Does Cash Buy Credit?

Evidence from the Timing of Child-Related Tax Benefits *

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Abstract

Many families are financially fragile after the birth of a first child, but it is unclear whether cash transfers can have lasting effects on their financial well-being. I link California birth records to a panel of first-time parents' credit reports and leverage a discontinuity in child-related tax benefits that shifts their first receipt by about one year across otherwise similar families. Regression discontinuity estimates show that among financially vulnerable mothers, earlier receipt relaxes borrowing constraints and produces persistent gains: in the third year after the birth, debt in collections is 8% lower, credit limits are 4% higher, and credit card balances are 5% higher. I also find suggestive evidence that mortgage holding rises after the initial credit improvements and that the credit expansion extends to the family as a whole. Financially non-vulnerable mothers show no comparable response, suggesting that the timing of transfers has durable impacts where liquidity constraints bind.

Keywords: tax credit, new parents, liquidity constraint, credit access, financial well-being

JEL Codes: G51, H24, H31, I38

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1 Introduction

Childbirth is a common and major life event with profound implications for a household's finances. Parents face a sharp increase in spending needs for medical care and for the child's food, housing, and childcare. At the same time, mothers' earnings fall after birth, a pattern known as the "child penalty" (e.g., Kleven, Landais, and Søgaard, 2019). Survey evidence has documented this mismatch between the income stream and these needs, and public support provides only a partial buffer (Stanczyk, 2020; Hamilton et al., 2023b). As many first-time parents are young and have relatively limited savings or access to credit, they are likely to be liquidity constrained and face great financial pressures.

This concern has given rise to a policy proposal that has attracted increasing interest: providing timely cash transfers to parents of a newborn around the time of birth (e.g., Hamilton et al., 2023a).[1] Cash transfers can be particularly helpful for alleviating hardship when they arrive, but it is theoretically ambiguous whether they can change a family's financial trajectory. In a canonical buffer-stock model (Deaton, 1991; Carroll, Hall, and Zeldes, 1992; Carroll, 1997), for instance, a liquidity-constrained household consumes the transfer and then returns to its target buffer, and the gains would be transitory. On the other hand, financial well-being can be self-reinforcing: because a borrower's history is closely examined in the formal credit market, a temporary improvement in financial conditions can still raise their standing with lenders and enable them to borrow on better terms later (e.g., Brevoort, Grodzicki, and Hackmann, 2020). Under this scenario, well-timed liquidity support does change the trajectory and generates effects that outlast the cash transfer. Because an empirical examination of this question requires both variation in the provision of cash transfers and detailed longitudinal data on financial outcomes, there has been limited evidence, particularly among the population of new parents.

This paper provides such evidence. I link together two large-scale administrative datasets: California birth records between 2005 and 2023, which document all births in California and contain information on the parents, and the University of California Consumer Credit Panel (UC-CCP) between 2004 and 2025, which consists of quarterly credit report data for all California residents with a credit file. The linked dataset allows me to track each matched parent from approximately two years before to three years after the first birth, and observe their engagement with the formal credit market and their financial well-being at a very detailed resolution.

In my main analysis, I focus on the mothers of first-time births who have an established credit record

[1] In the US, for example, the American Family Act proposes an expanded, fully refundable Child Tax Credit (CTC) that is paid monthly, with a payment several times larger in the month of a child's birth. The Supporting Newborn Parents Act proposes a credit of up to $2,000 for each newborn, separate from the CTC and payable within weeks of the birth. There are also proposals to provide cash transfers during pregnancy, such as the Family Income Supplemental Credit Act.

before the pregnancy. As a proxy for how tightly liquidity constraints bind, I rely on each mother's baseline credit score, the summary measure that lenders often review to set the terms of credit. I separately examine mothers with a baseline score at or below 660, whom I call financially vulnerable, and mothers with a higher baseline score, whom I call financially non-vulnerable. The latter group is less likely to be liquidity constrained and thus serves as a natural benchmark.

For causal identification, I leverage the discontinuity introduced by the tax year cutoff at January 1 that substantively changes the eligibility for child-related tax benefits, the two largest of which are the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC). A family with a December birth can immediately claim the child on that year's tax return and receives its first benefits within a few months of the birth. A family with a January birth, however, can claim the child only on the return for the year that has just begun, and its first benefits arrive more than a year after the birth. Both families are eligible for these benefits for the same number of years over the child's life, and the variation introduced by the birth date represents a timing shift of about one year and at a point when liquidity support most likely matters. Within my analysis sample, I predict the first-year benefit to be approximately $3,800 (in 2025 dollars) for an average financially vulnerable mother, and $3,200 for a financially non-vulnerable mother.

My main empirical strategy is a regression discontinuity (RD) design that builds on this quasi-random variation and uses the child's exact date of birth relative to the January 1 cutoff as the running variable. To alleviate concerns related to birth timing manipulation and holiday effects, I follow earlier literature and implement a donut RD specification that excludes births closest to the cutoff and thus most subject to such influence. The identifying assumption is that potential outcomes evolve smoothly around January 1. I show that a wide range of demographic, birth, and baseline credit characteristics do not exhibit discontinuous jumps at the cutoff, which lends support to its validity, and the small number of imbalances are unlikely to impact my results.

Results show that earlier receipt produces financial gains for financially vulnerable mothers that persist over time. In the first year after the birth, when the earlier benefits arrive, December-side mothers hold about $60 less debt in collections than their January-side counterparts, their credit limits are $328 higher, and their credit card balances are $99 higher, though the last difference is not statistically significant. By the third year, they hold $60 (8% of the January-side mean) less debt in collections, their credit limits are $436 (4%) higher, and their credit card balances are $212 (5%) higher, all three statistically significant. Scaled by the mean predicted value of the benefits brought forward, each $1,000 received a year earlier translates into $16 less in collections, $115 more in credit limits, and $56 more in balances in the third year. These results are robust to a range of alternative RD specifications, to a stricter linkage requirement, to an alternative definition of financial vulnerability, and to changes in the tax regime over the sample period, and they pass a series of placebo tests. December-side financially non-vulnerable mothers, on the other hand, do not see a change in debt in collections or in credit limits, and instead pay down credit card balances.

Part of the credit expansion among the financially vulnerable is on the extensive margin. In the first year, December-side mothers are 1.3 percentage points more likely to apply for a new credit card and 0.9 percentage points more likely to hold any open credit card. This gap in card-holding remains at 1.2 percentage points in the third year, against a January-side mean of 72%, so January-side mothers do not fully catch up within this window. A bounding exercise suggests that most of the new borrowing capacity nonetheless accrues to mothers who already held a credit card.

Turning to other financial outcomes, I find that earlier receipt raises financially vulnerable mothers' credit scores by 2.4 points in the first year, a gain that later fades. Their auto loan holding is a marginally significant 1.1 percentage points higher in the first year, consistent with families using lump sum transfers for large-ticket purchases. Their mortgage holding does not move in the first year but is 1.0 percentage point (9%) higher in the second year and a marginally significant 0.7 percentage points (5%) higher in the third.

Taken together, these results are consistent with a dynamic process that produces persistent impacts: earlier receipt eases a mother's liquidity constraint and improves her credit profile in the first year, which relaxes her borrowing constraint as lenders extend more credit, and she draws on part of that new capacity. Extrapolating from the responses to quasi-random credit limit increases in Agarwal et al. (2018), a back-of-the-envelope calculation puts the additional purchases from this credit expansion channel at roughly $410 over the three years after the birth. I also find suggestive evidence that the credit expansion is not confined to the mother but extends to the father and to the two parents combined. However, I find no detectable effects on proxies for neighborhood quality, parental co-residence, or the parents' financial integration, so the financial gains may not have extended to these domains.

Nonetheless, these financial gains can carry broader welfare implications, as borrowing capacity can provide insurance value and smooth consumption against unexpected shocks (Braxton, Herkenhoff, and Phillips, 2024; Collier et al., 2025), particularly for households with limited liquid savings. Additionally, parents' access to credit can enable them to make investments in their children, and consistent with this channel, Braxton et al. (2025) document that it is associated with higher future earnings for children, conditional on parental income. Financial well-being may also produce spillover effects to other aspects of well-being that I do not observe, such as labor productivity (Kaur et al., 2025) and health (Dobbie and Song, 2015).[2]

[2]The relationship between financial well-being and health is debated, since some observed correlations may not be causal.

This paper makes three primary contributions. First, it directly contributes to work on the EITC's effect on household finances, which has tied the benefit to higher liquid savings (Jones and Michelmore, 2018) and to paying down unsecured and past-due debt (Shaefer, Song, and Williams Shanks, 2013; McGranahan, 2016; Jones and Michelmore, 2019), though its phase-out design can also distort savings (Weber, 2016). Most of this work relies on survey or aggregated data, and my main advance is to link micro-level administrative data from birth records and credit reports. This allows me to track each person's outcomes in the formal credit market at a granular level over time, and it is less subject to the measurement error and underreporting that affect self-reported financial measures (Zinman, 2009; Brown et al., 2015; Meyer, Mok, and Sullivan, 2015). I also contribute to the broader research on the effects of public policies on individuals' financial well-being.[3] Child-related tax benefits represent a context that differs along three key dimensions: they primarily serve the population of working families with children, they are delivered at an annual frequency through tax filing, and they come with distinct labor supply incentives, so it is plausible that their impacts would differ.

Second, this paper extends research on the design of transfer programs. Most work examines new enrollments or policy expansions that materially change the level of household resources or the risks families face, so the value of receiving a given benefit earlier has been hard to observe. The variation I exploit instead shifts the timing of child-related tax benefits by about one year among otherwise similar families; this operates predominantly through a liquidity channel, with a minor role for (lifetime) income effects.[4] Similar to factors like administrative barriers (Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019; Homonoff and Somerville, 2021; see Herd and Moynihan, 2025 for a review) and uncertainty about the transfer amount (Caldwell, Nelson, and Waldinger, 2023), timing of benefit delivery is a design feature that can carry welfare consequences of its own, in particular for families that cannot borrow against the benefit.

Finally, this paper contributes to the understanding of how tax policies that raise family resources can improve children's health, welfare, and human capital.[5] Because parents serve as intermediaries who

Kluender et al. (2025), for instance, find that medical debt relief does not improve self-reported health.

[3] This literature has studied health insurance (e.g., Gross and Notowidigdo, 2011; Finkelstein et al., 2012; Mazumder and Miller, 2016; Hu et al., 2018; Brevoort, Grodzicki, and Hackmann, 2020; Blascak and Mikhed, 2023; Bornstein and Indarte, 2025), food assistance (Homonoff, Lee, and Meckel, 2026), unemployment insurance (Hsu, Matsa, and Melzer, 2018), disability insurance (Deshpande, Gross, and Su, 2021), minimum wages (Aaronson, Agarwal, and French, 2012; Dettling and Hsu, 2021), and disaster loans (Collier et al., 2025).

[4] For children born in the 1980s and 1990s, Barr, Eggleston, and Smith (2022) bound the increase in the present value of lifetime family income from the first-year transfer at 0.2%, against an increase in first-year income of 10% to 20%. The gain is smaller still if the trajectory of pre-tax income over time does not respond to earlier receipt, because families on the December side eventually age out of eligibility one year earlier.

[5] Earlier research in this domain has examined birth weight (Hoynes, Miller, and Simon, 2015), maltreatment (Rittenhouse, 2025), educational attainment (Dahl and Lochner, 2012), and long-run labor market outcomes (Bastian and Michelmore, 2018; Cole, 2021; Barr, Eggleston, and Smith, 2022). Michelmore (2025) reviews the recent evidence from the US, the UK, and Canada.

determine how the transfer reaches the child, several of these studies hypothesize that transfer income relaxes the parents' liquidity constraints, which reduces financial strain and the adverse events it brings. This paper thus tests this channel directly by studying whether the transfer affects the mothers' finances. I also add to a related strand of work on parental outcomes, which finds reductions in crime (Bhardwaj, 2026) and improvements in maternal health (Evans and Garthwaite, 2014) that can in turn benefit children through what Michelmore (2025) calls the "family process." More broadly, my quasi-experimental setting based on child-related tax benefits complements evidence from randomized controlled trials of unconditional cash transfers, which have examined the consumption, balance sheets, parenting behavior, and well-being of low-income adults and new mothers (Gennetian et al., 2024; Bartik et al., 2025; Magnuson et al., 2025; Krause et al., 2026).

The rest of the paper proceeds as follows. Section 2 introduces background information about the child-related tax benefits at stake. Section 3 describes the data sources and sample construction process. Section 4 lays out the empirical strategy. Section 5 presents the results and the robustness checks, Section 6 further interprets the findings, and Section 7 concludes.

2 Policy Context

Families with children in the US are eligible for several child-related tax benefits. The two largest are the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC), and for most of my sample period, filers could also claim a personal exemption for each dependent. An unmarried parent additionally becomes eligible for the head of household filing status, and a family with childcare expenses can claim the Child and Dependent Care Credit (CDCC). California also introduced its own EITC, the CalEITC, in 2015 and a Young Child Tax Credit (YCTC) in 2019.

An important feature of the US tax code is that eligibility for these benefits is determined on an annual basis, and a child can be claimed starting from the year of birth. This implies that a family that gives birth to a child in December can still claim the child as a dependent for that tax year and receive these benefits at the next tax season, only a few months after the child's birth. However, a family that gives birth to a child a few weeks later in the next January can only claim the newborn from the tax year that has just begun, so its first child-related tax benefits arrive more than a year after the birth. Importantly, both families are still eligible for the benefits for the same number of years over the child's life.[6] This discontinuity around a January 1

[6]Several benefits limit eligibility by the child's age at the end of the tax year: under 19 for the EITC and the CalEITC (under 24 for a full-time student), under 17 for the CTC, and under 6 for the California YCTC. The CDCC instead covers care provided while the child is under 13.

birth date has motivated several earlier studies,[7] and I discuss the empirical specification in more detail in Section 4.

To get a sense of the amounts at stake, I use the NBER TAXSIM program (Feenberg and Coutts, 1993) and simulate the change in tax liability among hypothetical families in California from claiming a first child as a dependent, which I also refer to as the "tax value" of a first birth. Panels (a) and (b) of Figure A.1 plot the tax value in 2025 dollars against household earnings for tax year 2015, separately for a married couple filing jointly and an unmarried parent filing as single. The value peaks just above the Federal Poverty Level (FPL), at over $6,000 for the married couple and about $7,000 for the unmarried parent. It then declines as the EITC phases out but remains at a few thousand dollars well into the middle of the earnings distribution, and it is larger for the unmarried parent at most earnings levels.

The tax value has also changed over time. Panels (c) and (d) of Figure A.1 plot the tax value in each tax year from 2005 to 2022 for families with earnings fixed at 50%, 100%, 200%, and 300% of the FPL, again separately for the two filing statuses. The value is broadly stable in real terms through 2017. Successive policy changes have since raised it: the Tax Cuts and Jobs Act (TCJA) in 2018, the expansion of the CalEITC and the introduction of the YCTC in 2019, and the pandemic-era expansions.

3 Data and Sample

3.1 Data Sources

This paper primarily draws on two administrative data sources: California birth records between 2005 and 2023 from the California Department of Public Health (CDPH) and the University of California Consumer Credit Panel (UC-CCP) between 2004 and 2025 from the California Policy Lab (CPL).

California Birth Records The birth records cover the universe of births in California. Importantly, I observe the exact date of birth of each child, allowing me to implement the RD design, as well as each child's birth order, which lets me focus on first births. These data also contain detailed demographic information on the parents, including age, race and ethnicity, education, the mother's birthplace, and her ZIP code of residence, which I use to impute income and tax benefits. I also observe characteristics of the birth itself, including gestation length, birth weight, infant health at delivery, and delivery method (C-section or vaginal), which I use to test for manipulation of birth timing around the January 1 cutoff.

[7] Earlier studies have used this discontinuity to study labor supply (Wingender and LaLumia, 2017; Mortenson et al., 2018; Lippold and Luczywek, 2023; Goldin et al., 2024), children's outcomes (Cole, 2021; Barr, Eggleston, and Smith, 2022; Rittenhouse, 2025), and crime (Bhardwaj, 2026).

UC-CCP Data The UC-CCP is a quarterly panel of credit report information for California residents with a credit file, sourced from a major national credit bureau. I observe the snapshot of each individual's credit report, including account balances, delinquency status, bankruptcy records, and credit score, at the end of each calendar quarter. The account information covers both sole and jointly held accounts, including those where the individual is an associated borrower rather than the primary owner, so I capture each individual's full portfolio of credit obligations. I also observe each individual's ZIP code in every quarter of the sample period, and census block information starting from June 2010.[8] A unique advantage of the UC-CCP is that individuals remain in the dataset after they first enter, even if they have since migrated out of California.[9] Attrition is limited to occasional bureau merges or splits of erroneous profiles, and even individuals who stop using credit remain on file for an extended period of time, typically 7–10 years. This bypasses a common limitation of administrative data from a single state, where individuals who migrate out of this state would drop out of the sample, a concern that is especially relevant if the policy of interest might itself encourage moves.[10]

Record Linkage I link parents from the birth records and individual consumers in the UC-CCP by matching on their first and last names, date of birth, residential address, and ZIP code. For data privacy reasons, the CPL hashes the personally identifiable information (PII), and I never observe it in clear text. Because string distance measures cannot be computed on hashed values, the CPL additionally supplies hashed versions of the first few letters and of a phonetic encoding of each name and address field, and I match either on the full string or fuzzily on these alternative versions. The credit panel supplies linkable identifiers in the March files in each year, and I match every birth to the three closest years around it. Additional details of this exercise are available in Appendix C.1.

A match can fail for two distinct reasons. The parent may have no credit file at all, which is most likely for young adults who have never interacted with the formal credit market. Alternatively, the parent may have a credit file whose identifying fields do not agree closely enough with the birth record, for instance when the address given at the time of the birth differs from the credit bureau's version.

In my main analysis, I focus on mothers' financial outcomes, since maternal information is more complete in the birth records and yields higher-quality linkages. I later relax this and examine the father's and family-

[8]Census geographic variables are geo-coded from a person's address and are missing for approximately 20–25% of all observations. ZIP code, by contrast, is readily available in the credit data, since an address is provided whenever a person applies for a credit product.

[9]For anyone who lived in California at some point between 2004 and 2019, the panel also covers the periods before they first arrived in the state; those who first appeared after 2019 are observed only from that point on.

[10]Bastian and Black (2024) find that the EITC enables women to migrate out of rural and distressed areas, although not across states.

level outcomes for the subsample of births in which the father can also be linked. Overall, 69.6% of first-time mothers during my sample period match to an individual in the credit panel, which is comparable to match rates in other studies that link administrative records to credit data.[11]

3.2 Financial Outcomes

Prior studies find that families use tax benefits and similar lump-sum transfers to build savings and pay down unsecured debt (Shaefer, Song, and Williams Shanks, 2013; McGranahan, 2016; Jones and Michelmore, 2018, 2019), and to finance large-ticket purchases, such as vehicles (Barrow and McGranahan, 2000; Smeeding, Phillips, and O'Connor, 2000; Goodman-Bacon and McGranahan, 2008; Parker et al., 2013). These findings guide the four groups of financial outcomes I study: unpaid debt, credit access and utilization, overall financial health, and durable consumption. The UC-CCP provides detailed information on each account reported to the credit bureau, and I aggregate this information at the individual-by-quarter level to construct the outcomes in each group. Unless otherwise noted, I express all dollar values in December 2025 dollars and winsorize at the 99th percentile of positive values in the panel to reduce the impact of extreme observations.

Unpaid Debt For unpaid debt, I examine whether an individual has any active debt in collections, and the amount owed.[12] I also construct two delinquency indicators: whether any account is at least 30 days past due, and whether any account is at least 90 days past due, which proxy for mild and severe distress, respectively. By construction, these measures reflect a payment problem that has occurred with a delay: a delinquency flag reflects a payment that is 30 or 90 days late, and a collections record represents a further delay while the debt is turned over for collection and reported to the credit bureau. They therefore trail the initial missed payment by several months.

Credit Access and Utilization I measure credit access by a person's total credit limit across all open credit card accounts, which is the most common and flexible-to-use credit product available to households. I measure utilization by the total balance on all open credit cards at the end of the quarter, which captures both new purchases and revolving debt carried forward from earlier months. As additional measures of credit

[11] The setting in this paper best resembles studies that link administrative records to credit data, but without access to Social Security Numbers, including Finkelstein et al. (2012) (match rate 68.5%), Dobbie, Goldsmith-Pinkham, and Yang (2017) (68.9%), Miller, Wherry, and Foster (2023) (82.0% in the Near Limit group and 76.3% in the Turnaway group), Collinson et al. (2024) (61.3% in Cook County and 68% in New York), and Mello (2025) (82.3%). Whether first-time mothers should match at higher or lower rates is ex ante unclear: they are younger and less likely to hold an established credit file, but are not necessarily poorer than the disadvantaged populations in these studies, and so may be as well represented in credit data.

[12] Specifically, I focus on collections that are active within the past 12 months, as is commonly done in related work, because information on collections is infrequently updated and the stock may be out of date (Gibbs et al., 2025).

access, I also examine two extensive margins: whether the individual applies for a new credit card, which is recorded as an inquiry on the credit report, and whether they hold any open credit card account.

Overall Financial Health I examine an individual's credit score as a summary measure of their overall financial health and creditworthiness, which is also widely used by financial institutions and landlords to screen applicants and set the terms of credit. The UC-CCP reports a score similar to the FICO score. Scores of 661 or above are classified as prime or super-prime, while scores at or below 660 are classified as subprime or near-prime. Because a credit score is built from a person's prior credit market activity, they may not have a valid score if the credit history is not long enough, or if they do not have active credit accounts.[13]

Durable Consumption Finally, I proxy for durable consumption with two indicators: whether the individual carries a positive auto loan balance, and whether they carry a positive mortgage balance. Vehicles are a common large-ticket purchase shifted by lump sum transfers, and the arrival of a first child also changes a family's housing needs. At several thousand dollars, the benefit at stake is modest relative to a home price, but if it is enough to help a household clear a down payment barrier (Gupta, Hansman, and Mabille, 2025), or to improve their credit profile enough to secure better mortgage terms, earlier receipt could still move families toward homeownership.

For an individual with no account of a given type, I impute the corresponding balance or limit as zero. Because these outcomes are constructed from a person's credit report, they best capture the person's engagement with the formal credit market. Certain components of the household financial conditions, such as savings, cash purchases, and payday loans, are not reflected in the data.

