The Effects of Losing Medicaid in Body Weight and Health Behaviors *†

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Abstract

Tennessee's 2005 Medicaid reform resulted in approximately 4% of the state's childless adults losing their health insurance coverage. Leveraging this policy-induced variation, I study the effects of Medicaid disenrollment on body weight and health behaviors. Using Behavioral Risk Factor Surveillance System (BRFSS) data from 1997 to 2010, I estimate synthetic difference-in-differences models that compare Tennessee's outcomes before and after the reform with those of a data-driven, synthetic Tennessee. The results suggest that the reform led to a 0.37-point increase in BMI among Tennessean childless adults, contributing to a 4% rise in the prevalence of overweight or obese (BMI$\geq$25) within this group. The effects were large among young adults aged 20-39 and females, who likely experienced significant coverage losses due to the reform. Examining health behaviors, I find that decreased participation in moderate physical activities (e.g., brisk walking, gardening) and inclusion of less vegetable in diet explain nearly two-third of the weight gain. Finally, the findings indicate that the effects of reducing Medicaid coverage on body weight and related health behaviors may not mirror those of increasing Medicaid access.

Keywords: TennCare reform, Medicaid disenrollment, Body Mass Index
JEL Codes: I13, I18, I38

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With over 40% of U.S. adults classified as obese, defined by a Body Mass Index (BMI) of 30 or higher, the proportion of Americans with excess body weight continues to grow (Fryar et al., 2020). This trend has significant impact on healthcare costs, as obesity increases annual direct medical costs by an average of $2,505 per person, totaling $260.6 billion in 2016 alone (Cawley et al., 2021; Cawley and Meyerhoefer, 2012). The role of public health insurance in addressing weight gain among American adults, however, remains hotly debated. While existing research has explored the link between gaining public health insurance and BMI,[1] there remains a notable gap in understanding the potential impact of losing coverage on body weight.

The effects of losing versus gaining public health insurance can be asymmetric for many reasons. One could be the difference in underlying population subject to Medicaid contraction versus expansion in terms of the history of their pre-intervention healthcare exposure, and consequently, knowledge of their own health and health capital (Grossman, 1972). Behavioral reasons may include differential reaction to loss versus gain, as discussed in (Tello-Trillo, 2021), and distortion of reference point for making healthcare decisions (Schwartz et al., 2008). Additionally, prior research suggests that the effects of reducing Medicaid access are more pronounced and asymmetrical compared to outcomes of expanding it. A comparison of the effects of Tennessee's 2005 Medicaid contraction on labor supply (Garthwaite et al., 2014), in-patient care (Maclean et al., 2023), preventive healthcare utilization and health (Tello-Trillo, 2021), and financial well-being (Argys et al., 2020) to those of expansions under the Affordable Care Act (ACA) and the Massachusetts healthcare reform corroborates this notion.[2] Such asymmetries may eventually influence body weight and related health behaviors in complex ways.[3]

In this paper, I examine the effects of losing public health insurance on body weight and relevant health behaviors, in the context of Tennessee's 2005 Medicaid reform, which abruptly terminated Medicaid coverage for approximately 170,000 Tennessean childless adults. Using data from the 1997 to 2010 Behavioral Risk Factor Surveillance System (BRFSS), I apply the Synthetic Difference-

[1]The point estimates range from -0.22 to +0.19 BMI point, corresponding to a weight change of -1.54 to +1.33 lbs for 5 feet 10 inches tall adult (Allen and Baicker, 2021; Soni, 2020; Courtemanche et al., 2018; Simon et al., 2017; Barbaresco et al., 2015; Courtemanche and Zapata, 2014; Baicker et al., 2013; Newhouse, 1993).

[2]For comparison, see Duggan et al. (2019); Maclean and Saloner (2019); Hu et al. (2018); Mazumder and Miller (2016); Courtemanche et al. (2018)

[3]For example, (Garthwaite et al., 2014) found that Tennessee's Medicaid contraction increased among childless adults by nearly 4 percentage points (Garthwaite et al., 2014), while an effect of opposite magnitude is not observed for ACA Medicaid expansions Duggan et al. (2019). All else constant, formal employment to obtain employer sponsored health insurance in response to Medicaid disenrollment may alter time and income allocated to food preparation, type of food consumed (e.g., fast food or homemade), and physical activity, potentially affecting balance between calorie intake and expenditure.

in-Differences (SDiD) method to evaluate the impact of Tennessee's Medicaid reform on childless adults. The analysis compares the pre- and post-reform outcomes for Tennessee's childless adults with those of a synthetic control group. This synthetic control is constructed as a weighted average of states that did not implement significant changes to Medicaid income eligibility criteria during the study period.

My main findings suggest that the reform significantly increased the share of uninsured and led to a 0.37 point increase in BMI among Tennessean childless adults, corresponding to $\sim$3 pounds (1.36 kg) weight gain for someone 5 feet 10 inches tall. The effects were large among young adults aged 20-39 and females, who likely experienced significant coverage losses due to the reform. I further document that the BMI effect increased the share of childless adults overweight or obese [BMI$\geq$ 25] by at least 2.5-4 percentage points ($\sim$4-7%).

Examining health behaviors, I find that the reform significantly reduced access to care (due to cost barrier), participation in moderate physical activities (e.g., brisk walking, gardening, and lawn mowing) and the number of vegetable servings included in diet. Although these are imperfect proxies of calorie intake and expenditure, back-of-the-envelope calculations suggest that calorie imbalance indicated by these estimated effects explain nearly two-third the estimated effect on BMI.

Amid 2023-24 Medicaid unwinding, disenrolling more than 24 million non-elderly adults and children in the U.S., this study provides timely evidence on the effects of Medicaid disenrollment on body weight and health behaviors.[4] The results can also proactively inform policymakers about the potential impacts of future Medicaid contractions, especially if stricter eligibility criteria, such as Medicaid work requirements, are implemented. Furthermore, recent evidence shows that, even after the introduction of the Patient Protection and Affordable Care Act (ACA), risk of losing health insurance for Medicaid or health insurance exchange enrollees remains at 20 percent, and those who lose coverage remain uninsured for a long period (Einav and Finkelstein, 2023). Consequently, the findings of this study have important policy implications.

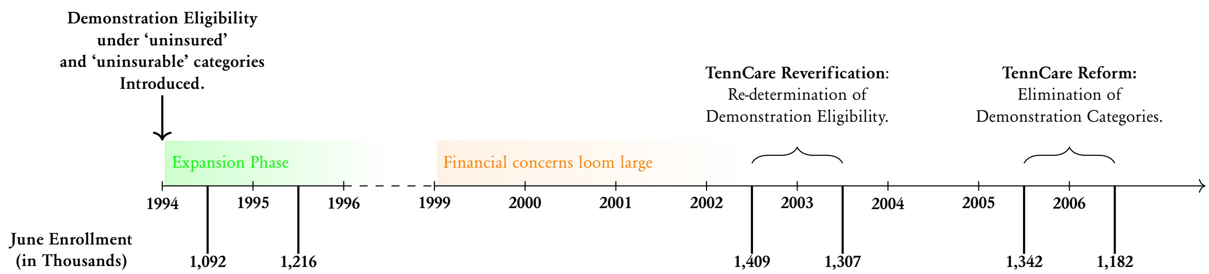
1 Background on Tenneesee's Medicaid Reforms

With approval from the Health Care Financing Administration, Tennessee launched its reformed Medicaid program, TennCare, in January 1994. This marked (i) transition from a fee-for-service model to capitation under managed care to reduce costs per enrollee and (ii) expansion of Medicaid

[4] Medicaid Enrollment and Unwinding Tracker. Link: https://www.kff.org/report-section/medicaid-enrollment-and-unwinding-tracker-overview/

eligibility under Section 1115 Demonstration waiver. The Demonstration waiver allowed any Tennessean to obtain Medicaid coverage regardless of income, either by certifying they were uninsured as of March 1 of the prior year or by proving denial of coverage due to preexisting conditions. By 1995, about 200,000, or 44%, of demonstration enrollees had incomes above 100% of the Federal Poverty Level (Wooldridge et al., 1996).[5]

Figure 1: TennCare Timeline



Data Source: TennCare Division of the Tennessee State Government.

Facing rising TennCare costs and budget shortfalls from 1999 to 2002, Governor Sundquist proposed removal the federal matching fund cap and dividing enrollees into two categories (Bennett, 2014).[6] Starting in July 2002, TennCare II required re-verification of eligibility (TennCare Bureau, 2002). Enrollees had to prove they lacked access to other insurance or undergo a physical exam to confirm preexisting conditions. This process found 28,892 ineligible and disenrolled 146,584 for non-response (TennCare Bureau, 2003). Despite these efforts, TennCare costs continued to rise, consuming one-third of the state budget by 2004 (Farrar et al., 2007).

At the request of Governor Bredesen, McKinsey & Company produced two reports on TennCare's financial sustainability in 2003 and 2004. The latter predicted that abolishing demonstration categories would result in substantially more net savings than any other viable TennCare reform (McKinsey and Company, 2004). Following this, in January 2005, Governor Bredesen announced return to traditional Medicaid income and children based Medicaid eligibility criteria (Chang and Steinberg, 2009). Approved on March 24, 2005, this change resulted in the termination of coverage for approximately 170,000 childless adults in the "uninsured" and "uninsurable" categories between July 2005 and June 2006 (Chang and Steinberg, 2009).

[5]These demonstration enrollees had to pay premium and were subject to cost sharing through deductibles and co-pays. The premium depended on income as percent of FPL and family size.

[6]Since the initial waiver for TennCare was supposed to expire on December 30, 1999 [see https://www.tn.gov/content/dam/tn/tenncare/documents/qualitystrategy.pdf]. The rationale for dividing the TennCare enrollees into broad categories allowed modification of change of services or eligibility for one set of individuals without affecting those in the other categories.

2 Related Literature and Contributions

2.1 TennCare, Difference-in-Differences, and Parallel Trend

Over the last decade, studies examining the effects of the 2005 TennCare reform relied on Difference-in-Difference designs in various ways, commonly with other southern states as the control group. However, for many outcomes such as employment (Garthwaite et al., 2014; Ham and Ueda, 2021), risky health behaviors (Tello-Trillo, 2021), mortality (Ghosh et al., 2023), and crime (Deza et al., 2024), substantive evidence supporting the parallel trend assumption was not present in the raw data.[7]Moreover, studies analyzing the impact of the 2006 Massachusetts healthcare reform on body weight, lacked evidence to support the parallel trend assumption [see Courtemanche and Zapata (2014) and McInerney and Meiselbach (2020)].

I contribute to the empirical policy analysis literature by applying the synthetic difference-in-differences method, addressing two key limitations in analyzing single treated units. First, inference methods under difference-in-differences design, including Cluster Robust Variance Estimator (CRVE), pairs-bootstrap, and block matrix bootstrap methods severely understate the standard error in the case of a single treated unit (MacKinnon and Webb, 2020; Conley and Taber, 2011; Angrist and Pischke, 2009).[8] Second, evidence supporting the parallel trend assumption under DiD design is not always present in the raw data, potentially leading to biased estimates. In many cases, this remains true even after covariate adjustment and selection of specific pre-intervention period.

To address these concerns and avoid subjectively choosing comparison states, I construct comparison group of states that did not significantly change the Medicaid income eligibility criteria between 1997 and 2010 for the demographic group under study (e.g., childless adults, parents). I then estimate synthetic difference-in-differences models, which incorporate unit as well as time weights to correct for potential bias affecting DiD coefficients. While the unit weights remove bias stemming from nonparallel outcome evolution in the treated unit and the potential donor group, the time weights compensate for inexact approximation of the treated unit's pre-trend. Under ideal conditions, these weights reduce bias to a negligible level. At a high level, the SDiD estimator is doubly robust and consistent if either the model is well specified or the weights are optimally chosen and inherits double robustness properties (Arkhangelsky et al., 2021).

[7]This occurred either in the form of high volatility in pre-treatment effects or discernible monotonic pattern in the pre-treatment estimates. While the former case is quite evident in most TennCare studies [see Garthwaite et al. (2014); Ham and Ueda (2021); Tello-Trillo (2021)], the latter case is subtle but observable in a few cases [see Deza et al. (2024)].

[8]For detailed discussion, see Ferman and Pinto (2019), MacKinnon and Webb (2020), and section 8.2.3 of Angrist and Pischke (2009).

In this paper, I report estimates for body weight outcomes using difference-in-differences, synthetic difference-in-differences, and synthetic control methods. I show that BMI estimates do not vary substantially across these methods. However, in Appendix (*will be added shortly*), I show that when pre-trends between the treated and unweighted donor units differ substantially, as observed for mortality outcomes, imputation of the counterfactual using a data-driven, synthetic unit can lead to coefficient sign reversal [see Table (*will be added shortly*))]. This finding sharply contradicts the all-cause mortality estimate of Ghosh et al. (2023).

