Medicaid Expansion and Parental Health Insurance

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Abstract

This paper examines how the Medicaid expansion in the late 1980s and early 1990s focused on children and pregnant women impacted parents' health insurance coverage. Using state-of-art difference-in-difference methodology, I find that while the expansion significantly increased the number of parents with children covered by Medicaid, it also substantially reduced parents' private insurance coverage. For mothers, the decline in private insurance is, to a large extent, crowded out by public insurance. However, for fathers, it largely reflects a decrease in health insurance coverage, indicating a spillover effect. Overall, I find that Medicaid expansion explains almost 48% of the decline in private insurance in this period and accounts for nearly one-third of the increase in the uninsured. These findings highlight the importance of analyzing health insurance decisions at the household level, as expanding coverage for some family member can significantly alter the household's willingness to pay for private insurance. Moreover, this study uncovers a novel mechanism contributing to the long-term decline in private health insurance since the late 1980s.

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I Introduction

In the United States, the Omnibus Budget Reconciliation Act (OBRA) of 1986 initiated a series of legislative efforts to expand public health insurance for low-income children and pregnant women. The Act allowed states to relax traditional Medicaid eligibility requirements and implement more generous guidelines for specific family members. Subsequent legislative changes in the late 1980s further broadened Medicaid eligibility, extending coverage to older and less impoverished children and pregnant women, while adult eligibility remained largely unchanged.

This paper examines how the Medicaid expansions of the late 1980s and early 1990s, primarily targeting children and pregnant women, affected parental health insurance outcomes. While previous research has extensively analyzed the impact of public insurance expansions on directly eligible populations, this study takes a novel approach by exploring their effects on parents, who were excluded from direct eligibility except for pregnant women. This distinction provides a unique setting to assess family-level interdependencies and the broader implications of public health insurance availability for non-eligible family members.

My findings show that between 1987 and 1993, Medicaid expansion increased the share of parents with children enrolled in Medicaid by 5.2 percentage points, reduced private insurance coverage by 2.8 percentage points, and increased public insurance coverage by 2.3 percentage points. However, the expansion also contributed to a 1.1 percentage point rise in the uninsured rate among parents. Gender-specific analysis highlights notable differences: for women, the expansion led to significant crowding out of private insurance (a 3.0 percentage point decline) and an increase in public coverage (3.3 percentage points), with no significant change in the uninsured rate. In contrast, for fathers, private insurance declined by 2.3 percentage points, with no corresponding increase in public coverage, leading to a 1.7 percentage point increase in the uninsured rate.

Using these point estimates as a reference, I find that Medicaid expansion had a significant impact on parental health insurance outcomes during this period. Specifically, it accounts for 78% of the observed increase in the share of parents with Medicaid-enrolled children, 48% of the decline in parental private insurance coverage, 88% of the rise in parental public coverage, and 31% of the increase in uninsured parents.

While the crowding out effect of public insurance expansions is well-documented, this paper uncovers a novel spillover effect. Specifically, fathers, a non-eligible group under these expansions, experienced a significant decline in private coverage, leading to a higher uninsured rate. This puzzling outcome highlights behavioral responses in health insurance demand and emphasizes the importance of considering family interdependencies when analyzing the effects of public insurance availability.

To identify the effect of Medicaid expansion on parental health insurance outcomes, I exploit within-state variation in eligibility determined by the age restrictions of targeted children. Specifically, I construct within-state control and treatment group and estimate state-specific effect using Callaway and Sant'Anna (2021) methodology. I then aggregate these estimates and present aggregate causal effects for the U.S. To define control and treatment groups I exploit a unique characteristic of Medicaid reforms during this period.[1] These policies primarily targeted children born on or after October 1, 1983. This eligibility cutoff provides a natural framework to identify within-state group of families with high exposure to these legislative changes. Particularly, I define the treatment group as parents with at least one child born in 1983 or later, while the control group consists of parents with no children born in 1983 or after.[2] Since pregnant women would automatically belong to this group,[3] the treatment group is highly exposed to both children and pregnant women eligibility expansion. To assign state expansion dates, I follow the methodology of East, Miller, Page, and Wherry (2023), which accounts for state-level heterogeneity in Medicaid eligibility prior to expansion.

This approach offers several methodological advantages. First, by estimating these effects using Callaway and Sant'Anna (2021) methodology, I avoid the pitfalls inherent in standard "static"

[1] This includes OBRA 1986, OBRA 1987, MCCA 1988, OBRA 1989, and OBRA 1990. Further details in Appendix B.

[2] Ideally, I would select families with children born on or after October 1, 1983. However, since birth dates are not directly available, I define the treatment group based on birth years instead.

[3] My sample include parents with dependent children. First-time pregnant women who were pregnant the year before the interview and remain pregnant during the March CPS interview are not included.

or "dynamics" two-way fixed effect linear regression (Goodman-Bacon (2021); Sun and Abraham (2021)). Second, I estimate state-specific effects and construct aggregate causal parameters. Medicaid eligibility before this expansion, was defined by states-level guidelines, and therefore, there was substantial state heterogeneity in Medicaid access. Moreover, populations characteristics across states were also heterogeneous. Under this scenario, it is safe to assume that expanding medicaid in each state, would have heterogeneous effects. Third, I identify these effects non-parametrically. This is crucial because states not only differed significantly in their adopted new income eligibility rules, but also differed significantly in non-income-related aspects of their expansions. For example, states were also heterogeneous in their adoption of administrative measures and other efforts to facilitate Medicaid take-up for eligible families (National Governors Association, 1993). With non-parametric identification, I overcome any potential issue driven by these state-specific behaviors.

The key identifying assumption is that, in the absence of Medicaid expansion, treatment and control groups would have followed parallel trends. I test this assumption during the pre-expansion period and find no evidence of differential trends before the policy change. While this assumption cannot be directly tested post-expansion, I conduct a placebo test using the income dimension of Medicaid eligibility. If treatment and control groups exhibited different underlying trends, differences should appear even among parents whose incomes exceeded Medicaid eligibility thresholds. To assess this, I compare outcomes between parents in the treatment and control groups at different income levels to determine whether disparities emerge where Medicaid eligibility should not have played a role. The results show significant differences between groups of parents only within income ranges targeted by the expansion, with no effects among higher-income parents who remained ineligible, reinforcing the validity of the identification strategy.

I also examine whether my results reflect a broader economic trend affecting low-income families rather than the impact of expanding Medicaid. Since Medicaid expansion primarily targets low-income families, a mechanical correlation arise between increased Medicaid enrollment for children, and changes in parental health insurance coverage. To test this, I examine differences in outcomes between low- and high-income parents within both the treatment and control groups. If broader economic trends were driving the decline in insurance coverage, similar differences should appear between low- and high-income parents, regardless of whether they belonged to the treatment group (highly exposed to Medicaid expansion) or the control group. However, the results do not support this hypothesis. While significant differences emerge between low- and high-income parents within the treatment group, no effects appear in the control group. These findings reinforce that Medicaid expansion, rather than broader economic trends, drove the observed shifts in insurance coverage.

This paper contributes to several strands of the literature. First, it introduces spillover effects as a novel mechanism in the debate on Medicaid expansion and private insurance crowding out, showing how public coverage for some family members influences the insurance decisions of non-targeted members. Second, it provides a new explanation for the decline in private insurance and the rise in uninsurance among working-age adults from the late 1980s to 2010, quantifying these effects from 1987 to 1993. Third, it emphasizes the importance of family-level insurance decisions over an individual-level approach, showing how policies targeting children and pregnant women shape broader household coverage patterns.

Following OBRA-1986 and subsequent Medicaid reforms, researchers have extensively examined Medicaid expansion's impact on health insurance coverage for targeted populations. Cutler and Gruber (1996) first showed that while Medicaid expansion increased public coverage, it also reduced private insurance enrollment, highlighting crowding out effects among newly eligible individuals. Since then, numerous studies have investigated this issue.[4] Gruber and Simon (2008) provides a comprehensive review of these studies and further evidence of crowding out during the State Children's Health Insurance Program (SCHIP) expansion. Moreover, crowding out effects arise not only from Medicaid expansion but also from reductions in public insurance availability (Garthwaite, Gross, and Notowidigdo, 2014). These studies primarily examine how changes in

[4] A non-exhaustive list includes Dubay and Kenney (1996), Dubay and Kenney (1997), Thorpe and Florence (1998), Blumberg et al. (2000), Yazici and Kaestner (2000), Card and Shore-Sheppard (2004), Ham and Shore-Sheppard (2005), and Shore-Sheppard (2008), which analyze a similar time frame as this paper. Additionally, Hudson, Selden, and Banthin (2005) and Sasso and Buchmueller (2004) focus on the SCHIP expansion of the late 1990s.

public insurance affect those who gain or lose eligibility. However, Medicaid expansion can also influence the insurance decisions of non-eligible family members, creating spillover effects. My analysis highlights how changes in public insurance availability for some family members affect the coverage decisions of those who remain ineligible.

The second related strand of literature examines the factors behind the decline in private insurance coverage and the rise in the uninsured rate among the working-age population from the late 1980s to the introduction of the Affordable Care Act (ACA) in 2010.[5] During this period, private coverage declined sharply, while the share of uninsured adults increased substantially. Despite extensive research, the underlying causes of these trends remain only partially understood. Gruber (2008) provides a comprehensive survey of this literature and highlights the puzzling nature of these patterns. Existing research attributes these trends primarily to rising healthcare costs and insurance premiums (Chernew, Cutler, and Keenan, 2005a,b; Kronick and Gilmer, 1999; Shen and Long, 2006), as well as declining take-up rates among low-income and less-educated adults, rather than a reduction in employer-sponsored insurance offerings (Farber and Levy, 2000; Cutler, 2003). Using a structural model with endogenous health insurance choices, Hai (2015) find that rising healthcare costs and skill-biased technological change explain a substantial share of the observed trends. Similarly, Zhao (2017) examine how social insurance policies, including Medicaid, influence savings, health insurance, and labor supply. Their findings suggest that these policies can significantly affect private insurance demand, though they do not provide any empirical validation nor quantification.

While this literature focuses on broader economic forces, it largely overlooks how changes in public insurance eligibility for some household members affect non-eligible individuals within the same family. This paper introduces a novel and empirically unexplored mechanism: the spillover effects of Medicaid expansion on parental insurance coverage. Previous studies fail to account for family-level interdependencies and do not consider how Medicaid expansions targeting children and pregnant women influence parental insurance decisions. My findings show that Medicaid

[5] The ACA introduced significant regulatory changes that transformed the health insurance market.

expansion increased the likelihood that parents dropped private coverage and remained uninsured. While the decline in private coverage among women is explained by a crowding out effect, the decline among men and the rise in uninsurance stem from spillover effects. Additionally, this paper quantifies the extent to which Medicaid expansion contributed to these trends between 1987 and 1993.

Finally, this paper contributes to the emerging literature on health insurance demand by emphasizing the importance of family-level insurance decisions. It aligns closely with Hamersma, Grossman, and Tello-Trillo (2023), a contemporary study which revisit Medicaid expansion by focusing on the family as the primary unit of analysis. Similarly, Grossman, Tello-Trillo, and Willage (2022) explore the impact of Medicaid expansion for children on maternal outcomes. Both studies leverage on state-level variation in simulated eligibility to identify their effects. This paper is most closely related to Koch (2015), who finds that Medicaid eligibility for children reduces private insurance coverage for parents. However, our approaches differ in key ways. Koch abstracts from adult and pregnancy-related eligibility, employs a regression discontinuity design based on income thresholds, focuses on the SCHIP expansion,[6] and employs the Medical Expenditure Panel Survey. In contrast, I account for both adult and pregnancy-related eligibility and apply a difference-in-differences framework to analyze earlier Medicaid expansions from the late 1980s and early 1990s, using the Current Population Survey. Despite these differences, our studies provide evidence that Medicaid eligibility for family members influences the insurance coverage of non-eligible household members.

The rest of the paper is structured as follows. Section 2 briefly overviews Medicaid expansions during the late 80s and early 90s. Section 3 outlines the methodology and describes the dataset. Section 4 presents the baseline results and the robustness exercises. Section 5 quantifies these effect toward observed trends. Section 6 discusses my results. Finally, Section 7 concludes.

[6]The SCHIP expansion targeted older children and those from higher-income families compared to the Medicaid expansions of the late 1980s and early 1990s.

II Expanding Medicaid for children and pregnant women

Established in 1965, Medicaid initially aimed at providing health insurance to impoverished adults and their dependent children. The first notable expansion of Medicaid for children and pregnant women began with OBRA 1986. Medicaid eligibility was originally tied to the State Aid to Families with Dependent Children (AFDC) eligibility, where state authorities determine eligibility guidelines. OBRA 1986 marked the first endeavor to detach Medicaid eligibility from AFDC eligibility by establishing specific income thresholds for targeted populations and leveling up Medicaid eligibility rules across states.

Under this act, states had the option to cover children up to 5 years old and pregnant women with income up to 100% the Federal Poverty Line (FPL). However, the coverage for children was not immediate. The program started by covering children under 1 year old and then gradually incorporated older children on a yearly basis.[7] Subsequently, the OBRA of 1987 presented states with the option to raise the eligibility threshold to 185% of the FPL and to expedite the phase-in process.[8]

Then, the Medicare Catastrophic Coverage Act (MCCA) of 1988 mandated states to cover pregnant women and infants from families with income up to 100% FPL.[9] The OBRA 1989 required states to cover pregnant women and children up to 6 years old with a family income up to 133% of the FPL.[10]

Subsequently, OBRA 1990 required states to cover children born on or after October 1st, 1983 in families with income up to 100%. This act aimed to provide coverage for all children below poverty by the year 2002.[11] However, in 1997, the State Children's Health Insurance Program was launched, giving states the choice to cover children under 19 in families with income up to 185%

[7] Under OBRA 1986, the oldest cohort covered were children born on October 1, 1985.

[8] Those states that took advantage of this option accelerated the phase-in process, resulting in the oldest cohort covered being children born on October 1, 1983.

[9] The MCCA 1988 was implemented in a two-year gradual process. It requires states to cover the targeted group up to 75% of the FPL by July 1989, and up to 100% of the FPL by July 1990.

[10] Effective date April 1990. The oldest cohort affected by this policy were children born on April 1, 1984.

[11] Dependent children up to 19 years old.

of the FPL.

Federal acts and state adoptions did not occur simultaneously. Adoption and eligibility criteria were ultimately determined by states. Specific federal guidelines, such as OBRA 1988, MCCA 1989, and OBRA 1990, provided a lower bound for eligibility criteria, but states had the liberty to expand upon these criteria. In addition to income-based eligibility expansions, states implemented varied administrative measures to facilitate Medicaid enrollment for the targeted population (National Governors Association, 1993). Importantly, with the exception of pregnant women, parental Medicaid eligibility remained tied to AFDC guidelines throughout this period.[12]

A distinctive feature of this expansion is that starting with OBRA 1986, the oldest targeted cohort of children were those born on October 1st. 1983, or after. Older cohorts typically qualified for Medicaid through AFDC eligibility. Since expansion for pregnant women was tied to those of infants, this regulatory characteristic provides a natural framework to study the effects of increasing eligibility for children and pregnant women on parents' outcomes. The following section provides details of the empirical strategy.

