Does Welfare Inhibit Success? The Long-Term Effects of Removing Low-Income Youth from Disability Insurance

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Abstract

I estimate the long-term effects of removing low-income youth with disabilities from Supplemental Security Income (SSI) on the level and variance of their earnings and income in adulthood. Using administrative data from the Social Security Administration, I implement a regression discontinuity design based on a change in the probability of SSI removal at age 18 created by the welfare reform law of 1996. I find that SSI youth who are removed earn on average \$4,000 per year, an increase of just \$2,600 relative to those who remain on SSI. This increase in earnings covers only one-third of the \$7,700 they lose in annual SSI income, and they lose an additional 10% each year in other transfer income. As a result, removed SSI youth experience a present discounted income loss of \$73,000, or 80% of the original SSI income loss, over the 16 years following removal. In addition to the large drop in income levels, the within-person variance of income quadruples as a result of the SSI loss. Based on back-of-the-envelope calculations assuming risk aversion and limited intertemporal consumption smoothing, I find that up to one-quarter of the recipient's welfare loss from SSI removal is attributable to the increase in income volatility rather than to the fall in income levels. This result suggests that ignoring the income stabilization effects of welfare and disability programs could substantially underestimate their value to recipients.

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1 Introduction

The perennial debate surrounding welfare programs reflects the tradeoff between consumption smoothing and moral hazard in social insurance. Supporters defend welfare programs as a vital lifeline for those who face barriers to work, while critics charge that these programs create perverse incentives to qualify and perpetuate dependency. In his influential work *Losing Ground*, Murray (1984) argued that the short-term benefits provided by welfare programs lead to long-term losses for recipients by eroding human capital and sapping work ethic. Arguments like Murray's formed the intellectual basis for the 1996 U.S. welfare reform law that placed limits on welfare benefits and instituted work requirements for recipients.

Encapsulating this debate is the controversy over the U.S. Supplemental Security Income (SSI) program, a rapidly expanding disability insurance program that provides cash payments and Medicaid eligibility to low-income children and adults with disabilities. SSI is now the largest cash welfare program in the United States, paying about $50 billion each year to 8 million recipients, including over one million children, or 10% of children living in poverty.[1] The SSI children's program has been singled out by policymakers and the media for potential perverse incentives. Critics argue 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.[2] Supporters argue that SSI payments help families care for children with disabilities and may improve the outcomes of children.[3]

In this paper, I address two long-standing questions about SSI that reflect the broader debate over means-tested programs. First, how much does SSI inhibit labor market success and self-sufficiency among youth? There has been little work on the long-term effects of disability programs on children and youth, even though their formative stage of development might make them most vulnerable to any perverse incentives or discouragement of achievement.[4] Second, how much insurance does SSI provide to recipients? Empirical work on social insurance programs has thus far considered their effects on the *level* of earnings and income, but their effects on the *stability* of income are also relevant if recipients are risk averse. In the presence of risk aversion, a monthly stream of welfare payments is more valuable than earnings of the same average monthly amount if welfare payments are more stable. Earnings volatility is especially relevant for low-income

[1] U.S. Congressional Budget Office, "Growth in Means-Tested Programs and Tax Credits for Low-Income Households," February 2013.

[2] See, e.g., Patricia Wen, "The Other Welfare," *Boston Globe*, December 12, 2010; U.S. Congress, House Subcommittee on Human Resources of the Committee on Ways and Means, Hearing on Supplemental Security Income Benefits for Children, October 27, 2011; and Nicholas Kristof, "Profiting from a Child's Illiteracy," *New York Times*, December 7, 2012.

[3] See, e.g., Kathy Ruffing and Ladonna Pavetti, "SSI Children with Disabilities: Just the Facts," Center on Budget and Policy Priorities, December 14, 2012; and Vallas and Alfano (2012).

[4] Several recent studies have examined the effects of adult disability programs on labor supply and human capital, including Bound (1989), Chen and van der Klaauw (2008), von Wachter, Song and Manchester (2011), French and Song (2014), Maestas, Mullen and Strand (2013), Moore (2013), Autor, Maestas, Mullen and Strand (2013), and Hemmeter and Stegman (2013). Kubik (1999) and Coe and Rutledge (2013) study children with disabilities.

populations whose employment opportunities are limited to jobs with high turnover and unpredictable hours.[5]

To answer these questions, I study the long-term effects of removing low-income youth with disabilities from Supplemental Security Income on the level and variance of their earnings and income in adulthood. I take advantage of a policy change in the Personal Responsibility and Work Opportunity Act (PRWORA) of 1996—more commonly known as welfare reform—that increased the number and strictness of medical reviews for 18-year-olds. The law applied only to children with an 18th birthday after August 22, 1996—the date of PRWORA enactment—creating a discontinuity in the likelihood of removal via age 18 medical review at that date. I implement a regression discontinuity design based on this change using administrative data from the Social Security Administration (SSA). To the best of my knowledge, this is the first paper to estimate the causal impacts of program removal on the large and critical population of youth with disabilities and to follow them over multiple decades. It is also the first to consider the effect of disability insurance on income stability in addition to income levels.

I find that SSI youth who are removed from the program increase their earnings minimally and, as a result, experience a large drop in income levels. Removed SSI youth earn on average \$4,000 per year in adulthood, an increase of just \$2,600 relative to those who remain on SSI. This increase in earnings covers only one-third of the \$7,700 they lose in annual SSI income, and they lose an additional 10% each year in Social Security Disability Insurance (DI) income because they are less likely to apply for DI. As a result, removed SSI youth lose on average \$73,000 in present discounted observed income over the following 16 years, which is 80% of the original SSI cash income loss. Even those in the top decile of the earnings response barely recover the full amount of the lost SSI income.

Despite the large average losses, removal does spur some SSI youth to earn at full-time, full-year levels. The likelihood of maintaining annual earnings above \$15,000—approximately the full-time minimum wage annual earnings level—increases by an average of 11 percentage points over the post-period, off of a near-zero baseline for those who remain on SSI. This effect also increases over time, which suggests that SSI removal may have long-term effects on earnings behavior, perhaps through skill accumulation or greater taste for work. However, using survey and administrative data on the broader disadvantaged youth population, I find that removed SSI youth have substantially lower earnings levels and lower earnings growth than their disadvantaged but non-disabled counterparts.

In addition to the fall in income levels, income volatility increases considerably as a result of SSI removal. The within-person coefficient of variation of income quadruples, putting income variance for removed SSI youth at the 95th percentile of the control group distribution. If recipients are risk averse and unable to

[5]For example, Edin and Lein (1997) write that poor single mothers in their study "had to weigh the utility of work against the real possibility that a subsequent layoff or reduction in hours could lead to serious material hardship. The jobs these mothers could get were among the least reliable in the U.S. economy."

smooth consumption intertemporally, then both the fall in income levels and the rise in income volatility from SSI removal have welfare consequences. Under various assumptions about the relationship between income and consumption for this very poor population, I do back-of-the-envelope calculations of the welfare loss experienced by SSI youth from removal. I find that up to one-quarter of the recipient's welfare loss from SSI removal is attributable to the increase in income volatility rather than to the fall in income levels.

The SSI context is a useful setting for studying the effects of welfare programs for several reasons. First, SSI is the largest cash welfare program in the United States, with annual expenditures more than double those of the Temporary Assistance to Needy Families (TANF) program.[6] Second, the effects of SSI may be particularly consequential for SSI youth because they are at risk for poor life outcomes. SSI children grow up in households with incomes near or below the poverty line, generally with fewer than two parents. Mental conditions other than intellectual disability—including ADHD, speech delay, and autism spectrum disorder—have accounted for nearly all of the expansion in the SSI children's program in the past two decades and now constitute the primary diagnosis for the majority of SSI children. SSI youth with mental conditions other than intellectual disability have school drop-out rates of 45%, school suspension rates of 52%, and arrest rates of 28% (Hemmeter, Kauff and Wittenburg (2009)). Their outcomes do not improve substantially in adulthood: former SSI children have employment rates of just 20-50% as adults, depending on the cohort (Davies, Rupp and Wittenburg (2009)).[7] Third, SSI is a relevant context for studying the income stabilization effects of welfare and disability programs because the SSI population is a low-income, low-education population whose employment opportunities are restricted to jobs with high turnover and unpredictable hours. The bottom quintile of the earnings distribution in the United States has a within-person earnings variance (normalized by the mean) more than double that of the middle quintile.[8] Therefore the primary alternative source of income for the SSI population is highly volatile earnings.

The findings in this paper inform long-standing issues in the debate over welfare programs. With respect to whether SSI inhibits labor market success and self-sufficiency, I find that most SSI youth would earn well below subsistence levels if removed from SSI. I find no evidence for the hypothesis that SSI holds recipients back from self-sufficiency or that removing even relatively healthy SSI recipients would make them better off in the long run. Instead, removing SSI youth leads to a large reduction in lifetime income and a large increase in the volatility of that income. With respect to the level of insurance provided by SSI, I find that SSI affords a greater amount of insurance than suggested by previous analyses because it has substantial

[6] Congressional Budget Office, "Growth in Means-Tested Programs and Tax Credits for Low-Income Households," February 2013.

[7] Cognizant of these poor life outcomes, the Social Security Administration recently launched two programs intended to address the self-sufficiency of SSI children in adulthood: the Youth Transition Demonstration and the Promoting the Readiness of Minors in Supplemental Security Income Program.

[8] Author's calculations from the Continuous Work History Sample.

income stabilization benefits. Ignoring the income stabilization benefits of disability programs, and possibly other social insurance programs, could substantially underestimate their value to recipients.

The paper proceeds as follows. Section 2 describes the administrative data from the Social Security Administration used in this paper, provides background on the SSI program, and outlines the empirical strategy based on changes in the welfare reform law. Section 3 presents estimates of the first stage for SSI enrollment. Section 4 presents and discusses IV estimates for earnings and income levels, and Section 5 presents and discusses IV estimates for income volatility. In Section 6, I consider the effect of SSI removal on the welfare of the individual recipient and on social welfare. Section 7 concludes.

2 Data, background, and empirical strategy

2.1 Data

I use confidential administrative data from the Social Security Administration. The Supplemental Security Record (SSR) provides demographic information on SSI children, including date of birth, sex, county code, primary and secondary diagnosis, application date(s), and decision date(s). My extract also includes monthly benefit history information starting from the inception of the program in 1974 up to 2013. For each month, the SSR gives the child's payment status code—either in-pay or the reason for nonpayment—as well as the actual benefit payment in that month and the correct benefit amount in that month, which is calculated after the fact when new information (e.g., income changes) comes to light that affects the recipient's benefit amount. The SSR also identifies the parents of SSI children since SSA uses parental income and assets to determine the child's benefit amount.

I link SSR records to the Continuing Disability Review Waterfall File. This file gives information on all medical reviews for children and adults, including age 18 medical reviews going back to 1997. Each medical review observation lists demographic information for the reviewed recipient, the type of medical review, and the date and outcome of the case at each level of adjudication.

I link children to their long-term outcomes using several Social Security Administration databases. From an extract of the Master Earnings File (MEF), I observe earnings for each individual in each year up to 2012. These earnings include wage, salary, and tip income reported on W-2 and W-3 forms and self-employment income reported on 1040 Schedule SE forms. The 831 Records provide information on SSI and SSDI applications up to 2013, and the Master Beneficiary Record (MBR) gives benefit history information for the SSDI program up to 2012. The Numident file documents date of death. Since the SSR identifies the parents of SSI children, I use the same data sources (MEF, 831, MBR, SSR, and Numident) to link children to their parents' earnings, SSI and SSDI applications, SSI and SSDI benefit receipt, and mortality. I also observe SSI application and receipt for siblings of SSI children by linking SSR child records with the same parent.

Although SSI in most states provides categorical Medicaid eligibility, SSA administrative data do not include information on Medicaid enrollment or utilization. Instead, I do a back-of-the-envelope calculation for Medicaid enrollment following the age 18 medical review using estimates from Hemmeter (2011). I then use estimates of the value of Medicaid from Finkelstein, Hendren and Luttmer (2014) and the 1997 Current Population Survey and inflate or deflate these numbers over time using the Bureau of Economic Analysis (BEA) Health Deflator.

Finally, I use a number of other data sources to put my findings on SSI youth in context. Using the 2008 Panel of the Survey of Income and Program Participation, I compare the characteristics of households with SSI children to the characteristics of all households with children and disadvantaged households with children. I use the Continuous Work History Sample (CWHS) and the National Longitudinal Survey of Youth-1997 (NLSY97) to track the evolution of earnings in broader disadvantaged populations of the same age as the SSI youth in my sample. The CWHS provides tax data on annual earnings for a 1% sample of the U.S. population, but it has very limited demographic information. I use the CWHS to calculate the variance of earnings for different parts of the earnings distribution. The NLSY97 provides self-reported annual earnings from 1997 to 2011, but it has richer demographic information and therefore allows me to construct a sample of similar socioeconomic status to my SSI sample. I use the Bureau of Labor Statistics Consumer Expenditure Survey to determine a reasonable consumption floor for the low-income population. In addition, I tabulate descriptive statistics on the SSI youth population from the National Survey of SSI Children and Families (NSCF), including living arrangements, family transfers, public assistance, and educational achievement. Finally, I use county unemployment rate in 1997 and county poverty rate in 1999 from the Bureau of Economic Analysis Regional Economic Accounts and the U.S. Census Bureau, respectively.

2.2 Background on the SSI program and SSI children

SSI provides monthly cash payments and Medicaid eligibility to children and adults who qualify on the basis of disability or old age and have limited income and assets. The maximum federal benefit amount for an individual was \$721/month (\$8,652/year) in 2014, and most states provide a small supplement to this amount. SSI also provides categorical eligibility for Medicaid in most states.

Table 1 compares the characteristics of households with SSI children to the characteristics of all households with children in the United States, based on calculations from the Survey of Income and Program Participation 2008 Panel. Households with SSI children are severely disadvantaged by nearly every measure. Annual earnings are less than one-third of earnings for all households with children, and total household income is approximately one-half. SSI-child households are twice as likely to be black, and the head of household is twice as likely to be a single mother and a high school dropout. To see how households with SSI children compare to disadvantaged households more broadly, in the third column I present estimates for households with children whose head has a high school education or less. Even relative to low-education households, SSI-child households have 50% lower earnings and 20% lower total income and are two-thirds more likely to be headed by a single mother. Nearly 80% of SSI-child households receive free or reduced-price lunch, compared to 54% for low-education households, and income from cash transfers is more than seven times larger in SSI-child households.

Under current law, SSI children must requalify for the program as adults by undergoing an age 18 medical review. About 40 percent of all SSI children and two-thirds of SSI children with mental conditions other than intellectual disability are currently removed from SSI at the age of 18 (Hemmeter and Gilby (2009)). These high cessation rates can be attributed to differences in medical eligibility criteria between the child and adult SSI programs. In the adult disability programs, disability is defined as an inability to work. Adults who earn above the "substantial gainful activity" limit ($1070/month for non-blind individuals in 2014) are ineligible for disability benefits. Since children do not work, their eligibility for the SSI program is based on age-appropriate activity. Children must have "marked and severe functional limitations" that limit their activities, which can include social interaction and school performance.[9] Conditions such as ADHD and other learning disabilities may qualify a child for SSI because they limit age-appropriate activity, but they are less likely to qualify an adult unless they are severe enough to prevent work. Children who qualify on the basis of these conditions are likely to be removed at 18 unless they have another disability that meets the adult criteria (Hemmeter (2012)).

In the SSI children's program, the income and assets of the parents are used to determine both financial eligibility and monthly benefit amount. Once a child turns 18, however, only the child's own income and assets are considered. The maximum monthly SSI benefit amount ($721 in 2014) is reduced based on income. SSI work incentives treat earned income more generously than unearned income. The monthly benefit is reduced by $1 for every $1 of unearned income (after a $20 exclusion), but by only $1 for every $2 of earned income (after a $65 exclusion). This puts the annual break-even point for earned income at about $18,000. SSI recipients are generally terminated after 12 consecutive months of having a zero benefit amount. Certain public assistance benefits like SNAP (food stamps) are not considered income for SSI purposes. The SSI asset limit is $2,000 for an individual and $3,000 for a couple and excludes the value of a home and one vehicle.

[9] Social Security Act Sec. 1614, 42 U.S.C. 1382c(a)(3).

2.3 Welfare reform policy change

SSI was established by Congress in 1972 and experienced rapid expansion after the relaxation of adult disability criteria in 1984. In 1990, the Supreme Court decision in *Sullivan v. Zebley* also relaxed medical eligibility criteria for children. As shown in Figure 1, child enrollment in SSI surged after the decision, with nearly all of the growth coming from mental conditions other than intellectual disability, including ADHD, speech delay, and autism spectrum disorders. This rapid growth drew the attention of the media and the general public, with stories of parents "coaching" their children to appear disabled featuring prominently in the press.[10] In response, Congress enacted changes to the SSI children's program to limit entry onto the program and to remove children believed to have been allowed improperly.

I take advantage of changes to the SSI program enacted as part of welfare reform to identify the effect of SSI on the long-term outcomes of transition-age youth. The Personal Responsibility and Work Opportunity Act (PRWORA) of 1996 made two changes to age 18 medical reviews, which are medical reviews used to determine whether SSI children qualify for the program as adults. First, it required the Social Security Administration to redetermine the eligibility of all SSI children at the age of 18, up from virtually zero age 18 medical reviews previously. Second, it increased age 18 medical review eligibility requirements to use the stricter adult standard rather than the child standard. Prior to the legislation, SSI children were continued on SSI as adults whether or not they met the adult disability standard, as long as they did not demonstrate medical improvement in their childhood condition. PRWORA instead required all SSI children to re-qualify for the program as adults. Importantly for my empirical strategy, these changes in the number and strictness of age 18 medical reviews applied only to children with an 18th birthday after August 22, 1996, which was the date of PRWORA enactment.

Figure 2 summarizes the empirical strategy for this regression discontinuity (RD) design in date of birth. The x-axis shows the date of the child's 18th birthday, with the vertical line denoting the August 22, 1996, cutoff. The graph plots the proportion of children in each birthweek bin who receive an age 18 medical review, receive an unfavorable age 18 medical review, and ever (up to age 35) receive an unfavorable continuing disability review. The figure confirms that the PRWORA changes were enforced: while almost no children with an 18th birthday immediately before the cutoff (hereafter, "control" group) received an age 18 medical review, nearly 90 percent of children with an 18th birthday immediately after the cutoff ("treatment" group) received one.[11] This discontinuity in the likelihood of receiving an age 18 medical review translates into a 39 percentage point discontinuity in the likelihood of receiving an *unfavorable* age 18 medical review. Age 18

[10] See, e.g., Bob Woodward and Benjamin Weiser, "Costs Soar for Children's Disability Program—How 26 Words Cost the Taxpayers Billions in New Entitlement Payments," *Washington Post*, February 4, 1994, p. A1; Jonathan Yardley, "America's Faultless Performance," *Washington Post*, February 7, 1994, p. C2; ABC *PrimeTime Live*, October 13, 1994.

[11] Based on my data, most of the 10% of children with an 18th birthday after the cutoff who did not receive an age 18 medical review had already been flagged for other violations.

medical reviews are a specific type of the more general continuing disability review used to verify continued medical eligibility for both adults and children. As shown in Figure 2, children with an 18th birthday after the date of PRWORA enactment are 28 percentage points more likely to ever receive an unfavorable medical review until the last time I observe them at age 35. This discontinuity is smaller than the previous ones since children on the left hand side of the graph, who do not receive an age 18 medical review, are more likely to continue on SSI as adults and receive adult medical reviews.

