Three Essays on Income Support Policy, Family Violence, and Household Economic Arrangements

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Abstract

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This dissertation focuses on the relationships between income support policy, economic security, and family dynamics that contribute to health and wellbeing. Chapters 1 and 2 examine potential consequences of exposure to tax credits, the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC), for child maltreatment reports and intimate partner violence – two outcomes that are strikingly common in the United States and disproportionately experienced by those in low-income households. The third chapter is also related to the economic security of families. Typically, in studies of economic-wellbeing, measurement tends to occur at the household level, thereby obscuring patterns of ownership of economic resources within families. In chapter 3, I examine trends and patterns of liquid asset ownership within couples with children, with particular attention to how these patterns are shaped by gendered power dynamics.

Chapter 1 leverages a natural experiment – created by a legislated change in the timing of the annual disbursement of EITC and CTC transfer payments to families – to estimate the association between EITC and CTC payments and child maltreatment reports in the period shortly after families receive payments from these programs. Using weekly tax refund data from the Internal Revenue Service linked to state-specific child maltreatment report data from the National Child Abuse and Neglect Data System during the 2015 through 2018 tax seasons, I find evidence that reports of child maltreatment made to child welfare authorities decline in the weeks immediately following issuance of these lump-sum credits. My results imply that child maltreatment risk is responsive to not only chronic economic hardship but also to more immediate income availability among caregivers.

Chapter 2 focuses on pregnant women's exposure to intimate partner violence, which affects maternal health and birth outcomes. Using individual-level data from the 1996-2018 Pregnancy Risk Assessment Monitoring System (PRAMS) and a difference-in-differences methodological approach, I find that expansions to the EITC were associated with lower risk of self-reported physical abuse during pregnancy. These effects are concentrated among mothers who are most likely to receive EITC benefits during pregnancy – those with lower levels of education and previous children. Given a broader literature documenting improvements in maternal-infant health associated with EITC expansions, reductions in violence against pregnant women may be one channel through which the EITC leads to maternal-infant health improvements.

Chapter 3 uses data from the 1998-2019 waves of the Survey of Consumer Finances to examine patterns and trends in asset ownership within couples with children. Drawing on theories of bargaining power, I test the premise that differentials between partners – in their

education, employment, and health status – are associated with each partner's liquid asset ownership. I find that the proportion of couples with children who hold liquid assets in separately owned accounts has increased between 1998 and 2019. Both men and women have increasingly used separately owned accounts, but the median account balance was greater for men than women. I also find that proxies for intra-partner bargaining power are associated with the likelihood that women, but not men, separately own liquid assets. This study demonstrates the importance of considering intrahousehold allocations in studies of economic wellbeing. Together, these chapters contribute to our understanding of the downstream effects of income support policies for the health and wellbeing of recipients and their children as well as demonstrate the importance of considering how intrahousehold dynamics might condition the effects of such transfers.

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1. Short-Term Effects of Tax Credits on Rates of Child Maltreatment Reports in the United States

1.1. Abstract

Poverty and low income are associated with increased risk for child maltreatment. The Earned Income Tax Credit (EITC) and Child Tax Credit (CTC) are among the largest anti-poverty programs in the United States. This paper leverages the natural experiment of a legislated change in the timing of EITC and CTC transfer payments to low-income families and quasi-experimental methods to estimate the association between EITC and CTC payments and child maltreatment reports in the period shortly after families receive payments from these programs. I linked weekly EITC/CTC refund data from the Internal Revenue Service (IRS) to state-specific child maltreatment report data from 48 states and the District of Columbia during the 2015-2018 tax seasons (January-April). I find that EITC and CTC payments were associated with lower state-level rates of child maltreatment reports. For each additional $1000 in per-child EITC/CTC tax refunds, state-level rates of reported child maltreatment declined in the week of and four weeks following refund payments by an overall estimated 5.0% (95% confidence interval = 2.3%, 7.7%). These results suggest that federal income assistance programs are associated with immediate reductions in child maltreatment reporting.

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1.2. Introduction

Childhood maltreatment -- which encompasses neglect and physical, sexual, and emotional abuse -- is associated with a wide array of short- and long-term negative outcomes, including immediate physical injuries, delayed cognitive development (Mills et al., 2011), mental health problems into adolescence and adulthood (Cicchetti & Toth, 2005; Springer, Sheridan, Kuo, & Carnes, 2007; Norman et al., 2018; Fletcher 2009), higher risk of suicide attempts and suicides (Dube et al., 2001; Gilbert et al., 2009), elevated rates of chronic disease and mortality in later life (Widom, Czaja, Bentley, & Johnson, 2012; Chen, Turiano, Mroczek, & Miller, 2016), and lower levels of educational attainment, employment, and earnings in adulthood (Currie, Spatz, & Widom 2010; Metzler, Merrick, Klevens, & Ports, 2017). The societal costs of child maltreatment are estimated to range from $428 billion to $2 trillion each year (Peterson, Florence, & Klevens, 2018), with a significant portion of these costs occurring in the health care, child welfare, and criminal justice systems.

Family poverty is a key risk factor for child maltreatment (Sedlack, et al., 2010; Pelton, 2015; Berger 2004). Limited economic resources increase the likelihood of maltreatment, particularly neglect, by preventing caregivers from adequately meeting children's basic material, safety, medical, and supervisory needs (Yang 2015). Economic adversity indirectly contributes to maltreatment risk by increasing parental stress or depression which can lead to harsh or neglectful parenting behaviors (Berger 2007, Berger & Waldfogel, 2011). In addition to chronic economic strain, income volatility and abrupt negative changes in financial resources have been shown to elevate maltreatment risk (Monahan 2020; Berger et al., 2015; Cai, 2021).

Income assistance programs have been proposed as a strategy for child maltreatment

prevention (Klevens, Barnett, Florence, & Moore, 2015; Feely, Raissan, Schneider, & Bullinger, 2020). However, evidence that additional income reduces child maltreatment is sparse, largely due to limitations in existing data, study methodologies, and ability to randomize exposure to income (Berger & Waldfogel, 2011). In the present study, I leveraged policy-induced variation in the payment schedule of two tax-benefit programs that are central components of the U.S. social safety net -- the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC) – to examine the effect of income transfer payments on child maltreatment report rates.

The EITC and CTC are among the most effective US anti-poverty programs, lifting 8.3 million individuals out of poverty, including 4.5 million children, in 2017 (Fox 2018). The EITC is a fully refundable tax credit, which means that if the credit amount exceeds a recipient's tax liability, the balance is received as a cash transfer with their tax refunds. In recent years, the CTC has provided a partially refundable credit to tax filers that is conditional on earnings. Both the EITC and CTC are paid to wage earners and designed to incentivize employment.[1] Because refundable tax credits are delivered to families as a lump-sum payment with their tax refunds, these credits create unusual "financial slack" at tax time for low-income families living on tight budgets (Halpern-Meekin, Greene, Levin, & Edin, 2018). Lump-sum refundable tax credits, worth up to 45% of a family's annual earnings (Jones & Michelmore, 2018), enable families to purchase household necessities, pay outstanding bills, and reduce debt upon receipt (Maag, Roll, & Oliphant, 2016; Mendenhall et al., 2012) which may reduce families' material hardship and improve family functioning. Studies indicate that families are sensitive to the timing of

[1] In 2021, after the end of the study period, Congress passed a one-year temporary expansion of the CTC as part of the American Rescue Plan. This expansion to the CTC in 2021 increased the size of the benefit, made the credit fully refundable, extended eligibility to households with little or no earnings, and converted payments from an annual lump-sum to periodic monthly transfers.

payments; a survey of EITC and CTC filers with children indicated that 86% used a portion of their tax refund within a month of receipt, and 30% anticipated that even a short-term delay in their payment could present a financial challenge (Maag, Roll, & Oliphant, 2016).

Considerable evidence suggests that poverty-reducing tax credits not only influence families' economic outcomes (Meyer & Rosenbaum, 2001; Hoynes & Patel, 2018) but they also improve recipient and child health and wellbeing. For instance, the EITC is associated with reduced low birthweight (Hoynes, Miller, & Simon, 2015) lower infant mortality (Arno, Sohler, Viola, & Schechter, 2009), and improved self-reported maternal and child health (Averett & Wang, 2018; Evans &, Garthwaite, 2014). Prior studies also provide evidence for an association between more generous EITCs and reductions in several indicators of child maltreatment – hospital admissions for pediatric abusive head trauma (Klevens et al., 2017), self-reported parenting behaviors that approximate neglect (Berger, Font, & Slack, 2017), foster care entries (Rostad et al., 2020), and child protective services involvement (Berger, Font, & Slack, 2017; Kovski et al., 2021). Past studies have not considered the effects of the EITC and CTC on administratively-reported child maltreatment in the period immediately after payments are distributed despite evidence that household financial well-being and spending on necessities reflect the timing of lump-sum EITC/CTC payments (Halpern-Meekin, Greene, Levin, & Edin, 2018; Jones & Michelmore, 2018; Maag, Roll, & Oliphant, 2016; Mendenhall et al., 2012; Aladangady et al., 2018).

In this study, I linked unique weekly EITC and CTC tax refund data from the Internal Revenue Service (IRS) to state-specific child maltreatment report data during the 2015-2018 tax seasons (January-April) and used a legislated policy change in the timing of tax refund receipt to estimate effects on child maltreatment report rates. My quasi-experimental estimation strategy

used comparisons from before and after the legislative change at the weekly timescale, controlling for other changes that might have influenced the outcome over longer time frames (e.g., expanded definitions of maltreatment or differential agency response to cases) (Font & Maguire-Jack, 2020). Based on prior evidence supporting an association between income, the EITC, and reductions in reports of child maltreatment, I hypothesized that short-term rates of child maltreatment would be reduced following families' receipt of EITC/CTC refunds.

1.3. Prior Literature on Economic Wellbeing and Child Maltreatment Risk

The literature on family economic wellbeing and child maltreatment is extensive. Generally, studies find that indicators of financial and material hardship are associated with child maltreatment and child welfare involvement. Studies have linked unemployment (Schenck-Fontaine, Gassman-Pines, Gibson-Davis, & Ananat, 2017; Brown & De Cao, 2020), housing insecurity (Warren & Font, 2015; Berger et al., 2015; Bullinger & Fong, 2020; Marcal 2018), economic uncertainty (Schneider, Waldfogel, & Brooks-Gunn, 2017), and poverty – at both the family and neighborhood levels (Slack et al., 2004; Sedlak, et al., 2010; Farrell et al., 2017) – to increases in the likelihood of child maltreatment and reports of child maltreatment made to CPS.

A body of research on the role of economic support programs is overall suggestive that cash and in-kind benefits reduce child maltreatment risk, and that reductions in these benefits increase subsequent child maltreatment risk (see Maguire-Jack, Johnson-Motoyama, & Parmenter, 2021, for a review of studies on the relationship between economic supports for working parents and child maltreatment). Receipt of child care subsidies, designed to remove barriers to work for low-income parents, is associated with lower rates of physical abuse and

neglect reports (Yang et al., 2019). Increases in family income through state-level minimum wage laws are also linked to lower rates of child maltreatment reports made to CPS (Raissan & Bullinger, 2017). Studies of welfare reform from the 1990s suggest that welfare benefit reductions and sanctions for noncompliance increased the incidence of substantiated child maltreatment cases (Paxson & Waldfogel, 2003), and particularly so when families' benefits losses were not offset by income from other sources (e.g., earnings, food stamps) (Slack, Lee, & Berger, 2007). Evidence from a rare, randomized control trial (Cancian, Yang, & Slack, 2013), which produced exogenous differences in child support payments received by low-income unmarried families, demonstrates how even small cash transfers can impact maltreatment risk; an additional $100 per year led to a 2-percentage point reduction in CPS investigations.

A handful of studies have directly examined effects of the EITC on various indicators of child maltreatment and child welfare system involvement. A study based on hospital admission data from 27 states spanning from 1995 to 2013 found an association between the presence of a state-level refundable EITC and fewer hospital admissions for pediatric abusive head trauma – a severe form of physical abuse that affects infants and young children (Klevens et al., 2017). Research using individual-level longitudinal data from the Fragile Families Study, which follows several thousand low-income mothers in urban areas, found an association between EITC-induced increases in household income and lower risk of self-reported neglectful parenting behaviors and CPS involvement (Berger, Font, Slack, & Waldfogel, 2017). Using administrative CPS data, two studies have demonstrated a link between state EITC benefits and declines in children entering foster care (Rostad, Ports, Tang, & Klevens, 2020) and fewer reports of child maltreatment (Kovski et al., 2020). No studies have directly evaluated effects of the CTC on child maltreatment risk; however, one study found an association between CTC expansions and

fewer childhood injuries requiring medical attention, the risk of which is associated with child maltreatment (Rostad, Klevens, Ports, & Ford, 2020). Despite consistent findings from these studies, one important area that yet to be examined is how these specific programs may interact to influence maltreatment risk.

1.4. The Current Study

I build on previous studies by examining patterns of child maltreatment reports in the period shortly after families receive EITC and CTC payments. To conduct my analysis, I draw on weekly tax refund data linked to state-specific child maltreatment report data. One major challenge with past studies that leverage CPS records and variation in EITC policies across states and over time is that temporal and between-state variability in child welfare system practices and policies may confound policy variation (Font & Maguire-Jack, 2020). For instance, states vary in their legal definitions of child maltreatment and who is mandated to reported cases (Rebbe 2018). Additionally, the number of screened-in reports of child maltreatment may be influenced by the size of agency budgets. If correlated with the timing of state EITC introductions and reforms, these factors would bias results (Kovski et al., 2020; Day, Tach, Mihalec-Adkins, 2021). By focusing on the weeks following EITC and CTC receipt, my study eliminates confounding factors in periods well beyond credit receipt that can hinder effect identification. In other words, changes in child welfare polies and practices might confound estimates based on longer time frames but are unlikely to change rates of investigations in the weeks surrounding EITC and CTC issuance.

1.5. Study Data and Methods

1.5.1. Refundable Tax Credits

This study used administrative IRS data on weekly state-level total tax refunds to tax filers claiming the EITC or the refundable portion of the CTC, obtained through a special request from the IRS's Research, Applied Analytics, and Statistics Group. My analysis covered the 2015 through 2018 tax filing seasons (January through April), during which over 99% of all refundable credits were delivered to tax filers each year. These data capture the timing of IRS issuance (i.e., when the IRS sent out refundable credits to households); families' receipt of payments follows within a few days (Aladangady et al., 2018).

I leveraged policy-induced variation in the payment schedule of EITC and CTC refunds due to the Protecting Americans Against Tax Hikes (PATH) Act. The PATH Act took effect on January 1, 2017, and mandated a delay in the IRS's issuance of tax refunds to families who claim the EITC or refundable portion of the CTC. This delay gives the IRS additional time to verify self-reported wages on tax returns against employer-reported wages, with the intention of reducing erroneous refunds. Figure 1-1 illustrates the extent of the refund delay due to the PATH Act with tax refund data from the 2015-2018 tax seasons (January through April). Refund amounts are adjusted for inflation and expressed in 2018 dollars. Specific dates associated with each week are presented in Table 1-1.

On average, the IRS issued $121 billion in refunds containing the EITC or refundable portion of the CTC to 27.7 million tax filers during the 2015 and 2016 tax seasons and $111 billion to 25.9 million filers during the 2017 and 2018 tax seasons. During the 2015 and 2016 tax

seasons, the IRS began releasing refunds containing these tax credits in late January (week 3), while during the 2017 and 2018 tax seasons, the first batch of such refunds was not released until late February (week 7). Following the PATH Act mandate, issuance in 2017 and 2018 spiked in week 7, compared to weeks 4-6 in pre-PATH Act years (2015 and 2016). These data show that a substantial share of all refundable tax credits issued to families was affected by this legislated delay. In pre-PATH Act years, $63.9 billion (53%) was issued to 12.6 million tax filers in late January and early February (weeks 3-6), while, in post-PATH Act years, no refunds were issued over the same period. Figures 1-2 and 1-3 present these weekly issuance amounts disaggregated by state and years before and after the PATH Act. In my analysis, I made EITC/CTC tax refunds comparable across states by dividing dollar amounts by the state child population (hereafter "per child tax refunds").

1.5.2. Child Maltreatment Reports

I obtained state-level counts of child protective services (CPS) reports and substantiations for each date over the study period through special request to the National Data Archive on Child Abuse and Neglect (NDACAN). Reports of child maltreatment are made by both mandatory reporters (e.g., professionals who have formal contact with children) and voluntary reporters (e.g., neighbors). Lags can occur between an incident (or pattern) of maltreatment and a report made to CPS. However, report dates are among the best available proxies for the timing of child maltreatment, and state laws and policies generally require mandated reporters to immediately report known or suspected child maltreatment to authorities. Although widely used as a proxy for maltreatment, CPS reports are not a direct measure of parenting behavior; they also reflect factors beyond potential underlying maltreatment such as children's visibility to mandatory

reporters (McDaniel & Slack, 2005) and bias on the part of potential reporters and the CPS system (Miller et al., 2013).

My primary analysis focused on all child maltreatment reports, regardless of substantiation status, since both substantiated and unsubstantiated cases predict similar risks of future re-victimization (Kohl, Jonson-Reid, & Drake, 2009) and poor behavioral, developmental, and health outcomes (Kugler et al., 2019). I tested the robustness of my results to using substantiated reports only, which comprise approximately 1 in 5 screened-in reports of child maltreatment made during the study period.

I assembled my outcome variable—weekly, state-level rates of child maltreatment reports per 100,000 children—using information about the number of reports made per week in each state and child population estimates from the U.S. Census Bureau. I constructed weeks to match those in the IRS administrative dataset and excluded observations for states that did not submit consistent child maltreatment data to NDACAN over the study period (128 state-week observations associated with two states: North Carolina and Vermont). In total, my panel dataset contained 3,136 state-week observations of child maltreatment report rates in 48 states and the District of Columbia from 2015 through 2018.

1.5.3. Statistical Analysis

My quasi-experimental approach used a fixed-effects, difference-in-differences linear regression model that relied on changes to the timing and magnitude of EITC/CTC payments in the weeks of tax season caused by the PATH Act. My models regressed weekly state child maltreatment report rates on weekly state per child tax refunds, controlling for state and time fixed effects. The models included a lag structure to test whether child maltreatment report rates

were associated with tax refund amounts up to four weeks after the initial cash transfer. Specifically, I estimated the following model specification:

$$CM_{swy} = \alpha + \sum_{i=0}^{4} \beta_i TaxRefunds_{swy-i} + \gamma_s + \lambda_w + \delta_y + \sigma_H + \varepsilon_{Swy}$$

where CM_{swy}, is the number of child maltreatment reports made per 100,000 children in state s in week w in year y. The primary variable of interest in my model is $TaxRefunds_{swy-i}$, which indicates the per-child EITC/CTC refund dollars issued i weeks before week w in *year* y in state s. β represents the change in the state-level child maltreatment report rate associated with an additional $1000 in per-child EITC/CTC tax refunds. I controlled for state fixed-effects (γ_s) to adjust for any time-invariant factors affecting each state that may be correlated with both EITC/CTC tax refunds and child maltreatment reports. For example, per-child EITC/CTC refunds are larger in states with higher poverty rates, and poverty is positively correlated with maltreatment risk. I also controlled for the week (λ_w), year (δ_y), and holiday (σ_H) fixed effects to adjust for any temporal factors that are correlated with both EITC/CTC tax refunds and child maltreatment report rates.

