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Are Neighborhood Effects Nonlinear? Estimates from the MTO Experiment*

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1. Introduction

Several important social science and public policy literatures hinge on the functional relationship between neighborhood and other social or environmental characteristics and individual outcomes. Sociological threshold models suggest that individual behaviors may change dramatically when the percentage of the population engaging in a behavior reaches a threshold level (Granovetter, 1978). Such a model underlies Wilson's (1987) theory of the black underclass. In Wilson's model, the deindustrialization of urban centers led to a concentration of joblessness and poverty; once the concentration of poverty reached a sufficient level, pathological behaviors arose. In the literature that derived from Wilson's work, a census tract poverty rate of 40 percent is often seen as the threshold that produces high levels of drug use, out-of-wedlock-births, high-school drop outs and welfare dependency. However, empirical evidence of such threshold effects is relatively sparse.[1]

Economic models of individuals sorting across neighborhoods, schools, and classrooms often find that inefficient equilibria can arise. In models in which an individual's outcome depends on the characteristics of his or her neighbors, this inefficiency generally arises because individuals do not take their external effect on their neighbors into account in deciding where to live. The existence and extent of these externalities depend on the relationship between peer group characteristics and individual outcomes (Henderson et al, 1978; Arnott and Rowse, 1987; de Bartolome, 1990; Fernandez and Rogerson, 1996; Benabou, 1993).

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[1] Crane (1991) finds that teenage child bearing and high-school dropout rates rise dramatically once the share of workers in the neighborhood who hold professional or managerial jobs falls below about 10 percent. A recent survey by Galster (2002) finds only a handful of more recent studies and concludes that "the empirical evidence ... is not only thin but arguably suffers from methodological shortcomings."

The recent econometric literature on the identification of social interactions and social multipliers (Manski 1993, 2000; Brock and Durlauf 2001a, 2001b; Moffitt, 2001; Glaeser, Sacerdote, and Scheinkman, 2002) has emphasized the distinction between exogenous and endogenous social interactions. Exogenous social interactions ("contextual interactions" in Manski's typology) are ones in which the characteristics of one's group or environment affects a person's outcomes, but there is no feedback between the individual's outcomes and the characteristics of the group or environment on which the outcomes depend. Endogenous interactions are ones in which the individual outcomes feed back into the group and neighborhood characteristics on which the individual outcomes depend, producing multiplier effects. Manski (1993) shows that in a standard linear regression model in which individual behavior varies linearly with mean behavior, it is not possible to distinguish between exogenous and endogenous social interactions. The more recent literature has highlighted conditions in which identification of endogenous interactions is possible. In particular, if the relationship between group mean behavior and individual behavior is nonlinear, and the specific nonlinear relationship is known, then identification is possible.

These theoretical considerations potentially have important policy ramifications as well. Should housing policy aim to reduce the concentration of poverty in urban neighborhoods? Should schools track students based on ability? Answering these sorts of questions depend on knowing the shape of the potentially nonlinear relationships between neighborhoods and peers and individual outcomes.[2]

[2] Note that a government role in financing or providing housing for low-income populations can be justified either through sorting models in which the market equilibrium is inefficient or through models in which low-income individuals are unaware of or underweight the benefits they or their children would accrue from better housing (Aaron, 1972).

Despite the broad relevance of the topic, there is essentially no convincing evidence on the functional form of the relationship between neighborhood characteristics and individual outcomes. In large part, this lacuna stems from the difficulty that arises in reliably demonstrating any impact of neighborhoods on individual outcomes using observational data. Because individuals self-select into neighborhoods, it is likely that individuals who appear to be observably equivalent in standard data sets differ on unobserved characteristics in ways that are correlated with the outcomes. In practice, estimates of neighborhood effects are notoriously sensitive to which individual and family background characteristics are included in the regression specification and models that include a larger number of background characteristics tend to find smaller (and often zero) neighborhood effects (Duncan and Raudenbush 2001). Moreover, it is hard to know what neighborhood characteristics matter for a given outcome and in practice researchers are often limited to the neighborhood measures and outcomes available at the Census tract level from the decennial Census of Population. Thus, findings of small or zero neighborhood effects are not reliable. Finally, in standard data sets, there is often limited variation in neighborhood types for people with a given set of background characteristics, either resulting in very small sample sizes or forcing the researcher to assume that the model fits well enough to extrapolate across people of widely different types.

This paper uses data from the interim (five-year) evaluation of HUD's Moving to Opportunity (MTO) demonstration to assess the extent of nonlinearities in relationships between neighborhood characteristics and individual outcomes. In this randomized experiment, 4600 families living in high poverty public housing projects in five cities were randomized into three groups: a control group in which families continued to be eligible to live in public housing and two treatment groups. In the first treatment group, the Section 8 group, families received a geographically unrestricted Section 8 voucher that could be used to rent an apartment in any neighborhood so long as it met the regular Section 8 rules. In the second treatment group, the Experimental group, families received restricted vouchers that could only be used to rent an apartment in a low-poverty neighborhood.

The MTO experiment offers several advantages over the non-experimental approaches to measuring the impact of neighborhood effects. First, the experiment eliminates the selection problem. Random assignment should produce populations in the three experimental groups who are balanced both on observable and unobservable characteristics. Any differences in outcomes that are observed across the groups can therefore be attributed to the different treatments the groups received. Second, the MTO intervention is a very large one and introduces much wider variation in neighborhood characteristics for a relatively homogenous population than would typically arises through the ordinary process of people choosing where to live. This wide variation in neighborhoods – ranging from some of the highest poverty, highest crime neighborhoods in the U.S. to very low poverty neighborhoods – should provide the ideal environment to evaluate whether neighborhoods due affect individual outcomes. In short, if we do not observe important neighborhood effects from MTO, it is still possible that they exist, but it seems very unlikely that any other research platform will be able convincingly to demonstrate their existence. It is also worth emphasizing that even for non-experimental analysis, the relative homogeneity of the population and the wide range of neighborhood locations induced by the experiment makes the MTO research platform quite valuable.

In the current draft of this paper we take the two polar analytical approaches to estimating the relationship between neighborhood characteristics and individual outcomes using the MTO data. The first relies solely on experimental variation to identify this relationship. We begin by presenting the experimental estimates of treatment effects for the Experimental and Section 8 groups and discuss what the pattern of outcomes across the two groups suggests about the relationship between neighborhood poverty and individual outcomes. We then treat each of the two experiments in each of the five sites as a separate experiment, creating a total of 10 experiments. We instrument for polynomial functions of the neighborhood poverty rate (using interactions of site dummies and treatment group dummies as the instruments) to trace out the potentially non-linear relationship between census tract poverty and individual outcomes.

The second analytical approach ignores the experimental variation and estimates the impact of census tract poverty on individual outcomes assuming that assignment to census tracts is conditionally random. This provides a useful comparison with the standard approach in observational data and helps to highlight the sources of discrepancies between experimental and nonexperimental estimates of neighborhood effects. We are currently working on a third approach that more flexibly models the choice of neighborhood poverty level as a function of individual characteristics and the treatment assignment and then estimates the outcomes conditional on this choice.

Section 2 describes the MTO setting and experimental design in more detail. Section 3 presents our econometric framework. Section 4 presents our core results using the MTO experimental design to estimate the effects of neighborhood poverty levels on health, risky behavior, and employment outcomes. Section 5 presents the results using nonexperimental variation in residential locations. Section 6 concludes.

2. The MTO Demonstration and Mobility Outcomes

The MTO demonstration has been operating in five cities — Baltimore, Boston, Chicago, Los Angeles, and New York — since the fall of 1994.[3] Families were eligible for participation in the demonstration if they had children and resided in public housing or project-based Section 8 assisted housing in census tracts with a 1990 poverty rate of 40 percent or more. Families were randomly assigned into the demonstration from the fall of 1994 through 1997.

