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 analysis on the role of neighborhood quality in the financial decisions of low-income borrowers. We use individual-level data from the Moving to Opportunity experiment linked to traditional credit reports and data from an "alternative" credit bureau that tracks payday loan usage. We find that receiving a voucher to move to a lower poverty neighborhood improves credit access and reduces payday loan usage. Much of the reduction in payday loan usage results from decreased use of online payday loans, indicating that neighborhoods affect demand for these loans in ways that extend beyond simply reducing physical access to brick-and-mortar lenders.

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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, to be liquidity-constrained and 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 impede 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 may facilitate the acquisition of knowledge about available financial products or simply provide easier access to mainstream financial institutions such as banks. While a number of studies have examined the effect of neighborhoods on health, intergenerational mobility, and educational outcomes (Ludwig et al. (2013), Chetty et al. (2014a), Chetty et al. (2014b), Chetty et al. (2016)), little work exists on the role of neighborhood environment in shaping financial choices and well-being. This is an important omission in the context of understanding financial outcomes and how subsequent household financial decisions can be improved.

In this paper, we evaluate the role of neighborhood environment on financial well-being and credit use 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 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 a traditional Section 8 voucher, which provided the same rent subsidy but did not require that they move to a low poverty neighborhood (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 Ludwig et al.

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

(2012), Chetty et al. (2016), Kling et al. (2007) and others, being randomized into either treatment group resulted in a substantial improvement in neighborhood quality.

Prior studies of the Moving to Opportunity experiment 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)). 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 who were involved in the experiment (Chetty et al. (2016)). Consistent with previous research, the authors find a precise null effect 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 effect for children who were older at random assignment. Our work builds on this literature by exploring an as-yet undocumented channel through which neighborhood could affect the outcomes of low-income individuals: credit market behavior and access. This analysis is 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 find that those who were induced to move to a higher quality neighborhood through the Moving to Opportunity experiment experienced improved access to mainstream credit. We find significant improvements in some measures of credit access even among those who were adults at random assignment, who experienced no changes in income or earnings as a result of MTO. For this group, we are able to rule out liquidity explanations that may have shifted income towards reducing debt.

We find the strongest improvements in credit access among those who were young children at random assignment, who also experienced an increase in earnings as a result of MTO. These results suggest that improved credit market access is an additional benefit to childhood exposure to a low poverty neighborhood.

At the same time, it could be the case that families within the Experimental group needed to take out more credit and loans to invest in better schooling and outcomes for their children. Chetty et al. (2016) find younger children within MTO treatment groups were more likely to attend college and earn higher incomes. Consistent with these results, we also find stronger positive impacts of credit access and reduce payday usage across children into adulthood. Our results suggest that policies aimed at improving neighborhood environments for families with children could have persistent and positive impacts for credit decisions across generations.

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 reduce intergenerational socioeconomic mobility (Shao et al. (2012)). Indeed, most studies find that low-income borrowers are liquidity constrained and 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.

B. The Moving to Opportunity Experiment

Authorized by Congress in 1992, the Department of Housing and Urban Development (HUD) set out 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 in 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 public housing or project Section 8 assisted housing to apply.

MTO recruited households living in high-poverty areas in government-provided publicly housing and randomly assigned these households into one of three groups: the experimental group, the traditional ("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 <10 percent in the first year. This group also received additional intensive housing relocation counseling services. The traditional voucher group received regular Section 8 vouchers that had no location restraint, while the control group received no assistance from the MTO experiment. 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; the Experimental group who received and used the low poverty voucher was more likely to reside in low poverty neighborhoods relative to both the control group and the Section 8 group. This sharp difference in neighborhood quality induced by the MTO experiment has been used to study the role of neighborhoods on outcomes ranging from health, to education and criminal outcomes, to labor market earnings.

MTO has been studied in other contexts to understand how neighborhoods affect economic and social well-being as well as health. Initial studies 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. (2012)). 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 precise zero effects on earnings for those who participated in MTO as adults. However, they find 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. Although adult MTO participants experienced no changes in earnings, their behavior may have been affected in other ways through changes in financial decision-making or access. In particular, given the finding that MTO had no effect on earnings, 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 and peer effects are potentially different. Thus, looking at financial outcomes may help us narrow down mechanisms to information, norms, 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 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 adults and children, directly from HUD. Baseline information collected on MTO participants is extremely thorough and include 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. Instead, we see the site with 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 under 18 at random assignment if they were under 18 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 will likely be mostly capture the broad age groups we seek to define (i.e., child or adult; younger or older children).

Table I reports summary statistics for our MTO sample. 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. The weights ensure that time and cohort effects do not confound our estimates. See Orr and Kling. (2003) for full details on

sample weights construction.

B. Traditional and Alternative Credit Data

We obtain individual-level credit reports for all 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. We obtain reports for both adults and those who were children at random assignment. 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 researchers 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 dataset provided by Clarity Services, Inc., a credit reporting agency that specializes in the subprime consumer market. Clarity supplements information on alternative credit behavior not available from traditional reporting agencies such as Experian. Given the low-income population of MTO participants, alternative lending sources may be particularly relevant. These data draw from information from alternative credit sources such as payday loans, rent-to-own, retailers, and alternative auto financing, and cell service providers. The Clarity 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 and other alternative credit products 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 percent 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. 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 ex-

tent 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 SSN. HUD provided valid SSNs for 11,512 MTO participants, including adults and children. Of those SSNs, we matched 95.1 (10,952) percent to Experian and 74 percent (8,515) to the alternative Clarity credit data. Match rates are similar across all three treatment arms.

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 18, in 2014. Such an individual would be observed for four years (2014 to 2017), while his parents might be observed for 17 years (2001 to 2017). On average, we observe matched participants for 11 years; those who were children at random assignment we observe for 9 years on average; those who were under age 13 at random assignment we observe for 8 years on average.

The original and follow-up MTO evaluations ensure that treatment and control groups are balanced 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 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 at random assignment.

Table I presents summary statistics and results of the balance tests for a key set of covariates.[2] MTO families came from very poor socioeconomic conditions. Table I shows that for our matched sample, less than a quarter of the head of households were employed, less than 40 percent of 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

[2]Balance tests for all 52 covariates are available upon request.

with the original MTO studies, more than 90 percent of the household heads were female and African-American or Hispanic.

