Fighting Crime in the Cradle:

The Effects of Early Childhood Access to Nutritional Assistance

Andrew Barr
Texas A&M University

Alexander A. Smith
U.S. Military Academy, West Point

April, 2018*

Abstract

Despite the extraordinary social costs of crime, relatively little is known about the early life determinants of later violent behavior. We explore the effect of access to food in early childhood using two natural experiments: (1) the rollout of the U.S. Food Stamp Program (FSP), and (2) a shift in Puerto Rico's Nutrition Assistance Program (NAP). Our results yield three important insights. First, we establish a previously unknown causal link between early childhood nutritional assistance and later violent behavior. Second, we demonstrate that, relative to an equivalent cash transfer, providing benefits restricted to the purchase of food has meaningful effects on adolescent violent behavior. This finding suggests the potential importance of childhood nutrition in influencing later violent behavior and has meaningful implications for the debate surrounding cash versus in-kind transfers. Finally, we estimate that, during the early years of the Food Stamp Program (FSP), the magnitude of the discounted external benefits generated by the nutritional assistance provided was larger than: (1) the inefficiencies generated from the social transfer, and (2) the costs of the FSP program itself.

*We thank Amanda Agan, Marianne Bitler, Jennifer Doleac, Mark Hoekstra, Hilary Hoynes, Jason Lindo, David Lyle, Katherine Meckel, Marianne Page, Richard Patterson, and participants at the 2016 Stata Applied Micro conference and the seminars of the Institute for Research on Poverty at UC-Davis and SMU for helpful comments and suggestions. We also thank Marianne Bitler for sharing CDP data collected from the National Archives. The opinions expressed herein reflect the personal views of the authors and not those of the U.S. Army or the Department of Defense. All errors are our own.

1 Introduction

The annual cost of crime to society is more than $2 trillion dollars, roughly 17% of GDP.[1] Despite these extraordinary 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.[2] 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.[3] Perhaps as a result of data constraints that make it difficult to credibly connect criminals to their early childhood environments, only a handful of papers investigate the link between early childhood interventions and later criminal behavior.[4]

We make three primary contributions to this literature. First, we establish a previously unknown causal link between early childhood nutritional assistance and later violent behavior. Second, we demonstrate that, relative to an equivalent cash transfer, providing benefits restricted to the purchase of food has meaningful effects on adolescent violent behavior. This finding suggests the potential importance of childhood nutrition in influencing later violent behavior and has meaningful implications for the debate surrounding cash versus in-kind transfers. Finally, we estimate 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: (1) the inefficiencies generated from the social transfer, and (2) the costs of the FSP program itself.

To investigate the link between early childhood nutritional assistance and later violent

[1] United States. Senate Committee on the Judiciary. Hearing on The Costs of Crime. September 19, 2006 (statement of Jens Ludwig)

[2] This is somewhat surprising given that a relatively small share of individuals (6%) account for 70% of violent offenses. A recent study using population data from Sweden suggests an even greater concentration of violent crimes, with 1% of the population accounting for 63.2% of all convictions (Falk et al. 2014).

[3] 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).

[4] While there are a few evaluations of early childhood education interventions that include criminal outcomes (Heckman 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).

behavior, we take advantage of two natural experiments: the introduction of the FSP starting in the 1960s and a more recent change in the Puerto Rican Nutritional Assistance Program (NAP) that converted a cash benefit to one in which recipients were required to spend 75% of their benefits on approved food items.

The introduction of the FSP was responsible for sharp reductions in severe hunger and malnutrition during the 1960s and early 1970s.[5] 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 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.[6] We combine these data with counts of births to construct county of birth by birth month cohort conviction rates, which we link with information on the availability of Food Stamps in each county and month.

[5] Due to the purchase requirement during this time period (depicted in Figure 1), the FSP led to pure increases in food expenditures for a large share of families, while increasing general purchasing power for others. A variety of estimates from the time period suggest that households used 53 to 86% of food subsidy income for the purchase of additional food (Hoagland 1977). Furthermore, Hoynes and Schanzenbach (2009) provide evidence that the adoption of the FSP by counties during this time period produced large increases in food expenditures, particularly for female-headed households, which contain children at high risk for later criminal behavior.

[6] Most administrative crime datasets do not contain county of birth, forcing researchers interested in the early childhood environment to make relatively strong assumptions about the relationship between location of arrest and earlier residence (for example, Reyes 2007), or to link multiple datasets together to obtain better measures of both childhood environment (or treatment status) and later criminal behavior. This latter strategy has been used in several small-scale experimental evaluations (Olds et al. 1998, 2007; Heckman et al. 2010; Campbell et al. 2012). While some survey datasets contain measures of criminal behavior and early childhood environment, small sample sizes, high rates of attrition, and well known issues with underreporting of criminal behavior present their own difficulties (Hindelang et al. 1981).

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 5 reduces the likelihood of a criminal conviction by age 24 by roughly 0.3 pp, a 3 percent reduction. These estimates are robust to the inclusion of time-varying county-level controls for birth cohort composition and the availability of other War on Poverty Programs, as well as baseline county characteristics interacted with birth month fixed effects or Consolidated Statistical Area by birth month fixed effects.[7] The legitimacy of the identification strategy is further bolstered by estimates that suggest that the availability of a 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.

FSP availability in early childhood has particularly strong long-run effects on the most costly crime types for society: violent and felony convictions. These effects are larger for non-whites, 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 black violent crime that began in the early 1990s (when heavily affected cohorts were in their early twenties).[8] Our NC results are buttressed by similar estimates produced from nationwide cohort-level data on arrests by agency from the FBI's Uniform Crime Reports (UCR).

To better understand how nutritional assistance in early childhood affects later violent behavior, we examine a previously unexplored change in the Puerto Rican nutrition assistance program that converted a cash benefit to one in which recipients were required to spend 75% of their benefits on approved food items.[9] As the total value of the benefit remained fixed, this shift allows us to isolate the impact of providing food relative an equivalent amount of cash.

[7]Furthermore, FSP availability is unrelated to other policy changes shown to affect crime (e.g., removal of lead from gasoline, changes to compulsory schooling law ages, or legalizations of abortion), which occurred at the state level and generally affected different cohorts of individuals.

[8]White violent crime also declined in the 1990s, but at a slower rate, according to national-level arrest rates reported by the Bureau of Justice Statistics.

[9]The program's high participation rate, with more than one third of residents receiving assistance, makes Puerto Rico an ideal setting to study this type of shift.

We provide a variety of evidence that the shift away from cash benefits led to large increases in nutrition among recipients. Using a difference-in-differences approach, we find substantial reductions in the likelihood of low birthweight for Puerto Ricans as well as large nutritional improvements in the diets of Puerto Ricans after the shift. Those who grew up after the shift are taller and more likely to be a normal weight as adolescents. These results buttress earlier findings that suggest cash and in-kind nutrition assistance have different effects on nutritional outcomes.[10] Results from the Youth Risk Behavior Survey (YRBS) indicate that the impact of the shift persists to adolescence with reductions in violent behavior. Taken together, the results suggest the potential importance of childhood nutrition in influencing later violent behavior and help to inform the related policy debate regarding cash versus in-kind transfers.

The robustness of estimates across geography, time periods, measures, specifications, and data sets provides strong evidence that early childhood nutritional assistance reduces later violent 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 FSP 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 over this time period, suggesting that this social transfer program may have improved efficiency on the grounds of cost savings from crime reduction alone.

2 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.[11] A number of evaluations of the Moving to Opportunity project provide mixed evidence on

[10] In a randomized control trial conducted by the U.S. Department of Agriculture in San Diego County in 1990, Ohls et al (1992) found that that households that received in-kind benefits spent 5-8 percent more food than households that received cash benefits.

[11] A handful of studies focus on the influence of conditions after the period of adolescence. For example Lindo and Stoecker (2014) examine the effect of compulsory military service on later criminal behavior.

the effect of neighborhood environment on criminal behavior (Sanbonmatsu et al. 2011).[12] 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).[13] 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 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 (e.g. through direct exposure to crime or criminal peers) rather than impacting the individual's development.[14]

Research focusing on earlier periods of development is somewhat less common, with mixed evidence of effects. While Garces et al. (2002) find that Head Start participation reduces later criminality, Deming (2009) finds no effect.[15] Experimental evaluations of somewhat more resource intensive early childhood education programs also provide mixed evidence. Heckman et al. (2010) suggests that HighScope Perry preschool participation led to large reductions in criminal behavior, but Campbell et al.'s (2012) evaluation of the Abcedarian program indicates limited effects of the program on crime. Furthermore, while these studies provide rigorous evidence driven by random assignment, both rely on small sample sizes to support their conclusions.[16]

While the evidence on the effects of early education investments on later criminal behav-

[12] 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) indicates 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 that one grew up in.

[13] 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.

[14] Deming (2011) suggests peer effects as one explanation for the effect of school quality on criminal behavior. Bayer, Hialmarsson and Pozen (2009) estimate criminal peer effects more directly, showing that juvenile offenders assigned to the same facility affect each others' subsequent criminal behavior.

[15] Moreover, as both of these studies use family fixed effects designs, we might worry that even within families, certain types of siblings select into treatment, which could lead to biased estimates of effects.

[16] Recent evidence that adjusts for multiple hypothesis testing suggests that neither program had statistically significant effects on crime and suggests there may not have been statistically significant benefits for boy participants in either program (Anderson 2008).

ior 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, 2010 and Billings and Schnepel 2015).[17] The Nurse-Family Partnership Program is a prenatal to age 2 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 parole violations for the children at ages 15 and 19 (Olds et al. 1998, 2007, 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 triggered for children in Mecklenburg County 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 (e.g., 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 evidence of the effects of an early health intervention on a number of later outcomes, including criminal behavior. However, the studies are limited by their focus on small and particular samples as well as by their potential measurement issues.[18] Interestingly, both interventions included nutritional assistance and/or advice, suggesting nutrition may be part of the mechanism through which the observed treatment effects operate.[19] Indeed, one of the short-term effects observed in the Nurse-Family Partnership

[17]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).

[18]Both studies rely on samples of fewer than 400 individuals (fewer than 200 treated) drawn from a single county. The Olds et al. studies face the usual concerns related to attrition and self-reported measurement of criminal behavior. Billings and Schnepel rely on outcome measures generated from imperfect matching across a number of administrative datasets for a single county, raising selection concerns related to whether individuals match across datasets, whether individuals migrate outside of Mecklenburg County, and whether individuals commit crime outside of Mecklenburg County.

[19]The link between early childhood nutrition and later violence has been suggested previously in correlational

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*.[20,21] 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).

2.1 Nutritional Assistance in Early Childhood and Violent Behavior in Adulthood

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.[22] Indeed, the criminology literature

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.

[20]Studies comparing twins or siblings find that low birth weight children 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; Oreopulos 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).

[21]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), and an increased probability of receiving welfare (Currie 2009).

[22]In 1975, 64% of households receiving Food Stamps were headed by females (Hoagland 1977).

provides some evidence that low birth weight predicts violent criminal activity, but not non-violent criminal activity and Almond, Hoynes, and Schanzenbach (2011) demonstrate that the availability of the program did result in improvements in birth outcomes.[23,24] However, this mechanism cannot 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 opportunity cost of committing crime.[25] In fact, Hoynes et al. (2016) suggest that the program had an effect on health outcomes. However, there is limited evidence of improvements in education or earnings potential among men, who account for nearly 90% of violent offenders, suggesting that this is unlikely to be the primary channel. 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. A growing number of studies find correlations between malnutrition in early childhood and externalizing behavior (i.e., physical aggression, rule breaking, etc.) in adolescence; not surprisingly, these behaviors are strong predictors of adult violence (Liu and Raine 2006; Galler 2013).[26] While the connection between nutrition and later behavior is not well understood, it is clear that malnutrition affects brain chemistry

[23]Tibbetts and Piquero (1999) describe the relationship between birthweight 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 et al. 2002; Kelly et al. 2001; Levy-Shiff et al. 1994) and a link between behavioral problems and criminal activity (Moffitt et al. 1994; Piquero 2001; Raine 2002; Raine et al. 1996).

[24]We observe the negative relationship between FSP availability and low birth weight in North Carolina as well (Appendix Table A1). The availability of a FSP results in a reduction in the likelihood of a low birth weight of roughly 0.2 pp (2.1%) for all mothers, 0.05pp (0.7%) for white mothers, and 0.5pp (3.7%) for non-white mothers. These results are statistically indistinguishable from Almond and coauthors' 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%).

[25]As discussed above, a number of papers demonstrate the causal link between education and crime (e.g., Lochner and Moretti 2004).

[26]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 (e.g., Tonkiss et al. 1987; Whatson et al. 1976; Levitsky and Barnes 1972).

through decreased cell growth, alterations in neurochemistry, and an increase in neurotoxic effects (Liu and Raine 2006).[27]

Alternatively, nutritional assistance may influence later violent behavior through an increase in household income. One possibility is that the income transfer from the program increases parental involvement with or expenditures on children. In the same way that improved nutrition could contribute to better intermediate outcomes, so could other investments. While we cannot isolate the exact channel through which nutritional assistance operates, we leverage a previously unexplored shift in the nutritional assistance program in Puerto Rico to test whether the form of the resource transfer matters, holding the size of the transfer constant.

3 Variation in Access to Food Assistance: Two Natural Experiments

To explore the connection between early childhood access to nutritional assistance and later violent behavior, we leverage two natural experiments. First, we examine the initial, county-level rollout of the Food Stamp Program (FSP) in the 1960s and 70s. Next, to isolate the impact of providing food relative to an equivalent amount of cash and help illuminate the possible mechanism for the impact of nutritional assistance, we turn to a recent change in Puerto Rico's Nutrition Assistance Program (NAP).

3.1 Rollout of Food Stamp Program

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

[27] 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 (e.g., Gailliot and Baumeister 2007; Virkkunen and Huttunen 1982; Vrikkunen 1986). Indeed, Hoynes et al. (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.

that allowed individuals to purchase stamps which 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 2 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, pp. 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.[28]

While the country lacked the relevant data systems to accurately quantify severe hunger and malnutrition, there is abundant anecdotal evidence that these problems improved during the rollout of the FSP.[29] In 1977, ten years after their first testimony to congress on hunger and malnutrition in America, a team of doctors testified that "[i]t is not possible any more 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 1960's." They observed that "the problem is not now primarily one of overt hunger and malnutrition," but instead "more subtle manifestations of malnutrition."[30] The central role of Food Stamps in this

[28]This conclusion is supported by a similar set of analyses conducted for North Carolina counties. We return to these analyses in Section 4.3.1.

[29]In a letter to congress, President Richard Nixon declared that "there can be no doubt that hunger and malnutrition exist in America, and that some millions may be affected."(Richard Nixon: "Special Message to the Congress Recommending a Program To End Hunger in America," May 6, 1969. Online by Gerhard Peters and John T. Woolley, The American Presidency Project). 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 10 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 two thousand children they observed were below the third percentile in weight, and in some counties they found the prevalence of anemia from malnutrition was above 80 percent (Hearing on Hunger and Malnutrition in America July 11 and 12, 1967). In fact, conditions were so bad in the Mississippi Delta that when a federally-funded health center was established, the doctors began writing prescriptions for food and organizing a farm co-op (Geiger, 2005, pp. 7).

