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DID MEDICAID EXPANSION IMPROVE HEALTH? NINE YEARS OF FOLLOW-UP DATA SHOW LIMITED IMPACT

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ABSTRACT

The Affordable Care Act's expansion of Medicaid is among the most significant and most studied health policies in recent US history. However, the existing literature has mainly focused on (a) the policy's effect on health coverage and access to care, rather than on health outcomes, and (b) short-run effects (one to three years). Little is known of the effects of Medicaid expansion on health outcomes during the COVID-19 pandemic. Using difference-in-differences and event study approaches with 2011–22 data from the Behavioral Risk Factor Surveillance System, we examine the long-run effects (up to nine years posttreatment) of expansion on the self-assessed health of adults targeted by the reform. Although we detect some evidence of short-run improvements in mental health and declines in health-related activity limitations, we find no consistent evidence of durable health gains. Results are generally robust to a wide range of specifications and sample definitions, including subgroup analyses of near-elderly individuals and people with chronic conditions.

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Did Medicaid Expansion Improve Health?
Nine Years of Follow-Up Data Show Limited Impact

1. Introduction

One of the central goals of the Affordable Care Act (ACA) was to improve the health of low-income adults without dependents, a population at high risk of being uninsured or underinsured. The primary mechanism to achieve this objective was to increase health coverage through the expansion of Medicaid to previously ineligible low-income adults. Since 2014, this expansion of state Medicaid programs has resulted in approximately 19 million new Medicaid recipients (KFF 2019). This increase in Medicaid enrollment is responsible for the majority of the gains in health coverage attributable to the ACA, prompting one expert to remark that the ACA might more aptly be called the Medicaid Expansion Act (Butler 2016). Initial evidence suggested that Medicaid expansion had resulted in health improvements in the targeted population (Sommers et al. 2016; McMorrow et al. 2017; Simon, Soni, and Cawley 2017), particularly improvements in self-assessed mental health. A mere 18 months after the policy's initial implementation, the Obama administration declared Medicaid expansion a success, stating that "expanding coverage through Medicaid improves mental and physical health" (Furman and Fiedler 2015).

In this paper, we examine the posttreatment effects of Medicaid expansion through the COVID-19 pandemic. This examination allows us to shed light on two important policy questions: (a) Did Medicaid expansion deliver durable health benefits to low-income adults, rather than temporary improvements, and (b) did Medicaid expansion have protective effects on low-income adults' health during the COVID-19 pandemic? Using 2011–22 data from the Behavioral Risk Factor Surveillance System (BRFSS), we leverage state variation in the decision to expand Medicaid to estimate difference-in-differences and event study models of the policy's effect on the self-assessed health of those targeted to gain coverage. Our outcomes include measures of general, physical, and mental health, as well as health-related limitations. We find no evidence of long-term health improvements attributable to Medicaid expansion up to nine years after its implementation. Following Hoodin, Marton, and Ukert (2022), we supplement the main analysis with a subgroup analysis of individuals with chronic health conditions; we also examine near-elderly individuals who tend to have higher healthcare needs than younger adults. Neither subgroup exhibits long-term gains from Medicaid expansion.

While Medicaid expansion has been studied extensively, the vast majority of studies have focused on the expansion's effects on insurance coverage, access to care, and use of medical services (Guth, Garfield, and Rudowitz 2020). Relatively little is known of Medicaid expansion's effects on direct health outcomes. Moreover, previous research on the effects of Medicaid expansion on self-assessed health has focused on short time horizons (generally one to three years posttreatment) and has found mixed results (Sommers et al. 2015, 2016; Wherry and Miller 2016; McMorrow et al. 2017; Miller and Wherry 2017; Simon, Soni, and Cawley 2017; Mazurenko et al. 2018). Studies that have examined additional years of data also report mixed results (Courtemanche et al. 2018, 2020; Miller and Wherry 2019).

We address several limitations of the existing literature. First, we examine more granular measures of self-assessed health than most previous studies, many of which rely on a single survey question pertaining to general health (usually reported on a "poor" to "excellent" scale). The BRFSS data we analyze contain separate questions pertaining to physical health, mental health, and health-related limitations, adding texture to the analysis. Physical and mental health may be affected differently by patients' gaining health insurance coverage, and the effects may be on different time horizons. Second, we examine more years of postexpansion data than any previous study. We track the effects of Medicaid expansion for nine years after the policy's implementation in most states, allowing potentially noteworthy effects to emerge with a lag. Our sample period includes the COVID-19 pandemic, allowing us to assess whether Medicaid expansion protected the health of low-income adults during the crisis.

Across a wide range of specifications and samples, we find little evidence that Medicaid expansion meaningfully and durably affected the health of those targeted to gain coverage. In some cases, we uncover suggestive evidence of a deterioration in health toward the end of our study period. To the extent health improvements occurred, they appear to have been concentrated in the years immediately following the policy's adoption.

The paper is organized as follows: In section 2, we provide background information on Medicaid expansion and the relevant literature. In section 3, we describe our data and methods, as well as how we construct our analysis sample. In section 4, we present our empirical results, including subgroup analyses and robustness checks. We discuss and contextualize our findings in section 5, and section 6 concludes.

2. Background: Medicaid Expansion and Existing Literature

Medicaid is the largest means-tested social assistance program and the largest health insurer in the United States, covering more than 73 million individuals in 2019 (MACPAC 2022). While historically Medicaid has primarily provided health coverage to low-income children and their parents, pregnant women, the elderly, and people with disabilities, the ACA of 2010 expanded Medicaid eligibility to all adults with incomes below 138 percent of the federal poverty level (FPL).[1] While the law's original intent was to require all states to expand their Medicaid programs, a US Supreme Court ruling in 2012 held that states could not be compelled to implement the policy, effectively making Medicaid expansion a state policy decision.

To date, 41 states (including the District of Columbia) have adopted the ACA's Medicaid expansion. Twenty-six states and the District of Columbia did so by the end of 2014, with seven additional states expanding between 2015 and 2019 and seven more states expanding since 2020 (see table A.1 for details). In 2019, new adult enrollees under the ACA's expansion accounted for approximately 22 percent of total Medicaid enrollment (KFF 2019). The Congressional Budget Office estimated that Medicaid expansion's federal budget impact was $116 billion in 2022, more than one-quarter of all federal Medicaid spending that year (Blase 2022).

The impact of Medicaid expansion has attracted intense scholarly interest, spawning hundreds of studies on a wide range of outcomes (Guth, Garfield, and Rudowitz 2020). Two central facts emerge from this literature: First, Medicaid expansion substantially increased rates of insurance coverage among low-income adults (Courtemanche et al. 2017; Decker, Lipton, and Sommers 2017; Frean, Gruber, and Sommers 2017). Second, newly covered Medicaid enrollees increased their use of healthcare services (Sommers et al. 2016; Wherry and Miller 2016). However, acquiring health coverage or visiting the doctor are ultimately more often inputs into the health production function and are not, generally speaking, valued in and of themselves, unless they generate improvements in health or mitigate health deterioration.

Yet Medicaid expansion's impact on direct measures of health outcomes has attracted relatively little scholarly attention. Of 404 studies on Medicaid expansion published from January 2014 to January 2020 and reviewed by the Kaiser Family Foundation (KFF), only 24 studies

[1] In 2024, 138 percent of the FPL corresponded to approximately $20,780 for an individual.

(approximately 6 percent) examined self-assessed health and 17 studies (approximately 4 percent) considered positive health outcomes. In contrast, 145 studies (approximately 36 percent) focused on access or utilization measures (Guth, Garfield, and Rudowitz 2020).

Previous research on the effects of Medicaid expansion on self-assessed health has found mixed results (Mazurenko et al. 2018). Some studies have documented short-run improvements in self-assessed health related to Medicaid expansion, including declines in psychological distress (McMorrow et al. 2017) and fewer days of self-assessed poor mental health (Simon, Soni, and Cawley 2017), as well as improvements in general health (Sommers et al. 2016; Simon, Soni, and Cawley 2017). Other studies have found no significant changes in self-reported health status (Sommers et al. 2015; Wherry and Miller 2016; Miller and Wherry 2017).

However, none of these studies analyze data after early 2016, less than two and one-half years after Medicaid expansion's implementation. The most convincing evidence of Medicaid's short-run effects on self-assessed health predates the ACA. In 2008, the state of Oregon offered Medicaid coverage to certain low-income adults through a lottery. These experimental results indicated that gaining Medicaid coverage led to a small, marginally significant improvement in mental health after approximately two years; no changes were detected in physical health (Baicker et al. 2013).

It is plausible that changes in population health caused by increases in health insurance coverage may emerge with a lag. It may take time, for example, for enrollees to become familiar with their new coverage, learn to navigate the healthcare system, and establish relationships with providers. Additionally, increased consumption of medical care—particularly preventive services—may not immediately produce better health. Consider a patient who gained Medicaid coverage, visited a provider for a checkup, and was diagnosed with a readily treatable form of early-stage skin cancer. In a counterfactual scenario in which Medicaid was not expanded, the patient did not receive the checkup, and the cancer was allowed to metastasize, the illness may have taken years to manifest in self-assessed measures of health. A few studies have examined a longer posttreatment period after Medicaid expansion. Miller and Wherry (2019) find no evidence of improvements in self-assessed health in the four years postexpansion (through the end of 2017), while Courtemanche et al. (2018) find that improvements in self-assessed health emerged in 2016, and Courtemanche et al. (2020) report gains in self-assessed health in 2017 and 2018. Our analysis substantially extends the posttreatment time horizon by adding data from 2019 to 2022. Our larger time horizon also provides insight into whether Medicaid expansion protected the health of low-income adults during the COVID-19 pandemic.

3. Methods

Data

Our primary data source is the BRFSS, a large cross-sectional annual survey of adults living in the United States that is conducted annually by state health agencies with technical assistance from the Centers for Disease Control and Prevention (CDC). We use data from the 50 states and the District of Columbia.[2] The BRFSS collects an extensive set of individual- and household-level data on self-assessed health, chronic disease, access to care, and sociodemographic characteristics. Because of the system's exceptionally large sample size (surveying about 400,000 people in a

[2] New Jersey and Florida did not collect enough BRFSS data in 2019 and 2021, respectively, to meet the minimum requirements for inclusion in the CDC's public data files. Therefore, for those years, we omit those states from the analysis.

typical year), the BRFSS has been widely used to study the effects of state health-related policy interventions, including Medicaid expansion (Benitez, Creel, and Jennings 2016; Cawley, Soni, and Simon 2018; Valvi, Vin-Raviv, and Akinyemiju 2019). We analyze the period from 2011 to 2022.[3]

Sample definition

To capture individuals targeted by Medicaid expansion, we construct a sample of adults ages 18–64 with incomes below 138 percent of the FPL and with no children living in the household.[4] We focus on residents of US states and the District of Columbia. Our main sample includes approximately 270,000 observations for each outcome.