2.4 Covariate balance and econometric specification

I use the discontinuity in birthdate to identify the effect of SSI removal on the long-term outcomes of SSI children. The key identifying assumptions of the regression discontinuity design are that assignment to the treatment is as good as random immediately around the cutoff and the outcome variable is counterfactually smooth across the cutoff. To test these assumptions, I use a parametric RD specification to examine whether the instrument predicts observable covariates for children around the cutoff:

$$\mathrm{Y}_i = \alpha + \beta \mathrm{Post}_i + \gamma \mathrm{DOB}_i^n + \kappa(\mathrm{Post}_i \times \mathrm{DOB}_i^n) + \epsilon_i \tag{1}$$

where Y_i is a covariate for child i; Post_i is a dummy for having an 18th birthday after the August 22, 1996, cutoff; and DOB_i^n is the date of birth running variable of polynomial order n.

Using a linear specification, I find that covariates are imbalanced at the August 22, 1996, cutoff under the standard regression discontinuity design. As shown in Table 2, SSI children with an 18th birthday immediately after the cutoff enter the program earlier and have lower pre-treatment earnings, lower pre-treatment parental earnings, and higher pre-treatment SSI payment. The same imbalances appear at the August 22 cutoff in the two neighboring years, 1995 and 1997 (see Online Appendix Table B.1), suggesting that the imbalances are the result of seasonality (covariate balance graphs for all three years are given in Online Appendix Figures B.1-B.3). Using a quadratic specification, in contrast, the discontinuities shrink in all years and the F-test fails to reject the null hypothesis of covariate balance (see Online Appendix Table B.2). Non-parametric local linear regression estimates show discontinuities in the same variables, though the F-test fails to reject for smaller bandwidths because of large standard errors (see Online Appendix Table B.3).

To account for potential seasonality in both observables and unobservables, I augment the standard regression discontinuity design by differencing out the discontinuity in neighboring years (1994, 1995, and 1997) from the discontinuity in 1996.[12] This regression discontinuity/difference-in-differences (RD-DD) design

[12] I exclude 1998 as a comparison year because it contains another (much smaller) discontinuity in age 18 medical reviews by

compares the discontinuity for the 1996 cohort to the discontinuity for comparison cohorts in 1994, 1995, and 1997 that were not affected by the PRWORA change, thereby netting out any effects of seasonality from the estimates. The RD-DD specification is identical to the standard RD in equation (1) except that it interacts a dummy for the 1996 cohort with each variable:

$$
\begin{aligned}
\mathrm{Y}_i &= \alpha_0 + \beta_0(\mathrm{Coh96}_i \times \mathrm{Post}_i) + \gamma_0(\mathrm{Coh96}_i \times \mathrm{DOB}_i^n) + \kappa_0(\mathrm{Coh96}_i \times \mathrm{Post}_i \times \mathrm{DOB}_i^n) \\
&+ \alpha_1 \mathrm{Coh96}_i + \beta_1 \mathrm{Post}_i + \gamma_1 \mathrm{DOB}_i^n + \kappa_1(\mathrm{Post}_i \times \mathrm{DOB}_i^n) + \epsilon_i. \qquad (2)
\end{aligned}
$$

Table 2 shows the covariate balance tests for the RD-DD design under both a linear and quadratic specification. Compared to the linear RD specification, the linear RD-DD specification yields somewhat smaller discontinuities, but some discontinuities remain and the p-value on the omnibus F test remains marginally significant. The quadratic RD-DD, however, completely eliminates the discontinuities in the covariates. The quadratic RD-DD also performs best in the falsification tests, shown in Table 3, that test for covariate balance at each day of the year. The motivation for these tests is that if covariates are balanced across the year then the rejection rate of the joint F test should approximately equal the significance level of the test. As shown in the table, the linear and quadratic standard RD specifications massively over-reject in 1996 and in the neighboring years. The linear RD-DD also over-rejects, but not as badly as the individual years, and the quadratic RD-DD specification has "correct" rejection rates—approximately equal to the significance level. For these reasons, I choose the quadratic RD-DD as my preferred specification, with standard errors clustered at the individual level, and I report the main results using this specification.[13] However, I find that the estimates are robust to different polynomial orders and to non-parametric estimation.

The sample is SSI children with an 18th birthday around the August 22 cutoff in 1996 and the comparison years (1994, 1995, and 1997). With a 37-week (260 day) bandwidth, the sample includes 81,800 children from the 1996 cohort and 219,089 children from the comparison cohorts. The first column of Table 4 gives summary statistics for the 1996 cohort. The sample is majority male, which is consistent with the known higher disability diagnosis rate among boys, and the average age at entry is 11.4 years. Mental conditions are by far the most common diagnosis, with 49% of the sample having an intellectual disability and 25% having a mental condition other than intellectual disability. As expected for a means-tested program, sample members come from very low-income households, generally with fewer than two parents. Average annual parental

birthdate as a result of a different policy change.

[13]Another potential way to deal with seasonality is to take a parametric approach by including controls for month or quarter of year. However, this approach would still require using multiple years in the sample, and it would require me to know the exact form of the seasonality, such as month of year or quarter of year. The nonparametric approach taken in equation (2) does not require me to specify the exact functional form that seasonality takes, and it consumes fewer degrees of freedom.

earnings between 1980 and 1996 for those with parents are \$9,592 and the median is \$4,121. Households receive a large amount of disability income relative to earnings, even excluding the youth's SSI payment. On average the households receive \$2,728 in disability income (either SSI or SSDI) for parents or siblings of the youth during this time period. Approximately half of sample members come from a household with a single mother. Another 16% have no parents on their record; these include children in foster care, children living with relatives other than parents, and children living in institutions.

3 First stage estimates for SSI enrollment

The causal relationship of interest is the effect of SSI removal on long-term outcomes:

$$\mathrm{Y}_{it} = \alpha + \beta \mathrm{OffSSI}_{it} + \epsilon_{it} \tag{3}$$

where Y_{it} is the outcome of interest for individual i in year t and OffSSI_{it} is an indicator for being off of SSI in year t. Based on the empirical strategy outlined in the previous section, I use having an 18th birthday after the August 22, 1996, cutoff as an instrument for being off of SSI. The first stage equation is equation (2) with covariates, where the left-hand-side variable is an indicator for being off of SSI in year t:

$$\begin{aligned} \mathrm{OffSSI}_{it} &= \alpha_0 + \beta_0(\mathrm{Coh96}_i \times \mathrm{Post}_i) + \gamma_0(\mathrm{Coh96}_i \times \mathrm{DOB}_i^n) + \kappa_0(\mathrm{Coh96}_i \times \mathrm{Post}_i \times \mathrm{DOB}_i^n) \\ &+ \alpha_1 \mathrm{Coh96}_i + \beta_1 \mathrm{Post}_i + \gamma_1 \mathrm{DOB}_i^n + \kappa_1(\mathrm{Post}_i \times \mathrm{DOB}_i^n) + \mathrm{X}_i + \epsilon_i \end{aligned} \tag{4}$$

The covariates in X_i include sex, diagnosis category, age at entry, pre-treatment parental earnings, and state.

I use removal rather than lost SSI income as the treatment variable for two reasons. First, SSI includes both cash payments and Medicaid eligibility in most states. The birthdate instrument is valid only for the combination of these benefits, not for the cash benefits alone. Second, if there is a nonlinear relationship between amount of SSI income lost and change in long-term outcomes, then the effect of removing a recipient from SSI cannot be scaled linearly to get the effect of reducing the recipient's SSI income by one dollar.

3.1 First stage results

Figure 3 presents the effect of having an 18th birthday after the cutoff on the probability of being enrolled in SSI four years after the year of the 18th birthday. SSI children with an 18th birthday immediately before the August 22, 1996, cutoff (the control group) are on average 24 percentage points more likely to be on SSI four years out than children with an 18th birthday immediately after the cutoff (the treatment group).

This figure also demonstrates the importance of using the RD-DD strategy. The hollow gray circles represent the comparison cohorts of 1994, 1995, and 1997; there is a small but statistically significant increase in the probability of being enrolled in SSI at the August 22nd cutoff in these years. The RD-DD strategy differences out this comparison discontinuity in producing the first stage estimate for the 1996 cohort. Since the discontinuity for the 1996 cohort is in the opposite direction of that for the comparison cohorts, the standard RD estimate would slightly underestimate the discontinuity for the 1996 cohort. Table 5 gives the first stage estimates for the baseline quadratic RD-DD specification that passes covariate balance tests. These estimates are robust to the inclusion of covariates, to polynomial order, and to standard RD estimation (see Appendix Table A.1 and Online Appendix Table B.6).

The magnitude of the first stage may change over time as treatment group members get back on the program and control group members leave the program. Indeed, Figure 4 shows that there is a large amount of heterogeneity in the first stage over time. The x-axis is the year relative to the year of the child's 18th birthday, when age 18 medical reviews are supposed to occur. The graph plots the quadratic RD-DD estimate for SSI enrollment estimated separately for each year using equation (4). As expected from quasi-random assignment, there is no difference between the control and treatment groups in the probability of SSI enrollment prior to age 18.[14] The difference between the control and treatment groups does not open measurably until two years after the year of the child's 18th birthday as a result of lags in decision time. These age 18 medical reviews were conducted after the welfare reform legislation passed and thus did not begin until after the child's 18th birthday. The median decision time from the child's 18th birthday is roughly three-quarters of a year overall, but it increases to 1.4 years for the 22% of cases that made it to the first appeals stage (reconsideration) and 2.4 years for the 10% of cases that made it to the second appeals stage (administrative law judge).

The first stage reaches a maximum four years after the child's 18th birthday, with the treatment group 24 percentage points less likely to be enrolled in SSI than the control group. After that, the first stage effect attenuates rapidly, declining to 10 percentage points eight years out and eventually plateauing at 5 percentage points. There are two possible explanations for this attenuation. The first is that the treatment group members who left SSI as a result of an age 18 medical review re-enter SSI as adults. The second is that the control group members who did not get an age 18 medical review leave SSI as adults for other reasons. I find that most of the attenuation is attributable to the latter explanation: control group members leaving the program in large numbers as adults, with 44% exiting by 2013 (see Appendix Figure A.1). The most common reasons for control group exit are removal via adult medical review (15% of all control group

[14] Although I construct the sample based on those who are enrolled in SSI around the age of 18, I do not place any restrictions on the year of entry onto SSI.

members), income and asset violations (13%), incarceration (7%), and death (5%). Some treatment group members return to the program after removal, but this proportion is much smaller, reaching 15% by 2013. Note that control group exit is also the reason that the first stage magnitude peaks at 24 percentage points rather than the 40 percentage points predicted by the unfavorable age 18 review estimates in the previous section.

3.2 Complier characteristics

The RD design identifies the effect of having an 18th birthday after the August 22nd cutoff for the complier population: the children who would receive an unfavorable age 18 review if they had a birthday after the cutoff, but would not receive an unfavorable age 18 review if they had a birthday before the cutoff. Examining the characteristics of the complier population is important for the external validity of the results. The "Review complier" column in Table 4 presents the complier characteristics analysis based on the procedure outlined in Angrist and Pischke (2008), where compliers are defined using unfavorable age 18 review as the treatment variable.[15] The table gives the proportion of the full sample and the review complier population with a given characteristic (for continuous characteristics, the proportion above the median). The review compliers are representative of the full sample on most characteristics affecting substantial proportions of the population. Mental conditions other than intellectual disability are overrepresented in the review complier population, which is expected since many mental conditions that qualify children for SSI—for example, ADHD and other learning disabilities—are less likely to qualify adults for SSI. Intellectual disability is slightly underrepresented in the review complier population.[16] The review complier population is representative of the full sample on pre-treatment outcomes, including parental and child earnings and family disability receipt, as well as demographic variables.

[15]The average of characteristic X over the complier population is the term $E(X|D_1 = 1, D_0 = 0)$ in the expression

$$E(X|D_1 = 1) = E(X|D_1 = 1, D_0 = 1)P(D_0 = 1|D_1 = 1) + E(X|D_1 = 1, D_0 = 0)P(D_0 = 0|D_1 = 1)$$

where $D_z = d$ indicates whether the individual was treated ($z = 1$) or not ($z = 0$), and whether the individual was moved by the instrument ($d = 1$) or not ($d = 0$). Intuitively, the average of a characteristic over treatment group members who receive an unfavorable age 18 review is the weighted average over always takers (those who receive an unfavorable age 18 review whether they are treated or not) and compliers (those who receive an unfavorable age 18 review if and only if they are treated), where the weights are the proportion of always takers and compliers in the population, respectively. The average over treatment group members who receive an unfavorable age 18 review is empirically known. The average over always takers is estimated from control group members who receive an unfavorable age 18 review. The proportion of compliers can be estimated by subtracting off the proportion of always takers (control members who receive an unfavorable age 18 review) and never takers (treatment group members who do not receive an unfavorable age 18 review). I estimate that 39% of the sample is in the complier population when defined in terms of an unfavorable age 18 medical review, and 22% of the sample is in the complier population when defined in terms of being on SSI four years after age 18. These proportions are similar to the 23% complier population estimated by Maestas et al. (2013) for adult SSDI recipients defined in terms of the disability examiner instrument.

[16]These results by diagnosis are consistent with findings by Hemmeter and Gilby (2009) that children with intellectual disabilities have relatively low age-18 cessation rates while those with other mental conditions have high cessation rates.

4 IV estimates for earnings and income levels

4.1 Estimating the earnings and income response to removal

Figure 5 presents the IV estimates for the SSI income loss in each year juxtaposed with the average earnings response in each year. SSI youth who are removed from the program after an age 18 medical review lose $7,700 per year, which is close to the maximum annual SSI benefit. Since the inflation-adjusted maximum benefit does not change over time, this effect is constant throughout the 16 years in which I observe their outcomes. SSI youth who "comply" for the next 16 years lose approximately $115,000 in SSI income, or $90,000 in present discounted value.

Figure 5 shows the change in earnings over time in response to this SSI income loss (ignore the dashed "re-weighted" line for now). Removed SSI youth increase their earnings by a statistically significant $2,600 annually, or approximately one-third of the SSI cash benefit loss. This earnings response represents a 62% increase over the control group mean of $4,200. Despite larger standard errors over time, the point estimates remain fairly constant throughout the post-period. The major exception is year 14, when the Great Recession hits removed SSI youth hard enough that their earnings fall to the same level as the control group. Removed SSI youth increase their earnings by $29,000 in present discounted value earnings over the 16-year post-period. The main IV estimates are given in Table 6.

In addition to mean earnings, another informative measure of the earnings response is the likelihood of having positive earnings or earning at a full-time, full-year level. Figure 6 presents IV estimates over time for various earnings cutoffs. These results present a more nuanced picture of the effects of removal than mean earnings. SSI removal increases the likelihood of having positive earnings by 24 percentage points averaging over the next 16 years, an increase of 60% relative to the control group mean. Removal increases the likelihood of earning above $15,000—approximately the full-time, full-year minimum wage level—by 11 percentage points and above $20,000 by 6 percentage points, both more than a 90% increase over baseline. The final graph in Figure 6 shows the effect of removal on the probability of earning more than the individual's original SSI cash benefit. There is a 20 percentage point increase in the likelihood of earning more than the lost SSI income, which represents a 100% increase over baseline. All of these estimates are statistically significant.

The higher earnings thresholds exhibit a clear upward trend over time. This evolution of the earnings response suggests that the earnings potential of those who are removed increases over time relative those who remain on SSI, at least for some fraction of SSI youth at the higher end of the earnings distribution. Potential explanations for this increase include gains in labor market experience and skills as well as changes in preferences for work relative to those still on SSI, but my data do not allow me to distinguish between these explanations. Once again, the effects of the Great Recession can be seen clearly in the estimates for the higher earnings cutoffs, which plummet to zero in year 14 but recover by year 16.[17]

I also examine heterogeneity in the earnings response by subgroup. I find that greater parental resources—as measured by parental earnings and family structure—are associated with a larger earnings response by SSI youth to removal from SSI, as shown in Figure 7. The earnings response of SSI youth in the lowest parental earnings quintile is close to zero, while SSI youth in the second-highest parental earnings quintile increase their earnings by a statistically significant $5,800 each year, which is approximately three-quarters of the lost SSI income. The earnings response of SSI youth in two-parent families is almost double the response of SSI youth in single-parent or no-parent families, though this difference is not statistically significant.

In contrast, I find little heterogeneity in the earnings response with respect to health status or local economic conditions, as shown in Table 7.[18] One explanation for the limited heterogeneity by subgroup is that the compliers within each category are more similar to each other than the full sample of SSI youth within each category. For example, even if the average SSI youth in a higher-severity category has lower work capacity than the average SSI youth in a lower-severity category, the higher-severity SSI youth who is *affected* by an age 18 medical review is more similar to the lower-severity SSI youth who is affected by an age 18 medical review. Indeed, I find that compliers across severity quintiles are more similar to each other on health-related characteristics than the full sample across quintiles, though this is not true for quintiles of local economic conditions. An alternative explanation for the lack of heterogeneity is that the human capital of SSI youth is so low to begin with that neither better health nor a tight labor market improves their earnings prospects.

The absence of heterogeneity across observable subgroups does not rule out the possibility of heterogeneous responses, as Bitler, Gelbach and Hoynes (2014) demonstrate in their study of the Connecticut Jobs First welfare reform program. I estimate quantile RD IV treatment effects for earnings and income using the estimator proposed by Frandsen, Frölich and Melly (2012).[19] Note that quantile estimation does not measure changes for particular *individuals* in the earnings and income distribution, but rather for particular *percentiles* of that distribution. In other words, the estimate for median earnings measures the change going from the median of the control distribution to the median of the treatment distribution, not the change for the particular individual at the median of the control distribution. The left panel of Figure 8 reveals a large

[17] As another approach to measuring long-term effects, I estimate the effect of cumulative years off of SSI on earnings, with results shown in Appendix Figure A.2. The effect is constant over time, with one additional year off of SSI increasing cumulative earnings by about $2,200 per year. This parameterization of the earnings response suggests limited long-term effects on average, consistent with the mean earnings graph in Figure 5.

[18] I measure health status using diagnosis, years on SSI (since children who entered the program earlier survived more medical reviews), and removal probability. I calculate removal probability by regressing removal via age 18 medical review on observables for those with a birthday after the cutoff and then predicting the probability of removal for both groups using observables.

[19] I implement this estimator using Melly (2014)'s "rddqte" Stata command.

degree of heterogeneity in the earnings response across different parts of the distribution. The bottom of the earnings distribution experiences very small increases as a result of SSI removal, while the highest decile earns enough to fully offset the lost SSI income. The increase in the lowest two deciles is a full order of magnitude smaller than the nearly $10,000 increase in the highest decile. Consistent with the earnings threshold results, the quantile IV estimates suggest that removal induces some SSI youth to earn at full-time, full-year levels.

