and lower access to care six months after the disenrollment.

Oregon went through a reform in 2003 that was similar to the one in Tennessee. The reform included a stricter premium payment policy, cutbacks on benefits, increases in premiums, and the introduction of co-payments resulting in individuals losing their public coverage. Carlson, et al. (2011) studied the effects of this reform. They collected their own survey data, eight and ten months after the reform.[13] They found that 31 percent of respondents reported losing public coverage and remaining uninsured, while another 15 percent reported continued disrupted coverage. Those who remained uninsured were less likely to have a primary care visit and more likely to report unmet health care needs than those who had continuous coverage.[14] A potential concern with this study is that technically the state did not terminate eligibility. Individuals chose to leave the program, which implies that the comparison groups could have unobserved characteristics that are correlated with the health outcomes under study, thus potentially biasing the estimated effects.

In 2005, Missouri also undertook a health reform that involved Medicaid cutbacks. This reform resulted in approximately 100,000 residents losing Medicaid coverage while others

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[13] They used the data to compare three groups: those who were not affected by the reform, those who lost it but reacquired it, and those who lost it and remained uninsured through their period of analysis.
[14] Those with disrupted insurance coverage had similar effects which were smaller in magnitude.

faced reduced benefits and higher cost-sharing. Zucherman et al. (2009) studied this reform using a combination of administrative data and interviews with providers and managers. Comparing outcomes before and after the reform (i.e. a single differences) they found an increase in the number of uninsured, an increase in uncompensated hospital care and a decrease of hospital revenues.[15]

There are also some relevant studies on the effects of losing health insurance that are not about losing public health insurance per se. For example, Anderson et al. (2012, 2014) have a two papers that studied individuals aging out of their parents' health insurance plans at the ages of 19 and 23. In both cases, they found a decrease in ED visits, with a larger effect on the older group. For the younger group, losing health coverage led to a 40 percent reduction in ED visits. For the older group, it led to an approximately 88 percent reduction. They explain that the disparity is due to the fact that individuals at age 19 have lower socioeconomic status which makes them more likely to be covered by a means-tested program while those at age 23 are typically not in school and are not working in jobs that provide health insurance.

Overall, the existing literature on the health effects of public insurance eligibility expansions has found a positive relationship between health insurance and health care access as well as self-assessed health, although the mechanism for the latter outcome has not been clearly established.[16] There is mixed evidence on the effects for preventive care and ER visits.[17]

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[15] They also found that community health centers were "forced" to apply for larger state grants and increase their prices.

[16] For example, people could be stating they have better health because they are in a better financial status because of insurance rather than having improved clinical outcomes.

[17] Existing theory provides an ambiguous prediction on the effect of losing health insurance on health. On one hand, losing health insurance increases medical care costs and lowers demand for medical care could end up having a negative impact on health (Grossman, 1972). On the other hand, losing health insurance coverage can lead to changes in preventive care efforts and health behaviors that have positive effects on health (Ehrlich and Becker, 1972). Exactly the opposite

This paper adds to this body of literature in economics and complements our understanding of the relationship between public health insurance and health.

3. Institutional Background

This section summarizes the context of the disenrollment that occurred in Tennessee. I describe a brief history of the program and the political context that led to the decision and timing of the reform. To an extent, people affected by the disenrollment were not necessarily aware if they would be disenrolled or when it would happen.

In the early 1990s a Tennessee state budget report projected a budget deficit of $250 million which was largely driven by increased Medicaid spending. In addition, a substantial part of the Medicaid funding (around $400 million) came from a special tax on hospitals and nursing homes and this provision was soon to end. This led Governor Ned McWherter to invoke a task force to identify three options for the state legislature. The three options were: 1) increase state taxes, 2) reduce health care or provider reimbursement rates, and 3) engage in a comprehensive restructuring of health care delivery and financial systems. Governor McWherter took this opportunity to push his vision of expanding Medicaid by pushing the third option to the state legislature. This third option would be a major overhaul of the way

effects are in place when an individual gains health insurance, but it is not clear that the magnitude of the effects needs to be symmetric. In terms of ED utilization, there is no clear ex-ante prediction on how losing health insurance would affect ED visits. It is possible that individuals who have had health insurance are more informed about how the system works and therefore would be less likely to use ED as their source of care. On the other hand it is possible that people who lose health insurance avoid going to the doctor long enough until it becomes an urgent enough situation for the patient to attend the ED.

Medicaid was delivered and funded in Tennessee. This reform would become the beginning of TennCare.[18]

TennCare had two main goals: to control costs and to expand coverage. In order to control costs, the state decided to enroll its Medicaid recipients into managed care insurance plans. The idea was to transfer the federal and state payments for indigent care from hospitals to insurance coverage. In addition, new state taxes were created to help finance the expansion. The savings from transitioning enrollees to a managed care organization and the new tax income were then used to expand coverage to uninsured individuals with incomes up to 400% of the federal poverty line and to those considered "uninsurable" by private insurance companies.[19]

Individuals who benefited from this expansion were mainly non-traditional Medicaid beneficiaries. Compared with traditional Medicaid recipients, the expansion group was more likely to be white, between the ages of 21 and 64 year old, and have higher income. This expansion allowed for childless adults, who had never been covered by Medicaid prior 1994, to be covered under TennCare. The enrollment into TennCare started in January of 1994. New enrollees had premiums based on their income level, though this did not deter applications.

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[18] The state legislature approved a federal waiver that authorized deviations from standard Medicaid rules. This waiver was part of a 5 year demonstration project. The credibility of Tennessee to have sustainable managed care depended on the participation of Blue Cross Blue Shield of Tennessee. The idea behind TennCare was two-fold: to control cost and expand Medicaid coverage. The first goal was to be achieved by enrolling all of their Medicaid recipients into a managed care insurance plans.

[19] To be considered "uninsured" in 1994, individuals had to be uninsured as of March 1, 1993; to be considered "uninsurable," individuals had to prove that they were denied private health insurance coverage (Moreno and Hoag, 2001).

By 2000, it was clear that the system was not sustainable, since health expenditures were rising faster than Tennessee's budget. Independent auditors recommended either reducing coverage, cutting benefits, or increasing taxes, but none of these suggestions were popular solutions.[20] In 2003, Democrat Phil Bredesen was elected as Tennessee's new governor. During his campaign, he promised to take care of TennCare's accrued debt. Although Bredesen assured Tennessee residents that he was going to work with the managed care organizations to find ways to cut costs without dropping people from the program, in January 2005 Bredesen announced that a major disenrollment would happen that year, and that it would affect the people covered under the 1994 expansions.[21] By August 2005, individuals started receiving letters stating that their TennCare health insurance coverage was terminated. This disenrollment continued until May 2006; in total, about 170,000 residents were dropped from the program. Figure 1 shows the monthly TennCare enrollment and confirms there was a very large and sharp decrease in the TennCare enrollments during this time period.

4. Empirical Strategy

My research design compares changes in outcomes of interest between Tennessee and other Southern states before and after the reform. In addition, I use the fact that this reform targeted

[20] In 2002, a re-verification process started in which everyone under TennCare had to be re-verified for program eligibility. Most of the people who applied for re-verification continued to be covered under TennCare (Ruble, 2003). The information from the re-verification process was used to determine who was covered under the 1994 expansion and who was covered under traditional Medicaid. In addition, eligibility requirements were changed for the uninsurable category. A Medical review of "insurability" was required instead of the regular of denial of coverage from private insurers.

[21] In fact, he told the press that people with disabilities and uninsurable status would still be covered.

mostly childless adults to compare the differential in outcomes of adults with children and adults without children in Tennessee to the same differential in other Southern states before and after the reform.[22] These specifications allow me to interpret my results as the causal effects of the disenrollment on health outcomes.[23]

The first approach makes use only of the relative change in outcomes in Tennessee versus other southern states in a Difference-in-Differences (DD) model. Specifically, I estimate the following equation:

$$(1)\ \mathrm{Y_{ist}} = \beta_0 + \beta_1 (\text{Post July 2005} \times \text{ TN})_{st} + \beta_2 \mathrm{X_{ist}} + \delta_t + \alpha_s + \epsilon_{ist}$$

Each outcome Y is measured for individual i in state s, at time t. Here, time is a month-year combination. $Post\ July\ 2005 \times TN$ is a variable that takes the value of 1 for individuals in Tennessee who reported outcomes after July 2005, and 0 for everyone else. The coefficient on this variable, β_1, represents the Difference-in-Differences treatment estimate of interest. I control for state fixed effects (α_s) and for year and month fixed effects (δ_t) which include year dummies as well as month dummies to account for any seasonality in outcome responses (i.e. the possibility of responding more positively during the summer months).[24] X_{ist} is a vector of individual level controls such as education, race, age, gender, and marital status. I

[22] For comparison purposes, the percentage of adults with no dependents who were affected by the Massachusetts health care reform and the Oregon Health Experiment was around 50%. Kenny et al. (2012) predict that the ACA expansion group will be composed of 82.4% childless adults.

[23] I also explored as control groups states that border Tennessee and states selected by the standard synthetic control method (Abadie et al., 2010); both yielded similar results. I use the definition of southern states given by the U.S. Census; this contains the following states: Alabama, Arkansas, Delaware, the District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, Tennessee, Texas, Virginia, South Carolina, and West Virginia.

[24] This is true in the BRFSS specification. In the NHIS specification I do not have information of month of interview for all observations and so I do not include this variable.

estimate this specification for the full-sample but also for the sample of adults with children (who were not targeted by the reform) and the sample of adults without children (who were targeted by the reform). My identifying assumption is that outcomes in Tennessee would have evolved in the same way as other Southern states in the absence of the disenrollment conditional on observable characteristics.

My second specification is the triple difference model which uses the fact that the TennCare disenrollment targeted childless adults. This model takes the form:

$$(2)\ Y_{igst} = \beta_0 + (\gamma_g \times \alpha_s) + (\gamma_g \times \delta_t) + (\alpha_s \times \delta_t) + \beta_1 (Post \times\ TN \times No\ Kid)_{\text{gst}} + \beta_2 X_{ist} + \epsilon_{igst}$$

As in the DD specification, I index individual i, in state s, at time t and group g which indicates if the individual is a childless adult.[25] In this DDD specification the estimate of interest is the coefficient on the triple interaction $Post \times TN \times No\ Kid$, β_1. This interaction terms takes the value of 1 if an individual does not have dependents under 18 in the household, lives in Tennessee and is reporting outcomes after July 2005, and 0 otherwise. γ_g is a dummy variable that indicates the childless status of the individual (i.e. if they have dependents in the household or not). Thus, I include state, year, and childless status fixed-effects in the model as well as any two-way interactions between these three sets of fixed effects. This makes my estimates robust to any state-year (e.g. a state program that does not differentially affect childless adults vs. adults with children), state-childless (e.g. a Tennessee specific outreach to childless adults that is constant over time), and year-childless status (e.g. any national outreach campaign that affects childless adults) specific effects. In this case, my identifying assumption

[25] I defined a childless adult as an adult who lives in a household with no other member under the age of 18

is that the difference between the two demographic groups (adults with children and adults without children) would have evolved similarly in Tennessee to the differential in other southern states in the absence of the disenrollment. In other words, the two demographic groups are allowed to evolved differently from each other, but the differential between these two groups would have evolved similarly in Tennessee to the rest of southern states in the absence of the disenrollment. For my estimates to be biased in the DDD, there has to be a trend or an event – around the time of Tennessee's disenrollment – that affects adults with children and adults without children differently and this pattern is not consistent across the control states. As an example, if we hypothesize that Medicaid premiums were changing in this period of time in southern states - with each state having different changes – then the effect of the premiums would also have to be different for adults with children and adults without children to bias my results.[26] I consider this specification to be more robust and have a weaker identifying assumption that the DD model; therefore, it is my preferred specification.[27]

To estimate appropriate standard errors, I use a modified version of block bootstrap developed by Garthwaite et al. (2014). Traditionally, I would need to account for serial correlation within states over time and this is usually done by clustering standard errors at the state level. However as MacKinnon et al. (2014) point out, clustering relies on the number of clusters being large. In this study the number of clusters is 17, and therefore the main

[26] As reviewed on the background of the reform, I am not aware of any other policies in Tennessee that affected childless adults and adults with children differentially around this time period.

