Chronicling the Loss of Public Health Insurance: Evidence from the TennCare Disenrollees

Sam Arenberg University of Houston
Sam Stripling

Once considered a model for national health insurance reform, Tennessee's Medicaid program, TennCare, later experienced one of the largest cuts to public health insurance in US history. The severe and sudden curtailing of Medicaid eligibility for a vulnerable population may have led to both short and long-term impacts to their health insurance, labor market, and health outcomes. Although the "TennCare disenrollment" has been studied in the past, never has the set of disenrollees been specified and tracked over a long period of time, as we do in this paper by linking administrative enrollment records from CMS to large household surveys from the Census Bureau. Our unique dataset allows us to pinpoint individuals who lost TennCare eligibility and trace their long-term outcomes. We first show that nearly half of the approximately 170,000 TennCare disenrollees were still uninsured several years after the disenrollment. Although many disenrollees obtained insurance through an employer, we do not find increases in labor force participation or employment among the disenrolled. We do find, however, evidence that employees increased hours worked, ostensibly to qualify for health insurance with their employer. Finally, we consider the health outcomes of the disenrollees, namely self-reported disability and mortality. This investigation yields mixed results. While disability shows a null result, we find statistically significant impacts on long-run mortality. Upon a more comprehensive analysis of public health insurance on mortality, however, we refrain from a determination, as our work warrants additional research into the relationship between public health insurance and health. Collectively, our results are largely consistent with the experimental literature but partially contrast with the quasi-experimental literature on the impacts of health insurance. Our findings come at a time when Medicaid faces budgetary challenges, and we conclude our paper by discussing several lessons from the TennCare experience that may inform current policy discussions. We also emphasize that our template can be applied to study other changes in access to Medicaid, such as the unwinding of pandemic provisions or large future Medicaid cuts, should they occur.

Disclaimer: *Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2603. (CBDRB-FY21-P2603-R09240, CBDRB-FY25-P2603-R11872/12193)*

1. Introduction

A pillar of the social safety net in the United States is the Medicaid program. Created by the Johnson administration in 1965, Medicaid initially provided low-cost health insurance to the neediest Americans, but it would soon become the largest means-tested program in the country, and, in 2023, enrollment surpassed 100 million people as spending approached one trillion dollars. The expansions that spurred this precipitous growth have been studied extensively, revealing large benefits that extend well beyond the

financial and health outcomes of recipients (Buchmueller et al., 2015; Currie & Duque, 2019). These impacts place Medicaid, despite its price tag, potentially among the most cost-effective programs in the United States today (Hendren & Sprung-Keyser, 2020).

Although the history of the program is marked by rapid growth, its future may be marked instead by dramatic cuts. In the past year alone, more than 25 million Americans have been dropped from the program as provisions related to the waning pandemic unwind (Government Accountability Office, 2025). The latest reconciliation bill proposes massive cuts to the Federal outlays on which Medicaid relies, and the Congressional Budget Office estimates suggest proposed Medicaid cuts could result in millions of uninsured (Gaffney et al., 2025; Kaiser Family Foundation, 2025). The possibility that millions of Americans, many of whom have accessed the healthcare system for years through Medicaid, could lose their health insurance is a harbinger for a new policy climate. Yet, our ability to predict what would happen in the wake of such an event is limited. Research on contractions of public health insurance is less studied than expansions due to the infrequency of contractions and the inability to monitor the outcomes of those disenrolled when they do occur.

To better understand the potential ramifications, this paper studies what was, at the time, the largest disenrollment from public health insurance in US history. In 2005, the state of Tennessee suddenly and swiftly cut from its Medicaid rolls four percent of the state's entire working-age population (Garthwaite et al., 2014). We will, for the first time, locate these individuals in administrative Medicaid records and link them to a host of other person-level data held by the Census Bureau. The linkages provide, among other things, detailed information on insurance status, employment outcomes, and even mortality over a long time horizon. As we chronicle the experiences of the disenrollees in this unique data environment, we will fill in several unknown gaps about how a large contraction of public health insurance might unfold.

We first characterize the disenrollment itself in new detail. In the Medicaid data, we identify approximately 170,000 people who qualified for TennCare under conditions that would ultimately be revoked. We note that these individuals had a long history with TennCare. Nearly two-thirds had been enrolled continuously since 2000, when our data begin. This figure gives important context. The affected individuals were not just newly or loosely attached to Medicaid when they lost coverage in the middle of 2005. We also note that about 30,000 of the 170,000 were kept on Medicaid rolls without interruption through other bases of eligibility, mostly maintaining coverage through bases of eligibility related to poverty or disability. We confirm that these re-enrollees were both poorer and less healthy than those who did not remain on the Medicaid rolls. This fact serves as a reminder that people are not disenrolled from Medicaid; rather, bases of eligibility are discontinued. Many people with those expiring bases will find other avenues to coverage, and, depending on the nature of future cuts, they could be the most vulnerable beneficiaries.

We also report on the insurance status of the disenrollees long after their disenrollment. We find critically that almost half of them remained uninsured several years later in 2008, well above the uninsured rates of a comparison group of similar Medicaid waiver recipients in other states. This estimate, related to "crowd-out," contributes to a large but mixed literature on the degree to which public insurance supplants private insurance rather than prevents uninsurance, a literature that is built almost exclusively on expansions of

public insurance rather than contractions (Buchmueller et al., 2015; Gruber & Simon, 2008). We find further that the uninsured half were in substantially worse health compared to those who obtained private health insurance. This difference reveals that those who needed insurance the most were least likely to acquire it when left to their own devices. We also show that the uninsured half would not see major increases in insurance coverage until the ACA Medicaid expansions came into effect in 2014.[1] Although many disenrollees have now aged onto Medicare, some TennCare disenrollees remained uninsured through the pandemic and into today.

We then address the employment outcomes of the disenrollees. Because the United States is unique in that many of its citizens obtain health insurance though their employer, an important policy question is whether disenrollees, suddenly needing insurance, seek new or different employment. We link administrative enrollment records from CMS to household surveys from the Census Bureau containing information on labor market status to assess this possibility. Plotting the employment outcomes of disenrollees over time, we find no evidence that disenrollees increased their employment or labor force participation rates. We find similar null effects when we compare TennCare disenrollees to other Tennesseans or Medicaid recipients in other states. Previous research has assessed "job lock" resulting from the TennCare disenrollment. While some papers estimate large labor market effects (Garthwaite et al., 2014), others question findings based on the use of aggregate public data (Ham & Ueda, 2021). Our estimates of job lock using linked administrative data rule out large changes in employment or labor force participation by TennCare disenrollees. However, we find some evidence that non-self-employed disenrollees may increase their hours worked in order to attain health insurance.

Finally, we consider the health of disenrollees, focusing on disability and mortality. Using a differences-in-differences approach, we fail to find that the TennCare disenrollment increased self-reported disability in either the short-run or long-run. We then consider whether Medicaid eligibility impacts mortality. Mortality is an exceptionally difficult outcome to study, but it is a uniquely important one. Even a modest effect on all-cause mortality would fundamentally alter the welfare implications of public health insurance. To study mortality, we link the disenrollees to their date of death, if deceased, using complete death records collected by the Social Security Administration and shared with the Census Bureau. We study the impact of TennCare disenrollment on mortality in both the short and long-run (two, five, and ten-year horizons). We find null or small effects in the short-run, but we find large and statistically significant effects in the long-run. However, we exhibit great caution in interpreting these results, which may be driven by unobserved heterogeneity in health profiles, and, upon further investigations of the impact of public health insurance on mortality, we refrain from a clear-cut conclusion on the causal impact of the TennCare disenrollment on mortality. Our results cast uncertainty on the pathways through which and the degree to which public health insurance affects mortality.

2. Background

On January 1, 2014, the major provisions of the Affordable Care Act were implemented, including the

[1] Notably, this increase was observed despite Tennessee remaining a non-expansion state.

ACA Medicaid expansion, which provided health insurance coverage to millions of Americans. Ten years after the implementation of the major ACA provisions, the uninsured rate in the United States was the lowest it had ever been (Council of Economic Advisers, 2025).

On January 1, 1994, exactly 20 years before the implementation of the major ACA provisions, Tennessee created TennCare, a public health insurance program that provided Medicaid coverage to Tennesseans (Bonnyman & Garr, 2014). TennCare expanded Medicaid eligibility through managed care plans to working-age adults, many of whom would not have been eligible for public health insurance or able to afford health insurance otherwise. Ten years after the implementation of TennCare, Tennessee boasted one of the highest rates of Medicaid coverage for working-age adults (Kaiser Family Foundation, 2019). Created during a period when universal health care for all Americans was hotly debated, TennCare was viewed by some policymakers as a steppingstone to national healthcare reform. Former President Bill Clinton later claimed, "Ultimately, I believe the nation will have a medical coverage plan that will look like TennCare" (Martin, 2011). In a stunning reversal, however, in 2005, due to budgetary pressures, Tennessee rapidly cut TennCare eligibility. The TennCare disenrollment became, what was at the time, the single largest contraction of public health insurance in US history.

Tennessee created the TennCare state Medicaid program in January 1994, shortly after receiving approval for a federal Section 1115 waiver from the Center for Medicare and Medicaid Services that provided generous federal matching funds (Bonnyman & Garr, 2014). Federal approval of the Section 1115 waiver allowed Tennessee to create a Medicaid managed care program with the goals of controlling costs and broadening Medicaid coverage. Compared to traditional Medicaid recipients, TennCare expansion recipients were more likely to be working age, childless, non-disabled, and have incomes above 100 percent of the Federal Poverty Level (FPL). TennCare also provided Medicaid coverage to Tennesseans deemed "uninsured" or "uninsurable" and subsidized insurance premiums and cost-sharing for individuals above the FPL. To some extent, there are important similarities of the population affected by TennCare expansions to those who gained coverage with the more recent Affordable Care Act Expansion. Both populations were more likely to be working age, non-disabled, and higher income than traditional Medicaid populations.[2]

Amidst a state debate on whether to implement a state income tax or reduce government spending, Tennessee decided to cut spending by drastically reducing its TennCare program (Bonnyman & Garr, 2014).[3] Tennessee sought and received federal approval to discontinue Medicaid eligibility that was previously granted through its Section 1115 waiver (Farrar et al., 2007). Beginning in the third quarter of 2005, Tennessee disenrolled an estimated 170,000 individuals from TennCare over a period of several months, many of whom where in the uninsured and uninsurable categories of eligibility (Chang & Steinberg, 2014; Garthwaite et al., 2014). Approximately 3 percent of the population of Tennessee abruptly lost Medicaid eligibility, which made the TennCare disenrollment, at the time, the largest increase in the

[2] See Garthwaite et al (2014) for a detailed background of TennCare and comparison of the TennCare population to populations affected by more recent public health insurance expansions such as the Affordable Care Act.

[3] Concurrently, Tennessee faced a reduction in federal Medicaid matching rates (Bonnyman & Garr, 2015).

number of uninsured in US history (Bonnyman, 2006; Farrar et al., 2007).

The sharp cut in TennCare eligibility provides a candidate natural experiment for studying the impact of health insurance. Garthwaite et al. (2014) advanced the study of health insurance by using the TennCare disenrollment to estimate the impact of public health insurance on labor supply. Economists have since studied the impact of TennCare disenrollment on evictions, crime, credit risk, labor, healthcare utilization, and health outcomes (Ali et al., 2024; Argys et al., 2020; DeLiere, 2019; Deza et al., 2024; Garthwaite et al., 2018; Ham & Ueda, 2021; Nikpay, 2022; Tello-Trillo, 2021; Tello-Trillo et al., 2023). The population disenrolled from TennCare was low-income and in much worse health than the typical American.[4] The severity of health insurance cuts to a vulnerable population may have led to long-lasting impacts to their labor market and health outcomes. Our unique dataset allows us to pinpoint individuals who lost TennCare eligibility and trace their long-term outcomes across a variety of datasets.

3. Data

One of the distinguishing features of this paper is the ability to identify TennCare disenrollees. We identify them in complete, administrative enrollment records from CMS called the Medicaid Statistical Information System, or MSIS. In every month since January of 2000, we observe the state through which a given person is eligible (e.g., Tennessee), some basic demographics about that person (e.g., age and sex), the days of eligibility he or she has in the month (e.g., 31), and a set of codes indicating his or her basis of eligibility (e.g., eligible as a disabled adult). In Tennessee, we found a code for a waiver (Section 1115 waiver), first appearing in the data in January of 2005, that designates eligibility under the TennCare expansion and indicates abrupt disenrollment in the second half of 2005. With these data alone, we can assess the size and timing of the disenrollment, including possible re-enrollment of disenrollees back onto Medicaid, through alternative bases of eligibility. That is how we will begin our analysis in Section 4.1.

Another distinguishing feature of this paper is the ability to link TennCare disenrollees to other data sources in order to track key facets of their well-being in the years leading up to and following their loss of eligibility. The Census Bureau maintains the version of the MSIS we use, and it generates a single person identifier that is common across all of its products, allowing for straightforward joins between once disparate data sources.[5]

The first linkage we utilize is to the Medicare Enrollment Database, or EDB, which is the Medicare analog of MSIS. The EDB shows who obtains Medicare and when. Turning 65 is a good, but imperfect proxy for enrollment in Medicare, particularly among low-income Americans. This file gives us an accurate count of Medicare beneficiaries when we break down the insurance rates and insurance sources of disenrollees in Section 4.1. The EDB also gives us a measure of disability, as anyone who enrolls in Medicare prior to

[4] Many of the disenrollees were in the "uninsurable" eligibility category of TennCare and were previously denied private health insurance coverage due to preexisting health conditions.

