Parents' Valuations of Cash vs. Near-Cash Benefits: Evidence from Supplemental Security Income

Angela Y. Gu[†]
University of California, San Diego

Christopher D. Earles[‡]
Social Security Administration

November 10, 2024

(Link to Latest Version)

Abstract

We examine the relative valuations of cash and near-cash benefits in a setting where the use of near-cash benefits incurs substantial administrative costs: the Supplemental Security Income program for children. We exploit a discontinuity in program approval time that affects whether children receive cash or near-cash benefits. Using Social Security Administration microdata, we assess the relative valuations of these two benefit types through parents' labor market responses to the income shocks they induce. We find that secondary earners in near-cash families work 10.8 percentage points more and earn nearly $1,470 more during the year of approval. Our estimates suggest that each near-cash dollar is valued at only 65% of a cash dollar.

[†]Address: La Jolla, CA. Email: aygu@ucsd.edu, corresponding author.
[‡]Address: Woodlawn, MD.

We thank Julie Cullen, Itzik Fadlon, Gordon Dahl, Craig McIntosh, Katherine Meckel, and UCSD applied seminar participants for their helpful comments and feedback. Gu also thanks Jeffrey Hemmeter and Joyce Nicholas for their help in facilitating this collaboration. This research was supported by a grant from Policy Research, Inc. as part of the U.S. Social Security Administration's (SSA's) Analyzing Relationships Between Disability, Rehabilitation and Work Program. The opinions and conclusions expressed are solely those of the authors and do not represent the opinions or policy of Policy Research, Inc., SSA or any other agency of the Federal Government. All errors are our own.

1 Introduction

A key debate in the design of safety-net programs revolves around whether benefits should be provided in cash, near-cash, or in-kind form. Cash benefits enable recipients to tailor their consumption choices to their specific needs. However, if there is concern that cash transfers might be used for less beneficial or even harmful goods and services, policymakers may restrict recipients' choices by offering near-cash or in-kind benefits instead.

The potential misuse of cash transfers is the main justification for offering children's benefits in near-cash or in-kind form. What policymakers may overlook is that these other benefit types can impose administrative burdens and distort consumption choices, reducing their overall value. These trade-offs raise the question: what is the optimal way to provide children's benefits?

In this paper, we provide evidence relevant to this question in a unique setting where the use of near-cash benefits incurs substantial administrative costs: the Supplemental Security Income (SSI) program for children with disabilities. SSI is one of the largest welfare programs in the U.S. Beneficiaries are entitled to back pay for the time they have waited for program approval. Children receive this back pay in cash if the amount owed is less than or equal to six times the maximum monthly benefit payment amount and in near-cash if it exceeds that sum. The near-cash benefit is known as a dedicated account. Back pay deposited in these accounts can only be used for SSI children's medical treatment, education, and job skills training. Funds cannot be used for general household expenses such as food, clothing, or shelter. Compliance with dedicated account policy rules requires obtaining written approval for specific purchases, annual reporting of fund usage, and repaying any misused funds.

We use parents' labor market outcomes to assess the relative valuations of cash and near-cash benefits. Parents of SSI children are highly sensitive to income fluctuations and adjust their labor supply to fully offset the receipt of SSI benefits (Deshpande 2016a; Guldi et al. 2024). The back pay children receive constitutes an additional income shock that could allow parents to work less. The effective size of this shock depends on parents' valuations of the benefit types: higher valuations lead to larger shocks and greater reductions in labor supply, while lower valuations result in smaller shocks and less pronounced decreases in labor supply. If parents value near-cash benefits less than cash benefits—due to having fewer children-specific goods in their preferred consumption bundle, mental accounting costs, or the administrative burden associated with using the near-cash benefits—they may work more after receiving near-cash benefits than cash benefits (Gahvari 1994; Currie and Gahvari 2008).

We use SSA microdata that link child beneficiaries to their parents' annual tax returns.

These data include children's benefit amounts, application and approval dates, dedicated account status, and other application characteristics along with parents' labor market outcomes, such as employment status and earnings. We use this information to determine children's program approval times and benefit types, as well as to analyze parents' work behavior during the year of program approval.

We employ a fuzzy regression discontinuity design and exploit a cutoff in program approval time that affects the likelihood of children receiving back pay in near-cash rather than cash form. In our setting, cash and near-cash beneficiaries receive the same amount in the first year of benefit receipt, but differences in payment amounts arise in the second year. Since these second-year payment differences may still influence household behavior in the first year, we bound what these potential benefit amount effects could be. We examine parental labor market outcomes for similar children at the next monthly cutoff, where all children receive near-cash benefits but one group receives additional near-cash payments in the second year. Thus, we identify the effects of cash vs. near-cash benefits at our main cutoff by subtracting the benefit amount effects measured at the next monthly cutoff from the total effects observed at the main cutoff during the program's first year.

While we do not detect benefit type effects across all households, marked differences emerge in those with secondary earners. Secondary earners in families with dedicated accounts work 10.8 percentage points more and earn nearly $1,470 more annually compared to their counterparts in families receiving cash benefits. These labor supply adjustments increase their total earnings by roughly 17%.

The lack of detectable effects in the full sample does not preclude the possibility that other households may value cash and near-cash benefits differently. Households with no active earners or a sole earner may face financial constraints that limit their ability to adjust labor supply and generally have less elastic labor supply. As a result, they may adjust other forms of consumption instead. Our findings suggest that each near-cash dollar is valued at only 65% of a cash dollar.

In the future, we will assess valuation differences between children approved earlier vs. later in the year, those with mental vs. non-mental diagnoses, younger vs. older children, and those served by SSA field offices with higher vs. lower compliance with dedicated account policy rules.

This paper makes two key contributions to the existing literature. First, we present new evidence on valuation differences between cash and near-cash benefits in a unique U.S. setting where near-cash benefits entail significant administrative burdens. Prior research on these valuation differences has primarily focused on the Supplemental Nutrition Assistance Program and its predecessor, Food Stamps. Although some studies estimate that food

stamps have nearly full cash-equivalent value (Smeeding 1982; Moffitt 1989), research using representative experimental data suggests that the cash-equivalent value of these near-cash benefits is closer to 80% (Schanzenbach 2002). This lower relative valuation for near-cash benefits is supported by recent research, most of which identifies higher marginal propensities to consume food from the near-cash benefits compared to cash income (Currie 2003; Hoynes and Schanzenbach 2009; Bruich 2014; Beatty and Tuttle 2015; Hastings and Shapiro 2018). In the SSI children's program, we find that parents value a near-cash dollar at only 65% of a cash dollar. This lower cash-equivalent value of near-cash benefits reflects differences in the nature of the program, the population it serves, and the design of the near-cash transfer itself.

Second, our work contributes to the literature on parental labor market responses to SSI children's benefits. Early studies using fixed effects and difference-in-differences designs have yielded mixed results. Kubik (1999) finds that increases in SSI children's benefits lead to lower parental labor force participation, whereas Duggan and Kearney (2007) find no significant parental labor supply effects at children's program entry. In contrast, recent research using regression discontinuity designs has identified significant labor market responses at the intensive margin. Deshpande (2016a) finds that a $1,000 loss in SSI benefits leads to an increase in parental earnings of $700 to $1,400. Guldi et al. (2024) report a symmetric response to the receipt of SSI benefits: mothers' earnings decrease to fully offset the benefits received. Notably, the magnitude of these labor market adjustments exceeds both theoretical expectations and empirical findings in other contexts.[1] We observe both extensive and intensive margin effects of near-cash benefits for secondary earners. Secondary earners in dedicated account families work and earn more than their counterparts in cash families. Our findings suggest that near-cash benefits are valued less, resulting in a smaller income shock compared to cash benefits. Differences in parental labor market outcomes between families receiving these two benefit types may reflect changes in household consumption that affect children's well-being.

The paper proceeds as follows. Section 2 provides background on the SSI program, its provision of back pay, and our approach to analyzing the relative valuations of cash and near-cash benefits. Section 3 describes our data. Section 4 outlines our empirical method and explains its relevance and validity. Section 5 summarizes our main results and discusses the next steps for the paper. Finally, we conclude in Section 6.

[1]This body of work includes studies on the labor market responses to bequests (Holtz-Eakin et al. 1994), lotteries (Imbens et al. 2001; Cesarini et al. 2015; Picchio et al. 2018; Golosov et al. 2021), and transfers from other programs (Krueger and Pischke 1992; Bengtsson 2012; Jacob and Ludwig 2012; Gelber et al. 2016; Feinberg and Kuehn 2018; Powell 2020; Jones and Marinescu 2022; Lippold and Łuczywek 2023).

2 Institutional Background

2.1 SSI Children's Program and the Provision of Back Pay

The SSI program for children is a critical part of the U.S. safety-net. It provides financial support to approximately 1.3 million children, with annual payments totaling $10.5 billion. To qualify, children must be under 18 and meet both the disability criteria and the means test.[2] Recipients are entitled to monthly benefit payments with small supplements in some states as well as back pay for the time spent waiting for program approval. Monthly benefit amounts depend on both parental income and family structure. For a household with one parent and one child receiving SSI in 2012, benefits began to phase out at a 50% marginal tax rate once parental earnings exceeded $1,481 per month. Roughly 60% of children receive the maximum monthly benefit payment, known as the Federal Benefit Rate (FBR), which was $698 in 2012.[3] Back pay is calculated by multiplying the monthly benefit payment amount by the number of first business days in the months that recipients have waited for program approval.

The program's benefit structure underwent significant changes during the 1990's. In response to concerns raised by news reports about parents misusing back pay, Congress reformed the provision of back pay through the *Personal Responsibility and Work Opportunity Reconciliation Act of 1996*.[4] Under this reform, children owed less than or equal to six times the FBR would receive their back pay in cash. Children owed more than six times the FBR would receive their back pay deposited in separate bank accounts with spending restrictions. Six times the FBR amounts to $4,188 in 2012 dollars and represents 59% of mean annual total household earnings. Back pay is not considered part of family resources and, therefore, does not affect monthly benefit payments.

The separate bank accounts with cash-use stipulations are known as dedicated accounts. Funds in dedicated accounts can only be used for expenses related to SSI children's disability

[2]Youth may continue receiving SSI children's benefits until age 22 if they are attending school. However, at age 18, they undergo re-evaluation for the program using the adult medical criteria, and their financial eligibility is assessed based on their own assets and income rather than their parents'. For this paper, we define children as individuals under age 18.

[3]The FBR is adjusted annually for inflation.

[4]According to O'Connell (2017), two reports, in particular, set off public outcry. First, on February 4, 1994, acclaimed journalists Bob Woodward and Benjamin Weiser published an article in *The Washington Post* titled "Cost Soar for Children's Disability Program: How 26 Words Cost the Taxpayers Billions in New Entitlement Payments." The article highlighted instances of some parents using their children's back pay to purchase cars and televisions, rather than spending it on items that would directly benefit their children. Second, on October 13, 1994, ABC's *PrimeTime Live* aired a segment titled "Crazy Checks." The show featured interviews with school teachers and counselors who claimed that parents were coaching their children to behave in ways that would help them qualify for the SSI program and receive large benefit checks.

and well-being, such as their medical treatment, education, and job skills training.[5] Funds cannot be used for basic family maintenance, such as food, clothing, or shelter.[6] The use of dedicated account funds involves non-trivial administrative costs. Before funds can even be disbursed, parents must first open a separate bank account specifically for this purpose. They are required to sign and date Form SSA-552, the Dedicated Account Use of Funds Statement, to acknowledge their understanding of the appropriate use of these funds and their reporting responsibilities. Written approval from the SSA is required for all purchases that do not clearly fall into the allowable categories. Additionally, parents must submit Form SSA-6233-BK, the Representative Payee Report of Benefits and Dedicated Account, annually to document how the funds were used, along with purchase receipts. Any misused funds must be repaid in full.

Compliance with these purchasing rules is generally high. In a random sample of dedicated accounts opened between 2017 and 2021, the Office of the Inspector General (2023) found that SSA's system contained annual reports for 91% of these accounts. The majority also had written SSA approval for purchases that did not clearly fall into the allowable categories.[7] Given that SSA field officers are known to grant verbal permission for purchases made with dedicated account funds, the actual percentage of parents who received approval to use their near-cash benefits is likely higher.