[27] I also estimate models by changing the timing of the DDD variable to different starting points. For some outcome variables BRFSS asks if a procedure was done in the past 12 months. In these cases, I create a variable that represents the number of months an individual is exposed to the reform by accounting for the months lapsed between disenrollment and the interview. It takes a fractional value, from 0-1. Separately, I aggregate the data at the state-year level and re-run the main specifications. The different specifications provided similar results to the ones presented in this paper.

assumption for Cluster Robust Variance Estimation (CRVE) becomes hard to justify. In addition, the percent of treated units matters for the finite sample properties of CRVE to hold. In simulations Mackinnon et al. (2014) show that this could lead to an over-rejection of the null hypothesis. In order to account for this issue, in additions to CRVE, I use a modified version of block bootstrap which is composed of a two stage sampling across states and within states. In the appendix, I use Monte Carlo simulations to test the finite sample properties of this method and to perform comparison across other standard error adjustment. I conclude that the modified version of block bootstrap has rejection rates closer to the appropriate value (using a p-value of 0.05, we would want 5% rejection rates).

Additionally, as it is becoming popular with single state interventions (Courtemanche and Zapata, 2014; Shah and Cunningham, 2014) I also implement the synthetic control method. This method was developed by Abadie et al. (2010) and is a generalization of the DD framework, it addresses the possible bias in a DD framework that comes from potentially not having a correct control group. Essentially, even if the control groups have parallel trends, there could be something inherently different about the control group that we are not able to observe which could end up biasing the DD estimates. To account for this, synthetic control uses a weighted subset of all possible controls, which is selected by matching to the treated group on pre-treatment dynamics.

When using synthetic controls the estimated effect is the difference between the outcome for the treated unit and the synthetic unit. To measure the causal effect I estimate:

$$Y_{1t} - \Sigma_{s=2}^{S+1} \omega_s Y_{st}$$

Here Y_{1t} represents the outcome of the treated unit, at time t, while ω_s stands for weights for all control states. These weights represent how much of each state in the control pool is contributing to the creation of the counterfactual outcome. Weights are calculated using a set of matching covariates which help determine how similar states are in the pre-treatment dynamics. An important thing to notice about this framework is that all the matching is made on observables and not unobservables.[28] Intuitively, if we are able to match the dynamics before treatment between the treatment and control group, then we will be able to predict what would happen in the absence of treatment, because we are assuming that nothing else changes.

For the analysis using administrative data on inpatient hospitalizations, I use a DD approach similar to the one presented above. This specification compares outcomes before and after the reform in Tennessee to other Southern states. Since I do not observe if the individuals who come in have children or not I am not able to use the DDD specification I proposed for BRFSS and NHIS. Hence I use the following model:

$$Y_{dhts} = \beta_0 + \beta_1\,(TN \times Post) + \gamma * X_{dhts} + \delta_t + \alpha_s + \rho_h + \epsilon_{hts}$$

Where Y is an outcome for a hospital discharge d, in hospital h, at time t, in state s. The estimate of β_1 provides the impact of the reform on outcomes. X_{dhts} is a vector of covariates that contains characteristics of the inpatient discharge such as age, age squared, sex, race dummies, number of diagnoses, dummies for quartile zip income level of the place where the

[28] However Abadie et al. (2010) mention that when the number of pre-treatment periods is large, matching on pre-treatment covariates helps control for any heterogeneity of unobserved and observed factors on the outcome in addition to accounting for the unobserved factors that affect the outcome.

inpatient lives and a set of inpatient risk adjusters.[29] In my specification, they serve as way to control for patient's health composition. In addition I include year-quarter fixed effects (δ_t), state-fixed effects (α_s) and hospital fixed effects (ρ_h). I use hospital fixed effects to account for the unbalanced panel nature of NIS; without hospital fixed effects the estimator could be capturing changes in the sample of hospitals across years. This is something to be cautious about since the data is not state-representative.

Identification under hospital fixed effects comes from within hospital changes in discharge outcomes before and after the reform compared to hospitals in other Southern states, allowing for national and state-specific linear trends.[30] The identification assumption is that outcomes of inpatients and hospitals in Tennessee would not have evolved differently from those in other southern states in the absence of the reform. Since uninsured individuals might avoid going to the hospital until a serious health event occurs, it is plausible that the pool of inpatients after the reform are relatively in worse health than the pool of patients before the reform and this could be driving changes in outcomes. However, in my preferred specification I do not control for this selection mechanism, as I am interested in the effects in the presence of this selection, since this is a consequence of the reform.[31] For estimation of standard errors I also use a modified block bootstrap procedure.

[29] These include comorbidities, and All Patient Refined Diagnostic Related Group (APR-DRGs) as well as All Patient Severity Diagnostic Related Groups (APS-DRGs). These measures are developed by an external organization that helps evaluate the patient before procedures are done and assigns a payment category given their health status and conditions.
[30] In an alternative specification I can estimate the model controlling for seasonality and year-quarter time trends, however this implies dropping Florida from the control pool since observations in Florida do not provide month of quarter date of admission. My preferred specification opts for including Florida since it represents 20 percent of the total sample I use.
[31] However, for robustness checks I propose two empirical alternatives to account for this selection. Ideally we would like to have information on the health status of the patient before any procedures. The NIS offer a set of measures of group risk-adjusters that aid in holding patient's health composition constant. I then compare results with and without risk

For all of the analysis above I study the period of 2000-2010, which allows to have enough pre and post periods of the reform to credibly identify its effects. However, following Garthwaite et al. (2014) I also perform my analysis using the 2000-2007 to avoid potentially confounding effects from the Great Recession on health outcomes (e.g. Tekin et al. 2013; Ruhm, 2000, 2002, and 2005; Cotti et al. 2014). For the recession to bias my estimates, the recession would have had to affect the differential of childless adults and adults with children in Tennessee differently than it did in other Southern states. Most of the results are robust to this alternative sample period. In the results section I point out which outcomes have different implications using the shorter period of time.

5. Data

In this section, I describe the datasets I used in my analysis. For population level outcomes I use two major datasets: the 2000-2010 Behavioral Risk Factor Surveillance System (BRFSS), and restricted versions of the 2000-2010 National Health Interview Survey with state identifiers.[32] To study the effects of the disenrollment on inpatient care, I use the 2000-2010 Nationwide Inpatient Sample.

adjuster to understand the degree of selection. An alternative to tackling selection is using ICD-9 codes to identify groups of diagnoses that should not be affected by health insurance status (urgent procedures) versus procedures that are more likely to be avoided if one does not have health insurance or procedures that the patient can have some control on the timing (elective procedures). The idea is to identify health shocks that one cannot wait for medical attention, and therefore would end up in a hospital admission regardless of health insurance status. NIS provides a classification for each discharge on the type of "Urgency", I used this classification to compare discharges that are elective and non-elective.

[32] I use the restricted version of the NHIS because the public version does not contain information on state of residence and time of interview.

5.1 BRFSS and NHIS Survey Data

BRFSS is an telephone survey that started in 1984. The survey includes information on a variety of self-reported health status and health behaviors as a monthly repeated cross-section. It also contains standard demographic characteristics such as age, race, marital status, education, and – importantly for my study – the presence of children in the household. The survey is administered by each state in collaboration with the Centers for Disease Control and Prevention (CDC), which compiles information into an annual dataset at the state level.

NHIS is a cross-sectional household interview survey in which sampling and interviewing are continuous throughout the year. The survey contains detailed information on health insurance, health access and utilization of medical care. The data is collected by interviewers trained at the U.S Bureau of the Census and the survey is administered by the National Center for Health Statistics (NCHS) and the CDC. This survey asks questions about members of the household but it also contains a section called "Sample Adult" which selects non-institutionalized individuals over the age of 18 and asks them more detailed information on their health and health care access. I use outcomes from the Household file and the Sample Adult file. For each sample file I use the NCHS provided weighing adjustments.[33]

These surveys complement each other well. On the one hand BRFSS contains a large number of observations which can be identified at the state-month level. Also, BRFSS contains several questions on health behaviors and preventive care and the questions are consistent over the sample period as opposed to NHIS which only asks about certain health

[33] In BRFSS and NHIS, I exclude from the sample individuals age 65 and older since they are eligible for Medicare, and I also require individuals to be at least 21 years old. Individuals under this age could be covered under their parent's health insurance and will be less likely to be treated.

behaviors and preventive care in some years. On the other hand, NHIS has detailed questions on the type of health insurance (which BRFSS does not provide) which is critical information since the reform should have induced predictable changes in different types of coverage. In addition NHIS contains questions on health access, utilization of medical care, and health spending that BRFSS does not offer. Since both surveys have their advantages and disadvantages they serve as useful complements of each other in studying the effects of the disenrollment.

I study five categories of outcomes: health care access, preventive care, health behaviors, self-assessed health and utilization of medical care. For health care access, I study three types of variables. The first variable is health insurance. Using NHIS I observe: having any health coverage, Medicaid, Medicare, Private Insurance or other type of health insurance.[34] In BRFSS, I construct a health coverage variable based on the question "Do you have any kind of health care coverage, including health insurance, prepaid plans such as HMOs, or government plans such as Medicare?" reporting "Yes" to this question is coded as one and "No" is coded as zero. In addition in NHIS, for people who report having no health coverage, the surveyors ask individuals to give reasons for not having insurance and one of the options is "Losing Medicaid". I use this variable as direct evidence of the disenrollment. The second margin on health care access derives from a question that is similarly worded in BRFSS and NHIS: "Was there a time in the past 12 months when you needed to see a doctor but could

[34] Although TennCare was considered to be an extension of Medicaid, it is possible that some people thought they had private health insurance even if they had TennCare. In the appendix I include an example of a TennCare report card, which illustrated how an individual could confuse their reporting of TennCare with private health insurance. Given the existing literature which raises the issue of misreporting on types of health insurance (Lynch et al. 2003), it is possible that a cleaner measure of the reform is a variable measuring having health insurance or not, which can be found both in NHIS and BRFSS.

not because of cost?" I assign the value of one if the response is yes and zero otherwise. In NHIS, I also use the question "During the past 12 months, has medical care been delayed for {person} because of worry about the cost? (Do not include dental care)" as a measure of access to medical care. Intuitively, losing health insurance means higher costs for most medical care, and therefore I expect an increase in the number of occasions that individual decides to forgo or delay medical care due to cost. The third margin derives from questions about seeing a doctor. The NHIS asks questions regarding seeing a variety of specialist doctors (e.g. pediatrician, mental health professional, ophthalmologist, etc.) as well as a general doctor. In BRFSS, I use the question "Do you have one person you think of as your personal doctor or health care provider?" to study the effect of the disenrollment on seeing a doctor and reporting having a doctor as measures of health care access. I expect a decrease in this outcome as well.

