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DO NEIGHBORHOODS AFFECT CREDIT MARKET DECISIONS OF LOW-INCOME
BORROWERS? EVIDENCE FROM THE MOVING TO OPPORTUNITY EXPERIMENT

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Do Neighborhoods Affect Credit Market Decisions of Low-Income Borrowers? Evidence from the Moving to Opportunity Experiment

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ABSTRACT

This paper provides new evidence on the long-term impacts of neighborhood environment on low-income credit decisions by analyzing financial outcomes and borrowing decisions of participants of the Moving to Opportunity (MTO) experiment. The MTO experiment was a unique, large-scale experiment that offered families vouchers to move to better neighborhoods via randomized lottery. Families received one of two types of housing vouchers: the first (Experimental) required families to move to very low poverty neighborhoods, and the second (Section 8) allowed families unrestricted choice of neighborhood. We find that participants who moved to lower poverty neighborhoods as young children experienced better access to and greater use of credit into adulthood, with the largest effects experienced by those who moved to the lowest poverty neighborhoods. While we do not find adult and older children experience the same benefits to credit outcomes, we do find they experience improvements in delinquency and debt behaviors, particularly among those who received unrestricted Section 8 vouchers. We also find evidence that children whose families received unrestricted Section 8 vouchers have significantly lower payday loan usage.

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The disparities in financial outcomes and access to credit between the poor and the non-poor have been widely documented. Relative to the non-poor, the poor are more likely to repeatedly borrow at high interest rates, use high-interest credit products such as payday loans, have limited access to formal credit, have bills sent to third party collection agencies due to non-payment, and be categorized as high-risk or sub-prime borrowers.¹ These differences in the use of and access to credit can have profound welfare effects on low-income individuals and their ability to invest in their future and in their children, potentially limiting intergenerational economic mobility and further entrenching poverty.

Neighborhood characteristics may be particularly relevant in the context of financial decisions, where peer effects have been shown to exert considerable force (Gross and Souleles (2002), Duflo and Saez (2012), Bursztyn et al. (2014), Lieber and Skimmyhorn (forthcoming)). Better neighborhoods could facilitate the acquisition of knowledge about available financial products or simply provide easier access to mainstream financial institutions such as banks. A number of studies have shown that the neighborhood environment has a strong causal impact on health, education, crime, and earnings, among other measures of well-being (Ludwig et al. (2013), Chyn (2017), Chetty et al. (2014a), Chetty et al. (2014b), Chetty et al. (2016)). Yet despite the importance of neighborhood environment on these economic outcomes, the impact of neighborhoods on the financial behavior and credit market use of low-income households remains largely unknown. This is an important omission in the context of understanding disparities in financial outcomes and how household financial decisions can be improved.

In this paper, we evaluate the role of the neighborhood environment on low-income credit decisions by analyzing financial outcomes and choices of participants of the Moving to Opportunity (MTO) experiment. The MTO experiment was a unique, large-scale randomized controlled trial conducted by the Department of Housing and Urban Development between 1994 and 1998. This experiment randomized individuals living in low-income subsidized housing projects into one of three groups. The first group received a voucher that could only be used to rent housing in a Census tract with a less than 10 percent poverty rate (Experimental group). The second group received an unrestricted Section 8 voucher, which provided the same rent subsidy but allowed the

¹See, for example, Rhine et al. (2006), Dobbie and Skiba (2013), Brevoort et al. (2015), Adams et al. (2009), Miller et al. (2018) and Finkelstein et al. (2012).

the recipient to move to a neighborhood of their choice (Section 8 group). The third group was a control group and did not experience a change in their public housing assistance. As previously documented in Kling et al. (2007), Ludwig et al. (2012), Chetty et al. (2016), and others, being randomized into either treatment group resulted in a substantial reduction in neighborhood poverty rate.

Prior studies of the Moving to Opportunity experiment found no effect of being randomized into the Experimental or Section 8 group on adult economic income or earnings, and mixed results for children, with some positive effects detected for female children but negative effects on male children (Kling et al., 2007). Additional long-term follow up found that those in the MTO treatment groups reported improved subjective well-being and suggestive improvements in mental and physical health, but again no improvements in labor market outcomes (Ludwig et al., 2012). Finally, a recent study using tax data analyzed the long-term impacts of MTO on both children and adults (Chetty et al., 2016). Consistent with previous research, the authors find no effect of voucher receipt on earnings for adults. However, they find improvements along several dimensions for children in the treatment groups who were younger than 13 at the time of random assignment. In particular, they find an approximately 30 percent increase in earnings, a 2.5 percentage point increase in college attendance rates, and lower single parenthood rates for children in the Experimental group. The authors find no improvement for children who were older at random assignment.

Our work builds on this literature by examining a critical, yet undocumented channel through which neighborhoods may affect the outcomes of low-income households: credit market behavior and access. Access to mainstream credit is a crucial component to qualify for basic utilities, better housing, internet, and other tools for supporting economic mobility. While previous literature has documented that moving to a low poverty neighborhood can create benefits, the impact of MTO participation on credit outcomes is unclear *ex ante*. Families who move may experience disruption costs and lose access to social and familial networks that provided access to informal credit. Better neighborhoods could also mean higher costs of living, creating further potential for pressure on credit constraints. Limited access to mainstream credit can also leave options for only high-risk, high-interest credit products on the alternative lending market. Even those who earn more after moving may not necessarily experience better access to credit; income is not a direct input into credit score and studies document that income is only moderately correlated with credit score

(Beer et al. (2018)).² This analysis is thus directly relevant both for the literature on the role of neighborhood environment and for studies that seek to understand the borrowing behavior of low-income consumers

We first examine the impact of the MTO neighborhood experiment on the long-term access and the use of credit of both those who were adults and children at random assignment. We find that younger children of Experimental families who were required to move to the lowest poverty neighborhoods experience the greatest benefits in credit into adulthood. Younger children within Experimental families who used a voucher have credit scores approximately 11 points higher relative to the control group. This increase in credit score has a meaningful impact on credit limits, credit use, and credit availability. Younger children from Experimental families are approved for \$821 more in credit limits as adults, or 62% more relative to the control group mean. They utilize more credit and also have more revolving credit available. We find similar impacts of smaller magnitude among children within Section 8 families who also moved to better neighborhoods relative to the control group, but did not experience as large of a reduction in neighborhood poverty rate as the Experimental families.

We do not find adults or older children of MTO families experience the same positive impacts on higher credit scores or credit limits. We do, however, find improvements on delinquency behaviors across adults and older children, concentrated on those who received a Section 8 voucher. Adults who moved within the Section 8 treatment group have \$333 or 35% less in amounts more than 30 days overdue relative to the control group average. This debt is significant given that Chetty et al. (2016) finds annual reported earnings for MTO participants of less than \$12,000. Our estimates also show that adults and older children (ages 13-17) assigned to the Section 8 voucher group also hold less debts in court judgments and overdue taxes. Surprisingly, we do not find these same benefits for adults and older children within the experimental treatment group. Among younger children, we see suggestive evidence ($p=0.058$) of reductions in delinquency behavior among those in the Experimental group, but no significant effects on delinquency behavior for those randomized to the Section 8 group.

Given the low-income profiles of the MTO sample, we further evaluate whether participants

²In particular, Beer et al. (2018) find that the raw correlation between income and credit score is less than 0.30, and that adding income to a regression including other credit attributes contributes virtually no additional explanatory power.

use alternative subprime credit options such as payday loans. Payday loans are short-term, non-collateralized small loans that coincide with the payday of a borrower’s employer. These loans typically come with very high fees ranging from \$10 to \$20 per \$100 borrowed. We find that younger children within the Section 8 voucher group show the largest and most significant reductions in payday borrowing. Younger children within families assigned to Section 8 vouchers borrow about 50% less in payday loans annually relative to the control group.

Our results show that moving to a better neighborhood improved credit access and use into adulthood for children who moved when they were young. While we do not find positive impacts on credit access for adults and older children, we do find improvements in their ability to repay overdue delinquencies and debts, particularly among those who were assigned unrestricted Section 8 vouchers. We know from prior studies, however, that income and earnings only increased for younger children. Thus, we further examine neighborhood characteristics particularly relevant for credit market decisions to understand and interpret the context of our results.

To do this, we use a 4 million random sample of credit reports to calculate the average credit and delinquency behavior of peers in zip codes adults and children moved to after the MTO experiment. We find that MTO participants adults and children across both treatment arms moved to neighborhoods where residents had better access to credit on average, with the largest impacts for the youngest children randomized into the Experimental treatment arm, mirroring our results on individual credit outcomes. We do not find that MTO participants moved to neighborhoods with better peer delinquency behavior across any age or treatment groups. Thus, we do not find evidence that suggests peer effects are a channel that led adults in the Section 8 group to improve their delinquent behavior.

We then use data from the Census Business Patterns to examine whether MTO participants moved to neighborhoods with a greater number of physical banks or payday loan stores. We do not find that MTO families moved to neighborhoods with greater physical access to traditional lending institutions, suggesting that the benefits to credit access we observe for younger children are not likely due to physical proximity to banks. We do find, however, that younger children within the Section 8 families who reduced their payday loan usage, moved to zip codes with fewer payday loan stores. This suggests that the physical presence of payday loan stores may be an important channel through which neighborhoods affect payday borrowing behavior.

