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# Fighting Crime in the Cradle

## The Effects of Early Childhood Access to Nutritional Assistance

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### ABSTRACT

*Using variation in the rollout of the Food Stamp Program (FSP), combined with criminal conviction data from North Carolina, we find that FSP availability in early childhood leads to large reductions in later criminal behavior. Each additional year of FSP availability in early childhood reduces the likelihood of a criminal conviction in young adulthood by 2.5 percent, with stronger effects for violent and felony convictions. These effects are substantially larger for nonwhites, consistent with their higher levels of FSP participation. The discounted social benefits from the FSP's later crime reduction exceed the costs of the program over this time period.*


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## I. Introduction

The annual cost of crime in the United States is more than \$2 trillion, roughly 17 percent of GDP.<sup>1</sup> This estimate may even understate the true cost, as the psychological toll of crime on those not directly impacted, such as the families of victims and those incarcerated, is difficult to measure. Communities with high rates of victimization and incarceration, such as poor and Black communities, bear a disproportionate share of these countable and uncountable burdens. Despite the extraordinary and inequitably distributed costs, relatively little is known about the early life determinants of later criminal behavior. Most existing evidence focuses on contemporaneous environmental factors that might promote or inhibit the commission of crime. In contrast, research on the developmental factors that influence the likelihood that an individual will become a violent criminal are relatively rare, with much of the evidence focused on the period of adolescence.<sup>2</sup> Perhaps as a result of data constraints that make it difficult to connect criminals credibly to their early childhood environments, only a handful of studies investigate the link between early childhood interventions and later criminal behavior.<sup>3</sup>

We make two primary contributions to this literature. First, we establish a previously unknown causal link between early childhood nutritional assistance and later criminal behavior. Second, we demonstrate that, during the early years of the Food Stamp Program (FSP), the size of the discounted external benefits generated by the nutritional assistance provided was larger than (i) the inefficiencies generated from the social transfer and (ii) the costs of the FSP program itself.

To investigate the link between early childhood nutritional assistance and later violent behavior, we take advantage of the introduction of the FSP, which was responsible for sharp reductions in severe hunger and malnutrition during the 1960s and early 1970s. Following the approach of Hoynes, Schanzenbach, and Almond (2016), we leverage the staggered rollout of the oversubscribed program to identify the effect of county-level Food Stamp availability in early childhood on later criminal behavior in young adulthood. Consistent with federal funding constraints dictating the movement of counties off the waiting list, baseline county characteristics and indexes of predicted changes in criminal behavior explain little of the variation in the timing of FSP adoption.

We estimate the effect of FSP availability on criminal behavior using individual-level administrative data for the universe of convicted criminals in North Carolina between 1972 and 2015. These administrative data are unique in that they include county of birth, allowing us to overcome a variety of measurement and endogeneity concerns that likely inhibited earlier attempts to investigate the effects of the early childhood environment on later criminal behavior. We combine these data with counts of births to construct county

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1. United States. Senate Committee on the Judiciary. "Hearing on the Costs of Crime." September 19, 2006 (testimony of Jens Ludwig).

2. For example, several studies show that improved education or education quality during adolescence results in reductions in later criminal behavior (Lochner and Moretti 2004; Deming 2011).

3. While there are a few evaluations of early childhood education interventions that include criminal outcomes (Heckman et al. 2010; Campbell et al. 2012), the most closely related to our work are studies investigating the effects of a Nurse-Family Partnership program (Olds et al. 1998) and an intervention targeted at children with high lead levels in their blood (Billings and Schnepel 2015). There is also some interesting recent work indicating a link between contemporaneous Food Stamp availability and criminal behavior (Carr and Packham 2019).

of birth by birth month cohort conviction rates, which we link with information on the availability of Food Stamps in each county and month.

We find that FSP availability in early childhood and in utero results in large reductions in later criminal conviction. *Each year* of FSP availability from conception to age five reduces the likelihood of a criminal conviction by age 24 by roughly 0.23 percentage points, a 2.5 percent reduction relative to the average rate of 9.2 percent. These estimates are robust to the inclusion of time-varying county-level controls for birth cohort composition and exposure to other War on Poverty Programs, as well as baseline county characteristics interacted with birth month fixed effects or Consolidated Statistical Area by birth month–year fixed effects. The legitimacy of the identification strategy is further bolstered by estimates that suggest that the availability of the FSP for birth cohorts in a particular county has no effect on the likelihood of conviction for individuals who likely moved to that county after the period of early childhood.

Using several data sets and approaches, we demonstrate that FSP availability during childhood has no effect on the likelihood of residing in one’s state of birth, supporting the notion that our results are not driven by differential migration of criminals out of North Carolina (where they might continue to commit crimes that do not show up in our North Carolina conviction data). Given the modest level of migration out of one’s state of birth, our estimates then provide a lower bound for the overall effect on criminal behavior.

Food Stamp Program availability in early childhood has strong long-run effects on the most costly crime types for society: violent and felony convictions. These effects are larger for nonwhites, consistent with the substantially higher levels of FSP participation in this population. Given the timing of the rollout in the late 1960s and early 1970s, the FSP likely accounts for part of the trend reversal in violent crime that began in the early 1990s (when heavily affected cohorts were in their early twenties).

We also use cohort-level data on arrests by agency from the FBI’s Uniform Crime Reports (UCR) to construct county by birth cohort by age arrest rates for a nation-wide sample. In addition to expanding our geographic scope, the UCR data allow us to explore the effect of childhood FSP availability on *arrests* rather than convictions. The estimates on violent crime arrests using these data are somewhat smaller than, but consistent with, the conviction estimates produced for North Carolina.

The robustness of estimates across geography, measures of criminal behavior, specifications, and data sets provides strong evidence that early childhood FSP availability reduced later criminal behavior. Using recent estimates of the social cost of Part I violent crimes, we calculate that the external benefit from crime reduction associated with the rollout of the program was between \$230 and \$510 billion (2015 dollars). Under conservative assumptions, the discounted external benefits exceed the welfare loss from the labor market distortions of the program during this time period, suggesting that this social transfer program may have improved efficiency on the grounds of cost savings from crime reduction alone.

## II. Evidence on the Origins of Criminal Behavior

Research on the developmental factors that influence the likelihood that an individual will become a criminal is limited, with many studies focusing on the period of adolescence. A number of evaluations of the Moving to Opportunity project provide mixed evidence on the effect of neighborhood environment on criminal

behavior (Sanbonmatsu et al. 2011).<sup>4</sup> Evaluations of the effect of family environment, specifically assignment to foster care, suggest it has an important role in affecting both contemporaneous and later criminal behavior (Doyle 2007, 2008).<sup>5</sup> A larger number of studies have focused on the relationship between education and crime, suggesting that additional years of schooling, increases in school quality, and changes in the composition of school peers can affect the likelihood of criminal behavior for several years later (Lochner and Moretti 2004; Deming 2011). Because these adolescent treatments occur at an age when individuals typically first decide to engage in crime, they may directly impact the costs or benefits of crime (for example, through direct exposure to crime or criminal peers) rather than impacting the individual's development. Indeed, Deming (2011) suggests peer effects as one explanation for the impact of school quality on criminal behavior. Bayer, Hjalmarrsson, and Pozen (2009) estimate criminal peer effects more directly, showing that juvenile offenders assigned to the same facility affect each others' subsequent criminal behavior. There is also recent evidence of the spillover effects of parent involvement with the criminal justice system. While Dobbie et al. (2018) find that adolescents who have an incarcerated parent are more likely to commit crime themselves, Billings (2018) finds positive short-term effects of incarceration.<sup>6</sup>

Research focusing on earlier periods of development is somewhat less common, with mixed evidence of effects across quasi-experimental studies of Head Start and small-scale randomized evaluations of more intensive preschool programs (Garces, Thomas, and Currie 2002; Deming 2009; Heckman et al. 2010; Campbell et al. 2012). Anders, Barr, and Smith (2020) attempt to reconcile the information contained in these studies and provides new evidence on the effects of Head Start and Smart Start using administrative crime data in North Carolina. They conclude that early childhood education reduces criminal behavior, with these effects primarily showing up in less affluent areas.

While the evidence on the effects of early education investments on later criminal behavior is limited and mixed, there is even less evidence on the impact of early shocks to health on later criminal outcomes. Perhaps most closely related to our work are evaluations of two early childhood health interventions: the Nurse-Family Partnership Program and the CDC's recommended treatment protocol for lead-poisoned children (Olds et al. 1998, 2007; Kitzman et al. 2010; Billings and Schnepel 2015).<sup>7</sup> The Nurse-Family Partnership Program is a prenatal to age two nurse home visitation program targeted at low-income first-time mothers. Early evidence from a small-scale randomized control trial indicates improved health outcomes for the children of participants, while longer-term follow-ups indicate substantial reductions in arrests, convictions, and

4. While early evaluations of the program found mixed evidence of effects on involvement with the criminal justice system at different ages (Katz, Kling, and Liebman 2001; Kling, Ludwig, and Katz 2005; Ludwig and Kling 2007), Sanbonmatsu et al. (2011) report no clear pattern of significant effects on arrests or delinquent behavior. Any effects that exist appear to be a result of current neighborhood conditions rather than the neighborhood where one grew up.

5. Specifically, Doyle (2008) finds that those on the margin of placement are two to three times more likely to enter the criminal justice system as adults if they are placed in foster care.

6. In a Norwegian context, Bhuller et al. (2020) find no effect of parental incarceration on a child's criminal activity or school performance.

