Testing for Pre-Move Balance in Movers Exposure Designs Using Birth Outcomes*

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Abstract

A growing literature identifies the causal effects of neighborhood environments on a broad range of outcomes by exploiting differences in the ages at which children move across neighborhoods. Prior work has lacked data on pre-move outcomes to test the key constant selection by age assumption that underlies this "movers exposure" design. Here, we use data on birth outcomes from the universe of children born in California as a pre-move outcome. We find that children who move to higher-upward-mobility Census tracts at earlier ages have birthweights and gestation lengths that are comparable to children who make similar moves at older ages, supporting the identification assumption underlying the movers exposure design.

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Social scientists have been interested in understanding the causal effects of neighborhood environments on outcomes for decades, but separating selection effects across neighborhoods from causal effects has proven challenging. A recent, rapidly growing literature has studied the causal effects of neighborhood environments on children's long-term outcomes by exploiting differences in the ages at which children move across neighborhoods. The idea of this research design, introduced by Chetty and Hendren (2018), is to compare children who move at younger vs. older ages to neighborhoods of differing quality to identify "exposure" (or dosage) effects of growing up in a higher quality environment. Because it can be implemented in many modern longitudinal datasets, this "movers exposure" design has been applied to study a wide variety of outcomes across different settings: from economic mobility in the United States (Chetty and Hendren 2018, Chetty, Friedman, Hendren, Jones, and Porter 2020), to levels of mobility in many other high-income countries using administrative data (e.g., Faurschou (2018) in Denmark, Deutscher (2020) in Australia, Laliberte (2021) in Canada), to low- and middle-income countries using survey data (e.g., Alesina, Hohmann, Michalopoulos, and Papaioannou (2021) in Africa, Britto, Fonseca, Pinotti, Sampaio, and Warwar (2022) in Brazil). The same movers exposure design has also been used to analyze the impacts of childhood environments on political attitudes (Brown, Cantoni, Chinoy, Koenen, and Pons 2022), rates of innovation (Bell, Chetty, Jaravel, Petkova, and Reenen 2018), racial disparities (Derenoncourt 2022), and the formation of gender norms (Coran, Laczek, and Miserocchi 2022).

The key identification assumption underlying the movers exposure research design used in all of these studies is that selection effects in neighborhood choice – the extent to which different types of people move to higher vs. lower quality neighborhoods – do not vary with the age of their children at the point they move. Prior work has tested this assumption using two approaches: comparing the outcomes of siblings within the same family to rule out selection based on fixed family characteristics and outcome-based placebo tests exploiting heterogeneity in place effects across subgroups, showing for instance that movers' outcomes are strongly predicted by the outcomes of residents in the place to which they move in their own birth cohort but not prior or later birth cohorts.

Although these prior tests support the validity of the movers exposure research design, there have been no tests of balance in the movers design to date using *pre-move* outcomes. In panel event-study settings, it is typical to examine outcomes both before and after the event to test for balance in pre-event outcomes. For example, in movers designs that estimate treatment effects for repeatedly observed outcomes such as health spending, studies typically plot the evolution of outcomes before and after the move and test for divergence in outcomes post-move (Finkelstein, Gentzkow, and Williams 2016). In the movers exposure design, this intuitive approach has not been feasible to date, since children's earnings in adulthood are observed only after the treatment effect of interest (childhood exposure to a different neighborhood environment).

In this note, we implement a test of balance based on pre-move outcomes inspired by the work of Eshaghnia (2023), focusing on outcomes at birth (birthweight and length of gestation). Since these birth outcomes are predetermined relative to childhood neighborhoods, measured accurately in administrative data for large populations, and correlated with many long-term outcomes of interest (Black, Devereux, and Salvanes 2007, Royer 2009), they provide ideal pre-move outcomes on which to test for balance. We revisit movers designs that estimate neighborhood effects on economic mobility and test for balance in birth outcomes in the same samples. Those studies have established that children who move to neighborhoods with higher average rates of upward mobility in income at earlier ages have higher incomes themselves in adulthood. Here, we ask: are those same children less likely to be born premature or with low birth weight – which would violate the assumptions of the design – or are their birthweights and lengths of gestation comparable to those who make similar moves at later ages?