III. Empirical Analysis and Results

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

$$Y_i = \beta_i + \beta_1 I_i + \beta_2 Site_i + \epsilon_i \tag{1}$$

where I_i is an indicator for experimental, Section 8, or control group. $Site_i$ indicates fixed effects for experimental site; it is necessary to include these fixed effects because the probability of being assigned to the treatment group varied across site. This estimation measures the differences between treatment 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 in Table IV should understate the results as not all families that were offered a MTO voucher actually used them. Thus, we further 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, I_i. We estimate

$$Y_i = \beta_i + \beta_1 I_i^{Takeup} + \beta_2 Site_i + \epsilon_i \tag{2}$$

where I_i^{Takeup} is now an indicator for now actually using the treatment voucher. We estimate with 2SLS and instrument I_i^{Takeup} with I_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 MTO. 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 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 set of outcomes in order to reduce the number of hypotheses tested and to improve power. Outcomes are grouped into three broad "domains": access to 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 of the same concept into a single index can also improve power if each component is a noisy measure of some underlying concept.

It is important to note that in our setting, some domains have components with missing values. For example, utilization is the percent of revolving credit used by an individual, with higher utilization indicating less credit available. If an individual has no revolving accounts, utilization cannot be defined. As a result, z-scores for the credit access and delinquency do not have exactly mean zero for the control group. Our focus for these domains is on the sign and significance of the coefficients rather than directly interpreting the levels within the different experimental arms.

We cluster all standard errors by household 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, we calculate family 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. The Impact of MTO on Neighborhood Credit Characteristics and Physical Proximity to Credit Institutions

Previous research has demonstrated that MTO was successful in inducing participants in the experimental group to move to low-poverty neighborhoods Kling et al. (2007); Chetty et al. (2016). In this section, we briefly characterize how those neighborhoods varied in dimensions that may be relevant for credit market decisions: the behavior of neighbors and the geographic proximity of

credit intermediaries such as community banks and payday loan stores.

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 derived from two sources.[3] First, we use a 4 million random sample of Experian credit reports to derive zip code level borrowing and delinquency behavior. Specifically, we calculate the average in the zip code of the following variables: credit limit, credit score, utilization, amount 30 days past due, bankruptcies, and amount in third party collections for each year from 2001 to 2017. Second, we attain data from the Census Business Patterns to derive the number of banks and lending institutions (NAICS 522110) and number of payday loans (NAICS 522291 and 522390, following Bhutto (2014)). The first set of zip code characteristics characterize the types of peer behavior that MTO participants are exposed to while the second set capture the physical access to lending intermediaries.

We match zip codes for all years we observe participants (i.e., 2001 through 2017), so it is important to note that these variables characterize zip code characteristics of MTO participants many years after they received the MTO voucher. These describe the persistent and long-term impact of MTO voucher receipt on neighborhood characteristics. Also note that because we utilize zip codes from credit reports, these measures are the neighborhood characteristics of MTO participants into adulthood. Thus our estimates for MTO children indicate the the characteristics of neighborhood they chose to move to as adults.

The results are presented in Tables II and **??**. We find strong evidence that receiving an Experimental or Section 8 voucher substantially improved the credit market access characteristics of the zip codes recipients lived in even many years after the original programs. Estimates indicate that between 2001 and 2017, voucher recipient lived in zip codes where the typical resident had better access to formal credit relative to the average resident of zip codes where families within the control group lived, particularly in terms of credit limits and credit scores.Note that Experimental families were only required to stay in a low poverty neighborhood for one year after receiving the voucher. Thus while prior results document that some families did move back, our results show that MTO treatment still led families to remain in neighborhoods with overall improved credit access in the long run.

[3]Note that because we are using zip codes recorded on the credit record, we only observe neighborhood characteristics for those with a credit report; i.e., adults.

By subgroup, impacts on credit limits and scores are positive and significant ($p < 0.01$) for adults and younger children of both treatment groups. Older children show marginal impacts on credit limit, but no significant difference in credit scores. Only younger children show to live in neighborhoods with significantly lower utilization. The number of traditional banks within treatment group neighborhoods did not differ significantly from those of the control group across any age group comparison. Only younger children within the Section 8 voucher group show to live in neighborhoods with significantly fewer payday establishments. Note that because we average characteristics of zip

Table III reports estimates for neighborhood characteristics of delinquency behavior. Results show that Section 8 voucher recipients lived in neighborhoods with improved delinquent behaviors. Estimates show that adults assigned to the Section 8 group moved to zip codes with fewer overdue credit card amounts and debts in 3rd party collection agencies. Younger children in the Section 8 group lived in zip codes with fewer collections and bankruptcies, while we do not observe significant differences in delinquency characteristics for older children within either treatment group.[4]

Overall, voucher recipients from both treatment groups moved to neighborhoods with improved credit characteristics. Families within the Section 8 group moved to zip codes with improved peer delinquency behaviors as well. Treatment groups on average did not move to zip codes with more banks relative to the control group. Our estimates indicate that only younger children of Section 8 families moved to zip codes with fewer payday establishments. In the following sections we examine whether the MTO treatment improved the individual credit outcomes of voucher recipients themselves.

B. Impact on Access and Use of Mainstream Credit

Table IV presents the ITT and TOT estimates of MTO treatment on credit access outcomes. The following columns split the sample by those who were adults (18 and older) and those who were children (under18) at random assignment of the MTO program. Because prior MTO results have found significant impacts for younger children in particular, we further split our sample of children between those who were younger (under 13) and those who were older (13-17) at random

[4]Note children's outcomes are generally all measured into adulthood since most individuals do not incur a credit report until after 18.

assignment (Chetty et al.(2016)). Estimates include no controls except randomization site dummies. Each panel reports the estimates for the Experimental and then Section 8 group, while the third row of each panel reportsthe control group mean for reference.

The first panel presents the summary index across our credit outcomes, and each subsequent panel displays individual estimates for each of our credit outcomes. Since we standardize each summary index into the z-score, the magnitude of the summary index estimates represent the difference of the treatment means from the control group in standard deviation. We orient the signs such that a higher z-score indicates better credit access. For example, we orient the signs for utilization such that they are negative, in that higher utilization indicates less available credit. We interpret greater credit limits, greater credit scores, and more traditional banks with better credit access, and more payday establishments with poorer credit access.

The first two columns of Table IV show that those in both the Experimental and Section 8 treatment groups experienced overall improvements in credit access. Both ITT and TOT impacts of the Experimental group are significant at the $p < 0.10$ level. Point estimates indicate that relocating to a lower-poverty neighborhood increased the summary index of credit access by 0.02 standard deviations. Interestingly, impacts are even larger and more precise for those in the Section 8 treatment group. ITT and TOT estimates indicate receiving a Section 8 voucher improved credit access by 0.029 standard deviations and 0.043 standard deviations respectively, with $p < 0.05$.

