The Effect of Disability Payments on Household Earnings and Income: Evidence from the SSI Children's Program

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September 10, 2013

Abstract

Disability programs have expanded rapidly in recent decades across the developed world. Despite substantial evidence on the effects of these programs on labor supply, less is known about their effects on household income and consumption, or the extent to which labor supply discouragement is driven by income versus substitution effects. The Supplemental Security Income (SSI) children's program, the rapidly expanding means-tested disability program for children in the United States, has been singled out among social insurance programs for the potentially grave consequences of its incentive effects. In this paper, I use variation in continuing disability reviews to identify the effect of the SSI children's program on household outcomes. I find that the loss of $1000 in the child's SSI payment increases parental earnings by at least $600, with even greater responsiveness for certain subgroups. This estimate is much larger than the few existing estimates of the elasticity of earnings to unearned income. Using the unique institutional context of the SSI children's program, I determine that this labor supply discouragement effect is driven largely by an income effect. In contrast to households' dramatic substitution to earned income, I find that the loss of the child's SSI payment discourages disability applications by other family members, suggesting that the loss leads households to update their beliefs about disability insurance as reliable source of income. Finally, I find evidence of the importance of household-level shocks in the decision to apply for disability insurance.

*I am grateful to David Autor, Amy Finkelstein, and Michael Greenstone for invaluable advice, discussions, and support. I am indebted to Paul Davies, Manuel de la Puente, Jim Fahlfeder, Judi Papas, Bob Somers, Alexander Strand, and Ray Wise of the Social Security Administration for providing access to data, explaining institutional detail, answering countless questions, and making my work at SSA possible. I thank Françoise Becker, Thuy Ho, Bill Lancaster, Mike Risha, and Tom Solomon of the Social Security Administration for assistance with data and computing; and Judy Hicks, Carmine Fuccillo, Jeff Hemmeter, Suzanne Luther, Tiya Marshall, and Art Spencer of the Social Security Administration for discussions of SSA programs. I thank the following people for helpful comments: Josh Angrist, Paul Davies, Jeff Kling, Jeff Liebman, Day Manoli, Noah Meyerson, Tim Moore, Hoai-Luu Nguyen, Devin O'Connor, Kalman Rupp, Jack Smalligan, Alexander Strand, Melanie Wasserman, Heidi Williams, Clara Zverina, as well as seminar participants at the Social Security Administration, Congressional Budget Office, MIT Labor Lunch, and MIT Public Finance Lunch. Funding from the National Institute on Aging, through Grant Number T32-AG000186 to the National Bureau of Economic Research, is gratefully acknowledged. The findings and conclusions expressed are solely those of the author and do not represent the views of NIH, SSA, any agency of the Federal Government, or the NBER.

Key words: disability insurance, Supplemental Security Income, child disability, earnings elasticity, income and substitution effects

Disability programs across the developed world have expanded rapidly in recent decades as a result of aging and expanding workforces, wage stagnation, and the liberalization of medical eligibility criteria (Bound and Waidmann 2002; Autor and Duggan 2003; Autor and Duggan 2005). In the United States, the Social Security Disability Insurance program has experienced a sixfold increase since 1970, and enrollment in the Supplemental Security Income program among disabled adults and children has tripled since 1980. Although these expansions have sparked considerable concern, assessing their normative implications is difficult. Like all social insurance programs, disability programs have perverse incentives that must be weighed against the benefits of these programs.

Several studies have documented the potentially substantial effects of disability programs on labor supply and human capital (Bound 1989; Chen and van der Klaauw 2005; French and Song 2011; von Wachter, Song, and Manchester 2011; Autor et al. 2011; Maestas, Mullen, and Strand 2012). However, there is little evidence on the extent to which program rules—rather than simply the transfer of income—affect labor supply behavior. Distinguishing between income and substitution effects is important for evaluating the labor supply discouragement effect, since only substitution effects are distortionary.

Even less is known about the effect of these programs on household consumption and welfare. If disability payments provide income that would otherwise be very costly to obtain, then their benefits may outweigh the perverse incentives that they create. If, however, these payments simply substitute for other easily available sources of income, then their consumption smoothing benefits are likely to be small relative to their distortionary effects, making the rapid expansion of disability programs cause for concern. Assessing the effect of disability insurance on household income and consumption is a first step to understanding its benefits, although it is also necessary to evaluate the efficiency of consumption smoothing.

The Supplemental Security Income children's program in the United States has been singled out by policymakers and the media for the potentially grave consequences of its incentive effects. Critics charge that the SSI children's program encourages households to present their children as disabled, possibly at the expense of the child's health and educational achievement (Wen 2010; Kristof 2012; U.S. Congress 2011). Supporters of the program argue that SSI is a critical lifeline for children with disabilities and their struggling families, who might fall into poverty without its monthly cash transfers and health insurance provision (Ruffing and Pavetti 2012; Vallas and Alfano 2012). In this paper, I measure the effect of the SSI children's program on parental earnings and household income. In addition, since the SSI children's program imposes few restrictions on parental labor supply, it offers a rare opportunity to measure the earnings response in a setting where income effects, rather than substitution effects, are likely to predominate.

Previous attempts to evaluate the effect of the SSI children's program on household income and child outcomes have encountered two major challenges. First, since SSI is a federal program with standardized payments, there is no meaningful state-level or other cross-sectional variation in benefit amounts. Second, causal studies thus far have been restricted to survey data, with sample sizes too small to provide sufficient statistical power. Kubik (1999) uses the Current Population Survey and the National Health Interview Survey to find that the 1990 liberalization of medical eligibility criteria for SSI children increased the number of children with a diagnosed disability. Duggan and Kearney (2007) estimate the effects of SSI child payments on household income using an event-study design from a Survey of Income and Program Participation panel. They find that enrolling a child in SSI increases household income by $400 per month without significant offsets from other transfer programs or earnings.

The contribution of this paper is to use credibly exogenous variation and administrative data to examine the effect of the SSI children's program on parental earnings and household income. I use quasi-random assignment of continuing disability reviews as a source of variation in SSI benefits for children. Continuing disability reviews (CDRs) are used to verify that children are still medically eligible for SSI, and they substantially increase the likelihood that a child will be removed from SSI. In 2005, large budget cuts forced the Social Security Administration (SSA), which administers the SSI program, to scale back CDRs for children. As a result, children who were eligible for review at the end of FY2004 were nearly 60 percentage points more likely to receive a CDR than children who were eligible for review at the beginning of FY2005. This leads to a discrete fall in the removal probability of SSI children at the FY2004/2005 CDR eligibility cutoff, and thus a discrete jump in the amount of time spent on the SSI program.

I find that the loss of a child's SSI payment triggers a large response in parental earnings. I estimate an elasticity of earnings to unearned income of at least 0.6: the loss of $1000 in the child's SSI payment increases parental earnings by $600. For younger children, whose families lose more in net present value, the earnings elasticity is as high as 1, meaning that parents fully offset the loss in SSI payment. These elasticity estimates are much higher than the few existing estimates. Considered in the unique context of the SSI children's program, which has generous parental income limits relative to other welfare programs and to the potential earnings of its target population, this large response suggests that the majority of the labor supply discouragement effect of disability programs more generally comes from an income effect rather than an incentive effect. In other words, households reduce their labor supply in response to the income transfer itself rather than marginal tax rates on earned income. This result has important implications for the design of disability insurance programs.

In contrast to the large effect on parental earnings, I find no substitution—nor, surprisingly, attempts to substitute—to alternative sources of unearned income in response to losing the child's disability payment. In fact, the loss of a child's SSI payment leads to a statistically significant decrease in SSDI and SSI applications by the parents and siblings of the removed child. In response to the termination of SSI child payments, households appear to update their beliefs about disability insurance as a reliable income stream, with the discouragement effect dominating households' desire to substitute to alternative sources of unearned income. However, the reduction in applications does not translate into a statistically significant reduction in the family's SSDI and SSI receipt, suggesting that the removal of children from SSI deters marginal SSDI and SSI applicants who would not be allowed onto disability insurance anyway. Despite the large parental earnings response, the failure to substitute to alternative sources of unearned income results in a net decline in total household income after the loss of the child's SSI payment. Many households do not compensate fully for the resulting drop in consumption.

In addition, I find evidence of the importance of household-level shocks in the decision to apply for disability. Among the SSI children in my sample, approximately 3 percent have a parent who applies for SSDI within 60 days of the child's application, 4 percent have a parent who applies for SSI within 60 days, and 12 percent have a sibling who applies for SSI within 60 days. The decision to apply to disability programs is affected not just by individual health shocks but also by household-level shocks, examples of which include a decline in household income from job loss or new information about the availability of disability programs. Indeed, I find that households experience a precipitous drop in earnings in the year in which the child enters SSI. This drop has two potential explanations: 1) households apply for disability after experiencing an income shock like job loss, or 2) households reduce their earnings after they begin receiving the child's disability payments (or in anticipation of the payments). Of course, the normative implications of the large earnings response of parents on the entry and exit margins are unclear, since work may be costly in the sense that it decreases the amount of time parents spend caring for disabled children.

The paper proceeds as follows. Section 1 provides background on the SSI children's program, including the rapid increase in enrollment in recent decades and the concomitant policy debate. Section 2 details the administrative data from the Social Security Administration used in this study. Section 3 describes the empirical strategy and provides more detail on continuing disability reviews and how they affect the amount of time that children spend on the SSI program. Sections 4 and 5 present regression discontinuity design and difference-in-differences results of the effect of SSI child payments on household earnings, substitution to other welfare programs, and total household income. In Section 6, I provide robustness checks, including estimates from two alternative identification strategies. Section 7 discusses the findings of the paper and concludes.

1 Background on SSI Children's Program

Established by Congress in 1972, the Supplemental Security Income program began providing cash assistance in 1974 to low-income aged, blind, and disabled individuals. Congress allowed disabled children to qualify for the program under the reasoning that a child's disability may impose additional expenses on a family or require parents to forgo earned income to care for the child. The SSI children's program makes monthly cash payments to the parents of children who have a qualifying disability and limited income and assets, and in most states it automatically qualifies children for Medicaid.

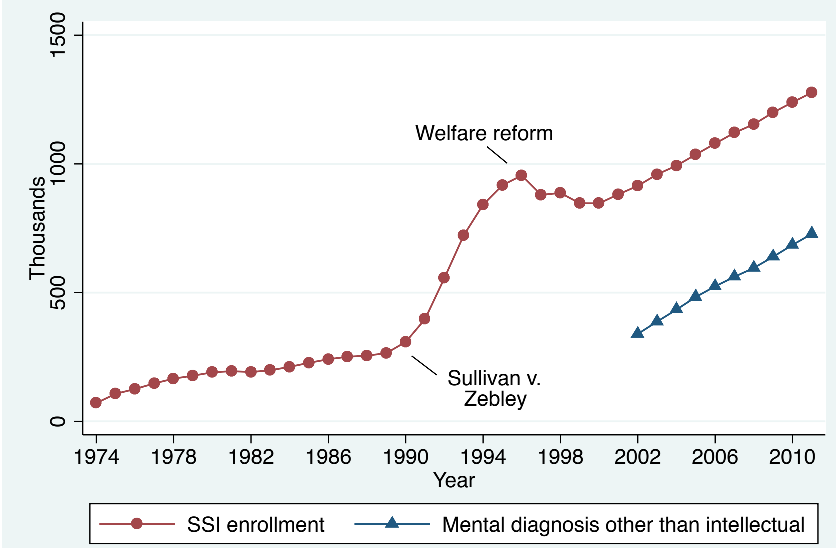
Currently SSI spends $10 billion annually on payments to 1.3 million children, or about 10 percent of children living in poverty. In 2013, the maximum monthly federal payment per child was $710, with small supplements made by some states. The program is means-tested, and parental income is "deemed" onto a child based on the type of income and allowances for other household members. For a typical single-parent household with two children, one of whom is on SSI, the SSI child benefit is taxed starting at $2000/month in parental earnings and is completely phased out by $3000/month. Other than state-level variation in supplemental payments, benefit levels vary only with the child's income, including deemed income.

1.1 Rapid Growth in Enrollment

Figure 1 shows the growth in enrollment in the SSI children's program over time. Between 1974 and 1990, the program experienced small and steady growth, with about 300,000 children enrolled by 1990. In 1990, two major events triggered a surge in enrollment. First, the Supreme Court ruled in the case of *Sullivan v. Zebley* that the Social Security Administration (SSA) must use comparable standards for adults and children. In response, SSA revised its rules to allow children to qualify for SSI if they demonstrated a disability with "comparable severity" to those in the official medical listings, rather than restricting to disabilities in the listings. SSA also allowed children to qualify if they showed deficiencies in specific domains—such as cognitive, communicative, motor, social, and behavioral—even if the disability did not medically equal those in the listings. Second, SSA expanded the childhood medical listings to include more mental disorders, including attention deficit disorder, autism, personality disorders, and mood disorders.

As a result of the 1990 changes, enrollment in the SSI children's program increased by 200 percent over the next five years. Mental conditions accounted for much of this increase, a trend that drew attention and criticism from policymakers and the media. Reports of parents "coaching" their children to appear disabled featured prominently in the news. In response, as part of welfare reform, Congress revised the medical eligibility criteria for the SSI children's program to be tighter than *Zebley* reforms but still more lenient than the pre-*Zebley* era. Congress also required the reassessment of hundreds of thousands of children who had

Figure 1: SSI Enrollment Among Children Under 18, 1974-2011



Note: Data for mental diagnoses only available for 2002-2011.
Sources: SSI Annual Statistical Reports, 2002-2011

entered the program under *Zebley* criteria and succeeded in taking some of these children off of SSI.

As a result of welfare reform, the SSI children's program experienced a small drop in enrollment followed by another increase, this time less dramatic than the post-*Zebley* surge. As shown by the shorter line in Figure 1, the post-welfare reform increase in enrollment can be attributed almost entirely to mental conditions other than intellectual disability (formerly known as mental retardation). In FY2010, ADHD and speech delay were the most prevalent mental conditions in medical allowances for SSI.[1]

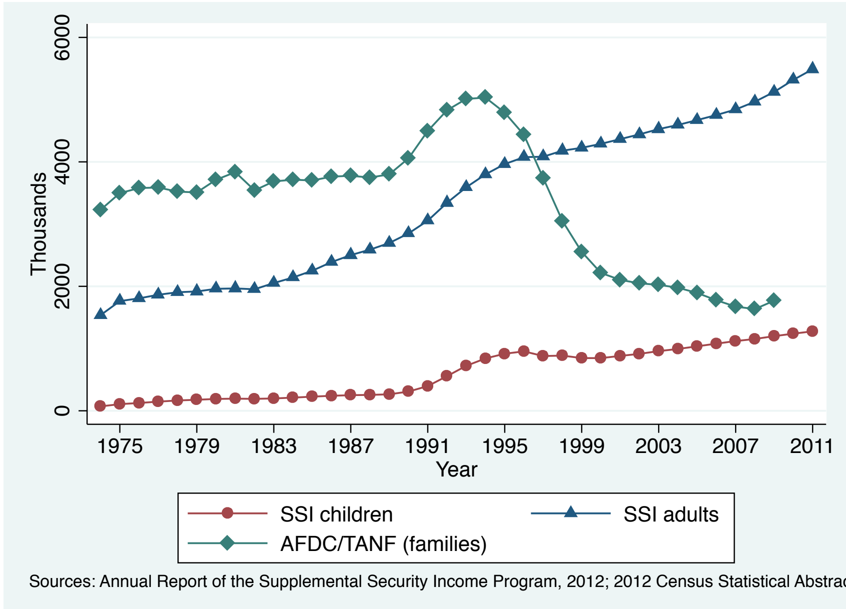
Figure 2 shows the rise in SSI enrollment mirroring declining TANF enrollment. Families have an incentive to shift from TANF to SSI for several reasons. First, SSI payments can be several hundred dollars a month more than TANF benefits, and even more if multiple family members are enrolled in SSI. In addition, parents can have substantially higher earned and unearned income under SSI than TANF if a child qualifies for SSI. Finally, SSI is a guaranteed income stream until the qualifying child turns 18, not unlike an annuity, provided that the family can demonstrate a lack of medical improvement. In contrast, TANF benefits became time-limited under welfare reform.

States also have incentives to move families from TANF to SSI, since SSI is a federally funded entitlement and TANF is a block-grant program that draws on state coffers. Kubik (2003) finds that many states took cost-saving measures to move children from AFDC (the predecessor to TANF) to SSI after the liberalization of medical eligibility criteria in 1990.

Table 1 provides a snapshot of the SSI child population in December 2011. SSI children, a majority of

[1] GAO, "Supplemental Security Income: Preliminary Observations on Children with Mental Impairments," October 27, 2011.

Figure 2: Enrollment in Welfare Programs, 1974-2011



whom receive the maximum monthly payment, share many characteristics with the broader at-risk youth population. The vast majority (79%) live in households with fewer than two parents. Average earned income across all households is just $632 and average income is $762. In one-parent families, which compose a majority of SSI child households, 68% have no earned income and 48% report no income (other than SSI payments) at all. More than half of SSI children have a primary diagnosis of mental disorder other than intellectual disability.

The poor life outcomes of SSI children have been well-documented. Using the National Survey of SSI Children and Families, Hemmeter et al. (2009) find that SSI children ages 17 and 18 have drop-out rates of 30% and suspension rates of 43%. These numbers are even higher for SSI children with mental conditions other than intellectual disability, at 45% and 52%, respectively. Arrest rates are 16% for SSI children and 28% for the subgroup with other mental conditions. Davies et al. (2009) find that the outcomes of these children do not improve much in adulthood: former SSI children have positive earnings rates of just 20-50% as adults, depending on the cohort. Less is known about the parents of SSI children, many of whom are single mothers. Rupp and Ressler (2009) document substantial heterogeneity in the amount of time spent on home health care for SSI children by their family members. While 62% of parents of SSI children report no home health care provision, 12% report spending 35 or more hours per week on home health care for their child.

Table 1: Profile of SSI Children, December 2011

Payment		**Male**	66%
Average Payment	$601		
Receiving max payment ($674)	63%	**Age**	
		Under 5	21%
Family Structure		6-12	45%
No parents	12%	13-17	34%
One parent	67%		
Two parents	21%	**Primary Diagnosis**	
		Mental disorders	68%
Parental income (monthly)		*Intellectual disability*	*10%*
All		*Autistic disorders*	*8%*
Average earned income	$632	*Developmental disorders*	*20%*
Average income	$762	*Not elsewhere classified*	*20%*
One parent		*Other mental*	*9%*
No earned income	68%	Physical diseases	23%
No income	48%	*Nervous and sense*	*8%*
Two parents		*Respiratory*	*3%*
No earned income	37%	*Congenital*	*5%*
No income	20%	Other	10%

Source: SSI Annual Statistical Report, 2011 (SSA)

1.2 Policy Debate

The SSI children's program has been a lightning rod for criticism since welfare reform. The most recent policy debate can be traced to a series of articles by Patricia Wen in the *Boston Globe* in 2010. These articles document anecdotal evidence of perverse incentives created by this program, including parents putting their children on powerful psychotropic medication with the belief that the child must be medicated to qualify for SSI benefits. (SSA denies such a requirement but officials acknowledge that some parents appear to believe this claim.[2]) More generally, she observes that conditioning a family's welfare check on the child's disability may reduce parental incentives to invest in health improvements for their children or to encourage achievement in school. Wen cites examples of SSI children who have lost their motivation for achievement. She also quotes adolescents who say that they would like to get work experience but fear putting their family over the program's income limits.

The *Boston Globe* series sparked intense policy interest, including SSA and GAO investigations into the veracity of claims in the articles and a House Ways and Means Committee hearing on potential perverse incentives in the SSI program. In a *New York Times* op-ed in 2012 that calls for defunding the SSI children's program, Nicholas Kristof cites anecdotal evidence of parents in rural Appalachia pulling their children out of literacy programs for fear that learning how to read would disqualify their children from SSI benefits.

Well-aware of the poor life outcomes of SSI children, the Social Security Administration, in conjunction

[2] GAO, "Supplemental Security Income: Preliminary Observations on Children with Mental Impairments," October 27, 2011.

with other federal agencies, has initiated programs to evaluate interventions and support services for SSI children who are transitioning to adulthood, including the Youth Transition Demonstration (YTD) and Promoting Readiness of Minors in Supplemental Security Income (PROMISE). The stated goals of these programs are to improve the life outcomes of SSI children and decrease their reliance on disability benefits.

The policy debate surrounding the SSI children's program can be summarized as follows. On the one hand, transfer payments to poor families may help them care for children with disabilities and may increase consumption for the entire family, including parents and siblings. SSI also provides health insurance to enrolled children, though the net effect on health insurance eligibility is small since most of them would qualify for Medicaid or CHIP based on low income. On the other hand, opponents of the program argue that the welfare or disability label may change a child's taste for school and work, and that conditioning cash payments on disability may promote perverse behaviors—such as discouraging educational achievement—to demonstrate medical eligibility. In addition, income limits may prevent teenagers from developing human capital. SSI may also have adverse effects on parents and siblings by, for example, slowing or reversing their accumulation of human capital through disincentives for work.