III Empirical strategy

Estimating the causal effect of Medicaid expansions for children and pregnant women presents several challenges. As described above, the late 1980s and early 1990s saw a wave of heterogeneous Medicaid reforms, with multiple policy changes occurring simultaneously and targeting different populations. States differed in pre-expansion AFDC guidelines, population characteristics, and post-expansion income eligibility criteria. Additionally, variation in administrative measures for Medicaid enrollment presumably played a significant role in differences in program take-up. Given these factors, the impact of Medicaid expansion likely varied across states. To address this heterogeneity, I implement a state-level difference-in-differences design that leverages two key

[12]Some states expanded parental eligibility in the early 1990s through new state public programs or AFDC modifications. However, in most states, these guidelines remained largely unchanged during this period. For states that implemented significant changes, I include them in the analysis only for the years prior to their expansion or modification of parental eligibility. Appendix B provides a detailed description.

features of Medicaid expansion. For each state, I first assign treatment date by isolating significant policy-driven changes in eligibility. Second, I define treatment and control groups by focusing on a subset of the population explicitly targeted by the expansion. I then compare relative outcomes for these groups before and after the policy change. Finally, I aggregate these effects to present national-level causal effects. The remainder of this section details the dataset, treatment date determination, assignment of control and treatment groups, and estimation strategy for the average treatment effect of Medicaid expansion for children and pregnant women.

III.1 Data

My data on health insurance coverage comes from the Current Population Survey (CPS). I use data from the March Annual Social and Economic Supplement of the Current Population Survey (March CPS) which contains specific questions on income, poverty, and health insurance status. This survey offers a comprehensive view of all states in the United States through a nationally representative sample. It captures individual-level data, such as age, gender, household relationships, state of residence, income, and health insurance coverage for each household member. To determine a respondent's health insurance status for 1987-1993, I use questions from the 1988-1994 March CPS which refer to the respondent's health insurance coverage in the previous year. For all health insurance variables, I use health insurance variables and sample weights created by the State Health Access Data Assistance Center at the University of Minnesota.[13] Since the wording of health insurance questions changed in 1995 March CPS (U.S. Census Bureau, 2014), I restrict my analysis to 1987–1993 to avoid introducing biases from survey rephrasing.

My household definition differs from the household definition used in the CPS. I conduct my analysis at the Health Insurance Unit (HIU) level. The HIU encodes family relationships relevant to health insurance coverage and eligibility criteria, allowing for a more accurate and consistent assessment of health insurance outcomes.[14] I obtain the 1988-1994 March CPS from IPUMS.[15] I

[13] A full description can be found at: https://cps.ipums.org/cps-action/variables/HINSWT#description_section

[14] A full description can be found at: https://cps.ipums.org/cps-action/variables/HIUID#description_section.

[15] Flood, King, Rodgers, Ruggles, Warren, Backman, Chen, Cooper, Richards, Schouweiler, and Westberry (2023)

restrict my analysis to parents aged 25-64 with dependent children.

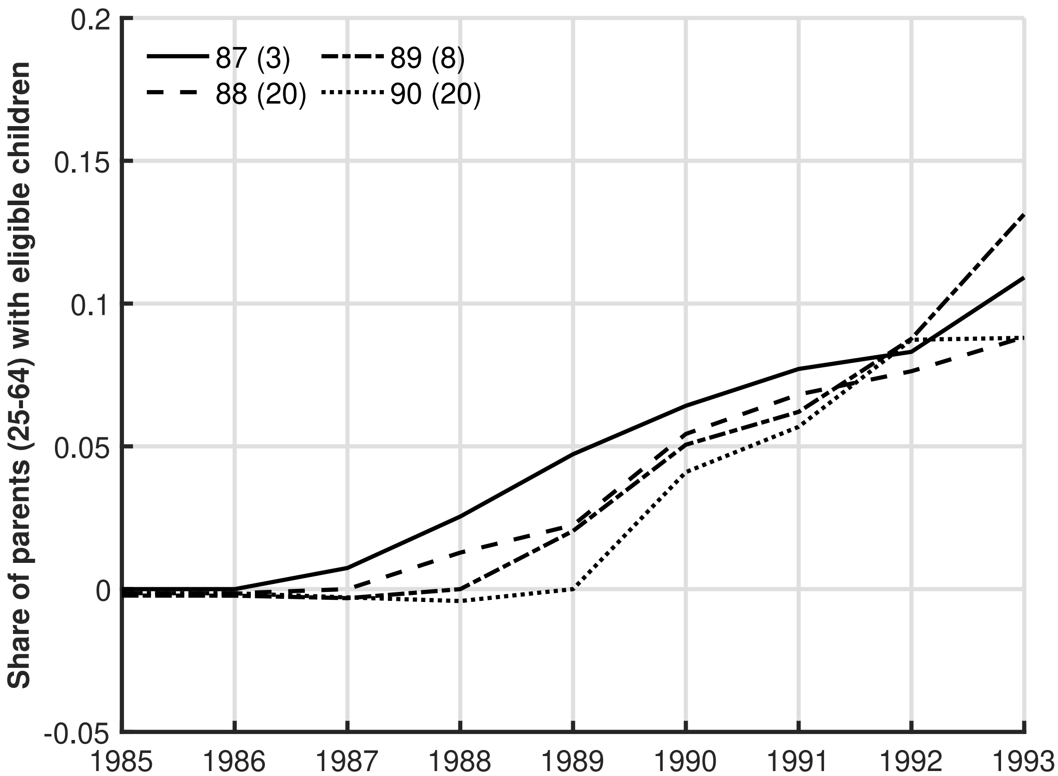
I construct Medicaid eligibility rules for parents, pregnant women, and children using the source files from Brown, Kowalski, and Lurie (2020), supplemented with additional historical data. Specifically, I develop state-specific Medicaid income eligibility rules based on age, pregnancy status, and year. I then apply these rules in two ways. First, I use them to create state-specific aggregate *simulated eligibility* measures, which help determine expansion dates for each state, as described in Section III.2. Second, I measure Medicaid eligibility directly for both the treatment and control groups, allowing me to validate my research design. In both cases, I define two key Medicaid eligibility measures: child eligibility, which identifies parents with at least one Medicaid-eligible child, and adult eligibility, which identifies Medicaid-eligible parents.

As in much of the Medicaid expansion literature, I focus only on income-based eligibility rules, abstracting from other factors such as asset tests or disability status. While this simplifies the complexity of the Medicaid eligibility system, I do not use these rules directly to estimate the effect of Medicaid expansion. Instead, they serve two key purposes: (1) verifying whether treatment and control group assignments accurately capture within-state differential exposure to Medicaid expansion and (2) constructing a state-specific simulated eligibility measure to track legislative changes and identify expansion years for each state.

III.2 Assigning treatment date

Given state heterogeneity in pre-expansion AFDC guidelines and post-expansion income eligibility criteria, determining how these policy changes influenced Medicaid eligibility in each state is challenging. To determine the expansion date of each state, I rely on simulated eligibility measures to assign treatment dates in a similar approach than East et al. (2023). Particularly, using a simulated sample of parent and their children, I apply medicaid eligibility rules and calculate the share of parents whose children are Medicaid eligible as well as the share of parents that are medicaid eligible themselves. This approach allows me to derive aggregate simulated eligibility measures that reflect changes in policy rules alone. I then aggregate these measures by year and assign expansion dates based on a straightforward criterion: When did parents experience a significant increase in child eligibility relative to their own eligibility? Figure 2 illustrates simulated eligibility dynamics by state, grouped by their assigned expansion year. As shown, all states experienced a sharp eligibility expansion in 1987, 1988, 1989, and 1990, highlighting the rapid pace of expansion in the late 1980s. Appendix B provides a detailed breakdown of simulated eligibility by state from 1985 to 1993, along with the corresponding assigned expansion dates. Because health insurance data only became available in 1987, estimating the effects of Medicaid expansion for states that expanded that year is not possible, therefore I exclude them from my analysis.

Figure 1: Medicaid Expansion



Notes: Author's calculations from the Current Population Survey and Medicaid eligibility rules. The figure shows trends in eligibility relative to the expansion year by year. The groups are defined based on the assigned expansion year. The number of states in each group is listed in parentheses. See Appendix B for further details.

III.3 Treatment and control groups

A key feature of this expansion was that Medicaid eligibility for low-income children applied only to those born on or after October 1, 1983, while older children remained eligible under AFDC guidelines. This creates a natural exclusion restriction to identify families with high versus low exposure to the expansion. Ideally, parents with at least one child born on or after this date would experience the policy change directly. Additionally, pregnant women during the analysis period would automatically belong to this group.

However, because the CPS only reports children's ages at the time of the interview, I can only infer birth years rather than exact birth dates. Consequently, I define the treatment group as parents with at least one child born in 1983 or later, while the control group consists of parents with no children born in 1983 or after.

This age-based eligibility cutoff provides a reliable approach for defining high- versus low-exposure groups, and avoid exploiting the income dimension to define treatment and control group. While Medicaid expansion could influence fertility decisions, leading some households to move between control and treatment groups, the more immediate behavioral response would likely involve adjusting labor supply or under reporting income to qualify for Medicaid for already age-eligible children. Defining treatment and control groups based on income would require selecting an income threshold that ensures such behavior did not occur in the control group. Since any such threshold would be arbitrary, I avoid using income for primary group definitions and instead incorporate income-based analyses as a robustness check.

Nevertheless, there are two potential threats to identification that exist with this design. The first is that the control and treatment groups differ in their underlying composition, which may lead to a potential violation of parallel trend assumption between groups. I first test for pre-trends in states with sufficient pre-expansion data and find no evidence of a violation. While testing this assumption after expansion is not feasible, I conduct a placebo test, leveraging on the income dimension of Medicaid expansion. If treatment and control groups exhibited differential trends, I would expect to see differences even at income levels where children and pregnant women were ineligible for Medicaid. This placebo test shows no significant difference in outcomes between these groups for income levels typically not targeted in this expansion. On the other hand, when I compare outcomes between groups in income regions targeted by this expansion, I find significant differences between these groups. This exercise provides evidence that the age composition of children is not driving my results.

The second threat stems from state-level Medicaid expansions in the early 1990s, which extended eligibility to children born before October 1, 1983, or to adults through AFDC guideline changes. These expansions could contaminate the control group and introduce additional policy variation, invalidating my research design. To address this, I restrict the analysis for each state to periods where the control group remained untreated and no significant AFDC guideline changes occurred. Table 6 details these restrictions.

III.4 Identification, estimation and inference

Let $Y_{i,s,t}(0)$ denote the untreated potential outcome for unit i at time t in state s, that is the potential outcome if unit remains untreated throughout all available time periods, T_s. Let g represent the time period when unit i first receives treatment. Similarly, $Y_{i,s,t}(g)$ is the potential outcome for unit i at time t and state s, conditional on first receiving treatment in period g. Let $G_{i,g,s}$ indicate whether unit i in state s received treatment at time g. The observed outcome is related to potential outcomes by:

$$Y_{i,s,t} = Y_{i,s,t}(0) + \sum_{g} \big(Y_{i,s,t}(g) - Y_{i,s,t}(0)\big) 1\{G_{i,g,s} = 1\}$$

where $1\{G_{i,g,s} = 1\}$ equals 1 if unit i first receives treatment in period g, and 0 otherwise.

Since different potential outcomes cannot be observed simultaneously, I estimate the average treatment effect on the treated (ATT) for units in state s belonging to group g at time t, defined as:

$$ATT(s,g,t) = E[Y_{s,t}(g) - Y_{s,t}(0) | G_{g,s} = 1]$$

III.4.1 Identification:

To identify $ATT(s,g,t)$, the following assumptions are imposed:

Assumption 1: No treatment anticipation

$$E[Y_{s,t} | G_{g,s} = 1] = E[Y_{s,t}(0) | G_{g,s} = 1] \quad \text{for all } t < g$$

This implies $ATT(s,g,t) = 0$ for all pre-treatment periods.

Assumption 2: Parallel trends based on a "Never Treated" group

For each g and t such that $t \geq g$:

$$E[Y_{s,t}(0) - Y_{s,t-1}(0) | G_{g,s} = 1] = E[Y_{s,t}(0) - Y_{s,t-1}(0) | C_s = 1],$$

where $C_s = 1$ if $G_{g,s} = 0$ for all g, and 0 otherwise.

Under these assumptions, the ATT can be expressed as:

$$ATT(s,g,t) = E[Y_{s,t} - Y_{s,g-1} | G_{s,g} = 1] - E[Y_{s,t} - Y_{s,g-1} | C_s = 1].$$

Finally, the $ATT(s,g,t)$ parameter can be obtained by first subsetting the data to only contains observations at time t and $g-1$ for state s, from units with either $G_{i,s,g} = 1$ or $C_{i,s} = 1$ and running

$$Y_{i,s,t} = \alpha_1^{s,g,t} + \alpha_2^{s,g,t} G_{i,s,g} + \alpha_3^{s,g,t} 1\{T = t\} + \beta^{s,g,t}\big((G_{i,s,g} \times 1\{T = t\}\big) + \epsilon_{i,s,t}^{s,g,t} \qquad (1)$$

Then, $\beta^{s,g,t} = ATT(s,g,t)$.

III.4.2 Summarizing average treatment effects

To derive interpretable aggregate causal estimates, I aggregate $ATT(s, g, t)$ while avoiding the issues inherent in standard ”static” or ”dynamic” two-way fixed effects (TWFE) linear regression models. Recent studies Goodman-Bacon (2021); Sun and Abraham (2021) show that TWFE regression coefficients often recover a weighted average of treatment effects, with some weights potentially being negative. By applying the methodology of Callaway and Sant'Anna (2021), I aggregate these causal parameters in a straightforward and transparent manner to address specific questions.

First, I estimate the average treatment effect at event-time $e = t - g$ for states that participated in the treatment for exactly e periods and initiated treatment in period g:

$$\theta_{es}(e, g) = \frac{1}{k} \sum_{s} \omega(s, g, g + e) ATT(s, g, g + e) \tag{2}$$

where $\omega(s, g, g + e) = 1\{g + e \leq T_s\} \sum_i \omega_{i,s,g+e} 1\{G_{i,s,g} = 1\}$ and $k = \sum_s \omega(s, g, g + e)$. Here $\omega_{i,s,g+e}$ is the a specific weight of unit i, at state s, at time period $g + e$

Next, I aggregate ATT to estimate the average treatment effect at event-time $e = t - g$ across all groups observed to have participated in treatment for e periods:

$$\theta_{es}(e) = \frac{1}{k} \sum_{g} \sum_{s} \omega(s, g, g + e) ATT(s, g, g + e) \tag{3}$$

where $\omega(s, g, g + e)$ is defined as before, and $k = \sum_g \sum_s \omega(s, g, g + e)$.

This parameter, $\theta_{es}(e)$, serves as a natural target for event-study regressions commonly used in applied research, while fully addressing the pitfalls of dynamic TWFE models. Importantly, the weights account for the relative sizes of treated groups across states, reflecting how heterogeneous treatment effects contribute to these aggregate parameters.

Finally, I aggregate ATT into an overall effect of treatment participation:

$$\theta^{post} = \frac{1}{k} \sum_{g} \sum_{s} \sum_{t} \omega(s, g, t) ATT(s, g, t) \tag{4}$$

for $t \geq g$, where $k = \sum_g \sum_s \sum_t \omega(s, g, t)$ ensures that weights sum to 1.

By assumption, $ATT(s, g, t) = 0$ for $t < g$. To evaluate pre-treatment dynamics, I estimate $ATT(s, g, t)$ for these periods using pre-treatment data and construct corresponding aggregate parameters for $\theta_{es}(e)$ when $e < -1$. Additionally, I define θ^{pre} as the counterpart to Equation 4, capturing the average pre-treatment effects for $t < g$. I can then test for pre-trend differences between groups.