7. Related to Billings and Schnepel (2015), there is also a growing literature on the effects of lead exposure on criminal behavior—for example, Aizer and Currie (2017) and Feigenbaum and Muller (2016).

parole violations for the children at ages 15 and 19 (Olds et al. 1998, 2007; Kitzman et al. 2010). Interestingly, there was no observed effect on high school graduation or measures of economic self-sufficiency. Billings and Schnepel (2015) evaluate a more recent intervention in Mecklenburg County for children whose blood tested above a certain lead threshold. The intervention, based on CDC recommendations, may have included a nutritional assessment and a referral to public assistance programs (for example, WIC) in addition to lead information and a medical evaluation. The authors find that individuals who received the intervention exhibited substantially less adolescent antisocial behavior, including reductions in the likelihood of arrest.

Both studies provide well-identified small-sample evidence of the effects of an early health intervention on a number of later outcomes, including criminal behavior. Interestingly, both interventions included nutritional assistance and/or advice as a component of a much broader intervention, suggesting nutrition may be part of the mechanism through which the observed treatment effects operate.<sup>8</sup> Indeed, one of the short-term effects observed in the Nurse-Family Partnership evaluation was an improvement in the mother's prenatal diet (Olds et al. 1998).

While the evidence linking early health interventions and later criminal behavior is limited, a growing body of recent work suggests a link between in utero and early childhood health and other later adult outcomes. This literature demonstrates a positive relationship between *neonatal* health and a variety of later educational, health, and labor market outcomes, as well as the long-term negative impact of poor health during childhood.<sup>9,10</sup> Hoynes, Schanzenbach, and Almond (2016) build on this literature by using the rollout of the FSP in the 1960s to demonstrate that access to Food Stamps in utero and early childhood improved later health outcomes (obesity, high blood pressure, and diabetes) and economic self-sufficiency for women. This result follows earlier work in which they demonstrate contemporaneous effects of the FSP on food consumption and birth weight (Hoynes and Schanzenbach 2012).

### III. Linking Food Stamp Availability in Early Childhood to Later Crime

While existing evidence suggests that the FSP improved short- and long-term health outcomes, it is less clear how the availability of Food Stamps might have affected criminal behavior. Is the potential effect on criminal behavior a direct

8. The link between early childhood nutrition and later violence has been suggested previously in correlational studies. For example, one recent study of a nationally representative sample found that individuals who experienced frequent hunger during childhood had significantly greater impulsivity, worse self-control, and greater involvement in violent behavior (Vaughn et al. 2016). However, we are unaware of any studies that have established that this is a causal relationship.

9. Studies comparing twins or siblings find that children with low birth weight have lower test scores, lower educational attainment, lower levels of health, higher rates of mortality, and lower rates of employment in adulthood (Currie and Hyson 1999; Black, Devereux, and Salvanes 2007; Johnson and Schoeni 2007; Figlio et al. 2014; Oreopoulos et al. 2008). Other studies find similar results using natural experiments featuring shocks to infant health, such as hospital desegregation in southern states in the 1960s (Chay, Guruyan, and Mazumder 2009) or exposure to the 1918 Influenza epidemic (Almond 2006).

10. Studies of the consequences of poor childhood health find that physical and mental health problems lead to reductions in cognitive ability (Salm and Schunk 2008) and earnings (Smith and Smith 2010), as well as an increased probability of receiving welfare (Currie 2009).

result of improvements in childhood nutrition or, instead, a general increase in family income that could operate through a variety of channels? In this section we outline the nutritional landscape that existed in the country prior to the rollout of the program, how the program affected this landscape, and how these changes may have contributed to later criminal behavior.

### *A. How Food Stamps Changed the Nutritional Landscape*

In a letter to Congress in 1969, President Richard Nixon declared, “there can be no doubt that hunger and malnutrition exist in America, and that some millions may be affected.”<sup>11</sup> While the country lacked the relevant data systems in the 1960s to quantify the problem, anecdotal evidence was abundant. The CBS documentary “Hunger in America” shocked the nation with images of babies dying of starvation in pockets of poverty across the country. In a nation-wide study of hunger, the Citizens’ Board of Inquiry into Hunger and Malnutrition in the United States estimated that at least ten million people were suffering from hunger and malnutrition. A team of doctors reported to Congress that the diets of children in many impoverished areas rarely contained food other than bread. In Mississippi, they estimated that half of the 2,000 children they observed were below the third percentile in weight, and in some counties they found the prevalence of anemia from malnutrition was more than 80 percent.<sup>12</sup>

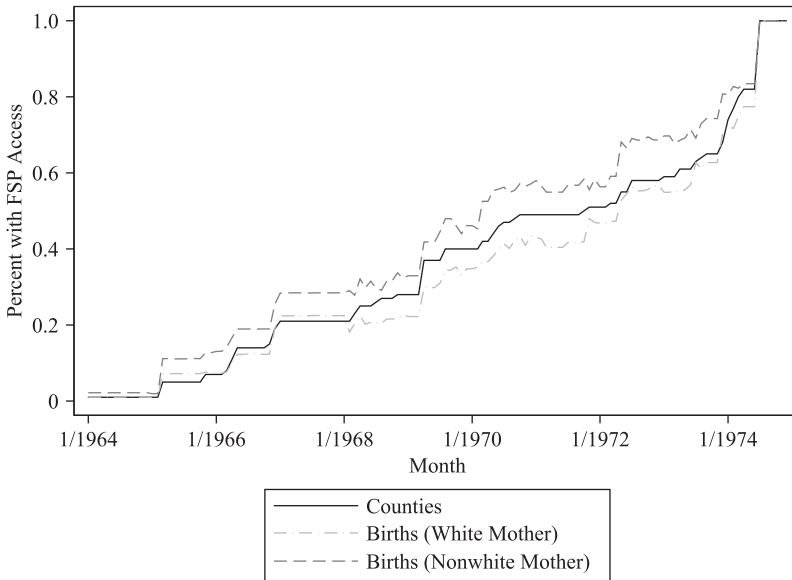
In 1964, President Lyndon Johnson signed the Food Stamp Act as part of his broader “War on Poverty.” The Act expanded a pilot program initiated by President Kennedy that allowed individuals to purchase stamps that could be used to buy food at a steep discount. The program grew dramatically county by county, from 380,000 participants in 43 counties in 1964 to 15 million in all counties in 1974. This gradual county-level rollout provides substantial variation in FSP adoption at the county–month level even within a state in a given year. Figure 1 illustrates this variation in the context of North Carolina, with an almost linear increase in the share of counties with a FSP between 1964 and 1974. While the adoption of the FSP was voluntary until 1974, accounts of the period suggest that high demand led the expansion of the program to be dictated by the availability of limited funding rather than the choices of local governments (Berry 1984, p. 36–37). This narrative is supported by Hoynes and Schanzenbach (2009) and Almond, Hoynes, and Schanzenbach (2011), who find that “county characteristics explain very little of the variation in adoption dates” across the country.<sup>13</sup>

In 1977, ten years after their first testimony to Congress on hunger and malnutrition in America, the same team of doctors returned and testified that “[i]t is not possible anymore to find very easily the bloated bellies, the shriveled infants, the gross evidence of vitamin and protein deficiencies in children that we identified in the late 1960s.” They observed that “the problem is not now primarily one of overt hunger and malnutrition,” but instead “more subtle manifestations of malnutrition.” The central role of Food Stamps in this turnaround is reinforced by the economics literature. Hoynes and Schanzenbach (2009)

11. Richard Nixon, “Special Message to the Congress Recommending a Program To End Hunger in America,” May 6, 1969. (<https://www.presidency.ucsb.edu/documents/special-message-the-congress-recommending-program-end-hunger-america>, accessed May 31, 2022).

12. Hearing on Hunger and Malnutrition in America July 11 and 12, 1967.

13. This conclusion is supported by a similar set of analyses conducted for North Carolina counties. We return to these analyses in Section V.



**Figure 1**  
*Fraction with Food Stamp Program in North Carolina*

Notes: Authors calculations using FSP administrative data obtained from Hoynes and Schanzenbach (2009) and aggregated county-month birth records by race from North Carolina. Dotted lines show the fraction of all North Carolina births in a given month to mothers of a given race that occurred in counties that had implemented a FSP.

demonstrate that access to Food Stamps increased food expenditures substantially among groups likely to receive benefits, while Almond, Hoynes, and Schanzenbach (2011) find that access to the FSP increased birth weight. These effects were larger for Blacks, who had greater rates of participation in the program.

### ***B. Only Nutrition or Income Too?***

While the evidence suggests that the FSP increased food consumption and improved short- and long-term health outcomes, the FSP also provided an income transfer to some families. The structure of the FSP in the early years can help shed some light on the relative importance of these two channels. Until amendments to the program in the 1970s, households participating in the FSP were required to purchase a set amount of food coupons sufficient for a “low-cost nutritionally adequate diet.”<sup>14</sup> As a result of this purchase requirement, theory predicts different impacts on a household’s food expenditures depending on a priori food consumption.

Under conservative assumptions and in the absence of the FSP, between 17 and 41 percent of households in this sample spent less than the purchase requirement on food ( $F < \bar{F}$ ), suggesting that a substantial fraction of eligible households would receive

14. Food Stamp Act of 1964, Pub. L. No. 88-525, § 7(a).

purely an increase in food from the program.<sup>15,16</sup> Overall, our calculations imply that the FSP resulted in a pure increase in food expenditures for a large share of participating households while increasing general purchasing power (including food consumption) for many others. This conclusion is consistent with a variety of estimates from the time period that suggest that households used 53–86 percent of food subsidy income for the purchase of additional food (Hoagland 1977).<sup>17</sup> While the FSP clearly had some effect on general purchasing power, the stated goal and the primary observed impact of the program was an increase in the quantity and quality of food consumed. This prompts the question: How does improved nutrition in early childhood affect criminal behavior in adulthood?