Consistent with Chetty et al. (2016), we find that the significance and magnitude in the overall credit index estimates within the Experimental group are primarily driven by younger children (Table IV). Estimates for adults are positive but not significant, while estimates for older children are negative though not significant. In contrast to prior MTO results, however, we do find some significant impacts on adults within the Section 8 group. ITT and TOT impacts indicate a Section 8 voucher improved credit access for adults by 0.032 and 0.048 standard deviations, at $p < 0.10$ levels. Children in the Section 8 group show positive impacts of credit access but estimates are not significantly different than the control group. Note that adults in the control group have a positive access index on average relative to the negative index for the whole sample.

The subsequent panels show the effects of the MTO treatment on each component in the credit access index. The second panel displays 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 for

the control group is $2930, indicating a fairly credit constrained sample; the typical adult in the Experian database has a credit limit of over $20,000 on their credit cards (Miller and Soo (2018)). The ITT and TOT estimates show positive impacts on the Experimental treatment group, with an on average $374 greater credit limit or a 13% increase relative to the control group mean. TOT point estimates show that among those who ever used the voucher, moving to a better neighborhood resulted in $769 greater credit limit or 26% increase relative to the control group average. These estimates are significantly different than 0 with $p < 0.05$. The traditional Section 8 vouchers similarly show significant positive impacts on credit limits, with ITT and TOT estimates at $307 and $463 respectively at $p < 0.10$ levels. Estimates remain significant based on p-values adjusted for multiple hypotheses, which we report in square brackets below the bootstrapped per comparison p-values.

Table IV shows that impacts within the Experimental group are again driven by younger children of the MTO program. Young children of the Experimental group have credit card limits that are $425 to $506 higher than the control group with $p < 0.05$. This represents a 32% to 37% increase over the control group mean. Younger children within the Section 8 group also show significantly greater credit limits relative to the control group mean. ITT and TOT estimates for Section 8 younger children show increases of $256 and $455 at the $p < 0.10$ level, still 20% to 38% greater than the control group mean.

Estimates for older children are again negative for both treatment groups, though not significant. Impacts on credit limits for adults are positive, but also not significant. We find the direction of these results consistent with the income and earnings results in Chetty et al. (2016), who also find a negative impact for older children and positive but not significant impacts on earnings for adults. Without higher earnings, we would not expect individuals to be approved for significantly higher credit limits. Note that the average credit limit is $4374 for adults and $2315, significantly higher than the control group average for the younger children subsample. We would expect this to be the case since the youngest children of the MTO program are in early adulthood and would have much shorter credit histories.

The third panel shows the impacts of the MTO program on credit scores. We measure credit through Experian's Vantage Score, a more recent and comparable model 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[5], and scores below 600 indicate subprime borrowers.

Column 1 of the third panel shows that the control group has an average credit score of 529. Vantage Scores below 500 are considered "deep subprime," so the control group mean is only just above this critical level. Adults in the control group have slightly higher credit scores of 558 on average, while older children in the control group are only just above the subprime threshold at 519 and younger children in the control group have an average score just below at 495. Again since credit history is a major component of credit score, we would expect younger individuals tend to have much lower credit scores than the older subgroups.

As with credit limits, we find younger children in the Experimental arm of the MTO program to show the largest and most significant impacts. The ITT estimates show that young children in the experimental group have credit scores that are 5.6 points higher than the control group mean, while the TOT estimates show that those who were in families who actually took up the experimental voucher and moved when they were young have credit scores that are 8.2 points higher. While these increases are yet to have a large impact on credit access in early adulthood, the trajectory of these positive impacts could add up to significant differences in credit access later as these young adults build up their credit histories.

The estimated effect of being offered of a MTO voucher is positive across all ages and both treatment groups, although not statistically significant. Younger children in the Section 8 group do not experience an improvement in credit scores relative to the control group mean. Impacts for adults and older children are negative for the Experimental group and positive in the Section 8 group, but similarly not significant. Again given that Chetty et al. (2016) show that adults and older children experience null effects on their income and earnings, we would not expect these subgroups to be able to earn higher credit scores.

The third panel shows the estimates for utilization, i.e. the ratio of a borrower's outstanding balance relative to their credit availability, across all available credit cards. The utilization for the control group mean is relatively high across the whole sample, with borrowers on average using 70% of their available credit. For comparison, a typical individual in the Experian credit database

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

has a utilization of about 53 percent (Miller and Soo (2018)). Adults in the control group have a lower utilization of 67% while younger and older children both have slightly higher utilization rates of 75%.

Estimated ITT and TOT impacts of the MTO Experimental group are negative, but not significantly different than 0. Being offered a traditional Section 8 voucher, on the other hand, does have significant impact utilization across the overall sample at the $p < 0.01$ level. Unlike the other outcomes, this significance is driven by those who were older children and adults at random assignment of the Section 8 vouchers. The magnitude of the estimates suggest that using a Section 8 voucher reduces utilization by about 2 to 3 percentage points. The TOT estimates for older children show impacts of even lower utilization by 4 percentage points, with $p < 0.05$. Overall we find that these results are consistent with prior MTO impacts on income and earnings, and similarly align with a simple model that balances positive impacts of lower poverty neighborhood with the disruption cost of moving (Chetty et al. (2016)). Children who were young at random assignment of the MTO program and had the most years of exposure to low poverty neighborhoods experience the greatest impacts on credit access, particularly on credit limits and credit scores. Adults and older children experience little to no impacts on credit limits or credit scores, which is consistent with the null effects on their income and earnings.

We do, however, find significant impacts on lower utilization for adults and older children in families who received the Section 8 voucher. We find null effects of utilization, on the other hand, for adults and older children within the Experimental group. In addition to the disruption cost of moving, we would expect a lower poverty neighborhood to come with higher living costs. Thus adults, particularly in the experimental group, might have had to take out more credit to keep up with the costs of very low poverty ($< 10\%$) neighborhoods in order to invest in their children. Adults within the Section 8 group, who were able to choose where they used the voucher, might have been able to utilize the voucher to relieve liquidity constraints and manage their credit more effectively. While moving to a better neighborhood provides significant positive impacts for young children, we might be concerned that doing so might also have a costly impact on debts and insolvency of their parents. Thus in the next section we examine the impact of the MTO program on delinquency and debt outcomes.

C. *Impacts on Delinquency and Public Records*

Table V reports delinquencies and public records, including bankruptcies and judgments, across accounts reported on a standard credit report. As in Table IV, the first panel reports the estimates for summary index across our delinquency outcomes. The second panel reports the dollar amount individuals hold that are already more than month past due. The third panel reports debts that have been taken to court and owed through a debt collection case. The fourth panel shows the debt balance currently held by a third party collection agency. The fifth panel includes amounts owed in delinquent taxes, and the final outcome shows any filed bankruptcies of any chapter.