The improvements in credit access we find for younger—but not older—children are consistent with the simple model that trades off the benefits of lower poverty exposure and the costs associated with moving. These costs could include the disruption of informal support systems or lending networks in addition to the direct moving costs experienced by participants. The improved delinquency outcomes we find among adults, particularly among those assigned unrestricted Section 8 vouchers, are consistent with a model that assumes unrestricted choice should allow adults to maximize their utility and lead to better subsequent outcomes. Without an increase in income, adults among Section 8 families may have not chosen to move to the same low poverty neighborhoods. Experimental families were required to move to due to higher living costs. We further investigate this channel by proxying for the neighborhood cost of living with the average level of peer credit card balances within a zip code. We find that adults within the Experimental group moved to neighborhoods with higher average expenditures than adults in the Section 8 group, suggesting the positive impacts on delinquent behavior across adults may be due to this channel. Our results highlight the trade off between the benefits of moving to a better neighborhood on children with the barriers of being able to repay debts without greater assistance.

The paper is organized as follows. Section I provides background. Section II describes our data sources and match rates for our MTO sample. Section III describes our empirical analysis and presents our main results. Section IV concludes.

I. Background

A. *Financial outcomes among low-income populations*

Financial decision-making and access to credit among low-income populations are topics that have generated substantial interest from both policymakers and researchers. In part this is due to the fact that the poor often experience worse access to traditional financial institutions, which may reduce their ability to invest in the future and impede intergenerational socioeconomic mobility (Shao et al., 2012). Indeed, most studies find that low-income borrowers are over-represented in sub-prime borrowing populations (e.g., Adams et al. (2009)).

Exacerbating this issue is that low-income borrowers sometimes appear to make financial decisions that perpetuate poverty; for example, borrowing repeatedly at very high interest rates or

becoming delinquent on payments in a way that precludes further access to credit markets. The reasons for these choices are not well understood. Some economists assert that this is a rational response to being liquidity constrained; however, discount rates that are required to rationalize these decisions are often too high to be believable (Skiba and Tobacman, 2008). Alternatively, some have claimed that this is the result of an information failure, and that the poor would make different financial choices if provided with better information. Although there is some evidence that information provision changes decisions on the margin, it does not appear to be enough to fully explain the differences in high cost borrowing across income groups (Dobbie and Skiba (2013), Bertrand and Morse (2011)). Additionally, there has been a recent surge of interest in the “scarcity” hypothesis, which posits that experiencing scarcity, defined as having less than you feel you need (Mullainathan and Shafir (2013), p.4), itself can reduce cognitive functioning and result in worse decisions. Although this explanation is intuitively appealing, evidence on this hypothesis has been mixed (see Shah et al. (2013), Mullainathan and Shafir (2013), Carvalho et al. (2016)).

Despite the widespread interest in this topic, no work exists (to our knowledge) on the role of the neighborhood environment in shaping and informing financial choices. We believe examining the effects of neighborhood environment is an important first step to understanding what factors drive financial decisions and outcomes of low-income households. Moving out of public housing projects and into a low poverty neighborhood will provide individuals the opportunity to interact with a different set of peers in ways that can facilitate the transfer of information or provide social pressure for certain beneficial behaviors such as paying bills on time. Additionally, it could also improve access to adequate financial advice and traditional lending institutions if these institutions tend to locate in low poverty areas. At the same time, moving to a low poverty neighborhood could separate individuals from their support networks or avenues of informal borrowing (e.g. from friends and family), which might make it more difficult for families to deal with unexpected negative shocks. Low poverty neighborhoods might also be associated with higher transportation or livings costs which could create hardships for low-income families. Ultimately, the impact of neighborhood quality on access, borrowing, and delinquency behavior remains an open question.

B. The Moving to Opportunity Experiment

The Department of Housing and Urban Development (HUD) was authorized by Congress in 1992 to conduct a unique large-scale experiment across five U.S. cities named the Moving to Opportunity (MTO) experiment. The project aimed to address whether moving from a high to low poverty neighborhood would improve the socioeconomic prospects of low-income families. From 1994 to 1998, HUD randomly allocated rental assistance vouchers to households with children living in Baltimore, Boston, Chicago, Los Angeles, and New York. Participation in the program was voluntary, but due to excess demand for vouchers, the MTO program was able to allocate vouchers by randomized lottery. Families had to be currently residing in a high poverty census tract (>40 percent) and living in a public housing project or Section 8 assisted housing to apply.

MTO randomly assigned participating households into one of three groups: the Experimental group, the unrestricted (“Section 8”) voucher group, and a control group. The Experimental group received housing vouchers that could only be used in Census tracts with poverty rates less than 10 percent in the first year. This group also received additional intensive housing relocation counseling services. The Section 8 voucher group received regular unrestricted vouchers that had no location restraint. The control group experienced no change in their housing assistance. The MTO experiment enrolled a total of 4,608 low-income families into its program.

As intended, the MTO experiment created significant variation in the types of the neighborhood in which adult participants lived and their children were raised. Those who received and used the Section 8 voucher moved to lower poverty neighborhoods relative to the control group, and the Experimental group who received and used the low poverty voucher was more likely to reside in lower poverty neighborhoods relative to both the control group and the Section 8 group (see, e.g.,Kling et al. (2007)).

Initial studies of MTO found no effect of being randomized into the Experimental or Section 8 group on adult economic self-sufficiency or earnings, and mixed results for children, with some positive effects detected for female children but negative effects on male children (Kling et al., 2007). Follow-up on longer-term effects found that adults in the MTO treatment groups had improved health as evidenced by lower rates of obesity and elevated glycated hemoglobin (a measure of diabetes risk) (Ludwig et al., 2011). Additional long-term follow up found that those in the MTO

treatment groups reported improved subjective well-being and suggestive improvements in mental and physical health, but no improvements in labor market outcomes (Ludwig et al., 2012). Finally, a study using tax data analyzed the long-term impacts of MTO on both children and adults who were involved in the experiment (Chetty et al., 2016). Consistent with previous research, the authors found null effects on earnings for those who participated in MTO as adults. However, they found substantial improvements along several dimensions for children in the treatment groups who were younger than 13 at the time of random assignment. In particular, they find an approximately 30 percent increase in earnings, a 2.5 percentage point increase in college attendance rates, and lower single parenthood rates for children in the Experimental group. The authors find no effect for children who were older at random assignment.

The results from the previous MTO literature are valuable in informing any effects we might detect on financial outcomes and decisions of those who were adults at random assignment of the MTO experiment. Given studies find that MTO had no effect on earnings for this group, we would expect households randomized to receive low poverty vouchers should not have experienced changes in liquidity constraints. At the same time, their access to information, physical access to lending institutions, and peer effects are all potentially different. Thus, looking at financial outcomes in this group may help us narrow down mechanisms to information, neighborhood access, and peer effects versus just an increase in available resources. Furthermore, if we find no financial effects among adult participants and children older than 13 at random assignment, but do find effects on children younger than 13 at random assignment, it would also suggest that improved labor market and educational outcomes are important mechanisms through which neighborhoods shape financial behavior.

II. Data

A. *MTO Data*

The data used in our analysis relies on two sources. First, we obtain information on all MTO participants, including both those who were adults and children at the time of the experiment, directly from HUD. Baseline information collected on MTO participants is extremely thorough and includes variables such as employment status, income, government benefits, neighborhood

characteristics, and reasons for participating in MTO. These baseline surveys also report details on children within each MTO household, including school behaviors or learning disabilities for older children and birth weight for younger children. Detailed descriptions of all variables can be found in Sanbonmatsu and Lindau (2011).

In contrast to Chetty et al. (2016), our data do not contain information on the exact date of random assignment for each participant. Instead, we see the site at which each individual is associated, and we know the range of dates during which random assignment occurred at each site. We therefore classify individuals as being, e.g., under 13 at random assignment if they were under 13 at the midpoint of their site’s random assignment period. Although this will likely result in some mis-classification between older and younger children, this assignment mechanism should capture the broad age groups we seek to define (i.e., child or adult; younger or older children).

Following prior studies on MTO, we apply sampling weights to address changes in random assignment ratios during course of the MTO program (Kling et al. (2007), Chetty et al. (2016)). Each individual is weighted by the inverse of his or her probability of being assigned to the Experimental group. See Orr et al. (2003) for full details on sample weights construction.

B. Traditional and Alternative Credit Data

We obtain individual-level credit reports for MTO participants from Experian, one of the three major credit bureau agencies. Our credit report data contain a snapshot of a consumer’s credit profile observed annually from 2001 to 2017 in June of each year. Credit reports were matched by name and social security number (SSN), which were provided by HUD to Experian. Data was matched through a blinded process in order to protect privacy and all personally identifying information was removed from the credit records before being provided to the authors for analysis.

Our data allow us to observe adult MTO participants for up to 17 years. Those who were children only enter the data set as adults, and most commonly around the age of 20 according to Brevoort et al. (2015). Thus, all children will be eligible to enter the dataset by 2017, but we will only observe credit outcomes for children in later years.