Removal may also affect whether SSI youth apply for and receive Social Security Disability Insurance (DI) benefits. DI has the same medical requirements as SSI but requires a work history and pays a benefit amount based on past earnings. Since SSI removal increases work activity, and DI work history requirements are low for young workers, removed SSI youth may be more likely to have the work history to qualify for DI.[20] On the other hand, removal from SSI may discourage removed youth from applying for DI since the medical criteria are the same. In general, SSI recipients do not have to undergo a medical evaluation when applying for DI benefits, meaning that SSI youth who continue on SSI know they can get DI benefits once they have a work history. I find that SSI youth who are removed from SSI submit a total of 0.73 fewer DI applications than those in the control group over the next 16 years, a 100% reduction from the control group mean (see Appendix Figure A.3). This discouragement effect decreases DI receipt among the removed by a marginally significant $730 per year (see Appendix Figure A.4). Although this loss is only 10% of the SSI cash income loss, it is a 100% reduction in DI benefits from the baseline, meaning that those who are removed from SSI lose all of their potential DI income. The present discounted value of the DI income loss is $12,000 over 16 years.

Figure 9 shows the effect over time on total observable income, which includes earnings, SSI income, and DI income. SSI youth lose on average $6,100 per year as a result of the SSI loss. The observed income loss totals $73,000 in present discounted value over 16 years, which is 80% of the original SSI income loss. Figure 8 shows quantile IV estimates for the income drop. The income losses are largest below the median of the income distribution, in excess of $6,000 annually.

In addition to income levels, another important measure is the amount of time spent below some "subsistence" level of income where the marginal utility of income is very high for a risk-averse individual. Although there is no consensus on what constitutes a subsistence level of income, the federal poverty line is a commonly used benchmark. Since most of these youth, whether on or off the program, have observed income below the poverty line, I instead measure the effect of SSI removal on the likelihood of having observed income below 50% of the poverty line—often referred to as "deep" poverty. SSI removal has a very large effect on the number of years spent with observed income below half of the poverty line. As shown in Table 6, SSI

[20] Workers under the age of 24 qualify with 1.5 years of work. Workers between 24 and 31 qualify with a work history of half the time between age 21 and the onset of disability.

youth who are removed from SSI spend on average nearly 16 years—the entire observed post-period—with observed income at deep poverty levels, compared to 5 years for the control group.

Finally, I do back-of-the-envelope calculations to estimate the Medicaid loss from SSI removal since I do not observe Medicaid enrollment in my data. Using self reports of Medicaid enrollment from the National Survey of SSI Children and Families, Hemmeter (2011) finds that Medicaid enrollment among SSI youth who remain on SSI after an age 18 medical review is 96%, versus 25% for those who are removed. Multiplying the SSI enrollment first stage in each year by 71 (=96-25) percentage points gives an approximate Medicaid enrollment first stage. I then multiply this Medicaid enrollment first stage by the value of Medicaid to get the value of the Medicaid loss. Estimates for the value of Medicaid vary widely, and few focus on the specific population of disability recipients. Finkelstein et al. (2014) use estimates from the Oregon health insurance experiment to estimate an annual value of Medicaid of $2,600 for the broader low-income population. The value of Medicaid may be larger for the disabled population if they have higher out-of-pocket expenditures without insurance, or if the covariance between the marginal utility of consumption and the Medicaid transfer is higher for them. Using $2,600 as a lower-bound estimate for the value of Medicaid for this population, I estimate that the total value of the SSI loss including Medicaid is at least $117,000 in present discounted value, or 30% greater than the value of the cash benefit loss alone.[21]

4.2 Adjusting for complier composition and probing robustness

Recall from Figure 4 that the first stage effect on SSI enrollment changes considerably over time. If these changes reflect nonrandom entry and exit, then the composition of the compliers—those control group members who stay on SSI and those treatment group members who stay off of SSI—will change over time. In this case, the evolution of the year-by-year IV estimates would reflect not only true changes in the earnings response over time but also changes in the composition of the complier population over time. To investigate this issue, I measure changes in the composition of the complier population over time in the final columns of Table 4. The "Off SSI Year 2 compliers" are the group of children who were off the program in year two if assigned to the treatment group and on the program in year two if assigned to the control group, and analogously for "Off SSI Year 16 compliers." Between year 2 and year 16, the complier population becomes poorer and less healthy. Later year compliers enter the program at a younger age and are much more likely to have a diagnosis of intellectual disability. They are also more likely to come from poor and single-mother-headed households. Recall from Section 3 that most of the attenuation in the first stage comes from control group members falling out of compliance by leaving the program, either because their health improves or because

[21] The 1997 March Supplement to the Current Population Survey values Medicaid at $6,000 for individuals aged 20-34 who are on SSI. Using this figure, the total value of the SSI loss including Medicaid would be $182,000 in present discounted value, or more than 100% greater than the value of the cash benefit loss alone.

they violate income and asset rules. The members of the control group who stay on SSI—i.e., who continue to "comply" over time—are likely to be those with more severe disabilities (no medical improvement) and the greatest financial need (no income or asset violations). Therefore, it makes sense that later-year compliers are on average poorer and in worse health than early-year compliers.

I adjust for this selection problem by re-weighting the IV estimates over time to reflect the year 2 compliers, following the methodology in Angrist and Fernandez-Val (2010). The basic method is as follows: identify the observable characteristics which differ across the complier populations and have predictive power over reduced form outcomes, divide the sample into cells based on these characteristics, estimate the earnings response within each cell, and take a weighted average of the IV estimates across the cells, where the weights are the composition of the reference population. Formally, the calculation is

$$\hat{\beta}_{t,reweight} = \sum_{c} \omega_c \hat{\beta}_{t,c}$$

where $\hat{\beta}_{t,reweight}$ is the re-weighted IV earnings estimate in year t, ω_c is the proportion of the reference population in cell c, and $\hat{\beta}_{t,c}$ is the IV estimate for earnings in year t within cell c. I construct cells based on intellectual disability diagnosis and parental earnings, for two reasons. First, these two characteristics change substantially over time in the complier population, as shown in Table 4. Second, as discussed above, parental earnings are highly predictive of the youth's earnings response; the earnings response is somewhat larger for SSI youth with intellectual disabilities in the first five years, though the difference is not statistically significant and dissipates in later years. Since I use year 2 compliers as the reference population, the weights are the proportion of year 2 compliers in each cell.

The dashed line in Figure 5 gives the complier re-weighted IV estimate for earnings in each year. Overall, re-weighting does not change the IV estimates for mean earnings substantially, nor are the differences statistically significant, but in later years the re-weighted estimates are somewhat larger than the unweighted estimates. This is the expected direction based on how the complier population changes over time. Compared to later years, the complier population in year 2 has a larger proportion of SSI youth with high parental earnings because control group members with high parental earnings are more likely to leave SSI as adults. Re-weighting by the year 2 complier population therefore shifts weight to the earnings response of SSI youth with higher parental earnings, who are far more responsive than those with low parental earnings. Thus the earnings response increases after re-weighting to reflect the more-responsive year 2 compliers. As shown in Figure 6, the effects of re-weighting are more striking for higher earnings thresholds, especially in later years, though the confidence intervals are large. Of course, an important caveat is that this re-weighting exercise does not correct for unobservable differences across complier populations.

The attenuation of the first stage over time also affects the interpretation of the IV estimates relative to the control group. The percentage changes in Table 6 are calculated relative to the control group mean. However, because many control group members leave SSI at a later date, the control group mean for post-period average annual SSI income understates the SSI income of the control group members who stay on SSI. Similarly, the control group mean for post-period average annual earnings overstates the earnings of the control group members who stay on SSI, since control group members who leave SSI will increase their earnings after exiting the program due to the reversal of income and substitution effects. Therefore, the percentage increase in earnings relative to the full control group understates the percentage increase relative to those who stay on SSI, which is the effect of interest. In Appendix Table A.4, I calculate the percentage changes in earnings and income relative to control group members who stay on SSI. As expected, the percentage increase in earnings increases substantially, from 62% relative to the full control group mean to 190% relative to the control group subset that stays on SSI. In addition, the estimated *level* of earnings for removed SSI youth falls substantially, from \$6,860 (= \$2,638 + \$4,222) using the full control group mean to \$4,024 (= \$2,638 + \$1,386) using the control group subset that stays on SSI. However, the nonrandom attrition of the control group from SSI means that the control group members who stay on SSI are a selected sample—as discussed earlier, their earnings capacity is likely to be lower than that of other control group members. The true control "complier" earnings mean is likely somewhere in between the mean of the full control group and the mean of the control subset that stays on SSI.

Finally, I probe the robustness of the main parametric RD-DD IV estimates to different polynomial orders and covariates (Appendix Tables A.2-A.3), to standard RD IV estimation (Online Appendix Tables B.7-B.10), and to non-parametric RD-DD IV estimation (Online Appendix Tables B.11-B.14). For the parametric RD-DD and RD specifications, I report the polynomial order selected by two goodness-of-fit tests: the Akaike information criterion and the bin selection method outlined in Lee and Lemieux (2010). The non-parametric estimates use local linear regression with a triangle kernel and a bandwidth varying between 60 and 260 days, and I calculate the optimal bandwidth for each outcome using cross-validation. The results are robust to different specifications. The standard RD IV estimates are very close to the corresponding RD-DD IV estimates (within 15%). The estimates change modestly with polynomial order, with the linear RD-DD estimate larger than, but statistically indistinguishable from, the quadratic specification. The non-parametric RD-DD IV estimates are close to the parametric RD-DD estimates (within 30%) and do not vary substantially with bandwidth. The results are also robust to the inclusion of covariates.

4.3 Comparing removed SSI youth to non-disabled disadvantaged youth

The IV estimates indicate that, on average, SSI youth have a small earnings response to being removed from the program. In interpreting this response, it is informative to know the long-term earnings of children who come from similar low-income households but do not have disabilities. This comparison gives a benchmark for a "successful" labor market outcome for children of the same socioeconomic status as the SSI children in my sample, whose parents have earnings of just \$10,000 on average in the year of the child's 18th birthday. I construct a disadvantaged but non-disabled comparison using the National Longitudinal Survey of Youth-1997 (NLSY97) since youth in this survey are similar in age to youth in my sample, with years of birth between 1980 and 1984. Since detailed data on parental earnings over multiple years are not available in the NLSY97, I instead restrict to NLSY97 youth whose parents have a high school education or less. Figure 10 compares the outcomes of these disadvantaged but non-disabled NLSY97 youth to the outcomes of the disadvantaged and disabled SSI youth in my sample. By age 30, mean earnings for the NLSY97 sample are \$27,000 for those whose parents have a high school education or less, and \$22,000 for those whose parents have strictly less than a high school education. Mean earnings of removed SSI youth are much lower at just \$5,900 (\$8,600 after re-weighting to reflect the healthier year 2 compliers). The gap between the two groups is less stark at the upper end of the earnings distribution. The proportion of NLSY97 youth earning above \$15,000 by age 30 is 62% for those with parents with a high school degree or less and 55% for those with parents with less than a high school degree; it is 31% for removed SSI youth (46% after re-weighting).[22]

These comparisons suggest that while a substantial minority of removed SSI youth perform reasonably well in the labor market relative to their non-disabled counterparts, the majority of removed SSI youth fall far behind. The comparisons also provide insight into the long-term effects of SSI. From Figure 10, the earnings growth of removed SSI youth lags well behind that of the broader disadvantaged youth population in the first decade after age 18. SSI does not appear to substantially limit the potential earnings of those who stay on SSI over the long term.

5 IV estimates for income volatility

If individuals are risk averse and unable to smooth consumption intertemporally, SSI income has the benefit not only of increasing income levels, as demonstrated in the previous section, but also potentially of reducing the variance of income. The reliability of a steady stream of monthly SSI income makes it more valuable

[22]These figures compare self-reported earnings data for the NLSY97 youth to administrative earnings data for the SSI youth. As a check of validity, I do an analogous comparison between my sample and disadvantaged youth (proxied by black males of the same age) in the Continuous Work History Sample (CWHS), which contains administrative earnings data for a 1% sample of all Americans. I find nearly identical results in both comparisons.

than earned income with the same face value, especially for a low-education population whose employment opportunities are mostly limited to occupations with high turnover and unpredictable hours, such as food service and retail.[23]

To measure within-person intertemporal variance of income, I use the coefficient of variation:

$$CV_i = \frac{\sigma_i}{\mu_i}$$

where σ_i is the standard deviation of individual i's income over post-treatment years 2 to 16 and μ_i is mean income over the same time period. Dividing by the mean allows for a standardized measure of variance, so that the variance does not mechanically increase with the level of income. I exclude event years 0 and 1 because these are the years immediately after the age 18 medical review, when SSI income permanently falls and earned income permanently rises for those who are removed. Since I am interested in measuring fluctuations rather than permanent changes, including the initial event years would artificially inflate the coefficient of variation.

I calculate the coefficient of variation using de-trended earnings and income to avoid picking up secular earnings and income growth. Since the earnings trajectory is steepest during the early years in the workforce, large changes in earnings and income may not be indicative of the ups-and-downs that harm risk-averse individuals but rather of large "ups" due to skill accumulation and experience among those removed from SSI. A more informative measure of earnings and income volatility is the change from the previous year *after* accounting for the secular upward trajectory of earnings and income over time. To construct a measure of de-trended changes in earnings and income, I run the following regression for each individual in my sample:

$$y_t = \alpha + \beta \tau_t + \epsilon_t$$

where y_t is the individual's earnings (income) in year t and τ_t is an event year trend. The residual from this regression is a measure of de-trended earnings:

$$\tilde{y}_t = y_t - (\hat{\alpha} + \hat{\beta} \tau_t)$$

I construct the coefficient of variation using the de-trended $\tilde{y}_t$, which reflects the annual change in earnings after accounting for the time trend in earnings. It leaves the "undesirable" portion of earnings volatility: the

[23]NPR's *Planet Money* uses the American Community Survey to identify the most common jobs in the bottom two deciles of wage and salary income: nursing aides, cooks, cashiers, janitors, housekeepers and maids, retail sales clerks, other teachers, secretaries, child care workers, servers, and truck drivers. See Quoctrung Bui, "The Most Common Jobs for the Rich, Middle Class, and Poor," NPR *Planet Money*, October 16, 2014.

part that reflects idiosyncratic shocks rather than a secular upward age-earnings trajectory.

I find that SSI removal increases the coefficient of variation for income from 0.458 to 1.850, a highly statistically significant 300% increase. The average coefficient of variation of income for removed SSI youth falls in the 95th percentile of the control group distribution. Figure 11 depicts the large increase in income volatility visually.

Interestingly, the coefficient of variation of *earnings* actually *falls* by a statistically significant 0.810 as a result of SSI removal, which is a 65% decrease relative to the control group mean. This decrease in earnings volatility suggests that SSI removal prompts SSI youth to seek more stable employment than they would have if they had stayed on SSI. Still, the greater earnings stability is not enough to offset the loss of the reliable SSI income stream, and the volatility of their overall income quadruples as a result of removal. The estimates for the coefficient of variation are similar in magnitude using non-detrended earnings and income, likely because there is limited earnings growth in practice for this population (see Appendix Table A.3).

The magnitude of the increase in income volatility resulting from SSI removal is striking. In general, the increase in income volatility is the result of switching from reliable SSI benefits as the primary source of income to volatile earnings as the primary source of income. Earnings are especially volatile for low-education and low-income populations. I use the Continuous Work History Sample, which contains annual earnings from administrative tax data for 1% of the U.S. population, to compare earnings volatility by earnings level in the general population. Restricting to individuals in the CWHS of the same age as the SSI youth in my sample, I calculate the coefficient of variation of earnings between the ages of 20 to 34 across earnings quintiles. I find that the individuals in the bottom quintile of average earnings over this period have a coefficient of variation of earnings of 1.845 on average, compared to 0.704 for the middle quintile and 0.561 for the highest quintile. Therefore the primary alternative source of income for SSI recipients is likely to be highly volatile earnings. Morduch and Schneider (2014) also document substantial income volatility for poor households: poor households experience a large relative income drop (more than 25% of median income) every one in four months, compared to one in every five months for non-poor households.

These results on income volatility indicate that SSI benefits have a large income stabilization effect. When recipients are unable to smooth consumption intertemporally, greater income stability translates into greater consumption stability, which increases utility under risk aversion. One implication of this result is that, under risk aversion and limited intertemporal consumption smoothing, the SSI benefit is more valuable than its face value would suggest because it is much less volatile than earned income. In the next section, I attempt to quantify the income stabilization benefits of SSI using back-of-the-envelope welfare calculations.

6 Welfare implications

6.1 Considering the relationship between income and consumption

The results show a large drop in total observed income and a large increase in income volatility as a result of SSI removal. The relevant questions in interpreting the income drop are 1) whether it is off of a low or high baseline, and 2) to what extent it reflects a drop in total *consumption*. With respect to the first question, baseline income and consumption are likely to be quite low given that SSI is heavily means-tested; average annual parental earnings for my sample is $10,000 in the year of the child's 18th birthday and average observed household income (which includes parent earnings, parent SSI and DI payments, and sibling SSI payments) is $16,000. Median parental earnings and income are several thousand dollars lower.

The second question—whether the fall in total observed income reflects a fall in consumption—involves a number of considerations. As noted by Meyer and Sullivan (2003), limited saving and dissaving in low-income populations mean that income may be a reasonable proxy for consumption for this group. However, it is possible that part of the lost SSI income is made up by sources of income that I do not observe, such as monetary and in-kind transfers from family members, non-disability public assistance, and unreported earnings. I consider each of these sources in turn. With respect to family member transfers, I estimate from the National Survey of SSI Children and Families (NSCF) that 54% of youth between the ages of 22 and 24 years who were on SSI as children live with one or both parents; this proportion is higher for those still on SSI (60%) than those no longer on SSI (45%). Among those living independently, 19% report receiving monetary financial assistance from family members, and this proportion is higher for those no longer on SSI (see Appendix Table A.5).

Given the number of SSI youth potentially living with parents or receiving transfers, it is informative to estimate the response of parents and other family members to the youth's removal. Linking SSI youth to their parents in SSA administrative data, I find that parents do not respond to the loss of their child's SSI income by increasing their earnings or income substantially. Parents increase their earnings by $1,600 annually after the SSI loss—compared to a $7,700 loss in SSI income—but this effect is not statistically significant, nor does the IV estimate appear to increase in the years immediately after the treatment (see Appendix Figure A.5). Parents do not adjust their DI and SSI applications or income in response to an unfavorable age 18 decision, though sibling SSI applications and income fall (see Online Appendix Table B.5).

With respect to non-disability public assistance, rates of welfare receipt estimated from the NSCF are nearly identical for those still on the program and those off the program, at around 10% for non-SSI cash welfare and 27% for Food Stamps. As expected, health insurance coverage is much higher for those still on SSI (96%) than for those no longer on SSI (52%), with most of the difference coming from Medicaid enrollment.[24] Of course, the NSCF estimates are descriptive and not causal. Taken together, however, these figures suggest that even if family transfers and public assistance make up some of the lost SSI income, they are unlikely to be making up a large portion of the loss. Unfortunately, the NSCF provides no information for those 25 years and older, whereas I observe members of my sample until they are 34 years old.

Finally, the very limited research on unreported work suggests that it is common in low-income communities but, with the exception of lucrative illicit activities, generally accounts for a modest share of a given individual's income. Edin and Lein (1997) estimate that 40% of single mothers receiving welfare benefits engage in some unreported work, and unreported earnings constitute 12% of income averaged over all single mothers receiving welfare.[25]

Taken together, this causal and descriptive evidence on unobserved sources of income—family transfers, other public assistance, and unreported earnings—suggests that unobserved income is unlikely to be a large, reliable source of income for removed SSI youth. The drop in observed income may therefore translate into a substantial, if not equally large, drop in consumption.