To assess the robustness of my results, I repeated my analyses but included state-specific linear time trends (at the year and week levels) in the model to account for any temporal changes within states that may be correlated with both changes in the child maltreatment report rates and EITC/CTC refunds. I also tested the sensitivity of my estimates to the exclusion of either post-Path Act year (2017 or 2018) since survey evidence suggests that households claiming the EITC/CTC were largely unaware of the coming delays in payments in 2017 (Maag, Roll, & Oliphant, 2016). Given this financial shock, financially vulnerable families may have had

difficulty smoothing their consumption or paying off bills over the delay period, which in turn may have increased hardship, thus affecting child maltreatment risk. Since many households claim the EITC/CTC over subsequent years, awareness of the change in timing of EITC/CTC payments likely increased between 2017 and 2018. Given differences in awareness of the EITC/CTC delay between 2017 and 2018, I explored the potential for differential effects on child maltreatment reports in 2017 and 2018.

1.6. Results

Among 48 states and the District of Columbia, the average weekly rate of reported child maltreatment was 67 reports per 100,000 children. Average state-level per-child EITC/CTC refund dollars were $1,467 per child. These two variables are shown in Table 1-2. Regression results in Table 1-3 indicate a statistically significant reduction in rates of child maltreatment reports associated with higher EITC/CTC tax refund amounts in the week of issuance, the week after issuance, and three weeks after issuance. The largest impact of EITC/CTC refunds occurred three weeks after refund issuance, with child maltreatment reports decreasing by 7.1 per 100,000 children (95% confidence interval = -10.2, -3.9), which may reflect the time needed for tax refunds to alleviate forms of material hardship -- such as food insecurity or problems paying for utilities or rent -- that can increase maltreatment risk. The total cumulative effect over the week of refund issuance plus the four weeks following issuance is shown in the bottom row of Table 1-3. In total, state-level rates of reported child maltreatment decreased by an estimated -16.8 reports per 100,000 children (95% confidence interval = -26.0, -7.7), equivalent to a 5.0% reduction in maltreatment reports (95% confidence interval = 2.3%, 7.7%), with each additional

$1000 per child in the state. The estimate of the cumulative EITC/CTC effect based solely on substantiated cases was smaller in size and less precise (-1.3 substantiated reports per 100,000 children; 95% confidence interval = -3.0, 0.3), partially reflecting that 1 in 5 reports reached substantiated status.

These results were largely unchanged by the inclusion of state-specific linear time trends in my models (Table 1-4). Estimates including only 2017 (Table 1-5) were qualitatively similar but slightly larger in magnitude than those including only 2018 (cumulative estimates for 2017 only: -21.1 reports per 100,000 children; 95% confidence interval = -29.3, -12.8; cumulative estimates 2018 only: -16.4 reports per 100,000 children; 95% confidence interval = -28.6, -4.1). Larger point estimates for 2017 might suggest that the unanticipated delay (and any associated difficulty in smoothing through the EITC/CTC delay period) led to an increase in child maltreatment risk. However, the qualitative similarity of results indicates that my baseline results, which pooled together child maltreatment rates for all years in the sample, are not substantially driven by the unanticipated delay.

1.7. Discussion

I used a legislative change in the timing of EITC/CTC payments to estimate the association between state per child income assistance and state reports of child maltreatment in the weeks after families received the additional income. Results of this study suggest an association between EITC/CTC tax refunds and lower rates of maltreatment reports. I found that for each additional $1000 in per-child EITC/CTC tax refunds, rates of reported maltreatment declined in the several weeks immediately following refund payments by an estimated 5.0%.

The study design had several strengths. First, given the difficulty of randomizing an income support intervention, the PATH Act created variation in weekly issuance that is unlikely to have been affected by individual tax filers behavior or by other economic and policy factors, including changes in state's child welfare policies and practices, which might confound a purely observational study. Second, the size of the population affected by the legislated shift in the timing of EITC/CTC refund issuance was large: the PATH Act shifted the timing of receipt of refunds containing the EITC or CTC for an estimated more than 12 million families in the tax seasons following implementation. Third, evidence points to lump-sum tax credits as a salient income source, which many low-income families spend shortly after receipt on necessities or to pay down debt, increasing the likelihood of an observable effect on child maltreatment in the short-term.

These results align with a small body of evidence on the role of income support policies in child maltreatment incidence. One study found a $1000 increase in income via the EITC led to a 3%-4% decrease in child neglect and an 8%-10% decrease in CPS involvement among low-income single-mother families (Berger, Font, & Slack, 2017). Another study found that the introduction of a refundable EITC credit, offered at the state level, was associated with a 13% decrease in state-level rates of hospital admissions for pediatric abusive head trauma (Klevens et al., 2017). These studies indicate how even relatively small increases in income may decrease maltreatment risk.

The short-term changes to child maltreatment rates that I observed after EITC/CTC refund issuance aligns with the notion that child maltreatment risk is responsive to not only chronic economic hardship among parents but also to more immediate income availability. This finding has important implications for policies seeking to improve families' economic security,

particularly amidst rising levels of intra-year income volatility among low-wage U.S. workers (Hardy & Ziliak, 2014). Policymakers might consider how income support programs can mitigate income volatility such that short-term financial insecurity does not translate to long-term negative consequences (e.g., long-term health problems due to child maltreatment). In 2021, the American Rescue Plan temporarily expanded the CTC to a nearly universal child benefit, partially paid to families in monthly installments from July to December 2021. Early evaluations of the CTC extension found that these monthly supplements reduced child poverty (Parolin, Collyer, & Curran, 2021) and material hardship (Shafer et al., 2022; Parolin et al., 2021). More frequent payments of benefits to families may reduce intra-year material hardship and financial stress (Greenlee et al., 2020) by encouraging savings (Halpern-Meekin, Greene, Levin, & Edin, 2018) and aligning payments with the timing of critical financial events throughout the year.

This study had some limitations. First, my analysis was based on cases of suspected maltreatment reported to CPS. A substantial fraction of incidents goes unreported, however, and some subsets of reported cases do not reflect actual maltreatment. Therefore, observed declines in CPS reports might reflect a change in reporting rather than a true change in maltreatment. Nonetheless, official reports are considered among the best available indicators of child maltreatment risk and track other key indicators of risk (e.g., infant mortality) (Drake et al., 2011). Furthermore, if tax credits reduce needless CPS reports, they would alleviate the burden of unnecessary reports on families and potentially improve the functioning of the child welfare system (Font & Maguire-Jack, 2020). Second, the administrative child maltreatment dataset that I obtained and could be matched to the weekly IRS data did not include information on child or family characteristics -- such as child age, race, ethnicity, or family income level -- nor on the type of maltreatment alleged (e.g., physical abuse, neglect, sexual abuse) due to privacy

concerns. In the future, an analysis focused on neglect, which is strongly associated with families' economic circumstances and makes up the single largest report category (US Department of Health and Human Services, 2018), would be informative. Increasingly, researchers have suggested economic and anti-poverty policies to reduce neglect rates (Feely, Raissian, Schneider, & Bullinger, 2020), which have remained persistently high over recent decades, whereas rates of substantiated abuse have declined (Finkelhor, Saito, & Jones, 2018; Bullinger, Feely, Raissan, & Schneider, 2020). Understanding heterogeneity by race would also be important because tax policies such as the EITC and CTC may influence long-standing racial and ethnic inequities in the child welfare system, with rates of CPS contact especially elevated among Black child and Native American children (Edwards, Wakefield, Healy, & Wildeman, 2021). Third, these data did not allow us to assess the precise effect of EITC/CTC refunds on maltreatment reports at the family level. Consequently, these results should be viewed as pertaining to state-level rates of CPS reports and caution should be taken in generalizing these aggregate estimates to the individual level. Fourth, my EITC/CTC measure is based on the timing of IRS issuance. The refund delivery method – such as direct deposit, paper check, or refund anticipation loan – can influence the exact timing of receipt, which I was only able to approximate from the IRS data. Finally, I could not separately determine the effects of tax refunds containing the CTC or EITC since refund amounts were aggregated. Due to differences between the EITC and CTC, in terms of eligibility rules and credit structure (e.g., full versus partial refundability), the two tax credits have different distributional consequences and thus may have differential impacts that depend on which households are targeted with benefits.

1.8. Conclusion

Child maltreatment increases risks for behavioral, physical, and mental health problems over the life course. This study adds to evidence that poverty-reducing tax credits improve several outcomes for children, as well as to a body of evidence that child maltreatment is malleable to social policy changes. Past studies indicate that expansions of minimum wage laws (Raissian & Bullinger, 2017) the Affordable Care Act (Brown et al., 2019), and state paid family leave (Klevens et al., 2016) are each associated with reductions in various indicators of child maltreatment. Collectively, these findings highlight how expansive public policies and programs can influence children's wellbeing by reducing maltreatment risk and diverting families from CPS. These findings are particularly relevant because the EITC and CTC are active areas of policymaking at both the state and federal levels. It is rare for child maltreatment and child welfare outcomes to be considered in the design and implementation of social policies which operate independently of the child welfare system (Berger & Slack, 2020); however, these issues should receive significant attention in policy debates.

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1.10. Tables and Figures

Table 1-1 Dates associated with each week in analysis

YEAR	WEEK	DATES
2015	1	1/12/15 — 1/18/15
2015	2	1/19/15 — 1/25/15
2015	3	1/26/15 — 2/1/15
2015	4	2/2/15 — 2/8/15
2015	5	2/9/15 — 2/15/15
2015	6	2/16/15 — 2/22/15
2015	7	2/23/15 — 3/1/15
2015	8	3/2/15 — 3/8/15
2015	9	3/9/15 — 3/15/15
2015	10	3/16/15 — 3/22/15
2015	11	3/23/15 — 3/29/15
2015	12	3/30/15 — 4/5/15
2015	13	4/6/15 — 4/12/15
2015	14	4/13/15 — 4/19/15
2015	15	4/20/15 — 4/26/15
2015	16	4/27/15 — 5/3/15
2016	1	1/11/16 — 1/17/16
2016	2	1/18/16 — 1/24/16
2016	3	1/25/16 — 1/31/16
2016	4	2/1/16 — 2/7/16
2016	5	2/8/16 — 2/14/16
2016	6	2/15/16 — 2/21/16
2016	7	2/22/16 — 2/28/16
2016	8	2/29/16 — 3/6/16
2016	9	3/7/16 — 3/13/16
2016	10	3/14/16 — 3/20/16
2016	11	3/21/16 — 3/27/16
2016	12	3/28/16 — 4/3/16
2016	13	4/4/16 — 4/10/16
2016	14	4/11/16 — 4/17/16
2016	15	4/18/16 — 4/24/16
2016	16	4/25/16 — 5/1/16
2017	1	1/9/17 — 1/15/17
2017	2	1/16/17 — 1/22/17

2017	3	1/23/17 — 1/29/17
2017	4	1/30/17 — 2/5/17
2017	5	2/6/17 — 2/12/17
2017	6	2/13/17 — 2/19/17
2017	7	2/20/17 — 2/26/17
2017	8	2/27/17 — 3/5/17
2017	9	3/6/17 — 3/12/17
2017	10	3/13/17 — 3/19/17
2017	11	3/20/17 — 3/26/17
2017	12	3/27/17 — 4/2/17
2017	13	4/3/17 — 4/9/17
2017	14	4/10/17 — 4/16/17
2017	15	4/17/17 — 4/23/17
2017	16	4/24/17 — 4/30/17
2018	1	1/8/18 — 1/14/18
2018	2	1/15/18 — 1/21/18
2018	3	1/22/18 — 1/28/18
2018	4	1/29/18 — 2/4/18
2018	5	2/5/18 — 2/11/18
2018	6	2/12/18 — 2/18/18
2018	7	2/19/18 — 2/25/18
2018	8	2/26/18 — 3/4/18
2018	9	3/5/18 — 3/11/18
2018	10	3/12/18 — 3/18/18
2018	11	3/19/18 — 3/25/18
2018	12	3/26/18 — 4/1/18
2018	13	4/2/18 — 4/8/18
2018	14	4/9/18 — 4/15/18
2018	15	4/16/18 — 4/22/18
2018	16	4/23/18 — 4/29/18

Notes: Weeks match those in the tax refund data that we obtained from the IRS's Research, Applied Analytics, and Statistics Group. The first week of each year begins with the second full week in January. All weeks begin on a Monday.

Table 1-2 Average EITC/CTC dollars per child and child maltreatment rates, January-April, 2015-2018

	EITC/CTC Dollars Per Child ($2018)	Weekly Reports of Child Maltreatment per 100,000 Children	Weekly Substantiated Reports of Child Maltreatment per 100,000 Children
Alaska	1067	94	21
Alabama	2023	48	13
Arkansas	1827	102	20
Arizona	1502	58	9
California	1323	53	10
Colorado	1110	55	12
Connecticut	1199	49	15
District of Columbia	1627	121	18
Delaware	1540	65	9
Florida	2037	83	13
Georgia	1844	73	10
Hawaii	1408	13	5
Iowa	1256	81	20
Idaho	1230	42	6
Illinois	1526	54	13
Indiana	1538	151	22
Kansas	1309	71	6
Kentucky	1694	111	27
Louisiana	1993	42	14
Massachusetts	1131	67	29
Maryland	1274	33	9
Maine	1427	69	18
Michigan	1470	84	22
Minnesota	1049	51	8
Missouri	1537	108	6
Mississippi	2351	76	20
Montana	1259	86	17
North Carolina	-	-	-
North Dakota	1040	48	13
Nebraska	1278	52	8
New Hampshire	1108	79	5
New Jersey	1269	60	5

New Mexico	1806	87	25
Nevada	1728	49	9
New York	1589	81	23
Ohio	1523	65	14
Oklahoma	1538	75	17
Oregon	1177	66	14
Pennsylvania	1437	35	3
Rhode Island	1630	66	21
South Carolina	1812	71	19
South Dakota	1291	22	7
Tennessee	1849	99	10
Texas	1679	54	11
Utah	988	46	15
Virginia	1357	38	4
Vermont	-	-	-
Washington	1148	47	5
Wisconsin	1203	44	6
West Virginia	1736	135	21
Wyoming	1133	45	8

Sources: EITC/CTC refund data from the Internal Revenue Service: Research, Applied Analytics, and Statistics Group; Child maltreatment data from the National Data Archive on Child Abuse and Neglect, January-April, 2015-2018.
Notes: Weekly, state-level rates of child maltreatment per 100,000 children were constructed using information about the date of each report and the state in which each report was made. All state-level EITC/CTC refund amounts are expressed in 2018 dollars. Refund amounts were made comparable across states by dividing the total EITC/CTC dollars by state child population. Annual state-level child population estimates were obtained from the U.S. Census Bureau (https://www.census.gov/data/tables/time-series/demo/popest/2010s-state-detail.html). Observations for Vermont and North Carolina were excluded because these states were not consistently observed in the child maltreatment dataset.

Table 1-3 EITC/CTC tax refund effects on child maltreatment report rates

	Number of Child Maltreatment Reports Per 100,000 Children	Number of Substantiated Child Maltreatment Reports Per 100,000 Children
EITC/CTC: Week of Issuance	-3.6 (-6.0, -1.2)**	-0.9 (-1.8, 0.1)
EITC/CTC: Issued 1 Week Before	-3.8 (-6.9, -0.8)*	-0.4 (-1.0, 0.2)
EITC/CTC: Issued 2 Weeks Before	-2.4 (-5.1, 0.3)	0.1 (-0.6, 0.8)
EITC/CTC: Issued 3 Weeks Before	-7.1 (-10.2, -3.9)**	-0.9 (-1.7, -0.1)*
EITC/CTC: Issued 4 Weeks Before	0.1 (-4.2, 4.3)	0.7 (-0.1, 1.5)
Cumulative Effect	-16.8 (-26.0, -7.7)**	-1.3 (-3.0, 0.3)
N	3,136	3,136

Sources: Tax refund data from the Internal Revenue Service: Research, Applied Analytics, and Statistics Group; Child maltreatment data from the National Data Archive on Child Abuse and Neglect, January-April, 2015-2018. Data are at the state-week-year level.
Notes: 95% confidence intervals shown in parentheses. Standard errors were clustered at the state level. Models included state, week, year, and holiday fixed effects. EITC/CTC refund amounts were adjusted for inflation and expressed in 2018 dollars. Refund dollars were made comparable across states by dividing dollar amounts by the state child population. Each coefficient represents a change in the state-level child maltreatment report rate associated with an additional $1000 in per-child EITC/CTC tax refunds.
*P < .05; **P < .01

Table 1-4 EITC/CTC tax refund effects on child maltreatment report rates; robustness check with state-specific linear time trends

	Number of Child Maltreatment Reports Per 100,000 Children	Number of Substantiated Child Maltreatment Reports Per 100,000 Children
EITC/CTC	-2.8 (-5.2, -0.4)*	-0.8 (-1.7, 0.2)
EITC/CTC: 1 Week Before	-3.5 (-6.6, -0.5)*	-0.4 (-1.0, 0.3)
EITC/CTC: 2 Weeks Before	-2.2 (-5.0, 0.6)	0.1 (-0.6, 0.9)
EITC/CTC: 3 Weeks Before	-6.9 (-10.1, -3.7)**	-0.9 (-1.6, -0.2)*
EITC/CTC: 4 Weeks Before	0.1 (-4.2, 4.4)	0.7 (-0.1, 1.5)
Cumulative Effect	-15.3 (-24.4, -6.1)**	-1.2 (-2.8, 0.4)
N	3,136	3,136

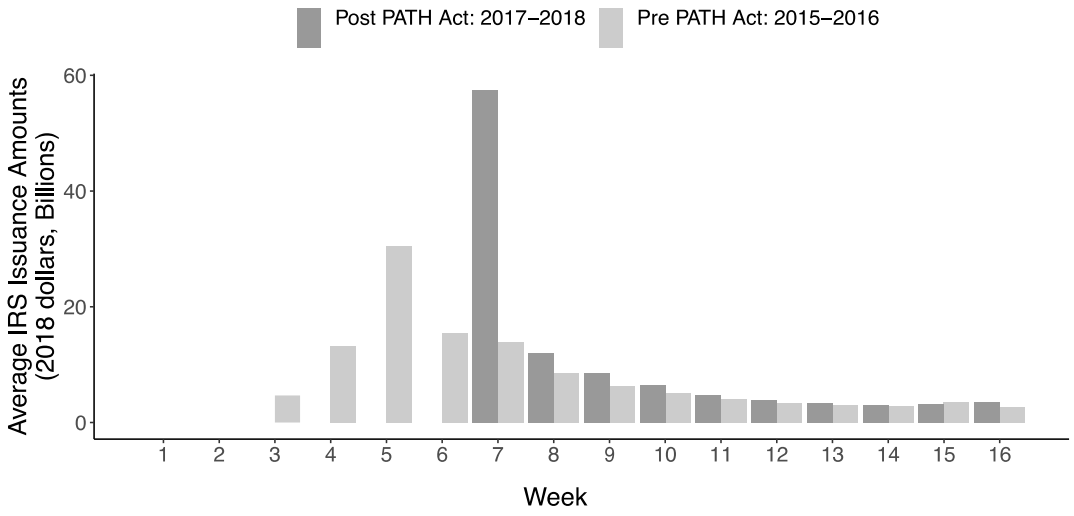
Sources: EITC/CTC refund data from the Internal Revenue Service: Research, Applied Analytics, and Statistics Group; Child maltreatment data from the National Data Archive on Child Abuse and Neglect, January-April, 2015-2018. Data are at the state-week-year level.
Notes: 95% confidence intervals shown in parentheses. Standard errors were clustered at the state level. Models included state, week, year, holiday fixed-effects, and state-specific linear time trends. EITC/CTC refund amounts were adjusted for inflation and expressed in 2018 dollars. Refund dollars were made comparable across states by dividing dollar amounts by the state child population. Each coefficient represents a change in the state-level child maltreatment report rate associated with an additional $1000 in per-child EITC/CTC tax refunds.
*P < .05; **P < .01