III.4.3 Estimation and inference

I estimate $ATT(s, g, t)$ by estimating $\beta^{s,g,t}$ in equation (1) using data from time t and $g - 1$ for state s, restricting observations to those that satisfy either $G_{i,s,g} = 1$ or $C_{i,s} = 1$. While inference for $\hat{ATT}(s, g, t)$ relies on standard asymptotic distribution theory, conducting inference on the aggregate measures $\hat{\theta} = (\hat{\theta}_{es}(e, g), \hat{\theta}_{es}(e), \hat{\theta}^{post}, \hat{\theta}^{pre})$ requires estimating the full variance-covariance matrix of $\hat{ATT}$. Since I conduct the estimation separately for each state, I compute standard errors using a block-bootstrap procedure. Specifically, for each state and survey year, I sample CPS families with replacement using the households' ASEC sampling weights. In each resample, I adjust individual weights so that they aggregate to the state-year totals reported in the CPS. I then estimate $\hat{\theta}$ for each bootstrapped sample and repeat this procedure 100 times. I compute the bootstrap-based standard error as the standard deviation of the point estimates across replications. This procedure allows me to derive standard error for these aggregate measures and at the same time to accounts for the sampling error in cell-means.[16]

[16]Garthwaite et al. (2014) discuss survey response errors in health insurance variables in the CPS and implement a bootstrapped procedure similar to the one used in this study.

IV Results

This section present the main empirical results. I first examine the impact of Medicaid expansion on public health insurance coverage, followed by its effects on private insurance and overall insurance rates. In Section IV.3, I analyze heterogeneous effects by gender, and in Section IV.4, I assess the robustness of my findings.

Table 1: Main estimates

$\theta_{es}(e)$	Children Medicaid Elig.			Children on Medicaid			Medicaid Elig.			Medicaid			Private			Any		
	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women
	1.a	1.b	1.c	2.a	2.b	2.c	3.a	3.b	3.c	4.a	4.b	4.c	5.a	5.b	5.c	6.a	6.b	6.c
-3	-0.007	-0.001	-0.009	-0.02	-0.009	-0.025	-0.005	0.003	-0.009	-0.009	0.004	-0.017	0.011	-0.011	0.026	0.004	0	0.009
	(0.007)	(0.008)	(0.011)	(0.011)	(0.013)	(0.016)	(0.007)	(0.007)	(0.011)	(0.011)	(0.013)	(0.015)	(0.011)	(0.015)	(0.016)	(0.008)	(0.011)	(0.01)
-2	0.007	0.005	0.009	-0.001	-0.005	0.005	0.009	0.008	0.01	0.004	-0.002	0.01	-0.008	-0.01	-0.008	-0.005	-0.007	-0.003
	(0.006)	(0.007)	(0.01)	(0.008)	(0.011)	(0.012)	(0.006)	(0.007)	(0.01)	(0.008)	(0.01)	(0.012)	(0.011)	(0.014)	(0.014)	(0.007)	(0.012)	(0.01)
-1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
0	0.041	0.041	0.041	0.021	0.018	0.025	0.017	0.007	0.027	0.018	0.01	0.024	-0.013	-0.015	-0.011	-0.003	-0.011	0.005
	(0.004)	(0.005)	(0.007)	(0.007)	(0.008)	(0.01)	(0.004)	(0.004)	(0.007)	(0.007)	(0.008)	(0.01)	(0.007)	(0.01)	(0.011)	(0.005)	(0.008)	(0.007)
1	0.099	0.089	0.107	0.031	0.028	0.032	0.022	0.005	0.036	0.015	0.006	0.022	-0.027	-0.029	-0.024	-0.012	-0.023	-0.003
	(0.005)	(0.006)	(0.007)	(0.006)	(0.009)	(0.01)	(0.004)	(0.005)	(0.007)	(0.006)	(0.009)	(0.01)	(0.007)	(0.01)	(0.01)	(0.006)	(0.009)	(0.007)
2	0.139	0.125	0.151	0.04	0.041	0.038	0.026	0.009	0.041	0.017	0.011	0.021	-0.027	-0.028	-0.025	-0.013	-0.021	-0.005
	(0.006)	(0.006)	(0.009)	(0.007)	(0.007)	(0.011)	(0.005)	(0.005)	(0.008)	(0.007)	(0.007)	(0.01)	(0.008)	(0.011)	(0.011)	(0.005)	(0.008)	(0.007)
3	0.172	0.148	0.191	0.055	0.05	0.058	0.032	0.009	0.051	0.025	0.009	0.037	-0.026	-0.021	-0.029	-0.007	-0.015	-0.001
	(0.006)	(0.007)	(0.009)	(0.008)	(0.01)	(0.012)	(0.005)	(0.006)	(0.008)	(0.008)	(0.009)	(0.011)	(0.008)	(0.013)	(0.011)	(0.006)	(0.01)	(0.008)
4	0.174	0.139	0.201	0.088	0.076	0.096	0.027	0	0.048	0.042	0.018	0.061	-0.045	-0.032	-0.055	-0.015	-0.024	-0.008
	(0.006)	(0.007)	(0.01)	(0.009)	(0.011)	(0.012)	(0.005)	(0.005)	(0.009)	(0.008)	(0.01)	(0.012)	(0.009)	(0.014)	(0.013)	(0.008)	(0.012)	(0.01)
5	0.171	0.132	0.198	0.085	0.072	0.092	0.02	-0.002	0.036	0.044	0.024	0.057	-0.057	-0.038	-0.068	-0.019	-0.016	-0.021
	(0.008)	(0.007)	(0.012)	(0.011)	(0.013)	(0.014)	(0.006)	(0.005)	(0.011)	(0.01)	(0.012)	(0.014)	(0.011)	(0.014)	(0.015)	(0.008)	(0.012)	(0.012)
θ^{pre}	0.001	0.003	0.002	-0.008	-0.007	-0.006	0.004	0.006	0.002	-0.001	0.001	0	-0.001	-0.01	0.005	-0.001	-0.004	0.002
	(0.005)	(0.006)	(0.009)	(0.008)	(0.01)	(0.012)	(0.005)	(0.006)	(0.009)	(0.008)	(0.01)	(0.012)	(0.009)	(0.012)	(0.013)	(0.006)	(0.01)	(0.008)
θ^{post}	0.128	0.11	0.142	0.048	0.043	0.051	0.025	0.005	0.04	0.024	0.012	0.034	-0.029	-0.026	-0.031	-0.011	-0.018	-0.004
	(0.004)	(0.004)	(0.006)	(0.005)	(0.006)	(0.008)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.008)	(0.006)	(0.008)	(0.008)	(0.004)	(0.007)	(0.006)
N	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644

Notes: Estimates are based on parents aged 25–64 between 1987 and 1993. Columns (left to right) report the following outcomes: (1) Share of parents with Medicaid-eligible children; (2) Share of parents with children covered by Medicaid; (3) Share of parents eligible for Medicaid; (4) Share of parents enrolled in Medicaid or public health insurance; (5) Share of parents with private health insurance; and (6) Share of parents with any health insurance. Sub columns a, b, c correspond to estimates for for the full sample, the male subsample, and the female subsample, respectively. N represent the number of state-year-group observations. State-year clustered bootstrapped standard errors are reported in parentheses. See text for additional details.

IV.1 Medicaid expansion and increase in Medicaid enrollment

The top panel of Table 1 reports the estimates for $\theta_{es}(e)$, while the bottom panel presents the estimates for θ^{Post} and θ^{Pre}. Figure 2 illustrates $\theta_{es}(e)$ in an event-study design. Columns 1.a and 3.a refer to the estimates for child and parent eligibility, respectively. Column 1.a captures the estimated change in the share of parents whose children became Medicaid-eligible in the treatment group relative to the control group. Column 3.a captures the estimated change in the share of parents who became Medicaid-eligible in the treatment group relative to the control group.

Difference in pre-expansion trend between treatment and control groups are small and no significant. Following the Medicaid expansion, the treatment group experienced a substantial increase in child eligibility. Parental eligibility also increased significantly. The subsequent gender analysis attributes this increase primarily to expanded eligibility for pregnant women. Notably, the magnitude of the increase in parental eligibility is significantly smaller than the increase in child eligibility. These findings align with the treatment and control group assignment and the anticipated effects of the Medicaid expansion.

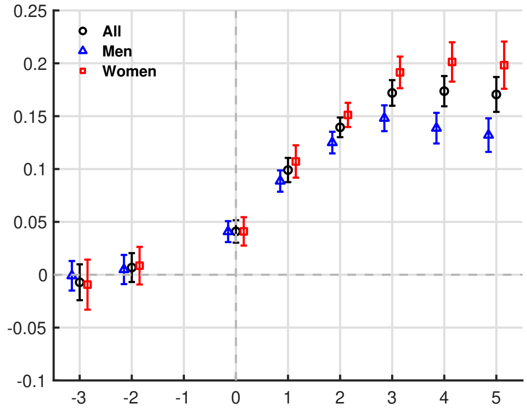
Eligibility increases alone do not guarantee changes in parental outcomes unless they result in higher enrollment rates. Column 2.a provides evidence by presenting estimates of $\theta_{es}(e)$ for the fraction of parents with children enrolled in Medicaid. As expected, pre-expansion coefficients indicate no significant differences between treatment and control groups. Post-expansion, parents began to enroll their children. Take-up estimates,[17] defined as the ratio between $\theta_{es}(e)$ between Column 2.a and 1.a, range between [0.51-0.28]. Also, the exercise shows that the increase in enrollment was slower than the registered increase in eligibility, suggesting that the program took time to take off.

Column 3.a presents the estimates of $\theta_{es}(e)$ for the fraction of parents enrolled in Medicaid. The results indicate a significant increase in public coverage for adults. Consistent with the smaller scale of the pregnant women expansion, the rise in adult Medicaid enrollment is substantially smaller than the increase in children's Medicaid enrollment. This pattern suggests that while many parents ensured their children obtained Medicaid coverage, they themselves remained largely ineligible. When analyzing the estimated Pre- and Post-expansion average effects, I find that on average, children's eligibility increased by 12.8 percentage points (column 1.a), while actual Medicaid enrollment increased by 4.8 percentage points (column 2.a), yielding an average take-up rate of 37.7%. For adults, eligibility increased on average by 2.5 percentage points (column 3.a), while

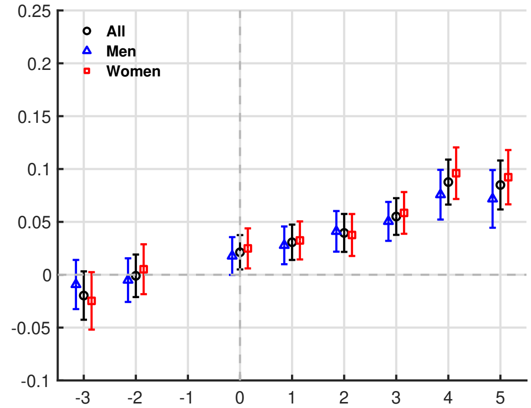
[17]This definition of take-up rate,is based on whether parents with at least one eligible child, enroll at least one of their child. Importantly, because Medicaid eligibility is determined by factors beyond income thresholds, these estimates should be interpreted with caution.

actual enrollment increased by 2.4 percentage points (column 4.a), suggesting a higher take-up rate among adults

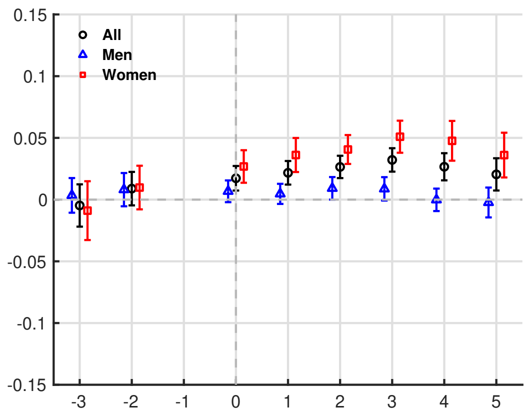
Figure 2: Event-study estimates



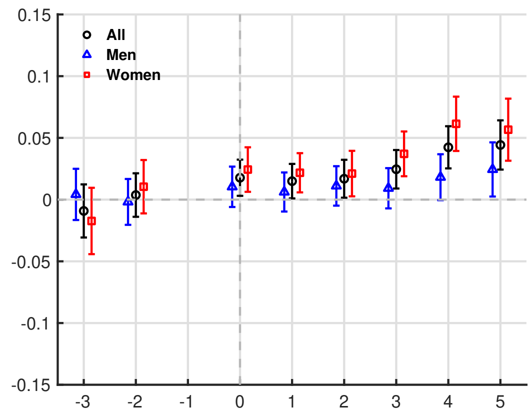
(a) Parents w/child Medicaid eligible



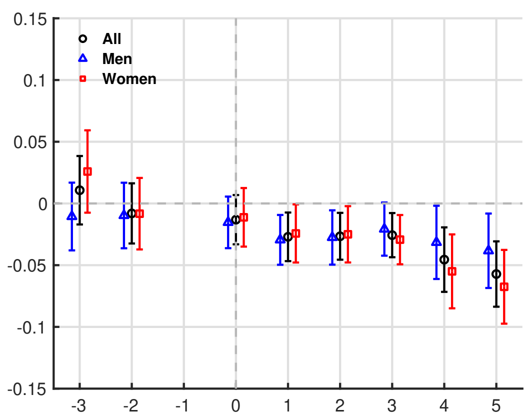
(b) Parents w/child on Medicaid



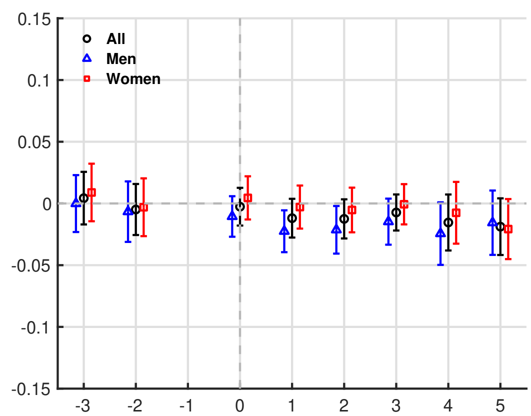
(c) Parents Medicaid eligible



(d) Parents on Medicaid



(e) Parents on private



(f) Parents on any insurance

Notes: Event time estimates refer to $\hat{\theta_{es}}(e)$. Estimates are based on parents aged 25–64 between 1987 and 1993. Black, blue, and red lines represent estimates for All, Men, and Women, respectively. Panel (a) shows the share of parents with Medicaid-eligible children. Panel (b) shows the share of parents with children covered by Medicaid. Panel (c) shows the share of parents eligible for Medicaid. Panel (d) shows the share of parents on Medicaid/public health insurance. Panel (e) shows the share of parents with private health insurance. Panel (f) shows the share of parents with any health insurance. Bootstrapped standard errors are used, with 95% confidence intervals reported. See text for additional details.

IV.2 Medicaid expansion, private coverage and overall insurance status

I examine the effect of Medicaid expansion on private and overall insurance. Columns 5.a and 6.a report estimates for the share of parents covered by private health insurance and any health insurance, respectively. Pre-expansion coefficients for private health insurance are close to zero and not statistically significant, indicating no evidence of pre-trend differences. Following the expansion, private coverage declined significantly for parents in the treatment group relative to the control group, with effects statistically significant at conventional levels. For overall insurance, pre-trend estimates are near zero and not significant. Post-expansion, I find negative but statistically insignificant point estimates. When I examine pre- and post-expansion averages, I find no significant pre-treatment differences in either outcome. Post-expansion, I find that private coverage declines by 2.9 percentage points, while overall coverage falls by 1.1 percentage points. Both point estimates are statistically significant

IV.3 Heterogeneous effect by gender

The estimates above show that Medicaid expansion led to significant changes in parent's health insurance outcomes. During this period, adult Medicaid eligibility was primarily link to AFDC eligibility, with a notable exception: Low-income pregnant women. To further explore these findings, I re do my analysis separately by gender.

