Part A: Justification

A.1 Circumstances Making Information Collection Necessary

This request is for the clearance of survey instruments, youth achievement testing, adult biomarker data collection and interviewer observations of respondent's neighborhoods for the Final Evaluation of the Moving to Opportunity for Fair Housing (MTO) demonstration program. MTO is a unique experimental research demonstration designed to answer the question of whether moving from a high-poverty neighborhood to a lower-poverty community improves the social and economic prospects of low-income families. Originally authorized by Congress in Section 152 of the Housing and Community Development Act of 1992, MTO made use of Section 8 rental assistance, in combination with intensive housing search and counseling services, to assist low-income families with children living in public or Section 8 project-based housing to move from some of our nation's most distressed urban neighborhoods to lower-poverty communities. The program was run in five cities: Baltimore, Boston, Chicago, Los Angeles, and New York.

The authorizing Congressional legislation of 1992 also required that HUD undertake a long-term evaluation of MTO's impacts on families, which is the subject of this request. HUD recognized that more families would wish to participate in the program than HUD had Section 8 rental assistance. This excess demand for Section 8 rental assistance allowed program participation to be determined by a random lottery, which in turn has been central to HUD's ability to support rigorous research efforts to understand the MTO program's causal impact on participating families. The enrollment and randomization phase of MTO ended in February 1999, but MTO families continue to receive the housing vouchers they were offered under the program and there have also been several phases of HUD-supported research to capture MTO impacts over time.

The MTO demonstration has two broad research goals. The first short term goal was to compare the costs and services of the MTO program with the routine implementation of the Section 8 tenant-based rental assistance program. HUD reported to Congress in 1996 on these findings regarding the progress and effectiveness of the demonstration. The second longer term goal is to assess the impact of the demonstration on family and children's well-being including their housing conditions, mental and physical health, employment and earnings, receipt of social program assistance and income, education, and delinquent or risky behavior of children.

To meet these goals, HUD took advantage of the excess demand for Section 8 rental subsidies and randomly assigned through a lottery-like process all families who signed up for MTO into one of three groups:

> ➤ the **MTO EXPERIMENTAL GROUP**, which received Section 8 certificates or vouchers usable only in low-poverty areas (areas with less than 10 percent of the population below the poverty line in 1990), along with counseling and assistance in finding a private unit to lease;

➢ the **SECTION 8 COMPARISON GROUP**, which received regular Section 8 certificates or vouchers (geographically unrestricted) and ordinary briefings and assistance from the Public Housing Authorities (PHA); and

➢ the **IN-PLACE CONTROL GROUP**, which received no certificates or vouchers but remained eligible for public or project-based housing and other social programs that families would otherwise have been entitled to

The participants assigned to these three research groups have been tracked and surveyed 2-3 years after random assignment (the "short term" MTO evaluation) and 4-7 years after random assignment (the "interim" MTO evaluation) to understand the effects of the demonstration on participating families. The experience of families assigned to the Experimental group receiving the special MTO assistance can be compared with that of families who receive the "regular" Section 8 treatment. The in-place control group is essential to correctly estimate the impacts of Section 8 rental assistance separate from the impacts of MTO assistance with counseling, providing a benchmark against which the outcomes of the two other groups can be measured.

The MTO final evaluation (the subject of this request) will examine many aspects of family life measured on average 10 to 12 years after enrollment in the demonstration program, with a focus on those outcome domains that may have been affected by MTO participation. The final evaluation represents the first attempt since the interim evaluation to interview sample members in depth, using common survey instruments and data collection techniques across all sites.

A total of 4,608 families enrolled in the MTO demonstration and were randomly assigned to one of the three research groups between September 1994 and August 1998. All enrolled families completed a baseline information form under a clearance granted by OMB in 1994.[1] The MTO research team maintained contact with MTO enrolled families to update information about addresses, and changes in family status, employment and receipt of program services through brief canvasses conducted in 1997 and 2000, under clearances previously granted by OMB.[2] The interim MTO evaluation of program impacts after 4 to 7 years was completed in 2002. Abt Associates has tracked addresses and changes in family status of MTO families since the interim evaluation and will continue to do so, under contract with HUD, through September 2007.[3]

This submission is a request for clearance of the following instruments, and data collection, to provide HUD with the information needed to determine the long-term effects of the MTO demonstration on the lives of participating low-income families and children:

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[1] Clearance No. 2528-0161, initially expiring June 1997, finally expiring November 30, 2000.

[2] Clearance of the MTO canvass data collection was originally granted by OMB under clearance number 2528-0189, expiration date January 1999, extended to April 1999 (see Notice of Short Term Extension from Donald R. Arbuckle, OMB, dated 1/19/1999). This clearance was subsequently extended through June 30, 2002 (see Notice of Office of Management and Budget Action from Donald R. Arbuckle, OMB, dated June 24, 1999).

[3] Clearance of the MTO Interim evaluation was granted by OMB under clearance number 2528-0218, expiration date October 31, 2004. Clearance of additional tracking activities was granted by OMB under clearance number 2528-0233, expiration date November 30, 2007.

- ➢ An adult respondent survey, designed to gather data on outcomes and mediating factors concerning the respondent and other members of the household;

- ➢ A youth survey, designed to gather information on outcomes and mediating factors for youth who are ages 10 to 20 at the end of 2007 (just prior to the start of our survey data collection period) and who were residing with MTO families at time of enrollment;

- ➢ Educational achievement data through administration of portions of the math and reading assessments of the U.S. Department of Education's Early Childhood Longitudinal Study of Kindergartners in 1998 (ECLS), with supplementation of items from the reading and math assessments of the U.S. Department of Education's National Educational Longitudinal Survey of 8th graders in 1988 (NELS). Further details regarding collection of achievement data are provided in section A.2.1 below.

- ➢ Biomarker data collection including dried blood spots, blood pressure, and height and weight readings among adults, and height and weight readings among youth;

- ➢ Language assessments of adults and youth through audio-taping of open-ended survey questions and a reading passage;

- ➢ Interviewer observational data on the characteristics of the respondent's residence and the immediate neighborhood through trained interviewer neighborhood "walk-arounds."

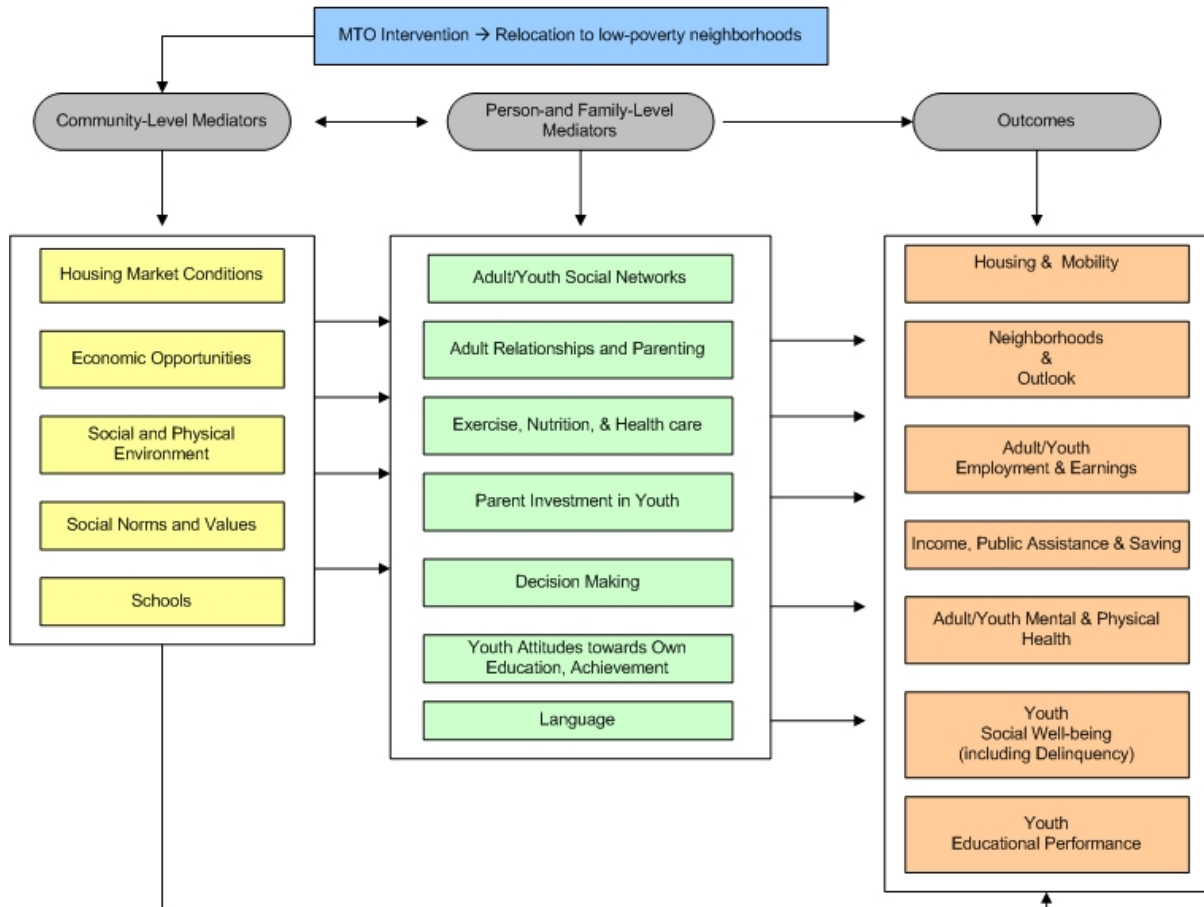## A.2 Purpose and Use of Information

HUD selected the National Bureau of Economic Research (NBER) to perform the final impact evaluation (contract #C-CHI-00808). The data collected during the final evaluation survey interview will be used by NBER and its team of researchers to measure and assess MTO's impacts in seven primary domains:

1. Housing assistance and mobility

2. Neighborhoods and social outlook

3. Employment and earnings

4. Household income, public assistance and savings

5. Mental and physical health

6. Delinquency and risky behavior

7. Academic achievement and educational attainment

The hypothesis underlying the MTO evaluation is that relocation of families to low-poverty neighborhoods will lead to improved well-being for adults and children in these seven domains. Exhibit 1 summarizes a general model outlining the different pathways through

which relocation to low-poverty neighborhoods leads to improved outcomes for families, based in part on the theoretical framework presented by Jencks and Mayer (1990).

Exhibit 1: Hypothesized Pathways of MTO Impacts

MTO Intervention → Relocation to low-poverty neighborhoods

Community-Level Mediators	Person-and Family-Level Mediators	Outcomes

Housing Market Conditions	Adult/Youth Social Networks	Housing & Mobility
Economic Opportunities	Adult Relationships and Parenting	Neighborhoods & Outlook
Social and Physical Environment	Exercise, Nutrition, & Health care	Adult/Youth Employment & Earnings
Social Norms and Values	Parent Investment in Youth	Income, Public Assistance & Saving
Schools	Decision Making	Adult/Youth Mental & Physical Health
	Youth Attitudes towards Own Education, Achievement	Youth Social Well-being (including Delinquency)
	Language	Youth Educational Performance

At the most general level MTO might affect the outcomes and well-being of participants by changing either the ***institutional / physical environment*** of the neighborhoods in which families reside, or else by changing the ***social environment*** of these neighborhoods. These two general characteristics of the neighborhood environment may in turn affect the specific daily experiences of MTO families including their attitudes towards or information about work, schooling or different health-risk behaviors, the frequency or nature of their social interactions, their language patterns, basic features of their decision making, or the quality of the household environments that MTO children experience through changes in parenting This general distinction between the role of the neighborhood institutional / physical environment versus the social environment is important because in principle policymakers can directly modify the institutional or physical characteristics of neighborhoods without moving families across areas. On the other hand, changing the social environments that low-income families experience will require either a very detailed understanding of precisely how

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local social interactions affect behavior in order to change the social functioning of neighborhoods, or else policy interventions that change the composition of communities by providing people with subsidies to move into different types of neighborhoods.

Institutional models of "neighborhood effects" on parents and children emphasize the role played by local public and private institutions, including schools (Rivkin et al., 2005), policing services (Sherman, 2003), access to high-quality health care services, or proximity to suitable job opportunities or adequate public transportation options (Kain, 1968). The physical environments of neighborhoods might matter for family behavior as well, since for example exposure to environmental toxins may vary across areas and influence through obvious pathways physical health and mental health as well (for example if exposure to noise from highways or train tracks contributes to anxiety or depression).

The social environment of neighborhoods could be at least as important in understanding the ways in which MTO might affect the behavior and well-being of participating families. Epidemic models emphasize the power of peers to spread behaviors through learning, pure preference externalities (individuals enjoy imitating their peers), stigma effects (the negative signal from anti-social behaviors declines when more people do them), and physical externalities (e.g., higher rates of crime reduce the chances of getting arrested) (see Cook and Goss, 1996, Glaeser and Scheinkman, 1999, Manski, 2000, and Moffitt, 2001). Some epidemic models predict peer influences on behavior that varies with the prevalence of the behavior within a community, which can lead to non-linearities in peer effects or "tipping points" (Jencks and Mayer, 1990, Crane, 1991). In this case the overall volume of anti-social behavior could be reduced by ensuring that no community has a prevalence of high-risk families above the relevant threshold. Collective socialization models concentrate on the way adults in a neighborhood influence young people who are not their children, for example by acting as role models (Wilson, 1987) or by enforcing shared values as in the "collective efficacy" model of Sampson, Raudenbush and Earls (1997); see also Coleman (1988). Neighbors can also be a source of information, such as about job opportunities, or about the consequences of engaging in risky or illegal behavior.

In addition new research in *behavioral economics* implies that moving to a less distressed area could improve youth outcomes by influencing future orientation, altruism and other aspects of decision making (Loewenstein and O'Donoghue, 2004).

Ultimately, these changes in family- and person-level mediators lead to the outcomes specified in the model: improvement in the family economic situation, improved health for adults, youth and children in the family, improved social well-being for youth and children, and improved educational achievement for youth and children. However the theoretical social-science literature raises the possibility that MTO moves to less distressed communities could also have adverse effects on adult or youth outcomes. For example *competition models* emphasize the competition between neighbors for scarce resources like grades or jobs, and r*elative deprivation models* focus on the psychological impact on individuals or self-evaluation based on relative standing in the community (Luttmer, 2005). These suggest the importance of also measuring future aspirations, peer and social networks, self-esteem and outlook, and discrimination.

It is important for the evaluation to collect information on these mediating factors as well as on outcomes, in order to make MTO as informative as possible for policy design. We wish to structure the impact analysis to shed light not only on the ultimate impacts of moving out of public housing but also on the causal mechanisms through which those effects occur. Therefore, in each domain we not only specify the outcomes of interest but also describe alternative pathways through which impacts on those outcomes might occur and the mediating factors along those pathways. Estimation of impacts on those mediating factors, as well as on final outcomes, can help to distinguish the causal mechanisms responsible for the estimated impacts.

A.2.1 Evaluation Overview

The MTO Demonstration

The Moving to Opportunity (MTO) demonstration was originally authorized in Section 152 of the Housing and Community Development Act of 1992. HUD is working closely with the National Bureau of Economic Research (NBER) to fulfill the mandates of this legislation in evaluating MTO's long-term effects. The demonstration combines Section 8 rental assistance with intensive housing search and counseling services that are intended to ease families' relocation to low-poverty communities and help them become self-sufficient. The legislation set the basic parameters of the demonstration as follows:

Family eligibility: Eligibility for the voluntary MTO program was limited to low-income families with children living in public housing or Section 8 project-based housing that were located in census tracts with poverty rates of 40% or more. Families were recruited by Public Housing Authorities (PHAs) in each city through fliers, tenant associations and other means. Everyone who was interested was given the chance to apply for the program. Local PHAs in each of the demonstration's five sites placed interested households on the MTO waiting list and provided group orientation sessions where they learned about the demonstration, its experimental nature, and the fact that they would be randomly assigned to one of three groups. Households that remained interested after the briefing were asked to both complete the extensive Baseline Survey and sign an Enrollment Agreement. Before being formally accepted into the program, families were screened for Section 8 eligibility. Almost all of the 4,608 households that signed up for MTO were headed by a female, nearly two-thirds of whom were African-American and most of the rest were Hispanic. Three-quarters of household heads were on welfare at baseline, fewer than half had graduated from high school, and on average these households had three children. While we do not know the immigration status of MTO participants, most speak English.

Site eligibility: The demonstration was restricted to no more than six **very large cities** with populations of at least 400,000 in metropolitan areas of at least 1.5 million people. Of the 21 cities eligible to participate in MTO, five cities were selected by a competitive process for the demonstration. They are **Baltimore, Boston, Chicago, Los Angeles, and New York;**

Demonstration operations: Local programs were created via grant agreements between the Secretary of HUD and **nonprofit organizations (NPOs)** to provide counseling

and services in connection with the demonstration, and **public housing agencies (PHAs)** to administer the rental assistance. The NPOs were funded to help pay for the costs associated with counseling participating families, assisting them in finding appropriate units, and working with landlords to encourage their participation in the MTO program. Local programs had to match federal counseling funds with funds from state or local public or private sources. PHAs received administrative funds for the increased number of Section 8 certificates or vouchers made available through the MTO program.

