Income and Child Maltreatment: Evidence from a Discontinuity in Tax Benefits

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Abstract

Poverty is one of the leading predictors of child maltreatment, yet the causal relationship is not well-understood. In this paper I provide new evidence of the effects of income on child protection system (CPS) referrals, investigations and foster care placements. I exploit a discontinuity in child-related tax benefits around a January 1 birthdate, which results in otherwise-similar families receiving considerably different refunds during the first year of a child's life. I use 20 years of linked administrative data from California to determine the effects of this additional income on CPS involvement. A one-time $1,000 transfer to low-income households decreases the number of referrals to CPS in the first 3 years of a child's life by approximately 3%. These effects persist throughout the system, decreasing investigations (3%) and days spent in foster care (8%). Effects also persist throughout childhood, reducing CPS involvement through at least age 8. Heterogeneity analyses by allegation and reporter category as well as by child race and gender suggest that these effects capture true reductions in maltreatment, as opposed to changes in reporting behavior. These findings suggest that providing low-income families with additional resources during the first year of a child's life are a fruitful strategy for reducing child maltreatment.

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1 Introduction

Safeguarding child well-being is a central U.S. policy goal. While transfers to children are often made in-kind to avoid resources being diverted to other family members, there is increasing interest in using unconditional cash transfers to improve child welfare. However, evidence on the effects of cash transfers to parents on direct measures of child well-being is limited. I examine the causal effects of additional household income during the first year of a child's life on a central measure of child well-being: child maltreatment.

Child maltreatment is both widespread and costly. Over one third of children in the U.S. are investigated for maltreatment by the child protection system (CPS) by age 18 (Kim et al. 2017).[1] Victims of maltreatment obtain less education, are less likely to be employed, have lower earnings and assets as adults (Currie and Spatz Widom 2010), and are more likely to themselves commit crime later in life (Currie and Tekin 2012). Likelihood of maltreatment is highly correlated with household poverty, but causal evidence is scarce (Sedlak et al. 2010). It is not well-understood whether additional income would cause a reduction in child maltreatment, or if instead the two are related through third factors such as parental mental health or substance abuse.

To study the causal relationship between poverty and maltreatment, I exploit a discontinuity in tax benefits around the January 1 birth date which substantially affects low-income families' available resources during the first year of a child's life. Child-related tax benefits include the personal exemption, as well as (for eligible families) the Earned Income Tax Credit and Child Tax Credit. Together, these tax benefits make up a substantial portion of household income for low-income families. Families may claim children born in December on their taxes for that year, and receive payments as early as the third month of their child's life. Families of children born just a few days later in January may not claim their newborn until the following year, delaying tax payments until 15 months after birth. The eligibility cutoff around January 1 motivates a regression discontinuity design that compares children born just before to those born just after the New Year.

I use rich administrative data that links the universe of California birth records from 1999 through 2020 with CPS referrals, home investigations, and foster care placements over that same time period. This linked dataset allows me to track each child's CPS involvement over time and to observe comprehensive information about the birth, the child, and the parents as well as information about any maltreatment allegations, investigations and foster care placements.[2]

[1] Maltreatment includes both neglect and abuse.

[2] Referrals come from third-party individuals including mandated reporters and community members who have a concern that a child is being abused or neglected. For a subset of these referrals, CPS caseworkers investigate the allegations by visiting

In my main results, I focus on families for whom these tax benefits are likely to represent a substantial share of household income: first births to low-income households. I limit my sample to first births, as cash benefits to these families are not split between multiple children. The birth records do not report income, so I use the American Community Survey (ACS) to predict household income based on observable demographics such as parental race, age, and education. I define as "low-income" households with a predicted after-tax income below 200% of the poverty line for a family of three, but confirm my results are robust to several alternate definitions. For first births to low-income households, I estimate that having a child born in December as opposed to January results in on average $2,881 additional income in tax benefits during the first year of life.

In order to identify the causal effects of income on maltreatment, I use a regression discontinuity design that compares infants born before January 1 to those born after. I exclude a "donut" around the January 1 cutoff to account for any birth timing manipulation on the part of parents or medical providers. The identifying assumption is that births around January 1 and outside this excluded region are as good as randomly assigned. I test this assumption by verifying that a range of birth and parent characteristics reported in the birth records are smooth around the cutoff.

Additional income during the first year of a child's life significantly reduces involvement with CPS.[3] I estimate that infants born before January 1 receive 0.019 (7.6%) fewer referrals to CPS through age 2 than those born directly after the cutoff. Scaling this effect to my estimate of the tax benefits for these families suggests that a $1,000 transfer to low-income families decreases total number of referrals through age 2 by 2.6%. This is driven by both the extensive and intensive margins. Effects persist throughout the child protection system, decreasing both investigations (3.1%) and days spent in foster care (7.9%).

The protective effect of cash persists without attenuation through at least age 8. This indicates either that additional income during the first year of a child's life causes long-lasting changes in household stability and well-being, or that CPS involvement may be persistent (i.e., a child who is investigated once may be more likely to have continued involvement). Overall, I estimate that eligibility for tax benefits reduces the number of referrals, investigations, and days spent in foster care through age 8 by 9.4%, 11.0% and 22.4%, respectively.

Reductions in CPS referrals may reflect reporting effects if, for example, additional income reduces interactions with health professionals. However, I find the largest effects of income on reductions in foster

the child's home. In cases of serious and severe maltreatment, the child may be removed from their home and placed in foster care.

[3] In my main analysis I limit the birth cohorts to 1999 through 2017, and observe a three year follow up for each birth. In additional analyses I reduce the number of birth cohorts in order to observe outcomes through age 8.

care placements, which are a last-resort approach to substantiated and severe maltreatment. This implies that the marginal children diverted from CPS would have been *more* likely than average to be placed in foster care, or that for some children, additional income improves well-being enough to keep them out of foster care but not enough to keep them from being referred. If more severe maltreatment is most likely to be caught by reporters, this shows that additional income is affecting rates of maltreatment, and my results can be interpreted as capturing true changes in child well-being. Heterogeneity by child race and gender as well as by reporter and allegation category provide additional support for this interpretation.

My data provide detailed information on child demographics, as well as reporter and allegation categories for CPS referrals. Heterogeneous effects by child race speak to the possible causes of racial disparities within CPS. For children born to Black mothers, additional income has no effect on reports or investigations but substantially reduces days spent in foster care, suggesting improvements in child well-being which do not affect reporting behavior. Effects are substantially larger for boys, suggesting either that parents are more likely to invest additional income in boys, or that boys are more sensitive to changes in resources. Results through age 2 are driven primarily by allegations of neglect and physical abuse from medical professionals, suggesting that doctors notice differences in infant health caused by increased family resources. Overall, these patterns are most consistent with effects on maltreatment rates, as opposed to reporting rates.

Results are robust to variations of my main specification, including a wide range of alternative bandwidth and donut sizes, and alternative definitions of low-income households. Several placebo tests provide additional support for my results. Regression discontinuity effects around other holidays with no associated tax benefits are close to zero and statistically insignificant. I also find null effects on CPS outcomes before the arrival of tax benefits, and among sub-samples with lower likelihood of tax benefit eligibility and for whom the expected effect of additional income is small.

Overall, these findings suggest that providing low-income families with additional resources during the first year of a child's life may be a fruitful strategy for improving child well-being. The results have important implications for policy discussions around expanding the Child Tax Credit, as they point to an additional benefit of providing cash assistance to low-income parents. In particular, my results suggest that an additional $1,000 offered to every child born to a low-income California household in 2017 ($\sim$140,000 children) would result in over 600 fewer child-years spent in foster care over the first nine years of life. Using estimates for per-victim lifetime costs of maltreatment, each $1,000 spent would return up to $4,764 in long-term benefits including productivity gains and reduced health care, child welfare, and criminal justice costs.[4]

[4]This estimate multiplies my estimated effect on likelihood of any investigation in the first nine years of life by Peterson, Florence, and Klevens (2018)'s estimates of per-victim lifetime costs of maltreatment.

This paper primarily contributes to two literatures: (1) the effects of early childhood investments on long-term outcomes, and (2) causal research related to the institution of CPS. Early childhood investments have been found to have long-term benefits to health, educational outcomes, labor market outcomes, and involvement in the criminal justice system.[5] A subset of this literature studies effects of income transfers in particular, using EITC expansions (Dahl and Lochner 2012; Hoynes, Miller, and Simon 2015; Bastian and Michelmore 2018), "baby bonus" policies (Gendre et al. 2021; Borra et al. 2021), and the discontinuity in child-related tax benefits around the January 1 birth date (Barr, Eggleston, and Smith 2022; Cole 2021).[6] Finally, an ongoing randomized control trial studies effects of large monthly unconditional cash transfers on child development and parental behavior.[7] Early results suggest that cash transfers increase infant brain activity (Troller-Renfree et al. 2022), spending on child-specific goods, and time spent by mothers in early-learning activities (Gennetian et al. 2022). My work contributes to this literature by studying the effects of income on child maltreatment, a first-order measure of child well-being and a possible mechanism for the established longer-term effects.

This paper also contributes to the economics literature on child maltreatment and the child protection system. This body of work has largely focused on identifying the effects of foster care placements. In different contexts, researchers have found both positive and negative effects of foster care placements for children on the margin of removal (Doyle 2007, 2013, 2008; Roberts 2019; Gross and Baron 2022; Baron and Gross 2022; Bald, Chyn, et al. 2022). Bald, Doyle, et al. (2022) provide an overview of foster care practices, trends and research. Grimon (2020) is the first to study effects of earlier-stage interaction with CPS, and the first to study effects on parents, finding that opening a child welfare case increases mothers' enrollment in substance abuse and mental health services. Whereas much of this literature identifies effects of CPS involvement, my work speaks to the causal drivers of maltreatment.

Two interdisciplinary papers study the link between maltreatment and poverty, finding suggestive evidence that tax-driven increases in household income reduce maltreatment reports among a small and selected group of low-income households (Berger et al. 2017) and in the month after benefit receipt (Kovski et al. 2022). I contribute to this literature by leveraging quasi-experimental variation and using micro-level

[5] Studies have found long-run effects of access to nutritional programs (e.g. Hoynes, Schanzenbach, and Almond (2016), Barr and Smith (2021), and Bitler and Figinski (2019)), education (e.g. Ludwig and Miller (2007), Heckman et al. (2010), and Thompson (2018)), and health insurance (e.g. Goodman-Bacon (2021) and Brown, Kowalski, and Lurie (2020)).

[6] Cole (2021) exploits the discontinuity in tax benefits around the January 1 birth date to find that an extra $1,000 during infancy increases the likelihood of a child being grade-for-age by high school. Barr, Eggleston, and Smith (2022) use a similar approach, and find that the increased tax benefits associated with a December birth improve test scores and the likelihood of graduating high school, and increase earnings in early adulthood by 1-2 percent for infants born to low-income parents.

[7] The Baby's First Years study recruited 1,000 mothers of newborns to receive 52 monthly payments of either $333 (treatment) or $20 (control). The study will collect data annually through age 4.

administrative data for the state of California over two decades. My setting allows me to study effects for different sub-samples of the population, and follow children over the course of childhood. Moreover, I speak to the specific effects of providing additional income during the very first year of a child's life, a time in which families may face increased liquidity constraints and during which children are particularly vulnerable.

The paper proceeds as follows: In Section 2, I discuss background information on child-related tax benefits, child maltreatment, and the child protection system in California. Section 3 describes my data, Section 4, my empirical approach, and Section 5, my results. Section 6 concludes.

2 Background and Institutional Details

2.1 Tax Policy

Families with children in California are eligible for several federal and state tax benefits, including a personal exemption for a dependent, as well as (for eligible families) the Earned Income Tax Credit (EITC), the Child Tax Credit (CTC) and the Child and Dependent Care Credit. However, households are able to claim children on their taxes for a given year only beginning in the calendar year the child was born. Note, all children are eligible to be claimed for the same total number of years. A December birth thus represents a shift in the timing of benefits, without affecting total lifetime benefits.

The Earned Income Tax Credit is a refundable tax credit offered to low-income working households.[8] The amount of the credit varies with marital status, number of children and earned income. The EITC has experienced significant expansions over time, but has been relatively stable through the period I study (1999-2020).[9] The EITC is the largest anti-poverty program for workers with children in the U.S. In tax year 2019, 2.9 million households in California (16% of all tax returns) claimed the EITC, receiving on average $2,303 (Tax Policy Center 2022a). California also has a state earned income credit, the CalEITC. CalEITC was introduced in tax year 2015, and targets a lower range of household income than does the federal EITC. In tax year 2019, 3.7 million households claimed the CalEITC, receiving an average of $192, or $440 for families with children (Franchise Tax Board, n.d.).

The Child Tax Credit has expanded in terms of eligibility, credit size and portion refundable since it was first enacted in 1997.[10] Between 1999 and 2020, the CTC grew from a $500 per child non-refundable credit to a $2,000 per child credit with up to $1,400 refundable. The most significant of these expansions occurred

[8] A refundable credit allows for negative tax liability, meaning that recipients may get cash back from the government.
[9] See Tax Policy Center (2022b) for EITC parameters over time.
[10] For an overview, see Congressional Research Service (2021).

simultaneously with a reduction in personal exemptions as part of the Tax Cuts and Jobs Act in 2017. Beginning in tax year 2019, California has offered an additional Young Child Tax Credit of up to $1,000 for low-income households with at least one child under the age of 6. While this credit was not available over the course of my data sample, it will certainly impact the value of a December birth in future years. There has been significant policy discussion around the future of child tax credits, and whether recent large but temporary expansions should be extended.

Parents claiming a child on their taxes may also receive a larger personal exemption. The personal exemption has increased over time from $2,750 per person in the household in 1999 (the start of my data) to $4,050 per person in tax year 2017.[11] In tax year 2018, the personal exemption was eliminated as part of the Tax Cuts and Jobs Act. Note, the personal exemption only serves to reduce the amount of taxable income, and is not refundable (i.e. can only reduce tax obligations to zero). This benefit may be less salient for low-income households with low tax obligations. Californians may also claim a personal exemption for dependents on their state taxes.

Finally, the Child and Dependent Care Credit provides a tax credit of up to $8,000 in tax year 2021 for child care expenses which were necessary to work or look for work.[12] This credit may be less significant for children born around the January 1 cutoff date, as child care spending in the first few weeks of life is likely small. In my calculations in the remainder of this paper, I assume zero child care expenses.

For low-income families in particular, these benefits can together make up a substantial portion of household income. Figure A1 illustrates the tax value of a December first birth for single and married filers across the income distribution in tax year 2010. For low-income households, the combined benefits can account for over 20% of after-tax income, most of which comes in the form of refundable credits.

2.2 Child Protection System

This paper focuses on the effects of income on outcomes related to child maltreatment. Broadly, the child protection system is charged with investigating allegations of child abuse and neglect, and ensuring the safety of children. While major regulation related to child welfare is set at the federal level, states may set their own policies, and vary in their approaches to child protection. In California, child welfare services are operated in a state-administered, county-implemented model. At the state level, the California Department of Social Services sets policy, oversees county programs, and operates the statewide database Child Welfare

[11] See Tax Policy Center (2022b) for historical individual income tax parameters.