### *C. The Link between Childhood Nutrition and Crime*

Existing evidence suggests a number of potential channels through which nutritional assistance in early childhood could influence later criminal behavior either through effects on nutrition or through effects on family income. Among the potential nutrition channels, one possibility is that the availability of nutritional assistance leads to improvements in birth outcomes as a result of improved nutrition among mothers. Indeed, the criminology literature provides some evidence that low birth weight predicts violent criminal activity (but not nonviolent criminal activity), and Almond, Hoynes, and Schanzenbach (2011) demonstrate that the availability of the program did result in improvements in birth outcomes.<sup>18,19</sup> However, this mechanism is unlikely to explain the effects of access to nutritional assistance on children who were already born when the program became available.

Another potential nutrition channel is that nutrition improvements in early childhood lead to improvements in intermediate outcomes, such as better health, increased investments in education, or greater earnings potential, and that these changes raise the

15. See [Online Appendix D](#) for a more complete description of the theory and empirics behind these calculations.

16. Similar calculations suggest that households who would experience the program as a pure cash transfer, estimated as the fraction that spent more on food than the value of the food coupons they would receive under the FSP ( $F > \hat{F}$ ), make up 36–45 percent of FSP-eligible households.

17. The higher 86 percent figure comes from households surveyed between 1968 and 1972, while the 53 percent figure is from 1975, after the purchase requirement was decreased substantially. Hoynes and Schanzenbach (2009) also estimate positive effects of FSP availability on food consumption, with large implied increases in food expenditures (52.6 percent) for participating female-headed households, which have children with dramatically higher risk of becoming criminals. During this period female-headed households were almost always single parent households.

18. Tibbetts and Piquero (1999) describe the relationship between birth weight and violent criminal activity. The criminology literature also contains two descriptive relationships that hint at a possible effect: a link between low birth weight and behavioral problems (Chilcoat and Breslau 2002; Elgen, Sommerfelt, and Markestad 2002; Kelly et al. 2001; Levy-Shiff et al. 1994) and a link between behavioral problems and criminal activity (Moffitt, Lynam, and Silva 1994; Piquero 2001; Raine 2002; Raine et al. 1996).

19. We observe the negative relationship between FSP availability and low birth weight in North Carolina as well ([Online Appendix Table A1](#)). The availability of a FSP results in a reduction in the likelihood of a low birth weight of roughly 0.2 percentage points (2.1 percent) for all mothers, 0.05 percentage points (0.7 percent) for white mothers, and 0.5 percentage points (3.7 percent) for nonwhite mothers. These results are statistically indistinguishable from Almond, Hoynes, and Schanzenbach (2011) estimates for the effect of FSP availability on southern births by race. We also find large effects for mothers who are high school dropouts (3.5 percent).

opportunity cost of committing crime. In fact, Hoynes, Schanzenbach, and Almond (2016) suggest that the program had an effect on health outcomes.<sup>20</sup> While they found no improvements in education or earnings potential among men, who account for nearly 90 percent of violent offenders, a recent working paper (Bailey, Sun, and Timpe 2021) using Census data suggests positive effects on education and economic self-sufficiency. However, these effects are not significant for nonwhites, who are both more likely to be eligible for Food Stamps and more likely to be convicted of a crime. It thus seems unlikely that the employment channel would be the dominant pathway by which FSP availability reduces later criminal behavior, unless perhaps the effects are concentrated among white individuals.

A third possible nutrition channel is that improvements in early childhood nutrition have lasting effects on physiological functions that result in improved self-control and less aggressive and violent behavior. Moffitt (1993) and Moffitt, Lynam, and Silva (1994) suggest that the development of persistent offenders is a result of a complementarity between neuropsychological deficits developed in early childhood (potentially as a result of poor nutrition) and a social environment that fosters criminal behavior. Moffitt notes that both factors are more likely to be present in the context of severe poverty, a situation experienced by many Food Stamp-eligible families during the 1960s. A growing number of studies find correlations between malnutrition in early childhood and externalizing behavior (that is, physical aggression, rule breaking, etc.) in adolescence—not surprisingly, these behaviors are strong predictors of adult violence (Liu and Raine 2006; Galler 2013).<sup>21</sup> While the connection between nutrition and later behavior is not well understood, it is clear that malnutrition affects brain chemistry through decreased cell growth, alterations in neurochemistry, and an increase in neurotoxic effects (Liu and Raine 2006).<sup>22</sup>

While we view prenatal and early childhood nutrition improvements as a likely channel given the observed improvements in nutrition and subsequent health outcomes, there are other mechanisms through which Food Stamp availability could affect crime. The income transfer provided by the FSP may have resulted in increased parental involvement with or expenditures on children. In the same way that improved nutrition could contribute to better intermediate outcomes, so could other investments. Children may

20. In more recent years (1996–2003), East (2020) uses variation in FSP eligibility for immigrant families to find that early childhood access to the FSP leads to large improvements in health in later childhood and adolescence.

21. Further substantiating this link, a recent study of a nationally representative sample found that individuals who experienced frequent hunger during childhood had significantly worse self-control and greater involvement in violent behavior as adults (Vaughn et al. 2016). While these correlations are merely suggestive, randomized control trials in rats reveal a causal effect of malnutrition in early life in reducing playful social behavior and increasing aggression in adulthood; these effects appear to be magnified by the presence of other stressors in the rat's environment (for example, Tonkiss, Smart, and Massey 1987; Whatson, Smart, and Dobbing 1976; Levitsky and Barnes 1972).

22. A related physiological explanation is that changes in nutrition in utero and early childhood may have altered individual's ability to regulate blood sugar, which is correlated with self-control issues and violent behavior (for example, Gailliot and Baumeister 2007; Virkkunen and Huttunen 1982; Virkkunen 1986). Indeed, Hoynes, Schanzenbach, and Almond (2016) find evidence that FSP introduction dramatically reduces the incidence of metabolic syndrome, also known as "insulin resistance syndrome" as most of its defining symptoms are linked to issues with the regulation of blood sugar (via insulin), particularly among men.

have had access to better housing, neighborhoods, healthcare, or childcare as a result of the income transfer.

The income transfer may also have reduced parental stress, possibly through the reduction in mother's work hours observed by Hoynes and Schanzenbach (2012). This may have led to reductions in smoking, drinking, and abusive behavior, which may have direct effects on a child's neuropsychological development (through brain development) or may carry over to the child in terms of learned behavior, particularly during the formative period of early childhood.<sup>23</sup> Similarly, the many potential effects of changes in income may have generated the effects on health observed by Hoynes, Schanzenbach, and Almond (2016) and Aizer, Ferrie, and Lleras-Muney (2016).

## IV. Data

In order to measure the effect of FSP availability on criminal behavior we use administrative conviction data from the state of North Carolina. We combine these data with information on the number of births in counties over time to calculate rates of conviction for birth month cohorts across counties. We use information on the availability of Food Stamps within a county and month to calculate Food Stamp exposure for each birth month cohort. Finally, we link this exposure measure to county by birth month cohort "crime rates" to estimate the effect of Food Stamp availability on crime. We supplement our main analyses using categorical arrest counts from the FBI's Uniform Crime Reports (UCR).

### A. North Carolina Data

We obtained administrative data on all individuals convicted of a crime in North Carolina between 1972 and 2015 from the North Carolina Department of Public Safety. This administrative database contains information on the type of crime, including the statute of the offense and whether it was a felony, as well as the name, date of birth, gender, and race of the perpetrator. An important advantage of the North Carolina data over other state criminal databases is the inclusion of county of birth for each individual. Combining information on criminals' months and counties of birth with birth counts from the National Center for Health Statistics allows us to construct conviction rates for birth month cohorts of individuals born in North Carolina. For example, to generate the birth month cohort conviction rate for county  $c$  in January 1965, we divide the number of convicted individuals born in county  $c$  in January 1965 by the total number of individuals born in county  $c$  in January 1965. Panel A of Table 1 provides summary statistics. Roughly 9 percent of individuals born between 1964 and 1974 were convicted of a crime by age 24.<sup>24</sup> Looking by type of crime, 4 percent of individuals were convicted of a

23. It is even possible that FSP availability influenced the commission of crime by parents (as in Carr and Packham 2019) and that this resulted in "spillover effects" on the children. However, for this to explain our results, criminal spillovers would have to be particularly important between conception and age five. While possible, we are not aware of evidence for why this would be the case.

