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THE HIDDEN SUBSIDY OF THE AFFORDABLE CARE ACT

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ABSTRACT

Under the Affordable Care Act, the federal government paid a substantially larger share of the medical costs of newly eligible Medicaid enrollees than of those previously eligible. States could save up to 100 percent of their per-enrollee costs by reclassifying original enrollees into the newly eligible group. We examine whether this fiscal incentive changed states' enrollment practices. We find that Medicaid expansion caused a large decline in the number of beneficiaries enrolled in the original Medicaid population, suggesting widespread reclassifications. In 2019 alone, this phenomenon affected 4.4 million Medicaid enrollees at a federal cost of $8.3 billion. Our results imply that reclassifications inflated the federal cost of Medicaid expansion by 18.2 percent.

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DISCLAIMER

The Hidden Subsidy of the Affordable Care Act

1. Introduction

Medicaid is the single largest source of health insurance in the United States. The program provides coverage to an estimated 85 million people and costs the federal government and states approximately $750 billion per year (Mitchell et al. 2023). The Affordable Care Act (ACA) of 2010 made substantial changes to the program, including permitting states to expand Medicaid eligibility to all nonelderly adults with incomes up to 138 percent of the federal poverty level (FPL), hereafter referred to as the *new adult group*. States that opted to expand Medicaid received enhanced federal matching funds for the new adult group. From 2014 to 2019, states covered a median of about 40 percent of the medical costs for the original Medicaid population,[1] but covered at most 7 percent of the medical costs of enrollees in the new adult group. These provisions implied that the median state could save at least 82.5 percent of its per-enrollee costs by reclassifying members of the original Medicaid population into the new adult group. We define *reclassification* as enrollment in the new adult group when, in the absence of the ACA, the individual would have been a part of the original Medicaid population.[2] Given that Medicaid expenditures represent, on average, about one-fifth of states' general fund expenditures (MACPAC 2017), reclassifications could represent a substantial hidden subsidy from the federal government to the states.

We examine the existence and extent of the ACA's hidden subsidy by investigating the effect of expansion on enrollment in the original Medicaid population and estimating the fiscal impact of these enrollment changes. To quantify possible reclassifications, we examine the change in the original Medicaid population in expansion states relative to nonexpansion states. Drawing on state administrative records from 2014 to 2019 and leveraging variation in the implementation of Medicaid expansion across states and time, we find that Medicaid expansion is associated with an average decline of 9.93 percent in the number of original Medicaid enrollees. In 2019 alone, this figure represents 4.4 million fewer original Medicaid enrollees.

Since a Medicaid beneficiary's reclassification is primarily an administrative matter concerning states' requests for federal reimbursement, Medicaid enrollees would likely have no knowledge of how they were being classified. Additionally, reclassifications would not necessarily affect enrollees' coverage or benefits. In some cases, such reclassifications were permitted under the ACA and subsequent federal rulemaking.[3] In other cases, states may have deliberately reclassified enrollees in violation of federal law. States may also have incorrectly reclassified people into the new adult group because of carelessness or poor training of case managers. Irrespective of their legal status, such reclassifications are financially attractive to states.

Reclassifications, as well as the associated hidden subsidy, had substantial fiscal implications for states and the federal government. Our results indicate that the fiscal impact of Medicaid expansion on the US Treasury would have been substantially lower if the effect we document had

[1] Throughout this paper, we use the term *original Medicaid population* or *original Medicaid enrollees* to refer to people who were eligible for Medicaid under pre-ACA eligibility rules, such as poor children and people with disabilities.

[2] This definition accommodates several forms of reclassification, ranging from deliberate, improper actions by state Medicaid administrators to legitimate shifts in enrollment stemming from natural life cycle events. These possibilities are discussed in more detail in sections 2.3 and 5.

[3] We discuss these dynamics in more detail in section 2.3.

not occurred. Our estimates imply that the federal government distributed $52.9 billion to states from 2014 to 2019 as a result of these reclassifications, distributing $8.3 billion in 2019 alone. On the basis of these results, we revise Congressional Budget Office (CBO) estimates of the federal fiscal impact of Medicaid expansion and find that reclassifications increased federal costs by 18.2 percent (Fritzsche, McNellis, and Vreeland 2019).

While many factors influence Medicaid enrollment, we argue that rival explanations are inadequate to account for our results. Where possible, we subject alternative theories to empirical scrutiny (see sections 5.1 and 5.4). For example, we account for possible state-level changes to income thresholds for Medicaid eligibility, as well as changes in other administrative practices in Medicaid. We discuss competing explanations in greater detail, including other changes to the healthcare system introduced by the ACA such as the availability of premium tax credits for certain low- and middle-income households (see section 5.2). In our view, none of the explanations provide a convincing justification for the enrollment patterns that unfolded from 2014 to 2019. We conclude, therefore, that reclassifications played a key role in shrinking the size of the original Medicaid population in expansion states relative to the size of the population had the reform not occurred.

Previous research based on household surveys shows that Medicaid expansion triggered a robust *woodwork effect*—a large increase in enrollment among already-eligible people "coming out of the woodwork" (Frean, Gruber, and Sommers 2017; see also Hudson and Moriya 2017; Gruber and Sommers 2019; Sacarny, Baicker, and Finkelstein 2022). This increase in awareness of Medicaid eligibility and of the enrollment of already-eligible people suggests that expanding Medicaid would increase the original population relative to a counterfactual where a state does not expand. However, our analysis of state administrative records indicates the opposite effect, suggesting that states engaged in large-scale reclassifications from the original Medicaid population to the new adult group that swamped the magnitude of the woodwork effect. Thus, our results should be considered a lower-bound estimate of reclassifications.

In addition to exposing a previously overlooked fiscal effect of the ACA, our work sheds light on several facts related to Medicaid expansion that have previously not been well understood:

1. Despite larger-than-expected enrollment, the fiscal impact of Medicaid expansion on states has been small (Sommers and Gruber 2017; Gruber and Sommers 2020). In fact, for some states, Medicaid expansion appears to have been a net fiscal benefit (Levy et al. 2020; Simpson 2020). The hidden subsidy we document helped states offset the direct costs of Medicaid expansion.
2. Projections of the size of the new adult group have been greatly exceeded (Blase 2016). Reclassifications from the original Medicaid population to the new adult group, which were not contemplated by forecasters, may be important mechanisms behind these discrepancies.
3. Per-enrollee spending in the new adult group has been substantially higher than expected, and the ratio of per-enrollee spending in the new adult group to that of other nonelderly adults on Medicaid has exceeded actuarial expectations (Truffer et al. 2013, 2018). Our results are consistent with this pattern. Members of the original Medicaid group tend to have higher medical spending than members of the new adult group, so reclassifications from the former to the latter would tend to increase per-enrollee spending in the new adult group.

2. Background and Policy Context

2.1. The Affordable Care Act and Medicaid financing

The expansion of Medicaid, arguably the centerpiece of the ACA's coverage provisions, has been responsible for a substantial decline in the uninsured rate among working-age Americans (Butler 2016). The implications of this expansion for the health system and population health have been extensively studied (Buchmueller, Miller, and Vujicic 2016; Peng 2017; Huh 2021; Zhang and Zhu 2021; Neprash et al. 2021; Miller, Johnson, and Wherry 2021; Nikpay 2022). These studies have added to other work on the effects of eligibility changes in public health insurance programs (De La Mata 2012; Arenberg, Neller, and Stripling 2024). To date, 40 states and the District of Columbia have expanded Medicaid under the ACA. In 2019, 12 million members of the new adult group were enrolled in Medicaid, accounting for about 16.2 percent of the program's total enrollment.

Less is known about the impact of reforms made under the ACA to Medicaid's financing structure. Although Medicaid is operated by the states, the federal government contributes the majority of the program's funding. In 2021, contributions by states accounted for approximately 31 percent of total Medicaid spending; the federal government paid 69 percent. Still, Medicaid represents a large and growing share of state budgets. In 2016, Medicaid accounted for nearly 20 percent of states' general fund expenditures, roughly double the program's share in the early 1990s (MACPAC 2017). Medicaid is, by a wide margin, the most prominent example of fiscal federalism in the United States. Most federal Medicaid dollars are distributed to states on the basis of a formula that provides more assistance to states with low per capita personal income relative to the national average. The Federal Medical Assistance Percentage (FMAP), the share of Medicaid benefit spending reimbursed by the federal government, generally ranges from the statutory minimum of 50 percent to about 77 percent, depending on the state. Over the decades, however, federal rules governing Medicaid funding have grown complex, with special treatment given to certain groups and service categories. The largest deviation from the traditional FMAP structure relates to Medicaid's expansion under the ACA to cover all low-income, nonelderly adults. Expansion states receive an FMAP rate for these enrollees that substantially exceeds the FMAP rate for most other Medicaid-eligible populations.[4]

From 2014 to 2016, the federal government paid 100 percent of the medical costs of the new adult group, declining to 95 percent in 2017, 94 percent in 2018, 93 percent in 2019, and 90 percent in 2020 and remaining at 90 percent in perpetuity. These enhanced federal reimbursement rates, unlike the FMAP rates for most of the remaining Medicaid population, are not dependent on state average income and were designed to ease the fiscal burden on states and increase political support for the ACA.

In 2014, when the new adult group was reimbursed by the federal government at a rate of 100 percent, the FMAP rate for the original Medicaid population ranged from 50 percent to 73 percent, depending on the state; 27 states received FMAP rates below 60 percent. In 2019, despite the FMAP rate for the new adult group declining to 93 percent, the gap between the new adult group's FMAP rate and that of the original population remained large. That year, FMAP rates

[4] Other exceptions to the traditional FMAP rate include enhanced federal matching for family planning services, smoking cessation programs for pregnant women, certain immunizations, and certain women with breast or cervical cancer. These carve outs represent a very small proportion of total Medicaid spending, partly because the eligible populations are narrowly defined and partly because the FMAP rate enhancement is typically small. Therefore, we ignore these nuances for the purposes of our analysis.

ranged from 50 percent to 76 percent, with 26 states receiving FMAP rates below 60 percent. Moreover, since states that have opted to expand Medicaid tend to have higher average incomes than do nonexpansion states, this group also has disproportionately low FMAP rates for its original population, resulting in an even larger spread between the FMAP rate for the new adult group and the FMAP rate for the original Medicaid population.

To put the difference in federal support between the new adult group and the original Medicaid population in perspective, consider that the Coronavirus Aid, Relief, and Economic Security (CARES) Act of 2020, which provided additional Medicaid resources to states during the COVID-19 pandemic, increased the traditional FMAP rate by a mere 6.2 percentage points—roughly one-fifth of the size of the FMAP rate spread established by the ACA.

Previous research has shown that states are responsive to changes in federal Medicaid funding (Grannemann and Pauly 1983). Adams and Wade (2001) find that states succeed in substituting federal funds for state revenues, resulting in a reduction in state tax burdens for Medicaid. Leung (2022) exploits a kink in the match rate formula to estimate that a percentage point increase in the federal Medicaid match raises per-enrollee spending by 3 to 6 percent. Bundorf and Kessler (2022) estimate that the ACA's enhanced FMAP rate led states to increase spending for each original Medicaid enrollee by approximately 15 percent, showing that state Medicaid spending is sensitive to the magnitude of the federal subsidy. We extend this work by examining how states reacted to the unprecedented fiscal incentives to reclassify enrollees embedded within the ACA's Medicaid expansion.