MTO research offers a unique platform to study the impact of government mobility interventions like MTO and, in turn, the effects of large changes in neighborhood context, on low-income families and children. Building on valuable lessons learned from the MTO interim evaluation, the final evaluation design has several features that will contribute to policy discussions as well as to research science. Most importantly, prior non-experimental studies of other mobility programs have been unable to determine conclusively whether observed outcomes were attributable to the impact of the program or simply reflected the characteristics of the families who chose to enter the program. The MTO demonstration is carefully designed to answer crucial questions about the impact of neighborhood on social and economic opportunity for very low-income families.

The mechanism that HUD has chosen to address these questions is an experimental research design involving the three-way random assignment of participants to:

➢ The **MTO experimental group,** which received certificates or vouchers usable only in low-poverty areas, along with counseling and assistance in finding a private unit to lease;

➢ The **Section 8 comparison group,** which received regular Section 8 certificates or vouchers (geographically unrestricted) and ordinary briefings and assistance from the PHA; or

➢ The **in-place control group,** which received no certificates or vouchers and could continue to receive project-based assistance.

Interim findings from MTO.[4] HUD previously sponsored the MTO "interim" evaluation that was designed to measure MTO's impacts on outcomes of participating families measured 4 to 7 years after enrollment in the MTO demonstration. The key findings

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[4] See, Orr, L., J.D. Feins, R. Jacob, E. Beecroft, L. Sanbonmatsu, L. F. Katz, J.B. Liebman, and J.R. Kling (2003). Moving to Opportunity Interim Impacts Evaluation. Washington, DC: U.S. Department of Housing and Urban Development. Also, see Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz (2007) "Experimental Analysis of Neighborhood Effects." *Econometrica*. 75(1): 83-119; Kling, J. R., J. Ludwig, and L. F. Katz (2005) "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment." *Quarterly Journal of Economics.* 120(1):87-130; Ludwig, J. and J. R. Kling (2007) *Journal of Law and Economics;* Sanbonmatsu, L., J. R. Kling, G. J. Duncan and J. Brooks-Gunn (2006) "Neighborhoods and Academic Achievement: Results from the Moving to Opportunity Experiment." *Journal of Human Resources*.

from the interim evaluation are summarized here as they set the stage for measures to be collected in the long-term.

Among the households assigned to the Experimental group, 47 percent used a MTO voucher to relocate to a low-poverty Census tract, while 62 percent of those assigned to the Section 8 group relocated through MTO. The explicit goal of MTO was to help move families into less economically distressed communities, and by this measure MTO was successful. One year after random assignment families in the two MTO treatment groups live in Census tracts with average poverty rates 11-13 percentage points (25-30%) below those of the Control group. The gap declines somewhat over time in part because of subsequent mobility among all groups. But even 6 years after random assignment, the treatment-control differences in tract poverty equal 7-8 percentage points (20% of the control mean), while the differences in cumulative exposure to neighborhood poverty (duration weighted averages) are 9-10 percentage points (20-25% of the control mean).

The interim MTO evaluation found that assignment to either of the MTO mobility groups led participating adults to feel safer and more satisfied with their housing and neighborhoods (Orr et al., 2003, and Kling, Liebman and Katz, 2007). The MTO intervention had no detectable effect on the labor market outcomes or social program participation of adults, but did improve adults' mental health as well as several important aspects of physical health such as obesity and health-risk behaviors including diet and exercise.

The effects of MTO on youth outcomes in the interim evaluation were different for males versus females, a difference that we aim to understand better with the long-term data collection being requested for clearance here. For a wide range of measures of risky and delinquent behaviors as well as school engagement, the interim MTO evaluation found that MTO improved outcomes for female youth but on balance had deleterious impacts on male youth. MTO had no detectable impacts for either boys or girls on academic achievement, as measured by Woodcock-Johnson tests in reading and math, or school dropout (Sanbonmatsu, Kling, Duncan, and Brooks-Gunn, 2006).

The MTO interim evaluation data has helped eliminate some hypothesized theoretical mechanisms through which neighborhood environments or mobility programs more generally might affect adults and youth. The disruption of moving *per se* does not appear to explain the gender differences in MTO effects for youth, as suggested by the fact that MTO's deleterious impacts on male youth do not show up until a few years after random assignment (Kling, Liebman and Katz, 2007; Kling, Ludwig and Katz, 2005). The gender difference in impacts is also not due to families with boys versus girls moving to different types of neighborhoods, since moves are generally similar across families with boys and girls. Brothers and sisters within the same families also appear to respond differently to the MTO intervention. One important implication of this last finding is that most existing theories of "neighborhood effects" on behavior cannot explain the gender difference in MTO impacts, since none of these theories in their standard form predict such sharp differences in how boys versus girls will respond to neighborhood environments.

Qualitative interviews conducted after the time of the MTO interim evaluation suggest that the nature of how boys and girls interact socially with peers may enable girls to more

successfully adapt to life in low-poverty areas. Girls were more likely to visit with friends on their porches or inside their homes, in part because some parents may place girls on a "shorter leash" than they do boys. Boys, on the other hand, often "hang out" in public spaces, which puts them at elevated risk for conflict with neighbors and police, and increases their exposure to delinquent peer groups as well as opportunities to engage in delinquent activities themselves (Clampet-Lundquist, Edin, Kling, Duncan, 2005).[5]

The final impact evaluation provides an opportunity to examine the same set of outcomes studied at the interim evaluation point in 2002, to learn more about how MTO impacts on participating families change over time. There are plausible reasons to believe that the effects of MTO moves on participants' behaviors may increase over time as families become more socially integrated into their new lower-poverty communities, and learn more about how to take advantage of new schooling or work opportunities available to them in these areas. The MTO final evaluation also provides a chance to collect additional, more detailed information compared to the interim MTO evaluation on certain outcome domains that are important for public policy and the interim MTO findings suggest could have been affected by the intervention, such as mental health or crime victimization.

The final impact evaluation is designed to exploit the randomized experimental design of the MTO demonstration to best address the key questions described below:

1. What are the long-term effects of MTO on participating families, and how do these impacts evolve over time?

> If differences in average neighborhood characteristics across MTO groups persist over time, MTO's effects on well-being and behavior may increase over time as ties to old social networks diminish and families become more socially integrated into their new communities and learn more about how to take advantage of new neighborhood institutional resources. Social integration itself might require families to learn new modes of dress, language or interactions to "fit in."

2. What are the long-term effects of MTO on those who were young children at baseline?

> A growing body of research suggests the malleability of behavior may decline with age (Becker and Murphy, 2000; Shonkoff and Phillips, 2000; Carniero and Heckman, 2003; Knudsen et al., 2006). While MTO children who were very young at randomization were too young to provide meaningful measures for outcomes like dropout and risky behavior in the interim study, enough time will have elapsed for our long-term study to measure

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[5] This collaborative project between Jeffrey Kling, Greg Duncan and Kathy Edin started in 2003 to conduct semi-structured open-ended interviews of 233 households in Baltimore and Chicago. One of the main purposes of this effort was to generate new hypotheses about the mechanisms through which neighborhoods affect youth, and particularly about drivers of differences in effects by gender. Interviews were conducted with the mothers of youth, and with youth themselves; another component of the study included classroom observations and teacher interviews for younger children. The households were a random sample from the two sites including members from the experimental, Section 8, and control groups. Interviews were completed with 83% of households. At the time of this proposal re-submission analysis of the transcripts from these interviews is still in progress.

such impacts. This is important because these behaviors may be more susceptible to social policy influences than are outcomes such as achievement test scores.[6]

3. Do neighborhood effects vary non-linearly with neighborhood characteristics over the long run?

This hypothesis implies the existence of "tipping points" in neighborhood composition, whereby a critical mass of compositional factors is needed to achieve positive neighborhood effects on the well-being of neighborhood residents (Jencks and Mayer 1990). Evidence for tipping points would be important because it would mean that policies to resort disadvantaged families across neighborhoods could be fine-tuned to control the number of such families that are introduced into targeted neighborhoods of specified composition in order to affect not only the distribution of outcomes across neighborhoods, but also to minimize the prevalence of negative outcomes in the population as a whole. Note that evidence on the existence of tipping points is also directly relevant to determining the likely impacts of the MTO intervention on the outcomes of other people who live in the neighborhoods into which MTO families are moving, which for a variety of reasons cannot be estimated directly in this intervention.[7]

4. What are the mechanisms through which MTO affects long-term outcomes?

The long term evaluation is an opportunity to learn more about the mechanisms through which MTO affects the behavior and life chances of participants. One particular focus will be to measure the same mediating mechanisms hypothesized by the interim MTO evaluation to affect behavior, to determine whether these theories are better able to explain the behavioral patterns of MTO families over the long term as they become more socially integrated into their new communities. We also seek to better understand differences in MTO program impacts on male versus female youth, as this may inform gender differences in the educational and economic progress made over time in the U.S. as a whole among African-American males and females. Our key hypotheses for the source of these gender differences in MTO impacts include the following:

H1: Males more than females retain social ties to old neighborhoods and peer groups.

H2: MTO reduces domestic violence victimization and sexual abuse, or changes sex roles and relationships, which is more beneficial for female than male youth.

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[6] Outcomes like dropout rely mostly on the behavioral decisions of MTO youth. In contrast, cognitive achievement test scores are the result of a more elaborate "production function" that includes other inputs such as parent behavior, school quality and academic track assignments. Moreover, economists usually conceive of test scores as a function of the child's entire history of school and out-of-school inputs, so pre-randomization exposure to less developmentally productive inputs may affect the degree to which differences in post-randomization inputs modify test scores.

[7] One problem is that MTO families choose, rather than are randomly assigned, into their new neighborhoods, which makes causal inference complicated – their neighborhoods could have different trends in outcomes compared to other areas for reasons entirely unrelated to the in-migration of MTO families. In addition by design MTO avoided clustering new families in particular areas, and so any MTO impact on destination areas would be too small to be detected with available data sources. Finally, confidentiality concerns rule out the collection of more targeted information on people who live very close to MTO families.

H3: Neighborhood institutions and schools in particular, are better able to deal with the problems of disadvantaged female than male youth.

H4: Parental investments of time, money, and attention in youth in response to mobility will favor females relative to males.[8]

H5: Effects of neighborhood mobility on basic decision making processes, if whatever makes females better able than males to delay gratification more generally (for example Silverman, 2003) also makes decision-making processes by females more responsive to environmental influences.

H6: Females more than males adapt better to changes in their relative social position or competencies and tend to sort themselves into more pro-social peer networks.

H7: Females more than males respond to positive role models that may be present in their new neighborhoods and schools.

The Final Impact Evaluation

Background. While the number of people living in concentrated poverty[9] neighborhoods declined somewhat during the 1990s, it is still the case that in 2000 there were nearly 8 million people in America living in high-poverty urban neighborhoods, more than the number living in such areas three decades earlier in 1970 (Jargowsky, 2003). A wide range of government policies contribute to the concentration of poor people in high-poverty areas, including decisions about whether to enable suburban municipalities to zone out low-cost housing, the vigor with which government agencies enforce fair-housing or anti-discrimination laws, and the ways in which government agencies provide housing assistance to low-income families. For instance some of the nation's most notorious public housing projects such as the Robert Taylor Homes or Cabrini-Green in Chicago contributed to dramatic concentrations of poverty by constructing high-density housing for poor families, which in turn became so dangerous few working- or middle-class families were willing to live nearby.

It is well known that rates of employment, school dropout or criminal involvement vary dramatically across neighborhoods in the U.S., which raises the possibility that neighborhood environments – and the government policies that contribute to the concentration of poverty – may affect the well-being of poor families. However determining whether and how neighborhood context exert causal influences on the long-term life chances of poor families remains difficult. A large non-experimental literature finds that neighborhood characteristics do appear to predict adult and child development outcomes, even after conditioning on

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[8] For example data from the New Hope anti-poverty experiment revealed that mothers preferred to use resources to help sons over daughters to in part compensate for the greater threats posed to boys from living in disadvantaged neighborhoods (Romich 2000). In this case improvements in neighborhood conditions may cause parents to reduce the resources that they allocate to boys. Other reallocations of resources are possible depending on how neighborhood mobility affects the marginal productivity of parental investments in sons and daughters, and how parents value equity versus efficiency in allocating resources across children; see Becker (1991).

[9] We follow convention in the social science literature and define "concentrated poverty neighborhoods" as census tracts with poverty rates of 40 percent or more.

observable individual or family attributes (Sampson et al., 2002; Kawachi and Berkman, 2003; Leventhal and Brooks-Gunn, 2000; Ellen and Turner, 2003). However these studies essentially compare behavior and life outcomes of low-income people who have chosen to live, or been able to secure residence in, low-poverty neighborhoods with those of other low-income families who are living in higher-poverty areas. Such comparisons potentially confused the effects of neighborhood with the effects of the characteristics of families who lived in those two types of residential areas. Put differently, these studies may confound the effects of neighborhoods *per se* with the effects of unmeasured or difficult-to-observe individual- or family-level attributes that are associated with residential selection and location.

Motivated by the encouraging findings from the Gautreaux mobility program in Chicago (Rubinowitz and Rosenbaum, 2001), HUD launched the Moving to Opportunity (MTO) demonstration to support direct analysis of neighborhood impacts by employing an experimental design (random assignment) to measure the effects of neighborhood without these confounding factors. To fulfill Congress' mandate in the original authorization of MTO, HUD commissioned the interim MTO evaluation that 4-7 years after random assignment measured outcomes across a wide range of domains as well as candidate mediating mechanisms. This interim MTO evaluation serves as the starting point for the long-term MTO evaluation proposed here, which will measure outcomes 10-12 years after random assignment.

Under contract with HUD to carry out the long-term MTO evaluation, NBER has proposed an evaluation plan that: focuses on the same key outcome domains as in the interim MTO study, in order to learn more about how MTO impacts change over time; expands measurement of policy-relevant outcome domains that were not intensively measured in the interim study but on the basis of both social science theory and other interim MTO findings we have reason to believe might be affected over the long term (such as mental health or biomarkers indicating risk of long-term disease); and includes items designed to measure new hypotheses about mediating mechanisms suggested by the interim MTO findings (such as those relating to the gender difference among youth in MTO impacts). The sample frame for the final impact analysis consists of one adult from each of the 4,604 households in the MTO experiment and up to 6,311 youth ages 10-20 (up to three per MTO household) who resided with MTO families at time of enrollment.

To carry out the survey data collection, NBER is partnering with the Survey Research Center (SRC) of the Institute for Social Research at the University of Michigan, one of the nation's leading social science research firms with extensive experience carrying out high-quality large-scale household surveys with high response rates.[10] SRC fieldwork features rigorous

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[10] SRC consistently achieves high response rates, even for longitudinal studies that focus on low-income urban populations similar to the MTO study group. For example for the African-American Health study SRC achieved re-interview response rates of 90-95% for each follow-up survey from 2001 to 2004 for a sample of nearly 1,000 African Americans drawn from two areas in St. Louis (inner city and nearby suburbs). For the Women's Employment Study SRC achieved a 92% follow-up response rate among a sample of 753 welfare recipients in Flint, Michigan. For the Panel Study of Income Dynamics, an ongoing longitudinal study that has collected information on more than 65,000 individuals spanning more than 36 years of their lives, SRC has

interviewer training, extensive field quality control monitoring, and careful data post-processing, all of which will be implemented in the MTO long-term follow-up survey.[11] The impacts of both the MTO experimental treatment and regular Section 8 assistance will be estimated for a wide range of outcomes in the domains specified above. Data for this analysis will come from a combination of sources, including interviews with heads of household and with youth ages 10-20, achievement tests administered to youth by the University of Michigan's Institute for Social Research, and extraction of data from administrative records of earnings, welfare benefits, housing assistance, student tests, and involvement with the criminal justice system.