Table 1-5 EITC/CTC tax refund effects on child maltreatment report rates; analyses excluding either post-PATH Act year 2017 or 2018

	Number of Child Maltreatment Reports Per 100,000 Children		Number of Substantiated Child Maltreatment Reports Per 100,000 Children	
Post-PATH Act Years Included	**2017 Only**	**2018 Only**	**2017 Only**	**2018 Only**
EITC/CTC	-4.9 (-7.1, -2.7)**	-3.3 (-6.8, 0.3)	-0.8 (-1.8, 0.2)	-1.4 (-2.8, 0.1)
EITC/CTC: 1 Week Before	-4.3 (-6.7, -1.9)**	-2.3 (-6.2, 1.6)	-0.5 (-1.3, 0.2)	0.0 (-0.8, 0.7)
EITC/CTC: 2 Weeks Before	-3.0 (-5.7, -0.3)*	-3.9 (-7.7, -0.2)*	0.1 (-0.7, 1.0)	-0.4 (-1.3, 0.5)
EITC/CTC: 3 Weeks Before	-8.7 (-12.3, -5.1)**	-5.4 (-8.6, -2.3)**	-1.4 (-2.2, -0.5)**	-0.5 (-1.2, 0.2)
EITC/CTC: 4 Weeks Before	-0.2 (-3.6, 3.2)	-1.4 (-7.3, 4.5)	0.8 (0.0, 1.5)	0.3 (-0.6, 1.2)
Cumulative Effect	-21.1 (-29.3, -12.8)**	-16.4 (-28.6, -4.1)**	-1.9 (-3.8, 0.1)	-2.0 (-4.2, 0.2)
N	2,352	2,352	2,352	2,352

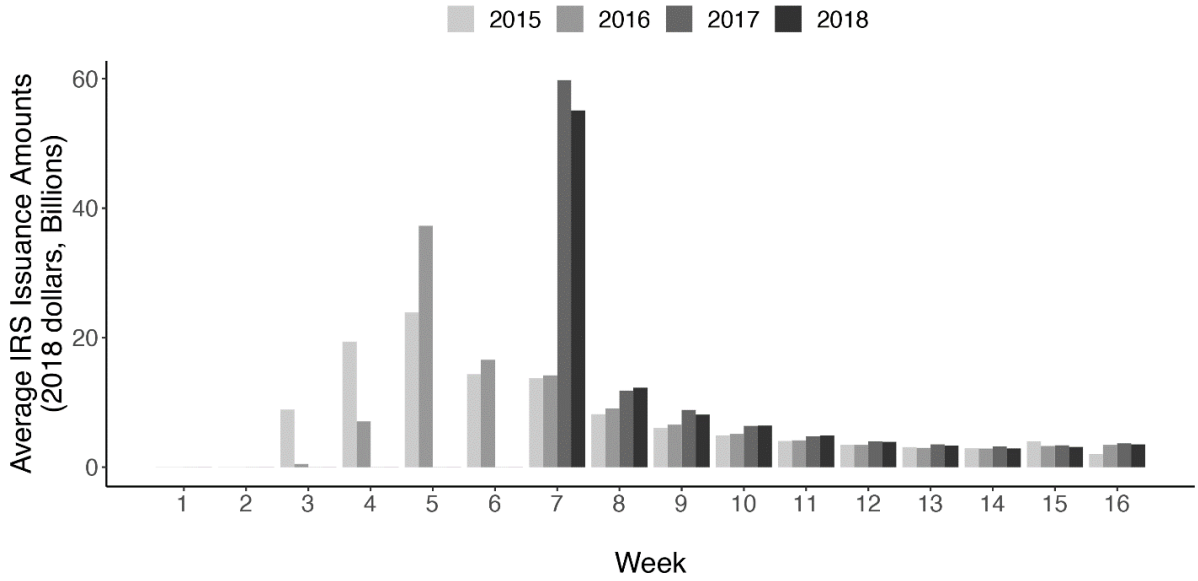
Sources: EITC/CTC refund data from the Internal Revenue Service: Research, Applied Analytics, and Statistics Group; Child maltreatment data from the National Data Archive on Child Abuse and Neglect, January-April, 2015-2018. Data are at the state-week-year level.
Notes: 95% confidence intervals shown in parentheses. Standard errors were clustered at the state level. Models included state, week, year, holiday fixed-effects. EITC/CTC refund amounts were adjusted for inflation and expressed in 2018 dollars. Refund dollars were made comparable across states by dividing dollar amounts by the state child population. Each coefficient represents a change in the state-level child maltreatment report rate associated with an additional $1000 in per-child EITC/CTC tax refunds.
*P < .05; **P < .01

Figure 1-1 EITC and CTC refunds: January-April, 2015-2018



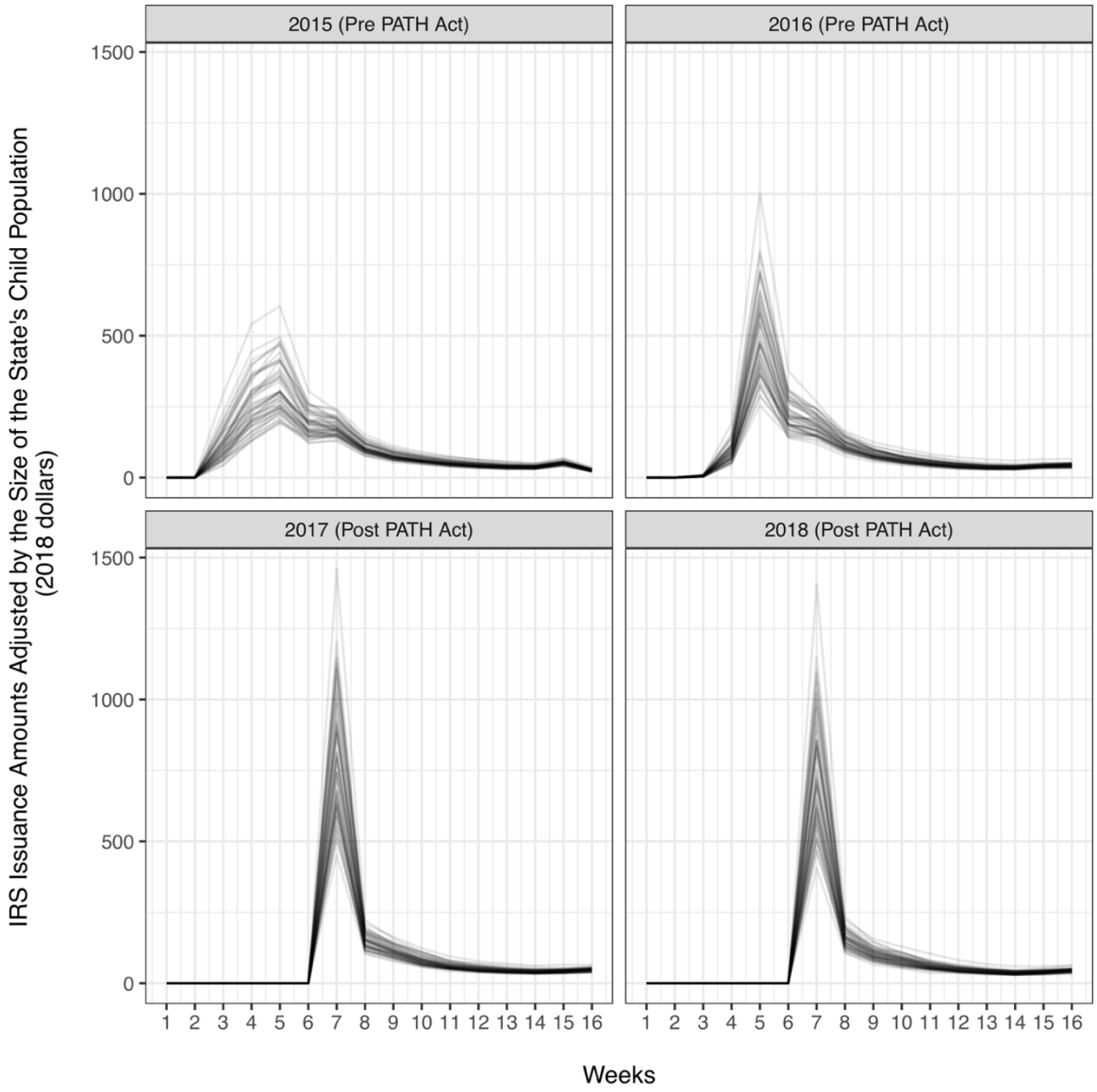
Source: Authors' analysis of data from the Internal Revenue Service: Research, Applied Analytics, and Statistics Group. *Notes*: Dollar amounts include entire refunds containing the EITC or refundable portion of the CTC. All refunds amounts expressed in 2018 dollars.

Figure 1-2 EITC/CTC refunds, January-April, 2015-2018



Source: Internal Revenue Service: Research, Applied Analytics, and Statistics
Notes: Dollar amounts include entire refunds containing the EITC or refundable portion of the CTC. All refunds amounts are expressed in 2018 dollars.

Figure 1-3. Per-child EITC/CTC refunds by state, January-April, 2015-2018



Source: Internal Revenue Service: Research, Applied Analytics, and Statistics
Notes: Dollar amounts include entire refunds containing the EITC or refundable portion of the CTC. All refunds amounts are expressed in 2018 dollars. Weekly EITC/CTC refund amounts were made comparable across states by dividing the amounts by the state child population. Each line represents refunds containing the EITC or refundable portion of the CTC for individual states.

2. The Earned Income Tax Credit and Intimate Partner Violence During Pregnancy

2.1. **Abstract**

The Earned Income Tax Credit (EITC), a refundable tax credit available to low-income households with earnings, is one of the largest cash transfer programs in the United States. The EITC has been shown to have positive spillover benefits for maternal-infant health among low-income populations. The EITC may also affect prenatal exposure to intimate partner violence, the risk of which is heightened by poverty and financial strain. In this study, I use individual-level data from the Pregnancy Risk Assessment Monitoring System (PRAMS), covering births in years 1994-2017, and a difference-in-differences methodological approach to examine whether policy-induced expansions to the EITC at both the state and federal levels affect risk of intimate partner violence among pregnant women in the U.S. Overall, I find that expansions to the EITC reduce intimate partner violence among pregnant women. The effect is concentrated among women with lower levels of educational attainment. In race-stratified analyses, I find some evidence that effects were particularly pronounced for non-Hispanic Black women.

These findings suggest that reducing poverty during the prenatal period would reduce adverse maternal exposures such as intimate partner violence, which are consequential for both maternal and infant health.

2.2. **Introduction**

Intimate partner violence against women is a major public health challenge facing the United States. The Centers for Disease Control and Prevention (CDC) estimate that around 1 in 3 U.S. women will experience intimate partner violence in their lifetime (Smith et al., 2018). The adverse consequences of intimate partner violence for health are well-established (Rivara et al., 2019). Although women of all ages experience intimate partner violence, women of reproductive age face higher risks (Rivara et al., 2009), especially those living in low-income households (Braveman et al., 2010). Risk of exposure to intimate partner violence is heightened by economic stressors such as poverty and financial hardship (Matjasko, Niolon, & Valle, 2013; Capaldi, Knoble, Shortt, & Kim, 2012; Schneider, Harknett, & McLanahan, 2016), which are common among women around the time of childbirth in the United States (Taylor et al., 2021; Braveman et al., 2010). Exposure to violence during early life (including in utero exposure) has serious long-term implications for physical, mental and cognitive health, and thus, is thought to contribute to the integrational transmission of inequality (Aizer & Currie, 2014).

Given the connection between economic hardship and intimate partner violence, there is growing interest in economic support policies as a potential means of prevention (Niolon et al., 2017). High prevalence of poverty and financial hardship around the time of childbirth have also increased calls for policy efforts to improve economic security among peripartum women. In contrast to many other wealthy nations, the U.S. lacks certain federal policies designed to protect against economic hardship around the time of childbirth, such as paid maternity leave or a universal (or near-universal) child-raising allowance (i.e., a cash benefit to help parents cover the

costs of children's basic needs).[2] Without such targeted supports, it is important to assess the role of other general public income supports such as the Earned Income Tax Credit (EITC), which women might access around the time of childbirth.

In recent decades, the EITC has dramatically expanded as a policy tool for reducing poverty, especially among households with children, and is now the largest cash transfer program in the U.S. In 2021, around 25 million U.S. households received approximately $60 billion in EITC benefits (IRS, 2021). Each year, the EITC lifts millions of individuals, including several million children, above the federal poverty line and reduces the severity of poverty for many more (Renwick & Fox, 2016; Hoynes & Patel, 2018; Meyer & Wu 2018). Forty-four percent of tax filers with children receive the credit (Hoynes, 2019). In a context of well-documented evidence that economic factors are related to intimate partner violence, and high prevalence of poverty and financial hardship for women around the time of childbirth, the EITC may have implications for the risk of intimate partner violence among a group of particularly vulnerable individuals: pregnant women.

Using nationally representative survey data from the Pregnancy Risk Assessment Monitoring System (PRAMS), covering births in years 1994-2017, and a difference-in-differences methodological approach, I investigate whether the EITC affects intimate partner violence among pregnant women in the U.S. By focusing on women during pregnancy, this study can increase our understanding of how the EITC affects health during a critical period for maternal health and infant development. Growing evidence suggests that in-utero exposures,

[2] In the U.S., federal supports that target perinatal women are typically provided in the form of in-kind transfers (e.g., nutritional assistance and coverage for health care). Nine states and D.C. have enacted paid family and medical leave laws. The U.S. is the only OECD country lacking some type of paid parental leave at the national level. Many OECD countries provide a universal cash benefit (i.e., a "child allowance") to all families with children (Shaefer et al., 2018).

including intimate partner violence, are particularly impactful for newborn health, with lasting negative effects on later life health and socioeconomic outcomes (Aizer & Currie, 2014). By studying impacts on this population, this study can also help inform policy discussions about whether expansions of the EITC or other income support policies might improve the lives of both women and their children, as well as reduce the societal costs of violence against women during pregnancy, which recent research suggests exceed $3.8 billion per year (Currie, Mueller-Smith, & Rossin-Slater, 2022).

I also investigate variation in effects of the EITC on intimate partner violence during pregnancy by race/ethnicity, education, and number of previous births. Assessing the impact of the EITC by race/ethnicity is important because rates of intimate partner violence are disproportionately high among racial/ethnic minority women (Breiding, Chen, & Black, 2014), and differential exposure to violence and other adversities during developmentally sensitive periods for health, including the prenatal period, is a key determinant of health disparities (Almeida et al., 2018; Koning & Ehrenthal, 2019). Additionally, recent research highlights how tax policies such as the EITC play a role in structuring the distribution of economic resources along racial/ethnic lines in the U.S. (Huang & Taylor, 2019; Acevedo-Garcia et al., 2021; Brown, 2021; Michelmore, & Lopoo, 2021; Hardy, Hokayem, Ziliak, 2022). Understanding whether there are differential effects of the EITC by maternal race/ethnicity can better inform policy efforts to improve birth outcomes and reduce health disparities. In addition to race-stratified analyses, I conduct subgroup analyses by education and number of previous births in order to assess whether my results are concentrated among demographic groups that are most likely to be affected by the EITC due to higher rates of participation (i.e., those with less education and at least one previous birth).

2.3. Background

2.3.1. The Earned Income Tax Credit

The federal EITC, first enacted in 1975, was originally designed as a small credit to offset payroll taxes among low-wage workers. In the past few decades, the EITC has grown dramatically, both in terms of the size of the credit available to eligible families and the share of families who receive the credit. Between 1975 and 2018, the maximum federal EITC payment for which an individual or household might be eligible grew from around $1,850 to around $6,700 (in 2021 dollars), and the total number of annual EITC recipients increased from 6 million to over 25 million. Today, the EITC is one of the largest cash transfer programs in the U.S (Hoynes, 2019).[3] Spending on the EITC is around $60 billion annually (IRS, 2021). The EITC's growth is reflective of a larger policy shift in recent decades in how the U.S. provides income assistance to low-income families. Over time, the U.S. has expanded "in-work" tax credits (i.e. credits that require earnings in order to qualify for benefits) such as the EITC, while scaling back other major cash assistance programs (e.g., Temporary Assistance for Needy Families [TANF]) and cutting benefits available to families without earnings (Moffitt 2015; Parolin, 2021).

[3] The EITC has undergone a series of legislative expansions over recent decades. See Crandall-Hollick (2022) for a detailed description of the legislative history of the EITC. In brief, the Tax Reform Act of 1986 increased the credit size and permanently adjusted the credit for inflation; the Omnibus Budget Reconciliation Acts of 1990 and 1993 increased the credit size and altered formulas depending on number of qualifying children (the later also created a new credit for families without children); the American Recovery and Reinvestment Act of 2009 created a new larger credit for families with three or more children; and the American Rescue Plan Act of 2021 temporarily increased the size of the "childless" EITC) and adjusted other parameters (e.g., increased the "phase-in" and "phase-out" rates).

Because the EITC is administered through the income tax system, EITC payments are disbursed to recipients once per year at tax time (i.e., as a "lump-sum" payment). In order to claim EITC benefits, eligible individuals must file a tax return. The overall take-up rate for the federal EITC (i.e., the share of eligible individuals or families who actually receive the credit) is around 80% for households with children (Jones, 2014). Eligibility for the EITC is determined by several household factors – prior tax year's earnings, number of dependent children, and marital status. Benefit levels increase with each additional child residing in the household up to three children. A relatively small credit is available to childless individuals;[4] however, the vast majority of total federal EITC dollars (an estimated 97% in 2018) go to low-income families with dependent children (Crandall-Hollick, Falk, & Boyle, 2021).

In 2020, the maximum federal credit for individuals with no dependent children was $538, compared to $3584, $5920, and $6660, for families with one, two, and three or more dependent children, respectively (the average credit was $302, $2,369, $3,847 and $4,311 for recipients with zero, one, two, and three or more children; Crandall-Hollick, Falk, Boyle, 2021). Eligibility for the EITC is conditional on earnings in the tax year prior to actual EITC benefit receipt. The credit "phases-in" with earnings up to a plateau range, where households are eligible for the maximum credit. Eventually, as a household's earning increase, the credit "phases-out" with further earnings. The rate at which the credit "phases-in" and "phases-out" depends on the tax filer's number of dependent children and marital status.

In addition to the federal credit, many states have introduced their own state-level version of the EITC to supplement the federal credit. Over the past 25 years, state EITCs have grown as way in states provides income support to working families. In 1986, Rhode Island became the

[4] The American Rescue Plan of 2021 temporarily expanded the "childless" EITC from $543 to $1,502 for 2021 (Crandall-Hollick, 2021), which is beyond the study period.

first state to introduce a state-level version of the EITC. As of 2021, 30 states and the District of Columbia have enacted their own EITC. Most state-level credits are expressed as a percentage of the federal credit. While the federal EITC is fully refundable (i.e., available in full to tax filers regardless of their income tax liability), several states offer a credit that is non-refundable (most state EITCs are fully refundable). This refundability status has implications for poverty-reduction, with fully refundable credits most effective at achieving poverty-reduction (Pac et al., 2020). While the majority of state EITCs follow the federal credit's eligibility criteria, there is considerable variation across states in the size of benefits offered as well as in the timing of enactments and reforms to existing credits. Thus, in addition to the temporal variation in the federal EITC, my analysis is able to leverage variation in EITC policies between states and within states over time.