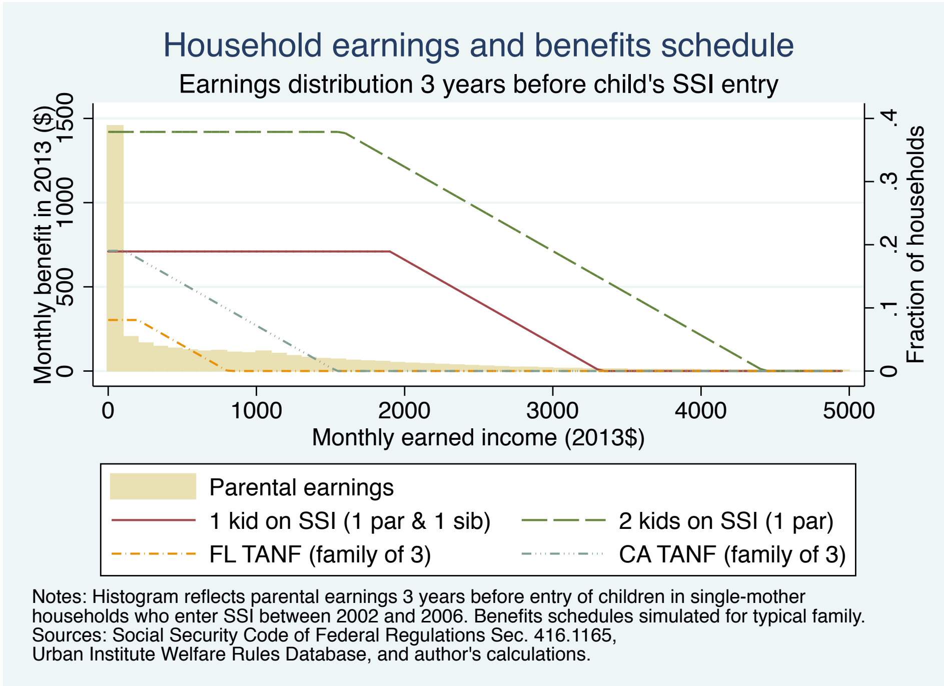
1.3 Income and Substitution Effects in the SSI Children's Program

The SSI children's program is unique among welfare programs in its relatively generous treatment of earned and unearned income. In adult disability programs, disabled individuals face "substantial gainful activity" limits that restrict how much they can work. In 2013, SSI adults were limited to $1040 in monthly earnings, above which they faced a suspension of benefits—a marginal tax rate of 70,000 percent. But because the SSI children's program makes payments to families for a child's disability, the parents of SSI children face no stipulations on how much they can work provided that they continue to meet the means test on income and assets. Moreover, income eligibility for the SSI children's program is determined by "deeming" parental income onto the child with relatively generous allowances for other household members. Deeming results in a much higher effective household income limit for the SSI children's program than for other welfare programs.

Figure 3 plots benefit schedules for the SSI children's program and TANF relative to monthly earned income. In most states, TANF begins phasing out in the first $100 of monthly earned income and is completely phased out by $1500. In contrast, a typical family with one child on the SSI children's program could earn up to $2000 per month with no penalty and would not see a complete phase-out until about $3000 per month. A family with two children on the program could earn up to $4000 per month before a complete phase out.

The relatively high phase-outs in the SSI children's program provide a unique institutional context in which income effects, rather than substitution effects, are likely to predominate. In most programs, like

Figure 3: Comparison of SSI and TANF Benefit Amounts and Earnings Limits



TANF or adult SSI, program rules make the income and substitution effects of welfare payments difficult to disentangle. The income effect reduces earnings by making the family wealthier and inducing a shift away from work towards leisure. The substitution effect also reduces earnings by increasing the marginal tax rate faced by families on the next dollar of earnings through income phase-outs (adult SSI and TANF) or substantial gainful activity limits (adult SSI). The income and substitution effects are nearly impossible to distinguish in these contexts, since payments are stipulated on having little or no work capacity. In contrast, the relatively high income phase-outs of the SSI children's program, especially in comparison to the potential earnings of the target population, mean that substitution effects are far less relevant and most of the measured earnings response can be attributed to income effects.

Isolating an income effect would require the sample to households whose potential earnings are below the phase-out range. Although I do not see potential earnings, I do see a household earnings in the years before the child enters SSI, when SSI rules should not impact parental earnings. As shown in Figure 3, three years before entering the program, 90 percent of single-mother households have earnings under the point where the SSI payment is taxed. Assuming that their incomes would not have risen substantially had they not entered SSI, these 90 percent of households face only income effects without confounding substitution effects from taxes on their SSI payment.

Distinguishing between income and substitution effects is important for drawing normative conclusions about the effect of social insurance on labor supply, since income effects are non-distortionary and substitution effects are distortionary. The distinction is also critical for policy design. Work incentive programs like Ticket to Work, which attempt to encourage work by reducing marginal tax rates on earnings, have generally failed to increase employment among disabled individuals. A leading hypothesis for this failure is that income effects dominate substitution effects (Autor and Duggan 2007). In other words, it may be that disabled individuals have low labor force participation not because they face high marginal tax rates on earnings but simply because the income from disability payments produces a shift from work to leisure. The SSI children's program provides an opportunity to test this hypothesis by distinguishing income and substitution effects.

2 Data

I use confidential microdata from the Social Security Administration. The main data file is an extract of the Supplemental Security Record (SSR) containing demographic information and benefit histories of all children enrolled in SSI for any period of time between fiscal years 2002 and 2011.

I link the Supplemental Security Record extract to records from the Office of Quality Performance (OQP) in the Office of Quality Data Management at the Social Security Administration. The OQP file contains one record for every child eligible for a continuing disability review in each of fiscal years 2002-2012. Each record includes demographic information on the child beneficiary, the month and year in which the child was eligible for a CDR, whether and when the CDR was conducted, and the date and decision of the CDR and each subsequent adjudication level.

To get outcomes on household earnings and income, I first link the SSR record of the children to the corresponding parental records, which SSA collects to assess the child's income and asset eligibility. This is the first study of which I am aware to take advantage of this link between SSI children and their parents. The SSR contains limited demographic information about parents, including sex and date of birth. I link the SSR parental records to extracts of several SSA data files to get outcomes for the parents of SSI children. The Master Earnings File extract provides annual earnings of the parents of SSI children from 1985 to 2011. The earnings variable consists of wage, salary, and tip income reported on W-2 and W-3 forms, and self-employment income reported on 1040 Schedule SE forms. The Master Beneficiary Record extract contains information on Social Security Disability Insurance payments to parents of SSI children from 1964 to 2011. The Supplemental Security Record contains Supplemental Security Income payments to parents of SSI children from 1974 to 2012. From the 831 records, I get applications for SSDI and SSI filed by parents of SSI children from 1985 to 2011.

The link between parents and children also allows me to evaluate certain outcomes for the siblings of SSI children. I get SSI applications of the siblings of SSI children using SSI approvals and SSI denials in the

Supplemental Security Record from 1992 to 2012. The Supplemental Security Record also provides data on SSI payments to the siblings of SSI children for the same time period.

3 Empirical Strategy

3.1 Variation in Continuing Disability Reviews (CDRs)

I use variation in continuing disability reviews (CDRs) as an instrument for SSI benefits to evaluate the effect of SSI payments on household earnings and income. The Social Security Administration uses CDRs to verify that SSI beneficiaries are still medically eligible for the program.[3] At the time of the initial decision or the most recent CDR, the disability examiner in charge of a child's case assigns the child a "medical diary date"—the date of the next CDR—based on the child's prognosis. The majority of children on SSI get a medical diary date that is 3 years from the initial decision. However, children with lower severity conditions may get more frequent CDRs, and children with higher severity conditions may get less frequent CDRs.

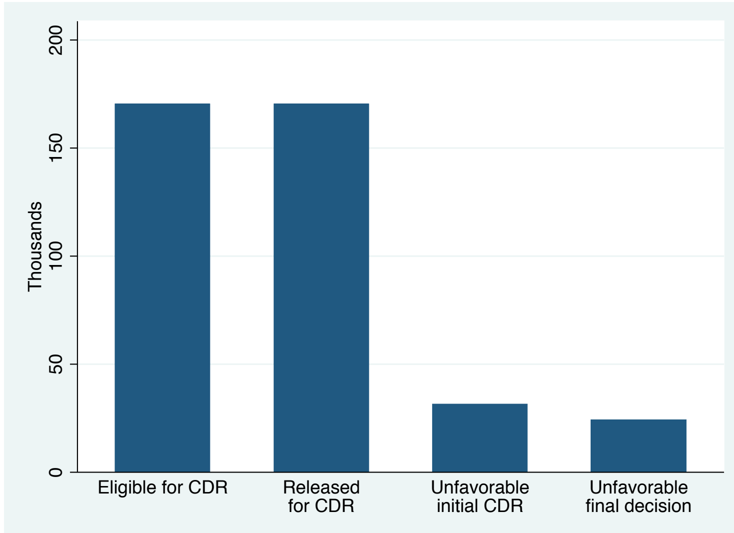
The implementation of CDRs is relevant to my empirical strategy. Each month, the Social Security Administration "releases" children with a medical diary date in that month or a previous month to state disability determination services (DDS) offices. SSA determines the number of children to be released each month based on the state's budgetary resources to conduct CDRs in that month. Once a case is released for a CDR, it is assigned to a disability examiner in the DDS office, who conducts a CDR using a "medical improvement" criterion. Under this criterion, an examiner cannot remove a child simply because the child does not meet current medical eligibility criteria; rather the examiner must show that the child has experienced medical *improvement* since the initial determination or the last CDR. This criterion affects the type of children who are removed via CDR.

CDR decisions are highly consequential: unfavorable decisions generally result in a child's removal from the program. Children who do not receive a CDR cannot be removed from the program for medical reasons. Figure 4 illustrates the CDR process in FY2002, the last year in which SSA was current on its childhood CDRs. Of the approximately 900,000 children enrolled in SSI in FY2002, about 170,000 of them were eligible for a CDR in that year (recall that most children are not eligible every year). Since SSA had full budgetary resources to conduct CDRs in FY2002, all of the eligible children were released to state DDS offices for CDRs. About 30,000 of those children received an unfavorable CDR decision. A few thousand successfully appealed that unfavorable decision, but most were removed from the program. Overall, about 15% of the children who were eligible for a CDR in FY2002 were removed from the program for medical reasons.

I use a dramatic cut in the CDR budget between FY2004 and FY2005 as the main source of variation to

[3] Unlike adults, children receive only full medical reviews (FMRs), not mailers.

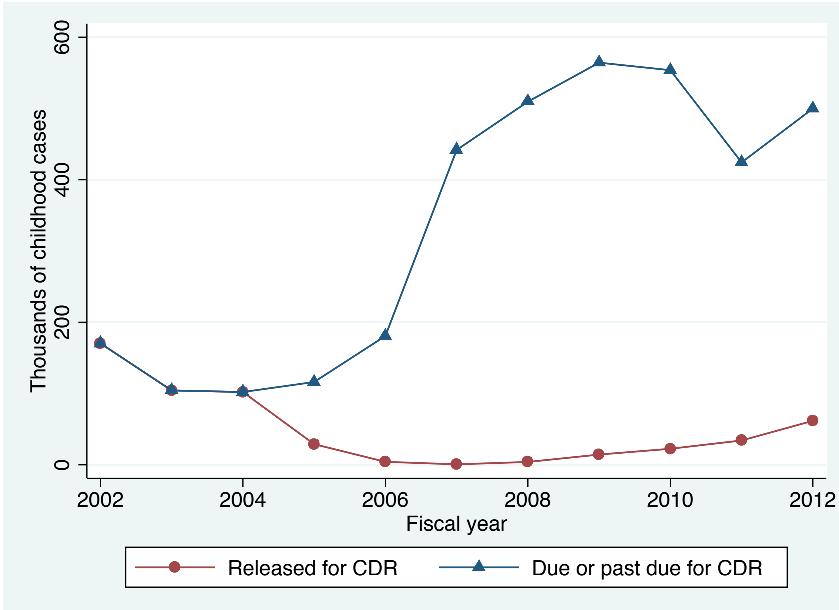
Figure 4: CDR Eligibility and Decisions in FY2002



identify the effect of SSI receipt on household earnings and income. Figure 5 shows the number of SSI child cases due or past due for a CDR in a given year versus the actual number of children who were released for a CDR in that year. Up to FY2004, SSA released all of the children who were due for a CDR. However, in FY2005 there was a dramatic cut in the CDR budget, resulting in many fewer childhood CDRs than in previous years. Therefore children who were eligible for a CDR in FY2004 were much more likely to receive a CDR than children who were eligible for a CDR in FY2005. Children who were eligible for a CDR in FY2005 but did not receive one became eligible to receive a CDR in subsequent years; however, as Figure 5 makes clear, these children were unlikely to receive a CDR in future years either. Children who do not receive a CDR cannot be removed from SSI for medical reasons.

I use two other sources of variation to probe the robustness of the results from the main identification strategy. The first alternative identification strategy is the Social Security Number (SSN) assignment rule for childhood CDRs in FY2005, when the sharp reduction in budgetary resources for CDRs forced SSA to ration CDRs for children. In certain states, SSA ordered children who were eligible for a CDR in ascending order of Social Security Number and determined a SSN cutoff according to the state's capacity to conduct CDRs. Children with SSNs smaller than the cutoff received a CDR in FY2005, while children with SSNs greater than the cutoff did not. I use the SSN assignment rule as a source of variation in CDRs. The second alternative identification strategy is the quasi-random assignment of childhood CDRs to CDR examiners, analogous to the initial examiner instrument used by Maestas, Mullen, and Strand (2012). Results for both alternative identification strategies are presented in the Appendix.

Figure 5: SSI Child Cases Due or Past Due for CDRs, 2002-2012



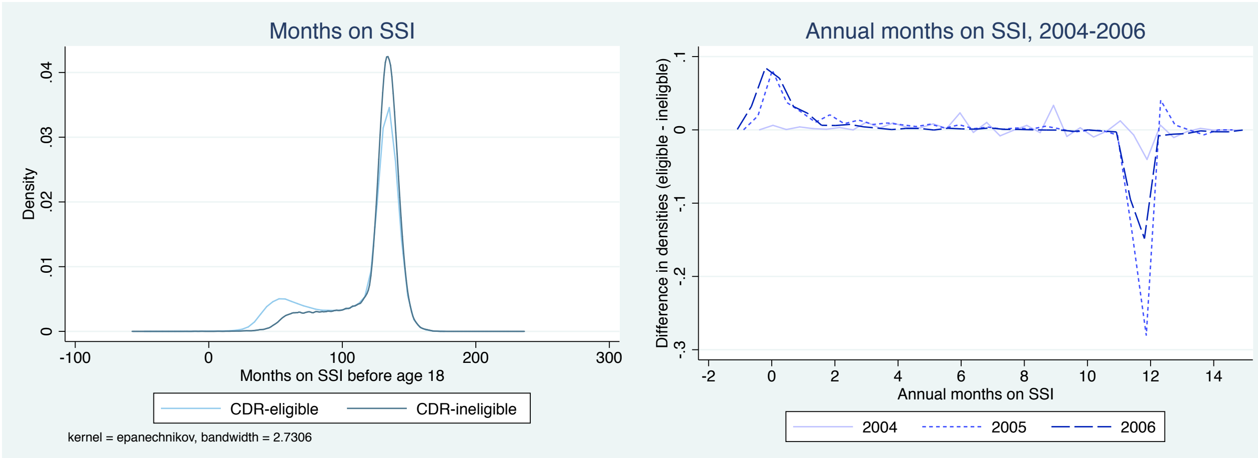
3.2 Which Children Do CDRs Affect and How?

Figure 6 provides a sense of how CDRs affect the amount of time spent on SSI. The graph on the left shows kernel density plots of the total number of months spent on SSI for two groups: children who were eligible for a CDR in FY2004, when nearly all eligible children were released for a CDR, and children who were eligible for a CDR in FY2005, when many fewer eligible children were released for a CDR. The kernel densities are adjusted for sex, age, age at entry, severity, and diagnosis. Rather than shifting the entire distribution, CDRs take children from the peak at 140 months to a much lower SSI duration of 50 months.

The graph on the right gives the *difference* in the densities of the two groups in 2004, 2005 and 2006. In 2004, the number of months spent on SSI has almost exactly the same distribution for the two groups. In 2005, however, when many of the FY2004 CDRs have been completed and the resulting terminations enforced, there is a spike at zero months among the FY2004 eligibles. By 2006, this spike has grown even larger and it remains large in subsequent years.

Table 2 compares the characteristics of the full SSI child population, the CDR sample used in this paper, and the so-called CDR "compliers"—children who would be removed from the program if they received a CDR but would not be removed in the absence of a CDR. The first column gives the population median for certain characteristics. The second column shows the proportion of children with a given characteristic among those who are enrolled in the SSI program in either FY2004 or FY2005. For example, 15% of the full SSI population is in the lowest severity category. The third column does the same for children in the CDR sample used in this paper: 17% of children who are eligible for a CDR in either FY2004 or FY2005

Figure 6: Continuing Disability Reviews and Time Spent on SSI



are in the lowest severity category. The fourth column gives the proportion of CDR "compliers" with a given characteristic, calculated using the method outlined in Angrist and Pischke (2008). These are the marginal children within the CDR sample who are affected by CDRs; they are removed if they get a CDR but stay on the program if they do not get a CDR. In the case of severity, 12% of CDR compliers are in the lowest category and the remaining 88% are in the middle category.

The last three columns in Table 2 provide information on how representative the CDR sample and CDR compliers are of the full SSI population. These columns give the ratio of the CDR sample proportion to the population proportion, the ratio of the CDR complier proportion to the CDR sample proportion, and the ratio of the CDR complier proportion to the population proportion. A ratio of less than one in the last column, for example, indicates that the characteristic is underrepresented in the complier sample relative to the full SSI population, while a ratio of greater than one indicates that the characteristic is overrepresented in the complier population.

The most obvious difference across the three groups is in severity. Children with lower and medium severity conditions are overrepresented in the CDR sample and the CDR complier group because children in the highest severity category—medical improvement not expected—either do not receive CDRs or receive them very infrequently. Another major difference across the groups is in child age. In FY2003, a Deputy Commissioner of the Social Security Administration ordered that all children ages 13 through 17 be put on hold from CDRs. This order was still in effect in FY2004 and FY2005, the years from which the CDR sample is drawn. Thus all children in the CDR sample (and consequently the CDR complier group) are below the age of 13. This makes the CDR sample and CDR complier group on average much younger than the full SSI population. It also means that the children in the CDR sample entered SSI at a younger age on average than children in the full sample. The Deputy Commissioner moratorium on adolescent CDRs explains the higher proportion of young parents in the CDR sample and CDR complier group, since younger children on average have younger parents.

Other than severity and child age, the CDR sample and CDR complier group are remarkably representative of the full CDR population. Despite the media and policy attention paid to mental diagnoses, CDRs do not disproportionately remove children with mental conditions. Indeed, the three groups have nearly identical proportions of children with a mental diagnosis, which is the most common SSI child diagnosis category. This is likely a result of the "medical improvement" criterion: just as mental conditions are more difficult to verify than other conditions, they are also less likely to show definitive medical improvement. CDRs do disproportionately remove children with neoplasms and respiratory diagnoses. Both of these are diagnoses with potential for demonstrable improvements in health: children with cancer may go into remission, and children with severe asthma often grow out of their condition. At the other extreme, CDRs rarely remove children who are deaf or blind (sensory), or children with infectious, peri-natal, congenital, nervous, or endocrine diagnoses. All of these diagnoses constitute small proportions of full SSI population.

The CDR sample and CDR complier group are also representative of the full SSI population in household characteristics. The three groups have similar proportions of children in single-mother-headed households or living with no parents. Pre-treatment parental earnings are nearly identical across the three groups, with 46% of the CDR sample and 47% of the complier sample above the population annual median of $4,891. Children in the CDR complier group are somewhat more likely to have family members who have applied for disability programs in the past, but those family members are not disproportionately likely to actually receive disability payments.

