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Effects of losing public health insurance on preventative care, health, and emergency department use: Evidence from the TennCare disenrollment

Daniel Sebastian Tello-Trillo

Public Policy and Economics, Frank Batten School of Leadership and Public Policy, University of Virginia, Charlottesville, Virginia, USA

Correspondence
Daniel Sebastian Tello-Trillo, Public Policy and Economics, Frank Batten School of Leadership and Public Policy, University of Virginia, Charlottesville, VA, USA.
Email: dst2c@virginia.edu

Abstract

This paper studies the effect of losing public health insurance eligibility on preventative care, self-reported health, and emergency department use. I exploit the 2005 TennCare disenrollment in which 190,000 residents–mainly non-elderly childless adults–lost public health insurance eligibility due to budget cuts. I use two surveys, the Behavioral Factor Surveillance System and the National Health Interview Survey, in a difference-in-difference methodology to study the effects of the reform. I find that the reform lead to a 4%–5% reduction in reporting having mammograms and breast exams. An increase of 20% in number of days with health incapacitation and no strong evidence of changes of emergency department visits (nor number of visits). I document margins of heterogeneity of the effects across demographic characteristics. Finally, I explore the margins of symmetry between gaining and losing public insurance by comparing estimates to those from the Affordable Care Act Medicaid Expansions.

KEYWORDS

disenrollment, emergency department use, health reform, Medicaid, preventative care, public health insurance, TennCare

JEL CLASSIFICATION
I10; I12; I13

1 | INTRODUCTION

This paper studies the effects of losing health insurance on preventative care, self-reported health, and emergency department (ED) use. I consider the impact of one of the largest public health insurance disenrollments in the United States: the 2005 Tennessee disenrollment in which ∼190,000 residents were disenrolled from the state's Medicaid Program, known as "TennCare." The disenrollment amounted to 10% of those enrolled in TennCare and ∼3% of the total state population (Chang & Steinberg, 2008). I examine survey level data coupled with a differences-in-differences methodology to study the effects of the disenrollment.

Evidence from the TennCare disenrollment can provide insights into the policies that result in individuals (especially vulnerable populations) losing public health insurance. For example, in the United States, there have been many Congressional and constitutional attempts to repeal different measures of the Affordable Care Act (ACA), including repealing the recent Medicaid expansions (e.g., 115 Congress of the United States [2017]).[1] There are also calls from policymakers to convert Medicaid from an entitlement program to a block grant program. Such a change in program structure could lead to large-scale coverage losses (Goodman-Bacon & Nikpay, 2017). Further, 15 states have a 1115 Medicaid Waiver pending or approved that compels some Medicaid enrollees to work, seek employment, or perform other pro-social activities to remain eligible (Kaiser Family Foundation, 2019). Imposing Medicaid work requirements can lead to coverage loss. A recent study by Sommers et al. (2019) found that the Arkansas Medicaid work requirement led to a 6.8 percentage point decrease in Medicaid enrollment. A simulation analyses and first glance evidence implied that these waivers, if applied nationally, will cause up to 4M of the 23.5M currently eligible enrollees to lose coverage (Brantley & Ku, 2018; Garfield et al., 2018).[2]

There are two components of this reform that elevate its importance. First, is the fact that the TennCare disenrollment targeted non-elderly childless adults, which is a group targeted by the recent ACA Medicaid expansions.[3] Second, the treatment studied is people

[1]A first attempt was in 2011, with the "H.R.2—Repealing the Job-Killing Health Care Law Act," while at the same time there was a lawsuit that ended in the Supreme Court ruling the ACA as constitutional. In 2013, a bill was submitted by Michelle Bachmann—H.R.45—113th Congress (2013–2014)—to repeal the ACA. CNN produced an article found here (https://edition.cnn.com/2015/02/03/politics/obamacare-repeal-vote-house/index.html), which stated that "This latest vote marked the 67th time the House has voted to entirely repeal, defund or change some provisions of President Barack Obama's signature health care law." In addition, see, for example, https://www.politico.com/story/2019/01/11/trump-bypass-congress-medicaid-plan-1078885 (last accessed February 3, 2020). In the 115th Congress, the bills used to repeal the ACA have been named: American Health Care Act, Better Care Reconciliation Act, and the Health Care Freedom Act.

[2]Further, there are other situations that could involve the loss of Medicaid. For example, a paper by Halliday et al. (2019) studies a reform in the State of Hawaii, which stopped covering the majority of migrants from countries belonging to the Compact of Free Association (COFA) in its Medicaid program. In the global context, since 2010 countries have started to restrict access to public health insurance to different types of migrants. Spain enacted such a change in 2012, and in turn, there was a spike in mortality of the undocumented population (Mestres et al., 2018).

[3]This makes the TennCare reform more similar to the ACA Medicaid expansions than the previous well-studied health reforms. See Table A1 in Garthwaite et al. (2014) for comparisons of targeted population across reforms.

losing health insurance rather than gaining health insurance, a more studied treatment. The effects of losing health insurance may not necessarily be symmetric to the effects of gaining health insurance. The main difference relies on the accumulation of information and health capital: a person who has had health insurance for an extended period of time could have higher levels of health capital and information than a person who has not had health insurance. For instance, consider a woman with diabetes who has had health insurance for an extended period of time. During this time, she has been able to learn about her chronic condition, the degree of the problem, and how to handle it (e.g., she has received information about the importance of an adequate diet, and what type of drugs she should be taking). Once this person loses health insurance, even though her health care access is reduced (e.g., less access to drugs due to higher pricing), she does not lose the information she has on her health condition and how to manage it (e.g., diet). In contrast, consider the same woman who starts without health insurance. In that case, 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 will her health care access increase, but she may also experience substantial and immediate information gains. A second major difference may be the "shock" of losing something and its impact on mental health. This is related to evidence that people react differently to losing versus gaining, an idea that comes from the social psychology literature (Keysar et al., 2008) and theoretical background of the endowment effect and loss aversion (Mullainathan & Thaler, 2000). These and other examples illustrate the possibility of asymmetries in the effects of losing and gaining health insurance.

This paper belongs to an emerging literature on studying disenrollments globally (Mestres et al., 2018) and locally (Halliday et al., 2019). Studies of the TennCare disenrollment have focused on employment (DeLeire, 2019; Garthwaite et al., 2014), financial outcomes (Argys et al., 2020; Garthwaite et al., 2018), health-care access (DeLeire, 2019; Tarazi et al., 2017), mental health (Maclean et al., 2019), and hospitalizations (Ghosh & Simon, 2015). This paper is the first to use the National Health Interview Survey (NHIS) and to look at outcomes of preventative care, self-reported health and ED utilization. First, I present evidence that the disenrollment did leave people uninsured (and specifically reporting having "Lost Medicaid"). The effect is larger for childless adults rather than adults with children, though the latter group was still treated. Second, I find evidence that the disenrollment led to a decrease in having mammograms and breast exams of 3%–4%. Third, I find evidence of increases in the number of days with a health limitation (i.e., a deterioration of health). Finally, I do not find evidence of changes in ED visits, in either the extensive or intensive margin. Even though these rates are not changing, I present suggestive evidence that the rate at which people are coming in uninsured is increasing. I also provide evidence of people reporting *not going to any place* to get healthcare when they are sick or need preventative care. I also explore margins of health behaviors, but all of these fail to pass support of parallel trends, making it harder to interpret the results from these models.

2 | LITERATURE REVIEW

In contrast to the numerous studies on the effects of gaining public insurance eligibility on health outcomes, studies on losing public insurance eligibility on health outcomes are fewer and recent in the economics literature. DeLeire (2019) uses the Survey of Income and Program

Participation (SIPP) to study the effects of this reform on employment and health outcomes. The author does not find evidence that the disenrollment increased employment but found decreases in metrics of access to care and self-reported health. Tarazi et al. (2017) use a difference-in-difference setting where they compare Tennessee to other southern states before and after the reform. They use the Behavioral Risk Factor Surveillance System (BRFSS) to study three outcomes: health coverage, having a personal doctor, and having cost as a barrier to access health care. They find that the reform led to a decrease in coverage and health-care access, namely higher report of "Cannot afford care due to cost." In this paper I use BRFSS as well, but I use a number of outcomes not used in the Tarazi et al. paper (namely, number of days with bad physical health, bad mental health and with an incapacitation, measures of preventative care, as having a flu shot, breast exam, pap exam, and prostate cancer screening (PSA) exam, and health behaviors like exercising, drinking and smoking). In the results section, I compare my results to theirs.[4] Ghosh and Simon (2015), using the nationwide inpatient sample (NIS), study the effect of the TennCare reform on inpatient hospitalizations. They find that the disenrollment decreased the share of hospitalizations covered by Medicaid by 21%. They also find a 75% increase in uninsured hospitalizations originating from the ED. Maclean et al. (2019) study the effects of the TennCare disenrollment on mental health hospitalizations; they do not find evidence of changes in the number of mental health related hospitalizations but do find evidence of a slight decrease in substance-use related hospitalizations. There are other papers that studied the TennCare disenrollment that focus on labor market outcomes (Garthwaite et al., 2014) and financial health (Argys et al., 2020).[5]

Although this paper carries some similarities with the previous papers, it provides contributions in different margins: this paper is the first to use the NHIS to study the effects of the reform, it focuses on two outcomes that have not been studied before (preventative care and ED usage), it applies a method for statistical inference that is more appropriate for the setting, it uses larger samples (NHIS and BRFSS provided much greater sample-size for the treated group than SIPP), and it provides estimates that are representative at the state level (as opposed to the NIS, which is not state representative).

3 | INSTITUTIONAL BACKGROUND

Before 1994, Tennessee had a traditional fee-for-service Medicaid program. One of the main funding sources for Tennessee's Medicaid was a special tax on hospitals and providers that was going to expire in 1994. Given the termination of the tax revenue source and the increasing Medicaid costs, there was a need to re-think how Medicaid was funded.[6] The nationwide political

[4]There are other empirical differences with the Tarazi el al. analysis and this paper. The major differences are: (a) they subset their sample to household below 200% FPL, (b) they use years 2003–2008, (c) they use a selected set of comparison states: AL, AR, GA, KY, and VA., and (d) they use weighted standard errors.

[5]Garthwaite et al. (2014) study the effects of the 2005 TennCare disenrollment on employment and labor force participation. They find that the reform was associated with a 4.6 percentage point increase in employment for childless adults. In addition, in Garthwaite et al. (2018), the authors use the Tennessee reform to study the effects of the disenrollment on uncompensated care provided by hospitals. They found that the disenrollment caused an increase of $138 million dollars in uncompensated care.

[6]A Tennessee state budget report projected a budget deficit of $250 million that was largely driven by increased Medicaid spending. Around $400 million of the funding for Medicaid came from the special tax on hospitals and nursing homes.

context of this time carried the push for more expansive public health insurance reform at the federal level; it was in this context that Governor McWherther decided to create a major reform of Tennessee's Medicaid program. The first step—started in 1994—was to transition all Medicaid beneficiaries into a managed care program that contracted with the renamed TennCare. Establishing TennCare had two goals: to cost control and to expand eligibility of public health insurance. The expected savings from transitioning current enrollees into managed-care were allocated to support a (categorical) expansion of Medicaid eligibility to non-disabled childless adults and uninsurable adults. Uninsurable adults were defined as adults with pre-existing conditions who have prohibitively high premiums (Farrar et al., 2007).[7] During this period TennCare enrollment surged and covered up to 19.29% of the adult population (21–64) in early 2005.[8]

By 2000, health expenditures were rising faster than Tennessee's budget.[9] Independent auditors recommended to reduce coverage, cut benefits, or increase taxes as ways to mitigate financial burden.[10] 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. In January 2005, Bredesen announced that a major disenrollment would happen that year, and that it would roll back the 1994 expansions, specifically the categorical eligibility requirements of childless adults and "uninsurables."[11]

By August 2005, individuals started receiving notifications stating that their TennCare health insurance coverage was terminated. This disenrollment continued until May 2006; in total, ∼190,000 residents were dropped from the program. This represents around 3%–5% of the non-elderly (18–64) population in Tennessee or around 10% of the total Medicaid population.[12]

Figure 1 illustrates the disenrollment with raw administrative data from TennCare. This graph shows the number of people enrolled in TennCare; the data are annual before 2005 and monthly after 2005. These raw data confirm there was a very large and sharp decrease in the TennCare enrollments during this period of time. Garthwaite et al. (2014) provides a table in their online appendix (table A1) which describes the population that was targeted by the disenrollment: in their estimations around 91% were childless adults, around 70% of them were 35–64, 58% were female, 75.9% were white, and 86% had a high school degree or less.

I emphasize disenrollment effects in this study; however, it is important to note that, in addition to the insurance losses, coverage generosity was curtailed to some extent among

[7]The uninsurable adults' enrollment was capped in 1995, resumed in 1997, capped again in 2000 and reopened in 2002.

[8]The income eligibility for childless adults was the same as traditional Medicaid. However, for the uninsurable category, the income eligibility was expanded up to 400% of the federal poverty line, with the inclusion of premiums and co-pays that scale up with income. Table 4 in this document (https://www.urban.org/sites/default/files/publication/62071/309341-The-Role-of-TennCare-in-Health-Policy-for-Low-Income-People-in-Tennessee.PDF) presents an example of the premiums scale in 1998 (Conover, 2000).

[9]In 2004 TennCare accounted for one-third of the state budget (Farrar et al., 2007).

[10]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 (Kaiser, 2009). 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.

[11]Most people were not aware if they would be dropped, since it was difficult for them to know which path made them eligible for Medicaid. In addition, given the back-and-forth of the past 2 years, people affected by the disenrollment were not necessarily aware that they would be disenrolled or when it would happen. Importantly, I do not observe in the data any evidence of high demand before the disenrollment.

[12]There is no official report of the exact number of people who lost TennCare due to the changes in 2005. All the numbers are estimates, these range between 170,000 and 190,000.