For preventive care, I have a total of 8 outcomes, all of which derive from questions of the following kind: "In the past 12 months have you had a (Preventive Test) ?" I assign the value of one if individuals responded "Yes" and zero if individuals responded "No". [35] For questions about preventive care that are gender and age specific, I define the variables only for those that are recommended by the United States Preventive Services Task Force (USPSTF). These are having a mammogram for women over 50, having a breast exam for women over 21 and having a Pap test for women that are over 21. For men, I code having a PSA test for men over 40 and having a rectal exam for men over 40 as well.[36]

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[35] I also create index variables that summarize the information from each separate question of preventive care. I construct the index using the method proposed by Anderson (2008).
[36] I do not study colonoscopies because the question was introduced in 2004.

For health behaviors, I use BRFSS and NHIS questions on alcohol consumption, smoking, consumption of fruits and vegetables, and exercise. I create a variable named "Physical Activity" which takes the value of one if the individual answered "Yes" and zero if the individual answered "No" to questions of performing more than 10 minutes of vigorous physical activity.[37] There are also several questions regarding consumption of fruits and vegetables. I use the information from these variables to create a variable representing the average number of daily servings of fruits and vegetables. For drinking alcohol, I report three variables. "Binge Drinking" is coded one if the individual reported having 5 or more drinks in one occasion in the past 30 days. "Any drink in the past 30 days" takes the value of one if the individual reported having at least one drink of any alcoholic beverage in the past 30 days. Finally, I use a self-reported average of number of alcoholic drinks per occasion to create "Drinks per occasion in the past 30 days" variable. I also created a variable named "Currently a smoker" which takes the value of one if an individual reported that currently he is smoking either every day or some days, and it takes the value of zero otherwise.

For health outcomes I use questions in both BRFSS and NHIS regarding self-rated health and number of days being sick.[38] The first question asks individuals to rate their health from 1-5, 5 being excellent and 1 being a poor level of health. I use as outcomes the probabilities of reporting each level. Each variable takes the value of 1 if in the respective

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[37] In BRFSS "During the past month, other than your regular job, did you participate in any physical activities or exercises such as running, calisthenics, golf, gardening, or walking for exercise?" and in NHIS is "How often do you do vigorous leisure-time physical activities for at least 10 minutes that cause heavy sweating or large increases in breathing or heart rate?" and similarly for moderate activity.

[38] The advantage of using self-assessed health is that it encompasses all the potential health related problems, including those that a physician may not observe.

category and 0 if not.[39] Second, I use a question that asks individuals to report the number of days they had bad physical health (BRFSS), bad mental health (BRFSS), and any type of bad health that prevented them from performing daily tasks or made them miss work (BRFSS and NHIS). I use each outcome in two ways. First, I use the raw variable reporting the number of days. Second, I create a variable that takes the value of one if they reported a positive number of days and zero otherwise.

For utilization of medical care, I use questions from NHIS regarding changes in the usual place of medical care, place of care the respondent goes when sick, number of times in the Emergency Department, nd number of times spent overnight in a hospital.

In Table 1, I present summary statistics for Tennessee and other Southern states using BRFSS. Most of the variables across both groups are similar and not statistically different from each other. Notably, the health insurance rate was 8 percentage points higher in Tennessee than in other southern states before the reform; this difference reduces to 4 percent after the reform. Tennessee has less reporting of forgone medical care and higher reporting of people having a personal doctor, which is consistent with the higher insurance rates.[40] In Table 2, I present summary statistics for Tennessee and other southern stats using NHIS. [Fill in]

[39] I focus on the extreme ratings since recent evidence by Greene et al. (2015) mention that the middle ratings are usually inflated. These outcomes are available in the online appendix. In addition, there has been recent research that has suggested using ordered probit for this specific outcome (Contoyannis et al. 2004) I have estimated these models as well and the results are similar to the linear probability model.

[40] In Appendix Table 1 I show a table of summary statistics for demographic characteristics. Comparing pre-reform means across Tennessee and other southern states the difference that is most stark is the racial composition of Tennessee (much less Hispanic than other southern states) and the percent of high school graduates and some college is bigger in Tennessee than other southern states. I take into account this observables difference by controlling for race and levels of education for each individual in my regression specifications.

5.2 NIS Inpatient Data

I expand my analysis of changes in inpatient stays by using the Nationwide Inpatient Sample (NIS). These data are a nationally representative database developed by the Healthcare Cost and Utilization Project (HCUP) that is the largest publicly available all-payer inpatient healthcare database in the U.S. These data contain information on inpatient discharges from community hospitals. Given the period used in this analysis (2000-2010) the design of NIS contains the universe of discharges from a sample of community hospitals. [41] The sample of hospitals aims at representing 20 percent of a stratified sample of all U.S community hospitals. This amounts to 5 to 8 million hospitals stays each year coming from about 1000 hospitals. The NIS provides information on both hospitals (location, teaching status, size, ownership type, number of discharges, etc.) and patient discharge characteristics (payment type, diagnosis, length of stay, cost of stay, admission type, etc.). Notably it also contains month of admission, which is particularly useful since the reform was implemented over the course of 8 months (August 2005 to May 2006). Therefore given the relatively large sample sizes in each monthly bin, these data allow me to identify changes at the monthly level and to compare outcomes before the reform, during the reform and after its full implementation.

There are two major limitation with these data. First, they are designed to be nationally representative, but are not designed to be representative at the state-level. Second, NIS contains data on inpatient visits. In regards to the first limitation, I estimate summary statistics for the main variables in NIS and compare them to state representative analogous variables

[41] Community hospitals are all non-federal, short-term general, and other specialty hospitals, excluding hospitals units of institutions. Ninety percent of all hospitals in the U.S are considered community hospitals. Examples of non-community hospitals are hospitals for prison inmates or veterans' hospitals.

these are represented in appendix Table 6. The comparison demonstrates that the NIS estimates are generally similar to state representative datasets. Regarding the second limitation, this paper offers analyses of the reform with other population-based outcomes, which complements the results found with the NIS data.

For this analysis, I use all discharges from hospitals for years 2000 through 2010 that are located in the South, as defined by the U.S Census. Since not all states report to the HCUP database, this pool of states is composed of Arkansas, Florida, Georgia, Kentucky, Maryland, North Carolina, South Carolina, Tennessee and West Virginia.[42] I exclude patients over the age of 65 and under the age of 20, since both of these populations were not directly targeted by the reform. In Table 3 I provide statistics that describe the NIS data. The final sample consist of 1583 hospitals over the span of 11 years (2000-2010), this equals a total of 31,55,042 of inpatient discharge records. The average appearance per hospital is 3.8 times over the 11 years, hence it is an unbalanced panel. Comparing Medicaid rates before and after the reform in Tennessee I find a 5 percent decline, while in other Southern states I find a 10 percent increase. I also find there is a decrease in the percentage of admission coming from the ED in Tennessee of about 2 percent, which is much smaller than the 11 percent decrease experienced by other Southern states.

[42] I also estimated the analysis excluding Virginia, Oklahoma and Texas. The first two states come in and out in the sample over this period. Texas has non-trivial differences in the way they report it reports its outcomes compared to the rest of the states.

6. Results

In this section I describe the main findings of the effects of the disenrollment on health. I show that the disenrollment decreased overall health insurance rates, which in turn decreased access to care. I then show decreases in health status and how changes in the places where people obtain care.

6.1. How did the disenrollment affect health insurance rates?

I begin by providing evidence of the reform, namely that there is an increase in reporting having "lost Medicaid". I complement evidence from this outcomes by reporting the effects of the reform on having Medicaid and any health insurance.

To show graphically the effect of the reform, I use BRFSS data to plot a graph of health insurance rates.[43] Figure 2 illustrates that before the disenrollment the two demographic groups within each state move similarly, but once the reform occurs the group of childless adults in Tennessee majorly diverges from the group of adults with children in Tennessee. Notably, there is no divergence between childless adults and adults with children in other Southern states.[44] After 2009 there is another visible drop in the health coverage rate for childless adults possibly driven by the recession. In which case the $year \times childless\ status$ coefficient should take into account any national trend that affects childless adults and adults with children differently for each year. Relatedly, the results on health insurance rates are consistent when I restrict the sample to end in 2007.

[43] I use BRFSS since it has a larger sample size and reporting is in monthly bins

[44] Since the sample size for NHIS is smaller the data contains more noise. In the Appendix Figure 5 I provide changes in Medicaid rates using NHIS for the same sample period. In this graph there is still a divergence of childless adults in Tennessee from adults with children in Tennessee that is not observed for the same groups in other southern states.

In Table 4, I present the results from different specifications using data from BRFSS and NHIS to provide evidence of the disenrollment. The first three panels are specifications based on the DD models and the bottom panel is based on the DDD. The columns represent different outcomes: the first three columns are outcomes from NHIS while the last column is an outcome from BRFSS.

I first look at the effects of the reform on the likelihood of reporting having lost Medicaid. I take this as the most direct evidence of the reform. The estimate in the first row and column of Table 4 is the DD estimator for the full sample, which indicates that the TennCare reform increased the likelihood of reporting having lost Medicaid by 1.1 percentage points. I then proceed to estimate the DD by the sample of adult with children and adult without children. I expect the effect to be mostly driven by the sample of childless adults. When I estimate the same model using the sample of adults with children (second panel) and childless adults (third panel) it is noticeable that the DD effect for the full sample is driven by the sample of childless adults. In the DDD model, I estimate a significant 1.8 percentage point increase in the likelihood of report of losing Medicaid. This represents a 128 percent increase over the pre-reform mean.

Since not everyone gets asked the question on losing Medicaid, I use reporting on having Medicaid. The second column provides the estimates on reporting having any Medicaid as health coverage. In the DD full sample model, I estimate a 2.8 percentage point reduction in the probability of reporting having Medicaid. When comparing across sub-samples for the DD, the full sample effect is mainly driven by the sample of childless adults. The DDD specification estimated a significant 2.7 percentage point reduction in the probability of reporting Medicaid, which represents a 30 percent reduction over the mean. These findings confirm a strong treatment effect of the reform.

