

From Healthcare to Household Welfare: Extended Postpartum Medicaid Coverage and Household Spending

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Abstract

We study how extending coverage of Medicaid can impact household spending and resource reallocation. The Families First Coronavirus Response Act (FFCRA), temporarily mandated continuous Medicaid enrollment during the COVID-19 public health emergency, which effectively extended coverage for all women enrolled in Medicaid during pregnancy. Using monthly product-level purchase records from the NielsenIQ Consumer Panel (2019–2020), linked to state-specific Medicaid income thresholds, we analyze how spending patterns of households with newborns responded to FFCRA. Difference-in-differences estimates show that extended postpartum coverage roughly doubled spending on baby care products and increased total household expenditures by about 21 percent. The increase in spending occurs after 60 days postpartum, providing evidence that coverage extension acted as an expansion to the budget constraint. Further, we find that the increase in household resources translates to an increase in healthcare utilization using data from the Pregnancy Risk Assessment Monitoring Survey (PRAMS). Taken together, the results show how a major public insurance program can shape household resource allocation beyond healthcare use alone.

Keywords: Medicaid, postpartum coverage, FFCRA, consumer spending, healthcare policy

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I. Introduction

How effective are social safety net programs at allowing households to reallocate resources across competing needs and shielding from financial insecurity? Two strands of economics research provide evidence of the effectiveness of safety net programs along these dimensions. The first focuses on the impact of Medicaid expansion on household finances, while the second focuses on the impact of cash and food assistance programs on resource allocation. In this paper, we combine the two contexts to examine how a large-scale public insurance program, rather than a cash or food transfer, affects household resource reallocation during a time of significant vulnerability.

At its core, Medicaid is meant to protect families from the financial risk that comes from healthcare expenses and reduce out-of-pocket healthcare costs. Consistent with this, Hendren and Sprung-Keyser (2020) conservatively estimate the willingness to pay for Medicaid coverage as \$1,737. This substantial reduction in financial exposure suggests that expanded coverage may free up household resources for reallocation, however, which spending categories are most affected remains unclear a priori.

To explore spending changes, we exploit an abrupt and meaningful change to Medicaid coverage, the Families First Coronavirus Response Act (FFCRA). The FFCRA suspended Medicaid disenrollment from March 2020 to March 2023. This rule effectively extended postpartum Medicaid coverage to those who would otherwise have lost coverage 60 days after delivery. In doing so, it created a large and sudden increase in postpartum coverage, especially in states that had not expanded Medicaid for low income adults under the Affordable Care Act (ACA). Moreover, this impacts a large population given that approximately 42 percent of all births are paid for by Medicaid.

We examine how continuous coverage impacts household spending and resource allocation using detailed monthly purchase data from NielsenIQ Homescan Consumer Panel from 2019-2020 and the Pregnancy Risk Assessment Monitoring Survey (PRAMS) from 2018-2023. We

use a difference-in-differences estimation strategy and identify women newly affected by the FFCRA using household income relative to each state’s pregnancy related Medicaid threshold. We compare households below and above these thresholds before and after the FFCRA began. To account for other COVID-era policies, we also estimate event study models that trace spending around childbirth instead of the FFCRA.¹ This design allows us to study how responses evolve over the postpartum period.

First, we show evidence that expanded coverage through the FFCRA increased the likelihood that newly eligible postpartum mothers retained Medicaid after pregnancy by 26 percent in states that had not expanded Medicaid under the ACA, where the change in coverage was largest. Extending insurance coverage may impact the distribution of household resources between medical care and other purchases by easing the financial burden of medical care. In fact, Hu et al. (2018) estimate that Medicaid expansion decreases unpaid balances in collection by \$1140. However, the impact of Medicaid continuity from the FFCRA on finances may be even higher than previously estimated because it is extending coverage for women postpartum, a period of significant healthcare costs. More than 70 percent of postpartum spending occurs after 90 days and the total spending for those with private insurance over during the first year postpartum is \$3,100 (Bloschichak and Martin, 2020).

Our results show that this financial easement translates into changes in spending and in reallocation of resources. Particularly, extended coverage increased household spending at the grocery store for households with newborns. Relative to households with newborns whose incomes were above the Medicaid eligibility threshold, income-eligible households with newborns increased total spending by about \$105 and baby care spending by about \$32 after the FFCRA, but spending on alcohol or over the counter medications did not change. The effects are concentrated in non-ACA states, where we see the sharpest postpartum coverage cliff. Spending around childbirth increased only after the first 60 days postpartum, which is when coverage would have ended without the FFCRA. These results show that continuous

¹We address additional concerns relating to COVID-19 further in Section V.5.

Medicaid coverage functions not only as health insurance, but also as a source of financial security during a period of elevated need. Households respond to more stable insurance by increasing spending on essential goods for infants, highlighting that the value of public insurance extends beyond healthcare utilization to broader family welfare.

To understand the extent to which households are internalizing the change in budget constraint, we also estimate how extending Medicaid coverage impacts healthcare spending and utilization in the postpartum period. Using PRAMS data from Wisconsin and Illinois, we present evidence showing that after extended coverage women were more likely to attend postpartum checkups, had higher reports of depression, and a lower likelihood of not attending postpartum checkups due to lack of insurance. These results suggest that prolonging postpartum care directly impacts healthcare take-up as households no longer need to fear spending additional money on postpartum visits, testing, reproductive procedures, vaccines, or surgery, much of which occurs after 60 days postpartum (Bloschichak and Martin, 2020).

In answering how an in-kind transfer impacts reallocation within a household, we contribute a few strands of economics research. Firstly, we contribute to a limited literature studying the direct impact of Medicaid expansion on financial outcomes. Studies of the Medicaid expansions through ACA show substantial improvements in coverage continuity, reductions in medical debt, and gains in household financial security (Gross and Notowidigdo, 2011; Frean et al., 2017; Hu et al., 2018). Specifically, Dillender (2017) shows that gaining Medicaid eligibility reduces the likelihood of any medical spending by \$47 per quarter and lowers families' out-of-pocket insurance expenses.² Our research provides new insights by showing that Medicaid not only increases financial security, it also causes people to reallocate their resources and spend more on groceries and purchases related to their baby specifically.

Secondly, this study contributes to a growing body of research on how welfare programs shape household resource allocation and spending. A broad literature shows that in-kind

²Dillender (2017) estimates that an additional family member becoming eligible for Medicaid increases family Medicaid coverage by 0.135–0.142 people and reduces the likelihood of any medical spending in a quarter by 2.7 percentage points.

transfers and income support programs generate sizable changes in household spending patterns. Studies of SNAP, WIC, and other food benefit programs document substantial level shifts and composition changes in grocery purchases following benefit receipt, with evidence that households treat targeted benefits differently from cash (Hoynes and Schanzenbach, 2009; Grummon and Taillie, 2017; Hastings and Shapiro, 2018; Marcus and Yewell, 2022; Bauer et al., 2024).³ Evidence from benefit changes, such as benefit cuts or the expiration of emergency allotments, shows similarly sharp declines in food-at-home spending, often accompanied by reallocation toward other vulnerable household members and essential needs (Kim et al., 2020; Bitler et al., 2023; Martinchek et al., 2025).⁴ Related research on paid family leave finds that income support around childbirth improves food security and raises total household expenditures (Lenhart, 2021; Kim and Lenhart, 2024). We contribute to this literature by studying Medicaid, a much larger public program than SNAP or WIC that operates through insurance rather than cash or targeted in-kind transfers. Medicaid also serves a broader population and is not designed to directly target the specific spending margins we study, so whether it translates into measurable changes in household consumption is an open empirical question. By documenting detailed spending responses to extended postpartum Medicaid coverage, we show how a major public insurance program can shape household resource allocation beyond healthcare use alone.

Lastly, this paper adds to research emphasizing the importance of policy supports during the postpartum period. Prior work shows that extending paid maternity leave, increasing family members' workplace flexibility, and improving postpartum depression screening can meaningfully improve maternal health and postpartum outcomes (Bütikofer et al., 2021; Persson and Rossin-Slater, 2024; Currie and Malinovskaya, 2025). However, direct evidence regarding Medicaid coverage in the postpartum year remains scarce. A very limited set

³Hastings and Shapiro (2018) show that households have a higher marginal propensity to consume SNAP-eligible food out of SNAP benefits than out of cash, reflecting that households plan to use SNAP benefits differently from cash as a separate budget category.

⁴Bitler et al. (2023) find that the loss of WIC benefits at age five reduces maternal caloric intake and increases food insecurity among adult women, suggesting that mothers shield children from the impact of reduced benefits.

of studies examines postpartum Medicaid during the FFCRA period and focuses primarily on self-reported health or access measures (Lyu and Wehby, 2024; Daw et al., 2024). These papers show the FFCRA caused increases in continuous coverage, with mixed evidence on the causal impact on healthcare utilization for those with Medicaid. We add to this developing literature by directly studying the impact of continuous coverage separately in ACA and non-ACA expansion states on similar measures of healthcare utilization and asking how it impacts broader household resources. By studying how sustained insurance continuity affects household consumption, we provide the first evidence on the economic consequences of postpartum coverage continuity, offering timely insight for states implementing twelve-month postpartum Medicaid extensions.

II. Background

II.1. Medicaid Postpartum Coverage Prior to 2020

Medicaid is the single largest payer of maternity care in the United States, covering approximately 42 percent of all births nationwide (Centers for Medicare & Medicaid Services, 2020). Despite its central role in financing childbirth, the program’s postpartum coverage rules remained narrow for decades. Prior to 2020, federal law mandated that pregnancy-related Medicaid eligibility extend only through 60 days after delivery, after which most women were subject to a redetermination based on non-pregnancy income thresholds. For postpartum women, the most relevant non-pregnancy pathway was typically Medicaid eligibility as a parent. Because parent eligibility thresholds were substantially lower than pregnancy eligibility thresholds, many women who qualified for Medicaid during pregnancy no longer qualified after the 60-day postpartum period. The 60-day cutoff therefore created a predictable “coverage cliff,” causing many low-income women to lose insurance precisely when they are recovering from childbirth, managing mental health needs, and caring for a newborn. The Congressional Budget Office estimates that about 45 percent of women become

uninsured at this point (Congressional Budget Office, 2021), even though nearly 70 percent of total postpartum healthcare spending occurs after 90 days (Brown et al., 2020).

The Affordable Care Act (ACA) significantly changed Medicaid eligibility starting in 2014.⁵ In states that adopted the Medicaid expansion, coverage was extended to adults with incomes up to 138% of the federal poverty level (FPL). This allowed many low-income women who would otherwise have lost pregnancy-related Medicaid after 60 days to remain eligible under the expansion category. Although the expansion clearly reduced rates of postpartum uninsurance, important coverage gaps remained. In 2020, the average income eligibility threshold for pregnancy-related Medicaid was around 218% of the FPL. All states set their eligibility above 138% FPL, and about 36 states set it above 200% FPL (Kaiser Family Foundation, 2025b). As a result, a substantial share of women remained at risk of losing coverage after 60 days, either because their income exceeded the adult expansion limit or due to administrative barriers during renewal. Between 2015 and 2017, approximately 25% of women in expansion states experienced a disruption in insurance from delivery to the postpartum period (Daw et al., 2019).

In non-expansion states, the coverage cliff was much larger. Without an adult category, postpartum women generally had to qualify under the parent category after pregnancy-related coverage expired. Because these parent eligibility cutoffs were often extremely low, many women who qualified during pregnancy became ineligible after 60 days.⁶ Thus, women with incomes below the pregnancy cutoff faced a high risk of losing Medicaid coverage postpartum. These patterns led to considerable cross-state heterogeneity in postpartum insurance stability prior to the pandemic, with variation observed both between expansion and non-expansion states and within states by income level.⁷ By the end of 2020, 37 states had expanded Medicaid under the ACA. Thus, states that had yet to expand Medicaid by 2020,

⁵Table A1 reports each state's ACA Medicaid expansion status and effective date.

⁶Before 2020, parent eligibility thresholds ranged from 17% to 100% of the FPL among non-ACA states, with an average of approximately 46% of the FPL.

⁷Prior to 2020, Section 1115 waivers technically allowed states to extend postpartum Medicaid beyond 60 days, but in practice almost no states implemented a full-year postpartum extension before the pandemic

have the largest have the largest “coverage cliff” and have the largest proportion of women that newly retain coverage with the FFCRA.

II.2. FFCRA and Medicaid Coverage Continuity

In March 2020, Congress passed the Families First Coronavirus Response Act (FFCRA). It offered states enhanced federal matching funds (FMAP) on the condition that they maintain continuous enrollment for all Medicaid beneficiaries during the federally declared Public Health Emergency (PHE). This requirement prevented states from disenrolling anyone, resulting in a significant scaling up in Medicaid enrollment. Total Medicaid/CHIP rolls grew by about 23 million people (about 31 %) between February 2020 and March 2023, reaching nearly 95 million enrollees (Kaiser Family Foundation, 2023).

FFCRA had a direct but not targeted effect on postpartum coverage. It did not establish a separate postpartum eligibility category but instead suspended disenrollment for all Medicaid enrollees during the PHE. This provision automatically extended pregnancy-related Medicaid beyond the standard 60-day cutoff. From April 2020 to March 2023, postpartum individuals who qualified for Medicaid during pregnancy remained enrolled regardless of income fluctuations, administrative delays, or routine redetermination. This represented a sharp break from pre-pandemic policy.

The effect was particularly pronounced in non-expansion states, where FFCRA induced a large and unanticipated increase in coverage duration for those eligible during pregnancy. For example, in Wisconsin, all women with pregnancy related Medicaid coverage up to 306 percent of the federal poverty level stayed enrolled throughout the emergency.⁸ Without the FFCRA, women with incomes between 100 and 306 percent of the federal poverty level would typically lose coverage 60 days postpartum. In Wisconsin, this represents an estimated 700 women per month (O’Matz, 2025). Expansion states experienced a smaller though still meaningful change. Women with incomes above the cutoff for adult Medicaid of 138 percent

⁸Notably, Wisconsin has the highest pregnancy income threshold in the US.

of the FPL but below the state’s pregnancy related eligibility limit, such as those under 213 percent of the FPL in Illinois, also extended coverage that they would normally lose. Furthermore, the FFCRA reduced insurance churn by eliminating the need for individuals to transition into an adult eligibility category or to complete renewal processes during the postpartum period.

While FFCRA’s continuous coverage requirement was temporary, it took effect during a period of growing policy interest in postpartum coverage.⁹ Beginning in 2022, many states approved or pursued 12-month postpartum extensions through Section 1115 waivers or the state plan amendment option under the American Rescue Plan Act (ARPA).¹⁰ These extensions were rolled out while FFCRA was still in place, so they did not bind in practice because states could not disenroll postpartum women during the PHE.¹¹ These developments provide useful context but do not affect identification in this study. This also makes FFCRA a clean setting for examining how extended postpartum coverage affects household behavior. Although these policy changes are beyond the scope of this study, they highlight why understanding the effects of postpartum coverage continuity remains important.

III. Data and Sample

III.1. Medicaid Expansion and Eligibility

We collect detailed Medicaid policy information for each state from multiple public sources. We use expansion status and implementation dates from the Kaiser Family Foundation (KFF)(Kaiser Family Foundation, 2025a). We use data on state income eligibility thresholds from KFF’s ”Trends in Medicaid Income Eligibility Limits” database, which provides annual

⁹The FFCRA continuous enrollment provision ended in March 2023, triggering a 12-month “unwinding” period during which states resumed Medicaid redeterminations. About 25 million people lost coverage by 2024 (Kaiser Family Foundation, 2024).

¹⁰Virginia is the only state that extended coverage to 12 months postpartum in Nov 2021 through Section 1115 waiver.

¹¹As of 2024, most state has adopted or plans to adopt a full-year postpartum extension, but these policies became active only after FFCRA ended.

updates on eligibility criteria (Kaiser Family Foundation, 2025b). We also obtain federal poverty guidelines from the U.S. Department of Health and Human Services, Office of the Assistant Secretary for Planning and Evaluation (ASPE, 2025). We then merge these policy variables by state and year to define each state’s expansion status and assign income eligibility cutoffs that match the respondent’s state of residence in both the household and individual datasets.

To study how the FFCRA affected Medicaid coverage among postpartum women, we use the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) from 2019 to 2024. The CPS ASEC is a yearly national survey conducted by the U.S. Census Bureau that collects detailed information on income, health insurance, and demographics. We focus on women with an infant younger than one year at the time of the March interview who reported Medicaid coverage during the prior calendar year. This group includes mothers whose pregnancy-related Medicaid coverage could have expired before the FFCRA, allowing us to study postpartum coverage continuity. The dataset includes state identifiers allowing classification by ACA expansion status, along with information on current Medicaid coverage and demographics. To focus on women more likely to face a postpartum coverage cliff, we exclude those who were likely to remain eligible through the parent category even without the FFCRA. In non-ACA states, we exclude women with family income below the state-year parent eligibility cutoff. In ACA states, we exclude mothers with family income below 100 percent of the FPL, which corresponds to the highest parent eligibility cutoff observed among non-ACA states. This restriction removes very low-income mothers most likely to remain eligible after pregnancy-related Medicaid expired, while preserving ACA states as a lower-exposure subgroup using a consistent definition. The final sample includes 1,423 postpartum mothers, as shown in Table 1.

Medicaid enrollment among mothers with an infant is high overall, about 86 percent. This is expected because the sample is restricted to mothers with Medicaid coverage in the prior calendar year and includes years after the FFCRA continuous coverage provision

took effect. Coverage is higher in ACA expansion states, around 89 percent, than in non-expansion states, around 81 percent, consistent with broader Medicaid eligibility in expansion states. Other demographic characteristics, including age, marital status, race, ethnicity, employment, and education, are broadly similar across states. These patterns suggest that coverage differences mainly reflect policy variation rather than large differences in observable population composition.

III.2. Household Spending

Additionally, we estimate the impacts of FFCRA on household spending using the NielsenIQ Consumer Panel, which records detailed product purchases by households at grocery, drug, and mass-merchandise stores. We use purchase data from January 2019 to December 2020, matched with household demographic surveys that report income range, race or ethnicity, education, family composition, and state of residence. The panel structure makes it possible to track the same households before and after the FFCRA began.

We construct expenditure measures at the household-month level. Total spending includes all recorded purchases. Baby-care spending includes diapers, wipes, infant formula, baby food, and other baby products identified by product codes. We remove households with low scanning activity, missing demographics, or missing state codes, and we drop extreme spending values to reduce the influence of outliers.¹² We winsorize expenditures at the 1st and 99th percentiles to adjust for skewness.

Because the Consumer Panel does not record actual Medicaid enrollment or coverage information, we construct Medicaid eligibility based on reported household income and assign treatment status accordingly. Income is reported in ranges rather than exact amounts, so we classify households based on whether their income range falls clearly below or above the relevant eligibility thresholds. As in the CPS analysis, we first exclude households that were likely to remain eligible through non-pregnancy Medicaid pathways even without the

¹²Extreme expenditure values are identified using the interquartile range criterion, where observations beyond 2 times of the upper-bound are considered outliers and excluded prior to winsorization.

FFCRA. This restriction removes households below the state-year parent eligibility cutoff in non-ACA states and households below 100 percent of the FPL in ACA states.

Among the remaining households, we define treated households as those whose income range falls entirely below the state’s pregnancy-related Medicaid eligibility threshold. These households were likely eligible for pregnancy-related Medicaid and would have faced a higher risk of losing coverage after the traditional 60-day postpartum period. Households with income ranges entirely above the pregnancy threshold serve as the control group. Those with overlapping income ranges are excluded to ensure clear classification. We also exclude households that report different income ranges across survey waves to avoid changes in treatment status.

A key challenge is identifying the timing of childbirth. The panel lists the birth year of each member but not the birth month. We first identify households with a newborn in 2020 using demographic information, then infer the birth month based on the pattern of baby-product purchases. For each household, we calculate average baby-care spending during the previous three months and identify the month when the spending ratio rises most sharply.¹³ That month is defined as the estimated birth month. This approach captures the sudden and sustained increase in baby-product spending that typically occurs after childbirth. Appendix Figure A1 shows that baby-care spending peaks around the estimated birth month and remains elevated thereafter.

The final sample includes households with estimated births no earlier than February 2020, ensuring that mothers were pregnant or within 60 days postpartum when the FFCRA began in April 2020. We merge state identifiers with Medicaid expansion and eligibility data. The resulting dataset is an unbalanced household-month panel with 3,276 observations, as shown in Table 3. About 17 percent of households fall below the pregnancy-related Medicaid threshold, and around 36 percent live in non-expansion states. Average monthly spending is about \$499. Total, baby care, and grocery spending is somewhat higher among treated

¹³We add 0.1 to the denominator when the average spending over the preceding three months is zero.

households with lower income, likely reflecting larger family size, as these households tend to have more children. Other demographics are similar across treatment and policy groups.

III.3. Healthcare Utilization

To study how extended Medicaid coverage affects postpartum healthcare utilization, we use data from the Pregnancy Risk Assessment Monitoring System (PRAMS). PRAMS is a survey conducted by the Centers for Disease Control and Prevention (CDC) and state health departments. It collects data from women with recent live births on prenatal and postpartum care, health behaviors, and infant outcomes.

We have access to data from Illinois and Wisconsin. These two states provide a useful comparison, because Illinois expanded Medicaid under the ACA while Wisconsin did not. The analytic sample includes women who gave birth between 2018 and 2023 for Wisconsin and 2018 to 2022 for Illinois, covering both the pre- and post-FFCRA periods. We study outcomes that indicate postpartum care such like indicators for postpartum checkups, not having a checkup due a lack of insurance, and self reported depression. We also use PRAMS data to validate the results from the CPS on Medicaid retention by estimating the impact of FFCRA on the likelihood a mother is currently on Medicaid and the likelihood that she was on Medicaid during birth and in the postpartum period.

We present summary statistics for this data in Table 2. Comparing outcomes prior to the FFCRA for postpartum women below and above pregnancy eligibility cutoff, we show that people with reported income below the pregnancy eligibility cutoff are more likely to be on Medicaid, have no insurance, and have retained medicaid after pregnancy. We also show that low income postpartum women are more likely to have reported postpartum depression and not have a postpartum checkup.

IV. Empirical Strategy

IV.1. Postpartum Medicaid Retention

In this paper, we study whether the FFCRA increased Medicaid enrollment among postpartum women who had pregnancy-related coverage. Our goal is to provide descriptive evidence on whether the policy effectively extended Medicaid coverage after childbirth. We use CPS ASEC survey years 2019–2023 and exclude 2024 because the FFCRA continuous coverage provision had ended by then and states had begun Medicaid unwinding. We estimate this specification for the full sample and separately for ACA and non-ACA states:

$$\text{Medicaid}_{isy} = \alpha + \beta \text{Post}_y + X_i' \gamma + \mu_s + \varepsilon_{isy} \quad (1)$$

where Medicaid_{isy} equals 1 if mother i in state s and survey year y is enrolled in Medicaid at the time of the survey. Post_y is an indicator for survey years 2021–2023, representing the period when the FFCRA continuous-coverage requirement affected Medicaid retention in the CPS ASEC data.¹⁴ X_i denotes a set of individual characteristics, including age, marital status, race, ethnicity, education, and employment status. State fixed effects μ_s control for time-invariant differences across states. ε_{isy} is the error term. Standard errors are clustered at the state level, and CPS person weights are applied.¹⁵

IV.2. Policy Effects on Spending and Healthcare Utilization

We next examine whether postpartum Medicaid extensions translated into changes in household spending behavior. We use a difference-in-differences design that exploits variation be-

¹⁴Since the CPS ASEC is conducted in March and asks about Medicaid coverage in the prior calendar year, the effect of the FFCRA is most likely to appear beginning in the 2021 survey year rather than in 2020. We therefore define the post period as survey years 2021–2023.

¹⁵This specification should be interpreted as descriptive evidence rather than a fully causal estimate. One limitation is that the CPS ASEC does not report the exact month of childbirth. Some mothers may still be within the standard 60-day postpartum coverage period at the March interview and would remain covered even without the FFCRA. As a result, the measured baseline retention rate is likely overstated.

fore and after the FFCRA across households with different exposure to the postpartum Medicaid coverage cliff. The household spending outcomes are monthly measures of household purchases, including total, baby-care, and health-related spending. Our main specification is:

$$Y_{hsym} = \alpha + \delta(\text{Post}_{ym} \times \text{Treated}_h) + X'_h\theta + \mu_h + \tau_{sym} + \epsilon_{hsym} \quad (2)$$

Y_{hsym} is the monthly spending of the household h in state s , year y and month m , measured in dollars or logarithmic form. Post_{ym} equals one for months after April 2020. δ measures the relative change in household spending for treated households after the FFCRA began. X_h is a set of household demographics listed in Table 3. We include household fixed effects μ_h and state-by-year-month fixed effects τ_{sym} , which absorb shocks common to all households in a given state and month. Standard errors are clustered at the state level.

Treated_h equals one for households whose income falls below the state specific pregnancy related Medicaid eligibility cutoff. These households are the treated group because the mother would have been eligible for pregnancy related Medicaid during childbirth and, under the FFCRA, would be more likely to retain Medicaid coverage beyond the traditional 60 day postpartum period. Households with income above the cutoff serve as the control group. As described in Section III.2., the analysis sample excludes households likely to remain eligible through non-pregnancy Medicaid pathways. This definition focuses on households for whom the FFCRA most directly changed postpartum Medicaid access. Since treatment is assigned based on eligibility rather than observed enrollment, our analysis estimates the intent-to-treat (ITT) effect of the policy.

The parallel trend assumption requires that, in the absence of the FFCRA, households below the state specific pregnancy related Medicaid eligibility cutoff and households above the cutoff would have experienced similar spending trends over time. To test this and show dynamic patterns, we estimate an event-study specification grouping months into four-month bins:

$$Y_{hsym} = \alpha + \sum_{k \neq -1} \delta_k \mathbf{1}\{Q - Q_b = k\} \times \text{Treated}_h + X'_h \theta + \mu_h + \tau_{sym} + \epsilon_{hsym} \quad (3)$$

, where $\mathbf{1}\{Q - Q_b = k\}$ is an indicator for four-month bin k relative to the FFCRA implementation date. Each δ_k measures the difference in spending for households with incomes below their state’s Medicaid pregnancy eligibility relative to control households with income above the Medicaid pregnancy cutoff in bin k , compared to baseline bin ($k = -1$). Other terms are defined as in equation (2).

We further examine whether extended postpartum Medicaid coverage affected health care utilization. This analysis helps assess whether households not only retained coverage but also used health care differently during the postpartum period. The outcomes are indicators for postpartum checkups, not having a checkup due to lack of insurance, and self-reported depression. Using the similar design, we estimate:

$$Y_{hy} = \alpha + \delta(\text{Post}_y \times \text{Treated}_h) + X'_h \theta + \mu_e + \tau_y + \epsilon_{hy} \quad (4)$$

X_h is a set of household controls for mother’s age, education, and race. We also include controls for interaction between stimulus check timing and eligibility. We include eligibility fixed effects μ_e and year fixed effects τ_y . Post_y equals one for months after April 2020. Treated_h equals one for households whose income falls below the state specific pregnancy related Medicaid eligibility cutoff.¹⁶ Standard errors are clustered at the eligibility level, and we use PRAMS survey weights.

IV.3. Spending Patterns Around Childbirth

The main specification measures the time relative to the implementation of FFCRA. As a complementary approach, we also study spending patterns around childbirth. This life-

¹⁶Income in PRAMS is reported in bins, so a household is defined as treated if the household income is in a bin entirely below the pregnancy eligibility cutoff.

cycle design shifts the time dimension from policy timing to months relative to delivery. It serves three purposes: first, to illustrate how the FFCRA affected household spending over the life-cycle around childbirth; second, to explore potential mechanisms; and third, to account for concurrent COVID-era policies that may confound our baseline estimates. FFCRA could affect behavior through both realized and expected extensions of postpartum coverage. By tracking spending before and after delivery, this design helps distinguish these channels and identify when responses occurred. It also directly addresses concerns that the timing of FFCRA coincided with the rollout of other COVID-19 policies that directly impact low-income households.

We estimate difference-in-differences and event-study models corresponding to those in Section IV.2:

$$Y_{hsym} = \alpha + \delta(\text{PostBirth}_{ym} \times \text{Treated}_h) + X'_h\theta + \mu_h + \tau_{sym} + \rho_r + \epsilon_{hsym} \quad (5)$$

$$Y_{hsym} = \alpha + \sum_{r \neq -1} \delta_r \mathbf{1}\{R - R_b = r\} \times \text{Treated}_h + X'_h\theta + \mu_h + \tau_{sym} + \rho_r + \epsilon_{hsym} \quad (6)$$

In equation (6), PostBirth_{ym} equals one for months after birth year-month while Treated_h remains the same and marks households with income below their state’s pregnancy-related Medicaid cutoff. We include relative-to-delivery month fixed effects ρ_r to control for common life-cycle spending patterns across households. $\mathbf{1}\{R - R_b = r\}$ in equation (7) is an indicator for the period relative to delivery ($r = 0$).¹⁷ All other components follow Section IV.2.

IV.4. Identification

Our identification strategy relies on the assumption that, absent the FFCRA, treated and control households would have followed similar spending trends. The main concern is that the FFCRA began at the onset of the COVID-19 pandemic, when pandemic-era shocks and

¹⁷Months are grouped into seven periods: less than -11 (pre-pregnancy), -10 to -7 (early prenatal), -6 to -4 (mid prenatal), -3 to -1 (late prenatal, baseline), 0 to 2 (early postpartum, 60 days), 3 to 6 (mid postpartum), and 7 to 10 (late postpartum).

other federal relief programs may have affected low-income households differently. In that case, our estimates could partly capture pandemic-related changes or other contemporaneous policies rather than the effect of extended postpartum Medicaid coverage.

We address this concern through both the research design and a set of robustness checks. To account for difference in state policies, in the main spending specification, household fixed effects control for fixed household differences, while state-by-year-month fixed effects absorb state-specific monthly shocks. Thus, identification comes from comparing treated and control households within the same state and month. The event-study results also provide support for similar pre-FFCRA trends and show the timing of the spending response after implementation.

We further use the childbirth timing design as a complementary test. This design defines the post period relative to the estimated month of delivery rather than relative to April 2020, which helps separate spending changes around childbirth from shocks that occurred at the same calendar time for all households.

We also test whether the results are sensitive to alternative outcome transformations, winsorization rules, clustering levels, sample restrictions, and balanced event windows. To address the concern that other COVID-era transfers drive the results, we control for transfer issuance timing, exclude issuance months, and restrict the sample to households likely eligible for those benefits. These checks help assess whether the estimated effects reflect extended postpartum Medicaid coverage rather than differential pandemic shocks, other policies, or specific modeling choices.

V. Main Results

V.1. Postpartum Medicaid Retention

We first present evidence that the FFCRA extended postpartum Medicaid coverage for mothers enrolled in Medicaid during pregnancy. Before turning to regression estimates,

Figure 1 illustrates the raw trends in postpartum Medicaid retention rates for ACA and non-ACA states.¹⁸ The figure shows that non-ACA states experienced a sharp increase after 2020, corresponding to the start of the FFCRA, followed by a gradual decline in later waves as the unwinding phase began. Meanwhile, ACA states had a consistently higher retention rate prior to the FFCRA and experienced a mild increase during the policy period.

Table 4 reports descriptive evidence on postpartum Medicaid retention during the FFCRA period, based on equation (1). Column (1) uses the full sample, while columns (2) and (3) estimate the same specification separately for ACA expansion states and non-ACA states. The results show that postpartum Medicaid retention is higher during the FFCRA period. In column (1), Medicaid retention is 13.1 percentage points higher on average, or about 16.6 percent relative to the pre-FFCRA mean. Estimating the regressions separately by state expansion status shows a larger increase in non-ACA states. In column (2), Medicaid retention is 8.1 percentage points, or 9.5 percent, higher in ACA expansion states, while column (3) shows an 18.9 percentage points, or 26.0 percent, increase in non-ACA states. These results provide first-stage evidence that the FFCRA substantially increased postpartum Medicaid retention, especially in non-ACA states.

In addition to showing Medicaid retention using the CPS, we can show retention using PRAMS data. In Table 5 columns (1) and (2), we find a 24.7 percent increase in women within one year postpartum on Medicaid and a 25.6 percent increase in Medicaid retention for the pooled sample of mothers in Wisconsin and Illinois. In panels B and C, we show that results are stronger in Wisconsin, a non-ACA state, than in Illinois, a state that expanded Medicaid through the ACA. We present corresponding event studies for the pooled estimates in Figure 3. These results confirm that the FFCRA increased postpartum retention in a meaningful way especially in non-ACA states.

¹⁸These raw trends are intended mainly to illustrate the timing and pattern of changes in postpartum Medicaid retention. As discussed in Section IV.1., the baseline retention rate is likely overstated because some mothers may still be within the standard 60-day postpartum eligibility period at the time of the survey.

V.2. Policy Effects on Household Spending

We next examine whether postpartum Medicaid extensions translated into changes in household spending behavior. Because the FFCRA created the largest change in postpartum coverage in non-ACA states, we begin by showing the dynamic spending responses for this group. This event-study evidence allows us to assess pre-policy trends and examine when spending began to change after the FFCRA took effect.

Figure 4 presents the event study estimates for non-ACA states across five spending categories, with Panels A through E corresponding to total purchases, baby care products, groceries, alcohol, and over-the-counter (OTC) medications, respectively. Across categories, the coefficients in the pre-policy periods are generally small and statistically indistinguishable from zero, which provides support for the parallel trends assumption. After the FFCRA took effect, treated households in non-ACA states show clear increases in total purchases, baby care purchases, and grocery purchases. The post-policy coefficients become positive and larger in magnitude, especially in the later post periods. By contrast, the event study estimates for alcohol and OTC medication purchases remain centered around zero throughout the post-policy period, with no clear evidence of a sustained increase. The patterns in the log specifications closely mirror those in levels.

Table 6 then reports the corresponding difference-in-differences estimates as defined in equation (3), and compares the effects across the full sample, ACA states, and non-ACA states. This comparison confirms that the spending response is concentrated in non-ACA states, where the FFCRA generated the largest extension of postpartum Medicaid coverage. We estimate the model using spending amounts in dollars.¹⁹

Panel A presents the results for total purchases. The estimates show that household spending increased after the FFCRA took effect, mainly among treated households in non-ACA states. In column (3), compared with households not eligible for pregnancy-related

¹⁹Appendix Table A2 reports analogous specifications using log spending outcomes. In non-ACA states, the log point estimates are 0.191 for total purchases and 0.859 for baby-care purchases. The remaining categories show a similar qualitative pattern to the dollar-level results.

Medicaid, monthly spending rose by about \$105.3. In contrast, the effect in ACA states, shown in column (2), is much smaller and statistically insignificant. Panel B presents the results for baby care purchases and shows a similar pattern. In non-ACA states, treated households increase baby-care spending by about \$32.1 per month after the FFCRA. By contrast, the estimates for ACA states are small and imprecise. These results suggest that part of the overall increase in spending came from higher purchases of infant-related goods, which are especially relevant for households with newborns. Notably, our estimates are consistent with prior work on SNAP's effect on household spending. Hastings and Shapiro (2018) find an increase in spending on SNAP-eligible items of \$110.

Panels C through E further explore how households with newborns reallocated spending across specific categories. Panel C shows positive effects for grocery purchases. In non-ACA states, grocery spending rises by about \$91.4. This pattern suggest that newly covered households increased spending on basic household consumption, not only on baby specific items. Panel D shows no meaningful change in alcohol purchases. The estimates are small and generally statistically insignificant. This pattern suggests that the increase in household spending after the FFCRA was not driven by discretionary spending on alcohol. Panel E similarly shows little evidence of a change in OTC medication purchases. The estimates are close to zero and statistically insignificant in both ACA and non-ACA states. Overall, we do not detect a meaningful shift in household spending in this category following the policy.

Overall, these findings indicate that the FFCRA was associated with a significant increase in household spending among affected low-income families in non-ACA states. The spending response is concentrated in total purchases, baby care goods, and groceries, while we find no comparable increase in alcohol or over-the-counter medication purchases. Taken together, the results point to a targeted increase in essential consumption rather than a broad rise in all categories of spending. This pattern is consistent with the view that extended Medicaid coverage eased short term financial constraints and allowed households with newborns to spend more on basic needs.

V.3. Spending Patterns Around Childbirth

The policy-timing estimates capture average changes after the FFCRA began. Because the policy becomes relevant around childbirth, when pregnancy-related Medicaid would otherwise expire, we next use the childbirth timing design to examine when spending responses emerge. Firstly, Figure 2 shows how spending changes around childbirth for treated and control households in both ACA and non-ACA subgroups. Spending increases after childbirth for all groups, but most extremely in treated household living in non-ACA states who are most likely impacted by the postpartum extension from the FFCRA. Table 7 and Figure 5 present the results from equations (5) and (6), which examine how household spending changed before and after childbirth.²⁰ Figure 5 shows the dynamic effects of the FFCRA on household spending around childbirth in non-ACA states. Across the five categories, the pre-childbirth coefficients are generally close to zero, suggesting no differential pre-trends between the treated and control households. Panel A shows that total household spending remains fairly stable before childbirth and then trends upward after delivery, with the largest increase appearing in the later postpartum period.

Panel B shows the clearest response for baby care purchases. Spending remains flat during pregnancy and during the first 60 days after delivery, which is the period typically covered by pregnancy-related Medicaid. The increase begins only after this point, when the FFCRA extended postpartum coverage beyond the traditional limit. Spending continues to rise in following months, indicating that the policy’s coverage extension played a direct role in sustaining higher consumption of baby-related goods.

Panel C shows a similar upward trend for grocery purchases as in total spending. Panels D and E show noisier patterns for alcohol and over-the-counter medication purchases and do not show a clear post-childbirth shift. So, we do not find evidence of changes in spending in these two categories.

²⁰Appendix Table A3 reports analogous specifications using log spending outcomes. The results show a similar qualitative pattern. In non-ACA states, baby-care purchases increase significantly, with a log-point estimate of 1.32.

Panel A of Table 7 shows that household spending rose after childbirth among treated households, with magnitudes similar to those reported in Table 6. However, the estimates for non-ACA states are not statistically significant. One explanation is that most births in the sample occurred after the FFCRA had been announced, so households were already aware of the policy at delivery. As a result, some spending response may have occurred before childbirth, reflecting expectations of continued coverage rather than a sharp change after birth. This anticipation effect likely smoothed spending patterns around delivery and reduced statistical precision, even though the point estimates remain consistent with a positive effect of the FFCRA. This result is particularly relevant for policy contexts outside of the FFCRA, where the expansion of coverage beyond 60 days postpartum will be known to women at the start of pregnancy.

In Panel B, spending on baby-care products in non-ACA states increases significantly by about \$59.1 per month. Baby-care items such as diapers and formula represent needs that rise sharply after delivery, while total spending reflects broader budget adjustments that may adjust more gradually with coverage expectations. The stronger treatment effect in the baby-related category suggests that the realized extension of Medicaid coverage played a more important role in driving purchases of these time-sensitive goods.

One potential concern is that the estimated increase may be driven by how we impute birth month, since the algorithm identifies the delivery month as the period with the largest rise in baby-related purchases. However, this is unlikely to explain our results, as the estimates come from a difference-in-differences model comparing treated and control households. If the pattern were purely mechanical, similar increases would appear in both groups and be absorbed by the model's fixed effects. The absence of such a pattern in ACA expansion states further suggests that the estimated increase reflects actual behavioral changes rather than a measurement artifact.

Panel C shows a similar pattern for grocery purchases. In non-ACA states, grocery spending rises by about \$87.8 after childbirth, and the estimate is marginally statistically

significant in the levels. Similar to total purchases in Panel A, this pattern suggests that changes in grocery were less sensitive to the exact timing of delivery. Panels D and E present results for alcohol and over-the-counter medication purchases. Again, we don't find evidence of a change in spending in these two categories with the estimates being small and statistically insignificant.

These results suggest that the spending response around childbirth was concentrated in categories most directly tied to the needs of households with newborns. The strongest and most consistent increase appears in baby care purchases, with some evidence of higher grocery spending as well. By contrast, we find weaker and less stable evidence for alcohol and over-the-counter medication purchases.

V.4. Heterogeneous Analysis

We show that the FFCRA increases purchases for groceries and baby-related items. However, these aggregate impacts may mask important difference across households which we explore in this section. We estimate difference-in-differences models that allow the treatment effect to vary by three household characteristics: whether the female household head has a college degree, whether she is older than 30, and whether the household is White. We focus on the three spending categories that showed the clearest main effects in the earlier analysis: all purchases, baby care purchases, and grocery purchases. The results are shown in Table 8. Panel A follows the policy timing design in equation (2) and compares household spending before and after the FFCRA. Panel B turns to the childbirth timing design in equation (5) and compares spending in the months before and after childbirth. In both panels, we extend the baseline specification by interacting the DID estimator with each household characteristic.

Overall, the heterogeneity results provide limited evidence that the spending response differed systematically across household characteristics. For all purchases and grocery purchases, the interaction terms are generally imprecise and do not show a consistent pattern

across the two designs. The clearest evidence of heterogeneity appears for baby care purchases, which are the category most directly tied to newborn needs.

The spending response for baby care purchases is larger for households with college educated female heads. In both the policy timing and childbirth timing designs, the interaction with college is positive and statistically significant. One possible interpretation is that more educated households may face lower frictions in maintaining coverage and may be better able to translate the coverage extension into purchases of time sensitive newborn items.

There is also some evidence that the baby care response is smaller for households with female heads above age 30, although this pattern is only significant in the childbirth timing design. Education and age may capture different margins. Education may reflect differences in information, resources, and the ability to respond to the policy, while age may reflect prior parenting experience and existing infant-related supplies. Households with older female heads may therefore have less need to increase baby care purchases after childbirth. The results by race provide no clear evidence of differential responses between white and non-white households. Although the estimates are negative in sign, they are not statistically significant.

V.5. Robustness Checks

Tables 9 and 10 report a broad set of robustness checks for the non-ACA sample under the policy timing and childbirth timing designs, respectively.²¹ We first examine whether the findings depend on how spending is measured or on the treatment of extreme values. Column (1) uses the inverse hyperbolic sine transformation, and column (3) winsorizes spending at the 0.5 and 99.5 percent levels rather than the 1 and 99 percent levels used in the main specifications.

Because the sample includes pre-birth periods, baby-care spending can be zero in some months. Column (2) estimates a Poisson (PPML) specification for baby-care purchases as

²¹Except for column (1) and (2), all outcomes are measured in dollar levels and are therefore most directly comparable to column (3) in Tables 6 and 7.

a robustness check. This provides evidence that the baby-care results are not sensitive to model choice in the presence of many zeros.

We next consider whether the results are sensitive to inference choices or sample definition. Column (3) clusters standard errors at the county level rather than the state level. Column (4) restricts the sample to households with any female aged 18 to 44, which more closely aligns the sample with the population most directly targeted by postpartum Medicaid coverage and reduces concern that the results are driven by households with less typical caregiving arrangements. Column (5) reintroduces households whose income changes over time and may cross the Medicaid eligibility threshold, and further controls for baseline income group specific shocks. This addresses the concern that excluding households with changing treatment status in the main sample may induce selection near the eligibility margin.

The final two columns address concerns related to timing. Column (6) imposes a balanced event window of nine months before and nine months after the policy or childbirth date. Column (7) excludes March 2020 as a simple check of whether the results are sensitive to unusually distorted spending behavior at the onset of the pandemic.

Tables 9 and 10 show that the robustness results closely mirror the main estimates in Tables 6 and 7. Across the alternative specifications, the sign, magnitude, and overall significance pattern remain very similar under both the policy timing and childbirth timing designs. Under the policy timing design, the estimated effects for total, baby care, and grocery purchases remain positive and statistically significant throughout. Under the childbirth timing design, the estimates are likewise stable and continue to line up closely with the baseline results. Baby care purchases remain the clearest margin of response, while the estimates for total purchases and grocery purchases stay similar in both magnitude and significance to the main specification.

A concern in our setting is that other COVID-era transfer programs coincide with the FFCRA implementation and may differentially affect low-income households.²² The first

²²We also considered other transfer programs, such as SNAP and WIC. To the best of our knowledge, however, there were no major policy changes to these programs during our study period that would plausibly

round of Economic Impact Payments (EIPs) began in April 2020, which overlaps with FFCRA initiation. The second round began in December 2020, which falls within our sample window and provides additional transfer-related variation. Since treatment status is defined by income relative to the Medicaid eligibility cutoff, treated households are more likely to be eligible for and responsive to these transfers. If EIP disbursements generate short-run spending spikes, they could load onto post-period estimates and complicate the interpretation of the FFCRA effect.

We address this concern in several ways and report the results in Table A4. Column (1) reports the baseline estimates. Column (2) adds an indicator for the second stimulus check disbursement period interacted with the treatment indicator. This specification allows spending among treated households to shift differently during the months when the second-round payments were issued. Column (3) excludes the months corresponding to the first and second stimulus check issuance windows, so identification comes from variation outside the months most directly affected by stimulus timing. Column (4) restricts the sample to households with income below the eligibility cutoff for EIP, which reduces heterogeneity in stimulus eligibility and payment amounts within the analysis sample such that both the treatment and control group likely receive the stimulus check.²³

Across specifications, the main conclusion is unchanged. In the policy timing design, the estimated effects remain positive for total purchases, baby care, and groceries, and the magnitudes reduces modestly when we control for the second-round EIP period or exclude the EIP months. The income restriction in column (4) yields similar point estimates, with reduced precision as expected given the smaller sample. The childbirth timing design itself eases the concern that additional COVID-era policies are directly correlated with the timing of treatment as the post period is no longer defined by FFCRA, but instead individual

confound our estimates.

²³The NielsenIQ Consumer Panel reports income in bins. The top bin is \$100,000 or above, and the adjacent bin is \$70,000-\$99,999, so the data do not allow an exact cutoff at the EIP full-payment thresholds (\$75,000 for single filers and \$150,000 for married filers). We implement a conservative restriction by excluding the top bin (income \geq \$100,000). As a stricter alternative, we can also exclude households with income \geq \$70,000. This further reduces sample size, but the estimates are similar.

estimated birth timing. Nevertheless, across specifications in Table A4 the childbirth timing design also delivers the same conclusion. The baby care estimates remain positive and statistically significant across all columns. For total purchases and groceries, point estimates remain positive and are broadly similar in magnitude. Overall, the evidence is consistent with our main findings, while suggesting that short-run stimulus-related dynamics do not materially alter the results.

V.6. Further Effects of the FFCRA

We have shown that extending postpartum coverage increased total purchases for baby care items and groceries, suggesting that households change mental accounting and experience an increased budget constraint. To further understand how households internalize Medicaid extension as a benefit we test how they change their utilization of health care. To do this, we use PRAMS data from Illinois and Wisconsin. Table 5 shows difference-in-differences estimates of extended coverage on measures of health and healthcare utilization where the treated group are those with incomes below the pregnancy eligibility cutoff who are most likely impacted by the FFCRA. Panel A reports the pooled estimates for mothers in Wisconsin and Illinois, while Panels B and C report estimates separately for Wisconsin and Illinois, respectively. In the pooled sample, in Columns (3) through (5), we show that extending postpartum insurance has no impact on postpartum checkup likelihood, not having a postpartum checkup due to insurance status, and postpartum depression.

We find that the pooled results mask important heterogeneity. In Panel B which only includes mothers in Wisconsin, a non-ACA expansion state, there is a 3.0 pp or 3.4 percent increase in the likelihood of postpartum checkup. Moreover, there is a 54.5 percent decrease in women reporting not having a postpartum checkup due to not having insurance, implying that continuity of coverage is particularly relevant for the population that would have skipped appointments due to financial constraints. Next, we present impacts on reported postpartum depression. In column (5) we show that the reports of postpartum depression increases by

3.7 pp or by 23.3 percent. The increase in postpartum depression rates could indicate an increase in actual depression rates or result from the increase in postpartum checkups for a particularly vulnerable population that leads to an increase in diagnoses. These mechanisms are difficult to disentangle in PRAMS because the data is self reported; however, the increase in likelihood of postpartum visits provides some evidence pointing toward an increase in diagnosis rather than rates of depression. In panel C, we do not see any evidence of a change in postpartum care or postpartum depression for the sample of Illinois women. Because Illinois is a state that expanded Medicaid through the ACA prior to the FFCRA, there are less women that newly gained access to coverage, which mutes the possible impact on health and utilization.

VI. Conclusion and Discussion

In this paper, we study how the extension of Medicaid coverage affects the reallocation of household resources and spending. We use the temporary nationwide coverage extension created by the FFCRA, which suspended Medicaid disenrollment from March 2020 to March 2023. This rule extended postpartum Medicaid coverage for women who would have lost insurance sixty days after delivery, especially in states that had not expanded adult Medicaid under the ACA.

First, we show that extending postpartum coverage increased Medicaid retention for postpartum women, especially in non-ACA states. Then, we turn to evidence of household resource allocation. Using household purchase data from the NielsenIQ Consumer Panel in 2019 and 2020, we find that the policy increased household spending among affected families with newborns. The effects are concentrated in non-expansion states and are strongest for baby care purchases, with positive effects also for total spending and groceries. When comparing the periods before and after the FFCRA took effect, the coverage extension increased total spending by about 21 percent and roughly doubled baby care spending among households below the postpartum Medicaid eligibility threshold, relative to households above

the threshold. When comparing spending before and after childbirth, we find a similar pattern, with the clearest increase again appearing in baby care purchases.

Our findings suggest that continuous Medicaid coverage helps families maintain spending at a time when household needs rise sharply after childbirth. When coverage remains in place beyond the traditional sixty day cutoff, families appear better able to spend on essential goods. The composition of the spending response is informative. The largest and most stable effects appear in baby care purchases, with additional evidence for groceries, while we find little evidence of systematic increases in more discretionary categories like alcohol. The increase in baby care purchases suggests that families use the added financial security from continued coverage to spend more on essential infant goods. The overall evidence is consistent with a relaxation of the budget constraint during a critical time.

The timing of the spending response further supports this interpretation. In the event study around childbirth, baby care spending does not rise immediately at delivery and instead increases more clearly after the point at which pregnancy related Medicaid coverage would traditionally expire. This pattern is difficult to reconcile with a purely mechanical response to childbirth itself. Instead, it suggests that the realized extension of coverage shaped how families adjusted spending in the postpartum period. Finally, using PRAMS data, we show that households internalized the budget expansion and increase healthcare utilization. In particular, FFCRA caused a 3.4 percent increase in postpartum check-up and decreased the likelihood that a postpartum mother had no checkup because she lacked insurance by 54.5 percent.

The implications of our study have direct relevance for current postpartum Medicaid policy. During the FFCRA period, the federal continuous coverage requirement temporarily provided a version of what many states have since adopted through twelve-month postpartum extensions. Our results suggest that the benefits of extended postpartum coverage are not limited to health care access. By reducing insurance loss during a financially vulnerable period, continuous coverage may also support basic household consumption and improve

material conditions for mothers and infants. For policymakers, this implies that postpartum Medicaid extensions may generate benefits that are not captured by standard utilization or enrollment measures alone. The results also suggest that these gains may be especially important in non-expansion states, where postpartum coverage losses would otherwise be sharper.

Overall, we show that extending postpartum Medicaid coverage changed household spending behavior in meaningful ways. The FFCRA increased spending on essential goods among affected families with newborns, with the clearest effects appearing in baby care purchases and in states where postpartum coverage would otherwise have ended quickly. These results highlight a broader role for postpartum Medicaid than is usually emphasized in policy debates. In addition to improving insurance continuity and access to care, continuous postpartum coverage may help families navigate the economic demands of early parenthood.

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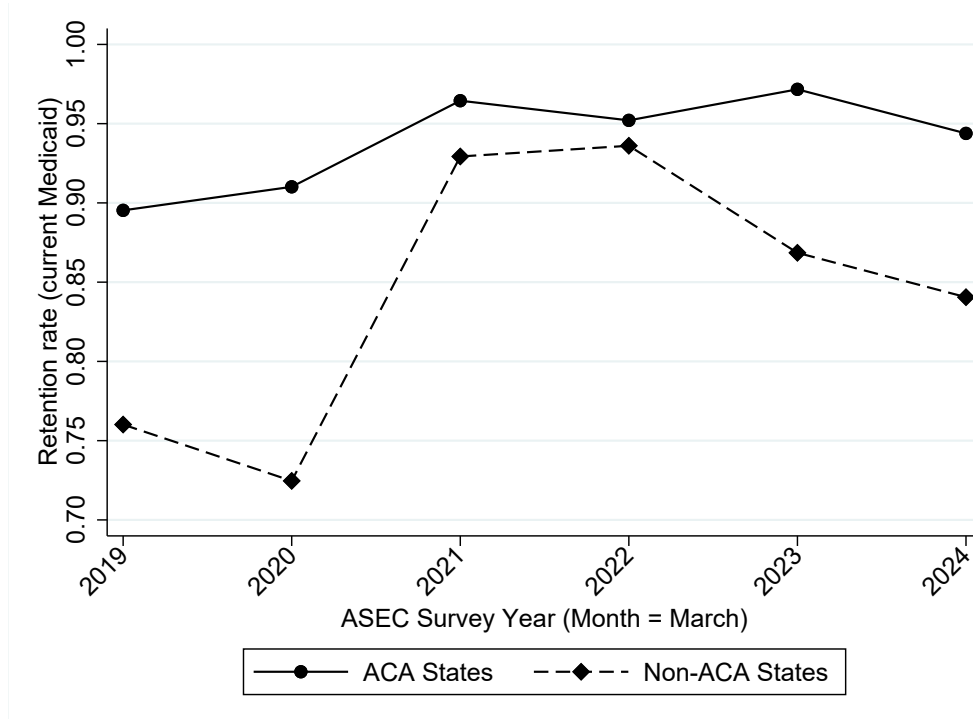
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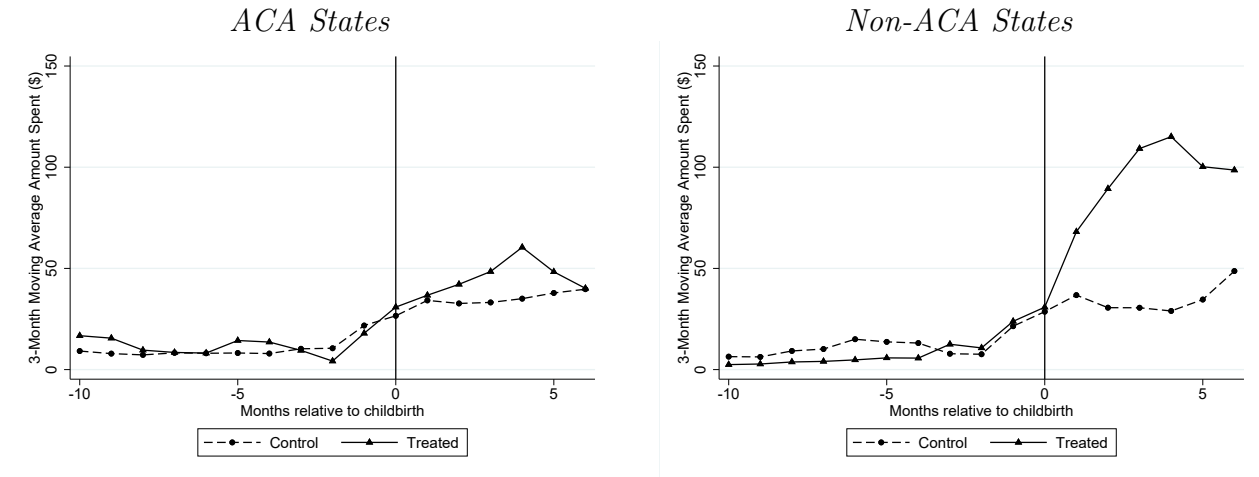
Figures and Tables

Figure 1: Raw Trends in Medicaid Retention by ACA Expansion Status



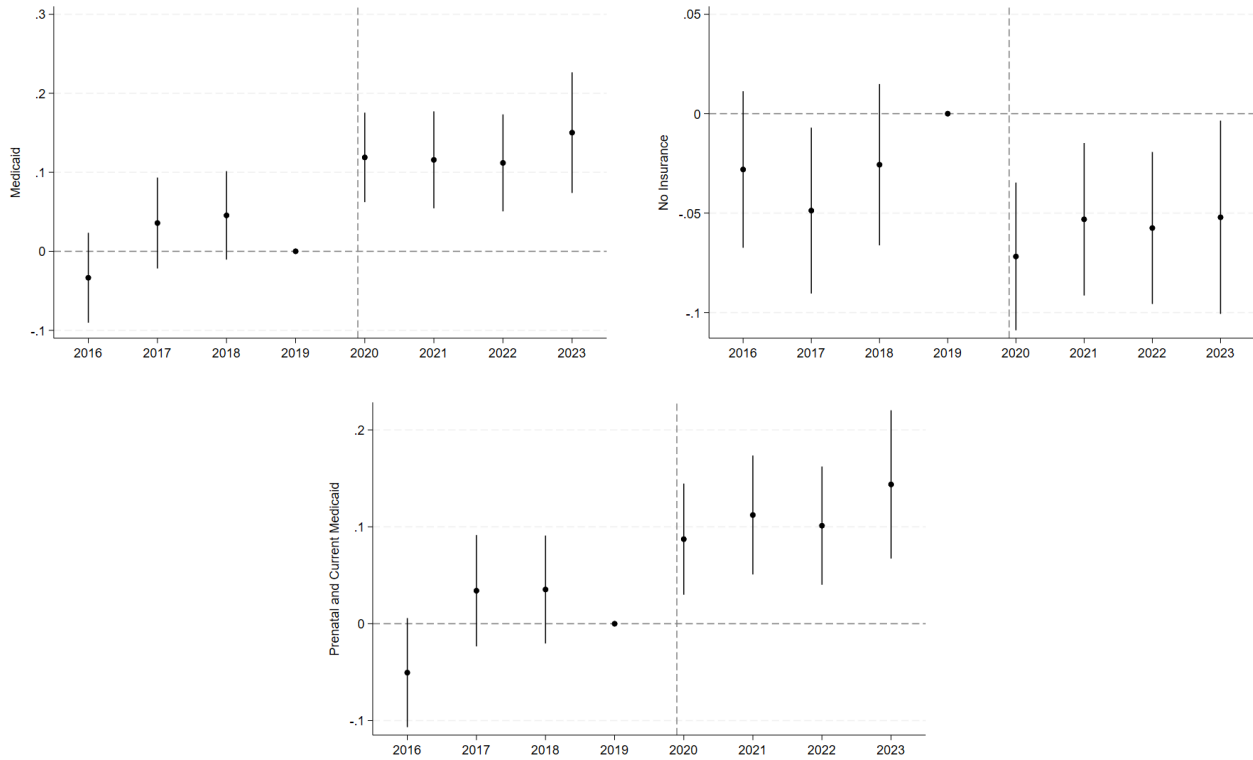
Notes: Data are from Current Population Survey Annual Social and Economic Supplement (CPS ASEC), 2019–2024. The figure plots the share of mothers with infants who remain enrolled in Medicaid at the March ASEC interview among those with Medicaid coverage during the prior calendar year. Estimates are weighted using ASEC person weights and shown separately for ACA and non-ACA expansion states. FFRCA began in 2020 and unwinding began in 2023.

Figure 2: Trends in Baby-Care Spending Around Childbirth by ACA and Treatment Status



Notes: Data are from the NielsenIQ Consumer Panel, 2019-2020. The figure shows the three-month moving average of baby-care spending around childbirth (month 0), separately by ACA expansion status and treatment group.

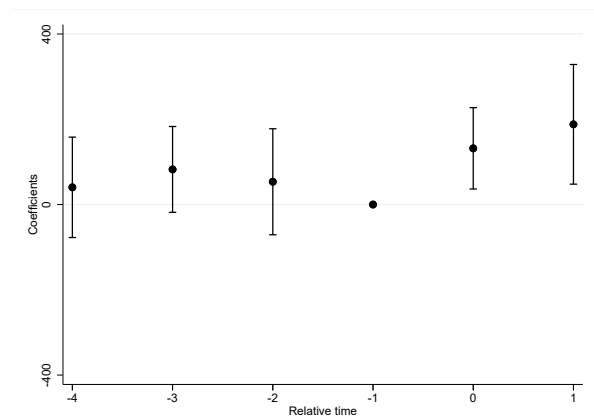
Figure 3: Event Study Estimates of FFCRA Effects on Health Insurance
Policy Effect: Comparing Pre- and Post-FFCRA



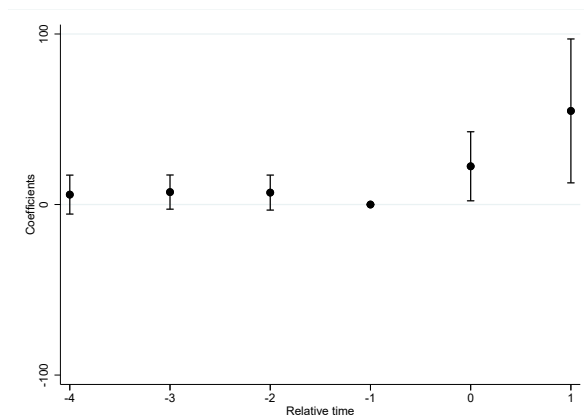
Notes: Data source is Illinois and Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2016-2022 and 2016-2023, respectively. Each figure reports pooled survey-weighted regression estimates for mothers with an infant born in the last year. Treatment is equal to 1 if the entirety of the household income is below the state's pregnancy eligibility cutoff after 2021. Outcomes are indicators for currently using Medicaid, currently no insurance, and used Medicaid during pregnancy and now. All models include maternal demographics controls and standard errors are clustered at the treatment level. Vertical bars indicate 95% confidence intervals.

Figure 4: Event Study Estimates of FFCRA Effects on Spending Among Non-ACA States
Policy Effect: Comparing Pre- and Post-FFCRA

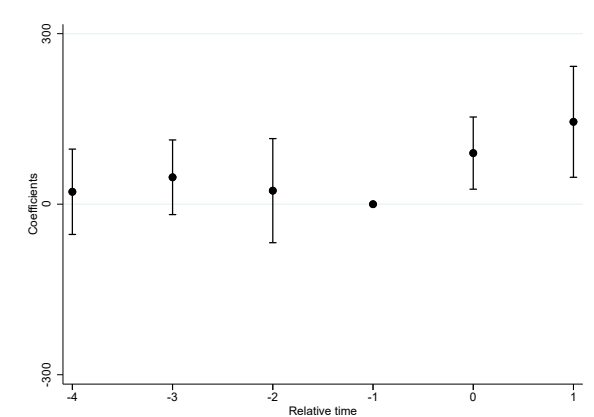
Panel A. All purchases



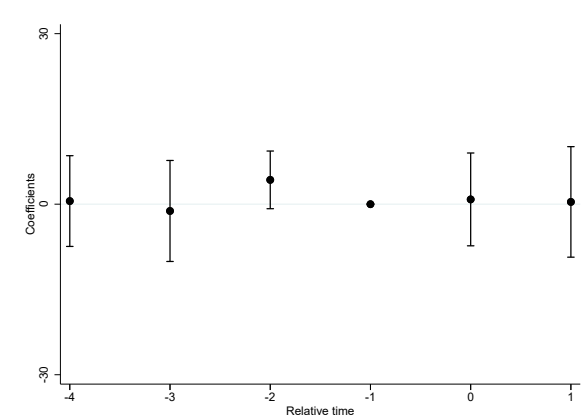
Panel B. Baby-care purchases



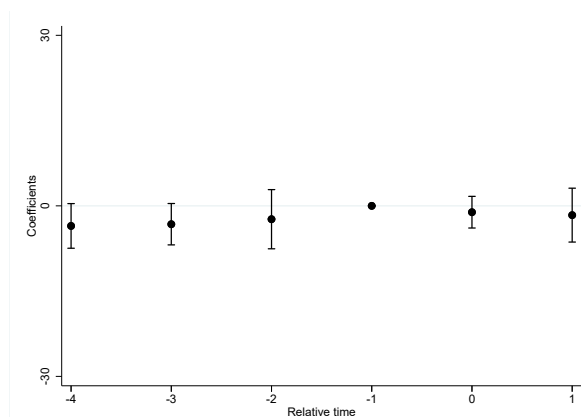
Panel C. Grocery purchases



Panel D. Alcohol purchases



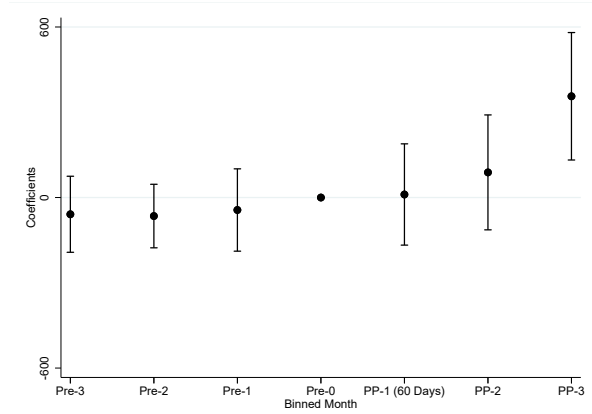
Panel E. OTC medication purchases



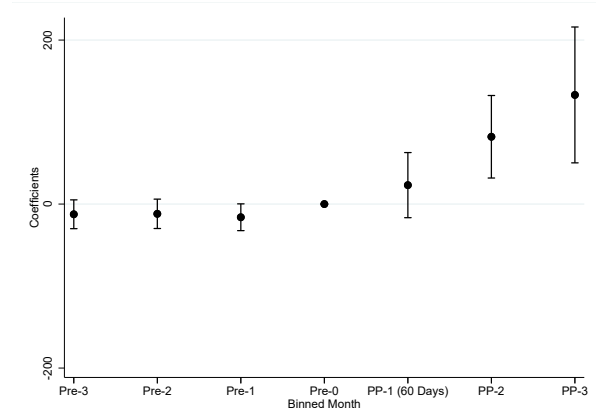
Notes: Data source is NielsenIQ Consumer Panel, 2019-2020. Event time is grouped into four-month intervals relative to FFCRA implementation. Vertical bars indicate 90% confidence intervals. Standard errors are clustered at the state level. All regressions include household and state-by-month fixed effects.

Figure 5: Event Study Estimates of FFCRA Effects on Spending Among Non-ACA States
Life Cycle: Comparing Before and After Childbirth

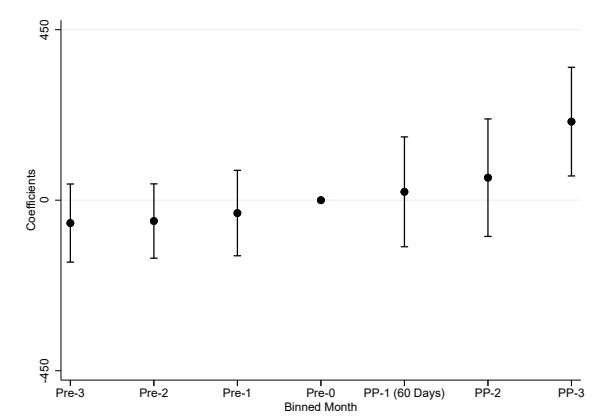
Panel A. All purchases



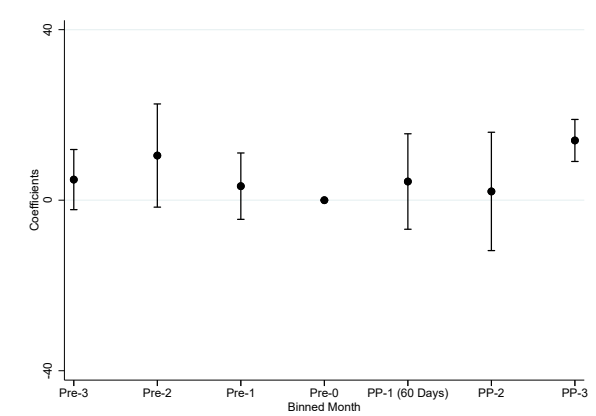
Panel B. Baby-care purchases



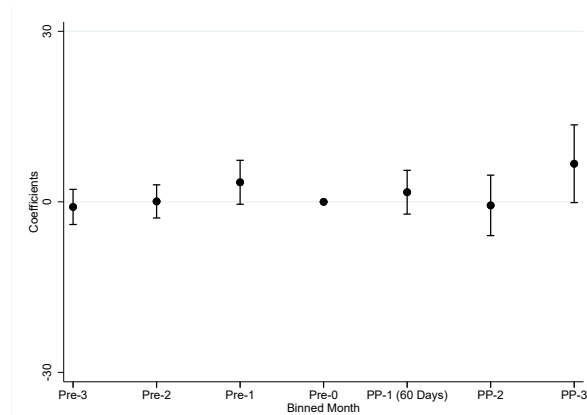
Panel C. Grocery purchases



Panel D. Alcohol purchases



Panel E. OTC medication purchases



Notes: Data source is NielsenIQ Consumer Panel, 2019-2020. Event time is grouped into bins: months -15 to -11 (pre-pregnancy), -10 to -7 (early prenatal), -6 to -4 (mid prenatal), -3 to 0 (late prenatal, omitted baseline), 1 to 2 (early postpartum, 60 days), 3 to 6 (mid postpartum), and 7 to 10 (late postpartum). Vertical bars indicate 90% confidence intervals. Standard errors are clustered at the state level. All regressions include household and state-by-month fixed effects.

Table 1: Summary Statistics - CPS ASEC Sample

	Total	Not ACA	ACA
Medicaid Coverage (=1)	0.86 (0.35)	0.81 (0.39)	0.89 (0.31)
Age	28.28 (5.73)	28.00 (5.61)	28.48 (5.81)
Married (=1)	0.53 (0.50)	0.48 (0.50)	0.56 (0.50)
Hispanic (=1)	0.28 (0.45)	0.27 (0.44)	0.30 (0.46)
White (=1)	0.78 (0.42)	0.76 (0.43)	0.79 (0.41)
Employed (=1)	0.59 (0.49)	0.53 (0.50)	0.63 (0.48)
Some College (=1)	0.50 (0.50)	0.46 (0.50)	0.53 (0.50)
<i>N</i>	1423	615	808

Notes: Data source is Current Population Survey Annual Social and Economic Supplement (CPS ASEC), 2019–2024. Each column reports means and standard deviations (in parentheses) for the sample of mothers with an infant who had Medicaid coverage during the prior calendar year. “Not ACA” refers to non-expansion states.

Table 2: Summary Statistics - Pregnancy Risk Assessment Monitoring Survey

	Below Pregnancy Eligibility		Above Pregnancy Eligibility	
	Mean	Std. dev.	Mean	Std. dev.
	(1)		(2)	
Medicaid	0.558	0.497	0.173	0.379
No Insurance	0.116	0.320	0.055	0.229
Prenatal and Current Medicaid	0.497	0.500	0.150	0.357
Postpartum Checkup	0.889	0.315	0.954	0.209
PP Depression	0.158	0.364	0.086	0.281
No PP Checkup due to Ins	0.018	0.131	0.007	0.085
Observations	5704		3962	

Notes: Data source is Illinois and Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2018-2022 and 2023 respectively. Descriptive statistics include the pooled means and standard deviations for IL and WI for the listed outcomes before any treatment from 2018-2019. Columns (1) and (2) present means and standard deviations for the women with household income below the pregnancy eligibility cutoff and Columns (3) and (4) present means and standard deviations for women with household income above the pregnancy eligibility cutoff.

Table 3: Summary Statistics - Main Sample (NielsenIQ Consumer Panel)

	Total	Inc > Preg	Inc < Preg	Not ACA	ACA
Total spending (\$)	498.87 (276.53)	485.17 (267.12)	567.82 (310.94)	514.87 (292.60)	490.05 (266.92)
Baby-care spending (\$)	15.01 (35.63)	14.07 (33.76)	19.70 (43.56)	14.65 (35.84)	15.20 (35.51)
Grocery spending (\$)	381.15 (213.54)	366.99 (201.19)	452.43 (255.73)	391.66 (227.98)	375.36 (204.97)
Alcohol spending (\$)	9.05 (22.71)	9.91 (24.06)	4.75 (13.23)	7.86 (18.90)	9.71 (24.54)
Health spending (\$)	5.85 (10.39)	6.09 (10.76)	4.66 (8.14)	5.94 (10.20)	5.81 (10.49)
Household size	3.28 (1.25)	3.17 (1.17)	3.84 (1.47)	3.20 (1.37)	3.32 (1.18)
Single family (=1)	0.86 (0.35)	0.88 (0.32)	0.72 (0.45)	0.84 (0.36)	0.86 (0.34)
White (=1)	0.78 (0.42)	0.79 (0.41)	0.72 (0.45)	0.79 (0.41)	0.77 (0.42)
Black (=1)	0.07 (0.26)	0.07 (0.25)	0.09 (0.28)	0.11 (0.32)	0.05 (0.22)
No. children under 18	1.11 (1.08)	1.02 (0.99)	1.56 (1.35)	0.94 (1.05)	1.20 (1.08)
Any HH head employed	0.94 (0.23)	0.97 (0.18)	0.83 (0.38)	0.94 (0.24)	0.95 (0.23)
Any HH head hsch	0.95 (0.21)	0.96 (0.18)	0.89 (0.31)	0.97 (0.17)	0.94 (0.23)
HH head age > 30	0.61 (0.49)	0.58 (0.49)	0.76 (0.43)	0.64 (0.48)	0.59 (0.49)
Married	0.89 (0.32)	0.90 (0.31)	0.84 (0.37)	0.88 (0.33)	0.89 (0.31)
Inc lower bound (\$1,000)	71.62 (23.99)	78.72 (18.94)	35.92 (11.73)	66.31 (22.53)	74.55 (24.26)
Inc higher bound (\$1,000)	75.76 (26.51)	86.86 (18.96)	42.48 (15.92)	74.48 (26.11)	76.67 (26.76)
<i>N</i>	3276	2733	543	1164	2112

Notes: Data source: NielsenIQ Consumer Panel, 2019–2020. Table reports mean values with standard deviations in parentheses. Columns compare the full sample (Total), households below vs. above the pregnancy-related Medicaid threshold (Inc < Preg and Inc > Preg), and households in ACA vs. non-ACA states.

Table 4: First Stage-Effects of FFCRA on Postpartum Medicaid Retention

	(1) Pooled Sample	(2) ACA	(3) Non-ACA
Post	0.131*** (0.035)	0.081** (0.037)	0.189*** (0.044)
Observations	1275	719	556
R-squared	0.219	0.255	0.229
Mean	0.789	0.855	0.726
Individual Controls	X	X	X
State FE	X	X	X
Cluster level	State	State	State

Notes: Data source is Current Population Survey Annual Social and Economic Supplement (CPS ASEC), 2019–2023. Each column reports survey-weighted regression estimates for mothers with an infant who had Medicaid coverage during the prior calendar year. The dependent variable is an indicator for current Medicaid enrollment at the March ASEC interview. “Post” equals 1 in 2021–2023 surveys. All models include maternal demographics, state, and year fixed effects. Standard errors are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5: DID Estimates of FFCRA Effects Health Utilization
Policy Effect: Comparing Pre- and Post-FFCRA

	PP Med.	Retained	PP Checkup	No Chk-no ins	PP Dep.
<i>Panel A. Pooled</i>					
FFCRA	0.107*** (0.016)	0.100*** (0.016)	-0.006 (0.010)	-0.003 (0.003)	0.000 (0.012)
Mean	0.433	0.390	0.905	0.009	0.134
Observations	17447	17462	17007	17462	16959
<i>Panel B. Wisconsin Only (non-ACA)</i>					
FFCRA	0.139*** (0.024)	0.133*** (0.024)	0.030* (0.016)	-0.006* (0.004)	0.037* (0.020)
Mean	0.507	0.454	0.884	0.011	0.159
Observations	9171	9180	8806	9180	8763
<i>Panel C. Illinois Only (ACA)</i>					
FFCRA	0.090*** (0.021)	0.082*** (0.022)	-0.013 (0.014)	-0.002 (0.004)	-0.018 (0.016)
Mean	0.351	0.319	0.927	0.008	0.108
Observations	8276	8282	8201	8282	8196

Notes: Data source is Illinois and Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2016-2022 and 2016-2023, respectively. Each column reports survey-weighted regression estimates for mothers with an infant born in the last year. Outcomes from the left to right are, indicators for currently on Medicaid, currently on Medicaid and had Medicaid during pregnancy, postpartum check-up, did not attend postpartum check up due to not having insurance, postpartum depression measure, and postpartum depression symptoms. The CDC measure of postpartum depression refers to a combined measure of reported postpartum depression and reported symptoms. Treatment is equal to 1 if the entirety of the household income is below the state's pregnancy eligibility cutoff after 2021. All models include controls for age, race, and education and standard errors are clustered at the treatment level. Estimates also control for the timing of Economic Impact Payments during COVID. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6: DID Estimates of FFCRA Effects on Spending
Policy Effect: Comparing Pre- and Post-FFCRA

	(1) All	(2) ACA	(3) Non-ACA
<i>Panel A. All Purchases</i>	31.872 (29.327)	17.298 (33.208)	105.310** (46.730)
Mean	484.755	484.562	485.112
Within R-sq.	0.021	0.029	0.034
<i>Panel B. Baby-Care Purchases</i>	8.669 (10.660)	-4.832 (8.499)	32.142* (15.969)
Mean	9.560	9.951	8.838
Within R-sq.	0.025	0.017	0.132
<i>Panel C. Grocery Purchases</i>	21.166 (27.174)	4.822 (31.649)	91.441** (33.040)
Mean	358.228	361.272	352.608
Within R-sq.	0.024	0.035	0.033
<i>Panel D. Alcohol Purchases</i>	-1.270 (1.887)	1.186 (2.531)	-1.936 (2.921)
Mean	9.851	10.828	8.046
Within R-sq.	0.011	0.024	0.001
<i>Panel E. OTC Medication Purchases</i>	-0.635 (0.810)	-1.097 (1.190)	0.367 (0.711)
Mean	6.218	6.037	6.551
Within R-sq.	0.002	0.001	0.011
Observations	3,276	2,112	1,164
Household Controls	X	X	X
Household FE	X	X	X
State \times Month FE	X	X	X
Cluster level	State	State	State

Notes: Data are from the NielsenIQ Consumer Panel, 2019-2020. This table reports DID estimates of the FFCRA's effects on household spending outcomes among newborn households. Outcomes are measured in dollar levels. Each coefficient is from a separate regression of the listed outcome on Post \times Treated. Columns labeled ACA and Non-ACA split the sample by state Medicaid expansion status. All regressions include household controls, household fixed effects, and state-by-month fixed effects. Standard errors are clustered at the state level and reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 7: DID Estimates of Spending Before and After Childbirth
Life-Cycle Effect: Comparing Pre- and Post-Childbirth

	(1) All	(2) ACA	(3) Non-ACA
<i>Panel A. All Purchases</i>	34.690 (40.513)	36.184 (49.225)	102.909 (67.756)
Mean	476.725	476.316	477.445
Within R-sq.	0.025	0.037	0.029
<i>Panel B. Baby-Care Purchases</i>	13.007 (15.520)	-5.577 (11.594)	59.110** (25.113)
Mean	9.183	9.058	9.402
Within R-sq.	0.050	0.040	0.187
<i>Panel C. Grocery Purchases</i>	22.689 (35.712)	19.230 (42.885)	87.765* (45.658)
Mean	357.709	361.132	351.680
Within R-sq.	0.028	0.045	0.028
<i>Panel D. Alcohol Purchases</i>	-0.496 (2.676)	1.550 (3.595)	-0.338 (4.964)
Mean	9.350	9.866	8.441
Within R-sq.	0.011	0.024	0.001
<i>Panel E. OTC Medication Purchases</i>	-0.526 (1.170)	-1.109 (1.397)	1.071 (1.612)
Mean	6.050	5.857	6.388
Within R-sq.	0.002	0.002	0.012
Observations	3,276	2,112	1,164
Household Controls	X	X	X
Household FE	X	X	X
State \times Month FE	X	X	X
Cluster level	State	State	State

Notes: Data are from the NielsenIQ Consumer Panel, 2019-2020. This table reports DID estimates of changes in household spending before and after childbirth among newborn households. Outcomes are measured in dollar levels. Each coefficient is from a separate regression of the listed outcome on Post-Birth \times Treated. Columns labeled ACA and Non-ACA split the sample by state Medicaid expansion status. All regressions include household controls, household fixed effects, and state-by-month fixed effects. Standard errors are clustered at the state level and reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Heterogeneous Analysis (Non-ACA Sample, Level Outcomes)

	All	Baby Care	Grocery
<i>Panel A. Pre and Post FFCRA</i>			
Post \times Treated	105.310** (46.730)	32.142* (15.969)	91.441** (33.040)
Post \times Treated \times College	43.497 (154.613)	60.571*** (12.923)	50.636 (114.070)
Post \times Treated \times Above 30	48.419 (91.926)	-34.703 (26.069)	-21.982 (62.034)
Post \times Treated \times White	25.599 (73.723)	-20.697 (36.277)	-48.024 (87.700)
<i>Panel B. Pre and Post Childbirth</i>			
Post \times Treated	98.653 (62.797)	54.557** (23.585)	85.367* (45.266)
Post \times Treated \times College	101.045 (192.534)	81.056*** (19.702)	120.881 (151.195)
Post \times Treated \times Above 30	26.681 (69.390)	-94.073** (29.824)	-90.039 (59.349)
Post \times Treated \times White	-46.085 (109.273)	-55.963 (38.302)	-103.064 (119.262)
Observations	1164	1164	1164

Notes: Data are from the NielsenIQ Consumer Panel, 2019-2020. The table reports heterogeneity in DID estimates of the FFCRA's effects on household spending for the non-ACA sample. Panel A reports estimates comparing pre and post FFCRA periods, and Panel B reports estimates comparing pre and post childbirth periods. The outcomes are spending in levels for all purchases, baby care purchases, and grocery purchases. Each specification includes the main DID term, *Post \times Treated*, and its interaction with the indicated household characteristic. All regressions include household and state-by-month fixed effects, and standard errors are clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Robustness Checks (Non-ACA Sample, Policy Timing)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	asinh(Y)	PPML	Wins 0.5/99.5	County CL	18-44 HH	Keep switcher	± 9 mon	Drop Mar2020
<i>Panel A. All Purchases</i>								
Post \times Treated	0.192* (0.096)	.	105.695** (47.319)	105.310* (60.948)	109.221** (48.466)	91.853* (44.742)	102.181* (55.483)	96.970** (43.457)
Observations	1164	.	1164	1164	1023	1289	880	1114
Mean (Pre, Control)	6.681	.	485.313	485.112	453.156	484.196	488.022	482.567
Within R-sq.	0.031	.	0.034	0.034	0.048	0.029	0.038	0.033
<i>Panel B. Baby-care Purchases</i>								
Post \times Treated	0.944** (0.398)	1.826*** (0.680)	41.157* (22.101)	32.142** (15.305)	28.070* (14.129)	27.411 (15.791)	32.543* (16.089)	30.891* (15.319)
Observations	1164	956	1164	1164	1023	1289	880	1114
Mean (Pre, Control)	1.068	11.428	9.202	8.838	9.795	8.503	8.862	7.662
Within R-sq.	0.048	.	0.138	0.132	0.099	0.089	0.108	0.136
<i>Panel C. Groceries Purchases</i>								
Post \times Treated	0.164 (0.105)	.	92.976** (34.351)	91.441* (47.169)	91.990** (37.382)	81.963** (32.418)	89.989** (40.055)	86.324** (30.141)
Observations	1164	.	1164	1164	1023	1289	880	1114
Mean (Pre, Control)	6.340	.	352.741	352.608	339.556	354.091	360.160	348.304
Within R-sq.	0.012	.	0.033	0.033	0.040	0.029	0.038	0.034

Notes: Data are from the NielsenIQ Consumer Panel, 2019–2020. The table reports robustness checks for the DID estimates of the FFCRA’s effects on household spending for the non-ACA sample, comparing pre and post FFCRA periods. All outcomes are measured in dollar levels, except in column (1), which uses the inverse hyperbolic sine transformation. Column (2) reports a Poisson (PPML) specification for baby-care spending. Column (3) uses spending winsorized at the 0.5 and 99.5 percent levels. Column (4) clusters standard errors at the county level. Column (5) restricts the sample to households with any female aged 18–44. Column (6) keeps income-bin switchers. Column (7) uses a balanced event window (± 9 months). Column (8) excludes March 2020. All regressions include household and state-by-month fixed effects unless otherwise noted. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

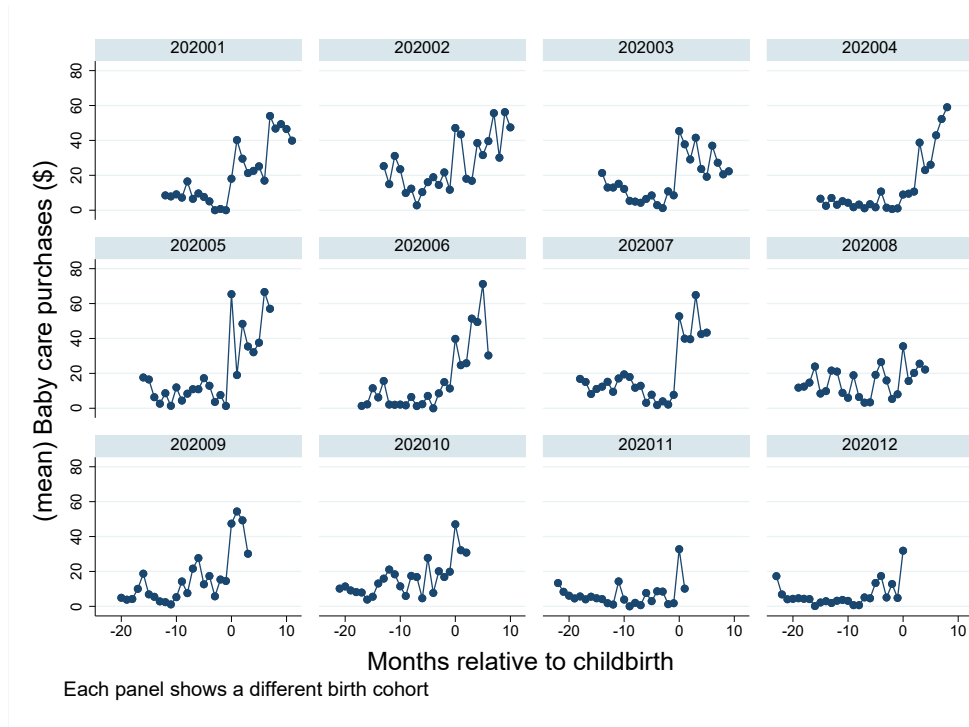
Table 10: Robustness Checks (Non-ACA Sample, Birth Timing)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	asinh(Y)	PPML	Wins 0.5/99.5	County CL	18-44 HH	Keep switcher	± 9 mon	Drop Mar2020
<i>Panel A. All Purchases</i>								
Post \times Treated	0.145 (0.121)	.	102.768 (68.659)	102.909 (85.784)	103.330 (71.602)	82.062 (64.991)	86.039 (92.320)	89.077 (54.712)
Observations	1164	.	1164	1164	1023	1289	652	1114
Mean (Pre, Control)	6.657	.	477.743	477.445	449.101	476.136	485.287	475.694
Within R-sq.	0.028	.	0.029	0.029	0.042	0.025	0.022	0.029
<i>Panel B. Baby-care Purchases</i>								
Post \times Treated	1.443** (0.603)	1.300*** (0.460)	76.994* (36.632)	59.110** (23.148)	53.710** (23.198)	50.585* (24.721)	60.221** (24.127)	59.253** (25.649)
Observations	1164	956	1164	1164	1023	1289	652	1114
Mean (Pre, Control)	1.128	11.937	9.649	9.402	10.212	8.876	11.061	8.869
Within R-sq.	0.087	.	0.198	0.187	0.155	0.148	0.136	0.195
<i>Panel C. Groceries Purchases</i>								
Post \times Treated	0.096 (0.138)	.	90.199* (47.402)	87.765 (66.646)	83.091 (52.509)	73.727 (46.418)	64.665 (61.181)	80.963* (36.745)
Observations	1164	.	1164	1164	1023	1289	652	1114
Mean (Pre, Control)	6.323	.	351.863	351.680	339.487	353.390	366.222	349.396
Within R-sq.	0.011	.	0.028	0.028	0.034	0.025	0.020	0.030

Notes: Data are from the NielsenIQ Consumer Panel, 2019–2020. The table reports robustness checks for the DID estimates of the FFCRA’s effects on household spending for the non-ACA sample, comparing before and after childbirth. All outcomes are measured in dollar levels, except in column (1), which uses the inverse hyperbolic sine transformation. Column (2) reports a Poisson (PPML) specification for baby-care spending. Column (3) uses spending winsorized at the 0.5 and 99.5 percent levels. Column (4) clusters standard errors at the county level. Column (5) restricts the sample to households with any female aged 18-44. Column (6) keeps income-bin switchers. Column (7) uses a balanced event window (± 9 months). Column (8) excludes March 2020. All regressions include household and state-by-month fixed effects unless otherwise noted. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