[5] These data are available to qualified researchers in Federal Statistical Research Data Centers located across the country. The data are subject to strict disclosure rules to protect the privacy of respondents. Consequently, we cannot share the microdata we use, but we are happy to share our codes (within the data center infrastructure) with researchers with the necessary credentials.

age 65 is likely to have a disability. This measure is useful when we assess the baseline health of disenrollees.

The second linkage is to the American Community Survey, or ACS. The ACS is a household survey of approximately 1.5 percent of the US population each year since 2005 and of approximately 0.5 percent in each prior survey year. As we will show (and as others have shown), the number of disenrollees is close to 177,000. Therefore, in most years, we observe about 2,500 disenrollees in the ACS and, in the earliest years, about 1,000.[6] The questionnaire has detailed demographic information, which gives unprecedented detail about who was (and wasn't) disenrolled in Section 4.1. Beginning in 2008, the ACS collects health insurance status, enabling a medium and long-term follow-up on not only public insurance (from MSIS and EDB) but also private insurance coverage. This information gives a unique opportunity to estimate substitution or crowd-out, the possibility that public insurance substitutes for private insurance rather than reduces rates of uninsurance. The ACS also contains work information needed to assess job lock, powering Section 4.2, which is devoted to labor market outcomes. The ACS also features a series of questions about self-reported disabilities, for instance, difficulty going out, which we utilize in Section 4.3.

The third and final linkage is to the Social Security Numerical Identification File, or Numident. The Numident is the Social Security Administration's complete record of persons with an SSN and their dates of death, which allows us to study mortality, a ubiquitous, unambiguous measure of health, in Section 4.3.

This data structure allows us to estimate impacts of health insurance coverage with unprecedented precision.[7] Past research, by necessity, has relied on geographic variation (e.g., uninsurance rates by county) or on samples of individuals among which only a small fraction (often only 2 to 5 percent) is treated by a policy change (e.g., all adults in a state without a college degree).[8] In such designs, statistical power and reliability of estimates may be lacking (Black et al., 2021).. For mortality, however, a rare outcome even among disadvantaged groups, power remains at a premium, highlighting how exceptionally difficult it is to study mortality. Consequently, we will go beyond the TennCare disenrollment in our discussion of what can (and cannot) be learned about the causal relationship between insurance and death in Section 4.3. Throughout the results section, these linked data will also allow us to understand in the ramifications of losing public health insurance in both the short-run and long-term. In the analyses to follow, we will primarily present intent-to-treat (ITT) estimates.

4. Results

4.1. Insurance

4.1.1. Medicaid

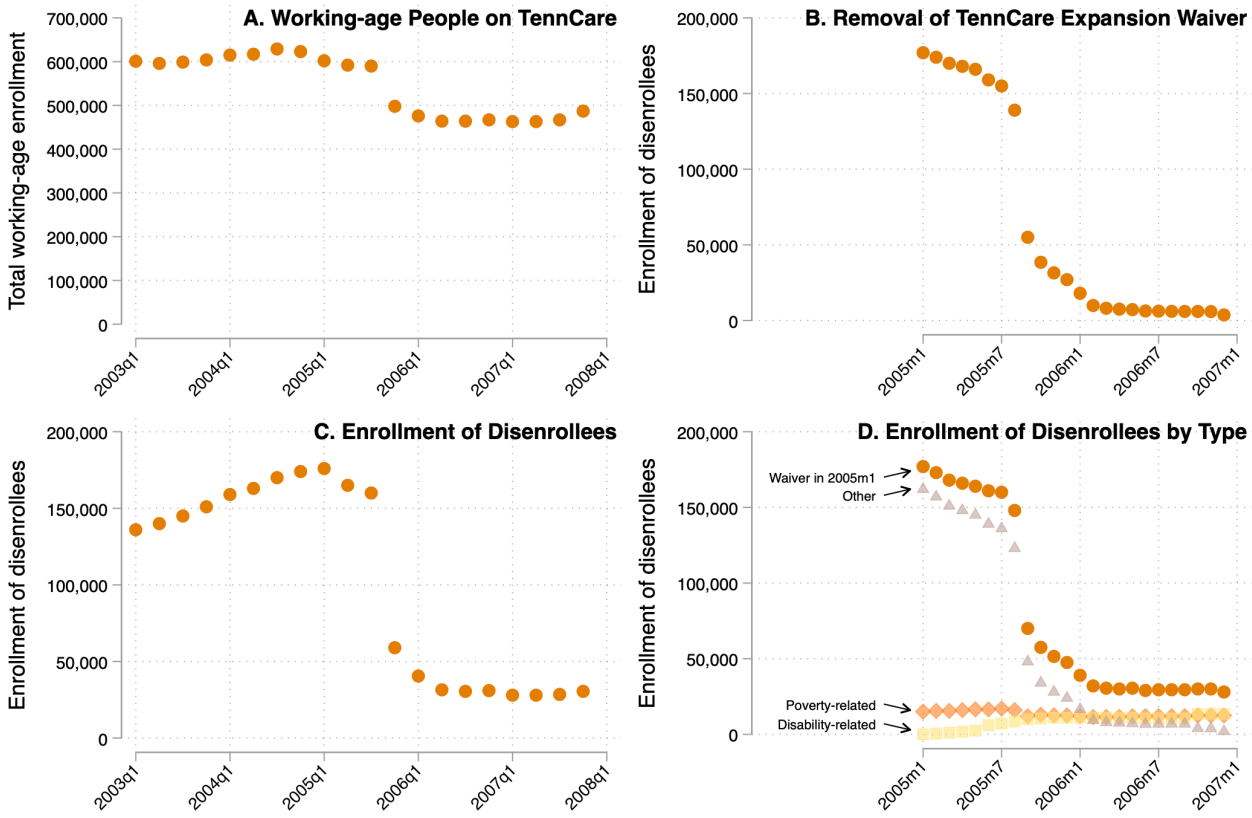
[6] Note that this forms a repeated cross-section rather than a panel.

[7] Our results are rounded to significant figures depending on sample size in accordance with RDC guidelines.

[8] Black et al. (2021) contextualizes these shortcomings through simulated power analyses, and their examples indicate the difficulty of finding samples in aggregate data that can reliably detect small changes in mortality. Miller et al. (2021) and Wyse & Meyer (2025) advance the literature on the impact of public health insurance on mortality using linked administrative data, which allows them to study samples with first stages well above 10 percent.

We begin by documenting the TennCare disenrollment itself in new detail. Figure 1 plots the sudden contraction in TennCare in several different ways. In Panel A, we show the number of working-age people (specifically, ages 21 to 64 in 2005) enrolled in Medicaid by quarter between 2003 and 2007. Enrollment among the working-age population remains stable around 600,000 prior to the disenrollment. In the last half of 2005 there is a precipitous decline in coverage of over 100,000, which we unpack in the remaining panels.

Fig. 1: TennCare Disenrollment



Panel B of Figure 1 homes in on a group of TennCare recipients who were eligible for Medicaid through the TennCare expansion. Specifically, it identifies the subset of TennCare enrollees who qualified for Medicaid under a special Section 1115 waiver. Panel B shows, in January 2005, the first month for which MSIS reports waiver status, 177,000 Tennesseans qualified for Medicaid under the Section 1115 waiver. Panel B shows that over 100,000 TennCare enrollees lost Section 1115 waiver eligibility from July 2005 to October 2005. By 2006, Section 1115 waiver eligibility had all but disappeared. The 177,000 individuals who received TennCare through the Section 1115 waiver in January 2005 we define as "disenrollees." In the analyses going forward, these disenrollees serve as our treatment group.

Panel C of Figure 1 highlights two important facts about these treated individuals. It shows the number of disenrollees eligible for TennCare in each quarter between 2003 and 2007. First, a large majority of the the TennCare disenrollees had been enrolled on TennCare for many quarters prior to the disenrollment.

136,000 of the 177,000 TennCare disenrollees (77 percent) received TennCare at the beginning of 2003, more than two years before the TennCare disenrollment took place. Although not shown in the figure, approximately 60 percent of TennCare disenrollees received TennCare as far back as the first quarter of 2001, which is the first quarter for which we have linked MSIS data. The implication is that the treated group is losing health insurance coverage on which they had relied, not for several months, but for several years. In the results to follow, we will separately estimate the impact of health insurance loss for disenrollees who previously had TennCare coverage for several years, as they may experience the greatest hardships from the sudden loss of health insurance.

The second fact seen in Panel C is that not all the TennCare "disenrollees" lost Medicaid coverage. Despite initially losing Section 1115 waiver eligibility, some of the treated group were able to regain Medicaid coverage through other bases of eligibility. Approximately 30,000, or 17 percent, of the "disenrollees" remained covered by Medicaid through 2007. Their retention serves as an important reminder about the nature of cuts. Policymakers do not directly remove individuals from coverage; their primary policy lever is to end or restrict particular bases of eligibility. Certain individuals on discontinued bases will maintain or regain coverage through other means, which leads us to the final panel in Figure 1.[9]

Panel D of Figure 1 drills down on the eligibility of treated individuals. Specifically, Panel D shows the same Medicaid coverage information as Panel C, but at the monthly level (orange dots) and by category of eligibility: poverty status, health status, and other (diamond, square, and triangle). It shows that many disenrollees retained coverage through poverty or health-related circumstances after the cut in the second half of 2005. Of the 28,000 individuals in the treated group who retained Medicaid coverage in December of 2006, 12,500 (45 percent) received coverage through health status, 13,500 (48 percent) through poverty status, and 2,000 (7 percent) from other categories. We will confirm in the next subsection that treated individuals who retained Medicaid coverage had substantially worse health than those that did not, consistent with a concerted effort to keep a vulnerable population on Medicaid (or Medicare eligibility for those under 65 years of age). Similar efforts by state administrators may prove consequential in future cuts to Medicaid.

4.1.2. Uninsurance

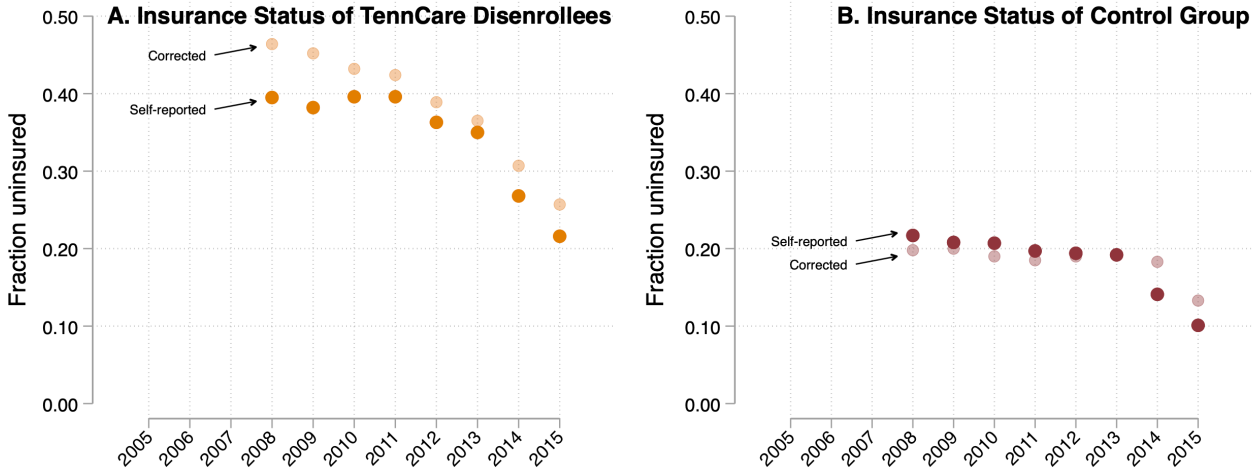
We have shown that the TennCare disenrollees lose Medicaid, but a broader question is whether they lose insurance coverage entirely. Figure 2 shows that they do.

Panel A of Figure 2 shows the fraction of TennCare disenrollees who are uninsured from 2008 to 2015. The time series begins in 2008 because that is the first year that the ACS collects health insurance status. In 2008, three years after the disenrollment, approximately 40 percent of surveyed disenrollees aged 21 to 64 report having no health insurance of any kind (private or public). Approximately 40 percent remain uninsured through 2011. Only after the major provisions of the Affordable Care Act are implemented in 2014 does the uninsured rate drop below 30 percent, suggesting that the Affordable Care Act successfully

[9] Importantly, unlike most research studying changes in public health insurance, Figure 1 highlights that we know who in our sample is treated, or lost TennCare eligibility.

met a need for a group that had been persistently uninsured (despite Tennessee not adopting the ACA Medicaid expansion).

Fig. 2: Insurance Status



These uninsurance rates are based on self-reported health insurance coverage. A previous literature has shown that self-reported health insurance coverage has sizable error, particularly for publicly provided health insurance.[10] Therefore, we use administrative Medicare and Medicaid records (EDB and MSIS, respectively) to adjust for misreporting. These corrected uninsurance rates are shown in faded dots in Figure 2. The corrected rates are even more striking. Almost half of disenrollees were uninsured in 2008, even though Figure 2 includes people who were immediately reenrolled in Medicaid after the 2005 cuts. If we were to exclude those reenrolled individuals, the remaining disenrollees would have uninsurance rates close to 60 percent in 2008 and 50 percent through 2011.

