Does Welfare Reduce Mortality? Evidence from the Supplemental Security Income Program

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Abstract

In this thesis, I analyze whether transfer payments from the Supplemental Security Income (SSI) program result in a reduction in infant mortality or homicide rates in U.S. counties. SSI is a government program that provides payments to low-income people who are either aged (65 or older), blind, or disabled. In 1990, in it's *Sullivan v. Zebley* decision, the Supreme Court relaxed medical eligibility criteria for children on SSI. This resulted in a dramatic rise in child enrollment rates and a large influx of income into some of the poorest areas in the U.S. I use an instrumental variable strategy, exploiting geographical variation in the expected treatment effect of this federal policy change, to determine whether this increase in child SSI enrollment had a causal impact on infant mortality or homicide rates. I find that a 1 percentage point increase in child SSI enrollment leads to a 1.45 percentage point reduction in infant mortality rates and an insignificant change in homicide rates.

Keywords: welfare, Supplemental Security Income, crime, health

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1 Introduction

Since their establishment, American welfare programs have been plagued by controversy. Many criticize welfare programs, and particularly cash assistance programs, for being wasteful, ineffective, or corrupt. Others argue that welfare programs are necessary to ensure the well-being of America's poor.

In an attempt to address this controversy, researchers and policy-makers have devoted extensive resources to studying the efficacy and impact of numerous welfare programs. For the most part, this literature has focused on the social and economic costs associated with welfare — namely, a reduction in labor supply (e.g. Hoynes (1996), Moffitt (1983), and Killingsworth (1983)). More recently, a small but substantial literature has examined the benefits of initiatives such as the Earned Income Tax Credit (EITC) and the Food Stamp Program (FSP). This literature has generally concluded that welfare programs improve health and education outcomes for their recipients (Hoynes et al. (2015), Strully et al. (2010), Baker (2008), and Hamad and Rehkopf (2016), Almond et al. (2011), Hoyne et al. (2016)). There remains, however, a large gap in the literature, both in terms of the welfare programs that have been studied and the outcomes that have been examined.

One anti-poverty initiative that has received scant attention in the literature is Supplemental Security Income (SSI). SSI is a federally administered welfare program that provides monthly stipends to low-income individuals who are either aged, blind, or disabled. Although it is less well known than its more traditional counterparts, such as EITC, Temporary Assistance for Needy Families (TANF), and Food Stamps, the program is a key part of the social safety net and helps millions of elderly and disabled Americans meet their basic needs of food, clothing, and shelter. In fact, in recent years SSI has become the largest nonwork-based cash welfare program in the U.S. In 2015, SSI paid over $54 billion to approximately 8.1 million people, 1.2 million of whom were children (SSI (2016)).

Over the past 30 years, the SSI program, and in particular the child SSI program, has undergone massive growth in enrollment. In 1974, SSI served just under 0.3 percent of the child population, compared to over 1.7 percent in 2013 (Duggan et al. (2015)). Importantly, most of this growth occurred over the 1990s, due to the famous *Sullivan v. Zebley* Supreme Court decision that expanded the medical eligibility criteria for children. In the 7 years following the decision, the child SSI program grew by over 200 percent. I use this exogenous federal policy change, in combination with county- and state-level variation in treatment intensity, to measure the impact of child SSI enrollment on mortality. In particular, I examine whether SSI payments to child recipients have an impact on homicide and infant mortality rates in US counties.

Using this strategy, I find that a 1 percentage point increase in the percent of children on SSI leads to a 1.4 percentage point reduction in infant mortality rates and an insignificant change in homicide rates. My results are robust to a number of alternate specifications.

My results are consistent with the literature on EITC and the FSP as well as with economic and sociological theory that suggests that poverty can lead to deteriorating health and increased crime rates. These results have important policy implications, as they demonstrate the benefits associated with SSI, and more generally with the American welfare state, which many politicians and voters have criticized. Importantly, these benefits may not be limited to the recipients and may improve the welfare of communities as a whole.

2 Background

SSI was established in 1974 by the Social Security Administration (SSA) as a replacement for three state programs: *Old-Age Assistance*, *Aid to the Blind*, and *Aid to the Permanently and Totally Disabled*. It was created, in part, to standardize disability and elderly assistance programs across states. As a result, SSI's medical eligibility criteria, need standard, and federal benefits are uniform throughout the country.

SSI essentially serves three separate low-income populations — blind and disabled children, blind and disabled adults, and adults aged 65 and above — each of which have distinct eligibility requirements and benefit levels. I focus on the child SSI population for two reasons: first, its well-documented impact on child recipients and families of those recipients, and second, its large policy-induced growth in enrollment during the 1990s.

SSI has been shown to play an important role in aiding impoverished families of youths with disabilities. Counting SSI benefits as a component of income reduces the number of SSI recipients living below the poverty line from 58 percent to 34 percent (Stegman and Hemmeter (2015)). This is particularly important given that SSI is not considered a temporary program and thus the the average time spent receiving SSI is much higher than that of other welfare programs (Rupp and Scott (1995)). Child SSI enrollment increases total household income by an average of about 22 percent and is associated with an 11 percentage point reduction in the probability that a child lives in poverty (Duggan and Kearney (2005)). Furthermore, higher SSI benefits have been shown to result in increased detection and treatment of health impairments in low-income children (Kubik (1999)).

The impact of SSI on its recipients is in part due to the generosity of its benefits, which are much higher than those of other cash assistance programs. Unlike many other programs, SSI benefits are inflation adjusted. Over the course of the past 30 years, these adjustments have resulted in SSI providing much higher payments to its recipients than most other welfare programs. For example, in 2016 the average SSI benefit, excluding state supplements, for a family of a disabled child was \$645. Although they vary state by state, Temporary Assistance for Needy Families (TANF, formerly Aid to Families with Dependent Children (AFDC)) benefits were generally much lower. For a family of three, the average TANF benefit in 2016 was \$303 in Florida, \$403 in Pennsylvania, \$409 in Virginia, and \$521 in Washington (Floyd and Schott (2014)). On average, monthly TANF benefits for an entire household are estimated to be approximately \$200 lower than the average SSI individual benefit (Wittenburg et al. (2015)). Even lower are EITC benefits, which averaged \$265 a month for a family with children in 2016.

2.1 SSI Enrollment Growth

Another important characteristic of the child SSI program is the relaxation of medical eligibility criteria, and subsequent enrollment growth, that resulted from the 1990 *Sullivan v. Zebley* Supreme Court decision. Before 1990, child SSI claims were evaluated using a "listing-only" standard. This meant that in order to qualify a child needed to have a condition that met the quite restrictive medical *Listing of Impairments* definitions. In 1990, however, the Supreme Court ruled that these requirements were inconsistent with the Social Security Act's statutory standard of "comparable severity." Two major changes to the child eligibility criteria ensued. First, the *Listing of Impairments* definitions that addressed children with mental impairments was expanded, allowing children with mental issues other than intellectual disabilities[1] be eligible for the program. As a result, under the new criteria, children with less severe mental impairments had the opportunity to enroll in SSI. Second, the SSA was forced to transition to an assessment of children's disabilities that relied less on the testimony of medical professionals and more on the that of parents, teachers, and counsellors. These assessments were known as "Individualized Functional Assessments" or "IFAs" and were designed to judge whether a child's impairment interfered with their ability to participate in age-appropriate activities — usually referring to school. This change, in conjunction with the change in the *Listing of Impairments*, allowed for a more flexible definition and diagnosis of child disabilities and spurred a large growth in the child SSI caseload (Duggan et al. (2015), Rupp et al. (2015), and Kubik (1999)).

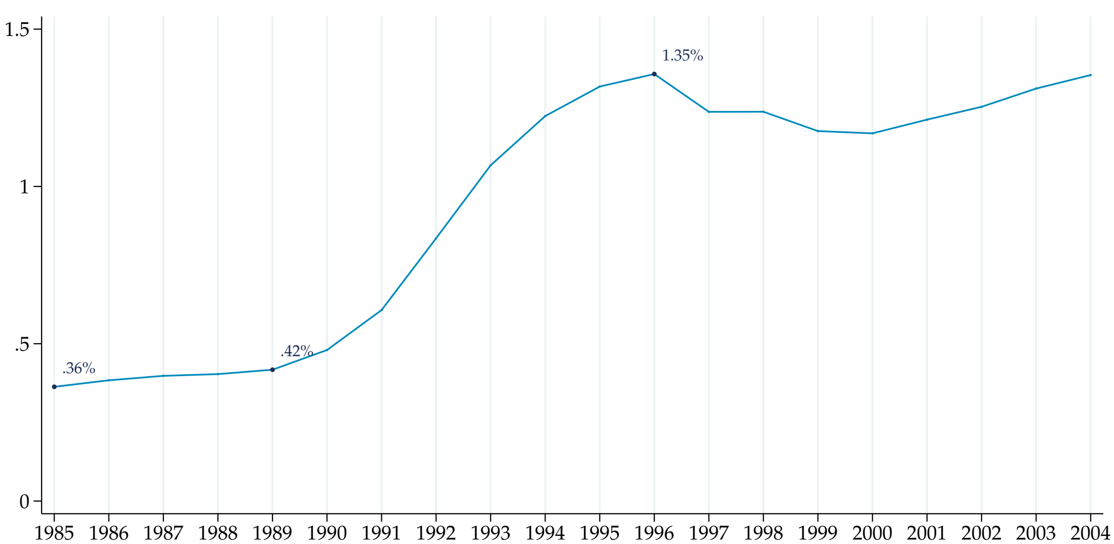
In 1996, in response to this caseload increase, congress tightened the SSI enrollment criteria. Under the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), children must have a medically determinable physical or mental impairment

[1] The term intellectual disabilities is defined by the American Association on Intellectual and Developmental Disabilities (AAIDD) as "a disability characterized by significant limitations both in intellectual functioning (reasoning, learning, problem solving) and in adaptive behavior, which covers a range of everyday social and practical skills." Past literature refers to intellectual disabilities as "mental retardation." Although "mental retardation" is still used in some contexts, the AAIDD has declared intellectual disabilities to be the preferred term for this impairment and since 2010, the term "mental retardation" is no longer used in government legislation and documents.

resulting in marked and severe functional limitations to be deemed eligible for SSI payments. PRWORA eliminated the IFA and required SSA to redetermine the eligibility of children at age 18 under adult rules. These changes resulted in the termination of nearly 100,000 children from the program in 1997 (Duggan et al. (2015)).

The *Zebley* decision had massive effects on the reach of the child SSI program. Figure 1 shows that after 1990, the program experienced a large growth in child SSI enrollment — defined as the percentage of all children that are enrolled in SSI. The program went from serving about 250,000 children in 1989 to just under 1,000,000 in 1996. Over these 6 years the enrollment rate increased by 220 percent, from 0.42 percent in 1989 to 1.35 percent in 1996. This growth stands in contrast to the 16 percent increase in enrollment that occurred over the four years prior to the decision. The drop in enrollment that occurred in 1997 as a consequence of PRWORA can also be seen in Figure 1.

