

DOES WELFARE PREVENT CRIME? THE CRIMINAL JUSTICE OUTCOMES OF YOUTH REMOVED FROM SSI*

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We estimate the effect of losing Supplemental Security Income (SSI) benefits at age 18 on criminal justice and employment outcomes over the next two decades. To estimate this effect, we use a regression discontinuity design in the likelihood of being reviewed for SSI eligibility at age 18 created by the 1996 welfare reform law. We evaluate this natural experiment with Social Security Administration data linked to records from the Criminal Justice Administrative Records System. We find that SSI removal increases the number of criminal charges by a statistically significant 20% over the next two decades. The increase in charges is concentrated in offenses for which income generation is a primary motivation (60% increase), especially theft, burglary, fraud/forgery, and prostitution. The effect of SSI removal on criminal justice involvement persists more than two decades later, even as the effect of removal on contemporaneous SSI receipt diminishes. In response to SSI removal, youth are twice as likely to be charged with an illicit income-generating offense than they are to maintain steady employment at \$15,000/year in the labor market. As a result of these charges, the annual likelihood of incarceration increases by a statistically significant 60% in the two decades following SSI removal. The costs to taxpayers of enforcement and incarceration from SSI removal

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are so high that they nearly eliminate the savings to taxpayers from reduced SSI benefits. *JEL Codes:* I38, J14, K42.

I. INTRODUCTION

What are the long-term effects of providing welfare benefits to disadvantaged youth on employment and criminal justice involvement? On the one hand, cash assistance could provide a basic level of income and well-being to youth who face barriers to employment and thereby reduce their criminal justice involvement. On the other hand, welfare benefits could discourage work at a formative time and discourage the development of skills, good habits, or attachment to the labor force, potentially even increasing criminal justice involvement.

In the United States, this question is particularly relevant to the Supplemental Security Income program, which provides cash assistance to the families of 1.2 million low-income children with disabilities and 5.2 million low-income adults with disabilities ([Social Security Administration 2020](#)). These children are disadvantaged in terms of both low income and disability. When children who receive SSI turn 18, they are reevaluated to determine whether their medical condition meets the eligibility criteria for adult SSI. About 40% of children who receive SSI just before age 18 are removed from SSI as a result of this reevaluation. Relative to those who stay on SSI in adulthood, these children lose nearly \$10,000 annually in SSI benefits in adulthood.

In this article, we study the effects of removing youth from SSI at age 18 on their employment and criminal justice outcomes over the next two decades. Building on [Deshpande \(2016\)](#), we use a regression discontinuity design in which children who had an 18th birthday after the date of welfare reform enactment—August 22, 1996—had their eligibility for the adult SSI program reviewed, while nearly all of those with an 18th birthday before that date were allowed onto the adult program without review. We merge Social Security Administration (SSA) SSI records to administrative earnings records from SSA and administrative criminal records from the Criminal Justice Administrative Records System (CJARS) created by [Finlay and Mueller-Smith \(2021a\)](#).¹ These data linkages allow us to estimate, for the first time, the effect of

1. See [Finlay, Mueller-Smith, and Papp \(2022\)](#) for more information about this novel data resource.

SSI on joint employment and criminal justice outcomes and follow the outcomes of youth for two decades after they are removed from SSI. Based on this data linkage, nearly 40% of recent SSI cohorts are involved in the criminal justice system in adulthood, making criminal justice involvement a high-powered outcome for this population.²

We find that SSI removal at age 18 in 1996 increases the number of criminal charges by a statistically significant 20% (2.04 to 2.50 charges) over the following two decades. The increase in criminal charges is concentrated in activities for which income generation is a primary motivation. The number of income-generating charges (which we define as burglary, theft, fraud/forgery, robbery, drug distribution, and prostitution) increases by 60%, compared with just 10% for charges not associated with income generation. As a result of these charges, the annual likelihood of incarceration (i.e., likelihood in each year from ages 18 to 38, averaged over the 21 years) increases from 4.7 to 7.6 percentage points, a statistically significant 60%, in the two decades after SSI removal.

Men and women respond differently to SSI removal. For men, the largest and most precise increase is for theft charges. We also estimate less precise increases in burglary and drug distribution charges for men. There are also some increases in non-income-generating charges related to income-generating activities—for example, criminal trespass (related to burglary) and resisting arrest or other obstruction (mechanically related to another charge). The annual likelihood of incarceration for men increases from 7.2 to 10.8 percentage points (50%).

The effect of SSI removal on criminal charges is even larger for women than for men, and for women is concentrated almost exclusively in activities associated with income generation. Like men, the largest increase for women is in theft charges. But women also have large and precise increases in fraud and forgery charges and prostitution charges. The estimate for fraud and forgery charges—most commonly bad checks, retail fraud, and identity theft—is close to zero in the full sample but large

2. Previous papers provide descriptive statistics of outcomes for individuals who received SSI as children. [Hemmeter, Kauff, and Wittenburg \(2009\)](#) use the National Survey of SSI Children and Families (NSCF) linked to SSA administrative data to measure health care needs, education and employment status, and criminal justice involvement before and after the age 18 redetermination. [Hemmeter \(2011\)](#) uses the NSCF to compare health care access for individuals who stay on SSI versus leave SSI after age 18.

and precise for women. The annual likelihood of incarceration for women increases from 0.7 to 2.4 percentage points (220%). All of these estimates are statistically different from zero.

Overall, the effects on criminal charges and incarceration persist over the two decades following removal in 1996, even as the effect of SSI removal at age 18 on actual SSI receipt declines. The effect of SSI removal on the likelihood of a criminal charge associated with income generation increases from 3.6 percentage points in 1997–2001 to 5.7 percentage points during the Great Recession in 2007–2012. Similarly, the effect on the annual likelihood of incarceration remains around 3 percentage points over this time period. Yet the effect of SSI removal at age 18 on the likelihood of receiving SSI decreases from 45 percentage points in 1997–2001 to just 14 percentage points in 2007–2012.³ These estimates suggest that contemporaneous SSI income during adulthood is not the primary driver of criminal justice involvement. Instead, it is more likely the loss of SSI income in early adulthood that permanently increases the propensity to commit crimes throughout adulthood.

We investigate heterogeneity in the effects by family structure, family income, gender, race, and type of disability. We see large responses across all subgroups in the likelihood of being charged with an income-generating crime. However, the incarceration effects of SSI removal are disproportionately higher for the most disadvantaged groups, especially Black youth and youth with low parent earnings. SSI removal exacerbates existing inequality in incarceration rates.

How do our estimates on criminal justice involvement square with the finding from [Deshpande \(2016\)](#) that SSI removal increases work activity? We first replicate the earnings and employment results from [Deshpande \(2016\)](#) and then investigate the relationship between legal employment and illicit income generation. Youth who are removed from SSI at age 18 are twice as likely to be charged with an illicit activity associated with income generation (9.3 percentage points) than they are to maintain steady employment at or above minimum-wage levels (4.4 percentage points).⁴ Over time, youth specialize in either legal employment

3. This decline occurs mostly because individuals in the control group, who did not receive a medical review at age 18, are removed from SSI in adulthood for medical and nonmedical reasons.

4. We observe earnings, not wages or hours, so references to minimum-wage levels are intended in an annualized sense.

or criminal activity to generate income: although some youth with steady formal employment have one criminal charge in adulthood, almost none of the steadily employed youth have multiple criminal charges.

We compare the cost savings to taxpayers from terminating SSI benefits to the costs to taxpayers from higher criminal activity and incarceration rates. Each individual removed from SSI in 1996 saves the government \$37,700 in SSI spending and \$8,400 in Medicaid spending over the next two decades, plus an additional \$3,000 in tax revenue from higher earnings. Each removal creates \$10,800 in police and court costs and another \$30,200 in incarceration costs, meaning that the administrative costs of crime alone almost eliminate the cost savings from removing these young adults from SSI. We calculate a marginal value of public funds (MVPF) of SSI ([Hendren and Sprung-Keyser 2020](#)), defined as the value of the program to recipients for every dollar spent by the government, of 5.6. Including victim costs (\$85,600 per removal using conservative assumptions) brings the MVPF to 16.1.

Taken together, these results indicate that the income effect from losing SSI eligibility in young adulthood in 1996 is not solely—or even mostly—limited to an increase in formal employment. Although some youth who are removed from SSI at age 18 respond by working at minimum-wage levels, a much larger fraction respond by engaging in illicit activities to replace the lost SSI income, behavior that generates potentially wide-reaching negative externalities on society at large. These effects on illicit income-generating activities substantially increase the likelihood of incarceration, including and especially for women.⁵

What explains the persistence of the effects of SSI removal on criminal justice involvement? One possibility is that the Great Recession, which occurred about one decade after the reform, drew individuals who had previously responded with formal employment into criminal activity. Another possibility is path dependence: those who initially responded to SSI removal with criminal activity continued doing it because they developed criminal capital or could not find formal employment due to their criminal record. We find suggestive evidence for both stories. Decomposing

5. A relevant question is to what extent these effects from SSI removal in 1996 speak to the effects of removing youth from SSI today, given certain policy reforms since 1996 aimed at improving the outcomes of children leaving SSI at age 18 (e.g., increased spending on preemployment transition services for youth with disabilities).

the effect of SSI removal on income-generating charges during the Great Recession and beyond, we find that about one-third of the effect is driven by individuals who were previously charged with an income-generating crime, another third by those previously charged with a non-income-generating crime, and the final one-third by those with no criminal charge who previously worked in the formal labor market.⁶

We build on a substantial literature on the effect of public assistance on criminal justice involvement. In terms of cash assistance in the United States, [Yang \(2017\)](#) finds that public assistance eligibility reduces recidivism among ex-offenders, while [Palmer, Phillips, and Sullivan \(2019\)](#) find that one-time cash assistance reduces violent crime for a broader low-income population. [Andersen, Dustmann, and Landersø \(2019\)](#) find increases in crime among refugees in Denmark who receive lower welfare benefits. The crime effects of a number of other public programs have been evaluated, including housing interventions ([Kling, Ludwig, and Katz 2005](#)), food stamps ([Tuttle 2019](#)), summer jobs ([Heller 2014](#)), Medicaid ([Arenberg, Neller, and Stripling 2020](#); [He and Barkowski 2020](#); [Jácome 2020](#)), and educational interventions ([Garces, Thomas, and Currie 2002](#); [Deming 2009](#)). Other research has examined how the timing of benefit receipt influences patterns of criminal activity (see [Belfield et al. 2006](#); [Dobkin and Puller 2007](#); [Foley 2011](#); [Carr and Packham 2019](#)). We also build on evidence of the effect of cash on criminal activity from the developing world ([Blattman, Jamison, and Sheridan 2017](#); [Khanna et al. 2019](#)) and in historical contexts ([Melander and Miotto 2021](#)), as well as research that distinguishes between financially motivated crime and crime that is motivated by other factors ([Corman, Dave, and Reichman 2014](#); [Tuttle 2019](#); [Khanna et al. 2019](#)).