24. We focus on those 24 and younger for comparability with the UCR estimates (arrests are reported in bins for ages beyond 24) and because most violent crime is committed by younger individuals. North Carolina

**Table 1**  
*Summary Statistics of Conviction and Arrest Rates*

Variables	Mean (1)
<b>Panel A: North Carolina Data (Monthly)</b>	
Any conviction by age 24	0.090
Violent conviction by age 24	0.015
Property conviction by age 24	0.023
Felony conviction by age 24	0.040
Violent felony conviction by age 24	0.006
Property felony conviction by age 24	0.007
Observations	13,173
<b>Panel B: Uniform Crime Report Data (Annual)</b>	
Violent Part I arrests per 100 individuals	0.97
Property Part I arrests per 100 individuals	3.39

Notes: Panel A contains summary statistics for the North Carolina sample. Each observation corresponds to a birth county and birth month. The sample is restricted to cohorts born between January 1964 and December 1974. Mirroring FBI Part I definitions, violent crimes are defined only as offenses containing the words “murder,” “assault,” or “robbery” (rape is not included). Property crimes are defined only as offenses containing the words “burglary” or “larceny.” Panel B contains summary statistics for the Uniform Crime Report (UCR) sample. Each observation corresponds to a county, birth year, and age. The arrest data are restricted to cohorts of individuals aged 18–24. The sample is restricted to cohorts who were born between 1964 and 1974. There are 30,453 observations for violent crimes and 82,122 observations for property crimes.

felony, 1.5 percent were convicted of a violent crime, and 2.3 percent were convicted of a property crime by age 24.<sup>25</sup> While the data contain the universe of convicted criminals in North Carolina and allow us to observe counties of birth, they are limited in that they do not allow us to observe convictions for individuals who are born in North Carolina and then leave the state.<sup>26</sup>

results are robust to looking at rates of conviction by age 35 (results reported below). We are unable to look at younger ages (below 16) because juvenile convictions are not observed in the data.

25. Mirroring FBI Part I definitions, violent crimes are defined only as offenses containing the words “murder,” “manslaughter,” “assault,” or “robbery” (rape is not included). Property crimes are defined only as offenses containing the words “burglary,” “larceny,” or “theft.” Since all offenses are observed for each conviction, a single conviction may be coded as both a property crime and a violent crime if it involves offenses in both categories. These definitions are intended to capture the most serious crimes (with the greatest costs to society), while also creating a measure comparable to the FBI’s Uniform Crime Reporting data. By focusing on these serious crimes, we limit the possibility of measurement error due to definitional changes over time in lower level offenses (for example, reckless driving).

26. More than 78 percent of individuals born in North Carolina during this period reside in North Carolina between ages 18 and 24. This share is even higher (more than 80 percent) for those with the highest rates of criminal behavior (nonwhites and those with less than a high school degree). We explore the relationship between measures of childhood Food Stamp availability and the likelihood of living in one’s state of birth in Section V.B.4.

### B. Uniform Crime Reporting Data

We supplement our North Carolina analyses with analyses using the FBI's Uniform Crime Reporting (UCR) data. The UCR data focus on arrests, one step closer than convictions to the commission of crime, and cover a larger and more diverse set of counties than the North Carolina data. The UCR data contain the number of individuals arrested in each county and year, broken down by the crime category and the age and gender of the offender. We use the county by age arrest counts combined with birth counts from the National Center for Health Statistics to construct arrest counts by age per 100 individuals for birth cohorts of individuals born in each county in the UCR.<sup>27</sup>

We focus on Part I violent crimes (murder, robbery, and aggravated assault) and property crimes (larceny and burglary) for 1980–2000. Because rape and arson are inconsistently reported during the early part of our sample, our main results exclude these crimes.<sup>28</sup> We present summary statistics from the UCR data in Panel B of Table 1. There is roughly one violent arrest per 100 people age 18–24 years each year. The number of arrests for property crimes is substantially higher, with roughly 3.4 property arrests per 100 people age 18–24 years each year.

### C. Food Stamp Rollout Data

We link the North Carolina and UCR-constructed birth cohort crime rates with a measure of Food Stamp exposure in early childhood. Following Hoynes, Schanzenbach, and Almond (2016), we construct a measure of Food Stamp exposure in early childhood. For the Food Stamp exposure measure linked to the North Carolina conviction rates, we use the fraction of months from conception (nine months prior to birth) to age five that Food Stamps were present for birth month cohort  $t$  in county  $c$  (the same definition as Hoynes, Schanzenbach, and Almond 2016). For the Food Stamp exposure measure linked to the UCR-constructed crime rates, we use the fraction of years from age zero to age five that Food stamps were present.

## V. Estimation Strategy

To estimate the effect of Food Stamps on crime, we leverage within-county variation in the availability of the FSP generated by the rollout of the program in the 1960s and 1970s. Our basic specification is:

$$(1) \quad C_{ct} = \alpha_c + \lambda_t + X_{c60} * t\beta_1 + \gamma FS_{ct} + \epsilon_{ct}$$

where  $C_{ct}$  is a measure of the crime rate for individuals who were born in county  $c$  in birth cohort  $t$ , and  $\alpha_c$  and  $\lambda_t$  are birth county and birth cohort (birth year by birth month)

27. For example, if 50 18-year-old individuals were arrested in county  $c$  in 1980, we would divide 50 by the number of births in county  $c$  in 1963 and multiply by 100 in order to generate an arrest count per 100 individuals for the 1963 birth cohort in county  $c$ .

28. This inconsistency hints at broader concerns with the UCR data. In [Online Appendix B](#), we discuss these data quality concerns, detail our data restrictions and choices, and demonstrate the robustness of the results to a multitude of alternative choices.

fixed effects. Standard errors are clustered at the county of birth level. For the North Carolina estimates, the crime rate is the likelihood of criminal conviction by age 24, and birth cohorts  $t$  are defined at the month level.<sup>29</sup> Following Hoynes and Schanzenbach (2009) and Hoynes, Schanzenbach, and Almond (2016), we include the interaction of 1960 pretreatment county characteristics  $X_{c60}$  with time trends in our preferred specification. The county characteristics include the percent of people living in families making less than \$3,000 (1960 dollars), the percent living in urban areas, the percent Black, the percent younger than five years old, the percent older than 65 years old, the percent of land in farming, and the percent of employment in agriculture.  $FS_{ct}$  is the measure of Food Stamp exposure for individuals born in county  $c$  and in birth cohort  $t$ .

We are interested in the coefficient  $\gamma$ , which represents the effect of Food Stamp availability in early childhood on adult crime. The key identifying assumption is that, conditional on birth county and birth cohort fixed effects, Food Stamp availability  $FS_{ct}$  is uncorrelated with other factors that would lead a particular birth cohort to be more or less likely to commit crime. While a variety of anecdotal and prior empirical evidence suggest that this assumption is likely to hold, we provide additional support below relevant to our context.

### A. North Carolina Estimates

Our baseline results in Table 2 indicate that each additional year of FSP availability in early childhood (in utero to age five) reduces the likelihood of any criminal conviction by age 24 by 0.23 percentage points.<sup>30</sup> This is a sizeable reduction of 2.5 percent off of a base of 9 percent. We focus on those 24 and younger for comparability with the UCR estimates reported below (arrests are reported in bins for ages beyond 24) because most violent crime is committed by younger individuals and because migration becomes a greater concern at higher ages, but the estimates are robust to other age restrictions.<sup>31</sup> The estimates are robust to the inclusion of pretreatment (1960) county characteristics interacted with time trends, as in Hoynes, Schanzenbach, and Almond (2016). While our baseline inference relies on standard errors clustered at the county of birth level, our  $p$ -values are robust to an even more conservative approach: randomization inference.<sup>32</sup>

Figure 2 presents graphical evidence of the effects, demonstrating the relationship between the age at county FSP adoption and later criminal behavior. Given the nature of

29. Estimates are weighted by number of births in the county in 1964, but unweighted estimates do not differ significantly

30. This is obtained by dividing the point estimate by 5.75, where the 0.75 corresponds to the nine months in utero.

31. [Online Appendix Table A2](#) presents estimates for convictions among those 35 and younger. The estimates are broadly similar.

32. Under this procedure (essentially a large set of placebo assignments), we randomly assign the month and year of the introduction of the FSP in each county and estimate our basic specification. We do this 1,000 times. The distribution of these estimates is contained in [Online Appendix Figures A3 and A4](#). As can be seen in the figures, the estimates we observe are quite unlikely under random assignment. Our randomization inference  $p$ -values are similar to those obtained using our baseline approach.

**Table 2**  
*Foodstamps in Early Childhood and Rate of Crime Conviction in North Carolina by Age 24*

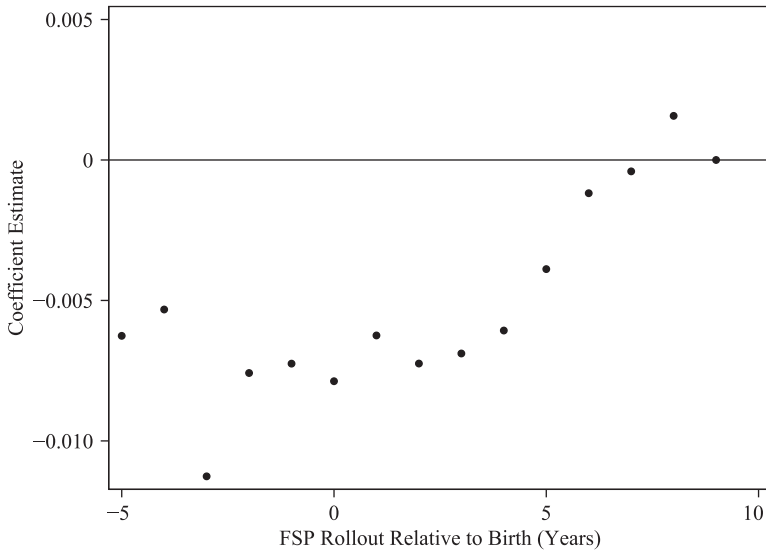
	Any (1)	Violent (2)	Property (3)
Any conviction			
FSP IU-5 exposure	-0.013** (0.007)	-0.005** (0.002)	-0.003 (0.003)
Mean	0.090	0.015	0.23
Felony conviction			
FSP IU-5 exposure	-0.007* (0.004)	-0.002* (0.001)	-0.001 (0.002)
Mean	0.040	0.006	0.007
Observations	13,173	13,173	13,173

