Three Essays on Family Economics

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Abstract

Three Essays on Family Economics

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Using the National Longitudinal Survey of Youth data, my dissertation investigates several key issues in family economics. The first chapter studies the role of family relocation on children's schooling and youth behavior problems. By exploiting the variation in sibling's age at the time of family relocation, we find no detectable negative effects of family relocation on various children's outcomes. We extend our discussion to the context of school mobility and child outcomes. In the second chapter, we use individual school change history from the NLSY 97 and control for sibling fixed effects to estimate how the variation in children's age at school change would affect a set of outcome variables. We find school change made at age 16-18 would significantly reduce children's education achievement by age 20 and increase their possibility for repeating grade in school. In the third chapter, we examine the impact of family size on maternal health outcomes by exploiting the exogenous change in family size using contraceptive failure as instrument variable. This result indicates that mothers' mental health at age 40 is negatively affected by having additional child while their physical health stays intact.

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Chapter 1

Estimating the Effects of Family Relocation on Children's Education and Youth Risky Behavior

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Abstract: Using individual-level data from the NLSY79 and the NLSY79 Children and Young Adults, we empirically investigate the role of family relocation on children's schooling and youth behavior problems. By exploiting the variation in sibling's age at the time of family relocation, we find no detectable negative effects of family relocation on various children's outcomes. In addition, while the OLS estimates vary by gender and ethnicity, this variety disappears in the sibling fixed effects estimates. Our empirical results indicate that the unobserved family characteristics that drive the decision of family relocation are responsible for children's schooling and behavior outcomes in the long run.

Keywords: family relocation, education, youth risky behavior, sibling fixed effects

JEL classification: D10, J13

I. Introduction

Mobility is a common occurrence for American families. Approximately 20% of the U.S. households change the location of their residences in any given year. Despite the fact that mobility rates differ by age group, the figures for young school aged children are about the same as for the population generally. American children have the highest rate of residential and school mobility (Long, 1992). Residential mobility, especially its consequence on children has captured the attention of social scientists for more than a half century. However, opinions have cycled about whether residential mobility has positive or negative implications for children's life course.

This paper investigates the impact of family relocation on children's education outcome and their youth risky behavior. It also addresses the question that whether the timing of family relocation matters for children's outcomes. In most cases, families are selected into relocation for positive reasons as upward social mobility and increased economic opportunity for the family (Kopf, 1977) or negative reasons as job loss, divorce or other forms of family disruption. Longitudinal data from the Panel Study of Income Dynamics (PSID) shows that parental divorce sharply increases the annual probability that children will move out of their neighborhoods (South, Crowder and Trent, 1998). Conditional upon moving, they move to significantly poorer neighborhoods than do children in stable two parent families (South, Crowder and Trent, 1998). We consider the endogeneity and self-selection nature of family relocation to pose both a challenge and potentially a clue in explaining the positive or negative correlation between family relocation and various children's outcomes later on.

Family relocation is closely tied to children's growing environment and critically contributes to their mental and physical development. Existing studies link residential mobility to a range of child and adolescent outcomes and develop much diversified conclusions (Astone and

McLanahan, 1994; Simpson and Fowler, 1994; Kerbow, 1996; Alexander, Entwisle and Dauber, 1996 Tucker, Marx and Long, 1998; Scanlon and Devine, 2001). Most of these researches document a clear pattern that residential and school moves are associated with poor academic performance, poor health condition and delinquent youth behavior, whereas other researches point that the association between moving and children's outcome may be spurious. The negative correlation may be a function of other characteristics of people who move often. Using longitudinal data, they are able to identify most of the negative effect of moving is due to preexisting differences between the movers and non-movers (Pribesh and Downey, 1999).

Most of the previous researches take the naïve approach to evaluate how family relocation would affect children's outcome. That is to estimate the parameters of a regression equation in which the dependent variable is children's outcome (measured at a specific age), and the explanatory variables include an indicator for whether the family relocates, demographic variables, and at times, variables such as family income and labor market participation of the mother. The coefficient of the relocation indicator is meant to capture the effect of family relocation on children's outcome. The prerequisite for the coefficient from this naïve approach to make economic sense is random sampling. However, as families mostly self-select into relocation, the results from this approach are subject to selection bias. The negative effects of family relocation on children's outcome could be amplified when choosing families with low social economic status and vice versa. While families selecting into relocation creates measurement error for the direct effects of family relocation on children's outcomes, it also indicates that all the children that are present in the household are jointly affected by this event which leaves us with natural variation in sibling's age at family relocation.

This paper uses the NLSY79 and the NLSY79 Children and Young Adults to study the impact of residential mobility on children's schooling and youth risky behavior. By implementing sibling fixed effects regression model, the model permits us to hold constant effects which are common to all siblings, we can then control for all sources of observed and unobserved heterogeneity at the family level. The main virtue of this strategy is that it allows us to circumvent problems of selection in a clean and straightforward way. Since the within-family strategy relies on differences in ages of children when family relocates, we control for birth order and birth cohort effect in all specifications We find that the seemingly significant negative impact of family relocation for children at school age (13-18) on children's schooling and risky behavior from the naïve OLS estimation disappear after controlling for sibling fixed effects. This finding points to the potential bias existing in previous literature due to the selection of family relocation and unobserved family characteristics.

II. Literature

Despite over decades of research on the topic of family relocation on children's outcomes, current research in this area has been limited in several ways:

First, most research in this area fails to consider that it is not moving per se but rather the underlying reasons why the mobility occurs in the first place leads to negative or positive educational or behavioral outcomes. Since families do not choose to relocate randomly, there may be important differences between mobile and non-mobile families which account for the observed relationship between mobility and academic and behavioral outcomes and those underlying differences might explain the deleterious effects of mobility commonly found in previous literature.

Second, lots of research has focused on fragile families with low social economic status which makes the negative relation between mobility and children's academic performance and youth risky behavior more pronounced. Youth in those families who move may already be performing worse academically, at a higher risk of dropping out and being more involved in a variety of delinquent and problem behaviors. In these cases, any observed relationship between mobility and delinquency may be spurious rather than the causal effect of moving. Therefore, negative relation captures not only the effects of residential mobility but also the unobserved family characteristics that initiate the move in the first place.

A large body of literature focuses on residential instability and children's school performance. These studies find that, on average, students who experience residential moves perform less well than students who do not. Specifically, moving is related to reduced academic performance (Ingersoll, Camman and Eckerlin, 1989; Haveman, Wolfe and Spaulding, 1991; Pribesh and Downey, 1999; Wood, Halfon, Scarlata, Newacheck and Nessim, 1993). Coleman (1988) suggests that geographic mobility is a strong predictor of high school dropout. Residential mobility can lead to school mobility, especially in condensed urban area. Astone and McLanahan (1994) use data from High School and Beyond to show that residential mobility is associated with a greater probability of school dropout, after controlling for a number of family and demographic factors. Rumberger and Larson (1998) and Swanson and Schneider (1999) use data from the National Education Longitudinal Study to examine the relationship between school mobility and high school dropout and find that students changing schools frequently are also at greater risk of dropping out.

While the correlation between residential mobility and school performance is well established in previous research, parallel developments in criminology have also been documenting links between residential mobility and delinquency at the individual and community levels (Crutchfield, Geerken and Gove 1982). Moving may bring on feelings of loss, caused by separation from loved ones, friends, or community supports. As children deal with feelings of loss, they require extra support from parents in order to adequately transit to new environment. A child's emotional needs may be overlooked as parents and caretakers are faced with their own emotional, physical, and social demands of moving (Simpson and Fowler, 1994). Therefore, moving could affect important aspects of youth development, especially problem behaviors like high school dropout, running away from home, smoking, drinking and drug use. Inquiry into the relationship between psychological and behavior problems and moving has also been undertaken (Mundy, Robertson, Greenblatt and Robertson, 1989; Simpson and Fowler, 1994; Stacks, 1994; Tooley, 1970). Almost all of the researches done in this area show negative associations between residential mobility and youth outcomes (Adam and Chase-Lansdale, 2002; Astone and McLanahan, 1994; DeWit, 1998; Haynie and South, 2005; Hoffmann and Johnson, 1998; Wood , Halfon, Scarlata, Newacheck, and Nessim, 1993). Mobile children are more likely to be psychiatrically hospitalized, more likely to initiate drug and alcohol use (Catalano, Hawkins, White and Pandina, 1985), and more likely engage in premarital sexual behavior (Stacks, 1994).

It is of great importance to understand whether residential mobility directly contributes to the reduced academic performance or youth risky behavior such as running away from home, smoking, drinking, or using drugs at an early age. On the one hand, parents relocate in an effort to improve family conditions, especially if job opportunity arises. If the move is associated with increasing family income and upward mobility in social economic status, ex ante, we would not expect too much harmful impact on children's academic performance and behavior problem. Many parents are even hesitant to relocate their children precisely because they think it will be traumatic or harmful to educational performance (DeLuca and Rosenblatt, 2010). Likewise, recent policy initiatives to relocate poor families from urban ghettos are premised on the idea that moving to safer neighborhoods relieves family stress, reduces the exposure of adolescents to violence, and even improves youth development through access to higher quality schools and positive adult role models (Ainsworth, 2006; Clampet-Lundquist, Edin, Kling and Duncan, 2006; Sanbonmatsu, Kling, Duncan and Brooks-Gunn, 2006). Such moves to improved contexts are expected to benefit youth in the long run. On the other hand, residential instability may influence educational achievement and behavior problems through its relationship with increased school mobility (Kerbow, 1996). This school mobility, or frequently changing schools, is associated with worse academic outcomes (Crowder and South, 2003; Swanson and Schneider, 1999; South, Haynie and Bose, 2007) and emotional and behavioral problems (Pittman and Bowen, 1994), as well as reduced social competence and self-esteem which lead to youth risky behavior. Children's emotional competence to deal with mobility may not be well expected when parents make the decision about moving. The lack of participation of children in the decision process may compromise the potential benefits of moving, especially for those less advantaged groups such as fragile and low income families.

Our reading of these seminal and influential work is that they well document the correlation between mobility and a number of children's outcomes, however, we believe prior research has not done an adequate job of examining whether mobile youth are selected into both mobility and problem behaviors. We are not the first, however, to raise the possibility that important selection effects may be driving the association between mobility and youth outcomes. Pribesh and Downey (1999) find that preexisting differences accounted for 90% of the difference in test scores between movers and non-movers. Francisca M. Antman(2012) claims that migrants and nonimmigrants are likely to differ in unobservable ways that also affect children's educational outcomes. Her paper uses Mexico data and addresses selection problem by looking within the family to exploit variation in siblings' ages at the time of parental migration to the US and finds positive effects of father migration to the U.S. on daughters' education outcome. Using randomized housing-mobility experiment, Jens Ludwig and others (Ludwig, Duncan, Gennetian, Katz, Kessler, Kling and Sanbonmatsu, 2013) closely examine the impact of residential mobility on low-income families using data from the Moving to Opportunity (MTO), which offers some public-housing families but not others the chance to move to less-disadvantaged neighborhoods. Their results show after 10-15 years MTO has no detectable effect on economic outcomes, youth schooling and youth physical health and they discover mixed results by gender on other youth outcomes, with girls doing better on some measures. Their empirical results resonate with our findings from the NLSY79 that family relocation itself should not have influential impact for children's outcome in the long run whereas the underlying family characteristics that trigger the decision of moving should be responsible for the differential outcomes between mobile and non mobile children.

Using individual level data from the NLSY79, we examine whether residential mobility has detrimental effects on children's academic performance and whether it leads to risky youth behavior as smoking, drinking and substance use at an early age and find no detectable negative effects of family relocation on children's outcomes. We believe that our paper represents a more rigorous attempt to adjudicate between causal and selection hypotheses about the effects of mobility than what we have seen in previous work.

Our scholarly contribution is twofold. First, our paper addresses the endogeneity problem of family relocation that are overlooked by prior research and proposes an empirical strategy that could potentially control for selection bias of family relocation. It is a relatively novel idea to use the natural age variation generated by sibling order at the event of moving to evaluate the effects of family relocation on children's outcomes. There hasn't been enough discussion in terms of how to choose the right timing of family relocation. We believe our econometrics strategy constitutes an important contribution to the improved understanding of whether and when family relocation would lead to negative or positive children's outcomes.

Second, we use dummy variables indicating different age periods when estimating how age at family relocation would affect children's academic performance and risky youth behavior. Previous research has been vague about how they handle multiple relocations happened at different age when evaluating the impact of family relocation on children's outcomes and many of them choose to treat this as one time shock to children. Our paper tackles this problem by including dummy variables on different age intervals at family relocation. We will return to the specific details subsequently in interpreting the results of our econometric analysis.

III. Empirical Specification and Identification Strategy

In this research, we first evaluate the long run impact of relocation on children's academic achievements of children. The academic outcome variables include whether a child finishes high school, whether a child ever repeated grade, and the highest grade completed by a certain age. This certain age is set to be 20 in our specification so that the outcome variables measure long run effects and the sample is sufficiently large. We then examine the second set of outcome variables which are youth risky behavior problems. The outcome variables are whether a child starts running away from home, smoking, drinking, or using drug (primarily marijuana use) by age 16.

We begin by introducing the baseline model for this paper illustrated by equation (1):

$$y_{ij}=\beta^{'}X_{ij}+\delta_1 BirthCohort_{ij}+\delta_2 SiblingOrder_{ij}+\gamma 1_{Move\_ij}+\alpha_j+\varepsilon_{ij}$$

$$\text{for } i=1,2 \text{ and } j=1,2,\ldots,n, \qquad (1)$$

where y_{ij} is the outcome variable of interest for sibling i in family j, X_{ij} is a vector of covariates which can vary between siblings in family j, $BirthCohort_{ij}$ is a birth cohort dummy variable, 1_{Move_ij} is an indicator variable which equals to 1 if sibling i in family j ever relocates throughout the sample period from 1986 to 2010 and equals to zero otherwise, α_j denotes unobserved family characteristics, and ε_{ij} is idiosyncratic error term. In this model, γ represents the impact of family relocation on outcome variables and it is the parameter we want to identify for this particular exercise. The vector of covariates, X_{ij}, may include variables that representing shared family characteristics such as ethnicity, mother's education, mother's marital history information and total number of children in the household and variables that representing non shared individual characteristics such as gender, whether mother was less than 19 at the birth of children, and mother's employment status when children's outcome is evaluated.

We include sibling order in model (1) as younger sibling may pick up some behavior pattern from elder siblings. This is because first born children will experience family relocation at later age than their younger siblings. Failure to account for birth order could entail a bias in the estimate of age at family relocation[1]. Moreover, if there is any spillover effect among siblings, for instance, younger children pick up the negative behavior problem from older siblings and therefore perform worse in terms of outcome variables. For instance, if the first child starts smoking at an early age, the second sibling might start smoking at an early age as well. We can capture this learning experience by controlling sibling order in our model. This model allows 1_{Move} interact with X_{ij} variables such as gender and ethnicity. To account for any general time trend in children's education outcomes and behavior pattern, we also control for the birth cohort in our analysis.

[1] Recent studies show that among families with more than one child firstborn children on average outperform their younger siblings in terms of educational outcomes (Kristensen and Bjerkedal, 2007, Booth and Kee, 2009).

It is important to note that the conventional OLS estimation using the above equation may suffer from the endogeneity problem because families choose to relocate based on family income, job opportunities, marital status and other family characteristics. As these reasons are likely to be family specific rather than individual specific, we make additional assumptions that individual specific components which are not included by the family specific effects are idiosyncratic. To eliminate the fixed unobserved family heterogeneity, this paper proposes taking the difference between sibling 1 and sibling 2 equations. Suppose that only one of the siblings is the child who has ever relocated from 1986 to 2010. Then, we have

$$\mathrm{E}\left[y_{1j}\text{-}y_{2j}\middle|X_{1j}=\widetilde{X_{1j}},\ X_{2j}=\widetilde{X_{2j}},\ 1_{1,\mathrm{Move}}=1,1_{2,\mathrm{Move}}=0\right]$$

$$=\beta^{'}\widetilde{\Delta X}_j+\delta_1\Delta \mathrm{Birth}\widetilde{\mathrm{Coh}}\mathrm{ort}_j+\delta_1\Delta \mathrm{Sibli}\widetilde{\mathrm{ngO}}\mathrm{rder}_j+\gamma \qquad (2)$$

To identify γ, we further need to take care of the difference in the birth cohort and sibling order dummies. This can be done by including families that do not change residential address, but have identical $\widetilde{\Delta X}_j$, $\Delta \mathrm{Birth}\widetilde{\mathrm{Coh}}\mathrm{ort}_j$, and $\Delta \mathrm{SiblingOrder}_j$ to family j. Including non-mobile families in estimation, we are essentially comparing the difference in outcomes between siblings with the same age difference in mobile families and non-mobile families and relating this to the children's different age at post relocation experiences. To incorporate this idea, we apply the following approach:

$$\mathrm{E}\left[y_{1j}\text{-}y_{2j}\middle|X_{1j}=\widetilde{X_{1j}},\ X_{2j}=\widetilde{X_{2j}},\ 1_{1,\mathrm{Move}}=1,1_{2,\mathrm{Move}}=0\right] - \mathrm{E}\left[y_{1j'}\text{-}y_{2j'}\middle|X_{1j'}=\widetilde{X_{1j}},\ X_{2j'}=\widetilde{X_{2j}},\ 1_{1,\mathrm{Move}}=0,1_{2,\mathrm{Move}}=0\right]$$

$$=(\beta^{'}\widetilde{\Delta X}_j+\delta_1\Delta \mathrm{Birth}\widetilde{\mathrm{Ch}}\mathrm{ort}_j+\delta_1\Delta \mathrm{Sibli}\widetilde{\mathrm{ngO}}\mathrm{rder}_j+\gamma)\text{-}(\beta^{'}\widetilde{\Delta X}_j+\delta_1\Delta \mathrm{Birth}\widetilde{\mathrm{Coh}}\mathrm{ort}_j+\delta_1\Delta \mathrm{Sibli}\widetilde{\mathrm{ngO}}\mathrm{rder}_j)$$

$$=\gamma \qquad (3)^2$$

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[2] Taryn Ann Galloway (2012) applies similar identification strategy in the study of timing of divorce and its impact on children’s crime-related and educational outcomes.

Now γ can be identified from equation (3). This illustration takes families with 2 children as an example. However, this identification approach can be easily generalized to situations with more than two siblings. To sum up, the main virtue of this strategy is that it allows us to account for family specific characteristics that might be correlated with educational outcomes of children and parental relocation patterns. Our main specification for the model would be using sibling fixed effects to identify γ . Meanwhile, we will also present results based on the pooled population without sibling fixed effects.

One important virtue of the estimation strategy used here is that it can be easily extended to allow the impact of family relocation to vary depending on the age of the child at the time of the event. Distinguishing effects based on the children's age at the time of the family relocation also brings this paper into relation with the literature on child development and family dynamics which investigates the effects of residential mobility on children at different age groups in other context such as divorce and separation. Despite these two topics share lots of similarities, an important feature of family relocation is that it happens more frequently over the life course of the individual, therefore creates multiple shocks at different age for individuals. To control for the impact of multiple relocation happened at different age for the same individual, we allow age at relocation indicator variables to be non exclusive to each other.

Equation (4) represents our extended model:

$$y_{ij}=\beta^{'}X_{ij}+\delta_1 BirthCohort_{ij}+\delta_2 Sibling\ Order_{ij}+\sum_{s=1}^{6}\gamma_s I_{MoveAge[3s\text{-}2,3s]ij}+\alpha_j+\varepsilon_{ij}$$

$$i=1,2,\ldots,m;\quad j=1,2,\ldots,n;\quad s=1,2,\ldots,6\ \text{ or }\ s=1,2,\ldots,5 \tag{4}$$

where $I_{MoveAge[3s-2,3s]}$ represents a dummy variable indicating whether a child i in family j relocate at age between $3s-2$ and 3s for s=1,2,3,4,5. For example, if a child relocate at age between 1 and 3, we have $I_{MoveAge[1,3]}=1$ for s=1. Similar explanations apply to other index variables representing child age group when family relocates. We include 6 dummy variables (or 5 age interval dummies depending on the outcome variable) indicating children's age at relocation in a much smaller interval intending to capture impacts of the multiple relocations happened during different age periods. The model (4) can capture the potentially discontinuous nature of age at family relocation on children's outcome variables. The baseline group is chosen as relocation age greater than 18 (or 16) or children who never relocate throughout the sample period[3].

IV. Data

The data sets we use for the study is the National Longitudinal Study of Youth 1979 (the NLSY79) and the NLSY79 Children and Young Adults. The National Longitudinal Survey of Youth 1979 cohort (the NLSY79) is a multi-purpose panel survey that originally included a nationally representative sample of 12,686 men and women who were all 14 to 21 years of age on December 31, 1978. Annual interviews have been conducted with the NLSY79 main Youth respondents since 1979, with a shift to a biennial interview mode after 1994. To acquire child specific information, we also employ the NLSY79 Children and Young Adults data which is a separate survey on all children born to the NLSY79 female respondents. The child survey includes assessments of each child as well as additional demographic and development information collected from either the mother or child.

The advantage of using the NLSY79 over the PSID (Panel Study of Income Dynamics) or other national survey data is that mother's marital and relocation history can be linked with her children through a unique ID, therefore, we can identify siblings from the same household and track down a complete family relocation history based on mother's response to questions on their residence

[3] For the education outcomes, we leave out children who relocate above age 18 and children who never relocate during our sample period as control group for our sibling effects estimation. For the risky behavior outcomes, we leave out children who relocate above age 16 and children who never relocate during our sample period as control group as the cut off age for risky behavior is 16.

each survey year. Moreover, the panel structure of the NLSY data can be useful in terms of tracing children's academic performance and the development of youth risky behavior for the entire youth period (up to age 20). Instead of looking for the intermediate impact of family relocation, we can study the impact of family relocation on children's outcome when children reach a certain age later on.

Our main sample is constructed from the NLSY79 Children and Young Adults. Since this ongoing project surveys all children born by mothers from the NLSY79 on a biennial basis from 1986, by the year of 2010, we have 11,498 children from 4,390 households. The birth cohort of our samples ranges from 1970 to 2010. For our analysis, we construct two samples with different age eligibility. The first sample is for the study of family relocation and children's education outcomes. The age eligibility for being included in this sample is age 20 by year 2010. 7,638 individuals meet this age criterion. We drop observations whose mothers are in active military force then we obtain 4,926 individuals with non missing values on education and other independent variables. The second sample is for the study of youth risky behavior such as smoking, drinking and using drugs such as marijuana and running away from home. The age eligibility for second sample is 16 by year 2010. 10,223 Individuals achieve age 16 in our data. We retain 8,247 individuals for the second analysis.

In terms of sample attrition, the NLSY79 children and young adult is linked with the NLSY79 mother, the effective attrition rate for the NLSY79 is 18 percent[4]. Since there is an increased attrition rate in more recent wave, so we only include individual observation up to 2010. If attrition is non random, then that could bias our estimation results in a systematic way. By comparing the demographic characteristics of the sub samples and the master sample, the gender composition,

[4] "Excluding these subsets of respondents means that effective attrition for those who would otherwise be eligible for interview is about 18 percent" - NLSY79 Children and Young Adult User Guide, 2002.

household size, mother's education level are quite similar, though the sub sample for education has a relative higher proportion of Hispanic and Black samples5.

1) Descriptive Statistics

(Table 1: Approximately Here)

Table 1 provides the summary statistics of the master sample and two subsamples for the key variables used for this study. In total, we have 11,498 individual observations from 4,390 households from 1984 to 2010. Based on the regression specification later, we restrict our sample to a subset of all the individual observations available. The figures in the table describe children's demographic and family characteristics. From table 1, in terms of gender distribution, we have a relatively balanced sample with about 50% of the observation being female. The ethnic composition represents the over sampling of Hispanic and Black Americans in the NLSY[6]. In the subsample, Hispanic and Black take larger component than the master sample. The number of children in the household reveals the information about the average family size for a typical American family would be 3. Our table also shows family characteristics that are shared or not shared among siblings. For instance, mother's education is shared feature among siblings in the same household while whether mother was less than 19 at the birth of the child may be different for individual child. One important feature on the NLSY79 data is it directs children's information with their biological mother which makes it easy to make inference under an intergenerational context. One the other hand, this data doesn't have all the similar information regarding to children's biological father as their mothers.

[5] Including more disadvantaged individuals could bias downward the impact of family relocation on children's outcome. However, even with is potential downward bias, we do not detect the negative impact of family relocation on children's outcome using sibling fixed effects.

[6] To account for the over sampling of Hispanic and Black Americans, we apply sample weight to all the OLS regression and sibling fixed effects regression and record the results in the appendix.

On the issue of family relocation, about 77 % of children experience family relocation before age 18 and the average number of family relocation (at county level) is about twice per child by 18. Since we don't have the direct information from children on their relocation history, we infer the relocation history from their mothers' and we assume that children live with their mothers before age 18. We believe this assumption is reasonable in the sense that the vast majority of children reside with their parents before going to college and especially with their mother following divorce.

2) Number of Family Relocation, Age at Family Relocation and Children's Outcome Variables

In order to show how frequent children move before age 20, we construct the following bar plot indicating the percentage of children who relocate with family (at county level) by children's age. As is presented by this graph, at any given age, 20 % of the children relocate at the county level with family.

(Graph 1: Approximately Here)

(Table 2: Approximately Here)

The descriptive statistics on the dependent variables from the above tables begin to shed light on the questions motivating our study. The key outcome variables for this analysis include measures for children's academic achievements and their youth risky behavior such as smoking, drinking, using drugs (primarily marijuana) and running away from home. More specifically, we use individual's highest education achieved and whether the individual finishes high school by age 20 and whether ever repeated grade by age 20 to represent academic achievement. As is mentioned above, individuals who are less than age 20 by year 2010 are excluded from this part of analysis.

And we use whether children start smoking, drinking, using drug and running away from home before age 16 as indicators for children's youth risky behavior. Table 2 gives us a sense of the number of families on which our main identification strategy rests. As we can see, comparing with children who never experience family relocation before age 18, children relocate before 18 on average received about 0.13 year less education, in general have lower possibility (1 % less) to finish high school by the age of 20[7] and display higher probability repeating grade by age 20 (3% more), engaging youth risky behavior such as smoking, drinking using drugs and running away from home before age 16. In general, children who relocate before age 16 have 18% higher probability smoking, 26% higher probability of drinking and 18% higher likelihood to use drugs and 6% higher probability of running away from home comparing to the baseline group.

(Table 3: Approximately Here)

(Table 4: Approximately Here)

Table 3 summarizes our outcome variables of interest by number of family relocation experienced before age 18. As the number of family relocation increases, children on average receive less years of education by age 20, have lower probability of finishing high school by age 20 and they have higher probability of repeating grade by age 20 , engaging in risky behavior such as smoking, drinking, using drugs and running away from home before age 16. This table demonstrates the importance of accounting for multiple relocation going through by children as the impact of family relocation could be accumulating over time.

[7] In this paper, high school graduation rate is defined by finishing high school by age 20. GED recipients are not counted as high school graduates. See Heckman and LaFontaine (2010) "The American High School Graduation Rate: Trends and Levels" for the detailed discussion of using different sources of data (the NLSY 79, Census and the PSID) to get comparable high school graduate rate.

