ESTIMATING NEIGHBORHOOD EFFECTS ON LOW-INCOME YOUTH
August 20, 2009

Brian A. Jacob
Walter Annenberg Professor of Education Policy, Professor of Economics
University of Michigan

Jens Ludwig
McCormick Foundation Professor of Social Service Administration, Law, and Public Policy
University of Chicago

Jeffrey Smith
Professor of Economics
University of Michigan

Contact:
Jens Ludwig
University of Chicago
1155 East 60th Street
Chicago, IL 60637
(773) 834-0811
jludwig@uchicago.edu

This research was supported by a grant from the William T. Grant Foundation (6802). All opinions and any errors are our own.

I. INTRODUCTION

Youth outcomes vary dramatically across neighborhoods. For example in Wilmette, Illinois, a North-shore Chicago suburb with a median home value of $441,000, almost everyone graduates from high school and a majority go on to attend – and complete – college.[1] In contrast, the dropout rate exceeds 20% in more than one-quarter of Chicago's public high schools, most of which can be found in the city's disadvantaged South- and West-side neighborhoods, while six high schools have dropout rates over 30%.[2] A similar pattern holds for homicide, the second-leading cause of death for people age 15 to 24.[3] Murder rates in some of Chicago's South Side neighborhoods are on the order of 60 or 70 per 100,000, about 10 times the national average. The homicide rate in 2002 in Wilmette was exactly 0.

These patterns are of policy concern in part because residential segregation by income has been increasing since 1970 (Watson, 2009); in 2000 there were 8 million people living in high-poverty Census tracts (≥40%), nearly twice the number as in 1970 (Jargowsky, 2003). Because poor and minority Americans are over-represented in high-poverty areas, there is concern that "neighborhood effects" contribute to overall inequality in life outcomes across race and class lines (Kawachi and Berkman, 2003). A wide range of housing and education policies helps shape the distribution of poor families across neighborhoods, schools and other social settings. The Obama Administration has initiated a number of policies designed to directly improve conditions in some of the nation's most disadvantaged neighborhoods, including the new "Promise Neighborhoods" program, increased funding for the Community Development Block Grant (CDBG) program, and the new Full Service Community Schools program.

The substantial variation across neighborhoods in youth outcomes raises the possibility that neighborhood environments themselves could exert a *causal* effect on behavior. A large theoretical literature within the social sciences dating back at least to the 1930s, discussed further below, has considered why the social, physical and institutional attributes of neighborhoods might influence schooling, work, crime, health and other outcomes.

A large body of empirical research in the social and medical sciences has tried to determine whether variation across neighborhoods in youth outcomes is due to geographic sorting of different types of families into different types of neighborhoods, or instead to the causal effects of neighborhood context. The non-experimental literature in this area typically seems to provide more supportive evidence of important neighborhood effects on youth outcomes (e.g., Sampson et al., 2002) than does the one true randomized experiment that has been conducted in this area, the U.S. Department of Housing and Urban Development's Moving to Opportunity (MTO) experiment (Kling, Ludwig and Katz, 2005, Sanbonmatsu et al., 2006, Kling, Liebman and Katz, 2007). This has led to considerable debate about the degree to which the different findings are due to problems of selection bias with the non-experimental studies, or instead to limitations of the MTO experiment (see for example Clampet-Lundquist and Massey, 2008, versus Ludwig et al., 2008).

In this paper we seek to learn more about the ability of non-experimental research

1 For example, 97% of current adults in Wilmette have completed high school while 73% have a BA.
2 IL State Department of Education, http://206.230.157.60/publicsite/getSearchCriteria.aspx, 10/16/04.
3 *National Vital Statistics Reports*, Volume 52, Number 9, November 7, 2003, p. 13.

methods, which dominate this literature, to identify "neighborhood effects" on youth. Randomized experiments are costly, as evidenced in part by the fact that just a single experiment has ever been carried out in the area (MTO). Randomized experiments also have their own limitations; for example in the MTO study, which offered housing vouchers via random lottery to some but not other public housing families, voucher recipients wound up experiencing large improvements in neighborhood socio-economic composition and social processes, but did not experience much change in neighborhood racial segregation or local school quality. This limits the range of neighborhood effects questions that can be directly tested by the MTO data. And if there is "treatment heterogeneity" where different types of families respond differently to neighborhood environments, as seems possible, MTO's findings will be most relevant for the subset of public housing families who are living in particularly distressed neighborhoods and volunteer to move. For these and many other reasons, it is important to understand whether non-experimental methods provide a viable alternative. Understanding the potential and limits of non-experimental estimation is also crucial for making sense of the existing neighborhood effects research, and is a fundamental issue for a much broader set of social science research applications as well.

Our paper exploits a randomized housing-voucher program in Chicago that provides us with the opportunity to generate unbiased "experimental" estimates of the effects of simultaneously changing a broad range of neighborhood characteristics on low-income youth initially living in public housing projects. We use data from a randomized housing-voucher lottery to generate unbiased estimates of neighborhood effects on youth by comparing those in families with "good" and "bad" lottery numbers. Using state and local administrative records, we are able to examine a variety of youth outcomes including achievement test scores, graduation rates and criminal activity, employment and earnings, and participation in welfare programs. We then characterize the bias of various non-experimental estimators that rely on different samples of youth in place of those whose families had a bad draw in the Chicago housing voucher lottery.

Our paper seeks to make at least three important contributions. First, the existing literature on bias with non-experimental estimates dating back initially to LaLonde (1986) has focused largely on the effects of job training programs (with a few studies now in education as well), and perhaps as a result has not received a great deal of attention outside of labor economics, statistics and to some extent education. Our research may receive more attention across the social sciences given the broad interest in neighborhood effects, particularly in the discipline of sociology, where the idea of neighborhood effects on youth outcomes is central to many of the foundational ideas in the field.

Second, recent work by Cook, Shadish and Wong (2008) suggests some conditions that are hypothesized to increase the chances that non-experimental estimates are able to produce unbiased estimates – namely, the availability of similar types of outcome measures for treatment and non-experimental comparison groups, the availability of comparison groups drawn from the same local area as treatment group members, and rich data on a wide range of characteristics are available for treatment and comparison group members, including pre-baseline measures of the outcomes of interest and data relevant to the process through which people select into treatment. The massive integrated database that we have assembled, pulling together a wide range of adult and youth characteristics from multiple administrative data systems, meets most of the key conditions that are hypothesized to increase the chances of generating unbiased non-experimental estimates, and in addition our samples of treatment and comparison families are extremely large as well. In sum, our application seems to provide a good test of the ability of non-experimental estimates to generate the "right" answer.

Finally, previous job training studies focus on selection into program participation with respect to labor market outcomes such as earnings or labor force participation. We have data on a variety of different outcomes for the same samples of youth, and so are able to examine the relative importance of neighborhood selection with respect to different behavioral domains without having to compare findings across different studies and samples.

The remainder of our (preliminary) paper is organized as follows. The next section briefly reviews the theoretical literature about how and why neighborhood environments might matter for youth outcomes, with a focus on the predictions of leading theories about how youth should respond to voucher-assisted moves to less disadvantaged neighborhoods. The third section of the paper briefly reviews the existing evidence about neighborhood effects on youth outcomes, including a few of the comparisons of experimental versus non-experimental estimates that have been carried out with MTO data. The fourth section reviews our preliminary estimates to date from the Chicago voucher data studied here, and discusses some of the logistical challenges in working with the administrative data that have slowed our progress. The final section outlines the key next steps that we will undertake as part of this project.

II. CONCEPTUAL FRAMEWORK

Since the early days of the "Chicago School," sociologists have theorized about the ways in which neighborhood environments may impact child development. Early theories emphasized the role of social disorder and the ecological competition for resources. These early scholars viewed the city in terms of an urban ecology in which different ethnic groups, in various stages of assimilation and economic integration, compete for vital resources and niches in neighborhoods, in the same way that species compete in the natural world. Therefore, the most disadvantaged populations naturally end up in the least desirable locations and disproportionately are exposed to high crime, limited institutional resources, and the physical dangers and health risks of the zones closest to industry (Park et al 1967).

Furthermore, the early sociological literature saw the urban environment as a place where the density and heterogeneity of the population contributed to the disruption of strong social ties that may help maintain order and deter deviant behavior, such as crime and poor school performance, in small towns (Simmel 1997, Wirth 1997). This was especially true in the 'slums' where high residential mobility and large numbers of family-less individuals further contributed to the deterioration of the social and moral order (Zorbough 1983). While current research has largely abandoned these relatively deterministic models of human ecology, there remains a great deal of interest in the potential relationship between neighborhood environments and children's life chances.

In their 2002 review of neighborhood effects, Harvard sociologist Robert Sampson and his colleagues identify four general social processes through which neighborhood characteristics are currently thought to affect those who live in them (Sampson et al 2002: 457-8; see also

Jencks and Mayer, 1990). First, the social ties and inter-personal interactions with co-residents in a neighborhood provide different opportunities to accumulate social capital. In terms of school outcomes, children living in poor neighborhoods may have diminished access to well-educated adults to help them with homework or act as pro-academic role models (Wilson 1987, 1996). Parents in poor neighborhoods may also be less involved in their children's schools and less able to activate the social capital necessary to advocate for school improvement (Coleman 1991). Moreover, children's peer groups often come overwhelmingly from their neighborhood. Pro-social and anti-social neighborhood peer groups may influence student achievement either directly by affecting the level of instruction in the classroom (Hoxby 2000, Zimmer and Toma 2000), or indirectly by shaping the social rewards to pro- versus anti-social behavior (Gavaria and Raphael 2001).

Second, neighborhoods may influence school outcomes not through the direct ties of the residents, but through their social norms and capacity for informal social control (Sampson et al 1999). In other words, it may not be who the local children know directly, but rather the general levels of trust and expectations for behavior that prevail in the neighborhoods. For example, children may be less likely to get into trouble that would interfere with school when their neighbors are willing to intervene and keep an eye out for them. High expectations about overall educational attainment and achievement may also lead students to be more willing to work hard in school.

Third, the quantity and quality of neighborhood institutional resources may matter. When it comes to academic achievement, neighborhood schools are probably the most important – but not the only relevant – local institutions. Resources ranging from adequate medical care facilities, child care centers, parental employment opportunities, and after-school social and academic organizations could all influence children's academic performance in potentially important ways (Jencks and Meyer 1990, Brooks-Gunn et al 1993).

Finally, children's routine activities and those of their neighbors are shaped by the geography of neighborhoods, and may also have a direct influence on student achievement. Land use, such as the presence of bars, parks, or high-rise versus single- family homes, may shape the type of people that children interact with and the types of places in which they can interact with their peers (Sampson et al 2008). These ecological factors may also have a direct impact on the safety level of the neighborhood by for instance affecting the degree to which public spaces can be easily monitored by police or community residents (Jacobs 1997). Furthermore, the patterns of adult activity that children experience on a daily basis may also affect their own behavior in and outside of school which may indirectly impact their achievement levels. For example, children who observe their parents and neighbors coming and going regularly to work and attend formally organized activities during the standard workday may more quickly learn the value of routine and punctuality needed to excel in school. They may also learn how to navigate the world of formal interactions and organizations, such as schools, better than children who spend their time playing informally in the street (Lareau 2003).

Implicit in most of the above mechanisms is the assumption that "better" (i.e. less poor or otherwise disadvantaged or distressed) neighborhoods should always lead to improvements in child achievement. However, especially when considering housing voucher programs, it is important to note that this need not necessarily be the case. For example, Small and Stark (2005) find that poor neighborhoods often have more vital resources appropriate to low-income households, such as affordable childcare centers than more affluent neighborhoods. It is possible that, just like childcare centers, the after school resources available for children in poor neighborhoods may be more affordable and accessible than those in their new more affluent neighborhoods. Furthermore, feelings of relative deprivation and low social and academic standing with respect to their new neighbors and classmates may be discouraging to students and reduce the effort they make in school, or make them less happy with potentially adverse consequences for their schooling engagement and outcomes (Jencks & Meyer, 1990, Luttmer, 2005).

In sum, there are many reasons to theorize that moving children out of poor neighborhoods may improve their test scores and school outcomes. But there are also reasons to hypothesize that voucher-assisted moves to less distressed areas may not produce the desired outcomes. This means that the actual impact of moving children to less disadvantaged areas is ultimately an empirical question.

III. PREVIOUS EVIDENCE

Over time a growing number of longitudinal micro-datasets have been geo-coded, which has enabled social science and medical researchers to run some version of the following regression – some individual outcome of interest (schooling, crime, work, health, etc.) regressed against some measure of the socio-economic or racial composition of the local area, usually measured at the ZIP code or Census tract level, controlling for a variety of individual and family level covariates. This body of research generally finds evidence for neighborhood effects on most of the outcomes considered (see for example Sampson et al., 2002, Ellen and Turner, 2003, and Kawachi and Berkman, 2003).

To address the limitations of much of this literature in terms of measurement of neighborhood environments, a consortium of funders including MacArthur, NIH and NIJ made possible the large-scale Project on Human Development in Chicago Neighborhoods (PHDCN). While the PHDCN is an observational, not experimental study, the dataset has a longitudinal structure, a rich set of observable covariates, and detailed measurements of neighborhood environments, including surveys of community members and even video tapes of street life. The PHDCN is a longitudinal study of a random sample of approximately 3,000 children ages 0 to 18 at wave one, in randomly selected Chicago neighborhoods. The children are followed for three waves over 7 years to wherever they moved in the United States. Sampson and his colleagues exploit the longitudinal nature of the data and the rich set of covariates to predict selection into and out of disadvantaged neighborhoods and then use those predicted probabilities to estimate the effect of moving out of a disadvantaged neighborhood (Sampson et al 2008).

The explanatory variable of interest in their analysis is a measure of “concentrated disadvantage” that comes from a factor analysis of the concentration of welfare receipt, poverty, unemployment, female-headed households, African-Americans and children under 18 years old (Sampson et al 2008: 848). In Chicago, the only ethnic group in the sample living in neighborhoods with the most extreme levels of concentrated disadvantage (the bottom quartile of the Chicago distribution) is African-Americans. Therefore, Sampson et al restrict their analysis only to African-American children, and find that children who leave severely disadvantaged neighborhoods experience a 0.25 standard deviation increase in their later verbal test scores (a combination of the Wechsler Intelligence Scale vocabulary test and the Wide Range Achievement reading test) compared to other African-American children in the PHDCN (Sampson et al 2008).

Using the PHDCN data, Sampson, Morenoff and Raudenbush (2005) find that neighborhood-level differences in explain a large proportion of the difference between blacks and whites in self-reported violent activity. More specifically, the proportion of immigrants and proportion of population in professional or managerial occupations are negatively and significantly related to individual rates of violence. Interestingly, measures of concentrated disadvantage, collective efficacy, friendship ties, and youth organizations are not significantly associated with individual violence after controlling for individual and family characteristics along with immigrant and professional occupation concentrations. Perhaps this suggests that measures of social cohesion that are related to neighborhood-level crime rates may have more to do with where violent crimes are committed rather than who commits them. The only measure of social processes (rather than neighborhood demographics) that is significantly related to self-reported violent crime is moral/legal cynicism, or tolerance for deviance. This collective moral cynicism continued to be related to individual violence even after controlling for prior neighborhood crime rates.

The first quasi-experimental study of the effects of neighborhoods on youth outcomes arose out of a 1966 lawsuit filed by a Chicago public housing resident named Dorothy Gautreaux. Her lawsuit claimed that the Chicago Housing Authority (CHA) and U.S. Department of Housing and Urban Development (HUD) did not provide adequate opportunities for public housing residents in Chicago to live in racially integrated neighborhoods. The case eventually reached the U.S. Supreme Court, which in 1976 ruled in her favor.

As a result of the Supreme Court's ruling, what came to be known as the Gautreaux program began offering public housing residents the opportunity to use housing vouchers in racially integrated neighborhoods (less than 30 percent Black) in the city and suburbs. Participants who volunteered for the program were assigned housing based on where there happened to be openings. Once the program was fully established in the 1980s, around 1,700 to 2,000 families a year signed up to participate, out of whom about 19 percent of those, or 300 families a year, were placed in racially and economically integrated, mostly suburban, neighborhoods using the vouchers (Rubinowitz and Rosenbaum 2000: 67). Many of the remaining families wound up being placed in neighborhoods that were still poor and segregated, but judged to be improving, which were usually located within the Chicago city limits (Mendenhall et al 2006). While in theory, participants could choose not to accept the housing units assigned to them, most families reportedly accepted the first available apartment (Kaufman and Rosenbaum 1992).

In 1988, a random sample of 342 Gautreaux participants was surveyed in an attempt to compare the suburban movers with those who had stayed in the city of Chicago. The surveyed families had enrolled in the Gautreaux program between 1976 and 1981, and so were surveyed from 7 to 12 years after their Gautreaux-assisted neighborhood moves. Compared to the surveyed students who remained in the city of Chicago, suburban movers were four times less likely to have dropped out of school (20 percent vs 5 percent); more likely to be in a college track in high school (24 versus 40 percent); twice as likely to attend any college (21 percent versus 54 percent); and almost seven times as likely to attend a four-year college (4 percent versus 27 percent). The only educational attainment measure for which the suburban students did not appear to be doing significantly better than the city students was their grade point average, which could simply reflect higher grading standards in suburban schools (Rubinowitz and Rosenbaum 2000: 134-6).

While the Gautreaux program results were quite encouraging, there necessarily remains some question about whether the Gautreaux families surveyed in the suburbs were comparable in all respects to the surveyed city movers. For example there is now some evidence that the initial residential placements of Gautreaux families are systematically correlated with the characteristics of families and their neighborhoods at baseline (Mendenhall et al., 2006, Votruba and Kling, 2009). It is possible that at least part of the differences in schooling outcomes observed between city and suburban movers in Gautreaux are due to differences in the background attributes of the families who are being compared.