2.2 Public Health Insurance and BMI

To date, the literature linking Medicaid and adult body weight only examined the effects of gaining coverage on BMI and obesity. While studies leveraging experimental variation in Medicaid coverage found insufficient evidence to rule out the null effect on BMI (Allen and Baicker, 2021; Baicker et al., 2013; Newhouse, 1993), studies exploiting quasi-experimental variation induced by the recent Medicaid expansions documented mixed effects. Evidence suggests that both 2006 Massachusetts healthcare reform and dependent coverage provision under ACA led to a roughly 0.2 point reduction in BMI, corresponding to $\sim$1.5 pounds weight loss for someone 5 feet and 10 inches tall (Courtemanche and Zapata, 2014; Barbaresco et al., 2015). Barbaresco et al. (2015) also estimated a 1.4 percentage point decline in obesity prevalence among 23-26 year olds following implementation of the dependent coverage provision. On the other hand, studies examining the effects of Medicaid expansions under the ACA found insufficient evidence to establish any robust relationship between gaining Medicaid and body weight (Soni, 2020; Courtemanche et al., 2018; Simon et al., 2017).

Using 5 years of post-expansion data, Soni (2020) documented that following ACA Medicaid expansions, risky health behaviors such as smoking declined and participation in physical exercise increased significantly. These contrast the estimated effects using data for only one or two post-expansion periods Courtemanche et al. (2018), suggesting possible divergence of the medium-term outcomes from those observed within a short-term.

McInerney and Meiselbach (2020) examined the impact of the three large-scale Medicaid expansions mentioned above on quantiles of BMI distribution. Their findings suggested large reduction in BMI for individual over or near the severe obesity threshold (BMI=40).

I contribute to this literature by providing the first evidence on the effect of losing public health insurance coverage on BMI and relevant health behaviors. I also provide evidence that losing public health insurance may alter health behaviors, having direct effects on body weight.

3 Data

3.1 Behavioral Risk Factor Surveillance System (BRFSS)

To estimate effects of 2005 TennCare disenrollment, I use publicly available data from the Behavioral Risk Factor Surveillance System (BRFSS)—a monthly cross-sectional telephone survey conducted by Centers for Disease Control and Prevention (CDC). BRFSS offers at least two advantages. First, BRFSS asks questions related to various preventive healthcare utilization, physical activity, and fruit and vegetable consumption through which health insurance may affect BMI. Second, the sample size of BRFSS is comparatively larger than other health surveys that extend to the 1990s, allowing analysis at the sub-population-level. In the sample of childless adults, there are more than 1200 observations in the state$\times$year cells for Tennessee over the pre-reform period. This allows use of sufficiently long pre-period to test the internal validity of empirical methods devised in this paper. However, one limitation is that BRFSS does not allow distinguishing among health insurance coverage sources. I can only observe whether an individual has health insurance, but not if one is covered by Medicaid, Medicare, employer sponsored, or any other type of health insurance.[9]

The primary outcome considered in this paper, Body Mass Index (BMI), is calculated using self-reported height and weight measures (Kg/m^2). To exclude observations with implausible BMI values, I drop 0.01% observations at the tails of BMI distribution for the nationally representative data pooled across 1997-2010.[10] This results in BMI range of 12.47 to 60.56 for the nationally representative data for 1997 to 2010 and remains identical for the main study sample of 39 states.[11] According to CDC, BMI is defined as weight in kilograms divided by height in meters squared.[12] BMI score within range 18.5-24.9, 25-29.9, and $\geq$ 30 respectively indicates healthy weight, over-

[9] Furthermore, BRFSS did not survey cell phone users until 2010. If there is a systemic relationship between cell phone use and BMI or receiving public health insurance, the estimated coefficients may be biased in an unknown direction since these individuals do not appear in the data.

[10] The outliers dropped through this operation are observed likely due to misreporting rather than natural variation in BMI. This is because BMI values under 12 is an indication of extreme malnutrition and likely fatal (Frølich et al., 2016). On the other end of the distribution, I observe BMI values in excess of 200, which are hardly interpretable.

[11] Studies using BMI computed from self-reported height and weight measures sometime correct BMI for misreporting using method developed by Cawley (2004). However, studies that use BMI as a dependent variable finds that the procedure hardly influence the results Courtemanche et al. (2015). As a result, I do not use this correction in this study. Furthermore, previous research shows that misreporting increases with weight Cawley et al. (2015). Given that post-period averages are consistently greater than pre-period averages, my estimates may only be understated.

[12] CDC: About Adult BMI. Link: https://www.cdc.gov/healthyweight/assessing/bmi/adult_bmi/index.html [Accessed: 2023-09-30]

weight, and obese status for adults 20 years old or older. Following this, I restrict the sample to 20-64 year childless adults and construct binary indicators for overweight or obese (BMI$\geq$ 25) and obese (BMI$\geq$30) categories.

3.2 State Medicaid Income Eligibility Changes: 1997-2010

At the time I am writing this paper, data on state-level Medicaid income eligibility for childless adults dating back to 1997 do not exist. This partially explains why TennCare literature always considered the other southern states or Tennessee's neighbor states as comparison group.[13] I compile information from several sources[14] and identify states that changed Medicaid income eligibility threshold as a percentage of FPL by at least 20 percentage points for non-elderly adults in any year between 1997 and 2010. Policies modifying Medicaid income eligibility for childless adults or parents are exclusively considered; hence, I exclude expansions or contractions for pregnant and breastfeeding women, children, and other special demographic groups. Additionally, changes in the Medicaid eligibility that did not modify the income eligibility threshold are not considered. Table 1 lists the states that changed Medicaid income eligibility threshold by at least 20 percentage points of FPL for childless adults as well as parents.

The baseline comparison group for Tennessee consists 38 states that did not change Medicaid income eligibility threshold for childless adults by 20 percentage points or more.[15]

Table 2 presents the pre- and post-reform averages of key outcomes and demographic characteristics for childless adults in Tennessee and the potential donor states. Relative to pre-reform level, Health Insurance coverage in Tennessee declined by 4.8 percentage points and only 0.7 pp for the comparison states. Consistent with national trend, adults in both Tennessee and comparison states gained weight. The gain for Tennessean childless adults, however, is 0.5 BMI points higher relative to the comparable group. The relative increase in overweight and obesity prevalence among Tennesseans is also larger. Notably, the share of college graduates declined for Tennessee while, for other states, the share increased substantially. Also, employment declined among Tennesseans more relative to those in other states.

[13] Ham and Ueda (2021) are the only exception who constructed a control group with 33 states having per capita income similar to Tennessee.

[14] See Table 1 notes.

[15] The 38 states are: Alabama, Alaska, California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Indiana, Iowa, Kansas, Kentucky, Louisianan, Maryland, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Jersey, North Carolina, North Dakota, Ohio, Pennsylvania, Rhode Island, South Carolina, South Dakota, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, and Wyoming. Note that this list contains all southern states except Arkansas and District of Columbia.

3.3 Empirical Strategy

To estimate the effects of 2005 TennCare reform, I estimate the synthetic Difference-in-Differences (SDiD) models, specified as follows.

$$y_{jt} = \mu + \alpha_j + \beta_t + D_{jt}\tau + \nu_{jt} \tag{1}$$

where, y_{jt} is the aggregate outcome of interest for state j at year t. α_j and β_t non-parametrically adjusts for state- and year-specific effects. $D_{jt} = \mathbf{1}\{j = Tennessee\} \times \mathbf{1}\{t \geq 2006\}$ is the policy variable which equals 1 for the treated state in the post-intervention periods and 0 otherwise. The coefficient of interest, τ estimates the intention to treat effect. ν_{jt} is the idiosyncratic mean-zero error term.

Unlike difference-in-differences, the SDiD imputes the counterfactual trend for Tennessee using a weighted average of the potential donors. These unit-weights are chosen to minimize the outcome difference between Tennessee and comparison states for all pre-treatment periods up to a constant.[16] Additionally, SDiD assigns year-specific (time) weights to pre-period outcomes—often assigning higher weights to periods predictive of post-treatment period outcomes. Intuitively, the time weights ensure that the weighted average of the pre-period outcomes approximates the average post-outcomes for the donor pool up to a constant. Finally, SDiD estimator is computed by minimizing the unit- and time-weighted sum of squared error.

$$(\hat{\tau}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{j=1}^{N} \sum_{t=1}^{T} (y_{jt} - \mu - \alpha_j - \beta_t - D_{jt}\tau)\hat{\omega}_j \hat{\lambda}_t \right\} \tag{2}$$

where, $\hat{\omega}_j$ and $\hat{\lambda}_t$ represent state- and year-specific weights. All other notations are preserved from equation 1.

The SDiD estimates are computed in three simple steps: compute regularization parameters,[17] compute the unit and time weights, and then estimate $\hat{\tau}$ using equation 2. Similar to Synthetic

[16] This replaces the prevalent assumption in the TennCare literature that in the absence of the 2005 TennCare dis-enrollment, the outcome for Tennessee would have followed the trend of the other southern states with a data-driven assumption that a data-driven weighted average of the donor units' outcome series provide a credible counterfactual for the treated series.

[17] The weights are regularized using data-driven parameters. These parameters are shown in equation 4 and 5. For further details, see Arkhangelsky et al. (2021) and Clarke et al. (2023) for more details.

Control (SC) Method, inference using SDiD relies on placebo estimates. However, the difference is that SDiD estimates sample variance of the target estimand using a bootstrap procedure—where, in each iteration, a placebo treatment is assigned to a state from the donor pool with replacement. Once the desired number of iterations is complete, standard error is estimated from the distribution of estimated effect over all iterations [see Algorithm 4 in Arkhangelsky et al. (2021)]. The standard error for the SC and DiD estimates reported in Table 6 are estimated using identical procedure.[18]

For TennCare setting, SDiD offers several key advantages over DiD and SC methods. First, SDiD estimator is consistent if either but not both DiD or SC assumptions are satisfied. Second, the bias due to inexact pre-trend matching is compensated using time weights. In the DiD context, researchers often rely on adjustments using covariates or selecting a pre-intervention window that best supports the common trend assumption (Callaway and Sant'Anna, 2021; Freyaldenhoven et al., 2019). Violations of the common trend assumption is often left unaddressed in the DiD setting.[19] On the other hand, Abadie et al. (2010) prescribes against using the SC if pre-treatment fit is imperfect or pre-intervention period is short.[20] Third, SDiD penalizes heavy contribution from single donor state, reducing the risk of overfitting the target outcome series. Finally, SDiD implements a built-in bias correction mechanism through differential weighting of differences between the treated and the synthetic unit over the pre-intervention periods. The upshot being that even if units weights fail to remove bias stemming from nonparallel pre-period evolution, the combination of unit and time weights can compensate for such failures (Arkhangelsky et al., 2021). Under ideal conditions [discussed in section 5], these weights can be oracle equivalent and reduce bias to a negligible level.

4 Results

4.1 Health Insurance and Body Weight

I begin by examining the approximation of Tennessee's pre-intervention outcomes by counterfactual imputed using difference-in-differences, synthetic control, and synthetic differences-in-differences models. Figure 2a plots the evolution of health insurance coverage for childless adults

[18]Note that, this type of placebo inference may not inherit the properties of randomization tests (Firpo and Possebom, 2018; Bottmer et al., 2024). In appendix C.4, I discuss why RMSPE-based inference proposed by Abadie et al. (2010) is not ideal for our case, and estimate the main results using bias-correction proposed by Abadie and L'hour (2021).

[19]In cases where the parallel trend assumption is satisfied using the donor pool averages, SDiD does not break the DiD by assigning differential unit-weights to donors.

[20]This is because inference using SC relies on the post- over pre-RMSPE ratio. Failure of the synthetic unit to match the treated unit well-enough in the pre-treatment period will lead to incorrect p-values given pre-treatment outcomes for at least a few placebos are significantly better approximated by the respective synthetic unit.

for Tennessee, synthetic units, and the control group.[21] The synthetic units produce slightly improved fit compared to control group, minimally changing the estimated coverage effects [see Column 1 Table 3].[22] The figure highlights a sharp decline in health insurance coverage for Tennessean childless adults between 2005 and 2006. Following this drop, health insurance coverage remained substantially lower than pre-2005 level. In contrast, coverage drops between 2002 and 2003—due to re-verification—and between 2007 and 2008—largely due to financial crisis—were temporary and returned to the respective pre-period level. This highlights that 2005 TennCare disenrollment was a permanent shock to health insurance coverage among Tennessean childless adults. The column 1 of Table 3 shows that 2005 TennCare disenrollment resulted in a significant 4.4 percentage points drop in health insurance coverage for childless adults in Tennessee.

Next I focus on BMI trends plotted in Figure 2b. While the unweighted control group show a flatter pre-trend relative to Tennessee, the SDiD unit weights observably improved the pre-trend fit, partially desbiasing the estimate from possible contamination due to nonparallel evolution in the raw data. The inexact pre-treatment matches[23] are then accounted for using time weights, as shown in equation 2. Column 2 of Table 3 shows that, following 2005 reform, BMI among Tennessean childless adults increased by roughly 0.37 points relative to synthetic Tennessee, corresponding to a 3lb weight gain for a 5 feet 10 inches tall adult.