2.2 Benefit Type and Wait Time

The amount of back pay owed to a child is based on her wait time for program approval, specifically the number of first-of-the-month days she has waited past. Since disability determination decisions are only made on business days, the back pay calculation is ultimately affected by the number of first business days in the elapsed months. Consider two similar children with the same household income and structure. One applied for SSI on January 31, 2012, and the other applied just one day later on February 1, 2012. Both received approval on August 1, 2012. If both children qualify for the FBR as their monthly benefit amount, the first child would receive seven times the FBR in back pay because she waited past seven firsts of the month for program approval. The second child, having waited past only six firsts of the month for program approval, would receive six times the FBR in back pay. This

[5]The complete list of allowable expenses is as follows: medical treatment, education, job skills training, and if related to the child's impairment: personal needs assistance, special equipment, housing modification, therapy, rehabilitation, or other items and services deemed appropriate by SSA (GN 00602.140).

[6]Exceptions are made in cases where SSI children face starvation or homelessness.

[7]Most purchases identified by the Office of the Inspector General (2023) as missing SSA's written permission pertained to housing. Some of these expenses included furniture and other housing modifications that may be important for children's health and could qualify as valid purchases under dedicated account policy rules.

difference in wait time creates quasi-random variation in the amount of back pay children receive. **Figure 1** illustrates the discontinuities in the amount of back pay FBR children receive for each additional first business day of the month they pass while waiting for program approval. In the figure, each point represents a business day, with the seventh first of the month normalized to zero. Since there are typically 20-22 business days in a month, the cutoffs for the other firsts of the month occur approximately every 21 business days. There are clear jumps in back pay amounts at each of these monthly cutoffs.

Children receive near-cash rather than cash benefits when their back pay exceeds six times the FBR. This is equivalent to waiting more than seven firsts of the month for program approval for children who receive the FBR as their monthly benefit amount. Since both benefit type and benefit amount change at this cutoff, we must disentangle the effects of benefit type from the effects of benefit amount in order to estimate the cash-equivalent value of near-cash benefits. We leverage two program features to accomplish this. First, the maximum amount of back pay that can be disbursed in the twelve months after program approval is capped at six times the FBR. This means that both cash and near-cash recipients receive the same benefit amount in Year 1 if they qualify for the FBR as their monthly benefit amount; differences in their payment amounts arise only in Year 2. Second, there are near-cash children at other monthly cutoffs who differ only in the benefit amount received in Year 2. We use these near-cash children to estimate what the benefit amount effects could be for children at our main cutoff.

We illustrate our identification strategy with the payment schedule for three groups of FBR children in **Figure 2**. The first group of children waited over six but less than seven firsts of the month for program approval, receiving back pay equal to six times the FBR in cash. The second group waited more than seven but less than eight firsts of the month, receiving seven times the FBR in near-cash. The third group waited over eight but less than nine firsts of the month and receives eight times the FBR in near-cash. The first and second groups represent the children at our main cutoff or the seventh-month cutoff. The first group is just to the left of the cutoff; the second group, just to the right. Both groups receive two lump-sum payments of three times the FBR during their first year. The key distinction is that the first group receives these payments in cash, while the second group receives them in near-cash. In Year 2, the second group receives an additional lump-sum payment of one FBR in near-cash. Since this additional payment could influence parents' behaviors in Year 1, we estimate the potential Year 1 benefit amount effects for children at our main cutoff using the second and third groups of children. Both groups receive near-cash benefits, with the third group receiving an additional FBR payment in Year 2. If benefit amount effects are similar in anticipation of each additional month of near-cash payments and children at both

cutoffs are comparable, then the estimated benefit amount effects at the eighth-month cutoff can approximate those at our main cutoff. We then isolate the cash vs. near-cash effects by subtracting the Year 1 benefit amount effects observed at the eighth-month cutoff from the Year 1 parental behavioral differences at our main cutoff. Given resource constraints and limited savings among low-income populations, the Year 1 benefit amount effects of future transfers are likely negligible (Meyer and Sullivan 2003).

2.3 Valuing Benefit Types

Valuing the two benefit types is challenging due to the lack of comprehensive consumption data linked to SSA administrative records. As a result, we rely on parents' labor supply decisions to estimate the cash-equivalent value of near-cash benefits. Back pay, whether received in cash or near-cash form, acts as an income shock that allows parents to reduce their labor supply. The extent of this reduction depends on how they value the back pay: higher valuations lead to larger income shocks and greater reductions in labor supply, while lower valuations result in smaller income shocks and more modest labor supply changes.

Two factors influence parents' relative valuations of cash and near-cash benefits. First, parents may value near-cash benefits less than cash benefits if they would allocate less to their children's health, education, and job skills training had they received the transfer in cash form. Second, near-cash benefits may impose additional administrative costs that diminish their overall transfer value. Anecdotally, accessing and using dedicated account funds require considerable time and effort.[8]

The empirical challenge in examining valuations using parental labor supply outcomes is that we can only observe changes during the months post-approval that remain in Year 1, since our outcome variables are measured annually. For instance, if a child is approved in June, they begin receiving all back pay and monthly benefits starting on the first business day of July. Consequently, our outcome variables only capture parental labor supply changes from July through December of that year. To address this limitation, we incorporate month-of-approval fixed effects into our empirical design and examine heterogeneity along this dimension.

[8]We consider the time and effort required to open a separate bank account as part of these administrative costs. The SSA data management system generates an alert when dedicated accounts are required but have not been created. Field officers then contact the parents to remind them to open accounts to receive the funds. The Office of the Inspector General (2010; 2019; 2023) found that some parents had not opened dedicated accounts even after repeated reminders from SSA field officers, ultimately forfeiting their children's back pay. Others had delayed creating accounts and receiving this back pay for years. Households that forgo or delay benefits are included in our analysis. Our valuations of cash vs. near-cash benefits capture both the lack of value perceived by parents who entirely forgo dedicated account funds and the diminished value for those who delay establishing accounts and receiving back pay during the first year of program approval.

3 Data

This project uses SSA microdata. Our main dataset is the Supplemental Security Record, which contains children's SSNs, application dates, decision dates, monthly benefit amounts, birth dates, gender, zip codes of residence, medical diagnoses, and parents' SSNs.[9] Using children's SSNs, we link the Supplemental Security Record to the Modernized SSI Claims System. The latter provides information on which child beneficiaries had dedicated accounts as well as information on account balances.

We use parents' SSNs to link child beneficiaries to their parents' labor supply data in the Master Earnings File, which contains employer and earnings information from parents' W-2 and 1040 Schedule SE forms. Our main outcomes include parents' employment status, annual wages, self-employment earnings, and total earnings. We assess these outcomes at the household level and separately for parents identified as primary and secondary earners based on their total earnings in the five years preceding their children's SSI approval.

To construct our analysis sample, we focus on child beneficiaries who applied for and were approved for SSI between 2010 and 2019. We choose this period for its data reliability, consistency in dedicated account rules, and the fact that it precedes the COVID-19 pandemic. We also restrict the dataset to first-time SSI recipients who did not receive benefits during their waiting period under the assumption of having qualifying disabilities.[10] This allows us to focus on children whose back pay type and amount were influenced by their program approval time. We further refine our sample to include only children who received the FBR, since these children would receive the same amount in their first year had they waited six or more months for program approval. 60% of children on SSI receive the FBR as their monthly benefit.

We use children who waited around seven months for benefit approval to estimate the cash-equivalent value of near-cash benefits and children who waited around eight months to bound any possible benefit amount effects. Based on current SSA application processing times, these children predominantly qualified for SSI during their initial review, although some were approved after their appeals.[11] To identify children who waited roughly seven or eight months for program approval, we use each child's application date to determine the first day she would have received near-cash benefits and the first day she would have

[9] We note that no racial data are available for children in the dataset, since the SSA stopped actively collecting racial information in the late 1980's.

[10] Strict medical regulations govern eligibility for these benefits, with only specific medical conditions qualifying. These benefits are not awarded based on financial need.

[11] If children are denied at the initial review, they can appeal the decision multiple times. The subsequent appeal stages include reconsideration, a hearing with a judge, a review of the hearing decision, and filing a lawsuit in federal district court.

become eligible for an additional month of near-cash benefits. We use these days to calculate hypothetical wait times, marking each child's exact seventh-month and eighth-month cutoff points. We then compare each child's actual program approval time to these benchmarks to determine her relative wait times around these two cutoffs. All our calculations are based on business days, since these are the only days on which the SSA operates. There are 28,015 children with benefit approval times in a 17-business-day window around the seventh-month cutoff, which we use for our main analysis, and 17,832 children in the 17-business-day window around the eighth-month cutoff, which we use to estimate the benefit amount effects.[12]

Table 1 presents the descriptive statistics for child beneficiaries at the two cutoff points as well as for all child beneficiaries who received program approval between 2010 and 2019. Child beneficiaries at the seventh- and eighth-month cutoffs exhibit a high degree of similarity across all covariates, including children's diagnoses, demographics, and parental labor market outcomes in the five years prior to program approval. Although our estimates are local to these cutoffs, the table indicates that child beneficiaries in the full sample are largely comparable to those at these cutoffs across most dimensions. The most notable differences are that children in the full sample are slightly more likely to have congenital and other conditions as their primary diagnosis, less likely to have mental conditions as their primary diagnosis, are younger on average, and come from households with higher total and wage earnings but lower self-employment earnings in the five years before program approval.

4 Empirical Strategy

To estimate the cash-equivalent value of near-cash benefits, we leverage variation in program approval time. Children who received approval before the first day of the seventh month are more likely to receive their back pay in cash form; those who waited longer are more likely to receive theirs in dedicated accounts.

4.1 Estimation

Our first-stage estimation equation is as follows:

$$\begin{aligned} NearCash_i = \alpha_1 + \beta_1 Cutoff_i + \gamma_1 WaitTime_i \\ + \delta_1 (Cutoff_i \times WaitTime_i) + \mathbf{X_i'}\Pi_1 + \varepsilon_i \end{aligned} \tag{1}$$

[12]To avoid including children affected by changes at multiple cutoffs, we use 17- instead of 21-business-day windows.

where $NearCash_i$ is an indicator variable for whether child i was required to open a dedicated account; $Cutoff_i$ is a variable that takes the value of one if child i waited past the first business day of the seventh month for benefit approval and zero otherwise; and $WaitTime_i$ is the number of business days between the seventh-month cutoff and when child i received SSI approval. $\mathbf{X_i'}$ is a vector of children's pre-treatment characteristics, including indicator variables for primary diagnosis codes, age, gender, household size, whether the application was approved at the initial review, and fixed effects for the month, year, and state of approval. While not necessary for an unbiased estimate, these variables increase precision. In this equation, the coefficient of interest is β_1, which measures the change in the likelihood that children receive their back pay in near-cash form at the seventh-month cutoff.

To estimate the reduced form for the outcome variables, we use the following equation:

$$\begin{aligned} Y_i = \alpha_2 + \beta_2 Cutoff_i + \gamma_2 WaitTime_i \\ + \delta_2(Cutoff_i \times WaitTime_i) + \mathbf{X_i'}\Pi_2 + \eta_i \end{aligned} \tag{2}$$

where Y_i is one of the outcome variables discussed above. To improve precision, we also include an autoregressive term for the previous year's outcome variable in the vector $\mathbf{X_i'}$. The coefficient of interest, β_2, is an intent-to-treat measure and captures how the outcome changes at the cutoff. The local average treatment effect is then given by $\frac{\beta_2}{\beta_1}$. We use a triangular kernel, giving greater weight to children closer to the cutoff. Our standard errors are clustered at the state level to account for intra-state correlations in dedicated account policy compliance and employment patterns.

To identify the causal difference in parents' valuations of cash vs. near-cash benefits, there must be randomness in wait times around the seventh-month cutoff and no other discontinuities at that point. In our case, near-cash children receive an additional month's worth of lump-sum payments in Year 2. We take two steps to account for this difference. First, we focus our analysis on parents' labor supply outcomes in Year 1, during which both groups of children receive the same total lump-sum payments, just in different forms. Second, to address the possibility that near-cash parents' Year 1 behavior may be influenced by their anticipation of the additional lump-sum payment in Year 2, we estimate and bound what the benefit amount effects could be in Year 1 for children at our main cutoff.

To estimate these benefit amount effects, we focus on the eighth-month cutoff, which includes children similar to those at the seventh-month cutoff. At the eighth-month cutoff, all children receive the same back pay amount in near-cash during Year 1. However, children who waited more than eight months for program approval receive an additional month's worth of near-cash payments in Year 2 compared to those who waited less. Using Equation 2,

we test whether parents adjust their labor supply in Year 1 in anticipation of the additional payments in Year 2. Note that the anticipation effect at the seventh month reflects the expectation of an additional month of near-cash payments in Year 2, while the anticipation effect at the eighth month accounts for the expectation of a second month's worth of near-cash payments in Year 2. If benefit amount effects are similar in anticipation of each additional month of near-cash payments, we can use the eighth-month estimates to bound the total effects observed at the seventh-month cutoff and identify parents' valuations of cash vs. near-cash benefits.