Policy Context. While a majority of Americans agree that the government should provide housing assistance to low-income families,[12] what form should this assistance take? One possibility is for the government to directly provide housing units to low-income families, a strategy that began in earnest with the U.S. Housing Act of 1937 and led to a system of public housing that peaked in size in the mid-1990s at around 1.4 million units (Olsen, 2003). An alternative approach is for the government to subsidize low-income households to rent housing in the private market – a type of "tenant-based" subsidy that over time has accounted for a growing share of all new federal commitments for low-income housing (Quigley, 2000).[13]

In addition to their impacts on the quality of housing consumed by low-income families, housing policy decisions may have important implications for their neighborhood environments as well. By providing families with more choice over where they live, recipients of tenant-based subsidies on average wind up living in lower-poverty tracts than do families with project-based subsidies (Newman and Schnare, 1997; Khadduri et al., 1998; Devine et al. 2003; Olsen, 2003).[14] Other housing policy decisions that affect residential outcomes include choices about where to locate project-based units, and how to fund and operate tenant-based subsidy programs. Of particular concern to the public has been the sub-set of project-based housing in very high-poverty areas (Kotlowitz, 1992, Jones and Newman, 1998). In fact the 1998 Housing Act requires local public housing authorities (PHAs) to convert "distressed" public housing to vouchers, and makes it easier for PHAs to convert other projects to vouchers as well. If neighborhood context exerts an independent

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achieved response rates of 95% or higher for each wave. And in addition to carrying out the NCS, SRC carried out the NCS follow-up, which re-interviewed original respondents a decade after the original interviews. A response rate of 88% was obtained in that follow-up survey despite the fact that active household tracking was not used to monitor the movement of NCS respondents over the decade between the two surveys.

[11] SRC maintains an active national field staff of more than 600 interviewers, including a bilingual interviewing staff. Interviewers for MTO would receive up to 6 days of training in Ann Arbor that would introduce the study and cover refusal aversion / conversion procedures; confidentiality; consent; administration of the questionnaires; and sample management and reporting time and progress.

[12] A 2001 survey by NPR, Kaiser Family Foundation and Harvard found 75% of respondents support more spending "for housing for poor people" by government.
www.npr.org/programs/specials/polls/poverty/staticresults3.html

[13] There is a third possibility: government subsidies to private providers of specific housing units, although from the perspective of MTO these impose locational constraints on recipients as do other project-based subsidies.

[14] In addition, a number of studies provide suggestive evidence that per-unit costs may be lower for tenant-compared to project-based subsidy programs (Olsen, 2000; Shroder and Reiger, 2000; GAO, 2001).

effect on family behavior, housing programs could have cascading influences on other aspects of family's lives such as earnings and health, as well as the development of their children. This is important because the goal of federal housing policy since at least the Housing Act of 1949 has been to improve family well being, broadly defined.

The MTO final evaluation will contribute to a variety of questions facing policy makers in the arenas of poverty, housing assistance, and education. The basic experimental contrast between project-based assistance and tenant-based vouchers addresses a fundamental policy choice that first arose in the 1970s and has not been fully resolved in the intervening decades. Over that period, there has been increasing concern that the high concentration of poverty associated with public housing projects may adversely affect resident families. Partly for that reason, a large part of the expansion of housing assistance since 1980 has taken the form of certificates and vouchers that provide subsidies to obtain housing in the private market.[15] Absent compelling evidence of adverse effects, however, we have continued to maintain the existing stock of project units. This evaluation will cast new light on the desirability of replacing some of these units with rental assistance in the private market.

Within this broad policy issue, there is a question as to whether it is sufficient to move families out of projects into the surrounding community or whether it is necessary to change their environment substantially. Left to their own devices, public housing tenants who receive vouchers will tend to move to areas that still have relatively high rates of poverty. It is not clear whether such moves are stable or sufficient to overcome any deleterious effects associated with project-based assistance. Long term effects examined through the experimental contrast between the effects of regular Section 8 vouchers, which place no restriction on where the recipient moves, and those of the MTO experimental vouchers, which require that the recipient move to a low-poverty area, speak to this issue.

The experiment is not, however, simply a test of two specific assistance programs. More fundamentally, it seeks to measure the effects of neighborhood on the lives of low-income families with children and, by extension, the potential benefits of policies designed to disperse those families into low-poverty areas. What we learn about the effects of neighborhood on the lives of low-income families may also speak to the desirability of policies that seek to change the neighborhoods in which these families currently live. If the truly comprehensive changes induced by MTO have little or no effect on outcomes, then the more modest changes that can be made in their existing neighborhoods seem unlikely to have the potential for meaningful effects. Alternatively, large estimates of neighborhood effects may indicate that important changes in individual outcomes can be brought about by community influences. Specific mechanisms may also be identified that will help target issues that can be directly addressed in today's high-poverty communities, such as the physical safety of areas in which children play or the availability of after-school or summer programs to encourage constructive activities over risky behaviors.

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[15] Between 1980 and 1997, over 40 percent of the net growth in the number of assisted families resulted from increases in household-based assistance in existing housing (U.S. House of Representatives (1998), Section 15, Table 15-26).

Participant Data Collection for the Final Impact Evaluation. Clearance is being requested for:

> ➤ a survey instrument for female adult respondents
>
> ➤ a survey instrument for youth ages 10 to 20,[16]
>
> ➤ youth achievement testing using the Early Childhood Longitudinal Study math and reading assessments supplemented with questions from the NELS
>
> ➤ adult biomarker data collection of height, weight, blood pressure and dried blood spots, and collection of youth height and weight
>
> ➤ audio-taping to allow language assessments of adults and youth, and
>
> ➤ interviewer observations of the respondent's residence and neighborhood.

Exhibit 2 summarizes these and other components of the MTO long-term evaluation data collection effort.

Exhibit 2 Participant Data to be Collected for the MTO Long-Term Evaluation

	Adult	Youth
Survey sampling plan	One adult per household, female caregiver Up to 4,604 adults	Up to three youth per household ages 10 to 20 as of December 2007 Up to 6,311 youth total
Survey content	Household roster Housing and mobility Neighborhoods, networks Education Employment & earnings Income, public assistance Savings and assets Mental & physical health Decision making Relationships & parenting Reports on household outcomes	Neighborhood & social network Education and schooling Employment and earnings Risky behavior & behavior problems Mental and physical health Decision making Youth reports on parenting
Direct measurement	Height, weight, blood pressure	Height & weight
Test assessments	None	ECLS math and reading assessment
Biomarkers	Dried Blood Spots	

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[16] We exclude children under age 10 from the survey sample frame because most of these children will have been born after randomization into the MTO study. The possibility that MTO treatment assignment may affect fertility behavior means that the average background characteristics for children born after randomization may not be balanced across MTO groups, which would introduce the possibility of bias to our estimates.

	Adult	Youth
Language assessments	Audio taping of open-ended questions and reading passage for pre-selected random subsample	Audio taping of open-ended question and reading passage for pre-selected random subsample
Interviewer observations	Neighborhood walk-around	
Administrative data	Addresses, housing program participation, earnings, social program participation, arrests, post-secondary school enrollment, mortality	Elementary and secondary school records, post-secondary school enrollment, arrests, earnings, social program participation, mortality
Neighborhood indicators	Census	Census

The survey questionnaires[17] are provided in Appendix A (Final Survey of Adult Respondents) and Appendix B (Final Survey of Youth Respondents).

The Adult Respondent Survey. The final survey of adult respondents consists of a 75-minute interview with one adult per core MTO household. This adult is in most cases the female head of the MTO core family, as defined by the applicant during the Section 8 eligibility determination process. The respondents will be asked questions about his/her mobility, housing and neighborhood conditions, employment status and history, educational attainment, physical and mental health, and household composition.

In developing the final survey of households, we have drawn heavily on the MTO Interim Evaluation instrument and other existing studies and instruments including the National Health Interview Survey, the Panel Study of Income Dynamics, the Current Population Survey and surveys from the Project on Human Development in Chicago Neighborhoods. All of the items on mental health disorders, victimization and substance abuse have been drawn from the National Comorbidity Surveys (NCS) under the close guidance of Ronald Kessler, principal investigator of the NCS, Professor of Health Care Policy at the Harvard Medical School, and co-investigator of NBER's long-term evaluation of MTO. By drawing whenever possible on questions from the interim MTO study and national sources we will be able to compare impacts from the MTO interim to final evaluation, to use measures that have been extensively pre- and field-tested and proven significant in other research, and to have national data with which to compare the MTO results.

The adult survey also includes a series of questions on basic features of decision making. These questions are additions to items collected at the interim MTO survey, motivated by recent research in behavioral economics suggesting the possibility that MTO moves to lower-crime, lower-poverty neighborhoods could change future orientation, responses to risk, and even altruism by reducing exposure to neighborhood stressors and temptations (Loewenstein and O'Donoghue, 2004). In addition to these questions, a subsample of MTO respondents

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[17] Licensing agreements or request for permissions to use proprietary items are in progress for the Child Behavior Checklist (from Achenback System of Empirically Based Assessments http://www.aseba.org/products/cbcl6-18.html; Achenback and Edelbrock, 1981), the SF-36 questions on chronic health conditions (from Quality Metric Incorporated in Lincoln, Rhode Island), and the self-reported physical health thermometer (from the European Health Survey System: http://ec.europa.eu/health/ph_information/dissemination/reporting/ehss_en.htm).

will participate in a simple decision making exercise. The details of this nested decision-making exercise and resulting respondent payment is described in Section A.9. Developed in the experimental economics literature, this decision-making exercise is designed to elicit time preferences under conditions where respondents face real incentives to choose according to their underlying preferences. Our choice exercise is original to the MTO evaluation, but designed to be a simplified version of those employed in this literature.[18]

The Youth Survey. The final survey of youth will be administered to sample children between the ages of 10 and 20 as of December 2007. The youth survey will be 60 minutes in length for youth aged 13 to 20, and approximately 30 minutes for youth aged 10 to 12. The length of our proposed MTO youth surveys is comparable to several other large scale youth surveys including the National Longitudinal Survey of Youth 1997, National Study of Adolescent Health, The Project on Human Development in Chicago Neighborhoods, the National Co-Morbidity Survey, and the fifth grade follow-up of the Early Head Start evaluation. Our MTO youth surveys will cover education and schooling, employment behavior, social networks, ties to the neighborhood, mental and physical health, decision making, and risky behavior. As with the Adult Respondent survey, we have taken great care to select questions from the MTO Interim Evaluation and existing surveys whenever possible, ensuring that the questions are developmentally appropriate, have been successfully administered to similar populations, and for which national distributions are available.

Educational Achievement Testing for Youth. Sampled children ages 10 to 20 will be asked to complete an educational math and reading achievement test, as described in Appendix C.[19] The achievement assessment will be 45 minutes in length (approximately 20 minutes allocated to an assessment of math and 25 minutes allocated to an assessment of reading). Our primary measures of educational achievement will be derived from these reading and math tests administered directly to youth from MTO families. We will not conduct any testing of individual aptitude (such as IQ) as distinct from academic achievement.

For all children age 10 to 20 we will administer the reading and math assessments designed for the 5th and 8th grade follow-up waves of the U.S. Department of Education's ECLS study. We have chosen not to replicate administration of the Woodcock-Johnson-Revised (WJ-R) tests that were used in the interim MTO study because the test exhibited several limitations including evidence of important "interviewer effects" (test scores for MTO children varied systematically by who administered the test) and little variation in average test scores by age across MTO youth ages 13 to 18 (Sanbonmatsu et al. 2006). Donald Rock of the Education

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[18] See Holt and Laury (2002) who infer risk aversion coefficients from choices over lotteries. Harrison, Lau, and Williams (2002) and Coller, Harrison, and Rutstrom (2003) infer individual discount rates from choices over alternative streams of payments. Eckel, Johnson, and Montmarquette (2004) follow similar procedures in offering risk and time choices to low- and middle-income subjects.

[19] The ECLS-K direct child assessments were adapted from several copyrighted assessment batteries. Therefore, individual items from the direct child assessments are not available for review and are not submitted as part of this OMB request. A detailed description of the direct child assessment is available in the ECLS-K User's Manuals and Psychometric Reports (http://nces.ed.gov/ecls/kinderinstruments.asp).

Testing Service (ETS), a leading expert in the development of achievement assessments, is a consultant on our team and has been assisting us with the selection of appropriate new achievement assessments. Dr. Rock's recommendation was to employ the ECLS achievement tests for the long-term MTO youth data collection for the following reasons:

First, the ECLS tests are designed to more closely measure what children learn in school compared to other tests such as the Woodcock-Johnson, which measure achievement or aptitude more generally. Thus, the ECLS tests may provide a more sensitive measure of the degree to which MTO moves children into improved schooling and learning environments.

Second, the ECLS assessments, up to 5th grade, have had extensive pre-testing and piloting. This latter feature is important as it will decrease interviewer coding variation, which can contribute to interviewer effects. Pollack, Najarian, Rock, Atkins-Burnett and Hausken (2005) show that the tests adequately capture the content of school learning and have enough range in difficulty of items so that students distribute relatively evenly across the three second stage forms. The 5th grade test also has high internal consistency, which is measured by the correlation of achievement test scores of same children who take the test at multiple points in time. These reliability ratios range from 0.72 (on the lowest form) to 0.82 (on the highest form) for reading and 0.58 to 0.78, respectively, for math. .

Third, the 5th and 8th grade ECLS tests together will be appropriate for the wide range of ages covered by the youth survey sample for our long-term MTO evaluation (youth who will be ages 10 to 20 at the time). Because the ECLS tests are intended to be used with a nationally representative sample, there will be low risk of floor effects from administering the ECLS 5th grade tests to MTO 10 year olds. The risks of ceiling effects from administering the ECLS 8th grade test to MTO children ages 13 to 20 should be low as well in part because given their baseline socio-economic characteristics and previous research on the strong association between socio-economic background and student achievement, we expect the low-income MTO youth sample to be performing below grade level. In any case to insure against potential ceiling effects, the ECLS 8th grade test will be supplemented with a small set of math and reading items from the U.S. Department of Education's National Educational Longitudinal Survey 10th and 12th grade test. Donald Rock is assisting the NBER team with choosing the items that will best meet the needs of the MTO long-term evaluation.[20]

Fourth, the ELCS assessments are adaptive -- respondents first take a short "routing test" that then directs them to test forms of different difficultly levels – in order to reduce the time

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[20] There are two reasons why the ECLS 8th grade test will be supplemented with items from the NELS-88 10th and 12th grade tests rather than more recent testing through the Education Longitudinal Study. First, ETS constructed all the items for NELS as part of a contract with the National Center for Education Statistics so they may be released by NCES without infringing on copyright issues. Copyright issues may be further exacerbated because many of the items in ELS were also taken from other sources including the Program for International Student Assessment (PISA). Second, several of the ELS reading items that might have been appropriate for supplementing the MTO assessments are PISA reading items that tend to be based on long passages which would present a problem with MTO time constraints.

required to accurately measure a subject's academic achievement level.[21] Many other tests are good at determining whether or not a student is at a given grade level, but they are not good at distinguishing how far below or above grade level a student may be; the ECLS has good discriminating power across a wide range of ability levels. We hypothesize that many MTO children may be below grade level, and we want to have a test that can measure their specific achievement levels with some precision. If for instance children in the experimental group were performing at a level that was 2 years below their grade of school enrollment but control children were 4 years below grade, this would represent an important benefit from the MTO intervention – even though all children in both groups are still performing below grade level – because previous research documents that improvements in academic skill directly improves future life outcomes such as earnings (Murnane et al., 1995).

Fifth, the ECLS is administered to national samples, which provides a nationally representative sample against which we can benchmark the MTO results.

The ECLS 5th and 8th grade tests are also among the few existing achievement assessments that can allow us to measure youth achievement in both the reading and math domains within the time we have available (45 minutes) to assess MTO youth. The ECLS 5th grade reading and math achievement tests together on average take around 45 minutes, and are designed with a "discontinue" rule so that children are not asked to continue when they are not able to proceed through a progressively more difficult cluster of items. That is, the ECLS tests asks questions that get increasingly difficult as the test progresses, and as soon as children answer incorrectly to some specified number of questions in a row then they stop taking the test. The ECLS 8th grade reading and math tests do not have the same "discontinue" rule, and so preliminary data suggest that on average they have taken more than 45 minutes to administer to ECLS study participants. However our team is currently working with Donald Rock to develop a "discontinue" rule for ECLS 8th grade tests similar to those employed by the ECLS 5th grade test in order to keep the ECLS 8th grade assessments within 45 minutes for an average administration time.

Direct Measurement of Blood Pressure, Height, Weight

In part because the MTO interim evaluation found some changes in diet and exercise among some MTO adults and youth, as well as reductions in stress and improvements in mental health, we will complement the survey data collection described above with direct measurements of blood pressure, height and weight for adult respondents and of height and weight for youth respondents. These measurements will be taken in the home, using appropriate and up-to-date equipment, by interviewers well-trained in these procedures. We estimate that these measurements will take approximately 10 minutes to complete.

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[21] The ECLS 5th and 8th grade tests are two stage self-administered paper and pencil tests. The first stage is a short routing test so that the combination of age, grade and performance help determine the difficulty of the test (as compared to age or grade alone). This routing test is scored on-site by an assessor and is then used to direct children to the appropriate level for the second stage. The second stage 5th grade test is available in three levels and the second stage 8th grade test is available in two levels, where items in the highest level of the 5th grade test, for example, overlap with some items in the lowest level of the 8th grade test.