Table 4 compares children's academic performance and risky youth behavior across 7 (or 6) groups based on their age at family relocation. We use children who experience family relocation above age 18 (or age 16) and children who never relocate over our sample period as a benchmark for comparison. Noticing that children who relocate during age 13-15 are mostly affected by family relocation as their school performance is poorer and they show a higher probability of engaging in risky youth behavior such as smoking, drinking, using drugs and running away from home before age 16 comparing to other age groups.

The NLSY79 also gathers series information on children's behavior development. The behavior problems index is asked parents to children from age 4-14. There are 26 questions asked for all children and 2 questions asked only for children have been to school. For each question, parents reply that the statement is "often true", "sometimes true", or "not true". To convert into a total score, the NLSY sets "not true" equal to zero and "often true" or "sometimes true" equal to one, then sums the answers to the questions (so the maximum score is either 26 or 28). The NLSY then standardizes the total score by child's age. We use the standardized behavior index. Since the questions are formed in a way to detect potential behavior problems for children, therefore a higher numerical score representing worse behavior problems.

(Graph 2: Approximately Here)

(Graph 3: Approximately Here)

From graph 2, there is some persistent pattern existing for children who go through different numbers of family relocation. The behavior problem indexes for children who experience family relocation twice and above rises above the other groups indicating frequent relocation would be positively related with increasing behavior problem.

Similarly noted from graph 3, the behavior problem index for children who ever relocate before age 18 is much higher comparing with those who never relocated before age 18 for all different age and the discrepancy increases with children's age.

V. Estimation Results

Before evaluating the results of the estimation of equation 7 with sibling fixed effects, a useful benchmark for comparison is the standard OLS regression with no family fixed effects. We employ a linear probability model (LPM) for discrete outcome variables. The following tables report these results for the overall sample using OLS.

(Table 5-6: Approximately Here)

In the above tables, model 1 displays the estimation results from our baseline model when we only include one dummy variable showing child relocation by age 18 for education outcome and by age 16 for youth risky behavior. Model 2 shows results when we include multiple dummy variables indicating relocation at different age intervals.

In table 5, regressions for the first 4 columns of education outcomes exclude samples that are below age 20 since we primarily use years of education by age 20 and whether finishing high school by age 20. There are other variables such as grade score in school or school drop rate available used in relevant studies. They are not our primary choice for the following reasons: 1) Grade scores are usually not standardized and not comparably across different schools, regions. 2) School drop rate doesn't preserve enough information on children's education level and therefore not informative enough to use in estimation. 3) We are more interested in the long run effect of family relocation on children's education attainment rather than some transient and short term effects. If in the long run, the assumed negative family relocation effect would go away, then we would have proper reason to believe family relocation could work out for children in the long run despite some temporally undesirable effects. Therefore, using highest education achieved by age 20 and a dummy variable indicating finishing high school by age 20 would be superior as outcome variables for education attainment than others. Besides, we also employ another outcome variable for education which is whether individual ever repeats grade in school. For risky behavior, we exclude samples that are below age 16 by 2010 as we primarily focus on youth behavior problem such as smoking, drinking, using drugs and running away from home by age 16. Studies show early involvement in these activities during young adulthood would lead to substantial high probability of youth delinquent behavior and even criminal behavior. As a starting point, it would make sense for us to start from identify the "gateway" effects of those youth behavior problem.

In both tables, the OLS estimates show persistent statistically negative effects of family relocation before age 18 (or 16) on children's education attainment, youth risky behavior such as smoking, drinking, using drugs and running away from home. The coefficients for our γ (ever relocated by age 18 or 16) are all statistically significant pointing to the potential correlation between family relocation and children's outcome. When we include multiple time intervals on family relocation in model 2, the coefficients are negative and statistically significant for children who experience family relocation ranging from age 13-15 and age 16-18 in terms of education attainment and positive for children who experience family relocation at age 13-15. The negative coefficients suggest potential negative impacts of family relocation on children's education outcome, while the positive coefficients for children's age at relocation on those behavior outcome variables signify that comparing with the benchmark group, they have a higher probability to engage in youth risky behavior at an early age. This result pertains most of the previous research done this area that is to present the negative relation between residential mobility and children's development. However, whether this correlation stands by causal correlation between these factors needs to be further examined.

To visualize the point estimates for the coefficients of interest and their confidence interval, we hereby present the following graph with the estimates of the coefficients and their 95% confidence interval. While the negative impact of family relocation during different age periods for education outcomes are mostly significant for age group 13-15, we can see a clear pattern for youth risky behavior outcomes with an increasing negative impact of family relocation as age at relocation increases. For education outcomes, this rising trend of increased impact of family relocation by relocation age is hardly detectable, however for risky behavior the OLS results suggest that relocation at later age causes larger negative impact on children's risky behavior. This trend seems to suggest that parents could choose the proper timing for family relocation in order to minimize the potential negative impacts of family relocation. We will see whether this phenomenon would be preserved in our sibling fixed effects.

(Graph 4: Approximately Here)

(Graph 5: Approximately Here)

In addition, all the other explanatory variables have the expected sign and are mostly statistically significant. From the regression results, ethnicity difference exists in the sense that comparing to non-Hispanic and non-Black group, Hispanic and Black American groups receive less education and have higher probability of engaging in risky behavior in general. Female children in the household have an average higher education years and higher probability to finish high school by the age of 20. The female children are more prone to report running away from home comparing with male children. The coefficients of sibling order for education outcome are negative and statistically significant indicating younger children have lower education achievement meanwhile the coefficients for risky behavior are significant positive implying younger children have higher probability of engaging risky behavior and there are some spillover effects of youth risky behavior from older sibling to younger sibling in the family. Family size also matters as it reduces education achievement for children. An interesting result for the coefficient of mother's working status (when outcome variable is evaluated) is that mother currently working would increase children's education achievement but it may also increase the risky behavior of children at the same time. Mother's involvement in work would reduce the time spent with children this would potentially increase the chance for them to pick up youth risky behavior. However, we should be careful when making causal inference based on this piece of information since these negative features from mother may be correlated with other unobserved variables which cause the increase of youth risky behavior. In other words, for instance, the dummy variable representing whether mother's age is less than 19 at birth of children would be correlated with the error term and therefore the coefficient from this regression could be biased. Therefore, the coefficient from this naïve regression method is subject to further investigation.

Here we present our main results from sibling fixed effects after controlling series of non shared characteristics among siblings such as birth cohort, birth order; mother's working status and whether mother is less than 19 when give birth of the child.

(Table 7: Approximately Here)

(Table 8: Approximately Here)[8]

Table 7 demonstrate sibling fixed effects estimates on children's education outcomes and behavior problem such as smoking, drinking, using drugs and running away from home.

[8] For sibling fixed effects model, we only retain families with at least two children and eliminate all the twin samples.

Surprisingly, the significant positive effects of family relocation on education outcomes and negative effects on youth behavior disappear and get substantially reduced after we use the within family variation as an estimation strategy. Comparing with groups who relocate above age 16 and those who never relocate, children who relocate at an early age display non detectable differences from the former group in terms of the relative outcome variables. And this result persists in model 2 when we include several relocation age intervals to address the timing issue of family relocation. We couldn't detect any consistent pattern or trend in terms of the timing of family relocation. The coefficients from the relocation age interval estimates are closely centered at zero.

From the estimation results of the sibling fixed effects, we find some interesting results. As for the coefficient of the female variable, it is still statistically significant and positive implying female children have higher education (about 0.4 years more education) and show higher probability (12% more) to finish high school and lower probability of repeating grade by age 20. Other family characteristics that are not shared among siblings become statistically insignificant in our sibling fixed effects suggesting that our concern on the coefficient from OLS is addressed. The previous statistically significant coefficients for family characteristics do not imply causal correlation but inferring a correlation between regressor and residual terms. Therefore, it is within our expectation that the coefficients for those variables after controlling the variation within family using sibling fixed effects become insignificant as they are differenced out by our estimation strategy.

(Graph 6: Approximately Here)

(Graph 7: Approximately Here)

Despite the fact that most of the regression coefficients are not statistically significant, the coefficients for sibling order are mostly significant indicating there are indeed spillover effects from older siblings to younger ones. To make the estimated coefficients comparable with the ones we get from previous OLS regression, we have the above graphs showing the point estimates and their relative confidence interval. As is shown from the pictures, all the estimated coefficients after controlling for sibling fixed effects are not statistically significant at a 5% level. The point estimates are all quite near to the zero vertical line.

We believe the following reasons may contribute to the regression results. Firstly, the negative effects of family relocation on children's education and youth behavior are overly exaggerated especially if we put the question under a bigger picture where we are not focusing on those disadvantaged families. When we use sibling fixed effects, we purge out family characteristics that are similar among siblings and only use the variation for age at relocation, therefore, it provides us with cleaner estimation strategy but not necessarily more significant results. By taking the difference among siblings we increase the noise signal ratio which leads to larger standard error. Secondly, children before school age (0-6) should not be affected by family relocation happened during this period since they have not yet attended school, therefore the school mobility story doesn't hold and their behavior patter has not been formed. On the contrary, we would argue that previous researches who find significant negative effects on young children actually capture other family characteristics rather than the effects of family relocation on children's outcome.

VI. Conclusion

By applying a sibling fixed effects regression model to get around the endogeneity problem of family relocation and selection bias, this paper has re-examined the link between family relocation and educational attainment and youth risky behavior. Unlike some of the previous research which emphasizes the negative impacts of family relocation for young children on their education attainment and youth risky behavior, our results imply that family relocation which happens at different stage of children's life could have non detectable impact on children's long run development. To sum up, for school age children ranging from age 13-18, we discover no evidence of the detrimental effects of family relocation on children's education achievement, risky behavior such as smoking, drinking, using drugs and running away from home in our sibling fixed effects model. Our research leads to an increased understanding of why, despite active intervention such as the Moving to Opportunity (MTO) project, the expected outcomes of improving the long term outcome for children who are prone to family shocks have often not been materialized. The specific family characteristics that derived the decision of family relocation could be contributing to children's outcomes in the long run. Our findings could help policy makers to form a good understanding on why family relocation is often associated with poor academic performance and increased youth risky behavior, therefore they could design solutions that address the genuine driving force of family relocation and offset the potential negative family shocks on children in the long run.

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Tables and Graphs

Table 1: Summary Statistics for Different Samples

	Master Sample		Subsample for Education		Subsample for Risky Behavior	
Variables	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Female	0.49	0.005	0.49	0.006	0.49	0.005
Sibling Order	2.08	0.012	1.92	0.013	1.99	0.011
Number of Children	2.93	0.013	3.01	0.018	2.93	0.014
Hispanic	0.20	0.004	0.23	0.005	0.20	0.004
Black	0.26	0.004	0.32	0.006	0.26	0.005
Non-Hispanic/Non-Black	0.54	0.005	0.45	0.006	0.54	0.005
Number of Relocation before 18	2.08	0.007	2.48	0.010	2.13	0.008
Mother's Age at Birth	26.5	0.055	23.9	0.055	25.1	0.046
Mother less than 19 at Birth	0.08	0.004	0.12	0.005	0.10	0.004
Mother Never Married	0.09	0.003	0.09	0.004	0.09	0.003
Mother Always Remain Married	0.48	0.004	0.41	0.006	0.47	0.004
Mother Ever Divorced	0.43	0.004	0.50	0.006	0.44	0.005
Mother – Finished High School	0.50	0.005	0.50	0.006	0.52	0.005
Mother – Some College	0.28	0.004	0.32	0.006	0.28	0.005
Mother – College and Above	0.22	0.004	0.18	0.005	0.20	0.004
Ever Relocated before age 18	0.77	0.004	0.87	0.006	0.78	0.004
Number of Observations	11,498		4,926		8,247	

Table 2: Summary Statistics for Dependent Variables

	Mig18=0		Mig18=1	
	Mean	Standard Deviation	Mean	Standard Deviation
Total Years of Education	11.98	0.066	11.85	0.024
Whether Completed High School by Age 20	0.72	0.018	0.71	0.007
Whether ever Repeated Grade by Age 20	0.20	0.011	0.24	0.005
Whether Started Smoking by Age16	0.19	0.009	0.37	0.006
Whether Started Drinking by Age16	0.27	0.010	0.53	0.006
Whether Started Using Drug by Age16	0.15	0.008	0.33	0.006
Whether Started Running away from Home by Age 16	0.04	0.005	0.10	0.004

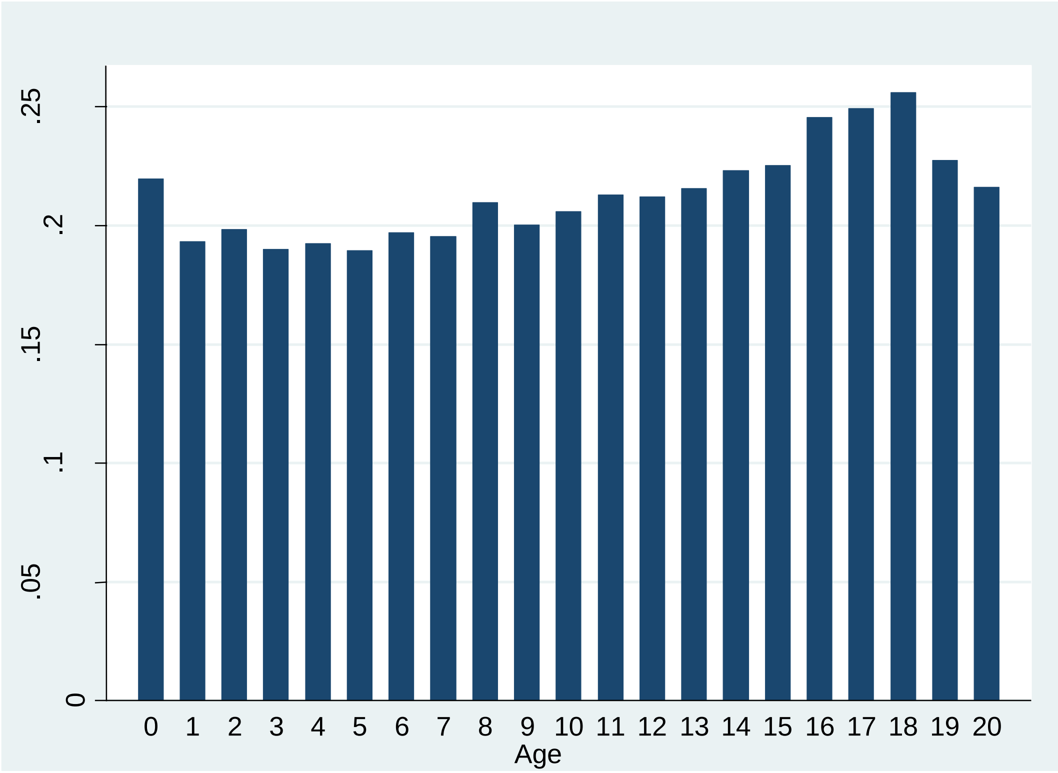
Table 3: Summary Statistics on Dependent Variables by

Numbers of Family Relocation before Age 18

	Sample 1-Education			Sample 2- Risky Behavior			
	Highest Education by 20	Finish High School by 20	Ever Repeated Grade by 20	Smoking Before 16	Drinking before 16	Using Drug before 16	Running away before 16
Never Relocate	11.98	0.72	0.08	0.19	0.27	0.15	0.04
Relocate Once	11.92	0.72	0.12	0.31	0.46	0.29	0.09
Relocate 2-3 times	11.89	0.71	0.10	0.37	0.53	0.33	0.10
Relocate 4 times and Above	11.74	0.68	0.13	0.43	0.59	0.40	0.13
Number of Observation	4,926			8,247			

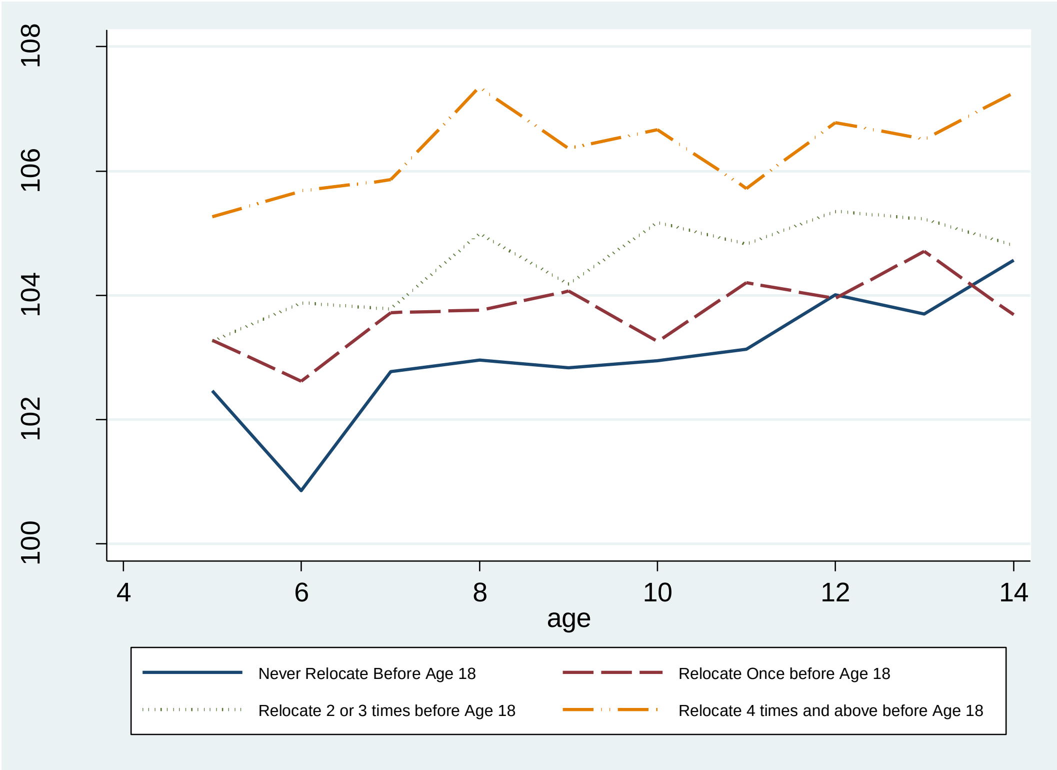
Table 4: Summary Statistics on Dependent Variables by Age at Relocation

	Sample 1-Education			Sample 2- Risky Behavior			
	Highest Education	Finish High School before 20	Ever Repeated Grade	Smoking before Age 16	Drinking before Age 16	Using Drug before Age 16	Running away before Age 16
Relocate at Age 0-3	11.80	0.69	0.21	0.36	0.49	0.33	0.10
Relocate at Age 4-6	11.80	0.69	0.22	0.34	0.46	0.30	0.09
Relocate at Age 7-9	11.95	0.73	0.20	0.34	0.52	0.34	0.10
Relocate at Age 10-12	11.90	0.72	0.20	0.38	0.57	0.34	0.10
Relocate at Age 13-15	11.84	0.69	0.22	0.43	0.61	0.39	0.12
Relocate at Age 16-18	11.68	0.67	0.25	0.48	0.65	0.44	0.13
All the others	11.76	0.68	0.22	0.37	0.50	0.33	0.09
Number of Observation	4,926			8,247			



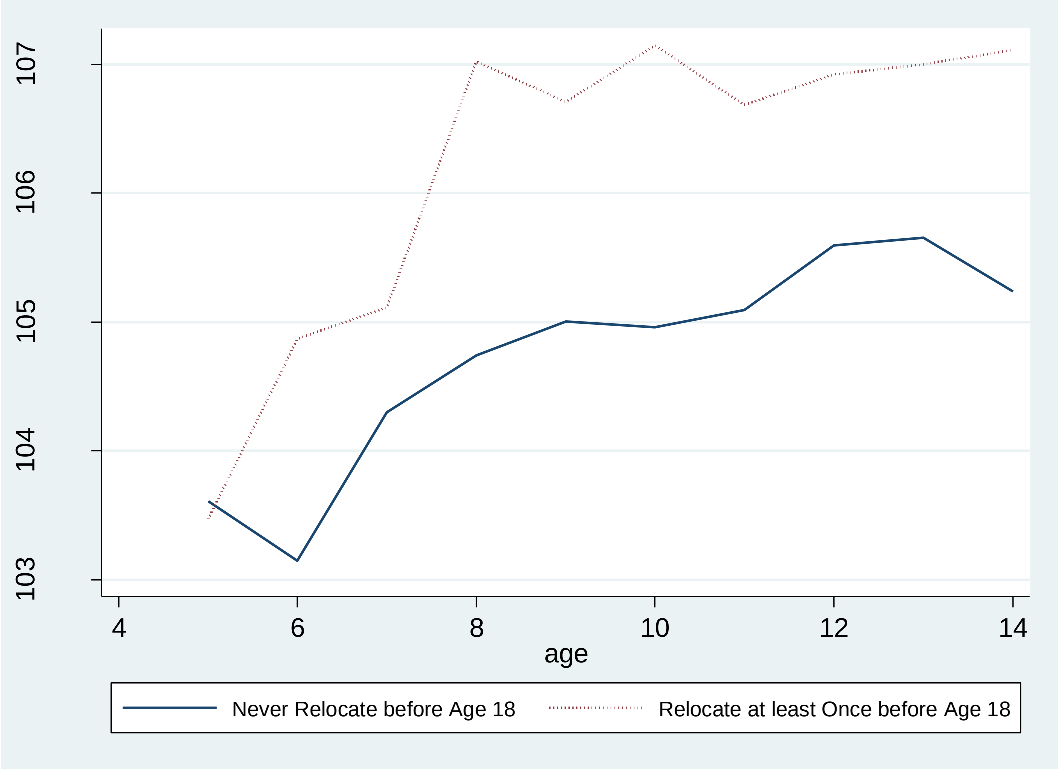
Graph 1: Percentage of Children who change residence by Age

(Source: The NLSY79 Children and Young Adults)



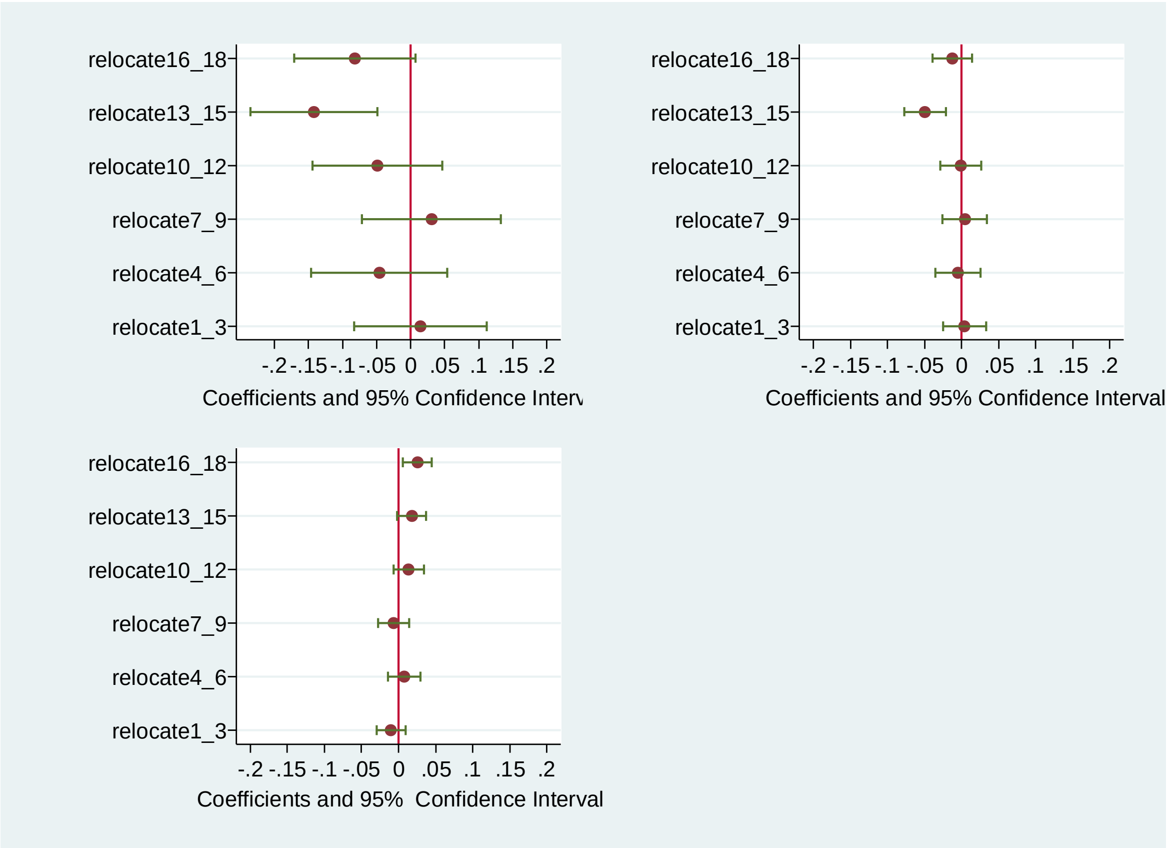
Graph 2: Behavior Index by Numbers of Family Relocation before Age 18

(Source: The NLSY79 Children and Young Adults)