Notes: Each cell represents a separate OLS regression with standard errors clustered at the birth county level in parentheses. Observations are at the birth county by birth month level and are weighted by the number of births in each county in 1964. The dependent variable is the fraction of individuals in a given birth county–birth month cohort who are later convicted of a crime or particular crime type in North Carolina by age 24. Columns indicate crime types (any, violent, property), and rows indicate severity (any conviction or felony). Mirroring FBI Part I definitions, violent crimes are defined only as offenses containing the words “murder,” “assault,” or “robbery” (rape is not included). Property crimes are defined only as offenses containing the words “burglary” or “larceny.” All specifications include birth county and birth month fixed effects, as well as baseline county characteristics interacted with a time trend in birth cohort. Baseline (1960) county characteristics include: percent of land in farming, percent of people living in families with less than \$3,000, percent of population in urban area, percent Black, percent less than age five, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to cohorts who were born between 1964 and 1974. Significance levels: \* $p < 0.10$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

treatment, the presentation is somewhat nonstandard.<sup>33</sup> The  $x$ -axis presents the number of years between the year of FSP adoption in a county and an individual’s year of birth. In other words, negative values represent individuals who were born after the adoption of a FSP within a county. Those individuals with a value of  $-1$  or less are “fully treated” in that a FSP was available in their county of birth from the time of their conception. As we move to the right, the age at FSP adoption increases (and therefore childhood FSP exposure decreases). As observed in the figure, the earlier that a FSP is adopted in an individual’s county, the larger the reduction in the likelihood of a criminal conviction. The reductions in criminal behavior are largest at or prior to conception and decrease between conception and age five, before leveling out.<sup>34</sup> Consistent with our estimates

33. We follow Hoynes, Schanzenbach, and Almond (2016) in this regard. All estimates presented are relative to FSP adoption at age ten. The start year of the North Carolina conviction data limit our ability to extend the  $x$ -axis to older ages.

34. That the estimates level after age five does not preclude the possibility of effects after age five as all effects are relative to FSP availability at age ten by construction. This is consistent with the presentation of Hoynes, Schanzenbach, and Almond (2016) and is necessary due to data constraints.



**Figure 2**  
*Event Study for Any Conviction by Age 24 (North Carolina Data)*

Notes: Circles indicate coefficients on indicator variables for a cohort's implied age at FSP introduction in a county (negative ages reflect cohorts that were born after FSP introduction). Observations are at the birth county by birth month level. The dependent variable is the fraction of individuals born in a particular county and birth cohort who were convicted of a crime by age 24. Regressions include birth month cohort and county fixed effects. Standard errors are clustered at the birth county level. Confidence intervals are excluded as all coefficient estimates are imprecisely estimated.

representing a causal effect of FSP availability, the timing of adoption prior to conception has no effect on the size of the reduction.

Moving across the columns of Table 2, we explore the effect of FSP availability on convictions for different categories of crime. We find large and statistically significant reductions in the likelihood of conviction for a violent crime, and smaller and nonsignificant reductions for property crime.<sup>35,36</sup> Moving to the second row of estimates, the reductions for violent felony convictions are especially large, with each year of Food

35. While this pattern holds for the white subsample, for the nonwhite subsample the estimates are similar for violent and property crime.

36. [Online Appendix Table A14 and A15](#) present similar results to Table 2 for individual crimes and alternative crime categories. Among individual violent crimes (following FBI Part I definitions but excluding rape), we find reductions in assaults and robberies that are statistically significant at the 10 percent level. Among individual property crimes, we find no statistically significant reductions. Among other crimes (not included in our categorization), we find statistically significant reductions in drug possession and breaking and entering. For alternative categories of crimes, we find a statistically significant reduction in "nonacquisitive" convictions (murder/manslaughter, assault, rape, drug possession, DWI, speeding, other driving violation) but no statistically significant reduction in "acquisitive" convictions (robbery, burglary, larceny/theft, breaking and entering, shoplifting, possession of stolen goods, drug sale, fraud).

Stamp availability in early childhood reducing the likelihood of violent felony conviction by 0.03 percentage points, or 6 percent (Table 2).<sup>37,38</sup>

### ***B. Threats to Internal Validity***

To interpret these estimates as the causal effect of Food Stamp availability, it must be the case that, conditional on county and year of birth fixed effects, the availability of FSP is unrelated to other factors that would affect the outcomes of children born to women who did and did not have the program available. While the evidence indicates large negative effects of Food Stamp availability on crime, we devote considerable attention to exploring alternative explanations.

#### *1. Endogenous Food Stamp Adoption by Counties*

Whereas the initial policy implementation occurred at the federal level, variation in the rollout of the policy occurred at the county level. Because we are controlling for variation over time (with birth cohort fixed effects) and differences between counties (with county fixed effects), the concern is that counties implemented the FSP when young children in those counties happened to be less likely to commit crimes as adults for some other reason.

If this type of endogenous policy implementation were occurring, we would expect to see some strong association between county characteristics and the timing of adoption. Hoynes and Schanzenbach (2009) and Almond, Hoynes, and Schanzenbach (2011) argue convincingly that this was not the case and that the rollout of the FSP was largely dictated by funding limits. Their finding is supported by anecdotal evidence that the main impediment to the implementation of the FSP was funding availability rather than the motivation of county officials. According to a review of the development of the program, it was “quite in demand, as congressmen wanted to reap the good will and publicity that accompanied the opening of a new project. At this time there was always a long waiting list of counties that wanted to join the program. Only funding controlled the growth of the program as it expanded” (Berry 1984, p. 36–37).<sup>39</sup>

37. We incorporate early childhood FSP access linearly for consistency and comparability with Hoynes et al. (2016). While we cannot reject that FSP access enters linearly, Figure 2 suggests that the effect of a year of access may be larger in very early childhood (in contrast to our analogous UCR event study, which suggests a linear effect). For completeness, we present estimates that allow the effect to differ by the timing of first access to FSP by using indicators for age at first access rather than a linear measure. The resulting estimates paint a similar picture. The magnitude of the effect of first FSP access in utero to birth is larger than first access between birth and age two, which is larger than the effect of first access between ages three and five ([Online Appendix Table A3](#)).

38. [Online Appendix Table A8](#) presents the results from Table 2 without weighting by the number of births. We find nearly identical results for violent and property crime, while the estimates for overall crime are smaller and less precise when including birth county covariates interacted with a time trend.

39. A related concern is that FSP adoption or program effects may be attenuated in counties with a preexisting commodity distribution program (CDP). Bitler and Figinski (2019) actually find the opposite: slightly larger FSP effects in counties with CDP. Our own criminal conviction estimates are very similar (but slightly smaller) when we restrict to North Carolina counties with a preexisting CDP ([Online Appendix Table A7](#)). Both sets of results are consistent with prior arguments made by Almond, Hoynes, and Schanzenbach (2011) that the attenuation associated with the CDP is likely small due to the distances faced by individuals picking up items, the infrequent and inconsistent distribution of items, and the very narrow set of commodities offered (Citizens' Board of Inquiry 1968).

In [Online Appendix Table A4](#) we explore the endogeneity of FSP adoption within North Carolina, regressing county characteristics on FSP timing. As with the national associations, we find that counties with larger Black populations in 1960 rolled out the FSP earlier than other counties, but that county characteristics explain little of the variation in FSP timing. We also explore whether the timing of adoption is correlated with either the predicted level or growth in crime during the period of the rollout. We construct an index of each county's future crime rate in 1974 and future crime rate growth (1964–1974) based on its characteristics in 1960.<sup>40</sup> We find no evidence of earlier FSP rollout in North Carolina counties that were predisposed to lower crime (or crime growth), based on their 1960 characteristics ([Online Appendix Table A4](#)).<sup>41</sup>

Consistent with this, the inclusion of 1960 county characteristics interacted with a trend in birth year has little impact on our estimates (Table 3). The results are also robust to the inclusion of controls that more flexibly allow for changes over time that vary across county characteristics or geography ([Online Appendix Table A6](#)). Specifically, the estimates are robust to the inclusion of 1960 county characteristics interacted with birth month–year fixed effects that allow for differential movements in crime across different types of counties. The estimates are similarly robust to the inclusion of Consolidated Statistical Area (CSA) indicators interacted with birth month–year fixed effects. This final approach identifies the effect of FSP availability off of differential availability of a FSP program within a CSA.

## 2. Other War on Poverty Programs

The rollout of FSP was one of several policy changes advanced by the Johnson administration as part of the broader War on Poverty. To the extent that the other policy changes coincided with the availability of FSP, we could be capturing the effect of these policies and not the effect of FSP. We address this possibility by controlling for access to War on Poverty programs with county-level variation. Specifically, we include indicators for the availability of Head Start at age four, Women and Infant Children (WIC) at birth, and a Community Health Center at birth for each birth county by month cohort. In Table 3, Columns 4 and 8 show that our results do not change qualitatively when including these additional controls.<sup>42,43,44</sup>

40. Specifically, we regress the crime measures on baseline county characteristics and use those coefficient estimates to predict the crime measure for each county.

41. We present the relationship between county characteristics, including the predicted crime rate and growth (based on county characteristics), and the timing of FSP adoption graphically in [Online Appendix Figures A6 and A7](#). As with our regression estimates, there is little relationship between county characteristics and the timing of adoption, supporting the validity of our identification strategy. We find similar results when you use the *order* of FSP adoption rather than the month of FSP adoption (see [Online Appendix Table A5 and Figures A8 and A9](#)).