3.3 Additional Outcomes

Beyond the financial outcomes of interest, I examine two additional families of outcomes. Because the credit panel supplies geographic identifiers at the census tract level, I can study measures of neighborhood quality. And for births where the father is also linked, I can study joint outcomes for the two parents, including family structure and financial integration between them. To avoid confusion, throughout the paper, "family-level" outcomes refer to the same two parents as named on the birth record, rather than to the mother's actual living arrangement at any given time.

Neighborhood Quality I map the census block recorded in each quarter onto 2010 census tracts and merge in publicly available tract-level measures from the Opportunity Atlas (Chetty et al., 2026). These

[13] Brevoort, Grimm, and Kambara (2016) estimate that in 2010, roughly 11% of US adults were credit invisible and not captured in the credit data, while another 8.3% were in the data but unscored. The UC-CCP's scoring model scores more consumers with thin or dormant credit files than conventional models do.

measures cover both the current characteristics of a tract, such as its median household income and poverty share, and estimates of the adult outcomes of children who grew up there, such as their household income and incarceration rate. Whether earlier benefit receipt moves families toward better neighborhoods is of particular interest because the neighborhood a child grows up in predicts their long-run outcomes, making residential moves one channel through which the timing of resources could reach the child.

Family Outcomes For births where both parents are linked to the credit panel, I examine three sets of family outcomes. The first is whether the two parents appear in the same ZIP code, the same census block, or the same credit bureau household in a given quarter,[14] three proxies for whether they live together. The second is whether the mother holds any credit account jointly with the father, a direct measure of financial integration between the parents. The third is the credit limit and credit card balance measured at the father level and at the family level.[15]

Both groups of outcomes are available only for subsets of the analysis sample: the neighborhood measures are limited to re-centered years 2012 through 2023 and require an observed census tract, while the family outcomes require that the father be linked. These restrictions cut the sample considerably, so the resulting estimates are less precise than those for the financial outcomes.

3.4 Sample Construction

I assign each birth to a "re-centered year", the calendar year of the January 1 closest to the date of birth, so that a late-December birth and an early-January birth a few days later share the same re-centered year. The two children are nearly the same age, but the December child can be claimed on the tax return for the year just ended while the January child cannot be claimed until a year later, so the December-side family receives its first child-related tax benefit roughly a year sooner.

I index the quarterly credit snapshots relative to this cutoff: $q = 0$ is the first snapshot after the relevant January 1, taken at the end of March, and $q = -1$ is the snapshot taken at the end of the preceding December, so the cutoff sits between $q = -1$ and $q = 0$. I also group quarters into years, so quarters 0–3 represent year 1 after the first birth, quarters 4–7 represent year 2, and quarters 8–11 represent year 3.

To build the analysis sample, I start from the birth records and keep first-time singleton births in re-centered years 2006–2023, and limit to births where the mother resided in California at the time of birth and

[14] In the UC-CCP, the credit bureau groups individuals into households based on address information. I observe the resulting household assignment but not the underlying address.

[15] For family-level outcomes, I sum over all of the two parents' applicable credit accounts, but count each credit account that is jointly held between the two parents only once.

delivered in a California birth facility.[16] I then turn to the UC-CCP and require each mother to match an individual in the credit panel, and that individual to be observed in every quarter from $q = -8$ to $q = +11$. The baseline period $q = -8$ is roughly two years before the birth and one year before conception, and the endpoint of $q = +11$ allows me to observe three years of each mother's credit history after her first birth. I also require a valid credit score at $q = -8$ so that the mother has an established baseline credit record. This lets me observe baseline financial outcomes.

A final restriction concerns the timing of the match. Because names and addresses can change around a birth, I attempt the linkage in three March snapshots of the credit panel: the year before the re-centered year ($q = -4$, roughly 9 months before the birth), the re-centered year itself ($q = 0$, roughly 3 months after), and the year after ($q = +4$, roughly 15 months after). I resolve each mother to a single individual when the snapshots yield more than one candidate.[17] A distinct concern is that the linkage itself could respond to the treatment: earlier benefit receipt may increase a mother's interactions with the formal credit market and keep her credit bureau profile more current, making her easier to match. To guard against this possibility, in a robustness check, I further restrict the sample to mothers who are matched in the first snapshot, which is taken before the January 1 cutoff, so that the linkage pre-dates any benefit receipt.

Because individuals rarely exit the credit panel once they enter, the credit score condition represents the most binding requirement and selects mothers who already have some baseline credit market engagement and for whom the credit data capture meaningful activity. Overall, the main analysis sample contains 50.3% of all first-time mothers, against a match rate of approximately 70%.[18]

3.5 Income and Tax Benefit Imputation

Neither the birth records nor the credit panel contains income or tax information. To fill this gap, I predict three variables for each family: earned income; after-tax income, measured before the first child is claimed; and the tax value of the first birth, the dollar amount of child-related tax benefits that a December birth brings forward by a year. Because all three predictions are functions of pre-determined demographics alone, they are unaffected by birth timing.

Following the approach of Rittenhouse (2025), I obtain all three imputed variables from prediction

[16] The location requirements match the coverage of the credit panel for California residents. The small number of out-of-state births is dropped to ensure the quality and consistency of birth information.

[17] I focus on those with a match in one of the first two snapshots. Because the linkage relies on the address recorded at the time of birth, the last snapshot is the most exposed to moves after the birth, and a match that appears only there is more likely to be a false positive. It still enters the resolution of multiple candidates, a procedure I describe in Appendix C.1.

[18] A natural complementary question is whether tax benefit access can draw otherwise credit invisible parents into the formal credit market and enable them to build up a credit profile. Unfortunately, this question lies beyond the scope of the current data, as the linkage is based on the address at the time of the birth.

models estimated on the American Community Survey (ACS), which contains the income, household composition, and residency information needed to simulate each household's tax liabilities. I fit the models on ACS households and apply them to the birth records through demographic characteristics common to both datasets.[19] Additional details of the implementation and validation tests are provided in Appendix C.2.

Predicted income also reveals who enters the linked sample. As Panel (a) of Figure A.2 shows, the share of mothers ever linked to the credit panel climbs from under half in the lowest predicted income bins to roughly 80% by $100,000, and above 90% at the top. The low-income mothers linked to the credit panel are therefore plausibly positively selected on credit market engagement, a point I return to when interpreting the results.

3.6 Financial Vulnerability Classification

An advantage of the credit panel is that I observe each mother's credit profile before her first birth, which lets me examine heterogeneity by baseline financial vulnerability. I split the sample by the baseline credit score at $q = -8$: mothers whose score is at or below 660 form the "financially vulnerable" group, and mothers whose score is above 660 form the "financially non-vulnerable" group. Scores at or below 660 fall in the subprime and near-prime tiers, which together make up the non-prime segment. The credit score aggregates many elements of a person's credit history into a single measure, and lenders rely on these ratings in deciding whether, and on what terms, to extend credit. Thus, financially vulnerable mothers are those who likely face the tightest borrowing constraints in the formal credit market.[20]

One limitation is that the credit score may contain algorithmic bias and misclassify borrowers. As a robustness check, I therefore also summarize each mother's financial vulnerability directly in a single index, in the spirit of Albanesi and Vamossy (2024) and Bakker et al. (2025): I predict the mother's probability of experiencing any 30-day delinquency during the first post-birth year (i.e., between $q = 0$ and $q = +3$). The prediction uses only her credit profile at the baseline $q = -8$ and her demographic characteristics, so the index, like the baseline credit score, pre-dates and is unaffected by any receipt of child-related tax benefits. Appendix C.3 details the construction of the index.

[19] The demographic characteristics are the age, race and ethnicity, and education of each parent, the mother's birthplace, and the child's birth order, each grouped into bins. The models also include fixed effects for the re-centered year and the Public Use Microdata Area (PUMA) of residence.

[20] Prior studies have targeted the households with the most at stake through income proxies (Barr, Eggleston, and Smith, 2022; Rittenhouse, 2025) or demographic characteristics (Cole, 2021; Bhardwaj, 2026). In my setting, the credit score is more informative of the financial outcomes I study: Panel (b) of Figure A.2 shows that predicted income is positively correlated with the credit score in my sample, but Figure A.3 shows that the score sorts realized delinquency more sharply than predicted income does, especially in the bottom deciles.

3.7 Summary Statistics

Table 1 presents summary statistics for all first-time births, the main analysis sample, and the financially vulnerable group whose baseline credit scores are at or below 660. Perhaps unsurprisingly, mothers in the analysis sample, who have a pre-existing credit profile, differ from mothers of all first-time births in several dimensions. Both parents are older, better educated (48% of the mothers have a college degree, versus 34%), and less likely to come from an ethnic minority background (5% of the mothers are non-Hispanic Black and 32% are Hispanic, versus 6% and 43%). The economic side points the same way: predicted earnings are more than a third higher, and the share of births paid for by Medicaid is about half as large (21% versus 39%).

By contrast, births in the financially vulnerable group stand apart within the analysis sample on the same dimensions. The parents tend to be younger and less educated (21% of mothers have a college degree, versus 48% in the analysis sample as a whole), and are more likely to be Black or Hispanic (9% and 47% of the mothers, versus 5% and 32%). The mothers' mean baseline credit score is 580, about 100 points below the sample average of 681 and well below the 660 threshold itself, and the split picks up genuine financial fragility: rates of delinquency and active collections are roughly 2.5 times those in the analysis sample overall. They also have far lower credit limits and higher utilization rates, pointing to tight borrowing constraints. Yet the group is not simply a proxy for low income: its mean predicted earned income is approximately \$85,000, and its Medicaid share, at 38.50%, is similar to that among all first-time births.

4 Empirical Strategy

4.1 Regression Discontinuity Specification

For each outcome of interest, I implement an RD regression based on a first child's exact date of birth relative to the January 1 tax year cutoff, as specified in Equation 1:

$$Y_i = \alpha + \tau D_i + \beta_1 d_i + \beta_2 D_i \times d_i + \mathbf{X}_i'\gamma + \theta_{c(i)} + \varepsilon_i \tag{1}$$

where i indexes births and Y_i is the financial outcome of interest for mother i, measured over a given time period (e.g., her average credit limit over the first post-birth year). The running variable d_i is the number of days between the child's date of birth and January 1 of the relevant re-centered year, negative for December-side births and positive for January-side births, and $D_i = \mathbb{1}\{d_i < 0\}$ indicates a December-side birth, which brings the first child-related tax benefit about a year sooner. The vector $\mathbf{X}_i$ collects pre-determined demographic covariates from the birth records and baseline credit outcomes measured at $q = -8$, and is included to improve precision; $\theta_{c(i)}$ is a fixed effect for the re-centered year that flexibly absorbs level differences across birth cohorts; and ε_i is an error term.

To alleviate concerns related to birth timing manipulation, in the baseline specification, I follow the donut RD approach in Barr, Eggleston, and Smith (2022), and estimate Equation 1 as a local linear regression on births within a 60-day bandwidth of the cutoff, excluding an 8-day donut on either side. The donut drops the births closest to January 1, where any shifting of delivery would concentrate: scheduling an induction or a C-section can move a delivery by a few days but rarely by a week or more, so a birth due more than about a week from the cutoff is unlikely to be moved across it. Section 4.2 tests this assumption directly, and the robustness checks vary the donut width up to 14 days. Observations are weighted using a triangular kernel in d_i so that births closer to the January 1 cutoff carry more importance in the estimation. Following Kolesár and Rothe (2018), I report heteroskedasticity-robust standard errors.

The coefficient of interest is τ, the discontinuity at the cutoff, which identifies the effect of receiving the first child-related tax benefit about a year earlier. Because not all parents claim or receive the tax benefits,[21] τ can be interpreted as an intent-to-treat (ITT) effect. Since I do not observe realized benefit receipt or a family's liquidity position, I estimate Equation 1 separately for mothers more and less likely to be liquidity constrained, as proxied by baseline credit scores. If earlier receipt matters because it relaxes a binding constraint or helps improve a weak credit profile, responses should concentrate among those with lower scores.

To explore the dynamics of the treatment effects, I estimate Equation 1 separately for each of the three post-birth years (quarters 0–3, 4–7, and 8–11),[22] as well as for each quarter from -7 to $+11$. For ease of interpretation, I later present yearly estimates as the main results and the quarterly paths of estimates in the appendix. Because control variables and the financial vulnerability classification are both constructed from the $q = -8$ snapshot, the path starts one quarter after this baseline.

Figure 1 aligns the quarter index with pregnancy, birth, and the receipt of the first child-related tax benefit for late-December and early-January births, and shows the one-year gap in receipt between the two sides. The comparison at each q therefore implicitly absorbs any business cycle or seasonal movements (Nelson and Drukker, 2018), as the quarter index is based on the January 1 cutoff and December- and January-side mothers are observed in the same calendar quarter; indexing to the birth instead would place a December-side

[21] Gee et al. (2026) estimate that in recent years 88–97% of US children are claimed on a tax return each year. And even when a child is claimed, the payments may not reach the parent I observe.

[22] For numeric variables, such as dollar amounts and the credit score, I calculate the mean over the four quarterly snapshots. For binary indicators, such as having active debt in collections, I take the maximum over the same four quarterly snapshots.

birth one calendar quarter earlier than a January-side birth. The drawback is that the quarters just before the cutoff compare mothers at different stages of pregnancy: for instance, at $q = -1$, December-side mothers have already delivered while January-side mothers have not. I therefore focus the interpretation on the pre-conception quarters ($q \leq -5$) and the postpartum quarters ($q \geq 0$), which are symmetric across the two sides, and any discontinuity in the pre-conception segment would reflect differences that predate the treatment rather than an effect of it.

4.2 Validity of the RD Design

The validity of the RD design rests on the assumption that births on either side of the January 1 cutoff are comparable, so that potential outcomes evolve smoothly through the cutoff. This could be violated if parents sort across the cutoff: if some parents can precisely control the date of birth, the composition of families on either side could differ in ways that matter for financial well-being. Separately, medical treatment may vary at the cutoff due to scheduling or hospital management around the holidays (Jacobson, Kogelnik, and Royer, 2021). Indeed, a birth in late December accelerates the family's tax benefits, and Dickert-Conlin and Chandra (1999) document birth timing responses to these incentives, although later estimates find the responses to be small, particularly for first births (LaLumia, Sallee, and Turner, 2015). Figure A.4 plots the number of births on each date of birth around the cutoff, for all first-time births and within the financially vulnerable group. In both samples, the distribution is smooth outside the 8-day donut, while the pronounced drop in deliveries around the Christmas and New Year holidays is confined to the excluded dates. Beyond deliberate timing, parents who give birth at different times of the year also differ on average (Buckles and Hungerman, 2013), making comparability around the cutoff ex ante unclear.

An empirical test of the identifying assumption is whether pre-determined characteristics are balanced across the cutoff. Figure 2 presents this test in summary form as regression discontinuity plots of three pre-determined measures, in the full sample and within the financially vulnerable group: predicted earned income, the predicted tax value of the first birth, and the baseline credit score. The three variables are particularly informative in this setting: the two predicted outcomes are each a function of many underlying demographic characteristics, so they condense into a single figure what a long list of separate tests would otherwise show, while the credit score summarizes the mother's baseline credit history and is the variable the financial vulnerability classification relies on. None of the measures visually jumps at the cutoff in any panel, suggesting that births on either side of the cutoff are broadly comparable.

The detailed regression results from balance tests are presented in Table B.1 for demographic and birth characteristics and in Table B.2 for baseline financial outcomes. The tests follow the donut RD specification of Equation 1 with re-centered year fixed effects as the only controls, and are run both on the full sample of births and within the financially vulnerable arm.

Across the 13 pre-determined demographic characteristics reported in Panel A of Table B.1, I do not detect any differences. Among the birth characteristics in Panel B, December-side mothers are 0.75 percentage points, or 2.5%, more likely to deliver by C-section, which is concentrated among the financially vulnerable. One concern is that parents who schedule a C-section to secure a December birth are also those who are more financially sophisticated. This is unlikely to be true, as gestation length is slightly higher on the December side by half a day, or 0.2%, among the financially vulnerable, while a tax-motivated acceleration of deliveries would predict shorter gestation lengths (Schulkind and Shapiro, 2014). I also do not detect any discontinuity in labor induction, the other margin through which a delivery can be brought forward, and there is no shift in the incidence of pre-term birth or low birth weight, or in the Apgar score. Reassuringly, my main results are essentially unchanged when I include delivery mode as a control. And to the extent that C-sections are more expensive and marginally delay the mother's return to work (Miller et al., 2025), the effects of earlier tax benefit access could be understated.[23] Finally, Panel C shows no discontinuity in predicted earned income or the predicted tax value of the first birth, echoing the graphical evidence in Figure 2, and there is also no discontinuity in selection into the main analysis sample.

In Table B.2, I test for differences in pre-existing financial outcomes. The table shows that a wide range of baseline credit outcomes are balanced across the cutoff, including the credit score and the financial vulnerability classification that the sample split relies on. The two exceptions are a 0.66 percentage point lower mortgage holding rate on the December side in the main sample, which does not extend to the financially vulnerable, and slightly higher auto loan balances among the financially vulnerable with no change in the share holding an auto loan. And while the baseline mortgage and auto loan amounts enter directly into the construction of the financial vulnerability index, neither the index nor the alternative group classification it defines shows a discontinuity either. In my main specification, I control for baseline credit outcomes, which include these mortgage and auto loan balances, and the estimates are stable when I drop the controls altogether, so these imbalances are unlikely to be driving the results I observe.

The design also requires that no other program changes discontinuously at the same cutoff. An important one is the school entry cutoff, as free public education can change the family's childcare needs and labor supply, both directly upstream of the financial outcomes I study.[24] To my knowledge, the birthdate cutoffs

[23] Winger, Rae, and Cox (2025) estimate that among people with employer coverage, the out-of-pocket costs throughout pregnancy, delivery, and postpartum care are $3,071 for a C-section birth and $2,563 for a vaginal birth, a difference of about $500.

[24] For studies of the labor supply impacts of preschool programs in the US, see, for instance, Fitzpatrick (2010); Wikle and Wilson (2023); Humphries et al. (2024).

for public school as well as early education programs in California have not fallen near January 1 at any point in my sample period, even as they moved with successive reforms. Under current rules, kindergarten, transitional kindergarten, and Head Start use September 1, the California State Preschool Program uses December 1, and eligibility for the programs that enroll three-year-olds does not change discontinuously at January 1.

5 Results

In this section, I report the effects of earlier access to child-related tax benefits on mothers' financial well-being and related outcomes. Each main table reports the RD estimates from Equation 1 separately by financial vulnerability and by post-birth year. To benchmark the effect sizes, I also report the unweighted mean of each outcome among January-side births. The underlying quarterly paths of the estimates are available in the appendix.

5.1 Unpaid Debt

I begin with debt in collections, the outcome that most directly measures financial distress. The two primary measures are whether the mother has any active debt in collections, and the amount owed. Because debts of any kind can be sent to collections, such as utility and medical bills (Avery et al., 2003; Gibbs et al., 2025), these measures capture financial strain even for mothers with limited access to formal credit.

In Panel A of Table 2, a December-side birth reduces both the incidence and the amount of active debt in collections among financially vulnerable mothers, and the effects persist at similar magnitudes across all three post-birth years. In year 3, December-side mothers are 1.62 percentage points less likely to have any active debt in collections than their January-side counterparts, and the amount owed is roughly \$60 lower. They represent a 4% and an 8% decline relative to the January-side mean, respectively.

As a benchmark for these effect sizes, health insurance coverage expansions reduce debt in collections by 10–25% (Finkelstein et al., 2012; Mazumder and Miller, 2016; Hu et al., 2018). The declines here amount to roughly one-third of those effects, even though the identifying variation comes only from accelerating the arrival of child-related tax benefits by about a year. To obtain the policy-relevant magnitude, I also scale the estimates by the mean predicted tax value of the first birth among the financially vulnerable, roughly \$3,800: these estimates imply that each \$1,000 of eligible tax benefits brought forward by a year reduces the incidence by 0.43 percentage points and the amount owed by about \$16.

By contrast, in Panel B, financially non-vulnerable mothers, who face the same timing shift with a mean predicted benefit of about $3,200, show small, positive, and statistically insignificant differences in both collections outcomes in all three years; in year 3, the 95% confidence intervals rule out declines larger than 0.25 percentage points and $10, about one-sixth of the corresponding effects among the financially vulnerable group.

Figure 3 provides visual evidence for these estimates, plotting the daily means of both outcomes over year 3 by the child's date of birth. Among the financially vulnerable, the means jump discontinuously at the January 1 cutoff, while the financially non-vulnerable panels, drawn to the same y-axis scale for ease of comparison, show no discernible break. Finally, the quarterly paths in Figure A.5 confirm that the declines among the financially vulnerable appear only after the first tax season and persist thereafter, while the financially non-vulnerable path stays essentially flat at zero.

I additionally report the effects on the two delinquency indicators, the precursor outcomes to a collections record, in Table B.3. In the financially vulnerable group, both measures decline in year 1 for December-side mothers: the incidence of 90-day delinquencies falls by 1.31 percentage points, or 9% of the January-side mean, and that of 30-day delinquencies by a marginally significant 1.03 percentage points, or 4%. The declines appear transitory and do not persist into years 2 and 3. The quarterly paths in Figure A.6 show some imbalances in delinquency around $q = -5$, roughly the quarter before conception, although the gap partly closes early in pregnancy; I therefore read these estimates as suggestive.[25]

5.2 Credit Access and Utilization

I now turn to credit card access and utilization. The two primary outcomes are the mother's total credit limit across her open credit card accounts and her total balance on those accounts. The limit measures access to flexible credit directly, but the direction of the effect on balances is ambiguous ex ante, because observed balances mix day-to-day spending with revolving debt. On the one hand, families often use tax refunds to pay down unsecured debt (Shaefer, Song, and Williams Shanks, 2013; Jones and Michelmore, 2018, 2019), which would lower balances; balances would also fall if families use the refund to pay cash for purchases they would otherwise have charged to a card. On the other hand, if families become financially better off and make additional purchases through their credit cards (McGranahan, 2016), or if the extra cash enables them to maintain the revolving balance without being charged off, balances would rise. Indeed, Agarwal, Liu, and Souleles (2007) document both responses after the 2001 tax rebates. Given this ambiguity, I interpret the

[25] One possibility is endogenous conception timing, where a woman who experiences a transitory financial shock may delay a planned pregnancy, so the quarters just before conception select on recent financial health. Although I cannot verify this channel directly, evidence from other settings shows that credit conditions are closely tied to household formation and stability (Dokko, Li, and Hayes, 2015; Dettling and Hsu, 2018) and that fertility responds to unemployment (Lindo, 2010; Del Bono, Weber, and Winter-Ebmer, 2012; Currie and Schwandt, 2014; Huttunen and Kellokumpu, 2016; Schaller, 2016).

estimates for the financially non-vulnerable group as a natural benchmark for how the financially vulnerable group would have responded had it not faced tight liquidity constraints.