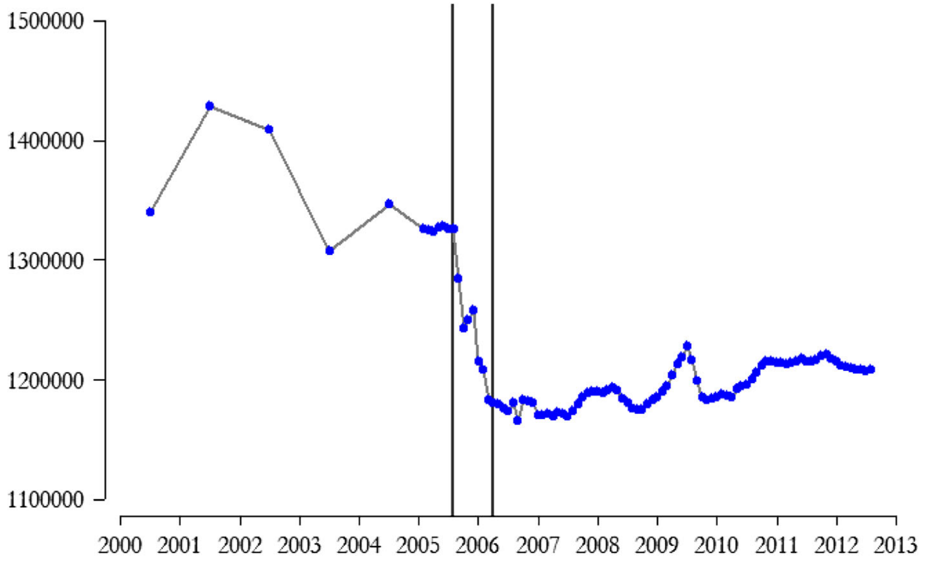

FIGURE 1 Number of people enrolled in TennCare over months. Before 2005, Tennessee only recorded annual counts on TennCare enrollment. Starting in 2005 they kept monthly records. This explain the noise between 2000 and 2005 and post 2005. Data from the bureau of TennCare [Color figure can be viewed at wileyonlinelibrary.com]

continuing enrollees.[13] Nonetheless, coverage remained generous and, in line with the broader TennCare literature, I assume that the effects of insurance losses dwarfed the effects of other changes. Moreover, most large-scale insurance policies (e.g., the ACA, Massachusetts healthcare reform) include various coverage changes as well.[14]

4 | DATA

In this section, I describe the data sets used in my analysis. I use two major data sets: the 2000–2009 BRFSS and restricted versions of the 2000–2009 NHIS with state identifiers.[15] To complement the survey data, I use administrative data on ED admissions provided by the Tennessee Department of Health.

[13]Three features of the overall reduction in Medicaid coverage are important to consider. First, coverage was curtailed for those individuals who remained eligible for TennCare. Continuing enrollees were limited to four prescriptions per month and 20 days of inpatient care per year. Second, the state developed a Health Care Safety Net program, funded with $184M (2018 dollars; inflated from the original estimates, $140M in 2005 dollars, using the Consumer Price Index), to provide care and assistance to disenrollees. This program included the Mental Health Safety Net (MHSN). Reports indicate that registration with the MHSN by disenrollees with serious mental health was only 65%. At best, the program was able to provide some temporary assistance to disenrollees. Third, community health centers and faith-based organizations were able to absorb some demand from the newly uninsured. Interviews with disenrollees suggest that many had substantial difficulty accessing needed healthcare services after TennCare was terminated. The available literature clearly shows that the disenrollment had a substantial and negative effect on Medicaid enrollment and coverage overall.

[14]To learn about the background of TennCare, I suggest reading Bennett (2014).

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

These surveys complement each other well: BRFSS contains a large number of observations that can be identified at the state level. Also, the BRFSS contains several questions on preventive care, and the questions are consistent over the sample period; by contrast, the NHIS only asks about certain preventive care in some years. On the other hand, the NHIS has detailed questions on the type of health insurance (which the BRFSS does not provide). This is critical information since the reform may have induced changes in different types of coverage. In addition, the NHIS contains questions on utilization of medical care that the BRFSS does not offer (importantly information on ED visits). Since both surveys have their advantages and disadvantages, they serve as useful complements of each other.

I study four categories of outcomes: health insurance, preventive care, self-assessed health, and ED use. Although both surveys contain many questions within these categories, I only include questions that appear at least 7 of out the 10 years of the main sample. For the BRFSS and the NHIS, I exclude from the sample individuals aged 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 parents' health insurance or the state children's health insurance program.

4.1 | Behavioral Risk Factor Surveillance System

The BRFSS is a telephone survey that started in 1984. The survey is a monthly cross-section, which includes questions on preventative care, self-reported health, and health behaviors. It also contains standard demographic characteristics such as age, race, marital status, education, and 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 the information into an annual data set at the state level.

In the BRFSS, for health insurance, 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?" I code a "Yes" as 1 and "No" as 0.

The preventive care outcomes derive from questions in the BRFSS that ask if the respondent has ever had a given preventative care test, and if they have, how long since the last time they took it. Based on these variables I create a variable labeled "In the past 12 months have you had a (Preventive care Test)?" I assign the value of 1 for a "Yes" and 0 for a "No" response. Note that if a person has never had a test, they will be coded as 0. For questions about preventive care that are specific to gender or age, I define the variables only for those that are recommended by the United States Preventive Services Task Force (USPSTF): having a mammogram for women over 50, having a breast exam for women over 21, and having a Pap test for women over 21, having a PSA for men over 40.

For the health outcomes, I use questions from the BRFSS regarding the number of days an individual has had (a) bad physical health, (b) bad mental health, and (c) been incapacitated due to a health issue.[16] These questions have the following format in the survey: "During the past 30 days, for about how many days did poor physical or mental health keep you from doing

[16]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.

your usual activities, such as self-care, work, or recreation?" Individuals report the number of days from 0 to 30. In the models estimated, I use two versions of these outcomes. I use the raw continuous number of days as a measure of "intensive margin," and then I create a binary variable that takes the value of 1 if the number of days is equal or greater than 5, and 0 otherwise. This variable is capturing the probability of reporting a minimal number of health days or the extensive margin.[17]

4.2 | National Health Interview Survey

The 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 National Center for Health Statistics (NCHS) and the CDC administer the survey, and interviewers—trained at the U.S. Bureau of the Census—collect the data. This survey asks questions about all 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 for more detailed information on their health and health care utilization. I use outcomes from the Household file and the Sample Adult file. For each sample file, I use the weights adjustment indicated by the NCHS.

The NHIS has detailed information about the health plan in which the individual is enrolled. From a question on what type of plan is their primary health coverage, I create five discrete variables: having any health coverage, having Medicaid, having Medicare, having private insurance, or other type of health insurance. In addition, the NHIS surveyors ask people who report not having health insurance to give reasons for not having health insurance, and one of the options is having "Lost Medicaid." I use this variable as direct evidence of the disenrollment, however, it should be noted that since this is a question to the uninsured, this represents an undercount, as some people may have obtained other sources of health insurance even though they lost Medicaid.[18]

For utilization of medical care, I use a question from the NHIS regarding number of times an individual has visited the ED in the past 12 months. Using the information from the number of times in the ED I construct two variables. One is the raw number of times (intensive margin) and the second one is the probability of ever going to the ED, which is a discrete variable that takes the value of 1 if the individual reports at least 1 visit in the past 12 months and 0 otherwise. The second question from the NHIS that I use is "Where do you usually get care when sick?," and one of the options is "does not go to a place" or "doesn't have a place to go." I create a variable called "Doesn't go to a place when sick" that takes the value of 1 if they responded yes, and 0 otherwise. Similarly, NHIS ask a question about where does one go to get preventative care, and one of the options is "does not go anywhere." I use this to code a variable does not to one place for preventative care, which takes the value 1 if the responded indicates they do not have a place to go or does not go.

[17]There is bunching that occurs at days 1 and 2 therefore I pick at least 5 days as a measure that should capture a nontrivial increase in the share of people reporting more days with bad health. The results are similar if I use the "at least 1" day margin.

[18]The surveyor asks "Which of these are reasons you stopped being covered?" The options involving a Medicaid-related reason are: "Medicaid/Medical plan stopped after pregnancy," "Lost Medicaid/Medical plan because of new job or increase in income" and "Lost Medicaid (other)."

In Table 1, I compare the characteristics of Tennessee versus other southern states (see next section for a list of states) using data from before the disenrollment. Although most differences are statistically significant, the differences are in most cases small. Notably, the health insurance rate was 6 percentage points higher in Tennessee than in other southern states before the reform; comparing pre-reform means across Tennessee and other southern states, the most stark difference is the racial composition of Tennessee: much less Hispanic than other southern states. The percentage of high school graduates and people with some college is larger in Tennessee than other southern states. Having different levels of these demographic characteristics should not be an issue since the identification strategy needs similar trends rather than levels to be well-identified. Nevertheless, I take these observables differences into account by controlling for race and levels of education for each individual in my regression specifications. I also note that there is little difference in the results when comparing using a model with control covariates and model without control covariates, this comparison can be found in Table A3.

5 | EMPIRICAL STRATEGY

The research design used in this paper compares changes in outcomes between Tennessee and other southern states before and after the reform. I use the definition of southern states given by the U.S. Census. This means the comparison group is formed by: Alabama, Arkansas, Delaware, the District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, Texas, Virginia, South Carolina, and West Virginia. I study the period of 2000–2009, which allows me to include enough periods before and after the reform to credibly identify its effects. I use 2009 as the end year since there can be some early effects of the ACA for the year 2010.[19]

The empirical approach makes the comparisons stated above using a difference-in-differences (DD) model. Specifically, I estimate the following equation:

$$Y_{\text{ist}} = \beta_0 + \gamma_{\text{dd}}(\text{PostJuly2005} \times TN)_{\text{st}} + \boldsymbol{\beta}' X_{\text{ist}} + \delta_{\text{y}} + \nu_{\text{m}} + \alpha_{\text{s}} + \epsilon_{\text{ist}} \ (\text{BRFSS}) \tag{1}$$

$$Y_{\text{ist}} = \beta_0 + \gamma_{\text{dd}}(\text{Post2Q2005} \times TN)_{\text{st}} + \boldsymbol{\beta}' X_{\text{ist}} + \delta_{\text{y}} + \alpha_{\text{s}} + \epsilon_{\text{ist}} \ (\text{NHIS}) \tag{2}$$

Each outcome Y is measured for individual i in state s, at time t. In the models using BRFSS, time is a month-year variable, in the NHIS time is a year variable. Post July 2005 × *TN* is a dummy variable that takes the value of 1 for individuals living in Tennessee who reported outcomes after July 2005, and 0 for everyone else. In the NHIS, there is no information by month, but for 2005 there is information for quarter, so the Post2Q2005 variable takes the value of 1 after the second quarter in 2005. The coefficient on the interaction, γ_{dd}, represents the difference-in-differences estimator and parameter of interest. I include state fixed effects (α_{s}), year fixed-effects (δ_{y}),and month fixed effects (ν_{m}, Only in BRFSS) to account for any seasonality in outcome responses (i.e., the possibility of responding more positively during the summer

[19]However, following Garthwaite et al. (2014), I have also estimated my analysis using 2000–2007 to avoid potentially confounding effects from the Great Recession on health outcomes (Cotti et al., 2015; Ruhm, 2000; 2005; Ruhm & Black, 2002; Tekin et al., 2013). The results are robust to this alternative sample period.

TABLE 1 Summary statistics for main outcomes 2000–2005 BRFSS and NHIS

	Pre-reform Tennessee	Pre-reform Southern States	Diff
Health insurance			
Has health insurance (BRFSS)	0.88	0.82	0.06***
Has Medicaid (NHIS)	0.09	0.04	0.05***
Has private (NHIS)	0.71	0.65	0.05***
Has Medicare (NHIS)	0.04	0.03	0.01**
Has lost Medicaid (NHIS)	0.01	0.02	<−0.001*
Preventative care (BRFSS)			
Had a mammogram in the past 12 months for women over 50	0.68	0.64	0.03***
Had a breast exam in the past 12 months for women over 21	0.74	0.69	0.05***
Had flu shot in the past 12 months	0.29	0.26	0.02***
Had a pap exam in the past 12 months for women over 21	0.74	0.69	0.05***
Health status (BRFSS)			
Number of days with bad physical health	3.71	3.73	−0.02
Number of days with bad mental health	3.70	4.01	−0.31***
Number of days being incapacitated	4.72	4.64	0.08
Health care utilization (NHIS)			
Pr(Going to the ED in the past 12 months)	0.22	0.20	0.02*
Number of times in ED, past 12 months	0.35	0.33	0.02
Childless status (BRFSS)			
Currently pregnant	0.04	0.04	<0.00
No children in the household under 18.	0.56	0.55	0.01***
Number of children	0.82	0.84	−0.03***
Demographics (BRFSS)			
Share Black	0.13	0.17	−0.04***
Share Hispanic	0.02	0.06	−0.04***
Share White	0.83	0.72	0.11***
Female	0.63	0.61	0.02***
Age	43.14	43.12	0.02
Educational (BRFSS)			
Less than high school	0.10	0.11	−0.01*
High school graduate	0.35	0.32	0.03***
Some college or more	0.55	0.58	−0.03***
Marriage (BRFSS)			
Married	0.60	0.58	0.02***

TABLE 1 (Continued)

	Pre-reform Tennessee	Pre-reform Southern States	Diff
Divorced	0.18	0.16	0.02***
Never married	0.14	0.16	−0.02***
Unmarried couple	0.01	0.02	−0.01***
N	13,518	334,003	

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; NHIS, National Health Interview Survey.
* p ≤ .10; ** p ≤ .05; *** p ≤ .01.

months).[20] X_{ist} is a vector of individual-level controls such as education, race, age, gender, and marital status.