[12] This credit has increased significantly over time and became fully refundable in tax year 2021, outside the range of my data.

Services/Case Management System. County agencies are in charge of implementing child welfare and foster care services.[13]

Alleged cases of abuse or neglect are brought to the attention of CPS by calls from third-party reporters to the county's Child Abuse Hotline. Some professionals (such as teachers, medical professionals, and law enforcement professionals) are required by law to report possible cases of maltreatment, and are referred to as mandated reporters.[14] Reports might also come from non-mandated reporters, such as family, friends, or neighbors of the alleged victim. Reporters make one or more allegations of abuse or neglect. Hotline screeners are tasked with deciding whether to "screen in" the call for an investigation, and, for screened-in calls, how quickly CPS should respond. In California, these decisions are made using a "Structured Decision Making" tool, essentially a guided checklist that hotline screeners complete at the time of the call.[15] Screened-in calls are assigned to a social worker for investigation. Through interviews with the child and household, the social worker determines whether each allegation of abuse or neglect is unfounded, inconclusive, or substantiated.[16] During this process, if the social worker uncovers additional risks to the child (i.e., other than the allegations included in the original referral), this may prompt a new report in the statewide database. Unfounded and inconclusive referrals are closed. If any allegation is substantiated, the family faces one of several possible outcomes, depending on the nature and severity of confirmed maltreatment. Families may receive voluntary or mandatory services or, if CPS believes that the child will not be safe in his or her home, the child may be removed and placed in foster care until a social worker determines it is safe for the child to be reunified with his/her parents.

2.3 Maltreatment and Poverty

A large body of research has established a strong correlation between poverty and child maltreatment. Of confirmed cases of maltreatment, the significant majority involve neglect.[17] While the legal definition of

[13] Reed and Karpilow (2009) provide an overview of California's child welfare system.

[14] For a complete list of mandated reporters in California, see California Penal Code Section 11165.7.

[15] See California Department of Social Services (2022) for an overview of structured decision making tools in California's child welfare system. Structured decision making was rolled out across counties in California between 1998 and 2016.

[16] California law defines each of these findings as follows: "(a) 'Unfounded report' means a report that is determined by the investigator who conducted the investigation to be false, to be inherently improbable, to involve an accidental injury, or not to constitute child abuse or neglect, as defined in Section 11165.6. (b) 'Substantiated report' means a report that is determined by the investigator who conducted the investigation to constitute child abuse or neglect, as defined in Section 11165.6, based upon evidence that makes it more likely than not that child abuse or neglect, as defined, occurred. A substantiated report shall not include a report where the investigator who conducted the investigation found the report to be false, inherently improbable, to involve an accidental injury, or to not constitute child abuse or neglect as defined in Section 11165.6. (c) 'Inconclusive report' means a report that is determined by the investigator who conducted the investigation not to be unfounded, but the findings are inconclusive and there is insufficient evidence to determine whether child abuse or neglect, as defined in Section 11165.6, has occurred." (California Penal Code Section 11165.12)

[17] In fiscal year 2019, 75% of confirmed maltreatment victims in the U.S. were neglected, 17.5% were physically abused, and 9.3% were sexually abused (Administration on Children and Families 2021).

"neglect" varies across states, in general it connotes a failure to provide necessary resources to the child.[18] As such, it is perhaps unsurprising that socioeconomic status is one of the largest predictors of involvement in the child protection system. Children in low socioeconomic-status households are three times as likely to experience abuse, and seven times as likely to experience neglect (Sedlak et al. 2010). The correlation between poverty and CPS involvement has been a source of concern for officials since the beginning of the modern-day foster care system. In 1909, at the first White House Conference on the Care of Dependent Children, the delegates resolved that "Except in unusual circumstances, the home should not be broken up for reasons of poverty, but only for considerations of inefficiency or immorality."[19] Today, agencies across the country still struggle to disentangle the two.[20]

Maltreatment and poverty may be related through many channels, including mental and physical health, substance abuse, or neighborhood effects. In this paper, I test the hypothesis that higher household income in the first year of life causes reduced maltreatment, as measured through involvement with CPS.

Identifying the causal relationship between poverty and maltreatment has long been a challenge for researchers.[21] Recent work has made progress on this question, often by exploiting county- or state-by-time variation in labor market conditions or EITC generosity, and studying maltreatment outcomes in the aggregate. Berger et al. (2017) exploit variation between states and across time in the EITC as an instrumental variable to study effects of household income on self-reported involvement with CPS, and behaviorally-approximated measures of maltreatment among households in the Fragile Families and Child Wellbeing Study. They find that an increase in income is associated with reductions in reported CPS involvement, in particular among low-income single mother families. However, the study is limited by a relatively small sample size (1,600 families observed in at most three waves), a selected group of families (the survey follows a sample of relatively disadvantaged urban children born between 1998 and 2000) and self-reports of CPS involvement. Perhaps due to these data limitations, their results tend to be suggestive or only marginally significant. Kovski et al. (2022) study the very short-term effects of EITC and CTC receipt on maltreatment reports at the state level by linking weekly tax refund data to state-level maltreatment data. They find that an additional $1,000 in per-child tax refunds leads to a 5% decrease in maltreatment

[18] For example, in California, general neglect is defined as the "negligent failure of a person having the care or custody of a child to provide adequate food, clothing, shelter, medical care, or supervision where no physical injury to the child has occurred." (Cal. Penal Code § 11165.2, subd. (b)).

[19] The meeting, called by President Theodore Roosevelt, included approximately 200 charity workers, academics, juvenile court judges and other child welfare experts, and laid the groundwork for the modern welfare system in America (*Proceedings Of The Conference On The Care Of Dependent Children held at Washington, D.C., January 25, 26 1909.* 1909).

[20] See, for example, Ketteringham (2017), Esquivel (2018).

[21] Bullinger, Lindo, and Schaller (2021) provide an overview of the challenges faced by researchers in identifying the economic determinants of maltreatment.

reports in the week of and four weeks following receipt. However, this paper does not speak to the longer term effects on CPS involvement, and cannot follow individual affected children over time. Moreover, the study does not look at foster care placements, and cannot separately identify effects by allegation or reporter category, or by child and family demographic information.

Finally, Lindo, Schaller, and Hansen (2018) study the effect of county labor market conditions on substantiated child maltreatment reports in California. They find that maltreatment decreases with indicators for male employment, but increases with indicators for female employment, suggesting that maltreatment increases when children spend relatively less time with mothers.

Other work related to income and maltreatment shows that at the state level, increases in the minimum wage are associated with reductions in reports of maltreatment (Raissian and Bullinger 2017), and that additional child support may reduce the likelihood of having a screened-in maltreatment report (Cancian, Yang, and Slack 2013).

My work contributes to this literature by leveraging 20 years of linked administrative data to study the short and longer-term effects of additional income during the very first year of a child's life on measures of child maltreatment. My data and empirical approach allow me to study effects for different subgroups, and precisely identify effects on referrals, investigations and time spent in foster care throughout the first nine years of life.

3 Data

I use data housed by the Children's Data Network, including the universes of California birth records, death records, child protection system referrals and foster care placements from 1999 through 2020.[22] Children's Data Network has access to personally identifiable information which enables linking across the different record sources. I do not have access to the underlying identified data, and so I rely on the links created by the Children's Data Network. Links between birth records and CPS uses first and last names of the child and parents/guardians, exact birthdate of the child and parents, exact address (or addresses) associated with either the child or any of his/her parents or guardians, and child gender.[23] Links between birth and death records use the same information, excluding information about decedents' parents. The linkage process allows for "fuzzy" matches across records to account for, e.g., nicknames, and spelling or data entry mistakes. Each link is given a "match probability" from 0 to 1, where higher numbers represent a greater likelihood

[22]See datanetwork.org for more information on the Children's Data Network.

[23]I allow for each unique birth ID to match with multiple CPS IDs, reflecting the fact that individuals may be accidentally entered into the system multiple times. I restrict each CPS ID to one birth link.

that the two records correspond to the same individual. I keep links with a match probability of above 0.8, and in this sample the average match probability is 99.4%.

Summary statistics at the birth record level are reported in Table 1, and presented separately for all children in the sample, for children who are linked to a CPS referral within the first three years of life (i.e., through age 2), children who are placed in foster care within the first three years of life, and children who die before age 3. The sample includes all children born in California to first-time mothers within 60 days of January 1 between November 1999 and March 2017. This is the sample of births I use in my main analysis, and allows me a three year follow up for each birth. In additional analyses I further restrict the sample in order to investigate longer-term outcomes.

A major pattern which emerges from these summary statistics is the correlation between income proxies and CPS involvement. In particular, children referred to CPS in the first three years of life are more likely to be born on MediCal, have a younger mother, and have a mother who (at the time of birth) had a high school degree or less. These correlations are exacerbated for children placed in foster care within the first three years of life.

In order to predict the average "value" of a December birth for households in my sample, I turn to the American Community Survey (ACS). The birth records do not include information on household income or estimated tax liabilities and benefits. As such, I use data from the ACS 1-year public use microdata sample for California from 2000 through 2019 to estimate the relationship between household characteristics observed in the birth records and household income.[24] Details on this estimation strategy are included in Section 4.1. Summary statistics for predicted after-tax income and tax value of births are included in Table 1. Although the predicted value of a December birth is similar across sub-samples, predicted household income is much lower for the sample of births who are referred to CPS, and the sample of births who are placed in foster care.

Summary statistics from CPS at the child-referral level are reported in Table 2. The sample includes children aged 0 to 2 on referrals made between January 1999 and December 2020.[25] Each referral may involve multiple children, and multiple allegations. Moreover, each child may be referred multiple times. 60% of referrals include an allegation of neglect, making this the most common allegation category. While the decision to investigate happens at the referral level, allegations and dispositions (i.e., substantiated, inconclusive or unfounded) occur at the child-referral level. Finally, foster care is not necessarily associated

[24] These data were downloaded from IPUMS.

[25] This includes children who are not in my main analysis sample, i.e. non-first born children, children born outside of California, children born outside a 60 day bandwidth around January 1, and children born outside of the date range of the main sample. This expanded sample provides a more complete picture of CPS in California.

with a given referral, and occurs at the child level. 77% of child-referrals involving children under the age of three are investigated, and 21% of child-referrals have any substantiated allegation. 12% of child-referrals are associated with a foster care placement within three months of the referral date.

4 Empirical Approach

To identify the effect of additional household income during the first year of a child's life, I exploit a discontinuity in tax benefits around the January 1 birth date.[26] This discontinuity represents a shift in the timing of benefits, where families with children born in December are eligible for benefits one year earlier, and lose eligibility one year earlier, than families with children born in January. This sharp increase in tax benefits motivates a regression discontinuity design. In particular, I estimate the equation:

$$Y_{it} = \alpha + \beta_0 D_i + \beta_1 Z_i + \beta_2 (D_i \times Z_i) + \theta_t + \epsilon \tag{1}$$

Where, Y_{it} is an outcome of interest for child i born in recentered birth year t, Z_i is equal to the difference between January 1 and child i's birth date ($Z_i \in [-60, -8] \cup [8, 60]$), D_i is an indicator variable equal to one if child i is born after December 31 and zero otherwise, and θ_t is a fixed effect for recentered birth year t.[27] Observations are weighted using a triangular kernel, meaning that observations closer to the cutoff receive a higher weight.

The coefficient of interest, β_1, is an intent-to-treat parameter which identifies the average effect of eligibility for child-related tax benefits among households in the sample. I do not observe the realized change in income for families with a December birth, and as such cannot precisely estimate the effect of income on outcome Y_{it}. Instead, I estimate Equation 1 separately for samples which are more and less likely to receive a substantial portion of their household income from child-related tax benefits. My main results estimate effects separately for births to households with a predicted income below 200% of the poverty line for a family of three (low-income households), and births to households with a predicted income above 200% of

[26] Previous work which uses this cutoff can be divided into two categories. The first category estimates the effects of these tax incentives on birth timing manipulation, and effects of that manipulation. LaLumia, Sallee, and Turner (2015) finds small effects on manipulation of birth timing, but substantial effects on income reporting for self-employed parents. Schulkind and Shapiro (2014) study health effects of birth timing manipulation, looking in particular at the effect of C-section scheduling around January 1. The second category of papers exploits the income shock to study longer-term effects on parent and child outcomes. This strand of the literature shows that an increase in resources during infancy causes a decrease in time to second birth for first-time mothers (Meckel 2015), decreases in maternal labor supply during the first year of a child's life (Wingender and LaLumia 2017), improvements in children's educational outcomes (Cole 2021) and labor market outcomes (Barr, Eggleston, and Smith 2022).

[27] I define a recentered year around January 1, so that December 2009, for example, falls in recentered year 2010.

the poverty line.[28] Additional analyses test the robustness of my results to alternative proxies for poverty. In my main analyses I limit my sample to first births. For higher-order births, any benefits of the increased income may be split between multiple children.

To scale these reduced-form results, I estimate the average value of a December birth for the relevant samples using the ACS.

4.1 Estimating the Tax Value of Birth

In order to estimate the effect of birth date on household income for families in my sample, I turn to the ACS Public Use Microdata Sample, which contains variables observed in the birth records, as well as information on income. I first organize the dataset into tax units.[29] I include in my prediction sample all tax units with a child under the age of 3.[30] I then treat the youngest child as if he is an infant, and use NBER's TAXSIM to calculate tax obligations both with and without declaring the child.[31] For each family, I create two variables of interest: (1) After-tax income is equal to the sum of the household's wage and salary earnings minus the sum of calculated federal and state income taxes, when the infant *is* declared; (2) the dollar value of a December birth is equal to the difference between the sum of federal and state income taxes with and without declaring the child. To the extent that not all families take up tax benefits for which they are eligible, this is an over-estimate of the realized tax value, making all scaled results an underestimate of the true effects per $1,000.[32]

Next, I create variables in the ACS data which I also observe in my main dataset. I create these variables in reference to the youngest child in the household, i.e., the child whose "tax value" I aim to estimate. I include the child order, mother's and father's age at the time of the child's birth, mother's and father's race and ethnicity, mother's and father's education, child's year of birth, and Public Use Microdata Area.[33] I

[28] I define the poverty line at the 2017 level of $20,420 for a family of three.

[29] Tax units are defined at the subfamily level, and exclude all people living in group quarters. That is, a tax unit is defined by (1) the ACS sample year, (2) the household, (3) the family unit within the household, and (4) the subfamily unit within the family unit. I use IPUMS variables RELATE and SFRELATE to identify relationships within the tax unit, relative to the head of household (for single-family units) or the reference person (within a subfamily). In particular I identify the household head, the spouse (if any) and dependent children (under the age of 18 only). I define the household head as the tax filer for the family.

[30] Although parents with a two-year-old child may differ than parents with an infant, the increased number of observations allows me greater power.