2.3.2. How the EITC May Affect Intimate Partner Violence

There are number of reasons that the EITC might affect risk of maternal exposure to intimate partner violence. First, by reducing low-income families' material hardship and financial pressure, the EITC may reduce stress and within-couple conflict, which are key predictors of intimate partner violence (Capaldi, Knoble, Shortt, & Kim, 2012; Matjasko, Niolon, & Valle, 2013; Schwab-Reese, Peek-Asa, & Parker, 2016; Lucero, Lim, & Santiago, 2016). Second, the EITC provides recipients with cash that they might then use to exit an abusive relationship. Financial resources are needed flee abuse, and concerns about subsequent hardships (e.g., housing instability, homelessness) are major reported barriers to leaving a violent

relationship (Adams et al., 2018). Prior research indicates that the EITC reduces certain housing hardships (e.g., cost burdens, crowding) (Pilkauskas & Michelmore, 2019); thus, the EITC may expand options for women to live independently of an abusive partner. Women may be particularly motivated during pregnancy to use additional income to exit an unhealthy relationship in order to protect their new baby from future exposures to violence (Schaefer et al., 2021; Alhusen & Wilson, 2015). Third, the EITC is explicitly designed to incentivize work and has been show to increase employment and long-term earnings potential among women (Michelmore & Pilkauskas, 2021; Dahl, DeLeire, & Schwabish, 2009; Schanzenbach & Strain, 2021), which in turn, may increase their autonomy, relative earnings, and bargaining power within relationships – factors previously found to be associated with lower risk of intimate partner violence (Aizer 2010; Matjasko, Niolon, & Valle, 2013). Although these aforementioned channels may decrease intimate partner violence, it is also possible that women's EITC receipt and earnings might trigger reactions from partners who feel threatened and use violence to reassert their dominance (Matjasko, Niolon, & Valle, 2013).

There are also a number of reasons that the EITC's design and payment structure may limit its effectiveness as a tool to prevent intimate partner violence. In particular, EITC benefits are contingent on having earned income, yet intimate partner violence victimization is associated with difficulties maintaining stable employment (Logan, Shannon, Cole, & Swanberg, 2007). Abuse can undermine work stability directly (e.g, if an abuser sabotages a victim's job) or indirectly through its effects on psychological or physical wellbeing (Logan, Shannon, Cole, & Swanberg, 2007; Tolman & Wang, 2005). If victims are unable to maintain employment, then the EITC may do little to help them achieve economic independence and/or leave abusive relationships. Similarly, abusers may monitor or exert control over victims' income or finances,

thereby limiting women's access to household EITC benefits or prohibiting their tax filing (the mechanism for EITC enrollment). Lastly, because the EITC is disbursed to families once per year at tax time (rather than as a periodic payment throughout the year), the timing of income receipt may poorly align with the timing of economic needs that can influence relationship dynamics or the decision to exit a violent relationship.

2.3.3. Prior Studies

Despite policy interest in the impact of the EITC on intimate partner violence and possible theoretical reasons for an association, empirical evidence on the effect of the EITC on intimate partner violence is limited. To date, only four studies have attempted to identify a causal relationship between the EITC and intimate partner violence, yielding mixed results. Some studies have found a relationship between higher EITC benefits and women's lower risk of experiencing various forms of intimate partner violence (Cesur, Rodriguez-Planas, Roff, & Simon, 2022; Spencer et al., 2020), while others have not found an association (Moe et al., 2020; Edmonds et al., 2020). These studies vary in terms of sample population, methodological approach, and the type of intimate partner violence considered.

Using data from the Fragile Families and Child Wellbeing Study, one study found a protective effect of refundable state-level EITCs on low-income mother's self-reported exposures to coercive isolation and economic abuse by an intimate partner (Spencer et al., 2020). Another recent study, using data from National Crime Victimization Survey, found that federal expansions to the EITC in the early 1990s reduced self-reports of physical or sexual assault

among women, especially for unmarried women and Black women (Cesur, Rodriguez-Planas, Roff, & Simon, 2022). However, two other studies indicate no association between higher state-level EITC benefits and state-level rates of intimate partner homicide in the National Crime Victimization Survey (Moe et al., 2020), state-level rates of self-reported violent victimization in the National Crime Victimization Survey (Edmonds et al., 2020), and a composite measure of low-income mothers' self-reported exposures to emotional abuse, physical abuse, or sexual abuse in the Fragile Families and Child Wellbeing Study (Edmonds et al., 2020).

Outside of the U.S., a larger number of studies on the role of cash transfers in intimate partner violence have been conducted. This research generally suggests that cash transfers are associated with lower risk of intimate partner violence. A recent review of studies conducted in low- and middle-income countries found that 11 of 13 quantitative studies and 5 of 8 qualitative studies reported evidence that cash transfer programs decrease intimate partner violence; with only two studies showing overall mixed impacts or increases in intimate partner violence (Buller et al., 2018). Importantly, however, the cash transfer programs examined in these studies tend to be functionally different from the EITC in a number of respects -- most target women as the main recipient, deliver benefits on a monthly basis, and deliver other services (e.g., health care, education). Furthermore, the effect of cash transfers on the risk of violence is likely contingent on other external factors, such as social and gender norms. Therefore, it is unclear the extent to which the findings of these studies might generalize to the EITC.

Overall, these studies provide mixed initial evidence about whether income supplementation reduces intimate partner violence. The focus of this study is to investigate the effects of the EITC on intimate partner violence among pregnant women. Intimate partner violence is a major problem in and of itself, but especially so among pregnant women since their

exposures not only matter for their own health but for birth outcomes as well (Almond, Currie, & Duque, 2017). There are also reasons why risk of intimate partner violence may be uniquely sensitive to income around the time of childbirth compared to other times. Declines in women's earnings and employment around the time of childbirth, which tend to be large (Stanczyk, 2020), likely increase financial strain and widen earnings differentials between partners, which may have implications for bargaining power and vulnerability to violence. Leveraging variation in EITC policies over time and across states, I use a difference-in-differences approach to estimate the effect of exposure to the EITC during the year before childbirth on intimate partner violence among pregnant women in the U.S. I also explore whether the EITC reduces financial hardship, a proposed mechanism by which economic support policies may influence intimate partner violence. The EITC has been shown to improve the financial circumstances of low-income mothers in the U.S. (e.g., Jones & Michelmore, 2018; Shaefer, Song, & Shanks, 2013; Michelmore & Lopoo, 2021); however, to my knowledge, whether this effect holds among a subgroup of pregnant women has not been previously tested.

2.4. Data and Methods

2.4.1. Data

This study uses data from the Pregnancy Risk Assessment Monitoring System (PRAMS), an ongoing, cross-sectional survey of recent live births conducted monthly by the U.S. Centers for Disease Control and Prevention (CDC) in collaboration with state health departments. Participating states use birth certificate records to select a representative sample of individuals

who gave birth in the past 2 to 6 months (Shulman et al., 2018). PRAMS is a well-suited data source for this study since mothers are asked questions about prenatal intimate partner violence and their responses are linked to demographic information on their child's birth record which can be used to construct a measure of maternal EITC exposure.

I use data from mothers who gave birth in years 1994-2017 (survey years 1996-2018) in 45 states -- the entirety of states that participated in PRAMS and met the CDC's response rate threshold for data release over the study period. See Table 2-1 for each participating state's data availability by year. I restrict the sample to all mothers who were age 18 or older at the time of childbirth. My analytic sample includes 682,358 mothers without missing outcome or covariate data. Table 2-2 presents descriptive statistics for mothers in the sample.

2.4.2. Measures

Outcomes

My primary outcome is a binary variable for experiencing physical abuse during pregnancy by either a current or former partner or spouse. This measure is defined as whether mothers responded affirmatively to the following survey question: "During your most recent pregnancy, did your husband/partner (or ex-husband/ex-partner) push, hit, slap, kick, choke, or physically hurt you in any other way?" Reductions in physical abuse are critical for the health of mothers and their children because prenatal physical abuse is associated with blunt physical trauma, negative maternal coping behaviors, inadequate prenatal care, and poor birth outcomes (Alhusen et al., 2014). Other types of intimate partner violence (e.g., sexual violence, stalking, and psychological aggression) are not explicitly assessed in the PRAMS survey.

To explore reductions in economic hardship as a potential mechanism for my findings,[5] I use a binary variable based on whether mothers responded affirmatively to the survey question: "During the 12 months prior to birth, I had a lot of bills I couldn't pay." Bill hardship is a commonly used indicator of economic-wellbeing in studies that report on families' abilities to afford their basic needs.[6] In previous surveys and interviews, EITC recipients describe "paying bills" as a primary use of their EITC dollars (Goodman-Bacon & McGranahan 2008; Maag, Roll, & Oliphant, 2016; Sykes, Križ, Edin, & Halpern-Meekin, 2015; Romich & Weisner, 2000; Smeeding, Phillips, & O'Conner, 2000).

The Earned Income Tax Credit

A typical challenge of identifying effects of the EITC is the endogeneity of the EITC eligibility and payment size with respect to outcomes of interest. For instance, according to EITC policy rules, EITC payments are directly determined by household characteristics including earnings and employment that are also predictive of individual or family-level outcomes (intimate partner violence in this case). Therefore, using a mothers' actual EITC payment amount would introduce positive bias in the estimate of EITC effects because mothers living in households with lower incomes are also more likely to experience intimate partner violence.

[5] Because this variable was measured at the same time as intimate partner violence, I cannot conduct a formal mediation analysis to test whether (or to what extent) any change in economic hardship mediates the association between the EITC and intimate partner violence. Importantly, the relationship between violence and bill hardship can be bidirectional. For instance, intimate partner violence can cause a mother's bill hardship by undermining her ability to manage household bills.

[6] Bill hardship it is the best option that I have in the PRAMS dataset to measure a prevalent form of economic hardship. However, bill hardship is only one dimension of economic wellbeing that might influence risk intimate partner violence. For instance, levels of saving and assets and other material hardships (e.g., food insecurity, housing insecurity, transportation insecurity) may also influence risk of intimate partner violence.

Therefore, to reduce concerns about endogeneity (or "selection bias"), following previous studies (e.g., Bastian & Michelmore, 2018; Michelmore & Lopoo, 2021; Bastian & Jones, 2021), I use policy-induced variation over time and across states in the size of the inflation-adjusted maximum EITC that a mother could expect to receive given her state and number of children (proxied by number of previous births) during the 12 months before childbirth.[7] This variable captures quasi-random variation brought about by EITC policy changes but is independent of household characteristics such as earnings and employment that would bias estimates (Bastian & Michelmore, 2018; Michelmore & Lopoo, 2021; Bastian & Jones, 2021).

I assign the maximum EITC exposure variable to mothers in the PRAMS sample according to the 1-12 months before her child's birth, thereby testing whether the EITC influences risk of intimate partner violence through the cash available to mothers during this period.[8] Figure 2-1 illustrates my method of mapping from a child's birth month onto a mother's 12 month EITC exposure period. Because this period spans adjacent tax years, I take a weighted average of maximum EITC payments across years (weighting by number of months in a given year). Following previous research (e.g., Hoynes, Miller, & Simon, 2015), I consider February the first month of each tax year because more than half of families receive their EITC in February or shortly thereafter (LaLumia 2013). The timing of exposure to the EITC in my analysis also accounts for the one-year lag between tax policy and the disbursement of payments at tax time.

[7] I exclude non-refundable EITC benefits from the calculation of state EITC benefits because most low-income tax filers owe insufficient income tax to benefit from non-refundable tax credits (Pac et al., 2020).
[8] In a sensitivity analysis shown in Table 2-6, I test whether my results are robust to an earlier exposure period 13-24 months before birth. Findings are similar to the exposure period 1-12 months before birth, although estimates are smaller in magnitude.

Figure 2-2 shows trends in both the maximum federal EITC by number of children in the household (top panel) and the generosity of state EITCs expressed as a refundable percentage of the federal credit (bottom panel). This figure demonstrates how policy changes to the EITC at both the state and federal levels have created considerable spatial and temporal variation in potential EITC benefits available to mothers over the study period.

Covariates

My models control for a set of maternal characteristics including age (18–19, 20-24, 25-34, or 35 or older), years of education (0-12 years, 13-15 years, or 16+ years), and race/ethnicity (categorized as Black non-Hispanic, White non-Hispanic, Hispanic, or other non-Hispanic) that are correlated with both EITC exposure and intimate partner violence. Each of these maternal characteristics is measured at delivery. I opt not to control for marital status because this characteristic is also measured at the time of delivery and unlike my other individual-level controls, is inherently time-varying during pregnancy.[9] Also, because relationship dissolution is a potential pathway though which the EITC may affect risk of intimate partner violence (e.g., by providing cash necessary to exit an relationship or live independently), controlling for post-baseline relationship status could bias my results (Schneider, Harknett, & McLanahan, 2016).[10]

[9] For instance, the PRAMS survey asks mothers whether they separated from or divorced a partner or spouse during pregnancy and 9% of all mothers in my sample report doing so. Other research has also found high levels of relationship instability during pregnancy, especially among low-income women (Braveman et al., 2010).

[10] I also ran an analysis including marital status as a control variable in the model and estimates were largely unchanged from the model without a control for marital status. In Table 2-7, I also present analyses ran separately by marital status. I find a decrease in the likelihood of intimate partner violence during pregnancy among both married and single mothers. The estimates were larger among single mothers, likely because rates of both intimate partner violence and EITC eligibility are higher among single mothers than married mothers. Around three quarters of total federal EITC dollars are delivered to single tax filers; the other quarter of total federal EITC dollars are issued to married tax filers (Hoynes, 2019). However, it is also possible that responsiveness to the EITC varies

In addition to maternal characteristics, I also control for a set of state-level economic conditions and policies that might be correlated with both EITC exposure and intimate partner violence. Specifically, I control for gross domestic product, the minimum wage (the federal minimum wage if it exceeded the state's minimum wage), the maximum monthly TANF benefit available to a 4 person family, an indicator variable for whether the state expanded Medicaid under the ACA, and an indicator variable for whether the state offered Paid Family Leave.[11] All state economic and policy variables were matched to the PRAMS sample on the calendar year before childbirth. All variables measured in US dollars were adjusted for inflation to the year 2017.

2.4.3. **Statistical Analysis**

My empirical strategy uses EITC variation created by policy changes at both the state and federal levels to identify effects of the EITC on prenatal intimate partner violence. Specifically, I estimate the likelihood of intimate partner violence during pregnancy with the following difference-in-differences linear probability regression model:

$$(1)\quad Y_{it} = \beta_0 + \beta_1 MaxEITC_{itsb} + \beta_2 X_{it} + \alpha_{ts} + \gamma_t + \delta_s + \theta_b + \varepsilon_{it},$$

where Y_{it} is an binary variable for intimate partner violence for mother *i* in year *t.* $MaxEITC_{itsb}$ represents the maximum EITC payment that a mother could receive living in state *s*, in year *t,*

according to marital status. Because married mothers are required to file jointly for the EITC with their spouse (single mothers can file separately from a partner), it may be particularly difficult for married mothers experiencing economic abuse to access the EITC.

[11]Data on Medicaid expansion came from the Kaiser Family Foundation; data on Paid Family Leave came from the National Partnership for Women and Families; and data on all other state-year economic and policy variables came from the University of Kentucky's Center for Poverty Research.

with number of previous births b. The key coefficient of interest, β_1, corresponds with an estimate of the change in the probability of intimate partner violence due to $1,000 increase in EITC exposure. X_{it} represents my set of controls for maternal characteristics and annual state economic conditions and policies that may be correlated with both EITC exposure and intimate partner violence. Additionally, in order to control for invariant differences across time, state, and family-size that may be correlated with both EITC payment size and intimate partner violence, the model also includes fixed effects for the calendar year before childbirth (γ_t), state (δ_s) and number of previous births (θ_b). Lastly, the model includes state-specific linear time trends (α_{ts}) to control for unobserved factors that change linearly over time within states and affect rates of intimate partner violence. Standard errors are clustered at the state level to account for correlation of the error term (ε_{is}) within states. PRAMS analysis weights are also applied in all analyses to adjust for survey non-response, non-coverage, and oversampling of subpopulations (Shulman et al., 2018).

In order to assess whether my results are concentrated among demographic groups that are most likely to be affected by the EITC due to higher rates of participation, I conduct several subgroup analyses. First, I investigate effects of the EITC across various levels of maternal educational attainment (0-12 years of education, 13-15 years of education, and 16+ years of education). I expect that less educated women will be particularly affected by EITC benefits because they tend to have higher rates of EITC eligibility due to lower household earnings (Hoynes & Patel, 2018). Many prior EITC studies use educational attainment as a proxy for EITC eligibility and find effects of the EITC concentrated among those with lower levels of

education (e.g., Pilkauskas & Michelmore, 2019; Dow, Godøy, Lowenstein, & Reich, 2019).[12] Second, I investigate differences in the effect of EITC benefits according to whether mothers have had a previous birth. Because the vast majority of EITC benefits are issued to households with children (Crandall-Hollick, Falk, & Boyle, 2021), I expect any effects of the EITC on intimate partner violence to occur among mothers with at least one previous birth.

2.5. **Results**

2.5.1. Intimate Partner Violence

Table 2-3 presents the population-level prevalence of intimate partner violence during pregnancy, overall and by maternal race/ethnicity. In the overall sample, approximately 3.0% of mothers reported abuse by an intimate partner during pregnancy, I found variation in rates of reported intimate partner violence during pregnancy across racial/ethnic groups. The prevalence was highest among non-Hispanic Black women at 5.7%, followed by Hispanic women at 3.5%, and lowest among non-Hispanic White women at 2.8%.

Table 2-3 also summarizes my main results for the likelihood of intimate partner violence during pregnancy as a function of maternal EITC exposure during the year before childbirth. Each cell contains coefficient estimates and 95% confidence intervals from a different model. Results indicate a negative association between EITC exposure and intimate partner violence.

[12] Because earnings among U.S. households tend to drop around the time of childbirth (Stanczyk, 2020), highly educated households may become newly eligible for the EITC. It is possible that EITC eligibility gaps by educational attainment may be less pronounced than usual during the prenatal period.

Column 1 shows that a $1,000 increase in EITC exposure during the year before childbirth was associated with a 0.5 percentage point decrease in the likelihood of intimate partner violence during pregnancy (95% CI: -0.008, -0.002), a 16.7% reduction in this sample.[13]

Columns 2-4 of Table 2-3 show the results by subgroups based on maternal race/ethnicity. Although the coefficients were negatively signed for all groups, the coefficient for Hispanic mothers was not statistically significant, whereas the coefficients for both non-Hispanic Black mothers and non-Hispanic White mothers reached marginal statistical significance ($p < .10$). The results suggest that maternal exposure to EITC benefits reduces prenatal intimate partner violence for non-Hispanic Black mothers and non-Hispanic White mothers by 0.8 and 0.3 percentage points, respectively. Although the magnitude of the effect is largest for non-Hispanic Black mothers, the confidence intervals overlap across groups.

Table 2-4 presents my results stratified by maternal characteristics correlated with EITC eligibility: years of education and whether this was a mother's first birth. The results from these stratified analyses indicate that the effect found in column 1 of Table 2-3 is primarily due to those who were most likely to receive EITC benefits: mothers with 0-12 years of education (column 1) and mothers with at least one previous birth (column 4). As anticipated, I find no statistically significant relationship between the EITC and intimate partner violence among either of the two more highly educated groups. (However, reports of intimate partner violence during pregnancy were very rare among mothers with 16+ years of education.) Similarly, I find no effects of the EITC among mothers for whom this was their first birth, consistent with the fact that childless households receive a very small share of total EITC dollars issued each year.

[13] Coefficients for the full set of control variables (maternal characteristics and annual state economic conditions and policies) for Column 1 of Table 2-3 shown in Table 2-8.