2.2. The woodwork effect

Economists have long recognized and sought to document the spillover effects of reforms to social assistance programs (Bartik 2002; Baicker 2005; Grabowski 2006; McInerney, Mellor, and Sabik 2017; Carey, Miller, and Wherry 2020). Expanding public programs to cover a new group of people tends to increase enrollment among those who were already eligible under the pre-expansion eligibility criteria. This phenomenon—known as the *woodwork* or *welcome mat* effect—may be particularly strong when a program's expansion is widely publicized. Millions of Americans are eligible for Medicaid but are not enrolled in the program (Sommers and Epstein 2011). Although forgoing Medicaid coverage may be a deliberate choice for some individuals, administrative barriers and a lack of awareness of program rules may play a decisive role in many cases. The passage of the ACA, of which the expansion of Medicaid was a core component, generated substantial media coverage and considerable public interest. Many states advertised Medicaid expansion on billboards and in television and radio ads, urging people to check their eligibility (Artiga and Stephens 2013). Moreover, the ACA instituted other policy changes, such as tax incentives to obtain health insurance and measures to streamline the Medicaid application process, that likely contributed to the woodwork effect.

The most reliable evidence of the woodwork effect in Medicaid predates the ACA. Sonier, Boudreaux, and Blewett (2013) estimate that health reforms adopted in Massachusetts in 2006, which align closely to key design features of the ACA, provoked large woodwork effects that substantially increased Medicaid enrollment. Sacarny, Baicker, and Finkelstein (2022) leverage data from the Oregon Health Insurance Experiment, in which Medicaid eligibility was determined by lottery, and calculate a short-run 6 percent increase in child enrollments when adults in the same household gained access to Medicaid.

Several studies have sought to quantify the woodwork effect in the context of the ACA using household survey data, typically drawn from the American Community Survey, an annual,

large-scale survey of US households. Frean, Gruber, and Sommers (2017) estimate that half of the impact on coverage attributable to Medicaid expansion in 2014 and 2015 came from the woodwork effect. The woodwork effect was found to be large in both expansion and nonexpansion states. Hudson and Moriya (2017) find that the ACA induced a large woodwork effect among children. They estimate that 710,000 low-income children gained Medicaid coverage through the woodwork effect in 2014 and 2015. McInerney, Mellor, and Sabik (2021) present evidence of a woodwork effect among seniors who were dually eligible for Medicare and Medicaid; they estimate that the ACA increased Medicaid enrollment in this population by 4.4 percent. Since care provided to the original Medicaid population was reimbursed by the federal government at an FMAP rate of only 50 percent to 77 percent between 2014 and 2019,[5] the size of the woodwork effect is—at least theoretically—a key parameter in estimating the impact of Medicaid expansion on states' budgets. In certain states, these costs were expected to be nontrivial (Price and Saltzman 2013).

However, our analysis of state administrative records suggests that previous research on Medicaid expansion and the woodwork effect, while perhaps an accurate reflection of the gains in Medicaid coverage among previously eligible individuals, should not be used to calculate the state fiscal costs of Medicaid expansion. This discrepancy arises because of trends in how states classified Medicaid enrollees when seeking federal reimbursement for program expenses. We find that many individuals who would otherwise likely have been reported as belonging to the original Medicaid population were reclassified into the new adult group in expansion states.

2.3. Reclassification of enrollees to the new adult group under the ACA

The ACA created several channels for states to shift Medicaid enrollees from being classified in the original Medicaid population (and being reimbursed at the traditional FMAP rate) to being classified in the new adult group (which received the enhanced FMAP rate).

First, adult Medicaid enrollees who were not eligible for full benefits prior to the ACA's passage could be transferred to the new adult group under the ACA; these enrollees include individuals receiving family planning services under waivers granted by the Centers for Medicare and Medicaid Services (CMS) or individuals eligible under special Medicaid rules for the medically needy. States could receive the enhanced FMAP rate for care provided to these individuals.

Second, the ACA created opportunities for individuals to join the new adult group prior to experiencing a health event (e.g., pregnancy or a disabling injury) that would otherwise have made them eligible for the original Medicaid population. For example, a woman may qualify for the new adult group and enroll in Medicaid before becoming pregnant. During her pregnancy, states are allowed to maintain her classification in the new adult group and receive enhanced FMAP rates for her pregnancy-related care. Similarly, enrollees in the new adult group who become disabled may remain in the new adult group. In the counterfactual, where Medicaid expansion had not occurred, many pregnant women and people with disabilities would presumably have joined the original Medicaid population. Instead, Medicaid expansion siphons off some of these enrollees, resulting in lower enrollment in the original Medicaid population and larger federal subsidies to states.

[5] The statutory maximum traditional FMAP rate is 83 percent, but in practice, no state's traditional FMAP rate exceeded 77 percent during the 2014–19 period.

In addition to the mechanisms described above, which were authorized under the ACA, some states may have—knowingly or unknowingly—reclassified enrollees in violation of federal laws and regulations. Medicaid administrative tasks, including eligibility verification, data management, and reporting to CMS, are almost entirely controlled by the states with minimal federal oversight. Moreover, CMS exerts little meaningful pressure on states to correct errors in eligibility classifications or deficiencies in data management practices. A recent report by the Government Accountability Office noted, "While CMS is generally required to disallow, or recoup, federal funds from states for eligibility-related improper payments. . . , it has not done so for decades. . . . [I]n July 2017, CMS issued revised procedures through which it can recoup funds for eligibility errors, beginning in fiscal year 2022" (Yocom 2020). Consequently, during our entire posttreatment period (2014–19), states faced no financial sanctions for eligibility errors.

The failure to properly determine enrollees' eligibility is widespread in Medicaid (Albanese and Blase 2022). Audits of state Medicaid records carried out by the US Department of Health and Human Services provide direct evidence that misclassifications of Medicaid enrollees—including individuals who should be classified in the original Medicaid population but are reported as belonging to the new adult group—occur on a fairly large scale. Investigations conducted in 2018 and 2019 in California, New York, and Colorado (states that expanded Medicaid in 2014) suggest that as many as 28.3 percent of individuals classified as new adult group enrollees may be ineligible (Levinson 2018; Chiedi 2019a, 2019b), a figure that matches closely with our estimates. Using the Payment Error Rate Measurement (PERM) system, CMS estimated in 2019 that improper eligibility determinations accounted for 8 percent of federal Medicaid payments, amounting to approximately $32.3 billion (CMS 2019).

3. Data

We construct a balanced state-level panel of the original Medicaid population from 2006 to 2019. We exclude later years because the COVID-19 pandemic and the government response to the public health crisis substantially affected Medicaid enrollment and altered states' fiscal incentives. Most importantly, states paused their normal eligibility redetermination processes from early 2020 to early 2023, leading to a nationwide surge in Medicaid enrollment. Other temporary policies included a 6.2 percentage point increase in states' traditional FMAP rates, which narrowed the FMAP spread between the traditional FMAP rate and the enhanced FMAP rate. It would be difficult to disentangle the enrollment effects of Medicaid expansion from the effects of these forces. Moreover, we believe the future of Medicaid is more likely to resemble the 2014–19 period than the anomalous pandemic years, so focusing on the pre-pandemic period is likely to yield more valuable insights.

Our data come from two sources. We obtain data for 2006–13 from issue briefs published by the Kaiser Family Foundation. The data were compiled by Health Management Associates, a research and consulting firm, on the basis of internal state Medicaid enrollment records.[6] Our

[6] CMS does not publicly release state-level Medicaid enrollment data for 2006–13. The figures reported represent "point-in-time" monthly Medicaid enrollment counts for June of each year (enrollment for December of each year was also reported but not used in our analysis). Every person with Medicaid coverage was counted as an enrollee with the exception of family planning waiver enrollees and pharmacy plus waiver enrollees. No adjustment was made for other people who were enrolled in Medicaid categories with less than full coverage. Therefore, the enrollment figures include a small number of individuals who are covered by Medicaid only for emergency services or services related to breast and cervical cancer, as well as persons with Medicare and Medicaid dual eligibility enrolled as Qualified Medicare Beneficiaries, Specified Low-Income Medicare Beneficiaries or Qualified Individuals, for whom Medicaid pays a portion of Medicare premiums, copays, and deductibles.

second source of data, which covers 2014–19, is Medicaid enrollment reports submitted by states to the CMS through the Medicaid Budget and Expenditure System.[7] Post-ACA enrollment information is a count of unduplicated individuals enrolled in the state's Medicaid program at any time during each month in the quarterly reporting period. The enrollment data identify the total number of Medicaid enrollees and, for states that have expanded Medicaid, provide specific counts for the number of individuals enrolled in the new adult group.[8] Enrollment figures for the month of June were used for each year analyzed. We define our dependent variable as the natural log of the number of individuals in the original Medicaid population.

The use of two different datasets to track Medicaid enrollment across time is not ideal, since differences in how each dataset is collected and compiled could potentially influence our results. In our case, this concern is compounded by the fact that the endpoints of each dataset coincide with the beginning of treatment for the largest cohort of states. However, we know of no alternative source of publicly available yearly Medicaid enrollment figures at the state level. Moreover, in section 5.4 we perform several empirical tests to determine whether our approach affects the main results; we find no such evidence.

Many factors affect the size of the original Medicaid population. We explore a variety of specifications with a range of state-level covariates that capture differences in Medicaid program rules, political conditions, demographics, and the state of the economy. We account for income eligibility thresholds for key subgroups within the original Medicaid population (children and parents) using data from the Kaiser Family Foundation. More stringent eligibility thresholds would tend to reduce the size of the original Medicaid population. Since Medicaid enrollment tends to be countercyclical, in some specifications we control for the state unemployment rate, the state poverty rate, the maximum level of welfare (Temporary Assistance for Needy Families, or TANF) benefits for a family of three, or the state food insecurity rate, all of which measure economic distress. These variables come from the University of Kentucky's Center for Poverty Research. We also consider the demographic composition of the state population (proportion non-White), since Medicaid enrollment varies across racial groups, as well as the size of the state population, drawing both datasets from the Census Bureau. Finally, in some specifications we use data from the University of Kentucky's Center for Poverty Research to adjust for the political party of the chief executive (the governor for states and the mayor for the District of Columbia) to account for potential differences in how the Medicaid program is administered. We control for baseline values of our state covariates in the last period before Medicaid expansion was implemented. The path of the original Medicaid population, in the absence of expansion, likely depends on these covariates, so a conditional parallel trends assumption may be more plausible than an unconditional parallel trends assumption.

We present descriptive statistics (means and standard deviations) of our outcome variable, as well as all state-level covariates, in table 1.

Persons in state-only health coverage programs and Medicaid expansion Children's Health Insurance Program (CHIP) enrollees not funded by Medicaid are excluded.