Interested eligible families who completed an application and survey were then selected from a waiting list and randomly assigned to one of three program groups: the Experimental group, the Section 8 group, and the Control group. Families in the Experimental group received a restricted housing voucher that could be used to help pay for rental housing from private landlords, but only in a low-poverty area (a census tract with under a 10 percent poverty rate in 1990). The Experimental group families also received counseling assistance from a local nonprofit organization to help them search for an apartment and adjust to a new neighborhood. Section 8 Comparison group members received a geographically unrestricted housing voucher and no counseling assistance. The Control group families did not receive rental assistance vouchers, although their eligibility for continued project-based assistance was unaffected. The Experimental and Section 8 Comparison group members were given four to six months to submit a request for approval of an eligible apartment they wanted to lease using a housing voucher, and

[3] See Orr et al. (2003) and Goering and Feins (2003) for background on the MTO social experiment.

the apartment then had to pass a quality inspection.

The participants at all five MTO sites are largely female-headed minority households. In fact, 92 percent of the MTO housholds had a female head at baseline (the time of random assignment), 66 percent of heads are African- American, and over one-third are Hispanic. There is some site heterogeneity in the racial and ethnic make-up of MTO families. The participants in Baltimore and Chicago are almost entirely African-American. The participants in Boston, New York, and Los Angeles are more ethnically diverse with over 40 percent Hispanics. The median MTO household had 3 children and listed public assistance payments (AFDC) as the primary income source at baseline. The vast majority (over 70 percent) of the household heads were not employed at baseline and most had limited education (less than a high school degree).
At the time of program enrollment, the main reason a majority of the families wanted to move out of public housing was fear of crime (to get away from drugs and gangs). These patterns are not surprising given program eligibility was limited to families with children living in some of the highest-poverty, inner city poverty tracts at each of the five sites.

The MTO program has had a substantial impact on the residential locations of households offered vouchers to live in a private market apartment in both the Experimental and Section 8 groups. The compliance rate – share of families able to lease up and make a program move using a housing voucher – was 63 percent for the Section 8 group and 48 percent for the Experimental group. Mobility through program moves using housing vouchers initially led the Experimental compliers (those making program moves) into low-poverty neighborhoods. A substantial fraction of the Experimental and Section 8 compliers subsequently moved again after their initial program move. The majority of treatment group noncompliers also have moved since random assignment. And most (about 70 percent) of the Control group families moved out of their baseline public housing units by the time of the interim evaluation survey.

The results in this paper are based on the interim evaluation which surveyed MTO families during 2002, five to eight years after random assignment. The overall and site-level distributions of neighborhood poverty (measured at the census tract level using poverty rate data from the 2000 Census) of the MTO families at the time of the interim evaluation survey are summarized in Table I. Table I also gives the compliance rates by treatment group and site. Figure 1 provides further details on the neighborhood poverty distributions by complier status for the treatment groups. The MTO demonstration succeeded in moving Experimental and Section 8 families into lower poverty neighborhoods on average than those in which members of the Control group resided. The mean neighborhood poverty rate is about 8 percentage points lower for the Experimental group and 6 percentage points lower for the Section 8 group than the Control group in 2002. Experimental group families are also much more likely to be living in low-poverty areas (census tracts with under a 12 percent poverty rate in 2000). Figure 1 shows that the differences in neighborhood poverty distributions of the treatment groups and the Control group are driven by those making program moves (the compliers). Indeed, the distributions for the control group as a whole and for treatment group noncompliers are quite similar. The distributions of neighborhood poverty differ for the Experimental and Section 8 groups and the treatment group distributions also vary considerably across the five sites. Thus, substantial experimental variation in the distributions of neighborhood poverty by treatment group is available to potentially estimate the impacts of exogenous variation in neighborhood characteristics on the outcomes of low-income adults and children from disadvantaged families.

3. Econometric Framework

Theories of neighborhood effects posit a relationship between neighborhood characteristics (such as neighborhood poverty or school quality) and child and adult outcomes. Residential neighborhoods may affect the human capital accumulation, health status, participation in risky activities (crime and drugs), and eventual labor market outcomes of children and youth through community resources, peer influences, and adult influences (Jencks and Mayer 1990). Adult economic self-sufficiency and health status also may be affected by residential access to labor market opportunities and community norms.

We explore the empirical importance of neighborhood effects on a range of socioeconomic and health outcomes for a sample of individuals (indexed by i) living in five major metropolitan areas (sites indexed by j). In a simple regression framework, the most direct test of theories of neighborhood effects would be to examine the coefficient vector (γ) in a regression of the outcome of interest (Y) on a set of observed neighborhood characteristics (W), conditioning on controls for individual background variables (X) and for metropolitan area fixed effects (δ_j):

(1) $$Y_{ij} = X_{ij}\beta + W_{ij}\gamma + \delta_j + \varepsilon_{ij}$$

There are several reasons why estimates of equation (1) on standard nonexperimental data sets are unlikely to provide convincing estimates of the causal effects of neighborhood attributes on outcomes. First, the selection problem arising from the systematic sorting of individuals across residential neighborhoods on the basis of important unobserved determinants of outcomes may lead to severely biased estimates. Second, it is difficult to specify and measure the appropriate neighborhood characteristics. In particular, the neighborhood variables in standard data sets typically measured at the zip code, census tract, or census block level may not correspond to the relevant neighborhood concept. Third, the functional form of the relationships between neighborhood attributes such as neighborhood poverty and outcomes is unknown and, according to some theories, may be highly nonlinear.

To estimate the causal effect of residential location on an outcome of interest, we must compare people living in different locations who would have experienced the same outcome, on average, if they had lived in the same location. The MTO demonstration provides a solution to this problem for a sample of families living in public housing units in high-poverty urban areas by randomly assigning assistance to move to lower-poverty neighborhoods. The comparison of average outcomes of the MTO treatment group families with the Control group families provides a causal estimate of the impact of the opportunity to move to wealthier neighborhoods. The exogenously induced differences in residential neighborhoods of the treatment groups and Control group provide a solution to the first problem in estimating equation (1) of the non-random selection of households into different neighborhoods.

More formally, let Z^e be an indicator variable for being randomly assigned to the MTO Experimental group and let Z^s be an indicator variable for being randomly assigned to the Section 8 group. The differences in outcomes of the treatment groups and the control group are known as "Intent-To-Treat" (ITT) effects and can be captured by the OLS estimates of the coefficients on the treatment group indicator variables in the following regression:

(2) $$Y_{ij} = X_{ij}\beta_1 + Z^e_{ij}\alpha^e + Z^s_{ij}\alpha^s + \delta_j + \varepsilon_{ij} ,$$

where only characteristics known prior to randomization are included in the vector of background characteristics (X) and where site dummies are included since randomization was separately done by site. Characteristics known prior to randomization should have the same distributions within the treatment and control groups because they are statistically independent of group assignment. Thus, including them in a regression like (2) will not affect the coefficients on the treatment group indicators (unless X happens to differ between groups due to variability in a small sample) but will improve the precision of the treatment estimates if they are related to Y and thereby reduce residual variance in the regression.

The ITT estimates from equation (2) measure the average causal effect of moves to neighborhoods with entire bundles of characteristics that differ from the residential neighborhoods of the control group households. Further assumptions are needed to use the experimental variation in residential neighborhoods from MTO to estimate the effects of the specific neighborhood characteristics (W) of equation (1). In the preliminary analysis of the current draft of this paper, we follow a large literature (e.g., Wilson 1987; Jargowsky and Bane 1990; Jargowsky 1997) in assuming that the relevant neighborhood construct in the Census tract and that the relevant measure of neighborhood quality is the neighborhood poverty rate (P).[4] The neighborhood poverty rate has also attracted much policy attention and theoretical interest in models of possible nonlinear and threshold effects of neighborhood quality on outcomes.