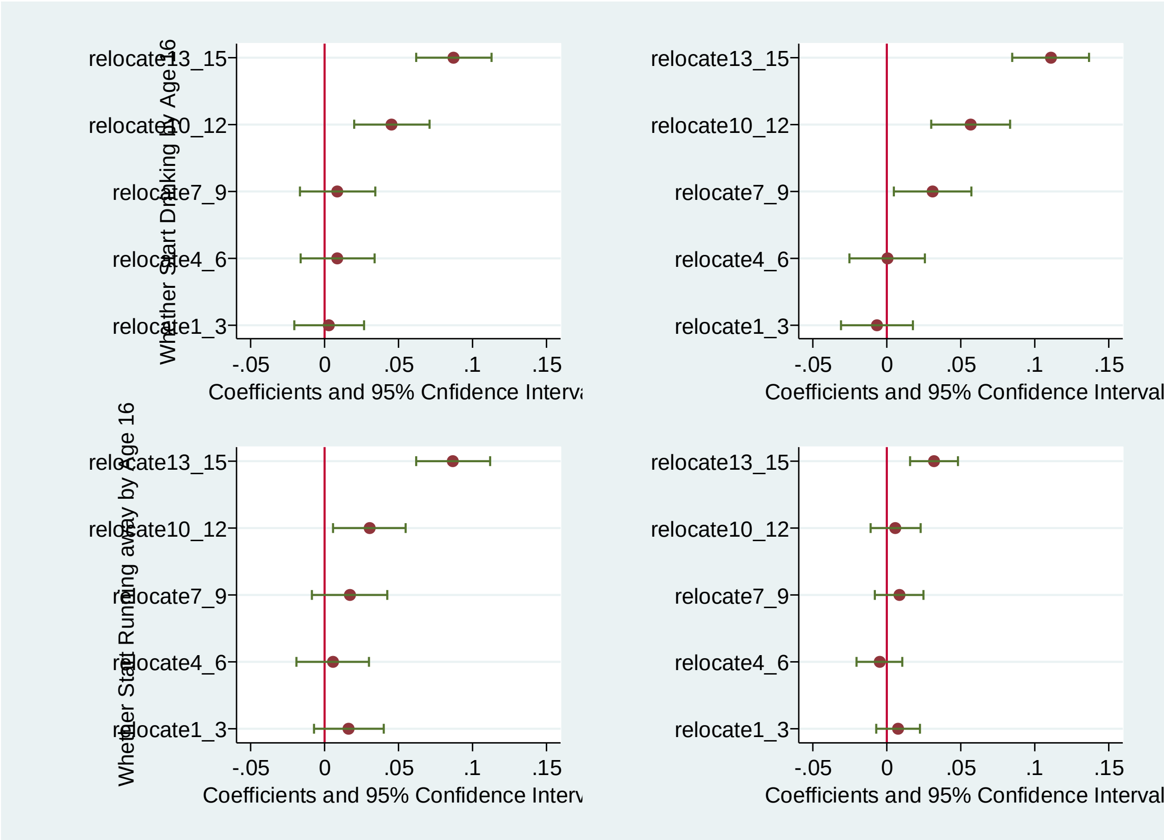


Graph 3: Behavior Index by Whether Relocate before Age 18

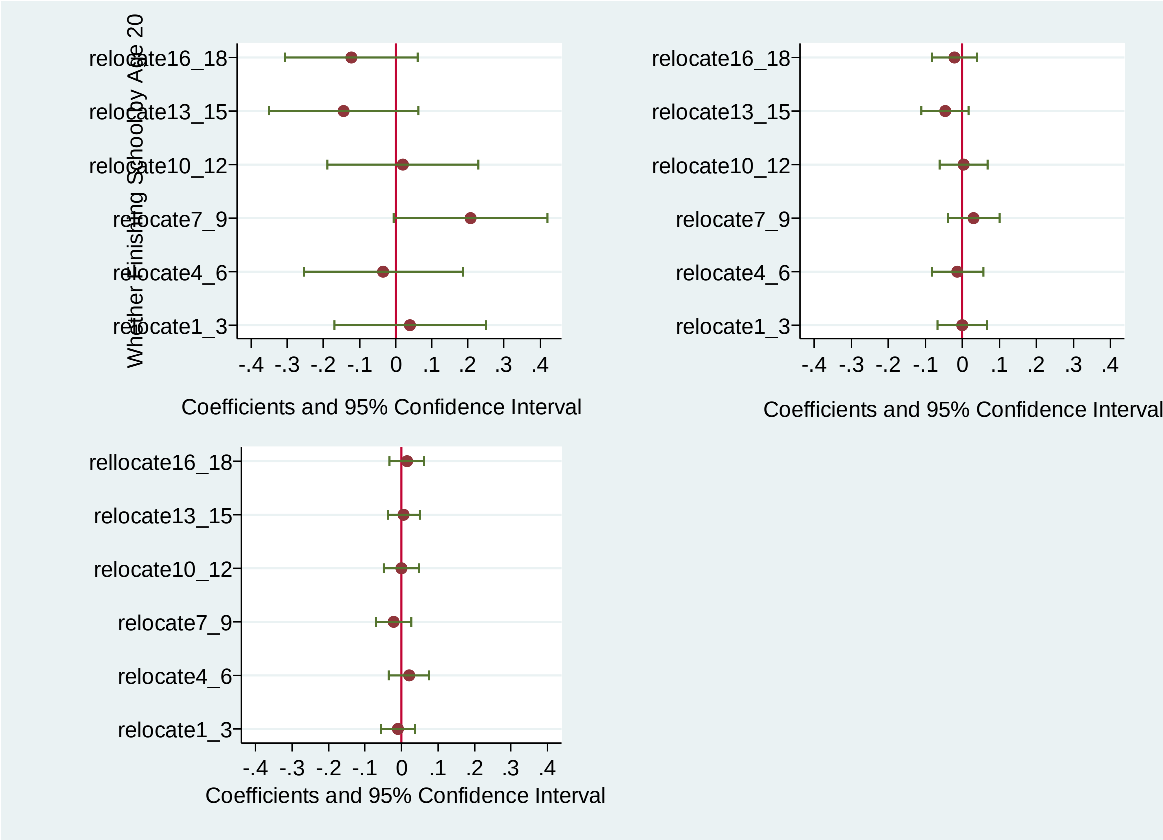
(Source: The NLSY79 Children and Young Adults)



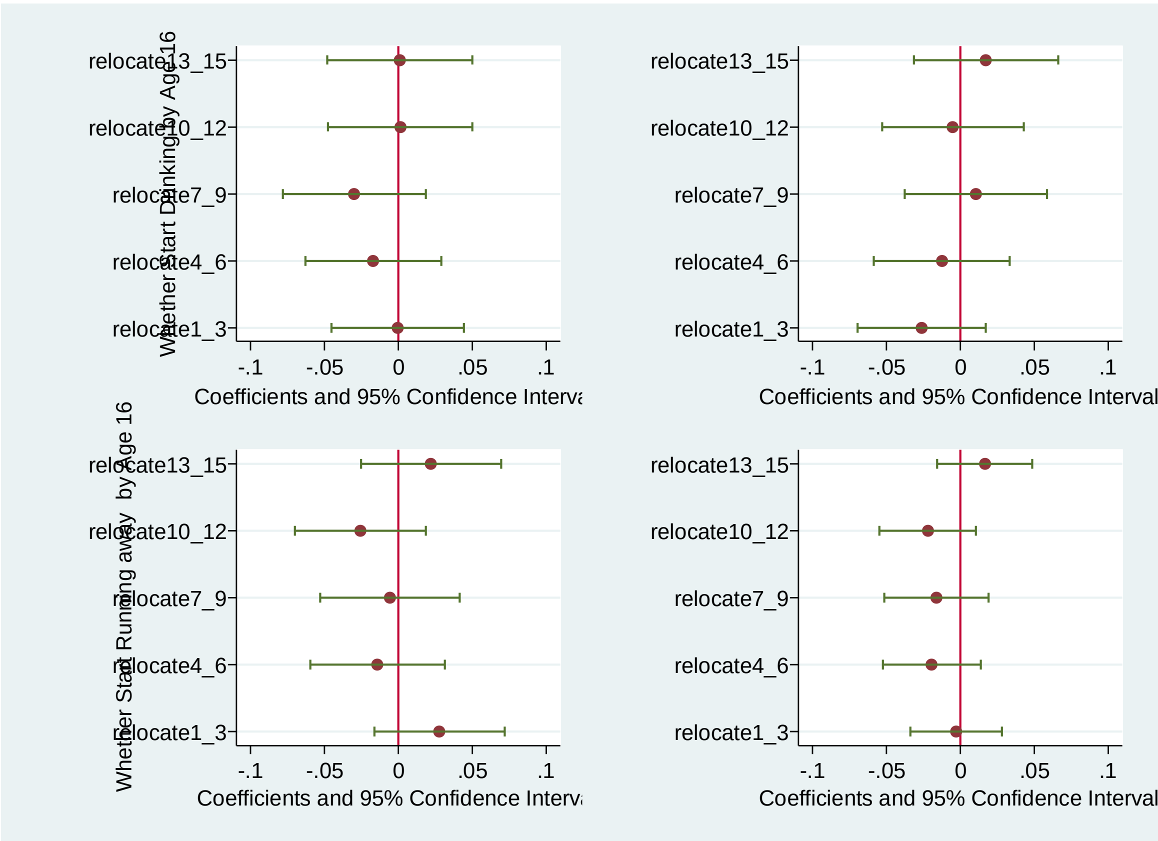
Graph 4: Point Estimates of the Coefficients and Confidence Intervals from OLS Regression Education Outcomes



Graph 5: Point Estimates of the Coefficients and Confidence Intervals from OLS Regression -Youth Risky Behavior Outcomes



Graph 6: Point Estimates of the Coefficients and Confidence Interval from Sibling Fixed Regression -Education Outcomes



Graph 7: Point Estimates of the Coefficients and Confidence Interval from Sibling Fixed Regression -Youth Risky Behavior Outcomes

Table 5: OLS Estimation for Children's Outcome Variables –Education Outcomes

	Highest Education		Finish High School by Age 20		Ever Repeated Grade by Age 20	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Female	0.371***	0.369***	0.105***	0.105***	-0.0512***	-0.0510***
	(0.0414)	(0.0415)	(0.0123)	(0.0122)	(0.00880)	(0.00877)
Hispanic	-0.132**	-0.132**	-0.0322*	-0.0324*	0.0199	0.0197
	(0.0641)	(0.0640)	(0.0180)	(0.0180)	(0.0123)	(0.0123)
Black	-0.0354	-0.0295	-0.00925	-0.00730	0.0617***	0.0607***
	(0.0567)	(0.0567)	(0.0166)	(0.0166)	(0.0121)	(0.0121)
Sibling Order	-0.129***	-0.128***	-0.0298***	-0.0297***	0.0221***	0.0216***
	(0.0274)	(0.0272)	(0.00846)	(0.00842)	(0.00663)	(0.00660)
Mother less than 19 at Birth	-0.237***	-0.235***	-0.0696***	-0.0699***	0.0254	0.0242
	(0.0863)	(0.0865)	(0.0253)	(0.0254)	(0.0177)	(0.0177)
Mother Working	0.401***	0.402***	0.112***	0.112***	-0.0162	-0.0165
	(0.0586)	(0.0588)	(0.0163)	(0.0163)	(0.0112)	(0.0111)
No. of Children	-0.0962***	-0.0948***	-0.0240***	-0.0235***	0.00997*	0.00981*
	(0.0223)	(0.0223)	(0.00657)	(0.00656)	(0.00566)	(0.00563)
Always Marry	0.491***	0.477***	0.129***	0.126***	-0.0535**	-0.0496**
	(0.0875)	(0.0876)	(0.0260)	(0.0260)	(0.0235)	(0.0233)
Ever Divorce	0.146*	0.144*	0.0532**	0.0530**	-0.0164	-0.0147
	(0.0842)	(0.0846)	(0.0253)	(0.0254)	(0.0235)	(0.0234)
Ever Relocate by 18	-0.148**		-0.0290		0.0296**	
	(0.0711)		(0.0189)		(0.0117)	
Age Relocate 1-3		0.0143		0.00384		-0.0102
		(0.0495)		(0.0148)		(0.0100)
Age Relocate 4-6		-0.0459		-0.00465		0.00737
		(0.0509)		(0.0156)		(0.0112)
Age Relocate 7-9		0.0307		0.00440		-0.00661

		(0.0521)		(0.0154)		(0.0108)
Age Relocate 10-12		-0.0486		-0.000734		0.0135
		(0.0487)		(0.0141)		(0.0104)
Age Relocate 13-15		-0.142^{***}		-0.0491^{***}		0.0177^{*}
		(0.0475)		(0.0144)		(0.01000)
Age Relocate 16-18		-0.0817^{*}		-0.0127		0.0256^{**}
		(0.0455)		(0.0137)		(0.00997)
Mo_SomeCollege	0.441^{***}	0.439^{***}	0.100^{***}	0.0997^{***}	-0.0631^{***}	-0.0626^{***}
	(0.0526)	(0.0525)	(0.0157)	(0.0157)	(0.0105)	(0.0104)
Mo_CollegeAbove	0.794^{***}	0.794^{***}	0.161^{***}	0.161^{***}	-0.0799^{***}	-0.0798^{***}
	(0.0628)	(0.0630)	(0.0174)	(0.0174)	(0.0109)	(0.0109)
_cons	11.18^{***}	11.14^{***}	0.488^{***}	0.482^{***}	0.0771^{**}	0.0840^{***}
	(0.156)	(0.148)	(0.0455)	(0.0440)	(0.0323)	(0.0323)
N	4926	4,926	4926	4,926	4926	4,926

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Table 6: OLS Estimation for Children's Outcome Variables –Youth Risky Behavior

	Start Smoking by 16		Start Drinking by 16		Start Using Drug by 16		Start Running away by 16	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Female	-0.0417***	-0.0405***	-0.0244**	-0.0228**	-0.0854***	-0.0842***	0.0175***	0.0178***
	(0.0100)	(0.00999)	(0.0104)	(0.0103)	(0.00967)	(0.00964)	(0.00633)	(0.00632)
Hispanic	0.0428***	0.0392**	0.113***	0.109***	0.119***	0.116***	0.0280***	0.0273***
	(0.0158)	(0.0157)	(0.0161)	(0.0160)	(0.0158)	(0.0158)	(0.00978)	(0.00980)
Black	-0.0496***	-0.0569***	-0.0158	-0.0250	0.0148	0.00886	-0.00918	-0.0109
	(0.0150)	(0.0150)	(0.0156)	(0.0156)	(0.0148)	(0.0148)	(0.00874)	(0.00878)
Sibling Order	0.0191***	0.0193***	0.00883	0.00868	0.0228***	0.0231***	-0.00378	-0.00378
	(0.00726)	(0.00723)	(0.00726)	(0.00724)	(0.00687)	(0.00685)	(0.00433)	(0.00433)
Mother less than 19	-0.00205	-0.00228	0.0152	0.0149	0.0148	0.0149	0.0230	0.0234
	(0.0215)	(0.0213)	(0.0214)	(0.0212)	(0.0213)	(0.0211)	(0.0145)	(0.0145)
Mother Working	0.194***	0.185***	0.293***	0.280***	0.159***	0.151***	0.0542***	0.0518***
	(0.0116)	(0.0117)	(0.0125)	(0.0124)	(0.0113)	(0.0113)	(0.00681)	(0.00696)
No. of Children	0.00227	0.00119	0.00375	0.00263	-0.00364	-0.00471	0.0132***	0.0130***
	(0.00585)	(0.00580)	(0.00637)	(0.00625)	(0.00602)	(0.00596)	(0.00360)	(0.00359)
Always Marry	-0.100***	-0.0952***	-0.0619**	-0.0549**	-0.0854***	-0.0818***	-0.0484***	-0.0470***
	(0.0229)	(0.0227)	(0.0241)	(0.0239)	(0.0221)	(0.0219)	(0.0136)	(0.0135)
Ever Divorce	0.0346	0.0330	0.0523**	0.0518**	0.0332	0.0304	-0.00649	-0.00688
	(0.0233)	(0.0230)	(0.0240)	(0.0239)	(0.0225)	(0.0223)	(0.0141)	(0.0141)
Ever Relocate by 16	0.0669***		0.0923***		0.0654***		0.0238***	
	(0.0126)		(0.0132)		(0.0123)		(0.00687)	
Age Relocate 1-3		0.00298		-0.00666		0.0164		0.00764
		(0.0121)		(0.0123)		(0.0119)		(0.00758)
Age Relocate 4-6		0.00881		0.000255		0.00559		-0.00484
		(0.0127)		(0.0130)		(0.0125)		(0.00789)
Age Relocate 7-9		0.00877		0.0311**		0.0170		0.00845

		(0.0130)		(0.0133)		(0.0130)		(0.00836)
Age Relocate 10-12		0.0454***		0.0566***		0.0303**		0.00592
		(0.0131)		(0.0135)		(0.0125)		(0.00855)
Age Relocate 13-15		0.0872***		0.111***		0.0867***		0.0319***
		(0.0130)		(0.0133)		(0.0128)		(0.00825)
Mo_SomeCollege	0.0167	0.0162	0.0426***	0.0423***	0.0108	0.0102	0.0126	0.0126
	(0.0140)	(0.0139)	(0.0142)	(0.0141)	(0.0137)	(0.0137)	(0.00861)	(0.00860)
Mo_CollegeAbove	-0.0482***	-0.0485***	-0.0163	-0.0167	-0.0325**	-0.0330**	0.00320	0.00300
	(0.0156)	(0.0155)	(0.0169)	(0.0168)	(0.0150)	(0.0150)	(0.00898)	(0.00900)
_cons	0.343***	0.370***	0.299***	0.341***	0.279***	0.299***	0.0477**	0.0548**
	(0.0342)	(0.0340)	(0.0356)	(0.0351)	(0.0336)	(0.0337)	(0.0213)	(0.0214)
N	8247	8,247	8247	8,247	8247	8,247	8247	8,247

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Table 7: Sibling Fixed Effects Estimation for Children's Outcome Variables
Education Outcomes

	Highest Education		Finish High School by 20		Ever Repeated Grade by 20	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Female	0.339***	0.348***	0.119***	0.121***	-0.0389**	-0.0389**
	(0.0704)	(0.0706)	(0.0225)	(0.0225)	(0.0168)	(0.0168)
Sibling Order	-0.162**	-0.164**	-0.0470*	-0.0476*	0.0307	0.0300
	(0.0775)	(0.0765)	(0.0263)	(0.0261)	(0.0213)	(0.0213)
Mother Single	-0.000314	-0.00586	0.0166	0.0140	-0.0160	-0.0161
	(0.158)	(0.157)	(0.0476)	(0.0476)	(0.0361)	(0.0361)
Mother Working	0.214	0.204	0.0744*	0.0712*	-0.0211	-0.0212
	(0.154)	(0.154)	(0.0427)	(0.0428)	(0.0338)	(0.0340)
Ever Relocate by 18	-0.347		-0.0768		0.0119	
	(0.236)		(0.0698)		(0.0449)	
Age Relocate 1-3		0.0403		-0.000316		-0.0103
		(0.107)		(0.0335)		(0.0235)
Age Relocate 4-6		-0.0338		-0.0132		0.0203
		(0.112)		(0.0353)		(0.0283)
Age Relocate 7-9		0.207*		0.0310		-0.0223
		(0.109)		(0.0357)		(0.0248)
Age Relocate 10-12		0.0204		0.00302		-0.000311
		(0.107)		(0.0331)		(0.0246)
Age Relocate 13-15		-0.144		-0.0466		0.00584
		(0.106)		(0.0323)		(0.0223)
Age Relocate 16-18		-0.123		-0.0217		0.0146
		(0.0940)		(0.0311)		(0.0241)
_cons	11.60***	11.35***	0.583***	0.541***	0.0890	0.0960*
	(0.279)	(0.236)	(0.0862)	(0.0737)	(0.0564)	(0.0514)
N	3606	3,606	3606	3,606	3606	3,606

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 8: Sibling Fixed Effects Estimation for Children's Outcome Variables
Youth Risky Behavior

	Start Smoking by 16		Start Drinking by 16		Start Using Drug by 16		Start Running away by 16	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Female	-0.0506***	-0.0496***	-0.0399**	-0.0411**	-0.0879***	-0.0889***	0.0364***	0.0354***
	(0.0162)	(0.0162)	(0.0163)	(0.0163)	(0.0155)	(0.0154)	(0.0114)	(0.0114)
Sibling Order	0.0380**	0.0361**	0.0519***	0.0525***	0.0379**	0.0382**	0.0146	0.0125
	(0.0170)	(0.0169)	(0.0159)	(0.0158)	(0.0156)	(0.0157)	(0.0104)	(0.0103)
Mother Single	-0.0448	-0.0459	0.0272	0.0284	-0.0291	-0.0293	0.0185	0.0163
	(0.0342)	(0.0343)	(0.0335)	(0.0334)	(0.0325)	(0.0326)	(0.0268)	(0.0268)
Mother Working	-0.0104	-0.00713	0.00866	0.00445	-0.0396	-0.0390	0.0402**	0.0412**
	(0.0286)	(0.0287)	(0.0273)	(0.0274)	(0.0262)	(0.0263)	(0.0202)	(0.0200)
Ever Relocate by 16	-0.0229		-0.0173		-0.0156		-0.00933	
	(0.0304)		(0.0287)		(0.0288)		(0.0196)	
Age Relocate 1-3		-0.00152		-0.0260		0.0289		-0.00420
		(0.0226)		(0.0219)		(0.0225)		(0.0157)
Age Relocate 4-6		-0.0168		-0.0112		-0.0136		-0.0195
		(0.0233)		(0.0234)		(0.0232)		(0.0169)
Age Relocate 7-9		-0.0329		0.00993		-0.00718		-0.0201
		(0.0247)		(0.0244)		(0.0240)		(0.0178)
Age Relocate 10-12		-0.00307		-0.00221		-0.0290		-0.0229
		(0.0247)		(0.0243)		(0.0225)		(0.0166)
Age Relocate 13-15		0.00196		0.0184		0.0202		0.0175
		(0.0251)		(0.0249)		(0.0241)		(0.0163)
_cons	0.510***	0.505***	0.524***	0.524***	0.428***	0.408***	0.0986***	0.106***
	(0.0482)	(0.0460)	(0.0442)	(0.0426)	(0.0439)	(0.0443)	(0.0333)	(0.0326)
N	6677	6,677	6677	6,677	6677	6,677	6677	6,677

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Chapter 2

Estimating the Effects of School Mobility on Children's Education, Youth Risky Behavior and Arrest History

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Abstract: This study employs the variation in sibling's age at the time of primary and high school changes (other than promotion from primary school to high school) in order to estimate the effects of school mobility on children's education achievement, youth risky behavior such as smoking, drinking and using drugs and youth arrest history. We use individual school change history from primary education up to age 20 by 2010 from the NLSY 97 and control for sibling fixed effects to estimate how the variation in children's age at school change would affect a set of outcome variables. After controlling for sibling fixed effects, we find school change made at age 16-18 would significantly reduce children's education achievement by age 20 and increase their possibility for repeating grade in school, however, we find none detectable negative impacts of school mobility on children's risky behavior and youth arrest history comparing with their non-mobile peers.

Keywords: school mobility, education, youth risky behavior, sibling fixed effects

JEL classification: D10, J13

I. Introduction

School mobility[9] is a frequent event in American education. Among most of the industrialized countries, U.S. students have the highest rate of residential and school mobility (Long, 1992). Approximately 20% of U.S. households change the location of their residences in any given year and residential mobility is closely associated with school mobility. According to the 2004 U.S. Census, 15%-20% of school-aged children move within one year of enrollment (Schacter, 2004). Researchers and parents have long voiced concern about the effects of school enrollment changes on children's adjustment and achievement (Blane, Pilling and Fogelman, 1985; Cramer and Dorsey, 1970) as it creates a hardship for schools and hampers attempts to properly monitor the progress of students (Newman, 1988; Sewell, 1982). Accordingly, the consequences of student mobility have been the focus of numerous studies. Obtaining accurate estimates of the effects of school mobility is also of interest to policymakers because mobility is potentially alterable by public policy.

The purpose of this study is to assess the impacts of school mobility on student academic achievement, youth risky behavior and arrest history in a general setting where mobile children are compared with a stable population. Moreover, the study is an examination of the impact of multiple school changes happened at different age periods for children on the outcomes of interest. It addresses the question that whether the timing of school change matters for children's outcomes. In most cases, school mobility is triggered by residential mobility. Mobility is self-selective in the sense that parents actively seek better job or living opportunities to another place. Meanwhile, children may serve as sufficient reasons for families relocating to better school districts in order to

[9] School mobility refers to changes in school enrollment at times other than those prompted by program design (Staresina, 2004).Although many (58%) of these changes are related to residential moves, 42% are initiated by the school or related to issues and problems arising at the school (Kerbow, 1996).

facilitate children's schooling. Residential and school mobility can be considered as a natural consequence of social mobility and increased economic opportunity (Kopf, 1977) for the family. On the other hand, residential and school mobility could also be triggered by negative events such as job loss, separation, divorce, and the decease of family member or other forms of disruption. Longitudinal data from the Panel Study of Income Dynamics (PSID) shows that parental divorce sharply increases the annual probability that children will move out of their neighborhoods. Residential mobility is closely associated with school mobility, especially in condensed urban area.

Existing studies link school mobility to a wide range of child and adolescent outcomes and develop much diversified conclusions (Astone and McLanahan, 1994; Simpson and Fowler, 1994; Kerbow, 1996; Alexander, Entwisle and Dauber, 1996; Tucker, Marx and Long, 1998; Scanlon and Devine, 2001). Most of these researches document a clear pattern that residential and school moves are associated with poor academic performance, poor health condition and delinquent youth behavior, whereas other researches point that the association between mobility and children's outcome may be spurious. In a seminal article, Schaller (1976) cautions researchers to control for preexisting differences when examining the effects of mobility. Among the studies that have used controls, results suggest that school mobility has a negative effect on academic performance, above and beyond the impact of other stressful features of a child's life (Astone and McLanahan, 1994; Haveman, Wolfe and Spaulding, 1991; Heinlein and Shinn, 2000). Some of the negative consequences associated with school mobility include: lower math and reading test scores (Mantzicopoulos and Knutson, 2000), a higher likelihood of being held back a grade (Simpson and Fowler, 1994) and an increased risk of behavior and delinquent problems (Tucker, Marx and Long, 1998; Wood, Halfon, Scarlata, Newacheck, and Nessim, 1993).

Most of the previous researches take the naïve approach to evaluate how school mobility would affect children's outcomes. That is to estimate the parameters of a regression equation in which the dependent variable is children's outcomes (measured at a specific age), and the explanatory variables include an indicator for whether the child changes school, demographic variables, and, at times, variables such as family income and labor market participation of the mother. The coefficient of the school mobility indicator is meant to capture the effect of school change on children's outcome. However, as most of the school changes are self-selective based on unobserved family characteristics, the results from this approach are biased. The negative effects of school mobility on children's outcome could be amplified when choosing families with low social economic status. While the selection of school change creates measurement error for estimating the direct effects of school mobility on children's outcomes, all the children that are present in the household share similar family characteristics which can be eliminated using family fixed effects, then we can rely on the natural variation in terms of children's age when school change to identify the timing effects of multiple school mobility on children's outcomes.

This paper uses the NLSY97 (National Longitudinal Survey of Youth 1997) to study the impact of school mobility on children's schooling, youth risky behavior and arrest history. The NLSY97 consists of a nationally representative sample of approximately 9,000 youths who were 12 to 16 years old as of December 31, 1996. Round 1 of the survey took place in 1997. Youths are interviewed on an annual basis. The NLSY97 is designed to document the transition from school to work and into adulthood. It collects extensive information about youth's educational experiences over time. Educational data includes youths' schooling history, performance on standardized tests, course of study, the timing and types of degrees, and a detailed account of progression through post-secondary schooling. The NLSY97 also contains self-administered information on alcohol, drug use, criminal behavior and arrest history. To sum up, we are able to extract the school change history from primary school up to the end of secondary education of the NLSY97 sample as well as their academic performance, youth risky behavior and youth arrest history by age 20 using this rich data set, meanwhile, we are also able to control for any observed family characteristics using the provided information from the NLSY97. By implementing sibling fixed effects regression model, it permits us to hold constant effects which are common to all siblings in the same household, we can then control for all sources of observed and unobserved heterogeneity at the family level. Since the within-family strategy relies on difference in age of siblings when changes school, we control for birth order and birth cohort effects in all specifications. We find significant negative impact of school change for children at school age (13-18) on children's schooling, risky behavior and arrest history during young adulthood by apply the naïve OLS estimation on the general sample constructed from the NLSY97. After controlling for sibling fixed effects, the negative impact of school mobility on children's education outcome persists if school change is made at age 16-18 while the negative impacts on risky behavior and youth arrest history diminishes to no detectable zero. This finding points to the potential bias existing in previous literature due to the selection of school change on unobserved family characteristics for youth delinquency problems.