In response to the apparent success of the Gautreaux program, federal funding was allocated for a true housing voucher experiment designed to test the effects of neighborhood poverty called Moving to Opportunity (MTO). Between 1994 and 1998, a total of 4600 low-income, mostly minority public housing residents in five U.S. cities (Chicago, New York, Boston, Baltimore and Los Angeles) signed up to participate in the MTO program. Through a random lottery, families who signed up for MTO were assigned to one of three different residential mobility groups. Families assigned to the *Experimental group* were awarded a housing voucher that could be used for private housing only in a low-poverty area (census tracts with 1990 poverty rates of less than 10 percent), and were also given counseling and assistance in finding their new apartment. Families assigned to the *Section 8-only group* were given a standard Section 8 housing voucher that could be used in any census tract in which the family wished to live and could find a suitable unit to lease. Families assigned to the *Control group* did not receive a voucher of any kind, but maintained their current project-based housing and maintained their eligibility for other social programs (Orr et al 2003).

Of the families assigned to the experimental group, around 47 percent relocated with a housing voucher through MTO while 62 percent of those assigned to the Section 8-only group relocated through MTO. Many of the families who moved through the MTO experimental group to a low-poverty tract eventually moved again and returned to higher poverty neighborhoods, while some families in the control group relocated on their own, even without MTO assistance. Nevertheless, random assignment to the MTO experimental rather than control group generates large differences in residential neighborhood characteristics, with differences in tract poverty rates equal to 25-30% of the control mean 1 year after random assignment and around 20-25% over the 6 years after assignment. MTO generates similarly large changes in other measures of neighborhood socio-economic composition, safety, social disorder, and social cohesion, but leads to more modest changes in neighborhood racial composition.

Data on children's outcomes collected on average 5 years after random assignment found that on average there is no statistically significant effect of MTO-induced moves on children's scores on the Woodcock-Johnson-Revised reading or math achievement tests (Sanbonmatsu et al., 2006). The estimates do not seem to be any larger for children who were relatively younger at the time of baseline. However it is important to keep in mind that these achievement test scores were recorded just 5 years after baseline, and so many of those children who were very young at the time of random assignment (and so could potentially benefit the most from MTO moves; see for example Shonkoff and Phillips, 2000, and Knudsen et al., 2006) were still too young to be tested at the time of the interim MTO evaluation.

Additional sub-group analyses find that there might be some effect of being assigned to the experimental rather than control group on the reading test scores of African-American children, with an intent to treat effect (ITT) equal to around 0.1 standard deviations so that the effect of actually using a voucher is more like 0.2 standard deviations (Sanbonmatsu et al 2006, Web Appendix). However these impacts seem to be driven by African-American children in just two of the five MTO sites – Baltimore and Chicago, where almost all of the MTO program population is African-American, although only the Baltimore results are statistically significant.[4] In the other three MTO cities (Boston, Los Angeles, and New York) the program sample is split between African-American and Hispanic children, and separate sub-group analyses reveal no statistically significant gains in test scores for either black or Hispanic children.[5]

In the area of crime, MTO moves clearly made parents and youth safer. Around 69 percent of the experimental group families report feeling safe in their neighborhoods at night, compared to just 55 percent of the control group. Around 17 percent of the experimental group households had someone victimized by a crime in the 6 months prior to the follow-up surveys, a large proportional change compared to the 21 victimization rate among control group households (Kling, Ludwig, and Katz 2005: 88; Orr et al. 2003).

The impacts of MTO moves on criminal behavior 5 years after baseline are complicated. When we look at male and female youth pooled together, we see statistically significant declines in violent crime arrests, but not for arrests for other types of offenses. Table 5 makes clear that this impact on violent crime seems to occur among both male and female youth, with ITT impacts that are large for both genders as a share of the relevant control means (-.077 for females compared to a control mean of .241, and -.045 for males relative to a control mean of .537).

But MTO impacts on arrests for other offenses, and for other types of behavioral measures more generally, vary greatly by gender. In general female behavior becomes more pro-

---

4 Ludwig, Ladd and Duncan (2001) analyzed short-term achievement test scores measured 2-3 years after random assignment for children in the Baltimore MTO site and found very sizable gains in test scores for children who relocated through the MTO demonstration. The site-specific analyses in the interim (5 year) MTO study, which examined data from all 5 MTO sites, suggest the way to reconcile the short-term and interim results is site heterogeneity in MTO impacts rather than "fade out" of MTO test gains.

5 While the estimated experimental treatment impact on reading scores is not statistically significant in either the Baltimore or Chicago site when analyzed separately, the impact is significant when data from those two sites are pooled together. In contrast the estimated impacts of MTO experimental group assignment on reading or math scores are very small both absolutely and relative to the standard errors when data from the other three MTO sites (Boston, Los Angeles and New York) are pooled together and analyzed overall or analyzing African-American and Hispanic children separately. Thanks to Jeffrey Kling for his helpful discussions on this point.

social after moving through MTO, while behavior of male youth becomes less pro-social after MTO relative to the control group, at least on some measures – for instance, the property arrest rate for experimental boys is .15 arrests per year higher, compared to a control mean of .474. This does not seem to be due just to differences in policing practices in low poverty areas, since we also see some increase in the behavior problem index that is self reported (ITT of .064, compared to a control mean of .343).

The MTO data also provide an opportunity to learn more about the ability of some alternative non-experimental estimation approaches to replicate experimental impact estimates, or what is known as a "within-study comparison." Ludwig and Kling (2007) use just the non-experimental variation in neighborhood attributes within the MTO data by restricting the sample to only those families who were randomly assigned to the MTO experimental group. All of the variation in post-random assignment neighborhood environments is then driven by the decisions of individual families about whether they wish to participate in the program. (Ludwig and Kling focus on within-group variation for the experimental rather than control group because there are larger differences in neighborhood environments across families). OLS regressions that condition on a rich set of baseline characteristics, including pre-lottery measures of criminal activity, suggest that a one standard deviation in tract share minority increases violent-crime arrests among MTO participants by .016 arrests per person over the 5 years of post-lottery data. The "right" answer comes from using data from all three groups and interactions between treatment assignment indicators and MTO sites as instruments for Census tract characteristics, and suggests an effect of tract share minority on violent crime arrests that is about four times as large as the OLS estimate (equal to .067).

Kling, Liebman and Katz (2007) carry out a similar exercise using only the non-experimental variation in neighborhood environments that is found within the MTO control group. OLS estimates are again typically quite far away from the benchmark experimental estimates that use MTO site-group interactions as instruments for specific Census tract characteristics. For example OLS estimates suggest that a 1 standard deviation increase in tract poverty rates impairs adult mental health by 0.13 standard deviations. The experimental instrumental variables (IV) estimates suggest instead that the actual causal effect is to improve adult mental health by fully 0.62 standard deviations, equal in magnitude to best-practice talk or psychotherapy treatment. The sign of this bias suggests that adults with unobservable attributes that harm their mental health are the ones who are most likely to choose to relocate to lower-poverty areas. On the other hand Kling et al. find the reverse bias with respect to male youth outcomes, where those families with teenage sons predisposed to better outcomes move into lower-poverty areas.

In principle it is possible that alternative non-experimental estimation approaches could generate less bias than the OLS findings discussed above. While propensity score matching techniques are unlikely to substantially improve on OLS (because the MTO program population is relatively homogenous already with respect to socio-demographic characteristics), the baseline MTO surveys include a rich set of questions about why families chose to sign up for MTO and what they are looking for in a new neighborhood that could help better model the self-selection process into neighborhood mobility.

The PI of the present project (Ludwig) is exploring those questions in parallel work that is an important complement to this paper. Nevertheless the MTO data have important limitations for carrying out within-study comparisons. First, most of the outcome measures available for MTO experimental families were collected through surveys administered by Abt Associates, and so similar measures may not be available for good candidate comparison groups.[6] More generally, the fact that MTO's program population is drawn from five cities across the country means that generating geographically matched comparison groups – thought to be an important condition for valid non-experimental inference – is complicated. Put differently, we are limited to using just the within-control group or within-experimental group variation to generate non-experimental estimates. Finally, MTO is a relatively small, voluntary program limited to families with children living in project-based housing located in selected, severely distressed areas. Whether neighborhood effects on MTO participants generalize to other populations is not clear.

IV. THE CHICAGO HOUSING VOUCHER LOTTERY

Housing vouchers subsidize low-income families to live in private-market housing.[7] Eligibility limits for housing programs are a function of family size and income, and have been changing over time. Since 1975 an increasing share of housing assistance has been devoted to what HUD terms "very low-income households," with incomes for a family of four that would be not more than 50 percent of the local median. (The federal poverty line is usually around 30 percent of the local median). The maximum subsidy available to families is governed by the Fair Market Rent (FMR), which equaled the 45th percentile of the local private-market rent distribution through 1995, was lowered to the 40th percentile in 1995, and then in 2001 selected metropolitan areas, including Chicago, have been allowed to set FMR equal to the 50th percentile. By way of background, the FMR for a two-bedroom apartment in the Chicago area was equal to $699 in 1994, $732 in 1997, and $762 in 2000.

Families receiving vouchers are required to pay 30 percent of their adjusted income toward rent. Adjusted income is calculated by subtracting from a family's (reported) gross income deductions of $480 per child, $400 per disabled member of the household, child care expenses, and medical care expenses over 3% of annual income. TANF assistance is counted toward the calculation of gross income, but EITC benefits and the value of Food Stamps, Medicaid and other in-kind benefits are not counted. The voucher covers the difference between the family's rent contribution and the lesser of the FMR or the unit rent. Starting in 1987, the government made these tenant-based subsidies "portable," meaning that families could use them to live in a municipality different from the one that issued them the subsidy.

As noted above, housing assistance is not an entitlement. In Chicago, as in other big cities, there are generally extremely long waiting lists to receive housing assistance, especially for housing vouchers. Once a family receives a housing voucher they can keep the subsidy for

---

6 The team of NBER and Abt researchers who developed the MTO mid-term survey did draw many questionnaire items from existing survey instruments such as the National Education Longitudinal Study of 1988 (NELS) or the National Longitudinal Survey of Youth in 1979 (NLSY). But because both the NELS and NLSY are intended to be nationally representative samples (albeit with some over-weighting of particular groups), they may not provide a good basis for constructing non-experimental comparison groups that are matched to the MTO treatment groups with respect to family and community disadvantage, or matched by city for that matter.
7 This discussion is based on the excellent, detailed and highly readable summary in Olsen (2003).

as long as they meet the program's income and other eligibility requirements. Despite the excess demand for housing vouchers, not all families offered vouchers wind up using them. Many apartments have rents above the FMR limit, some landlords may avoid renting to voucher families,[8] and families offered vouchers have a limited time (usually 3 to 6 months) to use the voucher to lease up a unit.

The Chicago housing voucher lottery that we evaluate in this study was conducted during a decade of considerable turmoil in the city's low-income housing programs. In 1995, the U.S. Department of Housing and Urban Development (HUD) took over the CHA's operations in response to the latter's poor management of the city's low-income housing programs. In addition to demolishing thousands of the city's project-based housing units and turning others over to private companies to operate,9 the new CHA management also made the decision to turn over operation of the city's voucher program to a new private organization, the Chicago Housing Authority Corporation (CHAC).

In July 1997, CHAC conducted an open registration for housing vouchers, the first time in twelve years that the city's voucher wait list had been opened. More than 105,000 households applied to CHAC for vouchers, of whom 82,607 were found to be income-eligible for tenant-based housing subsidies. While CHAC's initial plan had been to randomly assign 25,000 families to the voucher waiting list, given the strong demand for these subsidies the agency randomly ordered all eligible applicants and assigned the first 35,000 to the active wait list.[10] The other eligible households (that is, with lottery numbers from 35,001 to 82,607) were not placed on any waiting list; because these families had no realistic prospects of receiving a voucher in the foreseeable future we use this group as our preferred control group in our analyses.

By August 1997, CHAC notified families by mail of their position on the voucher wait list, and began the process of offering housing vouchers to a limited number of households with the lowest (that is, the best) lottery numbers. Roughly 4,625 families were offered vouchers in the first year of the program. Service of the 1997 wait list was interrupted in August 1998 as CHAC was required to provide vouchers to a special waiting list of Latino families in response to a discrimination lawsuit filed against the city of Chicago.[11] CHAC began to serve their original wait list again at the beginning of 2000, when 2,500 families were offered vouchers. Another 5,800 families were offered vouchers in 2001, while 4,700 were offered vouchers in 2002 so that by the end of that year nearly half the original 1997 active wait-list (17,663 out of 35,000) had been offered a voucher. In May 2003 CHAC had reached a point where the agency was over-leased, at which point the agency sent out a letter to all families still on the wait list

---

8 Some landlords may avoid renting to voucher families because of the paperwork requirements, the program's minimum housing quality standards (which must be verified by an inspection, although failed units can be modified and re-inspected), and a previous rule that has since been abolished that limited the ability of landlords to turn away future voucher applicants ("take one, take all").
9 "CHA Turnaround is No Overnight Project," Chicago Sun-Times, by Gilbert Jimenez, 12/3/95, p. 61, and "Room for Improvement at CHA," Gilbert Jimenez, Chicago Sun-Times, 5/26/96, p. 33.
10 "CHA to expand Sec. 8 waiting list by 10,000," Leon Pitt, Chicago Sun-Times, August 19, 1997.
11 "CHA, HUD Settle Suit Over Bias Against Hispanics," G. Jimenez, Chicago Sun-Times, 4/23/96, p. 12. At that point, CHAC notified families still on the waiting list that they could expect to be offered a voucher "*at least* one year later than originally planned" (emphasis in original).

asking them to verify their current address and continued interest in receiving a voucher,[12] and then notified the respondents that they would likely have to wait at least a year and in most cases longer for a voucher.[13] In the present study we focus on families who were offered vouchers by CHAC through 2003.

V. DATA

In this section we discuss how we identify children living in CHAC applicant households, since they are not listed by name on the voucher application forms submitted to CHAC, and then discuss the administrative data sources that we use to measure voucher impacts on children's cognitive and non-cognitive outcomes.

A. Identifying Children in Voucher-applicant Households

One challenge for our study is that the CHAC voucher application forms ask for the name, DOB and SSN for the household head and (if relevant) spouse, as well as the *number* of children in the home, but not for the names of children in the home. We take advantage of the fact that most of our families are low-income and received social services at some point prior to the CHAC lottery, and use these pre-lottery social program records to identify children. Our specific three-step process for identifying children is:

(1) First, we identify the most recent social program spells for CHAC applicant adults that occurred before the CHAC housing-voucher lottery (July, 1997);
(2) Identify the other people listed as household members in these program spells;
(3) Eliminate people who were obviously not living with the CHAC applicant as of July 1997 as best we can tell from social program data.14

Our process for identifying household members is, while carefully done, necessarily imperfect. For example the average household size identified by our imputation procedure is somewhat smaller than what is recorded on the CHAC application forms (2.4 versus 3.0). Our procedure seems to do a fairly good job identifying school-aged children in CHAC applicant households, obviously key for present purposes, and does a bit less well capturing children who are ages 0 to 5 at the time of the CHAC lottery.[15] We can test whether any errors in identifying

---

12 That is, CHAC had issued as many or more vouchers than it had funding to pay for, and the turnover rate was low enough that it only provided enough vouchers for a series of special programs such as public housing relocation, victim assistance, witness protection, etc. that receive the highest priority for vouchers.

13 Roughly 9,300 families responded by the September 5, 2003 deadline. Around 4,000 letters were returned by the post office and the remainder did not respond. In a follow-up letter, CHAC indicated that families with numbers between 18,110 and 20,853 should expect to wait at least one year; numbers between 20,854 and 27,455 at least two years; 27,457 to 33,902 at least three years and the remainder at least four years. Personal communication with Ken Coles, CHAC, on 4/8/2004.

14 Specifically, we examine whether the other candidate household members identified by the first two steps above show up on another welfare case that is subsequent to the start of the CHAC applicant's most recent pre-lottery welfare case. If so, we compare the address listed for the candidate household member's more recent welfare case with the address listed for the CHAC applicant's most recent welfare case, and then assume that people with a different address are not living with the CHAC applicant at baseline.

15 When families leased up with a CHAC voucher they are required to complete a HUD 50058 form that asks them to report the name and DOB of everyone in the home, including children. This lets us compare the age distribution of households according to our IDHS matching procedure described above with the age distribution according to the official 50058 records. One potential concern is that the household composition reported on the 50058 data is for the

baseline household members is related to the wait-list lottery outcome by regressing the difference in household size between our IDHS matching procedure and the CHAC application forms against wait-list lottery outcomes; we find no statistically significant relationship.

B. Administrative Data

We measure the outcomes of housing-voucher receipt using individual-level administrative records obtained from the Chicago Public Schools (CPS) and the Illinois State Police (ISP). To preserve the strength of the randomized voucher lottery design, we are careful to match these outcome data to CHAC applicant children only using information about these children that comes from pre-lottery data sources.

From the CPS we obtain student-level school records for the academic years 1994-5 through 2004-5 that include information about each child for each semester they were enrolled in the CPS, including the school attended, grade, home address, race, gender, legal guardian, and special education status. Children in grades 3 through 8 are required to take each May the standardized reading and math tests from the Iowa Test of Basic Skills (ITBS). For older children who are no longer administered standardized tests by the CPS, we examine school persistence outcomes such as graduation and dropout. We believe the former should be more reliably measured than the latter since the line between chronic absence and dropout may be blurry, and schools may have incentives to not report dropouts to boost enrollment counts (and hence funding). For additional details on these data see Jacob (2004).