Since it is possible that people who were dropped could have been able to obtain other sources of insurance, in columns 3 and 4 I estimate the effects of the reform on overall health insurance rates. In both datasets, when comparing the DD estimates by sub-sample it is clear that the effect is mainly driven by the sample of adults without children. Focusing on the DDDs, using the NHIS I estimate a statistically significant 4.5 percentage point increase in the likelihood of reporting being uninsured, which represents a 32 percent increase over the mean. Using BRFSS, the DDD specification estimates a 1.7 percentage point reduction in the probability of having any type of insurance, which represents a 2 percent reduction over the mean. Both of these estimates are statistically significant.[45]

In 2004, childless adults represented 52% of all adults in Tennessee between the ages of 21 to 64. Using the estimates from the DDD models I find a decline in health insurance rates of 4.5 (NHIS) and 1.7 (BRFSS) percentage points. These effects translate into

[45] In the appendix I explore how the reform affected the probabilities of reporting other types of health insurance. Using NHIS I find evidence of increases in reporting private insurance for the DD, but this effect is small and statistically insignificant. Using the DDD I estimate a 3.21 percentage point decline in reporting private coverage. This effect is statistically significant using the clustered standard errors but is not statistically significant using the block bootstrap p-values. It is possible that since people under TennCare were covered by managed care it is possible that individuals were reporting losing private health insurance as opposed to Medicaid when ask the question about their insurance. In Figure 3 of the Appendix I provide a TennCare card example which can illustrate the confusion when reporting health insurance. If this hypothesis is true I should find changes in private-payment types in NIS data, since these are records that come from the hospital administrative data and therefore are less likely to be contaminated the confusion in reporting. Using NIS data, I do not find a significant change in the rate of private payments. I also find a reduction of 0.01 percentage points in reporting having Medicare. In the NHIS results it is statistically significant at the 5 percent level. I also present results for other reasons for not having health coverage. Most of the effects are not statistically, significant and the largest coefficient is for the losing Medicaid outcome.

approximately 34,000 to 97,750 residents – about 20 to 57 percent of people losing eligibility - who did not get other types of coverage.[46]

Finally, I present evidence of the existence of the reform using the Synthetic Control Method for health insurance. I present the results using BRFSS since it has a larger sample size.[47] I graphically present the main results in Figure 3. For this estimation I use all states as my donor pool, however I have also restricted the donor pool to Southern states as I do in the DD framework. The results are similar and can be found in Appendix Table 7. The synthetic Tennessee's outcomes diverge significantly from actual Tennessee's outcomes after 2005, the year of disenrollment. The estimated effect of disenrollment on health insurance coverage is a reduction of 3.48 percentage points. This estimate is higher than the one obtained from the DD analysis but strengthens the evidence on the effects of disenrollment.

For the rest of the paper I only show results from my preferred specification, the DDD. In addition I will estimate this specification using the full-sample and using a sample of low-educated individuals (high school degree or less) and high-educated individuals (above high school degree). The idea behind the sub-sampling is that the population of low educated individuals would be more likely be affected by the reform since they are more likely to have income under 400% of the federal poverty line. In Table 5, I estimate the results using the sub-samples of low and high educated. I estimate that the probability of having Medicaid falls by 4.8 percentage points among low educated while for the high educated sample it falls 1.5

[46] These estimates are larger than those provided by Garthwaite et al. (2014): they estimated a decline in public coverage of 3.6 percentage points in their DDD specification and 4.6 percentage points in their DD model. However it is expected that their estimates are different since they are estimated from the differences between being publicly covered and having no coverage. They estimated the crowd-out effect, the ratio of the decrease in public coverage to the increase in private coverage, to be about 36.2%. Using Garthwaite et al. estimates on private coverage, their results imply a decline in overall health insurance coverage of 2.9 percentage points for the DD model and 1.4 percentage points for the DDD specification.
[47] The results for this method using NHIS can be found in the appendix, the results are similar.

percentage point. This corroborates the reform having a higher impact on the low educated sample than its counterpart.

6.2. Mechanism of the Losing Health Insurance of Health Status

This section studies the effects of the disenrollment on health care access, preventive care, and health behaviors. These are all potential mechanisms on how the disenrollment could affect health outcomes. I find evidence of decreases in health care access, increases in the likelihood of having a flu vaccine and suggestive evidence of reductions in risky health behaviors and increases in positive health behaviors. I do not find consistent evidence in changes in preventive care measures related to cancer detection.

6.2.1. Effects on Health Care Access

In order for losing health insurance to affect one's health care decisions and health it should be the case that losing health insurance reduces health care access as a result of increased cost. In this section I document that the reform increased reporting of forgone and delayed medical care specifically due to cost. In addition I also provide evidence of a reduction in the probability of reporting seeing a general care physician.

Table 6 explores the effects of the reform on health care access. Panel A presents results for health care access variables from NHIS while Panel B resents results for variables in BRFSS. The first outcome is forgone or delay medical care due to cost. It represents the probability of not going to see a doctor when needed because of cost, which is one of the main mechanisms through which lack of insurance affects health.[48] The full sample DDD estimates an effect of 3.2 (NHIS) and 1.3 (BRFSS) percentage points. This represents a 30 to 10 percent effect over the mean, respectively. The estimates for BRFSS are statistically significant only using the cluster-adjustment while the estimates from NHIS are significant under both standard error adjustments. In column 2 and 3, I re-estimate the model using the sample of people with a high school degree or less and people with more than a high school level of education. Comparing the estimates from the sample of less educated to more educated, I find that the low-educated sample has a larger increase on the probability of reporting forgone medical care then the high-educated sample in both datasets. The probability of reporting forgone medical care among the low educated group increases by 4.2 percentage points in NHIS and 3.8 percentage points in BRFSS. Both estimates are statistically significant for both inference methods. When looking at the higher educated sample, I find an increase of 2.3 percentage points in NHIS and a very small (0.7 percentage points) decline in BRFSS, in both cases this effect is significant under the cluster standard error adjustment.[49] I next study the effects of the reform on the probability of reporting not being able to afford prescription drugs. I find a 1.9 percentage point increase for the full sample and a 4.4 percentage point increase for the low educated sample. These estimates represent a 17 percent and 40 percent for the low educated sample.

[48] There is a potential problem with the timing component of this question, but given the set-up of the DD, we should still be able to detect effects. I have also tried a specification in which the DD is not binary but a fraction representing the possible amount of months treated by the reform, the results from this variation are similar to this specification.
[49] In Figure 3 of the appendix, I present the estimated effects of the disenrollment on this outcome using the Synthetic Control Method, an increase of 2.74 percentage points.

Finally, I can study the effects on seeing a general care physician (NHIS) or having a general check-up in the past 12 months (BRFSS). In both datasets I find negative coefficients across the full sample and the breakdown by education. The effect is also larger for the low-educated sample than the high-educated sample, following the pattern observed in the previous health access variable. All of this effects are statistically significant under the cluster procedure but not significant under the block bootstrap procedure. For the full-sample, these effects translate to a 2.8 to 4 percent decrease in the probability of seeing a general care physician.

6.2.2. Effects on Preventive Care

Delaying or forgoing medical care can be problematic for individuals with chronic health conditions since the lack of medical checks can cause delay in treatment and ultimately increase health risk. Another potential implication of avoiding medical care is that one can have fewer opportunities for getting preventive care. I find evidence of increases in the likelihood of getting a flu vaccine. I do not find consistent effects of preventive care related to cancer detections and cholesterol checks.

In Table 7, I use BRFSS to study the effects of the reform on preventive care. In Panel A, I report outcomes reflecting preventive care of interest to the whole population such as receiving flu vaccine and having a cholesterol check. In Panel B I report on preventive care for women and in Panel C I report on preventive care for men over the age of 40.

I find that the disenrollment is associated with a statistically significant 2.7 percentage point increase on likelihood of having a flu shot, which represents an 8 percent increase over the mean.[50] This effect is consistent over the low and high educated sample, however it is larger and statistically significant only for the high educated sample. Although this results might be counterintuitive, it could reflect moral hazard. Since people are losing health insurance, they are more likely to invest in low-cost prevention such as the flu shot since the expected cost of getting the flu are higher without health insurance. Barbaresco et al. (2015) found decreases in having flu shots when individuals gain insurance under the ACA dependent coverage provision. I also find negative effects on the probability of having a cholesterol check, although this effect is only statistically significant using the clustered standard errors in the full and high-educated sample.

In Panel B I study the effects on preventive care for women. The age reference in each outcomes follows the age recommendations for each preventive care measure provided by the USPSTF. Most of these coefficients are relatively small in size and not statistically significant. I only find statistical significance using the clustered standard error for the coefficient on breast exam, which represents a decrease of 2.3 percentage points or a 3 percent increase over the mean.

In Panel C, I study the effects on preventive care for men over 40.[51] I find that the disenrollment is associated with a 6.3 percentage point reduction in having a (Prostate-specific antigen) PSA exam for the low educated sample, which is significant using the cluster procedure. I also find a 4.1 percentage point increase in having a PSA exam for the high educated sample: again, this coefficient is only significant using the cluster procedure.

[50] In a result not reported the same outcomes variable in NHIS had a coefficient of 2.9 percentage point increase with a 9 percent increase over the mean

[51] The reference age was the only possible reference since that this is how it was asked in BRFSS.

6.2.3. *Effect on Health Behaviors*

The results provide mixed evidence on the effects of the disenrollment on preventive care. The only consistent significant result is the increase in having a flu shot which could be an indication of moral hazard. The idea is that when individuals lose health insurance they will be more likely to adopt behaviors to improve their health, since negative health shocks can induce costs that are no longer mitigated by health insurance. A possible way to investigate presence of moral hazard is to study health behaviors.[52] In Table 8, I study the effect of the disenrollment on health behaviors using BRFSS outcomes. The first panel shows a summary measure that includes information of risky and non-risky health behaviors. Following a methodology in Anderson (2008) that helps correct for multiple inference, I create an index which higher scores represent engagement in more positive health behaviors or less risky health behaviors. I study each behavior separately in Panel B and C. The estimates from panel A indicate that for the full sample there is an increase an overall improvement in health behaviors but this effect is not statistically significant. For the low educated sample it is also positive and statistically significant. For the high educated sample I find that the reform is associated with a statistically significant decline in the index. The size of this effect can be interpreted as changes in a z-score measure. Taken together, the results from Tables 7 and 8 are consistent with and suggestive of the presence of moral hazard for low educated individuals but not for high educated individuals. It is important to note that changes in health behaviors need not necessarily be consistent with only a moral hazard argument since

[52] Carpenter and Tello-Trillo (2015) provide evidence of moral hazard using health behaviors from the same data.

reduction in smoking and drinking could be driven by changes in the current budget constraint that were driven by lack of insurance.

6.3. Are people getting sicker?

In this section I study how the reform affects health status. In order to understand the effects of the disenrollment on population health I study two measures of population health: self-rated health and days with some sickness.