We further link MTO participants to a novel data set provided by Clarity Services, Inc., a subsidiary credit reporting agency of Experian that specializes in the subprime consumer market. Clarity supplements information on alternative credit behavior—in particular, applications for and

use of payday loans—that is not available from traditional reporting agencies such as Experian.³ Given the low-income population of MTO participants, these types of alternative lending sources may be particularly relevant. The Clarity payday loan data are provided to us at the loan level, allowing us to see the repayment history and outcomes for specific accounts for the years 2014 to 2017. We also observe inquiries for payday loans over the same time period. We aggregate this information to the year level to conduct our analysis.

Clarity data includes over 60 million consumers, and covers over 70% of non-prime consumers across the United States. However, despite their broad coverage, Clarity only obtains this information for loans originating with lenders who use their underwriting services, so their database may not include information on all products used by each borrower. This is an important caveat to our analysis because effects on payday lending may be present but not observable in the Clarity data. Nevertheless, we believe Clarity offers the best existing coverage of payday borrowing behavior across the U.S..

In addition, we note that online payday lenders are over-represented in Clarity’s database as they are more likely to need external information when processing loan applications. These lenders provide an interesting opportunity to evaluate the extent to which neighborhood affects borrowing beyond physical access to brick-and-mortar storefront lenders.

C. Match Rate and Summary Statistics

We link the MTO data to administrative credit bureau and alternative credit records by name and social security number (SSN). A total of 15,892 individuals participated in MTO, and HUD provided valid SSNs for 11,512 of these participants, including adults and children.⁴ Of those SSNs, we matched 95.2% (10,958 individuals) to Experian and 74% (8,515) to the alternative Clarity credit data. Match rates to Experian are similar across all three treatment arms as demonstrated in Table

³Payday loans are a short-term, single payment loans named after the fact that borrowers scheduled loan repayment coincides with their next payday from their employer. Payday loans are unsecured by any collateral, but require evidence of a regular income and a checking account. Lenders will typically accept a pay stub or Social Security check as income evidence. Loan amounts are typically very small, ranging from \$50 to \$300, and very short-term, two to four weeks, depending on the timing of the borrowers income. Fees associated with these single payment loans are typically very high relative to the loan amount, ranging from \$10 to \$20 per \$100 borrowed. While loan maturation is usually set to the borrowers next payday, lenders often provide the option for borrowers to roll over or re-borrow within a few days of the due date.

⁴The number of valid SSNs was provided to the authors by Experian and validity was determined through Experian’s internal analysis. The authors themselves did not have direct access to the SSN variable.

I; note that the match rate listed for the control group contains the overall match rate, including those without valid SSNs. It is also important to note that individuals only appear in the Clarity credit data if they have used a subprime loan product cover by this dataset; we therefore have no reason to expect a 100% match rate for this dataset.

Although most individuals with a valid social security number were matched to the Experian database, younger participants are typically observed for fewer years. For example, an individual who was age 5 when his family received a voucher in 1996 might not have a credit report until age 20, in 2011. Such an individual would be observed for seven years (2011 to 2017), while his parents might be observed for 17 years (2001 to 2017). On average, we observe matched participants for 8.6 years; those who were adults at random assignment we observe for 11 years on average; those who were under age 13 at random assignment we observe for 6 years on average. As shown in Table I, the number of years for which we observe an individual does not vary across treatment arm.

The original and follow-up MTO evaluations ensure that treatment and control groups are balanced across baseline characteristics. We replicate balance tests for our linked MTO-credit sample, and find that treatment and control groups remain balanced. Out of the 52 baseline covariates from the original MTO study, we find 3 significant differences between groups at the $p < 0.05$ level and 3 additional differences significant at the $p < 0.10$ level. Given that we do not adjust these t-tests for multiple comparisons, these differences are consistent of what we would expect with random assignment.

Table I presents summary statistics and results of the balance tests for a key set of covariates.⁵ MTO families came from very poor socioeconomic conditions. Table I shows that for our matched sample, less than a quarter of the heads of household were employed, less than 40 percent had completed high school, and most (80 percent) were receiving government assistance. Nearly a third of the families had a teenage birth, and most had never been married. Nearly half reported being a victim of a crime just 6 months prior to enrolling in MTO, and more than three-quarters reported that their primary reasons for moving was to get away from gangs or drugs. Consistent with the original MTO studies, more than 90 percent of the household heads were female and African-American or Hispanic.

⁵Balance tests for all 52 covariates are available upon request.

III. Empirical Analysis and Results

In our baseline analysis, we compare financial outcomes in the Experimental group and the Section 8 group to the control group that did not experience an improvement in neighborhood quality with the following standard specification:

$$Y_i = \beta_0 + \beta_1 Exp_i + \beta_2 Sec8_i + \beta_s + \epsilon_i \quad (1)$$

where Exp_i and $Sec8_i$ are an indicators for the Experimental or Section 8 arm, respectively, with the control group serving as the omitted category. We include site fixed effects (β_s) to control for potential differences across treatment sites. In this model, β_1 and β_2 measure the differences between treatment groups and control group means. Because not all families in MTO necessarily used their voucher, this baseline specification measures the intent-to-treat (ITT) effect, i.e. the effect of being offered a voucher in the MTO program.

The ITT estimates of Equation (1) should understate the effect of using a voucher as not all families that were offered a MTO voucher actually used them. Therefore, we also provide treatment-on-treated (TOT) estimates across our outcomes. Following prior studies on MTO, we instrument actual voucher takeup with the offer of a MTO voucher. We estimate

$$Y_i = \beta_0 + \beta_1 Exp_i^{Takeup} + \beta_2 Sec8_i^{Takeup} + \beta_s + \epsilon_i \quad (2)$$

where Exp_i^{Takeup} and $Sec8_i^{Takeup}$ are now indicators for actually using the housing voucher. We estimate with 2SLS and instrument Exp_i^{Takeup} and $Sec8_i^{Takeup}$ with Exp_i and $Sec8_i$. In order to interpret these estimates as causal, we must assume being offered a MTO voucher only affects financial outcomes through actual takeup and there was no average effect on borrowing of being just offered a MTO voucher if the family did not use it. Families who chose not to move still received counseling services, but these services provided only housing search advice and excluded any general services that we think might affect credit outcomes. Given these assumptions, we interpret β_1 and β_2 of Equation (2) as the causal effect of physically moving to a low poverty neighborhood or using a traditional Section 8 voucher (Angrist et al., 1996).

Following Kling et al. (2007), we collapse our outcome variables into one summary index per

topic of outcomes in order to reduce the number of hypotheses tested and to improve our power to detect effects if present. Outcomes are grouped into three broad “domains”: access to and use of credit, delinquency behavior, and payday borrowing. Within these domains, we standardize all outcome variables into z-scores by subtracting the mean and dividing by the standard deviation of the control group. We then average these z-scores with equal weighting into one summary outcome measure, represented by Y_{it} . This allows us to test whether the outcomes taken together as a whole indicate changes within these domains. Compressing multiple measures into a single index can also improve power if each component is a noisy measure of the same underlying concept. Our focus for these domains is on the sign and significance of the coefficients rather than directly comparing the levels across the different experimental arms.

We cluster all standard errors by family since the level of MTO random assignment occurred by family. P-values are based on a clustered bootstrap with 999 repetitions. Given the number of hypotheses we consider simultaneously, we may find a few false significant estimates based on sample probability alone. Thus, for the components of the indices, we calculate domain-wise error rate adjusted p-values and report these in square brackets below, using the methods described in Westfall and Young (1993). The appendix contains additional details on the construction of the p-values and the multiple testing correction procedure.

A. Impact of MTO on Access and Use of Mainstream Credit

We first examine the impact of MTO on participants’ access to and use of mainstream credit. Table II present the ITT and TOT estimates of MTO treatment for children and Table III presents our estimates for adults. Given the differential impacts on younger children found in prior MTO studies, we split our analysis on children by age: those who were under the age of 13 at random assignment (younger) and those ages 13-17 at random assignment (older).⁶ Our outcomes for children are measured when these children are adults, beginning from when we observe their first credit report.

The first two columns of each table present the ITT And TOT estimates for the summary credit index that combines all of our credit access and use measures. Since we standardize each summary index into the z-score, the units of the summary index are standard deviations. Components have

⁶We follow the age split implemented in Chetty et al. (2016) for consistent comparison to prior results.

been oriented so that a higher index indicates greater credit access and use. Panel A of Table II reports the impacts of the MTO treatment on credit outcomes on children who were below the age of 13 at random assignment.⁷ The first two columns in this panel show that younger children within families assigned to the Experimental group experienced overall improvements in credit use and access. Both ITT and TOT impacts of the Experimental group are significant at the $p < 0.05$ level. For children below the age of 13, our TOT estimate indicates that relocating to a lower-poverty neighborhood increased the summary index of credit access by 0.08 standard deviations. Though only significant at the 10 percent level, our estimates for the Section 8 group indicate receiving and using a voucher improved credit access. We estimate that using a Section 8 voucher increased this index by 0.035 standard deviations.