[7] Both sources capture only individuals whose coverage is funded through Medicaid (Title XIX of the Social Security Act); children and young adults funded through CHIP are excluded.

[8] The new adult group consists of two distinct populations: newly eligible and non–newly eligible. Non–newly eligible enrollees are a small, special class of Medicaid recipients already enrolled in Medicaid when the ACA was passed. To calculate the number of enrollees in the original Medicaid population, we subtract the number of newly eligible enrollees from the total number of Medicaid enrollees. This calculation will tend to bias our results against finding a decline in the size of the original Medicaid enrollment, since it is possible that states have reclassified enrollees from the original Medicaid population to the non–newly eligible group (Bundorf and Kessler 2022).

TABLE 1. Descriptive statistics

	Expansion states		Control states	
	Mean	SD	Mean	SD
Variable	(1)	(2)	(3)	(4)
Original Medicaid population (ln)	13.260	1.119	13.330	1.114
Eligibility limit, children (proportion of FPL)	2.563	0.583	2.231	0.385
Eligibility limit, parents (proportion of FPL)	1.167	0.537	0.542	0.354
State unemployment rate (%)	5.888	2.192	5.543	2.230
Governor's political party (1 = Democrat)	0.569	0.496	0.189	0.392
State population (ln)	15.08	1.055	15.24	0.980
Non-White (proportion of state)	0.211	0.142	0.232	0.128
Maximum TANF benefits ($)	485.5	163.6	347.9	139.1
Food insecurity (proportion of state)	0.132	0.0338	0.150	0.0329
Poverty rate	12.46	3.464	13.62	3.260
Observations	476		238	

Sources: The original Medicaid population is drawn from issue briefs published by the Kaiser Family Foundation and enrollment reports submitted by states to the Centers for Medicare and Medicaid Services. Medicaid eligibility limits are from the Kaiser Family Foundation. State unemployment rates are from the Bureau of Labor Statistics. The political party of the governor (or mayor, in the case of the District of Columbia) comes from the University of Kentucky's Center for Poverty Research. The state population and proportion of population that is non-White come from the Census Bureau. TANF benefits, food insecurity rates, and poverty rates come from the University of Kentucky's Center for Poverty Research.

Note: This table presents descriptive statistics for variables used in our analysis, split by state Medicaid expansion status. Expansion states consist of 33 states (and the District of Columbia) that expanded Medicaid under the Affordable Care Act before the end of 2019. Control states consist of 17 states that had not expanded Medicaid by the end of 2019. FPL = the federal poverty level (approximately $25,750 for a family of four in 2019); SD = standard deviation; TANF = Temporary Assistance for Needy Families.

Expansion and control states are broadly similar across several dimensions, including population size, racial diversity, and economic performance. Unsurprisingly, expansion states are substantially more likely to have a Democratic governor, provide more generous TANF benefits, and have higher income limits for parents on Medicaid.

4. Empirical Strategy

To identify the effect of Medicaid expansion on enrollment, we leverage variation in the adoption of Medicaid expansion across geographies and time, comparing trends between states that opted to expand Medicaid under the ACA and states that did not. Since nonexpansion states did not experience a relative change in their FMAP rates to cover different groups of Medicaid recipients, they represent a natural control group to test our reclassification hypothesis. Historically, two-way fixed-effects (TWFE) regressions have served as the workhorse models for estimating causal effects in the context of staggered policy adoption. However, recent studies have shown that the

TWFE estimator can yield inconsistent and misleading estimates of the average treatment effect on the treated (ATT) in the presence of treatment effect heterogeneity between groups or across time (De Chaisemartin and D'Haultfœuille 2020; Borusyak, Jaravel, and Spiess 2021; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Imai and Kim 2021; Sun and Abraham 2021).

The concerns related to TWFE models apply to our setting, in which states expanded Medicaid at different times. The first expansions in our data occur in 2014 and the last occurs in 2019 (see table A1 in the appendix for details on the treatment timing of individual states).[9] To overcome these limitations, in our main results we implement the robust difference-in-differences estimator proposed by Callaway and Sant'Anna (2021). This approach allows us to retain all states in our sample, including those that expanded Medicaid after the initial cohort in January 2014. The Callaway–Sant'Anna method delivers consistent ATT estimates, even in the presence of arbitrary heterogeneous treatment effects, by shutting down problematic 2 × 2 difference-in-differences comparisons between newly treated and already treated states. We implement the augmented inverse-probability weighting estimator described by Callaway and Sant'Anna (2021), in which both the treatment and outcome are modeled; recovering consistent estimates depends only on correctly specifying one of the models.

For our comparison group, we use only states that did not expand Medicaid before the end of 2019, when our sample ends (i.e., never-treated states). An alternative approach would be to include not yet treated states in the comparison group. We choose to restrict the comparison group to never-treated states for several reasons. First, our data include a relatively large number of never-treated states (17) and a relatively small number of late-expanding states—those that would serve as additional controls under the not yet treated option. Second, never-treated states are broadly similar to treated states, with geographic representation in the South, West, and Midwest. Third, the economic conditions during early and late treatments differ. Fourth, the parallel trends assumption is different between the two choices, and its interpretation is more straightforward when the comparison group is limited to never-treated states (Callaway and Sant'Anna, 2021).

There are a variety of ways to represent the results from the Callaway and Sant'Anna (2021) estimator. In our main results (table 3), we focus on the overall ATT, which is a (simple) weighted average of each $ATT(g, t)$, where g denotes the treatment group and t denotes the year. This calculation aggregates the ATTs within all treatment groups and time periods. In figure 2, we also present dynamic ATTs across treatment event time. In addition to highlighting treatment effects with respect to length of exposure to treatment, this dynamic specification allows us to assess the plausibility of the parallel trends assumption.

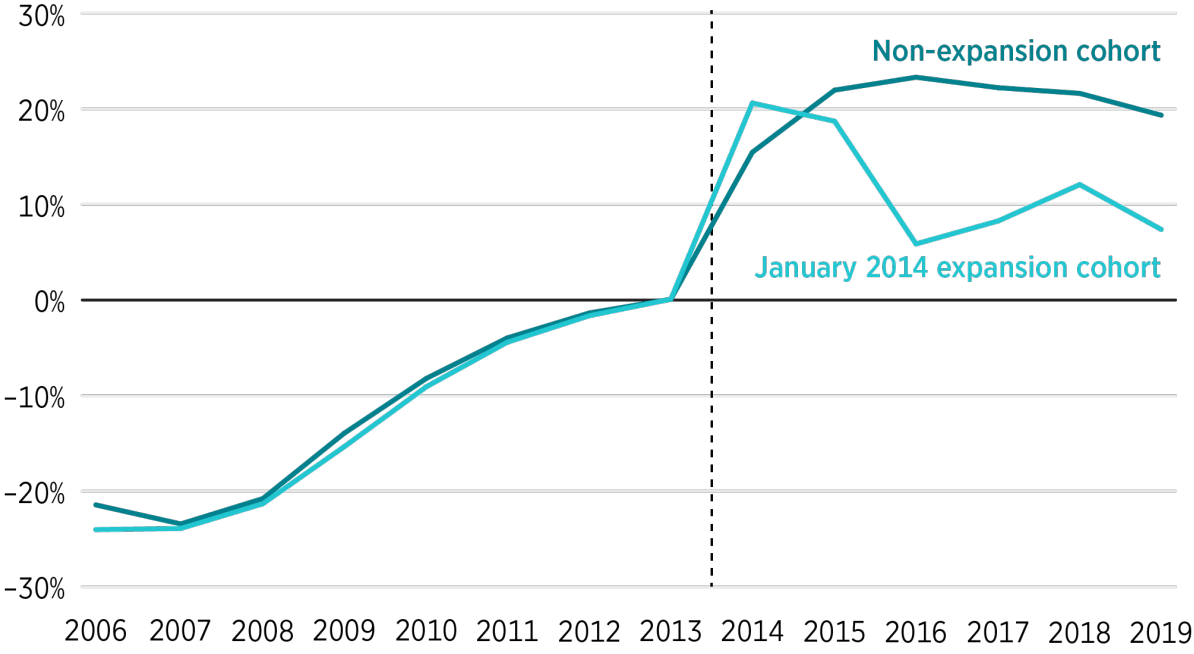
5. Results

5.1. Main results

In this section, we discuss our empirical results. Before turning to more sophisticated statistical methods, we present graphical evidence of longitudinal trends. Figure 1 plots the change in the size of the original Medicaid population (measured in the number of individuals enrolled in June of each year), contrasting states that expanded in January 2014 with those that had not expanded by the end of 2019.

[9] For this reason, the preliminary descriptive evidence presented in figure 1 and table 2 in section 5 is based on simple comparisons of the initial expansion cohort of states—the 25 states, including the District of Columbia, that expanded Medicaid in January 2014 and the 17 states that did not expand Medicaid by the end of 2019, when our sample ends. In these exhibits, we exclude the nine states that expanded Medicaid between February 2014 and December 2019.

FIGURE 1. Original Medicaid population enrollment (% change compared to 2013)



Sources: The authors compiled data from Kaiser Family Foundation issue briefs for 2006–13 and from reports from the Medicaid Budget and Expenditure System for 2014–19; see section 3 for more details. We define the original Medicaid population as total Medicaid enrollment minus the number of enrollees reported by states as newly eligible under the Affordable Care Act (ACA).

Note: This figure plots the change in the size of the original Medicaid population in nonexpansion states and in states that expanded Medicaid in January 2014. Values are normalized to zero in 2013, the last pre-expansion year. We use enrollment figures for the month of June in each year. The vertical dashed line denotes the implementation of the Medicaid expansion under the ACA. The January 2014 expansion cohort consists of 25 states (including the District of Columbia). The nonexpansion cohort consists of 17 states that have not expanded Medicaid under the ACA (as of April 2024) as well as states that expanded after 2019. The remaining nine states expanded Medicaid in a staggered fashion between February 2014 and December 2019; for simplicity, we omit these states from the graph.

For each cohort and year, we sum enrollment across all states. For ease of comparison, for both cohorts we express the change in enrollment relative to 2013, the last pre-expansion year. From 2006 to 2013, both cohorts tracked closely together. For both groups, enrollment in 2006 was approximately 21 to 24 percent lower than in 2013. From 2013 to 2014, the first treated year, both cohorts continued to follow very similar growth paths, with expansion states showing slightly larger gains in enrollment. Beginning in 2015, however, the cohorts began to diverge. Nonexpansion states continued to experience positive enrollment growth in 2015 and 2016, before declining gradually through 2019, a pattern broadly consistent with how one would expect Medicaid enrollment to evolve given the strengthening state of the national economy during this period and the national reach of the woodwork effect triggered by Medicaid expansion. Meanwhile, states that expanded Medicaid in January 2014 reported negative enrollment growth in 2015 and 2016, followed by a small rebound in 2017 and 2018 and a renewed decline in 2019. In total, from 2013 to 2019, enrollment in the original Medicaid group declined by 1.7 percent in

states that expanded in January 2014. Over the same period, nonexpansion states reported a 19.2 percent increase in enrollment.