II. Literature

The influence of student mobility upon schooling and academic achievement has been widely studied. There is a distinction between school mobility and residential mobility, as a change of residential address does not necessitate a change of school. School mobility is more closely associated with school attainment. School attainment has been measured by achievement tests, age–grade progress and highest education level. Most of the recent studies have documented the negative association between school mobility and school performance (Gao, 1994; Mehana, 1997). Using nationally representative data from the U.S. Department of Education's Prospects Study and controlling for family income, the Gao report reveals that students who have attended three or more schools since entering first grade are much more likely by third grade to have low reading test scores and are more likely to have repeated a grade. Rumberger and Larson (1998) and Swanson and Schneider (1999) use data from the National Education Longitudinal Study to examine the relationship between school mobility and high school dropout. Some studies (Blane, Pilling and Fogelman, 1985; Reynolds, 1991) indicate that the estimated effects of school mobility on achievement are often minimal or inconclusive. Establishing a relationship between school mobility and educational attainment is a complex problem, however, due to the many confounding variables that must be considered. Children who live in homes characterized by multiple types of socioeconomic disadvantage have been found to be more vulnerable to the effects of stressful life events, such as school transfer, compared to children who experience fewer disadvantages (DuBois, Felner, Meares and Krier, 1994). For example, children who move with greater than average frequency are more likely to be poor, more likely to come from a single-parent home, and more likely to be in a household where the householder is unemployed or failed to graduate from high school (Long, 1992).

Staresina (2003) notes that mobility’s effects on student achievement are potentially substantial. Mobile children experience an array of issues other than academic difficulties. The disruption of learning, gaps in content, behavioral problems and social difficulties result in mobile students being at a greater risk for dropping out of school and engaging in risky and delinquent behavior. Besides, school mobility may disrupt children's relationships with peers and teachers, and reduce the stability and predictability of established patterns of activities so important for optimal adjustment (Cole, 1993). Rumberger (1998, 2002)'s research concludes that mobility is a host of issues related to children' learning and behavior. Recent studies have reported significant effects of school mobility on an array of adolescent behaviors, including high rates of school dropout (Teachman, Paasch and Carver, 1996), drug and alcohol abuse (Hoffman and Johnson, 1998), and other emotional and behavioral problems (Tucker, Marx and Long, 1998). Research also shows that changing schools is often associated with reduced social competence and self-esteem which could lead to increased youth risky and criminal behavior (Gutman and Midgley, 2000). Overall, the line of reasoning is that changing school is psychologically disruptive and disorienting to school aged children. Their social networks are disrupted and they may struggle adjusting to new peers (Pane, McCaffrey, Kalra and Zhou, 2008)

Three methodological problems existing in previous literature can limit causal interpretations about the relation between school mobility and children's outcome.

First, most research in this area fails to consider that it is not changing school per se that leads to negative or positive educational, youth behavioral and delinquent outcomes, but rather the underlying reasons why the school mobility occurs in the first place. There may be important differences between mobile and non-mobile students which account for the observed relationship between school mobility and academic, behavioral and youth delinquent outcomes and those underlying differences might explain the deleterious effects of school mobility commonly found in previous literature. Most studies of mobility do not take pre-mobility achievement into account. For those that have used controls (Blane, Pilling and Fogelman, 1985; Reynolds 1989 and Reynolds 1991), they find that mobile children have lower school achievement than school stable children prior to moving. We control for pre-mobility differences at the household level using sibling fixed effects model across mobile and non mobile students in an attempt to isolate the impact of school mobility on education achievement, youth risky and delinquent behavior by the end of secondary education. Our objective is to obtain estimated effects of school mobility that are not tainted by selection bias caused by the unobserved family heterogeneity and the nonrandom assignment of mobility across students.

Second, lots of research has focused on fragile families with low social economic status which makes the negative relation between school mobility and children's academic performance, youth risky and criminal behavior more pronounced. Youth who changes school frequently may already be performing worse academically, less involved in school activities, at a higher risk of dropping out, and more involved in a variety of delinquent and problem behaviors due to the their low social economic status. There are relatively few studies on the effects of mobility using the general population of the U.S. children. This paper attempts to fill this gap by using national representative sample constructed using the NLSY 97.

Third, most of the studies focus on a single recent move seeking to differentiate movers in the new setting from non-movers. Few have been based on the idea that the effects of moving may be cumulative and related to the number of past moves. This approach is rare, but has been used in several recent explorations of the potential harmful effects of school mobility on various dimensions of children's school performance and behavior. Our paper considers the timing as well as the frequency of past school changes on children's outcome in order to segregate the impacts and make proper estimation.

We believe prior research has not done an adequate job of examining why children are selected into mobility. We are not the first, however, to raise the possibility that important selection effects may be driving the association between mobility and youth outcomes. Pribesh and Downey (1999) find that preexisting differences account for 90% of the difference in test scores between movers and non-movers. Strand and Demie (2006) investigate the association between student mobility and academic achievement in a sample of 2,279 elementary school students in London, England. Mobility is defined as any change in schools during the elementary years. Results indicate a negative association between mobility and achievement. However, the association decreases once demographics are added to the model and the association lost significance once prior achievement is added to the model. Francisca M. Antman(2012) claims that migrants and nonimmigrants are likely to differ in unobservable ways that also affect children's educational outcomes. Her paper uses Mexican data suggesting addressing selection problem by looking within the family to exploit variation in sibling's ages at the time of parental migration to the U.S. and finds positive effects of father migration to the U.S. on daughters' education outcome.

In this paper, we not only investigate the relationship between school mobility and youth outcomes such as academic performance, risky youth behavior as smoking, drinking and substance use at early age and youth arrest history but also address the issue of timing of multiple school changes on children's outcomes. Using the NLSY97, we are able to identify the complete school change history for each individual from primary school to the end of secondary education. We examine whether school mobility has detrimental effects on children's academic performance and whether it leads to risky youth behavior and criminal behavior at early age of those children. In order to control for selection bias, we explore the natural age variation between siblings at the event of school change. There hasn't been enough discussion in terms of the timing of school change. It is a relatively novel idea to use the age variation generated by sibling order to evaluate the effects of school change on children's outcome. Another highlight for this paper is that we use dummy variables indicating different age periods when estimating how age at school change would affect children's academic performance, risky youth behavior and arrest history. This could potentially capture the bumpy or discontinuous nature of the impact of interest. We believe that this paper represents a more rigorous attempt to adjudicate between causal and selection hypotheses about the effects of school mobility than we have seen in previous work. Our paper addresses the endogeneity and selection problem of school change that are overlooked by prior research. Moreover, we find that the timing of school change still matters for children's academic achievements but doesn't matter much for the form of youth risky behavior and youth being arrested once the family heterogeneity is controlled.

III. Methods and Estimation Strategy

1) Identification Strategy

The following section introduces the identification strategy our paper relies on. We begin by introducing a baseline econometrics model for this paper illustrated by equation (1). Then we briefly explain the dependent variables and independent variables present in our model. School mobility is defined as changes in school enrollment at times other than those prompted by program design (Staresina, 2004). By taking the difference between siblings who changes school before age 18 (age 16 for risky behavior outcomes and arrest history), we get equation (2) which is not sufficient for us to identify γ. We then take another step which is to include families whose children don't change school before age 18 (age 16 for risky behavior outcomes and arrest history) and take the conditional difference between these two types of family and eventually we arrive at equation (3) where γ is identified. Here is how our identification lays out.

Here we present our baseline model for estimation.

$$y_{ij}=\beta' X_{ij}+\delta_1 BirthCohort_{ij}+\delta_2 SiblingOrder_{ij}+\gamma 1_{schoolchange}+\alpha_j+\varepsilon_{ij} \quad i=1,2\ ;\ j=1,2,\ldots,n \quad (1)$$

y_{ij} represents outcome variable of interest (dependent variable) for sibling i in family j. Academic achievements of children are among the most important outcome variables in relevant studies that draw our attention. In this research, we want to evaluate the long run impact of school change on children's ability to finish high school before certain age (age 20 in our specification) and their highest years of education achieved by age 20. We choose this criterion based on the following concern. We want to investigate the long run impact of school change on children's education outcome. Nonstandard test scores would be hard to compare across different residential area and school districts. Moreover, we want to fully utilize the schooling information for each individual in the sample. An age criterion is chose to compare education outcomes for children to conserve the sample size and maintain comparability.

Inquiry into the relationship between psychological and behavior problems and moving has also been undertaken (Mundy, Robertson, Greenblatt and Robertson, 1989; Simpson and Fowler, 1994; Stacks, 1994; Tooley, 1970). Mobile children are more likely to be psychiatrically hospitalized, more likely to initiate drug and alcohol use (Catalano, Hawkins, White and Pandina, 1985), and more likely engage in premarital sexual behavior (Stacks, 1994). It is not unreasonable to expect similar mobility effects on children in terms of youth risky behavior such as smoking, drinking, using drugs and being arrested due to youth delinquency behavior at early age. Therefore, we will also investigate how the age difference at school change contributes to youth risky and delinquent behavior starting at early age. So in addition to education outcome variables we also include youth risky behavior outcomes and youth arrest history as dependent variables in our model. Our outcome variables include discrete variables indicating whether finishing high school by age 20, ever repeated grade by age 20 and whether starting smoking, drinking, using drug by age 16 and ever been arrested by age 16 and continuous variable representing the highest grade achieved by age 20.

X_{ij} is a vector of covariates which can vary between siblings in family j. It may include children's age at school change, whether mother is less than 19 years old when giving birth of the child and whether children live with both parents at age 6 and age 12. Living with both parents at a certain age is meant to capture marriage stability for the family and control for shocks coming from marriage dissolution. $BirthCohort_{ij}$ represents the birth year therefore birth cohort each individual belongs to. $1_{schoolchange}$ is an indicator variable which equals to 1 when the observation ever changes school before the end of their secondary education and equals to zero otherwise.$1_{schoolchange}$ can interact with X_{ij} variables such as gender and ethnicity.

α_j denotes unobserved family characteristics and ε_{ij} is an idiosyncratic error term. We make additional assumptions about individual fixed effects which include shared characteristics among siblings such as mother's education, relative family income status comparing to the federal poverty level and unobserved idiosyncratic errors. The first part can be captured by including family fixed effects. The idiosyncratic errors are assumed to be an i.i.d process. Notice that α_j might be correlated with the decision of school change. γ represents the impact of school change on outcome variables and it is the parameter we want to identify for this particular exercise.

Traditional OLS estimation using the above equation suffers from endogeneity problem as the unobserved family heterogeneity may contribute to the decision of school change for children. OLS regression fails to control for the unobserved time invariant characteristics α_j. To eliminate the unobserved family effect α_j we can just take the difference between sibling 1 and sibling 2 using equation (1) conditional on children who have ever changed school since primary school to the end of secondary school:

$$E\left[y_{1j}-y_{2j}\middle|X_{1j}=\widetilde{X_{1j}},\ X_{2j}=\widetilde{X_{2j}},\ 1_{1,schoolchange}=1, 1_{2,schoolchange}=0\right]$$

$$=\beta'\Delta\widetilde{X}_j+\delta_1\Delta\widetilde{BirthCohort}_j+\delta_1\Delta\widetilde{SiblingOrder}_j+\gamma \tag{2}$$

This identification relies on sibling difference which includes families with at least 2 children. The outcome variables y_{1j} and y_{2j} are correlated in the sense that the second sibling might be picking up some behavior pattern from the first sibling. For instance, if the first child starts smoking at an early age, the second sibling might be affected and starts to smoking at an early age as well. To control for this correlation between y_{1j} and y_{2j}, we include sibling order in the regression model (1). After we take difference between siblings conditional on family whose children both change school, any characteristics of the family that do not vary across siblings, i.e. parents' education, total number of siblings, family income, etc., would also be differenced out (and therefore should not be included in estimation). Nevertheless, we still could not unique identify γ the coefficient of interest. To tackle this problem, we should also include families whose children only change schools for grade promotion in estimation since these can be used to control for any possible general trends in the outcome variables across siblings from different birth cohorts. In other words, under appropriate assumptions which we will discuss shortly that strategy identifies the differences in outcomes due to differences in the age at school change rather than simply the difference in age per se. When including non-mobile children in estimation, we are essentially comparing the difference in outcomes between siblings with the same age difference in mobile children and non-mobile children and relating this to the children's different age at post school change. To incorporate this idea, we apply the following approach.

$$E\left[y_{1j}-y_{2j}\middle|X_{1j}=\widetilde{X_{1j}}, X_{2j}=\widetilde{X_{2j}}, 1_{1,schoolchange}=1, 1_{2,schoolchange}=0\right]$$

$$-E\left[y_{1j'}-y_{2j'}\middle|X_{1j'}=\widetilde{X_{1j}}, X_{2j'}=\widetilde{X_{2j}}, 1_{1,schoolchange}=0, 1_{2,schoolchange}=0\right]$$

$$=\beta'\Delta\widetilde{X}_j+\delta_1\Delta\widetilde{BirthChort}_j+\delta_1\Delta\widetilde{SiblingOrder}_j+\gamma-(\beta'\Delta\widetilde{X}_j+\delta_1\Delta\widetilde{BirthCohort}_j+\delta_1\Delta\widetilde{SiblingOrder}_j)$$

$$= \gamma \tag{3}$$[10]

From equation (3), γ can be uniquely identified from equation (3). This illustration takes families with two children as an example. However, this identification approach can be easily generalized to situations with more than two siblings.

Our identification strategy is able to control several sources of endogeneity that leads to bias in estimation results. Firstly, traditional OLS is unable to control for unobserved individual characteristics, therefore yields to biased results. It is important to note that idiosyncratic differences or other time-varying differences among siblings are themselves not sufficient to bias results; that difference must be somehow systematically related to the age or birth rank of the siblings in a way that we are unable to sufficiently capture with other included time varying (sibling-varying) covariates. By taking difference between siblings, we remove the unobserved individual and family characteristics and are able to produce persistent estimates for our coefficients. Secondly, another source of endogeneity comes from selection bias for children change school frequently. Selection bias is associated with underlying family characteristics that essentially trigger school change. Controlling for sibling fixed effects also takes care of this selection bias by eliminating the unobserved family characteristics that lead to school change. To sum up, the main virtue of this strategy is that it allows us to account for family specific characteristics that might be correlated with outcomes of children. Our main specification for the model would be using sibling fixed effects to estimate the impacts of school change at different age periods on children's outcomes of interest. Meanwhile, we will also present results based on the pooled population of the relevant cohorts without sibling fixed effects.

[10] Taryn Ann Galloway (2012) applies similar identification strategy in the study of timing of divorce and its impact on children's crime-related and educational outcomes.

2) Empirical Model

In the empirical model, we extend our baseline model (1) to include more discrete variables representing school changes for children during different age periods. The baseline group is chosen as children who haven't made make school changes up to 18 (or 16). We are interested in the impact of school change on children whose age is less than 18 (or 16), therefore our empirical model amounts to:

$$Outcome_{i,j} = \beta' X_{ij} + \delta_1 BirthCohort_{ij} + \delta_2 Sibling\ Order_{ij} + \gamma_1 1_{6-9} + \gamma_2 1_{10-12} + \gamma_3 1_{13-15} + \gamma_4 1_{16-18} + \alpha_j + \varepsilon_{\mathrm{ij}}$$

Or

$$Outcome_{i,j} = \beta' X_{ij} + \delta_1 BirthCohort_{ij} + \delta_2 Sibling\ Order_{ij} + \gamma_1 1_{6-9} + \gamma_2 1_{10-12} + \gamma_3 1_{13-15} + \alpha_j + \varepsilon_{\mathrm{ij}} \qquad (4)$$

where $Outcome_{i,j}$ is the outcome variables for child i in family j. We construct outcome variable by exploring individual's life history from child (age 6) to young adulthood (by the age of 20 or 16). Those outcome variables include discrete variables indicating the probability of finishing high school by age 20 , ever repeated grade by age 20 or starting smoking, drinking alcohol, using drug and whether ever been arrested by the age of 16 and continuous variable representing the highest grade achieved by the age of 20. Instead of only including one dummy variable for whether children change school from age 6 to 18, in the estimation part, we include four dummy variables (three age interval dummies depending on the outcome variables) indicating children's age at school change in a much smaller interval intending to capture the multiple school changes happened during different age periods. I_{6-9} represents dummy variable indicating whether children change school between age 6 to age 9, if yes this variable equals to 1, otherwise, it equals to zero. Similar explanations apply to other index variables representing child age groups when children make school change. For education outcome, we leave out children who never change school other than grade promotion before age 18 as control group for our sibling effects estimation while for risky behavior outcomes, we leave out children who never change school by age 16 as control group as the cut off age for risky behavior is 16.

Our independent variables include variables that representing shared family characteristics such as ethnicity, mother's education, adjusted family income relative to the federal poverty level and total number of children in the household and variables that representing non shared individual characteristics such as gender, birth cohort, birth order, whether mother was less than 19 at the birth of children, and whether the child lives with both parents at age 6 and age 12.

Several recent studies show that among families with more than one child first born child on average outperforms their younger siblings in terms of educational outcomes (Kristensen and Bjerkedal, 2007, Booth and Kee, 2009). For this reason, it is important to include controls for sibling order in the analysis and to use non mobile children to control for general difference in the outcomes related to birth order among siblings. Moreover, if there is any spillover effects among siblings, for instance, younger child pick up the negative behavior problem from older sibling and therefore perform worse in terms of outcome variables, we can capture this learning experience by controlling sibling order in our model. To account for any general time trend in children's education outcomes and behavior pattern, we also control for birth cohort in our analysis.

One important virtue of the estimation strategy used here is that it can be easily extended to allow the impact of school change to vary depending on the age of the child at the time of the event. Distinguishing effects based on the children's age at the time of the school change also brings this paper into relation with the literature on child development and family dynamics in other context such as divorce and separation. Despite these topics share lots of similarities, an important feature of school mobility is that it happens more frequently over the life course of individual therefore creating multiple shocks at different age for individuals. Previous research has been vague about how they handle multiple school changes happened at different age when evaluating the impact of school mobility on children's outcomes and many of them choose to treat this as one time shock to children. To control for the impact of multiple school changes happened at different age for the same individual, we allow the age at school change indicator variables to be non-exclusive to each other. If the individual changes school at age 7, then 1_{6-9} will be set to 1 and if the same individual change school the second time at age 11, then 1_{10-12} will be set to 1 as well. The exclusive category for our estimation is those who never change school before age 18 (or 16).

IV. Data

The data sets we use for the study is the National Longitudinal Study of Youth 1997 (NLSY97). The survey sample is designed to represent U.S. residents in 1997 that are born during the years 1980 through 1984. The original sample includes 8,984 respondents originated from 6,819 unique households. The NLSY97 collects extensive information on respondents' labor market behavior and educational experiences. The survey also includes data on the youths' family and community backgrounds to help researchers assess the impact of schooling and other environmental factors. Educational data includes youths' schooling history, performance on standardized tests, course of study, the timing and types of degrees, and a detailed account of progression through post-secondary schooling. Aside from education information, the NLSY97 contains detailed information on many other topics. Subject areas in the questionnaire include: youths' relationships with parents, marital and fertility histories, dating, sexual activity, criminal behavior, alcohol and drug use. Areas of the survey those are potentially sensitive, such as sexual activity and criminal behavior, comprise the self-administered portion of the interview. One unique aspect of the NLSY97 is that round one contains parent questionnaire that generates information about the youths' family background and history. Information in the parent questionnaire includes: parents' marital and employment histories, relationship with spouse or partner, ethnic and religious background, health (parents and child), household income and assets and so on.

The advantage of using these data sets over others is that we can track down a complete school change history for each child since primary school to the end of secondary education. Moreover, the panel structure of the NLSY data can be useful in terms of tracking children's academic performance and the development of youth risky behavior and arrest history for the entire youth period (up to age 20). Instead of looking for the short run and intermediate impact of family relocation, we can study the long run impact of school change on children's outcome.

Our main sample is constructed from the NLSY97. For our analysis, we build on two samples with different age eligibility. First sample is for the study of school change and children's education outcomes. The age eligibility for this sample is age 20 by 2010. We obtain 7,654 individuals with non-missing values in education and other independent variables. Second sample is for the study of youth risky behavior such as smoking, drinking and using drugs such as marijuana and youth arrest history. The age eligibility for second sample is 16. We retain 7,847 individuals for the second analysis. By comparing the descriptive characteristics from the sub samples and the master sample, we don't detect systematic difference in demographic factors such as gender composition, ethnic composition and others between our subsamples and master sample.

1) Descriptive Statistics

(Table 1: Approximately Here)

Table 1 provides the summary statistics of the master sample and two subsamples for the key variables used for this study. In total, we have 8,984 individual observations from 6,819 households. Based on the regression specification later, we restrict our sample to a subset of all the individual observations available. The figures in the table describe children's demographic and family characteristics for our samples. From table 1, in terms of gender distribution, we have a relatively balanced sample with about 50% of the observation being female. The ethnic composition represents the over sampling of Hispanic and Black Americans in the NLSY. In the subsample, Hispanic and Black take larger component than the master sample. The number of children in the household reveals the information about the average family size for a typical American family would be 1.55. Our table also shows family background information or more precisely family characteristics that are shared or not shared among siblings. For instance, mother's education is shared feature among siblings in the same household while whether mother was less than 19 at the birth of the child may be different for individual child. The NLSY97 data has relative complete information on their biological mother but it doesn't contain all the similar information about children's biological father as their mothers. About 60% of the NLSY97 children's mothers have finished high school education. In terms of marriage stability, 50% of the children live with both parents when age 6 and 48% of them live with both their parents at age 12.

On the issue of school change, about 82 % of children experience school change before age 18 and the average number of school change is 1.42 by age 18. We can see that all three samples have quite similar demographic characteristics, therefore our subsamples for education outcome and youth risky behavior and youth arrest history are representative samples of the master sample of the NLSY97.

2) Number of School Change, Age at School Change and Children's Outcome Variables

(Table 2: Approximately Here)

The descriptive statistics on the dependent variables from the above tables begin to shed light on the questions motivating our study. The key outcome variables for this analysis include measures for children's academic achievements and their youth risky and delinquent behavior such as smoking, drinking, using drugs (primarily marijuana) and being arrested. More specifically, we use individual's highest education achieved and whether the individual finishes high school by age 20 and whether ever repeated grade by age 20 to represent academic achievement. As mentioned above, individuals who are less than age 20 by the year of 2010 are excluded from this part of analysis. And we use whether children start smoking, drinking, using drug and ever been arrested before age 16 as indicators for children's youth risky and delinquent behavior. Table 2 gives us a sense of the number of families on which the main identification strategy rests. As we can see, comparing with children who haven't change school before age 18, the relative mobile children on average have lower possibility (1 % less) to finish high school by the age of 20[11] and display higher probability repeating grade by age 20 (8% more). However, in terms of risky and arrest history, the mobile and non-mobile children display similar characteristics except the mobile children group has slightly higher probability reporting ever been arrested by age 16 (3% higher than non-mobile children).

(Table 3: Approximately Here)

(Table 4: Approximately Here)

[11] In this paper, high school graduation rate is defined by finishing high school by age 20. GED recipients are not counted as high school graduates. See Heckman and LaFontaine (2010) "The American High School Graduation Rate: Trends and Levels" for the detailed discussion of using different sources of data (NLSY 79, Census and PSID) to get comparable high school graduate rate.

Table 3 summarizes our outcome variables of interest by number of school changes children experienced before age 18. As the number of school change increases, children on average receive less years of education by age 20 and have lower probability of finishing high school by age 20 and they have higher probability of repeating grade by age 20, engaging in risky behavior such as smoking, drinking, using drugs and being arrested before age 16. This table demonstrates the importance of accounting for multiple relocation going through for children as the impact of school changes could be accumulating over time.

Table 4 compares children's academic performance and risky youth behavior across 4 groups based on their age at school change. We use children who have never experience school change before age 18 (or age 16) as a benchmark for comparison. Noticing that children who relocate from age 16-18 are mostly affected by school change as their highest education by age 20 are the lowest and have the lowest possibility of finishing school by age 20 and have the highest possibility of repeating grade by age 20.

V. Estimation Results

Before evaluating the results of the estimation of equation 4 with sibling fixed effects, a useful benchmark to explore for comparison is the standard OLS regression with no sibling fixed effects. We employ a linear probability model (LPM) for discrete outcome variables. The following tables report these results for the overall sample using OLS (Linear Probability Model)

(Table 5: Approximately Here)

(Table 6: Approximately Here)

In those tables we see that for the sample as a whole, the OLS estimates show persistent statistically negative effects of school change before age 18 (or 16) on children's education attainment, youth risky and delinquent behavior such as smoking, drinking, using drugs and ever have been arrested by age 16. From the estimation results, children who change school at age 16-18 are mostly negatively affected in terms of their education achievement and changing school at age 13-15 significantly increases the possibility of engaging in risky behavior. Changing school at age 10-12 is associated with increased possibility of being arrested by age 16. This result pertains most of the previous research done this area that is presenting a negative relation between school mobility with children's education and behavior development. However, whether this correlation stands by causal correlation between these factors needs to be further examined.

To visualize the point estimates for the coefficients of interest and their confidence interval, we hereby present the following graph with the estimates of the coefficients and their 95% confidence interval. While the impacts of school change during different age periods for education outcomes are mostly significant for age group 13-18, we can see that for youth risky and delinquent behavior outcomes there are increasing negative impacts of school mobility as age at school change increases.

(Graph 1: Approximately Here)

(Graph 2: Approximately Here)

In addition, all the other explanatory variables have the expected sign and are mostly statistically significant. From the OLS regression results, ethnicity difference exists in the sense that comparing to non-Hispanic and non-Black group, Hispanic and Black American groups receive less education and have higher probability of repeating grade by age 20, however they do not perform worse in terms of youth risky and delinquent behavior. Notice that, female children in the household have an average higher education years, higher probability to finish high school by the age of 20 and lower probability engaging in youth risky behaviors . The coefficient of sibling order for education outcome is negative and statistically significant indicating younger children have lower education achievement meanwhile the coefficients for risky behavior are significantly positive implying younger children have higher probability of engaging risky behavior and there are some spillover effects of youth risky behavior from older sibling to younger sibling in the family. The variable that mother being less than 19 when give birth of the child would reduce the child's education achievement by age 20. Whether the child lives with both parents at age 12 provides a close measure for family stability and it has significant impact over children's education and youth risky behavior. However, we should be careful when making causal inference based on this piece of information since these negative features from mother may be correlated with other unobserved variables which cause the increase of youth risky behavior. In other words, for instance, the dummy variable representing whether mother's age less than 19 at birth of child would be correlated with the error term and therefore the coefficient from this regression could be biased. Therefore, the coefficient from this naïve regression method is subject to further investigation.

(Table 7-9: Approximately Here)

(Table 10-13: Approximately Here)

In table 7-9, we present OLS regression for education outcomes and youth risky behavior by gender. In terms of education outcomes, male children who change school at age 13-18 are more negative affected than female children, whereas in risky behavior, female samples that relocate at age 13-15 are more adversely affected with higher probability engaging in youth risky behavior. A possible explanation for these results is that school mobility selection patterns are driven by heterogeneity across families. Therefore, including family fixed effects addresses this concern by comparing siblings within the same family. Table 10-13 record OLS regression for education outcomes and youth risky behavior by ethnic composition. As we can see from the results, education attainment for Hispanic and Black sample reduces more than Non Hispanic Non Black samples when changing school at age 15-18. In regard to youth risky behavior, Hispanic samples and Black American samples show relative higher probability of engaging risky behavior when relocate at age 13-15.

Here we present our main results from sibling fixed effects after controlling series of non shared characteristics among siblings such as birth cohort, birth order, whether mother is less than 19 when give birth of the child and whether live with both parents at age 6 and 12.