Figure 1: Percent of Child Population on SSI



In reality, SSI enrollment rates underestimate the impact of the program. This is particularly true for child recipients, who are likely to have parents, siblings, and other live-in relatives who can access SSI stipends. Furthermore, SSI stipends have the potential to alter the economic well-being of recipients' communities as a whole. If, for example, families respond to the receipt of SSI income by increasing their demands for goods and services, then the economic activity of communities, and in particular low-income communities where SSI recipients are concentrated, may increase. Thus, it is likely that, in practice, the impact of the post-*Zebley* enrollment growth on impoverished communities was much larger than suggested by the simple percentage change in enrollment.

In addition to inducing a large growth in enrollment, the *Zebley* decision prompted a substantial shift in the characteristics of child SSI recipients. Quite a bit of the post-*Zebley* growth came from children with mental impairments other than intellectual disabilities — children who would have previously been ineligible for the program. As can be seen in table 1, the percentage of child SSI recipients who qualified because of these impairments, which included ADHD, eating disorders, and other behavioral conditions, increased from 6.3% in 1989 to 23.8% in 1995. As a result, cohorts of children who enrolled in SSI after 1990, on average, had less severe disabilities and a smaller proportion of physical disabilities than their preceding cohorts. This trend was somewhat reversed in 1996, presumably because the eligibility criteria was tightened under PRWORA.

Table 1: Share of SSI Child Caseload by Diagnosis and Year

	1986	1989	1992	1995	1998
Intellectual disabilities	43.5	42.0	41.4	38.3	37.9
Other mental impairments[a]	6.2	6.3	15.9	23.8	23.5
Diseases of the nervous system & sense organs	25.1	25.7	16.4	11.5	12.1
Congenital anomalies	10.3	9.1	6.3	5.1	5.4

[a]Includes mood disorders, autistic disorders, schizophrenic and other psychotic disorders, and childhood and adolescent disorders not elsewhere classified.

It is important to note that SSI enrollment did not grow uniformly across communities. Among counties with populations over 100,000, SSI child enrollment growth varied from a .039 percentage point growth in Litchfield, CT, to 5.07 percentage point growth in Portsmouth, VA. Although some of this variation is random, much of it can be explained by county- and state-level characteristics. For obvious reasons, counties with higher SSI enrollment rates in 1989 experienced larger absolute growths in enrollment. Furthermore, past literature has shown that local SSI enrollment growth was influenced by the level of SSA outreach efforts, the generosity of state welfare benefits, and the aggressiveness of state outreach (Stapleton et al. (2001)).

Table 2 shows the states with highest and lowest growth in child SSI enrollment in the years following the *Zebley* decision. In general, states located in the southeast of the United States experienced larger absolute growths in enrollment. Many of these same states had among the highest child SSI enrollment in 1989 (Figure 2), immediately before the *Zebley* decision.

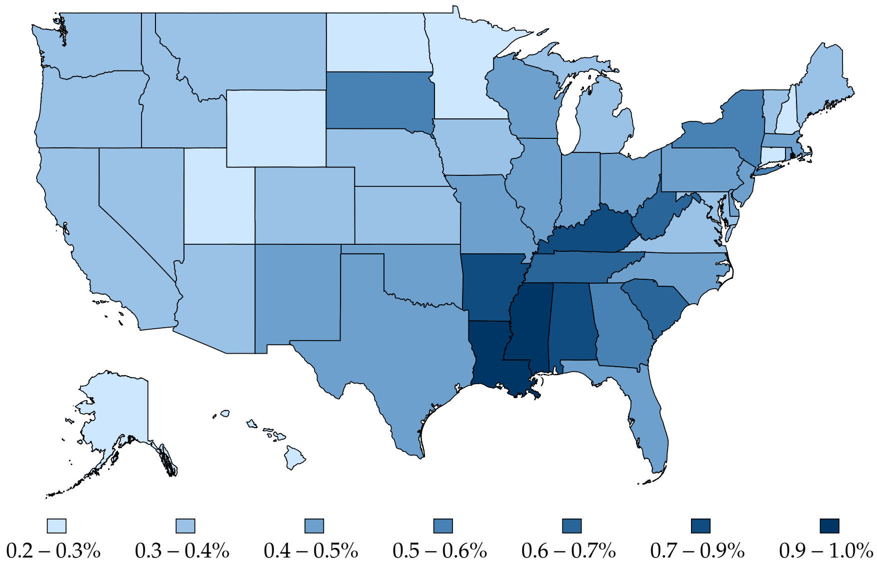
Table 2: States with Highest and Lowest Percentage Point Growth in Child Enrollment (89-96)

Low Growth		High Growth	
State	Growth	State	Growth
Hawaii	0.14	DC	1.72
Alaska	0.34	Alabama	1.81
Utah	0.43	Arkansas	1.87
Connecticut	0.44	Louisiana	2.14
New Hampshire	0.46	Mississippi	2.18

2.2 Geographic variation in SSI enrollment

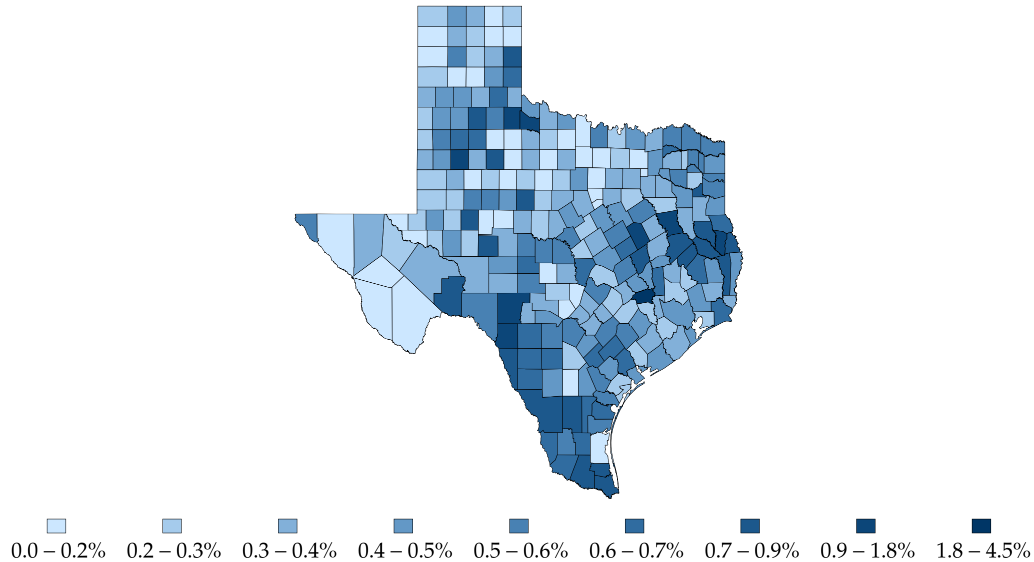
There is considerable inter-state variation in the percent of children on SSI. As can be seen from Figure 2, states in the southeast tend to have higher child SSI enrollment states, with Alabama, Arkansas, Louisiana, Mississippi, and Kentucky having the highest rates.

Figure 2: Percent of Children on SSI, by State, in 1989



There is also quite a bit of intra-state variation in the percent of children on SSI. If we isolate Texas, as can be seen in Figure 3, it is clear that there are a number of factors other than state-level AFDC generosity that drive geographical variation in child enrollment rates. Differential sizes of the impoverished and disabled populations are obvious components, but they do not explain all of the geographical variation. In fact, child SSI enrollment rates also vary quite a bit between counties with the same proportion of people in poverty. For example, in 1989, although Washington, TX and Hamilton, TX both had poverty rates of 17.1 percent, 1.74 percent of the Washington population was enrolled in SSI versus only 0.21 percent of the Hamilton population. This is a substantial difference given that in 1989, Washington had the highest percent of children on SSI in Texas whereas that of Hamilton was in the lowest quartile of children on SSI.

Figure 3: Percent of Children on SSI, by County, in Texas in 1989



Previous research has cited various drivers of SSI enrollment other than poverty and disability. Rutledge and Wu (2013) find that those in poor health are more likely to apply for and eventually enroll in SSI, making health status a significant predictor of SSI enrollment rates. Relatedly, counties with higher rates of low birth weight, ADHD diagnoses, and students receiving special education services tend to have higher child SSI enrollment rates. On the school-district level, the percent of children who are English language learners is negatively associated with child SSI receipt (Sevak, Schmidt, et al. (Sevak et al.)). Additionally, on average, counties with larger impoverished black populations have much higher SSI enrollment rates than their counterparts with large impoverished white or hispanic populations (Ben-Shalom et al. (2014); Schmidt et al. (2013)). Finally, as discussed above, Kubik (1999), Garrett and Glied (2000), Stapleton et al. (2001) find that many families used SSI and AFDC as substitutes, making the generosity of state AFDC benefits a strong predictor of child SSI enrollment rates.

3 Literature Review

As with any government policy, there is a key tradeoff with means-tested programs between the benefits they provide and the costs they impose, both to recipients and society as a whole. More often than not, discussion around means-tested programs focuses on their costs, which primarily come in the form of behavioral distortions — in response to changes in incentives — and fiscal costs to tax payers and to the government. In particular, most of the economic literature on the effect of welfare programs is interested in the degree to which they impact the labor supply and earnings of their recipients. More recently, a smaller literature has attempted to quantify the benefits of means-tested programs, including improvements in health and educational attainment of their recipients. There remains, however, a substantial gap in the literature in the examination of the impact of means-tested programs, and in particular SSI, on outcomes such as community crime and health.

A sizeable literature has examined whether means-tested programs lead people to work less or obtain less income and, consistent with economic theory, much of it concludes that they do in fact reduce labor supply. Although estimates vary, literature on the AFDC program concludes that the program's transfer payments reduce labor supply by anywhere between 10 and 50 percent – or by 208 and 1024 work hours per person per year (Hoynes (1996), Moffitt (1983), Killingsworth (1983)). Hoynes and Schanzenbach (2012) find that the Food Stamp Program has similar impacts. They exploit geographical variation in the timing of Food Stamp introduction in the 1960s and 1970s to find that the program led to reductions in employment and hours worked. These estimates are consistent with earlier results from Fraker and Moffitt (1988), who find that the Food Stamp Program reduces work by 1 hour per week for recipients. Findings from literature impact of EITC on labor supply, on the other hand, are somewhat mixed. Eissa and Liebman (1995) and Dickert et al. (1995) examine the impact of EITC by exploiting the large growth in EITC included by the various tax reforms through the 1980s and 1990s. Eissa and Liebman (1995) find that the 1986 EITC expansion increased labor force participation rate for single-mothers by 4 percent but found no significant effect on hours worked by any group. Although Dickert et al. (1995) also find that the labor force participation rate for single-mothers increase, instead by 6 percent, they conclude that this increase was offset by a significant reduction in hours worked.