2.5.2. Mechanisms: Financial Hardship

Prior research suggests that the EITC improves household financial stability (Jones & Michelmore, 2018), which in turn may affect relationship quality and abusive behavior (Niolon et al., 2017). In Table 2-5, I explore reductions in the risk of financial hardship (measured as bill-paying hardship) during the year before childbirth as a possible mechanism for how the EITC may lead to reductions in the likelihood of intimate partner violence during pregnancy. Column 1 of Table 2-5 shows that EITC benefits are negatively associated with financial hardship: a $1000 increase in EITC exposure during the year before childbirth decreases the likelihood that mothers report difficulty paying their bills by 2.1 percentage points (95% CI: -0.027, -0.014).[14] Given that around 23% of all the mothers in the sample reported experiencing trouble paying bills during the year before childbirth, this represents a 9% decrease.

Columns 2-4 of Table 2-5 show that the effects of the EITC on financial hardship are less pronounced for non-Hispanic Black mothers than for non-Hispanic White or Hispanic mothers, although the confidence intervals overlap across groups. Columns 5-9 of Table 2-5 also suggest the largest effects of the EITC on financial hardship occurs among mothers who are most likely to be eligible for the EITC: those with 0-12 years of education (column 5) and those with a previous birth (column 8). A statistically significant decrease in financial hardship, albeit smaller in magnitude than that among mothers with 0-12 years of education (column 7), was also observed among mothers with 16+ years of education.

[14] Coefficients for the full set of control variables (maternal characteristics and annual state economic conditions and policies) for Column 1 of Table 2-5 shown in Table 2-9.

2.6. **Discussion and Conclusions**

In this study, I examined whether the EITC, one of the largest anti-poverty programs in the U.S., reduces the risk of intimate partner violence for pregnant women: a group that is particularly vulnerable to the adverse effects of violence and experiences high rates of poverty and financial hardship (Taylor et al., 2021; Braveman et al., 2010). Using individual-level data on intimate partner violence during pregnancy and a quasi-experimental design, I found an association between increased EITC benefits and reductions in intimate partner violence: a $1,000 increase in maternal exposure to the EITC during the year before childbirth was associated with a 16.7% decrease in the likelihood of intimate partner violence during pregnancy.

Consistent with the hypothesis that economic support policies mediate reductions in household violence, the estimated effects of increased EITC generosity were largest for mothers in the sample most likely to be eligible for the program -- mothers with 12 or fewer years of education (a proxy for low-earnings) and mothers with a previous birth. I also found that EITC benefits improved financial wellbeing during the year before birth, suggesting a mechanism by which the EITC could reduce risk of violence victimization for many mothers.

When I examined associations between the EITC and intimate partner violence within racial/ethnic subgroups, I found the largest point estimates among non-Hispanic Black mothers. Research suggests that Black women and Hispanic women are more likely than White women to meet earnings-based eligibility requirements for the EITC (Michelmore & Lopoo, 2021). Historic and current forms of discrimination cause overrepresentation in jobs with low wages,

which the EITC supplements (Hardy, Hokayem, Ziliak, 2022). That the effect was largest for Black mothers could be due in part to higher rates of EITC receipt among Black mothers.

This study builds on larger interdisciplinary literature on the impacts of the EITC on maternal-infant health (e.g., Hoynes, Miller, & Simon, 2015). Maternal exposure to intimate partner violence during pregnancy is a major risk factor for adverse birth outcomes (Alhusen et al., 2014; Currie, Mueller-Smith, & Rossin-Slater, 2022), which compromise children's health and wellbeing across the life course (Aizer & Currie, 2014). Thus, if the EITC reduces prenatal exposure to violence, it could not only reduce health harms for mothers but also reduce adverse birth outcomes and improve children's long-term outcomes. Findings from recent studies suggest that the EITC improves maternal health and birth outcomes (Strully et al., 2010; Evans & Garthwaite, 2014; Hamad & Rehkopf, 2015; Boyd-Swan et al., 2016; Markowitz et al. 2017; Wicks-Lim & Arno, 2017; Hill & Gurley-Calvez, 2019; Gangopadhyaya et al., 2020; Morgan et al., 2022), especially among infants born to Black mothers (Hoynes, Miller, & Simon, 2015; Komro et al., 2019; Batra, Karasek, & Hamad., 2022; Pearlman & Robinson, 2022). Reductions in violence against pregnant women may be one channel through which the EITC leads to maternal-infant health improvements.

Among public benefit programs, the EITC provides some of the highest levels of cash or near cash support to low-income families around childbirth (Stanczyk, 2020). Thus, it is encouraging to find a significant association between higher EITC benefits and lower risk of violence victimization among pregnant women. However, it is also important to note that certain features of the credit might limit its effectiveness at reducing prenatal victimization. First, the EITC is a lump-sum benefit received only once per year; thus, it may poorly align with the timing of economic needs that arise throughout the prenatal period. Second, the EITC is

conditional on earnings, which means that the credit may not reach mothers who are outside the labor force or face violence-based barriers to work. Declines in women's employment and earnings around the time of childbirth can affect EITC eligibility and benefit levels. Research suggests that on average, EITC recipiency rates tend to increase around childbirth (Stanczyk, 2020); however, some households will lose access to EITC benefits. Critically, the EITC does not guarantee any minimum level of support for low-income mothers during pregnancy, making it poorly suited as singular a strategy for targeting prenatal economic conditions. Given my findings that increases in EITC benefits are associated with lower risk of intimate partner, policymakers should consider whether other supplemental economic assistance targeted specifically at the prenatal period might further reduce adverse prenatal exposures such as intimate partner violence.

2.7. References

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2.8. Table and Figures

Table 2-1 PRAMS data availability by state and interview year

State	Years
AK	1996-2018
AL	1996-2004, 2014-2018
AR	1997-2017
CO	1998-2018
CT	2014-2018
DE	2007-2018
FL	1996-2006
GA	1996-1998, 2004-2014, 2017-2018
HI	2000-2017
IA	2013-2018
IL	1997-2018
KS	2017-2018
KY	2017-2018
LA	1998-2005, 2015-2018
MA	2007-2018
MD	2001-2018
ME	1996-2018
MI	1996-1997, 2001-2018
MN	2002-2014
MO	2007-2018
MS	2003-2010
MT	2017-2018
NC	1997-2009, 2017-2018
ND	2017-2018
NE	2000-2018
NH	2013-2018
NJ	2002-2018
NM	1997-2006, 2011-2018
NY	1996-2018
OH	1999-2016
OK	1996-2016
OR	2003-2016
PA	2007-2018
RI	2002-2018
SC	1996-2008
SD	2017-2018
TN	2008-2010, 2012-2016
TX	2009-2011, 2015-2017
UT	1999-2018
VA	2015-2018
WA	1996-2018
WI	2007-2018
WV	1996-2018
WY	2007-2018

Table 2-2 Descriptive Statistics, PRAMS sample

Maternal Demographic Characteristics	Mean (SD)
Age (%)	
18-19	6.3
20-24	23.8
25-34	55.3
35 or older	14.7
Race and Ethnicity (%)	
Non-Hispanic, Black	14.2
Non-Hispanic, White	63.8
Hispanic	15.3
Non-Hispanic, other	6.7
Education (%)	
0-12 years	42.9
13-15 years	25.9
16+ years	31.2
Number of previous births (%)	
0	39.1
1	33.5
2	16.7
3 or more	10.6
Maximum EITC Exposure	$3,055 (2,882)
State Characteristics	
Gross domestic product ($B)	$516 (287)
Maximum TANF benefit ($)	$589 (231)
Minimum wage ($)	$7.65 (0.83)
Medicaid expansion (%)	9.3
Paid family leave (%)	1.6
Number of Observations	682,358

Source: Pregnancy Risk Assessment Monitoring System (PRAMS) 1996-2018
Notes: The sample is restricted to mothers age 18 or older at the time of childbirth. All statistics were calculated using PRAMS analysis weights. All dollar amounts are adjusted to 2017 using the Consumer Price Index.

Table 2-3 Linear regression results: estimated effects of the Earned Income Tax Credit (in $1,000 of 2017 dollars) on the likelihood of intimate partner violence during pregnancy, overall and by race/ethnicity

		Race/Ethnicity		
	Full Sample	Black, non-Hispanic	White, non-Hispanic	Hispanic
	(1)	(2)	(3)	(4)
Maximum EITC Benefits	-0.005**	-0.008†	-0.003†	-0.005
	(-0.008, -0.002)	(-0.017, 0.001)	(-0.006, 0.000)	(-0.013, 0.002)
Outcome Mean	0.030	0.057	0.028	0.035
Number of Observations	682,358	118,281	377,205	94,645

Source: Pregnancy Risk Assessment Monitoring System (PRAMS) 1996-2018
Notes: Estimates for the effect of an additional $1,000 in EITC exposure 1-12 months before childbirth. 95% confidence intervals are shown in parentheses. All models control for maternal characteristics and state-level economic conditions and policies; fixed effects for state, calendar year before childbirth, and number of previous births; and state-specific time trends. All models are estimated with analysis weights. The sample is restricted to mothers age 18 or older at the time of childbirth.
†$p < .10$; **$p < .01$

Table 2-4 Linear regression results: estimated effects of the Earned Income Tax Credit (in $1,000 of 2017 dollars) on the likelihood of intimate partner violence during pregnancy, by years of education and previous births

	Years of Education			Previous Births	
	0-12 Years	13-15 Years	16+ Years	One or More Previous Births	No Previous Births
	(1)	(2)	(3)	(4)	(5)
Maximum EITC Benefits	-0.007***	-0.000	0.000	-0.006*	0.007
	(-0.011, -0.002)	(-0.004, 0.003)	(-0.002, 0.003)	(-0.011, -0.001)	(-0.056, 0.070)
Outcome Mean	0.046	0.030	0.008	0.032	0.027
Number of Observations	309,428	179,248	193,682	406,815	275,543

Source: Pregnancy Risk Assessment Monitoring System (PRAMS) 1996-2018
Notes: Estimates for the effect of an additional $1,000 in EITC exposure 1-12 months before childbirth. 95% confidence intervals are shown in parentheses. All models control for maternal characteristics and state-level economic conditions and policies; fixed effects for state, calendar year before childbirth, and number of previous births; and state-specific time trends. All models are estimated with analysis weights. The sample is restricted to mothers age 18 or older at the time of childbirth.
*$p < .05$; ***$p < .001$

Table 2-5 Linear regression results: estimated effects of the Earned Income Tax Credit (in $1,000 of 2017 dollars) on the likelihood of financial hardship during the year before childbirth

		Race/Ethnicity			Years of Education			Previous Birth	
	Full Sample	Black, non-Hispanic	White, non-Hispanic	Hispanic	0-12 Years	13-15 Years	16+ Years	One or More Previous Births	No Previous Birth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Maximum EITC Benefits	-0.021***	-0.011†	-0.022***	-0.029**	-0.025***	-0.009	-0.011**	-0.021**	-0.085
	(-0.027, -0.014)	(-0.024, -0.001)	(-0.029, -0.015)	(-0.050, -0.008)	(-0.032, -0.017)	(-0.021, 0.002)	(-0.018, -0.003)	(-0.034, -0.008)	(-0.243, -0.074)
Outcome Mean	0.230	0.313	0.211	0.255	0.297	0.279	0.096	0.189	0.256
Number of Observations	682,358	118,281	377,205	94,645	309,428	179,248	193,682	406,815	275,543

Source: Pregnancy Risk Assessment Monitoring System (PRAMS) 1996-2018

Notes: Estimates for the effect of an additional $1,000 in EITC exposure 1-12 months before childbirth. 95% confidence intervals are shown in parentheses. All models control for maternal characteristics and state-level economic conditions and policies; fixed effects for state, calendar year before childbirth, and number of previous births; and state-specific time trends. All models are estimated with analysis weights. The sample is restricted to mothers age 18 or older at the time of childbirth.

†$p < .10$; **$p < .01$; ***$p < .001$

Table 2-6 Linear regression results: estimated effects of the Earned Income Tax Credit (in $1,000 of 2017 dollars) on the likelihood of intimate partner violence during pregnancy; EITC exposure 13-24 months before childbirth

		Race/Ethnicity			Years of Education			Any Previous Births	
	Full Sample	Black, non-Hispanic	White, non-Hispanic	Hispanic	0-12 Years	13-15 Years	16+ Years	One or More Previous Births	No Previous Births
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Maximum EITC Benefits	-0.005***	-0.007†	-0.003*	-0.005	-0.007***	0.000	0.000	-0.006**	0.016
	(-0.007, -0.002)	(-0.014, 0.000)	(-0.006, 0.000)	(-0.014, 0.004)	(-0.011, -0.003)	(-0.003, 0.004)	(-0.002, 0.002)	(-0.010, -0.002)	(-0.025, 0.057)
Outcome Mean	0.030	0.057	0.028	0.035	0.046	0.030	0.008	0.032	0.027
Number of Observations	682,358	118,281	377,205	94,645	309,428	179,248	193,682	406,815	275,543

Source: Pregnancy Risk Assessment Monitoring System (PRAMS) 1996-2018

Notes: Estimates for the effect of an additional $1,000 in EITC exposure 1-12 months before childbirth. 95% confidence intervals are shown in parentheses. All models control for maternal characteristics and state-level economic conditions and policies; fixed effects for state, calendar year before childbirth, and family-size; and state-specific time trends. All models are estimated with analysis weights. The sample is restricted to mothers age 18 or older at the time of childbirth.

†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

Table 2-7 Linear regression results: estimated effects of the Earned Income Tax Credit (in $1,000 of 2017 dollars) on the likelihood of intimate partner violence hardship during pregnancy, overall and marital status

		Marital Status	
	Full Sample	Married	Unmarried
	(1)	(2)	(3)
Maximum EITC Benefits	-0.005**	-0.003*	-0.008*
	(-0.008, -0.002)	(-0.006, -0.003)	(-0.014, -0.002)
Outcome Mean	0.030	0.014	0.061
Number of Observations	682,358	432,434	249,924

Source: Pregnancy Risk Assessment Monitoring System (PRAMS) 1996-2018
Notes: Estimates for the effect of an additional $1,000 in EITC exposure 1-12 months before childbirth. All models control for maternal characteristics and state-level economic conditions and policies; fixed effects for state, calendar year before childbirth, and family-size; and state-specific time trends. All models are estimated with analysis weights. The sample is restricted to mothers age 18 or older at the time of childbirth.
*$p < .05$; **$p < .01$

Table 2-8 Linear regression results: estimated effects of the Earned Income Tax Credit (in $1,000 of 2017 dollars) on the likelihood of intimate partner violence hardship during pregnancy; coefficients for the full set of control variables; N = 682,358

	B	(95% CI)	
Number of Children:			
0 (reference)			
1	0.020	(0.011, 0.029)	***
2	0.037	(0.021, 0.053)	***
3+	0.051	(0.033, 0.069)	***
Age			
18-19			
20-24	-0.016	(-0.019, -0.012)	***
25-29	-0.034	(-0.037, -0.030)	***
30-34	-0.042	(-0.047, -0.037)	***
35-39	-0.045	(-0.048, -0.041)	***
40+	-0.044	(-0.049, -0.038)	***
Race/ethnicity			
Non-Hispanic Black (reference))			
Hispanic	-0.020	(-0.023, -0.016)	***
Non-Hispanic other	-0.014	(-0.018, -0.011)	***
Non-Hispanic white	-0.022	(-0.025, -0.019)	***
Years of education			
0-12 years			
13-15 years	-0.007	(-0.009, -0.005)	***
16+ years	-0.018	(-0.021, -0.015)	***
Maximum EITC ($1,000s in 2017 dollars)	-0.005	(-0.008, -0.002)	**
Minimum wage (in 2017 dollars)	-0.003	(-0.004, -0.001)	**
Gross state product (in 2017 dollars)	0.000	(0.000, 0.000)	
State expanded Medicaid under the ACA	0.000	(-0.004, 0.004)	
State offered Paid Family Leave	0.001	(-0.004, 0.005)	
Maximum monthly TANF benefit (in 2017 dollars)	0.000	(0.000, 0.000)	

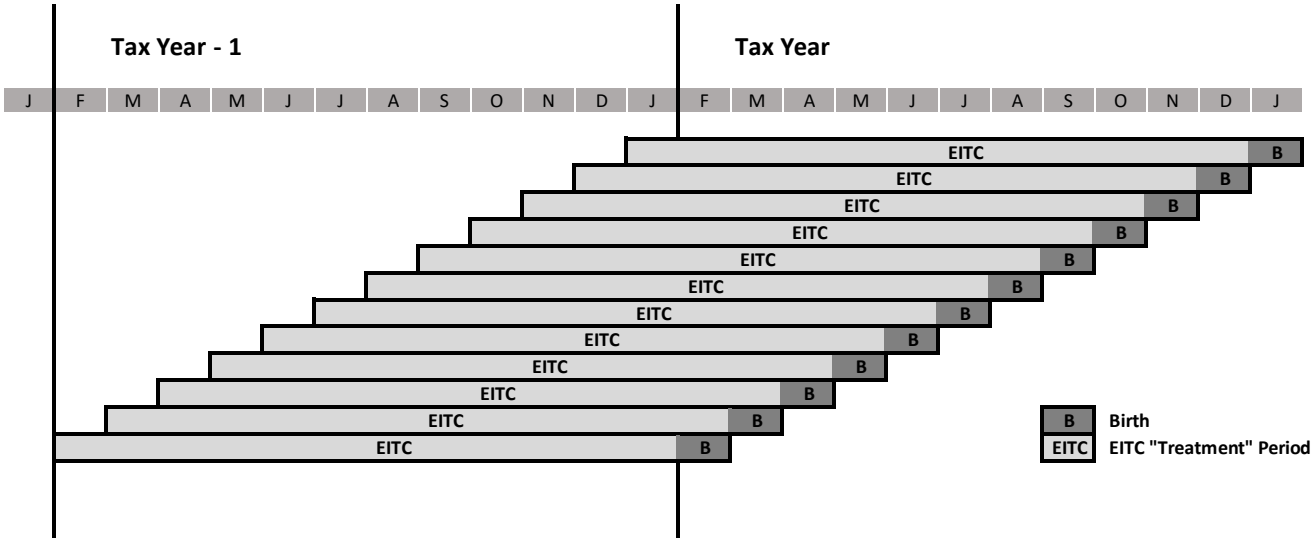
Source: Pregnancy Risk Assessment Monitoring System (PRAMS) 1996-2018
Notes: Estimates for the effect of an additional $1,000 in EITC exposure 1-12 months before childbirth. 95% confidence intervals are shown in parentheses. All models control for fixed effects for state and calendar year before childbirth and state-specific time trends. All models are estimated with analysis weights. The sample is restricted to mothers age 18 or older at the time of childbirth.
**$p < .01$; ***$p < .001$

Table 2-9 Linear regression results: estimated effects of the Earned Income Tax Credit (in $1,000 of 2017 dollars) on the likelihood of financial hardship during the year before childbirth; coefficients for the full set of control variables; N = 682,358