[30]United States. Senate Subcommittee on Nutrition. Hearing on Hunger in America: Ten Years Later. April 30,

turnaround is reinforced by the economics literature. Hoynes and Schanzenbach (2009) 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.[31]

3.2 In-Kind Transition in Puerto Rico's Nutrition Assistance Program

In September 2001, the Puerto Rican NAP transitioned from a 100 percent cash redeemable EBT card, to one for which 75 percent of the benefits had to be spent on approved food items. While 25 percent of the benefit remained redeemable in cash, the government for the first time made it clear that 100 percent of the benefit was intended for food.[32]

Given the high reliance of many Puerto Ricans on NAP benefits, the shift led many of them to increase consumption of food. As one young mother put it "I'm going to have the cupboard full, but I will not have the light to cook what I bought... That's logical, they know it, I do not have any more money..." Indeed, there is ample evidence from a number of sources that her case was not unique. First, the in-kind component of the benefits accounted for the majority of resources in 72 percent of recipient households, while between 35 and 40 percent of recipient households had *no other source* of income.[33,34] Since the average Puerto Rican household spends only 18 percent (and the average welfare recipient spends only 21 percent) of their income on food, this suggests that a large portion of NAP recipients were forced to spend more on food as a result of the benefit shift. Second, food industry revenue estimates at the time imply a roughly 40 to 60 percent increase in food expenditures for

1979 (statement of Raymond Wheeler)

[31] In more recent years (1996 to 2003), East (2016) 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.

[32] 25 percent was kept in cash due to concerns that some Puerto Ricans would not be able to access an approved retailer for all of their purchases.

[33] Gotay, Benjamin Torres. "Empieza hoy el nuevo sistema de uso del PAN." *El Nuevo Dia* 1 Sep. 2001 (translated to English).

[34] Authors' calculations using data from Trippe, C., Gaddes, R., Suchman, A., Place, K., Mabli, J., Tadler, C., DeAtley, T., Estes, B. (2015). "Examination of Cash Nutrition Assistance Program Benefits in Puerto Rico." Prepared by Insight Policy Research under Contract No. AG-3198-C-14-0006. Alexandria, VA: U.S. Department of Agriculture, Food and Nutrition Service.

NAP recipients as a result of the benefit shift.[35] Third, surveys of NAP recipients indicate that despite the requirement to spend 100 percent of their benefit on food (including the 25 percent cash component), only 32 percent report using the cash benefit on food, suggesting at least 68 percent are constrained.[36] Finally, EBT expenditure data from after the benefit shift indicate that while over 60 percent of NAP recipients spend roughly 75 percent of their benefit on food (the in-kind share of the benefit), only 6 percent spend something in between 80 and 99 percent of their benefit on food.[37] This level of bunching suggests that many households would prefer to spend less than 75 percent on food. In Section 5.1, we add additional evidence to this list. We estimate the contemporaneous impact of the benefit shift in Puerto Rico on nutrition and health with a difference-in-differences approach using the Behavioral Risk Factor Surveillance Survey (BRFSS) and natality data. We find large statistically significant increases in fruit and vegetable consumption and decreases in the likelihood of low birthweight.

4 Nutritional Assistance and Later Crime: Evidence from FSP Rollout

In order to measure the effect of FSP availability on criminal behavior we use administrative conviction data from the state of North Carolina. An important advantage of the North Carolina data over other state criminal databases is the inclusion of county of birth

[35]The president of the Association of Marketing, Industry and Food Distribution (MIDA) indicated that "the economic boom experienced by the food industry is the result of a fundamental factor: the modifications to the Nutritional Assistance Program (PAN) under the formula 75-25 through the Family Card." A MIDA report suggests that the benefit shift increased food sector revenue by $300 to $400 million, 22.5 to 30 percent of the total PAN budget. Given that roughly 80 percent of benefits were spent on food after the shift, we obtain a rough estimate of the increase in food expenditure among recipients by dividing the $300 to $400 million by the implied share spent on food prior to the shift, calculated as the share of total PAN expenditures spent on food (0.8*$1335 million), minus the estimated increase in food sector revenue ($300 to $400 million). Román, Miguel Díaz. "Próspera la industria de alimentos." 22 Oct. 2001 (translated to English).

[36] Rosado-González, R., Puerto Rico Department of the Family, Administration for Socioeconomic Development of the Family (ADSEF). (2008). PANECO pregunta. Trujillo Alto, PR: RRG Universe and Assoc.

[37]Trippe et al. (2015) "Examination of Cash Nutrition Assistance Program Benefits in Puerto Rico." Prepared by Insight Policy Research under Contract No. AG-3198-C-14-0006. Alexandria, VA: U.S. Department of Agriculture, Food and Nutrition Service.

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.[38] Summary statistics are contained in Panel A of Table 1. Roughly 9 percent of individuals born between 1964 and 1974 were convicted of a crime by age 24. Looking by type of crime, 4 percent of individuals were convicted of a felony, 7.2 percent were convicted of a misdemeanor, 1.5 percent were convicted of a violent crime, and 2.3 percent were convicted of a property crime by age 24.[39] 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.[40]

We supplement our North Carolina analyses with analyses using the FBI's nationwide 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.[41] For individuals between 18 and 24 there is roughly 1 violent arrest per hundred 18 to 24 year-olds each year. The number of arrests for property crimes is substantially higher with roughly 3.4 property arrests per hundred 18 to 24 year-old each year.

We use information on the availability of Food Stamps within a county and month to calculate Food Stamp exposure for each birth month cohort.[42] We link this exposure measure

[38] 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.

[39] 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" or "larceny".

[40] Over 78% of individuals born in North Carolina during this period reside in North Carolina between age 18 and 24. This share is even higher (over 80%) for those with the highest rates of criminal behavior (non-whites and those with less than a high-school degree). In Appendix Table A4, 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) non-significant relationship between childhood Food Stamp availability and the likelihood of living in one's state of birth. The point estimates for non-whites, 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).

[41] See UCR Data Appendix for further details on UCR data construction.

[42] Following Hoynes, Schanzenbach, and Almond (2016), we construct a measure of Food Stamp exposure in early

to county by cohort "crime rates" to estimate the effect of Food Stamp availability on crime.

4.1 Estimation Strategy

To estimate the effect of FSP access in early childhood on criminal behavior in adulthood, we leverage within county variation in the availability of the FSP generated by the rollout of the program in the 1960s and 70s. Our basic specification is as follows,

$$C_{ct} = \alpha_c + \lambda_t + X_{c60} * t\beta_1 + \gamma FS_{ct} + \epsilon_{ct}, \tag{1}$$

where C_{ct} is a measure of the crime rate for individuals who were born in county c in birth cohort t; α_c and λ_t are birth county and birth cohort fixed effects. Standard errors are clustered at the county of birth level. For the NC estimates, the crime rate is the likelihood of criminal conviction by age 24 and birth cohorts t are defined at the month level.[43] Following Hoynes and Schanzenbach (2009) and Hoynes et al. (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 with less than $3,000 (1960 dollars), the percent living in urban areas, the percent black, the percent under 5 years old, the percent over 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 γ, 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

childhood. For the Food Stamp exposure measure linked to the NC conviction rates, we use the fraction of months from conception (9 months prior to birth) to age 5 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 0 to age 5 that Food stamps were present.

[43] Estimates are weighted by number of births in the county in 1964.

assumption is likely to hold, below we provide additional support relevant to our context.

4.2 North Carolina Estimates

Our baseline results in Table 2 indicate that each additional year of FSP availability in early childhood (in utero to age 5) reduces the likelihood of any criminal conviction by age 24 by 0.26 to 0.38 percentage points. This is a sizeable reduction of 3-4 percent off of a base of 9 percent. The estimates are robust to the inclusion of pretreatment (1960) county characteristics interacted with time trends as in Hoynes et al. (2016). While our baseline inference relies on standard errors clustered at the county of birth level, we have also explored the robustness of our p-values to an even more conservative approach: randomization inference. 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 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.[44]

Figure 3 presents graphical evidence of the effects, demonstrating the relationship between the age at county FSP adoption and later criminal behavior. Given the nature of treatment, the presentation is somewhat non-standard.[45] 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. 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

[44] P-values presented are the two-tailed statistics calculated as the share of coefficient estimates obtained under random assignment of FSP timing that are larger in absolute magnitude than the estimate produced using the true timing of assignment.

[45] We follow Hoynes et al. (2016) in this regard. All estimates presented are relative to FSP adoption at age 10. The start year of the North Carolina conviction data limit our ability to extend the x-axis to older ages.

conviction. The reductions in criminal behavior are largest at or prior to conception and decrease between conception and age 5 before leveling out.[46] Consistent with our estimates 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. The effect on convictions appears to be driven largely by reductions in the likelihood of conviction for a violent crime, with smaller and non-significant reductions for property crime. Moving to the second row of estimates, the reductions for violent felony convictions are especially large, with each year of Food Stamp availability in early childhood reducing the likelihood of violent felony conviction by 0.04 to 0.05 pp, or 10 to 12 percent (Table 2).[47]

4.3 Threats to Internal Validity

To interpret these estimates as the causal effect of Food Stamp availability, it must be the case that the availability of a Head Start program is, conditional on county and year of birth fixed effects, 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. Specifically, we address concerns related to the endogeneity of FSP adoption, changes in the composition of mothers, and whether changes to other policies potentially associated with criminal behavior may have coincided with the treatment.

[46] That the estimates level after age 5 does not preclude the possibility of effects after age 5 as all effects are relative to FSP availability at age 10 by construction. This is consistent with the presentation of Hoynes et al. (2016) and is necessary due to data constraints.

[47] 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 3 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 2, which is larger than the effect of first access between ages 3 and 5. (Appendix Table A2).

4.3.1 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 over time variation (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. For example, counties that chose to rollout the FSP earlier may be those who were proactively improving other services for young children. If this were the case, we might observe reduced criminal behavior for these cohorts of young children due to a comprehensive effort to help them, and not because of Food Stamp availability.

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. The authors find that county characteristics in 1960 "explain very little of the variation in adoption dates... consistent with the characterization of funding limits controlling the movement of counties off the waiting list to start up their [Food Stamp Program]." Their finding is also 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, pp. 36-37).[48]

[48] A related concern is that FSP adoption or program effects may be different in counties with a preexisting commodity distribution program (CDP). In particular, one might expect that the nutritional benefit of a FSP program would be attenuated in counties with a preexisting CDP. However, recent draft work presented at the National Bureau of Economic Research Summer Institute by Marianne Bitler and Theodore Figinski (and correspondence with Marianne Bitler) suggests that the introduction of a FSP had similar effects in areas with and without a preexisting CDP. Our own criminal conviction estimates are very similar (but slightly smaller) when we restrict to

In Table A5 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. We find no evidence of earlier FSP rollout in NC counties that were predisposed to lower crime (or crime growth) based on their 1960 characteristics (Table A5). We present the relationship between county characteristics, including the predicted crime rate and growth, and the timing of FSP adoption graphically in 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. 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 over-time changes that vary across county characteristics or geography (Table A6). Specifically, the estimates are robust to the inclusion of 1960 county characteristics interacted with 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 month-year fixed effects. This final approach identifies the effect of FSP availability off of differential availability of a FSP program within a CSA.

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.[49] To address this, we estimate our main specifica-

NC counties with a preexisting CDP (Appendix Table A3). Both sets of results are consistent with prior arguments made by Hoynes and coauthors 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).

[49]Perhaps, for example, the availability of a FSP attracted soon-to-be mothers who placed greater emphasis on the

tions with the addition of "County Natality Characteristics" controls, which include mean mother's age, fraction of "legitimate" births, fraction of white births, and fraction of births with attending physician in a hospital. The estimates from this exercise are presented in Table 3. The controls have little effect on our point estimates.[50] The estimates are similarly robust to the inclusion of more specific county-cohort controls for the availability of various War on Poverty programs.

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 NC but were not born there (ensuring a substantial difference between their assigned and actual FSP exposure). As these individuals moved to NC 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.[51] If something other than FSP exposure is driving our main results, we might expect to see similar affects show up for individuals born outside of NC who now live in NC counties.[52]

Table A8 presents estimates of Equation 1, where the conviction rate for county c and

well-being of their children, ensuring they would not be criminals and leading to reductions in the rate of conviction. We note that for this type of effect to drive our results there would need to be a 23 to 27% increase in births, assuming that all of these additional births would not be convicted of a crime by age 24. Appendix Table A7 indicates that there is no effect of FSP access on the number of births.

[50] As these characteristics are only available beginning in 1968, we first show the robustness of our results to this shorter window (1968-1974).

[51] 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.

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

birth cohort t is the number of individuals born outside of NC in year t who are convicted in county c (by age a) divided by the total number of individuals born outside of NC in year t that reside in county c at age a. [53] While generally imprecise, we find that the estimated effects on conviction by age 24 are actually *positive*. Extending the conviction window to age to 30 (and therefore allowing more time for migration) yields negative estimates that are very small in magnitude and insignificant.

4.3.2 Other Determinants of Crime

It is possible that other determinants of crime, particularly those argued to have reduced crime in the 1990s, coincided with Food Stamp availability. It may be these changes, and not Food Stamp availability, that are driving our results. Among these changes are legalizations of abortion, increases in prison populations, the removal of lead from gas, and increases in compulsory schooling law ages. These factors share one important characteristic: most of the variation occurred at the state-year level. However, the North Carolina estimates are already netting out state-year variation with year fixed effects.[54] While it is possible that changes in state policies had heterogeneous effects across counties within a state that coincided with the timing of Food Stamp introduction, this pattern of timing seems unlikely to have occurred in practice.[55] Regardless, it is unlikely that the specific changes mentioned are responsible for our observed effects as the timing of the legalization of abortion and the removal of lead from gasoline (early to mid-70s) occurred too late, and the timing of most compulsory schooling

[53] 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.

[54] To control for any potential overlapping contribution of these factors in the UCR estimates (below), we include state by birth year fixed effects in our UCR regressions, essentially netting out changes that occurred at the state-year level and identifying the effect of Food Stamp availability using only variation in the timing of introduction within counties. This has little effect on our estimates, suggesting that these alternative explanations do not account for our estimates of the effect of Food Stamp availability on crime. The point estimate for violent arrests is -0.177*** (se 0.057).

[55] For example, this would require that (1) the timing of the legalization of abortion within a state coincides with the timing of adoption of Food Stamps by the earliest adopting counties within that state, and (2) that counties that complied with the legalization of abortion earlier were also more likely to introduce Food Stamps earlier in that state.

law changes occurred too early (before 1980), to affect most birth cohorts in our sample.[56,57]

4.4 Effect Size and Heterogeneity

As with much research on early childhood interventions, our estimated effects are substantial (Olds et al. 1998, 2010; Heckman et al. 2010, Garces et al. 2002). Indeed, Hoynes et al. (2016) report sizeable impacts of access to the Food Stamp program in early childhood on metabolic syndrome in adulthood (0.3 sd) and high-school graduation (18 percentage points) as well as self-reported good health (30 percentage points) and an index of economic self-sufficiency (0.3 sd) for women. These are all intent-to-treat estimates for a sample with a 43 percent participation rate, suggesting large effects of FSP availability in early childhood.

Our preferred estimates indicate reductions in the likelihood of any conviction of 1.3 percentage points, assuming FSP availability throughout childhood. 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.[58] While it is not straightforward to construct comparable measures of criminal behavior across studies, our implied TOT effects are less than half 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 less than 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).[59] Consistent with our findings, evaluations of both interventions

[56] Abortion was not legalized in North Carolina (and all states but New York, Washington, and Alaska) until 1973. The use of catalytic converters in *new cars* (1975) and an EPA timetable for the reduction of lead in gasoline (1974) resulted in a fall in blood lead levels that did not begin until at least 1976 and lasted for over a decade.