In Panel A of Table 3, December-side mothers in the financially vulnerable group see a persistent increase in their credit limits across the three post-birth years. In year 3, their limits are \$436, or about 4%, higher than those of their January-side counterparts. Credit card balances respond more slowly: the year 1 difference is positive but statistically insignificant; balances then rise, and by year 3 they are \$212, or 5%, higher. The increase in balances contrasts with the large declines found among marginally approved SNAP applicants (Homonoff, Lee, and Meckel, 2026). A simple division implies that the new balances take up just under half of the newly available limit.

For comparison, Dettling and Hsu (2021) find that a \$1 increase in the minimum wage raises credit limits by about 7%, so the response here is roughly two-thirds as large. Scaled against the roughly \$3,800 of benefits brought forward, the effects in year 3 translate into approximately \$115 in limits and \$56 in balances per \$1,000 accelerated.

In Panel B, I find no effects on credit limits among the financially non-vulnerable mothers, but their balances fall by about 3–4% of the January-side means, consistent with unconstrained families using the accelerated benefits to pay down credit card debt.

Figure 4 presents the RD plots for year 3 outcomes: the discontinuity at January 1 is clear among the financially vulnerable and much more muted among the financially non-vulnerable. The quarterly paths in Figure A.7 show both outcomes rising gradually for the financially vulnerable after the birth, with the balance response emerging more gradually than the limit response. For the financially non-vulnerable, the paths echo Panel B of Table 3, though the balance decline may partially reflect a small downward drift already visible in the pregnancy quarters.

As additional evidence, I examine two extensive margins of the credit expansion: whether the mother applies for a new credit card during the year (recorded as an inquiry on her credit report), and whether she holds any open credit card. Table B.4 reports the estimates. Among financially vulnerable December-side mothers, the flow of new applications, as measured by credit card inquiries, rises by 1.3 percentage points in year 1 when the accelerated benefits arrive. The two years after see no such difference. The share holding any open credit card rises by 0.9 percentage points in year 1 and remains 1.2 percentage points higher in year 3, compared to the January-side mean of 72%. The quarterly paths in Figure A.8 show a spike in inquiries in $q = 0$, the quarter of the first tax season, which then reverts.[26] The card-holding path is noisier but generally

[26]Part of the spike at $q = 0$ and the dip at $q = -1$ may reflect mothers being less likely to apply for new credit in their delivery quarter, the timing asymmetry discussed in Section 4.1.

positive after the first birth. The expansion in credit access therefore operates partly through the extensive margin, not only the existing accounts.

Taken together, earlier receipt expands credit access for financially vulnerable mothers on both the intensive and the extensive margin, and the expansion persists through year 3, well after the extra refund. Their credit card balances also rise with the new limits. Financially non-vulnerable mothers, whose limits do not move, instead pay balances down, a contrast that mirrors the heterogeneity by liquidity constraints that Agarwal, Liu, and Souleles (2007) document.

5.3 Overall Financial Health

I next consider overall financial health summarized by the credit score, which is widely used by lenders to screen applicants and set credit terms. The outcomes of interest are the score itself, averaged over the quarters in which the mother has a valid score, and the share of the year in which her score reaches the prime tier of 661 or above. Table 4 reports the estimates.

In year 1, December-side mothers in the financially vulnerable group score 2.43 points higher than their January-side counterparts, and are 1.07 percentage points more likely to hold a prime score in a given quarter, a 4% increase over the January-side mean, consistent with the decline in collections debt and the rise in credit limits documented above. The gain then fades to essentially zero by year 3. Two competing forces are present in the later years: higher limits and a larger credit portfolio raise the credit score, while new accounts may also lower the score through hard inquiries and a shorter average account history, and the impacts likely cancel out. In Panel B, financially non-vulnerable mothers also score higher in year 1, by a more modest 1.50 points, consistent with their credit card paydown, and the gain fades once January-side mothers receive their own first benefit. The quarterly paths in Figure A.9 are similar for both groups, showing that the gains are concentrated in the quarters just after the first tax season and gradually fade.

5.4 Durable Consumption

The final set of financial outcomes concerns two major durable goods, vehicles and homes. The credit panel does not allow me to observe purchases directly, but both are typically financed, so I proxy durable consumption with the associated secured debts: whether the mother carries a positive auto loan balance, and whether she carries a positive mortgage balance. Table 5 reports the estimates.

In Panel A, the two margins move at different times. In year 1, when the accelerated benefits arrive, auto loan holding rises by a marginally significant 1.10 percentage points, or 2.4% of the January-side mean, consistent with prior work showing that tax refunds help families purchase vehicles, for instance by meeting down payment requirements (Goodman-Bacon and McGranahan, 2008; Adams, Einav, and Levin, 2009). The effect fades in the two later years, possibly reflecting a combination of loan paydown and the January side catching up. In contrast, mortgage holding moves later: the year 1 estimate is small and insignificant, and the difference first appears in year 2, at 1.02 percentage points, or 8.7% of the January-side mean, and remains at 0.68 percentage points, or 5.1% of the mean, in year 3.

Because the effect on mortgage holding appears not when the accelerated benefits arrive but only after the initial credit improvements, and because the benefit amount is modest relative to a home purchase, I interpret the response as evidence that the impact runs through the improved credit profile rather than through directly funding the purchase. Consistent with this interpretation, large improvements to the credit record move mortgage holding in other settings, such as the removal of bankruptcy flags (Dobbie et al., 2020). The magnitudes align as well: the 5.1% increase in year 3 is comparable to the roughly 5% increase following flag removal. I read the comparison with caution, since the response here peaks in year 2 and fades somewhat by year 3.

In Panel B of Table 5, December-side financially non-vulnerable mothers again show no movement on either margin. The quarterly paths in Figure A.10 confirm the timing pattern uncovered above: within the financially vulnerable group, the auto loan response builds over the first two years before fading by the end of the panel, while the mortgage response is absent through year 1 and emerges only from the second year onward.

5.5 Additional Outcomes

Neighborhood Quality If the financial gains move families toward better neighborhoods, then we might expect to see improvements in census tract characteristics on the December side. Table B.5 reports the estimates for four measures from Chetty et al. (2026): the tract's median household income and poverty rate, and the predicted adult household income and incarceration rate of children who grew up there. The January-side means show that financially non-vulnerable mothers live in noticeably better tracts, with a median household income about 30% higher than in the financially vulnerable group. Across both groups, however, the estimates over years 1 through 3 are small and economically insignificant throughout. These results suggest that the gains from earlier benefit receipt may not have extended to realized moves to better neighborhoods.

Family Structure and Financial Integration Because financial strain can damage the stability of a relationship, easing it can potentially help preserve parental co-residence and yield additional benefits within the family. Within the subset of births where the father is also linked, I thus examine four outcomes related to the family structure and financial integration of the parents: three co-residence proxies, ordered from the coarsest to the finest, and an indicator for whether the mother holds any joint account with the father.

The estimates are reported in Table B.6. The January-side means show that these outcomes evolve considerably as the child ages: in the financially vulnerable group, the share of parents in the same ZIP code falls from 86% in year 1 to 79% in year 3, while the share with a joint account rises from 40% to 54%. Against these moving baselines, the estimates are small throughout. These results suggest that the accelerated benefit is unlikely to have shifted whether the parents stay together or the extent to which they jointly manage their finances.[27]

5.6 Robustness Checks

To establish the robustness of my main results among the financially vulnerable mothers, I conduct four groups of checks: alternative regression specifications, alternative sample definitions, the exclusion of individual tax regimes, and placebo tests. Throughout, I focus on the four primary outcomes that carry the results above: whether the mother has any active debt in collections, the amount owed, her total credit card limit, and her total credit card balance, each measured over the third post-birth year unless noted otherwise.

Regression Specification Table B.7 reports results under several regression specifications that differ from the main specification of Equation 1. They include: (i) additionally controlling for C-section delivery, which I find to be slightly imbalanced between the December-side and January-side mothers, (ii) removing all covariates, (iii) removing the triangular weights, (iv) using dollar outcomes that are not winsorized, (v) clustering the standard errors by the running variable (i.e., day of birth relative to January 1), and (vi) fitting local linear and local quadratic specifications using the `rdrobust` package developed by Calonico et al. (2017). The estimates are quite stable across all these specifications. In Panel D, removing the triangular weights attenuates the estimates moderately, as observations that are farther away from the cutoff receive more importance. The local quadratic specification in Panel H returns larger effect sizes with larger standard errors; the loss of precision is expected, since the fitted curve must extrapolate through the donut region.

I additionally test the sensitivity of the results to the bandwidth and donut size choices. In Figure A.11, I report estimates obtained using bandwidths ranging from 30 to 90 days, and in Figure A.12, using donut sizes ranging from 0 to 14 days. The bandwidth choice follows the common bias-variance tradeoff: a wider bandwidth increases the sample size and the precision of the estimate, but admits births farther from the cutoff, which may be less comparable. The donut size involves a similar tradeoff: a wider donut excludes

[27]The null on co-residence is also in line with Hankins and Hoekstra (2011), who find that positive income shocks from lottery winnings do not meaningfully change divorce rates.

more of the births whose timing might have been shifted, but rests the estimate on births farther away and forces the fit to extrapolate over a longer gap. Some movement along the two grids is therefore expected. The estimates are largely stable across both grids: they are generally larger but less precise at narrow bandwidths, and the confidence intervals contain the main estimate throughout, except for the collections estimates at the smallest donuts, where births most likely subject to timing shifts re-enter the sample.

Sample Definition I also test the robustness of my results to alternative samples, varying either the linkage requirement or the definition of financial vulnerability.

In Panel B of Table B.8, I take the conservative approach and restrict the sample to mothers matched through the credit snapshot in March of the year before the re-centered year, so that the linkage precedes any receipt of child-related tax benefits. Because this snapshot sits roughly 7–11 months before the births in my window, mothers who move in the interim become harder to match on the address recorded at delivery, and the sample shrinks by about 30%. In exchange, the requirement addresses the concern that earlier receipt could itself make a mother easier to match: the inquiry and auto loan responses documented in Section 5.2 and Section 5.4 are exactly the kind of credit activity that keeps a bureau profile current, although the balance tests in Table B.1 detect no discontinuity in selection into the main analysis sample. The estimates broadly hold under this stricter gate; the collections dollar estimate is somewhat smaller and loses statistical significance, though its confidence interval still contains the main estimate.

In Panels C and D of Table B.8, I reclassify mothers using the financial vulnerability index, their predicted likelihood of experiencing a 30-day delinquency in the first post-birth year, instead of their baseline credit score. The index is more transparent than the credit score, which comes from a proprietary model and may misclassify borrowers (Albanesi and Vamossy, 2024; Bakker et al., 2025). Panel C reports the results for mothers in the top 35% of the index distribution, an alternative financially vulnerable group similar in size to the main one, and Panel D reports those for mothers in the bottom 65%. All four main results replicate in Panel C, while Panel D finds no detectable response, as expected for a financially non-vulnerable group.

Tax Regime My sample pools re-centered years 2006 through 2023, a period in which tax policies have changed substantially. At the federal level, notable examples include the TCJA and the pandemic-era expansions; within California, the CalEITC was introduced in 2015 and the YCTC in 2019.[28]

To assess the sensitivity of the main results to the tax regime, I re-estimate the main specification excluding one re-centered year at a time. I also estimate a version that jointly excludes re-centered years 2019 through 2023, the cohorts who claim their first child-related tax benefits under the TCJA and pandemic-

[28] Recall that Panels (c) and (d) of Figure A.1 plot the tax value of a first birth over time for families in California with earnings between 50% and 300% of the Federal Poverty Level, and the large changes are indeed concentrated in the later years.

era rules. These exclusions shift the benefit at stake only modestly: compared to the main sample, the mean predicted tax value of a first birth ranges from about 10% lower under the joint exclusion to about 1% higher. Figure A.13 reports the estimates. Across all exclusions, the estimates are quite similar to the main ones, which rules out any one-time change affecting a single cohort as the driver of the results.

Placebo Tests Finally, I estimate two sets of placebo regressions: one on pre-conception outcomes, and one centered on cutoff dates other than January 1. Because neither involves any change in tax benefit eligibility, I expect to find no discontinuities.

In Panel B of Table B.9, I estimate the specification of Equation 1 on outcomes aggregated over the pre-conception quarters after the baseline ($q = -7$ to $q = -5$), which predate any receipt of child-related tax benefits. I also drop the baseline financial controls measured at $q = -8$, immediately before this window. As expected, I find null effects across all four outcomes.

In Panels C through E of Table B.9, I re-estimate Equation 1 centered on November 1, February 1, and March 1, respectively.[29] Only one of the twelve estimates is statistically significant: the collections incidence at November 1. The collections dollar estimates, in contrast, are informative zeros at every placebo date, with confidence intervals that exclude the main estimate.

6 Mechanisms

In this section, I explore the mechanisms behind these results along three dimensions: the channel through which earlier benefit receipt expands credit access and utilization, the type of constraint that explains the observed effects, and the incidence of the credit expansion within the family.

6.1 How Cash Buys Credit

I interpret the main results in Section 5 through the lens of relaxing a binding borrowing constraint, which connects the timing of child-related tax benefits to persistent gains in credit access and utilization among the financially vulnerable.

On the credit limit side, three features point to a relaxed borrowing constraint, as opposed to a transitory impact from cash transfers. First, the response does not extend to mothers with a good credit standing at baseline, whose access was likely unconstrained to begin with. December-side financially non-vulnerable mothers face the same timing shift, but they have credit limits nearly three times as high and much lower

[29] Some subsidized preschool programs enroll children at age 3 based on a December 1 birthday cutoff, and this represents a channel beyond the scope of this paper, so I do not use December 1 as a placebo date.

utilization rates than the financially vulnerable mothers. Accordingly, their limits show no detectable change. Second, although one might expect existing cardholders to be less constrained than non-cardholders, the expansion is nonetheless concentrated among cardholders. I conduct a simple bounding exercise: the share holding any card rises by 1.2 percentage points at year 3, and even if each mother who newly enters the credit market were approved at the mean credit limit of $13,700 among existing cardholders, this extensive margin would account for at most $160, or 37%, of the overall effect. Most of the new capacity therefore goes to mothers who were already borrowing, as higher limits on existing accounts or additional cards. Third, the credit limit expansion outlasts the extra refund, which arrives in year 1 only: the limits stay $328 to $436 higher on the December side across all three years. To the extent that a constrained household cannot borrow against future resources, a December-side one holds one more tax refund's worth until the child ages out, consistent with the documented persistency, whereas the same timing difference matters much less to a household that can borrow.

On the credit card balance side, the response also resembles what a pure marginal propensity to borrow out of new credit would produce. The ratio of the balance change to the limit change, just under one half based on the year 3 point estimates, is close to what constrained borrowers show in settings where credit limits are relaxed: Agarwal et al. (2018) report 0.59 in the first year for cardholders with credit scores of 660 or below,[30] Dobbie et al. (2020) report 0.59 three years after the removal of a bankruptcy flag, and Aydin (2022) reports 0.50 over three quarters for those with a baseline utilization rate of at least 75%. Expanded access alone would therefore produce balances of this size. I treat the comparison as suggestive: as Dobbie et al. (2020) note in their setting, the ratio should not be read as an estimate of the marginal propensity to borrow, as it cannot rule out whether other factors may impact the balances.[31]

Empirically, higher balances from credit limit expansions reflect a combination of both additional purchases and revolving debt (Agarwal et al., 2018), but other interpretations are also possible, and which one holds matters for interpreting the welfare effects of a credit expansion. I consider three alternatives in turn. The first is that December-side mothers simply spend the extra tax benefit through their cards, so that the higher balances would appear even if the new borrowing capacity played no role. Spending and credit card balances do rise sharply in high-EITC areas when refunds arrive (McGranahan, 2016; Aladangady et al., 2023). However, spending the refund does not explain why credit limits rise, nor, to the extent that the spending is a one-off response to the lump sum transfer, why balances remain higher in the later years.

[30] The 660 cutoff is the same as in this paper, but Agarwal et al. (2018) use the FICO score, which differs from the scoring model used in the UC-CCP.

[31] In Agarwal et al. (2018) and Aydin (2022), the source of variation is an exogenous change in the credit limit itself. The timing of child-related tax benefits in this paper, like the removal of a bankruptcy flag in Dobbie et al. (2020), may move balances through channels other than the limit, and the exclusion restriction does not necessarily hold.

The second concerns the debt component: the possibility that financially vulnerable mothers run up balances they cannot sustain, and that the additional credit harms rather than helps them. Since December-side mothers see consistently lower debt in collections, a higher likelihood of holding a mortgage in the later two years, and no worsening in credit scores relative to January-side mothers, these patterns suggest that they are unlikely to be borrowing irresponsibly. One caveat is that the aggregate results may mask the heterogeneity between a majority who borrow responsibly and a minority who are now induced to borrow beyond their means, and I cannot rule out such a mix. Under this scenario, the collections and credit score results would understate the gains to the majority who borrow responsibly.

The third also concerns the debt component: the mother moves other types of debt she would otherwise carry, such as payday loans or debt in collections, onto credit cards, so that the higher balances reflect relabeled debt rather than additional consumption.[32] Because alternative borrowing options may not involve the formal credit market, I cannot rule out this explanation directly in my data. It is not a rival to expanded access, however, since moving debt onto a card requires the capacity to do so, and where such substitution does occur, it likely reflects a gain. A payday loan carries a far higher interest rate than a credit card, and an unpaid collections account leaves a derogatory mark in the payment history and can remain on the credit report for several years, whereas high card utilization does less damage so long as the balances do not become delinquent. In the spirit of Mello (2025), substitution can represent an upward move in the financial hierarchy: from bad debt and high-cost credit to mainstream credit.

To interpret the size of the credit expansion channel, I translate the year 3 estimates into a proxy for the consumption gains through a back-of-the-envelope calculation. Agarwal et al. (2018) find that among cardholders with the lowest credit scores, each additional dollar in the credit limit raises cumulative purchases on the card by \$0.94 over three years after card opening, and that interest-bearing debt makes up about three-quarters of the higher balances at that horizon. At those rates, the unscaled estimates of \$436 in credit limits and \$212 in balances imply approximately \$410 in additional purchases over the three years after the birth and \$159 in outstanding revolving debt, which costs roughly \$40 per year at a 25% interest rate. To the extent that the additional purchases reflect substitution from cash to credit cards, the calculation overstates the consumption gains. And to the extent that my setting involves shifts in household resources that a pure credit limit increase does not, it likely overstates the revolving debt and therefore understates the net gains.

To summarize, I view the earlier receipt of child-related tax benefits as relaxing a binding borrowing constraint among the financially vulnerable mothers. The cash transfer enables them to improve their credit standing, obtain more credit access, and draw on part of that newly obtained borrowing capacity. The higher

[32] Payday loans are not banned in California, but heavily regulated. For instance, the amount cannot exceed \$300.

balances thus signal financial slack rather than financial strain. This pattern is also consistent with the effects on mortgages in Section 5.4, which appear only in the later years after an initial improvement in credit conditions.

6.2 The Role of Borrowing Constraints

The interpretation above rests on a binding borrowing constraint. Poverty and borrowing constraints tend to move together, but they correlate less strongly in my linked sample, reflecting both the selection into the credit panel and the broader evidence that income and credit standing carry distinct information (Bakker et al., 2025; Dobbie et al., 2020). A natural question is then whether the effects extend to the low-income population more generally. To probe this, I construct two low-income populations within the linked sample, classified using birth record information alone: births where the principal source of payment is Medicaid, and births in the bottom 30% of the predicted household income distribution. Panels B and C in Table B.10 report the results for these samples.

From the January-side means in columns (1) and (2), both populations are about as exposed to debt in collections as the financially vulnerable mothers: the incidence is 37–39% versus 40%, and the amount owed is $643–$680 versus $707. The effects of earlier tax benefit access on these outcomes are also similar, representing a 4% reduction in the incidence and a 7–8% reduction in the amount owed.

However, the results on credit access and utilization differ for the low-income samples. In columns (3) and (4), there are no detectable effects on credit limits or credit card balances in either income-based sample. Reading from the January-side means, mothers in the Medicaid sample carry credit limits about $1,180 lower than the financially vulnerable mothers, but their balances are about $1,400 lower, leaving more unused capacity relative to their limits. The pattern is similar for the bottom 30% in predicted household income, whose limits are slightly higher and whose balances are about $800 lower than those of the financially vulnerable mothers.

One suggestive interpretation of the contrast between the collections and the borrowing outcomes is that a borrowing constraint, rather than low income per se, is the margin that matters. Since debts of any kind can be sent to collections, the collections outcomes require no engagement with formal credit, so low-income families with unpaid bills are exposed regardless of their credit access. The credit market responses of expanded limits and higher balances, by contrast, appear only in the group defined by a low credit score, where a borrowing constraint is most likely to bind. Based on this reading, the classification by credit score is capturing that constraint, in line with Agarwal et al. (2018), who document in the credit card market more broadly that the marginal propensity to borrow out of a credit limit increase declines sharply with the credit score, and income proxies it only weakly in my linked sample.

This conclusion, however, is specific to my linked sample. Because mothers without pre-existing credit records never enter the analysis panel, the population I study excludes those who are credit invisible or carry only thin credit files, and they disproportionately come from low-income households (Brevoort, Grimm, and Kambara, 2016). As a practical matter, income may therefore remain the more informative targeting margin in the full population.

6.3 Incidence of Credit Expansion Within the Family

Because credit records are individual but families borrow across both parents' accounts, a separate question is who carries the credit expansion: the mother alone, or the family. How resources are allocated within households is a longstanding topic in economics (Chiappori and Mazzocco, 2017), and recent work in household finance shows that the division of financial accounts between spouses has real consequences for a couple's balance sheet and consumption (Kim, 2024; Vihriälä, 2025; Choukhmane, Goodman, and O'Dea, 2025). If the family as a whole experiences gains in credit access and utilization, the mother's higher balances reflect a general expansion. However, if the parents simply shift existing borrowing onto the mother's credit cards, the effects I detect may reflect a reallocation between them without more substantive gains. To separate the two, I restrict to the subsample of financially vulnerable mothers where the father is also linked to the credit panel and re-estimate the year 3 effects on the credit card limit and balance separately at the mother, father, and family levels. Because this reduces the sample size by about 40%, and the estimates are less precise, I treat the evidence here as suggestive.