Measurement of **adult blood pressure** is motivated in part by the fact that this is an important health indicator. Elevated blood pressure (hypertension) is well known to be associated with increased risk of cardiovascular disease, and is particularly prevalent among African-Americans (two-thirds of the MTO program population is African-American). We believe MTO might affect blood pressure at the time of our long-term evaluation data collection given interim MTO evaluation findings of program effects on stress, weight, and activity patterns such as exercise and diet. Interviewers will use automated sphygmomanometers approved by the American Association for the Advancement of Medical Instrumentation Standard, accepted by the FDA as the national standard.

Measurement of **height and weight** is also motivated by findings of changes in obesity, diet and exercise for adults and youth in the interim MTO evaluation, together with public health concerns associated with the social costs of escalating rates of obesity. Obesity even in young children has been found to be predictive of later health problems. Moves to lower-poverty neighborhoods may reduce obesity through several mechanisms including lower incidence of depression and stress, behavioral changes such as increased physical activity, or different social norms about eating habits. Interviewers will bring portable equipment into the home to carry out these measurements.

Direct Measurement of Blood The interim MTO results suggest that around 4-7 years after random assignment the intervention reduced stress and obesity, and improved mental health, diet, and exercise in ways that may lead to long-term improvements in important health outcomes, including reductions in long-term disease conditions that impose great costs on society. Yet many of these key health gains generated by MTO may not yet be in evidence in the form of noticeable health symptoms, even 10-12 years after random assignment. For example if MTO improves diet and exercise, as the interim MTO evaluation suggests, the result may be reductions in the lifetime risk of cardiovascular disease. Because the average MTO adult will be about 45 years of age at the time of the long-term survey interview, it may not be possible to detect the prevalence of actual symptoms or adverse health experiences associated with cardiovascular health, such as heart attacks, through survey self-reports.

As a result direct measurement of blood samples from MTO participants will be important for assessing the interventions effects on the precursors to important long-term health outcomes such as diabetes, cardiovascular disease (CVD), or metabolic disease that may result from MTO-induced changes in health behaviors (diet, exercise), obesity, or stress. We will use dried blood spots (DBS) to measure a number of "biomarkers" that have been demonstrated to be highly predictive of these long-term health problems, specifically we will seek to measure total cholesterol (TC), high density lipoprotein (HDP), glycosylated hemoglobin (HbA1c), C-reactive protein (CRP), cortisol, and Epstein-Barr virus (EBV) antibody levels. These biomarkers are affected by health-risk factors that have been shown to be affected by MTO in earlier evaluation rounds, including environmental toxicity, dietary quality, levels of physical activity, and psychosocial stress. In addition each of these biomarkers affect long-term health outcomes though well-known physiological pathways, and so the biomarkers representing these pathways provide reliable and valid measures of exposure to important health risks that are causally proximate to important health outcomes.

Because the long-term social and public costs of poor health has emerged as an important policy issue, collection of biomarker data of this type is becoming increasingly common in large national studies including for example, the National Study of Adolescent Health, the Health and Retirement Study, and several studies sponsored by the National Institute of Health. An additional benefit to MTO participants is that some of the results from blood measurement will be shared with them – such as total cholesterol levels.[22] Since MTO respondents will receive important health information as part of this biomarker measurement, the costs and benefits to participating in biomarker collection from the perspective of the MTO respondents themselves are similar to those of a regular visit to a primary care physician for a routine check-up.

Concentrations of *TC and HDL* are important clinical predictors of cardiovascular disease (CVD) risk. Higher total cholesterol—typically due to poor diet and/or low level of physical activity—is associated with greater risk for CVD. The interim MTO study's findings of experimental group impacts on diet and exercise would have important implications for long-term health outcomes if they translated into lower levels of TC, given that a 10% decrease in total cholesterol reduces the risk of heart disease by as much at 30%. HDL ("good" cholesterol) is protective, with higher concentrations independently associated with reduced risk of CVD. A 10% increase in HDL is associated with a 20-30% decrease in risk of CVD.

The assessment of *glycosylated hemoglobin (HbA1c)* provides an integrated measure of average blood glucose levels over the past two to four weeks and is relevant for understanding health problems associated with diabetes, a disease of glucose metabolism (we refer here to type II, non-insulin diabetes, which comprises approximately 95% of all cases). Higher blood glucose levels increase the proportion of HbA1c, and several clinical trials have demonstrated that HbA1c is the single best predictor of diabetes complications. HbA1c is also highly predictive of diabetes onset: In non-diabetic individuals with HbA1c of <5.6% at baseline, the annual incidence of diabetes is 0.8%. Diabetes incidence is 2.5% for individuals with HbA1c between 5.6 and 6.0%, and 7.8% for HbA1c greater than 6.0% (Edelman et al. 2004). The value of this measure for understanding MTO impacts on the long-term risk of diabetes is highlighted by previous research finding that HbA1c is a better predictor than is obesity of the onset of diabetes. Diabetes disproportionately affects minority populations, with the risk of disease for African-American men and women nearly twice that of European-Americans. Diet, physical activity, and obesity – all factors found to have been affected by MTO in the interim study – are major determinants of diabetes risk. Complications from diabetes include blindness, nerve damage, kidney failure, amputation, and increased CVD risk.

CRP is a biomarker of inflammation that is an important predictor of CVD and metabolic disease and is now being incorporated into clinical practice. Smoking, overweight, obesity, and stress – each of which may be affected by MTO -- are all associated with increases in CRP. Concentrations of CRP <1.0 mg/L are considered low risk, while concentrations >3.0 mg/L are considered elevated risk. Small increases in CRP are predictive of disease: individuals in the 2nd quartile of the CRP distribution are 2.7 times more likely to experience

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[22] Results on total cholesterol and HDL will be shared at the time of the interview by using instant measurement devices. Other results will be mailed back to the participant.

stroke or myocardial infarction (i.e., heart attack), and 2.8 times more likely to develop metabolic disease, than those in the lowest quartile (Ridker et al. 2003). EBV antibody levels, an indirect measure of cell-mediated immune function, has been shown to be among the strongest and most consistent immunological correlates of recent chronic stress. Conversely, stress management interventions and disclosure of previously repressed trauma have been associated with declines in EBV antibody levels. Concentrations of EBV can be assayed in dried blood spots.

We also plan to measure levels of the stress hormone cortisol for the long-term MTO evaluation given that concerns about crime was far and away the most important reason families signed up for the MTO program. The interim MTO evaluation did indeed find that the intervention reduced rates of crime victimization, which presumably is one important contributing factor to program impacts in the interim evaluation on adult self-reports of feeling stressed. Physiological stress systems in the body are designed to translate psychosocial and emotional stress into "physiological action," such as flight or fight reactions. In the long term, however, too frequent or chronic physiological stress activation negatively affect the functioning of stress systems and a wide variety of related physiological processes relevant to mental and physical health (Chrousos and Gold, 1992). Cortisol levels provide an important indicator of physiological stress system functioning, and predict the development of disease processes such as depression and obesity as well as other long-term health outcomes (Goodyer, Herbert and Tamplin, 2003; Rosmond, 2005; Chrousos and Gold, 1992).

TC, HDL, HbA1c, CRP, cortisol, and EBV can all be quantified in dried blood spot (DBS) samples. We are optimistic about our chances of obtaining a high compliance rate for MTO given the experience of prior research with compliance rates ranging from 75 to 98%, the experience of our senior research team that obtained DBS compliance rates as high as 99%, and given our survey subcontractor's (ISR) experience with biomarker collection.[23] We will also share with MTO respondents the results of their biomarker analyses, especially TC and HDL, which should further enhance compliance rates.

To collect a blood sample using the DBS method, the participant's finger is cleaned with an alcohol preparation, and then pricked with a sterile, disposable commercial lancet of the type commonly used in home pregnancy or HIV testing or by diabetics to monitor blood glucose. Up to five drops of whole blood are applied to standardized filter paper, where the sample dries and becomes stabilized. An additional drop of blood from the same finger stick can be placed into a portable "point-of-care" device for instant measurement, which can be used to report instant measures of selected health outcomes to MTO participants and thereby increase the benefits to MTO study members themselves. Dried blood spot samples are stacked and stored in re-sealable plastic bags, and then shipped to the laboratory for storage and analysis.

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[23] The Great Smoky Mountains Study samples around 1,000 youth ages 9-15 and obtained a DBS compliance rate of 75.2-80.7%. The Chicago Health, Aging and Social Relations Study obtained a DBS compliance rate of 97.9% for 200 respondents 50-67 years of age, while compliance rates for DBS equaled 80.1% for the National Social Life, Health and Aging Study (2,000 respondents 57-84 years of age, for which Thomas McDade, senior consultant on this project, was a co-investigator), and the Illinois Family Study pilot achieved a 99.5% compliance rate among 205 respondents 20-40 years of age (with which McDade and Duncan are involved). ISR has collected DBS and saliva samples for a number of studies, including the Health and Retirement Study.

Most biomarkers are stable at normal room temperature for at least two weeks. Interviewers are protected by wearing gloves during the procedure. Finger prick blood sampling provides a logistically feasible, minimally-invasive means for collecting physiological information in community-based settings.

Detailed protocols for collection of these biological samples are described further in Appendix D.

Audio-taping for language assessments. The interim MTO survey asked a variety of questions about discrimination by race, in order to learn more about whether or how racial discrimination influences the ability of MTO participants to take advantage of new job, schooling or other opportunities in their new communities. However address tracking data revealed that MTO moves to lower-poverty areas wound up generating surprisingly little change in neighborhood racial composition. As a result for the long-term MTO evaluation we are interested in learning more about MTO participants' experiences with discrimination along *social class lines*, as indicated for instance by styles of dress or manner or language.

As part of our effort to learn more about social class discrimination in MTO, we plan to analyze MTO's long-term impacts on the language patterns of participants by taking advantage of the fact that our survey subcontractor, ISR, is already planning to digitally audiotape the MTO long-term survey interviews for their own quality control purposes. Specifically, in order to assess MTO's effects on language we will ask a random sub-sample of MTO adults and youths to verbally respond to two language measurement tasks at the end of their survey interviews, which will include either open-ended questions or a passage to read out loud. These will be recorded via audio tapes, transcribed and then analyzed for specific characteristics with respect to vocabulary, grammatical structure or pronunciation. Appendix E details data collection for language assessments and the power to detect effects on proposed language outcomes.

There is good reason to believe that MTO moves to lower-poverty areas may affect language patterns among participating adults and youth given that language is socially constructed and serves as a salient indicator of the speaker's socio-economic background (Labov et al 1968; Wolfram 1969). Previous research has found substantial variation in language patterns such as grammatical structure, phonological features and pronunciation across neighborhoods in New York, Philadelphia and other areas, particularly among African-American and Latino samples. MTO participants may have experienced strong social pressure in their baseline neighborhoods, related in part to issues about social identity in high-poverty areas.[24] MTO families who moved to lower-poverty areas may have experienced additional pressure on their language patterns because they may have moved to areas where mainstream English is more common. This type of pressure on MTO movers could lead to what Labov (1972) terms "linguistic insecurity" or even "linguistic self-hatred." MTO moves might even lead to "hypercorrection" (for example: "Whom did you say was calling?"), or to the phenomenon of

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[24] In 1873 Gabriel Tarde argued: "It appears to me almost beyond dispute that language is a phenomenon of imitation … the acquisition of foreign words by fashion and their assimilation by custom, the contagion of accent, the tyranny of usage in itself, suffices to show at one glance its imitative character" (cited in Labov 2001, p. 23).

"code switching" when individuals alter their language patterns to respond to specific social contexts.[25]

Previous research suggests differences in language patterns across people serves as the basis for discrimination in both housing and labor markets. For example in a telephone audit study of landlords in Philadelphia in 1999, the likelihood of speaking with an agent and being told a rental property was available was affected by the language patterns of the caller. White males speaking White Middle-Class English (WME) were more likely to speak with an agent and be told about a property than were people speaking Black Accented English (BAE, defined as "standard English with a black pronunciation of certain words") and especially those speaking Black English Vernacular (BEV, defined as "the combination of nonstandard grammar with a black accent, [which] signals lower-class origins" (Massey and Lundy, 2001; Fischer and Massey, 2004).

MTO impacts on language may also help us better understand the gender differences in MTO impacts on youth that were documented in the interim MTO evaluation. Prior socio-linguistic research has documented gender differences in language patterns, and in particular that females are generally more likely than males to use "mainstream" language. This could help explain why female youth in MTO seem to adapt better to their new, lower-poverty neighborhoods compared to males, consistent with findings from the interim MTO qualitative research that "Non-dominant cultural capital skills (e.g. use of language) that male youth learned in their high-poverty neighborhoods may have isolated them through police harassment or through fear and misunderstanding and may have led to maladaptive behavior from the mainstream when they moved to lower-poverty contexts" More generally those MTO participants who were very young at the time of random assignment, and who serve as the key youth survey sample for the long-term evaluation discussed here, may be particularly susceptible to changes in cognitive outcomes including language skills. Language and speech patterns have been shown in previous research to be highly predictive of subsequent achievement in school.

Interviewer observations of residence and neighborhood. During the interim MTO study, interviewers documented observations of the interior of the respondent's housing unit (noise level, carpeting, peeling paint, etc.) and exterior of the respondent's home (type of building, physical condition, bars on windows, etc.) These interviewer observations of the residence will be repeated for the long-term survey. In addition to the observations of the respondent's residence, SRC interviewers will conduct short "walk-arounds" in the MTO respondent's neighborhood before conducting the long-term survey interviews. The interviewers will be asked to complete checklists for various indications of neighborhood physical and social conditions. The checklist is a subset of items that were developed for walk-arounds by interviewers as part of the Project on Human Development in Chicago Neighborhoods (PHDCN). The protocols developed for PHDCN were also conducted by SRC.

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[25] An adult respondent in one study described his own experiences with adapting his language to new social settings: "Ultimately I somehow learned to be polylingual and to become sensitive linguistically in the way that animals are able to sense the danger of bad weather" (Lippi-Green, 1997, p. 192).

For the PHDCN, interviewers walked around the 4 streets surrounding the PHDCN respondent's housing unit before the survey interview and completed a check-list of 102 survey questions about the social and physical characteristics of the neighborhood. These questions include for instance whether people on the street reacted to the interviewer in a friendly way or with suspicion, whether there are signs of gang graffiti or litter in the streets, and the presence or absence of different types of institutions such as liquor stores or commercial banks. On average these PHDCN walk-arounds took 78 minutes to complete, and the results of the PHDCN walk-around neighborhood observations wind up being highly predict of how neighborhood residents themselves report important features of the neighborhood social environment such as "collective efficacy" that has been demonstrated to be important in explaining variation in neighborhood crime rates and other outcomes (Sampson et al., 1997). Communities with high collective efficacy, defined as mutual trust among residents and a willingness to intervene for the collective good (Sampson et al., 1997, Sampson, 2003, Browning and Cagney, 2003), may be better able to monitor and deter anti-social behavior by residents (Sampson et al., 1997), and address neighborhood environmental risk factors such as abandoned buildings and graffiti (Browning and Cagney, 2003). Put differently, these neighborhood walk-arounds have the potential to provide key information about how other neighborhood residents who are not MTO program participants experience the neighborhood, which will serve as an important complement to the MTO participant self-reports about how they experience the neighborhood. Differences in how MTO families and other families experience these areas would be informative about, among other things, the degree of social integration of MTO participants into their communities.

Because of budget and time constraints we will not be able to have SRC interviewers devote a full 78 minutes to conducting walk-around check-lists of community social and physical characteristics. As a result we have conducted our own original re-analysis of the PHDCN walk-around data and find that having interviewers report on just a sub-set of the full PHDCN check-list (around 80 of the 102 items) for just a single street (rather than 4 streets) around the respondent's address is nearly as informative as the full-scale walk-around data collection conducted by the PHDCN (Bader, 2007). This scaled back version of the walk-around can be conducted in 13-15 minutes.

Participant Data Collection Procedures. NBER's contractor for survey data collection, the University of Michigan's Institute for Social Research, has designed data collection procedures to coordinate the various parts of this effort. The **Adult respondent survey** will be administered either in person by trained interviewers, using the Blaise Computer-Assisted Personal Interviewing (CAPI) system on a laptop computer, or over the phone also using computer-assisted interviewing technology. The survey will be administered in the respondent's home or over the phone to maximize survey response rates for a fixed budget, with the interview session scheduled at the respondent's convenience. As described in Section A.11 below, the in-person interview technology will permit the respondent to listen to the questions using a headset and self-administer sensitive questions.