[57] Another potentially confounding policy change, court-mandated school desegregation, is also unlikely to explain our results in North Carolina due to timing. In 1971, the Supreme Court ruled in *Swann v. Charlotte-Mecklenberg Board of Education* that busing could be used to overcome school segregation. As a result of this and other court rulings, schools in North Carolina and the rest of the South quickly became much less segregated. The percentage of black students in 90-100% minority schools in the South went from the highest in the nation (77.8%) in 1968 to the lowest in the nation (24.7%) in 1972 (North Carolina was already more integrated than other southern states in 1970) (Orfield 1983). Children born in birth cohorts 1968-1974 (for which we find similar effects of FSP availability in Table 3) would not have attended first grade until after North Carolina schools had satisfied the court' s desegregation mandate.

[58] We estimate Food Stamp participation rates of families with children 5 and under of approximately 17 percent using number of children, Food Stamp participation, and family weights from the family-level PSID for 1976-1979.

[59] The less intensive set of services, primarily information on how to reduce lead exposure and eat better, produced

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.[60]

While there is little evidence linking early childhood health and/or health interventions and later criminal behavior, there has been greater inquiry into the link between education and crime. Our implied TOT effects are between half and two-thirds of the size of effects on somewhat similar measures reported in evaluations of the Perry Preschool program (12 percentage points on arrest by age 27, 19 percentage points on five or more arrests by age 27). Of course, Perry Preschool enrolled a very particular type of student: extremely disadvantaged, black children in Ypsilanti, Michigan. If we split our estimates by race, we find substantially larger effects for non-whites (Table 4).[61] This is reassuring as the participation rate of non-white families (36%) was substantially higher than that of white families (12%) during this time period. Scaling our non-white estimates by their relatively higher rates of Food Stamp participation implies TOT reductions in the likelihood of conviction of 11 percentage points, similar to the effects of Perry Preschool.[62] 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 et al. 2002).

Of course it may not be reasonable to convert our estimates to TOT effects as there may be important spillover effects of program availability; indeed, it is not difficult to imagine that improving the behavioral trajectories of a significant share of a group results in improvements for the group as a whole that are substantially larger than what we might expect to see if an individual was treated in isolation. Unlike the Olds et al. experiment, in which fewer than 200 mothers were offered a spot in the treatment group, Food Stamps were used by

effects of a similar size to our implied TOT estimates.

[60] 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).

[61] During this period in North Carolina, blacks comprised more than 95% percent of the non-white population (1970 Census).

[62] Using the UCR data, discussed further below, we also find significantly larger effects in counties with a population that is greater than 10 percent black.

a substantial fraction of households. As children who grew up in participating households interacted with others in their cohort, effects of the program might have spilled over to the children of non-participants in a way that would have been unlikely with the smaller treatment and control groups in Olds et al. Particularly for violent crime it is easy to see how these spillovers might operate through peer effects. Given the potential for large spillovers, we focus our discussion on the estimated effects of Food Stamp availability rather than participation.

4.5 Nationwide 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 or incarceration.[63] To address these limitations and to generate inputs for our welfare calculations, we turn to estimates using the FBI's nationwide Uniform Crime Reports (UCR).

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 of arrests per hundred 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 hundred people, or roughly 3 percent.[64] These estimates are not directly comparable to the NC estimates because they measure the impact of Food Stamps availability on the number of arrests per hundred people at a specific age whereas the NC 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

[63] 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.

[64] As with the NC estimates, UCR estimate p-values are robust to randomization inference (Figure A5).

violent arrests (3 percent) are smaller than the corresponding effects in the North Carolina conviction data (7 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).[65] 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.[66]

While migration potentially explains the difference in magnitudes between the NC and UCR estimates, it also hints at a potential validity concern of the UCR estimates. Because we are not able to observe county of birth in the UCR data, we are implicitly assuming that individuals are committing crimes in their county of birth. To the extent that early childhood Food Stamp availability affects the mobility of individuals on the margin of committing crimes, this could potentially bias our estimates. Consider, for example, if early childhood Food Stamp availability causes potential criminals to be more likely to commit crimes outside of their birth county (in place of crimes within their birth county). Our basic strategy would suggest that Food Stamp availability had reduced crime when in reality it had only shifted crime from one county to another.

We address this concern in two ways. First, we use our North Carolina data to examine the extent to which early childhood Food Stamp availability affects the likelihood that individuals commit crimes in counties other than their birth counties. Estimates from Appendix Table A9 indicate that, in 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 our UCR estimates provide a lower bound on the true effect of early childhood Food Stamp availability on arrests, since they only capture the reduction in arrests for those who remain in their birth county. Second, we use the NLSY79

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

[66] 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.

to estimate whether FSP availability in early childhood has an effect on the probability that an individual moves outside of his birth county. We observe no effect of FSP availability on the probability that an individual moves out of his or her birth county (Table A10).[67]

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 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. Undiscounted, the benefits of the FSP from 1964-1974 are estimated at $510 billion (2015 dollars) based on the social costs of crime estimates of McCollister et al. (2010) or $230 billion (2015 dollars) based on the lowest estimates for each offense in the recent literature. With a discount rate of 7%, this range falls to $143 billion to $64 billion (2015 dollars).[68] We also evaluate the net welfare implications of the rollout. While the details of these calculations are in Appendix C, Table 7 shows FSP's welfare gains from crime reduction exceeding its welfare losses for nearly all parameter choices, suggesting that a complete accounting of the efficiency impact of the FSP rollout would likely show an improvement.

5 Just a Cash Transfer? Evidence from Puerto Rico

Does nutritional assistance improve children's later outcomes simply by increasing household incomes? A simple model of household consumption decisions would suggest that the early FSP was indistinguishable from a cash transfer for some households while for others it boosted food consumption by more than a cash transfer (and perhaps even reduced the consumption of other goods). Until amendments to the program in the 1970s, households

[67] We note that the NLSY 79 cohorts provide a limited degree of overlap with our identifying variation, which limits our statistical power to identify effects.

[68] The U.S. Office of Management and Budget has used a real discount rate of 7% for base-case analysis since 1992, while the U.K. government uses 3.5% (Kohyama, 2006).

participating in the FSP were required to purchase a set amount of food coupons sufficient for a "low-cost nutritionally adequate diet."[69] Figure 1 shows how the FSP would have altered the budget constraint for food (F) and other goods (x) of a household with income m.[70] Households consuming less than $\bar{F}$ in food (the amount of food purchasable at the same cost as the coupons, $\frac{0.3m}{P_f}$) prior to the FSP rollout would increase food expenditures while decreasing other expenditures. [71] We obtain rough estimates using the 1960-1961 Consumer Expenditure Survey (CES) that the majority of FSP-eligible households (55 to 64 percent) would be constrained by the FSP based on *ex ante* food expenditures and income (See Appendix D for details).[72]

While prior evidence suggests that the FSP increased food consumption and improved short- and long-term health outcomes, it is not clear whether these effects were driven, at least partially, by constraining households' consumption decisions, or whether an equivalent increase in income would have generated the same effects. To shed light on this question, we turn to a natural experiment where Puerto Rico converted a cash benefit to one in which recipients were required to spend 75% of their benefits on approved food items. Here, we can examine the impact of constraining household consumption decisions in the absence of any shock to overall income. This enables us to directly inform the policy debate over cash versus in-kind benefit transfers and explore the potential importance of nutrition as a mechanism for effects on later violent behavior. According to a simple model of household consumption decisions, this transition should increase food consumption for households with *ex ante* food consumption below the in-kind benefit amount.[73] For households with higher *ex ante* food

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

[70] We normalize the price of other goods x to be one and let P_f represent the relative price of food.

[71] In contrast, households consuming more than $\hat{F}$ in food (the food provided in coupons) prior to the FSP rollout would increase expenditures on food and other goods in the same way as if they had been given a cash transfer (assuming both F and x are normal goods); for this group, the food assistance is inframarginal. Households consuming between $\bar{F}$ and $\hat{F}$ prior to the FSP rollout would increase food expenditures F and expenditures on other goods x, but they may be constrained by the food coupons and increase food consumption by more than if they had received an equivalent cash transfer.

[72] In fact, between 17 and 41% 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 purely an increase in food from the program (with no increase in the consumption of other goods).

[73] It will also decrease consumption of other goods (unless households responded to the change by increasing labor supply).

consumption the transition should have no effect. As discussed above (in section 3.2), there is abundant evidence that the majority of households were constrained by this change and that food consumption increased as a result.

5.1 Effects on Contemporaneous Nutrition and Health

We can examine the effect on food consumption and nutrition of the shift from cash to in-kind benefits using the Behavioral Risk Factor Surveillance Survey (BRFSS) and natality data.

Our basic empirical specification is:

$$F_{ist} = \alpha_s + \lambda_t + \beta(PR_s * Post_t) + \gamma X_{ist} + \epsilon_{ist}, \tag{2}$$

where F_{ist} is one of several measures of food consumption of individual i in state (or territory) s in year t. α_s and λ_t are state and month or year fixed effects. X_{ist} are individual covariates, including age indicators interacted with a gender indicator for regressions with BRFSS data and mother's race, mother's education, plurality of birth, and birth order for regressions with natality data. $PR_s * Post_t$ is an interaction between an indicator equal to one for Puerto Rico and indicator equal to one for the period in which Puerto Rico's nutrition assistance program required participants to spend their benefits on approved food items.[74] The coefficient of interest, β, provides an estimate of the effect of in-kind benefits (relative to cash benefits) on the measures of food consumption, assuming that Puerto Rico and the comparison states would have had similar trends in food consumption if not for Puerto Rico's policy change.[75]

While our measures of food consumption are limited, our results are consistent with theory and the anecdotal evidence provided earlier. We find overall increases in consumption of

[74] The official change happened in September 2001, but there was a ramp up period through the end of 2001. Given this and data constraints (2001 is not available in the BRFSS), we set 2002 as the first post year.

[75] We present estimates using three different comparison groups: (1) all states, (2) the 10 poorest states, and (3) states matched to Puerto Rico on 1990 Census characteristics and available in the YRBS (Mississippi, Kentucky, and Tennessee).

juice and fruits and vegetables of 20 to 25 percent (Appendix Table A12). Our identification strategy is supported by event studies that demonstrate a flat (or slightly downward) trend in food consumption prior to a large increase just after the benefit shift (Appendix Figure A8).

To estimate the contemporaneous effect on health, we turn to natality data. We find substantial reductions in the likelihood of low birthweight for Puerto Ricans born after the shift (Table A11), again suggesting the importance of early childhood nutrition. These results are strongest among those most likely to be using nutritional assistance (mothers with lower education levels). These results buttress earlier findings that suggest cash and in-kind nutrition transfers have different effects on nutritional outcomes.

5.2 Effects on Later Violent Behavior

We turn to the Youth Risk Behavior Survey (YRBS) to demonstrate the importance of food provision in generating the effects of nutritional assistance on violent behavior. The YRBS was developed in 1990 to monitor priority health risk behaviors among adolescents in the United States and includes questions about fighting, bullying, and concerns about school safety.

As with the FSP rollout, we use individuals' year of birth to construct measures of age 0 to age 5 in-kind benefit exposure. Using this in-kind benefit exposure measure, we demonstrate that the shift in benefits yields reductions in adolescent violent behavior (Table 8).[76,77] Our identification strategy is supported by event studies that demonstrate stronger effects on violent behavior the younger an individual was when the benefit shift occurred, consistent with our FSP results (Figures 4 and 5).[78]

[76] The regressions focus on individuals in high school and condition on birth cohort and state fixed effects as well as age by gender fixed effects.

[77] Analogous results indicate the health improvements observed at birth persist to adolescence in the form of increased height and normal weight for age.

[78] While our baseline inference relies on standard errors clustered at the state level, we have also explored the robustness of our p-values to an even more conservative approach: randomization inference. The distribution of these estimates is contained in Appendix Figures A9a and A9b As can be seen in the figures, the estimates we observe are quite unlikely to be observed by chance.

Taken together, these results suggest the potential importance of childhood nutrition in reducing future violent behavior. While our findings do not rule out all other mechanisms, the pattern of effects supports the conclusion that nutrition may matter. At minimum, we can conclude that the form of the transfer is of consequence; providing benefits restricted to the purchase of food has meaningful effects on adolescent violent behavior, a finding with important implications for the debate surrounding cash versus in-kind transfers.

6 Discussion and Conclusion

Despite the enormous social costs of violent crime, relatively little is known about the early developmental factors that influence the likelihood that a child becomes a criminal. This is partially the result of data constraints that have made it difficult to credibly connect criminals to their early childhood environments. We overcome these constraints using a unique dataset 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 violent 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 adulthood by 3-4 percent. FSP availability has particularly strong effects on the types of crimes with the highest costs to society, violent and felony offenses. The effects are substantially larger for non-whites, consistent with their higher levels of participation in the program.

To better understand how nutritional assistance in early childhood affects later violent behavior, we examine a previously unexplored change in the Puerto Rican nutrition assistance program that converted a cash benefit to one in which recipients were required to spend 75% of their benefits on approved food items. As the total value of the benefit remained fixed, this shift allows us to isolate the impact of providing food relative to an equivalent amount of cash. Using data from birth records, the BRFSS, and the YRBS, we find that

this shift resulted in reductions in violent behavior in adolescence as well as improvements in weight at birth, nutrition, and measures of health in adolescence. Taken together, the results suggest the potential importance of childhood nutrition in influencing later violent behavior and help to inform the related policy debate regarding cash versus in-kind transfers.

The induced reductions in violent behavior from access to nutritional assistance 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 tradeoff. Focusing on the FSP, 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.

References

Aizer, A., and J. Currie (2017): "Lead and Juvenile Delinquency: New Evidence from Linked Birth, School and Juvenile Detention Records," Discussion paper, National Bureau of Economic Research.

Almond, D. (2006): "Is the 1918 Influenza pandemic over? Long-term effects of in utero Influenza exposure in the post-1940 US population," *Journal of Political Economy*, 114(4), 672–712.

Almond, D., H. W. Hoynes, and D. W. Schanzenbach (2011): "Inside the war on poverty: The impact of food stamps on birth outcomes," *The Review of Economics and Statistics*, 93(2), 387–403.

Anderson, M. L. (2008): "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects," *Journal of the American Statistical Association*, 103(484), 1481–1495.

Bayer, P., R. Hjalmarsson, and D. Pozen (2009): "Building criminal capital behind bars: Peer effects in juvenile corrections," *The Quarterly Journal of Economics*, 124(1), 105–147.

Billings, S. B., and K. T. Schnepel (2015): "Life Unleaded: E ects of Early Interventions for Children Exposed to Lead," Discussion paper, LCC Working Paper Series 2015-18.

Black, S. E., P. J. Devereux, and K. G. Salvanes (2009): "Like father, like son? A note on the intergenerational transmission of IQ scores," *Economics Letters*, 105(1), 138–140.

Campbell, F. A., E. P. Pungello, M. Burchinal, K. Kainz, Y. Pan, B. H. Wasik, O. A. Barbarin, J. J. Sparling, and C. T. Ramey (2012): "Adult outcomes as a

function of an early childhood educational program: an Abecedarian Project follow-up.," *Developmental Psychology*, 48(4), 1033.