[31] See Feenberg and Coutts (1993). The latest version of TAXSIM can can be accessed at the following site: taxsim.nber.org. I set the state to California and the year to the child's year of birth. Marital or filing status is set to married filing jointly for tax units where the household head is married with a spouse present, and single for tax units where the head of household is unmarried, or separated. Personal income of the primary taxpayer and spouse are identified through the INCEARN variable, which includes wage and salary income, as well as self-employment business and farm income. All other income, transfers and deductions are set to zero.

[32] In California, in tax year 2017, it's estimated that 73% of families who were eligible for the EITC claimed the credit. See Tax Policy Center (2022a) for take up years in other tax years and states.

[33] Child order is determined by the number of siblings the youngest child has living in the same household, and is top-coded

split the sample into a training and validation set (75/25), and run a linear regression on the training sample:

$$Y_i = \beta X_{it} + \lambda X_{it} X_{it} \tag{2}$$

Where Y_i is set to one of two outcome variables of interest: (1) after-tax income, (2) dollar value of a December birth. X_{it} is a vector of indicator variables describing child order, mother's and father's age at the time of the child's birth, mother's and father's race and ethnicity, mother's and father's education, year of birth, and Public Use Microdata Area, and is included with one level of interaction.[34] Observations are weighted using the household weights provided in the ACS.

Figure A2 shows scatter plots of out-of-sample predictions compared to true values, and reports out-of-sample R-squared statistics for each of the outcomes of interest. For after-tax income, the out-of-sample R-squared is 0.557; for dollar value the R-squared is 0.399.

Finally, I apply the coefficients to the birth records in order to predict nominal after-tax household income as well as the tax value for each birth. I then adjust these values to 2017 dollars using the CPI for the relevant recentered year. That is, I use the 2010 CPI for both December 2009 and January 2010 births. I define a birth as "low income" if the predicted aftertax household income, adjusted to 2017 $, is below 200% of the 2017 poverty line for a family of three ($20,420 ×2=$40,840).

4.2 Validity

This regression discontinuity design relies on the assumption that date of birth is as good as random around January 1. This assumption would be violated, for example, if parents manipulate birth timing to take advantage of the tax benefits, or if doctors have preferences against delivering on major U.S. holidays. Indeed, a visual examination of birth patterns around the end of the year is indicative of birth timing manipulation (see Figure 1). As such, I follow previous work in estimating a donut hole RD, dropping observations within eight days of the January 1 cutoff.[35] In robustness tests, I estimate results using different donuts, as well as

at three. Note, this is not exactly the same as child order from the birth record, in which previous births by the same mother are recorded. I bin parents' ages as follows: (1) < 23, (2) 23-27, (3) 28-33, (4) 34-39, (5) 40+, (6) missing. I include the following primary race/ethnicity categories: (1) White, (2) Black/African American, (3) American Indian/Alaska Native, (4) Asian/Pacific Islander, (5) Hispanic/Latino and (6) missing. I include the following educational attainment categories: (1) < 12th grade, (2) exactly 12th grade, (3) 1-3 years of college, (4) 4+ years of college, (5) missing. To the extent that parents have children before completing their education, this variable might overstate educational attainment by up to three years. Year of birth is set equal to the year of the interview minus the age of the youngest child. Public Use Microdata Area is a census-defined geography including at least 100,000 people, and is only available for ACS years 2005 on. To the extent that families move after the birth of a child, Public Use Microdata Area in the ACS may differ from Public Use Microdata Area in the birth record.

[34]The sample size restricts me from using a more fully-saturated model. Moreover, linear regression with limited interactions slightly out-performs LASSO in out-of-sample predictions, in terms of both mean squared error and R-squared.

[35]I follow Barr, Eggleston, and Smith (2022), who also use a donut of 8 days around January 1. Cole (2021) uses a data-driven approach to determine the appropriate donut size and uses an unbalanced omitted region of 20 days before January 1. and 9

different bandwidths.

I also directly test the assumption that children born on either side of the cutoff are similar on observable characteristics. In particular, I estimate Equation 1, where Y_{it} are characteristics of child i, birth/delivery characteristics for child i, and characteristics of child i's parents as listed on the birth record.

Finally, recall that birth records are probabilistically matched to CPS records. My identification strategy may be threatened if January births were better able to be matched than December births. For example, if improvements in birth recording coincided with the new calendar year, this could in theory discontinuously improve the likelihood that births are correctly matched to CPS referrals. To test for this issue, I again estimate Equation 1, where Y_{it} is the match probability (from 0 to 100 percent) for child i born in year t who is matched to a CPS referral.

The results of these validity tests are shown in Tables 3 and 4. Births are balanced on a large set of characteristics. Out of 22 regressions two are significant at the 10% level, which is statistically expected. Specifically, the probability of having "missing" maternal characteristics (age, race or education) shows a 3.5% increase (significant at the 10% level) around the threshold. This could indicate that some change in the birth recording process that coincides with the change in calendar year. This would be a problem if it affected the likelihood that a birth would be accurately matched to a CPS record. However, the match probability is balanced around the calendar year cutoff, mitigating this concern. The probability of being low-income shows a 1.2% increase (significant at the 10% level) around the threshold. I show robustness to several other definitions of poverty, which do not experience the same marginally significant discontinuity.

5 Results

5.1 Child Maltreatment

Results of estimating Eq. 1 on outcomes indicating various levels of involvement with CPS are reported in Table 5. In Panel A, Y_{it} is an indicator variable equal to one if the child has any referrals to CPS through age 2. In Panel B, Y_{it} is an indicator variable equal to one if the child has any *investigated* referrals to CPS through age 2. In Panel C, Y_{it} is an indicator variable equal to one if the child has any foster care placements through age 2. Results are reported separately for all first children (Column 1), first children born to households with predicted income below 200% of the federal poverty line (Column 2), and first children born to households with predicted income above 200% of the federal poverty line (Column 3).

days after. I show robustness of my main results to this and other donut sizes.

Average predicted tax value of a December birth in dollars and annual after-tax household income for a given sample are reported at the bottom of each column. The average tax value is larger for births to low-income households ($2,881) relative to births to higher-income households ($2,347) both in absolute value and as a share of baseline income. The average predicted after-tax household income in my sample is approximately $62,000, and for low-income households it is around $24,000.

For each outcome considered, effects of tax benefit eligibility are statistically insignificant in the full sample. However, for children born to low-income households, being born before January 1 decreases the likelihood of involvement in CPS at every stage of the system. Among this group of children, a December birth decreases the probability of any referral to CPS through age 2 by 0.58 percentage points, or 4.2% of the sample mean. The implied effect per $1,000 difference is a 0.20 percentage point decrease in the likelihood of referral, or 1.5% of the sample mean. This effect is significant at the 10% level, and the corresponding regression discontinuity plot is shown in Figure 2a. Effects for children born to higher-income households (whose average household income is $88,000) are close to zero and statistically insignificant.

Turning to Panel B, I find similar patterns for likelihood of investigation by CPS through age 2. In the full sample and the higher-income sample, effects are small and insignificant. For births to low-income households, being born before January 1 decreases the probability of being investigated by 0.55 percentage points, or 4.6% of the sample mean, implying a 0.19 percentage point (1.6%) effect per $1,000. This estimate is statistically significant at the 10% level, and the corresponding regression discontinuity plot is shown in Figure 2c.

In Panel C, these patterns persist. Again, effects are small and insignificant in both the full sample and the higher-income sample. For births to low-income households, being born before January 1 decreases the likelihood of placement in foster care through age 2 by 0.26 percentage points, or 13% of the sample mean, implying a .09 percentage point (4.4%) effect per $1,000. This estimate is significant at the 5% level, and the corresponding regression discontinuity plot is shown in Figure 2e.

Next, Panels D through F of Table 5 report results of estimating Eq. 1 on the number of CPS referrals through age 2, the number of investigated referrals through age 2, and days spent in foster care through age 2. Again, effects are shown separately for the full sample of first-born children (Column 1), first-born children to low-income households (Column 2), and first-born children to higher-income households (Column 3). In the full sample, children born before January 1 receive 0.0073 fewer referrals to CPS through age 2, or about 5.1% of the sample mean. They also undergo .0066 fewer investigations (6.1% of the sample mean) and spend 0.6 fewer days in foster care (14% of the sample mean). Each of these effects is significant

at the 5% level. As shown by the difference between Columns 2 and 3, these effects are driven entirely by births to low-income households. For these births, being born before January 1 decreases the number of referrals by 0.019, or 7.6% of the sample mean, corresponding to a decrease of 0.0066 referrals (2.6%) per $1,000. Similarly, being born after January 1 decreases the number of investigations by 0.017, or 8.9% of the sample mean, corresponding to a decrease of 0.0059 (3.1%) investigations per $1,000. Finally, the effect on days spent in foster care through age 2 is a decrease of 1.88 days, or 23% of the sample mean, corresponding to a 0.65 day (7.9%) decrease per $1,000. Each of these estimates is significant at the 1% level. The corresponding regression discontinuity plots are shown in Figures 2b, 2d, and 2f. Effects for non-firstborn children are shown in Figure A3. In line with lower average tax benefits for non-firstborn children and more family members among whom additional income may be split, effects are in general attenuated and statistically insignificant for non-firstborn children.

These results are most consistent with a reduction in maltreatment, as opposed to reporting rates. Effects are largest and most significant for days spent in foster care, the outcome which most indicates severe maltreatment. This indicates two possible patterns: (1) the marginal children diverted from CPS would have had higher-than-average rates of foster care placements, and/or (2) eligibility for tax benefits improves some childrens' well-being enough to keep them out of foster care, but not enough to keep them from being referred. To explain reductions in placements purely through reductions in referrals, it would have to be the case that the diverted children would have been three times as likely to be placed in foster care as the average child, had they been referred.[36] In order for my results to be explained purely through reporting effects, additional income affects must affect reporting primarily for higher-severity cases of maltreatment.

Observed effects are driven entirely by births to households with predicted income below 200% of the poverty line. Next, I take a closer look at where in the income distribution effects of benefit eligibility is greatest. If there are differences in the size of the effect across income, this may help policymakers target additional benefits where they are likely to be most effective. In particular, I estimate Eq. 1 for four groups: households with predicted income below 100% of the poverty line (very low income), between 100-200% of the poverty line (low income), between 200-400% of the poverty line, and above 400% of the poverty line. Coefficients and standard errors are reported visually in Figure 3, and regression results are reported in Table A3. Each column represents a sub-sample of the income distribution, and each panel a different outcome. Effects on referrals and investigations are largest and significant only for households with predicted income

[36] Among first-born low-income children referred to CPS through age 2, approximately 15% are placed in foster care at some point through age 2. If diverted children had average rates of foster care placements, we would expect 0.00579 x 0.15 = 0.00087 reduction in likelihood of foster care placement through age 2. Instead, we observe a reduction of 0.00255.

between 100-200% of the poverty line. There are several possible explanations for this pattern. First, very-low-income households are eligible for fewer benefits on average then the low-income households ($2,423 vs. $3,079). The average effects per $1,000 are thus closer in magnitude (0.0059 referrals per $1,000 for very-low-income households vs. 0.0071 referrals per $1,000 for low-income households). Moreover, if there are differences in benefit take-up by income, and in particular if very-low-income households are less likely to take up benefits for which they are eligible, this might also attenuate observed results for this sample. Second, a one-time increase in household income might be more impactful for families who are more stable. Very-low-income households may need additional help or services to see the same benefit.

Interestingly, the pattern is reversed for foster care placements. That is, effects are larger (in magnitude, although not percentage terms) for very-low-income households. This may be because foster care placements are substantially more common for very-low-income households, and suggests that although additional income may not reduce investigations for this group, it does reduce the number or length of foster care stays. These patterns are only suggestive, as coefficients are in general not statistically distinguishable between the very-low-income and the low-income samples. Effects for households with predicted incomes above 200% of the poverty line are small in magnitude and statistically insignificant for each sub-sample and outcome.

To study the persistence of these effects over the course of childhood, I use a sub-sample of birth cohorts from recentered years 1999 through 2011, for whom I observe 9 years of follow-up (i.e. through age 8). I separately estimate Eq. 1 for three age ranges (0-2, 3-5 and 6-8) and for each of three outcomes: number of referrals, number of investigations, and days spent in foster care. The coefficients and 95% confidence intervals for these separate regressions are shown in Figure 4, and regression results are reported in Table A5. For each outcome of interest, effects on CPS involvement for children born to low-income households persist through age 8, while effects for children born to higher-income households are small in magnitude and statistically insignificant over the course of childhood. This persistence may reflect the fact that involvement in CPS is often chronic, and early involvement may increase the likelihood of long-term involvement. Alternatively, short-term increases in household income may have long-run effects on family functioning. Previous research which exploits this discontinuity has found long-term effects on children's educational and labor market outcomes, so it is not surprising to find persistent effects in my context (Barr, Eggleston, and Smith 2022; Cole 2021).

Overall, these results imply that being born before January 1 decreases child maltreatment both at the extensive and intensive margins for children born to low-income households, and that these effects persist throughout childhood. Next, I test the robustness of my main results to alternative proxies for low-income

status. Ideally, I would observe for each household the exact tax value of a December birth. Instead, I report effects for several samples for whom I expect the value of a December birth to be large as a share of total household income. In particular, I estimate effects for (1) births to low-education mothers (with a high school education or less); (2) births which were paid for by MediCal; (3) births where at least some information about the father is missing, and (4) births to young mothers (age 24 or less). Table A4, Panel A reports results of estimating Eq. 1 on these four subsamples, where Y_{it} is equal to the number of referrals through age 2. In Column (1), effects for births to mothers with a high school degree or less are consistently negative and significant, although slightly attenuated relative to my main results. This may be because average income is substantially higher for this group ($34,000 vs. $24,000). Similarly, in Column (2), effects for births paid for by MediCal are statistically indistinguishable from effects for my main low-income sample, but again are slightly attenuated. In Column (3), I limit the sample to birth records where at least some information about the father (age, race or education) is missing. Although birth records in California do not report the marriage status of parents, missing paternal information is likely indicative of less involvement between the parents. This is a relatively small sample of the data, but again effects are statistically indistinguishable from my main results. Finally, maternal age at birth is highly correlated with predicted income. Column (4) presents results for births to mothers below age 24, which are again statistically significant and similar in magnitude to my main results.

Next, I test effects separately for U.S.-born vs. foreign-born mothers. Foreign-born mothers are less likely to be U.S. citizens, and as such less likely to be eligible for the relevant tax benefits. For this group, we would not expect a December birth to affect CPS outcomes. Results of estimating Eq. 1 separately for births to low-income foreign-born and native-born mothers are shown in Table A6. Effects are driven entirely by births to native-born mothers. For this sample, tax benefit eligibility results in 0.029 fewer referrals (8.3% of the sample mean), 0.026 fewer investigations (9.6% of the sample mean) and 2.5 fewer days spent in foster care (20.9% of the sample mean) through age 2. Effects for births to foreign-born mothers are close to and statistically indistinguishable from zero. Average CPS involvement is also lower among this group, possibly indicating that these children are more likely to move out of state soon after birth.

For the remainder of the paper, I focus on results for households with predicted income below 200% of the poverty line.