Next, using the same data, we formalize this comparison by deriving simple difference-in-differences estimates of Medicaid expansion's effect on enrollment in the original Medicaid population. Table 2 compares changes in the state-reported size of the original Medicaid population in the pretreatment period (2006–13) and the posttreatment period (2014–19) between the cohort of states that expanded in January 2014 and the cohort of states that had not expanded by the end of 2019. In the pretreatment period, the mean level of enrollment in the original Medicaid population in expansion states was 1.10 million, while the mean in nonexpansion states was 0.92 million. In the posttreatment period, the mean in expansion states grew to 1.35 million, while the mean in the nonexpansion cohort increased to 1.23 million. Hence, our simple difference-in-differences calculation implies that, on average, the original Medicaid population would have been larger by nearly 58,000 enrollees (4.29 percent) in expansion states in the absence of the expansion.

TABLE 2. Simple difference-in-differences estimate

State cohort	Pretreatment	Posttreatment	Difference (pre/post)	Difference-in-differences
January 2014 expansion states	1,101,718	1,350,646	+248,928	−57,894
Nonexpansion states	923,996	1,230,818	+306,822	

Sources: The authors compiled data from Kaiser Family Foundation issue briefs for 2006–13 and from reports from the Medicaid Budget and Expenditure System for 2014–19; see section 3 for more details. We define the original Medicaid population as total Medicaid enrollment minus the number of enrollees reported by states as newly eligible under the Affordable Care Act (ACA).

Note: This table compares the average level of enrollment in the original Medicaid population between pre- and posttreatment periods and expansion and nonexpansion states. We use enrollment figures for the month of June in each year. The January 2014 expansion cohort consists of 25 states (including the District of Columbia). The nonexpansion cohort consists of 17 states that have not expanded Medicaid under the ACA (as of April 2024) as well as states that expanded after 2019. The remaining nine states expanded Medicaid in a staggered fashion between February 2014 and December 2019; for simplicity, we omit these states from our calculations.

While informative, the comparisons presented in table 2 have three important shortcomings: First, they ignore potentially confounding factors. Second, they omit late-expanding states (i.e., those that expanded between February 2014 and December 2019). Third, they conceal the dynamic effects of Medicaid expansion across different treatment periods. In table 3 and figure 2, we address each of these limitations by implementing the difference-in-differences estimator described by Callaway and Sant'Anna (2021). Since our dependent variable is the log of enrollment in the original Medicaid population, our regression coefficients can be interpreted as (approximate) percent changes. To obtain a baseline, in column (1) of table 3, we drop late-expanding states and estimate the model without controls. The coefficient does not attain statistical significance ($p = 0.11$) but is similar in magnitude to our implied estimate in table 2. Each of the other specifications presented in table 3 include all states and account for the staggered adoption of Medicaid expansion across time. Column (2) shows the no-controls specification with all states. Once again, the coefficient is similar to the implied estimate from table 2 but is not statistically significant ($p = 0.13$). In column (3), we present our preferred

specification, adding controls for the Medicaid income eligibility threshold for parents, the political party of the governor, and the state unemployment rate. These variables account for a range of possibly confounding factors. The Medicaid income eligibility threshold for parents reflects changes to eligibility affecting the original Medicaid population. We also adjust for the political party of the chief executive because Democratic and Republican governors may administer their Medicaid programs differently, in ways that are difficult to capture explicitly (e.g., the level of outreach to eligible populations). Finally, the state unemployment rate helps to isolate our estimates from the impact of economic shocks on Medicaid enrollment. The magnitude of the coefficient in column (3) is large and statistically significant at the 1 percent level, indicating that Medicaid expansion leads to a 9.93 percent decline in the size of the original Medicaid population.

TABLE 3. Effects of Medicaid expansion on enrollment in the original Medicaid population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ATT	−0.0595	−0.0505	−0.0993***	−0.0675**	−0.1064***	−0.0912**	−0.1021**	−0.0638*	−0.0895*
Standard error	(0.0370)	(0.0336)	(0.0348)	(0.0320)	(0.0369)	(0.0382)	(0.0428)	(0.0331)	(0.0348)
Governor's political party			✓	✓	✓	✓	✓	✓	✓
Eligibility limit, parents			✓		✓	✓	✓	✓	✓
Unemployment rate			✓	✓		✓	✓	✓	
Eligibility limit, children				✓					
Poverty rate					✓				
State population (ln)						✓			
Non-White (% of state)							✓		
TANF benefits								✓	
Food insecurity rate									✓
N	602	714	714	714	714	714	714	714	714

Note: This table shows estimates of the average treatment effect on the treated (ATT) of Medicaid expansion on the size of the original Medicaid population (logged) across a range of models, all of which use the staggered difference-in-differences estimator described by Callaway and Sant'Anna (2021). The comparison group is never-treated units. The specification in column (1) is without controls and includes only states that expanded in January 2014. All other models include all states. Standard errors (clustered by state) are reported in parentheses. TANF = Temporary Assistance for Needy Families.

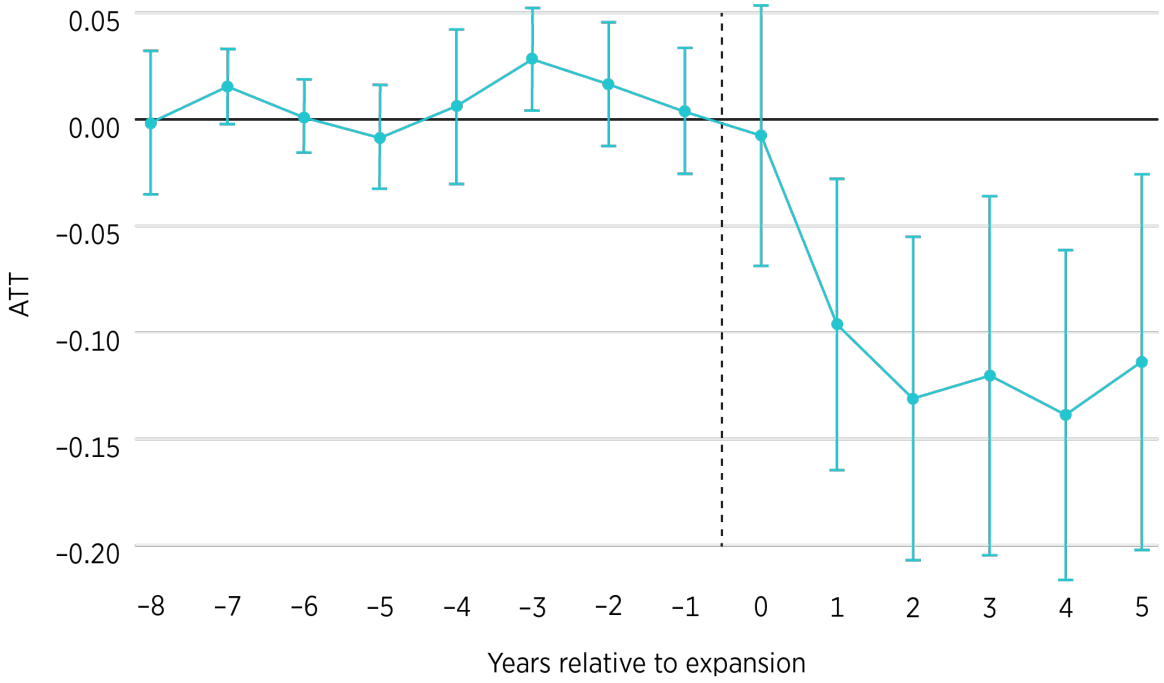
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In columns (4) through (9) we present a range of alternative specifications using our preferred specification, column (3), as a baseline. Column (4) adjusts for the Medicaid income eligibility threshold among children, another major subgroup of the original Medicaid population, rather than focusing on parents. In column (5), we use the poverty rate as a proxy for state economic conditions, rather than the unemployment rate. Column (6) adds the log of state population to adjust for interstate shifts in population. Column (7) adds the proportion of the state population that is non-White. In column (8), to account for the fact that the generosity of safety-net programs may have spillover effects on enrollment in other programs (Schmidt, Shore-Sheppard, and Watson 2019), we add the maximum level of TANF benefits for a family of three. Finally, column (9) shows the effect of using the food insecurity rate rather than the unemployment rate to measure economic distress. All alternative specifications yield similar results.

As a basis for later computations, we use the coefficient given in column (3), which is approximately in the middle range of our estimates.

Figure 2 plots the dynamic treatment effects derived from our preferred specification—that is, column (3) in table 3.

FIGURE 2. Dynamic effects of Medicaid expansion on enrollment



Sources: The authors compiled data from Kaiser Family Foundation issue briefs for 2006–13 and from reports from the Medicaid Budget and Expenditure System for 2014–19; see section 3 for more details. We define the original Medicaid population as total Medicaid enrollment minus the number of enrollees reported by states as newly eligible under the Affordable Care Act. ATT = average treatment effect on the treated.

Note: This plot shows dynamic effects across event time, based on our preferred specification—that is, column (3) in table 3. Bars represent 95 percent confidence intervals. The vertical dashed line represents the implementation of Medicaid expansion. We use enrollment figures for the month of June in each year.

The event study is generally supportive of the parallel trends assumption, showing little evidence of differential trends in the periods preceding the expansion of Medicaid. We also note that the magnitude of the effect, with the exception of the first year of Medicaid expansion's implementation (year 0 in figure 2), is statistically significant and roughly constant throughout the postexpansion period.

A range of additional checks are shown in the appendix. Table A2 gives results weighted by each state's 2013 Medicaid population to ensure that our findings are not unduly influenced by small states; dynamic effects of the main specification, with weights, are shown in figure A1. Weighting yields similar or slightly larger effects. In table A3, we show the sensitivity of our main specification to different choices in defining the treatment group. Specifically, we show the effects of dropping late-expanding states (i.e., those that expanded Medicaid after January 2014); states that implemented early ACA expansions during 2010–12; and states that covered low-income, childless adults prior to the ACA's passage in 2010. We report coefficients from both weighted and unweighted models. All specifications remain statistically significant at the 5 percent or 1 percent level, and seven out of nine alternative samples yield treatment effects larger than our main estimate. Finally, in table A4, we limit the sample to states that expanded in January 2014 and present the same set of specifications as in table 3; dynamic effects of the main specification with this narrower sample are shown in figure A2. The results are consistent.

5.2. Other ACA-related factors

Before turning to the fiscal implications of our empirical results, we consider several alternative explanations to our reclassification hypothesis and argue that no other explanation can plausibly account for the large decline in the original Medicaid population in expansion states relative to nonexpansion states over the 2014–19 period. In the discussion that follows, we focus on major provisions of the ACA (other than Medicaid expansion) that had substantial effects on the US healthcare system.

