

Comments: Urban Poverty on Educational Outcomes

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Among the factors influencing the learning and developmental processes that lead to educational achievement, most scientists believe that the social environment plays a significant role. The importance of residential location in affecting children's education, however, is subject on which there is conflicting evidence and little agreement. Given the increase in the spatial concentration of poverty, however, this question has taken on increasing urgency. Study of this issue has been hampered for at least two main reasons. First, the range of variation in residential location is often limited among otherwise similar children. Second, the very fact that observably similar children (for example, same age and demographics) are living in different locations may reflect choices made by their families which are indicative of factors unobserved by an analyst. It is difficult to credibly identifying the importance of residential location as distinct from those unobserved factors.

The research design in this paper utilizes a randomized demonstration program known as Moving To Opportunity (MTO), to address both of these issues. Section 8 rental vouchers from the U.S. Department of Housing and Urban Development (HUD) were provided by lottery to public housing residents. Some vouchers were valid only in low poverty neighborhoods and were bundled with counseling services (the Experimental group), while others could be used to move to any location (the Section 8 Comparison group). Those who did not receive a voucher (the Control group) retained their eligibility to continue to live in their original public housing project unit. The consequences of this design for research are that similar groups of families are living in vastly different neighborhoods, and that the randomization implies that, on average, the groups offered and not offered the vouchers through the lottery will be similar in both observable and unobservable characteristics. This is a tremendous advantage for these authors in having potentially convincing results that could identify the impacts of residential location on educational outcomes.

Other results using MTO data, collectively summarized in a volume edited by John Goering and Judie Feins (2001), indicate that the impact of residential location on children may be particularly important. Research from the New York site indicates reductions in child behavior problems (Leventhal and Brooks-Gunn 2001), in addition to the research cited by the authors on reductions in teen criminal behavior (Ludwig, Duncan, and Hirschfield, 2001) and children's physical health and behavior (Katz, Kling and Liebman, 2001). None of the other existing studies using the MTO demonstration effectively studies educational outcomes. This novel and innovative paper addresses a critical gap in our knowledge.

In addition to the randomization of the housing voucher offers, the other principal element of this research design is the linkage of MTO participants to administrative data on educational outcomes, such as test scores, grade retention, absences, and disciplinary actions. When Jens Ludwig first discussed the idea for this project with me in 1995, I was frankly skeptical that these data could be produced, due mainly to the multitude of government agencies involved. So I am very pleased to be able to congratulate this team of researchers for truly creating new knowledge through their combination of data entrepreneurship and analytical skill.

Use of administrative records in this research does have some fundamental limitations, however. For example, the data indicate increased grade retention and suspensions among teens in families offered vouchers through the MTO program. This may accurately describe the experiences of these teens. Yet, the schools attended by those who have moved through the MTO program have changed at the same time as their residential location. In particular, it is not clear whether administrative records are indicative of different behavior by the youth or of different standards being applied to the same behavior in different schools. My prior intuition was that the role of different standards could be very important, and the authors provide some evidence in support of this. Thus, I agree with the author's conclusion that the results on grade retention and suspensions for teens are difficult to interpret.

Another limitation of these administrative data is that many children do not have records of test scores. The author's are very clear about the reasons for missing data, including problems linking to records using available individual identifying information, schools testing children with differing frequency, and students missing administration of the test because of absence or because of special education classification. Among children ages 5-12, the result is that there is substantial missing data for the sample in the post-randomization time period. Depending on the

measures, test scores are available for 46%(CTSB math), 53% (CTSB reading), and 26% (MFT reading) of the sample. While data missing at random would not affect the results of these analyses, these reasons are not random and may well be related to residential location itself.

A number of sensitivity analyses have been provided by the authors to assess the potential impact of missing data on the results. There is substantially more missing data for the Section 8 Comparison group than in the other two groups, for example, which could be a source of bias. It is reassuring, therefore, to find that these differences, at least for test scores, appear to be largely driven by differences in the ages and the pre-program characteristics of children – which can occur by chance in any lottery with a small sample.

On the other hand, the results do display some instability. In some cases, the results depend upon the control variables used or the estimation method.¹ Even though the data are generated from a randomized experiment, non-experimental methods must be used to address the missing data issues, and these methods are subject to standard criticisms about the potential importance of functional form and omitted variables.

To give a concrete example of how missing data could lead to bias, consider those in the Experimental group, whose probability of using an offered MTO voucher to move is about 20 percentage points higher for those with valid CTSB scores than for those with missing scores. This clearly indicates that the test score data are not missing at random, but missing in a manner directly related to residential location choice. Now consider the counterfactual counterparts of these families in the Control group (the “Control compliers” who would have moved through MTO if offered a voucher). For unbiased estimation, given the author’s evidence that there is selection in mobility and that the compliers do differ from non-compliers, Experimental and Control compliers would need to be equally over-represented among those with valid test score

¹ Specifically, the sensitivity analysis of the CTSB math score for the Experimental group indicates that adding statistical controls for pre-program student and family characteristics in addition to age increased the estimated Intent-To-Treat (ITT) effect from a statistically insignificant 4.2 percentile points to a statistically significant difference of 7.5. However, the Treatment-On-Treated (TOT) effect estimated with covariates is not statistically significant, indicating sensitivity to the estimation method.

For the Section 8 Comparison group CTSB reading score, there is almost no difference between the estimated ITT and TOT effects. Since about 15 percent of this group did not move through the MTO program, we would expect the TOT estimate to be larger. As noted by the authors, the TOT will simply be the ITT divided by the compliance rate when covariates are used that are uncorrelated with treatment group assignment. The fact this does not hold indicates that the choices of covariates matters and that the results are at least somewhat sensitive to the estimation approach.

data. This would not be true if the over-representation of Experimental program movers was due to more extensive testing in schools outside the center city (not attended by the Control compliers). While further evidence would be needed to ascertain whether this potential source of bias is actually substantively important, the authors have not refuted the fact that it could be.

My principal concern is that it should be clearly acknowledged that a wide range of biases are possible given the large extent of missing data -- which may be missing for systematically different reasons across the MTO groups. The direction of most biases from the missing data are difficult to assess, and could be positive or negative. Given the extent of the missing test scores, the small sample sizes, and the focus on outcomes in one city, I believe we should be cautious in our interpretation of these results.

I do find the results in this paper to be highly suggestive and to be a very valuable contribution to our understanding of the impact of residential location on educational outcomes, although far from conclusive. These results should undoubtedly encourage HUD to make the study of children's educational achievement a top priority in their planned interim evaluation of MTO next year. To provide more conclusive evidence, I would recommend that consistent data from all five cities in which the MTO demonstration took place be collected on children's educational achievement and on the learning environment in their schools.

Additional References

Goering, John, and Judie Feins (eds). (2001). *Choosing a Better Life: A Social Experiment in Leaving Poverty Behind*. Unpublished manuscript, City University of New York, February.

Leventhal, Tama and Jeanne Brooks-Gunn. (2001). "Moving to Opportunity: What About the Kids?" Unpublished manuscript, Columbia University, February.