Subsequent columns report the effects of the MTO treatment on each component within the credit index. Columns 3 and 4 of Table II report the impacts of the MTO program on credit scores, as provided by Experian’s Vantage Score, a model that is comparable to the Fair Isaac Corporation (FICO) credit scores. The score incorporates characteristics across a borrowers credit report including payment history, delinquencies, number of accounts, and credit applications to assess his or her likelihood to be over 90 days delinquent on loans. Vantage Scores range from 300 to 850,⁸ and scores below 600 indicate subprime borrowers.

For younger children, the mean credit score of the control group is 495.6. Vantage Scores below 500 are considered “deep subprime,” so the control group mean is only just below this critical level. Younger children assigned to the experimental group have credit scores that are 5.7 points higher than the control group; once scaled by take-up, our estimates indicate that using such a voucher increases credit scores by 11 points. Prior studies have found that a 2% increase in credit scores for subprime borrowers leads to significant differences in credit use and borrowing behavior (Gross et al. (2018), Dobbie et al. (2017)). In subsequent results, we similarly find a change in credit score of this magnitude leads to meaningful differences in credit access and borrowing behavior for voucher recipients. Furthermore, since credit history is a component of credit score, we would expect impacts to accumulate as younger individuals continue to build their credit history. In contrast to what we observe among the Experimental group, we do not find improvements in credit

⁷The oldest child in this subgroup is 35 in 2017, the last year of our data sample, and the youngest 19 years of age.

⁸This is based on the Vantage Score 3.0 model. Prior VantageScore models ranged from 501 to 900.

score for younger children assigned to the Section 8 group relative to the control group mean.

Columns 5 and 6 present the MTO impacts on credit card limits. This is the total credit limit across all credit cards active on a borrower's credit report. The average credit limit across ages in the control group is \$2930, indicating a fairly credit constrained sample; the typical adult in a credit reporting database has a credit limit of over \$20,000 on their combined credit cards (Gross et al. (2018), Miller and Soo (2018)). Younger children of the control group have lower average credit limits of \$1333. Our ITT estimates indicate that younger children assigned to the Experimental group have credit card limits that are \$425, or 32%, higher than the control group mean. The TOT estimates indicates that voucher use generated an \$821 increase in credit limits which represents a 62% increase over the control group mean. Younger children within the Section 8 group also show marginally significant higher credit limits relative to the control group mean. The TOT estimates for Section 8 younger children show an increase of \$366 with $p < 0.10$ level, which is still 20% greater than the control group mean.

We find young children in the Experimental group not only have greater credit access in terms of scores and limits, but also appear to be more active users of mainstream credit than the control group. Columns 7 and 8 report the effect of MTO on the total balance across all accounts, including non-revolving accounts such as mortgages, car loans, and student loans. The ITT estimates show that younger children assigned to the Experimental group have \$2,224 (32%) more on the total balance of their accounts than the control group, and the TOT estimate shows that voucher use resulted in \$4,298 (62%) higher balances relative to the control group mean of \$6,742. We find significant differences at the $p < 0.10$ level for young children of Section 8 families, but again smaller magnitudes. This balance results in monthly payments that are approximately 20% higher among all younger children randomized to the Experimental group, indicating that using an Experimental voucher results in monthly payments about 37% higher. We also estimate that randomization into the Section 8 group results in significantly higher payments than the control group, although the effects are not as large as those observed among the Experimental group.

Finally in columns 11 and 12, we examine the amount of revolving credit MTO participants have available relative to those in control group families. We examine this outcome separately to evaluate whether they are credit constrained: Although our results indicate that younger children in the Experimental group have higher limits, we also see that they borrow more. Available

revolving credit captures whether there is remaining credit available after accounting for the amount borrowed. We find that younger children not only qualify for greater credit limits and are more active users of credit, but they also maintain their payments such that they have significantly more credit available to use across credit cards. The ITT estimates in column 11 show that younger children in the Experimental and Section 8 group have \$341 and \$257 more credit available to them respectively. Column 12 shows that TOT estimates are \$659 and \$368 more respectively. This translates to magnitudes that are nearly 80% higher relative to a control group mean of \$829.

Panel B reports estimates for older children who were ages 13 to 17 years at random assignment. We find no significant effects on the summary credit index and the point estimates are negative. Estimated impacts across all underlying components of the credit index are generally negative, though not significant, across both Experimental and Section 8 treatment arms. These results are similar those documented Chetty et al. (2016), who find that children who moved at older ages have, if anything, worse outcomes in adulthood. These MTO participants experienced the disruption costs associated with moving, but had fewer years to reap the benefits of exposure to the lower poverty neighborhood.

Table III presents estimates across the same credit outcomes for those who were adults (18 ages and up) at the time of MTO random assignment. For adults, estimates of the summary credit index are positive, but not significant.⁹ Estimated impacts are positive for credit limits and credit available, and negative for total balances and monthly payments, though again not significant across MTO treatment groups.

B. Impact of MTO on Delinquency Behavior

In the next section, we examine the impact of MTO on delinquency behaviors recorded on individual credit reports such as overdue payments and debts at collection agencies. Tables IV and V report the impact of the MTO program on delinquency behavior for children and adults at random assignment, respectively.

Like our credit outcomes, the first column of each table reports the estimates for summary index

⁹Note that adults in the control group have much higher credit scores of 558 relative to both child subgroups, though still within the subprime category. This is because length of credit history is a determinant of credit score. Control group means for credit limits, total balances, monthly payments, and credit available are also larger than the amounts of those for the child subgroups.

across our delinquency outcomes. Estimates in Panel A of Table IV show that younger children in the Experimental treatment arm have less delinquency behavior as compared to the control group ($p = 0.057$). Our estimates show that being randomized into the Experimental group reduced delinquency behavior by 0.018 standard deviations, implying a treatment effect of low poverty voucher use of a reduction in delinquency behavior of 0.035 standard deviations. Although the estimated effects on individual components of the delinquency index are not themselves significant, we note that the estimates of the impact of an Experimental voucher on tax liens and bills sent to third party collection agencies are particularly large and negative when compared to the control group mean. Estimates for younger children in the Section 8 treatment are also negative, but much smaller and not statistically significant.

Interestingly, we find significant impacts on delinquency behavior for older children and adults within the Section 8 treatment group. These estimates indicate that adults and older children who were assigned to the Section 8 group are more likely to have overall fewer delinquencies and debts in collection relative to adults and older children in the control group by 0.02 to 0.03 standard deviations. We find no effect on older children or adults assigned to the Experimental treatment group.

The following columns report results for specific delinquency outcomes within the summary index. Columns 3 and 4 report the impact of MTO vouchers on the amounts individuals hold that are more than a month past due. For children below the age of 13, individuals within the control group hold approximately \$448 thirty days or more of past due debt. This debt is of relative significant magnitude given that Chetty et al. (2016) finds annual reported earnings of less than \$12,000. This average is much higher (\$982 and \$944) for older children and adults.

Table V shows that estimates of the components that comprise the delinquency index are also negative and significant for the adults within the Section 8 voucher group. Our estimates show significant reductions in the amount of debt that is 30 days or more past due ($p < 0.05$) and marginally significant reductions in the amount held in tax liens and court judgments. The reduction in the amount of debt 30 days or more past due for this group represents a reduction relatively to the control group mean of 23%. The TOT estimate indicates that using the voucher reduced past due debts of adults by 35% (\$333) relative to the control group average. These results remain statistically significant even after p-values are adjusted for multiple hypotheses. Older

children who were assigned to the Section 8 group also hold \$300 to \$400 lower amounts overdue, and estimates are marginally significant based on per-comparison p-values with $p = 0.102$, although p-values are higher once adjusted for multiple comparisons. Estimated effects also generally negative for adults and children assigned to the Experimental group, but not statistically significant.

Columns 5 and 6 of Table V examines the effects of MTO on tax lien amounts, or overdue taxes. Children across any of the treatment groups do not show any significant impacts on tax lien amounts, though children in the control group owe relative small amounts in taxes, less than \$150. Adults in the Section 8 group, however, do show significant and negative effects on taxes due. The TOT estimates show that an adult within a Section 8 family that took up the voucher experiences lower tax debts by \$101 – 66% decrease relative to the control group average debt.

Columns 7 and 8 examine the impacts of MTO on debts that have been taken to court. These judgments could concern, for example, unpaid rent cases or child support, and can be used to seize collateral and wages. Adults in the control group owe approximately \$671 on average, while older children hold slightly less at \$625 and younger children owe much less at \$216. We again find significantly lower debts among adults in the Section 8 voucher group relative to the control group. Our ITT estimates show Section 8 adults owe \$184 less – 27% less than the control group, and the TOT estimates show adults who took up the Section 8 voucher owe \$280 less –42% less than the control group. Impacts are marginally significant based on both per comparison and adjusted p-values, with $p < 0.10$. Adults assigned to the Experimental group also have lower debts relative to the control group, with ITT and TOT estimates of $-\$188$ and $-\$400$ respectively. Estimates are significant at the $p < 0.10$, but no longer significant once adjusted for multiple comparisons. Older children in the Section 8 group again show large negative effects on court judgments; the TOT estimate suggests that using a Section 8 voucher lowers these debts by \$456. This estimate is marginally significant at $p < 0.12$. Younger children across both treatment groups and older children in the Experimental group otherwise do not show any significant estimates on court judgment amounts.