We make two primary contributions relative to the literature. The first is demonstrating a previously undocumented persistence of the criminal response to losing welfare benefits. We find that SSI removal at age 18 for this cohort had just as large, if not larger, effects 20 years after removal than in the initial years. Much of the later-life increase in criminal justice involvement appears to

6. Our finding of persistence is consistent with a growing pattern of causal evidence that contact with the criminal justice system encourages more criminality in the long run, especially when contact occurs during the critical period of transitioning to adulthood ([Mueller-Smith 2015](#); [Agan, Doleac, and Harvey 2021](#); [Augustine et al. 2021](#); [Mueller-Smith and Schnepel 2021](#)).

be the result of SSI removal interacting with the economic distress of the Great Recession. The second contribution is demonstrating that the traditional “income effect” associated with welfare benefits—that of reducing earnings and employment—is, at least in this context, second-order relative to another income effect: reducing income-generating illegal activity and criminal justice involvement. Although the traditional income effect is generally considered an argument against welfare benefits, the “anti-crime” income effect, which we find to be much larger, has long-term benefits for would-be offenders and society at large. These contributions are made possible by our unique data linkage involving administrative earnings records and administrative criminal records spanning more than two decades after the 1996 reform.

The rest of the article proceeds as follows. [Section II](#) discusses the various channels through which losing SSI benefits could affect criminal activity in adulthood. [Section III](#) describes the data and presents statistics on the adult criminal justice involvement of children who receive SSI benefits. [Section IV](#) presents the empirical strategy and results on the effect of SSI removal on criminal activity. [Section V](#) discusses the results, and [Section VI](#) concludes.

II. CONCEPTUAL FRAMEWORK

There are several potential ways that removing a young adult from SSI could affect criminal justice outcomes later in life. We discuss the expected direction and magnitude of each channel, and suggest empirical tests to distinguish among them. We return to these empirical tests in [Section V.A](#).

For background, we first discuss institutional details of the SSI program that are relevant to these channels. Children qualify for SSI based on their own disability and their parents’ low income and assets. SSI benefits for children under 18 are paid to parents or legal guardians. The monthly maximum SSI benefit (\$794 in 2021) is reduced as parent income increases. In December 2019, the latest month for which data are available, approximately 60% of SSI child recipients received the maximum monthly benefit ([Social Security Administration 2020](#)). In most states, SSI recipients also receive automatic or near-automatic Medicaid enrollment. The SSI medical eligibility criteria for children are based on age-appropriate activity. Most of the growth in the SSI children’s program in recent decades has come from mental and

behavioral conditions such as ADHD, autism, and speech delay (U.S. Government Accountability Office 2011).

When children receiving SSI reach 18 years of age, they must be reevaluated for SSI based on the adult criteria. Unlike the child criteria, adult SSI criteria are based on an inability to work. The age 18 redetermination process disproportionately removes children with mental and behavioral conditions such as ADHD (see [Online Appendix](#) Figure B1). For those who are found eligible for the adult program, SSI benefits are paid directly to the young adult. The maximum monthly benefit (\$794 in 2021) is reduced based on the adult's own income (and based on in-kind support). The adult's benefits can be terminated if their earnings are too high (\$1,310 per month for nonblind recipients in 2021) or if they are incarcerated for more than 12 months.⁷

Removing a young adult from SSI could affect criminal justice outcomes later in life through the following channels (arrows indicate hypothesized direction of effect of SSI removal on criminal justice involvement).

1. *Income Effect: Indirect* (↓) *and Direct* (↑). Perhaps the most obvious channel through which SSI removal at age 18 could affect criminal justice involvement is the loss of SSI income in adulthood. Annual SSI benefits are nearly \$10,000 for adults with no earnings. We distinguish between an “indirect” income effect that operates through increased employment, and a “direct” income effect that operates through the income loss itself. Regarding the indirect income effect, [Deshpande \(2016\)](#) finds that SSI removal at 18 increases the likelihood of earning at full-time, full-year minimum-wage levels (\$15,000/year) from 10% to 14%, and increases average annual earnings from \$4,200 to \$5,000. Higher employment and earnings levels could reduce criminal justice involvement either mechanically—that is, by occupying the time of youth who would otherwise offend ([Jacob and Lefgren 2003](#))—or by building good habits that reduce the likelihood of offending ([Heller 2014](#)). Randomized studies of ex-offenders from the 1970s find that welfare benefits increase recidivism, likely because they discourage work ([Rossi, Berk, and Lenihan 2013](#)). Of course, 86% of youth removed from SSI at age 18 still do not earn at self-sufficiency levels, so any indirect effects of employment are unlikely to apply to them. To test for an indirect income effect, we

7. See <https://www.ssa.gov/pubs/EN-05-10133.pdf> for more details.

estimate the effects of SSI removal on the joint outcomes of employment and crime. If reductions in crime are observed and concentrated among those who work in response to SSI removal, this would provide evidence in support of the indirect income effect.

Regarding the direct income effect, [Deshpande \(2016\)](#) also finds that SSI removal at 18 does not increase earnings enough on average to replace the lost SSI income. Therefore most youth who are removed from SSI likely experience a net income loss. SSI removal could increase illegal activities that generate income, such as theft, drug distribution, or prostitution. (Conversely, it is possible that SSI removal reduces illegal activities that require income, such as alcohol-related traffic offenses and weapons or liquor possession.) In his book *Evicted: Poverty and Profit in the American City*, Matthew Desmond follows the story of a young woman who loses SSI benefits: “SSI always come,’ she said. Until one day it didn’t. She had been approved for SSI as a minor, but her adult reevaluation found her ineligible. Now Crystal’s only source of income came from food stamps. Because she didn’t know what else to do, Crystal went ‘on the stroll’ and began selling sex” ([Desmond 2016](#), 268). To test for a direct income effect, we define a category of “income-generating” criminal charges that comprises burglary, theft, fraud/forgery, robbery, drug distribution, and prostitution. We categorize all other charges as “non-income-generating,” including nonrobbery violent charges, disorderly conduct/obstruction/resisting arrest, criminal trespass, drug possession, and driving under the influence.⁸

2. *Incentive Effect* (↑). Adult SSI benefits are temporarily suspended when a recipient is incarcerated and can be reinstated when the recipient provides documentation of their release from jail or prison. However, if a recipient is incarcerated for more than 12 months, SSI benefits are permanently terminated and the individual must reapply after leaving jail or prison to receive benefits again.⁹ SSI removal could therefore increase criminal justice involvement by reducing the cost of incarceration—an incentive

8. There could also be effects on non-income-generating charges as a result of income loss. [Becker \(1968\)](#) proposes that the deterrence effects of the criminal justice system are weaker for lower-income individuals with fewer outside opportunities. In practice, we find much smaller effects on non-income-generating charges than on income-generating charges.

9. See <https://www.ssa.gov/pubs/EN-05-10133.pdf> for more details.

effect. Although incarceration is likely a deterrent to criminal activity (Lee and McCrary 2017), SSI recipients might face extra pressure from family members who rely on their SSI income to avoid imprisonment. Losing SSI benefits at age 18 could potentially alleviate this pressure and encourage criminal activity as a result. To test for an incentive effect, we separately estimate effects on charges that are more or less likely to lead to incarceration.

3. *Medicaid Effect* ($\uparrow$). In most states, SSI provides automatic or near-automatic Medicaid eligibility for adults. The 18-year-olds who are removed from SSI in this quasi-experiment could lose Medicaid eligibility, especially since the 1996 reform precedes state Medicaid expansions to low-income adults under the Affordable Care Act. The loss of Medicaid could potentially have two effects. One is an income effect, similar to the income effect from losing SSI cash benefits, if Medicaid would have paid for treatments that the individual must now pay for out of pocket. The second is the effect of forgoing medical care in the absence of health insurance coverage. Jácome (2020) finds that losing access to Medicaid increases the likelihood of incarceration for low-income men, especially those with a history of mental health conditions.

To test for a possible Medicaid effect in our context, we use two main strategies. First, taking advantage of our long time horizon, we estimate effects on crime in states and years in which Medicaid was available to the general low-income adult population versus states and years in which it was not. Similarly, we estimate effects using variation (at the county-year level) in the local availability of federally qualified health centers (FQHCs), which Bailey and Goodman-Bacon (2015) have shown to improve health among vulnerable populations. We would expect to find that the treatment effect on crime is smaller when Medicaid or FQHCs are available. Second, if Medicaid has larger effects for youth with mental and intellectual conditions versus physical conditions, then we can test for a Medicaid effect by testing whether the effects of SSI removal are larger for the former group. For example, Jácome (2020) finds that the loss of Medicaid increases criminal justice involvement particularly for youth with mental conditions. We create a composite “antisocial” crime index that aggregates nonrobbery violent crime and disorderly conduct. We would expect the effect of SSI removal on this antisocial index to be larger for youth with mental

conditions than physical conditions if Medicaid is a key channel through which SSI prevents crime.

4. *Long-Term and Spillover Effects* ($\uparrow\downarrow$, *Depending on Other Effects*). The effects on criminal activity that operate through the income effect (indirect or direct), the incentive effect, or the Medicaid effect could have long-term effects or spillovers to other types of crime. The immediate effects of SSI removal on criminal activity could persist through path dependence if, as a result of that initial involvement, individuals do not complete education (e.g., drop out of high school), develop human capital in criminal activity, experience labor market scarring due to criminal histories, or build social networks that encourage criminal activity. [Mueller-Smith and Schnepel \(2021\)](#) show that a first-time felony conviction amplifies future criminal activity over the next two decades. Similarly, [Agan, Doleac, and Harvey \(2021\)](#) find that for the marginal defendant, the decision not to prosecute a nonviolent misdemeanor offense reduces the likelihood of a new criminal complaint over the next two years. Increases in income-generating criminal activity could spill over to violent crime, either through collateral effects (e.g., a robbery attempt that escalates into homicide) or deeper involvement in crime.

III. DATA AND DESCRIPTIVE STATISTICS

We merge SSA data linked with records compiled by the CJARS. Since these measures are unconditional on SSI status, they allow us to estimate the effect of SSI removal on criminal justice involvement in adulthood.

III.A. SSA Data

The Supplemental Security Record (SSR) includes benefit history and demographic information for all SSI recipients. For each month from 1974 to the present, the SSR records whether an individual is currently eligible for SSI and the amounts of the federal and state SSI payments for that month. The SSR also includes information on whether the individual's benefits were terminated as a result of a medical review, including the age 18 medical review. We link the SSR to the CDR Waterfall File, which provides the dates and outcomes of the age 18 medical review and other medical reviews. We also link the SSR to the Master Earnings

File, which provides annual earnings from W-2 and Schedule SE records for all individuals in the sample. Because the SSR includes information on the parents of children receiving SSI benefits, we also construct variables for family structure and parental earnings for all children in the sample. About 10% of children receiving SSI do not have a parent on their record, usually because they live with someone other than their parent or legal guardian or because they are wards of the state.

