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The Moving to Opportunity Experiment: What Do Heterogeneous Estimates of the Effect of Moving Imply About Causes?

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LINK TO ABSTRACT

The Moving to Opportunity (MTO) experiment was a federally funded program conducted between 1994 to 1998. It provided housing vouchers to low-income families to use to move out of public housing—either housing projects or project-based Section 8 housing—located in high-poverty areas (Sanbonmatsu et al. 2011). The experiment was conducted between 1994 and 1998 in five cities: Baltimore, Boston, Chicago, Los Angeles and New York.

Families in the MTO were randomized into three groups:

- those who received no voucher, or subsidy, to move (referred to as the 'control' group);
- an experimental arm that received a standard, government housing voucher to move (referred to as 'Section 8 voucher' arm);
- and an experimental arm that received a housing voucher that could be used only for housing in census tracts with poverty rates below 10 percent (referred to as 'experimental voucher' arm).

In an important article, Raj Chetty, Nathaniel Hendren, and Lawrence Katz (2016) report findings from a study of the long-term impact from moving out

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of public housing during childhood. Chetty et al. (2016)—henceforth CHK—examined the adult earnings of children who participated in MTO. Adult earnings information came from federal income tax records (e.g., W2).

In most analyses of earnings, CHK included children who were born between 1976 and 1988, and look at earnings in the years between 2008 and 2012.[2] CHK conducted analyses on children stratified into two groups by age when the randomization was done between 1994 and 1998: (1) those below 13 years old, and (2) those 13 to 18 years old. Results reported by CHK are as follows:

- The estimate of the effect of moving (i.e., treatment-on-the-treated, or TOT) for younger children (<13) in the experimental voucher arm was $3,477, which is an increase of 31 percent relative to the control group mean of $11,270.
- The estimate of the effect of moving (TOT) for younger children in the Section 8 voucher arm was $1,723, which is an increase of 15 percent relative to the control group mean of $11,270.
- The estimate of the effect of moving (TOT) for older children (13 to 18) in the experimental voucher arm was *negative* $2,427, which is a decrease of 15 percent relative to the control group mean of $15,882.
- The estimate of the effect of moving (TOT) for older children in the Section 8 voucher arm was *negative* $2,051, which is a decrease of 13 percent relative to the control group mean of $15,882.

Chetty et al. (2016) highlight that escaping poverty is the key to success

CHK recognized that neighborhood poverty is correlated with many attributes of the neighborhood, saying: "The treatment effects we report in this paper should thus be interpreted as the effect of changing a bundle of neighborhood attributes rather than any one feature of neighborhood environments" (p. 869). They elaborate some of their main results as follows: "The key implication of Table 2 for our analysis of exposure effects is that the younger MTO children received a much larger dosage of exposure to improved neighborhood environments than the older MTO children. The TOT effects on post-RA

2. To be more precise: CHK look at the earnings of such an individual only for the years 2008 to 2012 during which that individual was 24 years or older. That is, they do not include not-yet-24 include income data.

[random-assignment] neighborhood poverty rates are similar for the younger and older MTO children. That is, families who took up vouchers moved to similar neighborhoods irrespective of their children's age. However, the younger children got the improvements in neighborhoods starting at younger ages. On average the younger group got 9.8 years of childhood exposure to better neighborhoods up to age 18, because they were 8.2 years old on average at RA. In contrast, those in the older group received only 2.9 years of childhood exposure to better neighborhoods on average, because they were 15.1 years old on average at RA" (870).

Despite recognizing that neighborhoods are bundles of attributes each of which may affect children's adult well-being, the authors pervasively use language suggesting that their results show that moving to a *low-poverty* neighborhood increases adult earnings of children living in high-poverty areas. Here are some quotations:

- "We find that moving to a lower-poverty neighborhood when young (before age 13) increases college attendance and earnings and reduces single parenthood rates" (CHK, 855).
- "First, we hypothesize that moving to a lower-poverty area improves long-term economic outcomes for children who were young at the point of random assignment (RA)" (856).
- "On average from the date of RA until age 18, children below age 13 at RA in the control group lived in census tracts with a mean poverty rate of 41 percent. Children whose families took up the experimental voucher lived in census tracts with 22 percentage point lower poverty rates than those in the control group on average until age 18. Those who took up the Section 8 voucher lived in census tracts with 12 percentage point lower poverty rates than the control group" (857).
- "The fact that the experimental voucher had larger effects on children's outcomes than the Section 8 voucher therefore suggests that actively encouraging families to move to lower-poverty neighborhoods—either through counseling or by restricting their choice set—increases the impacts of housing vouchers on young children's long-term economic success" (857–858).
- "[T]he Section 8 voucher increased individual earnings of young children about half as much as the experimental voucher, consistent with the fact that it reduced neighborhood poverty rates half as much" (876).
- "Nonetheless, regardless of the underlying mechanisms, the experimental results are adequate to conclude that providing subsidized housing vouchers to move to lower-poverty areas produces larger benefits for younger children" (858).

- "We conclude that the Moving to Opportunity experiment generated substantial gains for children who moved to lower-poverty neighborhoods when they were young" (859).
- "These results further support the view that moving to lower-poverty areas improves outcomes when one moves as a young child but not at older ages" (879).
- "We estimate that moving a child out of public housing to a low-poverty area when young (at age eight on average) using an MTO-type experimental voucher will increase the child's total lifetime earnings by about $302,000" (859–860).
- "In short, subsidized housing vouchers produce durable benefits that persist into subsequent generations for children who moved to lower-poverty neighborhoods at young ages" (882).

Chetty et al. (2016) highlight that disruption is the explanation of poor outcomes of older children

For older children, CHK speculated that the disruption of a child's life is harmful to older children and more important than possible benefits of a lower-poverty neighborhood, particularly because older children were less exposed to lower-poverty neighborhood life.

- "Moving as an adolescent has slightly negative impacts, perhaps because of disruption effects" (CHK, 855).
- "The point estimates suggest that, if anything, moving to a lower-poverty neighborhood had slightly negative effects on older children's outcomes" (858).
- "One potential explanation for these negative impacts at older ages is a disruption effect: moving to a very different environment, especially as an adolescent, could disrupt social networks and have other adverse effects on child development (Coleman 1988; Wood et al. 1993; South, Haynie, and Bose 2007)" (858).
- "These three facts are consistent with a simple model that combines positive exposure effects from moving to lower-poverty neighborhoods with a negative disruption cost of moving to such a neighborhood" (876).

The heterogeneous effects of MTO raise doubts about the Chetty et al. (2016) conclusions

At first glance, the conclusions reached by CHK seem plausible. The younger children in the experimental arms moved more and to neighborhoods with lower poverty, compared to younger children in the control group, and those experimental children saw increased earnings after the age of 24. Also, the increase in earnings associated with moving for younger children in the experimental voucher arm was greater than it was for children in the Section 8 voucher arm, and the former group moved to lower poverty neighborhoods than the latter group.[3] Similarly, moving was associated with lower earnings for older children in the two experimental arms and there was little difference in the magnitude of estimates across experimental arms.

The estimates reported in CHK are average effects of moving on earnings for children across all five experiments (i.e., sites). Estimates for children from individual sites, found in the Online Appendix (**link**), were much more heterogeneous (see CHK, Appendix Table 7B). For example, the intention-to-treat (ITT) estimate of the effect of being randomized into the experimental voucher arm for young children in Baltimore was $415; in Boston, the analogous estimate was $2,619. In the Appendix, heterogeneous estimates were reported for other sites and age groups too. In the article itself, CHK provide only a brief assessment of the heterogeneity and conclude that the heterogeneous results are consistent with the main conclusions.

- "We find no systematic differences in the treatment effects of MTO on children's long-term outcomes by gender, race, *or site*" (CHK, 859, my emphasis).
- "In summary, the main lesson of the heterogeneity analysis is that the long-term benefits of childhood exposure to lower-poverty neighborhoods are highly robust across genders, racial groups, *and geographic locations*" (884, my emphasis).

3. Whether the estimate of the effect of moving on earnings among younger children differed significantly across experimental arms is unclear and unreported by CHK. There is considerable overlap between the confidence intervals of the two estimates.

In the article proper, there is no examination of site-specific results. For example, the word "Baltimore" appears only twice, and only in cursory listing along with the other four sites, thus "Baltimore, Boston, Chicago, Los Angeles, and New York" (CHK, 860, 884).