I first examine credit card balances, the margin a family can flexibly shift across the parents' accounts and therefore where reallocation would most likely appear. From column (2) of Table B.11, the effects on balances are positive on the December side at all three levels. The mother's balances in this subsample are \$194 higher, comparable to the \$212 estimate in the main analysis, though no longer statistically significant because of a larger standard error. The father's balances are \$266 higher, about 5% of the January-side mean, and the estimate is significant at the 10% level. Finally, the family-level balances are \$395, or 4%, higher on the December side, significant at the 5% level. This suggests that the effects I detect are unlikely to be driven purely by reallocation between the parents, which would predict a decrease in father-level balances and no change in family-level balances.

The credit limits in column (1) are less informative about reallocation, as existing limits are set by lenders and rarely decrease.[33] That said, the estimates are likewise higher on the December side at every level, at

[33] Consumer Financial Protection Bureau (2022) finds that in the first quarter of 2019, 0.31% of consumers without a recent

$391 for the mother, $348 for the father, and $563 for the family, although none is statistically significant. The mother's estimate is similar to the $436 estimate in the main analysis, and the confidence intervals rule out all but small declines at the father and family levels, at 2% and 1.4% of the respective January-side means.

Taken together, the family-level evidence is consistent with the interpretation that the arrival of new credit capacity, and the borrowing that follows it, form a family-level pattern, not an artifact of accounting between the two parents.

7 Conclusion

This paper studies the impacts of the timing of cash transfers on the financial well-being of households. I focus on the population of first-time mothers, who are likely financially fragile, and whose finances can impact not only their own well-being but also that of their children. My empirical approach leverages the discontinuity in child-related tax benefits, which introduces a one-year gap in first receipt among otherwise similar families.

The findings show that earlier benefit receipt relaxes borrowing constraints and produces persistent financial gains among financially vulnerable mothers, with no comparable effects among financially non-vulnerable mothers. For each $1,000 of tax benefits that are brought forward, I document a $16 reduction in debt in collections, a $115 increase in credit limits, and a $56 increase in credit card balances in the third year after the birth. I also find suggestive evidence that the likelihood of holding a mortgage rises after initial credit profile improvements, and that the credit expansion is shared more broadly at the family level.

Since parents serve as intermediaries through which cash transfers reach the child, these results provide support on the household finance side for a link that earlier studies on children's long-run outcomes have mostly posited (Cole, 2021; Barr, Eggleston, and Smith, 2022; Rittenhouse, 2025): the payments loosen parents' liquidity constraints and strengthen their financial position. I do not find detectable impacts of earlier benefit receipt on neighborhood quality or parental co-residence, but it is possible that these outcomes see improvements that are not captured within my time frame or based on my relatively coarse proxies.

The findings also speak to the design of transfer programs. Because variation in when a benefit arrives rarely comes apart from variation in eligibility or amount, the gains from delivering a given benefit earlier have been hard to observe and are likely underappreciated. The estimates in this paper provide evidence on this front: moving the first child-related tax benefits forward by about a year produces gains that remain

credit card delinquency and 1.33% of those with one experienced a credit limit decrease.

visible three years after the birth among financially vulnerable mothers, for whose liquidity constraints most likely bind. As in Collier et al. (2025), who study the availability of disaster loans that households eventually repay, there can be lasting gains from well-timed liquidity provision.

From a policy perspective, a family's labor market situation and financial needs change sharply around pregnancy and childbirth, so providing resources to parents during this period, rather than at the next tax filing, may realize such gains.[34] One caution is that the transfers I study in this paper are annual lump sums delivered through tax filing, so the results do not necessarily extrapolate to debates over the optimal payment frequency (e.g., Greenlee et al., 2021; Bonomo, Ruffini, and Schanzenbach, 2026), such as those surrounding the monthly Child Tax Credit payments in 2021 (Parolin et al., 2023). Another is that December-side families eventually age out of eligibility one year sooner during their child's adolescence. I cannot speak to this cost, but it is possible that the early childhood gains outweigh these later losses. Finally, due to limitations of my data, the impacts on what Michelmore (2025) calls the family processes within these households and on mothers who are not readily represented in credit data remain important directions for future work.

[34] Programs of this kind exist in many countries. In the US, beyond the proposals discussed in footnote 1, Rx Kids in Michigan and Ohio already provides unconditional cash transfers of $1,500 during pregnancy and $500 per month for babies for 6–12 months.

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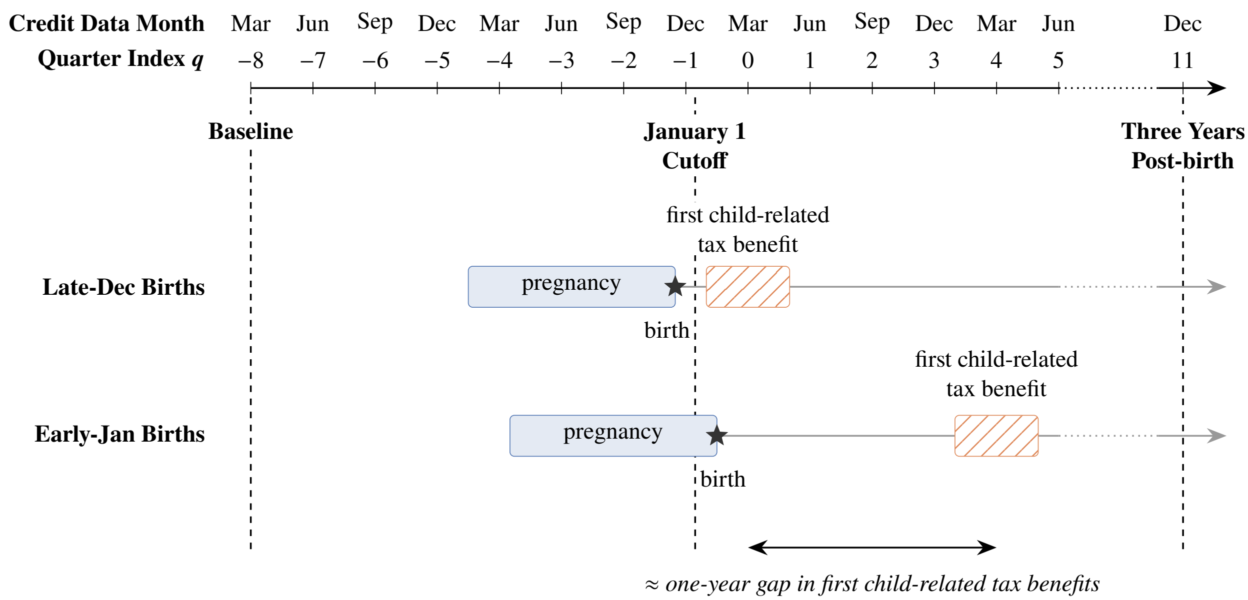
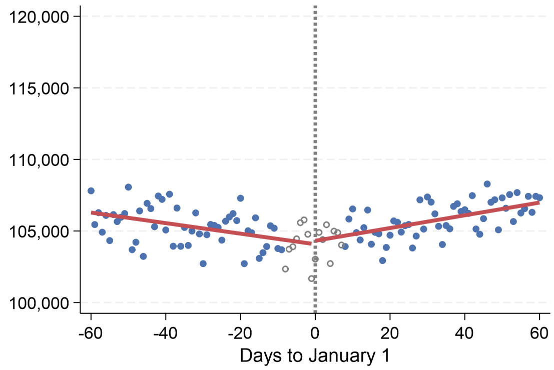
Figures

Figure 1: Definition of the Quarter Index q and the Timing of Child-Related Tax Benefits

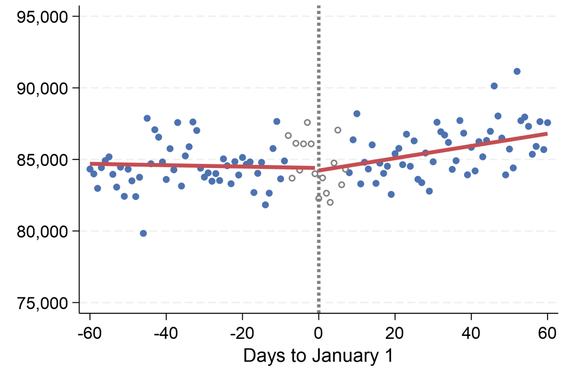


Notes: The figure defines the quarter index q and shows how it aligns with pregnancy, birth, and the receipt of child-related tax benefits for both late-December births and early-January births, born around the same January 1 cutoff. $q = 0$ is the first credit data snapshot after the January 1 cutoff. $q = -8$ falls roughly two years before the birth, about one year before conception, for both groups of births, and serves as the baseline snapshot from which I measure pre-determined controls and construct the financial vulnerability split.

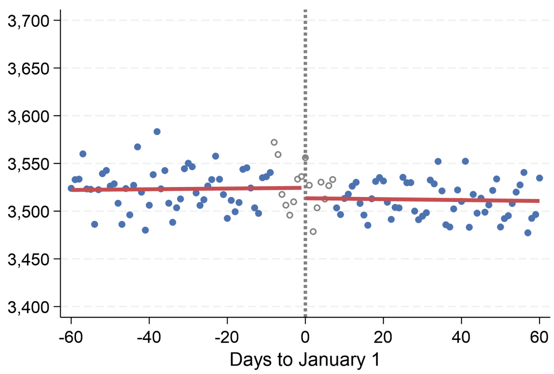
Figure 2: Smoothness of Covariates



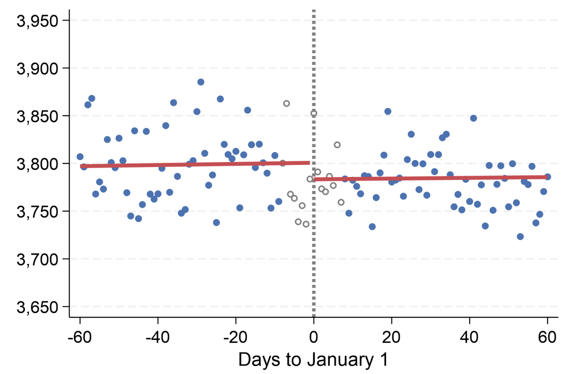
(a) Predicted Earned Income, All First-Time Births



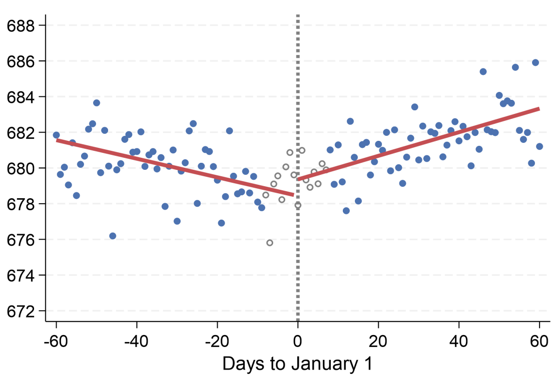
(b) Predicted Earned Income, Financially Vulnerable



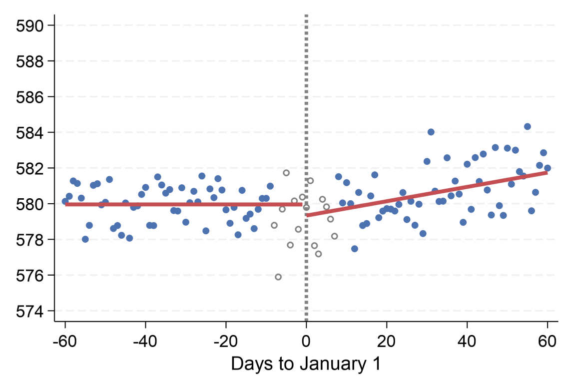
(c) Predicted Tax Value, All First-Time Births



(d) Predicted Tax Value, Financially Vulnerable



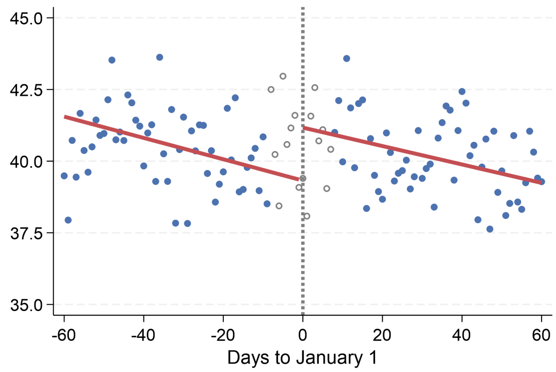
(e) Credit Score, Analysis Sample



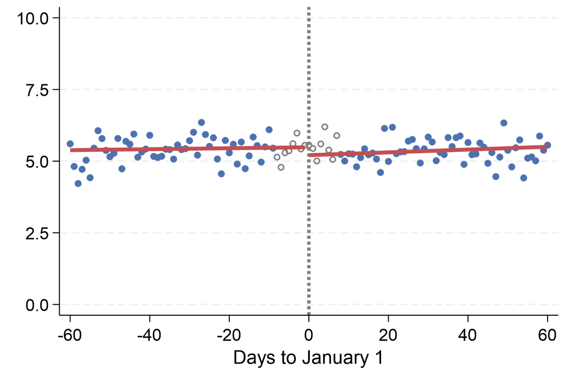
(f) Credit Score, Financially Vulnerable

Notes: This figure plots three pre-determined measures against the date of birth relative to the January 1 cutoff. Panels (a) and (b) plot predicted earned income, Panels (c) and (d) the predicted tax value of the first birth, and Panels (e) and (f) the mother's credit score, measured at the baseline quarter $q = -8$; the construction of the two predicted measures is detailed in Appendix C.2. In the left column, Panels (a) and (c) cover all first-time singleton births in re-centered years 2006–2023, and Panel (e) covers the main analysis sample constructed in Section 3.4; in the right column, the sample is further restricted to financially vulnerable mothers whose baseline credit score is at or below 660. Within each measure, the two panels are drawn to the same y-axis scale. Each dot represents the mean across all births on a given date within the 60-day estimation bandwidth, and hollow dots mark the dates inside the 8-day donut, which are excluded from estimation. Births to the left of the vertical line are December-side births, whose families receive the first child-related tax benefit about a year earlier. The red lines are linear fits to the daily means outside the donut on either side of the cutoff, weighted by the number of births on each date and the triangular kernel described in Section 4.1, and are extrapolated through the donut. All dollars are in 2025 values.

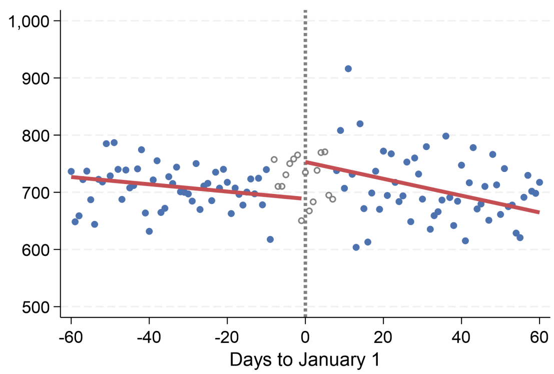
Figure 3: Discontinuity in Unpaid Debt in Year 3



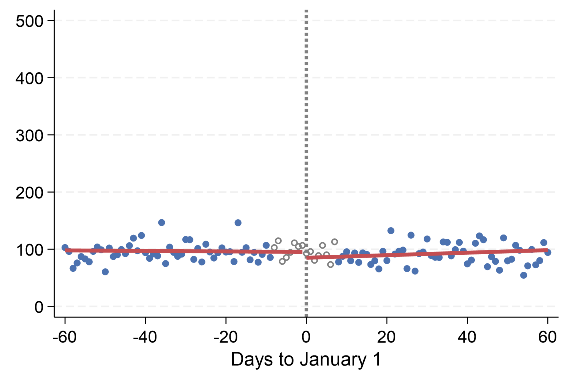
(a) Any Active Debt in Collections, Vulnerable



(b) Any Active Debt in Collections, Non-Vulnerable



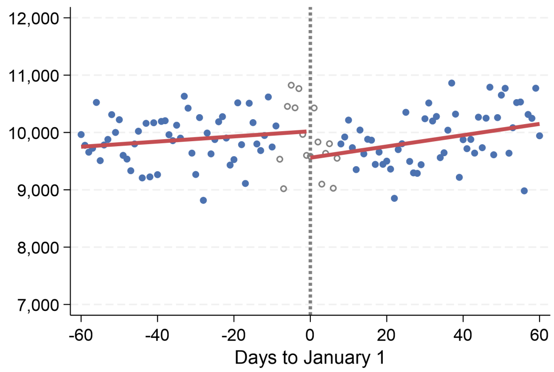
(c) Active Debt in Collections, Vulnerable



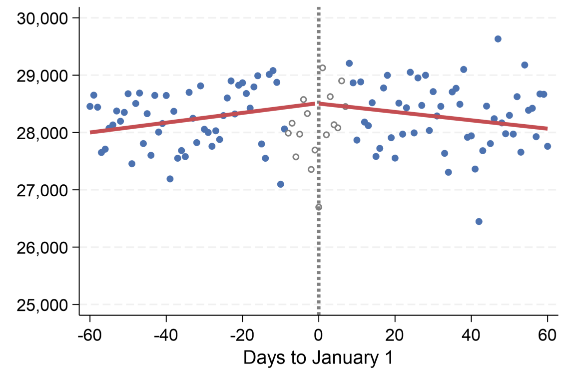
(d) Active Debt in Collections, Non-Vulnerable

Notes: This figure plots the two unpaid debt outcomes of Table 2, measured over the third post-birth year, against the date of birth relative to the January 1 cutoff. Panels (a) and (b) plot the share of mothers with any active debt in collections in any quarter of the year, in percentage points, and Panels (c) and (d) the amount of active debt in collections, averaged over the four quarters of the year. Panels (a) and (c) cover financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660, and Panels (b) and (d) cover financially non-vulnerable mothers, whose baseline score is above 660. Within each outcome, the two panels are drawn to the same y-axis scale. Each dot represents the mean across all births on a given date within the 60-day estimation bandwidth, and hollow dots mark the dates inside the 8-day donut, which are excluded from estimation. Births to the left of the vertical line are December-side births, whose families receive the first child-related tax benefit about a year earlier. The red lines are linear fits to the daily means outside the donut on either side of the cutoff, weighted by the number of births on each date and the triangular kernel described in Section 4.1, and are extrapolated through the donut. Dollar amounts are expressed in December 2025 dollars.

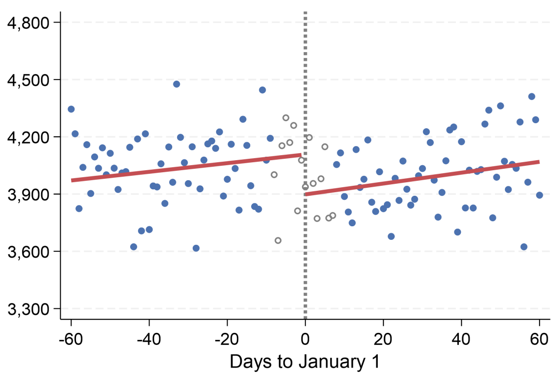
Figure 4: Discontinuity in Credit Access and Utilization in Year 3



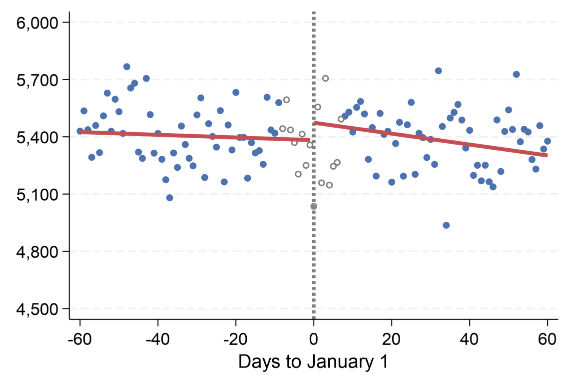
(a) Credit Limit, Financially Vulnerable



(b) Credit Limit, Financially Non-Vulnerable



(c) Credit Card Balance, Financially Vulnerable



(d) Credit Card Balance, Financially Non-Vulnerable

Notes: This figure plots the two credit access and utilization outcomes of Table 3, measured over the third post-birth year, against the date of birth relative to the January 1 cutoff. Panels (a) and (b) plot the mother's total credit limit across all open credit card accounts, and Panels (c) and (d) the total balance on those accounts, both averaged over the four quarterly snapshots of the year. Panels (a) and (c) cover financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660, and Panels (b) and (d) cover financially non-vulnerable mothers, whose baseline score is above 660. Within each outcome, the two panels are drawn to the same y-axis scale. Each dot represents the mean across all births on a given date within the 60-day estimation bandwidth, and hollow dots mark the dates inside the 8-day donut, which are excluded from estimation. Births to the left of the vertical line are December-side births, whose families receive the first child-related tax benefit about a year earlier. The red lines are linear fits to the daily means outside the donut on either side of the cutoff, weighted by the number of births on each date and the triangular kernel described in Section 4.1, and are extrapolated through the donut. Dollar amounts are expressed in December 2025 dollars.