5.2 Race and Gender Heterogeneity

Racial disparities in CPS involvement are a primary concern for agencies and the public.[37] In my sample, while only 5.8% of children are born to Black mothers, 13.5% of children referred to CPS through age 2 and 16.3% of children placed in foster care through age 2 are born to Black mothers (see Table 1). This racial disproportionality may be a result of (1) disparities in underlying drivers of maltreatment such as poverty, or (2) racial bias in CPS referrals, investigation and foster care placement decisions. If disparities are primarily a result of differences in household income, effects of additional income on CPS involvement should be similar across race.

In Table 6 I report results from estimating Eq. 1 separately for births to low-income households with White mothers, Black mothers, Hispanic mothers and Asian/Pacific Islander mothers.[38] Panel A reports results on number of referrals, Panel B on number of investigations and Panel C on days spent in foster care through age 2. Estimated effects on number of referrals and investigations are largest and most significant for births to White mothers. Among this group, eligibility for tax benefits reduces the number of referrals by 0.069 (14.5% of the sample mean) and number of investigations by 0.055 (16.4% of the sample mean). Effects on referrals and investigations for children born to Black mothers are an order of magnitude smaller, and statistically indistinguishable from zero. However, eligibility for tax benefits does reduce days spent in foster care by 8.0 days (40% of the sample mean) for children born to Black mothers. This suggests that bias may play a role in third-party reporting decisions, but that additional income does indeed improve child outcomes in Black families. Effects among children born to Hispanic mothers are attenuated, consistent with the lower level of citizenship and thus eligibility for tax benefits among this group. Effects among children born to Asian and Pacific Islander mothers are small and statistically insignificant.

Next, motivated by previous work which shows larger effects of household resources for boys, I estimate effects separately by child gender.[39] Table 7 reports results from estimating Eq. 1 separately for boys and girls born to low-income households, where Y_{it} is again equal to either the number of referrals through age 2 (Panel A), the number of investigations through age 2 (Panel B) or days spent in foster care through age 2 (Panel C). For boys, being born before January 1 decreases the number of referrals to CPS by 0.0278, 11% of the sample mean and corresponding to a 0.0096 (3.8%) decrease per $1,000. This estimate is significant at the 1% level. For girls born to low-income households, effects are smaller in magnitude and statistically

[37] See, for example, Gateway (2021) and Thomas and Halbert (2021).

[38] See notes to Table 1 for a description of the race and ethnicity variables used.

[39] Barr, Eggleston, and Smith (2022) and Dahl and Lochner (2012) each find that EITC-driven increases in household income has larger long-term effects for boys.

indistinguishable from zero. This gendered pattern of effects persists throughout the system.

These results are in line with a literature that explores gender heterogeneity in effects of the early childhood environment. For example, Autor et al. (2019) find that family disadvantage has larger effects for boys in terms of disciplinary problems and educational outcomes. Bertrand and Pan (2013) show that the gender gap in child behavioral issues is over twice as high for children raised by single mothers, relative to children raised by two biological parents. They provide evidence that this gap is primarily driven by gender differences in returns to parental inputs (rather than differences in parental inputs by child gender). Outside of economics, a large interdisciplinary literature provides evidence for the greater vulnerability of boys relative to girls, particularly as infants (see, for example, Golding and Fitzgerald (2017) and Schore (2017)). Interestingly, this "frail male" hypothesis may help explain the natural sex ratio at birth, which favors boys (Schacht, Tharp, and Smith 2019). The gender gap in vulnerability is observed in my data, where 51% of births are male, but 58% of deaths before age three are male (see Table 1). An alternative explanation for the gender heterogeneity in my results is discrimination, where parents may invest available resources more in boys than in girls.[40]

The differences in effects across race and gender provide additional support for the validity of the research design. That is, my results cannot be driven by some underlying data characteristic which causes a uniform discontinuous shift in likelihood of match across calendar years. More precisely, any unobserved characteristic which both drives CPS involvement and differs discontinuously across the January 1 cutoff must also differ by race and gender.

Due to the substantial differences in effects by gender, for the remainder of my results I estimate effects separately for boys and girls.

5.3 Allegation and Reporter Categories

Next, I explore heterogeneity in effects by allegation and reporter category, to gain insight into the mechanisms for my main results. There are several avenues through which additional household income may affect CPS involvement. First, additional income may allow parents to provide necessary or otherwise-beneficial resources for the child. In this case, we might expect substantial effects among allegations of neglect in particular. Second, the additional income may allow parents to take more time away from work to spend with their child. If this increases the time that parents spend with their child, and decreases the time that a child spends with a potentially unreliable babysitter, we might expect fewer accidents and potentially fewer

[40] Previous work has found parental gender discrimination in both the U.S. and developing contexts. See, for example, Dahl and Moretti (2008) and Bharadwaj and Lakdawala (2013).

reports of physical abuse and neglect.[41]

Third, previous theoretical and empirical work has documented a link between household income and domestic violence, and found in different contexts that increases in household income may either decrease (e.g. Aizer (2010) and Cesur et al. (2022)) or increase (e.g. Carr and Packham (2021)) intimate partner violence.[42] If child-related tax benefits affects maltreatment through this channel, we would expect to see changes in emotional abuse (largely an indicator for domestic violence) and reports from law enforcement.

Finally, one might worry that the results may reflect changes in reporting rates. Given my results on foster care placements, it would have to be true that effects of income on reporting rates are strongest for the most severe cases of maltreatment. Moreover, any heterogeneity across allegation and reporter category would have to be explained by differences in how additional income changes the visibility of maltreatment to potential reporters.

Aiming to distinguish between these various channels, I first estimate Eq. 1 separately for each major allegation category (as defined in Table 2). That is, I set Y_{it} equal to the number of referrals through age 2 involving an allegation of each of neglect, physical abuse, emotional abuse, and "other" maltreatment allegations. I separately estimate effects for boys and girls. Figure 5 shows coefficients and 95% confidence intervals from each of these regressions, reported in Table A7. For male births to low-income households, being born before January 1 significantly decreases allegations of neglect (0.022, or 13% of the sample mean) and physical abuse (0.0107, or 24% of the sample mean). Each of these effects is statistically significant at the 1% level. For girls born to low-income households, effects are statistically insignificant. However, the coefficient on neglect is negative, marginally insignificant, and would indicate a reduction of 6% relative to the sample mean.

I conduct a similar heterogeneity analysis by reporter category, estimating Eq. 1 separately for non-mandated reporters, school workers, law enforcement professionals, medical professionals, CPS workers and other reporters. (See Table 2 for a description of each major reporter category.) Figure 6 shows coefficients and 95% confidence intervals for each of these separate regressions, reported in Table A8. For male births to low-income households, being born before January 1 significantly decreases referrals through age 2 by medical professionals (0.011, or 23% of the sample mean), and CPS staff (0.0035, or 20% of the sample mean). Note, typically referrals from CPS staff indicate additional allegations discovered on an already-open

[41] Wingender and LaLumia (2017) finds that mothers whose children are born in December as opposed to January have lower probability of working during the first year of a child's life. Lindo, Schaller, and Hansen (2018) suggests that maltreatment may increase when mothers are working, and children are spending relatively more time with other caregivers.

[42] Additional studies outside of the U.S. context also find that household income may affect intimate partner violence in different ways. See, for example, Hidrobo, Peterman, and Heise (2016), Eswaran and Malhotra (2011), and Erten and Keskin (2021).

case or investigation. For girls, effects are insignificant for each of the reporter categories.

Interpreting this pattern of results through the first mechanism described above (additional income allows parents to provide additional resources to the child), medical providers may notice differences in infant health caused by, for example, better nutrition, or better living conditions.[43] Gender differences here could be explained by either discrimination or the higher vulnerability of boys, as discussed above.

The effects on allegations of physical abuse are consistent with the second avenue discussed above (additional income allows increased time off work, spent with the child). If children are less likely to spend time alone or in the care of unreliable babysitters, they may be less likely to have accidents and incur physical injuries which spark referrals to CPS. Again, the well-documented increased likelihood of injury for boys could explain the gender heterogeneity in effects.

The third hypothetical mechanism discussed above posits a link between the earlier receipt of child-related tax benefits and incidents of domestic violence. However, I find no effects on either allegations of emotional abuse (indicative of domestic abuse) or reports from law enforcement.

Finally, in order to interpret these results as evidence that additional income affects reporting rates, it would need to be the case that income only reduces reporting of neglect and physical abuse, and only from medical professionals and CPS caseworkers. For reports from medical professionals, this would indicate either that parents with additional income take kids to the doctor less, take kids to a different doctor, or are differently able to convince the doctor about the child's safety and well-being. Moreover, they would have to differentially be able to engage in this behavior by child gender, and in particular the additional income would have to only reduce visibility of maltreatment for boys. This is not consistent with the literature discussed above showing boys are more prone to injuries and accidents. Reports from CPS staff mostly indicate additional allegations coming to light on an already-open case. In this case, it would likely be difficult for parents to conceal maltreatment from a caseworker who is specifically investigating the household for issues which could impact child safety or well-being. Again, it is not clear why income would have a differential impact by child gender, or why it would only impact allegations of neglect and physical abuse. As such, it seems unlikely that concealment of maltreatment is the primary mechanism driving reductions in CPS involvement. Overall, there is substantial evidence that my results reflect true changes in incidence of maltreatment.

[43] Gendre et al. (2021) finds that children whose parents received the Australian Baby Bonus were less likely to be brought to the hospital with a respiratory infection. The authors link the difference in infant health to increased heating expenditures during winter months by parents who received the cash.

5.4 Robustness

I use five methods to test the robustness of my main results. First, I test robustness to alternative bandwidth and donut choices, estimating Eq. 1 on the sample of first births to low-income households, setting Y_{it} is equal to the number of referrals through age 2 and alternating the bandwidth and donut size.[44] Figure 7 shows regression discontinuity coefficients and 95% confidence intervals for each of these separate regressions, with a vertical red line in each plot indicating the effect size using my main specification. The effect is relatively stable across bandwidth and donut size, and in particular the estimate from my main specification falls within the 95% confidence interval of each alternative specification. Specifications with a 30 day bandwidth result in generally noisier effects, as expected with fewer observations, and specifications with no omitted region show some attenuation, in line with some amount of birth manipulation directly around the January 1 cutoff.

Next, I estimate effects separately excluding each recentered year, to test whether results are driven by any one year, or are generally consistent across years. Coefficients and 95% confidence intervals for 18 separate regressions (one for each excluded recentered year) are shown in Figure 8. The outcome variable is number of referrals through age 2, and the sample is births to low-income households. Estimated effects are consistent, and consistently significant, across excluded years. Moreover, the estimated coefficient in my main result (indicated by a red horizontal line) falls within the 95% confidence interval of every year-by-year coefficient.

I also run several placebo tests. First I estimate effects of being born before other holidays, where there are no tax benefits associated with birthdate on either side of the cutoff. That is, I estimate Eq. 1 for each federal holiday, setting Y_{it} equal to the number of referrals through age 2 and Z_i equal to the difference between child i's birth date and the holiday date ($Z_i \in [-60, -8] \cup [8, 60]$). Figure 9 shows estimated coefficients and 95% confidence intervals for each of these ten separate regressions. The estimated effect for January 1 is largest in magnitude and is the only effect statistically significant at the five percent level. This test rules out the possibility that effects are driven by medical staffing changes around holidays, or parents' ability to take time off work and bond with newborn children.

In my next placebo test, I estimate effects of being born before January 1 for households where I expect the effect of additional income on CPS involvement to be small (i.e., households that I expect to be higher-income or have a low tax value of birth). Specifically, I estimate Eq. 1 for first births to mothers with at least

[44] I set the bandwidth to each of: [-30,30], [-45,45], [-60,60] and [-90,90]. I set the omitted region to each of: [0,0], [-8,8], [-10,10], [-14, 14] and [-20,9]. In similar settings, Barr, Eggleston, and Smith (2022) use a bandwidth of [-30,30] and a donut of [-8,8], and Cole (2021) uses a bandwidth of [-60,60] and a donut of [-20,9].

some college education, first births which were *not* paid for by MediCal, first births where all information about the father is present, and first births to mothers who are aged 24 or above. Results of these regressions are reported in Panel B of Table A4. The average predicted household income for these four groups ranges between $68,061 (for birth records with all information about the father is present) and $86,843 (for first births to mothers with at least some college). For each of these samples, effects of being born before January 1 on referrals to CPS are small and statistically insignificant. Recall also that effects among births to mothers who are less likely to be eligible for benefits are statistically indistinguishable from zero (Table A6 reports effects for foreign-born mothers).

Finally, I test effects of being born before January 1 on CPS referrals, investigations and foster care placements in the first 60 days of life. Recall, tax benefits are not distributed until March at the earliest, and so any effects associated with those tax benefits should not exist in the first two months of life. Significant effects would indicate that there is another unobserved variable which differs discontinuously around the January 1 birthdate cutoff and drives involvement with CPS. Results are shown in Table A9. Effects are statistically indistinguishable from zero.

6 Conclusion

I study the effects of additional income during the first year of life on child maltreatment, a first-order measure of child well-being. I leverage a discontinuity in child-related tax benefits to show that an extra $1,000 in infancy reduces child maltreatment for at least nine years. These effects are driven by reports from medical professionals and allegations of neglect and physical abuse, suggesting that doctors notice differences in infant health caused by increased family resources.

These results have important policy implications. They provide evidence that the link between household income and maltreatment is causal, and that cash transfers during early childhood may be a fruitful strategy for reducing maltreatment. These additional benefits should be included in calculations of the welfare effects of, e.g., expanding the EITC, Child Tax Credit, and other benefits provided to families of young children.

A back-of-the-envelope calculation suggests that increasing payments to families during the first year of a child's life would pay for itself in terms of reduced long-term maltreatment costs. Estimates of the per-victim lifetime cost of maltreatment range from $210,000 (2010 USD) (Fang et al. 2012) to $830,000 (2015 USD) (Peterson, Florence, and Klevens 2018). These estimates include direct child welfare costs as well as productivity losses, healthcare, criminal justice and education costs. Conservatively defining a "new"

maltreatment victim as an additional child placed in foster care through age 8, an additional $1,000 to low-income families would reduce the rate of maltreatment by 260 per 100,000 children, resulting in $614 - $2,236 of benefits per $1,000 spent.[45] A less conservative definition of a new maltreatment victim is an additional child investigated through age 8. Using this definition, an additional $1,000 to low-income families would reduce the rate of maltreatment by 554 per 100,000 children, resulting in $1,307 - $4,764 in long-term benefits per $1,000 spent.[46]

[45] As reported in Table A5 column (4), the effect of eligibility for tax benefits on any foster care placement through age 8 is -.00738 (significant at the 1% level), or -.00260 per $1,000. Multiplying this number by $236,000 and $860,000 (2017 USD) gives the lower and upper bound estimate, respectively.

[46] As reported in Table A5 column (4), the effect of eligibility for tax benefits on any investigation through age 8 is -.0157 (significant at the 1% level), or -.00554 per $1,000. Multiplying this number by $236,000 and $860,000 (2017 USD) gives the lower and upper bound estimate, respectively.