(Table 14: Approximately Here)

(Table 15: Approximately Here)[12]

Table 14 and 15 regressions exclude sample that are below age 20 by 2010 since we primarily use years of education by age 20, whether finishing high school by the age of 20 and whether ever repeated grade by age 20.

From the estimation results of the sibling fixed effects, we find some interesting results. As for the coefficient for birth order, it is only statistically significant for education achievement by age 20 but not for risky behavior outcomes. Other family characteristics that are not shared among siblings become statistically insignificant in our sibling fixed effects suggesting that our concern from OLS is addressed. The previous statistically significant coefficients for family characteristics do not imply causal correlation but inferring a correlation between regressor and residual terms. Therefore, it is within our expectation that the coefficients for those variables after controlling the variation within family using sibling fixed effects become insignificant as they are differenced out by our estimation strategy.

[12] For sibling fixed effects model, we only retain families with at least two children.

(Graph 3: Approximately Here)

(Graph 4: Approximately Here)

Despite the fact that most of the regression coefficients are not statistically significant, we still get large negative impacts of school change at age 16-18 on various education outcomes resonating with previous research on school mobility and students' academic performance. To make the estimate coefficients comparable with the ones we get from previous OLS regression, we have the above graphs showing the point estimates and their relative confidence interval. As is shown from the pictures, most of the estimated coefficients except for school change at age 16-18 for education outcome after controlling for sibling fixed effects are not statistically significant at 5% level. The point estimates for risky behavior outcomes are all quite near to the zero vertical line.

Table 14-15 demonstrate sibling fixed effects estimates on children's behavior problem such as smoking, drinking, using drugs and being arrested by age 16. Studies show early involvement in these activities during young adulthood would lead to substantial high probability of youth delinquent behavior and even criminal behavior. As a starting point, it would make sense for us to start from identifying the "gateway" effects of those youth behavior problem. Surprisingly, the significant negative effects on youth behavior disappear and get substantially reduced after we use the within family variation as an estimation strategy. Comparing with groups who have not changed school by age 16, children who report have changed school at an early age display non detectable differences from the former group in terms of the relative outcome variables. We believe the following reasons may contribute to the regression results. The negative effects of school mobility on youth behavior are overly exaggerated especially if we put the question under a bigger picture where we are not focusing on those disadvantaged families. When we use sibling fixed effects, we purge out family characteristics that are similar among siblings and only use the variation for age at school change, therefore, it provides us with cleaner estimation strategy but not necessarily more significant results. By taking the difference among siblings we increase the noise signal ratio which leads to large standard error. However, we can still capture the negative impacts for school mobility on educational outcomes as school changes pose direct impact over grade promotion for children and therefore their progression towards higher education can be tampered by frequent school changes even the child may be moving to a better school. As for risky behavior, we can see that almost all the coefficients for school changes are close to zero. The negative impacts of school change at 13-16 on behavior outcomes for children from OLS go away once we control for family heterogeneity in sibling fixed effects model. This indicates the spurious relation between school change and youth risky behavior. Unobservable family characteristics and other school characteristics could be driving the negative impacts of school change and youth risky behavior.

We also do additional regressions with our sample split into male samples and female samples and into subsamples by ethnic composition. As we can see from those tables, the results we get from the pooled sample are preserved in the subsamples by gender and by ethnic group.

(Table 16-22: Approximately Here)

VI. Robustness Check and Model Extension

As the frequency of school change brings more information than the dummy variable that whether the child makes the school change, we modify out baseline model to address not only the timing of school change but also the frequency of school change on outcome variables in order to get more precise estimates.

$$Outcome_{i,j} = \beta' X_{ij} + \delta_1 BirthCohort_{ij} + \delta_2 Sibling\ Order_{ij} + \theta_1 SchoolChange_{6-9} + \theta_2 SchoolChange_{10-12} + \theta_3 SchoolChange_{13-15} + \theta_4 SchoolChange_{16-18} + \alpha_j + \varepsilon_{\text{ij}}$$

Or

$$Outcome_{i,j} = \beta' X_{ij} + \delta_1 BirthCohort_{ij} + \delta_2 Sibling\ Order_{ij} + \theta_1 SchoolChange_{6-9} + \theta_2 SchoolChange_{10-12} + \theta_3 SchoolChange_{13-15} + \alpha_j + \varepsilon_{\text{ij}} \quad (5)$$

where $Outcome_{i,j}$ is the outcome variables for child i in family j. The variable school change represents number of school changes made during a specific age interval. The coefficients are meant to capture the impact of a one time school change during an age interval on outcome variables. We do similar OLS regressions and sibling fixed effects are pertained in Table 23-26. The regression results show almost no difference from previous analysis. After controlling for sibling fixed effects, we still find negative impacts of school change made during age 16-18 on education outcome but none negative impacts on youth risky behavior and arrest record. Our results are robust to different model specifications.

VII. Conclusion

By applying sibling fixed effects regression model to get around the endogeneity problem of school change and selection bias, this paper has re-examined the link between school change and educational attainment, youth risky behavior and arrest history. Our results imply that school changes which happen at age 16-18 could have significant negative impacts on children's educational achievement but non detectable impact on children's risky behavior and arrest record.

Unlike some of the previous research which emphasizes the negative impact of school changes on young children, we only find significant negative impact for children who experience school change at age 16-18 which is the high school age for most of them. We discover no evidence of the detrimental effects of school mobility on children's risky behavior such as smoking, drinking, using drugs and their arrest history due to youth delinquency problem. To conclude, we find moderate negative effects of school change on children's education attainment only when the school change happens at age 16-18 but none negative impacts on risky behavior and arrest history for children who change school before age 16.

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Tables and Graphs

Table 1: Summary Statistics for Different Samples

	Master Sample		Subsample for Education		Subsample for Risky Behavior	
Variables	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Female	0.49	0.001	0.49	0.006	0.49	0.006
Sibling Order	1.27	0.001	1.27	0.006	1.27	0.006
Number of Children	1.54	0.001	1.55	0.008	1.55	0.008
Hispanic	0.21	0.001	0.21	0.005	0.21	0.005
Black	0.25	0.001	0.25	0.005	0.25	0.005
Non-Hispanic/Non-Black	0.54	0.001	0.54	0.006	0.54	0.006
Number of School Change before 18	1.42	0.002	1.43	0.004	1.42	0.005
Mother Age less than 19 at Birth	0.13	0.001	0.14	0.004	0.14	0.004
With Both Parents at Age 6	0.50	0.001	0.51	0.006	0.51	0.006
With Both Parents at Age 12	0.48	0.001	0.48	0.006	0.48	0.006
Mother – Finished High School	0.60	0.001	0.60	0.006	0.60	0.005
Mother – Some College	0.34	0.001	0.34	0.005	0.34	0.005
Mother – College and Above	0.06	0.000	0.06	0.003	0.06	0.004
Ever Change School before age 18	0.82	0.001	0.83	0.004	0.83	0.004
Number of Observations	8,984		7,654		7,847	

Table 2: Summary Statistics for Dependent Variables

	SchoolChange18=0		SchoolChange18=1	
	Mean	Standard Deviation	Mean	Standard Deviation
Total Years of Education	11.986	0.052	12.065	0.021
Whether Completed High School by Age 20	0.751	0.012	0.746	0.005
Whether ever Repeated Grade by Age 20	0.203	0.011	0.286	0.006
Whether Started Smoking by Age16	0.412	0.013	0.389	0.006
Whether Started Drinking by Age16	0.474	0.014	0.422	0.006
Whether Started Using Drug by Age16	0.219	0.011	0.215	0.005
Whether ever been Arrested by Age 16	0.102	0.008	0.131	0.004

Table 3: Summary Statistics on Dependent Variables by Number of School Change before Age 18

	Sample 1-Education			Sample 2- Risky Behavior			
	Highest Education by 20	Finish High School by 20	Ever Repeated Grade by 20	Smoking Before 16	Drinking before 16	Using Drug before 16	Ever Arrested before 16
Never Change School	11.986	0.751	0.202	0.411	0.474	0.218	0.102
Change Once	12.389	0.829	0.202	0.366	0.423	0.188	0.089
Change Twice	11.808	0.690	0.338	0.402	0.418	0.235	0.152
Change Three Times	11.600	0.605	0.415	0.415	0.402	0.251	0.181
Change Four Times and Above	11.074	0.482	0.566	0.481	0.441	0.294	0.305
Number of Observation	7,654			7,847			

Table 4: Summary Statistics on Dependent Variables by Age at Relocation

	Sample 1-Education			Sample 2- Risky Behavior			
	Highest Education	Finish High School before 20	Ever Repeated Grade	Smoking before Age 16	Drinking before Age 16	Using Drug before Age 16	Running away before Age 16
Chang at Age 6-9	11.770	0.687	0.312	0.428	0.530	0.346	0.142
Change at Age 10-12	11.935	0.664	0.329	0.484	0.515	0.276	0.191
Change at Age 13-15	12.145	0.763	0.268	0.381	0.413	0.206	0.127
Change at Age 16-18	11.375	0.566	0.482	0.451	0.451	0.273	0.211
All the others	11.986	0.751	0.202	0.411	0.474	0.218	0.102
Number of Observation	7,654			7,847			

Table 5: OLS Estimation for Children's Outcome Variables –Education Outcomes

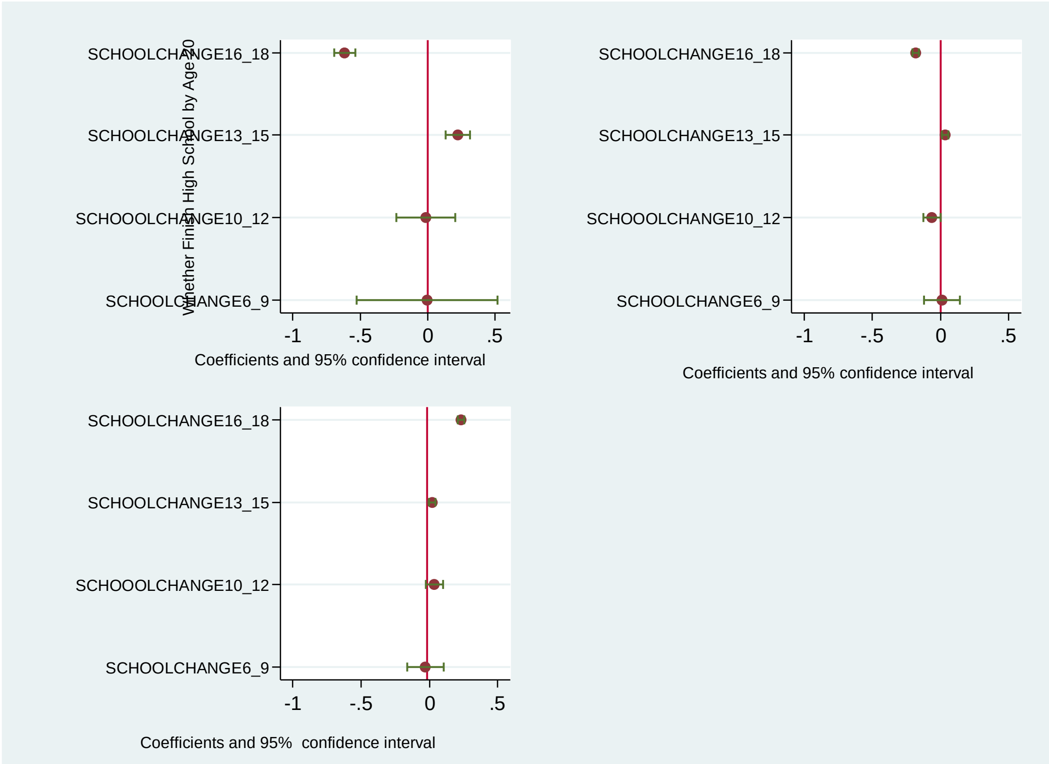
	Highest Education	Finish High School by Age 20	Ever Repeated Grade by Age 20
Female	0.410***	0.0794***	-0.0962***
	(0.0343)	(0.00925)	(0.00938)
Black	0.0544	0.00280	0.0614***
	(0.0477)	(0.0129)	(0.0130)
Hispanic	-0.182***	-0.0312**	0.0400***
	(0.0499)	(0.0133)	(0.0132)
Birth Order	-0.173***	-0.0522***	0.0318**
	(0.0462)	(0.0131)	(0.0139)
NO. of Children	-0.0282	0.00639	-0.00824
	(0.0350)	(0.0101)	(0.0101)
Mother less than 19 when Give Birth	-0.247***	-0.0545***	0.0580***
	(0.0571)	(0.0160)	(0.0162)
Mo. High School	-1.118***	-0.163***	0.124***
	(0.0652)	(0.0146)	(0.0180)
Mo. Some College	-0.460***	-0.0368***	0.0213
	(0.0645)	(0.0141)	(0.0176)
With Parents at Age 6	0.158	0.00845	-0.0478
	(0.108)	(0.0308)	(0.0319)
With Parents at Age 12	0.466***	0.109***	-0.0459
	(0.108)	(0.0309)	(0.0320)
Log of Relative Income	0.190***	0.0510***	-0.0407***
	(0.0156)	(0.00409)	(0.00420)
School Change at 6_9	-0.00543	0.0105	-0.0288
	(0.267)	(0.0667)	(0.0686)
School Change at 10_12	-0.0126	-0.0630**	0.0369
	(0.111)	(0.0320)	(0.0317)
School Change at 13_15	0.221***	0.0341***	0.0212*
	(0.0455)	(0.0118)	(0.0115)
School Change at 16_18	-0.616***	-0.181***	0.233***
	(0.0394)	(0.0122)	(0.0125)
_cons	11.61***	0.569***	0.406***
	(0.131)	(0.0334)	(0.0354)
N	7654	7654	7654

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

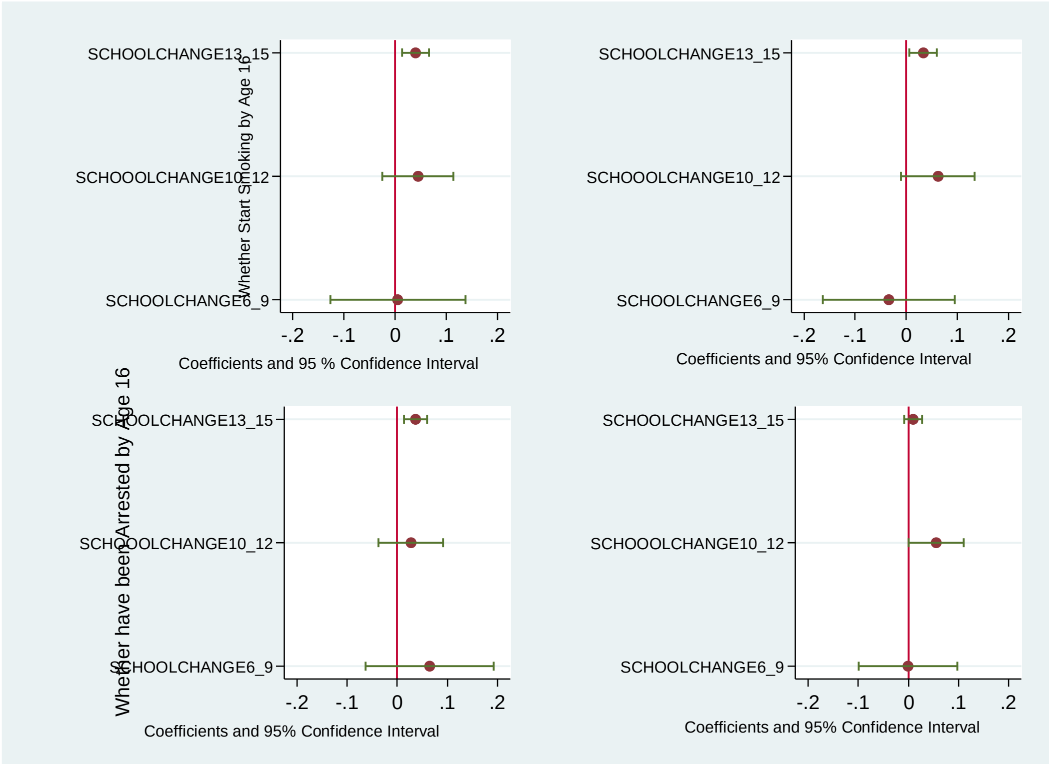
Table 6: OLS Estimation for Children's Outcome Variables –Youth Risky Behavior

	Start Smoking by Age 16	Start Drinking by Age 16	Start Using Drug by Age 16	Ever Being Arrested by Age 16
Female	-0.00800	-0.0212**	-0.0265***	-0.0845***
	(0.0106)	(0.0106)	(0.00893)	(0.00742)
Black	-0.198***	-0.144***	-0.0944***	-0.0252**
	(0.0144)	(0.0142)	(0.0120)	(0.0101)
Hispanic	-0.104***	-0.0478***	-0.0318**	-0.00295
	(0.0155)	(0.0149)	(0.0130)	(0.0105)
Birth Order	0.0478***	0.0383***	0.0313***	0.0275**
	(0.0136)	(0.0143)	(0.0119)	(0.0108)
NO. of Children	-0.0230**	-0.0383***	-0.0195*	0.00291
	(0.0116)	(0.0115)	(0.0101)	(0.00786)
Mother less than 19 when Give Birth	-0.00411	-0.00889	-0.0149	0.0294**
	(0.0169)	(0.0163)	(0.0143)	(0.0130)
Mo. High School	0.109***	0.0435*	0.0245	0.0328**
	(0.0236)	(0.0235)	(0.0185)	(0.0137)
Mo. Some College	0.0972***	0.0597**	0.0362*	0.0184
	(0.0236)	(0.0236)	(0.0185)	(0.0136)
With Parents at Age 6	0.0375	0.0475	0.0356	-0.0165
	(0.0318)	(0.0314)	(0.0290)	(0.0242)
With Parents at Age 12	-0.147***	-0.133***	-0.124***	-0.0582**
	(0.0322)	(0.0318)	(0.0292)	(0.0242)
Log of Relative Income	-0.00434	0.0189***	-0.00709	-0.0164***
	(0.00528)	(0.00515)	(0.00445)	(0.00378)
School Change at 6_9	-0.0344	0.00491	0.0649	-0.000927
	(0.0659)	(0.0672)	(0.0652)	(0.0500)
School Change at 10_12	0.0617*	0.0441	0.0271	0.0556**
	(0.0365)	(0.0356)	(0.0328)	(0.0278)
School Change at 13_15	0.0327**	0.0395***	0.0361***	0.00954
	(0.0135)	(0.0134)	(0.0118)	(0.00897)
_cons	0.557***	0.586***	0.428***	0.214***
	(0.0439)	(0.0428)	(0.0366)	(0.0303)
N	7847	7847	7847	7847

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)



Graph 1: Point Estimates of the Coefficients and Confidence Intervals from OLS Regression
Education Outcomes



Graph 2: Point Estimates of the Coefficients and Confidence Intervals from OLS Regression -Youth Risky Behavior Outcomes

Table 7: OLS Estimation for Children's Outcome Variables
Education Outcomes by Gender

	Highest Education		Finish High School by Age 20		Ever Repeated Grade by Age 20	
	Male	Female	Male	Female	Male	Female
Black	-0.0902	0.196***	-0.0383**	0.0419**	0.0942***	0.0274
	(0.0689)	(0.0628)	(0.0192)	(0.0167)	(0.0193)	(0.0170)
Hispanic	-0.234***	-0.129*	-0.0622***	0.00168	0.0660***	0.0131
	(0.0674)	(0.0693)	(0.0187)	(0.0179)	(0.0196)	(0.0174)
Birth Order	-0.210***	-0.132**	-0.0541***	-0.0501***	0.0252	0.0399**
	(0.0702)	(0.0666)	(0.0197)	(0.0180)	(0.0202)	(0.0198)
NO. of Children	-0.0325	-0.0196	0.00492	0.00826	-0.0117	-0.00571
	(0.0476)	(0.0505)	(0.0141)	(0.0140)	(0.0145)	(0.0141)
Mother less than 19	-0.284***	-0.205**	-0.0581**	-0.0517**	0.0596**	0.0560**
when Give Birth	(0.0801)	(0.0804)	(0.0229)	(0.0222)	(0.0235)	(0.0221)
Mo. High School	-1.080***	-1.156***	-0.172***	-0.152***	0.133***	0.113***
	(0.0930)	(0.0834)	(0.0217)	(0.0184)	(0.0265)	(0.0226)
Mo. Some College	-0.442***	-0.467***	-0.0284	-0.0408**	0.0328	0.00644
	(0.0919)	(0.0819)	(0.0211)	(0.0173)	(0.0262)	(0.0216)
With Parents	0.113	0.182	-0.0223	0.0344	-0.0102	-0.0837**
at Age 6	(0.156)	(0.145)	(0.0470)	(0.0405)	(0.0476)	(0.0406)
With Parents	0.532***	0.419***	0.158***	0.0629	-0.0877*	-0.00475
at Age 12	(0.156)	(0.145)	(0.0470)	(0.0405)	(0.0478)	(0.0407)
Log of	0.156***	0.222***	0.0474***	0.0547***	-0.0394***	-0.0419***
Relative Income	(0.0220)	(0.0221)	(0.00597)	(0.00563)	(0.00632)	(0.00559)
School Change	0.180	-0.190	0.0237	0.00521	-0.0879	0.0354
at 6_9	(0.298)	(0.465)	(0.0983)	(0.0904)	(0.0942)	(0.0986)
School Change	0.0403	-0.0611	-0.0262	-0.102**	0.000697	0.0765*
at 10_12	(0.155)	(0.163)	(0.0441)	(0.0471)	(0.0468)	(0.0429)
School Change	0.223***	0.216***	0.0178	0.0484***	0.0275	0.0139
at 13_15	(0.0631)	(0.0649)	(0.0167)	(0.0163)	(0.0171)	(0.0155)
School Change	-0.582***	-0.646***	-0.184***	-0.177***	0.246***	0.217***
at 16_18	(0.0547)	(0.0562)	(0.0170)	(0.0174)	(0.0176)	(0.0176)
_cons	11.81***	11.82***	0.596***	0.618***	0.375***	0.345***
	(0.183)	(0.178)	(0.0477)	(0.0449)	(0.0511)	(0.0466)
N	3887	3767	3887	3767	3887	3767

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Table 8: OLS Estimation for Children's Outcome Variables
Youth Risky Behavior by Gender (1)

	Start Smoking by Age 16		Start Drinking by Age 16	
	Male	Female	Male	Female
Black	-0.167***	-0.230***	-0.158***	-0.129***
	(0.0203)	(0.0196)	(0.0200)	(0.0194)
Hispanic	-0.0861***	-0.122***	-0.0493**	-0.0461**
	(0.0209)	(0.0216)	(0.0206)	(0.0208)
Birth Order	0.0539***	0.0401**	0.0282	0.0495**
	(0.0209)	(0.0198)	(0.0208)	(0.0210)
NO. of Children	-0.00710	-0.0406**	-0.0141	-0.0638***
	(0.0161)	(0.0164)	(0.0156)	(0.0163)
Mother less than 19 when Give Birth	0.000338	-0.00907	0.0124	-0.0313
	(0.0238)	(0.0236)	(0.0233)	(0.0228)
Mo. High School	0.0943***	0.125***	0.0576*	0.0338
	(0.0316)	(0.0335)	(0.0322)	(0.0330)
Mo. Some College	0.0833***	0.112***	0.0718**	0.0505
	(0.0317)	(0.0331)	(0.0325)	(0.0330)
With Parents at Age 6	0.0766*	0.00343	0.0848*	0.00998
	(0.0447)	(0.0438)	(0.0460)	(0.0417)
With Parents at Age 12	-0.177***	-0.120***	-0.151***	-0.117***
	(0.0448)	(0.0446)	(0.0463)	(0.0424)
Log of Relative Income	-0.00338	-0.00575	0.0146**	0.0229***
	(0.00739)	(0.00743)	(0.00739)	(0.00709)
School Change at 6_9	-0.107	0.0435	-0.0214	0.0359
	(0.0891)	(0.0956)	(0.0903)	(0.0987)
School Change at 10_12	0.0421	0.0825	0.0116	0.0799
	(0.0502)	(0.0529)	(0.0487)	(0.0516)
School Change at 13_15	0.0147	0.0529***	0.0269	0.0510***
	(0.0189)	(0.0188)	(0.0186)	(0.0189)
_cons	0.539***	0.571***	0.581***	0.568***
	(0.0590)	(0.0622)	(0.0595)	(0.0594)
N	3997	3850	3997	3850

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Table 9: OLS Estimation for Children's Outcome Variables
Youth Risky Behavior by Gender (2)

	Start Using Drug by Age 16		Ever Been Arrested by Age 16	
	Male	Female	Male	Female
Black	-0.0628***	-0.123***	-0.00567	-0.0476***
	(0.0173)	(0.0157)	(0.0162)	(0.0117)
Hispanic	-0.0200	-0.0424**	0.0136	-0.0208*
	(0.0179)	(0.0179)	(0.0162)	(0.0126)
Birth Order	0.0239	0.0390**	0.0361**	0.0182
	(0.0178)	(0.0170)	(0.0176)	(0.0137)
NO. of Children	-0.0133	-0.0267*	0.00999	-0.00509
	(0.0145)	(0.0137)	(0.0125)	(0.00962)
Mother less than 19 when Give Birth	-0.0259	-0.00458	0.0410**	0.0178
	(0.0202)	(0.0196)	(0.0203)	(0.0159)
Mo. High School	-0.00481	0.0585**	0.0376*	0.0239
	(0.0269)	(0.0236)	(0.0201)	(0.0174)
Mo. Some College	-0.00969	0.0869***	0.0288	0.00285
	(0.0269)	(0.0237)	(0.0204)	(0.0168)
With Parents at Age 6	0.0746*	-0.000330	-0.0304	-0.000745
	(0.0423)	(0.0379)	(0.0366)	(0.0304)
With Parents at Age 12	-0.153***	-0.0967**	-0.0618*	-0.0547*
	(0.0423)	(0.0383)	(0.0366)	(0.0304)
Log of Relative Income	-0.00399	-0.0101*	-0.0173***	-0.0164***
	(0.00630)	(0.00613)	(0.00595)	(0.00477)
School Change at 6_9	-0.0122	0.153	-0.0336	0.0409
	(0.0818)	(0.100)	(0.0670)	(0.0728)
School Change at 10_12	0.0140	0.0400	0.0771*	0.0273
	(0.0459)	(0.0483)	(0.0427)	(0.0335)
School Change at 13_15	0.0310*	0.0441***	0.0111	0.00901
	(0.0168)	(0.0161)	(0.0143)	(0.0106)
_cons	0.454***	0.369***	0.187***	0.168***
	(0.0505)	(0.0497)	(0.0455)	(0.0378)
N	3997	3850	3997	3850

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 10: OLS Estimation for Children's Outcome Variables
Education Outcomes by Race (1)