III.B. CJARS Data

CJARS compiles criminal justice records from many jurisdictions and agencies. In this article, we use two primary types of records: criminal court charges, which are classified by type (e.g., property, drug, violent, parole violations) and gravity (e.g., misdemeanor, felony); and correctional data, including incarceration, probation, and parole. These state and local government records go back to the 1980s or earlier (Finlay and Mueller-Smith 2021b). CJARS uses a probabilistic matching algorithm (see Gross and Mueller-Smith 2021) to track individual involvement in the justice system over time and across jurisdictions. The map in Online Appendix Figure B2 shows the coverage of CJARS records: a geographically diverse set of states and localities that cover about 50% of the U.S. population. CJARS does not include records from the juvenile justice system. Given high rates of criminal justice involvement in this population, it is likely that many of the youth in the sample have criminal charges prior to their 18th birthday that we do not observe.¹⁰

III.C. Linkage between SSA and CJARS Data

We link records from CJARS to SSA administrative data based on an exact match of full name (first, last, and middle), date of birth, and state. For the state of Virginia, where year of birth is not available on criminal records, we use day and month of birth. We define our crime measures to be nonmissing only for the youth in counties with longitudinal CJARS coverage, which can differ for the criminal court records versus the correctional records. In addition, since our main measures are defined over the 1997–2017

10. The age of majority varies by state and over time. Often, youth who do not meet the age of majority definition are still prosecuted through the adult criminal justice system when they engage in serious criminal activity and will appear in CJARS as a result.

period, our outcomes are nonmissing only for individuals in counties with CJARS records in at least 80% of these years. About 40% of the SSI child population born between 1976 and 1998, and about one-third of the original [Deshpande \(2016\)](#) sample, resides in a county with CJARS coverage of at least one outcome. [Online Appendix Table B1](#) shows that the CJARS-covered and non-CJARS-covered samples are similar on characteristics in the SSA data.

III.D. Summary Statistics for Children Receiving SSI

[Figure I](#) presents rates of criminal justice involvement for children born between 1976 and 1998, inclusive, who ever receive SSI benefits as children and who reside in a county with CJARS coverage. Overall, 29% of these children have some contact with the criminal justice system by 2019. Rates of criminal justice involvement are higher for more disadvantaged groups, including youth with a single parent or no parent (32%) compared with youth with two parents (20%), youth with lower parental earnings (34%) relative to those with higher parental earnings (23%), and Black youth (41%) relative to white youth (26%). About 35% of male children who receive SSI go on to be involved in the criminal justice system, nearly double the rate for female SSI children (18%), although even the rate for women is much higher than for women in the general population ([Finlay and Mueller-Smith 2021b](#)).

Most of the growth in SSI—and concomitantly, most of the controversy about who should receive benefits—has come from children with mental conditions other than intellectual disability, such as ADHD, autism spectrum disorder, and speech delay ([U.S. Government Accountability Office 2011](#)). [Figure I](#) shows that rates of criminal justice involvement are higher for children with mental conditions (37%) than they are for child SSI recipients with physical conditions (23%) or intellectual disabilities (30%).

The bottom graph of [Figure I](#) shows the crime-age profile for people who received SSI benefits as children. Here we use the likelihood of a felony conviction because this outcome is also available for the general population. Rates of felony conviction among individuals who received SSI benefits as children increase from 6.5% at age 20, to 18% at age 30, to 22% at age 38. Using [Finlay and Mueller-Smith \(2021b\)](#), we compare this crime-age profile to that of 1970s birth cohorts overall. Rates among former

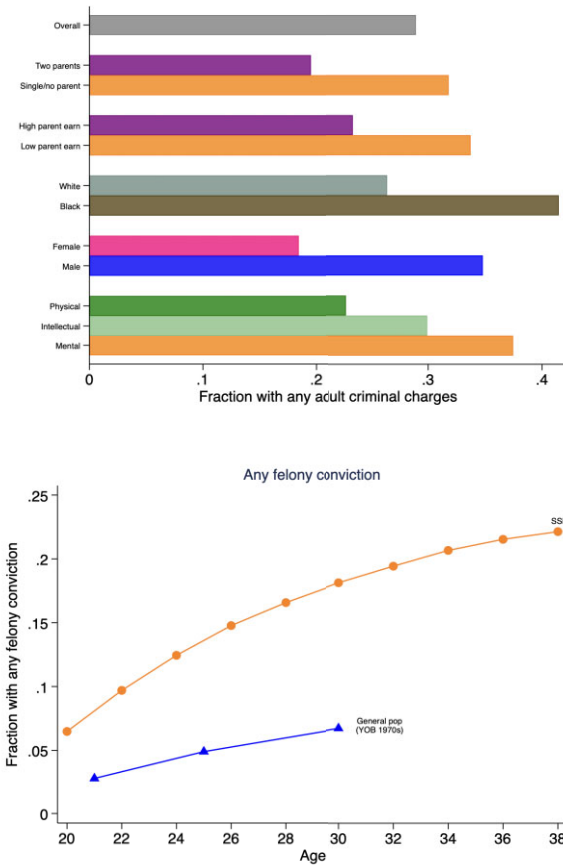


FIGURE I

Criminal Justice Involvement of SSI Children (Year of Birth 1976–1998) in Adulthood

Graphs plot the fraction of individuals who received SSI as children who are involved in the adult criminal justice system. The sample is individuals who received SSI as children with years of birth between 1976 and 1998, inclusive, who reside in a county with CJARS coverage. The top graph plots the fraction in each group with any adult criminal charge up to the last date in the data. Demographic characteristics are measured at the last time that the child appears in SSI records. “High/low parent earn” indicates parental earnings above/below the sample median (\$12,500 annually) when the child was between ages 12 and 17. “Physical” indicates a physical disability, “Intellectual” indicates an intellectual disability, and “Mental” indicates mental disability other than intellectual. The bottom graph plots the fraction in the sample with a felony conviction at each age from 20 years to 38 years. The 1970s birth cohort series uses [Finlay and Mueller-Smith \(2021b\)](#) to plot the fraction of people born in the 1970s with a felony conviction at ages 21, 25, and 30.

SSI youth are about double those of the general population in the early 20s and nearly triple by age 30.

These descriptive statistics have several implications for our causal analysis. First, the heterogeneity in baseline levels by diagnosis, sex, and family structure in the SSI child population highlights the importance of studying outcomes by subgroup. Still, rates of criminal justice involvement are high for all groups relative to the general population, including groups like women and youth from two-parent families that are often ignored in criminal justice analysis. Therefore it is still meaningful to study effects for these subgroups. Second, the crime-age profile suggests the importance of studying both the extensive margin (any involvement) and intensive margin (number of encounters or charges) of criminal justice involvement. Because we observe the children in the age 18 regression discontinuity design until age 41, it is possible that SSI removal affects offending behavior among those who would have offended by this age anyway, in addition to affecting first-time offending behavior.

IV. EMPIRICAL STRATEGY AND RESULTS

IV.A. Empirical Strategy: Discontinuity in Age 18 Medical Reviews

We estimate the effect of removal from SSI at age 18 on criminal justice involvement in adulthood using a regression discontinuity design originally documented and used by [Deshpande \(2016\)](#). The variation is based on a policy change in the 1996 welfare reform law. The Supreme Court ruling in the 1990 case *Sullivan v. Zebley* effectively required the SSA to allow children with mental and behavioral conditions of a certain severity to qualify for SSI. After a rapid increase in SSI child enrollment following that decision, Congress took measures in the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 to stem the tide of SSI child enrollment and remove children that it believed had been improperly allowed onto SSI.

PRWORA included two provisions regarding medical reviews for children receiving SSI who had reached 18 years of age. First, PRWORA required the SSA to redetermine the eligibility of all children receiving SSI when they turned 18, whereas previously SSA had conducted age 18 medical reviews for a very small fraction of children. Second, PRWORA required the SSA to use adult

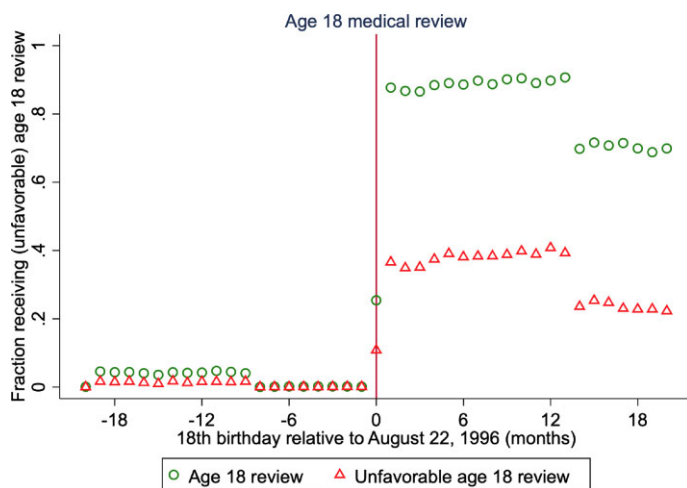


FIGURE II

First Stage: Likelihood of Age 18 Medical Review across Cutoff

Figure plots the likelihood of receiving an age 18 medical review and the likelihood of receiving an unfavorable age 18 review (i.e., being removed from SSI at age 18). The sample is SSI children with an 18th birthday within 18 months of the August 22, 1996, cutoff who reside in a county with CJARS coverage. Table I reports point estimates and standard errors.

medical criteria to determine eligibility at age 18, whereas previously age 18 reviews only required that children not demonstrate improvement in their medical condition to pass the review. The definition of disability is different for adults and children: for children, disability is defined in terms of age-appropriate activity, whereas for adults disability is defined as an inability to work. Conditions like ADHD may qualify a child for SSI but are less likely to qualify an adult unless it is judged to be severe enough to preclude work. Thus the new PRWORA requirement effectively made the age 18 review criteria more stringent.

Deshpande (2016) documents that the PRWORA rules were applied to children with an 18th birthday after August 22, 1996, the date of enactment. Figure II replicates, for the CJARS-coverage sample used in this article, the first-stage result from Deshpande (2016) showing that almost no children with an 18th birthday before PRWORA enactment received an age 18 medical review, while nearly 90% of children with an 18th birthday after PRWORA enactment received a review. (There is a decline in

review rates one year later after another policy change, but review rates remain high relative to the pre-PRWORA period.) This cutoff rule allows us to estimate equations of the form:

$$(1) \quad Y_i = \alpha + \beta \text{AfterPRWORA}_i + \gamma \text{DOB}_i + \delta (\text{AfterPRWORA}_i \times \text{DOB}_i) + X_i + \varepsilon,$$

where Y_i is a child outcome, AfterPRWORA_i is an indicator for having a date of birth after the date of PRWORA enactment (August 22, 1996), and DOB_i is the date of birth running variable. The X_i include covariates to improve precision, including sex, age at first SSI receipt, type of disability, family structure, parental earnings, and state. We residualize outcomes using these covariates in the RD scatter plots presented in the results. The coefficient of interest, β , gives the effect on outcomes of having an 18th birthday after the cutoff. We use a 10-month bandwidth in our main estimates. As robustness checks, we also present results without covariates (see [Online Appendix Table B2](#)), using a nonparametric analysis (see [Online Appendix Table B3](#)), and using a wide range of bandwidths (see [Online Appendix Figure B3](#)). Our results are robust to these alternative specifications. Because many children who receive a medical review are found eligible for the adult program, the discontinuity in the likelihood of receiving an unfavorable age 18 review falls to 36 percentage points. Children who receive an unfavorable age 18 medical review lose eligibility for SSI in adulthood, although they can reapply for SSI as adults. As shown in [Online Appendix Figure B1](#), young adults who are removed from SSI as a result of the age 18 medical review are disproportionately male and Black and have mental conditions other than intellectual disability. Medical conditions that are overrepresented in the “complier” population relative to the full sample include (starting with the most overrepresented): ADHD, asthma, anxiety disorder, personality disorder, cancer, bipolar/affective disorder, neurocognitive disorder, autism, and musculoskeletal conditions. Medical conditions that are underrepresented include (starting with the most underrepresented) cerebral palsy, blindness or deafness, nervous system conditions, injuries, blood disorders, schizophrenia, congenital disorders, and intellectual disability.