Arguably the main disadvantage of self-rated health is that it is a subjective measure and it could be representing changes in the individual's well-being rather than clinical health outcomes. However it also encompasses all potential health problems observed by individuals and not observed by a physician. Previous studies have shown that this measure correlates with objective measures of health outcomes (DeSalvo et al., 2005; Idler et al., 1997) and mortality (Bound, 1991; Burstrom and Fredlund, 2001; Mossey and Shapiro, 1982). In addition, this is a widely studied measure of health which helps compares estimates across policies. Table 9 presents the results on probability of reporting excellent health, and reporting fair or poor health.[53] In Panel A, I study the outcomes using NHIS. I find that the disenrollment is associated with an increase in the likelihood of reporting excellent health of about 2.6 percentage points for the full sample and it is of similar size and sign for the low and high educated sample. These effects are statistically significant using the cluster procedure but not the bootstrap procedure. In addition, I estimate a decrease in the probability of reporting fair and poor health of about 1.4 percentage points for the full sample and more

[53] The probit version of this specification as well as the ordered probit are available in the online appendix, all of the results presented in here have the same interpretation as those found in the non-linear models.

than double for the low educated sample. Similarly, these coefficients are only significant under the cluster procedure. In contrast to these results, using the BRFSS I estimate a decrease in the probability of reporting excellent health of 0.5 percentage points, and an increase in the probability of reporting fair and poor health of 0.9 percentage points. These estimates are significant using the cluster procedure but not the bootstrap procedure. Even though the effects do not seem to be significant, the magnitudes and signs of the coefficients across both surveys are puzzling, especially given the consistency across the first stage. As I do with all outcomes, I have estimated the specifications with the set of years 2000-2007 to account for any potential confounding from the recession. When I estimate these outcomes on those set of years, the coefficient on reporting excellent health for NHIS becomes negative (and remains insignificant) across all of the samples. However the coefficient in reporting fair or poor health remains negative. It is possible that the reporting of this measure is being affected by conditions of well-being that are not related to health.[54] Given the inconsistency of these results across surveys, I move to analyze arguably more "objective" measures of health such as number of days sick.

In Table 10 Panel A, I report estimates for the number of days over the past 12 months in bed due to sickness. In Panel B, I study the number of days over the past 12 months with bad physical health, bad mental health and days when the respondent was incapacitated. In Panel A, I find that the reform is associated with an increase of 0.6 days in the number of days in bed for the full sample, a 13 percent increase. This effect is much larger - 1.6 days - for the

[54] Another hypothesis could be that individual know less about their current health status since they have stopped going to the doctor and this is why we observe improvements in health. The problem with this hypothesis is that this doesn't explain the difference between BRFSS and NHIS.

sample of low educated individuals, a 30 percent increase over the mean. These results are statistically significant with the cluster procedure but not the block bootstrap adjustment. In Panel B, I find evidence of a small and insignificant decrease in bad physical health for the full sample but a 7 percent increase for the low educated sample, which is significant using the clustered standard errors. I also find positive coefficients on days of bad mental health. Finally, I find statistically significant increases in the number of days incapacitated: there is a 17 percent increase for the full sample, a statistically significant 25 percent increase for the low educated sample.

The evidence brought in Table 8 is much more consistent relative to the results on self-assessed health ratings and provides evidence of a decrease in health, especially for the low educated sample.

6.4. What kind of care do sick people use?

I now study how health care utilization changes after the disenrollment. I present evidence that people report changing their place of care because of their health insurance, as well as evidence that people increase reporting using the ED as their place of usual care. I then report increases in visits to the ED and decreases in the total number of nights stayed in the hospital. I further explore this decrease in number of nights in the hospital by studying administrative hospital data.

In Table 11 I use NHIS to study changes in place of usual medical care when sick. First I study if individuals change their health care place due to health insurance. I find that the disenrollment is associated with a 1.9 percentage point increase in the probability of changing their place of care due to health insurance. This effect is stronger for the low educated sample, I find a 3.9 percentage point increase. Both of these outcomes are statistically significant with any of the standard error adjustment. The second outcome is reporting having a usual place of care. I find a positive association across the three samples, however these effects are only statistical significant using the clustered standard errors. Given the results from these two outcomes, it is possible that individuals still consider they have a place of care, but that the place has changed. In Panel B, I investigate what type of places individuals report being their usual place of care. Focusing on the low-educated sample, I find that individuals are more likely to use clinics and Emergency Departments as their source of usual care and less likely to use the doctor's office. These effects are significant using the clustered standard errors. In contrast, for the high-educated sample, which may have more means of obtaining care, they are more likely to report clinics or the doctor's office as their source of usual care and less likely to report emergency departments as their source of usual care.

Changing place of care is a key part of the story on how health care utilization changes. In Table 12 I explore in more detail changes to health care utilization. In terms of Emergency Department use, I find consistent patterns with the previous findings. Low educated people are more likely to use the Emergency Department while high educated people are less likely to use it. For the low educated, I find a 6.8 percent increase in the probability of using the ED and a similar effect on the number of times in the ED. For the high educated I find a 9.5 percent decrease in the probability of using an ED and similar for the number of times in the ED.

Another margin to study health care utilization is to study procedures that are more complicated than just visiting the ED. These involve surgeries or procedures where the patient needs to stay overnight. The next outcomes in Table 12 focus on these outcomes related to more intensive procedures. I find significant effects under clustered standard errors for the full and low-educated sample, I find a 23 percent decrease in the likelihood of having a surgery for the full sample, and a 49 percent decrease for the low educated sample.

I then study the effect on the likelihood of having an inpatient admission. All the coefficients are positive but they are not statistically significant. I also study the outcomes of number of times being an inpatient in the past 12 months, I find for the full sample a reduction of about 10 percent, and a reduction of 44 percent for the low educated sample. Finally, I study the average number of nights spent overnight in a hospital. I find a statistically significant increase of almost 2 days for the low educated sample and a reduction of 2.8 days for the high educated sample. These results provide evidence that there are non-trivial differences across low and high educated samples, potentially since high educated individuals are more likely to have other means to get medical care.

6.4.1. Effects on Inpatient Visits

In order to complement the findings on utilization, I use the Nationwide Inpatient Sample to study the effects on the number of inpatient stays. The full sample from these regressions comes from discharges for individuals ages 21 to 64 in hospitals of Southern states for years 2000 to 2010.

I start by presenting evidence of the reform (fewer people reporting Medicaid) using NIS data. I can investigate the effects of the reform by looking at the type of payment the hospital received for each discharge. The six payment categories are: Medicaid, Medicare, private, self-pay, no charge and other types of payment. Since NIS provides information on primary and secondary payment information, I am able to analyze changes in payment composition as well as change in overall type of payments. The most direct prediction from the TennCare disenrollment would be a reduction in Medicaid payments for hospitals in Tennessee compared to hospitals in other southern states. In Table 13 I present the results of changes in payment structure using different payment measures. I estimate a significant 19 percent decrease in the likelihood of having providing Medicaid as the source of payment. For private coverage I also find a decrease in coverage of about 6 percent. In terms of self-payment, I see an increase in this category of about 30 percent. This provides further evidence that the composition of payments for inpatient stays was changing drastically after the disenrollment.

In Table 14, I present evidence of the effects of the reform on the number of discharges. These regressions come from analyzing the data at the hospital quarter level. I estimate that after the reform there are approximately 86 fewer discharges per hospital quarter, and this amounts to a 22 percent reduction over the mean. When dividing the effect into discharges by age groups, I estimate that 91 percent of this decrease is attributed to non-elderly adults. Using the sample of non-elderly discharges, I estimate a 43 percent decline in discharges. This complements the findings from NHIS that demonstrates decreases in the number of times a person has been an inpatient.

7. Discussion and Conclusion

In this paper I have provided the literature's first evidence on the effects of losing public health insurance eligibility on population health outcomes. I find that the 2005 Tennessee Medicaid disenrollment significantly decreased health care access by making people less likely to see a doctor. Since the doctor can recommend certain tests or check-ups, one would expect individuals to have less preventive care as a result, but I do not find strong evidence for changes in preventive care with the exception of having a flu shot.[55] This could indicate that individuals are willing to invest in low-cost preventive care to avoid getting sick in the future since the expected cost of being sick has now increased. This indicates the presence of moral hazard and it is highlighted by the adoption of positive health behaviors (eating healthier and exercising) and reduction of harmful health behaviors (drinking and smoking). If individuals take better care of themselves this could be a channel through which the disenrollment can improve their health rather than decrease it.[56] In contrast, I also find evidence for decreases in health care access which can lead to negative health outcomes. Even with these two conflicting effects, I find evidence that the reform significantly decreased health. Since not everyone who lost public health insurance eligibility remained uninsured, the effects identified by the research design are average effects, which implies that there were some people for which the reform had a substantial negative impact on their health. To illustrate this possibility, we should think of an individual who had an episode of sickness. Their health will start to deteriorate, and the rate at which this happens can be accentuated by the fact that the individual avoids going to the doctor because of cost and not having access to prescription drugs. This potentially leads to a more severe decrease in his or her health. Once an individual decides to get medical care

[55] This is a mixed problem of precision and economic significance. For example for Mammograms in the full sample, the effect is about 1 percent decrease over the mean, but the confidence interval also include effects of about 10 percent. However for Pap Exams we can rule out effects bigger than 1.6 percent with the clustered standard errors

[56] Note that changes in health behaviors need not only to be explained by moral hazard argument, one could also hypothesize that losing health insurance could affect people's budget constraint which makes them less likely to drink or smoke. In either case, this is a mechanism of how losing health insurance could be affecting health behaviors and in turn health outcomes.

the place where he/she receives it may not be the same. I estimate that approximately 30 percent of individuals affected by the reform change their place of care specifically due to health insurance reasons. These individuals are less likely to report the doctor's office or HMO provider as their source of usual care and more likely to use either emergency departments, hospital outpatient department or clinics. This last finding refers to people reporting what is their source of usual care, but I also investigate if they actually use ED more. I find that the likelihood of going to the ED increases, about a 7 percent increase for the low-educated individuals. This increase of ED attendance occurs in the extensive and intensive margin. I also find evidence of decreases in the number of surgeries and the probability of having a surgery. This in accordance with the findings from the administrative data which I estimate to have a 40 percent reduction in non-elderly discharges. That is, individuals who have some leverage of choosing whether or not to get a procedure or the timing of the procedure would be less likely to get it because of the increased cost. This could imply larger negative health effects in the long-run or simply that the individuals could have had better health during the current period if they had health insurance.

These results are subject to some limitations. First, the survey outcomes are self-reported; however, there is substantial research that indicates self-reported health outcomes and objective health outcomes are strongly correlated. Also, I control for sources of reporting heterogeneity such as income, age and gender (Zierbarth, 2010). Second, I am not able to pin down all the mechanisms of how losing health insurance affects health other than forgone medical care and changes in health behaviors; it is possible that there are other mechanisms which I am not able to identify. For instance, given that the TennCare disenrollment increased employment (Garthwaite et al., 2014), it is possible that part of my effects can be driven by the gain in employment that the reform caused.[57]

Nevertheless, this is the first paper to study the effects of a sizeable public health insurance disenrollment on health care access, preventive care, health behaviors, health care utilization and self-assessed health. In doing so, I provide evidence of potential mechanism of how the disenrollment could affect health outcomes and subsequent health care utilization. Most of these results are consistent across two population representative surveys as well as administrative data on hospitalizations. In addition, my study focuses on a largely understudied population which ACA's Medicaid expansion explicitly targets: childless adults. My results provide evidence that losing health insurance is significantly detrimental for health care access and health. It also induces the use of ED and reduces the demand for surgeries. Further research should focus on other aspects of the effects of the disenrollment, such as the time they remain uninsured, more detailed information on the effects on prescription drugs and how losing health insurance affects the household consumption bundle. Finally, for welfare analysis, another relevant set of outcomes to study would be the effects of the reform on the supply side of health (i.e. wages of health practitioners, their hours worked, etc.). This will help us have a more complete picture on the broad effects of this particular disenrollment, which eventually can help inform policy-makers when making choices about changes to public health insurance eligibility and other alternatives policies.