Since the reform specifically target the categorical eligibility of childless adults, it is speculated that childless adults were more "sharply" treated than adults with children. Other papers dealing studying the TennCare disenrollment use this fact to form a triple-difference strategy (Garthwaite et al., 2014) while other have focused solely on childless adults (Tarazi et al., 2017). I estimate the model for the full sample (both groups) but also break the sample into adults with children (who were not as sharply targeted by the reform) and adults without children (who were more sharply targeted by the reform). My identifying assumption is that outcomes in Tennessee would have evolved in a similar manner as other southern states in the absence of the disenrollment, conditional on observable characteristics. In order to provide evidence supporting the parallel trend assumption, I will visually present a model of leads and lags (or event-studies) where the estimating equation is:

$$Y_{ist} = \beta_0 + \boldsymbol{\beta}' X_{ist} + \sum_{y=2000}^{2009} \left(\delta_y \times TN\right)_{st} + \delta_y + \alpha_s + \epsilon_{ist} \tag{3}$$

Using specification (3), I plot the coefficients of the interactions between each year dummy and the Tennessee dummy. This should provide us with a graphical representation that before the disenrollment the difference between Tennessee and other southern states is constant and not statistically different and that after the reform this relationship diverges. For all the event study specifications, I use the childless adult sample, the sample with the largest treatment or the group targeted by the reform. The reference year is the year before the first one that appears in the graph (e.g., if the graph starts with 2001, the reference year is 2000). The baseline specification uses years 2000–2009. For the event studies, I use the same sample period, however for some of the BRFSS outcomes this would imply different number of pre-period coefficients. For example, using the health coverage variable, the omitted category is 2000, and the coefficients in the pre-period would represent 2001, 2002, 2003 and 2004. If I were to apply the same for mammograms, I would have 2000 as the omitted year and have 2002 and 2004 as the

[20]In the NHIS specification I do not have information of month of interview and so I do not include month fixed effects. The NHIS does have information on quarter of the year. This information is not available for years 2000 and 2004. This means that if the model includes quarter fixed effects the year 2000 and 2004 will drop, this is why I do not include quarter fixed effects in the models using NHIS. The results are very similar to the inclusion or exclusion of quarter fixed effects.

coefficients in the pre-period, only two coefficients. To standardize this, I extend the period of analysis for the event studies such that each figure has 4 points of pre-periods in all outcomes, which means some event studies will start before 2000. The results of the event studies without the years before 2000 would provide similar intuition as these, one can notice this by comparing the last two coefficients before 2005 to each other.

To estimate appropriate SEs, 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, which is usually done by clustering SEs at the state level. However, as MacKinnon and Webb (2017) point out, clustering relies on the number of clusters being large. In this study, the number of clusters is 17, and therefore the main 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 and Webb (2017) show that this could lead to an over-rejection of the null hypothesis. I account for this issue by using a modified version of block bootstrap that is composed of a two-stage sampling across states and within states. In the Appendix Figure A1 and Table B1, I use Monte Carlo simulations to test the finite sample properties of this method and to perform comparisons across other SE adjustments (including wild bootstrap). I conclude that the modified version of block bootstrap has rejection rates closer to the appropriate value (5%, using a *p* value of .05).

6 | RESULTS

6.1 | Disenrollment effect on coverage

As shown in Figure 1, there was a significant number of individuals who were disenrolled from TennCare; however, individuals could have found other types of coverage post-disenrollment. In Table 2, I present the results using data from the BRFSS and the NHIS to provide evidence of the disenrollment. The first panel estimates the main equation for the full sample, the second panel uses the sample of *adults with children,* and the last panel uses the sample of *adults without children or childless adults*. The columns represent different outcomes: the first four columns are outcomes from the NHIS while the last column is an outcome from the BRFSS.

I first present the most direct evidence of the effect of the reform: people specifically reporting "Losing Medicaid" as a reason for not having insurance. The TennCare reform increased the likelihood of reporting having lost Medicaid by 1.3 percentage points (93% effect). For childless adults the reform increased the likelihood by 1.6 percentage points, which represents more than a 100% increase over the pre-reform mean.[21]

Next, I present evidence of a decreases in reporting having Medicaid. For the overall population I find that the reform decreases reporting Medicaid by 2.9 percentage points, a 32% effect. The effect for childless adults is a decrease of 2.8 percentage points, a 31% effect. Finally, I do not find strong evidence of more people reporting having private or Medicare, which indicates

[21]The question asks reason why someone does not have health insurance coverage. Therefore, only people who are uninsured answer. I coded 0 for people whose reason was something else that is not losing Medicaid, and for people who *did* have health insurance, which is why the sample size is the same as the other outcomes.

TABLE 2 Effects of the disenrollment on coverage 2000–2009

	Has lost Medicaid (NHIS)	Has Medicaid (NHIS)	Has private insurance (NHIS)	Has Medicare (NHIS)	Is uninsured (NHIS)	Has health insurance (BRFSS)
Mean of dependent	0.014	0.09	0.71	0.04	0.14	0.89
DD model, all adults						
TN × Post	0.013***	−0.029***	<0.001	−0.004	0.037***	−0.028***
	(0.003)	(0.006)	(0.010)	(0.004)	(0.009)	(0.006)
	[0.000]	[0.000]	[0.993]	[0.309]	[0.000]	[0.000]
R^2	.03	.07	.20	.05	.16	.16
N	174,897	174,897	174,897	174,897	174,897	744,767
% Change	92.86	−32.22	<0.14	−10	26.43	−3.22
Mean of dependent	0.02	0.10	0.73	0.01	0.12	0.89
DD model, adults with children						
TN × Post	0.008	−0.016*	0.015	−0.002	0.019	−0.020**
	(0.007)	(0.009)	(0.015)	(0.004)	(0.013)	(0.009)
	[0.282]	[0.075]	[0.310]	[0.563]	[0.152]	[0.020]
R^2	.06	.11	.23	.02	.18	.19
N	69,455	69,455	69,455	69,455	69,455	318,683
% Change	40.00	−16.00	2.05	−20.00	15.83	−2.24
Mean of dependent	0.01	0.09	0.70	0.05	0.15	0.89
DD model, childless adults						

(Continues)

TABLE 2 (Continued)

	Has lost Medicaid (NHIS)	Has Medicaid (NHIS)	Has private insurance (NHIS)	Has Medicare (NHIS)	Is uninsured (NHIS)	Has health insurance (BRFSS)
TN × Post	0.016***	−0.028***	−0.012	−0.007	0.040***	−0.038***
	(0.004)	(0.007)	(0.012)	(0.006)	(0.010)	(0.008)
	[0.000]	[0.000]	[0.305]	[0.285]	[0.000]	[0.000]
R^2	.01	.07	.18	.05	.15	.14
N	102,518	102,518	102,518	102,518	102,518	425,378
% Change	160	−31.11	−1.71	−14.00	26.67	−4.27

Note: The reported coefficients are the interaction between Tennessee and the "post" variable. For BRFSS outcomes the post variable takes the value of 1 after July 2005. For the NHIS outcomes the post variable takes the value of 1 after the second quarter of 2005. The specifications include state fixed-effect, year fixed-effects, with month fixed-effect when using BRFSS. They also include controls for individual demographic characteristics as: race, gender, education, age and marital status. The models are weighted using survey weights. Block-bootstrap SEs can be found in parentheses, and block-bootstrap *p* values can be found in brackets. The mean of the dependent is the mean for the outcome variable in Tennessee before 2005.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; NHIS, National Health Interview Survey.

$^{*}p \leq .10$; $^{**}p \leq .05$; $^{***}p \leq .01$.

that either the take-up in this category was small or none.[22] All of these outcomes are novel evidence relative to the previous papers as they report loses in overall coverage rather than specific insurance programs or specifically reporting losing Medicaid. In fact, from our estimates I can estimate that at least 57% of the drop in Medicaid reporting from childless adults can be explained by people specifically reporting losing Medicaid, this confirms that most of the effect captured by the coverage variables are capturing the disenrollment. I do not expect the "Lost Medicaid" to explain the 100% drop in the Medicaid outcome since the question about lost Medicaid was asked to people that reported having no insurance. Therefore, if someone lost Medicaid and then obtained another form of insurance, they would not have answered this question.

The findings from the previous literature focus on overall drop in coverage (being uninsured or insured). The reasons to focus on these outcomes as the main estimates of the reform is due to misreporting on having Medicaid. This is documented by research from Lynch and Resnick (2003). The misreporting is due to the management in Medicaid, since in Tennessee Medicaid was run by managed care, there are some people who would have reported they have private insurance as opposed to Medicaid. To illustrate this, Figure A1 presents an example of a TennCare insurance card. Therefore, the literature suggest a cleaner measure of the reform would be a variable that measures having health insurance or not. I find that the reform led to a 3.7 (NHIS) or 2.8 (BRFSS) percentage point decline in overall coverage (a 4.3 (NHIS) and 3.2% (BRFSS) decline) for all adults.[23] For childless adults this represented a 4.0 (NHIS) or 3.8 (BRFSS) percentage point drop in coverage (a 4.8 (NHIS) and 4.3% (BRFSS) decline in coverage). Other papers have confirmed that the disenrollment did indeed result in less overall coverage. DeLeire (2019) finds a 5 percentage point decrease in the insurance rate using the SIPP. Tarazi et al. (2017) find a 5.4 percentage point drop in overall health coverage using the BRFSS. It is important to note that the difference in my estimate (3.8 pp) and Tarazi et al. (2017) (5.4 pp) is driven by model and sample selection: they use a sample of childless adults with income under 200% of the FPL, the years are 2003–2008 and they have five comparisons states, and my sample of adults are ages 21–64.

In 2004, adults between ages of 21 and 64 represented around 60% of the population in Tennessee. Using the estimates from Table 2, these effects translate into ~100,233–132,451 residents—about 52%–69% of all people losing eligibility—who did not get other types of coverage.

In Figure 2a, I plot the coefficients of the event study model using the outcomes of having health coverage (BRFSS). The graphical evidence supports the parallel trends assumption, this figure shows that before the disenrollment the health coverage rate was evolving similarly in southern states relative to Tennessee, and after the disenrollment these rates diverge.

6.2 | Disenrollment effect on preventative care and health behaviors

Table 3 show the effects of the reform on preventative care. Each metric has a different sample and this is indicated in the table under the title of each outcome. For example, I use the sample of Women over the age of 50 for the mammograms outcome. This age reference follows the age recommendations for each preventive care measure, as provided by the USPSTF. For

[22]Although TennCare was considered an extension of Medicaid, it is possible that some people thought they had private health insurance even though they had TennCare.

[23]The 4.3% for NHIS, is calculated as −0.037/(1−0.14) since the outcome is uninsured rather than insured as it is in the BRFSS.

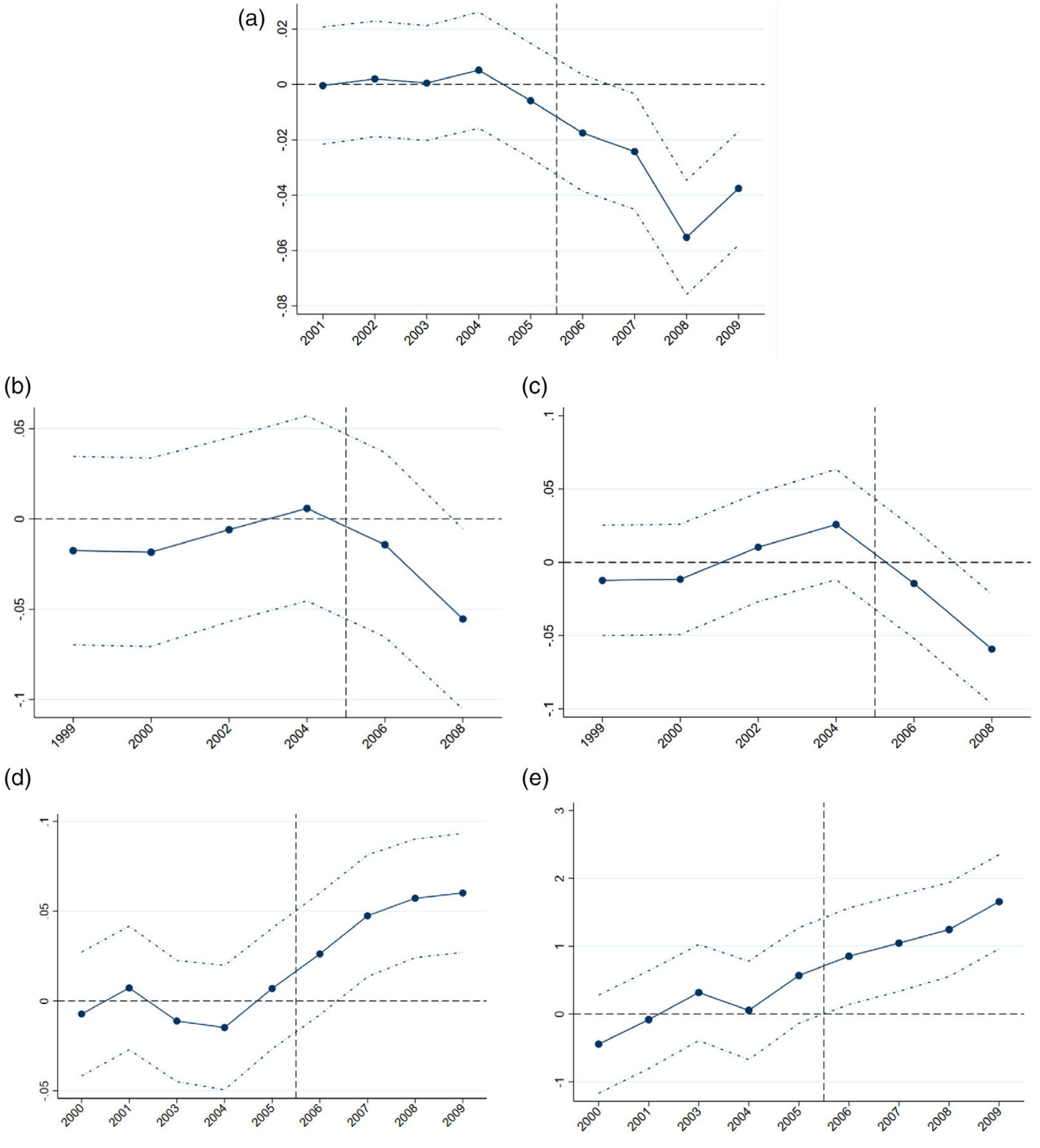

FIGURE 2 Event studies for childless adults. These figures plot the coefficients of TN × Year_i from the event study specification. The reform occurred in august of 2005, which means 2005 was partially treated. Therefore, one could observe effects starting in 2005. (a) Effects of TennCare reform on health coverage (BRFSS) for childless adults. (b) Effects of TennCare reform on "Had a Mammograms" (BRFSS) for childless adults. (c) Effects of TennCare reform on "Had a Breast Exam" (BRFSS) for childless adults. (d) Effects of TennCare reform on Pr(number of days being incapacitated ≥5) (BRFSS) for childless adults. (e) Effects of TennCare reform on number of days being incapacitated (BRFSS) for childless adults. (f) Effects of TennCare reform on Pr(having an ED visit in past 12 months) (NHIS) for childless adults. (g) Effects of TennCare reform on number of ED visits (NHIS) for childless adults. (h) Effects of TennCare reform on Pr(not going to a place if sick) (NHIS) for childless adults. (i) Effects of TennCare reform on Pr(not going to a place for preventative care) (NHIS) for childless adults [Color figure can be viewed at wileyonlinelibrary.com]