For older children in the CHAC housing-voucher lottery we examine involvement with criminal behavior by examining official arrest histories maintained by the Illinois State Police (ISP), which capture all arrests made by law enforcement at any level in the state of Illinois over the period 1990 to 2005. These data are intended to capture all arrests made to juveniles (under 17 in Illinois) and adults as well; in practice the degree to which these data capture juvenile arrests seems to improve over the course of the 1990s, which is good news for our study given our housing voucher lottery was conducted in 1997.[16] Arrests are linked to individuals using biometric data (fingerprints), and so we will capture a given person's entire arrest history even if they report a false name at the time of one of their arrests.[17] These arrest histories include information on the date of each arrest, all criminal charges filed as a result of the arrest, and the

household that actually moves with the CHAC applicant, which could be different from the set of people living with the CHAC applicant at the time they applied to CHAC, but in practice for families who leased up with a CHAC voucher the overall household size reported on their 50058 forms is quite close to what they reported on their CHAC application forms.

16 We also directly test the possibility that the ISP data under-count juvenile arrests by examining whether the age-crime curve "jumps" at the age of majority in the state of Illinois criminal justice system (17). We do not see any unusual deviation at age 17 from the general pattern of increasing arrest rates by age during adolescence, particularly starting in the late 1990s in our data. However the link between arrest data and court disposition information remains much better for adult than for juvenile arrests (except for arrests to juveniles who are then tried in adult court).

17 With the advent of electronic fingerprint processing most police departments began as a matter of course to submit fingerprint records for juvenile arrestees as well as adult arrestees. Chicago was one of the earlier adopters in Illinois of electronic fingerprint technology, in the early 1990s. (Personal communication, Jens Ludwig with Christine Devitt of the Illinois Criminal Justice Information Authority, 10/11/2007).

disposition of each arrest.[18] In cases where the arrestee is charged with multiple criminal charges (16% of all arrests), we assign the arrest the most serious criminal charge based on the class of the offense under Illinois state law. Because transfer programs may differentially influence different types of crime, we examine separately arrests for violent, property, drugs, and other crimes. For additional details see Kling, Ludwig and Katz (2005).

Finally, we track post-assignment addresses for a 10% randomly selected sub-sample of everyone on the CHAC waiting list (regardless of voucher receipt status) through a check of credit bureau records, change of address forms and other passive-tracking sources conducted for us by the National Opinion Research Center. (We track only a random sub-sample of families for budgetary reasons). These address histories also enable us to examine whether lottery numbers are systematically related to the probability that families move out of the state of Illinois, which in turn contributes to missing data and sample attrition in our study since we are relying on state-level administrative records to measure outcomes.

C. Constructing a Non-Experimental Comparison Group

To construct our non-experimental comparison group, we first identified every individual who received AFDC, Foodsamps or Medicaid benefits in Cook County, Illinois at any point in the three-year window immediately prior to the housing voucher lottery between July 1, 1994 and July 1, 1997. While not all individuals who applied for the housing vouchers had previously received benefits in Illinois, and the vast majority of applicants were residents of Chicago proper (and not simply Cook County), this sample reflects a very broad set of low-income families in the metropolitan area who conceivably could have been eligible and applied for housing assistance. In the analysis below, we restrict the sample in various ways in order to maximize the comparability between comparison individuals and our experimental control group.

Having identified this set of comparisons, we applied the procedure described above to identify others related to these target individuals. In this way, we were able to create a set of comparison households. The full comparison group consists of 463,406 unique households and 992,072 unique individuals, although some individuals appear in multiple households. For example, a young women could appear as a child recipient on her mother's AFDC case in 1994 and then also appear as the grantee in her own AFDC case in 1997. Similarly, an individual may appear as a non-grantee household member in more than one household if the individual switched residences at any point in our time window.

The next step in the data construction process is to match these comparison individuals to individuals in the experimental sample – i.e., those people who applied for the housing voucher during the 1997 lottery.

Of the 82,607 HHH in the experimental sample, 77,172 unique individuals had received

18 We have data on both date of offense and arrest for about 43% of all arrests. In 0.16% of cases the date of offense is before the date of arrest, while 95.6% of cases the date of the arrest and offense are the same. Fully 97.9% of all arrests are within 1 month of the offense data, 98.66% of arrest dates are within 3 months of the offense date, and 99.5% of cases have an arrest date within one year of listed offense date.

benefits in Illinois in the past and were thus eligible for matching to our comparison group.[19] We discard 691 of these individuals for whom we do not have proper identifiers[20] and 39 additional individuals for whom the identifers do not appear correct.[21] This leaves us with a set of 76,442 HHHs in our experimental file that we will attempt to match to our non-experimental comparison group.

We then merge the 463,406 household heads (HHH) in our comparison sample with the 76,442 HHHs in our experimental file. Of these, 7592 individuals from the experimental sample (roughly 10%) do not match to our comparison sample.[22] We disregard these individuals from our analysis sample.

This leaves us with 463,406 households in Cook County that received some social program benefits in the three-year window prior to the lottery, of whom 68,850 applied to the 1997 CHAC housing voucher lottery. For these 68,850 experimental households, we keep critical information including the lottery number. We then merge this information back to the full comparison sample (containing all household members, and not just grantees or household heads). This leaves us with a sample of 463,406 households and 992,072 individuals.

VI. PRELIMINARY RESULTS

The descriptive statistics for our full, unrestricted sample are presented in Table 1. The comparison group includes individuals in households that did not apply for a housing voucher (463,406 – 68,850 = 379,372). The treatment group consists of individuals in households that did apply for a housing voucher and received a lottery number less than 18,206 and thus received a voucher offer. The control group consists of individuals in households that also applied for a housing voucher but received a lottery number greater than 35,000 and thus did not receive a housing voucher offer. Note that we do not include individuals in households that applied for the housing voucher and received a lottery number between 18,206 and 35,000 since this group may have anticipated receiving a voucher (for a more detailed explanation of this issue, see Jacob and Ludwig 2008).

Scanning Table 1, we can see that while the comparison and control samples are roughly comparable, there are also important differences. For example, nearly 40 percent of household heads in the non-experimental comparison group are male compared with only 35 percent in the experimental control group. Over 93% of experimental control households lived in the city of

---

19 In future iterations, we will limit this sample further to those who received benefits only during our 3-year window.
20 We should be able to get these identifiers from the Chapin Hall Center for Children. We will need to correspond with John Dilts.
21 These are cases in which the link between 2004 and 2006 identifiers did not yield a unique match. This is something inherent in Chapin Hall's unduplication process and cannot be correct. Since this is a very small set of individuals, we can disregard them without any threat to the validity of our estimates.
22 Roughly 5,600 do not match to any individual in the non-experimental group. An additional 1,900 or so HHHs from the experimental file match to an individual in the comparison sample that is not a grantee. We probably do want to include these individuals in our sample in future iterations. More generally, it is not clear why the 5,600 or so individuals do not match. John Dilts at Chapin Hall does not appear to have any answer.

Chicago compared with only 74% of non-experimental comparison households.

In order to make the non-experimental comparison group as similar to the experimental control group as possible, we focus on a sample of households (a) with Black female heads who were age 18-65 at the time of the lottery, (b) who were living in the City of Chicago at the time of the lottery, and (c) who were receiving AFDC/TANF benefits during the two quarters immediately preceding the voucher lottery (i.e., in the first two quarters of 1997). We limit the sample in this way for several reasons. First, the group of Black working age women from Chicago represents the largest single subgroup of voucher applicants. Second, these families are those might likely to have children, which will facilitate our analysis of youth outcomes. (Note, a small but significant fraction of voucher applicants consist of seniors and/or disabled individuals.) The focus on families receiving AFDC/TANF benefits in the two quarters prior to the lottery is meant to help ensure that the non-experimental comparison and experimental control groups are similar in terms of socio-economic status.

Table 2 presents the descriptive statistics on the sample of household heads in this restricted sample. Here we can see that the non-experimental comparison group and the experimental control group appear extremely similar on a wide variety of observable characteristics, including the number and age-distribution of children, the number of prior arrests and neighborhood characteristics like tract poverty rates.

Table 3 presents descriptive statistics on the sample of children in this restricted sample. Here again, the groups appear quite similar. Children in the comparison households are slightly older than those in the control households, and thus have slightly higher rates of prior arrest.

The statistics presented in Tables 2 and 3 suggest that we can closely match experimental controls on key observable characteristics using commonly available administrative data. However, one may still be concerned with less easily observable differences between the comparison and control groups. To examine this possibility, the figures below show trends in various outcome measures separately for our comparison, control and treatment groups.

The first figure shows trends in the fraction of households receiving AFDC/TANF (in Illinois) from 1989 through 2005. The red vertical line in 1997 marks the quarter in which the voucher lottery occurred. By looking at levels and trends for this outcome measure *prior to the treatment*, one can assess the plausibility of various comparison groups. In the case of AFDC/TANF receipt, both the non-experimental comparison and the experimental control group look virtually identical to the treatment group prior to the lottery. Following the lottery, the treatment and control groups continue to track each other closely, suggesting that there was no treatment effect. The comparison group appears somewhat less likely to receive benefits during the post-period, indicating that the non-experimental estimators may not perfectly replicate experimental estimates.

The second figure shows comparable trends for the fraction of households receiving any public assistance (AFDC/TANF, Foodstamps or Medicaid) in Illinois from 1989 through 2005. Here the difference between the comparison and control groups is much more noticeable. While the treatment and control groups track each very closely prior to the lottery, comparison households are less likely to receive public assistance during this period. This may suggest that households that applied to the voucher lottery were even more disadvantaged than one would conclude based on the basis of race, gender, age, household composition, neighborhood residence and AFDC receipt at the time of the voucher offer. This highlights the difficulty of constructing a valid non-experimental estimator.

Looking at the trends across groups following the voucher lottery, we see public assistance receipt among the treatment group is somewhat higher than among the control group, indicating that the voucher offer had a small negative impact on self-sufficiency for this sample. However, if one were to compare the treatment group to the non-experimental comparison group, one would conclude that the voucher offer had a much larger negative impact.

The remaining figures examine yearly arrests for children in the analysis households (i.e., the sample shown in Table 3). The data runs from 1995 through 2005, and each point represents the average number of arrests over a four-quarter period spanning the third quarter of year 1 to the second quarter of year two. We aggregate the data in this way in order to cleanly differentiate the pre- versus post-treatment periods. The data point on the horizontal line, 1997, actually reflects arrests from 1997Q3 through 1998Q2, which is the first post-lottery year. Given the differences in criminal behavior among boys and girls, we present separate figures for each group. In addition, the sample reflected in the graphs only includes individuals who were 13 years of age or older during the quarter in which the outcome is measured. That is, the sample includes all children who were less than 18 years of age as of July 1, 1997 when the lottery was conducted. However, because the rates of criminal offending are extraordinarily low for youth below the age of 13 (even among this highly disadvantaged population), we omit person-year-quarter observations in which the individual was younger than 13.

Focusing on arrests for any crime among adolescent boys in the third figure, it appears that the comparison and control groups track each other quite closely. The same is true for adolescent girls in the fourth figure. While there are some differences that appear in some of the other figures, there is no obvious pattern and many of the differences are not statistically significant.

VII. NEXT STEPS: FORMAL ANALYSIS

In what follows we first discuss the two estimates of primary interest – the effects of offering public housing families a housing voucher, and the effects on such families from using a voucher. We then outline our basic approach for examining the bias associated with alternative non-experimental estimators, discuss the choice of alternative non-experimental comparison groups for this purpose, and then review in some detail the different non-experimental estimation strategies that we propose to employ.

A. Parameters of Interest

Let Y_{1i} represent youth (i)'s outcome – an achievement test score, indicator for high school completion, number of lifetime arrests, etc. – if her family utilizes a housing voucher from CHAC, and Y_{0i} represent her outcome if her family does not use a voucher. The causal effect of a housing voucher on her outcomes is equal to $\Delta = Y_{1i} - Y_{0i}$. The familiar evaluation problem within this "potential outcomes" framework is that for a given youth (i), her family either receives a voucher so that we observe Y_{1i}, or they do not so that we observe Y_{0i}, but we cannot observe both Y_{1i} and Y_{0i}.

The CHAC housing-voucher lottery helps solve this problem by randomly assigning families to their position on the program wait list.[23] Let $A_i=1$ for youth whose families apply to CHAC for a voucher and $A_i=0$ otherwise. Similarly let $O_i=1$ for youth whose families are offered a voucher by CHAC in the first year of the program, with $O_i=0$ for families assigned to wait list numbers between 35,000 and 80,000, and so stood no chance of being offered a voucher by CHAC in the foreseeable future. (For the time being we ignore the issue of how to handle families who received one of the 35,000 best wait list numbers but were not offered vouchers within the first year of the program; we return to this point below). Finally, let $D_i=1$ for youth whose families lease up a new apartment using a voucher by CHAC, with $D_i=0$ otherwise.

One causal parameter of interest is the *Intention to Treat (ITT) effect*, which represents the average causal effect of offering a family a housing voucher. The unbiased ("experimental") ITT effect is calculated with our CHAC data on program applicants as in (1), which represents the combined effects of offering public housing youth the chance to move to better housing and/or less distressed communities, together with whatever income effects families may experience from living in private rather than public housing and any effects that a voucher offer might have on families who do not successfully lease up under the program.

$$(1) \qquad ITT = E(Y_{1i} | A_i = 1,\ O_i = 1) - E(Y_{0i} \mid A_i = 1,\ O_i = 0)\,.$$

Another parameter of interest is the *Average Treatment Effect on the Treated (ATET)*. If the offer of a voucher has no effect on those youth whose families do not use a voucher, then the ATET parameter is equal to the ITT effect scaled by the difference in voucher take-up rates between those who are versus are not offered vouchers by CHAC, as in equation (2) (see, e.g. Bloom, 1984). We call this the experimental ATET estimate.

$$(2) \qquad ATET = \frac{E(Y_{1i} \mid A_i = 1, O_i = 1) - E(Y_{0i} \mid A_i = 1, O_i = 0)}{E(D_i \mid A_i = 1, O_i = 1) - E(D_i \mid A_i = 1, O_i = 0)}\,.$$

A third parameter of interest is the estimated effect of a specific neighborhood characteristic on youth behavior, what we term the *Experimental Instrumental Variables (Experimental IV)* estimate (Liebman, Katz and Kling, 2004). Let P_i represent youth (i)'s average post-lottery census tract poverty rate, one commonly-used measure for neighborhood disadvantage. The Experimental IV estimate uses the ITT impact as the numerator, as with the ATET parameter, but now scales the ITT by the difference in tract poverty rates between the treatment and control groups rather than by the treatment take-up rate. In notation,

23 Elsewhere we have documented that the randomized lottery implemented by CHAC in 1997 to assign voucher applicants to the waiting list was in fact random (Ludwig et al., 2004, Jacob and Ludwig, 2004).

$$(3) \qquad EXP-IV = \frac{E(Y_{1i} \mid A_i = 1, O_i = 1) - E(Y_{0i} \mid A_i = 1, O_i = 0)}{E(P_i \mid A_i = 1, O_i = 1) - E(P_i \mid A_i = 1, O_i = 0)}.$$

One natural objection to the Experimental IV estimator is that the CHAC housing-voucher lottery changes a variety of neighborhood characteristics simultaneously. However, we note that this concern is also valid for all of the non-experimental neighborhood effects studies that regress one or two neighborhood measures against some youth outcome. One potential justification for this approach is that neighborhood poverty or some other measure serves as a proxy for the constellation of contextual factors that influence youth behavior. In our empirical work, we plan to explore this possibility by examining the sensitivity of the Experimental IV estimate to using different neighborhood characteristics.

The key to the experimental parameters discussed above is that we have access to data on youth whose families applied to CHAC for a housing voucher but were not offered a voucher ($A_i=1$, $O_i=0$). Because each family's position on the CHAC wait list was randomly assigned, the outcomes of youth in non-offered ("experimental control") families will provide an unbiased estimate for the counter-factual of what would have happened to youth whose families applied and were offered vouchers ($A_i=1$, $O_i=1$, the "experimental treatment" group). But in most research in the neighborhood effects or housing literatures, analysts have access to data on families who use vouchers and then must construct some non-experimental comparison group. Our objective is to examine the types of selection biases that arise with different non-experimental estimators and comparison groups for the CHAC experimental treatment group.

All of these parameters are partial equilibrium parameters; that is, they are parameters defined under the assumption that the treated and untreated outcomes for each household are fixed and do not depend on the number or identify of other individuals treated. This assumption is called the Stable Unit Treatment Value Assumption (SUTVA) in the statistics literature. In our view, the voucher recipients we study represent a small enough proportion of the low income housing market in Chicago that failing to account for any spillovers or any general equilibrium effects on prices should not appreciably affect our estimates.

B. Basic Approach

The basic notion behind our project is to estimate the bias associated with different non-experimental comparison groups and estimators. The variety of administrative data sources available to us for CHAC applicants and our various non-experimental comparison groups described above provide allow us to learn a lot about the performance of methods that rely on controlling for selection on observables, as discussed further below within the context of such models.

Following Heckman, Ichimura, Smith and Todd (1998, pp. 1030-1) we can decompose the bias with our non-experimental ITT estimates into three components: (1) bias due to non-overlap in the distributions for the background characteristics between the treatment and comparison groups; (2) different distributions for treatment and comparison groups across those values of X where the treatment and control distributions overlap (so that some X's are over- or under-weighted in one distribution relative to the other); and (3) bias that persists after controlling for observed characteristics. Empirically (1) and (2) can be at least as important as the standard concern about selection on unobservables, based on previous findings from the job training application (see Heckman, Ichimura and Todd, 1997, Heckman, Ichimura, Smith and Todd, 1998). In notation,

(4) $E(Y_{0i} \mid A_i = 1, O_i = 1) - E(Y_{0i} \mid A_i = 0, O_i = 0) = B_1 + B_2 + B_3$.