Although this is a patently high uninsurance rate, we would still expect to observe some degree of uninsurance had the Section 1115 waiver in Tennessee not been discontinued. In other words, there is a natural rate of transition off Medicaid, which could be high for a lower income population. To provide a comparison, In Panel B of Figure 2, we plot the insurance coverage of individuals in other states that provided generous Section 1115 waivers for working age adults. Individuals in these states who received Medicaid coverage through Section 1115 waivers in January of 2005 form the basis of this comparison group. Panel B mirrors Panel A for these comparison states. There we see high (approximately 20 percent from 2008 to 2013), yet much lower uninsurance rates than the TennCare disenrollees.[11] Additionally, the uninsured rates for these comparison individuals drops noticeably after the major provisions of the Affordable Care Act were enacted in 2014.

Appendix Figure 1 shifts attention to the individuals who did obtain health insurance, broken down by

[10] See, for example, Boudreaux et al. (2019) and Noon et al. (2019).

[11] Corrected uninsurance rates using public administrative enrollment records are also around 20 percent until 2013, although the direction of correction is different for the comparison and treatment group.

whether surveyed disenrollees had public or private insurance (Panel B shows analogous results for comparison states). From Panel A, in 2008, nearly 34 percent of surveyed disenrollees reported having public health insurance), and approximately 26 percent reported having insurance from private sources (20 percent through employers, 7 percent purchased directly). Panel B shows the analogous numbers for the comparison group.[12] The most noticeable changes to both public and private health insurance rates occur after the introduction of the ACA in 2011, even though Tennessee was not an ACA Medicaid expansion state. The observed increase in private health insurance coverage may be attributable to the employer mandate and marketplace exchanges implemented by the Affordable Care Act.

Tab. 1: Summary Statistics

	2008-09			2014-15		
	Public	Private	Uninsured	Public	Private	Uninsured
	(1)	(2)	(3)	(4)	(5)	(6)
Employment	0.22	0.81	0.44	0.20	0.82	0.41
Disability	0.55	0.10	0.30	0.58	0.10	0.29
Mortality	0.11	0.03	0.06	0.12	0.02	0.06

Figure 2 reveals how many disenrollees did and did not reacquire health insurance in the aftermath of the TennCare disenrollment. Table 1 extends the study of TennCare disenrollees by examining the characteristics of disenrollees who were able to obtain health insurance to those who were not. Throughout this paper, descriptive statistics and estimates (including standard errors) may be rounded or modified to better conform with Census disclosure policies, and number of observations may not be directly reported or may include person-period numbers to better conform with existing rules. Column 1 shows that respondents with public health insurance had particularly low employment rates (20 percent) and high rates of self-reported disability (55 percent). This is partially mechanical, as low income and poor health may qualify them for public health insurance coverage. Columns 2 and 3 show a more interesting pattern. Respondents who were uninsured in 2008 or 2009 have markedly worse outcomes compared to those with private health insurance in the same period. Among those who did not obtain health insurance, 30 percent reported having a disability and approximately 6 percent died within five years of their ACS interview, despite having an average age at time of disenrollment of approximately 44. The strikingly poor outcomes of the uninsured have an important implication. When combined with persistence of uninsurance displayed in Figure 1, they reveal that a population who may have needed access to care was unlikely to have it for an extended period of time, and future cuts to Medicaid may demonstrate the same pattern of deficiency. On the other hand, their outcomes could be the causal result of losing TennCare

[12] The comparison group consists of working-age Medicaid recipients of Section 1115 demonstration waivers in the following states: AZ, DE, HI, MA, MD, ME, MI, MN, NJ, NY, OR, RI, UT, VY, and WI. We omitted states with Section 1115 waivers where our data clearly showed those programs were not targeted to a working-age or non-maternal population. Unless stated otherwise, we use this comparison group of Medicaid Section 1115 beneficiaries in these states in later analyses that use a Medicaid comparison group.

coverage.[13] In the next sections, we will estimate the impact of TennCare disenrollment on employment and health outcomes using the disenrollment as a natural experiment.[14]

4.2. Employment

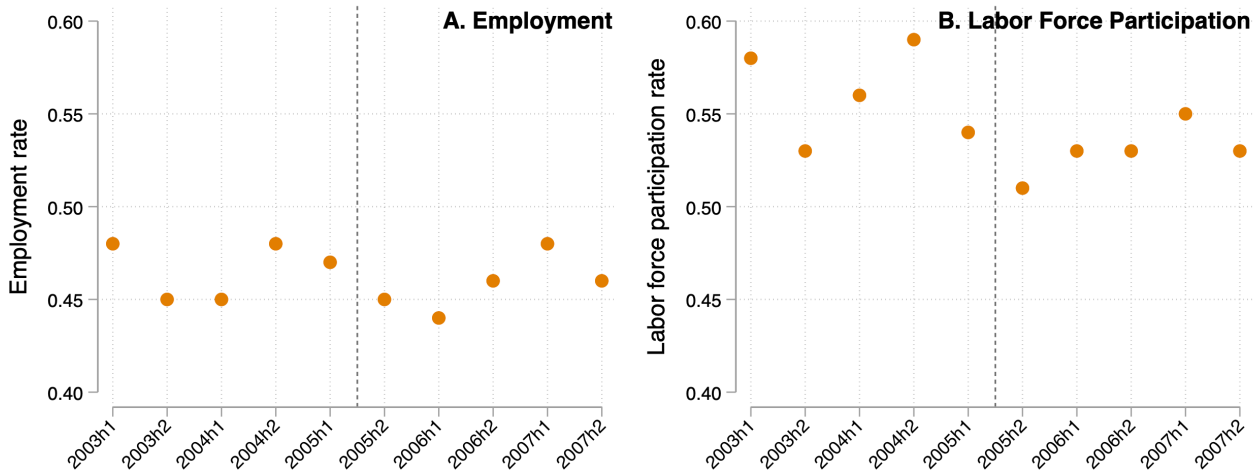
Individuals may jointly decide on labor supply and obtaining health insurance coverage, particularly in the United States where most private health insurance coverage is financed by employers. Given the high costs of health insurance, the link between health insurance coverage and employment may affect individuals' decisions to work, retire, change jobs, or increase hours (Madrian, 1994; Gruber & Madrian, 2002). Expansions and contractions of public health insurance may, therefore, provide natural experiments for testing this distortion of work incentives (Yelowitz, 1995; Garthwaite et al., 2014).[15]

The sudden cut to TennCare eligibility in 2005 was, at the time, the largest contraction of a public health insurance program in US history and, as shown in the previous sections, resulted in long-lasting declines to health insurance coverage for a vulnerable population. In this section, we examine the impact of the TennCare disenrollment on labor market outcomes for disenrollees in both the short and long-term.

4.2.1. Extensive margin

The Tenncare disenrollment is particularly well-suited to studying the question of "employment lock," or changes along the extensive margin. One might expect TennCare disenrollees to seek employment to gain private health insurance coverage following their sudden disenrollment from TennCare in the summer of

Fig. 3: Time-series



[13] The magnitudes shown in this table and subsequent figures (as well as existing research) strongly suggests that the treatment component cannot explain these measures, and the stark differences in the aggregate are due to selection. Furthermore, we could have illustrated these differences using pre-period information from the 2000 long-form.

[14] Additionally, observed differences in Table 1 could be driven by differences in baseline characteristics such as age and sex. We control for characteristics in our natural experiments in subsequent sections.

[15] Experimental evidence from the Oregon Health Insurance Experiment finds small and statistically insignificant effects of Medicaid on employment. (Baicker et al., 2014)

2005.[16] We will, however, find little evidence of such a response among disenrollees.

We begin by tracing the labor market outcomes of the TennCare disenrollees before and after the disenrollment. Figure 3 plots the employment outcomes of the TennCare disenrollees in the immediate vicinity of the disenrollment. Although the ACS is an annual survey, our restricted-access version includes the exact date of interview, which we leverage to zoom in on the time periods just before and after the disenrollment.[17] In Panels A and B of Figure 3, we plot the employment and labor force participation rates, respectively, of TennCare disenrollees by half-year between 2003 and 2007. Neither changes materially after the disenrollment. The employment rate, in the first half of 2005, is 47 percent. It was virtually unchanged by the second half of 2007, standing at 46 percent; the corresponding percentages for labor force participation were 54 and 53.

We emphasize first that the graphs in Figure 3, just as in Figure 2, isolate disenrollees. Each point consists entirely of people who held a 1115 waiver in Tennessee in January of 2005, and each half-year of the ACS contains at least a few hundred disenrollees. Therefore, the time series should be particularly informative in detecting all but the smallest changes. Nonetheless, we test for changes in these outcomes more formally and provide confidence intervals in Table 2 with a difference-in-differences design.

Table 2 shows the results of our differences-in-differences regression estimates, where each column represents a different outcome (employment rate or labor force participation rate) and each row represents a different specification. Panel A of Table 2 shows the regression estimates from our preferred differences-in-differences specification, where we compare the outcomes of TennCare disenrollees before and after the 2005 cuts to other working age adults in Tennessee. We chose other Tennesseans as a control group when examining labor market outcomes, as they are plausibly subject to similar economic trends as the TennCare disenrollees. We again use ACS respondents from 2003 to 2007 to estimate the treatment effect of TennCare disenrollment with a two-way fixed effects regression equation, and we additionally include controls for age, sex, race, Hispanic, and group quarters. Our differences-in-differences estimates confirm the conclusions from inspecting the time series presented in Figure 3. Panel A of Table 2 shows no statistically significant treatment effects on employment and labor force participation. The point estimate on employment is 0.006, which means the disenrollees were 0.6 percentage points more likely to be employed upon disenrollment compared to the rest of working age Tennesseans. This estimate is on a base period employment rate of 47.2 percent, and the estimate is statistically insignificant. The 95 percent confidence interval lies between -1.6 and 2.8 percentage points, ruling out large or moderate employment effects.

Our estimates on employment most closely resemble the employment lock evidence found in experimental studies, where Baicker et al. (2014) estimate Medicaid receipt has a -1.6 percentage point estimate on employment with 95 percent confidence interval of -4.4 to 1.2 percentage points. Our estimates on employment lock rule out the large employment effects found in quasi-experimental studies such as

[16] For example, Garthwaite et al. (2014)'s original study of TennCare on employment lock estimates that disenrollment caused 63 percent of disenrollees to increase their labor supply along the extensive margin.

[17] ACS interview dates are spread throughout the year.

Garthwaite et al. (2014)'s study of TennCare, which finds 63 percent of TennCare disenrollees increased their labor supply. Our results likely differ from Garthwaite et al. (2014) due to the data infrastructure we utilize, which fills in a gap described by Ham and Ueda (2021), "We conclude that, at best, the data [CPS and ACS] are relatively uninformative about [Garthwaite et al. (2014)'s] parameter of interest." Our employment estimates also differ from evidence using a well-designed natural experiment on state administrative data that finds Medicaid receipt for non-elderly, non-disabled, and childless adults (a population similar to the TennCare disenrollees) in Wisconsin reduced labor supply by an economically meaningful 5.2 percentage points (Dague et al., 2017). There are several reasons why our results may differ. The parameter estimated, state Medicaid program generosity, and population affected may not be directly comparable. Similar to our results on employment, we also find small and statistically insignificant effects for labor force participation. Overall, we do not find that TennCare disenrollees displayed job lock behavior on the extensive margin in the short run.

Tab. 2: Difference-in-differences Estimates

	Employment	LFP
	(1)	(2)
A. Preferred:		
Diff.-in-diff. est.	0.006	-0.003
	(0.011)	(0.011)
Obs.	158,000	158,000
Dep. var. mean	0.472	0.558
B. Robustness:		
Perennially enrolled	0.013	0.005
	(0.013)	(0.012)
Not re-enrolled	0.017	0.004
	(0.013)	(0.012)
Perennially enrolled and not re-enrolled	0.022	0.013
	(0.015)	(0.014)
Longer-run post period	0.010	-0.008
	(0.010)	(0.009)

Panel B of Table 2 provides a series of robustness and heterogeneity checks. In each row, we restrict the sample of TennCare disenrollees. We first restrict attention to the disenrollees who were on Medicaid for at least two years prior to disenrollment (Row 1 of Table 2), as these individuals may be more likely to seek health insurance through their employer. The point estimate on employment for these disenrollees (0.013) is larger than the estimate from the full sample (0.006), but it remains economically and statistically insignificant. The insignificant estimates for those who had Medicaid coverage for multiple years before enrollment may alleviate concerns of an "Ashenfelter Dip," in which disenrollees gained Medicaid

eligibility because their earnings and employment declined just before the treatment.[18] We also find a small and statistically insignificant point estimate (0.017) when we restrict our sample to individuals who were not immediately re-enrolled in Medicaid (Row 3). When we filter to disenrollees who both had TennCare for multiple years and did not immediately re-enroll in Medicaid, we again find a small and statistically insignificant point estimate (of 0.022 in Row 4). In Column 2 of Panel B, we examine labor force participation. Similar to our results on employment, we also find small and statistically insignificant effects for labor force participation, and the overall results from Panel B are consistent with evidence from the experimental literature of small and statistically insignificant employment effects (Baicker et al., 2014).

The treatment effects discussed above were estimated using survey data from 2003 to 2007, effectively giving the impact of TennCare disenrollment in the short run. While such short-run estimates are well-suited for assessing whether individuals display behavior consistent with job lock, the loss of public health insurance may affect individuals through other pathways that manifest more slowly. For example, individuals who lose health insurance may experience a slow deterioration in their health and, subsequently, are less likely to enter the labor force in the long-run.[19]While there have been advancements in understanding the long-term impact of losing Medicaid in childhood (e.g., Almond et al., 2018; Brown et al., 2020; Currie & Almond, 2011; Goodman-Bacon, 2021) the long-term impact of losing Medicaid in adulthood is less understood and has been empirically elusive due to the challenges in coming up with credible research designs. To fill this gap, we use ACS data that goes through 2019 to estimate the long-run effect of losing TennCare, and we report our results in Row 5 of Table 2 Panel B. We do not find statistically significant effects on employment or labor force participation rates. Our point estimate on employment is small and statistically insignificant at 0.010, which is nearly identical to our short-run estimate. Our results for adults stand in contrast to the long-run impact of childhood Medicaid, where the existing literature has established positive and economically important long-run employment effects (Goodman-Bacon, 2021).