The key question in interpreting the increase in income volatility is to what extent it translates into consumption volatility—in other words, to what extent the SSI population is able to smooth consumption over time. Low-income populations tend to have low saving rates and face barriers to credit access, both of which would exacerbate consumption volatility. Dynan, Skinner and Zeldes (2004) estimate annualized saving rates of 1% or less for the lowest income quintile in the United States, compared to 5-10% for the middle quintile. Meyer and Sullivan (2003) state that income and expenditures are approximately equal for low-income single mothers because "little saving and dissaving occurs for this group," though income and consumption may still differ due to the services that flow from durable expenditures. Morduch and Schneider (2014) find a ratio of annual deposits to year-end balance of 50:1 for low- and middle-income Americans, meaning that there is little accumulation of saving across years.

In addition to low savings rates, credit constraints among the poor have been well-documented in developing countries and are likely relevant to low-income populations in the United States as well. According to the FDIC (2014), 28% of households with incomes of less than $15,000 have no bank account, and another 22% have a bank account but rely on alternative financial services like check cashing and payday lending. Even among banked households, Morduch and Schneider (2014) document costly strategies to smooth con-

[24]These relatively low rates of welfare receipt (for a low-income population) are not surprising given that most non-disability public assistance in the United States is targeted at families with children and most SSI young adults do not live with children. In most states in the relevant time period, childless and noncustodial adults do not qualify for TANF or Medicaid; food stamps are limited to 3 months per 3 years for adults working less than 20 hours/week; and the maximum EITC for childless adults is $500/year.

[25]Venkatesh (2009), describing the underground economy in poor urban communities, reports that unreported work is common but "never steady enough to ensure this income stream for an entire year." For those hired off-the-books, "the work is really about survival, and it is doubtful that they are prepared to work full-time and pursue conventional employment paths."

sumption, such as using overdrafts, varying on-time and late bill payments each month, and using short-term credit from friends or family and alternative financial services. These figures suggest that credit is costly for very low-income families, though intra-household risk sharing may still allow for some consumption smoothing. Given this evidence on saving rates and barriers to intertemporal consumption smoothing, it is likely that the increase in income volatility translates into at least some volatility in consumption.

6.2 Estimating effects on recipient welfare

I start by considering the effect of SSI removal on the individual welfare of the former SSI recipient, and later consider social welfare. In the standard labor economics problem, the agent faces a tradeoff between consumption and leisure and maximizes utility with respect to those variables subject to a budget constraint:

$$\max_{c,l} u(c,l) \text{ s.t. } pc = w(T-l)$$

where c is consumption, l is leisure, p is the price of the consumption good, w is the wage, T is the total available hours, and $u_c > 0$ and $u_l > 0$. Under various assumptions outlined below, I use my estimates on income to approximate the recipient's consumption (c). The choice of work hours ($T-l$) may be especially pertinent for individuals with disabilities if their disutility of work is high. Unfortunately, I do not observe any measures of the disutility of work in my data and therefore cannot incorporate this potentially important parameter into my calculations. Assuming that the disutility of work is positive, I will likely underestimate the recipient's welfare loss from SSI removal by ignoring the effect of removal on work hours, since I find in Section 4 that removal increases earnings.

I use the certainty equivalent as a measure of the recipient's welfare. The certainty equivalent is implicitly defined as:

$$\frac{1}{T}[u(c_{CE}) + \beta u(c_{CE}) + ... + \beta^{T-1}u(c_{CE})] = \frac{1}{T}[u(c_1) + \beta u(c_2) + ... + \beta^{T-1}u(c_T)] \tag{5}$$

where c_{CE} is the certainty equivalent, c_t is actual consumption in year t, and T is the total number of periods (16 years). In words, the certainty equivalent is the annual guaranteed consumption amount that makes the recipient indifferent between receiving that amount in each period and receiving his actual, volatile consumption stream. I assume a constant relative risk aversion (CRRA) utility function with coefficient of relative risk aversion γ:

$$u(c) = \frac{c^{1-\gamma}}{1-\gamma}$$

Note that in this utility function, consumption levels work through the c and consumption volatility works through the γ.

I take three approaches to calculating consumption. In the first approach, I take consumption in each year to be the maximum of total observed income in that year and some consumption floor $\underline{c}$:

$$c_{1t} = \max(\text{earnings}_t + \text{SSI}_t + \text{DI}_t, \underline{c})$$

The assumption that consumption in each year equals income in that year might be reasonable for this low-income population, for the reasons discussed in the previous subsection. However, I follow the standard method (Brown and Finkelstein (2008); Hoynes and Luttmer (2011); Finkelstein et al. (2014)) of imposing a consumption floor to avoid utility values of negative infinity and to rule out implausibly low consumption levels.[26] Under the first approach, the proportion of the sample below the consumption floor in a given year ranges from 25% for a consumption floor of \$1,000 to 35% for a consumption floor of \$5,000. Since these are large proportions of the sample, I also use a second approach in which I attribute one-third of parental income to the recipient:

$$c_{2t} = max(\text{earnings}_t + \text{SSI}_t + \text{DI}_t + \frac{\text{parent income}_t}{3}, \underline{c})$$

Under the second approach, the proportion of the sample below the consumption floor in a given year ranges from 10% for a consumption floor of \$1,000 to 20% for a consumption floor of \$5,000.

In the third approach, I account for the possibility that I simply underestimate the income of all individuals in the sample by a fixed amount because I do not observe family transfers, non-disability public assistance, and unreported work (see discussion in Section 4.3). This "level shift" in consumption matters under risk aversion because the curvature of the utility function decreases with the consumption level. In this approach, I calculate consumption as the sum of total observed income and a consumption supplement $\tilde{c}$:

$$c_{3t} = \text{earnings}_t + \text{SSI}_t + \text{DI}_t + \tilde{c}$$

For each of the three consumption measures, I calculate the certainty equivalent c_{CE} from equation (5) for each individual in the sample and put c_{CE} on the left-hand-side of equation (3), using average annual SSI enrollment over the post-period as the endogenous regressor. Table 8 shows the certainty equivalent loss from SSI removal under the three approaches, for different values of the risk aversion parameter γ and different

[26] To determine a reasonable consumption floor, I estimate the distribution of annual expenditures for the low-education and low-income population from the Consumer Expenditure Survey. The 10th percentile of per-capita annual expenditures is between \$2,000 and \$3,000 for households with heads with less than a high school education, households with heads with a high school education or less, and households with income less than \$20,000. Therefore I choose consumption floor values ranging from \$1,000 to \$5,000.

values of the consumption floor ($\underline{c}$) or supplement ($\tilde{c}$). For a risk neutral individual ($\gamma = 0$), who considers only the average consumption loss and not the volatility of consumption, the certainty equivalent of the SSI loss when using only recipient income (c_{1t}) ranges from \$3,600 to \$5,900, depending on the consumption floor. For $\gamma > 0$, the loss is greater in certainty equivalent terms because the individual considers both the fall in consumption levels and the increase in consumption volatility after removal. The certainty equivalent loss from removal for $\gamma = 2$ ranges between \$3,900 and \$7,800. The range of the loss is similar but more compressed considering both recipient and parent income (c_{2t}). Using a consumption supplement instead of a floor (c_{3t}) increases the magnitude of the estimated loss somewhat.

I use these calculations to approximate the amount of the recipient's welfare loss from SSI removal that is attributable to an increase in consumption volatility rather than to a decrease in consumption levels. For some $\gamma > 0$, the proportion of the welfare loss from consumption volatility is:

$$\frac{(\text{Certainty equivalent loss for } \gamma > 0) - (\text{Certainty equivalent loss for } \gamma = 0)}{\text{Certainty equivalent loss for } \gamma > 0}$$

For $\gamma = 2$ using recipient income only (c_{1t}), these calculations indicate that 8-23% of the welfare loss is attributable to the increase in consumption volatility rather than to the fall in consumption levels, depending on the consumption floor. The proportion of the welfare loss from consumption volatility is similar using the other two consumption measures, though slightly lower for the measure that includes parental income (c_{2t}). For reasonable values of risk aversion, then, ignoring the income stabilization benefits of SSI could lead to a substantial underestimate of the recipient's welfare loss from SSI removal.

Three other considerations deserve mention. First, these calculations do not consider the Medicaid loss from SSI removal, nor its relative consumption supplementation versus consumption stabilization effects. Finkelstein et al. (2014) decompose the value of Medicaid into a "transfer" value, which corresponds to consumption levels, and a "pure insurance" value, which corresponds to consumption stability. I use their estimates to do back-of-the-envelope calculations incorporating the Medicaid loss. Relative to the case with SSI cash benefits only, including the Medicaid loss increases the magnitude of the certainty equivalent loss by 15-30% for $\gamma = 0$ and 25-50% for $\gamma = 2$. For $\gamma = 2$, approximately one-half of the Medicaid loss comes from the transfer or consumption level value of Medicaid, while the other half comes from the pure-insurance or consumption volatility value of Medicaid.[27]

Second, these welfare calculations assume that adult SSI is a guaranteed benefit. In practice, adults can be removed from SSI for medical or other reasons, though adult medical reviews have very low removal

[27] The Finkelstein et al. (2014) estimates are based on the Oregon health experiment, which studies the effects of Medicaid on the broader low-income population. The value of Medicaid may be larger for the SSI population since SSI recipients have worse health on average than the broader low-income population.

rates. As expected, the certainty equivalent loss falls when I allow for exit from SSI in adulthood. Both the consumption loss and the increase in consumption volatility from age 18 removal are smaller when there is a chance of removal in adulthood.

Finally, these welfare calculations do not consider the variance *across* individuals in the extent of the consumption loss, even though Figure 8 indicates substantial heterogeneity in income losses. For a concave utility function, the welfare loss for the representative agent who experiences the average income loss underestimates the welfare loss under heterogeneity. The increase in the welfare loss from those who lose more than the average is larger than the decrease in the welfare loss from those who lose less than the average. To calculate the welfare change under heterogeneous losses, I calculate the welfare loss for each quantile based on my quantile IV estimates from Section 4 and then calculate the average loss across quantiles. Relative to the representative agent, I find that the welfare loss approximately doubles with heterogeneous losses for $\gamma = 2$.

6.3 Estimating effects on social welfare

The certainty equivalent calculations show that SSI removal leads to a substantial reduction in welfare for the former recipient through both a drop in consumption levels and an increase in consumption volatility. However, the individual welfare gain from SSI (or welfare loss from SSI removal) is only one input into the ultimate question of how SSI affects *social* welfare. Diamond and Sheshinski (1995) show that the social planner weighs consumption smoothing and other benefits with moral hazard costs when the disutility of work is imperfectly observed. In this section, I consider the empirical inputs needed to estimate both sides of the social planner's problem.

I start with the benefits side of the social planner's problem. This paper contributes to the empirical estimation of the consumption smoothing benefits of SSI by estimating (under the assumptions discussed earlier) the increase in consumption levels and the decrease in consumption volatility resulting from SSI receipt. However, there are at least two additional parameters needed to estimate consumption smoothing benefits. First, it is necessary to know the magnitude of the consumption drop associated with having a disability, relative to consumption in the able-bodied state. Assuming that utility is not state-dependent, a larger consumption drop from disability means greater consumption smoothing benefits from disability insurance and thus a higher optimal level of disability insurance. My data do not allow me to estimate the consumption drop associated with disability because I do not observe individuals in the able-bodied state; in fact, the validity of my quasi-experiment rests on the assumption that the average level of disability is the same on both sides of the RD cutoff.[28] The second input needed to estimate consumption smoothing

[28]Using the Panel Study of Income Dynamics, Meyer and Mok (2013) estimate that chronic and severe disability in adults is

benefits is knowledge of the form of the utility function in the able-bodied state and the disabled state. Disability might increase the marginal utility of consumption through, for example, higher medical bills that leave the disabled individual starving. On the other hand, disability might decrease the marginal utility of consumption by making it difficult to enjoy types of consumption enjoyed by the able-bodied individual. For a population with near-poverty consumption levels, it may be reasonable to assume that the marginal utility of consumption remains high whether or not the individual is disabled, but there is little to no empirical evidence on this issue.

For pure insurance programs, it is sufficient to consider consumption smoothing benefits in calculating the benefits side of the social planner's problem. As a heavily means-tested program, however, SSI may have redistributive benefits in addition to consumption smoothing benefits, depending on the form of the social welfare function and the weights placed on different individuals. Thus another input into estimating the benefits side of the social planner's problem is the desired amount of redistribution in the social welfare function.

The other side of the social planner's problem is the moral hazard costs of disability insurance. These costs could take several forms in the context of SSI. The most obvious is the marginal tax rates on earnings imposed by the program: SSI benefit levels are reduced by $1 for every $2 in earnings after a small monthly allowance, which distorts the labor-leisure tradeoff facing the recipient. In Section 4, I find that entry onto adult SSI decreases annual earnings by $2,600 on average, but I am unable to determine how much of the reduction is attributable to marginal tax rates (substitution effects) versus the income transfer itself (income effects). Since only substitution effects are distortionary, this distinction is important for considering the social welfare impacts of SSI. Critics of the SSI program have argued that SSI may have other moral hazard effects, especially for children, such as harming health through the effort to demonstrate medical eligibility.[29] My data do not allow me to measure the effects of other types of moral hazard that do not show up in the earnings of SSI youth.

Clearly, calculating the optimal SSI benefit requires many inputs that I cannot estimate using my data and quasi-experiment. However, if I am willing to abstract from many of these issues and make strong assumptions, I can use my IV estimates for earnings and income, in combination with other data sources and estimates, to do back-of-the-envelope calculations of the optimal SSI benefit. As an illustrative exercise, I adapt the formula for optimal social insurance derived by Baily (1978), applied by Gruber (1997), and

associated with a 24% decline in food and housing consumption ten years after onset.

[29] See, e.g., Patricia Wen, "The Other Welfare," *Boston Globe*, December 12, 2010; U.S. Congress, House Subcommittee on Human Resources of the Committee on Ways and Means, Hearing on Supplemental Security Income Benefits for Children, October 27, 2011; and Nicholas Kristof, "Profiting from a Child's Illiteracy," *New York Times*, December 7, 2012.

generalized by Chetty (2006):

$$\epsilon_{e,b} = \gamma \frac{c_{able} - c_{dis}}{c_{able}}(b^*) \tag{6}$$

where $\epsilon_{e,b}$ is the elasticity of earnings to the SSI benefit; $\gamma = \frac{cu''(c)}{u'(c)}$ is the coefficient of relative risk aversion; c_{able} is consumption in the able-bodied state; and c_{dis} is consumption in the disabled state, including the SSI benefit. The term $\frac{c_{able}-c_{dis}}{c_{able}}$ is a function of the benefit level b. Roughly, this formula says that at the optimal benefit level b^* the cost to the government of providing SSI benefits—in the form of lost tax revenue from the earnings decline—equals the change in consumption between the able and disabled states, appropriately valued based on the level of risk aversion. I use this formula to estimate, under several assumptions, the optimal benefit level. I use a combination of my IV estimates from Table 6 and survey data to estimate all of the parameters in equation (6) except for risk aversion (γ). The assumptions and full methodology of the estimation are detailed in the Appendix.

The first two rows of Table 9 present the results for the globally optimal SSI benefit level for different values of relative risk aversion. As expected, the optimal benefit level increases with risk aversion. In these calculations, I assume—as is standard in optimal benefit calculations—that the optimal benefit has the same variance as other sources of income. However, my previous results on income volatility demonstrate that SSI is a more reliable source of income than earnings. To adjust for income volatility, I do a mechanical calculation of the zero-volatility SSI benefit that gives the same utility as the optimal benefit levels just calculated assuming nonzero (earnings-equivalent) volatility. The results of this exercise are given in the next set of rows of Table 9, with details on methodology in the Appendix. For $\gamma = 2$, optimal benefit levels shrink by 30% relative to the level-only scenarios: a lower SSI benefit level considering consumption stabilization benefits yields the same utility as a higher SSI benefit when consumption stabilization is not considered. Interestingly, the optimal benefit is no longer strictly increasing in risk aversion when consumption volatility is considered. This phenomenon reflects the opposing forces of risk aversion: higher risk aversion increases the optimal benefit level since there is greater value in mitigating the drop in income between the able-bodied and disabled states, but it reduces the optimal benefit level by making the SSI benefit more valuable relative to earnings.

7 Conclusion

I study the long-term effects of removing SSI youth from the program using a regression discontinuity design based on a 1996 welfare reform change, paired with administrative data from the Social Security Administration. I find that on average SSI youth who are removed from the program experience an income drop that is 80% of the lost SSI cash income. Of the $90,000 they lose in present discounted SSI income, they recover $29,000 in earnings but lose an additional $12,000 in Social Security Disability Insurance (SSDI) income. In addition to the fall in income levels, the within-person variance of income quadruples as a result of the SSI loss.

The results of this paper inform long-standing questions surrounding the effects of SSI and welfare programs more broadly. With respect to the question of whether SSI inhibits self-sufficiency and limits success in the labor market, I find that most SSI youth who are removed have a small earnings response and limited earnings growth over time, and therefore experience a large drop in income. I find no evidence for the hypothesis that removing even relatively healthy SSI youth would make them better off in the long run. Note that these findings contrast with evidence of the effect of the 1996 welfare reform law on single mothers. The general consensus of that literature (see, e.g., Blank (2008)) is that welfare reform increased employment and income and reduced welfare dependency among single mothers. Based on my results, SSI youth who were removed as a result of this legislation did not, on average, replace the lost SSI income with earnings. Instead, removed SSI youth experienced a large reduction in lifetime income and a large increase in the volatility of that income.

Addressing the question of how much insurance SSI provides, I find that considering the level of benefits alone could substantially underestimate the value of the SSI benefit. SSI removal increases the volatility of income, which under risk aversion and limited intertemporal consumption smoothing implies that SSI has income stabilization value in addition to income supplementation value. Under various assumptions, up to one-quarter of the value of SSI comes from its income stabilization effects.

These results raise the question of *why* SSI youth do not recuperate the lost SSI income. One explanation is that their disabilities either lower their earnings potential or raise their disutility of work enough that it outweighs the benefits of the additional consumption afforded by working. The substantially lower earnings of removed SSI youth relative to other low-education populations suggests that disability—either directly through health, or indirectly through societal expectations and incentives—plays an important role in the low earnings levels of removed SSI youth. Another explanation is that the effects of poverty, including low education levels, confine SSI youth to low-wage jobs and marginal labor force attachment. The fact that the earnings response varies with parental earnings and family structure, but not on any other observable dimension, suggests a potentially important role of the poverty channel. SSI youth who come from less-poor families have a much larger earnings response than those from households in abject poverty. In contrast, the earnings response does not vary with any observable measure of the severity of the disability or with local economic conditions. One interpretation of the lack of heterogeneity with respect to health is that the human capital of the SSI youth population as a whole may be so low that better health does not improve their labor market prospects. Understanding the relative roles of disability and poverty in the income drop is important for considering the usefulness of each as a tag for social insurance programs.