In this article, I explore the heterogeneity across sites to assess whether the pattern of estimates across and within sites is consistent with the explanations proposed by CHK. Those conclusions were based on the average estimates across all five sites. I conclude that the more detailed evidence does not support the two principal explanations offered by CHK: that neighborhood poverty is the key part of the explanation of the results for younger children, and that disruption is a likely explanation of the findings for older children. Moving increased adult earnings of young children on average, but there is little relationship between the poverty of the destination neighborhood and the earnings increase across or within experimental site. Similarly, moving was associated with similar decreases in adult earnings among older children across both experimental arms *on average*, but this is not the case in four out of five sites!

The low level of statistical power of the MTO experiment (e.g., lack of statistically significant estimates) and the incongruence between the explanations of the change in earnings associated with moving based on average estimates and the heterogeneous estimates from the different experimental sites suggest that we still do not know what caused the observed changes in earnings reported by CHK. Therefore, designing current (e.g., Seattle) and future experiments based on the CHK conclusions may be misguided. The uncertainty over the causes of the MTO results suggest that current and future experiments should be undertaken with a more exploratory approach. To me it seems much more prudent and sensible to design experiments that explore mechanisms besides neighborhood poverty, for example, moving to a neighborhood with better schools (e.g., Laliberté 2020), less crime, closer to jobs, or with greater walkability and infrastructure regardless of the poverty level.

Calculating TOT estimates by experimental site

In the Online Appendix, CHK reported ITT estimates of the effect of being randomized into the two experimental arms on earnings by child age (0–12, 13–18) and city/site. However, they do not report analogous TOT estimates. Therefore, I calculated them using the fact that the TOT estimate is the ITT estimate reported by CHK divided by the housing voucher (moving) take-up rate. The voucher take-up rate for each group was estimated from data provided by Jens Ludwig et al. (2012) who made available data from the MTO experiment. These data can be used to replicate published estimates from the final impact evaluation of the MTO experiment.[4]

The voucher take-up rates are ITT estimates of the difference in mean voucher use (moving) between families in the two treatment arms and families in the control arm. The sample consists of the 3,273 families present at the long-term follow-up.[5] I assume that the voucher take-up rate is the same for families of younger and older children. ITT estimates of the take-up rate by site and experimental group are shown in Table 1.[6]

TABLE 1. ITT estimates of voucher take-up rate by experimental group and city/site

City/Site	Experimental voucher	Section 8 voucher
Baltimore	0.551	0.769
	(0.044)	(0.047)
Boston	0.427	0.560
	(0.038)	(0.040)
Chicago	0.334	0.673
	(0.036)	(0.039)
Los Angeles	0.630	0.718
	(0.034)	(0.038)
New York	0.483	0.446
	(0.036)	(0.039)

Notes: Author's calculations using data from Ludwig et al. (2012). Standard errors in parentheses.

As indicated in Table 1, voucher take-up rates were relatively higher among families in the Section 8 voucher arm than in the experimental voucher arm (except in New York) and varied considerably across sites. Among families in the experimental voucher arm, the voucher take-up rate ranged from 0.33 (Chicago) to 0.63 (Los Angeles). A similar wide range characterizes estimates of take-up rates among families in the Section 8 voucher arm: 0.45 to 0.77. The substantial differences in take-up rates likely reflects the significant differences in the characteristics of participants and cities (e.g., housing market) across sites, and

4. See the NBER website that provides much useful information about the MTO study (**link**). The data are derived from aggregate data that "have been expanded to a pseudo individual level dataset (n=3273) that for each outcome mimics that outcome's mean value, standard deviation, and approximate number of observations within a cell" (**link** to documentation file; quotation is from its page 4).

5. This is a smaller sample than that used in CHK, who used 4,604 families who were initially randomized. To calculate the mean within each group, I follow the documentation (**link**) of Ludwig et al. (2012).

6. These are similar to the voucher take-up rates reported in the *Final Impacts Evaluation* (Sanbonmatsu et al. 2011, 15, Exhibit 1.4).

these differences highlight why an "average" effect found by CHK may not be particularly relevant to any specific city or location.