[57] For the purposes of policy-making it is important to highlight that the counterfactual world I am currently comparing is Tennessee without disenrollment and implicitly without its budget deficit. In a true counterfactual world, Tennessee would have taken an alternative action in order to deal with the budget deficit.

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Figure 1: Number of People Enrollment in Tenncare

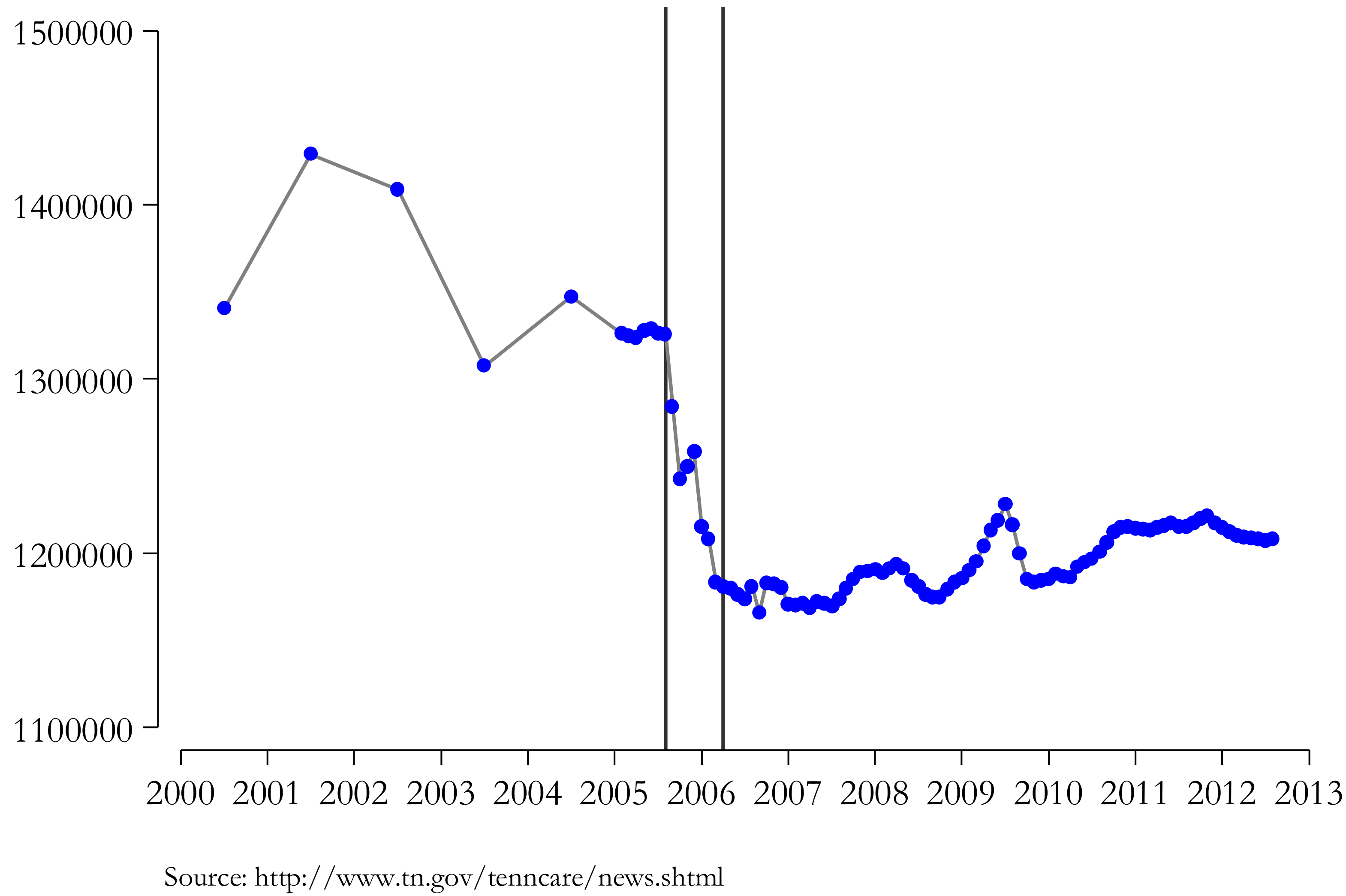
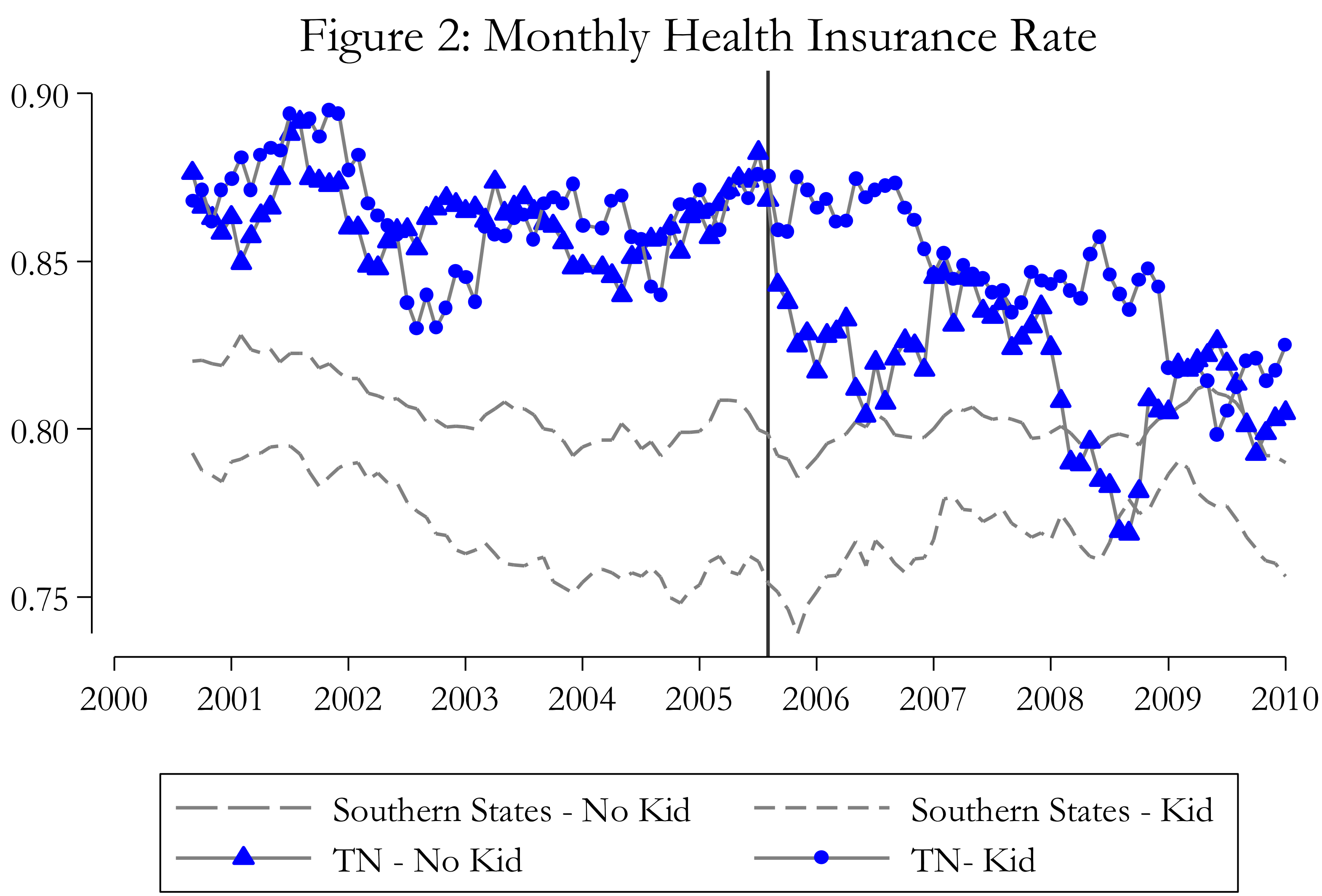


Figure 2: Monthly Health Insurance Rates Using BRFSS



All lines are tailing 8-month moving averages, and for Tennessee-Nokid the trailing moving average is computed separately for the time periods after August 2005.

Figure 3: Synthetic Control Method for Health Insurance Rates Using BRFSS 2000-2010