Data. Building on work by Kennedy-Moulton, Miller, Persson, Rossin-Slater, Wherry, and Aldana (2022), who link data on the universe of birth records in California to tax records housed at the U.S. Census Bureau, we analyze birth outcomes for children born in California between 1978-1999. We construct our analysis sample exactly as in Chetty, Friedman, Hendren, Jones, and Porter (2020), except that we (1) use more recently available data (up to the 2017 tax year), (2) expand the range of birth cohorts we study from 1978-1999, and (3) subset the sample to those who appear in the California birth record data (and hence were born in California). Our empirical analysis here focuses on children who moved across Census tracts exactly once after age 1 (over the age range we observe them), a sample that consists of 487,000 children. See Kennedy-Moulton, Miller, Persson, Rossin-Slater, Wherry, and Aldana (2022) for details on the California birth records data and how it was linked to the Census data and Chetty, Friedman, Hendren, Jones, and Porter (2020) for further details on the tax data used to measure parental income and children's outcomes and the construction of our analysis sample.

Results. We first replicate the findings of Chetty, Friedman, Hendren, Jones, and Porter (2020) in the subsample of children born in California described above. We begin by measuring average upward mobility rates by neighborhood based on the average household income percentile ranks at age 30 for children with parents at a given parental income level in the full national sample, excluding one-time movers, exactly as in Chetty, Friedman, Hendren, Jones, and Porter (2020). We then analyze how children's incomes at age 30 vary with the age at which they move to higher- vs. lower-mobility Census tracts for the subsample of children for whom we observe income at age 30 (the 1978-87 birth cohorts). Figure 1 presents this result by replicating Figure XI from Chetty, Friedman, Hendren, Jones, and Porter (2020) in our California subsample, plotting the coefficient on the difference in neighborhood upward mobility rates between the destination and origin Census tracts interacted with age at move. See the notes to Figure XI from Chetty, Friedman, Hendren, Jones, and Porter (2020) for details on the construction of this figure. Consistent with the findings of Chetty, Friedman, Hendren, Jones, and Porter (2020), we find that children who move at earlier ages to higher-upward-mobility neighborhoods have higher income ranks themselves in adulthood.

Next, we repeat this analysis using birth outcomes as placebos. Figure 2a replicates Figure 1, but replaces the dependent variable with the child's birthweight rank. In stark contrast with the pattern observed for income, there is no gradient in the relationship between birthweight and neighborhood upward mobility rates by age at move: the relationship is flat across the range and the estimated slope is not significantly different from 0. Since the relationship between children's incomes in adulthood and birthweights is highly non-linear – with the birthweights below the 20th percentile associated with particularly low incomes in adulthood (Appendix Figure 1) – we next replicate the placebo test using an indicator for birthweight below the 20th percentile (6 pounds 4.7 ounces). Again, we find no gradient by age at move (Figure 2b). Finally, in Figure 2c, we use an indicator for premature birth (before 259 days, or three weeks before term) as the outcome and again find no gradient by age at move. Hence, all of these pre-move outcome tests support the constant selection by age identification assumption that underlies the movers exposure design.[1]

To gauge the power of these placebo tests, note that the estimate of the slope in Figure 2a is sufficiently precise to rule out the hypothesis that the coefficient of birthweight rank on neighborhood upward mobility declines by more than 0.1% by age at move. In contrast, Figure 1 implies a slope of -1.7% on average over the ages where we observe income at age 30, an order of magnitude larger. Since the correlation between birthweight and income ranks is bounded above by 1, we can rule out the hypothesis that differential selection on factors captured by birthweight ranks drives more than 5% of the exposure effect on income.

We conclude that pre-move birth outcomes are balanced in the movers exposure design. Differences in children's outcomes emerge after, not before, they move to higher-mobility Census tracts in the U.S. Coupled with the other tests implemented in prior work, this finding provides further support for using the design to identify the causal effects of childhood environments.

[1]Figure 2 shows that the association between neighborhood upward mobility and children's birth outcomes (and, by extension, their potential earnings outcomes) do not vary with age at move, but it does *not* establish that those who move at younger vs. older ages have comparable birth outcomes or potential earnings outcomes. The latter assumption is not required for the movers exposure design.