	B	(95% CI)	
Number of Children:			
0 (reference)			
1	0.104	(0.082, 0.126)	***
2	0.193	(0.158, 0.228)	***
3+	0.237	(0.198, 0.275)	***
Age			
18-19			
20-24	0.093	(0.083, 0.104)	***
25-29	0.045	(0.032, 0.058)	***
30-34	0.002	(-0.012, 0.016)	
35-39	-0.004	(-0.019, 0.010)	
40+	0.008	(-0.013, 0.028)	
Race/ethnicity			
Non-Hispanic Black (reference))			
Hispanic	-0.055	(-0.078, -0.032)	***
Non-Hispanic other	-0.063	(-0.080, -0.045)	***
Non-Hispanic white	-0.049	(-0.061, -0.037)	***
Years of education			
0-12 years			
13-15 years	0.000	(-0.009, 0.009)	
16+ years	-0.147	(-0.157, -0.137)	***
Maximum EITC ($1,000s in 2017 dollars)	-0.021	(-0.027, -0.014)	***
Minimum wage (in 2017 dollars)	-0.004	(-0.010, 0.002)	
Gross state product (in 2017 dollars)	0.000	(0.000, 0.000)	
State expanded Medicaid under the ACA	-0.023	(-0.044, -0.001)	*
State offered Paid Family Leave	-0.001	(-0.012, 0.011)	
Maximum monthly TANF benefit (in 2017 dollars)	0.000	(0.000, 0.000)	**

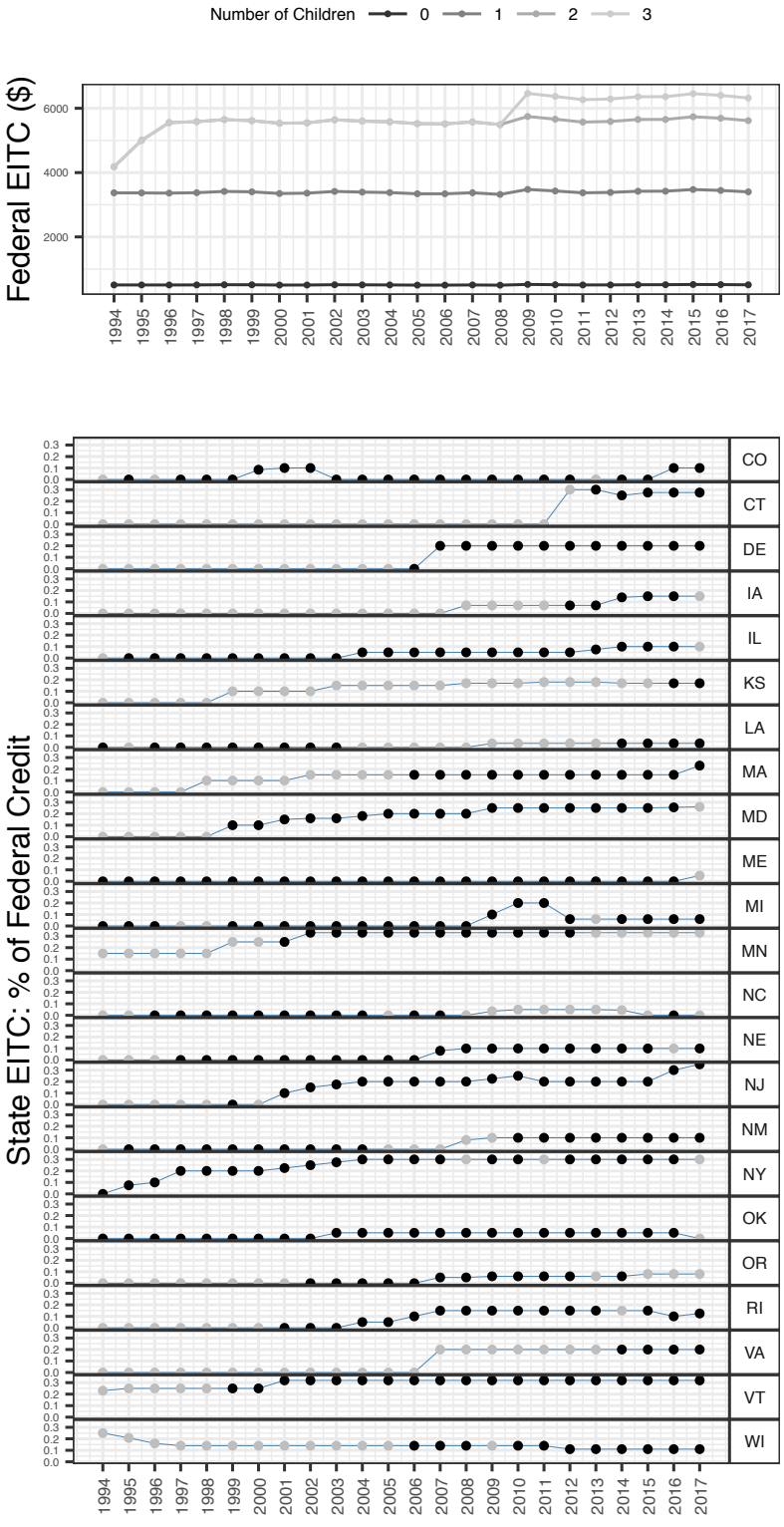
Source: Pregnancy Risk Assessment Monitoring System (PRAMS) 1996-2018
Notes: Estimates for the effect of an additional $1,000 in EITC exposure 1-12 months before childbirth. 95% confidence intervals are shown in parentheses. All models control for fixed effects for state and calendar year before childbirth and state-specific time trends. All models are estimated with analysis weights. The sample is restricted to mothers age 18 or older at the time of childbirth.
**$p < .01$; ***$p < .001$

Figure 2-1 Maternal EITC Exposure Period by Month of Birth



The figure above illustrates my method of mapping from a child's birth onto a mother's 12-month EITC exposure period. Because this period spans adjacent tax years, I took a weighted average of maximum EITC payments across years (weighting by number of months in a given year). Since families start receiving EITC payments in February, I considered February the first month of each tax year.

Figure 2-2 Maximum Federal and State EITC, 1994-2017



Source: The National Bureau of Economic Research's TAXSIM: https://users.nber.org/~taxsim/state-eitc.html; Tax Policy Center: https://www.taxpolicycenter.org/statistics/eitc-parameters

Notes: The figure depicts variation in the maximum potential federal and state EITC benefits from 1994 to 2017. The timing accounts for the one-year lag between tax policy and receipt of EITC payments. The top panel shows variation in the maximum federal EITC (in 2017 dollars) for different family sizes (0, 1, 2, and 3+ children). The bottom panel shows variation in state EITC generosity expressed as a refundable percent of the federal EITC for the 23 states with observations in my analysis that offered a refundable EITC at some point over the study period. Black dots represent available state-years; grey dots represent unavailable state-years

3. Patterns and Trends in Liquid Asset Pooling Among Couples with Children

3.1. **Abstract**

Studies of economic wellbeing tend to focus on household measures. However, household-level measures fail to capture access to and ownership over economic resources at the individual level, which may vary significantly with implications for family-member wellbeing. In this study, I analyze trends in individual level measures of liquid asset ownership within couples with children using data from the Survey of Consumer Finances (1998-2019). I find that proportion of couples with children who hold liquid assets in separately-owned accounts has increased over time. Both men and women have increasingly used separately-owned accounts, but the median account balance was greater for men than women. I also find that proxies for intra-partner bargaining power are associated with the likelihood that women, but not men, separately own liquid assets.

3.2. **Introduction**

Independent of income, ownership of assets is a positive predictor of economic and social well-being (Lerman & McKernan, 2008; Killewald, Pfeffer, & Schachner, 2017). Studies that measure assets tend to do so at the household or couple level and treat individuals within households as benefitting equally from resources. However, only measuring economic resources at the household level fails to capture the full range of access to and ownership over financial resources of individual household members (Klawitter & Fletschner, 2011; Bennett 2013).

Major demographic changes in family structure over recent decades may have affected the extent to which couples hold household assets in jointly- or separately- owned accounts. Increases in cohabitation, relationship turnover, non-martial childbearing, and stepfamilies are all key changes that have affected U.S. couples and families over recent decades (Berger & Carlson, 2020). Some evidence suggests that social norms around resource pooling within couples have shifted toward greater individualization over time (Lauer & Yodanis, 2014). Given these major demographic and cultural changes, rates of asset pooling within couples may have changed over time as well.

Despite considerable research on the distribution of assets *across* households, we know relatively little about how the ownership of assets is distributed *within* households. One reason that the issue of ownership of assets within households is relatively neglected in the economic-wellbeing literature is that surveys tend to only ask about the ownership of assets at the household level, rather than asking respondents to distinguish which individuals within households own or control assets (Killewald, Pfeffer, & Schachner, 2017). The Survey of Consumer Finances (SCF), however, asks questions about liquid assets held in accounts owned

at both the individual and couple levels, allowing separate asset measures for each partner within couples. (Unfortunately, the SCF does not collect information that would enable calculation of personal ownership of most other types of assets).

Liquid assets (holdings in checking and savings accounts), which can be easily accessed or converted to cash, influence one's ability to cover every-day expenses. Research suggests that ownership of bank accounts, in which liquid assets are typically held, reduces the likelihood of high-cost alternative borrowing (e.g., payday loans, check cashing) (Hogarth, Anguelov, & Lee, 2005). Liquid assets can also buffer against income declines and negative economic shocks (McKernan, Ratcliffe, & Vinopal, 2009) and may be particularly valuable for facilitating exchanges of support within families and parental spending on children.

Drawing on nationally representative data from the SCF, I extend the limited knowledge of patterns of liquid assets pooling and sole-ownership of liquid assets within couples in the United States. I focus on patterns of liquid assets pooling among couples with children – a group of particular policy relevance since children's welfare and long-term outcomes are strongly associated with parental economic resources (Kornrich & Furstenberg 2013; Lerman & McKernan, 2008; Elliott, Destin, & Friedline, 2011; Huang, Guo, Kim, & Sherraden, 2010). Additionally, the gendered consequences of parenthood (e.g declines in women's work force participation and earnings) may have implications for gender gaps in liquid assets ownership within couples with children (Pahl, 2005; Kenney 2006).

In this study, I ask two main questions about patterns of liquid asset pooling within couples with children. First, what are trends in liquid asset pooling among couples with children over time, and are there differences by gender in separate ownership of assets within couples? Second, how do individual characteristics of male and female partners affect the likelihood that

they and their partner have solely-owned liquid assets? To answer the first question, I use data from the 1998, 2001, 2004, 2007, 2010, 2013, 2016, and 2019 waves of the SCF to reveal trends in asset pooling at the couple and individual levels. I find that the proportion of couples with children who hold all of their liquid assets in jointly-owned accounts has consistently declined over time, with significant concomitant increases in the proportion of couples who hold all of their assets in separately-owned accounts. Disaggregating these trends by gender, my results show that both men and women were more likely to separately-hold liquid assets over time, increasing from 21%-26%, in 1998 to 40%-40% in 2019. Although sole-ownership of these assets increased for both men and women, I observe that the median account balance in sole-owned accounts was greater for men than women.

To answer the second question, I use data from the two most recent waves of the SCF, 2016 and 2019. To gain insights on patterns of account ownership by gender, I draw on theories emphasizing the importance of bargaining power as a determinant of each partner's separate asset ownership. I find that for women, but not for men, within-couple differentials in education, health, and employment are associated with separate ownership of liquid assets. I interpret these findings as reflecting that the distribution of bargaining power within couples matters for gendered allocation of liquid assets at the household level into separately owned accounts. The motivation for this study is to increase understanding of the patterns and correlates of asset ownership *within* households -- a typically overlooked dimension of economic well-being. I focus specifically on couples with children since their patterns of asset ownership may have implications for children's outcomes (Pahl, 2005; Kenney 2006).

3.3. **Background**

Measurement of economic wellbeing tends to occur at the household level, thereby obscuring whether household members have unequal access to economic resources. Given that the distribution of economic resources may be unequal within couples, researchers have called for better assessments of individual-level ownership of assets among couples (Killewald, Pfeffer, & Schachner, 2017). Understanding patterns of asset ownership within couples is critical because ownership grants direct access to and control over resources (Lee, 2022) affects one's ability to further build assets and credit (Barr & Sherraden, 2005); is associated with one's financial security in retirement, especially in the event of separation or the death of a partner (Kapelle & Baxter, 2021; Joseph & Rowlingson, 2012; Lewis et al., 2009); and influences the extent to which parents can pass along resources to their children (Pfeffer & Schoeni, 2016).

The distribution of assets within couples may also have implications for family-member well-being beyond financial security. For instance, gender-based disparities in asset ownership may influence the risk that women experience intimate partner violence (Oduro, Deere, & Catanzarite, 2015). Additionally, research has shown that women's greater control over household income is associated with higher spending on child-related goods (Lundberg & Pollak 1996; Phipps & Burton 1998) and lower risk of food insecurity for children (Kenney, 2008). If these findings extend to asset ownership, gendered patterns in asset ownership among parents may have import consequences for children's wellbeing.

To date, much of what is known about access to and control over household economic resources is based on studies that examine whether (or the extent to which) couples pool their resources (i.e., keep separate versus joint control over household income and assets). This

research has primarily focused on the role of marriage and the presence of children in resource pooling. Studies consistently find that married couples, couples in longer-term relationships, and couples with children are more likely to pool their financial resources than cohabitating couples and couples without children (Treas 1993; Kenney, 2006; Hamplova & Le Bourdais 2009; Eickmeyer, Manning, & Brown 2019; Klawitter, 2008). Several studies have investigated whether the biological relationship between adults and children matters for whether couples' pool their economic resources (Eickmeyer, Manning, & Brown 2019; Kenney 2004; Addo & Sassler, 2010), generally showing that couples with only biological children are more likely than stepfamilies and blended families to pool economic resources.

Another important trend suggested by recent studies is the increasing share of couples with "individualized" financial arrangements (financial arrangements that are either partially or completely separate) in recent decades (Lauer & Yodanis 2011; Yodanis &Lauer 2014). Given that separate financial arrangements are on the rise, scholars have debated the meaning of this change, particularly for the wellbeing of individual family-members (Bennett 2013; Vogler, Brockmann, & Wiggins 2006; Pahl 2005). While separate financial arrangements offer a degree of financial independence in asset building, some have also argued that separate financial arrangements within couples can reproduce traditional gender inequalities in access to household economic resources since on average, women earn less than men and are more likely to reduce time spent in paid work to perform childcare (Pahl 2005; Bennett 2013). Despite this debate, the issue of gendered-patterns in sole-asset ownership within couples has received very little empirical attention.

3.3.1. The Distribution of Assets within Couples

The bargaining model provides a useful framework for theorizing about the intra-household distribution of economic resources. As opposed to the "unitary model" (Chiappori & Molina, 2020), which is based on "a common preference" model of the family that assumes family members pool their incomes and act as though they are maximizing a single household-level utility function (Becker 1981), the bargaining model incorporates the relative power of individuals within families in decision processes (Lundberg & Pollak 1996). Each partner's personal resources (particularly social resources and those linked to financial security) as well as their alternatives outside of the relationship, determine their power over intra-household decision-making. According to this perspective, partners with greater earnings and human capital are more likely to have power over household financial decisions.

Scholars have used the bargaining model to explore intra-partnership gaps in wealth among married couples in Germany (Grabka, Marcus, & Sierminska, 2015) and Korea (Lee & Pocock, 2007). This prior research shows that women tend to separately own less wealth than their husbands and that women's greater bargaining power reduces intra-partnership gaps in personal wealth. This study also builds on a previous study that has addressed the relationship between gender and bank account ownership within couples in the U.S. Klawitter and Fletschner (2010) examined data from the 2001 and 2004 waves of the SCF to show the association between inferred female bargaining power (measured as the proportion of the couple's earnings earned by the female partner in different-sex relationships) and patterns of ownership of bank accounts within couples. The authors found evidence that is consistent with the theory of bargaining – increases in the proportion of the couple's earnings earned by the female partner was associated with an increase in the probability that women (and their families) hold bank

accounts. The present study considers a wider range of measures related to bargaining power -- specifically those that capture each partner's employment, education, and health status – as well as a broader population, examining couples with children across a range of income levels. To understand contemporary correlates of liquid asset ownership within a more recent cohort of couples, I focus on the two most recent waves of the SCF – from 2016 and 2019.

3.4. Study Data and Methods

3.4.1. Data

The data used in this study come from the Survey of Consumer Finances (SCF). The SCF is a nationally representative cross-sectional interview survey of U.S. households which is conducted by the Federal Reserve Board every three years. The SCF collects extensive information on household finances and is commonly considered the best data source for analyzing U.S. household wealth and assets (Killewald, Pfeffer, & Schachner, 2017). In addition to information about household finances, the SCF also collects considerable information on demographic and labor market characteristics of individuals within couples.

I use data from eight waves of the SCF (1998, 2001, 2004, 2007, 2010, 2013, 2016, and 2019) to detect longitudinal trends in liquid asset ownership among couples with children. For additional analyses that explore the correlates of sole-asset ownership within couples, I use data from only the two most recent waves of the SCF (2016 and 2019). I restrict both samples to married and cohabiting couples who are of working age (23-60 years old) with at least one child

under the age of 19 residing in the household. For the correlates analysis, I further restrict the sample to different-sex couples to examine gendered-dimensions of financial arrangements. A total of 10,429 couples met my sample criteria in the 1998 - 2019 waves of the SCF and a total of 2,611 couples met my sample criteria in the 2016 - 2019 waves. Table 3-1 presents summary statistics for the characteristics of these couples.

3.4.2. Measures

Liquid Asset Ownership

The SCF asks respondents to report all household liquid assets and the type of accounts in which liquid assets are held (checking accounts, savings accounts, certificates of deposit, or money market funds). To assess sole and joint ownership of couples' liquid assets, I leverage a question that the SCF asks about whether holdings in each transaction account are jointly-owned (by both partners) or separately-owned (by one partner): "Is this account held jointly with your (husband/wife/partner) or is it in your name, in your (husband/wife's/partner's) name or in someone else's"? Using responses, I identify whether each couple has any jointly owned and/or separately-owned liquid assets. For separately owned assets, I further distinguish female-owned from male-owned assets. I use an indicator variable to measure the presence of liquid assets. For some analyses, I consider the dollar value of liquid assets and use the Consumer Price Index to adjust all assets to their value in 2019 dollars.

Table 3-2 presents survey-weighted statistics on liquid asset ownership at both the individual and couple levels. In 2016-2019, most couples (95%) reported having liquid assets. In terms of separate-ownership of liquid assets, the proportion of men with separately owned liquid

assets was slightly greater than the proportion of women with separately owned liquid assets (39% vs. 36%). Table 3-2 also shows which demographic groups were the most and least likely to own liquid assets. At the couple level, married couples were more likely than cohabitating couples to have liquid assets (97% vs. 86%). At the individual-level, both cohabiting men and women were more likely to have sole-owned liquid assets than their married counterparts. Cohabitating women were significantly less likely to have separately owned liquid assets than cohabitating men (60% vs. 86%). Table 3-2 also shows that the distribution of liquid asset ownership differs strongly with household income; the disparity in liquid asset ownership between couples with a household income lower than $50,000 and couples with a household income over $100,000 was 18 percentage points (82% vs. 100%). Similar disparities in liquid asset ownership existed across education groups.

Measures of Bargaining Power

Bargaining models predict that power differences within couples influence the allocation and management of economic resources. To categorize potential power differentials within couples I use variables on each partner's education (high school or less [the reference category], some college, or college or higher [including an associate's degree]), employment (not working for pay [the reference category], part-time, or full-time), and self-rated health status (poor/fair [the reference category], good, very good, excellent). In some analyses, I collapsed the categories of each variable into a dichotomous variable. For instance, educational attainment was dichotomized to split individuals with and without a college degree. Similarly, health status was

dichotomized into “poor” or “good” health, and work status simplified to full-time employed or not.