The magnitude of this effect may be biased in unknown direction due to the 2002-03 TennCare re-verification process and the 2008 recession. To gauge the extent of the bias if any, I estimate the effect using alternative specifications in section 5. I show that, the BMI effect remains stable to across specifications. In section 4.3, I provide evidence that the BMI effect is largely driven by

[21] For comparability, I adjust the Figure A.2 plots for pre-period level difference. For a given outcome, I add the average pre-period difference between Tennessee and the control group to the control group's outcome series. I perform a similar operation for the SDiD unit, but not the SC unit. While this enhances visual comparability, the estimates remain identical.

[22] Although the synthetic unit constructed using SC weights perfectly fits pre-intervention coverage rate for Tennessean childless adults, the unit weights are non-sparse [see Figure A.1], indicating that the number of units assigned positive weights is greater than the number of predictors. This highlights potential risk of overfiting and existence on unstable, nonunique set of SC weights (Abadie, 2021; Chen and Li, 2024). In contrast, SDiD model is designed to assign norsparse weights at the expense of interpretability and prevents the model from overfitting by using data-driven penalties. Yet, SDiD unit weights construct an almost identical counterfactual with a level difference.

[23] Note that the SC and SDiD both offer similar but imperfect fit for Tennessee's BMI trend. While SDiD offer further bias correction using time weights, SC does not. Importantly, imperfect fit in the case of synthetic control method precludes inference, which relies on post- over pre-RMSPE ratio (Root Mean Squared Prediction Errors) for Tennessee and the potential donor units (Firpo and Possebom, 2018; Abadie et al., 2010). To highlight, the pre-RMSPE for Tennessee is 0.175 BMI points, whereas, the minimum and the second least values are 0.002 and 0.036, respectively. This means, for the RMSPE-based test to achieve a p-value less than 0.05 for Tennessee, Tennessee's post-RMSPE has to be at least 6 times larger than states with pre-RMSPE similar to the second least value.

demographic groups losing health insurance coverage due to the 2005 TennCare reform.

At the extensive margin, Table 3 column 4 estimates indicate that increase in BMI led to a 4 percentage points (∼6.5%) increase in the share of overweight or obese (BMI$\geq$ 25) individuals. Obesity estimates in column 3 provide evidence supporting the null effect. Taking together, these two effects suggest a shrinkage of the share of childless adults with healthy weight. Although point estimates for these outcomes vary substantially across different methods, SDiD remains the most efficient.

4.2 Health Behaviors and Potential Mechanisms

According to CDC, health behaviors such as physical activity, food consumption patterns, sleep quality, and screen time directly affects weight gain and obesity risk factor.[24] In this context, I examine the effects of 2005 TennCare contraction on access to care, risky health behaviors, physical activity participation, and consumption of fruits and vegetables for childless adults in Tennessee.

Evidence suggests that health insurance coverage increases access to care (Baicker et al., 2013; Newhouse, 1993; Courtemanche et al., 2018). Similar to Tello-Trillo (2021) and Tarazi et al. (2017), I find that Tenncare disenrollment decreased access to care among childless adults. Panel A of Table 4 shows that, share of individuals reporting lack of access to care due to cost barrier increased by 4 pp (34%) following the 2005 reform. This may affect health behaviors by reducing the frequency of tailored health advice one receives from healthcare professionals, leading to alteration in health behaviors directly affecting body weight and health (Campbell et al., 1994).

Table 4 panel B.1 reports estimates for risky health behaviors such as heavy drinking in the past month and smoking prevalence among Tennessean childless adults. Both estimates are statistically indistinguishable from zero.

Next, I turn to participation and level of physical activities in Panel B.2. While there exists a very weak evidence that the share of childless adults reporting participation in physical activity (exercise) increased following the reform, I find large declines for participation in calorie-intensive activities. Specifically, I document that participation in moderate-intensity physical activities (e.g., brisk walking, gardening, lawn mowing) declined by 7.3 percentage points (9.59%).

According to CDC, moderate-intensity activities on-average burn 145 calories per 30 minutes.[25] Given pre-period mean of 341 minutes per week, a nonparticipant refrained from burning nearly

[24]Risk Factors for Obesity: https://www.cdc.gov/obesity/php/about/risk-factors.html [Accessed: 04.10.2024]

[25]See Physical Activity and Your Weight and Health: Physical Activity and Your Weight and Health [Accessed 01.20.224]

86000 calories over a year. This corresponds to a mean effect of ~1.7 pounds gain per annum, explaining more than one-half the observed effect on BMI.

Estimate for vigorous physical activity participation is also large and negative. However, the effect is statistically insignificant.

Similar to Tello-Trillo (2021) and Tarazi et al. (2017), I find that following disenrollment, the share of individuals could not see a doctor due to cost barriers increased by ~4 percentage points [Column (2) Table ??]. I do not find alteration in propensity to engage in risky health behaviors such as smoking and heavy drinking following abolition of TennCare demonstration category.

Estimates in Panel B.3 of Table 4 suggest that vegetable consumption declined by ~2 servings (1 cup) per week. Since vegetables are less calorie-dense and helps maintain healthy weight, substitution of vegetables with more calorie-dense food such as fast food can substantially increase calorie intake.

Behavioral effects of lack of access to care aside, these behavioral effects could result from an negative income effect from losing TennCare coverage. Note that TennCare demonstration enrollees who had income over 100 percent FPL were required to pay premiums. The premiums varied across income and family size. Conspicuously, in 1994 enrollees with income between 100 and 200 percent FPL had to pay less than \$20 in monthly premiums, and those with income between 200 and 400 percent FPL were charged less than the actuarially fair premium (Wooldridge et al., 1996). Thus, removal of the demonstration categories may have led to consumption alteration in two possible ways. One, disenrollees seeking formal employment may alter their time allocation for food preparation. Garthwaite et al. (2014) finds large employment effect following the disenrollment, which I do not observe in the BRFSS data even when post-treatment periods after 2007 are dropped.[26] Two, disenrollees who could not secure group-based insurance coverage through employment or other sources either remained uninsured or purchased private health insurance by paying relatively higher premiums. While the first case may affect consumption choice through changes in allocation of time to food preparation, the latter case is likely to affect income allocation across consumption choices.

Loss of health insurance coverage may adversely affect body weight if health deteriorates as a consequence, restricting physical activity and consumption choice. The opposite relationship is true as well. Increased BMI may lead to poor health by increasing risk of contracting chronic conditions.

[26]Ham and Ueda (2021) estimated the employment effect using various survey samples, and found that the employment effect was relatively larger and unique to the CPS-March sample. The effect is even negative when estimated using the American Community Survey (ACS) Sample. DeLeire (2019) echoes similar finding using Survey of Income and Program Participation (SIPP) data.

Notwithstanding this reverse causality, a relative increase in the number of days incapacitated due to poor physical or mental health for Tennessean childless adults compared to the synthetic counterpart lends credibility to observed weight gain. And I find that, following the reform, Tennessean childless adults reported significantly higher number of days incapacitated due to poor physical health [see Table 4 Panel C].

4.3 Heterogeneity

I exploit the timing of health insurance coverage losses across demographic groups to identify those relatively more affected by the 2005 TennCare reform and less by the 2008 recession. Specifically, I assume that large coverage reduction between 2005 and 2006 is attributable to the TennCare reform and between 2008 to 2009 is driven by loss of employer sponsored coverage due to the 2008 recession. Figures 3 provide evidence that females relative to males and young (aged 20-39) relative to those older (aged 40-64) experienced significant coverage reduction (3-6pp) due to the 2005 reform.[27] On the other hand, females and Young childless adults did not experience notable coverage losses during the 2008-09 recession relative to their respective demographic complement.

If public health insurance coverage loss increased BMI and recession decreased BMI, we should observe larger increase in BMI for female and young childless adults relative to those demographic groups in the donor states.

Table 5 reports the heterogenous effects of 2005 TennCare reform on body weight across different demographic groups of Tennessean childless adults. The estimated effects for young adults show 0.74 point or $\sim$3% increase in BMI and 7pp or 14% increase in share of overweight or obese [see Panel A Table 5]. In contrast, for older childless adults the estimates are less than half in magnitude with much higher p-values. A similar heterogeneity is observed when compared the effects for female to male childless Tennessean adults.

Compared to the Males, females—who experienced larger coverage losses due to 2005 reform—experienced significantly larger increase in BMI and share of overweight or obese [see Panel B Table 5]. Notably, Panel B column 2 estimate shows a 2.5pp (10.4%) increase in the obesity prevalence for females.

The striking difference between the estimates for the demographic groups severely affected by the reform supports the notion that the main results of this paper are primarily driven the effects

[27] Studies using Current Population Survey (CPS) data also find consistent heterogeneous coverage shock (Garthwaite et al., 2014; Bullinger and Tello-Trillo, 2021). Additionally, I observe that segregating the observations by age 40 cutoff or by gender evenly splits the childless adult sample.

of the 2005 reform and not significantly affected by the 2008 recession. Prior literature further supports this notion that recessions promote healthy habit and better health Ruhm (2005, 2015).

Risk of developing adverse health conditions not only increases at the extensive margin (e.g., obese and not obese) but also along the intensive margin.[28] Using Unconditional Quantile Regression (Firpo et al., 2009), I provide further evidence that supports rise in overweight and obese share of childless adults in Tennessee following the reform. Figure 4 shows that the effects estimated for BMI values just below the overweight threshold $[BMI = 25]$ are significant and precisely estimated. Moreover, there were significantly large effects on BMI distribution of obese population. Specifically for BMI values between 31 and 34, increasing the risk factor Class II obesity within this group. Note that the effect at different unconditional quantiles of BMI is estimated under DiD design and likely lack support of common trend; hence, the interpretation of the estimates are assumed descriptive.

5 Robustness and Sensitivity

In this section, I examine the sensitivity and robustness of the estimated effects on body weight outcomes through a series of tests.

I first add aggregate covariates to the SDiD model.[29] These include the share of females, whites, married, high school graduates, and college graduates in Tennessee's childless adult population. Panel A Table 6 reports the estimates. Although the estimates are marginally lower in magnitude than those reported in Panel A of Table 3, they remain valid at 5 percent significance level.

Next I consider the 2002-03 TennCare demonstration eligibility reverification as a potential source of bias. The reverification process disenrolled more than 150,000 Medicaid enrollees between 2002 and 2003. To assess the magnitude of the potential bias due to this disruption, following Ham and Ueda (2021), I estimate the effects using a subset of the pre-treatment periods motivated by assumptions that the effect of TennCare re-verification on body weight is either permanent or temporary. If the re-verification process had a temporary impact on BMI, dropping years 2002-2004 from the sample suffices. On the other hand, had the effect been permanent, limiting the study

[28] For example, obesity is subdivided into three categories. BMI range of 30 to <35, 35 to <40, and >40 respectively correspond to class 1, 2, and 3 obesity. Class 3 obesity is also known as 'severe obesity' and significantly increases the risk of developing chronic conditions such as diabetes, arthritis, and heart problems. In addition, BMI is far from a perfect measure of adiposity, and underperforms in identifying overweight and obese adults (Burkhauser and Cawley, 2008).

[29] A brief description on how these covariates are incorporated in the model is provided in Appendix C.3.

period to 2002-2010 should remove bias due to the re-verification. In Panel B of Table 6, I report the main results using selected pre-treatment periods motivated by the assumption above. I find that, even if the reverification had permanent effect on body weight, the 2005 reform increased BMI by 0.32 points, which is again marginally lower than the main result. In contrast, the estimate under temporary effect assumption increases to 0.43 BMI points, significant at 95 percent confidence level.

In panel C Table 6, I further document that the increase in BMI and the share of overweight and obese remains statistically significant when the potential donor pool exclusively includes states that did not significantly changed Medicaid income eligibility criteria by at least 20 percentage points of FPL.

Finally, in appendix B, I address various concerns regarding the sensitivity of the estimates and consistency of estimated standard errors. In all cases, the main findings of this paper remain robust.

6 Discussion

Excess body weight is linked to a host of chronic conditions, increasing both direct medical expenditure and mortality risk (Cawley, 2015). This paper provides empirical evidence on the effects of losing public health insurance on body weight and relevant health behaviors. My findings provide new evidence on the asymmetry between gaining versus losing public health insurance. The estimated effect of losing Medicaid, 0.37 BMI points, is at least 50-percent larger compared to the estimates from the Medicaid expansion literature.

My results also shed light on health behaviors directly affecting body weight. Unlike previous literature, I find strong evidence to support the notion that limited access to healthcare can lead to health behaviors that negatively impact the ability to maintain a healthy weight. One explanation could be that, over time individuals with lack of access to care accumulates less information about their current health status and fail to take corrective measures. For instance, consider someone with back pain.[30] Regular brisk walking can help manage the pain in the long run. However, walking can be painful. Lack of access to care and frequent nudges can potentially affect one's tendency to perform this physical activity on a regular basis, leading to less calorie expenditure and mismanagement of pain.