Estimates in the first panel show that the summary delinquency index is significant ($p < 0.05$) for participants within the Section 8 group. The negative estimate indicates that Section 8 voucher participants have lower delinquency and collections due overall. Estimates for the experimental group are also negative, but not statistically significant. As with our results on utilization, significant results for the delinquency index are driven by the adults and older children within the Section 8 treatment group. Younger children estimates in both treatment groups are also negative, but not significantly different than their relative control group mean. 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.

The second panel reports the impact of MTO vouchers on the amounts individuals hold that are more than a month past due. Across all ages, individuals within our sample control group hold approximately \$778 thirty days or more past due debt. This average is much lower for younger children (\$448) of MTO families, and much higher (\$944 and \$982) for adults and older children. This debt is of relatively significant magnitude given that Chetty et al. (2016) finds annual reported earnings of less than \$12k. Estimates again are negative and significant for the adults within the Section 8 voucher group, holding \$219 less in debt on average relative to the control group mean. This magnitude is significant ($p < 0.10$), indicating adults in the Section 8 group hold 23% less debts overdue. TOT estimates show that an adult who actually utilized a Section 8 voucher holds even lower debts of 35% (\$333) than 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 $p = 0.102$. Estimated effects are not significant, but also negative for adults and children assigned to the Experimental group. Estimates for younger children in the Experimental group are slightly positive, though not significantly different than 0 based on per-comparison or adjusted p-values.

In the next panel, we examine the impacts of MTO on debts that have been taken to court. These judgments are most often concerning unpaid rent cases or child support in our sample, 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. ITT estimates show Section 8 adults owe $184 less – 27% less than the control group, and TOT estimates show adults who took up the Section 8 voucher owe $280 less –42% less than the control group. Impacts are significant based on both bootstrapped 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 −$184 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 – $281 and $456 lower debts based on ITT And TOT estimates – that are 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 judgement amounts.

The fourth panel of Table V we estimate the ITT and TOT effects 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. Being offered a Section 8 voucher lowers tax debts by $66, a 44% decline from the control group mean. 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. Estimates for adults in the experimental treatment group indicate higher debts, but are not statistically significant.

In last two panels, we 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 across all ages or across MTO children. We similarly find very little impacts on filed bankruptcies. As we would expect for this low-income group, very few have filed for bankruptcies and the control group averages across adults and children are less than 0.01. Younger and older children show significant impacts on filed bankruptcies, but the magnitude of the impacts are marginally greater than 0. Otherwise, we find no significant differences across treatment groups within adults or children.

In sum, we find that the Section 8 voucher has the greatest impact in lowering delinquency and debts for adults and older children. We find these effects consistent with our results on lower utilization that suggest the Section 8 voucher relieves liquidity and credit constraints for adults and older children. 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 more importantly we also do not find that the Experimental voucher raises delinquency or debts for across any ages. Moving to a very low poverty neighborhood with higher living costs without any improvement in income and earnings likely places a higher burden in credit and make it difficult to keep debts current. Thus while we do not find the Experimental voucher provides the benefits on overdue debts like the Section 8 voucher, we also do not find that it places any additional burden on the adult subgroup. We may still be concerned, however, that credit-constrained adults could turn to alternative subprime forms of credit to alleviate short-term costs. Thus, we turn to examine the impacts on payday loans in the next section.

D. Impacts on Payday Loan Usage

Given the subprime credit scores of our subsample, many MTO families may face limited access to the mainstream credit market and turned to alternative lending options such as payday loans. Payday loans have been the most popular alternative credit product among borrowers in this market. Payday 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.

Tables VI reports the effects of neighborhood on MTO participants' use of payday loans. The first panel again reports a summary index across our payday outcomes. The estimates show significant negative estimates within the 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, and very significant with $p < 0.01$. We find that by subgroup, these estimates are driven by the younger children within families that received that Section 8 voucher. We calculate negative impacts for adults and older children but effects are not statistically significant. The signs of the Experimental group are also negative across all age groups, but only statistically significant for older children with $p < 0.10$.

The subsequent panels again report estimates for each component of the payday summary index. The second panel reports the total amount held in payday loans across the year. On average, 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. Estimated impacts again are negative and significant for Section 8 families only, indicating lower payday borrowing. These negative effects are again driven by younger children within Section 8 families. For younger children, the mean total payday amount in the control group

is $22. 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 credit. 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 again negative across all subgroups, but not 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.

The second and third panels break up he 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 (per loan) 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 are driven by younger children within Section 8 families, from both online and storefront lenders. ITT and TOT estimates indicate that younger children within Section 8 families borrow $4 to $5.5 less on average than children within the control group, indicating they hold nearly 50% fewer debts in payday loans. 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 primarily to increased costs associated with physically accessing brick-and-mortar payday loan stores, and could instead be driven by peer effects, information, or neighborhood social norms.

Estimated impacts on storefront amounts for Section 8 younger children, however, are even greater in magnitude, indicating 60 to 80% fewer payday debts 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 than 0.

The fourth component of our index measures the number of payday inquiries, or applications. The Clarity data likely underestimates inquiries, as they are only able to capture applications for lenders that require their underwriting services. 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 by 0.03 applications. This is statistically significant at $p < 0.10$ and represents a 30% decline on the control group mean. Otherwise estimated impacts for all other age and treatment groups are not significant for payday applications.

The final component of our payday index captures whether an individual applied for a payday loan despite having significant traditional credit available. Studies such as Pagel and Olafsson (2016) find that some payday borrowers appear to engage in temptation spending suggestive of behavioral biases, applying for additional payday loans even with standard credit at their disposal. We create a variable that indicates whether an individual applied for any payday loan while having more than 20 percent available credit on existing credit cards. The estimates for the Section 8 group are negative and significant, indicating they are less likely to engage payday inquiries with standard credit available. Interestingly, these impacts are driven by adults and children, mirroring the impacts on low utilization estimated in our credit outcomes.

IV. Discussion

This paper examines the impact of better neighborhood on credit and subprime credit decisions of low-income households with the Moving to Opportunity Experiment. We uncover three major findings across our estimates. First, younger children within the MTO treatment groups experience the largest benefits in credit access in terms of approved credit limits and credit scores. We find these results consistent with Chetty et al. (2016) estimates that also find the largest benefits on income and earnings for young children of epxerimental MTO families. Second, we find significant improvements on credit utilization, delinquencies, and overdue debts across adults and older children who received a Section 8 voucher. Relative to prior MTO results, these results represent the first significant impacts on adults' economic outcomes. Finally, we observe significant reductions in payday loan usage among younger children within Section 8 families.