There does exists literature, albeit limited, that examines the impact of SSI or earnings and labor supply. Using SIPP data, Neumark and Powers (2000) and Neumark and Powers (1998) exploit differential state supplementation of SSI benefits to estimate the effects of SSI on pre-retirement labor supply and savings. They find that higher SSI benefits discourage both. Deshpande (2014b) instead studies the impact of removing children from SSI on parental earnings and household income. Using Social Security Administration data, she exploits a change in the budget for medical reviews to create a regression discontinuity design around the probability that a child is removed from SSI. She finds that for every $1000 loss in SSI income, parents fully offset the loss in earnings, in particular by anywhere from $700 to $1400. Deshpande (2014a) employs a similar strategy to examine the impact of removing a child from SSI on future earnings. She uses the 1996 welfare reform as an instrument for the probability that a child is removed from SSI. Contrary to her previous finding, she concludes that over their lifetimes children, on average, experience an income loss of $73,000, or 80 percent of the original loss.

Empirical literature questioning how welfare programs affect quality of life is much less extensive. Within the existing literature, EITC is by far the best studied program. Hoynes et al. (2015), Strully et al. (2010), Baker (2008), and Hamad and Rehkopf (2016) all find positive consequences from increases in EITC. To examine the effect of EITC, many studies exploit the differential expansion of EITC with respect to family sizes that resulted from the 1993 Omnibus Reconciliation Act. The expansion caused families with two or more children to receive substantial rises in income, relative to similar families with only one child. It thus provides the framework for a difference-in-differences model, using families with two or more children as the treatment group. Using this strategy, Strully et al. (2010), Hoynes et al. (2015), and Baker (2008) find that EITC expansion reduces the incidence of low birth weight in children by a considerable margin, and Evans and Garthwaite (2010) find that the expansion resulted in an improvement in the self-reported health of affected mothers. Dahl and Lochner (2012) also exploit the differential expansion of EITC but instead use a fixed effects instrumental variable strategy to conclude that increasing EITC raises child math and reading test scores by 6 percent of a standard deviation. Finally, Hamad and Rehkopf (2016) use this same strategy and find that higher EITC benefits are associated with improved Behavioral Problems Index scores for children.

Some authors have also examined the impact of Food Stamps and other means-tested programs on health and community outcomes. Almond et al. (2011) employ a difference-in-differences model, exploiting variation across counties and over birth cohorts, in availability of the food stamp program, to find that exposure to Food Stamps 3 months before birth reduced the incidence of low birth weight. Hoyne et al. (2016) employ the same strategy and find that among children from disadvantaged families, access to Food Stamps in utero leads to a large and statistically significant reduction in the rate of metabolic syndrome, a disease associated with numerous negative health consequences. Tuttle (2016) uses a regression discontinuity design around the PRWORA to measure the impact of food stamps on criminal recidivism. In 1996, PRWORA imposed a lifetime ban on food stamps for people who commit drug felonies. Using this cutoff and inmate-level data, Tuttle (2016) finds that the ban increased recidivism, primarily among financially-motivated crimes. In an attempt to measure the impact of the social safety net as a whole, Foley (2008) examines the relationship between the timing of welfare payments (Food Stamps, TANF, and SSI) and crime spikes. In particular, he uses exogenous variation in the timing of monthly welfare payments between 12 cities to estimate the impact of the payments. He finds that cities exhibit temporal patterns in financially motivated crime. In particular, he finds that crime rates are significantly lower in the first 10 days of the month in jurisdictions where payments are made at the beginning of the month and that, overall, crime rates grow as cities get further away from the date in which payments are made. Akee et al. (2010) have similar results. In 1997, a casino opened on the Eastern Cherokee reservation in North Carolina. In the following years, the casino divided and evenly distributed its profits to all Native American families in the area. Exploiting this exogenous income shock Akee et al. (2010), employ a difference-in-differences model to measure the impact of unconditional transfer income on child outcomes. They find that, for children from treated households, an average additional household income of $4,000 at ages 16 and 17 reduces the incidence of the child ever having committed a minor crime by 22 percent. They attribute this reduction to an improvement in parenting ability.

Despite its importance to the US safety net, SSI has received much less attention in the literature. Federal expenditures on SSI are comparable to those of EITC and Food Stamps, yet literature examining the impact of SSI is much less common. That being said, there is some literature that examines the impact of SSI and, in particular, the impact of the child SSI program. Kubik (1999) uses the 1990 liberalization of child medical eligibility criteria, in combination with cross-state variation in the financial gain to enrolling in SSI, as an instrument for child SSI enrollment. He finds that increases in SSI benefits raise the likelihood that parents identify medical impairments in their children. Duggan and Kearney (2005) use data from the SIPP to estimate the impact of a child's enrollment in SSI on household income and probability of living in poverty. The longitudinal nature of the data allows them to control for unobserved, time-invariant differences across households. Using this strategy, they find that enrollment in SSI reduces the probability that a child lives in poverty by 11 percentage points and that for every $100 increase in SSI income, household income increases by $72.

The findings of past literature on the impact of SSI — and means-tested programs more broadly — suggests that removal from or enrollment in SSI can have community-level impacts but this hypothesis has not been empirically tested. This paper serves to fill this gap in the literature by examining the impact of growth in child SSI enrollment on health and crime rates in counties.

4 Methodology

4.1 Foundation

I wish to test whether an increase in county-level SSI income causes a reduction in infant mortality or homicide rates. If I were to directly examine the relationship between SSI enrollment and health or crime rates, I would likely find that counties with higher rates of SSI enrollment also tend to have higher rates of infant mortality and homicides. In fact, columns (1) and (2) of Table 3 — which presents the results from a simple regression of child SSI enrollment on infant mortality and homicide rates — confirms this relationship. These positive coefficients on SSI are likely the result of some underlying characteristics, such as poverty rate, which may impact both SSI enrollment and mortality outcomes. In fact, after controlling for time-invariant county-characteristics in columns (3) and (4), the estimates of these relationships change significantly. In the case of infant mortality, the sign on the coefficient reverses and in the case of homicide, the magnitude of the coefficient decreases by over 80 percent. Although the regressor remains endogenous, these coefficients present an interesting underlying relationship: after controlling for county characteristics, counties with higher levels of child SSI enrollment also tend to have lower levels of infant mortality but higher levels of homicide rates. To examine this relationship in more depth, I employ an instrumental variable strategy. Doing so allows me to estimate the causal relationship between child SSI enrollment and mortality outcomes.

Table 3: OLS: Child SSI Enrollment and Infant Mortality and Crime

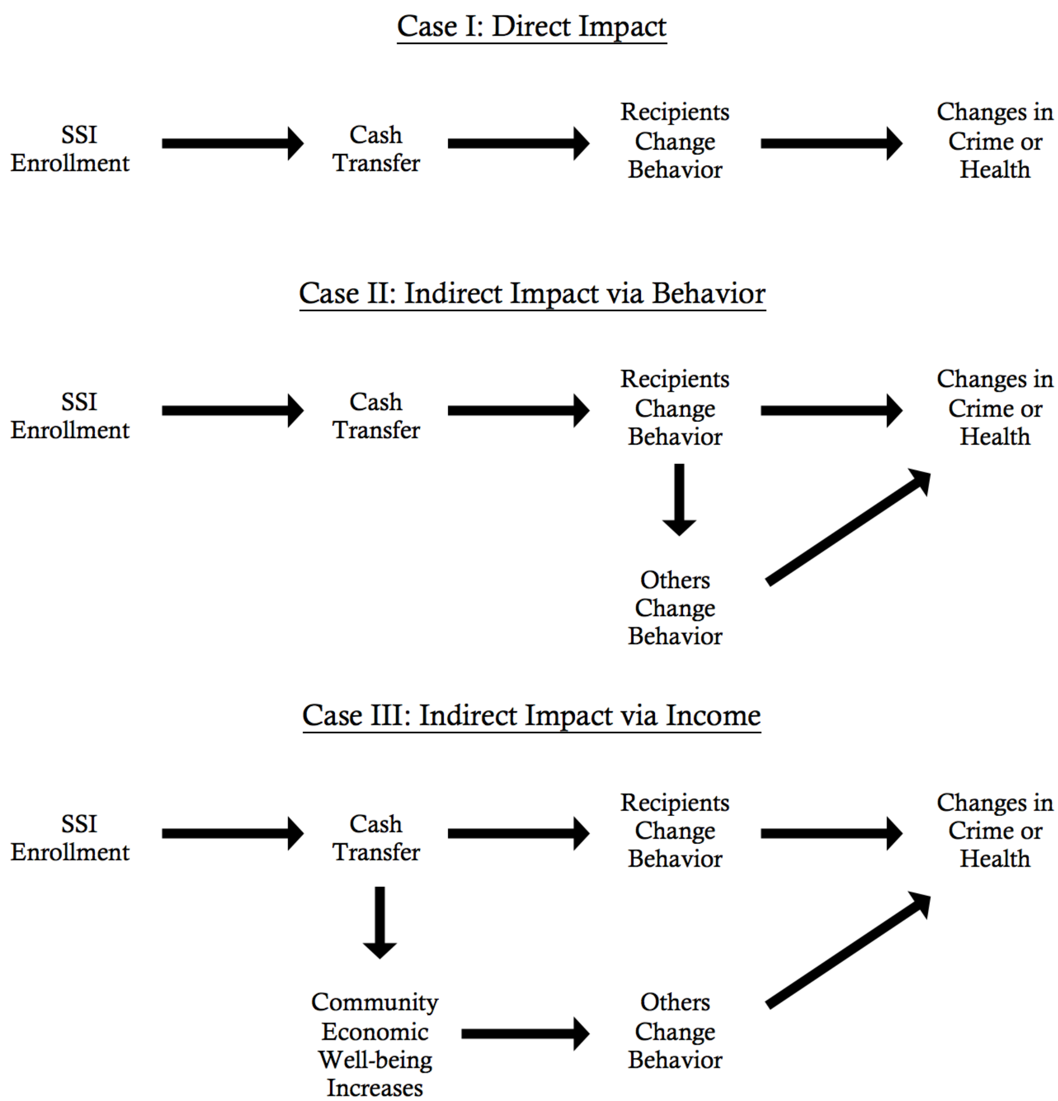
	(1)	(2)	(3)	(4)
	infmort	homicide	infmort	homicide
	β / SE	β / SE	β / SE	β / SE
SSI	3.020**	5.274**	–1.742**	0.830**
	(0.158)	(0.156)	(0.056)	(0.076)
Observations	5352	5352	5352	5352

* $p < .05$, ** $p < 0.01$. Standard errors in parentheses.

Note: Estimated using data from 1985-1996. Coefficients on constant and fixed effects terms excluded.