The findings of this paper also raise several key questions for future research on welfare and disability programs. First, this paper estimates a local average treatment effect for SSI youth when removal from SSI is unanticipated. In a dynamic framework, SSI youth who anticipate being removed from the program may adjust their education and human capital decisions in response to this belief—for example, by deciding to complete high school or enrolling in vocational training—and as a result may fare better in the labor market as adults. The question of how SSI youth respond to anticipated removal has important policy implications and also relates to the broader question in labor economics of how the expected return to education affects educational achievement. A second key question is to what extent the drop in observed income and the increase in the volatility of income found in this paper translate into a drop in consumption and an increase in consumption volatility. I use extensive survey and descriptive evidence on the SSI population and the broader low-income population to help inform this question. Future work can improve on this analysis by using data on more sources of income—such as family transfers, non-disability public assistance, and unreported earnings—as well as direct consumption data. Third, an important challenge for future research on disability insurance is measuring the disutility of work. The earnings gains of removed SSI youth may come at a cost to their well-being if their disutility of work is high, either because their disabilities make work difficult or because the only work available to them is unpleasant, low-wage jobs. Other important inputs into measuring the welfare implications of disability programs include effects on health and human capital as well as qualitative evidence on the activities and quality of life of current and former disability recipients, especially those who have little or no earned income.

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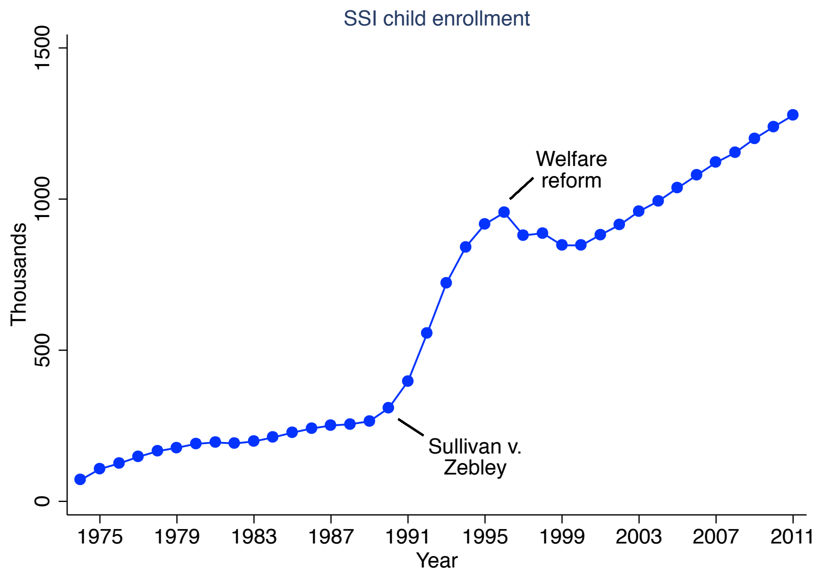
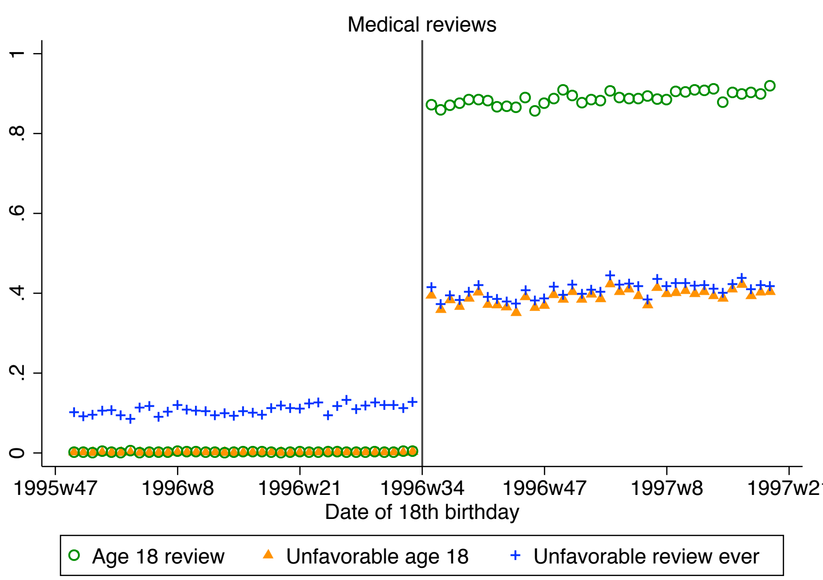
Main Figures

Figure 1: Enrollment in the SSI Children's Program, 1974-2011



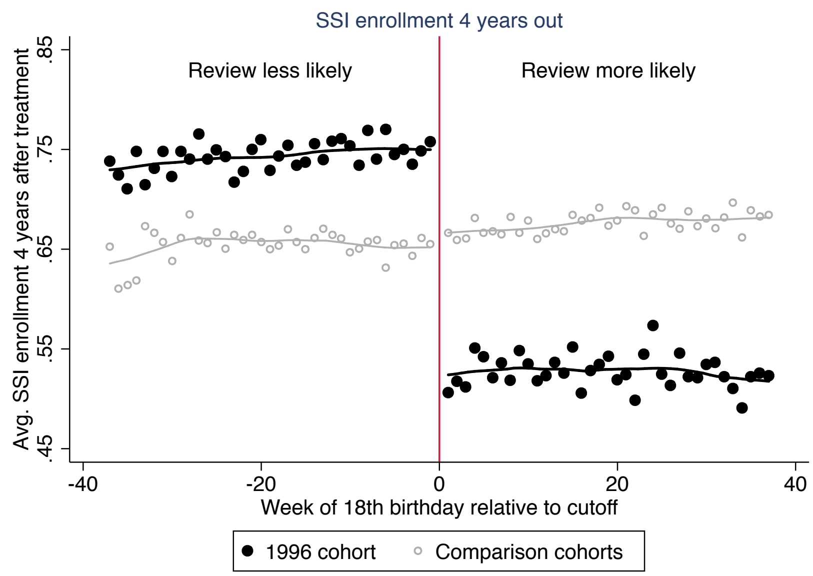
Source: SSI Annual Statistical Reports, 2002-2011.

Figure 2: Empirical Strategy Using Variation in Eligibility for Medical Reviews



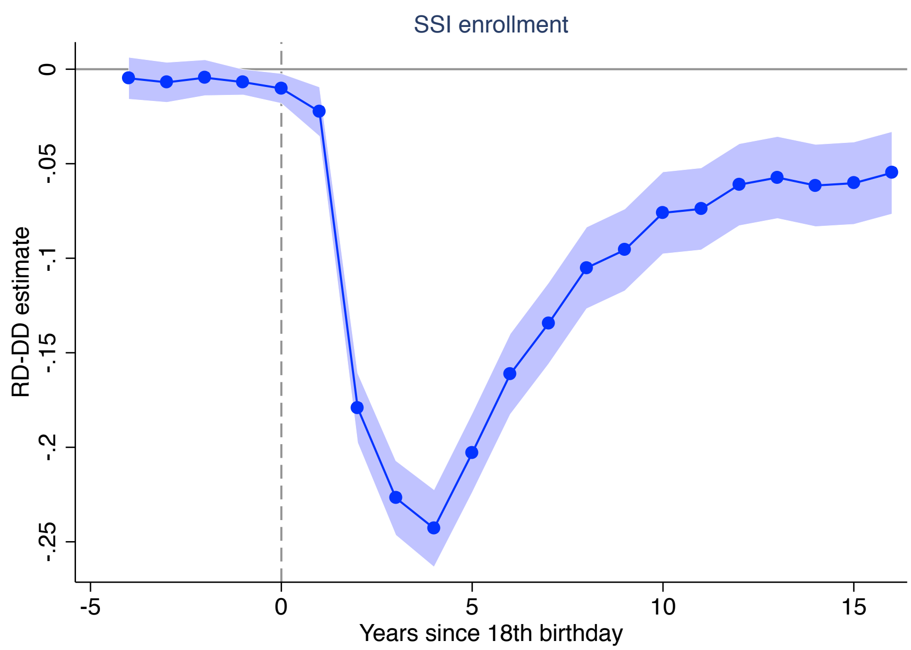
Notes: Figure plots the proportion of SSI children in each birthweek bin who receive an age 18 medical review, receive an unfavorable age 18 medical review, and ever receive an unfavorable medical review (through 2013). Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996.

Figure 3: First Stage Effect on SSI Enrollment



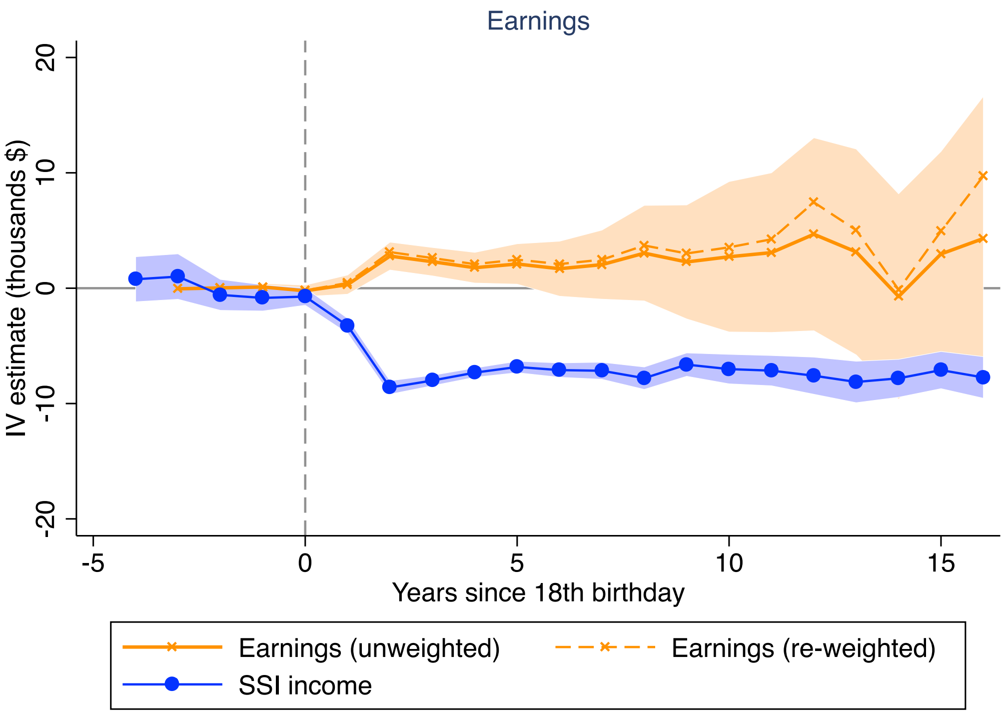
Notes: Figure plots average SSI enrollment four years after the year of the 18th birthday for each birthweek bin. Solid markers indicate the 1996 cohort, while hollow markers represent the comparison cohorts (1994, 1995, and 1997). Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997.

Figure 4: Change in First Stage for SSI Enrollment Over Time



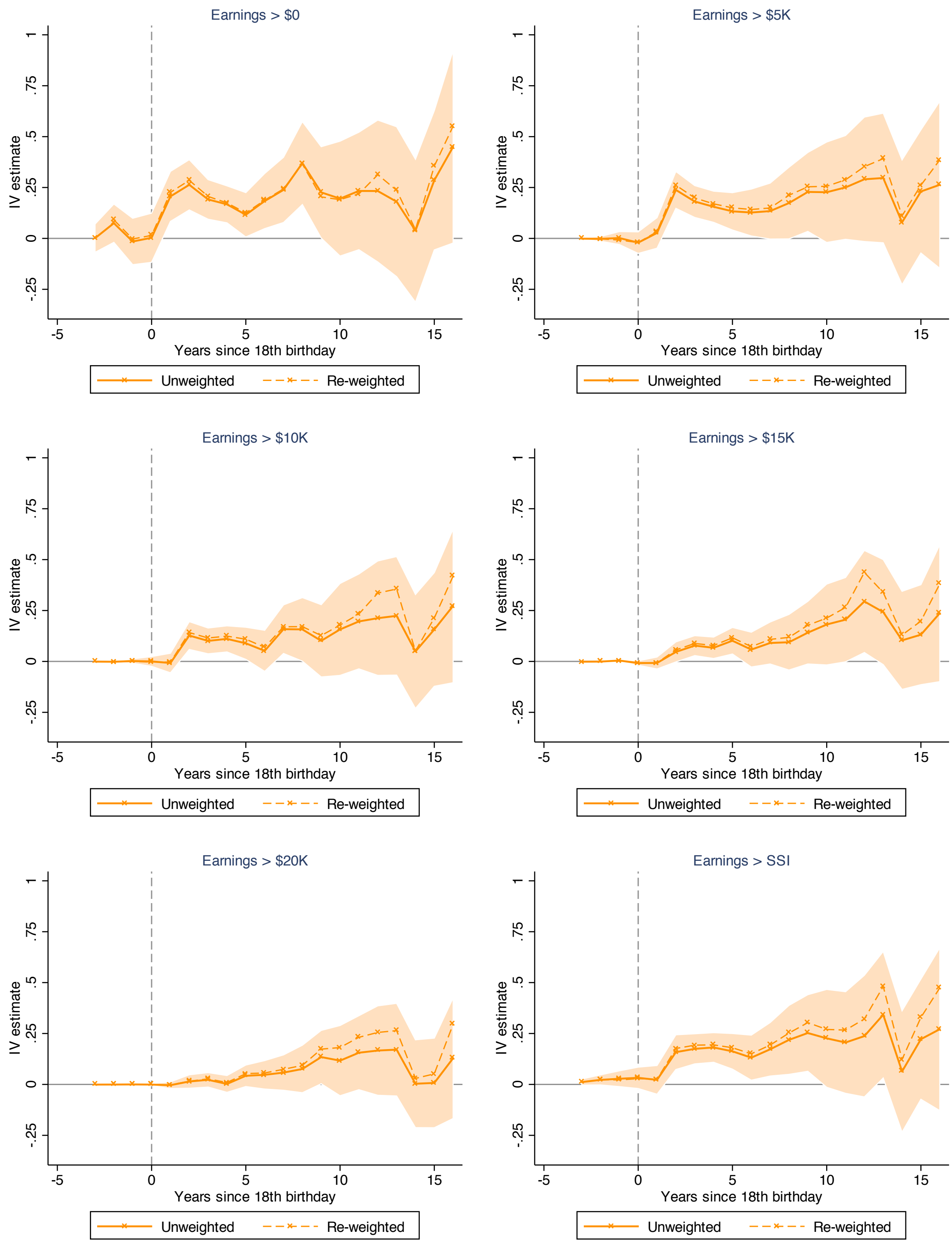
Notes: Figure plots the parametric RD-DD estimate of the effect of a child having an 18th birthday after the August 22 cutoff in the 1996 cohort versus the comparison cohorts (1994, 1995, and 1997), using a polynomial order of 2 with covariates. Shaded region is 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997. Standard errors clustered at individual level.

Figure 5: IV Estimates of the Effect of SSI Removal on Earnings



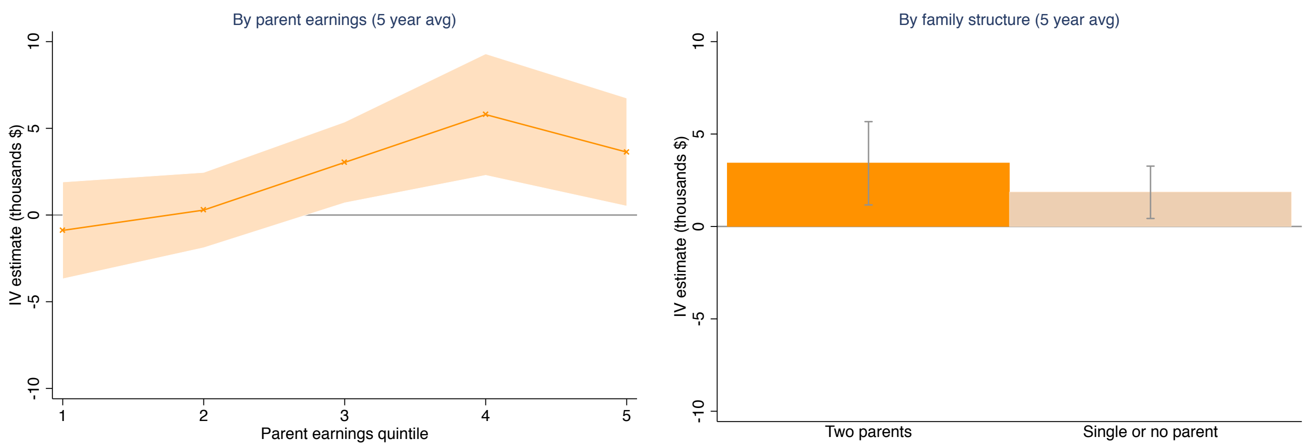
Notes: Figure plots the parametric IV RD-DD estimate of the effect of being off SSI on SSI income (circles) and earned income (X's) in each year, using a polynomial order of 2 with covariates. The solid earnings line represents unweighted IV estimates; the dashed line represents IV estimates re-weighted by the year 2 complier population (see Section 4.2 for details). Shaded region is 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997. Standard errors clustered at individual level.

Figure 6: IV Estimates of the Effect of SSI Removal on Earnings Thresholds



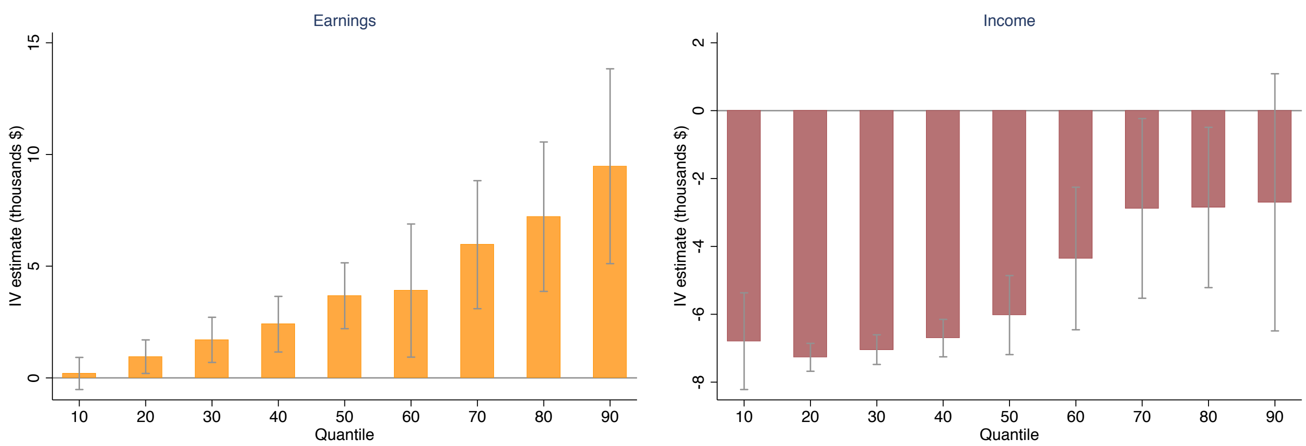
Notes: Figure plots the parametric IV RD-DD estimate of the effect of being off SSI on earnings thresholds in each year, using a polynomial order of 2 with covariates. The solid lines represent unweighted IV estimates; the dashed lines represent IV estimates re-weighted by the year 2 complier population (see Section 4.2 for details). Shaded region is 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997. Standard errors clustered at individual level.