The identifying assumption of this RD design is that criminal justice outcomes would have evolved smoothly across the cutoff in

the absence of the 1996 welfare reform law increasing the number and stringency of age 18 medical reviews. [Deshpande \(2016\)](#) presents tests of validity for the full sample of children around the August 22, 1996, birthdate cutoff. We conduct similar tests for the sample of children in CJARS-covered states. Manipulation of the birthdate running variable seems unlikely in this context because birthdate was determined decades before the 1996 welfare reform law was passed. In the mid-1990s, there was uncertainty about whether or exactly when such a law would pass and how it would be enforced. [Online Appendix Table B4](#) presents covariate balance tests for the CJARS-coverage sample. Although a joint F -test rejects the null hypothesis of no discontinuities in covariates across the cutoff (p -value .0246), the significant discontinuities are in less common diagnosis groups (e.g., nervous system and infectious) rather than in demographic characteristics. We also test for discontinuities in the density of SSI child birthdates across the cutoff and do not detect any breaks (see [Online Appendix Figure B4](#)). Finally, we conduct placebo tests for discontinuities in criminal justice involvement at August 22 in 1994, 1995, and 1997. As shown in [Online Appendix Table B5](#), we find that estimates of the discontinuity in criminal justice outcomes at these placebo cutoffs are small and insignificant, while estimates at the true cutoff are large and significant.

IV.B. Effects of SSI Removal on Criminal Justice Outcomes

[Figure III](#) shows results for our main outcomes in the top two graphs: the total number of criminal charges between ages 18 and 38 (top left) and the average annual likelihood of incarceration over the same period (top right). These graphs show a large and clear jump in charges and incarceration to the right of the cutoff, where the SSI removal rate is higher. From [Table I](#), the number of charges increases by a statistically significant 8% (from 2.04 to 2.21 charges) across the cutoff. The annual likelihood of incarceration, defined as the likelihood of incarceration in each year from age 18 to 38 averaged across the 21 years (where the indicator is turned on in any year with at least one day spent in prison), increases by a statistically significant 23% (from 4.7 to 5.8 percentage points). Scaling up the reduced-form estimates by the first-stage effect on the likelihood of an unfavorable age 18 review (36 percentage points), SSI removal at age 18 increases the number of charges by 20% and the annual likelihood of incarceration by 60%.

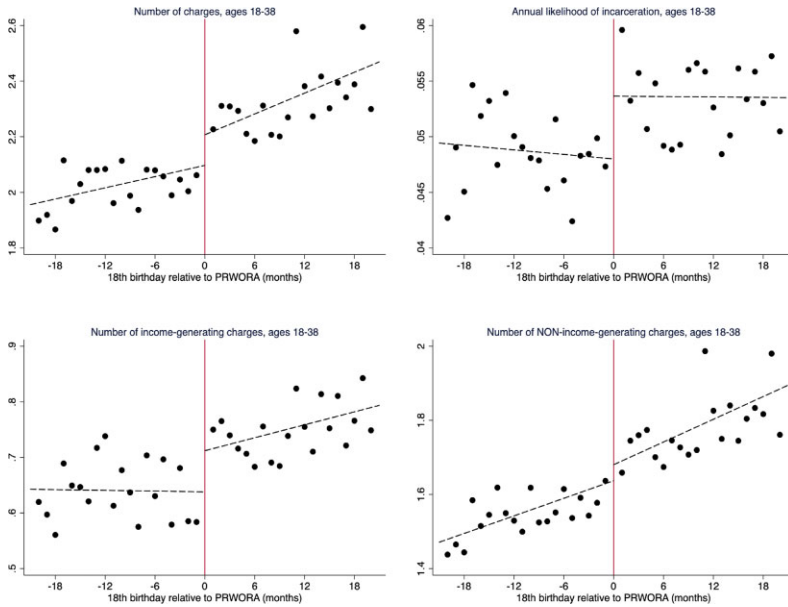


FIGURE III

Reduced Form: Criminal Justice Outcomes across Cutoff

The figure plots the total number of charges between ages 18 and 38 (top left); the average annual likelihood of incarceration from ages 18 to 38 (top right), defined as the likelihood of incarceration in each year from age 18 to 38 averaged across the 21 years (where the indicator is turned on in any year with at least one day spent in prison); the total number of income-generating charges (bottom left); and the total number of non-income-generating charges (bottom right). Outcomes are residualized (and then control mean added back) using the same covariates used in the main specification: medical diary (severity), sex, first age of SSI receipt, body system code, family structure, parent earnings, and state. The sample is SSI children with an 18th birthday within 18 months of the August 22, 1996, cutoff who reside in a county with CJARS coverage. [Table I](#) reports point estimates and standard errors.

Importantly, these results mean that the two main CJARS data sources—court charges and corrections—independently show a large and precise increase in criminal justice involvement across the cutoff.¹¹

11. What explains the upward trend in number of charges by birthdate? The answer appears to be a secular increase in criminal charges for younger birth cohorts. [Online Appendix Figure B5](#) shows an upward trend in criminal charges and incarceration for the general population with the same birthdate range.

TABLE I
RD AND IV ESTIMATES OF EFFECTS ON CRIMINAL JUSTICE OUTCOMES

	Extensive		Intensive	
	RD pt. est. (std. err.)	IV pt. est. (std. err.)	RD pt. est. (std. err.)	IV pt. est. (std. err.)
First stage ($N = 28,843$)				
Age 18 medical review	0.853*** (0.005)		0.002	
Unfavorable review	0.36*** (0.007)		0.0004	
On SSI from ages 19–22	–0.152*** (0.007)		0.822	
All crime				
Any charge ($N = 21,768$)	0.023** (0.012)	0.062** (0.032)	0.387	0.171* (0.092)
Incarcerated (annual likelihood/days) ($N = 26,991$)	0.011*** (0.004)	0.029*** (0.010)	0.047	0.464* (0.249)
Ever incarcerated ($N = 26,991$)	0.016** (0.008)	0.043** (0.021)	0.133	3.222*** (1.133)
Incarceration/parole/probation (ann likelihood/days) ($N = 22,705$)	0.011 (0.010)	0.029 (0.027)	0.229	8.791*** (3.092)
Charges related to income-generating activity ($N = 21,768$)				
Total	0.034*** (0.011)	0.093*** (0.03)	0.240	4.671** (2.328)
Burglary	0.005 (0.007)	0.014 (0.018)	0.068	12.72** (6.331)
Theft	0.029*** (0.009)	0.080*** (0.023)	0.121	0.380*** (0.105)
				0.023 (0.064)
				0.625
				0.129
				0.258

TABLE I
CONTINUED

	Extensive			Intensive		
	RD pt. est. (std. err.)	IV pt. est. (std. err.)	Mean	RD pt. est. (std. err.)	IV pt. est. (std. err.)	Mean
Fraud/forgery	0.007 (0.007)	0.018 (0.020)	0.076	0.006 (0.016)	0.016 (0.044)	0.135
Robbery	0.008* (0.005)	0.021* (0.012)	0.030	0.006 (0.008)	0.017 (0.021)	0.046
Drug distribution	0.005 (0.007)	0.013 (0.019)	0.071	0.019 (0.015)	0.050 (0.041)	0.125
Prostitution	0.005*** (0.002)	0.012*** (0.005)	0.004	0.014*** (0.005)	0.039*** (0.014)	0.010
Charges unrelated to income-generating activity ($N = 21,768$)						
Total	0.005 (0.012)	0.014 (0.031)	0.357	0.036 (0.076)	0.098 (0.205)	1.585
Nonrobbery violent	0.013 (0.010)	0.036 (0.027)	0.182	0.031 (0.026)	0.083 (0.072)	0.377
Disorderly conduct/obstruction/resisting arrest	0.016* (0.009)	0.043* (0.024)	0.128	0.049** (0.023)	0.132** (0.063)	0.260
Criminal trespass	0.004 (0.005)	0.010 (0.015)	0.045	0.027** (0.012)	0.073** (0.033)	0.062
Drug possession or paraphernalia	0.014 (0.009)	0.039 (0.024)	0.136	0.034 (0.028)	0.092 (0.076)	0.303
Driving under the influence	0.001 (0.006)	0.004 (0.015)	0.046	-0.010 (0.010)	-0.027 (0.028)	0.072

TABLE I
CONTINUED

	Extensive			Intensive		
	RD pt. est. (std. err.)	IV pt. est. (std. err.)	Mean	RD pt. est. (std. err.)	IV pt. est. (std. err.)	Mean
Employment ($N = 28,843$)						
Annual employment (earnings > \$5K/yr)	0.028*** (0.007)	0.077*** (0.018)	0.216			
Annual employment (earnings > \$15K/yr)	0.016*** (0.005)	0.044*** (0.014)	0.113			
Annual earnings				511.8*** (192)	1,421*** (528.6)	\$4,676

Notes. The table presents parametric RD estimates of the effect of having an 18th birthday just after the August 22, 1996, cutoff on outcomes measured between ages 18–38 (β from equation (1)). It also presents IV estimates of the effect of receiving an unfavorable age 18 medical review (β scaled up by the likelihood of an unfavorable medical review). Covariates include medical diary (severity), sex, first age of SSI receipt, body system code, family structure, parent earnings, and state. The sample is SSI children with an 18th birthday within 10 months of the August 22, 1996, cutoff, located in counties with CJARS coverage. "Extensive" indicates the likelihood of any charge (or, alternatively, incarceration spell or ever incarcerated). "Intensive" indicates the number of charges (or, alternatively, number of incarceration spells or number of days incarcerated). "Mean" indicates the average outcome for individuals with an 18th birthday within six months to the left of the August 22, 1996, cutoff. * $p \leq .1$, ** $p \leq .05$, *** $p \leq .01$.