IV.3.1 Male parent

Sub-columns b in Table 1 report the estimates for male parents. Pre-expansion differences between the treatment and control groups remain close to zero for both child (column 1.b) and parent Medicaid eligibility (column 2.b). Following the expansion, the share of male parents with Medicaid-eligible children rises significantly in the treatment group, while their own Medicaid eligibility remains unchanged (column 3.b). This suggests that the observed increase in adult eligibility stems entirely from the expansion targeting pregnant women.

Program participation, measured by the share of children covered by Medicaid, increases significantly for male parents in the treatment group, with negligible and statistically insignificant pre-expansion trend differences. The average take-up rate for male parents reaches 39.5%.

Regarding insurance outcomes, private insurance coverage declines significantly among male parents (column 5.b), leading to a reduction in overall insurance coverage (column 6.b). Public coverage (column 4.b) remains unaffected in the early years of the expansion, although it increases significantly by event time 5. Examining pre- and post-expansion averages, I find no significant pre-treatment differences in either outcome. Post-expansion, private coverage declines by 2.6 percentage points, overall coverage decreases by 1.8 percentage points, and public coverage rises by 1.2 percentage points.

IV.3.2 Female parents

Sub columns c in Table 1 reports the estimates for female parents. Pre-expansion differences between the treatment and control groups are close to zero for both child (column 1.c) and parent Medicaid eligibility (column 3.c). Following the expansion, the share of female parents with Medicaid-eligible children rises significantly in the treatment group, along with a smaller but significant increase in their own Medicaid eligibility.

The share of female parents with children on Medicaid increased significantly in the treatment group relative to the control group, with insignificant pre-expansion trend differences. Additionally, there was an increase in female parent enrollment in Medicaid. The average post-expansion take-up rate for female parents is 36.1%. while their own take-up rate—reflecting both adult eligibility and enrollment—reaches 84%.

Regarding insurance outcomes, female parents experienced a significant decline in private insurance coverage, a significant increase in public insurance coverage, and no significant change in overall insurance status. Looking at pre- and post-expansion averages, pre-treatment differences in either outcome are insignificant and close to 0. Post-expansion, I find a decline of 3.1 percentage points in private coverage, an increase of 3.4 percentage points in public coverage, and with non-significant change in overall coverage.

IV.4 Robustness exercises

In this section I perform two robustness exercises exploring the validity of my research design and also examining traditional determinant explaining these trends. The first exercise explore whether my results arise from potential violation of the parallel trend assumption. The second exercise examine whether my results are consistent with a broader response from low-income families rather than from the expansion of Medicaid.

IV.4.1 Placebo test

A key concern is the potential violation of the parallel trends assumption between treatment and control groups. If these groups would have followed different trends in the absence of the expansion, differences in outcomes should appear regardless of their exposure to Medicaid eligibility. To assess this, I conduct a placebo test by restricting the analysis to income groups that were typically ineligible under the expansion. If differential trends exist, I should observe disparities in outcomes among parents whose incomes exceeded the Medicaid eligibility thresholds.

I first estimate $\theta_{es}(e)$, θ^{post}, and θ^{pre} for a sample of individuals whose household incomes ranged between two and four times the Federal Poverty Line (FPL), making them ineligible for Medicaid under this expansion. Table 2 and Figure 4 present the estimates. Across all key variables, both pre-and post-expansion averages remain small and statistically insignificant. No significant differences emerge between treatment and control groups around the time of expansion. For private and overall insurance coverage, all $\theta_{es}(e)$ estimates are close to zero and non-significant. Public coverage shows some significance at later event times, but the estimated effects are small in magnitude. Similarly, estimates for θ^{post} and θ^{pre} across all adult insurance outcomes—public, private, and total coverage—are negligible and statistically insignificant. These findings suggest no systematic differences between treatment and control groups at income levels unaffected by Medicaid eligibility, reinforcing the validity of my design.

I then repeat the analysis for low-income households, defined as those with incomes below 2 times the Federal Poverty Line (FPL).[18] Table 3 and Figure 5 report the estimates. Within this group, I observe a substantial increase in Medicaid eligibility, aligning with my research design. Following the expansion, the share of parents with Medicaid-covered children rises significantly in the treatment group relative to the control group. Post-treatment estimates indicate a significant decline of 3.3 percentage points in private insurance, partially offset by a significant 2.9 percentage point increase in public coverage. Overall insurance coverage declines by 1.1 percentage points, although this estimate is not statistically significant.

These results provide additional support for the validity of my empirical strategy. The absence of significant differences within the high-income group suggests that pre-existing trends are not driving the observed effects. In contrast, within the low-income group, I find expected patterns of increased Medicaid eligibility and shifts from private to public coverage.

IV.4.2 Exploring correlation with traditional determinants

The decline in health insurance rates has been widely attributed to skill-biased technological change and rising health insurance costs. Since Medicaid eligibility primarily targets low-income households, a natural correlation emerges between these factors, Medicaid child enrollment, and declining insurance coverage among low-income parents. To assess whether income levels, rather than Medicaid expansion, explain the observed patterns, I segment the sample into high-income[19] and low-income[20] households within both the treatment and control groups. If broader economic trends drove the decline in insurance coverage, I would expect to see similar patterns among low- and high-income parents, regardless of Medicaid eligibility.

I redefine treatment and control groups based on income. This approach allows me to compare

[18] The expansion primarily targeted children in households earning below 133% of the FPL for children under six and below 100% for older children, though some states extended eligibility further. Instead of applying state-specific thresholds, I use 200% of the FPL as a consistent upper-income cutoff, as most states did not cover children or pregnant women above this level during this period. Additionally, since this threshold is well above the Medicaid eligibility limits, potential behavioral responses related to labor supply adjustments or income manipulation are likely to be less severe above this cutoff.

[19] Parents with household income between 2 and 4 times the Federal Poverty Line.

[20] Parents with household income below two times the Federal Poverty Line.

low- and high-income parents within two distinct samples: (1) those with age-targeted children, where I expect Medicaid expansion to play a role, and (2) those without age-targeted children, where no policy-driven changes should occur.

Table 4 and Figure 6 present the estimates for the treatment group comparison. Before the expansion, eligibility coefficients remain statistically insignificant, reinforcing the validity of my identification strategy. After the expansion, child Medicaid eligibility rises sharply for low-income households relative to higher-income parents. This increase translates into a significant rise in the share of parents with Medicaid-covered children. At the same time, private insurance coverage declines, while public coverage increases. On average, post-treatment estimates indicate a 4.2 percentage point reduction in private insurance and a 3.2 percentage point increase in public insurance. The overall decline in insurance coverage reaches 1.2 percentage points, though this estimate is not statistically significant. When disaggregating by gender, I find that both fathers and mothers in the treatment group experience declines in private insurance coverage. However, while mothers see no significant change in total insurance due to increased public coverage, fathers exhibit a significant 3.8 percentage point decline in overall insurance.

Table 5 and Figure 7 display the estimates for the control group comparison. As expected, no evidence suggests significant trend shifts between low- and high-income parents without Medicaid-eligible children. All pre- and post-expansion average treatment effects estimates remain small and statistically indistinguishable from zero. Examining $\theta_{es}(e)$ further confirms that no systematic changes occurred around the expansion period or in the years immediately following it.

These findings reinforce that Medicaid expansion—not broader income trends—drove the observed shifts in insurance coverage. Among parents with age-targeted children, low-income households experienced increases in Medicaid eligibility and participation, alongside declines in private coverage. In contrast, among parents without age-targeted children, no systematic differences emerge between low- and high-income groups.

V The effect of Medicaid expansion on observed trends

In this section, I quantify the effects of Medicaid expansions on parental health insurance trends. Using the estimated $ATT's$ and under Assumption 2, I compute counterfactual trends by state in a no-expansion scenario by subtracting the $ATT's$ from the observed trends in the treatment group. I then aggregate these state-level estimates to construct a national estimate for health insurance rates. I quantify the effect of Medicaid expansion as the difference between the observed and conterfactual rates. I perform the analysis for all parents aged 25–64, as well as separately for men and women. Using my block-bootstrapped procedure, I calculate this effect for each bootstrapped sample and report the 5th and 95th percentiles of these effects.

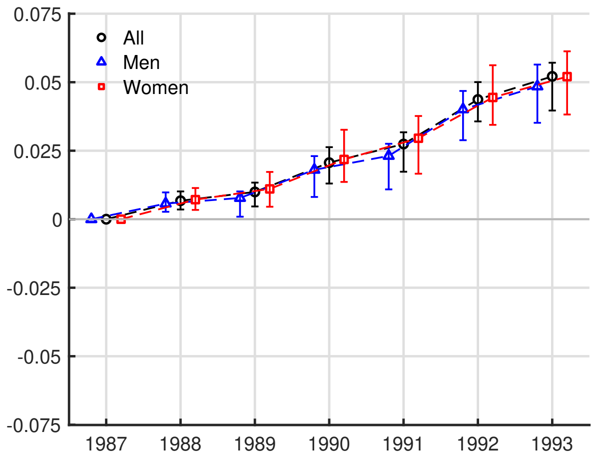
The analysis focuses on a subset of states that did not expand eligibility to children born before October 1, 1983, until 1993.[21] This restriction is necessary because I can not identify $ATT's$ for the full time period for those states.

Figure 3 showcase the estimated effects. From 1987 to 1993, Medicaid expansion significantly increased the share of parents with children enrolled in Medicaid by 5.2 percentage points [4.0 5.7]. It is also associated with a 2.8 percentage point [-3.7 -1.8] decline in private insurance coverage for parents, a 2.3 percentage point [1.3 2.9] increase in public insurance coverage, and a 1.1 percentage point [0.4 1.9] rise in the share of parents without any health insurance. These effects are heterogeneous by gender. Women experienced a 5.2 percentage point [3.8 6.1] increase in the share of mothers with children on Medicaid, along with a 3.0 [-4.4 -2.0] percentage point decline in private coverage and a 3.3 percentage point [2.1 4.3] increase in public insurance coverage. The change in uninsured mothers was statistically insignificant (-0.6 percentage point). Male parents, on the other hand, saw a 4.8 [3.5 5.6] percentage point increase in the share of fathers with children on Medicaid, a 2.3 [-3.3 -0.5] percentage point decline in private insurance coverage, and no significant change in public insurance (0.8 percentage point). For fathers, the lack of change in public insurance coverage translated into a 1.7 [0.6 2.7] percentage point increase in the share of

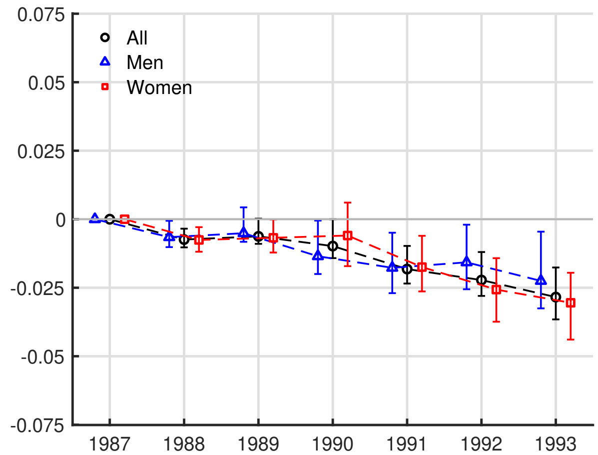
[21] These states are Georgia, Hawaii, Kentucky, New York, Vermont, Virginia and Washington.

uninsured male parents.

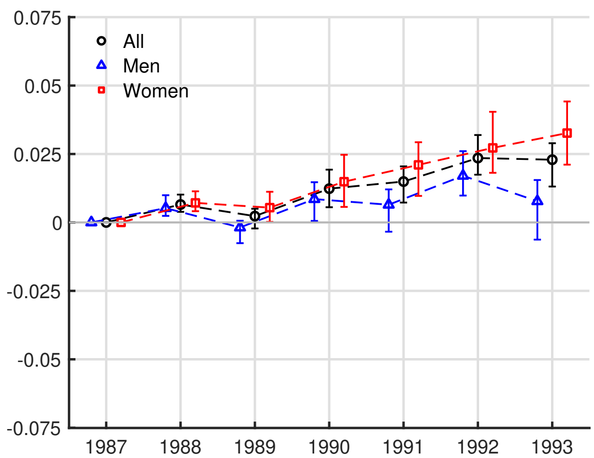
Figure 3: Effect of Medicaid expansion on the observed trends



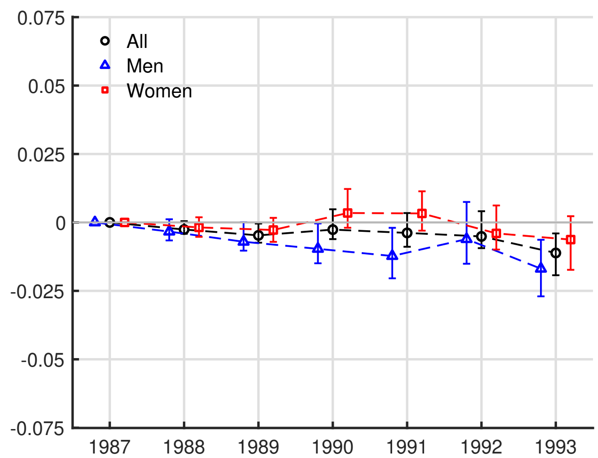
(a) Parents with Children covered by Medicaid



(b) Private Insurance



(c) Public Insurance



(d) Any Insurance

Notes: All estimates are reported in percentage points. Estimated for parents between 25-64 years old between 1987-1993. Regressions are weighted using CPS sample individual weights. Panel (a) refers to the share of parents with children covered by Medicaid. Panel (b) refers to the share of parents with private health insurance. Panel (c) refers to the share of parents with public health insurance. Panel (d) refers to the share of parents with any health insurance. [P5 - P95] bootstrapped confidence interval reported.

Taking the point estimates as a reference, Medicaid expansion accounts for a substantial portion of the observed trends in health insurance coverage during this period for these states. Specifically, it explains 78 percent of the increase in parents with children on Medicaid, 48 percent of the decline in private insurance coverage, 88 percent of the increase in public insurance coverage, and 31 percent of the rise in uninsured parents. These findings underscore the significant role Medicaid expansion played in shaping health insurance outcomes for parents during the late 1980s and early 1990s.

VI Discussion

My baseline specification suggests that the increase in Medicaid eligibility for children and pregnant women increases the number of parents with Medicaid-covered children and leads to a rise in public insurance enrollment. However, this increase does not fully offset the decline in private coverage, resulting in a higher share of uninsured parents.

A gender-specific analysis reveals strong heterogeneity in these effects. While both male and female parents experience declines in private insurance, their public insurance coverage responds differently. Among women, the increase in public insurance suggests a strong crowding out effect, as Medicaid substitutes for private coverage. In contrast, fathers, who were not directly targeted by the expansion, lose private insurance without a corresponding gain in public coverage, leading to a higher uninsured rate. This pattern reflects a spillover effect rather than a direct one, as the expansion primarily targeted children and pregnant women, yet indirectly influenced fathers' insurance outcomes through household-level adjustments in coverage decisions.

From a theoretical perspective, these findings can be explained by considering health insurance choices at the household level rather than the individual level. The interaction between public and private health insurance defines the set of health insurance contracts available to a household. Expanding Medicaid introduced new contract options for newly eligible families. For instance, families could now enroll children and pregnant mothers in Medicaid while other members either obtained private insurance or remained uninsured. In response, families adjusted their insurance decisions based on these new options.

More importantly, because Medicaid provided a free alternative, it altered households' willingness to pay for private insurance. From a unitary household perspective, increased public insurance availability for some family members can be interpreted as an increase in uncompensated care for uninsured households, as they no longer bear the full financial burden of medical care for Medicaid-covered household members. These shifts in uncompensated care help explain the decline in private insurance take-up rates (Finkelstein, Mahoney, and Notowidigdo, 2018).