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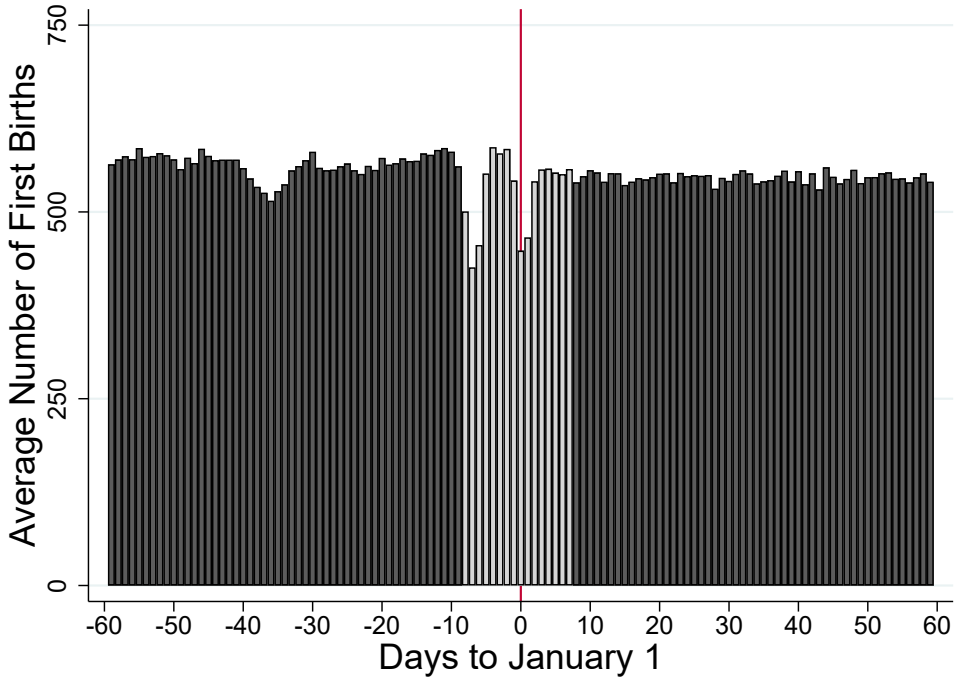
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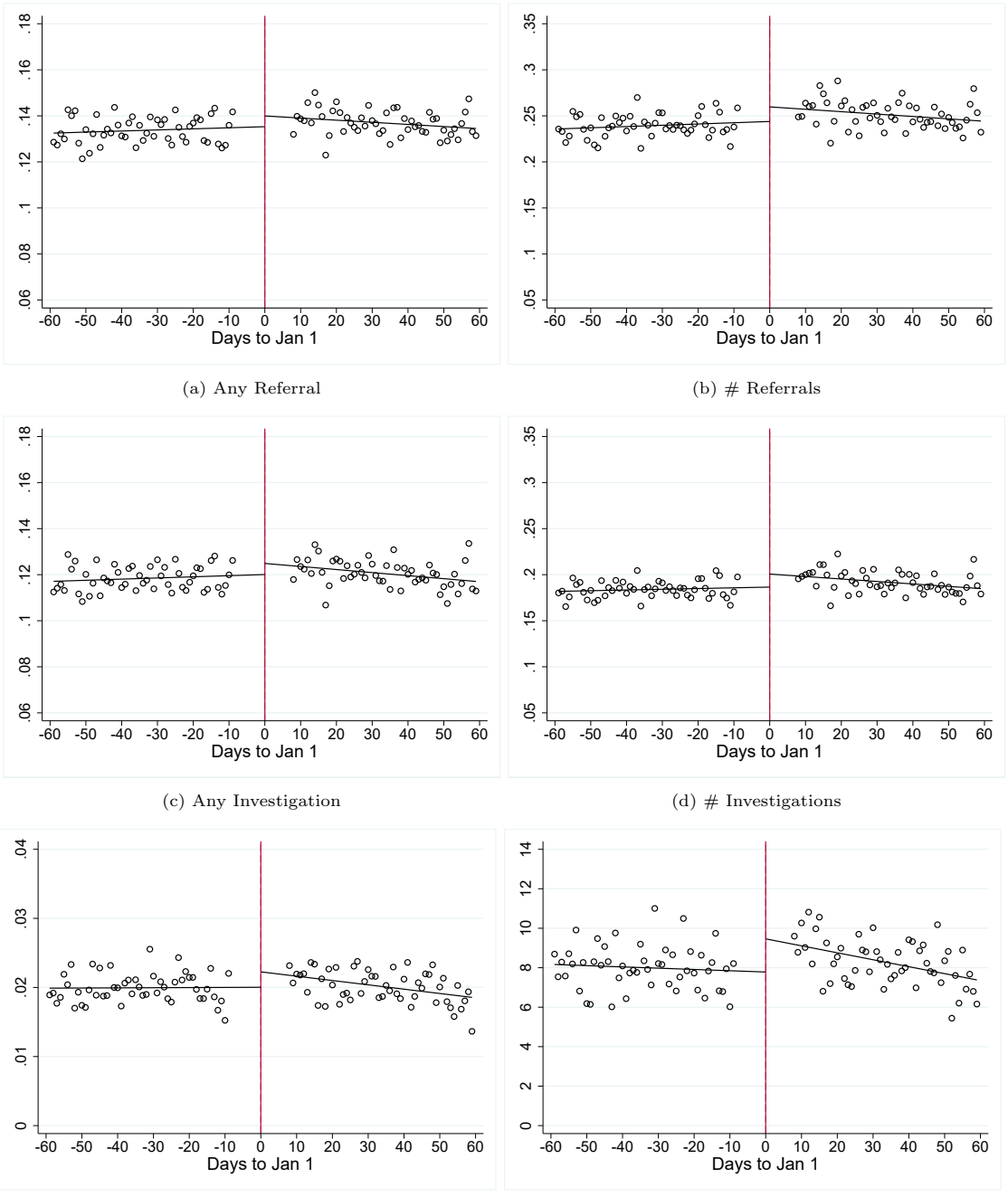
Figures

Figure 1: Birth Patterns Around January 1



This figure plots the average number of births to first-time mothers on each calendar day in a 60 day bandwidth around January 1. Negative x-axis values represent dates before January 1, while positive x-axis values represent dates after January 1. Days within eight days of January 1 are colored in white. See notes to Table 1 for a description of the data sample.

Figure 2: Regression Discontinuity Plots for CPS Involvement



(a) Any Referral

(b) # Referrals

(c) Any Investigation

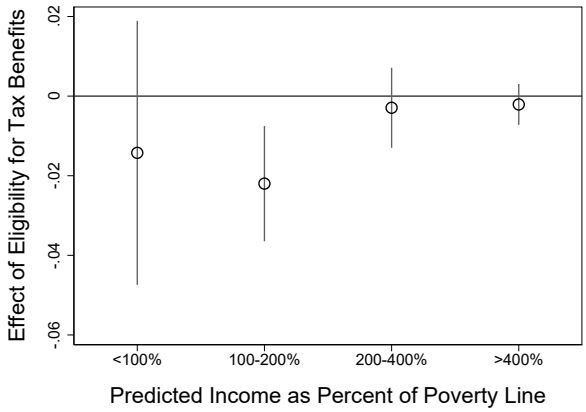
(d) # Investigations

(e) Any Foster Care

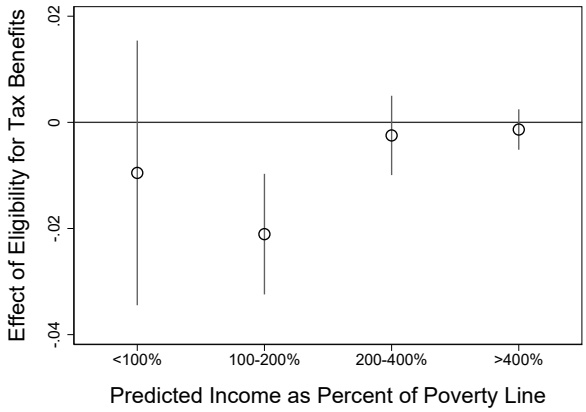
(f) # Days in Foster Care

For each outcome variable reported in Table 5, this figure plots the average on each birthdate relative to January 1, within a 60 bandwidth and excluding an 8 day donut. Birthdates to the left of the vertical line represent those which are eligible for child-related tax benefits within the first year of life. See notes to Table 1 for a description of the data sample. The sample is further restricted to households with predicted income below 200% of the poverty line.

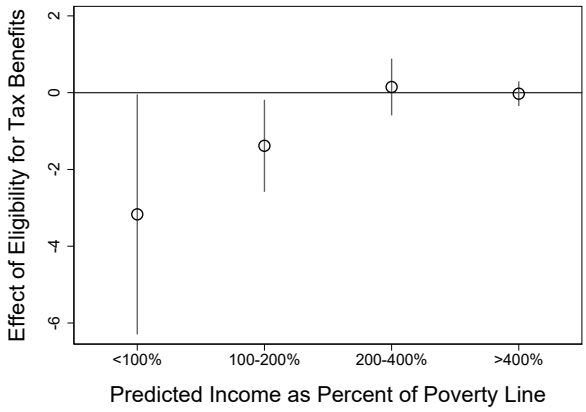
Figure 3: Effects Across the Income Distribution



(a) #. Referrals



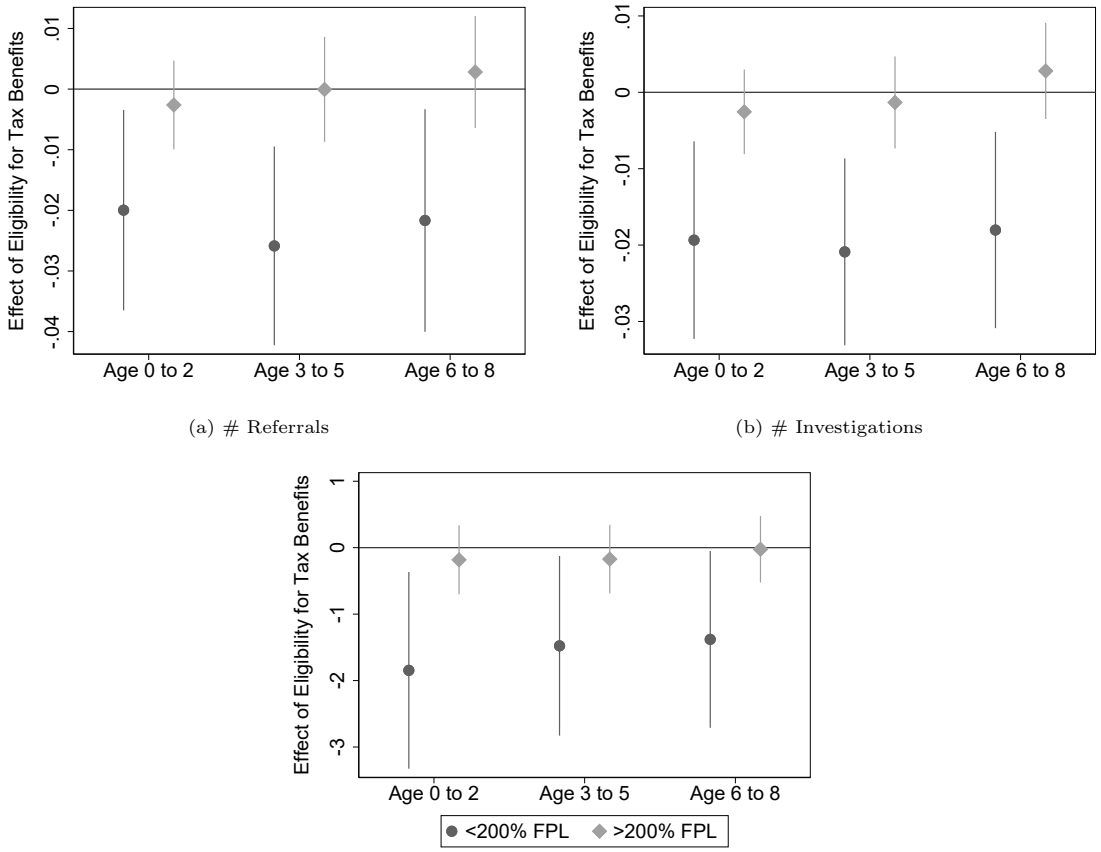
(b) # Investigations



(c) # Days in Foster Care

This figure shows coefficients and 95% confidence intervals from separate regressions estimating Eq. 1, where each regression is run on a separate sub-sample of first births. The sub-sample is described on the x-axis. See notes to Table 1 for a description of the data sample.

Figure 4: Effects Throughout Childhood



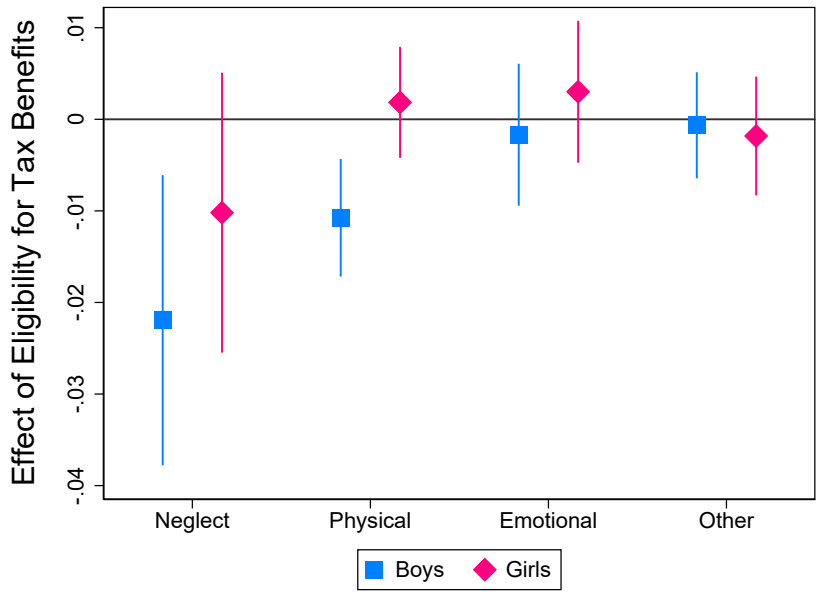
(a) # Referrals

(b) # Investigations

(c) # Days in Foster Care

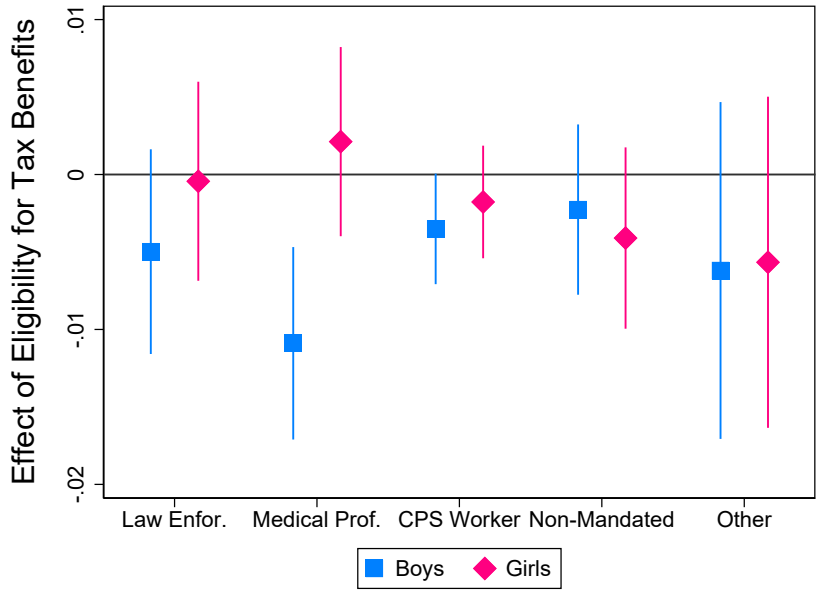
This figure shows coefficients and 95% confidence intervals from separate regressions estimating Eq. 1. In panel (a), the outcome variable is equal to the number of referrals received in a given age range (as described on the x-axis). In panel (b), the outcome is similarly the number of investigations in a given age range, and in panel (c), the outcome is days spent in foster care within a given age range. See notes to Table 1 for a description of the data sample. Effects are shown separately for households with predicted income below 200% of the poverty line, and those with predicted income above 200% of the poverty line. The sample is restricted to births in recentered years 2000 through 2011, to allow sufficient follow-up data for each birth. Corresponding regression results for the low-income sample are reported in Table A5, Columns (1)-(3).