Prior MTO results help us interpret these effects. Like Chetty et al. (2016), our findings for younger children within the MTO treatment groups suggest that moving to better neighborhood has significant benefits that persist across generations. The fact that we observe the strongest evidence of improved credit access among the group that also experienced the greatest increase in

income suggests that labor market earnings is an important channel through which neighborhoods can improve credit market outcomes. Nonetheless we also know from prior MTO studies that younger children were exposed to a host of other improved neighborhood characteristics including higher educated peers, fewer single-parent households, lower crime, and housing quality. Our own neighborhood analysis also shows that younger children experienced neighborhoods with better peer credit profiles both during childhood and into adulthood. Thus, the benefits we observe for younger children within MTO families also likely stem from the increased exposure to an overall better neighborhood environment.

We find little evidence that randomization into MTO treatment groups changed the access to the number of banks within resident neighborhoods. This suggests that the benefits to credit access we observe for younger children are not likely due to physical availability of banks within a zipcode. We do observe, however, both the physical presence of payday loan stores and amount of payday borrowing decline significantly for younger children within the Section 8 voucher group. This suggests that limiting access to brick-and-mortar storefronts plays a role in reducing this type of high-risk subprime borrowing.

From Chetty et al. (2016), we also know adults and older children of MTO treatment groups did not experience an increase in household income. Thus, it is not surprising that these groups did not experience significant improvements in credit limits, which may be explicitly based on predicted or reported income, or credit scores, which are indirectly a function of reported income through credit limits. We do, however, observe improved utilization and delinquency among adults and older children assigned to the Section 8 treatment group. Because previous research has provided precise null estimates of the effect of voucher receipt on earnings for adult recipients, we can rule out a labor market or earnings explanation for this result.[6] At the same time, our neighborhood analysis (Tables II and III) shows that Section 8 adults moved to zip codes with improved peer delinquency behavior, suggesting an important role for peer effects.

Nonetheless, we do not observe the same benefits to lower utilization and overdue for adults in the experimental group. From the consistently greater benefits we and prior results observe for

[6]To be eligible for MTO, families had to be already living in public housing. Those in the Section 8 group could have used the voucher to continue to subsidize the rental cost of their current living situation or move to housing that would accept a Section 8 voucher of their choosing. Thus, receiving a voucher would not have represented an additional income subsidy.

younger children of the experimental arm, our analysis and prior MTO results suggest that the parents of these families used their resources to invest in their children. Given that we know that adults did not experience higher income and earnings or greater credit access, it is unsurprising that we do not observe the same benefits to lower utilization on adults in the Experimental group because their remaining credit resources would have been used to invest in their children. This raises the concern that costs in neighborhood of much lower poverty could lead to larger delinquencies or higher usage of payday loans. The MTO program required experimental families to move to very low poverty neighborhoods of less than 10 percent. Moving to a neighborhood of such low poverty rate not only incurs a disruption cost but likely also incurs higher living costs. We do not, however, find evidence that adults assigned to the experimental group exhibited higher delinquencies or usage of payday loans.

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 lower poverty but higher cost zipcode. The null effects we find on credit scores and credit limits despite improved delinquency behaviors reveal that it is still very difficult to build credit without improved labor market earnings. Thus, our findings suggest 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
Household Head Completed GED	0.175	-0.0154	-0.00737
		(0.0166)	(0.0203)
Household Head Completed High School	0.394	0.000375	0.0280
		(0.0216)	(0.0265)
Household Head never married	0.380	0.0146	-0.0172
		(0.0207)	(0.0248)
Household Head had teenage birth	0.260	0.00415	0.00927
		(0.0189)	(0.0234)
Household victims of crime prior to MTO	0.429	0.0229	-0.00395
		(0.0211)	(0.0230)
Household Head Employed	0.267	0.00318	0.00706
		(0.0195)	(0.0238)
Household Head gets AFDC/TANF	0.782	0.0132	0.00461
		(0.0168)	(0.0180)
Household Head Female	0.913	-0.0119	-0.0120
		(0.0131)	(0.0135)
Household Head African American	0.700	0.00637	-0.0341*
		(0.0173)	(0.0201)
Household Head Hispanic	0.283	-0.00963	0.0216
		(0.0175)	(0.0201)
Move to get away from gangs or drugs	0.782	-0.00239	-0.0197
		(0.0175)	(0.0196)
Child susp./expelled in past 2 yrs.	0.0685	0.00965	-0.00372
		(0.00801)	(0.00823)
Number of MTO participants in Linked Data	3703	4760	3491

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 Neighborhood Credit Access Characteristics