Table 2: Characteristics of CDR Sample and "Complier" Populations

	Pop. Median	Composition: All SSI	Composition: CDR sample	Composition: CDR compliers	Ratios: Sample/ All	Ratios: Complier/ Sample	Ratios: Complier/ All
Severity							
Lowest (MIE)		15%	17%	12%	1.14	0.73	0.84
Middle (MIP)		69%	83%	88%	1.20	1.06	1.27
Highest (MINE)		16%	0%	0%	0.01	0.11	0.00
Diagnosis							
Mental		66%	65%	60%	0.97	0.92	0.90
Nervous		6%	7%	3%	1.19	0.47	0.56
Peri-natal		5%	2%	1%	0.42	0.69	0.29
Congenital		4%	5%	2%	1.10	0.36	0.40
Sensory		3%	3%	2%	0.99	0.54	0.54
Respiratory		3%	5%	14%	1.84	2.96	5.44
Ill-defined		2%	5%	7%	2.23	1.21	2.70
Endocrine		2%	1%	1%	0.31	1.12	0.35
Blood		1%	2%	1%	1.46	0.77	1.12
Infectious		1%	0%	0%	0.07	0.35	0.02
Neoplasm		1%	1%	5%	1.31	3.34	4.37
Demographics							
Male		65%	66%	63%	1.03	0.95	0.98
Age (above median)	11	46%	9%	20%	0.20	2.20	0.44
Age at entry (above median)	5.25	49%	27%	26%	0.55	0.94	0.52
Single mother		52%	55%	60%	1.06	1.09	1.15
No parents		10%	8%	8%	0.86	0.91	0.78
Young parent (YOB>=1975)		21%	30%	32%	1.45	1.05	1.52
Pre-treatment outcomes							
Parental earnings (above median)	$4891	50%	46%	47%	0.92	1.02	0.94
Household income (above median)	$8656	50%	43%	43%	0.86	1.00	0.86
Family disability applications (>0)	0	41%	46%	51%	1.11	1.11	1.24
Family disability receipt (>0)	0	31%	34%	34%	1.12	0.98	1.09
N		1,565,040	198,115	19,227 (est.)			

Notes: "All SSI" includes all children enrolled in SSI in FY2004 or FY2005. "CDR sample" includes all SSI children eligible for a CDR in FY2004 or FY2005. "CDR compliers" are defined as children who would be removed if given a CDR but would not be removed if not. The proportion of CDR compliers is estimated. "Lowest" severity means medical improvement expected (MIE). "Medium" severity means medical improvement possible (MIP). "Highest" means medical improvement not expected (MINE). Pre-treatment outcomes are annual averages over 1985-2002.

4 Regression Discontinuity Design

This section presents estimates from a regression discontinuity design based on a large drop in childhood CDRs at the beginning of FY2005. I first outline the estimation strategy and the regression discontinuity estimator. I then explain the RD design in more detail by explaining the first stage effects, and I test for manipulation of the running variable and the balance of covariates across the cutoff. Finally, I present reduced form estimates for parental earnings, household unearned income, and total household income.

4.1 Estimation

I use a regression discontinuity design that exploits the discrete drop in the Social Security Administration's childhood CDR budget between FY2004 and FY2005. As a result of this cut, children who were eligible for a CDR at the end of FY2004 were much more likely to receive a CDR than children who were eligible for a CDR at the beginning of FY2005.

A natural approach would be to use the child's medical diary date, which determines when a child is eligible for a CDR, as the running variable. Unfortunately, the medical diary date exists only at the month-year level, which is too coarse for this RD design. Instead, I take advantage of the fact that a majority of SSI children receive a medical diary date exactly three years after their award date. Restricting the sample to those with a medical diary date of three years, I go back exactly three years to the FY2001/2002 cutoff and use award date as the running variable since it exists at the day level.

I estimate the equation

$$Y_i = \alpha + \beta FY2001_i + \gamma AwardDate_i + \delta(AwardDate_i \times FY2001_i) + \kappa X_i + \epsilon_i$$

where Y_i is a first-stage or reduced-form outcome of interest for child i; $FY2001_i$ is an indicator equal to 1 if the child's award date is in FY2001 and 0 if the child's award date is in FY2002; $AwardDate_i$ is the child's award date; and X_i is a matrix of covariates. The coefficient of interest is β, which gives the effect on Y of having an award date on the last day of FY2001 compared to the first day of FY2002.

Using a rectangular kernel, I estimate the RD coefficients with three bandwidths for robustness: 250 days, 200 days, and 150 days. Regression coefficients for the three bandwidths are presented in Tables A.1, A.2, and A.3 of the Appendix.

Figure 7: RD Estimates for First Stage

4.2 First Stage Estimates

Figure 7 presents graphs of the effect of having an award date at the end of FY2001 instead of the beginning of FY2002 on first stage outcomes. Each dot represents the average first stage outcome across all children in the sample who enter SSI in a given week. The first graph shows the precipitous drop in the probability of receiving a CDR at the FY2001/2002 cutoff. A child who enters the program on the last day of FY2001 is 40 percentage points more likely to receive a CDR than a child who enters the program on the first day of FY2002 and 60 percentage points more likely than a child who enters one month into FY2002. As a result, the probability of receiving an unfavorable CDR decision drops by 5 percentage points across the cutoff, as shown in the second graph of Figure 7.

The bottom two graphs in Figure 7 show how the fall in removal probability translates into more time on SSI and a larger total SSI payment. A child who enters the program on the last day of FY2001 spends approximately four fewer months on the program and receives \$3000 less in total SSI payment before age 18 than a child who enters the program on the first day of FY2002. To get the effect for the average child who is *removed* as a result of a CDR, these effects must be scaled up by the removal probability, which is approximately 5 percentage points. Thus the average child who is removed as a result of a CDR spends 80 ($= 4 \times (1/0.05)$) fewer months on SSI and receives \$60,000 ($= \$3000 \times (1/0.05)$) less in SSI payment before age 18.

Note that time spent on SSI and total SSI payment are decreasing in award date on both sides of the discontinuity. This is a purely mechanical effect: since not all children in the sample have reached age 18 by the time I observe them in July 2012, children who enter SSI earlier will spend more time on the program and receive more in payment.

4.3 Tests for Manipulation and Covariate Balance Tests

A concern with any regression discontinuity design is that agents may be able to manipulate which side of the cutoff they end up on, possibly invalidating the identifying assumption that agents on either side of the cutoff are similar. Bunching on one side of the cutoff could be evidence of agents manipulating the award date, which could result in sorting across the cutoff. In this case, discontinuity estimates could reflect a selection effect rather than a treatment effect.

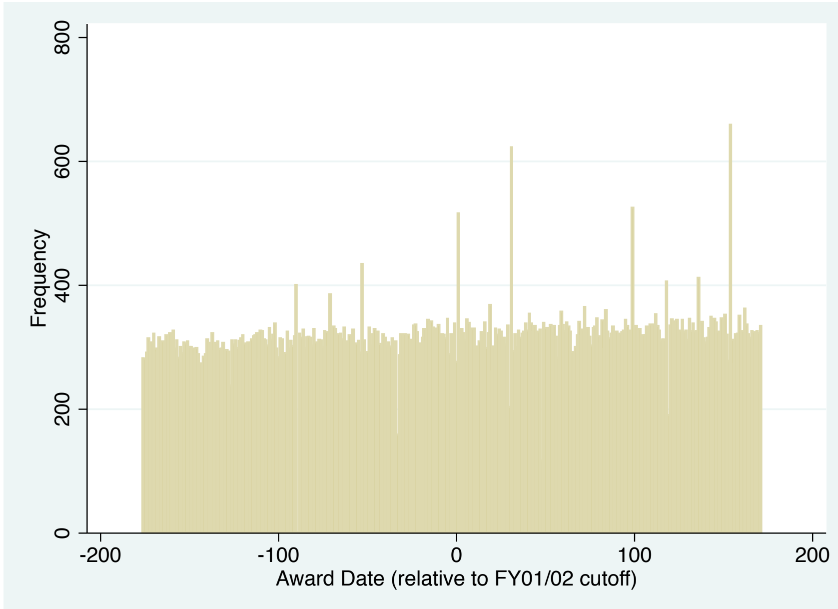
Manipulation of the running variable is highly unlikely in this institutional setting for two reasons. First, given considerable variation in waiting times, it would be nearly impossible for disability applicants to time their applications so as to receive an award in a specific week or even month. Second, and more importantly, neither disability examiners nor applicants knew three years in advance, when SSA was current on CDRs, that the Social Security Administration would cut CDRs for SSI children between FY2004 and FY2005.[4]

Nevertheless, I formally test for a discontinuity in the density function at the cutoff using the test proposed by McCrary (2007). Figure 8 presents the histogram of award dates centered at the FY2001/2002 cutoff, after controlling for day-of-month effects and week-of-year effects. As shown in the table below Figure 8, the McCrary test reveals a small but statistically significant discontinuity in the density of awards across the fiscal year cutoff for FY2001/2002. However, discontinuities of similar magnitude also exist at the two previous fiscal year cutoffs (FY2000/2001 and FY1999/2000), when there was no change in the probability of CDR release across the cutoff. Therefore I conclude that this small discontinuity is unlikely to be the result of manipulation by agents.

As another test of the validity of this RD design, I check whether the covariates are balanced across the award date cutoff. Table 3 presents covariate balance tests for diagnosis, sex, year of birth, age at entry,

[4]See, e.g., GAO Testimony to House Ways and Means, "Social Security Disability: Reviews of Beneficiaries' Disability Status Require Continued Attention to Improve Service Delivery," July 24, 2003. The impending backlog became apparent by the beginning of FY2003, after the FY2001/2002 cutoff. There was no suggestion that the backlog would be addressed by cutting CDRs for children, nor that the timing would be at the beginning of FY2005.

Figure 8: Histogram of Award Date for FY2001/2002 Cutoff



Discontinuity in density at cutoff

	Point Est.	*Std. Error*
FY01/02	0.058	(0.013)
FY00/01	0.030	(0.017)
FY99/00	0.046	(0.014)

household structure, and pre-treatment outcomes, including time on SSI, household earnings, family disability applications and receipt, and total household income. The only child characteristic with a statistically significant discontinuity across the cutoff is respiratory disease, which makes up just 3% of SSI child diagnoses. This imbalance falls within the margin of statistical chance. Appendix A contains covariate balance graphs (Figure A.1) and covariate balance estimates using alternative bandwidths (Table A.1).

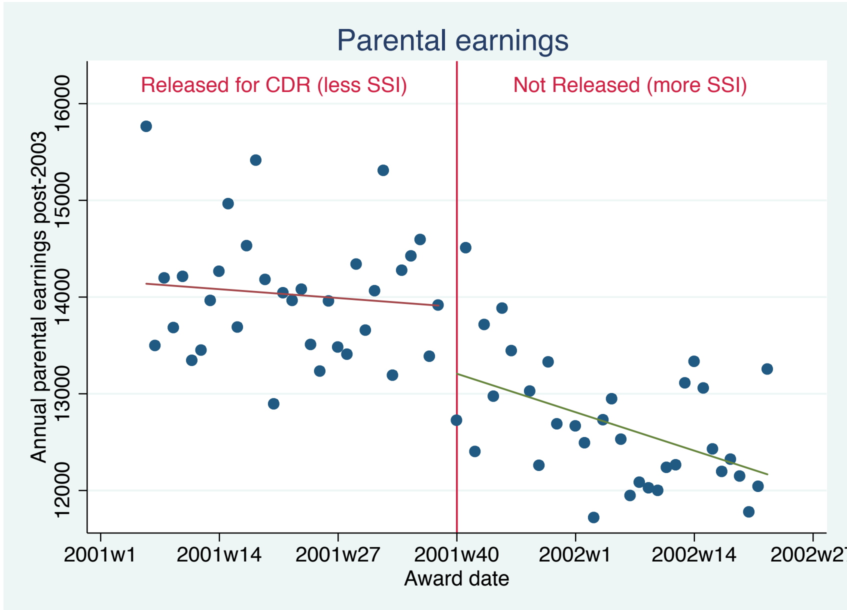
Unfortunately, there is a marginally significant discontinuity of $317 (approximately 5%) in household earnings prior to 2003 and a resulting discontinuity in total household income of $329 (3%) prior to 2003. This pre-treatment discontinuity in household earnings could confound the measurement of the effect of losing SSI on parental earnings. My investigations suggest the discontinuity is an artifact of the data. It is unlikely to be the result of seasonality in applicant demographics because there is no discontinuity in parental earnings across other fiscal year cutoffs. It is also unlikely to be the result of manipulation by disability examiners since examiners do not see the household's income when reviewing the child's case. Nevertheless, this discontinuity is cause for concern, and I address it by controlling for the household's full history of pre-treatment earnings in the RD regressions.

Table 3: Covariate Balance Tests

	Point Est.	Std. Error
Diagnosis		
Infectious	-0.0001	(0.0005)
Neoplasm	0.0005	(0.0019)
Endocrine	0.0011	(0.0013)
Blood	0.0014	(0.0019)
Mental	-0.0102	(0.0086)
Nervous	0.0063	(0.0043)
Sensory	-0.0044	(0.0033)
Circulatory	-0.0010	(0.0013)
Respiratory	0.0118***	(0.0039)
Digestive	0.0028*	(0.0017)
Genitourinary	0.0003	(0.0010)
Skin	0.0010	(0.0007)
Musculoskeletal	-0.0019	(0.0016)
Congenital	-0.0025	(0.0039)
Peri-natal	-0.0004	(0.0027)
Ill-defined	-0.0051	(0.0043)
Injury	0.0008	(0.0014)
Demographics		
Male	0.0078	(0.0086)
Year of birth	0.0157	(0.0477)
Age at initial receipt	-0.0245	(0.0475)
Single mother	0.0101	(0.0092)
Young parent	0.0095	(0.0092)
Pre-treatment outcomes		
Months on SSI	-0.0022	(0.0019)
Family disability applications	-0.0004	(0.0026)
Family disability receipt	50.07	(78.9)
Household earnings	319*	(189)
Total household income	329*	(191)
N	49,662	

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Estimated using bandwidth of 250 days and rectangular kernel.

Figure 9: RD Estimate of Household Earnings Response



4.4 Reduced Form Estimates

I present regression discontinuity graphs for several household outcomes, including parental earnings, family disability applications and receipt, and total household income. The theory of income effects predicts that households that experience a drop in unearned income will substitute toward both earned income and alternative sources of unearned income. Table 4 summarizes the first stage and reduced form RD estimates.

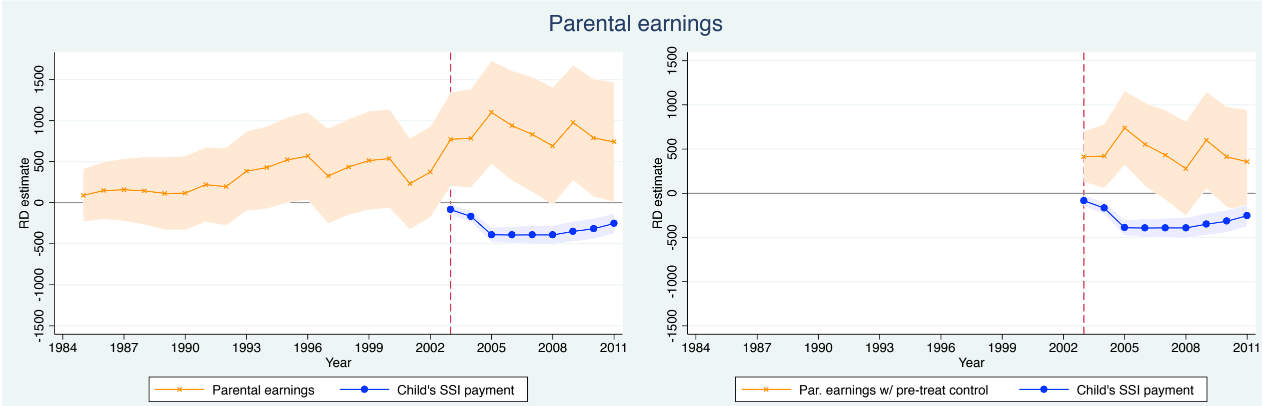
4.4.1 Parental Earnings

Figure 9 shows the reduced form effect on parental earnings. Recall that children on the left-hand side of the cutoff are removed from SSI at higher rates than children on the right-hand side of the cutoff. Consistent with the theoretical prediction of income effects, parents of children that are eligible for a CDR in FY2004 (left-hand side)—and are therefore more likely to lose their SSI payment—have substantially higher earnings after 2004 than parents whose children are eligible for a CDR in FY2005. The average annual parental earnings of children who are released for a CDR are $800 higher than the parental earnings of children who are not released for a CDR, off of a base of $13,200.[5]

Recall, however, that there is a marginally significant discontinuity in pre-treatment earnings. This pre-existing discontinuity could be artificially inflating the RD estimate of the parental earnings response. The left-hand-side graph of Figure 10 plots the RD estimate for household earnings by year going back to 1985.

[5]I use the terms "released" and "not released" as a loose shorthand for "FY2001 entry" and "FY2002 entry," respectively, in the text and the figures. As Figure 7 makes clear, CDR release rates were less than 100% for FY2001 entrants and greater than 0% for FY2002 entrants.

Figure 10: Household Earnings Response, Year by Year (RD estimates)



Notes: Figures plot the RD estimate of the effect of a child entering SSI in FY2001 (vs. FY2002) on household earnings and child's SSI payment in a given year. Right-hand-side figure controls for the full series of pre-2003 household earnings. Sample is SSI children with a 3-year CDR schedule and an award date within 250 days of the FY2001/2002 cutoff.

Each marker on this graph represents the RD estimate of the effect of entering SSI at the beginning of FY2002 (rather than the end of FY2001) on parental earnings in a given year. If the identifying assumption is valid, then entering SSI in FY2002 should have no effect on parental earnings prior to the start of CDRs in FY2004, meaning that the RD estimates in Figure 10 should be zero prior to 2003. Although the RD estimate is statistically different from zero in only one year before 2003, the estimate is always positive and reflects the existence of a discontinuity in pre-treatment parental earnings.

To account for the pre-existing discontinuity in earnings, I control for the household's full history of pre-treatment earnings, including quadratic functions, from 1985 to 2002 in the right-hand-side graph of Figure 10. Controlling for the full history of pre-treatment earnings causes the RD estimates to drop by approximately half in the years after the CDR event. Two years after the CDR event, parents whose children are released for a CDR have $600 higher annual earnings than parents whose children are not released, and this estimate is statistically different from zero. The earnings jump appears to decline substantially 7 to 8 years after the CDR event and becomes statistically indistinguishable from zero, possibly as a result of the Great Recession lowering employment rates for the treatment group. The average annual earnings increase for parents whose children are released for a CDR is $496. Since these children lose on average $327 in SSI payment, this increase in parental earnings translates to an elasticity of earnings to unearned income of greater than 1, meaning that the loss in SSI income is fully offset by increases in earned income.

4.4.2 Family Disability Applications and Receipt

Figure 11 presents the reduced form for family disability applications, which include parental DI application, parental SSI application, and sibling SSI application. Households whose children are released for a CDR have 0.013 fewer annual family disability applications than households whose children are not released for a CDR, a reduction of nearly 10%. This estimate is statistically different from zero at the 1% level. For households whose children are *removed* via CDR, family disability applications decline by fully 100% after the child's removal from SSI.

Table 4 presents reduced form estimates for the three sources of family disability applications separately. The decline in parental SSI applications is particularly large: releasing a child for a CDR reduces the number of parental SSI applications by 0.01 annually off of a base of 0.05, for a reduction of 20 percent. Scaled up by the removal probability, this amounts to 0.2 fewer parental SSI applications each year for households of removed children, or 1.6 parental SSI applications over the next 8 years. Appendix Figure A.2 presents RD graphs of each source of family disability applications and receipt separately.

Although family disability applications decline sharply as a result of CDRs, Figure 12 shows that there is no corresponding fall in family disability payment, which includes parental DI receipt, parental SSI receipt, and sibling SSI receipt. This suggests that the child's removal largely deters marginal applicants who would not be allowed onto SSDI and SSI anyway.

Who are the marginal applicants deterred from applying? The RD design allows me to look at not only the number of applicants on either side of the cutoff, but also at the composition of applicants. It would be reasonable to expect family members on the "discouraged" (left-hand) side of the cutoff to have higher severity conditions on average, since deterrence will be strongest for potential applicants with low cost of work effort and weakest for those with a very high cost of work effort. Interestingly, however, there appear to be few observable differences across the cutoff. Among parent applicants, the "discouraged" side is more likely to receive a flag for having a permanent condition, but there are no major differences across the cutoff in initial allowance rates, diagnosis, or education.

Aggregating all of the family's sources of disability income—the child's SSI payment, parental SSDI and SSI payments, and sibling SSI payments—shows a jump of $650 across the cutoff, meaning that households whose children are released for a CDR do not fully compensate for the loss of the child's SSI payment by substituting to other sources of unearned income. In fact, this decrease in total household disability income is greater than the average $450 loss of the child's SSI payment, though these two estimates are not statistically different.

These results suggest that unearned income is not an important margin of substitution for families that lose the child's SSI payment, and, if anything, households are less likely to apply for and receive disability payments after a child's removal from SSI. Households may respond to a child's removal from SSI by updating their beliefs about disability programs as a reliable source of income and may become discouraged from applying. As noted above, however, the discouragement effect appears to deter mostly marginal potential applicants who would not have been allowed onto SSDI or SSI anyway.