We attempt to use the experimental variation in the distribution of neighborhood poverty across the MTO treatment groups to identify the relationship between neighborhood poverty and outcomes of interest:

(3) $$Y_{ij} = X_{ij}\beta + f(P_{ij}) + \delta_j + \varepsilon_{ij} ,$$

[4]We will explore different neighborhood characteristics and neighborhood constructs in future drafts of this paper.

where f( ) is an unknown and possibly nonlinear function. Our experimental analysis of (3) is based on the strong assumption that differences in outcomes among the three MTO treatment groups at each of the five MTO sites are explained by the differences in the neighborhood poverty distributions among the groups summarized in Table I. We estimate equation (3) by instrumental variables making some parametric assumption about f() and using the 10 treatment group*site interaction dummy variables as the excluded instruments to identify the neighborhood poverty function. We begin by assuming a linear relationship between outcomes and poverty, and then we explore possible nonlinearities by estimating polynomials (quadratics and quartics) and exploring linear spline functions with various breakpoints and numbers of segments.

Our identification strategy essentially attempts to explain the mean outcomes of 15 different groups (3 random assignment groups times five sites) as a function of a full set of site dummies and variables measuring the distribution of neighborhood poverty of the members of each group. Alternatively, the approach can viewed as relating the 10 different site-level treatment effects (ITT's) to the differences in the underlying poverty distributions of each treatment group from the control group at its site. The site-level Experimental ITT's (α^e_j) and Section 8 ITT's (α^s_j) from the following regression are the key inputs to be explained:

(4) $$Y_{ij} = X_{ij}\beta_3 + \Sigma_j Z^e_{ij} d_{ij} \alpha^e_j + \Sigma_j Z^s_{ij} d_{ij} \alpha^s_j + \delta_j + \varepsilon_{ij} ,$$

where d_{ij} is an indicator variable for baseline residence at site j.

Under the assumption that $f(P_{ij})$ in equation (3) is linear ($f(P_{ij}) = P_{ij}\lambda$ where λ is a scalar unknown parameter), the instrumental variables estimation of (3) using site*treatment group dummies as instruments implies a first-stage regression of the form:

(5) $$P_{ij} = X_{ij}\theta + \Sigma_j Z^e_{ij} d_{ij} \pi^e_j + \Sigma_j Z^s_{ij} d_{ij} \pi^s_j + \delta_j + \nu_{ij} .$$

The coefficients π^e_j and π^s_j represent the ITT effects of the Experimental and Section 8 treatments on neighborhood poverty. In the linear case, the IV estimate of λ (the effect of neighborhood poverty on Y) simply reflects the (appropriately weighted) linear relationship between the 10 site-level ITT estimates from the reduced form equation (4) and the 10 site-level ITT estimates for neighborhood poverty from the first-stage regression (5).

Figure 2 illustrates this relationship for adult depression, a key mental health indicator. The figure shows ten data points, one for each treatment group for each of the five sites. The x-axis shows the covariate-adjusted mean poverty rate for the treatment group relative to the control group for the same site. The y-axis shows the covariate-adjusted mean outcome for each treatment group relative to the control mean for the outcome for that site. As we will see shortly, there is a statistically significant positive relationship between poverty and depression in our linear IV specification.

If $f(P_{ij})$ is nonlinear, then differences in the neighborhood poverty distributions among treatment groups by site (beyond mean differences) can potentially be used to identify the possibly nonlinear effects of neighborhood poverty on outcomes.[5] For example, Table I shows that the mean neighborhood poverty rates for the Section 8 and Experimental groups are almost identical, but the underlying distributions differ with the Experimental group having a larger share of households residing in low-poverty neighborhoods (those with poverty rate of under 12 percent) and the Section 8 group having a larger share in medium-low areas (census tracts with poverty rates from 12 to 24 percent).

[5]In future versions of this paper, we plan to explore whether variation in treatment effects and poverty distributions across sites by race/ethnicity and by age groups may also shed some light on the functional form of the relationship between neighborhood characteristics and outcomes.

The neighborhood poverty distributions by random assignment group and site in Table I are suggestive of the types of patterns of ITT estimates by group and site that would be apparent in the linear case and in specific nonlinear cases. Overall, the mean neighborhood poverty rate is 8 percentage points lower for the Experimentals and 6 percentage points lower for the Section 8 group than the controls. A linear effect of neighborhood poverty on outcomes should generate ITT estimates that are modestly (about 33 percent) larger for the Experimental group than the Section 8 group, but ITT's of similar magnitude for both groups would be quite reasonably consistent with an underlying liner relationship. Such a linear relationship also would imply particularly large ITT estimates for both Los Angeles treatment groups and New York Experimentals relative to the Chicago and Baltimore treatment groups. A non-linear relationship in which the benefits are particularly large for getting into a low-poverty (middle class) neighborhood (poverty rate of under 12 percent) would suggest a substantially larger ITT estimate for the Experimentals than the Section 8 group. This type of non-linear relationship implies a pattern of site-specific treatment effects that differ from the linear case with a particularly large ITT for the Boston Experimentals and particularly small impact for the Los Angeles Section 8 group. We next explore whether the cross-site and cross-treatment group variation in poverty distributions is sufficient to sort a linear impact of neighborhood poverty from specific non-linear alternatives.

The identification strategy outlined in this section rests on the assumption that the mean differences in outcomes by random assignment group at each MTO site are directly related to differences in neighborhood poverty distributions. We interpret the Census poverty rate as an index for a bundle of correlated characteristics of neighborhoods that are relatively stable, including education levels, occupations, etc; we do not interpret our model as holding these fixed while the poverty rates vary. Even under this less restrictive interpretation, threats to validity of this approach can arise from heterogeneity in treatment effects across sites being driven by other factors than differences in group poverty distributions (such as temporary fluctuations in labor market conditions or differences in the types of families able to take advantage of vouchers and move) that are not stable characteristics of neighborhoods.

We also compare instrumental variables estimates of the effects of neighborhood poverty on outcomes using the MTO experimental variation to OLS estimates using the non-experimental variation in neighborhood poverty of the Controls and the combined experimental and non-experimental variation in the full MTO sample. The usual prior is that the non-random sorting of households across neighborhoods will generate upward biases in estimates of the effects of neighborhood poverty on outcomes. The direction of selection biases in the MTO control group is less clear given major changes in housing policy affecting public housing residents and the disruptive effects of potentially involuntary moves to lower-poverty neighborhoods for those in public housing projects being demolished and remodeled.

4. Results Using Experimental Variation in Neighborhood Poverty

We analyze the relationship between neighborhood poverty and a selected set of youth and adult outcomes using data from the interim evaluation of MTO. The sample used in the interim evaluation consists of the 4248 families randomly assigned in the MTO demonstration through December 31, 1997. The outcome measures analyzed consist of respondent self-reports from the interim evaluation surveys of household heads and youth administered largely in person and collected between January and September 2002. The neighborhood poverty measure used in the analysis is the 2000 Census poverty rate of the census tract of the respondent's current residential location at the time of interim survey.[6]

Orr et al. (2003) provides a detailed description of the design and implementation of the interim evaluation of MTO and analyzes the treatment effects of MTO on a wide range of outcomes.[7] The residential moves with housing vouchers through MTO not only led families to areas with lower poverty rates (as shown in Table I) but also improved housing, neighborhood conditions and safety. Experimental and Section 8 families expressed greater satisfaction with their housing and neighborhoods and indicate much lower criminal victimization rates than Control group families at the time of the interim evaluation survey. These gains in perceived neighborhood quality and safety were greater for the Experimental families. MTO voucher eligibility does not appear to have a significant overall impact on adult economic self-sufficiency in the medium-term, but there is a little evidence that program moves are associated with short-run negative disruption effects on employment that dissipate after several years. The MTO interim survey results indicate moves to wealthier neighborhoods are associated with significant improvements in adult mental health and reductions in adult obesity. The interim impacts of MTO differ substantially for boys and girls. Girls in the Experimental group experience large

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[6]In a subsequent draft, we plan to explore the sensitivity of the results to using exposure measures of neighborhood characteristics that are a weighted average of the neighborhoods that an individual has lived in since random assignment.