Figure 5: Allegation Heterogeneity by Child Gender



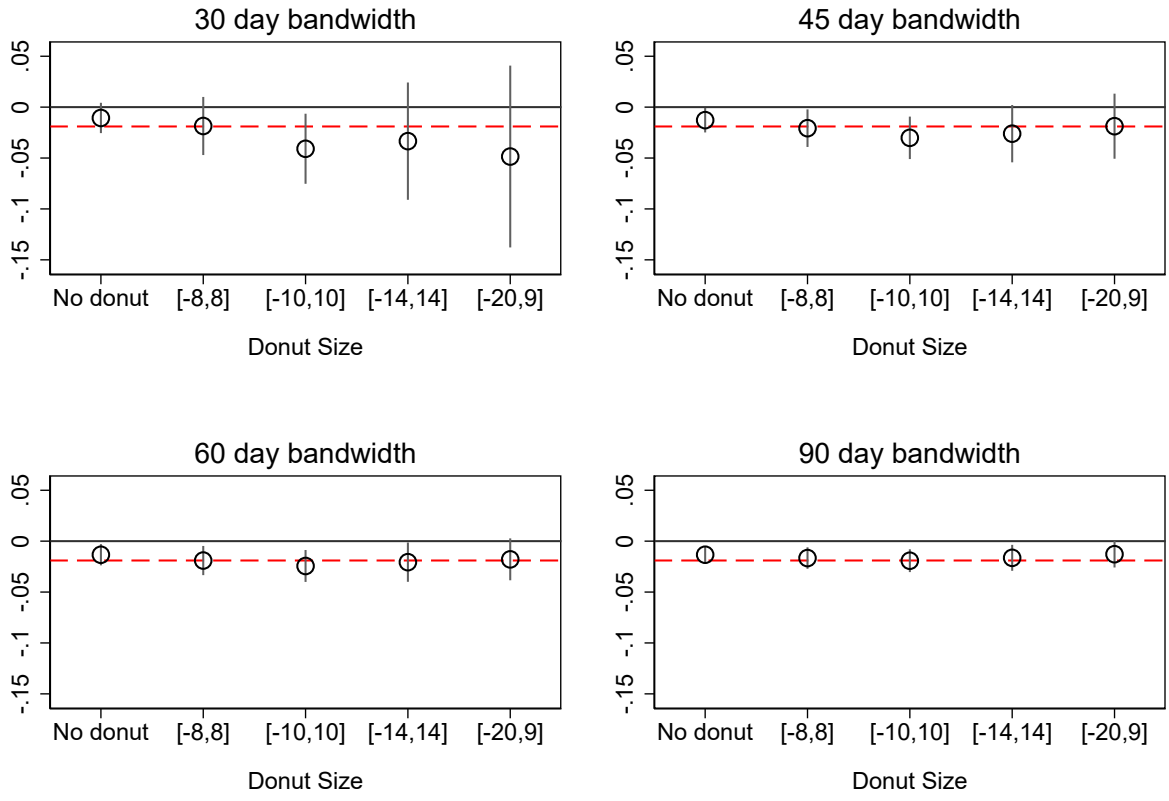
This figure shows coefficients and 95% confidence intervals from separate regressions estimating Eq. 1. Corresponding regression results are reported in Table A7, Panels B and C. See notes to Table A7 for a description of the outcome and data sample.

Figure 6: Reporter Heterogeneity by Child Gender



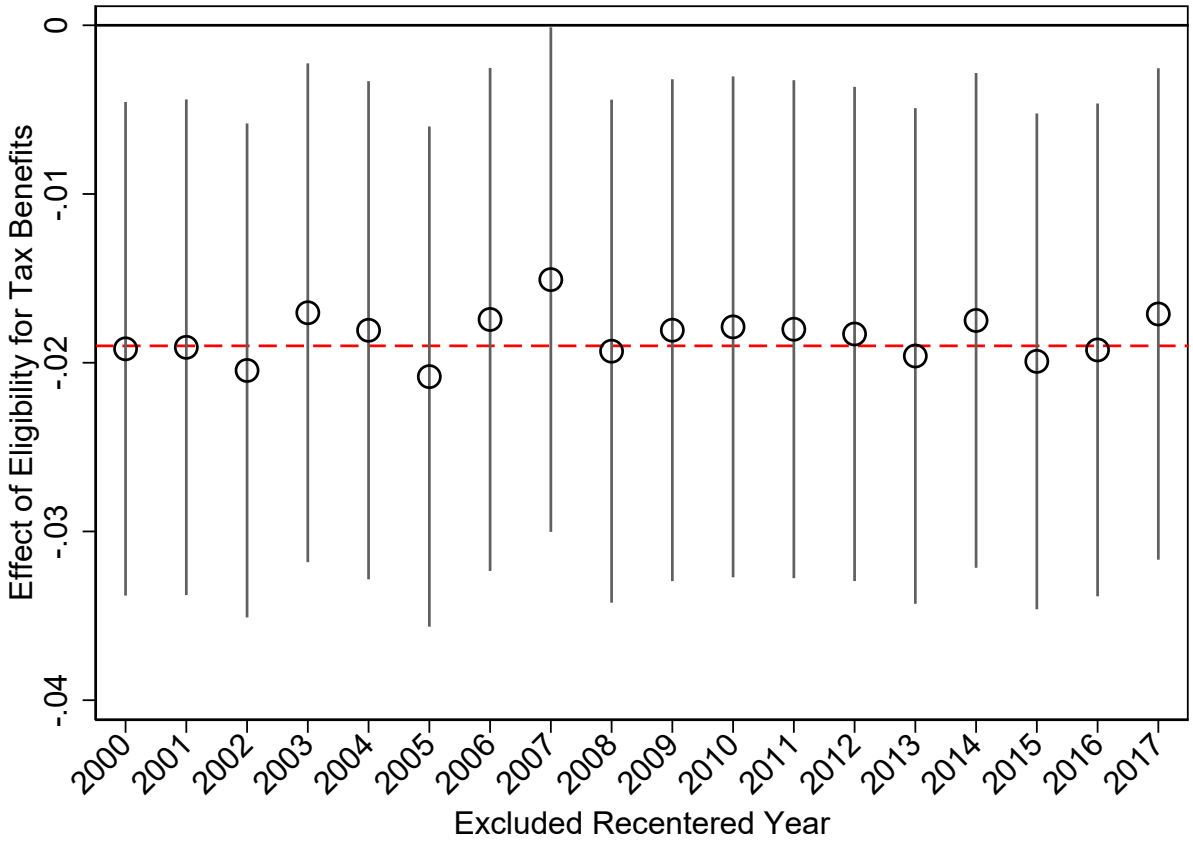
This figure shows coefficients and 95% confidence intervals from separate regressions estimating Eq. 1. Corresponding regression results are reported in Table A8, Panels B and C. See notes to Table A8 for a description of the outcome and data sample.

Figure 7: Robustness - Effect Across Bandwidth and Donut Choices



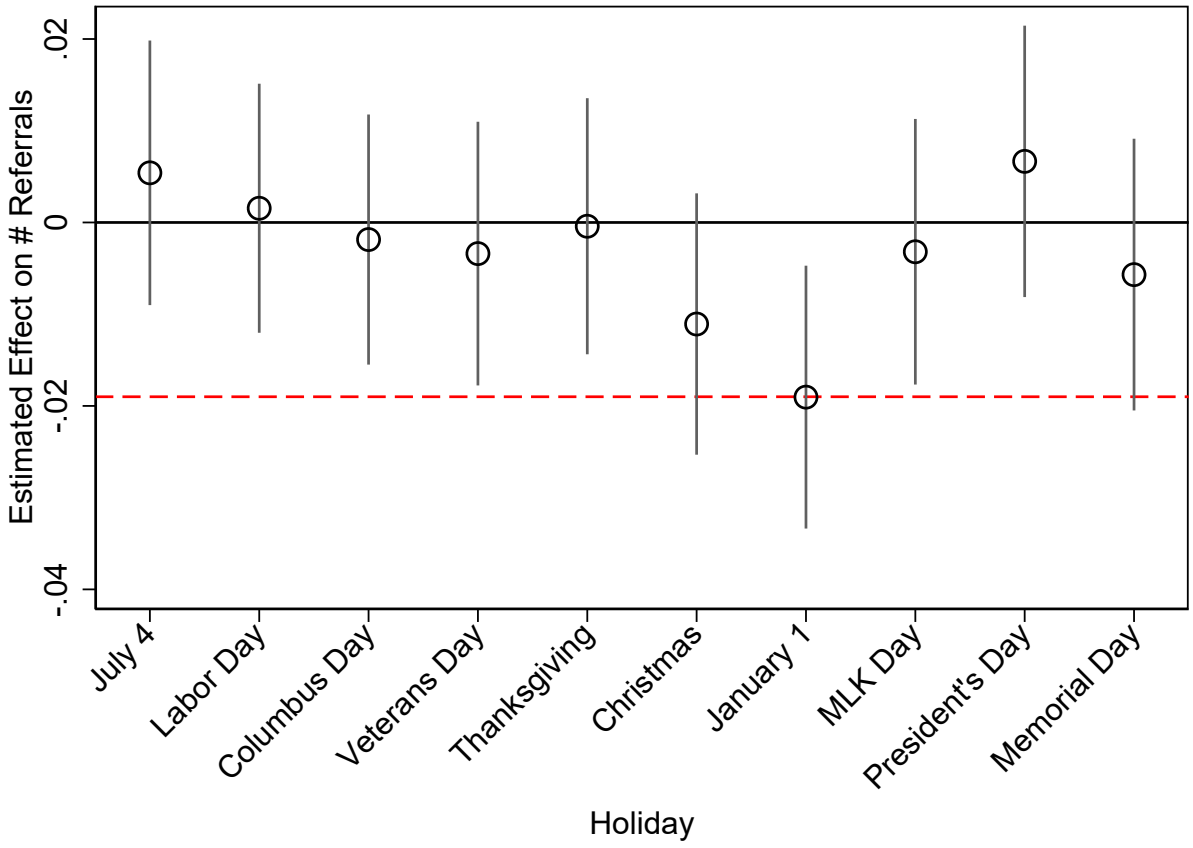
This figure presents coefficients and 95% confidence intervals for estimating Eq. 1, where Y_{it} is the number of referrals through age 2. Each reported regression alters the bandwidth and donut size, which features are reported in figure sub-titles and y axes, respectively. The vertical red line shows the coefficient from the paper's main specification. See notes to Table 1 for a description of the data sample. The sample is further restricted to households with predicted income below 200% of the poverty line.

Figure 8: Robustness - Effects Excluding Each Recentered Year



This figure presents coefficients and 95% confidence intervals for estimating Eq. 1, where Y_{it} is the number of referrals through age 2 and each regression limits the sample to exclude a single recentered year. The horizontal dashed red line shows the coefficient from the paper's main specification. See notes to Table 1 for a description of the data sample. The sample is further restricted to households with predicted income below 200% of the poverty line.

Figure 9: Placebo - Other Holidays



This figure presents coefficients and 95% confidence intervals for regressions estimating Eq. 1, where Z_i is redefined as the difference between child i's birthdate and the date of each federal holiday, respectively, and the recentered year is redefined similarly. The horizontal dashed red line shows the coefficient from the paper's main specification. See notes to Table 1 for a description of the data sample. The sample is further restricted to households with predicted income below 200% of the poverty line.

Tables

Table 1: Summary Statistics - Birth Records

	All	***CPS***	***Foster Care***	***Death***
	mean	mean	mean	mean
Panel A: Demographics				
Male Child	0.512	0.516	0.522	0.575
White Mother	0.317	0.310	0.350	0.281
Black Mother	0.058	0.135	0.163	0.110
Hispanic Mother	0.445	0.476	0.412	0.450
Asian/Pac. Is. Mother	0.158	0.051	0.038	0.127
American Indian Mother	0.004	0.011	0.016	0.006
Foreign-Born Mother	0.384	0.183	0.118	0.320
Panel B: Low-Income Proxies				
Birth Paid For by MediCal	0.409	0.670	0.742	0.495
Mother's Age (Years)	26.08	21.94	21.81	24.887
Mother's Educ. < HS	0.442	0.700	0.774	0.537
Predicted Tax Value of Dec. Birth (2017 $)	2568	2676	2549	2612
Predicted Household Income (2017 $)	61497	34353	27374	50256
Low Income (<200% FPL)	0.413	0.702	0.786	0.520
Panel C: CPS Involvement				
Referred to CPS Before Age 3	0.080	1.000	0.998	0.183
Placed in Foster Care Before Age 3	0.011	0.134	1.000	0.009
Age at First CPS Referral (Years)		0.748	0.378	0.606
# Referrals Before Age 3	0.141	1.753	2.631	0.290
Panel D: Mortality				
Died Before Age 3	0.003	0.006	0.002	1.000
Age at Death (Years)				0.473
Observations	1181675	94806	12681	3189

Notes: This table presents average characteristics for all children born in California to first-time mothers within 60 days of January 1 between November 1999 and March 2017. Averages are presented separately for four groups: all children in the sample (Column 1), children in the sample who ever are referred to CPS through age 2 (Column 2), children in the sample who are placed in foster care through age 2 (Column 3) and children who die through age 2 (Column 4). Variables in Panels A come directly from the birth records. In cases where multiple race codes are listed for the birth mother, I use the first listed race. White, Black, Asian/Pacific Islander and American Indian mothers are all non-Hispanic, and Hispanic mothers include all races. In Panel B, MediCal status, mother's age and education are each taken from the birth record. Predicted tax value and household income use information from both the birth records and the American Community Survey. In Panel C, referral and placement variables are equal to one if the birth record matches a CPS or foster care record, respectively. Age at first referral and number of referrals are observed in the CPS records. In Panel D, mortality is determined similarly by the link between birth and death records. Age at death is determined by the difference between death age (observed in death records) and birth age (observed in birth records).