In Appendix Figure 2, we provide event-study versions of our differences-in-differences design for the effect of TennCare on labor market outcomes. They provide tests of the parallel trends assumption, and they illustrate the dynamics of possible treatment effects. We do not change our conclusions in light of the event studies.[20]

4.2.2. Intensive margin

Our previously reported estimates did not find changes in employment or labor force participation in either the short-run or long-run. Individuals who lose health insurance, however, may attempt to gain private health insurance coverage through their existing employer by working more hours or moving from part-time to full-time positions. Additionally, losses of public benefits may cause other members of the

[18] Notably, the TennCare Section 1115 waiver allowed Medicaid eligibility at higher incomes rates than other state Medicaid programs, particularly those that required recipients to have incomes well below the poverty line.

[19] See Grossman (1972) and Grossman (2000) for models of health capital. Gaudette et al. (2018) also provides a discussion of health capital and labor markets.

[20] Additionally, although not shown, we find no evidence of a meaningful migratory response of disenrollees to the TennCare disenrollment (using linked administrative address data).

household to change their labor supply decisions. Accordingly, we test for changes along the intensive margin at both the individual and household levels.

We report regression estimates from our preferred differences-in-differences specification in Table 3. Row 1 and Column 1 of Table 3 shows a statistically significant increase in the log of annual wage and salary income. The point estimate is 0.083, which indicates that TennCare disenrollees annual income increased approximately 8.3 percent relative to the control group of Tennesseans, and the 95 percent confidence is 1.1 percent to 15.5 percent. In contrast to individual income, as shown in Row 1 and Column 2 of Table 3, there is no statistically significant effect of TennCare disenrollment on the log of household wage and salary income. The point estimate 0.011 and the 95 percent confidence interval is -0.034 percent to 0.056. The null result for household income may be consistent with other studies which find low-income household members adjust their labor supply decisions in response to benefits received by their partners (e.g., Vivalt et al., 2024), but we caution against trying to fit our intensive margin results into a particular theoretical narrative.

We further analyze the disenrollment effect along the intensive margin by examining hours worked and self-employment status. Row 1 and Column 3 of Table 3 shows estimates for the effect of disenrollment on hours worked for those who were employed. The point estimate for hours work is 0.75 hours, and the 95 percent confidence interval is -0.04 to 1.53 hours. Row 1 and Column 4 of Table 3 shows a statistically significant effect on the probability of working 40 hours. The point estimate is 3.1 percentage points, and the 95 percent confidence interval is 0.2 to 6.1 percentage points. We next split our sample by whether individuals were self-employed or not and estimate regressions separately by self-employment status. The effects of disenrollment along the intensive margin largely appear to load onto individuals who were not self-employed. Row 2 of Table 3 shows statistically significant effects on log income and the probability

Tab. 3: Intensive Margin

	ln(Wages)		ln(HH inc.)	Hours		Hours = 40		Hours 30-39
	(1)		(2)	(3)		(4)		(5)
All employed	0.083	*	0.011	0.745		0.032	*	-0.018
	(0.037)		(0.023)	(0.401)		(0.015)		(0.012)
Not self-employed	0.104	*	0.029	1.150	*	0.051	*	-0.028
	(0.037)		(0.031)	(0.443)		(0.017)		(0.015)
Self-employed	-0.206		-0.010	-0.284		-0.026		0.012
	(0.221)		(0.069)	(1.080)		(0.032)		(0.025)

of working 40 hours and marginally insignificant effects on the probability of working 30 hours for those who were not self-employed. The estimates may be consistent with a theory of job lock in which workers increase their work intensity to qualify for health insurance through their employers, who provide health insurance to full time workers during the period of study. We find additional support for workers making labor supply decisions to gain employer-provided health insurance when we examine the probability of

self-employment as an outcome. Our differences-in-differences regression point estimate on the probability of self-employment is -0.037, with a 95 percent confidence interval of -0.60 to -0.014, meaning TennCare disenrollees are 3.7 percentage points less likely to remain self-employed compared to other Tennesseans.

4.3. Health

The previous analyses have examined the loss of public health insurance eligibility on health insurance coverage and labor market outcomes in both the short and long run. The loss of health insurance eligibility, however, may be felt along another dimension: worsening health and rising mortality.

A primary pathway through which public health insurance could improve health is through access to the healthcare system.[21] Health insurance is a primary lever for accessing the healthcare system, so individuals with health insurance have greater access to healthcare treatments. The sudden loss of public health insurance, particularly for those who are lower income and sicker than average, could prevent individuals from receiving healthcare treatments, many of which have been shown through clinical trials to improve health. The resulting deterioration in health could manifest in the short or long run, causing even disability or death.

As shown in Section 4.1, many TennCare disenrollees remained uninsured well after the 2005 cuts. TennCare disenrollees were, furthermore, much sicker than average. Approximately 37 percent of disenrollees reported having a functional disability, such as difficulty walking or difficulty remembering, prior to their disenrollment. The ten-year survival rate of TennCare disenrollees in 2005 was approximately 88 percent. For perspective, this percentage is similar to the likelihood of a 60-year-old living to their 70th birthday. The average age of disenrollees was approximately 44.

It is not clear, however, the degree to which uninsurance would prevent TennCare disenrollees from accessing the healthcare system. There is a safety net of health care services in the United States for uninsured individuals, and healthcare providers frequently provide care regardless of health insurance status due to legal (e.g., EMTALA) and ethical obligations.[22] Tennessee took steps to strengthen its safety net programs to reduce the impact of the sudden influx of uninsured on the Tennessee healthcare system in the aftermath of the TennCare cuts (Farrar et al., 2007). Garthwaite et al. (2018) and Tello-Trillo et al. (2023) suggest that Tennessee hospitals continued to provide care for uninsured TennCare disenrollees, effectively acting as "insurers of last resort." Hence, TennCare disenrollees retained some degree of access to healthcare services.

We estimate the health effects of the TennCare public health insurance cuts using a differences-in-differences regression framework similar to the one we used to study labor supply effects in Section 4.2. We first estimate the effect of the TennCare disenrollment on self-reported disability before analyzing

[21] Public health insurance may also improve health through pathways outside of the healthcare system. For instance, receiving public health insurance could reduce financial stress and, thus, improve health.
[22] See McWilliams et al. (2009) for a review of uninsurance and healthcare access.

mortality effects. We present estimates on both the short-term and long-term effects of TennCare on health. Despite recent advances in the empirical literature, the causal effect of health insurance on mortality remains an outstanding question. Given the central importance of this question to health economics and human well-being, we then broaden our study of the effects of health insurance on mortality to include other forms of public health insurance (Medicare and the Affordable Care Act Medicaid expansions).[23]

4.3.1. Disability

In Table 4, we report the difference-in-differences estimates for the impact of TennCare disenrollment on self-reported disability. As in the preceding sections, we use ACS survey response data on working age adults (ages 21 to 64) for our estimates. We classify respondents as disabled if they respond affirmatively to having a cognitive difficulty, ambulatory difficulty, vision difficulty, hearing difficulty, self-care difficulty, or difficulty living independently. We analyze the impact of disenrollment over a short-term and long-term horizon. The shorter horizon includes ACS surveys through 2007; the longer horizon, through 2015, which is ten years after the TennCare cuts. Panels A and B of Table 4 separately report estimates for two plausible control groups. The first control group (Panel A) is individuals who had similar Medicaid Section 1115 eligibility waivers in other states (as in Section 4.1). The second control group (Panel B) is working-age individuals in Tennessee who were not TennCare disenrollees (as in Section 4.2).

Column 1 of Table 4 reports small, statistically insignificant estimates for the impact of TennCare disenrollment on disability in the short run. As shown in Panel A, the point estimate is 0.003, which indicates TennCare disenrollees are only 0.3 percentage points more likely to report a disability (on a base of 36.7 percent for the treatment group just prior to disenrollment) after disenrollment compared to individuals with Medicaid waivers in other states. The 95 percent confidence interval is -1.7 to 2.4 percentage points. The short-run results are similar when other Tennesseans are used as the control group. Panel B shows a small, statistically insignificant point estimate of 0.002; the 95 percent confidence interval is -1.7 to 2.0 percentage points. Our short-run results fail to find that the TennCare disenrollment increased disability, a key marker of health.

Because health is a stock measure, impacts of health insurance loss may not manifest in the short-run Therefore, we look for long-term effects in Column 2. As shown in Panel A, the point estimate for the long-term effect on disability, using data up to ten years after the disenrollment, is close to zero and statistically insignificant at -0.004, which is a 0.4 percentage-point differential between disenrollees and individuals with Medicaid 1115 waivers in other states; the 95 percent confidence interval is -2.2 to 1.4 percentage points. Our long-run results using Medicaid recipients in other states as a control group, therefore, fail to find that the TennCare disenrollment increased disability.[24]

[23] Like Miller et al. (2021) and Wyse & Meyer (2025), which have made tremendous advances in the study of public health insurance on mortality, our linked dataset is particularly well-suited for obtaining the statistical power to estimate mortality effects.

[24] It is important to note that differences-in-differences estimates of the impact of the TennCare disenrollment on health, in both the short-run and long-run, do not merely estimate an individual's loss of public health insurance coverage. The large-

Tab. 4: Disability

	Disability short-term (1)	Disability long-term (2)	
A. 1115 Waivers in Other States:			
Diff.-in-diff. est.	0.003	-0.004	
	(0.011)	(0.009)	
Obs.	72,500	227,000	
Dep. var. mean	0.367	0.367	
B. Working-age Population in Tennesee:			
Diff.-in-diff. est.	0.002	-0.017	*
	(0.009)	(0.008)	
Obs.	158,000	477,000	
Dep. var. mean	0.367	0.367	

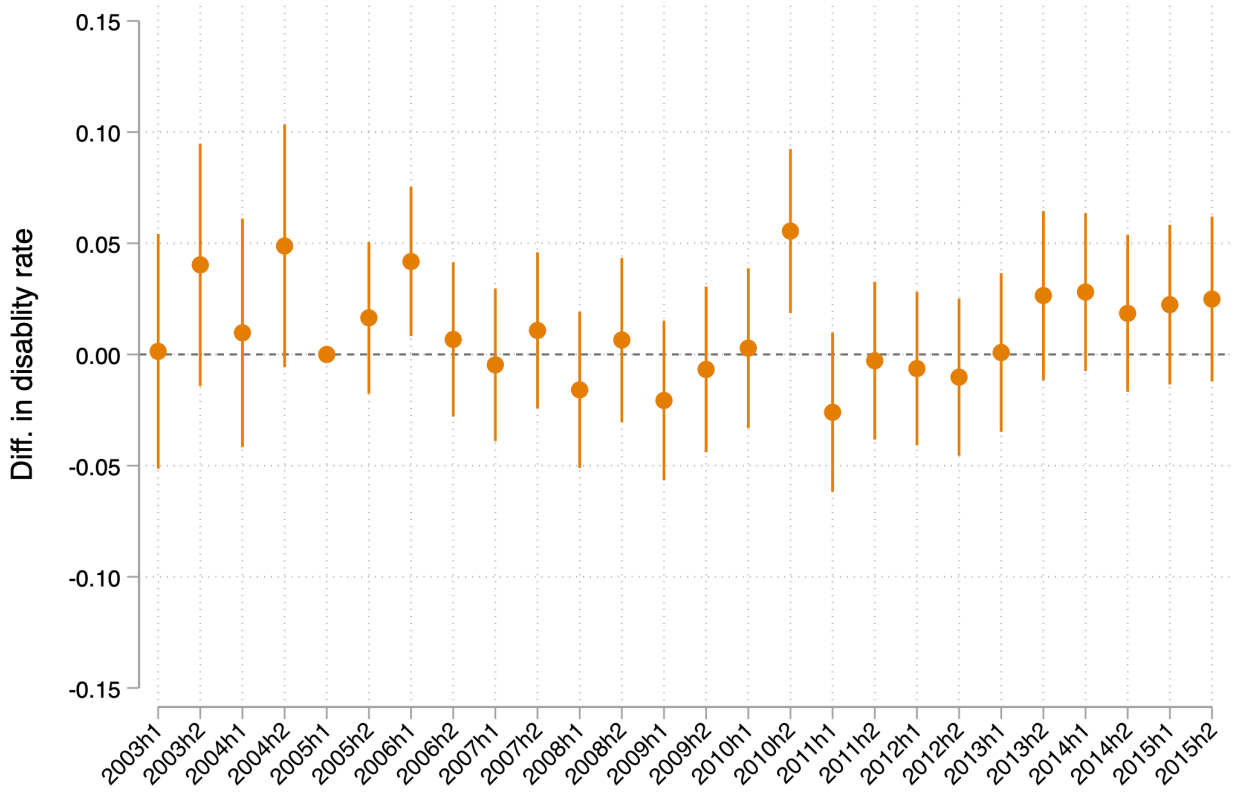
Panel B reports a statistically significant estimate of -0.017 for the long-term effect on disability, or a 1.7 percentage-point differential between disenrollees compared to other Tennesseans; the 95 percent confidence interval is -3.3 to -0.1 percentage points. While the Panel B estimate for the long-term effect is statistically significant, the sign of the point estimate runs counter to prevailing wisdom, suggesting the health of disenrollees improved relative to other adults in Tennessee. However, the control group of other Tennesseans (Panel B) may not prove as suitable of a counterfactual as the control group of Medicaid 1115 waiver beneficiaries in other states (Panel A) when examining health outcomes. Medicaid recipients across states may have more similar health profiles than Medicaid recipients versus non-recipients within a state. We present the event-study version of this differences-in-difference design (Panel A) to further assess the dynamic nature of our results.