	Highest Education			Finish High School by Age 20		
	Black	Hispanic	Other	Black	Hispanic	Other
Female	0.593***	0.412***	0.326***	0.136***	0.114***	0.0404***
	(0.0698)	(0.0775)	(0.0453)	(0.0197)	(0.0212)	(0.0119)
Birth Order	-0.169*	-0.170*	-0.184***	-0.0653**	-0.0536*	-0.0439**
	(0.0943)	(0.0957)	(0.0629)	(0.0257)	(0.0279)	(0.0179)
No. of Children	-0.0751	-0.0214	0.0242	0.00251	0.00150	0.0176
	(0.0664)	(0.0813)	(0.0472)	(0.0195)	(0.0236)	(0.0133)
Mother less than 19	-0.166*	-0.247**	-0.336***	-0.0371	-0.0596*	-0.0695***
when Give Birth	(0.0921)	(0.118)	(0.0924)	(0.0258)	(0.0338)	(0.0257)
Mo. High School	-1.246***	-1.249***	-1.075***	-0.178***	-0.182***	-0.152***
	(0.187)	(0.193)	(0.0750)	(0.0403)	(0.0374)	(0.0172)
Mo. Some College	-0.556***	-0.606***	-0.447***	-0.0394	-0.0333	-0.0426***
	(0.189)	(0.202)	(0.0726)	(0.0408)	(0.0395)	(0.0160)
With Parents	0.222	0.135	0.105	0.00951	0.0120	0.000530
at Age 6	(0.184)	(0.225)	(0.164)	(0.0537)	(0.0677)	(0.0452)
With Parents	0.166	0.395*	0.621***	0.0737	0.0839	0.136***
at Age 12	(0.193)	(0.221)	(0.163)	(0.0564)	(0.0667)	(0.0448)
Log of	0.264***	0.290***	0.114***	0.0657***	0.0719***	0.0349***
Relative Income	(0.0308)	(0.0369)	(0.0209)	(0.00823)	(0.0104)	(0.00527)
School Change	0.0549	0.436	-0.447	0.00137	0.0264	-0.0127
at 6_9	(0.454)	(0.294)	(0.478)	(0.117)	(0.142)	(0.100)
School Change	0.0518	0.154	-0.158	-0.0581	-0.0313	-0.0858*
at 10_12	(0.234)	(0.183)	(0.159)	(0.0637)	(0.0629)	(0.0443)
School Change	0.344***	0.349***	0.0917	0.0611**	0.0734**	0.000544
at 13_15	(0.0890)	(0.108)	(0.0604)	(0.0243)	(0.0289)	(0.0148)
School Change	-0.576***	-0.616***	-0.607***	-0.194***	-0.199***	-0.158***
at 16_18	(0.0773)	(0.0847)	(0.0547)	(0.0244)	(0.0260)	(0.0166)
_cons	11.31***	10.89***	12.08***	0.481***	0.400***	0.676***
	(0.288)	(0.312)	(0.170)	(0.0718)	(0.0771)	(0.0426)
N	1923	1604	4127	1923	1604	4127

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 11: OLS Estimation for Children's Outcome Variables
Education Outcomes by Race (2)

	Ever Repeated Grade by Age 20		
	Black	Hispanic	Other
Female	-0.138***	-0.123***	-0.0675***
	(0.0203)	(0.0220)	(0.0119)
Birth Order	0.0351	0.0882***	-0.000162
	(0.0291)	(0.0304)	(0.0178)
No. of Children	-0.00336	-0.0526**	0.00348
	(0.0195)	(0.0219)	(0.0141)
Mother less than 19 when Give Birth	0.0519*	0.0583*	0.0633**
	(0.0275)	(0.0326)	(0.0253)
Mo. High School	0.171***	0.0908	0.112***
	(0.0458)	(0.0744)	(0.0204)
Mo. Some College	0.0262	-0.00765	0.0303
	(0.0458)	(0.0759)	(0.0196)
With Parents at Age 6	-0.133**	-0.00145	0.00406
	(0.0569)	(0.0731)	(0.0443)
With Parents at Age 12	0.0626	-0.0571	-0.117***
	(0.0589)	(0.0730)	(0.0442)
Log of Relative Income	-0.0548***	-0.0599***	-0.0253***
	(0.00861)	(0.0105)	(0.00538)
School Change at 6_9	0.0624	-0.205*	0.0130
	(0.123)	(0.113)	(0.105)
School Change at 10_12	0.00935	0.0360	0.0458
	(0.0640)	(0.0622)	(0.0443)
School Change at 13_15	-0.0247	0.0674***	0.0286*
	(0.0251)	(0.0255)	(0.0148)
School Change at 16_18	0.228***	0.277***	0.209***
	(0.0243)	(0.0267)	(0.0173)
_cons	0.558***	0.512***	0.332***
	(0.0766)	(0.102)	(0.0444)
N	1923	1604	4127

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Table 12: OLS Estimation for Children's Outcome Variables
Youth Risky Behavior by Race (1)

	Start Smoking by Age 16			Start Drinking by Age 16		
	Black	Hispanic	Other	Black	Hispanic	Other
Female	-0.0472**	-0.0231	0.0128	-0.00005	-0.0335	-0.0272*
	(0.0206)	(0.0234)	(0.0146)	(0.0210)	(0.0236)	(0.0143)
Birth Order	0.0134	0.00898	0.0844***	0.0259	0.0212	0.0566***
	(0.0252)	(0.0284)	(0.0196)	(0.0269)	(0.0300)	(0.0204)
No. of Children	-0.00332	-0.0096	-0.0374**	-0.0325	-0.0292	-0.0423**
	(0.0200)	(0.0257)	(0.0171)	(0.0203)	(0.0246)	(0.0169)
Mother less than 19 when Give Birth	-0.0196	-0.0366	0.0350	-0.0440	0.00446	0.0173
	(0.0265)	(0.0358)	(0.0276)	(0.0271)	(0.0336)	(0.0256)
Mo. High School	0.0166	-0.143*	0.142***	-0.0589	-0.0557	0.0665**
	(0.0621)	(0.0851)	(0.0269)	(0.0627)	(0.0909)	(0.0265)
Mo. Some College	-0.0132	-0.0786	0.116***	-0.0335	0.00435	0.0676**
	(0.0621)	(0.0882)	(0.0265)	(0.0636)	(0.0935)	(0.0264)
With Parents at Age 6	0.0193	0.0307	0.0584	0.0486	0.0780	0.0319
	(0.0566)	(0.0727)	(0.0445)	(0.0520)	(0.0715)	(0.0466)
With Parents at Age 12	-0.0419	-0.116	-0.199***	-0.102*	-0.150**	-0.129***
	(0.0600)	(0.0725)	(0.0445)	(0.0556)	(0.0718)	(0.0463)
Log of Relative Income	-0.00801	-0.0017	-0.00564	0.0151	0.0299***	0.0124*
	(0.00988)	(0.0114)	(0.00740)	(0.00947)	(0.0111)	(0.00734)
School Change at 6_9	0.0480	-0.169	-0.00418	0.141	-0.0962	-0.0502
	(0.115)	(0.114)	(0.108)	(0.116)	(0.137)	(0.104)
School Change at 10_12	0.0405	-0.0048	0.107**	0.153**	0.0363	-0.0173
	(0.0697)	(0.0772)	(0.0512)	(0.0736)	(0.0679)	(0.0496)
School Change at 13_15	-0.00285	0.0186	0.0528***	0.0436*	0.00184	0.0434**
	(0.0254)	(0.0293)	(0.0191)	(0.0262)	(0.0297)	(0.0185)
_cons	0.449***	0.711***	0.535***	0.466***	0.599***	0.639***
	(0.0938)	(0.111)	(0.0586)	(0.0908)	(0.113)	(0.0577)
N	1956	1630	4261	1956	1630	4261

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Table 13: OLS Estimation for Children's Outcome Variables
Youth Risky Behavior by Race (2)

	Start Using Drug by Age 16			Ever Been Arrested by Age 16		
	Black	Hispanic	Other	Black	Hispanic	Other
Female	-0.0579***	-0.0291	-0.0115	-0.120***	-0.102***	-0.0619***
	(0.0168)	(0.0195)	(0.0125)	(0.0156)	(0.0170)	(0.00969)
Birth Order	0.0144	0.0220	0.0454**	0.0196	0.0113	0.0401***
	(0.0197)	(0.0257)	(0.0180)	(0.0213)	(0.0231)	(0.0148)
No. of Children	-0.00930	-0.0315	-0.0193	0.0203	0.00922	-0.0131
	(0.0172)	(0.0207)	(0.0155)	(0.0154)	(0.0179)	(0.0103)
Mother less than 19	-0.0530**	-0.0125	0.0194	0.0233	0.0328	0.0367*
when Give Birth	(0.0210)	(0.0284)	(0.0251)	(0.0205)	(0.0279)	(0.0209)
Mo. High School	0.0279	0.0178	0.0224	0.00853	0.0749*	0.0287*
	(0.0460)	(0.0691)	(0.0215)	(0.0405)	(0.0404)	(0.0156)
Mo. Some College	0.0388	0.0864	0.0208	-0.00394	0.0335	0.0221
	(0.0471)	(0.0718)	(0.0211)	(0.0404)	(0.0423)	(0.0154)
With Parents at	0.0258	0.0643	0.0309	-0.0495	-0.0112	0.00873
Age 6	(0.0469)	(0.0676)	(0.0436)	(0.0385)	(0.0561)	(0.0365)
With Parents at	-0.0995**	-0.119*	-0.132***	-0.0189	-0.0587	-0.0851**
Age 12	(0.0488)	(0.0673)	(0.0433)	(0.0402)	(0.0557)	(0.0360)
Log of Relative	-0.0113	0.00325	-0.0111*	-0.0187***	-0.0188**	-0.0132***
Income	(0.00744)	(0.0101)	(0.0066)	(0.00724)	(0.00897)	(0.00510)
School Change	0.224*	-0.191**	0.0864	0.00154	-0.144***	0.0761
at 6_9	(0.114)	(0.0788)	(0.108)	(0.0829)	(0.0279)	(0.0907)
School Change	0.0138	0.0729	0.00177	0.0981*	-0.0200	0.0711*
at 10_12	(0.0613)	(0.0659)	(0.0482)	(0.0578)	(0.0466)	(0.0392)
School Change	0.0194	0.0320	0.044***	-0.00037	-0.00537	0.0184
at 13_15	(0.0218)	(0.0264)	(0.0167)	(0.0180)	(0.0212)	(0.0119)
_cons	0.363***	0.348***	0.448***	0.206***	0.225***	0.194***
	(0.0710)	(0.0933)	(0.0513)	(0.0676)	(0.0689)	(0.0392)
N	1956	1630	4261	1956	1630	4261

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 14: Sibling Fixed Effects Estimation for Children's Outcome Variables
Education Outcomes

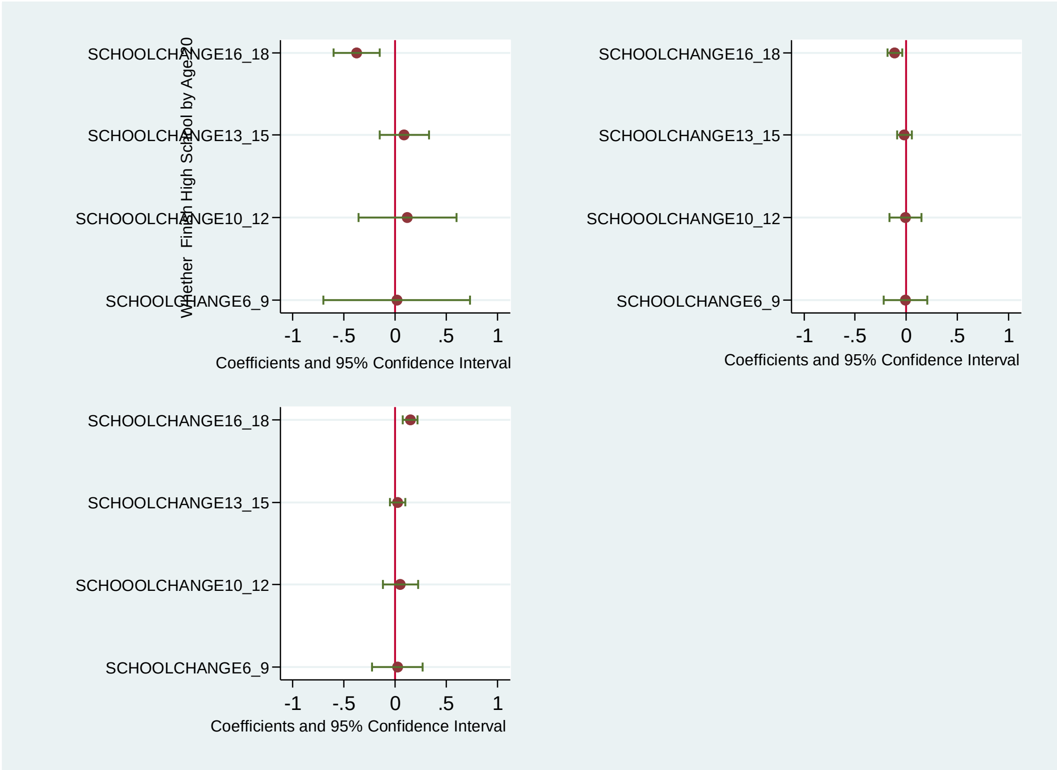
	Highest Education	Finish High School by Age 20	Ever Repeated Grade by Age 20
Birth Order	-0.184*	-0.0446	0.0318
	(0.110)	(0.0324)	(0.0375)
Mother less than 19 when Give Birth	-0.0648	-0.0131	-0.00727
	(0.195)	(0.0573)	(0.0658)
With Parents at Age 6	0.0958	-0.0268	-0.102
	(0.268)	(0.0820)	(0.0913)
With Parents at Age 12	0.0453	0.0208	-0.000376
	(0.312)	(0.100)	(0.117)
School Change at 6_9	0.0147	-0.00753	0.0221
	(0.366)	(0.109)	(0.126)
School Change at 10_12	0.118	-0.0103	0.0508
	(0.244)	(0.0799)	(0.0879)
School Change at 13_15	0.0857	-0.0203	0.0239
	(0.123)	(0.0370)	(0.0385)
School Change at 16_18	-0.377***	-0.112***	0.147***
	(0.115)	(0.0357)	(0.0370)
_cons	12.09***	0.834***	0.283***
	(0.186)	(0.0594)	(0.0684)
N	3194	3194	3194

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

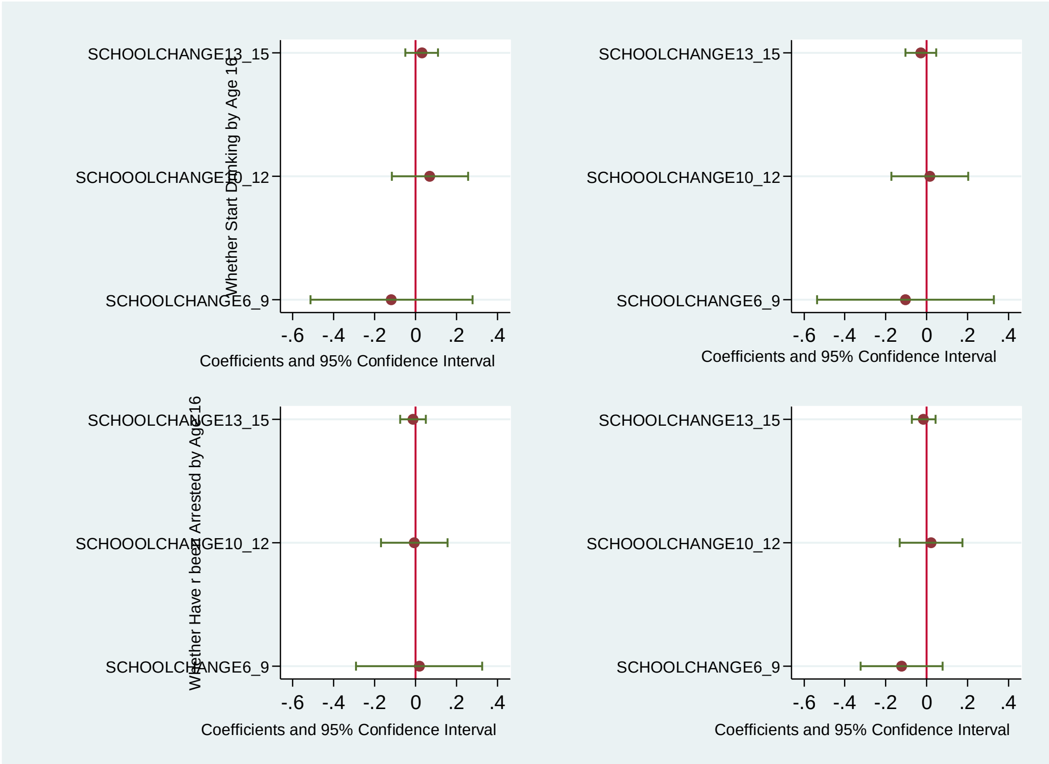
Table 15: Sibling Fixed Effects Estimation for Children's Outcome Variables
Youth Risky Behavior

	Start Smoking by Age 16	Start Drinking by Age 16	Start Using Drug by Age 16	Ever Being Arrested by Age 16
Birth Order	0.0128	0.00246	0.0189	0.00893
	(0.0316)	(0.0349)	(0.0292)	(0.0287)
Mother less than 19 when Give Birth	-0.0162	-0.00213	-0.0124	-0.00405
	(0.0581)	(0.0612)	(0.0477)	(0.0480)
With Parents at Age 6	-0.00113	-0.0268	-0.00413	-0.0600
	(0.0822)	(0.0778)	(0.0629)	(0.0570)
With Parents at Age 12	-0.0361	-0.000432	0.0744	0.0797
	(0.0954)	(0.102)	(0.0792)	(0.0740)
School Change at 6_9	-0.105	-0.119	0.0167	-0.123
	(0.220)	(0.202)	(0.157)	(0.103)
School Change at 10_12	0.0145	0.0693	-0.00660	0.0209
	(0.0951)	(0.0950)	(0.0825)	(0.0783)
School Change at 13_15	-0.0292	0.0294	-0.0142	-0.0163
	(0.0380)	(0.0410)	(0.0322)	(0.0296)
_cons	0.544^{***}	0.593^{***}	0.298^{***}	0.0914^{*}
	(0.0565)	(0.0596)	(0.0534)	(0.0492)
N	3302	3302	3302	3302

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)



Graph 3: Point Estimates of the Coefficients and Confidence Interval from Sibling Fixed Regression -Education Outcomes



Graph 4: Point Estimates of the Coefficients and Confidence Interval from Sibling Fixed Regression -Youth Risky Behavior Outcomes

Table 16: Sibling Fixed Effects Estimation for Education Outcome by Gender

	Highest Education		Finish High School by Age 20		Ever Repeated Grade by Age 20	
	Male	Female	Male	Male	Female	Male
Birth Order	-0.364	-0.121	-0.103	-0.0363	0.0323	0.0828
	(0.289)	(0.251)	(0.0777)	(0.0799)	(0.0922)	(0.0914)
Mother less than 19	0.250	0.188	0.0842	-0.0144	-0.0928	0.102
when Give Birth	(0.387)	(0.534)	(0.141)	(0.131)	(0.170)	(0.168)
With Parents at Age 6	-0.180	0.196	-0.121	0.0616	-0.142	-0.144
	(0.565)	(0.678)	(0.200)	(0.169)	(0.180)	(0.217)
With Parents at Age 12	0.295	-0.0754	0.134	-0.0908	-0.117	0.0576
	(0.557)	(0.798)	(0.230)	(0.221)	(0.264)	(0.295)
School Change at 6_9	0.239	0.368	0.0375	-0.0200	-0.0811	0.0448
	(0.609)	(1.677)	(0.0606)	(0.614)	(0.0751)	(0.608)
School Change at 10_12	0.750	-0.0092	0.179	0.00427	-0.00349	0.135
	(0.797)	(0.415)	(0.209)	(0.175)	(0.244)	(0.245)
School Change at 13_15	-0.181	0.0642	-0.104	-0.0131	0.0727	0.0107
	(0.273)	(0.353)	(0.0909)	(0.102)	(0.101)	(0.107)
School Change at 16_18	-0.317	-0.309	-0.0341	-0.0798	0.155*	0.0937
	(0.269)	(0.317)	(0.0870)	(0.0956)	(0.0929)	(0.102)
_cons	12.03***	12.15***	0.800***	0.855***	0.428***	0.178
	(0.432)	(0.435)	(0.153)	(0.136)	(0.162)	(0.167)
N	1655	1539	1655	1539	1655	1539

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 17: Sibling Fixed Effects Estimation for Risky Behavior by Gender (1)

	Start Smoking by Age 16		Start Drinking by Age 16	
	Male	Female	Male	Female
Birth Order	0.0337	0.0320	0.0539	0.00212
	(0.0826)	(0.0737)	(0.0855)	(0.0855)
Mother less than 19 when Give Birth	-0.138	-0.0107	-0.0488	0.0532
	(0.153)	(0.149)	(0.162)	(0.142)
With Parents at Age 6	0.148	-0.0943	0.0824	-0.0918
	(0.208)	(0.164)	(0.252)	(0.142)
With Parents at Age 12	-0.170	0.0601	-0.203	-0.0499
	(0.238)	(0.198)	(0.316)	(0.209)
School Change at 6_9	-0.151	0.0242	-0.176	0.186
	(0.498)	(0.525)	(0.381)	(0.631)
School Change at 10_12	-0.200	-0.122	-0.253	0.116
	(0.257)	(0.206)	(0.293)	(0.227)
School Change at 13_15	-0.0891	0.0109	0.0105	0.0276
	(0.0889)	(0.0936)	(0.108)	(0.103)
_cons	0.638^{***}	0.462^{***}	0.676^{***}	0.534^{***}
	(0.136)	(0.123)	(0.139)	(0.153)
N	1714	1588	1714	1588

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 18: Sibling Fixed Effects Estimation for Risky Behavior by Gender (2)

	Start Using Drug by Age 16		Ever Been Arrested by Age 16	
	Male	Female	Male	Female
Birth Order	0.0156	0.0269	0.00583	0.00525
	(0.0765)	(0.0726)	(0.0853)	(0.0591)
Mother less than 19 when Give Birth	-0.0708	-0.0347	-0.0321	0.0137
	(0.133)	(0.118)	(0.139)	(0.0964)
With Parents at Age 6	0.117	0.000387	-0.131	0.0194
	(0.162)	(0.113)	(0.152)	(0.107)
With Parents at Age 12	0.0315	-0.0111	-0.0486	0.119
	(0.186)	(0.177)	(0.183)	(0.142)
School Change at 6_9	0.164	-0.242	-0.109	-0.223
	(0.282)	(0.608)	(0.262)	(0.361)
School Change at 10_12	0.0708	-0.126	-0.0217	-0.0205
	(0.254)	(0.203)	(0.287)	(0.120)
School Change at 13_15	0.0303	-0.0348	0.0305	-0.0225
	(0.0842)	(0.0843)	(0.0881)	(0.0576)
_cons	0.293^{**}	0.283^{**}	0.198	-0.0137
	(0.129)	(0.139)	(0.142)	(0.0998)
N	1714	1588	1714	1588

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 19: Sibling Fixed Effects Estimation for Education by Race (1)

	Highest Education			Finish High School by Age 20		
	Black	Hispanic	Neither	Black	Hispanic	Neither
Birth Order	-0.315	0.115	-0.264*	-0.0815	-0.00284	-0.0504
	(0.199)	(0.240)	(0.159)	(0.0560)	(0.0771)	(0.0479)
Mother less than 19 when Give Birth	0.169	-0.217	-0.0309	0.105	-0.0898	-0.0345
	(0.319)	(0.379)	(0.335)	(0.0966)	(0.117)	(0.0978)
With Parents at Age 6	0.194	-0.151	0.0904	-0.0103	-0.0273	-0.0542
	(0.477)	(0.493)	(0.446)	(0.149)	(0.145)	(0.140)
With Parents at Age 12	-0.166	0.0163	0.240	0.0383	-0.0282	0.0821
	(0.550)	(0.616)	(0.499)	(0.185)	(0.162)	(0.179)
School Change at 6_9	0.117	-1.276***	0.324	0.0212	-0.492	0.111
	(0.576)	(0.218)	(0.489)	(0.111)	(0.410)	(0.144)
School Change at 10_12	-0.677	0.343	0.208	-0.172	0.000593	-0.00727
	(0.443)	(0.777)	(0.267)	(0.143)	(0.252)	(0.0891)
School Change at 13_15	0.414	-0.0471	-0.0919	0.0833	-0.0846	-0.0519
	(0.257)	(0.246)	(0.166)	(0.0765)	(0.0803)	(0.0501)
School Change at 16_18	-0.152	-0.520**	-0.413**	-0.0778	-0.195***	-0.0849
	(0.215)	(0.238)	(0.171)	(0.0696)	(0.0680)	(0.0540)
_cons	11.45***	12.05***	12.45***	0.609***	0.903***	0.917***
	(0.324)	(0.361)	(0.319)	(0.0967)	(0.124)	(0.101)
N	773	750	1671	773	750	1671

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 20: Sibling Fixed Effects Estimation for Education by Race (2)

	Ever Repeated Grade by Age 20		
	Black	Hispanic	Neither
Birth Order	0.0572	0.0400	-0.0179
	(0.0770)	(0.0791)	(0.0515)
Mother less than 19	-0.0239	-0.115	0.0532
when Give Birth	(0.128)	(0.132)	(0.0971)
With Parents at Age 6	-0.232	-0.0899	-0.00895
	(0.183)	(0.214)	(0.108)
With Parents at Age 12	0.166	0.0290	-0.151
	(0.231)	(0.218)	(0.161)
School Change at 6_9	0.305	-0.0500	-0.115
	(0.258)	(0.194)	(0.150)
School Change at 10_12	0.0274	-0.0100	0.106
	(0.202)	(0.267)	(0.0980)
School Change at 13_15	-0.0510	0.0653	0.0672
	(0.0849)	(0.0701)	(0.0547)
School Change at 16_18	0.116^{*}	0.234^{***}	0.118^{**}
	(0.0695)	(0.0795)	(0.0523)
_cons	0.451^{***}	0.142	0.286^{***}
	(0.120)	(0.148)	(0.107)
N	773	750	1671

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 21: Sibling Fixed Effects Estimation for Risky Behavior by Race (1)

	Start Smoking by Age 16			Start Drinking by Age 16		
	Black	Hispanic	Neither	Black	Hispanic	Neither
Birth Order	0.0166	-0.0661	0.0692	0.00794	0.0124	0.00445
	(0.0593)	(0.0668)	(0.0451)	(0.0582)	(0.0747)	(0.0541)
Mother less than 19 when Give Birth	-0.0816	-0.0589	0.0585	-0.248**	0.0935	0.106
	(0.112)	(0.103)	(0.0944)	(0.110)	(0.0988)	(0.0972)
With Parents at Age 6	-0.139	0.0492	0.0881	-0.158	-0.0335	0.0993
	(0.137)	(0.185)	(0.124)	(0.102)	(0.181)	(0.134)
With Parents at Age 12	0.0964	-0.0513	-0.135	-0.0352	0.118	-0.0820
	(0.175)	(0.171)	(0.150)	(0.156)	(0.191)	(0.177)
School Change at 6_9	-0.0216	0.378	-0.305	-0.436**	0.284	-0.149
	(0.393)	(0.472)	(0.270)	(0.204)	(0.504)	(0.310)
School Change at 10_12	0.169	-0.0209	0.0281	0.0399	0.000786	0.145
	(0.218)	(0.209)	(0.123)	(0.230)	(0.179)	(0.129)
School Change at 13_15	-0.0753	-0.00695	-0.0179	0.0373	-0.00164	0.0263
	(0.0747)	(0.0684)	(0.0610)	(0.0815)	(0.0763)	(0.0631)
_cons	0.435***	0.569***	0.549***	0.528***	0.543***	0.619***
	(0.0862)	(0.135)	(0.0916)	(0.0835)	(0.135)	(0.102)
N	779	766	1757	779	766	1757