42. Table 3 uses the 1968–1974 period (when natality data is available), but the results are similar for 1964–1974.

43. [Online Appendix Table A11](#) shows results for our preferred specification where exposure to individual War on Poverty programs are used as the dependent variable. If anything, these results suggest that exposure to FSP was associated with *less exposure* to these other programs.

44. One might similarly be concerned that a number of other major policy changes, such as desegregation, legalizations of abortion, increases in prison populations, the removal of lead from gas, or increases in compulsory schooling ages contributed to the observed estimates. However, the timing and geographic nature (most were statewide) of these changes make them unlikely to account for our results.

**Table 3**  
*FSP in Early Childhood and Rate of Crime Conviction in North Carolina: Robustness*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any conviction										
FSP IU-5 exposure	-0.019*** (0.008)	-0.014* (0.008)	-0.014* (0.008)	-0.011 (0.008)	-0.013*** (0.007)	-0.012* (0.006)	-0.013* (0.007)	-0.013* (0.008)	-0.013* (0.008)	-0.17** (0.009)
Violent conviction										
FSP IU-5 exposure	-0.007*** (0.003)	-0.006* (0.003)	-0.005* (0.003)	-0.003 (0.003)	-0.005** (0.002)	-0.004** (0.002)	-0.005** (0.002)	-0.006** (0.003)	-0.006** (0.003)	-0.006** (0.003)
Property conviction										
FSP IU-5 Exposure	-0.003 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.003)	-0.003 (0.003)	-0.003 (0.002)	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)	-0.005* (0.003)
Observations	13,173	8,373	8,332	7,160	13,173	13,173	8,373	8,373	8,332	7,160
Birth years: 1964–1974	Y	N	N	N	Y	Y	N	N	N	N
Birth years: 1968–1974	N	Y	Y	Y	N	N	Y	Y	Y	Y
Birth county chars. (1960) × Trend	N	N	N	N	Y	Y	Y	Y	Y	Y
Addl. birth county chars. (1960) × Trend	N	N	N	N	N	Y	N	Y	Y	Y
County natality chars. (Monthly)	N	N	Y	Y	N	N	N	N	Y	Y
WOP measures	N	N	N	Y	N	N	N	N	N	Y

Notes: Each cell represents a separate OLS regression with each row denoting a different dependent variable and each column denoting a different specification. The dependent variable is the fraction of individuals in a given birth county–birth month cohort who are later convicted of a crime or particular crime type in North Carolina by age 24. All specifications include birth county and birth month fixed effects. Baseline (1960) birth county characteristics include: percent of land in farming, percent of people living in families with less than \$3,000, percent of population in urban area, percent Black, percent less than age five, percent greater than age 65, and percent of employment in agriculture. “Additional birth county chars.” (also interacted with a trend in birth cohort) include population density, median income, median education, percent of adults with less than five years education, unemployment rate, per capita government expenditure, and Democratic vote margin in 1960 presidential campaign. Observations are at the birth county by birth month level and are weighted by the number of births in each county in the initial year of the sample period. The sample is restricted to cohorts who were born 1964–1974 or 1968–1974 as noted. The latter sample enables the inclusion of time-varying county characteristic controls (birth county by birth month level) constructed from natality files. These “County natality chars.” include mean mother’s age, fraction of births to married parents, fraction white births, and fraction of births with an attending physician in a hospital. War on Poverty (WOP) controls include access to WIC (at birth) and Head Start (at age four), as well as per capita expenditures on Public Assistance Transfers, Medicaid, Community Health Centers, and Community Action Agencies. Standard errors clustered at the birth county level are in parentheses. Significance levels: \* $p < 0.10$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

### 3. Falsification Test

While the estimates are robust to the inclusion of controls for cohort composition and the availability of early childhood programming, it is still possible that there were subsequent changes in a county that affected crime rates, such as changes to its criminal justice system, that are correlated with but not caused by the timing of a county's FSP adoption. In this case, we would expect to estimate a substantial reduction in convictions for anyone assigned an early childhood FSP exposure based on their birth cohort and county of residence (in adulthood), *even if their actual childhood FSP exposure was very different*. This logic suggests a natural falsification exercise using individuals who reside in North Carolina but were not born there (ensuring a substantial difference between their assigned and actual FSP exposure). As these individuals moved to North Carolina at some point after birth, the assigned FSP exposure measure (which we assign using their adult county of residence) is unlikely to be strongly correlated with their actual FSP exposure, but the measure is likely correlated with other county factors that affected rates of criminal conviction.<sup>45</sup> If something other than FSP exposure is driving our main results, we might expect to see similar effects show up for individuals born outside of North Carolina who now live in North Carolina counties.<sup>46</sup>

Online Appendix Table A17 presents estimates of Equation 1, where the conviction rate for county  $c$  and birth cohort  $t$  is the number of individuals born outside of North Carolina in year  $t$  who are convicted in county  $c$  (by age  $a$ ) divided by the total number of individuals born outside of North Carolina in year  $t$  who reside in county  $c$  at age  $a$ .<sup>47</sup> While generally imprecise, we find that the estimated effects on conviction by age 24 are small and actually positive in sign.<sup>48</sup> Extending the conviction window to age 30 (and therefore allowing more time for migration) yields negative estimates that are very small in magnitude and insignificant. Combined, these results suggest that factors occurring after the period of early childhood (but correlated with early childhood Food Stamp availability) are not responsible for our main results.

45. Of course it is possible that counties with somewhat earlier FSP availability attracted different types of individuals or families from outside NC to move there in the ensuing years. While this seems unlikely, we have been unable to develop a direct test due to data limitations. We proceed with this caveat in mind.

46. While peer effects or general equilibrium effects might result in some effect on those born outside North Carolina, we would expect the estimates to at least be smaller for this group.

47. We construct the conviction rate using population counts by age, county, and year from SEER, along with the fraction of county residents born out of state from the 1990 census. It is not possible to construct the conviction rate in the same way as Table 2 because birth counts by month, birth place, and adult residence are not available.

48. One potential explanation for the slight positive coefficients is that more disadvantaged (or crime prone) individuals moved across state lines to take advantage of the availability of these programs. If people more prone to commit crime were leaving their state to obtain Food Stamps elsewhere earlier (which we believe is unlikely given other evidence discussed below), presumably they would be leaving counties in which the Food Stamp program arrived later (and thus reducing in-state crime among individuals born in those counties and not the ones where Food Stamps arrived earlier). This should bias our results against finding the large negative effects we present in the core tables of the paper. In other words, the slight positive coefficients in Online Appendix Table A17 suggest that the negative effects on crime that we estimate may in fact be understated, because we are potentially missing criminal behavior for individuals who lacked Food Stamp availability in early childhood and moved out of their state of birth to access it.

#### 4. *Differential Migration out of North Carolina*

Another threat to validity is that FSP availability differentially influences the likelihood that an individual remains in North Carolina (and thus shows up in our North Carolina conviction data).<sup>49</sup> If FSP availability causes would-be criminals to leave the state, our estimates could reflect a shift in crime out of North Carolina rather than a reduction in overall crime.

In [Online Appendix Table A18](#), we explore the relationship between measures of childhood Food Stamp availability (at the state of birth by birth cohort level) and the likelihood of living in one's state of birth. Across a variety of approaches and subsamples, our estimates indicate a small and (with one exception) insignificant relationship between childhood Food Stamp availability and the likelihood of living in one's state of birth. Even if we assume the point estimates reflect a genuine effect on migration, to account for our criminal conviction estimates it would have to be true that between 52 and 220 percent of the additional individuals leaving the state would have been convicted of a crime had they stayed, implying a conviction rate 13–55 times the sample mean.<sup>50</sup> The point estimates for nonwhites, who have much higher rates of participation in the FSP than whites, indicate that, if anything, the FSP program may have caused individuals to be slightly more likely to remain in their state of birth (biasing us against finding a negative effect on crime).

While we focus on this “aggregate” check on migration because it is relatively well powered to rule out substantial migration effects, we are also able to provide several other pieces of evidence using individual-level data that suggest that differential out-migration is not at play. Specifically, we conduct two checks associated with the UCR analyses that suggest no differential out-migration from one's county of birth. First, we use the NLSY 79 and our basic specification and find no evidence that Food Stamp availability affects whether an individual moves out of their county of birth ([Online Appendix Table A19](#)). Similarly, there is no evidence in the NLSY 79 that Food Stamp availability affects the likelihood of moving out of one's state of birth ([Online Appendix Table A20](#)).<sup>51</sup> Second, we use the North Carolina conviction data to show that the effects on convictions are present both for one's commission of crime in their county of birth and their commission of crime in other counties in North Carolina ([Online Appendix Table A21](#)). While this doesn't rule out migration out of the state of birth, it does further suggest that the effects we find are not a result of differential migration out of one's county of birth.

49. A related concern is that the composition of births was related somehow to the availability of a FSP (perhaps through migration or family planning) and that these changes are responsible for the estimated effect. However, if the availability of a FSP attracted soon-to-be mothers whose children would not become criminals, there would need to have been a 23–27 percent increase in births to drive our results ([Online Appendix Table A16](#) indicates no FSP effect on number of births). Additionally, we find that controlling for “county natality characteristics” has little effect on our point estimates (Table 3). In [Online Appendix Table A12](#), we also show the associations between subsequent FSP exposure and mother's characteristics (at the time of birth), using our preferred specification but where these characteristics are now the dependent variables. With the exception of births occurring in a hospital (a variable with a mean of 0.98) we find no statistically significant estimates.