Chay, K. Y., J. Guryan, and B. Mazumder (2009): "Birth cohort and the black-white achievement gap: The roles of access and health soon after birth," Discussion paper, National Bureau of Economic Research.

Chilcoat, H. D., and N. Breslau (2002): "Low birth weight as a vulnerability marker for early drug use.," *Experimental and Clinical Psychopharmacology*, 10(2), 104.

Clarkson, K. W. (1975): "Food Stamps and Nutrition.," .

Currie, J. (2009): "Healthy, wealthy, and wise: Socioeconomic status, poor health in childhood, and human capital development," *Journal of Economic Literature*, 47(1), 87–122.

Currie, J., and R. Hyson (1999): "Is the impact of health shocks cushioned by socioeconomic status? The case of low birthweight," Discussion paper, National bureau of economic research.

Deming, D. (2009): "Early childhood intervention and life-cycle skill development: Evidence from Head Start," *American Economic Journal: Applied Economics*, 1(3), 111–134.

Deming, D. J. (2011): "Better schools, less crime?," *The Quarterly Journal of Economics*, 126(4), 2063–2115.

Doyle, J. (2007): "Child protection and child outcomes: Measuring the effects of foster care," *The American Economic Review*, 97(5), 1583–1610.

Doyle Jr, J. J. (2008): "Child protection and adult crime: Using investigator assignment to estimate causal effects of foster care," *Journal of Political Economy*, 116(4), 746–770.

East, C. N. (2015): "The Effect of Food Stamps on Children's Health: Evidence from

Immigrants' Changing Eligibility," in *2015 Fall Conference: The Golden Age of Evidence-Based Policy*. Appam.

Eckenrode, J., M. Campa, D. W. Luckey, C. R. Henderson, R. Cole, H. Kitzman, E. Anson, K. Sidora-Arcoleo, J. Powers, and D. Olds (2010): "Long-term effects of prenatal and infancy nurse home visitation on the life course of youths: 19-year follow-up of a randomized trial," *Archives of Pediatrics & Adolescent Medicine*, 164(1), 9–15.

Elgen, I., K. Sommerfelt, and T. Markestad (2002): "Population based, controlled study of behavioural problems and psychiatric disorders in low birthweight children at 11 years of age," *Archives of Disease in Childhood-Fetal and Neonatal Edition*, 87(2), F128–F132.

Falk, Ö., M. Wallinius, S. Lundström, T. Frisell, H. Anckarsäter, and N. Kerekes (2014): "The 1% of the population accountable for 63% of all violent crime convictions," *Social Psychiatry and Psychiatric Epidemiology*, 49(4), 559–571.

Feigenbaum, J. J., and C. Muller (2016): "Lead exposure and violent crime in the early twentieth century," *Explorations in Economic History*, 62, 51–86.

Figlio, D., J. Guryan, K. Karbownik, and J. Roth (2014): "The effects of poor neonatal health on children's cognitive development," *The American Economic Review*, 104(12), 3921–3955.

Gailliot, M. T., and R. F. Baumeister (2007): "The physiology of willpower: Linking blood glucose to self-control," *Personality and Social Psychology Review*, 11(4), 303–327.

Galler, J. R. (2013): *Nutrition and behavior*, vol. 5. Springer Science & Business Media.

Garces, E., D. Thomas, and J. Currie (2002): "Longer-term effects of Head Start," *The American Economic Review*, 92(4), 999–1012.

Heckman, J. J., S. H. Moon, R. Pinto, P. A. Savelyev, and A. Yavitz (2010): "The rate of return to the HighScope Perry Preschool Program," *Journal of Public Economics*, 94(1), 114–128.

Hindelang, M. J., T. Hirschi, and J. G. Weis (1981): *Measuring delinquency*. Sage Publications Beverly Hills.

Hoagland, G. W. (1977): *The food stamp program: income or food supplementation?* Govt. Print. Off.

Hoynes, H., D. W. Schanzenbach, and D. Almond (2016): "Long-run impacts of childhood access to the safety net," *The American Economic Review*, 106(4), 903–934.

Hoynes, H. W., and D. W. Schanzenbach (2009): "Consumption responses to in-kind transfers: Evidence from the introduction of the food stamp program," *American Economic Journal: Applied Economics*, 1(4), 109–139.

Hoynes, H. W., and D. W. Schanzenbach (2012): "Work incentives and the food stamp program," *Journal of Public Economics*, 96(1), 151–162.

Johnson, R. C., and R. F. Schoeni (2007): "Early-Life Origins of Adult Disease: The Significance of Poor Infant Health and Childhood Poverty," *UC Berkeley Unpublished Manuscript*, pp. 1–42.

Katz, L. F., J. R. Kling, and J. B. Liebman (2001): "Moving to opportunity in Boston: Early results of a randomized mobility experiment," *The Quarterly Journal of Economics*, 116(2), 607–654.

Kelly, Y. J., J. Y. Nazroo, A. McMunn, R. Boreham, and M. Marmot (2001): "Birthweight and behavioural problems in children: a modifiable effect?," *International Journal of Epidemiology*, 30(1), 88–94.

Kling, J. R., J. Ludwig, and L. F. Katz (2005): "Neighborhood effects on crime for female and male youth: Evidence from a randomized housing voucher experiment," *The Quarterly Journal of Economics*, 120(1), 87–130.

Levitsky, D. A., and R. H. Barnes (1972): "Nutritional and environmental interactions in the behavioral development of the rat: Long-term effects," *Science*, 176(4030), 68–71.

Levy-Shiff, R., G. Einat, D. Har-Even, M. Mogilner, S. Mogilner, M. Lerman, and R. Krikler (1994): "Emotional and behavioral adjustment in children born prematurely," *Journal of Clinical Child Psychology*, 23(3), 323–333.

Lindo, J. M., and C. Stoecker (2014): "Drawn into violence: Evidence on "what makes a criminal" from the Vietnam draft lotteries," *Economic Inquiry*, 52(1), 239–258.

Liu, J., and A. Raine (2006): "The effect of childhood malnutrition on externalizing behavior," *Current Opinion in Pediatrics*, 18(5), 565–570.

Lochner, L., and E. Moretti (2004): "The effect of education on crime: Evidence from prison inmates, arrests, and self-reports," *The American Economic Review*, 94(1), 155–189.

Ludwig, J., and J. R. Kling (2007): "Is crime contagious?," *The Journal of Law and Economics*, 50(3), 491–518.

McCollister, K. E., M. T. French, and H. Fang (2010): "The cost of crime to society: New crime-specific estimates for policy and program evaluation," *Drug and Alcohol Dependence*, 108(1), 98–109.

Moffitt, T. E., D. R. Lynam, and P. A. Silva (1994): "Neuropsychological tests predicting persistent male delinquency," *Criminology*, 32(2), 277–300.

Olds, D., C. R. Henderson Jr, R. Cole, J. Eckenrode, H. Kitzman, D. Luckey, L. Pettitt, K. Sidora, P. Morris, and J. Powers (1998): "Long-term effects of

nurse home visitation on children's criminal and antisocial behavior: 15-year follow-up of a randomized controlled trial," *JAMA*, 280(14), 1238–1244.

Olds, D. L., L. Sadler, and H. Kitzman (2007): "Programs for parents of infants and toddlers: recent evidence from randomized trials," *Journal of Child Psychology and Psychiatry*, 48(3-4), 355–391.

Oreopoulos, P., M. Stabile, R. Walld, and L. L. Roos (2008): "Short-, medium-, and long-term consequences of poor infant health an analysis using siblings and twins," *Journal of Human Resources*, 43(1), 88–138.

Orfield, G. (1983): "Public school desegregation in the United States, 1968-1980," .

Piquero, A. (2001): "Testing Moffitt's neuropsychological variation hypothesis for the prediction of life-course persistent offending," *Psychology, Crime and Law*, 7(1-4), 193–215.

Raine, A. (2002): "The biological basis of crime," *Crime: Public policies for crime control*, 43, 74.

Raine, A., P. Brennan, B. Mednick, and S. A. Mednick (1996): "High rates of violence, crime, academic problems, and behavioral problems in males with both early neuromotor deficits and unstable family environments," *Archives of general Psychiatry*, 53(6), 544–549.

Reyes, J. W. (2007): "Environmental policy as social policy? The impact of childhood lead exposure on crime," *The BE Journal of Economic Analysis & Policy*, 7(1).

Salm, M., and D. Schunk (2008): "The role of childhood health for the intergenerational transmission of human capital: Evidence from administrative data," .

Sanbonmatsu, L., J. Ludwig, L. F. Katz, L. A. Gennetian, G. J. Duncan, R. C. Kessler, E. Adam, T. W. McDade, and S. T. Lindau (2011): "Moving to Opportunity for Fair Housing Demonstration Program–Final Impacts Evaluation," .

Smith, J. P., and G. C. Smith (2010): "Long-term economic costs of psychological problems during childhood," *Social Science & Medicine*, 71(1), 110–115.

Tibbetts, S. G., and A. R. Piquero (1999): "The influence of gender, low birth weight, and disadvantaged environment in predicting early onset of offending: A test of Moffitt's interactional hypothesis," *Criminology*, 37(4), 843–878.

Tonkiss, J., J. Smart, and R. Massey (1987): "Effects of early life undernutrition in artificially-reared rats 2. Subsequent behaviour," *Physiology & behavior*, 41(6), 555–562.

Vaughn, M. G., C. P. Salas-Wright, S. Naeger, J. Huang, and A. R. Piquero (2016): "Childhood reports of food neglect and impulse control problems and violence in adulthood," *International Journal of Environmental Research and Public Health*, 13(4), 389.

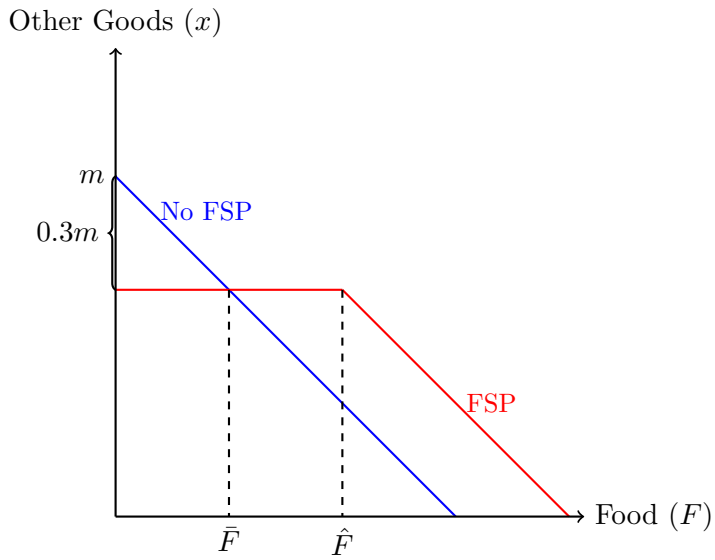
Virkkunen, M. (1986): "Insulin secretion during the glucose tolerance test among habitually violent and impulsive offenders," *Aggressive Behavior*, 12(4), 303–310.

Virkkunen, M., and M. Huttunen (1982): "Evidence for abnormal glucose tolerance test among violent offenders," *Neuropsychobiology*, 8(1), 30–34.

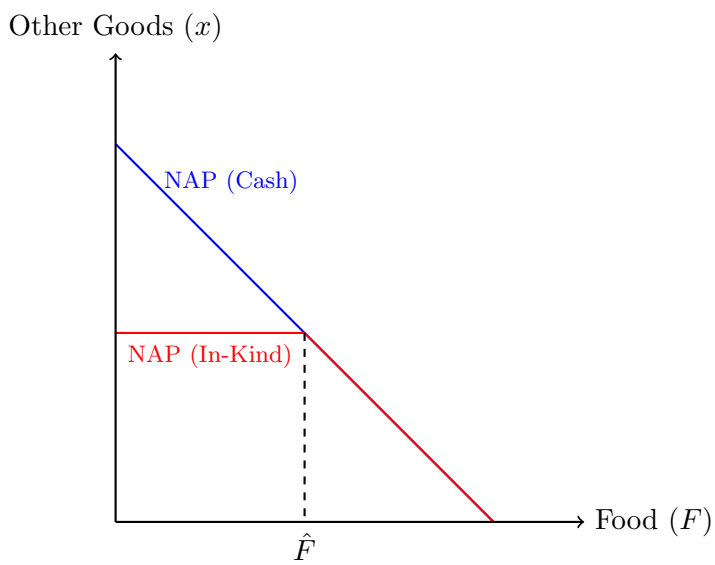
Whatson, T., J. Smart, and J. Dobbing (1976): "Undernutrition in early life: lasting effects on activity and social behavior of male and female rats," *Developmental Psychobiology*, 9(6), 529–538.

Figure 1: Household Budget Constraints and Nutritional Assistance Program Changes

(a) Initial Rollout of Food Stamps Program (FSP) with Purchase Requirement

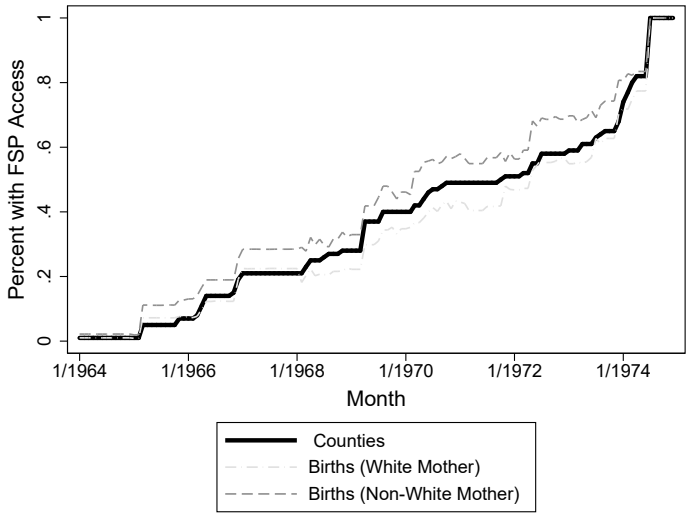


(b) Puerto Rico's Nutrition Assistance Program (NAP) Benefit Transition



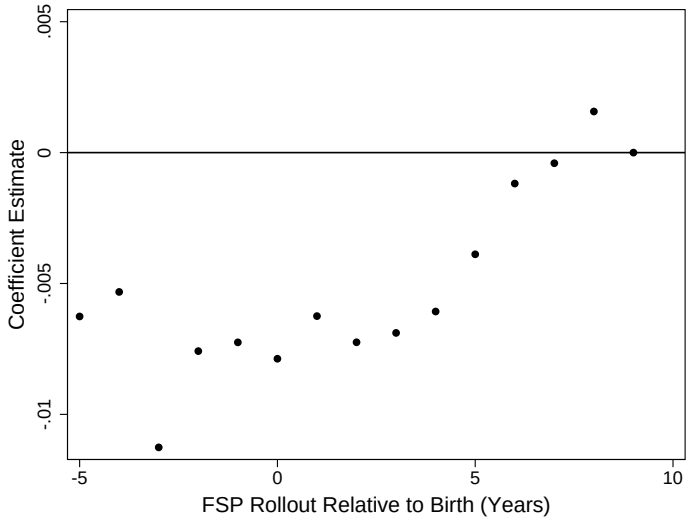
Note: In each figure, the blue line represents a household budget constraint prior to a change in the availability or features of a nutritional assistance program and the red line represents a household budget constraint after the change. *(a)* The blue line represents the budget constraint of a FSP-eligible household with income m that does not participate in the FSP or does not have access to it. The red line represents the budget constraint of an equivalent household that chooses to participate in the FSP. Until the 1970s, FSP participants were required to pay roughly 30% of income m (the "purchase requirement") to obtain the subsidized food coupons. *(b)* The blue line represents the budget constraint of a NAP recipient in Puerto Rico prior to 2001, when the program provided a 100% cash benefit. The red line represents the budget constraint of a NAP recipient after the transition to a benefit of the same total value but where 75% of the benefit was in-kind and 25% was in cash. $\hat{F}$ shows the amount of food purchasable with the in-kind portion of the benefits.