[7]Kling and Liebman (2003) examine medium-term impacts of MTO on youth human capital development. Katz, Kling and Liebman (2003) and Kling, Liebman, Katz, and Sanbonmatsu (2003) provide more detailed analyses of interim impacts on labor market and health outcomes respectively. Also, see Katz, Kling and Liebman (2001) and Goering and Feins (2003) for analyses of the short-run impacts of MTO.

improvements in mental health and reductions in delinquent and risky behaviors. Boys in the Experimental group do not show similar improvements and show some evidence of increased behavioral problems.

Our preliminary analysis of possible nonlinearities in the relationship between neighborhood poverty and outcomes focuses on those key outcomes with some evidence of substantial treatment effects (mental health outcomes for girls and adults, risky behavior for girls, and adult body mass index (BMI)). We also examine two other major outcomes: employment for adults and idleness (a combined measure of schooling and employment participation) for female youths. In this section, we first summarize the Experimental and Section 8 ITT estimates for each outcome and then explore the functional relationship of outcomes to neighborhood poverty using only the experimental variation in poverty distributions by random assignment group within sites. Section V looks at estimates of the effects of neighborhood poverty using nonexperimental variation.

A. Experimental Results for Female Youths

Table II summarizes experimental ITT estimates of MTO impacts on selected outcomes for female youths and presents the estimates of the impact of neighborhood poverty on these outcomes using the experimental variation in poverty rates.[8] The table compares instrumental variables estimates of the impact of neighborhood poverty (instrumenting for functions of

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[8]The ITT estimates for female youth reported in Table II are slightly different than those reported in Orr et al. (2003) for two reasons. The first is that the model in Table II is estimated only on female youths. The estimates in Orr et al. (2003) are based on pooled models of male and female youths allowing separate treatment effects by sex but constraining the effects of the site dummies and baseline covariates to be the same for girls and boys. The second reason is that the survey responses from one clearly problematic interviewer are dropped in estimates in Table II.

neighborhood poverty with a full set of interactions of site dummies and random assignment group dummies) under the assumptions that $f(P_{ij})$ can be approximated by a linear relationship, a quadratic, and a quartic (fourth-degree) polynomial. The regressions reported in Table II and in the subsequent tables all include a full set of baseline (pre-randomization) covariates covering individual and household characteristics, include a full set of site dummies, and are weighted to reflect variation in random assignment ratios over time in the implementation of the MTO demonstration.[9]

The effect of MTO and neighborhood poverty on the mental health of female youths aged 12 to 19 years is analyzed in the top two rows of Table II. The first row analyzes a lifetime depression scale that is meant to produce estimates of major depressive episodes. The second row analyzes a six-item scale of psychological distress during the past month. The results for both measures of girls' mental health are quite similar.[10] The ITT estimates suggest modest and marginally significant reductions in major depression and mental distress of rather similar magnitudes for both the Experimental and Section 8 groups. The linear IV models suggest strong and statistically significant dosage effects of neighborhood poverty on both depression and mental distress-- with moves to lower poverty reducing both measures of psychological problems. An examination of the site-specific pattern of treatment effects for both of these outcomes show larger negative ITT effects (improvements in mental health) for the treatment groups with larger declines in mean neighborhood poverty (Los Angeles treatment groups and

[9]See Orr et al. (2003) for a full listing of the baseline covariates and a discussion of the random assignment ratio weights.

[10]A third mental health measure (a lifetime generalized anxiety disorder scale) yields quite similar ITT estimates and a similar relationship to neighborhood poverty (using the experimental variation of treatment groups interacted with site dummies) as the depression and mental distress measures.

New York experimentals) than for the treatment groups with only modest reductions in mean neighborhood poverty (the Chicago and Baltimore groups). The quadratic and quartic polynomial models provide no evidence of strong nonlinearities in the relationship between neighborhood poverty and mental health for female youths, but the estimates are sufficiently imprecise that subtle nonlinearities cannot be ruled out. The top panel of Figure 3 depicts the predicted values for an IV model of mental distress on neighborhood poverty where neighborhood poverty is represented by a two-part linear spline with a kink at 25 percent poverty. It is clear that there are no significant deviations from linearity for this outcome in this specification.

The impacts of MTO on participation in risky behaviors, employment, and schooling for female youths aged 15 to 19 are explored in the bottom two rows of Table II. The risky behavior index is the fraction of four risky behaviors (alcohol use, marijuana use, cigarette smoking, and sexual intercourse) in which a youth reports to have ever engaged. The Experimental group experienced a large (7.6 percentage point) and highly statistically significant decline in risky behaviors relative to the Controls. This estimate is driven by declines in alcohol, marijuana, and cigarette use by the Experimental group and not by changes in sexual activity. The ITT estimate on risky behaviors for the Section 8 group is much smaller and not statistically significant. The site-specific ITT estimates of reductions in risky behavior are largest for the Boston and Baltimore Experimental groups and have the opposite sign for the Los Angeles treatment groups with particularly large reductions in mean poverty and the share living in high-poverty neighborhoods. This pattern suggests some deviations of linearity with little reduction (and maybe even perverse increases) in risky behavior in moving from very high to moderately high poverty area but with then large reductions in risky behavior in moving from medium to low poverty neighborhoods. The bottom panel of Figure 3 shows such a pattern in instrumental variables estimate of a two-part linear spline with a kink point at a 25 percent neighborhood poverty rate. The estimates in Table II are suggestive of some nonlinearity but the data are not strong enough to definitively reject a linear relationship of neighborhood poverty and risky behaviors.

Idleness is an indicator variable that takes on a value of one for individuals that are neither employed nor enrolled in school. The patterns of estimates for the idleness outcomes in the bottom row of Table II are rather comparable (but typically smaller in magnitude and statistical significance) to the estimates for the risky behavior index. The point estimates suggest some evidence of reductions in idleness for female youths in the Experimental group and weak evidence for a nonlinearity with reductions in idleness only occurring for reductions in poverty below a medium poverty level of around 25 percent are also apparent in the quartic polynomial and linear spline models.

B. Experimental Results for Adults

Table III summarizes results on selected outcomes using experimental variation in residential location for the MTO adults.[11] The adult sample consists of one adult from each MTO household with the first priority being the female core household head or the wife of the core head. The sample largely consists of female household heads.

[11]The ITT estimates in Table III differ slightly from the corresponding estimates in Orr et al. (2003) because survey responses from one clearly problematic interviewer have been dropped from the estimation sample.

The first two rows of Table III show the results for adults for major depression and mental distress (the same two mental health measures analyzed for female youth). Moderate but statistically significant reductions in major depression and mental distress are apparent for the Experimental group and smaller and insignificant reductions for the Section 8 group. The linear IV models show strong linear negative relationships between neighborhood poverty and both depression and mental distress. The nonlinear models provide little evidence of important nonlinearities. The IV two-part spline model shown in the top panel of Figure 4 provides strong evidence of linearity – the slopes of the two segments of the spline are nearly indistinguishable. Thus, a linear and substantial pattern of reductions in major mental health problems with lower neighborhood poverty is apparent for both female youth and adults.

The third row of Table III analyzes calmness, a distinctive measure of mental health, that is an indicator variable for whether the respondent felt "calm and peaceful" most of the time or more over the past month. The ITT estimate on calmness for the Experimental group is large and statistically significant. There is no apparent Section 8 impact. The IV estimates indicate a strong negative effect of neighborhood poverty on calmness with nonlinear models suggesting some nonlinear effect with gains in calmness only apparent once the poverty rate is into the medium poverty range of 25 percent and lower. The nonlinear pattern of the impact neighborhood poverty on calmness is illustrated in the bottom panel of Figure 4.