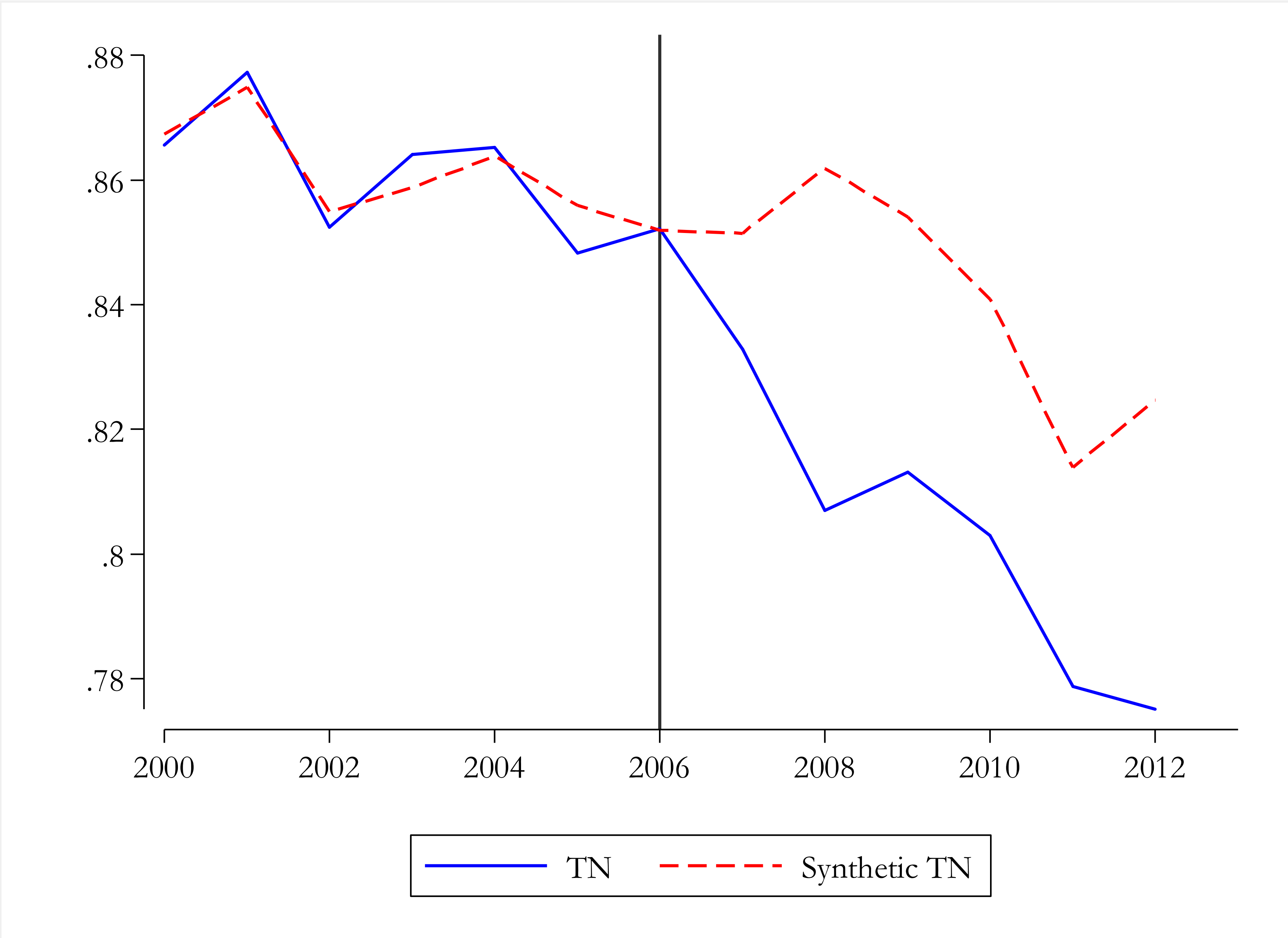


Table 1: Summary Statistics of Main Outcomes 2000-2010 BRFSS Data

	TN Before	Southern States Before	TN After	Southern States After	Total
Health Access					
Has Health Insurance	0.87	0.79	0.82	0.78	0.79
Forgone Medical Care in the past 12 months	0.13	0.17	0.19	0.19	0.18
Had a General Check-up in the past 12 months	0.76	0.69	0.74	0.67	0.68
Health Status					
Pr(Reporting Excellent Health)	0.21	0.23	0.19	0.22	0.22
Pr(Fair and Poor Self-reported Health)	0.17	0.15	0.18	0.15	0.15
Number of Days Physical Health Not Good	3.36	3.27	4.10	3.45	3.38
Number of Days Mental Health Not Good	3.42	3.63	3.66	3.82	3.72
Number of Days Incapacitated by Bad Health	4.34	4.03	5.90	4.51	4.32
Health Behaviors					
Participates in Physical Activity	0.70	0.74	0.72	0.75	0.74
Daily Servings of Fruits and Vegetables	4.21	3.61	3.74	3.67	3.66
Binge Drinking	0.09	0.16	0.10	0.15	0.15
Any Drink in past 30 Days	0.35	0.53	0.33	0.52	0.51
Avr. Drinks per Occassion is past 30 Days	0.81	1.32	0.76	1.29	1.28
Currently a Smoker	0.29	0.26	0.25	0.22	0.24
BMI Adjusted	27.73	27.51	28.69	28.25	27.88
Preventive Care					
Had Flu Shot in the past 12 months	0.28	0.24	0.34	0.31	0.28
Had a Blood Cholesterol check in the past 12 months	0.76	0.76	0.82	0.79	0.78
Had a Mammogram in the past 12 months for women over 50	0.68	0.65	0.65	0.64	0.64
Had a Breast Exam in the past 12 months for women over 21	0.74	0.68	0.67	0.65	0.67
Had a Pap Exam in the past 12 months for women over 21	0.75	0.69	0.66	0.64	0.67
Ever had a Prostate Specific Antigen Test for men over 40	0.53	0.55	0.52	0.59	0.57
Ever had a Rectal exam for men over 40	0.67	0.71	0.57	0.70	0.70
Chidless Status					
Currently Pregnant	0.04	0.04	0.03	0.04	0.04
No Children in the Household under age of 18	0.54	0.51	0.53	0.49	0.50
Number of Children	0.86	0.93	0.88	0.99	0.96

Table 2: Summary Statistics of Main Outcomes 2000-2010 NHIS Data

	TN Before	Southern States Before	TN After	Southern States After	Total
Health Access					
Uninsured Rate	0.14	0.23	0.20	0.25	0.23
Has Medicaid	0.09	0.04	0.08	0.05	0.05
Has Lost Medicaid in the past 12 months	0.01	0.01	0.03	0.02	0.02
Forgone Medical Care in the past 12 months	0.09	0.08	0.12	0.11	0.09
Has seen a doctor in the past 12 months	0.67	0.65	0.64	0.63	0.64
Health Status					
Pr(Reporting Excellent Health)	0.26	0.32	0.25	0.30	0.31
Pr(Fair and Poor Self-reported Health)	0.16	0.11	0.14	0.12	0.12
Number of Days Missed from Work	5.23	4.69	4.85	4.00	4.42
Number of Days Spent in Bed	7.11	4.86	6.98	5.26	5.15
Medical Care					
Change Health Care Place due to Health Insurance	0.03	0.03	0.03	0.02	0.03
Has an Usual Place of Care	0.86	0.82	0.84	0.79	0.81
Place of Usual Care					
Clinic	0.10	0.12	0.12	0.12	0.12
Doctor's Office or HMO	0.73	0.67	0.67	0.62	0.65
Emergency Department	0.01	0.01	0.01	0.02	0.01
Hospital Outpatient Department	0.00	0.01	0.01	0.01	0.01
Other Place	0.01	0.01	0.01	0.01	0.01
Health Care Utilization					
Visited the ED	0.23	0.21	0.22	0.21	0.21
Number of Times in ED in past 12 months — ED Visit==1	1.59	1.59	1.69	1.62	1.61
Had an Overnight Stay in the Hospital in the past 12 Months	0.09	0.08	0.08	0.08	0.08
Number of Overnights Stays	0.12	0.12	0.13	0.12	0.12
Average Number of Nights per Stay	3.45	4.11	3.96	4.28	4.16

Table 3: Summary Statistics of Main Outcomes 2000-2010 NIS Data

	TN Before	Southern States Before	TN After	Southern States After	Total
Payment Type					
Any Insurance	0.94	0.89	0.88	0.87	0.89
Medicaid	0.38	0.24	0.33	0.29	0.27
Medicare	0.20	0.16	0.24	0.18	0.17
Private	0.52	0.54	0.46	0.50	0.52
Self Pay	0.12	0.16	0.17	0.20	0.18
No charge	0.00	0.02	0.00	0.01	0.01
Other Type of Payment	0.02	0.05	0.02	0.05	0.05
Missing Information	0.01	0.00	0.01	0.00	0.00
Hospital Level					
Number of Hospitals	98	1060	91	1161	1583
Average Number of Discharges Per Hospital	23502	23815	20718	19983	22423
Average Total Charges in $millions	17,922	19,925	23,367	24,308	21,437
Number of diagnoses on this record	5.28	5.52	7.52	6.96	6.05
Number of procedures on this record	1.69	1.58	1.72	1.69	1.63
Discharge Type					
Emergency	0.44	0.47	0.47	0.45	0.46
Urgent	0.19	0.22	0.20	0.23	0.22
Elective	0.37	0.30	0.33	0.31	0.31
Discharge Admission					
ED Admission	0.47	0.45	0.45	0.34	0.43
Routine Admission	0.49	0.51	0.50	0.62	0.53
Transfer Admission	0.03	0.03	0.05	0.04	0.04

Table 4: Effects Disenrollment on Health Insurance Coverage Using BRFSS and NHIS 2000-2010

DD Model, All Adults	Lost Medicaid NHIS	Medicaid NHIS	Uninsured NHIS	Health Coverage BRFSS
TN X Post	0.011	-0.028	0.043	-0.031
	(0.002)	(0.002)	(0.004)	(0.002)
	{0.000}	{0.000}	{0.000}	{0.002}
	[0.012]	[0.000]	[0.000]	[0.000]
R-Square	0.03	0.07	0.17	0.16
N	193,086	193,086	193,086	841,757
DD model, sample with children < 18				
TN X Post	0.005	-0.009	0.013	-0.026
	(0.003)	(0.003)	(0.005)	(0.003)
	{0.115}	{0.008}	{0.019}	{0.003}
	[0.464]	[0.332]	[0.291]	[0.005]
R-Square	0.06	0.11	0.18	0.19
N	76,227	76,227	76,227	355,693
DD model, sample without children < 18				
TN X Post	0.014	-0.030	0.052	-0.036
	(0.001)	(0.002)	(0.005)	(0.003)
	{0.000}	{0.000}	{0.000}	{0.003}
	[0.001]	[0.000]	[0.000]	[0.000]
R-Square	0.01	0.07	0.15	0.14
N	113,610	113,610	113,610	485,175
DDD Model				
TN X Post X No Children under 18	0.018	-0.027	0.045	-0.017
	(0.003)	(0.003)	(0.003)	(0.002)
	{0.000}	{0.000}	{0.000}	{0.000}
	[0.012]	[0.015]	[0.003]	[0.089]
R-Square	0.04	0.08	0.16	0.17
N	189,837	189,837	189,837	840,868
Mean of Dependent	0.014	0.09	0.14	0.82

Each coefficient comes from a different specifications, all of them were estimated using OLS. All DD models include state and year fixed effects while DDD models include state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In each model I control for race, gender, education, age and marital status. Standards error in parenthesis are obtained from cluster standard errors. The P-values from with cluster standard errors are in {} while the p-values in [] were obtained using the standard errors from a modified block-bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee for non-elderly adults.

Table 5: Effects of Disenrollment on Health Care Access Using NHIS and BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Using NHIS				
Medicaid	0.09	-0.027	-0.048	-0.015
		(0.003)	(0.014)	(0.007)
		{0.000}	{0.002}	{0.039}
		[0.015]	[0.014]	[0.154]
Losing Medicaid	0.014	0.018	0.036	0.002
		(0.003)	(0.008)	(0.005)
		{0.000}	{0.000}	{0.692}
		[0.012]	[0.012]	[0.687]
Uninsured	0.14	0.045	0.052	0.040
		(0.003)	(0.024)	(0.018)
		{0.000}	{0.036}	{0.031}
		[0.003]	[0.060]	[0.014]
Panel B: Using BRFSS				
Insured	0.82	-0.017	-0.026	-0.011
		(0.002)	(0.006)	(0.003)
		{0.000}	{0.000}	{0.003}
		[0.089]	[0.170]	[0.308]

Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age, and marital status. Clustered Standards error in parenthesis and associated p-values are in {}. The block-bootstrapped p-values are in [ ] The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 6: Effects of Disenrollment on Health Care Access Using NHIS and BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Using NHIS				
Pr(Forgone or Delay Care due to Cost in past 12 months)	0.11	0.032	0.042	0.023
		(0.003)	(0.005)	(0.002)
		{0.000}	{0.000}	{0.001}
		[0.004]	[0.035]	[0.106]
Pr(Cannot Afford Prescription Drugs)	0.11	0.019	0.044	-0.001
		(0.004)	(0.008)	(0.005)
		{0.000}	{0.000}	{0.845}
Pr(Seen/talk to a general doctor in past 12 months)	0.59	-0.024	-0.042	-0.003
		(0.007)	(0.017)	(0.008)
		{0.003}	{0.025}	{0.712}
		[0.442]	[0.393]	[0.934]
Panel B: Using BRFSS				
Pr(Forgone Care due to Cost in past 12 months)	0.12	0.013	0.038	-0.007
		(0.002)	(0.007)	(0.002)
		{0.000}	{0.000}	{0.014}
		[0.259]	[0.055]	[0.582]
Pr(Had a Dr Check-up in the past 12 months)	0.76	-0.022	-0.039	-0.013
		(0.003)	(0.008)	(0.004)
		{0.000}	{0.000}	{0.010}
		[0.246]	[0.157]	[0.585]

Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age, and marital status. Clustered Standards error in parenthesis and associated p-values are in {}. The block-bootstrapped p-values are in [ ] The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 7: Effects of Disenrollment on Preventive Care Using BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample HS Degree or Less	Sample More than a HS Degree
Panel A: General Population				
Had a Flu shot in the past 12 months?	0.32	0.027	0.024	0.028
		(0.003)	(0.003)	(0.004)
		{0.000}	{0.000}	{0.000}
		[0.025]	[0.152]	[0.091]
Had Cholesterol Check in the past 12 months?	0.61	-0.021	-0.012	-0.033
		(0.004)	(0.012)	(0.005)
		{0.074}	{0.487}	{0.007}
		[0.219]	[0.643]	[0.113]
Panel B: For Women				
Had a Mammogram in the past 12 months?	0.70	-0.007	0.011	-0.030
(Over 50)		(0.031)	(0.039)	(0.040)
		{0.369}	{0.409}	{0.464}
		[0.831]	[0.791]	[0.474]
Had a Breat Exam in the past 12 months?	0.75	-0.008	0.009	-0.023
(Over 21)		(0.006)	(0.006)	(0.010)
		{0.195}	{0.174}	{0.043}
		[0.644]	[0.726]	[0.307]
Had a Pap Exam in the past 12 months?	0.72	0.002	-0.003	0.004
(Over 21)		(0.005)	(0.008)	(0.004)
		{0.706}	{0.704}	{0.419}
		[0.903]	[0.907]	[0.852]
Panel C: For Men over 40				
Had a PSA Exam in the past 12 months?	0.43	-0.003	-0.063	0.041
		(0.009)	(0.021)	(0.007)
		{0.771}	{0.008}	{0.000}
		[0.945]	[0.250]	[0.430]
Had a Rectal Exam in the past 12 months?	0.39	0.001	-0.017	0.012
		(0.0045)	(0.011)	(0.008)
		{0.836}	{0.147}	{0.169}
		[0.977]	[0.758]	[0.829]

Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, State, Year, Childless status Fixed Effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block-bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 8: Effects of Disenrollment on Health Behaviors Using BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or Less	Sample: More than HS Degree
Panel A: Summary Measure for Health Behaviors				
Index "Taking Better Care	-0.04	0.011	0.091	-0.049
		(0.022)	(0.040)	(0.025)
		[0.608]	[0.022]	[0.044]
Panel B: BMI and Non-risky Health Behaviors				
Participates in Physical Activity	0.73	0.004	0.012	0.001
		(0.003)	(0.006)	(0.003)
		{0.248}	{0.086}	{0.623}
		[0.742]	[0.539]	[0.918]
Daily Servings of Fruits and Vegetables	3.63	-0.095	0.0140	-0.178
		(0.019)	(0.029)	(0.038)
		{0.000}	{0.633}	{0.000}
		[0.210]	[0.906]	[0.101]
Panel C: Risky Health Behaviors				
Bing Drinking	0.12	-0.002	-0.021	0.017
		(0.003)	(0.001)	(0.003)
		{0.448}	{0.000}	{0.000}
		[0.849]	[0.217]	[0.190]
Any Drink in Past 30 Days	0.47	-0.015	-0.026	-0.006
		(0.005)	(0.007)	(0.005)
		{0.010}	{0.002}	{0.261}
		[0.324]	[0.219]	[0.769]
Drinks per Occassion in the Past 30 Days	1.08	-0.074	-0.222	0.068
		(0.020)	(0.037)	(0.016)
		{0.002}	{0.000}	{0.001}
		[0.421]	[0.132]	[0.276]
Currently a Smoker	0.27	0.006	-0.001	0.007
		(0.002)	(0.003)	(0.003)
		{0.004}	{0.679}	{0.019}
		[0.587]	[0.942]	[0.589]

Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, State, Year, Childless status Fixed Effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block-bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 9: Effects of Disenrollment on Self-Assesed Health Using NHIS and BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Using NHIS				
Pr(Reporting Excellent Health)	0.30	0.026	0.021	0.030
		(0.005)	(0.005)	(0.008)
		{0.000}	{0.001}	{0.001}
		[0.135]	[0.327]	[0.243]
Pr(Reporting Fair or Poor Health)	0.12	-0.014	-0.038	0.0003
		(0.002)	(0.003)	(0.002)
		{0.000}	{0.001}	{0.981}
		[0.296]	[0.093]	[0.982]
Panel B: Using BRFSS				
Pr(Reporting Excellent Health)	0.20	-0.005	0.002	-0.008
		(0.002)	(0.002)	(0.003)
		{0.023}	{0.360}	{0.007}
		[0.628]	[0.882]	[0.595]
Pr(Reporting Fair or Poor Health)	0.20	0.009	0.016	0.002
		(0.003)	(0.005)	(0.003)
		{0.005}	{0.005}	{0.638}
		[0.273]	[0.354]	[0.877]

Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 10: Effects of Disenrollment on Having Bed Days Using NHIS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Using NHIS				
Number of Bed Days in past 12 months	5.1	0.666	1.592	0.875
		(0.330)	(0.619)	(0.328)
		{0.060}	{0.020}	{0.012}
		[0.723]	[0.574]	[0.724]
Panel B: Using BRFSS				
Number of Days with Bad Physical Health	3.9	-0.067	0.294	-0.413
		(0.049)	(0.102)	(0.064)
		{0.190}	{0.011}	{0.000}
		[0.749]	[0.429]	[0.092]
Number of Days with Bad Mental Health	3.3	0.132	0.229	0.026
		(0.055)	(0.069)	(0.092)
		{0.028}	{0.004}	{0.786}
		[0.554]	[0.575]	[0.914]
Number of Days of Incapacitation	4.7	0.836	1.213	0.437
		(0.064)	(0.114)	(0.064)
		{0.000}	{0.000}	{0.000}
		[0.018]	[0.027]	[0.260]

Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block bootstrap procedure. The mean of the dependent is the pre-treatment mean in Tennessee for childless adults.

Table 11: Effects of Disenrollment on Place to go for Medical Care when Sick NHIS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Place of Usual Care when sick				
Pr(Change health care place due to health insurance)	0.03	0.019	0.039	0.002
		(0.002)	(0.003)	(0.003)
		{0.000}	{0.000}	{0.514}
		[0.075]	[0.019]	[0.916]
Pr(Has usual place of care)	0.81	0.033	0.039	0.033
		(0.006)	(0.012)	(0.004)
		{0.034}	{0.005}	{0.000}
		[0.149]	[0.291]	[0.218]
Panel B: Type of Usual Place Care when sick				
Pr(Usual place is Clinic)	0.12	0.022	0.021	0.025
		(0.004)	(0.005)	(0.005)
		{0.049}	{0.001}	{0.000}
		[0.270]	[0.512]	[0.301]
Pr(Usual place is Dr or HMO)	0.65	0.009	-0.015	0.033
		(0.007)	(0.012)	(0.007)
		{0.216}	{0.265}	{0.000}
		[0.748]	[0.738]	[0.366]
Pr(Usual place is ED)	0.01	0.004	0.021	-0.009
		(0.001)	(0.002)	(0.001)
		{0.001}	{0.000}	{0.000}
		[0.568]	[0.159]	[0.168]
Pr(Usual place is Hospital Outpatient Department)	0.01	0.002	0.006	-0.001
		(0.001)	(0.001)	(0.002)
		{0.062}	{0.000}	{0.624}
		[0.508]	[0.309]	[0.834]
Pr(Does not have a usual place of care)	0.19	-0.032	-0.037	-0.033
		(0.006)	(0.013)	(0.005)
		{0.000}	{0.012}	{0.000}
		[0.152]	[0.355]	[0.233]

Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 12: Effects of Disenrollment on Hospital Health Care Using NHIS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Pr(Going to the ED in the past 12 months)	0.22	-0.0003	0.015	-0.021
		(0.004)	(0.007)	(0.005)
		{0.941}	{0.048}	{0.000}
		[0.988]	[0.732]	[0.461]
Number of times in ED in the past 12 months	0.33	-0.008	0.026	-0.048
		(0.007)	(0.019)	(0.011)
		{0.269}	{0.190}	{0.000}
		[0.881]	[0.748]	[0.448]
Pr(Had surgery in the past 12 months)	0.12	-0.028	-0.062	-0.006
		(0.006)	(0.007)	(0.009)
		{0.000}	{0.000}	{0.509}
Number of surgeries in the past 12 months	0.15	-0.034	-0.109	0.028
		(0.008)	(0.009)	(0.012)
		{0.001}	{0.000}	{0.033}
Pr(Had any overnight hospital stay in the past 12 months)	0.08	0.008	0.003	0.010
		(0.003)	(0.004)	(0.003)
		{0.017}	{0.464}	{0.004}
		[0.474]	[0.845]	[0.474]
Number of times being an inpatient in the past 12 months	0.13	-0.013	-0.058	0.009
		(0.005)	(0.009)	(0.007)
		{0.019}	{0.000}	{0.217}
		[0.603]	[0.337]	[0.701]
Average number of nights per stayed if Overnight	3.55	-0.194	1.982	-2.841
		(0.413)	(0.653)	(0.186)
		{0.645}	{0.008}	{0.000}
		[0.883]	[0.244]	[0.206]

Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 13: Effects of the Reform on Payment Types Using NIS 2000 - 2010

	Medicaid	Self-Pay	Private	Medicare
TN X Post	-0.039	0.018	0.0280	-0.012
	(0.001)	(0.001)	(0.001)	(0.001)
	{0.000}	{0.000}	{0.000}	{0.000}
N	31,55,042	31,55,042	31,55,042	31,550,42
R-Squared	0.0612	0.0589	0.124	0.0337
Pre Mean in TN	0.27	0.11	0.43	0.21

Using 100 percent of Data. State, Year-Quarter, Hospital and Month FE Using Years 2000 thru 2010

Table 14: Effects of the Reform on Number of Discharges Using NIS 2000 - 2010

	Number of Discharges Total	Number of Discharges Non-Elderly Adults	Number of Discharges Elderly	Number of Discharges Under 18
TN X Post	-85.71	-78.42	-12.21	4.865
	(0.033)	(0.014)	(0.026)	(0.009)
	{0.000}	{0.000}	{0.000}	{0.000}
N	17,134	17,134	17,134	17,134
R-Squared	0.988	0.980	0.987	0.978
Pre Mean in TN	397.2	184.1	136.9	76.12

Each coefficient comes from a DD specifications for different sub-samples estimated using OLS. All models include state, year-quarter, state-year, and hospital fixed effects. I use 20 percent of the discharges per hospital. Standards error in parenthesis and P-values are in brackets both obtained from a modified block bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee.