When Health Insurance Extends Beyond Health: Postpartum Medicaid and Household Spending

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Abstract

Public health insurance can free up household resources by reducing medical expenses, yet its broader consumption effects remain understudied. We study how Medicaid coverage affects retention, household spending, and resource reallocation using coverage extensions from the Families First Coronavirus Response Act (FFCRA), which mandated continuous enrollment and effectively extended coverage for women enrolled in Medicaid during pregnancy. Using monthly product-level purchase records from the NielsenIQ Consumer Panel (2019–2020) and Pregnancy Risk Assessment Monitoring Survey (PRAMS) data linked to state-specific Medicaid income thresholds, we analyze how household spending responded to FFCRA among households with newborns. Difference-in-differences estimates show that Medicaid retention for newly eligible women increases by around 65 percent and that extended postpartum coverage roughly doubles spending on baby care products and increases total household expenditures by about 21 percent. The spending increase occurs after 60 days postpartum, consistent with the coverage extension relaxing household budget constraints. We find suggestive evidence that the spending response operates through reduced out-of-pocket medical costs rather than changes in labor supply.

Keywords: Medicaid, postpartum coverage, FFCRA, household spending, resource allocation
JEL Codes: I13, I18, D12

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I. Introduction

Public health insurance is the largest means-tested program in the U.S., with total Medicaid spending over 900 billion in 2024 (Kaiser Family Foundation, 2025a). Given its scale, a robust economics literature studies how access to Medicaid affects health outcomes (Aron-Dine et al., 2013; Goodman-Bacon, 2021; Miller et al., 2021). Medicaid also serves a broader purpose: it protects families from the financial risk associated with medical expenses. By reducing this financial exposure, public health insurance has the potential to free up household resources for other needs. However, there is limited direct evidence on whether this protection translates into observable changes in household spending and resource allocation.

If resources are fungible, a reduction in expected medical expenses should affect household consumption like an equivalent increase in disposable income, with spending allocated across categories according to households' preferences (Friedman, 1957; Hall, 1978). Alternatively, households may instead engage in mental accounting, treating resources differently depending on their source (Thaler, 1999; Hastings and Shapiro, 2018). These competing predictions make it an empirical question not only whether Medicaid changes household spending, but which categories respond.

In this paper, we ask two questions: does access to public health insurance shield households from financial insecurity during periods of heightened need? And does it allow them to reallocate resources across competing needs? We answer these questions by exploiting an abrupt and meaningful change to Medicaid coverage brought about by the Families First Coronavirus Response Act (FFCRA) to measure how public health insurance can affect spending patterns within households. 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 change impacts a large population given that approximately 42 percent of all births are paid for by Medicaid.

We examine how continuous coverage impacts Medicaid access during the postpartum period using Pregnancy Risk Assessment Monitoring Survey (PRAMS) from 2016-2022. Then we estimate the effect on household spending and resource allocation using detailed monthly purchase data from NielsenIQ Homescan Consumer Panel from 2019-2020. 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 and to understand when during the postpartum period the effects emerge, we also estimate event study models that trace spending around childbirth instead of the FFCRA.[1]

First, using PRAMS data from Wisconsin and Illinois, we show that expanded coverage through the FFCRA increases newly eligible postpartum mothers' retention of Medicaid after pregnancy by 16 percentage points, or 65 percent relative to the baseline mean. This is accompanied by a 61 percent reduction in the share of women with no insurance, which is evidence against crowd-out. These findings suggest that extending insurance coverage could impact the distribution of household resources between medical care and other purchases by easing the financial burden of medical care paid for out-of-pocket.

Our findings are consistent with Hu et al. (2018) which estimates that Medicaid expansion decreases unpaid balances in collection by $1,140. The financial impact of continuous coverage under the FFCRA may be even larger, since it extends coverage for women postpartum, a period of significant healthcare costs. More than 70 percent of postpartum spending occurs after 90 days and total spending for those with private insurance during the first year postpartum is $3,100 (Bloschichak and Martin, 2020).[2]

[1] We address additional concerns relating to COVID-19 further in Section IV.2.

[2] Within 90 days postpartum, healthcare utilization includes postpartum visits, testing, reproductive procedures, vaccines, and surgery.

We show that this financial easement translates into changes in spending and in reallocation of resources. Particularly, extended coverage increases 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 increase 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 ACA non-expansion states, where we see the sharpest postpartum coverage cliff. Spending around childbirth increases only after the first 60 days postpartum, which is when coverage would have ended without the FFCRA. These results show that continuous Medicaid coverage functions not only as health insurance, but also eases short term financial constraints. Households respond to more stable insurance by increasing spending on essential goods for infants rather than a broad rise in all categories, highlighting that the value of public insurance extends beyond healthcare utilization.

Finally, we show suggestive evidence that a reduction in out-of-pocket medical spending is a plausible mechanism behind the increase in grocery spending. Using PRAMS data, we find no statistically significant change in postpartum visits, though the estimate is imprecise we can rule out changes greater than 6 percentage points. This pattern is consistent with extended coverage allowing mothers to receive care they would otherwise have paid for out-of-pocket, effectively lowering their medical expenses without necessarily changing the amount of care received. Further, using CPS ASEC data, we find little evidence that extended coverage systematically changed postpartum labor market outcomes, suggesting that labor supply is unlikely to explain the observed spending response.

In answering how public health insurance impacts reallocation within a household, we contribute to 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.[3] 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 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).[4] 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).[5] 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 mar-

[3]Dillender (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.

[4]Hastings 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.

[5]Bitler 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.

gins 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 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 causes 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 Medicaid expansion states and ACA non-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 direct 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 re-

mained 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.[6] 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, 2025c). 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 ACA non-expansion states, the coverage cliff was much larger. Without an adult cate-

[6]Table A1 reports each state's ACA Medicaid expansion status and effective date, and Table A2 reports each state's adult, parent, and pregnancy-related Medicaid eligibility cutoffs in 2020.

gory, 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.[7] 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.[8] By the end of 2020, 37 states had expanded Medicaid under the ACA. Thus, states that had yet to expand Medicaid by 2020 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

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

[8]Prior 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.

sharp break from pre-pandemic policy.

The effect was particularly pronounced in ACA 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.[9] 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). Other ACA non-expansion states had even more extreme coverage gaps because Wisconsin implemented a partial expansion through their own program. Expansion states experienced a smaller though still meaningful change. Women with incomes above the cutoff for adult Medicaid of 138 percent 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.[10] 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).[11] 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.[12] 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

[9]Notably, Wisconsin has the highest pregnancy income threshold in the U.S.

[10]The 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, 2024b).

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

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

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. Eligibility and Medicaid Data

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, 2025b) and data on state income eligibility thresholds from KFF's "Trends in Medicaid Income Eligibility Limits" database, which provides annual updates on eligibility criteria (Kaiser Family Foundation, 2025c). 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 two data sources. First, we use the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) from 2019 to 2024 to provide national descriptive evidence on Medicaid retention among mothers with a newborn. The CPS ASEC is a yearly national survey conducted by the U.S. Census Bureau that collects detailed information on income, state of residence, health insurance, and demographics. We focus on women with an infant younger than one year at the time of the March interview.

We also use data from the Pregnancy Risk Assessment Monitoring System (PRAMS), which 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 PRAMS data

from Illinois and Wisconsin, which provide a useful comparison because Illinois expanded Medicaid under the ACA while Wisconsin did not.[13] The analytic sample includes women who gave birth between 2018 and 2022 in Wisconsin and Illinois, covering both the pre- and post-FFCRA periods.

In both the CPS ASEC and PRAMS samples, we focus on women more likely to face a postpartum coverage cliff. In ACA non-expansion states, we exclude women with family income below the state-year parent eligibility cutoff, and in ACA expansion states, we exclude mothers with family income below 100 percent of the FPL, which corresponds to the highest parent eligibility cutoff observed among ACA non-expansion states. This restriction removes very low-income mothers most likely to remain eligible after pregnancy-related Medicaid expired, while preserving ACA expansion states as a lower-exposure subgroup using a consistent definition.

Table 1 reports summary statistics for the CPS ASEC sample. About 20 percent of mothers report Medicaid coverage in the prior calendar year, and 18 percent report current Medicaid coverage. Among mothers with prior-year Medicaid coverage, Medicaid retention is 87 percent overall, with higher retention in ACA expansion states than in non-ACA states. Table 2 reports summary statistics for the PRAMS sample. Before the FFCRA, postpartum women below the pregnancy eligibility cutoff were more likely to be on Medicaid, more likely to be uninsured, and more likely to retain Medicaid after pregnancy than women above the cutoff.[14]

[13] Wisconsin is a unique ACA non-expansion state because it partially expanded Medicaid to adults up to 100 percent of the FPL through BadgerCare Plus.

[14] We also use these data sources to examine additional outcomes that help explore mechanisms underlying the spending responses. In PRAMS, we study postpartum healthcare utilization, including indicators for having a postpartum checkup and not having a checkup because of lack of insurance. In the CPS ASEC, we examine labor-market outcomes among mothers with a newborn, including employment, part-time work, usual hours worked, parental leave, and part-time work due to child-care constraints.

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

[15]Extreme 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.

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.[16] 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.[17] That identified 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 went into effect.[18] We merge state identifiers with Medicaid expansion and eligibility data. The resulting dataset is an unbalanced household-month panel with 3,276 observations, as we show in Table 3. About 17 percent of households fall below the pregnancy-related Medicaid threshold, and around 36 percent live in ACA non-expansion states. Average monthly spending is about $500. Total, baby care, and grocery spending is somewhat higher among treated households with lower income, likely reflecting larger family size, as these

[16] We also considered using households with income ranges entirely below the relevant adult or parent eligibility threshold as an alternative control group, since they were likely to remain Medicaid-eligible without the FFCRA. However, due to limited statistical power in our data, this group cannot serve as a valid control group.

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

[18] We restrict the sample to births on or after February, 2020, so that mothers were still within the standard 60-day postpartum Medicaid coverage period when the FFCRA continuous-enrollment requirement began on March 18, 2020. Mothers with January births may have reached the postpartum eligibility limit before the policy took effect, making their coverage status at implementation less clear.

households tend to have more children. Other demographics are similar across treatment and policy groups.

IV. Empirical Strategy

IV.1. Empirical Design

Our preferred estimation strategy for estimating the effects of the FFCRA is a difference-in-differences design that exploits variation before and after the FFCRA across households with different exposure to the postpartum Medicaid coverage cliff. First, we study whether the FFCRA increases Medicaid enrollment and retention among postpartum women using PRAMS data from Illinois and Wisconsin. Because the FFCRA began in 2020, we consider this the first treated year. Formally, we estimate models of the following form using ordinary least squares:

$$y_{hy} = \alpha_h + \alpha_y + FFCRA_{hy}\delta + X_{hy}\beta + \epsilon_{hy} \tag{1}$$

where y_{hy} is the Medicaid enrollment, retention, and a measure for no insurance for a given household h in year y. α_e and α_y represent eligibility and year fixed effects, respectively. $FFCRA_{hy}$ is a dummy variable equal to one if household h has income that falls below the state specific pregnancy-related Medicaid eligibility cutoff after 2020 and zero if the income is above the cutoff.[19] X_{hy} 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. Standard errors are clustered at the eligibility level and we use PRAMS survey weights.[20]

[19]Income 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.

[20]We also use the same framework to examine healthcare utilization outcomes in PRAMS as part of the mechanism analysis, including indicators for postpartum checkups and for not having a checkup due to a lack of insurance. In addition, we use a specification analogous to equation (1) to examine labor-market outcomes of new mothers in the CPS ASEC sample. The CPS analysis compares mothers with a newborn whose family income falls below versus above the relevant Medicaid eligibility cutoff before and after the FFCRA continuous coverage provision. The outcomes include employment, part-time work, usual hours worked, parental leave, and part-time work due to child-care constraints. These regressions include

We next examine reduced form impacts of postpartum Medicaid extensions on household spending behavior using a similar difference-in-differences design. The household spending outcomes are monthly measures of household purchases, including total, baby-care, and health-related spending. This data is reported monthly instead of yearly, so in this case, the formal specification is:

$$y_{hsym} = \alpha_h + \alpha_{sym} + FFCRA_{hsym}\delta + X_{hy}\beta + \epsilon_{hysm} \tag{2}$$

y_{hsym} is the monthly spending of the household h in state s, year y and month m, measured in dollars or logarithmic form. $FFCRA_{hsym}$ is a dummy variable equal to one for households whose income falls below the state specific pregnancy-related Medicaid eligibility cutoff after April 2020 and zero otherwise. $X_h y$ is a set of household demographics listed in Table 3. We include household fixed effects α_h and state-by-year-month fixed effects α_{sym}, which absorb shocks common to all households in a given state and month. Standard errors are clustered at the state level.

Households with incomes below their state's pregnancy eligibility cutoff 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. We define treatment eligibility based on households' pre-period income in 2019 to avoid conditioning treatment status on income changes that may themselves be affected by the policy or correlated with post-policy outcomes. 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.

demographic controls, along with state and year fixed effects, and we use CPS person weights.

To assess pre-FFCRA trends and show dynamic patterns, we estimate an event-study specification grouping months into four-month bins:

$$Y_{hsym} = \alpha_h + \alpha_{sym} + \sum_{k=-4,|,k\neq -1}^{4} (\tau_k \times \text{Treated}_h)\, \delta_k + \beta X_{hy} + \epsilon_{hsym} \tag{3}$$

where $\tau_k = 1Q - Q_b = k$ is an indicator for four-month bin k relative to the FFCRA implementation date. The post-FFCRA coefficients $(\delta_0, \ldots, \delta_4)$ measure 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).

The main specification measures time relative to the implementation of the FFCRA. To further characterize the timing of the spending response over the postpartum life cycle, we implement a secondary approach that replaces the policy-timing indicator with an indicator for months after childbirth. This approach serves three purposes. First, it shows when the spending response emerges over the childbirth life cycle, providing a more direct view of how household spending evolves around the birth. Second, it helps distinguish whether responses occurred before or after delivery, allowing us to assess whether the effects operate through expected or realized extensions of postpartum coverage. Lastly, by moving away from FFCRA timing, this approach also helps address concerns that the timing of the FFCRA coincided with the rollout of other COVID-19 policies that directly affected low-income households.

We estimate difference-in-differences and event-study models corresponding to equation (2) and (3):

$$Y_{hsym} = \delta(\text{PostBirth}_{ym} \times \text{Treated}_h) + X'_{hy}\theta + \alpha_h + \alpha_{sym} + \epsilon_{hsym} \tag{4}$$

$$Y_{hsym} = \sum_{r\neq -1} \delta_r\, \mathbf{1}\{R - R_b = r\} \times \text{Treated}_h + X'_{hy}\theta + \alpha_h + \alpha_{sym} + \epsilon_{hsym} \tag{5}$$

In equation (4), 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. $\mathbf{1}\{R - R_b = r\}$ in equation (5) is an indicator for the period relative to delivery ($r = 0$).[21]

IV.2. Identification

Our primary identification strategy relies on the assumption 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 postpartum Medicaid usage and spending trends over time. The main concern is that the FFCRA began at the onset of the COVID-19 pandemic, when pandemic-era shocks and 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 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

[21] Months 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).

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.

Lastly, because we use income to define treatment, we may be concerned that households with low income respond differently to the FFCRA or childbirth than households in our control group with higher income regardless of expanded Medicaid coverage. We directly address this concern in Section V.6 by restricting the sample of households to exclude those with incomes in the top income bins.

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 causal estimates, Figure 1 illustrates the raw trends in postpartum Medicaid retention rates for ACA and ACA non-expansion states.[22] The figure shows that ACA non-expansion 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 expansion states had a consistently higher retention rate prior to the FFCRA and experienced a mild increase during the policy period. In this sense, the FFCRA allowed mothers in ACA non-expansion states to catch up partially to the higher postpartum Medicaid retention rates already observed in ACA expansion states.

To estimate the causal impact of extended postpartum coverage on Medicaid retention we use PRAMS data for two states where the treated group are those with incomes below the states' pregnancy Medicaid eligibility cutoff after the FFCRA. We present results of equation

[22]These raw trends are intended mainly to illustrate the timing and pattern of changes in postpartum Medicaid retention. 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.

(1) for the pooled sample of mothers in Wisconsin and Illinois in panel A, Wisconsin mothers only in panel B, and Illinois mothers only in panel C of Table 4. Estimates in column (1) of Panel A indicate a 16.1 percentage point or 56.7 percent increase in any Medicaid coverage for women within one year postpartum.[23] Moreover, estimates in column (2) show that the overall increase comes from women who had Medicaid in the prenatal period and retain coverage in the postpartum period, as evidenced by a 16.3 percentage point or 65 percent increase. Finally in column (3) we show a 3.6 percentage point or 61 percent reduction in having no insurance in the postpartum period. In panels B and C, we show that results are stronger in magnitude in Wisconsin, a ACA non-expansion state, than in Illinois, a state that expanded Medicaid through the ACA although the estimates are not statistically different from one another at the 5 percent level. We present corresponding event studies for the pooled estimates in Figure 3 and for Wisconsin in Figure A2. Taken together, these results provide evidence that the FFCRA increases Medicaid retention within one year postpartum and the increase is not driven by crowd-out, but instead by women who would otherwise be uninsured. These results provide the first stage evidence for our subsequent analysis of household spending.

Because the causal estimates on Medicaid retention are only from two states, we also use CPS data to provide descriptive evidence of a similar increase in Medicaid retention for all states. Table 5 reports descriptive evidence on postpartum Medicaid retention during the FFCRA period.[24] Column (1) uses the full sample, while columns (2) and (3) estimate the same specification separately for ACA expansion states and ACA non-expansion states. The results show that postpartum Medicaid retention is higher during the FFCRA period.

[23]To put these magnitudes in population terms, KFF reports that Medicaid financed 21,628 births in Wisconsin and 51,681 births in Illinois in 2020 (Kaiser Family Foundation, 2024a). As a back-of-the-envelope calculation, applying our estimates to these cohorts implies roughly 11,800 additional postpartum women with Medicaid coverage per year across the two states.

[24]We estimate this specification for the full sample and separately for ACA and ACA non-expansion states: $\text{Medicaid}_{isy} = \alpha + \beta \text{Post}_y + X_i'\gamma + \mu_s + \varepsilon_{isy}$ 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. X_i denotes a set of individual characteristics and μ_s represents state fixed effects. Standard errors are clustered at the state level and we apply CPS person weights.

Across columns (1)-(3), estimates indicate a relative increase in Medicaid retention of 9.5 to 26 percent, with the highest increase in ACA non-expansion states. These results are consistent with the causal evidence from Illinois and Wisconsin showing that the FFCRA substantially increases postpartum Medicaid retention among newly eligible, particularly in ACA non-expansion states.

V.2. Policy Effects on Household Spending

We next examine whether postpartum Medicaid extensions translate into changes in household spending behavior. We do so to investigate how public insurance affects household budget constraints in a way that changes purchases. Because the FFCRA created the largest change in postpartum coverage in ACA non-expansion 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 ACA non-expansion 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 ACA non-expansion 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 (2), and compares the effects across the full sample, ACA expansion states, and ACA non-expansion states. This comparison confirms that the spending response is concen-

trated in ACA non-expansion states, where the FFCRA generated the largest extension of postpartum Medicaid coverage. We estimate the model using spending amounts in dollars.[25]

Panel A presents the results for total purchases. The estimates show that household spending increases after the FFCRA took effect, mainly among treated households in ACA non-expansion states. Compared with households not eligible for pregnancy-related Medicaid, monthly spending rises by about $105.3. In contrast, the effect in ACA expansion states, that we show in column (2), is much smaller and statistically insignificant.

Panel B presents the results for baby care purchases and shows a similar pattern. In ACA non-expansion states, treated households increase baby-care spending by about $32.1 per month after the FFCRA.[26] By contrast, the estimates for ACA expansion 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. We further separate baby-care spending into baby-food and baby-needs purchases in Appendix Table A4, Panel A. The results show that the increase in baby-care spending in ACA non-expansion states is driven primarily by baby-food purchases.

Panels C through E further explore how households with newborns reallocated spending across specific categories. Panel C shows positive effects for grocery purchases. In ACA non-expansion states, grocery spending rises by about $91.4. This pattern suggests that newly covered households increase 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 suggesting that the increase in household spending after the FFCRA was not driven by discretionary spending on alcohol. Finally, panel E similarly shows little evidence of a change in OTC medication purchases, and estimates are close to zero and statistically insignificant in both ACA and ACA non-expansion

[25]Appendix Table A3 reports analogous specifications using log spending outcomes. In ACA non-expansion 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.

[26]The tables report the pre-policy control-group mean. For baby-care purchases, this mean is low because the pre-period includes months before childbirth, when baby-care purchases are naturally limited. As an additional benchmark, the post-period control-group mean for baby-care purchases is about $22 per month.

states.

To interpret the magnitudes, we compare the grocery spending response to a benchmark estimate of the value of Medicaid coverage. Hendren and Sprung-Keyser (2020) estimate a willingness to pay for pregnancy-related Medicaid coverage of $1,737 in 2012 dollars, or about $1,958 in 2020 dollars.[27] Dividing this annual value by twelve gives a monthly benchmark of about $163. Relative to this benchmark, the $91 increase in monthly grocery spending corresponds to roughly 56 cents per dollar of Medicaid coverage value. This magnitude is close to Hastings and Shapiro (2018), who estimate a marginal propensity to consume out of SNAP benefits between 0.5 and 0.6.

Overall, these findings indicate that the postpartum Medicaid coverage expansion causes a significant increase in household spending among affected low-income families in ACA non-expansion 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. Timing of Spending Responses Around Childbirth

The main estimates in Section V.2. capture average changes after the FFCRA took effect, but they cannot speak to when within the life cycle of pregnancy and postpartum recovery these spending responses emerge. This timing matters for two reasons. First, households may adjust spending in anticipation of extended coverage if they learn about the policy before or around delivery. Second, because the FFCRA extended Medicaid coverage beyond the traditional 60-day postpartum limit, spending increases after this cutoff would

[27] We convert the Hendren and Sprung-Keyser (2020) estimate to 2020 dollars using the CPI-U annual average from the Bureau of Labor Statistics. The estimate is for pregnancy-related Medicaid coverage, so we use it as a benchmark for the value of Medicaid coverage during the postpartum period.

provide stronger evidence that households responded to the coverage extension rather than to childbirth itself.

To examine this life-cycle timing, we use a supplementary design that exploits delivery timing rather than policy timing, allowing us to trace spending responses across pregnancy and the postpartum period. Figure 2 first shows raw spending patterns around childbirth for treated and control households in both ACA and non-ACA subgroups. Spending rises after childbirth for all groups, but the increase is largest for treated households in ACA non-expansion states, the group most affected by the FFCRA postpartum coverage extension. Table 7 and Figure 5 present the results from equations (4) and (5). Figure 5 shows the dynamic effects for households in ACA non-expansion states. Across the five categories, the pre-childbirth coefficients are generally close to zero, suggesting no differential pre-trends between treated and control households. Total spending trends upward after delivery as shown in panel A, with the largest increase appearing later in the postpartum period.

Panel B presents estimates for baby-care purchases, which show the clearest increase. 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 after this point, when the FFCRA extended postpartum coverage beyond the traditional limit, and continues to rise in later months. This pattern indicates that the policy's coverage extension, rather than childbirth alone, played a direct role in sustaining higher consumption of baby-related goods.

The point estimates in Table 7 are broadly consistent with the policy-timing results. Panel A shows that total household spending rose after childbirth among treated households, with magnitudes similar to those in Table 6, although the estimates are not statistically significant. One possible explanation is that most births in the sample occurred after the FFCRA had been announced, so households may have been aware of the policy at delivery. Some spending responses may therefore have occurred before childbirth, reflecting expectations of continued coverage rather than a sharp change after birth. This anticipa-

tion could smooth spending patterns around delivery and reduce precision, even though the point estimates remain consistent with a positive FFCRA effect. This is relevant for policy contexts outside of the FFCRA, where coverage beyond 60 days postpartum may be known at the start of pregnancy.

Turning to baby-care purchases, Panel B shows that treated households in ACA non-expansion states increased baby-care spending by about $59.1 per month relative to ineligible households.[28] Appendix Table A4, Panel B, shows that this increase is driven primarily by baby-food purchases. Baby-care items 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.[29] Panel C shows a similar, though less precisely timed, increase in grocery spending. Panels D and E show no clear changes in alcohol or over-the-counter medication purchases.

Overall, these results reinforce our main findings while adding evidence on when the response occurs. Spending increases are concentrated in categories most closely tied to households with newborns, especially baby-care goods, and the timing of the baby-care response after the 60-day cutoff is consistent with households responding to the extension of postpartum Medicaid coverage.

[28] As an additional benchmark, the post-birth control-group mean for baby-care purchases in this design is about $28 per month.

[29] 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.

V.4. Heterogeneity Analysis

We next examine whether the spending responses differ across household characteristics, focusing on the three categories with the clearest main effects: total purchases, baby-care purchases, and grocery purchases. Using the policy-timing design in equation (2), we allow the treatment effect to vary by four characteristics: whether the female household head has a college degree, whether she is older than 30, whether the household is White, and whether the household's income bin is below the state-specific median income bin among treated households. As with the baseline specification, all households within the sample have infants. We extend the baseline specification by interacting the DID estimator with each household characteristic. Table 8 reports the results.

Overall, the estimates provide limited evidence of systematic heterogeneity. For total purchases and grocery purchases, the interaction terms are generally imprecise and do not show a consistent pattern. The clearest differences appear for baby-care purchases. The response is larger for lower-income households, suggesting that even among eligible households, the coverage extension had stronger effects for families with fewer financial resources. We also find a larger increase in baby-care spending among households with college-educated female heads, which may reflect differences in information or the ability to respond to the policy. The estimates by age and race provide no clear evidence of differential responses.

V.5. Mechanisms

We show that extending postpartum coverage increases total spending on baby care items and groceries, suggesting that households change mental accounting and experience a relaxation of the household budget constraint. The increase in the household budget could result from Medicaid freeing up resources through reduced out-of-pocket medical expenses or from increased labor supply as continuous enrollment removed the income-based eligibility cliff that previously discouraged mothers from increasing work hours or earnings. To disentangle

these mechanisms, we first examine whether out-of-pocket medical spending changes and by how much.

Because we do not have a direct measure of out-of-pocket spending, we use data on postpartum visits from PRAMS as a proxy. There are three possible patterns of changes in healthcare utilization with extended Medicaid coverage relative to a counterfactual in which households do not receive postpartum coverage, each with different implications for the household budget. First, if healthcare use is unchanged, then we expect an increase in the household budget because households receive the same amount of care but reduce out-of-pocket spending. Second, if healthcare use increases, extended Medicaid coverage may induce households to obtain additional care. In this case, the increase in the household budget would be smaller because some of the resources freed up by reduced out-of-pocket spending are spent on additional care. Third, if healthcare use falls, then extended postpartum coverage induces people to forgo care they would have previously paid for out-of-pocket. In this case, any increase in household budget may only reflect a reduction in out-of-pocket spending rather than an increase in resources available for reallocation.

Table 9 shows difference-in-differences estimates of extended coverage on measures of 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. Across all panels we fine no statistically significant impact of postpartum medicaid extension on postpartum checkup likelihood or not having a postpartum checkup due to insurance status. Moreover, as event study estimates in Figure A3 show, the estimates are noisy, and we cannot rule out null effects at the 95th percentile. However, we can rule out increases or decreases in postpartum checkups greater than approximately 6 percentage points, or 6 percent relative to the baseline. Overall, we do not find evidence of decrease in postpartum checkups, which suggests that we can rule out increases in the household budget arising simply because people receive less care under

extended postpartum coverage. Instead, there is likely some reduction in out-of-pocket spending, as evidenced by the stability in postpartum visits.

Next, we examine labor supply, another margin through which extended Medicaid coverage could affect household resources, either by improved health or because reduced medical spending risk may change the need or ability to work during the postpartum period. We examine whether the FFCRA affected labor market outcomes using the CPS ASEC sample. Panels A through E of Table 10 report difference-in-differences estimates for employment, part-time work, usual hours worked, parental leave, and part-time work due to child-care constraints. Overall, we find little evidence that extended postpartum Medicaid coverage changed labor supply. The estimates are mostly small and statistically insignificant across all outcomes. In ACA non-expansion states, we estimate a statistically significant increase in part-time work of 9.8 p.p., but we interpret this result cautiously. The event-study estimates in Figure A4, Panel B, do not show a consistent post-FFCRA trend in part-time work, and the other labor market outcomes show no clear pattern. Taken together these results provide evidence that the primary mechanism through which extended postpartum Medicaid affects the household budget is a reduction in out-of-pocket medical expenses rather than changes in labor decisions.[30]

V.6. Robustness Checks

Table 11 reports a broad set of robustness checks for the non-ACA sample under the policy-timing design.[31] Column (1) reports the baseline specification. We first examine whether the findings depend on how spending is measured or on the treatment of extreme values. Column (2) uses the inverse hyperbolic sine transformation, and column (4) winsorizes spending at the 0.5 and 99.5 percent levels rather than the 1 and 99 percent levels used in the baseline

[30]Appendix B presents supplementary evidence from the Survey of Income and Program Participation (SIPP), which includes monthly measures of Medicaid coverage, employment, food insecurity, and difficulty paying rent, mortgage, or utilities. The estimates show similar Medicaid coverage effects but little evidence of changes in employment or household hardship.

[31]Except for columns (2) and (3), all outcomes are measured in dollar levels and are therefore most directly comparable to the baseline estimates in column (1).

specification. In both cases, estimates remain robust to these alternative decisions.

Because the sample includes pre-birth periods, baby-care spending can be zero in some months. Column (3) reports estimates from a Poisson (PPML) specification for baby-care purchases as 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 (5) clusters standard errors at the county level rather than the state level. Column (6) 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 (7) 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 (8) imposes a balanced event window of nine months before and nine months after the policy date. Column (9) excludes March 2020 as a check of whether the results are sensitive to unusually distorted spending behavior at the onset of the pandemic.

Table 11 shows that the robustness results closely mirror the main estimates. Across the alternative specifications, the sign, magnitude, and overall significance pattern remain similar. The estimated effects for total purchases, baby-care purchases, and grocery purchases remain positive throughout.

A concern in our setting is that other COVID-era transfer programs coincide with the FFCRA implementation and may differentially affect low-income households.[32] The first round of Economic Impact Payments (EIPs) began in April 2020, which overlaps with

[32]We 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 confound our estimates.

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 A5. 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 stimulus checks.[33]

Across specifications, the main conclusion is unchanged. The estimated effects remain positive for total purchases, baby-care purchases, and groceries, and the magnitudes decrease 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. Overall, the evidence is consistent with our main findings and suggests that short-run stimulus-related dynamics do not materially alter the results.

[33]The 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.

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 increases Medicaid retention for postpartum women by around 16.3 percentage points or 65 percent, especially in ACA non-expansion 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 increases household spending by around $105 among affected families with newborns. The effects are concentrated in ACA 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 increases 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 for families with newborns, we find a similar pattern in ACA expansion and non-expansion states, 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

$59 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, we examine the mechanisms that may be driving the increase in household budget. Using PRAMS data, we show evidence that women increase postpartum visits by a small amount suggesting some realized reduction in out-of-pocket spending. Then, using CPS ASEC data, we find little evidence that the coverage extension systematically changed postpartum labor market outcomes. Therefore the increase in household budget is instead tied to a reduction in out-of-pocket expenses rather than labor changes.

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 healthcare 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 ACA non-expansion states, where postpartum coverage losses would otherwise be sharper. We note, however, that our time period is during the COVID-19 pandemic, a period of notable shortages at grocery stores. Given the potential differential consumer

responses during this time, future work directly studying the twelve-month extensions will help contextualize the generalizability of our results.

Overall, we show that extending postpartum Medicaid coverage changed household spending behavior in meaningful ways. The FFCRA increases 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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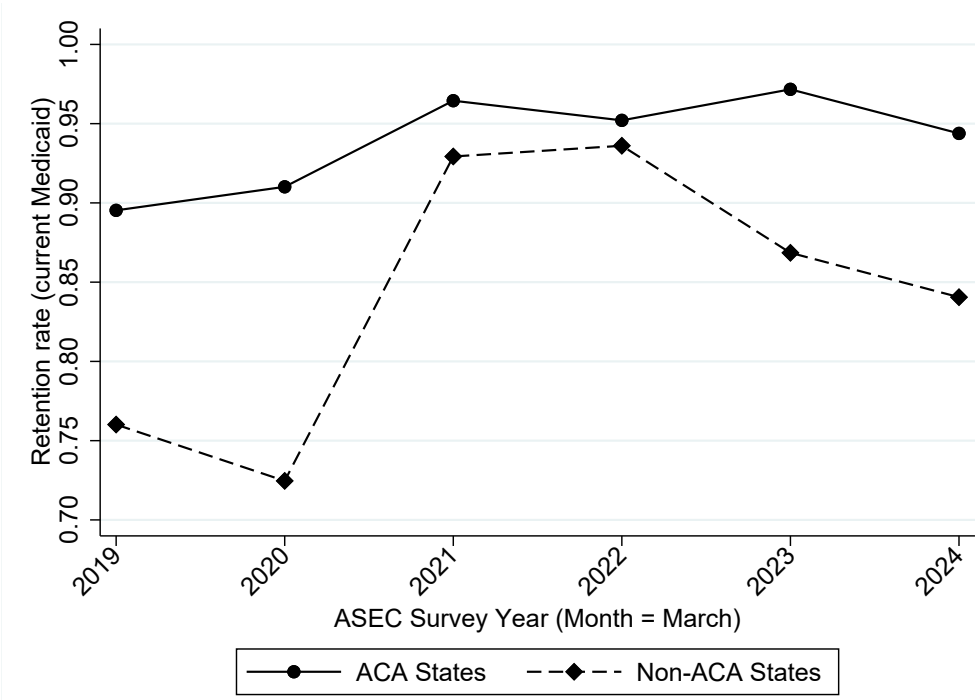
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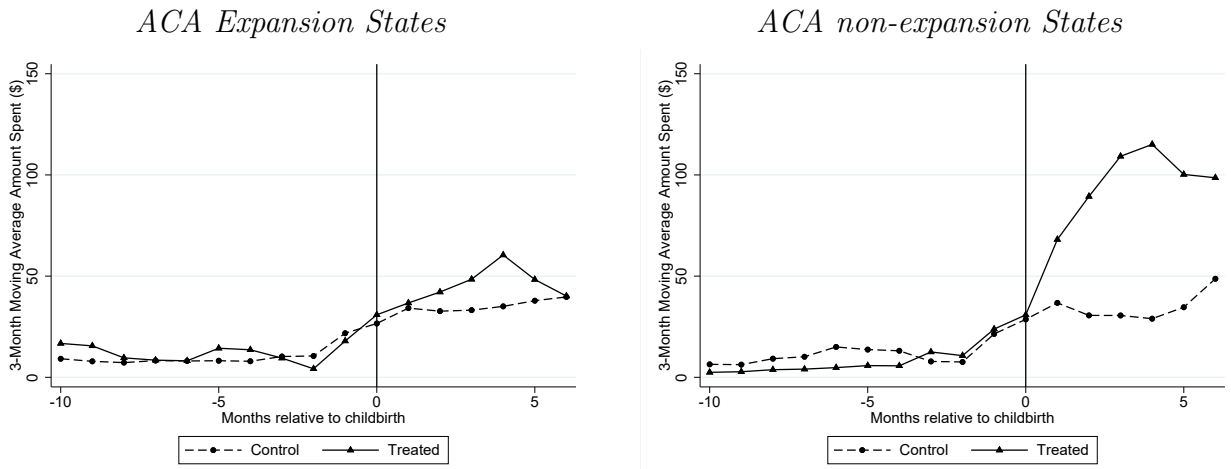
Figures and Tables

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 ACA non-expansion states. FFCRA began in 2020 and unwinding began in 2023.

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

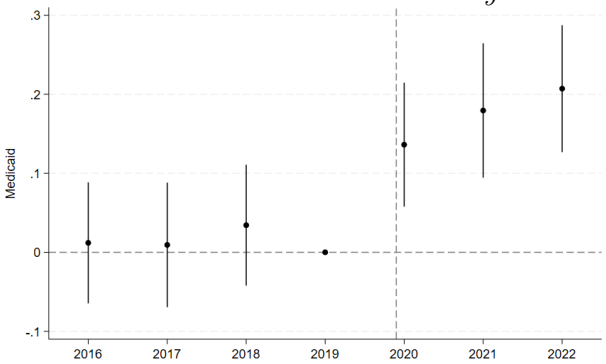


Notes: Data are from the NielsenIQ Consumer Panel, 2019-2020. The figure shows the three-month moving average of baby-care spending around childbirth (month 0), separately by ACA expansion status and treatment group.

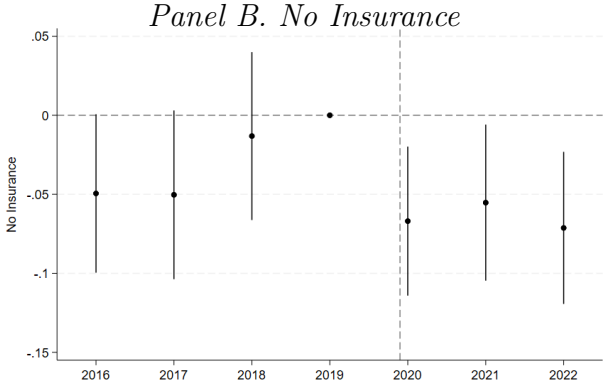
Figure 3: Event Study Estimates of FFCRA Effects on Health Insurance

Policy Effect: Comparing Pre- and Post-FFCRA

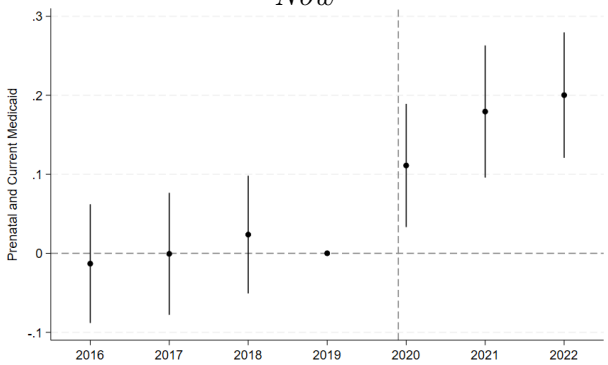
Panel A. Medicaid Currently



Panel B. No Insurance



Panel C. Medicaid During Pregnancy and Now

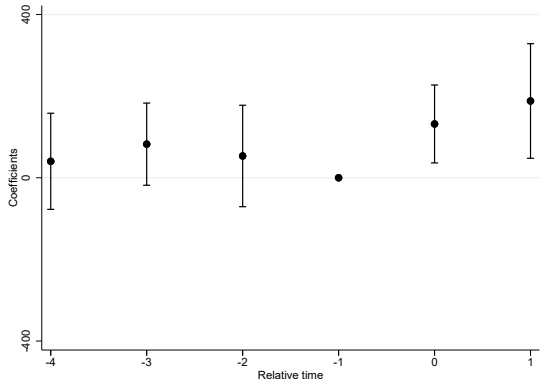


Notes: Data source is Illinois and Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2016-2022. Each figure reports pooled survey-weighted regression estimates for mothers with an infant born in the last year. Treatment is equal to 1 if the entirety of the household income is below the state's 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.

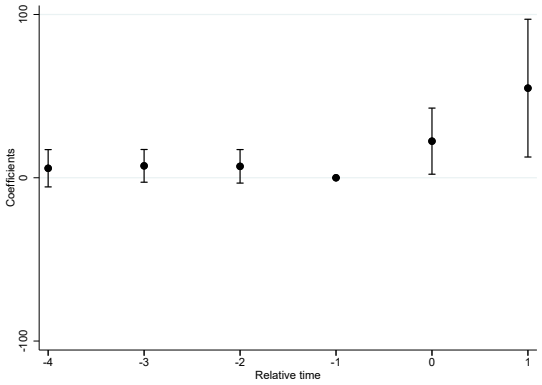
Figure 4: Event Study Estimates of FFCRA Effects on Spending Among ACA non-expansion States

Policy Effect: Comparing Pre- and Post-FFCRA

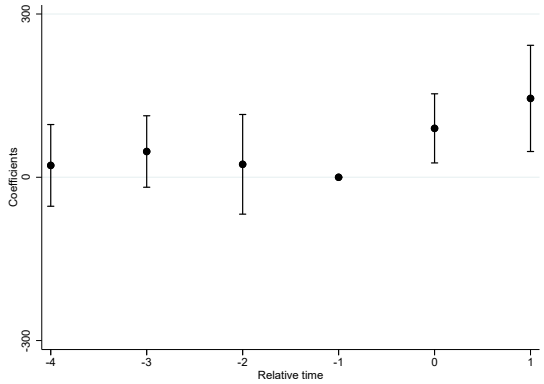
Panel A. All purchases



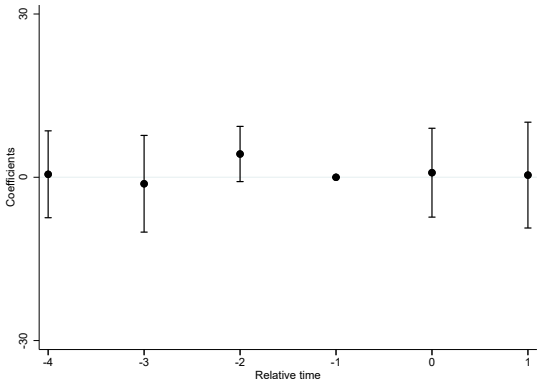
Panel B. Baby-care purchases



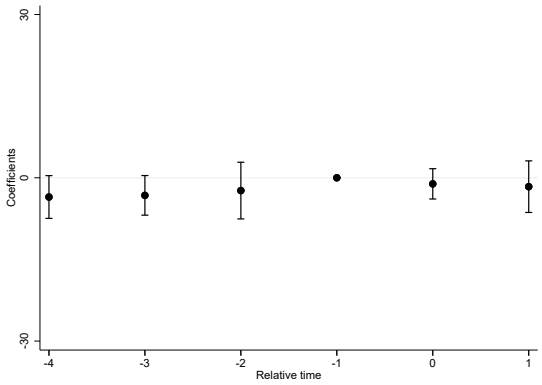
Panel C. Grocery purchases



Panel D. Alcohol purchases



Panel E. OTC medication purchases

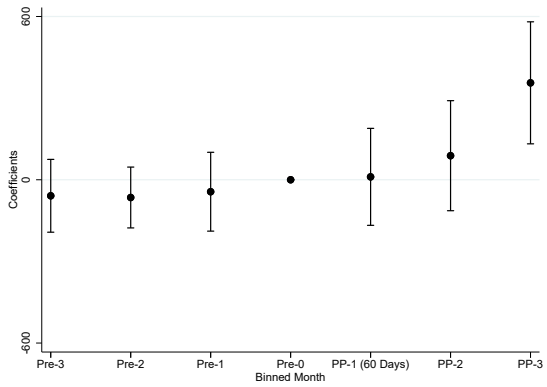


Notes: Data source is NielsenIQ Consumer Panel, 2019-2020. Event time is grouped into four-month intervals relative to FFCRA implementation. Vertical bars indicate 90% confidence intervals. Standard errors are clustered at the state level. All regressions include household and state-by-month fixed effects.

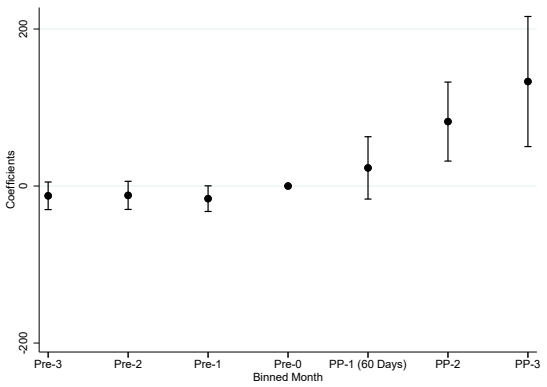
Figure 5: Event Study Estimates of FFCRA Effects on Spending Among ACA non-expansion States

Life Cycle: Comparing Before and After Childbirth

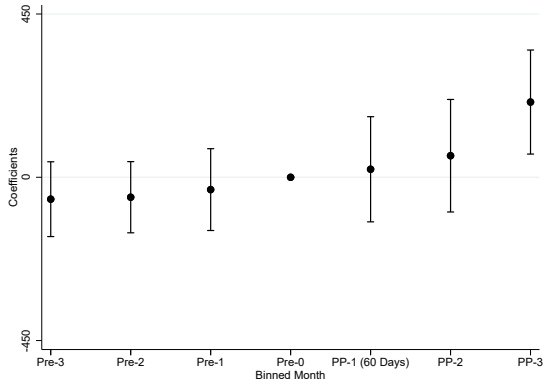
Panel A. All purchases



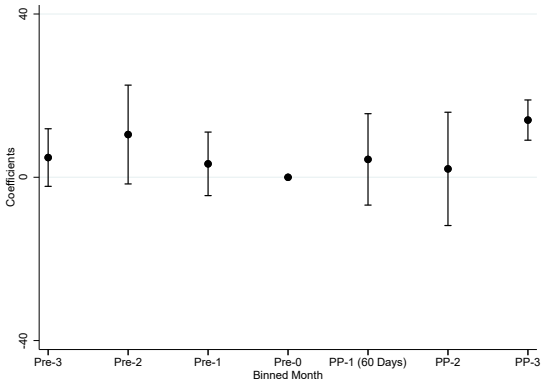
Panel B. Baby-care purchases



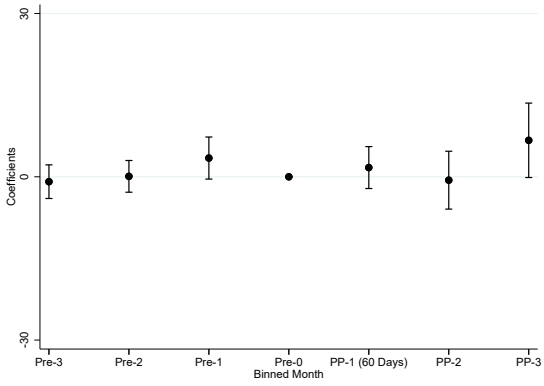
Panel C. Grocery purchases



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	Non-ACA	ACA
Medicaid previous yr (=1)	0.203	0.221	0.193
	(0.402)	(0.415)	(0.395)
Now Medicaid (=1)	0.179	0.184	0.177
	(0.384)	(0.387)	(0.382)
Medicaid Retention (=1)	0.869	0.813	0.901
	(0.338)	(0.390)	(0.299)
Age	30.801	30.080	31.171
	(5.343)	(5.399)	(5.277)
Married (=1)	0.795	0.749	0.818
	(0.404)	(0.434)	(0.386)
Hispanic (=1)	0.189	0.218	0.173
	(0.391)	(0.413)	(0.378)
White (=1)	0.816	0.817	0.815
	(0.388)	(0.387)	(0.388)
Employed (=1)	0.620	0.573	0.644
	(0.485)	(0.495)	(0.479)
Some college (=1)	0.746	0.704	0.767
	(0.435)	(0.457)	(0.422)
Part-time work (=1)	0.169	0.157	0.175
	(0.375)	(0.364)	(0.380)
Usual worked hours	22.198	20.741	22.950
	(19.621)	(19.905)	(19.432)
Parental leave (=1)	0.087	0.068	0.096
	(0.281)	(0.251)	(0.295)
Part-time b.c. child care (=1)	0.037	0.032	0.039
	(0.188)	(0.177)	(0.193)
Income higher bound ($1,000)	77.597	71.160	80.902
	(26.515)	(29.353)	(24.281)
Income lower bound ($1,000)	60.943	56.112	64.048
	(22.541)	(23.910)	(21.043)
N	8,221	2,789	5,432

Notes: Data source is the 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 a newborn. Medicaid Retention is defined as having Medicaid coverage conditional on having Medicaid coverage in the prior calendar year. "Not ACA" refers to states that had not adopted ACA Medicaid expansion.

Table 2: Summary Statistics - Pregnancy Risk Assessment Monitoring Survey

	Below Pregnancy Eligibility		Above Pregnancy Eligibility	
	Mean	Std. dev.	Mean	Std. dev.
	(1)		(2)	
Medicaid	0.395	0.489	0.175	0.380
No Insurance	0.104	0.305	0.054	0.225
Prenatal and Current Medicaid	0.341	0.474	0.151	0.358
Postpartum Checkup	0.929	0.256	0.956	0.205
No PP Checkup due to Ins	0.012	0.111	0.007	0.083
Observations	2327		3740	

Notes: Data source is Illinois and Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2016-2022. Descriptive statistics include the pooled means and standard deviations for IL and WI for the listed outcomes before any treatment from 2016-2019. 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 ($)	498.87	485.17	567.82	514.87	490.05
	(276.53)	(267.12)	(310.94)	(292.60)	(266.92)
Baby-care spending ($)	15.01	14.07	19.70	14.65	15.20
	(35.63)	(33.76)	(43.56)	(35.84)	(35.51)
Grocery spending ($)	381.15	366.99	452.43	391.66	375.36
	(213.54)	(201.19)	(255.73)	(227.98)	(204.97)
Alcohol spending ($)	9.05	9.91	4.75	7.86	9.71
	(22.71)	(24.06)	(13.23)	(18.90)	(24.54)
Health spending ($)	5.85	6.09	4.66	5.94	5.81
	(10.39)	(10.76)	(8.14)	(10.20)	(10.49)
Household size	3.28	3.17	3.84	3.20	3.32
	(1.25)	(1.17)	(1.47)	(1.37)	(1.18)
Single family (=1)	0.86	0.88	0.72	0.84	0.86
	(0.35)	(0.32)	(0.45)	(0.36)	(0.34)
White (=1)	0.78	0.79	0.72	0.79	0.77
	(0.42)	(0.41)	(0.45)	(0.41)	(0.42)
Black (=1)	0.07	0.07	0.09	0.11	0.05
	(0.26)	(0.25)	(0.28)	(0.32)	(0.22)
No. children under 18	1.11	1.02	1.56	0.94	1.20
	(1.08)	(0.99)	(1.35)	(1.05)	(1.08)
Any HH head employed	0.94	0.97	0.83	0.94	0.95
	(0.23)	(0.18)	(0.38)	(0.24)	(0.23)
Any HH head hsch	0.95	0.96	0.89	0.97	0.94
	(0.21)	(0.18)	(0.31)	(0.17)	(0.23)
HH head age > 30	0.61	0.58	0.76	0.64	0.59
	(0.49)	(0.49)	(0.43)	(0.48)	(0.49)
Married	0.89	0.90	0.84	0.88	0.89
	(0.32)	(0.31)	(0.37)	(0.33)	(0.31)
Inc lower bound ($1,000)	71.62	78.72	35.92	66.31	74.55
	(23.99)	(18.94)	(11.73)	(22.53)	(24.26)
Inc higher bound ($1,000)	75.76	86.86	42.48	74.48	76.67
	(26.51)	(18.96)	(15.92)	(26.11)	(26.76)
N	3,276	2,733	543	1,164	2,112

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: DID Estimates of FFCRA Effects on Insurance Status

Policy Effect: Comparing Pre- and Post-FFCRA

	(1) PP Med.	(2) Retained	(3) No Insurance
Panel A. Pooled			
FFCRA	0.161***	0.163***	-0.036***
	(0.022)	(0.022)	(0.012)
Mean	0.284	0.250	0.059
Observations	10203	10210	10202
Panel B. Wisconsin Only			
FFCRA	0.211***	0.195***	-0.010
	(0.032)	(0.031)	(0.019)
Mean	0.353	0.306	0.074
Observations	4344	4345	4343
Panel C. Illinois Only			
FFCRA	0.109***	0.123***	-0.056***
	(0.032)	(0.032)	(0.016)
Mean	0.234	0.209	0.048
Observations	5859	5865	5859

Notes: Data source is Illinois and Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2016-2022. 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, and no insurance currently. 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 5: Effects of FFCRA on Postpartum Medicaid Retention

	(1) Pooled Sample	(2) ACA	(3) Non-ACA
Post	0.131***	0.081**	0.189***
	(0.035)	(0.037)	(0.044)
Observations	1,275	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 6: DID Estimates of FFCRA Effects on Spending

Policy Effect: Comparing Pre- and Post-FFCRA

	(1) All	(2) ACA	(3) Non-ACA
Panel A. All Purchases	31.872	17.298	105.310**
	(29.327)	(33.208)	(46.730)
Mean	484.755	484.562	485.112
Within R-sq.	0.021	0.029	0.034
Panel B. Baby-Care Purchases	8.669	-4.832	32.142*
	(10.660)	(8.499)	(15.969)
Mean	9.560	9.951	8.838
Within R-sq.	0.025	0.017	0.132
Panel C. Grocery Purchases	21.166	4.822	91.441**
	(27.174)	(31.649)	(33.040)
Mean	358.228	361.272	352.608
Within R-sq.	0.024	0.035	0.033
Panel D. Alcohol Purchases	-1.270	1.186	-1.936
	(1.887)	(2.531)	(2.921)
Mean	9.851	10.828	8.046
Within R-sq.	0.011	0.024	0.001
Panel E. OTC Medication Purchases	-0.635	-1.097	0.367
	(0.810)	(1.190)	(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 × 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 × 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
Panel A. All Purchases	34.690	36.184	102.909
	(40.513)	(49.225)	(67.756)
Mean	476.725	476.316	477.445
Within R-sq.	0.025	0.037	0.029
Panel B. Baby-Care Purchases	13.007	-5.577	59.110**
	(15.520)	(11.594)	(25.113)
Mean	9.183	9.058	9.402
Within R-sq.	0.050	0.040	0.187
Panel C. Grocery Purchases	22.689	19.230	87.765*
	(35.712)	(42.885)	(45.658)
Mean	357.709	361.132	351.680
Within R-sq.	0.028	0.045	0.028
Panel D. Alcohol Purchases	-0.496	1.550	-0.338
	(2.676)	(3.595)	(4.964)
Mean	9.350	9.866	8.441
Within R-sq.	0.011	0.024	0.001
Panel E. OTC Medication Purchases	-0.526	-1.109	1.071
	(1.170)	(1.397)	(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 × 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 × 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
Post × Treated	105.310**	32.142*	91.441**
	(46.730)	(15.969)	(33.040)
Post × Treated × College	43.497	60.571***	50.636
	(154.613)	(12.923)	(114.070)
Post × Treated × Above 30	48.419	-34.703	-21.982
	(91.926)	(26.069)	(62.034)
Post × Treated × Lower income	-28.780	81.817***	-14.925
	(128.438)	(10.009)	(96.484)
Post × Treated × White	25.599	-20.697	-48.024
	(73.723)	(36.277)	(87.700)
Observations	1,164	1,164	1,164

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 ACA non-expansion sample. The outcomes are spending in levels for all purchases, baby-care purchases, and grocery purchases. Each specification includes the main DID term, *Post* × *Treated*, and its interaction with the indicated household characteristic: female household head with a college degree, female household head older than age 30, White household, or low-income household. For the income analysis, *Lower income* equals one if a household's income bin is below the median income bin among treated households in the same state. 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: DID Estimates of FFCRA Effects on Healthcare Utilization

Policy Effect: Comparing Pre- and Post-FFCRA

	PP Checkup	No Chk-up bc no ins.
Panel A. Pooled		
FFCRA	0.001	0.003
	(0.012)	(0.004)
Mean	0.941	0.007
Observations	9864	10210
Panel B. Wisconsin Only		
FFCRA	-0.014	-0.005
	(0.018)	(0.004)
Mean	0.926	0.010
Observations	4062	4345
Panel C. Illinois Only		
FFCRA	0.014	0.008
	(0.017)	(0.006)
Mean	0.951	0.005
Observations	5802	5865

Notes: Data source is Illinois and Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2016-2022.. Each column reports survey-weighted regression estimates for mothers with an infant born in the last year. Outcomes are postpartum check-up and did not attend postpartum check up due to not having insurance. 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 10: DID Estimates of FFCRA Effects on Labor Market Outcomes
Policy Effect: Comparing Pre- and Post-Treatment

	(1) All	(2) ACA	(3) Non-ACA
Panel A. Employed	0.011	-0.011	0.018
	(0.045)	(0.063)	(0.048)
Mean	0.703	0.728	0.651
R-sq.	0.175	0.167	0.205
Panel B. Part-time Work	0.041	0.003	0.098***
	(0.025)	(0.031)	(0.027)
Mean	0.170	0.174	0.162
R-sq.	0.040	0.045	0.071
Panel C. Usual Hours Worked	-0.747	-1.331	-0.938
	(1.668)	(2.431)	(1.609)
Mean	25.655	26.557	23.818
R-sq.	0.192	0.183	0.224
Panel D. Parental Leave	0.009	0.023	-0.018
	(0.019)	(0.026)	(0.031)
Mean	0.106	0.122	0.074
R-sq.	0.042	0.052	0.063
Panel E. Part-time b.c. Child Care	0.009	0.013	0.006
	(0.013)	(0.015)	(0.021)
Mean	0.037	0.038	0.034
Observations	7,294	4,844	2,450
R-sq.	0.031	0.040	0.042
Observations	7,294	4,844	2,450
Controls	X	X	X
State FE	X	X	X
Year FE	X	X	X
Cluster level	State	State	State

Notes: Data source is CPS ASEC from 2019-2023. The table reports DID estimates of FFCRA's effects on postpartum mothers' labor market outcomes. Each coefficient is from a separate regression of the listed outcome on Post × Treated. Columns labeled ACA and Non-ACA split the sample by state ACA Medicaid expansion status. All regressions include controls, state fixed effects, and year 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.

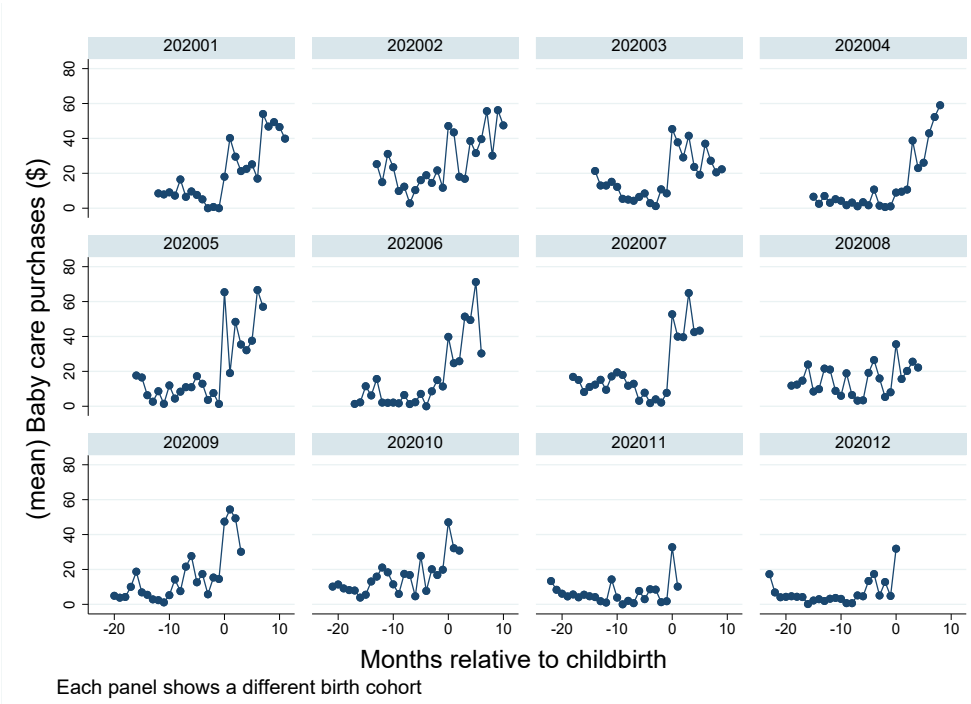
Table 11: Robustness Checks (Non-ACA Sample)

	(1) Baseline	(2) asinh(Y)	(3) PPML	(4) Wins.	(5) County SE	(6) 18–44	(7) Switchers	(8) ±9 mon	(9) Drop Mar.
Panel A. All Purchases									
Post × Treated	105.310**	0.192*	.	105.695**	105.310*	109.221**	91.853*	102.181*	96.970**
	(46.730)	(0.096)	.	(47.319)	(60.948)	(48.466)	(44.742)	(55.483)	(43.457)
Observations	1164	1164	.	1164	1164	1023	1289	880	1114
Mean (Pre, Control)	485.112	6.681	.	485.313	485.112	453.156	484.196	488.022	482.567
Within R-sq.	0.034	0.031	.	0.034	0.034	0.048	0.029	0.038	0.033
Panel B. Baby-care Purchases									
Post × Treated	32.142**	0.944**	1.826***	41.157*	32.142**	28.070*	27.411	32.543*	30.891*
	(15.969)	(0.398)	(0.680)	(22.101)	(15.305)	(14.129)	(15.791)	(16.089)	(15.319)
Observations	1164	1164	956	1164	1164	1023	1289	880	1114
Mean (Pre, Control)	8.838	1.068	11.428	9.202	8.838	9.795	8.503	8.862	7.662
Within R-sq.	0.132	0.048	.	0.138	0.132	0.099	0.089	0.108	0.136
Panel C. Grocery Purchases									
Post × Treated	91.441***	0.164	.	92.976**	91.441*	91.990**	81.963**	89.989**	86.324**
	(33.040)	(0.105)	.	(34.351)	(47.169)	(37.382)	(32.418)	(40.055)	(30.141)
Observations	1,164	1,164	.	1,164	1,164	1,023	1,289	880	1,114
Mean (Pre, Control)	352.608	6.340	.	352.741	352.608	339.556	354.091	360.160	348.304
Within R-sq.	0.033	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 the baseline DID estimates and robustness checks for the FFCRA's effects on household spending for the non-ACA sample, comparing pre- and post-FFCRA periods. Column (1) reports the baseline specification. Columns (2)–(9) report robustness checks. All outcomes are measured in dollar levels, except in column (2), which uses the inverse hyperbolic sine transformation. Column (3) reports a Poisson (PPML) specification for baby-care spending. Column (4) uses spending winsorized at the 0.5 and 99.5 percent levels. Column (5) clusters standard errors at the county level. Column (6) restricts the sample to households with any female aged 18–44. Column (7) keeps income-bin switchers. Column (8) uses a balanced event window (±9 months). Column (9) 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 A. Additional Figures and Tables

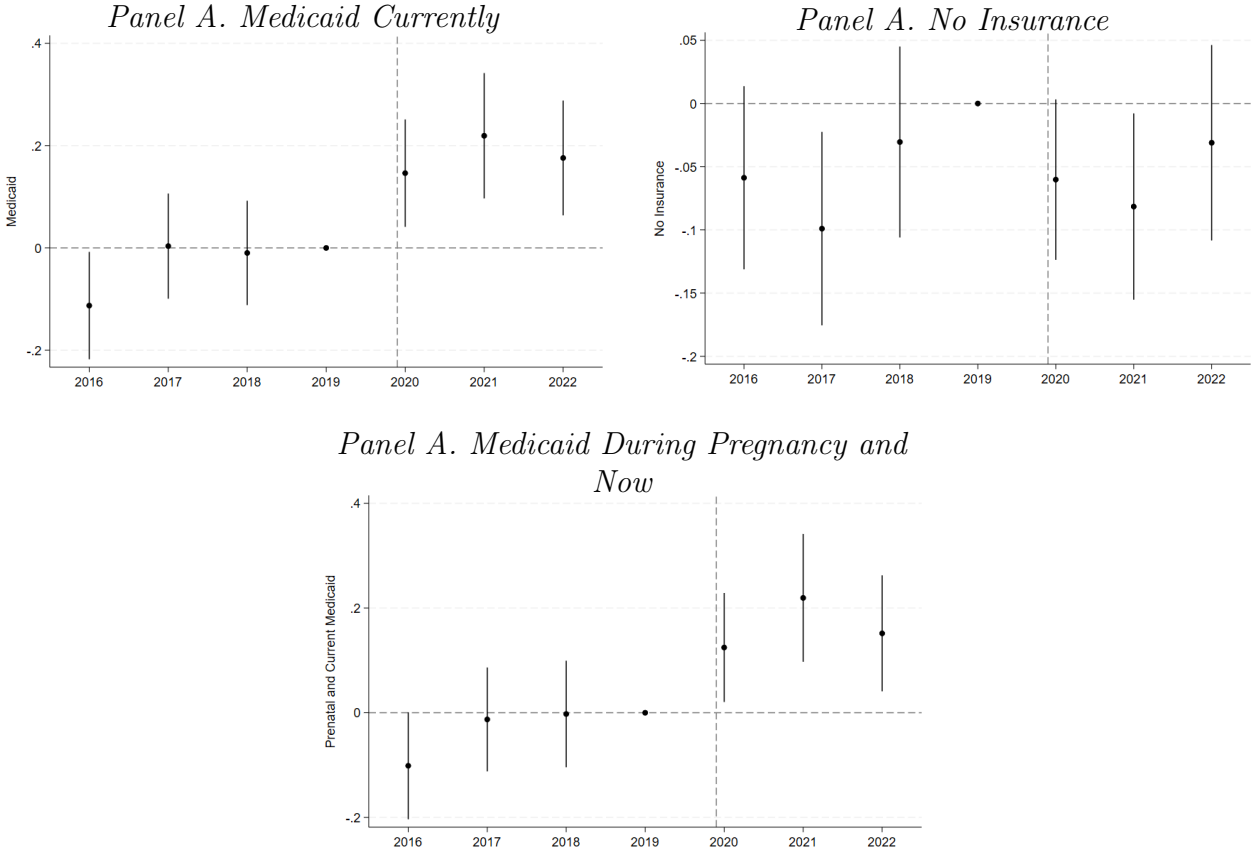
Figure A1: Average Monthly Baby-Care Purchases by Estimated Birth Cohort



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

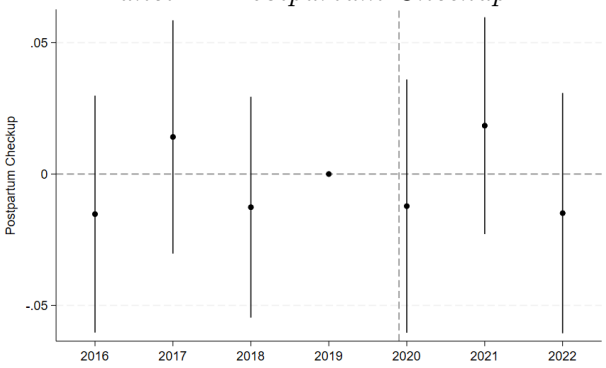


Notes: Data source is Wisconsin Pregnancy Risk Assessment Monitoring Survey from 2016-2022. Each figure reports pooled survey-weighted regression estimates for mothers with an infant born in the last year. Treatment is equal to 1 if the entirety fo the household income is below the state's pregnancy 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.

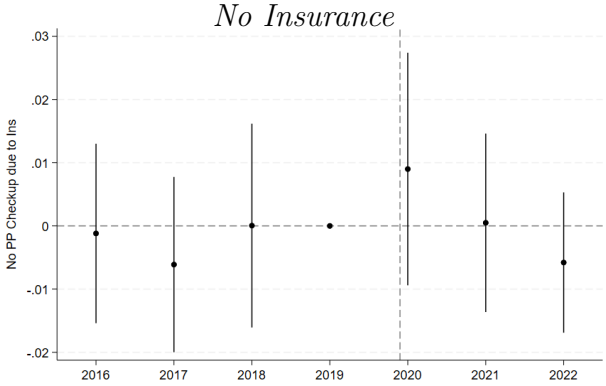
Figure A3: Event Study Estimates of FFCRA Effects on Healthcare in Wisconsin and Illinois

Policy Effect: Comparing Pre- and Post-FFCRA

Panel A. Postpartum Checkup



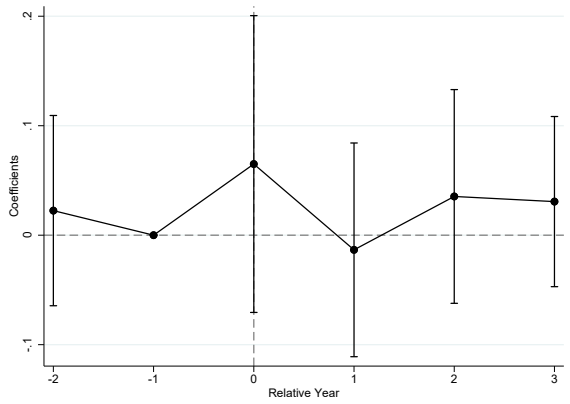
Panel B. No Postpartum Checkup because No Insurance



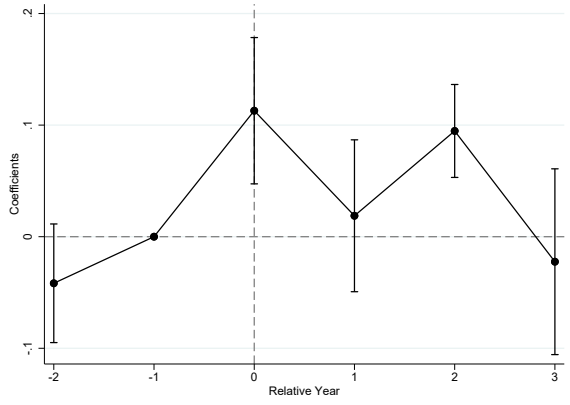
Notes: Data source is the Wisconsin and Illinois Pregnancy Risk Assessment Monitoring Survey from 2016-2022. Each figure reports survey-weighted regression estimates for mothers with an infant born in the last year. Treatment is equal to 1 if the entirety fo the household income is below the state's pregnancy eligibility cutoff after 2021. Outcomes are indicators for 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.

Figure A4: Event Study Estimates of FFCRA Effects on Labor Market Outcomes Among ACA non-expansion States

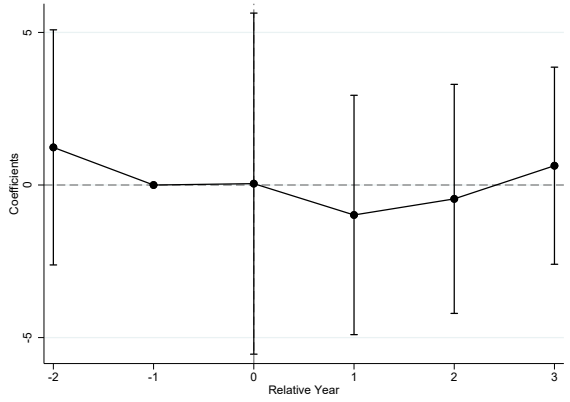
Panel A. Employed



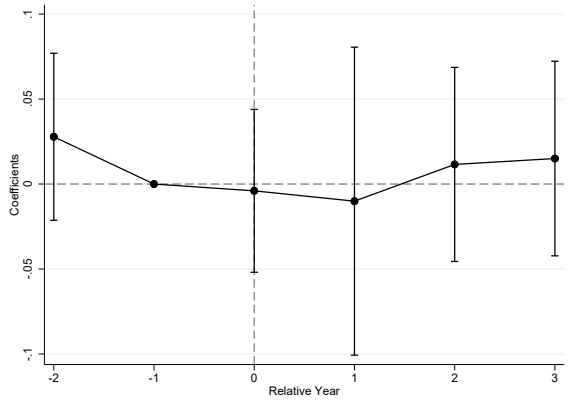
Panel B. Part-Time Work



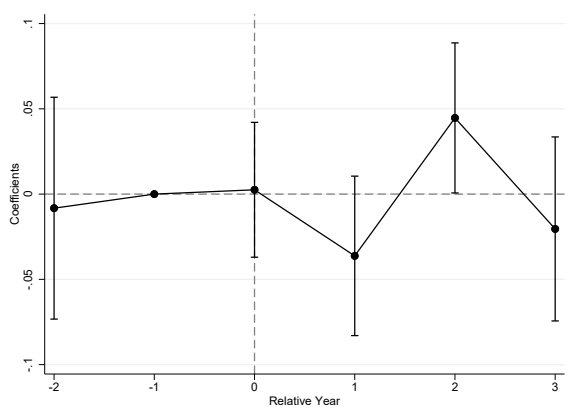
Panel C. Usual Hours Worked



Panel D. Parental Leave



Panel E. Part-Time b.c. Child Care



Notes: Data source is CPS ASEC from 2019-2023. This figure plots event-study estimates of the FFCRA's effects on labor market outcomes among mothers with a newborn in ACA non-expansion states. The omitted reference year is 2020. All regressions include demographic controls, state fixed effects, and year fixed effects. Standard errors are clustered at the state level. Vertical bars report 95 percent 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: Medicaid Eligibility Thresholds by State, 2020

State	Adult	Parent	Pregnancy	State	Adult	Parent	Pregnancy
Alaska	138	138	205	New York	138	138	223
Arizona	138	138	161	North Dakota	138	138	162
Arkansas	138	138	214	Ohio	138	138	205
California	138	138	322	Oregon	138	138	190
Colorado	138	138	265	Pennsylvania	138	138	220
Connecticut	138	160	263	Rhode Island	138	138	258
Delaware	138	138	217	Utah	138	138	144
District of Columbia	215	221	324	Vermont	138	138	213
Hawaii	138	138	196	Virginia	138	138	205
Idaho	138	138	138	Washington	138	138	198
Illinois	138	138	213	West Virginia	138	138	305
Indiana	138	138	218	Wisconsin	100	100	306
Iowa	138	138	380	Alabama	–	18	146
Kentucky	138	138	200	Florida	–	31	196
Louisiana	138	138	214	Georgia	–	35	225
Maine	138	138	214	Kansas	–	38	171
Maryland	138	138	264	Mississippi	–	26	199
Massachusetts	138	138	205	Missouri	–	21	305
Michigan	138	138	200	Nebraska	–	63	202
Minnesota	138	138	283	North Carolina	–	41	201
Montana	138	138	162	Oklahoma	–	41	210
Nevada	138	138	165	South Carolina	–	67	199
New Hampshire	138	138	201	South Dakota	–	48	138
New Jersey	138	138	205	Tennessee	–	94	255
New Mexico	138	138	255	Texas	–	17	207
				Wyoming	–	53	159

Notes: Data are from the Kaiser Family Foundation (KFF), as of 2025. Cutoffs are expressed as a percent of the federal poverty level (FPL) and correspond to Medicaid eligibility thresholds for adults, parents, and pregnancy-related coverage.

Table A3: DID Estimates of FFCRA Effects on Spending

Policy Effect: Comparing Pre- and Post-FFCRA

	(1) All (Log)	(2) ACA (Log)	(3) Non-ACA (Log)
Panel A. All Purchases	0.021	0.005	0.191*
	(0.062)	(0.083)	(0.096)
Mean	5.999	6.003	5.991
Within R-sq.	0.020	0.027	0.031
Panel B. Baby-Care Purchases	0.082	-0.415	0.859**
	(0.399)	(0.425)	(0.359)
Mean	0.847	0.826	0.884
Within R-sq.	0.019	0.028	0.055
Panel C. Grocery Purchases	0.012	-0.012	0.165
	(0.074)	(0.107)	(0.104)
Mean	5.667	5.674	5.654
Within R-sq.	0.015	0.022	0.013
Panel D. Alcohol Purchases	-0.261*	-0.127	-0.270
	(0.138)	(0.158)	(0.277)
Mean	0.877	0.896	0.842
Within R-sq.	0.014	0.028	0.006
Panel E. OTC Medication Purchases	-0.079	-0.086	-0.057
	(0.100)	(0.148)	(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 × 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 × 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: DID Estimates of FFCRA Effects on Baby-Care Spending (Level Outcomes)

	(1) All	(2) ACA	(3) Non-ACA
Baby-Food Purchases	9.208	-0.168	24.323*
	(8.390)	(8.322)	(12.270)
Mean	6.703	7.441	5.339
Within R-sq.	0.028	0.020	0.105
Baby-Needs Purchases	-1.050	-3.268	3.866
	(2.109)	(2.654)	(2.161)
Mean	2.236	2.087	2.512
Within R-sq.	0.007	0.005	0.060
Observations	3,276	2,112	1,164
Household Controls	X	X	X
Household FE	X	X	X
State × 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 baby-food and baby-needs spending among newborn households. Outcomes are measured in dollar levels. Each coefficient is from a separate regression of the listed outcome on Post × Treated. Columns labeled ACA and ACA non-expansion 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 A5: Controlling for Stimulus Checks (Non-ACA Sample, Level Outcomes)

	(1)	(2)	(3)	(4)
Panel A. All Purchases				
Post × Treated	105.310**	87.261*	106.274**	82.674
	(46.730)	(43.951)	(47.011)	(56.095)
Panel B. Baby-care Purchases				
Post × Treated	32.142*	24.808	27.960	33.695*
	(15.969)	(13.974)	(15.479)	(16.841)
Panel C. Grocery Purchases				
Post × Treated	91.441**	78.390**	88.583**	82.223*
	(33.040)	(33.659)	(34.273)	(43.811)
Observations	1,164	1,164	1,067	820

Notes: Data are from the NielsenIQ Consumer Panel, 2019-2020. The table reports difference-in-differences estimates for ACA non-expansion states. 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.

Appendix B. Supplement Analysis from the SIPP

We also use the Survey of Income and Program Participation (SIPP) to examine postpartum Medicaid coverage, employment, and household financial hardship. The SIPP is a nationally representative panel survey that follows individuals over time and collects monthly information on program participation, health insurance coverage, employment, income, and household material hardship. These data allow us to observe monthly Medicaid coverage and labor-market outcomes during the postpartum period, as well as broader measures of household financial hardship.

We use SIPP mother-month observations from 2018 to 2023 for mothers linked to a newborn and restrict the sample to postpartum months 3 through 12. We define baseline household income using the average household income-to-poverty ratio during pregnancy months 8 to 3 before childbirth, avoiding income changes close to delivery, and exclude households with income-to-poverty ratios above 10. The treated group consists of mothers whose baseline household income is below the state-year pregnancy-related Medicaid eligibility cutoff but above the relevant regular Medicaid cutoff. In ACA expansion states, the regular Medicaid cutoff is the adult Medicaid cutoff; in ACA non-expansion states, it is the parent Medicaid cutoff. The control group consists of mothers whose baseline household income is above the pregnancy-related Medicaid cutoff and also above the regular Medicaid cutoff.

We estimate difference-in-differences models comparing treated and control mothers before and after FFCRA's initiation. The outcomes include Medicaid coverage, employment, full-month work, food insecurity, inability to pay rent or mortgage, and inability to pay utilities. Table B1 reports estimates for the full sample and separately for ACA expansion and ACA non-expansion states.

The SIPP results are broadly consistent with the main analysis. Medicaid coverage increases by 12.3 percentage points in the full sample, and the split-sample estimates are similar in magnitude to the Medicaid retention estimates reported in the main text, although they

are imprecise. The employment estimates are small and statistically insignificant, consistent with the null labor-market effects we find using the CPS ASEC in the mechanism analysis. We further examine household hardship outcomes, including food insecurity and difficulty paying rent, mortgage, or utilities, and find little evidence of changes along these margins. We interpret the SIPP results as supplementary because the sample of mothers linked to a newborn is small, which limits statistical power.

Table B1: SIPP Estimates of FFCRA Effects on Postpartum Outcomes

	Medicaid coverage	Any job during month	Full-month work	Food insecure	Unable rent/ mortgage	Unable utilities
Panel A. Full Sample						
Treated × Post	0.1225*	0.0415	0.0025	0.0185	0.0077	0.0025
	(0.0559)	(0.0813)	(0.0747)	(0.0647)	(0.0306)	(0.0326)
Mean	0.130	0.676	0.617	0.051	0.031	0.039
Observations	6,181	6,181	6,181	6,181	6,181	6,181
R-squared	0.302	0.137	0.122	0.145	0.071	0.063
Panel B. ACA Expansion States						
Treated × Post	0.0735	-0.0141	-0.0133	0.0410	0.0648	0.0135
	(0.0823)	(0.1089)	(0.1025)	(0.0697)	(0.0357)	(0.0485)
Mean	0.123	0.675	0.614	0.027	0.025	0.038
Observations	3,788	3,788	3,788	3,788	3,788	3,788
R-squared	0.298	0.145	0.127	0.141	0.081	0.063
Panel C. ACA non-expansion States						
Treated × Post	0.1625	0.1019	0.0154	0.0031	-0.0526	-0.0290
	(0.0762)	(0.1268)	(0.1229)	(0.1031)	(0.0494)	(0.0486)
Mean	0.141	0.676	0.621	0.098	0.043	0.040
Observations	2,393	2,393	2,393	2,393	2,393	2,393
R-squared	0.306	0.141	0.124	0.144	0.070	0.084

Notes: Data are from the Survey of Income and Program Participation, 2018–2023. The sample includes mother-month observations for mothers linked to a newborn and is restricted to postpartum months 3 through 12. Treated mothers have baseline household income below the state-year pregnancy-related Medicaid eligibility cutoff and above the relevant regular Medicaid cutoff. Baseline household income is measured using the average household income-to-poverty ratio during pregnancy months 8 to 3 before childbirth. Each coefficient is from a separate difference-in-differences regression of the listed outcome on Treated × Post. Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.