Premium tax credits

The ACA created a system of tax subsidies (in the form of premium tax credits, or PTCs) to help lower- and moderate-income Americans purchase private health insurance on the nongroup market. During our sample period, households in expansion states were eligible for PTCs if their income fell between 138 percent and 400 percent of the FPL. In nonexpansion states, the households earning between 100 percent and 400 percent of the FPL were eligible for PTCs. The lowest-income households received more generous subsidies, with premium contributions capped at 2 percent of their annual income. For some original Medicaid enrollees, transitioning to private coverage—possibly perceived as being higher quality than Medicaid—may have been appealing. Yet there is little reason to think that such transitions are driving our results. First, under federal law, individuals eligible for Medicaid are not eligible for PTCs, so this hypothesis requires millions of households to have strategically adjusted their income or other characteristics to gain PTC eligibility. Second, this hypothesis requires PTC-induced transitions from Medicaid to ACA plans to have been substantially larger in expansion states than in nonexpansion states. Yet, despite more than 8 million Americans signing up for ACA plans during the 2013–14 open enrollment period (Frank 2014), we see no differential effect on original Medicaid enrollment that year (see figure A2). Third, we note that the individual mandate was eliminated in 2019, yet we detect strong effects that year.

The ACA's individual mandate

Under the ACA, most Americans were required to maintain health coverage or face a financial penalty. If this mandate had more "bite" in nonexpansion states than expansion states, it might account for the divergence in original Medicaid enrollment between the two groups of states. However, this explanation is tenuous for two reasons: First, the mandate would essentially have augmented the woodwork effect—drawing even more eligible-but-not-enrolled people to Medicaid. But, as we discuss in section 2.2, survey-based studies do not support the view that the woodwork was substantially larger in nonexpansion states. Second, since the individual mandate was enforced by the IRS, a federal agency, there is no reason to believe that residents of nonexpansion states experienced more vigorous enforcement. Third, if the individual mandate had played an important role in causing the divergence in the growth of the original Medicaid population from 2014 to 2019, one would expect figure 1 to show accelerating growth in both expansion and nonexpansion states, with growth in nonexpansion states rising faster. In reality, the divergence stems from stagnating growth among expansion states, not particularly rapid growth in nonexpansion states.

"Silver loading"

In the fall of 2017, the Trump administration stopped reimbursing health insurers operating in the ACA's exchanges for cost-sharing reductions (CSRs) that certain lower-income households are entitled to. The decision caused temporary disruption to the individual health insurance market, but ultimately resulted in lower premiums for millions of consumers on the exchanges as insurers built the cost of CSRs into premiums, triggering larger PTCs (Aron-Dine 2019; Fiedler 2021). By making exchange coverage more affordable, this phenomenon (known as *silver loading*) may have led some people to transition from Medicaid to private plans. We consider this implausible. First, the cessation of federal CSR payments that became the impetus for silver loading did not occur until October 2017. Therefore, silver loading cannot explain the clear effects we find in 2016 and 2017 (see figure A2).[10] Second, as mentioned previously, individuals eligible for Medicaid are not eligible for PTCs, so this hypothesis assumes that millions of households reacted to silver loading by altering their income or other characteristics to become eligible for PTCs. Moreover, Aron-Dine (2019) notes that silver loading was least beneficial for people with incomes between 100 percent and 200 percent of the poverty level—implying that the group for whom such strategic behavior may have been the most feasible had the least incentive to do so. Third, this hypothesis requires silver loading to have had a substantially larger effect on Medicaid enrollment in expansion states than in nonexpansion states. From state-level estimates of the number of consumers affected by silver-loading (Aron-Dine 2018), we see little evidence that this was the case.

5.3. Fiscal impact of reclassifications

Using our estimates of the effects of Medicaid expansion on enrollment in the original Medicaid population, we now turn to back-of-the-envelope calculations of the fiscal impact of Medicaid expansion on states and the federal government. For the purposes of deriving quantitative fiscal estimates, we assume that all those who would otherwise have been enrolled in the original

[10] Recall that our data on Medicaid enrollment represents the month of June in each year (see section 3), so our estimates for 2017 precede the elimination of federal CSR payments.

Medicaid population were reclassified into the new adult group.[11] Because of the difference in FMAP rates applicable to the original Medicaid population and the new adult group, reclassifications represent a substantial federal subsidy to states. We approximate the size of the subsidy for each state and year using the following formula:

$$Y_{i,t} = \text{estimated enrollees reclassified}_{i,t} \times \text{FMAP rate spread}_{i,t} \times \text{per-enrollee expenditures}_{t} \quad (1)$$

where $Y_{i,t}$ represents the reclassification-related Medicaid subsidy received by state i in year t, *estimated enrollees reclassified* represents the difference between actual enrollment in the original Medicaid population and our estimated counterfactual enrollment,[12] *FMAP rate spread* is the difference between the traditional FMAP rate and the enhanced FMAP rate,[13] and *per-enrollee expenditures* equal the national average of expenditures per nonelderly adult Medicaid enrollee (expressed in constant 2019 dollars), excluding the new adult group.[14] Our results are presented in table 4. The fiscal impact of the reclassifications we document is substantial. Our estimates imply that $52.9 billion in additional federal funding was distributed to states from 2014 to 2019 on the basis of these reclassifications. Over that period, approximately 26.2 million reclassifications (measured as enrollee years) may have occurred across all expansion states. However, since Medicaid expansion was adopted in a staggered fashion over our sample period, the cumulative totals are somewhat distorted by the fact that some states expanded Medicaid in later years. To address this, table 4 also shows estimates for 2019, the last year in our sample.[15] That year, the original Medicaid population had approximately 4.4 million fewer beneficiaries as a result of expansion, resulting in $8.3 billion in subsidies to states, assuming the decrease came from reclassifications into the new adult group. For context, federal Medicaid expenditures totaled $405 billion in fiscal year 2019. Therefore, we calculate that the ACA's hidden subsidy may have accounted for approximately 2.0 percent of federal Medicaid outlays that year.

[11] Despite this assumption, we likely still underestimate the number of original Medicaid enrollees reclassified to the new adult group, since research using household survey data indicates that the woodwork effect induced bythe ACA was larger in expansion states than in nonexpansion states. As a result, nonexpansion states are likely to underestimate the counterfactual level of enrollment in the original Medicaid population in expansion states. See section 2.2 for more details.

[12] We use our coefficient from column (3) in table 3 (–0.0993) to derive the counterfactual enrollment levels in each state. To do so, we multiply actual enrollment in a given state and year by $\frac{1}{1-0.0993}_{1} = 1.1102$.

[13] Over our sample period, the mean FMAP rate spread among expansion states was 0.39; the median was 0.43.

[14] We obtain per-enrollee expenditures from annual reports compiled by the Medicaid and CHIP Payment and Access Commission (MACPAC). Estimates are available for 2013, 2018, and 2019. Estimates were not published for 2014, 2015, 2016, or 2017. To estimate per-enrollee expenditures in the missing years, we perform a linear interpolation using 2013 and 2018 as endpoints. We aggregate per-enrollee expenditures up to the national level because of data quality concerns with state-level estimates. All years are converted to 2019 dollars using the consumer price index.The estimated annual per-enrollee expenditures (in 2019 dollars) rose from $4,612 in 2014 to $4,908 in 2019.

[15] Despite more states belonging to the expansion cohort in 2019 than in previous years, the total state subsidy in 2019 ($8.3 billion) is slightly smaller than the average annual subsidy ($\frac{\$52.9}{6 \text{ years}}$ = $8.8 billion) over the 2014–19 period because the enhanced FMAP rate declined from 100 percent in 2014 to 93 percent in 2019.

TABLE 4. Estimated state subsidies from reclassifications

State	(1) Enrollees reclassified (2014–19)	(2) Subsidy ($, 2014–19)	(3) Enrollees reclassified (2019)	(4) Subsidy ($, 2019)
Alaska	65,197	143,908,236	16,898	35,661,429
Arizona	1,143,180	1,532,214,257	193,490	220,223,764
Arkansas	415,174	531,401,967	63,461	70,049,185
California	6,301,045	14,249,028,204	964,592	2,035,712,772
Colorado	578,605	1,295,730,016	94,860	200,195,982
Connecticut	451,198	1,015,806,169	78,068	164,758,053
Delaware	125,772	251,946,480	18,527	32,234,178
District of Columbia	123,553	159,419,572	21,262	24,001,881
Hawaii	189,060	391,952,540	31,090	59,632,187
Illinois	1,490,936	3,319,867,980	222,405	465,988,054
Indiana	556,220	810,178,691	109,948	145,914,499
Iowa	296,977	565,883,377	49,309	80,032,042
Kentucky	558,725	706,783,889	92,468	96,802,505
Louisiana	381,252	565,793,857	123,108	169,180,471
Maine	27,384	38,277,057	27,384	38,277,057
Maryland	600,868	1,353,645,229	101,313	213,815,775
Massachusetts	1,148,144	2,592,755,094	160,450	338,620,310
Michigan	1,138,343	1,731,004,370	193,986	271,820,305
Minnesota	600,981	1,356,606,456	94,384	199,192,440
Montana	69,418	101,424,056	17,304	23,321,446
Nevada	252,055	390,746,502	41,558	57,375,621
New Hampshire	74,226	166,749,362	14,187	29,940,195
New Jersey	735,045	1,658,273,490	115,925	244,653,227
New Mexico	389,011	487,757,313	62,712	63,835,701
New York	3,871,472	8,735,439,467	635,420	1,341,015,875
North Dakota	47,045	106,003,360	7,721	16,294,819
Ohio	1,579,154	2,601,153,570	246,572	361,963,879
Oregon	397,494	635,702,735	63,689	95,151,371
Pennsylvania	1,146,557	2,466,059,165	233,785	467,571,680
Rhode Island	150,946	332,561,427	25,818	51,230,964
Vermont	122,864	253,484,030	18,352	35,226,675
Virginia	137,448	$290,074,709	137,448	290,074,709

State	(1) Enrollees reclassified (2014–19)	(2) Subsidy ($, 2014–19)	(3) Enrollees reclassified (2019)	(4) Subsidy ($, 2019)
Washington	802,355	1,808,644,578	131,438	277,391,244
West Virginia	246,826	293,702,925	39,591	36,258,941
Total	**26,214,527**	**52,939,980,131**	**4,448,523**	**8,253,419,233**

Source: Authors' calculations. See main text for details. We omit states that had not expanded by 2019, when our sample period ends.

Note: This table reports the estimated federal payments distributed to states on the basis of the reclassification of Medicaid enrollees from the original population to the new adult group.

Several strands of circumstantial evidence support the reclassification hypothesis. Despite fears that a large woodwork effect would put substantial strain on state budgets, subsequent analyses have revealed that the fiscal impact of Medicaid expansion has been smaller than anticipated (Sommers and Gruber 2017; Gruber and Sommers 2020), with some analyses appearing to show that Medicaid expansion resulted in net fiscal savings in some states (Levy et al. 2020; Simpson 2020). Reclassifications, by allowing states to blunt the woodwork effect and draw down additional federal Medicaid funding through the enhanced FMAP rate, help to explain this outcome. Relatedly, enrollment in the new adult group has exceeded projections in virtually every expansion state (Blase and Yelowitz 2019). Reclassifications, which were generally not contemplated by forecasters, provide a simple explanation. Finally, per-enrollee spending on the new adult group has been significantly higher than predicted. In 2013, CMS estimated that per-enrollee costs in the new adult group would be $3,625 in 2016 (Truffer et al. 2013). A subsequent report from the same source revealed that membership of the new adult group had, in fact, cost $5,959 per enrollee in 2016 (Truffer et al. 2018), nearly two-thirds more than originally predicted. This fact is consistent with the notion that some of the original Medicaid population—who are more costly to insure, on average, than members of the new adult group—were reclassified into the new adult group.