Similar decompositions follow immediately for the other parameters. Equation (4) presents the bias in terms of the difference between the outcomes of the experimental control group and the non-experimental comparison group. We can equivalently express it as the difference between the impact estimate constructed using the experimental treatment and control groups and that constructed using the experimental treatment group and the non-experimental comparison group:

(5)

$$[E(Y_{1i} \mid A_i = 1, O_i = 1) - E(Y_{0i} \mid A_i = 1, O_i = 1)] - [E(Y_{1i} \mid A_i = 1, O_i = 1) - E(Y_{0i} \mid A_i = 0, O_i = 0)] = B_1 + B_2 + B_3$$

Equations (4) and (5) help highlight the importance of accounting for baseline neighborhood characteristics in the construction of our non-experimental comparison group, either through matching or regression adjustment. If neighborhood characteristics matter for youth behavior then it is important to condition on baseline neighborhood measures. The bias formulation in equation (4) also helps to highlight the key to deriving an unbiased non-experimental estimate – adequately accounting for the selection process through which some families but not others decide to participate in the voucher program.

Estimators that assume selection on observables assume $B_3 = 0$; that is, they assume that conditioning on X leaves no residual selection on observables. The simple regression adjustment estimator assumes that conditioning linearly on X takes care of the first bias term. In regard to the second, the regression estimator either assumes no overlap problem or that the linear functional form is correct; in either case, the overlap term in the bias equals zero. The matching estimator, in contrast, does not provide an estimate of the impact for values of X for which the overlap condition does not hold in the data; thus, it avoids a functional form assumption at the cost of changing the precise nature of the parameter being estimated in a potentially unattractive way. Estimators that take account of selection on unobservables, such as the instrumental variables, bivariate normal and longitudinal estimators we describe below, do not assume $B_3 = 0$, but each estimator makes specific, different, assumptions about the exact nature of the selection on unobservables. The most obvious advantage of randomized experiments is that they overcome the self-selection problem described above – that is, selection into treatments or neighborhoods on the basis of both observed and unmeasured characteristics.

C. Estimation Strategies

Selection on Observables

The current literature on neighborhood effects is dominated by studies that adjust for neighborhood selection on observable characteristics, and assume that conditional on observables, selection on unobserved characteristics is empirically unimportant. The most common approach is to use Ordinary Least Squares (OLS) to adjust for differences in observable characteristics between treatment and comparison groups, as in

(6) $$Y_i = \beta_0 + \beta_A A_i + \beta_X X_i + e_i,$$

which is the OLS analog to the ITT estimate presented above. The experimental ITT estimate is calculated using the CHAC "offered" and "not offered" samples for $A=1$ and $A=0$. The non-experimental estimate is calculated replacing CHAC "not offered" sample for $A=0$ with the different non-experimental comparison groups described above. OLS can also be used to construct experimental and non-experimental estimates for ATET and IV, using A as an instrument for either voucher use (D) or neighborhood poverty (P) or some other neighborhood characteristic. For simplicity we focus our discussion of estimation strategies on the ITT parameter.

One important limitation with the standard regression approach is that the resulting estimates can in some cases be quite sensitive to assumptions about the functional form of the regression. If, for example, there is little overlap in the distribution of observable covariates (Xs) for the treatment and comparison groups, the linear functional form implicit in the OLS regression model "fills in" the missing counterfactual outcome in regions where there are treated observations but no untreated observations.

Matching estimators relax the linear functional form assumption underlying the standard regression and instead use only observations with the same or very similar characteristics to those of each treated individual to construct that individuals estimated untreated outcome. In addition, matching highlights the problem of limited overlap in a clear and direct way. As a result of these advantages relative to standard regression methods, matching estimators have received much attention in the recent econometric literature (see, for example, Heckman, Ichimura, Smith and Todd (1996, 1998) and Heckman, Ichimura and Todd (1997, 1998) as well Dehejia and Wahba (1999, 2002) and Smith and Todd (2004) for discussions). The cost of using matching methods is that they generally produce less precise estimates than regression-based estimation.

One of the methodological goals of our project is to compare matching and regression methods in a neighborhood effects context. In doing so, we will explore the variety of existing matching methods present in the literature (see, e.g. Smith and Todd, 2004, for a description and Frölich, 2004, for a Monte Carlo study). These include nearest neighbor matching, kernel matching and local linear matching, each of which we will apply using both the Mahalanobis distance and the estimated propensity score, $\Pr(A = 1 \mid X)$. We will use cross-validation methods, as applied in Black and Smith (2004), to select the bandwidth or number of neighbors and to choose among alternative kernels in the cases of kernel and local linear matching.

Another goal of our project is to learn more about *which* observable characteristics are the most important determinants of selection into voucher programs or specific types of neighborhoods. Selection on observables, whether implemented using OLS or matching, requires that we observe all the variables that affect *both* application to the program and outcomes in the absence of participation in the program. Given the pooled administrative data that we have, we can condition on a rich set of background characteristics including the socio-demographic characteristics of CHAC youth and their families as well as employment and earnings, and even detailed measures of previous youth outcomes such as grades, achievement test scores, and criminal history. We can also control for characteristics of a youth's baseline neighborhood using not only standard socio-demographic measures from the census (e.g., poverty rates, percent households headed by a female, percent households living in public housing), but also more sophisticated measures of neighborhood social capital, including factors such as "collective efficacy," captured in the PHDCN data (Sampson, Raudenbush and Earls, 1997). We define all of these background covariates as of the time of the CHAC lottery (July, 1997) for our experimental treatment and control groups as well as the non-experimental comparison groups.

Among those observable characteristics associated with selection into the voucher program, we expect the most important to be measures of youth criminal activity and schooling. As noted above, this hypothesis stems from the fact that MTO households volunteered for that program primarily because of concern about crime and their children's schooling, rather than because of other outcomes such as employment opportunities. Outcome data from MTO also suggests that among those families assigned to the MTO experimental or Section 8 groups, treatment take-up may be positively related to youth problem behavior.[24]

Finally, we plan to examine how selection into the voucher program is associated with the changes as well as levels of outcome measures. Ashenfelter (1978) found that participants in job training programs experience a decline in earnings before enrollment, the so-called "Ashenfelter dip," which leads to bias with cross-section or difference-in-difference estimates that compare enrollees to other eligible populations that do not experience a similar temporary decline in earnings. Heckman and Smith (1999, 2004) find that transitions in labor force status are more important than earnings changes in explaining job-training enrollment. Understanding how participation in housing voucher programs is related to both levels and trends in pre-lottery characteristics is central for overcoming selection bias in future non-experimental studies of voucher programs. In addition, knowledge about the process of selection is of both scholarly and policy interest in its own right for what it tells us about how households decide whether or not to apply to voucher programs and about the resulting over- or under-representation of particular groups in the voucher program relative to the proportion of the eligible population.

24 Short-term outcome data from the Baltimore and Boston MTO sites show that families that comply with treatment in the experimental or Section 8 groups have children at elevated risk for problem behavior (Katz, Kling and Liebman, 2001, Ludwig, Duncan and Hirschfield, 2001). Data from all 5 sites 4-7 years after random assignment tends to find a similar pattern for females. For males, families are more likely to comply with the experimental treatment in cases, while the pattern for males is somewhat more complicated (Kling, Ludwig and Katz, 2004).

Selection on Unobservables I: Longitudinal Estimators

Insofar as program participation and outcomes are correlated with unobservable characteristics of youth, the estimators described above are likely to be biased. A set of alternative estimators attempts to deal with such selection on unobservables by making make the assumption that selection into the voucher program depends on time-invariant unobserved differences among households (referred to as household "fixed effects"). The assumption of selection on a time invariant fixed effect is formalized in the literature as the Bias Stability Assumption (BSA). In terms of our notation, this is given by

(7)

$$BSA: E(Y_{0it} \mid A_{it}=1, O_{it}=0) - E(Y_{0it} \mid A_{it}=0, O_{it}=0) = E(Y_{0it'} \mid A_{it}=0, O_{it}=0) - E(Y_{0it'} \mid A_{it}=0, O_{it}=0)$$

,

where "t" denotes a period "after" treatment and "t'" denotes a period prior to treatment and where it is assumed that neither group has access to treatment in the "before" period. In words, the BSA states that the expected difference in untreated outcomes between the treated and untreated households is the same in the before and after periods, and can therefore be differenced out if data for both periods are available.

We plan to implement three different estimators that rely on variants of the BSA. The first is the standard regression-based difference-in-differences (DID) estimator, which relies on a version of (7) that also conditions on observed covariates *X*. In this case, we estimate

(8) $$Y_{it} - Y_{it'} = \beta_0 + \beta_1(X_{1it} - X_{1it'}) + \ldots + \beta_k(X_{kit} - X_{kit'}) + \delta A_i + (u_{it} - u_{it'})$$

where A_i is a dummy variable indicating whether or not a given household ever gets treated. Formally the DID estimator in (8) only requires the BSA to hold in the two periods used to construct the estimate. For this reason, it is sometimes calculated leaving out the period right around the period of participation, which effectively means "solving" the problem of the pre-treatment dip in mean outcomes among participants, which suggests selection on transitory outcome changes rather than fixed differences, by simply leaving out the relevant periods. Our choice of the "before" and "after" periods will depend on a preliminary examination of the temporal patterns of outcomes among participants and non-participants during the period prior to the housing lottery. Note that the longitudinal estimators described in this section are closely related to the cross-sectional estimators that include controls for pre-lottery measures of the outcome variable. Indeed, in the absence of other covariates, the DID estimator in equation (8) is identical to a cross sectional OLS estimator, like equation (6), that includes a single pre-lottery measure of the outcome variable as the only covariate and constrains the coefficient on this pre-treatment outcome to equal one.

In addition to the regression based version of conditional (on *X*) difference-in-differences, we will also implement the DID matching estimator developed in Heckman, Ichimura and Todd (1997) and Heckman, Ichimura, Smith and Todd (1998). Given that we have panel data, the DID matching estimator is the same as the cross-sectional matching estimator, but with the difference in outcomes ("after" minus "before") as the dependent variable in the matching in place of the "after" period outcome level. The DID matching estimator relies on the same, conditional on *X*, version of the BSA in equation (8) as the regression based DID estimator, but again has the advantage of conditioning more flexibly on the *X* and of highlighting any support problems that may exist.

Finally, given that we have multiple periods of data both before and after the month of the CHAC lottery, we can also use standard panel data methods, assuming that it appears plausible following our search for an analog to the "Ashenfelter dip" in our context. Such a dip is clearly inconsistent with the use of the panel estimator. These panel methods rely on a version of the BSA in (8) that applies in all of the periods included in the analysis. In notation, we assume the model

$$(9) \qquad Y_{it} = \beta_0 + \beta_1 X_{1it} + \ldots + \beta_k X_{kit} + \delta A_i + \mu_i + \varepsilon_{it}$$

where the household-specific fixed effects μ_i either get differenced out or directly estimated, depending on the particular panel data estimator used in the analysis. The additional periods of pre-program data allow more precise estimates of the effects of observable characteristics on the untreated outcomes and also allow the estimation of time trends, either in the aggregate or at the individual level, in the untreated outcomes. See, e.g. Blundell et al. (2001) for a very clear application of panel methods with time trends in the context of evaluating active labor market programs in Britain.

We will use the longitudinal estimators described here in three ways. First, we will use them to estimate the ITT parameter using the observations randomly assigned to voucher eligibility and each non-experimental comparison group. Second, we will use them to estimate the ATET parameter using the individuals within the group randomly assigned to eligibility for a voucher who actually use the voucher as the treated observations and those who do not use the voucher (the analogue to the "dropouts" in the case of training programs) as the comparison group. Third, we will again estimate the ATET parameter using the individuals who actually use a voucher as the treatment group in combination with each non-experimental comparison group. Comparing the second two estimates is informative about the relative importance of selection bias associated with non-random application and selection bias associated with non-random take-up of the vouchers among those randomly assigned to eligibility. The resulting knowledge is of great use in designing appropriate comparison group designs in future non-experimental studies.

Selection on Unobservables II: Sibling Differences

An alternative approach utilizes the different experiences of siblings to estimate neighborhood effects (Aaronson, 1998, Plotnick and Hoffman, 1999). In this approach, the difference in outcomes between siblings is modeled as a function of the difference in neighborhood characteristics experienced by each sibling as a child. For example, if a family moved to a more affluent neighborhood when the two children were ages 9 and 18, the younger sibling would be expected to have better outcomes if neighborhood affluence has a positive impact on child development. Such “sibling-difference” models can provide unbiased estimates neighborhood mobility is based on unobserved family characteristics that are “fixed” or time invariant.[25]

Formally, this type of sibling difference model can be estimated in the following OLS framework:

$$(10) \qquad Y_{ij} = \beta_0 + \beta_A A_{ij} + \beta_X X_{ij} + \gamma_j + e_{ij}$$

where *i* denotes individual youth and *j* denotes family and γ_j is a vector of family fixed effects. A_{ij} can represent either a measure of the neighborhood poverty level experienced by sibling *i* in family *j*, or an indicator of whether family *j* utilized a housing voucher while sibling *i* was younger than a certain age (note that A_{ij} can be specified to capture different aspects of neighborhood poverty, including the minimum, maximum or average poverty levels experienced by the child).

We plan to estimate sibling-difference models using two different samples in an effort to approximate the type of non-experimental analyses that researchers have conducted, or could conceivably conduct. The first sample will include a representative group of low-income families in Chicago, and is meant to replicate the type of analysis one might conduct using survey data such as the SIPP. For this sample, we will identify families with multiple siblings, and using state and local administrative data, construct a history of where the family has lived over a number of years. With this information, we can characterize the type of neighborhoods that each sibling experienced as a child (which is captured in the variable A_{ij} above). The second sample will be limited to applicants to the CHAC housing lottery who both received and utilized a housing voucher. This sample is interesting insofar as it approximates the type of study that could be done if one had access to administrative data on individuals who utilized a particular social program, but no information on individuals who either did not apply for or did not utilize the program. In our experience, given the way that the information technology systems in many public agencies are configured, this is an extremely common scenario.

Selection on Unobservables III: Approaches based on Exclusion Restrictions

A third and increasingly common strategy for accounting for selection on unobservables involves the use of instrumental variables (IV). As with other non-experimental approaches, it is generally difficult to assess how well the IV estimates approximate causal impacts. In this project, we will examine two different IV approaches to determine whether they can adequately account for selection biases.

The first IV approach exploits the sex composition of children within a family to estimate the impact of housing assistance and, by extension, neighborhoods, on youth. In a seminal

25 Similar models have been used in prominent studies of social programs such as Head Start (Currie and Thomas, 1995, Garces, Thomas and Currie, 2002).

paper, Currie and Yelowitz (2001) note that HUD voucher program rules that determine the size of the apartment for which a family is eligible specify that children of the same sex must share a bedroom while children of a different sex do not have to share a bedroom. The result is that families with two children are eligible for a more lucrative voucher if their children are of a different sex. Thus, sex composition will affect the expected benefits of receiving a housing voucher and so may help explain voucher program participation, while at the same time it may plausibly be otherwise unrelated to youth outcomes.

Implementing this strategy in a 2SLS framework, the second stage equation is:

$$(11) \qquad Y_{ij} = \beta_0 + \beta_A A_{ij} + \beta_X X_{ij} + e_{ij}$$

where all variables are the same as in equation (10). The first stage equation

$$(12) \qquad A_{ij} = \beta_0 + \beta_Z Z_j + \beta_X X_{ij} + u_{ij}$$

relates neighborhood poverty or voucher usage, A_{ij}, to the instrument Z_j, which takes on the value of one if family *j* consists of a boy and a girl and a value of zero if the family has either two boys or two girls. Following Currie and Yelowitz (2001), the sample is limited to families with two children for simplicity. Note that in all models – experimental and non-experimental – it will be important to account for the correlation of errors among siblings within the same family.

As with the sibling-difference approach, we plan to estimate gender composition models on two different samples: one that includes a representative sample of low-income families from Chicago and one that is limited to the set of families who applied for the 1997 CHAC housing voucher lottery. The first sample will inform the validity of estimates such as those constructed by Currie and Yelowitz (2001). As noted above, however, this estimator is limited to households with at least two children. Fortunately, the variation generated by the CHAC lottery allows us to construct experimental estimates for just such households. Thus, the IV estimates we obtain using the second CHAC lottery sample can be directly compared to the experimental estimates for two-child families in order to assess the validity of this non-experimental approach.

A second strategy, developed by Evans, Oates and Schwab (1992), utilizes MSA-level characteristics to describe the "neighborhood choice set" available to families. Unfortunately this exact IV strategy is not available to us given that our data is drawn mostly from Chicago. However we can implement a variant of the Evans et al. (1992) approach as follows: Data from recent mobility experiments suggest that most families (including public housing residents) who receive a voucher tend to stay close to their origin address. Presumably this tendency is driven by some combination of familiarity with nearby neighborhoods, the desire to maintain proximity to social networks, church, and (for those who are employed) work. Whatever the reason, for any given family in Chicago (in either our treatment or non-experimental comparison groups) we can construct an instrument using census-tract level data equal to the mean (or other moments) of the distribution of all census tracts within a certain distance of the family's 1997 address. To the extent to which the decision to apply for or use a voucher is related to a family's neighborhood choice set, and if families tend to limit their search to neighborhoods "near" their origin address, then these neighborhood "sub-markets" may help explain intra-MSA variation in voucher applications and (among those who apply) voucher take-up rates.[26]

When we limit our sample to the set of families who were offered vouchers in the 1997 CHAC lottery, we will use the desirability of surrounding census tracts as an instrument for voucher take-up or, alternatively, subsequent neighborhood poverty. In this strategy, we maintain the second stage equation shown in equation (11) and estimate the following first stage:

$$(13) \qquad A_{ij} = \beta_0 + \beta_Z Z_j + \beta_X X_{ij} + u_{ij},$$

where the instrument Z_j is, for example, the average poverty rate of all census tracts within a 3 mile radius of family *j*'s baseline (i.e., 1997) address. Note that this specification does not utilize any of the variation created by the lottery, but instead relies on the characteristics of surrounding neighborhoods in 1997 to generate variation in subsequent neighborhood characteristics.[27] For this reason, it may be appropriate to account for the correlation of errors among youth in the same baseline neighborhood rather than simply among siblings within the same household. Finally, we will also estimate similar models using a representative sample of poor families in Chicago in 1997 instead of only voucher applicants.