I use these voucher take-up rates along with the ITT earnings estimates reported by CHK to calculate TOT estimates of the effect of moving on earnings. In their Online Appendix CHK reported ITT estimates for earnings for the two randomized groups by site and I reproduce those estimates here in Table 2.

TABLE 2. ITT estimates for earnings by experimental group, child age and city/site

	Experimental voucher		Section 8 voucher	
City/Site	Younger children	Older children	Younger children	Older children
Baltimore	414.9	2720.4	865.1	−1120.4
	(1435.8)	(2235.1)	(1587.3)	(1903.2)
Boston	2618.7	−3456.7	3028.6	−1410.0
	(1713.2)	(2105.8)	(1824.3)	(2339.6)
Chicago	681.2	−2336.1	797.2	−4631.9
	(877.8)	(1467.8)	(1096.1)	(1525.2)
Los Angeles	2791.5	−508.3	964.7	936.9
	(1554.9)	(1629.8)	(1413.7)	(1699.8)
New York	1652.4	−583.2	−353.5	206.5
	(1612.4)	(2051.0)	(1428.2)	(2423.6)

Source: Chetty et al. (2016, Appendix Table 7b). *Note*: Standard errors in parentheses.

The TOT estimates of the effect of moving on earnings can be calculated from these ITT estimates by dividing estimates in Table 2 by the take-up rates in Table 1.[7] The heterogeneity of ITT estimates reported by CHK is reflected in the TOT estimates in Table 3. The impact of moving among younger children in the experimental voucher arm ranged from $754 in Baltimore to $6,090 in Boston. The analogous range of TOT estimates for younger children in the Section 8 voucher arm was −$786 in New York to $5,408 in Boston. Note how Boston has largest effects for both groups. Among older children in the experimental voucher arm, moving was associated with a change in earnings of between −$8,039 in Boston to $4,946 in Baltimore. The analogous range of TOT estimates for older children in the Section 8 voucher arm was −$6,913 in Boston to $1,301 in Los Angeles. Again, Boston has the largest effects.

7. To calculate standard errors of the TOT estimates in Table 3, I assumed that the ITT estimates in Tables 1 and 2 were normally distributed random variables with the mean being the ITT estimate and the standard deviation being the standard error. I then drew 5,000 samples of each ITT estimate and calculated the TOT for each of the 5,000 draws. The standard deviation of 5,000 TOT estimates is used as the standard error in Table 3. To assess the accuracy of this approach, I did the same thing using the ITT estimates reported in CHK. The standard errors I obtained using this approach were very close (i.e., within 3 percent) to those reported by CHK.

TABLE 3. Calculated TOT estimates of the effect of moving on earnings by child age and city/site

	Experimental voucher		Section 8 voucher	
City/Site	Younger children	Older children	Younger children	Older children
Baltimore	754.4	4946.2	1123.5	−1455.1
	(2627.5)	(4112.0)	(2071.8)	(2484.4)
Boston	6090.0	−8038.8	5408.2	−2517.9
	(4061.2)	(5070.7)	(3301.3)	(4237.4)
Chicago	2064.2	−7079.1	1189.9	−6913.3
	(2660.5)	(4570.2)	(1674.3)	(2305.5)
Los Angeles	4431.0	−806.8	1339.9	1301.3
	(2502.1)	(2575.3)	(2002.8)	(2386.1)
New York	3442.5	−1215.0	−785.6	458.9
	(3389.7)	(4213.8)	(3246.3)	(5524.0)

Notes: Author's calculations. Standard errors in parentheses.

Assessing explanations of the effect of moving on earnings

CHK argue that neighborhood poverty is a likely explanation of the average estimates of the effect of moving on earnings they found for younger children. Is the hypothesis consistent with the heterogeneity of estimates in Tables 2 and 3?

I used the same data provided by Ludwig et al. (2012) to calculate the TOT effect of moving on neighborhood poverty for each experimental arm and site. I used the neighborhood (Census tract) poverty rate five years post randomization. The ITT estimates for neighborhood poverty are presented in Table 4.

The TOT estimates of the effect of moving on neighborhood poverty are calculated by dividing estimates in Table 4 by analogous estimates of the ITT estimate of take-up in Table 1.[8] Again, I assume that younger and older children in each arm moved to the same places. Table 5 shows the calculated TOT estimates.