Figure 4 shows the event-study estimates of the long-term impact of TennCare disenrollment on disability, using Medicaid 1115 waiver beneficiaries in other states as the control group (as in Panel A). The baseline period immediately preceding the TennCare disenrollment (omitted coefficient) is the first half of 2005, and the results are estimated through 2015. The semi-annual coefficients are plotted and compared to the baseline period (horizontal dashed line at 0). We do not observe a readily apparent pattern consistent with long-term worsening health for TennCare disenrollees. In fact, most semi-annual estimates remain near or slightly below the baseline period (almost all of the confidence intervals contain zero), and do not

scale nature of the TennCare disenrollment would also impact the healthcare supply-side, as payments to providers change, potentially creating an aggregate effect (Finkelstein, 2007). We reiterate this pathway when we discuss mortality below.

suggest the results are driven by a trend.

Fig. 4: Disability



There is no consensus in the health economics literature on the degree to which health insurance affects health outcomes. Quasi-experimental designs, despite recent advancements in methodology, present mixed evidence across health outcomes, including self-reported health status, hospitalizations, mortality, risk prevalence, prevalence of chronic conditions, and health limitations and functional status (Gaudette et al.,2018; Sommers et al., 2017; Gruber & Sommers, 2019; Levy & Meltzer, 2008). Experimental designs, such as the RAND Health Insurance and Oregon Health Insurance experiments, find clear evidence that health insurance increases utilization, but the pathways through which health insurance impacts health remain unclear (Baicker et al., 2013; Finkelstein et al., 2012; Keeler et al., 1985; Newhouse, 1993). Our primary difference-in-differences design specification fails to reject the hypothesis that public health insurance disenrollment does not impact self-reported disability.

We discuss several possible explanations for our null effect on self-reported disability. We emphasize that our natural experiment tests the effects of a public health insurance *contraction,* while most of the existing literature has examined public health insurance *expansions*. First, differences-in-differences estimates of large public health insurance expansions or contractions do not merely estimate an individual's loss of public health insurance coverage (demand side); such public health insurance expansions or contractions may impact the supply side of health care, creating an aggregate effect which, in turn, would impact health

outcomes (Finkelstein, 2007; Finkelstein & McKnight, 2008). In contrast to public health insurance expansions such as the Affordable Care Act, which improve healthcare providers' financial positions (Duggan et al., 2022; Goettlib et al., 2025; Moghtaderi et al., 2020), evidence suggests the TennCare disenrollment led to a deterioration in providers' financial conditions (Garthwaite et al., 2018), plausibly creating an aggregate effect leading to even worse health outcomes. Our null results, however, do not appear to be consistent with this explanation, as the aggregate effects of public health insurance contractions would increase the likelihood of worsening health outcomes.

Second, gaining and losing health insurance need not have symmetric effects. Healthcare may have diminishing returns, leading to different effects of public health insurance expansions and contractions on health.[25] Individuals who receive health insurance coverage after a long period of uninsurance, such as gaining public health insurance through Medicare or ACA Medicaid expansions, may experience large improvements in health due to increased returns from initial health investments. For example, previously uninsured individuals who gain coverage may utilize health-improving diagnostic services (Myerson et al., 2020); those who have had uninterrupted health insurance for several years may have already realized much of the benefits from such diagnostic services or high-return investments. The majority of TennCare disenrollees, as shown in Section 4.1, had Medicaid coverage for several years before disenrollment, so the loss of coverage to their health may have been muted compared to a similar population, previously uninsured, affected by an expansion of Medicaid. This logic may be more applicable to the short-run as opposed to the long-run. If health capital depreciates over long periods without health insurance, one might expect long-run effects. Our disenrollees, indeed, remained uninsured for long-periods after disenrollment (Section 4.1), yet we find neither short-run nor long-run effects on disability.

Third, as is the case of experiments on health insurance - natural or otherwise - the results depend on context, which may be particularly true for Medicaid expansions and contractions. For example, the population affected by TennCare disenrollments may better overlap with the target beneficiaries of the ACA Medicaid expansions rather than the Oregon Health Insurance Experiment (Garthwaite et al., 2014). Additionally, the results from TennCare disenrollments, which affected working age adults, may not generalize to the youngest or the oldest in the population. Childhood is a sensitive period for health and other human capital investments (Almond et al., 2018). Therefore, the evidence of large and long-term health benefits of Medicaid receipt in childhood, such as shown by Brown et al. (2019), Currie & Gruber (1996), and Goodman-Bacon (2021), may not extend to the working-age adult population. Similarly, studies of the elderly population, such as Abaluck et al. (2021), may not extrapolate to working-age adults. For these reasons, we broaden our study of public health insurance beyond TennCare in the subsection to follow.

Finally, health insurance may affect health status in ways that our disability measures cannot capture. Therefore, in the last component of our analysis, we consider mortality, an unambiguous outcome that is perfectly measured in our data and reflects all insults and inputs accumulated over the life course, whether

[25] Grossman (1972) and Grossman (2000) develop a model of health capital. Gaudette et al. (2018) further discusses the relationship of health insurance coverage and health in the context of a health production function.

they manifest quickly or slowly. Still, it is far from obvious the pathway through which health insurance could materially affect mortality without changing disability in the long run.

4.3.2. Mortality

Estimating the relationship between health insurance and mortality is exceedingly difficult, yet several recent papers have overcome these empirical challenges to provide causal estimates across a variety of health insurance settings (Abaluck et al., 2021; Card et al., 2009; Chandra et al., 2024; Goldin et al., 2021; Miller et al., 2021; Roberts et al., 2025; Wyse & Meyer, 2025). Collectively, they indicate health insurance matters for reducing mortality. Like Miller et al. (2021) and Wyse & Meyer (2025), which have made tremendous advances in the study of public health insurance on mortality, our linked dataset is particularly well suited for obtaining the statistical power to estimate mortality effects.

4.3.2.1. TennCare

We begin by examining whether TennCare disenrollees experienced worse mortality outcomes after the Tenncare contraction compared to a matched control group. Mortality as an outcome poses several empirical challenges. It is rare, it is absorbing, and its determinants are myriad. It is the end result of factors both acute and accumulated over the life course, so the impact of Medicaid disenrollment on mortality may manifest in the short, medium, or long run.[26] Accordingly, we estimate the impact of the TennCare disenrollment on mortality two, five, and ten years after the disenrollment.

Medicaid status is determined by a wide variety of factors (including socioeconomics, labor, and health) associated with mortality. Non-experimental studies using Medicaid status as a treatment indicator may, therefore, be biased, and controlling for limited covariates may not eliminate bias (Freedman et al., 2021; Kronick, 2009; Levy & Meltzer, 2008). In addition to controlling for demographic variables such age, sex, race, Hispanic, and group quarters, we also use a battery of additional controls derived from the Census long-form such as income, employment, and type of functional disability to generate a propensity score for TennCare receipt. While we do not have a comprehensive set of covariates on health status, we can observe mortality for the months immediately preceding the TennCare disenrollment.[27] We use propensity scores to match the treatment and a control group of working-age adults on baseline (in the "pre-period" months immediately preceding the 2005 disenrollment) mortality levels.[28] We then estimate linear regressions (linear probability estimates) with the probability of death during a time-window (estimated

[26] See Levy & Meltzer (2008) for a discussion how health insurance may (or may not) impact mortality.

[27] Recall that our linked MSIS data begins to show 1115 waiver status at the beginning in January of 2005, so we observe mortality for our treatment group of TennCare 1115 waiver recipients from this time until disenrollment begins in the third quarter of 2005, giving us just over a half year of mortality in the "pre-period."

[28] See Black et al. (2017) for a discussion of matching methods in the context of long-run mortality as well as an analysis that uses a detailed suite of health-related covariates from the Health and Retirement Study.

for two, five, and ten-year windows separately) as a binary outcome on a dummy variable indicating TennCare disenrollment (treatment status).[29]

Tab. 5: TennCare Mortality

	Pre-period (1)	Within 2 years (2)	Within 5 years (3)	Within 10 years (4)
A. All disenrollees				
Relative risk	0.954	1.000	1.032	1.105
Absolute difference	-0.00026	0.00001	0.00154 *	0.01147 *
	(0.00021)	(0.00037)	(0.00059)	(0.00084)
Probability of death in control	0.0057	0.0181	0.0482	0.1090
B. Removing disabled				
Relative risk	0.950	0.985	1.021	1.106
Absolute difference	-0.00025	-0.00024	0.00092	0.01036 *
	(0.00020)	(0.00036)	(0.00057)	(0.00082)
Probability of death in control	0.0051	0.0160	0.0427	0.0975

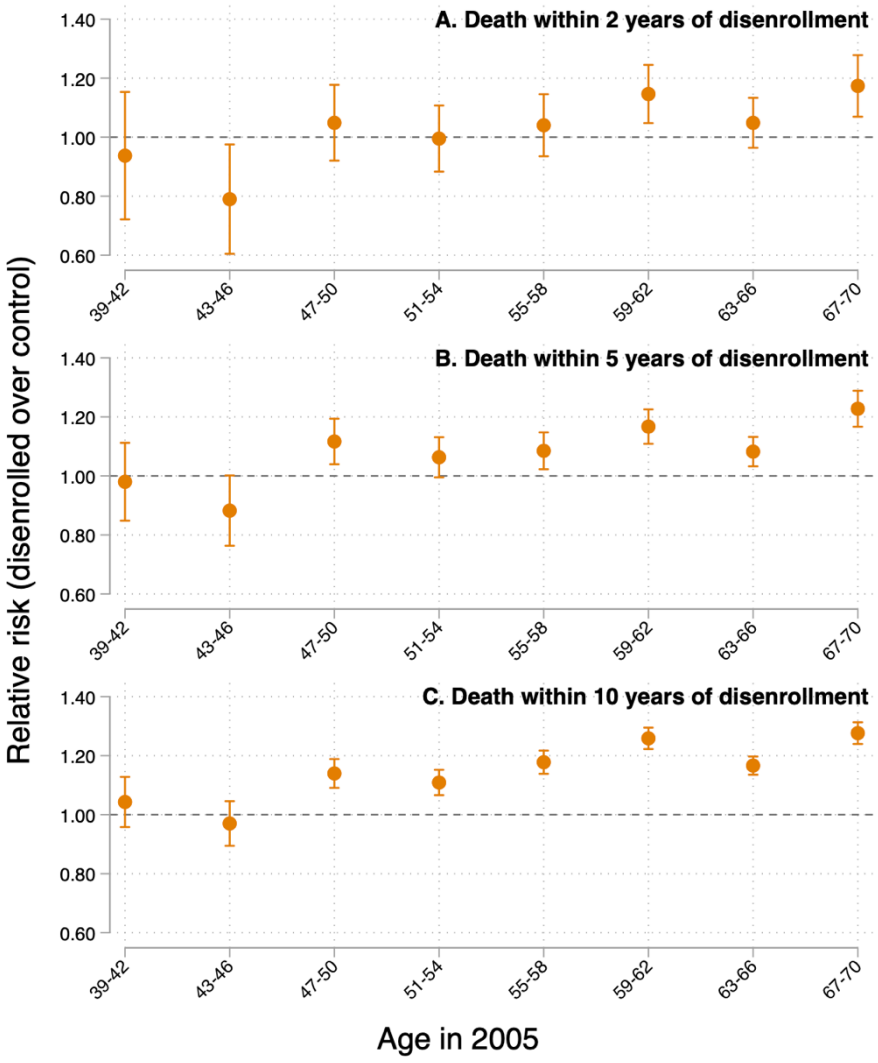
Table 5 Panel A shows our primary regression estimates of the impact of TennCare disenrollment on mortality, where each column represents a different death window (two, five, and ten years) and each row represents a different specification. The first rows of Table 5 show estimates from our preferred specification, where we compare the mortality outcomes of TennCare disenrollees (pooled ages 21 to 64) to a matched control group. The first row shows relative mortality risks of TennCare disenrollees to the control group, and ratios greater than one indicate the TennCare disenrollees were more likely to die in a given time window compared to the control group. While TennCare disenrollees die at somewhat similar rates to the control group two and five years after disenrollment (point estimates of 1.000 and 1.032, respectively), a large difference in mortality appears in the long run (ten years after disenrollment). Column 4, Row 2 of Table 5 shows that TennCare disenrollees were 1.147 percentage points (95-percent confidence interval: 0.982 to 1.312 p.p.) more likely to die than the control group over a 10-year time window after the TennCare disenrollment, relative to the 10.9 percent of the control group who died during this time period. The result, if valid, would indicate TennCare disenrollment increased mortality in the long run (but not in the short run). A similar result is shown in Panel B, where we remove individuals who were dual enrolled in Medicare (and, thus, less healthy and more likely to retain public health insurance following the TennCare contraction). For non-dually enrolled TennCare disenrollees, relative mortality is similar to the control group two and five years after disenrollment but increases ten years on.