While gaining Medicaid coverage can aid weight loss through increasing access to Medications

[30] In an extreme case, imagine someone with a disc bulge causing sciatic pain.

and surgeries, losing Medicaid is likely to act more through the informational channels, affecting health behaviors. This potentially explains why the effect of losing versus gaining Medicaid coverage can have asymmetric effects across the BMI distribution. Recent Medicaid expansions had large negative effects near the end of the BMI distribution and decreased the share of adults with severe obesity McInerney and Meiselbach (2020). As predicted, I document that the effects are more-or-less uniform across the distribution. However, the effects are significant for individuals below the overweight threshold (BMI=25) and those just over obesity margin (BMI=30). The behavioral implication is that weight changes from a healthy level to the overweight category may not immediately manifest as obvious health issues, leading individuals to become less diligent about managing their weight. On the other hand, individuals already overweight and with limited access to healthcare may lack consistent nudges and information on how to maintain a healthy living.

The policy relevance of the findings in this paper is elevated by the 2023 Medicaid unwinding, disenrolling over 24 million non-elderly adults and children as of July 2024. While future research will examine the effects of such mass-disenrollment, if the main finding of this study is any guide, the potential rise in body weight will increase healthcare cost and consequently may over-burden the healthcare system.

Table 1: Medicaid Expansion and Contraction across States.

State	State Fips Code	Expansion	Contraction
Alaska	2	2000, 2001	
Arizona	4	2001‡, 2003	
Arkansas	5	2006‡	
California	6	2000	
Colorado	8	2002, 2006, 2010	
Connecticut	9	2001, 2005, 2007	2002
District of Columbia	11	2002, 2009†	
Florida	12	2000	
Georgia	13	2001	
Idaho	16	2004†*	
Illinois	17	2002†, 2004, 2005	
Kentucky	21	2000	
Maine	23	1998, 2001, 2002†, 2006	
Maryland	24	2008	
Massachusetts	25	1997, 2006‡	
Michigan	26	2004†	
Missouri	29	1999	2003, 2005
Nevada	32	2006	
New Jersey	34	2000, 2003, 2005, 2008	2002
New Mexico	35	1998, 2005‡	
New York	36	2001‡	
North Dakota	38	1999, 2001	2004
Ohio	39	1998	
Oklahoma	40	2005‡	
Oregon	41	2002‡	
Rhode Island	44	1998	
South Carolina	45	2001	
Tennessee	47	2008	2005‡
Vermont	50	2000	
Washington	53	1998	
Wisconsin	55	1999	

Notes.- This table lists the states that expanded or contracted medicaid by changing income eligibility threshold as a percentage of FPL by at least 20 percentage points. Years without markers indicate expansion/contraction targeted towards parents.
† indicates that the expansion/contraction changed income eligibility threshold for childless adults only, and
‡ indicates that income eligibility threshold for both childless adults and parents have changed.
This table excludes Medicaid eligibility expansion/contraction for pregnant women, individuals with disability, children, and other demographic groups other than parents and childless adults.
Data sources: Birnbaum (2000); ?); Artiga and Mann (2005); Coughlin et al. (2006); Smith et al. (2007); Atherly et al. (2012); Sommers et al. (2012); Rudowitz et al. (2013); McMorrow et al. (2016), Medicaid demonstration and waiver list, and annual reports from Center on Budget and Policy Priorities.

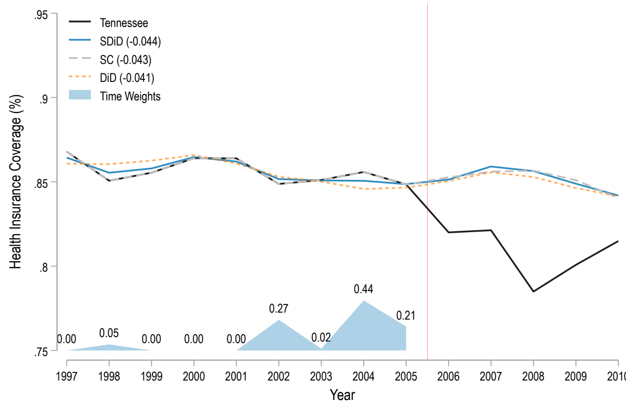
Table 2: Summary Statistics

	Tennessee		Donor Pool	
	1997-2005	2006-2010	1997-2005	2006-2010
Health Insurance Coverage	0.856	0.808	0.843	0.836
Body Mass Index (BMI)	27.04	28.55	26.71	27.81
Obese (BMI$\geq$30)	0.243	0.330	0.216	0.287
Overweight or Obese (BMI$\geq$25)	0.600	0.700	0.582	0.653
Female	0.458	0.452	0.457	0.460
White	0.844	0.810	0.848	0.829
Age	44.02	46.57	43.54	45.98
High School Graduate	0.887	0.901	0.923	0.935
College Graduate	0.249	0.239	0.327	0.361
Employed	0.624	0.531	0.644	0.602
Exercise	0.687	0.697	0.773	0.772

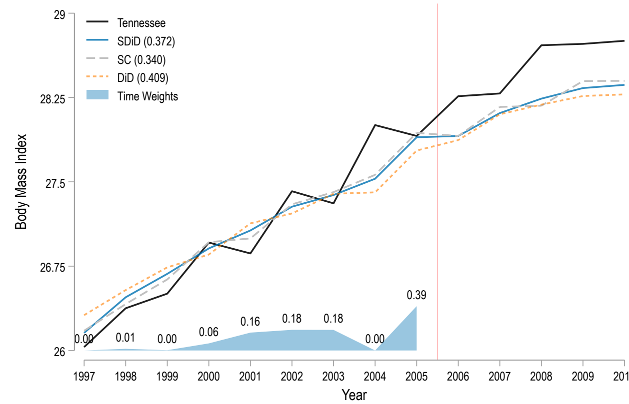
Notes.– This table reports mean characteristics of Tennessee and the potential donors for the pre- and post-reform periods. The sample is restricted to 20-64 year-old adults. All statistics are computed using BRFSS sample weights. The sample includes childless adult sample of age 20-64 in Tennessee and 38 control states that did not change Medicaid income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period. Pregnant women are excluded from this sample.

Figure 2: Health Insurance Coverage and Body Weight Outcome Trend

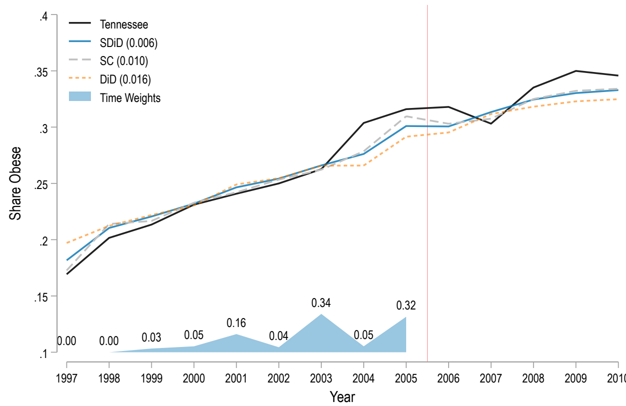
(a) Health Insurance Coverage



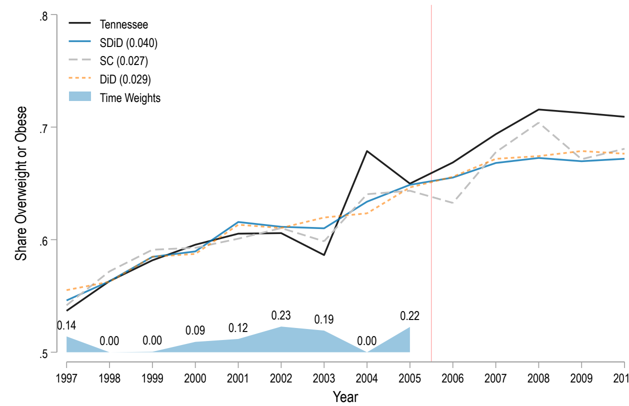
(b) Body Mass Index



(c) Obese $[BMI \geq 30]$



(d) Overweight or Obese $[BMI \geq 25]$



Notes.- State×year means are generated using BRFSS sample weights and childless adult sample of age 20-64 in Tennessee and 36 control states that did not change Medicaid income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period. Pregnant women are excluded from this sample. The synthetic control predictor vector includes all pre-reform outcomes.

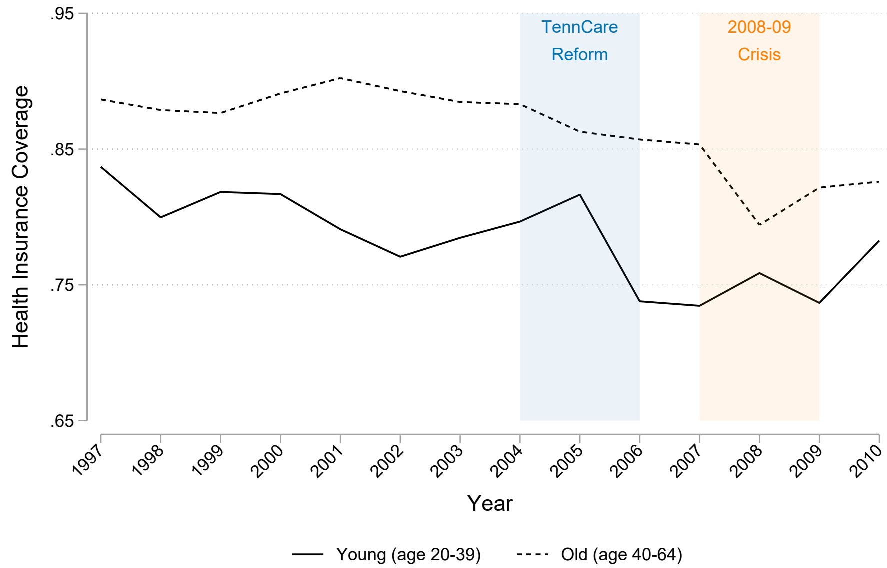
Table 3: Health Insurance and Body Weight Estimates

	Have Health Insurance? (1)	Body Mass Index (2)	Obese [BMI≥30] (3)	Overweight or Obese [BMI≥25] (4)
A. Synthetic Difference-in-Differences				
ITT	-0.044	0.372	0.006	0.040
	(0.013)	(0.130)	(0.011)	(0.010)
	[0.001]	[0.004]	[0.549]	[0.000]
B. Difference-in-Differences				
ITT	-0.041	0.409	0.016	0.029
	(0.016)	(0.155)	(0.012)	(0.010)
	[0.010]	[0.008]	[0.181]	[0.004]
C. Synthetic Control				
ITT	-0.043	0.340	0.010	0.027
	(0.013)	(0.157)	(0.013)	(0.011)
	[0.001]	[0.030]	[0.442]	[0.018]
Pre-reform Mean	0.86	27.04	0.24	0.60
N	546	546	546	546

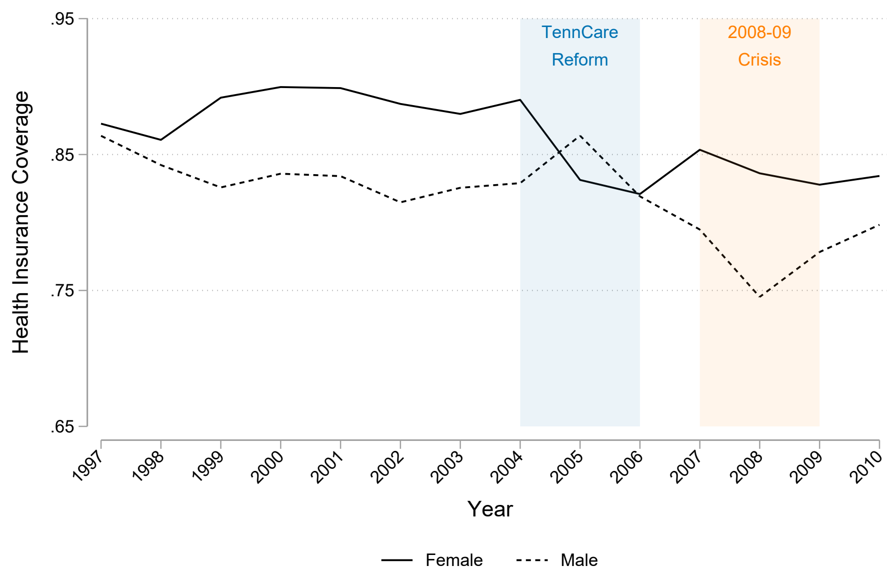
Notes.- Standard errors are reported in parentheses and p-values are reported in brackets. Placebo standard errors are estimated using 200 iterations. Estimates are based on aggregate data generated using BRFSS sample weights and childless adult sample of age 20-64 in Tennessee and 36 control states that did not change Medicaid income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period [see footnote 15]. Pregnant women are excluded.

Figure 3: Heterogeneous Coverage Shocks

(a) Young (age 20-39) V. Old (age 40-64)



(b) Female V. Male



Notes.- State×year×group level health insurance coverage are generated using BRFSS sample weights and childless adult sample of age 20-64 in Tennessee. Pregnant women are excluded from this sample.