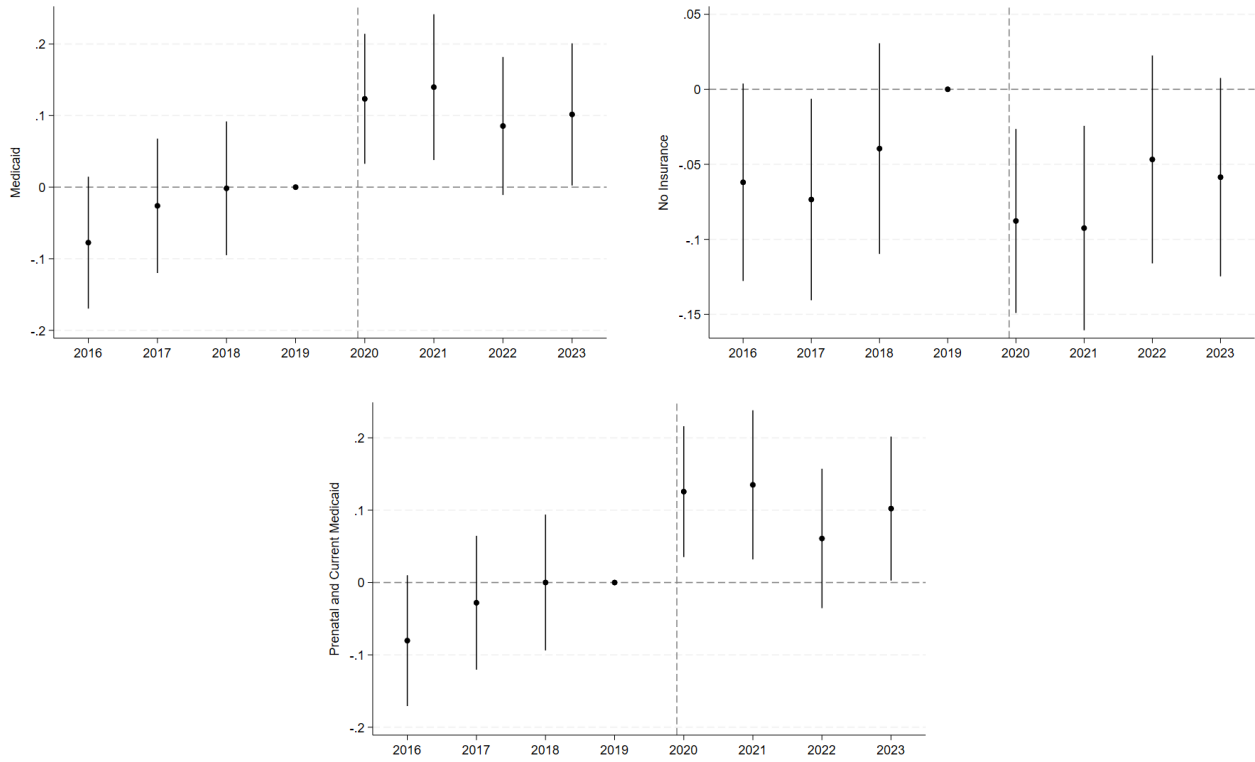
Appendix. Additional Figures and Tables

Figure A1: Average Monthly Baby-Care Purchases by Estimated Birth Cohort



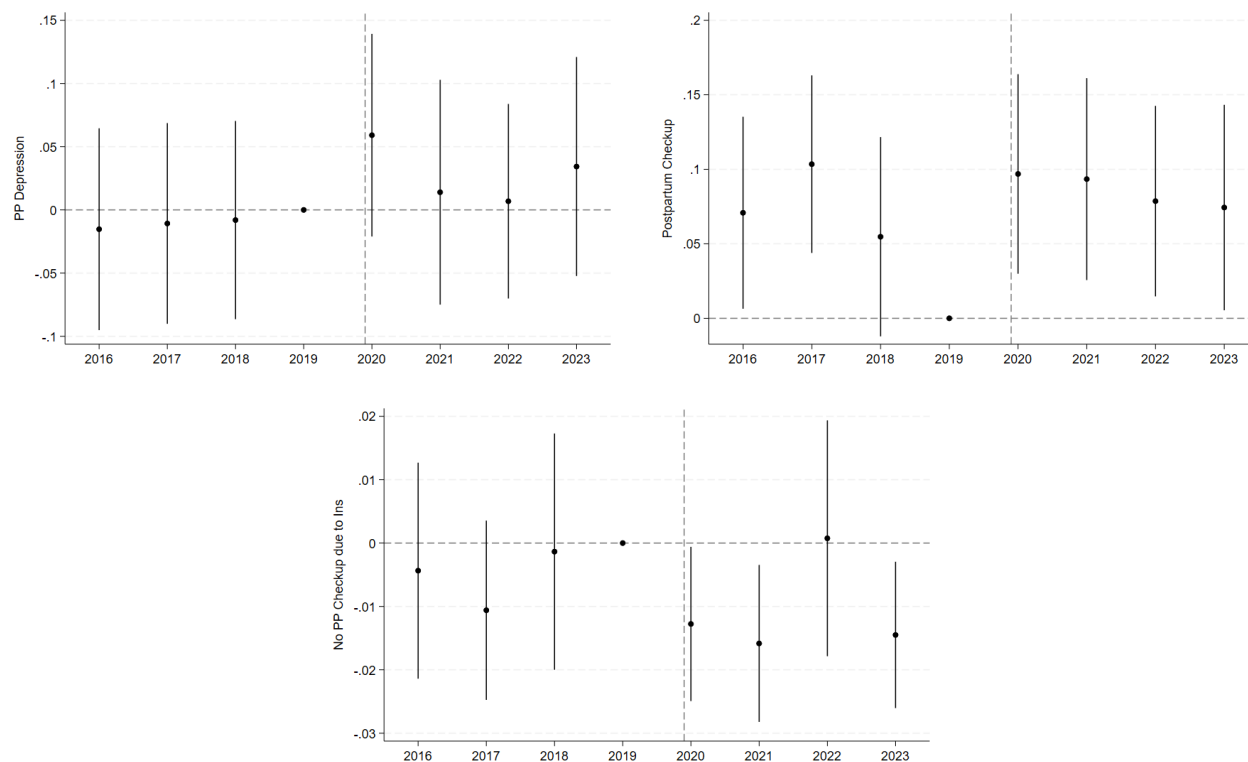
Notes: Data source is NielsenIQ Consumer Panel, 2019-2020. The figure plots average monthly baby-care purchases by estimated birth cohort, where month 0 denotes the inferred delivery month. Each panel corresponds to a distinct cohort identified by delivery year-month.

Figure A2: Event Study Estimates of FFCRA Effects on Health Insurance in Wisconsin
Policy Effect: Comparing Pre- and Post-FFCRA



Notes: Data source is Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2016-2023. Each figure reports pooled survey-weighted regression estimates for mothers with an infant born in the last year. Treatment is equal to 1 if the entirety of the household income is below the state's pregnancy eligibility cutoff after 2021. Outcomes are indicators for currently using Medicaid, currently no insurance, and used Medicaid during pregnancy and now. All models include maternal demographics controls and standard errors are clustered at the treatment level. Vertical bars indicate 95% confidence intervals.

Figure A3: Event Study Estimates of FFCRA Effects on Health Care in Wisconsin
Policy Effect: Comparing Pre- and Post-FFCRA



Notes: Data source is the Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2016-2023. Each figure reports survey-weighted regression estimates for mothers with an infant born in the last year. Treatment is equal to 1 if the entirety of the household income is below the state's pregnancy eligibility cutoff after 2021. Outcomes are indicators for postpartum depression, attended a postpartum check up, and did not attend a postpartum check up due to not having insurance. All models include maternal demographics controls and standard errors are clustered at the treatment level. Vertical bars indicate 95% confidence intervals.

Table A1: ACA Medicaid Expansion Effective Dates by State

State	Effective Date	State	Effective Date
Arizona	1/1/2014	New Hampshire	8/15/2014
Arkansas	1/1/2014	Pennsylvania	1/1/2015
California	1/1/2014	Indiana	2/1/2015
Colorado	1/1/2014	Alaska	9/1/2015
Connecticut	1/1/2014	Montana	1/1/2016
Delaware	1/1/2014	Louisiana	7/1/2016
District of Columbia	1/1/2014	Virginia	1/1/2019
Hawaii	1/1/2014	Maine	1/10/2019
Illinois	1/1/2014	Idaho	1/1/2020
Iowa	1/1/2014	Utah	1/1/2020
Kentucky	1/1/2014	Nebraska	10/1/2020
Massachusetts	1/1/2014	Oklahoma	7/1/2021
Maryland	4/1/2014	Missouri	10/1/2021
Michigan	4/1/2014	South Dakota	7/1/2023
Minnesota	4/1/2014	North Carolina	12/1/2023
Nevada	4/1/2014	Alabama	Not adopted
New Jersey	4/1/2014	Florida	Not adopted
New Mexico	4/1/2014	Georgia	Not adopted
New York	4/1/2014	Kansas	Not adopted
North Dakota	4/1/2014	Mississippi	Not adopted
Ohio	4/1/2014	South Carolina	Not adopted
Oregon	4/1/2014	Tennessee	Not adopted
Rhode Island	4/1/2014	Texas	Not adopted
Vermont	4/1/2014	Wisconsin	Not adopted
Washington	4/1/2014	Wyoming	Not adopted
West Virginia	4/1/2014		

Notes: Data are from the Kaiser Family Foundation (KFF), as of 2025.

Table A2: DID Estimates of FFCRA Effects on Spending
Policy Effect: Comparing Pre- and Post-FFCRA

	(1) All (Log)	(2) ACA (Log)	(3) Non-ACA (Log)
<i>Panel A. All Purchases</i>			
	0.021 (0.062)	0.005 (0.083)	0.191* (0.096)
Mean	5.999	6.003	5.991
Within R-sq.	0.020	0.027	0.031
<i>Panel B. Baby-Care Purchases</i>			
	0.082 (0.399)	-0.415 (0.425)	0.859** (0.359)
Mean	0.847	0.826	0.884
Within R-sq.	0.019	0.028	0.055
<i>Panel C. Grocery Purchases</i>			
	0.012 (0.074)	-0.012 (0.107)	0.165 (0.104)
Mean	5.667	5.674	5.654
Within R-sq.	0.015	0.022	0.013
<i>Panel D. Alcohol Purchases</i>			
	-0.261* (0.138)	-0.127 (0.158)	-0.270 (0.277)
Mean	0.877	0.896	0.842
Within R-sq.	0.014	0.028	0.006
<i>Panel E. OTC Medication Purchases</i>			
	-0.079 (0.100)	-0.086 (0.148)	-0.057 (0.084)
Mean	1.035	1.004	1.094
Within R-sq.	0.002	0.003	0.009
Observations	3,276	2,112	1,164
Household Controls	X	X	X
Household FE	X	X	X
State \times Month FE	X	X	X
Cluster level	State	State	State

Notes: Data are from the NielsenIQ Consumer Panel, 2019-2020. This table reports DID estimates of the FFCRA's effects on household spending outcomes among newborn households. Outcomes are measured in log form. Each coefficient is from a separate regression of the listed outcome on Post \times Treated. Columns labeled ACA and Non-ACA split the sample by state Medicaid expansion status. All regressions include household controls, household fixed effects, and state-by-month fixed effects. Standard errors are clustered at the state level and reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A3: DID Estimates of Spending Before and After Childbirth
Life-Cycle Effect: Comparing Pre- and Post-Childbirth

	(1) All (Log)	(2) ACA (Log)	(3) Non-ACA (Log)
<hr/>			
<i>Panel A. All Purchases</i>	0.023	0.057	0.145
	(0.088)	(0.122)	(0.120)
Mean	5.978	5.985	5.967
Within R-sq.	0.025	0.036	0.028
<i>Panel B. Baby-Care Purchases</i>	0.032	-0.582	1.323**
	(0.549)	(0.554)	(0.542)
Mean	0.823	0.760	0.933
Within R-sq.	0.071	0.092	0.096
<i>Panel C. Grocery Purchases</i>	0.031	0.063	0.100
	(0.103)	(0.140)	(0.136)
Mean	5.662	5.676	5.637
Within R-sq.	0.019	0.029	0.012
<i>Panel D. Alcohol Purchases</i>	-0.169	-0.131	0.073
	(0.190)	(0.257)	(0.291)
Mean	0.847	0.830	0.878
Within R-sq.	0.013	0.029	0.005
<i>Panel E. OTC Medication Purchases</i>	-0.011	0.015	-0.084
	(0.143)	(0.167)	(0.277)
Mean	1.007	0.978	1.057
Within R-sq.	0.002	0.003	0.011
Observations	3,276	2,112	1,164
Household Controls	X	X	X
Household FE	X	X	X
State \times Month FE	X	X	X
Relative-to-Birth Month FE	X	X	X
Cluster level	State	State	State

Notes: Data are from the NielsenIQ Consumer Panel, 2019-2020. This table reports DID estimates of changes in household spending before and after childbirth among newborn households. Outcomes are measured in natural logs. Each coefficient is from a separate regression of the listed outcome on $\text{PostBirth} \times \text{Treated}$. Columns labeled ACA and Non-ACA split the sample by state Medicaid expansion status. All regressions include household controls, household fixed effects, and state-by-month fixed effects. Standard errors are clustered at the state level and reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A4: Controlling for Stimulus Checks (Non-ACA Sample, Level Outcomes)

	(1)	(2)	(3)	(4)
1. Pre and Post FFCRA				
<i>Panel A. All Purchases</i>				
Post × Treated	105.310** (46.730)	87.261* (43.951)	106.274** (47.011)	82.674 (56.095)
<i>Panel B. Baby-care Purchases</i>				
Post × Treated	32.142* (15.969)	24.808 (13.974)	27.960 (15.479)	33.695* (16.841)
<i>Panel C. Grocery Purchases</i>				
Post × Treated	91.441** (33.040)	78.390** (33.659)	88.583** (34.273)	82.223* (43.811)
2. Pre and Post Childbirth				
<i>Panel A. All Purchases</i>				
Post × Treated	102.909 (67.756)	59.068 (68.327)	68.126 (82.301)	73.153 (64.552)
<i>Panel B. Baby-care Purchases</i>				
Post × Treated	59.110** (25.113)	47.367* (24.848)	52.018* (24.974)	64.111** (26.584)
<i>Panel C. Grocery Purchases</i>				
Post × Treated	87.765* (45.658)	56.481 (49.191)	56.951 (51.706)	62.650 (46.000)
Observations	1164	1164	1067	820

Notes: Data are from the NielsenIQ Consumer Panel, 2019–2020. The table reports difference-in-differences estimates for non-expansion states. Panel set 1 uses policy timing and Panel set 2 uses childbirth timing. All specifications include household and state-by-month fixed effects, and standard errors are clustered at the state level. Column (1) is the baseline. Column (2) adds an indicator for the second stimulus check period interacted with treatment. Column (3) drops months covering the first and second stimulus check periods. Column (4) restricts the sample to households with income below \$100,000. Outcomes are in dollars; standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.