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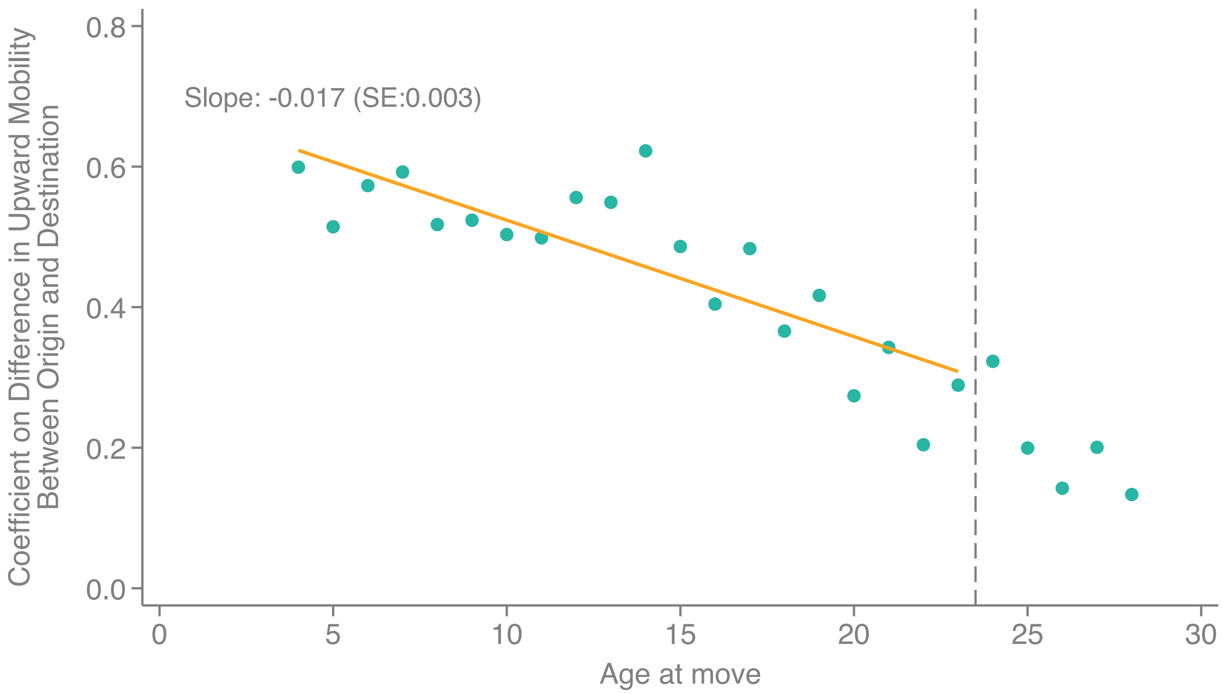
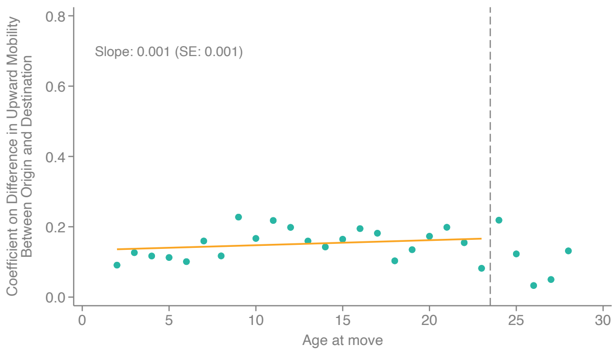
Figure 1: Impact of Moving to a Higher-Upward-Mobility Neighborhood on Income at Age 30, by Age at Move



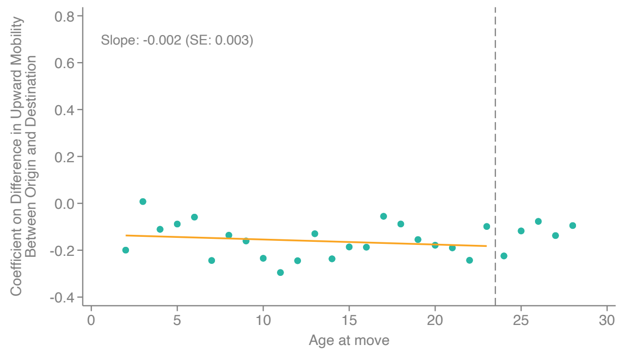
Notes: Figure 1 plots the effect of moving to a Census tract where children have one percentile point higher income ranks at the age of 30, by the age at which children move. To construct the figure, we first estimate mean observed outcomes in each tract following the methodology of Chetty, Friedman, Hendren, Jones, and Porter (2020), except that here we take advantage of more recently available data and pool across 1978-1987 birth cohorts to measure income at age 30. We then implement the same specification as in Figure XI of Chetty, Friedman, Hendren, Jones, and Porter (2020), taking the set of one-time movers born in California (for whom we have birth outcome data) who move between tracts that are more than 25 miles apart and regressing their household income ranks at age 30 on the difference in observational measures of upward mobility (predicted income rank at age 30 for non-one-time movers) between their destination and origin tracts interacted with age at move, as well as controls for parent income and origin upward mobility interacted with age at move. We then plot the resulting regression coefficients on the difference in upward mobility by age at move, along with a linear fit to these points below age 23. We report unweighted OLS linear regression slopes and standard errors of the coefficients on the age at move for available ages up to age 23. See notes to Figure XI of Chetty, Friedman, Hendren, Jones, and Porter (2020) for further details.