Our data provide little direct insight into the types of enrollees being reclassified. As we discussed in section 2.3, the ACA and subsequent federal rulemaking established some pathways whereby certain individuals who would otherwise have been enrolled in the original Medicaid population could be counted in the new adult group. For example, a woman who enrolls in Medicaid under the ACA rules and later becomes pregnant, thereby meeting eligibility criteria for the original Medicaid population, need not be reclassified into the original Medicaid population during her pregnancy. Similar logic applies to people who enroll in the new adult group and subsequently suffer a disabling injury that renders them eligible for Medicaid coverage under pre-ACA eligibility rules; they need not be transferred to the original Medicaid population for the purposes of obtaining federal reimbursements. These forces would tend to reduce the size of the original Medicaid population gradually, as more and more members of the new adult group experienced these health events. Yet our results are inconsistent with this prediction. Our estimated dynamic treatment effects indicate that the original Medicaid population contracted suddenly in the second posttreatment year and remained relatively stable over the succeeding four

years, rather than continuing to decline. Therefore, we conclude that the forces siphoning off enrollees from the original Medicaid population likely play a minor role in explaining our results.

Instead, our findings may partly be driven by reclassifications that occurred in violation of Medicaid rules. Under federal law, states are responsible for determining applicants' eligibility for Medicaid, including periodically redetermining eligibility, disenrolling individuals who are no longer eligible, and reclassifying enrollees who may no longer meet the criteria under one eligibility pathway but may still qualify for Medicaid coverage through a different pathway. Yet the enhanced FMAP rates for the new adult group offered under the ACA dramatically reduced states' incentives to maintain accurate Medicaid enrollments. Moreover, the federal government provides only token oversight of states' eligibility verification procedures. According to CMS, "When states submit their Medicaid expenditure reports, they certify the data are accurate and CMS conducts a limited review to assess whether the data [are] reasonable. The review consists of comparing the state-reported data to other readily available information, including state-reported performance indicators and expenditures, and follow-up with the state as needed." Yet states rarely face meaningful penalties for submitting incorrect enrollment records. During the entire posttreatment period that we examined (2014–19), it was the explicit policy of the federal government not to attempt to recoup funds distributed to states on the basis of eligibility errors (Yocom 2020). Previous research has noted that the ACA's Medicaid expansion was associated with large increases in Medicaid coverage among adults with incomes above 138 percent of the FPL, suggesting that states failed to adequately enforce eligibility rules (Courtemanche, Marton, and Yelowitz 2019).

Recent federal investigations into expansion states' Medicaid records provide direct evidence that improper reclassifications into the new adult group are common. In an audit of New York's Medicaid program, investigators reviewed eligibility documentation for a random sample of 130 Medicaid enrollees whom New York had classified as belonging to the new adult group and for whom New York had received funding through the ACA's enhanced FMAP rate. The audit found that New York incorrectly claimed enhanced reimbursement for 13.8 percent of these enrollees and did not provide sufficient documentation to verify that 1.5 percent of these enrollees were eligible for enhanced Medicaid reimbursement (Chiedi 2019b). A similar audit in California found that 18.0 percent of a randomly selected sample of enrollees in the new adult group were ineligible and 9.3 percent of enrollees were potentially ineligible under ACA rules (Levinson 2018). In Colorado, an investigation found that 23.3 percent of randomly selected enrollees in the new adult group were ineligible, while an additional 6.7 percent lacked sufficient documentation to determine eligibility (Chiedi 2019a).

Following Bundorf and Kessler (2022), we extrapolate from these audits to provide a general indication of the proportion of reclassifications that may be improper. To do so, we use the results of the New York audit as a lower bound, using only the proportion of enrollees that auditors verified as ineligible, 13.8 percent. We use the results of the Colorado audit as an upper bound, using the proportion of enrollees that auditors found to be definitely or potentially ineligible, 28.3 percent.[16] In 2019, states reported a total enrollment in the new adult group of 12.0 million. Applying these lower and upper bounds, we find that between 1.65 million and up to 3.4 million of these enrollees may have been improper. In light of our finding that the original Medicaid population declined by 4.4 million enrollees, these figures suggest that between 37.2 percent and 76.3 percent of all reclassified enrollees may have been reported in violation of federal law. These

[16] Out of the 60 Medicaid beneficiaries sampled, 14 were ineligible and 4 may have been ineligible, but one person was counted in both groups.

estimates should be interpreted cautiously, however, as enrollment patterns may vary by state and over time; other expansion states may have higher or lower misclassification rates than New York, Colorado, or California.

5.4. Robustness tests

In this section, we perform several tests to assess the sensitivity of our findings.

Data quality

As explained in greater detail in section 3, our main analysis uses two different data sources to measure state Medicaid enrollment; one covers 2006 to 2013, while the other covers 2014 to 2019. Since most expansion states began implementing the reform in 2014, it is conceivable that our findings could be an artifact of transitioning to a different data source. This could occur if our 2014–19 data systematically undercounted original Medicaid enrollment in expansion states relative to nonexpansion states. The lack of a clear discontinuity between expansion and nonexpansion states in 2014—visible in figures 1, 2, and A2—is reassuring. Still, we further explore this possibility in two ways. First, we rerun the analysis using only 2014–19 data. While this exercise restricts our sample and limits the number of pretreatment periods available, it obviates the need to combine different data sources. Results are presented in table 5. The treatment effect in our preferred specification, column (2), remains statistically significant, albeit somewhat smaller in magnitude (−0.0764 instead of −0.0993). Most specifications are no longer statistically significant at the 10 percent level, and estimated effect sizes generally shrink compared to our main results. The small number of observations in our restricted sample may contribute to a loss of statistical significance. Still, we note that all coefficients remain negative and economically meaningful.

As an additional check, we compare our 2013 Medicaid enrollment data from Kaiser Family Foundation with estimates from CMS of 2013 Medicaid and Children's Health Insurance Program (CHIP) enrollment.[17] The CMS data, which has been used as a benchmark to gauge ACA-induced changes in coverage, is not available for prior years, but this narrow overlap in 2013 provides some insight into whether Kaiser Family Foundation and CMS estimates systematically differ. Figure A3 in the appendix plots the log of enrollment in each state from Kaiser Family Foundation and CMS in 2013. Nearly all states lie very close to the diagonal, indicating no large differences between the two sources. Some states may lie slightly above the diagonal because CMS's data includes CHIP enrollees, whereas Kaiser Family Foundation's estimates exclude these enrollees. We also note the absence of any clear pattern between expansion states (in dark blue) and nonexpansion states (in light blue). Overall, these results assuage concerns that our main findings are driven by data discrepancies.

[17] The CMS estimates reflect the average monthly Medicaid and CHIP enrollment from July to September 2013. The Kaiser Family Foundation estimates reflect Medicaid enrollment in June 2013. CMS did not release 2013 estimates for Medicaid only.

TABLE 5. Effects of Medicaid expansion on enrollment: 2014–19 sample period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	−0.0813**	−0.0764**	−0.0604	−0.0526	−0.0744**	−0.0480	−0.0651	−0.0367
Standard error	(0.0334)	(0.0339)	(0.0417)	(0.0354)	(0.0330)	(0.0419)	(0.0408)	(0.0410)
Governor's political party		✓	✓	✓	✓	✓	✓	✓
Eligibility limit, parents		✓		✓	✓	✓	✓	✓
Unemployment rate		✓	✓		✓	✓	✓	
Eligibility limit, children			✓					
Poverty rate				✓				
State population (ln)					✓			
Non-White (% of state)						✓		
TANF benefits							✓	
Food insecurity rate								✓
N	150	150	150	150	150	150	150	150

Note: This table shows estimates of the average treatment effect on the treated (ATT) of Medicaid expansion on the size of the original Medicaid population (logged) across a range of models, all of which use the difference-in-differences estimator described by Callaway and Sant'Anna (2021), weighted by each state's 2013 Medicaid population. The comparison group is never-treated units. All other models include all states. Standard errors (clustered by state) are reported in parentheses. TANF = Temporary Assistance for Needy Families.

*$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Changes to enrollment practices

During our study period, some states implemented reforms to their administrative procedures that may have reduced enrollment in the original Medicaid population. In particular, Arbogast, Chorniy, and Currie (2024) document two major categories of new rules: (a) increases in the stringency and frequency of eligibility and income checks and (b) mechanisms to automatically disenroll beneficiaries deemed to no longer qualify for the program (e.g., canceling someone's coverage without notice if a person does not respond to a request for documentation within a certain time frame). To the extent that these policies coincided with Medicaid expansion and may have disproportionately affected populations in expansion states, they could influence our findings. To address this concern, we reestimate our main models after dropping the 13 states that implemented one or both of these policies from 2013 to 2019. The results of this exercise, which we report in table 6, are generally similar to our main estimates; all specifications that reached statistical significance in our main analysis remain statistically significant, and some coefficients—including our preferred specification—grow slightly in magnitude. The dynamic treatment effects we obtain from this more limited subset of states, shown in figure 3, are also similar to our main results.

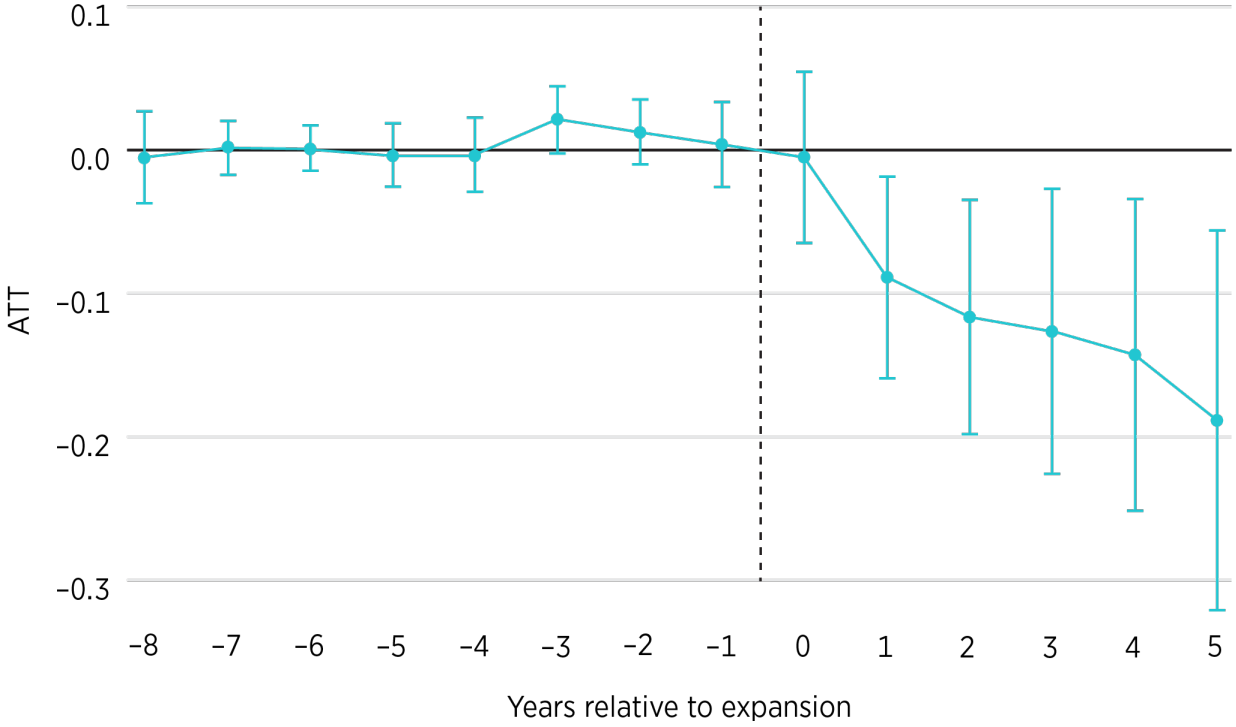
TABLE 6. Effects of Medicaid expansion on enrollment: Exclude observations from states that imposed administrative burdens