The bottom row of Table III indicates substantial negative effects of both Experimental and Section 8 MTO moves on body mass index (BMI).[12] The IV estimates indicate a substantial

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[12]Similar strong negative effects of the Experimental and Section 8 treatments are apparent when the BMI measures is converted into an a binary indicator for obesity.

negative linear effect of neighborhood poverty on BMI and little evidence for deviations from linearity in this relationship. The estimates in Table III also suggest no detectable effects either linear or nonlinear of neighborhood poverty on adult employment outcomes. The ITT estimates are sufficiently precise to rule out large employment effects, but they don't rule out some modest improvements from MTO moves.

The overall patterns of results in Tables II and III are suggestive of beneficial and rather linear reductions in neighborhood poverty on mental health outcomes for female youth and adults and on adult BMI (or obesity). The variation in ITT estimates by treatment group and site provide a little evidence of nonlinear (threshold) effects of getting from medium poverty neighborhoods (25 percent) into low-poverty neighborhoods. There is little (or no) evidence from the MTO experimental variation in neighborhood locations for contagion models of social problems in which neighborhood poverty moving over some threshold high poverty level (like 40 percent) leads to sharp reductions in mental well-being and increases in social problems.

5. Results Using Nonexperimental Variation in Neighborhood Poverty

We next explicitly ignore MTO's experimental design and examine the relationship between neighborhood poverty and individual outcomes assuming that assignment to census tracts at the time of the interim evaluation is random conditional on baseline covariates. This approach closely follows the approaches of standard observational (nonexperimental) studies analyzing neighborhood effects (e.g., Brooks-Gunn et al. 1993; O'Regan and Quigley 1996).

One difference from earlier studies is that our sample is more homogeneous and restricted to families originally living in public housing projects in high-poverty urban areas at baseline. Nevertheless, there is substantial nonexperimental neighborhood mobility in the MTO sample with 70% of the Control group having moved between random assignment and the time of the interim evaluation survey. Even among the noncompliers in the Experimental and Section 8 groups the majority had moved out of their baseline public housing unit by the time of the interim evaluation. Figure 1 and Table I show substantial variation in neighborhood poverty rates in 2002 for the Control group. In fact, only about 5 percent of the variation in neighborhood poverty for the full sample shown in Figure 1 is accounted for by pure experimental variation in residential location.[13]

We estimate the effects of neighborhood poverty on outcomes for the Control group using purely nonexperimental variation in residential locations. We supplement this approach by looking at the OLS relationship of functions of neighborhood poverty and outcomes using the full MTO sample. Thus, our approach is to estimate equation (3) by OLS for the Controls and full sample under different assumptions about the functional form of $f(P_{ij})$. Table IV presents the results for female youth. Table V presents the results for adults.

The analyses of the Control group for both female youths and adults in Tables IV and V show essentially no significant linear relationship of neighborhood poverty and any of the outcomes (with the exception of BMI for adults where the significant linear effect of census tract poverty goes in the opposite direction of the results using experimental variation). These findings contrast sharply with the linear instrumental variables estimates of mental health outcomes and body mass index in Tables II and III. Despite the usual presumption of positive

[13]In other words, a regression of census tract poverty on dummy variables for random assignment group yields an R^2 of around .05.

residential sorting on unobservables into lower-poverty areas, the OLS approach does not detect any linear impact of neighborhood poverty on mental health and employment outcomes for the Control group. The full sample analysis largely based on nonexperimental variation is similar but does imply significant gains in employment and calmness for adults moving to lower-poverty areas. These results are suggestive of greater positive selection bias in moving to lower poverty neighborhoods within the Experimental and Section 8 groups than in the Control group.

There are a greater number of statistically significant relationships between neighborhood poverty and outcomes using nonexperimental variation in residential locations for the Control group when nonlinear relationships are allowed, especially for girls' depression, idleness, and adult employment. The models with fourth degree polynomials in neighborhood poverty for the Controls suggest large reductions in girls' depression and substantial improvements in adult employment in moving from high to medium poverty neighborhoods (50 percent to 25 percent poverty rates) but no gains (or even losses) from moving to lower poverty areas. And the nonlinear estimates of the effects of neighborhood poverty on idleness indicate perverse increases in poverty in moving from high to medium poverty neighborhoods (in the range from 70% to 20% poverty rates).

The estimated relationships between neighborhood poverty and outcomes for female youths and adults are quite different using nonexperimental variation in residential locations from the estimates exploiting MTO's experimental design. In particular, the consistent evidence from the experimental variation of mental health gains and obesity reduction from moving to lower-poverty areas are not apparent in the nonexperimental analysis.

6. Conclusions

Our analysis using experimental variation in neighborhood poverty rates suggests that two patterns between neighborhood poverty and individual outcomes are prevalent. First, for several outcomes, including adult and youth depression and adult obesity, there are steady, apparently linear, improvements as the neighborhood poverty rate falls over a wide range of poverty rates. Second, for other outcomes, including youth risky behavior and idleness as well as adult calmness, there are little or no improvements in outcomes as neighborhood poverty rates are reduced from high to moderate levels. However, for these outcomes, significant improvements occur when individuals move to low poverty neighborhoods. In the outcomes we studied, we did not observe any threshold effects from moving from very high poverty neighborhoods into moderately high poverty neighborhoods, as some theories have suggested would occur. Our results using non-experimental variation were not at all consistent with those using experimental variation, suggesting further reason to be cautious in interpreting non-experimental studies of the impact of neighborhoods on individual outcomes.

The results in this paper are preliminary. In subsequent drafts we plan to expand this analysis to additional outcomes, including child test scores and to additional measures of neighborhood quality. Moreover, in this draft we have relied on two polar identifying assumptions. The first was based solely on experimental variation and the second ignored the experimental variation and estimated the impact of census tract poverty on individual outcomes assuming that assignment to census tracts is conditionally random. We are currently working on a third approach that builds on the recent work of Efron and Feldman (1991), Imbens (2000), and Heckman, Tobias, and Vytlacil (2000) and more flexibly models the choice of neighborhood poverty level as a function of individual characteristics and the treatment assignment and then estimates the outcomes conditional on this choice.

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Figure 1
Distribution of Census Tract Poverty Rates at Time of Interim Evaluation
(percent of persons in poverty)