Tables

Table 1: Summary Statistics

	All First-Time Births		Main Analysis Sample		Financially Vulnerable	
	Mean	SD	Mean	SD	Mean	SD
Panel A: Demographic Characteristics						
Mother's Age (years)	26.93	(6.34)	29.68	(5.34)	27.09	(5.08)
Mother's Education is High School or Less	37.28	(48.35)	19.37	(39.52)	34.03	(47.38)
Mother has College Degree	33.52	(47.21)	48.27	(49.97)	21.10	(40.80)
Mother is Non-Hispanic Black	5.59	(22.97)	4.83	(21.44)	9.32	(29.07)
Mother is Hispanic	43.32	(49.55)	32.41	(46.80)	47.37	(49.93)
Mother is Born Outside the United States	34.43	(47.51)	26.63	(44.20)	19.55	(39.66)
Mother Demographics Missing	5.63	(23.06)	5.63	(23.05)	5.07	(21.94)
Father's Age (years)	29.77	(7.31)	31.98	(6.38)	29.49	(6.38)
Father's Education is High School or Less	38.27	(48.60)	26.74	(44.26)	41.92	(49.34)
Father has College Degree	28.30	(45.04)	39.81	(48.95)	16.40	(37.03)
Father is Non-Hispanic Black	5.97	(23.68)	5.66	(23.10)	9.87	(29.82)
Father is Hispanic	38.20	(48.59)	29.68	(45.69)	42.04	(49.36)
Father Demographics Missing	14.21	(34.91)	10.57	(30.74)	14.43	(35.14)
Panel B: Birth Characteristics						
Principal Source of Payment is Medicaid	39.03	(48.78)	21.03	(40.75)	38.50	(48.66)
Gestation Length (days)	275.50	(15.45)	275.73	(14.96)	275.23	(16.29)
Birth Weight (grams)	3,258.84	(536.02)	3,275.99	(541.85)	3,274.80	(567.34)
Apgar Score at 5 Minutes	8.82	(0.73)	8.81	(0.74)	8.80	(0.77)
Labor is Induced	20.36	(40.26)	22.26	(41.60)	22.21	(41.57)
Delivery is by C-section	29.53	(45.62)	32.28	(46.75)	33.62	(47.24)
Panel C: Predicted Income, Tax Value, and Sample Selection						
Predicted Earned Income	105,584.95	(93,110.36)	140,977.52	(97,632.63)	85,083.00	(64,368.81)
Predicted Tax Value of First Birth	3,517.70	(1,218.45)	3,422.07	(1,212.29)	3,790.68	(1,174.01)
Mother is in Main Analysis Sample	50.33	(50.00)	–		–	
Panel D: Baseline Financial Characteristics						
Has Credit Card	–		83.87	(36.78)	64.74	(47.78)
Credit Card Balance	–		4,492.90	(8,182.74)	3,944.47	(8,197.67)
Credit Limit	–		19,398.29	(25,851.08)	6,995.67	(15,109.11)
Credit Card Utilization Rate	–		33.53	(38.92)	64.42	(43.13)
Has Auto Loan	–		37.89	(48.51)	33.79	(47.30)
Auto Loan Balance	–		8,338.82	(14,680.76)	7,518.61	(13,958.80)
Has Mortgage	–		18.42	(38.76)	4.48	(20.69)
Mortgage Balance	–		99,337.54	(253,621.66)	20,642.96	(115,659.96)
Any 30-Day Delinquency	–		5.48	(22.76)	13.70	(34.39)
Any 90-Day Delinquency	–		2.58	(15.87)	6.62	(24.87)
Any Active Debt in Collections	–		10.92	(31.18)	26.70	(44.24)
Active Debt in Collections	–		242.43	(1,401.25)	609.45	(2,168.79)
Any Bankruptcy Record	–		1.52	(12.22)	2.68	(16.14)
Credit Score	–		680.83	(94.03)	580.35	(58.28)
Mother is Financially Vulnerable (Score ≤ 660)	–		38.13	(48.57)	–	
Observations	956,973		481,616		183,625	

Notes: This table reports summary statistics for three samples. The All First-Time Births columns cover all first-time singleton births in re-centered years 2006–2023; the Main Analysis Sample columns cover the main analysis sample constructed in Section 3.4; and the Financially Vulnerable columns further restrict to mothers whose baseline credit score is at or below 660. All three samples are limited to births within the 60-day bandwidth and outside the 8-day donut, as defined in Section 4.1. Baseline credit outcomes are measured at the baseline quarter $q = -8$. All binary variables are multiplied by 100 to represent percentage points, predicted income and tax value are expressed in 2025 dollars, and credit outcome dollar values are expressed in December 2025 dollars.

Table 2: Effects on Unpaid Debt

	(1) Any Active Debt in Collections (%)	(2) Active Debt in Collections ($)
Panel A: Financially Vulnerable		
Year 1	-1.44^{**}	-60.71^{**}
	(0.57)	(25.89)
January-Side Mean	42.01	771.84
Observations	183,625	183,625
Year 2	-1.39^{**}	-69.80^{***}
	(0.58)	(26.62)
January-Side Mean	42.50	813.23
Observations	183,625	183,625
Year 3	-1.62^{***}	-59.74^{**}
	(0.58)	(25.37)
January-Side Mean	40.13	707.08
Observations	183,625	183,625
Panel B: Financially Non-Vulnerable		
Year 1	0.17	4.44
	(0.21)	(5.83)
January-Side Mean	4.44	50.55
Observations	297,990	297,990
Year 2	0.35	0.31
	(0.22)	(7.91)
January-Side Mean	5.42	85.73
Observations	297,990	297,990
Year 3	0.18	7.01
	(0.22)	(8.59)
January-Side Mean	5.34	91.06
Observations	297,990	297,990

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1. Panel A covers financially vulnerable mothers, whose baseline credit score is at or below 660, and Panel B covers non-vulnerable mothers, whose baseline score is above 660. Each cell reports the estimate from a separate regression, with the outcome measured over the post-birth year indicated by the row. Any Active Debt in Collections is an indicator that equals one if the mother has any active debt in collections in any quarter of the year and is multiplied by 100 to represent percentage points; Active Debt in Collections is averaged over the four quarters of the year and expressed in December 2025 dollars. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3: Effects on Credit Access and Utilization

	(1) Credit Limit ($)	(2) Credit Card Balance ($)
Panel A: Financially Vulnerable		
Year 1	327.87**	99.29
	(134.67)	(66.95)
January-Side Mean	8,999.10	3,740.01
Observations	183,625	183,625
Year 2	424.32***	219.64***
	(158.16)	(75.83)
January-Side Mean	9,557.87	3,891.21
Observations	183,625	183,625
Year 3	435.86**	211.73***
	(172.53)	(81.78)
January-Side Mean	9,912.10	3,998.88
Observations	183,625	183,625
Panel B: Financially Non-Vulnerable		
Year 1	−92.25	−230.83***
	(169.06)	(66.50)
January-Side Mean	29,418.96	5,138.72
Observations	297,990	297,990
Year 2	13.13	−167.36**
	(193.76)	(72.55)
January-Side Mean	28,996.25	5,305.33
Observations	297,990	297,990
Year 3	40.37	−156.73**
	(210.96)	(76.88)
January-Side Mean	28,287.58	5,387.82
Observations	297,990	297,990

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1. Panel A covers financially vulnerable mothers, whose baseline credit score is at or below 660, and Panel B covers non-vulnerable mothers, whose baseline score is above 660. Each cell reports the estimate from a separate regression, with the outcome measured over the post-birth year indicated by the row. Credit Limit is the mother's total credit limit across all open credit card accounts, and Credit Card Balance is the total balance on those accounts. Both outcomes are averaged over the four quarterly snapshots of each year and expressed in December 2025 dollars. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4: Effects on Overall Financial Health

	(1) Credit Score	(2) Credit Score is Prime (%)
Panel A: Financially Vulnerable		
Year 1	2.43***	1.07**
	(0.79)	(0.44)
January-Side Mean	607.62	28.88
Observations	182,834	183,625
Year 2	0.97	0.33
	(0.85)	(0.46)
January-Side Mean	611.23	30.16
Observations	182,340	183,625
Year 3	0.18	−0.21
	(0.88)	(0.47)
January-Side Mean	615.98	31.55
Observations	181,912	183,625
Panel B: Financially Non-Vulnerable		
Year 1	1.50***	0.65***
	(0.46)	(0.23)
January-Side Mean	753.45	91.16
Observations	297,749	297,990
Year 2	0.92*	0.42*
	(0.52)	(0.25)
January-Side Mean	754.13	89.62
Observations	297,415	297,990
Year 3	0.79	0.31
	(0.55)	(0.26)
January-Side Mean	755.92	88.86
Observations	297,112	297,990

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1. Panel A covers financially vulnerable mothers, whose baseline credit score is at or below 660, and Panel B covers non-vulnerable mothers, whose baseline score is above 660. Each cell reports the estimate from a separate regression, with the outcome measured over the post-birth year indicated by the row. The credit score is averaged over the quarters in which the mother has a valid score. Credit Score is Prime measures the share of the year's quarters in which the mother's score exceeds 660, and is multiplied by 100 to represent percentage points. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

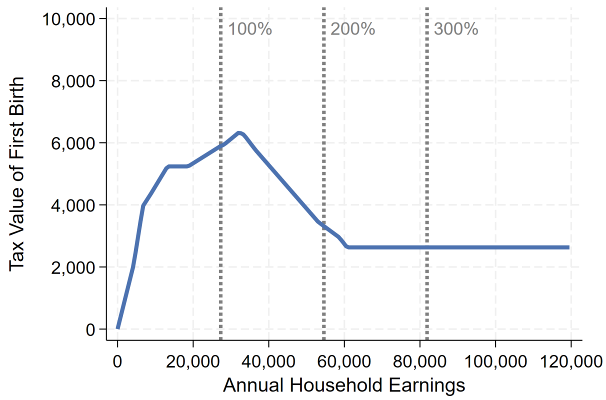
Table 5: Effects on Durable Consumption

	(1) Has Auto Loan (%)	(2) Has Mortgage (%)
Panel A: Financially Vulnerable		
Year 1	1.10*	0.42
	(0.57)	(0.34)
January-Side Mean	45.79	10.05
Observations	183,625	183,625
Year 2	0.84	1.02***
	(0.59)	(0.37)
January-Side Mean	45.65	11.74
Observations	183,625	183,625
Year 3	0.24	0.68*
	(0.61)	(0.40)
January-Side Mean	45.38	13.26
Observations	183,625	183,625
Panel B: Financially Non-Vulnerable		
Year 1	0.07	0.20
	(0.45)	(0.43)
January-Side Mean	50.91	45.06
Observations	297,990	297,990
Year 2	−0.07	−0.01
	(0.47)	(0.44)
January-Side Mean	49.40	48.75
Observations	297,990	297,990
Year 3	−0.55	0.32
	(0.48)	(0.45)
January-Side Mean	47.88	51.22
Observations	297,990	297,990

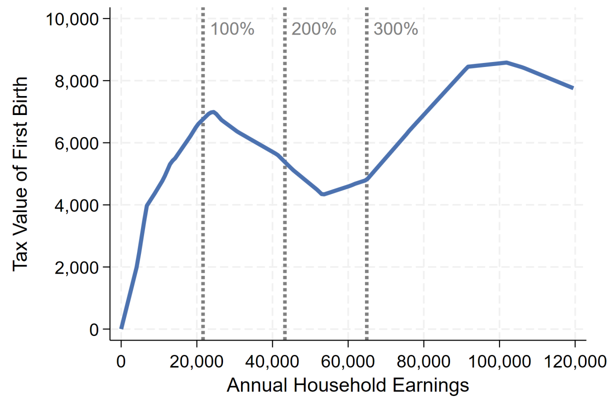
Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1. Panel A covers financially vulnerable mothers, whose baseline credit score is at or below 660, and Panel B covers non-vulnerable mothers, whose baseline score is above 660. Each cell reports the estimate from a separate regression, with the outcome measured over the post-birth year indicated by the row. Both indicators equal one if the mother carries a positive balance of the given type in any quarter of the year and are multiplied by 100 to represent percentage points. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

A Appendix Figures

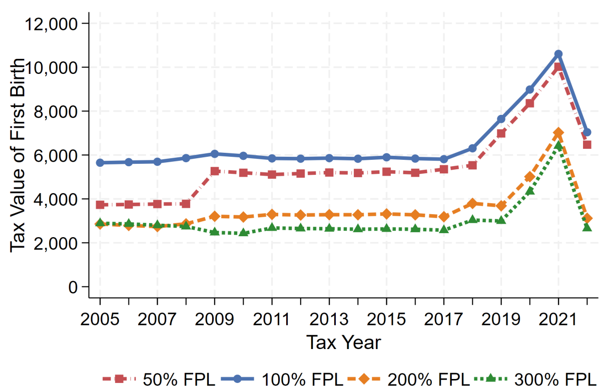
Figure A.1: Tax Value of First Birth in California, by Filing Status



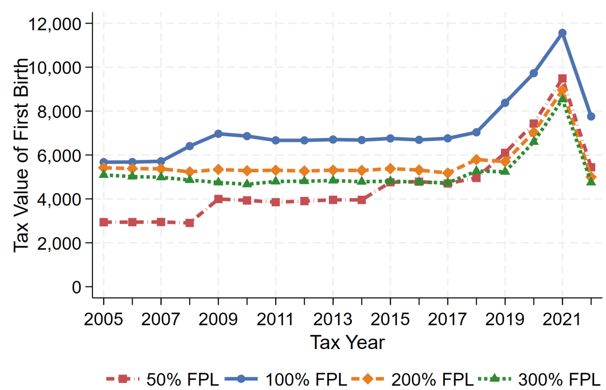
(a) By Household Earnings, Married Filing Jointly



(b) By Household Earnings, Single



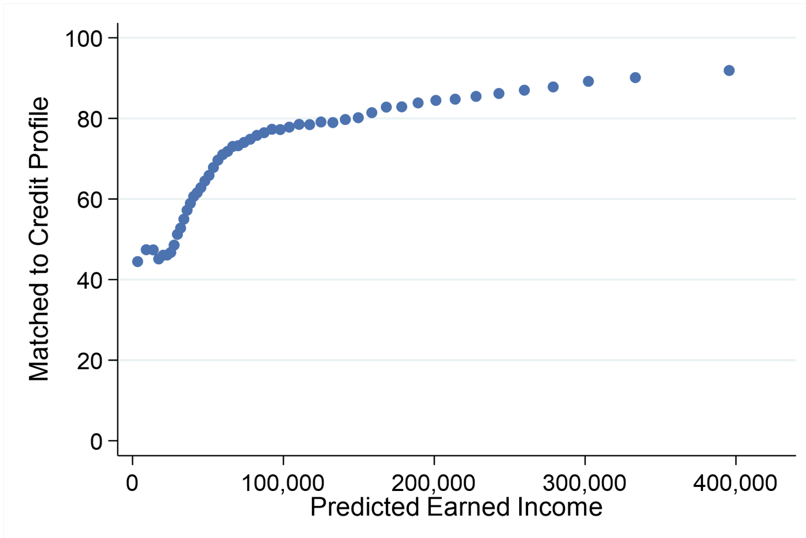
(c) By Tax Year, Married Filing Jointly



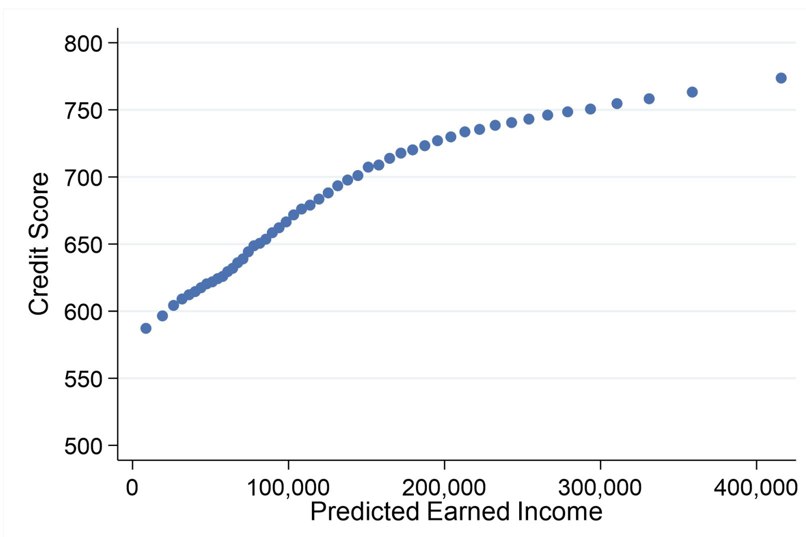
(d) By Tax Year, Single

Notes: This figure plots the tax value of a first child in California. Panels (a) and (c) cover a married couple filing jointly, and Panels (b) and (d) cover an unmarried parent filing as single. The tax value is defined as the change in the household's total tax liability from claiming the newborn child as a dependent and is simulated through the NBER TAXSIM program. Panels (a) and (b) plot this value as a function of household earnings for tax year 2015, and dashed vertical lines mark 100%, 200%, and 300% of the Federal Poverty Level (FPL). Panels (c) and (d) plot this value for households with earnings fixed at 50%, 100%, 200%, and 300% of the FPL in each tax year from 2005 to 2022. The FPL is the guideline for a family of three for the joint filers and for a family of two for the single filer. In all simulations, wage income is the sole source of income, divided equally between the two spouses for the married couple, and no childcare expenses are claimed on the tax return. The single filer also becomes eligible for the head of household filing status by claiming the child. All dollars are in 2025 values.

Figure A.2: Linkage Rate and Credit Score by Predicted Earned Income



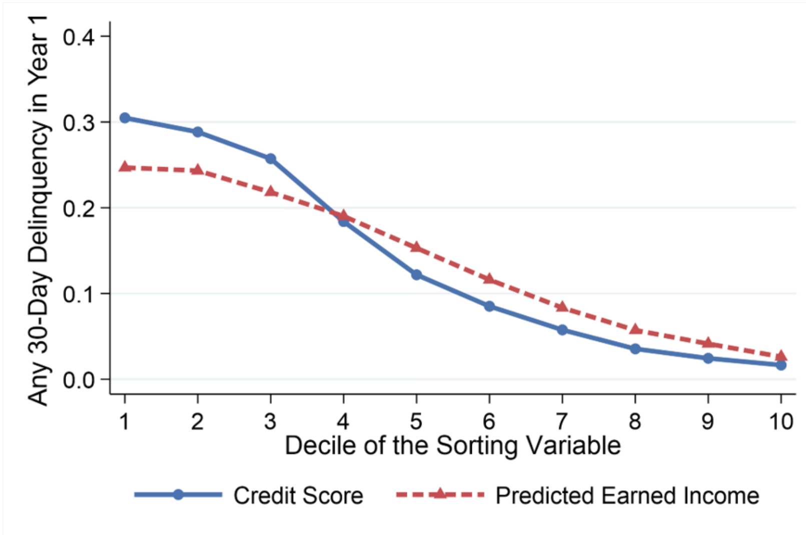
(a) Linkage Rate



(b) Credit Score

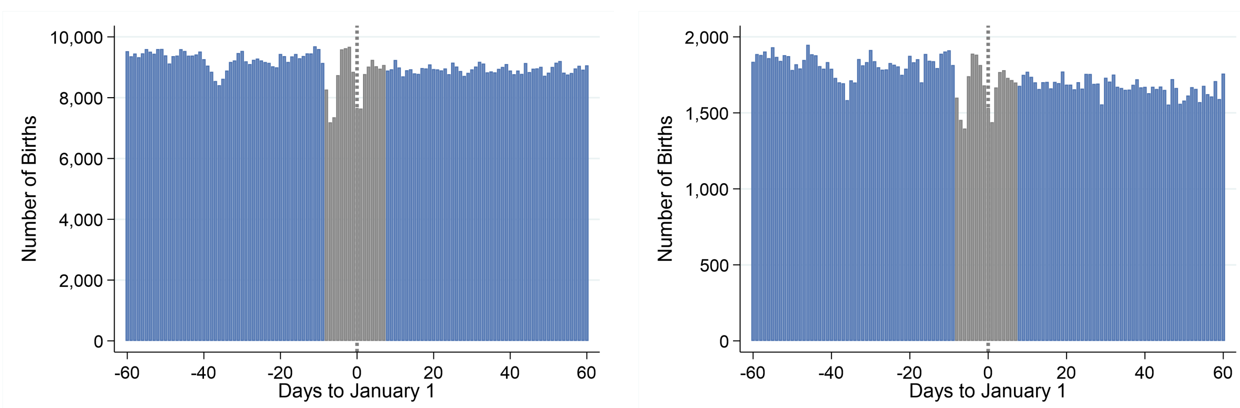
Notes: This figure plots the linkage rate and the baseline credit score by 50 equal-sized bins of predicted earned income. Panel (a) plots the share of mothers who are ever linked to the credit panel, in percentage points, among all first-time singleton births in re-centered years 2006–2023. Panel (b) plots the mother's credit score, measured at the baseline quarter $q = -8$, among mothers who satisfy the sample restrictions of Section 3.4. Each dot represents the mean within a bin.

Figure A.3: Delinquency by Decile of Baseline Credit Score and of Predicted Earned Income



Notes: This figure presents the relationship between subsequent delinquency and each of two baseline characteristics, the credit score and predicted earned income, for mothers of January-side births who satisfy the sample restrictions of Section 3.4. Mothers are sorted separately into deciles of each characteristic, and each dot represents the share of mothers with any 30-day delinquency in the first year after the first birth ($q = 0$ to $q = +3$) within a decile. The Credit Score series sorts mothers by their credit score measured at the baseline quarter $q = -8$, and the Predicted Earned Income series sorts them by their predicted earned income.

Figure A.4: Density of Births Around the January 1 Cutoff

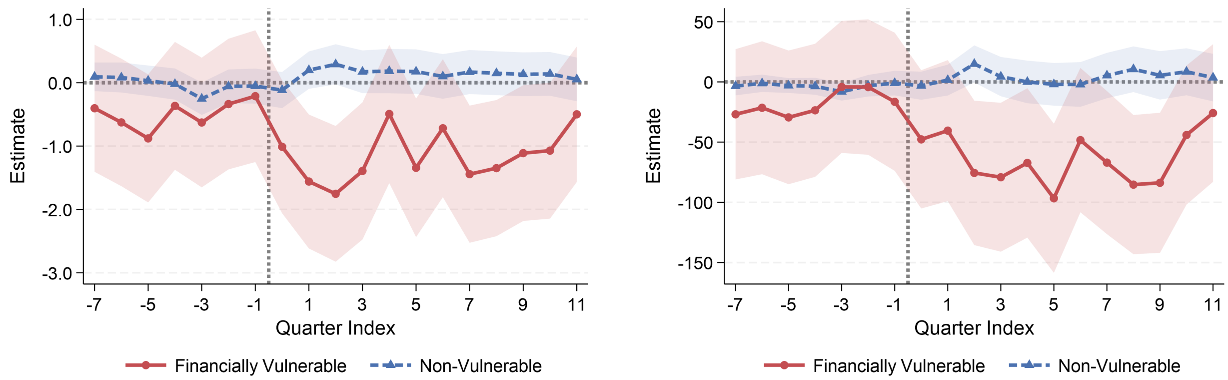


(a) All First-Time Births

(b) Financially Vulnerable

Notes: This figure plots the distribution of births by date of birth relative to the January 1 cutoff, pooling all re-centered years. Panel (a) covers all first-time singleton births in re-centered years 2006–2023, and Panel (b) covers financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660. Each bar represents the number of births on a given date within the 60-day estimation bandwidth, and grey bars mark the dates inside the 8-day donut, which are excluded from estimation.