Estimates in Table 5 indicate that there is significant variation across sites in the effect of moving on neighborhood poverty within experimental arms, and between arms within a site. For example, among families in the experimental voucher arm in Baltimore, moving lowered neighborhood poverty by 14 percentage points relative to the control group. In Chicago, Los Angeles, and New York, the analogous estimates are approximately 23 percentage points. Similar heterogeneity characterizes TOT estimates of the effect of moving on poverty

8. Standard errors are calculated as described in footnote 7.

among the Section 8 voucher families, although the change in poverty is lower than that among families in the experimental voucher arm.

TABLE 4. ITT Estimates of neighborhood poverty by experimental group and city/site

City/Site	Experimental voucher	Section 8 voucher
Baltimore	−0.077	−0.057
	(0.018)	(0.020)
Boston	−0.081	−0.058
	(0.012)	(0.013)
Chicago	−0.075	−0.038
	(0.018)	(0.019)
Los Angeles	−0.139	−0.127
	(0.015)	(0.016)
New York	−0.111	−0.058
	(0.013)	(0.014)

Notes: Author's calculations using data from Ludwig et al. (2012). Standard errors in parentheses.

TABLE 5. Calculated TOT estimates of the effect of moving on neighborhood poverty by experimental group and city/site

City/Site	Experimental voucher	Section 8 voucher
Baltimore	−0.14	−0.07
	(0.036)	(0.026)
Boston	−0.19	−0.10
	(0.034)	(0.025)
Chicago	−0.23	−0.06
	(0.061)	(0.028)
Los Angeles	−0.22	−0.18
	(0.026)	(0.024)
New York	−0.23	−0.13
	(0.032)	(0.034)

Notes: Author's calculations using data from Ludwig et al. (2012). Standard errors in parentheses.

Using the TOT estimates of the effect of moving on earnings and the TOT estimates of the effect of moving on poverty, I assess whether there is a correlation between these estimates, which would be expected if neighborhood poverty was an important explanation of the earnings effects. Focusing on the results for younger children, I plot estimates of the effect of moving on earnings from Table 3 against estimates of the effect of moving on neighborhood poverty from Table 5. If neighborhood poverty is an explanation of the effects of moving on earnings, it is reasonable to expect that there is a dose-response (positive) association where larger earnings estimates are associated with larger poverty changes.[9] Figure 1 graphs the relationship.

Figure 1. Estimates of the effect of moving on earnings (Y) and neighborhood poverty (X) by site for young children in experimental voucher and Section 8 voucher arms

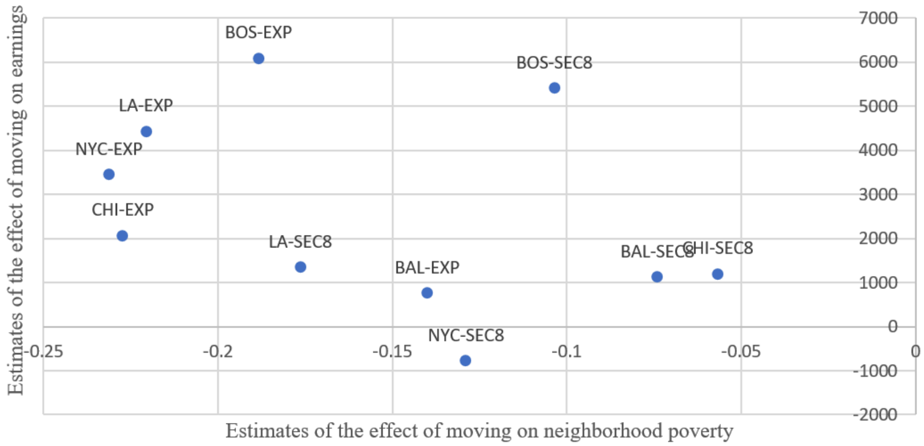


Figure 1 shows that, at best, there is only a weak positive relationship between estimates of the effect of moving on earnings and the change in poverty. An OLS regression of the earnings estimates on the poverty estimates yields an insignificant coefficient on poverty (p-value 0.34) in regressions both unweighted and weighted (by number of children at time of randomization in each site and experimental arm). The coefficient suggests that every 10 percentage-point decrease in poverty is associated with a $1,125 (approximately 10 percent) increase in earnings of younger children.[10] Note that Figure 1, and the regression results associated with it, are across sites and does not take into account a site-specific effect that is obvious in the data. For example, earnings effects are relatively high in Boston and low in Baltimore.