Recall from the conceptual framework that if the effect of SSI removal on criminal justice outcomes operates primarily through an income effect, then removal could have larger effects on charges for which the primary motivation is income generation. We classify charges as either income-generating (burglary, theft, fraud/forgery, robbery, drug distribution, and prostitution) or non-income-generating (all others, including assault, weapons possession, sex crimes, drug possession, and disorderly conduct). From the bottom two graphs of [Figure III](#), the effect of the age 18 birthdate discontinuity is larger and more precise for income-generating charges (bottom left) than for non-income-generating charges (bottom right). SSI removal increases income-generating charges overall by a statistically significant 60% (0.63 to 1.0 charges), compared with just 10% (1.6 to 1.7, not statistically significant) for non-income-generating charges (see [Table I](#)).

The effects on income-generating charges operate on the extensive margin (i.e., any charge) and the intensive margin (i.e., number of charges). In other words, SSI removal leads to more first-time offending and to more offending among youth who would have been involved in the criminal justice system even if they had retained their SSI benefits (see [Online Appendix Figure B6](#) for RD plots for extensive-margin outcomes). This distinction is important in light of recent evidence that having any involvement in the criminal justice system (i.e., the extensive margin) has large effects on people's lives ([Agan, Doleac, and Harvey 2021; Mueller-Smith and Schnepel 2021](#)). In [Section V](#), we further investigate the effects of SSI removal on the extensive and intensive margins.

Consistent with [Deshpande \(2016\)](#), we find that SSI removal also increases formal employment in our CJARS-covered analysis sample: the likelihood of earning more than \$15,000 annually increases by 4.4 percentage points, from 11% to 16% (see [Table I](#)).¹² However, the increase in the likelihood of having an income-generating criminal charge is nearly double at 9.3 percentage points (24% to 33%).

Which specific income-generating crimes drive this increase? From [Figure IV](#), by far the largest and most precise increase is for theft charges (0.24 increase, or 90%). The increase in theft charges is large and precise for both men and women. For other types of charges, however, there is substantial heterogeneity by

12. Average annual earnings increase by \$1,421, from \$4,676 to \$6,097 (30%).

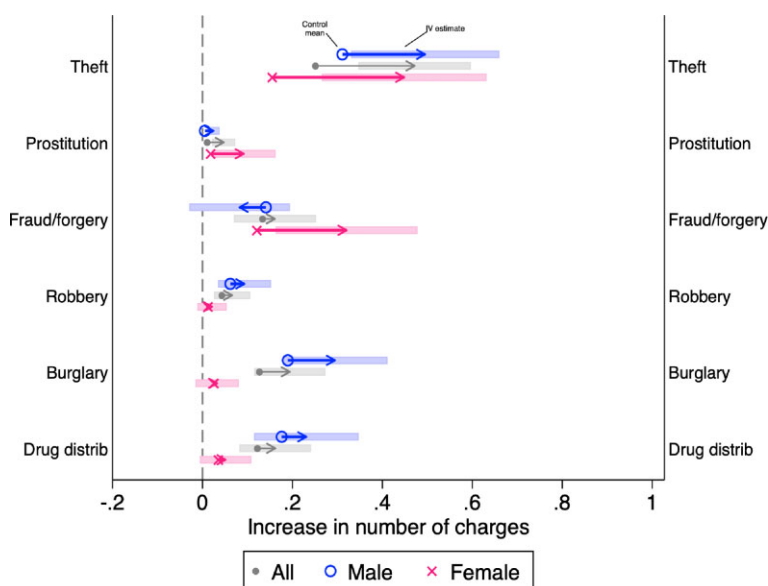


FIGURE IV

Income-Generating Illicit Activities for Men and Women

The figure plots the change in the number of charges from 1997 to 2017 resulting from SSI removal at age 18, by sex. Each arrow starts at the control mean for that subgroup and ends at the control mean plus IV estimate of the effect of receiving an unfavorable age 18 medical review (so that the arrowhead represents the level of criminal charges for those removed as a result of the age 18 review). Shaded bars represent confidence intervals for the estimate of the effect of the treatment on the treated. Sample is SSI children with an 18th birthday within 10 months of the August 22, 1996, cutoff who reside in a county with CJARS coverage. [Table I](#) and [Online Appendix](#) Tables B6 and B7 report point estimates and standard errors for the full sample, male sample, and female sample, respectively.

sex. For men, we estimate increases in burglary charges (0.10 increase, or 50%) and drug distribution charges (0.07 increase, or 40%), though these estimates are not statistically significant. There is also a small but precise increase in prostitution charges for men (0.02 increase, or 340% given the near-zero baseline). For women, SSI removal leads to a huge increase in the number of income-generating charges—nearly triple. In addition to theft, we estimate large and precise increases for women in fraud/forgery charges (0.19 increase, or 150%) and prostitution charges (0.08 increase, or 460%) as a result of SSI removal. Note that the estimate for fraud and forgery charges—most often bad checks, retail

fraud, and identity theft—is close to zero in the full sample but large and precise for women. [Online Appendix](#) Tables B6 and B7 present RD and IV estimates for men and women separately.

To be sure, we observe increases in some non-income-generating charges as a result of SSI removal. Even these non-income-generating charges, however, may be linked to income-generating offenses. For men, we estimate large and precise increases in criminal trespass charges (0.11, or 130%). This effect could be related to the effect on burglary charges, since burglary is really a combination of offenses: criminal trespass plus the attempt to commit another offense, or criminal trespass plus a completed offense ([Anderson 2011](#); [Mitchell 2001](#)). Men also have a large and precise increase in charges for disorderly conduct/obstruction/resisting arrest (0.18 increase, or 50%), the latter two of which are mechanically related to other charges. Nonrobbery violent charges for men also increase (0.15 increase, or 30%), but this estimate is not statistically significant and is driven by men who also have income-generating charges. This could mean that the violent charge occurs in the process of committing income-generating crimes (e.g., assault occurs during burglary), or simply that the same men who do more illicit income-generating activity also commit more violent offenses.

For women, we estimate a large and precise increase in drug possession charges (0.18 increase, or 170%). The reason for this increase is not obvious. It probably does not reflect drug distribution activity given that we do not find an increase in drug distribution charges for women. It could potentially reflect an attempt to self-medicate after the loss of SSI and Medicaid benefits, but we cannot test this hypothesis. We see no increase in violent charges for women.

As a result of the increase in criminal charges, the average annual likelihood of incarceration increases from 4.7 to 7.6 percentage points (60%) in the two decades after SSI removal, and the likelihood of ever being incarcerated increases from 13.3 to 17.6 percentage points (30%). For men, the annual likelihood of incarceration increases from 7.2 to 10.8 percentage points (50%). For women, it increases from 0.7 to 2.3 percentage points (220%). All of these estimates are statistically different from zero.

Does SSI removal lead to temporary increases in criminal activity among youth until they find more legal means of recovering the lost SSI income? Or does SSI removal permanently increase the propensity to offend throughout adulthood? [Figure V](#)

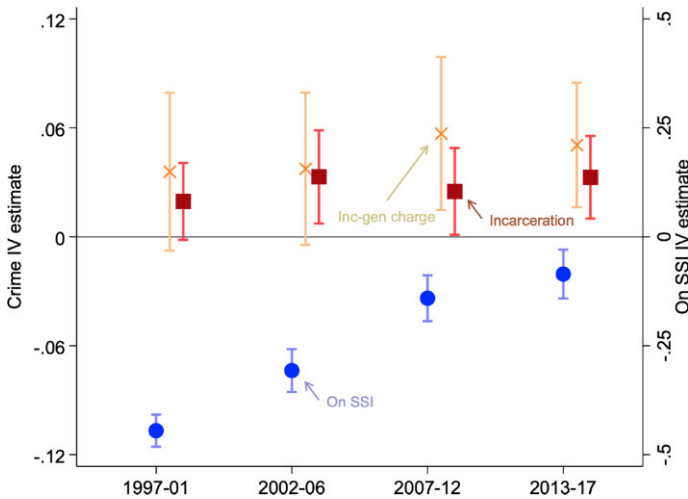


FIGURE V

Timing of Effects of SSI Removal on Criminal Justice Outcomes

The figure plots IV estimates of the effect of receiving an unfavorable age 18 medical review on the likelihood of receiving SSI benefits in each period (right axis), the likelihood of having a criminal charge for an income-generating crime in each period (left axis), and the likelihood of being incarcerated in each period (left axis). The estimates are within each period, not cumulative. The IV estimates for crime and incarceration are measured on the left axis, and the IV estimate for receiving SSI benefits is measured on the right axis. Sample is SSI children with an 18th birthday within 10 months of the August 22, 1996, cutoff who reside in a county with CJARS coverage. [Online Appendix Table B8](#) reports point estimates and standard errors.

presents the IV estimates of the effect of receiving an unfavorable age 18 medical review on SSI receipt, income-generating criminal charges, and incarceration over time. The figure, which plots period-by-period (not cumulative) estimates, shows that the effects of SSI removal on criminal activity are highly persistent over the following two decades. The effect of SSI removal on the likelihood of a criminal charge associated with income generation increases from 3.6 percentage points in 1997–2001, the period immediately after removal, to 5.7 percentage points during the Great Recession in 2007–2012. The effect on the likelihood of incarceration also remains steady at around 3 percentage points over this time. This persistence in criminal charges and incarceration is made more remarkable by the fact that the effect of age 18 removal on the likelihood of receiving SSI benefits declines over the

same time period. The effect of SSI removal on the likelihood of receiving SSI decreases from 45 percentage points in 1997–2001 to just 14 percentage points in 2007–2012, mostly because people in the control group leave SSI in adulthood for various reasons. This pattern suggests that contemporaneous SSI income during adulthood is not the primary driver of criminal justice involvement. Instead, the loss of SSI income in early adulthood permanently increases the propensity to commit crimes throughout adulthood. [Online Appendix Figure B7](#) shows the time path for the youth's earnings (constant positive effect) and income (attenuating effect over time), and [Online Appendix Figure B8](#) shows the time path for specific charges.¹³

We study heterogeneity in the effects of SSI removal by family structure, parent income, race, gender, type of disability, and the local unemployment rate. [Figure VI](#) shows that SSI removal increases the likelihood of an income-generating criminal charge substantially among all observable subgroups in the SSI population. These differences across subgroups are not statistically significant, but the point estimates suggest larger charge responses for women than for men, for youth with physical conditions than those with intellectual or other mental conditions, and for youth living in high-unemployment counties relative to those living in low-unemployment counties. The charge responses appear similar by family structure, parental earnings, and race.

[Figure VI](#) also shows that the incarceration effects are disproportionately large for groups that already have high incarceration rates, including Black youth and youth with a single or no parent or low parent earnings. The differences in incarceration

13. Among people in our sample with a prison spell, the first spell length (with censoring at the last available year of data, 2019) has the following distribution: 25th percentile is 313 days, median is 696 days, 75th percentile is 1,743 days. The incapacitation effects of incarceration (and possibly probation and parole) will lead us to underestimate the effect of SSI removal on the true propensity to engage in illicit activity, especially in later periods. To gauge the magnitude of the bias, we multiply the IV estimate of the effect on incarceration (and probation and parole) in a period by the criminal charge rate for the treatment group in that period. Based on these calculations, our estimates of the effect of SSI removal on income-generating charges would be 8% higher in period 3 and 6% higher in period 4 accounting for the incapacitation effect of incarceration. In addition, it is possible that sample composition changes in CJARS coverage affect the estimates across periods. To test for this possibility, in [Online Appendix Table B9](#) we estimate the period-by-period effects using different denominators and find that the estimates are similar across the approaches.