We next consider the magnitude and plausibility of the estimated coefficient of TennCare disenrollment on mortality. A 1.147 percentage point increase in 10-year probability of death means that an additional 1,147 individuals per 100,000 die within 10 years of disenrollment. For 177,000 TennCare disenrollees,

[29] Cox proportional hazard estimates show very similar results, although not presented here.

this translates into an additional 2,030 lives lost 10 years after disenrollment. A back of the envelope comparison to other (plausibly similar) Medicaid policy changes suggests our estimates may even exceed those from other studies. Wyse & Meyer (2025), for example, estimates that the ACA Medicaid expansion saved over 27,000 lives during a similar time frame. Our estimates, if crudely extrapolated to the approximately 10 million adults who gained Medicaid coverage under the ACA Expansions, could indicate an additional 114,700 lives saved over a ten-year period.[30] The social and economic importance of an effect of this magnitude warrants much greater scrutiny than our main regression analysis.

Fig. 5: TennCare Mortality by Age



[30] We emphasize the crudeness of this extrapolation. Notably, and as discussed in multiple instances, there could be differences in the symmetry of a public health insurance expansion or contraction. The populations impacted may not generalize, and studying one state Medicaid program may not translate into the institutional details of another state. Additionally, the parameters from other research may not directly compare to our estimates (e.g., other LATEs).

We, therefore, conduct additional tests to assess the robustness and validity of our initial finding. In Figure 5, we estimate regressions similar to those in Table 5 Panel A, but we estimate them separately by age of the disenrollees. Figure 5 displays regression estimates for individuals in their 40s through 60s. Each panel of Figure 5 shows a different time-window for death (two, five, and ten-year). The relative mortality of TennCare disenrollees appears greater for older ages, increasing, for example, from (a point estimate of) 1.04 for individuals ages 39-42 to 1.26 at ages 59-62 for death over a ten-year window (as shown in Panel C). However, even individuals ages 67-69, a "placebo" group, show similar estimates of 1.27.[31] Such elderly individuals had access to Medicare, and so they would have remained insured after the 2005 disenrollment. We view this result, consequently, as likely failing a falsification exercise.[32] Our presumption for the estimated increased mortality risk for these individuals is that our primary specification fails to control for unobserved heterogeneity and health profiles between TennCare disenrollees and the control group, despite our efforts to find a control group with a similar mortality level during our (short) pre-period.

To continue weighing this presumption nonetheless, we next expand the scope of our analysis beyond TennCare. Because our estimated mortality impact exceeds those of the recent literature, we revisit the ACA Medicaid Expansion using our linked dataset in a differences-in-differences design that follows very closely with the past literature, but with an important modification. Because our mortality effects are greatest among the near-elderly, we also examine whether *uninsured* near-elderly individuals experience reduced mortality when they gain Medicare eligibility at age 65.

4.3.2.2. Affordable Care Act

We first weigh the plausibility of our estimates by comparing to what is perhaps the most similar (albeit asymmetric) health policy. The Affordable Care Act Medicaid expansions were the largest *expansion* of public health insurance since the introduction of the program in 1965. While an early literature used quasi-experimental designs to estimate economically important mortality reductions from pre-ACA state Medicaid expansions (e.g., Sommers et al., 2017), there were concerns about reliability of estimates, particularly because public vital records may lack the statistical power needed to study a rare outcome such as mortality (Black et al., 2021).[33] Miller et al. (2021) advanced the mortality literature by linking administrative death records (Numident) to survey data (the ACS) in order to target beneficiaries likely to "take-up" Medicaid after gaining ACA Medicaid expansion eligibility, greatly increasing the statistical power over previous studies. They find the ACA Medicaid expansion had large, immediate, and persistent mortality effects. Wyse & Meyer (2025) build on the linked data framework of Miller et al. (2021), incorporating administrative tax records and further improving statistical power, finding similar results.

Our methodology follows in the path of Miller et al. (2021). We link ACS respondents to their death

[31] We observe individuals in our MSIS data who were enrolled in TennCare despite being over 65 years old and eligible for Medicare.

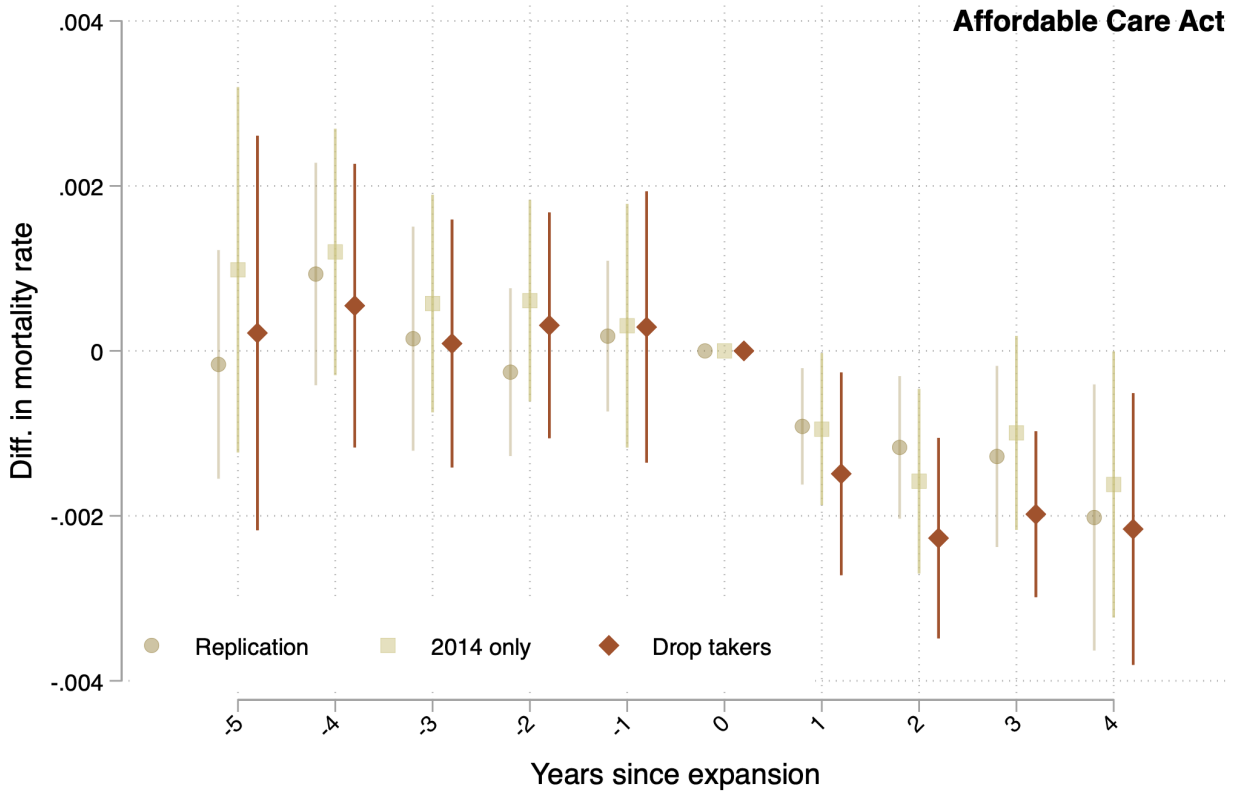
[32] It is possible, however, that losing Medicaid could alter costs or generosity of healthcare access even with Medicare.

[33] Experimental studies of Medicaid, which have sample sizes constrained by monetary costs, are not designed to study mortality and lack the statistical power to do so (Finkelstein et al., 2012).

records in Numident. However, we incorporate administrative Medicaid enrollment records (MSIS) to better identify individuals who gained or did not gain Medicaid coverage following the ACA expansions.

States received generous federal matching to expand Medicaid eligibility to low-income populations through the ACA, but they were not required to do so. The resulting geographic variation in ACA Medicaid eligibility expansions across states and time provides a natural experiment for studying a large

Fig. 6: Affordable Care Act



public health insurance expansion.[34]Accordingly, economists have used differences-in-differences designs as a common empirical strategy for studying the ACA Medicaid expansions (Gruber & Sommers,2019). In the context of mortality, Miller et al. (2021) use a differences-in-differences design with states that expanded Medicaid eligibility as the treated group, and states that did not expand as the control group. Their novel linked dataset allows them to subset their analysis to individuals likely to receive ACA Medicaid eligibility, near-elderly adults (55 to 64 years old) with low education (no high school degree) and low income (less than 138 percent of the FPL), greatly improving the statistical power over unlinked data. Their differences-in-differences event-study reveals the time dynamics of the ACA expansions, indicating that the expansion led to a large, immediate, and persistent reduction in mortality.

[34] See Miller et al. (2021) for institutional details.

We replicate the result from Miller et al. (2021) using our twin dataset in the first of three event studies we show in Figure 6 (gray circles). We follow their two-way-fixed-effect specification and derive the same set of point estimates and standard errors. The event-study coefficients prior to expansion show a parallel pre-trend between adopting and non-adopting states. The coefficient in the year immediately following expansion is negative and statistically significant, suggesting the ACA reduced mortality promptly. The coefficients up to four years after the expansion are also statistically significant, even increasing in absolute value, suggesting the mortality effects are persistent. In the second of three event studies (gray squares), we restrict expansion states to those who expanded in 2014, which is almost all states that expanded. We do this to circumvent issues with two-way fixed effects in the presence of heterogeneous treatment effects and staggered timing (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021). The event study is virtually unchanged.

The novelty of this analysis lies in the third and final event study in Figure 6 (red diamonds). There, we make one additional modification. We remove from the sample people who, per our administrative enrollment records, took up Medicaid immediately after the expansions became effective. Specifically, we exclude the ACS respondents who were not enrolled in Medicaid in the fourth quarter of 2013 but were enrolled in the first quarter of 2014. These new beneficiaries account for almost two thirds of the increase in Medicaid rolls in the first year of the expansions. When we drop them from the analysis, we see the same pattern as in the previous specifications: an immediate and persistent decline in mortality in states that expanded coverage. Indeed, the ACA expansions appear to have rapidly and substantially improved the mortality of a group that was, by construction, largely devoid of the ACA enrollees.

This result may seem puzzling, but we proffer an explanation that is consistent with existing research on public health insurance. Our result amounts to people who did not take up Medicaid benefiting from growth in Medicaid eligibility. The large changes in public health insurance from the ACA Medicaid expansion could impact the healthcare supply-side, as payments to providers change, potentially creating an aggregate effect (Finkelstein, 2007). These supply-side changes would have to manifest quickly in order to justify the result, but it is possible that providers anticipated such changes. If a supply-side story is at play, our result greatly narrows the class of explanations for Medicaid's strong impact on mortality. It means that the ACA's impact may be more about spillovers onto the healthcare system than meeting the immediate needs of newly enrolled Americans.

On the other hand, even in a high-impact sample (low education and low income) like the one Miller et al. (2021) created and that we duplicate, only about 7 percent of respondents took up Medicaid in 2014. It may not be surprising that removing about 7 percent of the sample has little effect on the point estimates. There could be something else about the select states who elected or declined to expand Medicaid that changed around 2014. Couillard et al. (2021), for instance, notes starkly different trends by geography among less educated Americans. As Miller et al. (2021) show, however, the confounding factors do not affect older age ranges or higher income individuals, which shuts down many competing explanations.

In either case, in light of our reanalysis of the ACA, we exhibit even greater caution with respect to the plausibility of our "treatment effect" of TennCare on mortality. Aggregate or supply-side changes may

matter socially and economically for impacting mortality, but, in the context of TennCare, we believe our point estimate on ten-year mortality is too large to reflect only supply-side considerations, particularly given that the disenrollees continued to receive uncompensated care (Garthwaite et al., 2018). The inability to control for unobserved heterogeneity would seem more likely, as our effect becomes even more of an outlier to the literature on the ACA and mortality when we caveat that literature with the result in Figure 6. We, nonetheless, check near-elderly mortality—where our estimated effect is strongest—in the context of Medicare to further assess the plausibility of our estimates.

4.3.2.3. Medicare

Medicare offers near-universal health insurance coverage for individuals over 65 years old, and it is the largest federal public health insurance expenditure in the United States. Studies have examined changes in insurance coverage, utilization, and health around the age-65 Medicare eligibility threshold, commonly employing regression discontinuity designs or similar empirical strategies (Card et al., 2008; Card et al., 2009). Our mortality-linked survey data allows us to observe mortality around the Medicare age-65 cutoff based on previous health insurance coverage and socioeconomic characteristics. We test mortality around the age-65 cutoff at the population-level, for those previously uninsured, and a low-income group.