Figure 11: RD Estimate for Family Disability Applications

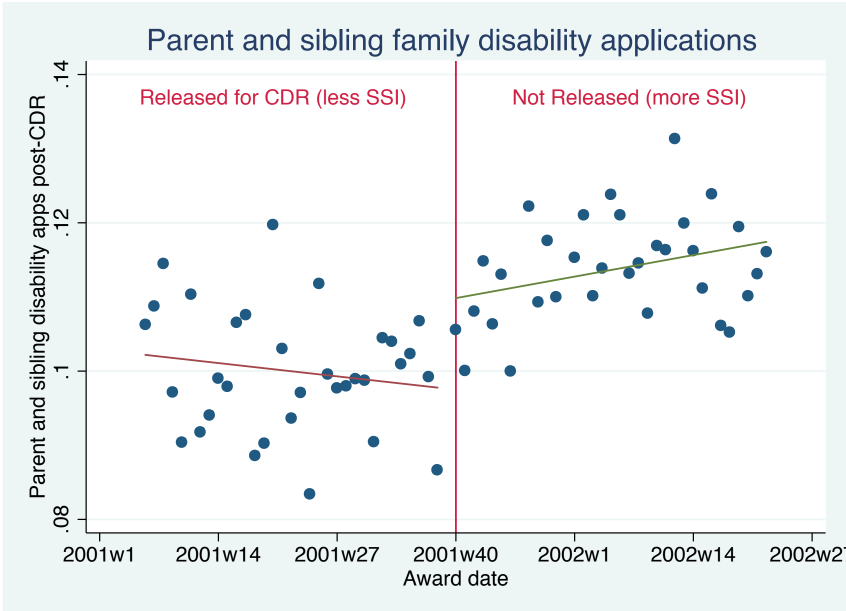


Figure 12: Family disability receipt (RD estimate)

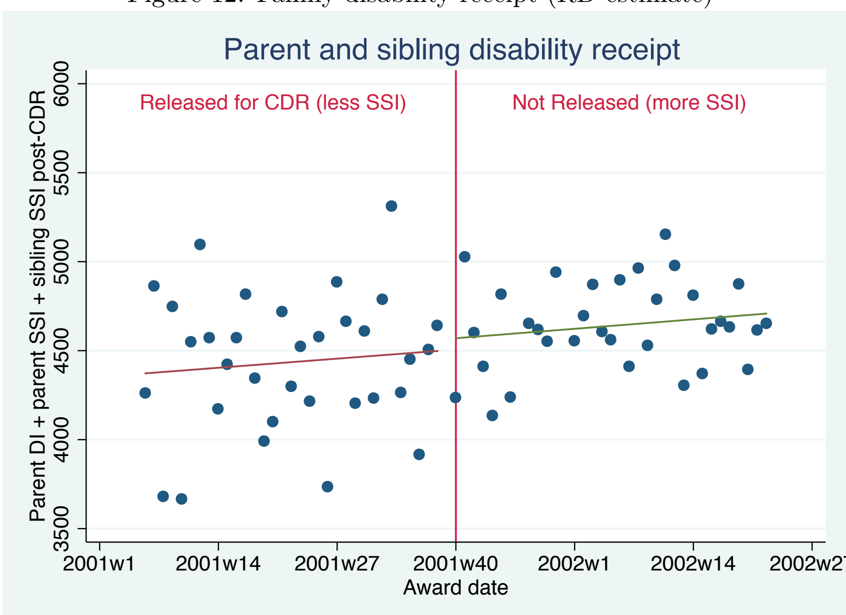


Table 4: RD Estimates for First Stage and Reduced Form Outcomes

	RD estimate		Control in	**Effect on removed**	
	Point Est.	Std. Error		Absolute	Percent
First Stage					
On SSI indicator	-0.0396***	0.0048	0.88		
Annual months on SSI	-0.5240***	0.0622	10.5	-13	-100%
Annual SSI payment	-326.7***	46.7	6,600	-8,250	-100%
Household earnings and income					
Household earnings	496.1**	201.6	13,200	12,528	95%
Unearned income (incl. child's SSI)	-260.1*	147.1	11,200	-6,568	-59%
Total household income	321.4	220.2	23,800	8,116	34%
Household disability applications					
Number of parent DI applications	-0.0036***	0.0012	0.0175	-0.09	-100%
Number of parent SSI applications	-0.0061***	0.0016	0.03	-0.15	-100%
Number of sibling SSI applications	-0.0031	0.0023	0.063	-0.08	-100%
Total family disability applications	-0.0128***	0.0036	0.11	-0.32	-100%
Household disability payment					
Parent DI payment	-15.9	43.6	650	-402	-62%
Parent SSI payment	-16.4	48.4	1,000	-414	-41%
Sibling SSI payment	104.8	110.2	3,200	2,646	83%
Total family disability payment	72.2	132.5	4,500	1,822	40%
N	49,662				

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimated using bandwidth of 250 days and rectangular kernel. Earnings and income estimates include controls for pre-treatment outcomes.

4.4.3 Total Household Income

Finally, I aggregate all of the household's observable income, including parental earnings, the child's SSI payment, the parental SSDI and SSI payments, and sibling SSI payments. The estimate is presented in Table 4. As with the parental earnings estimates, I estimate the effect of CDR release on total household income controlling for pre-treatment earnings. Remarkably, despite the large loss of the child's SSI payment for the treatment group, there is no statistically significant difference in total household income between the control and treatment groups. Households whose children are removed via CDR substitute to earned income enough to make their loss in total household income statistically indistinguishable from zero. Of course, because of data constraints, this analysis does not consider all sources of income, such as TANF or SNAP (food stamp) benefits. These are small enough benefits, however, that for most families they would replace only a fraction of the child's SSI benefit.

5 Difference-in-Differences Design

Although the regression discontinuity design produces estimates with a high degree of internal validity, its demanding data requirements limit statistical power, especially for those outcomes for which we expect small effects. In addition, the RD requires limiting the sample to children with a 3-year CDR schedule and the resulting small sample size makes it difficult to look for heterogeneity across demographic groups.

For these reasons, I turn to a difference-in-differences design that uses a larger sample of children who were eligible for a CDR in FY2004 or FY2005, rather than limiting the sample to those children with a 3-year CDR schedule. The treatment group consists of children who were eligible for a CDR in FY2004 since nearly all of them received a CDR. The control group consists of children who were eligible for a CDR in FY2005 since most of them did not receive a CDR. The reduced form difference-in-differences estimates give the difference in the outcomes of households with a child who were eligible for a CDR in FY2004, before and after the CDR event in FY2004.

Technical capacity constraints prevent me from running the full sample of children who are eligible in FY2004 or FY2005 with fixed effects. For this reason, I restrict the sample to children who enter SSI in either FY2001 or FY2002, the two years with the largest number of entrants. Restricting the sample to the two most common entry years also helps to minimize mechanical spikes and dips in pre-treatment outcomes that result from the large proportion of children with 3-year CDR schedules.[6]

[6] For example, the parental earnings reduced form estimate would dip in every pre-treatment event year that is a multiple of three because parental earnings drop in the year that children apply for and enter SSI.

5.1 Estimation

I compare children who were eligible for a CDR in FY2004 to children who were eligible in FY2005 since the former group was much more likely to receive a CDR. I estimate the equation

$$Y_{it} = \sum_{t=1992}^{2012} \beta_t (Year_t \times FY2004_i) + \sum_t \alpha_t (Year_t \times X_i) + \sum_t Year_t + \sum_i \mu_i + \nu_{it}$$

where β_t is the difference-in-differences estimate of the effect on outcome Y in year t of being eligible for a CDR in FY2004 instead of FY2005. Here μ_i is a child fixed effect; $Year_t$ is a year fixed effect; and X_i is a vector of demographic characteristics including sex, age, diagnosis, and severity. Since it uses a larger sample, the difference-in-differences estimation has more statistical power than the regression discontinuity design and is therefore more likely to produce precise results.

5.2 Mean Comparisons

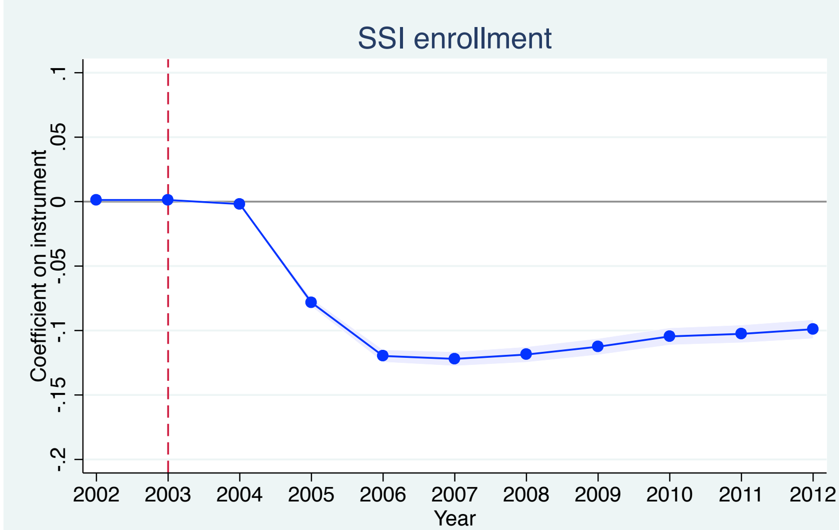
Table 5 presents comparisons of the control and treatment groups, for both the full sample and the sample of single mothers, on severity, diagnosis, sex, entry age, and year of birth. The biggest difference between the groups is in the severity of their conditions: the treatment group (FY2004-eligible children) has a smaller proportion of children with high-severity conditions. This difference is likely due to the 2001 recession, which could have pushed more marginal children onto the program in FY2001, most of whom would be eligible for a CDR in FY2004. There are also small but statistically significant differences in diagnosis, year of birth, sex, and age at entry. There are larger differences in pre-treatment outcomes, but I demonstrate below that pre-trends are minimal.

Table 5: Mean Comparisons for FY2004 and FY2005 CDR Eligible Children

	Full Sample		**Single mothers**	
	Point Est.	Std. Error	Point Est.	Std. Error
Severity				
Lower	0.119***	(0.0018)	0.122***	(0.0023)
Medium	-0.119***	(0.0018)	-0.122***	(0.0023)
Diagnosis				
Infectious	-0.0003	(0.00017)	-0.0002	(0.00024)
Neoplasm	0.0096***	(0.00081)	0.0068***	(0.00096)
Endocrine	0.0007	(0.00047)	0.0005	(0.00056)
Blood	0.0001	(0.00072)	-0.0001	(0.00109)
Nervous	-0.0005	(0.00140)	0.0006	(0.00168)
Sensory	0.0012	(0.00102)	0.0018	(0.00128)
Circulatory	0.0003	(0.00047)	-0.0004	(0.00059)
Respiratory	-0.0003	(0.00130)	0.0024	(0.00191)
Digestive	0.0007	(0.00054)	0.0006	(0.00066)
Genitourinary	0.0001	(0.00033)	0.0005	(0.00040)
Skin	-0.0001	(0.00022)	-0.0003	(0.00029)
Musculoskeletal	0.0007	(0.00054)	0.0003	(0.00067)
Congenital	0.0007	(0.00119)	0.0026*	(0.00136)
Peri-natal	-0.0002	(0.00083)	-0.0011	(0.00096)
Ill-defined	0.0120***	(0.00144)	0.0113***	(0.00187)
Injury	0.0002	(0.00045)	0.0008	(0.00057)
Demographics				
Male	-0.0015	(0.00291)	0.0014	(0.00384)
YOB	-0.854***	(0.0175)	-0.838***	(0.0231)
Age at initial receipt	0.121***	(0.0174)	0.105***	(0.0230)
Single mother	0.0012	(0.00309)		
No parents	-0.0011	(0.00176)		
Young parent (YOB>=1975)	-0.0407***	(0.00290)	-0.0481***	(0.00404)
Pre-treatment outcomes				
Family disability applications	0.0038***	(0.00095)	0.0050***	(0.00140)
Family disability receipt	187***	(27.6)	141***	(35.6)
Parental earnings	574***	(63.8)	429***	(55.5)
Total household income	986***	(64.3)	840***	(56.3)
N	103,631		58,233	
F-test	72211		41824	
p-value	0.0000		0.0000	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Full sample is children who were eligible for a CDR in FY2004 or FY2005 and entered SSI in FY2001 or FY2002. Single-mother sample is children from full sample in single-mother-headed households.

Figure 13: DD Estimate for First Stage



Notes: Figure plots the first stage estimate of the effect of a child being eligible for a CDR in FY2004 (vs. FY2005) on SSI enrollment in a given year. Sample is SSI children in single-mother-headed households who are eligible for a CDR in FY2004 or FY2005.

5.3 First Stage Estimates

As shown in Figure 13, children who are eligible for a CDR in FY2004 are 8 percentage points less likely to be on SSI one year after 2004 and 12 percentage points less likely two years out, relative to children who are eligible in FY2005. As a result, the treatment group receives on average $712 less in SSI payment each year relative to the control group. Scaling up this effect on SSI payment by the differential probability of removal translates to an average annual loss of $7,400—approximately the maximum annual SSI payment—for children who are removed as a result of a CDR.

It is interesting to note from Figure 13 that the treatment effect declines slightly over time. By 2012, the treatment group is only 10 percentage points less likely to be on SSI relative to the control group, compared to 12 percentage points in 2006. This decline indicates that some children who are removed from SSI as a result of a CDR successfully reapply to the program as either children or adults, or that children in the control group are eventually removed from the program.

5.4 Reduced Form Estimates

This section presents reduced form estimates for parental earnings, family disability applications and receipt, and total household income. In addition, I explore heterogeneity by child age in the parental earnings response and attempt to isolate the income effect of SSI payments by restricting the sample to households with pre-SSI earnings below the income phase-out range. I present parental earnings estimates for single mothers because these estimates are more precise than those for the full sample, though though they are comparable in magnitude. The full sample results are presented in Appendix B. Table 6 summarizes the difference-in-differences estimates.

5.4.1 Parental earnings

The first graph of Figure 14 shows the year-by-year effect of being in the treatment group on parental earnings for the sample of single mothers. Parental earnings exhibit parallel trends in the years before the treatment takes place. The notable exception, of course, is the clear drop in parental earnings in 2001, the year in which the treatment group children enter the program. This "Ashenfelter dip" in the year of program entry has one of two interpretations: 1) households respond to a drop in parental earnings (e.g., as a result of job loss) by applying for SSI for their children, or 2) households respond to securing SSI child benefits by reducing parental labor supply. Since this dip is the mechanical effect of the treatment and control groups entering SSI in different years, it does not violate the parallel trends assumption. To account for this mechanical difference, however, I exclude the year 2001 when calculating the difference-in-differences estimates.

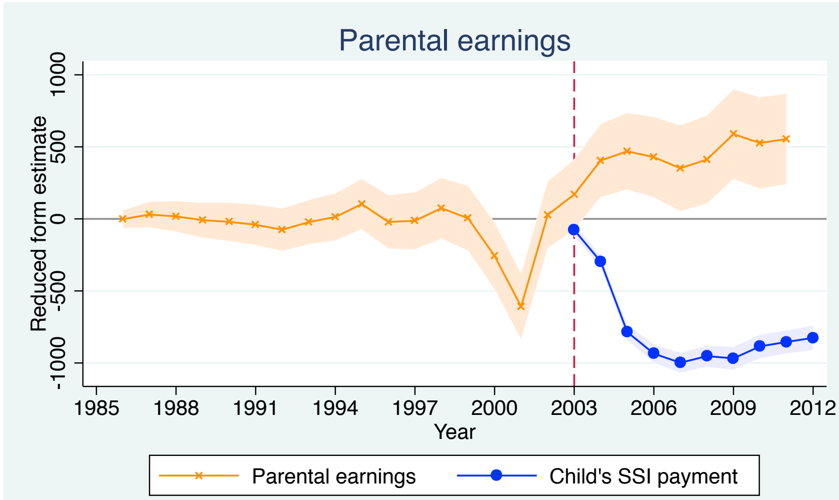
Households in the treatment group increase their earnings by $468 a year relative to households in the control group. Since the annual loss in SSI payment is on average $756 for the treatment group, this effect on parental earnings implies an elasticity of earnings to unearned income of 0.6 for this population.

I also explore heterogeneity in the parental earnings response by child age. Households with children of different ages may differ in their response to the loss of the SSI payment. On the one hand, parents of older children may find substituting to earnings less costly than parents of younger children, who require more parental supervision. On the other hand, the households of younger children lose more in net present value from SSI as a result of CDRs and may be more likely to increase their earnings. For example, if families expect to continue receiving SSI payments until the child's 18th birthday, then the family of a 5-year-old who is removed via CDR loses 13 years of payment, whereas the family of a 12-year-old loses just 6 years of payment.

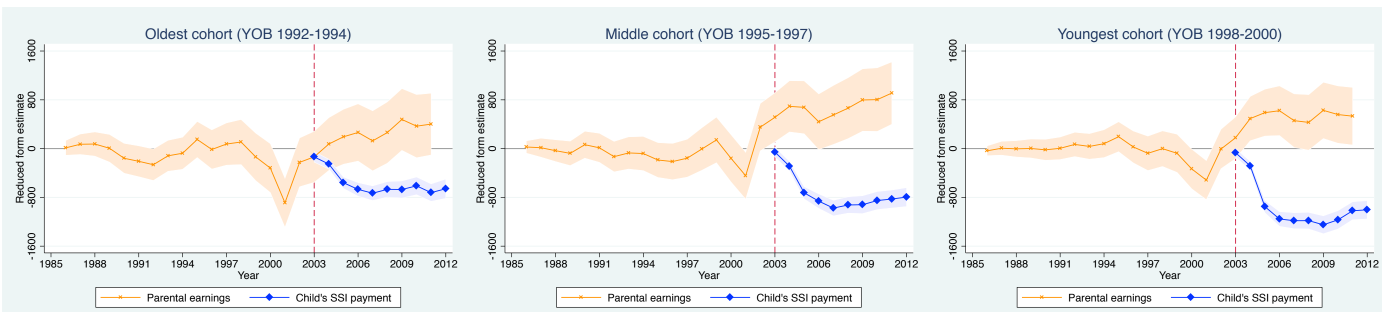
The bottom panel of Figure 14 presents parental earnings estimates by child age, controlling for parent age. Note that the first stage effect of CDRs on SSI payment is monotonically decreasing in child age. Older children are more likely to have received and survived previous CDRs and as a result of this selection are less affected by the current CDR round.

As we would expect based on net present value of the SSI loss, the parents of younger children are much more responsive to the loss of the child's SSI payment. The parents of the oldest cohort of children do not increase their earnings as a result of losing the child's SSI payment. In contrast, there is a large effect for the middle and youngest cohorts. In the middle age cohort, parents increase their earnings by around $800 annually, which is approximately equal to the loss in SSI payment. In the youngest age cohort, annual

Figure 14: Parental Earnings Response (DD estimates)



Notes: Figure plots the reduced form estimate of the effect of a child being eligible for a CDR in FY2004 (vs. FY2005) on household earnings and child's SSI payment in a given year. The dip in the earnings series in 2001 is a mechanical effect of low household earnings for the treatment group (FY2004 eligibles) in the year of entry (FY2001). Sample is SSI children in single-mother-headed households who are eligible for a CDR in FY2004 or FY2005.



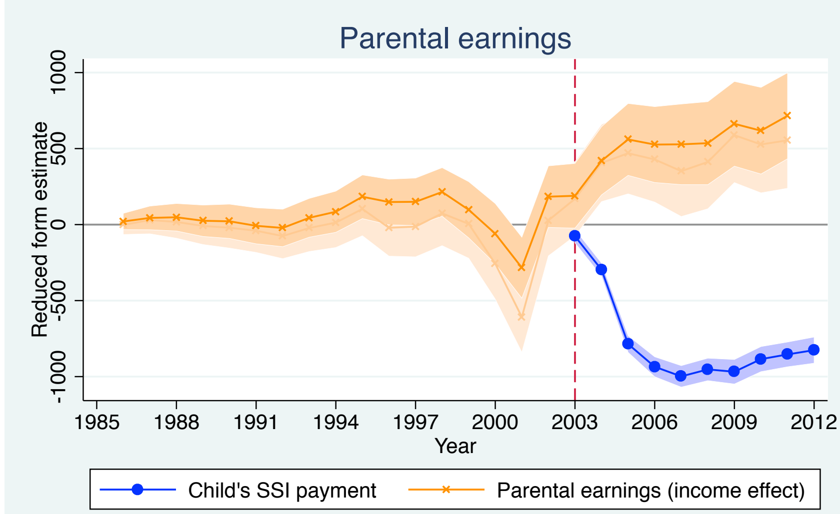
Notes: Figures plot the reduced form estimate of the effect of a child being eligible for a CDR in FY2004 (vs. FY2005) on household earnings and child's SSI payment in a given year for each age cohort, controlling for parent age. Sample is SSI children in single-mother-headed households who are in the given age range and eligible for a CDR in FY2004 or FY2005.

parental earnings increase by around $600, which is less than the annual loss in SSI payment of $1000. Of course, this heterogeneity by child age could be consistent with other factors that are correlated with child age. I control for parental age, which is possibly the most important confounding factor.