One of the identification assumptions for the exactly identified local average treatment effect is that crossing the seventh-month cutoff affects parental labor market outcomes solely through the use of dedicated accounts. Since this assumption may not hold in our case, we emphasize the reduced form effects in our paper.

4.2 Relevance

In order to estimate the cash-equivalent value of near-cash benefits, there must be a first-stage effect in the receipt of dedicated accounts at the seventh-month cutoff. **Figure 3** illustrates the fraction of children receiving dedicated accounts at this cutoff. In the figure, each point represents a business day, with the seventh-month cutoff normalized to zero. The share of child beneficiaries assigned dedicated accounts jumps by 32.5 percentage points from five business days before the cutoff to immediately after it. We shade the five points immediately to the left of the cutoff in gray. They represent cases where children were incorrectly assigned dedicated accounts, despite not qualifying for them.

Field officers frequently make errors when assigning dedicated accounts. Instead of limiting assignments to children who receive more than six times the FBR in back pay or those at the seventh-month cutoff, they also assign accounts to children receiving exactly six times the FBR or those at the sixth-month cutoff. Other assignment errors appear to be more random.[13] The high number of errors may be attributed to understaffing, low morale, and high turnover among SSA field officers.[14] These misassignments contribute to an increase in

[13]We sent SSA's operations team a random sample of children we believed were misassigned and requested explanations for why they received cash or near-cash benefits. The team acknowledged that some of these assignments seemed to be errors but could not determine the reasons behind them.

[14]The SSA is understaffed and has faced hiring challenges in recent years due to federal budget constraints. The agency's compensation is often not competitive. According to SSA's 2019 Federal Viewpoint Survey, 36% of respondents felt their workload was unreasonable, 38% did not believe that differences in performance were meaningfully recognized, and 28% felt employees were not acknowledged for providing high-quality products and services. These factors contribute to low morale and potential attrition, and overall dissatisfaction in these areas has remained consistent in each annual survey. In a new question included in the 2023 survey, 35% of respondents indicated they were considering retiring or seeking another job within the next year.

the fraction of children receiving dedicated accounts before the seventh-month cutoff. Consequently, following Desphande (2016a) and Desphande et. al (2021), we employ a "half-donut" fuzzy regression discontinuity design that excludes children in the five business days to the left of the seventh-month cutoff. Our corresponding regression results are presented in **Table 2**. The F-statistic is 356, indicating the strength of the first-stage regression.

4.3 Validity

Our empirical design assumes that wait times around each month's cutoff are random. A threat to identification arises if either parents or the SSA can manipulate children's program approval times. However, this is unlikely in practice. Parents cannot time their applications to ensure approval in a specific month or day. Additionally, due to SSA understaffing and backlogs, no office can expedite or delay application processing. In fact, Autor et al. (2015) document large variations in wait times. We use the McCrary Density Test to examine whether the density is discontinuous at the seventh-month cutoff and present the results in **Figure 4**. Although we observe a slight bump past the seventh-month cutoff, there is no statistically significant discontinuity at this point. Our investigations indicate that this bump likely reflects SSA field office processing patterns, where there are more applications and approvals at the beginning of the month than at the end, when another month of back pay is triggered. A similar bump appears at the eighth-month cutoff in **Appendix Figure A.1**, though there is a marginally significant discontinuity at this point. Overall, our results do not provide evidence of manipulation in the running variable.

As further support, we examine whether children's characteristics and parents' labor market outcomes in the five years preceding SSI approval are balanced across the seventh-month cutoff. We use Equation 2 for this analysis, treating each covariate as Y_i and including fixed effects for the month, year, and state of approval in $\mathbf{X_i'}$.[15] As before, we apply a half-donut design, excluding the five business days immediately before the cutoff. **Table 3** presents the covariate balance tests, while **Figures 5-7** display the corresponding regression discontinuity graphs. Most covariates are balanced across the cutoff. However, percent respiratory, percent female, and household size exhibit at least marginally significant, though generally small, differences. To determine whether these findings reflect true differences between cash and near-cash children, we evaluate the joint significance of all the covariates. Following Kling et al. (2007), we construct an index variable with all the characteristics. Our analysis indicates that the covariates are not jointly significant, suggesting that the

[15]To ensure consistency in our analyses of parents' labor supply decisions, we include children's diagnoses and demographic characteristics in the regression specifications for parents' pre-treatment labor market outcomes.

observed differences are likely due to chance rather than manipulation. Additionally, we test for balance in children's characteristics and parents' pre-treatment labor market outcomes at the eighth-month cutoff. The corresponding results, presented in **Appendix Table A.1** and **Appendix Figures A.2-4**, show balance across all the covariates.

5 Results

5.1 Main Results

Table 4 and **Appendix Table A.2** present the main employment and earnings outcomes at the seventh- and eighth-month cutoffs. **Figures 8-10** along with **Appendix Figures A.5-7** display the corresponding regression discontinuity plots. When examining all households, we do not observe large differences in parental labor market outcomes between cash and near-cash families. In fact, we can rule out differences greater than 2 percentage points in the likelihood of any parent being employed, $400 in total household earnings, $600 in total wage earnings, and $400 in total self-employment earnings during the first year of children's program approval. However, these small differences likely reflect the fact that many parents in these households do not actively participate in the labor force and, thus, have limited ability to adjust their labor supply in response to income shocks.

Consequently, we focus on the labor supply outcomes of parents who were employed in the year preceding their children's program approval, since they may be more responsive to income shocks. We find no significant differences for primary earners in dedicated account families, and large standard errors limit our ability to rule out meaningful effects in either direction. In contrast, we observe notable effects for secondary earners: those in dedicated account families are 10.8 percentage points more likely to work in the first year compared to their counterparts in cash families. They also earn approximately $1,470 more that year, increasing their total annual earnings by roughly 17%.

We do not detect significant benefit amount effects at the eighth-month cutoff for all households, primary earners, or secondary earners. Due to the large standard errors for many of these estimates, we cannot rule out the possibility that parents might adjust their labor supply decisions in anticipation of an additional month's worth of payments in the second year. Therefore, the total effects observed at the seventh-month cutoff likely represent a lower bound on the true near-cash effects.

What does it mean when benefit type effects are only detectable among secondary earners? There are two possible explanations: either the effective income shock varies depending on the number of earners in the household or it remains constant across different family

structures. However, the needs of children with the same type of disability do not vary based on the number of earners in the family. Additionally, time and resource constraints are likely more pressing for households with no active earners or only a single earner. The more plausible explanation is that secondary earners have a more elastic labor supply, allowing them to adjust their work participation and hours more readily in response to income shocks. This interpretation aligns with Guldi et al. (2024), who find significant intensive margin labor market effects for mothers but no effects for fathers when low-birthweight infants enter SSI, although they do not differentiate between single-earner and secondary-earner mothers. In contrast, Deshpande (2016a) finds similar intensive-margin effects for all households and those headed by single mothers when children under age 13 lose SSI benefits. The differences between our findings and previous research highlight the need to explore our extensive margin effects further and examine how benefit types impact earners by gender.[16]

If the effective income shock is uniform across family structures, there may still be valuation differences between cash and near-cash benefits for households with no active earners or a sole earner. Dedicated accounts may reduce household consumption in ways we cannot currently measure. Although Deshpande (2016a) and Guldi et al. (2024) report different findings on effect heterogeneity, both studies establish a nearly one-to-one replacement of SSI children's benefits with changes in parental labor supply. Given that the total earnings drop for secondary earners in households receiving near-cash benefits is $1,470 less than that for their counterparts in cash households, the $4,188 in back pay they receive in Year 1 is effectively valued at only $2,718. Thus, our back-of-the-envelope calculations suggest that a near-cash dollar is valued at only 65% of a cash dollar.[17]

5.2 Next Steps

We are currently examining effect heterogeneity and robustness. The following section outlines our analysis plan. We will discuss our findings in future versions of the paper.

We will explore heterogeneity based on the month of approval. Since our outcome variables are measured annually, they only capture parental labor supply adjustments in the months following children's program approval. This makes it harder to detect changes for parents of children approved later in the year, since fewer months are available for observation. Consequently, we anticipate larger labor market responses and valuation differences among parents of children approved earlier in the year compared to those approved later. To examine this, we will compare valuation differences separately for children approved in the

[16] Currently, we only have the parent-child SSN linkage file. We will merge in parent characteristics for future analyses.

[17] We calculate this as $\frac{\$2,718}{\$4,188} \approx 0.65$.

first six months vs. the last six months of each year. We will also interact the discontinuity with the number of months treated to assess how each additional month affects valuation differences between cash and near-cash benefits.

Next, we will also explore heterogeneity in parental labor market responses based on children's medical diagnoses. Child needs vary by disability type, which affects both the household income required to support the child and the amount of time parents must dedicate to caregiving. Previous research has shown that parents of children with physical disabilities have lower earnings elasticities than parents of children with mental disabilities (Rupp and Ressler 2009; Deshpande 2016a). These differences may also affect how parents respond to cash vs. near-cash benefits. We will analyze the relative effects of these benefit types separately for children with each type of diagnosis.

We will also report results by child age. Older children may receive and spend the transfers themselves, making them less dependent on household income. Consistent with this, Deshpande (2016b) does not find significant parental labor supply responses to the loss of SSI benefits for youth aged 18, when they are re-evaluated for the program under the adult disability criteria and assessed for financial eligibility using their own assets and income. This weaker connection suggests that valuation differences between cash and near-cash benefits may be smaller for parents of older children compared to those with younger children. To assess this, we will categorize children by developmental stage: early (newborns to age 5) and late (ages 6 to 17) and report valuation differences separately for each group.

Additionally, we will examine heterogeneity in cash vs. near-cash effects based on differences in compliance with dedicated account policy rules across field offices. Higher compliance entails greater administrative costs and leads to increased consumption distortions. Field offices will be categorized based on data on household purchases made with dedicated account funds: offices with more missing approval data or approvals for purchases outside the allowable categories will be classified as low-compliance, while those with stricter adherence to the policy rules will be considered high-compliance. We will assess the relative effects of benefit type separately for children served by these two field office categories.

We will also test the robustness of our main results by using alternative bandwidth and donut choices.

6 Conclusion

We study how parents value cash vs. near-cash benefits in the SSI program for children with disabilities. In this program, children receive either cash or near-cash back pay depending on how long they have waited for program approval. Near-cash benefits are held in dedicated

accounts and are restricted to expenditures on children's medical treatment, education, and job skills training. These benefits come with non-trivial administrative costs: parents must obtain SSA approval for purchases that do not clearly fall within these categories, submit annual reports, and repay any misused funds.

We use parental labor market outcomes from SSA microdata to assess parents' relative valuations of cash vs. near-cash benefits. Since back pay represents an income shock, the value parents place on these benefits can influence their labor supply decisions. If parents value near-cash benefits less than cash benefits, they may work more after receiving near-cash benefits compared to cash benefits. Our analysis leverages the discontinuity in program approval time that determines whether children receive cash or near-cash benefits and employs a fuzzy regression discontinuity design to estimate the relative benefit type effects.

We find substantial differences in how households value cash vs. near-cash benefits. Secondary earners in near-cash families are more likely to work and earn more during the year. Although we only detect significant differences in labor market outcomes for this group of parents, this does not preclude potential valuation differences in households with no active earners or a sole earner. The observed differences in labor market outcomes may be more pronounced for secondary earners due to their greater labor elasticity. Back-of-the-envelope calculations suggest that one dollar in near-cash benefits is worth only 65% of a dollar in cash.

Our findings have important implications for benefit type provision in safety-net programs. Since parents value cash and near-cash benefits differently, consumption patterns are likely to differ between households receiving these two benefit types. These differences can manifest in spending on children-specific goods such as healthcare, education, and job skills training, non-children-specific goods like food and housing, and the time parents spend with their children. Each of these factors can impact children's welfare. Future research should examine the direct effects of these benefit types on children's well-being.

7 References

Autor, David H., Nicole Maestas, Kathleen J. Mullen, and Alexander Strand. 2015. "Does Delay Cause Decay? The Effect of Administrative Decision Time on the Labor Force Participation and Earnings of Disability Applicants." *NBER Working Paper*.

Beatty, Timothy K. M. and Charlotte J. Tuttle. 2015. "Expenditure Response to Increases in In-Kind Transfers: Evidence from the Supplemental Nutrition Assistance Program." *American Journal of Agricultural Economics*, 97(2): 390–404.