Figure 7: Heterogeneity in the Earnings Response by Parental Earnings and Family Structure



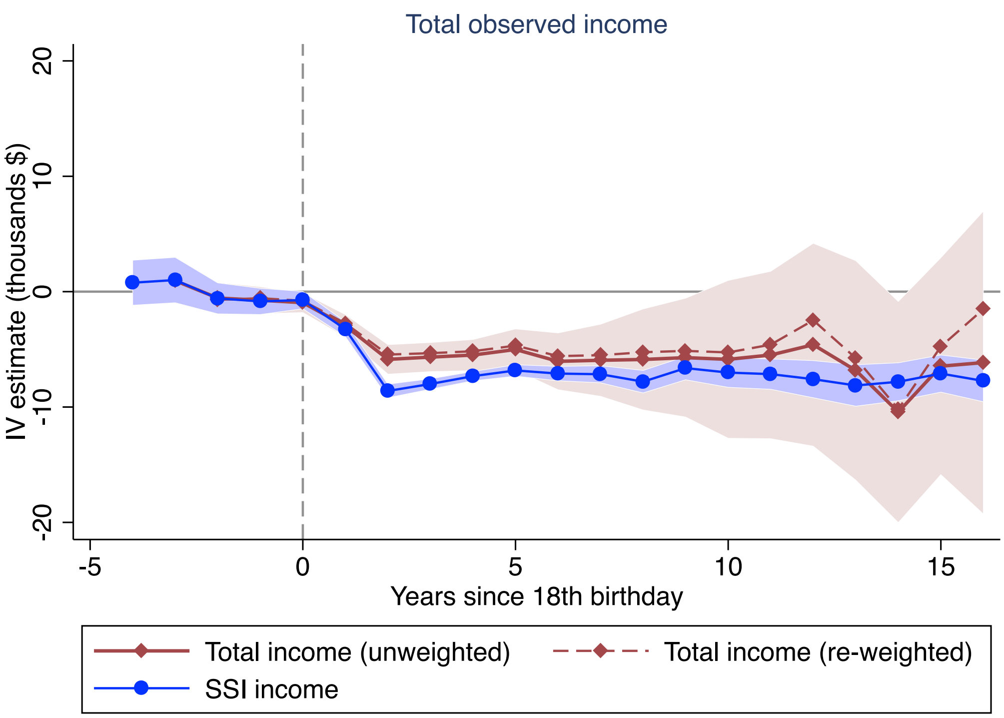
Notes: Left panel plots parametric IV RD-DD estimates of the effect of being off of SSI on earnings in the first 5 years after age 18 by parental earnings quintile, using a polynomial order of 2. Individuals with no parents are assigned a parental earnings value of \$0. Right panel plots the same IV estimates by family structure. Shaded region (or error bar) is 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997. Standard errors clustered at individual level.

Figure 8: Quantile IV Estimates for Earnings and Total Observed Income



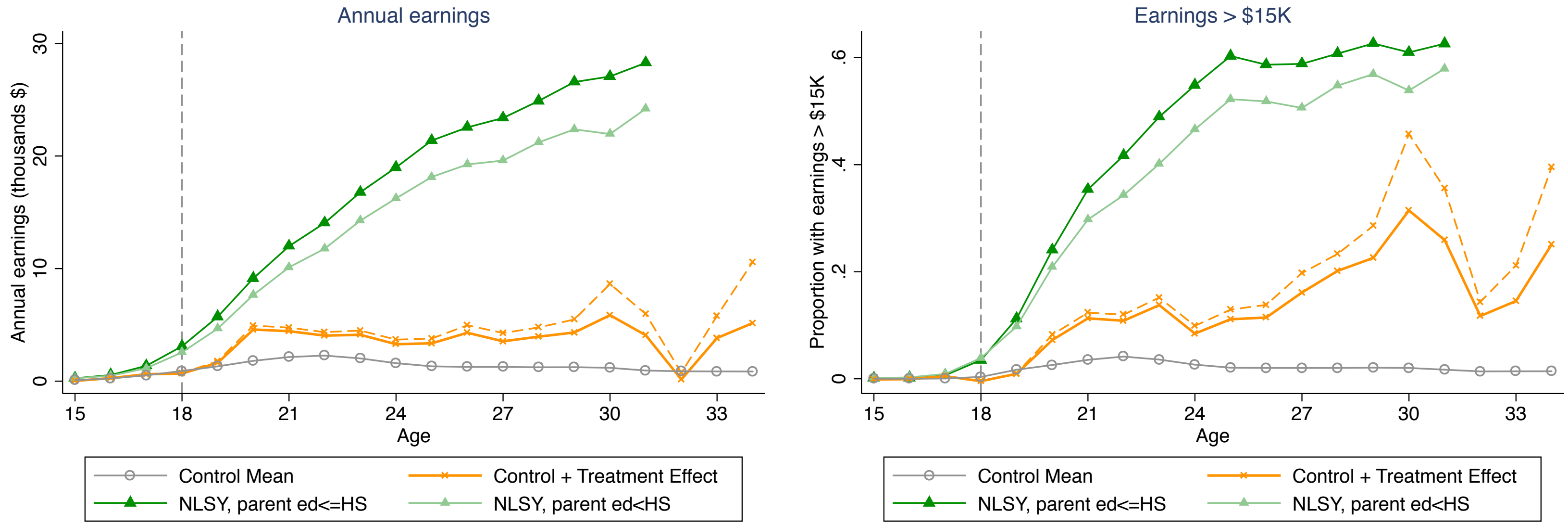
Notes: Graphs plot quantile IV RD estimates of the effect of being off of SSI on earnings and total observed income (earnings plus SSI inome plus SSDI income), using the estimator in Frandsen et al. (2012) and Melly (2014)'s "rddqte" Stata command. Error bars are 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996.

Figure 9: IV Estimates of the Effect of SSI Removal on Total Observed Income



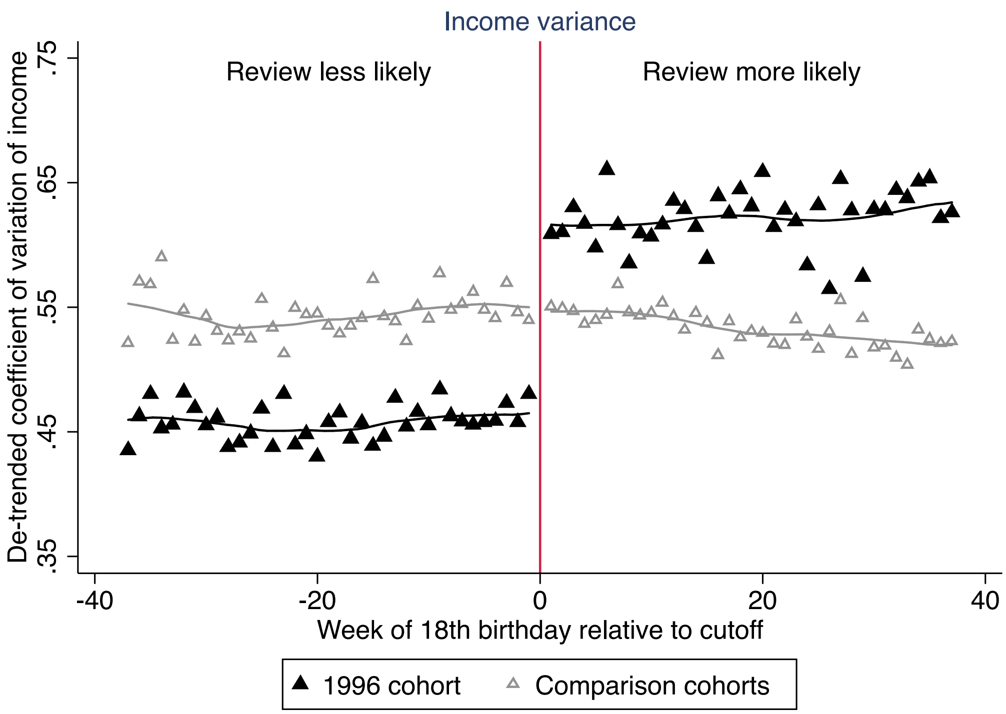
Notes: Figure plots parametric IV RD-DD estimates of the effect of being off SSI following an age 18 medical review on SSI income (circles) and total observed income (diamonds) in each year, using a polynomial order of 2 with covariates. Total observed income equals earnings plus SSI income plus SSDI income. The solid line represents unweighted IV estimates; the dashed line represents IV estimates re-weighted by the year 2 complier population (see Section 4.2 for details). Shaded region is 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997. Standard errors clustered at individual level.

Figure 10: Earnings of Removed SSI Youth vs. Broader Disadvantaged Population



Notes: Series marked with hollow circles is the average earnings (or likelihood of earning more than $15,000) for control group members who are still on SSI in that year. Series marked with X's plot control on SSI mean plus the treatment effect for average earnings (left panel) or likelihood of earnings more than $15,000 (right panel); dashed line is re-weighted by the year 2 complier population based on intellectual disability and parental earnings. Both series marked with triangles, from the National Longitudinal Survey of Youth-1997, plot the average earnings (or likelihood of earning more than $15,000) of NLSY97 youth whose parents have a high school education or less.

Figure 11: Reduced Form Effect on Income Volatility



Notes: Figure plots the average coefficient of variation for de-trended income over post-years 2 to 16. Solid markers indicate the 1996 cohort, while hollow gray markers represent the comparison cohorts (1994, 1995, and 1997). De-trended income is obtained by regressing income on a time trend for each individual in the sample. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997.

Main Tables

Table 1: Comparison of Households with SSI Children to All Households with Children

	All households with children	Households with SSI children	Low-education households with children
Household size	3.9	4.1	4.2
Annual earnings	$65,457	$19,221	$39,738
Annual total income	$71,332	$36,001	$45,325
Black	17%	38%	19%
Single mother head	28%	58%	35%
High school dropout head	14%	25%	37%
Received child SSI	3%	100%	4%
Received cash public assistance	13%	100%	20%
Received cash or noncash public assistance	54%	100%	76%
Received free or reduced price lunch	36%	78%	54%
Received housing assistance	8%	28%	12%
Annual SSI income (child or adult)	$375	$3,708	$589
Annual cash transfer income	$750	$8,684	$1,150
N	10,375	277	3,750

Source: Author's calculations from Survey of Income and Program Dynamics 2008 Panel. Notes: Table presents estimates of household characteristics using longitudinal weights. "Low-education household" indicates a household whose head has a high school education or less. Noncash public assistance includes food stamps, WIC, Medicaid, rent for public housing or government subsidized rent, government energy assistance, free or reduced-price lunches, and free or reduced-price breakfasts. Housing assistance includes public housing, government subsidized rent, and Section 8 vouchers.

Table 2: Covariate Balance Tests for RD-DD and Standard RD Specifications

	Standard RD				RD-DD			
	Linear		Quadratic		Linear		Quadratic	
	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.
Demographics								
Male	0.0038	(0.0066)	0.0039	(0.0098)	0.0122	(0.0075)	0.0155	(0.0114)
Age at entry	-0.3140***	(0.0609)	-0.0480	(0.0898)	-0.220***	(0.0696)	-0.108	(0.106)
Single mother	0.0066	(0.0068)	-2.37e-05	(0.0101)	-0.00048	(0.0077)	-0.00082	(0.0118)
No parents	-0.0062	(0.0049)	0.0015	(0.0072)	-0.0023	(0.0056)	-0.0043	(0.0085)
Latest record date	-87.7***	(18.4)	0.9350	(27.2)	-40.1*	(21.0)	-8.51	(32.1)
Diagnosis								
Mental	0.0022	(0.0060)	0.0064	(0.0089)	-0.0056	(0.0070)	-0.0025	(0.0107)
None	-0.0035*	(0.0020)	-0.0015	(0.0029)	-0.0045	(0.0043)	-4.41e-05	(0.0065)
Nervous	0.0066**	(0.0031)	0.0069	(0.0045)	0.0085***	(0.0031)	0.0062	(0.0048)
Endocrine	0.0013	(0.0026)	0.00075	(0.0038)	-0.0015	(0.0029)	-0.0036	(0.0044)
Sensory	0.0064***	(0.0025)	-0.0023	(0.0036)	0.0011	(0.0025)	-0.0032	(0.0039)
Infection	-0.0135***	(0.0026)	-0.0118***	(0.0040)	0.0002	(0.0021)	0.00086	(0.0033)
Musculoskeletal	0.00066	(0.0015)	0.0024	(0.0022)	0.00064	(0.0016)	0.0024	(0.0025)
Respiratory	0.00074	(0.0014)	0.0003	(0.0022)	0.00062	(0.0016)	0.00060	(0.0025)
Neoplasm	0.00042	(0.0013)	0.00085	(0.0020)	-1.59e-05	(0.0015)	0.00049	(0.0023)
Pre-treatment...								
Child SSI payment	152.1***	(28.8)	11.4	(42.5)	95.2***	(32.9)	27.4	(50.2)
Child earnings	-34.7***	(9.9)	-0.709	(13.2)	-6.99	(11.2)	-5.19	(15.5)
Family dis. apps	-0.0055	(0.0042)	-0.0088	(0.0062)	-0.0075	(0.0048)	-0.0114	(0.0074)
Family dis. receipt	39.7	(73.2)	-45.9	(107.6)	-2.35	(82.8)	6.68	(126.5)
Parent earnings	-574.3***	(182.0)	-421.5	(268.8)	-280.3	(205.3)	-338.2	(314.1)
N	81,799		81,799		300,888		300,888	
Joint F test	109.07		31.79		40.32		22.49	
p-value	0.0000		0.2833		0.0619		0.7582	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Table presents covariate balance tests for the linear and quadratic standard RD specification (equation (1))and the linear and quadratic RD-DD specification (equation (2)). Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 (for standard RD), plus SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1994, 1995, and 1997 (for RD-DD). Standard errors clustered at individual level shown in parentheses.

Table 3: Falsification Covariate Balance Tests

	Percent of days with p-value below...			
	0.05	0.01	0.001	0.0001
Linear specification				
RD 1995	80%	67%	59%	49%
RD 1996 (treatment)	75%	67%	61%	53%
RD 1997	75%	68%	53%	43%
RD-DD	15%	7%	0%	0%
Quadratic specification				
RD 1995	16%	6%	1%	0%
RD 1996 (treatment)	21%	10%	4%	0%
RD 1997	18%	10%	3%	1%
RD-DD	7%	2%	0%	0%

Notes: Table reports the results of covariate balance falsification tests for the standard RD in each of the years 1995, 1996, and 1997, as well as the RD-DD that compares 1996 to surrounding years. Percentages are the proportion of days in each year in which the joint F-test rejects the null hypothesis of covariate balance, using the same covariates from Table 2. The idea behind the test is that if covariates are balanced throughout the year, then the rejection rates of the joint F-test should equal the significance level of the test.

Table 4: Sample and Complier Characteristics

	Full sample		Review compliers		Off SSI compliers			
					Year 2		Year 16	
	Mean (median)	Prop.	Prop.	Ratio	Prop.	Ratio	Prop.	Ratio
Demographics								
Male	0.63	63%	67%	1.07	68%	1.09	64%	1.03
Age at entry (> median)	11.4 (13)	44%	47%	1.06	36%	0.82	-112%	-2.52
Single mother	0.51	51%	57%	1.13	59%	1.16	78%	1.53
No parents	0.16	16%	11%	0.73	7%	0.47	-19%	-1.21
Diagnosis								
Mental	0.73	73%	78%	1.06	82%	1.12	110%	1.50
Intellectual	0.49	49%	45%	0.92	47%	0.96	80%	1.63
Other	0.25	25%	33%	1.34	35%	1.41	31%	1.24
Nervous	0.05	5%	2%	0.38	2%	0.30	5%	0.84
Infectious	0.04	4%	3%	0.87	0%	0.02	-21%	-5.43
Endocrine	0.04	4%	6%	1.63	6%	1.71	10%	2.79
Sensory	0.03	3%	1%	0.38	1%	0.42	3%	0.80
None	0.02	2%	1%	0.36	2%	0.78	-6%	-2.93
Musculoskeletal	0.01	1%	1%	1.20	1%	1.08	1%	1.09
Congenital	0.01	1%	1%	0.49	0%	0.35	-2%	-2.06
Respiratory	0.01	1%	2%	2.01	2%	1.91	5%	4.52
Blood	0.01	1%	1%	0.59	0%	0.50	2%	2.19
Neoplasm	0.01	1%	1%	1.44	1%	0.94	-4%	-3.97
Pre-treatment outcomes (>median)								
Child's SSI payment	\$3,075 (\$2,403)	50%	42%	0.83	40%	0.81	28%	0.56
Child earnings	\$289 (\$0)	49%	55%	1.13	49%	1.00	-64%	-1.32
Parent earnings	\$9,592 (\$4,121)	56%	54%	0.97	57%	1.03	37%	0.66
Par. and sib. disability applications	0.16 (0)	44%	51%	1.14	48%	1.07	60%	1.35
Par. and sib. disability income	\$2,728 (\$0)	50%	54%	1.07	49%	0.98	55%	1.11
N	81,800		31,870 (est.)		12,763 (est.)		3,632 (est.)	

Notes: The full sample is SSI children with an 18th birthday within 37 weeks of the August 22, 1996, cutoff. Pre-treatment outcomes are annual averages taken over 1980 to 1996 for family outcomes and 1990 to 1996 for child outcomes. Compliers calculated using the methodology in Angrist and Pischke (2008). "Review compliers" are children who would receive an unfavorable age 18 medical review if in the treatment group but not if in the control group. "Off SSI Year 2 compliers" are children who would be off of SSI in year 2 if in the treatment group and on SSI in year 2 if in the control group, and analogously for "Off SSI Year 16 compliers." Since the proportions for compliers are estimated, they can be negative when the characteristic is very rare in the complier population.

Table 5: First Stage Estimates

	No covariates		With covariates	
	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.
Received age 18 medical review	0.863***	(0.0056)	0.864***	(0.0056)
Unfavorable age 18 medical review	0.383***	(0.0080)	0.386***	(0.0077)
Ever received unfavorable medical review	0.280***	(0.0096)	0.284***	(0.0092)
Average annual SSI enrollment	-0.109***	(0.0091)	-0.110***	(0.0085)
N	300,887		300,887	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Table presents parametric RD-DD estimates using a polynomial order of 2 based on covariate balance results. Results for different specifications presented in Appendix Table A.1 and Online Appendix Table B.6. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 (treatment year) and 1994, 1995, and 1997 (comparison years). Standard errors clustered at individual level shown in parentheses.

Table 6: IV Estimates of the Effect of Being Off of SSI

	IV estimate		Control	
	Pt. Est.	Std. Err.	mean	%Δ
SSI and DI income				
SSI income (avg. ann.)	-$7,704***	(305)	$4,055	-190%
Lifetime SSI income	-$130,620***	(7,390)	$122,064	-107%
DI income (avg. ann.)	-$734*	(388)	$688	-107%
Cumulative DI applications	-0.728***	(0.201)	0.375	-194%
Earnings and total income				
Earnings	$2,638*	(1,551)	$4,222	62%
Earnings thresholds				
> $0	0.240***	(0.0664)	0.406	59%
> $5K	0.196***	(0.0574)	0.214	92%
> $10K	0.131***	(0.0504)	0.149	88%
> $15K	0.113***	(0.0429)	0.103	110%
> $20K	0.0599*	(0.0361)	0.070	86%
> SSI	0.199***	(0.0554)	0.195	102%
Total income	-$6,052***	(1,657)	$9,041	-67%
Volatility of earnings and income				
Coefficient of variation				
Income	1.392***	(0.131)	0.458	304%
Earnings	-0.810***	(0.255)	1.253	-65%
Income cutoffs				
Years below 50% of poverty	10.53***	(1.08)	5.2	201%
Years below poverty line	0.294	(0.953)	12.8	2%
Parent and sibling income				
Parent earnings	$1,560	(2,259)	$11,974	13%
Parent and sibling disability applications	-0.0691**	(0.0321)	0.089	-78%
Parent and sibling disability income	-$11.68	(1,401)	$4,812	0%
Parent and sibling total income	$3,083	(2,310)	$16,780	18%
N	300,899			

Notes: *** p<0.01, ** p<0.05, * p<0.1. Table presents parametric RD-DD IV estimates of the effect of being off of the SSI program, using a polynomial order of 2 based on covariate balance results. Percent decrease from control group mean may exceed 100% because some control group members leave SSI after the age of 18; see Appendix Table A.4 for comparison with control group members who stay on SSI. Earning and income estimates are given as average annual measures unless otherwise specified. "Poverty" indicates the federal poverty level for a single person, around $12,000 in 2012. Results across different specifications presented in Appendix Tables A.2-A.3 and Online Appendix Tables B.4-B.5 and B.7-B.14. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 (treatment year) and 1994, 1995, and 1997 (comparison years). Standard errors clustered at individual level shown in parentheses.