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	−0.0299	−0.1031**	−0.0654**	−0.1086**	−0.0950*	−0.0943*	−0.0607*	−0.0749**
Standard error	(0.0383)	(0.0404)	(0.0332)	(0.0429)	(0.0499)	(0.0488)	(0.0339)	(0.0378)
Governor's political party		✓	✓	✓	✓	✓	✓	✓
Eligibility limit, parents		✓		✓	✓	✓	✓	✓
Unemployment rate		✓	✓		✓	✓	✓	
Eligibility limit, children			✓					
Poverty rate				✓				
State population (ln)					✓			
Non-White (% of state)						✓		
TANF benefits							✓	
Food insecurity rate								✓
N	669	669	669	669	669	669	669	669

Note: This table shows estimates of the average treatment effect on the treated (ATT) of Medicaid expansion on the size of the original Medicaid population (logged) across a range of models, all of which use the difference-in-differences estimator described by Callaway and Sant'Anna (2021). The table excludes observations from states that had imposed administrative burdens of more stringent eligibility checks (Colorado, Florida, Hawaii, Idaho, Illinois, Louisiana, Mississippi, and Texas) and states that implemented automatic disenrollment policies during our sample period (Arkansas, Illinois, Louisiana, Missouri, North Carolina, Ohio, and Tennessee). For more, see Arbogast, Chorniy, and Currie (2024). The comparison group is never-treated states. Standard errors (clustered by state) are reported in parentheses. TANF = Temporary Assistance for Needy Families.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 3. Dynamic effects of Medicaid expansions on enrollment: Exclude observations from states that imposed administrative burdens



Sources: The authors compiled data from Kaiser Family Foundation issue briefs for 2006–13 and from reports from the Medicaid Budget and Expenditure System for 2014–19; see section 3 for more details. We define the original Medicaid population as total Medicaid enrollment minus the number of enrollees reported by states as newly eligible under the Affordable Care Act.

Note: This plot shows dynamic effects across event time, from column (2) in table 6, excluding observations from states that had imposed administrative burdens of more frequent or stringent eligibility checks (Colorado, Florida, Hawaii, Idaho, Illinois, Louisiana, Mississippi, and Texas) and states that implemented automatic disenrollment policies during our sample period (Arkansas, Illinois, Louisiana, Missouri, North Carolina, Ohio, and Tennessee). For more, see Arbogast, Chorniy, and Currie (2024). Bars represent 95 percent confidence intervals. The vertical dashed line represents the implementation of Medicaid expansion. We use enrollment figures for the month of June in each year. ATT = average treatment effect on the treated.

6. Conclusion

The expansion of Medicaid under the ACA was a significant development in US health policy. We examine a previously overlooked fiscal effect of this reform. Although past research using household survey data has documented a robust woodwork effect in Medicaid associated with expansion, we find no evidence of such an effect in states' administrative enrollment records. Rather, we find evidence that the original Medicaid population contracted sharply following Medicaid expansion's implementation, defying forecasters' expectations. We argue that this discrepancy is a mirage caused by the reclassification of individuals who otherwise would have been counted in the original Medicaid population to the new adult group. While these reclassifications were a purely administrative phenomenon that did not affect the coverage or

benefits of individual Medicaid enrollees, reclassifications have had substantial fiscal effects on states and the federal government.

Our estimates imply that these reclassifications resulted in nearly $52.9 billion in federal Medicaid payments to states from 2014 to 2019, including $8.3 billion in 2019 alone. The hidden subsidy we document represents a sizable share of Medicaid expansion's impact on federal spending. According to CBO, the direct federal costs of Medicaid expansion—that is, reimbursements made to states to cover medical services for the new adult group—were $66 billion in 2019 (Fritzsche, McNellis, and Vreeland 2019). This figure, however, implicitly assumes that members of the new adult group would not have received federal subsidies in the absence of the ACA's expanded eligibility rules. Our results indicate that 4.4 million Medicaid enrollees classified in the new adult group might have been counted as original Medicaid enrollees and reimbursed at states' traditional FMAP rate if Medicaid expansion had not occurred. Our results imply that the federal government may have provided $12.1 billion to states in 2019 to cover these enrollees.[18] Therefore, the federal fiscal impact of Medicaid expansion is approximately $53.9 billion (the difference between $66 billion and $12.1 billion), substantially smaller than CBO's estimates suggest. Nevertheless, these downward revisions imply that reclassifications inflated the federal cost of Medicaid expansion by 18.2 percent.

It is likely that similar subsidies occurred in more recent years, although forces linked to the COVID-19 public health emergency may have changed their magnitude. On the one hand, the temporary increase in the traditional FMAP rate during the COVID-19 public health emergency narrowed the FMAP rate spread with the enhanced FMAP rate, which would have reduced the size of the hidden subsidy. On the other hand, the continuous enrollment requirement imposed during the COVID-19 public health emergency substantially increased Medicaid enrollment and may have increased the size of the hidden subsidy.

Our results suggest that state policymakers are sensitive to incentives created through Medicaid's joint financing structure. The ACA's hidden subsidy has had a substantial fiscal effect on the federal government and expansion states. Accounting for strategic behavior by states is crucial for accurately predicting the effects of policy changes to Medicaid and similar federal-state programs. More stringent federal monitoring of states' enrollment practices may help to mitigate such behavior. Alternative financing methods, such as federal block grants, could also reduce or eliminate opportunities to draw down additional federal Medicaid funding through administrative reclassifications.

[18] We arrive at this result by multiplying the estimated number of reclassified Medicaid enrollees in 2019 in each state (totaling 4.4 million) by the average per-enrollee cost nationwide in 2019 ($4,908) and the applicable state's traditional FMAP rate.

Appendix

TABLE A1. State Medicaid expansion status

State	Implementation date	Designation in our analysis
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

State	Implementation date	Designation in our analysis
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	Nonexpansion
Utah	1/1/2020	Nonexpansion
Nebraska	10/1/2020	Nonexpansion
Oklahoma	7/1/2021	Nonexpansion
Missouri	10/1/2021	Nonexpansion
South Dakota	7/1/2023	Nonexpansion
North Carolina	12/1/2023	Nonexpansion

Source: Kaiser Family Foundation.

TABLE A2. Effects of Medicaid expansion on enrollment: Weighted by Medicaid population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	−0.0819**	−0.1195***	−0.0956**	−0.0986**	−0.1195***	−0.1188***	−0.1079**	−0.0742*
Standard error	(0.0345)	(0.0392)	(0.0383)	(0.0405)	(0.0377)	(0.0445)	(0.0547)	(0.0394)
Governor's political party		✓	✓	✓	✓	✓	✓	✓
Eligibility limit, parents		✓		✓	✓	✓	✓	✓
Unemployment rate		✓	✓		✓	✓	✓	
Eligibility limit, children			✓					
Poverty rate				✓				
State population (ln)					✓			
Non-White (% of state)						✓		
TANF benefits							✓	
Food insecurity rate								✓
N	714	714	714	714	714	714	714	714

Note: This table shows estimates of the average treatment effect on the treated (ATT) of Medicaid expansion on the size of the original Medicaid population (logged) across a range of models, all of which use the difference-in-differences estimator described by Callaway and Sant'Anna (2021) weighted by each state's 2013 Medicaid population. The comparison group is never-treated units. All other models include all states. Standard errors (clustered by state) are reported in parentheses. TANF = Temporary Assistance for Needy Families.

*$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A3. Effects of Medicaid expansion on enrollment in the original Medicaid population: Robustness of main specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ATT	−0.0993***	−0.1195***	−0.1118***	−0.1246***	−0.0800***	−0.1266**	−0.0902**	−0.1576***	−0.1079***	−0.1798***
Standard error	(0.0348)	(0.0392)	(0.0395)	(0.0433)	(0.0350)	(0.0492)	(.0349)	(0.0578)	(0.0414)	(0.0684)
Governor's political party	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Eligibility limit, parents	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Unemployment rate	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Drop late-expanders			✓	✓					✓	✓
Drop 2010–12 expanders					✓	✓			✓	✓
Drop pre-2010 expanders							✓	✓	✓	✓
Weighted		✓		✓		✓		✓		✓
N	714	714	602	602	630	630	574	574	462	462

Note: This table shows estimates of the average treatment effect on the treated (ATT) of Medicaid expansion on the size of the original Medicaid population (logged) across a range of specifications, all of which use the difference-in-differences estimator described by Callaway and Sant'Anna (2021). Specifications that drop late expanders exclude the following states from the sample: Alaska, Indiana, Louisiana, Maine, Montana, New Hampshire, Pennsylvania, and Virginia. Specifications that drop 2010–12 expanders exclude the following states: California, Connecticut, District of Columbia, Minnesota, New Jersey, and Washington. Specifications that drop pre-2010 expanders exclude the following states: Delaware, District of Columbia, Massachusetts, New York, and Vermont. The comparison group is never-treated units. All other models include all states. Standard errors (clustered by state) are reported in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