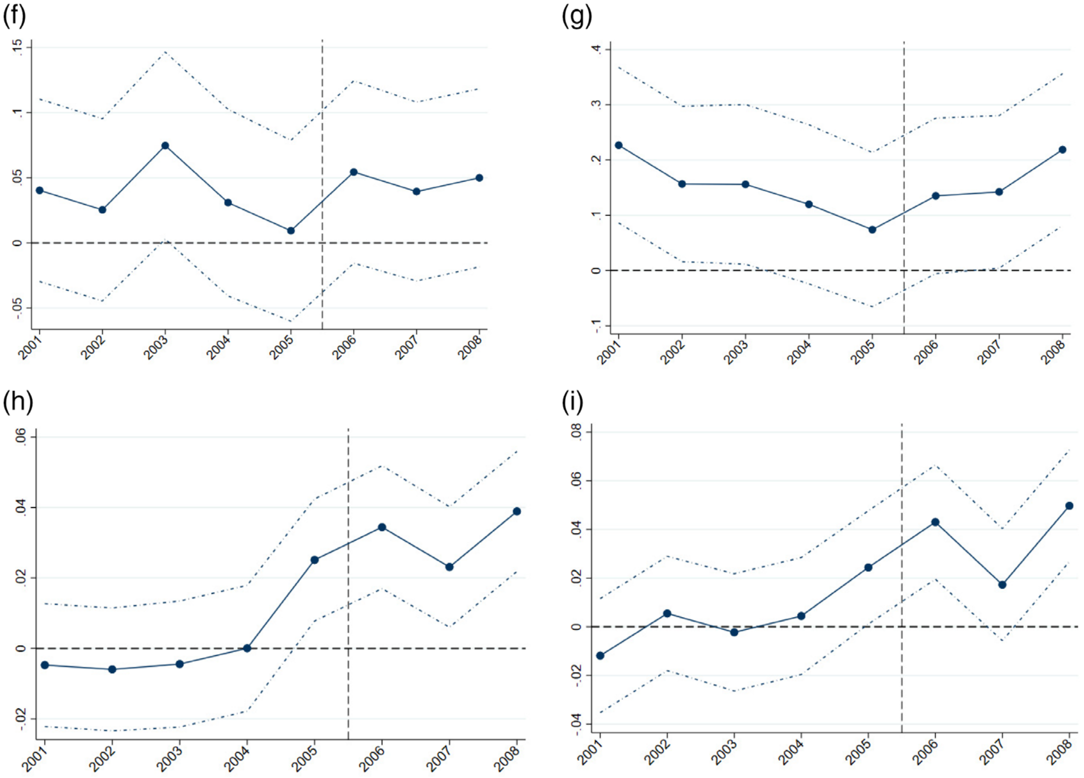

FIGURE 2 (Continued)

mammograms, the full sample indicates that there was a 3.1 percentage point decline in the likelihood of reporting having a mammogram. Looking at the panels below, I find that this effect is mainly driven by the sample of childless adults with a 3.4 percentage point decrease (4.9%). For breast exams, I find a decrease of 2.8 percentage points (3.7%) for the full sample and the effect mostly driven by childless adults with a decrease of 3.3 percentage points (4.4%). The event study estimates for these outcomes can be found in Figure 2b,c. These figures provide supporting evidence that before the reform, these outcomes were trending similarly and then diverge after the reform. In order to understand these intent-to-treat (ITT) estimates, I divide the effect of mammograms and breast exam by their appropriate first stage for the same sample (this is not shown in tables). For childless adults, the mammogram elasticity is −0.72 (0.033/0.0472) while for breast exams the implied elasticity is −0.83 (0.033/0.04). This means that out of women losing health insurance I estimate that around 72%–82% of decrease their preventative care of mammograms and breast exams.

I present results for preventable care measures including flu shots, pap exam, and PSA for completeness but I do not draw conclusions from them as they do not appropriately satisfy empirical evidence of parallel trends from the event-studies. Similarly, in Table A1, I report the effects of the disenrollment on health-behaviors. Although I do find effects of increases in physical exercise, these effects are not robust to specifications that explore evidence for parallel-trends, and hence I do not make conclusions based on these results.

TABLE 3 Effects of the disenrollment on preventative care 2000–2009

	Had a mammogram in the past 12 months? (BRFSS)	**Had a breast exam in the past 12 months? (BRFSS)**	**Had a flu shot in the past 12 months?[a] (BRFSS)**	**Had a pap exam in the past 12 months?[a] (BRFSS)**	**Had a PSA in the last 12 months?[a] (BRFSS)**
Sample	Women Age ≥ 50	Women Age ≥ 21	Full sample 64 ≥ Age ≥ 21	Women Age ≥ 21	Men Age ≥ 40
Mean of dependent	0.68	0.74	0.29	0.74	0.40
DD model, all adults					
TN × Post	−0.031*	−0.028***	0.003	−0.026**	−0.039**
	(0.016)	(0.011)	(0.008)	(0.010)	(0.019)
	[0.058]	[0.007]	[0.670]	[0.012]	[0.034]
R^2	.03	.04	.05	.05	.13
N	107,400	271,790	707,673	271,623	87,370
% Change	−4.56	−3.83	1.14	−3.57	−9.74
Mean of dependent	0.61	0.74	0.24	0.77	0.33
DD model, adults with children					
TN × Post	−0.011	−0.023	−0.000	−0.031**	−0.033
	(0.040)	(0.014)	(0.011)	(0.015)	(0.031)
	[0.773]	[0.118]	[0.992]	[0.039]	[0.295]
R^2	.04	.05	.03	.05	.09
N	14,910	124,349	301,649	124,375	26,797
% Change	−1.88	−3.08	−0.04	−4.01	−9.78
Mean of dependent	0.69	0.74	0.33	0.70	0.44

TABLE 3 (Continued)

	Had a mammogram in the past 12 months? (BRFSS)	Had a breast exam in the past 12 months? (BRFSS)	Had a flu shot in the past 12 months?[a] (BRFSS)	Had a pap exam in the past 12 months?[a] (BRFSS)	Had a PSA in the last 12 months?[a] (BRFSS)
DD model, childless adults					
TN × Post	−0.034**	−0.033**	0.008	−0.020	−0.042*
	(0.016)	(0.014)	(0.010)	(0.014)	(0.022)
	[0.035]	[0.020]	[0.433]	[0.157]	[0.054]
R^2	.21	.04	.06	.05	.13
N	92,421	147,202	405,350	147,011	60,506
% Change	−4.90	−4.47	2.49	−2.90	−9.70

Note: The reported coefficients are the interaction between Tennessee and the "post" variable. For BRFSS outcomes the post variable takes the value of 1 after July 2005. The specifications include state fixed-effect, year fixed-effects, with month fixed-effect when using BRFSS. They also include controls for individual demographic characteristics as: race, gender, education, age and marital status. The models are weighted using survey weights. Block-bootstrap SEs can be found in parentheses, and block-bootstrap *p* values can be found in brackets. The mean of the dependent is the mean for the outcome variable in Tennessee before 2005.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; NHIS, National Health Interview Survey.

[a]These outcomes do not provide evidence of parallel trends.

$^{*}p \leq .10$; $^{**}p \leq .05$; $^{***}p \leq .01$.

6.3 | Disenrollment effect self-reported health

Tables 4 and A2 study the effects of the reform on self-reported health outcomes. BRFSS records two types of self-reported health outcomes. The first type records the self-rating of the responder's health, and the second type asks for the number of days the responder has felt some type physical or mental illness in the past 30 days. I focus on the second type of responses as these are less subjective and more standard across people's understanding than the health ratings. Tables 4 and A2 both provide evidence that the disenrollment weakened people's health. I focus on the results of Table 4 as these outcomes have more support of the parallel trends assumption being held (Figure 2d,e). In Table 4, I report two measures of the same question, one represents the extensive margin (probability of reporting more than 5 days with an incapacitation) while the second one represents the intensive margin (number of days with an incapacitation). I use the metrics of at least 5 days to avoid issues of bunching around day 1, the results are very similar if I use the probability of at least 1 day. For the full sample, I find that the disenrollment led to an increase of 2.7 percentage points (12%) in the likelihood of people reporting at least 5 days with an incapacitation. Most of this effect is concentrated among the childless adults, for which I find a 4.8 percentage point increase in the likelihood of reporting at least 5 days with a health-incapacitation (a 20% effect). The number of days with a health-incapacitation estimate is 0.74 days increase for the full sample, and 1.2 more days for the sample of childless adults, a 15% and 22% increase respectively. These effects are larger than expected, and larger than the effects of gains in coverage from other recent reform ACA Medicaid expansions (Simon et al., 2017), Oregon health insurance experiment (Finkelstein et al., 2012), and Massachusetts health reform (Courtemanche & Zapata, 2014; Kolstad & Kowalksi, 2012). An unlikely but possible confounder could be that the during the pre-recession period (after 2007 and before 2009) the health of people in Tennessee was deteriorating faster than in than other southern states. However, even when I estimate the models using a different sample of years (2000–2007) the effects are smaller but still larger than the effects of the other reforms (Table A3). The other possibility is that this is evidence of asymmetries between losing and gaining insurance. I expand on this point in the discussion section.

6.4 | Disenrollment effect on ED use

In Table 5, I present the results of the disenrollment on ED usage. I report two outcomes: the probability of going to the ED in the past 12 months and the number of times in the ED in the past 12 months. In the first outcome (column [1]) the models indicate a decrease in the likelihood of reporting a visit to the ED, however for the full and both sub-samples the coefficients are not statistically significant at conventional levels. A similar pattern occurs when using the intensive margin (number of times in the ED). These coefficients indicate a decline in the number of times, but none of them are statistically significant. The event-study graphs in Figure 2f, g, do not provide much evidence that this seems to be a stark decline, rather they indicate somewhat flat trends or very noisy trends to make a precise conclusion. I supplement this finding with two other empirical facts. First, I obtained a count on the number of ED visits in Tennessee from the department of health, by year, payer, and age. I obtained the respective population counts and divided the number of ED visits by their respective population to calculate an ED rate. With this information I created Figure 3a, which essentially shows the rate of

TABLE 4 Effects of the disenrollment on self-reported health outcomes 2000–2009

	Pr(number of days being incapacitated ≥5) (BRFSS)	Number of days incapacitated in the past 30 days (BRFSS)
Mean of dependent	0.22	4.72
DD model, all adults		
TN × Post	0.027***	0.741***
	(0.009)	(0.187)
	[0.003]	[0.000]
R^2	.06	.07
N	371,137	371,137
% Change	12.34	15.71
Mean of dependent	0.19	3.86
DD model, adults with children		
TN × Post	0.004	0.259
	(0.013)	(0.257)
	[0.750]	[0.313]
R^2	.05	.06
N	159,396	159,396
% Change	2.24	6.72
Mean of dependent	0.24	5.42
DD model, childless adults		
TN × Post	0.048***	1.193***
	(0.012)	(0.263)
	[0.000]	[0.000]
R^2	.07	.07
N	211,443	211,443
% Change	19.89	22.02

Note: The reported coefficients are the interaction between Tennessee and the "post" variable. For BRFSS outcomes the post variable takes the value of 1 after July 2005. The specifications include state fixed-effect, year fixed-effects, with month fixed-effect when using BRFSS. They also include controls for individual demographic characteristics as: race, gender, education, age and marital status. The models are weighted using survey weights. Block-bootstrap SEs can be found in parentheses, and block-bootstrap p values can be found in brackets. The mean of the dependent is the mean for the outcome variable in Tennessee before 2005.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; NHIS, National Health Interview Survey.

$^{*}p \leq .10$; $^{**}p \leq .05$; $^{***}p \leq .01$.

ED visits by age-groups.[24] Overall, there are no strong signs of a drastic change after the reform, only a stagnation of the ED visits which is happening a right before the reform. Although I do not have a proper control group, I use the rate of ED visits for individuals age 65 and on. This is

[24]I also de-trend by quarter the rate when plotting the figure.