	All Ages		18+ at RA		Under 13 at RA		Age 13-17 at RA	
	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Neighborhood Credit Access Index								
Experimental v Control	0.0570***	0.117***	0.0493**	0.104**	0.0821***	0.146***	0.0341	0.0724
	(0.0169)	(0.0344)	(0.0202)	(0.0426)	(0.0227)	(0.0442)	(0.0305)	(0.0645)
Section 8 v Control	0.0650***	0.0980***	0.0575***	0.0876***	0.0926***	0.123***	0.0308	0.0498
	(0.0183)	(0.0277)	(0.0212)	(0.0323)	(0.0258)	(0.0387)	(0.0341)	(0.0551)
Neighborhood Credit Limit								
Experimental v Control	980.1***	2,014***	1,004***	2,126***	1,202***	2,149***	594.6*	1,263*
	(179.2)	(360.1)	(219.6)	(456.4)	(236.6)	(459.7)	(323.1)	(676.6)
Section 8 v Control	781.2***	1,178***	714.2***	1,087***	1,044***	1,383***	460.1	745.2
	(185.9)	(280.5)	(224.1)	(341.0)	(253.6)	(373.2)	(347.7)	(562.4)
Control Group Mean	18921	18921	18598	18598	19207	19207	19225	19225
Neighborhood Credit Score								
Experimental v Control	4.716***	9.694***	4.591***	9.713***	6.081***	11.24***	2.921	6.204
	(1.093)	(2.225)	(1.345)	(2.827)	(1.392)	(2.707)	(2.084)	(4.395)
Section 8 v Control	4.654***	7.019***	4.064***	6.186***	6.630***	9.220***	2.494	4.038
	(1.212)	(1.831)	(1.405)	(2.139)	(1.707)	(2.552)	(2.269)	(3.665)
Control Group Mean	606.1	606.1	605.5	605.5	606	606	607.5	607.5
Neighborhood Utilization								
Experimental v Control	-0.0325	-0.0664	0.0767	0.165	-0.198	-0.401	0.0349	0.0741
	(0.172)	(0.354)	(0.212)	(0.450)	(0.215)	(0.428)	(0.309)	(0.656)
Section 8 v Control	-0.330*	-0.499*	-0.287	-0.437	-0.521**	-0.763**	-0.0816	-0.132
	(0.185)	(0.280)	(0.226)	(0.346)	(0.237)	(0.354)	(0.327)	(0.529)
Control Group Mean	58.20	58.20	58.06	58.06	58.54	58.54	57.95	57.95
Observations	125,361	125,361	59,536	59,536	41,613	36,795	24,212	24,212
# of Banks								
Experimental v Control	0.0601	0.124	-0.0259	-0.0560	0.221	0.278	0.205	0.435
	(0.104)	(0.215)	(0.116)	(0.247)	(0.172)	(0.332)	(0.179)	(0.379)
Section 8 v Control	0.0832	0.126	0.134	0.203	-0.00384	-0.101	0.117	0.189
	(0.116)	(0.175)	(0.125)	(0.190)	(0.181)	(0.274)	(0.215)	(0.348)
Control Group Mean	3.771	3.771	3.183	3.183	4.794	4.794	3.530	3.530
# of Payday Establishments								
Experimental v Control	-0.00261	-0.00529	0.0212	0.0451	-0.00779	-0.00543	0.0214	0.0453
	(0.0344)	(0.0708)	(0.0345)	(0.0731)	(0.0589)	(0.120)	(0.0676)	(0.144)
Section 8 v Control	-0.0534	-0.0807	-0.0182	-0.0278	-0.123**	-0.155*	0.0161	0.0260
	(0.0340)	(0.0514)	(0.0355)	(0.0540)	(0.0575)	(0.0880)	(0.0605)	(0.0976)
Control Group Mean	1.417	1.417	1.203	1.203	1.810	1.810	1.296	1.296
Observations	114,812	114,812	55,757	55,757	36,519	32,091	22,536	22,536

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 in the first column. Note that this analysis does not condition on payday loan amounts being positive. Family-wise error rate adjusted p-values are reported under each estimate. See text for more details. Significance levels: *=10 percent; **=5 percent; ***=1 percent

Table III Effect of MTO on Neighborhood Delinquency Characteristics

	All Ages		18+ at RA		Under 13 at RA		Age 13-17 at RA	
	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Neighborhood Delinquency Index								
Experimental v Control	-0.0102	-0.0210	0.00835	0.0179	-0.0346*	-0.0554	-0.00144	-0.00306
	(0.0132)	(0.0272)	(0.0152)	(0.0322)	(0.0186)	(0.0371)	(0.0249)	(0.0528)
Section 8 v Control	-0.0335**	-0.0506**	-0.0163	-0.0248	-0.0659***	-0.0980***	-0.00654	-0.0106
	(0.0148)	(0.0224)	(0.0160)	(0.0245)	(0.0222)	(0.0334)	(0.0265)	(0.0429)
Neighborhood Amount Past Due								
Experimental v Control	-18.86	-38.74	-8.131	-17.01	-20.99	-34.69	-13.00	-27.60
	(12.68)	(26.08)	(14.05)	(29.77)	(19.20)	(38.74)	(22.65)	(47.98)
Section 8 v Control	-30.13**	-45.48**	-31.85**	-48.51**	-32.76	-51.73	-10.21	-16.53
	(14.43)	(21.86)	(14.79)	(22.55)	(23.45)	(35.37)	(24.90)	(40.32)
Control Group Mean	976.4	976.4	915.7	915.7	1090	1090	926.5	926.5
Neighborhood Bankruptcies								
Experimental v Control	0.00122*	0.00251*	0.00214**	0.00453**	-0.000449	-0.000418	0.00107	0.00227
	(0.000732)	(0.00150)	(0.000945)	(0.00199)	(0.000933)	(0.00185)	(0.00135)	(0.00287)
Section 8 v Control	5.36e-05	7.75e-05	0.00127	0.00193	-0.00240**	-0.00382**	0.00174	0.00281
	(0.000840)	(0.00127)	(0.00101)	(0.00154)	(0.00116)	(0.00174)	(0.00147)	(0.00238)
Control Group Mean	0.0719	0.0719	0.0734	0.0734	0.0692	0.0692	0.0728	0.0728
Neighborhood Collections								
Experimental v Control	-27.26*	-56.00*	-11.27	-23.68	-51.46**	-86.59**	-9.972	-21.18
	(14.17)	(29.20)	(15.14)	(32.10)	(20.94)	(41.07)	(28.06)	(59.53)
Section 8 v Control	-49.15***	-74.18***	-29.60*	-45.08*	-77.07***	-107.3***	-36.37	-58.81
	(15.06)	(22.87)	(15.91)	(24.33)	(22.37)	(32.98)	(28.46)	(46.07)
Control Group Mean	1355	1355	1279	1279	1477	1477	1328	1328
Observations	125,373	125,373	59,542	59,542	41,615	36,797	24,216	24,216

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 in the first column. Note that this analysis does not condition on payday loan amounts being positive. Family-wise error rate adjusted p-values are reported under each estimate. See text for more details. Significance levels: *=10 percent; **=5 percent; ***=1 percent

Table IV Effect of MTO on Access to Credit (Annual)