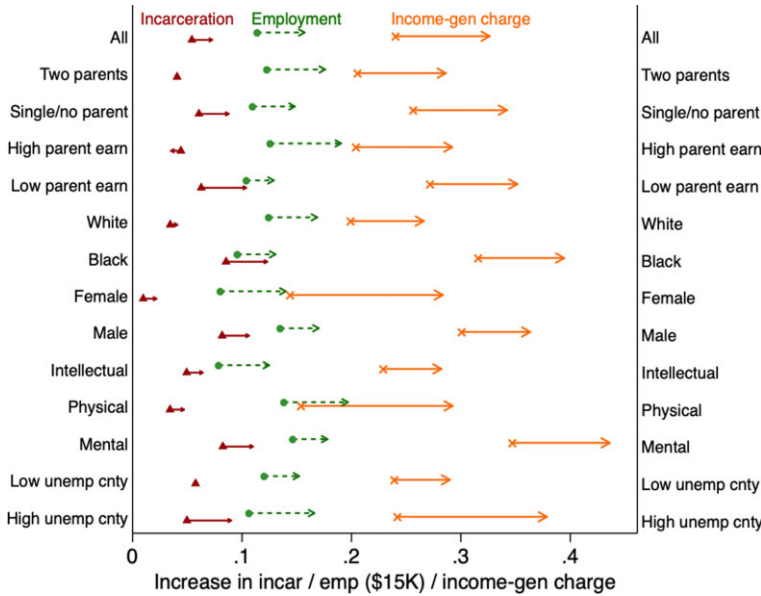


FIGURE VI
Heterogeneity in Effects of SSI Removal by Subgroup

The figure plots the change in the outcome variable (annual incarceration rate, likelihood of income-generating charge, or likelihood of earning more than \$15,000 annually from 1997 to 2017) resulting from SSI removal at age 18, by subgroup. Each arrow starts at the control mean for that subgroup and ends at the control mean plus IV estimate of the effect of receiving an unfavorable age 18 medical review (so that the arrowhead represents the level of criminal charges for those removed as a result of the age 18 review). “High/low parent earn” indicates children with parental earnings above/below the sample median (\$5,500 annually) when the child was between 12 and 17 years of age. “High/low unemp cnty” indicates children living in an above/below median unemployment rate county, where the unemployment rate is the county’s average annual unemployment rate from 1997 to 2017 from the Bureau of Labor Statistics. The sample is SSI children with an 18th birthday within 10 months of the August 22, 1996, cutoff who reside in a county with CJARS coverage. The following tables report point estimates and standard errors for different groups: [Table I](#) for all, [Online Appendix Table B6](#) for men, [Online Appendix Table B7](#) for women, [Online Appendix Table B10](#) by parent earnings and family structure, [Online Appendix Table B11](#) by race and diagnosis, and [Online Appendix Table B12](#) by county unemployment rate.

effects by parental earnings, race, and gender are statistically significant. These results suggest that SSI removal exacerbates existing inequality in incarceration. What explains the differences in treatment effects across subgroups? There are a few potential explanations: different complier characteristics of the subgroups,

differences in the types of crime committed across subgroups, or different policing and prosecution across subgroups conditional on crime response. To disentangle these potential explanations, we first compare complier characteristics of the different subgroups in [Online Appendix Figure B9](#) and find fairly similar complier characteristics across subgroups. That leaves differences in the types of crime and differences in enforcement as the two possible explanations for heterogeneity in the incarceration response. In [Online Appendix Figure B10](#), we find some significant differences in types of criminal charges across subgroups. We estimate larger and more precise effects on burglary, nonrobbery violent, and weapons charges for Black youth compared with white youth. We estimate larger effects on theft, burglary, drug distribution, prostitution, and weapons charges for youth with low parent earnings compared with youth with higher parent earnings. Although we cannot distinguish definitively between different types of crime and enforcement conditional on crime response, the differences across groups in the types of charges suggest at least some role for different crime responses across subgroups.¹⁴

[Figure VI](#) also shows, consistent with [Deshpande \(2016\)](#), that SSI removal modestly increases the likelihood of steady employment (annual earnings above \$15,000) for most subgroups. Do the same youth respond on the employment and crime margins, or do some youth pursue legal earning opportunities while others engage in illicit income-generating activity? [Figure VII](#) suggests the latter explanation. The figure plots IV estimates of the effect of an unfavorable age 18 review on joint employment and

14. We also estimate effects for a number of other subgroups. [Online Appendix Table B13](#) presents effects by age at entry, pre-18 criminal record, and predicted removal probability. Regarding age at entry, the effects are largest for those who enter at a young age (which is highly correlated with having a physical disability). The effects are large and precise for youth who were charged in the adult criminal system before age 18, despite the small sample size. On both the employment and crime margins, treatment effects are larger for youth with below-median removal probabilities than above-median. This counterintuitive result could be explained by selection into the complier population identifying each effect, or could simply reflect that our prediction-of-removal regression has limited explanatory power. We regress an indicator for receiving an unfavorable age 18 review on observable characteristics for children with an 18th birthday after the PRWORA cutoff and then predict the likelihood of receiving an unfavorable age 18 review for the entire sample. The *R*-squared of the prediction regression is 0.16, indicating that we as researchers observe only a limited number of factors that enter the disability examiner's decision.

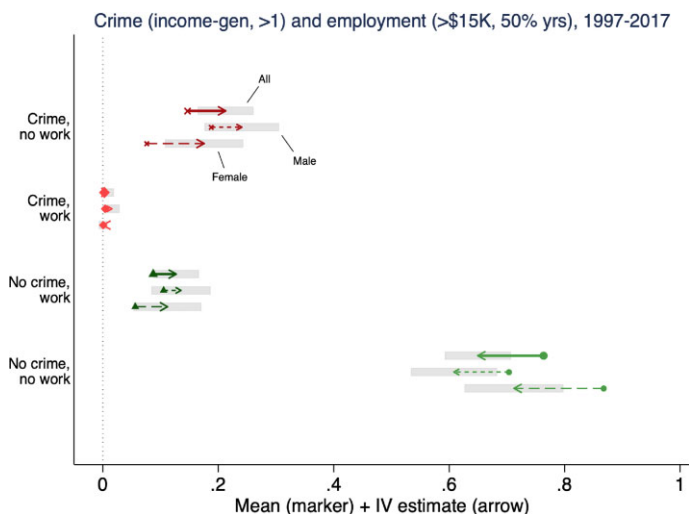


FIGURE VII

Effect of SSI Removal on Joint Crime and Employment Outcomes

Figure plots IV estimates of the effect of receiving an unfavorable age 18 medical review on the joint likelihood of having more than one criminal charge for an income-generating crime from 1997 to 2017 and having annual earnings of at least \$15,000 (in 2012 dollars) in at least half of the years from 1997 to 2017. “Crime, no work” indicates the outcome of having more than one criminal charge but not earning at least \$15,000 in at least half of the years. “Crime, work” indicates the outcome of having more than one criminal charge and earning at least \$15,000 in at least half of the years. “No crime, work” indicates having one or fewer criminal charge and earning at least \$15,000 in at least half of the years. Sample is SSI children with an 18th birthday within 10 months of the August 22, 1996, cutoff who reside in a county with CJARS coverage. [Online Appendix](#) Table B15 reports point estimates and standard errors.

offending outcomes. Employment is defined as earning at least \$15,000 annually in at least half of the years from 1997 and 2017, and crime is defined as having more than one criminal charge associated with illicit income-generating activity during that time.¹⁵ The largest increase is for the arguably worst outcome—responding on the crime margin but not on the employment margin. Among those who respond on the employment margin, about half have at least one criminal charge associated with income generation and the other half do not.

15. See [Online Appendix](#) Figure B11 for crime defined as having at least one charge, which gives similar results. Using a \$5,000 instead of a \$15,000 threshold also gives similar results.

The distribution of outcomes is bimodal: youth either respond with steady employment, or, more commonly, with criminal involvement. This bimodal distribution of outcomes is especially stark for women (see [Online Appendix Figure B12](#), which shows the breakdown in [Figure VII](#) by subgroup). Youth with physical disabilities respond almost exclusively with crime, whereas youth with mental conditions are one of the few groups with an employment response that is larger than the crime response. Youth in high crime areas (and in high unemployment areas) have a much larger crime response than employment response, whereas the opposite is true for low-crime areas, suggesting that the available opportunities for legal or illegal income generation matter for the response to SSI removal.¹⁶

The results suggest that over time, youth specialize in either legal work or criminal activity to generate income. To be sure, we cannot directly measure the true increase in criminal activity or compare total illegally generated income to the legal earnings that we observe in SSA data. But if anything, our estimate of the effect on criminal charges underestimates the true effect on criminal activity, since the vast majority of offenses go unreported to law enforcement or uncharged by prosecutors. Using estimates from other sources on reporting rates and charging rates (see [Online Appendix Table B21](#) for details), we do a back-of-the-envelope calculation of the number of crimes created by each SSI removal. Scaling up the IV estimates yields an increase of 4.9 thefts, 1.2 burglaries (and 1.3 criminal trespass offenses), 0.64 fraud/forgery offenses, and 0.21 robberies for each SSI removal. Since “victimless” crimes like drug dealing, drug possession, and prostitution are rarely reported, the scaled-up IV estimates for these offenses (per SSI removal) are in the hundreds.

We also estimate effects on criminal justice outcomes for siblings and parents of the 18-year-old sample, presented in [Online Appendix Table B16](#). As in [Deshpande \(2016\)](#), we can

16. We also study how the effect of SSI removal on criminal charges varies with the local unemployment rate, minimum wage, and cost of living. In general, we estimate effects in the expected direction (larger effects for higher unemployment rates, lower minimum wages, and lower cost of living), but the estimates are small and imprecise (see [Online Appendix Table B14](#)). From [Figure VI](#), the charge and incarceration responses are larger for youth living in above-median unemployment rate counties, where the unemployment rate is measured as the average across 1997 to 2017 (though the employment response is also slightly larger for higher unemployment counties).

only observe siblings who themselves receive SSI benefits at some point in their life. We estimate effects for these siblings in the same direction as the effects for the 18-year-olds, with statistically significant increases in criminal charges and incarceration rates. This crime response is consistent with the finding in [Deshpande \(2016\)](#) that younger siblings of 18-year-olds who are removed have lower earnings in adulthood. Unlike the 18-year-olds, however, younger siblings respond to the 18-year-old's removal with both income-generating and non-income-generating crime in adulthood (though these estimates individually are not statistically significant). This pattern suggests that younger sibling criminal justice involvement may not be driven by income-generation motives (as appears to be the case for the 18-year-old) but by other channels—for example, it could be that the 18-year-old's removal reduces the resources available to younger siblings, which adversely affects their human capital development and leads to a higher propensity to commit crime in adulthood. Regarding parents, we find that parent criminal justice involvement, although high at baseline, does not respond to the 18-year-old's removal. This lack of response is also consistent with the negligible parent earnings response in [Deshpande \(2016\)](#).