Table 2: Summary Statistics - Child Protection System Records

	(1) Mean
Panel A: Reporter category	
Non-Mandated	0.115
School	0.073
Law Enforcement	0.181
Medical Professional	0.189
CPS Caseworker	0.063
Other Reporter	0.379
Panel B: Allegation category	
Neglect	0.604
Physical Abuse	0.127
Emotional Abuse	0.195
Other Allegation	0.263
Panel C: Outcomes	
Investigated	0.772
Any Substantiated	0.210
Any Inconclusive	0.234
Foster Care within 90 days	0.118
Foster Care within 180 days	0.137
N	2329846

Notes: This table presents average characteristics at the child-referral level for children aged 0 to 2 on referrals made between January 1999 and December 2020. Panel A reports the share of child-referrals made by different reporter categories. Non-Mandated includes immediate and extended family members, friends, neighbors, godparents, landlords, and "no relation." School includes school personnel and teachers. Law Enforcement includes law enforcement, parole officers, and probation officers. Medical Professional includes doctors, counselors and dentists. CPS Worker includes CPS staff. Other Reporter includes "other professionals," government agency, day care staff, child advocate, clergy, guardian ad litem, substitute care provider, and Indian custodian. Panel B reports the share of child-referrals which include at least one allegation of a given category. Other Allegation includes sexual abuse, caretaker absence/incapacity, severe neglect, exploitation, and at risk (sibling abused). Finally, Panel C reports characteristics of the outcomes of referrals. Investigated is an indicator variable equal to one if the referral is investigated. Any Substantiated is an indicator variable equal to one if any of the allegations made about the child were substantiated. Any Inconclusive is an indicator variable equal to one if any of the allegations were found to be inconclusive. Foster Care within 90 days is an indicator variable equal to one if the child is placed in foster care within 90 days of the referral date. Foster Care within 180 days is defined similarly.

Table 3: Validity - Child/Birth Characteristics

	(1) Male	(2) Low Birthweight	(3) MediCal Delivery	(4) Preterm	(5) Low Income	(6) Match Probability
Born before Jan. 1	0.0000232	-0.000373	0.0000345	0.000605	0.00490*	-0.000142
	(0.00274)	(0.00129)	(0.00269)	(0.00157)	(0.00276)	(0.000451)
Outcome mean	0.512	0.0585	0.405	0.0902	0.408	0.994
Obs.	1029468	1029468	1029468	1029468	969938	220834

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is described in the column titles. Observations are at the birth record (child) level. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample.

Table 4: Validity - Parent characteristics

	(1) White	(2) Black	(3) Hispanic	(4) Other Race	(5) Age	(6) Low Educ.	(7) High Educ.	(8) Missing Info.
Panel A: Mother's Characteristics								
Born before Jan. 1	-0.00183	-0.00128	-0.000446	0.00263	-0.00861	-0.00261	0.000752	0.00260*
	(0.00255)	(0.00129)	(0.00272)	(0.00202)	(0.0345)	(0.00269)	(0.00271)	(0.00140)
Outcome mean	0.321	0.0575	0.442	0.162	26.11	0.437	0.530	0.0697
Panel B: Father's Characteristics								
Born before Jan. 1	-0.00218	-0.00158	0.00326	0.00269	-0.0408	0.0000206	-0.000900	0.000760
	(0.00255)	(0.00125)	(0.00271)	(0.00186)	(0.0419)	(0.00269)	(0.00271)	(0.00207)
Outcome mean	0.321	0.0544	0.429	0.135	29.15	0.423	0.455	0.170
Obs.	1029468	1029468	1029468	1029468	938258	1029468	1029468	1029468

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is described in the column titles. Low Educ. indicates the parent is reported as having a high school education or less. High Educ. indicates the parent is reported as having at lease some college education. Missing Info. is equal to one if there is any information (race, age, education) missing for the relevant parent, and zero otherwise. Observations are at the birth record (child) level. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample.

Table 5: Results - Regression Discontinuity Effects on CPS Involvement (Age 0 to 2)

	(1) All	(2) <200% FPL	(3) ≥200% FPL
Panel A: Any Referrals			
Eligible for Tax Benefits	-0.00100	-0.00579*	0.000840
	(0.00149)	(0.00301)	(0.00145)
Outcome mean	0.080	0.137	0.040
Panel B: Any Investigations			
Eligible for Tax Benefits	-0.000897	-0.00547*	0.000965
	(0.00140)	(0.00286)	(0.00135)
Outcome mean	0.070	0.120	0.034
Panel C: Any Foster Care			
Eligible for Tax Benefits	-0.000775	-0.00255**	-0.0000166
	(0.000569)	(0.00124)	(0.000461)
Outcome mean	0.011	0.020	0.004
Panel D: # Referrals			
Eligible for Tax Benefits	-0.00729**	-0.0190***	-0.00202
	(0.00344)	(0.00731)	(0.00302)
Outcome mean	0.142	0.251	0.065
Panel E: # Investigations			
Eligible for Tax Benefits	-0.00658**	-0.0171***	-0.00161
	(0.00263)	(0.00561)	(0.00224)
Outcome mean	0.108	0.192	0.048
Panel F: Days Foster Care			
Eligible for Tax Benefits	-0.612**	-1.880***	0.0795
	(0.290)	(0.646)	(0.216)
Outcome mean	4.255	8.284	1.339
Tax value mean ($)	2567	2881	2347
Aftertax income ($)	61588	24007	87901
Obs.	1029468	399439	570499

Notes: This table presents point estimates and standard errors from estimating Eq. 1 on CPS involvement through age 2. The specific outcome variable is described in the panel titles. Each column reports results for a separate sub-sample of the data: Column (1) reports results for the entire analysis sample; Column (2) reports results for births to households with a predicted income below 200% of the federal poverty line; Column (3) reports results for births to households with a predicted income above 200% of the federal poverty line. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample.

Table 6: Heterogeneity - Mother's Primary Race/Ethnicity

	(1) White	(2) Black	(3) Hispanic	(4) Asian/PI
Panel A: # Referrals				
Eligible for Tax Benefits	-0.0690**	-0.00415	-0.0132*	0.00565
	(0.0276)	(0.0337)	(0.00734)	(0.0163)
Outcome mean	0.475	0.482	0.186	0.105
Panel B: # Investigations				
Eligible for Tax Benefits	-0.0554***	0.00511	-0.0131**	0.000205
	(0.0200)	(0.0264)	(0.00584)	(0.0134)
Outcome mean	0.338	0.379	0.147	0.0809
Panel C: Days Foster Care				
Eligible for Tax Benefits	-4.065*	-7.988**	-1.025*	-0.116
	(2.413)	(3.549)	(0.620)	(1.622)
Outcome mean	16.54	20.22	5.300	3.788
Tax value mean ($)	2624.4	2447.1	3044.2	2708.4
Aftertax income ($)	23903.2	19246.2	25168.2	23110.0
Obs.	58351	33863	267894	24645

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is described in the Panel titles. Each column reports results for a separate sub-sample of the data. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample. The sample is limited to births to households with predicted income below 200% of the federal poverty line for a family of 3.

Table 7: Heterogeneity - Gender

	(1) Boys	(2) Girls
Panel A: # Referrals		
Eligible for Tax Benefits	-0.0278***	-0.00985
	(0.0104)	(0.0103)
Outcome mean	0.254	0.248
Panel B: # Investigations		
Eligible for Tax Benefits	-0.0211***	-0.0131
	(0.00788)	(0.00799)
Outcome mean	0.194	0.189
Panel C: Days Foster Care		
Eligible for Tax Benefits	-2.287**	-1.463
	(0.910)	(0.916)
Outcome mean	8.414	8.149
Tax value mean ($)	2881.9	2880.6
Aftertax income ($)	24033.4	23978.5
Obs.	204186	195250

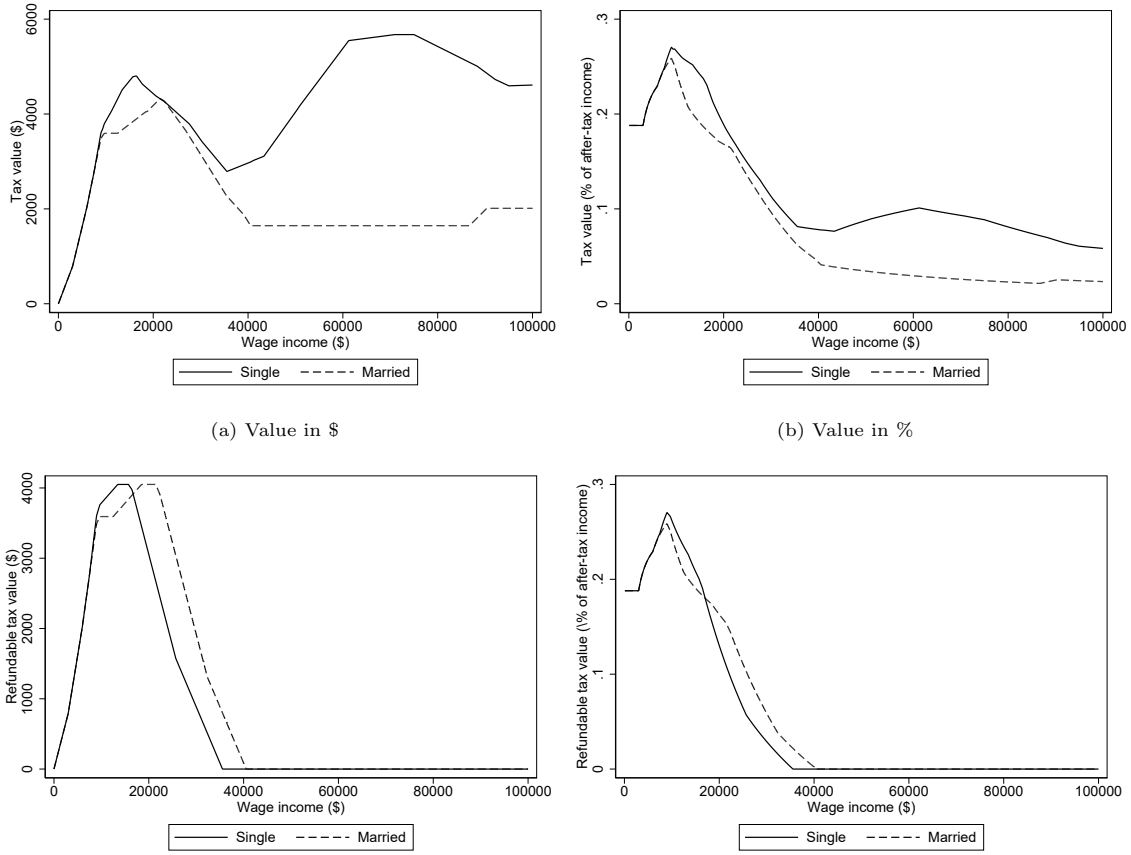
Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is described in the Panel titles. Each column reports results for a separate sub-sample of the data. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample. The sample is limited to births to households with predicted income below 200% of the federal poverty line for a family of 3.

7 Appendix Figures and Tables

Figure A1: Tax Value of December Birth



(a) Value in $

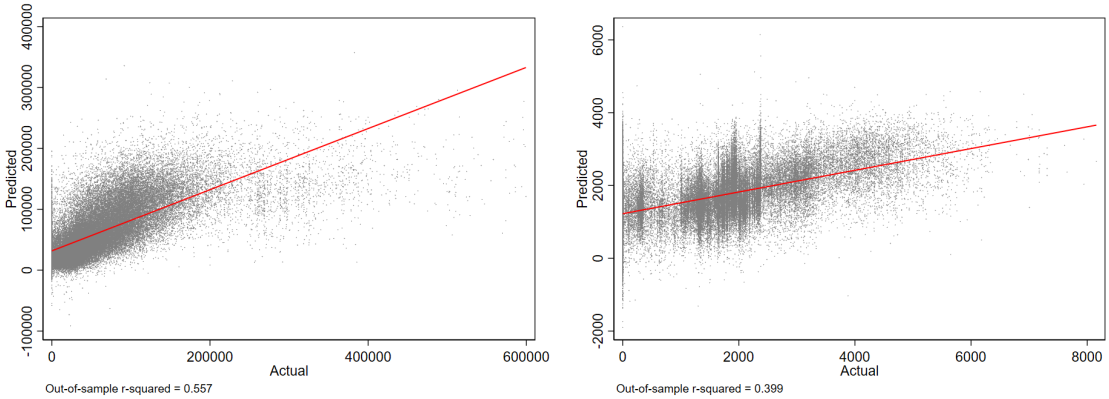
(b) Value in %

(c) Refundable value in $

(d) Refundable value in %

This figure illustrates the tax value of claiming one child (as opposed to no children) in tax year 2010 in California, assuming no non-wage income or transfers. Estimates were made using NBER's TAXSIM tool. In particular, for simulated households with wage earnings between $0 and $100,000 in increments of $100, I estimate four sets of taxes/refunds due. For each of households filing as a single person and jointly, I calculate taxes with one dependent is claimed vs. no dependents are claimed. I treat the difference between these amounts as the tax value (in dollars) of a December birth. The tax value (in %) is equal to the dollar value divided by after-tax income. Since I assume zero childcare expenses, the Child Dependent Care Credit is equal to zero, and the tax value is made up of (1) an increase in the personal exemption, (2) EITC benefits, and (3) the Child Tax Credit. Panels (a) and (b) show tax value including both refunds and reductions in tax liability, and Panels (c) and (d) show just the refundable portion of tax value.