As illustrated in Figure 4, there are several channels by which SSI recipiency may impact infant mortality and homicide rates. First (case I of Figure 4), SSI income may have a direct effect on health or crime. This could occur if the influx of income families receive from SSI payments result in behavioral changes that either reduce their probability of committing a crime or improve their health. For example, it may be the case that receiving SSI income reduces the economic need for certain families to commit crime or allows them to buy more nutritious food, which may then improve their health. Results from Hoynes et al. (2015), Baker (2008), Hoyne et al. (2016), Tuttle (2016) and a variety of other papers support this hypothesis, as they each conclude that transfer programs have a direct impact on the health or crime patterns of their recipients. Second (case II of Figure 4), SSI income may both directly and indirectly affect health or crime if the behavioral changes of SSI recipients spread to non-recipients. Continuing with the example above, it may be the case that upon seeing others reduce their crime or begin to buy more nutritious food, non-SSI recipients alter their behavior in similar ways. In this case, changes in health or crime would be coming from two different channels. Third (case III of Figure 4), SSI income may both directly and indirectly affect health or crime if SSI transfer payments are dispersed to the community at large through an improvement in economic well-being. With more income, SSI recipients may increase their demand for goods and services, thereby improving the economic wellbeing in an area. This could in turn result in the establishment of better grocery stores, pharmacies, doctors offices, or other institutions that impact health or crime. In this case, behavioral changes sparked by an increase in income.

Figure 4: Ways in Which SSI May Impact Health and Crime



Importantly, the impact of SSI income could be contemporaneous or could take a few years to manifest. Because SSI is a means-tested program, its recipients are very poor and likely living with little to no disposable income. Thus, it may take a number of months or even years for the SSI payments to induce behavioral changes. Since I focus exclusively on contemporaneous impacts of child SSI enrollment, it's possible that this delay in behavioral change will render my results insignificant.

Although this paper attempts to establish a causal relationship between child SSI enrollment and mortality, it's important to note that, for numerous reasons, this is no easy task. SSI is a federally administered program that provides benefits to only a small subset of the population. Obtaining individual-level data is outside the scope of this paper and thus I cannot observe the actions of those directly impacted by the program. As I will discuss below, these characteristics render me unable to exploit many of the econometric strategies frequently used to establish a causal relationship.

Since SSI is a federal program administered by the Social Security Agency, any changes made to the program are instituted simultaneously across the country. In fact, the 1990 policy change that I exploit in my analysis impacted every county at the same time. This makes it very difficult to isolate the effect of SSI from the effect of other changes that occurred during or after 1990. In particular, if health rates or crime rates changed differentially between counties after 1990, I would be unable to account for this in my analysis. Thus, I rely on the assumption that counties followed on the same health and crime trends post-1990 as they did pre-1990. To help account for this issue, I use both county and year fixed effects. County fixed effects absorb any unobserved time-invariant characteristics, such as political beliefs or geography, on the county level that could be impacting crime or health. Year fixed effects absorb any unobserved country-wide shocks, such as elections and recessions, that could be impacting crime or health.

Additionally, only a small proportion of the population is enrolled in SSI, a characteristic that produces a very high noise to signal ratio in the data. Assuming that the parents and siblings of SSI recipients — in addition to the recipients themselves — are directly impacted by enrollment in the program, at best, the *Zebley* decision directly impacted 3 to 4 percent of the population. Thus, even if enrolling a child in SSI has a large effect on mortality, this effect could be masked by the mortality outcomes of the remaining 96 percent of the population. As a result, my study allows for, and measures the impact of, a possible spillover effect. Since I cannot isolate those directly impacted by SSI income (SSI recipients and their families) from those indirectly impacted by SSI income (the county at large) I measure the impact of both channels together.

4.2 Identification Strategy

To examine whether SSI enrollment impacts county-level health and homicide rates, I exploit inter- and intra-state variation in the predicted treatment "intensity" of the 1990 *Zebley* decision. As discussed earlier, there was significant variation in absolute child SSI enrollment growth after the *Zebley* decision. Importantly, the size of SSI growth in a county is well predicted by a combination of pre-*Zebley* county and state characteristics, in particular the size of the AFDC-eligible population in a county and the magnitude of the financial incentive to enrolling in SSI, from AFDC, in the state. I thus use these characteristics, in combination with the treatment effect of the 1990 decision, as an instrument for SSI. My empirical strategy is essentially a county-level adaptation of an IV strategy used by past literature to measure the impact of child SSI enrollment. I hypothesize that, relative to the average county, counties with higher treatment intensities experienced higher reductions in crime and improvements in health status. This would suggest that an increase in SSI enrollment is associated with better health and less crime.

4.2.1 State-Level Variation in Treatment Intensity

After the relaxation of SSI eligibility criteria in 1990, a significant portion of children on AFDC became newly eligible for SSI. As a result, many AFDC families opted to switch one of their children to SSI. This created a large-scale transition of children away from AFDC to SSI.

Much of this transition was driven by financial incentives. Legally, families can receive both SSI and AFDC benefits, but a child cannot. Since AFDC benefits are a function of family size, families with two or more children can remove one child from AFDC and loose only a portion of their benefits. If they then enroll this child in SSI, they gain the full SSI benefit, which is not dependent on family size. Because SSI benefits are relatively high, some families were able to increase their net benefits by switching one child from ADFC to SSI. Importantly, the size of the potential gain to switching to SSI varied quite a bit state-to-state. This is because AFDC benefit schedules, unlike those of SSI, are set on the state level and thus depend on the welfare policies, budget size, and political leanings of the state. In 1989, the size of the potential gain to enrolling in SSI varied by $173 a month, from $401 in California to $574 in Missouri. The calculation of this gain for a family of three and a family of two in Alabama and Massachusetts is demonstrated in Table 4.

Table 4: Financial Gain from Switching One Child to SSI

	AFDC benefits			SSI benefits	Financial Gain/Loss	
	Family of 2	Family of 3	Difference	Child	2 Children	1 Child
Alabama						
1989	$88	$118	$30	$368	$338	$280
1995	$137	$164	$27	$458	$431	$321
Massachusetts						
1989	$446	$539	$93	$368	$275	-$78
1995	$486	$579	$93	$458	$365	-$28
Formula	(a)	(b)	$(c) = (b) - (a)$	(d)	$(d) - (c)$	$(d) - (a)$

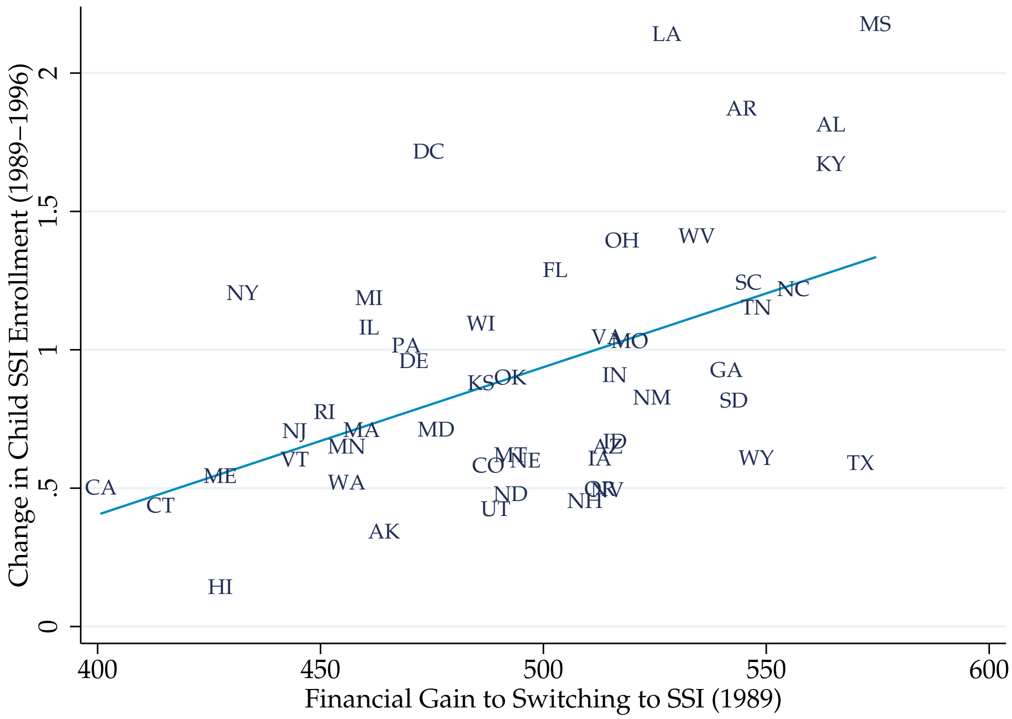
In addition to family-level incentives to switch children to SSI, state governments had incentives to switch children from AFDC to SSI. This is because unlike SSI, the AFDC program is funded mostly by states. Thus, by encouraging families to switch children to SSI, states could save money by shifting the costs of welfare payments to the federal level (Kubik (2003)).

The transition from AFDC to SSI was made possible both by the *Zebley* decision and the relatively high rate of disability among the AFDC population, which made many families eligible for both programs. Acs and Loprest (1999) find that in 1990, about 12 percent of AFDC families had children with disabilities. Notably, these were most commonly mental, rather than physical, impairments: impairments which were newly eligible for SSI after 1990. Stapleton et al. (2001) instead find that only 5 percent of children on AFDC have a reported disability. Although the latter estimate appears quite small, it is still meaningful, as it accounts for about 345,000 children— 77 percent of the child SSI caseload at the time (Stapleton et al. (2001)).

A large body of research has shown that families and states did in fact respond to these incentives, sparking a major transition of children from AFDC to SSI from 1990 onward. Kubik (1999) finds that the size of child SSI enrollment growth is positively associated with the generosity of state AFDC benefits, Stapleton et al. (1995) find that a 10 percent reduction in AFDC benefits was associated with a 1 percentage point increase in SSI enrollment, and Schmidt and Sevak (2004) find that in states aggressively pursuing welfare reform, female headed households were 21.6 more likely to receive SSI. Furthermore, Kubik (2003) and Stapleton et al. (1995) find evidence that states actively encouraged families to switch their children over to SSI from AFDC and played a key role in the size of SSI growth. In fact, past literature has found that anywhere from 32 to 52 percent of the child SSI enrollment growth from 1990 to 1996 can be attributed to transitions from children formerly on AFDC (Garrett and Glied (2000), Stapleton et al. (2001), Ziliak (2004)).

My data also supports this conclusion. Figure 5, plots the change in child SSI rates between 1989 and 1996 against the SSIGain variable in 1989. It shows that states with the highest financial gain to enrolling in SSI saw over a 2 percentage point increase in child SSI enrollment whereas states with the lowest gain saw just a 0.5 percentage point increase in child SSI enrollment.

Figure 5: Change in Child SSI Enrollment Rates (1989-1996) and Financial Gain to Enrolling in SSI from AFDC (1989)



Following Kubik (1999), I exploit this state-level variation in the financial gain to enrolling in SSI, in combination with the 1990 liberalization of medical eligibility criteria induced by the *Zebley* decision, as an instrument for child SSI enrollment. Because AFDC benefits are set on the state-level, and as will be shown later, are largely uncorrelated with SSI enrollment before 1990, they can plausibly be considered a source of exogenous variation.