Finally, my research design cannot distinguish whether these effects are driven by the increase in child eligibility or pregnant women's eligibility. Since both were linked, isolating their individual contributions is not feasible within my period of analysis.

VII Conclusions

Since the mid-1990s, Medicaid expansion has attracted substantial research attention. A large body of literature extensively documents its direct impact on health insurance coverage. This paper contributes to this discussion by exploring a relatively under explored mechanism—the spillover effects of Medicaid expansion on the health insurance coverage of non-eligible family members.

My analysis focuses on how increased Medicaid eligibility for children and pregnant women affects parental health insurance coverage. I find that while Medicaid expansion leads to a rise in public insurance enrollment among parents, it also reduces private coverage. However, this shift does not fully offset the loss of private insurance, resulting in an increase in uninsured parents, particularly among men.

Moreover, my analysis highlight the importance of analyzing health insurance decisions at the household level rather than the individual level. An individual-based approach overlooks the interactions between public health insurance availability for family members and overall household insurance decisions.

By incorporating income-based analyses as a robustness check, I confirm that the observed changes in parental coverage are driven by Medicaid expansion rather than broader economic trends. My results indicate that Medicaid expansion played a major role in shaping parental health insurance coverage—explaining much of the increase in parents with Medicaid-covered children and those enrolled in public insurance, while also accounting for a substantial share of the decline in private coverage and the rise in uninsured parents.

These results highlight the broader implications of Medicaid expansion, showing that its effects extend beyond the directly eligible population. Moreover it highlights that households willingness to pay for private coverage can be widely shaped by extending coverage to family members. Finally, these findings open a new avenue for future research and policy discussions, emphasizing the importance of analyzing health insurance demand at the household level rather than solely from an individual perspective.

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A Tables and figures

Table 2: Robustness exercise 1 a: High Income

$\theta_{es}(e)$	Children Medicaid Elig.			Children on Medicaid			Medicaid Elig.			Medicaid			Private			Any		
	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women
	1.a	1.b	1.c	2.a	2.b	2.c	3.a	3.b	3.c	4.a	4.b	4.c	5.a	5.b	5.c	6.a	6.b	6.c
-3	0	0	0	-0.019	-0.016	-0.021	0	0	0	-0.008	-0.004	-0.012	0.013	0.01	0.016	0.008	0.015	0.002
	(0)	(0)	(0)	(0.012)	(0.017)	(0.017)	(0)	(0)	(0)	(0.011)	(0.016)	(0.015)	(0.013)	(0.022)	(0.017)	(0.01)	(0.019)	(0.013)
-2	0	0	0	0.012	0.014	0.009	0	0	0	0.018	0.018	0.018	-0.007	-0.016	0.003	0.005	0.003	0.007
	(0)	(0)	(0)	(0.01)	(0.015)	(0.014)	(0)	(0)	(0)	(0.009)	(0.012)	(0.014)	(0.011)	(0.018)	(0.017)	(0.009)	(0.015)	(0.013)
-1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
0	0	0	0	0.016	0.014	0.017	0	0	0	0.014	0.009	0.019	0.002	0.006	-0.002	0.01	0.01	0.01
	(0)	(0)	(0)	(0.007)	(0.011)	(0.011)	(0)	(0)	(0)	(0.007)	(0.01)	(0.011)	(0.008)	(0.012)	(0.012)	(0.007)	(0.01)	(0.01)
1	0	0	0	-0.011	-0.011	-0.011	0	0	0	-0.016	-0.015	-0.016	0.008	0.011	0.006	0.003	0.004	0.003
	(0)	(0)	(0)	(0.008)	(0.012)	(0.011)	(0)	(0)	(0)	(0.007)	(0.011)	(0.011)	(0.008)	(0.013)	(0.012)	(0.007)	(0.012)	(0.009)
2	0.01	0.011	0.009	-0.002	0.003	-0.006	0	0	0	-0.005	0.002	-0.011	0.018	0.017	0.017	0.007	0.009	0.004
	(0.001)	(0.002)	(0.002)	(0.007)	(0.012)	(0.01)	(0)	(0)	(0)	(0.008)	(0.011)	(0.011)	(0.01)	(0.016)	(0.013)	(0.008)	(0.012)	(0.009)
3	0.015	0.016	0.013	0.021	0.028	0.017	0	0	0	0.006	0.008	0.004	0	0	-0.001	0.006	0.007	0.005
	(0.002)	(0.003)	(0.002)	(0.008)	(0.012)	(0.012)	(0)	(0)	(0)	(0.007)	(0.011)	(0.011)	(0.008)	(0.016)	(0.011)	(0.007)	(0.014)	(0.01)
4	0.019	0.019	0.019	0.035	0.037	0.033	0	0	0	0.019	0.024	0.016	-0.015	-0.009	-0.021	-0.005	0.002	-0.01
	(0.002)	(0.003)	(0.003)	(0.009)	(0.013)	(0.014)	(0)	(0)	(0)	(0.008)	(0.011)	(0.012)	(0.01)	(0.017)	(0.015)	(0.008)	(0.015)	(0.012)
5	0.003	0.003	0.003	0.036	0.039	0.034	0	0	0	0.018	0.023	0.015	-0.024	-0.024	-0.024	-0.01	-0.008	-0.01
	(0.001)	(0.001)	(0.001)	(0.012)	(0.016)	(0.015)	(0)	(0)	(0)	(0.011)	(0.016)	(0.013)	(0.013)	(0.018)	(0.018)	(0.011)	(0.016)	(0.016)
θ^{pre}	0	0	0	0	0.002	-0.002	0	0	0	0.008	0.009	0.006	0.001	-0.006	0.008	0.006	0.008	0.005
	(0)	(0)	(0)	(0.009)	(0.013)	(0.013)	(0)	(0)	(0)	(0.008)	(0.012)	(0.013)	(0.01)	(0.017)	(0.014)	(0.008)	(0.014)	(0.011)
θ^{post}	0.008	0.008	0.007	0.012	0.015	0.011	0	0	0	0.004	0.006	0.002	0.001	0.003	-0.001	0.003	0.005	0.002
	(0.001)	(0.001)	(0.001)	(0.005)	(0.007)	(0.007)	(0)	(0)	(0)	(0.005)	(0.007)	(0.007)	(0.006)	(0.009)	(0.008)	(0.005)	(0.008)	(0.007)
N	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644

Notes: Estimates are based on parents aged 25–64 with family income between 2 and 4 times the Federal Poverty Line between 1987 and 1993. Columns (left to right) report the following outcomes: (1) Share of parents with Medicaid-eligible children; (2) Share of parents with children covered by Medicaid; (3) Share of parents eligible for Medicaid; (4) Share of parents enrolled in Medicaid or public health insurance; (5) Share of parents with private health insurance; and (6) Share of parents with any health insurance. Sub columns a, b, c correspond to estimates for for the full sample, the male subsample, and the female subsample, respectively. N represent the number of state-year-group observations. State-year clustered bootstrapped standard errors are reported in parentheses. See text for additional details.

Table 3: Robustness exercise 1 b: Low Income

	Children Medicaid Elig.			Children on Medicaid			Medicaid Elig.			Medicaid			Private			Any		
$\theta_{es}(e)$	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women
	1.a	1.b	1.c	2.a	2.b	2.c	3.a	3.b	3.c	4.a	4.b	4.c	5.a	5.b	5.c	6.a	6.b	6.c
-3	-0.021	-0.008	-0.02	-0.037	-0.026	-0.033	-0.012	0.013	-0.018	-0.011	0.023	-0.019	0.008	-0.044	0.029	0.005	-0.004	0.013
	(0.022)	(0.027)	(0.031)	(0.025)	(0.042)	(0.031)	(0.022)	(0.027)	(0.031)	(0.026)	(0.038)	(0.032)	(0.026)	(0.041)	(0.036)	(0.02)	(0.038)	(0.028)
-2	0.011	0.013	0.011	-0.015	-0.035	0.005	0.018	0.026	0.015	-0.007	-0.019	0.008	-0.015	-0.022	-0.016	-0.014	-0.025	-0.007
	(0.017)	(0.031)	(0.024)	(0.022)	(0.034)	(0.029)	(0.017)	(0.03)	(0.023)	(0.021)	(0.031)	(0.026)	(0.025)	(0.043)	(0.031)	(0.019)	(0.041)	(0.027)
-1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
0	0.108	0.126	0.097	0.018	0.013	0.022	0.039	0.016	0.058	0.012	0.004	0.021	-0.014	-0.044	0.004	-0.007	-0.046	0.017
	(0.013)	(0.019)	(0.018)	(0.018)	(0.029)	(0.024)	(0.013)	(0.018)	(0.018)	(0.018)	(0.024)	(0.023)	(0.019)	(0.028)	(0.023)	(0.017)	(0.025)	(0.02)
1	0.271	0.278	0.265	0.062	0.065	0.059	0.05	0.006	0.08	0.033	0.016	0.042	-0.049	-0.08	-0.029	-0.019	-0.066	0.008
	(0.015)	(0.018)	(0.019)	(0.015)	(0.025)	(0.021)	(0.013)	(0.018)	(0.018)	(0.016)	(0.023)	(0.02)	(0.016)	(0.03)	(0.024)	(0.016)	(0.029)	(0.02)
2	0.364	0.367	0.355	0.07	0.082	0.057	0.059	0.019	0.082	0.02	0.009	0.027	-0.03	-0.031	-0.019	-0.011	-0.028	0.006
	(0.016)	(0.02)	(0.016)	(0.017)	(0.031)	(0.024)	(0.016)	(0.021)	(0.015)	(0.018)	(0.027)	(0.023)	(0.018)	(0.031)	(0.025)	(0.016)	(0.03)	(0.022)
3	0.443	0.443	0.44	0.083	0.071	0.081	0.074	0.023	0.104	0.027	-0.009	0.046	-0.027	-0.012	-0.023	-0.009	-0.031	0.01
	(0.017)	(0.02)	(0.018)	(0.018)	(0.028)	(0.023)	(0.016)	(0.02)	(0.018)	(0.018)	(0.025)	(0.023)	(0.016)	(0.032)	(0.023)	(0.016)	(0.028)	(0.02)
4	0.436	0.411	0.447	0.153	0.149	0.147	0.052	-0.005	0.085	0.064	0.015	0.088	-0.047	-0.049	-0.031	-0.004	-0.056	0.031
	(0.017)	(0.026)	(0.026)	(0.022)	(0.035)	(0.026)	(0.016)	(0.02)	(0.024)	(0.022)	(0.028)	(0.027)	(0.023)	(0.041)	(0.029)	(0.021)	(0.037)	(0.029)
5	0.441	0.413	0.442	0.115	0.095	0.106	0.021	-0.038	0.039	0.03	-0.011	0.037	-0.038	0.005	-0.033	-0.014	0.005	-0.016
	(0.023)	(0.034)	(0.03)	(0.029)	(0.05)	(0.034)	(0.022)	(0.033)	(0.029)	(0.027)	(0.044)	(0.036)	(0.026)	(0.052)	(0.037)	(0.023)	(0.042)	(0.031)
θ^{pre}	-0.002	0.005	-0.001	-0.024	-0.032	-0.01	0.006	0.021	0.002	-0.008	-0.003	-0.003	-0.006	-0.03	0.002	-0.006	-0.017	0.001
	(0.016)	(0.025)	(0.023)	(0.02)	(0.031)	(0.026)	(0.016)	(0.024)	(0.023)	(0.02)	(0.029)	(0.024)	(0.022)	(0.035)	(0.028)	(0.016)	(0.033)	(0.024)
θ^{post}	0.333	0.332	0.33	0.077	0.074	0.073	0.052	0.008	0.079	0.029	0.005	0.042	-0.033	-0.038	-0.021	-0.011	-0.039	0.01
	(0.011)	(0.014)	(0.013)	(0.014)	(0.023)	(0.017)	(0.011)	(0.014)	(0.013)	(0.015)	(0.019)	(0.017)	(0.013)	(0.023)	(0.017)	(0.013)	(0.019)	(0.015)
N	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644

Notes: Estimates are based on parents aged 25–64 with family income below 2 times the Federal Poverty Line between 1987 and 1993. Columns (left to right) report the following outcomes: (1) Share of parents with Medicaid-eligible children; (2) Share of parents with children covered by Medicaid; (3) Share of parents eligible for Medicaid; (4) Share of parents enrolled in Medicaid or public health insurance; (5) Share of parents with private health insurance; and (6) Share of parents with any health insurance. Sub columns a, b, c correspond to estimates for for the full sample, the male subsample, and the female subsample, respectively. N represent the number of state-year-group observations. State-year clustered bootstrapped standard errors are reported in parentheses. See text for additional details.

Table 4: Robustness exercise 2 a: Between Income: Treatment group

$\theta_{es}(e)$	Children Medicaid Elig.			Children on Medicaid			Medicaid Elig.			Medicaid			Private			Any		
	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women
	1.a	1.b	1.c	2.a	2.b	2.c	3.a	3.b	3.c	4.a	4.b	4.c	5.a	5.b	5.c	6.a	6.b	6.c
-3	0.001	0.009	0.005	-0.01	-0.017	0.008	0.011	0.03	0.007	0.001	0	0.016	0.021	0.007	0.019	0.016	0.006	0.024
	(0.015)	(0.019)	(0.022)	(0.02)	(0.036)	(0.027)	(0.016)	(0.019)	(0.023)	(0.02)	(0.032)	(0.027)	(0.024)	(0.036)	(0.033)	(0.017)	(0.028)	(0.022)
-2	0.02	0.011	0.023	-0.015	-0.039	0.005	0.027	0.023	0.027	-0.011	-0.032	0.005	0.004	0.02	-0.009	0.002	-0.002	0.004
	(0.012)	(0.02)	(0.02)	(0.019)	(0.028)	(0.024)	(0.012)	(0.02)	(0.02)	(0.017)	(0.021)	(0.022)	(0.022)	(0.032)	(0.026)	(0.016)	(0.028)	(0.024)
-1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
0	0.106	0.129	0.092	0.009	0.013	0.01	0.037	0.018	0.053	-0.001	-0.004	0.001	-0.017	-0.033	-0.001	-0.018	-0.035	-0.001
	(0.008)	(0.014)	(0.014)	(0.012)	(0.02)	(0.019)	(0.009)	(0.013)	(0.014)	(0.013)	(0.017)	(0.018)	(0.014)	(0.022)	(0.018)	(0.011)	(0.021)	(0.015)
1	0.278	0.294	0.265	0.061	0.069	0.055	0.057	0.022	0.08	0.027	0.024	0.028	-0.039	-0.061	-0.019	-0.016	-0.035	-0.003
	(0.011)	(0.014)	(0.016)	(0.012)	(0.019)	(0.017)	(0.009)	(0.011)	(0.015)	(0.013)	(0.016)	(0.016)	(0.015)	(0.022)	(0.018)	(0.012)	(0.02)	(0.015)
2	0.325	0.34	0.31	0.078	0.095	0.065	0.033	0.005	0.049	0.023	0.016	0.025	-0.038	-0.056	-0.019	-0.008	-0.03	0.008
	(0.011)	(0.012)	(0.014)	(0.013)	(0.02)	(0.018)	(0.01)	(0.01)	(0.013)	(0.013)	(0.017)	(0.017)	(0.014)	(0.023)	(0.018)	(0.012)	(0.021)	(0.016)
3	0.404	0.412	0.395	0.109	0.113	0.103	0.053	0.014	0.075	0.051	0.027	0.063	-0.056	-0.073	-0.038	-0.011	-0.044	0.011
	(0.011)	(0.014)	(0.016)	(0.014)	(0.02)	(0.017)	(0.011)	(0.013)	(0.014)	(0.013)	(0.016)	(0.016)	(0.015)	(0.023)	(0.017)	(0.012)	(0.019)	(0.016)
4	0.413	0.404	0.412	0.14	0.148	0.13	0.036	-0.008	0.058	0.052	0.016	0.071	-0.062	-0.082	-0.04	-0.015	-0.061	0.015
	(0.013)	(0.021)	(0.019)	(0.014)	(0.027)	(0.021)	(0.012)	(0.014)	(0.02)	(0.014)	(0.021)	(0.021)	(0.014)	(0.03)	(0.024)	(0.015)	(0.026)	(0.021)
5	0.425	0.431	0.413	0.129	0.137	0.117	0.005	-0.018	0.009	0.056	0.014	0.074	-0.047	-0.041	-0.036	-0.003	-0.021	0.009
	(0.017)	(0.023)	(0.028)	(0.019)	(0.032)	(0.03)	(0.016)	(0.022)	(0.026)	(0.018)	(0.026)	(0.03)	(0.019)	(0.038)	(0.035)	(0.019)	(0.033)	(0.027)
θ^{pre}	0.013	0.011	0.016	-0.013	-0.03	0.006	0.021	0.026	0.019	-0.006	-0.02	0.009	0.011	0.015	0.002	0.007	0.001	0.012
	(0.011)	(0.015)	(0.017)	(0.017)	(0.025)	(0.021)	(0.011)	(0.015)	(0.017)	(0.016)	(0.021)	(0.02)	(0.019)	(0.026)	(0.024)	(0.014)	(0.023)	(0.02)
θ^{post}	0.314	0.325	0.303	0.081	0.09	0.075	0.04	0.009	0.059	0.032	0.016	0.04	-0.042	-0.058	-0.024	-0.012	-0.038	0.006
	(0.008)	(0.009)	(0.012)	(0.01)	(0.016)	(0.014)	(0.008)	(0.01)	(0.011)	(0.011)	(0.013)	(0.014)	(0.011)	(0.018)	(0.015)	(0.01)	(0.016)	(0.013)
N	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644