As previously noted, income as a proportion of the FPL plays a crucial role in defining our sample of interest. The only income-related question available in all years of the BRFSS is a range-based estimate of annual household income. Categories include less than $10,000, $10,000 to $14,999, $15,000 to $19,999, and so on. To estimate each respondent's income as a proportion of the FPL, we follow the *midpoint method* (Moore et al. 2015; Hest 2019). First, we convert the categorical income variable to a continuous measure by taking the midpoint of the relevant range (e.g., a respondent reporting household income of $10,000 to $14,999 is assigned an income of $12,500).[5] We then compare the imputed household income and the household size with the applicable FPL threshold to derive an estimate of household income as a proportion of the FPL.[6]

Timing of Medicaid expansion

We rely on data from the KFF to determine the status of Medicaid expansion in each state over time. We focus on the implementation of expansion (i.e., when new enrollees could begin receiving coverage). In some states, there were substantial lags between the time of the enactment of legislation to expand Medicaid and the time in which the policy entered into force. The focus on implementation is a reasonable choice since we hypothesize that expansion's effect on health is primarily mediated through insurance coverage and increased use of services. We link our BRFSS data to KFF's information about state expansion implementations at the year level; the vast majority of states began their expansions on January 1.

Outcome variables

Our analysis is based on four questions related to general, physical, and mental health, as well as functional limitations, asked annually in the BRFSS:

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[3] We do not include BRFSS waves before 2011 because of substantial changes in the survey's methodology implemented that year. The CDC discourages researchers from combining pre-2011 data with earlier years. For more details, see CDC (2011). Still, our sample provides three pretreatment observations (2011, 2012, and 2013) for the initial cohort of expansion states and an even longer pretreatment period for later expanders.

[4] We restrict the sample to individuals without children in the household to exclude parents who were often (depending on the state) eligible for Medicaid before expansion. In extensions to the main analysis, we lower the income threshold to 100 percent of the FPL and, separately, include respondents with children living in the household.

[5] The highest income category is "$75,000 or more." To apply the midpoint method, we impose an arbitrary cap of $100,000. Therefore, these respondents are assigned an income of ($75,000 + $100,000)/2 = $87,500. This imputation method is unlikely to meaningfully affect our results since our analysis focuses on lower-income households.

[6] As a robustness check, we adopt a more conservative income imputation method using the upper bound of the income categories. A detailed description is provided in section 4. Results are similar.

- Would you say in general that your health is excellent, very good, good, fair, or poor?
- Now, thinking about your physical health, which includes physical illness and injury, for how many days during the past 30 days was your physical health not good?
- Now, thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?
- During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?

These questions have been widely used in surveys and empirical research to capture broad dimensions of well-being and to track changes in population health (Moriarty, Zack, and Kobau 2003; Horn, Maclean, and Strain 2017; Slabaugh et al. 2017; Courtemanche et al. 2020; Department of Health and Human Services 2020). Although measures of self-assessed health are inherently subjective, they are generally predictive of objective health outcomes, including disease prevalence (Wu et al. 2013), high-risk behaviors (Brown et al. 2003; Strine et al. 2005), hospitalization (Nielsen 2016), healthcare utilization (Dominick et al. 2002), and mortality (Heistaro et al. 2001).

In defining our outcome measures, we follow established practice to maximize comparability with previous studies of Medicaid expansion's impact. In particular, we adopt the same definitions as Courtemanche et al. (2020) and Hoodin, Marton, and Ukert (2022). We consider six outcomes:

1. *Good or better health* is defined as a binary variable[7] that takes a value of 1 if the respondent reported good, very good, or excellent health and a value of 0 otherwise.
2. *Very good or excellent health* is defined as a binary variable that takes a value of 1 if the respondent reported very good or excellent health and a value of 0 otherwise.
3. *Excellent health* is defined as a binary variable that takes a value of 1 if the respondent reported excellent health and a value of 0 otherwise.
4. *Days not in good physical health* is defined as a count measure of the number of days the respondent's physical health was not good in the past 30 days.
5. *Days not in good mental health* is defined as a count measure of the number of days the respondent's mental health was not good in the past 30 days.
6. *Days with health-related limitations* is defined as a count measure of the number of days the respondent's poor health interfered with their usual activities in the past 30 days.

Control variables

We include state, year, and month fixed effects in all regressions. State fixed effects control for time-invariant differences across states that may affect health outcomes and be correlated with Medicaid expansion, such as geographic barriers to healthcare access. Year fixed effects account for any changes over time that are common to all states, such as changes in federal regulations or broadly adopted advances in medical technology. We include month fixed effects in recognition of the fact that health outcomes—particularly self-assessed measures—exhibit seasonal patterns. To further mitigate confounding, we control for a set of time-varying state covariates. These covariates include policies aimed at helping low-income individuals and families: the maximum Temporary Assistance for Needy Families (TANF) benefit for a family of three, the state Earned

[7] The dichotomization of Likert scales reduces granularity but enhances interpretability and mitigates measurement error (Baker, Stabile, and Deri 2004).

Income Tax Credit (EITC) as a percentage of the federal EITC, and the effective minimum wage. These data were obtained from the University of Kentucky's Poverty Research database. To capture local economic conditions, we also control for the average annual unemployment rate from the Bureau of Labor Statistics and for personal income per capita from the Bureau of Economic Analysis. All monetary values are expressed in constant dollars using the Consumer Price Index for All Urban Consumers (CPI-U).

Given the repeated cross-sectional nature of the BRFSS, it is not possible to include respondent-level fixed effects. To address this limitation, our main models include respondent-level controls for age, race or ethnicity, sex, marital status, and educational attainment.

Empirical strategy

To determine the effects of Medicaid expansion on self-assessed health, we estimate difference-in-differences and event study models that leverage temporal and geographic variation in the implementation of Medicaid expansion. Our methods produce estimates of the impact of Medicaid expansion by comparing the evolution of outcomes in expansion and nonexpansion states over time. Our difference-in-differences coefficients capture the aggregate effect of Medicaid expansion over the entire posttreatment period, whereas our event study specifications allow us to trace Medicaid expansion's impact on health outcomes over time—an important exercise, since it may take several years for the targeted population to learn about the new policy, enroll, and begin using care. And it may take longer still for increased healthcare utilization to manifest in measures of self-assessed health.

For our difference-in-differences models, we estimate the following regression:

$$Y_{itsm} = \beta_0 + \beta_1 DD_{st} + State_s + Year_t + Month_m + X_{it} + Z_{st} + \varepsilon_{itsm}, \quad (1)$$

where Y_{itsm} is the outcome variable for individual i, residing in state s and interviewed in year t and month m; β_0 is the equation intercept; DD_{st} is the difference-in-differences estimator (i.e., an indicator that takes a value of 1 in expansion states if the observation occurred after expansion was implemented and 0 otherwise); $State_s$ is a time-invariant state effect; $Year_t$ is a state-invariant year effect; $Month_m$ is a fixed effect for the month of interview; X_{it} is a vector of respondent-level controls (e.g., sex, age); Z_{st} is a vector of state-specific economic and policy controls (e.g., unemployment rate); and ε_{ist} is the error term. Under conventional difference-in-differences assumptions, the coefficient of interest, β_1, captures the average causal effect of Medicaid expansion.

For our event study models, we estimate the following regression:

$$Y_{itsm} = \sum_{\tau=-q}^{-1} \gamma_t D_{s\tau} + \sum_{\tau=0}^{p} \delta_t D_{s\tau} + State_s + Year_t + Month_m + X_{it} + Z_{st} + \varepsilon_{itsm}, \quad (2)$$

where, as in equation (1), i denotes individual respondents, s reflects their state of residence, and t and m refer, respectively, to the year and month of interview. Leads or anticipatory effects are indexed by q and p represents lags or posttreatment effects. X_{it} and Z_{st} are vectors of individual controls and state controls, respectively. Event time is measured in years relative to the year Medicaid expansion was implemented in a given state. We designate the last pretreatment year as the reference period.

We estimate equations (1) and (2) using ordinary least squares regression models with heteroskedasticity-robust standard errors clustered at the state level. To obtain estimates representative of state populations, we apply BRFSS sampling weights in all regressions. The number of observations varies across outcomes because of missing data for some respondents.

A causal interpretation of difference-in-differences and event study models relies on the assumption that if expansion states had not expanded Medicaid, their outcomes would have evolved similarly to the outcomes in nonexpansion states. While we cannot test this assumption directly, comparing the pretreatment trends in each group of states can help assess whether this assumption is plausible. Visual inspection of the event study plots allows us to observe whether the outcomes in expansion states diverged from those in nonexpansion states in the years leading up to expansion. This exercise is generally supportive of the parallel trends assumption, providing confidence that our results plausibly reflect the causal impact of Medicaid expansion.

4. Results

Descriptive statistics

Table 1 presents descriptive statistics for the outcome variables, respondent-level controls, and state controls used in our analysis. We calculate statistics separately by expansion/nonexpansion states and pre-/posttreatment period. For continuous variables, we provide means and standard deviations. For categorical variables, we provide proportions.

TABLE 1. Descriptive statistics

	Expansion states		Nonexpansion states	
	Preexpansion	Postexpansion	Preexpansion	Postexpansion
Outcomes				
Good or better health	0.548 [0.498]	0.610 [0.488]	0.487 [0.500]	0.565 [0.496]
Very good or excellent health	0.256 [0.437]	0.302 [0.459]	0.200 [0.400]	0.268 [0.443]
Excellent health	0.085 [0.278]	0.108 [0.310]	0.064 [0.244]	0.095 [0.294]
Days not in good physical health	10.070 [12.139]	8.688 [11.625]	10.828 [12.379]	9.197 [11.863]
Days not in good mental health	8.709 [11.441]	8.446 [11.176]	8.516 [11.528]	8.383 [11.293]
Days with health-related limitations	10.844 [11.949]	9.987 [11.508]	11.644 [12.301]	10.380 [11.754]
Health insurance coverage	0.705 [0.456]	0.851 [0.356]	0.621 [0.485]	0.693 [0.461]
Individual controls				
Age				
18–29 years	0.258	0.320	0.148	0.259
30–39 years	0.090	0.119	0.079	0.101
40–49 years	0.165	0.137	0.191	0.170
50–59 years	0.323	0.270	0.387	0.308
60–64 years	0.163	0.155	0.195	0.161
Sex				
Female	0.496	0.473	0.511	0.487
Male	0.504	0.527	0.489	0.513

TABLE 1 (*continued*)