Bengtsson, Niklas. 2012. "The Marginal Propensity to Earn and Consume out of Unearned Income: Evidence Using an Unusually Large Cash Grant Reform." *The Scandinavian Journal of Economics*, 114(4): 1393–1413.

Bitler, Marianne, Amelia Hawkins, Lucie Schmidt, and Hilary Seligman. 2022. "Cash vs. Food? How Does Food Stamp Eligibility Affect Food Stamp Enrollment and Food and Health Outcomes of SSI Recipients?" *NBER Working Paper*.

Bruich, Gregory A. 2014. "The Effect of SNAP Benefits on Expenditures: New Evidence from Scanner Data and the November 2013 Benefit Cuts." *In Mimeo*.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2015. "Optimal Data-Driven Regression Discontinuity Plots." *Journal of the American Statistical Association*, 110: 1753–1769.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma. 2018. "Manipulation Testing Based on Density Discontinuity." *Stata Journal*, 18: 234–261.

Cesarini, David, Erik Lindqvist, Matthew J. Notowidigdo, and Robert Östling. 2017. "The Effect of Wealth on Individual and Household Labor Supply: Evidence from Swedish Lotteries." *American Economic Review*, 107(12): 3917–3946.

Cunha, Jesse M. 2014. "Testing Paternalism: Cash Versus In-Kind Transfers." *American Economic Journal: Applied Economics*, 6(2): 195–230.

Currie, Janet. "Welfare and the Well-Being of Children: The Relative Effectiveness of Cash and In-Kind Transfers," in *Tax Policy and the Economy*, Volume 8. 1994.

Currie, Janet. "U.S. Food and Nutrition Programs," in *Means-Tested Transfer Programs in the United States*, edited by Robert Moffitt. 2003.

Currie, Janet and Firouz Gahvari. 2008. "Transfers in Cash and In-Kind: Theory Meets the Data." *Journal of Economic Literature*, 46(2): 333-383.

Deshpande, Manasi. 2016a. "The Effect of Disability Payments on Household Earnings and Income: Evidence from the Supplemental Security Income Children's Program." *Review of Economics and Statistics*, 98(4): 638-654.

Deshpande, Manasi. 2016b. "Does Welfare Inhibit Success? The Long-Term Effects of

Removing Low- Income Youth from Disability Insurance." *American Economic Review*, 106(11): 3300-3330.

Deshpande, Manasi, Tal Gross, and Yalun Su. 2021. "Disability and Distress: The Effect of Disability Programs on Financial Outcomes." *American Economic Journal: Applied Economics*, 13(2): 151-178.

Duggan, Mark and Melissa Kearney. 2007. "The Impact of Child SSI Enrollment on Household Outcomes." *Journal of Policy Analysis and Management*, 26(4): 861-886.

Duggan, Mark, Melissa Kearney, and Stephanie Rennane. "The Supplemental Security Income Program," in *Economics of Means-Tested Transfer Programs in the United States*, Volume 2, edited by Robert Moffitt. 2016.

Feinberg, Robert M. and Daniel Kuehn. 2018. "Guaranteed Nonlabor Income and Labor Supply: The Effect of the Alaska Permanent Fund Dividend." *The B.E. Journal of Economic Analysis & Policy*: 18(3): 20180042.

Gahvari, Firouz. 1994. "In-Kind Transfers, Cash Grants and Labor Supply." *Journal of Public Economics*, 55: 495-504.

Gelber, Alexander M., Adam Isen, and Jae Song. 2016. "The Effect of Pension Income on Elderly Earnings: Evidence from Social Security and Full Population Data." *In Mimeo.*

Golosov, Mikhail, Michael Graber, Magne Mogstad, and Davis Novgorodsky. 2021. "How Americans Respond to Idiosyncratic and Exogenous Changes in Household Wealth and Unearned Income?" *NBER Working Paper.*

Greater Boston Legal Services. "Materials from Roundtable Presenters." Accessed March 19, 2021. https://www.ssab.gov/research/roundtable-reexamining-long-standing-dedicated-account-challenges/.

Greater Boston Legal Services. "Tackling a Systemic Problem: How the Children's Disability Project Is Fighting the Dedicated Account." Accessed January 5, 2023. https://childrensdisabilityproject.wordpress.com/2019/09/11/tackling-a-systemic-problem-how-the-childrens-disability-project-is-fighting-the-dedicated-account/.

Guldi, Melanie, Amelia Hawkins, Jeffrey Hemmeter and Lucie Schmidt. 2024. "Supplemental Security Income for Children, Maternal Labor Supply, and Family Well-Being: Evidence from Birth Weight Eligibility Cutoffs." *Journal of Human Resources*, 59(4): 975-1010.

Hastings, Justine and Jesse M. Shapiro. 2018. "How Are SNAP Benefits Spent? Evidence from a Retail Panel." *American Economic Review*, 108(12): 3493-3540.

Holtz-Eakin, Douglas, David Joulfaian, and Harvey S. Rosen. 1993. "The Carnegie Conjecture: Some Empirical Evidence." *The Quarterly Journal of Economics*, 108(2): 413–435.

Hoynes, Hilary W. and Diane Whitmore Schanzenbach. 2009. "Consumption Responses to In-Kind Transfers: Evidence from the Introduction of the Food Stamp Program." *American Economic Journal: Applied Economics*, 1(4): 109–139.

Imbens, Guido W., Donald B. Rubin, and Bruce I. Sacerdote. 2001. "Estimating the Effect of Unearned Income on Labor Earnings, Savings, and Consumption: Evidence from a Survey of Lottery Players." *American Economic Review*, 91(4): 778–794.

Jacob, Brian A. and Jens Ludwig. 2012. "The Effects of Housing Assistance on Labor Supply: Evidence from a Voucher Lottery." *American Economic Review*, 102(1): 272–304.

Jones, Damon and Ioana Marinescu. 2022. "The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund." *American Economic Journal: Economic Policy*, 14(2): 315–340.

Kling, Jeffrey, Jeffrey Liebman, and Lawrence Katz. 2007. "Experimental Analysis of Neighborhood Effects." *Econometrica*, 75(1): 83-119.

Krueger, Alan B. and Jörn-Steffen Pischke. 1992. "The Effect of Social Security on Labor Supply: A Cohort Analysis of the Notch Generation." *Journal of Labor Economics*, 10(4): 412–437.

Kubik, Jeffrey D. 1999. "Incentives for the Identification and Treatment of Children with Disabilities: The Supplemental Security Income Program." *Journal of Public Economics*, 73:187–215

Lee, David S. and Thomas Lemieux. 2020. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature*, 48(2): 281-355.

Lepidus Carlson, Barbara, Leslie Foster, Stacy B. Dale, and Randall Brown. 2007. "Effects of Cash and Counseling on Personal Care and Well-Being." *Health Services Research*, 42(1): 467-487.

Leroy, Jef L., Paola Gadsden, Sonia Rodríguez-Ramírez, and Teresa González de Cossío, 2010. "Cash and In-Kind Transfers in Poor Rural Communities in Mexico Increase Household Fruit, Vegetable, and Micronutrient Consumption but Also Lead to Excess Energy Consumption." *Journal of Nutrition*, 140(3): 612-617.

Lieber, Ethan M. J. and Lee M. Lockwood. 2019. "Targeting with In-Kind Transfers: Evidence from Medicaid Home Care." *American Economic Review*, 109(4): 1461–1485.

Lippold, Kye and Beata Łuczywek. 2023. "Estimating Income Effects on Earnings using the 2021 Child Tax Credit Expansion." *In Mimeo.*

McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics*, 142(2): 698-714.

Meyer, Bruce D. and James X. Sullivan. 2003. "Measuring the Well-Being of the Poor Using Income and Consumption." *Journal of Human Resources*, 38: 1180–1220.

Moffitt, Robert. 1989. "Estimating the Value of an In-Kind Transfer: The Case of Food Stamps." *Econometrica*, 57(2): 385–409.

O'Connell, Mary E. 2017. "Supplemental Security Income's 'Dedicated Account': A Debunked Urban Legend and Twenty Years of Waste." *Northeastern University School of Law Research Paper*.

Office of the Inspector General, Social Security Administration. 2010. "Dedicated Account Underpayments Payable to Children." Report No. A-09-09-29110. https://oig-files.ssa.gov/audits/full/A-09-09-29110_7.pdf.

Office of the Inspector General, Social Security Administration. 2019. "Dedicated Account Underpayments Payable to Children Receiving Supplemental Security Income." Report No. A-04-18-50607. https://oig-files.ssa.gov/audits/full/A-04-18-50607.pdf.

Office of the Inspector General, Social Security Administration. 2023. "Dedicated Accounts for Supplemental Security Income Recipients." Report No. A-04-21-51031. https://oig.ssa.gov/assets/uploads/a-04-21-51031.pdf.

Picchio, Matteo, Sigrid Suetens, and Jan C. van Ours. 2018. "Labour Supply Effects of Winning a Lottery." *The Economic Journal*, 128(611): 1700–1729.

Powell, David. 2020. "Does Labor Supply Respond to Transitory Income? Evidence from the Economic Stimulus Payments of 2008." *Journal of Labor Economics*, 38(1): 1–38.

Rupp, Kalman and Steve Ressler. 2009. "Family Caregiving and Employment among Parents of Children with Disabilities on SSI." *Journal of Vocational Rehabilitation*, 30: 153–175.

Schanzenbach, Diane W. 2002. "What Are Food Stamps Worth?" *Princeton University Industrial Relations Section Working Paper*.

Smeeding, Timothy M. 1982. "Alternative Methods for Valuing Selected In-Kind Transfer Benefits and Measuring Their Effect on Poverty." *U.S. Bureau of the Census Technical Paper*.

Social Security Administration. "2019 Federal Employee Viewpoint Survey Results." Accessed October 10, 2024. https://www.ssa.gov/careers/assets/materials/fevs/2019-fevs.pdf.

Social Security Administration. "2020 Federal Employee Viewpoint Survey Results." Accessed October 10, 2024. https://www.ssa.gov/careers/assets/materials/fevs/2020-fevs.pdf.

Social Security Administration. "2021 Federal Employee Viewpoint Survey Results." Accessed October 10, 2024. https://www.ssa.gov/careers/assets/materials/fevs/2021-fevs.pdf.

Social Security Administration. "2022 Federal Employee Viewpoint Survey Results." Accessed October 10, 2024. https://www.ssa.gov/careers/assets/materials/fevs/2022-fevs.pdf.

Social Security Administration. "2023 Federal Employee Viewpoint Survey Results." Accessed October 10, 2024. https://www.ssa.gov/careers/assets/materials/fevs/2023-fevs.pdf.

Social Security Administration. "GN 00602.140 Permitted Expenditures from Dedicated Accounts." Last Modified April 20, 2022. https://secure.ssa.gov/poms.nsf/lnx/0200602140.

Social Security Administration. "SI 02101.010 Past-Due Benefits Payable - Individual Alive Under Age 18 with Representative Payee - Dedicated Account Required." Last Modified January 13, 2014. https://secure.ssa.gov/apps10/poms.nsf/lnx/0502101010.

Social Security Administration. "SI 02101.020 Large Past-Due Supplemental Security Income (SSI) Payments by Installments – Individual Alive." Last Modified February 7, 2014. https://secure.ssa.gov/poms.nsf/lnx/0502101020.

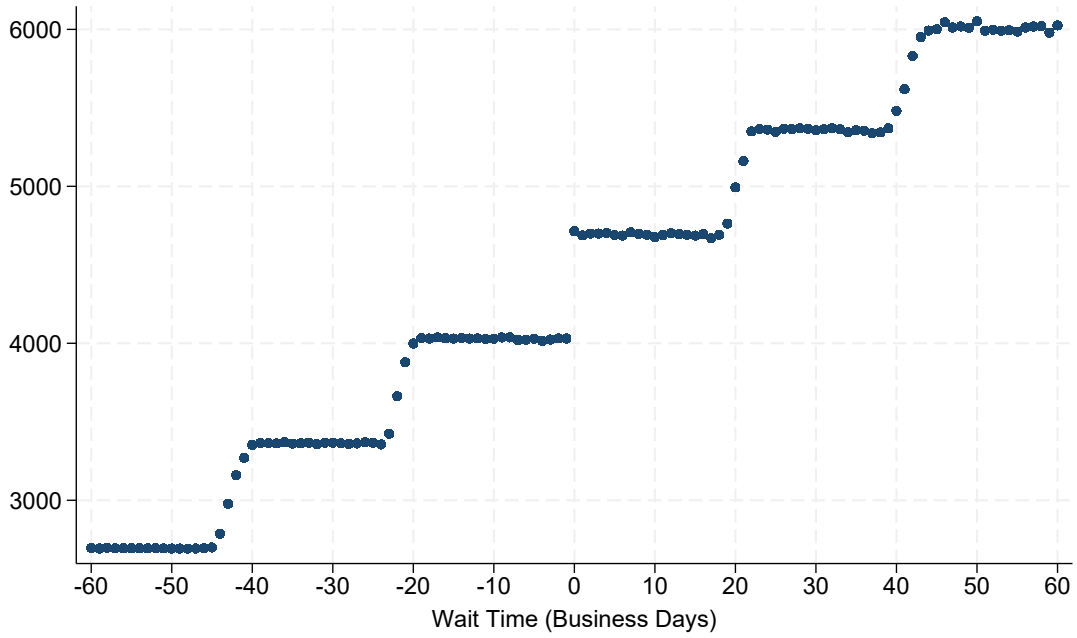
Social Security Administration. "SI 02220.060 Misapplication of Funds in a Dedicated Account." Last Modified October 16, 2015. https://secure.ssa.gov/apps10/poms.nsf/lnx/0502220060.