Figure 2: Access to FSP in North Carolina



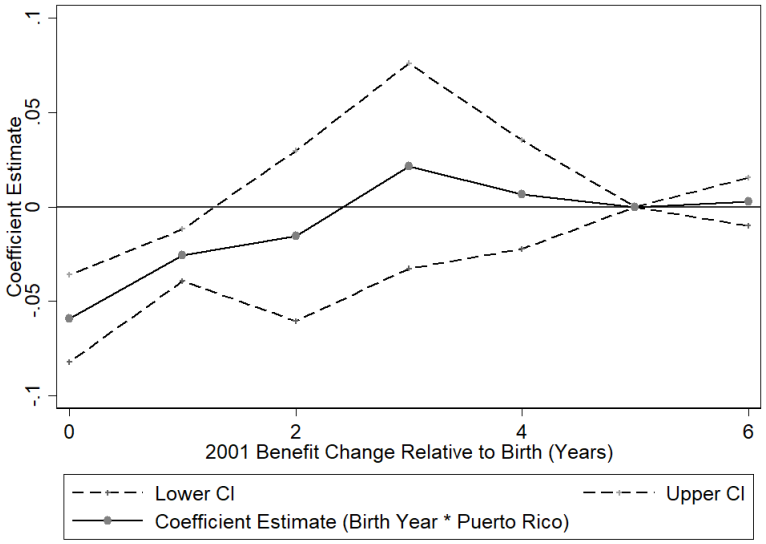
Note: Authors calculations using FSP administrative data obtained from Hoynes and Schanzenbach (2009) and aggregated county-month birth records by race from North Carolina.

Figure 3: Event Study for FSP Rollout: Any Conviction by Age 24 (North Carolina)



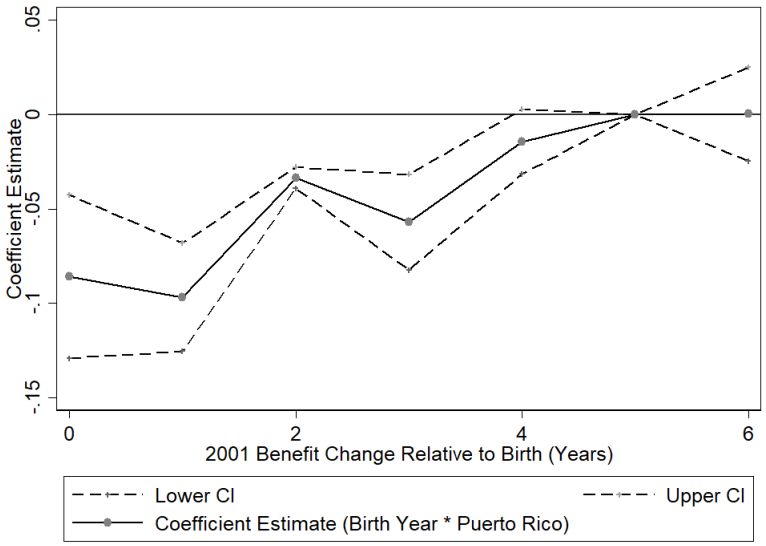
Note: Circles indicate coefficients on indicator variables for a cohort's implied age at FSP introduction in a county (negative ages reflect cohorts that were born after FSP introduction). End points of implied age range are bins including ages outside the range. Observations are at the birth county 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.

Figure 4: Event Study for Puerto Rico's Shift to In-Kind Benefit: Fighting



Note: Circles indicate coefficients on indicator variables for a cohort's implied age at shift to in-kind benefit introduction in Puerto Rico (relative to age 5). Dependent variable is an indicator for fighting (in adolescence). Regressions use data from the Youth Risky Behavior Survey (YRBS) and include state/territory fixed effects, birth year fixed effects, and age by gender indicators. Matched states contain those states matched to Puerto Rico on 1990 Census characteristics and available in the YRBS (Mississippi, Kentucky, and Tennessee). Standard errors are clustered at the birth state/territory level. 95% confidence intervals indicated by dashed lines.

Figure 5: Event Study for Puerto Rico's Shift to In-Kind Benefit: Bullied



Note: Circles indicate coefficients on indicator variables for a cohort's implied age at shift to in-kind benefit introduction in Puerto Rico (relative to age 5). Dependent variable is an indicator for bullied (in adolescence). Regressions use data from the Youth Risky Behavior Survey (YRBS) and include state/territory fixed effects, birth year fixed effects, and age by gender indicators. Matched states contain those states matched to Puerto Rico on 1990 Census characteristics and available in the YRBS (Mississippi, Kentucky, and Tennessee). Standard errors are clustered at the birth state/territory level. 95% confidence intervals indicated by dashed lines.

Tables

Table 1: Summary Statistics of Conviction and Arrest Rates

VARIABLES	(1) Mean
Panel A: North Carolina Data (monthly)	
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
Panel B: Uniform Crime Report Data (annual)	
Violent Part I Arrests per 100 Individuals	0.97
Property Part I Arrests per 100 Individuals	3.39
Observations	13,808

Note: 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, birthyear, and age. The arrest data are restricted to cohorts of individuals aged 18 to 24. The sample is restricted to cohorts who were born between 1964 and 1974.

Table 2: FSP in Early Childhood and Rate of Crime Conviction in NC by Age 24

	(1) Any	(2) Violent	(3) Property
Any Conviction	-0.013**	-0.005**	-0.003
	(0.007)	(0.002)	(0.003)
Mean	0.090	0.015	0.023
Felony Conviction	-0.007*	-0.002*	-0.001
	(0.004)	(0.001)	(0.002)
Mean	0.040	0.006	0.007
Observations	13,173	13,173	13,173

Note: 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 that are later convicted of a crime or particular crime type in NC 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 $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to cohorts who were born between 1964 and 1974. Significance levels indicated by: * ($p<0.10$), **($p<0.05$), ***($p<0.01$).

Table 3: FSP in Early Childhood and Rate of Crime Conviction in NC: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any Conviction	-0.019**	-0.014*	-0.014*	-0.014*	-0.013**	-0.013*	-0.013*	- 0.013
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.008)
Violent Conviction	-0.007***	-0.006*	-0.005*	-0.005*	-0.005**	-0.005**	-0.005**	-0.005*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Property Conviction	-0.003	-0.002	-0.002	-0.003	-0.003	-0.003	-0.003	-0.003
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	13,173	8,373	8,332	7,160	13,173	8,373	8,332	7,160
Birthyears: 1964-1974	Y	N	N	N	Y	N	N	N
Birthyears: 1968-1974	N	Y	Y	Y	N	Y	Y	Y
Birth County Chars. (1960) x Trend	N	N	N	N	Y	Y	Y	Y
County Natality Chars.	N	N	Y	Y	N	N	Y	Y
WOP Measures	N	N	N	Y	N	N	N	Y

Note: 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 that are later convicted of a crime or particular crime type in NC 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 $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Observations are at the birth county by birth month level and are weighted by the number of births in each county in 1964. The sample is restricted to cohorts who were born 1964-1974 or 1968-1974 as noted. The latter sample enables the inclusion of time-varying county characteristic controls (birth county by birth month level) constructed from natality files. These "County Natality Chars." include mean mother's age, fraction of "legitimate" births, fraction white births, and fraction of births with an attending physician in a hospital. WOP (War on Poverty) measures include an indicator for WIC availability at birth and an indicator for Head Start availability in birth county when birth cohort is age 4. Standard errors clustered at the birth county-level are in parentheses. Significance levels indicated by: * ($p<0.10$), **($p<0.05$), ***($p<0.01$).

Table 4: FSP in Early Childhood and Rate of Crime Conviction in NC: Heterogeneity

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

Note: 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 non-white individuals in a given birth county-birth month cohort that are later convicted of a crime or particular crime type in NC 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 $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to cohorts who were born between 1964 and 1974. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table 5: FSP in Early Childhood and Part I Arrests (per 100 individuals)

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

Note: 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 $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to individuals age 18-24 unless otherwise noted. Sample restricted to agencies accounting for at least 20% of a county's population. Sample sizes vary due to differences in reporting across offenses. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

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

	Cost Estimate *($ Million, 2015)*	Est. Δ Arrests *(1,000s)*	Est. Δ Crimes *(1,000s)*	Discounted Social Benefits *($ Million, 2015)* 0%	3%	5%	7%
McCollister, French, and Fang (2010) Crime Cost Estimates:							
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
			TOTAL:	**510,216**	**291,990**	**203,650**	**143,311**
Low Crime Cost Estimates:							
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
			TOTAL:	**229,285**	**131,217**	**91,518**	**64,402**

Note: 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 et al. (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 national reported offenses to arrests for each offense type. 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 Appendix C for details.

Table 7: Welfare Change from FSP (1964-1974) in Millions $2015
Transfer & Labor Mkt Losses vs. Crime Reduction Gains (18-24 Year Olds)

Social		Welfare Loss		Δ Welfare		Gain-Loss Ratio	
Discount Rate	Welfare Gain	(Min)	(Max)	(Min)	(Max)	(Min)	(Max)
McCollister, French, and Fang (2010) Crime Cost Estimates:							
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
Low Crime Cost Estimates:							
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

Note: 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 Table A13. 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.

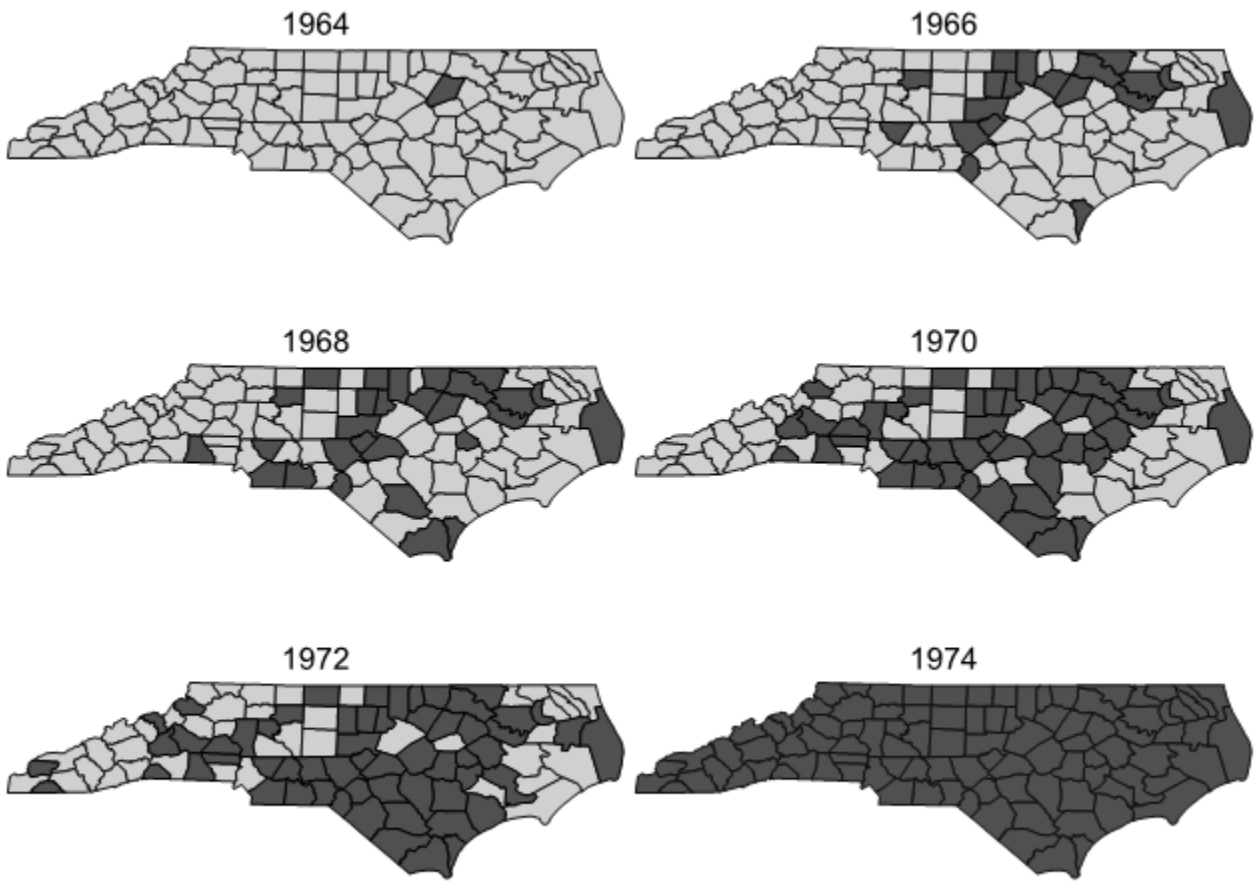
Table 8: Impact of Puerto Rico's Shift to In-Kind Benefit on Adolescent Outcomes

	Height (m) (1)	Normal Weight (2)	In a Fight (3)	Bullying (4)	Absent Unsafe (5)
A) Comparison: All States					
0-5 In-Kind Exposure	0.016***	0.041	-0.029**	-0.039*	-0.051**
	(0.005)	(0.027)	(0.014)	(0.024)	(0.022)
Obs	297,089	297,089	310,646	296,416	295,409
B) Comparison: High-Poverty States					
0-5 In-Kind Exposure	0.018***	0.051*	-0.058***	-0.046*	-0.066***
	(0.005)	(0.029)	(0.016)	(0.026)	(0.023)
Obs	45,544	45,544	48,217	43,074	45,664
C) Comparison: Matched States					
0-5 In-Kind Exposure	0.017***	0.050*	-0.049***	-0.040	-0.066***
	(0.005)	(0.030)	(0.016)	(0.028)	(0.023)
Obs	27,732	27,732	29,154	24,498	28,229
Mean	1.663	0.678	0.129	0.227	0.113

Note: Each panel by column shows the coefficient of interest (fraction of first five years of life under 75% in-kind benefit). The specification includes birth cohort fixed effects, state fixed effects, and age by gender fixed effects. The in-kind benefit period is defined as beginning in September 2001, when Puerto Rico switched from an all cash food supplement benefit to a primarily in-kind benefit. Each panel shows results for a different comparison sample. Panel A contains all states which included the relevant questions in the 2011, 2013, and 2015 YRBS surveys (including Puerto Rico). Panel B contains the 10 poorest states based on the 1990 Census and Puerto Rico. Panel C contains states matched to Puerto Rico on 1990 Census characteristics and available in the YRBS (Mississippi, Kentucky, and Tennessee). The time period of the sample covers survey years 2007-2015. Actual survey years included vary by state. Robust standard errors clustered at the state/territory level are in parentheses. Significance levels indicated by: * ($p < 0.10$) **($p < 0.05$), ***($p < 0.01$).