5.4.2 Income versus substitution effects

The reduced form effect of SSI payments on parental earnings involves both income effects, which are non-distortionary, and substitution effects, which are distortionary. The transfer of income to the family causes the household to shift from work to leisure, while marginal tax rates on earnings also discourages parents from working. Recall from Section 1.3, however, that the SSI children's program treats parental earnings generously relative to the potential earnings of the target population. For example, a single parent with two children, one of whom is on the SSI children's program, can have up to $2000 in monthly earnings before the SSI payment is phased out.

Figure 15: Isolating the Income Effect of the Parental Earnings Response



Notes: Figure plots the reduced form estimate of the effect of a child being eligible for a CDR in FY2004 (vs. FY2005) on household earnings and child's SSI payment in a given year. Sample is SSI children in single-mother-headed households eligible for a CDR in FY2004 or FY2005. 'Income effect' sample means households with annual earnings less than $24,000 three years before SSI entry.

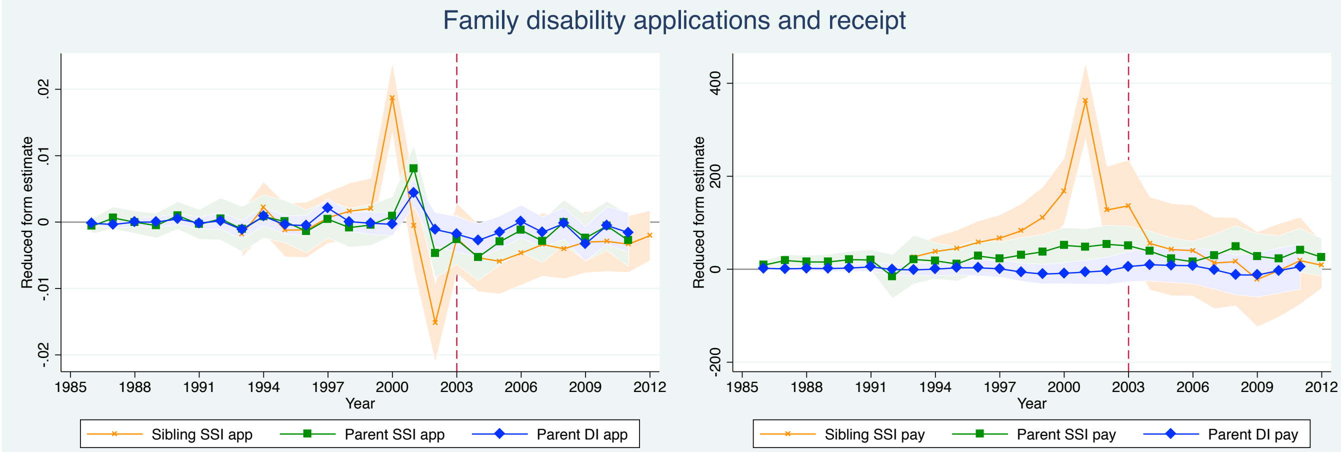
To measure the parental earnings response in a context in which income effects are likely to predominate, I restrict the sample to households with potential earnings below the benefit phase-out range. Approximately 90 percent of single-parent households with children on SSI have earnings below the benefit phase-out range prior to the child's entry onto SSI. Figure 15 overlays the reduced form parental earnings estimates for this 90 percent of single-mother households onto the estimates for the full single-mother sample. The two series are statistically indistinguishable. The "income effect" sample increases its earnings by $480 annually, compared with $468 for the full single-mother sample. This suggests that the income effect could explain the entire labor supply discouragement effect of SSI children's payments.

5.4.3 Family Disability Applications and Receipt

Figure 16 presents reduced form estimates for parental and sibling disability applications and receipt. The graph on the left shows the household's disability application response, while the one on the right shows the household's disability receipt response. Consistent with the RD results, the difference-in-differences approach estimates a statistically significant decline in parental DI and SSI applications as well as sibling SSI applications across the cutoff, with no corresponding statistically significant changes in family disability payments.

The most noticeable feature of these graphs is that both parental and sibling disability applications spike for the treatment group relative to the control group in 2000 and 2001, the years in which the treatment group children apply for and enter SSI. (Sibling applications also dip in 2002, when control group siblings

Figure 16: Household Unearned Income Response (DD estimates)



Notes: Figures plot the reduced form estimates of the effect of a child being eligible for a CDR in FY2004 (vs. FY2005) on parental SSI and DI applications and sibling SSI applications (LHS) as well as parental SSI and DI payments and sibling SSI payment (RHS) in a given year. The spikes and dips in 2000-2002 are mechanical effects of parents and siblings applying for and entering disability concurrently with the treatment group (FY2004 eligibles) in its year of entry (FY2001). Sample is SSI children who are eligible for a CDR in FY2004 or FY2005.

are still applying but treatment group siblings have finished applying.) It appears that multiple members of a household apply for disability in the same year, suggesting the importance of household-level shocks in the decision to apply for disability. For example, a job loss may induce a household to submit applications for SSDI and SSI for multiple members, or simply hearing about the availability of these programs could lead households to apply for multiple members at once. For parents, this spike in applications does not translate into a spike in SSDI or SSI receipt. For siblings, however, there is a clear increase in SSI receipt in the year in which the child enters the program. As with the mechanical dip in parental earnings, these spikes do not violate the parallel trends assumption.

Simple tabulations of the data confirm nontrivial rates of co-application among family members. Of the approximately 200,000 children who are eligible for a CDR in FY2004 or FY2005 and have a current medical diary date, 4.0 percent have a parent who applies for SSI within 60 days and 3.4 percent have a parent who applies for SSDI within 60 days. Similarly, 12.1 percent of children have a sibling who applies for SSI within 60 days of their application. In total, 15 percent of children have a parent who applies for DI or SSI or a sibling who applies for SSI within 60 days. About 46 percent have a parent who ever applies for SSDI or SSI, 48 percent have a sibling who ever applies for SSI, and 65 percent have either a parent or a sibling who ever applies for disability.

From Table 6, aggregating all sources of disability income (the child's SSI payment, parental SSDI and SSI payments, and sibling SSI payments) reveals that households in the treatment group experience an annual loss of nearly \$800 in disability income, or the full amount of the loss in the child's SSI payment. With the child's SSI payment excluded, there is no statistically significant change in other family members' disability

Table 6: Difference-in-Differences Estimates

	DD estimate		Avg effect for removed	
	Point Est.	Std. Error	Absolute Δ	Percent Δ
First stage				
On SSI indicator	-0.0957***	(0.0022)		
Months on SSI	-1.32***	(0.027)	-13.8	-100%
SSI payment	-712***	(19.3)	-7439	-100%
Household earnings and income				
Household earnings	437***	(115)	4563	32%
Household earnings (single mothers)	468***	(109)	4638	55%
Household earnings (income effect)	480***	(106)	4755	57%
Unearned income (incl. child's SSI)	-793***	(54.2)	-8287	-72%
Total household income	-368***	(114)	-3840	-15%
Household SSI and DI applications				
Number of parental SSI applications	-0.0013**	(0.0006)	-0.0133	-71%
Number of parental DI applications	-0.0018**	(0.0008)	-0.0189	-58%
Number of sibling SSI applications	-0.0042***	(0.0010)	-0.0434	-69%
Total family disability applications	-0.0072***	(0.0018)	-0.0751	-66%
Household disability payment				
Parental DI payment	-0.50	(17.6)	5.3	1%
Parental SSI payment	7.6	(15.6)	79.4	8%
Sibling SSI payment	-59.3*	(34.0)	-620	-20%
Total family disability payment	-25.5	(47.3)	-267	-6%

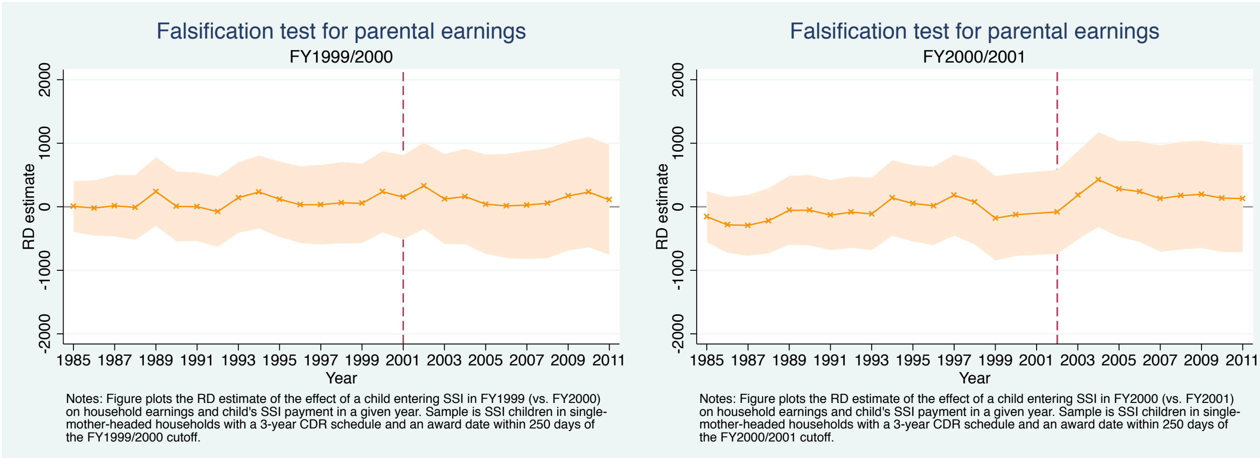
Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Average effect for removed calculated by scaling up first stage and reduced form estimates by the removal probability (i.e., the estimate for On SSI Indicator).

payments after the CDR event, consistent with the RD results.

5.4.4 Total income

Finally, I aggregate all observable sources of household income: parental earnings, the child's SSI payment, parental SSDI and SSI payments, and sibling SSI payments. In contrast to the RD estimates, which showed no decline in total household income, the DD design produces a statistically significant decrease of $368 in total household income. In the DD design, the increase in parental earnings does not fully offset decline the decrease in the child's SSI payment, and alternative sources of unearned income do not increase to compensate.

Figure 17: Placebo Tests for Regression Discontinuity Design



6 Robustness checks

6.1 Regression discontinuity placebo tests

To probe the validity of the regression discontinuity results, I conduct placebo tests at the two previous fiscal year cutoffs. For the FY1999/2000 cutoff, for example, I limit the sample to children who are eligible for a CDR in FY2002 or FY2003, have a 3-year CDR schedule, and have an award date within 250 days of the FY1999/2000 cutoff. I estimate the equation

$$Y_i = \alpha + \beta FY1999_i + \gamma AwardDate_i + \delta(AwardDate_i \times FY1999_i) + \kappa X_i + \epsilon_i$$

where Y_i is parental earnings in a given year between 1985 and 2011. I make analogous sample restrictions and estimate the analogous equation for the FY2000/2001 cutoff.

Figure 17 presents the results of these placebo tests. In contrast to the FY2001/2002 award date cutoff, which shows a large increase in the parental earnings estimate after the CDR event, there is no change in the parental earnings estimates for the placebo tests. This bolsters the credibility of the results of the true quasi-experiment.

6.2 Alternative identification strategies

I use two alternative identification strategies to estimate the effect of the SSI children's program on household income and earnings. First, I use the Social Security Number cutoff rule implemented by SSA to allocate CDRs to children in FY2005. Under this assignment rule, SSA ordered eligible children from each state in ascending order of Social Security Number and chose an SSN cutoff based on the state's capacity to conduct

CDRs. Children with SSNs less than the cutoff were released for a CDR, while children with SSNs greater than the cutoff were not released. Appendix C presents the results of this regression discontinuity design using SSN as the running variable.

The second alternative identification strategy is quasi-random assignment of CDR examiners to childhood cases, analogous to the Maestas, Mullen, and Strand (2012) initial examiner instrument. I use records from the Disability Operational Data Store (DIODS) on childhood CDRs conducted between October 2004 and December 2006. I control for DDS office, having a permanent flag, and body code. Appendix D presents the results of the CDR examiner instrument.

Table 7 summarizes the estimates from the four identification strategies used in this paper: the main RD design, the main DD design, the Social Security Number RD design, and the CDR examiner DD design. The estimates from the two alternative identification strategies are generally consistent with the main identification strategies, though they are imprecise because of much smaller sample sizes.

Table 7: Estimates from Main and Alternative Identification Strategies

	Strategy 1: Main RD			**Strategy 2: Main DD**			**Strategy 3: SSN RD**			**Strategy 4: Examiner DD**		
	Point Est.	Std. Error	Sig.	Point Est.	Std. Error	Sig.	Point Est.	Std. Error	Sig.	Point Est.	Std. Error	Sig.
First stage												
On SSI	-0.040	(0.005)	***	-0.096	(0.002)	***	-0.102	(0.009)	***	0.130	(0.040)	***
Months on SSI	-0.524	(0.062)	***	-1.32	(0.027)	***	-1.32	(0.120)	***	1.52	(0.489)	***
SSI payment	-327	(46.7)	***	-712	(19.3)	***	-845	(93.4)	***	917	(305)	***
Earned and unearned income												
Parental earnings	496	(202)	**	437	(115)	***	486	(315)		-647	(451)	
Unearned income	-260	(147)	*	-793	(54.2)	***	-1369	(292)	***	1083	(477)	**
Total income	321	(220)		-368	(114)	***	-662	(351)	*	661	(487)	
Family disability applications												
Parental DI applications	-0.0036	(0.0012)	***	-0.0013	(0.0006)	**	-0.0029	(0.0038)		7.82E-06	(0.0019)	
Parental SSI applications	-0.0061	(0.0016)	***	-0.0018	(0.0008)	**	-0.0033	(0.0054)		-0.0005	(0.0025)	
Sibling SSI applications	-0.0031	(0.0023)		-0.0042	(0.0010)	***	-0.0035	(0.0045)		0.0001	(0.0030)	
Total applications	-0.0128	(0.0036)	***	-0.0072	(0.0018)	***	-0.0108	(0.0099)		0.0009	(0.0051)	
Family disability payments												
Parental DI payment	-15.9	(43.6)		0.504	(17.6)		108	(92.8)		-60.9	(55.7)	
Parental SSI payment	-16.4	(48.4)		7.60	(15.6)		-20.2	(112)		49.7	(45.4)	
Sibling SSI payment	105	(110)		-59.3	(34.0)	*	-475	(192)	**	21.0	(93.5)	
Total payments	72.2	(133)		-25.5	(47.3)		-422	(263)		-92.8	(138)	
N (clusters)	49,687			103,631			9,914			5,308		

Notes: *** p<0.01, ** p<0.05, * p<0.1.

7 Discussion and Conclusion

The RD and difference-in-differences estimates paint a clear picture of household substitution patterns after the loss of a child's SSI payments. Parents respond to the loss of the SSI payment by increasing their earnings dramatically. I estimate an elasticity of earnings to unearned income of between 0.6 and 1, meaning that a loss of $1000 in annual SSI payment results in an annual earnings increase of at least $600. For households of younger children, who lose more in net present value than households of older children, this estimate range is closer to 1.0. The sign of this effect is consistent with economic theory, but its magnitude is at odds with both neoclassical theory and the few existing empirical estimates. Neoclassical economic theory predicts an elasticity of earnings to unearned income of less than one because earned income, unlike unearned income, imposes a cost of work effort. Indeed, the few existing empirical estimates of this elasticity are well below one. Keane and Moffitt (1998) estimate an elasticity of 0.2 in the response of low-income families to welfare programs, and Imbens, Rubin, and Sacerdote (2001) find an elasticity of 0.1 for lottery winners.

Why are the elasticity estimates found in this paper so much larger than previous estimates? I see three (not mutually exclusive) potential explanations.

The first explanation is that the elasticity is large for this very low-income population, but previous estimates have focused on higher-income populations for whom we expect lower elasticities. There are many reasons to expect a large elasticity for the SSI population. Low-income populations are more likely to face liquidity constraints that prevent them from smoothing consumption, and they may also have a higher cost of work effort because of the low-quality jobs available to them. Both of these circumstances could lead to a large responsiveness of labor supply to unearned income. Moreover, for this particular sample of single mothers with disabled children, the opportunity cost of working may be very high because it means leaving a disabled child at home unsupervised.

Second, a child's SSI payment functions like an annuity until the child turns 18, provided that the family can demonstrate a lack of medical improvement. SSI is a guaranteed income stream of a set amount each month until the child's 18th birthday. Thus the SSI payment is worth more in consumption value than an equivalent amount of earned income because earned income comes with a probability of job loss, especially for households with low education levels that are marginally attached to the labor force. If a household were to lose this guaranteed income stream unexpectedly, it would have to find employment that offers a higher annual income than the SSI payment to maintain an income stream of equal value. This "overcompensation" effect is increasing in the risk aversion of the household. Of course, since earned income comes with a disutility of work, the household may choose not to replace the SSI payment more than one-for-one with earned income.

The third explanation is that households adapt to a certain consumption level when they receive the

SSI payment. Since my analysis observes changes on the exit margin, all of the households in the sample have been receiving SSI income for some years prior to the CDR event. Receiving a reliable stream of income may accustom families to a consistent consumption level or increase their valuation of certain types of consumption afforded to them by SSI income. SSI income may, for example, provide medical treatment or educational opportunities to children that the household comes to value highly. As a result of this adaptation, the household has a much higher marginal utility of consumption upon losing the SSI payment than it had before entering SSI, and it may attempt to maintain its SSI consumption level by increasing its earnings to match the SSI payment.

In future work, I will attempt to test the third explanation by examining the household earnings elasticity when households enter SSI. If households behave asymmetrically with respect to earnings when they are gaining versus losing unearned income, then this is evidence for an adaptation story. If they are equally responsive on the entry and exit margins, then adaptation is not a likely explanation for the large elasticity estimate on the exit margin.

Considered within the unique institutional structure of the SSI children's program, where income effects are likely to predominate, the large response in parental earnings to the SSI payment suggests that income effects are an important component of the overall labor supply discouragement effect of disability insurance. This finding may explain the general failure of work incentive programs like Ticket to Work, which attempt to increase program participants' labor force participation by reducing marginal tax rates on their benefits. Work incentive programs are unlikely to work if participants have low labor force participation simply because the income from disability payments shifts their preferences from labor to leisure, as this finding suggests. In a normative sense, this result also indicates that much of the labor supply discouragement effect of disability insurance programs is non-distortionary.

In contrast to the large elasticity of earned income, I find that families that lose the child's SSI payment do not substitute or attempt to substitute to other sources of unearned income. In fact, parental SSDI and SSI applications, as well as sibling SSI applications, fall after the loss of the child's SSI payment, suggesting that households become discouraged from applying for disability and that this deterrence effect trumps any desire to substitute to other cash welfare programs. However, it appears that losing the child's SSI payment mostly discourages marginal potential applicants who would not be allowed onto disability insurance anyway.

The results also provide substantial evidence of the importance of household-level shocks in the decision to apply for disability. The difference-in-differences results reveal that several members of a household apply for disability together, suggesting that households may be responding to an income shock such as job loss or to an information shock like hearing about disability programs. Of course, this finding is not inconsistent with individual health shocks as an important determinant of disability application decisions.

Beyond the distinction between income and substitution effects, the normative implications of these findings on household earnings and income are not clear. Parental substitution toward work may be costly in the sense that it reduces the amount of time available for parents to care for their disabled children. In future work, it will be important to assess the normative effects of the SSI children's program through the educational and employment outcomes of enrolled children.