TABLE 5 Effects of the disenrollment on emergency department visits & reporting not going to a place to get health care 2000–2009

	Pr(going to the ED in the past 12 months) (NHIS)	Number of times in ED, past 12 months (NHIS)	Pr(does not go to place when sick) (NHIS)	Pr(does not go to one place for preventative care) (NHIS)
Mean of dependent	0.221	0.350	0.005	0.009
DD model, all adults				
TN × Post	−0.025	−0.043	0.027***	0.024***
	(0.015)	(0.031)	(0.005)	(0.005)
	[0.104]	[0.176]	[0.000]	[0.000]
R^2	.03	.03	.01	.01
N	78,209	78,209	78,380	78,366
% Change	−11.31	−12.29	540.00	266.67
Mean of dependent	0.257	0.423	0.004	0.011
DD model, adults with children				
TN × Post	−0.032	−0.056	0.033***	0.011
	(0.025)	(0.053)	(0.009)	(0.007)
	[0.199]	[0.297]	[0.000]	[0.138]
R^2	.04	.04	.01	.01
N	29,020	29,020	29,069	29,062
% Change	−12.45	−13.24	825.00	100.00
Mean of dependent	0.200	0.310	0.005	0.009
DD model, childless adults				
TN × Post	−0.016	−0.028	0.025***	0.032***
	(0.019)	(0.041)	(0.006)	(0.007)
	[0.390]	[0.501]	[0.000]	[0.000]
R^2	.02	.03	.01	.01
N	48,557	48,557	48,678	48,671
% Change	−8.00	−9.03	500.00	355.56

Note: The reported coefficients are the interaction between Tennessee and the "post" variable. For the NHIS outcomes, the post variable takes the value of 1 after the second quarter of 2005. The outcomes presented in this table come from the Sample Adult File, which is why they have different sample sizes from the coverage variables of the NHIS. The specifications include state fixed-effect, year fixed-effects, with month fixed-effect when using BRFSS. They also include controls for individual demographic characteristics as: race, gender, education, age and marital status. The models are weighted using survey weights. Block-bootstrap SEs can be found in parentheses, and block-bootstrap p values can be found in brackets. The mean of the dependent is the mean for the outcome variable in Tennessee before 2005.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; NHIS, National Health Interview Survey.

$^{*}p \leq .10$; $^{**}p \leq .05$; $^{***}p \leq .01$.

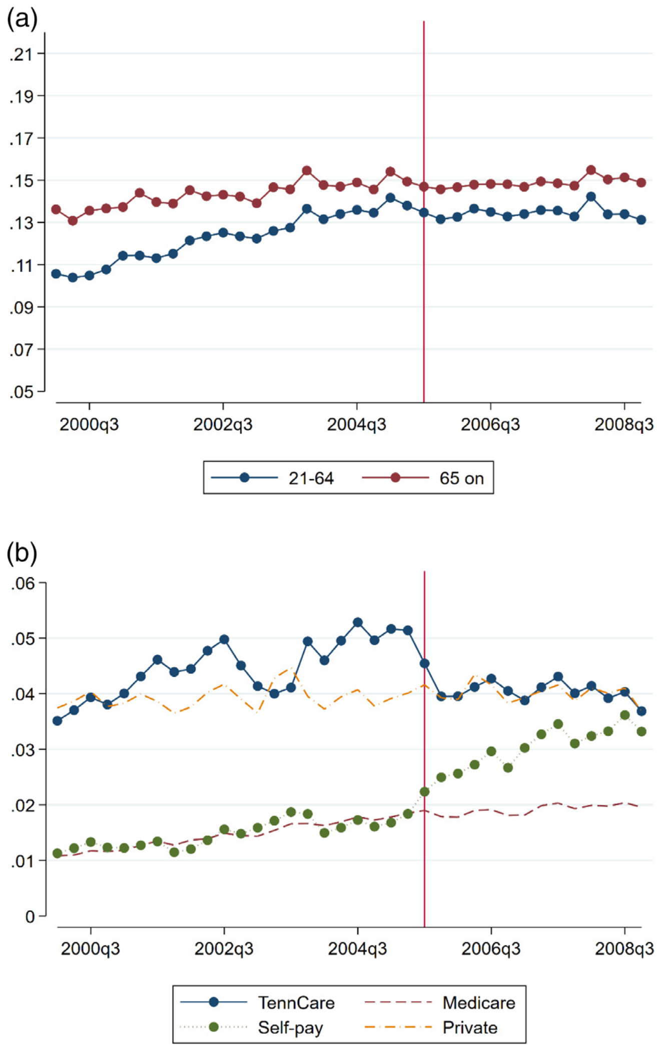

FIGURE 3 (a) Trends in ED visits by age group in Tennessee. (b) Evolution of payer-share of ED visits in Tennessee [Color figure can be viewed at wileyonlinelibrary.com]

a group that even though they could have lost Medicaid eligibility they would still have access to insurance through Medicare. They are an imperfect control group given the observable and unobservable difference that age causes on ED visits. Overall they follow somewhat similar trends before the disenrollment and neither line shows a stark divergence after the disenrollment, in other words this comparison confirms that there is no stark change in ED visits for 21–64 years old. Hence models from the NHIS and trends analysis from the ED discharge data confirm the finding that there are not drastic changes in ED visits after the reform.

Even though, the overall rate may not have changed, the payer composition could have. In Figure 3b, I plot out of everyone who is 21–64, the rate by payer (e.g., number of TennCare-paid visits over population 21–64). This figure is telling of two things: (a) that there is a sharp and quick increase on the rate of self-pay visits at the same time (b) there is a decrease in TennCare payments, while Medicare and Private payment rates remain relatively flat. This graph is consistent with the idea that people are losing TennCare and not obtaining other sources of coverage. I do not have a control group for this graph but the timing of the shifts in payer mix is consistent with the timing of the reform. These two graphs combined with the estimates from NHIS provide the following insight: It may be the case that the rate at which patients are going to the ED is not changing, but the payer-composition seems to be drastically changing and including more people coming in with self-pay. This is not a trivial point, as the main point of insurance is to mitigate financial distress, and having a visit to the ED while uninsured could result in an expensive medical bill. These financial implications have been explored more deeply in complementary paper by Argys et al. (2020). This paper studies the effect of the TennCare reform on financial outcomes. This paper finds that although not everyone had drastic changes in debt accumulation, there were people that started accumulating debt right after the reform. In their event-studies for debt, they show outcomes deteriorating right after the second quarter of 2005. This is consistent with the idea that some people are showing up to the ED, with no insurance and end up with bills that are large enough to delay payments and accumulate debt. If this is true, one would expect unpaid bill for hospitals to increase, this is what Garthwaite et al. (2018) found in their case study of Tennessee. They found that after the TennCare reform, unpaid hospital bills increased relative to other southern states (Figure 2 of their paper).

The evidence on ED shows that there is no detectable shift in ED visits, only in composition of payer. This does not rule out the possibility of individuals still changing where they get care when sick. While some individuals cannot change their ED use occurrence, some individuals -at least in the short term- may avoid any type of place to get care given the cost. The NHIS asks the question of where does one to go to get healthcare once sick. I use the answer "does not go anywhere" as an outcome. I find that for the full-sample an increase in reporting "not going to any place" of 2.7 percentage points and for the childless adult's sample this is 2.5 percentage points. I use a similar outcome for preventative care to see if the pattern repeats, and I also find an increase in people reporting "not going to a place for preventative care" of 2.4 percentage points in the full-sample and 3.2 percentage points for the sample of childless adults. These results can indicate that some people are shifting the location of primary care consumption, presumably not in the ED margin, but away from doctor's visit. These models are supported by evidence for parallel trends using the event studies presented in Figure 2h,i.

7 | ROBUSTNESS AND HETEROGENEITY

For all the outcomes that have support of parallel trends, I modified the main model in several margins to address different concerns and show the robustness of the results. I use the sample of childless adults throughout these models. The results from this exercise can be found in Table A3. In Row (1), I present the results from the baseline model for childless adults. In the second panel, I use only the years 2000–2007. This is done to avoid any potential confounder of the effects of the recession itself. Across the main outcomes, only the outcomes related to mammograms and breast exams become not statistically significant, the rest remain a sizeable and statistically significant effect. For mammograms, the size of the coefficient remains the same but SEs become large enough and do not allow me to reject the null at conventional levels. For breast exams, the coefficient becomes smaller while the SEs increase as well, given the same result.

In the third panel, I use no weight adjustment. I am using information from two different surveys and for each survey there are different weighting adjustments to be made. I run models without weights to see if the effects I am finding are simply driven by some sort of weighting specification. The results remain stable or not qualitatively different between using weights or no weights. Similarly, I run a specification without controls. The summary statistics showed that there were differences in demographic characteristics across treatment and control group, hence these were added as controls. The identification should take care of these level-differences as long as both groups have similar trends, even without controls. The specification without controls provides very similar results relative to the one with controls, providing further evidence of the robustness of the identification strategy. Finally, I aggregate all data at the state-quarter level and re-estimate the models, this aggregation provides similar results as well.

In Table A4, I explore how the heterogeneity of effects across demographic characteristics.[25] For the mammograms and breast exams, I find larger effects for whites, and lower educated individuals (less than high-school degree). For the outcome of number of days incapacitated, I find larger effects for whites and people over 40. The effects have similar size for males and females, and similar for people with just a high-school degree than people with college or more.

The ED outcomes are mostly not-significant however the breakdown by race provides an interesting result. I find an increase in the number of times people visit the EDs for Blacks, while for whites and Hispanics I find a decrease (though not statistically significant). This large discrepancy in the size of the coefficient is puzzling and can be a byproduct of what types of services are available geographically for places with higher Black population. Most of the Black population in Tennessee live in the southern western counties, and a number of them are considered primary care shortage areas. Therefore, this increase in ED visits for Blacks could represent (or be confounded with) a lack of access to primary care.

Finally, for the outcomes of not going to a place when sick or needed preventative care, I find the larger effects for Blacks, males, people with a high-school degree or less, and people of age 21–40.[26]

8 | DISCUSSION

In this paper, I provide novel evidence on the effects of losing public health insurance eligibility on preventative care, self-reported health, and ED visits. I find that the disenrollment did lead to overall decreases in health insurance as found by previous literature (DeLeire, 2019; Tarazi et al., 2017). Specifically, I am the first to show directly that people report losing Medicaid right after the reform, and this effect is mostly concentrated among childless adults.

In terms on preventative care, I find decreases between 3% and 4% for mammograms and breast exams. The other preventative care metrics did not have supporting evidence for parallel trends. These effects are larger for whites and individuals with less than a high school degree. I find that the disenrollment led to significant decreases (20%) in health, namely increases in the number of days with a health-related incapacitation. These effects are larger for whites and people between ages 40 and 64. In terms of ED visits, I do not find evidence of changes in the extensive or intensive margin. This result may mask heterogeneity across race since I do find that Blacks increase their ED usage, as the number of times increased by 50% while for the

[25]Blacks are defined as non-Hispanic Blacks and whites are non-Hispanic whites.

[26]An important consideration when understanding heterogeneities is that the sample-size is changing drastically across samples, so I tend to focus more of difference in coefficient size rather than statistical significance as these may be driven by higher or smaller sample size.

other groups the coefficient is negative and not statistically significant. Even though ED visits rates are not changing in the full sample, I show—descriptively—that more people are coming to ED uninsured, which has severe financial implications. Additionally, the likelihood of reporting not going to a place when sick or to get preventative care is also increasing and its largest for male, Blacks, 21–40, lower-educated individuals.

To understand the magnitude of these effects I compare the effects relative to their first stage as an elasticity. For the mammograms and breast exam, the first-stage estimate implies an elasticity of 0.7 and 0.8. For the health outcome, the estimate implies an elasticity of 0.89 for the number of days with bad health. I test for sensitivity of the size of the effects across different margins (sample of years, definition of the dependent variable, weighting) and the results remain robust. An important note is that these results seem large relative to what we know about *gains* in insurance and health outcomes. It could be the case that these estimates are indicating a level of asymmetry between gaining and losing health insurance. In Table A5, I compare the estimates for the childless adults' sample with the estimates from papers on the ACA Medicaid Expansions, which is the closest treatment relative to the other popular studied health care reforms (Miller & Wherry, 2017; 2019; Nikpay et al., 2017; Simon et al., 2017). I have also summarized the effects of two other high-profile studies: the Massachusetts health reform (Courtemanche & Zapata, 2014; Kolstad & Kowalski, 2012; Miller, 2012a; 2012b) and the Oregon Health Insurance Experiment (Finkelstein et al., 2012). It is important to note that these are just comparing the coefficients, and ideally one would have the same population, in the same period, and randomly assign (and take-away) Medicaid eligibility. Therefore, differences between these estimates could also be explained away by any of these margins (i.e., different states, demographics, etc.). Since these reforms have different size of changes in coverage, in the first rows I present the estimates on coverage for the largest sample available.

The ACA Medicaid expansions had a larger effect on coverage than the TennCare Reform. Comparing the effect on Medicaid, I found that the TennCare reform decreased Medicaid rates by 2.8 percentage points, while the ACA Medicaid expansion increased it by 15.6 percentage points. Comparing overall health insurance rates, I find a decrease of 4 percentage points while the ACA Medicaid expansion finds an increase of 8.2 percentage points. Since the ACA Medicaid expansion had a larger effect on coverage, it is expected that the effects of the ACA are larger than the ones from TennCare if the effects are symmetric. In order to compare the other outcomes with the coverage effects in mind, I use the "LATE" column to compare the effects from both reforms. This is an estimate obtained by either dividing the coefficient over the appropriate "first-stage" coverage effect, or if a paper provides it, I use their estimate of the "LATE."[27]

In terms of mammograms, the calculated "LATE" is 0.72 for the TennCare reform, while it is 0.74 for the ACA Medicaid expansions. Miller and Wherry (2017) implied LATE is 0.74 while Simon et al. (2017) calculate a LATE of 0.67. These are very similar estimates and symmetrical. For breast exams, I calculate a LATE of 0.83, while Simon et al. (2017) estimate a LATE of 0.082, in addition their finding is not statistically significant. For pap test, both reforms do not find statistically significant changes.