Skoufias, Emmanuel and Teresa González-Cossío. 2008. "The Impacts of Cash and In-Kind Transfers on Consumption and Labor Supply: Experimental Evidence from Rural Mexico." *World Bank Policy Research Working Paper*.

Wilde, Parke E. and Christine K. Ranney. 1996. "The Distinct Impact of Food Stamps on Food Spending." *Journal of Agricultural and Resource Economics*, 21(1): 174–185.

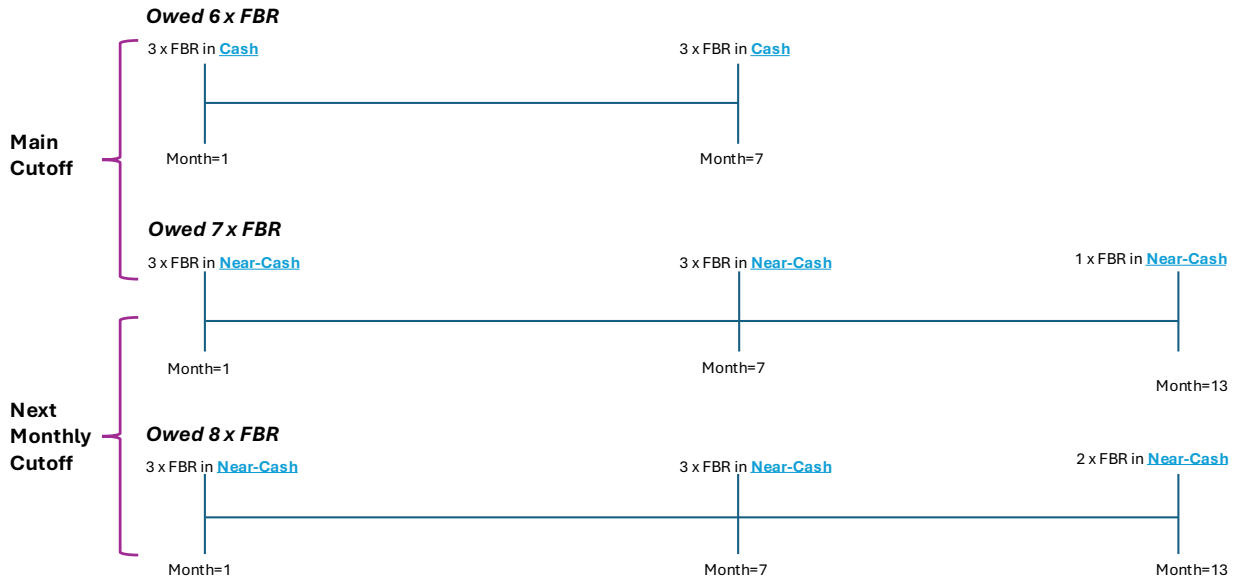
8 Figures

Figure 1: Back Pay Amounts by Wait Time (2012 Dollars)



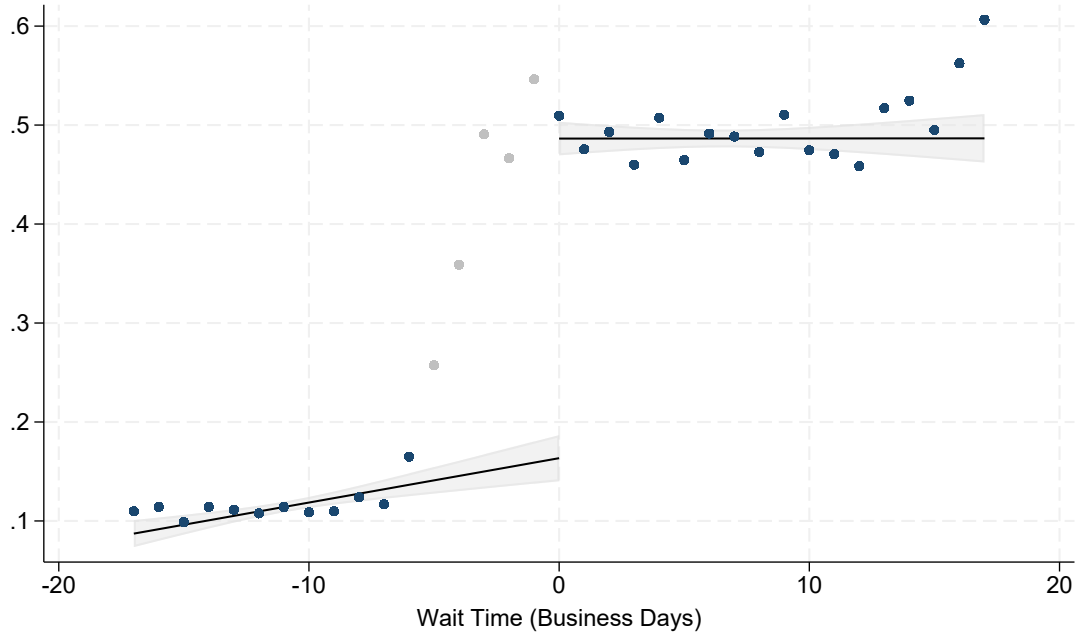
Notes: This figure displays the average total back pay amounts received by children, adjusted to 2012 dollars, based on their wait time in business days relative to the first day of the seventh month.

Figure 2: Back Pay Disbursement Schedule



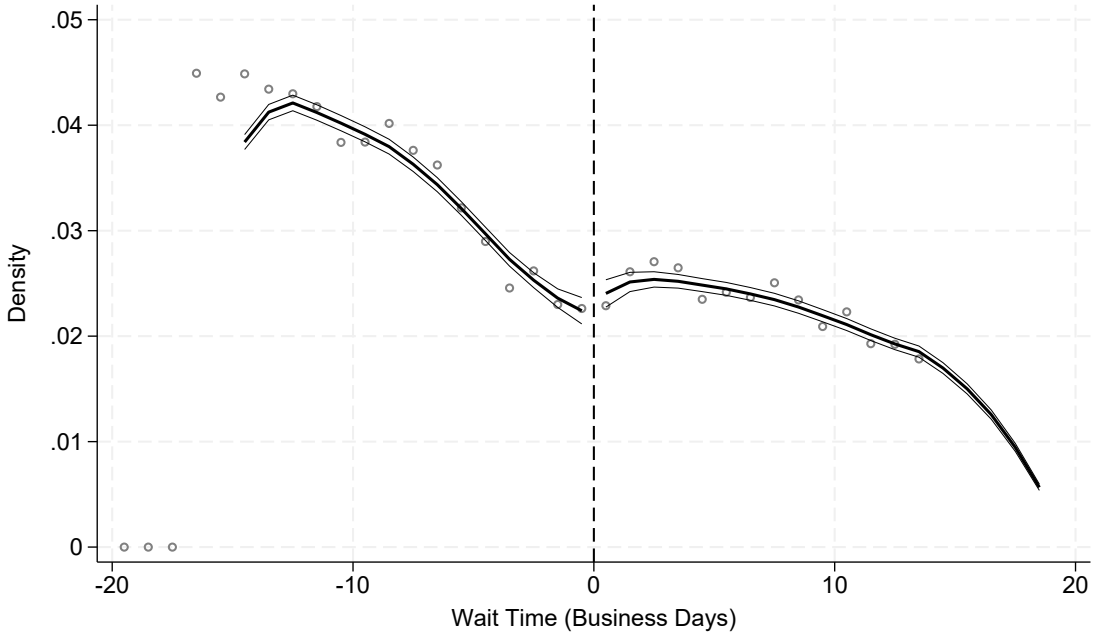
Notes: This figure shows the back pay disbursement schedule for children at two cutoffs: our main cutoff (the seventh-month cutoff) and the next monthly cutoff (the eighth-month cutoff). Children at the main cutoff are owed either six or seven times the FBR, while those at the next monthly cutoff are owed either seven or eight times the FBR.

Figure 3: Fraction of Children Receiving Dedicated Accounts



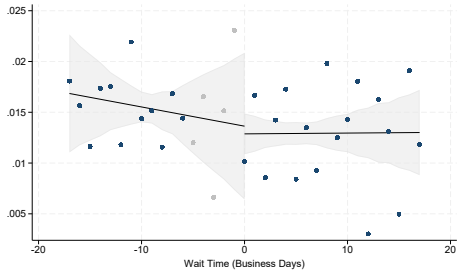
Notes: This figure shows the fraction of children receiving dedicated accounts based on their wait time in business days relative to the first day of the seventh month. The sample includes children within a seventeen-business-day window around this cutoff. The five business days immediately to the left of the cutoff are shaded in gray and fall within our half-donut exclusion region. See Table 1 for a complete description of our data sample.

Figure 4: McCrary Density Test at the Seventh-Month Cutoff

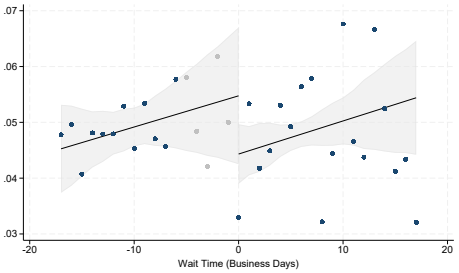


Notes: This figure presents the results of an examination of potential manipulation in the density around the cutoff. Each business day of wait time is measured relative to the first day of the seventh month. The sample includes children within a seventeen-business-day window around this cutoff. The discontinuity point estimate is 0.064 with a standard error of 0.050.

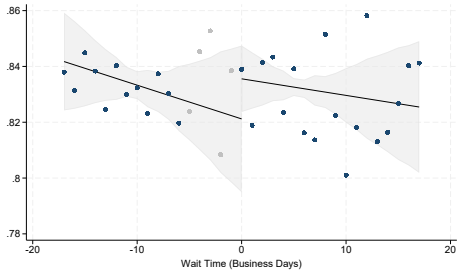
Figure 5: Regression Discontinuity Balance Plots for Children's Diagnoses at the Seventh-Month Cutoff



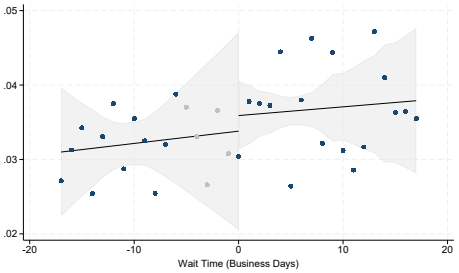
(a) Congenital



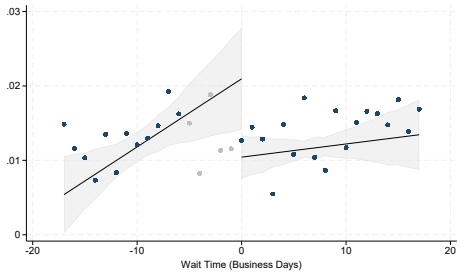
(b) Other



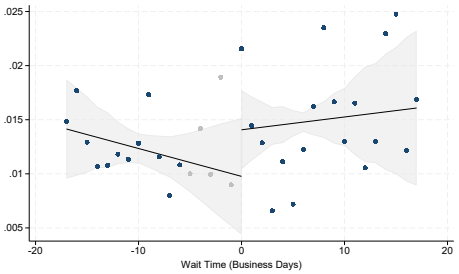
(c) Mental



(d) Nervous



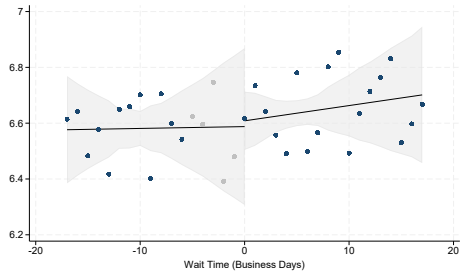
(e) Respiratory



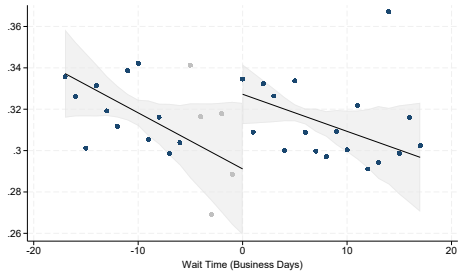
(f) Sensory

Notes: The figure shows the average for each business day of wait time relative to the first day of the seventh month. The sample includes children within a seventeen-business-day window around this cutoff. The five business days immediately to the left of the cutoff are shaded in gray and are part of our half-donut exclusion. Only diagnoses constituting more than 1% of the sample are displayed. The remaining categories—Blood, Circulatory, Digestive, Endocrine, Genitourinary, Infection, Injury, Musculoskeletal, Neoplasm, and Skin—are not statistically significant. See Table 1 for a complete description of our data sample.