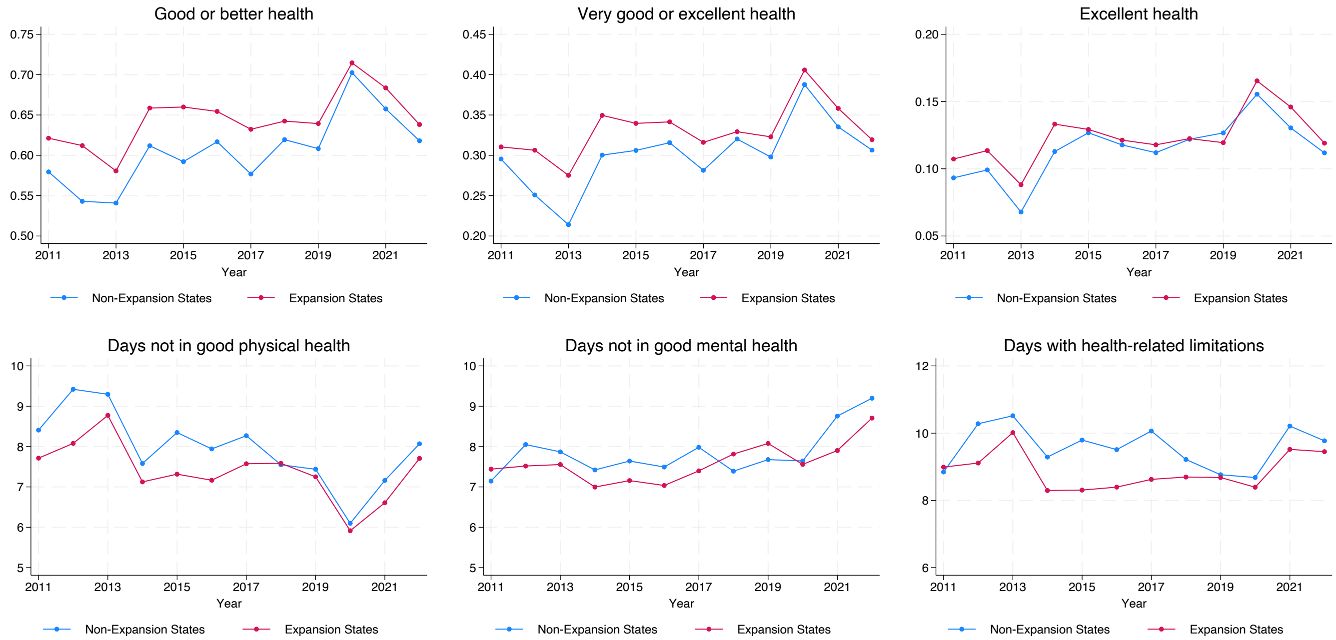
	Expansion states		Nonexpansion states	
	Preexpansion	Postexpansion	Preexpansion	Postexpansion
Race				
White only, non-Hispanic	0.590	0.479	0.532	0.481
Black only, non-Hispanic	0.148	0.135	0.248	0.239
Hispanic	0.166	0.263	0.176	0.224
Other race	0.096	0.122	0.044	0.056
Marital status				
Married/unmarried couple	0.298	0.277	0.348	0.318
Divorced/separated	0.229	0.209	0.263	0.244
Never married	0.423	0.473	0.323	0.387
Widowed	0.049	0.041	0.066	0.051
Educational Attainment				
Less than high school	0.251	0.245	0.297	0.273
High school graduate/some college	0.651	0.643	0.628	0.638
College graduate	0.099	0.112	0.074	0.089
State controls				
Unemployment rate (%)	6.339 [2.248]	4.950 [1.669]	7.999 [1.667]	4.678 [1.395]
Income per capita ($)	56,485 [8,214]	64,901 [11,094]	50,255 [5,615]	56,126 [6,276]
TANF benefit ($)	545 [180]	611 [210]	385 [159]	386 [154]
EITC rate	0.712 [0.093]	0.166 [0.181]	0.024 [0.054]	0.047 [0.145]
Minimum wage ($)	9.57 [0.70]	10.88 [1.90]	9.35 [0.21]	8.73 [0.76]
N[a]	47,829	140,634	23,580	53,767

Note: For nonexpansion states, the preexpansion period is defined as the years before 2014; all later years comprise the postexpansion period. BRFSS sampling weights are applied. Arithmetic means/proportions are shown. For continuous variables, the standard deviation is given in brackets. EITC = Earned Income Tax Credit; TANF = Temporary Assistance for Needy Families.

a. Values represent the total number of observations in each sample; the sample size varies for each variable because of missing data.

Health measures consistently improved over time in both groups of states, though gains were often larger in nonexpansion states. Although our samples from expansion and nonexpansion states were broadly similar demographically, respondents in nonexpansion states tended to be older, were more likely to be Black, and were less likely to have a college degree. Expansion states had higher per capita incomes and more robust social safety nets than did nonexpansion states.

FIGURE 1. Trends in self-assessed health, by year and expansion status



Note: This figure shows the trends in the means of each outcome variable in expansion and nonexpansion states over our study period (2011–2022). Expansion states are defined as those that implemented Medicaid expansion at any point from 2014 to 2022; nonexpansion states include states that have not yet expanded, as well as states that expanded after the end of our study period (i.e., South Dakota and North Carolina, both of which expanded in 2023). BRFSS sampling weights are applied.

Figure 1 plots the unconditional mean of each outcome variable by year and by expansion or nonexpansion group. Across our full study period, residents of nonexpansion states generally report being in worse health than do their counterparts in expansion states, though there is noticeable convergence in later years. We also observe broadly parallel pretrends for most outcomes, which supports a causal interpretation of our analysis.

Effects of Medicaid expansion on health insurance coverage

We begin by estimating a "first-stage" model of the impact of Medicaid expansion on health insurance coverage (see table 2 and figure 2). Consistent with previous research, we find that Medicaid expansion had a strong, lasting effect on the proportion of our sample that reported any kind of health insurance coverage. We note that since the BRFSS does not consistently collect data about Medicaid coverage specifically, we were obliged to use a variable that measures any type of health insurance coverage, irrespective of the source.[8] In our difference-in-differences model (table 2), we find a 7.2 percentage point increase in health coverage, which corresponds to a nearly 11 percent increase relative to the preexpansion mean in expansion states.

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[8] The question asked is, "Do you have any kind of healthcare coverage, including health insurance, prepaid plans such as HMOs, or government plans such as Medicare, or Indian Health Service?"

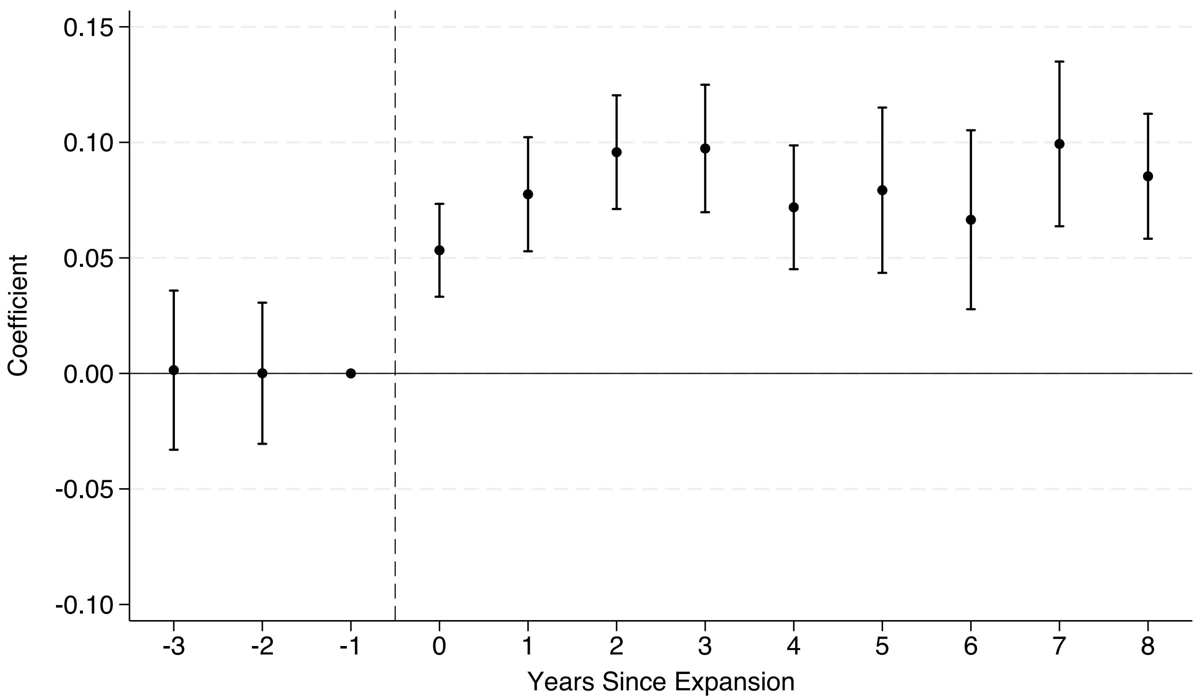
TABLE 2. Difference-in-differences estimates of the effects of Medicaid expansion on health insurance coverage

	Health insurance coverage
Difference-in-differences coefficient	0.072*** (0.011)
Mean (pre)	0.670
N	271,520

Note: The model includes controls for state and year fixed effects, unemployment, per capita income, Temporary Assistance for Needy Families, Earned Income Tax Credit, minimum wage, respondent sex, respondent race or ethnicity, respondent age, respondent marital status, and respondent educational attainment. Behavioral Risk Factor Surveillance System sampling weights are applied. The standard error appears in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

FIGURE 2. Event study plot of the effects of Medicaid expansion on health insurance coverage



Note: The vertical dashed line represents the implementation of Medicaid expansion. The model includes controls for state and year fixed effects, unemployment, per capita income, Temporary Assistance for Needy Families, Earned Income Tax Credit, minimum wage, respondent sex, respondent race or ethnicity, respondent age, respondent marital status, and respondent educational attainment. Behavioral Risk Factor Surveillance System sampling weights are applied. The figure shows 95 percent confidence intervals.

Our event study specification (figure 2) confirms large, sustained increases in health coverage throughout the posttreatment period. We see virtually no pretreatment differences between groups. Given that Medicaid expansion crowded out some private coverage (Leung and Mas 2016; Kaestner et al. 2017), our results imply that the increase in Medicaid enrollment was larger than our estimates suggest. These results give us confidence that our sample is reasonably well-targeted to the population made eligible for Medicaid under the ACA.

Effects of Medicaid expansion on self-assessed health

We now turn to the health effects of Medicaid expansion. Our main difference-in-differences results, presented in table 3, reveal a statistically significant decline of 0.58 days with health-related limitations within the past 30 days. Relative to the preexpansion mean, this finding implies a 6.2 percent improvement in this measure. However, other results are generally economically small (< 2.4 percent of the preexpansion mean) and statistically insignificant. These results are also inconsistent in sign, implying improvements in good or better health and mental health but declines in very good or excellent health, excellent health, and physical health.