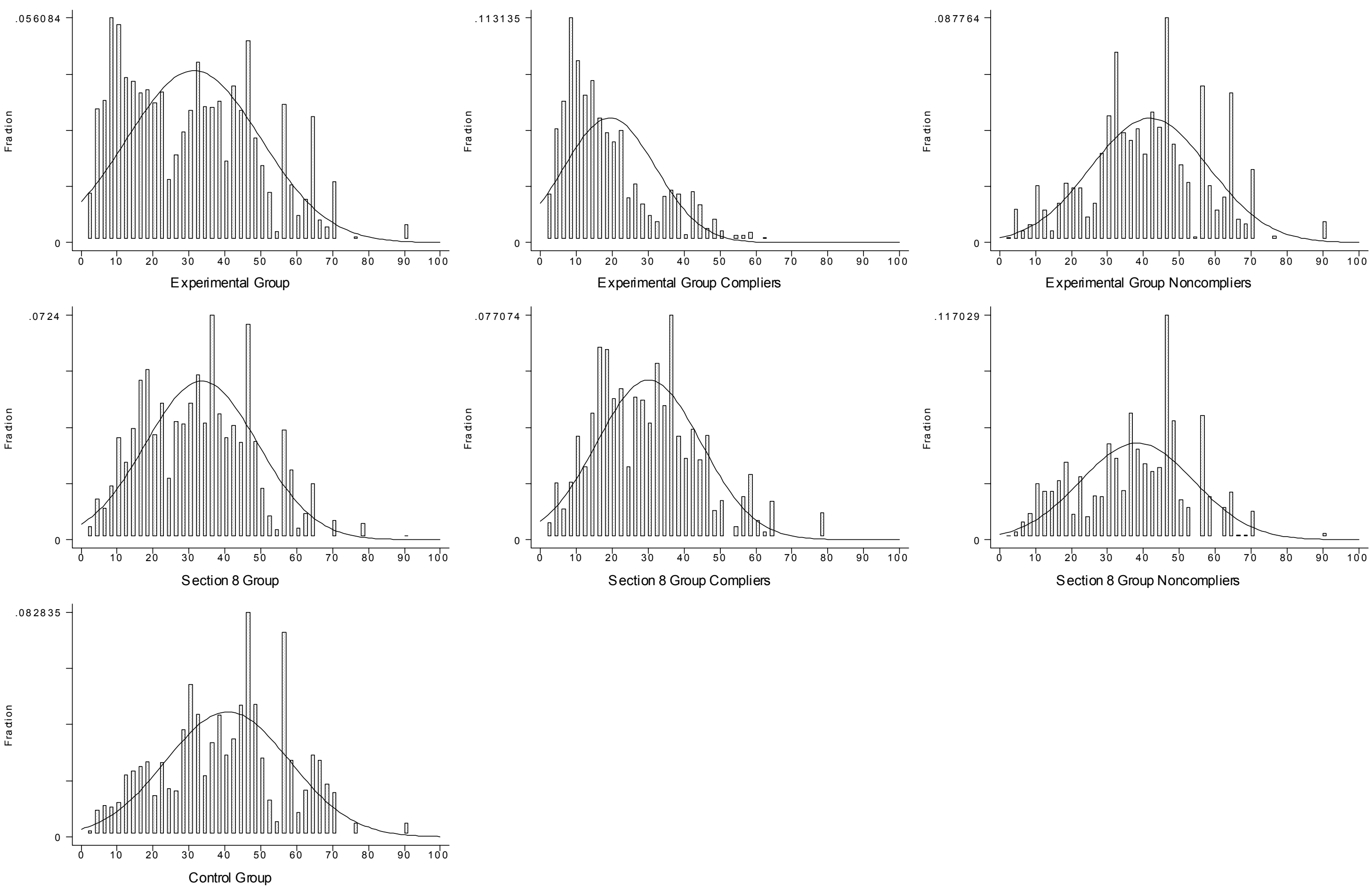


Figure 2
Linear IV with Site-by-Treatment Interactions

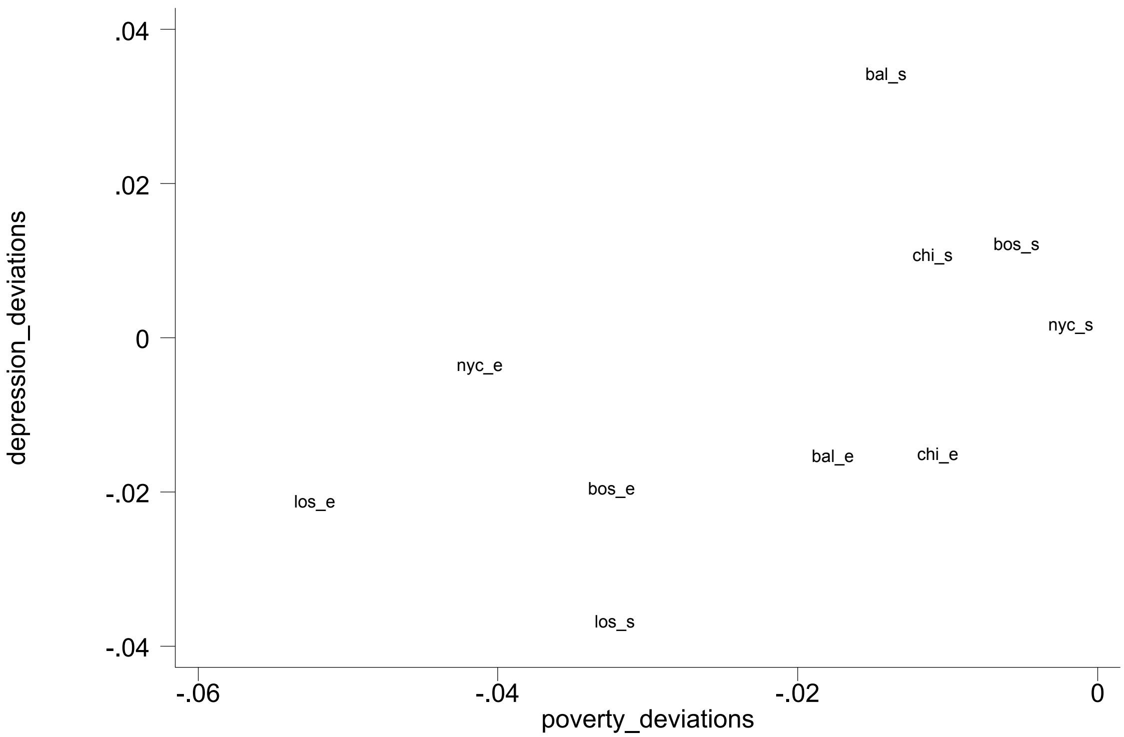
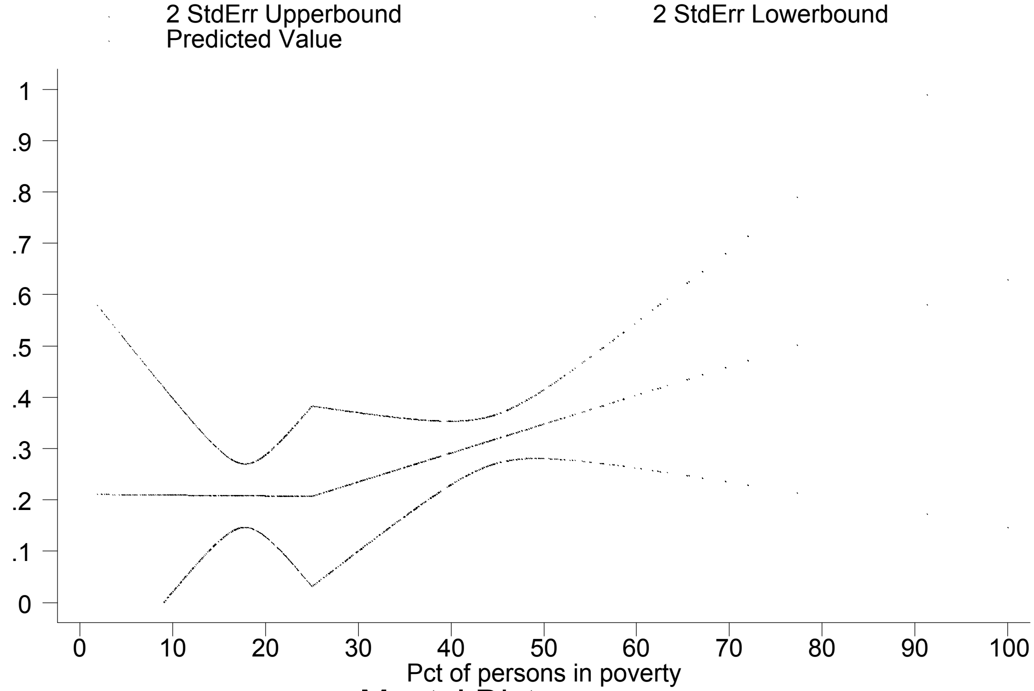
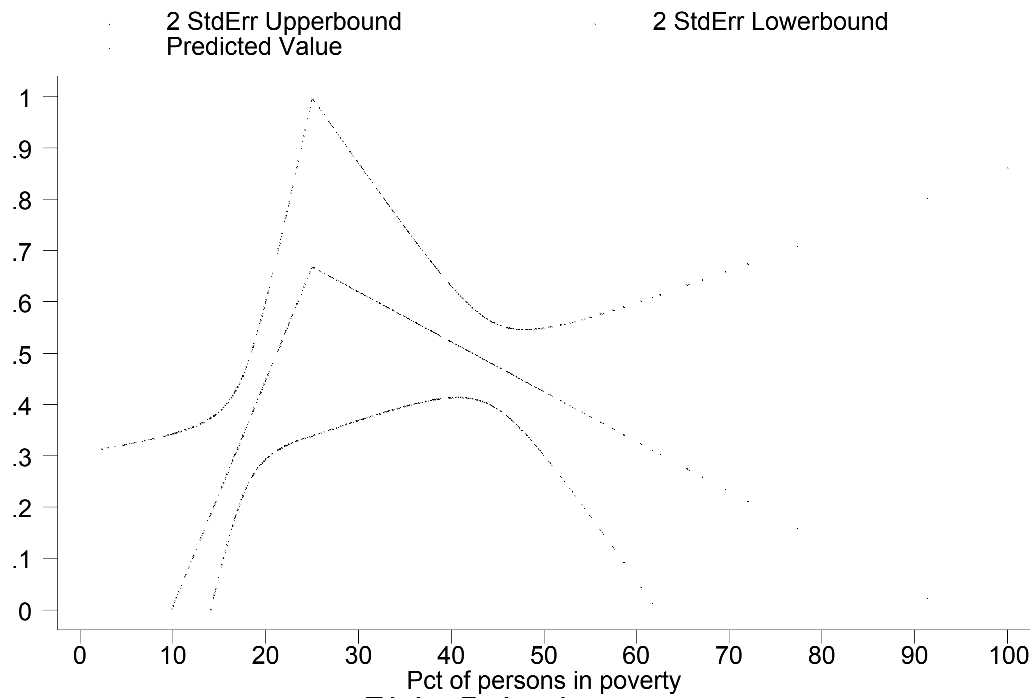