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 22: Sibling Fixed Effects Estimation for Risky Behavior by Race (2)

	Start Using Drug by Age 16			Ever Been Arrested by Age 16		
	Black	Hispanic	Neither	Black	Hispanic	Neither
Birth Order	0.0330	0.0379	0.0161	-0.00372	-0.0722	0.0611
	(0.0513)	(0.0602)	(0.0499)	(0.0564)	(0.0640)	(0.0393)
Mother less than 19	-0.0851	-0.0249	0.0557	-0.110	0.0303	0.0390
when Give Birth	(0.0789)	(0.0802)	(0.0863)	(0.0784)	(0.0951)	(0.0764)
With Parents at Age 6	-0.0851	-0.0134	0.0578	-0.0935	-0.0837	-0.0371
	(0.0925)	(0.149)	(0.103)	(0.0723)	(0.140)	(0.0970)
With Parents at Age 12	0.0430	0.156	0.0349	0.00813	-0.0222	0.182
	(0.123)	(0.134)	(0.142)	(0.116)	(0.159)	(0.119)
School Change at 6_9	-0.0467	0.356	-0.0426	-0.0539	0.0559	-0.109
	(0.404)	(0.527)	(0.176)	(0.158)	(0.0474)	(0.138)
School Change at 10_12	0.145	-0.0330	-0.0449	0.111	-0.0872	0.0544
	(0.193)	(0.210)	(0.105)	(0.212)	(0.152)	(0.0971)
School Change at 13_15	-0.0320	-0.00164	-0.0110	-0.0495	-0.0121	0.00516
	(0.0564)	(0.0618)	(0.0526)	(0.0571)	(0.0611)	(0.0431)
_cons	0.290***	0.235**	0.298***	0.156**	0.231**	-0.0565
	(0.0760)	(0.102)	(0.103)	(0.0715)	(0.104)	(0.0915)
N	779	766	1757	779	766	1757

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Table 23: OLS Estimation for Children's Outcome Variables –Education

	Highest Education	Finish High School by Age 20	Ever Repeated Grade by Age 20
Female	0.418***	0.0810***	-0.0969***
	(0.0344)	(0.00927)	(0.00939)
Black	0.0308	-0.00304	0.0658***
	(0.0478)	(0.0129)	(0.0131)
Hispanic	-0.186***	-0.0317**	0.0398***
	(0.0501)	(0.0133)	(0.0132)
Birth Order	-0.192***	-0.0574***	0.0367***
	(0.0466)	(0.0130)	(0.0139)
NO. of Children	-0.0186	0.00895	-0.0104
	(0.0353)	(0.0101)	(0.0102)
Mother less than 19 when Give Birth	-0.254***	-0.0560***	0.0588***
	(0.0575)	(0.0161)	(0.0163)
Mo. High School	-1.147***	-0.169***	0.127***
	(0.0651)	(0.0145)	(0.0178)
Mo. Some College	-0.474***	-0.0397***	0.0228
	(0.0643)	(0.0140)	(0.0173)
With Parents at Age 6	0.116	-0.000864	-0.0425
	(0.109)	(0.0310)	(0.0320)
With Parents at Age 12	0.497***	0.115***	-0.0480
	(0.109)	(0.0310)	(0.0321)
Log of Relative Income	0.194***	0.0517***	-0.0410***
	(0.0158)	(0.00412)	(0.00419)
No. of School Change at 6_9	-0.0870	-0.0122	-0.00897
	(0.204)	(0.0562)	(0.0586)
No. of School Change at 10_12	-0.0327	-0.0531*	0.0361
	(0.0999)	(0.0272)	(0.0280)
No. of School Change at 13_15	-0.0933***	-0.0303***	0.0399***
	(0.0235)	(0.00651)	(0.00650)
No. of School Change at 16_18	-0.322***	-0.100***	0.126***
	(0.0227)	(0.00748)	(0.00756)
_cons	11.79***	0.609***	0.396***
	(0.129)	(0.0331)	(0.0349)
N	7654	7654	7654

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Table 24: OLS Estimation for Children's Outcome Variables –Youth Risky Behavior

	Start Smoking by Age 16	Start Drinking by Age 16	Start Using Drug by Age 16	Ever Being Arrested by Age 16
Female	-0.00739	-0.0205*	-0.0258***	-0.0844***
	(0.0106)	(0.0106)	(0.00891)	(0.00739)
Black	-0.195***	-0.143***	-0.0919***	-0.0215**
	(0.0144)	(0.0142)	(0.0120)	(0.0100)
Hispanic	-0.103***	-0.0472***	-0.0308**	-0.00163
	(0.0155)	(0.0149)	(0.0129)	(0.0104)
Birth Order	0.0487***	0.0384***	0.0320***	0.0288***
	(0.0136)	(0.0143)	(0.0119)	(0.0109)
NO. of Children	-0.0233**	-0.0382***	-0.0198**	0.00227
	(0.0116)	(0.0115)	(0.0101)	(0.00783)
Mother less than 19 when Give Birth	-0.00647	-0.0108	-0.0172	0.0274**
	(0.0169)	(0.0164)	(0.0142)	(0.0128)
Mo. High School	0.107***	0.0415*	0.0227	0.0326**
	(0.0236)	(0.0235)	(0.0185)	(0.0136)
Mo. Some College	0.0960***	0.0586**	0.0350*	0.0178
	(0.0235)	(0.0236)	(0.0185)	(0.0136)
With Parents at Age 6	0.0398	0.0470	0.0374	-0.0123
	(0.0316)	(0.0314)	(0.0287)	(0.0239)
With Parents at Age 12	-0.143***	-0.130***	-0.120***	-0.0568**
	(0.0320)	(0.0317)	(0.0289)	(0.0240)
Log of Relative Income	-0.00340	0.0199***	-0.00608	-0.0161***
	(0.00526)	(0.00514)	(0.00443)	(0.00375)
No. of School Change at 6_9	0.00484	-0.0134	0.0651	-0.00304
	(0.0577)	(0.0588)	(0.0593)	(0.0448)
No. of School Change at 10_12	0.0561*	0.0340	0.0119	0.0442*
	(0.0295)	(0.0294)	(0.0281)	(0.0251)
No. of School Change at 13_15	0.0378***	0.0251***	0.0381***	0.0364***
	(0.00659)	(0.00657)	(0.00579)	(0.00555)
_cons	0.542***	0.587***	0.416***	0.188***
	(0.0435)	(0.0426)	(0.0362)	(0.0301)
N	7847	7847	7847	7847

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Table 25: Sibling Fixed Effects Estimation for Children's Outcome Variables-Education

	Highest Education	Finish High School by Age 20	Ever Repeated Grade by Age 20
Birth Order	-0.185*	-0.0438	0.0303
	(0.111)	(0.0323)	(0.0373)
Mother less than 19 when Give Birth	-0.0700	-0.0145	-0.00260
	(0.194)	(0.0570)	(0.0652)
With Parents at Age 6	0.0653	-0.0339	-0.0911
	(0.269)	(0.0819)	(0.0911)
With Parents at Age 12	0.0795	0.0315	-0.0122
	(0.314)	(0.0999)	(0.115)
No. of School Change at 6_9	-0.227	-0.0448	0.118
	(0.280)	(0.0762)	(0.0997)
No. of School Change at 10_12	0.0692	-0.0207	0.0506
	(0.193)	(0.0643)	(0.0698)
No. of School Change at 13_15	-0.0765	-0.0317	0.0558***
	(0.0683)	(0.0202)	(0.0206)
No. of School Change at 16_18	-0.196***	-0.0729***	0.0752***
	(0.0666)	(0.0214)	(0.0226)
_cons	12.20***	0.846***	0.258***
	(0.179)	(0.0555)	(0.0624)
N	3194	3194	3194

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Table 26: Sibling Fixed Effects Estimation for Children's Outcome Variables

Youth Risky Behavior

	Start Smoking by Age 16	Start Drinking by Age 16	Start Using Drug by Age 16	Ever Being Arrested by Age 16
Birth Order	0.0124	0.00132	0.0180	0.00830
	(0.0317)	(0.0349)	(0.0291)	(0.0289)
Mother less than 19	-0.0114	-0.00385	-0.0113	-0.0000897
when Give Birth	(0.0580)	(0.0613)	(0.0478)	(0.0477)
With Parents	-0.000664	-0.0271	-0.00118	-0.0589
at Age 6	(0.0819)	(0.0772)	(0.0626)	(0.0572)
With Parents	-0.0358	-0.000205	0.0723	0.0797
at Age 12	(0.0954)	(0.102)	(0.0791)	(0.0746)
No. of School Change	0.0226	-0.142	-0.0163	-0.0857
at 6_9	(0.158)	(0.120)	(0.146)	(0.0634)
No. of School Change	0.0115	0.0558	-0.0612	0.0254
at 10_12	(0.0741)	(0.0753)	(0.0821)	(0.0607)
No. of School Change	0.0122	0.00221	0.00434	0.0156
at 13_15	(0.0181)	(0.0204)	(0.0172)	(0.0169)
_cons	0.512^{***}	0.613^{***}	0.288^{***}	0.0663
	(0.0533)	(0.0564)	(0.0501)	(0.0465)
N	3302	3302	3302	3302

(Note: Birth Cohorts are included in the regression. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1)

Chapter 3

An Intergeneration Analysis of Quantity and Quality Model Using the NLSY79

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Abstract: In this paper, we examine the impact of family size on maternal health outcomes by exploiting the exogenous change in family size using contraceptive failure as instrument variable while controlling for the unobserved genetic heterogeneity at the family level. Using data from the National Longitudinal Survey of Youth 1979, we find that having additional number of child would significantly decrease mother's mental health measurement and increase the probability of depression at age 40, but it has none significant impact on mother's physical health. We don't find the negative impacts of family size on mother's BMI suggested by results from developing countries using fixed effects model. This result indicates a different story for developed country where resources are more affluent that mother's nutrition intake and long run physical health would be taken care of while the mental pressure and time investment of raising children would be a major health concern for mothers in the long run.

***Keywords**:* Maternal health, Quantity-quality tradeoff

JEL classification: O15, J13, I10

I. Introduction

There has been extensive research investigating the question that if there is tradeoff between children's quantity and quality within household. The empirical evidence presented by Becker (Becker and Tomes, 1976; Becker and Lewis, 1973) that income has grown and fertility declines over the last several decades implies the existence of children's quantity and quality tradeoff (QQ tradeoff for short). QQ tradeoff is often modeled as arising from parental preferences for equal levels of quality across children combined with a binding budget constraint (Rosenzweig and Wolpin, 1980). Numerous empirical studies have attempted to test the QQ tradeoff and either confirm the prediction by observing a negative correlation between family size and child quality or find no such correlation (Angrist, Lavy and Schlosser, 2005; Caceres-Delpiano, 2006; Conley and Glauber, 2006; Anh, Knodel, Lam, and Friedman, 1998; Van Wey, 1999; Knodel, Havanon, and Sittitrai, 1990; Knodel and Wongsith 1991). Qian (2006) even reports a positive causal influence of family size on children's education in China.

The QQ tradeoff exists as families face the binding budget constraint where previous researches focus on how the limited resources are allocated among children. However, the intra-family resource allocation should not be limited to among children themselves but also between parents and children. Whether resources are allocated in a way that not only guarantees the healthy development of the children but also maintains mother's physical and mental fitness is neglected by researchers and yet is a major concern for this paper. There exits some research for developing countries (Wu and Li, 2010) where the probability of being underweight would increase with the number of children as potentially there is an intergeneration tradeoff between children's "quantity" and mother's "quality" working through the budget constraint mechanism, that is, having more children decreases the resource allocated to mothers and affects their health outcomes. Few studies have explored the maternal health return from giving birth to and raising children in the household in the long run. The intergenerational QQ tradeoff, the one between child number and parental mental and physical health, might be different from the traditional QQ tradeoff. It remains unclear how the number of children would affect parents' especially mother's health outcomes in the long run. Given the constraint of family income budget and the increasing cost of raising children, resources available for mothers are directly affected by the number of children they have. While for developing countries where family budget constraint is often binding making this intergeneration tradeoff more prominent, in this paper, we are interested in investigating the impact of family size on parental health especially on mother's long run health outcome using U.S. data to see if we have a different story for developed countries.

In order to identify the impact of household size on maternal health outcomes in the long run, we need to tackle two potential problems. Firstly, the number of children may be endogenous to mother's health conditions. Healthier mothers are able to bear more children, yet their opportunity cost of childbearing is also higher. One important method dealing with endogeneity is to find proper instrument variable for family size to isolate the causal effect of family size on outcome variables. Rosenzweig and Wolpin, in their pioneering study in 1980 propose using the natural occurrence of twins as instrumental variable for number of children in the household and this method has been widely used by researchers to study the classical QQ tradeoff question. Angrist (2006) uses both twin births and gender composition of the first two children in a multiple birth context as instrumental variables for family size. Both of those instrumental variables require relative large sample of twins and are subjects to critics as weak instrument for family size (Daouli, Demoussis, Giannakopoulos, 2009).

Hotz, McElroy and Sanders (1997) use the incidence of miscarriage as a type of fertility shock for women. If miscarriages are largely exogenous, women who have a miscarriage should on average have children later and have a smaller family size than those who don't. However, there are limitations with this instrument. Firstly, reported miscarriages are uncommon. In the NLSY79, just over 10% of women report having a miscarriage with their first pregnancy. And a miscarriage usually results only in modest delay, not avoidance altogether. At least 80% of women reporting a first miscarriage are later observed to have a child, usually a two year delay (Ellwood,Wilde and Batchelder, 2004). Thus this variable could at best be used to explore the impact of delayed birth timing on outcome variables rather as an instrument for family size. Alternatively, we could use "undesired" or "unexpected" pregnancies. For example, Millar (2003) proposes using as an instrument whether the woman reports that she was using contraception at the time of conception. Therefore, contraceptive failure will generate variations in family size if the female has a live birth. Contraceptive use (and thus the odds of failure) as an instrument could also be problematic as it is likely to depend upon the competence and knowledge of the woman. The NLSY also includes prospective questions about when people expect to become pregnant. The questions are asked prior to pregnancy. One might count as truly "undesired" pregnancy only when the woman do not expect to become pregnant and who is using contraceptive measure at the time of pregnancy. For this paper, we combine the information of contraceptive use together with the "expectancy of being pregnant at the time of contraception" from the NLSY79 and generate "undesired pregnancy" as the instrument variable for family size. In this work, we also try other instrument like miscarriage as a type of fertility shock. The estimates it produces are generally in the "right" direction but not statistically significant as it is an instrument for birth timing rather than birth number.

The second difficulty lies in the unobserved biological influence of childbearing and family background on mother's health in the long run. In this paper, we control heterogeneity at the family level and use family fixed effects together with instrument variable strategy to examine the impacts of number of children on mother's health status in the long run and see if the intergenerational quantity-quality tradeoff exists.

This paper exploits the exogenous change in family size using contraceptive failure as an instrument on various health measures for women by age 40. Using data from the National Longitudinal Survey of Youth 1979, we find that having additional number of child would significantly decrease mother's mental health measurement and increase the probability of depression at age 40, but has none significant negative impact on mother's physical health. We don't find the negative impacts of family size on mother's BMI suggested by results from developing countries using fixed effects model. This result indicates a different story for developed country where resources are more affluent that mother's nutrition intake and long run physical health would be taken care of while the mental pressure and time investment of raising children would be a major health concern for mothers in the long run.

The remainder of this paper is organized as follows. Section 2 reviews the literatures on "undesired pregnancies" and introduces the contribution of this paper. Section 3 presents a theoretical model on intergeneration QQ tradeoff between mother's health and family size. Section 4 proposes the empirical identification/estimation strategy. Section 5 describes the data sets. Section 6 reports the results from the estimation and robustness check. Section 7 briefly summarizes the conclusion.

II. Literature

Most existing literature that relates household size to maternal health concentrates on how the maternal mortality rate is affected. Generally, it is found that women with higher birth parity and shorter birth spacing face a higher risk in pregnancy. Winikoff (1983), EcKholm and Newland (1977), Royston and Armstrong (1989) find evidence of high mortality rate for mothers with high parity births. Boerma (1987) and Chen et al. (1974) find that maternal mortality rate is significantly reduced under family planning in developing countries. In impoverished settings, not only the children but also the mother is likely to be malnourished since she has less energy to care for the demands of an increasingly large family (Graves, 1976; Scrimshaw and Scrimshaw, 1990). While morality rate at birth may be a big concern for developing countries, obstetrical risk is much lower for women in developed countries like U.S. Few studies have examined the long-term impact of children on mothers' overall fitness for developed countries.

As the number of children is largely endogenous to parents' decision, we use "undesired pregnancy" as an instrument for family size. In the United States, nearly one half of all pregnancies are unintended, 42 percent of these end in abortion (Finer and Henshaw, 2006). In 1995, the Committee on Unintended Pregnancy of the Institute of Medicine concludes that "the consequences of unintended pregnancy are serious, imposing appreciable burdens on children, women, men, and families" (Brown and Eisenberg, 1995). Gipson, Koenig and Hindin (2008) provide a comprehensive review on studies that assessed the relationship between pregnancy intention and health outcomes. Other parental situations related to health could be exacerbated by the occurrence of an unintended pregnancy (for example, intimate partner violence), whereas other conditions (for example, anxiety and depression) may arise as a direct consequence of the unintended pregnancy. Contraceptive failure that lead the unintended pregnancy and the variation in family size serve as a potential instrumental variable for number of children in our paper.

In this paper, we are more interested in how family size affects mother's health status rather than father. Mother's health condition is more closely connected with children mainly through two channels: 1) Maternal time devoted to the birth of children. It is the biological influence associated with childbearing. On one hand, women gain weight during their pregnancy. On the other hand, children consume a mother's energy and nutrition during the nursing period. 2) Time spending at home raising children at the expense of labor market work or leisure. For developing countries, both channels mostly work through the economic budget constraint. Mothers face competition on resources within the family. The more children they have, the fewer resources left for themselves. Other things being equal, mothers in larger families are likely to have a smaller investment on their health, and the pressure of caring for kids could also deteriorate their health status. If this is true, we expect to see a negative impact of number of children on maternal health outcomes under impoverished setting. As for developed countries like U.S., maternal health return from children may vary substantially as mother's nutrition and basic physical health could be guaranteed while the mental stress and pressure of raising children in the long run may exacerbate.

The intergenerational QQ tradeoff between child number and mother's long run health could be different from the traditional quantity quality tradeoff for children, but we could apply the same logic here. This paper adopts similar idea but applies the idea to a different framework which greatly extends the application of the traditional quantity and quality model. By looking into some empirical evidence present in some developing countries that mothers' health status is negatively related to number of children in the household, we raise the question that if there is any tradeoff in terms of mother's quality (physical and mental health) versus the number of children in the household using the US data. More specifically, we want to investigate if mother's health condition would be negatively affected by the number of children she raised.

One major contribution for this paper is to investigate QQ tradeoff under an intergenerational framework. Previous research emphasizes QQ tradeoff among children and doesn't take the feedback from children to parents, especially to mothers into account. By evaluating those effects from children to parents, we can develop a better understanding about how the fertility cycle works for the entire family over the long run. Under the standard QQ tradeoff, parents are treated homogeneously. However, since women bear the fertility responsibility, it would make more sense to think about how this fertility process affect women in ways that are different from men, especially in the long run.

This paper also has good measures of health rather than self reported health conditions. The NLSY79 provides relatively extensive measures for women's health. These health measures have not been fully explored in previous research. I am using BMI as general measure for women's physical condition and other measures are composite measures which are survey based and doctor evaluated. Due to the panel structure of the NLSY79, I am also be able to access to the complete contraceptive history, education, working history, wage, family income for all my samples. This is something that other data sets are not comparable. In terms of estimation strategy and the choice of instrumental variables, I use the contraceptive failure as an instrument for number of children in the household and controlled the individual heterogeneity at the family level.

III. Theoretical Framework

1) Health Production Function

This paper makes an effort to provide an exact link between the theoretical model and the specification of the empirical model. In the subsequent section we use the results from this preliminary analysis to specify and interpret feasible empirical specifications of health production functions that are consistent with a theoretical model of a household utility optimization. We first make the following assumptions to simplify the theoretical model.

1) Mother is the only agent in the household.

2) Children are homogeneous. Quality of children in the household are standardized as 1, therefore the number of children with unit quality enters in the utility function.

3) Assume mother's utility function has the following form:

$$U(X_i, Y_j, N, H), i = 1, \ldots, n; j = n + 1, \ldots m \quad (1)$$

Mother's utility is affected by n X good, m-n Y good that would affect mother's health as well as provide direct utility (such as gym membership for regular exercise, smoking, drinking, maternal nutrition food or supplement, etc).

4) The production of mother's health can be described by the production function

$$H = \Gamma(Y_j, I_k, N, \mu), k = m + 1, \ldots, r \quad (2)$$

Where the r-m I_k are health inputs which do not augment utility other than through their effects of H (e.g., the use of medical care) and μ represents family specific health endowments known to the family but not controlled by them, for example, genetic traits or environmental factors.

5) The budget constraint for the household in terms of r goods plus children (as normal good) has the following form:

$$F = \sum_t Z_t \, p_t + wN \quad t = 1, \ldots r \quad (3)$$

F represents the household income. p_t is exogenous price $Z = X \cup Y \cup I$. The price for each child w would be the opportunity cost of mother raising children instead of working.

6) The household reduced form demand functions for the r goods, including the r - n health inputs, derived from the maximization of (1) subject to (2) and (3), are

$$Z_t = S_t(p, w, F, \mu),\ N = S_t(p, w, F, \mu) \quad \text{t=1,…,r} \tag{4}$$

Where Z_t is the demand function for r goods and N is the demand function for children. The reduced form health equation has the following:

$$H = \Psi(\ p, w, F, \mu)$$

Due to the availability of data for r goods, empirically a lot of people estimate the following health equation:

$$H = \Theta(\ N, Y_m, p_l, w, F, \mu), l = 1, \dots, m-1, m+1, \dots, r \tag{5}$$

Where N is the number of children, $Y_m$is some health related input (e.g. as gym membership for regular exercise, smoking, drinking behavior), p_l is the price for all the other products. Those items are regressed on some kind of health measure. This could be problematic for two things: 1) N (number of children) is endogenously decided by the family, therefore the regression coefficients would be biased. 2) μ represents health heterogeneity. If not properly addressed it could also generate bias. To address the issues above, we use instrument variable for number of children and use family fixed effects to control for potential health heterogeneity at the household level.

2) Empirical Model

The estimation of mother's health function would have the following form:

$$\begin{aligned} \text{Health Measure}_{i,j} &= \alpha_1 BirthYear_{i,j} + \alpha_2 Smoking_{i,j} + \alpha_3 Drinking_{i,j} + \alpha_4 Exercising_{i,j} \\ &+ \sum_{i=1}^{m} \beta_m X_{m,i,j} \quad + \gamma NumChild_{i,j} + \mu_j + \varepsilon_{i,j} \end{aligned}$$

(6)

here $\text{Health Measure}_{i,j}$ represents mother i's physical or mental health in family j. We control for individual's birth cohort to capture any time trend on individual's health measure. We also control for individual i's health behavior such as whether the mother smokes regularly and drinks alcohol regularly and does exercise regularly[13]. X_m represent individual characteristics such as ethnicity, gender, mother's education, family income and community characteristics such as whether lives in urban area or rural area. μ is the unobserved health endowment effects. ϵ would be random error which follows i.i.d distribution.

The NLSY 79 has a rich selection of individual health measures to choose from. It records individual weight and height on an annual basis from 1979 (biannual after 1994) up to 2010. We use them to construct the Body Mass Index for individual samples. It also has subjective and objective health measures both physical and mental for each sample at age 40 and age 50. Depending upon the nature of the data, we apply fixed effects model for panel data sets and the two stage least square method (2SLS) plus family fixed effects model for the cross sectional data sets.

Firstly, we have retained a panel data structure on individual's BMI from 1979 -2008. Therefore fixed effects model is proposed to estimate the above equation. μ and other time constant variables are excluded from the fixed effects model. By including only time varying variables such as number of children, age, age squared, education in years, marital status, whether the individual has any child under 6 at home, whether the individual is currently working and

[13] The NLSY79 recorded questions on "whether the individual smoke daily?" for survey years in 1992, 1994,1998,2008,2010. We define the individual as a regular smoker if he smokes daily at age 40 or 41. For drinking behavior, we define individual "who has 6 or more drinks on one occasion during the last 30 days" at age 40 or 41 as an active alcohol user in our sample. From the questions of the NLSY 79, we also defined individual "who does regular exercise at least once per week "at age 40 or 41 as maintaining a regular exercise behavior.

whether living in SMSA area or urban/rural area. We should be able to identify the effects of having additional child on individual's BMI.

Secondly, we would like to briefly introduce the other types of health measure in the NLSY 79. SF-12 scale is a 12-question health survey designed by John Ware of the New England Medical Center Hospital. It is designed to provide a measure of the respondents' mental and physical health irrespective of their proclivity to use formal health services. CES-D-7 is provided by Center for Epidemiological Studies Depression Scale. Those measures are only available when individual reaches age 40 or 50. We choose SF12 and CES-D-7 as health measures when individual reaches age 40 and build a cross sectional data upon the information. These measures cover physical health as well as mental health, therefore provide more scientific benchmark for comparison among all of our samples.

We would take the following procedure to estimate model (6).

a) Number of Children is endogenous decided. We can conduct a 2SLS procedure or IV procedure.

i) Contraceptive failure [14] is chosen to instrument for number of children in the household. Therefore the first step of estimation would be estimating the following:

$$Number\ of\ Children = \beta_0 + \beta_1 Contraceptive\ Failure + \epsilon \qquad (7)$$

ii) Get the fitted value for $Number\ of\ Children$, then we plug in the fitted value in our main regression (6). We control for birth cohort, ethnicity, education, AFQT score, whether ever been married, family income, whether currently working at age 40, whether the individual is career

[14] The NLSY 79 records the contraceptive use and birth history from 1982 to 2010 .It has asked questions regarding the pregnancy expectation for each birth. By combine the contraceptive use history and pregnancy expectation, we construct instrument variable "contraceptive failure" for number of children. It is defined as the individual use contraceptive measure but end up with a live birth and also the individual indicate that particular birth is unwanted before it happens.

oriented[15], whether individual actively exercises at age 40, whether individual is a regular smoker and alcohol user.

b) To further control for the health heterogeneity, we include family fixed effects together with 2SLS (IV) procedure in the regression.

IV. Data

Data for this study come from the National Longitudinal Survey of Youth 1979. The NLSY is a national probability sample of young adults who were between ages 14 and 21 in 1979, the first year of the survey (Center for Human Resources 1994). The respondents have been interviewed yearly since that time. The sample originally comprised 12,686 youths, approximately half of whom are women. The retention rate for the survey has been extremely high, as of 1993, it is 91% of the eligible population. The longitudinal design features of the NLSY make it particularly suited to our research objectives. It allows us to relate information about pregnancy intention collected during pregnancy or shortly after birth to various health measures. In contrast to most previous research, we differentiate between unwanted[16] and mistimed births in our empirical analysis. The NLSY also contains an extremely rich set of controls. It offers extensive longitudinal information about the respondents, including their family background, education and labor market experiences, income, marital history, cognitive ability and achievement (i.e., Armed Services Vocational Aptitude Battery) and so on. Besides, the NLSY 79 also has rich information on health measures on individual's weight, height and physical and mental health status at age 40.