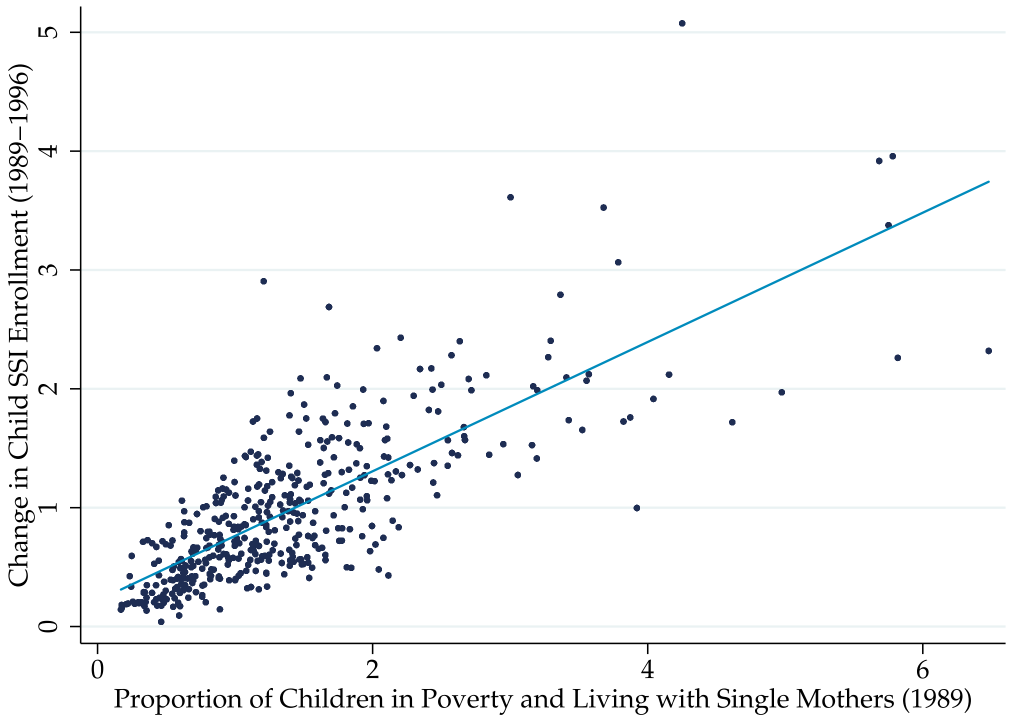
4.2.2 County-Level Variation in Treatment Intensity

Because the financial gain to enrolling in SSI from AFDC can only be measured on the state level, using it alone would not allow me to exploit any intra-state variation and thus limit my statistical power. As a result, I choose to interact the financial gain to enrolling in SSI with the size of the AFDC-eligible population, proxied by the proportion of children in poverty and living in households headed by single-mothers.

To qualify for AFDC, families must be very poor and include at least one child under the age of 18 who can be considered deprived of parental support. During the 1990s, this almost exclusively translated to families in poverty headed by single-mothers. Furthermore, transitions from AFDC to SSI were strongest among children in single-mother households and among households with two or more children. Stapleton et al. (2001) find that AFDC children in single-mother households are 3.3 times more likely to apply for SSI than their peers in single-father households, and 4.5 more likely than their peers in both-parent households. Thus, children in poverty with single-mothers are both the primary recipients of AFDC and the primary source of AFDC to SSI transfers.

My data supports evidence of a strong relationship between the size of the AFDC-eligible population, proxied by the proportion of children in poverty and living with single-mothers, and the growth in child SSI enrollment rates post-*Zebley*. Figure 6 plots the change in child SSI rates between 1989 and 1996 against the AFDCpop variable in 1989. It shows that counties with the highest levels of AFDCpop saw a 2.3 percentage point increase in child SSI enrollment whereas states with the lowest levels of AFDCpop saw a 0.14 percentage point increase in child SSI enrollment.

Figure 6: Change in Child SSI Enrollment Rates (1989-1996) and Proportion of Children that are AFDC-eligible (1989)



4.2.3 Empirical Specification

To estimate the impact of child SSI enrollment on infant mortality and homicide rates, I perform the following regression:

$$\begin{aligned} y_{ct} =& \delta_c + \gamma_t + \beta_1 SSIgain_{st} + \beta_2 AFDCpop_{c,1989} * POST_t + \\ & \beta_3 SSIgain_{st} * POST_t + \beta_4 \widehat{SSI}_{ct} + \epsilon_{ct} \end{aligned} \tag{1}$$

The dependent variable is the outcome y in county c and year t; this includes homicide rates per 100,000 people and infant mortality rates per 1,000 people. $AFDCpop_{c,1989}$ is the proportion of children in poverty and living with single-mothers in county c in 1989. $SSIgain_{st}$ is the inflation adjusted financial gain to enrolling in SSI from AFDC, for a family with 2 children, per \$100 (as defined in Table 4) in state s and year t. $POST_t$ is an indicator equal to one after 1989. δ_c are county-fixed effects; these control for time-invariant characteristics across counties. γ_t are time-fixed effects; these control for nationwide shocks in each year. Finally, $\widehat{SSI}_{ct}$ is estimated by:

$$\begin{aligned} SSI_{ct} =& \delta_c + \gamma_t + \lambda_1 SSIgain_{st} + \lambda_2 AFDCpop_{c,1989} * POST_t + \lambda_3 SSIgain_{st} * POST_t + \\ & \lambda_4 AFDCpop_{c,1989} * SSIgain_{st} * POST_t + \epsilon_{ct} \end{aligned} \tag{2}$$

My instrument is therefore the interaction between the financial gain to enrolling in SSI in year t, the size of the child AFDC eligible population in 1989, and a treatment indicator equal to one after 1989: $AFDCpop_{c,1989} * SSIgain_{st} * POST_t$. I hypothesize that an increase in child SSI enrollment results in a reduction in infant mortality and homicide rates. If this is the case, one would expect β_4, the coefficient on $\widehat{SSI}_{ct}$, to be statistically significant and negative. Furthermore, because $AFDCpop$ and $SSIgain$ measure the *Zebley* treatment intensity, one would expect the coefficient on λ_4 to be positive and significant. Note that if I were to include the $POST$ and $AFDCpop_{c,1989}$ variables on their own, they would drop out of the regression due to the inclusion of county and year fixed effects. For this reason, I omit them from my specification.

Of course, size of the AFDC-eligible population and the generosity of state AFDC payments are not randomly assigned and may be endogenous to health and crime. In fact, it is likely that counties with higher proportions of AFDC-eligible populations tend to have higher crime rates and worse health for reasons other than SSI receipt. Poverty is one such reason. Thus, on its own, the size of the AFDC population cannot be considered a valid instrument. If the same can be said for the $SSIgain$ variable, then the exclusion restriction will be violated and my estimates will not be causal. In order to accurately measure the impact of SSI on health and crime, it must be the case that the only channel by which the interaction of the size of the AFDC-eligible population and the financial gain to enrolling in SSI impacts homicide and infant mortality rates is through SSI.

There is quite a bit of evidence to support this requirement. Because SSI is a federal program, the 1990 decision to alter eligibility criteria is uncorrelated with county-level crime and health rates. There is concern, however, that the degree to which counties respond to the change in eligibility criteria could be tied to unobservable characteristics of the population that also impact crime and health. For this reason, I interact the policy change with the financial gain to enrolling in SSI and size of the AFDC eligible population: an observable source of variation in the degree to which counties respond to the change in eligibility criteria. Importantly, because the size of the financial gain to enrolling in SSI is determined by state policy, it can reasonably be considered exogenous to county-level outcomes. Empirical support for this is provided in my findings section.

One plausible threat to the exogeneity requirement is that states may have changed their AFDC benefit schedules in response to changes in health or crime trends. If this is the case, then the instrument may capture the impact of underlying state trends, rather than SSI enrollment itself. To account for this, I run a falsification test using the adult SSI enrollment rate in place of the child rate. If my results are driven by state or county trends, they should be similar for the adult and child enrollment rates.

5 Data

The ideal data to examine whether child enrollment in SSI impacts health or crime would consist of individual-level variables on the health status, crime history, income, and SSI enrollment of children and families as well as their communities at large. Having individual-level longitudinal data would allow me to isolate unobserved differences across families, which otherwise could bias the estimates. Individual-level data is, however, quite difficult to obtain and using it was infeasible for this study. I thus instead use county-level data from a variety of sources to examine the impact of SSI enrollment on community outcomes. All variables of interest are described below and summarized in Table 6.

5.1 Health and Crime Data

To measure infant mortality and homicide rates, I use natality and mortality data from the National Center of Health Statistics. These datasets contain detailed information from every birth and death certificate in the U.S. including precise cause of death, as well as the age, gender, race, weight, and location of residence of the decedent or born infant. Using the precise cause of death, I am able to tally the number of homicides and infant deaths in a given county and year.

Importantly, I choose to use NCHS mortality data rather than crime datasets from the FBI to measure homicide rates. This is because the use of FBI crime data for county-level analysis is highly controversial and possibly unreliable. Every year, the FBI publishes the Uniform Crime Reports (UCR), which include county-level statistics on a variety of violent and property crimes[2]. The data set is, however, somewhat flawed. The UCR program is voluntary and although participation among police departments is increasing, there remain a number of departments that do not report statistics (Maltz (2006), Rokaw et al. (1990)). Furthermore, even among departments that do report, there is a substantial amount of missing data. This creates a bias in the data that some researchers have argued renders it ineffective for county-level analysis (Maltz and Targonski (2002)). Others claim that the missing data isn't systematic and does not affect the outcomes of crime analyses (Lott Jr and Whitley (2003)). Since estimates of missingness in UCR data are significant, especially among smaller counties (Maltz (1999)), I instead use the NCHS mortality data to extract homicide counts.

One drawback of using NCHS data is that not all counties are represented in the data. This is because, in order avoid disclosing the identity of an individual, the NCHS chooses to suppress the county codes of observations from individuals who live in counties with populations under 100,000[3]. They do not, however, suppress the state codes of these observations.

[2]The reports include data on murder and nonnegligent manslaughter, forcible rape, robbery, aggravated assault, burglary, larceny-theft and motor vehicle theft.