50. In the North Carolina data, rates of conviction for individuals who move to North Carolina are similar to those who are born there.

51. We have conducted similar analyses using the much larger sample sizes provided by restricted ACS data that contain individuals' counties of birth. While the specific estimates are undergoing disclosure review, the evidence is consistent with the conclusions we have reached using the NLSY 79 data, with much greater precision. The larger sample sizes allow us to investigate this question only for individuals born in North Carolina. The resulting estimates are consistent with the other evidence we have provided.

**Table 4**

*Foodstamps in Early Childhood and Rate of Crime Conviction in North Carolina: Heterogeneity*

	Any (1)	Violent (2)	Property (3)
White			
FSP IU-5 exposure	-0.005 (0.005)	-0.003** (0.001)	-0.002 (0.003)
Mean	0.060	0.007	0.015
Observations	9,737	9,737	9,737
Nonwhite			
FSP IU-5 exposure	-0.038** (0.017)	-0.009* (0.005)	-0.011** (0.005)
Mean	0.143	0.032	0.037
Observations	9,795	9,795	9,795

Notes: Each cell represents a separate OLS regression with standard errors clustered at the birth county level in parentheses. Observations are at the birth county by birth month level and are weighted by the number of births in each county in 1964. The dependent variable is the fraction of white or nonwhite individuals in a given birth county–birth month cohort who are later convicted of a crime or particular crime type in North Carolina by age 24. All specifications include birth county and birth month fixed effects, as well as baseline county characteristics (1960) interacted with a trend in birth month. Baseline (1960) birth county characteristics include: percent of land in farming, percent of people living in families with less than \$3,000, percent of population in urban area, percent Black, percent less than age five, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to cohorts who were born between 1964 and 1974 in the 75 counties where the number of births is available by race prior to 1968. Significance levels: \* $p < 0.10$ ; \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

### C. Effect Size and Heterogeneity

As with much research on early childhood interventions, our estimated effects are substantial (Olds et al. 1998; Kitzman et al. 2010; Heckman et al. 2010; Garcés, Thomas, and Currie 2002). Our preferred estimates indicate reductions in the likelihood of any conviction of 1.3 percentage points, assuming FSP availability throughout childhood.<sup>52</sup> To put our results in the context of recent literature with similar outcome measures, these estimates imply treatment-on-the-treated (TOT) effects of roughly 7.6 percentage points.<sup>53</sup>

52. These are likely underestimates of the overall effect on convictions as only four out of five individuals born in North Carolina reside in the state by age 24. The roughly equal rates of criminal conviction of individuals residing in their state of birth and those who move out of their state of birth (implied by Table 2 and [Online Appendix Table A21](#)) suggests scaling our point estimates by 1/0.8, implying a reduction in the likelihood of any conviction of roughly 1.6 percentage points.

53. We estimate Food Stamp participation rates of families with children five and under of approximately 17 percent (authors' calculations using number of children, Food Stamp participation, and family weights from family-level PSID for 1976–1979).

While it is not straightforward to construct comparable measures of criminal behavior across studies, our implied TOT effects are just under half the size of the effects estimated for the Nurse-Family Partnership by age 19 (16 percentage points on likelihood of conviction or arrest). The effects are also just under half of the effects estimated for the full set of services provided by a more recent intervention targeted at children with high blood lead levels (17 percentage points on likelihood of arrest).<sup>54</sup> Consistent with our findings, evaluations of both interventions estimated substantially stronger effects on violent offenses. Interestingly, both interventions included nutritional assistance and/or advice, suggesting that nutrition may be part of the mechanism of effect for all three studies.<sup>55</sup>

If we split our estimates by race, we find substantially larger effects for nonwhites (Table 4).<sup>56,57,58</sup> This is reassuring, as the participation rate of nonwhite families (36 percent) was substantially higher than that of white families (12 percent) during this time period. Scaling our nonwhite estimates by their relatively higher rates of Food Stamp participation implies TOT reductions in the likelihood of conviction of 11 percentage points. While estimates by race are unavailable for studies linking early childhood health or health interventions and later criminal behavior, these estimates are similar to the effects of Perry Preschool, which enrolled Black children. Early evaluations of the effects of Head Start, an early childhood program with nutritional components, also found larger effects (12 percentage points on the likelihood of conviction) for Black participants (Garces, Thomas, and Currie 2002). Anders, Barr, and Smith (2020) and Smith (2020) find similar results in more recent years for Smart Start in North Carolina and Universal Pre-K in Oklahoma, respectively.

## VI. UCR Estimates and Welfare Calculations

Estimates from the North Carolina data provide convincing evidence that FSP availability in early childhood reduces the likelihood of a criminal conviction later in life; however, the North Carolina estimates are limited in terms of their geographical scope. Furthermore, conviction is several steps removed from the commission of crime. If the behavior of courts, sentencing guidelines, or the availability of legal aid changes, shifts in criminal behavior may not map perfectly into changes in conviction rates.<sup>59</sup> To address these limitations and to generate inputs for our welfare calculations, we turn to estimates from the UCR data.

The estimates in Columns 1 and 2 of Table 5 present the effect of having Food Stamps available in one's county for a larger fraction of early childhood (age 0–5) on the number

54. The less intensive set of services, primarily information on how to reduce lead exposure and eat better, produced effects of a similar size to our implied TOT estimates.

55. Indeed, one of the short-term effects observed in the Nurse-Family Partnership evaluation was an improvement in the mother's prenatal diet (Olds et al. 1998).

56. During this period in North Carolina, Blacks constituted more than 95 percent of the nonwhite population (1970 census).

57. Table 4 uses the subsample of 75 counties for which we observe county by race prior to 1968.

58. [Online Appendix Table A9 and A10](#) show largely similar results for a number of alternate specifications, though some specifications where the sample is limited to 1968–1974 yield smaller reduction estimates.

59. There is no reason to believe that these types of changes occurred or that they were correlated with the early childhood availability of a FSP.

**Table 5**  
*Foodstamps in Early Childhood and Part I Arrests (per 100 Individuals)*

Dependent Variable	Violent Crime (1)	Property Crime (2)	Murder (3)	Aggravated Assault (4)	Robbery (5)
0–5 FSP exposure	–0.151** (0.048)	–0.128 (0.091)	–0.032* (0.014)	–0.064** (0.030)	–0.042*** (0.014)
Observations	30,453	82,122	32,128	96,386	60,292
Mean	0.97	3.39	0.0427	0.559	0.195

Notes: Each column presents coefficients from a separate OLS regression with standard errors clustered at the county level in parentheses. Observations are at the county by birth cohort by age level and are weighted by the number of births in each county in 1964. The dependent variable is the number of individuals per 100 within a given county cohort who are arrested at a particular age. All specifications include birth year, age, and county fixed effects, as well as baseline county characteristics (1960) interacted with a trend in birth year. Baseline county characteristics include: percent of land in farming, percent of people living in families with less than \$3,000, percent of population in urban area, percent Black, percent less than age five, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to individuals age 18–24 unless otherwise noted. Sample restricted to agencies accounting for at least 20 percent of a county's population. Sample sizes vary due to differences in reporting across offenses. Significance levels: \* $p < 0.10$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

of arrests per 100 people in a county and birth cohort at a particular age. For example, the estimate in Column 1 indicates that an additional year of FSP availability during early childhood results in a reduction of 0.03 arrests per 100 people, or roughly 3 percent.<sup>60</sup> These estimates are not directly comparable to the North Carolina estimates because they measure the impact of Food Stamps availability on the number of arrests per 100 people at a specific age, whereas the North Carolina estimates measure the impact on the likelihood that an individual is convicted by age 24. Still, it may be somewhat surprising that the percentage effect sizes for violent arrests per year of exposure (3 percent) are smaller than the corresponding effects in the North Carolina conviction data (6 percent). This is potentially explained by measurement error in our treatment variable, which attenuates the estimates of the effect of Food Stamp availability (classic errors-in-variables).<sup>61</sup> More specifically, if individuals are unlikely to remain in their county of birth, we may be incorrectly assigning our measure of Food Stamp exposure to many individuals.<sup>62,63</sup>

60. As with the North Carolina estimates, the UCR estimate  $p$ -values are robust to randomization inference (Online Appendix Figure A5). Sample sizes vary across outcomes due to differences in reporting across offenses.

61. Alternatively, there may be different effects on the amount of criminal behavior versus the commission of any crime at all.

62. Indeed, if we scale our arrest estimates by the fraction of individuals who will remain in their birth county by age 18 (about 50 percent), the percentage reductions implied are quite similar to our estimates from the North Carolina data (fraction remaining in county of birth produced using NLSY 79 data). Scaling by the ratio of estimates using county of arrest versus county of birth in the North Carolina data (1.5 to 4) produces similar results.

63. While migration potentially explains the difference in magnitudes between the North Carolina and UCR estimates, it also hints at a potential validity concern of the UCR estimates if Food Stamp availability affects the mobility of individuals on the margin of committing crimes. In [Online Appendix Table A21 and A19](#), we show

What implications do our estimates of the impact of the FSP on later violent crime have for social welfare? We assess the welfare gains by applying estimates of the dollar value of each offense's social cost and discount the stream of future cost reductions associated with each FSP year for the period 1964–1974 (see [Online Appendix C](#) for details). Table 6 presents the resulting back-of-the-envelope calculations of social welfare gains for various choices of discount rate and estimates of the social costs of crime, counting only the effects on crimes committed by 18–24-year-olds. With a 3 percent discount rate, the benefits of the FSP from 1964 to 1974 are estimated at \$292 billion (2015 dollars), based on the social costs of crime estimates of McCollister, French, and Fang (2010), or \$131 billion (2015 dollars), based on the lowest estimates for each offense in the recent literature.