Columns 9 and 10 of Tables IV and V examine MTO impacts on the balance held in 3rd party collection agencies and filed bankruptcies. Adults and older children within the control group hold approximately \$2000, while younger children hold slightly lower amounts of \$1775. We do not find significant impacts in collection balances for any age group.

In sum, we find suggestive evidence that young children in the Experimental group experienced lower delinquency rates, and stronger evidence that the Section 8 voucher lowered delinquency for those who were adults and older children at the time of random assignment. We estimate negative significant impacts on the amounts past due, amounts owed in court, and amounts owed in taxes for adults in the Section 8 treatment group. Estimates on individual components of delinquency are negative and marginally significant for older children in the Section 8 voucher group, but we find negative and significant effects on the overall summary delinquency index, which provides greater power. Note that the older children subgroup is of the smallest sample size, and thus likely contains more noise compared to the adult and younger children subsamples. We do not find the Experimental voucher lowers delinquency or debts for adults and older children, but we also do not find that the Experimental voucher raises delinquency or debts for across any ages.

C. Impacts of MTO on Payday Loan Usage

Given the subprime credit scores of most of the MTO sample, MTO families may turn to alternative lending options such as payday loans. Tables VI and VII reports the effects of neighborhood on MTO participants' use of payday loans.

As with our previous tables on delinquency and credit, the first column reports estimates for a payday index that summarizes the impacts of the underlying components. Panel A of Table VI show significant negative effects among younger children assigned to Section 8 families. The negative coefficient indicates that the Section 8 group holds significantly fewer payday debts relative to the control group. These estimates are negative across the whole sample with $p < 0.05$. We calculate negative impacts for adults and older children but effects are not statistically significant.

The following columns of each table reports estimates for each component of the payday summary index. Columns 3 and 4 reports the total amount held in payday loans across the year. On average across all ages, MTO participants within the control group take out approximately \$19 in payday loans per year; note that this is averaged over many people who take out \$0 in payday loans and some people who take out larger amounts.¹⁰ Estimated impacts again are negative and significant for younger children of Section 8 families. For younger children, the mean total payday

¹⁰The size of a typical payday loan in our sample is \$250, although some users take out many loans within a single year. On average, those who use payday loans take out 2.8 loans per year.

amount in the control group is \$22. The ITT estimates indicate younger children assigned to the Section 8 group on hold \$11 less on average in payday debts with $p < 0.01$. This represents a 50% decrease in payday loan usage. Younger children within families that actually took up a Section 8 voucher hold \$16 less in payday loans – a 72% decline in debt relative to the control group mean. Estimated impacts within the Experimental treatment group are negative across all subgroups, but not statistically significant. The estimated impact on payday amounts for adults within the Section 8 group are essentially equal to zero, and effects for older children are positive but not significantly different than 0.

Columns 5 through 8 of Tables VI and VII break up the total payday amounts into internet and storefront. Payday loans were originally offered alongside check cashing storefront vendors, but have more recently expanded to offer online options as well. The amount borrowed across these types are both approximately \$9 on average (including zeroes for non-users) for online and storefront within the MTO control group. Adults borrow slightly less online than younger children (\$7 versus \$10), while older children within the control group borrow nearly twice the amount (\$17) online. Younger children within the control group also borrow more than adults from physical stores (\$12 v \$7), while older children of the control group borrow much less from physical stores (\$2).

We again find the negative impacts on payday usage among younger children within Section 8 families, from both online and storefront lenders. The TOT estimates indicate that younger children within Section 8 families who used the voucher borrow \$5.5 less in online payday loans on average than children within the control group, indicating they hold nearly 50% fewer debts in internet payday loans. Younger children within Section 8 families also borrow \$10.7 less in storefront payday loans, more than 60% relative to the control group mean. The reduction in usage of online payday loans is particularly interesting because online payday loans are essentially accessible from anywhere. This suggests that the reduction in payday loan usage is not due exclusively to increased costs associated with physically accessing brick-and-mortar payday loan stores, and could instead be driven by peer effects, marketing, information, or neighborhood social norms.

Estimated impacts on storefront amounts for Section 8 younger children are also large in magnitude, indicating 80% less payday debt than the control group mean with $p < 0.05$. Effects for adults, older children, and younger children within the Experimental group for both internet and storefront payday borrowing are not significantly different from zero.

The final component of our index measures the number of payday inquiries, or applications. The control group applies for an average of 0.1 payday loans per year. Older children within Section 8 families show to make fewer payday loan applications, although the effect is not statistically significant. Other estimated impacts for all other age and treatment groups are also not significant for payday applications. Although Clarity covers a large percentage of sub-prime borrowers, and over 70 percent of MTO recipients were linked to the Clarity database, it is important to note that these data underestimate payday loan usage and inquiries. The Clarity data only contain information on payday loans and inquiries for lenders that use their underwriting services, so decreases in payday loan usage from lenders not included in the Clarity database would not be captured in our analysis.

D. Discussion and Policy Implications

Our analysis shows that the MTO experiment improved access to and use of credit across a number of measures for those who moved when they were young, with little change in these outcomes for those who moved as adults or as older children. At the same time, we find improvements in delinquency behavior among those who move as adults, particularly those in the Section 8 group who were permitted to choose any neighborhood without restrictions. We also detect some improvement in payday loan usage among children in this group into adulthood.

Previous studies give us context for interpreting these results. Other work has demonstrated that MTO was successful in inducing participants in the Experimental and Section 8 groups to move to low poverty neighborhoods (Kling et al., 2007). MTO families in both treatment arms continued to live in low poverty neighborhoods past the first year required by the program. The youngest children in the Experimental group received the greatest exposure to low poverty rates during childhood relative to older children within MTO treatment groups (Chetty et al., 2016). We find the strongest effects of MTO on credit access and use among these children with the greatest exposure to a low poverty environment, implying a causal relationship between neighborhood quality in childhood and access to mainstream credit later in life.

MTO also changed dimensions of neighborhood quality other than the poverty rate. For example, we know from prior studies of that MTO families moved to neighborhoods with better educated peers, less distress, and higher quality housing (Sanbonmatsu and Lindau (2011)). Particularly

relevant for credit market decisions, neighborhoods may have changed the credit or delinquency behavior of peers, physical access to financial intermediaries such as banks, and the cost of living for families. To provide additional context for interpreting our results, we examine whether MTO families moved to neighborhoods with different characteristics that may have impacted their credit outcomes. To do this, we use the zip code of residence in each year as recorded on the credit record to merge each observation with zip code characteristics.¹¹ First, we use a 4 million random sample of individuals from Experian’s credit report data base to derive zip code level credit outcomes. Specifically, we calculate the average in the zip code of the access and delinquency indices we reported in the previous sections. Second, we use data from the Census Business Patterns to derive the number of banks and lending institutions (NAICS 522110) and number of payday loan stores (NAICS 522291 and 522390, following Bhutto (2014)) in each zip code. In addition, we measure cost of living indirectly through peers’ expenditures, as proxied by the average credit card balance within the zip code.¹² We calculate these variables for all years we observe participants (i.e., 2001 through 2017). It is important to note that these variables characterize zip code characteristics of MTO participants many years after they received the MTO voucher.¹³ These results therefore demonstrate the persistent and long-term impact of MTO voucher receipt on these neighborhood characteristics. Also note that because we use zip codes from credit reports to define neighborhoods, these measures are the neighborhood characteristics of MTO participants in adulthood. Our estimates for MTO children therefore indicate the the characteristics of neighborhood they chose to move to as adults.

The first two columns of Table VIII and IX combines all credit access and use measures into one peer credit index. This index describes the average credit access and use of neighborhood residents within the zipcodes MTO adults moved to after random assignment and MTO children moved to into adulthood. The positive and significant coefficient reported in the first two columns of Tables VIII and IX demonstrates that being randomized into the Experimental or Section 8 group led individuals to live in neighborhoods where a typical resident has better access and greater use of

¹¹Zip code is the finest geographic level available on the credit report.

¹²Gross et al. (2018) develop a model showing the relationship between credit card balances and consumption. In this work, the authors show that in many empirical settings credit card balances are a good measure of consumption, particularly in low income populations with low levels of liquid savings such as the MTO participants.

¹³A small number of MTO participants live in zip codes for which too few people are observed in the 4 million Experian sample to calculate zip code level means. These individuals are excluded. This affects fewer than 8 percent of the observations.

mainstream credit markets.¹⁴ We observe this positive effect on neighborhood credit quality for all age groups, but this effect is largest among young children randomized into the Experimental group. We find suggestive evidence of improvements among those in the Experimental group who were randomized as older children (panel 2), although the effect sizes are smaller. These neighborhood measures mirror the results we found when analyzing participants themselves; i.e., the largest increases in access to and use of credit are among those under age 13 at random assignment, with larger effects among the Experimental group relative to those in Section 8, but little evidence of improvements for those who were older children at random assignment.

Columns 3 and 4 of Tables VIII and IX examines the average delinquency behavior of peers within neighborhoods MTO participants moved to into adulthood. We do not find any significant difference in peer delinquency behavior across any treatment groups for any age subgroup. Particularly for adults, this suggests that peer effects are not a likely channel that led adults in the Section 8 group improved their delinquent behavior.