Table 7: Earnings IV Estimates by Subgroup

Panel A: Demographics

Parent earnings			Family structure			Gender		
Quintile 1	-$886	($1,424)	Two parents	$3,418***	($1,150)	Female	$2,334***	($716)
Quintile 2	$285	($1,107)	Single or none	$1,848**	($723)	Male	$2,280**	($906)
Quintile 3	$3,035**	($1,188)						
Quintile 4	$5,794***	($1,786)						
Quintile 5	$3,631**	($1,588)						

Panel B: Diagnosis and severity

Diagnosis			Removal probability			Years on SSI		
Non-mental	$1,702	($1,411)	Quintile 1	$1,489	($3,482)	1-3 years	$1,514	($1,182)
Other mental	$2,215*	($1,158)	Quintile 2	-$964	($3,481)	4-5 years	$3,044***	($913)
Intellectual	$2,869***	($789)	Quintile 3	$2,538**	($1,222)	6-9 years	$1,836	($1,375)
			Quintile 4	$3,308***	($936)	10-13 years	$3,405	($2,817)
			Quintile 5	$2,656***	($933)	14-18 years	-$610	($4,488)

Panel C: Local economic conditions

County unemployment rate			County poverty rate		
Quintile 1	$2,588*	($1,534)	Quintile 1	$985	(1,845)
Quintile 2	$3,134*	($1,603)	Quintile 2	$3,143***	($1,210)
Quintile 3	$201	($1,290)	Quintile 3	$806	($1,449)
Quintile 4	$2,860**	($1,272)	Quintile 4	$3,120**	($1,395)
Quintile 5	$3,226***	($1,246)	Quintile 5	$3,090***	($1,123)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Table presents parametric RD-DD IV estimates by subgroup for earnings using a polynomial order of 2. "Removal probability" calculated by regressing removal on demographic and diagnosis covariates for the treatment group and then predicting removal for both the treatment and control groups. County unemployment rate is the 1997 unemployment rate. County poverty rate is the 1999 poverty rate. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 (treatment year) and 1994, 1995, and 1997 (comparison years). Standard errors clustered at individual level shown in parentheses.

Table 8: Certainty Equivalent Loss from SSI Removal

Floor or supplement	Certainty equivalent loss					% of loss from consump. volatility				
	Relative risk aversion (γ)					Relative risk aversion (γ)				
	0	1	2	3	4	0	1	2	3	4
Recipient income with consumption floor (c_{1t})										
\$1,000	\$5,940	\$6,942	\$7,758	\$7,891	\$7,774	0%	14%	23%	25%	24%
\$2,000	\$5,437	\$6,109	\$6,701	\$6,837	\$6,753	0%	11%	19%	20%	19%
\$3,000	\$4,879	\$5,316	\$5,748	\$5,859	\$5,782	0%	8%	15%	17%	16%
\$4,000	\$4,262	\$4,514	\$4,826	\$4,912	\$4,848	0%	6%	12%	13%	12%
\$5,000	\$3,603	\$3,702	\$3,917	\$3,983	\$3,942	0%	3%	8%	10%	9%
Recipient and parent income with consumption floor (c_{2t})										
\$1,000	\$5,273	\$5,674	\$6,294	\$6,473	\$6,427	0%	7%	16%	19%	18%
\$2,000	\$5,154	\$5,453	\$5,945	\$6,121	\$6,079	0%	5%	13%	16%	15%
\$3,000	\$4,982	\$5,179	\$5,545	\$5,689	\$5,652	0%	4%	10%	12%	12%
\$4,000	\$4,710	\$4,806	\$5,072	\$5,184	\$5,150	0%	2%	7%	9%	9%
\$5,000	\$4,374	\$4,370	\$4,534	\$4,598	\$4,560	0%	0%	4%	5%	4%
Recipient income with consumption supplement (c_{3t})										
\$1,000	\$6,394	\$7,387	\$8,166	\$8,292	\$8,186	0%	13%	22%	23%	22%
\$2,000	\$6,394	\$7,171	\$7,843	\$8,088	\$8,043	0%	11%	18%	21%	21%
\$3,000	\$6,394	\$7,034	\$7,621	\$7,919	\$7,940	0%	9%	16%	19%	19%
\$4,000	\$6,394	\$6,936	\$7,453	\$7,772	\$7,851	0%	8%	14%	18%	19%
\$5,000	\$6,394	\$6,860	\$7,322	\$7,643	\$7,765	0%	7%	13%	16%	18%

Notes: Table presents estimates of the certainty equivalent loss from SSI removal, where certainty equivalent is defined in equation (5). The certainty equivalent loss is the estimate of β in the RD-DD IV equation (3), where the dependent variable is the individual's certainty equivalent over the entire 16-year post-period and the endogenous regressor is average annual SSI enrollment over the same post-period. First panel gives the certainty equivalent loss where consumption is measured as the maximum of a consumption floor and total observed recipient income. Second panel gives the certainty equivalent loss where consumption is measured as the maximum of a consumption floor and total observed recipient income plus a fraction of parental income. Third panel gives the certainty equivalent loss where consumption is measured as the sum of recipient income and a consumption supplement. See Section 5 for details.

Table 9: Optimal SSI Benefit Calculations

	Relative risk aversion							
	0.5	1	1.5	2	2.5	3	3.5	4
No chance of SSI removal in adulthood								
Not accounting for consumption volatility								
Able comparison: HS graduate-dropout average	$3,500	$10,100	$12,200	$13,300	$14,000	$14,400	$14,700	$15,000
Able comparison: HS graduate	$6,300	$14,000	$16,600	$17,900	$18,700	$19,200	$19,600	$19,800
Accounting for consumption volatility								
Able comparison: HS graduate-dropout average	$3,100	$8,000	$9,100	$9,300	$9,300	$9,200	$9,100	$9,000
Able comparison: HS graduate	$5,500	$11,200	$12,300	$12,500	$12,400	$12,300	$12,100	$11,900
Medium chance of SSI removal in adulthood								
Accounting for consumption volatility								
Able comparison: HS graduate-dropout average	$3,100	$8,100	$9,200	$9,500	$9,500	$9,400	$9,300	$9,200
Able comparison: HS graduate	$5,500	$11,300	$12,500	$12,700	$12,700	$12,500	$12,300	$12,200
High chance of SSI removal in adulthood								
Accounting for consumption volatility								
Able comparison: HS graduate-dropout average	$3,300	$8,800	$10,300	$10,700	$10,900	$11,000	$10,900	$10,900
Able comparison: HS graduate	$5,800	$12,300	$13,900	$14,400	$14,600	$14,600	$14,500	$14,400

Notes: Table presents optimal benefit calculations using equation (6). Estimates of the earnings elasticity and the slope of disabled consumption with respect to the SSI benefit come from Table 6. Estimates of able-bodied consumption and the intercept for disabled consumption come from the March Supplement to the 1997 Current Population Survey. See Appendix for details.

Appendix: Social welfare

Diamond and Sheshinski (1995) model

Diamond and Sheshinski (1995) model the social planner's problem when the disutility of work is observable and unobservable. When the disutility of work θ is continuous (with distribution $F(\theta)$) and observable, the social planner sets consumption for workers (c_a), consumption for those on disability insurance (c_d), and the disutility cutoff θ^* above which people are allowed onto disability insurance. Those with disutility below θ^* become workers, and those with disutility above θ^* become non-workers. Workers produce 1 and non-workers produce 0. The social planner maximizes the utility of workers and non-workers subject to a resource constraint R:

$$\max_{c_a, c_d, \theta^*} \int_0^{\theta^*} [u(c_a) - \theta] dF(\theta) + \int_{\theta^*}^{\infty} v(c_d) dF(\theta) \tag{7}$$

$$\text{s.t.} \quad \int_0^{\theta^*} (c_a - 1) dF + \int_{\theta^*}^{\infty} (c_d) dF = R \tag{8}$$

where $u(c_a)$ is the utility from consumption for workers and $v(c_d)$ is the utility from consumption for non-workers. Solving this maximization problem yields two key conditions that characterize the optimum:

$$u'(c_a^*) = v'(c_d^*) \tag{9}$$

$$u(c_a^*) - \theta^* - v(c_d^*) = u'(c_a^*)(c_a^* - 1 - c_d^*) \tag{10}$$

Condition (9) says that consumption levels are set to equalize the marginal utility of consumption for workers and non-workers. When the disutility of work is perfectly observable, there is perfect consumption smoothing across the able and disabled states. In condition (10), the left-hand-side is the utility loss from work for someone with disutility of work θ^*, and the right hand side is the marginal utility of consumption from work. The social planner awards disability insurance to people for whom the social cost of work (in the form of private disutility) outweighs the social benefit of work (in the form of additional consumption).

When the disutility of work is imperfectly observed, the cutoff θ^* is no longer set by the social planner but is endogenously determined by agents who maximize their utility taking c_a and c_d as given. A higher level of c_d, in addition to smoothing consumption as before, now induces people with a marginal disutility of work to apply for disability insurance and stop working if allowed (i.e., θ^* falls). This tightens the government's resource constraint by lowering the output from work and increasing the expenditures of the disability program. The phenomenon of an endogenous θ^* is commonly referred to as moral hazard. When the disutility of work is imperfectly observed, the social planner does not perfectly smooth consumption across the able and disabled states because of the moral hazard costs of doing so.

Optimal benefit calculations

I use my IV estimates for earnings and income to calculate optimal benefit levels under various assumptions by adapting the formula for optimal social insurance derived by Baily (1978), applied by Gruber (1997), and generalized by Chetty (2006):

$$\epsilon_{e,b} = \gamma \frac{c_{able} - c_{dis}}{c_{able}}(b^*) \tag{11}$$

where $\epsilon_{e,b}$ is the elasticity of earnings to the SSI benefit; $\gamma = \frac{cu''(c)}{u'(c)}$ is the coefficient of relative risk aversion; c_{able} is consumption in the able-bodied state; and c_{dis} is consumption in the disabled state, including the SSI benefit. The term $\frac{c_{able}-c_{dis}}{c_{able}}$ is a function of the benefit level b. Roughly, this formula says that at the optimal benefit level b^* the cost to the government of providing SSI benefits—in the form of lost tax revenue from the earnings decline—equals the change in consumption between the able and disabled states, appropriately valued based on the level of risk aversion. Moral hazard enters the formula through the government's budget constraint. Since the program is funded out of tax revenue, the elasticity of (taxable) earnings with respect to benefit levels, on the left-hand-side of equation (11), represents the cost to the government of funding the disability insurance program. The right-hand-side of the equation represents the benefits of the program in closing the gap in consumption between the able-bodied and disabled states of the world.

The derivation of this formula involves a number of assumptions relevant to this context. First, this version of the optimal benefits formula assumes that third-order terms in the utility function are negligible (i.e., $u''' = 0$), which amounts to an assumption of no precautionary savings motive. Second, the derivation of this formula assumes that utility is not state-dependent, meaning that the marginal utility of consumption does not change across the able-bodied and disabled states. For a population with near-poverty consumption levels, it may be reasonable to assume that the marginal utility of consumption remains high whether or not the individual is disabled, but there is little to no empirical evidence on whether this assumption holds. Third, this formula holds for marginal changes in benefit levels, so it should be applied with caution to the non-marginal change of SSI removal. Finally, I apply equation (11) to find the globally optimal benefit level, which requires concavity of the Social Planner's problem.

I calculate $\epsilon_{e,b}$ from the IV estimates in Table 6. Since the change in the benefit level is large, the point elasticity is highly sensitive to the choice of starting point. To avoid this problem, I calculate the arc elasticity for earnings, which uses the midpoint as the baseline:

$$\hat{\epsilon_{e,b}} = \frac{\frac{e_{no}-e_{SSI}}{(e_{no}+e_{SSI})/2}}{\frac{b_{no}-b_{SSI}}{(b_{no}+b_{SSI})/2}}$$

where e_{no} is earnings when not on SSI, e_{SSI} is earnings when on SSI, and analogously for the SSI benefit amounts b_{no} and b_{SSI}. I calculate an earnings elasticity of 0.255. The elasticity estimate is similar if I follow Meyer and Mok (2013) and use participation elasticities instead: 0.234 for the \$0 threshold, and 0.325 for the \$5,000 threshold.

Unlike Gruber (1997), who applies equation (11) to optimal unemployment insurance, I cannot calculate $\frac{c_{able}-c_{dis}}{c_{able}}(b^*)$ as a single estimate. In Gruber's case, the change in benefit levels corresponds to a change in unemployment status; therefore he is able to estimate the effect of the benefit level on the drop in consumption between the employed and unemployed states. In my case, SSI removal does not correspond to a change in disability status; I estimate the effect of the benefit decrease on consumption for those in the disabled state only. Therefore I must estimate $c_{able}(b)$ and $c_{dis}(b)$ separately.

I estimate consumption in the able-bodied state (c_{able}) from personal income in the March Supplement to the 1997 Current Population Survey (CPS), based on disability status, education level, and age. For simplicity, I assume that c_{able} does not vary with the benefit level; this may not be true if the availability of disability benefits affects savings rates, but it is probably a reasonable approximation for a low-income population.

I estimate c_{dis} from a combination of my data and the CPS. Since my quasi-experiment provides an exogenous change in benefit levels, I use my data to estimate how c_{dis} varies with the benefit level. From Table 6, reducing the benefit amount from \$7,700 to \$0 decreases total income from \$9,000 to \$3,800, which, assuming a linear slope, means that a \$1 increase in benefits increases consumption by \$0.675. To improve consistency between c_{able} and c_{dis} in what type of income is observed, I estimate the "intercept" (i.e., consumption in the disabled state when the benefit level is zero) from the same CPS source and get approximately \$7,300. Therefore the relationship between consumption in the disabled state and the benefit level is

$$c_{dis} = 0.675b + 7300.$$

I solve for the globally optimal benefit level by plugging the estimates of $\epsilon_{e,b}$, c_{dis}, and c_{able} into equation (11).[30] Table 9 presents the calculation of the globally optimal SSI benefit for different values of relative risk aversion and different education levels of the able-bodied comparison. Choosing the appropriate able-bodied comparison income involves important considerations. According to my tabulations from the National Survey on SSI Children and Families (SSA (2012)), just over half of former SSI child applicants and recipients have a high school diploma or GED certificate, while just under half are high school dropouts. The most obvious able-bodied comparison income for this group is therefore an average between high school graduate and

[30] To estimate local optimality, I compare the magnitude of each side of equation (11) at the current benefit level. I find that the SSI benefit is locally too small for $\gamma \geq 1$ and locally too large for $\gamma < 1$.

high school dropout income. But if disability *restricts* educational achievement, then the more appropriate comparison may be a high school graduate income, since SSI youth would have achieved this level of income in the able-bodied state of the world. A deeper consideration still is whether SSI is solely insurance against disability, or if it is also insurance against poverty behind the veil of ignorance. If the latter, then the argument for using a higher able-bodied comparison income is stronger. The results are given in the first two rows of Table 9.

Given the finding that SSI substantially decreases income volatility, I do a mechanical calculation to adjust for the income volatility effects of the SSI benefit. The goal is to find the zero- or low-volatility SSI benefit level that gives the same utility as the earnings-equivalent optimal benefit calculated in the first set of columns. I use a quadratic utility function for this calculation because it makes the mean-variance tradeoff explicit and therefore makes calculations straightforward:

$$\begin{aligned} U(c) &= c - \frac{k}{2}c^2,\ k < \frac{1}{c} \\ E[U(c)] &= E(c) - \frac{k}{2}E(c^2) \\ E[U(c)] &= E(c) - \frac{k}{2}[Var(c) + (E(c))^2] \end{aligned}$$

The expected utility of the SSI benefit is

$$E[U(b)] = \mu_b - \frac{k_b}{2}[\sigma_b^2 + \mu_b^2]$$

where μ_b is the mean SSI benefit level, σ_b^2 is the variance of SSI income, and $k_b = \frac{\gamma}{(1+\gamma)\mu_b}$. The expected utility of earnings equivalent benefit level e is

$$E[U(e)] = \mu_e - \frac{k_e}{2}[\sigma_e^2 + \mu_e^2]$$

where μ_e is the earnings-equivalent optimal benefit from above, σ_e^2 is the variance of earnings, and $k_e = \frac{\gamma}{(1+\gamma)\mu_e}$.

I compute the μ_b that makes the utility from the reliable SSI benefit equal to the utility from volatile earnings:

$$\begin{aligned} \mu_b - \frac{k_b}{2}[\sigma_b^2 + \mu_b^2] &= \mu_e - \frac{k_e}{2}[\sigma_e^2 + \mu_e^2] \\ \implies \mu_b &= \frac{[1 - \frac{\gamma}{2(1+\gamma)}(V_e^2 + 1)]}{[1 - \frac{\gamma}{2(1+\gamma)}(V_b^2 + 1)]}\mu_e \end{aligned} \tag{12}$$

where V_e is the coefficient of variation for earnings ($= \frac{\sigma_e}{\mu_e}$) for those removed, from Appendix Table A.3. I calculate the volatility-adjusted optimal benefit level μ_b by plugging in the earnings-equivalent optimal benefit calculated above for μ_e.

I first consider the case in which SSI income has no volatility. Equation (12) reduces to

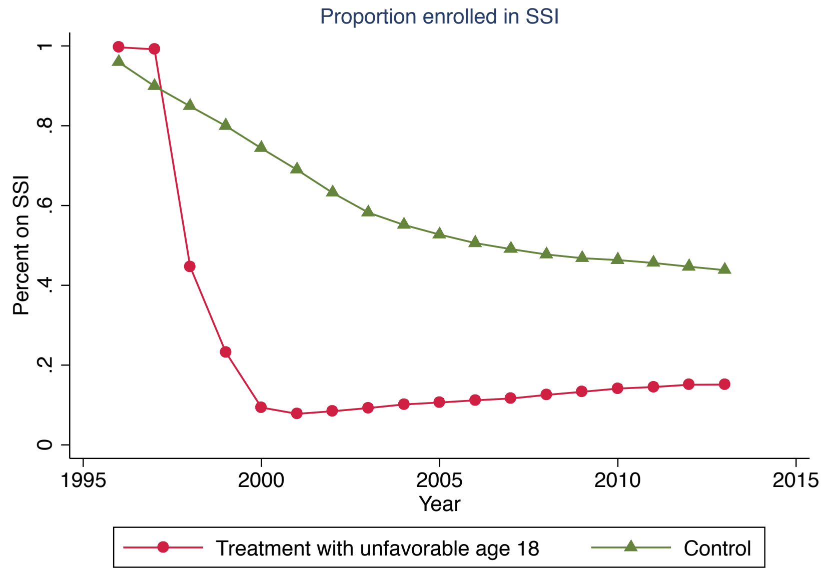
$$b = \frac{[1 - \frac{\gamma}{2(1+\gamma)}(V_e^2 + 1)]}{1 - \frac{\gamma}{2(1+\gamma)}} \mu_e$$

since $\mu_b = b$ and $V_b = 0$. The results are given in the second set of rows in Table 9.