V. DISCUSSION

Our results indicate that youth are more likely to respond to the SSI loss by generating income through illicit activity than through legal employment, and this illicit activity increases their likelihood of going to prison. Because SSI is a means-tested program, children who receive SSI are doubly disadvantaged: they have a disability and live near or below the poverty line. When they lose SSI benefits, they may not have the skills to succeed in formal employment and might face discrimination (with respect to disability, race, or class) or other barriers in the labor market.

[Online Appendix](#) Figure B13 compares our estimates with other estimates of the effects of transfers on criminal charges, specifically for housing transfers (from [Kling, Ludwig, and Katz 2005](#)) and Head Start (from [Garces, Thomas, and Currie 2002](#); [Deming 2009](#)). We scale all effect sizes so as to measure effect size per \$1,000 transfer. Conditional on transfer size, our estimates are substantially larger than the housing and Head Start estimates. This is mostly attributable to the persistence of effects shown in [Figure V](#). Even as the differential in the transfer amount declines,

the effect on criminal charges increases. These comparisons suggest that SSI benefits in young adulthood lead to larger and more persistent reductions in crime than do other types of transfers.

V.A. *Evidence on Mechanisms*

In [Section II](#), we proposed various channels through which SSI removal at age 18 could affect criminal justice involvement in adulthood: a direct income effect, an indirect income effect (operating through employment), and a Medicaid effect. Overall, we interpret our results as pointing primarily to a direct income effect—many youth who lose SSI income respond by engaging in illegal activities to generate income.¹⁷ Here we discuss the evidence for each of the alternative channels. We then try to understand the persistence of the effects as well as the relative roles of the extensive and intensive margins.

1. *Indirect Income Effect through Employment.* If there is an indirect income effect that reduces crime through higher employment, it is small relative to the direct income effect working in the other direction. [Figure VII](#) suggests that youth who respond by working more in the formal sector are unlikely to engage in criminal activity in the long run. This could mean that the indirect income effect applies to the small minority of youth who respond with formal employment. However, the large increase in criminal charges means that the indirect income effect is dominated by a much larger direct income effect.

2. *Incentive Effect.* We also consider an incentive effect in which removed youth may be less likely to avoid crimes that could land them in prison because they no longer face the threat of losing SSI benefits if incarcerated. We estimate a large increase in the likelihood of incarceration, but it is not obvious whether this effect operates through the incentive effect or through the direct income effect via income-generating criminal activity. To distinguish the incentive effect from the direct income effect, we can look at effects on charges that are likely to lead to incarceration but are not income-generating, such as nonrobbery violent crime. We estimate an imprecise 20% increase in nonrobbery violent crime. Moreover, we find that this increase is driven by men

17. We find no evidence of decreases in the category of “income-requiring charges”—DUI, drug possession, liquor possession, weapons possession.

who also respond on income-generating charges, suggesting that they could be collateral damage from income-generating crimes. Although we cannot rule out the incentive effect entirely, the evidence suggests that the incentive effect is second-order relative to the direct income effect.

3. *Medicaid Effect.* Youth who were removed from SSI in 1996 mostly lost eligibility for Medicaid unless they lived in one of the states that provided Medicaid to low-income adults.¹⁸ The Medicaid loss could affect crime through either an income effect or an effect of forgone medical care.

To test for a Medicaid effect, we use two strategies discussed in [Section II](#). First, taking advantage of our long time horizon, we estimate crime treatment effects in states and years in which Medicaid was available to the general low-income adult population versus states and years in which it was not. Similarly, we compare treatment effects in counties and years in which an FQHC, which provides health care to the poor and uninsured, was available. As shown in [Online Appendix Table B17](#), we find no evidence that crime treatment effects are smaller in area-years with Medicaid or FQHC availability. If anything, estimates of the interaction of SSI removal with Medicaid/FQHC availability are positive (though not statistically significant).

Second, we compare the crime response for youth with other mental conditions relative to those with intellectual or physical conditions, given evidence from [Jácome \(2020\)](#) of large effects of Medicaid loss on criminal justice involvement for youth with mental conditions. We find similar effects across disability types, which is contrary to what we would expect if the Medicaid effect were strong. If anything, we see in [Figure VI](#) larger responses for youth with physical conditions than for those with mental or intellectual disabilities. In [Online Appendix Figure B14](#), we combine all individuals with mental conditions (both intellectual and other mental) to align with the methodology in [Jácome \(2020\)](#), and compare crime treatment effects for youth with physical versus mental conditions. The effects on income-generating charges are larger for those with physical conditions than mental

18. Among states in our sample, states that provided Medicaid to low-income adults in 1997 include Oregon and Maryland. Arizona expanded Medicaid access to adults in 2001. Michigan, Indiana, and Wisconsin expanded access between 2001 and 2010 ([Rudowitz, Artiga, and Arguello 2013](#)).

conditions, and the effects on “antisocial” charges (nonrobbery violent crimes and disorderly conduct) are much larger for those with physical conditions than mental conditions. Assuming that Medicaid has larger effects on individuals with mental conditions, and especially on their propensity to commit “antisocial” crimes, this result is the opposite of what we would expect if Medicaid were a major channel through which SSI affects criminal outcomes. Together, these results suggest that the Medicaid effect is small relative to the direct income effect.

4. *Alternative Explanations.* We estimate large effects of SSI removal on criminal charges and incarceration, which we interpret as demonstrating a large effect of SSI removal on criminal activity. However, it is also possible that SSI removal affects the likelihood that a prosecutor charges someone with a crime conditional on underlying criminal activity, either because the prosecutor treats SSI recipients more leniently or because the SSI income helps recipients pay for legal representation. We think this is unlikely to be a primary explanation for the increase in criminal charges for two reasons. First, the number of criminal charges for the control group is high: an average of 2.04 criminal charges over the 21 years after age 18. Individuals receiving SSI are clearly not exempt from criminal charges. Second, if the effect of SSI removal were operating primarily through the likelihood of charging, we would expect the effect to show up in a broad set of charges. Instead, we see effects almost exclusively in activities aimed at generating income, even though the control mean for non-income-generating charges is high.

V.B. Understanding the Persistence of the Crime Effects

Figure V shows that the effects of SSI removal on income-generating charges and incarceration are highly persistent. In fact, the largest effects on income-generating charges occur more than a decade after SSI removal, even though the effect of SSI removal on SSI income attenuates over time. A key question is how much of this persistence reflects true path dependence—criminal activity in early adulthood leading to more criminal activity in later adulthood—and how much is simply a function of the Great Recession happening about a decade after removal. Since the youth in this natural experiment are removed at the same time and at the same age, the reduced-form estimates reflect age

effects, relative time effects (i.e., the path dependence story), and calendar time effects (i.e., the Great Recession story).

We use two strategies to better understand the persistence of criminal justice effects in our setting. First, we study the effect of SSI removal on employment and criminal involvement in the years immediately after removal (1997–2001) relative to later years (2002–2017). People could respond by increasing work in both periods, increasing criminal justice involvement in both periods, increasing work in the first period and criminal justice involvement in the second, or vice versa. [Online Appendix Figure B15](#) shows that the largest responses are those of specialization: increasing work in both the initial and later periods, or increasing criminal justice involvement in both the initial and later periods. In addition, a sizable fraction of youth switch from a work response (or no response) in the initial period to a crime response in the later periods. However, almost no one switches from a crime response in the initial period to a work response (or no response) in the later period. The crime response appears to be a one-way street: once one is engaged in criminal activity in response to SSI removal, there is no transitioning to a formal employment response. That the crime response is an absorbing state explains some of the persistence, though this exercise cannot distinguish what underlying mechanism drives the path dependence (e.g., formation of criminal capital versus a scarring effect of having a criminal record).¹⁹

Second, we attempt to explain the large effects of SSI removal on criminal justice involvement in the second decade after removal, when the gap in SSI receipt has significantly narrowed between treatment and control groups. In [Online Appendix Figure B16](#), we decompose the effect of SSI removal on criminal charges in 2007–2017 into the share driven by early entrants into the justice system (i.e., those who initiated criminal activity in years immediately following removal) versus late entrants. If the former account for most of the effects in these later years, then the path dependence story might better explain the persistence of criminal justice effects. If the latter account for a large share, then the Great Recession story might better explain the

19. [Mueller-Smith and Schnepel \(2021\)](#) provide evidence that avoiding a felony conviction increases the likelihood of employment in industries that typically exclude those with criminal histories, suggesting that at least the scarring effect of having a criminal record plays some role in our findings.

persistence.²⁰ [Online Appendix](#) Figure B16 plots IV estimates of the effect of SSI removal on the likelihood of having a charge during 2007–2017. The figure decomposes that IV estimate into the following categories that may be suggestive of the source of persistence:

- Individuals who have a previous charge in the same crime type in 1997–2006, which might suggest specialization in that type of crime and/or scarring;
- Individuals who had a previous charge of a different crime type in 1997–2006, which might suggest a more general criminal propensity or scarring;
- Individuals who had no previous charges in 1997–2006 and no employment during that period, which might suggest that they were pulled into crime as a result of the Great Recession;
- Individuals who had no previous charges in 1997–2006 but were employed in at least some years during that period, which might suggest that they were marginally attached to the labor market during good economic times but then turned to crime after losing their jobs in the Great Recession.

About one-third of the increase in the likelihood of any income-generating charge in 2007–2017 is driven by individuals with a previous income-generating charge, one-third by those with a previous non-income-generating charge, and one-third by those with no previous charge. Among those with no previous charge, the majority have prior employment, suggesting that they may have lost their jobs and been pulled into crime by the Great Recession. Of course, it is possible that even those already involved individuals would not have committed more offenses in the absence of the Great Recession, and therefore this exercise may provide only a lower bound on the role of the Great Recession.²¹

20. To be sure, this exercise provides only suggestive evidence on the source of persistence, since we can only observe illicit activity that results in a criminal charge. If some individuals engaged in crime successfully avoid law enforcement initially, they will appear as late entrants.

21. These ratios differ for specific types of income-generating charges. For fraud/forgery, most of the increase in 2007–2017 comes from people who previously were not involved with this particular type of crime, which makes sense since opportunities for fraud/forgery have increased in recent years ([Federal Trade Commission 2020](#)). In contrast, the increase in burglary and theft charges are driven

V.C. Decomposing the Effects into Extensive and Intensive Margins

As discussed in [Section IV](#), determining whether SSI removal pushes people into crime who would not otherwise be criminally involved, or only increases involvement among those who would otherwise be involved, is important for interpreting the results. Recent evidence shows that any involvement in the criminal justice system has large effects on people's lives ([Agan, Doleac, and Harvey 2021](#); [Mueller-Smith and Schnepel 2021](#)).