While there is a relatively weak positive relationship between the effect of moving on earnings and the effect of moving on poverty across sites, that is not the evidence that CHK relied on to support their claim. Instead, the primary evidence used by CHK to support their claim is the average differences between the effect

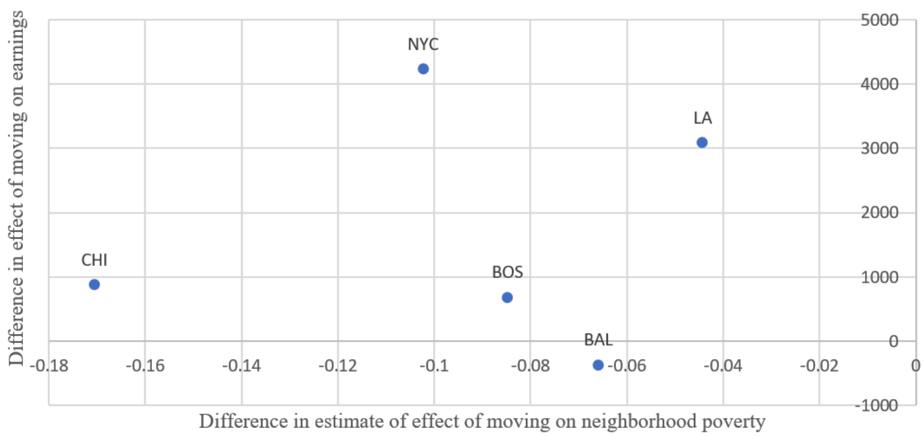
9. For a similar analysis see Kling et al. 2007, Figure 2.

10. CHK (p. 873) report the ratio of the TOT estimate of the effect of moving on earnings and the TOT, which yields a measure of the earnings change per unit of poverty change. The calculated ratios indicate that a 10 percentage-point reduction in poverty is associated with a 13 percent earnings gain for the Section 8 voucher children and a 15 percent earnings gain for children in the experimental voucher arm. Note that the ratio does not calculate the association between the change in poverty and change in earnings, which is what my calculation above does.

of moving on younger children's earnings and neighborhood poverty between the experimental voucher and Section 8 voucher arms (see quotes above). Again, such argumentation loses sight of how heterogeneous the results are between sites.

To assess the evidence used by CHK to support the poverty explanation more fully, I plot the difference in estimates of the effect of moving on earnings between experimental arms (experimental voucher minus Section 8 voucher) *within a site* against the difference in the effects of moving on poverty between experimental arms *within a site*. This approach accounts for site-specific effects. If poverty was an explanation of the estimates of the effect of moving on earnings, then it is reasonable to expect the difference in estimates of the effect of moving on earnings between experimental arms within a site to be positively related to the difference in the effect of moving on poverty between experimental arms within a site.

Figure 2. Difference in estimates of the effect of moving on earnings (Y) and moving on poverty between experimental voucher and Section 8 voucher arms for young children by site



As observed in Figure 2, there is no relationship between the difference in the effect of moving on earnings between experimental arms and the difference in the effect of moving on poverty between experimental arms. This no-relationship finding is obscured in CHK because of their focus on average effects. While there is a larger earnings gain and a larger poverty reduction for children in the experimental voucher arm than in the Section 8 voucher arm, that relationship only holds on average. The same dose-response relationship is not present in the underlying constituent parts of that average. Differences in earnings between experimental arms are large in Los Angeles and New York while differences in poverty between experimental arms are large in Boston, Chicago, and New York.

Unlike New York, however, differences in earnings between experimental arms in Boston and Chicago are relatively small. Overall, the heterogeneity of results by experimental site and experimental arm are not supportive of the poverty explanation for the effect of moving on earnings.