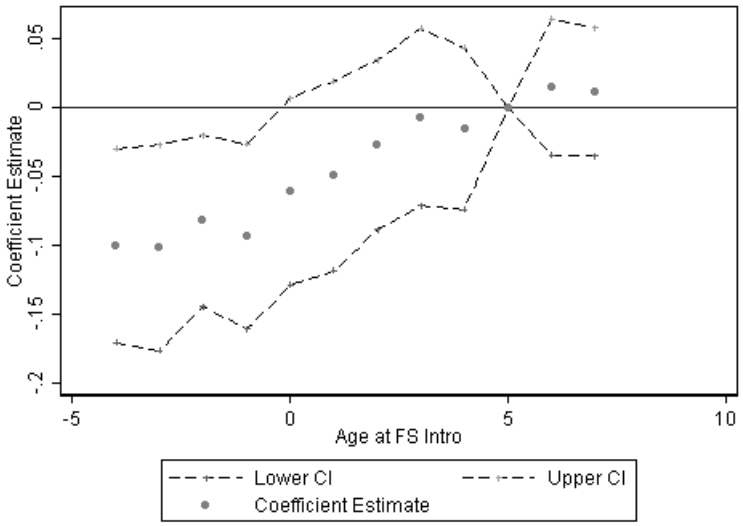
Appendix A: Supplementary Figures and Tables

Figure A1: North Carolina County Food Stamp Availability by Year



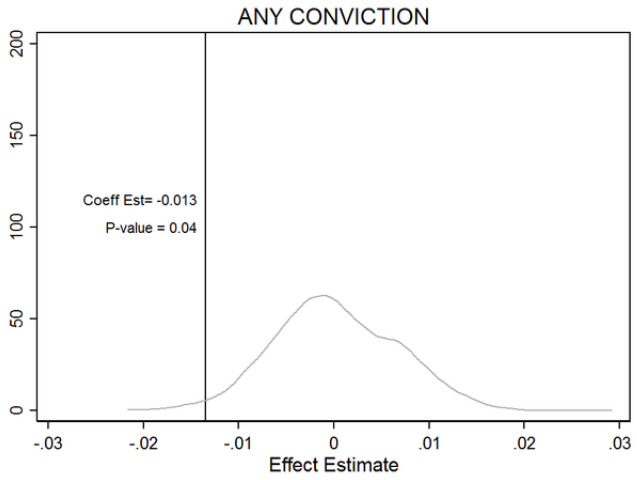
Note: Dark grey counties are those with a FSP in the given year according to FSP administrative data obtained from Hoynes and Schanzenbach (2009).

Figure A2: Event Study for FSP Rollout: Violent Crimes (UCR)



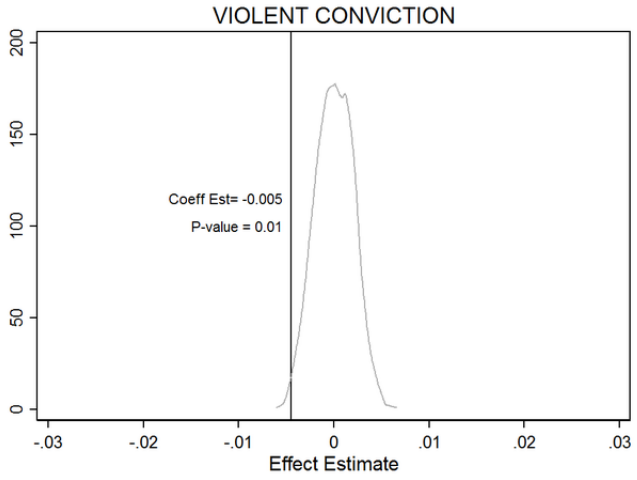
Note: Circles indicate coefficients on indicator variables for a cohort's implied age at FSP introduction in a county. Observations are at the county by birth cohort by age level. The dependent variable is the number of arrests per 100 individuals in a county cohort who are arrested at a particular age. 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 5, percent greater than age 65, and percent employment in agriculture. Standard errors are clustered at the county level. The sample is restricted to individuals age 18-24. Sample restricted to agencies accounting for at least 20% of a county's population.

Figure A3: All Crimes: Randomization Inference (North Carolina)



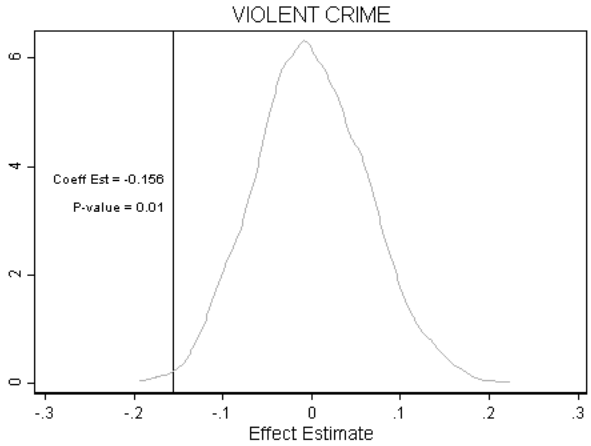
Note: The figure plots the smoothed distribution of coefficient estimates of 0-5 FS Exposure for 1000 random assignments of the timing (start month and year) of the introduction of Food Stamps in each county. The vertical line indicates the coefficient estimate using the actual timing of Food Stamp introduction in each county. P-value presented is the two-tailed statistic calculated as the share of coefficient estimates obtained under random assignment of Food Stamp introduction timing that are larger in absolute magnitude than the estimate using the actual timing of introduction.

Figure A4: Violent Crimes: Randomization Inference (North Carolina)



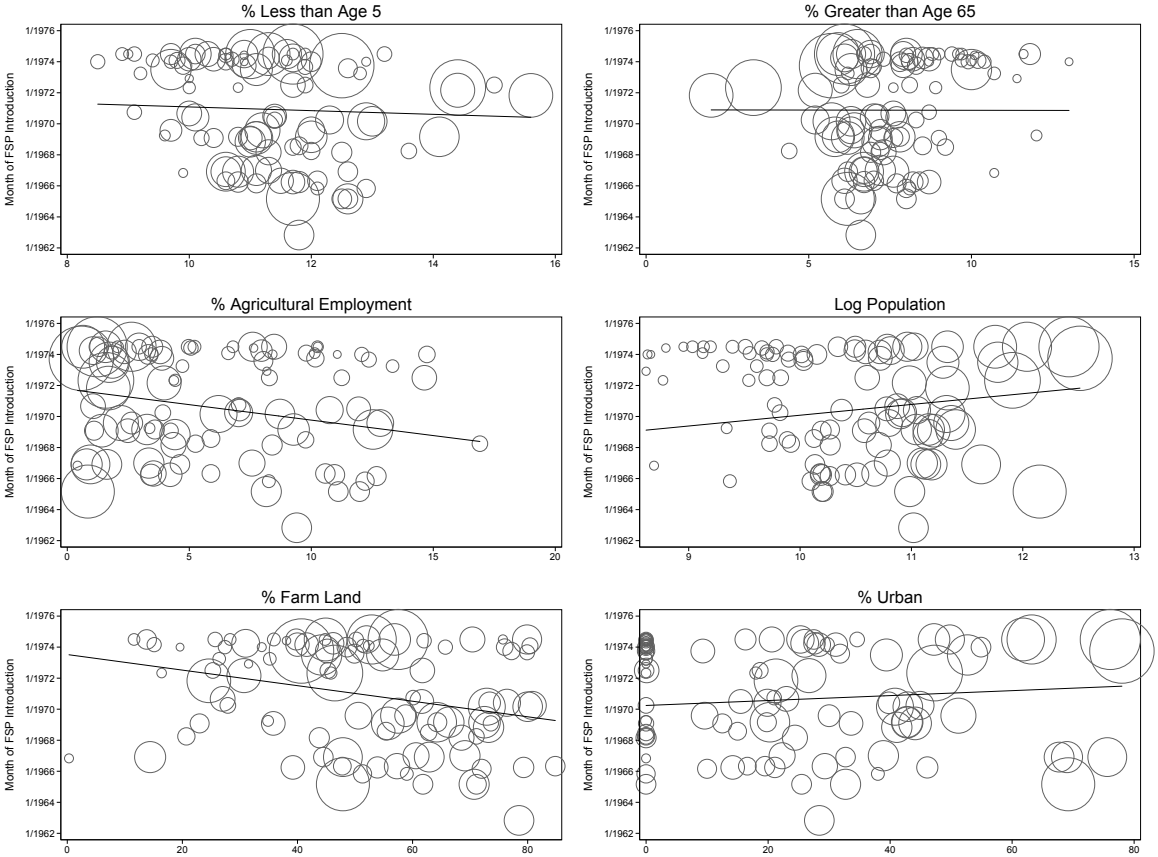
Note: The figure plots the smoothed distribution of coefficient estimates of 0-5 FS Exposure for 1000 random assignments of the timing (start month and year) of the introduction of Food Stamps in each county. The vertical line indicates the coefficient estimate using the actual timing of Food Stamp introduction in each county. P-value presented is the two-tailed statistic calculated as the share of coefficient estimates obtained under random assignment of Food Stamp introduction timing that are larger in absolute magnitude than the estimate using the actual timing of introduction.

Figure A5: Violent Crimes: Randomization Inference (UCR)



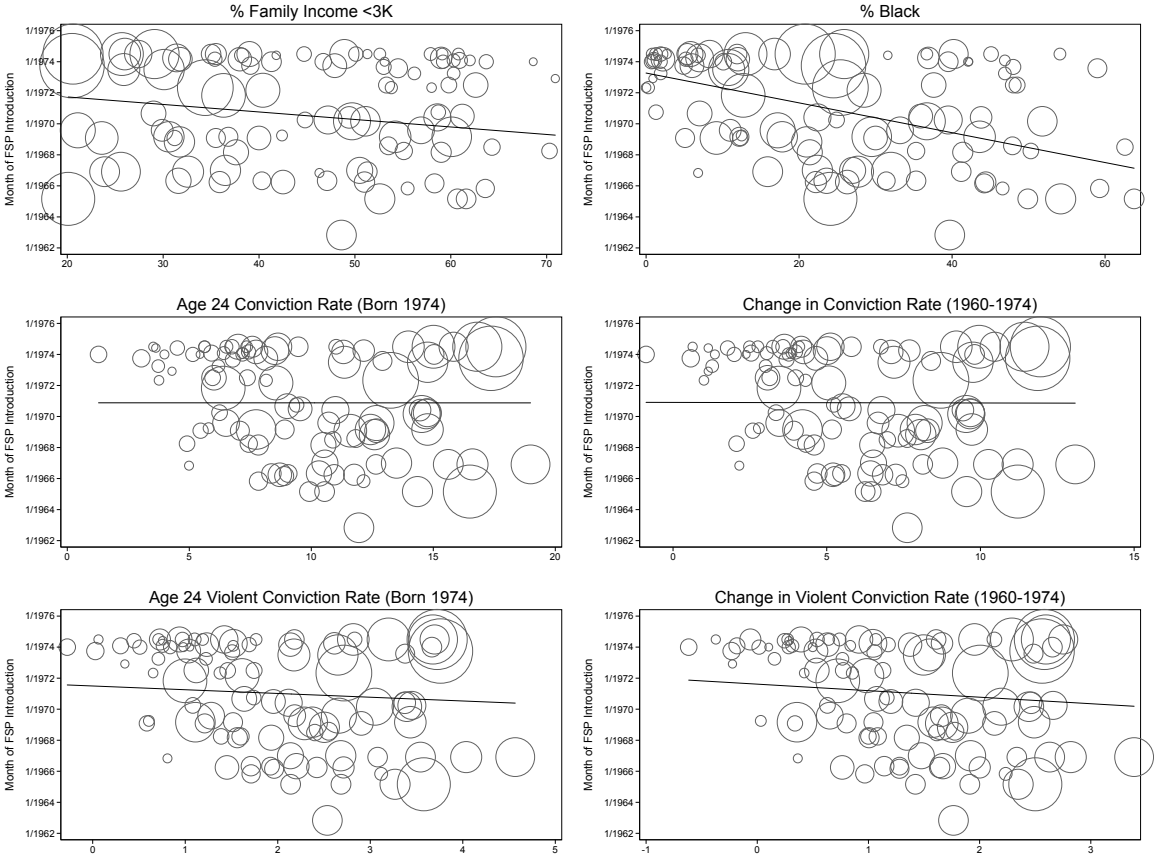
Note: The figure plots the smoothed distribution of coefficient estimates of 0-5 FS Exposure for 1000 random assignments of the timing (start year) of the introduction of Food Stamps in each county. The vertical line indicates the coefficient estimate using the actual timing of Food Stamp introduction in each county. P-value presented is the two-tailed statistic calculated as the share of coefficient estimates obtained under random assignment of Food Stamp introduction timing that are larger in absolute magnitude than the estimate using the actual timing of introduction.

Figure A6: Exploring Endogeneity of Food Stamp Adoption (North Carolina)



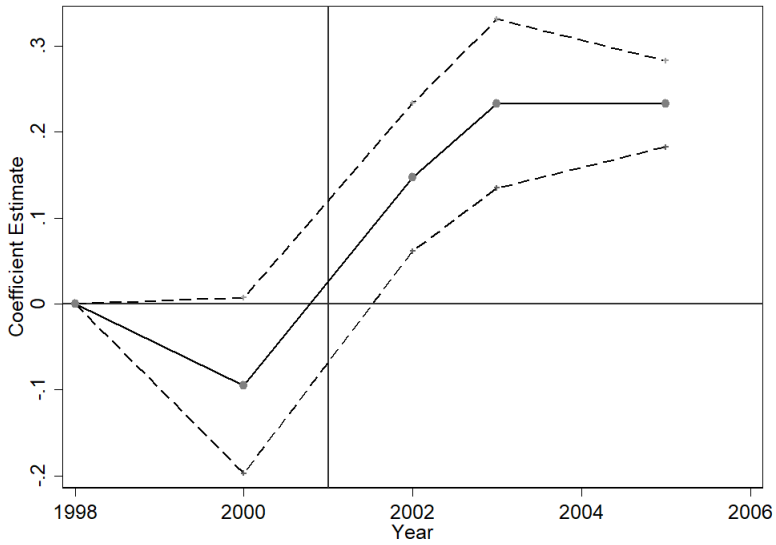
Note: Each scatter plot shows the relationship between baseline (1960) county characteristics and the month of FSP introduction in that county. The data are at the county-level and contain 99 (out of 100) counties in North Carolina for which the relevant information was available. Bubble size is weighted by number of births in each county in 1960.

Figure A7: Exploring Endogeneity of Food Stamp Adoption (North Carolina)



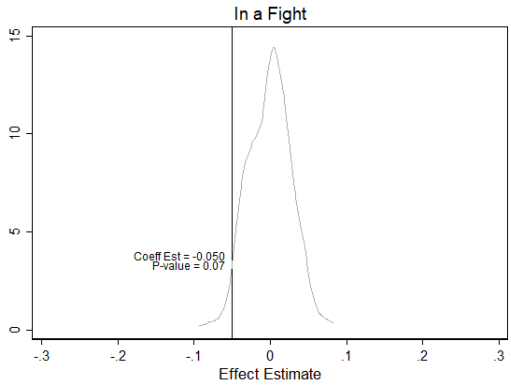
Note: Each scatter plot shows the relationship between baseline (1960) county characteristics and the month of FSP introduction in that county. The data are at the county-level and contain 99 (out of 100) counties in North Carolina for which the relevant information was available. The conviction rate (or change in conviction rate) variables are indexes predicted by baseline county characteristics. Bubble size is weighted by number of births in each county in 1960.