Table 4: Access to Care, Health Behaviors, and Poor Health Days

	ITT (se) (1)	p-value (2)	Pre-reform Mean (3)	Percent Change (4)
A. Access to Care				
Have primary care physician[a] (=1)	-0.005 (0.013)	0.705	0.797	-
Couldn't see a doctor due to cost barrier[b] (=1)	0.041 (0.013)	0.001	0.118	34.34
B. Health Behaviors				
B1. Risky Behaviors				
Current Smoker (=1)	0.004 (0.013)	0.777	0.294	-
Heavy Drinking[a] (=1)	-0.001 (0.008)	0.907	0.049	-
B2. Physical Activity				
Exercise[c] (=1)	0.021 (0.014)	0.125	0.686	-
Moderate Physical Activity[d] (=1)	-0.073 (0.023)	0.001	0.758	-9.59
Moderate Physical Activity[d] (=mins/week)	12.325 (30.482)	0.686	340.553	-
Vigorous Physical Activity[d] (=1)	-0.058 (0.038)	0.128	0.392	-
Vigorous Physical Activity[d] (=mins/week)	23.130 (21.594)	0.284	239.987	-
B3. Food Consumption (#Servings/Week)				
Fruit Juice[e]	-0.372 (0.293)	0.204	4.779	-
Fruit[e]	0.057 (0.355)	0.872	5.103	-
Green Salad[e]	0.015 (0.158)	0.927	3.543	-
Vegetables[e]	-1.975 (0.412)	0.000	12.020	-16.43
C. Days in Poor Health Last Month				
Overall Poor Health[f]	1.395 (0.383)	0.000	4.646	30.02
Poor Physical Health[f]	0.538 (0.279)	0.054	3.780	14.23
Poor Mental Health[f]	0.110 (0.271)	0.684	3.490	-

Notes.- Placebo standard errors are estimated using 200 iterations. Estimates are based on aggregate data generated using BRFSS sample weights and childless adult sample of age 20-64 in Tennessee and 36 control states that did not change Medicaid income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period [see footnote 15]. Pregnant women are excluded.
A given outcome listed in this table is available for
[a] 2001 to 2010;
[b] 1997 to 2000 and 2002 to 2010;
[c] 1999 to 2010;
[d] odd years after 2000;
[e] 1997, 1998, 2001, 2003, 2005, 2006, 2007, and 2009; [f] all years except 2002.

Table 5: Effect Heterogeneity for Body Weight Outcomes

	BMI	Obese	Obese or Overweight
	(1)	(2)	(3)
Panel A. Heterogeneity by Age			
Estimate for Age 20-39	0.740	0.016	0.072
	(0.306)	(0.019)	(0.024)
	[0.015]	[0.384]	[0.003]
Pre-reform Mean (Age 20-39)	25.88	0.19	0.49
Estimate for Age 40-64	0.292	0.011	0.021
	(0.138)	(0.012)	(0.009)
	[0.035]	[0.329]	[0.020]
Pre-reform Mean (Age 40-64)	27.66	0.27	0.66
Panel B. Heterogeneity by Gender			
Estimate for Female	0.556	0.025	0.033
	(0.168)	(0.011)	(0.015)
	[0.001]	[0.028]	[0.030]
Pre-reform Mean (Female)	26.65	0.24	0.52
Estimate for Male	0.244	0.001	0.025
	(0.185)	(0.013)	(0.014)
	[0.185]	[0.917]	[0.071]
Pre-reform Mean (Male)	27.38	0.24	0.66

Notes.- See Table 3 notes.

Figure 4: Unconditional Quantile Effects (DiD)

Notes.- These graphs plot the unconditional quantile effects for 5th to 99th percentiles of BMI distribution with 1 point increment. The Bootstrapped standard errors are estimated using 50 iterations. The sample is restricted to 20-64 year-old adults. Estimates are based on BRFSS childless adult sample of age 20-64 in Tennessee and 38 potential donor states that did not change income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period. Pregnant women are excluded from this sample. The covariates include age, education level, and binary indicators for female, white, and marital status.

Table 6: Effects of TennCare Disenrollment

	Have Health Insurance? (1)	Body Mass Index (2)	Obese [BMI≥30] (3)	Overweight or Obese [BMI≥25] (4)
Alternative Specification & Methods				
A. Add Covariates				
ITT	-0.036	0.326	0.005	0.037
	(0.014)	(0.139)	(0.011)	(0.010)
	[0.011]	[0.019]	[0.679]	[0.000]
Alternative Study Periods				
B. Study Period 2002-2010				
ITT	-0.045	0.322	0.004	0.039
	(0.013)	(0.127)	(0.011)	(0.010)
	[0.000]	[0.011]	[0.711]	[0.000]
C. Study Period 1997-2001 and 2005-2010				
ITT	-0.042	0.426	0.008	0.031
	(0.015)	(0.147)	(0.012)	(0.011)
	[0.007]	[0.004]	[0.511]	[0.005]
Alternative Control Group				
D. 20 Potential Donor States				
ITT	-0.045	0.385	0.006	0.044
	(0.010)	(0.163)	(0.013)	(0.010)
	[0.000]	[0.018]	[0.630]	[0.000]

Notes.- Standard errors are reported in parentheses and p-values are reported in brackets. Placebo standard errors are estimated using 200 iterations. The sample is restricted to 20-64 year-old adults. If not mentioned otherwise, estimates in column (1)-(3) are based on aggregate data generated using BRFSS sample weights and childless adult sample of age 20-64 in Tennessee and 38 potential donor states that did not change Medicaid income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period [see footnote 15]. Pregnant women are excluded.