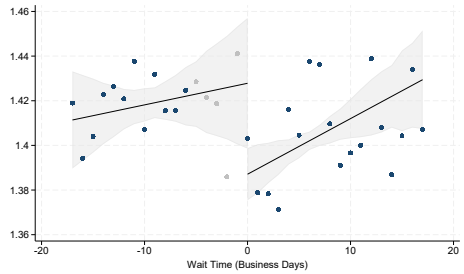
Figure 6: Regression Discontinuity Balance Plots for Children's Demographics at the Seventh-Month Cutoff



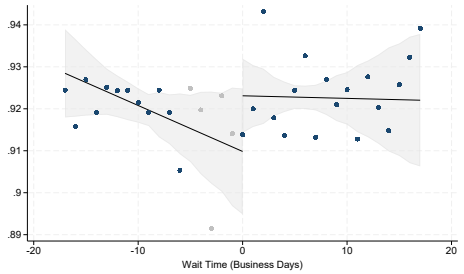
(a) Age



(b) Female



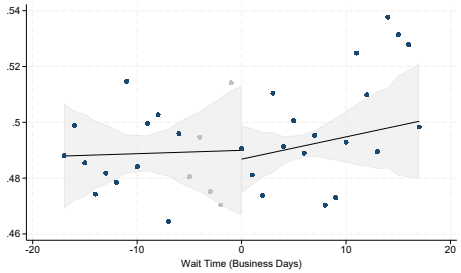
(c) Household Size



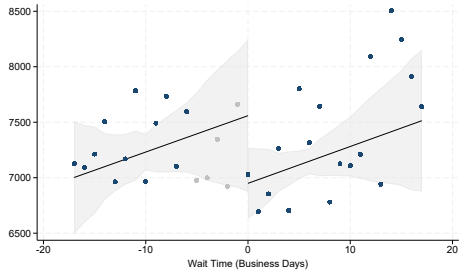
(d) Initially Allowed

Notes: The figure shows the average for each business day of wait time relative to the first day of the seventh month. The sample includes children within a seventeen-business-day window around this cutoff. The five business days immediately to the left of the cutoff are shaded in gray and are part of our half-donut exclusion. See Table 1 for a complete description of our data sample.

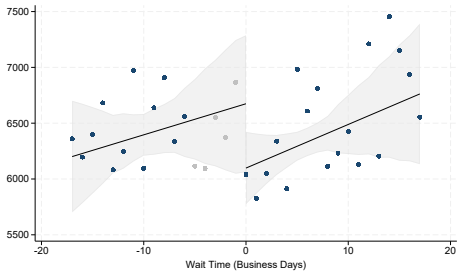
Figure 7: Regression Discontinuity Balance Plots for Parents' Pre-Treatment Outcomes at the Seventh-Month Cutoff



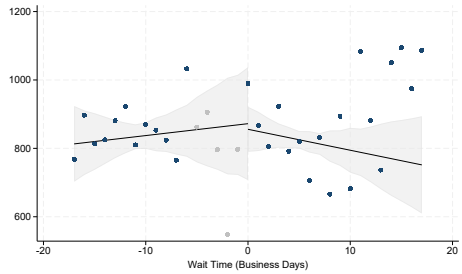
(a) Any Employment



(b) Total Earnings



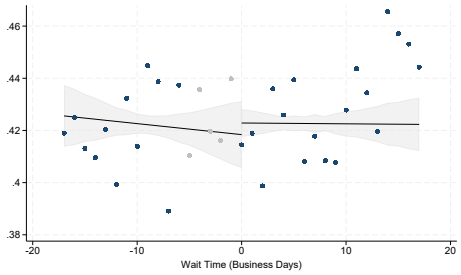
(c) Wage Earnings



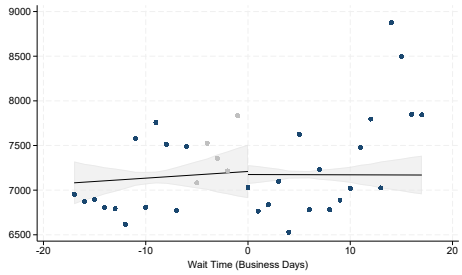
(d) Self-Employment Earnings

Notes: The figure displays the average wait time in business days relative to the first day of the seventh month. The sample includes children within a seventeen-business-day window around this cutoff. The five business days immediately to the left of the cutoff are shaded in gray and are part of our half-donut exclusion. Pre-treatment outcomes reflect whether parents have ever worked and their average earnings over the five years prior to children's program approval. Earnings are adjusted to 2012 dollars. See Table 1 for a complete description of our data sample.

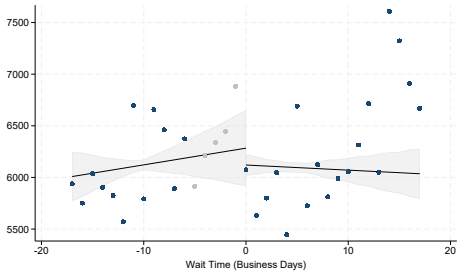
Figure 8: Reduced Form Effects on Employment Status and Earnings for All Households at the Seventh-Month Cutoff



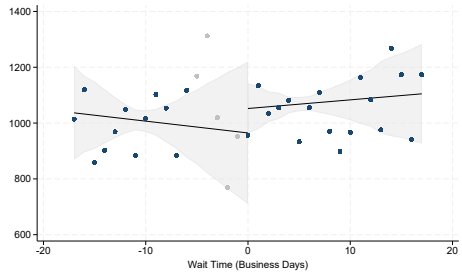
(a) Any Employment



(b) Total Earnings



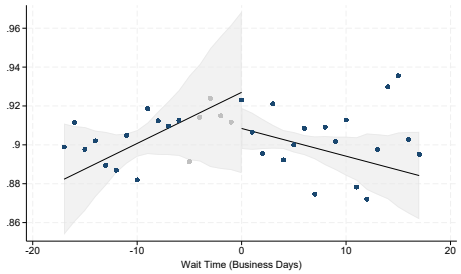
(c) Wage Earnings



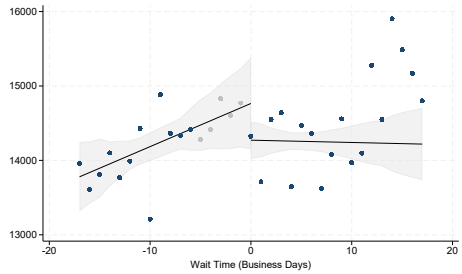
(d) Self-Employment Earnings

Notes: The figure shows the average for each business day of wait time relative to the first day of the seventh month. The sample includes children within a seventeen-business-day window around this cutoff. The five business days immediately to the left of the cutoff are shaded in gray and are part of our half-donut exclusion. Earnings have been adjusted to 2012 dollars. See Table 1 for a complete description of our data sample.

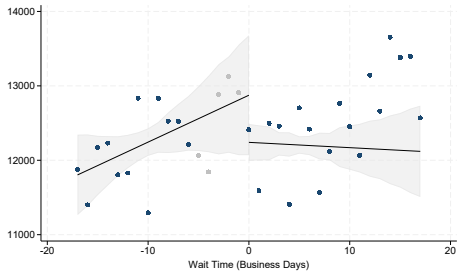
Figure 9: Reduced Form Effects on Primary Earner's Employment Status and Earnings at the Seventh-Month Cutoff



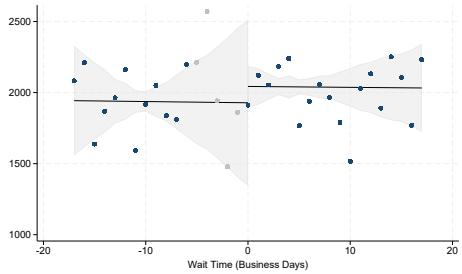
(a) Employment Status



(b) Total Earnings



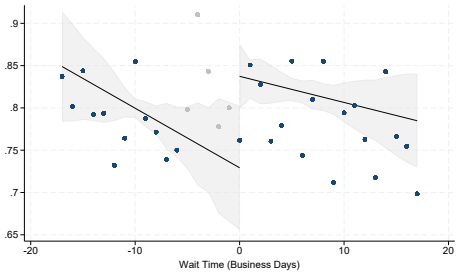
(c) Wage Earnings



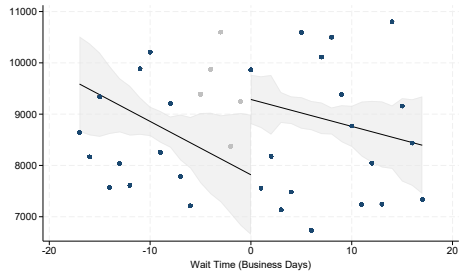
(d) Self-Employment Earnings

Notes: The figure shows the average for each business day of wait time relative to the first day of the seventh month. The sample includes children within a seventeen-business-day window around this cutoff. The five business days immediately to the left of the cutoff are shaded in gray and are part of our half-donut exclusion. Earnings have been adjusted to 2012 dollars. See Table 1 for a complete description of our data sample.

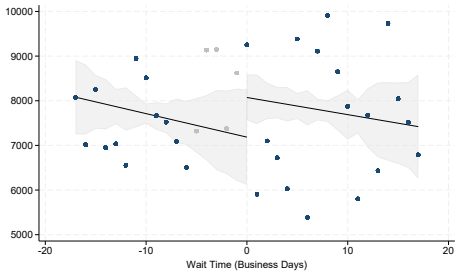
Figure 10: Reduced Form Effects on Secondary Earner's Employment Status and Earnings at the Seventh-Month Cutoff



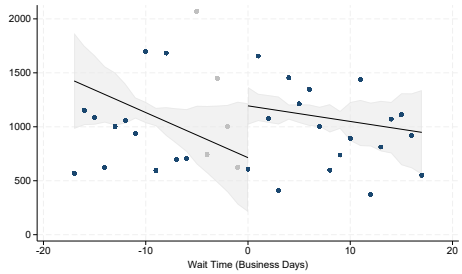
(a) Employment Status



(b) Total Earnings



(c) Wage Earnings



(d) Self-Employment Earnings

Notes: The figure shows the average for each business day of wait time relative to the first day of the seventh month. The sample includes children within a seventeen-business-day window around this cutoff. The five business days immediately to the left of the cutoff are shaded in gray and are part of our half-donut exclusion. Earnings have been adjusted to 2012 dollars. See Table 1 for a complete description of our data sample.

9 Tables

Table 1: Summary Statistics

	All	Seventh-Month Cutoff	Eighth-Month Cutoff
Children's Diagnoses			
Congenital	0.033	0.014	0.013
Other	0.085	0.048	0.047
Mental	0.737	0.831	0.829
Nervous	0.037	0.034	0.036
Respiratory	0.017	0.013	0.013
Sensory	0.015	0.014	0.015
Children's Demographics			
Age	5.987	6.612	6.717
Female	0.329	0.317	0.313
Household Size	1.397	1.412	1.410
Initially Allowed	0.897	0.922	0.924
Months Post-Approval	5.414	5.458	5.600
Parents' Pre-Treatment Outcomes			
Any Employment	0.497	0.493	0.498
Total Earnings	8,286.26	7,315.53	7,463.61
Wage Earnings	7,590.88	6,454.77	6,563.22
Self-Employment Earnings	695.38	860.75	900.39
Observations	725,672	28,015	17,832

Notes: This table presents the average characteristics of children approved for SSI from 2010 to 2019. The second column includes FBR children in a 17-business day window around the seventh-month cutoff, excluding the 5 business days immediately before the cutoff. The third column includes FBR children in a 17-business day window around the eighth-month cutoff. We exclude the diagnosis categories Blood, Circulatory, Digestive, Endocrine, Genitourinary, Infection, Injury, Musculoskeletal, Neoplasm, and Skin, since each constitutes less than 1% of the sample. Pre-treatment outcomes reflect whether parents have ever worked and their average earnings over the five years before children's program approval. Earnings are adjusted to 2012 dollars.