Figure A2: Out-of-Sample ACS Predictions

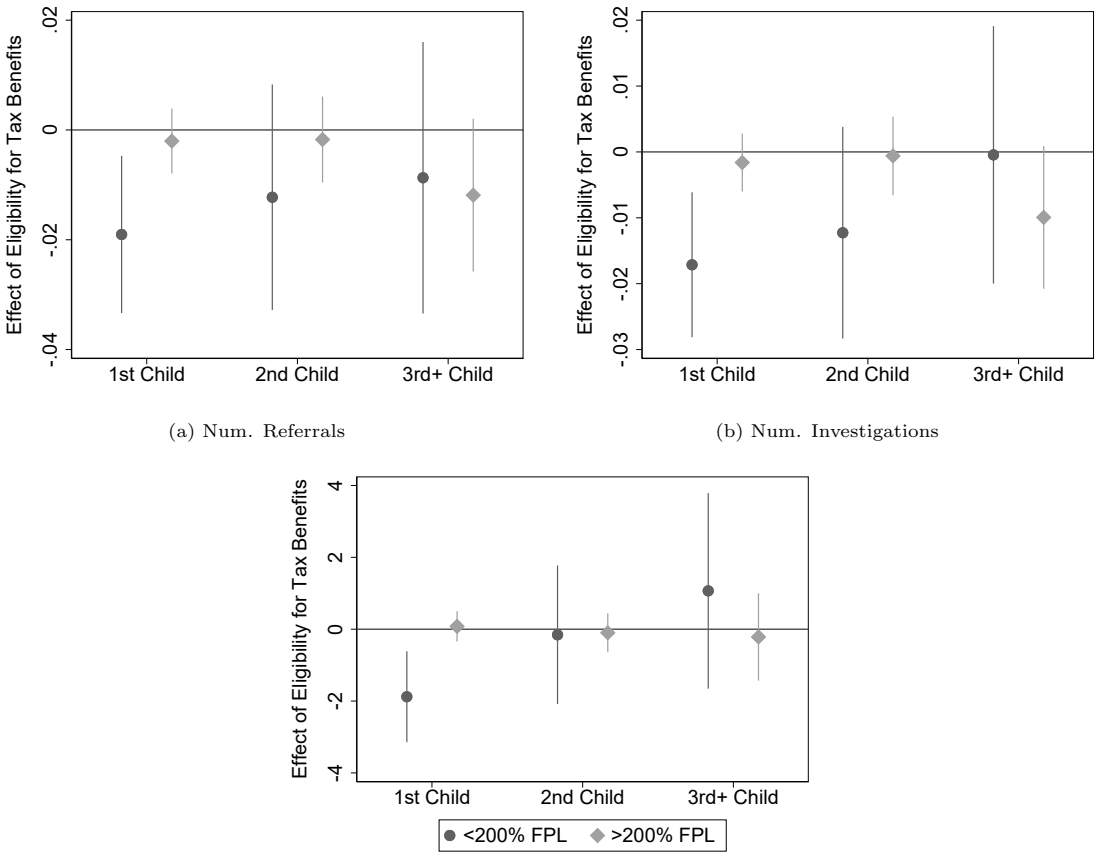


(a) After-tax income in $

(b) Value of December birth in $

This figure compares predicted values to true values in the validation subsample of ACS, for two variables of interest. The x-axis shows the true value, the y-axis the predicted value, and each grey dot is a separate observation. The red line shows the line of best fit through the data. Out-of-sample R-squared is reported for each variable below the graphs.

Figure A3: Effects by Child Order



(a) Num. Referrals

(b) Num. Investigations

(c) Days in foster care

This figure shows coefficients and 95% confidence intervals from separate regressions estimating Eq. 1. In panel (a), the outcome variable is equal to the number of referrals through age 2. In panel (b), the outcome is equal to the number of investigations through age 2, and in panel (c), the outcome is days spent in foster care through age 2. See notes to Table 1 for a description of the data sample. Effects are shown separately for households with predicted income below 200% of the poverty line for a family of 3, and those with predicted income above 200% of the poverty line for a family of 3.

Table A1: Validity - Child/Birth Characteristics (No Donut)

	(1) Male	(2) Low Birthweight	(3) MediCal Delivery	(4) Preterm	(5) Low Income	(6) Match Probability
Born before Jan. 1	0.000139	0.000368	-0.00110	0.00586***	0.00196	0.000158
	(0.00202)	(0.000961)	(0.00199)	(0.00118)	(0.00204)	(0.000338)
Outcome mean	0.512	0.0587	0.406	0.0902	0.410	0.994
Obs.	1181675	1181675	1181675	1181675	1113386	253880

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents point estimates and standard errors from estimating Eq. 1, where there is no excluded donut and where the outcome variable is described in the column titles. Observations are at the birth record (child) level. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample.

Table A2: Validity - Parent characteristics (No Donut)

	(1) White	(2) Black	(3) Hispanic	(4) Other Race	(5) Missing Race	(6) Age	(7) Missing Age	(8) Low Educ.
Panel A: Mother's Characteristics								
Born after Jan. 1	0.00123	-0.000968	-0.000741	-0.000239	0.0101	-0.00274	0.000983	0.00186*
	(0.00187)	(0.000954)	(0.00201)	(0.00149)	(0.0255)	(0.00199)	(0.00200)	(0.00103)
Outcome mean	0.320	0.0578	0.443	0.162	26.10	0.438	0.528	0.0698
Panel B: Father's Characteristics								
Born before Jan. 1	0.000602	-0.000431	0.00219	0.0000344	-0.0404	0.00144	-0.000169	-0.000462
	(0.00187)	(0.000927)	(0.00200)	(0.00138)	(0.0310)	(0.00199)	(0.00200)	(0.00153)
Outcome mean	0.319	0.0546	0.430	0.135	29.14	0.424	0.454	0.170
Obs.	1181675	1181675	1181675	1181675	1076797	1181675	1181675	1181675

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents point estimates and standard errors from estimating Eq. 1, where there is no excluded donut and where the outcome variable is described in the column titles. Observations are at the birth record (child) level. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample.

Table A3: Results - Effects across the income distribution

	(1) <100% FPL	(2) 100-200% FPL	(3) 200-400% FPL	(4) >400% FPL
Panel A: # Referrals				
Eligible for Tax Benefits	-0.0142	-0.0220***	-0.00292	-0.00209
	(0.0169)	(0.00739)	(0.00513)	(0.00262)
Outcome mean	0.381	0.196	0.0995	0.0260
Panel B: # Investigations				
Eligible for Tax Benefits	-0.00953	-0.0211***	-0.00247	-0.00135
	(0.0127)	(0.00580)	(0.00381)	(0.00194)
Outcome mean	0.288	0.151	0.0747	0.0188
Panel C: Days Foster Care				
Eligible for Tax Benefits	-3.169**	-1.384**	0.148	-0.0264
	(1.594)	(0.612)	(0.378)	(0.164)
Outcome mean	15.21	5.320	2.230	0.351
Tax value mean ($)	2423.3	3079.3	2741.4	1897.0
Aftertax income mean ($)	8286.6	30801.8	58127.8	121865.8
Obs.	120552	278887	304009	266490

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is described in the Panel titles. Each column reports results for a separate sub-sample of the data. See notes to Table 1 for a description of the analysis sample.

Table A4: Results - Alternative Poverty Proxies

	(1) Mother HS Ed.	(2) MediCal Birth	(3) Father Missing Info.	(4) Young Mother
Panel A: # Referrals - Low Income Proxy				
Eligible for Tax Benefits	-0.0174***	-0.0122*	-0.0184	-0.0184**
	(0.00664)	(0.00707)	(0.0118)	(0.00728)
Outcome mean	0.233	0.244	0.265	0.261
Tax value mean ($)	2856	2860	2473	2862
Aftertax income mean ($)	34064	34296	29752	31074
Obs.	453877	420365	175530	412287
Panel B: # Referrals - High Income Proxy				
Eligible for Tax Benefits	-0.000239	-0.00392	-0.00516	0.000166
	(0.00300)	(0.00306)	(0.00334)	(0.00292)
Outcome mean	0.064	0.072	0.117	0.063
Tax value mean ($)	2339	2369	2586	2374
Aftertax income mean ($)	86843	80048	68061	81527
Obs.	540893	609103	853938	617181

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is number of referrals through age 2. Each column reports results for different splits of the analysis sample. Column (1) reports results for sub-samples of births to mothers with a high school degree or less (low-income proxy) and to mothers with at least some college education (high-income proxy). Column (2) reports results for sub-samples of births which were paid for by MediCal (low-income proxy) and which were not paid for by MediCal (high-income proxy). Column (3) reports results for sub-samples of births with some information about the father missing from the birth record (low-income proxy) and with no information about the father missing from the birth record (high-income proxy). Column (4) reports results for sub-samples of births to mothers younger than 24 (low-income proxy) and to mothers aged 24 and older (high-income proxy). Panel A reports results for low-income proxy sub-samples, and Panel B reports results for high-income proxy sub-samples. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample.

Table A5: Results - Effects Throughout Childhood

	(1) Age 0 - 2	(2) Age 3 - 5	(3) Age 6 - 8	(4) Age 0 - 8
Panel A: # Referrals				
Eligible for Tax Benefits	-0.0200**	-0.0259***	-0.0217**	-0.0675***
	(0.00843)	(0.00836)	(0.00937)	(0.0187)
Outcome mean	0.236	0.227	0.252	0.715
Panel B: Any Referrals				
Eligible for Tax Benefits	-0.00787**	-0.0124***	-0.00563	-0.0143***
	(0.00356)	(0.00350)	(0.00362)	(0.00475)
Outcome mean	0.130	0.125	0.135	0.276
Panel C: # Investigations				
Eligible for Tax Benefits	-0.0194***	-0.0209***	-0.0180***	-0.0583***
	(0.00660)	(0.00624)	(0.00655)	(0.0137)
Outcome mean	0.183	0.169	0.179	0.531
Panel D: Any Investigations				
Eligible for Tax Benefits	-0.00769**	-0.0106***	-0.00828**	-0.0157***
	(0.00338)	(0.00330)	(0.00338)	(0.00458)
Outcome mean	0.114	0.109	0.114	0.246
Panel E: Days Foster Care				
Eligible for Tax Benefits	-1.846**	-1.477**	-1.380**	-4.704***
	(0.755)	(0.690)	(0.679)	(1.524)
Outcome mean	7.499	7.057	6.443	20.998
Panel F: Any Foster Care				
Eligible for Tax Benefits	-0.00247*	-0.00493***	-0.00360***	-0.00738***
	(0.00145)	(0.00141)	(0.00128)	(0.00200)
Outcome mean	0.019	0.019	0.015	0.038
Tax value mean ($)	2835	2835	2835	2835
Aftertax income ($)	23968	23968	23968	23968
Obs.	272623	272623	272623	272623

This table presents point estimates and standard errors from estimating Eq. 1, for different child age ranges as described in the Column titles. The outcome variable is described in the Panel titles. See notes to Table 1 for a description of the analysis sample. The sample is limited to births to households with predicted income below 200% of the federal poverty line for a family of 3, and is limited to recentered years 2000 through 2011, to allow for nine years of followup for each birth.

Table A6: Heterogeneity - Mother's Country of Birth

	(1) Native-Born Mother	(2) Foreign-Born Mother
Panel A: # Referrals		
Eligible for Tax Benefits	-0.0292***	-0.00164
	(0.0110)	(0.00618)
Outcome mean	0.351	0.0880
Panel B: # Investigations		
Eligible for Tax Benefits	-0.0256***	-0.00278
	(0.00844)	(0.00497)
Outcome mean	0.267	0.0697
Panel C: Days Foster Care		
Eligible for Tax Benefits	-2.528**	-0.794
	(0.997)	(0.488)
Outcome mean	12.09	2.082
Tax value mean ($)	2818.3	2981.9
Aftertax income ($)	23598.4	24659.2
Obs.	245721	153718

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is described in the Panel titles. Each column reports results for a separate sub-sample of the data. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample. The sample is limited to births to households with predicted income below 200% of the federal poverty line for a family of 3.

Table A7: Heterogeneity - Allegation Categories

	(1) Neglect	(2) Physical	(3) Emotional	(4) Other
Panel A: All				
Eligible for Tax Benefits	-0.0162***	-0.00458**	0.000575	-0.00123
	(0.00562)	(0.00226)	(0.00280)	(0.00221)
Outcome mean	0.168	0.0419	0.0582	0.0425
Tax value mean ($)	2881.3	2881.3	2881.3	2881.3
Obs.	399439	399439	399439	399439
Panel B: Boys				
Eligible for Tax Benefits	-0.0219***	-0.0107***	-0.00168	-0.000652
	(0.00808)	(0.00328)	(0.00396)	(0.00296)
Outcome mean	0.171	0.0441	0.0579	0.0406
Tax value mean ($)	2881.9	2881.9	2881.9	2881.9
Obs.	204186	204186	204186	204186
Panel C: Girls				
Eligible for Tax Benefits	-0.0102	0.00185	0.00301	-0.00182
	(0.00780)	(0.00309)	(0.00395)	(0.00331)
Outcome mean	0.166	0.0397	0.0585	0.0445
Tax value mean ($)	2880.6	2880.6	2880.6	2880.6
Obs.	195250	195250	195250	195250

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is number of referrals through age 2 involving a given category of maltreatment allegation. The column title reports the relevant allegation category. Each Panel presents results for a separate sub-sample of the data, which is restricted to first births to households with predicted income below 200% of the poverty line. See notes to Table 1 for a description of the analysis sample. Refer to Table 2 for more detailed information on allegation categories.

Table A8: Heterogeneity - Reporter Categories

	(1) Law Enforcement	(2) Medical Prof.	(3) CPS Caseworker	(4) Non-Mandated	(5) Other
Panel A: All					
Eligible for Tax Benefits	-0.00278	-0.00452**	-0.00267**	-0.00313	-0.00593
	(0.00235)	(0.00222)	(0.00130)	(0.00205)	(0.00389)
Outcome mean	0.0527	0.0459	0.0167	0.0324	0.103
Tax value mean ($)	2881.3	2881.3	2881.3	2881.3	2881.3
Obs.	399439	399439	399439	399439	399439
Panel B: Boys					
Eligible for Tax Benefits	-0.00498	-0.0109***	-0.00351*	-0.00226	-0.00620
	(0.00337)	(0.00317)	(0.00182)	(0.00280)	(0.00555)
Outcome mean	0.0538	0.0474	0.0173	0.0325	0.103
Tax value mean ($)	2881.9	2881.9	2881.9	2881.9	2881.9
Obs.	204186	204186	204186	204186	204186
Panel C: Girls					
Eligible for Tax Benefits	-0.000437	0.00212	-0.00177	-0.00410	-0.00566
	(0.00328)	(0.00311)	(0.00185)	(0.00299)	(0.00545)
Outcome mean	0.0517	0.0443	0.0160	0.0322	0.104
Tax value mean ($)	2880.6	2880.6	2880.6	2880.6	2880.6
Obs.	195250	195250	195250	195250	195250

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is number of referrals through age 2 made by a given category of reporter. The column title reports the relevant reporter category. Each Panel presents results for a separate sub-sample of the data, which is restricted to first births to households with predicted income below 200% of the poverty line. See notes to Table 1 for a description of the analysis sample. Refer to Table 2 for more detailed information on reporter categories.

Table A9: Placebo - Regression Discontinuity Effects on Child Involvement in CPS in First 60 Days of Life

	(1)	(2)	(3)
	Any Referral 60 days	Any Investigation 60 days	Any Placement 60 days
Eligible for Tax Benefits	-0.00124	-0.00101	-0.000791
	(0.00149)	(0.00135)	(0.000626)
Outcome mean	0.0286	0.0236	0.00513
Tax value mean ($)	2881.3	2881.3	2881.3
Aftertax income mean ($)	24006.6	24006.6	24006.6
Obs.	399439	399439	399439

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table presents point estimates and standard errors from estimating Eq. 1, where the outcome variable is described in the column titles. Statistical significance is denoted by * ($p < 0.1$), ** ($p < 0.05$) and *** ($p < 0.01$). See notes to Table 1 for a description of the analysis sample. The analysis sample is further restricted to households with predicted income below 200% of the poverty line.