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A Figures and Tables

Figure A.1: Effect Distribution and Unit Weights - Health Insurance Coverage

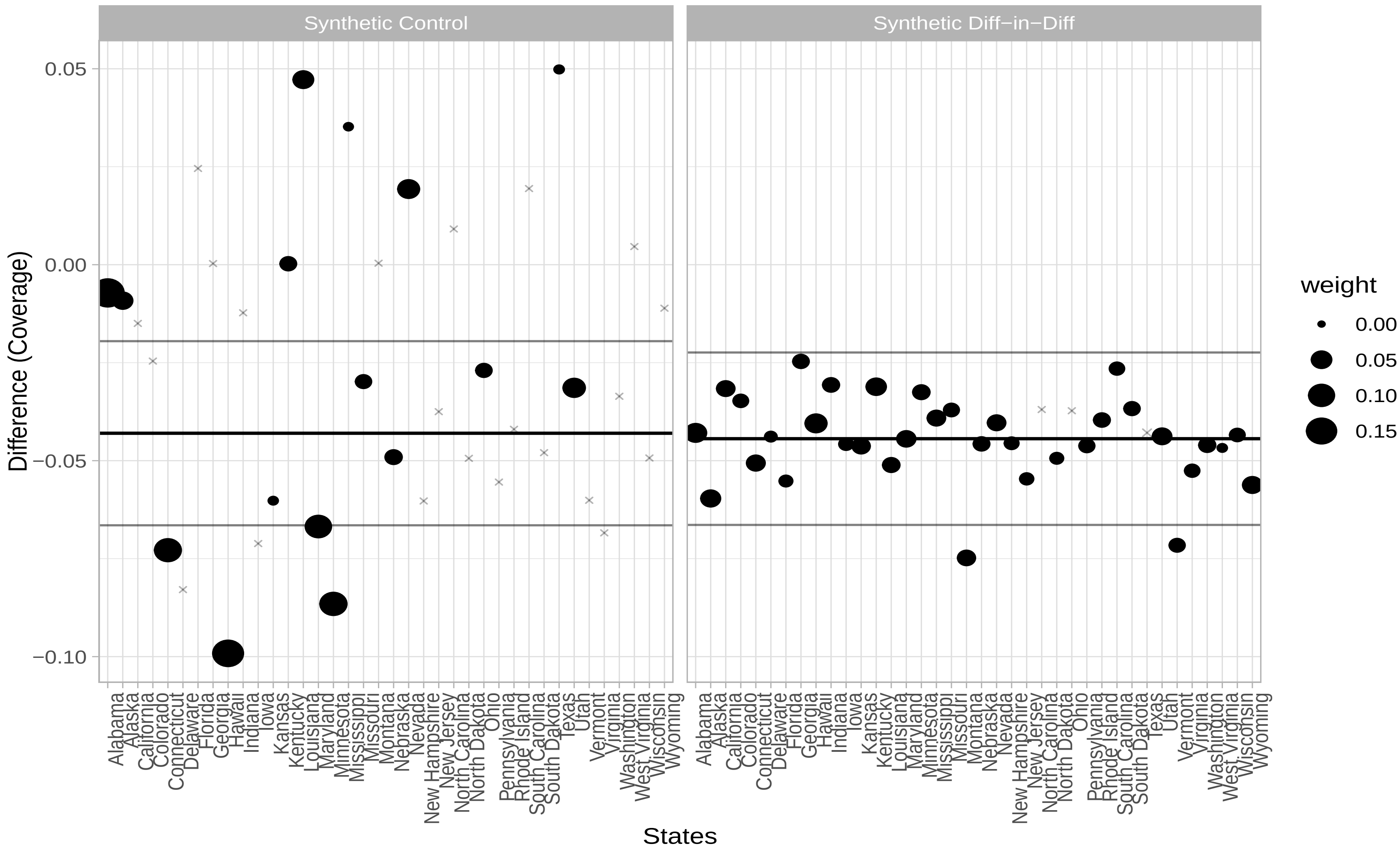


Figure A.2: Effect Distribution and Unit Weights - Body Mass Index

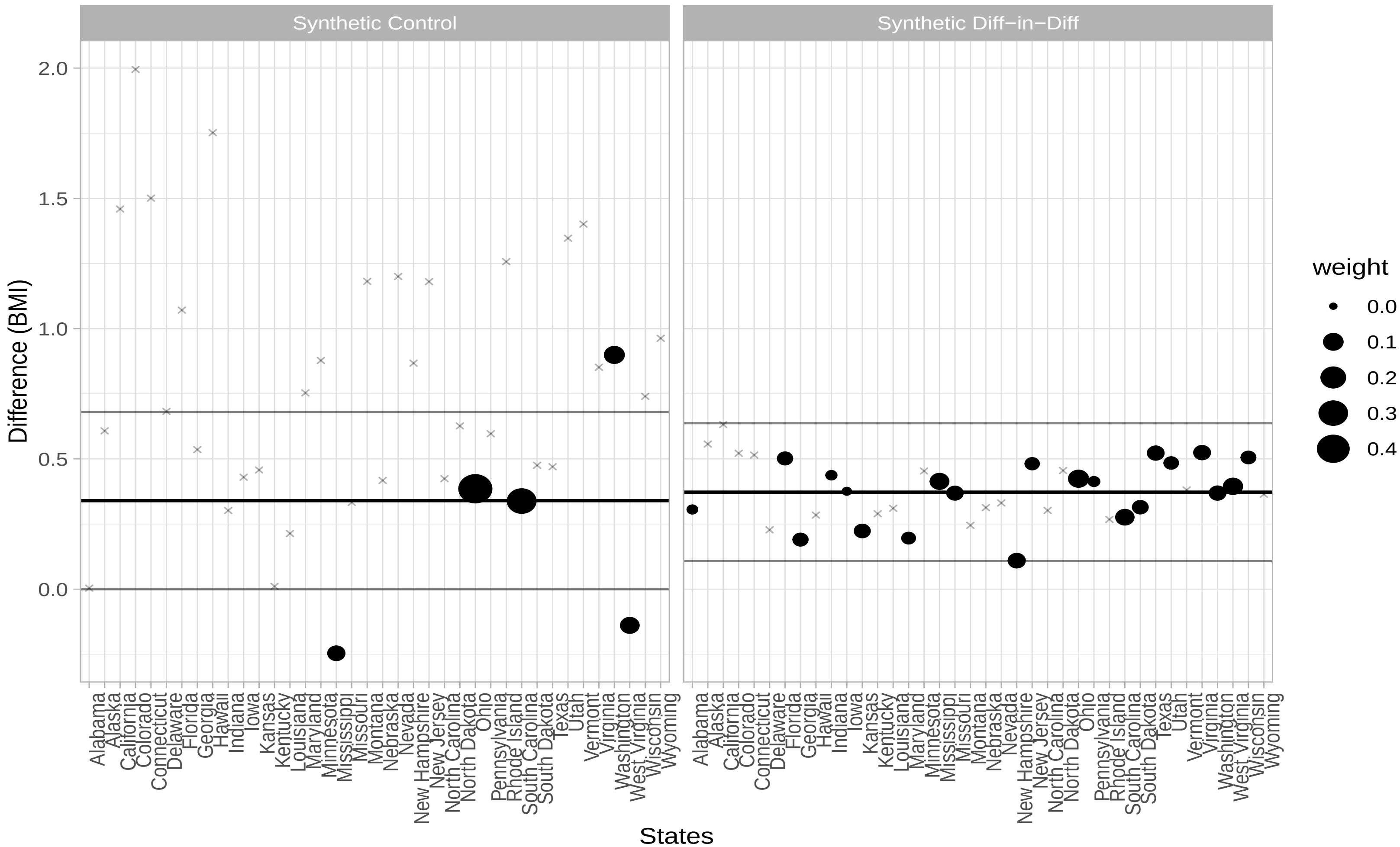


Figure A.3: Effect Distribution and Unit Weights - Obesity Prevalence

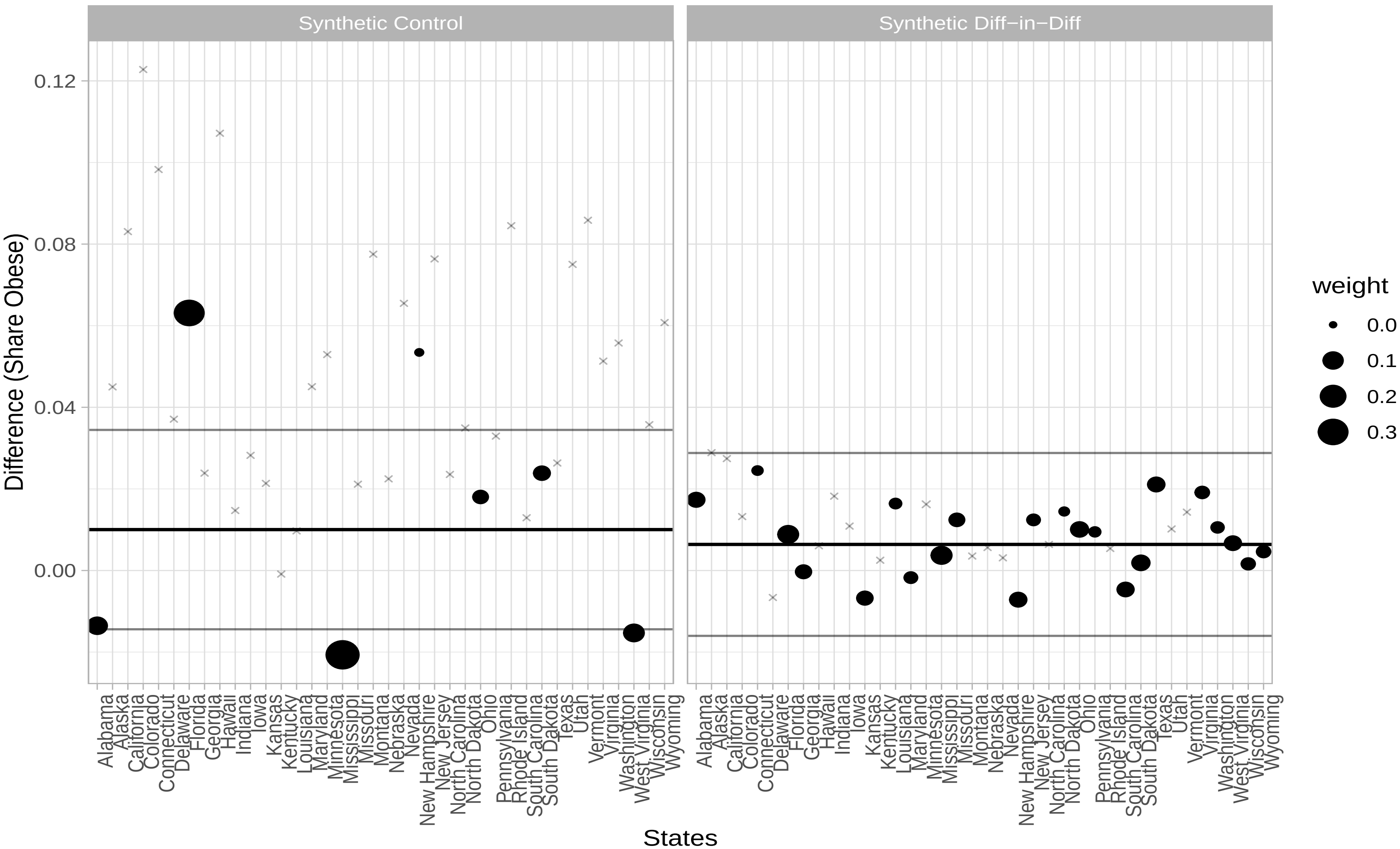
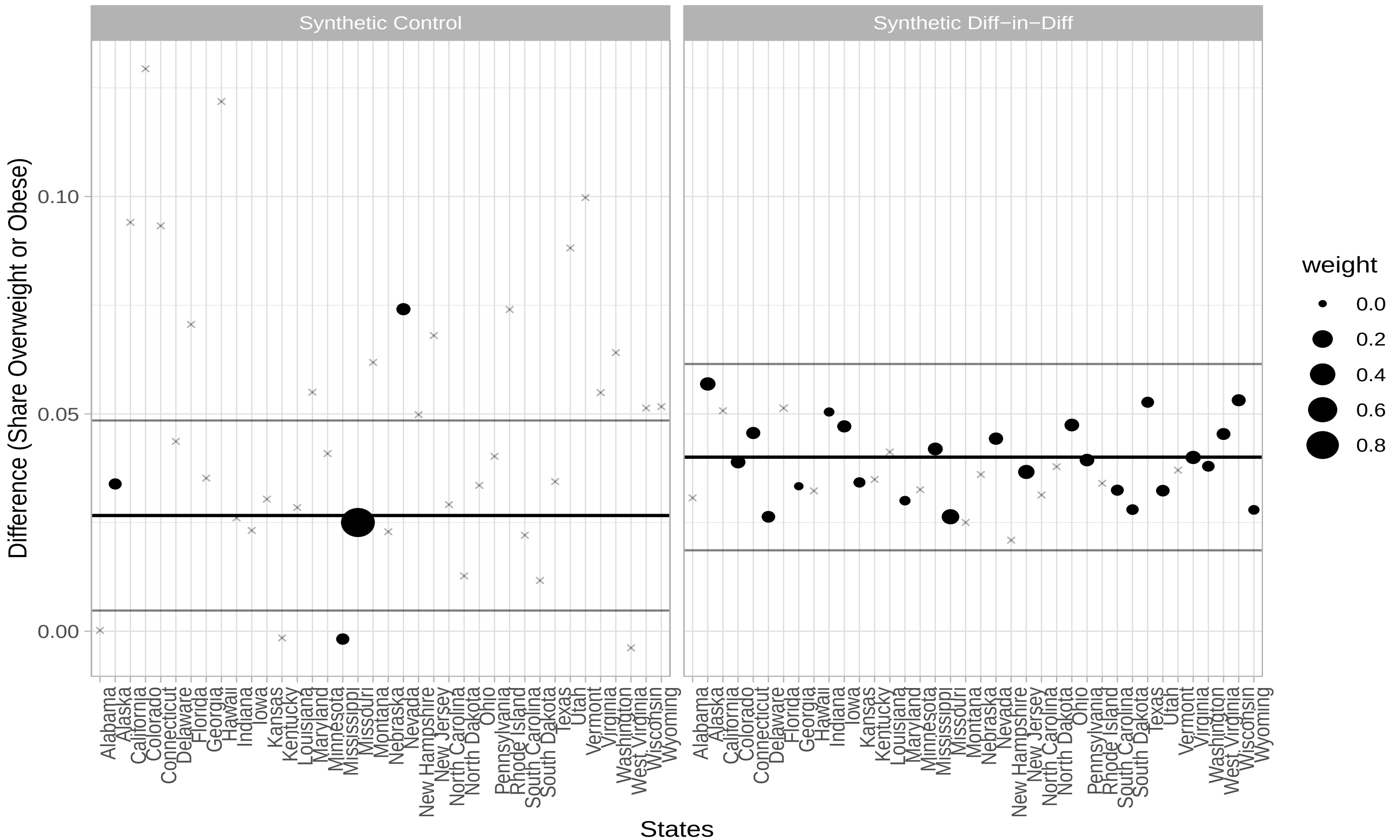


Figure A.4: Effect Distribution and Unit Weights - Overweight or Obese Prevalence



B Further Robustness and Sensitivity Checks

This section carries out additional robustness and sensitivity analyses. I address key questions regarding the validity of the estimates and their statistical significant.

The 2008-09 Recession.- The 2008-09 recession is unlikely to result in overestimation of the effect. The 5-quarter balanced moving average shown in figure B.1 highlights that during the recession (2008-2009) average BMI for Tennessean childless adults declined relative to potential donor units.[31] Figure 2b similarly exhibits a flattening of of BMI trend for childless Tennessean adults. Note that figure 2a exhibits a relatively larger decline in health insurance coverage for childless adults in Tennessee during the recession. And following recession, average BMI declined for these individuals [see Figure B.1]. This is consistent with the previous research findings that the effect of recession net of the effects from loss of health insurance is positive on healthy weight management practices and decreases BMI (Ruhm, 2000, 2005, 2015).

Donor Pool and Sensitivity.- The data-driven SDiD weights approaches the oracle weights and reduce bias to a negligible level under three conditions (Arkhangelsky et al., 2021).

First, the number of control units is comparable to the number of pre-intervention periods. In our case, we can reduce the number of potential donors to 20 by restricting the sample to only states that did not change Medicaid income eligibility by 20 percentage points for neither childless adults nor parents. The estimated effects for BMI and the share of overweight or obese are larger and remains significant at all conventional levels [see Panel C of Table 6].

Taking cues from Abadie et al. (2015), I estimate the effects of the 2005 reform on BMI and the overweight or obesity prevalence with very few donor states to which SC method assigned strictly positive weights [32] [see Figure B.2]. In Table B.1], I show that these sparse estimates, although show compromised pre-intervention fit, remain stable for BMI and overweight or obese prevalence.

Second, fewer post-treatment periods. The estimated effect remains similar only three post-intervention periods: 2006-2008. Note that the effect on BMI remains significant as long as I consider a study period up to the first quarter of 2008. This highlights the notion that the short term effect of losing Medicaid coverage can substantially differ from its medium to long term effects, a point shown to hold in case of Medicaid coverage expansions (Soni, 2020).

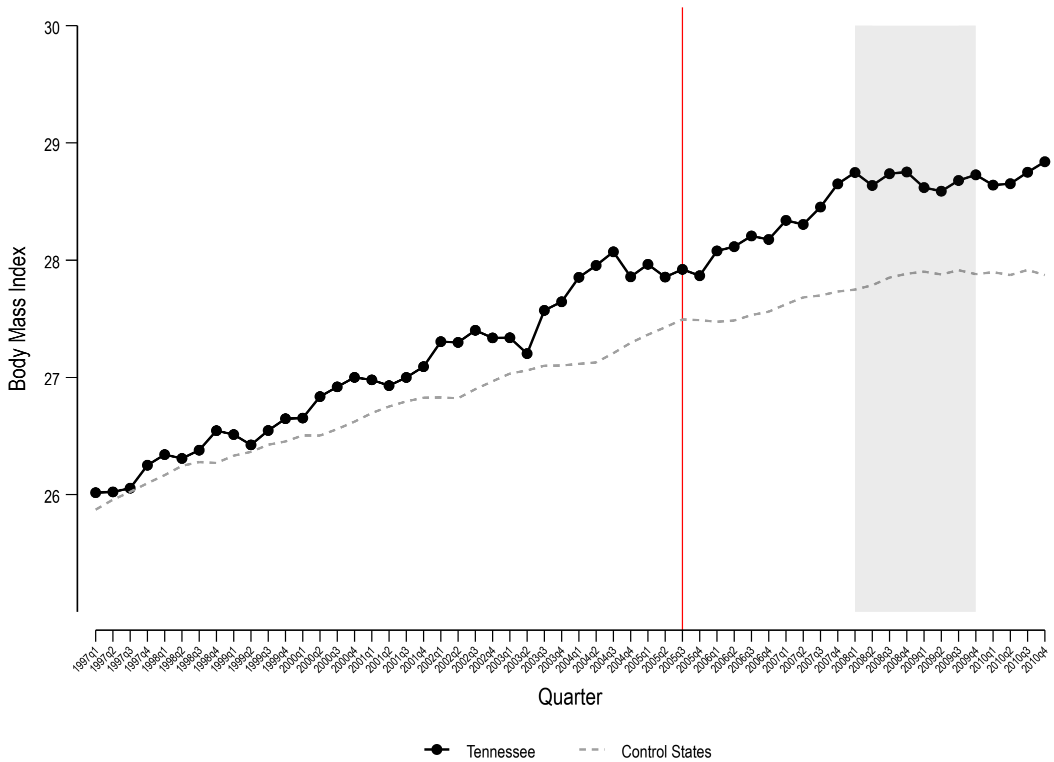
Third, the fewer the number of treated units the lower the bias. In our case, Tennessee is the only treated unit.

[31] The 5-point moving average not only theoretically reduces noise by more than one-half but also removes seasonal patterns to reveal the underlying trend.

[32] This is driven by the observation that the donor units that received strictly positive weight under the SC method also received highest weights under the SDiD method. [see Figure A.2-A.4].