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Appendix A: Regression Discontinuity Design

Figure A.1: RD Covariate Balance Graphs

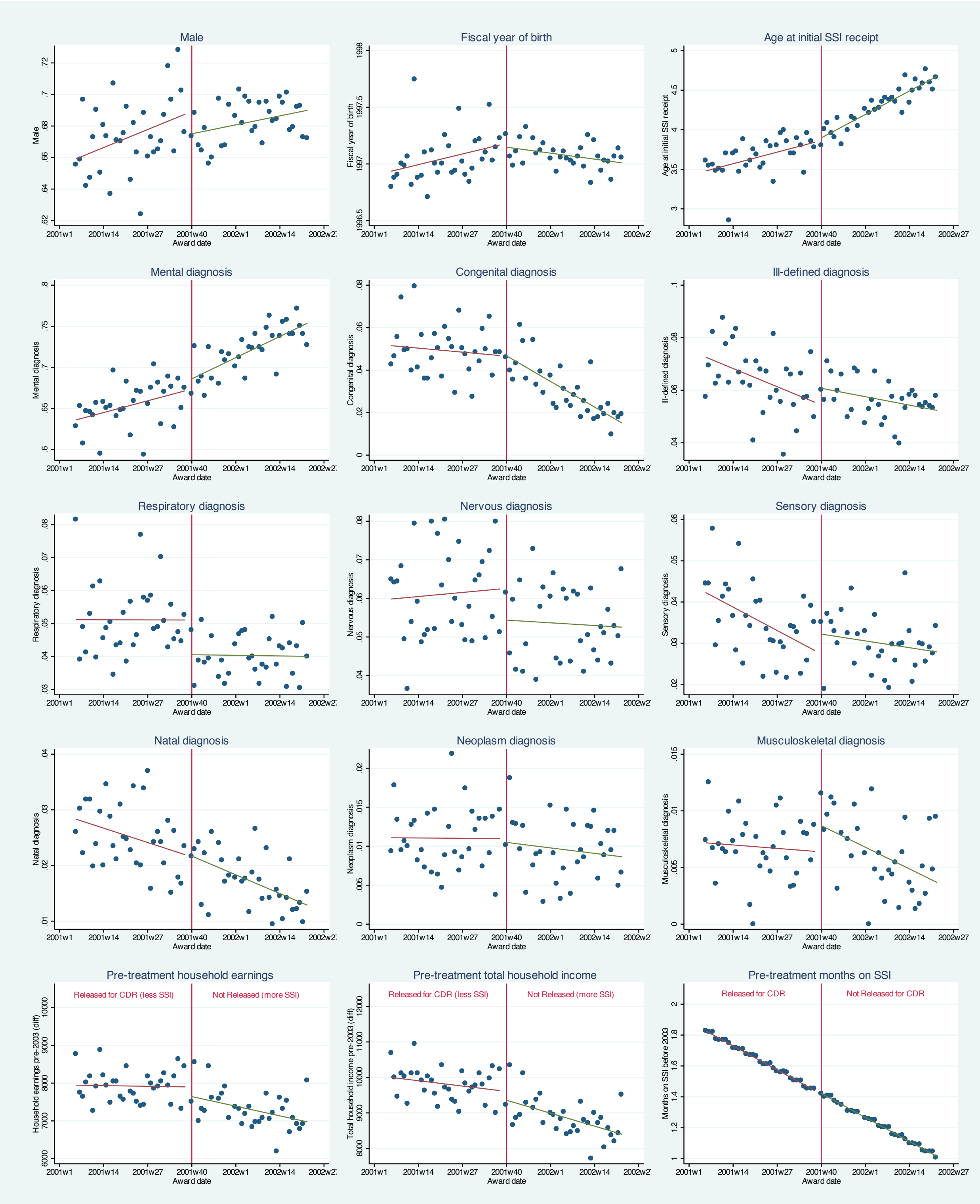


Figure A.2: RD Family Disability Application and Receipt Graphs

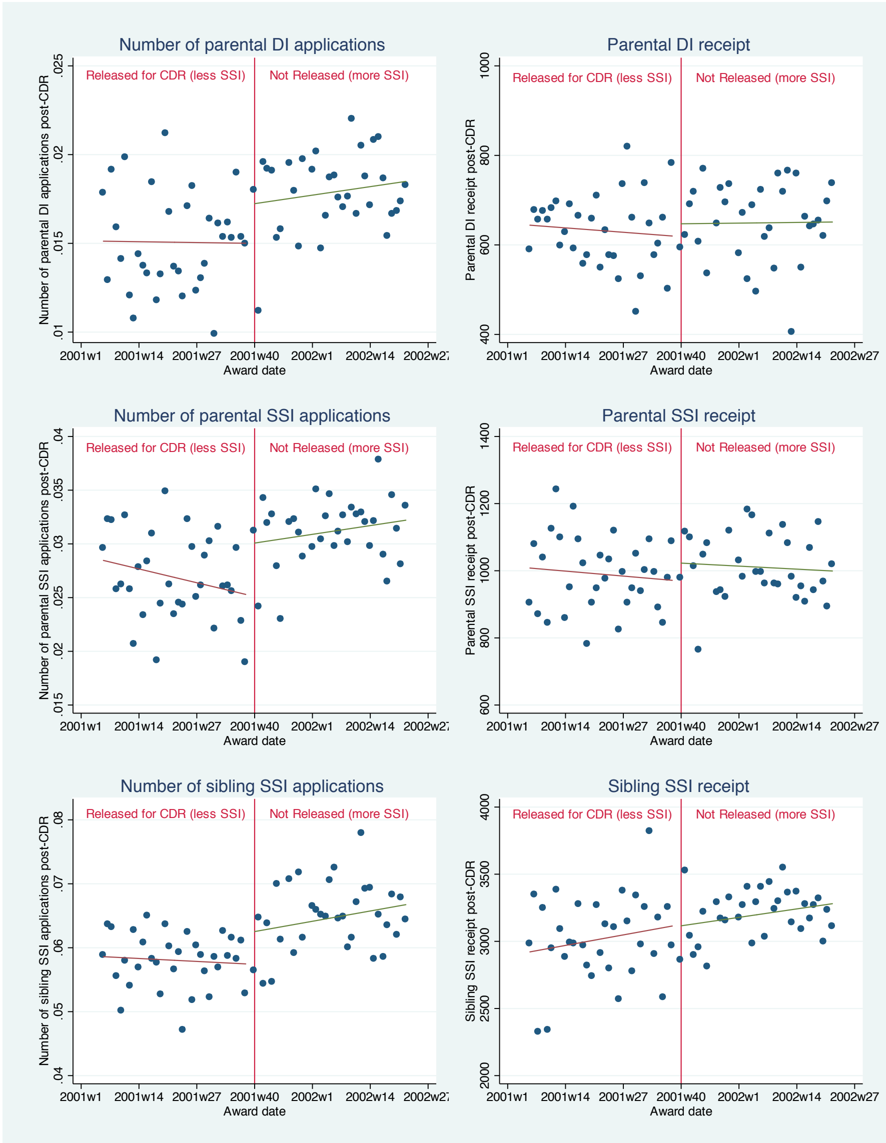


Table A.1: RD Covariate Balance Estimates Using Alternative Bandwidths

	Bandwidth: 250 days		**Bandwidth: 200 days**		**Bandwidth: 150 days**	
	Point Est.	Std. Error	Point Est.	Std. Error	Point Est.	Std. Error
Diagnosis						
Infectious	-0.0001	0.0005	-0.0007	0.0006	-0.0007	0.0007
Neoplasm	0.0005	0.0019	0.0019	0.0021	0.0006	0.0025
Endocrine	0.0011	0.0013	0.0006	0.0015	0.0003	0.0017
Blood	0.0014	0.0019	0.0019	0.0021	0.0005	0.0025
Mental	-0.0102	0.0086	-0.0077	0.0097	-0.0105	0.0114
Nervous	0.0063	0.0043	0.0038	0.0049	0.0041	0.0058
Sensory	-0.0044	0.0033	-0.0049	0.0037	-0.0047	0.0044
Circulatory	-0.0010	0.0013	-0.0013	0.0015	-3.68e-05	0.0018
Respiratory	0.0118***	0.0039	0.0128***	0.0043	0.0102**	0.0051
Digestive	0.0028*	0.0017	0.0033*	0.0019	0.0049**	0.0023
Genitourinary	0.0003	0.0010	-0.0001	0.0011	-0.0006	0.0013
Skin	0.0010	0.0007	0.0011	0.0007	0.0010	0.0009
Musculoskeletal	-0.0019	0.0016	-0.0036**	0.0018	-0.0023	0.0021
Congenital	-0.0025	0.0039	-0.0002	0.0043	-0.0006	0.0052
Peri-natal	-0.0004	0.0027	0.0005	0.0030	-0.0005	0.0035
Ill-defined	-0.0051	0.0043	-0.0084*	0.0049	-0.004	0.0057
Injury	0.0008	0.0014	0.0011	0.0016	0.0024	0.0018
Demographics						
Male	0.0078	0.0086	0.012	-0.0096	0.0216*	0.0113
Year of birth	0.0157	0.0477	0.0273	0.0536	0.0372	0.0631
Age at initial receipt	-0.0245	0.0475	-0.0381	0.0533	-0.0512	0.0627
Single mother	0.0101	0.0092	0.0156	0.0103	0.0129	0.0121
Young parent	0.0095	0.0092	0.0108	0.0103	0.0153	0.0121
Household characteristics						
Months on SSI	-0.0022	0.0019	-0.0025	0.0021	-0.0012	0.0025
Family disability applications	-0.0004	0.0026	-0.0001	0.0030	0.0047	0.0035
Family disability receipt	50.07	78.92	29.46	88.59	104.5	104.3
Parental earnings	319.2*	189.1	363.5*	212.3	420.4*	249.3
Total household income	328.8*	190.6	358.0*	214	470.1*	251.2
N	49,687		39,596		28,461	

Table A.2: RD First Stage Estimates Using Alternative Bandwidths

	No covariates		**With covariates**	
	Point Est.	Std. Error	Point Est.	Std. Error
	Bandwidth: 250 days (N=49,687)			
Released for CDR	0.342***	(0.00627)	0.345***	(0.00594)
Unfav CDR before 18	0.0470***	(0.00574)	0.0471***	(0.00562)
Months on SSI before 18	-4.704***	(0.536)	-4.474***	(0.528)
SSI payment before 18	-3,249***	(438.4)	-3,155***	(426.1)
Annual months on SSI (post)	-0.547***	(0.0631)	-0.524***	(0.0622)
Annual SSI payment (post)	-336.5***	(47.78)	-326.7***	(46.68)
On SSI (post)	-0.0413***	(0.00491)	-0.0396***	(0.00484)
	Bandwidth: 200 days (N=39,596)			
Released for CDR	0.334***	(0.00692)	0.333***	(0.00665)
Unfav CDR before 18	0.0502***	(0.00652)	0.0496***	(0.00638)
Months on SSI before 18	-4.656***	(0.607)	-4.389***	(0.597)
SSI payment before 18	-3,234***	(495.0)	-3,029***	(480.7)
Annual months on SSI (post)	-0.541***	(0.0714)	-0.512***	(0.0704)
Annual SSI payment (post)	-332.9***	(53.99)	-311.8***	(52.71)
On SSI (post)	-0.0405***	(0.00556)	-0.0382***	(0.00548)
	Bandwidth: 150 days (N=28,461)			
Released for CDR	0.274***	(0.00770)	0.270***	(0.00756)
Unfav CDR before 18	0.0441***	(0.00776)	0.0433***	(0.00758)
Months on SSI before 18	-4.017***	(0.725)	-3.751***	(0.713)
SSI payment before 18	-2,859***	(587.7)	-2,603***	(569.9)
Annual months on SSI (post)	-0.471***	(0.0852)	-0.440***	(0.0839)
Annual SSI payment (post)	-294.9***	(64.16)	-266.4***	(62.54)
On SSI (post)	-0.0348***	(0.00664)	-0.0323***	(0.00653)

Table A.3: RD Reduced Form Estimates Using Alternative Bandwidths

	No covariates		**With covariates**		**+Pre-treat controls**	
	Point Est.	Std. Error	Point Est.	Std. Error	Point Est.	Std. Error
			Bandwidth: 250 days (N=49,687)			
Family disability applications						
Parent DI applications	-0.00368***	(0.00120)	-0.00356***	(0.00119)		
Parent SSI applications	-0.00632***	(0.00165)	-0.00612***	(0.00164)		
Sibling SSI applications	-0.00443*	(0.00236)	-0.00312	(0.00234)		
Family disability applications	-0.0144***	(0.00365)	-0.0128***	(0.00360)		
Child SSI applications	0.00118**	(0.000470)	0.00121**	(0.000470)		
Family disability receipt						
Parent DI payment	-18.39	(43.67)	-15.93	(43.60)		
Parent SSI payment	-19.73	(48.98)	-16.41	(48.43)		
Sibling SSI payment	41.70	(111.8)	104.8	(110.2)		
Family disability payment	6.819	(135.1)	72.15	(132.5)		
Household earnings and income						
Household earnings	825.7***	(293.0)	855.9***	(289.8)	496.1**	(201.6)
Household income	631.3**	(298.7)	741.6**	(298.1)	321.4	(220.2)
HH unearned income	-334.7**	(150.6)	-260.1*	(147.1)		
			Bandwidth: 200 days (N=39,596)			
Family disability applications						
Parent DI applications	-0.00205	(0.00134)	-0.00197	(0.00133)		
Parent SSI applications	-0.00481***	(0.00183)	-0.00462**	(0.00182)		
Sibling SSI applications	-0.00251	(0.00265)	-0.00138	(0.00263)		
Family disability applications	-0.00936**	(0.00407)	-0.00797**	(0.00402)		
Child SSI applications	0.00111**	(0.000523)	0.00112**	(0.000523)		
Family disability receipt						
Parent DI payment	-19.37	(48.94)	-17.45	(48.89)		
Parent SSI payment	-34.78	(55.14)	-31.42	(54.55)		
Sibling SSI payment	109.5	(126.1)	177.1	(124.5)		
Family disability payment	51.53	(152.7)	121.2	(149.9)		
Household earnings and income						
Household earnings	783.8**	(330.6)	801.4**	(326.3)	353.0	(226.4)
Household income	668.3**	(337.0)	779.8**	(336.0)	213.6	(247.9)
HH unearned income	-285.3*	(170.4)	-194.8	(166.6)		
			Bandwidth: 150 days (N=28,461)			
Family disability applications						
Parent DI applications	-0.00245	(0.00157)	-0.00243	(0.00156)		
Parent SSI applications	-0.00535**	(0.00212)	-0.00531**	(0.00211)		
Sibling SSI applications	-0.000532	(0.00310)	0.000272	(0.00308)		
Family disability applications	-0.00833*	(0.00477)	-0.00747	(0.00471)		
Child SSI applications	0.000205	(0.000618)	0.000188	(0.000616)		
Family disability receipt						
Parent DI payment	-17.47	(57.24)	-20.47	(57.19)		
Parent SSI payment	19.85	(64.63)	23.22	(63.97)		
Sibling SSI payment	201.7	(148.2)	262.5*	(146.4)		
Family disability payment	196.7	(179.3)	255.4	(176.0)		
Household earnings and income						
Household earnings	900.6**	(386.6)	911.9**	(381.0)	428.1	(263.4)
Household income	977.0**	(394.1)	1,070***	(392.8)	494.2*	(289.9)
HH unearned income	-100.6	(200.4)	-13.83	(195.8)		

Appendix B: Difference-in-Differences

Figure B.1: Parental Earnings and Household Income Response, Full Sample (DD estimates)

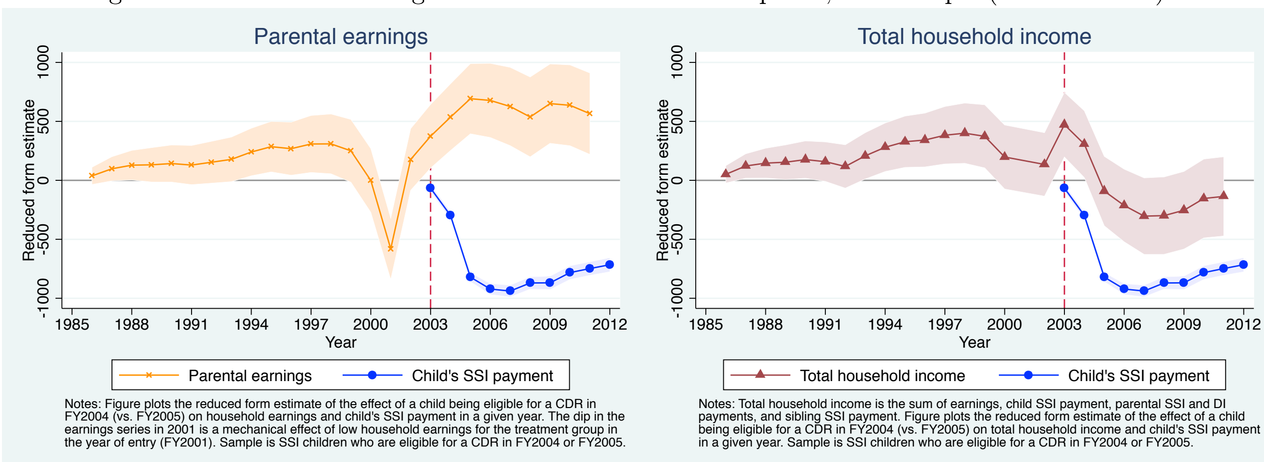
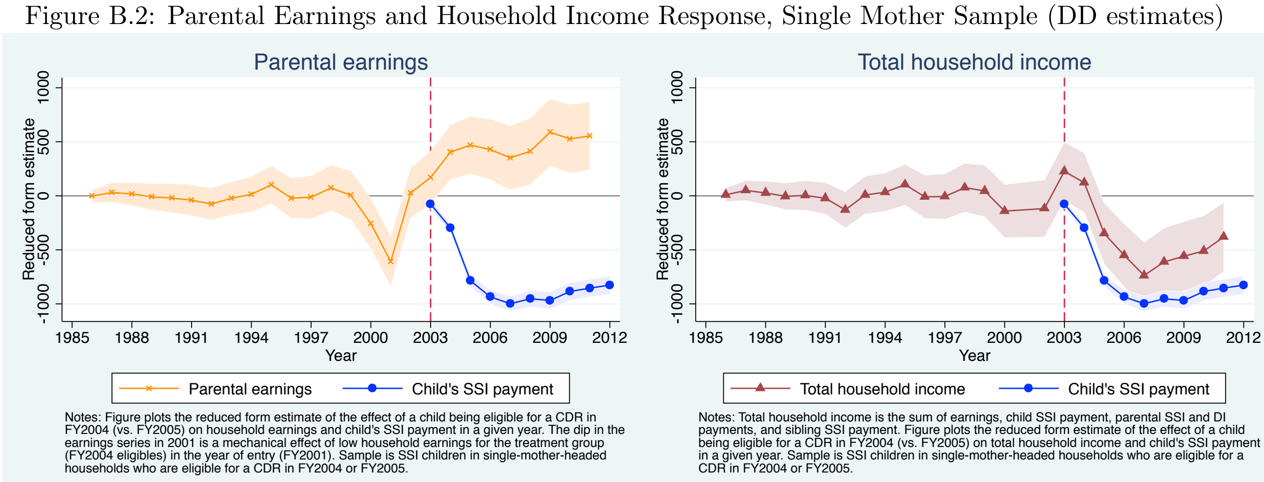


Figure B.2: Parental Earnings and Household Income Response, Single Mother Sample (DD estimates)



Appendix C: SSN Regression Discontinuity Design

The first alternative identification strategy comes from variation in CDR assignment induced by a the Social Security Administration's method of allocating the limited number of CDRs available in FY2005. Under the Social Security Number cutoff rule, SSA ranked eligible children from each state in ascending order of Social Security Number and chose an SSN cutoff based on the state's capacity to conduct CDRs. Children with

SSNs less than the cutoff were released for a CDR, while children with SSNs greater than the cutoff were not released.

Estimation

I employ a regression discontinuity approach using the Social Security Number cutoff rule for assigning CDRs to children in FY2005. I estimate the equation

$$Y_i = \alpha + \beta BelowCut_i + \gamma SSN_i + \delta SSN_i \times BelowCut_i + \kappa X_i + \epsilon_i$$

where $BelowCut_i$ is an indicator equal to 1 if the child has a SSN below the cutoff and is therefore released for a CDR; and SSN_i, the running variable, is an affine transformation of the child's Social Security Number. Once again, the coefficient of interest is β, which gives the effect on Y of having a SSN just below the cutoff rather than just above the cutoff.

As with the main identification strategy, I use alternative bandwidths to probe the robustness of these results. With the largest bandwidth including 9,914 children, this identification strategy has a much smaller sample size than the main identification strategy is therefore less likely to produce precise estimates.

Covariate Balance Tests

The key identifying assumption of this quasi-experiment is that the only difference on average between children just below and just above the SSN cutoff is their probability of receiving a CDR in FY2005. A necessary condition for this assumption to hold is quasi-random assignment of SSNs. Given the sensitive nature of Social Security Numbers, SSA publishes limited information on the assignment of SSNs, which take the form AAA-GG-NNNN. [7] The first AAA digits are an "area" number corresponding to a particular state. The middle GG digits ("group" number) are assigned based on date of application for SSNs, which in general corresponds to birth date. SSA states that the final NNNN digits, known as the serial number, are randomly assigned. Acquisti and Gross (2009) find that SSN assignment, including the serial number, is correlated with birth state and birth date.

Table C.1 presents RD estimates for covariates. The covariates are balanced after controlling for the factors correlated with SSN: current state, year of birth, birth state, and mover status. Figure C.1 also shows that the covariates are balanced across the SSN cutoff.

[7] This discussion applies to SSN assignment prior to 2011. According to SSA, SSN assignment became fully random in 2011.