Figure A.5: Quarterly Effects on Unpaid Debt

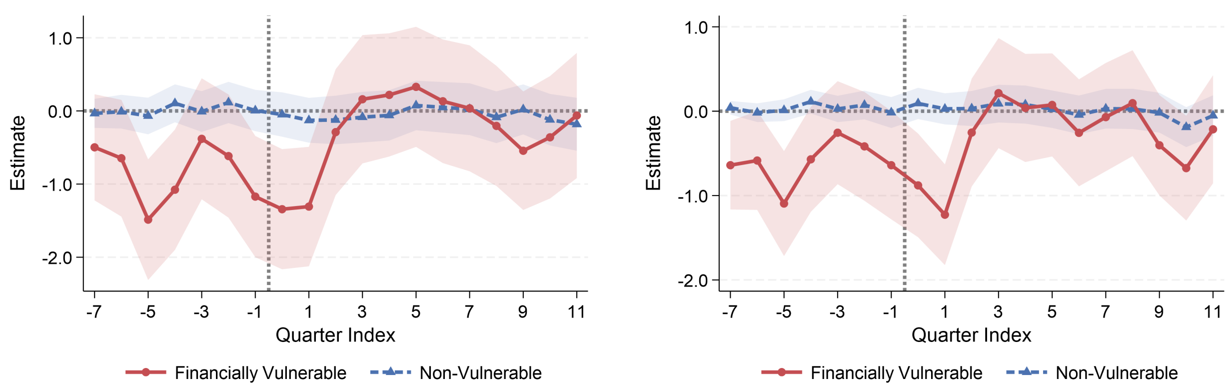


(a) Any Active Debt in Collections

(b) Active Debt in Collections

Notes: This figure plots quarter-by-quarter RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1 and estimated separately for each quarter from $q = -7$ to $q = +11$, as described in Section 4.1. Panel (a) plots whether the mother has any active debt in collections in the quarter, and Panel (b) the amount of active debt in collections; Table 2 reports the corresponding year-level estimates. Solid red circles trace the estimates for financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660, and dashed blue triangles trace the estimates for financially non-vulnerable mothers, whose baseline score is above 660; shaded bands are 95% confidence intervals based on heteroskedasticity-robust standard errors. The vertical dashed line marks the January 1 cutoff, which sits between quarters −1 and 0. The binary outcome in Panel (a) is multiplied by 100 to represent percentage points, and dollar amounts are expressed in December 2025 dollars.

Figure A.6: Quarterly Effects on Unpaid Debt – Additional Outcomes

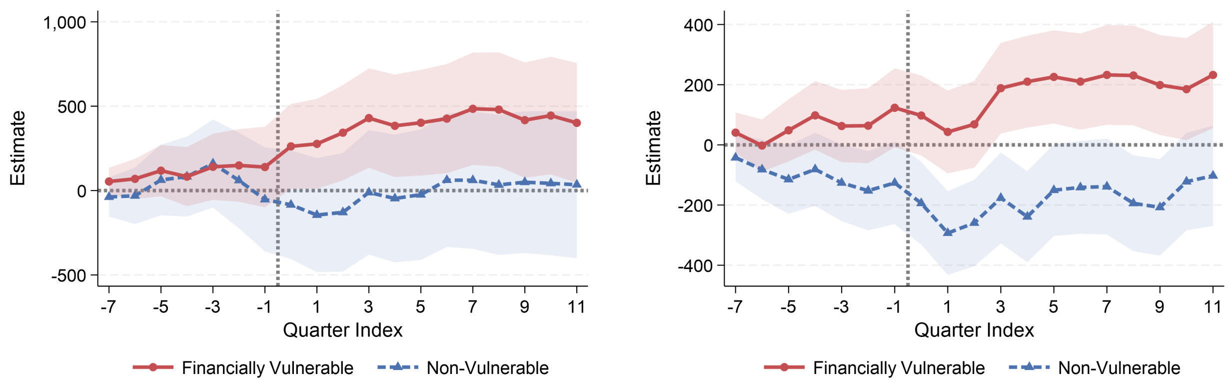


(a) Any 30-Day Delinquency

(b) Any 90-Day Delinquency

Notes: This figure plots quarter-by-quarter RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1 and estimated separately for each quarter from $q = -7$ to $q = +11$, as described in Section 4.1. Panel (a) plots whether the mother has any account at least 30 days past due in the quarter, and Panel (b) whether she has any account at least 90 days past due; Table B.3 reports the corresponding year-level estimates. Solid red circles trace the estimates for financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660, and dashed blue triangles trace the estimates for financially non-vulnerable mothers, whose baseline score is above 660; shaded bands are 95% confidence intervals based on heteroskedasticity-robust standard errors. The vertical dashed line marks the January 1 cutoff, which sits between quarters −1 and 0. Binary outcomes are multiplied by 100 to represent percentage points.

Figure A.7: Quarterly Effects on Credit Access and Utilization

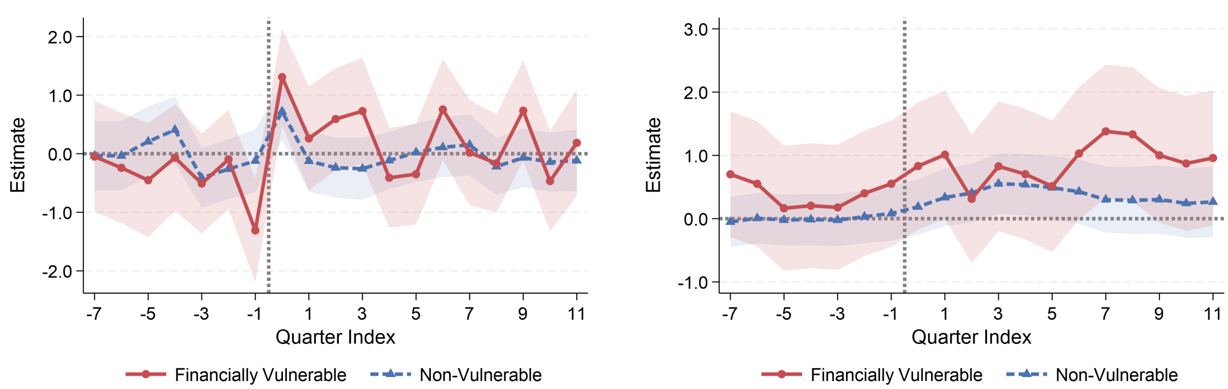


(a) Credit Limit

(b) Credit Card Balance

Notes: This figure plots quarter-by-quarter RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1 and estimated separately for each quarter from $q = -7$ to $q = +11$, as described in Section 4.1. Panel (a) plots the mother's total credit limit across all open credit card accounts, and Panel (b) the total balance on those accounts; Table 3 reports the corresponding year-level estimates. Solid red circles trace the estimates for financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660, and dashed blue triangles trace the estimates for financially non-vulnerable mothers, whose baseline score is above 660; shaded bands are 95% confidence intervals based on heteroskedasticity-robust standard errors. The vertical dashed line marks the January 1 cutoff, which sits between quarters -1 and 0. Dollar amounts are expressed in December 2025 dollars.

Figure A.8: Quarterly Effects on Credit Access and Utilization – Additional Outcomes

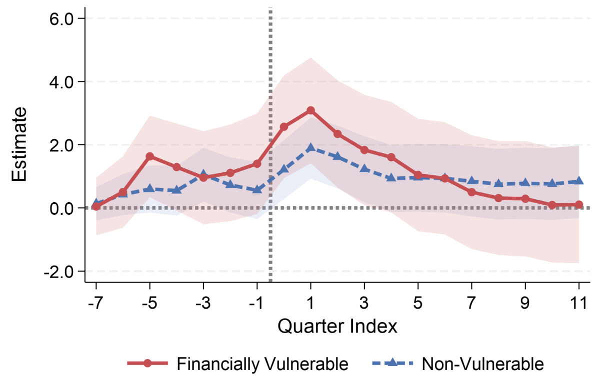


(a) Any Credit Card Inquiry

(b) Has Credit Card

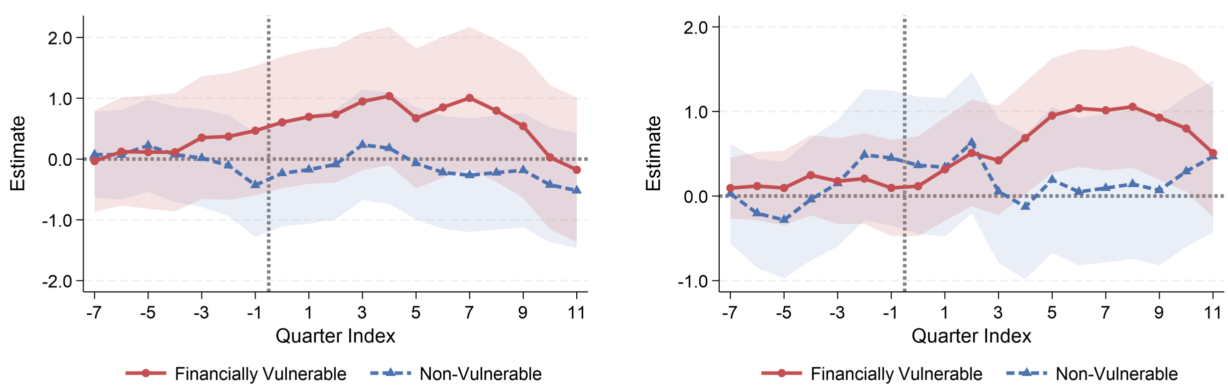
Notes: This figure plots quarter-by-quarter RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1 and estimated separately for each quarter from $q = -7$ to $q = +11$, as described in Section 4.1. Panel (a) plots an indicator for any credit card inquiry during the quarter, and Panel (b) an indicator for holding any open credit card account in the quarter; Table B.4 reports the corresponding year-level estimates. Solid red circles trace the estimates for financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660, and dashed blue triangles trace the estimates for financially non-vulnerable mothers, whose baseline score is above 660; shaded bands are 95% confidence intervals based on heteroskedasticity-robust standard errors. The vertical dashed line marks the January 1 cutoff, which sits between quarters -1 and 0. Binary outcomes are multiplied by 100 to represent percentage points.

Figure A.9: Quarterly Effects on Overall Financial Health



Notes: This figure plots quarter-by-quarter RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1 and estimated separately for each quarter from $q = -7$ to $q = +11$, as described in Section 4.1. The outcome is the mother's credit score in the quarter, among mothers with a valid score; Table 4 reports the corresponding year-level estimates. Solid red circles trace the estimates for financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660, and dashed blue triangles trace the estimates for financially non-vulnerable mothers, whose baseline score is above 660; shaded bands are 95% confidence intervals based on heteroskedasticity-robust standard errors. The vertical dashed line marks the January 1 cutoff, which sits between quarters -1 and 0. Estimates are in credit score points.

Figure A.10: Quarterly Effects on Durable Consumption

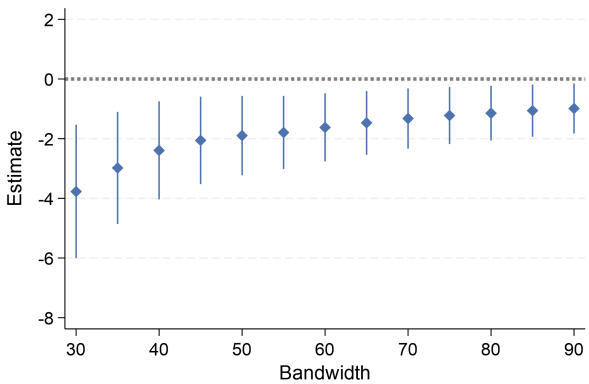


(a) Has Auto Loan

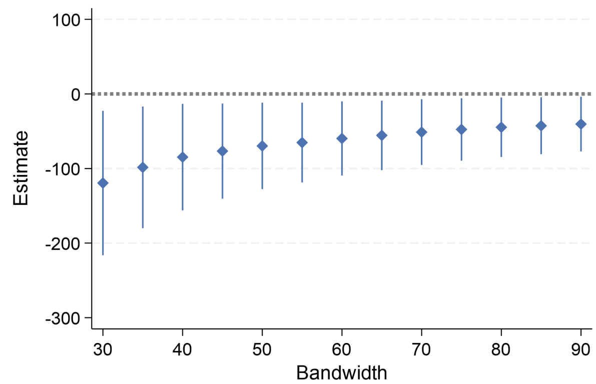
(b) Has Mortgage

Notes: This figure plots quarter-by-quarter RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1 and estimated separately for each quarter from $q = -7$ to $q = +11$, as described in Section 4.1. Panel (a) plots an indicator for carrying a positive auto loan balance in the quarter, and Panel (b) an indicator for carrying a positive mortgage balance in the quarter; Table 5 reports the corresponding year-level estimates. Solid red circles trace the estimates for financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660, and dashed blue triangles trace the estimates for financially non-vulnerable mothers, whose baseline score is above 660; shaded bands are 95% confidence intervals based on heteroskedasticity-robust standard errors. The vertical dashed line marks the January 1 cutoff, which sits between quarters -1 and 0. Binary outcomes are multiplied by 100 to represent percentage points.

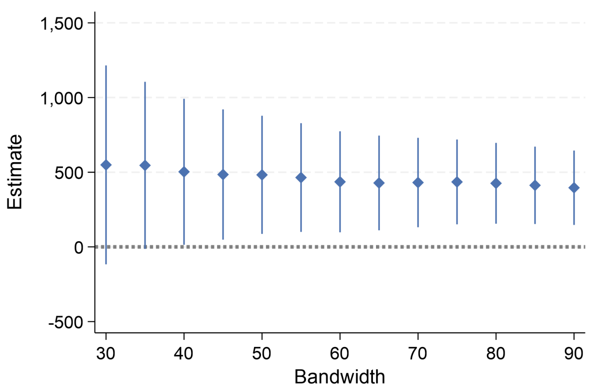
Figure A.11: Robustness Checks – Bandwidth



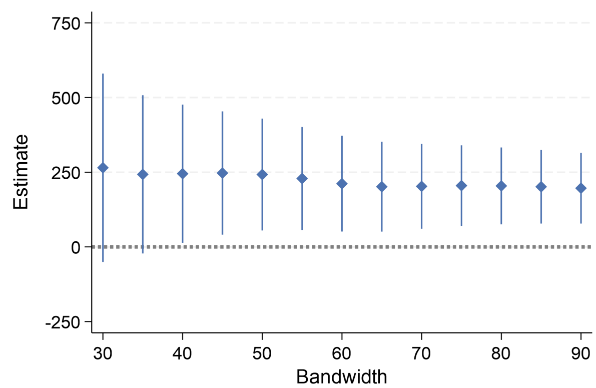
(a) Any Active Debt in Collections



(b) Active Debt in Collections



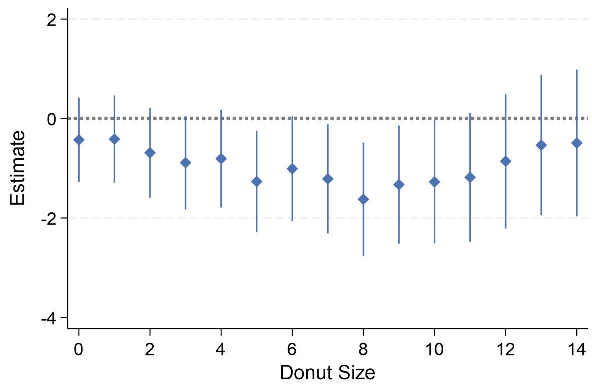
(c) Credit Limit



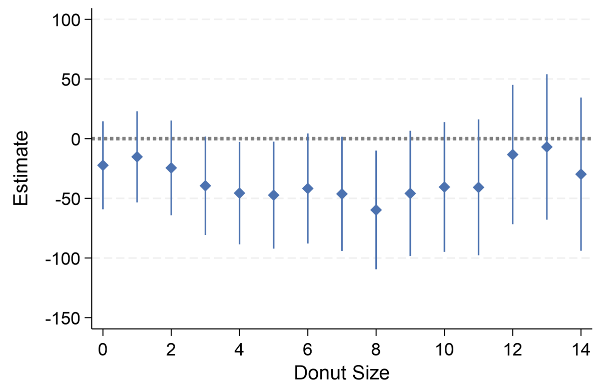
(d) Credit Card Balance

Notes: This figure plots RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1, re-estimated at each bandwidth from 30 to 90 days while holding the donut at 8 days, for financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660. Each outcome is measured over the third post-birth year. Vertical spikes are 95% confidence intervals based on heteroskedasticity-robust standard errors. The binary outcome in Panel (a) is multiplied by 100 to represent percentage points, and dollar amounts are expressed in December 2025 dollars.

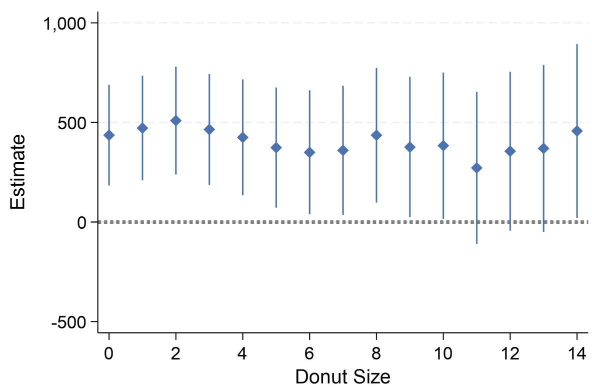
Figure A.12: Robustness Checks – Donut Size



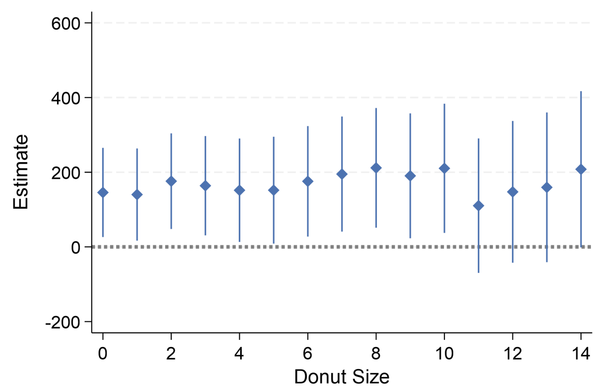
(a) Any Active Debt in Collections



(b) Active Debt in Collections



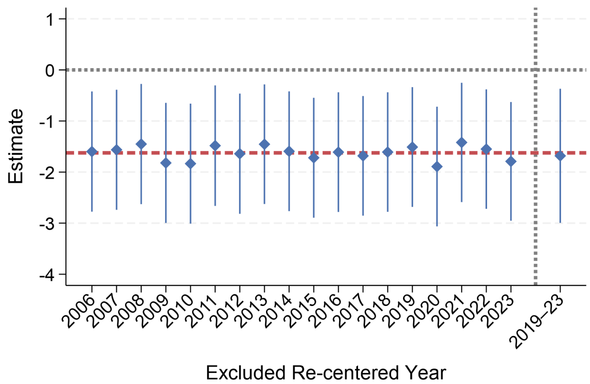
(c) Credit Limit



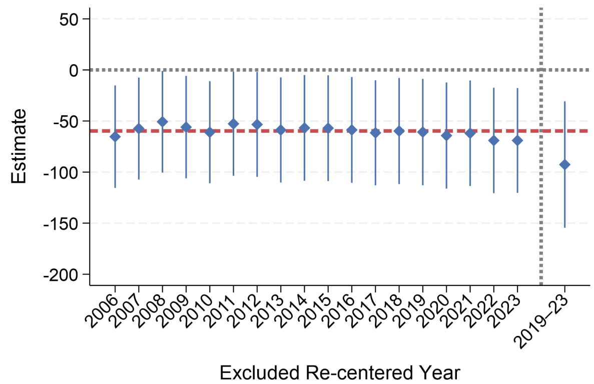
(d) Credit Card Balance

Notes: This figure plots RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1, re-estimated at each donut size from 0 to 14 days while holding the bandwidth at 60 days, for financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660. Each outcome is measured over the third post-birth year. Vertical spikes are 95% confidence intervals based on heteroskedasticity-robust standard errors. The binary outcome in Panel (a) is multiplied by 100 to represent percentage points, and dollar amounts are expressed in December 2025 dollars.

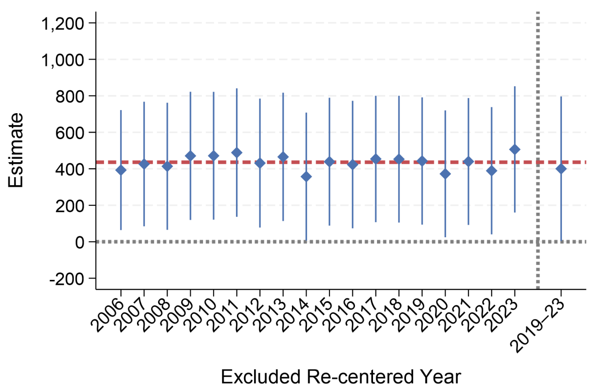
Figure A.13: Robustness Checks – Tax Regime



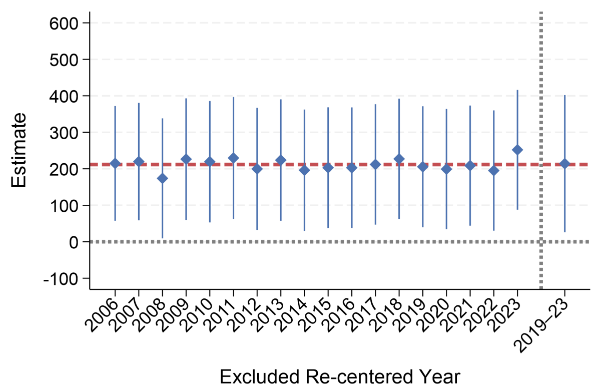
(a) Any Active Debt in Collections



(b) Active Debt in Collections



(c) Credit Limit



(d) Credit Card Balance

Notes: This figure plots RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1, re-estimated excluding one re-centered year at a time, for financially vulnerable mothers in the main analysis sample constructed in Section 3.4, whose baseline credit score is at or below 660. Each outcome is measured over the third post-birth year. The rightmost estimate excludes re-centered years 2019 through 2023 jointly. Vertical spikes are 95% confidence intervals based on heteroskedasticity-robust standard errors. The horizontal dashed red line marks the estimate obtained under the main specification, reported in Table 2 and Table 3. The binary outcome in Panel (a) is multiplied by 100 to represent percentage points, and dollar amounts are expressed in December 2025 dollars.