As for the effects of moving on earnings for older children, CHK suggest that the disruption of moving is a plausible explanation. If so, then whether a child was in the experimental voucher arm or Section 8 voucher arm should matter little because the disruptive effects of moving are arguably the same in both cases. Figure 3 plots the difference in estimates of the effect of moving on earnings between experimental arms (experimental voucher minus Section 8 voucher) for older children.

Figure 3. Difference in estimates of effect of moving on earnings (Y) between experimental voucher and Section 8 voucher arms for older children by site

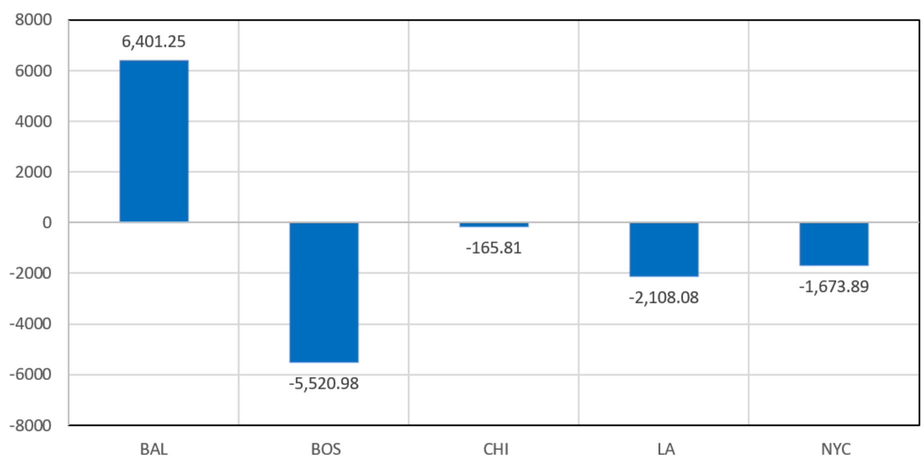


Figure 3 shows that the difference in estimates of the effect of moving on earnings by experimental arm among older children were similar in Chicago, somewhat different (roughly −\$2000) in LA and New York, and very different in Baltimore and Boston. Thus, evidence in Figure 3 does not lend support to the disruption explanation for the earnings results of older children. In only one case is the effect of moving on earnings among older children relatively similar for those in the experimental voucher and Section 8 voucher arms.

Differences in the effect of moving on poverty by experimental arm and site are unlikely to reconcile the results in Figure 3 with the disruption explanation. For example, in Chicago, older children in the experimental voucher arm experienced a much larger reduction in poverty than older children in the Section 8 voucher arm. If living in a lower poverty neighborhood was beneficial for children and offset some of the disruption effect of moving, then the differences between the experimental voucher and Section 8 voucher groups in Chicago should be the largest. In fact, they are the smallest. Similarly, in Baltimore, the difference in poverty among children in the experimental voucher and Section 8 voucher arms was relatively small, but older children in the experimental voucher arm had much higher earnings than older children in the Section 8 voucher arm. Overall, the heterogeneity of results across sites and experimental arms is not supportive of the disruption explanation.

Discussion

The findings reported by CHK about the effects of moving out of public housing during childhood on adult earnings are important. On average, moving clearly had effects on earnings and these effects differed by experimental arm and age. However, the average effects reported by CHK, which were the basis of their conclusions, obscured a significant amount of heterogeneity of results across the five experimental sites. Once that heterogeneity is considered, the conclusions put forth by the authors, that neighborhood poverty explains the (average) positive effect of moving on earnings for young children and that disruption explains the (average) negative effects of moving on earnings for older children, are not reliable and may be misleading.

The MTO was five separate experiments conducted in five cities. The composition of families differed significantly across sites (see Orr et al. 2003, Table C1.2). For example, only 33 percent of families were African-American in Boston while 98 percent of families were African-American in Chicago. Similarly, only 51 percent of families in Boston relied on AFDC as their primary source of income whereas 65 percent of families did in New York. In New York 54 percent of families were headed by a never-married person (mostly women), but in Baltimore the figure is 74 percent. It is also the case that the baseline neighborhood quality differed substantially across sites (Aliprantis and Richter 2014). Finally, housing markets and other characteristics (e.g., segregation, racial/ethnic composition) also differed by city, as did the voucher take-up rates across sites and experimental arms.