Control variables

In addition to these key measures of bargaining power, my models control for other features of a couple’s relationship and socioeconomic status that may be associated with asset ownership. I include controls for marital status (married or cohabitating), relationship duration in years, number of resident children under 18, household income ($0-$50,000, $50,001-$100,000, >$100,001), whether either partner has children under 18 living in another household,[15] whether either partner receives or provides support through intra-household financial transfers,[16] the race/ethnicity of the respondent (non-Hispanic Black, non-Hispanic White, Hispanic, other race or ethnicity [the SCF only asks about the respondent’s race or ethnicity]), and each partner’s age in years.

3.5. Analysis

First, I use data from 1998 to 2019 to evaluate longitudinal trends in the weighted proportion of couples who have (i) no liquid assets, (ii) all liquid assets in jointly-owned

[15] Ideally, I would have more information about the biological ties between children in the household and each partner, but the SCF does not ask any direct questions that would allow me to distinguish between biological and stepchildren.
[16] This variable captures any receipt or payment of alimony, separation payments, or child support, as well as any financial support provided to relatives or friends who live in another household.

accounts, (iii) liquid assets in both jointly-owned and separately-owned accounts, and (iv) all liquid assets in separately-owned accounts. Next, because changes in liquid asset ownership may not be distributed equally by gender, I examine longitudinal trends in the weighted proportion of men and women within couples with sole-ownership of any household liquid assets. I also examine trends by gender in the median balance held in separately-owned accounts among couples with separately-held liquid assets. Because the distribution of liquid assets is skewed, I present the median rather than mean values of assets over time.

After examining overall trends in ownership of liquid financial assets, I then examine factors that might influence which individuals are likely to have separately-owned liquid assets in the contemporary period (using the two most recent waves of the SCF: 2016 and 2019). Specifically, I investigate whether individual characteristics of male and female partners affect the likelihood that they and their partner have solely-owned liquid assets. By incorporating characteristics of both partners in my models, I test the premise that bargaining power within relationships may influence the distribution of intrahousehold liquid assets. (For this analysis, I excluded households without liquid assets: 5% of the overall sample). Furthermore, this analysis examines individual potential determinants of bargaining power within couples– such as employment, education, and health. I use linear probability models to estimate the associations between different measures of bargaining power and sole ownership of liquid assets by gender among couples with children. These analyses represent descriptive rather than the causal effects of bargaining position.

3.6. Results

3.6.1. Trend in Liquid Asset Ownership by Gender

Figure 3-1 displays trends in liquid asset ownership among married and cohabitating couples with children between 1998 and 2019. Overall, the proportion of couples without any liquid assets decreased by 3 percentage points (from 7.4% in 1998 to 4.3% in 2019). The proportion of couples who held all of their liquid assets in jointly-owned accounts consistently declined over time (Figure 3-1, 62.4% in 1998 to 44.1% in 2019). Although the share of couples who partially pooled their liquid assets increased over this period (from 25.0% in 1998 to 31.1% in 2019), the largest portion of the decline in joint assets was matched by growth in the share of couples who held all liquid assets separately (from 12.6% in 1998 to 24.9% in 2019).[17]

I further examined trends in separate ownership of liquid assets by gender over time. Figure 3-2 visualizes the rate of sole ownership of liquid assets by men and women in couples between 1998 and 2019. Men and women were about equally likely to have separately-owned liquid assets. Similar trends in sole-ownership of liquid assets were also observed for men and women. Overall, the proportion of both men and women with sole-owned liquid assets steadily increased (from 21% in 1998 to 40% in 2019 for men and from 26% in 1998 to 40% in 2019 for women). Figure 3-2 shows increases for both checking and savings accounts, but savings accounts are held by fewer individuals. Taking a closer look at differences in sole-owned liquid asset patterns between men and women, Figure 3-3 presents the median account balances in

[17] These observed trends in liquid assets pooling and sole ownership of liquid assets within couples are likely explained to some extent by compositional changes over time in the population of couples with children. In Table 3-4, I present characteristics of couples with children across multiple time points of the study period (years 1998, 2004, 2013, and 2019). Large increases in educational attainment between 1998 and 2019 and a decrease in the share of married couples stand out as compositional changes that may contribute to the increase in separate ownership of liquid assets within couples.

men's and women's accounts over time (this analysis is restricted to couples with at least one separately-owned account) and shows evidence for gender differences in liquid asset ownership. The figure shows that the median account balance is greater for men than women across all years of the data. The magnitude of the difference in liquid assets was greatest in 2019 ($1,000 vs. $2,200).

3.6.2. Investigating Correlates of Liquid Asset Ownership by Gender

Figure 3-4 presents linear probability models that consider whether the education, employment, and health characteristics of each partner affect the likelihood that they and their partner have separately held liquid assets (full results with controls shown in Table 3-5). I first turn to estimates of liquid asset ownership among women in panel A. After adjusting for controls, results suggest that the likelihood of a woman having separate assets increases with her education level (associate's degree or higher: $b = 0.06$, $p < 0.05$ [reference category = high school degree or less]) and her employment (part-time work: $b = 0.13$, $p < 0.001$; full-time work: $b = 0.16$, $p < 0.001$ [reference category = not working for pay). I do not find a statistically significant association between women's health and separate ownership of liquid assets. Considering separately the characteristics of male partners, results suggest that a woman's likelihood of separate liquid asset ownership decreases with her male partner's education level (associate's degree or higher: $b = -0.08$, $p < 0.01$ [the reference category = high school or less]) as well with his health status (in good health $b = -0.09$, $p < 0.001$; in excellent health: $b = -0.07$, $p < 0.05$ [the reference category = poor or fair health]). Therefore, my results suggest that while a woman's

ownership of liquid assets is more strongly predicted by her own characteristics, the characteristics her partner do factor into her likelihood of owning separate liquid assets.

While I found that individual-level characteristics are associated with account ownership, I also sought to test explicitly whether couple-level indicators of the differentials between partners in their education, employment, and health characteristics were associated with each partner's account ownership. Table 3-3 presents my analysis considering the effect of *within-couple* differentials on the likelihood that each partner has separately owned liquid assets. Linear probability models of sole liquid asset ownership were estimated separately for men and women. The regression model coefficients should be interpretated as relative to the "neutral" reference categories, where both partners are categorized as having similar education, employment, or health status. The "female-skewed" categories refer to partnerships in which the female partner has a higher level of education, employment, or health. The "male-skewed" categories refer to partnerships in which the male partner has a higher level of education, employment, or health.[18]

The results in Table 3-3 suggest that the likelihood of a woman having separate liquid assets is strongly associated with her relative education, health, and employment within the relationship. The likelihood that a woman separately holds liquid assets is approximately 9 percentage points higher ($b = 0.09$, $p < 0.001$), 8 percentage points higher ($b = 0.12$, $p < 0.001$);, and 12 percentage points higher ($b = 0.12$, $p < 0.001$); when education, health, or employment status is "female-skewed," respectively. Additionally, I observed that a woman's likelihood of separately owning liquid assets is 8 percentage points lower when employment is "male-skewed" ($b = 0.12$, $p < 0.001$). The results show, however, that the likelihood that men separately own

[18] Table 3-6 shows the within-couple distribution of these three characteristics. There are noteworthy gender differences in employment and education within couples; employment is "male-skewed" for 41% of couples vs. "female-skewed" for 6% of couples, education is "male-skewed" for 8% of couples vs. "female-skewed" for 17% of couples. Health is "female skewed" for 10% of couples vs. "male-skewed" for 8% of couples.

liquid assets is not significantly associated with any within-couple differential in education, health, or employment.

3.7. **Discussion and Conclusion**

In this study, I sought to examine couples' patterns of liquid asset ownership using data on liquid assets from the 1998-2019 Survey of Consumer Finances. Given the importance of parents' economic resources for children's outcomes (Lerman & McKernan, 2008) and gender differences in the effects of parenthood on employment and earnings (Stanczyk, 2020), I focused my analyses specifically on couples with children. Given that information about assets is typically collected and measured at the household-level, the SCF data provided a rare opportunity to assess both joint and personal liquid asset ownership within couples.

In this study, I documented a rising share of couples with separately-owned liquid assets between 1998 and 2019. I find that men's and women's use of separately-owned transaction accounts followed similar trends, although women's median liquid asset holdings were less than men's. Consistent with previous research (Klawitter & Fletschner, 2011), I find that intra-partnership bargaining power is associated with separate liquid asset ownership for women, but not for men.

Although I did not directly study the consequences of the rising share of couples with separately-owned liquid assets or gendered patterns in liquid asset ownership, this study's finding have potential implications for individual family-member's wellbeing. Previous research suggests that couples' pooling of economic resources may reflect joint spending on children

(Treas, 1993; Eickmeyer, Manning, & Brown 2019; Kenney 2008) and that mother's greater control over household economic resources is associated with better child outcomes (e.g., food security) and more spending on children (Lundberg & Pollak 1996; Phipps & Burton 1998; Kenney, 2008). Furthermore, patterns of asset ownership within couples during relationships have implications for how each partner fares after a relationship ends or the death of a partner (Kapelle & Baxter, 2021; Joseph & Rowlingson, 2012; Lewis et al., 2009), especially for cohabitating couples for whom there is no or little legal structure to determine the division of assets upon relationship dissolution.

However, ultimately, whether an increase in separate financial arrangements is a positive or negative outcome for individual family members is unclear. For instance, separate financial arrangements may provide a degree of financial autonomy and distributes control over money is spent. This study cannot assess the reasons for or consequences of observed patterns in couple's financial arrangements for individual family-member's wellbeing. There is also likely substantial heterogeneity in the reasons that individuals within couples pool or keep liquid assets separate.

This study has several limitations. Although the SCF data allow me to capture detailed information about the ownership of liquid financial assets within couples, the SCF does not collect information about sole-ownership of other important (and potentially much larger and/or more inequitably distributed) components of household wealth, such as debts or tangible assets in which savings can be accumulated (e.g., vehicles, home ownership).[19] It is also possible that levels of liquid assets measured in this study could be the result of investing in other types of

[19] The SCF collects information about many types of household wealth but does not ask any direct questions about separate ownership of types other than liquid asset, with one exception: sole ownership of retirement assets (which are all owned at the individual level). In Figure 3-5, I show trends in whether each partner has any retirement assets by gender. I find evidence of gender gaps in ownership of retirement assets (male partners are more likely to have retirement assets than female partners; for example, in 2019, 18.6% of female partners had any retirement assets compared to 22.1% of male partners).

assets (e.g., housing) If data is available on separate ownership of other types of household assets, future studies that consider whether there are similar patterns or trends in asset ownership for other asset types would help us gain a more complete picture on this topic.

Another limitation is that although my results show a rise in the share of couples with separately owned liquid assets, the cross-sectional nature of the SCF does not permit observation of changes in asset ownership within any particular couple over time. Therefore, additional data sources are needed to consider whether changes in the education, employment, or health characteristics of couples are associated with changes in their financial arrangements.

A further limitation of this study is that the SCF does not provide information on actual control over household economic resources, and therefore I could only categorize couples based on formal, legal ownership of liquid assets. It is possible that each partner's control over these assets may differ from formal ownership. Despite these caveats, legal restrictions on accounts and asymmetric information between partners mean that account ownership likely matters in determining control over liquid assets (Klawitter, 2008).

Finally, to measure bargaining power, I focus on each partner's relative education, employment, and health status; however, each of these characteristics may be a function of previous intra-household decisions shaped by bargaining power. Similarly, there may be reverse causality in that each partner's aforementioned characteristics may have been influenced by their past asset ownership. Therefore, it is possible that my estimates are biased. Future work is needed that can determine the temporal ordering of measures of bargaining power and liquid asset ownership.

In conclusion, I demonstrate that couples with children have increasingly kept their financial arrangements separate over recent decades. I also find evidence that partners' relative

education, employment, and health (proxies for bargaining power) are significantly associated with the distribution of liquid asset ownership within couples. In particular, women's separate-ownership of liquid assets is sensitive to intrahousehold bargaining power. These findings question the unitary model of the household – i.e., the assumption that economic resources are shared equally within couples – and suggests greater need for data collection efforts and assessment of individual-level ownership of assets and debts within families. This study also points more generally to the need for greater understanding of intra-household distributions to help inform policies that aim to meet the needs of families. The success of many policies -- such as the measurement of poverty, the targeting of government transfers, and taxation – depends on assumptions about resource sharing within families. Results of this study can also inform policies that that aim to support families, especially policies that are designed to reduce barriers for women to combine work and family. Bargaining power is shaped by factors outside of the household, such as family policies and labor markets (Lundberg & Pollak 1996). Since, I found find that differentials between men's and women's employment are particularly predictive of women's asset ownership, "family friendly" policies that improve gender equality in the labor market – such as affordable childcare, paid family leave, and "secure scheduling" laws – may increase asset ownership among women.

3.8. **References**

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3.9. Tables and Figures

Table 0-1 Descriptive statistics of the Survey of Consumer Finances samples

	SCF 1998-2019	SCF 2016-2019
Married (%)	88	86
Relationship duration (in years), mean	11	10
Household income (%)		
$0-$50,000	23	24
$50,001-$100,000	35	33
>$100,001	41	44
Race/ethnicity of the respondent (%)		
Non-Hispanic Black	9	10
Hispanic	15	17
Other race or ethnicity	6	8
Non-Hispanic White	70	65
Number of children, mean	2	2
Any children living in another household (%)	6	5
Any intra-household transfers (%)	22	21
Characteristics of women		
Employment (%)		
Not working for pay	31	28
Part-time	17	17
Full-time	52	55
Self-rated health (%)		
Poor	2	2
Fair	13	14
Good	51	51
Excellent	35	33
Education (%)		
Less than high school	10	8
High school	24	16
Some college	24	15
Associate's degree or higher	42	60
Age (years), mean	38	38
Characteristics of men		
Employment (%)		
Not working for pay	7	7
Part-time	4	4
Full-time	89	90
Self-rated health (%)		

Table 3-1 (continued)

Poor	2	2
Fair	14	16
Good	52	54
Excellent	32	28
Education (%)		
Less than high school	12	11
High school	28	24
Some college	20	12
Associate's degree or higher	40	52
Age (years), mean	40	40
N	10,429	2,611

Notes: Weighted using SCF sampling weights. Sample limited to married or cohabitating couples who are working age (in the age range 23-60 years old) with at least one child under the age of 19 residing in their household. Sample sizes by year: 1168 (1998), 1230 (2001), 1172 (2004), 1142 (2007), 1646 (2010), 1460 (2013), 1386 (2016), 1225 (2019)
Source: Survey of Consumer Finances (SCF)

Table 0-2 Proportion of couples with liquid assets, married or cohabitating couples with children, SCF 2016-2019

	Couple has any liquid assets	Female partner has separately-owned liquid assets	Male partner has separately-owned liquid assets
All couples (%)	95	36	39
Marital status (%)			
Married	97	32	36
Cohabitating	86	60	86
Household income (%)			
$0-$50,000	82	27	32
$50,001-$100,000	99	42	42
>$100,001	100	37	41
Race/ethnicity of the respondent (%)			
Non-Hispanic Black	92	54	50
Hispanic	88	32	36
Other race or ethnicity	97	37	47
Non-Hispanic White	98	35	38
Any children living in another household (%)	88	37	41
Any intra-household transfers (%)	93	60	54
Characteristics of women			
Employment (%)			
Not working for pay	91	21	34
Part-time	98	36	39
Full-time	97	45	43
Self-rated health (%)			
Poor	86	35	39
Fair	89	35	38
Good	96	38	41
Excellent	98	35	38
Education (%)			
Less than high school	72	18	26
High school	93	36	42
Some college	95	41	43
Associate's degree or higher	99	38	40

Table 3-2 (continued)			
Characteristics of men			
Employment (%)			
Not working for pay	86	41	29
Part-time	90	39	40
Full-time	96	36	40
Self-rated health (%)			
Poor	82	34	31
Fair	91	43	37
Good	96	35	41
Excellent	98	35	38
Education (%)			
Less than high school	80	27	32
High school	94	42	39
Some college	97	47	47
Associate's degree or higher	99	33	40

Notes: Weighted using SCF sampling weights. Sample limited to married or cohabitating couples who are working age (in the age range 23-60 years old) with at least one child under the age of 19 residing in their household. N = 2,611
Source: Survey of Consumer Finances (SCF) 2016-2019

Table 0-3 Bargaining power and separately-owned assets by gender, 2016-2019

	Couples in which the woman separately owns any assets		Couples in which the man separately owns any assets	
Intercept	0.52 ***	(0.07)	0.74 ***	(0.08)
Bargaining Position				
Education				
Neutral (reference)				
Female-skewed	0.09 ***	(0.02)	-0.00	(0.03)
Male-skewed	-0.01	(0.03)	0.06	(0.04)
Health				
Neutral (reference)				
Female-skewed	0.08 **	(0.03)	0.01	(0.03)
Male-skewed	-0.05	(0.03)	0.05	(0.03)
Employment				
Neutral (reference)				
Female-skewed	0.12 ***	(0.04)	-0.03	(0.04)
Male-skewed	-0.08 ***	(0.02)	-0.01	(0.02)
Controls				
Marital Status				
Cohabitating (reference)				
Married	-0.31 ***	(0.03)	-0.29 ***	(0.03)
Relationship duration (in years)	-0.02 ***	(0.00)	-0.01 ***	(0.00)
Household income				
$0-$50,000 (reference)				
$50,001-$100,000	0.10 ***	(0.03)	0.06 *	(0.03)
>$100,001	0.09 **	(0.03)	0.10 ***	(0.03)
Race/Ethnicity of the respondent				
Non-Hispanic Black (reference)				
Hispanic	-0.12 ***	(0.04)	-0.06	(0.04)
Other race or ethnicity	-0.09 *	(0.04)	-0.01	(0.05)
Non-Hispanic White	-0.14 ***	(0.03)	-0.10 ***	(0.03)
Number of children	-0.01	(0.01)	-0.03 **	(0.01)
Any children living in another household	-0.03	(0.04)	0.04	(0.05)
Any intra-household transfers	0.12 ***	(0.02)	0.06 *	(0.02)
Age, female	0.01 ***	(0.00)	-0.002	(0.003)

Table 3-3 (continued)

Age, male	0.00	(0.00)	0.005 *	(0.002)

Notes: The table display coefficients interpreted as the percentage point increase or decrease in the probability of having any separately-owned assets with the variable of interest. Assets are defined as holdings in checking or savings accounts. Standard errors in parentheses. Models are weighted using SCF sampling weights. *Female-skewed education* = the female (but not the male) partner has an associate's degree or higher; *Male-skewed education* = the male (but not the female) partner has an associate's degree or higher; *Neutral education* = the male and female partner have the same level of education; *Female-skewed health* = the female (but not the male) partner has "good" or "excellent" health (self-rated); *Male-skewed health* = the male (but not the female) partner has "good" or "excellent" health (self-rated); *Neutral health* = the male and female partner have the same health status; *Female-skewed employment* = the female (but not the male) partner is employed full-time; *Male-skewed employment* = the male (but not the female) partner is employed full-time; *Neutral employment* = the male and female partner have the same level of employment. Sample limited to different-sex married or cohabitating couples with assets who are working age (in the age range 23-60 years old) with at least one child under the age of 19 residing in their household. N = 2,471 (couples).