All of the IV estimators have analogous estimators based on the bivariate normal selection model of Heckman (1979). The bivariate normal selection makes stronger functional form assumptions regarding the error terms in the participation and outcome equations than does IV; in particular, it assumes that the two error terms are jointly normally distributed. It is the correlation between the two errors that leads to the selection problem. Although the bivariate normal selection model is technically identified without an exclusion restriction (that is, without a variable that appears in the participation equation but not in the outcome equation – the direct analog of the instrument in the IV estimator), it is well known in the literature that the model performs poorly and tends to instability when identified on functional form alone; see, e.g. the survey by Puhani (2000) on this point. The potential advantage of the bivariate normal model relative to IV is that the estimates are more efficient if the normal functional form assumption is correct, and the model provides a direct estimate of the correlation between the two error terms, which is informative about the nature of the selection on unobservables and therefore of substantive interest. For continuous outcomes, we will apply both the Heckman (1979) two-stage estimator and the FIML estimator; the advantage of the latter is efficiency while the advantage of the former is greater robustness to departures from joint normality. For binary outcomes, we will use the bivariate probit version of the model.

---

26 As with Evans et al. (1992), the estimates from this specification may be biased if the characteristics of a family's surrounding neighborhood influences our outcome measures in a way other than through the residential location of the family.

27 Given the heterogeneity of Chicago neighborhoods, and the substantial changes that these neighborhoods were experiencing in the late 1990s due to, among other things, the demolition of many public housing units, the exclusion restriction may be less likely to hold in our analysis as compared with the analysis in Evans, Oates and Schwab (1992).

REFERENCES

Aaronson, Daniel (1998). "Using Sibling Data to Estimate the Impact of Neighborhoods on Children's Educational Outcomes." *Journal of Human Resources*. 33(4): 915-946.

Agodini, Roberto and Mark Dynarski (2004) "Are Experiments the Only Option? A Look at Dropout Prevention Programs." *Review of Economics and Statistics*. 86(1): 180-194.

Ashenfelter, Orley (1978). "Estimating the Effect of Training Programs on Earnings." *Review of Economics and Statistics*. 60: 47-57.

Ashenfelter, Orley and Alan Krueger (1994). "Estimates of the Economic Returns to Schooling from a New Sample of Twins." *American Economic Review*. 84(5): 1157-83.

Bell, Stephen, Larry Orr, John Blomquist and Glen Cain (1995). "Program Applicants as a Comparison Group in Evaluating Training Programs." Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

Bingenheimer, Jeffrey B., Robert T. Brennan, and Felton J. Earls. 2005. Firearm violence exposure and serious violent behavior. *Science* 308, (5726): 1323.

Black, Dan and Jeffrey Smith. (2004). "How Robust is the Evidence on the Effects of College Quality? Evidence from Matching." *Journal of Econometrics*. 121(1): 99-124

Bloom, Howard (1984). "Accounting for No-Shows in Experimental Evaluation Designs." *Evaluation Review*. 8: 225-46.

Blundell, Richard, Monica Costa Dias, Costas Meghir and John Van Reenan (2001). "Evaluating the Impact of a Mandatory Job Search Assistance Program." IFS Working Paper No. WP01/20.

Brooks-Gunn, J., Duncan, G. J., Klebanov, P. K., & Sealand, N. (1993). Do Neighborhoods Influence Child and Adolescent Development? AMERICAN JOURNAL OF SOCIOLOGY. 99 (2), 353.

Brooks-Gunn, Jeanne, Greg J. Duncan and J. Lawrence Aber, Eds. (1997). *Neighborhood Poverty: Context and Consequences for Children*. New York: Russell Sage.

Browning, C. R., S. Feinberg, and R. D. Dietz. 2004. The paradox of social organization: Networks, collective efficacy, and violent crime in urban neighborhoods. *SOCIAL FORCES* 83, (2): 503-34.

Clampet-Lundquist, S., & Massey, D. (2008). Neighborhood Effects on Economic Self-Sufficiency: A Reconsideration of the Moving to Opportunity Experiment. AMERICAN JOURNAL OF SOCIOLOGY. 114 (1), 107-143.

Cook, Thomas D., William R. Shadish, and Vivian C. Wong (2008) "Three conditions under which experiments and observational studies produce comparable causal estimates: New findings from within-study comparisons." *Journal of Policy Analysis and Management*. 27(4): 724-750.

Currie, Janet and Duncan Thomas (1995). "Does Head Start Make a Difference?" *American Economic Review*. 85(3): 341-64.

Currie, Janet and Yelowitz, Aaron (2000). "Are Public Housing Projects Good for Kids?" *Journal of Public Economics*. 75(1): 99-124.

Dehejia, Rajeev and Sadek Wahba (1999). "Causal Effects in Nonexperimental Studies: Re-evaluating the Evaluation of Training Programs." *Journal of the American Statistical Association*. 94(448): 1053-1062.

Dehejia, Rajeev and Sadek Wahba (2002). "Propensity Score Matching Methods for Non-Experimental Causal Studies. *Review of Economics and Statistics*. 84: 151-175.

Duncan, Greg J. and Jens Ludwig (2000) "Can Residential Mobility Improve the Life Chances of Poor Children?" *Brookings Children's Roundtable Policy Brief #3*. Washington, DC: Brookings.

Ellen, Ingrid Gould and Margery Austin Turner (2003). "Do Neighborhoods Matter and Why?" In *Choosing a Better Life? Evaluating the Moving to Opportunity Social Experiment*. Edited by John Goering and Judie Feins. Washington, DC: Urban Institute Press. pp. 313-338.

Evans, William N., Oates, Wallace E. and Robert M Schwab (1992). "Measuring Peer Group Effects: A Study of Teenage Behavior." *Journal of Political Economy* 100(5): 966-991.

Fraker, T. and Rebecca Maynard (1987). "The Adequacy of Comparison Group Designs for Evaluations of Employment-Related Programs." *Journal of Human Resources*. 22: 194-227.

Frolich, Markus (2004). "Finite-Sample Properties of Propensity-Score Matching and Weighting Estimators." *Review of Economics and Statistics*. 86(1): 77-90.

Garces, Eliana, Duncan Thomas and Janet Currie (2002). "Longer Term Effects of Head Start." *American Economic Review*. 92(4): 999-1012.

Gaviria, A., & Raphael, S. (2001). SCHOOL-BASED PEER EFFECTS AND JUVENILE BEHAVIOR. The Review of Economics and Statistics. 83 (2), 257-268.

Geronimus, Arlene and Sanders Korenman (1992). "The Socioeconomic Consequences of Teen Childbearing Reconsidered." *Quarterly Journal of Economics*. 107(4): 1187-1215.

Heckman, James J. (1979). "Sample Selection Bias as a Specification Error." *Econometrica.* 47(1): 153-162.

Heckman, James, Jeffrey Smith and Nancy Clements (1997). "Making the Most Out of Programme Evaluations and Social Experiments: Accounting for Heterogeneity in Programme Impacts." *Review of Economic Studies*. 64(4): 487-535.

Heckman, James J. and V. Joseph Hotz (1989). "Alternative Methods for Evaluating the Impact of Training Programs." *Journal of the American Statistical Association*. 84(804): 862-874.

Heckman, James, Hidehiko Ichimura, and Petra Todd (1997). "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme." *Review of Economic Studies*. 64: 605-654.

Heckman, James, Hidehiko Ichimura, Jeffrey Smith and Petra Todd (1996). "Sources of Selection Bias in Evaluating Social Programs: An Interpretation of Conventional Measures and Evidence on the Effectiveness of Matching as a Program Evaluation Method." *Proceedings of the National Academy of Sciences*. 93: 13416-13420.

Heckman, James, Hidehiko Ichimura, Jeffrey Smith and Petra Todd (1998). "Characterizing Selection Bias Using Experimental Data." *Econometrica*. 66(5): 1017-1098.

Heckman, James and Jeffrey Smith (1999). "The Pre-Program Earnings Dip and the Determinants of Participation in a Social Program: Implications for Simple Program Evaluation Strategies." *Economic Journal*. 109(457): 313-348.

Heckman, James and Jeffrey Smith (2004). "The Determinants of Participation in a Social Program: Evidence from the Job Training Partnership Act." *Journal of Labor Economics*. 22(4): 243-298.

Heymann, J. and A. Fischer (2003) "Neighborhoods, health research, and its relevance to public policy." In *Neighborhoods and Health*, Eds. I. Kawachi and L.F. Berkman. NY: Oxford. Pp. 335-348.

Holzer, Harry J. (1991). "The Spatial Mismatch Hypothesis: What Has the Evidence Show?" *Urban Studies*. (February).

Hoxby, C. (2000). Peer effects in the classroom Learning from gender and race variation. NBER working paper, no. W7867. Cambridge, MA: National Bureau of Economic Research.

Jacob, Brian A. (2004). "Public Housing, Housing Vouchers and Student Achievement: Evidence from Public Housing Demolitions in Chicago." *American Economic Review*. 94(1): 233-258.

Jacob, B. and Ludwig, J. (2004). "The Impact of Section 8 Housing Vouchers on Educational and Labor Force Outcomes: Evidence from a Housing-Voucher Lottery in Chicago." Unpublished manuscript.

Jargowsky, Paul (1997). *Poverty and Place*. New York: Russell Sage.

Jargowsky, Paul (2003). *Stunning Progress, Hidden Problems: The Dramatic Decline of Concentrated Poverty in the 1990's*. Washington, DC: Brookings Institution.

Jencks, Christopher. and Susan E. Mayer (1990). "The Social Consequences of Growing Up in a Poor Neighborhood." *Inner-City Poverty in the United States*. L. E. Lynn and M. G. H. McGeary. Washington, DC, National Academy Press**:** 111-186.

Kain, John F. (1968). "Housing Segregation, Negro Employment and Metropolitan Decentralization." *Quarterly Journal of Economics*. 82(2): 175-197.

Katz, Lawrence, Jeffrey Kling, Jeffrey Liebman (2001). "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment." *Quarterly Journal of Economics*. 116(2): 607-654.

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz (2007) "Experimental analysis of neighborhood effects." *Econometrica*. 75(1): 83-119.

Kling, Jeffrey R., Jens Ludwig, and Lawrence F. Katz (2005) "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Mobility Experiment." *Quarterly Journal of Economics*. 120(1): 87-130.

Kirk, D. 2008. The neighborhood context of racial and ethnic disparities in arrest. *Demography* 45, (1): 55-77.

LaLonde, Robert J. (1986). "Evaluating the Econometric Evaluations of Training Programs with Experimental Data." *American Economic Review*. 76(4): 604-620.

Lareau, A. (2003). Unequal childhoods: Class, race, and family life. Berkeley: University of California Press.

Leventhal, Tama and Jeanne Brooks-Gunn (2000). *Moving to Opportunity: What About the Kids?* New York City, Teachers College, Columbia University.

Leventhal, Tama and Jeanne Brooks-Gunn (2002). *The Early Impacts of Moving to Opportunity on Children and Youth in New York City.* New York City, Teachers College, Columbia University.

Ludwig, Jens, Greg J. Duncan and Paul Hirschfield (2001). "Urban Poverty and Juvenile Crime: Evidence from a Randomized Housing-Mobility Experiment." *Quarterly Journal of Economics*. 116(2): 655-680.

Ludwig, Jens, Helen F. Ladd and Greg J. Duncan (2001). "Urban Poverty and Educational Outcomes." *Brookings-Wharton Papers on Urban Affairs*. Edited by William Gale and Janet Rothenberg Pack. Washington, DC: Brookings Institution. pp. 147-201.

Ludwig, Jens, and Jeffrey R. Kling. 2007. Is crime contagious? *The Journal of Law & Economics.* 50, (3): 491.

Ludwig, J., Liebman, J., Kling, J., Duncan, G., Katz, L., Kessler, R., et al. (2008). What Can We Learn about Neighborhood Effects from the Moving to Opportunity Experiment? AMERICAN JOURNAL OF SOCIOLOGY. 114 (1), 144-188.

Ludwig, Jens, Helen F. Ladd and Greg J. Duncan (2001) Urban Poverty and Educational Outcomes. Brookings-Wharton Papers on Urban Affairs. Edited by William Gale and Janet Rothenberg Pack. Washington, DC: Brookings Institution. pp. 147-201.

Luttmer, E. F. P. (2005). Neighbors as Negatives: Relative Earnings and Well-Being. The Quarterly Journal of Economics. 120 (3), 963-1002.

Manski, Charles F. (2000). "Economic Analysis of Social Interactions." *Journal of Economic Perspectives*. 14(3): 115-36.

Massey, D. (1996). The Age of Extremes: Concentrated Affluence and Poverty in the Twenty-First Century. Demography 33: 395-412.

Mendenhall, R., S. DeLuca, and G. Duncan. 2006. Neighborhood resources, racial segregation, and economic mobility: Results from the gautreaux program. *Social Science Research* 35, (4): 892-923.

Michalopoulos, Charles, Howard S. Bloom and Carolyn J. Hill (2004). "Can Propensity-Score Methods Match the Findings from a Random-Assignment Evaluation of Mandatory Welfare-to-Work Programs?" *Review of Economics and Statistics*. 86(1): 156-179.

Morenoff, J. D., R. J. Sampson, and S. W. Raudenbush. 2001. NEIGHBORHOOD INEQUALITY, COLLECTIVE EFFICACY, AND THE SPATIAL DYNAMICS OF URBAN VIOLENCE. *CRIMINOLOGY* 39, : 517-60.

Olsen, Edgar O. (2003). "Housing Programs for Low-Income Households." In *Means-Tested Transfer Programs in the United States*. Edited by Robert A. Moffitt. Chicago: University of Chicago Press. pp. 365-442.

O'Regan, Katherine and John Quigley (1999). "Accessibility and Economic Opportunity." In *Essays in Transportation Economics and Policy: A Handbook in Honor of John R. Meyer*. Edited by Jose A. Gomez-Ibanez, William B. Tye, and Clifford Winston. Washington, DC: Brookings Institution. pp. 437-468.

Puhani, Patrick (2000). "The Heckman Correction for Sample Selection and Its Critique." *Journal of Economic Surveys*. 14(1): 53-68.

Plotnick, Robert and Saul Hoffman (1999). "The Effect of Neighborhood Characteristics on

Young Adult Outcomes: Alternative Estimates." *Social Science Quarterly*. 80(1): 1-18.

Quigley, John M. (2000). "A Decent Home: Housing Policy in Perspective." In William Gale and Janet Rothenberg Pack, editors. *Brookings-Wharton Papers on Urban Affairs*. 53-100.

Raphael, Steve (1998). "The Spatial Mismatch Hypothesis of Black Youth Joblessness: Evidence from the San Francisco Bay Area." *Journal of Urban Economics*. 43(1): 79-111.

Rubinowitz, Leonard S. and James E. Rosenbaum (2001). *Crossing the Class and Color Lines: From Public Housing to White Suburbia*. Chicago: University of Chicago Press.

Sampson, R. J. 2008. Moving to inequality: Neighborhood effects and experiments meet social structure. *AMERICAN JOURNAL OF SOCIOLOGY* 114, (1): 189-231.

Sampson, Robert J., Stephen W. Raudenbush and Felton Earls (1997). "Neighborhoods and Violent Crime: A Multilevel Study of Collective Efficacy." *Science*. CCLXXVII: 918-924.

Sampson, Robert J., Jeffrey D. Morenoff and Thomas Gannon-Rowley (2002). "Assessing 'Neighborhood Effects': Social Processes and New Directions in Research." *Annual Review of Sociology*. 28: 443-478.

Sampson, Robert J., Jeffrey D. Morenoff, and Stephen Raudenbush. 2005. Social anatomy of racial and ethnic disparities in violence. *American Journal of Public Health* 95, (2): 224.

Sampson, R., Sharkey, P., & Raudenbush, S. (2008). Durable effects of concentrated disadvantage on verbal ability among African-American children. PROCEEDINGS-NATIONAL ACADEMY OF SCIENCES USA. 105 (3), 845-852.

Sanbonmatsu, Lisa, Jeanne Brooks-Gunn, Greg J. Duncan and Jeffrey Kling (2004). "Neighborhoods and Academic Achievement: Results from the MTO Experiment." Working Paper.

Shaw, Clifford and Henry McKay (1942). *Juvenile Delinquency and Urban Areas*. Chicago: University of Chicago Press.

Shroder, Mark (2002). "Does housing assistance perversely affect self-sufficiency? Review of evidence." Department of Housing and Urban Development, Office of Policy Development and Research. Unpublished manuscript.

Smith, Jeffrey and Petra Todd (forthcoming). "Does Matching Overcome LaLonde's Critique of Nonexperimental Estimators?" *Journal of Econometrics*.

Wilde, Elizabeth Ty and Robinson Hollister (2002). "How Close Is Close Enough? Testing Nonexperimental Estimates of Impact Against Experimental Estimates of Impact with Education Test Scores as Outcomes." University of Wisconsin, Institute for Research on Poverty Discussion Paper 1242-02.