In terms of self-reported health outcomes, I calculate a LATE of 13.02 while estimates from Simon et al. (2017) are −9.054. The estimate for mental health for the TennCare reform is not statistically significant, while Simon et al. finds a decrease of 1.063. Note the only estimate I find evidence for parallel trends is number of days with a health limitation. I find an LATE of 22.09

[27] I document how I obtained every estimate in the table notes. The letters indicate which paper I used to obtained

while the ACA Medicaid expansion suggest a decrease of 12.93. For both reforms, this estimate is statistically significant. Comparing the estimates in this category, I am finding larger effects under the TennCare reform. A case from asymmetry could be explained here either by fast deterioration of health combined with loss-aversion's perceptions that result in larger self-reported health outcomes. In terms of health care utilization, I calculate a LATE effect of 0.31 for Pr(ED visits), using an estimate from Miller and Wherry (2017), they are finding LATE of 0.048, these are relatively asymmetric effects but both of them are not statistically significant. Finally, in terms of access to care, I use estimates from Tarazi et al. (2017). I calculate a LATE on having a personal doctor of −0.07, while using estimates from Simon et al, they present an LATE of 0.405. For "cannot afford due to cost," Tarazi et al. have an implied LATE of 0.72 while Simon et al have an implied LATE of −0.40. This means that access to care may present some asymmetric effects in terms of size. This is consistent with the perception of accumulation of health information. Coming off from health insurance, one is less informed on ways to obtain care while uninsured and that may result in only finding cost-prohibiting options. While being a person that has been in an uninsured spell for a while, may have learned of ways to access lower-cost care, making a gain in health insurance an overall improvement but to a lesser degree.

Overall, I find that there is sign-symmetry in many of these outcomes (i.e., more insurance, improved access to care, and less insurance, worsening metrics of access to care). However, I do not find support for symmetry with respect to size of the effects, the point-estimates are somewhat similar, but the implied elasticities or LATEs are larger in the losing insurance margin than the gaining insurance margin. There are many reasons why one could find larger elasticities when losing rather than gaining, and they are outcome dependent. For example, if the places where one usually received preventative care was through a primary care doctor, once one loses insurance it may be harder to find alternatives. In the case of gain, this effect could be mitigated because while uninsured -for a long period- the individual could have learned about other places where one could go to obtain these measures (with a low cost). In other words, there could be search frictions that one learns while uninsured but needs to be learned when newly uninsured.

Acknowledging the drawbacks from this comparison, a lesson from this exercise is that that when thinking about "rolling back" -in any form- the recent Medicaid expansions, one could expect the estimates from the ACA Medicaid expansions to be a lower-bound of the potential effects.

Further research should focus on other aspects of the effects of the disenrollment, such as the amount of time people remain uninsured or more detailed information on the effects on prescription drugs. Finally, for welfare analysis, another relevant set of outcomes to study would be the effects of the reform on the supply side of health (e.g., wages of health practitioners, their hours worked). This will help us to form a more complete picture of the broad effects of this disenrollment, which can eventually help inform policymakers when they make choices about changes to public health insurance eligibility and other alternatives policies.

ORCID

Daniel Sebastian Tello-Trillo https://orcid.org/0000-0003-4746-434X

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APPENDIX A

TABLE A1 Effects of the disenrollment on health behaviors 2000–2009

	Pr(participates in physical activity)[a] (BRFSS)	Pr(any drink in past 30 days)[a] (BRFSS)	Pr(currently a smoker)[a] (BRFSS)
Mean of dependent	0.69	0.34	0.28
DD model, all adults			
TN × Post	0.018**	0.013	−0.007
	(0.007)	(0.010)	(0.007)
	[0.015]	[0.193]	[0.335]
R^2	.08	.10	.09
N	745,423	694,856	743,678
% Change	2.58	3.90	−2.51
Mean of dependent	0.71	0.34	0.29
DD model, adults with children			
TN × Post	0.017*	0.008	−0.016
	(0.010)	(0.013)	(0.010)
	[0.093]	[0.557]	[0.111]
R^2	.08	.09	.10
N	318,887	295,955	318,225
% Change	2.48	2.35	−5.59
Mean of dependent	0.68	0.35	0.28
DD model, childless adults			
TN × Post	0.018*	0.019	0.000
	(0.011)	(0.012)	(0.010)
	[0.095]	[0.107]	[0.981]
R^2	.08	.11	.08
N	425,819	398,257	424,744
% Change	2.59	5.42	0.08

Note: The reported coefficients are the interaction between Tennessee and the "post" variable. For BRFSS outcomes the post variable takes the value of 1 after July 2005. The specifications include state fixed-effect, year fixed-effects, with month fixed-effect when using BRFSS. They also include controls for individual demographic characteristics as: race, gender, education, age and marital status. The models are weighted using survey weights. Block-bootstrap SEs can be found in parentheses, and block-bootstrap *p* values can be found in brackets. The mean of the dependent is the mean for the outcome variable in Tennessee before 2005.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; DD, difference-in-differences.

[a]These outcomes do not provide evidence of parallel trends.

$^{*}p \leq .10$; $^{**}p \leq .05$; $^{***}p \leq .01$.

TABLE A2 Effects of the disenrollment on self-reported health outcomes 2000–2009

	Pr(days with bad physical health ≥5) (BRFSS)	Pr(days with bad mental health ≥5) (BRFSS)	Number of days with bad physical health[a] (BRFSS)	Number of days with bad mental health[a] (BRFSS)
Mean of dependent	0.16	0.16	3.70	3.70
DD model, all adults				
TN × Post	0.020***	0.005	0.431***	0.037
	(0.006)	(0.008)	(0.118)	(0.143)
	[0.001]	[0.531]	[0.000]	[0.795]
R^2	.05	.04	.06	.04
N	696,489	696,329	696,489	696,329
% Change	12.73	3.01	11.64	1.01
	0.13	0.17	2.80	3.74
DD model, adults with children				
TN × Post	0.019**	−0.006	0.436***	−0.097
	(0.009)	(0.009)	(0.166)	(0.196)
	[0.029]	[0.545]	[0.009]	[0.621]
R^2	.04	.04	.04	.04
N	298,582	298,125	298,582	298,125
% Change	15.00	−3.39	15.59	−2.60
	0.18	0.16	4.41	3.67
DD model, childless adults				
TN × Post	0.022***	0.013*	0.456***	0.147
	(0.008)	(0.008)	(0.165)	(0.170)
	[0.007]	[0.099]	[0.006]	[0.387]
R^2	.06	.04	.06	.04
N	397,243	397,543	397,243	397,543
% Change	11.94	8.47	10.35	4.02

Note: The reported coefficients are the interaction between Tennessee and the "post" variable. These outcomes refer to have a bad day of physical health or mental health in the past 30 months. For BRFSS outcomes the post variable takes the value of 1 after July 2005. The specifications include state fixed-effect, year fixed-effects, with month fixed-effect when using BRFSS. They also include controls for individual demographic characteristics as: race, gender, education, age and marital status. The models are weighted using survey weights. Block-bootstrap SEs can be found in parentheses, and block-bootstrap p values can be found in brackets. The mean of the dependent is the mean for the outcome variable in Tennessee before 2005.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; DD, difference-in-differences.

[a]These outcomes do not provide evidence of parallel trends.

$^{*}p \leq .10$; $^{**}p \leq .05$; $^{***}p \leq .01$.

TABLE A3 Robustness of main outcomes across different specifications

Outcome:	Had a mammogram in the past 12 months? (women over 50) BRFSS	Had a breast exam in the past 12 months? (women over 21) BRFSS	Pr(number of days being incapacitated ≥5) BRFSS	Number of days incapacitated in the past 30 days BRFSS	Pr(going to the ED in the past 12 months) NHIS	Number of times in ED, past 12 months NHIS	Pr (does not go to place when sick) NHIS	Pr(does not go to one place for preventative care) NHIS
Pre-mean in TN	0.69	0.74	0.24	5.42	0.200	0.310	0.005	0.009
Adults 21–64								
Baseline (00–09)	−0.034**	−0.033**	0.048***	1.193***	−0.016	−0.028	0.025***	0.032***
	(0.017)	(0.014)	(0.012)	(0.262)	(0.02)	(0.04)	(0.01)	(0.01)
	[0.042]	[0.019]	[0.000]	[0.000]	[0.327]	[0.488]	[0.000]	[0.000]
Observations	92,421	147,202	211,443	211,443	48,557	48,557	48,678	48,671
2000–2007	−0.034	−0.020	0.039***	0.973***	−0.00	−0.02	0.03***	0.03***
	(0.022)	(0.016)	(0.014)	(0.318)	(0.024)	(0.047)	(0.008)	(0.009)
	[0.120]	[0.199]	[0.007]	[0.002]	[0.854]	[0.601]	[0.001]	[0.000]
Observations	68,604	113,796	155,904	155,904	39,988	39,988	40,086	40,081
No weights	−0.030**	−0.026**	0.043***	1.218***	−0.00	0.01	0.02***	0.02***
Adjustment	(0.012)	(0.010)	(0.009)	(0.215)	(0.016)	(0.032)	(0.005)	(0.006)
	[0.014]	[0.013]	[0.000]	[0.000]	[0.980]	[0.861]	[0.000]	[0.000]
Observations	92,421	147,202	211,443	211,443	48,557	48,557	48,678	48,671
No controls	−0.039**	−0.038***	0.057***	1.406***	−0.01	−0.02	0.02***	0.03***
	(0.018)	(0.014)	(0.012)	(0.282)	(0.019)	(0.040)	(0.006)	(0.007)
	[0.029]	[0.007]	[0.000]	[0.000]	[0.518]	[0.636]	[0.000]	[0.000]

TABLE A3 (Continued)

Outcome:	Had a mammogram in the past 12 months? (women over 50) BRFSS	Had a breast exam in the past 12 months? (women over 21) BRFSS	Pr(number of days being incapacitated ≥5) BRFSS	Number of days incapacitated in the past 30 days BRFSS	Pr(going to the ED in the past 12 months) NHIS	Number of times in ED, past 12 months NHIS	Pr (does not go to place when sick) NHIS	Pr(does not go to one place for preventative care) NHIS
Observations	92,815	147,840	212,402	212,402	49,258	49,258	49,400	49,396
Aggregate data	−0.034*	−0.030*	0.046***	1.284***	−0.01	0.00	0.02***	0.03***
State-quarter-	(0.019)	(0.017)	(0.013)	(0.302)	(0.023)	(0.046)	(0.008)	(0.008)
Year levels	[0.086]	[0.085]	[0.000]	[0.000]	[0.809]	[0.966]	[0.003]	[0.001]
Observations	107	107	158	158	170	170	170	170

Note: The reported coefficients are the interaction between Tennessee and the "post" variable. For BRFSS outcomes the post variable takes the value of 1 after July 2005, and after the second quarter of 2005 for NHIS outcomes. The specifications include state fixed-effect, year fixed-effects, with month fixed-effect when using BRFSS. They also include controls for individual demographic characteristics as: race, gender, education, age and marital status. The models are weighted using survey weights. Block-bootstrap SEs can be found in parentheses, and block-bootstrap p values can be found in brackets. The mean of the dependent is the mean for the outcome variable in Tennessee before 2005.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; NHIS, National Health Interview Survey.

$^{*}p \le .10$; $^{**}p \le .05$; $^{***}p \le .01$.

TABLE A4 Heterogeneity of results

Outcome:	Had a mammogram in the past 12 months? (women over 50) BRFSS	Had a breast exam in the past 12 months? (women over 21) BRFSS	Pr(number of days being incapacitated ≥5) BRFSS	Number of days being incapacitated BRFSS	Pr(going to the ED in the past 12 months) NHIS	Number of times in ED, past 12 months NHIS	Pr (does not go to place when sick) NHIS	Pr(does not go to one place for preventative care) NHIS
Sample: Black	−0.014	−0.014	0.013	0.028	0.067	0.174*	0.042***	0.037**
	(0.05)	(0.04)	(0.03)	(0.66)	(0.05)	(0.09)	(0.02)	(0.02)
	[0.782]	[0.747]	[0.691]	[0.965]	[0.181]	[0.058]	[0.006]	[0.033]
N	12,431	22,151	30,917	30,917	11,075	11,075	11,106	11,105
Pre-mean	0.74	0.73	0.25	5.49	0.222	0.338	0.000	0.007
Sample: Hispanic	−0.086	−0.022	−0.034	0.790	−0.140	−0.144	0.007	0.063*
	(0.11)	(0.09)	(0.11)	(2.01)	(0.09)	(0.17)	(0.01)	(0.03)
	[0.445]	[0.798]	[0.751]	[0.695]	[0.127]	[0.399]	[0.321]	[0.073]
N	2832	5355	8681	8681	7034	7034	7049	7046
Pre-mean	0.75	0.74	0.21	4.64	0.206	0.294	<0.000	<0.000
Sample: White	−0.037**	−0.033**	0.055***	1.432***	−0.034	−0.070	0.020***	0.026***
	(0.02)	(0.01)	(0.01)	(0.29)	(0.02)	(0.04)	(0.01)	(0.01)
	[0.035]	[0.026]	[0.000]	[0.000]	[0.101]	[0.108]	[0.005]	[0.002]
N	73,382	113,056	160,385	160,385	28,998	28,998	29,066	29,064
Pre-mean	0.68	0.74	0.24	5.45	0.194	0.304	0.006	0.010
Sample: Female	−0.034**	−0.033**	0.041***	1.034***	−0.022	−0.031	0.012**	0.014**
	(0.02)	(0.01)	(0.01)	(0.29)	(0.03)	(0.05)	(0.01)	(0.01)
	[0.049]	[0.021]	[0.005]	[0.000]	[0.420]	[0.564]	[0.025]	[0.034]
N	92,421	147,202	136,614	136,614	24,311	24,311	24,389	24,385
Pre-mean	0.69	0.74	0.25	5.63	0.203	0.321	0.004	0.004