Economic theory suggests that the rollout of the FSP likely also had welfare costs through two contemporaneous distortions. First, while program benefits represent transfers from one group to another that should not themselves reduce social welfare, program administration costs and utilization of government revenue raised from distortionary taxes could lead to efficiency losses from these transfers. [Online Appendix Table A22](#) shows back-of-the-envelope estimates of these welfare losses in each program year, which total \$14–35 billion (2015 dollars) during the rollout period (see [Online Appendix C](#) for details). Second, the FSP could reduce efficiency through distortions in the labor market. This would occur if Food Stamp receipt disincentivizes work for recipients. Hoynes and Schanzenbach (2012) investigate precisely this question. They find that FSP access reduces annual work hours, but only for female household heads (with children). Hoynes and Schanzenbach report the effect of FSP access on the earnings of these female household heads, but the loss in efficiency may exceed the earnings loss if labor demand is not perfectly elastic. [Online Appendix Table A22](#) shows back-of-the-envelope estimates of deadweight loss from the contemporaneous labor market distortions of the FSP in year  $\tau$ , which total \$63–80 billion (2015 dollars) over the rollout period (see [Online Appendix C](#) for details).

Table 7 compares our back-of-the-envelope estimates of the welfare gains from the FSP's subsequent crime reduction effects on 18–24-year-olds to our estimates of the contemporaneous welfare losses from the program's administrative costs, use of distortionary tax revenue, and labor market distorting incentives. Broadly speaking, welfare gains exceed losses with some sensitivity to parameter choice.<sup>64</sup> These results suggest that a complete accounting of the efficiency impact of the FSP rollout would likely show an improvement, particularly after considering other potentially beneficial contemporaneous effects of the FSP (for example, health) or potential future effects on margins other than crime (for example, health, education).

An alternative approach to analyzing the welfare impact of a policy, discussed in detail in recent work by Hendren and Sprung-Keyser (2019), is to calculate the

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that Food Stamp availability has no statistically significant effect on whether someone resides outside of their birth county and that, within North Carolina, Food Stamp availability actually caused individuals to be less likely to commit a crime in counties *other* than their birth county (as well as in their birth county). This suggests that the UCR estimates likely provide a lower bound for the true effect.

64. [Online Appendix Table A23](#) reports the same estimates, but allows for welfare gains from subsequent crime reduction for 13–40-year-olds. This table shows welfare gains exceeding welfare losses from the FSP for all parameter choices.

**Table 6**  
*Estimates of Crime Reduction Welfare Gains from FSP (1964–1974) among 18–24-Year-Olds*

	Cost Estimate (\$ Million 2015)	Est. $\Delta$ Arrests (1,000s)	Est. $\Delta$ Crimes (1,000s)	Discounted Social Benefits (\$ Million 2015)			
				0%	3%	5%	7%
<i>McCollister, French, and Fang (2010) crime cost estimates</i>							
Murder	9.89	-50	-48	477,195	273,092	190,469	134,036
Robbery	0.05	-66	-224	10,423	5,965	4,160	2,928
Assault	0.12	-101	-192	22,598	12,933	9,020	6,347
			<b>Total:</b>	<b>510,216</b>	<b>291,990</b>	<b>203,650</b>	<b>143,311</b>
<i>Low crime cost estimates</i>							
Murder	4.56	-50	-48	220,176	126,004	87,882	61,844
Robbery	0.02	-66	-224	4,580	2,621	1,828	1,286
Assault	0.02	-101	-192	4,530	2,592	1,808	1,272
			<b>Total:</b>	<b>229,285</b>	<b>131,217</b>	<b>91,518</b>	<b>64,402</b>

Notes: Table shows back-of-the-envelope calculations of the discounted social benefits of later crime reduction from the 1964–1974 implementation of the FSP. Social cost estimates for each crime type (Column 1) are adopted from the preferred estimates of McCollister, French, and Fang (2010) and the lowest estimates from their literature review, both of which may be underestimates. The former estimates include victimization costs, criminal justice system costs, and the lost value of criminals' time, but do not include private expenditures on crime prevention. The latter estimates include only victimization costs. The estimates of the change in arrests due to FSP implementation (Column 2) are based on the coefficient estimates from Equation 1 for each offense (contained in Table 5). The change in arrests is converted to a change in offenses (Column 3) using the ratio of offenses to arrests for each offense type. This ratio is operationalized conservatively as the minimum of the annual ratio of the UCR national estimate of offenses known to the UCR national estimate of arrests for 1980–2000 for the given offense. For murder/manslaughter this ratio is less than one due to either the UCR imputation process or a high rate of offenders per murder/manslaughter offense. This results in our estimated changes in murder/manslaughter arrests exceeding our estimated changes in murder/manslaughter offenses, potentially leading us to *underestimate* the social benefit from reductions in murder/manslaughter. Estimates of the discounted social benefit are produced by multiplying the dollar value of each offense's social cost by the change in offenses implied by our estimates, discounted using various social discount rates. See [Online Appendix C](#) for details.

**Table 7**  
*Welfare Change from FSP (1964–1974) in Millions \$2015—Transfer and Labor Market Losses vs. Crime Reduction Gains (18–24-Year-Olds)*

Social Discount Rate	Welfare Gain	Welfare Loss		Δ Welfare		Gain–Loss Ratio	
		(Min.)	(Max.)	(Min.)	(Max.)	(Min.)	(Max.)
<b>McCollister, French, and Fang (2010) crime cost estimates</b>							
0%	510,216	34,591	114,437	475,625	395,779	14.8	4.5
3%	291,990	34,591	114,437	257,399	177,553	8.4	2.6
5%	203,650	34,591	114,437	169,059	89,213	5.9	1.8
7%	143,311	34,591	114,437	108,721	28,874	4.1	1.3
<b>Low crime cost estimates</b>							
0%	229,285	34,591	114,437	194,695	114,848	6.6	2.0
3%	131,217	34,591	114,437	96,626	16,780	3.8	1.1
5%	91,518	34,591	114,437	56,927	–22,919	2.6	0.8
7%	64,402	34,591	114,437	29,812	–50,034	1.9	0.6

Notes: The table presents the estimates of welfare gains from crime reduction due to FSP implementation from Table 6 and the range of estimates of the welfare losses due to the program from Online Appendix Table A22. Welfare losses are the sum of the FSP’s contemporary work disincentives, program administrative costs, and distortionary taxes needed to raise government revenue. “Min.” and “Max.” column titles correspond to the minimum and maximum estimates of welfare loss. “Min.” (“Max.”) welfare loss uses the low (high) end of the range of marginal deadweight loss from government revenue reported by Ballard, Shoven, and Whalley (1985), the smaller (larger) estimates of hours and wage changes from Hoynes and Schanzenbach (2012), and the low (high) end of the range of elasticity of labor supply estimates reported by McClelland and Mok (2012). The change in welfare is the difference between the welfare gain and the welfare loss and the gain–loss ratio is the welfare gain divided by the welfare loss. See [Appendix C](#) for details.

marginal value of public funds (MVPF). This approach divides the willingness to pay for the benefits of a program (WTP) by the net cost of the program *to the government*. Combined with existing estimates incorporating FSP effects on other margins, our estimates of the long-run effects of FSP on violent crime dramatically increase FSP's MVPF from \$1.04 to between \$3.86 and \$7.74. This has important implications for comparing FSP to other policies. For example, the more comprehensive MVPF estimate for FSP is in line with the MVPF of other early childhood interventions, moving it from the bottom half to the top third of programs in terms of cost-effectiveness in improving social welfare.<sup>65</sup>

## VII. Discussion and Conclusion

Despite the enormous social costs of crime, relatively little is known about the early developmental factors that influence the likelihood that a child becomes a criminal. This is partially a product of data constraints that have made it difficult to credibly connect criminals to their early childhood environments. We overcome these constraints using a unique data set containing the county of birth of the universe of convicted criminals in North Carolina. We use these data to explore the effect of Food Stamp availability in early childhood on the likelihood of criminal behavior later in life. Using variation in the rollout of the FSP in the 1960s, combined with criminal conviction data from North Carolina, we find that each additional year of Food Stamp availability in early childhood reduces the likelihood of a criminal conviction in young adulthood by 2.5 percent. Food Stamp Program availability has strong effects on the types of crimes with the highest costs to society—violent and felony offenses. The effects are substantially larger for nonwhites, consistent with their higher levels of participation in the program.

Analogous estimates derived from the FBI's Uniform Crime Report data indicate similar reductions in arrests for violent crime. These results reveal an important additional benefit of Food Stamps and point to a possible link between childhood nutritional assistance and later criminal behavior.

The induced reductions in violent crime translate to large external benefits for society. These types of future external benefits are frequently ignored in discussions of the value of social safety net programs; instead, debate focuses on the contemporaneous equity–efficiency trade-off. We find that even under conservative assumptions, the social savings from crime reduction alone outweigh the cost of the program, and likely any inefficiencies generated by the program, during its early years.

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65. Confidence intervals for MVPF estimates are very wide, making it difficult to make definitive comparisons. For example, Hendren and Sprung-Keyser (2019) estimate MVPF confidence intervals for FSP that includes 0 and  $\infty$ , and for top tax rate reductions that include 1.35 and  $\infty$ .

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