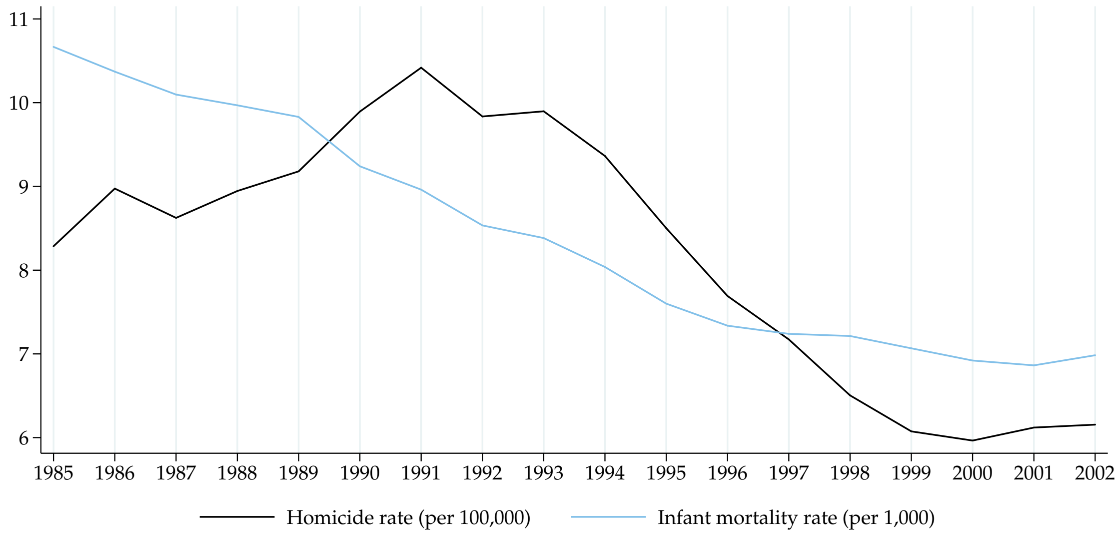
[3]as determined by the 1980 census

To account for this issue, I aggregate the observations from all counties in a state with populations under 100,000 into a "rest of state" category. This result is one additional "county" observation per state, per year that represents all of the smaller counties in a state. The process generates 450 observations a year, with some states containing many observations and others — such as Alaska, Hawaii, North Dakota, and Wyoming — containing just 1 observation. Thus, in some states I am unable to exploit any intra-state variation. This only occurs, however, in low-population states. In many states, such as California, Florida, New York, Ohio, Pennsylvania, and Texas there are over 20 counties with populations over 100,000, which allows me to examine variation between counties with equivalent financial gains to switching to SSI. Importantly, although 87 percent of counties are suppressed, these counties house only 31 percent of the total population, allowing me to perform county-level analysis despite the data suppression. Appendix A shows the number of counties and percentage of population aggregated into the "rest of state" category in each state.

NCHS mortality and natality data is categorized according to the International Classification of Diseases (ICD) codes. ICD codes are maintained by the World Health Organization and are used in over 117 countries as a diagnostic tool for classifying diseases (WHO (2016)). To determine homicide counts, I tally the number of deaths caused by any form of intentional assault[4], with the exception of legal intervention and terrorist attacks. Homicide rates are calculated by dividing this count by the total population in a county, obtained from demographic data described below. To determine the infant mortality rate, I combine data from both the mortality and natality files. I use the mortality file to determine the number of deaths of people aged 1 year or less and the natality file to determine number of births in that year. Infant mortality rates are simply the number of infant deaths divided by the number of births. The variables in my data correspond to homicide rates per 100,000 and infant mortality rates per 1,000 people. This is the most common way of measuring these variables. Homicide and infant mortality rates for 1985-2002 are plotted in Figure 7.

[4]These deaths correspond to codes X85-Y09 for ICD-9 and E960-E969 for ICD-10.

Figure 7: Homicide and Infant Mortality Rates



5.2 SSI Enrollment Data

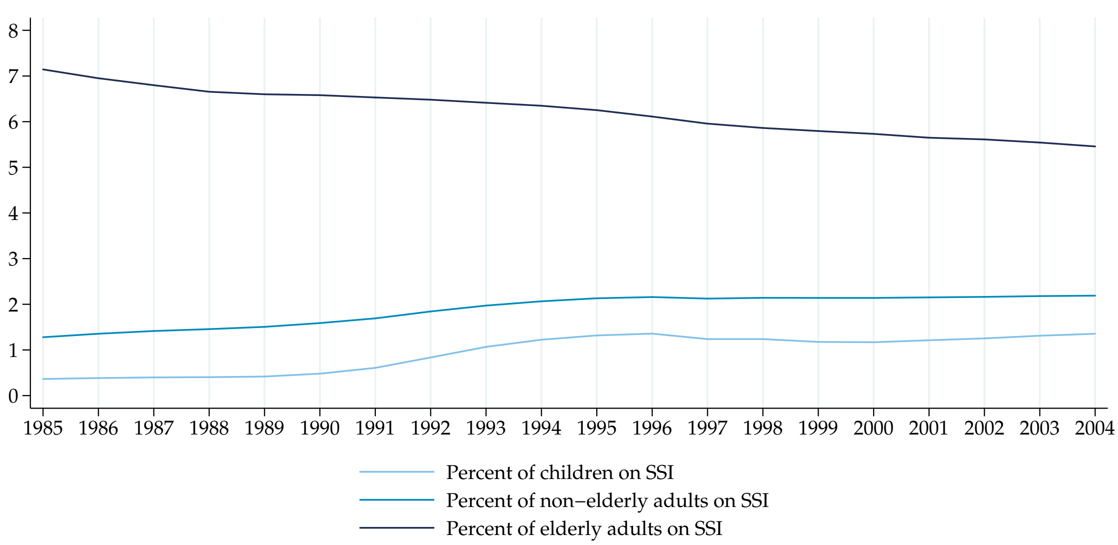
My second dataset consists of data produced by the SSA on SSI enrollment from their annual *SSI Recipients by State and County* publications. Although I acquired data from 1985 to 2004, I focus on the 1985-1996 period due to its proximity to the *Zebley* decision. The publication provides state and county statistics on SSI enrollment and benefit levels among children, non-elderly adults, and elderly adults in December of each year. From 1998 on, PDF and excel documents of the publications can be found on the SSA's website under Program Statistics and Data Files. To obtain data from earlier years, I scanned hard copies of the publications, which can be found at Stanford University's Green Library. I then used an Optimal Character Recognition program called *ABBYY FineReader* to translate the scanned PDFs into machine readable data in Excel. Finally, I manually checked the data to ensure it was properly transcribed. I found very few errors.

Using this process, I obtain 37,212 observations from 3,101 counties and 12 years of data. Since the *SSI Recipients by State and County* publication does not contain county geographic code, I used a user-written Stata command called reclink to match the names of counties from SSI data to county FIPS codes. From there, I was able to link my SSI dataset with the mortality and natality datasets. To match the format of the mortality and natality dataset, I combined counties with populations under 100,000 into a "rest of state" category.

The aggregate nature of this data presents a few challenges for the empirical analysis. First, I cannot evaluate the impact of SSI enrollment on individuals — I can only examine county-wide trends. Second, I am unable to control for family-level characteristics that may impact the use and dispersion of SSI income. Because of these challenges, I perform my analysis on the county level. Instead of examining the direct impact of SSI on health and crime, I examine the impact of the influx of SSI income — proxied by the child enrollment rate — into a county on county-level health and crime rates. I discuss this in more detail below.

Figure 8 uses this SSA data to plot yearly SSI enrollment rates for the three categories of recipients. The data shows a large growth in child enrollment during the early 1990s as well as a small reduction in enrollment between 1996 and 1997, which is consistent with the relaxation of child enrollment criteria in 1989 and re-evaluation of welfare policy in 1996. Figure 8 also shows steady growth in non-elderly — and decline in elderly — adult enrollment rates.

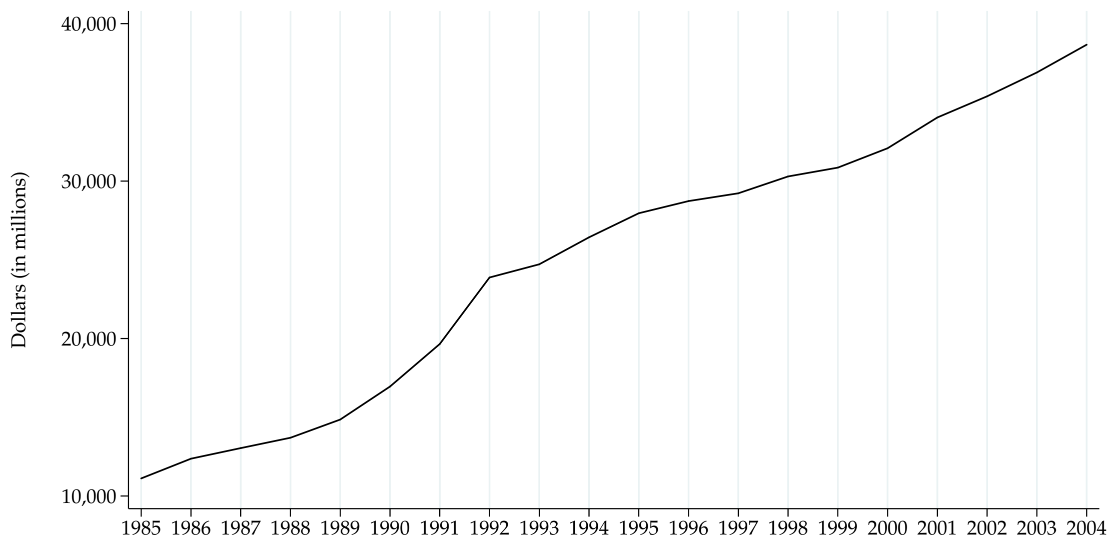
Figure 8: Annual SSI Enrollment by Age (by percent of population)



The upward trend of adult SSI enrollment, in addition to child SSI enrollment, poses a possible issue for my analysis. Due to the correlation between child and adult SSI enrollment rates, it is likely that in places where child SSI enrollment increased, adult SSI enrollment increased as well. Since I do not have individual data, it is difficult to isolate the impact of these two trends. In my findings, I show that this doesn't pose a threat to the validity of my results.

As shown in Figure 9, federal payments for the SSI program also grew substantially over this 20 year period. This growth is primarily driven by higher levels of participation in SSI.

Figure 9: Yearly Real SSI Payments in 2004 Dollars



5.3 SSI and AFDC Benefit Level Data

My third dataset consists of SSI and AFDC benefits from 1989 to 1996 obtained from various years of the annual *Green Book*. The *Green Book* is produced by the Committee of Ways and Means in the U.S. House of Representatives and contains historical data on all programs under the jurisdiction of the Committee of Ways and Means including Social Security, SSI, AFDC/TANF, and Medicare. For each year, I obtain the national SSI benefit for an individual ($SSIbenefit$) and the state-level AFDC benefits for families of 2 and 3

($AFDC3$, $AFDC2$). I adjust each of these for inflation in 2007 dollars. The financial gain to enrolling in SSI from AFDC (represented by the $SSIgain$ variable) is calculated using the following formula, as presented in Table 4:

$$SSIgain_{st} = SSIbenefit_t - (AFDC3_{st} - AFDC2_{st}) \tag{3}$$

The $SSIgain$ variable captures the financial gain to enrolling in SSI from AFDC for a family of three. Because I do not have individual level data, I cannot personalize this variable for each family. I thus choose to use the financial gain for a family of three to represent the financial gain for families of all sizes in a state – since the measures are likely to be highly correlated. As mentioned earlier, if a family has only 1 child, then they cannot retain any AFDC benefits upon switching this child to SSI. As a result, the relevant metric for families with only 1 child is simply the difference between AFDC benefits and SSI benefits. This does not pose a significant issue, however, as 87.2 percent of AFDC families have 2 or more children in the household (Stapleton et al. (2001)) and the two metrics have a correlation coefficient of 0.7.

5.4 Demographic Data

Finally, to control for county characteristics, I acquire a variety of demographic data. This data is important given my interest in the determinants of child SSI growth after the 1990 *Zebley* decision.