Wilson, William Julius (1987). *The Truly Disadvantaged: The Inner-City, the Underclass and Public Policy*. Chicago, IL: University of Chicago Press.

Table 1 - Descriptive Statistics for Unrestricted Sample

	Full Sample			**Comparison**			**Treatment**			**Control**		
	Obs	Mean	Var	Obs	Mean	Var	Obs	Mean	Var	Obs	Mean	Var
male	959,274	0.390	0.238	791,884	0.398	0.240	36,452	0.359	0.230	96,635	0.353	0.228
white	957,724	0.169	0.140	790,876	0.198	0.159	36,337	0.032	0.031	96,327	0.031	0.030
black	957,724	0.578	0.244	790,876	0.503	0.250	36,337	0.933	0.063	96,327	0.934	0.062
Hispanic	957,724	0.221	0.172	790,876	0.261	0.193	36,337	0.031	0.030	96,327	0.031	0.030
other race	957,724	0.033	0.032	790,876	0.039	0.037	36,337	0.005	0.005	96,327	0.004	0.004
age as of 1997	960,496	25.7	458.5	792,924	26.8	494.1	36,497	20.7	258.5	96,739	20.5	258.0
born after 1997	960,496	0.000	0.000	792,924	0.000	0.000	36,497	0.000	0.000	96,739	0.001	0.001
high school drop-out	257,394	0.509	0.250	212,305	0.517	0.250	9,776	0.466	0.249	26,038	0.474	0.249
high school graduate	257,394	0.353	0.228	212,305	0.348	0.227	9,776	0.380	0.236	26,038	0.376	0.235
some college	257,394	0.117	0.104	212,305	0.112	0.100	9,776	0.141	0.121	26,038	0.139	0.119
college graduate	257,394	0.021	0.020	212,305	0.023	0.022	9,776	0.013	0.012	26,038	0.011	0.010
target case address in Chicago	960,590	0.773	0.176	793,017	0.738	0.193	36,498	0.936	0.060	96,739	0.935	0.061
target case address in Cook County	960,590	0.957	0.041	793,017	0.950	0.048	36,498	0.990	0.010	96,739	0.991	0.009
target case address in Illinois	960,590	0.987	0.013	793,017	0.985	0.015	36,498	0.998	0.002	96,739	0.997	0.003
number of kids < 3 in HH	960,590	0.303	0.299	793,017	0.297	0.291	36,498	0.321	0.322	96,739	0.335	0.338
number of kids 3-5 in HH	960,590	0.401	0.416	793,017	0.387	0.401	36,498	0.463	0.480	96,739	0.474	0.480
number of kids 6-11 in HH	960,590	0.652	0.885	793,017	0.618	0.850	36,498	0.800	0.993	96,739	0.817	1.031
number of kids 12-17 in HH	960,590	0.429	0.625	793,017	0.411	0.610	36,498	0.505	0.680	96,739	0.512	0.685
number of other adults in HH	960,590	0.335	0.462	793,017	0.331	0.449	36,498	0.354	0.517	96,739	0.352	0.533
number of prior violent crime arrests	960,590	0.092	0.290	793,017	0.091	0.296	36,498	0.097	0.281	96,739	0.097	0.260
number of prior property crime arrests	960,590	0.094	0.710	793,017	0.094	0.722	36,498	0.101	0.953	96,739	0.097	0.543
number of prior drug crime arrests	960,590	0.075	0.217	793,017	0.072	0.211	36,498	0.087	0.227	96,739	0.092	0.257
number of prior other crime arrests	960,590	0.114	0.499	793,017	0.112	0.501	36,498	0.122	0.465	96,739	0.120	0.538
any prior violent crime arrests	960,590	0.052	0.050	793,017	0.051	0.048	36,498	0.059	0.055	96,739	0.059	0.056
any prior property crime arrests	960,590	0.042	0.040	793,017	0.041	0.039	36,498	0.047	0.045	96,739	0.047	0.045
any prior drug crime arrests	960,590	0.039	0.038	793,017	0.037	0.036	36,498	0.048	0.045	96,739	0.049	0.047
any prior other crime arrests	960,590	0.056	0.053	793,017	0.055	0.052	36,498	0.064	0.060	96,739	0.062	0.059
other HH members total violent arrests	960,590	0.078	0.221	793,017	0.068	0.192	36,498	0.116	0.345	96,739	0.129	0.392
other HH members total property arrests	960,590	0.045	0.167	793,017	0.041	0.165	36,498	0.053	0.169	96,739	0.061	0.178
other HH members total drug arrests	960,590	0.091	0.345	793,017	0.073	0.265	36,498	0.161	0.534	96,739	0.183	0.776
other HH members total other arrests	960,590	0.110	0.459	793,017	0.094	0.390	36,498	0.179	0.763	96,739	0.190	0.832
receiving Medicaid 1997q2	960,590	0.645	0.229	793,017	0.630	0.233	36,498	0.709	0.206	96,739	0.717	0.203
receiving foodstamps 1997q2	960,590	0.490	0.250	793,017	0.449	0.247	36,498	0.674	0.220	96,739	0.687	0.215
receiving TANF 1997q2	960,590	0.338	0.224	793,017	0.299	0.209	36,498	0.514	0.250	96,739	0.529	0.249
receving Med, fs, or TANF 1997q2	960,590	0.674	0.220	793,017	0.655	0.226	36,498	0.757	0.184	96,739	0.766	0.179
number of quarters on TANF,1994-96	960,590	4.496	25.675	793,017	4.073	24.429	36,498	6.415	26.774	96,739	6.532	26.676
number of quarters on any assistance,1994-96	960,590	8.155	18.034	793,017	8.015	17.776	36,498	8.773	18.881	96,739	8.819	18.759
fraction of tract that lives in public housing	869,246	0.080	0.043	702,850	0.072	0.037	36,248	0.113	0.067	96,069	0.112	0.066
census tract percent minorty	854,131	0.885	0.037	689,165	0.871	0.041	35,963	0.946	0.017	95,196	0.946	0.017
census tract percent black	848,400	0.641	0.173	684,239	0.593	0.183	35,781	0.841	0.078	94,757	0.842	0.078
census tract povery rate	847,923	0.307	0.029	683,799	0.297	0.028	35,757	0.345	0.029	94,718	0.346	0.029
census tract poverty rate <20%	876,627	0.311	0.214	709,898	0.334	0.223	36,307	0.209	0.165	96,268	0.211	0.166
census tract property crime	847,923	84.8	2893.7	683,799	82.3	2993.4	35,757	94.5	2229.0	94,718	95.3	2386.4
census tract violent crime	847,923	20.2	264.1	683,799	19.0	241.4	35,757	25.1	315.6	94,718	25.4	333.0

Notes: The sample is any person receving Foodstamp, Medicaid and/or AFDC/TANF at some point between July 1994 and July 1997, and their household members. The comparison group are those people in the sample that do not appear in our experimental analysis. The control group and treatment groups are those people in the sample that appear in our experimental analysis.

Table 2 - Descriptive Statistics for Adult Restricted Sample

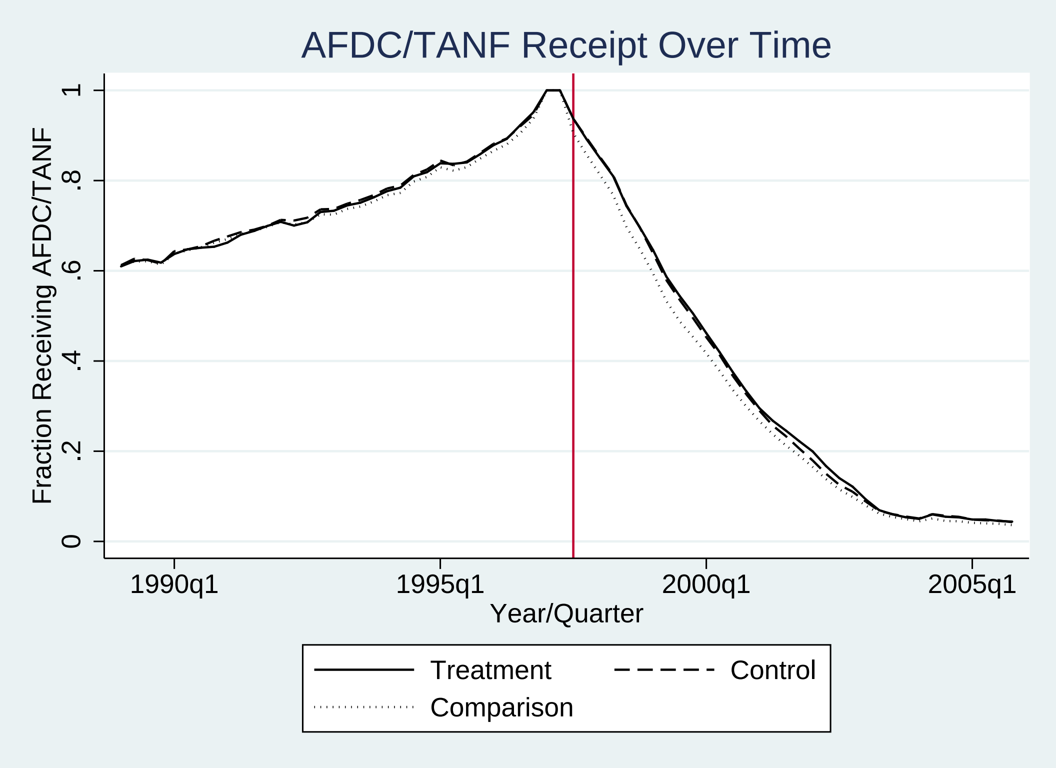
	Full Sample			Comparison			Treatment			Control		
	Obs	Mean	Var	Obs	Mean	Var	Obs	Mean	Var	Obs	Mean	Var
male	56,577	0.000	0.000	33,175	0.000	0.000	5,077	0.000	0.000	13,570	0.000	0.000
white	56,577	0.000	0.000	33,175	0.000	0.000	5,077	0.000	0.000	13,570	0.000	0.000
black	56,577	1.000	0.000	33,175	1.000	0.000	5,077	1.000	0.000	13,570	1.000	0.000
Hispanic	56,577	0.000	0.000	33,175	0.000	0.000	5,077	0.000	0.000	13,570	0.000	0.000
other race	56,577	0.000	0.000	33,175	0.000	0.000	5,077	0.000	0.000	13,570	0.000	0.000
age as of 1997	56,577	30.5	61.2	33,175	31.1	65.1	5,077	29.6	54.3	13,570	29.7	54.2
born after 1997	56,577	0.000	0.000	33,175	0.000	0.000	5,077	0.000	0.000	13,570	0.000	0.000
high school drop-out	51,785	0.510	0.250	30,647	0.512	0.250	4,574	0.498	0.250	12,271	0.506	0.250
high school graduate	51,785	0.368	0.233	30,647	0.363	0.231	4,574	0.382	0.236	12,271	0.375	0.235
some college	51,785	0.115	0.102	30,647	0.117	0.103	4,574	0.115	0.101	12,271	0.112	0.099
college graduate	51,785	0.007	0.007	30,647	0.008	0.008	4,574	0.005	0.005	12,271	0.007	0.007
target case address in Chicago	56,577	1.000	0.000	33,175	1.000	0.000	5,077	1.000	0.000	13,570	1.000	0.000
target case address in Cook County	56,577	1.000	0.000	33,175	1.000	0.000	5,077	1.000	0.000	13,570	1.000	0.000
target case address in Illinois	56,577	1.000	0.000	33,175	1.000	0.000	5,077	1.000	0.000	13,570	1.000	0.000
number of kids < 3 in HH	56,577	0.389	0.350	33,175	0.388	0.349	5,077	0.388	0.348	13,570	0.390	0.352
number of kids 3-5 in HH	56,577	0.481	0.458	33,175	0.469	0.448	5,077	0.499	0.479	13,570	0.505	0.468
number of kids 6-11 in HH	56,577	0.740	0.884	33,175	0.730	0.870	5,077	0.744	0.885	13,570	0.760	0.908
number of kids 12-17 in HH	56,577	0.430	0.560	33,175	0.443	0.567	5,077	0.403	0.539	13,570	0.410	0.545
number of other adults in HH	56,577	0.163	0.225	33,175	0.145	0.193	5,077	0.190	0.264	13,570	0.188	0.268
number of prior violent crime arrests	56,577	0.132	0.225	33,175	0.134	0.225	5,077	0.130	0.200	13,570	0.134	0.254
number of prior property crime arrests	56,577	0.182	1.040	33,175	0.197	1.270	5,077	0.163	0.708	13,570	0.167	0.799
number of prior drug crime arrests	56,577	0.110	0.212	33,175	0.121	0.240	5,077	0.090	0.172	13,570	0.097	0.180
number of prior other crime arrests	56,577	0.162	0.613	33,175	0.178	0.548	5,077	0.126	0.275	13,570	0.150	1.023
any prior violent crime arrests	56,577	0.099	0.089	33,175	0.100	0.090	5,077	0.100	0.090	13,570	0.099	0.089
any prior property crime arrests	56,577	0.093	0.084	33,175	0.096	0.087	5,077	0.090	0.082	13,570	0.090	0.082
any prior drug crime arrests	56,577	0.074	0.069	33,175	0.080	0.074	5,077	0.062	0.058	13,570	0.068	0.063
any prior other crime arrests	56,577	0.103	0.092	33,175	0.109	0.097	5,077	0.088	0.080	13,570	0.096	0.087
other HH members total violent arrests	56,577	0.073	0.205	33,175	0.067	0.183	5,077	0.079	0.266	13,570	0.085	0.252
other HH members total property arrests	56,577	0.036	0.146	33,175	0.035	0.172	5,077	0.030	0.103	13,570	0.039	0.107
other HH members total drug arrests	56,577	0.108	0.417	33,175	0.099	0.371	5,077	0.102	0.308	13,570	0.126	0.531
other HH members total other arrests	56,577	0.105	0.454	33,175	0.097	0.429	5,077	0.111	0.561	13,570	0.119	0.487
receiving Medicaid 1997q2	56,577	1.000	0.000	33,175	1.000	0.000	5,077	1.000	0.000	13,570	1.000	0.000
receiving foodstamps 1997q2	56,577	0.979	0.021	33,175	0.975	0.024	5,077	0.983	0.017	13,570	0.984	0.016
receiving TANF 1997q2	56,577	1.000	0.000	33,175	1.000	0.000	5,077	1.000	0.000	13,570	1.000	0.000
receving Med, fs, or TANF 1997q2	56,577	1.000	0.000	33,175	1.000	0.000	5,077	1.000	0.000	13,570	1.000	0.000
number of quarters on TANF,1994-96	56,577	10.128	10.468	33,175	10.065	11.003	5,077	10.208	9.700	13,570	10.231	9.675
number of quarters on any assistance,1994-96	56,577	10.917	6.405	33,175	10.837	7.027	5,077	11.037	5.496	13,570	11.022	5.578
fraction of tract that lives in public housing	56,541	0.126	0.076	33,141	0.123	0.073	5,077	0.129	0.078	13,570	0.132	0.079
census tract percent minorty	56,522	0.962	0.010	33,126	0.960	0.011	5,076	0.965	0.009	13,569	0.965	0.009
census tract percent black	56,513	0.882	0.052	33,119	0.876	0.056	5,075	0.889	0.046	13,568	0.889	0.047
census tract povery rate	56,513	0.353	0.029	33,119	0.347	0.028	5,075	0.359	0.030	13,568	0.363	0.030
census tract poverty rate <20%	56,539	0.193	0.156	33,138	0.204	0.162	5,076	0.179	0.147	13,570	0.178	0.147
census tract property crime	56,513	97.2	2323.2	33,119	96.6	2276.3	5,075	97.5	2312.9	13,568	98.1	2360.8
census tract violent crime	56,513	26.3	320.6	33,119	26.0	310.3	5,075	26.6	331.6	13,568	26.9	335.7

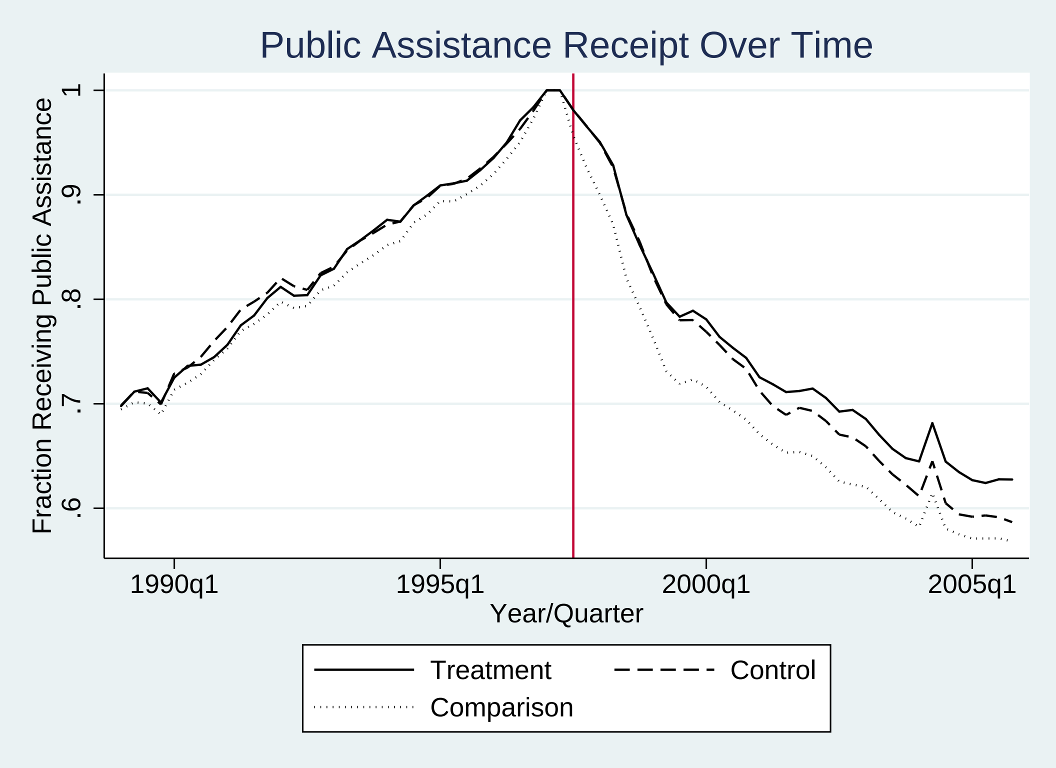
Notes: The sample is 18-65 year old black females, who are Foodstamp, Medicaid and/or AFD/TANF grantees at some point between July 1994 and July 1997, who's most recent address while they were a grantee ("target case address") was in Chicago, and who were receiving AFDC/TANF in the first and second quarters of 1997. The comparison group are those people in the sample that do not appear in our experimental analysis. The control group and treatment groups are those people in the sample that appear in our experimental analysis.