1) Summary Statistics

Table 1: Approximately Here

[15] Career Oriented is a dummy variable defined as individual received college and college above education and have at least 10 years of full time working experience before age 40.
[16] Contraceptive failure information is combined with the expectancy of having child before getting pregnant to reflect the true willingness of having child.

Table 1 describes sample characteristics by gender. From the summary statistics of Table 1, we can see that male and female sample preserve similar demographic characteristics comparing to the whole sample. Female sample has slightly poorer health measure than male sample as we can see from the descriptive statistics.

2) Dependent Variables

Graph 1- 4: Approximately Here

The health measure numbers are coded in a way that the larger numerical value indicates better health condition and we compare the demeaned health measures by number of children in the household. The above graphs trigger my interest in investigating the question that whether women's health measure would decrease with the increase of family size. For all the health measures we have, it consistently pointes to the fact that there is a potential negative relationship between the numbers of children in the household and the average health measures provide.

Graph 5-6: Approximately Here

Graph 5-6 show the BMI change by age and by number of children for male sample and female sample. BMI is increasing by age and also slightly increasing by number of children which presents different empirical evidence from developing countries where mother's BMI is decreasing with household size.

VII. Estimation Results

1) Fixed Effects on BMI

Table 2-3: Approximately Here

Table 2 displays the fixed effects results on BMI by gender. Fixed effects model removes the effects of time invariant variables such as gender, ethnicity, and family background on outcome variables. We include time varying variables such as age and age squared to capture if there is any u shape impact from age on BMI and other variables as log of the wage income, marital status, whether there is any child under 6 in the household, whether individual is currently working and some community variables such as whether lives in urban area or whether lives in SMSA area. From the regression results in table 2, we can see that having another child will increase BMI for both gender for samples. Moreover, we define a dummy variable "overweight" (BMI greater than 25) and find that having additional child will also increase the probability of being overweight for male and female samples. Fixed effect results resonate with the graph 5-6 we present above where BMI is increasing by number of children.

2) First Stage OLS Regression

Table 4: First Stage Regression- Failure of Contraceptive Protection

As number of children is endogenous to mother's health status, we use contraceptive failure during pregnancy to instrument number of children in the household. For the first stage regression, we regress the number of children by age 40 on failure of contraceptive use and see how the well contraceptive failure predicts number of children in the household. Since we have women's fertility information from 1979 to 2010, we can gather the contraceptive use history up to age 40 where most women have finished their fertility cycle. First stage estimation results indicate contraceptive failure predicts 0.841 more children and it seems to be a valid instrument.

3) 2SLS with Fixed Effects

Table 5: 2SLS with Fixed Effects Model

We plug in the fitted value for number of children from the first state regression and also include family fixed effects, the results suggest that having another child will make mother's health condition worse and some of the results for mental health are statistically significant. The results suggest having another child will decrease women's mental health and increase depression symptoms at age 40. Besides, low AFQT score is associated with reduced health measure, and comparing with never married samples ever married ones have higher health measures in our regression. Different health behaviors often have different impacts on health measure. As we expect, regular exercise tends to increase health measure at age 40 while smoking and drinking would reduce the health status by age 20.

V. Conclusions

To conclude, from the fixed effects model we find that using the NLSY79 data, women's BMI is increasing with number of children ever born and having additional child is associated with increased possibility of being overweight. From the 2SLS family fixed effects regression, we find significant negative impact of having additional child on women's health measures of mental health and depression symptoms, but not on physical health measures. The regression results suggest a different QQ tradeoff story for mothers in developed countries. Nutrition deprivation would not be a concern for mother with multiple children, but the mental pressure of raising children may playing a role in lowering mother's overall health in the long run.

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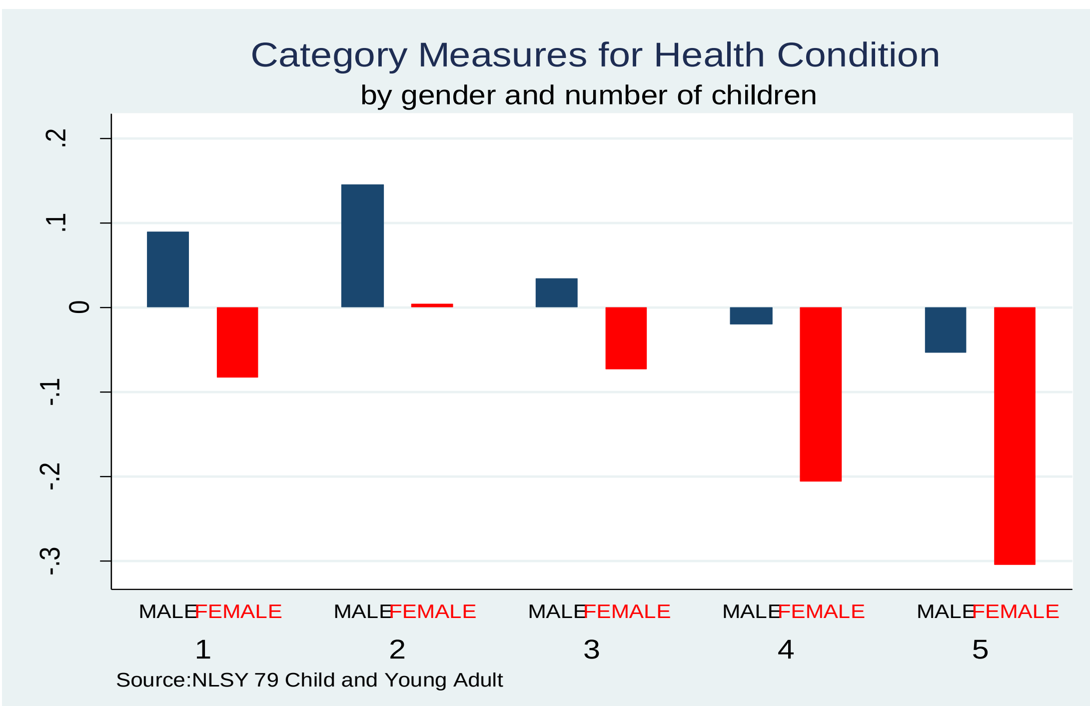
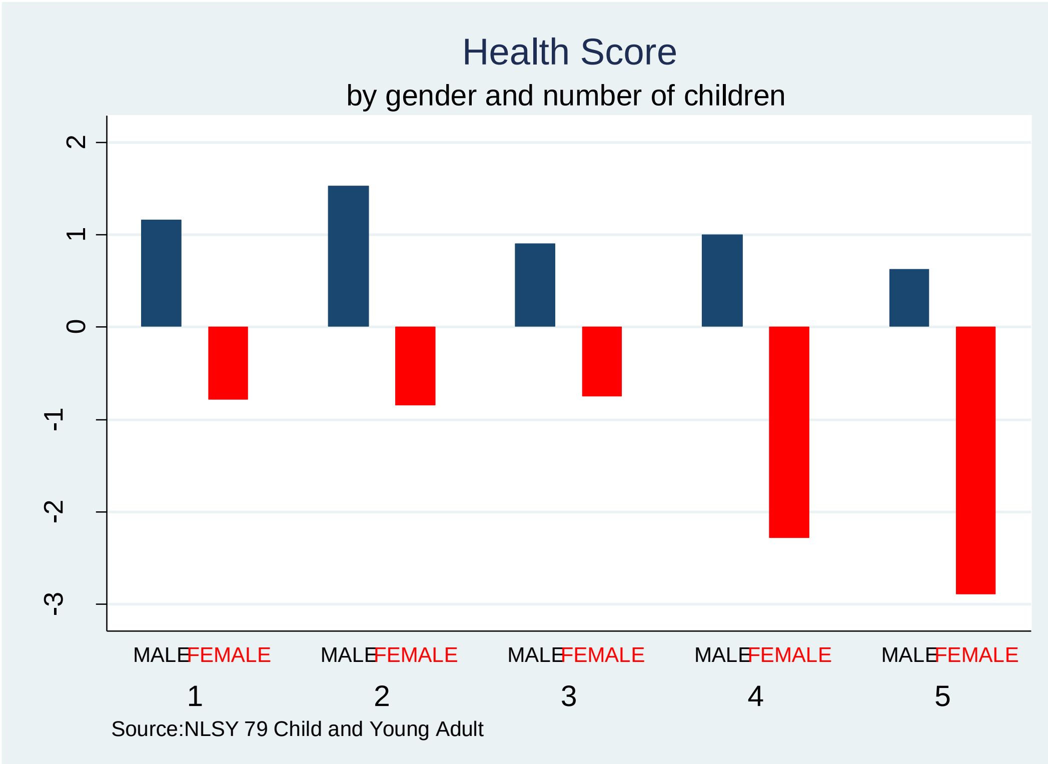
Tables and Graphs

Table 1: Summary Statistics

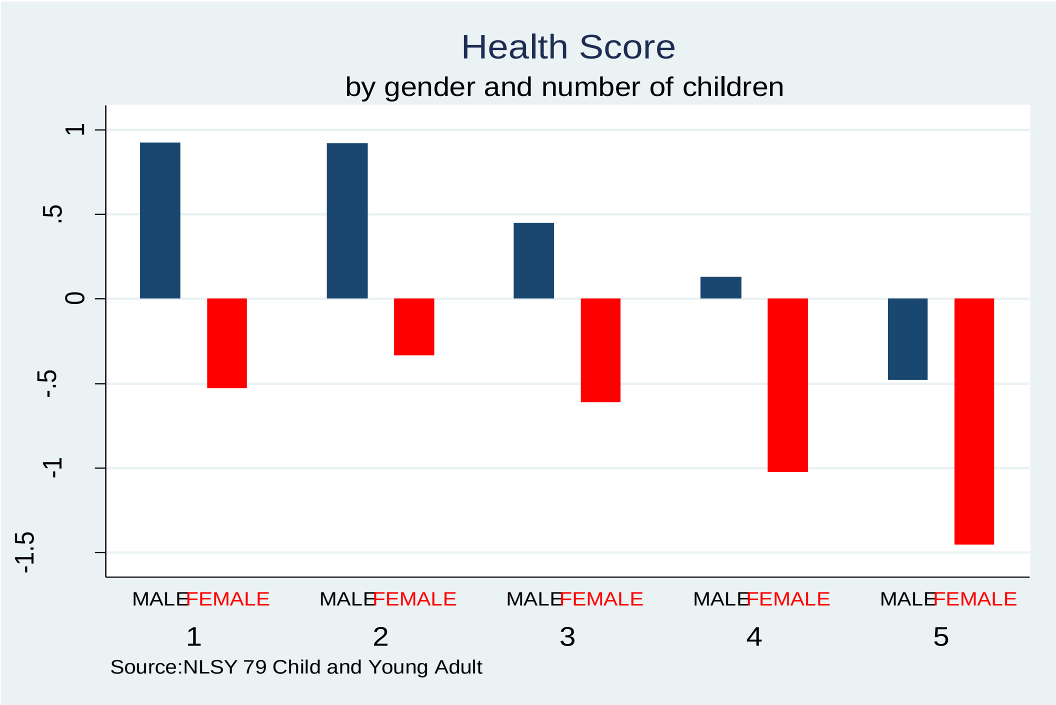
	All Sample		Male		Female	
	Mean	S.E.	Mean	S.E.	Mean	S.E.
Hispanic	0.19	0.001	0.19	0.002	0.19	0.002
Black	0.31	0.002	0.31	0.002	0.31	0.002
Other	0.50	0.002	0.50	0.002	0.50	0.002
Number of Children	2.01	0.005	1.95	0.008	2.07	0.007
BMI	24.75	0.016	25.21	0.020	24.32	0.026
Score12_physical	52.00	0.028	52.58	0.036	51.46	0.041
Score12_mental	53.02	0.028	54.14	0.037	51.97	0.042
Score7_depression	17.66	0.014	18.17	0.019	17.18	0.021
Highest Education	13.21	0.008	13.09	0.012	13.32	0.012
Currently Married	0.43	0.002	0.39	0.002	0.46	0.002
Ever Married	0.79	0.001	0.76	0.002	0.81	0.002
Live in SMSA area_	0.78	0.001	0.77	0.002	0.79	0.002
Live in Urban Area	0.81	0.001	0.80	0.002	0.81	0.002
AFQT Score	39.21	0.098	40.12	0.147	38.36	0.132



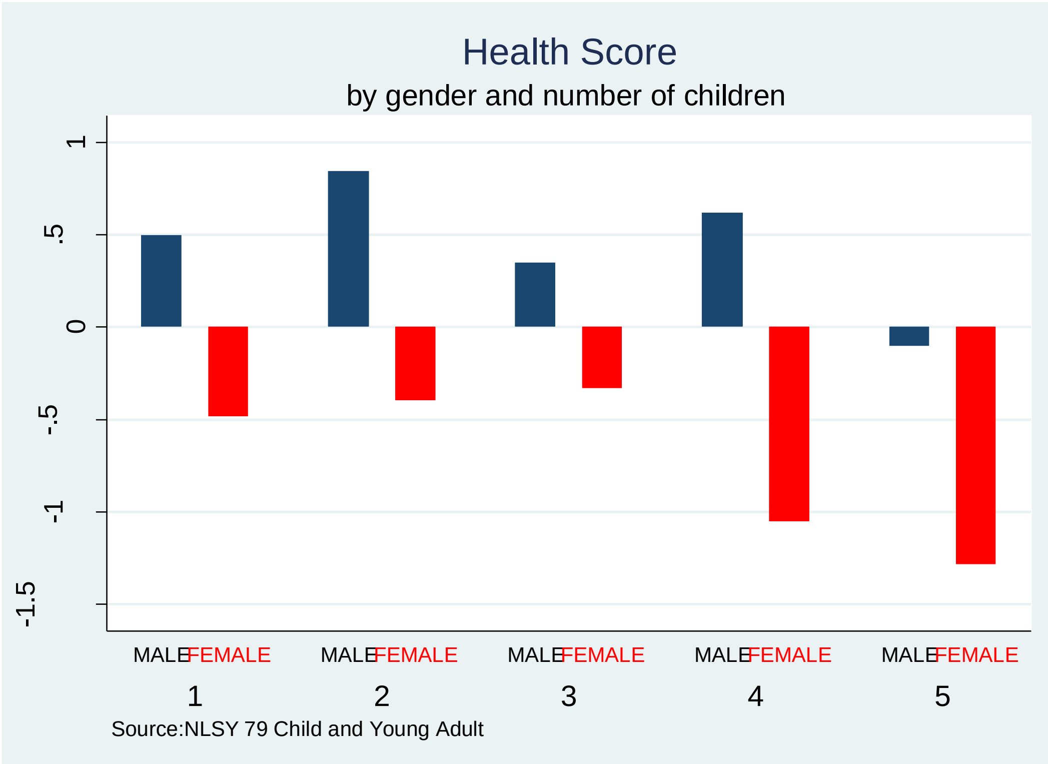
Graph 1: Gender Differences for General Health



Graph 2: Gender Differences for General Health Score12-Mental



Graph 3: Gender Differences for General Health Score12-Physical



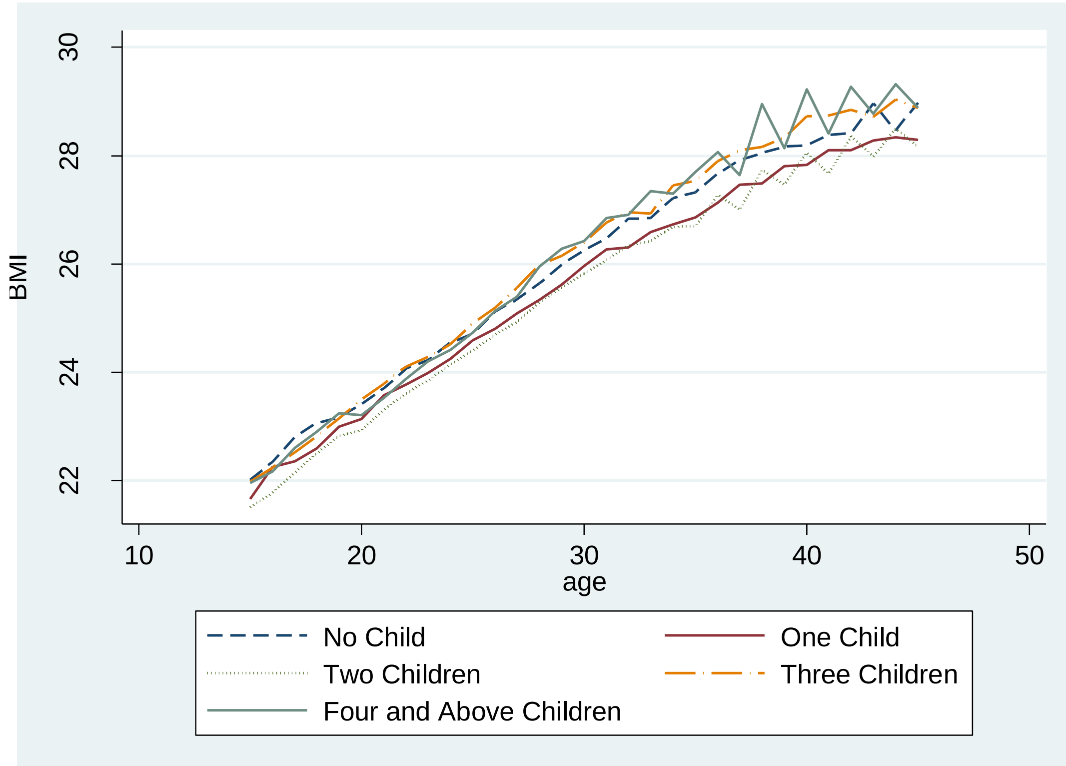
Graph 4: Gender Differences for General Health Score17- Depression Measure

Table 2: Fixed Effects Results for BMI

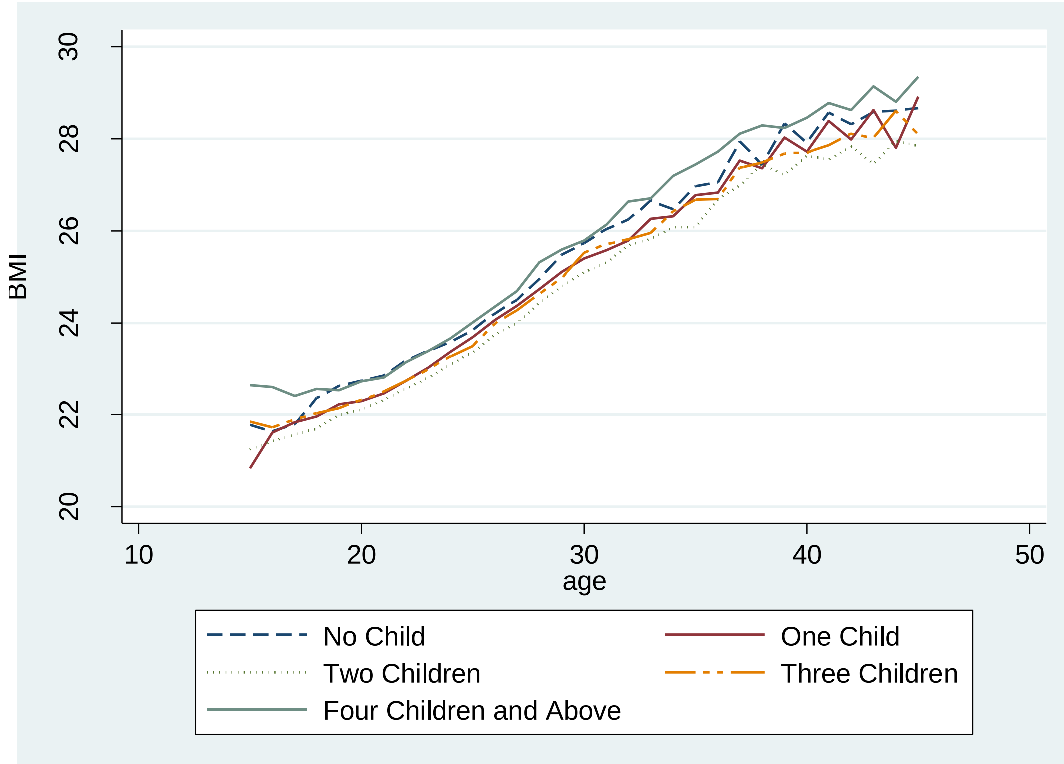
	Male	Female	All Sample
No. of Children	0.112***	0.143***	0.136***
	(0.0196)	(0.0278)	(0.0169)
Age	0.247***	0.103	0.181***
	(0.0511)	(0.0657)	(0.0420)
Age*Age	0.000514	0.00399***	0.00216***
	(0.000943)	(0.00121)	(0.000774)
Education	-0.116***	-0.112***	-0.112***
	(0.0171)	(0.0241)	(0.0147)
Log(Wage)	0.00887**	0.00410	0.00383
	(0.00432)	(0.00510)	(0.00337)
Married	0.250***	0.339***	0.286***
	(0.0294)	(0.0341)	(0.0226)
Child under 6 at Home	-0.0532*	0.0303	-0.0216
	(0.0321)	(0.0351)	(0.0237)
Work	0.0512*	-0.130***	-0.0538**
	(0.0287)	(0.0344)	(0.0227)
Urban	-0.114**	0.0103	-0.0536
	(0.0495)	(0.0667)	(0.0416)
SMSA	-0.0821*	-0.109*	-0.0982**
	(0.0475)	(0.0632)	(0.0397)
Constant	19.54***	19.85***	19.63***
	(0.683)	(0.889)	(0.564)
N	41400	43875	85275

Table 3: Fixed Effect Results for Being Overweight by Gender

	Male	Female	All Sample
No. of Children	0.0162***	0.0186***	0.0160***
	(0.00341)	(0.00348)	(0.00242)
Age	0.0335***	0.0196**	0.0263***
	(0.00888)	(0.00822)	(0.00604)
Age*Age	-0.0000575	0.0000808	0.0000138
	(0.000164)	(0.000152)	(0.000111)
Education	-0.00751**	-0.00710**	-0.00706***
	(0.00297)	(0.00301)	(0.00211)
Log(Wage)	0.000590	0.000550	0.000533
	(0.000750)	(0.000638)	(0.000485)
Married	0.0384***	0.0363***	0.0365***
	(0.00510)	(0.00426)	(0.00325)
Child under 6 at Home	-0.00993*	0.0126***	0.00691**
	(0.00557)	(0.00440)	(0.00341)
Work	-0.00147	-0.0124***	-0.00738**
	(0.00498)	(0.00431)	(0.00326)
Urban	0.00209	0.00914	0.00591
	(0.00859)	(0.00835)	(0.00599)
SMSA	-0.00597	-0.00662	-0.00553
	(0.00824)	(0.00790)	(0.00570)
Constant	-0.319***	-0.192*	-0.255***
	(0.119)	(0.111)	(0.0812)
N	41400	43875	85275



Graph 5: BMI by Age for Male Sample



Graph 6: BMI by Age for Female Sample

Table 4: First Stage Regression for Contraceptive Failure

	No. of Children
Contraceptive Failure	0.841***
	(0.0595)
Hispanic	0.228***
	(0.0573)
Black	0.187***
	(0.0532)
Low Education	0.737***
	(0.138)
Mid Education	0.366***
	(0.123)
Low AFQT	-0.0983
	(0.0636)
Mid AFQT	-0.0954*
	(0.0570)
Ever Married	0.838***
	(0.0537)
Work Experience	-0.0901***
	(0.00501)
Currently Working	0.141***
	(0.0487)
Log (Family Income)	0.0700***
	(0.0217)
Ever Has Health Problem	0.183***
	(0.0467)
Whether Career Oriented	-0.0285
	(0.128)
Regular Working Out	-0.0351
	(0.0525)
Regular Smoker	-0.130***
	(0.0482)
Regular Alcohol User	-0.0200
	(0.0723)
Urban Area	-0.0996**
	(0.0482)
SMSA Area	-0.0283
	(0.0598)
Constant	1.254***
	(0.257)
N	4073

Table 5: 2SLS with Fixed Effects Model

	General Health	SF12-Physical	SF12-Mental	SF12-Depression
No. of Child.	-0.333***	-0.643	-1.989*	-1.264**
	(0.123)	(0.971)	(1.085)	(0.546)
Hispanic	0.654	1.218	16.82*	7.102
	(0.977)	(7.716)	(8.624)	(4.338)
Black	0.349	0.786	10.98*	3.130
	(0.652)	(5.145)	(5.750)	(2.892)
Low Education	0.375	1.522	-1.073	0.540
	(0.237)	(1.869)	(2.089)	(1.051)
Mid Education	0.0962	0.239	-1.940	-0.216
	(0.187)	(1.477)	(1.650)	(0.830)
Low AFQT	-0.354***	-0.578	-1.304	-1.196**
	(0.123)	(0.973)	(1.088)	(0.547)
Mid AFQT	-0.123	-0.158	-0.204	-0.525
	(0.0916)	(0.723)	(0.808)	(0.406)
Ever Married	0.318**	0.416	2.369**	1.230**
	(0.127)	(1.004)	(1.122)	(0.564)
Work Experience	-0.0151	-0.0294	-0.0374	-0.0298
	(0.0127)	(0.1000)	(0.112)	(0.0562)
Currently Working	0.191***	2.736***	1.579**	0.289
	(0.0695)	(0.549)	(0.614)	(0.309)
Log (Family Income)	0.0242	0.143	0.914***	0.262*
	(0.0310)	(0.245)	(0.274)	(0.138)
Ever Has Health Problem	-0.176**	-3.712***	0.484	-0.0561
	(0.0756)	(0.597)	(0.667)	(0.336)
Whether Career Oriented	0.120	0.942	-2.401	-0.273
	(0.186)	(1.465)	(1.637)	(0.823)
Regular Working Out	0.267***	1.387**	0.612	0.407
	(0.0783)	(0.618)	(0.691)	(0.347)
Regular Smoker	-0.116	-0.177	-1.682**	-1.450***
	(0.0792)	(0.625)	(0.698)	(0.351)
Regular Alcohol User	-0.281***	-0.0990	-2.366**	-1.168**
	(0.107)	(0.841)	(0.940)	(0.473)
Urban Area	-0.0589	-0.339	0.367	-0.839**
	(0.0780)	(0.616)	(0.688)	(0.346)
SMSA Area	0.0985	0.775	-1.118	0.218
	(0.116)	(0.913)	(1.020)	(0.513)
Constant	4.055***	53.00***	42.34***	-0.0640
	(0.631)	(4.983)	(5.569)	(0.120)
N	1239	1239	1239	1239

Note:

1) Low Education is defined as years of education less than 12. Mid Education is defined as years of education between12 and 16. High Education is years of education 16 years and above.

2) Low AFQT is standardized AFQT score less than 30; Mid AFQT is score between 30 and 60. High AFQT is score equal or greater than 60.

3) Career Oriented is defined as at least has 16 years of education and 10 years of working experience before age 40.

4) Standard errors are clustered at family level.