Figure A8: Event Study for Puerto Rico's Shift to In-Kind Benefit: Fruit and Veg Index

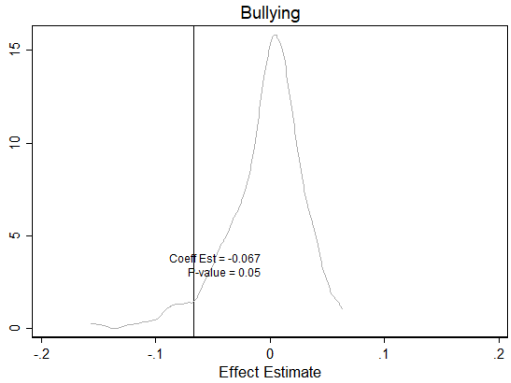


Note: Circles indicate coefficients on indicator variables for year interacted with Puerto Rico. The dependent variable is an index of fruit and vegetable consumption produced by the BRFSS. All specifications include state/territory and year fixed effects as well as age by gender indicators. Matched states contain those states matched to Puerto Rico on 1990 Census characteristics and available in the YRBS (Mississippi, Kentucky, and Tennessee). Standard errors are clustered at the state/territory level.

Figure A9: Randomization Inference (Puerto Rico)



(a) Fighting



(b) Bullied

Note: Each figure plots the smoothed distribution of coefficient estimates of 0-5 FS Exposure for all potential assignments of the timing (start year) of the shift to in-kind benefit in each state/territory. The vertical line indicates the coefficient estimate using the actual timing of shift in Puerto Rico. P-value presented is the two-tailed statistic calculated as the share of coefficient estimates obtained under all potential assignments of shift timing that are larger in absolute magnitude than the estimate using the actual timing of the shift.

Table A1: Impacts of FSP Introduction on Infant Low Birth Weight (North Carolina)

	All (1)	White (2)	Non-White (3)	HS Dropout (4)
FSP Access	-0.0019	-0.0005	-0.0050	-0.0039
	(0.0019)	(0.0024)	(0.0041)	(0.0031)
Percent of Mean	*-2.1%*	*-0.7%*	*-3.7%*	*-3.5%*
Mean	0.09	0.07	0.14	0.11
Obs	636,817	446,661	190,073	216,656

Note: Each column represents a separate OLS regression. The estimation sample includes observations at the individual level for 1968-1974 (years when detailed birth information is available) for births in North Carolina. FSP Access reflects whether FSP is available at birth for a given county-month cohort. The dependent variable is an indicator for low birth weight. 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 5, percent greater than age 65, and percent of employment in agriculture. Standard errors clustered at the birth county-level are in parentheses. Significance levels indicated by: * (p <0.10) **(p <0.05), ***(p <0.01).

Table A2: Impacts of FSP Introduction: Alternative Specification of Treatment (North Carolina)

	Any Crime		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
FSP Access IU-Birth	-0.018**	-0.014**	-0.007***	-0.005***	-0.004*	-0.004
	(0.007)	(0.007)	(0.003)	(0.002)	(0.002)	(0.003)
FSP Access Age 0-2	-0.014**	-0.012**	-0.005**	-0.004***	-0.003*	-0.003*
	(0.006)	(0.005)	(0.002)	(0.001)	(0.002)	(0.002)
FSP Access Age 3-5	-0.007**	-0.008***	-0.003***	-0.003***	-0.002**	-0.003**
	(0.004)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
Mean	0.091	0.091	0.016	0.016	0.023	0.023
Obs	13,173	13,173	13,173	13,173	13,173	13,173
Birth County Chars. (1960) x Trend	N	Y	N	Y	N	Y

Note: Each column 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. Each row represents a mutually exclusive indicator variable for the timing of first exposure to FSP access: In-utero to birth, birth to age 2, or age 3 to 5. The dependent variable is the fraction of individuals in a given birth county-birth month cohort that are later convicted of a crime or particular crime type in NC by age 24. All specifications include birth county and birth month fixed effects. Baseline (1960) county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to individuals born between 1964 and 1974. Significance levels indicated by: * ($p<0.10$), **($p<0.05$), ***($p<0.01$).

Table A3: Impacts of FSP Introduction: Counties with Pre-existing CDP Program (North Carolina)

	Any Crime		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
FSP IU-5 Exposure	-0.018**	-0.010	-0.006**	-0.004*	-0.002	-0.002
	(0.008)	(0.006)	(0.003)	(0.002)	(0.002)	(0.003)
Mean	0.090	0.090	0.015	0.015	0.023	0.023
Obs	11,985	11,985	11,985	11,985	11,985	11,985
Birth County Chars. (1960) x Trend	N	Y	N	Y	N	Y

Note: Each column 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 sample is restricted to birth cohorts between 1964 and 1974 *in the 91 counties with a pre-existing commodity distribution program (CDP)*. A county is determined to have had a CDP if it is mentioned in Federal Outlay Files, Aid to Families with Dependent Children surveys of case workers, or other documents from the National Archives and Records Administration (this information was obtained from Marianne Bitler). The dependent variable is the fraction of individuals in a given birth county-birth month cohort that are later convicted of a crime or particular crime type in NC by age 24. All specifications include birth county and birth month fixed effects. 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 5, percent greater than age 65, and percent of employment in agriculture. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A4: FSP in Early Childhood and Likelihood of Living in State of Birth (Census)

	(1)	(2)	(3)	(4)
	Weighted FS Exposure at Birth		Weighted 0-5 FS Exposure	
	18-24	18-30	18-24	18-30
All	-0.006	-0.009	-0.025	-0.012
	(0.008)	(0.011)	(0.016)	(0.014)
White	-0.008	-0.010	-0.028*	-0.014
	(0.009)	(0.010)	(0.017)	(0.013)
Non-white	0.016	0.004	0.010	0.017
	(0.019)	(0.019)	(0.031)	(0.040)
Male	-0.007	-0.007	-0.022	-0.008
	(0.008)	(0.011)	(0.015)	(0.014)
White	-0.008	-0.007	-0.025	-0.009
	(0.009)	(0.011)	(0.017)	(0.013)
Non-white	0.009	0.001	0.018	0.019
	(0.015)	(0.020)	(0.027)	(0.039)

Note: Each cell represents a separate OLS regression with standard errors clustered at the state of birth level (in parentheses). Observations are at the individual level from the 1990 and 2000 Census. Age restrictions indicated by columns. The dependent variable is whether an individual is currently living in his or her state of birth (nationwide mean is 70 percent versus 78 percent in North Carolina). The key explanatory variables are measures of Food Stamp availability for a birth cohort in a particular state. In columns (1) and (2), this is calculated as the share of a state's population with Food Stamp availability during an individual's year of birth. In columns (3) and (4) it is the weighted average of the FS exposure variable across counties in a state, where the weights are the number of births in each county in 1960. All specifications include birth state and birth year fixed effects as well as indicators for race, age, and sex. Significance levels indicated by: * ($p < 0.10$) **($p < 0.05$), ***($p < 0.01$).

Table A5: Exploring Endogeneity of Food Stamp Adoption

Dependent Variable (1)	Mean (2)	FSP Month Coeff (3)	% of Mean (4)
County Characteristics (1960)			
% Family Income <3K	37.4	-0.075	-0.2%
		(0.057)	
% Black	24.6	-0.141***	-0.6%
		(0.034)	
% Less than Age 5	11.7	-0.002	-0.0%
		(0.003)	
% Greater than Age 65	6.6	-0.000	-0.0%
		(0.004)	
% Agricultural Employment	4.3	-0.025*	-0.6%
		(0.014)	
Log Population	11.2	0.004	0.0%
		(0.004)	
% Farm Land	52.5	-0.103**	-0.2%
		(0.047)	
% Urban	40.3	0.076	0.2%
		(0.109)	
Indexes Constructed from County Characteristics			
Age 24 Conviction Rate (Born 1974)	11.5	-0.007	-0.1%
		(0.011)	
Δ Conviction Rate (1964-1974)	5.1	-0.004	-0.1%
		(0.008)	
Age 24 Violent Conviction Rate (Born 1974)	2.4	-0.003	-0.1%
		(0.004)	
Δ Violent Conviction Rate (1964-1974)	1.4	-0.002	-0.1%
		(0.003)	

Note: Estimates show the relationship between baseline (1960) county characteristics and the month of FSP introduction in that county. Each cell represents a separate regression, weighted by number of births in 1964, where the variable in column (1) is the dependent variable and the calendar month (normed to zero in January 1960) of FSP introduction is the sole independent variable. The data are at the county-level and contain 99 (of 100) counties in North Carolina for which the relevant information was available. The indexes are constructed by regressing the crime measure on county characteristics and using those coefficient estimates to predict the crime measure for each county. Robust standard errors are in parentheses.

Table A6: Food Stamps in Early Childhood and Rate of Crime Conviction: Additional Robustness (North Carolina)

	Any Crime		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
FSP IU-5 Exposure	-0.015*	-0.016*	-0.005***	-0.006**	-0.004	-0.004
	(0.008)	(0.009)	(0.002)	(0.003)	(0.003)	(0.003)
Mean	0.091	0.093	0.016	0.016	0.023	0.024
Obs	13,173	8,298	13,173	8,298	13,173	8,298
Birth County Chars. (1960) x Month-Year FE	Y	N	Y	N	Y	N
Consol. Statistical Area x Month-Year FE	N	Y	N	Y	N	Y

Note: Each column 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 that are later convicted of a crime or particular crime type in NC by age 24. All specifications include birth county and birth month fixed effects. Baseline (1960) county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Consolidated Statistical Areas (CSA) are defined by the U.S. Census Bureau and consist of two or more adjacent metropolitan and micropolitan statistical areas that have substantial employment interchange. Sample size changes result from some counties in North Carolina not being included in a CSA. Results are robust to combining these counties into an additional CSA. The sample is restricted to birth cohorts between 1964 and 1974. Significance levels indicated by: * ($p<0.10$), **($p<0.05$), ***($p<0.01$).

Table A7: FSP and Fertility (North Carolina)

	Log(Births)		Births	
	(1)	(2)	(3)	(4)
FSP Access	0.013	0.010	0.435	0.485
	(0.015)	(0.013)	(1.077)	(1.089)
Mean	3.8	3.8	78.1	78.1
Obs	13,173	13,173	13,173	13,173
Birth County Chars. (1960) x Trend	N	Y	N	Y

Note: Each column 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. The dependent variable is the number of births or log of the number of births. FSP Access reflects whether FSP is available in a given county-month. All specifications include birth county and birth month fixed effects. 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 5, percent greater than age 65, and percent of employment in agriculture.

Table A8: FSP and Rate of Crime Conviction of Non-Native Residents (North Carolina)

	(1) Any	(2) Violent	(3) Property
Conviction by Age 24	0.010	0.002	0.005**
	(0.008)	(0.002)	(0.002)
Mean	0.124	0.015	0.026
Conviction by Age 30	-0.002	-0.000	0.004
	(0.011)	(0.002)	(0.003)
Mean	0.201	0.027	0.037
Observations	1,100	1,100	1,100

Note: Each cell represents a separate OLS regression with standard errors clustered at the county level in parentheses. Observations are at the county by birth year level. The dependent variable for county c and birth cohort t is the number of individuals born outside of NC in year t who are convicted of a particular type of crime in county c (by age a) divided by the total number of individuals born outside of NC in year t that reside in county c at age a. It is constructed 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. All specifications include county and birth year fixed effects as well as baseline (1960) county characteristics interacted with a trend in birthyear. 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 5, percent greater than age 65, percent of employment in agriculture. The sample is restricted to individuals born between 1964 and 1974. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table A9: FSP and Rate of Crime Conviction in *Non-Birth* County (North Carolina)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Any	Any	Violent	Violent	Felony	Felony
0-5 FS Exposure	-0.014***	-0.011**	-0.008***	-0.006**	-0.003**	-0.004***
	(0.005)	(0.005)	(0.006)	(0.003)	(0.001)	(0.001)
Birth County Characteristics (1960) x Time Trend		X		X		X
Observations	13,173	13,173	13,173	13,173	13,173	13,173
Outcome Mean (1964 Birth Cohort)	0.039	0.039	0.005	0.005	0.015	0.015

Note: Each column 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. The dependent variable is the fraction of individuals in a given birth cohort that are later convicted of a particular crime type in a NC different than their birth county by age 24. All specifications include birth county and birth month fixed effects. 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 5, percent greater than age 65, percent of employment in agriculture. The sample is restricted to individuals born between 1964 and 1974. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table A10: FSP and Likelihood of Residing in One's County of Birth (NLSY 79)

VARIABLES	(1) Moved (79)	(2) Moved (80)	(3) Moved (81)	(4) Moved (82)
0-5 FS Exposure	-0.030	-0.029	-0.006	0.008
	(0.040)	(0.045)	(0.046)	(0.047)
Observations	5,420	5,215	5,243	5,219
Mean	0.444	0.465	0.475	0.490

Note: Each column represents a separate OLS regression with standard errors clustered at the birth county-level in parentheses. Observations are at the individual level. The dependent variable is indicated by the column title. For example, "Moved (79)" indicates an individual living outside of his or her birth county in 1979. Given the birth cohorts included in the NLSY 79 (1957-64), this includes individuals aged 15 to 22. Similarly, "Moved (82)" includes individuals aged 18 to 25. All specifications include birth county and birth year fixed effects; indicators for race, age, and sex; and baseline (1960) county characteristics interacted with a birth year time trend. 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 5, percent greater than age 65, percent of employment in agriculture. Significance levels indicated by: * (p <0.10) **(p <0.05), ***(p <0.01).

Table A11: Impact of Puerto Rico's Shift to In-Kind Benefit on Infant Low Birth Weight

		Mother's Educ		
	All Births	Dropout	HS Grad	Some College+
	(1)	(2)	(3)	(4)
A) Comparison: All States				
PR X POST	-0.0044***	-0.0075***	-0.0043**	-0.0030**
	(0.0010)	(0.0021)	(0.0018)	(0.0014)
Percent of Mean	*-5.6%*	*-8.1%*	*-5.1%*	*-4.4%*
Mean Low Birthweight	0.08	0.09	0.08	0.07
Obs	25,416,980	5,230,191	8,084,049	12,102,741
B) Comparison: High-Poverty States				
PR X POST	-0.0049***	-0.0068***	-0.0054***	-0.0039***
	(0.0010)	(0.0022)	(0.0018)	(0.0014)
Percent of Mean	*-5.1%*	*-5.8%*	*-5.4%*	*-4.8%*
Mean Low Birthweight	0.10	0.12	0.10	0.08
Obs	3,820,814	869,203	1,316,487	1,635,124
C) Comparison: Matched States				
PR X POST	-0.0045***	-0.0059**	-0.0054***	-0.0034**
	(0.0011)	(0.0025)	(0.0020)	(0.0015)
Percent of Mean	*-4.6%*	*-4.9%*	*-5.4%*	*-4.1%*
Mean Low Birthweight	0.10	0.12	0.10	0.08
Obs	1,519,818	350,266	513,074	656,478

Note: Each panel by column shows the coefficient of interest (Puerto Rico indicator interacted with Post period indicator) in a difference-in-difference regression where the dependent variable is an indicator for low birthweight. The specification includes birth month-year fixed effects, state fixed effects, and controls for mother's race, plurality of birth, and order of birth. The post period is defined as beginning in September 2001, when Puerto Rico switched from an all cash food supplement benefit to a primarily in-kind benefit. Each panel shows results for a different comparison sample. High-Poverty States are the 10 states with the highest poverty rates. Matched states contain those states matched to Puerto Rico on 1990 Census characteristics and available in the YRBS (Mississippi, Kentucky, and Tennessee). Each column shows results by mother's level of education. The time period of the sample covers 1998-2004. Robust standard errors in parentheses. Significance levels indicated by: * (p <0.10) **(p <0.05), ***(p <0.01).