I next consider the case in which SSI income has some volatility ($V_b > 0$) because there is a risk of removal from the program in adulthood. Table 9 gives results for a medium chance of removal (1% annually) and a high chance of removal (3% annually). I use equation (12) to calculate the volatility-adjusted benefit in these cases. Introducing the possibility of removal from SSI in adulthood increases the optimal benefit level since SSI has lower consumption volatility benefits. As shown in the final rows of Table 9, introducing the possibility of removal from SSI in adulthood works to increase the optimal benefit level since SSI has lower consumption stabilization benefits.

Appendix Figures and Tables

Figure A.1: Sources of Attenuation in the First Stage

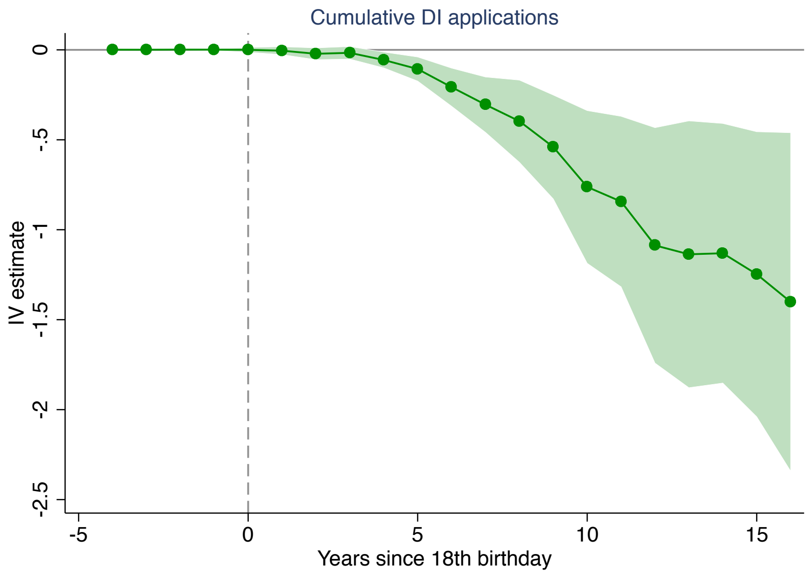


Notes: "Control" series plots the proportion of SSI children with an 18th birthday before the cutoff (less likely to get an age 18 review) who are on SSI in a given year. "Treatment with unfavorable age 18" series plots the proportion of SSI children with an 18th birthday after the cutoff and an unfavorable age 18 medical review who are on SSI in a given year.

Figure A.2: IV Estimates of the Effect of One Additional Year Off of SSI on Cumulative Earnings

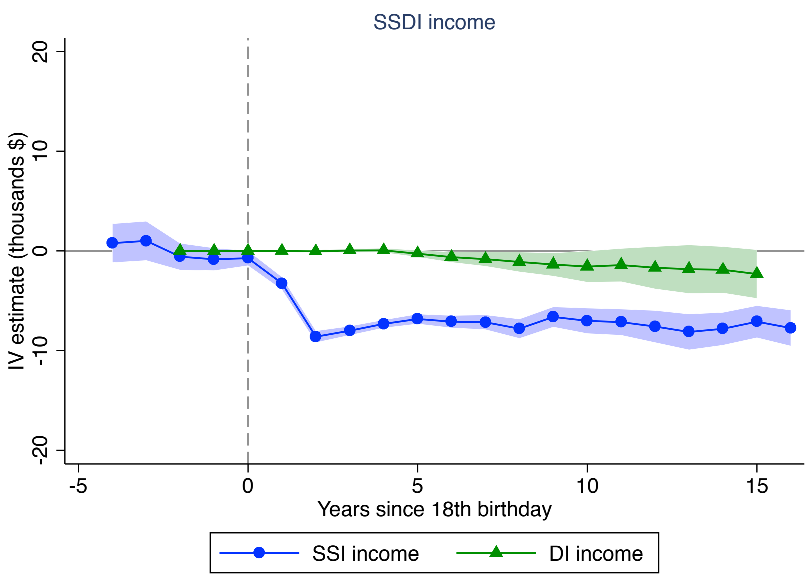
Notes: Figure plots the parametric IV RD-DD estimate of the effect of being off SSI *for one additional year* on cumulative earnings in each year, using a polynomial order of 2 with covariates. The endogenous regressor is cumulative years off of SSI up to the given year and the dependent variable is cumulative earnings up to that year. Shaded region is 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997. Standard errors clustered at individual level.

Figure A.3: IV Estimates of the Effect of SSI Removal on DI Applications



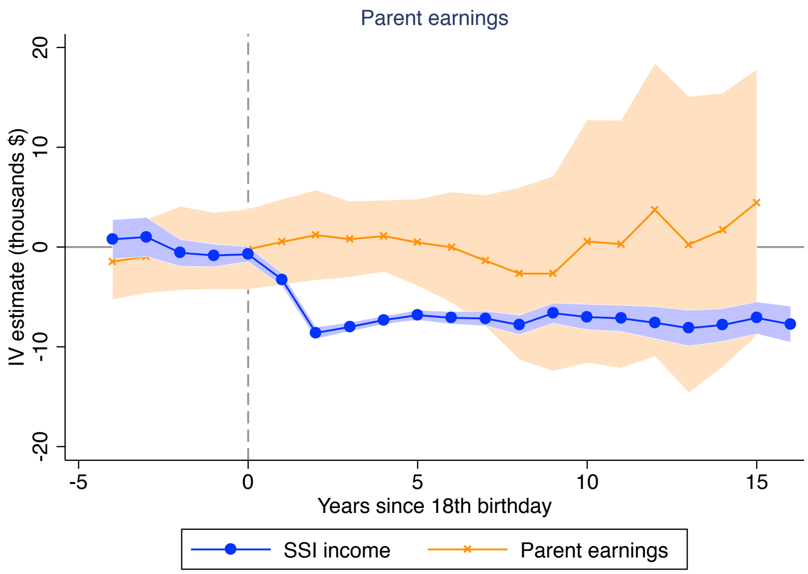
Notes: Figure plots the parametric IV RD-DD estimate of the effect of being off SSI on the cumulative number of the youth's Social Security Disability Insurance applications in each year, using a polynomial order of 2 with covariates. Shaded region is 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997. Standard errors clustered at individual level.

Figure A.4: IV Estimates of the Effect of SSI Removal on DI Income



Notes: Figure plots the parametric IV RD-DD estimate of the effect of being off SSI on SSI income (circles) and Social Security Disability Insurance income (triangles) in each year, using a polynomial order of 2 with covariates. Shaded region is 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997. Standard errors clustered at individual level.

Figure A.5: IV Estimates of the Effect of SSI Removal on Parental Earnings



Notes: Figure plots the parametric IV RD-DD estimate of the effect of being off SSI on SSI income (circles) and parents' earned income (X's) in each year, using a polynomial order of 2 with covariates. Shaded region is 95% confidence interval. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 and in 1994, 1995, and 1997. Standard errors clustered at individual level.

Table A.1: RD-DD First Stage Estimates

	Poly. order		Linear		Quadratic		Cubic		Quartic	
	AIC	Bins	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.
No covariates										
Received age 18 review	1	4	0.902***	(0.0037)	0.863***	(0.0056)	0.830***	(0.0076)	0.833***	(0.0094)
Unfavorable age 18	1	1	0.405***	(0.0051)	0.383***	(0.0080)	0.378***	(0.0109)	0.381***	(0.0132)
Unfavorable review ever	1	1	0.299***	(0.0062)	0.280***	(0.0096)	0.278***	(0.0130)	0.283***	(0.0157)
Avg. ann. SSI enrollment	1	1	-0.111***	(0.0060)	-0.109***	(0.0091)	-0.119***	(0.0123)	-0.121***	(0.0149)
With covariates										
Received age 18 review	1	4	0.903***	(0.0037)	0.864***	(0.0056)	0.832***	(0.0076)	0.836***	(0.0093)
Unfavorable age 18	1	1	0.409***	(0.0049)	0.386***	(0.0077)	0.380***	(0.0106)	0.380***	(0.0128)
Unfavorable review ever	1	1	0.304***	(0.0059)	0.284***	(0.0092)	0.279***	(0.0125)	0.282***	(0.0151)
Avg. ann. SSI enrollment	1	1	-0.115***	(0.0055)	-0.110***	(0.0085)	-0.116***	(0.0114)	-0.112***	(0.0138)
N			300,887		300,887		300,887		300,887	

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Table presents parametric RD-DD estimates for the first stage, with and without covariates. Covariates include sex, diagnosis category, age at entry, pre-treatment parental earnings, and state. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 (treatment year) and 1994, 1995, and 1997 (comparison years). Standard errors clustered at individual level shown in parentheses. "AIC" indicates the Akaike Information Criterion goodness-of-fit method for selecting the optimal polynomial order; "Bins" indicates the bin selection method outlined in Lee and Lemieux (2010).

Table A.2: IV Estimates for Youth Outcomes: RD-DD without Covariates

	Poly. order		Linear		Quadratic		Cubic		Quartic		Mean
	AIC	LL	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	
SSI and DI income											
SSI income (annual)	1	1	-7,828***	(210)	-7,795***	(325)	-8,000***	(401)	-8,270***	(488)	$4,055
SSI payment (life)	3	2	-118,694***	(6,581)	-127,561***	(10,075)	-142,463***	(12,166)	-144,303***	(14,630)	$122,064
DI income (annual)	1	1	-360	(251)	-742*	(398)	-386	(489)	-91.3	(588)	$688
DI applications (cum.)	1	1	-0.484***	(0.127)	-0.782***	(0.211)	-0.341	(0.244)	-0.400	(0.298)	0.375
Earnings and total income											
Earnings	1	1	3,894***	(997)	2,534	(1,610)	2,927	(1,968)	1,664	(2,426)	$4,222
Earnings thresholds											
> $0	1	1	0.281***	(0.0439)	0.220***	(0.0695)	0.248***	(0.0845)	0.266***	(0.101)	0.406
> $5K	1	1	0.242***	(0.0371)	0.183***	(0.0597)	0.222***	(0.0722)	0.205**	(0.0874)	0.214
> $10K	1	1	0.173***	(0.0325)	0.122**	(0.0523)	0.146**	(0.0636)	0.114	(0.0777)	0.149
> $15K	1	1	0.125***	(0.0280)	0.110**	(0.0442)	0.134**	(0.0539)	0.080	(0.0665)	0.103
> $20K	1	1	0.075***	(0.0235)	0.060	(0.0373)	0.068	(0.0456)	0.0295	(0.0560)	0.070
> SSI	1	1	0.218***	(0.0362)	0.189***	(0.0574)	0.217***	(0.0698)	0.197**	(0.0846)	0.195
Total income	1	1	-4,553***	(1,067)	-6,260***	(1,733)	-5,666***	(2,113)	-6,953***	(2,607)	$9,041
Volatility of income											
Coefficient of variation											
Income (de-trended)	1	1	1.446***	(0.0879)	1.404***	(0.135)	1.294***	(0.160)	1.371***	(0.197)	0.458
Earnings (de-trended)	1	1	-0.769***	(0.170)	-0.844***	(0.265)	-0.382	(0.314)	-0.364	(0.378)	1.253
Income (raw)	1	1	1.138***	(0.0898)	1.183***	(0.139)	1.167***	(0.170)	1.219***	(0.205)	0.682
Earnings (raw)	1	1	-0.561***	(0.165)	-0.624**	(0.257)	-0.123	(0.314)	0.0299	(0.379)	1.363
Income cutoffs											
Years below 50% FPL	1	1	10.26***	(0.713)	10.81***	(1.130)	10.36***	(1.377)	10.70***	(1.665)	5.2
Years below FPL	1	1	-0.723	(0.613)	0.473	(0.996)	-0.646	(1.187)	-0.485	(1.433)	12.8
N			300,899		300,899		300,899		300,899		

Notes: *** p<0.01, ** p<0.05, * p<0.1. Table presents parametric RD-DD IV estimates without covariates of the effect of being off SSI in the 16-year post-period on youth outcomes. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 (treatment year) and 1994, 1995, and 1997 (comparison years). Standard errors clustered at individual level shown in parentheses. "AIC" indicates the Akaike Information Criterion goodness-of-fit method for selecting the optimal polynomial order; "Bins" indicates the bin selection method outlined in Lee and Lemieux (2010). "FPL" indicates the federal poverty level for a single person.

Table A.3: IV Estimates for Youth Outcomes: RD-DD with Covariates

	Poly. order		Linear		Quadratic		Cubic		Quartic		Mean
	AIC	LL	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	Pt. Est.	Std. Err.	
SSI and DI income											
SSI income (annual)	1	1	-7,827***	(193)	-7,704***	(305)	-8,083***	(395)	-8,199***	(503)	$4,055
SSI payment (life)	3	2	-132,150***	(4,674)	-130,620***	(7,390)	-140,904***	(9,492)	-141,197***	(11,993)	$122,064
DI income (annual)	1	1	-338	(241)	-734*	(388)	-425	(497)	-164	(631)	$688
DI applications (cum.)	1	1	-0.423***	(0.120)	-0.728***	(0.201)	-0.343	(0.247)	-0.447	(0.319)	0.375
Earnings and total income											
Earnings	1	1	4,124***	(941)	2,638*	(1,551)	2,446	(1,994)	642	(2,609)	$4,222
Earnings thresholds											
> $0	1	1	0.305***	(0.0413)	0.240***	(0.0664)	0.253***	(0.0845)	0.251**	(0.107)	0.406
> $5K	1	1	0.255***	(0.0352)	0.196***	(0.0574)	0.215***	(0.0731)	0.187**	(0.0935)	0.214
> $10K	1	1	0.182***	(0.0308)	0.131***	(0.0504)	0.138**	(0.0644)	0.094	(0.0833)	0.149
> $15K	1	1	0.129***	(0.0266)	0.113***	(0.0429)	0.123**	(0.0547)	0.055	(0.0715)	0.103
> $20K	1	1	0.0774***	(0.0223)	0.060*	(0.0361)	0.055	(0.0463)	0.004	(0.0604)	0.070
> SSI	1	1	0.231***	(0.0343)	0.199***	(0.0554)	0.210***	(0.0707)	0.174*	(0.0907)	0.195
Total income	1	1	-4,299***	(1,001)	-6,052***	(1,657)	-6,280***	(2,135)	-7,993***	(2,794)	$9,041
Volatility of income											
Coefficient of variation											
Income (de-trended)	1	1	1.438***	(0.0841)	1.392***	(0.131)	1.301***	(0.164)	1.411***	(0.213)	0.458
Earnings (de-trended)	1	1	-0.725***	(0.161)	-0.810***	(0.255)	-0.439	(0.318)	-0.417	(0.404)	1.253
Income (raw)	1	1	1.116***	(0.0864)	1.164***	(0.137)	1.156***	(0.174)	1.214***	(0.221)	0.682
Earnings (raw)	1	1	-0.512***	(0.157)	-0.587**	(0.248)	-0.156	(0.319)	0.0241	(0.406)	1.363
Income cutoffs											
Years below 50% FPL	1	1	9.942***	(0.672)	10.53***	(1.084)	10.49***	(1.388)	10.92***	(1.771)	5.2
Years below FPL	1	1	-0.820	(0.579)	0.294	(0.953)	-0.359	(1.202)	-0.0921	(1.534)	12.8
N			300,899		300,899		300,899		300,899		

Notes: *** p<0.01, ** p<0.05, * p<0.1. Table presents parametric RD-DD IV estimates with covariates of the effect of being off SSI in the 16-year post-period on youth outcomes. Covariates include sex, diagnosis category, age at entry, pre-treatment parental earnings, and state. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 (treatment year) and 1994, 1995, and 1997 (comparison years). Standard errors clustered at individual level shown in parentheses. "AIC" indicates the Akaike Information Criterion goodness-of-fit method for selecting the optimal polynomial order; "Bins" indicates the bin selection method outlined in Lee and Lemieux (2010). "FPL" indicates the federal poverty level for a single person.

Table A.4: IV Estimates of the Effect of Being Off of SSI, Relative to Control Group On SSI

	IV estimate		Full control group		Control on SSI	
	Pt. Est.	Std. Err.	Mean	%Δ	Mean	%Δ
SSI income	-$7,704***	(305)	$4,055	-190%	$6,835	-113%
DI income	-$734*	(388)	$688	-107%	$618	-119%
Earnings	$2,638*	(1,551)	$4,222	62%	$1,386	190%
Earnings thresholds						
> $0	0.240***	(0.0664)	0.406	59%	0.287	83%
> $5K	0.196***	(0.0574)	0.214	92%	0.090	217%
> $10K	0.131***	(0.0504)	0.149	88%	0.047	281%
> $15K	0.113***	(0.0429)	0.103	110%	0.022	506%
> $20K	0.0599*	(0.0361)	0.070	86%	0.010	600%
> SSI	0.199***	(0.0554)	0.195	102%	0.077	259%
Total income	-$6,052***	(1,657)	$9,041	-67%	$8,839	-68%
N	300,899					

Notes: *** p<0.01, ** p<0.05, * p<0.1. Table presents parametric RD-DD IV estimates of the effect of being off of the SSI program, using a polynomial order of 2 based on covariate balance results. Estimates are given as average annual measures. "Full control group" gives the mean for all sample members with an 18th birthday before August 22, 1996; "control on SSI" gives the mean for those control group members who stay on SSI in a given year, averaged over all 16 post-treatment years. Sample is SSI children with an 18th birthday within 37 weeks of the August 22 cutoff in 1996 (treatment year) and 1994, 1995, and 1997 (comparison years). Standard errors clustered at individual level shown in parentheses.

Table A.5: Descriptive Survey Statistics for Former SSI Children Ages 22 to 24 Years

	Full sample			Living apart from parents		
	All	Still On	Off SSI	All	Still On	Off SSI
Living arrangements						
Lives with parents	54%	60%	45%			
Lives with own child	18%	15%	24%			
Health insurance						
Any	77%	96%	52%			
Medicaid	63%	90%	23%			
Financial assistance						
Non-SSI cash welfare	10%	11%	8%	15%	19%	10%
Food Stamps	27%	26%	28%	39%	44%	34%
Any public assistance	30%	29%	31%	43%	50%	35%
Transfers from family outside HH	9%	6%	16%	19%	13%	27%
N	989	616	373	421	238	183

Source: Author's calculations from National Survey of SSI Children and Families. Notes: Table reports proportions for all individuals in the NSCF between the ages of 22 and 24 years who were on SSI as children, and for the subsample of these individuals who live apart from parents. "Still On" means the former SSI child is still on SSI at the time of the survey. "Off SSI" means the fomer SSI child is no longer on SSI at the time of the survey.