TABLE A4 (Continued)

Outcome:	Had a mammogram in the past 12 months? (women over 50) BRFSS	Had a breast exam in the past 12 months? (women over 21) BRFSS	Pr(number of days being incapacitated ≥5) BRFSS	Number of days being incapacitated BRFSS	Pr(going to the ED in the past 12 months) NHIS	Number of times in ED, past 12 months NHIS	Pr (does not go to place when sick) NHIS	Pr(does not go to one place for preventative care) NHIS
Sample: Male			0.053***	1.309***	−0.012	−0.023	0.036***	0.048***
			(0.02)	(0.41)	(0.03)	(0.05)	(0.01)	(0.01)
			[0.007]	[0.001]	[0.659]	[0.633]	[0.001]	[0.000]
N			74,829	74,829	24,246	24,246	24,289	24,286
Pre-mean			0.22	4.98	0.196	0.300	0.005	0.013
Sample: Less Than HS	−0.100**	−0.095**	0.014	1.635	−0.033	−0.090	0.035	0.078***
	(0.05)	(0.04)	(0.04)	(1.03)	(0.05)	(0.11)	(0.02)	(0.03)
	[0.037]	[0.025]	[0.727]	[0.114]	[0.487]	[0.394]	[0.114]	[0.006]
N	10,431	14,342	25,758	25,758	8279	8279	8311	8308
Pre-mean	0.63	0.61	0.44	9.77	0.219	0.356	0.007	0.007
Sample: HS Graduate	−0.045*	−0.019	0.059***	1.696***	0.014	0.067	0.036***	0.032*
	(0.02)	(0.02)	(0.02)	(0.43)	(0.03)	(0.09)	(0.01)	(0.02)
	[0.052]	[0.331]	[0.006]	[0.000]	[0.660]	[0.445]	[0.007]	[0.052]
N	31,292	46,022	64,665	64,665	13,719	13,719	13,762	13,763
Pre-mean	0.68	0.70	0.25	5.59	0.200	0.314	0.008	0.008
Sample: College or	−0.006	−0.028	0.046***	0.715**	−0.035	−0.079	0.015***	0.017**
or More	(0.02)	(0.02)	(0.01)	(0.30)	(0.03)	(0.05)	(0.01)	(0.01)
	[0.788]	[0.117]	[0.001]	[0.016]	[0.189]	[0.086]	[0.007]	[0.020]

(Continues)

TABLE A4 (Continued)

Outcome:	Had a mammogram in the past 12 months? (women over 50) BRFSS	Had a breast exam in the past 12 months? (women over 21) BRFSS	Pr(number of days being incapacitated ≥5) BRFSS	Number of days being incapacitated BRFSS	Pr(going to the ED in the past 12 months) NHIS	Number of times in ED, past 12 months NHIS	Pr (does not go to place when sick) NHIS	Pr(does not go to one place for preventative care) NHIS
N	50,698	86,838	121,020	121,020	26,559	26,559	26,605	26,600
Pre-mean	0.71	0.79	0.18	4.22	0.193	0.292	0.001	0.009
Sample: Age: 21–40		−0.002	0.018	0.305	0.000	−0.080	0.034**	0.069***
		(0.03)	(0.02)	(0.43)	(0.03)	(0.07)	(0.01)	(0.02)
		[0.962]	[0.400]	[0.481]	[0.991]	[0.286]	[0.010]	[0.000]
N		24,682	42,093	42,093	17,834	17,834	17,873	17,873
Pre-mean		0.74	0.13	2.91	0.216	0.367	0.006	0.012
Sample: Age: 40–64	−0.034**	−0.044***	0.057***	1.492***	−0.029	−0.005	0.020***	0.010*
	(0.02)	(0.02)	(0.02)	(0.33)	(0.02)	(0.05)	(0.01)	(0.01)
	[0.046]	[0.004]	[0.000]	[0.000]	[0.219]	[0.910]	[0.001]	[0.087]
N	92,421	122,520	169,350	169,350	30,723	30,723	30,805	30,798
Pre-mean	0.69	0.74	0.28	6.28	0.191	0.282	0.004	0.007

Note: The reported coefficients are the interaction between Tennessee and the “post” variable. For BRFSS outcomes the post variable takes the value of 1 after July 2005, and after the second quarter of 2005 for NHIS outcomes. The specifications include state fixed-effect, year fixed-effects, with month fixed-effect when using BRFSS. They also include controls for individual demographic characteristics as: race, gender, education, age and marital status. The models are weighted using survey weights. Block-bootstrap SEs can be found in parentheses, and block-bootstrap *p* values can be found in brackets. The mean of the dependent is the mean for the outcome variable in Tennessee before 2005. A group of rows contains the information from the model run for a given sample.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; NHIS, National Health Interview Survey.

$^{*}p \leq .10$; $^{**}p \leq .05$; $^{***}p \leq .01$.

TABLE A5 Comparison of TennCare reform across other health-insurance reforms

	TennCare—Childless adults		ACA Medicaid expansions (childless adults)		OHE		Massachusetts reform	
Insurance								
Has Medicaid (NHIS)	-0.028^{***a}		0.156^{***c}		0.197^{***d}			
Has health insurance (NHIS)	-0.040^{***a}		0.082^{***c}		0.179^{***d}		0.043^{**h}	
Has health insurance (BRFSS)	-0.038^{***a}		0.101^{***c}		0.179^{***d}		0.050^{***f}	
							0.056^{*e}	
Preventative care	β	"LATE"	β	"LATE"	β	"LATE"	β	"LATE"
Had a mammogram in the past 12 month?	-0.034^{**a}	0.72^{a}	0.008^{b}	0.074^{b}	0.055^{***d}	0.187^{d}	-0.0136^{f}	0.27
			-0.055^{c}	0.670^{c}				
Had a breast exam in the past 12 months?	-0.033^{**a}	0.825^{a}	0.007^{b}	0.082^{b}				
Had a Pap test within last 12 month?	-0.020^{a}	0.50^{a}	-0.016^{b}	-0.304^{b}	0.051^{***d}	0.183^{d}		
Self-reported health								
Number of days with bad physical health (BRFSS)	0.456^{***a}	13.02^{a}	-0.842^{***b}	-9.054^{b}	-0.381^{**d}	-1.317^{d}	-0.098^{**e}	-1.721^{e}
Number of days with bad mental health (BRFSS)	0.147^{a}	4.08^{a}	-1.063^{***b}	-9.208^{b}	-0.603^{***d}	-2.082^{d}	-0.059^{**e}	-1.082^{e}
Number of days being incapacitated (BRFSS)	1.193^{***a}	22.09^{a}	-1.436^{***b}	-12.930^{b}	-0.459^{***d}	1.585^{d}	-0.076^{**e}	-1.360^{e}
Health care utilization								
Pr(Going to the ED in the past 12 months)	-0.016^{a}	0.31^{a}	0.004^{c}	0.048^{c}	0.0065^{d}	0.022^{d}	-0.02^{g}	
							0.011^{h}	0.25^{h}
Number of ED visits in the past 12 months	-0.028^{a}	0.54^{a}	0.0025^{***i}		0.0074^{d}	0.026^{d}	-0.017^{***g}	
Access to care								
Have a personal doctor (BRFSS)?	-0.004^{j}	-0.07^{j}	0.041^{***b}	0.405^{b}	0.081^{***d}	0.280^{d}	0.0126^{**f}	0.25^{f}
Cannot afford care due to cost (BRFSS)?	0.039^{*j}	0.72^{j}	-0.039^{***b}	-0.403^{b}	0.069^{***d}	0.239^{d}	-0.0306^{***f}	0.616^{f}

Note: The estimates from the first three rows or the "Insurance" Panel are the estimates on coverage for each reform. I refer to these as "first-stage." After this panel every outcome estimate has two columns. The left column is titled β, it represents the point-estimate or the effect of the reform on the outcome variable. The estimates from the right column represents the "LATE" estimate or an approximation of it. The approximation is constructed by dividing the point-estimate in β by the appropriate first-stage coverage effect. By "appropriate" I mean the best approximation of the first-stage for that specific sample. For example, for mammograms under the TennCare reform, 0.34 is the coefficient from table 3, Panel 3 (childless adults). To obtain an appropriate coverage effect, instead of using the full-sample effect on insurance, I estimate the effect on insurance for the sample of Women over 50—the sample of the mammograms outcome. This estimate is −0.0472. One can obtain the first-stage by dividing the coefficient from the left over the coefficient from the right. −0.034/0.72 = −0.0472. For each outcome and paper, I provide the coverage estimate for the same sample of the outcome in the notes of each paper or what type of coverage estimate I used.
Abbreviations: ACA, Affordable Care Act; BRFSS, Behavioral Risk Factor Surveillance System; NHIS, National Health Interview Survey.
[a]Tello-Trillo (2021) or estimates from this paper. This table was constructed using the estimates from the sample of childless adults using NHIS and BRFSS for years 2000–2009. The estimates on coverage can be found on table 2, from the panel for "Adults without children." Estimates for β of preventative care can be found in table 3, on the panel "adults without children." Estimates of β for self-reported health can be found in tables 4 and A3 under the panel "adults without children." Estimates for physical and mental health were not supported by parallel trends. Estimates of β on health care utilizations can be found on table 5 under the panel "Adults without children." The coverage estimates for each outcome is presented in tables in this paper, however I have estimated them and report them here: the first-stage for the mammograms outcomes is −0.0472, for breast exams and pap exam is −0.04. The coverage estimate for the estimate of bad physical health is −0.035, for bad mental health −0.036, for incapacitation is −0.054. The coverage effect estimate for the estimate of Pr(ED visits) and Number of ED visits is −0.052.
[b](Simon et al., 2017). The estimates of β for health insurance come from the sample of childless adults, on table 1, Panel 1 Column (7). The estimate for the "LATE" come from the table F1 in the column related to childless adults, where they use IV to obtain the estimate. The paper does not report the coverage result for the sample of childless adults' women over 50, therefore table F1 seems like the best approximation. They have made some of the estimates of the implied elasticities in appendix table H, however these are only done for the statistically significant results. Hence, I concentrate on results from table F1, they are not as different as the ones from appendix table H when comparing them.
[c](Miller & Wherry, 2017). This paper looks at the effects of the ACA Medicaid expansion on many outcomes. They do-not subsample by childless adults, but most of the Medicaid gains do come from childless adults. For, I am using the estimable from table 2, Year 2. This paper does not provide coverage effects for the sub-samples, therefore I use the coverage effect for the full-sample for all their outcomes. For example, for mammograms, although the sample is women over 50, I use the full sample coverage effect, that is 0.055/0.082 = 0.67.
[d](Finkelstein et al., 2012). For the coverage effects, I use estimates of β from table III. For the "Medicaid-NHIS" row I use row (7) "Currently in Medicaid (self-report)" from the column of survey respondent. For the estimate of health insurance, I use row (5) "Currently have any insurance (self-report)." This paper does not sub-sample for childless adults. The mammogram and pap-exam results can be found in table VI. The mammogram result is for women over 40 rather than women over 50. I use the estimate of the ITT and LATE. The estimates on self-reported health are from table IX. I use column (2) "ITT" and column (3) or "LATE" for the estimates. Note that the outcomes in this paper are labeled as number of "good" health days, rather than "bad" health days. I invert the signs, since I assume that an increase in × number of days means a decrease in × number of bad days. For the results of emergency department visits I use the results from table V contains information of ED visits based on survey results. For the probability of visiting an ED, I use column (2) "ITT" and (3) "LATE" under the "extensive margin" column. For the number of visits, I use results from column (6) "ITT" and (7) "LATE" under the "Total Utilization" column. Note that the question in the survey for this paper asks about ED visits in the past 6 months rather than 12 months. For access to care, I use estimates from table X. I use column (2) "ITT and column (3) "LATE" respectively. I use the rows "Have a personal doctor" and "Got all needed medical care, last six months."
[e](Courtemanche & Zapata, 2014). This paper studies the effect of the Massachusetts reform on health outcomes. This paper uses BRFSS for their analysis. They do not sub-sample for childless adults. For the coverage effects I use the estimate on $MA \times After$ from table 8—"first stage." For the self-reported health outcomes, I use estimates from table 6, the coefficient of $MA \times After$. I use column 1–3 with titles "Days not in good physical health," "Days in not good mental health," and "Days with health limitations." To obtain the "LATE" estimate, I use estimates on table 10, which are the estimates from an IV model. I use the coefficient of the second stage as the LATE.

[f](Kolstad & Kowalksi, 2012). This paper uses the NIS and BRFSS to analyze the effects of the Massachusetts reform. They do not have an estimate for childless adults. I use the author's estimate with BRFSS. I use the estimates from table 4, the *MA* × *After* coefficient. For the coverage coefficient, I use the one from column 1 "Any Health Plan." For the mammogram outcome I used the estimate from column (7) "Mammogram last year." For the access to care metrics, I use columns (2) "Any personal doctor" and column (3) "Cannot access care due to cost." In order to obtain the "LATE" estimate I divide the coefficient estimate over the coverage effect of 0.0496.

[g](Miller, 2012a). This paper uses data from NHIS to study the effects of the Massachusetts reform. For the Pr(ED visits) outcome I use table 2, the row "ER visit." There are no coverage effects in this paper so I cannot obtain the "LATE." For the number of ED visits, I use the results from (Miller, 2012b) from table 2, column 2 (visits per capita). I do not divide this because this is already per-capita estimate. There are no coverage effects in this paper to calculate a "LATE."

[h](Long & Stockley, 2011). This paper uses data from the NHIS and evaluated the Massachusetts reform. For the coverage I use estimate from table 1, and Model 3 "income-eligible childless adults in large states." For the outcome Pr(ED visits), I use the estimate from table 1, Model 3, "income-eligible childless adults in large states," and the row "Any emergency room visit." For this outcome I am able to provide a "LATE" by dividing the coefficient over the coverage effect from table 1, Model 3 of 0.043.