TABLE 3. Difference-in-differences estimates of the effects of Medicaid expansion on self-assessed health: Main results

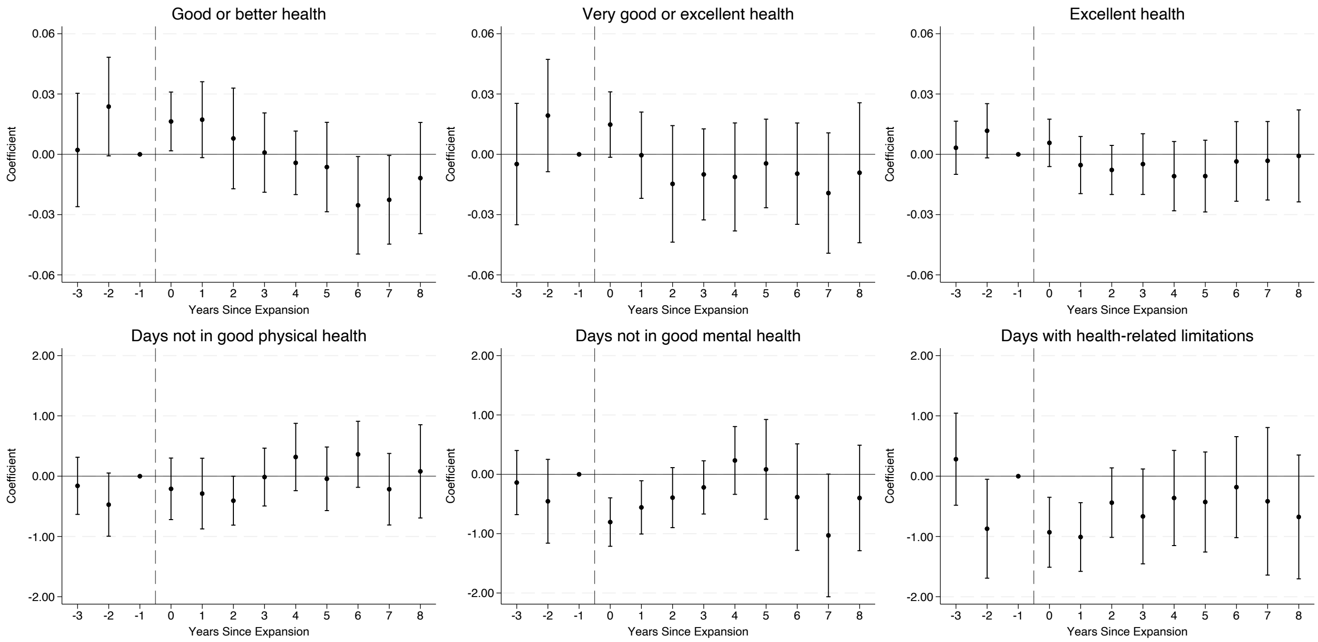
	Good or better health	Very good or excellent health	Excellent health	Days not in good physical health	Days not in good mental health	Days with health-related limitations
Difference-in-differences coefficient	0.001 (0.006)	−0.004 (0.006)	−0.007 (0.004)	0.108 (0.159)	−0.182 (0.208)	−0.575** (0.192)
Mean (pre)	0.613	0.310	0.110	8.087	7.777	9.340
N	273,588	273,588	273,588	267,208	268,354	195,404

Note: The models include controls for state and year fixed effects, unemployment, per capita income, Temporary Assistance for Needy Families, Earned Income Tax Credit, minimum wage, respondent sex, respondent race or ethnicity, respondent age, respondent marital status, and respondent educational attainment. Behavioral Risk Factor Surveillance System sampling weights are applied. The standard errors appear in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Our event study models (plotted in figure 3 and presented in tabular form in table A.2 in the appendix) add granularity to these results. Consistent with findings in existing literature, figure 3 shows that good or better health and mental health improved in the initial years of expansion. Yet we see no indication of lasting positive effects. In the case of good or better health, early improvements were followed by a downward trend; in some later years, we detect a statistically significant decline in good or better health. Similarly, figure 3 indicates that the decline in health-related limitations reported in table 3 was concentrated in the first two years of expansion, with no statistical evidence of enduring improvements. Across all outcomes, we find no evidence of any health improvements beyond the third posttreatment year.

FIGURE 3. Event study plots of the effects of Medicaid on self-assessed health: Main results



Note: The vertical dashed line represents the implementation of Medicaid expansion. The models include controls for state and year fixed effects, unemployment, per capita income, Temporary Assistance for Needy Families, Earned Income Tax Credit, minimum wage, respondent sex, respondent race or ethnicity, respondent age, respondent marital status, and respondent educational attainment. Behavioral Risk Factor Surveillance System sampling weights are applied. The figure shows 95 percent confidence intervals.

Subgroup analyses

To determine whether Medicaid expansion affected the health of those most likely to benefit from health coverage, we conduct supplemental analyses of two subgroups within our main sample: individuals with at least one chronic health condition and individuals 50 to 64 years old (which we term *near-elderly*). Individuals with chronic health conditions who were uninsured before the ACA may have a particularly large capacity for health improvements, since Medicaid coverage may provide access to effective—but formerly unaffordable—treatments (e.g., insulin for diabetic patients). We identify individuals with chronic health conditions using a series of questions included in the BRFSS, taking the same approach as Hoodin, Marton, and Ukert (2022). We include those who report ever having been diagnosed with a heart attack, angina, or coronary heart disease, a stroke, asthma, cancer, chronic obstructive pulmonary disease (emphysema or chronic bronchitis), arthritis, or diabetes.

We report the difference-in-differences estimates for both subgroups in table 4. The results are similar to our main sample. In both subgroups, we detect a meaningful drop in health-related limitations; as a proportion of the preexpansion mean in each subgroup, the magnitude of the coefficient is slightly larger than in our main sample. We find no evidence of changes in our other outcomes related to Medicaid expansion. Event study plots for the chronic health conditions and near-elderly subgroups are presented in figures 4 and 5, respectively. Among those with a chronic health condition, we see an increase in the likelihood of reporting very good or excellent health in the first posttreatment year but no subsequent differences. We also observe short-run improvements in mental health and health-related limitations, but these effects are not statistically significant in later years. Turning to near-elderly respondents, we detect short-run gains in mental health and health-related limitations but no evidence of effects lasting past the second posttreatment year.

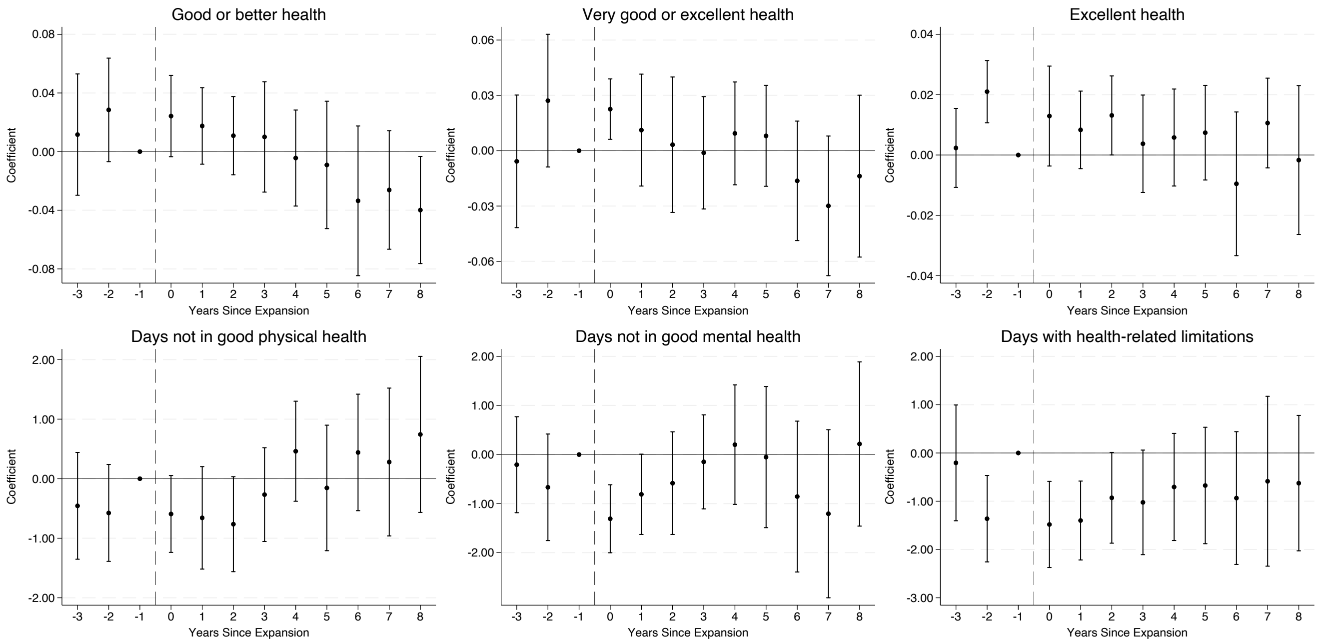
TABLE 4. Difference-in-differences estimates of the effects of Medicaid expansion on self-assessed health: Chronic health conditions and near-elderly subgroups

	Good or better health	Very good or excellent health	Excellent health	Days not in good physical health	Days not in good mental health	Days with health-related limitations
Chronic health conditions subgroup	0.001 (0.008)	0.004 (0.007)	0.002 (0.005)	−0.078 (0.232)	−0.360 (0.334)	−0.734** (0.252)
Mean (pre)	0.443	0.170	0.046	12.003	9.992	11.863
N	172,113	172,113	172,113	167,448	168,399	137,664
Near-elderly subgroup	0.004 (0.009)	0.004 (0.006)	0.001 (0.003)	−0.038 (0.217)	−0.457 (0.269)	−0.775** (0.275)
Mean (pre)	0.488	0.200	0.059	10.766	8.306	11.405
N	164,847	164,847	164,847	160,388	161,226	119,482

Note: The models include controls for state and year fixed effects, unemployment, per capita income, Temporary Assistance for Needy Families, Earned Income Tax Credit, minimum wage, respondent sex, respondent race or ethnicity, respondent age, respondent marital status, and respondent educational attainment. Behavioral Risk Factor Surveillance System sampling weights are applied. The standard errors appear in parentheses.