Given these differences in family, neighborhood, and city characteristics, it is not surprising that effects of moving out of public housing during childhood on adult earnings differed qualitatively across places.[11] While confidence intervals of the estimates of the effect of moving on earnings by site are large, the point

11. In fact, while few previous MTO studies reported estimates separately by experimental site, there is evidence of such heterogeneity in the interim evaluations. For evidence on test scores, see Sanbonmatsu et al. 2006.

estimates are quite heterogeneous and reasonably suggest that there were substantial differences in the effects of moving on earnings across sites. It is also evident that the likely site-specific heterogeneity was not taken into account when designing the experiment, as sample sizes within each site are generally too small to detect reliably anything but large effect sizes. Even when pooling samples across sites, standard errors on estimates of the effect of moving on earnings reported in CHK are not sufficiently precise to detect reliably an effect size smaller than 20 percent to 30 percent. Effect sizes of this magnitude are large when measured against the fact that the exposure to a low-income (<20 percent poverty) neighborhood among experimental groups is approximately one to two years during an entire childhood. In fact, if estimates in CHK are judged solely by conventional levels of significance, as one may want to do for the analysis presented here, there are few results of importance, as most are not statistically significant and most differences of estimates across experimental arms, and even age groups, are not statistically significant.[12]

The implication of my analysis is that taking into consideration the substantial heterogeneity, between sites and arms, of estimates of the effect of moving on earnings weakens the evidence supporting the conclusions that neighborhood poverty is a primary explanation of the (average) increase in earnings associated with moving among younger children, and that disruption from moving is a primary explanation of the (average) decrease in earnings associated with moving among older children. Therefore, it seems imprudent to undertake policies and implement experiments based on the conclusions of CHK. The next experiment could just as likely replicate the results of Baltimore in which younger children did worse than older children and older children experienced a positive increase in earnings on average than replicate the average effect. Or it could be like New York where children in the Section 8 voucher arm experienced little change in earnings. We should ask ourselves: If a new MTO experiment takes place in Houston, Phoenix, Philadelphia, or any other city, will the results align with the conclusions that CHK published in the *American Economic Review*? Based on the evidence I review, I see little reason to think so.

As Nancy Cartwright (2013) argues, in most cases evidence from one experiment, for example in Boston, will not travel well and will not be replicated in another experiment, for example in Baltimore. The reason, she says, is that the structure and context that underlies the causal effect is likely to differ across the two experiments. She refers to supporting factors, which in the case of MTO are

12. The age-specific estimates of the effect of moving on earnings reported in CHK's Figure 2 and Appendix Figure 2 are almost all not statistically different from zero or each other, and do not suggest a strong age gradient even if one ignores statistical significance.

the characteristics of the participants and their baseline circumstances, and the characteristics of the cities, such as the spatial configurations of neighborhoods, racial/ethnic composition, extent of segregation, and the features of the housing market. She argues that it is only when these supporting factors are the same across experiments is it likely that the results of one experiment will replicate in another experiment. In the case of MTO, the supporting factors across sites clearly differed and so did the results. More importantly, there is no unique set of 'average' supporting factors underlying the average effects of the MTO experiment. The old saying that no one is the average is particularly apt in the case of the MTO. Therefore, it is highly unlikely that implementing an experiment designed on the basis of the average effects of the MTO experiment will produce results similar to that average.

In summary, while we have some evidence that moving increased adult earnings, at least for younger children, we do not know much about what caused this effect, and it is based on a noisy, large average experimental estimate obtained from an under-powered experiment. While CHK suggest it is the poverty rate in the neighborhood, a closer look at the evidence casts doubt on that explanation even when considering the noisy nature of the evidence. It seems reasonable to proceed more cautiously when drawing conclusions about the causes of the effects from moving and designing future experiments. So, yes, we should conduct more experiments about the effects of a child's environment on their current and future outcomes, but we should do so in an exploratory way that tests mechanisms beyond neighborhood poverty (e.g., Laliberté 2020).

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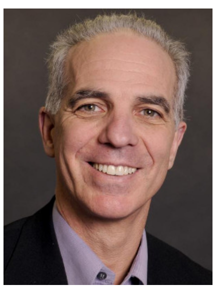

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