Table 2: Discontinuity in the Receipt of Dedicated Accounts at the Seventh-Month Cutoff

	Receipt of Dedicated Accounts
Post Seventh-Month Cutoff	0.325***
	(0.017)
Control Mean	0.288
Observations	28,015

Notes: This table presents the point estimate and standard error from estimating Equation 1 on the receipt of dedicated accounts at the seventh-month cutoff. We use a half-donut design and exclude the 5 business days immediately to the left of the cutoff from the estimation. The standard error is clustered at the state level. Statistical significance is denoted by * for $p < 0.1$, ** for $p < 0.05$, and *** for $p < 0.01$. See notes to Table 1 for a description of the analysis sample.

Table 3: Regression Discontinuity Covariate Balance Tests at the Seventh-Month Cutoff

	Reduced-Form		Second-Stage		Control
	Pt. Et.	SE	Pt. Et.	SE	Mean
Children's Diagnoses					
Congenital	−0.001	(0.004)	−0.002	(0.011)	0.014
Other	−0.010	(0.008)	−0.032	(0.024)	0.048
Mental	0.014	(0.018)	0.044	(0.054)	0.831
Nervous	0.002	(0.008)	0.006	(0.024)	0.035
Respiratory	−0.011**	(0.004)	−0.032***	(0.012)	0.013
Sensory	0.004	(0.003)	0.013	(0.010)	0.013
Children's Demographics					
Age	0.021	(0.166)	0.063	(0.503)	6.611
Female	0.036*	(0.020)	0.110*	(0.060)	0.316
Household Size	−0.041**	(0.017)	−0.126**	(0.054)	1.412
Initially Allowed	0.013	(0.010)	0.041	(0.032)	0.922
Parents' Pre-Treatment Outcomes					
Any Employment	−0.003	(0.014)	−0.006	(0.041)	0.491
Total Earnings	−609.66	(386.49)	−1,716.28	(1,156.89)	7,245.63
Wage Earnings	−576.30	(382.00)	−1,652.84	(1,130.63)	6,391.52
Self-Employment Earnings	−16.42	(98.14)	−63.44	(290.95)	854.11
Index	−0.003	(0.005)	−0.010	(0.014)	−0.014

Notes: The table presents point estimates and standard errors for covariate balance that are estimated using Equation 2. We use a half-donut design, excluding the 5 business days immediately before the cutoff from the estimation. Standard errors are clustered at the state level. Statistical significance is indicated by * for $p < 0.1$, ** for $p < 0.05$, and *** for $p < 0.01$. Only diagnoses constituting more than 1% of the sample are listed. The remaining categories—Blood, Circulatory, Digestive, Endocrine, Genitourinary, Infection, Injury, Musculoskeletal, Neoplasm, and Skin—are not statistically significant. Parents' pre-treatment outcomes reflect whether they have ever worked and their average earnings over the five years before children's program approval. Earnings are adjusted to 2012 dollars. The index variable is created following Kling (2007) to test the joint significance of all the covariates. Column 1 presents the reduced form effects, Column 2 presents the fuzzy estimates, and Column 3 presents the control mean. See notes to Table 1 for a description of the analysis sample. $N = 28,015$.

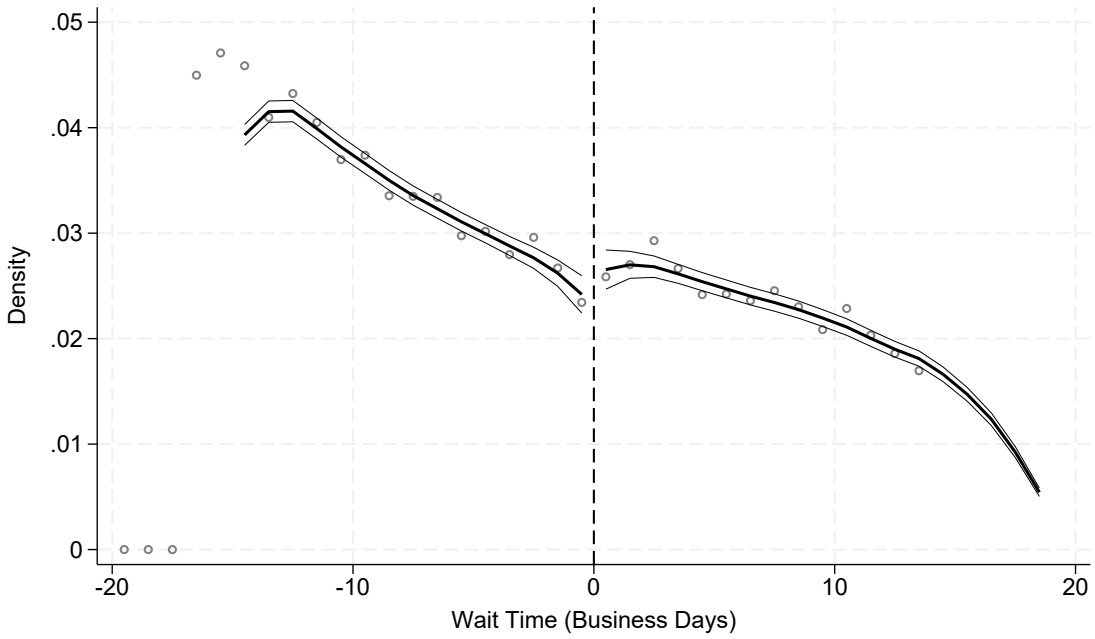
Table 4: Regression Discontinuity Effects on Parental Labor Market Outcomes at the Seventh-Month Cutoff

	Reduced-Form		Second-Stage		Control
	Pt. Et.	SE	Pt. Et.	SE	Mean
All Households ($N = 28,015$)					
Any Employment	0.004	(0.007)	0.013	(0.019)	0.421
Total Earnings	−34.07	(178.20)	−101.95	(528.15)	7,097.02
Wage Earnings	−163.30	(197.94)	−489.18	(588.18)	6,072.69
Self-Employment Earnings	87.61	(134.60)	263.63	(397.89)	1,024.33
Primary Earner ($N = 11,814$)					
Employment Status	−0.019	(0.022)	−0.050	(0.059)	0.904
Total Earnings	−493.17	(396.23)	−1,310.11	(1,051.68)	14,240.50
Wage Earnings	−633.51	(466.57)	−1,712.93	(1,243.53)	12,263.77
Self-Employment Earnings	114.90	(294.62)	310.11	(779.16)	1,976.73
Secondary Earner ($N = 2,692$)					
Employment Status	0.108**	(0.045)	0.288***	(0.111)	0.787
Total Earnings	1,468.41**	(704.83)	3,998.62**	(1,850.88)	8,511.03
Wage Earnings	888.19	(697.87)	2,296.36	(1,818.86)	7,495.38
Self-Employment Earnings	480.19	(312.38)	1,299.86*	(711.95)	1,015.65

Notes: This table presents point estimates and standard errors for the effects on parental labor market outcomes that are estimated using Equation 2. We use a half-donut design, excluding the 5 business days immediately before the cutoff from the estimation. Standard errors are clustered at the state level. Statistical significance is indicated by * for $p < 0.1$, ** for $p < 0.05$, and *** for $p < 0.01$. Earnings are adjusted to 2012 dollars. Column 1 presents the reduced form effects, Column 2 presents the fuzzy estimates, and Column 3 presents the control mean. See notes to Table 1 for a description of the analysis sample.

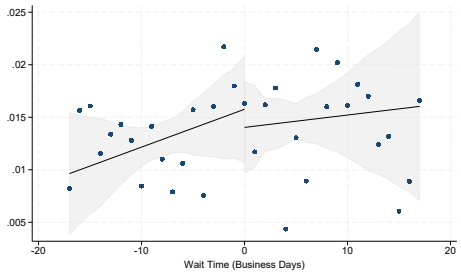
10 Appendix

Figure A1: McCrary Density Test at the Eighth-Month Cutoff

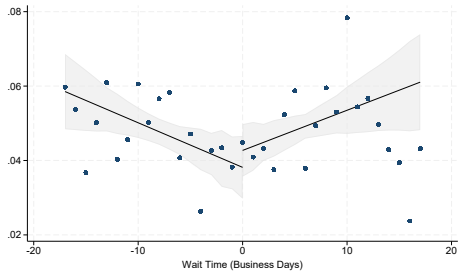


Notes: This figure presents the results of an examination of potential manipulation in the density around the cutoff. Each business day of wait time is measured relative to the first day of the eighth month. The sample includes children within a seventeen-business-day window around this cutoff. The discontinuity point estimate is 0.120 with a standard error of 0.067.

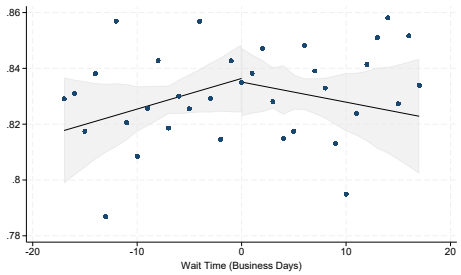
Figure A2: Regression Discontinuity Balance Plots for Children's Diagnoses at the Eighth-Month Cutoff



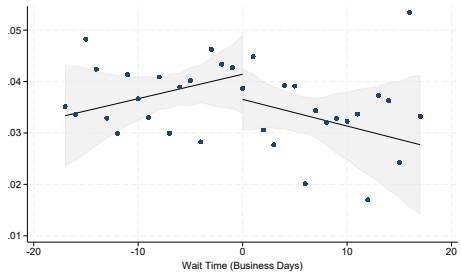
(a) Congenital



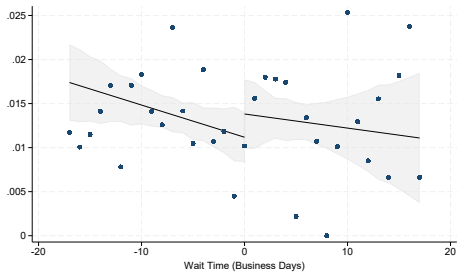
(b) Other



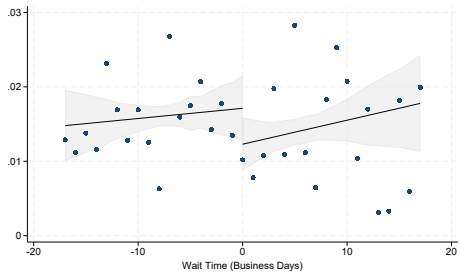
(c) Mental



(d) Nervous



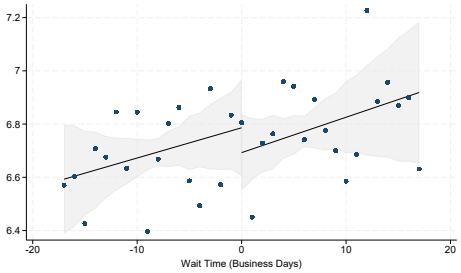
(e) Respiratory



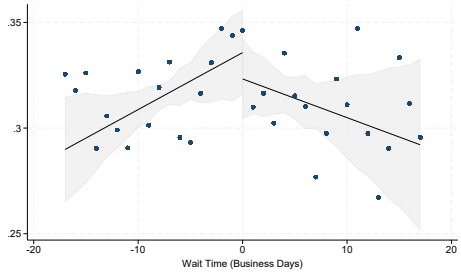
(f) Sensory

Notes: The figure shows the average for each business day of wait time relative to the first day of the eighth month. The sample includes children within a seventeen-business-day window around this cutoff. Only diagnoses constituting more than 1% of the sample are displayed. The remaining categories—Blood, Circulatory, Digestive, Endocrine, Genitourinary, Infection, Injury, Musculoskeletal, Neoplasm, and Skin—are not statistically significant. See Table 1 for a complete description of our data sample.