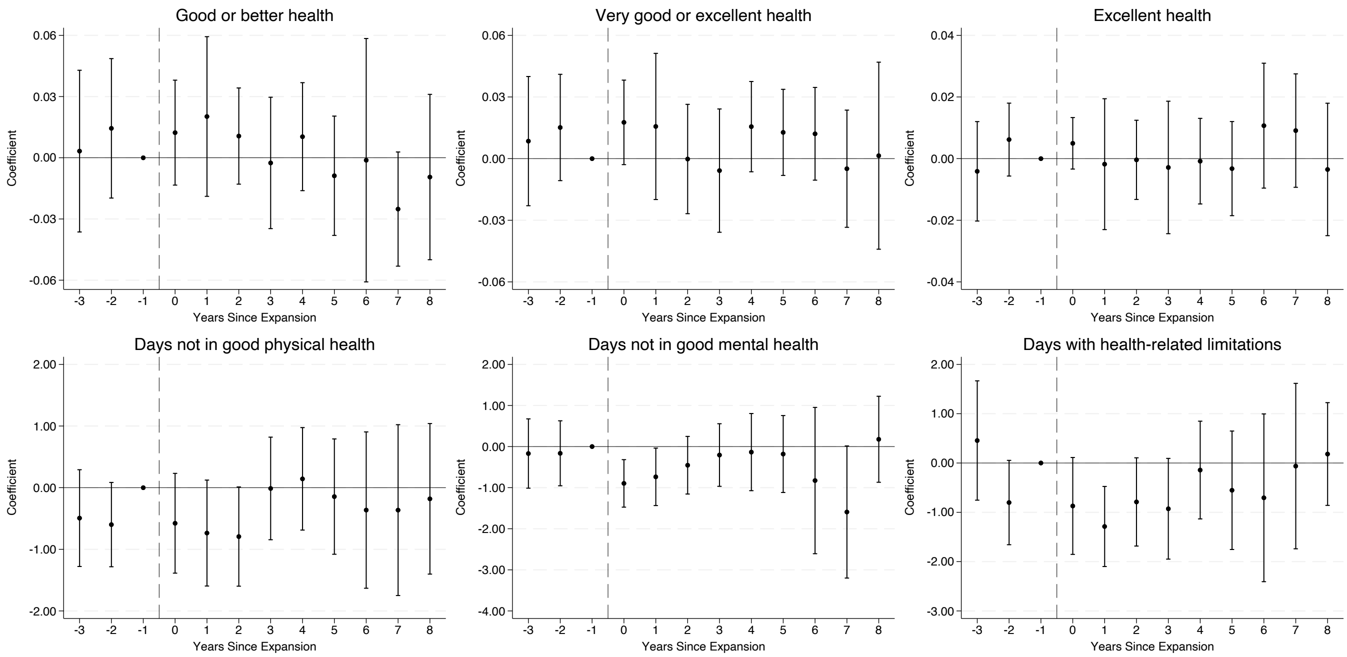
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

FIGURE 4. Event study plots of the effects of Medicaid on self-assessed health: Chronic health conditions subgroup



Note: The vertical dashed line represents the implementation of Medicaid expansion. The models include controls for state and year fixed effects, unemployment, per capita income, Temporary Assistance for Needy Families, Earned Income Tax Credit, minimum wage, respondent sex, respondent race or ethnicity, respondent age, respondent marital status, and respondent educational attainment. Behavioral Risk Factor Surveillance System sampling weights are applied. The figure shows 95 percent confidence intervals.

FIGURE 5. Event study plots of the effects of Medicaid on self-assessed health: Near-elderly subgroup



Note: The vertical dashed line represents the implementation of Medicaid expansion. The models include controls for state and year fixed effects, unemployment, per capita income, Temporary Assistance for Needy Families, Earned Income Tax Credit, minimum wage, respondent sex, respondent race or ethnicity, respondent age, respondent marital status, and respondent educational attainment. Behavioral Risk Factor Surveillance System sampling weights are applied. The figure shows 95 percent confidence intervals.

Robustness checks

We perform a series of checks to assess the sensitivity of our main results. These adjustments fall into two broad categories: changes to the model and changes to the sample. The results of these tests for our difference-in-differences estimates are reported in table 5. Robustness results for our event study models are presented graphically in figures 4, 5, and 6.

Changes to the model

First, to provide a benchmark for our other specifications, we estimate models with state and year fixed effects only. Our difference-in-differences models indicate a 1.7 percentage point decline in very good or excellent health and a 0.1 percentage point decline in excellent health, but these effects are not reflected in the event study models. Moreover, although the difference-in-differences coefficient for days with health-related limitations remains negative and economically meaningful, it is no longer statistically significant when we drop time-varying state controls and respondent characteristics. Other results are consistent with our main specification.

TABLE 5. Difference-in-differences estimates of the effects of Medicaid on self-assessed health: Robustness checks

		Changes to the model			
	Main results	Fixed effects only	Pared-down controls	State linear time trends	State quadratic time trends
Good or better health	0.001 (0.006)	–0.011 (0.006)	0.002 (0.006)	0.017* (0.008)	0.017 (0.010)
Mean (pre)	0.613	0.613	0.613	0.613	0.613
N	273,588	280,182	275,599	273,588	273,588
Very good or excellent health	–0.004 (0.006)	–0.017* (0.008)	–0.004 (0.006)	0.004 (0.009)	0.006 (0.010)
Mean (pre)	0.310	0.310	0.310	0.310	0.310
N	273,588	280,182	275,599	273,588	273,588
Excellent health	–0.007 (0.004)	–0.010* (0.004)	–0.005 (0.003)	–0.003 (0.005)	0.003 (0.008)
Mean (pre)	0.110	0.110	0.110	0.110	0.110
N	273,588	280,182	275,599	273,588	273,588
Days not in good physical health	0.108 (0.159)	0.348 (0.176)	0.080 (0.138)	–0.021 (0.247)	–0.319 (0.297)
Mean (pre)	8.087	8.087	8.087	8.087	8.087
N	267,208	273,420	269,099	267,208	267,208
Days not in good mental health	–0.182 (0.208)	–0.246 (0.248)	–0.293 (0.235)	–0.411** (0.129)	–0.871*** (0.203)
Mean (pre)	7.777	7.777	7.777	7.777	7.777
N	268,354	274,629	270,247	268,354	268,354
Days with health-related limitations	–0.575** (0.192)	–0.320 (0.191)	–0.499* (0.198)	–0.679** (0.250)	–0.872** (0.275)
Mean (pre)	9.340	9.340	9.340	9.340	9.340
N	195,404	199,913	196,702	195,404	195,404

TABLE 5 (*continued*)

		Changes to the sample				
	Main results	Drop early expansion states	Drop late expansion states	Different income bounds	Under 100% of FPL	Include adults with children
Good or better health	0.001	0.002	0.008	0.001	0.003	0.003
	(0.006)	(0.007)	(0.010)	(0.006)	(0.008)	(0.003)
Mean (pre)	0.613	0.605	0.618	0.602	0.595	0.687
N	273,588	238,104	204,273	243,340	154,996	543,955
Very good or excellent health	–0.004	–0.003	0.002	–0.001	0.002	–0.002
	(0.006)	(0.007)	(0.010)	(0.006)	(0.006)	(0.005)
Mean (pre)	0.310	0.303	0.309	0.301	0.301	0.345
N	273,588	238,104	204,273	243,340	154,996	543,955
Excellent health	–0.007	–0.004	–0.007	–0.008	–0.005	–0.001
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.004)
Mean (pre)	0.110	0.106	0.111	0.108	0.114	0.126
N	273,588	238,104	204,273	243,340	154,996	543,955
Days not in good physical health	0.108	–0.033	0.104	0.077	–0.041	0.160
	(0.159)	(0.161)	(0.300)	(0.166)	(0.237)	(0.098)
Mean (pre)	8.087	8.463	7.736	8.300	8.398	6.266
N	267,208	232,414	199,555	237,477	151,025	532,950
Days not in good mental health	–0.182	–0.202	–0.171	–0.164	–0.234	0.188
	(0.208)	(0.216)	(0.330)	(0.215)	(0.252)	(0.153)
Mean (pre)	7.777	8.049	7.325	7.911	8.214	6.635
N	268,354	233,444	200,429	238,552	151,736	534,975
Days with health-related limitations	–0.575**	–0.603**	–0.743*	–0.600**	–0.684*	–0.263*
	(0.192)	(0.190)	(0.281)	(0.189)	(0.257)	(0.099)
Mean (pre)	9.340	9.601	8.922	9.571	9.873	7.497
N	195,404	170,584	144,582	175,337	112,846	362,333

Note: Behavioral Risk Factor Surveillance System sampling weights are applied. The standard errors appear in parentheses. FPL = federal poverty level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

FIGURE 6. Event study plots of the effects of Medicaid on self-assessed health: Robustness checks