The next columns of Tables VIII and IX examine whether MTO participants moved to zip codes with fewer payday loan stores or greater access to physical banks. Our estimates in columns 5 and 6 in Table VIII indicate that only younger children of Section 8 families moved to zip codes with fewer payday establishments. Our TOT estimate suggest that using a Section 8 voucher lowered the number of payday low stores in the zip code in adulthood by 0.16 stores, or about 8 percent among this group. In addition to being the only group for which we observe reductions in payday store access, the youngest Section 8 voucher recipients are also the only subgroup in which we observe reductions in payday loan use. Taken together, these results suggest that the physical presence of payday loan stores in a neighborhood may be an important mechanism by which neighborhoods affect subprime borrowing behavior. We find no change in the number of banks (Columns 7 and 8) in neighborhoods across any of the treatment arms and age groups, further suggesting that the benefits to credit access we observe for younger children are not likely due to physical availability of banks within a zip code.

Our results across children appear consistent with a simple model that trades off the disruption cost of moving and the benefits of moving to a better neighborhood environment. Younger children

¹⁴The underlying components are the same as those presented in Tables II and III, including credit scores and credit limits. Results available from the authors upon request.

who experienced the longest exposure to better neighborhood quality also experience the greatest benefits to credit access and use into adulthood, while children who were required to move when they were older (and thus likely incurred a greater disruption cost) do not exhibit the same benefits to their credit outcomes. These results are consistent with the patterns found for children across earnings and income outcomes in Chetty et al. (2016). The same bundle of characteristics associated with a better neighborhood that led to positive impacts on earnings likely also lead to the positive outcomes for credit decisions we observe for younger children. However, we do not find evidence that physical access to traditional banks contributed to improved credit access and use for children’s credit decisions into adulthood.

While we do not find significant impacts on credit access and use for adults and older children, we do find MTO neighborhood improved delinquency outcomes, particularly among those assigned Section 8 vouchers. These results are consistent with a model that assumes the unrestricted choice of Section 8 vouchers should allow adults to maximize their utility and lead to subsequent better outcomes. Prior studies of MTO, however, have thus far found little to no benefits to any economic outcomes across any MTO treatment group for adults and older children, including income and earnings (Chetty et al. (2016)). These results therefore represent the first significant impacts on adults’ economic outcomes.¹⁵ The null estimates on income for adults found in prior MTO studies allow us to rule out labor market earnings as an explanation for these results.

Interestingly, we do not find the same benefits to delinquencies for adults in the Experimental treatment group. The MTO program required families in the Experimental arm to move to neighborhoods of very low poverty rates, while Section 8 families could utilize their vouchers with no restriction. Moving to neighborhoods of such low poverty likely incurs higher living costs and could disrupt informal peer lending networks. Given the null estimates on income from prior studies, we know Experimental adults may have had to use credit to support their families and invest in their children. Thus in columns 9 and 10 of Table IX we examine this channel by comparing the average level of peer expenditures across treatment arms as a proxy for neighborhood cost of living. We find that while adults in both treatment arms live in zip codes where peers have higher spending on average relative to the control group, adults in the Experimental arm live in neighborhoods

¹⁵Kling et al. (2007) find significant positive impacts on mental health across adults and Ludwig et al. (2011) find improvements in physical health.

with higher average expenditures than those in the Section 8 group. Our estimates show that using a Section 8 voucher causes participants to live in zip codes in adulthood where average credit card balances are approximately \$182 higher relative to the control. Adults using an Experimental voucher move to zip codes where average balance are \$380 higher, more than double the increase in peer expenditures of those who moved using a Section 8 voucher. We find a similar pattern across children, although a smaller magnitude (\$195) among those randomized as older children. This increase in peer expenditures could indicate higher cost of living or generally higher levels of consumption in the neighborhoods where MTO participants live in adulthood, and may in part explain why we observe beneficial effects among the Section 8 group, in addition to the potential disruption of informal lending networks or support systems.

The results in Columns 9 and 10 also demonstrate that young children in both treatment arms live in zip codes where peers have higher spending on average relative to the control group. These effects are larger among the Experimental group. Our estimates show that using an Experimental voucher causes participants to live in zip codes in adulthood where average credit card balances are approximately \$425 higher. Similarly, using a Section 8 voucher leads participants to live in zip codes where average balances are \$239 higher.

Our results have several implications for policymakers. First, this analysis indicates that allowing voucher recipients to choose their own neighborhood, rather than require they move to neighborhoods designated as “high quality” by policy makers, has beneficial effects not previously documented. Although our analysis does not directly speak to the mechanism, it could be that allowing recipients to choose their own neighborhood preserves informal support systems that reduce delinquency behavior, or that Section 8 recipients are less constrained by the cost of living. Second, despite the possible disruption associated with moving to a low poverty neighborhood, we at least do not observe *increased* delinquency behavior among adults in the Experimental group. Finally, our results provide support existing evidence on the benefits of moving to a low poverty neighborhood early in childhood.

IV. Conclusion

This paper examines the impact of better neighborhood on credit and subprime credit decisions of low-income households within the Moving to Opportunity Experiment. Consistent with prior MTO studies, we find that the youngest children in the MTO experimental group experienced the greatest benefits in terms of credit access. Young children of families in the experimental arm were required to move to the lowest poverty neighborhoods (<10 percent) and received the longest exposure to low poverty neighborhoods. We find that these children have higher credit limits and credit scores in adulthood relative to the control group, along with greater active use of credit and revolving credit available. We do not find the same benefits in credit limits and scores for older children of MTO treatment groups. Like Chetty et al. (2016), we find these results consistent with a simple model in which moving generates costs, but also provides benefits that increase with the duration of exposure to the better neighborhood environment.

In contrast to prior MTO results, however, we document positive impacts for adults on other dimensions. We find adults and older children within the Section 8 voucher group experience significant improvements in delinquencies and overdue debts. Interestingly, we do not find the same benefits to delinquencies for adults in the Experimental treatment group. Our analysis suggests that adults within the Experimental group moved to neighborhoods with higher average expenditures, and thus may have been unable to repay debts as easily as those given an unrestricted choice of neighborhood. Requiring families to move to low poverty neighborhoods could have also reduced access to informal support systems and avenues of borrowing, such as from family and friends. Our results suggest that policies that aim to improve neighborhood environments may also consider providing flexibility in neighborhood choice in order to retain informal support networks already in place or provide additional assistance to keep up with increased costs of living.

We also note that in most cases, the magnitude of the effects we uncover are large and economically meaningful when compared to the baseline mean in the control group, but small when compared to the disparities observed between MTO group and the overall population. For example, the \$821 increase in average credit card limits experienced by those randomized to the Experimental arm as young children is more than 60% higher when compared to their control group counterparts. However, it is trivial compared to the average credit card limit in the population, which is

over \$20,000 (Gross et al., 2018). This implies that while improving neighborhood quality makes progress in alleviating the credit constraints of low income borrowers, it will likely not make a meaningful impact on the observed disparities between rich and poor.

Our results highlight the policy concern that many local housing authorities still face in relocating families to low poverty neighborhoods. While many acknowledge the benefits of a better neighborhoods, local authorities still face challenges on how to help families maintain economic self-sufficiency once in a higher cost neighborhood. In our conversations with local housing authorities, even if voucher recipients are able to find housing that accepts Section 8 vouchers in lower poverty neighborhoods, families face challenges in building the credit to qualify for or remain in neighborhoods with low poverty rates. Since the rental subsidy of a Section 8 voucher is set by metropolitan area, the amount of the subsidy is of lower value in a higher cost zipcode. The null effects we find on credit scores and credit limits for adults, reveal that it is still very difficult to build credit with improved improved delinquency behaviors alone. Thus, our findings highlight that while increasing low poverty exposure has important intergenerational benefits for credit behavior, assisting families in building and maintaining the credit to move to these neighborhoods remains an important policy concern.

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Table I Match Rate Across Treatment Arms and Covariate Balance among Matched Sample

	Control Group Mean	Experimental v Control	Section 8 v Control
<i>Match Quality</i>			
Linked to Credit Data	0.767	0.003 (0.009)	0.006 (0.009)
# Years Observed in Credit Data	8.55	0.153 (0.145)	0.109 (0.154)
# Years Observed: Under 13 at RA	6.23	-0.02 (0.153)	0.06 (0.165)
Number of MTO participants Linked	3406	4360	3192
<i>Baseline Characteristics</i>			
Household Head Completed GED	0.175	-0.0154 (0.0166)	-0.00737 (0.0203)
Household Head Completed High School	0.394	0.000375 (0.0216)	0.0280 (0.0265)
Household Head never married	0.380	0.0146 (0.0207)	-0.0172 (0.0248)
Household Head had teenage birth	0.260	0.00415 (0.0189)	0.00927 (0.0234)
Household victims of crime prior to MTO	0.429	0.0229 (0.0211)	-0.00395 (0.0230)
Household Head Employed	0.267	0.00318 (0.0195)	0.00706 (0.0238)
Household Head gets AFDC/TANF	0.782	0.0132 (0.0168)	0.00461 (0.0180)
Household Head Female	0.913	-0.0119 (0.0131)	-0.0120 (0.0135)
Household Head African American	0.700	0.00637 (0.0173)	-0.0341* (0.0201)
Household Head Hispanic	0.283	-0.00963 (0.0175)	0.0216 (0.0201)
Move to get away from gangs or drugs	0.782	-0.00239 (0.0175)	-0.0197 (0.0196)
Child susp./expelled in past 2 yrs.	0.0685	0.00965 (0.00801)	-0.00372 (0.00823)

Note: These summary statistics are baseline characteristics among individuals matched with a credit report only. This table presents only a sample of the full set of available covariates; see the appendix for the complete set of covariates.