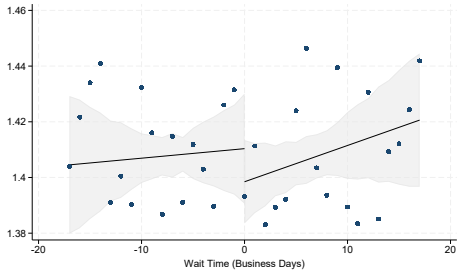
Figure A3: Regression Discontinuity Balance Plots for Children's Demographics at the Eighth-Month Cutoff



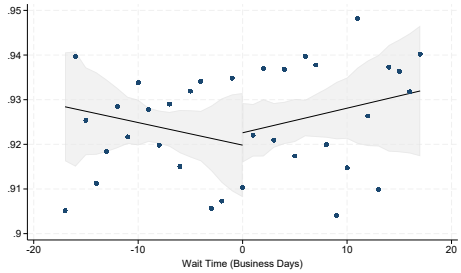
(a) Age



(b) Female



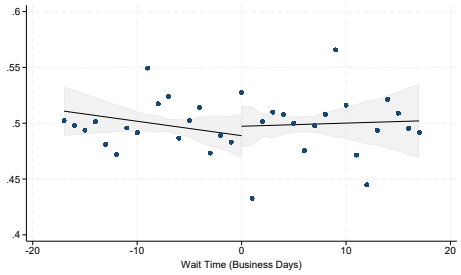
(c) Household Size



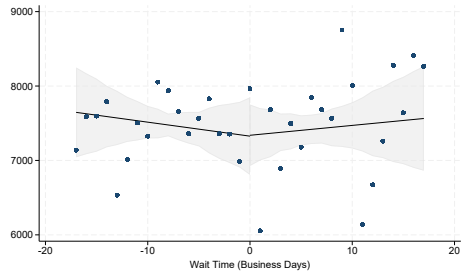
(d) Initially Allowed

Notes: The figure shows the average for each business day of wait time relative to the first day of the eighth month. The sample includes children within a seventeen-business-day window around this cutoff. See Table 1 for a complete description of our data sample.

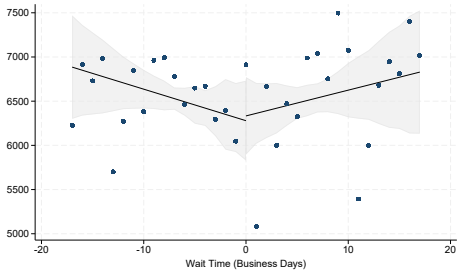
Figure A4: Regression Discontinuity Balance Plots for Parents' Pre-Treatment Outcomes at the Eighth-Month Cutoff



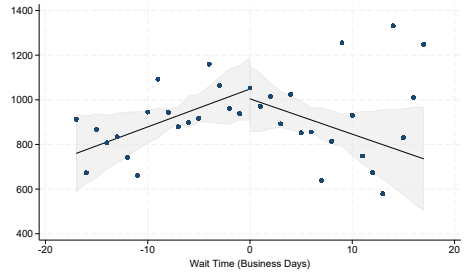
(a) Any Employment



(b) Total Earnings



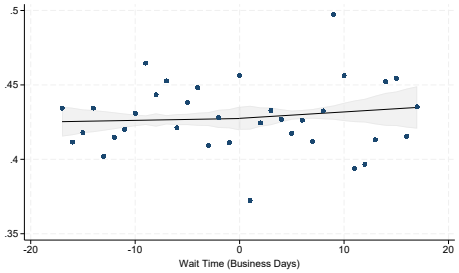
(c) Wage Earnings



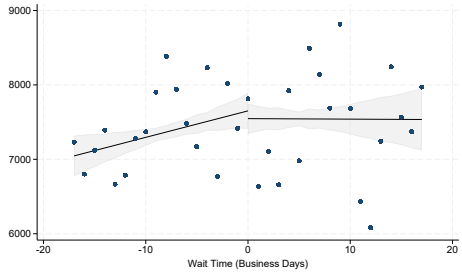
(d) Self-Employment Earnings

Notes: The figure shows the average for each business day of wait time relative to the first day of the eighth month. The sample includes children within a seventeen-business-day window around this cutoff. Pre-treatment outcomes reflect whether parents have ever worked and their average earnings over the five years before children's program approval. Earnings are adjusted to 2012 dollars. See Table 1 for a complete description of our data sample.

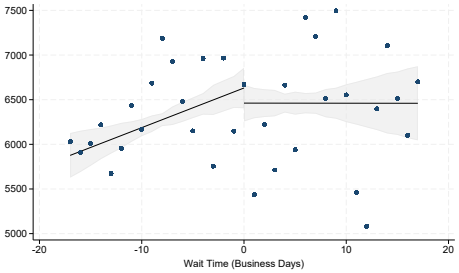
Figure A5: Reduced Form Effects on Employment Status and Earnings for All Households at the Eighth-Month Cutoff



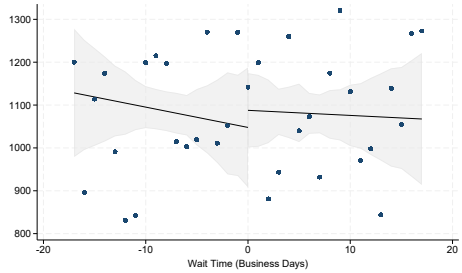
(a) Any Employment



(b) Total Earnings



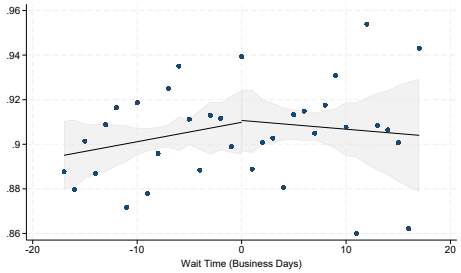
(c) Wage Earnings



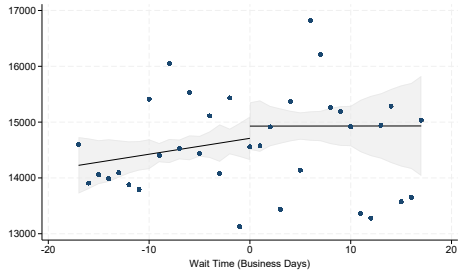
(d) Self-Employment Earnings

Notes: The figure shows the average for each business day of wait time relative to the first day of the eighth month. The sample includes children within a seventeen-business-day window around this cutoff. Earnings have been adjusted to 2012 dollars. See Table 1 for a complete description of our data sample.

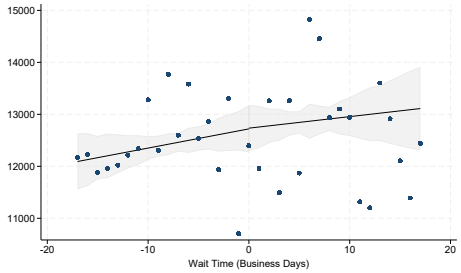
Figure A6: Reduced Form Effects on Primary Earner's Employment Status and Earnings at the Eighth-Month Cutoff



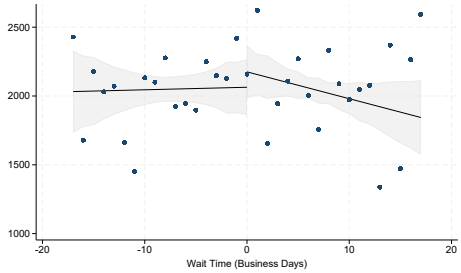
(a) Employment Status



(b) Total Earnings



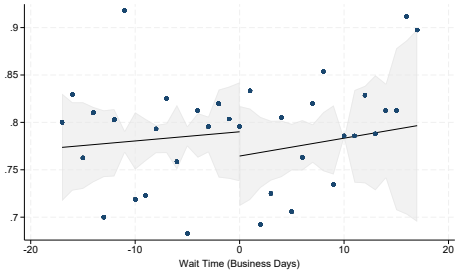
(c) Wage Earnings



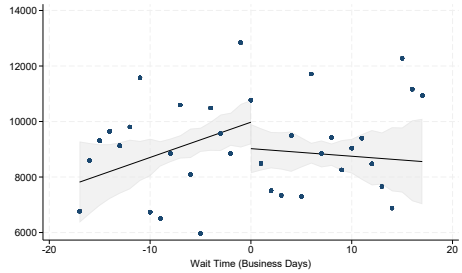
(d) Self-Employment Earnings

Notes: The figure shows the average for each business day of wait time relative to the first day of the eighth month. The sample includes children within a seventeen-business-day window around this cutoff. Earnings have been adjusted to 2012 dollars. See Table 1 for a complete description of our data sample.

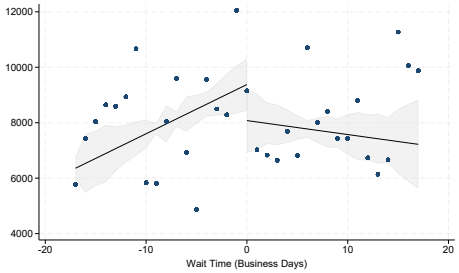
Figure A7: Reduced Form Effects on Secondary Earner's Employment Status and Earnings at the Eighth-Month Cutoff



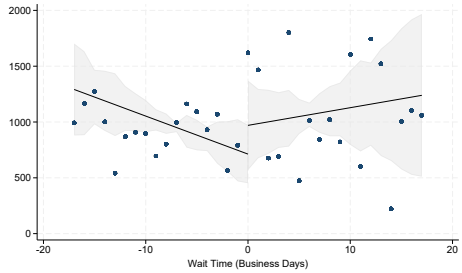
(a) Employment Status



(b) Total Earnings



(c) Wage Earnings



(d) Self-Employment Earnings

Notes: The figure shows the average for each business day of wait time relative to the first day of the eighth month. The sample includes children within a seventeen-business-day window around this cutoff. Earnings have been adjusted to 2012 dollars. See Table 1 for a complete description of our data sample.

Table A1: Regression Discontinuity Covariate Balance Tests at the Eighth-Month Cutoff

	Pt. Et.	SE	Control Mean
Children's Diagnoses			
Congenital	−0.002	(0.004)	0.014
Other	0.005	(0.007)	0.047
Mental	−0.001	(0.010)	0.829
Nervous	−0.005	(0.005)	0.036
Respiratory	0.003	(0.003)	0.013
Sensory	−0.005	(0.003)	0.015
Children's Demographics			
Age	−0.093	(0.134)	6.725
Female	−0.012	(0.019)	0.313
Household Size	−0.012	(0.014)	1.409
Initially Allowed	0.003	(0.008)	0.925
Parents' Pre-Treatment Outcomes			
Any Employment	0.008	(0.017)	0.498
Total Earnings	11.59	(415.39)	7,428.82
Wage Earnings	53.95	(383.24)	6,496.98
Self-Employment Earnings	−44.00	(120.8)	931.84
Index	−0.002	0.006	−0.010

Notes: The table presents point estimates and standard errors for covariate balance that are estimated using Equation 2. Standard errors are clustered at the state level. Statistical significance is indicated by * for $p < 0.1$, ** for $p < 0.05$, and *** for $p < 0.01$. Only diagnoses constituting more than 1% of the sample are listed. The remaining categories—Blood, Circulatory, Digestive, Endocrine, Genitourinary, Infection, Injury, Musculoskeletal, Neoplasm, and Skin—are not statistically significant. Parents' pre-treatment outcomes reflect whether they have ever worked and their average earnings over the five years before children's program approval. Earnings are adjusted to 2012 dollars. The index variable is created following Kling (2007) to test the joint significance of all the covariates. See notes to Table 1 for a description of the analysis sample. $N = 17,832$.

Table A2: Regression Discontinuity Effects on Parental Labor Market Outcomes at the Eighth-Month Cutoff

	Pt. Et.	SE	Control Mean
All Households ($N = 17,832$)			
Any Employment	0.000	(0.007)	0.428
Total Earnings	−104.31	(182.90)	7,474.50
Wage Earnings	−169.16	(200.91)	6,393.96
Self-Employment Earnings	39.92	(113.40)	1,080.55
Primary Earner ($N = 7,566$)			
Employment Status	0.001	(0.013)	0.906
Total Earnings	219.12	(397.78)	14,707.90
Wage Earnings	14.08	(397.97)	12,643.32
Self-Employment Earnings	111.83	(188.74)	2,064.58
Secondary Earner ($N = 1,703$)			
Employment Status	−0.026	(0.047)	0.779
Total Earnings	−952.29	(859.12)	9,041.80
Wage Earnings	−1,299.24	(894.66)	8,058.71
Self-Employment Earnings	257.42	(295.94)	983.09

Notes: This table presents point estimates and standard errors for the effects on parental labor market outcomes that are estimated using Equation 2. Standard errors are clustered at the state level. Statistical significance is indicated by * for $p < 0.1$, ** for $p < 0.05$, and *** for $p < 0.01$. Earnings are adjusted to 2012 dollars. See notes to Table 1 for a description of the analysis sample.