Figure 3
Adolescent Outcomes: Relationship Between Poverty and Outcome in IV Model
(two-part linear spline with kink at 25 percent poverty)



Mental Distress



Risky Behavior

Figure 4
Adult Outcomes: Relationship Between Poverty and Outcome in IV Model
(two-part linear spline with kink at 25 percent poverty)

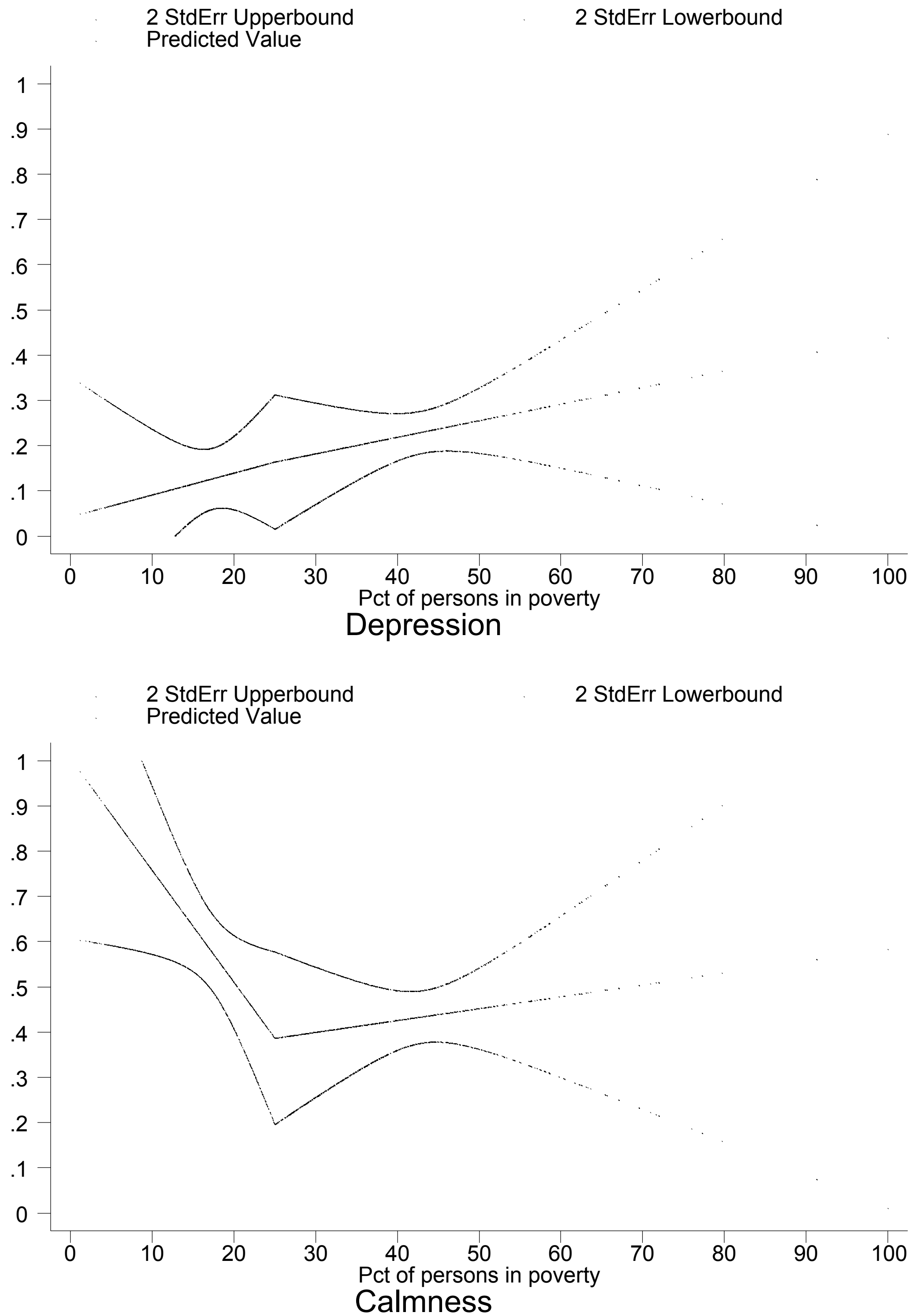


TABLE I

DISTRIBUTION OF CENSUS TRACT POVERTY BY RANDOM ASSIGNMENT GROUP AND SITE

	Mean Poverty Rate	Compliance Rate	Share of Households in Census Tract with 2000 Poverty Rate in Each Range				
			0-12	12-24	24-36	36-48	48+
All							
Control	0.384	---	0.0533	0.154	0.200	0.221	0.372
Section 8	0.323	0.627	0.0867	0.228	0.212	0.224	0.249
Experimental	0.302	0.483	0.174	0.216	0.180	0.176	0.254
Baltimore							
Control	0.349	---	0.0704	0.229	0.175	0.129	0.397
Section 8	0.296	0.770	0.0962	0.214	0.248	0.159	0.283
Experimental	0.295	0.535	0.174	0.184	0.212	0.171	0.259
Boston							
Control	0.319	---	0.0652	0.200	0.286	0.288	0.161
Section 8	0.265	0.515	0.139	0.294	0.247	0.193	0.127
Experimental	0.235	0.436	0.230	0.260	0.254	0.149	0.107
Chicago							
Control	0.407		0.0564	0.200	0.217	0.133	0.394
Section 8	0.366	0.667	0.0895	0.226	0.215	0.185	0.285
Experimental	0.363	0.334	0.152	0.208	0.168	0.159	0.313
Los Angeles							
Control	0.436	---	0.0288	0.106	0.166	0.257	0.442
Section 8	0.327	0.788	0.0341	0.245	0.183	0.306	0.232
Experimental	0.307	0.672	0.140	0.239	0.173	0.206	0.242
New York							
Control	0.418		0.0440	0.0465	0.131	0.273	0.506
Section 8	0.351	0.463	0.0619	0.163	0.167	0.283	0.325
Experimental	0.313	0.467	0.162	0.183	0.0960	0.202	0.357