Table II Effect of MTO on Credit Outcomes, Children

	Credit Index		Credit Score		Credit Limit		Total Balance		Monthly Payment		Credit Avail	
	ITT (1)	TOT (2)	ITT (3)	TOT (4)	ITT (5)	TOT (6)	ITT (7)	TOT (8)	ITT (9)	TOT (10)	ITT (11)	TOT (12)
<i>Panel A: < 13 years at Random Assignment</i>												
Experimental v Control	0.0423*** (0.000)	0.0818*** (0.001)	5.670** (0.023)	10.94** (0.026)	425.0*** (0.010)	821.1*** (0.010)	2224** (0.004)	4298** (0.005)	23.55** (0.004)	45.52** (0.003)	341.0** (0.015)	659.1** (0.017)
Section 8 v Control	0.0244* (0.062)	0.0348* (0.066)	-0.159 (0.958)	-0.276 (0.947)	256.4* (0.080)	366.0* (0.081)	1588* (0.042)	2270* (0.044)	18.43* (0.045)	26.37* (0.050)	257.2* (0.072)	367.9* (0.072)
Control Group Mean			495.6 [0.958]	0.947 [0.947]	1333 [0.081]	6742 [0.081]			120.2 [0.093]		828.7 [0.072]	
Observations	46851	46851	46851	46851	46851	46851	46851	46851	46851	46851	46851	46851
<i>Panel B: 13-17 at Random Assignment</i>												
Experimental v Control	-0.0223 (0.273)	-0.0474 (0.271)	-5.263 (0.130)	-11.19 (0.128)	-323.1 (0.249)	-687.2 (0.250)	-538.4 (0.714)	-1145 (0.715)	1.799 (0.904)	3.828 (0.903)	-287.6 (0.118)	-611.6 (0.116)
Section 8 v Control	-0.00849 (0.699)	-0.0138 (0.697)	4.002 (0.305)	6.462 (0.309)	-41.19 (0.881)	-67.34 (0.878)	-1352 (0.363)	-2188 (0.367)	-12.74 (0.358)	-20.60 (0.359)	-136.6 (0.592)	-221.6 (0.585)
Control Group Mean			519.7 [0.541]	0.552 [0.552]	2315 [0.881]	14050 [0.878]			195.2 [0.358]		1567 [0.592]	
Observations	25942	25942	25942	25942	25942	25942	25942	25942	25942	25942	25942	25942

This table reports the difference in means for recipients of Experimental or Section 8 vouchers relative to the control group. The mean of the outcome variable in the control group is reported at the bottom of each respective column. Per comparison p-values are reported in parentheses, and family-wise error rate adjusted p-values are reported in square brackets under each estimate. See text for more details. Significance levels: * = 10 percent; ** = 5 percent; *** = 1 percent

Table III Effect of MTO on Credit Outcomes, Adults

	Credit Index		Credit Score		Credit Limit		Total Balance		Monthly Payment		Credit Avail	
	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Experimental v Control	0.0163 (0.499)	0.0347 (0.494)	-0.971 (0.685) [0.685]	-2.090 (0.683) [0.683]	469.0 (0.115) [0.240]	993.6 (0.113) [0.239]	-698.8 (0.657) [0.657]	-1465 (0.660) [0.660]	-1.394 (0.935) [0.935]	-2.855 (0.938) [0.938]	444.4 (0.158) [0.280]	942.8 (0.160) [0.283]
Section 8 v Control	0.00204 (0.793)	0.00310 (0.790)	3.317 (0.193) [0.193]	5.053 (0.192) [0.192]	357.6 (0.205) [0.309]	544.2 (0.208) [0.312]	-2505 (0.106) [0.236]	-3814 (0.105) [0.229]	-13.34 (0.387) [0.387]	-20.31 (0.385) [0.385]	165.2 (0.278) [0.278]	251.3 (0.280) [0.280]
Control Group Mean			558		4374		23001		310.8		3270	
Observations	63410	63410	63410	63410	63410	63410	63410	63410	63410	63410	63410	63410

This table reports the difference in means for recipients of Experimental or Section 8 vouchers relative to the control group. The mean of the outcome variable in the control group is reported at the bottom of each respective column. Per comparison p-values are reported in parentheses, and family-wise error rate adjusted p-values are reported in square brackets under each estimate. See text for more details. Significance levels: * =10 percent; ** =5 percent; *** =1 percent

Table IV Effect of MTO on Delinquency Outcomes, Children

	Delinquency Index		30 Days Past Due		Tax Liens		Judgment Amount		Collections	
	ITT (1)	TOT (2)	ITT (3)	TOT (4)	ITT (5)	TOT (6)	ITT (7)	TOT (8)	ITT (9)	TOT (10)
<i>Panel A: Age < 13 years at RA</i>										
Experimental v Control	-0.0179* (0.057)	-0.0345* (0.058)	34.51 (0.457)	67.13 (0.449)	-125.0 (0.117)	-241.5 (0.117)	-6.409 (0.921)	-12.08 (0.922)	-220.2 (0.228)	-425.6 (0.228)
Section 8 v Control	-0.00648 (0.469)	-0.00919 (0.476)	122.9 (0.191)	176.9 (0.191)	-89.81 (0.151)	-128.4 (0.156)	72.64 (0.547)	104.5 (0.547)	-172.3 (0.256)	-246.5 (0.256)
Control Group Mean			447.5 [0.476]	[0.472]	143.8 [0.476]	[0.472]	216.5	[0.547]	1775	[0.256]
Observations	46851	46851	46851	46851	46851	46851	42654	42654	46851	46851
<i>Panel B: Ages 13-17 years at RA</i>										
Experimental v Control	-0.00477 (0.580)	-0.0101 (0.584)	-58.10 (0.674)	-123.5 (0.677)	-24.64 (0.527)	-52.40 (0.526)	-96.90 (0.554)	-208.3 (0.558)	67.18 (0.431)	142.9 (0.431)
Section 8 v Control	-0.0258*** (0.005)	-0.0417*** (0.006)	-246.6 (0.106)	-399.0 (0.108)	-26.49 (0.147)	-42.90 (0.145)	-281.4 (0.114)	-456.9 (0.115)	-143.5 (0.417)	-231.9 (0.417)
Control Group Mean			981.9 [0.190]	[0.192]	105.4 [0.147]	[0.145]	625.1	[0.146]	2028	[0.417]
Observations	25942	25942	25942	25942	25942	25942	18206	18206	25942	25942

This table reports the difference in means for recipients of Experimental or Section 8 vouchers relative to the control group. The mean of the outcome variable in the control group is reported at the bottom of each respective column. Per comparison p-values are reported in parentheses, and family-wise error rate adjusted p-values are reported in square brackets under each estimate. See text for more details. Significance levels: *=10 percent; **=5 percent; ***=1 percent

Table V Effect of MTO on Delinquency Outcomes, Adults

	Delinquency Index		30 Days Past Due		Tax Liens		Judgment Amount		Collections	
	ITT (1)	TOT (2)	ITT (3)	TOT (4)	ITT (5)	TOT (6)	ITT (7)	TOT (8)	ITT (9)	TOT (10)
Experimental v Control	0.00353 (0.718)	0.00768 (0.717)	-84.55 (0.477)	-177.9 (0.483)	77.86 (0.056)	165.9 (0.056)	-188.0 (0.348)	-400.5 (0.338)	105.6 (0.158)	224.2 (0.151)
Section 8 v Control	-0.0216*** (0.002)	-0.0329*** (0.002)	-218.9** (0.024)	-333.4*** (0.026)	0.204 (0.050)	-101.0* (0.050)	0.348 (0.014)	0.338 (0.014)	0.367 (0.996)	0.472 (0.996)
Control Group Mean			944.4	[0.026]	151.9	[0.071]	671.4	[0.069]	1915	
Observations	63410	63410	63410	63410	63410	63410	41661	41661	63410	63410

This table reports the difference in means for recipients of Experimental or Section 8 vouchers relative to the control group. The mean of the outcome variable in the control group is reported at the bottom of each respective column. Per comparison p-values are reported in parentheses, and family-wise error rate adjusted p-values are reported in square brackets under each estimate. See text for more details. Significance levels: * =10 percent; ** =5 percent; *** =1 percent