The **Youth survey** will be conducted in conjunction with the administration of achievement tests. Youth will be interviewed and tested in their homes, coordinated with the household adult survey whenever possible. Interviewing and testing in households where both adults

and youth are in our sample frame will be conducted by sending a team of two trained interviewer/testers to the home together. The purpose of coordinating the youth and adult data collection is three-fold: to reduce the degree of intrusion and time burden; to ensure that the parent is home at the time of the youth data collection (for reassurance); and to occupy parent and youth separately so that their interview and testing sessions can be separately completed (so that the parent does not influence the youth's answers or performance).

A.2.2 Purpose of the Data Collection

As discussed above, prior studies of mobility programs have been unable to demonstrate whether observed outcomes were the result of program impacts, or instead due to the characteristics of the families who chose to enroll in the program. The MTO study has been carefully designed to allow comparison of well-matched groups of families in three different locations: public housing in high-poverty areas; private housing in moderate-to-high poverty areas; and private housing in low-poverty areas. The purpose of the final MTO evaluation data collection is to support and extend the interim research on MTO families. While the interim MTO evaluation measured program impacts 4-7 years after random assignment, with the final evaluation we seek to determine the impacts of moving out of public housing in high-poverty areas over the *long term,* which is ultimately of the greatest policy importance. The proposed data collection activities will provide reliable measures of a broad range of outcomes; impacts on these outcomes will be estimated for both the MTO experimental group, who moved to low-poverty areas, and the Section 8 comparison group, who were free to move to any area (but who primarily moved to moderate-to-high poverty areas). In both cases, the impact of moving will be measured relative to the outcomes of the in-place control group, who initially remained in public housing.

A.2.3 Who Will Use the Information

The primary beneficiary of the final evaluation data collection will be HUD, which will use the information to assess longer-term effects of MTO for families who have been in the demonstration between ten and twelve years. These data will begin to answer HUD's questions about impacts in the domains of housing, employment and earnings, cash assistance, educational achievement, health, and delinquency and risky behavior, for the families assisted under the demonstration program. Evaluation contractor NBER will produce a Final Report for the final MTO evaluation in October 2009.

Secondary beneficiaries of this data collection will be those in the social science and policy research community who have expressed interest in the MTO demonstration and in working with the MTO data. The survey data collected for the interim MTO evaluation have been made available to several research teams for secondary analysis through HUD subject to standard confidentiality provisions. Specifically these data have been analyzed by researchers at Harvard, MIT, Princeton, the University of Pennsylvania, Northwestern University, the Urban Institute, the City University of New York, the University of Illinois, the University of South Carolina, Indiana University, and Rutgers University. The NBER team is also currently working with HUD to discuss ways of creating a public use file (PUF) version of the interim MTO survey data that would be constructed in a way to make identification of individual MTO respondents impossible, which could then be disseminated

more broadly by reducing the time burden on HUD to review and monitor applications for access to the MTO data.

Findings from previous waves of MTO research have also been presented at the annual research meetings of almost all of the major social science disciplinary organizations (economics, sociology, public policy, demography, epidemiology, and child development), as well as to policy audiences across the country at conferences and presentations sponsored by the Brookings Institution, the Administration on Children and Families at the U.S. Department of Health and Human Services, the National Academy of Sciences, the House of Representatives Ways and Means Committee, and the Joint Center for Poverty Research (among others). Additionally, previous MTO findings have been discussed in news stories in major media outlets such as the *New York Times*, *Washington Post*, *Wall Street Journal*, and television programs such as NBC Dateline.

Ultimately, these data will benefit researchers and policy analysts in a wide range of areas. The effect of neighborhood context on the well-being of low-income families is likely to manifest itself in numerous ways, and may be relevant to a broad array of public programs. This project offers the first opportunity to obtain reliable measures of these effects. The long-term indirect benefits of this research are therefore likely to be substantial.

A.2.4 Instruments, **Item by Item Justification**

In this section, we present our justification of these instruments and their contents. Two survey instruments have been developed for the final evaluation, because of the interest in measuring impacts on adults and youth. The household (adult) instrument has several sections concerning the respondent (head) and the household, including housing consumption and mobility, neighborhoods, education, employment, income and assistance, savings and assets, decision making, mental and physical health modules, parenting and decision making. It also contains a Parent-on-Youth module (to be administered if the parent has a child ages 10-20 who is in our MTO long-term youth survey sample frame), and a module designed to obtain updated information about core MTO household members. Finally, the household instrument contains a member roster and an update on secondary contact information.

The youth instrument contains sections on education, employment and earnings, delinquency and risky behavior, mental and physical health, neighborhood, decision making, and family interactions. The youth instrument has been designed to focus on the topics best reported by the children and youth themselves. The youth survey for the youngest of this group, youth aged 10 to 12 will exclude modules on risky behavior, conduct disorder, and several of the modules on other mental health disorders. The contents of the Parent-on-Youth module have been coordinated with the youth instrument so that they focus on topics better reported by the parent, on topics where a uniform report is needed across all ages of sampled children, or on a topic where more can be learned by obtaining parent as well as youth reports. For example, parent reports of monitoring (such as reporting on whether she knows her child's close friends) can often be confounded by youth willingness to communicate or disclose information with the parent. Obtaining youth reports of the same measure (does your mother know who your close friends are?) and comparing parent with youth reports can help reveal the extent to which youth

disclosure is affecting the measure of parental monitoring and also itself provides information about the nature of the parent-youth relationship.

Because of their length, the item-by-item justifications for these instruments have been provided in appendixes rather than within this section's text. Appendix F provides item-by-item justifications of the questions in the Adult Respondent Survey. It shows not only the content and reason for inclusion but also the source of the survey question. Appendix G provides the same item-by-item information for the Youth Survey.

The largest substantive revision to the long-term MTO adult and youth survey is the addition of several modules designed to measure clinically significant mental health disorders, i.e. those that meet the criteria for clinical significance as outlined by the APA's Diagnostic Standards Manual, version IV (DSM-IV). These survey modules were developed and validated from the National Co-Morbidity Replication Study, directed by Ronald Kessler, Professor of Health Care Policy at Harvard Medical School and one of the Co-Investigators of the MTO long-term evaluation NBER team.[26] The interim MTO evaluation found some impact on an indicator of general psychological distress. However the personal and social costs associated with mental health problems depend crucially on both the specific type and severity of the mental health disorder that people experience. So for public policy purposes there is great value in obtaining more detail in the long-term MTO evaluation about the degree to which MTO participants meet the clinical criteria for different specific mental health disorders.

The adult respondent survey includes modules designed to measure a number of clinical mental health disorders including major depressive disorder, general anxiety disorder, panic disorder, mania, post-traumatic stress disorder, intermittent explosive disorder, bipolar I disorder and bipolar II disorder. For youth aged 13 to 20, the survey includes modules designed to measure the same mental health disorders as for adults as well as conduct disorder, attention deficit disorder and oppositional defiant disorder. Several of these disorders are important in their own right as described in the justification column of these tables. Measuring each specific disorder is also important for deriving an overall assessment of experiencing "any disorder." This cluster of modules includes one module that serves as a screener for the disorders. The items in the screening section determine the extent to which a respondent appropriately skips out of the remaining modules. The NCS mental health items

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[26] These assessments will be identical to those used to assess the same disorders in the recently completed National Comorbidity Survey Replication (NCS-R; Kessler & Merikangas, 2004), thus providing a nationally representative benchmark to the MTO results. The diagnostic instrument used will be the version of the World Health Organization's (WHO) Composite International Diagnostic Interview (CIDI) that was expanded and updated for the WHO World Mental Health Survey Initiative (Demyttenaere et al., 2004). This instrument, which revised the original CIDI to make diagnoses according to the definitions and criteria of the DSM-IV (American Psychiatric Association, 1994), was recently approved by WHO as the official version of CIDI to be used throughout the world until the publication of ICD-11 in the year 2011. Kessler and Ustun (2004) present a detailed discussion of the CIDI, and Kessler et al. (2005) discuss analyses of onset, prevalence and comorbidity of DSM-IV disorders in the National Comorbidity Survey Replication. It is important to recognize that the CIDI is a fully structured diagnostic interview. This means that it is designed for use by trained lay interviewers rather than by clinicians and that clinical judgments are not required in scoring. However, methodological research has documented good concordance between diagnoses made by the CIDI and independent diagnoses made by clinical interviewers.

add a great number of questions to the MTO survey, but, importantly, because many respondents will skip the mental health modules altogether on the basis of negative answers to a short set of NCS screener questions, or else will answer just enough questions in the NCS mental health modules themselves to determine that they do not have the disorder, *average time of completion for these new mental health modules is **substantially** less than what would be expected if every respondent answered each item.*

NBER has devoted great care to accurately estimate the amount of time that the mental health modules will require of MTO respondents. From previous waves of the NCS study itself, we know the exact fraction of respondents in the nationally representative NCS sample who answer each of the mental health questions that we ask (versus skip over the question on the basis of previous survey responses). Using data from the interim MTO study, we estimate how the expected prevalence rates for mental health disorders for MTO adults and youth should compare to national samples. We then use these estimates to calculate the average number of NCS mental health questions that MTO respondents would complete in our mental health modules given the expected mental health disorder rates for our MTO sample.

A.3 Use of Improved Information Technology

Improved information technology will be used in this evaluation in three distinct ways:

➢ to assist the sample tracking and locating efforts;

➢ to measure certain outcomes through data abstracted from administrative records; and

➢ to facilitate collection of the survey data in standardized and accurate ways that also accommodate the confidential collection of sensitive data.

The administrative data collection will significantly reduce the burden on respondents to the household and youth surveys, as will the linkage of final evaluation data with data collected at interim and baseline for MTO families.

A.3.1 Information Technology and Sample Tracking

Tracking the MTO population prior to survey data collection will use several electronic databases as part of the passive locating effort, in order to minimize respondent burden. The searches of electronic data files include:

➢ Periodic comparisons of administrative databases; and

➢ Quarterly searches of electronic data maintained by outside vendors.

These methods do not involve direct contact with the MTO families; they are not intrusive and are effective ways to maintain current information on the MTO families. Each strategy is described briefly below.

Routine Checks of Administrative Databases. The Institute for Social Research (ISR) and the NBER research team will collect periodic extracts of tenant characteristics and

certification data (HUD Form 50058 data) for MTO families, from the Multifamily Tenant Characteristics System (MTCS) at HUD.

Searches of Other Electronic Databases. Passive tracking for the MTO sample also involves use of electronic databases. ISR and NBER will routinely check the National Change of Address Database (which catalogs U.S. Postal Service change-of-address notices). ISR and NBER will also check national consumer credit databases which list address information provided by creditors based on credit applications and ongoing account maintenance to obtain updated address information prior to and during the survey data collection period.

A.3.2 Information Technology and Administrative Data Collection for the Evaluation

The second way in which improved information technology will benefit the MTO final evaluation is through the collection of administrative data on selected outcomes and mediating factors. By accessing administrative information at the state, national, and local level, the evaluation contractor has been able to reduce the scope and burden of the survey instruments. Exhibit 3 shows the sources for collecting administrative data on employment and earnings, delinquency and risky behavior, social programs, health and housing assistance. The administrative data collection strategy seeks to provide a representative picture of the outcomes of everyone in all three of the MTO groups without collecting administrative data from the hundreds of jurisdictions in which MTO families have lived. The sampling strategy has been designed to balance the objectives of minimizing the number of data-sharing agreements that need to be established with local or state government agencies while at the same time minimizing bias and sampling variance for the final impact estimates. After determining the combinations of jurisdictions in which MTO participants have lived, these combinations will be stratified by the per participant costs of obtaining data. Strata with lower per person costs of obtaining data (i.e., jurisdictions with relatively more MTO participants) will be sampled at higher rates. For analysis, the data will be weighted by the inverse probability of selecting the jurisdictions.

Exhibit 3 Administrative Data Sources for Outcomes[27]

Domain	Outcomes	Data Sources
Employment and Earnings	Quarterly employment and earnings	State Unemployment Insurance (UI) wage records
		Census Bureau's Longitudinal Employer-Household Dynamics (LEHD)

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[27] HUD's access to these data sources depends on the data sharing agreements that govern them. In some cases, the state or local agency with which NBER makes an agreement will require that HUD co-sign the agreement, in which case HUD will have access to the data. In other cases, the agreement will be between only NBER and the state or local agency (some agreements expressly prohibit NBER from sharing the data), in which case HUD will not have access to the data. At this time, NBER cannot specify the data sources to which HUD will have access because all of the data sharing agreements have not been reached.

Domain	Outcomes	Data Sources
Delinquency and Risky Behavior	Arrests and court dispositions	State and local agencies that maintain data on adult and juvenile criminal records
Welfare and other Social Programs	Monthly TANF benefits, monthly Food stamp benefits, exits from cash assistance, date of TANF time limit, and TANF sanctions	State welfare agency records
Education	Primary and secondary school test scores, graduation, absences, grade retention, and disciplinary actions	School district student records
	College enrollment	National Student Clearinghouse
Health	Mortality and cause of death	National Center for Health Statistics' National Death Index
Housing Assistance	Receipt of housing assistance Amount of housing assistance	Multifamily Tenant Characteristics System (HUD) Tenant Rental Assistance Certification System (HUD)

Administrative Data Sources for Mediating Factors

Domain	Mediating Factors	Administrative Data Sources
Education	school quality	US Dept. of Education Common Core of Data on schools and Private School Survey
	school resources	
	crime rates for local area	FBI, local police departments, Census 2000, Bureau of Labor Statistics (BLS)
	unemployment rate	
	school attendance, grade completion, test scores	Local school district web sites, National School-Level State Assessment Database, and published data
Employment and Earnings	crime rates for local area	FBI, local police departments
	unemployment rate	BLS
Delinquency and Risky Behavior	crime rates for local area	FBI, local police departments
	school resources	US Dept. of Education Common Core of Data and Private School Survey
	school quality	

Domain	Outcomes	Data Sources
	student SES level	
Welfare and other Transfer Programs	unemployment rate	BLS
	receipt of public assistance in the local area	Census 2000
	crime rate in the local area	FBI, local Police Departments
Housing Assistance	Fair Market Rents (FMRs) for local area, by housing unit size	HUD

A.3.3 Information Technology and Survey Administration

The surveys for the MTO final evaluation will be administered using computer-assisted personal interviewing (CAPI) technology. The system that ISR will use is Statistics Netherland's Blaise interviewing system. Part of the youth questionnaire will be programmed to use Audio CAI, where the respondent can choose to either listen to the question with headphones or read from the screen and enter the answers directly into the interviewer's laptop. This will be done to assure confidentiality and encourage accurate answers to sensitive questions. The Blaise authoring system facilitates the entry of question text, response categories, and range and logic checks, to ensure data quality.

ISR's sample management system, SurveyTrak, allows interviewers, questionnaires, and survey assignments to be managed from one central site, with secure transmission of sample and completed interviews between the interviewer's laptop and the central office on a daily basis. As an added level of security, SurveyTrak removes completed interview data from the interviewer's laptop after it has been received and verified in the central office. Interviewers also have direct e-mail access to supervisors, allowing prompt responses to questions that arise.

A.4 Efforts to Identify Duplication

The purpose of the surveys for the MTO final impact evaluation is to obtain current information on the status and well-being of adults and youth in the MTO program population. Detailed information about key aspects of these respondents' educational achievement, employment and job skills development, physical and mental health, delinquency and risky behavior, and neighborhood ties is not available through any other source.[28]

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[28] Some basic information about some of these outcome domains is available from administrative data, as summarized in the table above, but the administrative records are silent about many key outcomes in each of

Duplication is also being avoided in this study because we will use data collected from families in the Participant Baseline Survey (and during the 1997 and 2000 canvasses) and the Interim Survey (conducted in 2002). These data will be combined with the data newly collected for the final evaluation. For example, there is no need to ask about a variety of personal characteristics or background factors for known household members, because these were covered at baseline.

The educational achievement data for this study, to be collected by testing sampled MTO children who are 10-20 at the time of our field period, represent an important complement to collection of administrative student-level school records. The advantage of administrative student-level school records is that they are longitudinal and so enable us to examine how MTO's impacts on children's academic achievement has changed since random assignment. The *disadvantage* of administrative records is that children in the MTO study are now living in over 300 school districts across the U.S. These school districts differ widely with respect to the specific achievement tests that they administer to children for their own pedagogical purposes (for example, use of the Iowa Test of Basic Skills versus the Stanford 9 versus some "home grown" state-specific achievement assessment). States also differ with respect to what grade levels are assessed in any given year. There are ways of standardizing the results of different types of achievement assessments (for example by converting results to national percentile ranks or "Z scores") and dealing with missing administrative test data for some children in some calendar years (if their grades were not tested), but the necessary adjustments may introduce additional noise to the data in any calculations that pool information across localities and over time. Administering achievement tests to MTO youth in the long-term survey sample frame will not provide us with longitudinal information on achievement, but will have the important benefit to the MTO final evaluation of providing us with a consistent standardized measure of reading and math achievement across MTO groups at a particular point in time.