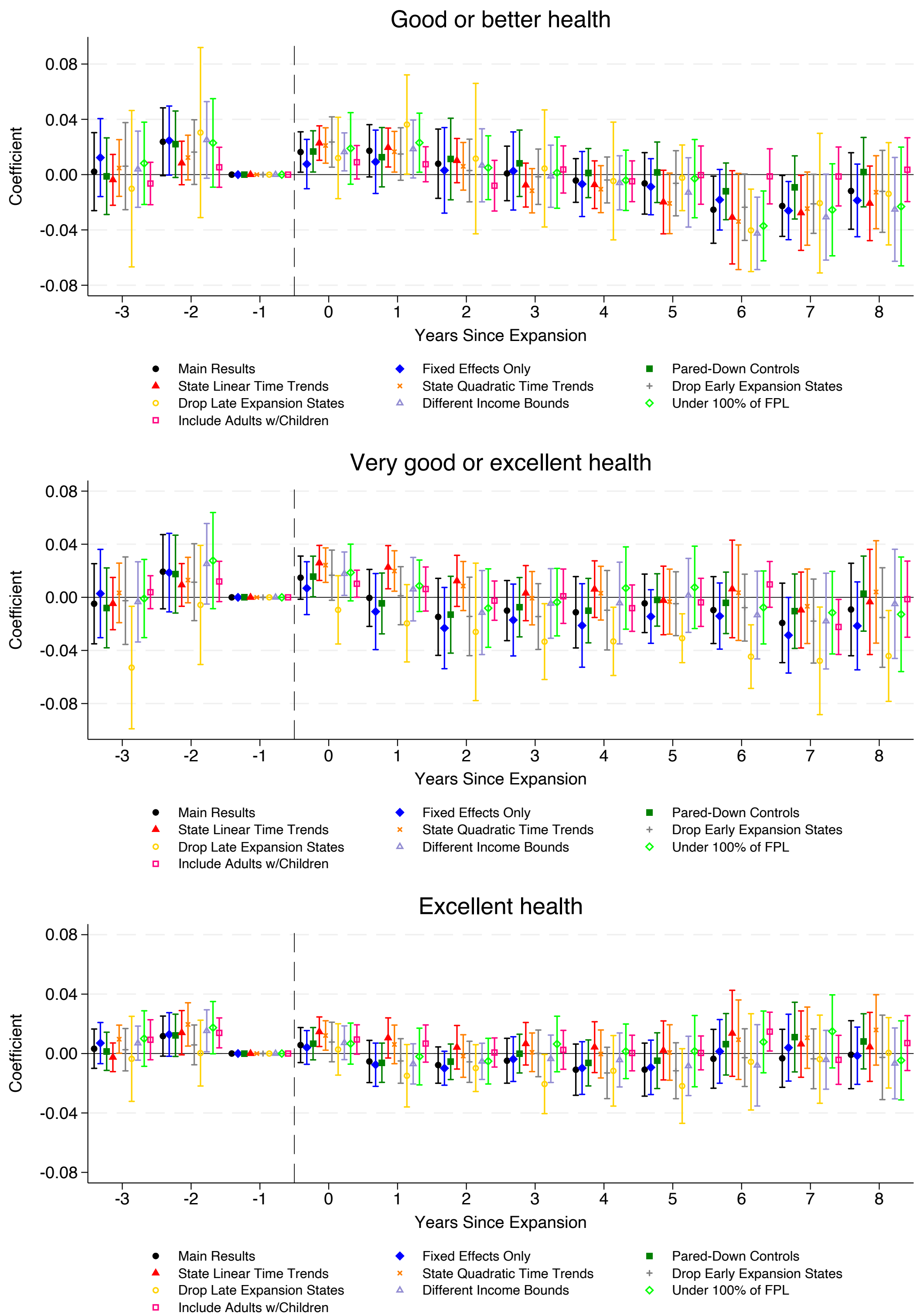
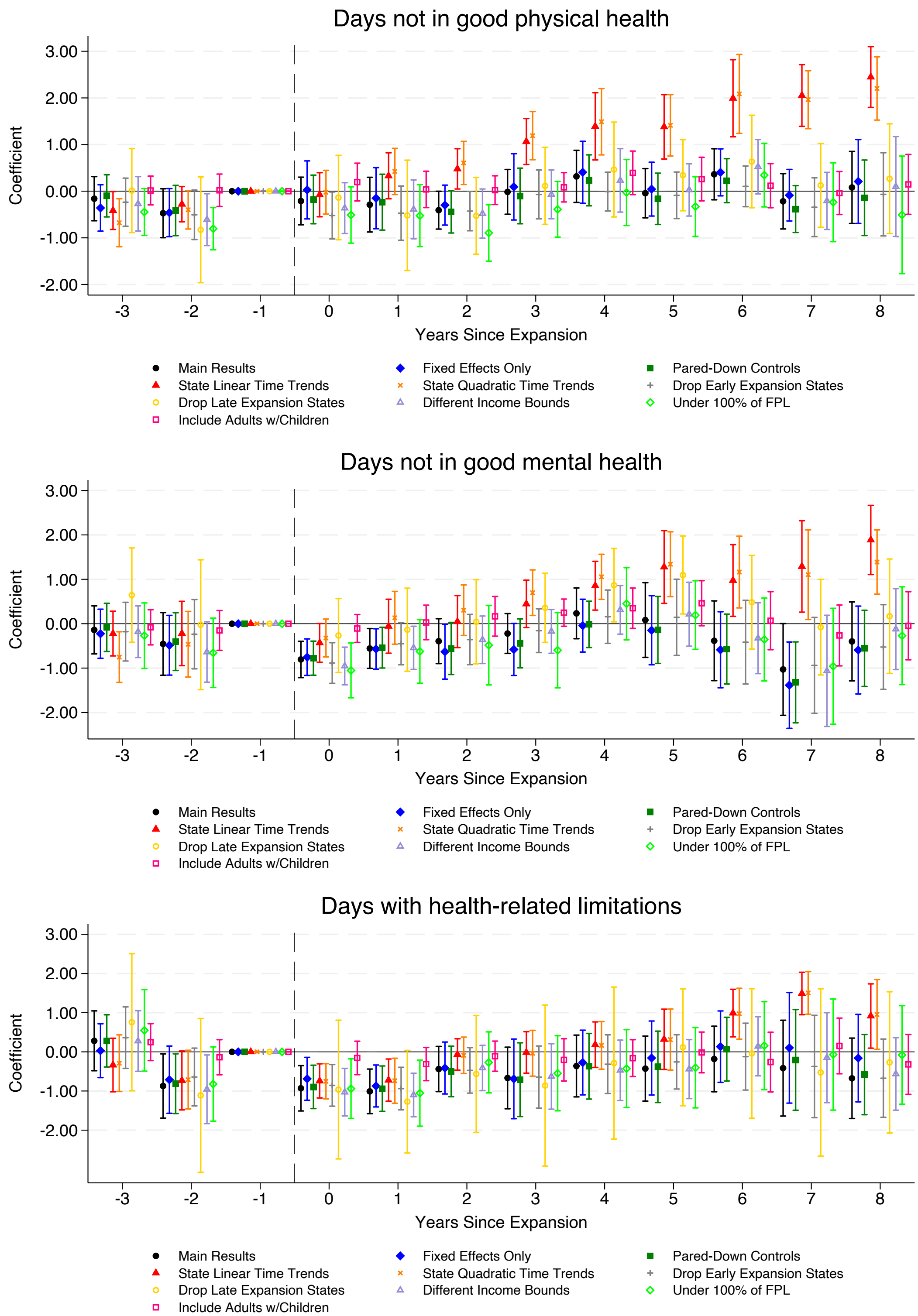


FIGURE 6 (*continued*)



Note: The vertical dashed line represents the implementation of Medicaid expansion. Behavioral Risk Factor Surveillance System sampling weights are applied. The figure shows 95 percent confidence intervals. FPL = federal poverty level.

Second, we estimate models with a reduced set of controls (we refer to this specification as *pared-down controls*). We drop all time-varying state controls (unemployment rate, per capita income, the generosity of TANF benefits, the state EITC rate, and the minimum wage level), as well as potentially endogenous respondent-level controls (educational attainment and marital status). Previous research has shown that Medicaid expansion may have affected labor market behavior (Peng, Guo, and Meyerhoefer 2020) and marital decisions (Hampton and Lenhart 2022). Therefore, including these controls in our main results may attenuate the measured effect by closing causal channels. We retain state and year fixed effects and respondent-level controls for sex, age, and race or ethnicity. Both difference-in-differences and event study estimates are broadly consistent with our main results.

Third, we add state linear time trends and state quadratic time trends, separately, to our models. These specifications allow each state's health outcomes to evolve differently over time. We identify the effect of Medicaid expansion using deviations from these trends. In our difference-in-differences models, adding state linear trends yields a 1.7 percentage point improvement in good or better health and a decline of 0.41 days not in good mental health; both effects are statistically significant. Adding quadratic time trends produces an even larger and highly significant reduction of 0.87 days not in good mental health. Our event study models, however, suggest that the difference-in-differences results are driven by early posttreatment gains that reverse as the time horizon is extended. Event study plots with linear and quadratic time trends both reveal increases in days not in good physical health beginning in the third posttreatment year and building over time. We also detect worsening mental health and increases in days with health-related limitations toward the end of our sample period. Otherwise, results are broadly consistent with our main specification.

Changes to the sample

We consider whether our results are sensitive to changes to our sample.

First, we drop respondents from six jurisdictions—California, Connecticut, Minnesota, New Jersey, Washington, and the District of Columbia—that expanded Medicaid between 2010 and 2013 to some or all of the low-income adults targeted under the ACA reform (Sommers et al. 2013). Since much of the expansion group had been eligible for Medicaid coverage in these states for several years, one would expect the effects of the January 1, 2014, expansion to be smaller in these states than in the other expansion states, where enrollment increased sharply in 2014. Dropping early expansion states has little effect on our results.

Second, we drop respondents from nine states—Alaska, Indiana, Louisiana, Maine, Michigan, Montana, New Hampshire, Pennsylvania, and Virginia—that expanded Medicaid after January 1, 2014, and before the end of our study period, December 31, 2019. Traditional difference-in-differences and event study models can yield biased estimates in the presence of staggered treatment adoption—that is, when different units are treated at different times (Sun and Abraham 2021; Baker, Larcker, and Wang 2022). Consequently, treatment is not staggered in our sample; we consider a single treatment, imposed on January 1, 2014. Event study specifications indicate a decline in good or better health, very good or excellent health, and physical health in later treatment years. Other results are broadly similar to our main sample.

Third, we construct our sample using a different imputation technique for deriving estimates of income as a proportion of the FPL from the BRFSS. In our altered sample, we assign a point estimate of income to each respondent using the upper income bound rather than using the midpoint of the reported income category. If a respondent reported a household income between $10,000 and

$14,999, the midpoint method used in our main results would assign the respondent an income of $12,500. Using the upper income bound, however, we assign the respondent an income of $14,999. While widely used, the midpoint technique could cause some respondents to be misclassified. The income threshold for a single adult to qualify under expanded Medicaid was approximately $17,600 in 2019. Under the midpoint method, a single adult reporting an income of $15,000 to $19,999 would be assigned an imputed income of $17,500 and be included in our analysis sample. Yet the respondent could have earned $19,999 and, therefore, been ineligible for coverage. Using the upper bound of the income range to impute income allows us to determine whether these types of cases are diluting our results. Imposing this more stringent selection criterion causes our sample to shrink by about 12 percent. Results are consistent with our main specification.

Fourth, we restrict our sample to individuals earning less than 100 percent of the FPL (recall that our main sample sets the income threshold at 138 percent, in accordance with Medicaid eligibility rules under the ACA). We chose to reduce the income threshold in response to the fact that in states that opted not to expand their Medicaid programs, the federal government offered subsidized private insurance to those earning above 100 percent of the FPL, while such subsidized coverage was available only to those earning above 138 percent of the FPL in expansion states. As a result, respondents earning between 100 percent and 138 percent of the FPL did not have access to the same type of health insurance coverage in expansion and nonexpansion states. This difference could attenuate the measured effect of Medicaid expansion. Consistent with this hypothesis, limiting the analysis to respondents in poverty generally produces larger point estimates (e.g., in our difference-in-differences models, the number of days with health-related limitations declines by 0.68 days rather than 0.58 days). Overall, results are similar to our main sample.

Fifth, we expand our sample to include adults with children in the household. Although all states permitted some parents and other caregivers to qualify for Medicaid during our sample period, the income thresholds for eligibility were often far lower than 138 percent of the FPL, the threshold established under the expansion. Therefore, many parents became newly eligible for Medicaid under the expansion. Extending our sample to include adults with children in the home allows us to measure this effect. We find a smaller effect on days with health-related limitations, consistent with the fact that including parents may dilute the proportion of treated individuals in our sample. Other results are broadly consistent with our main findings.