Table VI Effect of MTO on Payday Outcomes, Children

	Payday Index		Payday Amount		Internet Amount		Storefront Amount		Payday Inquiries	
	ITT (1)	TOT (2)	ITT (3)	TOT (4)	ITT (5)	TOT (6)	ITT (7)	TOT (8)	ITT (9)	TOT (10)
<i>Panel A: < 13 years at RA</i>										
Experimental v Control	0.000582 (0.956)	0.00106 (0.962)	-1.649 (0.718) [0.718]	-3.306 (0.715) [0.715]	1.159 (0.611) [0.611]	2.279 (0.613) [0.613]	-2.809 (0.456) [0.708]	-5.585 (0.451) [0.703]	0.00964 (0.158) [0.453]	0.0191 (0.159) [0.455]
Section 8 v Control	-0.0229*** (0.002)	-0.0328*** (0.004)	-11.28*** (0.002) [0.010]	-16.17*** (0.002) [0.008]	-3.809*** (0.017) [0.017]	-5.473*** (0.016) [0.016]	-7.475*** (0.028) [0.041]	-10.69*** (0.027) [0.041]	0.00202 (0.844) [0.844]	0.00281 (0.846) [0.846]
Control Group Mean			22.11		9.946		12.17		0.0857	
Observations	23204	23204	23204	23204	23204	23204	23204	23204	23204	23204
<i>Panel B: 13-17 years at RA</i>										
Experimental v Control	-0.0215 (0.694)	-0.0483 (0.701)	-5.728 (0.862) [0.862]	-12.91 (0.864) [0.864]	-7.852 (0.049) [0.100]	-17.62* (0.049) [0.099]	2.124 (0.618) [0.721]	4.713 (0.620) [0.724]	-0.0160 (0.989) [0.989]	-0.0357 (0.989) [0.989]
Section 8 v Control	-0.000378 (0.626)	-0.000664 (0.628)	5.633 (1.000) [1.000]	9.231 (1.000) [1.000]	-2.776 (0.591) [0.883]	-4.570 (0.596) [0.884]	8.409 (0.827) [0.827]	13.80 (0.826) [0.826]	-0.0310 (0.563) [0.883]	-0.0509 (0.564) [0.884]
Control Group Mean			19.93		17.13		2.803		0.146	
Observations	7716	7716	7716	7716	7716	7716	7716	7716	7716	7716

This table reports the difference in means for recipients of Experimental or Section 8 vouchers relative to the control group. The mean of the outcome variable in the control group is reported at the bottom of each respective column. Per comparison p-values are reported in parentheses, and family-wise error rate adjusted p-values are reported in square brackets under each estimate. See text for more details. Significance levels: *=10 percent; **=5 percent; ***=1 percent

Table VII Effect of MTO on Payday Outcomes, Adults

	Payday Index		Payday Amount		Internet Amount		Storefront Amount		Payday Inquiries	
	ITT (1)	TOT (2)	ITT (3)	TOT (4)	ITT (5)	TOT (6)	ITT (7)	TOT (8)	ITT (9)	TOT (10)
Experimental v Control	-0.00410 (0.148)	-0.00879 (0.140)	-0.801 (0.338)	-1.723 (0.329)	-2.623 (0.098)	-5.636 (0.099)	1.822 (0.675)	3.913 (0.683)	-0.000647 (0.308)	-0.00129 (0.313)
Section 8 v Control	-0.00527 (0.987)	-0.00822 (0.989)	-0.000826 (0.548)	-0.000654 (0.553)	-0.873 (0.765)	-1.360 (0.765)	0.872 (0.209)	1.359 (0.208)	-0.0183 (0.069)	-0.0285 (0.077)
Control Group Mean			14.58 [0.548]	[0.553]	7.077 [0.765]	[0.765]	7.503 [0.288]	[0.281]	[0.257]	0.105 [0.265]
Observations	17628	17628	17628	17628	17628	17628	17628	17628	17628	17628

This table reports the difference in means for recipients of Experimental or Section 8 vouchers relative to the control group. The mean of the outcome variable in the control group is reported at the bottom of each respective column. Per comparison p-values are reported in parentheses, and family-wise error rate adjusted p-values are reported in square brackets under each estimate. See text for more details. Significance levels: * = 10 percent; ** = 5 percent; *** = 1 percent

Table VIII Effect of MTO on Neighborhood Outcomes, Children

	Peer Credit		Peer Delinquency		Payday Stores		Banks		Peer Expenditures	
	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Panel A: Age < 13 years at RA</i>										
Experimental v Control	0.0825*** (0.000)	0.159*** (0.000)	0.00182 (0.861)	0.00349 (0.861)	0.00763 (0.844)	0.0142 (0.851)	0.197 (0.111)	0.380 (0.112)	220.2*** (0.000)	425.3*** (0.000)
Section 8 v Control	0.0697*** (0.000)	0.0999*** (0.000)	-0.00509 (0.693)	-0.00736 (0.693)	-0.113** (0.006)	-0.164** (0.006)	-0.0657 (0.625)	-0.0963 (0.616)	167.1*** (0.000)	239.3*** (0.000)
Control Group Mean	0.0502	[0.000]	0.0708	[0.693]	1.902	[0.006]	5.155	[0.616]	2632	[0.000]
Observations	41615	41615	41615	41615	41615	46851	46851	46851	41615	41615
<i>Panel B: Ages 13-17 years at RA</i>										
Experimental v Control	0.0359* (0.028)	0.0763* (0.023)	-0.00706 (0.641)	-0.0150 (0.640)	0.0359 (0.466)	0.0763 (0.474)	0.165 (0.207)	0.352 (0.209)	91.69* (0.021)	194.7* (0.019)
Section 8 v Control	0.0332 (0.055)	0.0537 (0.053)	-0.0206 (0.171)	-0.0334 (0.170)	0.0194 (0.657)	0.0314 (0.655)	0.0805 (0.610)	0.131 (0.602)	47.26 (0.284)	76.60 (0.280)
Control Group Mean	0.0228	[0.126]	-0.0517	[0.286]	1.399	[0.655]	3.910	[0.602]	2790	[0.284]
Observations	24216	24216	24216	24216	25942	25942	25942	25942	24216	24216

This table reports the difference in means for recipients of Experimental or Section 8 vouchers relative to the control group. The mean of the outcome variable in the control group is reported at the bottom of each respective column. Per comparison p-values are reported in parentheses, and family-wise error rate adjusted p-values are reported in square brackets under each estimate. See text for more details. Significance levels: *=10 percent; **=5 percent; ***=1 percent

Table IX Effect of MTO on Neighborhood Outcomes, Adults

	Peer Credit		Peer Delinquency		Payday Stores		Banks		Peer Expenditures	
	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Experimental v Control	0.0737*** (0.000)	0.156*** (0.000)	0.00286 (0.706)	0.00615 (0.699)	0.0303 (0.223)	0.0647 (0.219)	-0.00682 (0.920)	-0.0156 (0.919)	179.9*** (0.000)	380.9*** (0.000)
Section 8 v Control	0.0450*** (0.000)	0.0686*** (0.000)	-0.0106 (0.163)	-0.0162 (0.158)	-0.0241 (0.371)	-0.0367 (0.372)	0.144 (0.152)	0.219 (0.151)	119.5*** (0.000)	181.9*** (0.000)
Control Group Mean	-0.0273		-0.104	[0.158]	[0.371]	[0.372]	3.540	[0.151]	2715	[0.000]
Observations	59542	59542	59542	59542	63410	63410	63410	63410	59542	59542

This table reports the difference in means for recipients of Experimental or Section 8 vouchers relative to the control group. The mean of the outcome variable in the control group is reported at the bottom of each respective column. Per comparison p-values are reported in parentheses, and family-wise error rate adjusted p-values are reported in square brackets under each estimate. See text for more details. Significance levels: * = 10 percent; ** = 5 percent; *** = 1 percent

Appendix A. Calculation of adjusted p-values

We calculate p-values that are adjusted to account for the fact that we examine multiple outcomes within broad domains (“families”). We group outcomes into families based on topic: payday borrowing, delinquency, debt, and public records. The method that we use controls the probability that we incorrectly reject at least one true null hypothesis within a family of outcomes to the level of the test (e.g., 5 percent). We calculate these p-values using a free step-down re-sampling algorithm, following Kling et al. (2007), Anderson (2012), Finkelstein et al. (2012) and others. The algorithm is implemented as follows:

1. We generate the original treatment effect for each outcome j , $\beta_1 \dots \beta_m$, and the original p-values, $p_1 \dots p_m$, using Huber-White standard errors clustered at the family level and order these outcomes by significance, 1 to m .
2. We re-sample families from the data with replacement and re-estimate treatment effects for each outcome $(\beta_1^*, \dots, \beta_m^*)$.
3. We generate p-values under the null hypothesis by testing $\beta_j^* = \beta_j$ for each $j = 1 \dots m$ and denote each p-value as p_j^* .
4. We enforce the significance ordering of our original inference by computing $p_j^{**} = \min(p_j^*, p_{j+1}^*, \dots, p_m^*)$, where j denotes the original significance rank of the outcome, with $j = 1$ being the most significant and $j = m$ the least significant. This is referred to as enforcing monotonicity.
5. We repeat steps 2 through 4 999 times, generating many p_j^{**} s.
6. We add up the number of times that $p_j^{**} < p_j$. Call the total number S_j . We then calculate $p_j^{fwer*} = S_j/1000$. This serves as our unadjusted p-value, following Romano and Wolf (2016).
7. We enforce monotonicity by defining $p_j^{fwer} = \max(p_1^{fwer*}, p_2^{fwer*}, \dots, p_j^{fwer*})$. This ensures that larger unadjusted p-values always correspond to larger adjusted p-values.