A.5 Involvement of Small Entities

No small businesses or other small entities are involved as respondents in the proposed data collection effort. Respondents are all members of families participating in the MTO demonstration.

A.6 Consequences of Less Frequent Data Collection

HUD's original plan for the maintenance and evaluation of the Moving to Opportunity demonstration program was designed to minimize the frequency of data collection from participants while at the same time maintaining the longitudinal panel for a ten-year period. The plan involved significant participant data collection only in the baseline period, at the

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these domains. For example student-level school records will include information on grade retention and perhaps dropout status, but will in most school districts be silent about things like school marks (letter grades), school engagement, and curriculum choices and academic track placements that are typically not available from administrative student-level school records. Similarly, administrative UI earnings records provide information on total quarterly earnings, but do not provide any information about the number of hours worked (a key measure of labor supply by MTO participants), hourly wages (a key measure of individual worker productivity and access to good-paying jobs), or fringe benefits such as employer-provided health insurance.

mid-point of the observation period, and at the end. Sample tracking, primarily with passive methods would be used to maintain the panel in the intervening years.

This request is for the final data collection, to conduct the final impact evaluation. There are no plans in place for data collection of a similar scale for the MTO respondents. We do anticipate small-scale qualitative studies in the future with a sub-sample of MTO families.

A.7 Special Circumstances

The proposed data collection activities are consistent with the guidelines set forth in 5 CFR 1320.6 (Controlling Paperwork Burden on the Public, General Information Collection Guidelines). There are no circumstances that require deviation from these guidelines.

A.8 Consultation Outside the Agency

In accordance with the Paperwork Reduction Act of 1995, the Department of Housing and Urban Development (HUD) published a notice in the Federal Register announcing the agency's intention to request an OMB review of data collection activities for the MTO Final Evaluation. The notice was published on April 4 2007 in Volume 72, Number 64, pages 16382-83 and provided a 60-day period for public comments. A copy of this notice appears in Appendix H.

The MTO final impact evaluation design was developed and is being implemented with the assistance of National Bureau of Economic Research, the prime contractor. Key members of the NBER team include Drs. Larry Katz and Ronald Kessler of Harvard University, Dr. Jens Ludwig of the University of Chicago, Dr. Jeffrey Kling of The Brookings Institution, Dr. Greg Duncan of Northwestern University, Dr. Lisa Gennetian of The Brookings Institution and Dr. Lisa Sanbonmatsu of NBER.

HUD staff consults with the NBER team on the design at critical junctures in the study. The purpose of such consultation is to ensure the technical soundness and usefulness of the data collection instruments, as well as the accessibility of the data required from the MTO tracking system for carrying out the evaluation.

We have drawn on the expertise of a team of leading researchers who have agreed to serve as consultants on the project. In particular the design of health outcomes, including biological samples that we propose to collect (subject to final HUD approval), has been developed in close consultation with *Thomas McDade* and *Emma Adam* of Northwestern University. McDade is Associate Professor of Anthropology, Faculty Research Fellow of IPR, and Associate Director of Northwestern's Cells to Society Center on Social Disparities and Health. His research focuses on understanding health and human development in the context of social and cultural processes, with a particular emphasis on stress, and the integration of biomarkers into population-based social science research. He is the recipient of NSF's Faculty Early Career Development Award and a Presidential Early Career Award for Scientists and Engineers. Emma Adam is Assistant Professor of Human Development and Social Policy, Faculty Fellow of IPR, and a leading expert on the study of stress effects on

human behavior and health through the measurement of the stress hormone cortisol. She has received prestigious post-doctoral awards from the National Academy of Education / Spencer Foundation and the William T. Grant Foundation.

The NBER team has also engaged consultants *Donald Rock*, Senior Associate, Center for Global Assessment, Educational Testing Service, to advise on the final design of the MTO achievement assessments and adaptation of the ECLS 5th and 8th grade tests. And, John Rickford, Professor of Linguistics at Stanford University and a leading expert in sociolinguistics, specifically the relationship between language and ethnicity, social class and style, and language variation and change to advise on assessments of language.

HUD and NBER have also formed a Technical Review Panel (TRP) who have commented significantly on the design of this research: *Charles Brown*, Professor of Economics, Program Director of the Institute for Social Research and Co-Investigator of the Health and Retirement study, University of Michigan; *Thomas Cook*, Professor of Sociology, Psychology, Education and Social Policy, Northwestern University; *Felton Earls*, Professor of Social Medicine, Harvard Medical School; *Ronald Ferguson*, Faculty Chair & Director, the Achievement Gap Initiative, Harvard University; *Kathleen Mullan Harris*, Gillian T. Cell Distinguished Professor of Sociology, Director & Principal Investigator, National Longitudinal Study of Adolescent Health, UNC Chapel Hill; *Christopher Jencks*, Malcolm Wiener Professor of Social Policy, Harvard University; *Terrie E. Moffitt*, Professor of Psychology, University of Wisconsin and Professor, Centre for Social, Genetic and Developmental Psychiatry, Institute of Psychiatry, London; *Robert Sampson*, Henry Ford II Professor of the Social Sciences, Harvard University; *Eldar Shafir*, Professor of Psychology and Public Affairs, Princeton University; and *Laurence Steinberg*, Distinguished University Professor and Laura H. Carnell Professor of Psychology at Temple University. These aforementioned TRP members are further joined by distinguished experts in child and youth development (such as *Jeanne Brooks-Gunn*, Virginia and Leonard Marx Professor of Child Development and Education, Columbia University, and Lawrence Aber, Professor of Applied Psychology and Public Policy at the Steinhardt School of Education, New York University) to advise on the youth component of the MTO long-term evaluation. Members of the TRP will be consulted on an ongoing basis throughout the project, and will be convened twice during the course of the proposed project.

Significant input was also received from the team of researchers involved with designing and implementing MTO in the 1990s and undertaking the interim evaluation, including Dr. *Lawrence Orr* and Dr. *Judith Feins* of Abt Associates, Dr. *John Goering* of the City University of New York, and *Margery Turner* and Dr. *Susan Popkin* of the Urban Institute. Additional input was provided from the teams of researchers who have completed qualitative work after the interim evaluation, including Dr. *Katherine Edin* of Northwestern University, Dr. *Xavier Briggs* of MIT, and Dr. *Susan Clampet-Lundquist* of St. Joseph's University).

No comments were received as a result of the Federal Register Notice.

A.9 Payments to Respondents

Payments to respondents were authorized by OMB for the MTO canvass in 1997, and

1999, and for the Interim evaluation in 2002. The incentives were used for household respondents and contributed to successful canvasses and interviewing efforts. At this time, **HUD is requesting OMB approval of continued use of incentives for MTO respondents for the final impact evaluation.**

The use of incentive payments for the MTO final evaluation can be justified on the same grounds that were cited when first requesting their use for the MTO canvasses and interim evaluation:

The MTO panel is small. A total of 4,608 households joined the program and were randomly assigned to one of the three groups during the course of the demonstration. A total of 1,676 families in the MTO experimental and Section 8 comparison groups used Section 8 certificates or vouchers issued through the program to move.[29] This population size will permit detection of impacts in the likely size range only if panel attrition is kept very low and survey response rates are high.

The MTO study period is long. A 10-year study is needed to provide sufficient time to detect a wide range of program impacts on the education, employment, and social well-being of the families in the program. It is important for this final impact evaluation to make every effort to encourage participation in this important final data collection.

The MTO population is responsive to incentive payments. Previous research has shown that sample members with low incomes and/or low educational attainment have proven responsive to incentives, as have minority group members. These characteristics are heavily represented in the MTO panel.[30] Experience with MTO canvasses in 1997 and 2000, and the Interim Evaluation in 2002 bears out the value of the incentive.

Incentive payments can reduce the cost of locating mobile panel members before the main survey data collection. ISR is planning a focused locating effort leading up to the survey data collection for the final evaluation. Based upon prior research as well as the MTO tracking experience, the use of an incentive payment is estimated to significantly reduce the need for expensive field locating.[31] We also plan to offer a small number of contact persons a 'finder's fee' of $5-$10 for providing the research team with information that helps us locate respondents who are lost to tracking. ISR has successfully used finder's fees to help locate difficult to find and low-income respondents in studies such as the Panel Study of Income Dynamics. Respondent confidentiality is maintained at all times – the contact person is simply told that we are trying to locate the named respondent for "a study being conducted by the University of Michigan" and is not given the name of the study or the sponsor. If the contact person is able to provide us

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[29]Families assigned to the third group, the in-place control group, remain in their current public or Section 8 project-based housing.

[30] See among the sources documenting this recommendation: Allen P. Duffer et al., "Effects of Incentive Payments on Response Rates and Field Costs in a Pretest of a National CAPI Survey" (Research Triangle Institute, May 1994), passim; see also Erica Ryu, Mike P. Couper, and Robert W. Marans, "Survey Incentives: Cash vs. In-Kind; Face-to-Face vs. Mail; Response Rate vs. NonResponse Error" (International Journal of Public Opinion Research Vol.18 No.1 July 2005).

[31] See Duffer et al., ibid.

with an updated telephone or address information, or can get the respondent to phone our 800 line, we offer to pay the contact person a small amount of money as a token of appreciation for taking the time to help us with our research activities.

The final evaluation data collection is the final major step in testing the impacts of MTO. By late 2008, when the surveys are to be conducted, nine to twelve years will have elapsed since enrollment, and other positive incentives to cooperate with the data collection (such as willingness to fulfill the commitment made at enrollment) are likely to be low. Singer (2002) reports that financial incentives are effective for increasing response rates in all modes of survey data collection.[32] At a 1992 OMB-sponsored symposium on the topic of incentive payments, "most participants agreed with the general thesis that incentives should be considered whenever the positive forces to cooperate are low."[33]

 The final evaluation data collection is extensive. The combination of the household survey with interviewing and testing youth and children represents a substantial time commitment for the sample members. It seems necessary to recognize the extent of this data collection (compared to the brief canvasses) by offering larger incentive amounts. The biomarker data collection activities will require additional respondent time and commitment to the project.

For all these reasons, HUD is requesting authorization for a coordinated set of incentive payments for this study:

- Incentive payments of $50 for the household adults surveyed, who will be asked to complete a 75-minute interview;

- Incentive payments of $50 for youth ages 10-20, who will be asked to respond to a 60-minute interview and cooperate with 45 minutes of achievement testing;

- Additional incentive payments of $25 for adults who are asked to provide blood spot samples, height, weight and blood pressure measurements; and

- Increased incentive payments of up to $100 for adults and youth who are selected for the phase 2 subsample or are difficult to locate. The use of increased incentives is an important tool in maximizing response rates for long and complex studies such as MTO.

In addition to these aforementioned incentive payments for data collection, a randomly selected subset of 1,000 youth respondents ages 13 to 20 will receive payments as part of a nested decision-making exercise included as part of the long-term evaluation. This decision-making exercise will seek to measure time preferences, and will be supported using funding entirely from private foundations – without any federal support. As HUD and NBER have discussed with OMB, after completing the survey questionnaire and achievement tests MTO youth respondents selected for the decision making exercise will be thanked for their

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[32] See "The Use of Incentives to Reduce Nonresponse in Household Surveys," in Survey Nonresponse, eds. Robert M. Groves, Don A. Dillman, John L. Eltinge, and Roderick J. A. Little, (Wiley, 2002) pp. 163-77.
[33] See Providing Incentives to Survey Respondents: Final Report (Council of Professional Associations on Federal Statistics, September 1993), p. 10.

participation in this important HUD study, and provided with their compensation for participating. We will then indicate to the respondents that with private foundation support we are also able to offer them some additional compensation as part of a decision-making exercise, then reading them the question that asks whether they would prefer that ISR mail them $20 tomorrow or $25 on their next birthday.

We believe this exercise is important because previous research in behavioral economics suggests that changes in neighborhood environments that affect mood and stress (based on findings from the MTO interim evaluation) may affect basic features of decision making such as the willingness of people to defer gratification, i.e. their rate of "time discounting." As a result MTO impacts on time preferences could help explain program impacts on a wide range of behaviors including decisions to stay in school versus drop out, engage in health-risk behavior, or participate in criminal activity. As part of our decision making exercise respondents will be offered a choice between a payment of $20 that would be put in the mail the day after the long-term interview date, and a payment of $25 put in the mail on the respondent's next birthday. Following a brief explanation of the terms of the offer, the respondent is asked: "Which would you prefer - $20 sent tomorrow, or $25 sent on your next birthday?" This choice is then realized by remitting the payment for the specified amount on the chosen date. Variation in time between the survey date and the respondent's next birthday will generate variation in the ranges into which the respondent's choice brackets her time discount rate.

A.10 Arrangements and Assurances of Confidentiality

Informed Consent and Permission for Youth Data Collection

At the initial intake session for the MTO demonstration program between 1994 and 1998, applicants heard an explanation of the program and of the research design (including the random assignment to three groups). Those who then decided to join MTO signed an Enrollment Agreement acknowledging informed consent and permitting collection of various data about themselves and their family members. A copy of the MTO Enrollment Agreement is provided in Appendix I.

For the MTO final impact evaluation, we plan to obtain the permission of the core household heads for completing the survey, and separately for biomarker data collection that will seek separate permission for height, weight and blood pressure from collection of f blood spots. The core household consent will also seek permission for testing and interviewing their children under the age of 18. For youth ages 18-20 we will seek these youths' own consent to collect data at this time. The evaluation contractor will also obtain the assent of those under 18. Appendix J contains the three consent forms and one assent form for youth 10 to 17 years old. These forms incorporate appropriate language reflecting OMB disclaimers, NIH Certificate of Confidentiality, and requirements of the University of Michigan's Institutional Review Board

Data Confidentiality Protections

The data collected in the surveys for the MTO final evaluation, as well as the educational

achievement test results and the administrative data from the states, will all be used for research purposes only (for analysis of the research sample). Mailings to potential respondents and all in-person introductions will include assurances that participation is voluntary, that all information will be kept confidential, and that the respondents' answers will be reported as part of a group only.

In addition, HUD is applying for a National Institute of Mental Health (NIMH) Confidentiality Certification for the MTO final evaluation. This certification strengthens the privacy protections otherwise applicable to such research, by virtue of the language in the Public Health Service Act Section 301(d),[34] which says:

> The Secretary may authorize persons engaged in biomedical, behavioral, clinical, or other research...to protect the privacy of individuals who are the subject of such research by withholding from all persons not connected with the conduct of such research the names or other identifying characteristics of such individuals. Persons so authorized to protect the privacy of such individuals may not be compelled in any Federal, State or local civil, criminal, administrative, legislative, or other proceedings to identify such individuals.

The certification is being requested for the entire MTO final evaluation. The study's data collection plan, this OMB statement, and the proposed survey instruments has been reviewed and approved by the University of Michigan Institutional Review Board. Pending the receipt of revised consent forms and clarification of minor questions, copies of the revised consent forms are presented in Appendix J. The IRB's approval is required in order to obtain NIMH certification.

A.11 Sensitive Questions

The household and youth surveys contain several modules with sensitive questions, in the areas of substance use, mental health, and other risk behaviors. Some of the most sensitive questions are drawn from the National Co-Morbidity Survey (NCS) of adults and adolescents. All other questions dealing with these topics also have been drawn from existing survey instruments, including the MTO Interim Evaluation Survey, the National Study of Adolescent Health, the Project on Human Development in Chicago Neighborhoods and the National Longitudinal Study of Youth. These former studies successfully have collected these data under similar circumstances proposed for the MTO long-term evaluation. All of these questions have been answered without particularly high non-response rates in other data collection efforts.

Asking these questions about mental health disorders, victimization and risk behaviors is of considerable importance to this study given that findings from the MTO interim evaluation point to effects on these outcomes, and given that each of these outcomes impose very large costs on the government and society as a whole. The long-term evaluation is an opportunity to examine whether effects persist—a significant question for policy as social costs from

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[34] 42 U.S. Code Section 241(d).

criminal activity and mental health disorders, for example, are quite high—and whether effects on new aspects of behavior emerge. Several of the hypotheses about why MTO's effects on youth differ by gender including sexual harassment, adult role-modeling, and perceptions of discrimination, are another important reason why sensitive questions are part of the long-term survey design. An extensive literature (summarized in Brock and Durlauf, 1999) posits various theories that neighborhoods may affect social pathologies such as delinquency, substance use, and early childbearing. For example, as briefly described in Section A.2 models of "relative deprivation" suggest that well-off neighbors may provoke resentment among those from poorer backgrounds so that poor youth could be more likely to develop (or fall into) a deviant sub-culture when living in low-poverty neighborhoods. These models also suggest youth in the MTO experimental group may also show higher levels of delinquent behaviors than youth in the control group. Findings from the MTO interim study indicate that MTO increased criminal activity among adolescent males, for example, and that these young males continue to have ties to friends from their old neighborhoods. Another example of how neighborhoods may affect social pathologies comes from collective socialization models that posit that adults in a neighborhood may influence young people who are not their children. More affluent adults may act as role models who demonstrate that success is possible if you work hard and play by the rules; and high-SES adults may act as "enforcers" who help maintain public order. In this model, as MTO families become more integrated into their new communities, youth in the MTO treatment groups may have lower social pathologies than control group members since MTO movers end up in neighborhoods with a larger proportion of high-SES adults.   Some evidence in support of this emerged during the MTO interim evaluation where female youth showed improvements in mental health and reductions in risky behaviors.