State-year panel analysis

As an extension to our main analysis, we construct a state-year panel using BRFSS sampling weights to derive representative estimates.[9] This approach allows us to implement panel methods to address concerns over staggered treatment adoption in conventional difference-in-differences and event study designs. Specifically, we apply the estimator introduced in Callaway and Sant'Anna (2021), which delivers consistent estimates under weaker assumptions than do conventional two-way fixed-effects regressions. In table 6, we report the average treatment effect for all groups across all periods (comparable to our main difference-in-differences models). We also compute dynamic average treatment effects for each period relative to the period first treated, across all cohorts (comparable to our main event study models). We present fixed-effects-only specifications in figure 7 and specifications with our full set of state-level controls (unemployment rate, per capita income, TANF benefits, EITC rate, and minimum wage) in figure 8.

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[9] Because of missing data in 2019, we drop New Jersey from the panel. This leaves us with 500 observations: 50 jurisdictions (including the District of Columbia) observed annually from 2010 to 2019.

TABLE 6. Alternative estimator, Callaway–Sant'Anna method

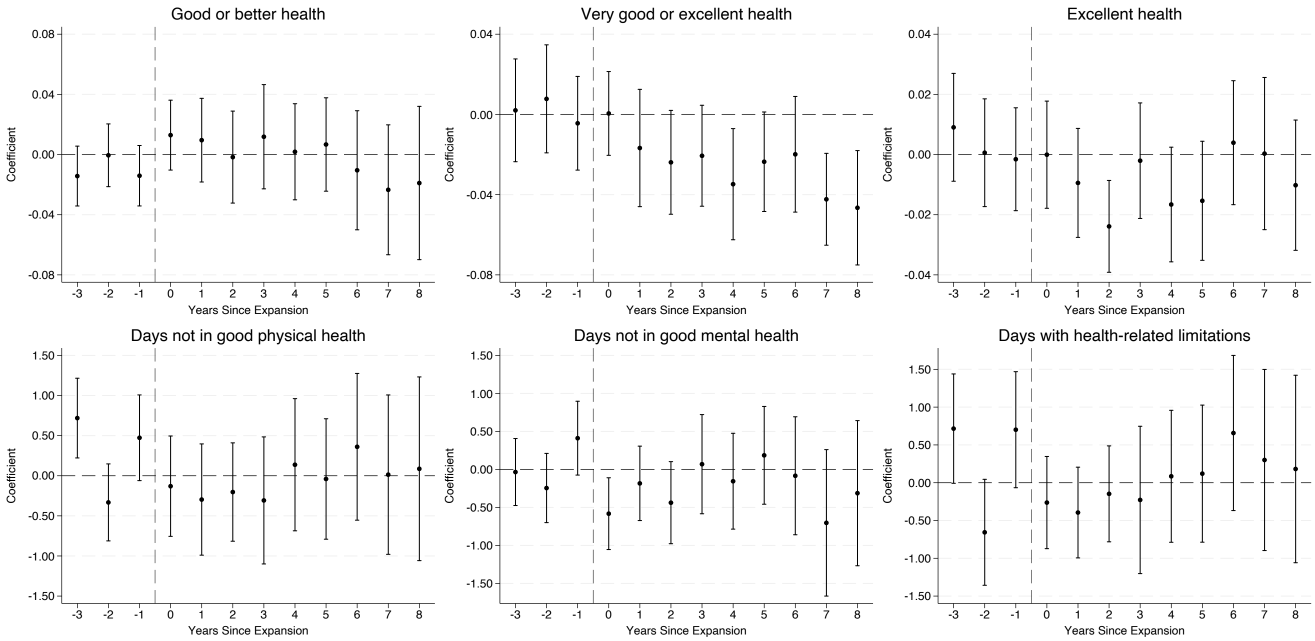
	Good or better health	Very good or excellent health	Excellent health	Days not in good physical health	Days not in good mental health	Days with health-related limitations
Fixed effects only	−0.000 (0.014)	−0.024* (0.010)	−0.008 (0.007)	−0.057 (0.339)	−0.249 (0.270)	0.008 (0.367)
Mean (pre)	0.613	0.305	0.107	8.375	7.810	9.466
N	610	610	610	610	610	610
With state controls	0.025 (0.029)	−0.000 (0.015)	−0.001 (0.013)	−0.181 (0.552)	0.133 (0.426)	0.894 (0.479)
Mean (pre)	0.613	0.305	0.107	8.375	7.810	9.466
N	607	607	607	607	607	607

Note: Data are collapsed to a state/year panel using the Behavioral Risk Factor Surveillance System sampling weights to obtain representative estimates. State controls include unemployment rate, per capita income, Temporary Assistance for Needy Families benefits, Earned Income Tax Credit rate, and minimum wage. We use "not yet treated" states as the comparison group. The standard errors appear in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The results using the Callaway and Sant'Anna (2021) estimator differ from our main findings in several ways. First, the reduction in days with health-related limitations disappears when the Callaway and Sant'Anna estimator is used, regardless of whether state characteristics are accounted for. The fixed-effects-only Callaway and Sant'Anna difference-in-differences specification also suggests a decline in very good or excellent health, a phenomenon we did not find in our main results, but adding state controls eliminates this effect. Turning to our event study models, we find that our fixed-effects-only specifications (figure 7) show a gradual decline in very good or excellent health, as well as a temporary improvement in mental health immediately after expansion.

FIGURE 7. Event study plots of the effects of Medicaid on self-assessed health, Callaway–Sant'Anna method, fixed effects only



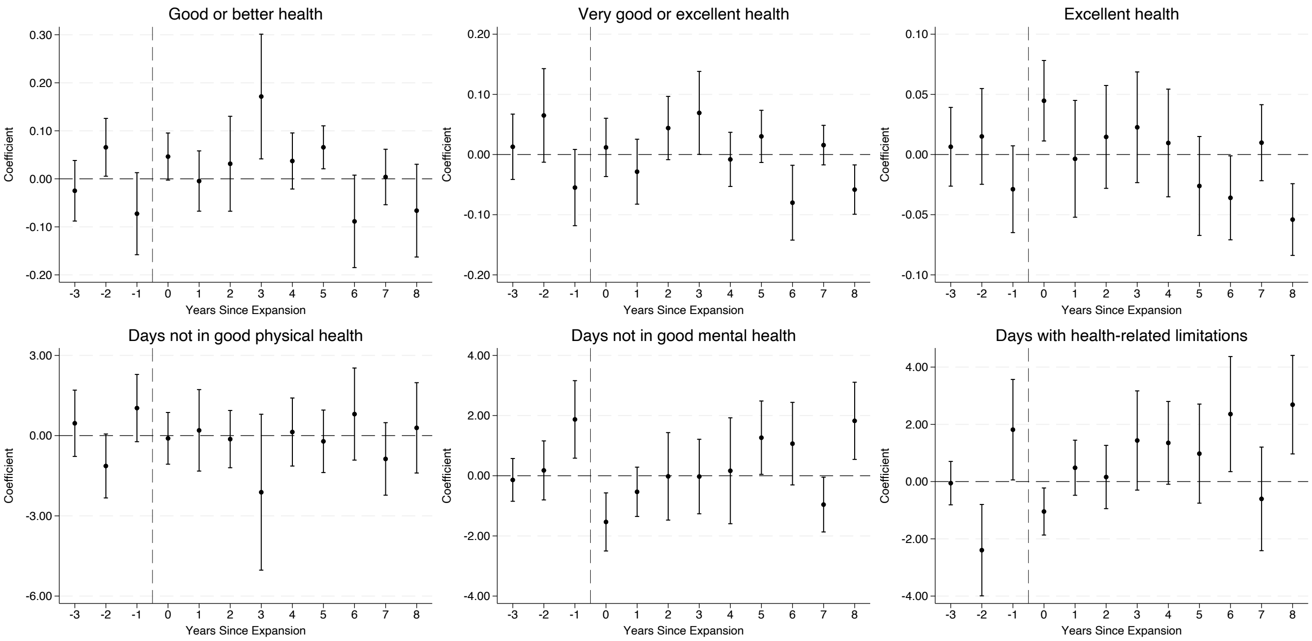
Note: Data are collapsed to a state/year panel using Behavioral Risk Factor Surveillance System sampling weights to obtain representative estimates. All models include state and year fixed effects only. We use "not yet treated" states as the comparison group. The figure shows 95 percent confidence intervals.

Adding state-level controls to our event study models (figure 8) suggests an improvement in good or better health in the early posttreatment period, a possible decline in very good or excellent health and excellent health in later years, and short-run improvements in mental health and health-related limitations that reverse in later years. We find no evidence of changes in physical health.

5. Discussion

Improving the health of low-income, nonelderly adults was a key motivation for expanding Medicaid under the ACA. Our analysis finds some evidence that this goal was achieved in the short run, at least as measured by self-assessments of the targeted population, but gives little indication of long-run health gains. In our preferred difference-in-differences models, the only statistically significant effect is a 6.2 percent decline in the number of days with health-related limitations. We detect no change in general, physical, or mental health. Moreover, the signs of the coefficients are inconsistent across outcomes, sometimes pointing to beneficial effects (in the case of good or better health and mental health) and sometimes implying harmful effects (in the case of very good or excellent health, excellent health, and physical health). We note that some coefficients are estimated imprecisely, allowing clinically meaningful effects—positive and negative—to fall within the 95 percent confidence interval.

FIGURE 8. Event study plots of the effects of Medicaid on self-assessed health (Callaway–Sant'Anna method, with state controls)



Note: Data are collapsed to a state/year panel using Behavioral Risk Factor Surveillance System sampling weights to obtain representative estimates. All models include state-level controls for unemployment rate, per capita income, Temporary Assistance for Needy Families benefits, Earned Income Tax Credit rate, and minimum wage. We use "not yet treated" states as the comparison group. The figure shows 95 percent confidence intervals.

Our event study models provide important insights about the lasting effects of Medicaid expansion. Consistent with previous research (Sommers et al. 2016; McMorrow et al. 2017; Simon, Soni, and Cawley 2017), we find evidence of health gains across multiple outcomes immediately following Medicaid expansion. In particular, respondents in expansion states were significantly more likely to be in good or better health and reported fewer days not in good mental health or with health-related limitations. These effects, however, were temporary. We find no evidence of health improvements lasting beyond the third posttreatment year. These results are largely consistent with the findings of Courtemanche et al. (2020), who examine identical outcomes from the BRFSS up to 2018. In 2017 and 2018—corresponding to the fourth and fifth posttreatment years—the only detectable health improvement reported in the study is an increase in the probability of being in excellent health.

Of note, we find no consistent evidence that Medicaid expansion helped to protect the self-assessed health of low-income adults during the COVID-19 pandemic.

The subgroup analyses of respondents with a chronic health condition and, separately, near-elderly adults provide no consistent evidence that individuals with high health needs benefited disproportionately from Medicaid expansion. Similar to the full sample, both subgroups show short-run improvements in mental health and reductions in health-related limitations. These findings are consistent with Hoodin, Marton, and Ukert (2022), who speculate that the chronically ill may require substantially more health investments to improve their health than those with a higher baseline level of health.