[i](Nikpay et al., 2017). This paper uses data from AHRQ Fast Stats ED data. I use estimates from table 1, column "Adjusted DID." I use the estimates of "Total visits per 1000" and divide by 1000 to obtain an estimate of individual level probability. The estimate is 2.47 visits per 1000. This estimate does not have a p value, but it is statistically different from zero as its confidence interval is between 1.06 and 3.88, I use the **** stars because in Table E3 the estimate shows a p value of .008 using the wild-cluster bootstrap procedure. There is no coverage effect in this paper; therefore, I do no present an elasticity effect or "LATE" effect.

[j](Tarazi et al., 2017). I use the estimates from table 2. Note that their sample uses low-income childless adults, and the comparison states are AL, AR, GA, KY, and VA and years are 2003–2008. The coverage effect for the estimate of having a personal doctor and for cannot afford care due to cost is −0.054, which is the coverage estimate from table 2.

$^{*}p \leq .10$; $^{**}p \leq .05$; $^{***}p \leq .01$.

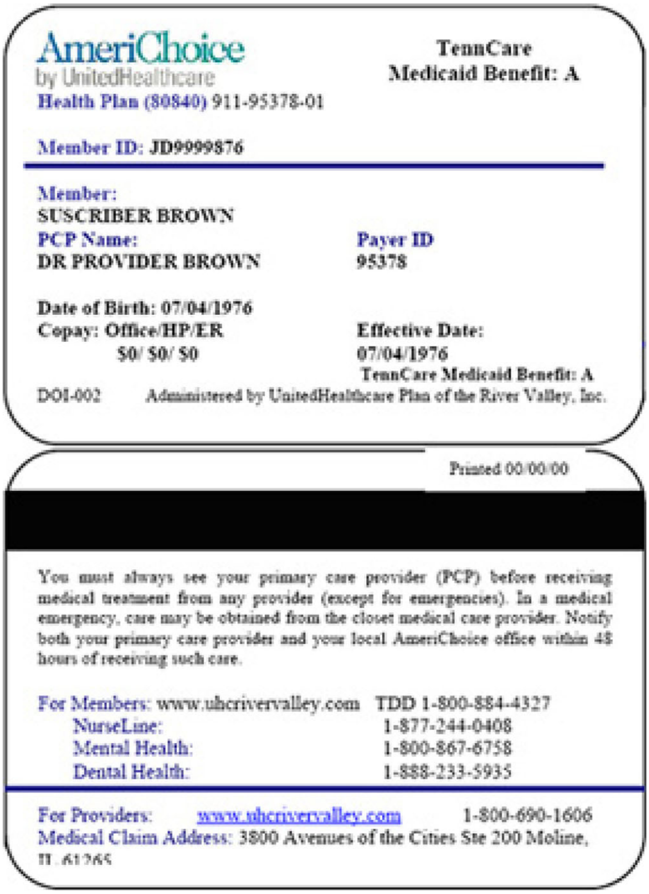

FIGURE A1 Example of insurance card for TennCare [Color figure can be viewed at wileyonlinelibrary.com]

APPENDIX B

Monte Carlo Simulations

Most studies that use a difference-in-difference framework usually account for within-group serial correlation by using cluster-robust SEs (Bertrand et al., 2004). The idea behind this method is that across clusters, the errors are heterogeneous and drawn differently, but within the same cluster the errors are homoskedastic and possibly serially correlated over time. Even though this approach is sensible in most occasions, in this study this is not the case. This is due to two reasons: first, the total number of clusters is not appropriate given the final sample properties from this method; and second, the percentage of treated clusters matters. If this percentage is low—as in this case—the cluster-robust adjustment fails to report accurate SEs (MacKinnon & Webb, 2017).

As to the first issue, Cameron et al. (2008) show that if the number of clusters is not sizeable, then the common cluster-robust adjustment does not work very well because the asymptotics of the cluster procedure have not been met. (Cameron & Miller, 2015) explain that when the number of clusters is low the predicted residuals are closer to zero than the true errors. This leads to

TABLE B1 Monte Carlo simulations

Method	% Rejection of null when p value is .05, $H_o : \beta_{dd} = 0$
OLS	0.098
Robust	0.178
Cluster	0.642
Wild Bootstrap	0.001
Modified block bootstrap	0.055

Note: These results come from 10,000 repetitions using 17 cluster groups.

a downward bias cluster-robust variance matrix. One common alternative that improves cluster-robust methods when the number of cluster is low is wild-bootstrap. This method resamples on the error term for the whole sample and provides an efficiency gain by always using the full sample when bootstrapping.

As to the second issue, MacKinnon and Webb (2017) show that if the proportion of treated clusters is small (the threshold varying from a range of less than 1, 10, 20 and 30%) then most of the methods that there are available thus far, including bootstrap methods, would fail to provide appropriate SEs. Webb and Mackinnon provide simulations from a DD framework and show that the percentage of units treated affects the precision of the rejection rate for each method. In my study I have one treated state and compare it to 17 other states, resulting in about 0.059% treated units of the sample which is right around the threshold when the methods could or could not perform well.

Given the set-up of my analysis it is not clear which SE methods are most appropriate. Hence, I perform a Monte Carlo simulations to study the rejection rates of a different set of SEs. The idea behind these simulations is that the method having the closest to the adequate rejection rate would be the one selected. The simulation process is the following, I create an outcome variable Y under a null imposed, in this case the null is $\beta = 0$, where β is the DD coefficient. Hence, if I regress this outcome using the DD specification, I will have an estimate for β, and a respective SE which I can use to test if β is statistically different from zero. I can do this several times for a specific threshold of rejection, such as $p = .05$, using different SE methods.[28]

The procedure for each iteration is the following:

Obtain a predicted value of Y under the null. The null here is that the coefficient on the difference-in-difference estimate is 0.

$$\text{Estimate } Y_{\text{ist}} = \beta_0 + \beta_1 \times (\text{Post July 2005} \times \text{TN}) + \beta_2 \text{X}_{\text{ist}} + \beta_3 \delta_{\text{t}} + \beta_4 \alpha_{\text{s}} \text{Predict } \tilde{Y}_{\text{ist}}$$

$$= \hat{\beta_0} + 0 \times (\text{Post July 2005} \times \text{TN}) + \hat{\beta}_2 \text{X}_{\text{ist}} + \hat{\beta}_3 \delta_{\text{t}} + \hat{\beta}_4 \alpha_{\text{s.}}$$

Draw errors from a normal distribution. I used errors with mean 0 and variance 0.25 (N (0.25))

Draw errors from a normal distribution in which the variance changes by state.

Create a new outcome variable Y which is the predicted Y under the null adding the errors from step 2 and errors from step 3.

[28]This mean that I want a rejection rate of 5%, which implies that 95% of the time the method is failing to reject the null.

$$\epsilon_{\text{st}} = \nu_{\text{st}} + \eta_{\text{st}}$$

$$\nu_{\text{st}} \sim N(0, 25)$$

$$\eta_{\text{st}} \sim N(0, S)$$

$$S, \text{is the fips code for each state}$$

Once this new set of Y's are created, use this as if they were the original outcomes and estimate the regression analysis with the desired method of SEs. The null that should be tested here is $\beta = 0$, which was imposed previously.

$$Y^*_{\text{ist}} = \tilde{Y}_{\text{ist}} + \epsilon_{\text{st}}$$

Pick a value for the rejection rate, for example, $p = .05$.

Given the results from step 5, count if the method rejected or failed to reject the null.

Repeat steps 2–6 for a number of repetitions.

Finally, calculate the percentage of rejection of the null by dividing the number of times the null was rejected in step 6 over the number of repetitions in step 7.

I performed this simulation for each method using 10,000 repetitions. In addition, I performed the simulations using individual level observations and aggregated state-year observations. The results were similar in both simulations. I report the results from both simulations in the table below. I have also tried different outcome variables and the results were similar as well.

I used a rejection threshold of $p = .05$, which indicates one would want to reject the null 5% of the time. The results indicate that using regular SE with no adjustment, the rejection rate is 9.8%; hence this would be over-rejecting. Using the robust-variance adjustment which accounts for heterogeneity across observations provides a rejection rate of 17.8% and using the cluster option I obtain a 64.2% rejection rate. That is, state clustering performs the worst out of those evaluated and strongly *over*-rejects the null. Wild bootstrap gave a rejection rate of 0.096%, which severely *under*-rejects the null and is also a problem because this could lead one to conclude that there is no effect when in fact there is. Finally, the block-bootstrap procedure gives a rejection rate of 6% which is the closest rejection rate to the preferred 5%. It is worth noting that this is a modified version of Block Bootstrap since I resample within state-year cell and across states observations. This is a two-sample procedure bootstrap and is the same one issued by (Garthwaite et al., 2014). These results are consistent across the various specifications of simulations and consistent with the findings of (MacKinnon & Webb, 2017). From this exercise I conclude that the most appropriate SE correction to be used in this setting is a modified-block-bootstrap.

APPENDIX C

Data

This section covers the data sources used in this project. It also explains how to obtain each data set and the construction of main variables as well as sample restrictions. I have created a replication kit than can be found on my website for full reproduction of the results. I used STATA 15.1 for the analysis.

Behavioral Factor Surveillance System

I obtained the BRFSS from the CDC website (https://www.cdc.gov/brfss/annual_data/annual_data.html). The user should be able to download the raw data for the relevant years. I have also included the raw data in the replication package for ease of use. The raw data should be in folders with titles "CSBRFSyyXPT." Each folder should have an "xpt" file. This is the file I use to import into STATA.

First, I extract each year, keep the southern states and destring the date variables. Then, I append all the years into one file and save it under "brfss_before.dta."

Second, I clean the brfss_before.dta. This process involves recoding binary variables and cleaning other demographics characteristics, namely: age, race, education, gender, marital status and childless status. I also create the main outcome variables used in the analysis. A couple of notes on some key variables. For creating a sample of childless adults, I create a variable called "num_children" which estimates the number of children under the age of 18 in a given household. From this variable, I create a binary variable that takes the value of 1 if the number of children is one or greater than 0 otherwise. For the outcomes of preventative care, I use two variables to create the outcome variable. The first variable identifies if the person has ever had a given preventative care test. The second variable ask how long since the respondent has had that particular test. I create the main outcome variable, by assigning 1 if the respondent has had a preventative care test in the past 12 months, and 0 if the respondent has never had a test or has had the test more than 12 months ago.

In terms of sample restriction, I keep individuals living in states that are in the South region as defined as the US Census, which includes: Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia and West Virginia, Alabama, Kentucky, Mississippi, Tennessee. Arkansas, Louisiana, Oklahoma and Texas. Then I keep individuals ages 21 and up. I also create a variable called full sample which includes everyone who is 21–64. There are some variables that change the name across the years I standardized all of these variables that I need for the analysis. The script in the replication folder shows how I standardize each variable. Finally, I save a file that only includes what is needed for the analysis called brfss_final.dta. This is the data set I use to run the main analysis. For ease of use, I have included this data set in the replication kit, so that the user can replicate the results by running the analysis do-file using these cleaned data.

National Health Interview Survey

The National Health Interview Survey is available online and can be downloaded from (https://www.cdc.gov/nchs/nhis/data-questionnaires-documentation.html). All the needed variables are in these files with the exception of state identifiers. In order to obtain state identifiers, it is required to submit an application to a research data center, the researcher can find more information about the process here. (https://www.cdc.gov/rdc/leftbrch/userestricdt.htm). For completeness, I have a script in the replication kit that cleans the database from the NHIS and runs the analysis, but I do not include the NHIS data.

The data from the RDC comes all together, so no extraction or merge is needed. In the cleaning of the NHIS, there are a couple of things to note: For the insurance variables, I use the recode variables that the NHIS offers. That is, I use the variables named medicaid, medicare, private, and notcov. I standardize them so that they are binary variables that take the value of 1 if they have the plan and 0 if not. In order to create a sample of childless adults, I use three variables to construct it: A family relationship variable (frrp), marital status, and age group. Overall I follow the following rules:

- if the individual is a married parent of a kid in the same household or not, then both parents are not childless adults.
- If the individual is someone over 18 who is a child or brother of the respondent, and do not seem to have a kid of their own in the household, then this person is considered a childless adult. Given the 21 and up restrictions, this effectively becomes any person above 20-year-old who lives with their parent is considered a childless adult as long as they do not have dependents.
- If the individual is a respondent under 18, you are missing (will be dropped from the sample too)
- If the individual is a sibling of a respondent, that is over 18 and never married, and has no dependent in the household, then the individual is considered childless adult.
- If there are two adults living together with no kids in the household, (regardless of marriage) they are considered childless adults.
- If a person is living with someone (not married) and one of them has a kid under 18, the one with the kid will not be a childless adult but the partner will be a childless adult.
- If the individual is a grandparent of the respondent, you are also considered a childless adult.

For sample restrictions, I keep only states from the southern region as defined as the U.S Census and individuals ages 21 and up. I also create a variable called fullsample, which takes the value of 1 if the individuals are of age 21–64.

TennCare Enrollment data

I obtained these data from this site (https://www.tn.gov/tenncare/information-statistics/enrollment-data.html). This data is publicly available but only available in PDF. I have converted these data into a dta file and its available in the replication kit.

Emergency Department Data

I obtained these data by submitting a data request to the Tennessee department of health. One can submit a request in this site (https://www.tn.gov/health/health-program-areas/statistics.html). The data request comes with the information that one has requested, so each file is different. Once the data is aggregate it to a year level, one can use the script I provide in the replication kit to produce the figures.

Tennessee population by age

The age data can be obtained from the census from this site: https://www2.census.gov/programs-surveys/popest/datasets/2000-2005/counties/asrh). These data have the number of people in each county in Tennessee for years 2000–2005 by age. I use these data to create a rate of ED visits in Tennessee overtime from Figure 3a,b. Since this is public access, I have created a cleaned file, that is ready to use in my replication kit.