Table C.1: RD Estimates for Covariates

	Bandwidth: 10000000		**Bandwidth: 1000000**	
	Point Est.	Std. Error	Point Est.	Std. Error
Demographics				
Male	-0.0304	(0.0193)	-0.0388	(0.0241)
Age at initial SSI receipt	-0.0116	(0.0963)	0.0080	(0.1250)
Diagnosis				
Neoplasm	0.0074	(0.0053)	0.0058	(0.0061)
Endocrine	-0.0046**	(0.0022)	-0.0030	(0.0023)
Blood	0.0019	(0.0053)	0.0007	(0.0071)
Mental	0.0163	(0.0175)	0.0108	(0.0219)
Nervous	-0.0060	(0.0095)	-0.0019	(0.0113)
Sensory	-0.0035	(0.0068)	-0.0072	(0.0087)
Circulatory	-0.0075***	(0.0028)	-0.0084**	(0.0034)
Respiratory	-0.0008	(0.0088)	0.0012	(0.0108)
Digestive	0.0022	(0.0038)	0.0041	(0.0048)
Genitourinary	-0.0003	(0.0020)	0.0007	(0.0017)
Skin	0.0022	(0.0016)	0.0044	(0.0027)
Musculoskeletal	0.0036	(0.0042)	0.0034	(0.0058)
Congenital	0.0000	(0.0074)	-0.0055	(0.0084)
Peri-natal	-0.0037	(0.0047)	-0.0047	(0.0059)
Ill-defined	-0.0014	(0.0083)	0.0002	(0.0101)
Injury	-0.0024	(0.0033)	0.0011	(0.0037)
Pre-treatment outcomes				
Months on SSI	0.0315	(0.0590)	0.0360	(0.0764)
Household earnings	249	(209)	76.43	(267)
Total household income	-4.35	(212)	-144	(267)
N	9,914		7,455	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Includes controls for factors correlated with SSN assignment: YOB, current state, birthstate, and mover status.

Figure C.1: Covariate Balance Tests for SSN Regression Discontinuity Design

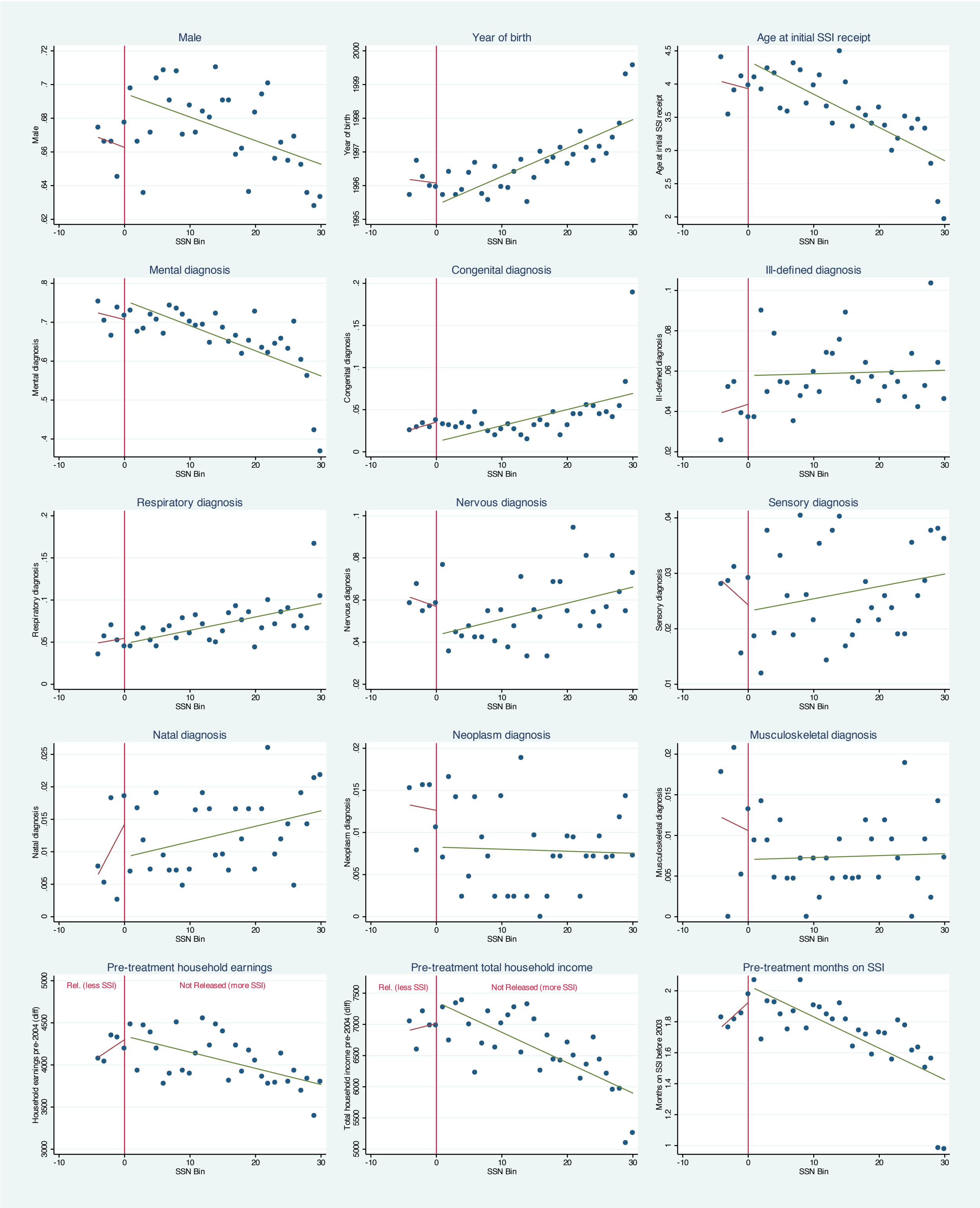
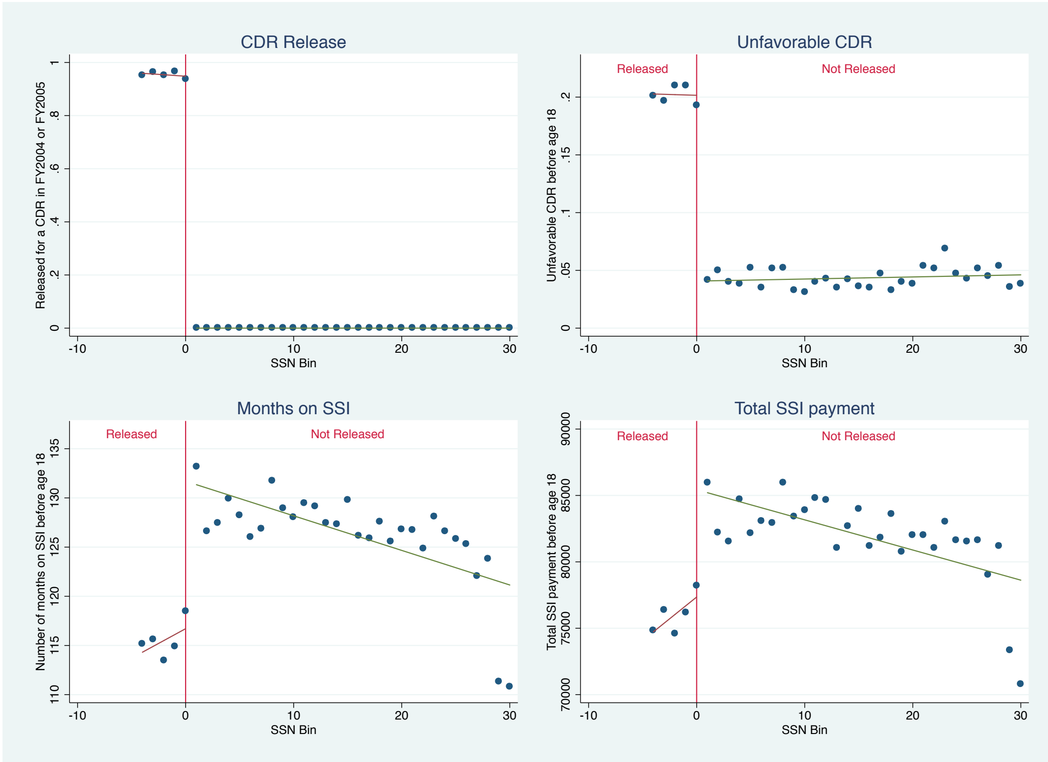


Figure C.2: First Stage Graphs for SSN RD Design



First Stage Estimates

The first graph of Figure C.2 confirms that nearly all children with SSNs below the cutoff received a CDR, while no children with SSNs above the cutoff received a CDR. As a result, as shown in the second graph, children below the cutoff had unfavorable CDR rates of 20 percent, compared to less than 5 percent for children above the cutoff.

The bottom two graphs of Figure C.2 depict the resulting jump at the cutoff of about 15 months spent on SSI before age 18 and more than $8000 in SSI payment before age 18. Reassuringly, as seen in Figure C.1, there is no discontinuity at the cutoff in the number of months spent on SSI before 2004, nor any discontinuity in pre-treatment earnings or income. Table C.2 presents RD estimates controlling for covariates.

Reduced Form Estimates

Figure C.3 and Table C.3 present reduced form results for parental earnings, total household income, household disability applications, and household disability receipt. The reduced form estimates for the SSN RD design are generally consistent with results from the main identification strategy, though they are underpowered due to a much smaller sample size. Family disability applications decline for households whose children are released for a CDR, though the estimates are not statistically significant. Households whose children

Table C.2: First Stage Estimates for SSN RD Design

	Bandwidth: 10000000		Bandwidth: 1000000	
Released for CDR	0.953***	(0.00656)	0.946***	(0.00875)
Time to next CDR	-6.044***	(0.125)	-5.953***	(0.170)
Unfavorable CDR before age 18	0.148***	(0.0135)	0.138***	(0.0178)
Months of payment before age 18	-9.981***	(1.463)	-8.767***	(1.884)
Total SSI payment before age 18	-6,322***	(1,131)	-5,097***	(1,441)
Annual on SSI indicator (post)	-0.102***	(0.00944)	-0.0937***	(0.0124)
Annual months on SSI (post)	-1.318***	(0.120)	-1.210***	(0.157)
Annual SSI payment (post)	-845.2***	(93.41)	-744.0***	(120.2)
N	9,914		7,455	

are released for a CDR experience a statistically significant decline in total household income relative to households whose children are not released, since the parental earnings increase is not large enough to offset the loss of the child's SSI payment. Figure C.4 presents reduced form graphs for parent and sibling disability applications and receipt separately.

Appendix D: CDR Examiner Difference-in-Differences Design

The second alternative identification strategy is quasi-random assignment of CDR examiners to childhood cases, analogous to the Maestas, Mullen, and Strand (2012) initial examiner instrument. I use records from the Disability Operational Data Store (DIODS) on approximately 112,000 childhood CDRs conducted by about 5000 examiners between October 2004 and December 2006. Based on my conversations with DDS offices, initial child applications and childhood CDRs are treated similarly to adult applications and CDRs in that they are randomly assigned to examiners. Following Maestas, Mullen, and Strand (2012, I include DDS office fixed effects to restrict to within-DDS variation and control for body code since it is a highly observable feature of a case. I also control for having a permanent flag, another highly observable feature of a case.

Variation in Examiner Allowance Rates

Figure D.1 plots a histogram of examiner allowance rates and shows a large spread. I condition on the examiner reviewing at least 50 child cases to ensure that the variation in examiner allowance rates is not coming from small sample sizes. There are 611 examiners with at least 50 cases.

Figure C.3: Reduced Form Graphs for SSN RD Design

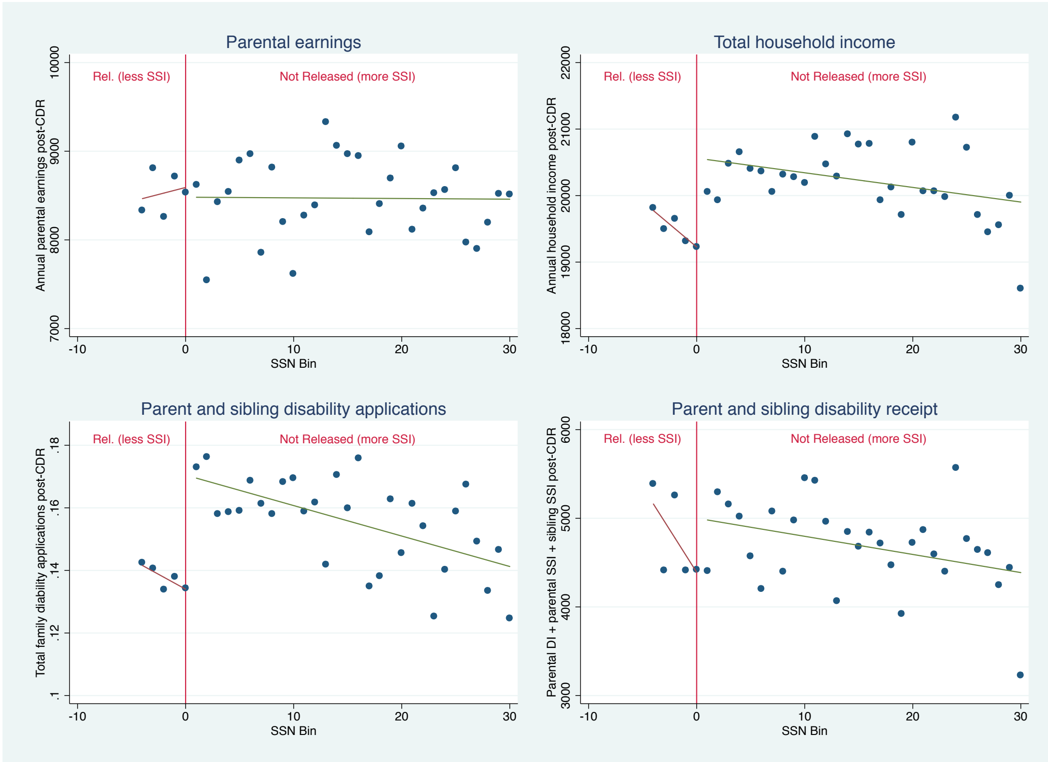


Table C.3: Reduced Form Estimates for SSN RD Design

	Bandwidth: 10000000		**Bandwidth: 1000000**	
	Point Est.	Std. Error	Point Est.	Std. Error
Household earnings and income				
Household earnings	441	(334)	441	(417)
Household unearned income	-1,376***	(291)	-1,255***	(361)
Total household income	-858**	(366)	-924**	(451)
Family disability applications				
Parental SSI applications	-0.0038	(0.0054)	-0.0022	(0.0069)
Parental DI applications	-0.0024	(0.0038)	-0.0043	(0.0046)
Sibling SSI applications	-0.0035	(0.0045)	-0.0044	(0.0052)
Total family disability applications	-0.0109	(0.0099)	-0.0112	(0.0122)
Family disability payments				
Parental SSI payment	-34.4	(110)	129	(139)
Parental DI payment	116	(93)	142	(117)
Sibling SSI payment	-475**	(192)	-617***	(237)
Total family disability payments	-430	(263)	-393	(324)
N	9,914		7,455	

Figure C.4: Family Disability Application and Receipt Graphs for SSN RD Design

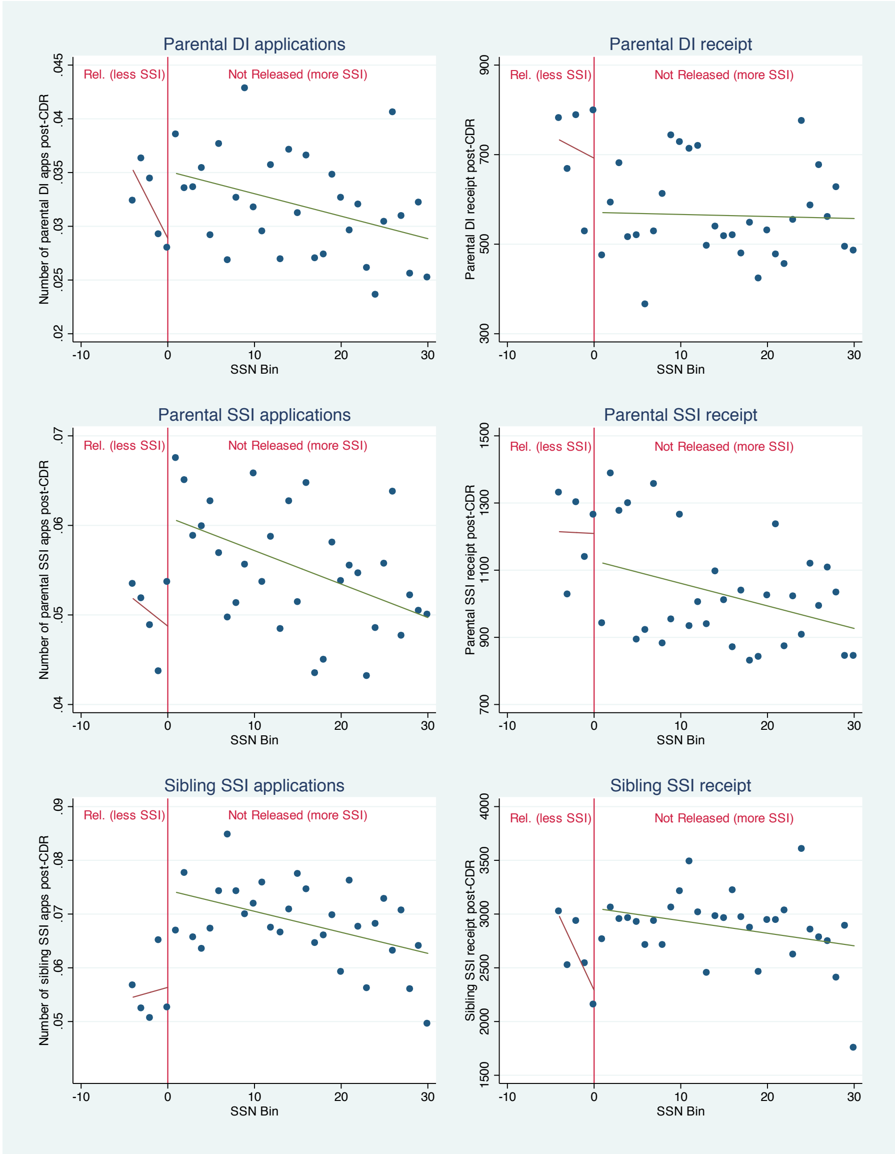


Figure D.1: Histogram of Examiner Allowance Rates

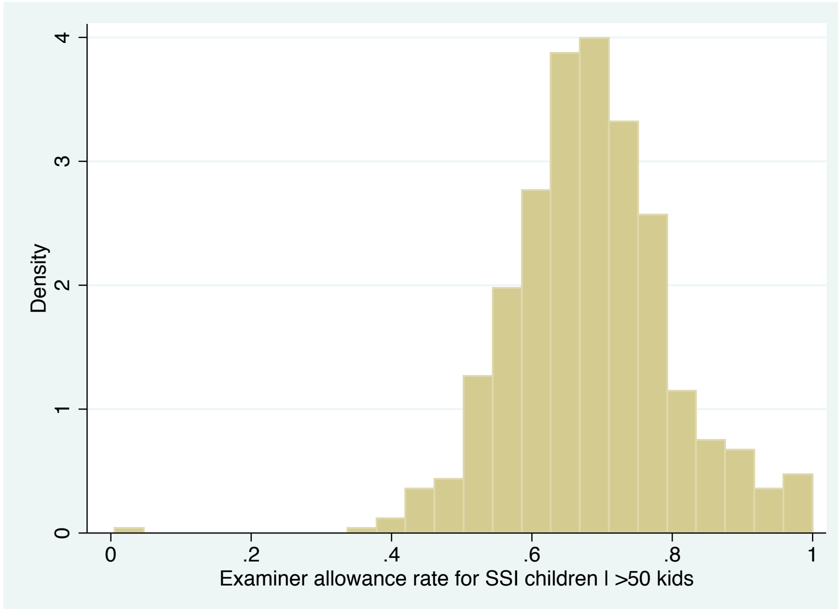


Table D.1: CDR Examiner Instrument Covariate Balance Tests

	p-values on joint F tests		
	No covariates	*Permanent*	*Permanent, body code*
LHS variables in SUR			
Permanent indicators	0.0000	NA	NA
Body code indicators	0.0000	0.0026	NA
(Permanent,) (body code,) CDR age, CDR month, CDR year, first age, YOB	0.0000	0.0000	0.0038
Previous list except CDR age	0.0000	0.0020	0.2782
Pre-treatment covariates	0.0019	0.0213	0.0647
All covariates	0.0000	0.0000	0.0030
All covariates except CDR age	0.0000	0.0019	0.2635

Covariate Balance Tests

Table D.1 presents p-values for the joint F-tests from seemingly unrelated regression. Without any covariates, the F test rejects the null of covariate balance for all groups of covariates. As in Maestas, Mullen, and Strand (2012), controlling for a permanent indicator and body code indicator leads most groups of covariates to pass the F test.