As stated earlier, we will explain the privacy protections of this study to each respondent and assure them that their responses will be kept completely confidential and anonymous. Institutional reviews by the University of Michigan's, Northwestern University's and Harvard Medical School's Institutional Review Board, and the Certificate of Confidentiality being sought from NIMH, offer additional protections.

We will explain to respondents that these questions are about just one aspect of their lives and that their answers will not be treated any differently than other data collected. They will be treated with the same protections of privacy and confidentiality. In addition, we will offer special means to make the respondents comfortable with answering these questions. Adult and youth respondents will be given the chance to enter their answers directly into the automated CAPI (Computer-Assisted Personal Interviewing) system listening to the questions on a headset or reading the questions from the screen and entering the answers directly into the interviewer's laptop computer.

A.12 Estimate of Annualized Burden Hours and Costs

The data collection for the MTO final evaluation is a one-time effort. The present request covers only the final data collection effort to be carried out in 2008-2009.

Exhibit 4 shows the actual respondent burden for the MTO population to date. It shows the time, in hours, initially spent by all applicants who completed the MTO enrollment form and

the baseline survey. It then shows the actual burden resulting from the two MTO canvasses conducted to date, in 1997 and 2000, the MTO interim evaluation, and subsequent tracking of MTO families in 2005 and 2006. The total burden of MTO data collection from participants to date is 15,194 hours over a period of seven years.

Exhibit 4 Actual Respondent Burden (Through December 31, 2006) Under Prior OMB Clearances

Form	Respondent	Number of Respondents	Time to complete (minutes)	Frequency	Total Burden (hours)
Enrollment Form	Eligible MTO applicants	5,301	5 minutes	1 per respondent	442 hours
Participant Baseline Survey	Eligible MTO applicants	5,301	40 minutes	1 per respondent	3,534 hours
1997 Canvass	Families randomly assigned in MTO through 12/31/96	2,624[a]	Long form 19 min.; short form 13 min.	1 per respondent	756 hours
2000 Canvass	All families randomly assigned in MTO	3,808[b]	13 minutes	1 per respondent	825 hours
Interim Survey of Households	Adult head of core household[a]	3,526[c]	65 minutes	1 per respondent	3,820 hours
Interim Survey of Youth	Sampled youth ages 12-19 from MTO core households	2,829[c]	30 minutes	1 per respondent	1,415 hours
Interim Survey of Children	Sampled children ages 8-11 from MTO core households	1,780[c]	15 minutes	1 per respondent	445 hours
Educational Achievement Battery (WJ-R)	Sampled youth and children (ages 5-19) from MTO core households	2,759 Youth age 12-19; 1,770 age 8-11; 735 age 5-7	45 min. for youth and children 8-11; 30 min. for children 5-7.	1 per respondent	3,764 hours
2005 Tracking Mailing	Adult head of core household	1,304	8 minutes	1 per respondent	174 hours
2006 Tracking Mailing	Adult head of core households deemed "hard-to-locate"	139	8 minutes	1 per respondent	19 hours
TOTAL					15,194 hours

[a] Total sample for the 1997 MTO canvass was 2,883; response rate was 91 percent. A portion of the sample was administered the long form canvass (at 19 minutes) while the remainder was administered the short form (13 minutes in length).

[b] Total sample for the 2000 MTO canvass was 4,608; response rate was 82.6 percent. The entire sample was administered the short form of the canvass (13 minutes in length).

c Total sample for the 2001 Interim Evaluation was 4,248 adults (83% response rate); 3,537 for youth (80% response rate); 2,202 for children age 8-11 (81% response rate); 6,683 total sample for WJ-R testing (79% response rate).

Exhibit 5 shows the estimated respondent burden for the data collection associated with the MTO final evaluation, the data collection for which clearance is being sought in this package.

Exhibit 5 Estimated Future Respondent Burden for the MTO Final Evaluation Data Collection

Form	Respondent	Number of Respondents [b]	Time to complete (minutes)	Frequency	Total Burden
Final Survey of Households	Adult respondent [a]	3,545	75 minutes	1 per respondent	4,431 hours
Physical Measurements	Adult respondent	3,545	10 minutes	1 per respondent	591 hours
Dried Blood Spots	Adult respondent	3,545	10 minutes	1 per respondent	591 hours
Final Survey of Youth	Sampled youth ages 10-12 from MTO core households	840	30 minutes	1 per respondent	420 hours
Final Survey of Youth	Sampled youth ages 13-20 from MTO core households	4,019	60 minutes	1 per respondent	4,019 hours
Educational Achievement Battery (ECLS)	Sampled youth and children (ages 10-20) from MTO core households	4,859	45 minutes	1 per respondent	3,644 hours
MTO Final Evaluation (all)		8,404 respondents total			13,696 hours total

[a.] The core household refers to the set of persons expected to move together through the MTO program. This household's membership is defined by the applicant for MTO, during the process of completing HUD Form 50058 with the PHA staff. The applicant listed all individuals who planned to move into a new unit with a Section 8 certificate or voucher, if the family were to be assigned to the MTO experimental or Section 8 comparison group and succeeded in leasing up. An adult respondent will be selected from each core household using the same criteria that were applied during the interim evaluation. In most, but not all, cases, this is the same person who completed the Enrollment Agreement and Participant Baseline Survey when applying to join MTO.

[b.] The number of respondents for each form reflect achieving an effective response rate of 88 percent by interviewing 77 percent of the sample using a two-phase sampling process. For example, we might achieve a 70% response rate during the first phase of interviewing. In the second phase, we would select a random 4-in-10 subsample of hard-to-locate cases for more intensive follow-up efforts. If we achieved a 60% response rate with the hard-to-locate cases, this would yield interviews with 77% of the sample frame (.70 + .30*.40*.60) and an effective response rate of 88% (.70 + .30*.60). The study's target response rate is 85 percent, but we will seek a higher response rate if resources allow.

Total time burden of the adult and youth surveys are derived through a variety of methods. First, for many of the items we were able to obtain time estimates from the interim survey. This includes, for example, most of the components of the housing, neighborhood, education and economic modules. Second, as previously described in Section A.2.4, total time burden of the new modules such as mental health were estimated through our own careful new analyses of micro-data from the National Co-Morbidity Survey (NCS). Finally, ISR conducted a pre-test of the proposed long-term surveys with four low-income, minority female adults and five low-income minority youth. The results of this pre-test indicated that, excluding the modules on mental health disorders, victimization and risky behavior, average time of completion for the adult survey was approximately 65 minutes and average time of completion for the youth survey was approximately 45 minutes. Given the expected time required for the mental health sections that were not pre-tested, and that pre-tests generally require somewhat more time to administer on average than field surveys (because interviewers are just beginning their familiarity with administering the survey instruments), the existing survey instruments that submitted in this OMB package should be quite close to the target average administration time of 75 minutes for MTO adults and 60 minutes for MTO youth.

A.13 Estimated Recordkeeping and Reporting Cost Burden on Respondents

The cost to respondents will be the time required to respond to the survey.

A.14 Estimate of Cost to the Federal Government

Exhibit 6 shows the costs to the federal government of past and current data collections for the Moving to Opportunity demonstration. The first row of the exhibit shows the actual cost of MTO data collection during the baseline period, when families were joining MTO and when site agencies were submitting data monthly to HUD's implementation contractor.

The second and third rows of Exhibit 6 show the actual cost of the MTO canvasses conducted in 1997 and 2000, which together totaled $1,269,824. For the number of families in the first canvass sample (only part of the full MTO population, which was not yet complete at the time), the 1997 canvass cost came to $154 per family. The per family cost in 2000 came to $179. For the MTO interim evaluation, cost to the U.S. Department of Housing and Urban Development (HUD) was $1,365,526 and cost to other federal agencies was $2,693,847, for a total of $6,367,246. Ongoing tracking of MTO families through 2005 and 2006 cost $719,238. Estimated cost to HUD of the final evaluation is $1,779,821 and, approximately $3,669,000 per committed resources from other federal agencies as of July 2007. Pending costs currently being reviewed in proposals to federal entities include one at the National Institute on Aging of $2,206,000 and one at the Institute for Educational Sciences of $549,000, for a total additional pending cost of $2,755,294. Private funding for a request of $1,350,000 for data collection is also pending with The Bill and Melinda Gates Foundation.

Exhibit 6 Actual and Estimated Costs to the Federal Government

Actual and Estimated Costs to the Federal Government for Data Collection

Line Item	Cost to the Federal Government	Total Cost
Total costs for MTO data collection during program operations (1994-1999)[a]	$689,491	$689,491
Total costs for 1997 MTO canvass (including incentive payments)	$444,711	$444,711
Total costs for 2000 MTO canvass (including incentive payments	$825,113	$825,113
Costs for Interim Evaluation participant data collection (including incentive payments)		
U.S. Department of Housing and Urban Development	$1,365,526	
Other federal agencies (NICHD, NSF, NIMH)	$2,693,847	
Subtotal	$4,059,373	$6,367,246
Costs for the continued participant tracking	$719,238	$719,238
Costs for Final Evaluation data collection (including incentive payments)		
U.S. Department of Housing and Urban Development	$1,779,821	
Other federal agencies (NICHD, NSF, CDC, NIMH)	$3,795,393	
Subtotal	$5,575,214	$ 7,326,455

[a] Includes Enrollment Agreements and Participant Baseline Surveys, as well as data collection from site agencies.

The last row of Exhibit 6 shows the estimated costs for the final evaluation data collection covered in this request for OMB clearance. These estimates were prepared by HUD's current Contractor, NBER. Costs to be funded by HUD for the evaluation's survey data collection (including educational testing) will total $1,779,821. Four other federal agencies, NICHD, NSF, CDC and NIMH, have agreed to provide $3,669,623 in resources to this data collection.

Grants from several private foundations for this research bring the total data collection current levels of funding to $7.326 million.

A.15 Changes in Burden

This request for clearance does not involve a change in burden due to any program changes or adjustments. It concerns a new data collection not previously submitted to OMB for review.

A.16 Plans for Tabulation, Analysis, and Publication

The data collected for the MTO final impact evaluation will be analyzed, tabulated, and reported to HUD by the evaluation contractor. This section describes the basic analytic framework for the evaluation.

A.16.1 Impact Estimates: The Basic Model

A central objective of the evaluation is to estimate the impacts of the housing vouchers and certificates received by the MTO experimental group and the Section 8 comparison group (the "treatment groups") on a wide range of outcomes in the domains discussed in the remainder of this chapter. Random assignment assures that simple comparisons of raw mean outcomes between each of these groups and the in-place control group will provide unbiased estimates of these impacts. These differences across groups in average outcomes estimate the causal effect of eligibility for MTO Experimental or Section 8 subsidies, known as intention-to-treat (ITT) effects. To improve the precision of the estimates, we will use regression analysis to control for any chance differences between the treatment and control groups on a number of characteristics measured at baseline.

We illustrate our analytical approach in a simple regression framework. Let D be an indicator for use of a voucher to move through the MTO program, or treatment compliance. Let Z be an indicator for treatment group assignment. Let subsidy use be a function of a set of observed characteristics from the MTO baseline survey either known prior to randomization or retrospectively reported as existing prior to randomization (X) as well as other factors ε_1, as in equation (1.1).

1.1 $\qquad D = Z\pi_1 + X\beta_1 + \varepsilon_1$

The ITT effect is captured by the estimate of the coefficient π_2 in a regression of one of the outcome measures discussed above (Y), which might come from either administrative or survey data sources, on an indicator for assignment (Z) to a treatment group as in equation (1.2).

1.2 $\qquad Y = Z\pi_2 + X\beta_2 + \varepsilon_2$

This ITT parameter is an average of the causal effects for those who do and do not take-up the treatment. Note that conditioning on baseline characteristics (X) improves the precision of our estimates (that is, increases statistical power to detect program effects) by accounting for residual variation in the outcome of interest. These baseline characteristics should have the same distribution within the treatment and control groups because they are statistically independent of group assignment. Thus, including them will not change the coefficient π_1 or π_2 (unless X happens to differ between groups due to the variability in a small sample).

Because the random assignment probabilities under MTO changed over time for different cohorts of participating families, our team helped develop sampling weights for the interim MTO survey that we would also use for the proposed final impact evaluation. Note that our ITT estimates are *not* biased by self-selection in MTO treatment take-up within the Experimental or Section 8 groups, because Controls are compared to *all* families assigned to a treatment group – whether or not the latter decide to accept the invitation to participate in the voucher program. This estimate is known as the "intent to treat" (ITT) estimate, because it reflects the effect of the treatment on all those to whom it was offered, whether or not they actually received it.

A.16.2 Impact Estimates: Effects of the Treatment on the Treated

While the ITT estimates produced by the basic model are useful for some purposes, it is also important to know the effect of the treatment on those who actually availed themselves of the subsidy, i.e., who leased up and moved. Fortunately, we can derive this estimate of the impact of the "treatment on the treated" (TOT) directly from the ITT estimates and knowledge of the proportion of treatment group members who leased up.

In this framework TOT is π_2/π_1, or ITT divided by the proportion receiving the treatment (Bloom, 1984; see also Heckman, LaLonde and Smith, 1999). [35] The TOT estimate is useful for gauging the magnitude of the causal effects and assessing the substantive importance of effects on individuals. Although the estimates from equation (1.3) are average causal effects, the entire marginal distribution of effects is identified and can be analyzed for continuous outcomes such as achievement test scores (Imbens and Rubin 1997).

1.3 $\quad Y = D\gamma_5 + X\beta_5 + \varepsilon_5$

There are two treatment groups in this application, so we will compute separate estimates for each (the Experimental and Section 8 groups) by making D and Z matrices of two indicator variables for these groups. We will stack data for all three MTO groups and present regression-adjusted ITT and TOT estimates based, respectively, on simple regression estimates of (1.1)-(1.2), and, as described below, on estimates of (1.4-1.5), using treatment group assignments as "instrument" or predictor of voucher use through MTO. For an application of these methods to data from the interim study see Orr et al. (2003), Kling, Ludwig and Katz (2005) and Kling, Liebman and Katz (2007).

This adjustment provides an unbiased estimate of the impact of the treatment on those who leased up, under the relatively weak assumption that the treatment had no effect on those who failed to lease up. It is important to note that this adjustment requires no assumption about the characteristics of those who leased up and/or those who did not; in particular, the adjusted estimate will be unbiased even if those who lease up differ markedly from those who do not.

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[35] When equations (1) and (2) are estimated using ordinary least squares (OLS), this is numerically identical to a two stage least squares regression of Y on D with Z used as an instrumental variable for D, as in equation (5), where both the first and second stage equations are estimated as linear probability models with Huber-White standard errors that are adjusted to account for heteroskedasticity.

We will produce both "intent-to-treat" and "treatment-on-treated" impact estimates for both the MTO experimental group and the Section 8 comparison group. Great care must be exercised in interpreting comparisons of the impacts on the two treatment groups, however, because the proportion of families who leased up, and therefore the subset of families on whom the treatment had an effect, differed substantially between the two groups. Thus, when we compare the intent-to-treat estimates, we might find that the regular Section 8 subsidies had a larger effect on certain outcomes, either because they had a larger effect on those families who leased up or because a larger proportion of families leased up in the Section 8 comparison group (or both). And, as noted earlier, in comparing the impact of the treatment on the treated in the two treatment groups, we must be mindful that these represent impacts on different subsets of families, corresponding to the different lease-up rates in the two groups. We might, for example, find that the MTO subsidy had a larger effect on those who leased up than the regular Section 8 subsidy either because it would have a larger effect for any subset of families or because the subset of families who leased up in the MTO experimental group were more susceptible to such effects than those who leased up in the Section 8 comparison group.

A.16.3 Impacts on Subgroups

Perhaps the main puzzle to arise from the interim MTO evaluation is the gender difference in program effects among youth. To test these sub-group effects, let W represent a set of indicator variables for different subgroups (such as gender) of interest. The effects for different subgroups are estimated by including interaction terms (Z*W) in equation (1.4); note the main effect of W is subsumed in X. The coefficient α_3 represents the difference in the treatment effect across subgroups (such as between boys and girls).