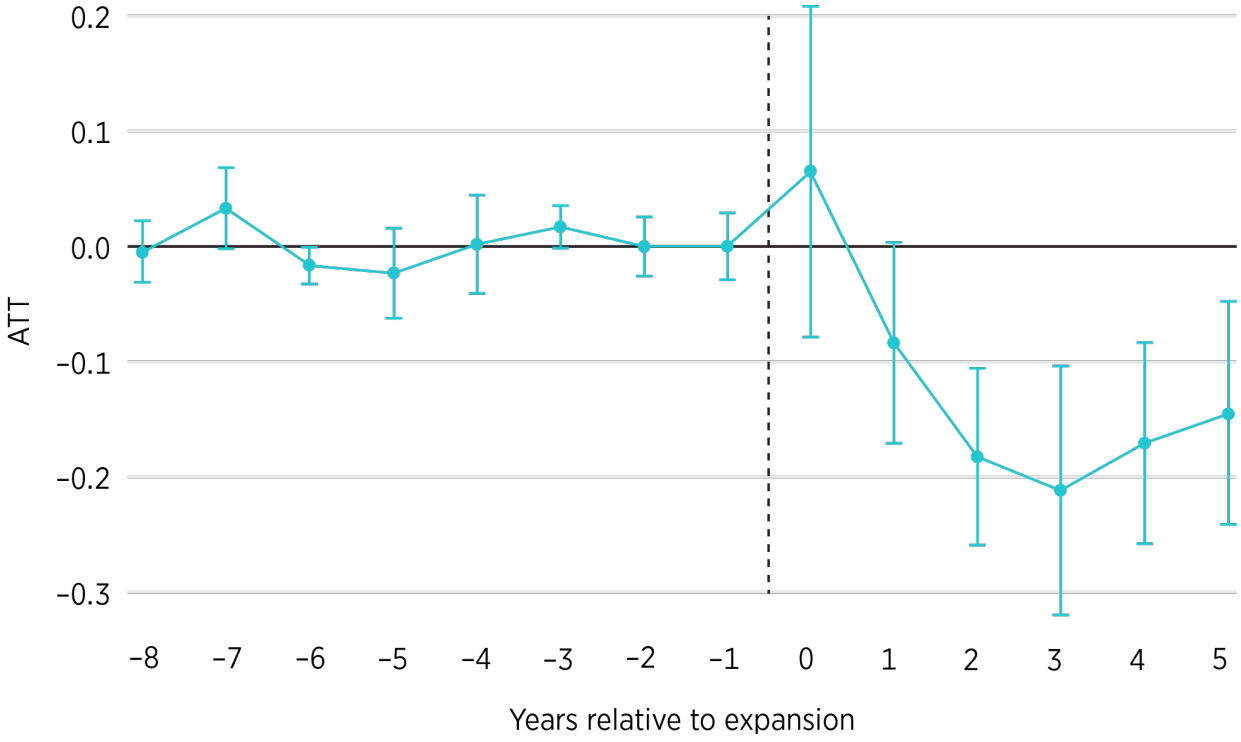
TABLE A4. Effects of Medicaid expansion on enrollment in the original Medicaid population: 2014 cohort only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	−0.0595	−0.1118***	−0.0777***	−0.1209***	−0.1040**	−0.1021**	−0.1159**	−0.0755**
Standard error	(0.0370)	(0.0395)	(0.0362)	(0.0417)	(0.0382)	(0.0429)	(0.0496)	(0.0353)
Governor's political party		✓	✓	✓	✓	✓	✓	✓
Eligibility limit, parents		✓		✓	✓	✓	✓	✓
Unemployment rate		✓	✓		✓	✓	✓	
Eligibility limit, children			✓					
Poverty rate				✓				
State population (ln)					✓			
Non-White (% of state)						✓		
TANF benefits							✓	
Food insecurity rate								✓
N	602	602	602	602	602	602	602	602

Note: This table shows estimates of the average treatment effect on the treated (ATT) of Medicaid expansion on the size of the original Medicaid population (logged) across a range of models, all of which use the difference-in-differences estimator described by Callaway and Sant'Anna (2021). The treatment group in this table is states that expanded in 2014. The comparison group is never-treated units. All other models include all states. Standard errors (clustered by state) are reported in parentheses. TANF = Temporary Assistance for Needy Families.

*$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

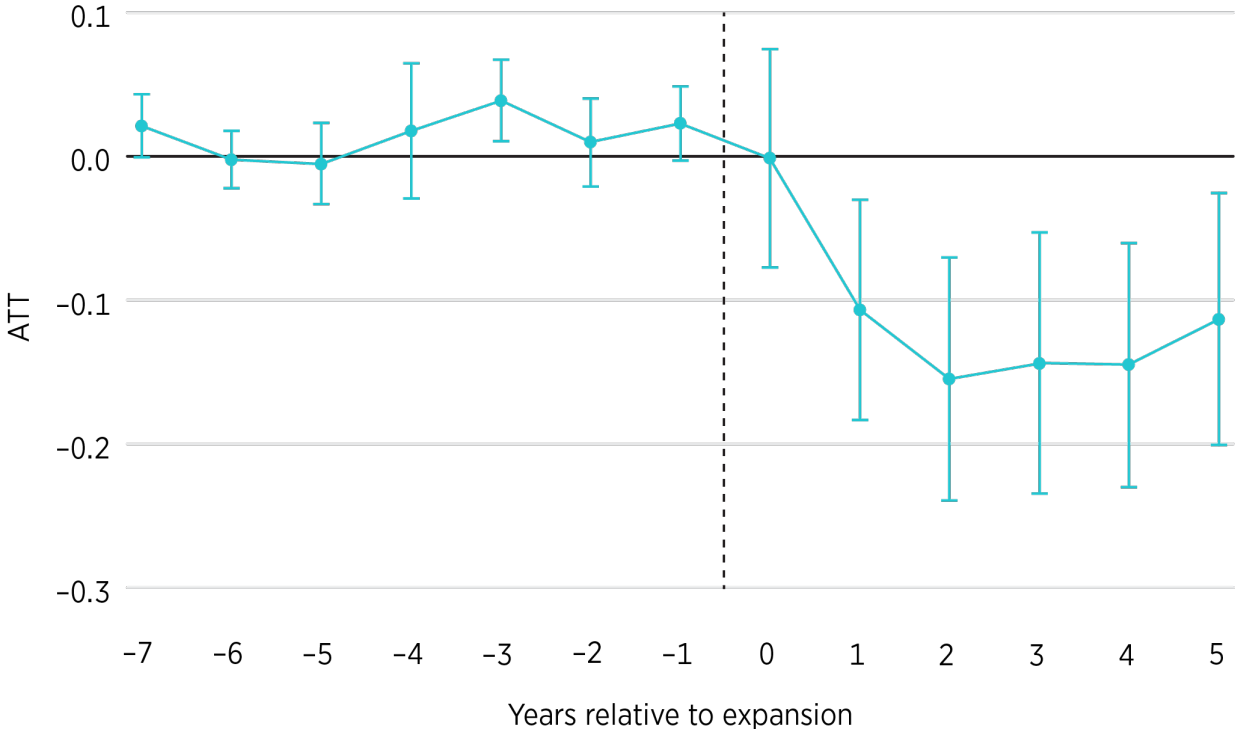
FIGURE A1. Dynamic effects of Medicaid expansions on enrollment: Weighted by Medicaid population



Sources: The authors compiled data from Kaiser Family Foundation issue briefs for 2006–13 and from reports from the Medicaid Budget and Expenditure System for 2014–19; see section 3 for more details. We define the original Medicaid population as total Medicaid enrollment minus the number of enrollees reported by states as "newly eligible" under the Affordable Care Act.

Note: This plot shows dynamic effects across event time, from column (3) in table A2, which are weighted by each state's 2013 Medicaid population. Bars represent 95 percent confidence intervals. The vertical dashed line represents the implementation of Medicaid expansion. We use enrollment figures for the month of June in each year. ATT = average treatment effect on the treated.

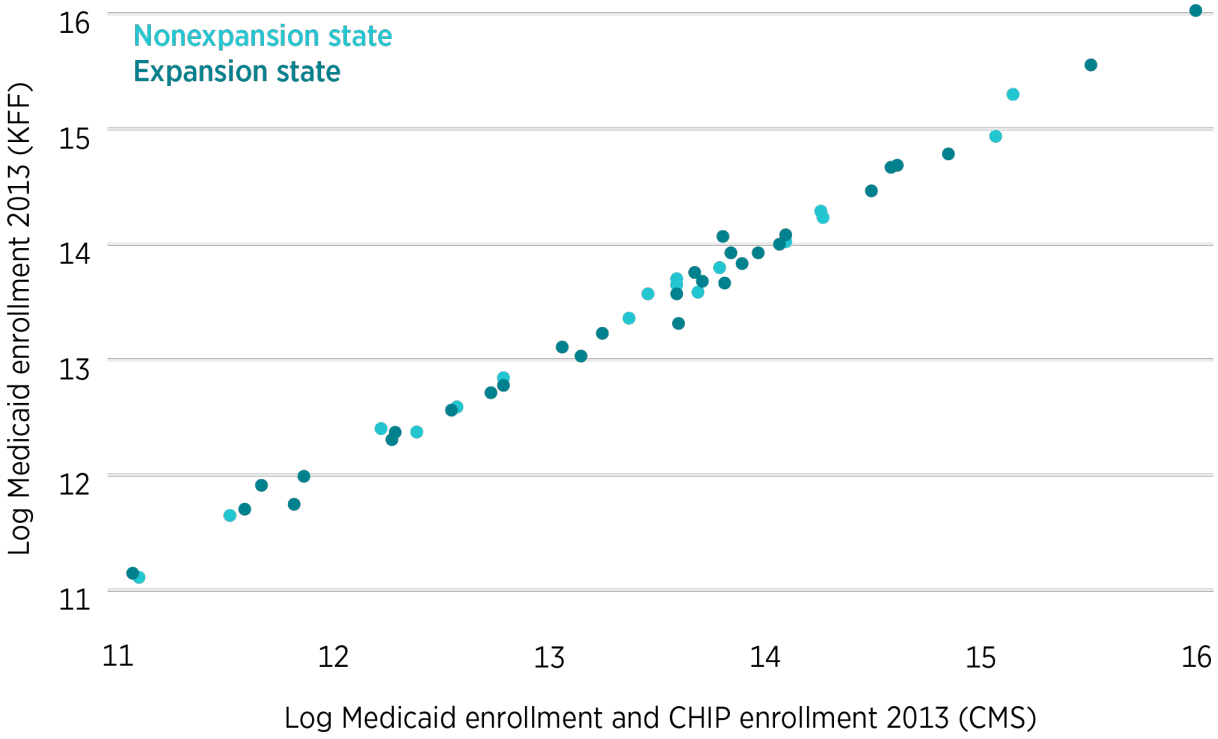
FIGURE A2. Dynamic effects of Medicaid expansion on enrollment: 2014 cohort only



Sources: The authors compiled data from Kaiser Family Foundation issue briefs for 2006–13 and from reports from the Medicaid Budget and Expenditure System for 2014–19; see section 3 for more details. We define the original Medicaid population as total Medicaid enrollment minus the number of enrollees reported by states as newly eligible under the Affordable Care Act.

Note: This plot shows dynamic effects across event time, from column (3) in table A4. Bars represent 95 percent confidence intervals. The vertical dashed line represents the implementation of Medicaid expansion. We use enrollment figures for the month of June in each year. ATT = average treatment effect on the treated.

FIGURE A3. Medicaid enrollment in 2013 by data source



Source: The authors compiled the data from Kaiser Family Foundation (KFF) issue briefs for 2006–13 and from reports from the MBES for 2014–19; see section 3 for more details.

Note: This figure plots the (log) Total Medicaid Population for December 2013 from the Medicaid Budget and Expenditure Sytem (MBES) of the Centers for Medicare and Medicaid Services (CMS) against the (log) Total Medicaid and Children's Health Insurance Program (CHIP) population in KFF reports from 2013. This graph excludes Connecticut and Maine as data were not available for these states for December 2013 in CMS-MBES.

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