First Stage Estimates

I estimate the first stage using three different specifications. The first specification includes no covariates:

$$
\begin{aligned}
Y_{ijt} &= \alpha + \sum \beta_t(EventYr_t \times Allow_{ij}) \\
&+ \psi_t + \gamma_1 DecisionDate_i + \sum \gamma_{2t}(EventYr_t \times DDS_j) \\
&+ \epsilon_{ijt}
\end{aligned}
$$

Here Y_{ijt} is an outcome for child i assigned to examiner j in year t. The instrument in this equation is $Allow_{ij}$, the examiner's allowance rate for SSI kids; it is subscripted by both i and j because it is the leave-one-out mean. DDS_j is a matrix of disability determination services offices fixed effects, so that effects are identified within DDS office across examiners. The estimating equation also includes calendar year effects (ψ_t) and the CDR decision date.

The second specification includes a permanent indicator and body code indicators:

$$
\begin{aligned}
Y_{ijt} &= \alpha + \sum \beta_t(EventYr_t \times Allow_{ij}) \\
&+ \psi_t + \gamma_1 DecisionDate_i + \sum \gamma_{2t}(EventYr_t \times DDS_j) \\
&+ \sum \gamma_{3t}(EventYr_t \times Permanent_i) + \sum \gamma_{4t}(EventYr_t \times BodyCode_i) + \epsilon_{ijt}
\end{aligned}
$$

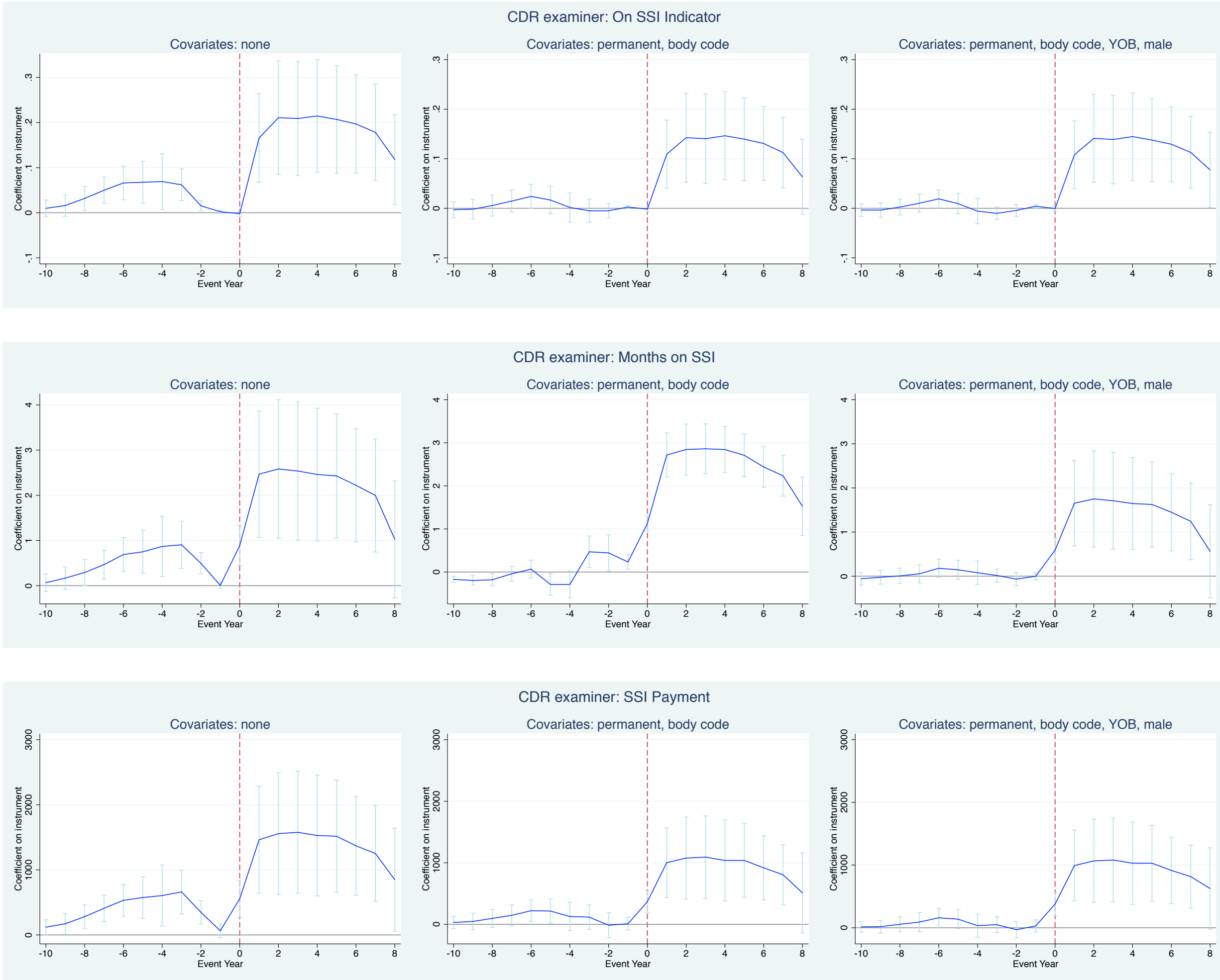
The third specification includes permanent indicator, body code, YOB, and male:

$$
\begin{aligned}
Y_{ijt} &= \alpha + \sum \beta_t(EventYr_t \times Allow_{ij}) \\
&+ \psi_t + \gamma_1 DecisionDate_i + \sum \gamma_{2t}(EventYr_t \times DDS_j) \\
&+ \sum \gamma_{3t}(EventYr_t \times Permanent_i) + \sum \gamma_{4t}(EventYr_t \times BodyCode_i) \\
&+ \sum \gamma_{5t}(EventYr_t \times YOB_i) + \sum \gamma_{6t}(EventYr_t \times Male_i) + \epsilon_{ijt}
\end{aligned}
$$

For all specifications, I cluster standard errors at the examiner level. The graphs below plot the β_t coefficients from these equations.

Figure D.2 presents first stage estimates for the CDR examiner instrument. Being assigned to a hypothetical examiner with a 100% allowance rate increases the probability of remaining on SSI by 20 percentage points relative to being assigned to an examiner with a 0% allowance rate. Consequently, children who are assigned to the most lenient examiners receive \$1300/year more in SSI payment relative to children assigned to the strictest examiners. Scaled up by the removal probability, this amounts to \$6500/year gained by the

Figure D.2: First Stage Graphs for CDR Examiner



treatment group, nearly the maximum yearly SSI payment. Although the confidence intervals are large, I can reject a zero first stage effect. Once I control for permanent indicator and body code, the pre-trends in the first stage become minimal.

Reduced Form Estimates

The main estimating equation for the reduced form outcomes is identical to that for the first stage, except for the addition of person fixed effects (α_i):

$$
\begin{aligned}
Y_{ijt} &= \alpha_i + \sum \beta_t(EventYr_t \times Allow_{ij}) \\
&+ \sum \gamma_{1t}(EventYr_t \times DDS_j) + \sum \gamma_{2t}(EventYr_t \times Permanent_i) \\
&+ \sum \gamma_{3t}(EventYr_t \times YOB_i) + \sum \gamma_{4t}(EventYr_t \times Male_i) + \epsilon_{it}
\end{aligned}
$$

In the subsections below, I present reduced form graphs for parental earnings, unearned income, and total household income. The results are summarized in Table D.2.

Parental Earnings and Household Income

Figure D.3 presents reduced form estimates for household earnings, total household income, family disability applications, and family disability receipt. There are no pre-trends in either parental earnings or household income. Two years after the CDR decision, household earnings decline by approximately $1000 after the year of the CDR for those assigned to the most lenient examiner, but because of large standard errors this decline is not statistically different from zero. Total household income increases immediately after the CDR for those assigned to the most lenient examiner, but then it drops as earnings increase.

Given a first stage change in SSI payment is about $1300, the increase of $1000 in parental earnings is consistent with the magnitude of the parental earnings response found using the main identification strategy. The major difference is that the main identification strategy yields an immediate response in household earnings, whereas the effect for the examiner instrument is delayed by two years. I attribute this difference to greater parental knowledge in the CDR assignment context. In the case of CDR *assignment*, the parent knows in advance of the CDR that the child has been selected for a CDR and that this increases the child's chance of being taken off of the program. In the case of CDR *examiners*, the parent has no information about the strictness of the examiner assigned to the child's case and therefore has no advance knowledge of the child's probability of removal. Thus it is only after the child is removed from the program that parents appear to respond to examiner leniency.

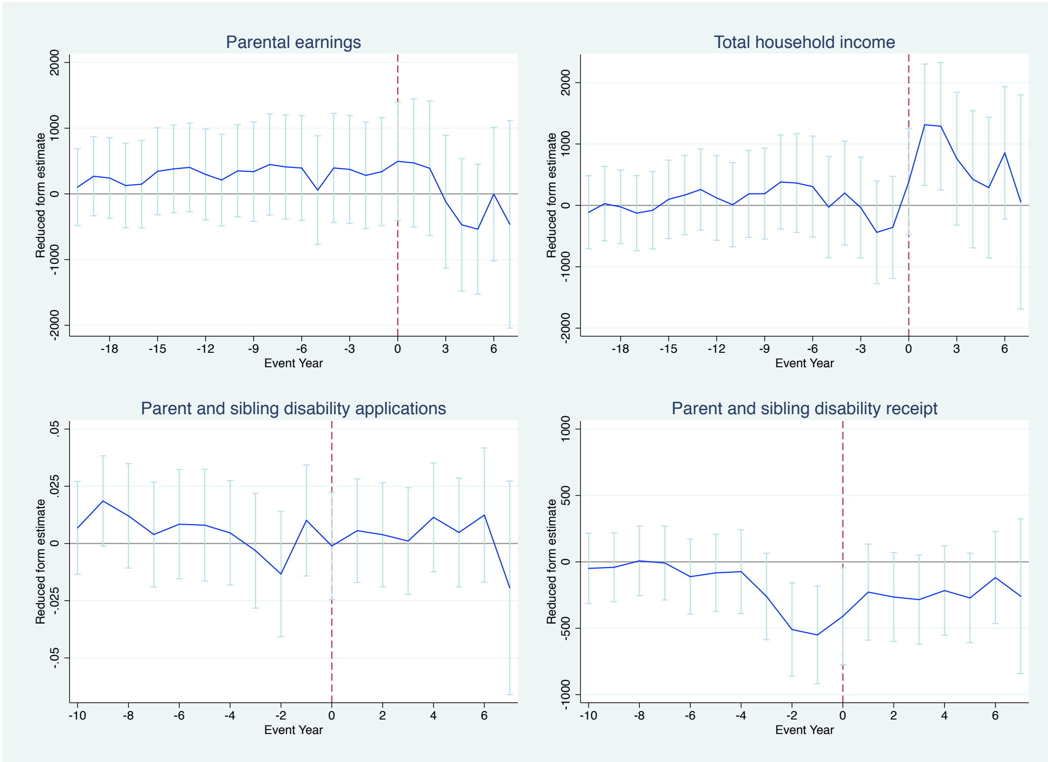
In Figure D.4, I attempt to get a more precise estimate of the household earnings and income responses by controlling for pre-treatment outcomes. Similar to the first stage, I estimate three equations *without* person fixed effects. The first specification includes no covariates other than pre-treatment earnings and DDS fixed effects:

$$\begin{aligned} Y_{ijt} &= \alpha + \sum \beta_t (EventYr_t \times Allow_{ij}) \\ &+ \psi_t + \gamma_{11} YMinus1_i + \gamma_{12} YMinus2_i + \gamma_{13} YMinus3_i + ... + \gamma_2 DDS_j \\ &+ \epsilon_{ijt} \end{aligned}$$

Here $YMinus1_i$ is the household's earnings (or income) in event year -1, $YMinus2_i$ is the household's earnings in event year -2, and so on.

The second specification includes a permanent indicator, YOB, and male covariates, not interacted with

Figure D.3: Earnings and Income Reduced Form Graphs for CDR Examiner



event year:

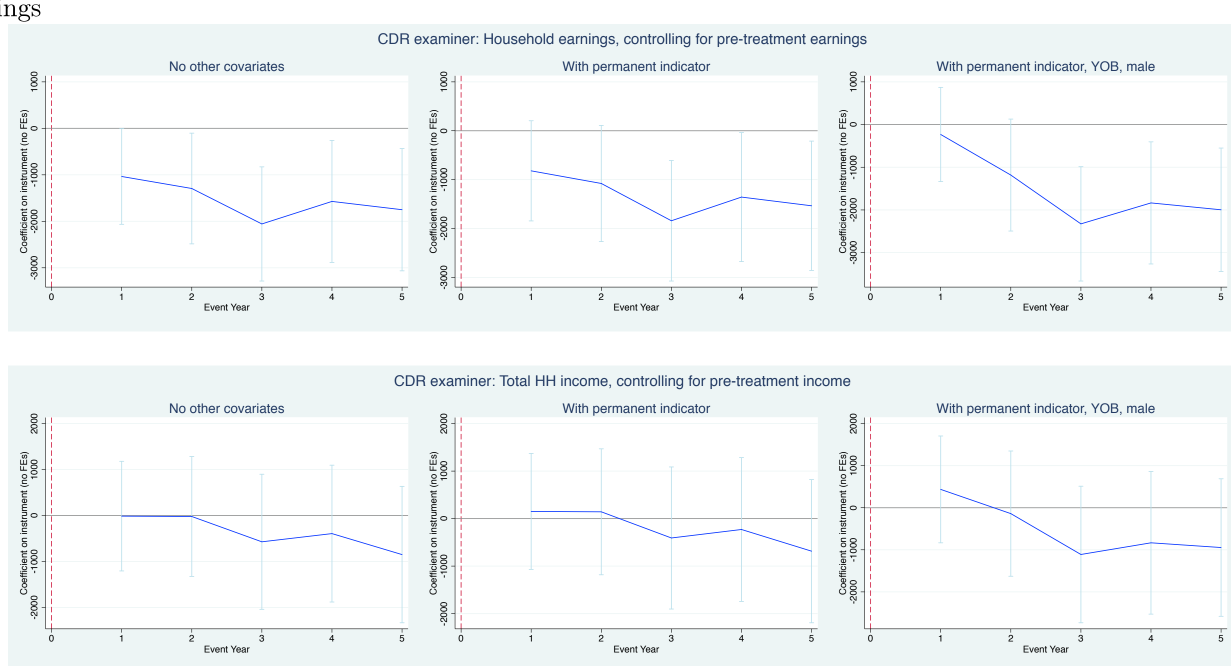
$$
\begin{aligned}
Y_{ijt} &= \alpha + \sum \beta_t (EventYr_t \times Allow_{ij}) \\
&+ \psi_t + \gamma_{11} YMinus1_i + \gamma_{12} YMinus2_i + \gamma_{13} YMinus3_i + ... + \gamma_2 DDS_j \\
&+ \gamma_3 Permanent_i + \gamma_4 YOB_i + \gamma_5 Male_i + \epsilon_{ijt}
\end{aligned}
$$

The third specification interacts the covariates with event year:

$$
\begin{aligned}
Y_{ijt} &= \alpha + \sum \beta_t (EventYr_t \times Allow_{ij}) \\
&+ \psi_t + \gamma_{11} YMinus1_i + \gamma_{12} YMinus2_i + \gamma_{13} YMinus3_i + ... + \gamma_2 DDS_j \\
&+ \sum \gamma_{3t} (EventYr_t \times Permanent_i) + \sum \gamma_{4t} (EventYr_t \times YOB_i) + \gamma_5 (EventYr_t \times Male_i) + \epsilon_{ijt}
\end{aligned}
$$

Figure D.4 presents household earnings and income estimates using the three specifications above. These estimates suggest a very large earnings response of between \$1000 and \$2000, though the confidence intervals

Figure D.4: Household Earnings Response for CDR Examiner, controlling for pre-treatment household earnings



are wide. As a result of this huge earnings response, there is very little change in total household income. Once again, these results are consistent with the results from the main identification strategy.

Family disability applications and receipt

Figure D.3 shows no discernible effects on family disability applications or family disability receipt. Figure D.5 presents separate reduced form estimates for parental DI application and receipt, parental SSI application and receipt, and sibling SSI application and receipt. The effects are imprecise as a result of large standard errors, and in general there is no detectable effect of CDR examiner assignment on household disability applications. Assignment to a lenient examiner appears to increase parental and sibling SSI applications, which is consistent with the CDR instrument results, but I cannot reject a zero effect.

Figure D.5: Family Disability Application and Receipt Graphs for CDR Examiner

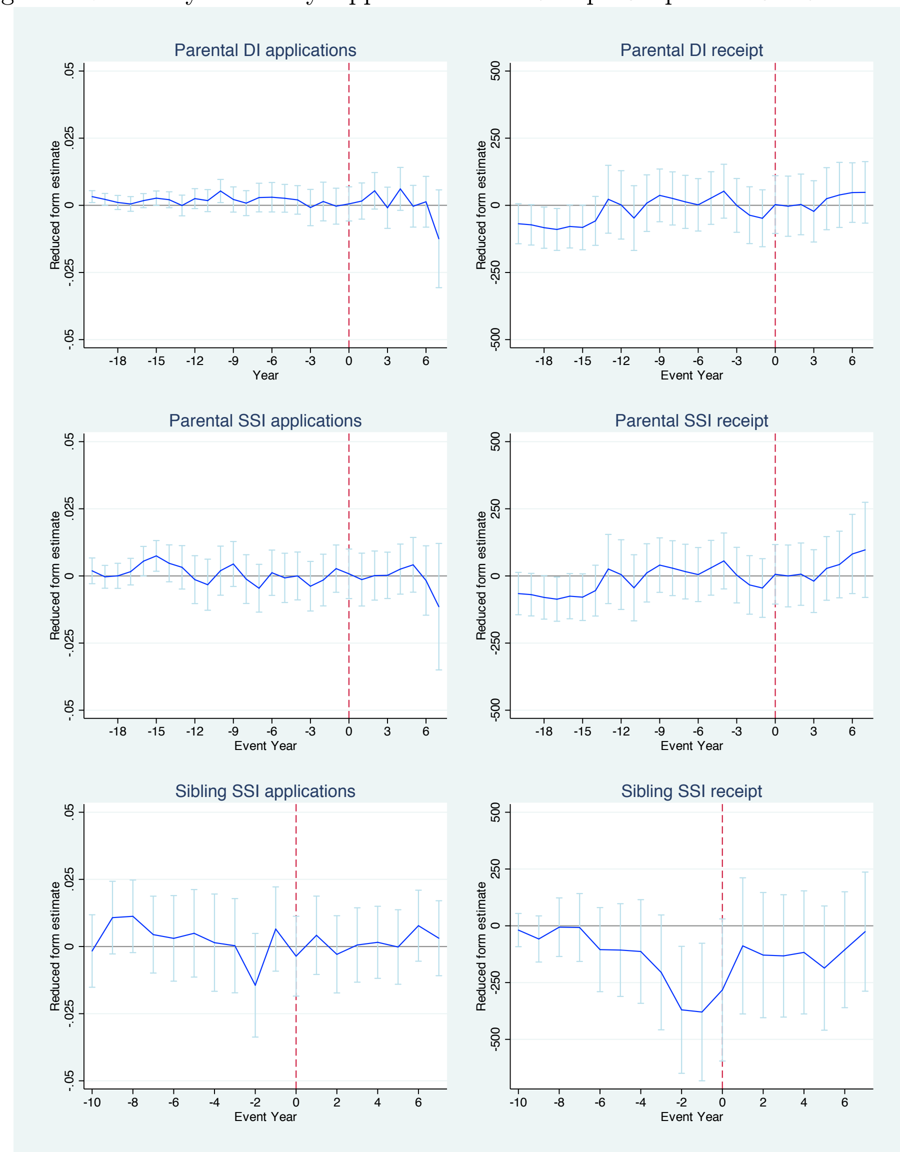


Table D.2: First Stage and Reduced Form Estimates for CDR Examiner

	Covariates, no FEs		**Fixed effects**		**Pre-treat control**	
	Point Est.	Std. Error	Point Est.	Std. Error	Point Est.	Std. Error
First Stage Estimates						
On SSI	0.1300***	(0.0407)				
Months on SSI	1.52***	(0.4889)				
Total SSI payment	917***	(305)				
Earnings and income						
Parental earnings			-359	(394)	-647	(451)
Unearned income			1083**	(477)		
Total household income			723	(426)	661	(487)
Family disability applications						
Parental DI applications			7.82E-06	(0.0019)		
Parental SSI applications			-0.0005	(0.0025)		
Sibling SSI applications			0.0000678	(0.0030)		
Total applications			0.0009	(0.0051)		
Family disability payments						
Parental DI payment			-60.9	(55.7)		
Parental SSI payment			49.7	(45.4)		
Sibling SSI payment			21.0	(93.5)		
Total payments			-92.8	(138)		
N	112,075		112,075		112,075	
Clusters	5,308		5,308		5,308	