Homoscedasticity and Inference.– The SDiD method relies on placebo inference, assuming homoscedastic noise distribution across Tennessee and the donor states. This assumption is particularly strong, and in application unlikely to hold due to difference in sample size across states (Ferman and Pinto, 2019). While current statistical tools do not offer heteroscedasticity-robust inference under SDiD design, I rely on rearrangement test under the DiD design to estimate the maximal relative variance of a given outcome for Tennessee up to which there is strong statistical evidence against the null (Hagemann, 2020). The test is briefly described in Appendix C.5. Column 2 of Table C.2 shows that the DiD estimate for BMI is valid at 95-percent confidence level as long as the variance of the BMI distribution for Tennessean childless adults does not exceed 3.5 times the outcome variance of all but one control state. For overweight and obese prevalence, the estimated relative heteroscedastcity is lower, however, still allows Tennessee's variability to be 2.8 times. Note that the rearrangement test rejects effects of the reform on mortality measures at all levels of relative hereoscedaticity.

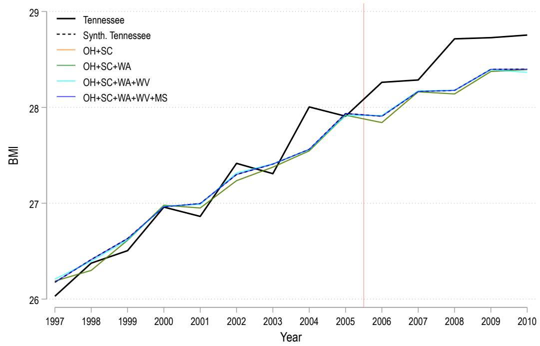
Figure B.1: Quarterly BMI Trend in Tennessee and Comparison States



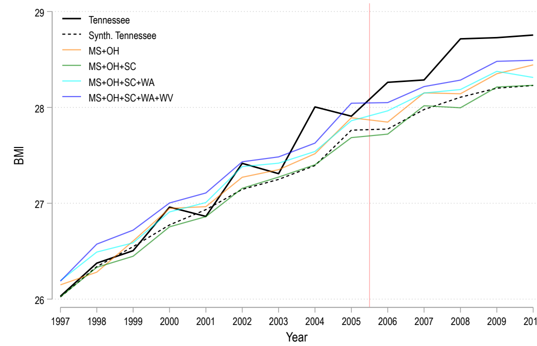
Notes.- This figure plots 5-quarter moving average of quarterly BMI for childless adults in Tennessee and potential donor states. The quarterly averages are calculated using the BRFSS sample weights. Estimates are based on BRFSS childless adult sample of age 20-64 in Tennessee and 38 potential donor states that did not change income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period. Pregnant women are excluded from this sample. Both lines present 5-quarter moving averages calculated using the following formula. For each time point, $q = t$, $BMI_{q(5)} = \frac{1}{5}\left(\sum_{q=t-2}^{t+2} BMI_q\right)$. The shaded area marks the time horizon for 2008 recession.

Figure B.2: Sensitivity - Leave One Out

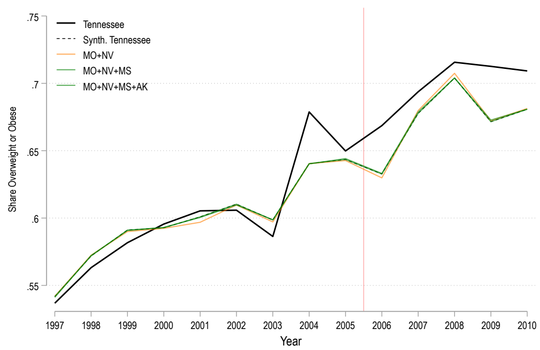
(a) Body Mass Index (SC)



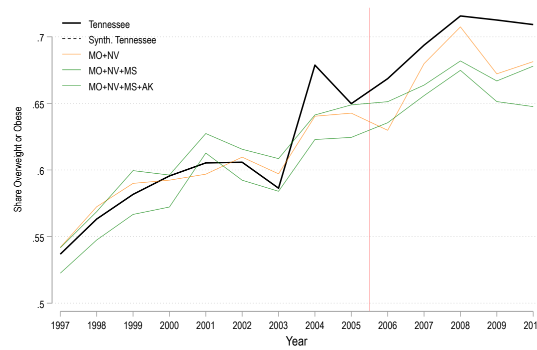
(b) Body Mass Index (SDiD)



(c) Overweight or Obese Share (SC)



(d) Overweight or Obese Share (SDiD)



Notes.- Donor states are those to which Synthetic Control method assigned positive weights [See Figure A.2 and A.4]. In the following, donor states are ordered from largest to smallest contributors to the original synthetic control. Donors are: Ohio, South Carolina, Washington, and West Virginia, and Mississippi for BMI; Missouri, Nevada, Mississippi, and Alaska for Overweight or obese share.

Table B.1: Leave-one-out Estimates

	Number of Donor States				
	5	4	3	2	1
A. Synthetic Control					
Body Mass Index	0.340	0.344	0.366	0.366	0.385
Share Overweight or Obese	.	0.027	0.026	0.026	0.025
B. Synthetic Difference-in-Differences					
Body Mass Index	0.397	0.400	0.280	0.328	-
Share Overweight or Obese	-	0.039	0.031	0.033	-

Notes.- See Figure B.2 notes.

C Methodology Appendix

C.1 Unconditional Quantile Regression

Unconditional quantile regression uses the Recentered Influence function (RIF) to estimate the effect of a policy change on a distributional statistic of the outcome variable.The RIF is simply re-centered version of an Influence function (IF), representing how much of an influence an observation has on the distributional statistic of interest. For quantile κ of the unconditional distribution of y, the IF is defined as $IF(y;q_\kappa) = (\kappa - \mathbb{I}\{y \leq q_\kappa\})/f_y(q_\kappa)$; where, $f_y(q_\kappa) = Q_\kappa[y]$ is the κth quantile, and $f_y[q_\kappa]$ represents the probability density of y at quantile κ of the unconditional distribution of y. The RIF is then constructed by adding the IF to the quantile statistic itself: $RIF(y;q_\kappa) = q_\kappa + IF(y;q_\kappa)$. The effect of TennCare disenrollment is then estimated by regressing the $RIF(y;q_\kappa)$ on the policy variable.

$$RIF(y_{ijt};q_\kappa) = \mu^{uqr} + \alpha_j^{uqr} + \beta_t^{uqr} + \tau_\kappa \cdot D_{jt} + \nu_{ijt}^{uqr} \tag{3}$$

Where, ijt indexes individual i in state j surveyed in year t. The right hand side terms preserve notation from equation 1.

In case of Unconditional Quantile estimates for a single treated cluster, available econometric tools do not facilitate estimation of consistent standard errors. Following suggestions provided by Angrist and Pischke (2009), I report standard error estimates using cluster robust (at the state level), heteroscedasticity consistent (HC2), and bootstrap methods.

C.2 SDiD Unit and Time Weights

Let $j = 1$ index Tennessee, and $t = \{1, 2, ..., T_{pre}, T_{pre} + 1, ..., T\}$ index time. The optimal state-specific weights (ω_j) minimize

$$(\hat{\omega}_0, \hat{\omega}) = \underset{\omega_0 \in \mathbb{R}, \omega \in \Omega}{\arg\min} \sum_{t=1}^{T_{pre}} \left(\omega_0 + \sum_{j=2}^{N} \omega_j y_{jt} - y_{1t} \right)^2 + \zeta^2 T_{pre} ||\omega||_2^2 \tag{4}$$

where, $\Omega = \left\{ \omega \in \mathbb{R}_+^N, \;\; with \;\; \sum_{j=2}^{N} \omega_j = 1 \;\; and \;\; \omega_j \geq 0 \right\}$. It follows that the weights are chosen to minimize the difference between Tennessee and the comparison states up to a constant. The unit-weight calculation for SDiD differs from SC in two key ways. First, the intercept term, ω_0, allows the weighted average of the donor units to match the pre-intervention treated outcomes with difference up to a constant. Second, the ridge penalty term, including $||\omega||_2^2$, ensures that the weights are not as sparse as in Synthetic control method, allowing more donors to contribute to the counterfactual. Intuitively, this may prevent the model from over-fitting the noise in treated state's outcome series since the counterfactual do not rely on very few donors. The role of time weights then become apparent in removing the bias due to divergence of pre-intervention trend, if any remains after the unit-weights are applied.

Let T_{post} denote the number of post-intervention periods. The year-specific time weights minimize the following expression

$$(\hat{\lambda}_0, \hat{\lambda}) = \underset{\lambda_0 \in \mathbb{R}, \lambda \in \Lambda}{\arg\min} \sum_{j=2}^{N} \left(\lambda_0 + \sum_{t=1}^{T_{pre}} \lambda_t y_{jt} - \frac{1}{T_{post}} \sum_{t=T_{pre}+1}^{T} y_{jt} \right) + \xi^2 T_{pre} ||\lambda||_2^2 \tag{5}$$

where, $\Lambda = \left\{ \lambda \in \mathbb{R}_+^T, \;\; with \;\; \sum_{t=1}^{T_{pre}} \lambda_t = 1 \;\; and \;\; \lambda_t = \frac{1}{T_{pre}} \forall t > T_{pre} \right\}$. It follows that post-intervention periods are given equal weights, and the pre-treatment period weights are distributed to minimize the difference between pre- and post-period outcomes up to a constant across the potential donors. Intuitively, pre-treatment years relatively more predictive of post-intervention average outcomes than others are assigned higher weights. The SC counterpart of these weights are weights attached to pre-treatment outcomes included in the model as predictors.

C.3 Synthetic Difference-in-Differences with Covariates

Although SDiD can correct for covariates even if they are not explicitly included in the model, time varying covariates can be incorporated similar to how they are incorporated in the DiD setting. Arkhangelsky et al. (2021) proposes to regress Y_{jt} on the vector of covariates $\mathbf{X}_{jt}$ and using the estimated coefficients ($\hat{\beta}$) to residualize the outcome: $Y_{jt}^{res} = Y_{jt} - \mathbf{X}_{jt}\hat{\beta}$. The Y_{jt}^{res} $\forall j = 1, ..., N$

are then used as data to estimate $\hat{\tau}^{sdid}$. Apart from practical issues in adjusting for covariates in this way,[33] Kranz (2022) shows that when $\mathbf{X}$ is correlated with time and group the proposed residualization may result in incorrect adjustments. Kranz (2022) proposes a simple alternative covariate adjustment that regresses Y_{jt} on the covariates as well as time and group fixed effects over the sample excluding treated outcomes.[34] Then using Y_{jt}^{adj}, where $Y_{jt}^{adj} = Y_{it} - \mathbf{X}_{jt}\hat{\beta}^{adj}$, the SDiD estimate is calculated from equation 2. In panel B of Table 6, I estimate the SDiD coefficients using this alternative method.

C.4 Bias-Corrected Synthetic Control

The SC method developed by Abadie et al. (2010) is different from SDiD in two primary ways. This is illustrated by comparing the expressions each method minimizes. The SC estimates solve

$$(\hat{\tau}_{sc}, \hat{\mu}, \hat{\beta}) = \underset{\tau,\mu,\beta}{\arg\min} \left\{ \sum_{j=1}^{N} \sum_{t=1}^{T} (y_{jt} - \mu - \beta_t - D_{jt}\tau)\hat{\omega}_j^{sc} \right\} \tag{6}$$

Compared to expression 2, 6 does not contain unit fixed effects (α_j) and time weights (λ_t). The former drives the requirement that the pre-treatment outcome series of the treated state must lie in the convex hull of the donor series. The missing time weights make stronger requirement for the pre-treatment fit of the synthetic unit to closely match that of the treated unit. Furthermore, the pre-treatment fit is crucial in the SC context for at least three reasons. First, it serves as a measure for the credibility of the counterfactual (Abadie et al., 2010, 2015). Second, in application, poor pre-treatment fit can lead to wrong inference. This is because SC relies on placebo inference procedure that derives p-values from ranking post- over pre-Root Mean Squared Prediction Error (MSPE) ratio for all units.[35] It follows that if pre-treatment fit is poor for the treated state, the pre-MSPE will be higher than placebos with relatively better fit. The treatment effect in this case has to be sufficiently large to account pre-treatment mismatches. For example, consider figure C.1a. The synthetic Tennessee constructed using SC weights deviates from the outcome series of Tennessee in most pre-treatment years. The pre-RMSPE for Tennessee is 0.175, and the minimum pre-RMSPE across all 39 states is 0.0019. For the test to achieve a p-value of (1/39) 0.026, the post-RMSPE must be infinitely larger than that of any state with pre-RMSE close to the minimum. Moreover, the pre-MSPE for Tennessee is ∼70 percent higher than the Median (0.108). This leads to the third reason–SC is not well suited to detect small effects, especially if the outcome variable of interest is

[33] See Clarke et al. (2023) for details.

[34] $Y_{jt} = \mathbf{X}_{jt}\beta^{adj} + \gamma_j + \rho_t + u_{jt} \qquad \forall D_{jt} \neq 1$

[35] RMSPE Ratio = $\frac{Post-RMSPE}{Pre-RMSPE}$

relatively volatile (Abadie, 2021).