	All Ages		18+ at RA		Under 13 at RA		Age 13-17 at RA	
	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Credit Access Index								
Experimental v Control	0.0219*	0.0451*	0.0174	0.0366	0.0344***	0.0474***	-0.0299	-0.0635
	(0.080)	(0.081)	(0.351)	(0.352)	(0.009)	(0.009)	(0.158)	(0.160)
Section 8 v Control	0.0286**	0.0431**	0.0317*	0.0483*	0.0138	0.0304	0.0204	0.0329
	(0.026)	(0.028)	(0.095)	(0.092)	(0.283)	(0.289)	(0.356)	(0.354)
Credit Limit								
Experimental v Control	374.1**	768.7**	469	993.6	425.0**	506.5**	-323.1	-687.2
	(0.035)	(0.035)	(0.115)	(0.113)	(0.019)	(0.021)	(0.244)	(0.249)
	[0.0831]	[0.0821]	[0.2723]	[0.2703]	[0.590]	[0.590]	[0.712]	[0.713]
Section 8 v Control	307.5*	463.1*	357.6	544.2	256.4*	449.2*	-41.19	-67.34
	(0.060)	(0.060)	(0.205)	(0.208)	(0.088)	(0.090)	(0.892)	(0.888)
	[0.0861]	[0.0881]	[0.3093]	[0.3123]	[0.181]	[0.177]	[0.932]	[0.934]
Control Group Mean	2930	2930	4374	4374	1333	1333	2315	2315
Vantage Score								
Experimental v Control	1.805	3.706	-0.971	-2.09	5.670**	8.149**	-5.263	-11.19
	(0.342)	(0.343)	(0.685)	(0.683)	(0.027)	(0.028)	(0.146)	(0.153)
	[0.5075]	[0.5085]	[0.6847]	[0.6827]	[0.676]	[0.675]	[0.840]	[0.841]
Section 8 v Control	2.978	4.489	3.317	5.053	-0.159	1.456	4.002	6.462
	(0.128)	(0.127)	(0.193)	(0.192)	(0.957)	(0.945)	(0.296)	(0.303)
	[0.1281]	[0.1271]	[0.1932]	[0.1922]	[0.096]	[0.095]	[0.999]	[1.000]
Control Group Mean	529.1	529.1	558	558	495.6	495.6	519.7	519.7
Utilization								
Experimental v Control	-0.457	-0.9	-0.472	-0.945	-0.498	-0.499	0.652	1.263
	(0.478)	(0.485)	(0.570)	(0.583)	(0.574)	(0.573)	(0.578)	(0.576)
	[0.4785]	[0.4855]	[0.7928]	[0.8068]	[0.780]	[0.784]	[0.123]	[0.120]
Section 8 v Control	-1.774***	-2.706***	-1.523*	-2.317*	-1.456	-2.947	-2.661**	-4.169**
	(0.005)	(0.005)	(0.083)	(0.084)	(0.137)	(0.138)	(0.028)	(0.029)
	[0.0210]	[0.0220]	[0.2220]	[0.2102]	[0.215]	[0.217]	[0.861]	[0.862]
Control Group Mean	70.83	70.83	67.1	67.1	75.57	75.57	75.35	75.35
Observations	136,203	136,203	63,410	63,410	46,851	41,640	25,942	25,942

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 in the first column. Family-wise error rate adjusted p-values are reported under each estimate. See text for more details. Significance levels: *=10 percent; **=5 percent; ***=1 percent

Table V Effect of Neighborhood on Delinquency (Annual)

	All Ages		18+ at RA		Under 13 at RA		Age 13-17 at RA	
	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Delinquency Index								
Experimental v Control	-0.00228	-0.00467	-0.00162	-0.00328	-0.00287	-0.00845	-0.0122	-0.0260
	(0.696)	(0.695)	(0.859)	(0.868)	(0.599)	(0.597)	(0.211)	(0.213)
Section 8 v Control	-0.0131**	-0.0198**	-0.0204*	-0.0311*	-9.87E-05	-0.0108	-0.0208**	-0.0337**
	(0.014)	(0.015)	(0.034)	(0.035)	(0.988)	(0.992)	(0.032)	(0.035)
Amount 30 Days Past Due or More								
Experimental v Control	-25.45	-52.18	-84.55	-177.9	34.51	82.54	-58.1	-123.5
	(0.668)	(0.669)	(0.477)	(0.483)	(0.438)	(0.435)	(0.671)	(0.670)
	[0.6677]	[0.6687]	[0.4775]	[0.4835]	[0.617]	[0.615]	[0.983]	[0.983]
Section 8 v Control	-96.7	-145.9	-218.9**	-333.4**	122.9	-7.858	-246.6	-399.0
	(0.108)	(0.106)	(0.024)	(0.026)	(0.193)	(0.196)	(0.102)	(0.103)
	[0.2112]	[0.2102]	[0.0240]	[0.0260]	[0.551]	[0.550]	[0.584]	[0.584]
Control Group Mean	777.8	777.8	944.4	944.4	447.5	447.5	981.9	981.9
Amount Owed through Court Judgements								
Experimental v Control	-80.41	-165	-188.0*	-400.5*	-6.409	-99.03	-96.9	-208.3
	(0.141)	(0.141)	(0.056)	(0.056)	(0.915)	(0.919)	(0.595)	(0.594)
	[0.3243]	[0.3233]	[0.2042]	[0.2082]	[0.855]	[0.855]	[0.671]	[0.669]
Section 8 v Control	-84.88	-127.1	-183.8*	-279.5*	72.64	90.32	-281.4	-456.9
	(0.167)	(0.169)	(0.050)	(0.050)	(0.557)	(0.554)	(0.128)	(0.131)
	[0.7720]	[0.9920]	[0.0721]	[0.0711]	[0.112]	[0.113]	[0.586]	[0.583]
Control Group Mean	471.1	471.1	671.4	671.4	216.5	216.5	625.1	625.1
Tax Lien Amount								
Experimental v Control	-10.52	-21.53	77.86	165.9	-125	-258.9	-24.64	-52.40
	(0.840)	(0.839)	(0.348)	(0.338)	(0.205)	(0.201)	(0.436)	(0.441)
	[0.8398]	[0.8388]	[0.3483]	[0.3383]	[0.078]	[0.080]	[0.642]	[0.645]
Section 8 v Control	-66.10*	-99.74*	-66.24**	-101.0**	-89.81	-137	-26.49	-42.90
	(0.052)	(0.048)	(0.014)	(0.014)	(0.227)	(0.224)	(0.423)	(0.428)
	[0.0721]	[0.0681]	[0.0701]	[0.0691]	[0.041]	[0.042]	[0.663]	[0.661]
Control Group Mean	140.5	140.5	151.9	151.9	143.8	143.8	105.4	105.4
Balance in 3rd Party Collections								
Experimental v Control	-4.621	-9.381	105.6	224.2	-220.2	-461.3	67.18	142.9
	(0.934)	(0.935)	(0.158)	(0.151)	(0.125)	(0.128)	(0.533)	(0.538)
	[0.9339]	[0.9349]	[0.3684]	[0.3604]	[0.315]	[0.318]	[0.633]	[0.632]
Section 8 v Control	-83.12	-125.5	0.367	0.472	-172.3	-292.1	-143.5	-231.9
	(0.188)	(0.185)	(0.996)	(0.996)	(0.180)	(0.180)	(0.162)	(0.157)
	[0.1882]	[0.1852]	[0.9960]	[0.9960]	[0.125]	[0.123]	[0.325]	[0.323]
Control Group Mean	1887	1887	1915	1915	1775	1775	2028	2028
Bankruptcies								
Experimental v Control	0.000474	0.000976	0.000931	0.00199	0.00149*	0.00273*	-0.00344**	-0.00731**
	-0.67	-0.668	-0.658	-0.661	-0.051	-0.054	-0.044	(0.045)
	[0.8839]	[0.8849]	[0.6577]	[0.6607]	[0.099]	[0.100]	[0.633]	[0.632]
Section 8 v Control	-0.00082	-0.00124	-0.00141	-0.00215	-0.00059	-0.000614	0.00105	0.00168
	-0.505	-0.508	-0.551	-0.553	-0.428	-0.419	-0.665	(0.669)
	[0.5045]	[0.5075]	[0.5506]	[0.5526]	[0.623]	[0.629]	[0.396]	[0.393]
Control Group Mean	0.00884	0.00884	0.0149	0.0149	0.00248	0.00248	0.00567	0.00567
Observations	136,203	136,203	63,410	63,410	46,851	41,640	25,942	25,942