Intuitively, our results are somewhat perplexing. Before the ACA, the population targeted by Medicaid expansion had high rates of uninsurance (Hoffman and Paradise 2008) and frequently faced delays or financial barriers to accessing care (Lasser, Himmelstein, and Woolhandler 2006). Moreover, studies have shown that Medicaid expansion was successful in increasing health insurance coverage and improving access to medical services (Guth, Garfield, and Rudowitz 2020). Our first-stage results confirm this effect: Medicaid expansion led to marked increases in health insurance coverage in our sample. Why did Medicaid expansion fail to produce lasting improvements in self-assessed health?

To explain and contextualize our findings, we consider several factors that may attenuate the effect of Medicaid expansion on health: crowd-out, moral hazard, spillover effects, and the value of health insurance. We discuss each in turn.

Medicaid expansion has been associated with substantial crowd-out of private insurance coverage (Leung and Mas 2016), possibly exceeding 50 percent (Lennon 2023). Although Medicaid provides more cost-sharing protection (e.g., very low copays) than private insurance, private insurance plans generally offer broader provider networks and easier access to care than Medicaid does (Allen et al. 2021).[10] Consequently, it is possible that our findings obscure distinct effects for different subgroups. On the one hand, those transitioning from private plans to Medicaid may have experienced worse access to care and, over time, increasingly negative health effects. On the other hand, those gaining Medicaid coverage who had previously been uninsured likely experienced improvements in health; these effects may have been concentrated in the early posttreatment period, when new enrollees would have sought treatment for their most serious illnesses. While we cannot isolate these groups in our data, we find this hypothesis plausible.[11] Future research should attempt to disentangle these groups and estimate Medicaid expansion's effect on each.

Another possible factor is moral hazard. Health insurance protections—especially those provided by Medicaid, which require little or no cost sharing—create ex ante moral hazard. In particular, by raising income in the sick state, insurance reduces the value of investments in health (Dave and Kaestner 2009). For example, among newly covered adults, increases in risky behaviors such as smoking and unhealthy eating may have offset gains from improvements in healthcare access and affordability. However, existing literature offers little support for the notion that Medicaid expansion induced substantial moral hazard effects (Simon, Soni, and Cawley 2017; Cotti, Nesson, and Tefft 2019; De 2021), so this mechanism likely does little to drive our results.

Another possibility stems from the fact that only a portion of our sample in expansion states is likely to have joined Medicaid under the ACA. The rest of the sample includes individuals who were uninsured, had private insurance plans, were already on Medicaid under pre-ACA eligibility rules, or obtained health insurance from other government programs. In addition to measuring the effects of Medicaid coverage on newly enrolled beneficiaries, our estimates also capture potential spillover effects on these groups. These spillover effects may have partially offset health gains among newly enrolled beneficiaries. Previous research has documented that public insurance expansions can exacerbate health system strain, potentially preventing other groups from

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[10] Efforts to increase physician supply in Medicaid under the ACA by temporarily enhancing reimbursement rates appear to have had little permanent effect on increasing physician participation in the program (Decker 2018), particularly in specialties like psychiatry (Holgash and Heberlein 2019).

[11] One piece of evidence that cuts against this explanation is that Miller, Johnson, and Wherry (2021) find similar effects of Medicaid expansion on mortality across previously insured and uninsured individuals.

obtaining needed care. These effects include shorter office visits (Garthwaite 2012), longer wait times for appointments (Miller and Wherry 2017), slower ambulance response times (Courtemanche et al. 2019), and longer wait times in emergency rooms (Allen, Gian, and Simon 2022). Therefore, our null findings may reflect the positive health effects of Medicaid expansion on newly insured enrollees, as well as negative effects on spillover groups.

Finally, our results are consistent with experimental evidence suggesting that the effect of health insurance coverage on health, including self-assessed measures of health, is limited. The RAND Health Insurance Experiment, conducted from 1971 to 1982, found that, while lower rates of cost-sharing in insurance contracts led to higher rates of healthcare utilization, there was little evidence that this resulted in better health (Brook, Lohr, and Keeler 2006). Results using two-year posttreatment data from the Oregon Health Insurance Experiment showed no statistically significant improvements in the physical health component score and a quantitatively small and marginally significant improvement in the mental health score (Baicker et al. 2013).

We acknowledge that improving health is not the only goal of health insurance, which also serves as a financial instrument, shielding beneficiaries from large medical expenses and smoothing consumption across time. Moreover, in the United States, where uncompensated care is provided to those who cannot pay for emergency services, insurance also transfers resources to nonrecipient parties, particularly hospitals (Finkelstein, Mahoney, and Notowidigdo 2018). Even in the absence of health improvements, such goals may provide a rationale for expanding health programs to low-income populations.

6. Conclusion

The ACA brought the uninsured rates in the United States to record lows, primarily through the expansion of Medicaid coverage to adults with incomes below 138 percent of the FPL. In 2019, more than 19 million individuals were enrolled in Medicaid because of the expansion. Previous research has found that this reform led to increases in a wide range of outcomes, including access to care, healthcare use, and financial security, but relatively little is known about its impact on direct health measures, especially beyond the short run.

In this study, we examined the effects of Medicaid expansion on self-assessed general, physical, and mental health, as well as health-related limitations, up to nine years after Medicaid expansion's implementation. We find no consistent evidence of lasting health improvements among those targeted by the reform, implying that Medicaid expansion failed to achieve one of its primary objectives. These results can inform ongoing policy discussions related to the future of Medicaid and the broader question of how to promote the well-being of low-income individuals.

Appendix

TABLE A.1. State Medicaid expansion decisions

State	Implementation of Medicaid expansion	Analysis group
Alabama	Has not expanded	Nonexpansion
Florida	Has not expanded	Nonexpansion
Georgia	Has not expanded	Nonexpansion
Kansas	Has not expanded	Nonexpansion
Mississippi	Has not expanded	Nonexpansion
South Carolina	Has not expanded	Nonexpansion
Tennessee	Has not expanded	Nonexpansion
Texas	Has not expanded	Nonexpansion
Wisconsin	Has not expanded	Nonexpansion
Wyoming	Has not expanded	Nonexpansion
Arizona	1/1/2014	Expansion
Arkansas	1/1/2014	Expansion
California	1/1/2014	Expansion
Colorado	1/1/2014	Expansion
Connecticut	1/1/2014	Expansion
Delaware	1/1/2014	Expansion
District of Columbia	1/1/2014	Expansion
Hawaii	1/1/2014	Expansion
Illinois	1/1/2014	Expansion
Iowa	1/1/2014	Expansion
Kentucky	1/1/2014	Expansion
Maryland	1/1/2014	Expansion
Massachusetts	1/1/2014	Expansion
Minnesota	1/1/2014	Expansion
Nevada	1/1/2014	Expansion
New Jersey	1/1/2014	Expansion
New Mexico	1/1/2014	Expansion
New York	1/1/2014	Expansion
North Dakota	1/1/2014	Expansion
Ohio	1/1/2014	Expansion
Oregon	1/1/2014	Expansion
Rhode Island	1/1/2014	Expansion

TABLE A.1 (*continued*)

State	Implementation of Medicaid expansion	Analysis group
Vermont	1/1/2014	Expansion
Washington	1/1/2014	Expansion
West Virginia	1/1/2014	Expansion
Michigan	4/1/2014	Expansion
New Hampshire	8/15/2014	Expansion
Pennsylvania	1/1/2015	Expansion
Indiana	2/1/2015	Expansion
Alaska	9/1/2015	Expansion
Montana	1/1/2016	Expansion
Louisiana	7/1/2016	Expansion
Virginia	1/1/2019	Expansion
Maine	1/10/2019	Expansion
Idaho	1/1/2020	Expansion
Utah	1/1/2020	Expansion
Nebraska	10/1/2020	Expansion
Oklahoma	7/1/2021	Expansion
Missouri	10/1/2021	Expansion
South Dakota	7/1/2023	Nonexpansion
North Carolina	12/1/2023	Nonexpansion

Source: Kaiser Family Foundation.

Note: Expansion decisions are as of July 2024. For the purposes of our analysis, South Dakota and North Carolina are treated as nonexpansion states because their expansions occurred after the end of our study period.

TABLE A.2. Event study of the effects of Medicaid on self-assessed health: Main results

	Years since expansion	Good or better health	Very good or excellent health	Excellent health	Days not in good physical health	Days not in good mental health	Days with health-related limitations
Preexpansion	−3	0.002	−0.005	0.003	−0.160	−0.139	0.282
		(0.014)	(0.015)	(0.007)	(0.236)	(0.269)	(0.380)
	−2	0.024	0.019	0.012	−0.472	−0.454	−0.872*
		(0.012)	(0.014)	(0.007)	(0.261)	(0.351)	(0.408)
	−1	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
Postexpansion	0	0.016*	0.015	0.006	−0.210	−0.805***	−0.930**
		(0.007)	(0.008)	(0.006)	(0.254)	(0.203)	(0.289)
	1	0.017	−0.000	−0.005	−0.289	−0.557*	−1.009***
		(0.009)	(0.011)	(0.007)	(0.292)	(0.223)	(0.284)
	2	0.008	−0.015	−0.008	−0.407*	−0.393	−0.439
		(0.012)	(0.014)	(0.006)	(0.202)	(0.252)	(0.287)
	3	0.001	−0.010	−0.005	−0.014	−0.221	−0.667
		(0.010)	(0.011)	(0.008)	(0.239)	(0.223)	(0.392)
	4	−0.004	−0.011	−0.011	0.318	0.233	−0.362
		(0.008)	(0.013)	(0.009)	(0.278)	(0.284)	(0.393)
	5	−0.006	−0.005	−0.011	−0.044	0.083	−0.428
		(0.011)	(0.011)	(0.009)	(0.263)	(0.419)	(0.413)
	6	−0.025*	−0.010	−0.004	0.363	−0.384	−0.182
		(0.012)	(0.013)	(0.010)	(0.273)	(0.447)	(0.417)
	7	−0.023*	−0.019	−0.003	−0.216	−1.029	−0.417
		(0.011)	(0.015)	(0.010)	(0.295)	(0.515)	(0.609)
	8	−0.012	−0.009	−0.001	0.080	−0.398	−0.676
		(0.014)	(0.017)	(0.011)	(0.385)	(0.442)	(0.511)
	Mean (pre)	0.613	0.310	0.110	8.087	7.777	9.340
	N	273,588	273,588	273,588	267,208	268,354	195,404

Note: Models include controls for state and year fixed effects, unemployment, per capita income, Temporary Assistance for Needy Families, Earned Income Tax Credit, minimum wage, respondent sex, respondent race or ethnicity, respondent age, respondent marital status, and respondent educational attainment. Behavioral Risk Factor Surveillance System sampling weights are applied. The standard errors appear in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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