In case of poor pre-treatment fit, Abadie and L'hour (2021) proposes a simple bias correction method that relies on matching on predictors.[36] Intuitively, given predictors including pre-intervention outcomes, we want to correct for the bias resulting from pre-treatment mismatch in predictor values between Tennessee and synthetic Tennessee. Denote the vector of predictors by $\mathbf{X}$, and let $j = 1$ index Tennessee. Estimate $\hat{m}_{0t}(\mathbf{x}) = \mathbb{E}[y|D = 0, \mathbf{X} = \mathbf{x}]$ by regressing y_{jt} over $\mathbf{X}_{jt}$ for all $j = \{2, ..., N\}$ $\;and\;$ $t = \{T_{pre} + 1, ..., T\}$. The bias at period t can then be expressed as $\sum_{j=2}^{N} \hat{\omega}_j^{sc} \hat{m}_{0t}(\mathbf{x}_j) - \hat{m}_{0t}(\mathbf{x}_1)$. This is simply correcting for the deviations in the predicted synthetic outcome from the predicted outcome for the treated state at period t. The bias correction can be mathematically expressed as follows

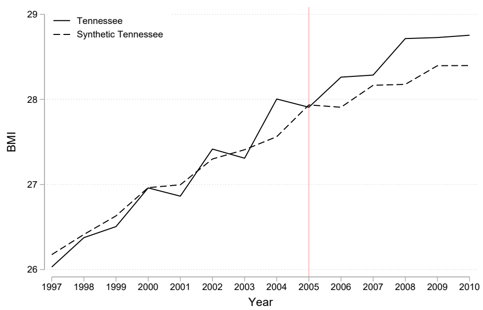
$$
\begin{aligned}
\hat{\tau}_{1t}^{bc} &= \hat{\tau}_{1t} - bias_t \\
&= (y_{1t} - \hat{m}_{0t}(\mathbf{x}_1)) - \sum_{j=2}^{N} \omega_j^{sc} \left(y_{jt} - \hat{m}_{0t}(\mathbf{x}_j) \right)
\end{aligned} \tag{7}
$$

I implement the bias-corrected SC to estimate the effect of 2005 TennCare disenrollment on body weight outcomes. The predictor vector, $\mathbf{X}$, includes all pre-intervention outcomes. Figure C.1a presents the classic SC fit for Tennessee with pre-intervention outcomes. Given the arguments above, one can deduce that the imperfect pre-treatment fit is not ideal to draw inference upon. The bias corrected version [in figure C.1b] shows perfect fit for the pre-reform values serving as predictors. The placebo estimates [C.1d] suggest that for the initial year following the disenrollment, the bias corrected BMI growth has been highest for Tennessee increased at a lower rate since then. A similar depiction is observed for overweight and obese share.

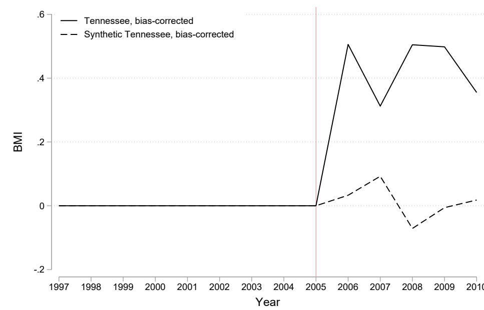
[36] I also estimate the bias-corrected SC with lasso as well as elastic net penalty (a linear combination of both lasso and ridge penalty terms). The results remain identical.

Figure C.1: Body Mass Index

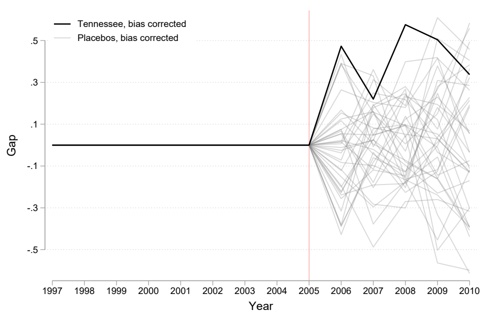
(a) Classic SC



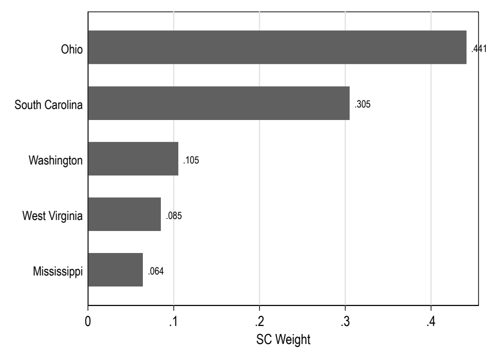
(b) Bias Corrected SC



(c) Bias Corrected SC Placebo Effects



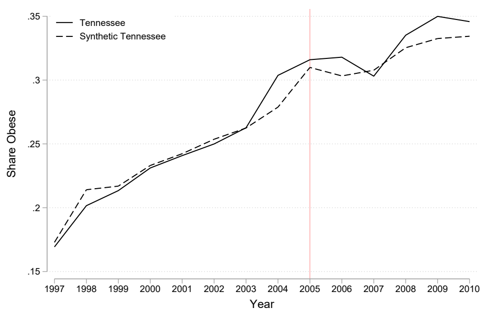
(d) SC Unit Weights



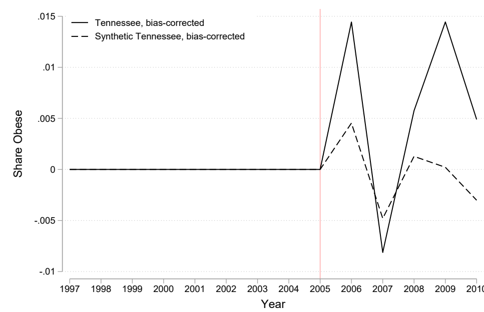
Notes.- Estimates are based on aggregate data generated using BRFSS sample weights and childless adult sample of age 20-64 in Tennessee and 36 potential donor states that did not change Medicaid income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period [see footnote 15]. Pregnant women are excluded. The predictor vector for Figure (a)-(c) include all pre-intervention outcomes only. Figure (d) presents placebo graph for specification with 7 pre-intervention outcomes and demographic predictors—share of adults with health insurance, employment share, average age, and the share of adults female, with high school diploma, and college degree.

Figure C.2: Obesity Prevalence

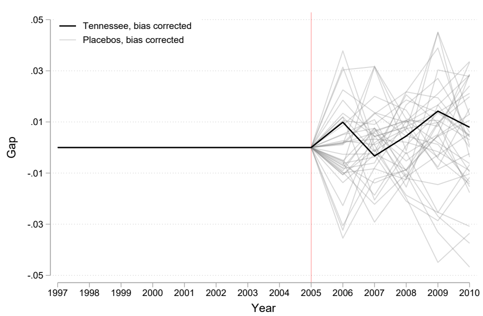
(a) Classic SC



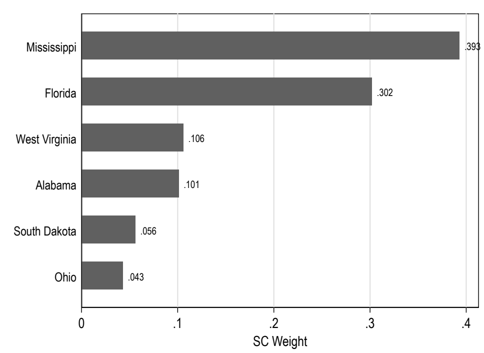
(b) Bias Corrected SC



(c) Bias Corrected SC Placebo Effects



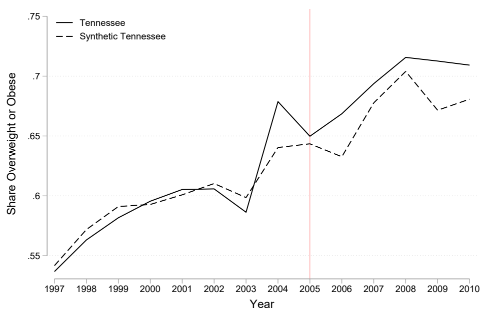
(d) SC Unit Weights



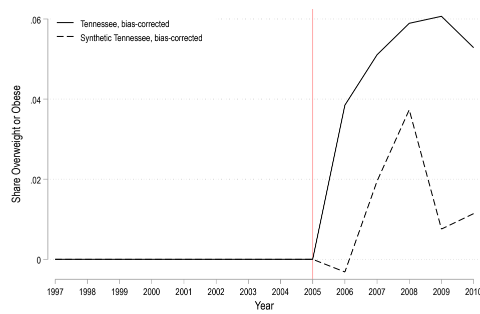
Notes.- See Figure C.1 notes.

Figure C.3: Overweight or Obesity Prevalence

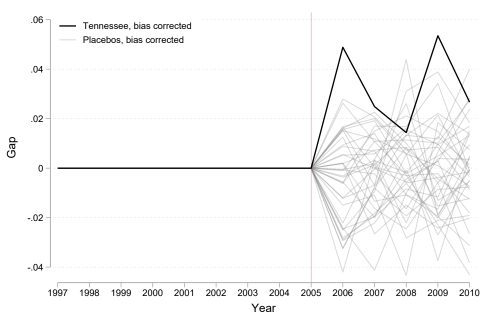
(a) Classic SC



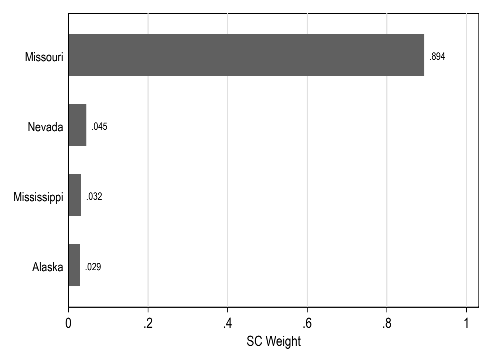
(b) Bias Corrected SC



(c) Bias Corrected SC Placebo Effects



(d) SC Unit Weights



Notes.- See Figure C.1 notes.

Table C.1: Bias-Corrected Synthetic Control Estimates

	Body Weight Outcomes			Mortality (Deaths per 100,000)		
	BMI (1)	Obese (2)	Overweight or Obese (3)	All-cause (4)	Amenable Cause (5)	Non-Amenable Cause (6)
ITT	0.422 [0.026]	0.007 [0.385]	0.034 [0.026]	-14.525 [0.136]	-3.960 [0.409]	-7.684 [0.500]

Notes.- P-values are reported in the brackets. Estimates in column (1)-(3) are based on aggregate data generated using BRFSS sample weights and childless adult sample of age 20-64 in Tennessee and 36 potential donor states that did not change income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period [see footnote 15]. Pregnant women are excluded. The mortality estimates in column (4)-(6) are based on state×year level data generated from state×year×cause-of-death level mortality rate (deaths per 100,000) for adults of age 20-64 in Tennessee and other 20 potential donor states that did not change income eligibility threshold for childless adults and parents by 20 percentage points of FPL or more in any year during the study period [see footnote **??**].

C.5 Rearrangement Test

The Rearrangement Test (RT) under DiD framework relies on the joint normality of the 'post' estimators estimated using $N_0 + 1$ regressions specified below.

$$y_{jt} = \alpha_0 + \beta_j \cdot \mathbb{I}\{t \geq 2006\} + \eta_{jt} \quad \forall j = 1, ..., N \tag{8}$$

Let $j = \{1\}$ index Tennessee and $j = \{2, ..., N\}$ index control states. The weighted means within state-year cells serve as inputs for estimation of this equation. The test then considers the ordered vector of $\hat{\beta}_j$'s as data and compare to its permutations attaching special weights that provide precise probabilistic control over the size of the test. Inference method remains the same as described in Algorithm 3.3 of Hagemann (2020).

Importantly, RT allows heteroscedasticity of unknown form by allowing the treated state to be boundlessly more variable than all but one control states until the null can no longer be rejected.

Table C.2: Rearrangement Test Estimates for 2005 TennCare Disenrollment

		Body Weight			Deaths per 100,000		
	Have Health Insurance? (1)	Body Mass Index (2)	Obese [BMI≥30] (3)	Overweight or Obese [BMI≥25] (4)	All-Cause (5)	Amenable Cause (6)	Non-amenable Cause (7)
ITT	-0.041	0.409	0.016	0.029	7.936	7.124	0.812
$\varrho_{0.10}$	2.68	2.48	-	2.18	-	-	-
$\varrho_{0.05}$	2.08	1.88	-	1.68	-	-	-

Notes.- ϱ_α is the estimated maximal relative heteroscedasticity at α level of significance. Estimates in column (1)-(3) are based on aggregate data generated using BRFSS sample weights and childless adult sample of age 20-64 in Tennessee and 36 control states that did not change income eligibility threshold for childless adults by 20 percentage points of FPL or more in any year during the study period [see footnote 15]. Pregnant women are excluded. The mortality estimates in column (4)-(6) are based on state×year level data generated from state×year×cause-of-death level mortality rate (deaths per 100,000) for adults of age 20-64 in Tennessee and other 19 potential donor states that did not change income eligibility threshold for childless adults and parents by 20 percentage points of FPL or more in any year during the study period [see footnote ??].