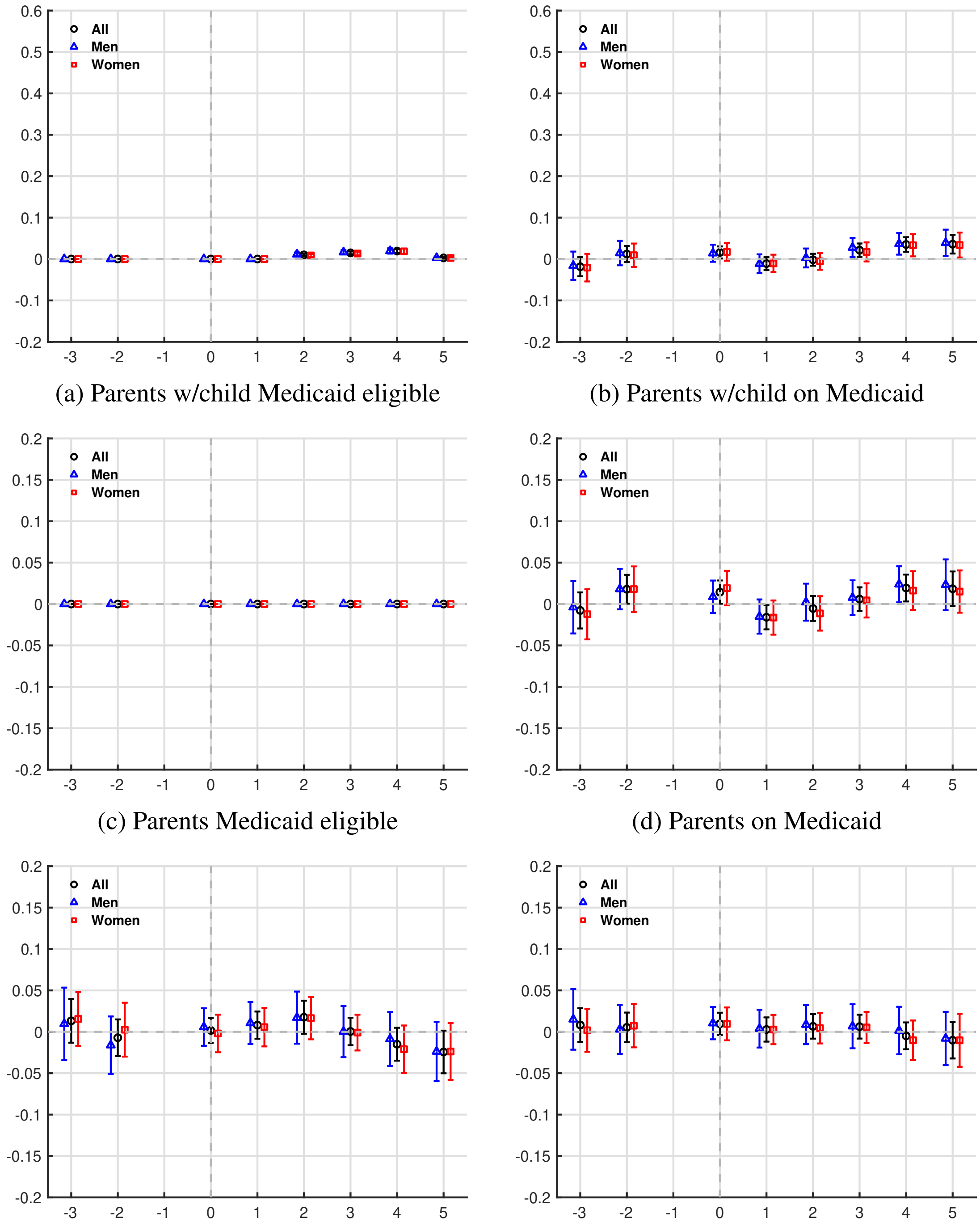
Notes: Estimated for parents aged 25-64 years old with at least a child born during 1983 or later between 1987-1993. High-income refers to parents with family income between 2 and 4 times the Federal Poverty Line. Low-income refers to parents with family income below 2 times the Federal Poverty Line. Columns (left to right) report the following outcomes: (1) Share of parents with Medicaid-eligible children; (2) Share of parents with children covered by Medicaid; (3) Share of parents eligible for Medicaid; (4) Share of parents enrolled in Medicaid or public health insurance; (5) Share of parents with private health insurance; and (6) Share of parents with any health insurance. Sub columns a, b, c correspond to estimates for for the full sample, the male subsample, and the female subsample, respectively. N represent the number of state-year-group observations. State-year clustered bootstrapped standard errors are reported in parentheses. See text for additional details.

Table 5: Robustness exercise 2 b: Between Income: Control group

$\theta_{es}(e)$	Children Medicaid Elig.			Children on Medicaid			Medicaid Elig.			Medicaid			Private			Any		
	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women
	1.a	1.b	1.c	2.a	2.b	2.c	3.a	3.b	3.c	4.a	4.b	4.c	5.a	5.b	5.c	6.a	6.b	6.c
-3	0.02	0.013	0.022	0.006	-0.012	0.019	0.02	0.013	0.022	0.004	-0.027	0.02	0.02	0.056	0.001	0.012	0.02	0.007
	(0.015)	(0.02)	(0.017)	(0.017)	(0.03)	(0.025)	(0.015)	(0.02)	(0.017)	(0.017)	(0.03)	(0.024)	(0.02)	(0.033)	(0.023)	(0.017)	(0.027)	(0.021)
-2	0.01	-0.005	0.015	0.021	0.019	0.018	0.01	-0.005	0.015	0.021	0.019	0.019	0.007	0.02	0.006	0.026	0.036	0.023
	(0.011)	(0.019)	(0.014)	(0.014)	(0.026)	(0.021)	(0.011)	(0.019)	(0.014)	(0.014)	(0.026)	(0.019)	(0.017)	(0.033)	(0.023)	(0.014)	(0.032)	(0.019)
-1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
0	-0.001	0.004	-0.004	0.007	0.012	0.006	-0.001	0.004	-0.004	0.002	0.004	0.002	-0.001	0.021	-0.01	0.004	0.029	-0.007
	(0.008)	(0.013)	(0.011)	(0.011)	(0.017)	(0.014)	(0.008)	(0.013)	(0.011)	(0.011)	(0.017)	(0.014)	(0.013)	(0.023)	(0.018)	(0.012)	(0.018)	(0.015)
1	-0.001	0.008	-0.005	-0.012	-0.008	-0.014	-0.001	0.008	-0.005	-0.015	0.005	-0.025	0.018	0.023	0.018	0.014	0.038	0.004
	(0.008)	(0.013)	(0.01)	(0.011)	(0.019)	(0.016)	(0.008)	(0.013)	(0.01)	(0.01)	(0.017)	(0.016)	(0.012)	(0.024)	(0.019)	(0.01)	(0.022)	(0.016)
2	-0.024	-0.011	-0.031	0.011	0.016	0.011	-0.024	-0.011	-0.031	0.006	0.019	-0.002	0.007	0	0.008	0.018	0.022	0.014
	(0.01)	(0.014)	(0.012)	(0.012)	(0.022)	(0.016)	(0.01)	(0.014)	(0.012)	(0.013)	(0.022)	(0.016)	(0.015)	(0.027)	(0.019)	(0.014)	(0.022)	(0.018)
3	-0.018	-0.008	-0.026	0.047	0.07	0.041	-0.02	-0.01	-0.028	0.03	0.039	0.026	-0.03	-0.057	-0.024	0.009	-0.003	0.01
	(0.01)	(0.014)	(0.014)	(0.014)	(0.022)	(0.018)	(0.01)	(0.014)	(0.014)	(0.013)	(0.021)	(0.018)	(0.013)	(0.027)	(0.018)	(0.013)	(0.022)	(0.015)
4	-0.015	0	-0.019	0.026	0.037	0.024	-0.017	-0.003	-0.022	0.01	0.024	0.005	-0.038	-0.056	-0.034	-0.021	-0.015	-0.026
	(0.011)	(0.017)	(0.016)	(0.016)	(0.03)	(0.021)	(0.011)	(0.017)	(0.016)	(0.017)	(0.025)	(0.02)	(0.02)	(0.035)	(0.021)	(0.017)	(0.033)	(0.021)
5	-0.017	0.008	-0.026	0.038	0.052	0.039	-0.018	0.008	-0.028	0.033	0.022	0.041	-0.028	-0.069	-0.019	0.006	-0.035	0.02
	(0.013)	(0.021)	(0.017)	(0.022)	(0.037)	(0.03)	(0.013)	(0.021)	(0.017)	(0.02)	(0.032)	(0.027)	(0.021)	(0.04)	(0.028)	(0.02)	(0.031)	(0.021)
θ^{pre}	0.014	0.003	0.018	0.014	0.005	0.018	0.014	0.003	0.018	0.014	-0.001	0.019	0.012	0.036	0.004	0.02	0.029	0.016
	(0.011)	(0.016)	(0.013)	(0.013)	(0.024)	(0.019)	(0.011)	(0.016)	(0.013)	(0.013)	(0.024)	(0.018)	(0.016)	(0.028)	(0.02)	(0.012)	(0.025)	(0.017)
θ^{post}	-0.01	0	-0.016	0.014	0.023	0.012	-0.011	-0.001	-0.016	0.006	0.016	0.002	-0.005	-0.008	-0.006	0.007	0.017	0.002
	(0.006)	(0.01)	(0.008)	(0.009)	(0.014)	(0.011)	(0.006)	(0.01)	(0.008)	(0.009)	(0.013)	(0.011)	(0.01)	(0.019)	(0.013)	(0.009)	(0.014)	(0.011)
N	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644	644

Notes: Estimated for parents aged 25-64 years old with at least a child born during 1983 or later between 1987-1993. High-income refers to parents with family income between 2 and 4 times the Federal Poverty Line. Low-income refers to parents with family income below 2 times the Federal Poverty Line. Columns (left to right) report the following outcomes: (1) Share of parents with Medicaid-eligible children; (2) Share of parents with children covered by Medicaid; (3) Share of parents eligible for Medicaid; (4) Share of parents enrolled in Medicaid or public health insurance; (5) Share of parents with private health insurance; and (6) Share of parents with any health insurance. Sub columns a, b, c correspond to estimates for for the full sample, the male subsample, and the female subsample, respectively. N represent the number of state-year-group observations. State-year clustered bootstrapped standard errors are reported in parentheses. See text for additional details.

Figure 4: Robustness Exercise 1 a: High Income



(a) Parents w/child Medicaid eligible

(b) Parents w/child on Medicaid

(c) Parents Medicaid eligible

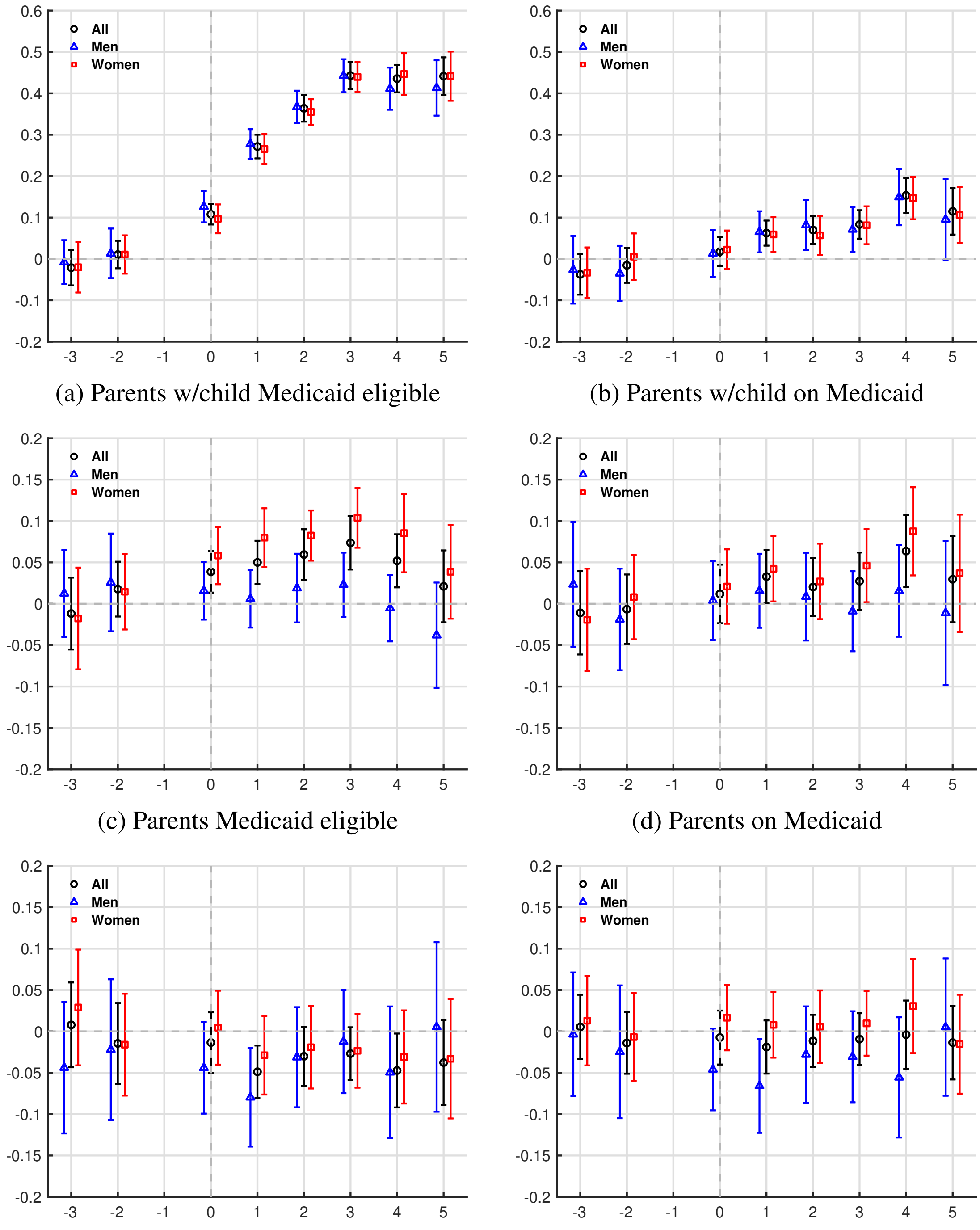
(d) Parents on Medicaid

(e) Parents on private

(f) Parents on any insurance

Notes: Event time estimates refer to $\hat{\theta_{es}}(e)$. Estimates are based on parents aged 25–64 with family income between 2 and 4 times the Federal Poverty Line between 1987 and 1993 . Black, blue, and red lines represent estimates for All, Men, and Women, respectively. Panel (a) shows the share of parents with Medicaid-eligible children. Panel (b) shows the share of parents with children covered by Medicaid. Panel (c) shows the share of parents eligible for Medicaid. Panel (d) shows the share of parents on Medicaid/public health insurance. Panel (e) shows the share of parents with private health insurance. Panel (f) shows the share of parents with any health insurance. Bootstrapped standard errors are used, with 95% confidence intervals reported. See text for additional details.