Figure 2: Placebo Tests: Impact of Moving to Higher-Upward-Mobility Neighborhood on Birth Outcomes, by Age at Move

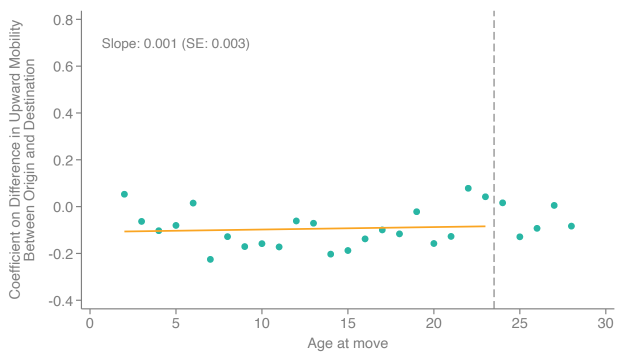
(a) Birthweight Percentile Rank



(b) Low Birthweight

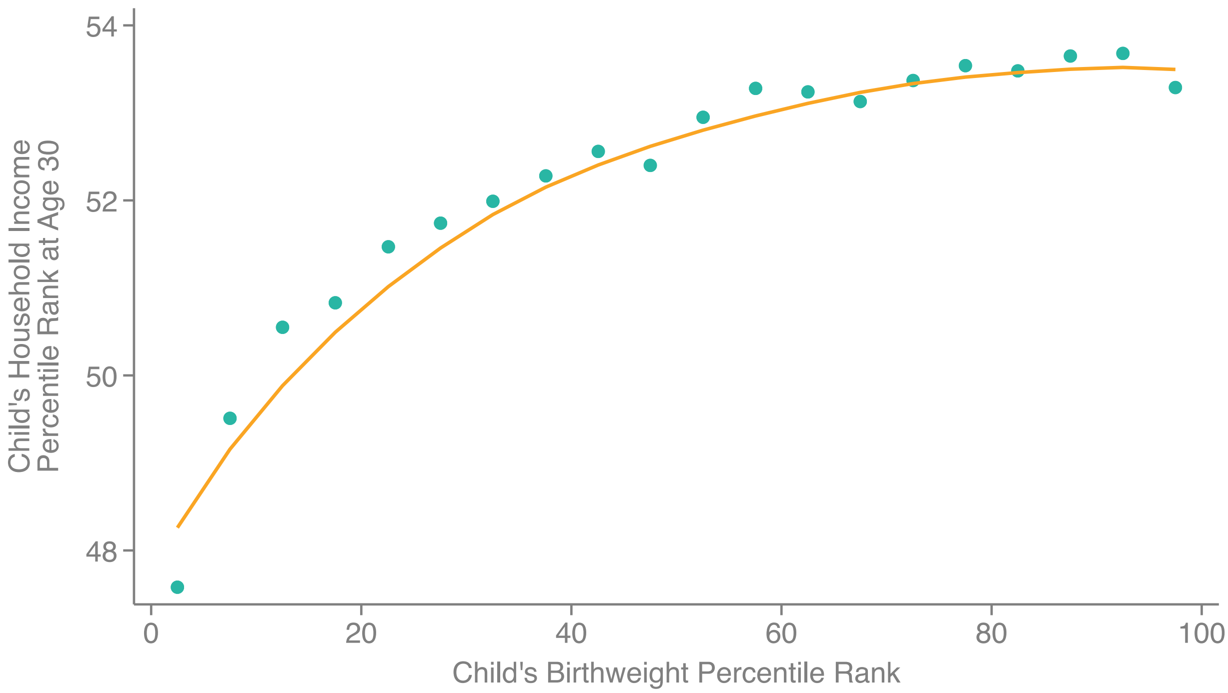


(c) Preterm Birth



Notes: Figure 2 replicates Figure 1 with different outcome variables on the left hand side of the regression. In Panel 2a, the outcome is within-birth-cohort birthweight percentile rank. In Panel 2b, it is an indicator for whether a child is low birthweight, defined by having birthweight below the 20th percentile of the within-cohort birthweight distribution. In Panel 2c, the outcome is an indicator for whether the child was born preterm (gestation length below 259 days, 3 weeks less than full term).

Appendix Figure 1: Household Income Rank at Age 30 vs. Birthweight Rank



Notes: Figure 1 is a binned scatter plot of children's household income percentile rank at age 30 against their birthweight percentile rank. A lowess fit is shown on the points of the binned scatter plot.