B Appendix Tables

Table B.1: Smoothness of Demographic and Birth Covariates

	All First-Time Births		Financially Vulnerable	
Variable Name	Coefficient	Jan-Side Mean	Coefficient	Jan-Side Mean
Panel A: Demographic Characteristics				
Mother's Age (years)	−0.03	26.95	0.06	27.17
	(0.04)		(0.07)	
Mother's Education is High School or Less	−0.38	36.98	−0.46	33.46
	(0.27)		(0.61)	
Mother has College Degree	0.04	33.74	0.60	21.39
	(0.26)		(0.53)	
Mother is Non-Hispanic Black	−0.03	5.58	−0.39	9.20
	(0.13)		(0.37)	
Mother is Hispanic	0.11	43.08	0.96	47.16
	(0.30)		(0.68)	
Mother is Born Outside the United States	0.05	34.11	0.58	19.41
	(0.27)		(0.51)	
Mother Demographics Missing	−0.04	5.71	0.42	5.11
	(0.14)		(0.30)	
Father's Age (years)	−0.02	29.78	−0.04	29.56
	(0.04)		(0.09)	
Father's Education is High School or Less	−0.21	38.09	−0.65	41.47
	(0.27)		(0.64)	
Father has College Degree	−0.14	28.39	−0.31	16.59
	(0.25)		(0.47)	
Father is Non-Hispanic Black	0.05	5.96	−0.30	9.79
	(0.13)		(0.38)	
Father is Hispanic	0.21	38.01	1.05	41.68
	(0.29)		(0.68)	
Father Demographics Missing	−0.26	14.19	0.24	14.34
	(0.21)		(0.49)	
Panel B: Birth Characteristics				
Principal Source of Payment is Medicaid	−0.12	38.81	−0.51	38.10
	(0.28)		(0.63)	
Gestation Length (days)	0.21**	275.47	0.59***	275.24
	(0.09)		(0.21)	
Pre-Term Birth	−0.15	8.00	−0.33	8.58
	(0.15)		(0.36)	
Birth Weight (grams)	2.92	3,259.76	19.97***	3,274.78
	(3.02)		(7.26)	
Low Birth Weight	−0.20	6.43	−0.48	6.96
	(0.14)		(0.33)	
Apgar Score at 5 Minutes	0.001	8.82	−0.005	8.80
	(0.004)		(0.010)	
Labor is Induced	0.33	20.51	0.46	22.33
	(0.23)		(0.53)	
Delivery is by C-section	0.75***	29.53	2.10***	33.43
	(0.26)		(0.61)	
Panel C: Predicted Income, Tax Value, and Sample Selection				
Predicted Earned Income	−219.34	105,860.23	259.85	85,747.87
	(513.41)		(827.32)	
Predicted Tax Value of First Birth	8.01	3,511.45	13.86	3,781.35
	(4.95)		(8.59)	
Mother is in Main Analysis Sample	0.39	49.69	–	–
	(0.28)			

Notes: This table reports the donut RD estimate from separate regressions where the listed characteristic serves as the outcome variable, following the specification of Equation 1 with re-centered year fixed effects as the only controls. Heteroskedasticity-robust standard errors are in parentheses. Jan-Side Mean is the unweighted mean among January-side births. The All First-Time Births columns cover all first-time singleton births in re-centered years 2006–2023; the Financially Vulnerable columns restrict to the main analysis sample constructed in Section 3.4, further limited to mothers whose baseline credit score is at or below 660. Mother is in Main Analysis Sample indicates that the birth meets every requirement of that sample. All binary variables are multiplied by 100 to represent percentage points, and dollar values are expressed in 2025 dollars. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.2: Smoothness of Baseline Financial Covariates

	Main Analysis Sample		Financially Vulnerable	
Variable Name	Coefficient	Jan-Side Mean	Coefficient	Jan-Side Mean
Has Credit Card	0.15	84.03	0.70	64.90
	(0.30)		(0.61)	
Credit Card Balance	19.84	4,502.02	−71.50	3,982.41
	(63.71)		(102.89)	
Credit Limit	−151.23	19,466.86	−31.88	7,051.92
	(201.76)		(187.69)	
Credit Card Utilization Rate	0.35	33.43	−0.52	64.43
	(0.34)		(0.70)	
Has Auto Loan	0.48	37.95	0.94	33.91
	(0.39)		(0.61)	
Auto Loan Balance	149.75	8,309.31	331.01*	7,528.80
	(116.06)		(174.86)	
Has Mortgage	−0.66**	18.39	−0.01	4.56
	(0.31)		(0.26)	
Mortgage Balance	−4,365.52**	98,633.49	−1,304.71	20,864.62
	(1,986.82)		(1,420.92)	
Any 30-Day Delinquency	−0.05	5.42	−0.29	13.69
	(0.18)		(0.44)	
Any 90-Day Delinquency	−0.11	2.54	−0.37	6.59
	(0.13)		(0.32)	
Any Active Debt in Collections	−0.09	10.72	−0.44	26.42
	(0.25)		(0.57)	
Active Debt in Collections	−1.26	237.07	−4.91	599.68
	(11.40)		(28.20)	
Any Bankruptcy Record	−0.05	1.52	−0.10	2.69
	(0.10)		(0.21)	
Credit Score	−0.92	681.58	0.66	580.75
	(0.74)		(0.75)	
Financial Vulnerability Index	0.07	13.98	−0.03	26.73
	(0.10)		(0.11)	
Mother is Financially Vulnerable (Score ≤ 660)	0.23	37.72	–	–
	(0.39)			
Mother is in Top 35% of Vulnerability Index	0.26	34.50	–	–
	(0.38)			

Notes: This table reports the donut RD estimate from separate regressions where the listed baseline credit outcome, measured at the baseline quarter $q = -8$, serves as the outcome variable, following the specification of Equation 1 with re-centered year fixed effects as the only controls. Heteroskedasticity-robust standard errors are in parentheses. Jan-Side Mean is the unweighted mean among January-side births. The Main Analysis Sample columns cover the main analysis sample constructed in Section 3.4; the Financially Vulnerable columns further restrict to mothers whose baseline credit score is at or below 660. All binary variables, the credit card utilization rate, and the financial vulnerability index are multiplied by 100 to represent percentage points, and dollar values are expressed in December 2025 dollars. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.3: Effects on Unpaid Debt – Additional Outcomes

	(1) Any 30-Day Delinquency (%)	(2) Any 90-Day Delinquency (%)
Panel A: Financially Vulnerable		
Year 1	-1.03^{*}	-1.31^{***}
	(0.56)	(0.45)
January-Side Mean	27.30	15.21
Observations	183,625	183,625
Year 2	0.80	0.01
	(0.56)	(0.45)
January-Side Mean	26.31	14.55
Observations	183,625	183,625
Year 3	−0.66	−0.43
	(0.55)	(0.45)
January-Side Mean	25.40	14.21
Observations	183,625	183,625
Panel B: Financially Non-Vulnerable		
Year 1	−0.35	−0.02
	(0.25)	(0.15)
January-Side Mean	6.39	2.33
Observations	297,990	297,990
Year 2	0.09	−0.01
	(0.26)	(0.17)
January-Side Mean	7.21	2.89
Observations	297,990	297,990
Year 3	−0.24	−0.24
	(0.26)	(0.17)
January-Side Mean	7.28	3.10
Observations	297,990	297,990

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1. Panel A covers financially vulnerable mothers, whose baseline credit score is at or below 660, and Panel B covers non-vulnerable mothers, whose baseline score is above 660. Each cell reports the estimate from a separate regression, with the outcome measured over the post-birth year indicated by the row. Any 30-Day Delinquency equals one if the mother has any account at least 30 days past due in any quarter of the year, and Any 90-Day Delinquency equals one if she has any account at least 90 days past due in any quarter of the year; both indicators are multiplied by 100 to represent percentage points. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.4: Effects on Credit Access and Utilization – Additional Outcomes

	(1) Any Credit Card Inquiry (%)	(2) Has Credit Card (%)
Panel A: Financially Vulnerable		
Year 1	1.27**	0.88*
	(0.62)	(0.50)
January-Side Mean	38.75	72.51
Observations	183,625	183,625
Year 2	−0.08	0.70
	(0.62)	(0.52)
January-Side Mean	38.33	72.03
Observations	183,625	183,625
Year 3	−0.06	1.16**
	(0.62)	(0.52)
January-Side Mean	38.03	72.10
Observations	183,625	183,625
Panel B: Financially Non-Vulnerable		
Year 1	0.00	0.26
	(0.42)	(0.21)
January-Side Mean	22.89	95.23
Observations	297,990	297,990
Year 2	0.29	0.54**
	(0.42)	(0.23)
January-Side Mean	22.49	94.07
Observations	297,990	297,990
Year 3	−0.24	0.29
	(0.42)	(0.25)
January-Side Mean	22.50	92.85
Observations	297,990	297,990

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1. Panel A covers financially vulnerable mothers, whose baseline credit score is at or below 660, and Panel B covers non-vulnerable mothers, whose baseline score is above 660. Each cell reports the estimate from a separate regression, with the outcome measured over the post-birth year indicated by the row. Any Credit Card Inquiry equals one if the mother has any credit card inquiry during the year, and Has Credit Card equals one if she has any open credit card account in any quarter of the year; both indicators are multiplied by 100 to represent percentage points. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.5: Effects on Neighborhood Quality

	(1) Median Income ($)	(2) Poverty Share (%)	(3) Future Income ($)	(4) Future Incarceration (%)
Panel A: Financially Vulnerable				
Year 1	342.74	0.16	149.12	0.02
	(387.34)	(0.16)	(205.20)	(0.02)
January-Side Mean	60,978.32	15.37	50,583.45	1.51
Observations	107,768	107,783	107,744	107,742
Year 2	394.90	0.27*	34.39	0.02
	(389.17)	(0.16)	(205.20)	(0.02)
January-Side Mean	61,198.69	15.23	50,488.22	1.53
Observations	107,011	107,029	106,981	106,975
Year 3	274.35	0.22	37.79	0.02
	(392.46)	(0.16)	(207.49)	(0.02)
January-Side Mean	61,309.38	15.13	50,428.74	1.54
Observations	105,883	105,895	105,839	105,838
Panel B: Financially Non-Vulnerable				
Year 1	154.78	−0.10	−135.56	0.04
	(363.00)	(0.10)	(209.03)	(0.02)
January-Side Mean	79,472.73	10.83	59,565.70	1.20
Observations	187,222	187,252	187,129	187,126
Year 2	347.54	−0.08	52.03	0.02
	(366.39)	(0.10)	(207.57)	(0.02)
January-Side Mean	80,117.52	10.58	59,498.64	1.22
Observations	185,464	185,499	185,376	185,362
Year 3	487.34	−0.15	−121.28	0.01
	(374.25)	(0.10)	(210.48)	(0.02)
January-Side Mean	81,053.43	10.31	59,613.10	1.22
Observations	183,416	183,446	183,296	183,279

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier on characteristics of the mother's census tract of residence, following the specification of Equation 1. The sample is restricted to re-centered years 2012 through 2023, and each outcome is averaged over the quarters of the year in which the mother's tract is observed. Panel A covers financially vulnerable mothers, whose baseline credit score is at or below 660, and Panel B covers non-vulnerable mothers, whose baseline score is above 660. All four outcomes are tract-level measures from the Opportunity Atlas (Chetty et al., 2026). Column (1) is the tract's median household income, measured in the 2012–2016 American Community Survey; column (2) is the share of tract residents below the Federal Poverty Level, measured in the 2006–2010 American Community Survey; column (3) is the predicted mean household income in adulthood of children born between 1978 and 1983 who grew up in the tract with parents at the 25th percentile of the national income distribution; and column (4) is the fraction of the same children incarcerated on April 1, 2010. Each cell reports the estimate from a separate regression, with the outcome measured over the post-birth year indicated by the row. Dollar amounts are as reported in the source data. Share outcomes are multiplied by 100 to represent percentage points. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.6: Effects on Family Structure and Financial Integration

	(1) Same ZIP Code (%)	(2) Same Census Block (%)	(3) Same Credit Bureau Household (%)	(4) Any Joint Account with Partner (%)
Panel A: Financially Vulnerable				
Year 1	−0.66	0.04	−0.06	0.04
	(0.46)	(0.81)	(0.61)	(0.70)
January-Side Mean	85.73	74.06	71.46	39.96
Observations	111,074	60,732	111,074	111,074
Year 2	0.11	0.13	0.91	0.32
	(0.54)	(0.86)	(0.65)	(0.71)
January-Side Mean	81.93	72.05	69.81	47.25
Observations	110,527	60,094	110,527	110,527
Year 3	0.20	−0.28	0.36	0.69
	(0.60)	(0.91)	(0.67)	(0.73)
January-Side Mean	78.66	70.50	67.95	54.48
Observations	109,562	58,579	109,562	109,562
Panel B: Financially Non-Vulnerable				
Year 1	0.24	0.50	0.24	−0.63
	(0.24)	(0.40)	(0.31)	(0.40)
January-Side Mean	91.80	87.73	86.43	64.39
Observations	252,101	142,970	252,101	252,101
Year 2	0.12	0.27	0.19	−0.77**
	(0.28)	(0.44)	(0.34)	(0.39)
January-Side Mean	89.78	86.12	85.08	71.18
Observations	251,187	140,673	251,187	251,187
Year 3	−0.03	0.17	−0.06	−0.52
	(0.30)	(0.46)	(0.35)	(0.37)
January-Side Mean	88.77	85.42	84.45	77.30
Observations	250,179	137,721	250,179	250,179

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1, restricted to births where the father is also linked to the credit panel. Panel A covers financially vulnerable mothers, whose baseline credit score is at or below 660, and Panel B covers non-vulnerable mothers, whose baseline score is above 660. Each cell reports the estimate from a separate regression, with the outcome measured over the post-birth year indicated by the row. Same ZIP Code, Same Census Block, and Same Credit Bureau Household are indicators that the two parents appear in the same ZIP code, census block, and credit bureau household in a given quarter, respectively; each is defined only in quarters where both parents appear in the credit panel and is averaged over those quarters. The credit bureau assigns individuals to households based on address information. Same Census Block is restricted to re-centered years 2012 through 2023. Any Joint Account with Partner is an indicator that equals one if the mother holds at least one credit account jointly with the father in any quarter of the year. All outcomes are multiplied by 100 to represent percentage points. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.7: Robustness Checks – Alternative Regression Specifications

	(1) Any Active Debt in Collections (%)	(2) Active Debt in Collections ($)	(3) Credit Limit ($)	(4) Credit Card Balance ($)
Panel A: Main Specification				
RD Estimate	-1.62^{***}	-59.74^{**}	435.86^{**}	211.73^{***}
	(0.58)	(25.37)	(172.53)	(81.78)
Panel B: Control for C-Section				
RD Estimate	-1.68^{***}	-60.72^{**}	437.24^{**}	210.50^{**}
	(0.58)	(25.37)	(172.51)	(81.78)
Panel C: Remove Covariates				
RD Estimate	-1.90^{***}	-66.89^{***}	501.12^{**}	224.90^{**}
	(0.63)	(25.75)	(215.55)	(94.94)
Panel D: Remove Triangular Kernel				
RD Estimate	-0.97^{*}	-39.71^{*}	355.43^{**}	146.51^{**}
	(0.52)	(22.49)	(153.52)	(73.11)
Panel E: No Winsorization				
RD Estimate	–	-75.81^{**}	471.48^{***}	245.89^{***}
	–	(33.19)	(178.43)	(94.42)
Panel F: Standard Errors Clustered by the Running Variable				
RD Estimate	-1.62^{***}	-59.74^{*}	435.86^{***}	211.73^{**}
	(0.62)	(33.68)	(160.64)	(95.13)
Panel G: Local Linear using `rdrobust`				
RD Estimate	-1.67^{***}	-61.05^{**}	441.57^{**}	216.17^{***}
	(0.59)	(25.73)	(174.93)	(82.93)
Panel H: Local Quadratic using `rdrobust`				
RD Estimate	-3.42^{***}	-118.73^{**}	627.14^{*}	318.25^{*}
	(1.25)	(54.35)	(372.82)	(176.79)

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier on the four primary financial outcomes, measured over the third post-birth year for financially vulnerable mothers, whose baseline credit score is at or below 660, under alternative regression specifications. Panel A repeats the main specification of Equation 1. Panel B additionally controls for C-section delivery. Panel C drops all demographic and financial covariates and retains only the re-centered year fixed effects. Panel D replaces the triangular kernel with a uniform kernel inside the bandwidth. Panel E uses the un-winsorized dollar outcomes. Panel F clusters standard errors by the running variable d_i, the day of birth relative to January 1. Panels G and H fit a local linear and a local quadratic specification using the `rdrobust` package (Calonico et al., 2017). Binary outcomes are multiplied by 100 to represent percentage points, and dollar amounts are expressed in December 2025 dollars. In Panels A through E, standard errors in parentheses are heteroskedasticity-robust; Panels G and H report extttrdrobust's conventional standard errors. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.8: Robustness Checks – Alternative Sample Definitions

	(1) Any Active Debt in Collections (%)	(2) Active Debt in Collections ($)	(3) Credit Limit ($)	(4) Credit Card Balance ($)
Panel A: Main Financially Vulnerable Sample				
RD Estimate	-1.62^{***}	-59.74^{**}	435.86^{**}	211.73^{***}
	(0.58)	(25.37)	(172.53)	(81.78)
January-Side Mean	40.13	707.08	9,912.10	3,998.88
Observations	183,625	183,625	183,625	183,625
Panel B: Matched Before the January 1 Cutoff				
RD Estimate	-1.81^{***}	-34.60	373.20^{*}	198.58^{**}
	(0.70)	(29.39)	(204.30)	(96.67)
January-Side Mean	39.72	687.87	9,735.18	3,950.57
Observations	128,442	128,442	128,442	128,442
Panel C: High Predicted Financial Vulnerability (Top 35%)				
RD Estimate	-1.73^{***}	-74.65^{***}	308.38^{*}	185.08^{**}
	(0.61)	(27.36)	(177.22)	(84.14)
January-Side Mean	41.69	751.54	9,913.13	4,022.74
Observations	168,566	168,566	168,566	168,566
Panel D: Low Predicted Financial Vulnerability (Bottom 65%)				
RD Estimate	0.09	11.69	101.46	-121.16
	(0.23)	(8.49)	(203.35)	(75.01)
January-Side Mean	6.22	97.89	27,384.56	5,307.03
Observations	313,049	313,049	313,049	313,049

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1, under alternative definitions of the estimation sample. Panel A repeats the main estimates for financially vulnerable mothers, whose baseline credit score is at or below 660. Panel B further requires the mother to be matched to the credit panel in the March snapshot preceding the January 1 cutoff. Panels C and D reclassify the financially vulnerable mothers using the financial vulnerability index constructed in Section 3.6; Panel C covers mothers in the top 35% of the index distribution, and Panel D covers mothers in the bottom 65% of the index distribution. Each cell reports the estimate from a separate regression, with the outcome measured over the third post-birth year. Outcomes are averaged over the four quarterly snapshots of the year, and dollar amounts are expressed in December 2025 dollars. Binary outcomes are multiplied by 100 to represent percentage points. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.9: Robustness Checks – Placebo Tests

	(1) Any Active Debt in Collections (%)	(2) Active Debt in Collections ($)	(3) Credit Limit ($)	(4) Credit Card Balance ($)
Panel A: Main Specification (January 1 Cutoff)				
RD Estimate	-1.62^{***}	-59.74^{**}	435.86^{**}	211.73^{***}
	(0.58)	(25.37)	(172.53)	(81.78)
January-Side Mean	40.13	707.08	9,912.10	3,998.88
Observations	183,625	183,625	183,625	183,625
Panel B: Pre-Conception Quarters				
RD Estimate	−0.77	−26.26	−36.61	−65.67
	(0.59)	(25.78)	(170.75)	(89.82)
January-Side Mean	34.90	631.87	7,354.40	3,879.69
Observations	183,625	183,625	183,625	183,625
Panel C: November 1 Placebo Cutoff				
RD Estimate	-1.22^{**}	−8.94	78.38	−60.21
	(0.57)	(24.82)	(167.11)	(78.90)
Right-of-Cutoff Mean	40.59	713.65	9,917.75	4,040.52
Observations	198,262	198,262	198,262	198,262
Panel D: February 1 Placebo Cutoff				
RD Estimate	−0.60	24.72	−91.19	−21.28
	(0.60)	(26.29)	(175.55)	(83.58)
Right-of-Cutoff Mean	39.28	685.55	10,265.63	4,106.12
Observations	175,698	175,698	175,698	175,698
Panel E: March 1 Placebo Cutoff				
RD Estimate	0.99	2.30	−160.22	−44.18
	(0.61)	(25.66)	(186.90)	(87.17)
Right-of-Cutoff Mean	38.76	671.07	10,446.51	4,133.46
Observations	168,293	168,293	168,293	168,293

Notes: This table reports placebo RD estimates on the four primary financial outcomes for financially vulnerable mothers, whose baseline credit score is at or below 660. Panel A repeats the main specification of Equation 1 at the true January 1 cutoff, with each outcome measured over the third post-birth year. Panel B replaces each outcome with its aggregate over quarters −7 to −5 before the January 1 cutoff and does not control for the baseline financial covariates. Panels C through E re-center the estimation window on November 1 of the preceding year, February 1, and March 1, respectively. Binary outcomes are multiplied by 100 to represent percentage points, and dollar amounts are expressed in December 2025 dollars. January-Side Mean is the unweighted mean among January-side births over the same outcome window, and Right-of-Cutoff Mean is its analogue for births to the right of the placebo cutoff. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.10: Effects under Income-Based Sample Definitions

	(1) Any Active Debt in Collections (%)	(2) Active Debt in Collections ($)	(3) Credit Limit ($)	(4) Credit Card Balance ($)
Panel A: Main Financially Vulnerable Sample				
RD Estimate	-1.62^{***}	-59.74^{**}	435.86^{**}	211.73^{***}
	(0.58)	(25.37)	(172.53)	(81.78)
January-Side Mean	40.13	707.08	9,912.10	3,998.88
Observations	183,625	183,625	183,625	183,625
Panel B: Principal Source of Payment is Medicaid				
RD Estimate	-1.72^{**}	−44.26	−238.97	−103.99
	(0.75)	(32.49)	(201.66)	(80.78)
January-Side Mean	38.73	679.52	8,731.18	2,602.71
Observations	101,269	101,269	101,269	101,269
Panel C: Bottom 30% of Predicted Household Income				
RD Estimate	-1.62^{**}	-48.36^{*}	106.02	−25.75
	(0.63)	(26.22)	(181.31)	(76.00)
January-Side Mean	37.17	642.72	10,400.06	3,192.09
Observations	143,433	143,433	143,433	143,433

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1, with the outcome measured over the third post-birth year. Panel A repeats the main estimates for financially vulnerable mothers, whose baseline credit score is at or below 660. Panels B and C replace the credit score-based classification with income-based definitions of the low-income population: Panel B covers births whose principal source of payment is Medicaid, and Panel C covers births in the bottom 30% of the distribution of household income predicted as described in Section 3.5. Each cell reports the estimate from a separate regression. Outcomes are averaged over the four quarterly snapshots of the year, and dollar amounts are expressed in December 2025 dollars. Binary outcomes are multiplied by 100 to represent percentage points. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.11: Effects on Credit Access and Utilization Within the Family

	(1) Credit Limit ($)	(2) Credit Card Balance ($)
Panel A: Mother-Level		
RD Estimate	390.73	193.83
	(249.69)	(119.11)
January-Side Mean	12,488.08	4,971.89
Observations	110,155	110,155
Panel B: Father-Level		
RD Estimate	348.38	265.77*
	(343.54)	(142.46)
January-Side Mean	15,543.48	5,458.68
Observations	110,155	110,155
Panel C: Family-Level		
RD Estimate	562.86	395.32**
	(469.04)	(201.63)
January-Side Mean	25,623.08	9,424.92
Observations	110,155	110,155

Notes: This table reports RD estimates of the effect of receiving the first child-related tax benefit about a year earlier, following the specification of Equation 1, for financially vulnerable mothers whose baseline credit score is at or below 660, restricted to births where the father is also linked to the credit panel. Each cell reports the estimate from a separate regression, with the outcome measured over the third post-birth year. Panels A and B measure the named parent's total credit limit across all open credit card accounts and the total balance on those accounts; Panel C sums the two parents' accounts, counting each jointly held account once. Outcomes are averaged over the four quarterly snapshots of the year and expressed in December 2025 dollars. January-Side Mean is the unweighted mean among January-side births over the same year. Heteroskedasticity-robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

C Additional Context and Data Information

C.1 Data Linkage

This appendix details the record linkage introduced in Section 3.1, which matches parents in the birth records to individuals in the UC-CCP based on first and last names, date of birth, residential address, and ZIP code. The linkage procedure is adapted from Ma, Wang, and Yin (2026), where the source data and administrative infrastructure are shared.