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 in the first column. Family-wise error rate adjusted p-values are reported under each estimate. See text for more details. Significance levels: *=10 percent; **=5 percent; ***=1 percent

Table VI Effect of Neighborhood on Use of Payday Loans (Annual)

	All Ages		18+ at RA		Under 13 at RA		Age 13-17 at RA	
	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Payday Index								
Experimental v Control	-0.00646	-0.0135	-0.00744	-0.0159	0.000618	0.00114	-0.0265*	-0.0595*
	(0.226)	(0.224)	(0.442)	(0.441)	(0.928)	(0.930)	(0.058)	(0.059)
Section 8 v Control	-0.0157***	-0.0236***	-0.0124	-0.0194	-0.0204**	-0.0293***	-0.0107	-0.0175
	-0.004	-0.004	-0.178	-0.175	-0.004	-0.005	-0.635	(0.628)
Total Payday Amount								
Experimental v Control	-2.024	-4.212	-0.801	-1.723	-1.649	-3.306	-5.728	-12.91
	(0.401)	(0.404)	(0.840)	(0.841)	(0.703)	(0.700)	(0.337)	(0.332)
	[0.401]	[0.404]	[0.840]	[0.841]	[0.703]	[0.700]	[0.337]	[0.332]
Section 8 v Control	-4.517*	-6.801*	-0.000826	-0.000654	-11.28***	-16.17***	5.633	9.231
	(0.073)	(0.071)	(0.999)	(1.000)	(0.005)	(0.005)	(0.572)	(0.568)
	[0.130]	[0.126]	[0.999]	[1.000]	[0.015]	[0.014]	[0.572]	[0.568]
Control Group Mean	19.01	19.01	14.58	14.58	22.11	22.11	19.93	19.93
Payday Internet Amount								
Experimental v Control	-1.698	-3.534	-2.623	-5.636	1.159	2.279	-7.852	-17.62
	(0.183)	(0.181)	(0.071)	(0.067)	(0.593)	(0.598)	(0.101)	(0.109)
	[0.295]	[0.299]	[0.150]	[0.148]	[0.593]	[0.598]	[0.151]	[0.164]
Section 8 v Control	-2.577*	-3.878*	-0.873	-1.36	-3.809**	-5.473**	-2.776	-4.570
	(0.086)	(0.088)	(0.596)	(0.596)	(0.012)	(0.012)	(0.756)	(0.751)
	[0.086]	[0.088]	[0.596]	[0.596]	[0.012]	[0.012]	[0.756]	[0.751]
Control Group Mean	9.981	9.981	7.077	7.077	9.946	9.946	17.13	17.13
Payday Storefront Amount								
Experimental v Control	-0.326	-0.678	1.822	3.913	-2.809	-5.585	2.124	4.713
	(0.871)	(0.871)	(0.621)	(0.621)	(0.437)	(0.436)	(0.656)	(0.654)
	[0.871]	[0.871]	[0.621]	[0.621]	[0.672]	[0.671]	[0.656]	[0.654]
Section 8 v Control	-1.94	-2.923	0.872	1.359	-7.475**	-10.69**	8.409	13.80
	(0.367)	(0.365)	(0.848)	(0.848)	(0.028)	(0.029)	(0.209)	(0.214)
	[0.367]	[0.365]	[0.848]	[0.848]	[0.036]	[0.037]	[0.209]	[0.214]
Control Group Mean	9.032	9.032	7.503	7.503	12.17	12.17	2.803	2.803
Payday Inquiries								
Experimental v Control	0.00246	0.00512	-0.000647	-0.00129	0.00964	0.0191	-0.016	-0.0357
	(0.854)	(0.852)	(0.983)	(0.983)	(0.181)	(0.180)	(0.350)	(0.353)
	[0.854]	[0.852]	[0.983]	[0.983]	[0.442]	[0.443]	[0.350]	[0.353]
Section 8 v Control	-0.00958	-0.0145	-0.0183	-0.0285	0.00202	0.00281	-0.0310*	-0.0509*
	(0.468)	(0.460)	(0.584)	(0.584)	(0.838)	(0.843)	(0.071)	(0.068)
	[0.468]	[0.460]	[0.795]	[0.796]	[0.838]	[0.843]	[0.228]	[0.229]
Control Group Mean	0.102	0.102	0.105	0.105	0.0857	0.0857	0.146	0.146
Payday Inquiries despite Available Credit								
Experimental v Control	-0.00135	-0.00281	-0.00201	-0.00429	0.0000737	0.000142	-0.00449	-0.0100
	(0.124)	(0.127)	(0.209)	(0.214)	(0.939)	(0.940)	(0.047)	(0.054)
	[0.295]	[0.299]	[0.376]	[0.383]		[0.940]	[0.151]	[0.164]
Section 8 v Control	-0.00267***	-0.00402***	-0.00397***	-0.00619***	-0.00102	-0.00146	-0.00500**	-0.00821**
	0.000	0.000	(0.007)	(0.007)	(0.325)	(0.325)	(0.027)	(0.032)
	[0.008]	[0.006]	[0.188]	[0.187]	[0.325]	[0.325]	[0.199]	[0.207]
Control Group Mean	0.00966	0.00966	0.012	0.012	0.00711	0.00711	0.0121	0.0121
Observations	48,548	48,548	17,628	17,628	23,204	23,204	7,716	7,716

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 in the first column. Note that this analysis does not condition on payday loan amounts being positive. Family-wise error rate adjusted p-values are reported 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 $p_j^{fwer*} = S_j/1000$
7. We enforce monotonicity a final time 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.

Unadjusted ("per comparison") p-values are calculated using a bootstrap that resamples at the family level.