*** $p < .001$; ** $p < .01$; *$p < .05$

Source: Survey of Consumer Finances (SCF) 2016-2019

Table 0-4 Descriptive statistics by year, married or cohabitating couples with children

	1998	2004	2013	2019
Married	92	90	86	85
Any children living in another household	6	6	4	5
Any intra-household transfers	22	22	22	20
Household income (%)				
$0-$50,000	24	21	26	24
$50,001-$100,000	39	36	33	32
>$100,001	37	43	41	45
Race/ethnicity of the respondent (%)				
Non-Hispanic Black	7	8	11	10
Hispanic	11	14	16	16
Other race or ethnicity	4	5	6	9
Non-Hispanic White	78	73	67	65
Characteristics of women				
Employment (%)				
Not working for pay	30	31	35	27
Part-time	16	17	16	18
Full-time	55	51	49	56
Self-rated health (%)				
Poor	2	2	2	2
Fair	13	10	15	14
Good	50	50	51	51
Excellent	35	39	32	33
Education (%)				
Less than high school	10	11	9	7
High school	32	26	23	16
Some college	30	29	26	14
Associate's degree or higher	28	35	42	63
Characteristics of men				
Employment (%)				
Not working for pay	6	7	9	6
Part-time	3	4	4	3
Full-time	91	90	87	91
Self-rated health (%)				
Poor	2	2	2	2
Fair	13	13	15	17
Good	51	50	55	54

Table 3-4 (continued)				
Excellent	34	35	28	28
Education (%)				
Less than high school	15	12	11	10
High school	28	28	28	23
Some college	27	22	20	13
Associate's degree or higher	30	39	41	53

Notes: Weighted using SCF sampling weights. Sample limited to married or cohabitating couples who are working age (in the age range 23-60 years old) with at least one child under the age of 19 residing in their household.
Source: Survey of Consumer Finances (SCF)

Table 0-5 Linear probability model estimates predicting separately-owned liquid assets by gender, 2016-2019

	Couples in which the woman separately owns any assets		Couples in which the man separately owns any assets	
Intercept	0.43 ***	(0.08)	0.59 ***	(0.09)
Married	-0.29 ***	(0.03)	-0.28 ***	(0.03)
Relationship duration (in years)	-0.02 ***	(0.00)	-0.01 ***	(0.00)
Household income				
$0-$50,000 (reference)				
$50,001-$100,000	0.08 **	(0.03)	0.04	(0.03)
>$100,001	0.06 *	(0.03)	0.07 *	(0.03)
Race/ethnicity of respondent				
Black (reference)				
Hispanic/Latino	-0.12 ***	(0.04)	-0.06	(0.04)
Other	-0.07	(0.04)	0.01	(0.05)
White	-0.13 ***	(0.03)	-0.10 **	(0.03)
Number of children	-0.01	(0.01)	-0.03 **	(0.01)
Any children living in another household	-0.03	(0.04)	0.05	(0.04)
Any intra-household transfers	0.11 ***	(0.02)	0.06 **	(0.02)
Characteristics of women				
Education				
High school or less (reference)				
Some college	0.05	(0.03)	-0.01	(0.03)
Associate's degree or higher	0.06 *	(0.03)	-0.04	(0.03)
Employment				
Not working for pay (reference)				
Part-time	0.13 ***	(0.03)	0.03	(0.03)
Full-time	0.16 ***	(0.02)	0.03	(0.02)
Self-rated health				
Poor or fair (reference)				
Good	0.05	(0.03)	0.01	(0.03)
Excellent	0.01	(0.03)	-0.02	(0.03)
Age	0.01 **	(0.00)	0.00	(0.00)
Characteristics of men				
Education				

Table 3-5 (continued)				
High school or less (reference)				
Some college	0.01	(0.03)	0.05	(0.03)
Associate's degree or higher	-0.08 **	(0.02)	0.04	(0.03)
Employment				
Not working for pay (reference)				
Part-time	0.00	(0.06)	0.14 *	(0.06)
Full-time	-0.04	(0.04)	0.12 **	(0.04)
Self-rated health				
Poor or fair (reference)				
Good	-0.09 ***	(0.03)	0.03	(0.03)
Excellent	-0.07 *	(0.03)	0.00	(0.03)
Age	0.00	(0.00)	0.01 *	(0.00)

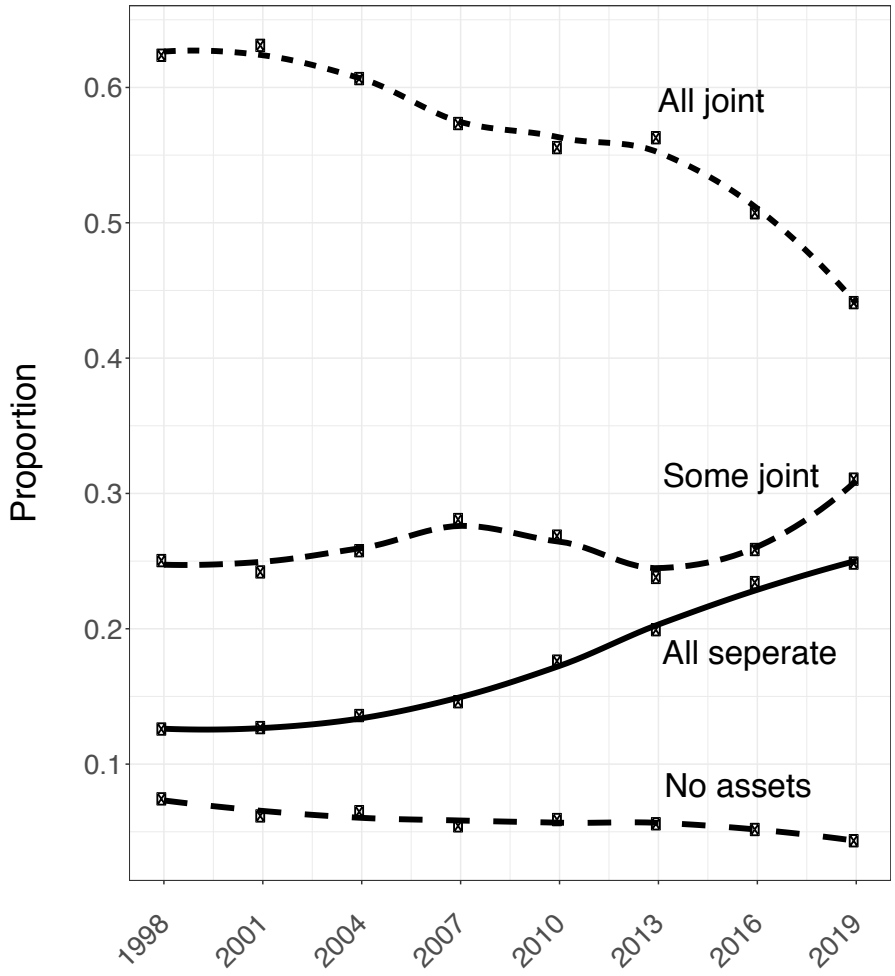
Notes: Coefficients interpreted as the percentage point increase or decrease in the probability of having any separately-owned assets with the variable of interest. Assets are defined as holdings in checking or savings accounts. Standard errors in parentheses. Models are weighted using SCF sampling weights. Sample limited to different-sex married or cohabitating couples who are working age (in the age range 23-60 years old) with at least one child under the age of 19 residing in their household. N = 2,471 (couples).
Source: Survey of Consumer Finances (SCF) 2016-2019

Table 0-6 Within-couple distribution of education, health, and employment, married or cohabitating couples with children, 2016-2019

	%
Education	
Neutral (reference)	75
Female-skewed	17
Male-skewed	8
Health	
Neutral (reference)	81
Female-skewed	10
Male-skewed	8
Employment	
Neutral (reference)	53
Female-skewed	6
Male-skewed	41

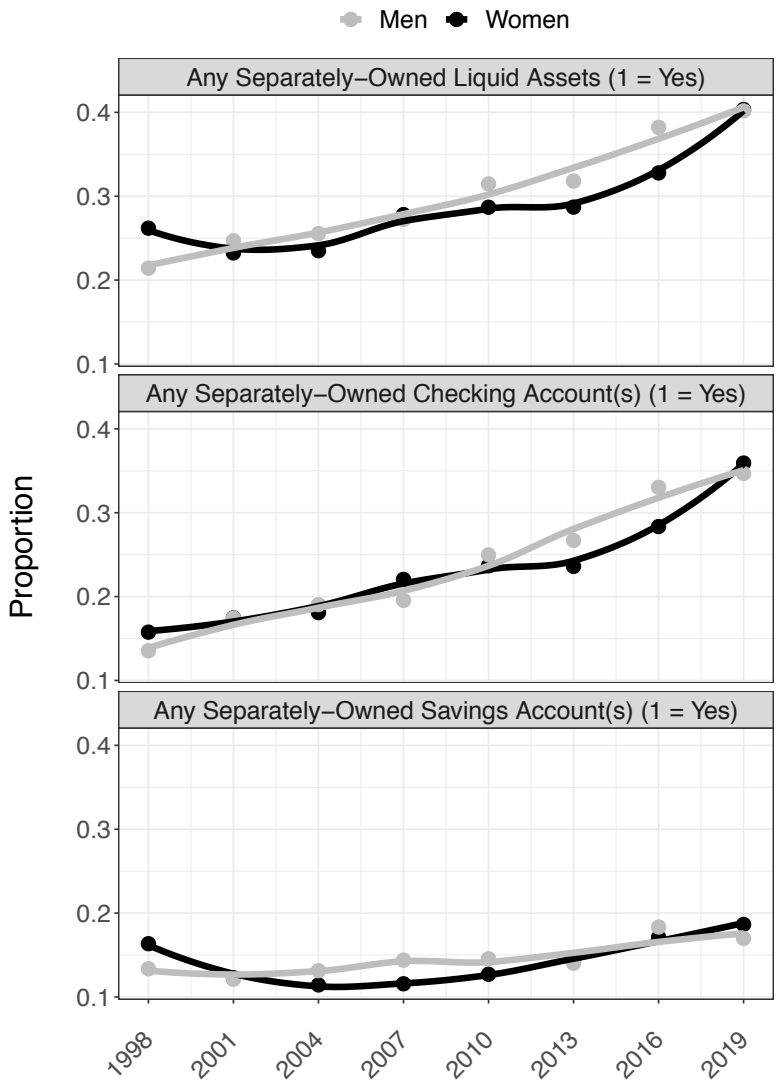
Notes: *Female-skewed education* = the female (but not the male) partner has an associate's degree or higher; *Male-skewed education* = the male (but not the female) partner has an associate's degree or higher; *Neutral education* = the male and female partner have the same level of education; *Female-skewed health* = the female (but not the male) partner has "good" or "excellent" health (self-rated); *Male-skewed health* = the male (but not the female) partner has "good" or "excellent" health (self-rated); *Neutral health* = the male and female partner have the same health status; *Female-skewed employment* = the female (but not the male) partner is employed full-time; *Male-skewed employment* = the male (but not the female) partner is employed full-time; *Neutral employment* = the male and female partner have the same level of employment. Estimates are weighted using SCF sampling weights. Sample limited to different-sex married or cohabitating couples who are working age (in the age range 23-60 years old) with at least one child under the age of 19 residing in their household. N = 2,471 (couples).
Source: Survey of Consumer Finances (SCF)

Figure 0-1. Liquid asset pooling, married or cohabitating couples with children, 1998-2019



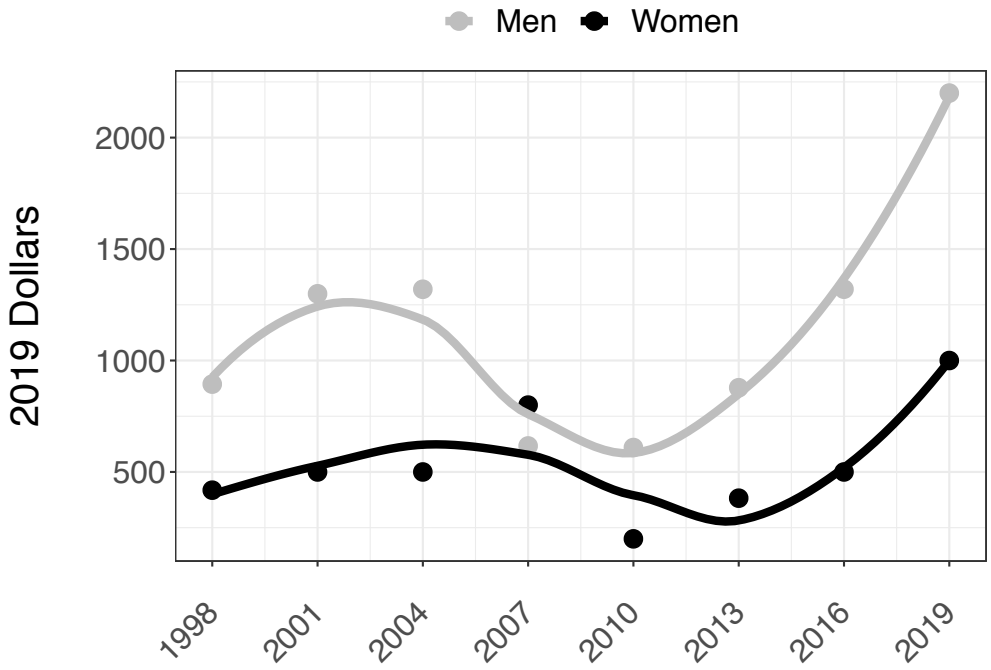
Notes: Estimates based on assets held in couples' checking and savings accounts. Proportions are weighted using SCF sampling weights.
Source: Survey of Consumer Finances (SCF) 1998-2019

Figure 0-2 Separate ownership of liquid assets by gender, married or cohabitating couples with children, 1998-2019



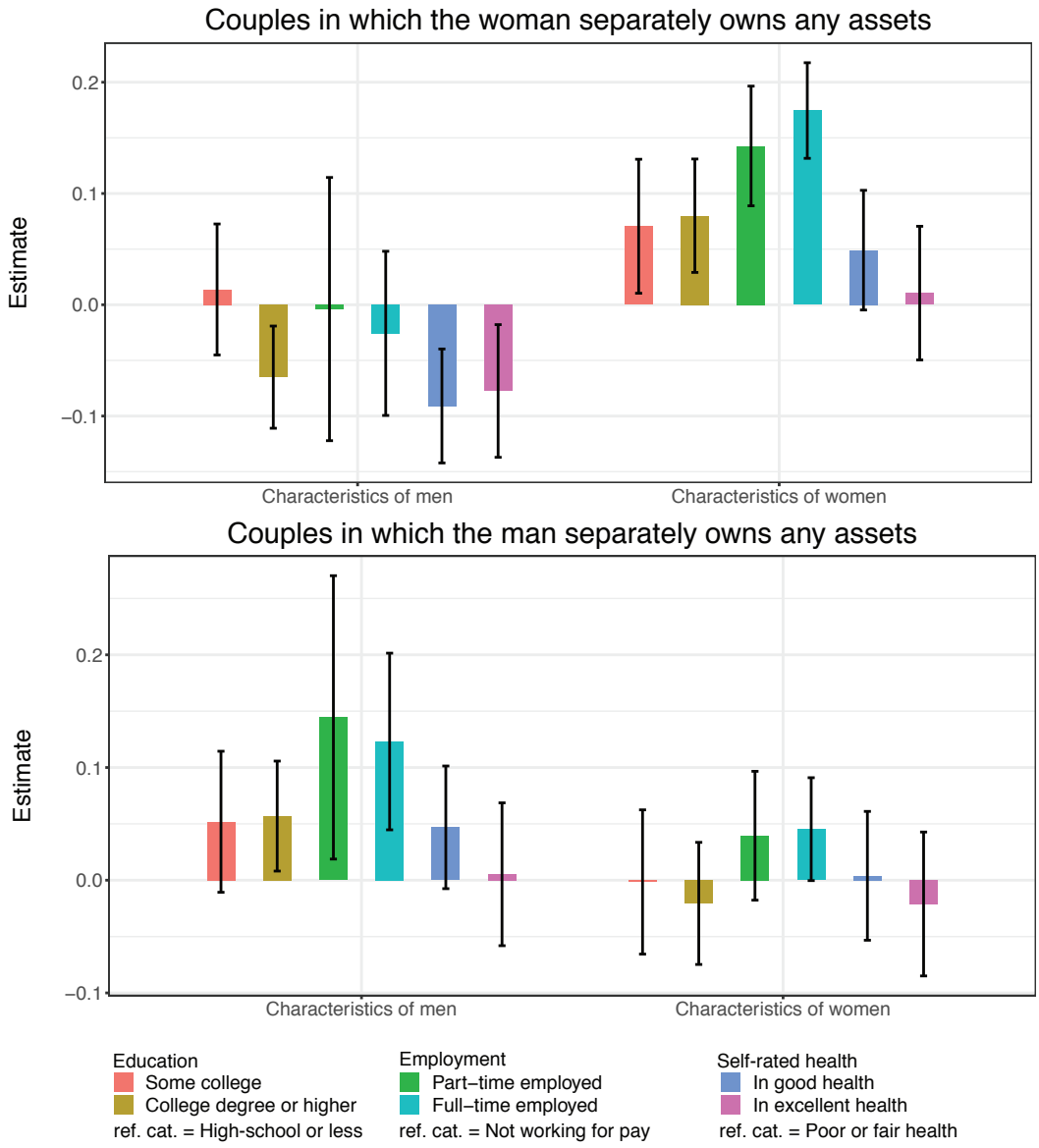
Notes: Proportions are weighted using SCF sampling weights.
Source: Survey of Consumer Finances (SCF) 1998-2019

Figure 0-3 Median liquid asset holdings, by gender, married or cohabitating couples with children, 1998-2019



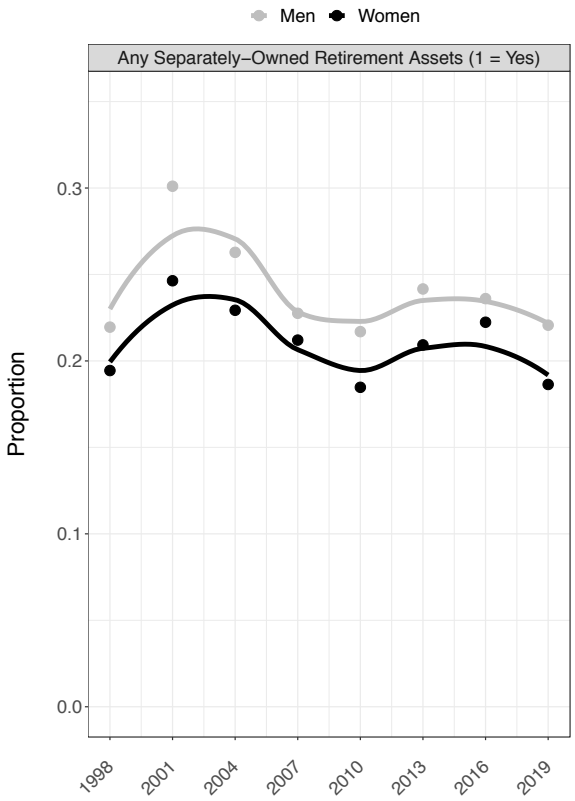
Notes: Sample limited couples any separately-owned assets (defined as holdings in checking or savings accounts). Proportions are weighted using SCF sampling weights.
Source: Survey of Consumer Finances (SCF) 1998-2019

Figure 0-4 Linear probability model estimates predicting separately-owned liquid assets by gender, 2016-2019



Notes: The figures display coefficients interpreted as the percentage point increase or decrease in the probability of having any separately-owned liquid assets with the variable of interest. Assets are defined as holdings in checking or savings accounts. Standard errors in parentheses. Models are weighted using SCF sampling weights. Sample limited to different sex married or cohabitating couples with liquid assets who are working age (in the age range 23-60 years old) with at least one child under the age of 19 residing in their household. N = 2,471 (couples).
Source: Survey of Consumer Finances (SCF) 2016-2019

Figure 0-5 Separate ownership of retirement assets by gender, married or cohabitating couples with children, 1998-2019



Notes: Proportions are weighted using SCF sampling weights.
Source: Survey of Consumer Finances (SCF) 1998-2019

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