1.4 $Y = Z\pi_3 + Z*W \, \alpha_3 + X\beta_3 + \varepsilon_3$

Of particular importance to housing policymakers may be evidence on which sub-groups of public housing families are able to benefit most from mobility programs like MTO. These questions can be examined by re-estimating MTO impacts for other subgroups defined on the basis of baseline characteristics. It will thus be important to identify in advance those subgroups that social science and other theories predict might benefit more from the MTO intervention. Subgroups of particular policy relevance that we aim to examine include families with relatively younger children in the home at the time of baseline, versus those with older children; families with chronic physical or mental health conditions at baseline; families who are chronically unemployed and detached from the labor market at baseline; and families who are more optimistic and resourceful at baseline.

A.16.4 Variation in Impacts Over Time

We expect that the effects of a change in neighborhood will take some time to materialize, i.e., that impacts will reflect the cumulative influences of living in a new environment. Therefore, it will be desirable to analyze the time path of impacts wherever possible. For some outcomes, this will not be possible because of data collection constraints; we will simply have point-in-time outcome measures taken at the time of the final evaluation survey. For these outcomes, equations 1.1 – 1.2 will yield estimates of impact at that point in time.

For some outcomes, however, we will have continuous histories from the point of random assignment through the follow-up period.

We plan to exploit the longitudinal information on all MTO participants available from the administrative records discussed above. We will construct a panel of all post-randomization periods for MTO participants. Period since random assignment (indexed by t) may be defined as the calendar quarter for administrative earnings or welfare data, and as the academic year for student-level school records. The regression model includes a set of indicators for periods since random assignment (R_{it}) and indicators for actual calendar period (Q_{it}) to capture residual variation in outcomes over time. We estimate this model separately for time periods such as 1-2 periods after random assignment, 3-4 periods after randomization, etc. The coefficient φ_1 is the average difference between the treatment and control groups in a specified time period.

1.5 $\quad Y_{it} = \varphi_0 + Z_i\varphi_1 + X_i\varphi_4 + R_{it}\varphi_5 + Q_{it}\varphi_6 + \psi_{it}$

The value of this type of longitudinal analysis is highlighted by the interim MTO analysis of youth arrests from administrative "rap sheets." Kling, Ludwig and Katz (2005) show that treatment assignment reduces male arrests for violent crimes during the first few years after randomization, but this effect dissipates and gives way by 3-4 years after random assignment to a positive treatment-control difference in property-crime arrests. This finding seems to rule out stories that focus on the difficulty male youth face dealing with the disruptions of moving, and might instead suggest a comparative advantage in the competition for anti-social rewards in these new neighborhoods that takes some time to discover. If this explanation is correct we would expect to see parallel impacts on educational outcomes, which could give way again to beneficial MTO effects on delinquency over the long term if accumulated exposure to "protective factors" in the new neighborhoods makes youth more competitive in local schools and labor markets.

A.16.5 Adjustments for Varying Random Assignment Ratios

The initial random assignment ratio in all MTO sites was set to yield equal numbers of leased-up families in the MTO experimental and Section 8 comparison groups, given the best available estimate of the lease-up rates that could be expected in the two groups (80 percent in the Section 8 group and 30 percent in the MTO group.)[36] Equal numbers of leased-up families would provide the most statistically efficient (i.e., minimum variance) estimates of differential impact between the two groups receiving certificates or vouchers.

As the demonstration proceeded, it became clear that the lease-up rates for the MTO experimental families in several sites were significantly higher than predicted, relative to the Section 8 lease-up rate. Continuing to assign families at the same random assignment ratio would have resulted in an unbalanced experimental sample, with substantially more leased-up families in the MTO experimental group than in the Section 8 comparison group. Not only would this have been statistically inefficient, but it would have exceeded the resources available to the nonprofit organizations responsible for providing counseling to the MTO

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[36] The initial ratio was 8 MTO experimental families to 3 Section 8 comparison families to 5 in-place control families.

experimental families. Therefore, the random assignment ratio was changed to a new ratio that, on the basis of the experience of the early random assignment cohorts in the site, was expected to produce equal numbers of leased-up families in the MTO experimental and Section 8 comparison groups. The random assignment ratio was changed at least once in every site.

When the ratio of treatment and control families randomly assigned differs among parts of the sample, a simple comparison of mean outcomes (or, equivalently, a regression of the form shown in equation 2.1, with a single treatment dummy) may yield biased impact estimates. This is true because such differences confound assignment to treatment group with site and time period, so that assignment is no longer random over the entire assigned sample. In this situation, unbiased impact estimates can still be obtained, however, by estimating the impact of the program within each "assignment set" (i.e., within each subsample assigned under the same random assignment ratio) and then computing the impact on the overall treatment group as the weighted average of the assignment set impacts. Since the treatment and control groups are well-matched within each assignment set, this yields an unbiased impact estimate. The impact within assignment sets can be estimated using a regression model with new variables that interact the treatment status indicator with a set of dichotomous indicators that represent membership in each random assignment set.

Alternatively, it is possible to weight individual sample members to correct for these variations in random assignment ratio. This approach is particularly useful for descriptive analyses where regression analysis is either not appropriate or not convenient. In earlier work, Abt Associates has developed such individual weights for the overall MTO sample. Similar weights will be used for the final analysis sample. In addition to adjustments to accommodate changes in the random assignment ratios, we will create new weights to adjust for two stage sampling process proposed for the long-term evaluation described in Section B.2. Whichever approach is taken, regression analysis or weighted descriptive statistics, care must be exercised in deriving the estimates and, especially, their standard errors, to ensure that the estimates are unbiased and that appropriate tests of statistical significance are applied.

A.16.6 Mediating Mechanisms

Another goal is to learn why, as well as how, MTO affects families, and whether any neighborhood effects on behavior are non-linear with respect to specific neighborhood attributes. To this end we will employ three other approaches that may help eliminate candidate mechanisms through which neighborhoods may affect outcomes.

First, we will examine the temporal patterns of MTO impacts on outcomes. By 2008 we will also have sufficiently long residential histories to explore differences between effects on long-term outcomes of neighborhood characteristics in 2008 versus the residential duration weighted averages of these characteristics or other aggregations of cumulative exposure.

Second, we can examine MTO impacts on mediator variables linked to the basic theories of neighborhood effects. Specifically, we generate ITT estimates using survey measures of candidate mediating mechanisms as the dependent variables in equation (2) and compare

patterns of outcomes for the full sample and across sub-groups defined by age and gender with what are observed for individual outcomes.

Evidence that MTO has an effect on a mediator variable is not conclusive evidence that the theory associated with that mediator is responsible for the program's effects on behavior, since numerous environmental factors are changing at once for families assigned to the MTO treatment groups. And, many of the candidate mediating variables of interest may in principle be simultaneously determined with youth outcomes in the schooling, work, delinquency or health domains. However, lack of an MTO effect on a mediator central to a given theory can help rule out the importance of that theory. For example, Kling, Ludwig and Katz (2005) hypothesized that the gender difference in MTO effects on youth arrests could be due to greater racial discrimination experienced by males than females. Yet data from the interim MTO youth survey suggests that the Experimental treatment reduces experiences with racial discrimination for both male and female youth. In this case, analysis of mediators helped to rule out an otherwise plausible mechanism through which neighborhoods might affect behavior.

Estimating the role of mediators in this way relies on a series of separate regression equations to estimate program effects on mediators, rather than on a mediated structural-equations model. We will do this to capitalize to the extent possible on the experimental nature of the data. The underlying insight here is that randomization occurred with respect to receipt of the bundle of program services and not on the basis of mediators. This means that the purity of the experimental design can be maintained only by treating each outcome and mediator in a separate set of experimental/control regressions.

Finally, we will use the method developed by Kling, Liebman and Katz (2007) to examine the effects of specific neighborhood attributes by exploiting variation across MTO sites in the effects of both the Experimental and Section 8 treatments on neighborhood characteristics. With this approach a socio-economic measure such as the Census tract poverty rates (P) is viewed as a summary index for a bundle of neighborhood characteristics that are changed as a result of MTO. Interactions between treatment group assignments (Z) and site indicators (S) are used as instrumental variables to isolate the experimentally-induced variation in P across sites and groups, as in equation (2.7), where the main site effects are subsumed in X. To explore non-linear effects they also use these interactions to instrument for higher-order terms in P.

1.6 $P = Z*S\ \mu_6 + X\beta_6 + \varepsilon_6$

1.7 $Y = P\lambda_7 + X\beta_7 + \varepsilon_7$

Kling, Liebman and Katz test for nonlinear effects of neighborhood socioeconomic composition (poverty rates as well as fraction college graduates, median income and share of households headed by a female) in the interim evaluation and find little evidence of nonlinearities. Using models with two endogenous variables, compliance and poverty rate, and ten excluded site-by-treatment instruments, they do find evidence that ITT effects appear to be driven by the poverty rates of neighborhoods rather than simply by use of a voucher at all. Ludwig and Kling (2007) extend this approach to also disentangle the effects of

neighborhood class and race composition, as well as neighborhood crime rates, each of which may have conceptually distinct effects on youth behavior. In preliminary analyses with the interim MTO data we find that there is enough independent variation in each across the ten separate MTO treatment/site "experiments" to generate meaningful estimates of multiple measures – although there are obviously limits on statistical power for the number of mechanisms that can be distinguished with these instruments. We can also use candidate mediating measures in the long-term evaluation as second-stage dependent variables and compare these results to those for youth outcomes to learn more about neighborhood processes. For example, Ludwig and Kling (2007) find in the interim data that tract poverty rates are more strongly associated than is tract racial composition with mediators implicated by leading theories such as community disorder, collective efficacy and policing quality. Yet tract racial composition is predictive of MTO youth arrests while poverty is not. These findings do not seem supportive of many of the leading theories about neighborhood effects on crime.

The strength of MTO is the demonstration's randomized experimental design, and so our proposed scope of work prioritizes the analysis of long-term data on MTO participants that exploits random assignment.

A.16.7 Threats to Internal Validity

The greatest threat to the internal validity of this study is the potential bias from attrition caused by failure to locate or complete interviews with some respondents. SRC's efforts to maximize the survey response rate will include interviewing a sub-sample of hard-to-find cases, which has been quite successful in previous research.

We will also explore several sensitivity checks to explore potential bias from missing data. Sample attrition that is systematically related to our outcomes of interest (Y) would presumably also be related to the distribution of baseline characteristics (X), and so bias from sample attrition would be reflected by sensitivity of our estimates to conditioning on baseline characteristics. We can examine this quite easily by comparing baseline characteristics of respondents to non-respondents, overall and by the random assignment research group status. We will also pursue more complicated analyses. For example, we can examine the sensitivity of our results to worst-case bounds, which enable us to bracket the true effects of MTO without imposing any assumptions about the unobserved outcomes of MTO participants for whom outcome measures are not available.[37] A final approach to addressing the problem of missing data would be to impute values using the data that are available on respondents from other data sources, such as the interim MTO surveys or administrative data sources on related outcome domains.

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[37] To take a simple example, suppose that data on welfare receipt are available for every adult in the Control group but are missing for 5% of adults in the Experimental group. A no-assumptions upper bound for any effects of MTO Experimental assignment to reduce welfare receipt rates comes from imputing outcomes of non-receipt of welfare to missing Experimental adults; logically, any beneficial effects of MTO on welfare receipt cannot be any larger than those obtained under this assignment of missing outcomes. See Manski (1989, 1990, 1995), Ludwig et al. (2005).

A.16.8 Multiple Testing and the "False Positive" Problem

One strength of the long-term MTO study proposed here is the collection of survey and administrative data on a wide range of outcomes and mediating factors. However, the availability of so many measures also presents a challenge. In particular, with so many outcomes, the probability of rejecting a true null hypothesis for at least one outcome is much greater than the significance level used for each test. Therefore, it is important to use methods that address this possibility of false positives.

We plan to use the techniques discussed in detail by Kling and Liebman (2004) to conduct hypothesis tests in three ways. First, following standard practice we will consider the statistical significance of individual treatment-effect estimates in isolation, or "per-comparison significance." Second, we will make summary statements about the effects of MTO on a given family of outcomes, such as the entire domain of mental health, by constructing summary measures equal to the sum of standardized treatment effects within the family of outcomes. Our calculation of statistical significance for summary measures will use a series of seemingly unrelated regressions that accounts for the covariance across estimates within the set of mental health outcomes. Third, we will discuss the statistical significance of the entire family of related hypotheses in each of our five outcome domains ("familywise significance") compared to the null hypothesis of no effect on any of the outcomes in the domain.

A.16.9 Analytic Techniques, Tabulations, and Reporting

The experimental design of MTO allows for use of fairly straightforward analytic techniques. The difference in mean outcomes between the in-place control group and either the MTO experimental group or the Section 8 comparison group provides an unbiased estimate of the impact of the treatment. To improve the precision of the estimates, OLS regression will be used to control for chance differences between groups in characteristics that affect the outcomes. For dichotomous outcomes, logistic regression will be employed.

The analytic results will be presented in tables in the same way that findings were presented for the MTO interim evaluation (see Orr et al., 2003) that show the control mean, the means for the MTO experimental and Section 8 comparison groups, the (regression-adjusted) differences in means and their statistical significance, and the MTO and Section 8 impacts as percentages of the control mean. The outcomes to be analyzed were discussed in detail in Section A.2. Exhibit 7 shows one way of displaying these results.

Exhibit 7 Sample Table Shell for Presentation of Impact Estimates Outcome measure	Control Mean	Experimental vs. Control ITT TOT	Section 8 vs. Control ITT TOT

The final report of the final evaluation, to be submitted to HUD in October 2009, will present a comprehensive analysis of all the data collected over the course of the evaluation. A draft outline of the report is shown in Exhibit 8. The report will include an Executive Summary suitable for dissemination to policy makers and the general public, as well as a more detailed explication of the results in the text and a series of appendices containing documentation of estimation methods and statistical results, data sources, and additional descriptive information. The text of the report will be written in language accessible to the layman.

A.16.10 Time Schedule for Analysis and Reporting

Collection of survey data from MTO participants is expected to begin in April 2008 and be completed by April 2009. The analysis of these data will be carried out between May 2009 and September 2009. A final report is due to HUD at the end of October 2009.

A.17 Expiration Date Display Exemption

All data collection instruments created for the MTO final impact evaluation will display prominently the expiration date for OMB approval.

A.18 Exceptions to Certification

This submission describing data collection requests no exceptions to the Certificate for Paperwork Reduction Act (5 CFR 1320.9).

Exhibit 8 Draft Outline: Final Report

Executive Summary

Chapter 1 - The Final Evaluation

> The Moving to Opportunity Demonstration
> Previous Studies of Mobility Programs and the Effects of Neighborhood
> Previous Analyses of the MTO Demonstration
> Research Questions
> Overview of This Report

Chapter 2 - Geographic Mobility in the MTO Final Evaluation Sample

> Hypotheses about mobility in MTO
> Mobility data sources and measures
> Baseline conditions and initial lease-ups
> Sample mobility in the follow-up period
> Geographic mobility impacts
> Interpretation of results

Chapter 3 – Impacts on Housing, Neighborhoods, and Safety

> Hypotheses about housing, neighborhood, and safety
> Data sources and measures
> Baseline housing and neighborhood status of MTO participants and control group context
> Impacts on housing, neighborhoods, and safety
> Interpretation of results

Chapter 4- Impacts on Adult and Youth health

> Hypotheses about adult and child mental and physical health
> Data sources and measures
> Context and baseline status of the sample
> Mediators for health impacts in MTO
> Impacts on adult and youth mental and physical health
> Interpretation of results

Chapter 5- Impacts on delinquency, criminal and risky behavior

> Hypotheses about adult and youth involvement with delinquent / criminal or risky behavior
> Data sources and measures
> Context and baseline status of the sample
> Effects on mediators for delinquent / criminal or risky behavior in MTO
> Impacts on delinquent / criminal or risky behavior
> Interpretation of results

Chapter 6 – Impacts on adult and youth education

> Hypotheses about adult and youth education in MTO
> Data sources and measures
> Baseline education experiences and control group context
> Impacts on hypothesized mediators of educational impacts
> Impacts on hypothesized outcomes
> Interpretation of results

Chapter 7 – Impacts on adult and youth employment and earnings

       Hypotheses about adult and youth employment and earnings
       Data sources and measures
       Context and baseline employment status of the sample
       Impacts on hypothesized mediators
       Impacts on employment and earnings of adults
       Impacts on employment and earnings of youth
       Interpretation of results

Chapter 8 – Impacts on income and receipt of public assistance

       Hypotheses about income and public assistance
       Data sources and measures
       Context and baseline employment status of sample
       Impacts on hypothesized mediators
       Impacts on income and public assistance

Chapter 9 – Summary and Implications of Estimated Impacts of MTO

       Summary of Impact estimates
       Assessing the Impact estimates
       Implications of the Final Evaluation Results for Public Policy

Appendix A - Estimation Methods and Derivation of Outcome Measures

Appendix B - Data Sources and Data Collection Methods

Appendix C - Descriptive Tables

Appendix D - Detailed Estimation Results