For data privacy reasons, the CPL hashed the PII in both datasets, and a third party chooses and enters the salt that initiates the hashing program. I then conduct the linkage based on the hashed variables and never observe the underlying PII in clear text. Normally, nicknames and misspellings would be handled with fuzzy matching based on string distance measures, but this is not feasible with hashed data. Instead, the CPL supplies hashed versions of the first few letters and of a phonetic (Soundex) encoding of each name and street address field, so I can match either on the full string or fuzzily on these alternative versions.

These linkage fields carry two caveats. First, the birth record documents the mother's first and maiden names and the father's first and last names (when present). A parent's last name in the credit file may differ from the one on the birth record, most often when a mother is recorded under her married rather than her maiden name. To accommodate this, I match each individual's last name in the credit data to either the parent's own last name or the partner's; that is, the mother's maiden name or the father's last name. Second, the birth certificate records only the mother's address, so it is plausible that matched fathers are disproportionately those who lived with the mother at the birth.

The birth records are only observed once, whereas the UC-CCP supplies linkable identifiers in the March files of each year. To allow for address and name changes around the birth, I match each birth to three UC-CCP snapshots: its re-centered year and the years before and after. For example, both December 2010 and January 2011 births are matched to the March 2010, 2011, and 2012 snapshots. Because December- and January-side births in a given re-centered year draw on the same three snapshots, the candidate pool does not change mechanically at the cutoff. This departs from Ma, Wang, and Yin (2026), where births are matched on the calendar year of birth, a convention that would threaten the validity of the RD design in this paper because the calendar year changes discontinuously at each January 1 cutoff. Had each birth instead been matched to the snapshots based on its own birth year, a December 2010 birth would draw on the March 2009–2011 files while a January 2011 birth a few days later would draw on the March 2010–2012 files. The two sides would then be linked through identifying fields measured at different snapshots, and the rate and quality of matching could jump at January 1 for purely mechanical reasons.

Matching to the same three snapshots on both sides of the cutoff keeps the candidate pool fixed, but the three snapshots are not equally informative. Because the linkage relies on the address recorded at the time of the birth, the last snapshot, taken about 15 months after the birth, is the most exposed to residential moves after the birth, and a match that appears only there is more likely to be a false positive. The main analysis therefore requires a match in the first or second snapshot, and the last snapshot enters the main analysis only through the resolution of multiple candidates described below. A separate concern is that earlier benefit receipt could itself make a mother easier to match, for instance if the mother becomes more likely to apply for new accounts and so updates her address with the credit bureau more promptly. The robustness checks address it by further restricting the sample to births matched in the first snapshot, which is taken before the January 1 cutoff, so that the linkage pre-dates any benefit receipt.

The UC-CCP's coverage of the matching fields is weaker in the early years of the panel. In particular, the pre-2011 snapshots do not record an individual's date of birth. I overcome this limitation by recovering it from the March 2011 file through the panel's person identifier, as long as these individuals are still present. Street address fields are also less furnished in this early period, and matches that rely on these early snapshots therefore draw on fewer and noisier fields.

Following CPL's hashed linkage guidelines, I form matches in a sequence of rounds that vary in how strictly the identifying fields must agree. Because a birth can have candidate matches in more than one of the three annual snapshots, and to more than one individual in the credit panel, I resolve each parent to a single match. When a parent has exactly one candidate individual, I retain it; when there are several, I keep the one that appears most often across the matched rounds and snapshots. The resulting crosswalk assigns each matched parent to a unique individual in the credit panel and is used throughout the paper. Table C.1 lists the match rounds and their criteria, together with the share of first-time births whose mothers are matched at each round.

C.2 Household Income and Tax Benefit Predictions

This appendix details how I predict the household income and the tax value of a first child for the linked families in my sample, since neither the birth records nor the credit panel records information on income or taxes. Following Rittenhouse (2025), I estimate the prediction models on the ACS, where income is observed and tax liabilities can be simulated, and apply them to the birth records through demographic variables common to both datasets.

ACS Data Processing I draw ACS data from IPUMS (Ruggles et al., 2025) for survey years 2005–2024 and keep California households with a child under age three, which best resembles the first-birth population

Table C.1: Data Linkage Match Rounds and Match Rates

Round	Match Criteria	New Matches (%)	Cumulative (%)
1	Exact: first name, date of birth, ZIP code Fuzzy: last name, address	38.3	38.3
2	Exact: date of birth, ZIP code Fuzzy: first name, last name, address	5.8	44.2
3	Exact: ZIP code Fuzzy: address, first name, last name, date of birth	1.4	45.6
4	Exact: ZIP code Fuzzy: address, first name, last name	8.9	54.5
5	Exact: date of birth Fuzzy: address, first name, last name	0.4	54.8
6	Exact: ZIP code Fuzzy: first name, own last name, and at least one of date of birth and address	9.4	64.3
7	Exact: ZIP code Fuzzy: first name, partner's last name, and at least one of date of birth and address	1.5	65.8
8	Exact: date of birth, ZIP code Fuzzy: at least two of first name, own last name, and address	3.5	69.3
9	Exact: date of birth, ZIP code Fuzzy: at least two of first name, partner's last name, and address	0.2	69.6

Notes: This table presents the match rounds and match rates for the linkage between mothers in the birth records and individuals in the UC-CCP. The sample consists of first-time singleton births in re-centered years 2006–2023, limited to those where the mother resided in California at the time of birth and delivered in a California birth facility.

in the birth records while allowing me to obtain a large estimation sample of about 196,000 tax units. I form tax units from family relationships, keep those with dependent children, and pass each unit's wage and self-employment earnings, Social Security income, investment income (interest, dividends, and net rental income), retirement income (pensions and IRA distributions), filing status, and the number and ages of its dependent children to the NBER TAXSIM program. TAXSIM performs two simulations for each family, once with and once without the youngest child claimed as a dependent. It then returns the federal, state, and payroll taxes in each case. Because investment income enters the simulation, the computed benefits also respect the EITC's investment income limit. Taxes are computed under the tax code of each birth cohort's December-side tax year, so the tax value measures the benefit at stake at that cohort's January 1 cutoff.

From the ACS data and TAXSIM results I build three household-level outcomes, all in 2025 dollars: earned income, calculated as the sum of wage and self-employment earnings; after-tax income with the youngest child not yet claimed, defined as earned income plus Social Security, investment, and retirement income minus the simulated taxes; and the tax value of the youngest child, calculated as the change in the household's total tax liability from claiming the child. This tax value proxies for the child-related tax benefits that a December-side birth brings forward by a year.

Regression Model To predict household income and the tax value of the youngest child, I estimate the following regression using ACS data:

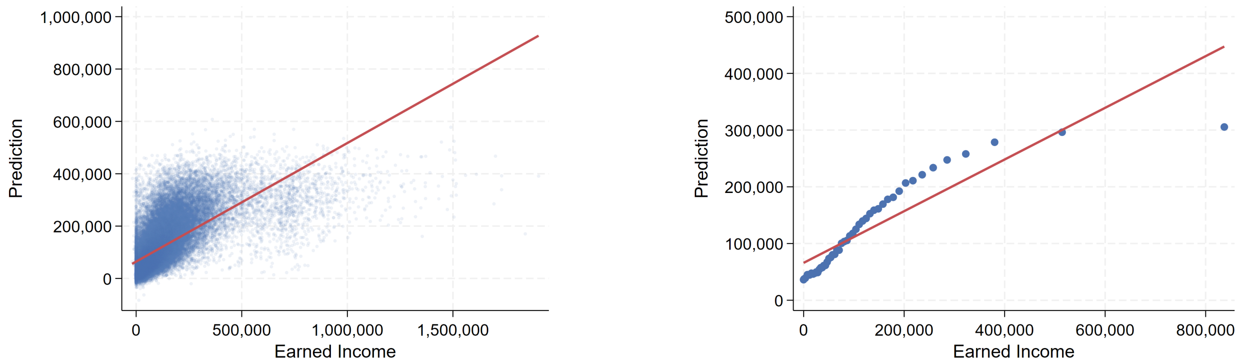
$$Y_h = \mathbf{X}_h'\beta + \theta_{y(h)} + \phi_{p(h)} + \varepsilon_h, \tag{C.1}$$

where h indexes households, and Y_h is one of the three main outcomes specified above. $\mathbf{X}_h$ collects the parental demographics, all their pairwise interactions, and their interactions with the re-centered year; $\theta_{y(h)}$ and $\phi_{p(h)}$ are re-centered year and Public Use Microdata Area (PUMA) fixed effects; and ε_h is an error term. The demographic variables included in the regression are the age, race and ethnicity, and education of each parent, the mother's birthplace, and birth order, each grouped into bins so that they can be coded identically in both the ACS and the birth records. The saturated demographic interactions let the prediction vary flexibly across household types, while the fixed effects absorb differences across cohorts and neighborhoods. Each household is weighted by its ACS household weight.

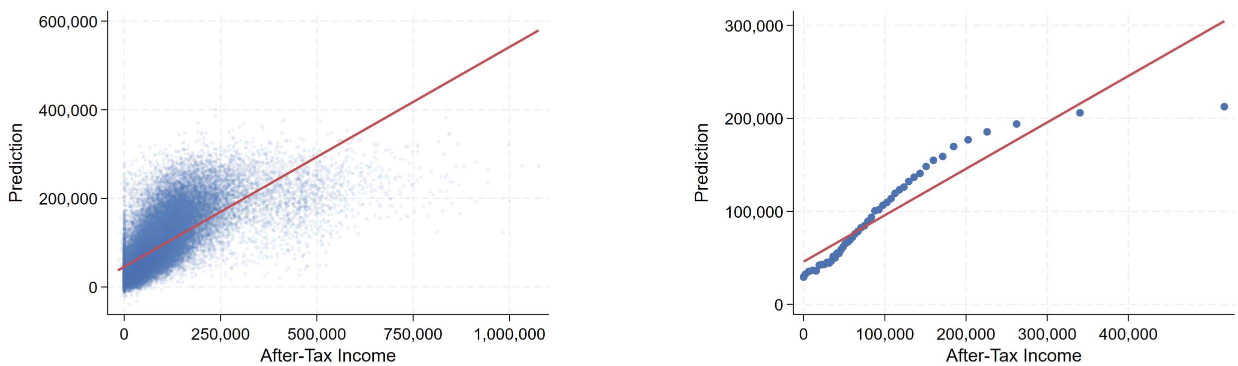
Validation and Application To gauge out-of-sample fit, I estimate the models on a random 75% of the ACS and evaluate them on the held-out 25%. The held-out R^2 is 0.45 for earned income, 0.50 for after-tax income, and 0.40 for the tax value, close to the in-sample values of 0.47, 0.51, and 0.43, so there is limited overfitting. Figure C.1 plots predicted against actual values for each of the three outcomes in the held-out sample, and the predictions track the ordering of the actual outcomes well.

For the predictions used in the paper, I refit each model on the full ACS sample and extrapolate the result to the birth records, coding every predictor from the birth record fields into the same bins used in the ACS, so that each birth record family is assigned the fitted value for its demographic cell. For each family, this exercise eventually yields a predicted earned income, which locates each family in the income distribution and defines the low-income comparison group; a predicted tax value of the first birth, which measures the size of the transfer; and a predicted after-tax income, the baseline against which the tax value produces the first-year jump in resources at the cutoff. I rank families on predicted earned income, as eligibility for the child-related tax benefits and the amounts of these benefits at low incomes depend primarily on earned income, so it best identifies the families for whom the benefits are largest. The distinction between earned income and total income is unlikely to be meaningful in this context: the correlation coefficient between the two measures is 0.98 in my estimation sample.

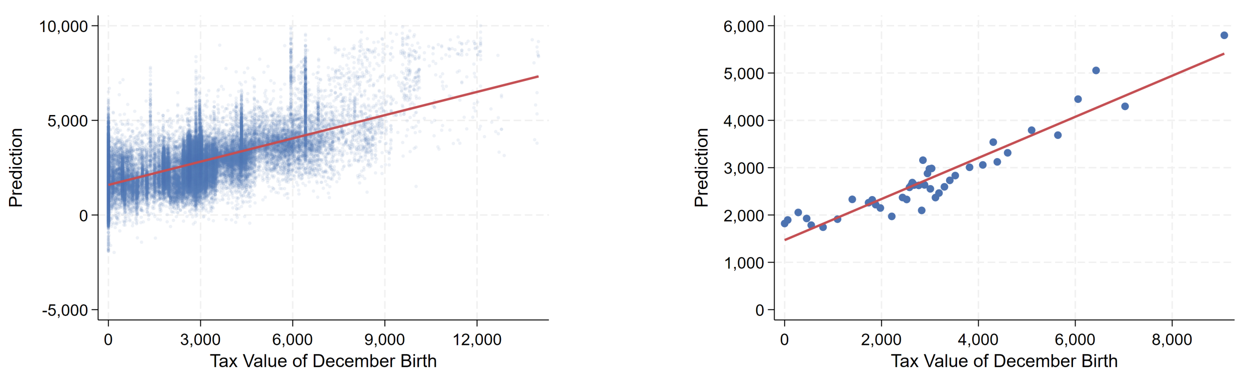
Figure C.1: Out-of-Sample ACS Predictions



(a) Earned Income



(b) After-Tax Income



(c) Tax Value of December Birth

Notes: This figure presents scatter plots and binned scatter plots for the three main ACS prediction outcomes in the 25% held-out sample. In each panel, the x-axis shows the actual value of the outcome, and the y-axis shows its predicted value. Left panels plot individual observations, and right panels plot the mean actual and predicted values within 50 equal-sized bins of the actual value. The red line is the line of best fit based on the underlying, unbinned data.

C.3 Financial Vulnerability Index Construction

This appendix details the financial vulnerability index of Section 3.6. Recall I focus on mothers who are matched in the March snapshot of the year before the re-centered year or of the re-centered year itself, have a valid credit score at $q = -8$, and are present in every quarter from $q = -8$ to $q = +11$. Within this sample, I construct a binary indicator for any 30-day delinquency in the first year after the first birth (from $q = 0$ to $q = +3$), which forms the basis of the index construction exercise.

Regression Model To predict each mother's financial vulnerability, I estimate the following regression:

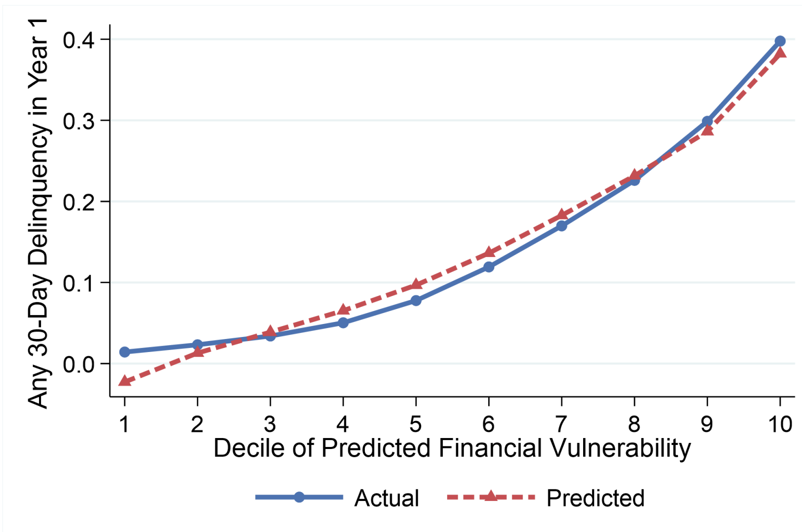
$$M_i = \mathbf{Z}_i'\gamma + \theta_{y(i)} + \phi_{p(i)} + \varepsilon_i, \tag{C.2}$$

where i indexes mothers, and M_i is the delinquency indicator defined above. $\mathbf{Z}_i$ collects both the parental demographics, also used in the ACS prediction exercise, and the mother's $q = -8$ credit profile; $\theta_{y(i)}$ and $\phi_{p(i)}$ are re-centered year and PUMA fixed effects; and ε_i is an error term. Specifically, from the $q = -8$ credit profile, I include indicators for the mother's credit score tier (subprime, near-prime, prime, superprime), her 30-day delinquency status, her revolving credit utilization (no credit line, up to 30%, 30–90%, and above 90%), and her account balances in each of the three account categories: credit card, auto loan, and mortgage (no account, zero balance, and terciles of positive balances). I also interact the mother's age bins with each of these credit measures to flexibly control for changes in credit usage patterns over the life cycle. Finally, the fixed effects absorb level differences across regions and birth cohorts.

Because the index is a predicted outcome that I then use to split the estimation sample, I follow the repeated split sample procedure proposed in Abadie, Chingos, and West (2018) to guard against endogenous stratification. Specifically, in each of 100 repetitions, I randomly split the January-side births (i.e., the control side) in half, fit the model on one half, and score only the mothers outside the fitting half: the held-out January half and every December-side mother. A mother's financial vulnerability index is her fitted value averaged across the repetitions in which she is excluded from the fitting sample, so the index never embeds any mother's own post-birth outcome or any December-side mother's outcome that may have been affected by tax benefit access.

Validation and Application I evaluate the performance of the financial vulnerability index against the actual incidence of any 30-day delinquency within the first post-birth year in Figure C.2. The sample is limited to January-side mothers, as they have not yet received the first child-related tax benefits during that window. Because delinquency is a binary variable, for ease of interpretation, I group mothers into deciles based on their index value. Two patterns stand out. First, the index predicts actual delinquency incidence quite well, as the two series closely track each other across all the deciles. Recall that each mother is scored only in the repetitions where she is excluded from the fitting sample, so this can be viewed as an out-of-sample fit. And second, there is large dispersion in the actual delinquency incidence across the index distribution. The probability of having any 30-day delinquency is only 1.5% in the bottom decile, while it is as high as 39.8% in the top. The index therefore sorts mothers well by their subsequent delinquency risk, with actual incidence rising monotonically across the deciles.

Figure C.2: Actual and Predicted Delinquency by Decile of Financial Vulnerability Index

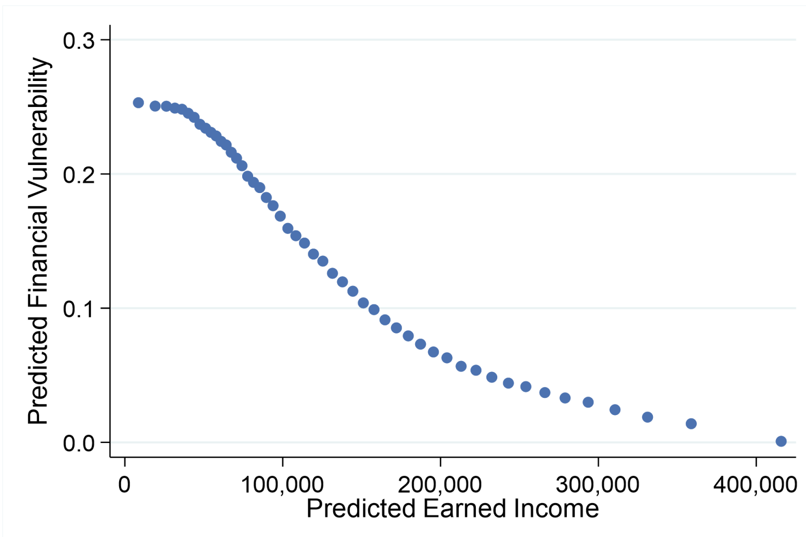


Notes: This figure presents the relationship between the financial vulnerability index and subsequent delinquency for mothers of January-side births who satisfy the sample restrictions of Section 3.4. Mothers are sorted into deciles of the index, measured at the baseline quarter $q = -8$, and each dot represents the mean across all mothers within a decile. The Actual series plots the share of mothers with any 30-day delinquency in the first year after the first birth ($q = 0$ to $q = +3$), and the Predicted series plots the mean value of the index.

The index is also not simply a proxy for income within my sample. As Figure C.3 shows, predicted vulnerability is negatively correlated with predicted earnings overall, but is close to flat across the lower part of the income distribution. This pattern is in contrast to the credit score, where Panel (b) of Figure A.2 shows a general positive gradient in predicted earnings, including within the low-income population.[35]

[35]While I cannot examine the algorithm that generates the credit score, it is plausible that the variation in credit score in this bottom range comes from factors other than delinquency risk, such as length of credit history or the mix of credit products, or algorithmic bias as studied in Albanesi and Vamossy (2024); Bakker et al. (2025).

Figure C.3: Baseline Financial Vulnerability by Predicted Earned Income



Notes: This figure plots the financial vulnerability index, whose construction is detailed above, against predicted earned income among mothers who satisfy the sample restrictions of Section 3.4. The index is measured at the baseline quarter $q = -8$, and sorted in 50 equal-sized bins of predicted earned income. Each dot represents the mean within a bin.