Fig. 7: Medicare Cutoff

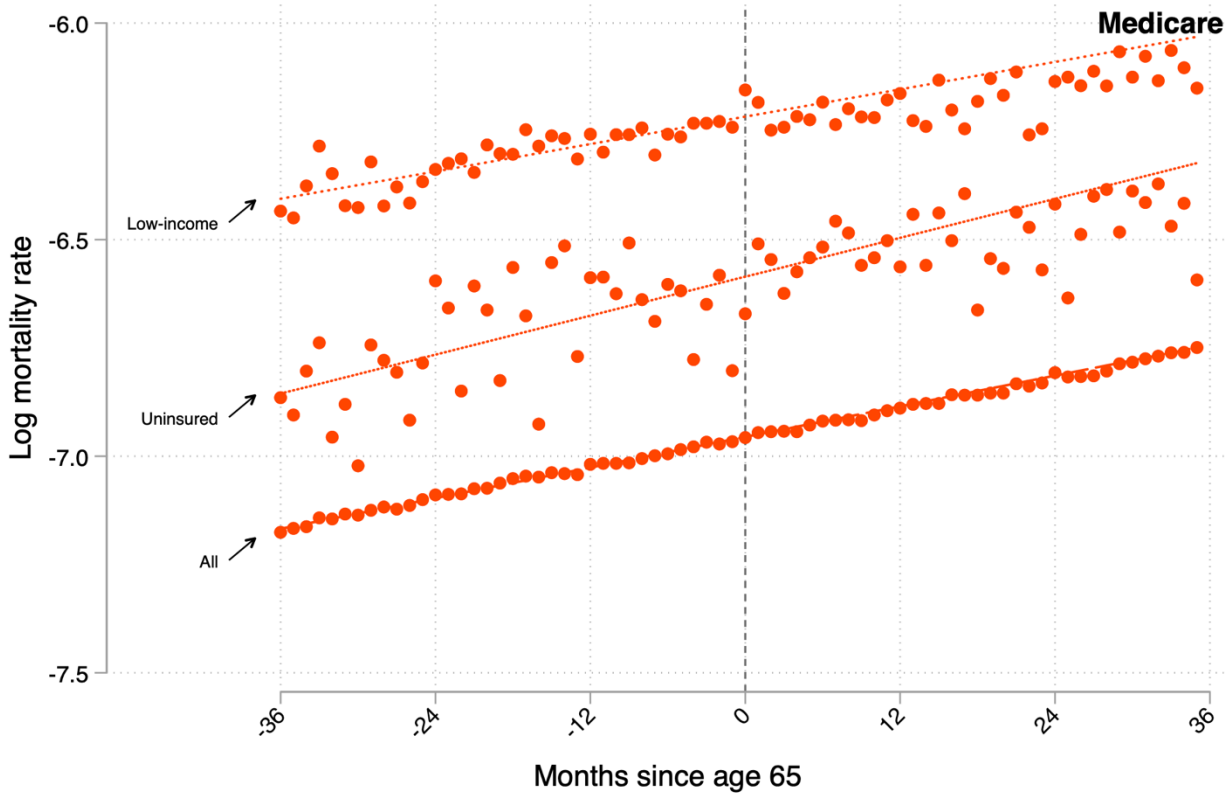


Figure 7 plots the log of monthly probability of death for ages 62 to 67, or three years on each side of the

Medicare age-65 eligibility cutoff for these three groups: full population, uninsured, and low-income. To construct our groups, we use Numident death records linked to ACS 2001-2017 survey responses for individuals born between 1940 and 1952. We examine health insurance and socioeconomic status for survey respondents ages 55 to 61 at the time of survey, to classify respondents as uninsured or as low-income (i.e., without a high school degree or below the FPL).

Visual inspection of the log monthly mortality for the full population and uninsured does not suggest short-run impacts of Medicare on mortality for these groups. Regression discontinuity estimates that fit linear trends on each side of the age-65 treatment cutoff do not find statistically significant jumps or slope changes at the cutoff for the full population or uninsured.[35] Visual inspection of log monthly mortality for the low-income population may be consistent with a trend break, as almost all the monthly mortality rates after 65 fall below the pre-existing trend line. Moreover, we estimate a statistically significant slope change for log monthly mortality for the low-income population using a regression discontinuity design. However, this result is sensitive to a "donut" or removing two months near the cutoff. The donut reduces the slope change such that the resulting estimate is not statistically significant.[36] Therefore, we take this as only suggestive evidence.[37] That said, such a trend break, if genuine, would be epidemiologically and economically important, warranting greater investigation.

We do not believe our analysis of Medicare yields clear enough evidence to support or reject our main TennCare result. The idea was that a large effect of gaining insurance around age 65 might justify our large effect of losing insurance at near elderly ages in our context, but we are unable to conclude whether Medicare has such an effect.

4.3.3. Summary of Health Effects

Admittedly, our results do not point to a clear-cut conclusion on the causal impact of TennCare disenrollment on mortality. There may be important heterogeneity in unobserved health status that confounds our results and manifests even for our mortality estimates for a placebo group, despite our attempts to find a reasonable control group for this large and sudden shock to health insurance status of over 150,000 people. We also find it difficult to reconcile our estimates of significant long-run mortality increases to an estimated null effect on long-run disability.[38] We, therefore, refrain from a determination, as our work warrants additional research into the relationship between public health insurance and population health outcomes.

5. Discussion

[35] The regression coefficients are not shown but are available by request.

[36] The donut is interesting in its own right, as its constituent outliers on the right side of the cutoff could reflect deferment of risky procedures, but that possibility is beyond our scope.

[37] Additionally, Chetty et al. (2016) utilizes an exceptionally well-suited dataset with tax records and does not find changes in mortality at age 65 for individuals in the 5th percentile of income.

[38] We qualify, however, that the determinants of mortality are varied to the point that it need not operate through functional disability.

We studied, what was at the time, the largest contraction of public health insurance in US history. While many TennCare disenrollees retained health insurance coverage through other bases of public health insurance eligibility (e.g., means related to disability), approximately half of the TennCare disenrollees were still uninsured several years after the disenrollment, despite having relatively poor health. We did not find changes in employment or labor force participation, but we did find evidence that employees increase their hours worked over 40 hours per week, which may be consistent with theories of individuals making labor market and health insurance decisions jointly. We found mixed evidence of the TennCare disenrollment on health outcomes, which motivated us to examine the impact of public health insurance on mortality more broadly.

Our findings come when the TennCare disenrollment is no longer the largest contraction of public health insurance in US history. The unwinding of pandemic provisions for Medicaid as well as looming cuts to the federal outlays that support public health insurance programs, which the CBO estimates could result in 7.5 million people becoming uninsured, would greatly eclipse the state-level contraction that we study (Government Accountability Office, 2025; Gaffney et al., 2025). They may also signal a departure, of unknown duration, from the steady expansion of the social safety net to the reallocation of funds to other enterprises. The extant literature may prove useful but inadequate to map out the implications of that new trajectory. While our findings may be only one step in that direction, our approach may yield many more.

The research *template* that we establish—research *findings* that we establish aside—will almost surely be of use for continued study of the social safety net. The FSRDC infrastructure contains not only administrative Medicaid eligibility records, but SNAP and TANF records for many states and many years that can also be linked to the survey instruments that we use here. Pooling these states and years may provide the scope needed to better predict the impacts of future cuts to the safety net at the national level, as opposed to the more fractured landscape of state-level data infrastructures. For example, researchers may consider the FSRDC to study and refine estimates of the aforementioned pandemic provisions, woodwork effects of the ACA Medicaid expansions, work requirements for Medicaid, SNAP, and TANF, or long-run impacts of omnibus bills such as OBRA90 and PRWORA. Chronicling these experiences and others is more feasible than ever before.

Indeed, investments into research that may have previously been harder for researchers to justify have newfound benefits and paramount importance in a climate of steep budget cuts. The TennCare experience exemplifies that arc. In the words of TennCare's leading advocate, Gordon Bonnyman, "A grand vision had become a political scramble to cut the program as fast as possible" (Lyman, 2004).

Abaluck, J., Caceres Bravo, M., Hull, P., & Starc, A. (2021). Mortality effects and choice across private health insurance plans. The Quarterly Journal of Economics, 136(3), 1557-1610.

Ali, M. M., Bradford, A. C., & Maclean, J. C. (2024). TennCare disenrollment led to increased eviction filings and evictions in Tennessee relative to other southern states: Study examines mandatory Medicaid disenrollments in Tennessee, eviction filings, and evictions. Health Affairs, 43(2), 269-277.

Almond, D., & Currie, J. (2011). Killing me softly: The fetal origins hypothesis. Journal of Economic Perspectives, 25(3), 153-172.

Almond, D., Currie, J., & Duque, V. (2018). Childhood circumstances and adult outcomes: Act II. Journal of Economic Literature, 56(4), 1360-1446.

Argys, L. M., Friedson, A. I., Pitts, M. M., & Tello-Trillo, D. S. (2020). Losing public health insurance: TennCare reform and personal financial distress. Journal of Public Economics, 187, 104202.

Baicker, K., & Finkelstein, A. (2011). The effects of Medicaid coverage—learning from the Oregon experiment. New England Journal of Medicine, 365(8), 683-685.

Baicker, K., Finkelstein, A., Song, J., & Taubman, S. (2014). The impact of Medicaid on labor market activity and program participation: evidence from the Oregon Health Insurance Experiment. American Economic Review, 104(5), 322-328.

Baicker, K., Taubman, S. L., Allen, H. L., Bernstein, M., Gruber, J. H., Newhouse, J. P., Schneider, E. C., Wright, B. J., Zaslavsky, A. M., & Finkelstein, A. N. (2013). The Oregon experiment—effects of Medicaid on clinical outcomes. New England Journal of Medicine, 368(18), 1713-1722.

Bailey, J., Blascak, N., & Mikhed, V. (2020). Missouri's Medicaid contraction and consumer financial outcomes.

Black, B., Espín-Sánchez, J.-A., French, E., & Litvak, K. (2017). The long-term effect of health insurance on near-elderly health and mortality. American Journal of Health Economics, 3(3), 281-311.

Black, B., French, E., McCauley, J., & Song, J. (2018). The effect of disability insurance receipt on mortality. Memorandum, 19, 2012.

Black, B., Hollingsworth, A., Nunes, L., & Simon, K. (2022). Simulated power analyses for observational studies: An application to the affordable care act medicaid expansion. Journal of Public Economics, 213, 104713.

Bonnyman, G. (2006). The TennCare Cuts: Plunging into the Unknown. Tennessee Justice Center, April, 25.

Bonnyman, G., & Garr, T. (2014). Obamacare: cautionary lessons from the Tennessee prequel. Journal of Health Care for the Poor and Underserved, 25(1), xxv-xxxi.

Brown, D. W., Kowalski, A. E., & Lurie, I. Z. (2020). Long-term impacts of childhood Medicaid expansions on outcomes in adulthood. *The Review of economic studies*, *87*(2), 792-821.

Buchmueller, T., Ham, J. C., Shore-Sheppard, L. D., & Moffitt, R. A. (2016). Economics of means-tested transfer programs in the United States. In: University of Chicago Press Chicago.

Call, K. T., Davidson, G., Davern, M., Brown, E. R., Kincheloe, J., & Nelson, J. G. (2008). Accuracy in self-reported health insurance coverage among Medicaid enrollees. INQUIRY: The Journal of Health Care Organization, Provision, and Financing, 45(4), 438-456.

Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. Journal of econometrics, 225(2), 200-230.

Card, D., Dobkin, C., & Maestas, N. (2008). The impact of nearly universal insurance coverage on health care utilization: evidence from Medicare. American Economic Review, 98(5), 2242-2258.

Card, D., Dobkin, C., & Maestas, N. (2009). Does Medicare save lives? The Quarterly Journal of Economics, 124(2), 597-636.

Chandra, A., Flack, E., & Obermeyer, Z. (2024). The health costs of cost sharing. The Quarterly Journal of Economics, 139(4), 2037-2082.

Chang, C., & Steinberg, S. (2014). TennCare timeline: Major events and milestones from 1992 to 2014. Memphis, TN: Methodist Le Bonheur Center for Healthcare Economics, University of Memphis.

Chang, C. F., & Steinberg, S. C. (2016). Tenncare timeline: Major events and milestones from 1992 to 2016. Methodist Le Bonheur Center for Healthcare Economics, University of Memphis.

Chetty, R., Stepner, M., Abraham, S., Lin, S., Scuderi, B., Turner, N., Bergeron, A., & Cutler, D. (2016). The association between income and life expectancy in the United States, 2001-2014. Jama, 315(16), 1750-1766.

Couillard, B. K., Foote, C. L., Gandhi, K., Meara, E., & Skinner, J. (2021). Rising geographic disparities in US mortality. Journal of Economic Perspectives, 35(4), 123-146.

Council of Economic Advisers, Executive Office of the President. (2025). Economic Report of the President (2024).

Currie, J., & Gruber, J. (1996). Health insurance eligibility, utilization of medical care, and child health. The Quarterly Journal of Economics, 111(2), 431-466.

Dague, L., DeLeire, T., & Leininger, L. (2017). The effect of public insurance coverage for childless adults on labor supply. American Economic Journal: Economic Policy, 9(2), 124-154.

Dague, L., & Ukert, B. (2024). Pandemic-era changes to medicaid enrollment and funding: Implications for future policy and research. Journal of Policy Analysis and Management, 43(4), 1229-1259.

DeLeire, T. (2019). The Effect of Disenrollment from Medicaid on Employment, Insurance Coverage, and Health and Health Care Utilization☆. In Health and Labor Markets (pp. 155-194). Emerald Publishing Limited.

Deza, M., Lu, T., Maclean, J. C., & Ortega, A. (2024). Losing medicaid and crime.

Duggan, M., Goda, G. S., & Jackson, E. (2019). The effects of the Affordable Care Act on health insurance coverage and labor market outcomes. National Tax Journal, 72(2), 261-322.

Duggan, M., Gupta, A., & Jackson, E. (2022). The impact of the Affordable Care Act: evidence from California's hospital sector. American Economic Journal: Economic Policy, 14(1), 111-151.

Farrar, I., Eichenthal, D., Coleman, B., & Reese, C. (2007). TennCare reform, one year later. Robert Wood Johnson Foundation.

Finkelstein, A. (2007). The aggregate effects of health insurance: Evidence from the introduction of Medicare. The Quarterly Journal of Economics, 122(1), 1-37.

Finkelstein, A., Mahoney, N., & Notowidigdo, M. J. (2018). What does (formal) health insurance do, and for whom? Annual Review of Economics, 10(1), 261-286.

Finkelstein, A., & McKnight, R. (2008). What did Medicare do? The initial impact of Medicare on mortality and out of pocket medical spending. Journal of Public Economics, 92(7), 1644-1668.

Finkelstein, A., Miller, S., & Baicker, K. (2024). The Effect of Medicaid on Crime: Evidence from the Oregon Health Insurance Experiment.

Finkelstein, A., Notowidigdo, M. J., Schilbach, F., & Zhang, J. (2024). Lives vs. livelihoods: The impact of the great recession on mortality and welfare.