Table A12: Impact of Shift to In-Kind Benefit on Food Consumption

	Fruit Juice (1)	Fruit and Veg Index (2)	Fruit (3)
A) Comparison: All States			
PR Post	0.227***	0.226***	0.531***
	(0.005)	(0.005)	(0.017)
Observations	1,198,295	1,236,218	1,236,218
B) Comparison: High Poverty States			
PR Post	0.225***	0.229***	0.558***
	(0.012)	(0.016)	(0.055)
Observations	213,962	222,938	222,938
C) Comparison: Matched States			
PR Post	0.241***	0.263***	0.701***
	(0.030)	(0.012)	(0.074)
Observations	88,219	92,739	92,739
Mean	0.808	2.332	2.670

Note: Each panel by column shows the coefficient of interest (Puerto Rico indicator interacted with Post period indicator) in a difference-in-difference regression where the dependent variable is indicated by the column title. Fruit juice and fruit indicated servings of each per day. The index is a summary of fruit and vegetable consumption generated by the BRFSS. The specification includes birth year fixed effects, state/territory fixed effects, and controls for age by gender indicators. The post period is defined as beginning in September 2001, when Puerto Rico switched from an all cash food supplement benefit to a primarily in-kind benefit. Each panel shows results for a different comparison sample. High-Poverty States are the 10 states with the highest poverty rates. Matched states contain those states matched to Puerto Rico on 1990 Census characteristics and available in the YRBS (Mississippi, Kentucky, and Tennessee). The time period of the sample covers 1998-2005. Robust standard errors clustered at the state level in parentheses. Significance levels indicated by: * ($p < 0.10$) **($p < 0.05$), ***($p < 0.01$).

Table A13: Estimates of the Welfare Loss from the FSP (1964-1974) in Millions of $2015

				Work Disincentive DWL			
		Transfer DWL		$\Delta h = -138, \Delta w = 1.70$		$\Delta h = -238, \Delta w = 3.07$	
Year	FSP Cost	$MDWL^G = 0.17$	$MDWL^G = 0.56$	$\epsilon_s = 0.1$	$\epsilon_s = 0.3$	$\epsilon_s = 0.1$	$\epsilon_s = 0.3$
1964	229	59	148	310	150	576	452
1965	256	66	165	1,033	501	1,919	1,505
1966	505	130	326	1,708	828	3,172	2,487
1967	809	208	523	2,423	1,175	4,499	3,527
1968	1,260	323	815	3,047	1,477	5,658	4,436
1969	1,602	411	1,036	4,049	1,964	7,520	5,895
1970	3,525	905	2,279	4,669	2,264	8,672	6,798
1971	9,176	2,355	5,934	5,139	2,492	9,543	7,481
1972	10,825	2,778	6,999	5,760	2,793	10,697	8,385
1973	11,787	3,025	7,622	6,549	3,176	12,162	9,534
1974	13,678	3,510	8,844	8,252	4,002	15,326	12,015
TOTAL	**53,651**	**13,768**	**34,692**	**42,939**	**20,822**	**79,745**	**62,514**

Min(Transfer DWL + Work Disincentive DWL) = 34,591
Max(Transfer DWL + Work Disincentive DWL) = 114,437

Note: The table shows back-of-the-envelope calculations of the welfare losses from FSP's contemporary work disincentives, program administrative costs, and distortionary taxes needed to raise government revenue. $FSPCOST_\tau$ is the total program cost in year τ, obtained from the Office of Management and Budget. $MDWL^G$ is the marginal deadweight loss from an additional $1 of government revenue. We use the range of $MDWL^G$ reported by Ballard, Shoven, and Whalley (1985) of $0.17 - 0.56$. Δh and Δw are the change in average hours worked and the change in average wage for female household heads with children, reported in Table 2 of Hoynes and Schanzenbach (2012). Finally, ϵ_s is the elasticity of labor supply for single women which ranges between 0.1 and 0.3, following a literature review by the Congressional Budget Office (McClelland and Mok, 2012). See Appendix B for details of the calculations and the sources for each parameter.

Table A14: Welfare Change from FSP (1964-1974) in Millions $2015
Transfer & Labor Mkt Losses vs. Crime Reduction Gains (13-40 Year Olds)

Social		Welfare Loss		Δ Welfare		Gain-Loss Ratio	
Discount Rate	Welfare Gain	(Min)	(Max)	(Min)	(Max)	(Min)	(Max)
McCollister, French, and Fang (2010) Crime Cost Estimates:							
0%	1,207,142	34,591	114,437	1,172,551	1,092,705	34.9	10.5
3%	628,090	34,591	114,437	593,500	513,653	18.2	5.5
5%	419,068	34,591	114,437	384,477	304,631	12.1	3.7
7%	285,755	34,591	114,437	251,165	171,318	8.3	2.5
Low Crime Cost Estimates:							
0%	538,368	34,591	114,437	503,778	423,931	15.6	4.7
3%	280,305	34,591	114,437	245,714	165,868	8.1	2.4
5%	187,074	34,591	114,437	152,484	72,638	5.4	1.6
7%	127,583	34,591	114,437	92,993	13,147	3.7	1.1

Note: The table presents the estimates of welfare gains from crime reduction due to FSP implementation and the range of estimates of the welfare losses due to the program as in Table 7, but assuming proportional effects for individuals aged 13 to 40. "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). See the description of Table 7 in the text and Appendix B for details.

Appendix B: Uniform Crime Reports (UCR) Data

We supplement our North Carolina analyses with analyses using the FBI's Uniform Crime Reports (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.[79]

We focus on Part I violent crimes (murder, robbery, and aggravated assault) and property crimes (larceny and burglary) for the period from 1980 to 2000. Because rape and arson are inconsistently reported during the early part of our sample, our main results exclude these crimes. This inconsistency hints at a broader concern with the UCR data. Because the UCR records are derived from the self-reported crime statistics of more than 10,000 city, county, and state law enforcement agencies, there are questions about the quality of the data. In addition to underreporting, variation in collection or categorization methods across agencies is a major concern. Despite these issues, prior research suggests that the UCR arrest data can be leveraged to produce unbiased estimates of the effects of policies on crime (e.g., Lochner and Moretti 2004; Gould et al. 2002; Bondurant et al. 2016). Furthermore, there is no reason to believe that the measurement error for certain cohorts of individuals is associated with the presence of Food Stamps in a county fifteen to twenty years prior.

[79] 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.

Appendix C: Welfare Calculations

In this appendix we provide the details of the calculations underlying our discussion of the welfare implications of the rollout of the FSP. First, we calculate back-of-the-envelope estimates of the dollar value of increased social welfare implied by our estimates of the reduction in violent crime due to FSP access in early childhood. Second, we compare these future benefits of the program to the potential contemporaneous efficiency costs of the program under various assumptions.

C.1 Quantifying Welfare Gains of FSP Crime Reduction

We convert our estimates of the effect of FSP access on later arrest rates to a dollar value benefit for each year of the FSP rollout from 1964-1974. First, we calculate the changes in the arrest rates, $\Delta R_{jat\tau}$, of offense j by age a individuals in year t that correspond to a given FSP year τ. This is given by,

$$\Delta R_{jat\tau} = \frac{1}{5} \times \eta_a \times \hat{\gamma}_j \times FS_{at\tau}, \tag{3}$$

where $\hat{\gamma}_j$ is the coefficient estimate from Equation 1 for offense j. $FS_{at\tau}$ is the fraction of the cohort aged a in year t that had access to the FSP (and were between age 0 and 5) in year τ. η_a adjusts for differences in the arrest rate between age a individuals and the 18-24 year olds used to estimate Equation 1 (for 18-24 year olds $\eta_a = 1$).[80] $\frac{1}{5}$ reflects the fact that γ_j is the coefficient for the fraction of 5 years of early childhood that a cohort had access to the FSP.

Next, we convert the changes in arrest rates by offense-age-year-FSP year, $\Delta R_{jat\tau}$, to changes in the number of offenses, $\Delta C_{jat\tau}$, as follows:

$$\Delta C_{jat\tau} = \frac{\tilde{C}_j}{\tilde{A}_j} \times \frac{POP_{at}}{100} \times \Delta R_{jat\tau}, \tag{4}$$

where POP_{at}, the age a population in year t, divided by 100 is used to convert a change in arrest per 100 people to a change in the number of arrests. $\frac{\tilde{C}_j}{\tilde{A}_j}$, the ratio of offenses to arrests for crime j, converts the change in the number of arrests to the change in the number of crimes.[81]

Finally, we apply estimates of the dollar value of each offense's social cost and discount the stream of future cost reductions associated with each FSP year τ for the period 1964-1974.

$$BENEFITS = \sum_{\tau=1964}^{1974} \sum_j \sum_a \sum_t \frac{1}{(1+r)^{t-\tau}} \times COST_j \times \Delta C_{jat\tau}. \tag{5}$$

Table 6 presents the resulting back-of-the-envelope social welfare calculations for various choices of discount rate, r, and social costs of crime, $COST_j$, counting only the effects on crimes committed by 18-24 year olds.

[80] η_a is operationalized as the ratio of the national average arrest rates for age a compared to age 18-24 for 1980-2000

[81] $\frac{\tilde{C}_j}{\tilde{A}_j}$ is operationalized conservatively as the minimum of the annual ratio of the total national crimes to arrests for offense j for 1980-2000

C.2 Quantifying Welfare Losses of the FSP

Economic theory suggests two primary areas where the rollout of the FSP may have had substantial contemporaneous distortionary effects that reduced efficiency. 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. Table A13 shows back-of-the-envelope estimates of these welfare losses (DWL_τ^G) in year τ, which total \$14-35 billion (\$2015) during the rollout period. Welfare losses from FSP transfers are calculated as follows:

$$DWL_\tau^G = MDWL^G \times (1 - P^A) \times FSPCOST_\tau + (1 + MDWL^G) \times P^A \times FSPCOST_\tau. \quad (6)$$

$MDWL^G$ is the marginal deadweight loss from an additional \$1 of government revenue. We use the range of $MDWL^G$ reported by Ballard, Shoven, and Whalley (1985) of $0.17 - 0.56$. P^A is the percent of program costs that do not go directly to program benefits for recipients. We use $P^A = 8.7\%$, the maximum that we observe during the rollout period.[82] $FSPCOST_\tau$ is the total program cost in year τ, obtained from the Office of Management and Budget.[83]

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, however the loss in efficiency may exceed the earnings loss if labor demand is not perfectly elastic. Table A13 shows back-of-the envelope estimates of deadweight loss from the contemporaneous labor market distortions of the FSP in year τ, which total \$63-\$80 billion (\$2015) over the rollout period. We calculate the welfare losses from labor market distortions, using Hoynes and Schanzenbach's estimates, as follows:

$$DWL_\tau^L = \frac{1}{2} \times \left(\frac{w\Delta h}{h\epsilon_s} + \Delta w\right) \times \Delta h \times N_\tau. \quad (7)$$

Where h, w, Δh, Δw are the average hours worked, wage, change in average hours worked, and change in average wage for female household heads with children, estimated in Table 2 of Hoynes and Schanzenbach (2012).[84] N_τ is the number of female household heads with children in counties with the FSP in year τ.[85]. ϵ_s is the elasticity of labor supply for single women which ranges between 0.1 and 0.3, following a literature review by the Congressional Budget Office (McClelland and Mok, 2012).

[82] We use annual total expenditure data by category (benefits vs other) available for $1969 - 1974$ from the USDA to calculate the maximum percent of annual program costs that are not directly transferred to beneficiaries during this period: 9%.

[83] Office of Management and Budget (2014). *Fiscal Year 2016 Historical Tables.* Table 11.3.

[84] w and Δw are constructed from reported hours, earnings, and the change in hours and earnings in Table 2 of Hoynes and Schanzenbach (2012). Our calculation assumes an initially undistorted labor market with simple linear labor supply and demand curves, where the labor supply curve is restricted to non-negative wages.

[85] N_τ is the number of female headed households in the U.S. with children in year τ (obtained from the Current Population Survey), multiplied by the percent of the population with FSP access in year τ (calculated by authors using county populations in 1970)

C.3 Welfare Gains vs. Losses

Table 7 compares our back-of-the envelope estimates of the welfare gains from the FSP's later crime reduction effects on 18-24 year olds to our back-of-the envelope estimates of the contemporaneous welfare losses from the program's administrative costs, use of distortionary tax revenue, and labor market distorting incentives. For a 3% social discount rate, we find a range of welfare changes due to the FSP's 1964-1974 rollout of $257 billion to $17 billion, depending on the various parameter choices. Looking across alternative parameter choices, our estimates of welfare gains exceed the range of welfare losses for social discount rates up to 7% when using the McCollister, French, and Fang (2010) estimates of the social costs of violent crimes, and social discount rates up to 3% when using the lowest violent crime costs in the literature reviewed by McCollister, French, and Fang (2010). Table A14 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. 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 (e.g. health) or potential future effects on margins other than crime (e.g. health, education). Notably, significant contemporaneous effects on infant mortality and food expenditure (Hoynes and Schanzenbach 2012; Almond, Hoynes, and Schanzenbach, 2011) as well as future effects on health (Hoynes et al. 2016) already exist in the literature.

Appendix D: Estimates of FSP Constrained vs. Unconstrained Households

The proportion of FSP-participating households that fall into different *ex ante* food consumption categories (e.g. $F < \bar{F}$ or $F > \hat{F}$ in Figure 1), provides one indication as to whether any long-run effects are likely to have arisen from direct nutrition improvements. We obtain rough estimates using the 1960-1961 Consumer Expenditure Survey (CES) which allows us to observe a nationally representative sample of food expenditures among soon-to-be-eligible households, shortly before the FSP rollout.[86] Under conservative assumptions and in the absence of the FSP, between 17 and 41% 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 purely an increase in food from the program.[87] 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 ($F > \hat{F}$), comprise 36 to 45% of FSP-eligible households.[88] Overall, these 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 which suggest that households used 53 to 86% of food subsidy income for the purchase of additional food (Hoagland, 1977).[89]

[86] We define the sample that will be FSP-eligible as those in income bins that fall entirely below the relevant state income threshold. CES only reports income in \$500-\$1,000 income bins. We use after-tax income as the closest proxy for net income. State FSP income eligibility thresholds as of 1966 were obtained from Clarkson (1975). We use the purchase requirement and benefit parameters of the 1975 program, which provide a conservative estimate of the fraction of households that would experience a pure increase in food consumption.

[87] The purchase requirement was roughly 30% of a household's net income during this period. The range reflects the fact that income in the CES is only reported in bins. We use the minimum and maximum of each household's income bin.

[88] We use FSP coupon allotment (by household size) in 1975 reported by Clarkson (1975) and deflate it to 1961 dollars.

[89] The higher 86% figure comes from households surveyed between 1968 and 1972, while the 53% 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%) 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.