Figure 5: Robustness Exercise 1b: Low-income



(a) Parents w/child Medicaid eligible

(b) Parents w/child on Medicaid

(c) Parents Medicaid eligible

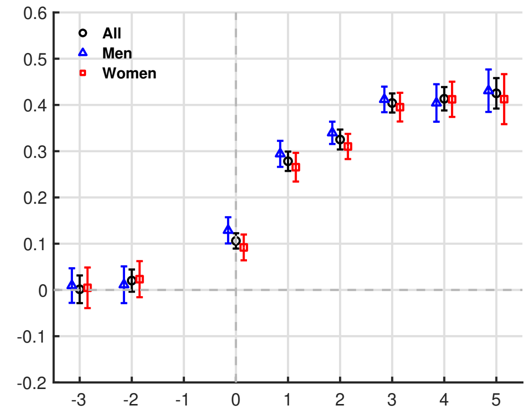
(d) Parents on Medicaid

(e) Parents on private

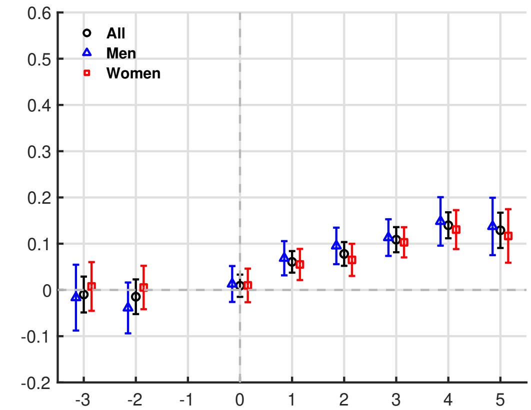
(f) Parents on any insurance

Notes: Event time estimates refer to $\hat{\theta_{es}}(e)$. Estimates are based on parents aged 25–64 with family income below times 2 the Federal Poverty Line between 1987 and 1993. Black, blue, and red lines represent estimates for All, Men, and Women, respectively. Panel (a) shows the share of parents with Medicaid-eligible children. Panel (b) shows the share of parents with children covered by Medicaid. Panel (c) shows the share of parents eligible for Medicaid. Panel (d) shows the share of parents on Medicaid/public health insurance. Panel (e) shows the share of parents with private health insurance. Panel (f) shows the share of parents with any health insurance. Bootstrapped standard errors are used, with 95% confidence intervals reported. See text for additional details.

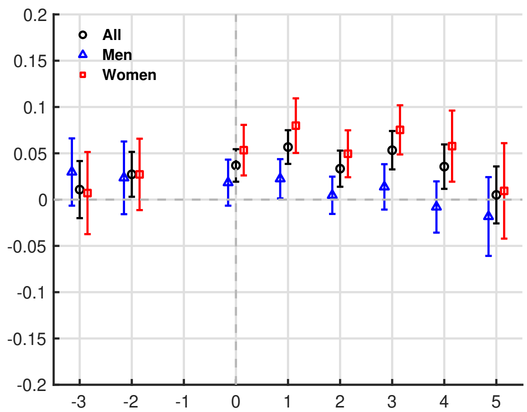
Figure 6: Robustness exercise 2 a: Between income groups with eligible children



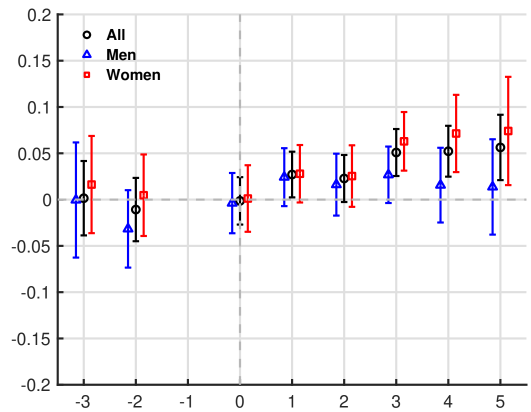
(a) Parents w/child Medicaid eligible



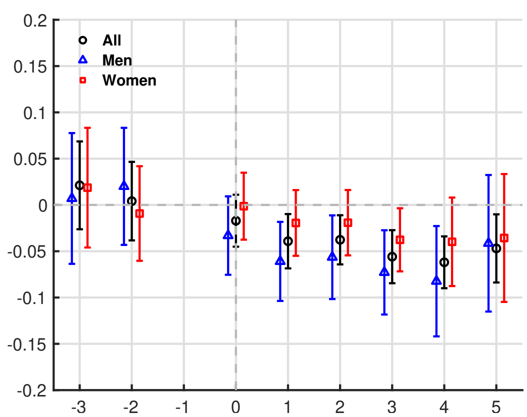
(b) Parents w/child on Medicaid



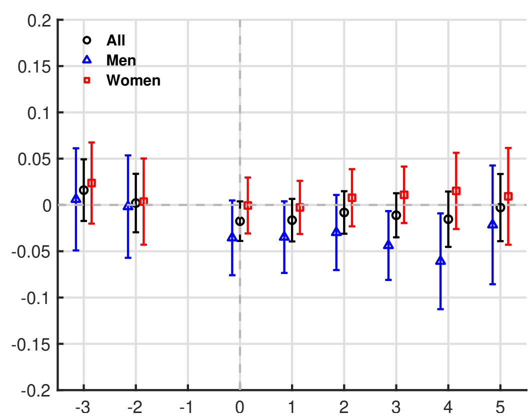
(c) Parents Medicaid eligible



(d) Parents on Medicaid



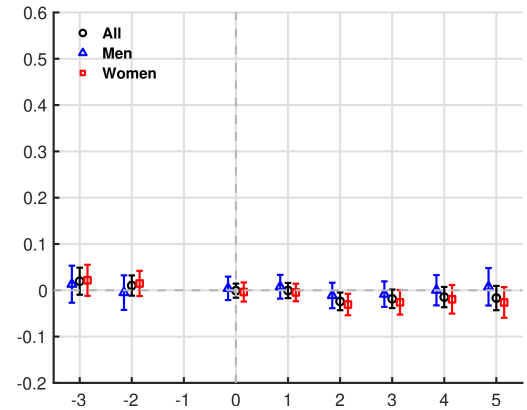
(e) Parents on private



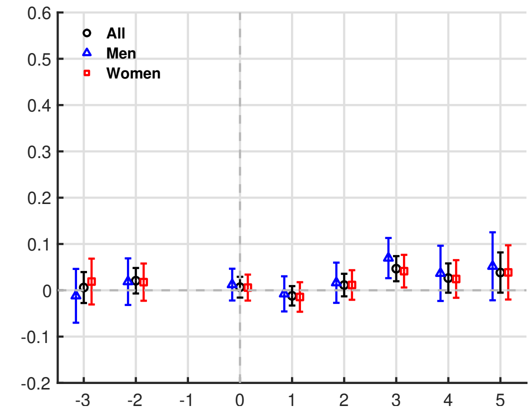
(f) Parents on any insurance

Notes: Event time estimates refer to $\hat{\theta_{es}}(e)$. Estimated for parents aged 25-64 years old with at least a child born during 1983 or later between 1987-1993. High-income refers to parents with family income between 2 and 4 times the Federal Poverty Line. Low-income refers to parents with family income below 2 times the Federal Poverty Line. Black, blue, and red lines represent estimates for All, Men, and Women, respectively. Panel (a) shows the share of parents with Medicaid-eligible children. Panel (b) shows the share of parents with children covered by Medicaid. Panel (c) shows the share of parents eligible for Medicaid. Panel (d) shows the share of parents on Medicaid/public health insurance. Panel (e) shows the share of parents with private health insurance. Panel (f) shows the share of parents with any health insurance. Bootstrapped standard errors are used, with 95% confidence intervals reported. See text for additional details.

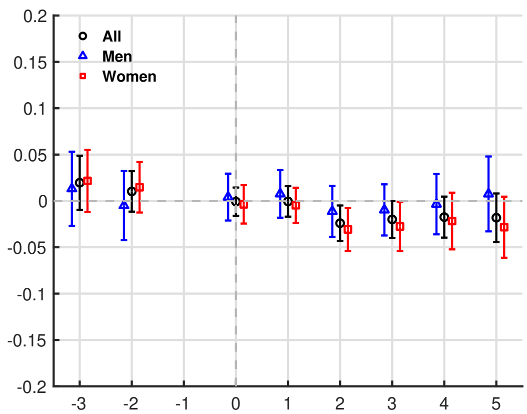
Figure 7: Robustness Exercise 2 b: Between income group without eligible children



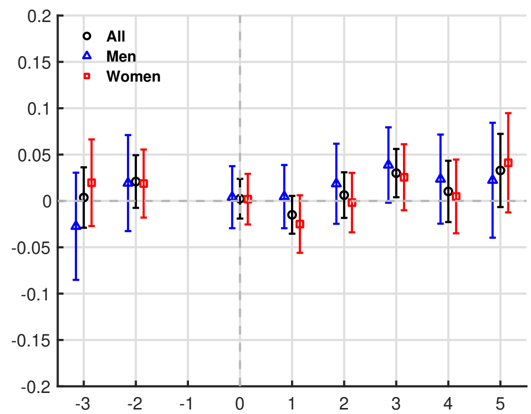
(a) Parents w/child Medicaid eligible



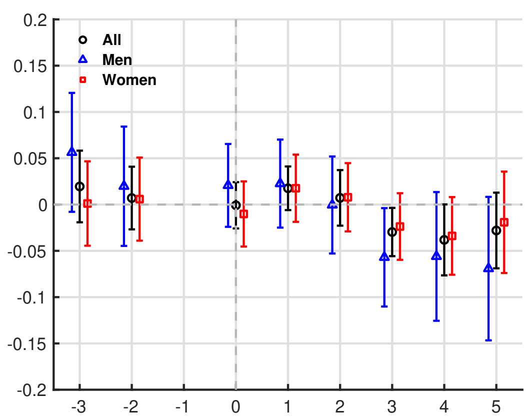
(b) Parents w/child on Medicaid



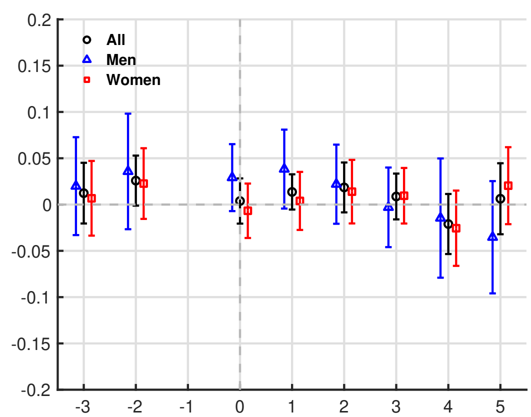
(c) Parents Medicaid eligible



(d) Parents on Medicaid



(e) Parents on private



(f) Parents on any insurance

Notes: Event time estimates refer to $\hat{\theta_{es}(e)}$. Estimated for parents aged 25-64 years old with no child born during 1983 or later between 1987-1993.. High-income refers to parents with family income between 2 and 4 times the Federal Poverty Line. Low-income refers to parents with family income below 2 times the Federal Poverty Line. Black, blue, and red lines represent estimates for All, Men, and Women, respectively. Panel (a) shows the share of parents with Medicaid-eligible children. Panel (b) shows the share of parents with children covered by Medicaid. Panel (c) shows the share of parents eligible for Medicaid. Panel (d) shows the share of parents on Medicaid/public health insurance. Panel (e) shows the share of parents with private health insurance. Panel (f) shows the share of parents with any health insurance. Bootstrapped standard errors are used, with 95% confidence intervals reported. See text for additional details.

B Medicaid expansions and eligibility

Before 1986, Medicaid primarily served the aged, disabled, medically needy, and low-income families with dependent children. However, policy shifts in the late 1980s and early 1990s significantly altered eligibility rules for this last group. In particular, these changes focused on expanding coverage to children from low-income families and pregnant women.

In this section, I describe the pathways of Medicaid eligibility during my study period. The main sources of information are the BLK Medicaid Calculator (Brown et al. (2020)) documentation and source files, supplemented with additional historical data extending back to 1985.

Using these files, I construct monthly income eligibility thresholds by age (0-64) and pregnancy status, covering both children and adults. For non-targeted individuals, whose eligibility typically follows AFDC guidelines, I apply Medicaid eligibility rules for a family of three.

Since eligibility criteria existed prior to Medicaid's expansion, I started my analysis in 1985 when computing eligibility measures. This allows me to asses eligibility dynamics at least two year before changes in eligibility driven by this expansion.

B.1 Medicaid expansion in the late '80s and early '90s

The Omnibus Budget Reconciliation Act (OBRA) of 1986 was the first legislation during my analysis period to significantly change Medicaid eligibility. Before its enactment, Medicaid eligibility was attached to the State Aid to Families with Dependent Children (AFDC) eligibility. Beginning in April 1987, OBRA 1986 allowed states to increase income eligibility thresholds above AFDC levels to a maximum of 100% of the Federal Poverty Line (FPL) for pregnant women, infants, and children up to 5 years old. The expansion for children was implemented gradually through a phased-in process. Under OBRA 1986, the oldest cohort covered included children born on October 1, 1985.

Subsequent legislative changes, including OBRA 1987, the Medicare Catastrophic Care Amendments (MCCA) of 1988, OBRA 1989, and OBRA 1990, gradually expanded public coverage eligibility. OBRA 1987 extended states' optional authority to raise income thresholds for pregnant women and infants up to

185% of the FPL and accelerated OBRA 1986's phase-in timeline by October 1988.[22] Additionally, OBRA 1987 allowed states to increase income thresholds for children up to age eight to 100% of the FPL, with implementation following a phased-in approach.[23]

The MCCA of 1988 mandated a minimum coverage level for pregnant women and infants at 100% of the FPL. This expansion was originally designed for a two-year phase-in, covering those below 75% of the FPL by July 1989 and reaching full implementation by July 1990. However, OBRA 1989 accelerated this timeline and further expanded the minimum federal coverage guarantee. By April 1990, all states were required to cover, at a minimum, pregnant women and children up to six years old below 133% of the FPL. Lastly, OBRA 1990 mandated that states extend coverage to children up to age 19 who were born on or after October 1, 1983, with family incomes below 100% of the FPL, effective July 1991.