Finkelstein, A., & Taubman, S. (2015). Randomize evaluations to improve health care delivery. Science, 347(6223), 720-722.

Finkelstein, A., Taubman, S., Wright, B., Bernstein, M., Gruber, J., Newhouse, J. P., Allen, H., Baicker, K., & Oregon Health Study Group, t. (2012). The Oregon health insurance experiment: evidence from the first year. The Quarterly Journal of Economics, 127(3), 1057-1106.

Finlay, K., & Genadek, K. R. (2021). Measuring all-cause mortality with the Census Numident file. American journal of public health, 111(S2), S141-S148.

Frean, M., Gruber, J., & Sommers, B. D. (2017). Premium subsidies, the mandate, and Medicaid expansion: Coverage effects of the Affordable Care Act. Journal of health economics, 53, 72-86.

Gaffney, A., Himmelstein, D. U., & Woolhandler, S. (2025). Projected effects of proposed cuts in federal Medicaid expenditures on Medicaid enrollment, uninsurance, health care, and health. *Annals of Internal Medicine*.

Garthwaite, C., Graves, J. A., Gross, T., Karaca, Z., Marone, V. R., & Notowidigdo, M. J. (2019). All Medicaid expansions are not created equal: the geography and targeting of the Affordable Care Act.

Garthwaite, C., Gross, T., & Notowidigdo, M. J. (2014). Public health insurance, labor supply, and employment lock. The Quarterly Journal of Economics, 129(2), 653-696.

Garthwaite, C., Gross, T., & Notowidigdo, M. J. (2018). Hospitals as insurers of last resort. American Economic Journal: Applied Economics, 10(1), 1-39.

Goldin, J., Lurie, I. Z., & McCubbin, J. (2021). Health insurance and mortality: Experimental evidence from taxpayer outreach. The Quarterly Journal of Economics, 136(1), 1-49.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. Journal of econometrics, 225(2), 254-277.

Goodman-Bacon, A. (2021). The long-run effects of childhood insurance coverage: Medicaid implementation, adult health, and labor market outcomes. American Economic Review, 111(8), 2550-2593.

Gottlieb, J. D., Polyakova, M., Rinz, K., Shiplett, H., & Udalova, V. (2025). The Earnings and Labor Supply of US Physicians. The Quarterly Journal of Economics, qjaf001.

Grossman, M. (1972). On the concept of health capital and the demand for health. *Journal of Political economy*, *80*(2), 223-255.

Grossman, M. (2000). The human capital model. In Handbook of health economics (Vol. 1, pp. 347-408). Elsevier.

Gruber, J., & Madrian, B. C. (1994). Health insurance and job mobility: The effects of public policy on job-lock. ILR Review, 48(1), 86-102.

Gruber, J., & Madrian, B. C. (2002). Health insurance, labor supply, and job mobility: A critical review of the literature.

Gruber, J., & Simon, K. (2008). Crowd-out 10 years later: Have recent public insurance expansions crowded out private health insurance? Journal of health economics, 27(2), 201-217.

Gruber, J., & Sommers, B. D. (2019). The Affordable Care Act's effects on patients, providers, and the economy: what we've learned so far. Journal of Policy Analysis and Management, 38(4), 1028-1052.

Gruber, J., & Sommers, B. D. (2020). Paying for Medicaid—state budgets and the case for expansion in the time of coronavirus. New England Journal of Medicine, 382(24), 2280-2282.

Hadley, J. (2003). Sicker and poorer—The consequences of being uninsured: A review of the research on the relationship between health insurance, medical care use, health, work, and income. Medical Care Research and Review, 60(2_suppl), 3S-75S.

Ham, J. C., & Ueda, K. (2021). The employment impact of the provision of public health insurance: A further examination of the effect of the 2005 TennCare contraction. Journal of Labor Economics, 39(S1), S199-S238.

Heavrin, B. S., Fu, R., Han, J. H., Storrow, A. B., & Lowe, R. A. (2011). An evaluation of statewide emergency department utilization following Tennessee Medicaid disenrollment. Academic Emergency Medicine, 18(11), 1121-1128.

Hendren, N., & Sprung-Keyser, B. (2020). A unified welfare analysis of government policies. The Quarterly Journal of Economics, 135(3), 1209-1318.

Kaestner, R., Garrett, B., Chen, J., Gangopadhyaya, A., & Fleming, C. (2017). Effects of ACA Medicaid expansions on health insurance coverage and labor supply. Journal of Policy Analysis and Management, 36(3), 608-642.

Kaiser Family Foundation. (2025, May 14). Tracking the Medicaid Provisions in the 2025 Reconciliation Bill | KFF. KFF. https://www.kff.org/tracking-the-medicaid-provisions-in-the-2025-budget-bill/

Keeler, E. B., Brook, R. H., Goldberg, G. A., Kamberg, C. J., & Newhouse, J. P. (1985). How free care reduced hypertension in the health insurance experiment. Jama, 254(14), 1926-1931.

Kronick, R. (2009). Health insurance coverage and mortality revisited. *Health services research*, *44*(4), 1211-1231.

Levy, H., & Buchmueller, T. C. (2025). The Impact of Health Insurance on Mortality. Annual Review of Public Health, 46.

Levy, H., & Meltzer, D. (2008). The impact of health insurance on health. Annu. Rev. Public Health, 29(1), 399-409.

Lyman, R. (2004, November 20). Once a Model, a Health Plan Is Endangered. The New York Times. https://www.nytimes.com/2004/11/20/us/once-a-model-a-health-plan-is-endangered.html

Maclean, J. C., Tello-Trillo, S., & Webber, D. (2023). Losing insurance and psychiatric hospitalizations. Journal of Economic Behavior & Organization, 205, 508-527.

Madrian, B. C. (1994). Employment-based health insurance and job mobility: Is there evidence of job-lock? The Quarterly Journal of Economics, 109(1), 27-54.

Martin, D. (2011, April 5). Ned McWherter, Former Tennessee Governor, Dies at 80. The New York Times. https://www.nytimes.com/2011/04/05/us/05mcwherter.html

McWilliams, J. M., Meara, E., Zaslavsky, A. M., & Ayanian, J. Z. (2009). Medicare spending for previously uninsured adults. Annals of internal medicine, 151(11), 757-766.

McWilliams, J. M., Zaslavsky, A. M., Meara, E., & Ayanian, J. Z. (2004). Health insurance coverage and mortality among the near-elderly. Health Affairs, 23(4), 223-233.

Michael McWilliams, J. (2009). Health consequences of uninsurance among adults in the United States: recent evidence and implications. The Milbank Quarterly, 87(2), 443-494.

Moghtaderi, A., Pines, J., Zocchi, M., & Black, B. (2020). The effect of Affordable Care Act Medicaid expansion on hospital revenue. Health Economics, 29(12), 1682-1704.

Myerson, R. M., Tucker-Seeley, R. D., Goldman, D. P., & Lakdawalla, D. N. (2020). Does Medicare coverage improve cancer detection and mortality outcomes? Journal of Policy Analysis and Management, 39(3), 577-604.

Newhouse, J. P. (1993). Free for all?: lessons from the RAND health insurance experiment. In: Harvard University Press.

Nikpay, S. (2022). The Medicaid windfall: Medicaid expansions and the target efficiency of hospital safety-net subsidies. Journal of Public Economics, 208, 104583.

Roberts, E. T., Phelan, J., Schwartz, A. L., Meara, E. R., Ruggiero, D., Estenson, L., ... & Figueroa, J. F. (2025). Loss of Subsidized Drug Coverage and Mortality Among Low-Income Medicare Beneficiaries. *The New England journal of medicine*, *392*(20).

Ruggles, S., Genadek, K., Goeken, R., Grover, J., & Sobek, M. (2015). Integrated public use microdata series. (No Title).

Sommers, B. D. (2017). State Medicaid expansions and mortality, revisited: a cost-benefit analysis. American Journal of Health Economics, 3(3), 392-421.

Sommers, B. D., Baicker, K., & Epstein, A. M. (2012). Mortality and access to care among adults after state Medicaid expansions. New England Journal of Medicine, 367(11), 1025-1034.

Sommers, B. D., Gawande, A. A., & Baicker, K. (2017). Health insurance coverage and health—what the recent evidence tells us. In (Vol. 377, pp. 586-593): Massachusetts Medical Society.

Sommers, B. D., Long, S. K., & Baicker, K. (2014). Changes in mortality after Massachusetts health care reform: a quasi-experimental study. Annals of internal medicine, 160(9), 585-593.

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. Journal of econometrics, 225(2), 175-199.

Tello-Trillo, D. S. (2021). Effects of losing public health insurance on preventative care, health, and emergency department use: Evidence from the TennCare disenrollment. Southern Economic Journal, 88(1), 322-366.

Trillo, S. T., Ghosh, A., Simon, K., & Maclean, J. C. (2015). Losing Medicaid: What happens to hospitalizations?

U.S. Government Accountability Office. "Medicaid and Children's Health Insurance: Disenrollments after COVID-19 Varied across States and Populations." Medicaid and Children's Health Insurance: Disenrollments After COVID-19 Varied Across States and Populations.

Vivalt, E., Rhodes, E., Bartik, A. W., Broockman, D. E., & Miller, S. (2024). The employment effects of a guaranteed income: Experimental evidence from two US states.

Wherry, L. R., & Miller, S. (2016). Early coverage, access, utilization, and health effects associated with the Affordable Care Act Medicaid expansions: a quasi-experimental study. Annals of internal medicine, 164(12), 795-803.

Woolston, G., & Finkelstein, A. (2024). Achieving Medicaid Goals Through Test And Learn: The Role Of RCTs. Health Affairs Forefront.

Wyse, A., & Meyer, B. D. (2025). Saved by Medicaid: new evidence on health insurance and mortality from the universe of low-income adults.

Yelowitz, A. S. (1995). The Medicaid notch, labor supply, and welfare participation: Evidence from eligibility expansions. The Quarterly Journal of Economics, 110(4), 909-939.

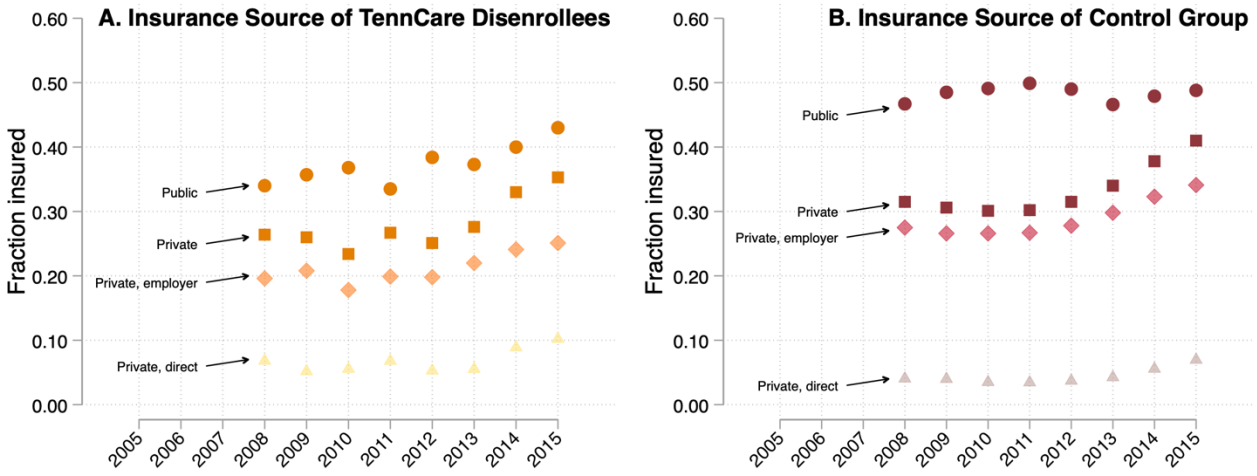
Chronicling the Loss of Public Health Insurance: Evidence from the TennCare Disenrollees

Appendix

Sam Arenberg University of Houston
Sam Stripling

Once considered a model for national health insurance reform, Tennessee's Medicaid program, TennCare, later experienced one of the largest cuts to public health insurance in US history. The severe and sudden curtailing of Medicaid eligibility for a vulnerable population may have led to both short and long-term impacts to their health insurance, labor market, and health outcomes. Although the "TennCare disenrollment" has been studied in the past, never has the set of disenrollees been specified and tracked over a long period of time, as we do in this paper by linking administrative enrollment records from CMS to large household surveys from the Census Bureau. Our unique dataset allows us to pinpoint individuals who lost TennCare eligibility and trace their long-term outcomes. We first show that nearly half of the approximately 170,000 TennCare disenrollees were still uninsured several years after the disenrollment. Although many disenrollees obtained insurance through an employer, we do not find increases in labor force participation or employment among the disenrolled. We do find, however, evidence that employees increased hours worked, ostensibly to qualify for health insurance with their employer. Finally, we consider the health outcomes of the disenrollees, namely self-reported disability and mortality. This investigation yields mixed results. While disability shows a null result, we find statistically significant impacts on long-run mortality. Upon a more comprehensive analysis of public health insurance on mortality, however, we refrain from a determination, as our work warrants additional research into the relationship between public health insurance and health. Collectively, our results are largely consistent with the experimental literature but partially contrast with the quasi-experimental literature on the impacts of health insurance. Our findings come at a time when Medicaid faces budgetary challenges, and we conclude our paper by discussing several lessons from the TennCare experience that may inform current policy discussions. We also emphasize that our template can be applied to study other changes in access to Medicaid, such as the unwinding of pandemic provisions or large future Medicaid cuts, should they occur.

App. Fig. 1: Insurance Status by Type of Insurance



App. Fig. 2: Event Studies