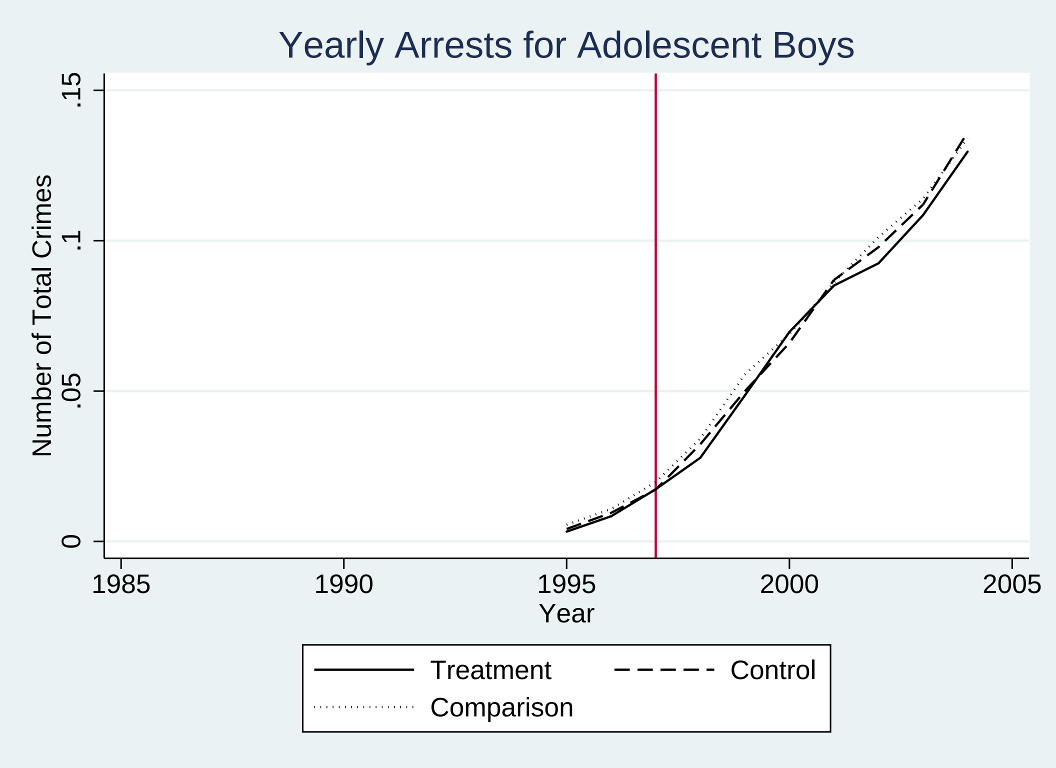
Table 3 - Descriptive Statistics for Child Restricted Sample

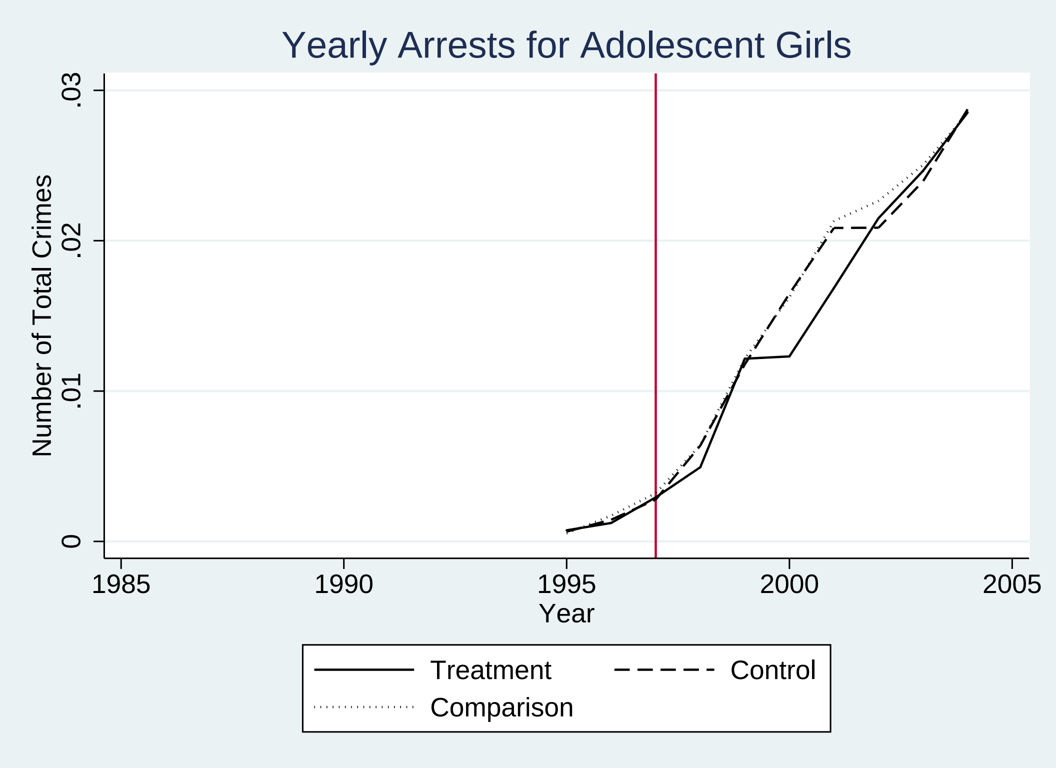
	Full Sample			Comparison			Treatment			Control		
	Obs	Mean	Var	Obs	Mean	Var	Obs	Mean	Var	Obs	Mean	Var
male	115,455	0.496	0.250	67,359	0.494	0.250	10,329	0.507	0.250	28,018	0.496	0.250
white	115,453	0.000	0.000	67,359	0.000	0.000	10,329	0.000	0.000	28,017	0.000	0.000
black	115,453	0.999	0.001	67,359	0.999	0.001	10,329	1.000	0.000	28,017	1.000	0.000
Hispanic	115,453	0.000	0.000	67,359	0.000	0.000	10,329	0.000	0.000	28,017	0.000	0.000
other race	115,453	0.000	0.000	67,359	0.000	0.000	10,329	0.000	0.000	28,017	0.000	0.000
age as of 1997	115,455	7.6	22.3	67,359	7.7	22.8	10,329	7.5	21.6	28,018	7.5	21.6
born after 1997	115,455	0.001	0.001	67,359	0.001	0.001	10,329	0.001	0.001	28,018	0.001	0.001
high school drop-out	2,080	0.950	0.047	1,280	0.944	0.053	167	0.958	0.040	467	0.959	0.039
high school graduate	2,080	0.046	0.044	1,280	0.052	0.050	167	0.036	0.035	467	0.039	0.037
some college	2,080	0.003	0.003	1,280	0.003	0.003	167	0.006	0.006	467	0.002	0.002
college graduate	2,080	0.000	0.000	1,280	0.001	0.001	167	0.000	0.000	467	0.000	0.000
target case address in Chicago	115,455	1.000	0.000	67,359	1.000	0.000	10,329	1.000	0.000	28,018	1.000	0.000
target case address in Cook County	115,455	1.000	0.000	67,359	1.000	0.000	10,329	1.000	0.000	28,018	1.000	0.000
target case address in Illinois	115,455	1.000	0.000	67,359	1.000	0.000	10,329	1.000	0.000	28,018	1.000	0.000
number of kids < 3 in HH	115,455	0.518	0.457	67,359	0.516	0.457	10,329	0.517	0.451	28,018	0.522	0.457
number of kids 3-5 in HH	115,455	0.723	0.621	67,359	0.703	0.611	10,329	0.751	0.651	28,018	0.757	0.621
number of kids 6-11 in HH	115,455	1.215	1.254	67,359	1.195	1.250	10,329	1.220	1.227	28,018	1.254	1.260
number of kids 12-17 in HH	115,455	0.659	0.865	67,359	0.670	0.876	10,329	0.628	0.833	28,018	0.645	0.851
number of other adults in HH	115,455	0.178	0.249	67,359	0.157	0.211	10,329	0.208	0.297	28,018	0.212	0.308
number of prior violent crime arrests	115,455	0.005	0.010	67,359	0.006	0.012	10,329	0.005	0.006	28,018	0.005	0.005
number of prior property crime arrests	115,455	0.003	0.005	67,359	0.003	0.004	10,329	0.002	0.003	28,018	0.003	0.008
number of prior drug crime arrests	115,455	0.009	0.018	67,359	0.009	0.019	10,329	0.006	0.010	28,018	0.008	0.017
number of prior other crime arrests	115,455	0.004	0.006	67,359	0.004	0.007	10,329	0.004	0.006	28,018	0.004	0.006
any prior violent crime arrests	115,455	0.005	0.005	67,359	0.005	0.005	10,329	0.005	0.005	28,018	0.004	0.004
any prior property crime arrests	115,455	0.002	0.002	67,359	0.002	0.002	10,329	0.001	0.001	28,018	0.002	0.002
any prior drug crime arrests	115,455	0.006	0.006	67,359	0.006	0.006	10,329	0.005	0.005	28,018	0.005	0.005
any prior other crime arrests	115,455	0.003	0.003	67,359	0.003	0.003	10,329	0.003	0.003	28,018	0.003	0.003
other HH members total violent arrests	115,455	0.087	0.251	67,359	0.082	0.230	10,329	0.089	0.323	28,018	0.101	0.290
other HH members total property arrests	115,455	0.043	0.155	67,359	0.041	0.169	10,329	0.038	0.156	28,018	0.047	0.122
other HH members total drug arrests	115,455	0.127	0.490	67,359	0.114	0.421	10,329	0.120	0.368	28,018	0.156	0.647
other HH members total other arrests	115,455	0.117	0.474	67,359	0.107	0.435	10,329	0.122	0.596	28,018	0.135	0.526
receiving Medicaid 1997q2	115,455	0.967	0.032	67,359	0.967	0.032	10,329	0.964	0.034	28,018	0.967	0.032
receiving foodstamps 1997q2	115,455	0.942	0.054	67,359	0.939	0.057	10,329	0.943	0.054	28,018	0.948	0.049
receiving TANF 1997q2	115,455	0.917	0.076	67,359	0.917	0.076	10,329	0.912	0.080	28,018	0.919	0.074
receving Med, fs, or TANF 1997q2	115,455	0.970	0.029	67,359	0.971	0.029	10,329	0.969	0.030	28,018	0.970	0.029
number of quarters on TANF,1994-96	115,455	9.333	16.336	67,359	9.278	16.630	10,329	9.350	16.183	28,018	9.437	15.821
number of quarters on any assistance,1994-96	115,455	9.954	12.962	67,359	9.891	13.342	10,329	10.050	12.304	28,018	10.038	12.507
fraction of tract that lives in public housing	115,406	0.140	0.084	67,311	0.134	0.081	10,329	0.148	0.090	28,018	0.145	0.087
census tract percent minorty	115,374	0.965	0.009	67,282	0.964	0.010	10,329	0.966	0.008	28,018	0.967	0.008
census tract percent black	115,359	0.883	0.052	67,272	0.878	0.055	10,326	0.889	0.046	28,016	0.890	0.047
census tract povery rate	115,359	0.365	0.030	67,272	0.357	0.029	10,326	0.373	0.031	28,016	0.375	0.031
census tract poverty rate <20%	115,400	0.174	0.144	67,304	0.187	0.152	10,329	0.158	0.133	28,018	0.157	0.133
census tract property crime	115,359	98.3	2480.6	67,272	97.5	2430.4	10,326	98.5	2436.5	28,016	99.4	2516.8
census tract violent crime	115,359	27.3	352.8	67,272	26.8	341.3	10,326	27.5	359.0	28,016	27.9	371.4

Notes: The sample is all children age 0-17 living in a houshold of a member of the adult sample from Table 2. The comparison group are those people in the sample that do not appear in our experimental analysis. The control group and treatment groups are those people in the sample that appear in our experimental analysis.

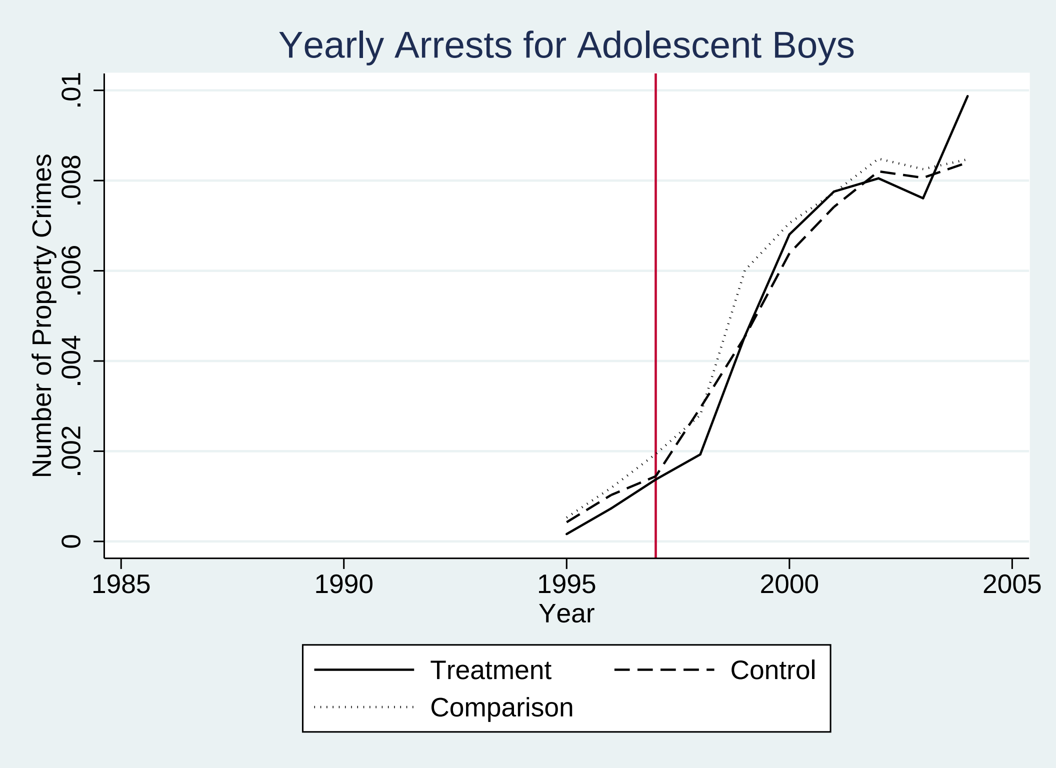

AFDC/TANF Receipt Over Time
Fraction Receiving AFDC/TANF
0
.2
.4
.6
.8
1
1990q1
1995q1
2000q1
2005q1
Year/Quarter
Treatment
Control
Comparison


Public Assistance Receipt Over Time
Fraction Receiving Public Assistance
1
.9
.8
.7
.6
1990q1
1995q1
2000q1
2005q1
Year/Quarter
Treatment
Control
Comparison

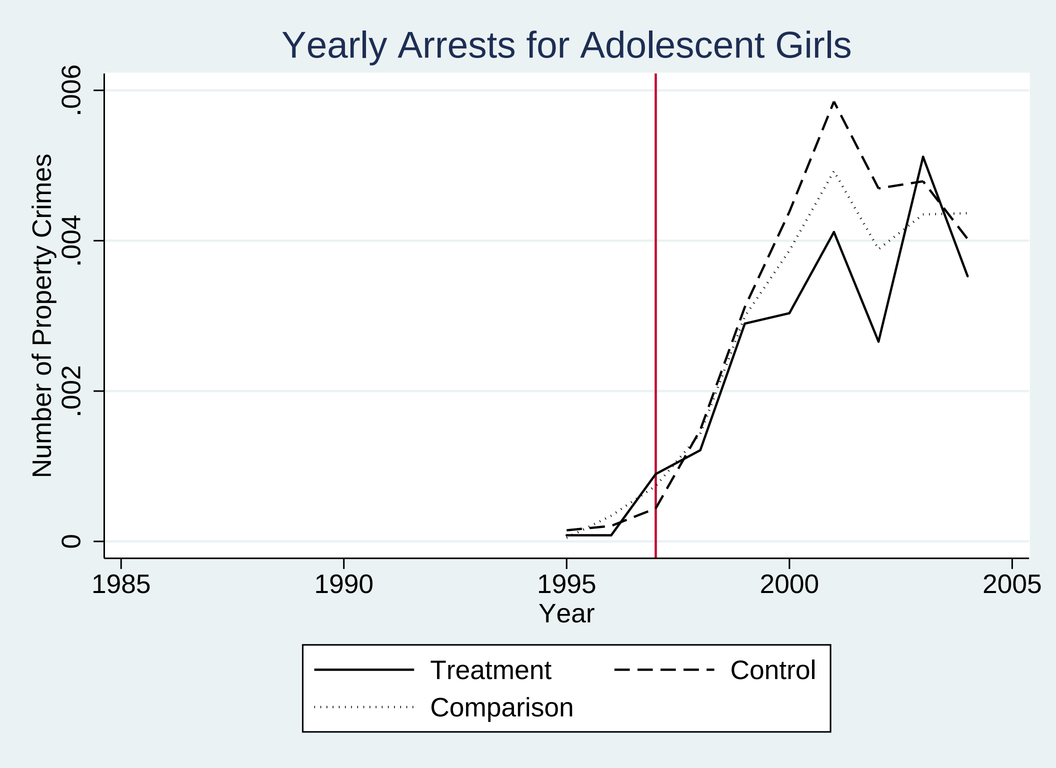

Yearly Arrests for Adolescent Boys
Number of Total Crimes
0
.05
.1
.15
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison

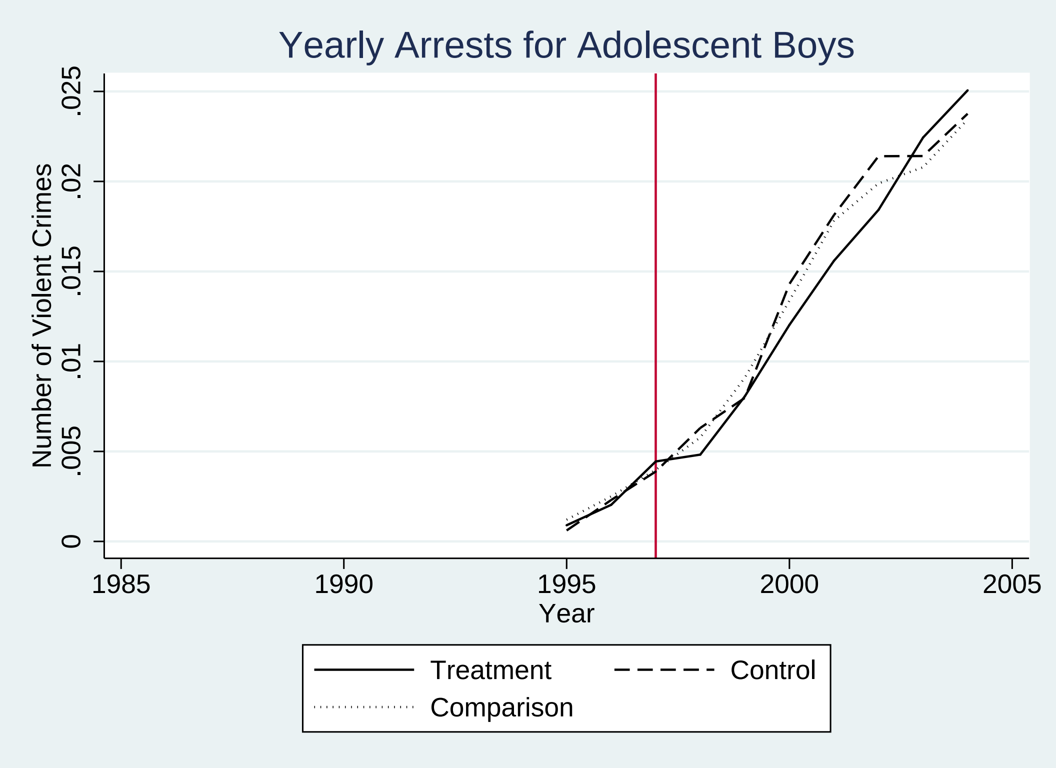


Yearly Arrests for Adolescent Girls
Number of Total Crimes
0
.01
.02
.03
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison

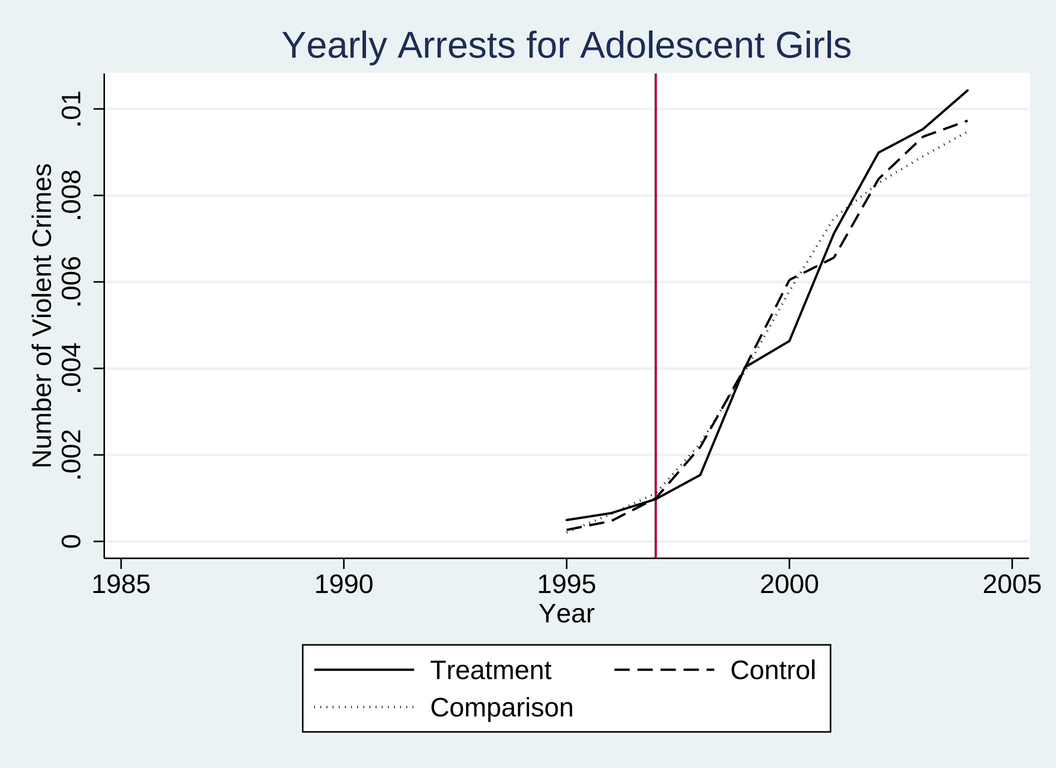


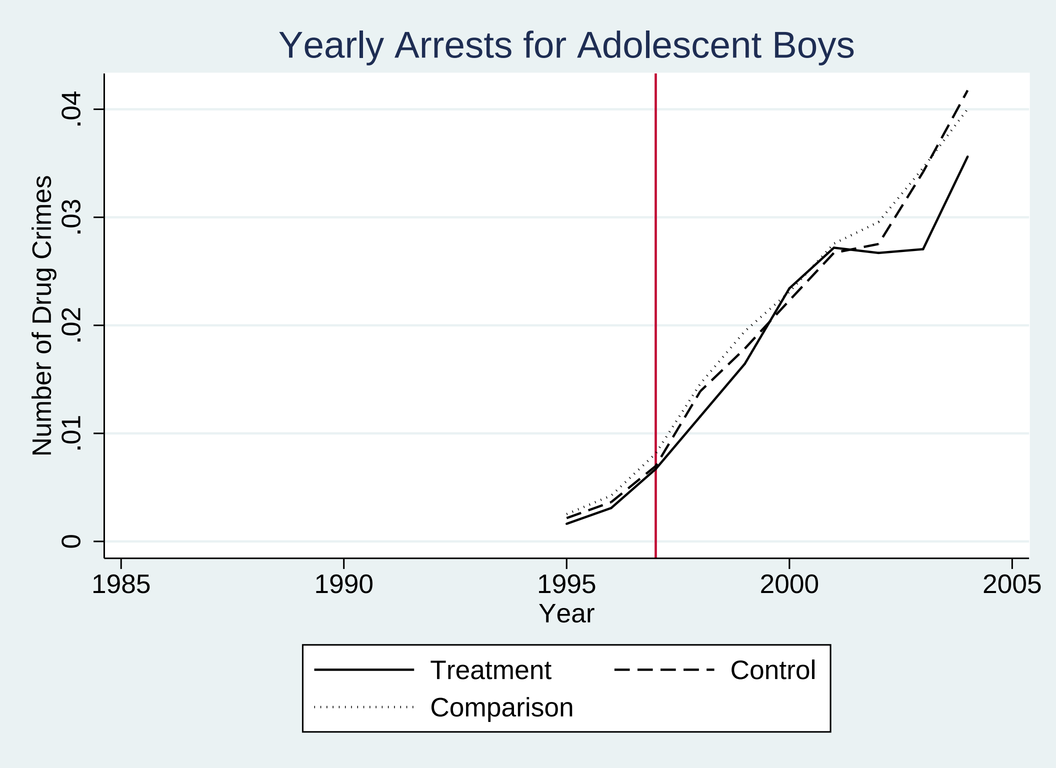
Yearly Arrests for Adolescent Boys
Number of Property Crimes
0
.002
.004
.006
.008
.01
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison


Yearly Arrests for Adolescent Girls
Number of Property Crimes
0
.002
.004
.006
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison

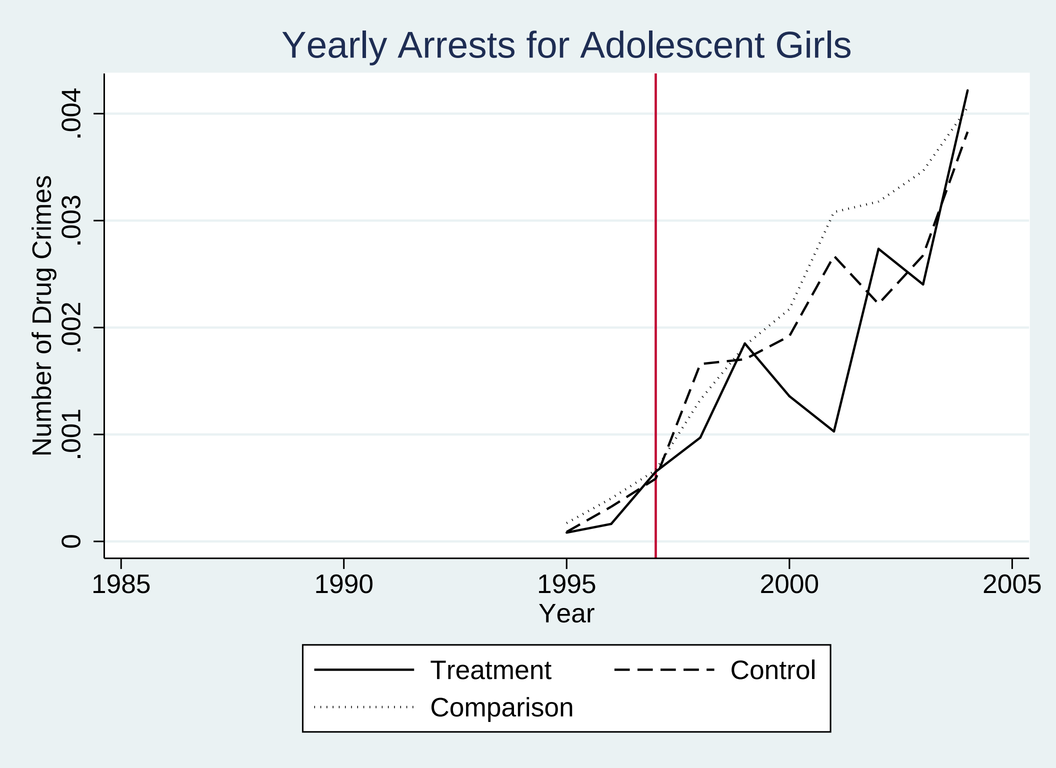


Yearly Arrests for Adolescent Boys
Number of Violent Crimes
0
.005
.01
.015
.02
.025
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison

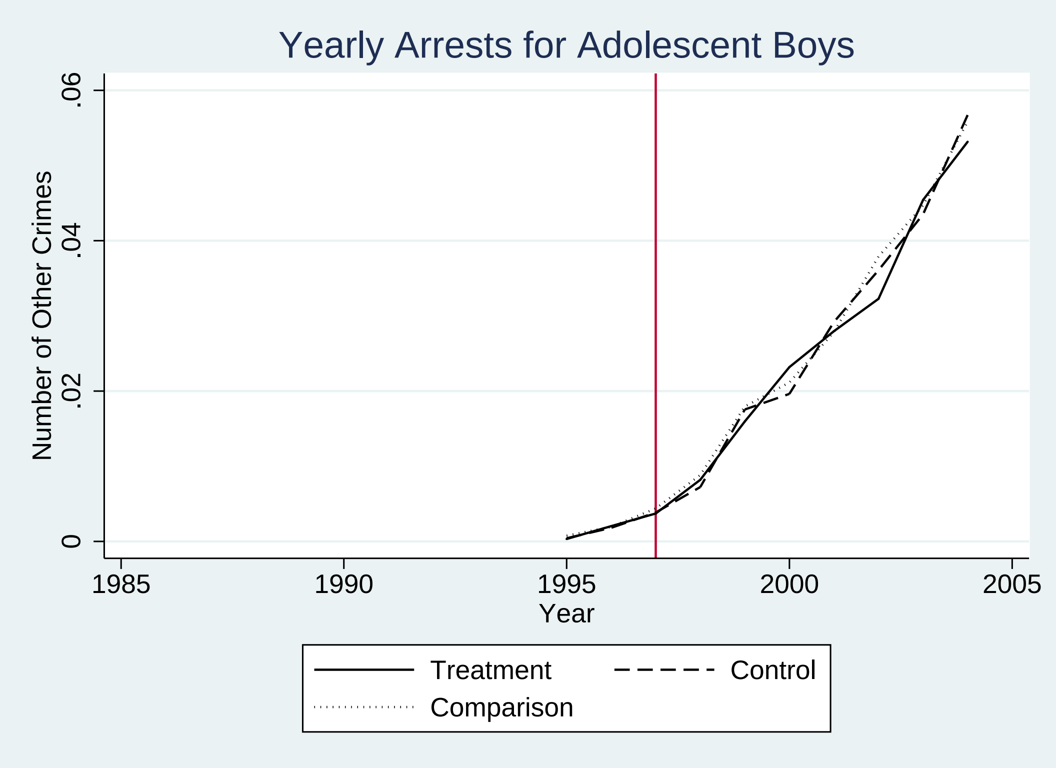

Yearly Arrests for Adolescent Girls
Number of Violent Crimes
0
.002
.004
.006
.008
.01
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison

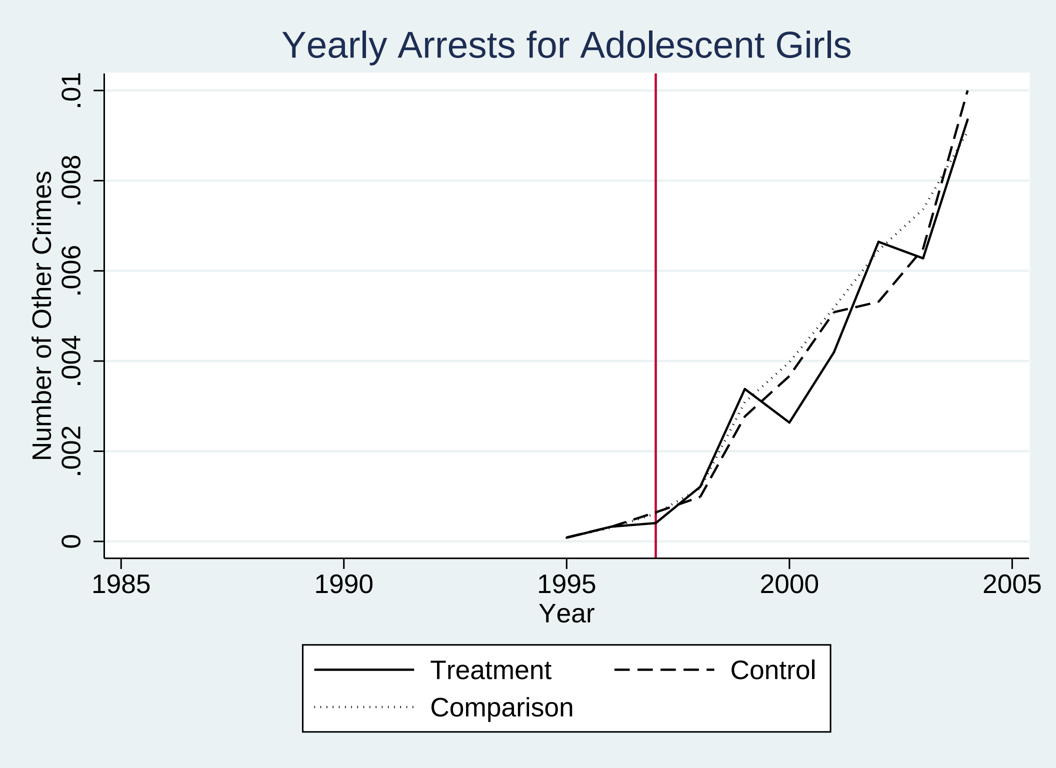


Yearly Arrests for Adolescent Boys
Number of Drug Crimes
0
.01
.02
.03
.04
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison



Yearly Arrests for Adolescent Girls
Number of Drug Crimes
0
.001
.002
.003
.004
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison


Yearly Arrests for Adolescent Boys
Number of Other Crimes
0
.02
.04
.06
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison



Yearly Arrests for Adolescent Girls
Number of Other Crimes
0
.002
.004
.006
.008
.01
1985
1990
1995
2000
2005
Year
Treatment
Control
Comparison