Despite these reforms, states retained substantial discretion in designing and implementing their Medicaid programs, except for federally mandated eligibility requirements. For instance, while OBRA 1986 permitted states to extend coverage to children up to age five, only some states adopted this expansion. Similarly, under OBRA 1987, some states accelerated coverage for older children, whereas others delayed expansion until April 1990, when OBRA 1989 mandates took effect. Additionally, because AFDC eligibility thresholds varied widely across states, the impact of Medicaid expansion differed both across states and over time.

B.2 Assigning treatment dates

To isolate significant policy-driven changes in eligibility, I rely on simulated Medicaid eligibility, which maps the complexity of Medicaid eligibility rules into a simplified indicator of eligibility at the state level. Specifically, using state- and time-specific Medicaid income thresholds by age and pregnancy status, I construct the share of parents aged 25–64 with at least one Medicaid-eligible child, as well as the share of parents eligible for Medicaid themselves. To achieve this, I simulate a population of parents and children and determine Medicaid eligibility based on household characteristics and Medicaid income thresholds. The simulated sample is the 1988 ASEC-CPS population of parents and children, which remains fixed

[22]States that exercised this option expedited the phase-in process, making children born on October 1, 1983, the oldest cohort covered.

[23]Children had to be born on or after October 1, 1983, to qualify.

throughout this exercise. This approach enables me to create state-specific time series of aggregate simulated eligibility measures, allowing me to track how changes in income eligibility rules influenced state level eligibility over time. I then assign each state's expansion year based on a simple criterion: When did parents experience a significant increase in child eligibility relative to their own eligibility?

Figure 8 displays simulated eligibility measures by state and year from 1985 to 1993. The black solid line represents the share of parents with at least one eligible child, while the gray solid line represents the share of parents eligible themselves. The left vertical red dotted line indicates the assigned expansion year, and the right vertical red dotted line marks the last year that state is included for estimating ATT. While most states are considered through 1993, some are included only until earlier years. This exclusion occurs when eligibility expanded to adults or when children in the control group became eligible, as these factors compromise the validity of my research design. Table 6 summarizes these restrictions.

As shown, all states significantly expanded children's Medicaid eligibility, while adult eligibility remained largely unchanged. However, states varied not only in the timing of adoption but also in the gradualism of implementation. Some states experienced sharp increases in eligibility during their assigned expansion year, whereas others showed a more gradual and continuous rise throughout the period. Additionally, baseline eligibility levels in 1985 varied across states.

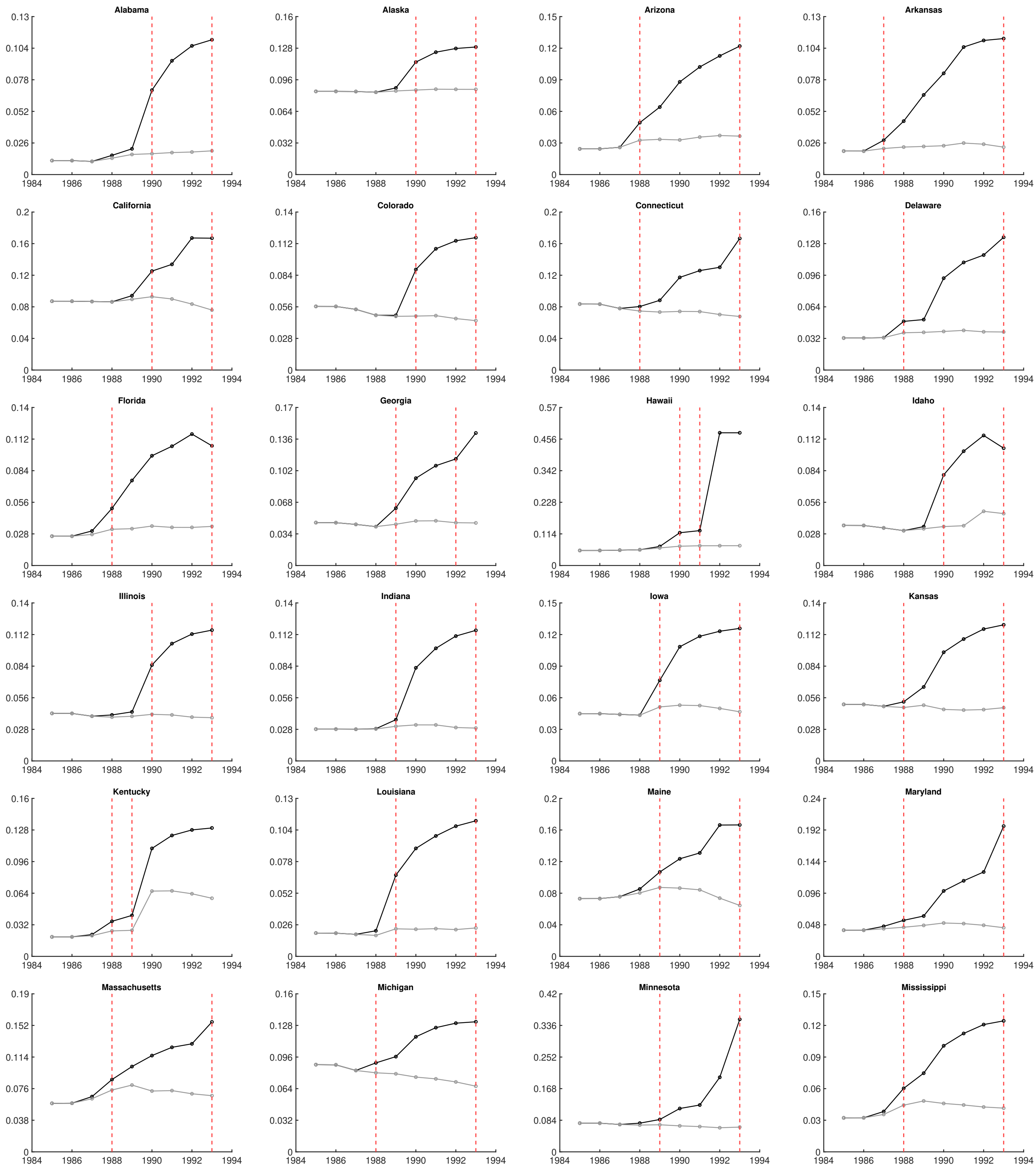
To provide a broader perspective on national trends, Figure 9 presents the aggregate simulated eligibility, calculated as the weighted average of state-level simulated eligibility measures using 1988 CPS weights. In 1985, approximately 5% of parents had a Medicaid-eligible child, and since child and adult eligibility were linked, the same proportion applied to adults. By 1993, policy changes had increased the share of parents with at least one Medicaid-eligible child to 15%, while adult eligibility remained unchanged.

Table 6: Assigned Expansion Year by State

State	**Expansion**	**Last Year**	**State**	**Expansion**	**Last Year**
Alabama	1990		Missouri	1988	
Alaska	1990		Montana	1990	
Arizona	1988		Nebraska	1988	
Arkansas	1987		Nevada	1990	
California	1990		New Hampshire	1990	
Colorado	1990		New Jersey	1988	
Connecticut	1988		New Mexico	1988	
Delaware	1988		New York	1990	1991
D.C.	1990		North Carolina	1988	
Florida	1988		North Dakota	1990	
Georgia	1989	1992	Ohio	1990	
Hawaii	1990	1991	Oklahoma	1988	
Idaho	1990		Oregon	1988	
Illinois	1990		Pennsylvania	1988	
Indiana	1989		Rhode Island	1987	
Iowa	1989		South Carolina	1988	
Kansas	1988		South Dakota	1990	
Kentucky	1988	1989	Tennessee	1988	
Louisiana	1989		Texas	1989	
Maine	1989		Utah	1990	
Maryland	1988		Vermont	1989	1992
Massachusetts	1988		Virginia	1990	1992
Michigan	1988		Washington	1988	1990
Minnesota	1989		West Virginia	1987	
Mississippi	1988		Wisconsin	1990	
			Wyoming	1990	

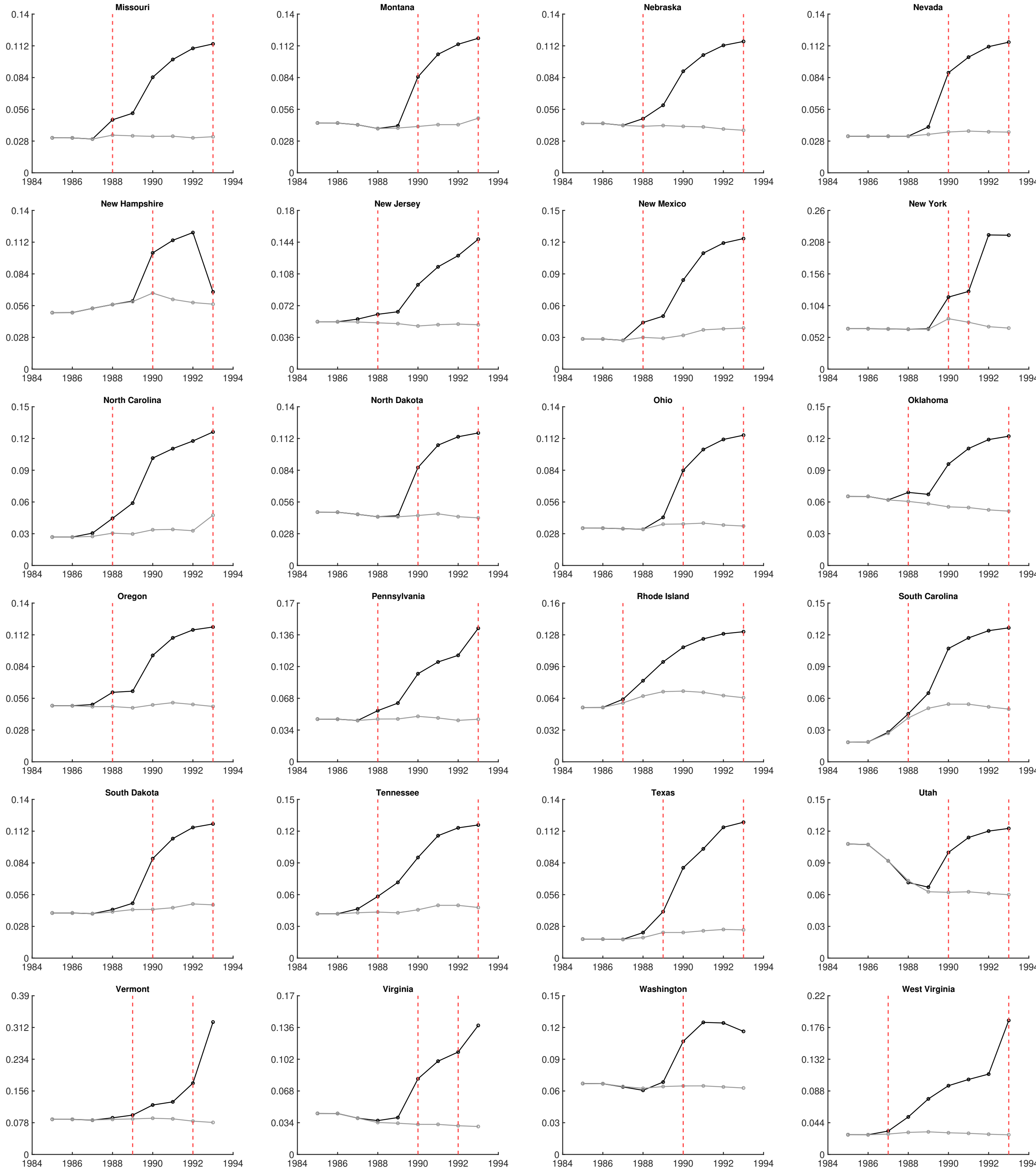
Notes: Expansion: The expansion year is assigned based on a significant increase in Medicaid eligibility for children relative to their parents. Last Year: Some states expanded eligibility for adults or older children beyond the initial expansion. To avoid contamination of control groups and ensure the validity of the parallel trends assumption, the analysis stops in the year before such expansions or policy changes took effect. See Appendix B for further details.

Figure 8: Simulated Eligibility by State: 1985-1993



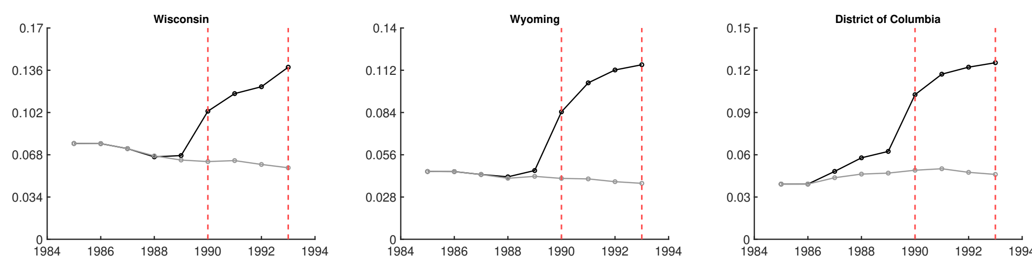
Note: Figures continue from the previous page.

Figure 8 (Cont.): Simulated Eligibility by State: 1985-1993



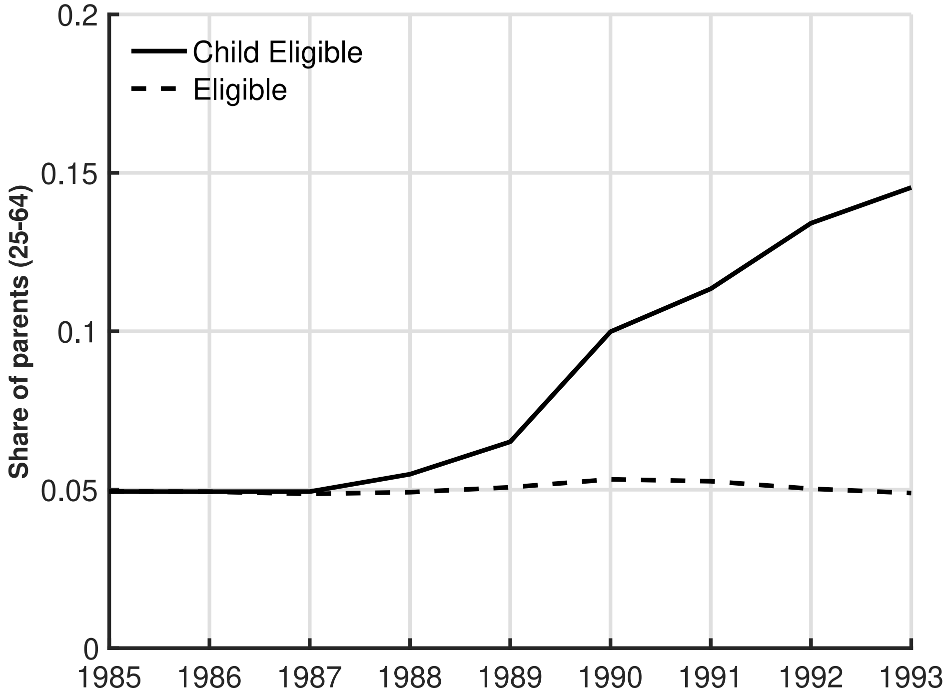
Note: Figures continue from the previous page.

Figure 8 (Cont.): Simulated Eligibility by State: 1985-1993



Notes: Author's calculation from the Current Population Survey and Medicaid Eligibility rules. The red dotted lines show the assigned expansion year. See Appendix B for further details.

Figure 9: Aggregate Simulated Eligibility: 1985-1993



Notes: The solid black line represents the share of parents with at least one child eligible for Medicaid, while the black dotted line shows the share of parents themselves eligible for Medicaid. The simulated sample of parents and children is the 1988 ASEC-CPS. Aggregate values are calculated as a weighted average of state-specific simulated eligibility levels. For further details, see Appendix B.