TABLE II

EXPERIMENTAL ESTIMATES OF THE RELATIONSHIP BETWEEN POVERTY AND OUTCOMES IN FEMALE YOUTH

	Control Mean {stdev}	ITT Experimental	ITT Section 8	Coefficient on poverty in linear IV	Coefficient on linear poverty term in quadratic IV	Coefficient on quadratic poverty term in quadratic IV	Reject linearity in fourth degree polynomial? [p-value]
Female Youth (ages 12-19)							
Major depression	.107	-.0302	-.0440**	.451**	-.565	1.35	No
[N=1250]	{.309}	(.0216)	(.0216)	(.204)	(1.016)	(1.36)	[.771]
Mental distress	.303	-.0309	-.0329	.414**	.694	-.372	No
[N=1295]	{.286}	(.0195)	(.0212)	(.183)	(1.120)	(1.465)	[.851]
Female Youth (ages 15-19)							
Risky behavior index	.445	-.0760**	-.0181	.394	2.39	-2.65	No
[N=733]	{.360}	(.0304)	(.0350)	(.303)	(1.57)	(2.12)	[.177]
Idleness	.258	-.0416	.0296	.271	1.02	-.992	No
[N=730]	{.438}	(.0401)	(.0432)	(.367)	(2.10)	(2.850)	[.176]

Notes: Robust standard errors in parentheses. P-values in square brackets. Standard deviations in braces. All regressions include site dummies and the full set of baseline covariates from Orr et al. (2003). The regressions are weighted using the MTO random assignment weights. ITT is intent to treat estimate. Major depression is a measure of lifetime depression using the scales developed for use by the National Comorbidity Survey Replication. Mental distress reports the fraction of six mental health outcomes (feeling “so depressed nothing could cheer you up,” “nervous,” “restless or fidgety,” “hopeless,” “everything was an effort,” or “worthless) the sample member reported feeling at least “some of the time” during the past 30 days. The risky behavior index is the fraction of 4 risky behaviors (alcohol use, cigarette smoking, marijuana use, and sexual intercourse) that a youth self-reported ever having engaged in. Idleness is defined as neither employed nor enrolled in school.

TABLE III

EXPERIMENTAL ESTIMATES OF THE RELATIONSHIP BETWEEN POVERTY AND OUTCOMES IN ADULTS

	Control Mean {stdev}	ITT Experimental	ITT Section 8	Coefficient on poverty in linear IV	Coefficient on linear poverty term in quadratic IV	Coefficient on quadratic poverty term in quadratic IV	Reject linearity in fourth degree polynomial? [p-value]
Major depression	.210	-.0328*	-.0128	.401**	.219	.259	No
[N=3291]	{.407}	(.0177)	(.0197)	(.200)	(.927)	(1.269)	[.611]
Mental distress	.322	-.0322**	-.00774	.366**	.940	-.818	No
[N=3415]	{.335}	(.0146)	(.01621)	(.166)	(.771)	(1.071)	[.296]
Calmness	.489	.0451**	.00108	-.544**	-2.16*	2.30	No
[N=3313]	{.500}	(.0223)	(.02465)	(.249)	(1.20)	(1.65)	[.111]
Employment	.525	.00785	.0154	-.156	.697	-1.21	No
[N=3309]	{.500}	(.02112)	(.0235)	(.233)	(1.091)	(1.50)	[.608]
Body mass index	30.7	-1.06**	-.808**	10.4**	15.5	-7.45	No
[N=3301]	{8.0}	(.34)	(.396)	(3.8)	(17.7)	(25.18)	[.390]

Notes: Robust standard errors in parentheses. P-values in square brackets. Standard deviation in braces. ITT is intent to treat estimate. All regressions include site dummies and the full set of baseline covariates from Orr et al. (2003). The regressions are weighted using the MTO random assignment weights. Major depression is fraction of sample that experienced an episode of major depression during the past year using the CIDI-SF major depressive episode scale. Mental distress reports the fraction of six mental health outcomes (feeling "so sad nothing could cheer you up," "nervous," "restless or fidgety," "hopeless," "everything was an effort," or "worthless) the sample member reported feeling at least "some of the time" during the past 30 days. Calmness is fraction reporting that they felt "calm and peaceful" at leaset some of the time during the past 30 days. Employment measures whether the adult was employed at the time of the interim evaluation survey. Body mass index is weight in kilograms divided by height in meters squared (a value of 30 or higher is considered obsese).

TABLE IV

NON EXPERIMENTAL ESTIMATES OF THE RELATIONSHIP BETWEEN POVERTY AND OUTCOMES IN FEMALE YOUTH

	Control Group Only				Full Sample			
	Coefficient on poverty in linear OLS	Coefficient on linear poverty term in quadratic OLS	Coefficient on quadratic poverty term in quadratic OLS	Reject linearity in fourth degree polynomial? [P-value]	Coefficient on poverty in linear OLS	Coefficient on linear poverty term in quadratic OLS	Coefficient on quadratic poverty term in quadratic OLS	Reject linearity in fourth degree polynomial? [P-value]
Female Youth (age 12-19)								
Major depression	.0357 (.1347)	-.728* (.376)	.864** (.378)	Yes [.0001]	.0888 (.0609)	-.0271 (.2214)	.153 (.306)	No [.222]
Mental distress	.139 (.104)	-.0952 (.3057)	.266 (.291)	No [.329]	.0730 (.0511)	.321* (.187)	-.329 (.252)	No [.278]
Female Youth (age 15-19)								
Risky behavior index	-.0207 (.1537)	-.241 (.468)	.243 (.452)	No [.961]	.0797 (.0805)	0.471 (.297)	-.504 (.386)	No [.122]
Idleness Index	-.0974 (.2644)	-1.78** (.76)	1.85** (.82)	Yes [.0001]	.111 (.111)	-.0159 (.3350)	.163 (.436)	Yes [.0008]

Notes. Robust standard errors in parentheses. P-values in brackets. All regressions include site dummies and the full set of baseline covariates from Orr et al. (2003). The regressions are weighted using the MTO random assignment weights. See Table II for variable definitions.

TABLE V

NON EXPERIMENTAL ESTIMATES OF THE RELATIONSHIP BETWEEN POVERTY AND OUTCOMES IN ADULTS

	Control Group Only				Full Sample			
	Coefficient on poverty in linear OLS	Coefficient on linear poverty term in quadratic OLS	Coefficient on quadratic poverty term in quadratic OLS	Reject linearity in fourth degree polynomial? [P-value]	Coefficien t on poverty in linear OLS	Coefficient on linear poverty term in quadratic OLS	Coefficient on quadratic poverty term in quadratic OLS	Reject linearity in fourth degree polynomial? [P-value]
Major depression	-.0227 (.0815)	.00481 (.33432)	-.0342 (.3944)	No [.550]	.0147 (.0410)	-.0986 (.1550)	-.115 (.199)	No [.759]
Mental distress	-.0172 (.0648)	.132 (.233)	-.186 (.266)	No [.710]	.0504 (.0365)	.260** (.125)	-.286* (.161)	No [.100]
Calmness	.0200 (.0996)	-.562 (.415)	.724 (.512)	Yes [.0031]	-.153** (.057)	-.819** (.220)	.910** (.297)	Yes [.0000]
Employment	-.0450 (.0959)	-.843** (.372)	.993** (.443)	Yes [.0442]	-.192** (.055)	-.0195 (.1952)	-.236 (.261)	No [.271]
Body mass index	-4.18** (1.63)	3.60 (5.67)	-9.69 (6.80)	No [.292]	-1.66* (.86)	7.05** (2.89)	-11.9** (3.8)	Yes [.0132]

Notes. Robust standard errors in parentheses. P-values in brackets. All regressions include site dummies and the full set of baseline covariates from Orr et al. (2003). The regressions are weighted using the MTO random assignment weights. See Table III for variable definitions.