I obtain cross-sectional county-level demographic data by age, race, and origin from the 1990 census from the online GeoLytics 1990 Long Form Census dataset. This dataset includes poverty rates, educational attainment, family status, and a variety of other county-level variables. To match the rest of the data, I combine counties with populations under 100,000 into a “rest of state” category.

I also obtain time-variant county-level data on population, unemployment rates, per capita income, and child poverty levels. Unemployment and per capita income data is obtained from the Bureau of Labor Statistics (BLS). The BLS houses county-level data for all years of interest on per capita income and from 1990 onwards on unemployment rates. I use a combination of state-level data pre-1990 and county-level data in 1990 to estimate the county-level unemployment rates from 1985 to 1990. County-level population estimates come from the Survey of Epidemiology and End Results. The data set contains estimates by age and race for each year from 1985 to 2004. Data on child poverty levels come from the Census Bureau.

6 Findings

6.1 Main Findings

The primary empirical findings from my IV estimates of the relationship between child SSI enrollment and mortality can be seen in Table 5. The estimates correspond to equation 1, with infant mortality and homicide rates as the outcome variables.

The results suggest that a 1 percentage point increase in the child SSI enrollment rate results in a 1.45 percentage point decrease in infant mortality rates and a statistically insignificant change in homicide rates. Taking into account the average infant mortality and SSI enrollment rates — 8.46 and 0.80 respectively — this estimate is quite substantial. It suggests that, at the average county, a 100 percent increase in SSI results in a 13.7 percent reduction in infant mortality. Given that between 1989 and 1996 the average county saw an increase in SSI of about 224 percent, the results also suggest that on average, the *Zebley* expansion resulted in a 16.4 percent reduction in infant mortality.

Table 5: Main Findings

	(1) infmort	(2) homicide
	β / SE	β / SE
SSI	–1.449**	–0.355
	(0.287)	(0.488)
Observations	5352	5352
ymean	8.887	9.134

* $p < .05$, ** $p < 0.01$. Standard errors in parentheses.

Note: Uses data from 1985-1996. Regressions are weighted by county share of the population. Coefficients on the constant and fixed effects excluded.

In the case of infant mortality, this results are consistent with my hypothesis and with previous findings of the impact of various welfare programs on health (Hoynes et al. (2015), Strully et al. (2010), Baker (2008), Hamad and Rehkopf (2016), and Hoyne et al. (2016)). They suggest that the impact of SSI on health is similar to that of other welfare programs, although it is not clear whether this impact is on recipients of SSI or the county as a whole.

In the case of homicides, these results are inconsistent with my hypothesis. There are several characteristics of my study that could be driving this result. First, I only measure the contemporaneous impact of SSI. It may be the case that child SSI enrollment does reduce homicide rates, but that the impact takes a few years to manifest. Although Tuttle (2016) suggests that welfare may have a near-immediate impact on crime, his results are driven by financially-motivated crime: an outcome that is is probably much more elastic to income than homicide. Second, I do not observe the criminal behaviors of SSI recipients themselves. It is possible that SSI income does result in a reduction in homicide, but that this outcome is masked in my data by the homicide trends among the remainder of the population. This could explain the disparity between my results and those of Akee et al. (2010). For these reasons, it would be useful for future research to examine the long-run impacts of child SSI recipiency on homicides and other, particularly less severe, crimes.

6.2 Robustness

To address concerns about the endogeny of my instrument, I run a variety of robustness checks on the first and second stages of my specification. For my results to capture the causal impact of SSI, it must be the case that the interaction between the financial gain to enrolling in SSI and the proportion of the population that is AFDC eligible is correlated with infant mortality and homicide only through it's impact on SSI recipiency. Below, I compare the results of my regression to those from a number of alternate specifications. In general, my estimates are quite stable, suggesting that my instrument is in fact exogenous and my results are capturing the true effect of child SSI enrollment on infant mortality and homicide rates.

6.2.1 First Stage Estimates

Column (1) of Table 9 presents the coefficient on $AFDCpop * SSIgain * POST$ from equation (2). It is clear that after 1990, SSI grew more in counties with higher financial gain to enrolling in SSI from AFDC and higher proportions of the AFDC eligible population. This suggests that child SSI enrollment growth is very sensitive to changes in financial incentives and size of the AFDC eligible population. In particular, I find that that a 1 percentage point increase in $AFDCpop$ or a \$100 increase in $SSIgain$ is associated with a child SSI enrollment growth of 0.299 percentage points after 1990.

The results also suggest that a significant amount of the variation in SSI enrollment growth can be explained by differences in the financial incentives to enrolling in SSI in a state and the size of the AFDc-eligible population in a county. In 1989, the financial gain to enrolling in SSI varied from about \$400 to \$600. Thus, my estimates suggest that there was a almost a 0.6 percentage point difference in SSI enrollment growth between states with the highest and lowest financial incentives to enrolling in SSI. Similarly, in 1989, the proportion of the child population that was AFDC-eligible varied from 0.17 percent in the lowest county to 6.5 percent in the highest county. This suggests that there was over a 1.7 percentage point difference in SSI enrollment growth between counties at the highest and lowest level of AFDC-eligible populations.

Columns (2)-(5) of Table 9 serve as specification checks for the estimates in column (1). Each column contains the baseline equation from column (1) and includes additional controls. The coefficient on the instrument is quite stable across specifications, varying from 0.264 to 0.300, which provides promising evidence that the instrument is valid.

Controlling for the child poverty rate, percent of population that is white, and the unemployment rate in each county (column (2)) has virtually no impact on the coefficient. This stability suggests that the results cannot be explained by income shock or demographic variation across counties. Controlling for the level of AFDC benefits for a family of three in the state (column (3)) reduces the coefficient by a small, but significant amount. This is to be expected, since the financial gain to enrolling in SSI is likely less important in states with a higher absolute AFDC benefits. The similarity between columns (1) and (3) also confirm that the relevant metric is the financial gain to switching to SSI, rather than the absolute level AFDC benefits. The inclusion of state-specific time trends (column (4)) reduces the magnitude of the coefficient by 12 percent. It does not, however, alter the sign of significance of the coefficient. This result suggests that only some of relationship between the instrument and SSI can be explained by state-level trends in health, income, or other characteristics.

Column (5) estimates a version of the first stage regression with an additional interaction term, $AFDCpop * SSIgain * I^{1989}$, where I^{1989} is equal to one in 1989. The coefficient on this term measures the relationship between $AFDCpop * SSIgain$ and SSI enrollment prior to the *Zelby* decision. The specification allows me to check whether the relationship between the instrument and SSI enrollment can be explained by characteristics of counties before the treatment. The insignificant of the coefficient on $AFDCpop * SSIgain * I^{1989}$ and the statistical equivalence of the coefficient on the instrument between columns (1) and (5) suggest that the results cannot be explain by pre-treatment county characteristics.

Finally, column (6) is equivalent to column (1) but uses the adult SSI enrollment rate as the outcome. As discussed earlier, over the 1990s, the adult SSI enrollment rate also grew significantly. The growth in adult SSI enrollment was, however, spurred by different factors than the growth in child SSI enrollment: neither the *Zelby* decision nor the financial gain to enrolling in SSI from AFDC were relevant to the adult SSI program. One concern with my instrument is that it may be capturing the effect of county-level changes in poverty or health — rather than SSI itself. If this were the case, one would expect it to also significantly predict the adult SSI enrollment rate. Column (6) thus serves as a falsification test for the instrument. The statistical insignificance of the instrument in column (6) provides evidence that the instrument satisfies the exclusion restriction.

I also estimate a flexible form equation (2) using the interactions of $AFDCpop$, $SSIgain$, and $AFDCpop * SSIgain$ with a full set of year dummies. Figure 10 plots the coefficients on the instrument from this specification. Precise estimates and standard errors can be found in Table 10. Prior to 1990, the coefficients are either very small or insignificant. After 1990, however, they become statistically significant and increase quite a bit in magnitude. These results suggest that the instrument only significantly predicts SSI enrollment after the *Zebley* decision, which is evidence that it is not significantly correlated with county- and state-level characteristics that might impact mortality rates. Thus, Figure 10 provides further evidence that the exclusion restriction holds.

6.2.2 Second Stage Estimates

Tables 11 and 12 serve as specification checks for the estimates presented in Table 5. Column (1) of each table is equivalent to Table 5 and the remaining columns correspond to identical specifications as those performed in the first stage. My results are robust to and very stable across alternate specifications. In some cases, the inclusion of controls inflates the magnitude of my coefficient but in each the coefficients retain their sign and significance.

In measuring the impact of child SSI enrollment on infant mortality (Table 11), the coefficient varies by at most 33 percent. As in the first stage, controlling for various county and state-level trends in crime, health, unemployment, and child poverty (columns (2), (3), and (4)) in the IV estimates impacts the coefficients very little, suggesting that the relationship cannot be explained by county-level trends apart from SSI enrollment. Furthermore, the inclusion of the pre-treatment level of $AFDCpop$ and $SSIgain$ (column (6)) has no significant impact on my coefficient. When I control for state-specific linear time trends, my estimates increase in magnitude. This suggests that my model may be suffering from omitted variable bias, in particular from unobserved state-level trends that resulted in a reduction in infant mortality. If present, however, the bias results in only a 33 percent change in the magnitude of the coefficient, suggesting that state-level trends cannot completely — or even mostly — explain the results. Finally, column (6) shows no significant relationship between the adult SSI enrollment rate and infant mortality, suggesting that my instrument is capturing the relationship between child SSI enrollment and infant mortality, rather than some unobservable county or state characteristic.

In measuring the impact of child SSI enrollment on homicide rates (Table 12), the coefficients remain an imprecisely estimated zero across all specifications. This provides evidence that, in the short term, child SSI enrollment has no significant impact on homicide rates.

7 Conclusion

This paper exploits exogenous geographic variation in the impact of the 1990 liberalization of child SSI medical eligibility criteria to test the effect of SSI enrollment on infant mortality and homicide rates. I find that a 1 percentage point increase in child SSI enrollment results in a 1.45 percentage point reduction in infant mortality rates. Taking into account the growth in the child SSI program between 1989 and 1996, my findings suggest that the 1990 liberalization resulted in an average infant mortality reduction of 16 percent in U.S. counties. I find no evidence that child SSI enrollment impacts homicide rates.

These findings are consistent with literature examining the impact of EITC and the Food Stamp Program on infant health and contribute to a growing literature that suggests that welfare programs have positive impacts on the health, educational, and economic outcomes of their recipients. My findings also suggest that the SSI program may have positive externalities, improving the health not just of its recipients, but also of their communities as a whole. In order to explore this theory more systematically, individual-level data on SSI recipients is needed.

My findings also have important policy implications. They show that while cash transfer programs may pose significant economic costs, they play an important role in improving the health of low-income Americans. They do not rule out the possibility that cash transfer programs reduce homicide rates, suggesting that social impact of welfare programs is net positive.

This analysis of the impact of SSI on infant mortality and homicide rates provides several directions for future research. First, future research could study the long-run impact of SSI on the health, crime, and other outcomes of its recipients. This would require individual-level data on a variety of recipient characteristics. Deshpande (2014b) and Deshpande (2014a) provide a foundation for such work. Second, future research might examine the impact of SSI on other county-level outcomes. It would be particularly interesting to examine the short-run impact of SSI on property and other less severe crimes. This would be a difficult task, as it requires accurate county-level crime data, which is currently unavailable.