Although we cannot say definitively how much of the increase in charges is attributable to a shift from zero to positive (versus positive to more positive), we present a number of exercises to provide suggestive evidence on this point. In [Online Appendix Figure B17](#), we plot effects on the distribution of the number of charges. We find larger effects at four, five, and six charges than at one, two, and three charges, which suggests possible intensive-margin effects. But the same figure also shows that the effect on the likelihood of having at least one charge is larger than the effect on the likelihood of having at least two (or at least three, or at least four, etc.) charges, which means that there is a sizable extensive-margin response.

The key missing parameter in decomposing the total effect on criminal charges into extensive- versus intensive-margin shares is the average number of charges for individuals who are shifted on the extensive margin. Assuming that people who are shifted on the extensive margin have on average 1.5 income-generating charges yields a lower bound of 37% for the extensive-margin share. Increasing this assumption to two (three) charges increases the extensive-margin share to 49% (73%).²²

more by individuals with previous offenses in that category. Note that criminal justice involvement does not increase for the control group during the Great Recession (see [Online Appendix Table B8](#)).

22. From [Table I](#), the increase in the likelihood of having any income-generating charge is 0.093 and the increase in the number of income-generating charges is 0.380. Suppose we assume that among individuals who are moved on the extensive margin (i.e., would have had zero charges if not removed from SSI but a positive number of charges if removed) the average number of charges if removed is 1.5 (a conservative assumption because by definition everyone has at least 1 charge). Then of the 0.380 increase in the number of income-generating charges, the extensive-margin portion is 0.140 ($= 0.093 \times 1.5$, or 37%) and the intensive-margin portion is the remaining 0.240 (63%). If instead we assume that individuals moved on the extensive margin have on average 2 charges if removed, then the

Another way to understand extensive- versus intensive-margin effects is to look at the cumulative effects over time of involvement. In [Online Appendix Figure B18](#), we show that the effect of SSI removal on the cumulative likelihood of having an income-generating charge (i.e., extensive margin) increases in the early 20s, plateaus in the late 20s, and picks up again in the 30s (during the Great Recession for this cohort). In contrast, the effect on the cumulative number of income-generating charges (i.e., combination of extensive and intensive) continues to rise in the late 20s and then accelerates in the 30s. This contrasting pattern suggests a particular pattern of the effect of SSI removal over time: in the initial years after removal (in their early 20s), it pushes people into criminal justice involvement who would not have otherwise been involved; in later years (late 20s), it increases involvement among those already involved; when the Great Recession hits (30s), it again pushes people into criminal justice involvement who would not have otherwise been involved.

V.D. Cost-Benefit Analysis of SSI Removal

Part of the motivation for removing youth from SSI at age 18—when the 1996 welfare reform law was passed and today—is to avoid spending taxpayer money on individuals who are deemed capable of working in adulthood. In [Table II](#), we calculate the savings to taxpayers from lower SSI and Medicaid benefits relative to taxpayer spending on increased crime and incarceration. We use our estimates of the effect of SSI removal on criminal justice involvement for both the 18-year-olds and their siblings (see [Online Appendix Table B18](#)). Here we summarize our cost-benefit calculations, with more details in [Online Appendix Section A](#).

We start with the taxpayers' perspective. From our IV estimates, each SSI removal saves taxpayers about \$37,700 in federal SSI benefits. Combining our IV estimates with other estimates, we calculate additional savings to taxpayers of \$8,400 in Medicaid costs from 1997 to 2013 (prior to Medicaid expansions under the Affordable Care Act in 2014). Taxpayers also benefit from higher tax revenue from those youth who work more in response to removal, on the order of \$3,000 per removal over the next 21 years according to our IV estimates.

extensive-margin portion is 0.186 ($= 0.093 \times 2$, or 49%) and the intensive-margin portion is 0.194 (51%). If we assume an average of 3 charges for extensive-margin responders, these portions become 0.279 ($= 0.093 \times 3$, or 73%) and 0.101 (27%).

TABLE II
COST-BENEFIT ANALYSIS

	Amount	Notes
Savings per removal to taxpayer		
SSI benefit savings	\$37,737	Authors' IV estimate (1997–2017)
Medicaid savings	\$8,400	Savings from 1997–2013
Likelihood of being off SSI	0.24	Authors' IV estimate
Likelihood of losing Medicaid	0.71	Hemmeter (2011) estimate
Annual net cost of Medicaid	\$2,900	Finkelstein, Hendren, and Luttmer (2019) estimate, doubled
Tax revenue from earnings	\$2,984	21-year period, assume 10% tax rate
Annual earnings increase	\$1,421	Authors' IV estimate
Total savings to taxpayer	\$49,121	
Costs per removal to taxpayer		
Enforcement costs	\$10,028	See Online Appendix Table B19 for details
Court costs	\$746	See Online Appendix Table B19 for details
Corrections costs	\$30,160	See Online Appendix Table B20 for details
Total costs to taxpayer	\$40,934	
Costs per removal to victims	\$85,628	See Online Appendix Table B21 for details

Notes. The table presents estimates of savings and costs to taxpayers resulting from each SSI removal, and costs to victims per removal. “SSI benefit savings” indicates cost savings to taxpayers from spending less on SSI from 1997 to 2017. “Medicaid savings” indicates cost savings to taxpayers from spending less on Medicaid from 1997 to 2013 (since state Medicaid expansions under the Affordable Care Act began in 2014). “Tax revenue” indicates the increase in tax revenue from higher earnings assuming a 10% average tax rate. “Enforcement costs” indicate the cost of more police investigations, and “court costs” indicate the cost of more prosecutions. “Corrections costs” indicate costs to the government from increased time spent in prison, jail, probation, and parole. “Costs to victims” indicates the increased number of offenses times estimates of victim costs for each offense type. See [Online Appendix A](#) for details.

On the cost side, SSI removal increases criminal activity and thereby increases the costs of enforcement and incarceration for taxpayers. Using our IV estimates, we calculate the enforcement and court cost per removal from police investigations and prosecutions, amounting to \$10,800 per removal over those 21 years. Taxpayers also pay more for incarceration. Over the 1997–2017 time period, each SSI removal increases prison time by 227 days, jail time by 70 days, probation time by 349 days, and parole time by 73 days. Together these correctional costs amount to \$30,200

per removal.²³ Comparing the \$49,000 in savings to taxpayers to the \$41,000 in costs to taxpayers, we find that the additional costs of enforcement and incarceration nearly eliminate the savings to the taxpayer.

Of course, a cost-benefit analysis of SSI removal could also consider the costs and benefits to society at large. In particular, SSI removal creates negative externalities on the victims of crime and on the broader public who may take more precautionary measures against crime. We calculate an estimate of victim costs from the increase in crime resulting from SSI removal. Using conservative assumptions and estimates detailed in [Online Appendix A](#), we estimate victim costs at \$85,600 per removal. Victim costs dwarf both the government savings from SSI and the government costs from charging and incarcerating offenders. We use the marginal value of public funds proposed by [Hendren and Sprung-Keyser \(2020\)](#) to put these figures in a single framework. The MVPF compares recipient willingness to pay for the program to the cost to the government and society of funding the program. First, we calculate the MVPF of the SSI program ignoring increases in criminal activity. Recipient willingness to pay for SSI includes the SSI and Medicaid benefits from [Table II](#) (total of \$46,100), while the cost to the government includes SSI and Medicaid but also lost tax revenue from reduced earnings (total of \$49,100); the ratio of these is 0.94. Next, we calculate the MVPF of SSI including taxpayer costs of enforcement and incarceration. The recipient willingness to pay remains the same (total of \$46,100), but the costs to the government are now partially offset by reductions in enforcement and incarceration costs from [Table II](#) (total of \$8,200); the ratio increases to 5.6. Finally, we calculate the MVPF of SSI including both the taxpayer costs of enforcement and incarceration and negative externalities to the victims of crime. Now willingness to pay for the program increases by the victim cost amount of \$85,600 from [Table II](#) (for a total of \$131,800), while the costs to the government remain the same (total of \$8,200); the ratio increases to 16.1. In summary, the MVPF of SSI increases from 0.94 to 16.1 upon

23. It is possible that SSI removals also lead to increases in federal crimes like interstate drug trafficking, resulting in enforcement, prosecution, and correctional costs incurred by the federal government, but these are outside the scope of CJARS and consequently unaccounted for in this exercise.

incorporating the program's effects on criminal justice involvement.²⁴

VI. CONCLUSION

In labor economics, the income effect associated with welfare programs traditionally refers to changes in employment and earnings induced by welfare benefits. In this article, we use a novel linkage between SSI records and crime records to document an alternative income effect: criminal activity. We show that even as some young people respond to losing SSI at age 18 by working more in formal employment in adulthood, a larger fraction respond by engaging in illicit income-generating activity, including theft, burglary, fraud/forgery, and prostitution. As they progress through adulthood, youth who have been removed from SSI appear to specialize in either illegal activity or formal employment to recover the SSI income that they have lost. Illegal income-generating activity leads to higher rates of incarceration, especially for groups with a high baseline incarceration rate, including Black youth and youth from the most disadvantaged families.

To what extent do our results on welfare payments to low-income youth with disabilities speak to the effects of welfare payments more broadly—for example, the 2021 Child Tax Credit expansion or a universal basic income? At least two factors are relevant to this assessment. The first is the extent to which the population of children receiving SSI benefits reflects the low-income child population more generally. We find that the SSI loss increases criminal justice involvement for youth with a wide range of disabilities, rather than being concentrated among youth with particular types of mental or intellectual disabilities. In fact, the largest effects are for youth with physical conditions—often conditions like asthma or cancer that may improve in adulthood—which suggests that our results may apply more broadly to low-income children. Moreover, the age 18 removal instrument estimates a local average treatment effect for youth with the least severe medical conditions who are deemed to have substantial capacity to work. This group is more similar to the general population than

24. Note that even though the distinction between extensive- and intensive-margin crime responses might matter for the welfare of the SSI recipient (as in [Section V.C](#)), this distinction does not enter into the MVPF calculation because it does not affect government costs or victim costs.

the average youth receiving SSI. A second issue is the extent to which the effects of losing SSI benefits after having received them as a child are symmetric to the effects of gaining cash benefits. Generalizing from our setting requires ruling out an endowment effect from receiving SSI benefits in childhood—for example, that receiving SSI somehow discouraged achievement and primed children for criminal behavior in adulthood. Further research in this area will be important to determining whether new or expanded general welfare programs would decrease crime by the same magnitude.

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SUPPLEMENTARY MATERIAL

Supplementary material is available at the *Quarterly Journal of Economics* online.

DATA AVAILABILITY

Code replicating the tables and figures in this article can be found in [Deshpande and Mueller-Smith \(2022\)](#) in the Harvard Dataverse, <https://doi.org/10.7910/DVN/HZKNIR>.

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