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Appendices

A Tables

Table 6: Summary Statistics and Variable Description

Variable	Definition	Source	Mean	Std	Min.	Max.
SSI_{ct}	SSI enrollment rate among children (percent of all children, 0-17, enrolled in SSI)	*SSI Recipients by State and County*	0.8	0.66	0.06	6.2
$adultSSI_{ct}$	SSI enrollment rate among adults (percent of all adults, 18-64, enrolled in SSI)	*SSI Recipients by State and County*	2.53	1.55	0.29	8.92
$AFDCpop_{c}$	Proportion of children that are in poverty and live in single-mother households in 1989	1990 census	1.39	0.94	0.17	6.48
$SSIgain_{ct}$	Financial gain (in hundreds of dollars) to enrolling in SSI from AFDC for a family of 3	Ways and Means *Green Book*	5.35	0.49	3.22	7.17
$infmort_{ct}$	Infant mortality rate per 1,000. Number of infant deaths ($<$ 1 year) divided by number of births in a year, times 1,000.	CDC Vital Statistics	9.02	7.92	0	63.36
$homicide_{ct}$	Homicide rate per 100,000. Number of homicides in a year divided by the total population, times 100,000.	CDC Vital Statistics	7.46	8.34	0	84.66
$unemprate_{ct}$	Unemployment rate	BLS	5.72	2.39	0.98	22.43
$childpov_{ct}$	Number of children in poverty divided by total number of children	Census Bureau	20.23	5.08	4.47	42.16
$pctpopwhite_{c}$	Percent of total population that is white	1990 census	86.10	12.75	24.6	99.36

Table 7: County Suppression by State

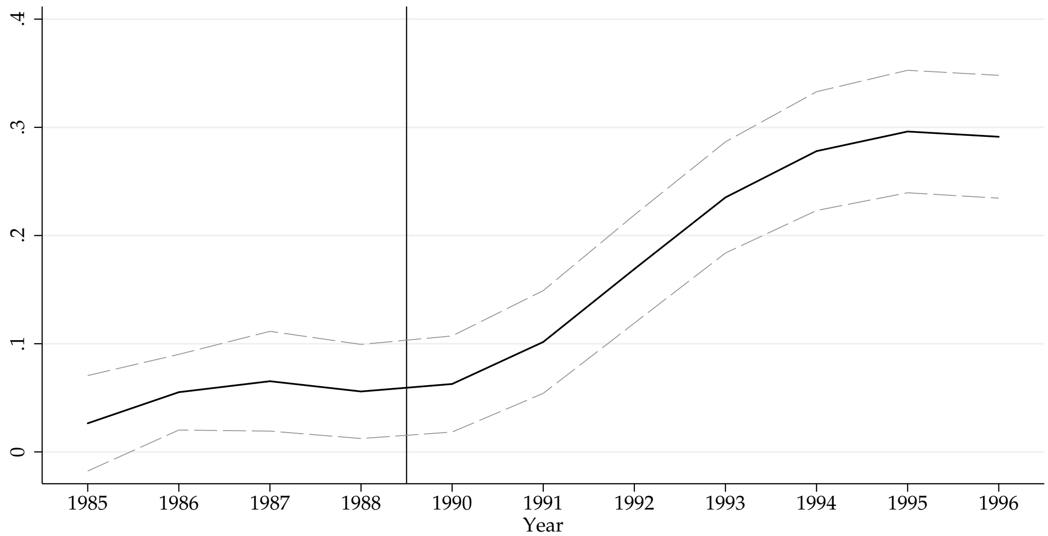
State	*Total Ctys*	*Suppressed Ctys*	*% of Pop Suppressed*	*State*	*Total Ctys*	*Suppressed Ctys*	*% of Pop Suppressed*
AL	67	61	56.8%	MT	55	54	85.8%
AK	21	21	100%	NE	93	91	60.1%
AZ	14	12	23.9%	NV	17	15	17.1%
AR	75	73	80.3%	NH	10	8	47.5%
CA	58	28	4.3%	NJ	21	4	4.7%
CO	59	55	66.7%	NM	32	31	68.3%
CT	8	1	3.1%	NY	62	37	12.2%
DE	3	2	33.7%	NC	100	87	55.1%
DC	1	1	100%	ND	53	53	100%
FL	67	47	31.6%	OH	88	66	28.9%
GA	159	150	55%	OK	77	73	55.9%
HI	5	5	100%	OR	36	30	35.5%
ID	44	43	79.6%	PA	67	38	15.3%
IL	102	85	19.6%	RI	5	3	24.5%
IN	92	80	48.3%	SC	46	36	44%
IA	99	95	72.2%	SD	66	65	82.2%
KS	105	101	56.3%	TN	95	90	56.9%
KY	120	117	72%	TX	254	231	30.3%
LA	64	55	44.9%	UT	29	25	22.5%
ME	16	12	45.4%	VT	14	13	76.6%
MD	23	15	16.7%	VA	112	99	48%
MA	14	3	1.5%	WA	39	29	19.1%
MI	83	65	23.1%	WV	55	54	88.4%
MN	87	80	42.9%	WI	72	61	43.4%
MS	82	79	79.2%	WY	23	23	100%
MO	115	107	43.7%				

Table 8: Average SSI Gain (1985-1996) by State

State	*SSIgain*	*Ranking*	*State*	*SSIgain*	*Ranking*
AL	$564	49	MT	$493	23
AK	$464	14	NE	$496	26
AZ	$514	33	NV	$514	32
AR	$544	43	NH	$509	28
CA	$401	1	NJ	$444	6
CO	$488	21	NM	$524	38
CT	$414	2	NY	$433	5
DE	$471	16	NC	$556	47
DC	$474	17	ND	$493	24
FL	$503	27	OH	$518	36
GA	$541	41	OK	$493	25
HI	$428	4	OR	$513	30
ID	$516	34	PA	$469	15
IL	$461	12	RI	$451	8
IN	$516	35	SC	$546	44
IA	$513	29	SD	$543	42
KS	$486	20	TN	$548	46
KY	$564	48	TX	$571	50
LA	$528	39	UT	$489	22
ME	$428	3	VT	$444	7
MD	$476	18	VA	$514	31
MA	$459	11	WA	$456	10
MI	$461	13	WV	$534	40
MN	$456	9	WI	$486	19
MS	$574	51	WY	$548	45
MO	$519	37			

B Figures

Figure 10: Impact of Financial Incentive to Enrolling in SSI on Child SSI Enrollment



C Regression Tables

Table 9: First Stage

	(1)	(2)	(3)	(4)	(5)	(6)
	SSI	SSI	SSI	SSI	SSI	adultSSI
	β / SE	β / SE	β / SE	β / SE	β / SE	β / SE
AFDCpop*SSIgain*POST	0.299**	0.294**	0.285**	0.264**	0.300**	–0.023
	(0.013)	(0.013)	(0.013)	(0.011)	(0.013)	(0.014)
AFDCpop*SSIgain*yr(1989)					0.004	
					(0.002)	
Observations	5352	5352	5352	5352	5352	5352
ymean	0.807	0.807	0.807	0.807	0.807	3.022
F-stat	88.401	89.550	91.521	137.664	88.253	606.362

* p < .05, ** p < 0.01. Standard errors in parentheses.

Note: Estimated using data from 1985-1996. Regressions are weighted by county share of the population. Coefficients on the constant term, controls, and fixed effects are excluded.

Table 10: First Stage: flexible form

	(1)
	SSI
	β / SE
AFDCpop*SSIgain*yr(85)	0.026
	(0.023)
AFDCpop*SSIgain*yr(86)	0.055**
	(0.018)
AFDCpop*SSIgain*yr(87)	0.065**
	(0.024)
AFDCpop*SSIgain*yr(88)	0.056*
	(0.022)
AFDCpop*SSIgain*yr(90)	0.063**
	(0.023)
AFDCpop*SSIgain*yr(91)	0.102**
	(0.024)
AFDCpop*SSIgain*yr(92)	0.169**
	(0.025)
AFDCpop*SSIgain*yr(93)	0.235**
	(0.026)
AFDCpop*SSIgain*yr(94)	0.278**
	(0.028)
AFDCpop*SSIgain*yr(95)	0.296**
	(0.029)
AFDCpop*SSIgain*yr(96)	0.291**
	(0.029)
Observations	5352
F-stat	727.109

* $p < .05$, ** $p < 0.01$. Standard errors in parentheses.

Note: Table estimates a flexible form of equation 2 with data from 1985-1996. Regressions are weighted by county share of the population. Coefficients on the constant and county and year fixed effects excluded.

Table 11: IV Estimates: Infant Mortality and Child SSI Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	mort	mort	mort	mort	mort	mort
	β / SE	β / SE	β / SE	β / SE	β / SE	β / SE
ssi	–1.449**	–1.514**	–1.555**	–2.426**	–1.454**	
	(0.287)	(0.292)	(0.302)	(0.338)	(0.286)	
interact89					–0.008	
					(0.015)	
ssiadult						18.841
						(11.964)
Observations	5352	5352	5352	5352	5352	5352
ymean	8.887	8.887	8.887	8.887	8.887	8.887

* $p < .05$, ** $p < 0.01$. Standard errors in parentheses.

Note: Estimated using data from 1985-1996. Regressions are weighted by county share of the population. Column (1) estimates equation 1. Column (2) controls for unemployment rate, child poverty rate, and percent of population that is White. Column (3) controls for the absolute AFDC benefit for a family of three. Column (4) controls for the level of $AFDCpop * SSIgain$ in 1989. Column (5) includes state-specific linear time trends. Column (6) estimates equation 1 with the adult SSI enrollment rate in place of the child enrollment rate. Coefficients on the constant term, controls, and fixed effects are excluded.

Table 12: IV Estimates: Homicide and Child SSI Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	homrate	homrate	homrate	homrate	homrate	homrate
	β / SE	β / SE	β / SE	β / SE	β / SE	β / SE
ssi	–0.355	–0.487	–0.342	–1.014	–0.218	
	(0.488)	(0.493)	(0.514)	(0.534)	(0.483)	
interact89					0.234**	
					(0.024)	
ssiadult						4.617
						(7.102)
Observations	5352	5352	5352	5352	5352	5352
ymean	9.134	9.134	9.134	9.134	9.134	9.134

* p < .05, ** p < 0.01. Standard errors in parentheses.

Note: Estimated using data from 1985-1996. Regressions are weighted by county share of the population. Column (1) estimates equation 1. Column (2) controls for unemployment rate, child poverty rate, and percent of population that is White. Column (3) controls for the absolute AFDC benefit for a family of three. Column (4) controls for the level of $AFDCpop * SSIgain$ in 1989. Column (5) includes state-specific linear time trends. Column (6) estimates equation 1 with the adult SSI enrollment rate in place of the child enrollment rate. Coefficients on the constant term, controls, and fixed effects are excluded.